On the Evidence for Prelinguistic Concepts

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ABSTRACT: Language acquisition is often said to be a process of mapping words into pre-existing concepts. If that is right, then we ought to be able to obtain experimental evidence for the existence of concepts in prelinguistic children. One line of research that attempts to provide such evidence is the work of Paul Quinn, who claims that looking-time results show that four-month old infants form "category representations". This paper argues that Quinn's results have an alternative explanation. A distinction is drawn between conceptual thought and the perception of comparative similarity relations, and it is argued that Quinn's results can be explained in terms of the latter rather than the former.

Keywords: concept acquisition, language learning, similarity

1. Some basic assumptions and some basic questions

Here are three *basic assumptions* that one might make about language learning: First, children interact with their nonlinguistic environment and on that basis acquire concepts. Second, children interact with people speaking language and on that basis figure out which words express which concepts. Third, as a result, children come to be able to express their thoughts in words, and come to be able to recognize the thoughts that other people are expressing.

These assumptions do in fact underlie the leading theories of language acquisition in psychology today. According to Paul Bloom,

> Learning a word involves mapping a form, such as the sound "dog," onto a meaning or concept, such as the concept of dogs. (2002, p. 89)

According to Gregory Murphy,

> … a word gets its significance by being connected to a concept or a coherent structure in our conceptual representation of the world. To put it another way, the meaning is built out of concepts. (2002, pp. 388-89)

The question I want to address in this paper is whether there is any empirical evidence to support these basic assumptions. In particular, I will consider whether there is any experimental evidence for the claim that children do acquire concepts apart from language.

We need to distinguish between two kinds of evidence that might be offered for the basic assumptions, direct and indirect. The indirect kind simply demonstrates that the basic assumptions are part of a thriving research program: they are more or less immediate corollaries of a general conception of the mind, which researchers take for granted in other areas of cognitive science as well. Moreover, within the framework of the basic assumptions about language learning there is a lively, empirically grounded debate over many details. According to that more general conception, cognition is in general a process of subsuming particulars under concepts, drawing general conclu-

sions by means of inductive reasoning and then making inferences from those generalizations. Specific issues that can be raised within the framework of the basic assumptions include: Does the child start out with very general concepts and specify kinds of thing within them, or does the child instead start out with very narrow concepts and abstract?[1] Does the child rely on syntactic cues to discover the meanings of words, or does the child instead learn syntax on the basis of an independent grasp of meanings?[2]

The problem with relying exclusively on this kind of indirect evidence for the basic assumptions is that this kind of evidence does not provide sufficient reason to set aside certain basic questions—philosophical questions, if you like—that ought to make us wonder whether we really understand what we are saying when we espouse the basic assumptions. Working within the basic assumptions, one might allow that the child observes the uses of words in order to determine which of all the concepts he or she already has are the ones that people express in words, or that observations of the uses of words accelerate or stimulate a process of concept-formation that might have taken place even apart from those observations; but one will not allow that learning to use a word is just what acquiring a concept consists in, for then it would not make sense to describe language learning as, in Bloom's words, a process of mapping words onto concepts. So one of the basic questions that needs to be answered by those who espouse the basic assumptions is: what does the child's distinguishing between kinds consist in if not its knowledge how to use the words that stand for those kinds?

Typically, proponents of the basic assumptions hold that the process by which a child learns which words map into which concepts involves a kind of mind-reading. This is certainly an integral feature of Bloom's account of language learning. The child is not supposed to rely on simple induction, generalizing from correlations between the uses of a word and the circumstances under which it is used. Rather, the child is supposed have some insight into what a speaker is, on a given occasion, *trying to say*. The child's hypothesis will be that the concepts that the speaker's words are mapped onto are those that comprise the thought that the speaker is attempting to convey. The obvious question this raises is: how can a child who cannot even talk yet form hypotheses concerning the contents of a speaker's communicative intention?

