

# Précis of *The Origin of Concepts*

**Susan Carey**

Department of Psychology, Harvard University, Cambridge, MA 02138  
scarey@wjh.harvard.edu

**Abstract:** A theory of conceptual development must specify the innate representational primitives, must characterize the ways in which the initial state differs from the adult state, and must characterize the processes through which one is transformed into the other. *The Origin of Concepts* (henceforth *TOOC*) defends three theses. With respect to the initial state, the innate stock of primitives is not limited to sensory, perceptual, or sensorimotor representations; rather, there are also innate conceptual representations. With respect to developmental change, conceptual development consists of episodes of qualitative change, resulting in systems of representation that are more powerful than, and sometimes incommensurable with, those from which they are built. With respect to a learning mechanism that achieves conceptual discontinuity, I offer Quinian bootstrapping. *TOOC* concludes with a discussion of how an understanding of conceptual development constrains a theory of concepts.

**Keywords:** bootstrapping; concept; core cognition; developmental discontinuity; innate primitive

## 1. Introduction

The human conceptual repertoire is a unique phenomenon, posing a formidable challenge to the disciplines of the cognitive sciences. How are we to account for the human capacity to create concepts such as *electron*, *cancer*, *infinity*, *galaxy*, and *wisdom*?

A theory of conceptual development must have three components. First, it must characterize the innate representational repertoire: the representations that are the input to subsequent learning processes. Second, it must describe how the initial stock of representations differs from the adult conceptual system. Third, it must characterize the learning mechanisms that achieve the transformation of the initial into the final state.

*The Origin of Concepts* (Carey 2009; henceforth *TOOC*) defends three theses. With respect to the initial state, contrary to historically important thinkers such as the British empiricists (Berkeley 1732/1919; Locke 1690/1975), Quine (1960) and Piaget (1954), as well as many contemporary scientists, the innate stock of primitives is not limited to sensory, perceptual, or sensorimotor representations; rather, there are also innate conceptual representations. With respect to developmental change, contrary to what has been written by continuity theorists such as Fodor (1980), Pinker (1994), Macnamara (1986), and others, conceptual development involves discontinuities, resulting in systems of representation that are more powerful than, and sometimes incommensurable with, those from which they are built. With respect to a learning mechanism that achieves conceptual discontinuity, I offer Quinian bootstrapping.

## 2. Relations between theories of conceptual development and theories of concepts

Obviously, our theory of conceptual development must mesh with our theory of concepts. Concepts are mental symbols, the units of thought. As with all mental representations, a theory of concepts must specify what it is that

determines the content of any given mental symbol (i.e., what determines which concept it is, what determines the symbol's meaning). (In the context of theories of mental or linguistic symbols, I take "content" to be roughly synonymous with "meaning.") The theory must also specify how it is that concepts may function in thought, by virtue of what they combine to form propositions and beliefs, and how they function in inferential processes. *TOOC* assumes, and ultimately argues for, a dual factor theory of concepts. The two factors are sometimes called "reference" and "narrow content." The contents of our mental representations are partly constituted by the set of entities they refer to. Some theories, (e.g., information semantics) claim that reference is determined by causal connections between mental symbols and the entities in their extensions. To the extent this is so, all current psychological theories of concepts are partly on the wrong track: Conceptual content is not exhausted by prototypes, exemplar representations, or theories of the entities in their extensions. The last chapter of *TOOC* reviews and endorses some of the arguments for information semantics. Contrary to philosophical views that deny that meanings are even partly determined by what's in the mind, however, *TOOC* argues that some aspects of inferential role are content determining (narrow content). The challenge for psychologists is specifying what aspects of mental representations at least partly determine their meaning, distinguishing these from those aspects that are simply what we believe about the represented entities. This is sometimes called distinguishing

SUSAN CAREY, Henry A. Morss, Jr., and Elizabeth W. Morss Professor of Psychology at Harvard University, has written extensively on conceptual development, with case studies of number representations, intuitive physics, and intuitive biology. Her work was recognized by election to the National Academy of Sciences in 2002 and the David E. Rumelhart Prize in 2009.

concepts from conception. One goal of *TOOC* is to explore how understanding of conceptual development requires a dual factor theory of concepts, and it suggests how we might approach characterizing which aspects of conceptual role are content determining. Considerations of conceptual development constrain a theory of narrow content.

*TOOC* also addresses a gap in the psychological literature on concepts. In cognitive psychology, concepts are taken to be representations of categories, and category learning is a large topic within the experimental literature on concepts and categories. Paradoxically, this literature does not try to account for the origin of the features that enter into the learning models it explores.

This was not always so. In theorizing about concepts, the British empiricists made accounting for acquisition a central concern. They, like many modern thinkers, assumed that all concept learning begins with a primitive sensory or perceptual vocabulary. That project is doomed by the simple fact that it is impossible to express the meanings of most lexical items (e.g., “cause,” “good,” “seven,” “gold,” “dog”) in terms of perceptual features. In response, some theorists posit a rich stock of developmental primitives, assuming that the definitional primitives that structure the adult conceptual repertoire, and the developmental primitives over which hypothesis testing is performed early in development, are one and the same set. A moment’s reflection shows that definitional primitives are poor candidates for developmental primitives. For example, the definition of *gold* within modern chemistry might be *element with atomic number 79*. Clearly, the primitives *element* and *atomic number* are not innate conceptual features. Or take the features that determine the prototype structure of bird concepts (flies, lays eggs, has wings, nests in trees, has a beak, sings, etc.). Subjects provide distinctive values for such features when asked to list the features of birds, and overlap in terms of these same features predicts prototypicality within the category *bird*. That is, this feature space definitely underlies adult prototypicality structure. Yet these features are not innate primitives; many are no less abstract and no less theory-laden than the concept *bird* itself. One of the goals of the *TOOC* is to characterize a learning process through which new primitives come into being.

### 3. The developmental primitives: Core cognition

Explaining the human capacity for conceptual understanding begins with the observation that evolution provides developmental primitives that are much richer than the sensorimotor representations that many hypothesize are the input to all learning. Some of these developmental primitives are embedded in systems of core cognition, and thus core cognition is the topic of the first half of *TOOC*. Core cognition resembles perception in many respects that distinguish it from other types of conceptual representations. These include the existence of innate perceptual input analyzers that identify the entities in core domains, a long evolutionary history, continuity throughout development, and iconic (or analog) format. The representations in core cognition differ from perceptual ones, however, in having conceptual content.

*TOOC* reviews the evidence for three systems of core cognition: one whose domain is middle-sized,

middle-distant objects, including representations of causal and spatial relations among them (*TOOC*, Ch. 2, 3, and 6); one whose domain is agents, including their goals, communicative interactions, attentional states, and causal potential (*TOOC*, Ch. 5 and 6); and the one whose domain is numbers, including parallel individuation, analog magnitude representations of the approximate cardinal values of sets, and set-based quantification (*TOOC*, Ch. 4 and 7).

#### 3.1. Conceptual content

Two logically independent and empirically distinct properties of core cognition representations lead me to attribute them conceptual content. First, contrary to empiricist proposals, they cannot be reduced to spatiotemporal or sensory vocabulary. One cannot capture concepts such as *goal*, *agent*, *object*, or *approximately 10* in terms of primitives such as locations, paths of motion, shapes, and colors. Second, they have a rich, central, conceptual role. The representations in core cognition are centrally accessible, represented in working memory models, and support decision and control mechanisms leading to action such as reaching. For example, infants make a working memory model of the individual crackers in each of two buckets and guide their choice of which bucket to crawl to from quantitative computations using those models. A variety of quantitative computations are defined over working memory representations of sets of objects. Preverbal infants can sum continuous quantities or compare models on the basis of one-to-one correspondence and categorically distinguish singletons from sets of multiple individuals. Moreover, young infants represent objects relative to the goals of agents, and infants’ representations of physical causality are constrained by their conceptualization of the participants in a given interaction (as agents capable of self-generated motion or as inert objects). Thus, the conceptual status of the output of a given core cognition system is confirmed by its conceptual interrelations with the output of other core cognition systems.

In other respects, the representations in core cognition resemble perceptual representations. Like representations of depth, the representations of objects, agents, and number are the output of evolutionarily ancient, innate, modular input analyzers. Like the perceptual processes that compute depth, those that create representations of objects, agents, and number continue to function continuously throughout the life span. And like representations of depth, their format is most likely iconic.

#### 3.2. Dedicated input analyzers

A dedicated input analyzer computes representations of one kind of entity, and only of that kind. All perceptual input analyzers are dedicated in this sense: The mechanism that computes depth from stereopsis does not compute color, pitch, or causality.

Characterizing the mechanisms that identify the entities in systems of core cognition is important for several reasons, including that knowing how an input analyzer works bears on what aspects of the world it represents. Analysis of the input analyzers underlines the ways in which core cognition is perception-like, and provides one source of evidence for the continuity of core cognition

throughout development. For example, the primacy of spatiotemporal information in creating object representations and tracing object identity through time is one of the signatures that identifies infant object representations with those of mid-level object-based attention and working memory in adults. Characterizations of the input analyzers bear on other theoretically important issues as well. For example, whether analog magnitude symbols are computed through a serial accumulation mechanism or by a parallel computation bears on whether they straightforwardly implement a counting algorithm, thus being likely to underlie learning to count (*TOOC*, Ch. 4 and 7). The evidence favors a parallel process. Or for another example, Chapter 5 (*TOOC*) presents evidence that infants use spatiotemporal descriptions of the motions and interactions among objects to assign agency, goals, and attentional states to them, and also that the static appearance of the entities in an event (e.g., presence of eyes and hands) also plays a role in creating representations of agency. Such results raise the question of whether one of these sources of information is primary. For example, infants may initially identify agents through patterns of interaction, and may then learn what these agents look like. Alternatively, the innate face detectors infants have may serve the purpose of identifying agents, allowing them then to learn how agents typically interact. A third possibility is that agency detection is like mother recognition in chicks, such an important problem for human infants that evolution hedged its bets with two dedicated input analyzers to do the trick.

### 3.3. Innateness

What I mean when I say that a representation is *innate*, is that the input analyzers that identify its referents are not the product of learning. That is, a representational capacity is innate, not necessarily any particular instantiated symbol for some entity in the world. This characterization is empty, of course, without a characterization of *learning mechanisms*. Broadly, these are mechanisms for which environmental input has the status of evidence. Obviously, theories of learning and theories of mental representations are mutually constraining. The first place we would currently look to explain the existence of innate input analyzers would be evolution; either they are adaptations under some direct selection pressure or byproducts of representational mechanisms that arose under selective pressure.

For the most part, the evidence reviewed in *TOOC* for core cognition does not derive from experiments with neonates. Rather, the evidence for the object representations of core cognition comes from studies of infants 2 months of age or older, and that for core representations of intentional agency comes from infants 5 months of age or older. Five months, and even two months, is a lot of time for learning. Why believe that the representations tapped in these experiments are the output of *innate* input analyzers, and why believe that the demonstrated inferential role that provides evidence for the content of the representations is unlearned? I discuss this question in each case study, appealing to four types of arguments.

First, that a given representational capacity may be innate in humans is suggested by evidence that it is manifest in *neonates* of other species. Examples offered were

depth perception, which emerges without opportunities for learning in neonate goats and neonate rats, and object representations, which are observed in neonate chicks. This line of evidence is obviously indirect, providing only an existence proof that evolution can build input analyzers that create representations with the content in question.

Second, the simultaneous emergence of different aspects of a whole system also provides indirect evidence for the innateness of the input analyzers and computational machinery that constitute core cognition. As soon as infants can be shown to form representations of complete objects, only parts of which had been visible behind barriers, they also can be shown to use evidence of spatiotemporal discontinuity to infer that two numerically distinct objects are involved in an event, and also to represent object motion as constrained by solidity (*TOOC*, Ch. 2 and 3). Similarly, different aspects of intentional attribution emerge together. For example, an infant's representing an entity as capable of attention increases the likelihood she will represent its action as goal directed, and vice versa (*TOOC*, Ch. 5). If the generalizations that underlie infants' behavior are learned from statistical analyses of the input (represented in terms of spatiotemporal and perceptual primitives), it is a mystery why all of the interrelated constraints implicated in the core cognition proposals emerge at once. Infants have vastly different amounts of input relevant to different statistical generalizations over perceptual primitives. Relative to the thousands of times they have seen objects disappear behind barriers, 2-month-old infants have probably never seen rods placed into cylinders, and have rarely seen solid objects placed into containers. Yet the interpretation of both types of events in terms of the constraints on object motion that are part of core cognition emerge together, at 2 months of age. Statistical learning of perceptual regularities would be expected to be piecemeal, not integrated.

Third, learnability considerations also argue that the representations in core cognition are the output of innate input analyzers. If the capacity to represent individuated objects, numbers, and agents are learned, built out of perceptual and spatiotemporal primitives, then there must be some learning mechanism capable of creating representations with conceptual content that transcend the perceptual vocabulary. In the second half of *TOOC*, I offer Quinian bootstrapping as a mechanism that could, in principle, do the trick, but this type of learning process requires explicit external symbols (e.g., words or mathematical symbols), and these are not available to young babies. Associative learning mechanisms could certainly come to represent regularities in the input. For example, a baby could form the generalization that if a bounded stimulus disappeared through deletion of the forward boundary behind another bounded stimulus, there is a high probability that a bounded stimulus resembling the one that disappeared will appear by accretion of the rear boundary from the other side of the constantly visible bounded surface. But such generalizations would not be formulated in terms of the concept *object*. There is no proposal I know for a learning mechanism available to nonlinguistic creatures that can create representations of objects, number, agency, or causality from perceptual primitives.



Fourth, success at some task provides support for some target representational capacity needed to perform it, whereas failure is not necessarily good evidence that the target capacity is lacking. Some other capacity, independent of the target one, may be needed for the task and may not yet be available (not yet learned or not yet matured). *TOOC* provides several worked-out examples of successful appeals to performance limitations masking putatively innate competences. For example, the A/not B error made by infants between ages 7 and 12 months is at least in part explained by appeal to immature executive function. For another example, infants' failure until 2 months of age to create representations of a complete rod partially hidden behind a barrier when they are shown the protruding ends undergoing common motion is at least partly explained by their failure to notice the common motion across the barrier. Thus, they lack the critical input to the putatively innate computation.

### 3.4. Iconic format

A full characterization of any mental representation must specify its format as well as its content and conceptual role. How are the mental symbols instantiated in the brain: are they language-like, diagram-like, picture-like, or something else? I intend the distinction between iconic and noniconic formats to be the same distinction that was at stake in the historical debates on the format of representation underlying mental imagery. Iconic representations are analog; roughly, the parts of the representation represent parts of the entities that are represented by the whole representation.

We know little about the format of most mental representations. Of the core cognition systems discussed in *TOOC*, the question of format is clearest for number representations, so my discussion of format was concentrated there (Ch. 4). The very name of “analog magnitude representations” makes a claim for their format. Analog representations of number represent as would a number line: the representation of 2 is a quantity that is smaller than and is contained in the representation of 3. We do not know how these analog representations are actually instantiated in the brain. Larger quantities could be represented by more neurons firing or by faster firing of a fixed population of neurons, for example. Many plausible models have been proposed (see *TOOC*, Ch. 4). That discrimination satisfies Weber's law (is a function of the ratio of set sizes) suggests that number representations work like representations of length, time, area, brightness, and loudness. All proposals for how all of these continuous dimensions are represented also deploy analog magnitudes.

*TOOC* speculates that all of core cognition is likely to be represented in iconic format. Consider the working memory models that constitute the parallel individuation system of object representations. The fact that these representations are subject to the set-size limit of parallel individuation implicates a representational schema in which each individual in the world is represented by a symbol in working memory. This fact does not constrain the format of these symbols. A working memory model for two boxes of different front surface areas, for instance, could consist of image-like representations of the objects ( $\square$ ), or they could be symbolic (*object*[3 square inches],

*object*[4 square inches]). These models must include some representation of size, bound to each symbol for each object, because the total volume or total surface area of the objects in a small set is computable from the working memory representations of the sets. The most plausible model for how this is done implicates iconic representations of the objects, with size iconically represented, as well as shape, color, and other perceptual properties bound to the symbols iconically. The iconic alternative laid out in *TOOC* Chapter 4 explains the set size limits on performance even when continuous variables are driving the response.

I have several other reasons for suspecting that the representations in core cognition are iconic. Iconic format is consistent with (though not required by) the ways in which the representations in core cognition are perception-like, assuming, as I believe to be the case (contrary to Pylyshyn 2002), that perceptual representations are iconic. Second, just as static images may be iconic or symbolic, so also may representations of whole events. If infants represent events in iconic format, like a movie that can be replayed, this could help make sense of the apparently retrospective nature of the representations that underlie many violation-of-expectancy looking-time experiments (Ch. 2–6). Finally, that core cognition may be represented in terms of iconic symbols, with some of its content captured in encapsulated computations defined over these symbols, may help to make sense of the extreme lags between understanding manifest in infant looking-time studies, and that manifest only much later in tasks that require explicit linguistic representations (*TOOC* Ch. 3, 5, 8–12). The guess that the format of *all* core cognition is iconic is just that: a guess. But the considerations just reviewed lead me to favor this hypothesis.

### 3.5. Constant through the life span

Continuity through the life span is an important property of core cognition for several reasons. We seek an account of cognitive architecture that carves the mind into meaningful sub-systems; and *most* conceptual representations are *not* continuous throughout development. Core cognition is one very distinctive part of the human mind: no other systems of conceptual representations share its suite of characteristics.

If innate input analyzers are the product of natural selection, one might think that they must be useful to adults as well as children. Expectation of continuity might seem to be the default. However, this first thought is not necessarily correct. Some innate representational systems serve only to get development started. The innate learning processes (there are two) that support chicks' recognizing their mother, for example, operate only in the first days of life, and their neural substrate actually atrophies when the work is done. Also, given that some of the constraints built into core knowledge representations are overturned in the course of explicit theory building, it is at least possible that core cognition systems themselves might be overridden in the course of development.

Thus, it is most definitely an empirical question whether core cognition is constant throughout the life span. *TOOC* argues that the answer is “yes” for the core cognition systems described therein. Evidence for continuity

includes the same signatures of processing in adulthood and infancy. Under conditions where core cognition is isolated from other conceptual resources, adults display the same limits on computations, and the same modular input analyzers, as do infants. For example, for both populations, the input analyzers that create object representations privilege spatiotemporal information over other perceptual features in the service of individuation and tracking numerical identity.

### 3.6. A dual factor theory of the concepts within core cognition

Dual factor theory straightforwardly applies to the representations that articulate core cognition. There are aspects of innate conceptual role that remain constant throughout development. These specify the narrow content of representations within core cognition. Furthermore, the representations within core cognition are the output of innate perceptual input analyzers. These input analyzers most probably come into being through natural selection, a process that, in this case, explains how the extension of the concepts within core cognition may be determined by causal connections between entities in their domains (objects, agents, goals, and cardinal values of sets) and the mental symbols that represent them.

## 4. Beyond core cognition: Central innate representations

### 4.1. Representations of cause

The existence of innate conceptual representations embedded within systems of core cognition does not preclude other innate conceptual representations as well, including non-domain-specific central ones. Chapter 6 takes the concept *cause* as a case study. Michotte (1946/1963) proposed that innate causal representations are the output of innate perceptual analyzers that take object representations and spatiotemporal relations among their motions as input. This is tantamount to the claim that causal representations are part of core object cognition, as Spelke (2002) suggested. Chapter 6 contrasts Michotte's hypothesis with the proposal that there may be innate central representations of causation. According to Michotte's proposal, the earliest causal representations should be sensitive only to spatiotemporal relations among events, and should be limited to reasoning about causes of object motion. An impressive body of empirical data establishes that by 6 months of age, infants represent Michottian motion events (launching, entraining, and expulsion) causally. Nonetheless, *TOOC* rejects Michotte's proposal on the grounds that causal cognition integrates across different domains of core cognition (object representations and agent representations), encompassing state changes as well as motion events, from as early in development as we have evidence for causal representations at all.

Innate central causal representations could come in either of two quite different varieties. There may be innate central processes that compute causal relations from patterns of statistical dependence among events, with no constraints on the kinds of events. Or there may be specific aspects of causality that are part of distinct core cognition systems (e.g., Michottian contact causality

within the domain of core object cognition, and intentional causality within the domain of agent cognition) and these may be centrally integrated innately. These possibilities are not mutually exclusive; both types of central integration of causal representations could be part of infants' innate endowment.

### 4.2. Public symbols, logical and linguistic capacity

*TOOC* says little about two important aspects of conceptual development. First, I assume that domain-specific learning mechanisms, jointly comprising a language acquisition device, make possible language acquisition, but I have made no effort to summarize the current state of the art in characterizing the language acquisition device. Whatever its nature, it is another way innate cognitive architecture goes beyond core cognition, for the symbols in language are not iconic, and language, like causality, integrates representations across separate core domains. Second, I have said almost nothing about the logical capacities humans and other animals are endowed with, although these are independent of core knowledge and I make use of various of them in my bootstrapping proposals. These are topics for other books.

Language acquisition and conceptual development are intimately related. The representations in core cognition support language learning, providing some of the meanings that languages express. *TOOC* Chapter 7 considers how pre-linguistic set-based quantification supports the learning of natural language quantifiers, and how pre-linguistic representations of individuals support the learning of terms that express sortals. But because my concern is the origin of concepts, I focused mainly on the complementary influence of language learning on conceptual development. Language learning makes representations more salient or efficiently deployed (Ch. 7; so-called weak effects of language learning on thought), and plays a role in conceptual discontinuities (strong effects of language learning on thought).

Chapter 7 reviews two cases in which very early language learning affects nonlinguistic representations. First, learning, or even just hearing, labels for objects influences the establishing/deploying of sortal concepts. Second, mastery of explicit linguistic singular/plural morphology plays a role in deploying this quantificational distinction in nonlinguistic representations of sets. Although *TOOC* argues that these are most likely weak effects of language learning on thought, "weak" does not entail "uninteresting" or "unimportant." Creating representations whose format is noniconic paves the way for integrating the concepts in core cognition with the rest of language.

Furthermore, most of the second half of the book concerns how language learning also shapes thought in a much stronger way. Language learning plays a role in creating new representational resources that include concepts that previously could not be entertained.

## 5. Discontinuity: The descriptive problem

Discontinuity in conceptual development arises at two different levels of abstraction. In terms of basic cognitive architecture, core cognition differs qualitatively from

explicit linguistically encoded knowledge. Consider the concepts *planet* or *germ*. These concepts are not the output of innate input analyzers, and therefore are neither innate nor causally connected to the entities they represent because of such analyzers, unlike the concepts in core cognition. They are not evolutionary ancient. Unlike core cognition representations, their format is certainly not iconic, and they are not embedded in systems of representation that are constant over development. Explicit conceptual representations can be, and often are, overturned in the course of conceptual development. Therefore, in terms of general cognitive architecture, explicit, verbally represented, intuitive theories are qualitatively different from, and hence discontinuous with, systems of core cognition.

Conceptual discontinuities are found at a more specific level as well: discontinuities within particular content domains. Establishing conceptual discontinuity at this level requires specifying the qualitative differences between two successive conceptual systems (CS1 and CS2). In some of the case studies in *TOOC*, new representational resources are constructed with more expressive power than those from which they are built. In other cases, theories are constructed whose concepts are incommensurable with those from which they are built. Both types of discontinuity (increased expressive power, incommensurability) involve *systems* of concepts and inferences, and so evidence for discontinuity must include evidence of within-child consistency over a wide range of probes of the underlying representational capacity. Also, discontinuity implies that mastery of CS2 should be difficult, and that there should be initial assimilation of input couched in the language of CS2 in terms of the concepts of CS1.

## 6. Discontinuities in the sense of increased expressive power; mathematical concepts

### 6.1. Natural number

Core cognition contains two systems of representation with numerical content: parallel individuation of small sets of entities in working memory models, and analog magnitude representations of number. Within the language acquisition device, a third innate system of representation with numerical content supports the learning of natural language quantifiers. These are the CS1s. CS2, the first explicit representational system that represents the positive integers, is the verbal numeral list embedded in a count routine. Deployed in accordance with the counting principles articulated by Gelman and Gallistel (1978), the verbal numerals implement the successor function, at least with respect to the child's finite count list. For any numeral that represents cardinal value  $n$ , the next numeral in the list represents  $n + 1$ .

CS2 is qualitatively different from each of the CS1s because none of the CS1s has the capacity to represent the integers. Parallel individuation includes no symbols for number at all, and has an upper limit of 3 or 4 on the size of sets it represents. The set-based quantificational machinery of natural language includes symbols for quantity (*plural, some, all*), and importantly contains a symbol with content that overlaps considerably with that of the verbal numeral "one" (namely, the singular determiner, "a"), but the singular determiner is not embedded within

a system of arithmetical computations. Also, natural language set-based quantification has an upper limit on the sets' sizes that are quantified with respect to exact cardinal values (*singular, dual, trial*). Analog magnitude representations include symbols for quantity that are embedded within a system of arithmetical computations, but they represent only approximate cardinal values; there is no representation of exactly 1, and therefore no representation of +1. Analog magnitude representations cannot even resolve the distinction between 10 and 11 (or any two successive integers beyond its discrimination capacity), and so cannot express the successor function. Therefore, none of the CS1s can represent 10, let alone 342,689,455.

This analysis makes precise the senses in which the verbal numeral list (CS2) is qualitatively different from those representations that precede it: it has a totally different format (verbal numerals embedded in a count routine) and more expressive power than any of the CS1s that are its developmental sources.

As suggested by CS2's being qualitatively different from each of the CS1s that contain symbols with numerical content, it is indeed difficult to learn. American middle-class children learn to recite the count list and to carry out the count routine in response to the probe "how many," shortly after their second birthday. They do not learn how counting represents number for another 1½ or 2 years. Young 2-year-olds first assign a cardinal meaning to "one," treating other numerals as equivalent plural markers that contrast in meaning with "one." Some 7 to 9 months later they assign cardinal meaning to "two," but still take all other numerals to mean essential "some," contrasting only with "one" and "two." They then work out the cardinal meaning of "three" and then of "four." This protracted period of development is called the "subset"-knower stage, for children have worked out cardinal meanings for only a subset of the numerals in their count list.

Many different tasks that make totally different information processing demands on the child confirm that subset-knowers differ qualitatively from children who have worked out how counting represents number. Subset-knowers cannot create sets of sizes specified by their unknown numerals, cannot estimate the cardinal values of sets outside their known numeral range, do not know what set size is reached if one individual is added to a set labeled with a numeral outside their known numeral range, and so on. Children who succeed at one of these tasks succeed at all of them. Furthermore, a child diagnosed as a "one"-knower on one task is also a "one"-knower on all of the others, likewise for "two"-knowers, "three"-knowers, and "four"-knowers. The patterns of judgments across all of these tasks show that parallel individuation and the set-based quantification of natural language underlie the numerical meanings subset-knowers construct for numeral words.

In sum, the construction of the numeral list representation is a paradigm example of developmental discontinuity. How CS2 transcends CS1 is precisely characterized, CS2 is difficult to learn, adult language expressing CS2 is represented by the child in terms of the conceptual resources of CS1, and children's performance on a wide variety of tasks consistently reflects either CS1 or CS2.



## 6.2. Rational number

*TOOC* Chapter 9 presents another parade case of developmental discontinuity within mathematical representations. In this case CS1 is the count list representation of the positive integers, enriched with an explicit understanding that there is no highest number, and so *number* means natural number. Early arithmetic instruction depends upon and further entrenches this representational system, representing addition and subtraction as counting up and counting down, and modeling multiplication as repeated addition. In CS2, “number” means any point on a number line that can be expressed  $x/y$ , where  $x$  and  $y$  are integers. In CS2, rather than it being the case that integers are the only numbers, there is an infinity of numbers between any two integers. The question of the next number after  $n$  (where  $n$  might be an integer or not) no longer has an answer. Therefore, CS2 has more expressive power than CS1 (CS1 cannot represent one-half as a number, nor any of the infinite noninteger rational numbers), and numbers are related to each other differently in the two systems. The new relation in CS2 is division. Division cannot be represented in terms of the resources of CS1, which model only addition, subtraction, and multiplication of integers. CS2’s division cannot be represented as repeated subtraction of integers.

CS2 is extremely difficult for children to learn. One-half of college-bound high school students taking the SAT exams do not understand fractions and decimals. Furthermore, explicit instruction concerning rational number is initially assimilated to CS1, and children are consistent over a wide range of probes as to how they conceptualize number. Whether children can properly order fractions and decimals, how they justify their ordering, how they explain the role of each numeral in a fraction expressed  $x/y$ , whether they agree there are numbers between 0 and 1 and whether they believe that repeated division by 2 will ever yield 0 are all interrelated. What the child does on one of these tasks predicts what he or she will do on all of the others. CS1 and CS2 are each coherent conceptual systems, qualitatively different from each other.

## 7. Discontinuities in the sense of local incommensurability: Natural kind concepts

Conceptual discontinuity is not only a matter of increased expressive power. Sometimes, two successive conceptual systems are qualitatively different because they are locally incommensurable and therefore not mutually translatable. One cannot express the beliefs that articulate CS2 in the concepts of CS1 and vice versa.

Incommensurability arises when episodes of conceptual development have required conceptual change. Conceptual changes are of several kinds, including differentiations such that the undifferentiated concept in CS1 plays no role in CS2, and is even incoherent from the point of view of CS2; coalescences in which ontologically distinct entities from the point of view of CS1 are subsumed under a single concept in CS2; and changes in conceptual type and in content-determining conceptual cores.

The analysis of incommensurability in *TOOC* illustrates the fruits of what Nersessian (1992) calls “cognitive

historical analysis,” in which philosophers and historians of science join forces with cognitive scientists to understand knowledge acquisition both in the history of science and over individual ontogenesis. *TOOC* shows that the same questions can be asked of episodes of knowledge acquisition in individual children and historical theory changes, in spite of the manifest differences between scientists and children, and that sometimes these questions receive the same answers in the two cases. Examples are: what is an “undifferentiated concept,” what counts as evidence for lack of conceptual differentiation, and what distinguishes episodes of conceptual development that merely involve belief revision from those involving conceptual change? Conceptual change occurs when sets of concepts that are interdefined are acquired together, *en suite*, with content determining interconnections that differ from those in CS1 and with new concepts emerging that are not representable in CS1. *TOOC* Chapter 10 sketches a historical example of conceptual change between the source–recipient and caloric theories of thermal phenomena, focusing on the differentiation of the concepts *heat* and *temperature*. The developmental example juxtaposed to this involves incommensurable intuitive theories of the physical world, focusing on the differentiation of the concepts *physical* and *material* and the concepts *weight* and *density*.

Chapter 10 describes many phenomena that suggest that children’s concepts of the physical world may be incommensurable with ours: their confidence that a small piece of Styrofoam weighs 0 grams, nonconservation of amount of matter and of weight, the claim that dreams are made of air, that shadows exist in the dark but we just can’t see them. At the heart of establishing local incommensurability is characterizing two successive physical theories, providing evidence that each is a theory children actually hold, and, of course, displaying the incommensurability. Chapter 10 characterizes an initial theory (CS1), in which an undifferentiated concept *weight/density* functions coherently. A translator’s gloss is provided, sketching the central concept *degree of heaviness* akin to the Florentine Experimenter’s *degree of heat*, which was analogously undifferentiated between *heat* and *temperature*. A sketch of CS1’s concept *physically real/substantial*, the concept closest to CS2’s *material*, was also part of the translator’s gloss, as was a sketch of the undifferentiated concept *air/nothing*. The concepts that articulate the child’s CS1’s undifferentiated concepts cannot be expressed in terms of any conceptual system that differentiates them; they are incoherent from the point of view of CS2.

Chapter 10 also characterizes conceptual changes other than differentiation. It documents ancestor concepts in CS1 that represent kinds as ontologically distinct, which CS2 unites under a single concept. An example is CS2’s *matter*, uniting what are vastly different kinds in CS1 (*object*, *liquid*, *air*). Ancestor concepts in CS1 also differ from their descendents in CS2 in type and features taken to be essential. The essential features of CS1’s undifferentiated concept *matter/physically real* are perceptual access and causal interaction with other external physical entities. The essential features of the CS2’s *matter* are weight and occupying space. An interconnected change occurs within the concept *degree of heaviness*. In CS1, degree of heaviness is a property of *some*

material/physically real entities, such as a large piece of Styrofoam but not a small piece. In CS2, weight is taken to be an essential feature of all material entities, a property that provides an extensive measure of amount of matter. The local incommensurability between CS1 and CS2 derives from the simultaneous adjusting these concepts to each other. Differentiations implicating incommensurability never occur independent of simultaneous coalescences, nor of changes of the causally deepest properties known of each of a system of interrelated concepts.

If this analysis is correct, CS2 should be difficult to learn, and indeed it is. Although CS2 is the target of science instruction, a large proportion of secondary school students fail to undergo the conceptual change. Finally, there is striking within-child consistency across the many disparate tasks that diagnose CS1 and CS2: sorting entities as matter/nonmatter, representing matter, weight, and volume as continuous extensive variables, modeling weight, density, and volume of a set of objects, ordering objects with respect to weight, density, and volume, and measuring weight, density, and volume.

Pondering children's responses on the tasks probing these concepts is what allows the reader to come to understand CS1. Constructing a set of coherent concepts that yield the same judgments as those of children with CS1 is a bootstrapping process. Aided by the translator's gloss, the reader must create a conceptual system in which *degree of heaviness* functions coherently.

