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The Poverty of the Stimulus Argument[1]

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ABSTRACT

Noam Chomsky's Poverty of the Stimulus Argument is one of the most famous and controversial arguments in the study of language and the mind. Though widely endorsed by linguists, the argument has met with much resistance in philosophy. Unfortunately, philosophical critics have often failed to fully appreciate the power of the argument. In this paper, we provide a systematic presentation of the Poverty of the Stimulus Argument, clarifying its structure, content, and evidential base. We defend the argument against a variety of philosophical criticisms, new and old, and argue that the Poverty of the Stimulus Argument continues to deserve its guiding role in the study of language and the mind.

[1] This paper is fully collaborative; the order of the authors' names is arbitrary.

1 Introduction

Noam Chomsky's Poverty of the Stimulus Argument is one of the most renowned arguments in all of cognitive science. Not only has it guided the vast majority of theorizing in linguistics over the last forty years, but it has inspired nativist accounts of a wide variety of cognitive abilities in other domains. At the same time, claims about the innate structure of the mind have always provoked controversy, so it is not surprising to find that the argument is embroiled in dispute.

Philosophers have long recognized the significance of the Poverty of the Stimulus Argument and have submitted it to a fair amount of scrutiny, much of it negative. In rejecting the argument, philosophers have claimed, among other things, that it is incoherent, unsubstantiated, and rooted in misconceptions about the nature of language and language acquisition. Such assessments trace back to Nelson Goodman's and Hilary Putnam's classic early discussions of the argument, but they continue to the present day. The latest installment in this tradition is a detailed critique by Fiona Cowie. She claims that the Poverty of the Stimulus Argument 'fails abysmally on both empirical and conceptual grounds to support nativism about language learning' and consequently that it doesn't offer 'the least reason to think that there is a special faculty for language acquisition' (Cowie [1999], pp. 310–1).

We believe that this whole tradition is misguided and that the criticisms that philosophers have brought to bear against Chomsky are deeply flawed. In this paper we undertake a full dress defense of the Poverty of the Stimulus Argument. We show that its philosophical critics frequently distort the logic of the argument and consistently underestimate the evidence in its favor. Quite simply, none of their criticisms provides any serious reason to question the argument's cogency. The Poverty of the Stimulus Argument is alive and well and continues to deserve its guiding role in the study of language and the mind.

We proceed as follows. In Section 2, we set out some of the relevant background on nativism and empiricism and what's at stake in the Poverty of the Stimulus Argument. In Section 3, we turn to the argument itself, clarifying its structure, content, and evidential support. As we see it, the Poverty of the Stimulus Argument offers an extremely powerful case for nativist approaches to language. Section 4 strengthens the case by canvassing a number of supplementary arguments for nativist models. We then turn to philosophical criticisms in Section 5, focusing initially on Goodman's and Putnam's arguments against Chomsky. In Section 6, following the work of Stephen Stich, Michael Devitt, Kim Sterelny, and Fiona Cowie, we distinguish several varieties of nativism and empiricism and also address an

important argument given by both Stich and Devitt and Sterelny against one form of nativism. Finally, in Sections 7 and 8 we evaluate Cowie's assessment of the Poverty of the Stimulus Argument, which draws on and develops the critical tradition originating with Goodman and Putnam.

2 Empiricism and nativism

Before we turn to the Poverty of the Stimulus Argument itself, we should say a word or two about what's at stake. The debate between empiricists and nativists goes back to the very beginning of philosophy. But what exactly is the issue?

Popular accounts sometimes suggest that while nativists hold that the mind has many innate features, empiricists hold that nothing is innate, that the mind is a 'blank slate'. Unfortunately, this overstates the difference between the two, since no one really maintains that the mind has no innate structure at all. It's widely recognized that there has to be *something* different about humans and, as it might be, rocks, that accounts for the fact that humans (but not rocks) learn to speak natural language, do arithmetic, recognize faces, etc. Any sensible account of the mind has to respect such platitudes and, consequently, is committed to some innate structure. The substantive question concerns the character of the innate structure, not whether there is any. As W. V. O. Quine has noted, even a behaviorist 'is knowingly and cheerfully up to his neck in innate mechanisms' ([1969], pp. 95–96).[2]

The difference between nativists and empiricists, then, resides in the quantity and richness of innate structure that they endorse. Though there is clearly a continuum of positions, empiricist models are on the side that attributes few innate ideas, principles, and mechanisms, and generally considers the innate material of the mind to be domain-neutral and relatively simple; empiricist models also tend to give special weight to the role of sensory systems, in the most extreme cases maintaining that all concepts are constructed from sensory constituents. By contrast, nativist models are on the side that views the mind as highly differentiated, composed of far more innate elements, including domain-specific systems of knowledge or principles of inference, and innate concepts of arbitrary levels of abstraction.

On either view, the question arises of what it is for something to be innate, or, as an earlier generation of philosophers might have put it, what it means to say that something is innate. This is a difficult issue, one that we won't be

[2] *Cf.* Chomsky as well: '[A]ll rational approaches to the problems of learning, including "associationism" and many others that I discuss, attribute innate structure to the organism [. . .] the question is not whether innate structure is a prerequisite for learning, but rather what it is' ([1980], p. 310).

able to settle here, though we do want to point to what we think is the correct orientation.[3] As we see it, 'innate' is a theoretical term, and like other theoretical terms it can only be elucidated in the course of theorizing about the target phenomena. For an initial sense of where things are heading, however, one can look to a number of paradigmatic cases. Pretty clearly, our having arms and legs and hearts and livers is part of our innate endowment. In contrast, scars and haircuts aren't innate. Examples like these may not be a substitute for a rigorous analysis of innateness, but they provide a good sense of what is at issue. They also help to further clarify what is *not* at issue.

Consider that a common characterization of innateness is that it concerns what's present at birth. This view of the matter is clearly inadequate. If anything is innate, having teeth is. So are the biological changes associated with puberty and, sadly, the genetically transmitted diseases whose symptoms don't appear until late in life. These cases show that present-at-birth isn't a necessary condition for innateness (Chomsky [1987]; Stich [1975]). Surprisingly, however, it isn't a sufficient condition either. Infants whose mothers repeatedly read the same story aloud during the last six weeks of pregnancy are, as newborns, selectively reinforced by the story in operant conditioning tasks (DeCasper and Spence [1986]). Indeed, the evidence suggests that infants begin the process of acquiring language prior to birth. For instance, two-day-olds are capable of discriminating their mother's language from other languages and, when given a choice, prefer to listen to the language that their mother speaks (Moon *et al.* [1993]). The likely explanation of these results is that newborns are sensitive to the distinctive prosodic features of the language, a sensitivity they acquire in the womb (see Jusczyk [1997]).

We will be focusing on the issue of nativism as it relates to natural language. Thus our question will be about the character of the innate ideas, constraints, or mechanisms that play an important role in language acquisition. At the heart of the disagreement between nativists and empiricists on this issue is their assessment of the Poverty of the Stimulus Argument. In the next section, we set out and elaborate in detail what we take the argument to be and explain why it provides a strong case for nativist models of language.

3 The prima facie case for nativist models of language

3.1 What is the Poverty of the Stimulus Argument?

The Poverty of the Stimulus Argument is more a collection of related, mutually supporting claims than a neatly circumscribed argument—at least in

[3] For some different views of what innateness amounts to, see Stich ([1975]), Ariew ([1996]), Sober ([1998]), and Samuels ([MS]).

Chomsky's writings. In fact, Chomsky's discussions of the empiricism/nativism dispute often range over a large variety of arguments all at once, making it difficult to get at the precise structure of each individual argument.[4] Nonetheless, the general idea behind the Poverty of the Stimulus Argument is clear. It is that the knowledge acquired in language acquisition far outstrips the information that is available in the environment (i.e., the 'primary linguistic data'); or as philosophers sometimes put it, the output of the language acquisition process is radically underdetermined by the input. Equally important, however, is the fact that the principles that embody our knowledge of language are by no means the simplest or most natural from a pretheoretic point of view.[5]

Bringing these points together, we arrive at what we call the Standard Philosophical Version of the Poverty of the Stimulus Argument, or the *Standard POS Argument* for short (see, e.g., Stich [1978], [1979]; Devitt and Sterelny [1987]):

The Standard POS Argument

1. An indefinite number of alternative sets of principles are consistent with the regularities found in the primary linguistic data.
2. The correct set of principles needn't be (and typically isn't) in any pretheoretic sense simpler or more natural than the alternatives.
3. The data that would be needed for choosing among these sets of principles are in many cases not the sort of data that are available to an empiricist learner in the child's epistemic situation.
4. So if children were empiricist learners, they couldn't reliably arrive at the correct grammar for their language.
5. Children do reliably arrive at the correct grammar for their language.
6. Therefore, children aren't empiricist learners.

By an *empiricist learner*, we mean one that instantiates an empiricist learning theory. Among other things, such a learner wouldn't have any innate domain-specific knowledge or biases to guide her learning and, in particular, wouldn't have any innate *language-specific* knowledge or biases. What the argument points out is that if a child were an empiricist learner, she'd be limited in the way that she could discern the correct set of principles underlying a target language. One might have thought that she could just opt

[4] Moreover, Chomsky often mixes discussions of the philosophical issue of nativism with presentations of sizeable chunks of linguistic theory. As will become clear, this is actually a very reasonable strategy, given the structure of the Poverty of the Stimulus Argument. But it does make it more difficult to extract the core structure of the argument.

[5] For ease of exposition, we will sometimes drop the qualifying phrase, but whenever we refer to a principle as being simple and natural, we mean simple and natural *from a pretheoretic point of view*.

for the simplest or most natural set in exactly the way that people are expected to operate when confronted with other tasks of inductive inference. But as it turns out, in the special case of language learning, this strategy fails to work. The correct set of principles isn't simple or natural in any pretheoretic sense. This means that the empiricist learner has to rely on there being sufficient environmental information to guide her through the vast number of competing sets of principles. But here the empiricist learner faces another obstacle: the decisive data just aren't available. The upshot is that, when it comes to language, children aren't empiricist learners.

We think that the Standard POS is aptly named and that philosophers have construed the argument in much this way. At the same time, the full power of the argument hasn't been generally acknowledged. We suspect this is due, in part, to the fact that the crucial premises about the number and the relative simplicity of alternative hypotheses are rarely elaborated at an appropriate level of detail.[6] We think this is a crucial mistake, one that must be rectified in order to gain a proper appreciation of the Chomskyan view of language. For this reason, we propose to spend much of the rest of this section clarifying the enormous difficulties facing an empiricist language learner. Though our discussion will be somewhat lengthy, we will still only be able to scratch the surface.

3.2 Obstacles for empiricist learners

According to the Standard POS Argument, there are numerous possible alternatives to the correct principles governing a language, and the correct ones aren't the simplest or most natural. Clearly, the best way to come to appreciate these points is to engage in linguistic theorizing—to actually do some linguistics. Yet not everyone has the time or inclination to devote themselves to such study, and needless to say, we don't have the space here to present a course in linguistics! Faced with this difficulty, Chomsky and many other authors have resorted to brief presentations of some sample linguistic regularities, along with various hypotheses that one might put forward to account for these in the context of a comprehensive grammar.

Let's begin with one of the more widely cited cases. Consider the following sentences:

(1) Ecuador is in South America.
(2) Is Ecuador in South America?
(3) Julie's telephone is working again now that the storm is over.
(4) Is Julie's telephone working again now that the storm is over?

[6] Often enough, they are simply asserted or else treated as a mathematical consequence of the fact that grammars are recursive systems.

What principle might be responsible for the regularity underlying (1)–(4)? The simplest and most natural hypothesis would seem to be that, to form a yes-no question, one moves the first 'is' to the front of the sentence. Unfortunately, this hypothesis is incorrect, as can be seen in (5) and (6):

(5) That woman who is walking her dog is Tom's neighbor.
(6) *Is that woman who walking her dog is Tom's neighbor?[7]

Of course, the correct yes-no question corresponding to (5) is (7):

(7) Is that woman who is walking her dog Tom's neighbor?

So what is the relevant principle governing these sentences? The standard answer is that one should move the auxiliary verb from the main clause to the front of the sentence.[8] The reason why (6) is ungrammatical is because the auxiliary that's been moved is not from the main clause but rather is one that is embedded in the subject NP.

Notice the level of sophistication that is required of a child whose task it is to learn principles like these. At the very least, she needs to possess the concepts MAIN CLAUSE and AUXILIARY VERB. These are extremely subtle concepts, by any standard. Consider, for instance, auxiliary verbs and imagine being in the position of a child learning her first language. Would you have a reason to distinguish them from lexical verbs? In a recent summary of her important work on English auxiliaries, the psychologist Karin Stromswold notes ([1999], p. 361):

> the two types of verbs are so semantically, syntactically, and lexically similar to one another that a learner who has no knowledge about auxiliary and lexical verbs (i.e., a simple, unbiased, correlational learner) is almost certain to confuse the two types [...] The remarkable degree of similarity can be appreciated by comparing pairs of sentences such as *he is sleepy* and *he is sleeping*, *he has cookies* and *he has eaten cookies*, and *he does windows* and *he does not do windows*.[9]

Stromswold goes on to argue that there are a large number of errors that might tempt children if they failed to distinguish main from auxiliary verbs. Thus in Figure 1, one could easily generalize from the patterns on the left to the patterns on the right. To someone who doesn't already know English, the patterns on the right would be straightforward extensions of the patterns on the left. And yet children don't make errors of this sort.

[7] Following the standard convention in linguistics, we will mark ungrammatical sentences with a '*'.

[8] This principle is *structure-dependent* in the sense that it is framed in terms of constituent structure. In contrast, the principle that generates (6)—move the first 'is'—is framed simply in terms of the order in which the words occur.

[9] In each pair, the first sentence includes a main verb ('is', 'has', 'does') and the second sentence includes a homophonic auxiliary ('is', 'has', 'does').

He seems happy. ⇒ Does he seem happy?	She could go. ⇒ *Does she could go?
He did eat. ⇒ He didn't eat.	He did a few things. ⇒ *He didn't a few things.
He did eat. ⇒ Did he eat?	He did a few things. ⇒ *Did he a few things?
I like going. ⇒ He likes going.	I can go. ⇒ *He cans go.
They want to sleep. ⇒ They wanted to sleep.	They are sleeping. ⇒ *They are'd (or be'd) sleeping.

Fig. 1. Some errors that might tempt children if they failed to distinguish main from auxiliary verbs, adapted from Pinker ([1994]) p. 272, based on data in Stromswold ([1990]).

Stromswold reports having examined fourteen children and 66,000 auxiliary-containing utterances, all told. In this entire corpus, she couldn't find any of the telling errors that would indicate the children were confounding auxiliary and lexical verbs. Children's facility with auxiliaries is no small feat. As Stromswold points out, there are on the order of 10^{22} combinations of the English auxiliaries, of which only 99 are in fact grammatically possible.[10] This number—the number of possible combinations of auxiliaries—is about 50,000 times the number of seconds since the beginning of the universe and about a hundred billion times the number of neurons in the human brain. Of course, these are possible orderings of auxiliaries, not possible sets of principles, but they do give some feel for the scope of the problem that the child faces.

