Catastrophic change and learning theory

David Lightfoot

Linguistics Department, University of Maryland, 1401 Marie Mount Hall, College Park, MD 20742-7515, USA

Abstract

This paper argues that language change sometimes takes place through an abrupt change in grammars, reflecting a new parameter setting. In that case, one cannot view language acquisition as a function of children matching their input, in the way that most learnability models portray it. The paper outlines a 'cue-based' theory of language acquisition, wherein the definition of a parameter incorporates a structural cue. Language acquisition proceeds as children scan their linguistic environment for these cues and set parameters accordingly. Two case-studies are offered, illustrating the cue-based learning theory and how it may be used to explain historical change.

1. Models of learnability

I take grammars to be mental entities which arise in the mind/brain of individuals when they are exposed as children to some triggering experience. In that case, the central mystery for historical linguists is why they have anything to study: Why do languages have histories? Why do changes take place and why are languages not generally stable? In particular, why do changes sometimes take place abruptly and 'catastrophically'? If people produce utterances corresponding fairly closely to the capacity of their grammars, then children exposed to that production would be expected to converge on the same grammar.

Not only is this what one would expect naively and pretheoretically, but it is also what many learnability models would lead one to expect. For example, Chomsky (1965) viewed children as endowed with a metric evaluating grammars which can generate the primary data to which they are exposed, along with appropriate structural descriptions for those data. The evaluation metric picks the grammar which conforms to the invariant principles of UG and is most successful in generating those data and those structural descriptions. Again, if the data and the associated structural

---

*This paper incorporates material from Lightfoot (1997). Thanks for helpful comments on that earlier paper from Stephen Crain, Michel DeGraff, Norbert Hornstein, Ceil Lucas, Jairo Nunes and Juan Uriagereka.

0024-3841/97/$17.00 Copyright © 1997 Elsevier Science B.V. All rights reserved
PII S0024-3841(93)00030-7
descriptions to which the child is exposed correspond fairly closely to the grammatical capacity of some older individual, one would expect the child's evaluation metric to select the same grammar as that older individual's.

The same point holds for more recent models. Gibson and Wexler (1994) posit a Triggering Learning Algorithm (TLA), under which the child-learner uses grammars to analyze incoming sentences and eventually converges on the correct grammar. If the child-learner cannot analyze a given sentence with the current grammar, then she follows a certain procedure to change one of the current parameter settings and tries to reprocess the sentence using the new set of parameter values. If analysis is now possible, then the new parameter value is adopted. So the TLA is error-driven and permits the child to pinpoint which parameter setting is incorrect when the learner's grammar does not give the right results. There is much to be said about the way that this model works (see Dresher, 1995, for illuminating discussion), but what is crucial here is that the model has the child seeking grammars which permit analysis of incoming data, where the data consist of more or less unanalyzed sentences.

Clark (1992) offers a similar kind of model but one which differs from that of Gibson and Wexler in that the child cannot pinpoint the source of a grammar's failure, revising particular parameter settings. Clark posits a Darwinian competition between grammars needed to parse sets of sentences. The child assesses how well each grammar fares and a 'genetic algorithm' picks that grammar whose elements are activated most often in order to parse what the child hears. The fittest grammars go on to reproduce in the next generation, while the least fit die out. Eventually the candidate grammars are narrowed to the most fit and the child converges on the correct grammar. Clark and Roberts (1993) used this model to give an account of changes affecting the verb-second properties of early French, by allowing an arbitrary degree of misconvergence by children.

What these models have in common is that learners eventually match their input in the sense that they select grammars which generate the sentences of the input, and models of this type can characterize instances of language stability. The child converges on a grammar which analyzes the input successfully, where the input consists of elements of E-language, sets of sentences. In that case the grammar will resemble closely the grammar/grammars which generate that input. Such models can handle cases of mixed input under conditions of population movement. There again the child is presented with a set of data, in this case data yielded by diverse grammars, and converges on a grammar which is most successful in generating that data-set, sometimes a grammar quite different from any of those in the previous generation. This would be a case of grammar change and the new grammar might yield structural descriptions and some sentences which differ from those of the input; but the new grammar would result from the child's effort to match the input sentences as closely as possible.

However, it is hard to see how these input-matching models can succeed when children are exposed to unusual amounts of artificial and degenerate data, which in fact are not matched. In particular, it is hard to see how they could account for the early development of creole languages, as described by Bickerton (1984, 1997, etc.) and others. In these descriptions, early creole speakers are not matching their input;
they go way beyond their input in some ways and in other ways fail to reproduce what they heard from their models, arriving at grammars which generate sentences and structural descriptions quite different from those of the input. Let us call this the ‘abrupt’ view of creolization (following Thomason and Kaufman, 1988). Under this view there is a dramatic discrepancy between what early creole speakers hear in childhood and what their mature grammars eventually characterize as well-formed, much greater than in non-creole contexts. Bickerton deals with plantation creoles, where new languages appear to be ‘formed in the space of a single generation’ (1997). He argues, surely correctly, that situations in which ‘the normal transmission of well-formed language data from one generation to the next is most drastically disrupted’ will tell us something about the innate component and how it determines acquisition.

The abrupt view of creolization is more controversial than it should be. It clearly offends a commitment to the proposition that languages generally change only gradually. This commitment is linked to a highly data-driven view of language acquisition, and it is widely and deeply held, including by creolists. Creolists committed to gradualism (e.g. Carden and Stewart, 1988) insist that creoles emerge gradually as a result of changes introduced primarily by adults, as they relexify their own languages. Clearly gradual change exists and this is part of the story. However, if this is generally true, if this is most of the story, and if creolization for the most part mirrors adult second language learning and is not abrupt and instantiated by children, then there is little reason for theoreticians to be interested in the phenomenon. Our data about the early stages of creole languages generally are not very rich, and if one is interested in adult second language learning, one is probably better off refining theories in the light of better data-sources.

Here I want to argue the following: existing models of learnability may commit one to insisting that language change and creolization in particular are inherently gradual, and gradualists may derive support from them, but a better model of learnability enables one to come to terms with the kind of story that Bickerton has told. Under this model one would expect language change sometimes to be abrupt, sudden and what I have called ‘catastrophic’ (Lightfoot, 1979).

