

# 19

## The Actuation Problem

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### 19.1 Introduction

The term *actuation problem* was first introduced and defined by Weinreich, Labov and Herzog (1968:102). In their words:

(1) **The actuation problem**

What factors can account for the actuation of changes? Why do changes in a structural feature take place in a particular language at a particular time, but not in other languages with the same feature, or in the same language at other times?

This actuation problem is one of five related problems that Weinreich, Labov and Herzog (1968) pose to historical linguists, in an attempt to break down the hows and whys of language change into manageable chunks. It is difficult to overstate the importance that Weinreich, Labov and Herzog accord to the actuation problem: they refer to it as ‘the very heart of the matter’ (1968: 102). Elaborating on their initial formulation, later in the paper they state:

... even when the course of a language change has been fully described and its ability explained, the question always remains as to why the change was not actuated sooner, or why it was not simultaneously activated wherever identical functional conditions prevailed. The unsolved actuation riddle is the price paid by any facile and individualistic explanation of language change. It creates the opposite problem – of explaining why language fails to change. (1968: 112)

This chapter will provide an overview of the problem in the domain of historical syntax, as well as evaluating a number of potential

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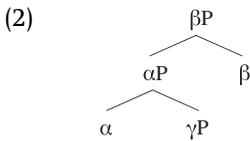
solutions.<sup>1</sup> In order to know whether a particular theory or explanatory principle succeeds in solving the ‘riddle’ of actuation, it is necessary to contrast the actuation problem with the other four key problems that Weinreich, Labov and Herzog outline for a theory of linguistic change; §19.2 serves this purpose, and also relates the actuation problem to questions of prediction and explanation. §§19.3 and 19.4 each survey a family of related proposals in the literature, dealing with ‘internal’ and ‘external’ factors respectively. §19.5 concludes.

## 19.2 The Actuation Problem in Context

### 19.2.1 Weinreich, Labov and Herzog’s Five Problems

Weinreich, Labov and Herzog introduce the actuation problem as the fifth, and toughest, of five problems that must be addressed by any comprehensive theory of language change, alongside the problems of *constraints*, *transition*, *embedding* and *evaluation*. I will outline each in turn.

The *constraints problem* involves formulating ‘constraints on the transition from one state of a language to an immediately succeeding state’ (Weinreich, Labov and Herzog 1968:100). The type of constraints involved can take a number of forms. One necessary general type of constraint is that no proposed language state should be synchronically impossible; following Lass (1997: 229), I will refer to these as *legality constraints*. To take a concrete example, Holmberg (2000) and Biberauer, Holmberg and Roberts (2007, 2008, 2014) present data motivating a constraint, the Final-over-Final Constraint, which serves to rule out the configuration given in (2), in which a head-final phrase immediately dominates a head-initial phrase.



If this constraint exists, then no language should be able to pass through a state in which structures like (2) are found. Biberauer, Newton and Sheehan (2009) argue, based on data from the history of English, Afrikaans and French, that this is correct; see also Ledgeway (2012: ch. 5) for an account of the role of this constraint in word-order change from Latin to Romance).

Lightfoot (1979 and subsequent publications) maintains that legality constraints in this sense are the only type of constraint that should form

<sup>1</sup> Some linguists use the term ‘actuation problem’ in a different sense, to refer to the puzzle of how innovations arise in general, or how change can be rare even when a constant bias is at work (see e.g. Baker, Archangeli and Mielke 2011). I will not address this usage here.

part of a theory of change: 'there is no theory of change to be had independent of theories of grammar and acquisition' (Lightfoot 2002: 127). Another logical possibility is that, independently of legality constraints, there are *process* constraints that restrict possible pathways of change. This type of reasoning has particularly been pursued in the literature on grammaticalization, where certain processes have been characterized as impossible due to unidirectionality (see Chapters 2 and 23, this volume, and Börjars and Vincent 2011, for discussion).

The second of Weinreich, Labov and Herzog's five problems is the *transition* problem, which is discussed only briefly. This is the question of what intervening stages can (or must) be posited between any two forms of a language separated by time. A core instance of the transition problem in syntax is the question of what happens after a change is first innovated but before it has become ubiquitous. Timberlake's (1977) notion of *actualization* is one suggestion: once reanalysis has taken place, a change trickles down through the grammar from one context to another, moving from least to most marked contexts (for a recent reinterpretation, see De Smet 2012). Another influential conception is that of competing grammars. Under this view, due to Kroch (1989, 1994), once innovation has taken place, the new grammar and the old grammar coexist for a period of time, with the new grammar increasing in frequency at the expense of the old. This view is well suited to accounting for Constant Rate Effects (see Kroch 1989; Pintzuk 2003), in which the rate of replacement of the old form by the new form is constant across linguistic contexts.