Perhaps the most basic question we can put to a proponent of the basic assumptions is this: How can something in the mind have a meaning and an extension? What is the relation between some thing in the brain, or some state of the brain, and the set of all dogs, or dogkind, such that we can say that that thing in the brain means *dog*? If the things in the mind that have meanings and extensions were themselves just spoken words, or were somehow derivative from spoken words, then in answering this ques-

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[1] On this question, compare Carey and Xu (Carey and Xu 2001, Xu 2002), who hold that conceptual development begins with a very broad object concept, and Goldstone and Barsalou who (1998), who conceive of concept formation of as process of abstraction from perception.

[2] On this question, see the debate between Lila Gleitman (Fisher, Hall, Rakowitz and Gleitman 1994) and Steven Pinker (1994).

tion we could appeal to the role that words play in interpersonal exchange. But if we have to answer the question without identifying concepts with words, then we will have to answer in a different way. For example, we might look for correlations between the activation (or "tokening") of a concept and events in the external world. Or we might posit some kind of mapping relation between whole structures of mental representations and structures in the world. Or we might look for an answer in terms of the functional role that concepts play in the processes of problem-solving. But these approaches have been tried, repeatedly, in philosophy, and all of them have run up against some very basic objections. (For my own criticisms, see my 1994, chapter 3, 1995, 2003, chapter 2). At this point in history, a more promising strategy (if only because it has not been refuted *yet*) is to seek an account of linguistic meaning in terms of the norms of discourse that govern interpersonal cooperation by means of language.

2. Conceptual thought versus similarity judgments

In order to have a good reason to persevere with the basic assumptions even in the absence of answers to the basic questions, we should be able to cite some *direct* evidence for the basic assumptions in addition to the indirect evidence. In particular, there should be experimental evidence giving us good reason to believe that children do acquire concepts apart from language. But in fact, it seems to me that we really have no evidence for that at all. I cannot here examine every line of research that someone might cite as promising, but I will look closely at one representative line of research and argue that it simply does not show what it is supposed to show. In my critique of this experimental work, I will rely on a distinction between *conceptual thought* and what I call *similarity judgments*. So I need to begin by drawing that distinction and explaining why similarity judgments in my sense are not a species of conceptual thought.

As I define it, a *basic* conceptual thought is a representation of a particular as belonging to some kind or of some particulars as standing in some relation. For example, my thought that *this is a chair* is a basic conceptual thought because it represents *this*, a particular, as belonging to the kind *chair*. In saying this, I take for granted that an agent represents a particular as belonging to a kind only in so far as he or she conceives of the kind as bounded in some way, although those boundaries may be vague. So someone who can think of this particular as a chair can also think of this table as *not* a chair. In addition to such basic conceptual thoughts there are other thoughts that qualify as conceptual thoughts because they are *inferentially related* to basic conceptual thoughts. For example, since *This is a dog* is a conceptual thought, and *This is vicious* is a conceptual thought, the thought *Some dogs are vicious* is a conceptual thought as well, since it is inferentially related to the first two.

What I call a *similarity judgment* is a representation of a relation among three things. If given three things x, y and z, an agent A represents x as more like y than like z, then that representation in A is a similarity judgment. One kind of similarity judgment is indeed a species of conceptual thought, as I have defined this. If an agent *accepts the*

proposition that x, y and z bear the 3-place similarity relation to one another, then the agent will have a basic conceptual thought whose content is that proposition. But it might also be possible for an agent to *perceive x* as more like *y* than like *z* or to *imagine x* as more like *y* than like *z*, and it is not obvious that such representations are basic conceptual thoughts. Even in cases where they are not, I will call these representations *similarity judgments*, although in such cases the term is misleading.

For example, consider the classic duck-rabbit (the ambiguous figure from Wittgenstein's *Philosophical Investigations*). It is tempting to suppose that when I perceive the duck-rabbit as a duck, what I do is subsume it under my concept *duck*, and that when I perceive it as rabbit, I subsume it under my concept *rabbit*. But seeing-as does not have to be understood as a matter of subsuming things under concepts in this way. Instead, it may be understood as a matter of perceiving a three-place similarity relation. When I see the duck-rabbit as a duck, what may be happening may be *something like* the following: I have in mind a visual image of a particular duck and a visual image of a particular rabbit; by means of these images, I compare the duck-rabbit with the duck and the rabbit; and I perceive this thing [the duck-rabbit] as more like this thing [the duck] than like that thing [the rabbit]. Of course, when we see the duck-rabbit as a duck, we may not have in mind a visual image of any particular duck, and so what goes on in us cannot be *exactly* what I have described. But we have seen ducks and rabbits and can imagine them if we choose. So there are imagistic representations of ducks and rabbits somewhere in our minds that are not in any obvious sense conceptual representations; so we may be able to compare the duck-rabbit to ducks and rabbits via imagistic representations and thereby, without making use of any conceptual representations of ducks and rabbits, perceive the duck-rabbit as more like a duck than like a rabbit.