## 8. Quinian bootstrapping

Ultimately, learning requires adjusting expectations, representations, and actions to data. Abstractly, all of these learning mechanisms are variants of hypothesis-testing algorithms. The representations most consistent with the available data are strengthened; those hypotheses are accepted. However, in cases of developmental discontinuity, the learner does not initially have the representational resources to state the hypotheses that will be tested, to represent the variables that could be associated or could be input to a Bayesian learning algorithm. Quinian bootstrapping is one learning process that can create new representational machinery, new concepts that articulate hypotheses previously unstatable.

In Quinian bootstrapping episodes, mental symbols are established that correspond to newly coined or newly learned explicit symbols. These are initially placeholders, getting whatever meaning they have from their interrelations with other explicit symbols. As is true of all word learning, newly learned symbols must of necessity be initially interpreted in terms of concepts already available. But at the onset of a bootstrapping episode, these interpretations are only partial. The learner (child or scientist) does not yet have the capacity to formulate the concepts the symbols will come to express.

The bootstrapping process involves modeling the phenomena in the domain, represented in terms of whatever concepts the child or scientist has available, in terms of the set of interrelated symbols in the placeholder structure. Both structures provide constraints, some only implicit and instantiated in the computations defined over the representations. These constraints are respected as much as possible in the course of the modeling

activities, which include analogy construction and monitoring, limiting case analyses, thought experiments, and inductive inference.

### 8.1. Bootstrapping representations of natural number

TOOC draws on Quinian bootstrapping to explain all the developmental discontinuities described in the previous section. In the case of the construction of the numeral list representation of the integers, the memorized count list is the placeholder structure. Its initial meaning is exhausted by the relation among the external symbols: They are stably ordered. "One, two, three, four..." initially has no more meaning for the child than "a, b, c, d..." The details of the subset-knower period suggest that the resources of parallel individuation, enriched by the machinery of linguistic set-based quantification, provide the partial meanings children assign to the placeholder structures that get the bootstrapping process started. The meaning of the word "one" could be subserved by a mental model of a set of a single individual (i), along with a procedure that determines that the word "one" can be applied to any set that can be put in one-to-one correspondence with this model. Similarly "two" is mapped onto a long term memory model of a set of two individuals (j k), along with a procedure that determines that the word "two" can be applied to any set that can be put in one-to-one correspondence with this model. And so on for "three" and "four." This proposal requires no mental machinery not shown to be in the repertoire of infants: parallel individuation, the capacity to compare models on the basis of one-to-one correspondence, and the set-based quantificational machinery that underlies the singular/plural distinction and makes possible the representation of dual and trial markers. The work of the subset-knower period of numeral learning, which extends in English-learners between ages 2.0 and 3.6 or thereabouts, is the creation of the long-term memory models and computations for applying them that constitute the meanings of the first numerals the child assigns numerical meaning to.

Once these meanings are in place, and the child has independently memorized the placeholder count list and the counting routine, the bootstrapping proceeds as follows: The child notices the identity between the singular, dual, trial, and quadral markers and the first four words in the count list. The child must try to align these two independent structures. The critical analogy is between order on the list and order in a series of sets related by *additional individual*. This analogy supports the induction that any two successive numerals will refer to sets such that the numeral farther in the list picks out a set that is 1 greater than that earlier in the list.

This proposal illustrates all of the components of bootstrapping processes: placeholder structures whose meaning is provided by relations among external symbols, partial interpretations in terms of available conceptual structures, modeling processes (in this case analogy), and an inductive leap. The greater representational power of the numeral list than that of any of the systems of core cognition from which it is built derives from combining distinct representational resources: a serially ordered list; set-based quantification (which gives the



child singular, dual, trial, and quadral markers, as well as other quantifiers); and the numerical content of parallel individuation (which is largely embodied in the computations performed over sets represented in memory models with one symbol for each individual in the set). The child creates symbols that express information that previously existed only as constraints on computations. Numerical content does not come from nowhere, but the process does not consist of defining “seven” in terms of mental symbols available to infants.

### 8.2. Bootstrapping in the construction of explicit scientific theories

Historians and philosophers of science, as well as cognitive scientists, working with daily records of scientists’ work, have characterized the role of Quinian bootstrapping in scientific innovation. Chapter 11 draws out some of the lessons from case studies of Kepler (Gentner 2002), Darwin (Gruber & Barrett 1974), and Maxwell (Nersessian 1992).

In all three of these historical cases, the bootstrapping process was initiated by the discovery of a new domain of phenomena that became the target of explanatory theorizing. Necessarily, the phenomena were initially represented in terms of the theories available at the outset of the process, often with concepts that were neutral between those theories and those that replaced them. Incommensurability is always local; much remains constant across episodes of conceptual change. For Kepler, the phenomena were the laws of planetary motion; for Darwin, they were the variability of closely related species and the exquisite adaptation to local environmental constraints; for Maxwell, they were the electromagnetic effects discovered by Faraday and others.

In all three of these cases the scientists created an explanatory structure that was incommensurable with any available at the outset. The process of construction involved positing placeholder structures and involved modeling processes which aligned the placeholders with the new phenomena. In all three cases, this process took years. For Kepler, the hypothesis that the sun was somehow causing the motion of the planets involved the coining of the placeholder concept *vis motrix*, a term that designated the force/energy emitted by the sun responsible for the motion of the planets. Analogies with light and magnetism allowed him to fill in many details concerning the nature of *vis motrix*, as well as to confirm, for him, its existence. It became the ancestor concept to Newton’s *gravity*, although Newton obviously changed the basic explanatory structure. Gravity due to the sun doesn’t explain the planets’ motion, but rather their deviations from constant velocity.

For Darwin, the source analogies were artificial selection and Malthus’ analysis of the implications of a population explosion for the earth’s capacity to sustain human beings. For Maxwell, a much more elaborate placeholder structure was given by the mathematics of Newtonian forces in a fluid medium. These placeholders were formulated in external symbols – natural language, mathematical language, and diagrams.

Of course, the source of these placeholder structures in children’s bootstrapping is importantly different from that of scientists. The scientists posited them as tentative

ideas worth exploring, whereas children acquire them from adults, in the context of language learning or science education. This difference is one reason meta-conceptually aware hypothesis formation and testing is likely to be important in historical cases of conceptual change. Still, many aspects of the bootstrapping process are the same whether the learner is a child or a sophisticated adult scientist. Both scientists and children draw on explicit symbolic representations to formulate placeholder structures and on modeling devices such as analogy, thought experiments, limiting case analyses, and inductive inference to infuse the placeholder structures with meaning.

### 8.3. Bootstrapping processes underlying conceptual change in childhood

Historically, mappings between mathematical structures and physical ones have repeatedly driven both mathematical development and theory change. In the course of creating the theory of electromagnetic fields, Maxwell invented the mathematics of quantum mechanics and relativity theory. Another salient example is Newton’s dual advances in calculus and physics.

In childhood, as well, constructing mappings between mathematical and physical representations plays an essential role in conceptual change both within mathematics and within representations of the physical world. Chapter 11 illustrates this with case studies of the creation of concepts of rational number and the creation of theory of the physical world in which weight is differentiated from density. These two conceptual changes constrain each other. The child’s progress in conceptualizing the physical world exquisitely predicts understanding of rational number and vice versa. Children whose concept of number is restricted to positive integers have not yet constructed a continuous theory of matter nor a concept of weight as an extensive variable, whereas children who understand that number is infinitely divisible have done both.

Smith’s bootstrapping curriculum (Smith 2007) provides insight into the processes through which *material* becomes differentiated from *physically real* and *weight* from *density*. Although developed independently, Smith’s curriculum draws on *all* of the components of the bootstrapping process that Nersessian (1992) details in her analysis of Maxwell. First, Smith engages students in explaining new phenomena, ones that can be represented as empirical generalizations stated in terms of concepts they already have. These include the proportionality of scale readings to overall size (given constant substance), explaining how different-size entities can weigh the same, predicting which entities will float in which liquids, and sorting entities on the basis of whether they are material or immaterial, focusing particularly on the ontological status of gases. She then engages students in several cycles of analogical mappings between the physical world and the mathematics of extensive and intensive variables, ratios, and fractions. She begins with modeling the extensive quantities of weight and volume with the additive and multiplicative structures underlying integer representations. The curriculum then moves to calculating the weight and volume of very small entities, using division. These activities are supported by thought experiments (which are themselves

modeling devices) that challenge the child's initial concept *weight as felt weight/density*, leading them into a contradiction between their claim that a single grain of rice weighs 0 grams, and the obvious fact that 50 grains of rice have a measurable weight. Measuring the weight of a fingerprint and a signature with an analytical balance makes salient the limits of sensitivity of a given measurement device, and further supports conceptualizing weight as an extensive variable that is a function of amount of matter.

To complete the differentiation of *weight* from *density* Smith makes use of visual models that represent the mathematics of extensive and intensive quantities. The visual models consist of boxes of a constant size, and numbers of dots distributed equally throughout the boxes. Numbers of dots and numbers of boxes are the extensive variables, numbers of dots per box the intensive variable. Students first explore the properties of these objects in themselves, discovering that one can derive the value of any one of these variables knowing the values of the other two, and exploring the mathematical expression of these relations: dots per box equal number of dots divided by number of boxes. The curriculum then moves to using these visual objects to model physical entities, with number of boxes representing volume and number of dots representing weight. Density (in the sense of weight/volume) is visually represented in this model as dots/box, and the models make clear how it is that two objects of the same size might weigh different amounts, for example because they are made of materials with different densities; why weight is proportional to volume given a single material, and so on. The models are also used to represent liquids, and students discover the relevant variables for predicting when one object will float in a given liquid and what proportion of the object will be submerged. This activity is particularly satisfying for students, because at the outset of the curriculum, with their undifferentiated *weight/density* concept, they cannot formulate a generalization about which things will sink and which will float. Differentiating *weight* from *density* in the context of these modeling activities completes the construction of an extensive concept *weight* begun in the first part of the curriculum.

The formula  $D = W/V$  (density equals weight divided by volume) is a placeholder structure at the beginning of the bootstrapping process. The child has no distinct concepts *weight* and *density* to interpret the proposition. As in all cases of bootstrapping, the representation of placeholder structures makes use of the combinatorial properties of language. Density here is a straightforward complex concept, defined in terms of a relation between weight and volume (division), and the child (if division is understood) can understand this sentence as: something equals something else divided by something (most children also have no concept of volume at this point in development). The dots per box model is also a placeholder structure at the beginning of the bootstrapping process, a way of visualizing the relations between an intensive variable and two extensive variables related by division, and therefore provides a model that allows the child to think with, just as Maxwell's visual models allowed him to think with the mathematics of Newtonian mechanics as he tried to model Faraday's phenomena. At the outset of the process the child has no distinct concepts *weight* and

*density* to map to number of dots and number of dots/box, respectively.

Although straightforward conceptual combination plays a role in these learning episodes (in the formulation of the placeholder structures), the heart of Quinian bootstrapping is the process of providing meaning for the placeholder structures. At the outset *weight* is interpreted in terms of the child's undifferentiated concept *degree of heaviness* (see Chapter 10) and *density* has no meaning whatsoever for the child. It is in terms of the undifferentiated concept *degree of heaviness* that the child represents the empirical generalizations that constitute the phenomena he or she is attempting to model. The placeholder structure introduces new mental symbols (*weight* and *density*). The modeling processes, the thought experiments and analogical mapping processes, provide content for them. The modeling process, using multiple iterations of mappings between the mathematical structures and the physical phenomena, makes explicit in a common representation what was only implicit in one or the other representational systems being adjusted to each other during the mapping.

That bootstrapping processes with the same structure play a role in conceptual change both among adult scientists and young children is another outgrowth of cognitive historical analysis. As I have repeatedly mentioned, of course there are important differences between young children and adult scientists engaged with metacognitive awareness in explicit theory construction. Without denying these differences, Chapters 8–12 illustrate what the theory–theory of conceptual development buys us. By isolating questions that receive the same answers in each case, we can study conceptual discontinuities and the learning mechanisms that underlie them, bringing hard-won lessons from each literature to bear on the other.

## 9. Implications for a theory of concepts

The first 12 chapters of the book paint a picture of how conceptual development is possible. The thirteenth steps back and explores the implications of what has been learned for a theory of concepts. I detail a range of phenomena that *prima facie* we might think a theory of concepts should explain. A theory of concepts should contribute to our understanding of the human capacities for categorization and productive thought (conceptual combination and inference). It must explain how mental symbols refer, and how they support epistemic warrant, and should allow us to formulate alternative theories of acquisition. And it must distinguish between our concepts of entities in the world and our beliefs about them.

As many commentators have noted, the empiricists' theory (sometimes called the "classical view") was unmatched in its scope, offering machinery that met all of the above desiderata. Of particular importance to me, the actual empiricists' classical view put explaining concept acquisition front and center. *TOOC* Chapter 13 summarizes the empiricists' theory, and sketches why both psychologists and philosophers abandoned it as hopeless.

*TOOC* is an extended argument against the classical view of concept acquisition. The primitives from which

our concepts are built are not solely sensory, and the processes of building them are not exhausted by assembling definitions (or even representations of prototypes) in terms of the primitive base. For the most part, however, the failure to account for acquisition was not the reason most psychologists abandoned the empiricists' theory. Rather, the culprit was its failure to account for categorization data. However, many considerations weigh against making categorization processes the central explanandum for a theory of concepts. Writers as disparate as Murphy (2002) and Fodor (1983) have argued that categorization is a holistic process, drawing on everything we know about the entities that fall under a concept, and I agree. But this is precisely why experiments on categorization decisions cannot be a pipeline to our analysis of concepts. We cannot isolate the contribution of conceptual content from the beliefs we hold about the entities that fall under a concept (concepts as distinct from conceptions) in determining the data from categorization experiments.

In my view, more telling arguments against the classical view derive from the philosophical literature on information semantics and wide content, and from the literature on psychological essentialism that was inspired by it. Kripke's analysis (Kripke 1972/1980) of the processes that fix the reference of proper names and Kripke's and Putnam's extension of this analysis to natural kind terms convince me (see also Putnam 1975) that reference is not determined solely by what's in the mind.

This work undermines not only the classical view of concepts, but also prototype/exemplar theories and a purely internalist theory—theory of concepts. The phenomena in support of psychological essentialism point to the same conclusion. We can and do deploy our concepts in the absence of any knowledge of the entities that fall under them that would allow us to pick out those entities, and under the assumption that everything we know about them is revisable. At least in the case of natural kind concepts, we assume that what determines their extension is a matter for science to discover, not for us to stipulate as a matter of definition, or for our current prototypes or theories to determine, even probabilistically. In my view, these considerations force a role for wide content in a theory of concepts.

One theory of information semantics, Fodor (1998), also places concept acquisition front and center. Fodor's view, at least at one time (e.g., Fodor 1975), led him to a radical concept nativism, according to which all concepts at the grain of single lexical items are innate. *TOOC* is also an extended argument against radical concept nativism. In addition, Chapter 13 argues against a pure information theory; we cannot ignore what is inside the mind in our theory of concepts. Conceptual role has potentially three different roles to play in such a theory. First, concepts underlie thought, inference, and planning. Therefore, whatever we posit concepts to be must be compatible with these functions, and this provides a constraint on what concepts can be. Second, on at least some approaches to information semantics, recognition processes are part of the causal connection between entities in the world and the mental symbols that represent them. Although the mechanisms that support categorization decisions may not determine content in the way that traditional theories envisioned, they may have a

place in a theory of how reference is determined. Both of the aforementioned functions of conceptual role are compatible with a pure information theory. A dual factor theory requires more. Thirdly, some aspects of conceptual role may be genuinely content determining. We may be justified in our commitment to narrow content.

*TOOC* Chapter 13 details many arguments for narrow content. Some of the desiderata for a theory of concepts, including accounting for conceptual productivity and accounting for incommensurability, require appeals to conceptual role. Conceptual role exhausts the content of logical connectives, and surely has pride of place in content determination of mathematical symbols. Furthermore, Block (1986) showed that even accepting the Kripke/Putnam arguments for wide content, conceptual role still has a place in the determination of the extension of natural kind concepts. It is a fact of our psychology that a given concept is a natural kind concept, that is, is assumed under psychological essentialism. That is, conceptual role determines the nature of the function between the world and a given mental symbol for a natural kind, even if everything we believe about the entities that fall under a given natural kind concept are up for revision.

Of course, the challenge for any theory of narrow content is specifying, at least in principle, how we can separate which aspects of conceptual role are content determining and which are merely part of what we believe to be true about the entities a given mental symbol picks out. Otherwise we must endorse a holistic approach to narrow content. Moreover, if we can specify how to separate conceptual role into these two components, we bolster our confidence in a dual factor theory.

Fully grasping the implications of conceptual discontinuities, plus the nature of the bootstrapping processes that achieve them, provides one route into motivating narrow content and specifying which aspects of conceptual role are content determining. An appreciation of Quinian bootstrapping allows us to see how new primitive symbols are coined in the first place. At the outset of a bootstrapping episode, the concepts in placeholder structures have only narrow content; it is entirely given by conceptual role within the placeholder structure. I propose that those aspects of conceptual role in descendent structures that maintain that initial conceptual role, or derive from it through conceptual change, are among those aspects of conceptual role that determine narrow content. This proposal is consistent with others in the literature: that the most causally or inferentially central aspects of conceptual role determine narrow content. The constraint satisfaction modeling processes that enrich the content of placeholder structures maximize causal/inferential coherence. In sum, I appeal to Quinian bootstrapping in my thinking about narrow content, as well as in my reply to Fodor's radical concept nativism.

## 10. Concluding remark

Explaining the human capacity for conceptual thought has animated philosophical debate since the time of the ancient Greeks, and has animated psychological debate since the dawn of experimental psychology in the nineteenth century. The many case studies in



TOOC illustrate the interdependence of the projects of explaining the origin of concepts and understanding what concepts are.

## Open Peer Commentary

### You can't get there from here: Foundationalism and development

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Jedediah W. P. Allen<sup>a</sup> and Mark H. Bickhard<sup>b</sup>

*Departments of Psychology<sup>a</sup> and Philosophy,<sup>b</sup> Lehigh University, Bethlehem, PA 18015.*

jwa204@lehigh.edu    mhb0@lehigh.edu

**Abstract:** The thesis of our commentary is that the framework used to address what are taken by Carey to be the open issues is highly problematic. The presumed necessity of an innate stock of representational primitives fails to account for the emergence of representation out of a nonrepresentational base. This failure manifests itself in problematic ways throughout Carey's book.

We are in agreement with Carey's proposal that there must be a way to generate new representational "primitives," and that the idea of implicit definition, which underlies her model of Quinian bootstrapping, is, in general, the correct approach to address this problem (Carey 2009). However, the overall model encounters serious troubles beyond those points. (Implicit definition can be a complex topic in model theory, but the central idea is that a [formal] system implicitly defines the class of all of its satisfiers or models; Bickhard & Terveen 1995; Bickhard 2009.)

Carey says that Quinian bootstrapping requires language, but gives no reason why such implicit definitions couldn't be constructed out of *internal* computational "empty symbols" – for example, perhaps via some version of conceptual role implicit definition. (To the extent that Quinian bootstrapping generates representational content via conceptual or inferential role, it is roughly a version of implicit definition. To the extent that phenomena such as analogy are considered to be *necessary* to Quinian bootstrapping for the generation of content, then it is no longer modeling a form of emergent representation – "primitives" – but relies on earlier representation in order to construct "new" representation.)

Clearly, evolution had some way to create emergent new representations out of non-representational phenomena, and not by using language. But, if implicit definition could work computationally (without language), why couldn't it work for infants and toddlers? That is, the success of Quinian bootstrapping in terms of implicit definition would obviate an innate representational base.

More generally, *whatever* the process was by which evolution supposedly created emergent representation – implicit definition or not – why couldn't that same or similar process be functioning for infants and toddlers? Why, in other words, are innate representations needed at all? Carey's position depends on Fodor's argument for the assumption of a necessary (though not full lexical-level) nativism, but she contradicts Fodor with her notion of the Quinian bootstrapping construction of "primitives" – and Fodor *also* has no answer to the question of why learning and development cannot make use of whatever process was involved in the evolutionary emergence of representation.

On the other hand, if representation is constituted as information semantics has it, then the generation of new

"representation" – of new covariations – should be in-principle trivial. Almost any input processing will generate covariations, which, by this model, should constitute representations of whatever the covariations are with. Many such covariations will probably not be useful, but there is no problem learning or developing more.

If generating new covariations is so easy, why does Fodor postulate nativism at all? Because covariation cannot by itself constitute *content*, and, as Fodor points out, we have no model of learning that can account for new content (Piattelli-Palmarini 1980, cf. p. 269). Carey proposes a way to generate emergent content – bootstrapping – but does not seem to see that any such model invalidates the basic arguments for nativism with which the whole approach begins.

Carey's reliance on an information semantic framework for her discussion of the many early development studies is not only contradicted by the possibility of something like "computational bootstrapping," it also generates problems of its own. In particular, within an information semantic framework, Carey's distinction between perceptual and conceptual representation is questionable. No input processing system is necessarily restricted to pure combinations of whatever "vocabulary" the basic transducers generate (e.g., a connectionist input processor is not, even though we would not argue that connectionist models are adequate). But it is solely in terms of such restrictions to "perceptual vocabularies" that she differentiates conceptual from perceptual. On her account, conceptual input processors are still just input processors, but are not restricted to combinatorics of perceptual "vocabularies." But, if no input processor is so restricted, then what happens to the distinction? Still further, the very intuition of a perceptual base is put into question by models such as Gibson's (1979), and such possibilities are not addressed.

We argue, then, that the framework within which these many studies have been done, and within which Carey interprets them, is itself flawed, and, therefore, does not support rigorous analysis and methodological design: Perceptual nativism and conceptual nativism are not the only alternatives, even for early development. Among other problems, there is an intrinsic and therefore systematic neglect of possibilities of developmental constructions of *emergent* representation in the methodological designs of these foundationalist studies (Allen & Bickhard, in preparation) – such emergentist possibilities do not exist in the conceptual framework being used, and, therefore, do not occur to researchers working within that framework in their designs.

In this regard, it is worth pointing out that Piaget had a model of emergent constructivism of representation (though not one that we fully endorse; Bickhard & Campbell 1989), but that Carey systematically mischaracterizes Piaget's model as an "empiricism" based on "sensorimotor representations." He had no such model, and, in fact, argued consistently against such approaches, seeking what he called a "third way" that would transcend both empiricism and rationalism (Müller et al. 2009). For one contemporary instance of a model of action-based emergent representation, see Bickhard (2009).

The basic problem here is that, aside from the bootstrapping model, Carey's discussion presupposes a foundationalism of representation – representation constructed solely in terms of an innate foundation of representational primitives. A foundationalism cannot account for its own foundations, and shifting the burden to evolution does not solve the problem; it only presupposes (without argument) that evolution can engage in processes generating emergent representation that learning and development cannot engage in. Quinian bootstrapping contradicts foundationalism, and thereby contradicts the framework for the "perceptual nativism" versus "conceptual nativism" debate, but this is not recognized, because of the assumption that implicit definition requires language (but, if so, then the *evolutionary* emergence of representation again becomes inexplicable).

So, foundationalism simply refuses to address questions of the origins of representation, and, thus, cannot be a successful

framework for understanding either cognition or cognitive development. If those are your goals, you can't get there from here.

## Border crossings: Perceptual and post-perceptual object representation

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Tyler Burge

Department of Philosophy, University of California, Los Angeles, Los Angeles, CA 90095.

burge@ucla.edu

**Abstract:** Carey's claim that no object representations are perceptual rests on a faulty view of perception. To delineate origins of post-perceptual ("conceptual" or "core cognitive") representation, we need a more accurate view of perceptual representation.

In *The Origin of Concepts*, Susan Carey argues that object representations are exclusively nonperceptual (Carey 2009). I believe that this view is mistaken. There are object perceptions – what I call “perceptual attributives as of objects” – at least in the visual and tactile perceptual modalities of a wide range of animals. There are, of course, also nonperceptual object representations. Some of these might be termed “concepts.” But perceptual object representations are not concepts. Carey's fine book describes important aspects of object representation. However, there is a need to evaluate which of the phenomena that she discusses are postperceptual and which have origins already in perceptual systems.

Carey claims, “representations of object cannot be stated in the vocabulary of perception” (Carey 2009, p. 63). She cites Piaget and Quine, with approval, as predecessors in denying that object representations are perceptual. Piaget is credited with the claim that object representations are nonperceptual because they are multimodal (Carey 2009, p. 34) (Piaget, 1954). Piaget does not show that all object representations are multimodal, only that some are. (There is evidence that visual object representations do not require intermodal experience.) Visual–perceptual object representations can underlie multimodal object representations.

Carey associates Quine (Quine 1960) with this further argument:

If perceptual representations are limited to what currently experienced entities look like... objects cannot be represented as individuals that persist through time, independently of the observer... As Quine pointed out, a perceptual vocabulary does not include fundamental quantificational devices. The child could not represent a given object as the same one as one seen earlier, for sensory representations do not provide criteria for numerical identity. (Carey 2009, p. 34; see also pp. 40, 63, 94–6)

Carey adds, seemingly in her own voice:

The criteria for individuation and numerical identity for ordinary objects go beyond perceptual primitives... Although perceptual primitives can specify a currently perceived, bounded entity and its current path of motion, they do not specify that the entity continues to exist when we lose perceptual contact with it. (Carey 2009, p. 36)

It is a mistake to require of a system that has object representations that it have quantificational devices, or representations of criteria for numerical identity, or *specifications* of continuity under loss of perceptual contact (using that “vocabulary”). Nor need object *perception* represent particulars *as* persisting, or *as* independent of an observer, or *as* unperceived, in order to perceptually represent something as an object or body. A perception as of objects need not represent persistence, observers, perceptual contact, or independence from observers. Such requirements confuse principles according to which perception operates with representations that occur in perceptual object representation.

It is enough that a perceptual system operate under principles that require of the perceiver a capacity to track objects perceptually – where tracking requires coordination of perception with perceptual memory and perceptual anticipation. Therefore, Carey's remarks about quantification, criteria, and the inability of perceptions to specify continuity under loss of perceptual contact, are irrelevant to whether perceptual representations can represent entities as objects or bodies. (I discuss the points of the last two paragraphs in Burge 2010, Ch. 7, 9, and 10, especially the section “Body Representation as Originating in Perception.” There I criticize arguments of Carey's colleague, Elizabeth Spelke, that no object representations are perceptual.)

No serious empirical account of perception takes its representations to depend for their content entirely on what happens at a given moment. Interrelations between perceptual systems, perceptual memory, and perceptual anticipation are common even in the simplest perceptual systems. Exercises of perceptual shape constancy or color constancy over time require coordination of current perception with perceptual memory and perceptual anticipation. Such coordinative capacities are present in insects and other relatively simple animals that may not have object perception, and plausibly lack “concepts.” Perception of motion and change are standard topics of vision research.

*Object* or *body* perception is constitutively dependent on coordination between the perceptual system and perceptual anticipations of persistence over time; commonly in motion and commonly behind barriers. Therefore, to be a perception as of a body, a representation must be associated with a tendency to perceptually anticipate certain types of continuity. A perceptual representation can present something as looking like a body in *current* experience, as long as the “look” is associated with perceptual anticipations of certain types of continuity.

If Carey and Quine assume that perceptual representation excludes connection to perceptual anticipation and perceptual memory in the individuation of perceptual representation, they have a mistaken view of perception. Apart from this assumption, and apart from the error of requiring that the perceiver must *have* quantification, or criteria, in order to perceive something as an object or body, the quoted arguments have no force against the view that perceptual representations include object (body) perceptions.

There is substantial evidence that perceptual body representations occur in the visual systems of many mammals and some birds. Anticipations of continuities that are relevant to perceiving entities as bodies are associated with very early vision. The anticipations are not matters of conception or prediction. (Peterson 2001; Wexler & Held 2005.) Steps have been taken toward localizing the physical basis for object determination in areas of the human brain specialized for vision. (Grill-Spector et al. 2001; Kourtzi et al. 2003; Nielsen et al. 2006.) The idea that body representation does not occur in visual (or tactile) perception simply does not accord with research in perceptual psychology. Carey gives no good reason to reject this research.

A deficient view of *perception* therefore underlies Carey's account of “conceptual” object representation. It is not clear which phenomena that she discusses are postperceptual, perceptual, or both. Most of her characterizations of object “concepts” apply equally to object perceptions (Carey 2009, pp. 67–68). The key to the distinction must lie in better characterization of “central inferential processes,” including richer distinctions between the ways postperceptual and perceptual object representations relate to other processes and representations. The deficiency in drawing the perceptual/post-perceptual distinction may hamper Carey's accounts of the other representations of attributes that she discusses: representations of quantity, agency, and cause. Unquestionably, human children and higher animals do have postperceptual representations of these three attributes, as well as the attribute of objecthood. A more accurate view of perceptual representation is needed to delineate origins of postperceptual representation.

## Infants' representations of causation

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Stephen A. Butterfill

Department of Philosophy, University of Warwick, Coventry, CV4 7AL, United Kingdom.

s.butterfill@warwick.ac.uk <http://www2.warwick.ac.uk/fac/soc/philosophy/people/faculty/butterfill/>

**Abstract:** It is consistent with the evidence in *The Origin of Concepts* to conjecture that infants' causal representations, like their numerical representations, are not continuous with adults', so that bootstrapping is needed in both cases.

Representations involving causation play a special role in *The Origin of Concepts*, grounding Carey's view that there are "central representational systems with innate conceptual content that is distinct from that of core cognition systems" (Carey 2009, p. 246). For infants' causal representations are held to be innate but not grounded in core cognition, unlike representations of objects, numbers, and agents, which do involve core cognition. After discussing the distinction between core cognition and central representational systems, I shall argue, contra Carey, that infants' causal cognition might depend on core cognition after all. This matters for two reasons. First, we are left without a clear case of innate representation outside core cognition. Second, it suggests that, as in the case of number (see Ch. 8), there may be a developmental discontinuity between infants' and adults' causal notions. If so, understanding "how the human capacity for causal representation arises" (Carey 2009, p. 216) will require explaining how humans bootstrap themselves across the discontinuity.

Let me first outline part of what motivates the distinction between core and central cognition. Officially, core cognition is characterised by six properties (pp. 67–68), but for my purposes it is useful to start from motivation for one aspect of the distinction. At what age do infants typically first know that solid barriers stop rolling balls? This is a hard question because, as Carey explains, there is compelling evidence for apparently inconsistent answers. Infants' looking behaviours reveal that infants have different expectations about the trajectories of objects depending on the presence and positions of solid barriers (Baillargeon et al. 1995; Carey 2009, pp. 76ff.; Spelke & Van de Walle 1993; Wang et al. 2003). Yet at 2½ years, their reaching behaviours systematically indicate a failure to understand interactions (Berthier et al. 2000; Carey 2009, pp. 111–15; Hood et al. 2003). Carey's view involves distinguishing "two kinds of knowledge" (p. 115) or "two types of conceptual representations" (p. 22). There are principles that are *known* (or, better, *cognised* [pp. 10–11]) in this sense: Abilities to individuate and track objects exploit the approximate truth of these principles. We can explain the sensitivity of infants to solid barriers on the hypothesis that they do know that solid barriers stop rolling balls in this sense of knowledge: Where the principle that solid barriers stop rolling balls is violated, infants cannot compute a continuous trajectory for an object, and their attention is drawn to it for the same sorts of reasons that cause them to attend to anomalous appearances and disappearances (p. 140). But in another sense, principles are *known* only if they can serve as reasons that explain and justify purposive actions, or only if they could in principle become elements in intuitive theories with explanatory potential (Ch. 10). We can explain the failure of 2-year-olds' reaching behaviours to deal with solid barriers on the hypothesis that they do not know that solid barriers stop rolling balls in this sense of knowledge.

If (as I think) Carey's interpretation of these apparently conflicting findings is right, we must be cautious when attributing knowledge, concepts, or representations. This applies to the case of causation. Some of the best evidence for causal

representations in infancy comes from extensions of Michotte's launching paradigm (Carey 2009, p. 218). What is the nature of these representations? Abilities to individuate and track objects involve sensitivity to cues to object identity such as continuity of movement and distinctness of surfaces. As Michotte noted, the causal representations he identified arise when there is a conflict between these cues (Michotte 1946/1963, p. 51). This and other evidence (see Butterfill 2009, pp. 420–21, for a review) suggests that we can characterise Michottian causal phenomena in roughly the same way that Carey characterises infants' knowledge that solid barriers stop rolling balls: it is a side-effect (although perhaps a developmentally significant one) of the computations involved in core cognition of objects.

