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Précis of *How Children Learn the Meanings of Words*

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Abstract: Normal children learn tens of thousands of words, and do so quickly and efficiently, often in highly impoverished environments. In *How Children Learn the Meanings of Words,* I argue that word learning is the product of certain cognitive and linguistic abilities that include the ability to acquire concepts, an appreciation of syntactic cues to meaning, and a rich understanding of the mental states of other people. These capacities are powerful, early emerging, and to some extent uniquely human, but they are not special to word learning. This proposal is an alternative to the view that word learning is the result of simple associative learning mechanisms, and it rejects as well the notion that children possess constraints, either innate or learned, that are specifically earmarked for word learning. This theory is extended to account for how children learn names for objects, substances, and abstract entities, pronouns and proper names, verbs, determiners, prepositions, and number words. Several related topics are also discussed, including naïve essentialism, children's understanding of representational art, the nature of numerical and spatial reasoning, and the role of words in the shaping of mental life.

Keywords: cognitive development; concepts; meaning; semantics; social cognition; syntax; theory of mind; word learning

> When [my elders] named any thing, and as they spoke turned towards it, I saw and remembered that they called what they would point out by the name they uttered. And that they meant this thing and no other was plain from the motion of their body, the natural language, as it were, of all nations, expressed by the countenance, glances of the eye, gestures of the limbs, and tones of the voice, indicating the affections of the mind, as it pursues, possesses, rejects, or shuns. And thus by constantly hearing words, as they occurred in various sentences, I collected gradually for what they stood; and having broken in my mouth to these signs, I thereby gave utterance to my will.
> – St. Augustine

The average English-speaking 17-year-old knows more than 60,000 words. Since children start learning their first words by about their first birthday, this comes to over ten new words per day. These can be acquired without any training or feedback; children can grasp much of a word's meaning after hearing it in the course of a passing conversation. Deaf and blind children learn words, as do those who are neglected and abused. In some cultures, parents make no efforts to teach their children to talk, but these children nonetheless also learn words. There is nothing else – not a computer simulation, and not a trained chimpanzee – that has close to the word learning abilities of a normal 2-year-old child.

Here is how they do it: Young children can parse utterances (either spoken or signified) into distinct words. They think of the world as containing entities, properties, events, and processes; most important, they see the world as containing *objects.* They know enough about the minds of others to figure out what they are intending to refer to when they use words. They can generalize; and so when they learn that an object is called "bottle" and an action is called "drinking," they can extend the words to different objects and actions. They can also make sense of pronouns and proper names, which refer to distinct individuals, not to categories; and so they understand that "Fido" refers to a particular dog, not to dogs in general.

These capacities improve in the course of development. Children become better at parsing utterances into words, at dividing the world into candidate referents for these words, and at figuring out what other people are thinking about when they speak. Months after their first words, they possess enough understanding of the language to learn from linguistic context, exploiting the syntactic and semantic properties of the utterances to which new words belong. This enables the learning of many more words, including those that could only be acquired through this sort of linguistic scaffolding.

This is how children learn the meanings of words. Although some of this might sound obvious, it is not the received view in developmental psychology. Many scholars would argue that some critical components are missing; an adequate account requires special constraints, biases, or principles that exist to make word learning possible. Others would argue that I have attributed far too much to children;

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simpler mechanisms suffice. Even those who are sympathetic to this approach can justly complain that it is terribly vague. What is meant by "object"? How important is children's understanding of other's intentions? What sorts of words are learned through syntactic support? Under what circumstances are children capable of word learning? And so on.

The goal of *How Children Learn the Meanings of Words* (henceforth *HCLMW*) is to fill out the details of the above account, to provide a substantive and explanatory account of word learning. More specifically, *HCLMW* is an extended defense of the claim that word learning emerges from the interaction of different capacities that humans possess – social, conceptual, and linguistic. These capacities are powerful, emerge early, and are to some extent uniquely human, but they are not special to word learning. I'll illustrate this perspective here by showing how it addresses some fundamental questions about words and how they are learned.

1. What determines the sorts of words that children first learn?

Whether a child is raised in the highlands of Papua New Guinea or amongst the cafes of Harvard Square, first words have a similar flavor. They include proper names for people and animals, and common nouns such as *ball* and *milk.* They include names for parts, like *nose,* modifiers, like *hot,* words that refer to actions, such as *up,* and words that are linked to social interactions, such as *good-bye.* To give a feeling for this, at the age of 15 months, my son Max knew the following words: *airplane, apple, banana, belly-button, book, bottle, bye bye, car, daddy, diaper, dog, eye, kitty, light, mommy,* and *uh oh.* This is a conservative list; these are the words he used many times in different circumstances, without the need for any prompting.[1]

At first blush, there is nothing to explain here. These are the words he heard, after all. And, not coincidentally, they correspond to notions that a child would be expected to understand. You would not expect a 1-year-old to start off with the words *mortgage* and *conference.* Children do not often hear such words and do not understand what mortgages and conferences are. Bottles and diapers are often discussed, and easily understood, and so names for these things are the sorts of words that are first learned.

But it is not that simple. The character of children's vocabularies cannot be predicted solely on the basis of the words they hear and the concepts they possess. For instance, children's vocabularies contain more names for objects, words such as *dog, cup,* and *ball,* than are present in the speech that is directed to them. This is true across languages and cultures. And when taught a new word in the presence of an object, children are prone to interpret it as labeling the entire object, not a part of the object, a property of the object, or the action that the object is taking part in. These facts have led many scholars to conclude that children possess a whole object bias, a preference to interpret novel words as referring to object categories (e.g., Macnamara 1972; Markman 1989).

Where could such a bias come from? It might be a constraint on learning, either innate or learned, that applies specifically to the interpretation of novel words. It might be a bias of mentalistic interpretation; that is, children assume that word-users are prone to refer to whole objects. Or, the roots of this bias could be syntactic: children might assume that nominals (or nouns, or count nouns – theories differ) refer to kinds of whole objects.

Each of these proposals has its supporters, but they are all too narrow in scope. The whole object bias is not limited to words, or to nominals; it does not apply only to our inferences about the thoughts of others. Spelke (e.g., Spelke 1994) has found that prelinguistic infants are strongly biased to parse the world into discrete bounded entities; these entities ("Spelke objects") correspond precisely to the notion of "object" used in theories of language acquisition. Similarly, there is a strong object bias in linguistic counting, and, earlier, in children's nonlinguistic enumeration of entities in the world. In fact, the number data motivated Dehaene (1997) to present his own version of the whole object bias: "The maxim 'Number is a property of sets of discrete physical objects' is deeply embedded in [babies'] brains." In light of these findings from outside the domain of language, the bias towards objects is best seen as a general stance towards the world, one that is manifested in several areas, including perception, numerical cognition . . . and word learning.

Objects are special, but this does not mean that object names are all that children can learn. In fact, such names typically constitute a minority of a child's vocabulary. (Max was a bit unusual in this regard). Young children also possess words that refer to actions, properties, and substances, as well as to parts, collections, holes, and other non-object individuals. Children's bias towards objects reflects a bias, not a conceptual limitation. In fact, adults show precisely the same bias; when placed in an experiment involving the interpretation of a new word, you would also tend to favor the object interpretation, even though you are fully capable of reasoning about parts, properties, collections, and so on.

How do children overcome this bias? They can learn a name for a non-object individual, such as *nose* or *family,* with little trouble, so long as the word is used when a candidate whole object is not present, when pragmatic cues dictate the word is not an object name, or when other individuals are made salient enough. Chapter 4 goes into some detail as to what makes some individuals more cognitively natural than others, for both children and adults (see also Giralt & Bloom, 2000).

When children hear a new word that refers to a specific object, such as a dog, they are faced with a choice. Does the word refer to a kind, as with common nouns such as *dog* and *animal,* or does it refer to an individual, as with a pronoun such as *he* or a proper name such as *Fido.* This information is not present in the input in any direct sense, and yet children always seem to get it right.[2] Chapter 5 explains how they do this. They attend to certain key differences between these types of words; such as their syntax ("I saw *the* dog" vs. "I saw Fido/him"), and the type of entities they refer to (proper names and personal pronouns tend to refer to animate entities; common nouns and other pronouns have no such restriction). Once a child knows whether a name refers to a kind or to an individual, there is the further problem of understanding the word's precise meaning, a topic that is pursued in Chapter 5 (for pronouns and proper names) and Chapter 6 (for common nouns).

Some words will never show up in children's early vocabularies. This is in part for the banal reason that children do not hear the words and do not possess the corresponding concepts. But it is also because there exist logical dependencies within language learning. No child will start off learning the meanings of *many, the,* and *some,* even if they hear these words and possess the corresponding notions. This is because in order to understand what these words mean, the child needs some inkling of the meanings of the common nouns that surround them. One can learn *dog* just by hearing it in isolation, but *many* requires a grasp of the larger semantic context to which it belongs. (The extent to which this is true of other types of words is a question we will turn to below.)

It turns out that the nature of children's first words has little to do with the fact that they are children. Rather, it has to do with the fact that they are people who don't yet know the language that everyone around them is using. Imagine being transported, with all your intelligence and memories intact, into the body of a 12-month-old raised in a foreign country. Like a normal child, you would have a whole object bias. You would be limited to learning only the words you hear. And you would be subject to the logical dependencies inherent within a language. So your first words might well be: *diaper, bottle, kitty,* and so on. (Your sole advantage would be your conceptual sophistication, so if you saw someone point to a modem and say the word for it, you might, unlike a normal child, learn the word *modem.*) In support of this, when adults are shown videos of parents interacting with toddlers, in which the words that the parents are using are replaced with beeps, adults tend to learn (and fail to learn) the very same words that the toddlers do (Gillette et al. 1999).

2. What determines the time-course of word learning?

If you read a textbook summary of word learning, you will learn the following: Children's first words have bizarre, non-adult meanings, and are learned slowly. Then, at about 16 months, or after learning about 50 words, there is a sudden acceleration in the rate of word learning. This is known as a naming explosion, vocabulary explosion, vocabulary spurt, or word spurt. Children now have a knack for word learning; and learn five, ten, or even twenty new words a day.

In Chapter 2 of *HCLMW,* I suggest that none of this is true. I am not denying that young children use words in odd ways. For instance, Bowerman (1978) notes that her daughter Eva used *moon* to talk about, among other things, a half grapefruit, the dial of a dishwasher, and a hangnail. Clark (1973) gives the example of a child who called a doorknob *apple.* And Max would put objects on his head and happily describe them as "Hat!" Adults don't talk this way.

But such examples can be explained without positing any qualitative difference between children and adults. Some might be speech errors, slips of the tongue. Others might not be errors at all. When a child calls a doorknob "apple," it could mean that the doorknob is *like* an apple. This is especially likely if children do not know the right word for what they wish to talk about, and lack the linguistic resources to use phrases such as "is like a. . . ." And children are prone to mischief – when Max put a slice of green pepper on his head and called it a hat, he found the situation hilarious, much more so than normal hat-naming. Furthermore, these odd usages are the exception. Large-scale studies of early language suggest that errors are rare and, when they do occur, they can be explained in terms of immature understanding, as when a child thinks that a cat should be called "dog" (e.g., Huttenlocher & Smiley 1987).

Two other facts attest to the sophistication of children's first words. First, one sign that children have a mature understanding of the referential nature of words is when they point at things and ask about their names. Two-year-olds can ask "What's that?", but younger children can ask the same question by pointing and saying something like "Wha?," "Tha," or "Eh?". (Max said "Doh?"). In Nelson's (1973) seminal study of 18 children's first words, she found that most had a word that was used in just this way before they learned 50 words – and six of the children had one among their first *ten* words.

Second, even very young children are surprisingly good at learning words. In one study by Woodward et al. (1994), 13-month-olds were told the name of a novel object nine times in a five minute session ("That's a tukey. See, it's a tukey. Look, it's a tukey . . ."). Another novel object was present and was also commented on ("Oooo, look at that. Yeah, see it? Wow, look at that. . . ."), but was not named. When later asked to point out "the tukey," they could do so better than chance – even after a 24-hour delay.

What about the word spurt? Does such a thing actually exist? There are countless studies that discuss this event, but these define it as a point at which children learn words at a certain *rate,* typically about 10 or more words in a 2–3-week period. By this definition, a spurt does exist and usually occurs in the second year of life. Given the size of the adult vocabulary, its existence is a mathematical necessity – in order to end up with tens of thousands of words, children must go through a lengthy period in which they are learning more than three words per week.

The problem with this definition is that it has nothing to do with a spurt (or burst or explosion) in any normal sense of the term. Instead of a sudden transition in word learning, children might reach this point of word learning as the result of a gradual increase in rate. Elman et al. (1996) suggest that this is what happens in normal development: the pace of vocabulary development exhibits a gradual linear increase. The data I summarize in Chapter 2, as well as more recent analyses (Ganger & Brent 2001), support the conclusion that this is true for most if not all children.

This is more than a pedantic point about the proper use of words such as "spurt"; it means that a common view of word learning should be abandoned. It is not that children start off learning words slowly and – boom! – they speed up to an adult rate. All the theories that posit some special event at this point in development – some nominal insight, conceptual change, restructuring of phonological memory, or shift to the use of syntactic cues – are explaining a phenomenon that does not exist. The real story of the course of word learning is more pedestrian: children start off slow and gradually get faster. As far as we know, this continues until puberty: A 2-year-old is learning words faster than a 1-year-old, a 3-year-old faster than a 2-year-old . . . and a 9-year-old is learning words faster than an 8-year-old (see Anglin 1993). There is no evidence for a qualitative shift.

Why does the rate of word learning increase as children get older? This is in part due to increasing experience with the language, which opens up the space of possible words that can be learned. A young child – like an adult learning a second language – is restricted to words whose meanings can be inferred through perceptual experience. As she learns more of the language, she gains access to linguistic information relevant to word learning. Later on, literacy exposes children to more words, and it is likely that the gargantuan vocabularies of some adults (over 100,000 words) could not arise without the ability to read.

But some of the reasons for the increase in rate have to do with developmental differences that would not apply for an adult learning a second language. As children get older, they get better at picking up words from context, at figuring out what people are intending to say when they use words, and at understanding the meanings of such words. Like any other skill, word learning is more efficient after several years of practice. And older children and adults are better in general at tasks that involve demands on attentional resources. For all of these reasons, 2-year-olds are slower word learners than 5-year-olds, and 5-year-olds are slower than 10-year-olds.

The rate of word learning has to stop increasing sometime; adults are not learning hundreds of words a day. This is not because of some "critical period" for word learning; there is no evidence that children learn words quicker or more efficiently than adults. It is because we run out of words. Unless we learn a new language, our only opportunities for word learning are proper names, archaic or technical terms, or new words that enter the language. Most 5-year-olds are exposed to many new words in the course of a day; most 25-year-olds are not.

3. How can we explain individual differences?

The above generalizations about the character and rate of early word learning might make some people uncomfortable, since I am talking as if all children use the same words, and develop in the same way. In fact, even if we restrict our focus to normally developing children raised in Western cultures, children do differ. Some learn words early and quickly; there are 18-month-olds who produce hundreds of words. And there are those who hardly speak at all until their third birthdays, and yet end up normal language users. Some adopt a referential style – first learning single words, mostly nouns, and then combining them into sentences and phrases. Other children are more expressive, starting off with memorized routines, such as *I want it,* and using them for social purposes.

The mere existence of individual differences tells us nothing about word learning. There is variation in any aspect of human psychology and physiology that can be measured on a continuous scale. It would be a different story if there were nothing but individual differences – if there were no generalizations that one could make about word learning – but nobody thinks that this is the case; the individual differences that exist can be seen as constrained variations on a universal theme.

This is not to deny that the study of individual differences can be of value. There is the obvious clinical question of what sort of development is a cause for alarm. And studies of individual differences might inform us about the nature of word learning; one could explore the dimensions of variation as a way to determine the number and type of distinct mechanisms that underpin normal word learning. For instance, if children learn words through different capacities than those involved in the learning of syntax, then one might expect to find dissociations between the two types of learning. There should be children who are good at word learning but bad at syntax, and vice versa. Or, one might find that the age of onset of certain syntactic milestones is highly heritable, while this is not the case for milestones of word learning – or vice versa.

Some of this work has generated interesting results (e.g., Fenson et al. 1994; Ganger et al. 1997), but I want to end on a dour note. Nobody knows how to explain individual differences. We do not yet know why they exist, and we do not know what causes them. The usual suspects in developmental psychology – sex, birth order, social class – have only small predictive power when it comes to explaining variation in early word learning (see Fenson et al. 1994). The only good predictor of children's rate of vocabulary learning is the vocabulary size of their parents, and this can be explained in several ways – direct and indirect effects, child-adult causation or adult-child causation. I do not think we will learn more by collecting more data. The real problem here is that the dimensions of variation that we look at are crude – we count words, measure the length of sentences, compare the ratio of nouns to verbs, and so on. We do this because we do not know what else to do. We will only come to understand individual differences in the context of a mature theory of how word learning works in general.

4. What is the role of the input in word learning?

Children who are raised in situations in which nobody is trying to teach them language nonetheless come to know the meanings of words. Even in the happiest and most supportive families, words are not always used to refer to what children are attending to, and yet mapping errors are virtually non-existent. And there is abundant experimental evidence that children can learn object names when they are not attending to the object that is being named by the adult. Children learn words, but they do not need to be *taught* words.

On the other hand, many Western parents do try to teach their children new words. They engage in "follow-in" labeling, in which they notice what their children are looking at and name it. They have an implicit understanding that children assume new nouns will be basic-level names, such as *dog* or *shoe,* and so, when presenting children with nouns that are not basic-level names, they use linguistic cues to make it clear that the words have a different status. For instance, when adults present part names to children, they hardly ever simply point and say "Look at the ears." Instead, they typically begin by talking about the whole object ("This is a rabbit . . .") and then introduce the part name with a possessive construction (" . . . and these are his *ears*"). Similar linguistic support occurs for subordinates ("A *pug* is a kind of dog") and superordinates ("These are *animals.* Dogs and cats are kinds of *animals*").

It would be perverse to say that all of this careful behavior on the part of adults is a waste of time. The argument that runs through *HCLMW* (see especially Ch. 3) is that children are remarkably good at figuring out the thoughts of adults. Does it make sense to claim that adults are so comically inept at figuring out the thoughts of their children that they go through elaborate, useless, efforts? It is more likely that parents know what they are doing. And in fact, there is considerable evidence that children do learn words best when the words are presented in just the circumstances that parents tend to teach them.

The natural conclusion here is that these naming patterns on the part of adults really are useful, they just aren't necessary. Environments differ in how supportive they are, and word learning is easier when speakers make the effort to clarify their intent and exclude alternative interpretations. But children are good enough at word learning that they can succeed without such support. (In this regard, as in many others, first language learning by children is similar to second language learning by adults.) This leads to the obvious prediction that children raised in environments in which this support is present should learn words faster than those in which it is absent. Although there is some anecdotal evidence that children raised in societies without object labeling are somewhat slower at word learning than those raised in most Western societies (Lieven 1994), there is as yet no systematic research into this issue.

5. What is fast mapping?

One striking fact about word learning is that young children can grasp aspects of the meaning of a new word on the basis of a few incidental exposures, without any explicit training or feedback. The classic study was done by Carey and Bartlett (1978). They casually asked 3- and 4-year-olds to walk over to two trays, a blue one and an olive one, and to "Bring me the chromium tray, not the blue one, the chromium one." All of the children retrieved the olive tray, correctly inferring that the experimenter intended "chromium" to refer to this new color. Even six weeks after hearing the new word, children typically retained some understanding of its meaning, if only that it was a color term. This process of quick initial learning has been dubbed "fast mapping."

Fast mapping is cited as evidence for the power, and uniqueness, of children's word learning. But does the same ability show up when children are learning items other than words? Markson and Bloom (1997) have pursued this question in a series of studies designed to be similar to the original Carey and Bartlett study. In one part of the study, three-year-olds, four-year-olds and adults were exposed to ten objects, and participated in a sequence of activities in which they were asked to use some of the objects to measure other objects. For one of the objects, they were told: "Let's use the koba to measure which is longer . . . We can put the koba away now." Subjects were not asked to repeat the word, and there was no effort made to ascertain whether they even noticed that a new word was being introduced. During the test phase, subjects were presented with the original array of ten items, and asked to recall which object was the koba ("Is there a koba here? Can you show me a koba?"). We found that even after a month, subjects tended to remember which object was the "koba," replicating and extending the original fast mapping findings.

The second part of the study involved the learning of a nonlexical fact. We told them: "Let's use the thing my uncle gave to me to measure which is longer . . . We can put the thing my uncle gave to me away now." Then they were tested with: "Is there something here that my uncle gave to me? Can you show me something that my uncle gave to me?" The results here are basically identical. All age groups again performed significantly better than chance even after a month.

Further studies found that children do equally well when a fact is combined with a new word: "This came from a place called Koba." We also found that children retain the *precise* word or fact that they have been taught. If taught "koba" or "given by uncle," and later asked "Where's the modi?" or "Where the one given by my sister?", they do not point to the object that was originally taught. Other studies suggest that 2-year-olds can also fast map words and facts, and that, as with older children and adults, there is no advantage for words.

It is conceivable that there are two distinct capacities or mechanisms explaining our results, one underlying the new word task, the other underlying the nonlexical "Uncle" task. But given two patterns of learning that are virtually identical, it is more parsimonious to see them as emerging from one mechanism, and not two. This suggests that, as with the object bias, fast mapping applies across domains, with words as just one instance of this. Ongoing studies, discussed in Chapter 3, are exploring the nature and scope of this learning mechanism.

6. What is the role of theory of mind in word learning?

A good way to teach children what *dog* means to is to point to a dog and say "Look at the dog!" Why does this work so well? One proposal, advanced by Locke, Hume, and many contemporary associationists, is that children learn words through a sensitivity to the statistical co-occurrence between what they hear and what they see. Because of this, the best way to teach a child an object name is to make sure the child is observing the referent of the name at the moment you say it. In this way, the sound "dog" will become reliably associated with the perception of dogs, and the meaning will be quickly and accurately learned.

The alternative theory is presented most eloquently by St. Augustine in the quote that begins this article. This is that children learn words through their sensitivity to the referential intentions of other people, through use of "theory of mind."[3] Because of this, the best way to teach a child an object name is to make it as clear as possible that you are intending to refer to the referent of that name; and the best way to do this is to point and say the word. In this way, the child can infer that the speaker means to pick out the dog when using this new word, "dog," and the meaning will be quickly and accurately learned.

Much of *HCLMW* is a defense of this Augustinian theory. Consider, first, the circumstances under which children learn words. As discussed above, words are not always used when the child is attending to what the word refers to. Some of the time a child hears "dog," she will be looking at a dog; but some other times, she will be looking at her foot. If children were simply attending to associations, one of two things should happen. Either they should make silly mistakes, and think *dog* means foot, or they should be extremely cautious, and only guess the meaning of a word when there have been enough instances of word use so that the child could be confident that the mapping is the correct one. But children do not make silly mistakes and they are not cautious; despite the noisiness of the input, they never think *dog* means foot, and they do not need multiple trials to learn the word. This suggests that children attend to an information source that is more robust and reliable than spatio-temporal contiguity.

In particular, there is a rich body of research suggesting that young children are exceptionally good at using mentalistic cues – such as eye gaze and emotional expression – to learn nouns and verbs, and can do so even when these cues conflict with information provided by the statistics in the scene. To take an example from a classic study by Baldwin (1991), imagine a child who is staring at a novel object when hearing a speaker say "Toma! There's a toma!" One might imagine, following Locke, that the child would naturally connect the word with the object she's looking at. And she will – but only if the speaker is also looking at that object. If the speaker is gazing in another direction, at another object, the child will follow the speakers' gaze and assume that this different object is the referent of the word. It is presumably though such a process that children avoid serious mapping errors.

