

# Motives for Language Change

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# 1 On change in ‘E-language’

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*Peter Matthews*

In a view that is widespread among linguists, change in language is not simply change in ‘speech’: what is affected is ‘a language’, and by that is meant a system, at an underlying level, that in any community constrains the forms that speech behaviour can take. As a system changes so the speech in that community, which is partly determined by it, also changes. But a historian is not concerned directly with observed shifts in how people behave. We are seen instead as trying to explain how languages, as underlying systems, change from one state to another. We may speculate that they are subject to specific structural laws. We may posit laws of history by which changes in their structure have to follow one route rather than another. In this light, we develop theories in historical linguistics of a sophistication quite unheard of in most other fields of history.

The distinction between speech and language goes back to Saussure, and arguably beyond. In the terms, however, in which Chomsky has recast it, every individual speaker has what he calls an ‘I-language’, and the underlying changes are among I-languages developed by a changing population in successive periods. In any individual, the one formed in childhood will determine, in part, how that individual will speak; and that speech, in turn, will be part of the experience by which new members of the community form their own I-languages. When I-languages are different, we will expect to see shifts in the way a population speaks. In corresponding terminology, these will be shifts in an ‘E-language’: in a language as it is ‘externalised’; but our primary concern is not, in this view, with E-language. I-languages are seen as subject to laws. In Chomsky’s account, their structure is at its ‘core’ constrained by our genetic inheritance. For Chomsky himself, the central problem is then to explain how languages can vary. For historians who follow this lead, it is to explain how speakers in one period can develop an I-language different from the ones developed in an earlier period.

The answer must, in part, lie in the speech that they experience. Let us suppose, for example, that a word is borrowed from a neighbouring language. In Saussurean terms, this is an element in a new ‘état de la langue’; in Chomsky’s terms, there is at least an additional lexical entry in the minds of new speakers.

But how does it come to be there? The ‘language’ we are positing would not, at one stage, have included it. Therefore, to the extent that speech is determined by that system, it too would not have included it. But then, despite that, it would be borrowed by some speakers; others would follow their example; and, in time, it would become an element indistinguishable from others in the speech that children were exposed to. It would therefore become part of the ‘language’ as they came to know it; and this is again the system that would be reflected in their speech from then on. In such cases at least, it seems that, for the underlying system to be different, speech must change first. In Chomskyan terms, a difference in I-language would then follow from a difference in the experience on which its development is based.

A conclusion like this is again quite widely implied. But it is reasonable to ask, at that point, why a change in language has to be conceived of at two separate levels. The word, in cases like this, would be borrowed by some speakers, whose example would be followed by other speakers. These could as naturally include those of new generations. Why are changes not straightforwardly at just one level?

Let us turn for comparison to another field of social history. As speech changes so too, for example, do the things that people drink; and, once upon a time, no one in Britain drank tea. Therefore, if we must talk after the manner of linguists, we will say that the community’s drinking habits were determined, in part, by an underlying system in which tea was not an element. Then some members came into contact with societies whose systems, we will say, were different, and, despite the one in which we say they were brought up, they acquired a habit of tea-drinking from them. This habit they brought home and introduced to other members of their own society. But these at first were people who, like them, would have to have been brought up to the earlier system. So, if they too started drinking tea, it would be because, despite that system, they were curious or it was recommended to them; because it was a new fashion; because they found they liked it. Such explanations bear directly on the behaviour of specific individuals, in response to that of other individuals. Then, at a later stage, some members of the community would be familiar with tea-drinking from their childhood. Therefore, if we still talk in the manner of linguists, we will say that their behaviour is constrained by a new system of drinking habits, in which tea, although in practice some might never touch it, had a place like that which it has had since. They would thus have ‘internalised’ a set of rules concerning times and circumstances in which it was drunk, what forms of silver or crockery were used in drinking it, and so on. But it is not at all clear why we should be obliged to talk in that way. Is it not sufficient to say simply that some people started to drink tea, at specific times or in specific circumstances, using specific kinds of vessel, and other people imitated them? This explanation is again in terms of the behaviour of individuals, in response to that of other

individuals. What else is there, that we have to explain in terms of changes at an underlying level?

