AGAINST MECHANISMS:
TOWARDS A MINIMAL THEORY OF CHANGE*

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Abstract A widespread view in the literature on language change is that there exist a finite number of *mechanisms* of change to which attested instances of change can be assigned. In this paper I argue against reifying 'mechanisms' as primitives of the theory of language change, on multiple grounds. The mechanisms in question, such as reanalysis and analogy, are commonly invoked in multiply ambiguous ways: cause, process, event, result, and more. This is related to the fact that the ontological status of mechanisms is extremely suspect: where do they reside, and/or what are they properties of? I defend the position that a theory of change should be entirely derivative of i) a theory of language in the individual (cognition, acquisition, and use) and ii) a theory of (human) populations, with at least the latter containing no principles or stipulations specific to language. From this it follows that mechanisms, insofar as they have a role in our diachronic narratives, are epiphenomenal. If so, debates around the status of notions such as reanalysis and grammaticalization may both turn out to hinge on less contentful issues than previously thought.

1 INTRODUCTION

What is reanalysis? The usual definition, from Langacker (1977: 58) and popularized by Harris & Campbell (1995), is well known:

> "change in the structure of an expression or class of expressions
> that does not involve any immediate or intrinsic modification
> of its surface manifestation"

In itself, however, this definition does not tell us what sort of thing reanalysis is. What set does it belong to? (It's neither fish nor fowl, for a start; more generally it doesn't seem to be animal, vegetable, or mineral.) What other members does this set have?

One widespread answer is that reanalysis is a *mechanism* of change. Depending on one's theory, other members of the set of mechanisms may include analogy, extension, borrowing, grammaticalization, pragmaticalization, constructionalization, and a few others. Harris & Campbell (1995), for instance, are among the linguists who explicitly posit the existence of mechanisms of change as part of their explanatory arsenal. These authors propose that the only mechanisms of syntactic change are reanalysis, extension, and borrowing: "no other mechanisms exist" (Harris & Campbell 1995: 50). It's usually stated or assumed that the set is finite and has between one and five members; Bybee (2001: 190) is one of the few linguists who are explicit about the finiteness of the set of mechanisms. Other taxonomies of mechanisms include hypo-, hyper-, crypt-, and metanalysis (Croft 2000), and CHANGE, CHANCE, and CHOICE in sound change (Blevins 2004). Furthermore, a common assumption is that all instances of linguistic change stand in some relation to at least one of these mechanisms: that is, that the mechanisms can exhaustively account for all instances of linguistic change.[1]

Of course, at the risk of sounding like a small child, we can then ask a similar question about mechanisms themselves: what are mechanisms? What sort of thing is a mechanism? It turns out to be much easier to answer this question extensionally (as in the taxonomies in the previous paragraph) than intensionally. But at the same time, whether something is a member of the set of mechanisms is usually assumed to be a contentful question, rather than trivial or innocent. The point of Campbell (2001: 201), for instance, is to argue that "grammaticalization is not a mechanism of change in its own right". Similarly, Walkden (2014: 39) attempts to reduce extension to reanalysis, arguing that this approach "is more parsimonious, as there is no need for a separate process of 'extension'". The implication is that a theory with fewer mechanisms is simpler, and (all else being equal) better. Evidently the granting of mechanism status is not done lightly.

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1 In this paper I focus on syntactic change. Reanalysis is often posited in other domains of linguistic change as well: at least phonology (Langacker 1977: 58–59; Ohala 1981, 1989), morphology (Deutscher 2001), and semantics (Eckardt 2006, 2012). The discussion in this paper is general enough that what I say about syntax should be valid for these domains too.

This paper aims to cast doubt on the notion of mechanisms of change in general, and to suggest that we're better off without them. The focus is on reanalysis, as the most intensively explored mechanism, but the argumentation here applies equally to all other proposed mechanisms in the literature. Section 2 explores the different uses of the term (and concept) reanalysis to be found in historical linguistics, arguing that the situation is confused, and that at least part of this confusion may have to do with the fact that the ontological status of mechanisms is incredibly unclear. This is taken up in Section 3, where it's argued that there is a good reason why mechanisms do not and cannot exist. Section 4 explores the alternative, a theory of change without mechanisms, which lays claim to the adjective 'minimal'. Section 5 then looks at what remains of reanalysis within such a minimal theory. Finally, Section 6 summarizes and concludes.