Theory of mind does more for the child than solving the mapping problem. In Chapter 3, I propose that some of children's basic assumptions about word learning, such as the belief that words will be arbitrary and bi-directional ("Sausserian") signs, or that words do not have overlapping reference ("mutual exclusivity"; Markman 1989), follow from their understanding of the mental states of people who use words. A further role of theory of mind concerns children's understanding and naming of human-made creations. One example of this, discussed in Chapter 7, concerns representational art. When young children name pictures, by themselves and by others, they name them based on the intent of the artist, and do not rely on what the picture looks like. Even something as apparently simple as a 2-year-old pointing to a scribble and calling it "Mommy" involves an impressive act of mentalistic attribution.

To appreciate the importance of theory of mind, consider what happens when it is missing. In *HCLMW,* I discuss three populations who do not learn words – nonhuman primates, autistic children, and pre-linguistic children. I suggest that in all of these cases, the problem in word learning lies in the failure to appreciate the representational intentions of other people. For babies and chimpanzees, this deficit is so extreme that it entirely precludes word learning. The autism example is more nuanced, as autistic individuals differ in the ability to learn language. Many do not speak at all, a few speak normally, and most fall in between. Following Frith and Happé (1994), I argue that the extent of an autistic individual's problems in word learning is a direct consequence of the extent of his or her impairment in theory of mind. With only a mild impairment, word learning is largely successful.[4] With more of an impairment, there are problems in word learning, including bizarre mapping errors and confusions in pronoun usage. And with a severe impairment in theory of mind, there is no word learning at all.

7. What is the role of linguistic cues in word learning?

Let's return to children's first words. These are learnable through observation of the world and attention to the intentional acts of word users, and include nouns such as *ball* and verbs such as *kiss.* But a child's first word could not be a determiner, modal, conjunction, or preposition. It could not be a verb such as *dreaming,* an adjective such as *former,* or a noun such as *tenure.* The meanings of these words are not accessible in the same way.

Children learn these words through the support of language. This might happen by having someone explicitly tell the child the meaning of a word, through definitions and examples. But such intentional teaching applies to a small minority of words at best. Nobody has ever learned what *the* means by having someone define it, because it is virtually impossible to define. Instead children learn its meaning from understanding phrases such as "the big dog" and inferring the semantic contribution that this morpheme makes towards the meaning of the entire phrase (see Ch. 3). For content words, other sources help. For instance, a child might hear a sentence like "I'm really annoyed that you kicked the dog," and could use the (nonlinguistic) fact that the speaker is quivering with anger to guess that the verb *annoyed* has a negative connotation, as well as the (linguistic) fact that the verb takes a sentential complement to infer that it is likely to refer to a mental state.

One can distinguish two sources of linguistic information – semantic context and syntactic context. Semantic context is provided by the meanings of the individual words and of the utterance as a whole. A child hearing "Do you want me to buy some *lobster* for dinner?" can infer from the semantics that lobster refers to the sort of stuff that one might willingly eat. Syntactic context is information provided by the syntactic category of the word. A child hearing "I would like you to clean your room" could infer from the syntax that *clean* refers to an action that one entity does to another entity.[5]

In *HCLMW,* I have little to say about learning from semantic context; there is little work on the topic, despite its obvious importance. Such learning seems to be the result of some general intelligence that has little to do with language per se. The research focus of many developmental psychologists has instead been on a far more encapsulated (and tractable) issue – the role of syntactic cues in word learning. This is the main topic of Chapter 8.

In the first study to explore the use of syntactic cues in word learning, Brown (1957) showed 3- and 4-year-olds a picture of an action performed upon a substance with an object. Some children were told "Do you know what it means to sib? In this picture, you can see sibbing" (verb syntax), others were told "Do you know what a sib is? In this picture, you can see a sib" (count noun syntax), and others were told "Have you seen any sib? In this picture, you can see sib" (mass noun syntax). The children were then shown three pictures, one depicting the same action, another depicting the same object, and a third depicting the same substance. They were asked, according to what they were initially told, "show me another picture of sibbing" (verb syntax), "another picture of a sib" (count noun syntax), or "another picture of sib" (mass noun syntax). Brown found that the preschoolers tended to construe the verb as referring to the action, the count noun as referring to the object, and the mass noun as referring to the substance, and he concluded: "young English-speaking children take the part-of-speech membership of a new word as a clue to the meaning of a new word" (p. 26).

Since this paper, there have been dozens of studies showing that young children can use syntactic cues to determine the aspects of the meanings of a word; some of this research is summarized in the table below. (For the purpose of illustration, English words are used in the examples, but the experiments themselves would use nonsense words; for instance, a child might hear "He feps the dog," and then be tested as to what she thinks "feps" means.)

Example	**Syntactic Cue**	**Usual Type of Meaning**
This is a *cat*	Singular count noun	Individual member of a category
These are *cats*	Plural count noun	Multiple members of a category
I see *John*	Lexical NounPhrase	Specific individual
Here is some *water*	Mass noun	Non-individuated stuff
He *sleeps*	Intransitive verb	Action with one participant
He *kisses* the dog	Transitive verb	Action with two participants
This is a *big* dog	Adjective	Property
The dog is *on* the table	Preposition	Spatial relationship

The fact that children can use syntax to learn aspects of the meanings of words in an experimental context raises the question of precisely what role syntax plays in real-world word learning.[6] We can quickly dismiss the idea that children can learn words solely through attention to this informational source. Syntax is far too crude; it can help the learner make broad distinctions, such as whether a word refers to an individual versus to a property, or to an event with one participant versus an event with two participants. Word meanings are much more fine-grained. Children have to learn the difference between *cup* and *dog* (both count nouns), *big* and *small* (both adjectives), and *two* and *six* (both quantifiers of precise numerosity). For this, syntax is no help at all.

Is syntax ever necessary? Plainly, some words can be learned without linguistic support. You can point to a rabbit and grunt "Rabbit!" and the word will be learned. But Gleitman (1990) has argued that syntax plays a significant role for the acquisition of certain other words, including many verbs. While a noun like *rabbit* can be learned through perceptual information and an attention to the actions of others, these information sources are inadequate for learning a verb like *thinking.* Even if children know what thinking is, the support of syntax is necessary in order to establish the mapping from this notion to the English word "thinking." This proposal is supported by experiments in which adults are exposed to much the same input as toddlers are, and have to guess the meanings of words. Adults find it harder to learn verbs such as *thinking* than nouns such as *rabbit;* and linguistic support is a large help for the verbs and no help at all for the nouns (Gillette et al. 1999).

It would be a mistake, however, to conclude that there are two types of words: those learned through nonlinguistic cues, and those learned through linguistic cues, syntactic, and otherwise. Many words are acquired through multiple mechanisms. Karen Wynn and I have looked at number words (discussed in Ch. 9) as a case-study of such learning. Number words are interesting because they are learned in two distinct stages. In the first, children learn that they correspond to numerosities of sets, but they do not know *which* numerosities they correspond to. For instance, a child might not know whether *two* applies to two entities as opposed to five entities or a hundred entities. This limited understanding emerges from a sensitivity to linguistic cues that tell the child that *two* is a number word – but nothing else. In the second stage, which can occur much later, children learn the precise meanings of the number words – they learn that *two* means two – and they do so through a developing sensitivity to extra-linguistic cues to number word meaning.

8. What are word meanings?

The assumption throughout *HCLMW* is that to know the meaning of a word is to have:

(1) a certain mental representation, or concept, that
(2) is associated with a certain form.

This is the sense of "knowing the meaning of a word" implicit in most discussions of language development, both scientific and informal. Saying, for instance, that a 2-year-old has mixed up the meanings of *cat* and *dog* implies that the child has the right concepts but has mapped them onto the wrong forms. Although there are no word meanings without corresponding concepts, there can be concepts that are not associated with forms. A child might have the concept of cat, but not yet know the word, and even proficient adult users of a language might have concepts that they do not have words for.

But what are concepts? Chapter 6 of *HCLMW* defends an essentialist theory of concept learning and representation. This differs from certain alternative accounts which posit that the mature understanding of dogs and chairs is best captured either in terms of a weighted set of features (as in prototype theories), or as a set of points in a multidimensional feature space (as in exemplar theories). While such representations might exist, I argue that any theory of concepts is incomplete if it does not acknowledge the essentialist bias that is central to our understanding of categories – the tacit belief that members of a category share their properties by dint of deeper properties that the members possess.

"Essentialist" is an epithet in some circles, but the sort of essentialism that children naturally endorse is adaptive and sensible. It is adaptive and sensible because it is true. Objects in the world are not randomly distributed with regard to the properties they possess; instead they fall into categories. These categories, such as mountains, tigers, and chairs, are the products of physical law, biological evolution, and intentional design. Such categories tend to be inductively rich, in the sense that once you know that something is a member of such a category, you know other relevant things about that entity. Examples of such categories are tigers and chairs. An example of an unnatural category is: "things that are not cows." Such a category is inductively poor; the only common property that members of this category share is that they are not cows – and so it is useless from a cognitive or scientific point of view (see also Bedford 1997).

Concepts are useful only insofar as they correspond to the inductively rich categories. Perceptual similarity is an excellent guide to the formation of such concepts, but it is not enough. To make sense of the world, to categorize entities in a maximally useful way, the child has to grasp that the category an object falls into is governed by deeper properties of the object (such as its internal structure), not solely by what it looks like. This proposal does not entail that people actually know what these deeper properties are. For instance, to be an essentialist about tigers does not require that you know the internal properties that make something a tiger, just that you believe some such properties exist. Hence, an essentialist can entertain the possibility that something might resemble a tiger but not actually be a tiger, or not resemble a tiger but be a tiger nonetheless. Some scholars suggest that essentialism arose only as the result of a scientific world-view (e.g., Fodor 1998a), but the opposite is more likely: the belief that certain entities have essences is what motivates scientific inquiry in the first place.

There is a large body of evidence suggesting that children's categorization, and their use of words, is governed by an essentialist conceptual system. Children believe that if two animals fall into the same category, they are likely to share the same hidden properties, even if they don't look alike (e.g., Gelman & Markman 1986). They know that category membership is not solely determined by appearance, and so a porcupine that has been transformed to look like a cactus is still a porcupine, a tiger that is put into a lion suit is still a tiger (Keil 1989). They know that if you remove the insides of a dog (its blood and bones) it is no longer a dog and cannot do typical dog things such as bark and eat dog food, but if you remove the outside of a dog (its fur), it remains a dog, retaining these dog properties (Gelman & Wellman 1991).

Most of the research into essentialism has focused on children's understanding of animals and animal names; in large part because essentialism within philosophy is typically restricted to so-called "natural kinds." But I argue in *HCLMW* that the same sort of essentialism holds for human-made entities, for artifacts. Here the essence is not internal, rather, it concerns the intention of the creator. This predicts that when children name and categorize artifacts they should be sensitive to what the artifact was created to be; something might not look like a typical clock, for instance, but if its design reflects the intention for it to be a clock, children will call it "a clock." In Chapters 6 and 7, I review the evidence in support of this extension of essentialism (see also Gelman & Bloom 2000).[7]

As noted above, the proposal here is that word meanings are just those concepts that happen to have word forms associated with them. Under this view, there is no "lexical semantics" separate from conceptual structure. If so, then the constraints and biases that hold for word meanings should not be lexically specific; they should instead follow from pragmatic knowledge, from cognitive and perceptual biases, and from properties of concepts themselves. In *HCLMW,* I suggest that two fundamental biases of word learning follow in a direct fashion from how concepts are understood.

The first is the bias to treat new words as referring to basic-level categories. Brown (1958a) noted that some names are more frequently used than others when talking about objects. For instance, we usually call Fido "a dog," not "an animal" or "a terrier." And when children learn new words, they most naturally treat them as falling into this intermediate level of abstraction, not too general (animal), and not too specific (terrier). What makes these basic-level meanings so special?

Brown (1958a) speculated that we describe things at the basic-level, "so as to categorize them in a maximally useful way" (p. 20). Subsequent work has led to more explicit formulations of this insight. One promising analysis is from Murphy and Lassaline (1997), who propose that the basic level is an optimal compromise between informativeness and distinctiveness: you can infer many unobserved properties once you know which basic-level category something belongs to, and it is also relatively easy to make this categorization. Hence, the focus on the basic-level reflects a conceptual bias toward carving the world into inductively rich categories; this bias, then, has its origin in conceptual structure (Markman 1989; but see Mandler & McDonough 1996, for a different perspective).

A second example is the shape bias: Many investigators have found that when given a new count noun that refers to a rigid object, children will typically extend that noun to other rigid objects of the same shape, not those of the same size, color, or texture.[8] It has been argued that this bias is special to word learning, and exists because children note the correlation between words used in a certain context and words that are extended on the basis of shape (e.g., Landau et al. 1988). The alternative, defended in *HCLMW,* is that shape is important for object names because shape is important for object categories: if two objects are the same shape, they are likely to belong to the same category.

If this is correct, then people should categorize the world into shape-based categories even when not learning words. When a child first sees a dog, for instance, she would be more likely to attribute properties of that dog to other objects of the same shape than to other objects of the same size, color, or texture. It should also follow that the shape should be sensitive to top-down information. (For instance, if two objects are the same shape, but this sameness of shape is clearly due to some factor independent of category membership, then children should *not* extend a new word from one object to the other.) Much of Chapter 6 defends the view that children's generalization of words on the basis of shape emerges from the essentialist nature of concepts, and is not special to the learning of words.

One final point about concepts: It is sometimes suggested that the conceptual system is undifferentiated prior to language learning, and that many concepts emerge through exposure to words. As I review in Chapter 10, there is surprisingly little support for this Whorfian claim, and plenty of evidence for the alternative – much of word learning involves mapping words onto pre-existing concepts. This is not to deny the banal fact that exposure to language can affect our thoughts (if not, why would you be reading this?); nor is it to deny that concepts might change over the course of development, sometimes radically. But words are, in the end, a tool for the expression of thought, not a tool for creating it.

9. How does this theory differ from alternatives?

Make everything as simple as possible, but no simpler.
– Albert Einstein

HCLMW is largely a synthesis of theory and research done by many people, and so I can be confident that at least some people will agree with at least some of what I say. But the broader conclusions that I draw are not consistent with the dominant theories in the field. As I have discussed above, many researchers believe that word learning, at least early on, can be explained in simpler terms. Simple associative mechanisms suffice. Children who hear the word "bottle," for instance, learn the word through foundational mechanisms of learning that associate this word with what is perceived at the time that it is used. Other scholars argue that the capacities I have discussed above are not enough. Instead, children must possess special constraints, assumptions, or biases that are earmarked for word learning. Children who hear the word "bottle" are guided by a constraint on word meaning that leads them to assume that the word refers to the whole bottle, and another constraint that guides them to extend the word to other objects of the same kind.

I discuss these proposals – the associationist view, and the special constraints view – in some detail in *HCLMW*, and I argue that they are mistaken. My arguments are not abstract; I am not worried that the associationist approach is too empiricist, or that the constraints approach is too nativist. The problem with these views is that they fail to adequately explain certain facts about the nature and course of word learning.

What about other theories? The proposal in *HCLMW* is consistent with the spirit of "cognitive," "syntactic," and "socio-pragmatic" theories; disagreements lie in the relative emphasis that is given to these different processes, and the specific claims as to their nature. I have nothing critical to say about connectionist theories of word learning except insofar as such theories assume that children learn words through associative learning, without theory of mind. (Unfortunately, all existing connectionist theories make this assumption.) Finally, there is some recent excitement over the claim that children exploit multiple converging cues in the course of word learning. I agree this "multiple cues" approach is the right one; but I don't know if anyone has ever doubted it. Current theories along these lines are interesting not because they insist on multiple cues, but because they make substantive claims about the precise nature of the cues and how they interact (e.g., Golinkoff et al. 1994).

HCLMW ends with a discussion of what special constraint theories and associationist theories have in common. They share the view that word learning proceeds in a reflexive manner, through "dumb associative mechanisms" or through the operation of encapsulated and specialized principles. In *HCLMW*, I defend an alternative that could best be called "Rationalist" – children learn words through the exercise of reason. They figure out what people are intending to say when they use words, and they bring all of their knowledge to bear when figuring out how a word should be understood. Word learning sometimes looks automatic, but only because children quickly become very good at this sort of reasoning. And it sometimes looks dumb, but only because, as researchers, we often put children in situations where the only right answer is the dumb one.

A careful examination of the facts of word learning reveals that the mechanisms underlying this process are rich and varied, and, at this point of time, largely mysterious. This might not be good news from the standpoint of research into this area, and this Rationalist account might be more complicated than the alternatives, but it is nonetheless the proposal that is most consistent with how children learn the meanings of words.

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NOTES

1. As with most parents and researchers, I am adopting what Lois Bloom has called "rich interpretation," and so I should admit that Max did not actually *say* any of these words – instead of "kitty," he said "gid-duh"; instead of "belly-button," he said "beh-buh," and so on.

2. Children do sometime use words like *Mommy* and *Daddy* to refer to people other than their parents. But this is not actually an error, since such words are also used as nouns, as in "my mommy" or "John's daddy."

3. The term "theory of mind" is chosen to make explicit the connection with the autism literature, but no commitment is made as to the precise nature of this capacity (such as whether it really is a theory). Also, it is sometimes said that children only have theory of mind when they can pass the notorious false belief task. I think this is mistaken (see Bloom & German 2000), but in any case, this is not how I'm using the term here; children could have "theory of mind" even if they could not reason about beliefs that are false.

4. But not *entirely* successful – even high functioning autistic individuals use and understand words in subtly different ways from normal children, especially with regard to names for representations (Bloom et al. 2002).

5. Some scholars would reject this semantic/syntactic distinction, arguing that these examples are better viewed as falling on opposite ends of a continuum. Even if one accepts that these types of context are distinct, it is difficult in practice to change the syntax without changing the semantics, and so there is controversy over the interpretation of certain experiments that purport to show that children use syntactic cues, as opposed to other linguistic information, to infer aspects of word meaning.

6. It also raises the question of how children learn the syntactic categories of words. The solution adopted in *HCLMW* is a variant of "semantic bootstrapping" (Pinker 1984); children use the meanings of some words, such as object names, to learn their syntactic category, and to learn the syntactic rules of their language. These rules enable the syntactic categorization of other words, and at this point, syntactic cues to word meaning can apply (see also Bloom 1999).

7. More speculatively, it might be that certain puzzling facts about how people use words such as *water* are due to an artifact-like understanding of the category that includes considerations of design and intent (Bloom 2002a; see also Chomsky 1995; Malt 1994).

8. These two biases are related, because members of a basic-level object kind will tend to have the same shape. But the shape bias is narrower in scope than the basic-level bias (applying only to rigid objects). Also, a child who had only a shape bias would tend to favor subordinate categories (terrier, rocking chair) over basic-level categories (dog, chair).

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Okay for content words, but what about functional items?

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Abstract: Though Bloom makes a good case that learning content-word meanings requires no task-specific apparatus, he does not seriously address problems inherent in learning the meanings of functional items. Evidence from creole languages suggests that the latter process presupposes at least some task-specific mechanisms, perhaps including a list of the limited number of semantic distinctions that can be expressed via functional items, as well as default systems that may operate in cases of impoverished input.

Bloom's book (*HCLMW*) is detailed, lucid, and laden with data supporting its claim that though the meaning of content words cannot be learned through Lockean associationism, the process requires no specially-dedicated apparatus, but rather a combination of strategies. However, functional items are words too, and here the issue is less clear-cut. The comments that follow should be interpreted less as criticism of the book itself than as a suggestion of further work that Bloom and others will have to undertake before we have a complete theory of word learning.

Bloom has very little to say about how functional items are learned. True, Chapter 5 is entitled "Pronouns and Proper Names," but seventeen pages are devoted to "Proper Names" and only four and a half to "Pronouns." If difficulty of the problems posed for acquisition was the criterion here, those proportions would be reversed. But discussion is confined to first and second person pronouns; third-person pronouns, acquired notoriously later than these, are not mentioned. Other functional items are treated in an equally cursory manner, if at all. For instance, the only mention of prepositions (pp. 204–205) involves their possible function in learning differences between noun classes; there is no discussion of how prepositions are learned in the first place. As for modals, quantifiers, complementizers, and question words, these are not mentioned anywhere.

It's not that Bloom is unaware that there is a problem; he seems not to know *what* the problem is. Discussing noun-determiner combinations (pp. 115–17), he notes that determiners may not be heard, or, if they are, that the phonological salience of the noun will distinguish it from them. Such factors, he claims "explain why it is harder to learn determiners than nouns" (p. 117). Actually, they do not explain, nor does the fact that "Nobody ever points and says 'The!' or 'Of!'" (p. 115). What really makes meanings of determiners and other functional items, bound or unbound, hard to learn is that there is a very large, if not infinite, number of semantic distinctions that they might in principle encode. In fact, the number of semantic distinctions that can be encoded, across the world's languages, by functional items (call these Potentially Encodable Distinctions, or PEDs) is finite and probably quite small. One of my dearest wishes, uttered every time I see a graduate student (but so far to no avail), is that someone would just count and list PEDs. Though strictly a no-brainer, it might seem at first sight a daunting task, but I would bet that by language No. 20 you would be getting diminishing returns, and by language No. 50 you would have the whole set or pretty close.

How do children learn the meanings of "a," "the," and other functional items? Bloom's remark that they "attend to NPs such as 'the dog' and 'the cups' and figure out the semantic role of the determiner across all of these phrases" (p. 116) renames the problem rather than solving it. What stops them from concluding that "the" and "a" embody a distinction between "rare" and "common," "pleasant" and "unpleasant," or any other pair of meanings? But Maratsos (1974) and much subsequent work shows that the meanings of definite and indefinite articles are mastered early in acquisition.

One possibility that Bloom probably won't like is that PEDs are innately specified in some way. The task would then be greatly simplified; instead of having to "figure out" semantic distinctions from a potentially infinite set, the child would simply select, from a smorgasbord provided by universal grammar, the particular set of PEDs that the target language had chosen. The task would be made easier still by the fact that PEDs would be at least partly presorted according to the grammatical categories of the lexical heads that each functional category modified. For instance, no child would expect a specific-nonspecific distinction to be marked by a modal or other verb modifier, or a realis-irrealis distinction (i.e., the distinction between events that have actually happened and those that have not) to be marked by a determiner or other noun modifier.

There is some empirical evidence in favor of such a proposal. Creole languages derive from pidgins that have very few functional items (see Roberts 1998 for historic records of the kind of pidgin input that the first creole-speaking generation in Hawaii received). Despite this, a wide range of functional items is created by creoles, most probably by first-generation children (Bickerton 1984; 1999). These include definite and indefinite markers that are both morphophonemically and semantically distinct from any equivalent items in the creole's substrate or superstrate languages. Simplifying slightly for reasons of space, many unrelated creoles have the following system: an overt article marking specific noun-phrases already established in discourse (derived usually from a superstrate demonstrative, "this" or "that" in English-influenced creoles), an overt article marking specific noun-phrases not yet established in discourse (derived almost always from the superstrate numeral "one"), and a zero form marking nonspecific (generic, undifferentiated or hypothetical) noun phrases, as well as mass nouns. In English, we have to say either "I am going to buy shirts" or "I am going to buy a shirt," even though we may not know whether we will buy one, or several, or none; in an English creole, such as Guyanese, we would say "mi go bai shirt." The only English equivalent is the cumbersome (and unidiomatic) "I am going shirt-buying." Moreover, in contrast to English, a plural is impossible with a bare (determiner-less) noun, even when this is a count noun: "di shirt-dem', "the shirts," but never "shirt-dem," "shirts."

The foregoing suggests that there is a "default" system of determiners in Universal Grammar (UG), and that children learning English pick up articles quickly because the determiner system in English, although different from the creole prototype, simply reshuffles and re-labels the handful of PEDs used by the default system. The obvious question then is, why do not children learning some article-less language (such as Japanese) try to impose articles on them?