But when it comes to change in language, linguists do talk in just such a manner. The issue is an old one, with which Roger Lass, to whom this essay is dedicated, has long been familiar. But recent work, ostensibly at least Chomskyan, has raised it in what seems to be a new form.

Let us begin with Ian Roberts's conception of a 'step' in syntax. The context in which it was defined is that of Chomsky's theory as it developed in the 1980s, and the changes that were of special interest were those in which a parameter of 'Universal Grammar' could be seen as reset. These are, as Roberts put it, 'diachronic relations among I-languages' (1993: 159). An E-language was described, in contrast, as 'some set or corpus of sentences' (158), and another kind of relation is, accordingly, 'between the E-language of one generation . . . and the I-language of a subsequent generation'. A step, however, is a mere relation between E-languages. This is, as Roberts saw it, 'the traditional notion of change', and can involve 'the appearance of a new construction, or a significant change in the frequency of a construction, in a set of texts'. But when 'a language takes a new step' this does not 'necessarily imply' a change (in alternative terminology) in 'the grammar'. Changes in the 'traditional' sense are thus the nearest equivalent, in linguistics, of a change in actual habits of drinking. Their explanation must, in part at least, be independent of I-languages or 'grammars', since these may not change. But, of course, when such a step is taken, the experience of a later generation of speakers will be different. Therefore the 'grammar', as they develop it, may, in the light of their experience, be different also.

I will return to Roberts's formulation in a moment. But a theory of change in 'grammars' has also been developed, for some twenty years, by David Lightfoot. Since 1990 he too has appealed to Chomsky's theory of parameters; and, for most resettings, we must again envisage differences in the speech experienced by successive generations of children. These must be due to 'nongrammatical factors' (1999: 225). 'Some changes', more precisely, 'take place while grammars remain constant' (1991: 160), relating, as he put it, 'to the ways in which grammars are used rather than to their internal structure' (1991: 166). These might be 'explained by claims about language contact or socially defined speech fashions' (1999: 166) or, as in his first book on syntactic change, by 'foreign influence, expressivity and "after-thought"' (1979: 381). But, once they happen, changes in the speech that children hear may subsequently 'trigger' changes in the 'grammar' itself.

Two questions naturally arise. The first concerns the kinds of 'triggering' change we must allow for. In what ways, for example, can the speech of a community be influenced, independently of 'grammars' that its members are already said to have, by 'socially defined speech fashions'? What kinds of 'step', in

Roberts's definition, can be explained entirely by what Lightfoot calls a 'non-grammatical' factor?

Whatever the answer, these are changes that affect the speech of individuals, regardless of their 'grammars', in response to their perception of the speech of other individuals. It is therefore reasonable, again, to ask what other explanation is needed. What is a change in language other than, in Lightfoot's words, a change in 'socially defined speech fashions'?

The first question cries out for an answer. But, although such theories are ostensibly Chomskyan, it seems clear that the relation of E-language to I-language cannot be as Chomsky himself originally conceived it. In his account, the former was 'the object of study in most of traditional or structuralist grammar or behavioral psychology'; and, since different structuralists, for a start, did not define 'a language' in the same way, that is perhaps not wholly illuminating. But whatever the definition of E-language, it was 'now regarded as an epiphenomenon at best' (Chomsky 1986: 25). For Roberts, as we have seen, it was 'some set or corpus of sentences'; for Lightfoot, in a passage I have not yet cited, it is 'external linguistic production' (1999: 66). But it is of the essence of their theory that such external production, or the character of such sets of sentences, can change independently of 'grammar' or I-language. Therefore, if this is what Chomsky also meant by an E-language, it cannot be merely epiphenomenal.