I have not yet suggested anything inconsistent with Carey's position. I also accept her arguments that (1) infants' causal representations are not limited to mechanical interactions and that (2) the Michottian phenomena are not "the [only] source of the human capacity for causal representations" (Carey 2009, p. 243; Saxe & Carey 2006). However, I do object to Carey's argument for the further claims that (3) "not all of an infant's earliest causal representations are modular" (p. 240) and that (4) infants "make complex causal inferences" (p. 242). The argument for all four claims hinges on an impressive series of studies showing that infants can represent causal interactions involving changes of state as well as of location, that they are sensitive to colliding objects' size or weight, and that they expect animate and inanimate objects to occupy different causal roles. These findings support (1) and (2) but not (3) and (4). Why not? Carey is surely right that these findings cannot be explained by supposing that there is core cognition of (or a module for) causation. But this is compatible with the hypothesis that sensitivity to causal principles is a feature of several individual modules. That many possibly modular processes require sensitivity to causal principles does not imply that any such principles are central. For example, cognition of speech is sometimes thought to be modular (Lieberman & Mattingly 1991) and might even be characterised as a species of core cognition. Speech cognition involves sensitivity to causation in ways analogous to those discussed in *The Origin of Concepts*. For example, it requires categorising things by whether they are potential producers of speech, and identifying which bits of speech are coming from a single source. In fact, on one view, cognition of speech involves identifying the phonetic activities most likely to be causing observable sounds and movements (Lieberman & Mattingly 1985). By itself, this is neither evidence for the existence of non-modular causal representations, nor for causal representations that are not aspects of core cognition.

But why think that causal representations are initially all embedded in core cognition?

As in the case of number, the answer concerns signature limits. Children in their second and third years of life show limited abilities to reason explicitly about the causal powers of solid objects and of agents. The principle considered earlier, that solid barriers stop rolling balls, is as much about causation as it is about objects. If infants' causal representations were nonmodular and if infants could make complex causal inferences, how could they fail to appreciate that solid barriers stop rolling balls?

If it is possible that infants' and adults' causal representations are discontinuous in the same sense that their numerical representations are (Ch. 4 and 8), how might humans bootstrap themselves across the discontinuity? In the case of number, numeral lists and counting routines play a key role. Tool use may play an analogous role in bootstrapping causal representations. Basic forms of tool use may not require understanding how objects interact (Barrett et al. 2007; Lockman 2000), and may depend upon core cognition of contact-mechanics (Goldenberg & Hagmann 1998; Johnson-Frey 2004). Experience of tool use may in turn assist children in understanding notions of manipulation, a key causal notion (Menziez & Price 1993; Woodward 2003).



Perhaps, then, non-core capacities for causal representation are not innate, but originate with experiences of tool use.

## Concepts are not icons

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Christopher Gauker

Department of Philosophy, University of Cincinnati, Cincinnati, OH 45221-0374.

christopher.gauker@uc.edu

<http://www.artsci.uc.edu/philosophy/gauker/>

**Abstract:** Carey speculates that the representations of core cognition are entirely iconic. However, this idea is undercut by her contention that core cognition includes concepts such as *object* and *agency*, which are employed in thought as predicates. If Carey had taken on board her claim that core cognition is iconic, very different hypotheses might have come into view.

In her book, Carey (2009) says that the elements of core cognition include some very basic concepts, such as the concept *object* (p. 41) and the concept *agent* (p. 186). She also says that the representations of core cognition have an “iconic” format (pp. 68, 458). This appears to be a contradiction, because conceptual representations, even as Carey understands them, cannot be iconic. I vote for a resolution in favor of iconic format.

Carey says that concepts are “units of thought, the constituents of beliefs and theories” (p. 5) and that “words express concepts” (p. 247). Moreover, she frequently interprets infants as using their concepts as predicates, in thinking of something as an object or as an agent (pp. 41, 187, 268). Thus, concepts, for Carey, appear to be components of the sorts of thoughts we can express in sentences. If an infant possesses the concept *object*, then it can think a thought that we can express in words thus: “That is an object.” That does not mean that only creatures that have language possess concepts, or that all concepts are word-like symbols, which Carey would deny (p. 68).

Iconic representations are mental images. Iconic representations do not represent only sensory qualities such as color and shape, as Carey well knows (p. 135). Mental images can represent a sequence of events among objects. Our understanding of objects may consist largely in our being disposed to imagine that events will unfold in certain ways rather than others (p. 460). If I imagine a ball disappearing behind a screen and the screen coming down to reveal that very ball, I represent the ball as persisting through time and as hidden behind an occluder; in doing so, I utilize knowledge of object permanence. In imagining a hand striking a box behind an occluder, I may exercise an understanding of causation.

Iconic representations are never concepts. Iconic representations of a red ball and a blue ball do not represent what the two balls have in common, as the concept *ball* does. A concept, such as *dog*, has an argument place. By substituting a representation of a particular object into that place, we can form a whole thought, such as that Fido is a dog. An iconic representation does not have an argument place. There could be a kind of thinking in pictures in which a mental image of a collie did the work of the concept *dog* in forming the sorts of thoughts we express in English with the word “dog.” But in that case, the mental image of a collie would cease to be an iconic representation of a particular collie. There are iconic representations of objects, but no iconic representation is the concept *object*.

Carey frequently infers that a representation is conceptual on the grounds that it cannot be defined in terms of sensory and spatiotemporal qualities and has an “inferential role” (pp. 97, 115, 171, 449). Infants’ representations of objects are not merely representations of statistical relations between sensory qualities (p. 34), and they are intermodal (p. 39). In saying that

a representation has an inferential role, she does not mean that it is governed by rules of inference defined over strings of symbols (p. 104). She means that it guides expectations (p. 61) and is integrated into several domains of cognition (p. 95). The concept *object* has an inferential role inasmuch as “objects are represented as solid entities in spatial and causal relations with each other” (p. 103).

But these considerations do not persuasively argue that the representations of core cognition can also be used as predicates. In expecting that there will be two objects behind the screen, not one, the child may be simply imagining two objects. There is no need to suppose that the child judges of each *that* it is an object and distinct from the other. An imagistic representation of the arrangement behind the screen may be a consequence of the infant’s understanding of the way objects behave; it is not merely a synthesis of sensory qualities. It can integrate contributions from several sensory modalities. An imagistic representation can demonstrate an understanding of causal and numerical relations. That understanding can take the form of an imagistic understanding of what one can expect to perceive in the course of observing real objects. An infant can represent an object as solid without judging that it is solid by imagining that it will behave as solid objects in fact behave.

Carey introduces yet a third kind of representation, *object files* (p. 70). She thinks it’s useful to imagine that the infant keeps a file on each object that it perceives, in which it stores information about that object’s past. But an object file is not iconic, because an iconic representation is not a thing in which information is collected. And an object file is not the concept *object*, which we might put to use in representing every object. Yet in Carey’s thinking, the notion of object file seems to blur the distinction between concept and iconic representation (p. 459).

Carey frequently attributes to infants a kind of conceptual thought that is not in any way reducible to iconic representation. She thinks experiments show that infants classify objects as agents (p. 186). In comparing explanations of looking-time studies of infants’ reactions to causal scenarios, she pits an explanation in terms of the general concept *cause* against an explanation in terms of generalizations “stated over perceptual features” (p. 241). In considering how infants might solve individuation problems, the only options she considers are that they conceive of objects as falling under kinds and that they conceive of them as having properties (pp. 268–70). None of the options on offer is an explanation in terms of iconic representations.

If Carey had taken on board her own claim that core cognition employs iconic representations, then very different hypotheses might have come into view. Quite generally, an infant’s understanding of the world might take the form of its being disposed to imagine certain sequences of events in preference to others. It is not obvious that skills in imagination can only be explained as deriving from prior conceptualizations.

## The case for continuity

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Rochel Gelman

Department of Psychology and Center for Cognitive Science, Rutgers University, Piscataway, NJ 08854-8020.

rgelman@rucss.rutgers.edu

[rucss.rutgers.edu/faculty/GnG/gelman.html](http://rucss.rutgers.edu/faculty/GnG/gelman.html)

**Abstract:** This article defends a continuity position. Infants can abstract numerosity and young preschool children do respond appropriately to tasks that tap their ability to use a count and cardinal value and/or arithmetic principles. Active use of a nonverbal domain of arithmetic serves to enable the child to find relevant data to build knowledge about the language and use rules of numerosity and quantity.

Carey's book is an outstanding contribution to cognitive development (Carey 2009). It reviews and updates findings that infants and young children have abstract or "core" representations of objects, agents, number, and causes. The number chapters feature the argument for discontinuity between infant and later cognitive development. They include evidence that infants use two separate number abstraction systems: an object-file, parallel system for the small numbers of 1 to 3 or 4; and a ratio-dependent quantity mechanism for larger numbers. This contrasts with adults, who use a ratio-dependent mechanism for all values (Cordes et al. 2001).

Further, Carey argues that verbal counting is first memorized without understanding and that the meaning of counting and cardinality is embedded in the learning of the quantifier system. She cites Wynn's (1990; 1992) "Give X" and LeCorre and Carey's (2007) tasks that children aged 2 years, 8 months to 3 years, 2 months typically fail as well as analyses on quantifiers, including *some* and *many*.

An alternative account runs as follows. Infants possess a core domain for arithmetic reasoning about discrete and continuous quantity, necessarily including both mechanisms for establishing reference and mechanisms for arithmetic reasoning. The nonverbal domain outlines those verbal data and uses rules that are relevant to its growth. The development of adult numerical competence is a continuous and sustained learning involving the mapping of the cultural system for talking about quantity into the inherited nonverbal system for reasoning about quantity. Counting principles constitute one way to establish reference for discrete quantity because they are consistent with and subservient to the operations of addition and ordering, that is, they are consistent with basic elements of arithmetic reasoning. In this view, the Carey account focuses too much on reference and almost ignores the requirement that symbols also enter into arithmetic reasoning. The well-established ability of infants and toddlers to recognize the ordering of sequentially presented numerosities, including small ones, requires a counting-like mechanism to establish reference. If the symbols that refer to numerosities do not enter into at least some of the operations that define arithmetic (order, addition, subtraction), then they are not numerical symbols. However, there is evidence that beginning speakers recognize that counting yields estimates of cardinality about which they reason arithmetically.

1. Infants can represent numerosity in the small number range. Cordes and Brannon (2009) show that, if anything, numerosity is more salient than various continuous properties in the 1–4 number range. Converging evidence is found in VanMarle and Wynn (in press).

2. Cordes and Brannon (2009) also show that 7-month-old infants discriminate between 4:1 changes when the values cross from small (2) to larger (8) sets. These authors conclude that infants can use both number and object files in the small *N* range, a challenge to the view that there is a discontinuity between the small number and larger number range for infants.

3. Two-and-a-half-year-olds distinguish between the meaning of "a" and "one" when tested with the "What's on the card WOC?" task (Gelman 1993). When they reply to the WOC question with one item, they often say "a \_\_\_". When told "that's a one-x card", the vast majority of 2½-year-olds both counted and provided the cardinal value on set sizes 2 and 3 and young 3-year-olds ( $\leq 3$  years, 2 months) provide both the relevant cardinal and counting solution for small sets as well as some larger ones. Syrett et al. (in press) report comparable or better success rates for children in the same age ranges. The appearance of counting when cardinality is in question is good evidence that these very young children, who can be inconsistent counters, nonetheless understand that counting renders a cardinal value.

4. Arithmetic abilities appear alongside early counting. Two-and-a-half-year-old children transferred an ordering relation between 1 versus 2 to 3 versus 4 (Bullock & Gelman 1977).

When these children encountered the unexpected change in numerosities, they started to use count words in a systematic way. This too reveals an understanding of the function of counting well before they can do the give-*N* task. Carey's claim that "originally the counting routine and the numeral list have no numerical meaning" (p. 311) is simply false.

5. Gelman's magic show was run in a number of different conditions and with 3-year-old children. Children this age distinguished between operations that change cardinal values (numerosities) and those that do not, across a number of studies. Moreover, when the cardinality of the winner comes into question, they very often try to count the sets, which are in the range of 2–4, and occasionally 5.

6. Further evidence that 3-year-olds understanding of cardinality comes from the Zur and Gelman (2006) arithmetic-counting task. Children started a round of successive trials with a given number of objects, perhaps doughnuts, to put in their bakery shop. They then sold and acquired 1–3 doughnuts. Their task was to first predict – without looking – how many they would have, and then to check. Their predictions were in the right direction, if not precise. They counted to check their prediction and get ready for the next round. They never mixed the prediction-estimation phase and the checking phase. Counts were extremely accurate and there was no tendency to make the count equal the prediction. Totals could go as high as 5.

7. The idea that understanding of the exact meaning of cardinal terms is rooted in the semantics of quantifiers is challenged in Hurewitz et al. (2006). They found that children in the relevant age range were better able to respond to exact number requests (2 vs. 4) than to "some" and "all."

8. An expanded examination of the Chiles database with experiments with the partitive frame (e.g., *zav of Y*) and modification by the adverb *very* (e.g., *very zav*) reveal that the Bloom and Wynn analysis of semantics is neither necessary nor sufficient to accomplish the learnability challenge (Syrett et al., in press).

The preverbal arithmetic structure can direct attention to and assimilate structurally relevant verbal data and their environments.

## Language and analogy in conceptual change

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Dedre Gentner and Nina Simms

Department of Psychology, Northwestern University, Evanston, IL 60208.

gentner@northwestern.edu

http://groups.psych.northwestern.edu/gentner/

ninasimms@northwestern.edu

**Abstract:** Carey proposes that the acquisition of the natural numbers relies on the interaction between language and analogical processes: specifically, on an analogical mapping from ordinal linguistic structure to ordinal conceptual structure. We suggest that this analogy in fact requires several steps. Further, we propose that additional analogical processes enter into the acquisition of number.

How humans come to possess such striking cognitive abilities and rich conceptual repertoires is perhaps *the* question of cognitive science. Susan Carey has explored this question in part by identifying specific areas – such as number – in which humans demonstrate unique and impressive ability, and exploring their development in great depth (Carey 2009). In her treatment of number, Carey argues that children gain an understanding of the natural numbers through a process of mapping the ordinal structure of the number list to quantity. We agree with this proposal, but we suggest (1) that analogy interacts with language in several additional and distinct ways to support the acquisition of number; and (2) that arriving at the analogy from ordered numerals to ordered quantities probably requires more than a single leap (Gentner 2010).

Carey and others have provided substantial evidence suggesting that two core capacities enter into numerical cognition: the analog magnitude system, which allows approximate judgments of quantity; and a system for keeping track of small numbers of items (up to three or four). Neither is sufficient for representing large exact numbers—nor indeed for representing the natural number sequence at all. As Carey reviews, two lines of evidence suggest that language is key in this achievement: (1) cross-linguistic studies of Amazonian peoples whose languages – Pirahã and Mundurukú – lack a full counting system, and who show marked deficiencies in dealing with exact numerosities greater than 3 (Everett 2005; Frank et al. 2008; Gordon 2004; Pica et al. 2004); and (2) developmental evidence that children at first learn the linguistic count sequence as a kind of social routine (Fuson 1988), and that learning this sequence is instrumental in their acquisition of the conceptual structure of the natural numbers.

We believe language is instrumental in achieving conceptual mastery of all sorts; indeed, we have proposed that language, combined with powerful analogical processes, is crucial to humans' remarkable abilities (Gentner & Christie 2008). Analogical processes conspire with language in several ways in conceptual development, including the acquisition of number. First, common labels invite comparison and subsequent abstraction (Gentner & Medina 1998). Hearing the count label "3" applied to three pears and three apples prompts comparison across the sets and abstraction of their common set size (Mix et al. 2005). Second, the repeated use of the same numerals for the same quantities helps stabilize numerical representation. English speakers consistently assign "3" to the same quantity, whether counting up from 1 or down from 10. This uniform usage might be taken for granted, except that Pirahã speakers, astoundingly, assign their numeral terms to different quantities when naming increasing vs. decreasing set sizes. A third way in which analogy interacts with language to support cognitive development is that linguistic structure invites corresponding conceptual structure (Gentner 2003). For example, learning and using the spatial ordinal series "top, middle, bottom" invites preschoolers to represent space in an ordered vertical pattern (Loewenstein & Gentner 2005).

This brings us to Carey's (2004; 2009) bold proposal that learning the natural numbers relies on an analogical mapping from ordinal linguistic structure to ordinal conceptual structure. One symptom of this analogical insight is a sudden change in the pace of learning. As Carey reviews, children first learn the count sequence as a social routine. Despite their fluency with this linguistic sequence, children may show only minimal insight into the binding to numerical quantity. A typical 2-year-old can count from "1" to "10," but cannot produce a set of five items on request. The binding of small numerals to quantities is slow, piecemeal, and context-specific (Mix et al. 2005; Wynn 1990). But once a child binds "3" or "4" to the appropriate quantity, the pattern changes; the child rapidly binds the succeeding numbers to their cardinalities. Further, the child shows understanding of the successor principle, that every natural number has a successor whose cardinality is greater by one.

How does the analogy between numeral order and quantity order emerge? The correspondence between *counting one further* in the linguistic sequence and *increasing by one* in set size is highly abstract. We suggest that children arrive at this insight in a stepwise fashion, roughly as follows (for simplicity, we consider the case where the insight occurs after "3" is bound to 3):

When "1", "2" and "3" are bound to their respective quantities, the child has two instances in which *further-by-one* in count goes with *greater-by-one* in set size:  $1 \rightarrow 2$  and  $2 \rightarrow 3$ . Should the alert child wonder whether this parallel continues to hold, s/he will find immediate confirmation: counting from "3" to "4" indeed goes with a set size increase  $3 \rightarrow 4$ . At this point the

child has a very productive rule of thumb:

IMPLIES(FURTHER-BY-ONE(count list),  
GREATER-BY-ONE (set size))

Over repeated use of this highly productive rule, the child re-represents the two parallel relations as the same (more abstract) relation – a successor relation – applying to different dimensions, such as:

GREATER-THAN[(count( $n$ ), count( $n + 1$ ))  
 $\leftrightarrow$  GREATER-THAN[(setsize( $n$ ), setsize( $n + 1$ ))]

At this point the analogy has revealed a powerful abstraction: the common relational structure required for the successor function.

Bertrand Russell (1920) memorably stated: "It must have required many ages to discover that a brace of pheasants and a couple of days were both instances of the number 2: the degree of abstraction involved is far from easy." Though English speakers may see the natural numbers as obvious, the evidence from the Pirahã bears out Russell's speculation: a conception of "two-ness" is not inevitable in human cognition. Carey's proposal provides a route by which this insight can be acquired.

## A unified account of abstract structure and conceptual change: Probabilistic models and early learning mechanisms

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Alison Gopnik

Department of Psychology, University of California at Berkeley, Berkeley, CA 94720.

[gopnik@berkeley.edu](mailto:gopnik@berkeley.edu) [www.alisongopnik.com](http://www.alisongopnik.com)

**Abstract:** We need not propose, as Carey does, a radical discontinuity between core cognition, which is responsible for abstract structure, and language and "Quinian bootstrapping," which are responsible for learning and conceptual change. From a probabilistic models view, conceptual structure and learning reflect the same principles, and they are both in place from the beginning.

There is a deep theoretical tension at the heart of cognitive science. Human beings have abstract, hierarchical, structured, and accurate representations of the world: representations that allow them to make wide-ranging and correct predictions. They also learn those representations. They derive them from concrete, particular, and probabilistic combinations of experiences. But how can we learn abstract structure from the flutter and buzz at our retinas and eardrums? Nativists, from Plato to "core cognition" theorists, argue that it only *seems* that we learn; in fact, the abstract structure is innate. Empiricists, from Aristotle to connectionists, argue that it only *seems* that we have abstract structure; in fact, we just accumulate specific sensory associations. When we see both abstract structure and learning – notably in scientific theory change – traditional nativists and empiricists both reply that such conceptual change requires elaborate social institutions and explicit external representations.

Carey has made major contributions to the enormous empirical progress of cognitive development (Carey 2009). But those very empirical discoveries have actually made the conceptual problem worse. Piaget could believe that children started out with specific sensorimotor schemes and then transformed those schemes into the adult's abstract representations. But Carey's own studies, along with those of others, have shown that this is not a feasible option. On the one hand, contra the empiricists, even infants have abstract structured knowledge. On the other hand, contra the nativists, conceptual theory change based on



experience takes place even in childhood, without the infrastructure of adult science.

This book tries to resolve that conceptual tension by a sort of division of labor between nativism and empiricism. Cognition in infancy and early childhood reflects “core cognition” – the nativist option. Conceptual change is a later development that operates on the representations of core cognition but requires language, and the somewhat mysterious process of “Quinian bootstrapping.” However, new empirical and computational work, much of it done only in the past few years, suggests that there are more coherent ways of solving this dilemma. We need not propose a radical discontinuity between the processes that are responsible for abstract structure and those that are responsible for learning and conceptual change. They are, in fact, both part of the same system, and they are in place from the beginning.

Empirically, we’ve discovered that even infants have powerful learning capacities (Woodward & Needham 2009). We can give children particular types of evidence and observe the types of structure that they induce. These studies have already shown that infants can detect complex statistical patterns. But, more recently, it has been discovered that infants can actually use those statistics to infer more abstract non-obvious structure. For example, infants can use a statistically nonrandom pattern to infer someone else’s desires (Kushnir et al. 2010), and can use statistical regularities to infer meanings (Graf Estes et al. 2007; Lany & Saffran 2010). In other experiments, giving infants relevant experience produces novel inferences both in intuitive psychology and in intuitive physics (Meltzoff & Brooks 2008; Somerville et al. 2005; Wang & Baillargeon 2008).

By the time children are 4 years of age there is consistent evidence both for conceptual change and for learning mechanisms that produce such change. Pressing children to explain anomalous behavior can induce a representational understanding of the mind (Wellman & Liu 2007), and giving them a goldfish to care for can provoke conceptual changes in intuitive biology (Inagaki & Hatano 2004). Most significantly, preschoolers can use both statistical patterns and active experimentation to uncover complex and abstract causal structure, inducing unobserved causal forces and high-level causal generalizations (Gopnik et al. 2004; Lucas et al. 2010; Schulz & Bonawitz 2007; Schulz et al. 2007; 2008). Empirically, even infants and very young children seem to use statistical inference, explanation, and experimentation to infer abstract structure in a way that goes well beyond association and could support conceptual change.

We can still ask how and even whether this sort of learning is possible computationally. Fortunately, new work in the “probabilistic models” framework, both in cognitive development and in the philosophy of science and machine learning, provides a promising answer (Gopnik & Schulz 2007; Gopnik et al. 2004; Griffiths et al. 2010; Xu & Tenenbaum 2007). On this view, from the very beginning, cognition involves the formulation and testing of abstract hypotheses about the world, and, from the very beginning, it is possible to revise those hypotheses in a rational way based on evidence.

The new idea is to formally integrate structured hypotheses, such as grammars, hierarchies, or causal networks, with probabilistic learning techniques, such as Bayesian inference. The view is that children implicitly consider many hypotheses and gradually update and revise the probability of those hypotheses in the light of new evidence. Very recently, researchers have begun to show how to use these methods to move from one abstract high-level framework theory to another: the sort of conceptual change that Carey first identified (Goodman et al. 2011; Griffiths & Tenenbaum 2007). Empirically, we can induce such change, producing, for example, a new trait theory of actions (Seiver et al. 2010). Of course, there is still a great deal of work to be done. In particular, we need more realistic accounts of how children

search through large hypothesis spaces to converge on the most likely options.

The new empirical work and computational ideas suggest a solution to Carey’s dilemma – one that does not require either core cognition as a vehicle for abstract structure, or language and analogy as agents of conceptual change. It is also quite possible, of course, that the balance of initial structure, inferential mechanisms, and explicit representational resources might differ in different domains. Mathematical knowledge, is, after all, very different from other types of knowledge, ontologically as well as epistemologically, and might well require different resources than spatial, causal, or psychological knowledge.

In general, however, there is real hope that the empirical work and theoretical ideas that Carey has contributed can be realized in an even deeper way in the new computational theories, and that the ancient tension she has elucidated so well can finally be resolved.

## Can multiple bootstrapping provide means of very early conceptual development?

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Maciej Haman<sup>a</sup> and Mikołaj Hernik<sup>b</sup>

<sup>a</sup>Faculty of Psychology, University of Warsaw, Stawki 5/7, 00-183 Warsaw, Poland; <sup>b</sup>Developmental Neuroscience Unit, Anna Freud Centre, London NW3 5SD, United Kingdom.

maciej.haman@psych.uw.edu.pl

m.hernik@ucl.ac.uk <http://www.annafreud.org/infantlab/mhernik/>

**Abstract:** Carey focuses her theory on initial knowledge and Quinian bootstrapping. We reflect on developmental mechanisms, which can operate in between. Whereas most of the research aims at delimitating early cognitive mechanisms, we point at the need for studying their integration and mutual bootstrapping. We illustrate this call by referring to a current debate on infants’ use of featural representations.

Carey presents a theory of the ontogenesis of concepts marked by the presence of innate core knowledge and the representation of causality on one side, and by Quinian linguistic bootstrapping on the other (Carey 2009). We endorse most of this view. We find the evidence for the core knowledge of *agents* and *objects* convincing, and we agree that concepts such as *electron* are learned by acquiring linguistic placeholder structures first. But what happens in-between these two developmental landmarks? Is there any genuine conceptual development, including forming new kind-sortals with theory-constrained representation of features, apart from linguistic bootstrapping? How is the integrative role of causal representations (Carey 2009, Ch. 6) being accomplished?

Something of a paradox emerges when one seeks evidence of such processes in infants less than 12 months of age. A massive body of research from the last 25 years documents impressive innate competencies and capacity for rapid conceptual development over first months and years of life (Carey 2009, Ch. 3–6). However, another body of research shows that early representations are in many aspects impoverished, and that their developmental trajectories sometime contain surprising gaps. One of the most striking is perhaps a decalage in using featural, kind-related information for object identification and object individuation (Carey 2009, Ch. 3; Xu 2007b). On the one hand, even 6-month-olds appreciate that when there are two differently colored objects in the launching event, these are two separate objects, and infants show surprise when one takes the other’s causal role (Carey 2009, Ch. 6; Leslie 1982). But on the other hand, infants less than 12 months of age are not concerned either with shape, color, or kind, when determining number of occluded objects (Carey 2009, Ch. 3; Xu & Carey 1996).

It seems that infants use two different, separate types of object representations, and that in their first year they do not form sortal categories representing permanent object-properties, that is, kind concepts. There is evidence for sortals *object* and *agent* in early infancy (Surian & Caldi 2010), but these could be the only sortals available during the very first months.

However, some recent results suggest more permanent, feature-based representations in the first year of life, and relatively complex representations of this kind only a few months later. A series of studies by Wilcox and others (McCurry et al. 2008; Wilcox et al. 2006) demonstrated use of featural information for determining number of objects in the event even at 4.5 months. Featural representations made by 5- and 3-month-olds support success in goal-attribution tasks (Luo 2010; Schlottmann & Roy 2009). Moreover, infants between 3.5 and 10 months studied by Hamlin et al. (2007; 2010) map permanent (at least in the experimental session's time-scale) dispositional states on feature-based object representations derived from one event (different colored shapes helping or hindering one another), and use this representation either to apprehend shapes' behavior or to select an object to reach for during a different test-event. Nine-month-olds use property-to-function mapping derived from one set of objects for individuation within another set (Wilcox et al. 2009). Early in the second year, children represent occluded colors to predict others' behavior (Luo & Beck 2010) and, as our own work in progress suggests, represent feature sequences of an object undergoing self-induced transformation (Hernik & Haman 2010). However, as most of these studies either do not test directly for numerical identity processing, or use simplified procedures, or objects-to-be-represented are involved in only a single event, it can still be argued that "infants represent objects relative to the goals of agents" (Carey 2009, p. 450) and feature-based kind-sortals remain a relatively late developmental achievement.

Other explanations are also possible, however (Xu 2007b). Wang and Baillargeon developed a research program aiming at demonstrating that at least in the physical domain, infants' reliance on event-representation leads to featural representations of objects and arguably to proto-representations of kinds (see Wang & Baillargeon 2009 for review). It can be presented as a three-way bootstrapping model where: (1) object-file representations, restricted by attention span, provide spatiotemporal cues for object individuation and placeholders for features assigned both bottom-up and top-down; (2) event category representations provide causal relations and guide attention to causally-relevant features; and (3) feature-based object representations by means of which temporary information provided by the aforementioned two systems is converted into permanent mental structures. Context-independence of these representations increases with development, and can be successfully trained (Wang 2011).

Although Wang and Baillargeon (2009) present their model in the context of early physical knowledge, we are tempted to conceive it is an example of complex bootstrapping potentially more common among mechanisms of conceptual development. The structures of causal knowledge involved in different categories of physical and social events, functional representations, or perhaps object transformations, as in the studies mentioned earlier, provide scaffolding for feature- and kind-representations. Different kinds of representations are used, and each of them provides placeholders, which can bootstrap elements of other systems of representation. This multiway bootstrapping may be further supported by at least three general-purpose learning mechanisms, such as weak linguistic influence (Carey 2009, Ch. 7; Xu 2007a) and learning by means of communication (Csibra & Gergely 2009; Futo et al. 2010), which may support both kind- and feature-based representations, as well as constrained Bayesian learning, which allows for quick detection of conditional interrelations between causes, objects, and their features. So far this is only a speculative hypothesis. Although

integration of mechanisms discussed here can in principle be explored in experimental designs, it rarely was. Most of the contemporary cognitive-developmental research aimed at delimitating and enumerating separate mechanisms. Whereas this strategy was justified and effective, it can be perhaps blamed for underestimating the developmental role of integrative processes, and in consequence underestimating infants' early representational potential. Now, the focus on cooperation of different systems in the first years of life is necessary (see Denison & Xu 2010 for a convergent argument focusing on Bayesian learning). We suppose that a more integrative research program could fill the gap in our understanding of how everyday concepts begin to be formed very early on, in the midst of capitalizing on two major developmental forces (initial knowledge and linguistic bootstrapping) described by Carey.

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### Presuming placeholders are relevant enables conceptual change

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Christophe Heintz

Department of Philosophy, Central European University, H-1051 Budapest, Hungary.

christophe.heintz@gmail.com <http://christophe.heintz.free.fr/>

**Abstract:** Placeholders enable conceptual change only if presumed to be relevant (e.g., lead to the formation of true beliefs) even though their meaning is not yet fully understood and their cognitive function not yet specified. Humans are predisposed to make such presumptions in a communicative context. Specifying the role of the presumption of relevance in conceptual change would provide a more comprehensive account of Quinian bootstrapping.

Quinian bootstrapping for conceptual change is a process that requires making use of a placeholder. Placeholders are public symbols that can induce significant changes in the mind. The external symbol leads to the acquisition of a new mental concept as it acquires its meaning. Before conceptual change is completed, however, placeholders are promissory notes as future thinking tools. They do not yet have inferential power and they are not yet fully functional cognitive tools with which to understand the world. Nonetheless, placeholders must be ascribed a role – even if an indefinite one – in cognition.

How do people undergoing conceptual change conceive of placeholders so that they can take the role they have in Quinian bootstrapping? What kinds of processes and dispositions are at work? I contend that placeholders, if they are to enable conceptual change, must be thought of as potentially useful cognitive tools and that this thought is initiated by the disposition to think that what is communicated is relevant. This disposition is triggered even in cases when what is communicated is not understood. In a communicative context (archetypically, when a communicator addresses an audience) the audience presumes that the placeholder has referential and/or inferential features.

For instance, children believe that "seven comes after six" even before they understand what "six" and "seven" mean and what the relation "coming after" really implies in this context. For this, children must hold representations whose content is that the meaning of "seven" (whatever it is) bears a specific relation to the meaning of "six" (whatever it is.) It is on the basis of such partially understood beliefs that bootstrapping occurs. The counting routine is likewise involved in the bootstrapping and is used by children before they can see that it actually

enables knowing precisely how many items there are in a given collection.

Quinian bootstrapping relies on the curious fact that humans have beliefs they do not understand, hold representations whose meaning they don't comprehend, and perform actions whose relation to goals is unclear to them. The psychological dispositions that cause these attitudes are therefore at work in Quinian bootstrapping and a psychological account of what these dispositions do and why they do it would complement Susan Carey's account of conceptual change (Carey 2009).

Dan Sperber (1975; 1997) has long argued that beliefs that are held but not fully understood are common among cultural beliefs. He has termed such beliefs "semi-propositional beliefs" because one has only partial knowledge of their truth-conditions. Sperber argues that such beliefs are held because they are embedded in meta-representations that assert or imply that they are true rather than just being held intuitively (i.e. without attentions to reasons for taking them to be true). The reasons such beliefs are held, he further argues, are to do with the communicative context in which they occur, which leads the audience to presume that what is conveyed is of relevance, even if they don't understand it. Typically, the authority of the communicator justifies accepting them even without fully understanding their content.

In parallel, György Gergely and Gergely Csibra (2006) have shown that, in a communicative context, infants imitate behavior even when they don't understand why the behavior would help achieve any goal better than easier alternatives (e.g., switching a lamp on with one's forehead rather than with one's hand). Such imitative behavior is triggered in a communicative context (e.g., with the demonstrator making eye contact, and addressing the infant directly), because the observers come to assume that the demonstrator is demonstrating an action for them to learn. The communicative context leads observers to presume that the actions demonstrated are an efficient means to achieve a worthwhile goal, even if they do not really understand why, how, or even what the goal is.

The presumption of the relevance of communicated representations and of the efficiency of demonstrated actions is a human disposition that enables Quinian bootstrapping. There is some evidence that supports the claim that this disposition is uniquely human (e.g., whereas children imitate inefficient actions of a demonstrator, these same actions are not imitated by nonhuman primates; Horner & Whiten 2005). This would confirm Carey's conjecture that conceptual change is human-specific even in cases in which the core cognitive abilities recruited for giving meaning to new concepts are shared with other species.