2 DIFFERENT UNDERSTANDINGS OF REANALYSIS

2.1 Reanalysis itself

Looseness with the term reanalysis has been around since at least Langacker (1977).[2] In referring to "one broad category of syntactic change, specifically reanalysis", Langacker (1977: 57) is clear that reanalysis is a taxonomic *category*, a class to which individual instances of change – change events – can be assigned. We can call this interpretation reanalysis$_1$.

Three pages later in the same paper, however, he uses the term differently. In a discussion of the origin of the Luiseno absolutive suffix *-ta*, he states that "The essence of this change is the reanalysis of a bimorphemic sequence ... as a single morpheme" (Langacker 1977: 60). Here, reanalysis is a *change event*, and we can use it as a count noun with an article, and speak of multiple reanalyses (as indeed Langacker 1977: 61 does). We can call this interpretation reanalysis$_2$. The relation between the two is clearly that reanalysis$_1$ is a class whose individual members are reanalyses$_2$.

In itself this is not a particularly pernicious polysemy: it's of a kind that's found quite frequently. Individual developments can be instances of development, individual changes can be instances of change, individual discussions can be instances of discussion, and so on. If this were the extent of the problem it'd hardly be a problem at all. But these are not the only senses of reanalysis to be found in the literature, or even in Langacker (1977), as the following

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2 Contrary to widespread belief, Langacker (1977) was not the first to introduce the term. Hale (1975) discusses the notion in the context of adjoined relative clauses becoming embedded, though the paper is not about reanalysis as Langacker's is, nor does he provide a definition (Mackie 2012). Cf. also the concept of metanalysis discussed in Hansen (this volume).

quote illustrates.

> "Not all diachronic developments in the domain of syntax involve reanalysis ... but this is clearly a major mechanism of syntactic evolution" (Langacker 1977: 58)

Here, for the first time, reanalysis is granted mechanism status. Is this different from reanalysis$_1$? This depends on what a mechanism is, which – as stated in Section 1 – is not at all clear. My hunch is that most historical linguists would not accept that a mechanism is simply a category of change: it's supposed to be something more, an answer to the 'how' question (if not the 'why' question). If so, then in describing reanalysis as a *mechanism* Langacker is using a different sense of the term, which I will call reanalysis$_3$. This is also the core sense it is used in by Harris & Campbell (1995: 50):

> "there are only three basic mechanisms: reanalysis, extension, and borrowing ... no other mechanisms exist"

Langacker (1977) warns against further extension of the term:

> "For coherent discussion ... we must distinguish between types of reanalysis, the causes of reanalysis, and the effects of reanalysis."

Langacker's paper focuses on all three of these issues, though devotes most space to and is most concrete about the types of reanalysis. Subsequent literature, however, uses the term 'reanalysis' for both cause and effect. For the *cause* usage, which I will call reanalysis$_4$, here are two uncontroversial examples in which the verb 'cause' itself appears:

> "surface changes essentially constitute the actualization of the changes in underlying structure previously caused by reanalysis" (Migge 2003: 110)

> "a further step of reanalysis caused elements to be reanalyzed from lower C heads to higher C heads" Bácskai-Atkári & Dékány (2014: 185)

Finally we have the term being used to denote an effect, *result* or outcome of some change event. This usage can be found, for instance, in Hilpert (2010: 183):

> "Present-day evidence for this reanalysis is that ..."

Strictly speaking, present-day evidence can only provide information about the effects of a reanalysis$_2$. Here, then, Hilpert is talking about the synchronically observable result of some change event. This I will call reanalysis$_5$.