If we grant this, we are left with a theory that in part at least is like the one developed by Eugenio Coseriu (1958) in the heyday of European structuralism. I have remarked on this parallel elsewhere (2001: 114f., 150f.), and will not labour it. But 'a language', in Coseriu's account, could be identified not only as a system, but as a system plus a set of 'norms' by which it is realised. The system of Latin included, for example, a *k* phoneme. But there were also norms by which it was realised, variably as, among other things, a front velar or a back velar. Change in 'a language' can then have its origin in individual departures from a norm. For example, a phoneme that was normally realised by a velar might sometimes have been realised, before front vowels, by an affricate. This might increasingly become a new norm; but, at that stage, such a change was still at the level of realisation only. Only later might the system itself change, as in the history of Romance, to a state in which the affricates realise a new phoneme.

In Coseriu's account the system was one of 'possibilities': it distinguished 'routes', or ways of speaking, that are 'open' to a speaker from others that implicitly are 'closed' (1962: 98). His examples were not from syntax; but the structures constituting an I-language will, in a similar sense, define a set of possible forms of sentences. Some arrangements of words, to speak in the most neutral manner, will be open and others closed, all else being equal, to the speaker whose language it is. But the frequency with which an open route is taken may then vary independently. A specific arrangement of words might

come to be 'used', for example, much more rarely. This would be one kind of step in Roberts's definition: 'a significant change', in his terms, 'in the frequency of a construction'. In Coseriu's theory, it would again be a change in norms by which constructions are realised. But, like any such step, it affects the speech to which a child of a new generation is exposed. If the construction is rare they may no longer have sufficient 'evidence', from what they hear, that the possibility is open. Therefore they may take it to be closed; and, with whatever accompanying repercussions, the 'language' they develop may come to exclude it. In this way, changes in the frequency of constructions, due to no more than a shift originally in usage, may be claimed, in Lightfoot's terminology, to trigger 'catastrophic' changes at the level of the 'grammar'. As Coseriu had put it earlier, the norms that a community follows may change to the point at which a system 'overturns' (1962: 107).

To what extent, then, might E-language, as determined by I-languages and an accompanying set of 'norms', change independently of I-languages themselves? In Coseriu's account, a change in norms would be within the 'possibilities' determined by the system. Each construction would represent a 'possibility', just as, in a case he did discuss, a pattern of word formation (1962: 78–9). But the system itself did not determine the range of words formed in a certain way. It would be a matter of norms that, for example, a noun formed from *reasonable* is realised as *reasonableness* not *reasonability*. Nor might the system determine, for example, which verbs take specific patterns of complementation. That too might be a matter of norms, and that too might change independently. The system itself would then change when new 'possibilities' are added or old 'possibilities' disappear. For example, English did not at one time have a productive formation in *-ee* (*employee, trainee*, and so on); as soon as it did, the system had to be in a new state.

But is the generativist theory quite the same? A step, in Roberts's definition, can again be a change in the frequency of a construction. But it can also be the 'appearance' of a new one. Is this also a step that does not 'necessarily imply' a change in the 'grammar'? Roberts did not confirm at this point that it was. But, if it could be, it would be a change in norms that would itself change what was 'possible' for a speaker. Only in the next stage, when it would have affected the experience of new members of the community, might the 'grammar' come to allow it.

How then do these theories account for new constructions? One answer is that they might arise directly through a process of reanalysis. A new generation of speakers would accordingly be said to have developed a 'grammar' based on reinterpretation of the speech heard from their elders. They could also be said to follow indirectly, when a parameter is reset for other reasons. In Lightfoot's account, parameters are set in accordance with specific 'cues' in speech that children experience. If a cue becomes, for example, rarer they will be set

differently by a new generation. This would then have repercussions; and the appearance of a new construction could in principle be one of them. But are these the only mechanisms that we must envisage? One ‘nongrammatical factor’, as we have seen, is ‘language contact’, and it is well known that, when languages are in contact, they may converge. There is no other way to explain a ‘linguistic area’ or *Sprachbund*. But what exactly is the process of convergence? Speakers said to have a ‘grammar’ of language A will be forced to communicate with ones who speak language B. To do so they may have, in the ordinary sense, to learn B. They may, in consequence, use words from B when they are speaking A: the nature of that mechanism is not in dispute. For convergence to be possible, it seems that they must also borrow new constructions from B. That would seem to involve a step in their E-language, independent of the ‘grammar’ of A that they will originally have developed.