Ironically, the best worked out model of parameter setting comes from phonology and the work of Dresher and Kaye (1990). Parameters have not played an extensive role in the phonological literature, but Dresher and Kaye identified parameters for stress systems, a rather well-studied area of phonology. Furthermore, they developed a ‘cue-based’ theory of learnability, now elaborated, clarified and generalized by Dresher (1995). Under this view UG specifies not only a set of parameters, but also for each parameter a cue. A cue is an element of I-language derived from the input, and the learner scans the linguistic environment for these cues and sets the parameters accordingly. In fact, the child scans the linguistic environment for cues only in simple domains; this is the ‘degree-0 learnability’ of Lightfoot (1991, 1994). Under this view, learners do not try to match the input; rather, they seek certain abstract structures in the input (elements of I-language), looking only at structurally simple domains, and they act on this without regard to the final result. That is, parameter settings are cued by grammatical structures
regardless of what the emerging grammar can generate; the output of the grammar is entirely a by-product of the cued parameter settings and the success of the grammar is in no way based on the set of sentences or structures that it generates, unlike in input-matching models. Dresher (1995) illustrates the cue-based model of learnability with some phonological parameters, and in section 3 I shall suggest that the model gives us a way of understanding two syntactic changes in the history of English. In section 4 I shall turn to the analysis of creoles and signed languages.

First, let us consider catastrophic change.

2. Catastrophic change

Historians have always known that languages sometimes undergo a period of rapid change, then settle into relative stasis (in a kind of 'punctuated equilibrium', to borrow a term from evolutionary biology). In earlier work I have argued that changes sometimes affect languages 'catastrophically'. One can raise or lower the temperature of water and at certain points catastrophes happen, as the water changes into steam or ice. Analogously, languages may change in piecemeal fashion, but at certain points more dramatic changes take place, often simultaneously. It is natural to try to interpret a cascade of changes in terms of a new setting for some grammatical parameter, sometimes having a wide variety of surface effects and perhaps setting off a chain reaction. Such catastrophic changes, resulting from a new parameter setting, have distinctive features and are quite different from the piecemeal, gradual, and chaotic changes which constantly affect the linguistic environment. Lightfoot (1991: ch. 7) identified six distinctive features of parametric change.

First, each new parameter setting is manifested by a cluster of simultaneous surface changes, and this is one element of their catastrophic nature. For example, the loss of the V-to-I operation in English (see 3.2 below) entailed the predominance of forms like Kim always reads the Bible instead of the earlier Kim reads always the Bible, and the obsolescence of inversion and negative sentences like reads Kim the Bible? and Kim reads not the Bible. These apparently unrelated changes took place in parallel.

Second, not only are new parameter settings typically manifested by clusters of changes, but they also often set off chain reactions. A clear example from the history of English is the establishment of verb-complement order at D-structure. Lightfoot (1991) showed that this entailed indirectly the analysis of the infinitival to as a transmitter of properties of its governing verb and the introduction of an operation analyzing speak to, spoken to, etc. as complex verbs. Such chain reactions can be understood through the acquisition process: a child with the new verb-complement setting is forced by the constraints of UG to analyze some expressions differently from the way they were analyzed in earlier generations.

Third, changes involving new parameter settings tend to take place more rapidly than other changes, and they manifest the S-curve of Kroch (1989). For example, grammaticalization and morphological change, involving the loss of gender markers
(Jones, 1988), the reduction in verbal desinences, or the loss of the subjunctive mood, generally take place over long periods, often several hundred years. In the transitional period, individual writers and speech communities show much variation in the forms they employ. This kind of gradual cumulativeness is generally not a hallmark of new structural parameter settings. The old inverted and negative patterns associated with the V-to-I raising operation (reads *Kim the Bible?* and *Kim reads not the Bible*) were quite robust and widely attested in the texts until their demise, which was rapid. The fast spread of new parameter settings is not surprising if one thinks of it in the context of language acquisition. Once the linguistic environment has shifted in such a way as to trigger a new parameter setting in some children, the very fact that some people have a new parameter setting changes the linguistic environment yet further in the direction of setting the parameter in the new fashion. That is, the first people with the new parameter setting produce different linguistic forms, which in turn are part of the linguistic environment for younger people and so contribute to the spread of the new setting (for interesting new work on the spread of new forms, see section 5).

Fourth, obsolescence manifests new parameter settings. When structures become obsolete, it is hard to see how to attribute their obsolescence to the ebb and flow of nongrammatical changes in the linguistic environment. A novel form may be introduced for expressive reasons, to focus attention on some part of the utterance by virtue of the novelty of the form, but a form can hardly drop out of the language directly for expressive reasons or because of the influence of another language. On the contrary, obsolescence must be due to a structural ‘knock-on’ effect, a by-product of something else which was itself triggered by the kind of positive data generally available to children (for a recent application of this methodology, see Warner, 1995: 542).

Fifth, any significant change in meaning is generally a by-product of a new parameter setting, for much the same reason that the obsolescence of a structure must be the indirect consequence of a more abstract change. Lightfoot (1991: ch. 6) discusses changes affecting the thematic roles associated with particular NP positions with verbs like *like, repent, ail*. These could not arise as idiosyncratic innovations that somehow became fashionable within the speech community. It is hard to see how the variation in meaning could be attained by children on a nonsystematic basis, and even harder to see how the variation could have been introduced as a set of independent developments, imitating properties of another language or serving some expressive function through their novelty. Rather, such changes must be attributed to some aspect of a person’s grammar which was triggered by the usual kind of environmental factors – for the English psych-verbs, the existence of only structural Cases.

Sixth, a defining property of new parameter settings, if degree-0 learnability is along the right lines, is that they occur in response to shifts in unembedded data only. They are not sensitive to changes or continuities in embedded domains. Embedded domains will be as likely as unembedded domains to reflect the usual toing and froing of the chaotic linguistic environment, but they have no effect on parameter setting.