Interestingly, the difficulties surrounding auxiliaries have always been at the forefront of linguistic theory. Linguists have long understood that the auxiliaries are intimately tied to basic clause structure. Clause structure, however, provides a fairly typical case of how complicated and unnatural linguistic principles can get. Consider, for example, the account of basic clause structure associated with a recent influential syntactic theory, Chomsky's ([1995]) 'minimalist' theory (see Figure 2).

We suspect that this isn't the first account that an unbiased learner would arrive at, especially an unbiased two-year-old! Nonetheless, it is part of an extremely elegant and powerful theory of the overall structure of English and other natural languages.

[10] Specifically, there are 23 auxiliaries, yielding 23! ($=2.59\times10^{22}$) possible combinations of all 23 auxiliaries. As Stromswold notes, 'the total number of orders including sets with fewer than 23 auxiliaries is considerably bigger. Because 23! is the largest term in the summation, it serves as a lower bound for the number of unique orders and suffices as an estimation of the number of orders' ([2000], p. 926).

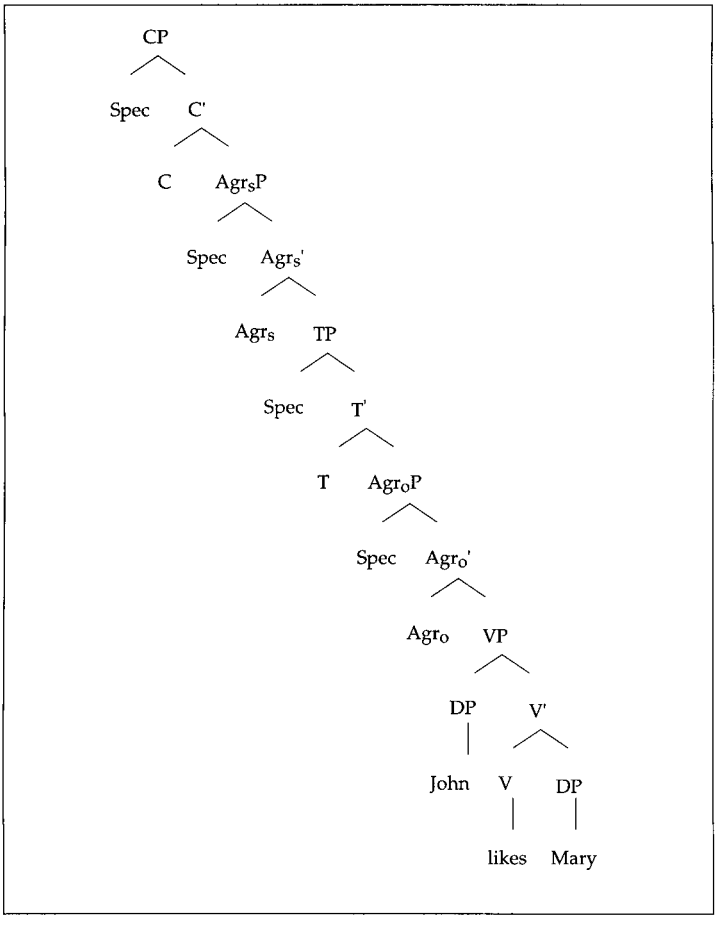

Fig. 2. The initial structure of the sentence 'John likes Mary' according to Minimalist Theory.[11]

In theorizing about a language's structure, it's sometimes easy to overlook the unnaturalness of its underlying principles if you already know the language; its regularities seem inevitable. So let's look at a non-English example. Consider the sentences (8)–(11).[12]

(8) Juan arregla el carro.
Juan fixes the car.

[11] After this stage, movement is assumed to take place. In fact, according to checking theory, the subject, verb, and object all move from their original positions in the derivation of the sentence in order to 'check' their features. When they move is a function of the 'strength' of the associated features. In English, the movement of objects and verbs occurs later than the movement of subjects and is not reflected in surface form.

[12] The example is taken from Chomsky ([1988]); we've renumbered the displayed sentences.

(9) Juan hizo arreglar el carro.
Juan made fix the car. (word for word translation)
Juan had someone fix the car. (natural translation)

(10) Juan afeita a Pedro.
Juan shaves to Pedro. (word for word translation)
Juan shaves Pedro. (natural translation)

(11) Juan hizo afeitar a Pedro.
Juan made shave to Pedro. (word for word translation)
Juan had someone shave Pedro. (natural translation)

There is a clear pattern in these sentences. Now suppose one wants to say that Juan didn't just have someone or other fix the car, but rather that Juan had Maria fix the car. How would this be done? The answer is to use (12).

(12) Juan hizo arreglar el carro a Maria.
Juan made fix the car to Maria. (word for word translation)
Juan had Maria fix the car. (natural translation)

But if (12) is the correct way to say that Juan had Maria fix the car, then it would be extremely natural to think that (13) is the correct way to say that Juan had Maria shave Pedro.

(13) *Juan hizo afeitar a Pedro a Maria.
Juan made shave to Pedro to Maria. (word for word translation)
Juan had Maria shave Pedro.

As it turns out, however, this isn't right at all. (29) is clearly ungrammatical for native Spanish speakers, just as *'Does she could go?' and *'He didn't a few things' are for native English speakers. The correct form is: 'Juan hizo que Maria afeitase a Pedro'. But to people who aren't themselves native Spanish speakers, this really ought to seem bizarre. Why use such a baroque construction when one could easily follow the obvious and simpler generalization embodied in (13)? The answer, of course, is that there is nothing special about Spanish here. All natural languages constantly flout comparable generalizations. What's amazing is that children somehow avoid these tempting errors; this fact—the core insight behind the poverty of the stimulus argument—is perhaps the most fundamental fact about language acquisition.

3.3 Further obstacles for empiricist learners

At this point, it might help to step back to get a better sense of the general epistemic situation in which empiricist learners find themselves. If children were empiricist learners, they couldn't make any assumptions whatsoever about language. As fluent speakers, we tend to think of language as an

objective phenomenon; words and sentences are manifest in our experience in much the same way that chairs and rocks are. But they wouldn't be that way to an unbiased learner. A truly unbiased learner couldn't even assume that some of the noises she hears are linguistic. Suppose, however, that she gets over that hurdle. An important early step would be to identify the basic sound units of the language. Notice, however, that not all of the sounds that people make are linguistic—coughs, whistles, gurgles, and so on must be filtered out. Moreover, all sorts of variations in speech aren't linguistically salient. Each voice has a different timbre. Male voices tend to be lower than female voices. People speak faster or slower, louder or softer, and with different intonations depending on a wide variety of variables. They sing, whisper, mumble, slur their speech, all affecting the sounds they make. None of these variations matters to language per se. But why should an unbiased learner suppose that?

Even moving beyond extra-linguistic sounds and features of speech, the sound system of natural language remains extremely complex. Words are represented in terms of abstract phonological features that aren't directly realized. Instead, they are mediated by an intricate set of principles that apply in a particular order, cyclically, in order to produce the actual sounds that we make in speech. For instance, the 'p' in 'pit' is qualitatively different from the 'p' in 'spit', despite the fact that the same phonological element is at play. It's given different phonetic realizations in these contexts—something that English speakers aren't the least bit conscious of. In other cases, the opposite effect occurs, that is, distinct phonological elements may be given the very same phonetic realization. For example, while the final sound in 'plot' and 'plod' are clearly distinct, this distinction evaporates once '-ing' is added. Thus, in a number dialects, 'plotting' and 'plodding'—and, similarly, 'wetting' and 'wedding', or 'butting' and 'budding'—are pronounced identically (Bromberger and Halle, [1991]). So words are not merely ordered bits of sounds, or even ordered lists of phonological elements. Similar points apply to a variety of other phonological properties, including (among others) stress, rhythm, intonation, and syllabification. For instance, the stress patterns of words are also determined by a complex set of principles that apply in strict cyclic order (see e.g. Halle [1998]). These rules explain the differing stress in otherwise closely related words such as 'origin', 'original', and 'originality'. Needless to say, all of this greatly complicates the infant's task of recovering the basic sound structure of her language. It also complicates the infant's task of recovering the lexicon, since roots of words can have different phonetic realizations and stress patterns in different contexts. Why should a child allow herself to think that a word like 'electric' is the same as the root in 'electricity' when the two are pronounced as differently as 'kick' and 'kiss'?

The ultimate goal in understanding language is to determine the sound/meaning pairing. But here as elsewhere there is nothing inevitable about the relevant principles. Why, for example, should the child assume that meaning is mediated by words? Why not try to attach meanings directly to more basic sounds like 'pa' and 'ba'? Given that word boundaries don't come pre-marked,[13] this would be an entirely natural supposition. Another possibility is to simply identify a small number of repeated units and assign them propositional contents, as other species apparently do.[14] Yet another possibility is to go in the other direction, treating longer stretches of sound (whole phrases and sentences) as the direct bearers of meaning. To a truly unbiased learner, there's nothing sacrosanct about words.

The sound/meaning pairing is also complicated by features of language that we all take for granted, for instance the pervasiveness of ambiguity. Ambiguities don't just arise because of the constraints on phonological realization ('plodding' and 'plotting'). There are also outright lexical ambiguities ('Charles went to the bank'), and structural ambiguities ('Visiting relatives can be a bore'). So children have to think that language can be ambiguous. But does it really make sense for them to think this? Notice that when people construct representational systems from scratch—artificial languages—they studiously avoid ambiguity. They do this out of a recognition that ambiguity leads to all sorts of problems and, in particular, that it hampers communication. Faced with an apparent ambiguity in learning her language, why shouldn't the child take this to be evidence that she has settled on an incorrect set of principles? And if she does come to think that language contains ambiguities, that just opens up another set of problems. How generous should she be? Each use of a word could be treated as yet another ambiguity. But if a child were to err one way or the other on this question, the results would be disastrous.

Sentence structure raises all sorts of other issues as well. Assuming that the child figures out that she must construct a phrase structure grammar with a transformational component, she still has to determine, among many other things, that the transformations are structure-dependent (as in the example of yes-no question formation). Now one might have thought that this is a simple matter, that the child could appeal to the sort of data that we ourselves cited above to establish structure-dependence. But this response underestimates the true extent of the problem that the child faces. Not only is it questionable whether these data are available to children, but there is an abundance of alternatives to the structure-dependent principle even when these data are included. The auxiliary to be moved might be tied to any number of

[13] Contrary to what is popularly believed, there are no pauses between words in normal speech. One way to appreciate this surprising fact is to listen to speech in an unfamiliar foreign language. Such speech sounds as if it is too fast, and one cannot hear any breaks.

[14] See e.g. Cheney and Seyfarth ([1990]) on vervet monkey calls.

variables—the meaning of the verb, an independent syntactic property, perhaps even a phonological property. Another possibility that the child has to discard—in some ways a very attractive one—is the principle that it's *optional* which auxiliary is moved to the front of the sentence. This principle is compatible with any sentence the child is likely to hear, and languages do seem to contain optional phenomena of this sort.

An added complication to all of these possibilities is whether the principles of language, or some part of language, have to be applied in a specific order and, if so, which order to choose. This issue drastically expands the options that the child confronts, since recalcitrant data could then be explained not just in terms of a mistaken principle, but in terms of a mistaken ordering of principles. And if the child has a large number of principles, the number of possible ordering may quickly exceed the available resources she has for testing hypotheses (as we saw with the possible orderings of English auxiliaries above).

A related issue concerns the types of principles that the child should consider. An important distinction here—one that has played an important role in the recent history of linguistics—is whether the principles should be construction-specific (that is, whether individual principles should explain particular construction types, such as passive constructions or yes-no questions). By and large, linguistic theory in the dominant tradition now makes little use of construction-specific principles. Instead, linguists rely upon a smaller number of powerful general principles that only work in interaction with one another. The fact that it has taken linguistics so many years to arrive at grammars of this kind suggests that they aren't so simple or natural that they'd be the first hypothesis of an unbiased learner. But the problem doesn't stop there. Even if the child opts for principles that aren't construction-specific, she'd have to deal with the various ways in which they might interact. A false prediction could reflect a misguided principle, but it could also reflect a misguided procedure for integrating what are otherwise entirely correct principles. Moreover, a child might easily discard a correct principle because it generates the wrong results in interaction with various other principles the child endorsed.

Another problem is that many of the elements and mechanisms posited by current linguistic theory are themselves unintuitive. One set of these are the so-called empty categories. These are morpho-syntactic elements crucial to sentential structure that have no phonological realization—in a sense, they're words that don't get uttered. For instance, according to a standard representation, the sentence 'What did you buy?' contains a trace that formally marks the occurrence of wh-movement:

(14) What$_i$ did you buy t_i?

Similarly, the sentence 'We persuaded John to finish college' contains an empty category, PRO (which has different properties than a trace):

(15) We persuaded John PRO to finish college.

Though empty categories play an extremely important role in current linguistic theory (see e.g. Chomsky [1981]), they are hardly manifest in ordinary speech.[15] As Chomsky notes ([1988], p. 91):

> [I]t is no simple matter for the scientist inquiring into language to discover that these elements exist and to determine their properties, and this task requires a broad range of evidence not available to the child, including evidence from a variety of languages and evidence acquired by sustained empirical inquiry informed by complex theory construction.

We could go on, but we hope this brief survey gives a sense of the innumerable challenges that confront empiricist learners. We've been focusing on the sheer number of possible principles that might underlie language and the sense in which the correct ones, so far as they are understood in broad terms, aren't pretheoretically simpler or more natural than the many alternatives that suggest themselves. But this isn't to say anything about the nature and quality of the primary linguistic data.

Linguists are apt to point out that the primary linguistic data all by itself casts considerable doubt on empiricist models of language learning. Not only are the data invariably an idiosyncratic finite sampling of an infinite set of sentences, but they're also degenerate. Children are constantly within earshot of ungrammatical utterances due to speech errors, false starts, run on sentences, foreign words and phrases,[16] and so on. Among other things, this means that children have to settle on a grammar that actually *rejects* a good number of the utterances they hear. At the same time, speech doesn't come pre-sorted into the categories of reliable data and noise.

In addition, there are deep problems about accessing and using the data at all. No matter how many grammatical sentences you are exposed to, they can't help in confirming a set of principles if you don't remember them. For an empiricist learner this means keeping track of a vast number of sentences. But that's just to place patently unrealistic demands on what children can do. As Chomsky remarks ([1988], pp. 24–25):

> We cannot assume that [certain ungrammatical sentences] are rejected because the child has not heard them; normal discourse regularly consists of novel utterances, and in fact people generally do not have the slightest idea whether they have heard a particular sentence or not. Surely few if any readers have come across the sentence they are now reading, and someone who had by chance heard or seen it could not possibly remember such a fact'.

[15] Unsurprisingly, the ungrammaticality of many strings turns on features of empty categories interacting with general grammatical principles, as in the following sentence: **Who*$_i$ *do pictures of* t_i *strike you as odd?*

[16] It is worth noting that a very substantial proportion of children are exposed to more than one language from birth. By some estimates, the number of native bilinguals approaches 50% of the world's population (de Houwer [1995]).

Remembering the data isn't the only difficulty. One also has to see the data for what it is. This involves having the means of representing the data in its linguistically salient terms. For instance, to use certain strings in order to test a syntactic principle, the child would already have to have a reasonably accurate representation of the syntactic structure of the sentence. But just as the data and noise don't come presorted, the data themelves don't come pre-marked in their syntactic categories.