There is more than one possible answer. First, perhaps they do try – to the best of my knowledge, no one has reviewed Japanese acquisition data to find out – but give up as soon as they see that Japanese employs a different set of PEDs. Second, perhaps more likely, children can tell the difference between a true language (with rich data and plentiful instantiations of PEDs) and a pidgin (with severely impoverished data and few, if any, instantiations of PEDs). In the former case, they keep looking in the input for things that might mark PEDs; in the latter, they realize that this would be a waste of time, and go with the default system.

Neither of these answers may be correct, there may not even be a default system, but at least the proposal has more substance than anything Bloom offers for functional items. It is significant that his book contains no mention of the acquisition problems faced by the first generation of creole speakers (Bickerton 1999) or, even more critically, by the creators of new sign languages (Kegl et al. 1999). If he had looked at these, he might not, inter alia, have stated so confidently that "syntax is an important informational source as to the meanings of words" (p. 212). Like motherese, syntax is a handy tool to have, but you can get along fine without it.

But Bloom throughout covers only "normal" acquisition cases, where input is rich and well-formed. He should remember that if hard cases make bad law, normal cases make bad science. Unlike our colleagues in the physical sciences, we in the behavioral sciences cannot put nature through a wringer with our experiments, using them to create not just unusual cases but circumstances that might never arise in the natural world. All the more reason for us to exploit to the full the few "natural experiments," flawed, incomplete, and subject to interpretation though they be, that nature has afforded us.

Concept modeling, essential properties, and similarity spaces

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Abstract: Bloom argues that concepts depend on psychological essentialism. He rejects the proposal that concepts are based on perceptual similarity spaces because it cannot account for how we handle new properties and does not fit with our intuitions about essences. I argue that by using a broader notion of similarity space, it is possible to explain these features of concepts.

In Chapter 6 of *HCLMW,* Bloom discusses the concepts expressed by common nouns. He argues that such concepts are based on a form of psychological essentialism (Medin 1989; Medin & Ortony 1989). As an alternative account he considers the "minimalist proposal" that concepts are based on perceptual similarity spaces. He rejects this account on two grounds: it cannot explain how we handle new properties, and it does not fit with our intuitions about essences.

Bloom seems to take for granted what is meant by "perceptual similarity." However, I believe that there is no purely perceptual notion of similarity. I want to argue that by using a broader notion of a similarity space, what I call a "conceptual space," it is possible to account for new properties and explain why our concepts seem to be based on some form of essences.

Bloom's first argument against the reduction of concepts to perceptual similarity spaces is that abstract concepts like "stockbroker," "menu," and "swearing" cannot be analysed in terms of perceptual dimensions alone. I basically agree with this statement, but it must be qualified. Firstly, it is not clear what exactly Bloom means by "perceptual dimension" or "perceptual similarity." It has been a long time since empiricist philosophers believed that there is a well-defined perceptual level of describing the world. There is no such thing as a naked eye. As Popper argued, even a seemingly innocent empirical statement like "This is a glass of water" is loaded with theoretical (nonperceptual) assumptions concerning the meaning of "glass" and "water."

Secondly, and more importantly, our judgments of similarity are seldom based on perceptual similarity alone. When we assess the similarity of different phenomena, we take "hidden" factors into account, such as forces and other causal variables. Also, the functions of objects partly determine our similarity assessments. By limiting his argument to perceptual similarity spaces, Bloom imposes a straitjacket on the role of similarity.

Bloom's second argument concerns the role of "psychological" essentialism in the formation of concepts. In modern psychological theories (e.g., Osherson & Smith 1981; Rips 1995), "core" properties are supposed to determine the meaning of concepts while other "peripheral" properties (Kövescezs 1993) only have diagnostic value. The core consists of those properties that are essential to a concept, whereas the peripheral properties do not determine whether something belongs to the concept or not, – though they may be helpful in "identification procedures" (Osherson & Smith 1981). Sometimes, the distinction is formulated in terms of perception versus conception: perception delivers the peripheral properties, which may serve as cues for conception where the essential properties are represented.

On the other hand, essentialist theories have been criticized as involving unnecessary metaphysical assumptions. A way out of the conflict is provided by Medin (1989, p. 1477), who gives the following explanation of why people have a tendency to behave as if psychological essentialism is valid:

> If psychological essentialism is bad metaphysics, why should people act as if things had essences? The reason is that it may prove to be good epistemology. One could say that people adopt an essentialist heuristic, namely, the hypothesis that things that look alike tend to share deeper properties (similarities). Our perceptual and conceptual systems appear to have evolved such that the essentialist heuristic is very often correct.

Bloom wants to explain how we can understand abstract concepts by applying a "theory-theory." This means that concepts should be thought of as embedded in knowledge that contains theories of the world (Murphy & Medin 1985). The "theory-theory" is closely related to theories of psychological essentialism: the theories express essential properties of the concepts.

Following the ideas in my recent book on conceptual spaces (Gärdenfors 2000), I propose that by exploiting similarity spaces that involve non-perceptual (theoretical) dimensions, one can account for the "essentialism lite" that Bloom (pp. 167–69) presents as a part of his analysis of concepts. (For a similar position, see Hampton 2001.)

A key idea is that the role core properties play in essentialist theories of concepts can be handled by considering the salience of different dimensions. A "core" (essential) property of a concept is a property that belongs to a dimension with high salience, while a "peripheral" property is associated with a dimension with lower salience. Radical essentialism, then, corresponds to assigning extreme salience to some "essential" dimensions when determining the content of a concept. Furthermore, the salience weights of different dimensions vary, depending on the context. Hence, what seems like a core property in one context may seem peripheral in another. The problems philosophers and psychologists have had in identifying the essential properties of concepts is a symptom of the fact that there is no sharp border between core and peripheral properties.

Once some dimensions other than the basic perceptual dimensions are given higher salience weights, then there can be no simple perceptual account of similarity. For example, folk botany may classify plants according to the color or shape of the flowers and leaves, but after Linnaeus the number of pistils and stamens became the most important dimensions. And these dimensions are perceptually much less salient than the color or shape dimensions. The general trend in the development of the concepts of a domain is towards less and less dependence on perceptual similarity.

Most of the role that the theories are supposed to play in representations of concepts can be taken over by those dimensions that have the highest salience. In many cases these dimensions may not be perceptual, but indeed correspond to what is conceived of as theoretical entities in science. (Medin & Ortony [1989] suggest that people's beliefs in ultimate causes of category membership are sometime so strong that such a causal variable can function as an unidentified placeholder in the core of a concept.) From this perspective, there is no fundamental conflict between a similarity-based account and a theory-theory based account. The upshot is that in theories too, there is similarity behind the scene. Hahn and Chater (1997, p. 50) conclude: "Thus theory-based views demand a better account of similarity, rather than no account of similarity in explaining concepts."

Word meaning, cognitive development, and social interaction

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Abstract: This review proposes that Bloom's linkage of word meaning with more general cognitive capacities could be extended through examination of the social contexts in which children learn. Specifically, the child's developing theory of mind can be viewed as part of the process by which children learn word meanings through engagement in social interactions that facilitate both language and strategic behaviours.

The publication of this book restores the study of word meaning to a central place in the study of language, especially since it links the learning of word meaning to more general emerging cognitive abilities rather than postulating a nativist or an empiricist theory. While the comprehensiveness of Bloom's coverage cannot be doubted, it is the exclusion of what is an important theoretical stance that makes the book appear lop-sided. Pitting Bloom's view that word learning is a manifestation of other more general cognitive abilities against a nativist (bias or constraint) view is akin to Tomasello's (2001) polarisation of the nativist view and the social pragmatic approaches.

An argument that does not dichotomise the theoretical positions can be mounted (see also Baldwin & Moses 2001). Social interaction has a fundamental role in the development of knowledge, in particular, knowledge of symbolic systems such as spoken language (including the meaning of words) and written language. Vygotsky (1962) specified that social interaction created language and that the integration of different tool (practical) and sign (symbolic) systems by the child gave rise to new and increasingly sophisticated behaviour. This view highlights both the social and the constructivist nature of the child's developing knowledge.

Bruner (1983) developed the notion that language is best encouraged and developed in a functional way through social interaction. Bruner sought to demonstrate how children's communicative intentions were guided and enhanced through interaction with a parent so that the product was not only an awareness of the functions of language, but also the use of language to communicate with others. Bruner examined various predictable and highly structured everyday events, formats, to show how these could give a child a framework within which to learn not just the meanings of words but also the uses of language.

In research on the benefits of collaborative problem solving on children's cognitive abilities (e.g., Garton & Pratt 2001), if both participants share a common conception of a problem and how to solve it, then the degree to which the two participants can work successfully on the task is enhanced. Such a shared task perspective can be achieved by talking. Social regulation via communication about role division and allocation, as well as planning and executing the task, facilitates problem solving particularly in less competent children.

This facilitation has been linked theoretically to the importance for children of being able to reflect on knowledge. Children's theory of mind is concerned with developing understanding of the nature of knowledge and refers to the ability to understand that others too know things, have beliefs and can think, based on knowledge which may be true or false. One crucial means to such understanding is through communication as evidenced in collaborative problem solving. However, it may be that communication can only be successful if there is an existing propensity, or awareness, in children to recognise the importance of the strategic knowledge of the other partner in the interaction. Most research neglects this aspect and fails to take account of the extent to which children explicitly acknowledge and discuss any differences in levels of understanding either before or during social interaction

A possible mechanism to draw together the research on communication and awareness of the other in interaction, and the way the meanings of words might be learned, is the child's developing theory of mind. Word meaning may indeed develop through more general cognitive capacities or through the operation of biases or constraints, but a promising lead is noted in the inclusion by Bloom of the child's capacity to infer the intentions of another as a cognitive ability. This allows for the development of an argument that links cognitive capacity, word learning, and theory of mind via social interaction.

The proposal is that word learning occurs in, is facilitated by, or perhaps is constrained by (Tomasello 2001), social contexts. Specifically, in the case of young children learning language, these contexts are highly predictable, regular, and structured and not only involve language but other non-verbal gestures (such as pointing and gazes) that provide support for the learning of whatever problem is the focus – be it language, jigsaw puzzles, or number. In each case, the participants, particularly the adult, often the mother, ensures the child takes part and adequately (in her view) responds to her encouragement.

In addition, in the case of learning the meanings of words, there would need to be an understanding that the child is responding to the aspects of the environment or problem to which the adult is drawing attention and to which the child in turn should attend. In general, the child, the less competent participant, sets the level of skill or expertise and the pace for instruction and learning. The adult, the more experienced participant, gauges the pre-existing skills and the need for instruction and/or assistance, and then divides the task or problem into manageable components. In this way, the adult takes responsibility for the management and monitoring of the interaction and for changing the definition of the task by the child. Such interactions have been demonstrated to enhance cognitive functioning in young children (Garton et al., submitted) as well as being linked to language learning, including word meaning (Garton & Pratt 1998).

Finally, it is worth noting that far from being a passive recipient in such social interactions, the child is, through the communicative and non-verbal cues, trying to make sense of what the adult intends. In this, there is agreement with Bloom's contention that cognitive abilities play a role, but I argue that, more importantly, the role of the other in social interaction contributes to the manifestation and deployment of these abilities in a supportive context which itself then enhances language and cognitive development. In accordance with Bloom's contention, why should the learning of word meanings be any different to other cognitive capacities that are enhanced by social interaction?

Don't preverbal infants map words onto referents?

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Abstract: Bloom provides a detailed account of children's word learning and comprehension. Yet, this book falls short of explaining the developmental process of word learning. The studies reviewed do not explain how infants begin to map words onto objects or the environment's facilitative role. Researchers must describe how several factors interact and explain the relative importance of each during the development of word learning.

Bloom provides a wealth of detail on the complex issue of children's word learning and comprehension. Several aspects of this book are very appealing. He presents a compelling rationale for children's learning of nouns before other lexical categories. Also important is that children learn the meanings of most lexical words before grammatical words. In light of recent evidence, that newborns (Shi et al. 1999) and 6-month-olds (Shi & Werker 2001) perceptually prefer lexical over grammar words, the present discussion is timely. Yet, this book falls short of explaining the developmental process of word learning.

The author acknowledges the different word learning capabilities in children during development. "Prelinguistic infants are interesting because they lack whatever capacities are necessary to start talking, one-year-olds are interesting because they are word learning novices, . . . and preschoolers are interesting because they represent an illustrative midpoint between novice and expert" (p. 13). Therefore, one expects but finds no developmental explanation of how prelinguistic or preverbal infants begin to learn word meanings. Preverbal infants are interesting subjects of study mainly because they can map word-like forms onto referents long before they can produce them (Fenson et al. 1994). Anecdotal evidence for mapping of speech onto objects is reported in 9-month-olds' use of idiosyncratic expressions that have a consistent meaning (Blake & Fink 1987; Halliday 1975b). A discussion of how preverbal infants begin to map words onto concepts is quintessential to our understanding of how children might learn the meanings of words. Yet, Bloom does not discuss a significant recent body of experimental research on the development of early word mapping in infants (Gogate & Bahrick 1998; Moore et al. 1999; Stager & Werker 1997; Tincoff & Jusczyk 1999; Werker et al. 1998). The focus is mainly on empirical studies of word learning starting with two-year-olds, innate, intuitive, and essentialist theories of children's word and concept acquisition, and philosophical theories of adults' word learning. Children of two years of age and up are adept at learning and understanding words. These studies do not reveal how, during the first year, infants might begin to map words onto objects. Focusing on the end-state deters researchers from investigating the origins and the developmental process of early word learning.

Several studies have attempted to explain how preverbal infants learn to map *novel* utterances onto novel objects. Gogate and Bahrick (1998) demonstrated that 7-month-olds learn the arbitrary relations between two vocalic syllables and two distinct objects only when the syllables are spoken in temporal synchrony with the motions of objects. This finding was supported when 8-month-olds did not learn the arbitrary relations between two moving objects and two word-like (CVC) syllables in the absence of temporal synchrony (Werker et al. 1998). Fourteen-month-olds in this study, however, mapped the words onto moving but not static objects, suggesting that by this age infants still require dynamic object presentations to learn word-object relations (also see Moore et al. 1999). Preverbal infants can, however, map *familiar* words onto objects in the absence of temporal synchrony. Six-month-olds mapped the words *daddy* and *mommy* onto moving but not temporally synchronous faces of their father and mother (Tincoff & Jusczyk 1999). These findings support the following general hypotheses about the process of infants' learning of names for objects. First, they suggest that during the development of lexical comprehension, infants learn the "goes with" relation between a word and its referent prior to learning that a word "stands for" a referent (Golinkoff et al. 1994). Second, when infants first learn novel word-object relations, they rely heavily on perceptual cues such as intersensory redundancy in the form of temporal synchrony between vocalizations and moving objects (7 and 8 months, Gogate & Bahrick 1998; Gogate, under review) and syllable distinctiveness (Stager & Werker 1997; Werker et al. 1998). Later, when infants are more adept at learning word-object relations, they do not need temporal synchrony, but still require object motion (14-months, Werker et al. 1998). Still later, during the second year, infants learn word-object relations under less obvious conditions, in the absence of object motion (Moore et al. 1999). They rely on social criteria such as adults' eye gaze at the referent (Baldwin et al. 1996) and referential intent (Tomasello & Barton 1994) to learn the names for referents.

The author undermines the facilitative role of the environment, in particular maternal communication, in infants' early word mapping. He remarks that aspects of early word learning cannot be attributed to parent's or other's interactions with infants (p. 45). But cross-cultural findings show that mothers use ostensive naming contexts to teach the names of objects and actions to their preverbal and verbal infants (Messer 1978; Zukow-Goldring 1990; 1997). Notwithstanding the cultural variations in the use of ostensive naming (Schieffelin 1979), it sets the stage for infants' word mapping early on. During ostensive naming, mothers use temporal synchrony between the uttered word and the hand held motions of an object. Recently, Gogate et al. (2000) provided evidence for the dynamic nature of maternal multisensory naming of objects and actions. Mothers of preverbal infants (5–8 mos.) often named objects and actions in temporal synchrony with the motions of objects, and simultaneously touched the infant with the object. Mothers of lexically advanced infants (21–30 mos.) used temporal synchrony less often. They named the objects and actions while holding an object static. Although ostensive naming alone cannot explain all of word learning, the changing nature of maternal naming and the changing perceptual needs of the developing infant suggest an ongoing bi-directional relation between the environment and the organism (Gogate et al. 2001). The critical issue is *not* whether one factor or another (e.g., maternal ostensive naming or infant's understanding of others' intentions) best explains how words are learned (p. 192). Rather, researchers must accurately describe how several factors interact at different points in time and explain the relative importance of each during the development of word learning in infants and children.

Innateness, abstract names, and syntactic cues in *How Children Learn the Meanings of Words*

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Abstract: Bloom masterfully captures the state-of-the-art in the study of lexical acquisition. He also exposes the extent of our ignorance about the learning of names for non-observables. *HCLMW* adopts an innatist position without adopting modularity of mind; however, it seems likely that modularity is needed to bridge the gap between object names and the rest of the lexicon.

HCLMW is a tremendously useful and interesting book because it tells us what is known about how children learn the meaning of words, and, by extension, what isn't. Bloom presents a thorough, up-to-the-minute millennial overview of the state of the art, integrated into an intellectually satisfying and elegant Big Picture. The Big Picture masterfully painted by Bloom includes some familiar elements and some surprising ones. Some elements of the story that seemed familiar already, thanks to Bloom's previous publications, included (a) the notion that children need a theory of mind to learn words, (b) they integrate clues from many different domains (including syntactic frames) to understand a novel word, (c) the concepts that we tend to choose to name with words are those that are inductively useful, and (d) word learning – especially "fast-mapping" – is not accomplished associatively: word learning is not statistical learning.

Surprising elements included the following: (a) Young children are not especially good at learning the meanings of words – older children and adults are better at it. (b) Learning the meanings of words is not qualitatively different from learning facts about the world, for instance, that a particular widget was given to someone by their uncle. (c) We can precisely characterize the way in which a human's "theory of mind" differs from that of other animals. Other animals may understand goal-directed behavior, but they do not understand the particular kind of goal-directed behavior that is *referential intent* (*HCLMW,* p. 85, from Tomasello 1998b).

This last is an especially interesting claim, to which little space is devoted. At first glance, it almost seems like begging the question: children learn the meanings of words because children understand reference innately, which is what words are used for. The reason that it's not begging the question about word-learning per se is that it is not only a nativist position, it is also a non-modular one. We suggest that this jibes well with Bloom's – in co-authorship with Steven Pinker in *BBS* – neo-Darwinian adaptationist explanation of the emergence of language. Words are symbols *par excellence,* but words are not the only things that can be symbols. Bloom reports that a number of studies show that babies up to about 18 months treat any communicative act, linguistic or non-linguistic, as a potential Saussurean sign (pp. 77–78). Only after 18 months do children figure out that signs are typically only phonological words. On the dust jacket of *HCLMW,* Gary Marcus (an author with connectionist propensities) congratulates Bloom for "steering between the extremes of nativism and empiricism." On this point, however, endorsing Tomasello's view of referential intent, Bloom is an unapologetic nativist, and rightly so. His nativism, though, does not entail the existence of an innate language faculty. Adaptationism is always at a loss in accounting for the specificity of the language apparatus, leaning rather towards the appeal to variants of general intelligence, "applied" to linguistic communication. Bloom is by far the most linguistically sophisticated of the adaptationists, but his proprietary dissociation of strong innatism from language modularity and specificity, though consistent with his neo-Darwinian positions, remains perplexing. The available evidence from word learning appears to entail Fodor's Representational Theory of Mind. It also appears to exclude Fodor's unrepentant atomism, and the segregation of the contents of the lexicon from all sorts of relevant knowledge about the facts of life in general.

Because Bloom's discussion is firmly grounded in experimental reports, and also, at least implicitly, on his evolutionist-adaptationist persuasions, most of the word-learning he discusses is the paradigm case of object-naming. (As Guerts 2000 remarks, "How Children Learn the Meanings of Nominal Expressions Denoting Middle-Sized Observables" might have been a more accurate title). He repeatedly emphasizes, however, that object-naming is in fact *not* the paradigm case. Children's vocabularies include many non-object words, usually more than 50 per cent of their first words (p. 91 – *naps* and *parties* and *tomorrow* are signal examples). The fact that object names are not the *sine qua non* of word learning, combined with Bloom's observation that older children and adults are in fact the best word-learners, makes the reader long for a whole new body of experimental literature dealing with learning words for non-observables. It seems very likely that *nearly all* the words that we learn over the course of a lifetime are words for non-observables – it's flat-out surprising to encounter a new middle-sized observable as an adult. (When moving to Arizona, it's a shock to learn that there's a middle-sized mammal you have never even heard of: the *coati.*)

Bloom does his best to address this question, given the limited literature, especially in his chapters on number words and syntactic cues to word-learning. The evidence that children use syntactic clues to detect word category, and that this leads them to important conclusions about possible meanings, seems overwhelming (both for object names, as in the English count/mass distinction, and for non-nominal categories like verbs and adjectives). However, it's equally obvious that recognizing syntactic category and consequently drawing broad conclusions about ontological category ("this is a *property,*" "this is an *action,*" etc.), is a far cry from really learning the meaning of a word. Bloom reports some interesting debate about verb-learning using syntactic clues, (pp. 202–203). For instance, Fisher et al. 1994 designed a verbal wug-test where the action that was being named "daking" or "nadding" was one that the children already knew a word for, like *eating* or *feeding.* Looking at this experiment in light of Bloom's earlier discussion of the importance of the lexical contrast principle (Ch. 2), it is particularly striking to learn that children did in fact map these novel words to essentially the same concepts as those denoted by the words they already knew. It seems possible that lexical contrast operates in an interestingly different way in the learning of non-object words.

Similarly, Bloom outlines the smallish body of literature on learning adjectival and prepositional words. He notes, for example, that the object-bias only applies to nouns; when exposed to novel adjectives, children extended the words to superficial properties, not objects. Further, the shape-bias does not apply to adjectives. One of the strange properties of adjectives is that, in set-theoretic terms, they cut across "natural kinds" (a white cloud and a white dog are both *white*); when used attributively, though, adjectives pick out subkinds. Bloom suggests that children may be sensitive to the difference between predication and modification. A particularly interesting case to look at experimentally might be that of relative clauses: *the dog that is fep versus the dog is fep* and *the fep dog* – a modificational use of a predicative use of an adjective. Similar questions arise with prepositional words, which refer to actual or "metaphorical" spatial relations. Bloom reports a study by Landau and Stecker 1990, comparing "This is a corp" to "This is acorp the box," where subjects concluded that *a corp* in the first frame was an object, but a spatial relation in the second. One wonders what subjects would have made of *This is melting acorp the box* or *The dog is eating this acorp the box* – or *The dog acorp the box is panting.* Finally, syntactic categories are famous for not matching up cross-linguistically, or even intralinguistically (Baker, in press; Hale & Keyser 1998). (What should we make of the difference between *Marie a chaud* vs. *Mary is hot* ? or *Mary sings* and *Mary's a singer*?) Also, syntactic cues, as Bloom points out, can only be used after a few closed-class terms have been learned (p. 207), along with their syntactic function (*a, -s, is, -ing,* etc.). Yet, those closed-class items are among the hardest words to learn (p. 116). There are clearly important things to be learned by looking at children's acquisition of such words, but it's hardly been touched, to judge from *HCLMW.*

Bloom's book is a tour-de-force of synthesis and analysis. Decidedly, we think that, should one recommend one book, and one only, on the acquisition of the lexicon, this would be it. However, many central problems still remain open. Bloom has made a virtuoso effort to cast a strongly mentalist and innatist theory of the lexicon, stripped of modularity, and lexical atomism. If anyone can succeed in doing that, then Bloom is the one. We have not been stopped from wondering, however, if even he can do it.