The *embedding problem* is stated as follows: 'How are the observed changes *embedded* in the matrix of linguistic and extralinguistic concomitants of the forms in question? (That is, what other changes are associated with the given changes in a manner that cannot be attributed to chance?)' (Weinreich, Labov and Herzog 1968: 101; emphasis original). The embedding problem has both a linguistic and a social aspect, which can and should be studied separately (Labov 1982: 28). In both cases, the problem can be approached by the study of correlations: between seemingly independent elements of the linguistic system in the first case, or between linguistic elements and elements of the extralinguistic system of social behaviour in the second case (Labov 1972: 162). Within the study of syntactic change, researchers working in the Principles and Parameters framework have been particularly concerned with the embedding problem, often attempting to relate apparently distinct syntactic phenomena as reflexes of a single parametric change (see Lightfoot 1991, 1999; Roberts 2007; Chapters 7 and 27, this volume).

Finally, the *evaluation problem* deals with the subjective evaluation of a change in progress by members of a speech community, both above and below the level of consciousness (Labov 1982: 28). It is probably fair to say that the evaluation problem, along with the social aspect of the embedding problem, is the least studied of Weinreich, Labov and

Herzog's (1968) five problems as far as syntactic change is concerned: those working on syntactic change often abstract away from the social (see, for example, Hale 1998: 5–6), and the majority of the sociolinguistic literature focuses on sound change (though Naro 1981, Romaine 1982, Cheshire, Adger and Fox 2013 are notable exceptions). This imbalance may not be entirely for sociological reasons: Labov and Harris (1986: 21) suggest that '[a]bstract linguistic structure has little or no social impact on members of the community', perhaps in part because it is rarely consciously evaluated. More recently, Ingason, Sigurðsson and Wallenberg (2012) have put forward a strong Antisocial Hypothesis as regards syntax, suggesting that word order is not socially evaluated unless identified with specific phonological or lexical material. Chapter 25, this volume, explores the issue of social conditioning in much more detail.

It is clear that a solution to the actuation problem will rely on solutions to all of these problems. Yet the actuation problem remains a distinct question: even with a full understanding of (i) the *constraints* governing a change, (ii) the process of *transition* involved, (iii) how the change is *embedded* in other aspects of the language and society and (iv) how it is *evaluated*, we still do not necessarily know why the change took place when and where it did.

### 19.2.2 Prediction, Explanation and The Actuation Problem

Weinreich, Labov and Herzog (1968: 99f.) distinguish between a 'strong' and a 'weak' theory of language change. A weak theory of language change is essentially coextensive with the problem of *constraints*. A strong theory, on the other hand, is one that is fully predictive: given such a theory and a description of the structure of a language and its speech community at a particular point in time, it should be possible to predict the course of its subsequent development. Solutions to the actuation problem are, of course, an essential component of any such predictive theory.

Weinreich, Labov and Herzog are sceptical: 'Few practicing historians of language would be rash enough to claim that such a theory is possible' (1968: 99). Others have expressed similar views. Meillet (1921: 16) noted that all the laws of linguistic history discovered up to that point were statements of possibility, not of necessity, and that the variable conditions which permitted or catalysed the changes which actually occurred remained to be discovered. The point has been most forcefully made by Lass (1980), who compares explanations proposed in the historical linguistic literature with a gold standard of deductive-nomological explanation, and finds them lacking.<sup>2</sup> Lass's conception of deductive-nomological

<sup>2</sup> Lass goes on to conclude that deductive-nomological explanations are *in principle* unavailable in historical linguistics due to the nature of language change (1980: ch. 4). Lass (1997: 336), while acknowledging some weaknesses of his earlier work, remains convinced that it is not the case that 'causal explanations are or ever will be available' in historical linguistics.

explanation, following Hempel and Oppenheim (1948), is said to be one that ‘characterizes the physical sciences (or a particular version of them)’, and is ‘in principle equivalent to a prediction’ (1980: 9). Given an explanans – a conjunction of statements specifying both general laws and antecedent conditions – the explanandum (here, the change in question) follows necessarily by the rule of inference *modus ponens*.

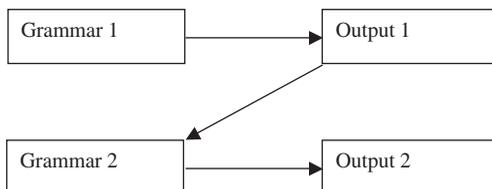
The actuation problem and the notion of deductive-nomological explanation are closely related. Specifically, some form of deductive-nomological explanation is a precondition for solutions to individual instances of the actuation problem: if we do not know what the sufficient conditions for a change are, we cannot say why it occurs when and where it does. At the same time, deductive-nomological explanation is not enough to guarantee a solution to the actuation problem. This is because the explanans consists only of conditions that are *sufficient* for the explanandum to occur; these conditions do not have to be *necessary*. A deductive-nomological explanation could then in principle explain why a change had to occur at time  $t$ , while the change in fact occurred at time  $t - 1$ ; from the point of view of the actuation problem, this is clearly unsatisfactory.