Let me conclude with two open questions:

1. Some partially understood representations lead to conceptual change, but many others remain forever the topic of interpretation and reinterpretation. There are paradigmatic cases of the former in science (e.g., the notion of "weight"), and paradigmatic case of the latter in religions (e.g. the notion of the "Trinity"). Why would representations with similar cognitive features eventually have such different cognitive roles? I hypothesize that the difference lies in the practices: in the former case, the initial presumption of relevance is held, but questioned. The individual eventually has to find a reliable way to achieve a positive cognitive effect – this is when the concept is understood. In the latter case the presumption of relevance is maintained via other means – typically a deference to the communicator (God or priest) and post-hoc reinterpretations (Sperber 1975; 2010).

2. I claimed that the presumption of relevance is triggered by the communicative context. It is clear how this can happen for children or adults listening to their parents or teacher. But what happens when scientists discover new ideas and concepts? In these cases, the placeholder does not seem to be involved in a communication event that would activate the presumption of relevance. Are we therefore bound to accept that a different process is at work? I suggest that, in fact, scientists are always in a

communicative context when they elaborate their theory: they talk, or mentally simulate talking, with their colleagues or with themselves. The presumption of relevance present in their attitude toward a partially understood idea that they take to be nonetheless worth investigating is triggered by a real or simulated communicative context – a context where the new idea is being argued for (Heintz & Mercier 2010).

## Can Carey answer Quine?

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Christopher S. Hill

Department of Philosophy, Brown University, Providence, RI 02912.

Christopher\_Hill@brown.edu

**Abstract:** In order to defend her claim that the concept *object* is biologically determined, Carey must answer Quine's *gavagai* argument, which purports to show that mastery of any concept with determinate reference presupposes a substantial repertoire of logical concepts. I maintain that the *gavagai* argument withstands the experimental data that Carey provides, but that it yields to an a priori argument.

One of the most important components of core cognition is the concept *object*, which is understood to apply to entities that are three-dimensional, bounded, and coherent, and that persist through time and have trajectories that are spatiotemporally continuous.

Carey maintains that core cognition is part of our biological heritage (Carey 2009). Accordingly, because the concept of an object belongs to core cognition, it is important that she be able to show that infants possess it, and that they acquire it by maturation rather than learning. As she recognizes, this brings her into opposition to W.V. Quine, who maintained (Quine 1960) that all concepts that refer to specific objects, or to specific types of object, are associated with explicitly represented principles of individuation, and are acquired either during or after the acquisition of a language. More specifically, Quine held that if one is to represent an entity that is governed by determinate principles of individuation, one must have the conceptual sophistication to be able to express those principles, and one must therefore have acquired a logical apparatus consisting of quantifiers, an identity predicate, numerals, and devices for forming plurals. Carey rejects these claims, at least insofar as they are concerned with concepts that belong to core cognition.

To appreciate the rationale for Quine's position, we need to consider his *gavagai* argument. Suppose that a field linguist has found that uses of the term *gavagai* are highly correlated with situations containing rabbits. Can the linguist conclude that the term *refers to* rabbits? According to Quine, the answer is "no." For all the linguist has thus far determined, it could be true that *gavagai* refers instead to momentary stages in the lives of rabbits, or to undetached parts of rabbits. In general, data concerning statistical correlations between uses of words and situations in the world leave questions about reference open, as do facts involving such phenomena as pointing gestures and the direction of gaze. In order to assign a unique reference to *gavagai*, Quine maintains, the linguist will have to formulate questions like "Is the *gavagai* that I'm pointing to now identical with the *gavagai* that I pointed to a moment ago?" and "Is there exactly one *gavagai* here or more than one?" The answers to such questions will make it possible for the linguist to settle questions that are otherwise empirically undecidable.

It is easy to reformulate Quine's argument so that it poses a direct challenge to Carey's view. Instead of asking about the reference of *gavagai*, we can ask whether the core concept *object* refers to three-dimensional entities that are bounded, coherent, persisting, and that move on continuous trajectories,



or it refers instead to stages of such entities, or to undetached parts thereof. It is clear that Carey must provide considerations that favor the first possibility in order to secure her claim about the reference of *object*. Can she do this?

Carey presents considerable evidence that infants possess perceptual input analyzers that are able to detect three dimensionality, boundedness, coherence, and so on, and she argues persuasively that infants' use of the core concept *object* is governed by the influence of such devices. Thus, for example, she reviews Baillargeon's experiment (Baillargeon et al. 1985) concerning the responses of 5-month-old infants to impossible scenarios in which planes rotate backwards into areas that have been seen to contain solid objects. The fact that Baillargeon's subjects registered surprise is naturally interpreted as indicating that they expected solid objects to *persist through time*. Doesn't this show that *object* stands for objects rather than temporal stages of objects?

Quine would answer "no," maintaining that there is an alternative interpretation of Baillargeon's experiment. Instead of supposing that Baillargeon's subjects expected that an object first seen at time *T* would continue to exist, we can suppose that they expected that an object-stage seen at *T* would be followed by a number of other stages, each linked seamlessly with its predecessor. They registered surprise because the initial stage seemed to be the final stage or a nearly final stage. Equally, Quine would offer reinterpretations of the experiments designed to establish that infants can register and make use of information about spatial boundaries. Instead of showing that infants can discriminate between objects that *have* separate boundaries, he would say, the experiments can be understood to show that infants can discriminate between *parts of objects* when they *fall within* separate boundaries.

As far as I can see, there is nothing in Carey's empirical data, considered in isolation from theoretical considerations, that precludes Quinean reinterpretations of this sort. I believe, however, that it is possible to defend her position with a more or less a priori argument. This argument has two components. (There is a related line of thought in Wright [1997].)

The first component is the observation that Quine is *committed to accepting* Carey's claim that infants represent whole, enduring objects. This is because his alternative hypotheses actually presuppose the truth of that claim. One of his alternative hypotheses is that infants represent temporal stages of *objects*. In order to represent an item as a stage of an *object*, a subject must do three things: represent an enduring object, represent the item, and represent the latter *as* a stage of the former. A similar point applies to Quine's other alternative hypothesis. In order to represent an item as an undetached part of an *object*, a subject must represent a whole object, represent the item, and represent the latter *as* a part of the former. In other words, because Quine's alternative hypotheses are concerned with *undetached* stages and *undetached* parts, they imply that it is necessary to represent enduring objects in order to represent stages, and that it is necessary to represent whole objects in order to represent parts.

This brings us to the second component of the answer. In view of the foregoing considerations, Quine must change his alternative hypotheses substantially. Thus, for example, instead of the hypothesis that infants represent stages *rather than* enduring objects, he would have to propose the more complex hypothesis that infants represent stages *in addition to* enduring objects, and that they represent the former in virtue of representing the latter. But it is clear that hypotheses of this sort are ruled out by considerations of simplicity. On any reasonable theory of representation, it takes time and energy to form a representation. Accordingly, it is appropriate to dismiss claims about multiple simultaneous representations unless they are warranted by positive evidence. As far as I know, there are no experiments in the literature that provide support for baroque Quinean hypotheses of the sort we are presently considering.

## Graceful degradation and conceptual development

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Frank Keil

Department of Psychology, Yale University, New Haven, CT 06520.

frank.keil@yale.edu

**Abstract:** In this book, Carey gives cognitive science a detailed account of the origins of concepts and an explanation of how origins stories are essential to understanding what concepts are and how we use them. At the same time, this book's details help highlight the challenge of explaining how conceptual change works with real-world concepts that often have heavily degraded internal content.

The history of psychology and philosophy is littered with the bloated carcasses of grand theories that tried to do far more than they were able in explaining the origins and growth of concepts. Many classic theorists proposed large-scale origins stories, but their accounts were invariably missing critical details, full of inconsistencies, or both. Carey's book is just the opposite (Carey 2009). It tackles head on some of the most vexing problems about the nature of concepts and their origins, and illustrates how the right origins stories are critical to understanding what concepts really are and how they are represented in adults. It provides specific mechanisms that go from core domains in infancy to more elaborated conceptual systems in children and adults. It is gratifying to see many precise and empirically testable claims, such as: initial concepts are represented iconically, Quinian bootstrapping enables children to circumvent Fodor's problem of testing new concepts that one cannot yet fully represent, and the concept of cause does not simply emerge out of perceptual primitives. Through such cases as the growth of numerical thought, Carey illustrates her theory in detail and makes sometimes startlingly precise claims.

These characteristics make this a landmark book that sets a new standard for the study of concepts and their development. One need not agree with all of Carey's proposals to appreciate the benefits of having such a detailed and verifiable account. There are, of course, many remaining questions. For example, how does analogy allow one to implement Quinian bootstrapping successfully as opposed to getting lost in a sea of irrelevant analogies? How does one unambiguously show that early representations are iconic in nature as opposed to being in other formats? To all such questions, Carey is usually refreshingly open to alternative answers and would simply want to know how they could be empirically tested against her views. There is, however, a broader question that poses a challenge for all approaches to concepts and their origins: namely, what happens when the information content of concepts is degraded?

Although traditionally used in Parallel Distributed Processing (PDP) models in which networks could suffer damage and still function approximately the same way (Rumelhart 1989), the idea of "graceful degradation" here raises the question of whether stories of origins and conceptual change remain elegant and coherent as concepts themselves become increasingly degraded. Carey is clearly aware of the problem of packing too much inside concepts proper and of the challenges of deciding on what information is associated with concepts and what information is part of their internal structure. Rejecting Fodor's (1998) minimalist program, she wants concepts to have some internal relational structures to act as vehicles for conceptual growth and change. The question is, how much internal structure is needed for Carey's account of the origins of concepts to go forward and how explicit must it be?

Consider three challenges. First, what people know about concepts such as "gold," "tiger," or "winter" can be startlingly degraded. Most accounts of concepts hold that prototypes or other bottom-up probabilistic tabulations of features are inadequate and also include beliefs with entailments. Yet the

details are elusive. What does the layperson know about tigers in particular, and not just animals, that has any content beyond the prototype? The answer may seem to lie in how we cognitively implement “sustaining mechanisms,” those ways of “locking” our concepts onto real-world entities (Laurence & Margolis 2002). Some sustaining mechanisms use theories to directly lock, some use deference to experts, and some use prototypes (Laurence & Margolis 2002). All three mechanisms usually work together, but it remains unclear just how they interact, what parts are in concept proper, and how they would figure in conceptual change. The theory component is often limited to large categories, such as living kinds; the prototypes can mislead; and deference cannot work without complex cognitive activities that specify relevant experts (Keil et al. 2008). Sustaining mechanisms remain troublingly mysterious.

Second, even when crisp explicit beliefs might be candidates for conceptual components, they often don't behave well. Adults routinely believe that winter is when the earth is farther away from the sun than in summer; yet they might also believe that it is summer in Australia when it is winter in Europe. They acknowledge this contradiction when it is explicitly pointed out to them; but they can go through their entire lives holding both beliefs and be unaware of the contradiction. If such beliefs are the basis for networks of entailments and other relations that enable Quinian bootstrapping, how can such a process work when local contradictions are prolific?

Third, there is the concepts-within-theories dilemma. Discussions of scientists learning the distinction between heat and temperature, or rejecting luminiferous aether, or embracing evolution, all assume theory-like systems that undergo change; but can we reduce such cases to laypeople and children? How degraded can a theory-like structure become and still work as a theory? If theories must be coherent and a basis for making ontological commitments, will disorganized sets of fragments suffice? If the coherent units of a child's intuitive biology are little more than two or three nodes and a few links between them (Inagaki & Hatano 2002), is that enough structure? In addition, do theories have to be couched in explicit beliefs or can they be implicit intuitions about causal patterns that lurk dimly outside awareness?

All of us, especially young children, reveal striking brilliance and cluelessness at the same time. We can track surprisingly abstract causal patterns in ways that clearly go beyond noting frequencies and correlations, yet we can also be profoundly ignorant of even the simplest mechanistic details (Lawson 2006; Keil 2010). From infancy onwards, we function much of the time at a level that senses causal regularities and seems to be inferential in nature, but which is nonetheless heavily degraded. Because of its specificity and rigor, Carey's book makes clear just how much of a challenge still remains in telling a full story of where concepts come from and how they change with development.

## The notion of incommensurability can be extended to the child's developing theories of mind as well

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Szabolcs Kiss

*Institute of Sociology of the Hungarian Academy of Sciences, 1014 Budapest, Uri. u. 49., Hungary.*

kiss.szabolcs@t-online.hu

**Abstract:** In this commentary I argue that the notion of incommensurability can be extended to the child's developing theories of mind. I use Carey's concept of Quinian bootstrapping and show that this learning process can account for the acquisition of the semantics of mental terms. I suggest a

distinction among three stages of acquisition and adopt the theory-theory of conceptual development.

*The Origin of Concepts* (Carey 2009) commits itself to the so-called theory-theory of conceptual development. At the same time, a central concern of the book is the incommensurability between children's different theories in the domain of naïve physics and number. In my view, the notion of incommensurability can be extended to the child's developing theories of mind as well. In my commentary, I will show that this is the case, while discussing the problem of meaning variance of mental terms in cognitive development (Kiss 2003). The question is: How does the child acquire the meaning of mental terms? I will address this question and argue that Quinian bootstrapping plays a central role in this learning process.

Within the mindreading literature, *folk functionalism* is the name of the commonsense theory in which the meanings of mental terms in adults are organised. According to folk functionalism, the input and output connections of a given mental state, as well as its connections to other mental states, are mentally represented. For example, the meaning of the term *pain* relates to the cause of the pain (e.g., touching a hot stove), the pain's connections to other mental states (the *desire* to get rid of the pain), and the pain's relationship to behaviour (pain produces wincing).

In the first stage of the ontogenetic acquisition of the semantics of mental terms the child already uses mental terms, but he or she is not fully aware of their meanings yet. At this stage the child uses mental terms referring to the behavioural components of the mental state only. For instance, the term *happiness* refers only to behavioural manifestations such as a smile. This phenomenon is called *semisuccessful reference* by Beckwith (1991). This is consistent with Wittgenstein's view (1953) according to which the attribution of mental states is always based on behavioural criteria. The phenomenon of semisuccessful reference of mental terms is also in line with Wittgenstein's well-known remark that we use words whose meanings become clear only later.

Clearly, in this case we can see the learning process of Quinian bootstrapping at work. One of the central components of this bootstrapping process is the existence of a placeholder structure. According to Carey, the meaning of a placeholder structure is provided by relations among external, explicit symbols. In our case, these external, explicit symbols are mental words and expressions represented in the child's long-term memory. Therefore, the child represents many mental words and lexical items whose full and complete meanings become available only at later stages of this famous bootstrapping process.

In the second stage of the change of meaning of mental terms, the child discovers the inner subjective component (feeling or *qualia*) of mental lexical items and realises that the reference of mental terms includes this component. In other words, the child recognises the phenomenological or experiential qualities of mental terms. In this stage, mental terms have gone through meaning variance in relation to the first stage, but the child does not yet possess the full representation of the meaning of mental terms found in the folk functionalist theory.

The third stage is the acquisition of this commonsense functionalist theory. It is the result of a long learning process during which the child comes to understand the relationship between mental states and their eliciting conditions and the interconnections of mental states to each other and to their behavioural consequences. This is the acquisition of a coherent theory by which the child understands specific causal processes such as the fact that perception leads to the fixation of beliefs, or that beliefs can bring about other beliefs by means of inference, and that beliefs and desires cause actions together.

Therefore, mental terms go through changes of meaning during semantic development. The successive naïve psychological theories of children determine the meanings of mental terms. This meaning variance of mental terms is similar to the meaning variance of scientific terms discussed by philosophers of science

(e.g., Feyerabend 1962). (In fact, I have borrowed the expression of *meaning variance* from this philosophical tradition.) As identical terms gain different meanings in different theories, the changes of meaning lead to the problem of *incommensurability* among various theories. In this way, we can extend the notion of incommensurability to the child's developing theories of mind as well.

According to Carey, radical conceptual change is often accompanied by local incommensurability in the domain of naïve physics, biology, and number. She shows that one important form of conceptual change is conceptual differentiation (e.g., weight and density in the domain of naïve physics). I argue that this kind of conceptual differentiation characterizes naïve psychology as well. A case in point is the notion of *prelief* (pretend + belief) developed by Perner et al. (1994) in which the action derived from false belief is not differentiated from the action derived from pretend play. This notion of prelief is part of the earlier conceptual system of children that emerges at the age of 3, and is absent from the adult folk functionalist theory. (Although Carey briefly touches upon the notion of a want/prelief psychology developed by Perner (Carey 2009, p. 204) she does not discuss this notion in detail or from the point of view of a possible case of local incommensurability between the child's developing theories of mind.)

## Concept revision is sensitive to changes in category structure, causal history

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Joanna Korman

Department of Cognitive, Linguistic, and Psychological Sciences, Brown University, Providence, RI 02912.

Joanna\_Korman@brown.edu

**Abstract:** Carey argues that the aspects of categorization that are diagnostic of deep conceptual structure and, by extension, narrow conceptual content, must be distinguished from those aspects that are incidental to categorization tasks. For natural kind concepts, discriminating between these two types of processes is complicated by the role of explanatory stance and the causal history of features in determining category structure.

Determining how explicit concepts such as *matter*, *eight*, and *dog* obtain their narrow content, and discriminating content-determining from merely associated conceptual features, is a crucial challenge that remains to be fully articulated in Carey's final chapter (Carey 2009). It is important to maintain some division between those mechanisms in the categorization literature that we consider diagnostic of category structure and those that reflect mere belief revision.

I will suggest that Carey's account of content-determination for natural kinds in particular should be amended to allow for revisions in narrow conceptual content that are not exclusively dictated by changes as wide-ranging as scientific progress. Although the suggestion that narrow content is only revised in response to scientific change ensures that Carey's criterion for concepts having *shared* content across individuals is met, this criterion comes at a cost. It does not allow for *bona fide* conceptual change as the result of exposure to regularities in smaller-scale causal processes. Such an amendment is therefore critical if *The Origin of Concepts* (henceforth *TOOC*) is to allow for natural kind concepts to participate in smaller-scale conceptual change throughout the life course.

Within domain theories, Carey argues, some features are more causally fundamental than others. That is, the category features that are most central within the domain of natural kinds may be those that determine the narrow content of these concepts. I will argue that domain theories, or "intuitive theories," of

natural kind concepts that guide this causal prioritizing are importantly malleable and sensitive to changes in the causal and explanatory role played by instances of these concepts.

Whereas Carey argues that the prototype effects found in the categorization literature do not reflect anything about the deep structure of concepts, she suggests that features that are most causally central in a conceptual structure (Ahn et al. 2000) provide a sound empirical starting point for the determination of narrow content. Carey further suggests that the causal status (Ahn et al. 2000) of a property within its intuitive theory is one model of conceptual structure upon which we might base narrow content. On this account, categories possess a causal structure. Features that are more often causes than effects within this structure have priority in categorization decisions.

One strength of the causal status effect has been its ability to account for previously documented content-based differences in feature centrality between natural kinds and artifacts (Ahn 1998; Keil 1989). However, the ability of this phenomenon to assimilate structurally diverse features (e.g., functional features for artifacts and molecular features for natural kinds) may be due in part to the structural guidance provided by the task itself, which often manipulates knowledge about properties by directly stating their statuses as causes and effects. Notably, when this strong manipulation is no longer present, and features are introduced within a functional, rather than a mechanistic, causal-explanatory structure, the causal status effect disappears (Lombrozo 2009). Along similar lines, Lombrozo and Carey (2006) found that preference for teleological explanations in accounting for the functions of natural kind and artifact objects depends on the causal history of the object's function and not on the domain of the object per se. As Lombrozo and Carey argue, with regard to the applicability of teleological explanation, object domains and causal processes do not exhibit a one-to-one mapping. Across natural kinds and artifacts, the causal history of an object influences the way it is explained; conversely, explanatory stance influences a feature's centrality in a category. Given the malleability of their category structure, a similar claim might be made with respect to the structure of natural kind concepts.

Consider how this one-to-multiple mapping operates in Lombrozo's (2009) stimulus set. Participants were presented with a fictional flower (a "holing") with a certain substance in its stem ("brom compounds") that caused a second feature ("bending over"). This second feature was also conceived of as a functional feature ("by bending over, the holing's pollen can brush against the fur of field mice, and spread to neighboring areas"). Participants were asked to explain "why holings typically bend over," and their explanations were classified as appealing to either a mechanistic or a functional structure. They were then asked to categorize new flowers, some of which lacked the mechanistic causal precursor feature ("brom compounds"), and others of which lacked the functional feature ("spreading pollen"). Overall, participants who offered a functional explanation for bending over regarded the functional feature as more causally central in categorization decisions, and those who offered a mechanistic explanation (appealing to the prior cause) regarded the mechanistic feature ("brom compounds") as more central. A direct manipulation of explanatory stance found similar results. These results indicate that the features of natural kind concepts do not exhibit a clear "default" structure. Participants both entered the experiment with slightly different prior intuitions about the explanatory mode most diagnostic of category structure, and responded to a category structure manipulation.

What do these findings tell us about conceptual change? If conceptual role is central to determining the narrow content of natural kind concepts, and this content only varies in the rare cases in which our scientific knowledge grows, then at best, there is much empirical work left to be done to show how the narrow content of a concept remains consistent across differences in causal history and explanatory stance. Otherwise, these differences suggest that these stances are themselves



representational resources highly sensitive to smaller scale causal (and functional) variations in the world.

Of course, learning a fabricated new species of flower is a task limited in scope; the concept token referred to will not likely transform all future instances of “natural kind” encountered. But if some measure of conceptual change remains possible throughout the life course, then concepts themselves, and not just their associated categories, must respond to smaller scale regularities in the causal structure of specific instances of those concepts that are encountered.

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## Conceptual discontinuity involves recycling old processes in new domains

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David Landy,<sup>a</sup> Colin Allen,<sup>b</sup> and Michael L. Anderson<sup>c</sup>

<sup>a</sup>Department of Psychology, University of Richmond, Richmond, VA 23173;

<sup>b</sup>Cognitive Science Program and Department of History & Philosophy of Science, Indiana University, Bloomington IN 47405; <sup>c</sup>Department of Psychology, Franklin & Marshall College, Lancaster, PA 17603.

dhlandy@gmail.com      www.richmond.edu/~dlandy

colallen@indiana.edu

michael.anderson@fandm.edu      http://www.agcognition.org

**Abstract:** We dispute Carey’s assumption that distinct core cognitive processes employ domain-specific input analyzers to construct proprietary representations. We give reasons to believe that conceptual systems co-opt core components for new domains. Domain boundaries, as well as boundaries between perceptual-motor and conceptual cognitive resources may be useful abstractions, but do not appear to reflect constraints respected by brains and cognitive systems.

In Carey’s proposal, core cognitive processes have a strictly circumscribed conceptual domain: “A dedicated input analyzer computes representations of one kind of entity in the world and only that kind” (Carey 2009, p. 451). This leads Carey to assume that conceptual change transforms representations within a specified domain. So, for example, Carey proposes that the natural numbers are constructed from number-based core processes: either parallel representation of individuals belonging to small sets, or analog magnitude representations. On this view, a CS2 “transcends” a CS1 – two conceptual systems cover the same domain, but one covers that domain more completely and more richly than the other.

Substantial empirical evidence suggests that instead, concepts in learned theories are often built out of processes and representational vehicles taken from widely different domains. This is especially so when the topic is abstract, as in the case of mathematics. For example, Longo and Lourenco (2007) present evidence that overlapping mechanisms modulate attention in numerical and spatial tasks (see also Hubbard et al. 2005). In their experiments, participants who show a high degree of left-side pseudoneglect in a physical bisection task also showed a high degree of small-number pseudoneglect in a numerical bisection task. Furthermore, this is unlikely to result just from an analogical mapping between number and space used during learning, because numerical bisection biases, like physical biases, depend on whether numbers are physically presented in near or far space (Longo & Lourenco 2009). This fact suggests an online connection between spatial and numerical attention. Longo and Lourenco (2009) interpret these results in terms of shared mechanisms: Foundational processes that guide attention in physical space are recycled to guide attention in numerical space.

Moreover, there is ample evidence for the reuse of motor-control systems in mathematical processing. For example,

Andress et al. (2007) report that hand motor circuits are activated by a dot-counting task; Badets and Pesenti (2010) demonstrated that observing grip-closure movements (but not nonbiological closure motions) interferes with numerical magnitude processing; and Goldin-Meadow (2003) recounts the many ways in which gesturing aids in the acquisition of mathematical concepts. These examples suggest that the motor system offers representational resources to disparate domains.

Similarly, several authors have reported that algebraic reasoning co-opts mechanisms involved in perception and manipulation of physical objects (Dörfler 2004; Kirshner 1989; Landy & Goldstone 2007; Landy & Goldstone 2008). For example, Landy and Goldstone (2007) report that reasoners systematically utilize processes of perceptual grouping to proxy for the ordering of algebraic operations. Again, the relationship does not appear to be merely analogical. As Carey emphasizes, one expects analogies to occur over extended durations. In contrast, Kirshner and Awtry (2004) report that, at least, the use of spatial perception in interpreting equation structure happens immediately upon exposure to the spatially regular algebra notation, and must be unlearned through the process of acquiring sophisticated algebraic knowledge. The most natural interpretation is that relevant computations are performed directly on spatial representations of symbol systems, and tend to work not because of developed internal analogies but because the symbolic notation itself generally aligns physical and abstract properties.

In basic arithmetic knowledge, and in the algebraic understanding of abstract relations, distinctly perceptual-motor processing is applied to do conceptual work in a widely different domain. Despite Carey’s assumption that cognitive resources are strongly typed – some are domain-specific input analyzers, some are components of core knowledge, others are parts of richer domain theories – it appears that at least some basic cognitive resources are used promiscuously in a variety of domains, and applied to a variety of contents. We suggest that this is possible for two reasons. First, on an evolutionary timescale it is more efficient to repurpose or replicate preexisting neural structures than to build entirely new ones. Second, many new symbolic environments, such as a math class utilizing a number line, or algebraic notation, form rich and multimodal experiences, which can themselves be analyzed using preexisting cognitive processes (“core” or not). Whenever such analyses yield largely successful results, a learner is likely to incorporate the relevant constraints and computational systems into the conceptual apparatus (Clark 2008). Therefore, initially dedicated mechanisms such as those governing perceptual grouping and attention, can be co-opted, given an appropriate cultural context, into performing highly abstract and conceptual functions.

When we make the claim that perceptual processes are co-opted for mathematical reasoning, for example, that automatically computed spatial arrangements of physical symbols are used as proxies for understanding generic relations, this is not a return to old-fashioned empiricism. It is not our view that all mathematical content can be reduced to perceptual content. Nor is it our view that because humans are able to co-opt perceptual processes to do mathematics, that this implies that the content of mathematical claims can be exhaustively reduced to perceptual primitives. Indeed, it is important to our story that they are not so reduced. We wish to point out that cognitive resources that are used for perceptual and motor reasoning in one domain are often usefully exploited for conceptual understanding in another domain. In fact, given the mounting evidence for the reuse of neural systems across the boundaries of traditional cognitive domains (Anderson 2010), it would be very surprising if many of our most important cognitive resources were domain-bound in the manner of Carey’s core processes.

In short, whether on a cultural or evolutionary timescale, learning systems apply any available resources to the understanding of new symbol systems, without regard for whether that old system is domain-specific, “perceptual,” or “conceptual.” One important role

of culturally constructed symbol systems is to serve, themselves, as rich environmental structures that can be the target of pre-existing cognitive mechanisms (“core” or otherwise). Carey’s “Quinian bootstrapping,” which treats novel symbol systems as mere placeholders, with no properties beyond conceptual role – that is, their inferential relationship to other symbols in their set – simplifies the process of learning new symbol systems at the cost of missing much of their value.

## What is the significance of *The Origin of Concepts* for philosophers’ and psychologists’ theories of concepts?

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Edouard Machery

Department of History and Philosophy of Science, University of Pittsburgh, Pittsburgh, PA 15260.

[machery@pitt.edu](mailto:machery@pitt.edu) [www.pitt.edu/~machery/](http://www.pitt.edu/~machery/)

**Abstract:** Carey holds that the study of conceptual development bears on the theories of concepts developed by philosophers and psychologists. In this commentary, I scrutinize her claims about the significance of the study of conceptual development.

Psychologists will probably come to view *The Origin of Concepts* (Carey 2009) as a landmark in the history of psychology, as important as Piaget’s (1954) *The Construction of Reality in the Child*. Among other virtues, it illustrates how extraordinarily successful the nativist research program in developmental psychology has been since the 1970s.

That said, *The Origin of Concepts* is not without shortcomings. Here, I focus on its significance for a general theory of concepts. In my view (Machery 2009; 2010a), philosophers and psychologists have usually focused on two distinct issues (a point Carey acknowledges; pp. 489–91):

1. *The philosophical issue:* How are we able to have propositional attitudes (beliefs, desires, etc.) about the objects of our attitudes? For example, in virtue of *what* can we have beliefs about dogs?

2. *The psychological issue:* Why do people categorize, draw inductions, make analogies, combine concepts, and so forth, the way they do? For example, why are inductive judgments sensitive to similarity?

Psychologists attempt to solve the psychological issue by determining the properties of the bodies of information about categories, substances, events, and so forth, that people rely on when they categorize, make inductions, draw analogies, and understand words.

In the introduction (p. 5) and in the last chapter of *The Origin of Concepts* (particularly, pp. 487–89, 503–508), Carey claims that the study of conceptual development casts light on the philosophical issue. However, the reader is bound to be disappointed, for Carey does not let the philosophical theory of reference she officially endorses – informational semantics – determine what babies’ and toddlers’ concepts refer to; instead, she relies on her *intuitions* to determine what their concepts refer to, and she uses philosophical theories of reference to *justify* her intuitions.

Two aspects of Carey’s discussion support this claim. First, quite conveniently, the philosophical views about reference Carey discusses never lead her to conclude that babies’ and toddlers’ concepts refer to something different from what she intuitively takes them to refer to.

Second, although Carey endorses an informational semantics, she in fact appeals to several distinct theories of reference, and she switches from one theory to the other when convenient. When Carey discusses the reference of the concept of object, she appeals to Fodor’s informational semantics (pp. 98–99),

according to which a concept (e.g., the concept of dog) refers to the property that nomologically causes its tokening (e.g., the property of being a dog). Elsewhere (pp. 17, 99), she seems to endorse some (quite unspecified) version of a teleological theory of reference (of the kind developed by Millikan and Neander): a concept refers to a particular property because the evolutionary function of this concept (roughly, what this concept evolved to do) is to be tokened when this property is instantiated. According to this view, the concept of dog refers to the property of being a dog because its function is to be tokened when dogs are perceived (whether perceiving dogs actually causes its tokening or not). Finally, when she discusses the reference of the analog magnitude representations (e.g., pp. 293–95), she implicitly endorses an isomorphism-based conception of reference (of the type developed by Cummins), according to which the reference of our thoughts depends on an isomorphism between the laws that govern them and the laws that apply to some domain of objects. On this view, thoughts about natural numbers are about natural numbers if and only if they obey laws that are isomorphic to the arithmetic operations defined over natural numbers.

These theories of reference are fundamentally distinct. They disagree about what determines the reference of concepts (their current nomological relations with properties outside the mind, their past evolutionary history, or the isomorphism between the laws that govern their use and other laws), and they also occasionally disagree about what concepts refer to. Furthermore, it would do no good to propose to combine these theories into an encompassing theory of reference because it would be unclear why this encompassing theory should be preferred to each of these theories considered on its own. Nor would it do to simply state that different theories of reference apply to different types of representations because one would then need to explain why a particular theory applies to a particular type of concept.

Carey also holds that the study of conceptual development casts some light on the psychological issue (p. 487), but what is curious is that, she overlooks much of the psychological research on concepts (for review, see Murphy 2002; Machery 2009), and she promptly dismisses the principal theories of concepts developed by psychologists working on categorization, induction, and concept combination (pp. 496–99). Her main argument is that one needs to distinguish people’s concepts from their beliefs or conceptions, that the psychological theories of concepts (prototype, exemplar, and theory theories) were developed to explain categorization, and that research on categorization casts light only on the nature of people’s conceptions because categorization is holistic (pp. 490–491, 498). She also holds that psychologists’ theories of concepts are descriptivist and that descriptivism is false.

I will briefly deal with Carey’s second argument. Psychologists’ theories of concepts are not committed to descriptivism because they can be combined with any theory of reference (Machery 2010a, p. 235). In addition, Carey’s appeal to Kripke’s and Putnam’s anti-descriptivist views is problematic in light of the cross-cultural variation in intuitions about reference (Machery et al. 2004; Mallon et al. 2009; Machery et al. 2009).

I now turn to the first argument. A theory of concepts that attempts to explain categorization (as prototype, exemplar, and theory theories do) is able to distinguish concepts and conceptions, for one can, and should, distinguish the information that is used by default, in a context-insensitive manner in categorization (people’s concepts) from the information used in a context-sensitive manner (their conceptions; Machery 2009, 2010b). Furthermore, the bodies of information that are used by default in categorization are also used by default in induction, in concept combination, and so forth. For example, typicality effects found in categorization, induction, and concept combination show that prototypes are used in the processes underlying all these cognitive competences. Therefore, the main psychological theories of concepts can not only distinguish concepts from conceptions, they are also essential to solve the psychological

issue: explain how we reason, make inductions, draw analogies, in brief explain how we think.