Social attention need not equal social intention: From attention to intention in early word learning

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Abstract: Bloom's eloquent and comprehensive treatment of early word learning holds that social intention is foundational for language development. While we generally support his thesis, we call into question two of his proposals: (1) that attention to social information in the environment implies social intent, and (2) that infants are sensitive to social intent at the very beginnings of word learning.

Steve Pinker put it best when he described Paul Bloom's book as a "tour de force" on the subject of word learning. For centuries, scholars pondered the nature of early word learning. In *How Children Learn the Meanings of Words* (*HCLMW*), Bloom articulates how this seemingly simple task "requires rich mental capacities – conceptual, social and linguistic – that interact in complicated ways" (Bloom 2000, p. 1). The book is a beautifully written, scholarly treatment of early language learning that draws on the author's rich command of psychology, philosophy, and linguistics.

At the core of Bloom's theory is a solution to the "mapping problem." How do infants map the words that they hear onto the objects, actions, and events in their environment? How do infants learn that the word "dog," signifies the category of canines? Philosophers, like Quine (1960), note that this is an intractable problem. What allows a child to learn that the word "dog" refers to the whole dog and not to dog parts? There have been many proposed solutions for the indeterminacy of word mapping. Some hold that infants operate with a set of constraints that limit the possibilities for word mapping (Golinkoff et al. 1994; Markman 1989). Others argue that infants have no internal working hypotheses, but rather associate the word heard with the most salient feature in the environment (Plunkett 1997; Smith 2000).

Bloom aligns himself with yet a third alternative, social pragmatics. Children are bathed in social environments, constantly interacting with others. Infants, keenly aware of their social surroundings, "read" the speaker's intent. When a speaker says the word "dog" while looking at the dog, the problem of reference fizzles away. Attention to the speaker's perspective narrows alternatives for word mapping, allowing children to apprentice with master word users (Baldwin & Tomasello 1998; Tomasello 1995b). Bloom writes,

> How do children make the connection between words and what they refer to? . . . they do so through their understanding of the referential intentions of others . . . children use their *naïve psychology* or *theory of mind* to figure out what people are referring to when they use words. (*HCLMW,* pp. 60–61, emphasis in the original)

Bloom contends that this ability to infer others' intentions is available from the outset. Even by 6 months of age, infants follow another's gaze (Butterworth 1991; Carpenter et al. 1998). This fact, Bloom suggests, is evidence that infants have a "broad" theory of mind. It is this early ability to read intent that paves the way for word learning:

> it is impossible to explain how children learn the meaning of a word without an understanding of . . . how children think about the minds of others. (*HCLMW,* p. 2)

We generally agree with Bloom's position. Speaker intent is a hallmark of word learning. We beg to differ, however, with two of Bloom's central tenets. First, attention to social information in the environment need not imply social intent. Second, infants are not sensitive to social intent at the very beginnings of word learning.

Social* attention *need not equal social intention. Social pragmatic theories regard attention to social cues as synonymous with social intent. Yet, the two must be distinguished. Six-month-olds who follow a speaker's eye gaze are attracted to social cues, but may not use them to infer speaker intent. Similarly, acts of joint attention, central to early word learning, might be just that – two people attending to an object. Indeed, in a joint attention task infants fail to learn novel words when the task requires that they shift attention from the object in *their* view to that which the mother indicates (Dunham et al. 1993; Tomasello & Farrar 1986). They are socially attentive to the mother, but do not use her intent as a platform for word learning. It seems a stretch to assume that because social objects command infant attention, the infant is blessed with the ability to attribute intent. Research in our laboratories is exploring this distinction in children with autism. Recent findings suggest that these children, who do learn some words, *are* aware of attentional social information (Leekham et al. 2000) but *cannot* read social intent (Baron-Cohen 1995; Bloom 2000).

Infants are not initially sensitive to social intent: Development proceeds from social attention to social intent. Bloom contends that infants are sensitive to social intent in word learning from the outset. Evidence suggests otherwise. In one study, infants 10 to 24 months old heard novel words uttered in the presence of an unfamiliar interesting object and an unfamiliar boring one. Speakers used eye gaze and pointing to indicate the intended referent. All infants attended to the speaker's gestures. Only 19- and 24-month-olds, however, used speaker intent to map the word onto the boring object (Hollich et al. 2000). Ten-month-olds did what Bloom argued "never happens" (p. 59). They mismapped – assuming that the label referred to the most interesting object regardless of the speaker's intent (Hennon et al. 2001). Preliminary data suggests that children with autism respond more like 10-month-olds than like their older counterparts. These findings accord well with others in the literature. Before 18 months of age, infants do not use social intent in a word learning task (Baldwin & Tomasello 1998).

One way to reconcile these findings with Bloom's is to suggest that when infants begin to learn words, people serve as salient perceptual objects that draw attention to, or highlight, word-to-world mappings. At around 18 months of age – around the time of the naming explosion – infants begin to note speaker intent and their word learning strategies shift. The Emergentist Coalition Model of word learning makes this prediction, offering a theoretical justification for the shift in word learning strategy (Hirsh-Pasek et al. 2000; Hollich et al. 2000). Under this scenario, children learn some words or "arbitrary signs" (p. 17) without social intent, but become more efficient word learners when social intent comes on-line for language learning. The challenge for those of us in the field becomes explaining how children who learn words like "Fido" transform into those who learn words like "Fred."

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Vocabulary and general intelligence

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Abstract: Acquisition of word meanings, or vocabulary, reflects general mental ability (psychometric *g*) more than than do most abilities measured in test batteries. Among diverse subtests, vocabulary is especially high on indices of genetic influences. Bloom's exposition of the psychological complexities of understanding words, involving the primacy of concepts, the theory of mind, and other processes, explains vocabulary's predominant *g* saturation.

The main message of Bloom's extraordinarily detailed and probing analysis, which I found completely convincing, is the primacy of conceptual thinking in the acquisition of vocabulary. The essential direction of causality is concept to word rather than word to concept, as is so commonly and mistakenly believed. The acquisition process is as different psychologically from the stimulus-response paradigm for paired-associate rote learning as one could imagine. Vocabulary is acquired when words fill conceptual "slots" that form in the course of mental development and seek to be filled. The wide range of individual differences in vocabulary reflects differences in the number "slots" much more than differences in the amount of exposure to words. The causes of the available number of "slots" are still largely unknown but are certainly related to chronological age and Spearman's *g* factor. The probability that an individual will know the meaning of a given word is mainly a multiplicative function of that individual's level of *g* and the frequency of that word in the individual's past experience.

I was first attracted to Bloom's book by my prior interest in vocabulary tests and in the fact that they are so strongly correlated with IQ, while individual differences in the paired-associates learning of words, nonsense syllables, or paralogs have such a relatively weak correlation with IQ and even with vocabulary. Although Bloom briefly alludes to this fact (p. 193 of *HCLMW*), I think much more should have been made of it, as it not only supports his thesis but extends it to psychometric findings and to the information processing theory of general intelligence. In combination with Bloom's conclusions, the psychometric and behavior-genetic facts about vocabulary should give pause to many psychologists and educators.

For a starter, consider the following observations. Recall that a factor in psychometrics is a source of variance (individual differences) common to a number of different tests; a test's *g* factor loading is its correlation with the one factor that is common to all of a number of diverse tests whose intercorrelations have been subjected to a factor analysis. In the national standardization sample of the Wechsler Intelligence Scales for Adults (WAIS), vocabulary has the largest *g* loading (.87) among the eleven diverse subtests. What may seem more surprising, however, is that when vocabulary is factor analyzed among only the six completely nonverbal subtests, its largest loading is only slightly lower (.82). Its loading among just the verbal subtests is .92. Vocabulary therefore reflects *g* much more than it reflects verbal ability residualized from *g*.

Bloom emphasizes, rightly, that the "theory of mind" plays an important part in the child's acquisition of word meanings. This leads one to predict that, among nonverbal tests, vocabulary should have its highest correlation with Picture Arrangement (PA). Wechsler (1944, p. 88) described PA as a form of social intelligence, involving human, practical situations, and inferring what the cartoon characters in a disarranged series of pictures are trying to do. The correlation of PA with vocabulary is .65, a value that is entirely predicted from these two tests' *g* loadings. The same results are found at every age level in the Wechsler Intelligence Scale for Children (both in the American and Japanese versions) and in the British Intelligence Scales. A factor analysis of a much larger number of diverse subtests, performed to examine Carroll's (in press) 3-stratum model of the factor structure of cognitive abilities, allows a detailed analysis of Oral Vocabulary, showing its percentages of variance in each of the three strata of an orthogonalized hierarchical factor analysis: (1) Verbal Ability (V), (2) crystallized intelligence (Gc), and (3) general intelligence (*g*). The test's specificity and measurement error are the residual (Res), that is, components of variance not common to other tests in the battery. The averaged results of two batteries (of 29 and 16 subtests) are: $g = 61\%$, $Gc = 16\%$, $V = 8\%$, $R = 15\%$. A similar hierarchical analysis of another large battery containing many nonverbal tests (Gustafsson 1988) shows the factor composition of vocabulary as $g = 55\%$, $Gc = 34\%$, $Res = 11\%$.

Although the acquisition of vocabulary naturally depends on exposure to words in some meaningful context, such exposure interacts strongly with innate biological factors, as indicated by the high degree of heritability of vocabulary tests. On a test of vocabulary, monozygotic (MZ) twins are more alike than dizygotic (DZ) twins (Newman et al. 1937). Vocabulary scores (with age partialed out) are correlated .86 for MZ twins and .56 for DZ twins; the broad heritability of vocabulary therefore, is estimated as $2(.86 - .56) = .60$. In this same data set, the heritability of Binet IQ is .50, of height, .57.

As a result of natural selection, advantageous traits typically show genetic dominance, a component of the trait's broad heritability. Dominance can be detected by the effect of inbreeding on the trait, a quantitative phenomenon known as inbreeding depression (ID), which is manifested in the offspring of parents who are closely related genetically, such as siblings or cousins (Jensen 1978; 1983). ID is measured as the percentage of depression of the trait among inbred offspring compared with the mean of an outbred group (i.e., offspring of genetically unrelated parents) that is matched (or statistically controlled) for parental socioeconomic and educational variables. The largest study of the effects of inbreeding (cousin matings) on children's mental test scores found that among the eleven subtests of the Wechsler Intelligence Scale for Children, vocabulary had the highest index of ID (11.45%) as compared with the average of ID of 6.58% for the other ten subtests (Schull & Neel 1965). The inbreeding effect on the various subtests was correlated about +.80 with the subtests' *g* loadings (Jensen 1983).

Bloom's book describes, more thoroughly than any other analysis I have read, the purely psychological processes crucially involved in children's acquisition of word meanings. This fascinating array of behavioral phenomena and its social-environmental context accounts for why measures of vocabulary reflect so much of the brain's power that is represented psychometrically by the encompassing, and still causally unfathomed, *g* factor (Jensen 1998). The aim of reductively understanding the causal mechanisms of individual differences in vocabulary is essentially the same as that of discovering the physical basis of g, its predominant latent trait.

Good intentions and bad words

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Abstract: Bloom makes a strong case that word meaning acquisition does not require a dedicated word learning system. This conclusion, however, does not argue against a dedicated language acquisition system for syntax, morphology, and aspects of semantics. Critical questions are raised as to why word meaning should be so different from other aspects of language in the course of acquisition.

The failure of attempts to explain or model the acquisition of language in terms of an all-purpose general learning system has been taken as supporting arguments that humans have a domain specific language acquisition system, variously thought of as a mental organ (Chomsky 1965), a module (Fodor 1983) or a specialized instinct (Pinker1994b). Children seem to be innately predisposed to prefer certain families of language structures over others, preferences that apparently cannot be explained by general principles of association, problem solving, or massive parallel processing. In addition, it has been difficult to expand the scope of this capacity beyond language to a somewhat larger domain such as hierarchically organized relations, temporal strings, or symbolic relations. Language in particular seems to be matched to specific mental faculties.

While this view remains convincing to many more than forty years after Chomsky proposed it, there is far less consensus on what aspects of language are innately determined and how that determination takes place. Paul Bloom's book offers an extraordinary re-examination of the claim that part of the language faculty involves a capacity to acquire the meanings of words. At first blush, it seems that learning words is surely near the center of the faculty. Children acquire words quickly and effortlessly; and they seem to rule out all sorts of alternative meanings that, on associative grounds, should be quite compelling. There seems to be a Word Acquisition Device (WAD), full of constraints that narrow down the extraordinary range of possible meanings that could be mapped onto words. Indeed, the impossibility of divining from scratch the meanings that another person attaches to their words is more striking on the surface than comparable arguments for syntax. The ease of laying out the indefinitely large set of logically possible alternative meanings for a word makes inescapable the conclusion that something must be profoundly limiting the child's conjectures (Quine 1960).

Bloom's book convincingly shows that there is no WAD as such. Fast mapping of words works for non-words equally well. There is no explosion of word learning that might grant it a special status. Finally, putative constraints on possible word meanings devolve into constraints on concepts, on pragmatics, and on how we apprehend objects and events. Especially impressive in this mix is the role of intentions. Bloom martials an extraordinary range of evidence showing how another module, for a theory of mind, constrains the inferences that a young child will make about possible word meanings. Whether it be word learning difficulties in autism or ways in which a toddler overrides powerful associative suggestions, it matters greatly to the success of word learning that we see word users as intentional agents. Bloom's analysis of the role of intentions illustrates how several putative constraints on word meaning might arise instead from broader principles concerning how we interpret the beliefs and desires of others in communicative contexts. There are no bad words, just bad concepts and bad pragmatics.

Given this dismantling of the WAD, it is natural to wonder if Bloom's book should be regarded as the nose of the camel that will eventually displace all need for a Language Acquisition Device (LAD). I suspect there are some who will make just such an inference, and others so devoted to the LAD that they will see Bloom's book as a threat to their enterprise as well. In both cases, the inferences are misguided. Word meanings differ from natural language syntax, and for that matter, from phonology and morphology, in profound ways that make the demise of the WAD largely irrelevant to the status of the LAD. If we are to believe Fodor (1983), word meanings are not modules in that they are clearly cognitively penetrable. It is an open question whether modules are the only places where domain specific constraints should exist (they might well apply in other cases; Keil et al., in press); but the modular nature of syntax and the non-modular nature of word meanings strongly suggest that the two are not likely to be part of the same acquisition system. Even if syntax sometimes helps the learning of words, that assistance does not make syntax and word meaning part of the same learning system. Syntax also helps one learn irony, even though no one thinks of irony as part of the LAD.

As someone who was once completely swayed by the idea of specific constraints on word meanings as necessary for their acquisition, I do now feel twinges of anxiety about other domains where great learning success with relative ease suggests domain specific constraints. If theory of mind works so well for words, why not for syntax, or even number? One lesson from Bloom's book is that poverty of the stimulus arguments on their own should not be taken as compelling evidence for a particular mental faculty, which is probably why such arguments are the most frequently attacked parts of nativist accounts (e.g., Elman et al. 1996). Such arguments, however, are still extremely useful to questions of acquisition. They pose a problem that must be addressed in one way or another. Bloom's analysis shows us that, for word meanings, the problem can be solved by converging sets of constraints, none of which are specifically devoted to word meanings. For syntax, no comparable set of converging constraints has been plausibly proposed. The difference may involve the modularity of syntax. Modules of cognition and perception are so self contained that their acquisition follows from their internal structure in ways not seen in other sorts of domains. A WAD may also be made unnecessary because of the close correspondence between word meanings and concepts where, contra Fodor, there may be rich constraints on concepts' internal structure that make unnecessary specific ones on words (Keil & Wilson 2000). A missing WAD, however, does not preclude the existence of powerful constraints on the semantics of language, such as relations governing scope, quantification, and anaphora (Chierchia & McConnell-Ginet 1990). Word meaning is special, not because it needs its own constraints, but because it is so different from the rest of language.

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How fast does a child learn a word?

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Abstract: This discussion argues that for many word meanings, the child has to assemble a new category, using relatively slow information-sifting processes. This does not cause high semantic errors, because children probably hold off using a word until much such sifting has occurred, rather than producing the new word as soon as they have any information on it.

First, this is a really excellent book, one of the most worthwhile I have read lately. Bloom provides *pro tempore* convincing arguments on many central issues, such as how skillfully children judge speaker's attention and intention, the lack of a need for language-specific heuristics in word learning, and (agreeing with Maratsos & Deak 1995) the essentially fox-like nature of most word meaning acquisition. All this is done with admirable clarity and open-minded moderation. The book is a model treatment.

While acknowledging my general agreement with many of Bloom's central analyses, my discussion centers on a set of issues where we probably disagree somewhat. These issues comprise of four intertwined problems: (1) At the most general level, in the child's learning of a word, how common are slower processes sifting information over different inputs of the word, as opposed to immediate or near-immediate analysis from one or very few inputs? (2) When children acquire a word meaning, to what degree do they just match with an already existing cognitive-perceptual category, which I will call *category recognition,* versus having to assemble a new conceptual structure from previously existing nonlinguistic cognitive-perceptual elements, a process called here *category assembly*? (3) Highly related to (1) and (2), do children immediately use a word upon processing some initial input, or does their own productive use occur only after processing a possibly large number of inputs? (4) Why are semantic errors so relatively low in children's uses?

Bloom is open on these problems, but his discussion favors the position that children use a word quickly after hearing initial input. Combined with the low rate of error, this means they must have the means for quickly making a mostly accurate analysis. Among the means for the latter, they probably do not have to assemble categories for new words; instead they mostly recognize which already existent categories match the word used. (Documentation of Bloom's preferences, which are not monolithic, exceeds the space available here.)

In many cases, relatively quick category recognition is probably correct. That is, children plausibly have already formed some cognitive-perceptual categories like "table" or "cat" prior to learning words for such categories. This prior assignment is most likely for what are called basic object concepts (Rosch 1977) and probably, as Bloom suggests, for proper names. But category assembly, which requires a slower process of sifting over many inputs and analyzing or constructing a new assemblage of elements, is intuitively more likely for many word meanings; it seems particularly likely for many verbs and other non-object words.

For example, we might think our word "eat" groups together the "natural" set of ingestion situations and features. But Brown (1998) documents how in Tzeltal, three more specific ingestion meanings are encoded by the three most common ingestion words. One means "to ingest crunchy-solid foods" (like carrots). One means "to ingest less-than-crunchy-solid foods" (like bananas). And one means "to ingest tortillas and other bread substances." These are not "intuitively obvious" nonlinguistic categories. To take another case, Bowerman (1996) discusses Korean change-of-location meanings like "kohta" (to put an object elongated – long or tall – in one dimension onto any base, e.g., a flower into a vase, a hairpin into hair), or "puwlhita" (join a flat surface to another flat surface) or "kkakta" (take off a covering layer with a bladed instrument), and many others.

It seems implausible – or at least worth not presupposing – that children naturally nonlinguistically form a category of actions like "put an object elongated in one dimension onto/into a base," or "eat breadlike substances." It seems unlikely that they have always pre-formed just the right cognitive-perceptual categories before figuring out all word meanings that languages have. In such cases, which are common among verb meanings compared across different languages, they probably have to analyze how the word is used over many different situations, in order to assemble the right collection of cognitive-perceptual elements. There would be no likely "initial best guess," or pre-fabricated conceptual category.

Suppose such slower "assembly" processes *are* often the case. What then becomes potentially puzzling, is that children make relatively few semantic errors in their word use, valid both as a general conclusion (Bloom) and as a conclusion about the Tzeltal and Korean cases (Bowerman 1996; Brown 1998). For if there is a somewhat protracted period of category assembly, so that each word meaning development has to cover a lot of ground, wouldn't children have to make many errors in the word use, before they finally get to the right meaning? This paradox exists, however, only if one assumes children immediately produce words immediately upon processing some initial input. What if, instead, children only produce a word after having gathered information about its use in the course of many different uses? This would give time for slower category assemblage processes to give a more accurate meaning, before the child actually used the word. In fact, there is plenty of evidence that children often work on a word meaning for some period before they actually produce the word. To start with, current estimates are that when children produce their first word, on average they comprehend around fifty. In general afterwards, many more words are at least partly comprehended for some time before they are produced. So there is good reason to think children often accumulate a good deal of information about a word before they try it out themselves. This delayed production process would give time for a constructed word meaning category to have become mostly accurate before actual use occurred, explaining the generally low degree of semantic error. Thus, slow acquisitional processes can also result in relatively accurate initial productions.

In summary, it is very likely that children sometimes do get to a mostly accurate meaning very quickly. But the kinds of word meanings discussed above imply that slower category assembly processes also play a part in acquisition. Part of the fox-like variety of the child's procedures lies in the use of various kinds of faster and slower processes. In the latter case, production delayed after initial input processing plays a crucial role in making initial production accuracy relatively high.

Fast-mapping children vs. slow-mapping adults: Assumptions about words and concepts in two literatures

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Abstract: Research on children's and adults' concepts embodies very different assumptions of how concepts are structured, as reflected in their experimental designs. Developmental studies seem to assume that categories contain highly similar objects that can all be identified from one or two examples. If concepts are more like those tested in adult experiments, research on word learning may be misleading.

Paul Bloom's thesis that much of word learning arises through general-purpose concept learning is one that I have great sympathy with (Murphy 1991). Children and adults can communicate to a great degree, suggesting a basic commonality to their word meanings and underlying conceptual structure. It is surprising, then, that the adult and developmental literatures seem to have very different ideas of what concepts are like.

A typical word-learning experiment in the developmental literature involves the naming of a single object, usually a very few times. The child is then tested on a number of similar objects, often differing from the learning models in only one dimension. Since Carey and Bartlett (1978) first demonstrated *fast mapping*, similar experiments have been carried out dozens of times, many of them described in Bloom's book. Apparently, the assumption is that children can learn the word's meaning fairly well based on a single example. Those familiar with this literature will probably think "They DO learn the word based on a single example! That's what the fast-mapping results show."

Before replying, let's consider the typical concept-learning experiment in the adult literature. Here, subjects are shown category exemplars one at a time and are told that some of them are called "Dax" and some "Kez." Subjects start by guessing the identity of the first few items, but they eventually learn what the two categories are and can name items correctly. The operative word here is "eventually." In fact, in a number of such experiments, subjects take an extremely long time to learn the categories, even when there are fairly few items involved. For example, in a classic paper by Medin and Schaffer (1978, Experiment 2), subjects were to learn to classify nine items into two categories. However, about 40% of the subjects failed to do so after 16 blocks of learning trials. Lamberts (2000) didn't stop his subjects, and they took an average of 38 blocks to learn these categories. Although this is an extreme example, bad performance is rife in studies of adult concept learning.

Clearly, taking 38 blocks to learn two categories is not fast mapping. Somehow, the bright children who could learn a category based on a single example grew up into dull college sophomores who could not learn two categories even when they saw all their exemplars over and over again. It would be easy to blame the educational system, but the categories and methods involved in these experiments are so different that one cannot really compare them. These differences reflect different assumptions, summarized below.

Category structure. To demonstrate learning in the fast-mapping experiment, a child is shown an object and then is tested on another object that differs little from the original. The assumption seems to be that members of the same category are very similar. In contrast, in the adult literature, categories often have very weak structure, with many overlapping features. In some cases, the categories seem perverse, with objects that differ in *every* dimension in the same category (e.g., Medin & Schwanenflugel 1981).

Learning procedure. In the fast-mapping learning phase, the child sees a single exemplar and hears it labeled a few times. The adult learner guesses the category, receives feedback, and continues until performance reaches a high criterion, usually perfect classification of all the items in a block. These differences in procedure follow from the assumptions of category structure: If the category is weakly structured, all the category members must be tested to discover whether they have been learned.

Learning models. These differences have led to very different kinds of explanations of category learning. In the developmental literature, the approach has been one of hypothesis-testing, focusing on biases and constraints on the child's hypotheses. In the adult literature, the focus has been on associative learning, in which properties and exemplars are associated to categories, or attention is gradually drawn to the critical dimensions that separate the categories. If one reads enough papers of each sort, one must come to the conclusion that both cannot be right. In fact, I suspect that both are wrong.