If a deductive-nomological explanation for a change is not available, then, it follows that the actuation problem has not been solved with respect to that change. It is in this respect that Lass’s (1980) arguments against proposed explanations become important in the context of the actuation problem. §19.3 brings out this relevance for individual types of explanation.

### 19.3 Internal Factors: Speakers, Hearers and Learners

Historical linguists are largely in agreement that the locus of diachronic discontinuity is the gaping chasm between the speaker and the hearer.<sup>3</sup> This is schematized in Figure 19.1, the Z-model of Andersen (1973: 767).

This section focuses on properties of individual language users (speakers, hearers, learners), and whether these properties help us to



**Figure 19.1** Language transmission and change

<sup>3</sup> Lass (1997) is a notable exception, arguing that ‘we don’t gain anything by invoking’ speakers and hearers (1997: 377, fn. 42).

solve the actuation problem. A variety of such properties have been suggested to play a causal role in language change. Here I focus first on reanalysis as a commonly assumed mechanism of syntactic change. I then address first-language acquisition and processing-based explanations in turn.

### **19.3.1 Reanalysis**

Langacker (1977: 58) defines reanalysis as ‘change in the structure of an expression or class of expressions that does not involve any immediate or intrinsic modification of its surface manifestation’ (see also Harris and Campbell 1995:61). Reanalysis on the part of the hearer/learner plays a central role in the historical narratives of linguists of all theoretical persuasions. Roberts (2007: 123), for example, considers reanalysis to be a central mechanism of syntactic change, and ‘intimately bound up with parameter change’; Hopper and Traugott (2003: 59) state that ‘grammaticalization always involves reanalysis’. Whatever the merits of reanalysis as a mechanism (see Chapter 4, this volume), given its centrality to historical syntax, we can ask: does reanalysis help us to solve the actuation problem?

A prerequisite for reanalysis is string ambiguity: a string has to be amenable to more than one analysis, the ‘old’ and the ‘new’ (see Timberlake 1977: 168). Questions arise immediately: why does the new analysis arise when it does? And why is the new analysis adopted at the expense of the old? These are questions to which the concept of reanalysis has no obvious answers in and of itself, though this is of course not to say that the concept is worthless: reanalysis is a mechanism, not an explanation. In order to address these issues, reanalysis needs to be supplemented at the very least with a theory of what the hearer/learner is actually doing: in other words, why one analysis arises and is preferred. This theory can take a number of directions, and the rest of §19.3 is devoted to exploring these directions.

### **19.3.2 L1 Acquisition Pressures**

The idea that the process of first-language (L1) acquisition by children is crucial to understanding language change is generally attributed to Paul (1880), and in recent years has been championed by diachronic syntacticians working in a generative framework (see, for example, Lightfoot 1991, 1999; Roberts 2007). The key notion is that learning may be ‘imperfect’, in the sense that it does not result in 100 per cent accurate replication of the grammar underlying the input data (see Chapter 24, this volume, for in-depth discussion). Assuming, then, that L1 acquisition plays some role in change: do explanations in these terms allow us to solve the actuation problem?

The first and most essential ingredient that any theory of acquisition needs in order to be in with a chance of solving the problem is that it be deterministic in the sense of Walkden (2012: 894): for any temporally ordered set of sentences (primary linguistic data or PLD), any and all learners exposed to it will converge on the same grammar. The assumption of determinism is necessary because, if the learner's behaviour exhibits randomness, then it is impossible to predict (except stochastically) how a given learner will react to a given set of PLD and consequently impossible to predict when and where an acquisition-driven change will take place. Lightfoot anticipates this point, stating that the ideal theory of grammar and of acquisition, possessed by the acquirer, 'will be so restrictive that for any given language [i.e. set of PLD; GW] there will be only one grammar capable of producing the relevant output in a way consistent with the theory' (1979: 16).

An early and influential proposal for a key component of acquisition is Lightfoot's (1979) Transparency Principle (see Chapter 15, this volume), a 'requirement of the theory of grammar that derivations be of limited complexity' (1979: 344). The Transparency Principle aims to address the problem raised at the end of the last subsection, by supplementing the notion of reanalysis with an acquisition-based explanation for when it will take place and the direction it will take. It is couched in the Extended Standard Theory of the day, and envisages an upper bound on the extent to which transformations can cause surface structure to differ from the initial (base) phrase marker. The idea is that once derivations reach a certain level of opacity and exceptionality, the Transparency Principle will force reanalysis to occur. Lightfoot attributes a high degree of explanatory power to this principle: 'such a principle of grammar will predict the point at which radical re-analyses will take place' (1979: 122), and states that the principle, properly formulated, will provide 'an *explanation* for the re-analysis and for the fact that it took place at this time' (1979: 123; emphasis original), clearly echoing Weinreich, Labov and Herzog's (1968) wording in their formulation of the actuation problem.