## What is the narrow content of *fence* (and other definitionally and interpretationally primitive concepts)?

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Eric Mandelbaum

Faculty of Philosophy, The Future of Humanity Institute, University of Oxford, Oxford OX1 1PT, United Kingdom.

eric.mandelbaum@philosophy.ox.ac.uk [http://www.fhi.ox.ac.uk/our\\_staff/research/eric\\_mandelbaum](http://www.fhi.ox.ac.uk/our_staff/research/eric_mandelbaum)

**Abstract:** It's unclear what narrow content is interpersonally shared for concepts that don't originate from core cognition yet are still definitionally and interpretationally primitive. A primary concern is that for these concepts, one cannot draw a principled distinction between inferences that are content determining and those that aren't. The lack of a principled distinction imperils an account of interpersonally shared concepts.

Carey's illuminating discussion of the empirical motivation for a dual factor theory of content is remarkably broad (Carey 2009), but there are some lingering worries one might have regarding narrow content. Carey wants to avoid radical semantic holism because of its troubles with explaining how interpersonal communication is possible. Yet she also wants narrow content to have a role in determining the semantics of concepts. In order to do so, she has to specify which computational roles are content-determining and which are merely extraneous beliefs or part of the sustaining mechanisms. Theories that cannot distinguish the content-determining inferences from the non-content-determining inferences must say that all inferences are content-determining, thereby embracing a radical semantic holism. Carey sees this problem and puts forth some forceful suggestions concerning the two types of concepts that her book focuses on: concepts stemming from core cognition, and bootstrapped concepts. She contends that for core cognitive concepts (e.g., *agent*, *object*), the inferences that are content-determining are the innately specified and unrevisable ones; for concepts that are bootstrapped (e.g., *force*, *division*) the inferences that form the original hermeneutic circle are the content-determining inferences (e.g., the narrow content of *force* is specified by its connection to *mass* and *acceleration*). These appear to be quite different criteria for the two different types of concepts. Presumably what is underwriting the criteria is that the narrow content in question is apt to stay constant across people, therefore allowing for interpersonal communication.

So far, so good. However, a worry arises once we look at concepts that are neither derived from core cognition nor bootstrapped in a Quinean way. Take, for example, the definitionally and interpretationally primitive concept *fence* (if it turns out *fence* isn't definitionally and interpretationally primitive, then substitute your favorite concept that is both definitionally and interpretationally primitive and not the output of core cognition, e.g., *fan*). Presumably, such a concept is neither the output of core cognition nor the result of some bootstrapping process (at the very least it does not fit the canonical pattern of placeholder structures that Carey describes for other bootstrapped concepts). Imagine three people introduced to fences in the following ways: one is introduced to fences by reading Robert Frost and comes to believe that what is most important about fences is that they make good neighbors. Another is introduced to fences in New York, where they are mostly chain-link and topped with barbed wire. The third has grown up next to a field that has a few feet of picket fence placed right in the middle of it, separating nothing of consequence. What content is the shared narrow content among these people?

The current worry is that for concepts that aren't the outputs of core cognition or bootstrapped via placeholders there will be no set of inferences that are interpersonally shared. In the above example, the central computational roles of *fence* don't appear to be shared. The concepts of these three people needn't even share beliefs about the function of fences (the third person may have the concept even without having the belief that fences are used to separate areas) nor need their concepts share any perceptual features. But it seems plausible that such a group could still communicate about fences. If this is so, how do we distinguish the inferences that are content-constitutive from the ones that are not? What types of narrow content must be shared among such people in order to facilitate communication? In other words, what is the narrow content of *fence*?

Carey says that this type of problem, the problem of specifying the gritty details of content, is endemic to all competitor theories. Certainly this is true, but there is more reason to be skeptical about her particular proposal than she lets on; what a theory of narrow content would need in order to ground shared content (and thus shared concepts) is nothing short of an analytic/synthetic distinction. Carey does an admirable job of motivating something like an analytic/synthetic distinction for core cognitive and bootstrapped concepts, which is no small feat. However, she accomplishes this via appealing to data that point to shared psychological structures that, as a matter of nomological fact, underwrite the acquisition of certain concepts. The question remains: for the multitudinous class of concepts that are simple yet not the result of innate machinery, what are the shared psychological structures underwriting the shared narrow content? They can't be prototypes, as Carey herself claims that "[prototype] theories also make a mystery of shared concepts, failing to address the problem of disagreement: my prototype of a dog (or the dog exemplars I represent) must be different from yours, yet we both have concepts of dogs" (Carey 2009, p. 497).

As suggested, Carey might respond by saying that all the competitor theories face similar problems of specifying the details. However, the burden of proof does not appear to be identical across theories. For example, Carey criticizes Fodor's theory for not detailing the sustaining mechanisms for concepts. But Fodor can't characterize the "mere engineering" (see, e.g., Carey 2009, p. 535) because he supposes that they are a heterogeneous array of mechanisms, and consequently research will have to proceed piece by piece (see, e.g., Fodor 1998). There is no in-principle argument against this piecemeal endeavor, even though surely we are quite a long way from the completion of any such task. In contrast, for Carey's dual content view to work out, we need something akin to an analytic/synthetic distinction which, even without the unnecessary epistemological baggage, is an elusive prize. Nevertheless, we may hope that such a distinction can be found: after all, Carey has been able to impressively describe something akin to it for certain kinds of concepts. That being said, those concepts seem like they may be the exception, not the rule. So I close by asking: what could the shared narrow content for simple, non-core, non-bootstrapped concepts be?

## A leaner nativist solution to the origin of concepts

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Jean M. Mandler

Department of Cognitive Science, University of California, San Diego, CA 92093.

jmandler@ucsd.edu <http://www.cogsci.ucsd.edu/~jean/>

**Abstract:** There must be innate conceptual machinery, but perhaps not as much as Carey proposes. A single mechanism of Perceptual Meaning Analysis that simplifies spatiotemporal information into a small number of



conceptual primitives may suffice. This approach avoids the complexities and ambiguities of interactions between separate dedicated analyzers and central concepts that Carey posits, giving learning a somewhat larger role in early concept formation.

There are few developmental psychologists who have attempted to elucidate in detail the origin of concepts in infancy. As one of that band, I applaud Carey's book (2009) but have some suggestions as to how to simplify her view of core cognition.

Carey states that it is impossible to derive concepts solely from sensorimotor information. I agree. However, elaborate conceptualizing machinery may not be necessary. Throughout her book, she defends discontinuity in development and the importance of bootstrapping. Therefore, we must consider the possibility that core cognition may be leaner than she suggests, with more of the work being accomplished by associative learning enriching a small number of innate conceptual representations (followed by linguistic and analogical bootstrapping).

Carey offers dedicated innate analyzers for objects, agents, and number plus a rather unspecified central processor that also has innate concepts. The latter can be combined with outputs from the dedicated analyzers to produce, for example, the concept of cause. Not much is said about the interactions between dedicated analyzers and innate central concepts. It is difficult to disentangle them, and this approach may allow too many alternative empirical outcomes. Furthermore, a more parsimonious system can accomplish the same goals. A single innate analyzer (such as Perceptual Meaning Analysis; Mandler 2004; 2008; 2010) that simplifies attended spatiotemporal information into a small number of conceptual primitives, can produce first concepts of objects, agency, and causality. It also allows combinations of them and provides first concepts for relations such as containment, occlusion, and support. The resulting representations, in conjunction with information directly supplied by the perceptual system, are sufficient to account for current infant data, including early language understanding (although this approach has not yet been extended to number).

The uncertainty Carey expresses as to how a concept of cause originates (Carey 2009, Ch. 6) illustrates the problem of assuming that some innate concepts are the product of separate analyzers and others part of a central mechanism. Carey argues against Michotte's (1946/1963) view that perceiving motion transferred from one object to another is obligatory and foundational for understanding causality, because she states that whether objects are inert or animate affects causal interpretation right from the beginning. However, not only is this conclusion debatable, it does not invalidate the view that motion transfer is obligatory and foundational for causal understanding. For example, the fact that infants are not surprised when animates move without contact does not refute Michotte's claim; there is no evidence that infants conceive of self-starting motion as causally based.

Carey's stronger argument depends on the claim of simultaneous emergence of concepts of contact causality and change of state causality. However, in my reading of the literature, I do not find enough evidence for simultaneous emergence. Adult-like response to both contact causality and entraining has been demonstrated at 3 to 4 months of age (Leslie 1982) whereas change of state causality has not been not shown before 8 months of age, and not even then unless a hand is involved. In infancy research, this is a sizeable gap. Leslie's experiments differ in detail from his definitive work with 6-month-olds, but the outcomes at 3 to 4 months are essentially the same.

Further, there is the dynamic aspect of causality. If forceful causality can be learned, then why cannot change of state causality also be learned, leaving motion transfer sufficient for core cognition? A way to do this (Mandler 2008; 2010) is by an innate conceptual primitive of "make move" based on seeing motion transfer from one object to another, with force added to the concept only when infants begin to move themselves around in the world and experience their own exertions in manipulating

objects. Three-to-four-month-olds have little, if any, such experience. Once they do in the second six months, there is an already organized representation of caused motion available to be integrated with feelings of bodily exertion. Change of state causality is apt to be conceptualized even more slowly than adding force, because although infants may notice the relevant correlations, they need a more complex chain of associations to reach the core "make move" concept. Even adults often misconstrue change of state causality when it is not associated with motion transfer; it is not obligatory in the same way.

Carey also rejects my single analyzer approach (p. 195) because she says there is no known way that Perceptual Meaning Analysis could transform spatio-temporal properties into representations of intentional agency. But agency (goal-directed behavior) *can* be defined in spatial-temporal vocabulary, and there is evidence that this is indeed how it begins in infancy, as the observation of repeated paths of motion taking the most direct possible paths to the same end point (e.g., Csibra 2008). Infants learn early on that people are the most likely agents, but they accept inanimate boxes as agents too if they follow contingent paths. Even adults sometimes do, suggesting the obligatory character of this core concept. Understanding agency in terms of mental intentions is not part of core cognition but a late development, requiring infants' own attempts (and failures) to reach goals to become associated with the earlier established representations of agency in terms of paths of motion. Associating eyes (or head turns) with goal-directed paths is easy enough to learn, but mental intentionality is difficult and may even require language to become established.

Another concern is how Carey's core cognition enables the recall of event sequences and mental problem solving that have been demonstrated in the second six months. Such mental activities require explicit concepts, but the latter are not part of her core cognition and in her account explicit concepts appear to require language (Ch. 1). Therefore, although concepts are defined as units of thought, it is not clear how preverbal infants manage such thoughtful processes as recall and problem solving. An advantage of a mechanism such as Perceptual Meaning Analysis is that it creates iconic representations enabling imaginal simulations that even preverbal infants can use for thought.

I agree with much that Carey proposes but suggest that a single innate mechanism may suffice as the origin of core concepts.

## Beyond the building blocks model<sup>1</sup>

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Eric Margolis<sup>a</sup> and Stephen Laurence<sup>b</sup>

<sup>a</sup>Department of Philosophy, University of British Columbia, Vancouver, BC, V6T 1Z1, Canada; <sup>b</sup>Department of Philosophy University of Sheffield, Sheffield S3 7QB, United Kingdom.

[margolis@interchange.ubc.ca](mailto:margolis@interchange.ubc.ca)

[http://web.mac.com/ericmargolis/primary\\_site/home.html](http://web.mac.com/ericmargolis/primary_site/home.html)

[s.laurence@shef.ac.uk](mailto:s.laurence@shef.ac.uk)

<http://www.shef.ac.uk/philosophy/staff/profiles/slaurence.html>

**Abstract:** Carey rightly rejects the *building blocks model* of concept acquisition on the grounds that new primitive concepts can be learned via the process of bootstrapping. But new primitives can be learned by other acquisition processes that do not involve bootstrapping, and bootstrapping itself is not a unitary process. Nonetheless, the processes associated with bootstrapping provide important insights into conceptual change.

Concept learning often involves the construction of complex concepts in accordance with a compositional semantics. It is widely assumed that the primitive concepts that form the basis of all complex concepts are themselves innate – a view we call the

*building blocks model* of concept learning. The building blocks model is central to Fodor's (1981) case for radical concept nativism but also to moderate forms of nativism, such as Pinker's (2007), and is assumed by virtually all empiricist accounts of concept learning. A central theme in *The Origin of Concepts* (Carey 2009), however, is that the building blocks model is mistaken; new primitives can also be learned. One of the most important ways of learning a new primitive, according to Carey, is via conceptual bootstrapping.

We agree with Carey both about the limitations of the building blocks model and about the significance of bootstrapping. However, bootstrapping, as Carey herself acknowledges, is not the only way of learning new primitive concepts. Nor is bootstrapping itself a single unitary process. Rather, bootstrapping consists of a number of distinct processes that resemble one another to varying degrees.

Carey cites six criteria for bootstrapping to occur, but the two that seem especially important are (1) the reliance on initially uninterpreted (or minimally interpreted) external symbols, and (2) the reliance on modeling processes. The external symbols serve as a placeholder structure, while the modeling processes facilitate their interpretation. When all goes well, the representations that correspond to the placeholder structure take on suitable inferential roles determining the new concepts' narrow content. Although analogical reasoning is often involved, other modeling processes include the use of thought experiments, limiting-case analyses, and abduction.

Our doubts about the unity of bootstrapping have to do with the character of the placeholder structure and the variety of modeling processes. As Carey describes the role of placeholders, they are initially uninterpreted (or minimally interpreted) and it is the rich relations among these external symbols that do most of the work in constraining the interpretation that bootstrapping achieves. These aspects of bootstrapping are especially clear in her flagship example of the positive integers. In other instances, however, the placeholder itself is well-understood (even if the concepts to be acquired are not) and there are few inter-symbol relations to speak of. Take Kepler's concept of *motive force*. According to Carey, the placeholder for Kepler's bootstrapping was the abductive hypothesis that something in the sun causes the motion of the planets, and the bootstrapping process led him to the idea of a force emanating from the sun that causes the motion of the planets. Although Kepler fully understood the placeholder hypothesis, the analogy he eventually hit upon did not depend upon the *structure of the placeholder* – unlike the number case, where the structural mapping between the ordered list of uninterpreted number words and ordered sets is crucial.

Regarding the various modeling processes that bootstrapping relies upon, the question is how alike they are once you get into the details. Analogy perhaps is to be accounted for in terms of structure mapping (Gentner 1983). But it is doubtful that structure mapping is essential to working through a thought experiment or engaging in abductive inference, and different instances of bootstrapping will appeal to different types of modeling processes. If these processes have anything in common, it would seem to be a loose affinity in how they contrast with empiricist learning strategies, such as association and statistical analysis.

Like bootstrapping, our own (Laurence & Margolis 2002) model of concept acquisition provides an account of primitive concept acquisition. On our model, new natural kind concepts are created by a dedicated acquisition system that employs a conceptual template. For example, on exposure to a new type of animal, the system creates a new mental representation with slots for information about the animal's salient perceptual properties (a "syndrome"), while ensuring that the representation's role in inference is governed by an essentialist disposition. Together, the syndrome and the essentialist disposition establish the appropriate mind-world dependency relations to underwrite

conceptual content. This account differs from Carey's in a number of important respects. One is that our account involves a dedicated system for acquiring new primitive concepts of a particular type. Also, our account does not require the use of external symbols but instead has the acquisition system directly deploy new mental representations; on our model, even an isolated individual who has no external symbol system could acquire a new animal concept. Finally, our account does not implicate modeling processes.

New primitives are not limited to those acquired via dedicated acquisition systems, however. Consider, for example, concepts for new rituals. One might acquire such concepts by deploying new representations that then serve as accretion points for conceptual roles. This might be facilitated by an external symbol system (e.g., words for aspects of the ritual), but a placeholder structure is not necessary. And since acquiring concepts on an accretion point model of this sort might be as easy as the gathering of factual information, the steps involved need not involve modeling processes or result in incommensurability. This model is inspired by Block's (1986) discussion of conceptual role semantics. But it is in fact compatible with a variety of theories of content that treat the new concepts as primitive. What allows the concepts to be primitive is the fact that the conceptual roles can be non-analytic and defeasible. As a result, there are at least two alternatives to bootstrapping – our earlier model and this accretion point model. Both of these alternatives to bootstrapping, however, are ill-suited for learning the more demanding concepts that Carey's bootstrapping account can accommodate – the kind that rely on formal education for children and intellectual breakthroughs for scientists. For this reason, bootstrapping processes are crucial.

Contrary to the building blocks model, human beings have a number of ways of fundamentally expanding their conceptual system. Though bootstrapping itself is not a single process, the sorts of cognitive operations that Carey draws attention to help us to understand some of the most challenging instances of conceptual change, particularly those that involve incommensurability.

#### ACKNOWLEDGMENT

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#### NOTE

1. This article was fully collaborative; the order of the authors' names is arbitrary.

## Can developmental psychology provide a blueprint for the study of adult cognition?

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Arthur B. Markman

Department of Psychology, University of Texas, Austin, TX 78712.

markman@psy.utexas.edu

<http://www.psy.utexas.edu/psy/FACULTY/Markman/index.html>

**Abstract:** In order to develop sophisticated models of the core domains of knowledge that support complex cognitive processing in infants and children, developmental psychologists have mapped out the content of these knowledge domains. This research strategy may provide a blueprint for advancing research on adult cognitive processing. I illustrate this suggestion with examples from analogical reasoning and decision making.

Carey marshals significant evidence supporting the idea that children have a series of core domains of knowledge that give them a rudimentary understanding of the world (Carey 2009). Over time, that knowledge is expanded to provide more elaborate

representations that ultimately support adult-like competence. For example, even infants are sensitive to basic causal events in which one moving object contacts a second and causes it to move. Early on, however, infants do not seem to perceive events that create a change of state in an object as causal.

What is impressive about this work is that a community of researchers has carefully mapped domains of knowledge to identify the limits of infant knowledge and to determine the progression of changes in that knowledge over time. From this pattern of knowledge change, it is possible to make detailed proposals about psychological mechanisms that may play a role in the development of knowledge, such as the role that language may play in the development of numerical competence.

This approach contrasts sharply with the standard mode of research on cognitive processing in adults. Most research on adults focuses on the structure of adult knowledge, but not its content. Whereas this strategy has enabled impressive gains in our understanding of cognitive processing, in many areas of research we have reached the limits of what we will learn about cognitive processing without at least some focus on the content of adult's knowledge.

Research on analogy provides an instructive example. In analogy, most researchers in the field subscribe to a basic set of principles that are encapsulated in Gentner's (1983) structure mapping theory (Gentner & Markman 1997; Holyoak & Thagard 1989; Hummel & Holyoak 1997). This theory proposes that people are able to draw analogies between distant domains (such as the atom and the solar system) by representing relations using structured representations that explicitly encode the connection between a relation and its arguments. Analogies involve finding parallel relational structures that are found by matching identical relations and ensuring that the arguments of those relations are also placed in correspondence. Analogical inferences allow one domain to be extended by virtue of its correspondence to another by copying knowledge from the base domain to the target domain when that knowledge is connected to the correspondence between domains (Holyoak et al. 1994). This theory makes proposals only about the structure of people's knowledge, but not its content.

This framework has allowed the field to learn a lot about the way children and adults form analogies and use that knowledge to learn new information. For example, studies have demonstrated that children and adults are better able to process analogies when they involve higher-order relations that bind together a lot of representational structure than when they involve only low-order relations (Clement & Gentner 1991; Gentner & Toupin 1986). Other studies have demonstrated that these relational connections are crucial for licensing which information will be carried from base to target as an inference (Markman 1997). Finally, this work supported the discovery that there is a psychological distinction between alignable differences in which each item has a corresponding property that differs, and nonalignable differences in which one object has a property that has no correspondence in the other (Markman & Gentner 1993).

Basic research on analogical reasoning has stagnated, however. There is some interest in factors that promote retrieval of analogies from memory, but the area is not a thriving source of new publications. A key reason for this decline in productivity is that it has become difficult to generate new predictions about analogical reasoning without knowing anything about the content of what people are reasoning about. Most of the studies in the 1980s and 1990s focused on novel pictures, analogies, stories, and insight problems that had little or no connection to the knowledge people had when they entered the laboratory (Gentner et al. 1993; Gick & Holyoak 1980).

The developmental research that Carey reviews suggests that it would be fruitful for researchers to focus more systematically on the content of people's knowledge to develop insights about analogy. There has been some research on the history of science that has examined the content of the knowledge used

in analogies (Gentner et al. 1997; Nersessian 1987). There has also been work with expert designers that has looked at the influence of domain knowledge on the use of analogy in generating ideas for new products (Christensen & Schunn 2007; Dunbar 1997; Linsey et al. 2008). However, there hasn't been any systematic study of particular domains of knowledge by communities of researchers. Consequently, there is little continuity in research from one laboratory to the next.

The impressive gains in our understanding of the development of children's knowledge in core domains has emerged from the commitment of a community of researchers to explore a common topic, despite theoretical disagreements about the underlying developmental processes. This success suggests that a similar strategy would be worthwhile in the study of cognition in adults.

Research on decision making provides an instructive case study here. As Goldstein and Weber (1995) point out, for many years research on decision making was dominated by studies of how people made choices from a set of risky gambles. Gambles were used because you did not need to know much about people's preference structures to know that they are likely to prefer more money to less money. In the modern study of decision making, studies of gambles have been supplanted by work on consumer behavior that explores what people know about brands of products and how they use that knowledge to make choices. In addition, research on decision making has begun to map out a broad set of people's motivational structures as a way of understanding how the waxing and waning of people's goals influences their preferences. The progress in this area suggests that a similar revolution ought to take hold in other areas of cognitive research, such as analogy, categorization, and problem solving.

## Representation development, perceptual learning, and concept formation

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I. P. L. McLaren,<sup>a</sup> Andy J. Wills,<sup>a</sup> and S. Graham<sup>b</sup>

<sup>a</sup>Washington Singer Laboratories, School of Psychology, University of Exeter, Exeter EX4 4QG, United Kingdom; <sup>b</sup>Clinical Imaging Research Centre, National University of Singapore, Singapore 117456

i.p.l.mclaren@exeter.ac.uk a.j.wills@exeter.ac.uk

psysg@nus.edu.sg

**Abstract:** We argue for an example of "core cognition" based on Diamond and Carey's (1986) work on expertise and recognition, which is not made use of in *The Origin of Concepts*. This mechanism for perceptual learning seems to have all the necessary characteristics in that it is innate, domain-specific (requires stimulus sets possessing a certain structure), and demonstrably affects categorisation in a way that strongly suggests it will influence concept formation as well.

In *The Origin of Concepts* (Carey 2009), there is an in-depth examination and analysis of how we come to represent the world. The argument is that starting with both perceptual primitives and a newly-specified set of innate cognitive primitives labelled "core cognition," we are able to bootstrap ourselves to the point where complex, abstract symbolic constructs such as "momentum" and "kinetic energy" are available to us. But in framing this argument, Carey neglects, to some extent, a portion of her own work. This commentary will argue that in so doing she omits some of the best evidence for her claim that "core cognition" is a key component of concept formation.

In 1986 Diamond and Carey published an influential article ostensibly on face recognition, but in actuality on representation development as a consequence of experience. In "Why faces are and are not special: An effect of expertise," they make the case for the face inversion effect being, at least in part, a product of



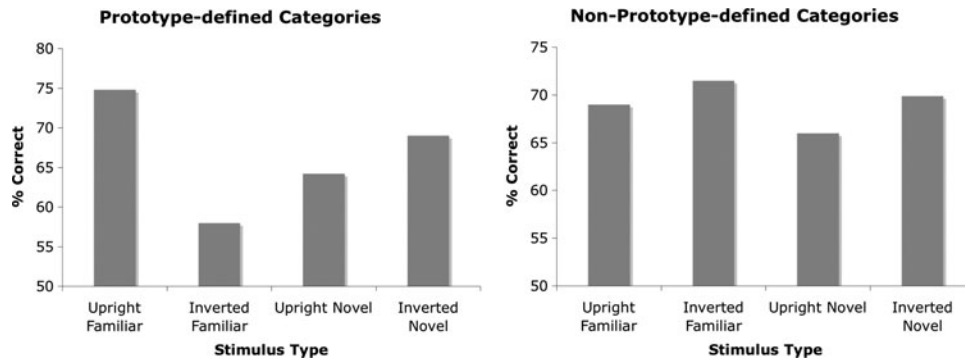


Figure 1 (McLaren et al.). The left panel shows results for the discrimination test after familiarisation with prototype-defined categories, the right results after familiarisation with the shuffled-row chequerboards. Adapted from McLaren et al. (1997).

experience with a category (faces of a given ethnic type) that possesses the requisite structure. This latter qualification refers to the idea that the category must be prototype-defined, in the sense that its exemplars vary in their second-order relational structure, and that this variation can, for present purposes, be loosely characterised as small deviations about the first-order relational structure defined by the prototype. Diamond and Carey (1986) made their case by showing that an inversion effect similar to that in faces could be obtained with certain classes of breed of dogs if the participants in the recognition experiment possessed the requisite expertise by virtue of being dog show judges of long standing. Novices, on the other hand, who were not as familiar with this category of stimuli, did not show a strong inversion effect.

We (McLaren 1997) have extended this result to abstract chequerboard categories defined by a prototype. In these experiments, randomly generated base patterns constructed from black and white squares (which served as prototypes) had noise added to them (some squares changed from black to white or from white to black) to create a number of exemplars of that category. Our participants were then trained to distinguish between two such categories, each defined by a different prototype or base pattern (i.e. between-category training), before being tested for their ability to discriminate between two members of a given category (i.e. a within-category test). The result was that discrimination was much better for the familiar categories than for exemplars taken from novel, control categories (Graham & McLaren 1998; McLaren et al. 1994), and this advantage was lost on inversion. We were also able to show that this result was not an inevitable consequence of familiarisation with a category constructed from chequerboards. By starting with base patterns as before, but then creating exemplars by randomly shuffling rows rather than randomly changing squares, we were able to create categories that could be discriminated just as easily as our earlier set, but when tested for within-category discrimination, participants were now no better on exemplars drawn from familiar categories than those drawn from novel categories, and there was no hint of an inversion effect. The results of one of these experiments (McLaren 1997, Experiment 1) are shown in Figure 1, and the necessary category type by familiarity by inversion interaction emerges to justify the assertion that the inversion effect is contingent upon familiarity with a category of the correct type. Our results, then, strongly support the Diamond and Carey thesis that inversion effects can be at least partly explained as a consequence of expertise with categories that have the requisite prototype-defined structure.

We have also shown (Wills & McLaren 1998) that categorisation does not require feedback for effective learning to take place (Fried & Holyoak 1984), and that this type of perceptual learning effect, that is, an enhanced ability to tell category members apart after familiarisation with the category if the

category is prototype-defined, also occurs under these “free classification” conditions. It also leads to a different classification scheme (a change in the number of groups used). Therefore, the effect is not contingent on training via feedback, and we have considerable data indicating that it occurs as a result of mere exposure rather than as a consequence of any particular training regimen, (Suret & McLaren 2003; Wills et al. 2004); and in animals other than human (Aitken et al. 1996; McLaren & Mackintosh 2000).

If the innate ability of indigo buntings to identify the axis of rotation of the night sky is an example of domain-specific and species-specific animal core cognition, then surely the ability of humans to engage in this sort of representation development contingent on the appropriate stimulus input is an example of core cognition as well, and one that is directly relevant to concept formation. Here we have a domain-specific learning device tuned to a certain type of stimulus structure that undoubtedly influences categorisation in a manner that must have implications for conceptual representation. Therefore, we would argue that the “expertise” identified by Diamond and Carey (1986) is one of the best examples of core cognition in humans, and strengthens the thesis advanced by Susan Carey (2009).

## The prehistory of number concept

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Karenleigh A. Overmann,<sup>a</sup> Thomas Wynn,<sup>b</sup> and Frederick L. Coolidge<sup>c</sup>

<sup>a</sup>Department of Psychology, University of Colorado at Colorado Springs, Colorado Springs, CO 80918; <sup>b</sup>Department of Anthropology, University of Colorado at Colorado Springs, Colorado Springs, CO 80918; <sup>c</sup>Department of Psychology, University of Colorado at Colorado Springs, Colorado Springs, CO 80918.

koverman@uccs.edu twynn@uccs.edu  
fcoolidge@uccs.edu

**Abstract:** Carey leaves unaddressed an important evolutionary puzzle: In the absence of a numeral list, how could a concept of natural number ever have arisen in the first place? Here we suggest that the initial development of natural number must have bootstrapped on a material culture scaffold of some sort, and illustrate how this might have occurred using strings of beads.

In her discussion of a child’s acquisition of the concept of natural number, Carey (2009) makes a strong case that a symbolic place-holder structure is necessary for Quinian bootstrapping. However, Carey’s argument leaves an important evolutionary puzzle unaddressed: In the absence of a numeral list, a count list of some kind, or perhaps even language itself, how could a concept of natural number ever have arisen?

Ethnographic and comparative linguistic data suggest that material phenomena can act as scaffolds for the development of a natural number concept. Drawing on a wide range of languages and number systems, both modern and ancient, Menninger (1992) noted that number words first appear in writing and only later in speech, in the form of ordered quantification adjectives up to “three” or “four,” a limit consistent with parallel individuation rather than some external system such as finger counting. Quantification adjectives later detach from their objects, and a transition to larger numbers consistent with magnitude representation occurs. Therefore, the purely linguistic evidence would appear to support the necessity of a full-blown linguistic model in support of a concept of natural number. However, Menninger also noted that many people who lack number words can and do arrange objects by one-to-one correspondence, and that tally sticks are “universal” (Menninger 1992, p. 224).

There are archaeological reasons for attributing number concept to much older cultures than those with writing. For several thousand years prior to the appearance of writing, Neolithic farmers in the Middle East used an accounting procedure in which individual clay tokens were matched in one-to-one correspondence to individual animals, measures of grain, and so on. The total of the tokens matched the quantity of the commodity (Schmandt-Besserat 1992). Earlier still are items such as the Tossal de la Roca plaque (Fig. 1), which is around 14,000 years old (d’Errico & Cacho 1994).

Someone engraved this “tally board” with sets of marks, using different tools, at different times. The groups of marks exceed the maximum of parallel individuation. Someone was keeping track of something, and whereas it is possible that it was simply a system of one-to-one correspondence, the use of different tools to add marks suggests some inchoate notion of individuated quantity beyond the limit of parallel individuation. The use of a material object in this case is provocative because it points to an alternative scenario for the emergence of natural number, one based in extended cognition (Wilson & Clark 2009), not mental representations of linguistic symbols. We suggest that

the initial development of natural number must have bootstrapped on a material culture scaffold of some sort. In place of a symbolic placeholding structure, there were material placeholding structures (Malafouris 2010). There are several possibilities, including tally boards or tokens of some kind, but the earliest example, and the one with the most potential, is the string of beads.

Archaeology provides a record of beads going back around 100,000 years. The best early examples come from Blombos Cave in South Africa (d’Errico et al. 2005). The beads are small, blue shells with punched holes (Fig. 2).

Wear traces around the hole edges indicate that the beads were strung. The Blombos beads are 77,000 years old; the nearby site of Pinnacle Point boasts similar beads that may be 20,000 years older (Bar-Matthews et al. 2010). The makers were modern human hunters and gatherers. Beads have also been recovered in the 40,000–50,000 year time range in Kenya and Turkey, and they are common beginning 30,000 years ago (Ambrose 1998; Kuhn et al. 2009).

A string of beads possesses inherent characteristics that are also components of natural number. There is individuation in the guise of individual beads, ordinality in their invariant sequence on the string, and a material instantiation of  $N + 1$ : every added bead increases the quantity of beads by the same amount. There is no necessity that people stringing beads understand them in this way, but the potential is there in a very real, tangible sense. And if our bead stringer makes an example with more than three or four beads, he or she will carry individuation beyond the range of parallel individuation. In a sense, the string of beads is a kind of feral number line, without an attendant number concept. When people use a string of beads as a record-keeping device, the beads come to play another role altogether. As Carey puts it: “The critical analogy that provides the key to understanding how the count list represents number is *between order on the list and order in a series of sets related by an additional individual*” (p. 477, emphasis ours). In the case of the string of beads, the analogy would be between sets in the real world and placeholder beads, which already possess

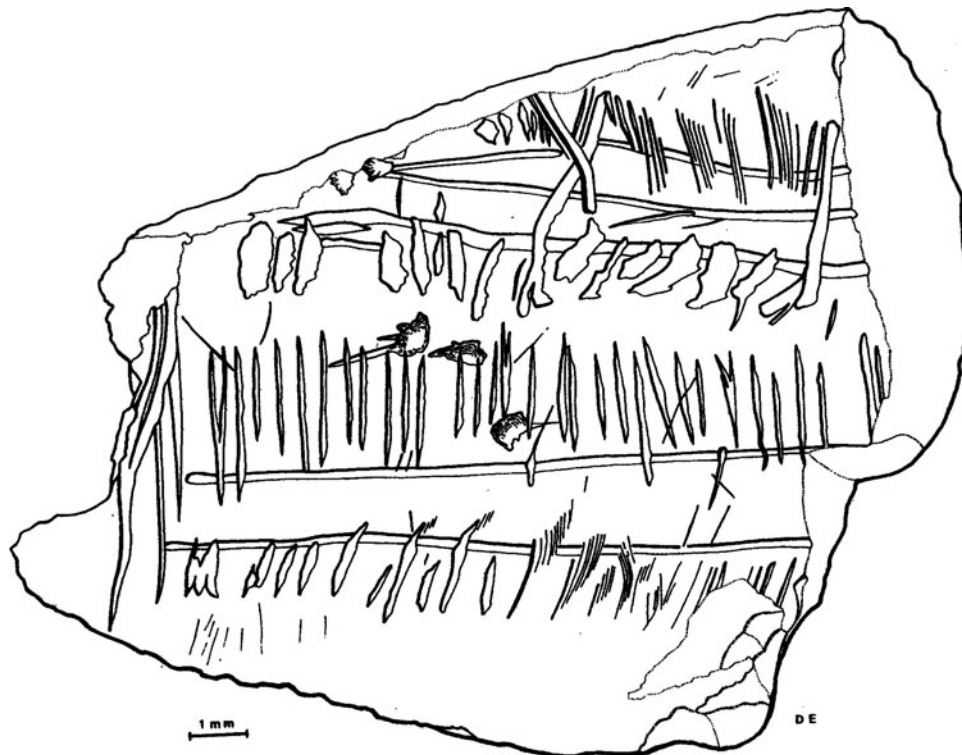


Figure 1 (Overmann et al.). Drawing of the Tossal de la Roca plaque.