Of course, this characterization of abrupt grammatical change makes sense only if one views grammars as individual mental entities, and not as some kind of social
entity codifying the data attested in the texts of some period. Failure to make this simple distinction has entailed confusion in the literature, as discussed by Lightfoot (1995a). There has also been more substantive discussion. Much of it has dealt with the replacement of one grammar by another, i.e. the spread of a change through the speech community. So, Kroch and his colleagues (Kroch, 1989; Kroch and Taylor, 1995; Pintzuk, 1991; Santorini, 1992; etc.) have argued for coexisting grammars, even grammars coexisting in one person in an 'internalized diglossia'. Warner (1993) has argued that some changes relating to the English modals were first instantiated earlier than had been claimed. Others have argued for lexicalist analyses, which provide a means of characterizing textual data more accurately (see Lightfoot, 1991: ch. 6, for discussion). Harris and Campbell (1995: 48) are sceptical about the usefulness of distinguishing gradual vs. abrupt syntactic change. Much of the work on parametric change has been conducted in some kind of Government-Binding model, but this conception of change is not tied to any particular grammatical model, and Warner (1995) discusses a persuasive example of parametric change in terms of a lexicalist, Head-driven Phrase Structure model. There is much to be said about all of these issues, but I shall assume that some changes do indeed reflect parametric change and take place catastrophically. My goal here is to ask how such changes can best be understood and what they suggest about language acquisition.

3. Cue-based learning and language change

This section deals with two well-studied catastrophic changes affecting the history of English, giving a cue-based account of the changes.¹ I will discuss the changes in conventional GB terms, the framework of most of the literature on the two topics. This is most convenient for my purposes here, but the change involving V-to-I discussed in section 3.2 raises questions about the relationship between morphological and syntactic properties and needs to be treated in the context of models which postulate a more substantive connection between morphology and syntax than that of GB models.

3.1. Loss of verb-second

Den Besten (1983) proposed what became the standard analysis for V2 languages (1).

(1) \( cP[Spec \ C_{IP}[Spec _VP[... \ V \ ... ] I]] \)

¹ This section is drawn from Lightfoot (1997), which in turn was a cue-based rendering of similar material in Lightfoot (1995b).
The finite verb moves to I and then to C, and some phrasal category XP moves to Spec of CP. But this standard analysis provides (a) no relation between XP-to-Spec movement and V-to-I-to-C movement, and (b) no explanation for the obligatory character of the V2 phenomenon (positing an attractive feature in C merely restates the problem). Linguists know that there is a relationship between the two movements, that an inflected verb must move to C if a XP moves to Spec of CP, and they know this because of negative data, i.e. ungrammaticality judgments: if some XP moves to Spec of CP and no finite verb moves to C, the resulting structure is ungrammatical. Therefore, the explanation for the relationship cannot be data-driven and it must come from UG. Similarly for the obligatory character of the V2 phenomenon. The question I want to raise here is: What does the V2 child learn and how does she learn it?

Lightfoot (1991) argued that V2 children learn that utterances begin with an arbitrary XP with no particular grammatical or thematic role. If Spec of IP is associated with subjecthood, then the arbitrary XP must be in the Spec of some functional category above I, i.e. Spec of CP. Furthermore, as noted, we need a UG condition that lexical material in Spec of CP needs to be licensed by a lexically filled C; one possibility, due to Uriagereka (1988), is that IP is a barrier to extraction unless it is governed by a lexical C. The only head which can move to C without violating the usual conditions on head movement is a verb, moving through I and therefore picking up finiteness features. This gives rise to the structure 2, and we account for why the finite verb must move to C.

(2) \( \text{Spec}_{CP}[XP] \rightarrow c[Vf] \rightarrow \text{IP} \)

If this analysis is correct, then the parameter which yields a V2 language is one which requires movement to Spec of CP. So what the V2 child learns is that utterances begin with an arbitrary phrasal category. Therefore, the cue is 3, an abstract structure and an element of I-language.

(3) \( \text{Spec}_{CP}[XP] \)

Dutch and German children know that the initial element is in Spec of CP, because it is invariably followed by a finite verb, unlike, say, topics in English, French and other non-V2 languages; the topics in these languages are not followed by a finite verb and therefore are not in Spec of CP (4), presumably reflecting some kind of adjoined structure.

---

2 This entails that in constructions where a wh-phrase is moved to the front of a clause but the finite verb does not move to second position, the wh-phrase occurs not in Spec of CP, but rather in some adjoined position or in the Spec of some other functional category: Brazilian Portuguese (BP) "o que a Maria comeu?" 'what did Maria eat?'; colloquial French "où il est parti?" 'where did he go?'. Jairo Nunes points out that BP also allows a fronted wh-element with an overt complementizer: "o que a Maria comeu?". But the complementizer may not occur if there is no wh-fronting: "*que a Maria comeu?". This suggests that BP fronts wh-items by adjunction and by movement to Spec CP, the latter requiring the C position to be filled.
(4) (a) Peter, I like (him)
(b) Pierre, je l’aime

Although the initial XP is in principle of arbitrary grammatical function, statistical counts for Dutch, German, Norwegian and Swedish show that it is a subject about 70% of the time in conversational speech (see Lightfoot, 1993, for details). Presumably it is those 30% non-subjects which are a crucial trigger for inducing children to postulate that the XP is in Spec of CP and not in Spec of IP (or whatever position is associated with subjecthood). That is to say, the cue (3) must be attested robustly in the primary linguistic data, and robustness means, in this case, that at least 30% of main clauses show this structure. In this case we can quantify the robustness of the cue, but there is no reason to believe that there is anything magical about the 30% figure and no reason to believe that there should be a general, cross-parameter definition of robustness. We shall return to the matter of the robustness of the cue below.

There are many questions glossed over here, but this seems to be a plausible account of how V2 properties are acquired, which addresses the problems of the standard analysis. Now I shall argue that this account provides a good basis for understanding how the V2 phenomenon was lost in the history of English.

Old English–Middle English appears to have optional V2, which became more frequent during the OE–ME period. If the appearance were real, this would be highly problematic, because one could now not invoke UG to explain the obligatoriness of the movement in Dutch and German. Happily, Kroch and Taylor (1995) have provided arguments for the existence of two dialects in Middle English; if they are right, then there is no homogeneous language with optional V2. First, there is a northern, Scandinavian-based V2 grammar (5).

(5) [Spec C [Spec I [V NP]]

Second, there is a southern, indigenous grammar which lacks V+I-to-C and is not V2.3 Early English shows three major alternations (6) (not necessarily three distinct parameters, of course): VP’s may show verb-object or object-verb order (6a), I may precede or follow the VP (6b), and there may or may not be an operation moving an inflected verb to C (6c). This leads us to expect the initial structures of (7) with/without V-to-I raising. This is enough to generate what one finds in the texts. Therefore, at least (8) exists alongside (5).