The Transparency Principle has been criticized on both an empirical and a conceptual level. See McMahon (1994: 116–29), and Chapter 17, this volume, for an accessible treatment of the proposal and its critics. Even if we assume that the Transparency Principle can be precisely formulated and motivated, however, the principle does not obviously have the explanatory force that Lightfoot attributes to it. This is because its predictions, such as they are, state only that when faced with a particular type of PLD, the acquisition process will take a particular course. In order to predict the time at which a change will take place, an additional ingredient is needed, namely a notion of when this particular triggering experience will arise. Lightfoot clearly acknowledges this limitation in recent work, stating that '[w]hat we cannot explain, in general, is why the linguistic environment

should have changed in the first place (as emphasized by Lass 1997 and by others)' (2006: 165). In order to have a truly predictive theory of history which is able to overcome the actuation problem, we need to be able to identify what Lightfoot terms the *local* cause of a change (2006: 166), which is necessarily external. Whether or not this type of explanation is possible will be the focus of §19.4.

The Transparency Principle itself does not feature heavily in more recent work on diachronic syntax. However, the notion that properties of L1 acquisition strategies may play a key role in explaining language change remains alive and well. In particular, Roberts and Roussou (1999, 2003) and van Gelderen (2004, 2009, 2011) have proposed preferences on the part of the acquirer that they suggest explain cross-linguistically observed diachronic pathways of grammaticalization. For Roberts and Roussou, grammaticalization is upward reanalysis, and this follows from (3) (Roberts and Roussou 2003: 201, their (23)).

(3) Featural Simplicity Metric

A structural representation R for a substring of input text S is simpler than an alternative representation R' iff R contains fewer formal feature syncretisms than R'.

Van Gelderen (2004, 2009, 2011) proposes two principles which have roughly the same effect: the Head Preference Principle, and the Late Merge Principle. In her recent work she suggests (2009: 186–9) that both follow from a principle of featural economy similar to (3), which prefers uninterpretable features over interpretable features over semantic features.

Lightfoot (2013), however, criticizes the work of Roberts and Roussou (2003) and van Gelderen (2004, 2011) on much the same grounds as I have criticized the Transparency Principle above: 'saying that it [a change; GW] results from an internal drive or a UG bias gives no explanation for why it happened when it did nor under what circumstances' (2013: e35), i.e. it does not solve the actuation problem.<sup>4</sup>

Van Gelderen (2009) explicitly contrasts her perspective with that of Lightfoot (1999, 2006), stating that '[f]or Lightfoot, change can only come from the outside, i.e. triggered by variable data ... I have argued the opposite: that change can come from the inside' (2009:189). However, under her approach, variable data are still required, as otherwise we have no account for why the economy principles take effect when they do. Lohndal (2009: 215), in a discussion of van Gelderen's principles, states that 'unless the external data is such that the principle can kick in, it won't'.

<sup>4</sup> However, as we have seen, Lightfoot is willing to recognize this limitation in *any* theory of change (2006: 165f). It is therefore odd that he should choose to criticize Roberts and Roussou (2003) and van Gelderen (2004, 2011) for not overcoming it.

As we have seen from the examples of the Transparency Principle and of formal approaches to grammaticalization, it is in principle impossible for a theory of change that consists only of a theory of grammar and of acquisition to provide solutions to the actuation problem. This is because acquirers are located in time and space, and are responsive to the PLD they receive: their exact response will depend on these PLD, yet the theory of acquisition by itself says nothing about what these PLD will look like or when they will arrive. At best such a theory can predict what an acquirer will do when faced with a particular set of PLD, and this capacity is likely to turn out to be an important component of any theory of change.

### 19.3.3 Functional Pressures

Another tradition of explanation that is firmly grounded in the behaviour of individual language users ascribes diachronic change to functional pressures that act indirectly through processes of production and perception to make the language more user-friendly in some respect, typically with regard to its communicative function (see Chapter 31, this volume, for an overview). I will here discuss one paradigm example: Hawkins's (1990, 1994, 2002a, b, 2004) theory of processing efficiency and complexity.

Functional approaches to language are often contrasted with formal approaches: however, this dichotomy is misleading, as functional explanation of the origin of linguistic features and typological tendencies through diachrony is perfectly compatible with a formalist stance on symbolic computation, and with a clear distinction between competence and performance (see Newmeyer 1998; Fischer 2007). The principles proposed by Hawkins, for instance, presuppose some notion of phrase structure similar to that found in X-bar theory. Furthermore, explanations for diachronic change proposed in the 'formalist' literature often have a heavily functional flavour: Lass (1980: 66) observes that the notion of optimization incorporated in King's (1969) classic work on language change and generative grammar is 'nothing if not "therapeutic"'. The same is true of both the Transparency Principle, which Lightfoot explicitly describes as therapeutic (1979: 124), and of Roberts and Roussou's and van Gelderen's simplicity metrics, which serve to eliminate excess complexity in representations. It therefore makes more sense to view functional and acquisition-based explanations of change as the same basic type of explanation: 'internal' – understood as pertaining to speakers, hearers and learners as the (mostly) unconscious agents of change – as opposed to 'external' (on which see §19.4).