There is clearly a sense in which fast mapping is true: If you use a word in front of your 2-year-old, you may find him or her using it just as you did. However, that correct performance is not a stringent test. If you label a robin *a bird,* your 2-year-old will not be correctly labeling ostriches, penguins, bats, and swans right afterwards. Indeed, a child may not be able to use this word with near-perfect accuracy until he or she has seen pictures and read books in which the entire variety of birds has been revealed: The child may not "learn the word" until the age of 4, 6, or adulthood. On the other hand, the very weak structure of many experimental categories is not justified either. Real categories often have a set of typical members that could allow fast mapping as well as a set of atypical members that would not (Rosch & Mervis 1975).

The two experimental paradigms I am contrasting seem to favor one aspect of the usual category structure at the cost of the other: Category members are either almost uniform or have little in common. What worries me is that neither of these situations may tell us how children learn to name realistic categories that are a mixture of the two.

What all this leads to is the conclusion that we need a better understanding of what real-world categories and word meanings are like. When we consider statistics of how many words children learn and how quickly, we may be overly liberal in attributing to them knowledge of "the meaning," based on correct uses for a few central cases. If categories have more atypical exemplars and difficult cases, learning might not be complete for months or years. I am not certain whether the long task of filling in the exceptions and unpredictable cases would greatly change Bloom's conclusions about the word-learning process, but it is important to explore more realistic and perhaps messier conceptual structures than has usually been done either with adults or children.

Why theories of word learning don't always work as theories of verb learning

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Abstract: Bloom's theory of word learning has difficulty accounting for children's verb acquisition. There is no predominant preverbal event concept, akin to the preverbal object concept, to direct children's early event-verb mappings. Children may take advantage of grammatical and linguistic information in verb acquisition earlier than Bloom allows. A distinction between lexical and grammatical learning is difficult to maintain for verb acquisition.

Striking parallels can be drawn between the magical classes in Harry Potter's Hogwarts School of WitchCraft and Wizardry (Rowling 1998) and the major types of word learning discussed in Paul Bloom's book *How Children Learn the Meanings of Words.* The Hogwarts class called Transfiguration, which teaches students how to transform teapots into tortoises, finds a match in Bloom's excellent discussion of how children learn names for natural kinds versus artifacts; the class called Potions, which is about creating substances from all sorts of raw material, finds a match in Bloom's insightful presentation of how children learn words for substances, and the class called Arithmancy shares its numerical focus with Bloom's detailed chapter on children's acquisition of names for numbers. Where Bloom's book suffers is just where the parallels with Hogwarts break down: Hogwarts classes also include Charms, where "Alohomora" is taught to enable the opening of locked places, and Defense against the Dark Arts, where "Expelliarmus" is taught to force the disarming of one's opponent. Bloom's book includes no comparable deep discussions of children's acquisition of the *words* for actions, events, and relations; that is, the acquisition of verbs. And just as Harry Potter and his friends would not have survived their adventures without their spells from Charms and Defense Against the Dark Arts, I argue here that Bloom's theory of word learning suffers on account of the subordinate position relegated to children's acquisition of verbs.

It is possible that Bloom's neglect of verbs does not harm his theory of word learning. If all word learning derives from the same roots, then everything that Bloom describes for children's noun learning can be easily and simply extended to verbs. However, there are ways in which verb learning is not like noun learning, so that what Bloom has proposed for nouns does not easily transfer to verbs.

Children's early nouns tend to refer to objects and individuals, such as *dog, cup, ball, baby,* and *mama.* This tendency – seen robustly across languages – has led some researchers to propose that children are using a specifically linguistic principle (e.g., object scope, whole object bias) to focus their initial attention on objects as the referents of unfamiliar words. As part of his program to avoid such specifically linguistic principles of lexical acquisition, Bloom proposes instead that the roots of the object bias lie in infant cognition. Numerous researchers have demonstrated a sustained focus throughout preverbal infancy on objects and individuals, and this focus could function in the word learning years to make objects the most salient referents around. Thus, Bloom resolves Quine's (1960) problem – that a given word in a given situation can have any number of possible meanings, including color, texture, parts, or activities – by postulating that children target the salient object in the situation as the default referent because of their preverbal focus on objects.

The problem is that the same "plethora of possibilities" exists for verb meanings, and there is no analogous "preverbal concept" resolution. For example, the event of "a child brings a doll to Grandma" can be referred to as *carrying* (if the manner of bringing is key), *bringing* (if the manner is not key), *giving* (if the transfer to Grandma is key), *getting* or *taking* (if receipt of the doll is key), *coming* or *arriving* (if the doll's motion is key), *seeing* (if the child's perceptions are key) and *wanting* (if Grandma's desire is key). Thus, different aspects of the event are highlighted, depending on which verb is used. But then how might a child learn any one of these verbs? The analogous resolution à la Bloom would involve the proposal that one or two predominant preverbal event concepts would direct children's attention to default events for their initial referent mappings. But children's early verbs, which are often produced within their first 50-word vocabularies (Bloom et al. 1993; Dromi 1987; Tardif 1996), are extremely heterogeneous in their semantic classes. They encompass both general and specific meanings (*come* and *draw*), both causal and noncausal meanings (*break* and *go*) and both externally-experienced and internally-felt meanings (*eat, give, see, want*). The variety of children's early verb meanings casts doubt on there being a unitary supporting event concept that predates verb acquisition.

Bloom's presentation of verb learning (*HCLMW,* pp. 201–203, 209–211) includes roles for general linguistic and specific syntactic information (so-called syntactic bootstrapping; Gleitman 1990; Naigles 1998) as clues to verb meaning. But his discussion has a curiously uncertain quality to it. Bloom spends considerable space debating the centrality of syntactic information in verb learning, both in terms of its necessity (which he finally accepts) and its sufficiency (which no one has ever claimed). Moreover, Bloom holds that syntax could not be providing information for the earliest verb learners because these children are still at the one-word stage of language production. And of course, if children don't know any syntax (e.g., Tomasello 2000a), they certainly couldn't be using it to learn about verbs. However, Bloom overlooks the now-considerable evidence that children's configural analyses of linguistic input, as well as their grammatical comprehension, are well in advance of their language production (e.g., Gomez & Gerken 1999; Hirsh-Pasek & Golinkoff 1996; Santelmann & Jusczyk 1998). Moreover, the finding that maternal utterance length is a stronger predictor of toddlers' subsequent spontaneous speech vocabulary than child-mother joint attention or maternal word frequency, shows that toddlers are *processing* entire utterances of maternal speech even when they are only *producing* one- and two-word utterances themselves (Hoff & Naigles 2002). Perhaps a dichotomy between the types of information that children use at the earliest versus later stages of word learning is not needed.

It is possible that Bloom chose not to discuss verb learning in depth because such a discussion invariably introduces – as it has in this commentary – a consideration of children's acquisition of grammar as well. Bloom makes it clear that he regards these types of language acquisition – the grammatical and the lexical – as distinct. But verbs do fit in both types; they are lexical items with distinct meanings as well as words with predictable relations to the sentences in which they appear. Any theory of child *word* learning that desires to encompass all words and not just nouns must grapple with this fact.

The name game updated

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Abstract: Bloom's domain general theory remains strictly cognitive and individualistic. By ignoring the contribution of social interaction and collective construction of concepts, he fails to solve the word learning problem.

Paul Bloom's book on learning the meaning of words (which, despite the word "children" in the title applies across the lifespan) covers a broad territory in a mostly balanced way, and especially welcome is the move away from language-specific constraints on word learning to the recognition of the necessary contributions of domain general abilities. Also welcome is the declaration that "children aren't like scientists who have theories; they are like scientists before they have theories: trying to make sense of some domain they know little about" (pp. 168–69). Highlighting meaning as the central problem is another welcome move, contrasting with most other current developmental theories. Meaning is defined in terms of "narrow content," the psychological (rather than social and contextual) aspect, "relevant to explaining people's intuitions about reference and categorization" (p. 17). The problem here is that "people's" intuitions are not independent of social and contextual uses of words.

Although it contradicts many current positions the new theory is not really new; it reformulates word learning theories from the 1970's (L. Bloom 1973; Bowerman 1976; Nelson 1974). Bloom recognizes and even relies on selected data from these studies, but he ignores their theoretical proposals. Data are cited from Nelson (1973) but the social interaction processes documented there, as well as the individual differences in both parents' and children's functional use of words and associated differences in form, are ignored.

For Bloom all the action in word learning is in the heads of children. The central focus of the theory is on children's ability to infer the intentionality of adults, but the influence of interactive social experience or knowledge is explicitly denied. Although Bloom views the word learning problem as belonging to the domain of social cognition, his theory remains individualistic to the core, and thereby fails to address the most critical problems.

Like most word learning theorists, Bloom is occupied with object words and categories (acknowledging but not explaining other kinds of words). His example of a novel concept learned through language – "hat trick" in hockey – is explained in perceptual-action terms. But as Searle (1995) points out, the concept of a game like hockey relies on collective (not individual) intentionality; like money, it is a social institution. You cannot learn the concept of money without knowing about buying and selling, prices, commodities, banks, stores, governments, and so on. These socially established entities depend upon the use of words to constitute their reality. Young children who use the word money in reference to coins and bills cannot yet have the concept of money or the conventional meaning of the word "money." How then can we account for their appropriate use of words in the absence of knowledge of their meaning, and how do children acquire meaning? Despite the promise of the title and the extensive discussion of concepts, Bloom fails to address these problems adequately. For Bloom, essences are assumed to guide children to the meanings of words for natural kinds and artifacts, but more abstract terms (like money and game) defy this kind of easy solution, as Wittgenstein (1953) emphasized. Even the simplest examples such as "chair" are not as clearcut as Bloom and other "essentialists" assume (Rosch 1978). Regarding the learning of abstract words, Bloom states: "I think it will remain a mystery for a long time" (p. 94).

Taking children's errors in word uses seriously, I proposed (Nelson 1974) that children focus on the function of things (from the child's perspective) in forming concepts and naming things. This disposition, although akin to Bloom's thinking about artifact essences, often does not accord with the collective meaning of the term, and thus children must tune their concepts and categories to the language they are learning, even for everyday animate and inanimate objects. They frequently use words without conventional meaning, and continue to do so, especially for abstract words like "money," or "know" and "think." How then do they acquire meanings? Gradually, through using and hearing words used in different contexts and observing the practices and discourses of people in relation to those words they build and trim their concepts to fit the uses of the language users around them (Nelson 1996).

By ignoring the fact, indeed the necessity, of this process, Bloom distorts the relation between language and thought. He asserts that "Words are not necessary for thought. Structured and abstract thought occurs without them" (p. 259). And, "Language . . . is a tool for the communications of ideas . . . not a mechanism . . . to generate and appreciate these ideas in the first place" (p. 258). Although he believes that we can learn new concepts from language (such as "hat trick"), language has nothing to do with generating or appreciating new concepts.

In this, Bloom greatly underestimates the significance of symbols to culture and cognition, and the complexity of concepts that people learn, use, and create, as well as the different cognitive structures that cultures make manifest in their languages with demonstrated effects on thinking and problem solving (Bowerman & Levinson 2001). He does not accept that our words are capable of constructing worlds that would never exist, in individual cognitive systems, or in communities, without the symbols that they depend upon. It is not possible to address the problem of word meaning without addressing the language and thought problem, and his rejection of any "contamination" of thought by language renders his theory at best inadequate to the problem and, at worst, incoherent.

An ideational account of early word learning: A plausibility assessment

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Abstract: The theoretical framework of Bloom's account of child word learning is here assessed only for initial plausibility and neural plausibility. The verdict on both dimensions is low, largely due to the size and character of knowledge it is claimed that the child brings to the task. It is suggested that elements of constructivist accounts could profitably be drawn from to reduce this implausibility.

Bloom construes early word learning as a mapping task in which the word maps onto a psychological entity that is a concept. His test for successful mapping of referential terms is getting their extensions right; a concept's role is to pick out the right category of things in order for the sole business of the language, communication, to proceed. The local linguistic context generally provides only the language-specific word to be mapped onto the pre- and non-linguistic concept, which plays much the same role as Locke's "ideas" did (Locke 1690/1975), minus his *tabula rasa.* To solve the mapping problem, the child uses multiple strategies, of which the central one is discerning the intentions of speakers. In the basket of competencies available to the child, essentialism, the assumption that many individuals are referred to by the same word because of a shared hidden essence, is also a significant asset. Drawing upon a wealth of experimental results, Bloom applies this explanation to defeat the alternative explanations of (1) empiricist associationism and (2) constraints that are specifically linguistic.

Initial plausibility. The account is low on initial plausibility. First, it violates the requirement that the direction of explanation be from the less understood to the better understood. Another concern derives from the mutually-supporting character of the basic assumptions of Bloom's explanation, for example, (1) an ideational theory of word meaning, and (2) the independence of human conceptualization and other higher cognitive competencies from language. One might argue for the Ideational Thesis on the basis of the Independence Thesis, and vice versa; but independent confirmatory evidence of either assumption is desirable. To these two assumptions Bloom adds the Essentialist assumption and the Theory of Mind assumption. Taken together, these assumptions yield a coherent explanation, to be sure, but no independently compelling evidence is given for any one of them. Instead, the argument strategy of inference to the best explanation is adopted, wherein it is sufficient to show that, of all available candidate explanations, only this one accounts for all the cases. But *have* all available competing explanations been considered?

Neural plausibility. A different concern arises from the amount of knowledge that is attributed to the prelinguistic infant, a concern that raises issues that breach the boundaries of a psychologically autonomous theory. That amount of knowledge makes Bloom's proposed explanation neurally implausible. Human brain plasticity and immature birth argue for human selection for adaptability over adaptedness (Lorenz 1965). That we are born knowing so little that we cannot do much, enhances our abilities to adapt to the environment through acquiring information from it – which the adaptedness of other species, to their disadvantage, prevents them from acquiring. Human brain plasticity, immature birth, and high degree of adaptability make plausible the hypothesis that humans have less in the way of species-specific ("off the rack") initial knowledge structures than are enjoyed by other species, enabling humans to build knowledge structures that are more useful to them because the structures are "tailor-made" through interaction with the environment. From a design stance, economy would be served if these structures were also general rather than domain specific.

Constructivism. The above considerations lead us to expect the development of new information-bearing and other cognitive neural structures in response to the organism's interactions with the environment, particularly structures that subtend major behaviorally manifested cognitive achievements like language development. The neural constructivist proposal (Quartz & Sejnowski 1997) offers just that, itself building on the long tradition of constructivism in psychology (see, e.g., Bruner1963; Bruner et al. 1956; Piaget 1971; Vygotsky 1978). In Nolan (1994a), I sketched an account of how new psychological cognitive structures might be constructed from structures already in place together with environmental input, and showed how that hypothesis might explain experimental results then available on word learning. And in Nolan (1994b), I proposed that a structure in which words are used purely referentially, representing perceptual categories, may likely precede the development from it, caused by environmentally presented problems, of true conceptual categories, understood as associated with predicates rather than as merely referring expressions. Here are two examples, oversimplified for brevity and intended only schematically, to illustrate the sort of relations proposed among cognitive structures. The word "dog" may initially be used as if it were the name of the family pet, then to refer to each neighborhood dog, both uses being based on perceptual categories alone; later, to solve problems with the extension of the word and to resolve the roles played by its superordinates and subordinates, that same word takes on predicative content, and is transformed qualitatively into a conceptual category. A second, much simpler example of the construction of a new cognitive structure from an old one in response to environmental complexities is the development from babbling to the phones of the local language. In each case, the shift may not be perceived as any simple act that the child does differently. These issues cannot be stated within the framework of Bloom's account. The proposal of Quartz and Sejnowski comes at a time when the interface between psychology and the neural sciences is in focus, making the lack of consideration of a psychological constructivist alternative to the three explanations considered by Bloom, striking. In the absence of consideration of all alternative explanations, the strategy of argument to the best explanation fails.

The exchanges generated by Quartz and Sejnowski (1997) confirm that determining what constitutes a new, constructed, cognitive neural structure requires cooperation from psychology, in the form of identifying which cognitive achievements might signal the emergence of a new cognitive structure, even though it may turn out that neural construction is in some way continual. In collecting and reviewing experimental results that are inconsistent with both associationist and language-specific constraints, Bloom's book presents significant data for the cooperative project of psychological and neurological constructivism. Furthermore, his arguments (1) that the significant cognitive structures evidenced in language development with respect to word learning are general knowledge structures, and (2) that the cognitive strategies used by the child in this task are multiple, cohere firmly with constructivist explanatory hypotheses, neural and psychological.

Some cognitive tools for word learning: The role of working memory and goal preference

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Abstract: We propose that Bloom's focus on cognitive factors involved in word learning still lacks a broader perspective. We emphasize the crucial relevance of working memory in learning elements of language. Specifically, we demonstrate through our data that in impaired populations knowledge of some linguistic elements can be dissociated according to the subcomponent of working memory (visual or verbal) involved in a task. Further, although Bloom's concentration on theory of mind as a precondition for word learning is certainly correct, theory of mind being a necessary condition does not make it a sufficient one. On the basis of our studies we point out the importance of a theory of mind related goal preference in acquiring spatial language. In general, we claim that more specific cognitive preferences and constraints should be outlined in detail for the preconditions of acquiring linguistic elements.

In Bloom's detailed and well-argued concept of the acquisition of word meaning, a rather ambitious general model is set up. According to this, both necessary and sufficient conditions for word learning are of a general nature – he speaks about computing intentions, concept formation, consideration of syntax, and "certain general learning and memory abilities" (*HCLMW,* p. 10). Rather than evaluating the impressive general model, our intention is to point out how one could elaborate on some of these assumed "general abilities."

Imagine a child who is equipped with a fully functioning theory of mind, but lacking any storage system to temporarily store forms of items. The child would certainly know that sounds have a dual relationship to the world and to the mental state of the speaker, but would not be able to consolidate the signs on account of the lack of memory. In order to consolidate both form and meaning, one has to have a working memory system among those general memory abilities. This is of course a trivial aspect, but it is the cornerstone of basic assumptions about how working memory is used in learning new words. Individual differences in word learning and vocabulary size and their relationship to working memory capacity have been aptly documented (Baddeley et al. 1998; Gathercole et al. 1992; 1997). Developmental pathology is of crucial interest in this regard.

In one of our studies (Lukács et al. 2001) we obtained some interesting dissociative relationships between vocabulary acquisition and working memory. As Table 1 shows, in Williams' syndrome subjects of a wide age range (between 6 and 20 years) the best predictor of picture naming performance for frequent words was the age of the subject. In contrast, knowledge of rare words was best predicted by verbal working memory measured in digit span. This suggests that working memory is an important cognitive resource for putting words into the mind.

Table 1 (Racsmány et al.). *Stepwise regression solutions for different vocabulary measures in Williams' syndrome*

Dependent variable	Regression coefficient	Equation	First R
Rare words	0.65	1.8.2 + 4.2 span	Span 0.65 F = 8.59
Frequent words	0.75	29.18 + 0.51 age	Age 0.75 F = 15.26

Interestingly, we also observed that a clear dissociation shows up between the acquisition of simple agreement related basic grammatical morphemes like plural and accusative, and spatial suffixes and postpositions (Racsmány et al., in preparation). In a series of suffix elicitation tasks constructed by Pléh et al. (1996), we contrasted the knowledge of spatial and non-spatial inflectional forms in Williams' syndrome children. As Table 2 shows, there is a clear dissociation between using spatial and non-spatial language (T-test for dependant samples: $t = 4{,}9$, $p < 0.01$; suffixes and postpositions taken together).

Table 2 (Racsmány et al.). *Percentage of correctly used spatial and non-spatial morphological forms in Williams' syndrome*

Non-spatial grammatical morphemes	Spatial suffixes	Spatial postpositions
87	46	49

Working memory capacity was a strong predictor of performance in morphological tasks. However, this time our analysis involved not only verbal working memory, but spatial working memory capacities as well. In a multiple regression analysis, the two modality-dependent working memory performances explained 93 percent of the variance of spatial morphological task performance ($F = 48{,}66$, $p < 0{,}001$, adjusted $R^2 = 0{,}93$). This result reveals that in the process of spatial language learning, children have to keep in mind phonological and spatial information at the same time. Spatial and verbal working memory capacity give crucial constraints for the rate and the level of spatial language learning.

There is another aspect where we would like to enrich the picture presented by Bloom. In present-day infant studies several lines of research have demonstrated that the theory of mind complex emphasized by Bloom goes through several preparatory stages until it reaches its full articulation. Bloom clearly sees not only the relevance of the theory of mind literature in explaining the details of early vocabulary acquisition, but the importance of preverbal preferences as well. We would like to point to one of these constraints that seems to be very important in both early and later stages: the notion of goal-directedness that is shown to be central in early mental representation of action (see e.g., Csibra et al. 1999). Goals are also of central importance in mental representations underlying language (Jackendoff 1994), especially the ones underlying spatial language (Landau & Jackendoff 1993). Some of our data show that this goal-directedness of human cognition is one of the easier cognitive templates of human language. In Hungarian, where there is an obligatory distinction between goal, static, and source relations, a study analyzing 12,000 utterances in children between 1;5 and 2;5 of the MacWhinney (1995) corpus showed the following percentages for spatial suffixes: goal 80%, static 13%, and source 7% (Pléh et al. 1996).

This clear preference for coding goal might well be explained by input factors. To examine this, following the model of Landau (1994), artificial spatial suffix and postposition learning situations were created where children between ages 3 and 6 ($n = 238$) had to learn the meaning of new spatial expressions (for the method and results, see Király et al. 2001; Pléh et al. 1999) in different spatial settings.

Table 3 (Racsmány et al.). *Learning artificial spatial markers with different forms. Correct binary choice percents*

Age	3;6		5;6	
	Goal	Source	Goal	Source
Suffixes	70	52	60	42
Postpositions	64	48	74	29

Table 3 shows that goal is easier to learn both in suffixes ($F_{2,92} = 8.77$, $p < 0.01$) and in postpositions ($F_{2,142} = 9.64$, $p < 0.01$), and neither the spatial settings (like *diagonal, under,* etc.) nor age had a significant effect.

Thus, goal as a cognitive template seems to be present at the earliest ages, but surprisingly enough it is still used as a template when primary language acquisition has already gone a long way.

Taking into consideration cognitive constraints such as working memory capacity and goal preferences may help to better explain individual differences and neuropsychological phenomena in language acquisition. How would strong individual differences in vocabulary size be possible among Williams' syndrome children with an unimpaired theory of mind? And how could autistic children learn language with a serious theory of mind deficit? A possible answer would be that simple cognitive mechanisms either speed up or block the process of word acquisition. The limited capacity of working memory supports language learning, works as a "language learning device" (Baddeley et al. 1998), while goals as a cognitive bootstrapping helps to unfold the meaning of words.

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Empiricist word learning

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Abstract: At first, Bloom's theory appears inimical to empiricism, since he credits very young children with highly sophisticated cognitive resources (e.g., a theory of mind and a belief that real kinds have essences), and he also attacks the empiricist's favoured learning theory, namely, associationism. We suggest that, on the contrary, the empiricist can embrace much of what Bloom says.

According to Bloom, the associationist believes "that the relationship between . . . words and what they refer to is established not through a process of reasoning and inference . . . but through a sensitivity to covariation"(*HCLMW,* p. 58). Bloom attacks associationism (Ch. 3), arguing convincingly that a correlation between a word and a type of object is not enough to make a child assign that object as the word's referent. The child must realize that someone intends to refer to the object, which entails that the child has a "theory of mind." But does this mean that the child must use a non-associationist "process of reasoning and inference" in order to learn the meaning of the word? We think not. Though Bloom establishes that word learning cannot result from a sensitivity to covariation between a word and an object, the evidence does not rule out the possibility that word learning could consist in the association of a word with *an intention to refer* to an object. Association occurs between representations, and the representations need not be basic sensory ones. For example, the associationist theory criticized by Bloom credits a child with the possession of and the ability to apply theoretical representations, namely, of words and object kinds. It is a separate (and open) question whether the empiricist can account for the acquisition of such sophisticated concepts. (For an account that could explain such acquisition, see Ryder & Favorov 2001.)