(6) (a) VO/OV
(b) I-medial/I-final
(c) V+I to C

---

3 I do not adopt the details of Kroch and Taylor’s analysis, for reasons discussed in Lightfoot (1995b), particularly in the Appendix.
If there were multiple grammars along these lines, then the problem concerning the loss of an optional V2 system is reconstrued: There were (at least) two coexisting grammars, one obligatory V2 (5) and one with no V+I-to-C movement, i.e. (8), not V2 in the usual sense. The first of these grammars was lost, ceased to be attained. The problem now is to find why this grammar died out.

Under our cue-based learnability model and assuming the diglossic analysis of ME, we can identify what is likely to have militated against the survival of the V2 grammar. First, children in Lincolnshire and Yorkshire, as they mingled with southerners, would have heard sentences whose initial elements were non-subjects followed by a finite verb less frequently than the required threshold; if we take seriously the statistics from the modern V2 languages and take the threshold to be about 30% of matrix clauses with initial non-subject in Spec of CP, then southern XP-Vf forms, where the Vf is not I-final and where the initial element is not a wh-item or negative, are too consistently subject-initial to trigger a V2 grammar. Ans van Kemenade (personal communication) has provided some statistics from Sawles Warde, an early 13th-century, southwest Midlands text. She counted 152 matrix clauses, excluding coordinate clauses with missing subjects, dislocated structures (i.e. with resumptive pronouns), and initial *hah. She found non-subject-Vf (where the initial element is not a wh-element, of course) in 26 cases, i.e. 17%, well below the required threshold. So the evidence suggests that 17% of initial non-subjects does not suffice to trigger a V2 grammar, but 30% is enough; somewhere between 17% and 30% is a phase-transition. Of course, we have no idea why there should be a transition at exactly this point, and one might turn for help to the literature on complex systems (e.g. Kauffman, 1995).

Second, as those northern children came into contact with southerners, they would have heard forms like (9), because southerners treated pronouns as clitics, according to most current analyses. But, Kroch and Taylor argue that pronouns are not clitics in northern grammars; therefore forms like (9) would not be analyzed with the initial constituent in Spec of CP. Therefore these forms would not have been consistent with the V2 grammars of the earlier generation and would have militated against V2 analyses, making such an analysis harder to attain, more opaque.

(9) XP – subject pronoun – Vf

subject pronoun – XP – Vf
Third, the V-to-I operation was being lost from English grammars, as discussed in the next section. If verbs did not raise to I, then finite verbs could not raise further to C (pace Vikner, 1994, and others; see note 5) and there could be no V2 effect with finite verbs. This would further reduce the degree to which the cue for a V2 system was manifested under the analysis we have offered.

If parameters are set as children scan for cues, and if \( \text{Spec}_{\text{C P}}[\text{non-subject}] \) is the cue for the parameter yielding a V2 system, then we can understand why English lost V2 at the time and in the way that it did, and we can understand why the change seems to have taken place rapidly, indeed catastrophically (van Kemenade, 1987). On the other hand, if learners simply seek grammars which match the input, there is no reason why V2 sentences should have been lost rapidly.

I have shown how it is plausible to assume that the trigger experience that northern children had came to differ in critical ways from the trigger experiences that their parents and ancestors had had as there was more contact with the south. This produced a distributional shift in utterances manifesting the cue for the V2 system. We know from acquisition studies that children are sensitive to statistical shifts. For example, Newport et al. (1977) showed that the ability of English-speaking children to use auxiliaries appropriately results from exposure to non-contracted, stressed forms in initial position in yes–no questions: the greater the exposure to these subject-auxiliary inversion forms, the earlier the use of auxiliaries in medial position. Also Richards (1990) demonstrated a good deal of individual variation in the acquisition of English auxiliaries as a result of exposure to slightly different trigger experiences. The question is when trigger experiences differ critically, when they differ in such a way as to set some parameter differently. That is where work on historical change is so illuminating: sometimes we can identify points at which there have been clear shifts in parameter settings and sometimes we can also identify prior changes in the primary linguistic data (PLD) – now what is relevant is shifts affecting specific elements of I-language, the designated cues. By hypothesis, this shows us changes in PLD having critical effects. In fact, for the immediate future it is work on language change that is likely to be the major source of insight into what triggers particular parameter settings.

### 3.2. V-to-I movement

Operations which associate inflectional features with the appropriate verb appear to be parameterized. Most grammars raise their verbs to the position containing the inflectional elements (10c,d), but English grammars, unusually, have an operation which lowers I on to an adjacent verb ((10a) but not (10b)). We know this because English finite verbs do not occur in some initial C-like position (11a) and cannot be separated from their complements by intervening material (11b).

(10) (a) Jill \( _{\text{VP}}[\text{leave+past}] \)
(b) Jill \( _{\text{VP}}[\text{leave, +past}] \)
(c) Jeanne \( _{\text{VP}}[\text{toujours e, les journaux}] \)
(d) \( _{\text{VP}}[\text{elle e, les journaux}] \)
What is it that forces French children to have the V-to-I operation and what forces English children to lack the operation and to lower their I's?

It seems reasonable to construe the English lowering operation as a morphological phenomenon, not syntactic: in general, lowering operations are unusual in the syntax, and a lowering operation here would leave behind a trace which would not be bound or properly governed. Furthermore, one would expect a morphological operation but not a syntactic operation to be subject to a condition of adjacency. Therefore the representation in (10a), reflecting a morphological operation, contains no trace of the lowered I. In any case, the English lowering needs to be taken as the default setting, as argued in Lightfoot (1993), Lasnik (1995), Roberts (1997); there is, as far as I can see, no non-negative evidence available to the child which would force her to select an I-lowering analysis over a V-raising analysis (10b) for English, if both operations could be syntactic and subject to an adjacency requirement (which would itself raise learnability questions; how could an adjacency requirement be learned?). In that case, let us take the morphological I-lowering analysis as the default setting, always available to children and requiring no particular triggering experience.