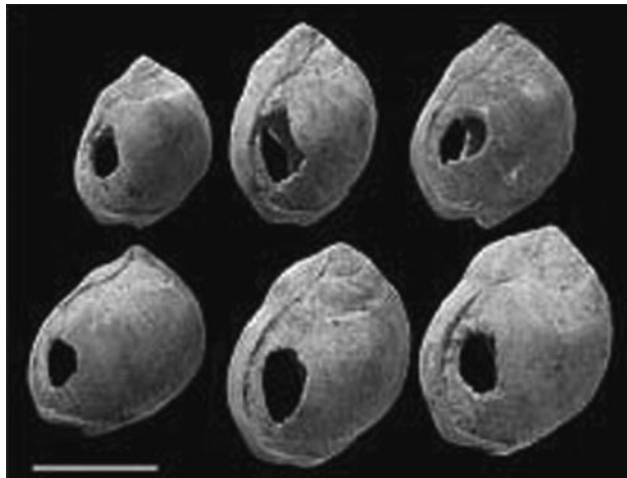


Figure 2 (Overmann et al.). Six of the 40 beads from Blombos Cave.

an inherent  $N + 1$ . A true numeral list emerges when people attach individual labels to the various placeholder beads. This material extended cognition scenario has greater natural potential than body-counting systems to scaffold development of natural number. Body-counting systems lack ordinality and lack the natural  $N + 1$ .

We do not know if the initial development of natural number concept took place in this fashion, or indeed even with beads. Our point is that the initial cultural construction of number could most easily have occurred using material objects rather than mental representations. Arguably, such a use of material markers is tantamount to symbol use; beads can stand for other things via one-to-one correspondence, a use preserved in the abacus and rosary. But this strikes us as far less general than the symbolic models Carey uses as examples of Quinian bootstrapping, and therefore more likely as an initial cultural construction.

Material objects with the potential to act as scaffolds for a natural number concept may have been available more or less continuously for perhaps 100,000 years. In cultural circumstances that required the development of more sophisticated numbering, such as formal record-keeping in an economic exchange system, these material scaffolds likely played a crucial role.

## How to build a baby: A new toolkit?

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Diane Poulin-Dubois

Department of Psychology, Concordia University, Montréal, Québec H4B 1R6, Canada.

[Diane.pouлиндubois@concordia.ca](mailto:Diane.pouлиндubois@concordia.ca)

<http://psychology.concordia.ca/facultyandstaff/faculty/pouлиндubois.php>

**Abstract:** Carey proposes a theory of conceptual development that specifies innate conceptual representations that get learning started. Those representations are the output of innate domain-specific input analyzers. I contend that innate core cognition about agency is itself a gradual construction and that the role of Quinian bootstrapping needs elaboration to account for the development of intuitive theories of psychology.

Cognitive development is one of the most difficult issues to understand in psychology. Any theory of cognitive development must include three components. First, it must characterize the

initial representational repertoire. Second, it must describe how the initial state differs from the mature conceptual system. Third, it must characterize the learning mechanisms that underlie the transformation from the initial to the mature state. Theoretical proposals on cognitive development can be (simplistically) classified in a theory space along two dimensions: (1) the nature of early representations and (2) how much conceptual change takes place in development. The classical Piagetian position is that infants have no representational system at birth and that a Copernican-type revolution occurs in the way children understand the world (Muller et al. 2009). At the extreme opposite, nativist theories hold that there are innate representations and no qualitative conceptual change, simply enrichment. Over the past two decades, a number of theorists have proposed theories of conceptual development that fall along this continuum (Bloom 2004; Elman et al. 1996; Gopnik & Meltzoff 1997; Mandler 2004; Siegler 1996; Shultz 2003). Carey has written an important book that advocates a rich initial representational state (core cognition) with a qualitative restructuring of concepts as a function of interaction with the world. I will concentrate on the success of *The Origins of Concepts* (Carey 2009; henceforth *TOOC*) with regard to the concept of agency.

With respect to the initial state, everyone agrees with Fodor that solipsism and blank-slate empiricism are too impoverished to characterize the human starting state. However, (almost) everyone disagrees with Fodor (1983) that this does not mean that adult commonsense psychology is implanted in the mind at birth or matures independent of experience. *TOOC*'s account has as its centerpiece the notion that the initial stock of human mental representations is not limited to sensory, perceptual, or sensorimotor primitives. Instead, there are innate conceptual representations (or core cognition) that are created by innate perceptual input analyzers. With regard to the concept of agency, Carey proposes that evolution provided humans with core cognition of representations of goals, information, and attentional states. Support for this proposal comes from research that shows that infants attribute goals to geometric figures and human hands and display sophisticated joint attention skills by the end of their first year. These precocious representations of agents are derived from two innate input analyzers, a face recognizer and an action analyzer. Carey believes that both types of analyzers are equally involved in agency core cognition. I agree but would argue that the weight of these cues is likely to change over time. For example, 5-month-olds demonstrate an overattribution of goals to a self-propelled box (Luo & Baillargeon 2007), whereas a recent study in my laboratory revealed that by 18 months, infants do not attribute reference to a contingent humanoid robot (O'Connell et al. 2009). Furthermore, there is well-established evidence for sophisticated face-processing skills in newborns whereas similar evidence has not been reported for animate-type motion (e.g., contingency) until many months later. More important, there is a dearth of data on infants' association between animate and inanimate motion and object kinds. When do infants know that dogs and people are self-propelled but chairs and spoons are not? It is somewhat troublesome that Carey believes that her hypothesis that representations in this domain are the output of innate input analyzers is not undermined by the late emergence of these analyzers. She argues that performance factors, such as limited executive functions or late maturation, could interfere with the expression of competence as some have argued recently about an implicit form of false belief in infancy (e.g., inhibitory control). Finally, Carey argues that the late emergence of core cognition systems is explained by learning to analyze patterns of contingency and what agents look like. One might object that this makes the innate input analyzer hypothesis nonfalsifiable.

One of the key components of any theory of conceptual change is to explain changes. Carey posits that Quinian bootstrapping is the mechanism by which core cognition blossoms into intuitive theories. By preschool age, and sometimes before, children



have developed theory-embedded conceptual knowledge. These intuitive theories emerge through bootstrapping processes such as those described in the literature on the history and philosophy of science. This corresponds closely to the foundations of Piaget's genetic (or developmental) epistemology. Unfortunately, these explanatory mechanisms boil down to garden-variety learning processes: association, mechanisms that support language learning, among others. Also, although Carey devotes a chapter to the transition from core cognition of number to mathematical representations, how Quinian bootstrapping works in the case of agency or object categories remain to be fleshed out.

In conclusion, I found the toolkit designed by Carey to provide almost all the necessary instructions to build a thinking baby. However, the baby described by Carey is a solitary one, constructing intuitive theories about number and objects in some sort of social vacuum. Indeed, there is little or no space devoted to the role of scaffolding and to cultural learning in early conceptual development.

In *TOOC*, Carey has proposed a theory that has the potential for transforming our understanding of conceptual development. The book also offers a comprehensive review of experimental findings from hers and others' laboratories. This provocative book provides important reading for investigators of early cognitive development as well as for cognitive scientists more generally interested in concepts and the role they play in related mental activities, such as the representation of objects and events, language, and consciousness.

## Rebooting the bootstrap argument: Two puzzles for bootstrap theories of concept development

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Lance J. Rips and Susan J. Hespos

Psychology Department, Northwestern University, Evanston, IL 60208.

rips@northwestern.edu <http://mental.psych.northwestern.edu>

hespos@northwestern.edu

<http://groups.psych.northwestern.edu/infantcognitionlab/sue.html>

**Abstract:** *The Origin of Concepts* sets out an impressive defense of the view that children construct entirely new systems of concepts. We offer here two questions about this theory. First, why doesn't the bootstrapping process provide a pattern for translating between the old and new systems, contradicting their claimed incommensurability? Second, can the bootstrapping process properly distinguish meaning change from belief change?

*The Origin of Concepts* (Carey 2009; henceforth *Origin*) is among the most interesting works in cognitive psychology to appear in decades. It takes central theoretical issues head on – for example, how people acquire notions of number and matter. And to a very impressive degree, Carey has turned ideas about these issues into workable experiments, often with intriguing results. Although we disagree with specific conclusions, her arguments are arresting ones, worth the time to stop and sift. The book's main point is that people learn concepts that are genuinely novel, concepts that can't be spelled out in the vocabulary of their earlier knowledge. The paradox of the book, however, is that the more carefully Carey explains the steps that take children's preexisting concepts to their later ones, the less convincing her case for conceptual discontinuity. In earlier articles, one of us examined Carey's theory of how children acquire concepts of the natural numbers (Rips et al. 2006; 2008). In this commentary, we take a more general look at the thesis that later concepts can be incommensurable with earlier ones.

## 1. Puzzle 1: Computation and translation

According to *Origin*, children shift from earlier to later concepts by learning the latter in terms of the former. Some mental process transforms one into the other, even when the later concepts are new ones, not expressible in terms of the old. In this discontinuous case, Carey calls the process *Quinian bootstrapping*, and it occupies a central focus of the book. Bootstrapping must meet the following requirements: (a) produce new concepts that can't be translated in terms of earlier ones, but (b) do so in a computationally feasible way. Moreover, previously understood concepts must be the input to bootstrapping: Carey rules out the possibility that children could simply introduce an entirely new set of mental tokens, relate them systematically to each other, and thereby provide these tokens with meanings of their own (p. 419). Although “placeholder” tokens – ones that have no antecedent meaning – play an important role in bootstrapping, (c) bootstrapping requires old, already meaningful concepts to ground the placeholders in the final system.

The puzzle is why requirement (b) and (c) don't collectively defeat requirement (a). If bootstrapping takes already understood concepts and combines them computationally to produce new concepts, doesn't that mean that the old and new concepts are intertranslatable by the same computable function? The key process in bootstrapping is a complex nondeductive inference, such as analogy, for which cognitive science has struggled to provide an adequate account. But troubles in formulating the bootstrap's inductive step don't show that the process is computationally impossible, and Carey proposes a computational approach to these inferences. If bootstrapping is computationally possible, though, why can't we use this process to achieve a translation between conceptual systems, contrary to (a)?

One way out of this dilemma is to reject requirement (b) and contend that bootstrapping is not computed, but something children do, for example, as a matter of brute maturation. However, this move would imply that cognitive development is unable to illuminate these interesting cases of concept acquisition. We favor giving up requirement (a), the claim for developmentally incommensurable systems.

## 2. Puzzle 2: Coordinating dual factor semantics

Not every change in inter-concept relations produces a change in meaning. Learning a new fact about daisies – for example, whether they cause hay fever – doesn't change the meaning of “daisy.” Otherwise, we would never be able to change our mind about the same concept, as every change would produce a novel concept (see the critique of Carey in Fodor 1994). This forces Carey to distinguish changes in beliefs that don't alter a concept's meaning from more sweeping changes of belief that do (e.g., in bootstrapping). It's to Carey's credit that she sees the difficulty of separating these, as discussions of this problem are rare in psychology.

To account for the stability of concepts over incidental changes in belief, many semantic theories anchor the meaning of concepts through causal relations to their external referents (e.g., the relation between actual daisies and the mental representation DAISY). These causal relations then remain constant even when a person's beliefs about the concepts change. *Origin* accepts the idea that these external relations are part of what confers a concept's meaning, but adds that its meaning also depends on its conceptual role, that is – on the inferential relations it bears to other concepts.

This dual theory of meaning, however, creates difficulties when conceptual change occurs. Bootstrapping obviously produces radical changes to the inter-concept relations. But in many cases (e.g., concepts of matter), it must also produce new external connections, as otherwise the new system of concepts would

remain tethered to the wrong referents (or to none). The second puzzle about bootstrapping is how it manages to do this.

Carey is attracted to theories by Laurence and Margolis (2002) and Macnamara (1986) in which a concept's internal content mediates its external content. Mental structures – *visual gestalts* in Macnamara or *sustaining mechanisms* in Laurence and Margolis – focus the external causal relations on the designated concept. These causal relations then stick to the concept through future change in beliefs. In *Origin*, however, no boundary separates the mental structure that determines reference from other mental representations. The focusing mechanisms are themselves inferentially connected to the rest of the conceptual system. Therefore, without further constraints on how and when the focusing process occurs, the theory threatens to collapse the two-part semantics to one internal part, thus failing to separate change in meaning from mere change in belief. Carey is forthright in acknowledging not having a fully worked-out semantic theory (p. 523), but we doubt the puzzle can be solved within her framework.

### 3. Coda

*Origin* should be on the reading list of all cognitive psychologists, as it combines striking theories with imaginative experiments. It raises new questions that will generate experiments and insights for years. The two questions we raise here concern whether its bold empirical claims mesh with theoretical requirements. We've offered two examples of this clash in the present commentary.

## Cognitive ethology, over-attribution of agency and focusing abilities as they relate to the origin of concepts

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Carolyn A. Ristau

Department of Psychology, Barnard College of Columbia University, New York, NY 10027.

car31@columbia.edu

**Abstract:** Carey's superb discussion of the origin of concepts is extended into the field of cognitive ethology. I also suggest that agency may be a default mechanism, often leading to over-attribution. The problem therefore becomes one of specifying the conditions in which agency is not attributed. The significance of attentional/focusing abilities on conceptual development is also emphasized.

Carey's insights and thorough explication of the literature on conceptual development (Carey 2009) could be extended into cognitive ethology, generally defined as the study of the mental experiences of non-human animals in their lives in their natural environment (Griffin 1976/1981; 1992/2001).

First, let us note that a preponderance of the experiments cited by Carey as evidence of attributing agency are based upon relative, not absolute, differences in the measures used. This is true in "looking time" and dishabituation experiments, whereby an infant spends more or less time looking or exhibiting degrees of habituation. Therefore, the infant has not indicated that one scenario represents an agent whereas the other does not; rather, the infant appears to be showing more or less of a proclivity to impute intentionality in circumstances in which one event is a more potent and compelling example than the other.

I agree with Carey that agency attribution is innate, but I suggest that it may be a default mechanism and that humans have a tendency to over-attribute intentionality/agency. In the young child, we see this in occasions such as: Eric, burned as

he bumps into the hot radiator, cries, kicks it, and shouts "Bad!" Infants crying, which can secure parental attention, may be promoted not only by the usual reinforcement processes but by the infants' attribution of agency, thus contributing to the infants' control of the parents.

Further extensions may include humans' tendency to seek causes for accidents/events, such as gods, myths, witches (rather than germs), paranoia, or the "other" being a source of personal misery or misfortune. From an evolutionary viewpoint, it may be safer to assume intentionality than not. One is thereby more alert to a potential predator or mate nearby rather than to a being with no such intentions; one may run or become amorous. A gazelle continues grazing when lions are visible but nonattentive, yet is alerted and may run when the lion's gaze becomes focused in the gazelle's direction.

In restricting her analysis to humans and nonhuman primates, Carey notes that "generally" (p. 203) primates do not point and show things to each other, do not establish joint attention. To carefully say "generally" suggests exceptions, as indeed there are. One is immediately led to wonder about the basis for such exceptions and likewise for the restriction in abilities to such exceptions.

Among the exceptions, bonobos are reported to point both in the wild and in captivity. In other instances, animals can simply look in a direction, that is, toward a potential predator, perhaps vocalizing, and individuals follow the gaze and are both alerted and "pointed" in the correct direction.

Exceptions exist beyond primates. Lions have been observed to hunt cooperatively, requiring joint attention to the prey and coordination of their own movements and roles with those of others in the pride.

Although not "joint attention" in the circumstances described by Carey, observations and experiments such as my own with piping plovers, ground nesting birds, do indicate a parent's attention to the direction of locomotion and even attention (gaze) of an intruder toward the plover's nest with eggs (Ristau 1991). In these experiments, there were cues available other than direction of gaze, namely the orientation of the human's face and frontal body. (Human intruders walked at a considerable distance from the nest, scanning either the dunes where the nest was located or oppositely toward the sea. Birds' arousal levels varied from mere head turning to leaving the nest.) Parent birds were more aroused by intruders who gazed toward the dunes/ nest location.

Exceptions also exist to the claim that nonhumans "do not create external public representations of quantifiers, sortals, epistemic states..." (p. 464). Various researchers have found evidence for referential information in animal calls. Vervet monkeys have distinctive calls that appear to refer to types of predators (Martial eagle, leopard, snake). Playbacks of such calls elicited different and appropriate reactions, including looking toward the predator's likely location (e.g., up for eagles, down for snakes) (Struhsaker 1967; Seyfarth et al. 1980). Slobodchikoff's (1991; 2009) studies of prairie dogs provide reasonable evidence for prairie dogs' ability to communicate color, size, and so forth of a human intruder, whereas earlier work by W. J. Smith (1977) revealed the complexity of prairie dog social systems and communication. The creation of new/modified vocalizations has also been reported: macaques for food sites (Green 1985), prairie dogs for various objects (Slobodchikoff et al. 2009), among others. Bonobo chimpanzees communicate quality of food encountered (Clay & Zuberbühler 2009).

Perhaps some capacities that may be operable in various "exceptions" are attentional and focusing abilities. I hesitate to term them "mere" performance factors, because they appear to vary so substantially between individuals, human and otherwise, and would seem significant in determining the level of competence that individuals/species can achieve. One example was the ability of a young female chimpanzee, Daisy, in the group I observed at the Afi Mountain Wildlife Preserve in Cross River State, Nigeria, to remain impervious to distraction. Others

would tempt her to play, but if she was intently digging a hole with her stick or preparing a stick for some use, she ignored their overtures. She was however, a social individual, playing with those same juveniles on other occasions. Most of the other chimpanzees were much more easily dissuaded from any task by social opportunities.

One expects that more focused attention can more readily lead to determining the agent or target from among an array of stimuli. Attentional/focusing abilities may well greatly affect the actual use of concepts and the attained level of conceptual abilities.

Even the ability to imitate, which colloquial term researchers refine into numerous “sub” attributes, can be influenced by attentional abilities as the focused imitator is able to attend to the goal of an action, and not be distracted by activity per se. A case in point: a young child wishing to imitate Mommy is given a small broom and proceeds to “sweep,” irrespective of the dirt.

This is but a partial list of “exceptions,” all requiring closer analysis of conceptual content.

## Oculomotor skill supports the development of object representations

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Matthew Schlesinger<sup>a</sup> and Dima Amso<sup>b</sup>

<sup>a</sup>Department of Psychology, Southern Illinois University Carbondale, Carbondale, IL 62901; <sup>b</sup>Department of Cognitive, Linguistic, & Psychological Sciences, Brown University, Providence, RI 02906.

matthews@siu.edu <http://matthew.siu.edu>  
dima\_ams@brown.edu

**Abstract:** Are infants’ initial object representations innately specified? We examine the development of perceptual completion in infants by highlighting two issues. First, perceptual completion is supported by neural mechanisms that rely on experience with the environment. Second, we present behavioral and modeling data that demonstrate how perceptual completion can emerge as a consequence of changes in visual attention and oculomotor skill.

In *The Origin of Concepts*, Susan Carey (2009) explores the idea that a small set of fundamental concepts – including objects, number, agency, and causality – are innately specified as core cognition within the human species. Carey addresses the issue by carefully describing and evaluating an impressive array of empirical studies that span from infancy through childhood.

There are numerous facets of Carey’s argument that not only contribute to the nature–nurture debate in a constructive way, but also illuminate areas of the debate where polarized dichotomies tend to prevail. A particularly valuable strategy is the use of analogies from ethology (e.g., how indigo buntings learn to exploit the North Star as a spatial cue), which create an intuitive anchor for elusive terms such as *sensory*, *perceptual*, and *conceptual representation*. In addition, Carey makes excellent use of comparative data, both within and across species. Specifically, Carey highlights behavioral “signatures” or profiles that provide a qualitative basis for comparing organisms systematically. We enthusiastically endorse the approach, and believe that the comparative analyses that Carey describes will have a positive impact on the field of developmental science.

Whereas much of the story that Carey presents is persuasive, there are a few places in her argument where important pieces of empirical evidence seem to be overlooked. To illustrate, we use the example of the development of core object cognition.

As a specific case study of innate object representation, Carey discusses the phenomenon of *perceptual completion*, which is the capacity to perceive partially-occluded objects as integrated wholes. An essential component of the capacity is a *fill-in mechanism* that “reconstructs” (i.e., infers the presence of) occluded

edges or surfaces of objects that are partially occluded. In proposing that perceptual completion is innate, Carey raises the challenge: “What learning process could create representations of complete objects that persist behind barriers taking only perceptual primitives as input?” (p. 59). We provide an outline here of a learning process that is well-suited to address this question.

### 1. Perceptual fill-in is supported by neural computations in visual cortex

In their investigation of contour perception in macaques, Peterhans and von der Heydt (1989) identify a unique class of neurons in visual cortex. These neurons respond optimally to edges in a particular orientation that move in a specific direction. Figure 1A illustrates how four different sets of these cells, each tuned to a different line orientation (e.g., 0°, 45°, 90°, and 135°, respectively) respond to a moving bar (“Visual Stimulus”). In this diagram, darker circles represent neurons with higher firing rates. Thus, note that the set of cells tuned to 135° responds at a high level, while the firing rates in the cells tuned to other orientations are proportionally lower.

An important property of these neurons is that they also respond to partially-occluded objects. In particular, note in Figure 1B that activation spreads in the set of neurons tuned to 135° from those that are stimulated by the visible portions of the moving bar, to neighboring neurons that have no direct visual input.

### 2. The neural substrate that supports perceptual fill-in develops during infancy

A mechanism that can help explain the spreading of activation is the growth of horizontal connections between neurons in visual cortex (e.g., Albright & Stoner 2002). Whereas these connections initially rely on endogenous input, their subsequent growth is experience-dependent and occurs in the weeks after birth (e.g., Ruthazer & Stryker 1996).

What role does visual activity play in the development of this neural substrate? We have hypothesized that oculomotor skill, and in particular the development of visual selective attention, is a critical ability that makes possible optimal information pick-up. In other words, we are proposing that progressive improvements in visual scanning ability provide the input into and help to drive the development of the perceptual fill-in mechanism. Therefore:

### 3. Perceptual completion in human infants is associated with the development of oculomotor skill

A series of perceptual-completion studies with 3-month-olds demonstrates that infants who have achieved the capacity for perceptual completion are more effective at deploying their attention than infants who have not yet reached the same milestone (e.g., Johnson et al. 2004; Amso & Johnson 2006). This difference between 3-month-olds in oculomotor skill is not limited to displays such as Figure 1A and 1B, but is also found on other measures of visual selective attention (e.g., visual search).

However, these findings do not specify the direction of developmental influence. Therefore, it may be the case that the onset of perceptual completion leads to improvement in oculomotor skill (i.e., a priori knowledge of objects leads to improvements in deploying attention). In order to address this issue, we have designed and tested an eye-movement model that simulates the development of oculomotor skill in infants.



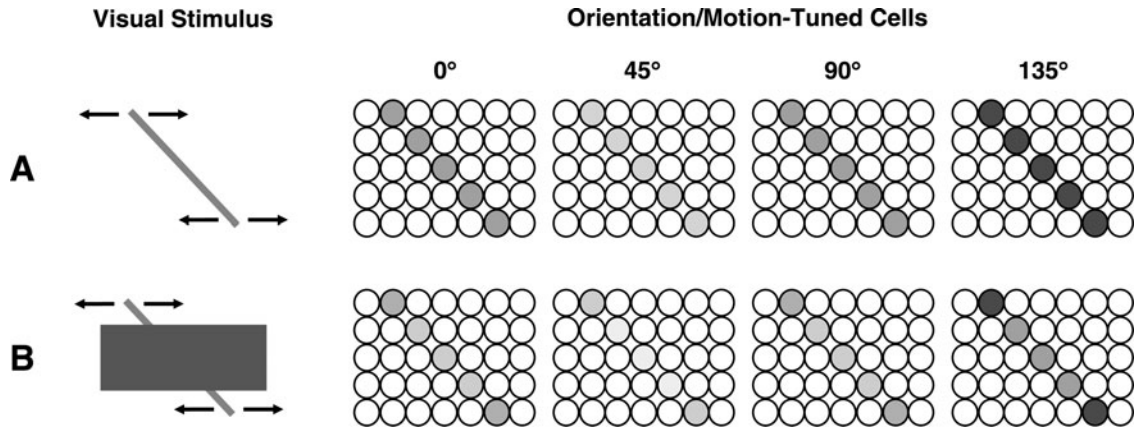


Figure 1 (Schlesinger et al.). Schematic diagram of a population of neurons that responds to partially occluded objects. The visual stimulus (a moving bar) is illustrated on the left, and the corresponding pattern of neural activity in four sets of orientation- and motion-specific cells is presented on the right. Darker circles represent higher firing rates. (A) A completely visible stimulus and (B) a partially occluded stimulus.

#### 4. Growth of the neural substrate that supports visual attention leads to developmental changes in perceptual completion

Our model, which is inspired by the structure and function of the mammalian visual system, includes a component that represents activity in the parietal cortex, an area of the brain that supports visual attention (e.g., Gottlieb et al. 1998). A key finding is that systematic changes in this component of the model result in corresponding improvements in perceptual completion (Schlesinger et al. 2007a; 2007b). Therefore, the model illustrates a plausible developmental pathway: as infants develop the ability to scan the visual world effectively and efficiently, they acquire a skill that provides necessary input into the neural system that learns to compute perceptual fill-in.

Whereas there is considerable overlap between our account and the one provided by Carey (e.g., the orientation- and motion-specific cells illustrated in Figure 1 resemble Carey's *innate perceptual analyzers*), there are two important issues in our account that should be emphasized. First, the development of the fill-in mechanism – a basic form of object representation – may not be innately specified, but is instead a product of multiple interactions between biology and environment. Second, active exploration is an essential ingredient: infants encode and represent the world in more complex ways through advances in sensorimotor skill.

#### Acquiring a new concept is not explicable-by-content

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Nicholas Shea

Faculty of Philosophy and Somerville College, Philosophy Centre, University of Oxford, Oxford OX1 4JJ, United Kingdom.

nicholas.shea@philosophy.ox.ac.uk

[http://www.philosophy.ox.ac.uk/members/nicholas\\_shea](http://www.philosophy.ox.ac.uk/members/nicholas_shea)

**Abstract:** Carey's book describes many cases in which children develop new concepts with expressive power that could not be constructed out of their input. How does she side-step Fodor's paradox of radical concept nativism? I suggest that it is by rejecting the tacit assumption that psychology can only explain concept acquisition when it occurs by rational inference or other transitions that are explicable-by-content.

Representational explanation is central to psychology. Mental processes are characterised in terms of causal transitions between token states, where we make sense of the transitions in terms of the content of the states. Can we explain concept acquisition in the same way? Only insofar as the acquired concept is constructed out of pre-existing concepts, according to Fodor (1975; 1981). As most lexical concepts do not seem to be so-constructed, Fodor concludes that they are innate – their acquisition is outside the ambit of psychological explanation.

Carey's book is a comprehensive refutation of radical concept nativism, offering many psychological explanations of concept acquisition, and the data to back them up (Carey 2009). How, then, does Carey side-step Fodor's argument? I want to suggest that she has to reject the assumption that all psychological explanations are explanations-in-virtue-of-content. Consider two of the stages in Carey's account of the acquisition of number concepts.

First consider the transition from parallel individuation to enriched parallel individuation (being a one-knower, two-knower, etc.). Numerosity is not represented explicitly anywhere in the parallel individuation system. It is implicit in the various operations that are performed on object files: adding, subtracting, and comparing by one-to-one correspondence. The child then comes to associate words with object files of a certain size, for example, "one" with having one object file of any kind open: {i}. Is this step explicable-by-content?

Before becoming a one-knower, the child was not representing one-ness explicitly at all: "one" was just a sound, and numerosity was merely implicit in the object file system. The child did not have resources out of which a hypothesis about one-ness could be constructed. But there were two important correlations that they could make use of: (1) between having one object file {i} open and singleton sets; and (2) between the word "one" and singleton sets (the mechanism for which involves the child's linguistic community). Although neither is a representation of one-ness, these are two pieces of *information*, of the purely correlational type (e.g., Shannon information). As the two mental items correlate with the same external-world property, they tend to occur together, and so they become associated. The association between "one" and {i} constitutes a new symbol. It explicitly represents the numerosity *one* (i.e., that is its wide content).

I would argue that that transition has not been explained-by-content. Instead, it is a transition to an entirely new representation, explained in terms of the correlational information carried by its precursors. One of those precursors ("one") was not representational at all and the other ({i}) was not made use

of for its content, but just for its informational connections. The transition to the new symbol (“one”/i) was no sort of inference or other rational transition from those pre-existing resources. Nevertheless, Carey has offered a recognisably psychological explanation. In my view, this shows that there can be psychological explanations in terms of correlational information that are not explanations-by-content.

For a second example, consider the transition from enriched parallel individuation to being a cardinal principle knower. This step involves Quinean bootstrapping, the process whereby a set of uninterpreted symbols interrelated by a network of inferential dispositions are connected up to the world so as to acquire a meaning. The child’s key resource here is the uninterpreted list of counting words: “one”, “two”, . . . . At the stage of enriched parallel individuation, the early words in this sequence have already been put into correlations with object files (“one”/i), hence numerosities (one-ness). To give the symbols from “five” onwards their content, according to Carey, the child generalises across three transitions in which moving to the next count word corresponds to adding one to the object file:

“one”/i → “two”/i j  
 “two”/i j → “three”/i j k  
 “three”/i j k → “four”/i j k l

The child makes a leap which generalises across the instances of adding one – by associating them all as instances of counting on. When the child does so, “five” is put into a content-constituting informational connection with sets of five things – kept track of as the successor to four, which the child can track directly with the enriched object file system using its symbol “four”/i j k l (similarly for “six”, . . .).

Is this transition explicable-by-content? Carey rightly rejects the idea that it is properly described as hypothesis testing. The child is doing something different: building an uninterpreted model with the counting words, and then giving that model an interpretation. The child does not test a hypothesis (a statement formulable with the child’s existing representational resources). Rather, the child comes to associate two previously correlated operations (counting on and adding one). By doing so, the child acquires the concept of successor (generalising over instances of adding one) and concepts of all the numbers for which the child has count words. Again, Carey has described a psychological process that depends upon correlational information but is not explicable as a rational inference or other transition-in-virtue-of-content.

In some places Carey suggests that Quinean bootstrapping is explicable-by-content, because even before the transition the uninterpreted placeholders have narrow contents in virtue of their inferential roles (p. 522). Carey has some good arguments for the existence of narrow content, but not in the case of dispositions to make transitions between uninterpreted symbols. The transitions are described in terms of connections between symbol types, where the symbols can only be individuated non-semanticly. Once the symbols are put in the right relations to acquire wide contents, then we can use inferential dispositions between concepts to characterise a second level of narrow content. However, on pain of regress or holism, the inferences that make up the narrow content of a concept should be individuated in terms of the wide contents of the concepts which figure in those inferences. Therefore, narrow contents cannot save explanation-by-content.

With hindsight, it is obvious that if we assume that psychological explanation is restricted to explanation-by-content, then psychology is going to have a problem explaining the acquisition of genuinely new representational resources, because the required contents would have to be available before the transition. Carey’s book gives us compelling reasons for relinquishing that assumption.

## Quinian bootstrapping or Fodorian combination? Core and constructed knowledge of number

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Elizabeth S. Spelke

Department of Psychology, Harvard University, Cambridge, MA 02138.  
 spelke@wjh.harvard.edu

**Abstract:** According to Carey (2009), humans construct new concepts by abstracting structural relations among sets of partly unspecified symbols, and then analogically mapping those symbol structures onto the target domain. Using the development of integer concepts as an example, I give reasons to doubt this account and to consider other ways in which language and symbol learning foster conceptual development.

In a masterful treatise, Carey (2009) argues that Quinian bootstrapping drives human conceptual development by building on innate domain-specific concepts, domain-general concepts and reasoning processes (especially logical operators and analogical reasoning), and capacities to learn “placeholder” symbols and their structural relations. She proposes, in particular, that preschool children construct the system of positive integer concepts in three steps. First they learn an ordered list of meaningless number words. Then they map the first four words to sets of 1–4 numerically distinct individuals, drawing on representations from two innate but highly limited systems that together allow for “enriched parallel individuation” (EPI). Finally, they make an analogy between the sequential structure in the counting list and the numerical structure of EPI. Armed with this analogy, and with the ordering of words in the count list, children bootstrap their way over the set-size limit of EPI (3 around 4 individuals) and construct the integers.