Central to Hawkins's work is the Performance–Grammar Correspondence Hypothesis (PGCH) given in (4) (Hawkins 2004: 3).

## (4) Performance–Grammar Correspondence Hypothesis

Grammars have conventionalized syntactic structures in proportion to their degree of preference in performance, as evidenced by patterns of selection in corpora and by ease of processing in psycholinguistic experiments.

Evidently, for this to work it is necessary to have an account of what is preferred in performance. Hawkins's key metric for this is given in (5) (Hawkins 2004: 107).

## (5) Early Immediate Constituents (EIC)

The human processor prefers linear orders that maximise PCDs [Phrasal Combination Domains; GW] (by maximizing their IC-to-nonIC [or IC-to-word] ratios), in proportion to the minimization difference between competing orders... The PCD for a mother node M and its I(mmediate) C(onstituent)s consists of the smallest string of terminal elements ... on the basis of which the processor can construct M and its ICs.

EIC creates a preference for harmonic orderings, i.e. those in which the direction of branching is consistently to the left or consistently to the right, and a dispreference for centre-embeddings. By the PGCH, then, we should see transitions away from disharmonic systems, all else being equal.

Hawkins is mute with regard to actual historical mechanisms that might underlie the PGCH or explain specific effects, as noted by Aitchison (2003: 742) and Walkden (2009: 69): we have no understanding of exactly how grammars might conventionalize syntactic structures that the parser prefers. Nevertheless, the idea that they might do so is not implausible. If a principle like EIC is attributed causal status, though, it is susceptible to the same kind of arguments adduced by Lass (1980: ch. 3) against functional pressures in sound change. In brief: if EIC is strong enough to cause disharmonic branching and centre-embeddings to disappear, then why does it (a) not cause them to disappear in all cases and all related languages, and (b) allow them to arise in the first place? To take a concrete example: early English possessed OV word orders – which for simplicity's sake we can treat as involving a head-final VP, following van Kemenade (1987), Pintzuk (1999, 2005) and subsequent work – at the same time as head-initial structure higher up the tree, for instance in the CP. According to EIC, this type of word order is dispreferred, since the parser will not be able to construct the mother node VP until after the verb is encountered. Therefore, EIC could be said to have played a causal role in the change to consistently right-branching structure in the history of English. However, the supposedly problematic word order is maintained in the history of German until the present day. If EIC is causally active, then why not here as well, especially given that English and German have a common ancestor?

One possible answer, of course, is that EIC is not the only principle of performance in Hawkins' theory. In his more recent work (2004, 2007), EIC is in fact subsumed under a principle of Minimize Domains (MiD), which operates alongside Maximize On-line Processing (MaOP) and Minimize Forms (MiF). Hawkins views these principles as competing with one another. For instance, in languages that are VO and NRel, MiD and MaOP are acting in unison; in languages which are OV and RelN, MiD defeats MaOP, while in languages which are OV and NRel, MaOP comes out on top (2002b: 221). The retention of OV word order with a head-initial CP in German could, then, potentially be analysed as an instance of another principle trumping MiD.

The prioritization of different functions in different languages is an argument that Lass (1980) anticipates, however, again in relation to an explanation for a sound change proposed by Campbell (1975):

The trouble with arguments like this is that you can't lose. What Campbell seems to be saying is that 'paradigm conditions' are important except where something else is. To my mind the virtual invulnerability of functional arguments like this strongly militates (to put it weakly) against their acceptability. (1980: 70)

For Hawkins's account, then, the problem is this: why would MiD be ranked above MaOP in one language but not another? Claiming priority for a specific principle in a given language is simply restating the (diachronic) problem: re-ranking of preferences must be possible (see, for instance, Bermúdez-Otero 2006 and the papers in Holt 2000 for diachronic work in Optimality Theory), yet we need an explanation for why and when this would occur.

In sum: functional preferences cannot be both universally active and causal in change, as this leads to a regress problem, but if they are not universally active then we need to know when they are active and when they are not, a question which cannot itself be answered in functional terms.

### 19.3.4 Internal Factors: Interim Conclusion

So far we have seen that internal approaches to motivations for language change – whether rooted in acquisition, cognition, or language use – are unable to provide satisfactory answers to the actuation problem, at least as long as they involve universal and invariant properties of human cognition or communication. This is not a new realization. Hale (1998) states:

Numerous 'causes' of change have been cited in the literature. Most mysterious is the frequent claim that various 'constant' factors are the 'cause' of change events. By 'constant' I mean merely factors that can be plausibly assumed to be active in every acquisition event. The fact that these factors were 'active' during the acquisition of the grammar that

serves as the target for a particular acquirer and did not induce a particular change during that earlier acquisition event would seem to preclude attributing changes during the second acquisition event to such factors.