All of these considerations taken together strongly suggest that children couldn't acquire language using the limited tools of an empiricist learner. They might, however, lead one to wonder how linguistics as a science is possible, since the very considerations that make language learning so monumentally difficult for children should make the science of language equally difficult. Or turning this around, given that linguistics *is* possible (indeed, thriving) and that it is broadly conducted following empiricist strictures, this shows that children could be empiricist learners after all (see, e.g., Sampson [1980]). The problem with this reasoning is that generative linguistics has developed over more than forty years of theorizing by a large group of adults with advanced degrees, who have yet to completely work out the grammar for even a single natural language. By contrast, language acquisition is accomplished by every normal toddler in just a few short years. This striking asymmetry is simply another version of the Poverty of the Stimulus Argument, one that Ray Jackendoff ([1994]) has labeled *The Paradox of Language Acquisition*.

Moreover, it's important to see that linguists have access to a wide variety of data that couldn't possibly be available to children.[17] Linguists typically know the language they are studying, or have native language informants who do, so they are able to generate an unbounded number of grammatical sentences. Clearly, children can't do that since they haven't learned the language yet. Much the same is true of what is probably the most distinctive and characteristic source of evidence in linguistics: the use of *un*grammatical sentences.[18] Likewise, recent linguistics makes extensive use of cross-linguistic data.[19] But it's not as if children are in consultation with foreign friends and

[17] See Stich ([1981]) for a response to Sampson that runs along these lines.

[18] If you are trying to figure out what set of principles will generate exactly those strings that are sentences in a given language, it helps enormously to have evidence not just about which strings are sentences of the language but also which string aren't (as well as an account of why they aren't). For this reason, linguists spend much of their time worrying about why such strings as the following are ungrammatical: **John is owning a house*, **Susan is expected that will win*, or **Who does it seem to be here*.

[19] Thus, for example, crucial elements of the Minimalist account of basic clause structure depicted in figure 2 derive from cross-linguistic data. The articulation of Infl into AGR_s, T, NEG, and AGR_o derives from comparative study of English and French, while the VP internal subject hypothesis (which accounts for the initial position of the subject) has been argued for on the basis of analysis of Germanic languages, and supported by considerations from (among others) Welsh, Arabic, and French. While these elements of the Minimalist account have widespread implications for interacting principles of grammars, it is not clear that they would ever have been proposed on the basis of the study of a single language.

colleagues, all of whom are trying to learn different languages. The epistemic situation of the working linguist is nothing like the far more limited situation of the child. There are also a variety of further sources of data that linguists and other cognitive scientists are increasingly making use of, and that may turn out to be crucial in solving the puzzle of language. These include data from developmental studies of normal language acquisition, data from developmental studies of abnormal language acquisition, data from the historical changes in a language, data from studies of psychological processing, data from brain imaging studies, data from studies of aphasias, and so on. In short, while linguists may be empiricist learners in the minimal sense that they lack access to the innate principles of universal grammar, they are a terrible model for the study of language acquisition. This makes the child's accomplishment all the more impressive. On the basis of a severely limited and degraded body of evidence, all normal children are able to acquire their language in a few years, while linguists and other cognitive scientists, with all of their education, their computers, and their rich high-quality data are still plugging away after decades of research.

3.4 Two arguments, not one

Before leaving this section, there is one further issue that we need to discuss to clarify the structure and content of the Standard POS Argument. This is that there are really two arguments here, not one. In both cases, the number and character of the potential hypotheses and the deficiency of the data are the core elements to the case against empiricism, yet there is a crucial difference in how they come together.

According to the first reading—which is the standard one (see, e.g., Stich [1978], [1979]; Devitt and Sterelny [1987])—the problem that the Poverty of the Stimulus Argument raises is that there are too many grammars that are compatible with the primary linguistic data and that many of these are more tempting to someone in the child's situation than the correct grammar. As a result, the child should end up with an incorrect grammar, one that is adequate to the primary linguistic data but whose projection beyond these data is inconsistent with the language spoken in the community. Some of the illustrations in Section 2.2 point in this direction, for example, the principles that might be thought to explain yes-no questions.

The second reading of the Poverty of the Stimulus Argument points in a somewhat different direction. The problem here is that, in addition to the huge number of hypotheses that are consistent with the primary linguistic data, there is also a huge number of hypotheses that are just tempting dead-ends. Since these dead-ends vastly increase what is already an enormous

hypothesis space, they make it increasingly likely that the child won't be able to arrive at *any plausible hypothesis at all,* not even one that generates a significant subset of the primary linguistic data. This is the thought that is at work in some of the other considerations canvassed in Section 2.2, for example, the worries about locating the right linguistic units for the assignment of meaning and of locating the right amount of ambiguity in a language.

These rather different strands to the Poverty of the Stimulus Argument ought to be distinguished. Though considerations of both sorts are raised in the literature, to our knowledge philosophers have not recognized the distinction between the two. Clearly, however, they are complementary. They provide relatively independent yet converging reasons for supposing that empiricist learners would not be successful language learners.

This concludes our initial sketch of the Standard POS Argument. We have tried to provide a sense of how compelling this argument is, particularly by elaborating some of the ways in which an unbiased learner would be tempted by simpler, more natural, but incorrect alternative hypotheses. Given this and the limitations on the data that are available to children—in contrast with the rich high-quality data that are available to professional linguists—the result is a powerful case against supposing that children are unbiased learners. An unbiased learner would hit too many dead ends, not arriving at a grammar that is even adequate to the primary linguistic data, or else, if she's *very* lucky, she might arrive at a grammar that correctly generates the primary linguistic data, but makes incorrect projections regarding the rest of the language. The central insight of the Poverty of the Stimulus Argument—and the guiding idea behind nativist models of language—is that children don't have either of these problems. They reliably acquire the language to which they are exposed.

The Poverty of the Stimulus Argument is at the center of the case for nativist models of language, but it doesn't stand alone. In the next section, we briefly discuss several further arguments, before turning to criticisms in the sections that follow.

4 Supporting arguments for nativism

As we noted at the outset, the Poverty of the Stimulus Argument is typically presented along with a variety of other arguments and, in Chomsky's writings as well as those of other nativists, they are often not carefully separated from one another. In the previous section, we did our best to clarify the situation by distilling what we take to be the essence of the Poverty of the Stimulus Argument and making explicit its logical structure. At the same time, our

presentation would not do justice to Chomsky's case if we did not at least point to some of these further sources of support for his overall position. In general, this other support comes from data that are naturally aligned with nativist models of language acquisition. Admittedly, none of this data is decisive in the sense of *proving* that a nativist approach to language is correct, but proof, in this area, is an entirely misplaced standard. Again, the debate between empiricists and nativists is about the innate endowment in virtue of which human beings are able to acquire language. The nature and character of this endowment is patently an empirical question; what matters is how the evidence generally coheres with one's theoretical approach. The answer, we suggest, is that there is a wealth of evidence that coheres with a nativist approach, evidence that just doesn't sit well with empiricist models.

All Normal Children Acquire Language. At the top of the list is the simple fact that all normal children, regardless of their education or style of upbringing, come to acquire the language of their community.[20] Notice that this is not true of a wide variety of tasks that are of comparable intellectual complexity. As Chomsky remarks ([1975], p. 144):

> To a very good first approximation, individuals are indistinguishable (apart from gross deficits and abnormalities) in their ability to acquire grammar [...] Individuals of a given community each acquire a cognitive structure that is rich and comprehensive and essentially the same as the systems acquired by others. Knowledge of physics, on the other hand, is acquired selectively and often painfully, through generations of labor and careful experiment, with the intervention of individual genius and generally through careful instruction. It is not quickly and uniformly attained as a steady state, but is transmitted and modified continually on the basis of controlled inquiry and an explicit record that provides the basis for the next stage of construction.

It is also worth noting in this context that, contrary to what is commonly thought, children are not taught to speak a language. One problem with this idea is that most of the regularities present in language have only been consciously noted (much less fully understood) in recent years, and most of the population doesn't know anything about linguistics. So how could these general principles be *taught*? Many people suppose that children learn language by continual instruction and correction. In fact, this picture of language acquisition has little basis in reality. Children are very rarely instructed or corrected as to the right (i.e., grammatical) way to say

[20] We note in passing that all known natural languages are of comparable complexity, including those spoken in pre-industrial and socio-economically underprivileged areas (*pace* racist and classist claims to the contrary), as well as natural sign languages, such as American Sign Language (ASL).

something in their language.[21] When children are corrected, it's usually because what they've said is false or impolite. People pay very little attention to the ungrammaticalities present in early speech, presumably because it never occurs to them that children won't learn the correct principles soon enough. If anything, poor grammar and pronunciation is more likely to be rewarded as 'cute' than it is to be corrected. People find it amusing when children say things like 'goed' instead of 'went'.

Language Acquisition Exhibits Ordered Developmental Stages. Not only do all children learn language, but they typically pass through identifiable stages. This even applies to children in vastly different circumstances. For example, deaf children who learn natural sign languages undergo the same milestones at the very same time as their hearing peers, including a manual form of babbling before achieving their first words (Petitto [1997]; Petitto and Marentette [1991]).[22] Blind children also learn language at the same pace and following the same pattern as other children (Landau and Gleitman [1985]). These profound differences in learning environment apparently have little effect on the language acquisition process.

Moreover, given a language such as English, certain aspects of its grammar are acquired in essentially the same order. One example is the grammatical morphemes (Brown [1973]; de Villiers and de Villiers [1973]); another is the system of auxiliaries (Stromswold [1990]). Karen Stromswold, whose work on the latter has already come up, notes that a single pattern of acquisition was a pervasive feature among the subjects she studied ([2000], p. 910).

> The order in which these 15 children acquired complex constructions—questions, negative constructions, passives, datives, exceptional case marking constructions, embedded sentences, preposition-stranding constructions, causative constructions, small clause constructions, verb-particle constructions, and relative clause constructions—was also extremely regular.

The probability of these regularities obtaining simply as a matter of chance is practically zero. Nor is it at all obvious why these patterns should be expected given an empiricist framework. In general, different people working on complex problems begin in different ways. As a result, they ought to reach

[21] What's more, children are remarkably resistant to correction of this sort anyhow (for discussion, see Pinker [1999]).

[22] In a striking demonstration of this, Petitto ([1997]) reports on a number of children raised in bilingual environments where one of the languages is spoken and the other signed. She writes, 'The parallels observed in the children's achievement of signed and spoken language milestones cannot be overemphasized [. . .] [all of the children] achieved vocal and manual babbling, first words and first signs, first two words and first two signs, and so forth, either on the same day or within one or two days of each other' (pp. 48–49).

distinct intermediate conclusions, ones that reflect the particular approaches they've chosen to take.

Children's Errors Are Highly Circumscribed And Exhibit Predictable Patterns. A related consideration is that children's grammatical errors are neither haphazard nor are they of the sort one would expect if children were approaching language learning without any language-specific biases. Relative to the huge space of possibilities, children actually make very few errors, and those they do make follow regular patterns. Most children go through a stage when they over-regularize verbs (saying 'goed' rather than 'went', 'holded' rather than 'held'), and many children make pronoun reversal errors (saying 'I' for 'you' and vice versa).[23] But children never say 'He didn't a few things' on analogy with 'He didn't eat', or 'Ate he something?' on analogy with 'Is he happy?' The nativist explanation is that children have a highly circumscribed hypothesis space that's fixed by a rich set of language-specific biases. Clearly, these severely limit the sorts of errors that children will make. On the other hand, empiricists have little they can say here. If children were empiricist learners, then, as they explore the huge space of possible grammars, they ought to make all sorts of errors that they never do.

Critical Period Effects. Outside of language it's a good sign that one has located a domain-specific system when its development unfolds within a fairly rigid biologically determined timeframe. The evidence suggests that language is like this, that its acquisition is limited, in many respects, to a critical period. Perhaps the most famous illustration of this phenomenon is the sad case of Genie, who was the victim of severe abuse and neglect as a child. Until the age of thirteen she grew up isolated and abused and was almost never spoken to. Needless to say, when she was finally rescued she had few cognitive or linguistic abilities and severe emotional problems. In time she was able to recover in significant respects, but interestingly she never acquired anything like normal linguistic abilities. Susan Curtiss, one of the main researchers to study Genie's development, describes her as a 'powerfully effective nonlinguistic communicator' ([1988], p. 98), but notes that Genie's knowledge of English didn't develop past the two-year-old level in the eight years in which she was studied. Of course, Genie's case is complicated by the abuse she suffered as a child and the lasting effects it may have had. For this reason, the case of another woman, Chelsea, is more significant.

[23] Even deaf children learning to speak ASL make this type of error at around the age of two, despite the fact that the signs for 'me' and 'you' are pointing gestures and that they, like their hearing peers, master the physically indistinguishable non-linguistic pointing gesture when they are around nine months old (Petitto [1987]).

Chelsea is severely hearing-impaired and was misdiagnosed as a child; it was thought that she was mentally retarded. Because of the incorrect diagnosis, she was never exposed to a natural sign language and, consequently, was completely deprived of any experience of natural language. When it was finally discovered that she had a hearing impairment—unbelievably, in her thirties—she was fitted with hearing aids, and an attempt was made to teach her language. Unfortunately, she has not been able to acquire much language, despite a relatively normal intelligence. She says things like 'The small a the hat', 'Breakfast eating girl', and 'They are is car in the Tim' (Curtiss [1988], p. 99). Her use of language is often so ungrammatical that it is unintelligible.

Systematic data are understandably difficult to come by in this area. Surprisingly, however, there is a large population of otherwise normal children who aren't exposed to language from birth. These are deaf children who lack early access to a natural sign language. Elissa Newport and her colleagues have studied the linguistic abilities of individuals who are exposed to sign language at different ages. By far, this work has provided the most extensive data demonstrating a critical period for language acquisition. Signers were classified into one of three categories, depending on the time of their initial exposure to American Sign Language (ASL). 'Native signers' had exposure from birth, 'early signers' from the age of six, and 'late signers' sometime after the age of twelve. It turns out that even after thirty years of exposure, there are systematic, consistent differences among these three groups. On a variety of tests on morphology and syntax, native signers do better than early signers, and early signers do better than late signers (Newport [1990]).

Language Acquisition Is Independent of Intelligence. Another important indication that a cognitive ability is subserved by a distinct system is that it can be dissociated from general intelligence, especially in development. A first point to note here is the obvious intellectual limitations that children have while language acquisition proceeds apparently without any effort. We are all extremely impressed if a two-year-old figures out to put the square blocks in the square holes and the round blocks in the round holes. Yet somehow by this age children are managing to cope with the extraordinarily difficult task of learning language. If the empiricist is right, we are to believe that children do both of these things using the very same domain neutral intellectual resources. This is all the more remarkable given that a complete grammar for a single language remains an uncompleted goal for professional linguists—Jackendoff's Paradox of Language Acquisition. The evidence for a dissociation between intelligence and language comes from more exotic cases as well. Children with Williams syndrome have basically intact linguistic functioning, despite severe intellectual impairments (Clahsen and

Almazan [1998]).[24] And going in the other direction, we've already come across Chelsea and Newport's late signers. One also finds subjects with Specific Language Impairment (SLI) who have normal intelligence yet intractable difficulties with certain points of grammar (Pinker [1999]).