A systematic corpus-linguistic study of these five different uses of the term has not been carried out. From these examples, however, it is clear that reanalysis can be (at least) a **category** of change (reanalysis$_1$), a **change event** (reanalysis$_2$), a **mechanism** (reanalysis$_3$), a **cause** (reanalysis$_4$), and a **result** (reanalysis$_5$). Combining all three uses in the same sentence, it would then not be unreasonable to say that a reanalysis$_2$ belonging to the category of reanalysis$_1$, which happens by means of reanalysis$_3$, can be caused by reanalysis$_5$ and lead to reanalysis$_4$. That is, a change event can belong to a category of change (this is essentially just set membership). The mechanism, reanalysis$_3$, and the cause, reanalysis$_5$, are answers to the 'how' question and the 'why' question with respect to the change event. Finally, the result – reanalysis$_4$ – is the consequence of the change event.

What are we to take away from this? Terminological policing for its own sake is not useful, and my intention here is not to criticize the individual authors cited above: if there's a problem with the use of the term reanalysis, it's a problem with the field of historical linguistics as a whole. And sometimes this polysemy is tolerable and unproblematic. But at other times the distinction is important. For instance, to avoid getting tied up in fruitless debates about causality it's probably useful to distinguish between *mechanisms* and *motivations* of change, in the sense of Traugott (2011). Taking this seriously means, of course, that reanalysis-based and analogy-based explanations are not necessarily mutually exclusive, as Traugott (2011) outlines.

2.2   What is reanalysed?

The differences in usage do not end here. Implicitly or explicitly, reanalysis (at least in the first three senses) needs a theme argument: what is reanalysed? Here, too, various possibilities have been proposed. Lightfoot (1979: 78), in referring to "a major re-analysis of the grammar", suggests that it is *grammars* that are reanalysed. On the other hand, for Lucas & Willis (2012: 461) it is *utterances*:

> "[Non-temporal *never*], we suggest, arose through reanalysis of utterances containing *never* together with a predicate that is potentially ambiguous ..."

Another possibility is that it is *structures* that are reanalysed, as in Fleisher (2006: 233):

> "The inchoative-to-passive change thus involves the reanalysis of structures like (8) as structures like (10)"

See also Denison (2000: 134), who writes of "a reanalysis of *constructions*" (my emphasis).

And here, of course, it should go without saying that what any of these objects are – grammars, utterances, structures, constructions – is dependent on one's theory. Langacker's classic definition of reanalysis (1977: 58) makes reference to the difference between underlying representations and surface manifestations:

> "I will define '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. Reanalysis may lead to changes at the surface level, as we will see, but these surface changes can be viewed as the natural and expected result of functionally prior modifications in rules and underlying representations."

Against the backdrop of 1970s generative grammar, especially the '(Extended) Standard Theory' proposed in Chomsky (1965) and subsequent work with its distinction between deep and surface structure, the notions of rules, underlying representations, and surface manifestations had clear content. By contrast, in the two most widely adopted frameworks for understanding syntactic change – flavours of Construction Grammar and of Minimalist syntax – none of these three notions has any obvious currency. As a non-transformational framework, Construction Grammar has no distinction between underlying and surface structure, and much of the work done by rules in early generative syntax is done by constructions, which can combine relatively freely according to general principles. Similarly, in the transition from the Government & Binding models of syntax to those developed in the context of the Minimalist Program, D-structure and S-structure were both eliminated (see e.g. Chomsky 1995: 186–199), such that there is no level of representation that corresponds straightforwardly with a traditional 'underlying representation'. Cross-linguistic differences – and hence diachronic differences – are captured in the lexicon (see Biberauer & Walkden 2015, Mathieu & Truswell 2017 for recent discussion), with different featural specifications of lexical

items combining again according to general principles such as Merge and Agree.

If these are mere differences of notation, then we can rest easy. But this remains to be shown. I suspect that the question of what is reanalysed – the input to reanalysis – may have as many answers as the question of what reanalysis itself is.