Off hand, these similarity judgments might seem to rest on the application of concepts. (Certainly many people have assumed so. See for example, Medin and Goldstone 1995.) To represent *x* as more like *y* than like *z*, it might be said, is to *judge* that *x* and *y* have some property in common that *z* lacks. But I do not think that that should be just obvious. Consider the three figures in Figure 1.

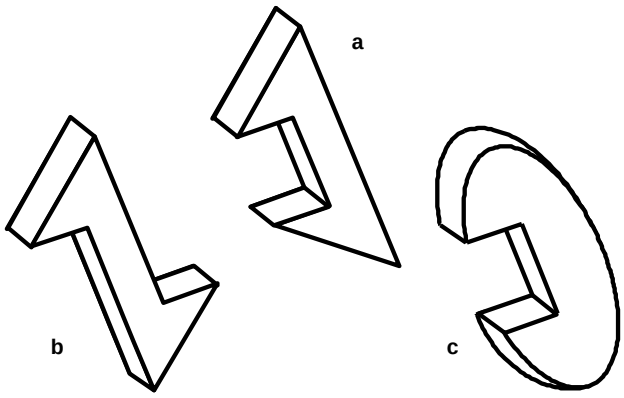

FIGURE 1

The reader will recognize that in one way *a* is more like *b* than like *c*. In representing *a* as more like *b* than like *c* in this way, is it *obvious* that we are thinking of some property that *a* and *b* have in common that *c* lacks? I think that should not be obvious. If that is obvious, then it should be obvious also whether a fourth figure, in Figure 2, has that property or not.

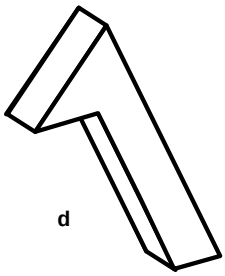

FIGURE 2

But surely the reader will not think that it is just obvious that in representing *a* as more like *b* than like *c* he or she had in mind a category (whether precisely defined or vague) that either did or did not include figure *d*. For example, we should not say that what *a* and *b* have in common that *c* lacks is *linearity* or *lack of curvature*, so that in recognizing the similarity between *a* and *b* we applied the concept of linearity or the concept of lack of curvature, because we can easily imagine figures that possess these properties but are far less like *a* than *c* is. Consequently, it is questionable whether in representing *a* as more like *b* than like *c*, one has to have in mind some more or less definite property that *a* and *b* have in common that *c* lacks.

3. The Quinn paradigm

So I ask the reader to consider the possibility that there might be a kind of representation of similarity that does not reduce to the application of concepts. With that possibility in mind, let us examine some purported evidence for the existence of concepts in prelinguistic children. The research program that I will examine is that of Paul C. Quinn of the University of Delaware. (Much of Quinn's research has been done in collaboration with Peter Eimas of Brown University.) I think it is important to look at Quinn's research because it is one of the few sustained attempts that I know of to demonstrate the presence of concepts in prelinguistic children.[3]

Quinn's experiments use a looking-time measure to test for concepts in infants who are just 3 or 4 months old and so not able to talk at all. In one version of the experiment (Quinn and Eimas 1998, experiment 2), Quinn showed infants (48 of them)

[3] The object-tracking research of Spelke (1990), Baillargeon (Wilcox and Baillargeon 1998), Carey and Xu (Carey and Xu 2001, Xu 2000) has been advertised as an investigation into the child's *concept of object*, but that research does not even purport to show that children subsume particular objects under various kinds.