Methodology. Empiricists sometimes write as if empiricism is the default position, the one that should be adopted in the study of language, all things being equal. The assumption is that a domain-specific mechanism of acquisition is plainly implausible. This is an odd position to maintain, however, especially in light of the approach that is widely taken with regard to the distinctive abilities in other species. These days there is little hostility to the idea that there are special mechanisms devoted to the acquisition of bee dance, bird song, nest building, web design, or mate selection (to name just a few). Quite the contrary; the best work in comparative psychology is widely recognized to overstep the restrictions that empiricism would impose. It's hard to believe that things should look differently just because the focus switches to Homo sapiens. What's more, outside of the psychological realm, one expects to analyze interesting phenomena in terms of highly differentiated structures. Chomsky often makes this point by appealing to physiology ([1977], p. 81):

> [E]mpiricism insists that the brain is a tabula rasa, empty, unstructured, uniform at least as far as cognitive structure is concerned. I don't see any reason to believe that; I don't see any reason to believe that the little finger is a more complex organ than those parts of the human brain involved in the higher mental faculties; on the contrary, it is not unlikely that these are among the most complex structures in the universe. There is no reason to believe that the higher mental faculties are in some manner dissociated from this complexity of organization.

So empiricism hardly wins points for lining up with the standard methodology of scientific investigation or comparative psychology.

In sum, there are a number of auxiliary arguments that reinforce the Poverty of the Stimulus Argument and provide a more complete picture of the context in which it ought to be evaluated.[25] Altogether, the case for nativist approaches to language is extremely impressive. For this reason, it would be of great interest if philosophers could show that the case is faulty and, in particular, that the Poverty of the Stimulus Argument is misguided. In

[24] A more dramatic case is that of the savant Christopher, studied by Neil Smith and Ianthi-Maria Tsimpli. Christopher was born with brain damage, leaving him with a nonverbal IQ between 60 and 70. In spite of this, he is a fluent English speaker and, amazingly, 'when given a passage written in any of some fifteen or sixteen languages—[he] simply translates it into English at about the speed one would normally read aloud a piece written in English' ([1991], p. 317).

[25] There are further arguments that could be mentioned in this context, but unfortunately space limitations prohibit our going into any more detail.

the next section, we examine the early philosophical resistance to the Chomskyan program.

5 Early philosophical criticisms

Early philosophical discussions of Chomsky's nativism were, even by philosophical standards, overflowing with rhetoric and acrimony. Nelson Goodman wrote one of his commentaries on Chomsky in the form of a dialogue because, in his words, this form offers 'advantages both in organization and in giving an appropriate tone to discussion of a theory that only my respect for its advocates enables me to take at all seriously' ([1967], p. 69). He goes so far as to refer to Chomsky's claims as 'unsubstantiated conjectures that cry for explanation by implausible and untestable hypotheses that hypostatize ideas that are innate in the mind as non-ideas' ([1967], p. 75).

Goodman's comments are taken from a symposium in which Hilary Putnam and Chomsky also participated. These papers, and the ensuing exchange between Goodman, Putnam, and Chomsky, have come to be the classic philosophical source for the contemporary debate over nativism (see Goodman [1967], [1969]; Putnam [1967], [1980a], [1980b]; and Chomsky [1967], [1969], [1980]). Unfortunately, many of Goodman's and Putnam's criticisms are based on misunderstandings of Chomsky's arguments or misunderstandings of the character of linguistic theory. Since these criticisms continue to influence how philosophers view the Chomskian program, it's important to see exactly where they go wrong. We begin with Goodman.

5.1 Goodman

The form of Goodman's discussion makes it difficult to pinpoint his exact worries,[26] yet he seems to have at least five:

(1) Chomsky's conception of 'innate ideas' is incoherent ([1967], p. 74, [1969], pp. 140–41).
(2) The regularities that linguists cite are 'tailored to fit the known natural languages' ([1967], p. 70).
(3) Language acquisition may be aided by prior learning of simpler symbol systems ([1967], pp. 71–72).
(4) The Chomskyan view of language is untestable ([1967], pp. 69–70).

[26] In particular, the dialogue form leaves it unclear whose voice is Goodman's and exactly when a remark is supposed to be taken seriously as an objection to Chomsky.

(5) Chomsky can't give an account of the difference between natural and unnatural languages short of explaining what makes a predicate like 'grue' unsuitable for induction ([1967], pp. 70–71).

Taken collectively, these are supposed to thoroughly undermine Chomsky's approach to language. We'll discuss them in turn.

(1) Goodman's first criticism is that one can't make sense of the notion of an innate idea as it is supposed to figure in Chomsky's account of language acquisition. Goodman presents Chomsky with a trilemma of sorts: If Chomsky's view isn't incoherent, then it must be either 'trivially true' or 'obviously false'. According to Goodman, the standard way of construing ideas implies that they are embodied in words or other external symbols. Yet clearly infants don't have such symbols present at birth; so Chomsky's claim would be obviously false on this reading. Perhaps, then, Chomsky would say that his innate ideas really only consist in a commitment to children having the inborn capacity to learn language. However, no one denies that they have this capacity. If this is all Chomsky's claim comes to, then it would be true but only trivially so. The only option left is one that Goodman finds absolutely incoherent, viz., that 'disembodied ideas' are 'in the mind from the start' ([1969], pp. 140–41).

One problem here is Goodman's assumption that whatever ideas are, if they are innate, they are present at birth. We've already seen that the present-at-birth interpretation of innateness is plainly wrong, so Goodman is off to a bad start. The main difficulty with his objection, however, isn't what it says about innateness but more what it says about ideas. In his insistence that ideas must be 'embodied', Goodman overlooks the possibility that they are expressed not in natural language or some other external symbol system, but in an *internal* symbol system such as a language of thought. This is now a familiar theoretical option in cognitive science—which is not to say it is uncontroversial—the idea being that certain aspects of cognition take place in an internal system of representation that has language-like structure, in the sense that it has a compositional syntax and semantics.[27] In the late 1960s, resistance to mental representations derived from broadly anti-mentalist sentiments, but these have since been answered by both philosophical argument and substantial empirical work.[28] While the Representational Theory of Mind, in all its forms, continues to have its critics, it isn't anything

[27] It is not clear that Chomsky need be committed to syntactic structure for the internal representational system; perhaps a more general commitment to some form of the Representational Theory of Mind would suffice.

[28] A second source of skepticism comes from Jerry Fodor's claim that the language of thought is entirely innate. However, this extra commitment is unwarranted and should not be taken to be part of the view under discussion. For discussion, see Margolis ([1998]), Laurence and Margolis ([MS]).

that Chomsky ought to be particularly worried about advocating. Accordingly, he can respond to Goodman's trilemma by denying the presupposition of its first horn, that ideas must be embodied in external symbols.

(2) Goodman's second criticism is that the regularities in linguistics are 'tailored to fit the known natural languages' ([1967], p. 70). Notice that this is tantamount to saying that linguistic theories are made to accommodate the available data. But how is this supposed to be a failing of linguistics? What would he rather, that linguistic theories *not* accommodate the data? It seems that Goodman's point must be that he thinks these theories are ad hoc attempts to accommodate the limited data from 'known natural languages'. Unfortunately, he does not provide any details to substantiate this claim. Certainly, it's true that linguists are limited in the sorts of data they can collect—for example, they can't make up natural languages at will to see if they conform to the principles of a given theory. But this sort of constraint is pervasive in science. Astronomers can't rearrange the cosmos just for their convenience, and paleontologists have to make do with the fossil deposits that history has given them. As far as we can tell, Goodman's objection here is based on a failure to appreciate that the methodology of linguistic theory is no different from the standard methodology of scientific inquiry.

(3) Goodman's third point against Chomsky is that Chomsky fails to notice an obvious alternative to the nativist model of language acquisition. Goodman writes ([1967], p. 71):

> A: [...] Don't you think, Jason, that before anyone acquires a language, he has had an abundance of practice in developing and using rudimentary prelinguistic symbolic systems in which gestures and sensory and perceptual occurrences of all sorts function as signs?
>
> J: Yes; but language-acquisition is what is at issue.
>
> A: You remember, though, that the real issue is over initial acquisition of languages, since once some language is available, the acquisition of others is relatively easy.

Goodman's argument seems to be this: Chomsky claims that first language acquisition is extremely difficult and that this motivates a nativist position. Second language acquisition, on the other hand, doesn't require the same explanatory apparatus, since people can use their first language to acquire their second. But why can't the acquisition of a first language be more like the acquisition of a second? Given that children have prelinguistic symbol systems, they can use these as the basis for acquiring their first language.

The natural response to this argument—and one that Chomsky ([1969]) makes—is to point out that Goodman's prelinguistic symbol systems don't

share significant properties with natural language, so it's a total mystery how they could explain first language acquisition.[29] In addition, Goodman's assumptions about the character of language acquisition are terribly naïve. In learning a second language, people are often taught some principles explicitly (e.g., word order, agreement, etc.) and they are often corrected for grammatical errors. But while this kind of support may facilitate second language acquisition, it isn't at all applicable to first language acquisition. As we noted earlier, children aren't taught their first language, and certainly they are not taught their first language in some prior gestural or perceptual system. The upshot is that the facts regarding second language acquisition don't in the least support a non-nativist model of first language acquisition.

(4) Goodman's fourth objection is that Chomsky's nativist model is in some sense untestable, the implication being that untestable claims are unscientific and hence disreputable. His suggestion is that the only way to test Chomsky's theory is to construct 'unnatural' languages—ones that lack the properties that are supposed to be universal—and try to see if children learn them. If they fail, then Chomsky would be supported, but otherwise not ([1967], pp. 69–70). He then goes on to remark that we can't do this (presumably for ethical reasons), implying that the situation somehow undermines Chomsky's nativism. We find these remarks puzzling. It's difficult to see why it should cast any doubt on Chomsky's position if, for ethical reasons, certain obvious ways of testing it were precluded. A common feature of scientific practice—one that Goodman himself acknowledges—is to employ any number of indirect means of confirmation. Indeed, it would be a terrible misunderstanding of science in general to think that a given scientific theory has a single experimental method that is privileged for being the only truly reliable guide to the theory's proper evaluation. So there is no reason to think that Chomsky's view is untestable.

What's more, even if one were to grant Goodman the need to expose children to unnatural languages, he is still wrong. Surprisingly, there are 'experiments of nature' where children are systematically exposed to 'unnatural' languages. One type of case involves the phenomena of creolization.[30] This process involves the creation of a new natural language, apparently in one generation, as children are exposed to a pidgin (i.e., a rough-and-ready system of communication that draws from several different languages but has little, if any, fixed grammatical structure) (Bickerton

[29] On the other hand, if the prelinguistic symbol systems did, by chance, share significant properties with natural language, then the need to explain their acquisition would recapitulate the nativist's arguments, just at a different level.

[30] See also the intensive investigation of so-called homesign by Susan Goldin-Meadow and her colleagues (e.g., Goldin-Meadow and Mylander [1990]).

[1981], [1984]). Remarkably, children exposed to the unnatural pidgin language reject it, inventing a new natural language—a creole—for which they effectively have *no model at all*. So Goodman's test has actually been run, and the outcome supports Chomsky.

(5) Goodman's fifth objection turns on his own explication of what an unnatural language may look like. His suggestion isn't a pidgin or a language that explicitly violates a universal proposed by linguists or anything of the sort. Instead, he mentions the language Gruebleen, which switches Goodman's peculiar predicates 'grue' and 'bleen' for 'green' and 'blue'.[31] As philosophers well know, this kind of language presents all sorts of difficulties about the nature of inductive inference. Goodman suggests that Chomsky's view of language acquisition is a nonstarter for not being able to solve these difficulties ([1967], pp. 70–71):

> A: Let us assume that we now have before us an example of a language that cannot be so acquired [an unnatural language]. Still, what in general is the difference between Grubleen-like and English like languages? I see by your gesture that you are painfully aware of the difficulties of answering that question.

Goodman is certainly right that Gruebleen reveals a deep problem in the theory of inductive inference, but it's not Chomsky's problem. Goodman is simply failing to appreciate the logic of the Poverty of the Stimulus Argument here. Perhaps the best way to see this is to note that *none* of the myriad alternative hypotheses discussed in Section 2 was at all grue-like.[32] So the Poverty of the Stimulus Argument is completely independent of Goodman's New Riddle of Induction.

None of Goodman's arguments raises any serious problems for nativism or the Poverty of the Stimulus Argument. We turn now to Putnam's arguments.

5.2 Putnam

Like Goodman, Putnam is highly critical of Chomsky's nativism. Among his central concerns are these:

(1) Chomsky's thesis is '*essentially* and *irreparably* vague' ([1967], p. 108).

[31] An object is 'grue' if it is either green and examined before time *t*, or else blue and examined after time t. 'Bleen' things are either blue and examined before time *t*, or green and examined after *t*. See Goodman ([1954]) for discussion.

[32] A truly grue-like alternative would be something along the lines of the principle that, to form a yes-no question, move the main *auxexical* verb to the front of the sentence (where an auxexical verb is an auxiliary verb encountered before time *t*, and a lexical verb encountered after time *t*). Adding such gruesome hypotheses would of course further complicate the child's task.

(2) Chomsky's argument is merely a "'What Else?" argument' ([1967], p. 115).

(3) There is a simpler explanation of language universals than Chomsky's, viz., that all natural languages have a common origin ([1967], p. 113).

(4) Language acquisition can be explained simply in term of 'memory capacity, intelligence, needs, interests, etc.' ([1967], p. 111).

Finally, Putnam and Goodman both urge that:

(5) the Poverty of the Stimulus Argument generalizes to other domains, providing a *reductio ad absurdum* of the argument (Putnam [1967], p. 115; Goodman [1969], p. 139).

We will consider each of these points in turn.

(1) Putnam does not elaborate on his remark that the nativist's thesis is vague, so it is not clear exactly what his objection is. One possibility is that he is worried about the unclarity of what it means for something to be innate, an issue that came up in Section 1. This is not particularly a problem for nativists, though, given that nativists and empiricists alike invoke innate machinery—the difference between them being only a matter of the richness and domain specificity of the innate machinery. Another sort of worry that Putnam might have in mind here is that Chomsky does not have a completely detailed theory of language acquisition to offer and has repeatedly revised his account over the years. But these points aren't particularly problematic either. *No one* has a fully articulated account of language acquisition to offer and between the two camps nativists clearly have the more detailed theories. In addition, the various theories Chomsky has advocated have been increasingly more detailed and powerful, so these 'changes of mind' are actually indicative of the strength of the nativist position. It looks like Putnam's charge of vagueness comes to nothing.

(2) Putnam's second argument, the 'what-else' argument, is no more than a feeble caricature of the Poverty of the Stimulus Argument. Putnam asks what it means to say that every child learns to speak her native language and, in particular, what is meant by 'grammar' when it's claimed that a child acquires a grammar. 'It means purely and simply the ability to learn what every normal adult learns. Every normal adult learns what every normal adult learns. What this 'argument' reduces to is 'Wow! How complicated a skill every normal adult learns. What else could it be but innate' ([1967], p. 115). As a general theoretical worry, this has very little power. The main point of the Poverty of the Stimulus Argument is to rule out a certain class of models

of language acquisition. Beyond this, nativists employ an argument to the best explanation to some form of nativism. Putnam's caricature does nothing to address these issues. Indeed, Putnam's argument acquires whatever plausibility it has by draining the Poverty of the Stimulus Argument and linguistic theory of all of their detail (see Section 2 above). The same could be done for any argument in science, creating an equally useless caricature. But it is just such details that turn mere what-else arguments into powerful arguments to the best explanation—the bread and butter of good science.