2.3   The scale of reanalysis

Finally, differences arise as to what I will call the 'scale' of reanalysis, if we conceptualize reanalysis as a category (reanalysis$_1$), a change event (reanalysis$_2$), or a mechanism (reanalysis$_3$). Does reanalysis apply at the individual level, or the population level?[3] In other words, when we talk about reanalysis, is the situation more analogous to a single person getting ill, or to the COVID-19 pandemic currently (at the time of writing) affecting large parts of the world? In epidemiology the distinction is clear and crucial, but in historical linguistics the waters are often muddied.[4] This is perhaps not surprising given that diachronic corpus evidence is always biased to varying extents: it only represents the written language of a small (usually non-representative) sample of the population, and errors in transcription, annotation, or in the precision and recall of corpus searches may lead to further divergence from the ground truth. In practice it is hard to date and localize a particular innovation with accuracy.

Some scholars are explicit about whether reanalysis is in principle an individual-level or a population-level phenomenon, and here both stances are found. Detges & Waltereit (2002: 151), for instance, state that "We will show reanalysis to be essentially a hearer-based procedure" – hence an individual-level phenomenon. On the other hand, the following quote from Hilpert (2010: 183) comes down clearly on the side of a population-level phenomenon:

> "the reanalysis ... was a gradual process rather than a sudden, catastrophic one ... Diachronic corpus data indicate that examples ... only gradually increase in frequency over time"

Since the corpus evidence Hilpert is discussing (from Hilpert & Koops 2008) covers seven centuries, an individual-level interpretation is ruled out due to the limited lifespan of humans.

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3 An alternative would be neither: reanalysis applies at the 'language' level. As I discuss in Section 3, however, languages do not exist in the relevant sense, so this can't be the answer.

4 On the issue of the scale of reanalysis see now also Waltereit (2018: section 2), who observes the same ambiguity noted here, and suggests that the scale assumed may partly be an artefact of one's approach to language change.

Reanalysis itself	Object of reanalysis	Scale of reanalysis
category (reanalysis[1])	grammars	individual-level
change event (reanalysis[2])	utterances	population-level
mechanism (reanalysis[3])	structures	
cause (reanalysis[4])	constructions	
result (reanalysis[5])		

Table 1   Understandings of reanalysis

2.4   *Interim summary*

Table 1 gives an overview of the different interpretations of reanalysis outlined in this section (cf. also Winter-Froemel, this volume).

The weakest conclusion that can be drawn from this is that this varied usage may lead to confusion and to talking at cross-purposes. In this paper I explore a stronger stance: the idea that some of the confusion stems from a deeper problem, namely that it is extremely unclear what reanalysis and other mechanisms in historical linguistics actually are, or could be. This is the topic of section 3.

3   The ontological problem of mechanisms

Mechanisms, as already pointed out in section 1, are easier to enumerate than to define intensionally. Assuming that mechanisms are more than just taxonomic categories, we can ask ontological questions: where do mechanisms reside, and what are they properties of?

The argumentation in this section is straightforward: if mechanisms are about the 'how' of language change (like reanalysis$_1$–reanalysis$_3$), they cannot exist. This is because languages (in the relevant sense) do not exist, and something that doesn't exist can't change, hence language change also does not exist. This part of the argument is not new; cf. Coseriu (1985: 149):

> "A language ... does not exist as an object or an organism of nature, and thus it does not have an organic continuity independent of the consciousness of its speakers."

If language change does not exist, then neither do mechanisms of change.

3.1 Languages and species

The rejection of the pretheoretical notion of 'language' as an ontologically meaningful entity is well-established within linguistics. In a prelude to his discussion of E-language and I-language, after alluding to the aphorism that a language is a dialect with an army and a navy, Chomsky (1986: 15) notes:

> "That any coherent account can be given of 'language' in this sense is doubtful; surely, none has been offered or even seriously attempted. Rather, all scientific approaches have simply abandoned these elements of what is called 'language' in common usage."

This is true regardless of whether one takes knowledge or usage to be primary. Isac & Reiss (2008: 71–72), in a textbook treatment, call this pretheoretical conception 'P-language', and note that "typically only a certain variety of philosopher' proposes its existence.[5] Following the consensus, I will assume here that languages in the relevant sense do not exist.