The empiricist supposes that word learning involves only *general* inferential abilities. Although Bloom acknowledges the relevance of such abilities (p. 211), and does not believe there is a mechanism whose special purpose is to facilitate word learning per se, he proposes a key role for mechanisms whose special purposes are more broadly defined – for example, a theory of mind, an understanding of principles of individuation, and an essentialist ability to form concepts. We think that the considerations Bloom marshals in favour of his theory in fact support the central involvement of a general purpose mechanism.

Consider the nature of the knowledge acquired through word learning. Despite his objections to associationism, Bloom describes learning the meaning of a word as acquiring an "association" (p. 17) or a "mapping" (p. 89) between a form and a concept or mental representation. Actually, given his position that word learning is a product of inference, it seems that what he should say is that learning the meaning of a word is learning some kind of fact. In the case of referring terms, one learns something about a word, for example, "cat": that it refers to instances of the kind *cat.* (At the same time, one learns about the kind *cat* that its instances are called "cat.") This knowledge does not differ in format from any other knowledge one might acquire about the environment.

Bloom's commitment to an essentialist theory of many concepts coheres well with the following picture of how a general purpose mechanism could explain word learning. Essentialism is adaptive (p. 153) because regularities in the world are organized around "sources of correlation" (Ryder, submitted) or "substances" (Millikan 1998; 2000). These are entities in the environment, for example, real kinds and individuals, that have a set of correlated features where this correlation occurs for a *reason.* Essentialism encourages the conceptual tracking of sources of correlation. Concepts allow one to re-identify kinds or individuals as the *same* again, so that you can infer the presence of their currently unobservable features, and learn more about them in order to facilitate re-identification in the future (Millikan 1998; 2000). This is what explains successful learning and induction (Bloom, p. 153). Obviously, it is to an organism's advantage to learn as many facts about a source of correlation as possible, to permit more induction and easier re-identification.

Now let us apply this idea to word learning. In order to believe that "cat" refers to cats, one needs three concepts: a concept of the word "cat," a concept of reference, and a concept of cats. Bloom is a psychological essentialist about the third concept; suppose that this is the correct stance to adopt towards the other two as well – after all, the word "cat" and intentions to refer are both kinds that have a set of features that are correlated for an underlying reason (social convention in one case, and human psychology in the other). According to the hypothesis bruited above, our cognitive system is set up to learn how to re-identify these kinds: here's a "cat" again, here's a cat again, and here's a case of reference again. One of the regularities these kinds participate in, and thus one way of re-identifying them, is that "cat" is used to refer to cats. Learning this fact will result from the general (essentialist driven) push to learn to re-identify each of these sources of correlation. In doing so, the general cognitive strategy will be *to make use of whatever cues are available* to help re-identify "cat"s, cats, and instances of reference. (Note that the learner need not possess any deep knowledge of the essences or individuative principles underlying the things it learns about, a possibility that Bloom acknowledges for some things [e.g., particular kinds, p. 168] but, puzzlingly, not others [e.g., individuals vs. kinds].)

Now, it emerges very clearly from Bloom's book how *resourceful* children are in learning words. It seems that if there is a valid cue available – whether artifact function, shape, substance, spatiotemporal history, others' intentions, object coherence, familiarity, importance, or syntactic context – at some point in her development, a child will make use of it. (Most of the cues Bloom rules out would actually constitute very poor evidence for what a word means.) This is just what one would expect if the above general purpose learning story were true.

All that is left to worry the empiricist is the apparent belief in essentialism that drives the whole process. We have proposed that there is no such belief, but rather that this cognitive habit emerges from a simple circuit that forms the basic building block for the entire cortical network (Ryder & Favorov 2001; Favorov & Ryder, submitted).

The other way to learn the meaning of a word

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Abstract: Bloom's book can be viewed as a long argument for an anti-Whorfian conclusion. According to Bloom, word learning is usually a process of mapping new words to pre-existing concepts. But an exception to this generalization – the learning of words from linguistic context – poses a problem for Bloom's anti-Whorfian argument.

Bloom's book is a well-supported, well-articulated, and very welcome argument against Whorfian linguistic relativism. But there may still be an important gap in the argument. According to Bloom, word learning is usually a process of mapping new words to pre-existing concepts,[1] and most of his specifically anti-Whorfian comments assume that this is the case. But there seems to be another way to learn the meaning of a word. Simply hearing a word in context, without a definition or obvious referent present, is in many cases enough to at least begin the process. The interesting thing about this type of word learning is that the concept appears to be acquired along with (or perhaps even because of) the word being learned.

Towards the end of his book, Bloom (2000, pp. 250–54) explicitly lays out some examples of the different ways to learn the meaning of a word. The first two are fairly uncontroversial examples of cases in which new words are mapped onto existing concepts (the "fendle" and "grommet" examples). At the other extreme, Bloom considers the hypothetical case of someone learning the meaning of the hockey term "hat trick" by being told that "a hat trick is when someone scores three goals in a row" (p. 254). With this definitional statement, the person learning the word is explicitly being handed a concept as well. This process of simultaneous word learning and concept acquisition can only occur because of the linguistic abilities of the teacher and the learner. Bloom correctly points out that this is not an embarrassment for the anti-Whorfian, since: "It is not the *form* of the language that causes the concepts to emerge; it is the *content* that the language conveys." (p. 254, his emphasis).

The problem with the "hat trick" example is that it is far too simple. The word "hat trick" seems to have a pretty good definition, and that definition is what is being provided to the person learning the word. So it is plausible that the learner is not acquiring a new *simple* concept HAT_TRICK,[2] but is forming a *compositional* concept something like THREE GOALS IN A ROW, which then gets mapped to the term "hat trick." In this case, it is quite clear that there is nothing Whorfian going on. In fact, the learner has not acquired any genuinely new conceptual content at all, just a convenient way of referring to a particular combination of his or her existing concepts.

But there are at least two reasons why this process of compositional concept acquisition (if that is indeed what is going on in this case) could not be very common. First of all, there is no empirical support for the idea that lots of concepts have a definitional structure, and as a result, most researchers have abandoned definitional theories of concepts altogether (Laurence & Margolis 1999). If it turns out that we need to sneak definitions back in for linguistically acquired concepts, it will be a bit of an embarrassment (also see Scott 2002).

Secondly, there are many concepts that seem to be acquired through language, but in a non-associative and non-definitional way. As an example, consider a child learning a word like "debate" by hearing it used repeatedly in conversation, but without an immediately obvious referent or definition to pair it with. I think Bloom would probably agree that a concept (not just a word) is being acquired in this example, since he comments in connection with the "debate" example that "nobody would deny that children can *learn* at least some words from hearing them used in conversation," and adds that "the precise nature of this *learning* process is a mystery" (Bloom 2000, p. 194, my emphasis).

Despite the lack of empirical evidence about this type of word learning, it is pretty clear what we have to say about it. Given the large number of words that must be learned this way, and given the lack of an obvious definition being provided, this type of exposure to new words must cause the child to instantiate a new, non-definitional concept place-holder (Bloom might prefer "essence-holder") to map to the new word (see also Millikan 1998). Prototypes or beliefs about the new concept can be formed and stored later, but the concept must be there first, or it can play no role in the child's mental life. Of course it is possible, for instance, that the child has a fully formed debate concept just waiting for an appropriate word (Fodor 1998a). But this seems fairly unlikely, and in any case, the child would have no way of knowing immediately that the word "debate" should be used to name this pre-existing concept.

So it seems that large numbers of non-definitional concepts must be at least partially formed by exposure to conversational language alone. If so, then this type of linguistic concept formation needs to be specifically addressed to keep the anti-Whorfian argument afloat. My only suggestion in this regard is that we do not need to cling to the idea that concepts are always (or even often) prior to language. All we need is a demonstration that linguistic ability *itself* does not give rise to or meaningfully constrain the ability to form concepts. Bloom's book stands as an impressive collection of empirical evidence that points towards this conclusion. But we are not quite there yet.

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NOTES

1. Following Laurence and Margolis (1999), a concept is a sub-propositional mental representation. I believe that this definition is mostly compatible with Bloom's usage.

2. Words in capitals refer to concepts; words in double quotes refer to lexical items.

Children request teaching when asking for names of objects

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Abstract: We propose that in addition to children's requests for word names being a reflection of an understanding of the referential nature of words, they may also be requests for adult's teaching. These possible requests for teaching among toddlers, along with other indications, suggest that teaching may be a natural cognition that may be related to the development of theory of mind.

In analyzing the process of learning word meanings by children, Bloom assigns a central role to children's developing theory of mind. According to this account, very young children have the ability to use mentalistic cues and figure out adults' referential intentions. As Bloom recognizes, in the normal course of learning the meaning of words, young children not only infer others' intentions when others speak, gesture, and so on, but they also actively request adults to name objects for them. In so doing, toddlers may be initiating teaching moments. We now explore this possibility.

It has been proposed (Strauss et al., in press a) that teaching, like language, can be regarded as a natural cognition. The combination of several points may support this notion. (1) Teaching without theory of mind may be found among non-primates (Caro & Hauser 1992; however, teaching with theory of mind may be species-specific to humans (Premack & Premack 1996). (2) Teaching is universal among human beings. (3) The vast majority of humans spontaneously engage in teaching even though they have not been taught how to teach, but merely exposed to it. (4) Teaching is remarkably complex. It involves myriad and multifaceted mental processes and assumptions about others' minds and how learning takes place in their minds. (5) The visible part of teaching is the external behaviors teachers exhibit. The assumptions and complex mental processes made by teachers while teaching are invisible, and they cannot be inferred from the visible part of teaching. (6) Already during their preschool years, young children show attempts to teach (Ashley & Tomasello 1998; Strauss et al., in press b; Wood et al.1995). In concert, these reasons suggest that teaching may be a natural cognition.

Frye & Ziv (in press) suggest that interpreting teaching in theory of mind terms highlights its two main components: the difference in states of knowledge of two parties as a prerequisite for teaching, and the intention to reduce the difference in knowledge by enhancing the knowledge, or understanding of the learner. Already at the age of 3 years, children can recognize that in order for teaching to occur there is a need for a knowledge gap between teacher and learner, or, in other words, that a knowledgeable person accompanied by an ignorant learner are the prerequisites for teaching. Several recent studies have investigated preschoolers' teaching strategies and suggested that children's teaching was related to their theory of mind understanding (Astington & Pelletier 1996; Wood et al. 1995). It wouldn't be surprising to hear a 3-year-old child say: "Dad, teach me (or show me) how to put this toy together."

There may be a possibility that a year earlier, toddlers at the age of 2 begin to realize that they can produce object naming on the part of others and, thus, have some understanding of the prerequisites of teaching. They may, for example, have some sensitivity to their own lack of knowledge and to the adult's different knowledge status that enables satisfying their request. According to this interpretation, the child may also have implicit knowledge that his/her request may result, on the adult's part, in an intentional reference to the specific object the child herself pointed, or referred to.

Another possibility is in line with what Bloom claims about the implicit reasoning stages underlying children's inferences about thoughts of others. Here the implicit reasoning process involved in a request for intentional teaching of objects' names may be the following:

1. Objects have names, or words that refer to them (based on previous experience/knowledge about words).
2. I don't know the word referring to this object.
3. Adults know the word referring to this object (2+3 – awareness of the knowledge gap).
4. If I point to this object the adult will pay attention to it, too (joint attention, social referencing).
5. If I ask "What's that?", the adult will name it (initiating someone else's intentional teaching).

This analysis suggests that the origins of understanding teaching as a natural cognition, specifically, beginning to appreciate the knowledge gap between the self and others, should be empirically investigated already in toddlers, and that requests for objects' names may provide a natural context for this exploration. Furthermore, exploring toddlers' emerging awareness of their own and others' knowledge may contribute to the understanding of the early developmental stages of what develops during the preschool years to children's theory of mind.

Could we please lose the mapping metaphor, please?

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Abstract: Although Bloom gives more credit to social cognition (mind reading) than do most other theorists of word learning, he does not go far enough. He still relies fundamentally on a learning process of association (or mapping), neglecting the joint attentional and cultural learning skills from which linguistic communication emerges at one year of age.

There are many things to like about this book. Most importantly, Bloom argues and presents evidence that learning words requires no special learning principles or specifically linguistic "constraints" but that, instead, it requires only general learning processes along with an understanding of other people's minds. Coming from a confirmed linguistic nativist, this proposal will hopefully be the end of the misguided search for a priori, specifically linguistic word learning constraints and principles (see Nelson 1988 and Tomasello 1992 for earlier arguments along these lines).

However, the welcome focus on mind-reading (social cognition) as fundamental to the word learning process is not taken far enough. Bloom still retains the mapping metaphor – basically just associationism – as fundamental to word learning (as do the majority of word learning theorists). Here is Bloom's summary (2000, p. 17):

> To know the meaning of a word is to have
> 1. a certain mental representation or concept
> 2. that is associated with a certain form

Under this view, two things are involved in knowing the meaning of a word – having the concept, and mapping the concept onto the right form.

But what is the nature of this association or mapping? Where is the understanding of minds here? As with many word learning theorists, Bloom's appeal to social "cues" from other persons or their minds is only on the surface – to help identify particular referents in particular circumstances. However, as I have argued in many places, a word is an intentional phenomenon through and through and this must be reflected in the fundamentals of the learning process itself, not just in "cues" (Tomasello 1992; 1995a; 1999; 2000b; 2001).

To see this most clearly, let us ask the question of why children start learning language at the specific age that they do, that is, at around one year of age. Could it be due to newly emerging skills of associative learning or mapping? No, because human infants are very good at associative learning from several months of age onwards (Haith & Benson 1997). Could it be due to word learning constraints or principles that emerge at one year of age? The problem here is that there is no independent way to observe or measure constraints; they are only inferred from the child's linguistic behavior, after the fact (this is why Smith [2000] refers to word learning constraints and principles as "skyhooks"). Could it be due to newly emerging skills of speech perception? Not really, because, although infants are gaining speech skills during this age period, they are clearly able to perceive and recognize particular isolated words from at least six months of age onwards (Juszyk 1999). Other hypotheses that Bloom considers and rejects include: syntactic knowledge, changes in parental speech, the motor control required for speech, phonological knowledge, memory, conceptual abilities, and theory of mind. His conclusion is that "In the end, nobody knows why word learning starts at about 12 months and not at six months or three years" (p. 45).

But I know why. Word learning begins when it does because it depends on a more fundamental social-cognitive skill, namely, the ability to share attention with other human beings – which emerges in nonlinguistic form near the end of the first year of life. Thus, many different studies have found that infants begin to develop joint attentional skills at around 9 to 12 months of age, including such things as following the gaze direction and gestures of adults, imitating adult actions on objects, and directing adult attention to outside objects using various kinds of gestural signals (see Tomasello 1995b, for a review). Children also show their first signs of comprehending language at around the same age, with the first linguistic productions coming soon after (Fenson et al. 1994).

Most importantly, Carpenter et al. (1998) found that children's initial comprehension and production of language correlated highly with their skills of joint attentional engagement with their mothers (i.e., their ability to engage in relatively extended bouts of attention directing and sharing). Indeed, they found that roughly half of the variability in the sizes of infants' word comprehension and production vocabularies was predicted by the amount of time (and style with which) infants spent in joint attentional interaction with their mothers during a 10-minute observation session (see Tomasello 1988, for a review of similar findings at older ages). The reason that linguistic skills are so highly correlated with joint attentional skills is simply that language is itself one type – albeit a very special type – of joint attentional skill. A language is a set of historically evolved social conventions by means of which intentional agents attempt to manipulate one another's attention.

Appeals to association or mapping do not help us to understand how children learn to use these social conventions. For instance, if we suppose that the child "maps" a novel word onto an external object, it is difficult to explain how she can then learn different words for the same object, such as *Fido, dog, animal, pet,* or *pest.* Perhaps one might say, as Bloom does in some places, that the child "maps" the word onto a concept. But not all word learning involves pre-existing concepts; much recent work shows that children sometimes learn concepts as a result of being exposed to words (Bowerman & Choi 2001). What would the mapping process look like in these cases?

In my opinion, the process is best conceived as one of establishing joint attention, in which the child must understand not just the adult's intentions to some outside entity but rather his intentions toward her attention to some outside entity; that is, the child must understand the adult's communicative intentions and then engage in a process of cultural learning in which she aligns herself with those intentions (Tomasello 1998a; 1999; 2001). A dog (or 6-month-old infant) may associate or map the sound "dinner" onto the object "food," but this does not constitute an intersubjectively understood linguistic symbol used to direct and share attention with other persons – so it is not word learning.

Thus, suppose that I am a one-year-old child encountering an adult making funny noises at me. What am I to make of this odd behavior? Perhaps nothing – perhaps it is just noise. But given that I have previous experience interpreting the adult's nonlinguistically expressed communicative intentions – in her behaviors such as pointing to and showing me things and events – I might decide that she is making these funny noises in an attempt to direct my attention to something. If I am lucky, I may figure out precisely what entity or event in the world she is attempting to direct my attention to. How is this mapping?

What we have here are not two things – a word and an object – being associated or mapped, but one person using a symbol (signifier) to indicate for another person some entity, situation, or activity (signified). Until word learning theorists understand this fundamental point and incorporate it into their theories – rather than effacing it with associationistic metaphors – their theories will continue to confuse processes of intention reading and cultural learning with those of association and mapping, and they will not be able to explain, among other things, why language emerges at the age that it does.

Could we please lose the mapping metaphor, please?

Words, grammar, and number concepts: Evidence from development and aphasia

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Abstract: Bloom's book underscores the importance of specifying the role of words and grammar in cognition. We propose that the cognitive power of language lies in the lexicon rather than grammar. We suggest ways in which studies involving children and patients with aphasia can provide insights into the basis of abstract cognition in the domain of number and mathematics.

Writing in the tradition of Brown (1973) and Macnamara (1982), Bloom provides a thorough review of research on how children learn the meanings of words. The rich texture of his book testifies to the diversity and depth of research in this area, and its implications for our understanding of how the mind of the young child works. Bloom claims that there is no mechanism that is uniquely dedicated to children's word acquisition, but that it is built on abilities that exist for other purposes such as theory of mind. He proceeds to address the impact of words and concepts in domains such as numerical reasoning, and draws upon the evidence from a range of conditions such as aphasia and deafness to examine the interplay between language and cognition. In this commentary, we examine the issues of words, grammar, and concepts and how they might reconfigure the human mind.

In the cognitive sciences, there are increasingly frequent claims that certain forms of reasoning can only be performed though access to the resources of the language faculty. In the recent research of Spelke and her colleagues, a link between language and cognition (e.g., in the form of numerical and spatial reasoning) has been demonstrated. For example, the ability to combine sources of visuo-spatial information has been reported to depend on language (Hermer-Vasquez et al. 1999). Moreover, exact arithmetic addition calculations have been shown to be associated with a language representational format, whereas estimations of magnitude are language independent (Dehaene et al. 1999).

However, there remains a critical need to establish the relative contributions of the components of language – the grammar and the lexicon – to cognitive operations. Some take the view that grammar is crucial in many sophisticated cognitive capacities such as theory of mind reasoning (e.g., Carruthers 1996; de Villiers & de Villiers 2000), whereas others maintain it is the lexicon – the pairing of concepts with linguistic forms – that configures some aspects of human cognition. Bloom describes two competing claims in the domain of numerical cognition: that of Chomsky (1988), who maintains that grammar provides a rule-based blueprint for the potentially recursive combination of individual units with potentially infinite outputs, and the alternative claim that number words create the potential to develop a mathematical faculty that extends beyond the numerical capacity apparent in preverbal infants and some non-human species (Sulkowski & Hauser 2001; Wynn 1998).

Evidence from aphasia provides important insights on the role of language in cognition, although the evidence is limited to the role of language in a mature cognitive system rather than in the initial configuring of the system. The relation of grammar to cognition can be determined from the performances of people with severe agrammatic aphasia on behavioural tasks, while the role of lexical knowledge can be established through cases of global aphasia where the system of word forms and meanings is itself profoundly impaired. Studies on theory of mind and causal reasoning in severe agrammatic aphasia have shown that the cognitive power of language lies not in the grammar (Varley 2002; Varley & Siegal 2000; Varley et al. 2001) as reasoning is retained in such instances. These studies prompt a shift in the language and thought debate from the relation of thought to an undifferentiated language faculty, to the more specific relation of the role of the lexicon in thinking.

Bloom sets out an agenda for future investigation of the numerical and mathematical abilities of people with aphasia. The challenge is to demonstrate, first, the extent to which the number faculty is retained in the absence of grammar, much as is the case for theory of mind and causal reasoning, and, second, to determine whether patients with number word processing problems are capable of dealing with numerical problems beyond the ability to estimate and discriminate small numbers that lie within the capacity of preverbal infants. Bloom's hypothesis of number words creating a capacity for mathematics is strictly developmental, with progression from small numerosities, to the acquisition of number words, leading in turn to increased mathematical understanding. In this respect, number concepts once acquired can be mapped to different surface symbolic representations. They can take the form of number words or other forms of numerical notation (such as Arabic or Roman numerals). In the established system, having acquired numerical symbols, the language scaffolding (i.e., the number words) may be removed – as occurs in cases of aphasia – but the capacity for reasoning may be retained in the form of numerals that sustain calculation. A similar situation might be observed in the case of a dissociation in the domain of music between the ability to name a note and to understand its symbolic musical value (Luria et al. 1965). Just as Bloom concludes that people without words are capable of rich mental lives because of non-linguistic conceptual structures, so might already established number concepts sustain mathematical reasoning despite impairment of surface word forms.

Bloom's survey rightly emphasizes the role of language in the acquisition of abstract concepts such as numbers. While many object and artifact names correspond to things with vivid perceptual features, others are abstract and have little existence outside of the language faculty. "Bees" differ from "beliefs," and "dogs" from "democracy." The investigation of numbers and other abstract concepts in aphasia may provide a window on the role of word forms and concepts in cognition, and the sustainability of such concepts without the associated language form. Bloom shifts the language and thought debate from grammar to the lexicon, but also provides elegant illustrations of how grammatical structure provides one ingredient among the cues that support word learning and related conceptual development. However, once established, the role of grammar as a facilitator of cognition may diminish or "switch off." In our view, it is the facility to construct and manipulate surface symbolic representations such as words and numerals that characterizes the huge adaptive advantage of human cognition and cultural transmission. An issue that cries out for investigation is to determine whether the loss of such representational systems in global aphasia necessarily accompanies impairment in numerical reasoning.

Word extension: A key to early word learning and domain-specificity

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Abstract: Bloom provides a masterful synthesis of recent advances in word-learning, placing them within the framework of abiding theoretical issues. I will augment and challenge his approach by underscoring the significance of *word extension* for questions concerning (a) the origin and evolution of infants' expectations, and (b) domain-specificity in word-learning.

1. Overview. Word learning, perhaps more than any other developmental achievement, stands at the crossroad of conceptual and linguistic organization. Consequently, the very best work engages key theoretical issues from each of these realms. This vitality on the theoretical front has been matched, pace for pace, by impressive advances on the empirical front. A major challenge in describing *how children learn the meanings of words* is to synthesize these empirical advances while placing them squarely within the context of guiding theoretical issues. Bloom has met this challenge masterfully. I will amplify and also challenge his approach by underscoring the significance of *word extension* in theories of acquisition.

2. The significance of extension. Many assume that word-learning is successful when a learner applies a new word to the same object named by her caregiver. This phenomenon, sometimes known as "fast mapping," is essential. But successful word-learning requires more. Rather than (merely) mapping words to the objects on which they were introduced, learners must extend novel words systematically (Waxman 1999; in press; Waxman & Booth 2001a).