(3) The argument from common origins is more interesting but equally flawed. One problem is that among natural languages, historical closeness isn't a particularly good predictor of shared features.[33] More importantly, though, even if linguistic universals could be explained by common descent, the explanation would be beside the point in the present context. This is because the Poverty of the Stimulus Argument isn't particularly concerned with linguistic universals; it's about the situation that every child faces as she comes to the task of acquiring language. As Chomsky ([1967]) notes, common descent can't help the child at all; linguists may know about the descent of a target language, but children most certainly do not.

(4) Putnam's suggestion regarding an alternative explanation of language acquisition is at least directed at the right target. He argues that it's actually easier and quicker for adults to learn a new language, estimating that a standard foreign language course is a mere 600 hours. He also argues that knowledge of the basic stock of phonemes occurs via rote learning and that this explains why there is there such a short list of phonemes—rote learning is difficult. He adds that the explanation of why language contains nouns is our interest in physical objects and that the reason for having transformational rules and a recursive phrase structure grammar is that such a system is simple and natural given the unbounded expressive power of natural languages.

None of these suggestions is at all damaging for Chomsky. We have already mentioned a number of facts relevant to Putnam's claim about the relative ease of acquisition. Again, adult first language acquisition is

[33] As Steven Pinker remarks, '[T]here appears to be a fairly poor correlation between language typology and historical relatedness of languages, except over very short time spans (a few millennia or less). That is, variable properties of language do not cluster within a single branch or clade of the tree of languages [. . .] For example, languages generally fall into three types in terms of how they package components of verb meaning (e.g. direction of motion, manner of motion, destination of motion, kind of entity undergoing a motion) into different verbs. One pattern is found in Romance, Semetic, Polynesian, and Nez Perce; a second is found in Chinese, English, and Caddo; a third is found in Navajo and American Sign Language. Conversely, cultural and spatiotemporal continuity does not imply linguistic continuity: in a few hundred years English changed typologically from a relatively free-word order, case marked language, to a rigid-order language with an impoverished morphological case system' ([1995], pp. 266–67).

extremely poor (Genie, Chelsea), and adult second language acquisition is reliably worse than child first (or second) language acquisition (Johnson and Newport [1989]). Putnam's other speculations are equally problematic. First, it's not the least bit obvious why rote learning should limit things to about forty phonemes in a language given that children are able to learn a far larger stock of words—something of the order of fifty to sixty thousand. Moreover, as we were at pains to emphasize in our presentation of the Standard POS Argument, phonemes are related in abstract and complex ways to their phonetic realization. So the list of phonemes in a language hardly scratches the surface of what we know about the sound structure of language as native speakers. Turning to morphology, the only fact Putnam considers is the existence of nouns. However, it's deeply puzzling how Putnam's explanation is supposed to work, even for this one fact. An interest in physical objects is perfectly compatible with a language that lacks nouns. Also, while other animals presumably have an interest in physical objects, their communicative systems don't contain nouns or any comparable syntactic categories. Finally, nouns have many properties that aren't the least bit predictable from the fact that they are used to refer to concrete particulars;[34] some of these pertain to morphological principles governing inflections, others to the interface between morphology and other areas of language (e.g., phonology). Turning to syntax, it should be clear by now that, pace Putnam, the actual principles governing the syntax of a natural language are by no means the simplest or most natural. Also, there's an enormous number of possible alternative generative grammars that incorporate a recursive phrase structure component. Putnam says nothing that indicates how children manage to negotiate this space of possibilities. We aren't even sure how Putnam's account is supposed to work. Are children supposed to figure out that languages are infinite and that they will need to develop a set of recursive principles to deal with this constraint? But why should children come to either of these conclusions without some innate direction?

(5) Putnam's arguments aren't looking any more promising than Goodman's. Still, there is one last argument to discuss, one they both employ. This is the idea that all sorts of learning tasks share the same structure as language learning and that this generates a *reductio ad absurdum* for Chomsky. Putnam mentions a game—'jump'—which he says can't be solved by a simple algorithm, yet people do manage to solve it with ease, presumably without relying upon any innate knowledge of games (Putnam [1967], p. 115).

[34] Actually, it is not even true that there is a neat correspondence between nouns and concrete particulars. Nouns can equally refer to places ('location', 'Paris'), events ('earthquake', 'concert'), properties ('redness', 'size'), and just about any other kind of entity (Jackendoff [1994]).

Unfortunately, this example isn't an especially good one, as it's clear that people use heuristics of one sort or another, not an algorithm, and that, in any case, the puzzle isn't particularly analogous to the problem of language acquisition. Goodman's example is better. He considers our acquisition of knowledge of art ([1969], p. 139):

> One might argue that the shapes and colors in paintings are in some sense surface (or obvious) features, while the features that identify a picture as by a certain artist or of a certain school or period are in some sense deep (or obscure). Yet we learn with rather few examples to make some of the latter rather subtle distinctions. Must the mind therefore have been endowed at birth with a 'schematism' of artistic styles?

Examples like this can easily be multiplied since people possess a great deal of sophisticated knowledge about the world, often acquired quickly and on the basis of fragmentary evidence—knowledge about plants, animals, artifacts, other minds, social practices, etc.

We postpone our main discussion of this argument until Section 8.1 below. For now, however, we'd like to note that the knowledge acquired in these various cases, and the circumstances under which it is acquired, are highly variable. Each needs to be treated on its own, and the extent of the analogy to language acquisition has to be evaluated with care. Also, we should point out that this argument is a double-edged sword. In some cases, the right thing to say may very well be that the Chomskyan picture generalizes, that a nativist model is entirely plausible in other domains. This is exactly the conclusion that some cognitive scientists have begun to draw in connection with folk physics and folk psychology (see, e.g., Leslie [1994]).

This ends our discussion of Goodman's and Putnam's classic early philosophical resistance to Chomsky's nativism. Though their criticisms of Chomsky continue to shape philosophical discussions of nativism, we have seen that not one of them stands up to scrutiny. Throughout their arguments, Goodman and Putnam consistently underestimate the complexity of language, fail to appreciate the true logic of the Poverty of the Stimulus Argument, and illicitly employ a distorted picture of scientific methodology when it comes to linguistics. Nonetheless, Goodman and Putnam have managed to fashion an anti-nativist tradition in the philosophy of language, one that has found many supporters over the years. Later critics have developed this tradition in the context of more sophisticated taxonomies of positions on innateness. It is to these matters of taxonomy that we must now turn.

6 A taxonomy of theoretical options

In an important series of papers in the late 1970s, Stephen Stich attempted to clarify the logic of Poverty of the Stimulus Argument and to distinguish a

variety of theoretical approaches that are consistent with the argument. Our Standard POS Argument can be traced to Stich's work (see Stich [1978], [1979], [1981]). One of the important outcomes of his investigation had been a clear recognition that the conclusion of the Poverty of the Stimulus Argument is, in the first instance, a negative thesis to the effect that a certain class of acquisition models is fundamentally inadequate. This thesis is to be distinguished from the positive claim that some specific nativist account is the correct way to proceed. Since Stich had been persuaded to accept the negative thesis, he was led to focus on Chomsky's justification for going beyond this to a model of acquisition based on innate knowledge of universal grammar (UG).

Stich's conclusion is that a range of nativist positions are compatible with the Poverty of the Stimulus Argument and that it would be fruitful to explore this entire space of options. As Stich sees it, Chomsky focuses on UG-based accounts only because he wants to explain the existence of linguistic universals. However, Stich notes that there may be an alternative explanation of these universals (e.g., Putnam's in terms of common descent), and that the universals that linguists find may simply be an artifact of their method (which involves looking for universals).

We don't accept this analysis. Chomsky's motivation for going beyond anti-empiricism to a UG-based account of acquisition isn't a perceived need to explain linguistic universals. Rather, the motivation is grounded in Chomsky's diagnosis of where empiricist accounts of language acquisition go wrong. As Chomsky says ([1965], p. 58):

> It is, for the present, impossible to formulate an assumption about initial, innate structure rich enough to account for the fact that grammatical knowledge is attained on the basis of the evidence available to the learner. Consequently, the empiricist effort to show how the assumptions about a language acquisition device can be *reduced to a conceptual minimum* is quite misplaced. The real problem is that of developing a hypothesis about initial structure that is sufficiently rich to account for acquisition of language, yet not so rich as to be inconsistent with the known diversity of language.

That being the case, if there exist linguistic universals,[35] then it is only sensible to suppose that these are part of the innate endowment that children bring to the task of language acquisition. Given the vast space of potential hypotheses that are available to the language learner, these have to be constrained in some manner. And clearly, universal features of language offer extremely useful constraints, since they go a long way in trimming down the space of

[35] The universals here needn't be strict. They may be conditional or parametric—i.e., it may be universal that if a language has property X, then it has property Y, or that all languages have either property X or property Y.

live options. Naturally, UG-based models are subject to empirical confirmation of the usual sort, but as a first pass at explaining language acquisition, they ought to be very compelling.

Michael Devitt and Kim Sterelny offer an account of the theoretical options that is similar to Stich's. However, whereas Stich has a whole range of different nativist positions, Devitt and Sterelny boil these down to two relatively broad taxonomic positions. The first they call 'the interesting thesis' ([1987], pp. 150–51):

> This is the thesis—strictly, two theses—that humans have an innate, richly structured, language-specific, learning device. And this device determines that the grammar of any language that a human can learn conforms to universal grammar. The thesis is interesting because it conflicts with the initially plausible view that the innate structures that make language learning possible are ones that make all learning possible; our general learning device selects a grammar from the linguistic data.

The second they call 'the very exciting thesis' (p. 151):

> This is the thesis, already mentioned, that the innate language-acquisition device consists in propositional knowledge of universal grammar. The thesis is very exciting because it goes against any empiricism, traditional or contemporary. It requires a commitment to innate ideas that no empiricist could allow.

This distinction nicely captures the core idea that's implicit in Stich's more fine-grained assessment of the options.

Fiona Cowie in her highly regarded book *What's Within* (Cowie [1999]) introduces a sophisticated and principled taxonomy that builds upon Stich's and Devitt and Sterelny's work. In all, Cowie teases apart five positions on language acquisition, characterizing them in terms of the following theses ([1999], p. 176):

(R) Representationalism: Explaining language mastery and acquisition requires the postulation of contentful mental states and processes involving their manipulation.

(B) Biological Boundedness: In virtue of the inborn structure of the human mind, there are constraints on the space of thinkable thoughts.

(DS) Domain Specificity: Learning a language requires that the learner's thoughts about language be constrained by principles specific to the linguistic domain.

(I) Innateness: The constraints on learners' thoughts during language learning are innately encoded.

(U) Universal Grammar: The constraints and principles specified in (DS) as being required for language learning are to be identified with the principles characterized in the Universal Grammar.

At one extreme in Cowie's taxonomy is behaviorism. According to Cowie, the behaviorist denies all five of the theses above. At the other extreme is a position that Cowie calls *Chomskyan Nativism*, which is characterized by its acceptance of all five theses. Cowie then identifies three types of accounts occupying the logical space between these two extremes. First, there is *Putnamian Empiricism*. This position accepts Innateness (I), but rejects both Domain Specificity (DS) and Universal Grammar (U). Second, there is *Enlightened Empiricism*, which accepts Domain Specificity (DS), but rejects both Innateness (I) and Universal Grammar (U). And finally, there is *Weak Nativism*, which accepts both Domain Specificity (DS) and Innateness (I), but rejects Universal Grammar (U).

Behaviorism isn't really a live option for anyone in the present debate, so that leaves four positions. Cowie's last distinction, between Weak Nativism and Chomskyan Nativism, is similar to Devitt and Sterelny's contrast between 'interesting' and 'very exciting' theses, and we agree that it is a useful distinction to draw.[36] We have our doubts about the distinction between Enlightened Empiricism and Putnamian Empiricism, but we will put them aside for the purposes of this paper.[37] As Cowie notes, this four-way taxonomy opens up the possibility that anti-empiricist arguments don't undermine all forms of empiricism or that pro-nativist arguments don't establish the strongest form of nativism.

Having introduced this taxonomy, Cowie goes on to develop what is by far the most sustained and forceful philosophical critique of contemporary nativism in general and of the Poverty of the Stimulus Argument in particular. Cowie's work is in many ways the culmination of the critical tradition that began with Goodman and Putnam. For this reason, her views constitute a useful focal point for evaluating the tradition as a whole. In the

[36] It isn't clear whether Devitt and Sterelny's distinction is precisely the same as Cowie's, since there are a number of orthogonal issues that are potentially involved in their respective distinctions. One issue is whether what's innate is a domain specific capacity for language acquisition, another is whether what's innate involves explicit representation, and a third is whether what's innate is characterized by universal grammar. Devitt and Sterelny seem to be more interested in the issue of explicit representation, while Cowie seems more interested in the other two issues. We agree with Cowie that the things to focus on are domain specificity and the role of universal grammar.

[37] The problem is that it is hard to see why anyone would stop at Putnamian Empiricism, holding back from adopting Enlightened Empiricism. The sole feature distinguishing the two is that a Putnamian Empiricist refuses to make use of any domain-specific information that empiricist learning mechanisms might generate in the course of language acquisition. We doubt that any empiricist (including Putnam or even Hume for that matter) has adopted this restriction.

next two sections we examine Cowie's rich and complex case against of the Poverty of the Stimulus Argument.

7 The a posteriori argument from the Poverty of the Stimulus

As Cowie sees it, there are two distinctive strands to the Poverty of the Stimulus Argument, one with a more a posteriori flavor and one with a more a priori flavor. She makes a point of distinguishing the two, arguing against them on somewhat independent grounds. It will become clear shortly that we think the two strands aren't so distinct after all and that one of Cowie's mistakes is her failure to see this. In the present section, we'll start things off with a brief characterization of the two strands. Then we'll focus on the a posteriori argument, waiting until the next section to begin our discussion of the a priori argument.

7.1 Cowie's two versions of the Poverty of the Stimulus Argument

Cowie's first version of the Poverty of the Stimulus Argument is what she calls *The A Posteriori Argument from the Poverty of the Stimulus* (APS). At one point Cowie characterizes the argument this way ([1999], p. 178):

> The a posteriori argument from the poverty of the stimulus (APS) takes as an example some specific rule of grammar and argues that the data to which the child is exposed during learning—the 'primary linguistic data' (or 'pld')—are insufficient to enable a learner, endowed only with a general purpose learning ability, to infer that rule. The other rules of the grammar being assumed to be the same in this respect, the argument is generalized to the grammar as a whole, and hence to support the conclusion that language-learning is subserved by a special faculty incorporating the linguistic information specified in Universal Grammar.

The example Cowie uses for illustration is the phenomena of yes-no question formation (see Section 2.2 above). As she sets things up, there are two possible hypotheses, one structure-independent (H1) and one structure-dependent (H2). The argument for the nativist is supposed to amount to the claim that the primary linguistic data don't allow children to choose between the two. Cowie's assessment of the APS is that it is a total failure. She says that it underestimates empiricist resources and as a result 'is impotent to establish any form of nativism about the faculties responsible for language learning' ([1999], p. 177).