a series of six pairs of pictures of cats, all different (randomly selected from a set of 18). While observing the infants through a peephole in the middle of the display, he carefully measured how much time they spent looking at each picture. Then he showed them a pair containing a novel cat and something from a different category, a human. He found that the infants looked reliably longer at the picture of the human. What he concludes from this is that the infants possess a "category representation" of cats. In other studies, he has used pictures of birds and dogs as well as cats (Quinn, Eimas and Rosenkrantz 1993), and pictures of humans, horses, fish and cars (Quinn and Eimas 1998, experiment 1B). He has also sought to identify the features of the animals on which children base their looking-time decisions (Quinn and Eimas 1996, Quinn, Eimas and Tarr 2001). Side experiments are performed to demonstrate that the infants are able to discriminate between individual exemplars and to demonstrate that the infants do not simply have a prior preference for one kind of object over another (or to provide a measure of this preference which can then be taken into account in interpreting the results of the main experiments). I should note also that Quinn thinks of himself as instilling category representations in the infants during the course of the experiment rather than as revealing category representations that they had already formed (Quinn 2002b, pp. 170-171).

Quinn formulates his conclusions with sometimes more, sometimes less caution. Here is one of his less cautious formulations (from *Current Directions in Psychological Science*):

> A lingering tradition has been to consider the acquisition of category representations to be a late achievement (i.e., of childhood or even early adolescence) that is dependent on the emergence of naming and language, the receipt of formal instruction, and the possession of logical reasoning skills …
>
> Generalization of familiarization to the novel instance from the familiar category and a preference for the novel instance from the novel category (measured in looking time) are taken as evidence that the infants have on some basis grouped together, or categorized, the instances from the familiar category and recognized that the novel instance from the novel category does not belong to this grouping (or category representation). (2002a, pp. 66-67)

In the first part of this quotation, Quinn makes clear who is on the other side, namely, those who think that language is necessary for concepts. He does not explicitly say that this is wrong, but it is clear from the context that he sets himself against it. In the second part of this quotation, the idea that the child has mentally *grouped* things is explicit. (For a more cautious formulation, see Quinn and Eimas 1996, p. 190.)

I do not believe that Quinn's experiments provide any evidence that the infants form a mental representation of a category. The results can be explained at least as well by supposing that the child is merely making the sort of three-place similarity judgment that I have described. When, after seeing a lot of cats, the child gets a picture of a human, the child, as it were, says to itself, "This thing [the human] is less like this thing [the cat with which it is paired] than any of those other things [the cats it had already been shown] were like it". Or, to put it the other way around, "Each of these things [the other cats] is more like this thing [the new cat] than that thing [the human] is like it". Assuming that, given a choice between two things, the child prefers

to look at the one that it perceives as least like the other things it has been seeing recently, we will get the looking-time results that Quinn does in fact get. (Obviously, since what I need here is an explanation that does not rest on conceptual thought, these quotations of the child's thought are to be taken only as pointing to a mental process that is not literally a tokening of a sentence in any kind of language, whether English or mentalese. The "real nature" of these so-called "judgments" might be one that we can explain only in neurophysiological terms.)

In terms of such three-place similarity judgments we might be able to explain certain features of Quinn's results even better than he can. With certain pairs of categories, his results are asymmetrical. When infants are familiarized to pictures of cats, they look longer at a picture of a human. But when they are familiarized to pictures of humans, they do not look reliably longer at a picture of a cat (Quinn and Eimas 1998, experiment 2). The same asymmetry is found in the comparison between humans and horses (experiment 1B). Similarly, when infants are familiarized to pictures of cats, they look longer at a picture of a dog; but when they are familiarized to pictures of dogs, they do not look longer at a picture of a cat (Quinn, Eimas and Rosenkrantz, 1993).

My own view is that we can easily explain the asymmetry in terms of three-place similarity judgments. Suppose we have a group of cat pictures—call it CAT—and a group of human pictures—call it HUMAN. And suppose we collect (somehow) an infant's three-place similarity judgments over all triples of the members of the *union* of these two sets. And suppose that on the basis of those we construct a multidimensional scaling (dimensions unspecified) in which similarity between the objects represented by the points is inversely related to the distance between the points. It is not at all implausible that we will find that the points representing individual humans occupy a broader region of this similarity space than the points representing individual cats, as depicted in Figure 3. (The points in the diagram represent representations in the mind of an infant, which in turn represent photographs of cats and humans, which in turn represent particular cats and humans.)