(1998: 8, fn. 9; *scare quotes original*)

Though stated in terms of acquisition, the same problem arises for any ‘constant’-based explanation. Similarly, Labov (2001: 503) formulates a *Principle of Contingency*, asserting that the ‘starting point of a historical and evolutionary approach to language change is that one cannot account for change by any universal trait of human beings or of language’; see also Kroch (2000: 699f.), Lightfoot (2006: 165) and Roberts (2007: 126). This is not to say that universal traits are unhelpful in understanding language change: just that, in Weinreich, Labov and Herzog’s (1968) terms, they help us to solve the constraints problem and perhaps the transition problem, but not the actuation problem.

There are two obvious responses to this situation, other than giving up. The first is to seek explanations in terms of external factors, such as those which might cause different sets of PLD to be available to different acquirers. The second is to abandon the assumption that the universal traits or preferences are in fact universal, or at least universally ranked. This second direction leads us to the type of competing-motivations approach that has been discussed in §19.3.4 in relation to the work of Hawkins (2002a, b, 2004). But, as observed in that section, this is merely a restatement of the problem, since now we need to account for the differences between individuals in terms of the preferences or rankings. Whichever response we choose, then, we are forced to turn to external factors in order to resolve the situation.

## 19.4 External Factors: Population Thinking

Lightfoot (1999, 2006, 2013) is not optimistic about the study of external factors. This is not surprising, since it is unlikely that we will ever be able to recover the full details of the PLD for any given acquisition situation. This situation leads Lightfoot (1999) to explore the notion that language change is ‘chaotic’, in the technical sense: in other words, if it is deterministic, then it is also extremely sensitive to initial conditions, so that we cannot predict the outcome in practice (see also Hale 1998: 9).

Under certain conditions, however, it might be possible to determine what sort of speakers/hearers/learners are present in what sort of situation, or what sort of PLD will be produced. Research in two closely related areas has addressed this question: language contact, and sociolinguistics.

These perspectives require us to abandon the stance that I-language (‘individual’, ‘internal’ and ‘intensional’ language in the sense of Chomsky 1986) is the only coherent object of study, and that a theory of

change must reduce to a theory of grammar plus a theory of acquisition (as argued by Lightfoot 1979 and Hale 1998). Instead we must view our I-language-bearing individuals as situated in populations, the composition and distribution of which may change. This population thinking, dominant in evolutionary biology since Darwin, is defined by Lewens (2007) as ‘any effort to abstract from a characterisation of individual psychological profiles, in a way that allows an exploration of the consequences of these individual-level dispositions for population-level properties’ (see also Mayr 1976: 26–9; 2004; Sober 1980). Importantly, this does not necessitate an abandonment of the insights derived from I-language approaches; in fact, it requires and presupposes a good characterization of the individual, as Lewens’s definition makes clear.

#### 19.4.1 Language Contact

Contact has had a chequered history as a type of explanation for change; see Chapter 8, this volume, for a full overview. A common twentieth-century stance has been to ascribe a change to the effects of contact only if there is ‘clear and overwhelming evidence’ (see Lass 1997: 201), with ‘endogenous’ explanations to be given priority. This stance has not gone without criticism: see Farrar and Jones (2002) and Filppula (2010: 449) for rejections of this ‘if-in-doubt-do-without’ methodology. If, as argued in §19.3, internal explanation cannot solve the actuation problem alone, it is worth exploring the explanatory power of contact-based explanations.

If there are speakers possessing two different types of grammar in contact within a population, there are three logical possibilities for the outcome. One is that nothing happens, and stable variation persists. The second possibility is that features of one grammar type are incorporated into the other (which can, of course, happen in both directions simultaneously). Thirdly, the grammar types may change due to contact, but in ways that do not cause them to resemble one another more.

Setting the first possibility aside as uninteresting for the study of the actuation problem, I will refer to the second as *transfer*, following Winford (2003; 2005: 376), who develops ideas first found in van Coetsem (1988, 1995, 2000). Winford develops an approach that might help to predict what type of transfer will occur and in which circumstances. The approach revolves around language dominance, understood in psycholinguistic terms as the language in which the speaker is most proficient. Winford (2005: 376) and van Coetsem (1988: 3) define two types of transfer. The first is *borrowing*, in which material is transferred under what has been called ‘recipient language agentivity’, i.e. when a speaker dominant in the language that will ultimately receive the feature is actively using features from a different grammar. The second

is *imposition*, in which the source language speaker is the agent (source language agentivity).

The distinction between imposition and borrowing is not merely terminological: according to van Coetsem (1988: 25) and Winford (2005: 377), recipient language agentivity and source language agentivity give rise to different types of change. In particular, Winford (2005) argues at length that borrowing standardly involves the transfer of open-class vocabulary items, whereas imposition is more likely to lead to the transfer of phonological or syntactic features. The distinction is not absolute: Winford suggests (2005: 383) that lexical transfer from French into Middle English was mediated by both source and recipient language agents. Similarly, structural transfer may occur under borrowing of lexical items together with their syntactic properties (Winford 2005: 385–8), as seems to be the case with phrase-final prepositions in Prince Edward Island French (King 2000), or it may occur as a result of imposition, as Winford (2005: 394f.) suggests is the case with the argument structure of verbs like *gustar* in Spanish in contact with English, which come to take a nominative experiencer. However, to the extent that it is possible to identify types of transfer characteristic of borrowing and imposition, then, given a characterization of a population in terms of the distribution and psycholinguistic dominance of its individuals, we can approach the question of what features will be transferred, and when and where this transfer will occur. Of course, more work needs to be done on linking different linguistic types of transfer to the two types of agentivity, and on investigating the individual-level processes of production and comprehension they are derived from. Winford (2005: 417–20) takes a step in this direction by looking at production in code-switching, following Myers-Scotton (2002, 2003).