Conspicuously absent from this account is a well-studied core system of numerical representation: the approximate number system (ANS) (Dehaene 2009). Carey discusses this system, by which animals and humans of all ages, including newborn infants (Izard et al. 2009), represent the cardinal values of arrays of objects and events with ratio-limited precision. She argues, however, that ANS representations play no role in the construction of integer concepts, even though they later become associated with those concepts and enhance their use. This argument is crucial for Carey’s bootstrapping account. If young children mapped number words both to (precise but set-size limited) representations of numerically distinct individuals and to (noisy but essentially unbounded) representations of cardinal values, they would not need the ordinal list structure, or analogical reasoning, to overcome the EPI limit of four. Instead, children could learn that “one” designates a set with a cardinal value around 1 (as given by the ANS) consisting of a single individual (as given by EPI), and that “two” and “three” designate sets with cardinal values around 2 and 3, composed by adding individuals to a set of one. Without regard to the order of these words, children could discover how these ANS and EPI representations relate to one another: progressively larger cardinal values result from progressive addition of one.

Carey presents compelling arguments that ANS representations are not the sole basis of children’s learning of number words and counting, but several considerations suggest that they contribute to this process. First, children who have recently mastered counting recruit ANS representations to solve novel tasks involving number words and symbols: they perform approximate symbolic addition and place symbolic numbers on a line in ways that reveal critical signatures of the ANS (Gillmore et al. 2007; Siegler & Opfer 2003). Second, when pre-counting children who already know number words to “three” are taught “four,” they first map the word to an ANS representation, showing more false alarms to nearby numerosities (such as five) than to distant ones (Huang et al. 2010). Third, some adults with impairments to the ANS but normal number word

comprehension and verbal counting have trouble determining whether “nine” denotes a larger quantity than “seven” (e.g., Dehaene & Cohen 1997). If the ordinal structure of the count list provided critical information about numerical order independently of the ANS, then impairments to the ANS should not obscure this order.

This observation suggests a final reason to question Carey’s bootstrapping theory of the development of integer concepts. Although children learn to recite ordered lists of meaningless words, and “one, two, three...” may be an example, it does not follow that children or adults can access and use the ordinal structure of such a list. Adults learn songs and poems without accessing this structure (if you can recite the United States national anthem, then consider which word comes first, “stripes” or “gleaming”? I can only answer this question by rattling off the song.) Early counting-based arithmetic strategies suggest that children who have mastered counting initially fail to access the ordinal positions of the words in their count list. A child who knows that “four” denotes the fourth word on the list should add  $4 + 3$  by starting with “four” and counting on. When children first use counting to solve such problems, however, more children start the count with “one” (Siegler & Jenkins 1989).

I suggest that Quinian bootstrapping – learning symbols as placeholders, deciphering the structural relations among them, and then using analogical reasoning to map those relations onto other conceptual domains – is probably not the source of integer concepts. Nevertheless, Carey reviews rich evidence that these concepts depend in some way on mastery of verbal counting. How else might language, and other symbol systems, support cognitive development in this domain and others? First, language may provide efficient ways to express and use concepts that children already possess (Frank et al. 2008). Second, words and other symbols may help learners to select, from among the myriad concepts at their disposal, those that are most useful or relevant in some context (Csibra & Gergely 2009; Waxman & Markow 1995). Third, language may serve as a medium in which information from distinct, domain-specific cognitive systems can be productively combined (Spelke 2003b). Learning the meanings of words like “two” and “three” may be useful to children, because the meanings of these terms combine information from distinct cognitive systems. Pre-linguistic infants and nonlinguistic animals possess these systems but may lack the means to combine their outputs flexibly and productively (Spelke 2003a). On any of these views, new concepts would arise from processes that repackage, select, or combine preexisting concepts, as envisioned by Fodor (1975), rather than from the constructive processes that Carey develops from Quine’s metaphors. Carey’s bold and rigorously argued case for Quinian bootstrapping sets a high standard for theories and research addressing this family of Fodorian conjectures.

## Language and mechanisms of concept learning

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Daniel A. Weiskopf

Department of Philosophy, Georgia State University, Atlanta, GA 30302.  
dweiskopf@gsu.edu <http://www2.gsu.edu/~phldaw/>

**Abstract:** Carey focuses her attention on a mechanism of concept learning called “Quinian bootstrapping.” I argue that this form of bootstrapping is not dependent upon language or other public representations, and outline a place for language in concept learning generally. Language, perception, and causal reasoning are all sources of evidence that can guide learners toward discovering new and potentially useful categories.

Two central questions for theories of concepts are: first, what is the nature of the developmentally primitive conceptual basis that humans are endowed with; and, second, what sorts of mechanisms are available for expanding this basis to capture the adult conceptual repertoire. I will focus on Carey’s answer to the second question (Carey 2009), which centers on a “Quinian bootstrapping” mechanism for concept learning.

The goal of bootstrapping is to arrive at a new set of primitive concepts that are incommensurable with the ones the learner now possesses; that is, whose content cannot be captured in terms of any of the concepts possessed initially. The first stage of bootstrapping occurs when a learner encounters a set of inter-related explicit public symbols, such as the sentences that compose a scientific theory or the formal notation of mathematics (p. 306). These public symbols are not initially mapped onto any already existing concepts. Rather, they are uninterpreted (or partially interpreted), hence largely meaningless to the learner. These placeholders are then taken up by various “modeling processes”: abstract forms of theoretical inference such as abduction, induction, and analogical reasoning that provide them with their content. Eventually, these symbols come to have conceptual content in virtue of acquiring a stable conceptual role in a new theoretical structure (p. 418).

From this account, it appears that Quinian bootstrapping requires language or another external representational medium. These give the vehicles that one learns to manipulate and that become endowed with content by the end of the process. But it is not clear why these representational media are essential. It is true that conceptual change is often driven by learning a theory in some social context, such as the lab or the classroom. Therefore, external symbols are necessary to convey the theory to new learners. Grasping the theory itself, however, involves constructing mental representations corresponding to its new theoretical terms and the propositions that they participate in. This process seems independent of language.

For example, Carey notes that in the case of Kepler’s explanation for why the planets revolve around the sun, he initially entertained the hypothesis that “something in the sun causes planets to move” (p. 427), which contains a linguistic placeholder structure. This structure was given various labels by Kepler (“*anima motrix*,” “*vis motrix*”), but when introduced, it was just as a thing, whatever it might be, that produces the motion of the planets. Introducing this new concept involves only hypothesizing the existence of a certain type of entity. Therefore, in logical terms, this placeholder is just the expression of a bound variable of the form “ $\exists x$ .” If central cognition has this minimal quantificational apparatus, then the equivalent thought containing a placeholder representation should be formable as well. Carey’s own discussion of the emergence of quantification gives convincing reasons to think that it does (pp. 254–263). In addition, there is reason to think that creating new primitive representations occurs in other cognitive domains as well, such as perceptual categorization (Schyns et al. 1998).

If the actual act of coining a new placeholder representation is not language-dependent, what is the role of language in concept learning? Probably there is no one single role that it plays, but a major one is that it serves as a signal to the child of the presence of an important type of thing in the environment. As Carey notes, linguistic labels are treated as special in a way that other conventional and natural signs are not (Xu 2002). But as a category indicator, language is not unique. The presence of a category of interest can be signaled in many ways. Perception is one: objects presenting a surprising or novel appearance, such as one’s first coelacanth or kangaroo, may also belong to new categories. In addition, causal powers and relations are signals of the presence of interesting categories. If there is a set of phenomena that display the signature of belonging to a common system of causal relations, then there is reason to posit some underlying – but unperceived – cause tying them together. That children attend to such factors is attested by the range of essentialist reasoning



that they display (Gelman 2003). It is also attested in more mundane ways by their use of causal properties to classify objects (Gopnik & Sobel 2000; Nazzi & Gopnik 2003). There are undoubtedly other sources of evidence that can be brought to bear, but perception, language, and causation are *prima facie* reliable (if sometimes conflicting) indicators of categories. And coining new concepts is performed in response to the detection of categories that are likely to prove useful for social, practical, or theoretical purposes.

Learning a new concept for a kind, property, substance, event, or individual may be a piecemeal or *atomistic* affair; think here of adding a new animal or food concept to one's repertoire. Quinian bootstrapping as Carey describes it often involves acquiring a set of interrelated concepts, such as the rational numbers or the adult's weight/density concepts (p. 370). It is thus a *locally holistic* process. The relationship between the two processes is that coining concepts for new categories is an essential prerequisite to building larger knowledge structures that include them. Kepler needed to coin a concept for the force emitted by the sun in order to hypothesize about its nature. Röntgen needed to coin a concept of X-rays in order to describe how they produced their characteristic effects. We need to conceptualize a new species as a distinct grouping in order to begin theorizing about its ethology, evolutionary history, and so on. To engage in Quinian bootstrapping, one needs theories, even local ones, and theories need theoretical concepts. Coining is precisely an atomistic process that can produce these concepts (Weiskopf 2008).

Carey holds that Quinian bootstrapping is one mechanism among many for producing conceptual change and discontinuity, albeit an important one. This seems correct. At a general level, I suggest that we see the capacity to coin new mental representations in response to an open-ended range of evidence and epistemic conditions as the common capacity that underlies much of our concept learning. This mechanism is the bottleneck through which many other pathways to concept learning flow, including many of those involved in Quinian bootstrapping, such as differentiation and coalescence. The capacity for creating new representations that is involved in these processes is one that may use language as one cue among many, but which is not in and of itself dependent upon language.

## Rational constructivism, statistical inference, and core cognition

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Fei Xu

Department of Psychology, University of California, Berkeley, CA 94720.

fei\_xu@berkeley.edu <http://babylab.berkeley.edu/>

**Abstract:** I make two points in this commentary on Carey (2009). First, it may be too soon to conclude that core cognition is innate. Recent advances in computational cognitive science and developmental psychology suggest possible mechanisms for developing inductive biases. Second, there is another possible answer to Fodor's challenge – if concepts are merely mental tokens, then cognitive scientists should spend their time on developing a theory of belief fixation instead.

Susan Carey's book, *The origin of concepts* (2009), is deep, comprehensive, and provocative. She articulates a view of the starting point of the human infant's conceptual apparatus and its subsequent development through middle childhood. Carey reviews much of her enormously influential research from the last few decades. She also reviews much of the literature on core cognition that focuses on revealing early competences in human infants in a number of core domains such as object cognition, number sense, causality, and agency. Carey takes on both the British empiricists' and Piaget's theory of development – on both counts, I

completely agree with her. Carey also takes on Fodor's challenge to cognitive science about learning, by discussing in detail her profound and groundbreaking work on conceptual change in childhood. I make two points in this commentary. (1) It may be too soon to conclude that core cognition is innate. Recent advances in computational cognitive science suggest possible mechanisms for developing inductive biases in a rational manner; new empirical work is also beginning to uncover the existence of these mechanisms in infants. As such, perhaps a different approach, a "rational constructivist" approach to cognitive development, is called for. (2) There is another possible answer to Fodor's challenge: that Fodor was wrong about what is interesting for psychologists to study – concepts or belief fixation. Belief fixation is an interesting and legitimate research enterprise. If concepts are merely mental tokens (fixed by a mysterious "nomological hookup" process, as per Fodor 1998), cognitive scientists and developmental psychologists should spend their time and energy on developing a theory of belief fixation. Bayesian belief updating may provide a framework that is potentially productive and fruitful in this regard.

The evidence for infants' early cognitive competences from the last 30 years of research is staggering. Despite many open issues about the format of the early representations and various methodological quibbles, there seems to be little doubt that the human infant is a completely different kind of creature from what Piaget or Quine or William James had thought. The first year of life, in spite of the fact that infants are still motorically and articulatorily incompetent, is in fact wonderfully rich in intellectual content. But is core cognition innate just because of the early appearance of these rather sophisticated reasoning abilities? Such a claim is based on the dissatisfaction of associationist accounts of learning in infants and children, that is, associative learning mechanisms do not appear to be able to explain the concepts and knowledge acquired by infants in the first year of life. However, there exist inductive learning mechanisms that may meet this challenge. Recent work in computational cognitive science and developmental psychology (e.g., Dewar & Xu 2010; Gopnik et al. 2004; Kemp et al. 2007; Schulz et al. 2007; Xu & Garcia 2008; Xu & Tenenbaum 2007) puts forward a proposal based upon principles of Bayesian inference. In various domains (e.g., causal learning, probabilistic reasoning, word learning, social cognition), empirical work now provides evidence for these conjectures. Instead of embracing a strong nativist view of early development, perhaps a different approach to development – a rational constructivist approach – is called for (Xu 2007; Xu et al. 2009). These inductive learning mechanisms are likely to be domain-general (e.g., Gweon et al. 2010), and they may provide the foundation for rapid learning in infants.

Fodor famously challenged Piaget in the Piaget–Chomsky debate in 1980 about what it means to be learning something genuinely new. Carey takes on this challenge by pointing to both the existence of conceptual change in childhood – where children acquire new concepts (e.g., the concept of 7, or the concepts of weight and density) and by providing a learning mechanism, namely Quinian bootstrapping. Here I suggest another possible answer to Fodor, namely, that if concept acquisition amounts to some mysterious process of "nomological hookup" (Fodor, 1998), then we should focus on developing a theory of belief fixation instead – because that is where the interesting psychological work is! Again, recent work on rational models of cognition (e.g., Chater & Oaksford 2008; Tenenbaum et al. 2006) provides a new framework for asking and answering questions about how people update their beliefs, that is change the probabilities they assign to different beliefs in light of data. Furthermore, within this new framework, not only do we ask questions about the probabilistic nature of inferences, but we also ask questions about whether human learners, big and small, represent probability distributions and to what degree our knowledge and representations themselves are probabilistic in nature (e.g., Vul & Pashler 2008). The emphasis on uncertainty – both

in representations and inferences – departs from the Fodorian view of concepts and beliefs as well. This burgeoning research enterprise focuses on a theory of belief fixation and belief updating, and it has already generated much innovative empirical work with both adults and children.

## Authors' Response

### Concept innateness, concept continuity, and bootstrapping

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Susan Carey

Department of Psychology, Harvard University, Cambridge, MA 02138.

scarey@wjh.harvard.edu

**Abstract:** The commentators raised issues relevant to all three important theses of *The Origin of Concepts* (henceforth *TOOC*). Some questioned the very existence of innate representational primitives, and others questioned my claims about their richness and whether they should be thought of as concepts. Some questioned the existence of conceptual discontinuity in the course of knowledge acquisition and others argued that discontinuity is much more common than was portrayed in *TOOC*. Some raised issues with my characterization of Quinian bootstrapping, and others questioned the dual factor theory of concepts motivated by my picture of conceptual development.

I am deeply moved by 29 thoughtful commentaries provided by 41 colleagues, both philosophers and psychologists. Following the organization of the précis, my responses begin with preliminary issues, turn next to innateness and core cognition, and then to the issue of conceptual discontinuity and my proposal for the bootstrapping process that underlies discontinuities in conceptual development. I end with a general discussion of the nature of concepts and conceptual development.

#### R1. Preliminary remarks

*TOOC* concerns mental representations – how they arise and how they come to have the meanings they do. The book is organized around case studies, such as representations of number and agency in childhood, or heat and temperature in the history of science. As commentator **Markman** points out, this approach typifies research on conceptual development at least since Piaget, and differs from much research on the cognitive psychology of adults' concepts, which tends to address conceptual representations in the abstract, independent of content. Like Markman, I believe that the psychology of concepts must follow the lead of the psychology of perception. The study of perception has long proceeded at both levels of abstraction in parallel. That is, scientists interested in perception have studied both particular cases, such as depth perception, motion perception, color perception, and taste perception, as well as seeking to illuminate how sensation and perception work in general.

Similarly, work on the nature of concepts in general and how human beings represent particular important concepts are mutually illuminating.

My commentators sometimes lost track of what my project in *TOOC* was – namely explaining how representations arise and how they come to have the meanings that they have. For example, commentators **McLaren, Wills, & Graham (McLaren et al.)** discuss the pattern-encoding mechanism that extracts an orientation-specific prototype of stimuli, such as faces that share a common configuration (e.g. Diamond & Carey 1986; Gauthier & Tarr 2002; McLaren 1997). They ask why I didn't include this work as an excellent example of core cognition, asking how it differs from the mechanism through which indigo buntings learn to identify the North Star. The reason is simple: Although this mechanism plays a role in creating an iconic representation of prototypical faces (or prototypical dogs, Greebles, or meaningless checkerboard patterns), and these iconic representations in turn support better discrimination among members of those categories, the question in *TOOC* is: How do these representations come to have the meanings that they do? That this mechanism works for meaningless checkerboard patterns shows just why it isn't a system of core cognition – there is no innately supported meaning assigned to the representations that are its output. What distinguishes this computational system from the mechanism through which indigo buntings identify the axis of rotation of the night sky is that the output of the latter computation has an innate conceptual role. So too do the representations of agents, objects, and number that articulate systems of core cognition.

#### R2. Innateness of representational primitives

Several commentators were not convinced that there are *any* innate representational primitives, whereas others argued for leaner ones than those suggested in *TOOC*. Let us take these two issues in turn.

##### R2.1. Distinguishing learning from development

The argument for innate representations that I offer in my book concerns theories of *learning*, not theories of *development* in general. The argument is simple, and I would think, uncontroversial. Learning is a computational process that operates on representations, and therefore if we believe that learning plays a role in the construction of representational resources, we are committed to there being *some* innate representations: those that are the input to the initial episodes of learning. Commentators **Schlesinger & Amso** critique the arguments in *TOOC* for innate object representations on the grounds that success on some of the tasks that provide evidence for them is dependent upon experience. Remember, I am using “innate” to mean “not learned.” They, instead, take “innate” to contrast with “experience-dependent,” and therefore fail to separate learning from development more generally. We do not learn to get taller, although physical growth is certainly experience-dependent (e.g., we need to eat and move). Some changes in neural connectivity implement learning processes and some do not. Hence it is perfectly compatible with my arguments for

nativism that these mechanisms are to some extent experience dependence.

Commentators **Allen & Bickhard** critique my argument for some innate representational primitives by pointing out that there must be processes that underlie the acquisition of some representations that do not themselves begin with representations. They ask, whatever these processes are, could they not underlie the developmental changes that we see in infants and toddlers? It is certainly likely that some of the developmental changes we see very early in childhood are maturationally driven changes (i.e., not underlain by learning), and indeed I discuss such changes in *TOOC*. But noting that there are some representations whose origin is not learning is a way of acknowledging innate representations, not denying them.

### R2.2. Keeping one's eye on the ball: Accounting for specific conceptual content

The work described by **Schlesinger & Amso** provides the field with an important challenge, namely, untangling domain-general developmental changes in visual search capacities from the acquisition of specific representational schemas (e.g., of objects). Schlesinger & Amso suggest that developmental changes in visual selective attention, oculomotor skill, visual scanning, and active exploration might explain developmental changes in the capacity for object completion over the first few months of life, and I agree. In *TOOC* (p. 58) I suggest that the development of visual selective attention might drive the process through which the input to innate computational mechanisms becomes available to the child. Schlesinger & Amso suggest instead that these domain-general changes *explain* the acquisition of object representations. But Schlesinger & Amso do not attempt to specify how representations of objects could be constructed from other primitives, such as sensorimotor ones. Absent such an account, they have not offered an alternative to the hypothesis that object representations are innate.

Both **Xu** and **Gopnik** clearly focus on learning, and suggest that the current exciting work on inductive learning in the Bayesian tradition offers a suggestion for how the structures I posit as *innate* core cognition might instead be *learned*. However, these models have not yet been applied to explaining any domain of core cognition. Therefore, this suggestion is little more than a guess that learning formulated over leaner primitives *might* yield the concepts of agent, number, and object that constitute the case studies in *TOOC*.

As **Xu** points out, we need not accept that core cognition is innate solely on the basis of evidence for early emergence of sophisticated reasoning abilities. Of course not; there are four further arguments for the innateness of the representations in core cognition systems (see Précis, sect. 3.3).

I agree with **Xu** that constraints on induction can be, and most typically are, learned. She might have offered her own exciting work on infants' learning of over-hypotheses as a worked example (Dewar & Xu 2010; in press). But an over-hypothesis such as "the objects in each bag are the same color" or "the objects in each bag are the same shape" requires representations of *object*, *bag*, *color*, and *shape* to be formulated. More generally, Xu advocates abandoning the project of explaining the

origin of concepts in favor of focusing on the project of understanding the processes of belief fixation. These are both important projects in cognitive science; we do not have the option of choosing between them. Belief-representations are composed of concept-representations. To decide among the hypotheses that "blicket" means *animal*, *dog*, or *dalmation*, (Xu & Tenenbaum, 2007) one must have these latter three concepts available. *TOOC* is concerned with how concepts arise.

**Gopnik** and I agree in endorsing the theory-theory of conceptual development, but we emphasize different lessons from the history of science for the understanding of the human mind. Gopnik, like **Xu**, emphasizes the processes that underlie belief fixation (learning from evidence), whereas I emphasize the processes that result in new representational primitives that articulate abstract conceptual structure previously unrepresented. The examples in Gopnik's commentary do not address the problem I have taken on. That statistical evidence is relevant to inferring desires, or to making novel inferences in chemistry and physics, offers no explanation for the capacity to represent desires at all, or to articulate the inferences in chemistry and physics at all.

**Gopnik** makes an important point: Quinian bootstrapping is not the only process that results in the creation of new concepts. I agree that the mechanisms in which hidden variables are posited in the course of Bayesian causal learning also may do so. It would be very interesting indeed to show that concepts such as *goal*, *belief*, *living thing*, *heat*, *object*, or *integer* emerge through such a process. I am placing my bets on rich innate primitives and on Quinian bootstrapping, not as a hunch, but on the basis of the analyses and evidence offered in *TOOC*.

### R2.3. Leaner innate primitives

Commentator **Mandler** accepts the challenge of specifying what leaner primitives than those argued for in *TOOC* might look like, and what the learning process that creates richer representational resources from them might be. The primitives Mandler posits are spatiotemporal; she calls the learning process "perceptual meaning analysis." Mandler says: "A single innate analyzer (such as Perceptual Meaning Analysis) collapses attended spatiotemporal information into a small number of conceptual primitives." The problem is this: Mandler does not explain how concepts of objects, agency, and causality arise from such a process. Perceptual analysis could certainly abstract the schema in common to Michottian launching, for example, or equifinal approaches by different paths to a common endpoint. And such schemata could be represented iconically. The question is, what would make these schemata representations of *cause*, (or *make move*, in Mandler's words), or *goal-directed action*, respectively? Where does the conceptual meaning come from?

*TOOC* provides an answer concerning the source of meaning: conceptual role, including innate conceptual role. It also provides an empirical argument against the suggestion that the perceptual representations through which entities in core cognition are identified are the result of perceptual meaning analysis. At least some of the representations of objects, agents, causality, and number that are created in infancy are the output of



*innate* input analyzers. They are present in neonates, both in humans and other animals.

#### **R2.4. TOOC's hypothesis space regarding innate conceptual primitives is too unconstrained**

**Mandler** points out that allowing the possibility of two types of innate conceptual representations – those embedded in domain-specific systems of core cognition and those embedded in central ones – greatly increases the problems of bringing data to bear on characterizing the representational repertoire of young infants. This is true, but unfortunately, we do not get to stipulate the space of possibilities. Acknowledging both a dorsal and a ventral visual system complicates our understanding of vision, but nobody would argue against this on the grounds of simplicity.

Commentator **Poulin-Dubois** raises a related worry, namely that the hypothesis that there are innate input analyzers that identify the entities in core cognition may be unfalsifiable. She points to the existence of developmental changes in the features that support core cognition. But why does this make the hypothesis of innate input analyzers unfalsifiable? An analogy with the developmental changes in depth perception might clarify what is at stake here. On some developmental accounts of depth perception, some cues to depth are innate, therefore allowing infants to represent depth, which in turn allows them to then learn other cues to specifying it (Kellman & Arterberry 2000). The innate cues can be discovered by experiments on neonates. Similarly, that there are changes with age in the processes through which children identify agents, as well as changes in the inferences infants draw about them, is to be expected from the perspective of a commitment to the existence of systems of core cognition. If some representational capacity is innate, there must be some mechanism that leads infants to create representations of entities in its domain. Therefore, the way to falsify this hypothesis is to counter the evidence that young infants have representations with the content in question, and also to provide a learning mechanism through which the relevant representations could be constructed from leaner primitives.

I plead not guilty to another of **Poulin-Dubois'** charges – namely, that I see developing children as solitary agents, forming their intuitive theories in a social vacuum. It is true that scaffolding and cultural learning do not underlie the acquisition of innate representations, by definition. But my general picture of conceptual development could not be further from this view; the whole second half of *TOOC* concerns the cultural construction of knowledge, and takes on the challenge of showing how scaffolding and cultural learning actually works.

Finally, commentator **Machery** accuses me of conveniently letting my intuitions decide what is the content of the representations that underlie infants' and children's performance in the tasks described in *TOOC*. I plead not guilty. During my career I have spent years diagnosing, on empirical grounds, what the experimenters' "heat" means (Wiser & Carey 1983), and what the child's "alive" (Carey 1985), "heavy" (Carey 2009) and "five" (Carey 2009) mean. In each of these cases I have argued that the relevant concepts are different from (and in some cases even incommensurable with) those we twenty-first

century minimally scientifically literate adults would express with the same words. In *TOOC*, I brought empirical data to bear on many different interpretations of the symbolic capacities the experiments reflect (both richer and leaner than those I settle on). Machery is right that figuring out the content of any given mental representation is an extremely difficult task, and one cannot let intuitions decide the matter.

#### **R2.5. Explicit versus implicit content in core cognition**

A point raised by **Mandler** underlines a confusion I am responsible for. *TOOC* uses the word "explicit" in two different ways. In some places, I use it to mean "public" as when I distinguish systems of core cognition from later developing public representations – those that articulate language, mathematical notations, diagrams, and the like. On this reading, no symbols in core cognition are explicit. But there is another sense of "explicit" that is important to *TOOC*. If we accept a representational theory of mind, we are committed to mental symbols; symbols with formats, meanings, extensions, and computational roles. These are the explicit symbols. But mental representation can also be implicit. For example, within working memory models of small sets of objects (Ch. 4), the explicit symbols are the object files themselves; these are symbols that are activated in the mind, and they represent the objects that are the focus of attention. The numerical content of this whole system of representation is implicit, embodied in the processes that determine whether to open a new object file, to update working memory models when objects are added to or removed from the attended set, or to compare two models on the basis of one-to-one correspondence. There are no explicit symbols for number in this system of representation.

**Mandler** asks how core cognition can support the recall of event sequences that have been demonstrated in the second year of life, given that core cognition contains no explicit symbols. There is no problem here; there are no public symbols in core cognition, but the explicit symbols within core cognition – in the second sense outlined earlier – are input to further computations, and therefore underlie working memory, long-term memory, action, and reasoning in infancy.

### **R3. Case studies of core cognition and other innate representational resources**

#### **R3.1. The case of object representations**

One issue I struggled with in writing *TOOC* was differentiating between the senses in which core cognition representations are perceptual and the senses in which they are conceptual. Core cognition clearly goes beyond sensorimotor representations, but in many ways it resembles other clearly perceptual representations, such as those of depth. I agree with several commentators who were not satisfied with the progress made on this issue; here is one place where there is much work to be done.

My strategy in *TOOC* was to consider many ways scholars have tried to distinguish perceptual from conceptual representations, and to argue that core cognition representations are conceptual in terms of all them, except perhaps being encoded in sentence-like propositional format. This

strategy was no doubt confusing, because I myself do not accept many of those previous analyses as actually drawing the relevant distinctions between conceptual and perceptual representations. For example, commentator **Burge** thinks that I am endorsing Piaget's and Quine's characterization of how perceptual representations differ from conceptual ones, but I am not. Piaget and Quine both argued that young infants created no representations of *object*, because infants are incapable of object individuation, of representing a later-encountered entity as the same object as one encountered earlier, and therefore are incapable of appreciating object permanence. I show that Piaget and Quine were wrong about the representational capacities of infants, and that therefore by their analyses, young infants have a *concept* of objects. That is, infants have object representations, where *object* means roughly *bounded, coherent, separately moving, spatiotemporally continuous material entity*. Note, this gloss reflects implicit content. Infants integrate information from different modalities, and go beyond stationary snapshots of objects, in creating such representations. But I fully agree with Burge that perceptual representations go beyond sensory ones in these ways, and that therefore these facts do not rule out that object representations in infancy are perceptual.

**Burge** also seems to think that because I take object representations to be conceptual, I am denying that there are or could be perceptual representations of objects as well. Again, I do not. Adults clearly have both. The question then becomes, as he points out, what reasons are there to believe that infants have conceptual object representations? My belief that they do hangs on the central inferential role of infants' object representations; their inferential interrelations with causal representations and agent representations, and the fact that they are input into working memory models of small sets over which many different quantitative computations are defined. The quantitative capacities in questions are not the within-module computations of object individuation and numerical identity that are relevant to arguments against Piaget and Quine. Rather, *TOOC* reviews evidence that working memory models of small sets of objects are input into processes that compute total surface area or volume (as in deciding which bucket has more cracker stuff in it) and into computations of one-one correspondence (as in deciding whether all of the objects placed into a container have been removed). Chapter 4 also reviews evidence that infants can create hierarchical models of sets (e.g., a model that is a set of two sets, each containing two objects [see Feigenson & Halberda 2004]), and Chapter 7 reviews evidence that prelinguistic infants have a mental symbol *plural* that applies to sets of objects. These are the quantitative computations that suggest central conceptual role to me. With respect to the integration of object representations with causal representations and agent representations, see Chapters 5 and 6.

Commentator **Gauker** argues that by my own characterization of concepts, icons cannot be concepts, so if the representations in core cognition are iconic, they cannot be conceptual representations. He illustrates his points in relation to the concept *object*. At issue is how we draw the distinction between conceptual representations, on the one hand, and other kinds of representations, on the other. Gauker stipulates that conceptual representations

must be part of a representational system with some sentence-like format; to have the concept *object* the child must be able to think the thought we can express in words "that is an object." (Therefore, Gauker claims that there also must be concepts of *that*, *is*, *a*, for there to be the concept *object*.) I do not agree with these claims.

By "conceptual," I do not mean "language-like." I mean what **Gauker** says I mean – expressing content not expressible in spatiotemporal or sensorimotor vocabulary, and participating in central, conceptual, inferential roles. The question is whether a symbol with the content we express by *object* can be carried by an iconic symbol, such as "🏀." Gauker takes it as obvious that such a symbol cannot represent what two objects have in common, or two balls, as the words "object" or "ball" do. Who can say that? What determines what "🏀" represents? The position taken in *TOOC* is that the answer to this question, abstractly, is the same as what determines what "object" represents – on a dual factor theory, the causal connections between the symbol and the entities in its extension, and some aspects of its computational role. Chapters 2 and 3 provide an extended argument that such a symbol (an object file) for young infants does indeed have the content *object* (i.e., it represents a particular object *as such*, in a particular location relative to the infant and relative to other objects, and Chapters 4 through 7 argue for central conceptual role: objects are represented in working memory models that support action, and in particular causal relations to other objects, and in particular intentional relations to agents. Some of this content is implicit (i.e., it is not required that the child have the ability to think the thought that one object is a different object from another), but I assume that object files are explicit symbols, most probably iconic. Chapter 7 argues that such a symbol decidedly does not have the same content as does the word "ball" until late in infancy, at the developmentally earliest. In sum, the "alternative" picture Gauker offers is a good sketch of how I think about core cognition, because I do not take "conceptual" to mean "language-like."

Commentator **Hill** also comments on object representations, and on my treatment of Quine's position in particular. Hill argues that by crediting infants with representations of *object stages*, or undetached *object parts*, Quine is already committed to their representing objects, as representations of objects are presupposed by representations of stages of objects or undetached parts of objects. I understand Quine to be imagining a representational state of affairs formulated over sensory snapshots and associations among them (and I believe there is textual evidence that something like this is what Quine had in mind for his "perceptual similarity space," Carey 1994; 2009, Ch. 2). On this exegesis of Quine's position, infants' representations of object stages does not require them to have representations of objects.

In *TOOC*, I concede one of **Hill's** points: that it would be possible, in principle, to formulate any one of the infant's expectancies (such as that revealed in the rotating screen experiment Hill takes as an example) in terms of statistical relations among such snapshots. I argue against this alternative on simplicity grounds. At issue is the classic problem of induction; what would lead the infant to focus in on the relevant statistical regularities

from all the properties of the world he or she could encode?

### R3.2. Representations of causality

Commentator **Butterfill** raises all the right questions about infants' causal representations. Might they be discontinuous with those of adults? Might causal representations constitute a system of core cognition, or, alternatively, might different causal representations discussed in *TOOC* actually be embedded in distinct systems of core cognition?