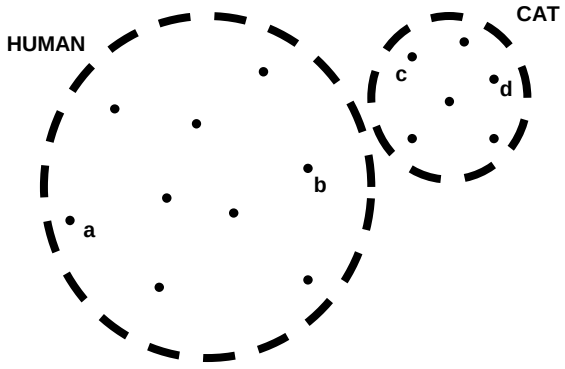

FIGURE 3

Now suppose that an infant is familiarized to pictures of cats and then is given the human/cat pair ⟨*h*, *c*⟩ (in Figure 3). Can the infant, as it were, say to itself that the novel human, *h*, is less like the novel cat, *c*, than any of the other members of the cat group is like *c*? Well, yes. So the human gets its attention. Next, suppose an infant is familiarized to pictures of humans and then is shown the pair ⟨*h*, *c*⟩. Can the infant say to itself that the novel cat, *c*, is less like the novel human, *h*, than any of the other members of the human group is like *h*? Well, no. So the novel cat does not get its attention. We can explain in exactly the same way why the infants familiarized to cats looked longer at a dog while the infants familiarized to dogs did not look longer at a cat, on the plausible assumption that the points representing individual dogs occupy a broader region of similarity space than the points representing individual cats.

In reply to this explanation, someone might object that in giving this explanation I myself have posited mental representations of the HUMAN category and the CAT category. But in fact, I have not done that. In representing the distribution of points in similarity space, I drew a circle around the dots representing pictures of humans and a circle around the dots representing pictures of cats. But those circles are just to indicate to the reader which points are which. Those circles may represent nothing psychologically real in the infants at all. My claim is that Quinn's results can be explained on the supposition that the infants possess a seamless, boundary-less, similarity space expressed in their similarity judgments. There is no need to suppose that they somehow group the humans together within one boundary and the cats together within another.

Quinn has his own explanation of the asymmetries. Quinn's explanation of the cat/dog asymmetry is that, for the infants, the category for the cats is strictly *included* within a broader category that also includes all the dogs (2002b, p. 174). He considers this to be a plausible hypothesis inasmuch as an analysis of the pictures used in his experiments reveals that, when objective measurements are taken of properties such as eye separation and ear length (in the pictures, not on the animals), "in almost all cases the distribution of each dog trait … subsumed the distribution for the corresponding trait for cats" (2002b, p. 174). What he means by that is that for each of these traits, most of the bell curve representing the distribution for cats falls within the bell curve representing the distribution for dogs.

To this, I say three things. First, this explanation of the asymmetry assumes that when the infants look at the pictures they do not see the pictures as representations of animals having characteristic dog size and characteristic cat size. If they do see the pictures as pictures of animals with their characteristic sizes, then they will surely not think of the eye-separation distances for the cats as falling within the range of the eye-separation distances for the dogs. Second, this explanation of why the infants familiarized to dogs do not look longer at a cat threatens to undermine Quinn's explanation of why the infants familiarized to cats look longer at dogs. If the category for the cats is strictly included within the category for the dogs, then we should find some dogs that fall within the cat category, and the infants familiarized to cats will not look longer at those dogs. Quinn does not report any such result. Third, if we do obtain

such a result and so are justified in thinking of the region of similarity space containing the cats as strictly included in the region containing the dogs, then we will be able to explain the data in terms of three-place similarity relations without positing any mental boundary around the cat region: Object *x* rather than object *y* elicits the infant's attention if and only if *x* is less like the things that the infant has been looking at recently than *y* is.