Lightfoot has as yet said very little about how ‘nongrammatical factors’ operate. They are simply there because, for ‘grammars’ to change, the speech that children hear must, at least in many cases, change first. But, if we are on the right track, an E-language would be still less of an epiphenomenon. Frequencies can change independently, as we have seen, of I-languages. This could logically include the case in which a construction disappears: its frequency, that is, will be reduced to zero. If new constructions can enter speech directly so too could, for example, an extension in the range of words with which an existing construction is used. Why, then, is the ‘traditional notion’ of change, as Roberts described it in the passage with which we began, not in itself sufficient?

A follower of Chomsky might reply in two ways. The most likely answer is that I-languages instantiate, in part, a Universal Grammar. We know that this exists; therefore we know that I-languages exist, in abstraction from E-languages, in every speaker; therefore we need, in addition, a theory of change in I-languages. Some changes are, moreover, inexplicable unless this theory of a Universal Grammar is assumed.

I will return to this claim in the last part of this essay. But another reply is simply to insist on the distinction between ‘languages’ and ‘speech’. If someone, for example, drinks tea they can literally be seen to do so; and, when others imitate them, their behaviour can be seen to be similar. The abstraction implied is minimal. But when different speakers use the same construction, what they say may literally be very different. We are therefore forced to talk of abstract structures that they have in common. In Chomskyan terms, they ‘know’ the language that they have acquired as children, and this ‘knowledge’, or I-language, must in principle be different from ‘performance’, or observed behaviour in ‘using’ it.

This form of answer can again be traced at least to Saussure. But how exactly would a new syntactic construction or new pattern of word order spread through a community? Some speakers, let us say, would ‘have’ the pattern. That means

that it would be within the constraints of the 'grammar' as they knew it. Others would not 'have' it, and, as it spreads, their number would of course diminish. But who exactly would be 'using' it? Are they only those who would be strictly said to 'have' it? Its spread, in that case, would be limited to changes in the frequency with which they 'used' it, and the 'grammars' of a newer generation who would hear them. Or could it also be acquired, directly from their speech, by others who did not 'have' it? Such speakers would thus have knowledge that they had acquired in childhood of what forms of speech are possible and not possible – but then, in later life, would pick up further forms of speech that would extend it.

If so, we must ask how they are able to do so. A pattern or construction is an abstraction and, by the argument with which we started, it cannot be 'picked up' in the same way as, we said, behaviour like tea-drinking. It would seem then that a speaker could acquire a second form of abstract 'knowledge', additional to the 'knowledge' that is originally claimed to constitute a 'grammar'. The 'external production' of language, as E-language was defined by Lightfoot, would then reflect both.

It would be easy to find ways in which these different forms of 'knowledge' might be labelled. One way is to distinguish a speaker's 'active' competence in a language, as acquired in childhood, from an initially 'passive' knowledge of the speech of people who are encountered later. But this second form of knowledge would itself then come to exercise an 'active' influence on their own speech. I explored devices like this more than twenty years ago (1979: 51–66), as one hypothesis of 'idiolectal multilingualism'. Another way is to distinguish a 'core' knowledge, much as Chomsky distinguished it in the 1980s, from a 'periphery'. The former would again be fixed in childhood; but the periphery might in principle be open, therefore new things could be added to it later in a speaker's life. This would in essence be a variant of an idea that was fashionable, thirty years ago, in generative phonology. New rules or patterns could again be tacked on without change to mental structures that a speaker has already developed. They too would therefore be reflected in speech, and this, again, would be the speech heard by the children of a following generation.

Our question, however, was why 'knowing a language' should be seen as anything other than the state of having 'picked up' certain forms of speech. Why, again, do we not talk simply of one level of 'knowledge', both developing and expanding in the same way? A community's mastery of its forms of speech would then be attested equally by both the continuities and the changes in 'E-language'.