Now one can ask what triggers a syntactic V-to-I raising operation in grammars where it may apply. Some generalizations have emerged over the last several years. One is that languages with rich inflection may have V-to-I operations in their grammars, and rich inflection could be part of the trigger. However, we cannot simply link the presence of V-to-I raising with rich inflection in a one-to-one fashion. It may be the case that if a language has rich inflection, it has V-to-I raising (Lightfoot, 1991; Roberts, 1997). If there is no rich inflection, a grammar may have the raising operation (Danish, Swedish,5 and, if Otani and Whitman, 1991, are right, Chinese, Japanese and Korean) or may lack it (English). Indeed, English verb morphology was simplified radically and that simplification was complete by 1400; however, V-to-I movement finally disappeared from English grammars only in the early 17th century, so there was a long period in which English grammars had very little verbal inflection but did have V-to-I movement. In that case, there will need to be a syntactic triggering experience for V-to-I movement. So, for example, a finite verb

---

5 Swedish is sometimes analyzed as lacking the V-to-I operation. So Vikner (1994) has verbs moving directly to C, because negatives precede finite verbs in embedded clauses: ... *om Jan inte köpte boken 'if John didn't buy the book'. But this indicates that *inte 'not' and other such adverbs occur to the left of I and does not provide evidence against the application of V-to-I. Presence of verbs in C is strong evidence of movement through I, given almost any version of the proper government condition on traces. Also, Swedish allows VP fronting and a dummy finite verb, analogous to English do, occupies the position of I (i).

(i) [läser boken] kanske Allan inte gör
reads the book maybe Allan not does
This provides direct evidence that the negative marker is left-adjacent to I even in matrix clauses.

In addition, Roberts (1997) invokes V-to-I movement for the Kronoby dialect of Swedish, citing work by Platzack and Holmberg (1989).
occurring in C, i.e. to the left of the subject NP (as in a V2 language or in interro-
gatives), could only get there by raising first to I, and therefore inversion forms like
(10d) in French could be syntactic triggers for V-to-I.

In that case, under the cue-based theory of learning, one would say that the cue for
the V-to-I parameter would be instances of finite verbs in I, i.e. \[V\], again an ele-
ment of I-language. One unambiguous instance of \([V]\) might be an I containing the
trace of a verb which has moved on to C, as in the structure of (10d). Indeed, I would
guess that these would be very important expressions of the cue, and I doubt that
structures like (10~) would be robust enough to trigger V-to-I in isolation, as we
shall see in a moment. Adopting terminology from Clark (1992), one can ask how
robustly the cue is ‘expressed’; it is *expressed* robustly if there are many simple
utterances which can be analyzed by the child only as \([V]\). So, for example, the sen-
tences of (10c,d) can only be analyzed by the French child if the V *lit* raises to I;6 a
simple sentence like *Jeanne lit les journaux* ‘Jeanne reads the newspapers’, on the
other hand, could be analyzed with *lit* raised to I or with the I lowered down into the
VP in the English fashion and therefore it does not express the cue for the V-to-I
parameter.

By quantifying the degree to which a cue for a parameter is expressed, we can
understand why English grammars lost the V-to-I operation and why they lost it after
the modal auxiliaries were reanalyzed as non-verbs, as the periphrastic *do* became
increasingly common, and as the V2 system was lost. We can reconstruct a plausible
history for the loss of V-to-I in English. What we are doing here is identifying when
a parameter came to be re-set and how the available triggering experiences, specifi-
cally those expressing the cue, seem to have shifted in critical ways prior to that
parameter resetting.7

The cue for the V-to-I operation, \([V]\), came to be expressed less and less in the
PLD in the light of three developments. First, there is good reason to believe that the
modal auxiliaries, while once instances of verbs which could raise to I, were re-cat-
egorized in such a way that they came to be base-generated as instances of I; they
were no longer verbs and therefore sentences with a modal auxiliary ceased to
include \([V]\), and therefore ceased to express the cue for the V-to-I operation. This
change has been discussed extensively in Lightfoot (1979, 1991), Kroch (1989),
Roberts (1993), Warner (1983) etc., and there is general agreement that it was com-
plete by the early 16th century.

Second, as the periphrastic *do* came to be used in negatives like *John did not leave*
and interrogatives like *did John leave?*, so there were still fewer instances of \([V]\) (I
will not address the question of why periphrastic *do* was introduced; for an interest-
ing account in terms of a need for verbs to stay adjacent to their complements, see
Arnold, 1995). Periphrastic *do* began to occur in significant numbers at the begin-
ing of the 15th century and steadily increased in frequency until it stabilized into its

6 I ignore here the very plausible suggestion of Iatridou (1990) that infinitival counterparts to (10c)
may not be direct evidence for movement of V across an intervening adverb, if French allows complex
verbs of the form [V Adv].

7 Warner (1995) also adopts this methodology explicitly.
modern usage by the mid-17th century. Ellegard 1953 shows that the sharpest increase came in the period 1475–1550.

Third, in grammars with the V2 system all matrix clauses had a finite verb in C. Therefore all matrix clauses expressed the cue for V-to-I, $[V]$, (on the assumption that V could move to C only by moving first to I). As these grammars were lost and as finite verbs ceased to occur regularly in C, so the expression of the cue for V-to-I raising was reduced.

The historical facts, then, suggest that lack of rich subject-verb agreement cannot be a sufficient condition for absence of V-to-I, but it may be a necessary condition. Under this view the possibility of V-to-I not being triggered first arose in the history of English with the loss of rich verbal inflection; similarly in Danish and Swedish. That possibility never arose in Dutch, French, German, etc., where verbal inflections remained relatively rich. Despite this possibility, V-to-I continued to be triggered and it occurred in grammars well after verbal inflection had been reduced to its present-day level. However, with the re-analysis of the modal auxiliaries, the increasing frequency of periphrastic do and the loss of the V2 system, the expression of $[V]$ became less and less robust in the PLD. That is, there was no longer anything very robust in the PLD which had to be analyzed as $[V]$, i.e. which required V-to-I, given that the morphological I-lowering operation was always available. In particular, sentences like (11b) with post-verbal adverbs and quantifiers must be analyzed with the V in I but these cues were not robust enough to set the parameter, and they simply disappeared quietly, a by-product of the loss of V-to-I.8

This suggests that the expression of the cue dropped below some threshhold, leading to the elimination of V-to-I movement. The next task is to try to quantify this generally, but meanwhile we should recognize that it is not the gradual reduction in the expression of $[V]$ which will play a role in that quantification, but rather identifying the point at which the phase-transition took place, when the last straw was piled on to the camel’s back.