In any case, Quinn believes he needs to give a different explanation of the cat/human asymmetry. He thinks he cannot explain that asymmetry in the same way he explains the cat/dog asymmetry because when undergraduate subjects were asked to rate the typicality of the members of the three sets of pictures, it emerged that they regarded the humans as *less* variable than the cats and the dogs (Quinn and Eimas 1998, p. 155; Quinn 2001b, p. 178). In explanation of the cat/human asymmetry, Quinn says various things. One thing he says is that "the representation of humans in psychological space is extremely broad" so that animals such as cats and horses "would often not be sufficiently distant or distinct in psychological space from the many represented human exemplars to be preferred during test trials". However a human will seem novel relative to cats and horses "as a consequence of the relatively narrow representation of cats and horses" (Quinn and Eimas 1998, p. 155). Another thing Quinn says is that the representations of humans are "individualistic in nature, that is to say, exemplar based, whereas the representations for cats and most likely for horses are based on some form of summary representation" (Quinn and Eimas 1998, p. 164). Also: "The extensive representation of humans would function as a perceptual magnet or reference point that attracts other species" (Quinn and Eimas 1998, p. 155). (See also Quinn 2001b, pp. 178-179 and 186-187.)

In reply, I say, first, that I am not sure we can take the undergraduates' typicality ratings of humans as a good indication of humans' actual variability in psychological space. It is not hard to think of reasons why an undergraduate would be reluctant to rate a picture of, say, an Asian-American as not a typical human being, other than regarding humans as actually less variable than cats. Second, Quinn nowhere explains in literal language what the "magnet" metaphor is supposed to mean or how the magnet idea, whatever it is, can be used to explain his data. His claim that representations of humans are "individualistic in nature" is supposed to be supported by the finding that infants familiarized to human faces look longer at novel human faces than at faces they have already seen (2001b, p. 180), whereas the same does not hold for cats. But in his original report he can only say that this effect "approached significance" (Quinn and Eimas 1998, p. 164), not that it really was statistically significant, and in any case, the effect may be another consequence of the fact that humans occupy a broader region of psychological space than cats. Finally, Quinn's hypothesis that the humans occupy a broader region of psychological space conflicts with his own claim that the humans are regarded as less variable than the cats. If we assume that the humans occupy a broader region of psychological space than the cats, then we can explain his data in just the way I have done in terms of three-place similarity judgments.

4. Thought without boundaries

My alternative explanation of Quinn's results leads to the following question: When do minds need to draw boundaries between points in their similarity spaces? There are various occasions for this, in fact. A squirrel needs to distinguish between a branch that is just close enough to leap to and another branch that is slightly too far. A baby needs to decide when it has waited long enough to be fed. In the present context, an important case is deciding to use a word. If I have in mind something that I want you to bring, I may have to draw a line between chairs and stools, so that I can decide whether to use the word "chair" or the word "stool". Similarly, I might need to draw a line in order to decide between "nail" and "tack", or "tree" and "shrub".

Some of these boundaries are quite ephemeral. The squirrel can jump farther when it is not too tired, or too heavy. The baby can wait longer when it is not too hungry. Perhaps only the use of words requires mental boundaries that are more or less permanent, so that everyone who uses a word can count on everyone else to draw the boundary in roughly the same place. This observation suggests a radical hypothesis, one that defines the opposite end of the spectrum of opinion from that of the basic assumptions with which we began. This hypothesis, which I am about to state, can serve as the target that proponents of the basic assumptions need to shoot down, on the basis of experiment and observation viewed in the light of diverse conceptions of the mind. Here it is: *Acquiring a concept is the very same thing as learning a word.*

This hypothesis raises in turn a question about the nature of cognition. As I observed at the start, the basic assumptions about language learning stem from a more general conception of the mind according to which all thought is conceptual thought. If we reject the basic assumptions, then we will have to posit some other kind of mental-processing that could yield, among other things, language learning. So the further question is: what kind of thinking might that be? Here I have hinted at one kind of alternative, in the distinction that I have drawn between conceptual thought and similarity judgments. Generalizing from this one example, I contend that we might be able to explain a lot of problem-solving in terms of a kind of nonconceptual, *imagistic* thought. In particular, we might appeal to such imagistic thought to explain how the rudiments of language are acquired.

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