Especially from the perspective of historical linguistics, one might think that this conclusion is too hasty. After all, since the days of Darwin (1859, 1871), Lyell (1863) and Schleicher (1873), languages have often been compared to biological species. The *locus classicus* for this comparison is Darwin (1871):

> "The formation of different languages and of distinct species, and the proofs that both have been developed through a gradual process, are curiously the same." (Darwin 1871: 59)[6]

If the notion of species is robust, and if languages are like species, then perhaps the notion has some role to play in a scientific approach to historical linguistics after all. This millennium the analogy between languages and species has been explicitly drawn in Croft (2000, 2008), Mufwene (2001, 2005, 2008), and Mendívil Giró (2006) (from a biolinguistic perspective) among others, and is implicit in all work on linguistic phylogenetics.

The first problem with this argument is that it is far from clear that the notion of species in biology is in fact robust. Darwin, as is well known, did

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5 Often as a Platonic object, hence outside the usual domain of scientific inquiry. See the essays in Behme & Neef (2018) for views on language from this perspective, and Stainton (2011) for an alternative that attempts to avoid the pitfalls of Platonism.

6 Darwin does not claim originality for this similarity, instead citing Lyell (1863: chapter 23). Moreover, the paragraph needs to be read in its context of nineteenth-century academic debates, on which see in particular Alter (1999). In later editions 'curiously the same' was replaced with 'curiously parallel', which is the more familiar phrasing.

not have a satisfactory theory of speciation Mayr (2004: 106–107): the core contribution of Darwin (1859), despite the title, was to develop and motivate a theory of adaptation and natural selection. The 'species problem' has remained a central problem in evolutionary biology; see Mayr (2004: 171–193), who puts it as follows:

> 'How can we reach meaningful conclusions in this research if one does not know what a species is and, worse, when different authors talk about different phenomena but use for them the same word – species?' Mayr (2004: 171)

In particular, what is needed is an *individuation criterion*: a way of telling whether we are dealing with two species or just one, in any given case.

For biological species, such a criterion exists, based on the *biological species concept* (BSC) of Dobzhansky (1937) and Mayr (1942). This criterion is reproductive isolation: iff members of a population are able to interbreed successfully with one another, in principle or in practice, then they are members of a single species.

The BSC is not unassailable. In particular, ring species – populations connected in the shape of a ring, where members of the population can interbreed with their nearest neighbours but not with others on the other side of the ring (see e.g. Pereira & Wake 2015) – are problematic for the individuation criterion: where does one draw the line in such a case? The extent of the existence of ring species remains disputed. What is important, however, is whether – assuming that the BSC works – there is anything analogous for languages. And here the answer is a clear and resounding 'no'.

The most obvious contender for an individuation criterion, mutual intelligibility, will not cut the mustard. As is well known, intelligibility may be asymmetric: speaker A may understand speaker B, but not vice versa. The ability to interbreed, however, is always symmetric. Croft (2000: 17) proposes potential for communicative interaction as an individuation criterion, but this fails for the same reason and more, since communicative interaction may be asymmetric, and in addition is often non-linguistic.[7]

Another contender, structural (typological) similarity, is even more of a non-starter. A reviewer of an earlier draft of this paper points out that 'there

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7 Croft (2000: 16) invokes 'sibling species' – species that are structurally very similar, but reproductively isolated – in support of his analogy, noting that "sibling languages are two linguistic varieties that are so similar that they are considered to be 'dialects of the same language', yet are perceived by the speakers ... as distinct languages.' Since Croft defines language status in terms of potential for communicative interaction, however, this second analogy – based on speaker perception – is orthogonal to it.

are features that make English English and French French'. The devil, however, is in the detail, specifically edge cases. Should Scots be considered a language, or a dialect of English? Are Serbian and Croatian separate languages? One can always *define* the essential features of a language in such a way that a line can (or cannot) be drawn, but the problem is that any such definition is linguistically arbitrary. There might be good sociopolitical reasons to deem one variety a language and not another, but these edge cases demonstrate that there are never any principled reasons for doing so that derive from the linguistic features of the varieties themselves.

Since all these linguistic individuation criteria fail, the sceptic is led to infer that there is no robust individuation criterion for languages, unlike species, and that as a result the analogy between languages and species is spurious (cf. e.g. Dalby 2002: ch. 2).