So children scan the environment for instances of $[V]$. This presupposes prior analysis, of course. Very young children may have difficulty in recognizing elements as such which are in fact instances of $[V]$ in the adult language. This is presumably why young children produce the optional infinitives noted by Wexler (1994): children acquiring grammars with V-to-I movement produce uninflected verbs in base-generated positions despite never hearing such forms from adults – another mystery.

---

8 Some readers balk at the notion that sentences like (11b) were too subtle and not robust enough to trigger a V-to-I operation. However, the fact of the matter is that these forms did not trigger V-to-I or anything else, because they dropped out of the language – compelling evidence, it seems to me. They also dropped out of the language at the same time as other putative reflexes of the V-to-I operation. This shows not only that they had no triggering effect, but also that they were incompatible with the grammatical operations that were being triggered. Hence their disappearance. I know of no alternative account of this particular change.

Furthermore, it also seems reasonable to take the periphrastic do forms as robust enough to act as a trigger for grammatical development. They appear in interrogative and negative statements and imperatives. There are several statistical studies showing that most of the speech directed at young children consists of interrogatives and imperatives (see Newport et al., 1977).
for a pure input-matching learning model. At this stage they know that finite verbs cannot be checked in clause-final position in matrix clauses, but they do not consistently know that finite verbs are instances of I.

The difficulty in recognizing \( [V] \) also explains the tendency of young Dutch children (noted by Evers and van Kampen, 1995) to produce do support forms (12).

(12) (a) ik doe ook praten
    'I do also talk' (adult: ik \([praat] \) ook \( e_i \))
(b) dat doe ik spelen
    that do I play
    'I am pretending that'

These forms do not occur in adult speech; the adult language uses do support only in VP topicalization (13).

(13) de roos treffen doet hij zelden
    hit the mark does he seldom
    'hit the mark he seldom does'

The cue-based account we have offered here for the loss of V2 and the loss of V-to-I works quite differently from the accounts offered by Clark and Roberts (1993) for the loss of V2 and the account offered by Roberts (1997) for the loss of V-to-I movement. Using the learnability model of Clark (1992), Clark and Roberts (1993) account for the loss of V2 in Middle French by having children track data which changes somewhat. Given the introduction of XP subject V \( ... \) forms, earlier grammars were no longer as successful in their fit with the input data and were therefore replaced by a new grammar which failed to generate a V2 language. The supposition here is that new forms were introduced for unspecified reasons and that therefore the child tracking input data along the lines of Clark's genetic algorithm, is forced to a new grammar which fits the new data better. This amounts to saying that V2 is lost because non-V2 forms were introduced into the language. The nature of this explanation is intimately linked to the input-matching nature of the learning model.

Roberts (1997) considers the loss of V-to-I in English and invokes an elegance condition from Clark and Roberts (1993: 315–316), "which, all other things being equal [my emphasis – DWL], favors those parameter-settings which generate relatively simple representations over those which generate relatively more complex ones". He takes simplicity to be in part a function of movement operations and therefore movement operations are marked. This element of UG, for Roberts, is a causal factor in the loss of V-to-I movement in English, biasing learners intrinsically against movement operations. That bias explains why V-to-I is not acquired when all other things are equal. This raises two obvious questions: if UG has a bias against V-to-I operations, why and how did V-to-I operations ever develop in grammars? Second, when exactly are 'all other things equal'? Presumably the answer to the latter question is that the UG bias is effective when the PLD do not demand a movement operation. But if the PLD do not demand a movement opera-
tion, then that is a sufficient explanation for the lack of movement and the UG bias is unnecessary.9

4. Creolization

Roberts (1997) goes on to argue that generally creoles have unmarked values of parameters, specifically that they lack V-to-I movement because the movement represents a marked value (a strong feature), and this despite movement in the lexifier language.10 It is to be expected, of course, that English-based creoles will lack V-to-I, since English lacks it. Also, it is not surprising that French-based creoles may lack it, because the most robust evidence for V-to-I in French, namely V-in-C, is limited to contexts where the subject NP is a pronoun: *lisent les ouvriers ces journaux? If V-to-I is less common in creoles than one expects, that might be a function of problems in recognizing the cue, [V], in the kind of restricted input that early speakers have.

Consider Berbice Dutch, a better worked out example which makes a similar point. A striking property of this Guyanese creole is that it has SVO order while its lexifier languages, Dutch and Eastern Ijo, are both underlyingly OV and V2, as discussed in Lightfoot (1991: ch. 7). Roberts (1997: 32–33) takes this to illustrate the fact that SVO order represents an unmarked option and that creoles generally adopt unmarked parameter settings. A cue-based, degree-0 approach to learnability would tackle things differently.

Dutch and Ijo have object-verb order at D-structure but verb-object order often occurs in matrix clauses because of a verb-movement operation that moves the verb to I and then to an initial C position, yielding V2. X-bar theory demands that verbs be generated adjacent to their complements, either right-adjacent or left-adjacent. A degree-0 learner sets the verb-order parameter on the basis of unembedded data which reveal the position of the verb, as argued in Lightfoot (1991). In Dutch these data are the position of separable clause-union structures (14d), each verb to the right of its complement.

(14) (a) Jan belt de hoogleraar op
    John calls the professor up

---

9 Norbert Hornstein points out that an input-matching model of language acquisition might incorporate an analogue of our notion of a threshold for the expression of a cue by requiring the child to match the input to, say, 70%. However, if the partial match is not keyed to specific parameters, this would predict random oscillation in grammars. By contrast, our thresholds are keyed to cues for specific parameters.