Not all effects of contact are cases of transfer, as Winford recognizes (2005: 376, fn. 3). Lucas (2009) discusses a further type of contact-induced change, which he labels *restructuring* and which corresponds to the third logical possibility mentioned above: ‘changes which a speaker makes to an L2 that cannot be seen as the transfer of patterns or material from their L1’ (2009: 145; see also Trudgill 2011).<sup>5</sup> Lucas illustrates this possibility using several case studies of L2 acquisition in which systematic deviations from the target grammar have been observed that cannot be interpreted as resulting from the acquirer’s L1 (2009: 135–8). Håkansson, Pienemann and Sayehli (2002) show, for example, that speakers of Swedish (a V2 language) learning German (another V2 language) as an L2 regularly produce non-V2 structures in their German output.

<sup>5</sup> Van Coetsem (1995: 70) and Winford (2005: 376f.) claim that a speaker is not necessarily psycholinguistically dominant in his/her first or native language, though the two will coincide in the majority of cases. See Lucas (2009: 111–20) for discussion of this issue.

The impact of L2 acquisition, and its potential to lead to contact-induced changes that are not direct transfer, has not been widely studied in the literature on diachronic syntax, though Weerman (1993) is an early exception. Weerman argues for the influence of Old Norse on English in the change from OV to VO (1993: 918–22), despite the fact that both languages were OV, on the grounds that L2 learners of V2 and OV languages tend to assume VO (Clahsen and Muysken 1986). Roberts (2007: 391–9) and Lucas (2009: 145–9, 167–9) provide further discussion. Though this particular example is debated, it is clear that explanations of this *kind* provide something that internal explanations cannot: they can help us identify the reasons that a change took place when and where it did, in population terms. In the case of the change from OV to VO in English, a contact-based explanation has the potential to explain why Dutch and German have not undergone this particular change.

#### 19.4.2 Sociolinguistic Explanations

Of course, not all linguistic changes occur in circumstances in which there are two clearly distinct ‘languages’ (in the pretheoretical sense) in contact. This is presumably what Lass (1997: 209) has in mind when he states that internal explanations are to be preferred on the grounds that ‘endogenous change *must* occur in any case’. Beyond clear cases of language contact, then, can a case be made for external causal factors? The search for such factors is a major part of the work of sociolinguists, in addition to the social embedding problem and evaluation problem as discussed in §19.2 (see also Chapter 23, this volume).

Hale (1998) is sceptical about the role of external factors. He draws a distinction between ‘change’, i.e. the emergence of a new structural feature, understood simply as a set of differences between two grammars mediated by a single acquisition event, and ‘diffusion’, whereby additional speakers acquire the new feature. Diffusion, according to Hale, ‘represents the trivial case of acquisition: accurate transmission’, and no special explanatory principles are required to account for it (1998: 5); furthermore, “populations” are irrelevant for those interested in studying the properties of I-language’ (1998: 6). Diffusion in Hale’s sense is indeed irrelevant for the study of the actuation problem, but this does not mean that the structure of populations is also irrelevant. Labov (2007) draws an important, but different, distinction between transmission and diffusion. Transmission, in Labov’s sense, refers to an ‘unbroken sequence of native-language acquisition by children’ (2007: 346), which he argues is likely to be highly accurate; diffusion in Labov’s sense, by contrast, involves the attempted replication of linguistic features between adults, often highly inaccurate.

Labov illustrates the difference between transmission and diffusion with reference to the New York short-*a* system (2007: 353–72), among

other case studies. He demonstrates that the alternation between tense and lax variants is conditioned by a complex array of factors: function words, for instance, have lax short-*a*, while the vowel is tense in corresponding content words, and there are a number of lexical exceptions to the general phonological rules. The New York system has diffused to other communities, but typically imperfectly: for instance, in New Jersey and Albany the function-word constraint has been lost. Diffusion, according to Labov, normally leads to a 'loss of structural detail' (2007: 357), typical of adult-language acquisition. Note that here we have an event of change, in Hale's terms, but one whose occurrence can be understood in terms of the structure of the population.