Discontinuity exists at two levels of abstraction; representations in core cognition are discontinuous in format from later public, linguistic symbols, and representations in core cognition may express content that is qualitatively different from that expressed by later acquired concepts. In addition, the content of any system of representation (core cognition or public) is partly implicit, and conceptual development often involves creating explicit symbols that express content that was earlier only implicit. **Butterfill** speculates that infants' causal representations, like number representations within the parallel individuation system, may be perception-like, encoded implicitly as constraints on mental models of events rather than explicitly, with a symbol that expresses the concept *cause*. I completely agree that this is a possibility. However, this question is orthogonal to that of whether there is a central system of causal representations that integrates output from distinct core cognition systems, or only causal representations within the already attested core cognition systems. It is also possible that symbols in the central system, if it exists, may be language-like; there may be an abstract symbol with argument slots that expresses *cause* ( $x, y$ ).

A relevant source of data that bear on deciding among these alternatives derives from the expression of causal concepts in the earliest language acquisition. There is no evidence for discontinuities in content. By contrast, the evidence from language learning strongly suggests discontinuities in number representations. Children use verbal numerals for 1½ years before they figure out how they represent number. "One"-knowers have "two" in their vocabulary for 6 to 9 months before they understand it to refer to the cardinality *two*. Before that, they use it as a plural marker, or to refer to sets with cardinalities greater than one. Nothing like a "one"-knower stage has been discovered in regard to the expression of causal concepts in language. Children have not been observed to use a lexical item "cause" or the causative use of "make" or a lexical causative such as "break" for months with non-adult meanings. Rather, children learn to form both lexical causatives ("he broke the cup") and periphrastic causatives ("he made the cup break") early in their third year of life, and as soon as they command the periphrastic causative construction, they create novel causal alternations across wide conceptual content (e.g., they say "eat the baby," meaning "feed the baby, make the baby eat"). Sometimes these violate adult restrictions on this construction and therefore could not have been in their input (Bowerman 1974). This suggests to me an abstract representation of cause available at the outset of language learning, one that does not require conceptual change for its acquisition.

**Butterfill** seems to suggest that 2-year-olds' lack of explicit access to some causal mechanisms (e.g., those underlying the solidity constraint) shows discontinuities in their concept *cause*. But one should not confuse lack of knowledge of a particular causal mechanism with lack of the concept of *cause*. Uncovering the causal structure of the world is an ancient ongoing project, deeply enmeshed in culture. Kuhn (1977) argues for continuity in the concept *cause* over the history of science, in spite of profound conceptual changes within theories of causal mechanisms. Obviously, children know very little about the causal mechanisms that adults around them understand, and even adults' understanding of causal mechanisms is extremely sketchy (Rozenblit & Keil 2002). But neither fact is inconsistent with the possibility of continuity through development of the concept *cause*.

### R3.3. Agent representations

Both **Kiss** and **Poulin-Dubois** suggest that the adult theory of mind might be discontinuous with infant representations of agency. As with causal representations, there are several ways this may be so: (1) the format of representation may certainly differ; infants cannot talk about their minds; (2) some of the content in external, public, representations of minds may be captured only implicitly in infants' models of the actions of agents, in the form of constraints on the computations these enter into; and (3) the content expressible, either implicitly or explicitly, in core cognition may be incommensurable with that of later developing theories of agents.

**Kiss** characterizes many ways infants' and young children's representations of minds may be qualitatively different from those of adults, but I know of no evidence for **Kiss's** specific proposals. For example, what is the evidence that the first meaning of "happy" is the behavioral manifestations of happiness, such as smiling? Contrary to **Kiss's** speculations, *TOOC* (Ch. 5) discusses the recent studies showing representation of perceptual and epistemic states by preverbal infants, suggesting continuity over development in at least *some* important conceptual content. *TOOC* also summarizes some of the empirical evidence for discontinuity, arguing that it is still an open possibility that developmental changes in the theory of mind involves qualitative discontinuities and requires Quinian bootstrapping.

*TOOC* endorses commentator **Ristau's** observation that there is good evidence for systems of core cognition in animals (sometimes the same ones as in humans, as in analog magnitude [AM] representations of number). Non-human animals' representations of agency is a particularly important topic of active research. It is clear that non-agents satisfy the innate input analyzers that identify agents (as in the experiments with computerized geometric shapes that are attributed goals, or furry robots that are attributed attentional states). However, I know of no evidence for **Ristau's** speculation that the innate input analyzers that identify agents may lead to *widespread* over-attribution of agency by infants of any species, such that we should think of attributions of agency to material entities a *default* representation.

**Ristau's** commentary raises the question of what accounts for the profound differences between humans and other animals in their ultimate conceptual repertoire.



I assume we can all agree that language is one part of the answer. Ristau qualifies this assumption with the observation that other animals also have calls with referential content. Therefore, this aspect of language cannot distinguish us from other animals. But clearly no nonhuman animal has a system of external symbols with the properties of human language. *TOOC* details the role that language plays in Quinian bootstrapping, which in turn plays a role in the expansion of our representational repertoire.

### R3.4. Number representations

Commentators **Landy, Allen, & Anderson (Landy et al.)** argue that number representations are neither modular nor domain specific, because they profligately draw on representations of space. These commentators review many fascinating phenomena that show that numerical representations share computational machinery with spatial ones. The question then arises, for each, whether it reflects mappings between the domains that occur in evolutionary or ontogenetic time.

I agree that computational machinery is recycled over evolutionary time, and that systems that evolved under selection pressures for spatial representations were later drawn upon for other purposes, including numerical representations. The worked-out example in *TOOC* is that of AM representations, which are used in representing many different dimensions of experience (brightness, loudness, duration, length, area) in addition to representing cardinal values of sets; there is even evidence of a common neural substrate for several of these representational systems (Walsh 2003). But these facts do not undermine the claim of innate, domain-specific number representations, as long as the infant does not confuse mental symbols for number with symbols for other dimensions of experience (which infants do not), and as long as the numerical symbols have a further innate numerical conceptual role, as is the case. The domain specificity in question here concerns content, not the nature of (or even the neural substrate of) the computational machinery.

**Landy et al.** endorse *TOOC*'s claim that conceptual discontinuity in the course of ontogenesis also involves recycling old representations and computational capacities in new domains. Quinian bootstrapping is a specific proposal for one way that this is actually accomplished. Such ontogenetic recycling also does not undermine the domain-specificity of innate systems of core number cognition.

### R4. On the possibility and extent of conceptual discontinuity

The commentaries express two incompatible sentiments with respect to conceptual discontinuity. Some deny it (**Rips & Hespos**) and some say it is much more common and comes in many more varieties than is suggested in *TOOC* (**Gopnik, Margolis & Laurence, Weiskopf**). I am persuaded by the commentaries that express the second sentiment.

Theories of conceptual development face two explanatory challenges: specifying how cognitive development is possible, and specifying why, in some cases, it is so hard.

The second half of *TOOC* concerns some of the hard cases, where years of input is mis-analyzed, and the target conceptual system is sometimes never grasped. There are many reasons conceptual development might be slow (including lack of access to relevant input), and one of them is that mastering the target system of representation requires building a representational system that is discontinuous with its input. In each case study *TOOC* characterizes the two successive conceptual systems, CS1 and CS2, specifying the senses in which CS2 is discontinuous with CS1 – having more expressive power, being incommensurable, or both.

Clearly, if we learn or construct new representational resources, we must draw on those we already have. **Rips & Hespos** characterize discontinuity as involving the construction of “entirely new systems” of concepts. This is true in some sense. Discontinuities involve creation of new representational primitives and new systems of concepts articulated in terms of those primitives. For example, the concepts, *weight*, *density*, *volume*, and *matter* are interdefined and acquired en suite, as are the concepts *fraction* and *division*. But the bootstrapping processes that explain how new representational resources are constructed do draw on already existing representational resources. In that sense they are not “entirely new.” Nonetheless, in cases of discontinuity, CS2 cannot be translated into the language of the CS1, and therefore cannot be learned by testing hypotheses stated in that language.

*TOOC* explicitly discusses the challenge **Rips & Hespos** offer to the very possibility of incommensurability. As Rips & Hespos put it, if bootstrapping is possible, then it must be possible to characterize a function from CS1 to CS2, and hence to translate between CS1 and CS2. The key issues here are what that function is, and whether it counts as a “translation” (Kuhn 1982). To translate is to express a proposition stated in the language of CS2 in the language one already has (CS1), as in translating “Je suis heureux” into “I am happy.” In cases of discontinuity in which Quinian bootstrapping is required, this is impossible. Bootstrapping is not translation; what is involved is language construction, not translation. That is, drawing on resources from within CS1 and elsewhere, one constructs an incommensurable CS2 that is not translatable into CS1.

**Margolis & Laurence** and **Weiskopf** point out that in addition to Quinian bootstrapping, there are also atomistic processes that create new representational primitives. Weiskopf (2008) makes a convincing case that these atomistic processes also result in increases of expressive power. I agree, and I also agree that what Weiskopf calls “coining” is always involved in the creation of new representational primitives. Discontinuities come in many different flavors, however, and I stand by my thesis that those that require bootstrapping are an important class. According to the analysis of Laurence and Margolis (2002), adding a new natural kind concept to one's repertoire depends upon the prior existence of a natural kind schema with the content “same natural kind as *x*” and filling in *x* with a kind syndrome (a prototype, a theory) for the kind *x*. This is clearly a different process than learning that a quark is a new kind of subatomic particle, if one does not currently have the capacity to entertain thoughts about subatomic particles. It is this latter type of

discontinuity, that which requires Quinian bootstrapping, that is my focus in *TOOC*.

## R5. On bootstrapping

### R5.1. Bootstrapping in general

Commentators **Haman & Hernik** emphasize that there is genuine conceptual development that does not require creation of new representational primitives, and that there are bootstrapping processes short of Quinian bootstrapping. I agree, and *TOOC* discusses syntactic and semantic bootstrapping processes in language acquisition as examples of bootstrapping processes that do not result in the creation of new representational primitives. Abstractly, all bootstrapping involves using mappings between different representations in the service of extending them in some specifiable way. Haman & Hernik's proposal for the creation of new object kind representations, drawing on core cognition of objects, causal/functional analysis, and the capacity to create long-term memory symbols would be a bootstrapping process by this characterization (and is essentially what I propose as well in Ch. 7 of *TOOC*). My account of the creation of long-term memory models of small sets of individuals that support the meanings of "one," "two," "three," and "four" in the subset-knower stage is another bootstrapping process. Working out exactly how such processes work is an important project in the field of cognitive development. But one major thrust of *TOOC* is that there are episodes of conceptual development that require more.

### R5.2. Must the placeholder structure be articulated in public symbols?

Placeholder structures consisting of semantically impoverished symbols are the key to how Quinian bootstrapping differs from the bootstrapping processes involved in the episodes of learning described in section 5.1. The placeholder symbols gain whatever initial meaning they have from their relations to each other. Several commentators (**Allen & Bickhard**, **Heintz**, **Weiskopf**) wonder why it is important that the placeholder structure be encoded in the external public symbols of language, mathematical symbols, diagrams, and the like.

**Allen & Bickhard** make use of the possibility that placeholders may be mental symbols to counter my argument for innate representational primitives. They suggest that infants might create a whole suite of interrelated new mental symbols, the content of each being exhausted by its role in the mental structure. By hypothesis, none is constructed from already existing concepts. I agree that this is a *logical* possibility, but what would ever lead an infant to do this?

Encountering a new external symbol from others' use of it (e.g., first hearing the word "mass" in the context of the sentence "force equals mass times acceleration," or "two" in the context of the count routine) is often the impetus for an individual's coining a new mental symbol. As **Weiskopf** points out, this is not the only impetus to create a new symbol in the language of thought. I agree with these commentators that from the point of view of how Quinian bootstrapping generates new representational resources, it is not necessary that the symbols be

public. It is certainly logically possible that a whole placeholder structure of interrelated empty symbols could be generated in the language of thought and used to model other domains of representations that already have wide content. As I said, the question is what might lead this to happen. Before I can even begin to evaluate this possibility, I would need to see a plausible worked example.

In all of the case studies of Quinian bootstrapping in *TOOC*, the placeholder structures *were* external symbols, probably for several reasons. As emphasized by **Heintz**, conceptual change is a social process, fueled by communication. But appeals to communication, even with its assumptions of relevance, will not do all the work we need here, for what is at issue is how listeners construct the representational resources to *understand* what is being communicated. The role of placeholder structures in conceptual change may also depend upon their being public. Some public symbols may be easier to think with. Maxwell explained that he used diagrammatic models that he knew captured the mathematics of Newtonian forces in a fluid medium (his placeholder structure) in his modeling of the empirical phenomena involving electricity and magnetism in exactly this way: that they are easier to think with (see Nersessian 1992). Finally, the modeling processes involved in conceptual change unfold over years, and the placeholder structures need to be stable; external symbols may facilitate that.

Although conceptual change is a social process, its first step, contrary to **Weiskopf**, is not always learning *from others* a set of new symbols and their interrelations. This is usually so in development, but obviously never so in the bootstrapping episodes in cultural history (see Ch. 11 of *TOOC*). In the case of Maxwell, an abductive guess led him to explore the hypothesis that the forces at work in electromagnetic phenomena were similar to the Newtonian forces in a fluid medium. That is, *nobody* was using the language of electromagnetic theory; its theoretical terms and equations were the *output* of the bootstrapping episode. In the case of historical bootstrapping, the placeholders derive from conceptual structures created in other contexts and seen to be relevant to the domain at hand through an abductive leap.

### R5.3. The prehistory of number representations

Commentators **Overmann, Wynn, & Coolidge (Overmann et al.)** consider the construction of explicit representations of natural number in cultural history. They assume, and I agree, that external tally systems (lines on clay tablets, notches on sticks), which are widespread both in the archeological and in the ethnographic records, were the first step in the process. Such external symbol systems constitute an example of "extended cognition," in which external objects take on symbolic functions. Note also that tally systems represent number as does parallel individuation, with the external tallies making up for the limits on working memory. That is, each tally stands in one-to-one correspondence with an individual in the set, such that the set of tallies as a whole represents the cardinality of the set. Overmann et al. speculate that strung beads, which go back in the archeological record for 100,000 years, may have been an artifact co-opted for this use.

This may be, but is there any evidence that beads were ever used as external tallies? If so, understanding the invention of tally systems would involve understanding how people came to the insight that beads could serve *this* symbolic function, rather than decorative uses, or as markers of wealth, or myriad others. That is, the availability of an artifact that *could* serve as the medium of a tally system doesn't explain how it came to be one. Now that we are in the realm of speculation, I believe, contra **Overmann et al.**, that body counting systems could well also play an extended cognition role in the cultural construction of integer representations. Bodies, before beads, may have been the basis of the first tally systems. Beads may go back 100,000 years, but fingers go back millions, and finger tally systems are also widely attested in the ethnographic literature. But even so, the question then becomes how the insight arose that fingers could be used in a tally system based on one-to-one correspondence.

#### R5.4. Acquiring representations of natural number

Commentators **Gelman, Gentner & Simms**, and **Spelke** engaged the fundamental issues concerning conceptual discontinuities in and bootstrapping of natural number representations. These debates might be hard to follow for those not steeped in the dialog, so here I will try to bring out the main issues.

**Gelman's** and my disagreements coexist within broad agreement on the big theoretical picture, and all of my work on number starts with hers. There are two major disagreements: (1) She presupposes that there is only one innate nonverbal representational system that plays a role in the extended developmental process we both endorse ("*the inherited...*"), whereas I believe that there are other innate resources and that they are more important in the initial construction of integer representations; and (2) I argue that innate number representations are discontinuous with representations of natural number, including verbal numerals deployed in a counting algorithm, whereas she argues that children understand how counting represents number upon first learning how to count.

**Gelman** and I agree that AM number representations are continuous throughout development, support arithmetic computations, and compute representations of small sets as well as large ones, both in infancy and adulthood (*TOOC*, Ch. 4). But, contra **Gelman**, I believe the parallel individuation system also has a big role to play in the construction of natural number. This system is also continuous throughout development, and also has numerical content, in the form of numerically relevant computations carried out over models of small sets. Additionally, there are other representational resources with innate support that contribute to the construction of representations of natural number. These include the logical capacities that underlie the semantics of natural language, including quantifiers. It is clear that the resources for building the culturally constructed representations of natural number include much more than those provided by the AM system. *TOOC* argues that none of these three systems of number representation, on their own, *contains* representations of natural number (there are no symbols for any natural numbers, for example) in

any of these systems. Therein lies the discontinuity. In her recent writings, **Gelman** acknowledges this; the AM system contains no representation of exactly one, and is not built on the successor function (Leslie et al. 2007). Therefore, AM representations are discontinuous with the numeral list representation of natural number, which has both of these properties. In creating a numeral list, or a symbolic tally system, for that matter, cultures create a representational system that can express thoughts and support arithmetic calculations that were not available to the infant or nonhuman animal, and when individuals master these culturally created systems, they similarly extend the expressive power of their representational repertoire.

The discontinuity at stake in **Gelman's** and my debate is between the numerical concepts expressed by the verbal count list and those in core cognition. What the child is creating goes beyond a way of talking about what they already represent. I stand by the evidence that learning how counting represents number is extremely difficult, and by the evidence that children who are cardinal principle-knowers are qualitatively different from those that are not. Much of the data **Gelman** alludes to in her commentary reflect performance by children who already are cardinal principle-knowers (i.e., many 3-year-olds), and therefore cannot directly bear on this debate. It would be very interesting to explore children's performance on Zur and **Gelman's** intuitive arithmetic tasks, for example, as a function of knower-level rather than age. Evidence is mounting that **Gelman** is right that *TOOC* underestimates the numerical understanding of children in the subset-knower stage. For example, before they have worked out the cardinal principle, some subset-knowers do know that numerals later in the list refer to larger cardinal values than do numerals earlier in the list (e.g., Shusterman et al. 2009). How counting represents number is worked out in small steps during the subset-knower stage. Still, subsequent work has also confirmed that only cardinal principle-knowers understand, even if implicitly, how counting implements the successor function (e.g., Sarnecka & Carey 2008; Shusterman et al. 2009). Therefore, the basic bootstrapping story stands.

**Spelke**, like **Gelman**, believes that I have underestimated the role of the AM system of number representation in the creation of public symbols for natural number. She offers an alternative bootstrapping proposal, in which the numerical content in the parallel individuation system is combined with that in the AM system directly, not in the service of learning verbal numerals. Notice that we still must explain how children learn the meanings of verbal numerals and how they learn how the count list represents number. However, if **Spelke's** bootstrapping process exists, and were to take place prior to mastering counting, it might support learning to count.

In Chapter 7 of *TOOC* I consider two bootstrapping proposals similar to **Spelke's**, although in the service of mastering counting. One involves mappings between the count list and AM representations alone, and the other involves mappings between AM representations, parallel individuation and the count list. I reject the first on the grounds that AM representations do not contain representations of "one" or the successor function, and therefore cannot support the induction that there is ample evidence



children make upon becoming cardinal principle-knowers – namely that two adjacent numerals in the count list represent sets that differ in cardinal values by one. The second proposal is very similar to that outlined in Spelke’s alternative. I note if children had mapped numerals to AMs and induced the “later in the list, larger AM” generalization, this could aid in working out how verbal numerals represent number. I rejected this proposal on empirical grounds – there was no evidence that subset-knowers have made this induction, and some evidence from my own laboratory that they had not (LeCorre & Carey 2007). However, since then, new evidence has come to light that perhaps they have done so (e.g., Shusterman et al. 2009). Nonetheless, learning how counting works requires inducing the relations between order in the list and the successor function, not only the relation between order in the AM system and the successor function. Therefore, I stand by my bootstrapping story for how children work out how *verbal numerals* represent number.

But is it possible that children and animals might create representations of *integers* through some process that does not involve mastering a count list? I am open to that possibility. Tally systems represent exact cardinal values without counting, but of course, they do not draw on Spelke’s proposed bootstrapping mechanism. Rather, they also draw on the representational resources of parallel individuation. However, I doubt that such a process occurs in the absence of the construction of some public representational system (a tally, a verbal count routine). First, one would need to specify a context in which such a mapping would be constructed. Second, one would need to understand why this construction is not achieved by humans who do not have a count list or a tally system in their public symbolic repertoire (e.g., the Pirahã: Gordon 2004; Frank et al. 2008; the Mundurucu: Pica et al. 2004; and homesigners: Spaepen et al. 2011).

Finally, although mastering counting requires understanding the relation between order in the list and numerical order, I nowhere claim that children derive representations of numerical order only from the count list. Numerical order is implicitly captured in the computations carried out in the parallel individuation system, and easily read off from AM representations. Once children have integrated AM representations with counting, they can and do use AM representations to support arithmetic reasoning using symbols. Therefore, I do not disagree with any of Spelke’s arguments that AM representations support, even are crucial to, certain adult arithmetic computations. But evidence derived from 5-year-olds and adults does not bear on the process engaged in by 2-year-olds as they create the first public representations of natural number.

Analogy underlies one of the modeling processes through which representational resources are combined during episodes of Quinian bootstrapping. I agree with Gentner & Simm’s observation that analogy has other roles to play in conceptual development as well. But I disagree slightly with their characterization of the steps in mastering verbal numerals. It is true that the child creates a mapping between sequential order in the numeral list and ordered quantities, but it is important to notice that the quantities so modeled (captured in enriched parallel individuation) are not yet natural

numbers. Gentner discusses the creation of the first natural number representations as a redescription or rerepresentation of the two parallel relations so that they are one and the same relations: SUCCESSOR [numeral  $n$ , numeral  $n + 1$ ]  $\leftarrow \rightarrow$  SUCCESSOR (set size  $n$ , set size  $n + 1$ ). I doubt whether the 3-year-old child has any representation of the successor function that labels the relation – that is, has any mental symbol *successor*. Rather, I would characterize the last step of this particular bootstrapping episode differently. The order relation in the numeral list is still merely serial order in a list, but what the child now has done is analyze “five, six, seven...” as summary symbols “ $n$ ,  $n + 1$ ” by analogy to “one, two, three” and “four.” Following up on Gentner & Simm’s central point, many more bootstrapping episodes are yet to come before the child will have a full representation of natural number and the successor function.

## R6. On concepts and concept acquisition, in general

### R6.1. On the theory–theory of narrow content

A commitment to the existence of narrow content requires distinguishing between those aspects of inferential role that determine conceptual content and those that do not. Commentator Keil suggests that evidence concerning what he calls the degraded nature of our knowledge militates against the possibility of succeeding at this project, as well as militating against the theory–theory of concepts. One thing Keil means by “degraded” is “sketchy.” I agree that our explanatory theories are sketchy and sparse in the way Keil’s research has shown us, but this may be irrelevant to the existence of narrow content if, as I believe, only a very small part of conceptual role is content determining. Narrow content does not exhaust a symbol’s content, which is why we have representations of entities about which we have the sketchiest of knowledge. There is also wide content; the motivation of the externalist movement was to explain how we borrow reference from those from whom we hear a term. The narrow content of a natural kind symbol like *dog*, is perhaps only that it refers to a natural kind, and therefore falls under the assumptions of psychological essentialism, or perhaps some placement in a framework theory (e.g., that it is a kind of animal, and therefore is constrained by a skeletal vitalist biology). That we hold contradictory beliefs about dogs is part of the motivation for this guess, not a problem for the existence of narrow content. That is, the existence of contradictory beliefs requires that we must be able to distinguish concepts from conceptions, and supports the hypothesis that content-determining conceptual role is a small part of everything we know about the entities categorized under our concepts. These considerations apply to metaconceptually held scientific theories as well as to intuitive ones: Only a few relations among concepts distinguish the source-recipient theory of thermal phenomena from the caloric theory, although those relations allow separate concepts of *heat* and *temperature* only in the latter case.

Keil raises a very interesting issue not touched upon in TOOC – how studies of the breakdown of conceptual representations with age, disease, brain damage, and the like, bear on our understanding of the nature of concepts (here

“degraded” means damaged). Deborah Zaitchik and colleagues (Lombrozo et al. 2007; Zaitchik & Solomon 2008; 2009) have found that patients with Alzheimer’s disease (AD) resemble preschool children when performing tasks that diagnose conceptual structure – they make animistic judgments that the sun, cars, and similar things are alive, and they demonstrate promiscuous teleology. These findings leave open two very different interpretations: (1) AD affects domain general processes that underlie the participant’s capacity to bring relevant conceptual resources to bear on a given task; and (2) AD affects the conceptual resources themselves, those that underlie the knowledge, and perhaps even the concepts, in the domain. For example, the disease might disrupt the skeletal inferential structures that constitute a vitalist biology, such that the participants no longer have a concept of living things. Gaining a fundamental knowledge of which interpretation is correct will be important to our understanding of the nature of concepts, perhaps even bearing on which aspects of conceptual role are content determining.

Contrary to commentator **Korman’s** assumption about what the theory–theory of concepts comes to, *TOOC* nowhere claims that conceptual changes occur only in the context of formal scientific development. Rather, the idea is that there are important commonalities between the conceptual changes in the history of science and those that occur in childhood. That is, many questions about conceptual development receive the same answers in both cases. These include: how is conceptual change distinguished from belief revision, what is an “undifferentiated concept,” and what is the role of Quinian bootstrapping in episodes of conceptual change? **Korman** advocates the existence of smaller-scale conceptual changes than those involved in the case studies in *TOOC*. I am open to that possibility (see sect. 4). Before I would be able to evaluate the proposal, I would need to know in what sense a developmental change is a conceptual change as opposed to a change in belief. *TOOC* provides such an analysis for the conceptual changes in its case studies, but **Korman** gives no examples of what kinds of changes she has in mind.

Although I mention the proposal that the causally deepest features of a concept are likely to be those that are content determining, I do not particularly endorse it. I agree with **Korman’s** reading of the literature on the causal status hypothesis concerning categorization decisions, but I am skeptical about making categorization decisions the central phenomenon in our exploration of the nature of concepts. My positive proposal is quite different; namely that in at least some circumstances narrow content is determined by the conceptual role that is exhausted by the placeholder structures in which new primitives are introduced.

### **R6.2. On the relations between the philosophers’ and the psychologists’ approaches to understanding concepts**

**Machery’s** commentary raises a deep issue on which we have opposing views. He believes that the traditional cognitive psychologists’ project (understanding categorization, inference, conceptual combination) and the philosopher’s project (understanding reference, truth,

propositional attitudes) are completely distinct, whereas I do not. My appeals to a part of the philosophical literature were aimed at sketching some of the arguments for wide content. I also showed that *psychological* work supporting psychological essentialism, explanation-based reasoning, and conceptual development converges with these arguments. So whereas he and I agree that the psychological and philosophical literatures emphasize different phenomena, many philosophers also endorse that understanding inference, categorization, conceptual combination, and conceptual development (omitted from **Machery’s** list) are part of the explananda of a theory of concepts. One goal of *TOOC* was to encourage psychologists to see that reference, truth, and propositional attitudes are as well. **Machery** suggests that I endorse information semantics, while failing to distinguish it from other approaches to wide content. Instead, *TOOC* endorses a dual factor theory and I draw from information semantics, the causal theory of reference, and other work, as different approaches to wide content. I distinguish among them, but do not choose among them.

**Machery** proposes a solution to the problem of distinguishing concepts from conceptions. He suggests that the content-determining aspects of the representations that underlie categorization and inference are those that are activated by default, independent of context, whenever we use a given concept. Insofar as the representational structures being considered here are prototypes, definitions, representations of exemplars, and/or intuitive theories, I am deeply skeptical of this proposal. I would bet dollars to donuts that there are no such default representations activated independent of context. Indeed, **Machery’s** (2009) masterly review of the literature in the service of his argument that cognitive psychology’s concept *concept* is not a natural kind undermines the existence of such default representations.

### **R6.3. On the possibility of a dual factor theory of concepts**

I disagree with **Rips & Hespos** that my version of dual factor theory collapses to a single factor. I offer general arguments for wide content, and I assume that the mechanisms through which it is determined will be complex and messy, encompassing aspects of social/causal history, as well as the machinery envisioned by developers of information semantics. In the last chapter of *TOOC*, I write that the challenge is to defend the claim that there is any narrow content at all. Although narrow content may play some role in the processes through which wide content is determined (e.g., by constraining the nature of the causal connections between certain classes of symbols and the entities they represent), it in no way exhausts them. Nonetheless, **Rips & Hespos** raise an important point – any workable dual factor theory will have to specify how the two factors work together.

Commentator **Mandelbaum** also raises concerns about the very possibility of a dual factor theory of concepts. He points out that my proposal for specifying the narrow content of symbols in core cognition differs from my proposal for how to do so in the case of concepts that arise through episodes of Quinian bootstrapping, and he rightly points out that neither proposal applies to newly acquired atomic concepts, such as, taking his example,

*fence*. I agree. I believe it is unlikely that there is one solution to the problem of specifying what aspects of conceptual role determine narrow content. Different kinds of concepts will require different solutions. The theoretical challenge is to find the principled distinctions among types of concepts, as well as to specify what aspects of conceptual role determine narrow content of each type.

I do discuss the question of how newly acquired atomic concepts might get their content, focusing on the proposal by Laurence and Margolis (2002) and of Macnamara (1986) (see also Weiskopf 2008). Their example is a natural kind term, such as *tiger*. The narrow content of a newly learned public symbol “tiger” or a newly coined mental symbol *tiger* is *same natural kind as [kind syndrome of a tiger]*, where the kind syndrome might be represented as a prototype, set of exemplars, or intuitive theory. The work done by the abstract schema for a natural kind is that the holder of this representation is open to almost everything in the kind syndrome being revisable; that is, this concept falls under all of the assumptions of psychological essentialism and the division of cognitive labor of deferring to tiger experts. Therefore, the relevant aspect of conceptual role that determines content here is not the whole kind syndrome, but that which determines the nature of the function between the symbol and the world, including that which determines that the symbol is a symbol for a natural kind. An analogous solution could apply for *fence*. My guess is that there is an artifact kind schema that supports meanings like *same artifact kind as [kind syndrome for a fence]*, where the information in the kind syndrome is taken to be tentative and revisable (i.e., not itself content determining). The schema specifies the importance of function and original intent of the designers, leaving open that current representations of these may be wrong. On this view, it would be possible to have the lexical item “fence” in one’s vocabulary without yet having assigned it to that minimal schema (e.g., if all one knew about fences are that they are something that make good neighbors – after all, good intentions and warm feelings make good neighbors too, but these are neither artifact kinds nor fences).

#### R6.4. Is concept learning “explicable by content?”

Commentator **Shea** raises the possibility that *TOOC* provides a recognizably psychological explanation of the acquisition of concepts that does not fit the dominant explanation schema in psychology. Specifically, he suggests that my accounts of concept acquisition neither rely (solely) on rational inference nor on explanation by content. I am not sure what philosophers mean by “explanation by content,” so my response is tentative. As I understand it, explanation by content is explanation in terms of non-metalinguistic inferences over mental representations alone, as when we explain an agent’s opening the refrigerator by appeal to the agent’s desire for an apple and belief that there is one in the refrigerator. This is explanation by content because it makes no appeal to the truth of the matter of the location of an apple.

I agree with **Shea**’s gloss of the bootstrapping process that underlies the transition between the subset-knower stage and the cardinal principle-knower stage: “Carey rightly rejects the idea that it is properly described as

hypothesis testing. The child is doing something different: building an uninterpreted model with the counting words, and then giving that model an interpretation.” My “uninterpreted” here refers to wide content; the model is not yet mapped to numbers. However, I believe that cognitive modeling is a rational process, and the cognitive model has narrow content given by inferential role. Therefore in one sense of “explanation by content,” this is an explanation by content. Shea criticizes this proposal because it would lead to pernicious holism. I do not see why. This proposal is important to my characterization as to which aspects of inferential role determine narrow content in the case of concepts whose origin lies in episodes of Quinian bootstrapping. And the holism is not pernicious, because not every aspect of the conceptual system is potentially content determining.

When the child induces that the each numeral represents a cardinal value one more than the preceding one, the child confirms that guess against further input. That is, cognitive modeling is a rational process. Importantly, the numerical content of the count routine is partly implicit, just as the numerical content of parallel individuation is implicit. Representations of the successor function, insofar as they are attributable to the newly minted cardinal principle-knower, are carried in the counting principles the child masters. That is, learning to count is learning a skill. Therefore, the question I would ask **Shea** is whether skill acquisition is explainable by content? If not, it is easy to find violations of the principle that all explanation in psychology is explanation by content. If so, then learning the count list is not a counterexample to the principle. My instinct is to favor the latter possibility.

The same questions arise with respect to **Shea**’s characterization of how the child becomes a “one”-knower, and then a “two”-knower. What the child is learning is a procedure, and the procedure the child builds draws on representational and computational resources that are part of core cognition. That is, procedurally, the child does have the resources out of which a hypothesis about one-ness could be constructed (even though the child has no symbol for *one* that could articulate a hypothesis about what the word “one” means.) The enriched parallel individuation account of the meaning of “one” in the subset-knower stage is that the child makes a long-term memory model of a single individual, a model that is identical in format to core cognition’s working memory models formulated in terms of individual files, and acquires the following procedure for applying the word “one:” if a working memory model of a given set can be put in one-to-one correspondence with that long-term memory model, then “one” applies to that set. Similarly for “two.” The process by which the child builds these procedures is rational; it does not depend only on correlations mediated by causal connections to the same external-world property.

#### R7. Concluding remark

I thank my commentators again for their clarifying remarks. The controversies aired in our debates confirm the interdependence of the projects of explaining the origin and nature of concepts.



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[The letters “a” and “r” before author’s initials stand for target article and response references, respectively]

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