One way round this is to give up the idea that languages are like *sexually* reproducing species. Thus, Mufwene (2001: 179) suggests that "languages are analogs of parasitic species"; similarly, Croft (2000: 8) claims that "language 'speciation' is more like plant speciation than animal speciation". On the face of it, this analogy between languages and asexually reproducing species is reasonable. Unfortunately, from a biological perspective it is incoherent. Asexually reproducing organisms are inherently reproductively isolated, and so do not form species (Grant 1981: 64; Hull 1988: 215). As Mayr (2004: 182) puts it, 'the BSC is inapplicable to asexual organisms, which form clones, not populations'. Whether or not it is the case that linguistic evolution behaves like the evolution of asexually reproducing organisms, there is clearly no mileage here in looking for an individuation criterion for languages. The species analogy is a dead end.

3.2   Languages and races

One might pursue a different analogy without leaving the biological domain: perhaps languages are like *races* rather than species. That is, much as we can categorize I-languages as belonging to 'English', 'Mandarin', 'Spanish', 'Swahili', etc., we can categorize humans into 'Asian', 'Black', 'White', etc.

This analogy works well – for the crucial reason that races, like languages, do not exist in any sense corresponding to the pretheoretical one. That is, just as there is no linguistic individuation criterion for languages, there is no biological individuation criterion for races. In the biological domain this point is entirely uncontroversial:

> "Genetics demonstrates that humans cannot be divided into biologically distinct subcategories ... the study of human genetics

challenges the traditional concept of different races of humans as biologically separate and distinct" (American Society of Human Genetics 2018)

"there is no biological basis for races, and there has never been one ... determining which taxonomic difference or genetic differentiation would be sufficient to distinguish races or subspecies is completely arbitrary and thus also makes the concept of races/subspecies in biology purely a construct of the human mind" (Fischer, Hoßfeld, Krause & Richter 2019)

This is not to say that there is no sense in which the terms 'language' and 'race' are useful. Indeed, it seems unlikely that racism can be combated effectively without at least acknowledging the harmful systemic effects that racial categorization has had, and continues to have – and the antiracist enterprise thus relies on acknowledging the existence and relevance of some version of racial categories themselves (Kendi 2019). As the last part of the quote from Fischer et al. (2019) makes clear, however, races are constructs of the human mind, not biological categories. Since the understanding of race is intersubjectively shared to some extent, races are *social* constructs.[8] Precisely the same is true of languages (Chambers & Trudgill 1998):

"Paradoxically enough, a 'language' is not a particularly linguistic notion at all ... it is clear that we consider Norwegian, Swedish, Danish and German to be single languages for reasons that are as much political, geographical, historical, sociological and cultural as linguistic." (Chambers & Trudgill 1998: 3)

Moreover, the constructs of language and race are not independent, and each influences the construction of the other (see the papers in Alim, Rickford & Ball 2016).

In sum, there is no good linguistic individuation criterion for languages, and looking to evolutionary biology does not help.[9] Therefore, the notion of 'language' should play no role in a scientific approach to linguistic change.

3.3   Historical linguistics without language change

What, then, is linguistic change? If the only sense in which languages exist is as social constructs, then this is also the only sense in which they can change.

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8 On the construction of race – "racecraft" – see Fields & Fields (2012).

9 This is recognized in more sophisticated defences of the pretheoretical conception of language, e.g. Stainton (2011: 484–485).

The study of language change then becomes the study of changing linguistic attitudes and ideologies: an admirable pursuit in its own right, to be sure, but not the project that most historical linguists are engaged in (or think they are engaged in). We will therefore put language-as-social-construct to one side for the rest of this paper.

If, say, English doesn't exist in the narrowly linguistic sense and hence can't change, then what do we mean when we say that English underwent the Great Vowel Shift, for instance? We can only mean one thing: that there were speaker-hearer-acquirers at one point in history who had a certain set of vowels in a certain set of words, and speaker-hearer-acquirers with a different set of vowels in that same set of words. The reason we can think of the words as being 'the same' in some sense has to do with the circumstances in which the words made their way from the first set of speaker-hearer-acquirers to the second, through the process of usage and acquisition within a population, but strictly speaking there is no continuity at all. We can, however, study what those speaker-hearer-acquirers were like, how they behaved, and how they related to one another. (Or, in the usual historical setting, we can hypothesize about this with more or less confidence.)