10 Bickerton (1997: n. 13) notes that he 'briefly adopted' the position that creoles have unmarked values for parameter settings, but 'rapidly rejected' this viewpoint. Roberts claims that the objection of Lightfoot (1991: 175–177) to this position disappears, but he does not address the point made there: Saramaccan allows wh-movement out of embedded clauses and that possibility presumably represents a marked parameter setting.
(b) Jan bezoekt de hoogleraar niet
   John visits the professor not
(c) Jan belt de hoogleraar niet/soms/morgen op
   John calls the professor up not/sometimes/tomorrow
(d) Jan moet de hoogleraar opbellen
   John must call up the professor

Furthermore there are uninflected, infinitival constructions in colloquial Dutch, which manifest object-verb order directly in unembedded contexts:

(15) (a) en ik maar fietsen repareren
    'I ended up repairing bicycles'
(b) hand uitsteken
    hand outstretches 'signal'
(c) Jantje koekje hebben?
    'Johnnie has a cookie?'
(d) ik de vuilnisbak buiten zetten? Nooit
    'me put the garbage out? Never'

We can construe this in terms of cue-based learning: the cue for object-verb order is \( v_2[\text{NP} V] \), where the \( V \) may be a trace. In each language children set the verb-order parameter on the basis of evidence in unembedded domains; the evidence may be indirect and show that the \textit{trace} of the moved verb is to the right of the direct object. The verb will necessarily be a trace for a degree-0 learner in a consistently V2 language, because in unembedded domains verbs are consistently moved to C and occur in C at S-structure. The verb is often a trace in a language like Dutch, where verbs generally but not always move to C. So (14a) contains the structure \( \textit{Jan belt}_{1} v_2[\text{de hoogleraar op e_{1}}] \) and the child knows that this is the structure by virtue of knowing that \textit{opbellen} is a phrasal verb and that \textit{belt} must therefore have moved from a position to the right of \textit{op}. Similarly the negative \textit{niet} occurs to the right of the direct object and marks the position from which the verb has moved (14b). In this way the child finds instances of the cue for object-verb order in unembedded domains in Dutch. If the evidence for the position of the verbal trace is obscured in some way, the PLD would fail to some extent to express the cue for OV order. Lightfoot (1991: 179) noted:

"In the case of Berbice Dutch, if the first speakers did not have robust evidence about the distribution of separable particles, or if negative elements were no longer retained in their D-structure position (marking the D-structure position of the verb), or if the verb-raising (clause union) operation was not triggered in the same way as in Dutch, then there would arise a situation comparable to that of late Old English: there would no longer be adequate data to trigger the object-verb setting. Negation, for example, works differently in Ijo and Dutch. In Dutch the negative element occurs to the right of an object NP, marking the position from which the verb moves, but in Ijo the negative particle 'is adjoined directly to the verb in its proposition-negating role' (Smith et al., 1987) and moves with it, as in Old English:"
Ijo provided the negative for the creole, *kane*, and because Ijo provided the basis for negation patterns, one of the Dutch indicators of the position of the verbal trace was obscured.

We lack good records for the early stages of Berbice Dutch, and therefore it is hard to be more precise and to show exactly how the PLD failed to express sufficiently the cue for object-verb order. However, the negation example is suggestive and shows that one indicator of underlying object-verb order may be nullified if the other language is dominant in the relevant aspect. Conversely, Dutch may have been dominant in an area of grammar that expressed the cue for the position of the verbal trace in Ijo. Of course, if children are not degree-0 learners, then the cue for object-verb order would be expressed robustly, because this is the standard order for embedded clauses in both Dutch and Ijo. In fact, early learners of Berbice Dutch were unaffected by this evidence, as expected if they were degree-0 learners, searching for the cue only in unembedded domains.

So it is not difficult to see how a cue-based learner might acquire a verb-object grammar when the lexifier grammars are object-verb and V2, and we do not need to say that creoles always manifest unmarked parameter settings. This leaves creole learners free to adopt marked settings when they interpret their input as expressing the relevant cues.

Under this view, creole children, just like all other children, scan their environment for cues and set parameters accordingly. So new languages may emerge rapidly and fully formed despite very impoverished input, and this view receives striking support from work on signed languages. The crucial fact here is that only about 10% of deaf children in the US are born to deaf parents who can provide early exposure to a conventional sign language. This means that the vast majority of deaf children are exposed initially to fragmentary signed systems which have not been internalized well by their primary models. This is often some form of Manually Coded English (MCE), which maps English into a visual/gestural modality. Goldin-Meadow and Mylander (1990) take these to be artificial systems and offer a useful review of work on how deaf children go beyond their models in such circumstances and 'naturalize' the system, altering the code and inventing new forms which are more consistent with what one finds in natural languages. Goldin-Meadow and Mylander show that children exposed to models who use morphological markers irregularly and spasmodically, nonetheless regularize the markers, using them consistently and "in a system of contrasts ... akin to the system that characterizes the productive lexicon in ASL." (1990: 341).

Newport (1997) extends these ideas by reporting work on a single child, Simon, showing how he comes to use morphology consistently and 'deterministically', where his models used it inconsistently and 'probabilistically'. She notes that Simon does not create "an entirely new language from his own innate specifications", as the language bioprogram hypothesis of Bickerton (1984) would suggest. "Rather, he appears to be following the predominant tendencies of his input, but sharpens them,
extends them, and forces them to be internally consistent” (Newport, 1997). Inconsistent input, then, presents no problem for young children, who simply generalize across-the-board; she reports that adult learners, on the other hand, are seriously impeded by inconsistent input and they often perform even more inconsistently than their models.

Work by Supalla (1990) on MCE casts more light on this and on themes that I have been addressing in this paper. MCE systems were invented by educators to teach English to deaf children. They rely on a lexicon borrowed heavily from ASL. However, while ASL morphology is generally ‘non-linear’, with simultaneous spatial devices serving as morphological markers, MCE morphology generally is ‘linear’ and uses invented signs which reproduce the morphological structure of English: those signs precede or follow the root word. The English take—took alternation is an example of non-linear morphology and walk—walked is an instance of a linear alternation. Supalla studied Signed Exact English (SEE2), the dominant version of MCE, where all bound morphemes are invented and based on English. For example, the SEE2 suffix -ING involves the single handshape ‘I’; the suffix -S (for singular present tense or plural) is a static upright S handshape in the neutral signing space; the -MENT, -TION, -NESS, and -AGE suffixes are all syllabic, /M/, /S/, /N/ and /G/ respectively. Of the 49 English affixes that have an equivalent in SEE2, 44 consist of at least one syllable. They are strictly linear and, importantly, phonologically independent of the root.

Supalla cites several studies showing that SEE2 morphology fails to be attained well by children, who fail to use many of the markers that they are exposed to and use other markers quite inconsistently and differently from their models. He focusses particularly on deaf children who are exposed only to SEE2 with no access to ASL, and he found that they restructure SEE2 morphology into a new system. The SEE2 “bound morphemes were rejected and replaced with devised forms. Moreover, in the devised forms, the affixation type was predominantly non-linear in nature ... not exactly like that of ASL, [but] formationally within the constraints of affixation in ASL” (1990: 46). Unlike in Newport’s study, children did not simply generalize markers which were used inconsistently in the input, and there were particular problems with inflectional morphemes.