Cowie's second version of the Poverty of the Stimulus Argument is what she calls *The Argument from the Logical Problem of Language Acquisition* (the *Logical Problem*, for short). According to Cowie this is a 'more a priori

variant of the poverty of the stimulus argument'. As she describes it ([1999], p. 205):

> [...] this argument derives not from any particular conception of the nature of linguistic knowledge or view as to the frequency of specific forms in the *pld*, but rather from the logical structure of the language-learning task. It seeks to show that the primary data are impoverished not just in fact but in principle, and that they are impoverished not merely with respect to the acquisition of some particular grammatical rule but with respect to the acquisition of any grammar powerful enough to generate a natural language.

The argument is inspired by what has been called 'the subset problem' in formal learning theory. The basic problem is that, in general, negative evidence—i.e., explicit information about what strings are not part of a language—is not available to language learners. This means that whenever a child hypothesizes a grammar that projects beyond the correct set of sentences in a learning situation, she would have no way to recover from her mistake.[38]

In a subsequent description of the Logical Problem, Cowie brings it into closer contact with The Standard POS Argument as we have presented it. She writes ([1999], p. 209):

> The empiricist's requirement that the learner prefer simpler, more general, more elegant (etc.) hypotheses provides little in the way of guidance: too little, surely to prevent a learner's falling irretrievably into the myriad possible pitfalls revealed by the Logical Problem. Language is so strange, its workings so abstruse, that a learner utterly uninformed as to its character surely must go wrong. The Logical Problem thus illuminates, in a particularly vivid and intuitively compelling way, the need for a task-specific helping hand in the linguistic domain.

In light of this consideration, Cowie takes the argument to establish (DS). She claims, however, that we must carefully distinguish this limited commitment from views that embrace (I) and (U) as well. That's to say, the Logical Problem may support something like Enlightened Empiricism, but it doesn't come close to supporting either Weak or Chomskyan Nativism. The Logical Problem, like APS, 'is completely unable to support *any* form of nativism about language-learning' ([1999], p. 177). Her final conclusion is that neither version of the Poverty of the Stimulus Argument works. They both 'fail abysmally', neither providing 'the least reason to think that there is a special faculty for language acquisition' ([1999], pp. 310–1).

[38] In the simplest case, the set of sentences in the actual language is a subset of those generated by the child's grammar. Hence the name 'the subset problem'.

7.2 What's wrong with Cowie's analysis of APS

Let's turn now to Cowie's discussion of the APS. One of her main criticisms here is that nativists have done little to substantiate their claim that the primary linguistic data are as impoverished as the APS requires. She notes that nativists have often advanced the APS with little empirical support for their pronouncements about the primary linguistic data. This is important because she thinks that nativists have terribly misjudged its quality. As she sees it, the relevant data for deciding between H1 and H2—the two hypotheses regarding yes-no questions—may not be so rare after all. Sentences such as (1)–(3) would favor the structure-dependent hypothesis over the structure-independent hypothesis.

(1) Is the milk that's in the jug fresh?
(2) Is that mess that's on the floor in there yours?
(3) Was that book that you were reading any good?

She also notes that Sampson ([1989]) finds sentences like these in a popular poem and in a children's encyclopedia, while Pullum ([1996]) finds some in the *Wall Street Journal* and in Oscar Wilde's *The Importance of Being Earnest*.

At the same time, Cowie is willing to back off a little bit from her claim that nativists rely solely on intuitive assessments of the primary linguistic data. She cites an influential study by Stephen Crain and Minehar Nakayama, in which they investigate whether three- to five-year-old children make the sorts of errors that would be expected if they were entertaining structure-independent principles such as H1 (Crain and Nakayama [1987]). Crain and Nakayama had children act out an episode with a puppet, which put them into a position where they had to form novel yes-no questions with more than one auxiliary verb. The results were that none of the thirty children studied made a single error of the sort associated with H1. This is powerful evidence that children aren't entertaining the structure-independent rules.

Still, Crain and Nakayama's results do not bear directly on the issue of whether the relevant data for choosing between these hypotheses is readily available in the primary linguistic data. At most, they show that the data would need to be prevalent enough to be reliably available to children prior to the age of three. As Cowie notes, '[Crain and Nakayama] studied children three years and older: it's surely possible (especially given what Pullum says about the likely frequencies) that a child would have found data falsifying H1 (if she had entertained it) before the age of three years' ([1999], p. 187). Moreover, she speculates that empiricist learners (e.g., Putnamian empiricists) might be equipped with a bias to seek out 'deeper regularities', ones that

lead children to jump ahead to structure-dependent principles even if the data don't conflict with structure-independent ones. In other words, children could be equipped with a very general learning bias, one that has nothing particularly to do with language or any other domain, yet one that has the special effect, in the case of language, of favoring structure-dependent principles. 'Properly conceived, then, a general-purpose hypothesis-generating mechanism such as Putnam's need not rank the structure-independent H1 above the structure-dependent H2, and the question of whether H1 can be falsified in the data is moot' ([1999], p. 189). Notice that if Cowie is right about this, then nativists have the dialectic all wrong. H2 is actually the hypothesis predicted by an *empiricist* model of language acquisition. Similar considerations are also supposed to apply to so-called Enlightened Empiricism. Cowie's suggestion is that, in the course of language learning, an enlightened empiricist may have discovered that structure-dependent rules are to be preferred in the domain of language. She can then recruit this knowledge in further acquisition of her grammar. Echoing Goodman and Putnam, Cowie concludes that 'the APS persistently underestimates the resources available to the empiricist learner and overestimates the difficulty that such a learner would have in acquiring knowledge of syntactic rules' ([1999], p. 204).

Now there are a number of problems with Cowie's case against the APS. The most fundamental of these is that Cowie completely distorts the nature of the Poverty of the Stimulus Argument by focusing solely on *two* alternative hypotheses concerning a single isolated linguistic phenomenon. As we were at pains to emphasize in our own treatment of the argument, there is actually a huge number of alternatives for regularities like this, and the correct ones are by no means the simplest or most natural from a pretheoretic point of view. To focus exclusively on the question of whether there is evidence available that would tell between H1 and H2 is an absurd oversimplification. The very history of the auxiliaries in linguistics shows this. After all, current theory doesn't take either H1 or H2 to be an underlying principle in one's knowledge of English; rather, each principle of grammar is relevant to the grammaticality of a number of different types of construction, and the grammaticality of any given construction depends on a cluster of non-construction-specific principles working together.

Cowie's oversimplification infects the remainder of her discussion as well. For instance, the idea that simply focusing on 'deeper' regularities will solve the problem of language acquisition acquires whatever force it may have from Cowie's illicitly attending to just two hypotheses. If one were really looking for 'deeper regularities', why stop with H2? Why not look for a way to map meaning directly onto phonetic elements? That would certainly be elegant. It may be difficult to do, but so is arriving at the correct linguistic

principles without innate guidance. Notice also that the deeper the principles, the harder the theorizing. It would seem that children would have to be persistent in the extreme in order to arrive at analyses of the depth that working linguists take seriously (see Figure 2 again).

What about the availability of data for choosing between H1 and H2? It's certainly striking that not a single child in the study by Crain and Nakayama seemed to be entertaining the hypothesis H1. Cowie suggests that this may be explained by the fact that children have already encountered the evidence that allows them to choose H2 over H1, perhaps before they have even produced a single yes-no question.[39] But if there are other possible hypotheses besides H1 and H2, why suppose that Cowie's purported evidence rules these out as well? Consider, for example, the very simple principle discussed above, namely, that it is optional which auxiliary to move. This principle is perfectly consistent with Sampson and Pullum's data, yet it would lead children to sometimes say things like **Is that mess that on the floor in there is yours?* The fact is, however, that children don't say things like this—certainly, there is no evidence that they do.[40]

In short, Cowie's conclusion that an empiricist learner would reliably settle on the structure-dependent rule H2 is extremely misleading and is, at best, an artifact of her illicit focus on just two hypotheses. Cowie herself recognizes some of the difficulties we've been raising, once she turns to her second version of the Poverty of the Stimulus Argument. Commenting on the character of linguistic theory, she says ([1999], p. 282):

> [I]t is very hard to read much of modern linguistic theory (especially that developed within the Principles and Parameters tradition) without coming away full of amazement at both the depth and the sheer weirdness of its generalizations about language. Language, if linguists are to be believed, is very strange indeed.

On the basis of this fact, she concludes that children must have a domain-specific helping hand, which guides them in acquiring language. 'The

[39] Another suggestion, owing to Sampson ([1978]), is that the structure-dependent rule follows from general considerations—ones having nothing in particular to do with language—that favor hierarchical structures. As Sampson sees it, *any* process formally resembling Darwinian natural selection is overwhelmingly likely to produce hierarchical structure. This argument is highly suspect. For one thing, it is hardly obvious that wings and teeth are 'hierarchically structured', despite having evolved, in part, through a process of natural selection. Second, the argument can't account for the structure of individual sentences (words, phonological strings, etc.), since they were not specifically selected for. Perhaps the *language faculty* should be hierarchically structured on this argument, but that says nothing about whether or not *sentences* will have hierarchical structure. Third, as Stich ([1979]) notes, Sampson's argument has little effect on the Poverty of the Stimulus Argument, since there are indefinitely many hierarchically structured alternatives to H2 as well.

[40] And Crain & Nakayama ([1987]) looked for just this sort of error. In contrast, children do make other errors at much the same age. Recall that they go through a stage when they overgeneralize the principles for past tense, saying 'goed', 'holded', etc. Moreover, these errors are common, persistent, and resistant to correction.

empiricist's requirement that the learner prefer simpler, more general, more elegant (etc.) hypotheses provides little in the way of guidance: too little, surely to prevent a learner's falling irretrievably into the myriad possible pitfalls revealed by the Logical Problem' ([1999], p. 209).

But, of course, these same considerations carry over to the APS. The essential feature of the Logical Problem that drives Cowie to accept domain-specificity is that there is a huge number of potential principles that could be underlying language and that the correct ones aren't the simplest and most natural. Though she doesn't draw attention to it, this very point is working in the background of her first version of the Poverty of the Stimulus Argument. Again, it's not as though the APS concerns, for any given linguistic phenomena, a competition between a mere two principles—that's the distortion in Cowie's presentation. Thus, by Cowie's own lights, once this distortion is corrected, (DS) follows from either version of the Poverty of the Stimulus Argument.

Still, Cowie doesn't want to be too concessive. Even when she is prepared to endorse (DS) (in the context of the Logical Problem), she isn't prepared to accept that the domain-specific helping hand is innate; instead, it is supposed to be learned. On the face of it, however, this position isn't especially promising. After all, Chomskyan nativists argue that the helping hand that's needed is knowledge of UG, and it is even harder to see how a child with no innate domain-specific guidance could glean knowledge of the principles common to all natural languages on the basis of exposure to only their native language (or languages—see fn 16). Cowie calls this the *Iterated Argument from the Poverty of the Stimulus*, and acknowledges its enormous appeal. 'Whatever intuitive force is possessed by the argument from the poverty of the stimulus formulated [...] as concerning the learning of particular grammatical rules for particular languages, is possessed by it tenfold or a hundredfold when it is formulated as a puzzle about the acquisition of UG' ([1999], p. 239).

7.3 The iterated argument

The Iterated Argument maintains that the domain-specific helping hand is knowledge of UG and that children aren't in any position to have learned UG prior to learning their first language. For UG to do its work, it simply has to be innate. Cowie is happy to admit the appeal of this argument, because she thinks she has a way out. Her strategy is to argue that the helping hand doesn't really have to be UG after all and that the two reasons for thinking it is are flawed. As she sees it, the first of these reasons is that (U) is to be accepted as a kind of stipulation, one that's tied to Chomsky's view of the nature of linguistics; the second is that it is supported by an inference to

the best explanation. For the rest of this section, we'll focus on these two rejoinders. We'll argue that Cowie is wrong on both counts and that, even if she weren't, the spirit of the Iterated Argument is still correct since a revised version of the argument is easily formulated, one that isn't particularly committed to (U).

Let's begin with her claim that (U) is a stipulation. As Cowie sees it, Chomsky's commitment to UG having a role in language acquisition stems from his idea that linguistics is a branch of psychology. On this view, the grammar for a particular language is internally represented in the minds of its speakers and, correspondingly, so is UG, specifying the general character of any potential grammar. Cowie summarizes this interpretation of linguistic theory by remarking that it precludes certain questions ([1999], p. 241):

> [I]t makes no sense to ask whether a particular grammar is a true theory of speakers. And it makes no sense to ask whether a certain element of UG is implicated in language-learning. If it's part of the grammar, it's ipso facto part of the speakers' competence. If it's part of UG, it's ipso facto incorporated into the initial structure of the acquisition device.

Cowie's response is to challenge Chomsky's interpretation of linguistics. The alternative she favors is one owing to Michael Devitt and Kim Sterelny ([1989]). On this view, linguistics is about *languages* (not aspects of speakers' minds), 'where languages are considered in abstraction from the people who speak them, in much the same way as (say) political powers, like the power to veto an act of Congress, can be considered in abstraction from the individuals who exercise them' (Cowie [1999], p. 242). The Devitt and Sterelny view likens linguistic properties to the property of being a pawn (in chess), or the property of being a dollar bill—properties that are dependent on the thoughts and intentions of people living in a particular type of society. In point of fact, that's all there is to Devitt and Sterelny's positive theory, namely, a suggestive but wholly undeveloped analogy. On the negative side, however, Cowie is able to cite several criticisms of the Chomskyan interpretation of linguistics that have been advanced by Devitt and Sterelny, Scott Soames, and others. These criticisms concern purported methodological differences between linguistics and psychology, differences that are supposed to make the two disciplines 'responsible for different domains of fact' ([1999], p. 245). On the one hand, linguistics is supposed to focus on facts about grammaticality of sentences, where linguists try to account for these using the simplest set of principles. On the other hand, psychology is supposed to focus on facts about linguistic behavior, where psychologists try to account for these using theories that capture the actual processes underlying the acquisition and use of language.

The main thing to say about Cowie's argument here is that it's absurd to suppose that the role that Chomsky assigns to UG is stipulative. Chomsky is extremely clear that he views linguistics as an empirical enterprise. Any given theory of UG is subject to empirical confirmation or disconfirmation. The mere fact that it is concerned with a particular subject matter does not make it stipulative. Linguistic theory is in the business of providing a theory of one part of the child's innate endowment, much as parts of molecular biology are in the business of providing a theory of the structure of DNA; linguistic theories are no more true by stipulation than biological theories are.

One can gain a better appreciation of this position by keeping in mind that there are two pressures on UG—first, UG aims to capture universal features of natural language (bearing in mind parametric variation) and, second, UG aims to provide an account of one part of the child's innate endowment. Nothing guarantees that these two projects are bound to stick together. Indeed, if they were to come apart, different theorists could continue to pursue one without the other. For instance, some could go on studying the universal features of natural language and claim these for UG, even though the theory has little bearing on language acquisition, while others could do the opposite. In either case, the outcome isn't a stipulation; it's an expression of theoretical interest and outlook.[41] At best, the true significance of Devitt and Sterelny's alternative interpretation of linguistics isn't that it frees linguistic theory of a stipulative commitment to (U). Rather, it's simply that it calls into question the overall plausibility of Chomsky's interpretation of linguistics, an interpretation that continues to guide many, if not most, working linguists.