The distinction between transmission and diffusion has not been investigated with regard to syntactic change, but the structure of populations plays a similar explanatory role in Kroch's (2000) and Eitler's (2006) accounts of the loss of V2 in English (see also Lightfoot 1999: 151–8). The starting point for these accounts is the observation that Old English had a mixed V2 system, in which the presence or absence of V2 was contingent on properties of the subject and initial adverbs as well as clause type. Northern dialects of English then developed a strict V2 system, comparable to that of present-day Continental Germanic and Scandinavian languages, through contact with speakers of Old Norse (Kroch and Taylor 1997). Speakers of the northern strict V2 and southern mixed V2 varieties would then have come into contact with one another as a consequence of population movements in the Middle English period. Kroch (2000) argues that the non-V2 grammar characteristic of modern English is a consequence of this contact, as northerners hearing surface V3 clauses interpreted the grammar underlying them as non-V2 (rather than mixed V2), and, in accommodating, innovated a non-V2 grammar. Building on Kroch's account, Eitler (2006: ch. 5) demonstrates that this grammar took off first, and fastest, in some London varieties and particularly in the northern and central Midlands. He ascribes this to the heavy immigration and weak social network ties in these areas at the time. These plausibly caused diffusion in Labov's sense/restructuring in Lucas's sense: imperfect acquisition of a grammatical system by adults. Again, if correct, this scenario yields insight into why the change happened when and where it did.

Discussing Milroy's (1992: 10) claim that '[i]n order to account for differential patterns of change at particular times and places, we need first to take account of those factors that tend to maintain language states and resist change', Campbell (2013: 195) queries: 'How can the actuation problem, the question about how changes get started in the first place, be approached with a model based solely on norm maintenance, that is on resistance to change but not on change itself?' Given the discussion in this section, the answer is not too difficult to sketch. According to Milroy, weak ties in social networks are the locus of change. If weak network ties are

likely to involve situations of adult language learning and hence lead to diffusion/restructuring, then these nodes in the network may be not only those through which new features are transmitted most rapidly, but also those at which innovations are most likely to take place.

### 19.4.3 External Factors: Interim Conclusion

Whereas internal factors are inherently incapable of addressing the actuation problem, external factors, i.e. factors to do with population composition and distribution, may have the potential to do so. At least, in contact-based or sociolinguistic explanations, we can gain some insight into why a change happened when and where it did.

This is not to say that particular instances of the actuation problem in the historical record are by any means solved. To pinpoint a precise time and location for a particular innovation, we would need access to detailed information about the PLD available to individual acquirers, and the structure of individual discourses – information which in almost all cases will be beyond our grasp due to the nature of the historical record. The situation is better in studies of change in progress, but not much better, as Lass (1980: 94–6) points out. Here at least we have the potential to observe the moment of actuation directly – but we are unlikely to do so except by accident, and even then we would be unlikely to know what we are observing.

All the potential explanation types given in this section also admit further ‘why?’-questions. For instance, with regard to the loss of V2 in English as discussed in §19.4.2: why did the relevant population movements take place during the Middle English period? Why were there weak network ties at just that time and in just that place? So these external factors do not allow us to solve the actuation problem in a completely satisfactory way, even if we had all the relevant historical linguistic data at our disposal. But to the extent that these further questions take us far beyond the domain of language, we are justified in leaving them to our historical and sociological colleagues.

## 19.5 Conclusion: An Unsolved Riddle

I have argued that internal factors – that is, factors relating solely to the knowledge or behaviour of individuals – are unable to account for the time and place of changes, i.e. to solve the actuation problem. An understanding of these factors is nevertheless crucial, as they form part of the *explanans* for any change, even if they cannot form all of it. External (population-based) factors fare better, as questions such as ‘Why did English lose V2 but not German or Dutch?’ can in principle be addressed. However, it is all too easy to invoke contact, weak network ties, imperfect learning, etc. as *post*

*hoc* accounts, but much harder to come up with a truly predictive theory, and many of the pieces of the puzzle are still missing. Furthermore, with regard to particular changes in the historical record, the specific details (e.g. PLD) we would need in order to achieve a confidence approaching certainty are essentially always unavailable.

It seems that the ‘actuation riddle’, if solvable at all, certainly has not yet been solved. Some have argued that the predictive power necessary for a solution to the actuation problem is not a reasonable goal for diachronic linguistics (see McMahon 1994: 44–6; Lightfoot 1999: 253–61; Campbell 2013: 333–5). These authors point to evolutionary biology and its successes as an example of a discipline in which explanation is possible without prediction. The problem is that it then becomes very hard to define what explanation actually is (Lass 1980: 160–9). In any case, in view of some of the proposals mentioned in this chapter, it does not seem necessary to abandon prediction just yet. It is reasonable to assume that a predictive account of language *acquisition* is possible (as discussed in §19.3.2). A predictive account of population dynamics is a much more challenging goal, but some of the results outlined in §19.4 suggest that general, weather-forecast-style predictions and explanations might be possible (see Lightfoot 1999: 267f.). Under this approach, we may not ultimately be successful in pinpointing the solution to the actuation problem for any specific change, but we may be able to narrow down the search space with a reasonable degree of confidence.

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