That is, I take it, Roberts's 'traditional notion' of change – that developed by Paul (1880) in particular. But the most likely riposte would again rest on the concept of a Universal Grammar. Although Chomsky's theory is not itself concerned with change in language, it has nevertheless to be admitted that, for

anyone who accepts it, much of what I have said so far is likely to seem neither here nor there. I must therefore refer to another recent essay (1998) for a fresh rehearsal of the reasons why I cannot myself take it for granted. It might be claimed, however, that the arguments for it are not only those that Chomsky himself originally proposed. Thus, in the account as popularised in Lightfoot's latest book, a 'grammar' will again develop in response to a specific set of cues that children can identify in the speech to which they are exposed. But a single cue does not determine just one aspect of a 'grammar'. Instead it will determine a whole range of them; so, if the experience of one generation of children differs crucially in one respect from that of earlier generations, the 'grammar' they develop may change drastically. This change in the 'grammar' will be reflected in E-language as observed from then on, which will in turn change in what would otherwise be unexpected ways. We can explain them only if we posit that the relation between cues and 'grammars' is as Universal Grammar determines.

Lightfoot's examples are from the history of English, a field I know at best at third hand. I will therefore restrict myself to asking how far such an explanation could in principle be convincing.

Let us first assume, for the sake of argument, that Chomsky is right. According to the theory that he elaborated in the 1980s, the properties that distinguish languages are then reduced, as far as possible, to different settings of genetically inherited parameters. But single parameters would not determine single properties. In setting, for example, the 'null subject' parameter children did not merely develop a language with or without null subjects. The relation would instead be one of what biologists call 'pleiotropy', in which a setting might be expressed by several characters that, at first sight, seem quite unconnected. By a 'character' we mean, for example, a construction or some individual pattern of word order. It therefore seems that Lightfoot too has got to be right. A cue will 'trigger' the setting of a parameter; and, when its setting changes, this will affect, potentially at least, all characters by which it may be expressed. The appearance of new characters might then be no more than a repercussion, as I put it earlier, of a change whose causes, in the 'triggering' experiences of children, are quite different.

This is indeed a very powerful theory. But it is not clear that it is necessarily what Chomsky's theory leads us to expect. Nor is it clear how Lightfoot's theory would be other than invulnerable.

The first doubt is suggested directly by my allusion to pleiotropy. For it does seem likely that the relation between languages and Universal Grammar would be very complex. Certain languages might be identified as having, for example, characters *a*, *b* and *c*. We might therefore conjecture, still in terms of Chomsky's theory as it was in the 1980s, that this reflects, in part, a setting of a parameter *P*. But we might not then be worried by the discovery of other languages that have

*a* and *b* but not *c*, or *b* but not *a* or *c*, and so on. We would simply conjecture that these differences reflect the setting of other parameters. The character identified as *a* might thus reflect a setting not of P alone, but of P and at least one other. The settings that are responsible for *b* and *c* would both be partly different. In this way we could account successfully for all the fine diversity of structures that is actually found. But it is less clear why historians should expect such structures to change suddenly and drastically. Could a single change in speech provoke a simultaneous change in many different parameters? If not, we might expect the changes we observe to be more gradual, as the expression of any that are reset is inhibited, at any stage, by that of others that have not been.

This is a question only; but it seems one that is at least worth raising. For if change were gradual, this would at best be a competing theory of what Winfred Lehmann, or Sapir before him, called 'drift'. One crucial change, relating to what Lightfoot calls a 'cue', would take place at the level of E-language. We would then expect that other changes of specific kinds should follow. But, of course, it would be easy to find explanations if they did not. Thus, in particular, some further 'nongrammatical factor', triggering change of a quite different kind, might be found to intervene.

But let us assume, in fairness, that the effect is instant. We would thus envisage crucial changes in cues; and, precisely because the expression of parameters is as complex as we have supposed, such a change, initially at the level of E-language, would then trigger changes, at the level of the underlying 'grammar', that cannot be other than pervasive. These will ensue directly in the 'grammars' of new members of the speech community. We should therefore expect their speech to differ strikingly from that of older members. We might also predict the same effects, in any other language, if the same cue were affected in the same way.