But even here, Cowie is wrong. The Devitt and Sterelny interpretation is a poor alternative.[42] First, it compares unfavorably to the Chomskyan interpretation on general theoretical and explanatory grounds. The Chomskyan interpretation, in conjunction with contemporary psychological and linguistic theory, offers a powerful, detailed, highly integrated account of the nature of linguistic properties and their role in central features of language use, including language processing and acquisition. By contrast, Devitt and Sterelny's interpretation offers no more than a gesture towards a possible ontological basis for linguistic properties that is otherwise unmotivated and has no articulated connection to language processing or acquisition. Second, regardless of what one says about the nature of linguistic properties, an account of language processing is still needed, and any remotely plausible

[41] We take it, however, that many linguists would opt for the second option. That is, most linguists hold that UG would be a failure if it didn't accurately capture the innate domain-specific constraints on language acquisition, even if it did provide a perspicuous account of the universal features of language.

[42] See Laurence (forthcoming) for more detailed discussion of these issues.

model is going to be built around representations of the sort articulated by working linguists. The result is that whether we call the knowledge base involved in language processing 'the language', or whether we think of language as some sort of public phenomena à la Devitt and Sterelny, we are going to be committed to the existence of the knowledge base that Chomskyans take linguistic theory to be about. While this fact doesn't mandate the Chomskyan account of UG, it certainly strengthens the overall Chomskyan position since it shows that the alternative, even if it were motivated, would do nothing to challenge the evidence for the constructs that the Chomskyan posits to explain language use. And given standard models of language use, this just means that we are back to requiring a mechanism that is able to yield a correct grammar under the conditions in which real language learners find themselves.

Of course, one may still wonder whether the role that Chomskyans assign to UG is plausible. Notice, though, that the alternative here isn't a new ontology for linguistic properties. Rather, it has to be a non-UG based account of language acquisition. This brings us to Cowie's second argument against (U). To the extent that she is willing to admit that (U) is an empirical claim, she rejects it as a faulty 'inference to the best explanation'.

Now one would think that in order to establish a claim like this that it would be necessary to supply an alternative explanation that approaches the explanatory power of UG-based models of acquisition. Certainly the mere possibility of an empiricist model does nothing to diminish the Chomskyan position. As Chomsky himself has said to Putnam on this very matter (Chomsky [1980], p. 323):

> I agree that 'for all we know' some notion of 'general intelligence' about which we have 'no idea' might explain everything I have ascribed to [the genetically determined initial state of the language learner]. Similarly, there would be little point in debating the claim that 'for all we know' some mysterious force, as to the character of which we have 'no idea,' might explain everything that physicists try to explain in terms of their complex constructions.

It's noteworthy, however, that Cowie doesn't take it upon herself to provide an alternative and that she cheerfully admits that she doesn't have one. She even goes so far as to make the bizarre claim that she is doing Chomsky a favor by not expecting him 'to show that his theory is better than rivals that do not yet exist' ([1999], p. 249). So what is the basis of her criticism? It's that an inference to the best explanation only works when the proposed explanation reaches a certain standard. The claim is that the explanatory status of UG falls short ([1999], p. 272):

> Chomskyans often write and speak as if their critics' lack of an alternative theory were in itself evidence for their view. I disagree. In

> general, I think, the fact that we might lack alternatives to a given theory is reason to retain a theory only when there exist independent reasons to accept it. If there is no independent reason to believe a theory, though, then that theory's being the only game in town is no argument in its favor.[43]

Her suggestion, then, is that there aren't any independent reasons to endorse (U).

This is a bold claim, and one that seems just false on the face of it. After all, the best theories of language acquisition are UG-based and, as Cowie herself notes, such work constitutes 'a research program in its vigorous prime' ([1999], p. 263). What's going on here? As best as we can tell, the entire force of her argument comes down to the observation that specific UG-based accounts of particular aspects of language acquisition are controversial. But this hardly shows that (U) lacks empirical support. Quite simply, *all* scientific theories are controversial and those in developing sciences are all the more so. Controversy by itself has no bearing on whether a theory or (more importantly) *a theoretical approach* has solid empirical credentials. In fact, in many areas of science, one can say with near certainty that the current theories are false in detail, all the while noting that they enjoy considerable empirical support, which in turn provides justification for the general theoretical approach they exemplify. Good scientific theories don't end the discussion with an absolute and final empirical stamp of approval. They offer a different take on things, casting the available data in a productive new light or leading to fruitful new questions and unexplored lines of inquiry. We think there is no question that UG-based approaches succeed in all of these respects.

Cowie's attack, suggesting there is 'no independent support' for (U), is plainly misguided. Interestingly, however, there is a more fundamental problem with Cowie's evaluation of the Iterated Argument. Her focus on UG obscures what's really at stake. Once one accepts that language acquisition requires a domain-specific helping hand, as Cowie does, a version of the Iterated Argument is virtually unavoidable. Whatever domain-specific information or constraints are acquired, they have to be sufficient to guide the language learner though a vast and intricate space of potential principles, many of which are simpler and more natural than the correct ones. So clearly the helping hand has to be rich and intricate and carefully orchestrated in how it supports language acquisition. For all anyone knows, *this means that it will be as demanding to learn as UG*. In other words, the Iterated Argument

[43] Here she is echoing Nelson Goodman: 'Let us now assume that for certain remarkable facts I have no alternative explanation. Of course, that alone does not dictate acceptance of whatever theory may be offered; for that theory might be worse than none. Inability to explain a fact does not condemn me to accept an intrinsically repugnant and incomprehensible theory' (Goodman [1967], pp. 73–74).

isn't really about UG per se; it's about the information or constraints that do the work that Chomsky assigns to UG. Since nothing that Cowie says alters the character of that work, nothing she says diminishes the power of the Iterated Argument. The result is that the very considerations that support (DS) argue for (I). This shouldn't be at all surprising. After all, if (as Cowie grants) domain-general strategies are unable to help a learner navigate through the huge space of potential hypotheses concerning her language, it's hard to see how they can readily give her the sort of helping hand she needs to get through them either.

In sum, Cowie's objections to her first version of the Poverty of the Stimulus Argument come to nothing. The main problem is the way she distorts the APS by framing it in terms of a small number of competing construction-specific hypotheses. Once the argument is corrected for this mistake, it undermines all of the variants of empiricism that Cowie distinguishes, providing excellent reasons for thinking that children are aided by innate domain-specific knowledge or constraints in acquiring language. Moreover, the argument is only strengthened when one examines the 'helping hand' that even Cowie has to admit facilitates language acquisition. For whether or not the helping hand is UG, it's not going to be the sort of thing that children can learn without considerable innate guidance. At this point, the case for nativist models of language acquisition is extremely powerful. Cowie's critique isn't over, however, since she still has objections to her second version of the Poverty of the Stimulus Argument. We turn to these next.

8 The argument from the logical problem of language acquisition

Cowie's second version of the Poverty of the Stimulus Argument is the one she characterizes as having a more a priori flavor. This version of the argument also focuses on the paucity of negative data, that is, explicit information about what strings are not part of the language. The idea is that children have little, if any, access to negative data and that this creates a serious problem for empiricist learners. If they find themselves embracing a grammar that is largely consistent with the primary linguistic data yet one that makes incorrect projections beyond this part of the language, they'll have no way of recovering from their mistake. Recall that it's in this context that Cowie comes to embrace (DS). She remarks on how complex and strange language is and how a general preference for simple hypotheses is a poor guide for navigating through the 'myriad possible pitfalls of the Logical Problem'. At the same time, however, she insists that the Logical Problem provides 'no support whatsoever' for nativism ([1999], p. 207). Her

main line of criticism for this position harks back to Putnam's and Goodman's attempts to provide a *reductio ad absurdum* of the Poverty of the Stimulus Argument. In this section, we'll argue that the *reductio* doesn't work.

8.1 Cowie's curries

Cowie's evaluation of the Logical Problem focuses on an example much in the spirit of Goodman's example involving styles of art, which came up at the end of Section 5 above ([1999], p. 215):

> Given some minimal gastronomic experience, virtually all normal humans are guaranteed to acquire 'culinary competence,' the ability to recognize and distinguish a variety of foods from each other and from non-foods. Yet, when learning about curries, say, no one ever systematically informs us that Irish stews, tacos, and quiches—let alone boats and babies and bison—are not curries. There are infinitely many things that might be curries, but that are not curries, and that we have no information about one way or the other. Yet, and despite the dearth of negative evidence, we all manage to converge on the view that a curry is itself and not another thing. We all, that is to say, manage to more or less converge in our intuitive judgements as to what is and what is not an instance of the kind curry.

The implication is supposed to be that the Logical Problem, if it were sound, would show too much. According to Cowie, the basic problem is that the features of language that give rise to the Logical Problem occur 'for all learning involving projection beyond our experience' ([1999], p. 215). Yet clearly, in most of these cases—including the concept of a curry—we can't have innate domain-specific mechanisms of acquisition. The result is a *reductio ad absurdum* of this version of the Poverty of the Stimulus Argument. For Cowie, this suggests that we need to rethink the claim about negative data. Children must have more information about what's not in their language than nativists allow.

For the moment, let's put aside Cowie's point about negative data. The central question is whether Cowie's curries really do constitute a *reductio* of the Poverty of the Stimulus Argument. Our answer is that they don't. Cowie's argument, like Goodman's, has a certain seductive appeal. But that appeal depends crucially on ignoring all the details of linguistic theory as well as the structure of the Poverty of the Stimulus Argument itself. Indeed, Cowie reduces the Poverty of the Stimulus Argument to the mere claim that 'learning involves projection beyond our experience'. Thus she treats the problem of language learning as if it were entirely on a par with any simple case of inductive reasoning. But this is simply to ignore all the distinctive characteristics of the Poverty of the Stimulus Argument that set it apart from

general concerns about inductive inference. In particular, it is to ignore the central facts emphasized by the Standard POS Argument, namely, that there are innumerable alternative hypotheses and that the correct hypothesis is by no means the simplest or most natural from a pretheoretic point of view. What makes language learning so interesting is that the correct hypotheses for a given language are *not* the simplest or the most natural ones. In contrast, it's precisely the simplest and most natural ones that ought to be selected in standard cases of inductive reasoning.

Of course, it's possible that acquiring the concept of a curry isn't a standard case of inductive reasoning either. At a minimum, however, in order for this case to pose a potential *reductio* for Chomsky, it must at least be true that the character of the learning task is closely analogous to the case of language learning. Here it's important to note the many asymmetries between the concept of a curry and language. We'll mention just a few. First, in contrast with what occurs with language acquisition, it's quite reasonable to maintain that acquisition of the concept of a curry is partly based on explicit instruction. It's certainly not unusual to be introduced to a new food by being told its distinctive properties. Second, the concept of a curry may also be supported by explicit correction. If you go wrong and suppose that a taco is a curry, you may very well be corrected. Third, many people may possess the concept of a curry only through a mechanism of conceptual deference.

We could go on, but we won't since we think it's implausible that the acquisition of the concept of a curry *is* mediated by machinery that is completely domain-general. Cowie presents the options as though one has to make the stark choice between accepting that the concept is entirely innate or embracing a domain-neutral account. But this is clearly a false dilemma. In between the two there is a wide variety of nativist positions, none of which are committed to a 'biologically encoded [...] "culinary faculty"' ([1999], p. 215). This point isn't particularly about the concept of a curry. Rather, it's a general claim about concept acquisition. There is simply no reason to think that concepts have to be either entirely innate or else acquired without any domain-specific support at all.

Consider, for instance, the simple fact that many concepts are acquired in the context of learning one's vocabulary. This suggests that concept acquisition may rely on similar constraints as those that support lexical acquisition. The literature on this topic is enormously interesting and resists a quick summary. However, we'll mention just one example to give a sense of current research. This is the proposal that young children learn the meaning of words (or perhaps just count nouns) by adhering to the constraint that a novel word refers to a whole object, as opposed a part of an object, its color, substance, etc. (see Markman [1989] for discussion). So if a child sees a new

item, say, a white ceramic coffee mug, and hears 'that's a dax', she'll ignore all sorts of potential word meanings. She won't think that the word refers to the mug's handle, whiteness, ceramic stuff, etc. Instead, she'll home in on the whole object that stands before her. (Of course, this explanation presupposes that the child has a prior grasp of what an object is, but there is considerable evidence that such an understanding traces back to infancy—see e.g., Baillargeon [1993]). The investigation of lexical acquisition has uncovered a number of constraints like this, each calling upon its own background of resources.[44] Notice that while these constraints have a wide application, they aren't, by any means, domain-neutral. The whole object constraint wouldn't be of much use in inductive learning tasks in general. Its utility clearly stops short with lexical and conceptual acquisition.

In any event, we are a bit surprised that Cowie is so attracted to an extreme domain-neutral account for the concept of a curry. To be sure, the concept isn't innately specified in all of its detail, but why think that people aren't endowed with a more general (but not wholly general) faculty related to food? From an ethological perspective, it would be surprising if we weren't. A similar intermediate yet still nativist position is quite plausible for all sorts of concepts. Their acquisition would depend, in part, on innate machinery that is not completely domain-neutral, including constraints like the shape bias, general constraints that reflect important conceptual distinctions (e.g., between substances, individuals, properties, etc.), as well as innate commonsense 'theories' (e.g., folk psychology, folk biology, and folk physics).

In summary, Cowie's response to the Logical Problem is deeply flawed. The fundamental problem lies in the way she trivializes the Poverty of the Stimulus Argument by ignoring the ways in which the problems associated with language learning go well beyond any problems associated with inductive inference in general. For this reason, and others, her curry example isn't the *reductio ad absurdum* that she advertises it to be. What's more, we take it that the problems that infect her discussion of the concept of a curry generalize to Putnam's and Goodman's previous stabs at a *reductio* (see Section 5).[45] So at this point, *all* of the major objections to the Poverty of the Stimulus Argument have been met.

[44] Other widely discussed constraints include the taxonomic assumption, the mutual exclusivity assumption, and the shape bias (Markman [1989]; Landau [1994]).

[45] Goodman's provocative example may also involve a hidden equivocation on 'knowledge of art styles', between a more sophisticated understanding of the artistic significance of the style—which is relatively difficult to acquire and typically involves explicit instruction—and a simpler ability to recognize or categorize instances of a particular style of art. Interestingly, pigeons have been trained to discriminate Picassos from Monets with a reasonable degree of accuracy, suggesting that the simpler ability may be exhausted by a low level perceptual process (Watanabe *et al.* [1995]).

8.2 Negative data, negative evidence, and nativism

Before closing this section we should say a word or two about the role of negative data in the nativism dispute. For even if Cowie's curry example doesn't raise any real worries for the Poverty of the Stimulus Argument, it may still be that Cowie is right to question the nativist's assessment of the availability of negative data in language acquisition. As Cowie sees it, while children may not have access to much negative data per se, they must have access to other, subtle forms of information about what strings aren't part of their language—what she calls 'negative evidence'—and that nativists ignore some standard insights about learning in general. These include the potential use of probabilistic information, holistic reasoning, and piecemeal learning strategies. Cowie also claims that nativists make the mistake of wrongly assuming that there are no differences in the end product among language learners (i.e., of the same natural language). All of these charges raise interesting and potentially challenging issues for the nativist. We will argue, however, that ultimately these worries pose no real difficulties.