There is an interesting wrinkle here: Different kinds of words highlight different aspects of experience. Consequently, each grammatical form supports a unique pattern of extension. In English, count nouns are typically extended on the basis of object categories; adjectives on the basis of object properties (e.g., color, texture); and proper nouns are not extended beyond the named individual. This is more than a descriptive fact; the relation between grammatical form and meaning has important logical consequences. Patterns of extension are tied to the semantics of grammatical forms. In particular, any count noun provides a logical means for tracing the identity of a designated individual within a category or kind (Macnamara 1986).

a. The origin and evolution of infants' expectations. How do learners solve this wrinkle? Which of these expectations, if any, do infants recruit at the onset of acquisition, and how do these evolve over development? I have proposed (1) that infants embark upon the task of word-learning equipped with a broad, universally-shared expectation linking novel content words (independent of grammatical form) to a wide range of commonalities among named objects; (2) that this initially general expectation is subsequently fine-tuned as infants detect the specific correlations between the surface cues for particular grammatical forms and their associated meanings in the native language; and (3) that as infants develop this more specific set of expectations, they first distinguish (count) nouns (as compared to other grammatical forms) and map them specifically to category-based commonalities; other expectations (e.g., linking adjectives to properties) emerge later (Waxman 1999b; in press).

Infants' broad initial expectation has been demonstrated by 9 to 12 months (Balaban & Waxman 1997; Waxman & Booth 2001c; 2002; Waxman & Markow 1995), early enough to guide the process of word-learning from the very outset. Following Brown (1958b), I have argued that at this early point, words serve as *invitations* to form rich inductive categories: providing a common name for a diverse set of objects directs infants' attention to commonalities among them, and initiates in infants a search for deeper, perhaps non-obvious, commonalities among named entities.

A more specific expectation, linking nouns to object categories, emerges a few months later, at approximately 14 months (Waxman 1999a; Waxman & Booth 2001b). This developmental unfolding accords well with current theories of acquisition which are predicated on an assumption that learners must first be able to map *nouns* to objects and categories of objects (Gentner 1982; Gleitman 1990; Grimshaw 1994; Huttenlocher & Smiley 1987; Maratsos 1998; Pinker 1984; Talmy 1985; Waxman 1999b). Indeed, the argument is that the acquisition of these other grammatical forms is grounded in the prior acquisition of nouns.

b. Word-learning: Domain-general or domain-specific principles? There is a broad consensus that language acquisition is supported by powerful principles of organization within the learner. Considerable debate remains as to whether these principles are dedicated specifically to language or whether they are domain-general cognitive abilities. Bloom and his colleagues have made a strong claim for the domain-general view (Bloom & Markson 2001; Markson & Bloom 1997). Booth and I have challenged this claim on both logical and empirical grounds (Waxman & Booth 2000; 2001a). Word extension figures importantly in this discussion, which began with an examination of "fast-mapping." Evidence revealed that children successfully map not only novel words (e.g., "This is a koba") but also novel facts (e.g., "My uncle gave this to me"). This indicates that fast mapping is not specific to word learning. It does not, however, constitute "evidence against a dedicated system for word-learning" as originally claimed (Markson & Bloom 1997).

The problem is the "slippage" between the scope of the evidence (that fast-mapping is not specific to word-learning) and the more sweeping claim (that word-learning is the product of domain-general cognitive abilities). Although fast-mapping figures in the acquisition of both words and facts, this does not mean that these two domains rest upon the same set of underlying principles. By analogy, discovering that one ingredient (whipped egg-whites) is involved in two recipes (lemon meringue and cheese soufflé) does not mean that the recipes share any other ingredients. The question is whether, in addition to their shared components, there are also distinct principles invoked in acquiring words versus facts.

We have documented at least one such distinction – in children's patterns of *extension.* We taught children either a novel fact or word for an object, and then examined their extensions beyond this object. The difference was dramatic. Children spontaneously and systematically extended nouns beyond the designated object to other members of the same object category, but revealed no systematicity in extending novel facts. This sharp contrast illustrates that there are indeed principles invoked in the acquisition of words that are not invoked for facts. Acquiring the appropriate extension for a word is actually a very different matter than doing so for a fact. The extension for any novel content word can be determined (roughly) by its grammatical form. These distinct patterns of extension do not rely upon previous knowledge about the particular word or the particular object to which it has been applied. In contrast, determining the extension of a fact depends crucially on knowledge about the kind of fact (e.g., enduring or transient) and the kind of object (e.g., animate or inanimate).

3. Conclusion. Word extension provides a window through which to view the evolution of word-learning and its consequences for language and conceptual development.

A multiplicity of constraints: How children learn word meaning

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Abstract: This book is an excellent and accessible overview of the position that children learn the meanings of words by applying a variety of nonlinguistic cognitive tools to the problem. We take issue with Bloom's emphasis on Theory of Mind as an explanatory mechanism for language learning; and with his claim that only unitary objects are nameable.

Psychological experiments reveal that human infants are capable of remarkable cognitive feats within the first few days of life, but most of these pass unnoticed outside of the lab. Most of us discern little behavior in young babies that we would not expect a skilled animal trainer to coax from a pigeon or a rat. Even the first tentative signs of language can be dismissed as mere parroting. At some point in the first year of life, it becomes clear that the healthy infant has a mind of his or her own. The child begins to participate actively in the symbolic world, demonstrating an ability not simply to repeat words in appropriate contexts, but also to understand what they mean.

How infants are able to master this complex task with such apparent ease is the subject of Paul Bloom's book – at least, its alleged subject. In his own final-chapter summary, Bloom baldly states that "Nobody knows how children learn the meaning of words" (p. 262). Though it is true that no one yet knows how children learn words, Bloom makes a wonderful guide to how Bloom believes they do. What he believes is worth knowing. A unifying theme is the idea that word-learning is not a unitary phenomenon at all, but rather a dynamic set of processes involving a multitude of disparate abilities. Bloom implies that acquisition of word meaning will ultimately be explained in terms of the same kind of complex set of biologically-based, evolved mechanisms by which cognitive scientists are coming to explain other cognitive abilities.

One way Bloom addresses this complexity is by presenting data suggesting that there are different ways to learn different parts of speech. He devotes individual chapters to discussing acquisition of the meanings of concrete nouns, pronouns and proper nouns, verbs, visual representations, abstract nouns, and number words. He is careful to point out that by acquiring new skills and words, the child's mind becomes different. It is not the same mind that acquires the first words and that acquires derivational morphology. There is not one single problem of "how children learn the meanings of words" but many distinct problems.

One criticism we have is that, in the early chapters, Bloom is slippery about when language starts, in large part because he sometimes uses the term "language" to refer to production and sometimes to comprehension, though the starting points of comprehension and production are quite distinct. This slipperiness could be particularly confusing to readers unfamiliar with language acquisition.

Although we are sympathetic to Bloom's general approach, we are not convinced by the emphasis he lays on Theory of Mind (ToM) as an explanatory mechanism for language learning. There are two reasons for our skepticism. One is that much of what Bloom wishes to attribute to ToM might more appropriately and more specifically be attributed to pragmatics. Although, clearly, pragmatic understanding and ToM are closely related, they are not identical. In attributing so much to ToM, Bloom abandons the fine-grained functional distinctions he has made so much of elsewhere in his book. A second problem is that ToM stands as much in need of explanation as the learning of word meaning. It is dissatisfying to explain one mystery in terms of another. We also feel it is unnecessary in this case. Although it is certainly true that neither ToM nor pragmatics are yet fully understood, a large literature has subjected them both to the same kind of fine-grained functional decomposition that Bloom is undertaking for learning of word meanings. A presentation of some of the functional components contributing to the development of ToM, pragmatics, and the learning of word meaning might have given readers a better appreciation of how they all may be underlain by similar functional primitives. We take issue in particular with the claim that "Theory of mind is the only area, other than language itself, in which [chimpanzees] are manifestly inferior to human beings" (*HCLMW,* p. 85). It is very clear that, for example, context-shifting under internal control and the related idea of inhibition of response show a similar – and relevant – disparity (Diamond 1988; 1991).

A brief analysis of functional primitives contributing to ToM would have allowed some reference to the underlying neurology and to its possible evolutionary significance. Many of the identified functional primitives believed to contribute to the development of these "abstract" cognitive skills are known to be largely under control of pre-frontal cortex. This brain region has been uniquely selected in human beings (Deacon 1997) and is widely implicated in imaging studies in adults of access to word meaning (Binder & Price 2000). If Bloom's readers were better able to appreciate the wide range of functions built upon pre-frontally-controlled functional primitives, some of the mystery of why access to word meaning has come to be possible at all might have been reduced.

Another area in which we felt Bloom's overview fell short, was in his claim (Ch. 4) that the objects underlying what can be named are unitary. Bloom writes that "a man on horseback is not an object because no children could ever learn a word that refers to such an object" (p. 93). The literature on children's compound word learning contains many findings that contradict this claim. Clark (1981) gave an example of the compound *apple juice chair,* used by a child to refer to the chair that was close to the apple juice. Another child referred to whiskers as *nose-beard* (Becker 1994). A child in Nicoladis and Yin (2001) referred to a girl holding a crab as the *crab girl.* Contrary to Bloom's claims, novel compound nouns appear in children's vocabulary very early in development and denote a variety of relationships between objects (Clark 1981).

In the context of a book about such a large and difficult topic, these criticisms are mere quibbles. We enjoyed this work, and recommend it to any reader interested in its topic. It requires little familiarity with linguistics or psychology of language, and could be used for a high-level undergraduate class on children's language acquisition. It is a good summary of a great deal of recent research in the development of meaning. Even researchers familiar with the body of work reviewed will find it handy to have as a reference. It will certainly remain on our bookshelves.

Rational statistical inference: A critical component for word learning

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Abstract: In order to account for how children can generalize words beyond a very limited set of labeled examples, Bloom's proposal of word learning requires two extensions: a better understanding of the "general learning and memory abilities" involved, and a principled framework for integrating multiple conflicting constraints on word meaning. We propose a framework based on Bayesian statistical inference that meets both of those needs.

Bloom argues that four cognitive capacities are necessary and sufficient to account for how children learn the meanings of words (*HCLMW,* p. 10): "an ability to infer the intentions of others, an ability to acquire concepts, an appreciation of syntactic structure, and certain general learning and memory abilities." We agree with many of Bloom's arguments for the necessity of these components, but perhaps more cognitive structure needs to be elucidated in order to explain how children solve the inductive problem of word learning: how to generalize a word from a very limited number of examples to an infinite set of possible referents. Specifically, more work is required on two fronts: a better understanding of the "general learning and memory abilities" involved, and an integrative framework for explaining how different sources of constraint interact to guide children's inductions of word meaning.

To see what is required, consider the following experimental word learning situation. A puppet is introduced to a 4-year-old child as having his own names for things. The puppet picks out an object (e.g., a terrier) and says, "This is a blicket." The child is then asked to give the puppet all and only the blickets from a large set of objects. In Bloom's account, several sources of knowledge act to constrain this inductive inference: linguistic knowledge – the child knows that the puppet just used a count noun; an understanding of the world – the child carves up the world in terms of objects, which belong to different natural kinds; and a theory of mind – the puppet looks at the object while naming it so that the reference of the speaker is clear. Yet, these three components are not sufficient for the child to figure out the meaning of the word "blicket" here. In particular, how does the child know whether the word refers to all animals, all dogs, or just all terriers? Each of these possible meanings is consistent with all of the above constraints, and indeed, each corresponds to a word in English that has to be learned somehow.

One possible answer that Bloom considers is a basic-level bias: children could assume that words refer to basic-level concepts. In this case, the child would assume that "blicket" refers to all and only dogs. However, two problems arise. First, the status of the basic-level bias is controversial. Callanan et al. (1994) found that the basic-level bias only showed up for familiar words, but not for unfamiliar words among children. Second, one of Bloom's arguments against the special constraints view is that such constraints (e.g., Markman's [1989] mutual exclusivity) in fact limit the kinds of words a child could learn, and the child would have to override the constraints later on. The proposed basic-level bias has exactly that problem, in limiting children to learning only words for basic-level concepts.

The induction problem becomes more interesting when multiple examples are presented. Suppose the puppet picks out three blickets, each one a terrier – is this a different situation from the one in which only a single blicket is presented? Or suppose the puppet picks out three blickets, one a terrier, the second a poodle, and the third a German Shepherd, or perhaps it picks out three blickets, one a terrier, the second one a pig, and the third a pelican – what would the learner do when asked to pick out the other blickets? Each scenario seems to suggest a different generalization as a function of the number and distribution of examples. We found that both adults and children were sensitive to these factors (Tenenbaum & Xu 2000; Xu & Tenenbaum 2001). Given one example, their generalizations tended to be graded with a soft threshold around the basic level (e.g., dog). Given three examples, they tended to choose all and only those objects matching at the level of the most specific concept including all three examples: when all three examples were terriers, they tended to choose only the other terriers (subordinate level matches), and likewise when the examples were spanned only at the basic or superordinate (e.g., animal) levels. This sharpening and focusing of generalization from one example to three examples cannot be explained by appealing to constraints imposed by learners' linguistic knowledge, conceptual knowledge, or theory of mind, because the addition of two new examples does not introduce additional information along these dimensions.

An alternative is to appeal to the fourth component of Bloom's proposal: "general learning and memory abilities." We endorse this move, but much work remains in specifying how these learning abilities could account for children's abilities to infer how far and in what ways to generalize the meaning of a word from very few examples. To address generalization, these capacities must go beyond the ability merely to remember words and facts (Ch. 2 of *HCLMW*). Neither can we appeal simply to standard general learning theories, such as those based on connectionism (e.g., O'Reilly & Munakata 2000; Rumelhart & McClelland 1986), which typically require many more than a few training examples, and both positive and negative examples, before they can generalize meaningfully to new objects.

We propose that word learning, and concept learning and reasoning more generally, is guided by domain-general mechanisms that operate according to the principles of rational statistical inference (Tenenbaum & Xu 2000; Tenenbaum & Griffiths 2001). These principles can explain why, when given three examples but not when given only a single example, learners tend to generalize up to, but no further than, the level of the most specific concept spanning the examples. If the examples represent a random sample from the extension of the word, then it would be a suspicious coincidence if the first three blickets encountered were all terriers when the word "blicket" actually referred to all dogs. This line of reasoning licenses the inference that "blicket" refers only to terriers. However, given just one example, it would not be so surprising to see a terrier labeled as "blicket" regardless of whether the word referred to just terriers, all dogs, or even all animals, which explains why learners do not restrict generalization to just terriers given only a single example.

We conjecture that this sort of statistical reasoning acts as an important inductive principle in many aspects of word learning. For instance, while children often show a "shape bias" – a tendency to extend novel words to other objects of the same shape (Landau et al. 1988) – this bias might be overcome by presenting several examples with rather different shapes but practically identical textures (Mintz & Gleitman 1998). Then more frequent generalizations based on texture might be expected, based on the reasoning that it would be a suspicious coincidence for all examples of a very general shape-based category to have the same texture if that feature was not essential to the word's meaning.

These cases also show the need for some additional cognitive structure beyond the components of syntax, concepts, theory of mind, and "general learning" that Bloom claims are necessary and sufficient to account for word learning. In particular, if the statistical force of multiple examples can override a fundamental conceptual bias towards basic-level or shape-based categories, then the learner must have some principled framework for integrating these different sources of constraint and adjudicating among them. We have suggested (Tenenbaum & Xu 2000) that the machinery of Bayesian inference might provide an appropriate integrative framework, by specifying how a rational learner ought to combine prior knowledge with input statistics. In brief, the learner's existing conceptual structure (e.g., the concepts of object, terrier, dog, animal) and her syntactic knowledge (e.g., count vs. mass noun) might jointly determine the prior probability of different candidate word meanings, while the likelihood of the observed data, given each hypothesis, would be a function of both the particular objects labeled and theory-of-mind factors such as joint attention and communicative intent that determine how labels are taken to correspond to objects. Likelihoods and priors are then combined according to Bayes' rule to yield the learner's posterior probability that each hypothesis corresponds to the true meaning of the word given all the available constraints. This framework explains why conceptual knowledge, such as the basic-level or shape biases, might carry more weight when only one example of a word has been encountered, than when multiple examples have been seen; these biases enter into the prior, which is independent of the number or distribution of examples encountered, while the force of the examples enters into the likelihood, which exerts increasing influence as the number of examples increases.

In sum, the inductive challenge of word learning – and children's success at meeting this challenge – seems to require a Bayesian inference framework, over and above the cognitive machinery that Bloom has claimed is necessary and sufficient. The Bayesian inference machinery alone is not sufficient to solve the problem, but it provides a rational framework in which the power of "general learning" abilities can be explained, and the conflicting constraints of multiple sources of information can be combined and reconciled.

Author's Response

Controversies in the study of word learning

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Abstract: *How Children Learn the Meanings of Words* (*HCLMW*) defends the theory that words are learned through sophisticated and early-emerging cognitive abilities that have evolved for other purposes; there is no dedicated mental mechanism that is special to word learning. The commentators raise a number of challenges to this theory: Does it correctly characterize the nature and development of early abilities? Does it attribute too much to children, or too little? Does it only apply to nouns, or can it also explain the acquisition of words such as verbs and determiners? More general issues are discussed as well, including the role of the input, the relationship between words and concepts, and debates over nativism, adaptationism, and modularity.

You could do worse than study word learning. It is a fascinating topic, one that has obvious clinical and educational relevance and bears directly on major issues in psychology, philosophy, and linguistics. It is a relatively easy area to study – researchers typically use simple methods such as observation of parent-child interactions, and experiments in which children and adults are taught new words and then tested as to what they think the words mean. And it is an easy field to get into. Anyone can become fully conversant in the major theories and core findings by reading a few books and a couple of dozen articles.

This last part is not entirely a good thing. For whatever reason, we know little about how children learn the meanings of words. It is a wide-open field, quite different from domains such as object recognition and syntactic processing, where there are articulated theories of the relevant psychological mechanisms and representations, and where the experimental literature is devoted to distinguishing subtle predictions that these different theories make.

My hunch is that word learning is intrinsically harder to understand than object recognition and syntactic processing. This is due to its nonmodular nature. If the theory proposed in *How Children Learn the Meanings of Words* (*HCLMW*) is right, then a complete theory of word learning awaits a complete theory of the mind in general, one that includes an explanation of how we form concepts and how we think about the minds of other people.

Still, we can make some progress. *HCLMW* is an attempt to explain precisely what goes into the act of learning a word, leaving open for future research the precise nature of these learning mechanisms. There are also claims about related issues, such as children's understanding of artwork, the role of language in our appreciation of the infinite nature of number, and the development of object cognition.

The physicist Wolfgang Pauli once sneered at another scientist's work by saying "This isn't right. It isn't even wrong." The commentators were smart, constructive, and gracious – and most of them paid me the compliment of arguing that at least some of *HCLMW* really is wrong. There are those who focus on the proposed mechanisms of word learning, suggesting that I have attributed too much to the child, or too little. Some are concerned that I do not do justice to words other than names for objects. Others hold different views about the role of input, or alternative perspectives about the relationship between language and thought. I will discuss these in turn, and conclude with some comments about the proper place of word learning in a theory of language and mind.

R1. Mechanisms of word learning

R1.1. General learning capacities

Word learning is the product of several aspects of the mind working together. Those emphasized in *HCLMW* are theory of mind, conceptual structures, and syntactic knowledge, but others include phonological abilities (to learn words, a person must be able to identity them in the input), perceptual capacities (a creature with no sense organs would lack the necessary information to learn words), and general learning and memory capacities.

Some of the commentators focus on learning and memory. **Racsmány et al.** discuss the role of working memory in word learning, focusing particularly on individual differences. **Jensen** approaches intellectual abilities, also from an individual difference perspective, and he expands on the observation that word learning is intimately related to more general cognitive capacities. (In this regard, it may be interestingly different from the learning of the rest of language: phonology, morphology, and syntax). These are exactly the sorts of issues that any complete theory of word learning is eventually going to have to address.

Xu & Tenenbaum propose a Bayesian inference framework, and argue that it is consistent with much of what we know about word learning, including the bias towards basic-level categorization, the shape bias, and children's appropriate use of multiple cues in different contexts.

There is a certain ambiguity in their presentation, however. They sometimes talk as if they are presenting a distinct and novel theory of word learning, something above and beyond what others have proposed. But for this to be convincing, they need to show that their approach can tell us something about word learning that we do not already know, or that does not follow from every other approach. As of yet, the data that they collect do not meet this criterion. It is no surprise, for instance, that someone who hears a word used to describe a terrier, a pig, and a pelican will think the word means "animal," and not "dog" – "animal" is the only interpretation consistent with the data.

A milder proposal, which they also endorse, is more plausible: Word learning is a Bayesian process, and thus all adequate theories must propose learning mechanisms that are consistent with this. We are all Bayesians, whether we know it or not.

R1.2. Theory of mind

Many researchers see word learning as the process of establishing a mapping between a form and a meaning. **Tomasello** objects to this. He worries that it is incompatible with what we know about the role of theory of mind in word learning. According to Tomasello, word learning is best understood as one person using a symbol to indicate something for another person – "mappings" is an associationist metaphor that we would do well to dispense with.

But the notion of a form-meaning mapping is not incompatible with theory of mind, is not associationist, and not a metaphor. It is a banal truth. You have not learned the meaning of a word until you have stored in memory a mapping between a phonological form and a conceptual representation. (If you do not like the word "mapping," you can replace it with "association," "relationship," or "link" – it doesn't matter.) I share **Tomasello**'s enthusiasm for theory of mind, but it shouldn't lead us into solipsism. To learn the English word "dog," it is not enough to be smart about the minds of other people. You also need to know something about English and store this knowledge in memory.

Part of the problem might be that although **Tomasello** starts off by correctly quoting me as endorsing a mapping between a word and a concept, he then moves to attacking something I do not defend – a mapping between a word and an actual object in the world. This is a different issue, but, still, I'll take the bait: What precisely is the problem with word-object mappings? **Hirsh-Pasek et al.** and **Gogate** note that when we learn to talk, part of what we learn is which words refer to which things in the world. Proper names are the simplest example of this. To learn a dog is named *Fido* is to establish a mapping between a word (*Fido*), and an object (a dog). This might be done through grasping a speaker's intention, but nonetheless, what you learn is not only about someone's intention. It is about a dog.

Hirsh-Pasek et al. are mildly skeptical about the role of theory of mind. They note that the ability to attend to social cues does not entail rich social cognition. A baby might follow her mother's gaze because of an innate mechanism, or through a past history of reinforcement, without any attribution of mental states. They also present some intriguing new data showing that 10-month-olds fail to use intentional cues when exposed to a new word in a preferential looking task.

These findings are presented as a challenge to the theory in *HCLMW,* but I don't see why. One of my central claims is that you need theory of mind to learn words. **Hirsh-Pasek et al.** find that 10-month-olds do not use theory of mind when exposed to new words. This is just as I would expect – since 10-month-olds do not learn words.

Children start to learn words by about their first birthday, and the theory in *HCLMW* predicts that at this point in development, they should show the relevant theory of mind capacities. And they do; see, for example, Johnson et al. 1998; Kuhlmeier et al., under review; Moses et al. 2001. More generally, I agree with **Tomasello** that the emergence of word learning at about the child's first birthday is the result of the emergence of the requisite social capacities. None of the data presented by **Hirsh-Pasek et al.** is inconsistent with this position.

Not everyone agrees that babies do not learn words. The sharpest challenge to this view is from **Gogate,** who suggests that more of *HCLMW* should have been devoted to discussing research with 6-month-olds. She asks, "Don't preverbal infants map words onto referents?"