Supalla’s approach to this was to postulate a Modality-Constraints Model, which limits signed languages to non-linear morphology, while spoken languages tend to have linear morphology. However, this approach seems suspect. First, the correlation does not hold reliably: spoken languages often have non-linear morphology (e.g. the take/took alternation of English above), and non-linear morphology is comprehensive in Semitic and other languages; and Supalla (1990: 20) points out that ASL has some linear morphology, e.g. agentive (analogous to the English -er suffix) and reduplicative markings. Second, the model fails to account for the fact that SEE2-type morphology does not exist even in spoken languages. What is striking about the inflectional morphemes of SEE2 is that they “are produced in terms of timing and formation as separate signs” (1990: 52). Supalla shows that they are not subject to assimilation; they are phonologically independent, articulated distinctly, even emphasized. In general, this kind of phonological independence is characteristic of
free morphemes but not of inflectional, bound morphemes, and the system seems to be unlearnable.

Clearly this cannot be modelled by an input-matching device of the kind discussed earlier, because the input is not matched. Furthermore, it is not enough to say that SEE2 morphology simply violates UG constraints, because that would not account for the way in which children devise new forms. More is needed from UG. The unlearnability of the SEE2 morphology suggests that children are cue-based learners, programmed to scan for clitic-like, unstressed, highly assimilable inflectional markers. That is what they find standardly in spoken languages and in natural signed languages like ASL. If the input fails to provide such markers, then appropriate markers are invented. Children seize appropriate kinds of elements which can be interpreted as inflectional markers. In signed languages there seems to be at least a strong statistical tendency to re-interpret linear elements in this fashion. It would be interesting to see work examining how this re-interpretation takes place and how new morphology is devised when children are exposed to unlearnable systems like SEE2. This would flesh out the general perspective of Goldin-Meadow and Mylander (1990) and Newport (1997).

Deaf children are often exposed to artificial input and we know a good deal about that input and about how it is reanalyzed by language learners. Therefore, the acquisition of signed languages offers a wonderful opportunity to understand more about abrupt language change, creolization and about cue-based learning.

5. Conclusion

I submit that work on abrupt creolization, the acquisition of signed languages, and on catastrophic historical change invites us to think of children as cue-based learners, who scan the environment for certain elements of I-language in unembedded domains. So a cue-based learner sets a Spec-head parameter (Spec precedes/follows its head) on the basis of exposure to data which must be analyzed with a Spec preceding its head, e.g. \[ [\text{SpecJohn's}] [\text{N hat}] \]. Less trivially, a cue-based learner acquires a V2 grammar not by evaluating grammars against sets of sentences but on exposure to structures commencing SpecP[XP], as discussed in section 3.1. This is the cue for a V2 system and the cue must be represented robustly in the PLD. The cue-based approach to parameter setting is implicitly assumed in some earlier work and it comports well with work on the visual system, which develops as organisms are exposed to very specific visual stimuli, horizontal lines for example (Hubel, 1978; Hubel and Wiesel, 1962; Sperry, 1968). Current theories of the immune system are similar;

---

11 Supalla (1990: 50–51) hints at a possible explanation for this tendency for non-linear morphology. He points to studies showing that individual ASL signs take about 50% longer to produce than English words, but comparable propositions take about the same amount of time. This is achieved by having signs with more morphemes per sign and non-linear morphological structure. This could be explained if language processing takes place naturally at a certain speed and if a language with ASL-type signs and a linear, affixal morphology is just too slow.
specific antigens amplify pre-existing antibodies. In fact, this is the kind of thing which is typical of selective learning quite generally. The cue-based approach has been productive for phonologists concerned with the parameters for stress systems (Dresher, 1995; Fikkert, 1994, 1995), and a similar approach has been invoked independently for some syntactic problems by Fodor (1995).

This is a radical departure from much current work on learnability, which postulates various forms of input matching. It is striking that so much of this work has children dealing with elements of E-language, often requiring that the system perform elaborate calculations in effect. For example, one of the best known results of work on learnability, the Subset Principle, calculates subset relations among sets of E-language and chooses among grammars accordingly. The model I have advocated here moves away from this practice and postulates children seeking elements of I-language in the input and selecting grammars accordingly; the model makes no reference to elements of E-language or to the output of the grammar.

Under this view, one would expect there to be grammatical changes which are abrupt, and one would expect languages to differ from each other in bumpy ways. It also provides a way of studying how parametric shifts affect populations of speakers, although this is an area where historical work has been bedevilled by confusion. It is generally agreed that certain changes progress in an S-curve (Kroch, 1989), but now Niyogi and Berwick (1995) have offered a model of the emergent, global population behavior which derives the S-curve. They postulate a learning theory and a population of child learners, a small number of whom fail to converge on preexisting grammars, and they produce a plausible model of population changes for the loss of null subjects in French. The fact that changes can be shown to progress through populations in an S-curve is not surprising to those who have thought in terms of chaotic systems and catastrophic, parametric changes (e.g. Lightfoot, 1991: ch. 7; Warner, 1995), but the success of Niyogi and Berwick is to show that it is not impossible difficult to compute (or simulate) grammatical dynamical systems; they show explicitly how to transform parameterized theories and memoryless learning algorithms to dynamical systems, producing results along the way. Under the model adopted here one would seek to quantify the degree to which cues are represented in the PLD, showing that abrupt, catastrophic change takes place when those cues are expressed below some threshold of robustness.

Of course, there is no theory of cues yet, i.e. no theory of what constitutes a possible cue. But then we also have no very substantive theory of parameters. These represent topics for future research.

If we can produce productive models for historical change along these lines, relating changes in simple cues to large-scale parametric shifts, our results will have consequences for the way in which we think about parameters, how they are set, and, therefore, for the way in which we study language acquisition. In particular, we shall not be surprised that changes sometimes occur abruptly. With the development of computer corpora, Niyogi and Berwick’s results, and an explicit cue-based theory of acquisition, we have all the ingredients for success in the historical domain, synthesizing work on language change, acquisition and variation.
References

Fodor, J.D., 1995. Fewer but better triggers. Ms., CUNY.