The problems then lie in the other factors that in principle could intervene. Let us suppose, for example, that a pattern *c* has formerly been common. That is in part because, we say, the older speakers have a 'grammar' that allows it. Then, for some extraneous reason, a new set of speakers form a 'grammar' whose parameters exclude it. Would we expect, in that case, not to find *c* in their speech? One possibility is that, in addition to a 'grammar' which excludes *c*, they might also be said to have one that allows it. This is again a hypothesis of 'idiolectal multilingualism', and, in a sophisticated version, we might again distinguish 'grammars' that develop in childhood, when an individual is in contact with a limited set of speakers, from subsidiary 'grammars' that develop in the course of wider contacts later. It might therefore be that younger speakers merely 'use' *c* less than older speakers, that they 'use' it most in 'accommodation' to older speakers, and so on. All the familiar effects of variation might thus be explained. But still, according to our hypothesis, there is a 'grammar' whose parameters have been reset; and, as more and more speakers have it, *c* will be doomed.

Such forms of explanation are explored by Lightfoot himself (1999: 92ff.). But another factor might again be the ‘periphery’. In Chomsky’s account, a part of each I-language follows from the setting of parameters: this was the ‘core’ as he defined it in the 1980s (1986: 147). Let us suppose, then, that our younger speakers have a ‘grammar’ whose core will exclude *c*. But the core of a ‘grammar’ is not claimed to be the whole of it: it is for that reason in particular that I have continued to put Lightfoot’s term in inverted commas. Could it be claimed then that the periphery of their I-language nevertheless allows *c*?

It is hard to know the answer, since the scope of a ‘periphery’ has not been explicitly constrained. We were told originally that it covers ‘exceptions’, such as irregular morphology or idioms. For Chomsky’s purposes, there was indeed no motive to say more. But constructions can also be exceptional. In English there are, for example, scattered patterns of inversion: after *neither* or *nor* (*Nor was I*), sporadically after *then* (*Then came the floods*), and so on. How exactly, then, would we describe their history? The pattern of *Then came the floods* was normal in the days of a ‘verb-second’ order; so the parameters would be said to have been set accordingly. Then their setting would have to change; this might be explained, in the terms that Lightfoot suggests, by changes in E-language such that some cue was no longer instanced with sufficient ‘robustness’. But would this pattern thereby vanish from I-languages affected? Let us claim, instead, that it was relegated to a periphery. It would then be exceptional, and we would expect it to be restricted lexically and, in time, to become rare. But no group of speakers would at once stop ‘using’ it.

By invoking either of these factors, or both, we could easily explain why sudden and pervasive changes in a ‘grammar’ might not, in reality, lead to either sudden or pervasive changes in speech. But there are two obvious comments. Firstly, it is only if the effects were sudden that the predictions of our theory might be confirmed. If they are gradual then, at any stage in any language, other changes, which would arise perhaps from new ‘speech fashions’ or from other ‘nongrammatical factors’, could again be claimed to intervene. What changes in speech could not then, in principle, be attested?

The second comment is that gradual shifts in speech are just what we expect if change is at a level of ‘E-language’ only. If Chomsky’s theory of I-language is right, we are again obliged to posit consequential changes at an underlying level. That is granted, and we would then have to consider whether they were likely to be local or pervasive. But do we again have any other motive, as historians, for positing an underlying ‘language’ of that kind?

It is appropriate to end with questions, since the theory that has provoked this essay may be further clarified or updated. But, in Chomsky’s later accounts, the core of an I-language may directly ‘instantiate’ a Universal Grammar (1995). The more, of course, this ‘core’ is simply invariant, the less historians of language will be concerned with it. Where languages vary systematically it is

said, conjecturally, to be a function of potential differences in their lexicon. Beyond what would be regular, it seems that there would still be a periphery; and, independent of all levels of I-language, we must then envisage Lightfoot's 'socially defined speech fashions', something like Coseriu's norms, and so on. We have to ask if there any reasons, other than a prior belief that knowledge of 'a language' must develop in the form that Chomsky says it does, why these proliferating levels should be seen as separate.

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