We begin with Cowie's remarks on the piecemeal nature of learning and the potential differences in the end products of language acquisition. Though Cowie does not really develop these points, we take it that in her own way she is challenging the idealizations that are the stock and trade of linguistics and the study of language acquisition. The problem is that she doesn't say what's wrong with these idealizations apart from the fact that they *are* idealizations. But nativists are well aware of this fact. They use them nonetheless because a certain amount of idealization advances the development of linguistic theory in exactly the same way that it does in other sciences. Whether an idealization is warranted is always an empirical question. In the case of linguistics there's no question that idealizing away from such things as minor differences in idiolects has paid off.

What's more, nativists aren't committed to ignoring the facts that Cowie claims are missed by their idealizations. For instance, we have already seen how nativists are able to appeal to subtle—and sometimes not so subtle—differences in the end products associated with first and second language acquisition by native and non-native speakers. This sort of evidence only provides further confirmation of a broadly nativist approach. In addition, nativists have also concerned themselves with the real-time course of language acquisition. For instance, one important theory of how children acquire the ability to segment a string into words is that very early on they focus on the stressed syllables. The evidence for this theory comes from, among other things, a close inspection of children's speech and how it changes in the course of development (see Gleitman and Wanner [1982]). The early speech of children exposed to English is often described as 'telegraphic'

partly because it omits function words and inflections. As it turns out, these items (in English) are unstressed. Further evidence along these lines includes the fact that first words are often pronounced using just their stressed syllables ('raff' instead of 'giraffe') and that children learning languages with different stress patterns exhibit a different course of acquisition (e.g., Turkish children learn inflections long before their English counterparts, since Turkish inflections occur in stressed syllables). Nativists are deeply interested in these and other facts concerning the developmental course of language acquisition. If they don't always focus on real-time acquisition, that's because it's extremely difficult to formulate *any* theory that is adequate to human linguistic competence given even just the gross facts about its acquisition.[46]

Regarding the use of negative evidence, Cowie accuses nativists of only paying 'lip service' to 'indirect sources of evidence in the linguistic data' ([1999], p. 222). As with the previous complaint, this turns out not to be completely accurate. In fact, the suggestions she herself makes for how children might make use of negative evidence draw heavily from the work of nativists. (One of her main sources is Steven Pinker, of all people!)

Let's work through one of these proposals. She suggests that children might use the non-occurrence of certain sentence types as data. For instance, she speculates that a child who says 'I falled the cup off the table' might come to reject sentences like this not because she is told *don't say 'falled'*, but because she doesn't actually hear 'falled' in her environment and, given a situation in which it would be used, she hears 'I caused the cup to fall off the table' instead. Generalizing from this point, Cowie goes on to suggest that the crucial fact may not be that the sentence per se hasn't occurred (since most sentences don't), but rather the nonoccurrence of sentences of a more general syntactic type. Thus she notes that it may be that neither (1) nor (2) has ever occurred in the child's linguistic environment.

(1) Steve enjoyed the curry
(2) Enjoyed curry Steve the

Even so, (1) may be judged acceptable in virtue of the fact that lots of other sentences of the general form $[_S\ [_{NP}\quad]\ [_{VP}\ [_V\quad]\ [_{NP}\ [_{DET}\quad]\ [_N\quad]]]]$ have occurred, whereas the same can't be said for the form exemplified by (2). As Cowie puts it, 'the relation of syntactic structures to strings is one to many: for any given structure, there will be many strings that are instances of it' ([1999], p. 224).

[46] E.g. the fact that the same child who acquires English growing up in Milwaukee could have acquired Japanese just as effortlessly had she grown up in Tokyo instead.

Actually, this isn't quite right. The relation of (possible) syntactic structures to strings is *many–many*—an important oversight. Just as it's true that for any given structure there are many strings that could have that structure, it's also true that for any given string there are many structures it could have. The result is that, depending on the theory that the child is currently employing, she may or may not take the nonoccurrence of a particular form to be relevant evidence. For instance, returning to an earlier example, a child who doesn't distinguish between lexical and auxiliary verbs might find herself in a position of being perfectly happy saying **Does she could go?*, despite its nonoccurence in her environment, since she hears lots of sentences of 'the same form'—e.g., *Does he seem happy?* (see Figure 1). In fact, the number and variety of ungrammatical forms that fit the model of any string is truly staggering. This is just the basic message of the Poverty of the Stimulus Argument, popping up again.[47] Cowie does show a little awareness of this issue when she notes that her discussion presupposes that the child is able to interpret the evidence properly. She claims that this is an assumption that everyone has to make. True enough, but as we were at pains to point out in our discussion of the Standard POS Argument, this observation itself shows that empiricists and nativists aren't on equal footing. Nativists can appeal to innate constraints that not only help the child frame hypotheses concerning her grammar but *also* help her interpret the data for selecting among her hypotheses. Empiricists have no such resources to rely upon.

Let's look at another one of Cowie's suggestions, the claim that children make use of subtle probabilistic cues as a form of indirect evidence. In particular, she discusses literature that documents correlations between the likelihood of adults repeating or expanding upon a child's utterance and the utterance's grammatical status. For example, in one study parents were more likely to repeat a two-year-old's utterance if it was grammatical (Hirsch-Pasek, Treiman, and Schneidermann [1984]). By contrast, expansions or contractions of children's utterances and requests for clarification are more likely to follow ungrammatical utterances (Demetras, Post, and Snow [1986]; Bohannon and Stanowicz [1988]). Summarizing these data, Cowie says that 'there are plenty of cues in the child's linguistic environment that a savvy learner (especially one who is sensitive to relative frequencies) could exploit in order to discover what *not* to say' ([1999], p. 230). This is important for Cowie, since she believes that the issue of

[47] Cowie's suggestion is also complicated by the demands it places on children's memory for sentences and by the plain fact that many sentence types are perfectly grammatical yet never occur all the same. Some are simply avoided for stylistic reasons, others because they are too difficult to parse (e.g., 'the mouse the cat the dog chased bit ran').

negative evidence is where 'the crux of the learnability problem for languages resides' ([1999], p. 222).

Nativists have long been aware of the potential role of negative evidence, and, not surprisingly, they aren't nearly as optimistic about its significance. Cowie cites two discussions as being representative of the nativist response. One is Morgan and Travis ([1989]), which notes that that negative evidence disappears before errors do and that the likelihood of 'correction' is inversely proportional to frequency of error (see also Morgan, Bonamo, and Travis [1995]). The other is Marcus ([1993]), which emphasizes the varying amounts of feedback that children get in different families and cultures. Cowie will have none of this. Unfortunately, her replies show a real failure to appreciate the logic of the present issue.

Against Morgan and Travis, her main claim is that their pronouncement is too categorical. For all we know, she claims, children may make use of corrections in some cases, even if they don't in others. And though corrective feedback disappears at what looks like a crucial time, it may still function in the child's evaluation of a principle, especially given the holistic nature of confirmation. The problem with these responses is the same. What matters isn't what's 'possible in principle' but rather what's empirically plausible. Is there any reason to think that children *do* regularly rely on the sort of negative evidence that Cowie cites? As far as we can tell, Cowie doesn't address this issue at all.

Her response to Marcus also misses the point. Marcus's claim isn't the same as Morgan and Travis's. If Morgan and Travis are right—and we think they are—there isn't a sufficient body of evidence to substantiate the view that children regularly use negative evidence of the sort that Cowie mentions. What Marcus would add is that even if negative evidence is available and even if it is sometimes used by children, it isn't a *necessary* feature of language acquisition. This is an interesting claim that finds support in a variety of types of evidence. One source of support comes from the study of the varying ways in which children and adults interact. Not all cultures treat their children in the way that middle class Westerners do. In some cultures, parents and other adults rarely address infants and toddlers directly, employing culturally disseminated theories of language acquisition markedly different from those of their Western counterparts.[48] Another source of evidence is creolization.

[48] E.g., Clifton Pye reports that K'iche Mayan parents 'address almost no speech to their babies' and even at the toddler stage 'did not talk very often with their children. They certainly lacked any concept of talking with their children for the sake of their language development and were not conscious of their children's particular stage of linguistic development. Al Tiya:n's mother told my assistant that although Al Tiya:n's speech was not yet clear, she was talking. Other parents told me that their children were not talking and could not understand why I would be interested in tape-recording their speech' ([1992], pp. 243–44). See also Crago *et al.* ([1997]) for similar points concerning the acquisition of Inuktitut and Schiefflin ([1985]) concerning the acquisition of Kaluli.

As the parents don't speak the creole, but only the pidgin on which it is loosely based, they aren't in a position to provide appropriate feedback at all. Yet another sources of evidence comes from a study of Karin Stomswold's concerning a boy with virtually no productive language yet intact comprehension. At the age of four, he could make grammaticality judgements and distinguish reversible active and passive constructions (e.g., 'the dog kicked the cat'/'the cat kicked the dog' and 'the dog was kicked by the cat'/'the cat was kicked by the dog') (Stromswold [2000]). Since he can't speak, he clearly has no access to such feedback as a parent repeating or expanding on what he says.

Cowie's response to this kind of data is, first, to remark that feedback isn't required for language acquisition and, second, to insist that when one type of negative evidence isn't available, another probably is. In the present context, the first of these claims is very odd. It's *exactly* what Marcus says and is even explicit in a passage that Cowie quotes (viz., ' "there is no evidence that noisy feedback is required for language learning" ' (Cowie [1999], p. 231, quoting Marcus [1993], p. 53). So the real point of disagreement is the second claim, that children always have access to some type of negative evidence or other and that it is bound to be rich enough to help them locate the right hypotheses amid the vast range of possible alternatives. The problem is that this is a bald pronouncement. Cowie doesn't actually give any reason to think that it's true.

Finally, we should note that there are difficulties with her suggestions concerning probabilistic and holistic reasoning. Both of these are surely correct to some extent, but there's no reason to think that they support non-nativist models of acquisition. For example, even if children employ probabilistic analyses, they still have an enormous range of properties on which they could focus. Why choose the ones that they do? In the case of word segmentation, we saw that children attend to the stressed elements in a string, and, in fact, this is a very useful strategy. But they could equally focus on any number of other properties, ones that are far less useful or have nothing in particular to do with language. Without a certain amount of innate domain-specific guidance, probabilistic methods are useless. On the other hand, within a nativist framework, they may have a significant role to play.[49] Similar things could be said about holistic learning strategies. In fact, we think they find a far more natural home in the nativist camp. This is because the principles underlying language aren't construction-specific. They are so holistic that it's hard to see how an unbiased learner could even come to formulate, better yet test, a grammar that's plausible by the standards of contemporary linguistics. Not too long ago linguists themselves weren't able

[49] For a particularly clear example, see Pinker ([1999]), which defends an associative model for irregular verbs within the context of a strongly nativist framework.

to do this, as the grammars formulated in the first two decades of generative grammar were themselves construction-specific.

In sum, Cowie's evaluation of the Logical Problem isn't any better than her evaluation of the APS. First, she doesn't have the reductio that she thinks she has. That's because the Poverty of the Stimulus Argument, in any correct formulation, isn't just a rendition of the problems associated with inductive inference. Second, her observations about negative evidence don't argue against nativist approaches to language; on the contrary, they actually seem to provide further support for nativist approaches.

This ends our discussion of Cowie's case against Chomsky. We can't find any reason in her book to abandon the Poverty of the Stimulus Argument. If anything, we think her criticisms show how strong and resilient the argument really is.[50] It bears repeating that Cowie's critique of the argument stands at the end of a long philosophical tradition and that her analysis is so detailed, systematic, and up-to-date, that it can safely be taken to represent the best that empiricist philosophy has to say against Chomsky. We conclude that the Poverty of the Stimulus Argument continues to offer a powerful case for nativist approaches to language and that a wide variety of supporting arguments only strengthen the case.

9 Final thoughts

Philosophical critics from Goodman to Cowie have seen very little merit in nativist arguments in general and in the Poverty of the Stimulus Argument in particular. They have variously charged that nativist claims and arguments are 'incoherent', 'unsubstantiated', 'untestable', 'irreparably vague', and that they fail 'abysmally on both empirical and conceptual grounds'. We have shown, to the contrary, that philosophical arguments against nativism are deeply flawed and that they often turn on a distorted understanding of the Poverty of the Stimulus Argument or a failure to appreciate its massive evidential support. The case for nativist approaches to language, based around the Poverty of the Stimulus Argument, remains extremely powerful.

Inevitably, though, the issue between nativists and empiricists is partly ideological, as is any large-scale theoretical dispute in science. Cowie implicitly recognizes this fact when she complains that the Poverty of the Stimulus Argument has stifled empiricist theorizing in cognitive science. In

[50] We should point out that while Cowie rejects the Poverty of the Stimulus Argument, her book ends with a provisional endorsement of Weak Nativism. Ironically, one of her main reasons for siding with the nativists is the phenomenon of creolization. 'Empiricists [...] face a real challenge in explaining why children should add structure to the language that they hear spoken around them' ([1999], p. 304). So true! The irony is that this is simply another version of the Poverty of the Stimulus Argument: The input (the pidgin) is highly impoverished relative to the output (the creole).

summing up her discussion of language acquisition, she writes ([1999], p. 308):

> [. . .] if I could urge *just one thing* as a 'take home' lesson [. . .] it would be this: To understand a psychological phenomenon as complex and distinctive as language acquisition, we need to look everywhere we can for relevant insights, data, and techniques. We need to approach language with an open mind—a *tabula rasa* in the right sense!—and not with minds writ all over with a priori and ill-substantiated theoretical prejudices. Only when psychology is 'finished'—only when we have on hand a workable theory of how languages are learned—will we be in a position to say with any certainty whether or not nativism in this domain is correct.

Cowie is right to take note of the practical consequences of the widespread adoption of a given theoretical approach. Nonetheless, it would be a serious mistake to suppose that because the issue is empirical one should put aside all assumptions, investigate every possible avenue, and just wait for the empirical results. Scientists wouldn't be able to interpret their data, or even know what sorts of data to look for, without a theoretical orientation. Moreover, it should be obvious that given the sheer number of theoretical possibilities in any area of research, it simply is not possible to pursue them all. General methodological considerations suggest that one should put one's efforts into developing and evaluating those hypotheses that appear to be most fruitful.

Still, we don't want to overemphasize ideological issues. The Poverty of the Stimulus Argument does offer a devastating critique of a broad class of empiricist models of language acquisition. And together with the supporting considerations that we mentioned in Section 4, it constitutes a very impressive case for pursuing nativist models. What's more, as Chomsky has urged, the real issue about language acquisition is trying to formulate a hypothesis that endows children with a rich enough set of innate constraints or principles to account for their ability to acquire language, not a set of principles that happens to conform to empiricists' scruples. Seen in this light, the Poverty of the Stimulus Argument isn't a mere curiosity of linguistics, a symptom of Chomsky's influence. Rather, it is an invaluable guide that has helped linguists ask productive questions and formulate fruitful theories. Indeed, it may well be the best source of insight into the nature of language and the mind that we currently have.

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