We can answer that question with a question: If preverbal infants are mapping words onto referents, why are they preverbal? Why is it that 6-month-olds do not speak, and, except in the most careful laboratory circumstances, show no understanding of even the most common words? My guess is that they do not learn words. They might associate sounds with objects, in much the same way that nonhuman animals can, but they have no conception of *reference,* no understanding that words are symbols that can refer to the external world. The experiments that **Gogate** summarizes are of great interest, but they bear on babies' conceptual, phonological, and associative capacities – not word learning.

R1.3. Conceptual capacities

Gärdenfors agrees that perceptual similarity spaces are inadequate as representations of even seemingly simple concepts, and that some version of psychological essentialism is true. But he suggests that the right approach is not to give up on similarity spaces, but instead to allow for spaces that involve nonperceptual, or theoretical, dimensions. For instance, while gold and fool's gold might be similar on more perceptual dimensions, they are dissimilar on the more abstract dimension of "atomic structure." One doesn't need "essences" or "theories" as part of our psychological theory, then – you can replace them with dimensions, and retain a similarity-space analysis of concepts.

The specifics of this proposal (see Gärdenfors 2000) deserve more discussion than is possible here, and **Gärdenfors** is surely correct that we do need a theory of similarity that extends beyond the perceptual – most of the time when we say that two things are similar, or dissimilar, we are not talking about physical features. (Consider what it is to talk about the similarity of politicians, universities, or journals.) At the same time, though, there is reason to be skeptical about the prospects of explaining concepts as entities clustered together in a multi-dimensional similarity space. The problem is that nobody has ever found the right dimensions.

For instance, it would be nice to show that we think of certain people as stockbrokers because we see them as similar in certain regards. And **Gärdenfors** is correct that the dimensions in which they are similar do not have to be perceptual. But they do have to be more basic than the concept that is represented. You have not made much progress if you say that all stockbrokers are similar along the dimension of *stockbroker-ness* (see also Fodor 1998b) The failure to actually reduce any real-world concepts to simpler components is a serious problem with the similarity space approach, at least as a complete theory of conceptual representation.

R2. Alternative approaches to word learning

Nolan says that babies cannot be learning words through capacities such as theory of mind, because babies do not have such capacities. Based on considerations such as our immature birth, our neural plasticity, and economy considerations from a design stance, she endorses Lorenz's view that there was human selection for adaptability over adaptedness. Other animals have innate special capacities; humans do not.

This may be right. Of course, there are arguments, based on considerations of learnability, and economy considerations from a design stance, that favor the idea that humans are actually not so different from other animals. These arguments may be right too.

This type of debate has its place, but when it comes to a matter as empirical as what babies know, a better approach might be to actually look at research into what babies know. Such research shows that they know surprisingly much. It is not an exaggeration to say that the major result from developmental psychology in the twentieth century concerns the extraordinary abilities that human possess before they speak their first words. Much of this work, from dozens of laboratories around the world, is reviewed in *HCLMW.* If **Nolan** thinks there is a different way to interpret these data, she should say what it is.

Nolan also objects that I never consider an excellent alternative theory: the "neural constructivist proposal" of Quartz and Sejnowski (1997). This is true. I also do not consider nativism, connectionism, dualism, empiricism, modularity, or evolutionary psychology. This is because none of these are theories of word learning. They are broad theories of the mind, and while they might motivate specific theories of word learning, they do not themselves make any concrete claims or predictions in this domain.

Nolan goes on to summarize her own specific theory of learning, motivated by neural constructivism. She suggests, for instance, that a word such as "dog" starts off being used very narrowly, as the name for the family pet, and gets increasingly broader in scope. This is the sort of claim that deserves serious consideration, and is discussed in Chapters 4 and 5 of *HCLMW.* In fact, when young children first hear a word like "dog," they do not narrowly restrict it; they extend it to other members of the same basic-level category (see **Waxman**).

Ryder & Favorov endorse an empiricist view of word learning – and see me as a fellow empiricist. This is in part because at one point I say that learning a word involves forming "an association" between a form and a concept (see the reply to **Tomasello** for more on that point), and in part because *HCLMW* endorses the view that word learning involves general learning processes. But word learning, according to the analysis in *HCLMW,* involves specific abilities as well, such as theory of mind and dedicated systems of object parsing. Since the empiricist is committed to the existence of "only *general* inferential abilities," this theory is not empiricist.

R3. Do we need special mechanisms of word learning?

R3.1. Word extension

Waxman reviews her research showing that children start off with a broad expectation that words map onto objects, and later converge on narrower category-specific expectations, such that count nouns refer to objects. She then suggests that children compute the extension of words in a unique manner, one specific to lexical items. To support this claim, she summarizes a study that compares the extension of a word, *koba,* to the extension of a fact, "the one that my uncle gave me" (Waxman & Booth 2000). The word, being a count noun, was extended to other objects of the same kind as the target object. The fact was not.

As discussed in Bloom and Markson (2001), this is unsurprising. It is well known that count nouns refer to kinds, while a fact such as "the one my uncle gave me" pertains to an individual, not a kind. Of course they were generalized differently. If Waxman and Booth chose different stimuli, they would have obtained different results. It is easy to find a word that behaves like an individual-based fact – consider a proper name like "Fred." And it is easy to get children and adults to generalize on the basis of kind without using a new word – if you ask them to "find the one that it is the same kind as that one," they behave just the same way as they do if you tell them "find another koba" (Diesendruck & Bloom, in press). Words just are not that special.

While there is a lot about word learning that could conceivably be language-specific, determining the extension of a common noun is an unlikely candidate for a dedicated process. **Waxman**'s own experiments show that young children generalize names on the basis of conceptual structure – the category that the word maps onto – and not on the basis of some unspecified word-specific principle. More generally, one would expect that people would extend words such as "dog," "mortgage," and "game" on the basis of what they think are dogs, mortgages, and games. What else could it be?

R3.2. Pragmatics

Westbury & Nicoladis raise the interesting suggestion that much of what *HCLMW* attributes to theory of mind is better seen as falling into the language-specific domain of pragmatics.

One objection to this is based on parsimony. We know there is such a thing as theory of mind – reasoning about the mental lives of other people – and we know that children who are starting to learn words possess this capacity to at least some degree. So the burden of proof is on someone who claims that something else is needed. This burden gets stronger given that, as I argued in Chapter 3, the same theory of mind capacities that are involved in nonlinguistic tasks suffice for word learning (see also Bloom 2002b).

More generally, there may be no such domain as pragmatics. There is a certain class of emotions that we call "road rage," but nobody views road rage as a distinct emotion – it is just the name we give to the anger that is evoked in a certain situation. Similarly, pragmatics might be the name we give to the exercise of theory of mind in the domain of language and communication. This does not mean that the study of pragmatics is uninteresting, just that it should be thought of as an instance of theory of mind, not an alternative to it.

R4. How can we best explain the learning of words that are not object names?

Several commentators noticed that *HCLMW* is preoccupied with objects and their names. This is for several reasons. Object names are very common in children's early language, and children are strongly biased to treat new words as object names (see also **Waxman**). Object names are plausibly viewed as the entry point to the semantics of language – when children start to refer, to communicate about the external world, they do so by talking about objects. And, finally, it is where the data is. As **Harley & Piattelli-Palmarini** point out, there is disappointingly little literature on the learning of words for nonobservables such as "party and "nap," even though such words dominate adult vocabularies.

At the same time, however, *HCLMW* includes an exhaustive review of all aspects of word meaning acquisition, including names for parts and collections, adjectives, verbs, and prepositions. A chapter is devoted to number words (with implications for quantifiers in general) and another devoted to names for representations. And the theory itself applies to the learning of all of these words, although there are differences in the extent to which different learning mechanisms apply (syntax is particularly important for number words, for instance, while theory of mind is highly relevant for second-person pronouns).

Naigles, on the other hand, doubts that the same sorts of conceptual constraints that are present for object names also exist for verbs. She is presumably not denying that such constraints exist – after all, if children interpret an event in a trillion ways, each equally salient, verb learning would be impossible. Naigles' own research designs (e.g., Naigles 1990) assume, reasonably, that children are highly constrained in their interpretations of events. She presents children with a scene that can (from an adults' perspective) be interpreted in two ways and demonstrates that syntactic cues reduce the potential interpretations of the verb from two to one. Presumably, then, her proposal is that the pre-linguistic conceptual biases are *weaker* for events than for entities. When the child sees Fido, the whole object "pops out" at them; but when the child sees grandma getting a doll, there are two or three or even more interpretations that come to mind, not just one. In support of this, Naigles points out (a) that the same event can be described in many different ways: a child bringing a doll to grandma, can be *carrying, bringing, coming, arriving,* and so on, and (b) that children's first verbs are semantically diverse, including general meanings (*come*), specific ones (*draw*), and so on.

The problem with this argument is that both observations apply just as well to objects and nouns. The family pet can be *Fido, a dog, an animal, a pet,* and so on. And children's first nouns are diverse: general (*toy*), specific (*cup*), and so on. So what's the difference? In any case, if there were an event/object difference of the sort **Naigles** proposes, it would suggest, at best, that syntax plays more of a role in verb learning than in noun learning, which is fully compatible with the theory in *HCLMW.* It does not suggest that verb learning differs in kind from noun learning.

Finally, I had suggested that 1-year-olds learn verbs before they have sufficient grasp of syntactic structure. **Naigles** disagrees – but the studies she cites are unpersuasive, since none of them actually explore syntactic knowledge in 1-year-olds. Note also that my general point was that children can learn at least some verbs without syntactic cues. Does Naigles doubt this? If a child is stuck with multiple interpretations, syntax can help pick out the right one – but so can other sources of information, such as pragmatic cues and repeated exposures across different situations (see Pinker 1994a).

On a more agreeable note, **Naigles** notes the rich parallels between the theory outlined in *HCLMW* and Harry Potter's Hogwarts School of WitchCraft and Wizardry. I hope the word gets out; it might help sales of the paperback.

Bickerton discusses the acquisition of functional items, such as pronouns and determiners, and argues, based on cross-linguistic comparisons and the sorts of functional items created by Creole speakers, that there is a limited class of meanings that such words can encode. For instance, two determiners might differ on the semantic notion of definiteness (*a* vs. *the*) or individual/stuff (*much* vs. *many*), but you are not going to find determiners that express a contrast between rare versus common, or pleasant versus unpleasant. This is an important point, one that should have received more emphasis in *HCLMW.*

Bickerton makes two other claims. The first is that functional items should be particularly hard to learn. For instance, he suggests that pronouns pose a harder acquisition problem than proper names. But this seems backwards. If Bickerton is right, and children only have to consider a small set of candidate meanings (fewer than 50, say) when they hear a new pronoun, this should make learning pronouns much easier than learning proper names. (By any account, proper names can have a lot of meanings – specific people, cities, books, etc.) In general, innate constraints make learning easier, not harder.

Bickerton also worries that I won't like it if there are innate constraints that specifically apply to the meanings of functional items, presumably because it would conflict with my theory. But I like it just fine. My beef is with a dedicated module for word learning, not with representational constraints on word meanings. It seems perfectly plausible that children start with expectations as to the meanings encoded by nouns, or by verbs, or by determiners (see also **Keil**). On the other hand, Bickerton is too quick to assume that such expectations would have to be specifically linguistic. While it is conceivable that universal grammar provides a list of semantic features that correspond, say, to any functional modifier of a noun, it might also be that these semantic features emerge from a more general intuitive metaphysics. For instance, one versus many, individual versus stuff, and so on, may be natural ways to conceptualize the sorts of things that Noun Phrases (NPs) refer to, and so any linguistic elements that semantically modify nouns would be constrained in this regard.

Bickerton ends with a methodological suggestion: one can get a lot of insight about word learning by looking at unusual cases in which children are exposed to particularly impoverished input. Agreed. But Bickerton's own examples are puzzling – he thinks it is a serious omission that *HCLMW* does not discuss first-generation Creole speakers and deaf children who create their own sign languages. But while such research is fascinating, and tells us much about the sorts of conceptual and linguistic capacities that exist in the absence of exposure to a pre-existing language, it does not bear directly on the question of how children learn the meanings of words. This is because, as Bickerton himself repeatedly argues, such children don't learn words; they *create* them.

R5. What is the role of the environment?

A few of the commentators, with considerable enthusiasm, have made the following claims:

People often speak to children
Children need to hear a word in order to learn it
Children learn words in a social context

It would make for a more exciting response if I were to disagree with any of this. But plainly, for children to learn words, they do have to hear them in situations where there is good evidence as to their meaning. Indeed, in many cultures, people try to teach words to children, and children benefit from this teaching. Adults do, too. If you wanted to learn French, you would be better off with friendly people who are naming objects for you than having to pick up the language by listening to strangers in a bar.

But while these facts constrain any account of word learning, they do not themselves constitute a theory – we need to explain how children use the available information to learn words.

This is apparently controversial. **Nelson** sees the approach in *HCLMW* as too individualistic, too focused on the "heads of children." But while I agree with much of what she says – words are used in a social context, words have rich meanings – it was never clear what her alternative is. **Gogate** sees the proper goal of researchers as describing how different informational cues interact at different points in time and characterizing their relative importance over the course of development. This is a useful step, but in the end we still need to know what it is, in the child's mind, that explains how the cues are understood and utilized. (Once we know this, by the way, we will be a better position to answer the descriptive questions that interest Gogate.)

One specific question that was raised in *HCLMW* is how powerful children's learning mechanisms are. Are children good enough at learning that they can succeed in an unsympathetic environment, one where nobody is making any effort to teach them words? My tentative answer, based on the very limited cross-cultural evidence, is that they can. For this reason, perhaps, **Gogate** suggests that I underestimate the role of maternal communication. To clarify the issue, she describes some research of her own showing that mothers are sensitive to their children's level of linguistic knowledge when they name objects for them. This is a nice finding, but it is not, however, about word learning in children. It is a finding about theory of mind in mothers.

I don't doubt that children benefit from this sort of fine-tuning by their parents – in Chapter 3, I review evidence showing that they do. But this does not show that the process is necessary, or even important. As an analogy, some parents will teach their children to play checkers. It would be surprising if parents did not modify their teaching behavior based on the child's age and ability, and it would also be surprising if children did not benefit from these efforts. Would you infer from this that the only way a child could learn checkers is by being taught the game by attentive and sensitive parents?

Oddly, word learning through social interaction is sometimes seen as an alternative to attributing rich capacities to children (e.g., **Nelson**). But, as **Garton** and **Strauss & Ziv** discuss, participation in social interaction itself requires powerful theory of mind capacities. The discussion of theory of mind in *HCLMW* focused mostly on children's ability to ascertain the intentions of the speaker, but both of these commentators point out that children do much more than this. For instance, they try to elicit names for things. Even 2-year-olds will point and ask, "What's that?", an apparently simple act that exhibits children's rich tacit knowledge of the mental states of themselves and others.

R6. What is the relationship between words and concepts?

Just as with claims about the importance of the input, the position that language affects thought is often put forth with some vigor, as if it were a bold conjecture. It isn't. Nobody doubts the importance of language for an individual or a society, or the power of language as a mechanism for communicating ideas, for persuasion, threat, seduction, religious conversion, and so on. How could you convey a notion such as "the speed of light is 186,000 miles per second," without language? In fact, Chapter 9 defends the stronger claim that you cannot even *understand* such a notion without language – in particular, you need the generative system that underlies the production of number words (see also **Varley & Siegal**).

So what's the fuss about language and thought? There are actually three controversial and interesting questions:

1. How much of thought exists prior to – and independently of – language?

While there is evidence that some forms of thought, such as precise numerical reasoning, do require a natural language, one can have rich and structured thought without words. The evidence from this comes from studies of babies, of nonhuman primates, and from adults with aphasia (see again **Varley & Siegal**). The only commentator to object to this conclusion is **Nelson,** who calls it a "distortion," but never says why.

A second question is:

2. Is thought shaped only by the content of what is communicated, or does the form (the grammar) also matter?

This is the classic debate over the Sapir-Whorf hypothesis. Whorf famously claimed, for instance, that English speakers think differently about time and space than Hopi speakers because of their linguistic experience. Importantly, Whorf focused on the form of language – his claim would be a lot less interesting if he were simply stating that Hopi adults tell their children one thing about time and space and Americans tell them another. Instead, he was saying that the structural properties of the different languages have this effect.

There has been some recent interest in this proposal, and some new empirical work. In the end, as argued in Chapter 10, I think that the evidence for it in general is still quite weak (see also Bloom & Keil 2001). But consider now a potentially related question:

3. To what extent are concepts shaped by patterns of naming?

To see the issue here, imagine a child who already has a mature understanding of what a dog is, and hears Fido being called "a dog." Assuming the child makes the proper mapping, he or she could go on to use the word exactly right; calling all and only dogs "dogs." To the extent that this is an accurate conception of what happens in word learning, naming plays no role in the shaping of concepts.

In contrast, consider **Scott**'s example of "debate." A child might hear the word for the first time but only have a vague idea as to what a debate is. Over the course of time, by hearing the word used to refer to diverse events and reasoning about what these events have in common, he or she learns more about what debates are. As Block (1986) points out, this sort of process is particularly relevant for the acquisition of more abstract and theory-laden notions – your understanding of genes, mortgages, and phonemes is likely to emerge as the result of hearing how people use the words "gene," "mortgage," and "phoneme." This is what **Maratsos** describes as "category assembly," where a concept is shaped through exposure to language, as opposed to "category recognition," where a pre-existing category is triggered by exposure to a word.

Both category-assembly and category-recognition apply for the learning of any given word, though they differ in their relative importance. **Maratsos** gives the example of the weird Tzeltal word that means "to ingest crunchy foods." Maratsos is correct that there is a lot of category assembly here; the word is unlikely to immediately map onto the right concept. But there is also category recognition – upon hearing the word for the first time, it is likely that the child has some notion of the appropriate concept, perhaps that it has something to do with eating. **Murphy** gives the example of the weird English word "bird," and notes that while children might start off with a pretty good rough idea as to the boundaries of the category (category-recognition), their concepts of bird will later be expanded, for instance, by hearing penguins described as "birds" – a clear case of category assembly.

Scott wonders if the existence of category assembly shows that Whorf was right. It does not. Recall that the interest of the Whorfian view is that language has its effects through the form of a language, and so it would really be striking evidence for this position if the child's concept of bird was affected by the fact that the English word "bird" has one syllable, or if her crunchy-eating notion was affected by the fact that Tzeltal has classifiers. But this does not happen. When a child learn that penguins are birds by hearing them called "birds," this is contentful learning of the most straightforward type, such as learning the nationality of an actor by hearing him described as "Canadian" or the status of a food by hearing it called "a fruit."

After all, if the effect of names were based on their form, not their content, you would expect children to have problems with different words that sound the same. The same sound is used to describe the flying creature and the object for hitting baseballs, and a child who assumed that same sound = same category, would form a very general category of *bat* that contains both objects. But children do not do this. In fact, the literature (e.g., Davidson & Gelman 1990) reviewed in Chapter 10 shows that children only use names to enlarge and restrict category membership when they have independent semantic support to do so. It is not enough to hear a penguin called "bird," the child needs good reason to believe that it actually is a bird – a member of the same category as robins and sparrows – before the category is enlarged.

Maratsos and **Murphy** discuss the relative contributions of category-recognition and category-assembly, and both note that *HCLMW* emphasizes the process of category-recognition. This is because of the observational and experimental evidence suggesting that when children learn their first words – *cup, drink, Fido,* and so on – they get the meanings pretty much correct from the very start. Serious errors never occur, suggesting that children do not need the shaping power of multiple exposures. Category-assembly might be needed for the fine-tuning of word meaning (learning that penguins are birds, and so on), but is not the dominant process of word learning.

Maratsos has a clever reply to this: He suggests that children really do need multiple exposures in early word learning. They make so few mistakes because they are cautious, and tend not to use a word until they have sufficient informational support that they have converged on the right hypothesis. I think the experimental data suggest that this is not the case for object name learning – but it is a more open question whether this sort of caution exists for verbs and names for abstract entities; this is an issue that warrants further research.

Murphy wonders why children who are so good at learning words taught to them by developmental psychologists turn into dull college undergraduates who are so bad at learning words taught to them by cognitive psychologists. He has several explanations for this, all of which make sense, and concludes by stressing the need to use more realistic conceptual structures in our experiments, which also makes sense. My problem is with his suggestion that the appropriate word learning experiments would have to explore "the long task of filling in the exceptions and unpredictable cases" – in other words, that there should be more focus on situations that require category assembly. **Murphy** notes that there is a sense in which a child does not actually learn the word "bird" until it is used with near-perfect accuracy, which may have to wait until adulthood. But this is not the usual conception of word learning. The perspective adopted in *HCLMW* is that children learn "bird" when they have a rough idea as to what the word refers to. In the end, this might be just a definitional issue, one that reflects different research interests, but from my perspective, the process of working out the hard cases and atypical instances is an important part of conceptual development – but it is not word learning.

R7. The big picture: Word learning, nativism, modularity, and natural selection

Keil observes that, while the learning of word meanings may be the result of general cognitive processes, the development of syntax, phonology, and morphology may not be. In fact, there are reasons to expect them to be different. These other aspects of language are likely to be modular; the conditions under which you can extract a NP from a complement are specified in a way that does not make reference to the rest of mental life, and the learning of the language-specific conditions for NP extraction might be similarly encapsulated. In contrast, word meanings are nonmodular; the meaning of the word "mortgage" is intimately tied to your understanding of other things as well, and the same holds true for how we come to learn this English word.

Keil is also correct that the position in *HCLMW* does not preclude the existence of powerful constraints on the semantics of language. The learning of word meanings might be the product of general processes, but the *representation* of word meanings might involve special constraints. At the same time, though, it is far from clear that such specifically semantic constraints exist. Possible words might be delimited by possible concepts (what ideas we can entertain) and by social considerations such as what notions are important enough to warrant their own word. This approach has obvious merit when it comes to words such as "mortgage" and "dog"; my bet is that it can be extended to modals, quantifiers, and the like (see also my response to **Bickerton**).

While **Keil** is careful to insulate the study of word learning from other acquisition issues and other representational issues, **Harley & Piattelli-Palmarini** take the opposite approach, and engage in gentle psychoanalysis. They are perplexed that "an unapologetic nativist" can endorse a nonmodular position on word learning, and they see the roots of this conflict following in subtle ways from my neo-Darwinian upbringing.

I think I can clear up some of this. Nativism, modularity, and adaptationism are logically distinct. One can be a nativist without believing that natural selection has played a substantive role in the mind's origin (e.g., Plato), one can be an adaptationist while rejecting the notion of modules (e.g., Skinner), and so on. Nonetheless, the three ideas are related. Most scholars who endorse the existence of innate capacities assume at least a weak notion of modularity. Strong adaptationism suggests a nativist and modular position on the mind (Cosmides & Tooby 1994), and conversely, if you assert that some capacity is an innate module, it most likely follows that it is the product of natural selection (Pinker & Bloom 1990). And so I agree with **Harley & Piattelli-Palmarini** that there really *is* a tension between adaptationism and nativism on the one hand, and a strong nonmodular position on the other.

The way to resolve this is to appreciate that all three of these doctrines – nativism, modularity, and adaptationism – are not global stances towards the mind; they are positions regarding certain specific capacities. Most everyone, for instance, holds all three of these positions towards low-level visual perception, while nobody holds them regarding the ability to play miniature golf. (When you call someone a "nativist" or "adaptationist" you don't mean that they believe *everything* is innate and adapted; you typically mean that they believe that more things are innate and adapted than you do.)

So I can now resolve **Harley & Piattelli-Palmarini**'s dilemma: It is fully consistent for me to claim that word learning is nonmodular because *I am not nativist or adaptationist about word learning.* The argument in *HCLMW* is that while many of the capacities that underlie word learning might be innate adaptations, word learning itself is not. It is a lucky accident. The mind is heterogeneous in its nature and development, and while some very interesting aspects of human nature may well be innate, modular, and adapted, some other very interesting aspects of human nature, such as the ability to learn the meanings of words, may not be.

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Letters "a" and "r" appearing before authors' initials refer to target article and response, respectively.

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