Since it denies the existence of language change in the relevant sense except as an epiphenomenon, this is a *diachronic reductionist* approach to historical linguistics. The next section is about what exactly we can reduce language change to.

4   Towards a minimal theory of change

I take the position that a theory of linguistic change should be entirely derivative of:

  i. A theory of language[10] in the individual
     (cognition, acquisition, usage)

 ii. A theory of variation over time in (human) populations

Point i. is, of course, the aim of most modern theoretical linguistics, though individual traditions take a broader or narrower view of exactly what it is that such a theory is supposed to account for. As for point ii., such a theory would incorporate factors that determine how and when humans interact with each other, including cultural, social and geographical predictors as well as the role of contingency, and there is absolutely no reason to assume that

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10 Here I use the term 'language' exclusively as a mass noun.

anything here is specific to language, as opposed to other kinds of diffusion and communication processes among human populations and perhaps beyond.[11]

Since nothing is changing (other than the individual), the term 'change' is not very apt: change is an epiphenomenon in this approach. 'Cultural evolution' might be a better term (as in e.g. Croft 2000), though here it's important to avoid naïve linguistic Darwinism (see McMahon 1994: chapter 12, Dahl 1999, Itkonen 1999 and Andersen 2006 for cautionary words; Hartmann 2020 provides recent general discussion of the 21st-century rapprochement between historical linguistics and language evolution research). What is of crucial importance is the notion of *population thinking*:

> "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" (Lewens 2007, following Richerson & Boyd 2005)

Another way of putting it is that population-level properties are weakly emergent in the sense of Bedau (1997), deriving from the behaviour of the members of the population (though not necessarily in any directly obvious way). For discussions of the importance of population thinking in the study of language change, see Walkden (2017) and especially Roberts & Sneller (2019).

4.1 Formal or functional?

An ontologically austere position like the minimal theory of change sketched above is a natural consequence of the view that the object of investigation is I-language, as in Chomsky (1986). However, nothing about this diachronic reductionist stance is specific to Chomskyan, 'formal' or generative linguistics. In fact, many linguists have advocated a focus on the individual speaker-hearer-acquirer and a scepticism towards independent diachronic processes of the kind being advocated here. This includes generative linguists such as Lightfoot (1979, 1991, 1999, 2006) and Hale (1998, 2007),[12] but also Coseriu (1958, 1985), Andersen (1973), and Enfield (2003), none of whom are generativists

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11 In order to ward off a possible misunderstanding, let me state here explicitly that I am NOT making the bizarre and untenable claim that linguistic variation has a genetic aetiology. The two components here are the same as the two crucial components of agent-based modelling: "a system of agents and the relationships between them" (Bonabeau 2002: 7280).

12 This focus and scepticism could even be said to be hallmarks of the diachronic generative syntax community, at least in principle: see Whitman, Jonas & Garrett (2012) for discussion, and Crisma & Longobardi (2021) on the history of diachronic generative syntax.

in any useful sense.[13] Croft (2000: 4) also states as a desideratum that 'a theory of language change must avoid the reification or hypostatization of languages'. Further back, these same traits are to be found in the work of William Dwight Whitney, Michel Bréal, and Philipp Wegener (Nerlich 1990), as well as Hermann Paul.[14]

De Smet (2009: 1731), in an important article on reanalysis, is espousing diachronic reductionism when he writes that "a mechanism of change that cannot be straightforwardly linked to the strategies of ordinary language use is automatically suspect". In fact, depending on how narrowly "ordinary language use" is construed – does it, for instance, include cognition, acquisition, perception? – this may be an even more restrictive position than the minimal theory of change.

Generative syntacticians working diachronically have certainly taken more restrictive positions, too. Lightfoot (2002: 127), for instance, claims that "there is no theory of change to be had independent of theories of grammar and acquisition". Similarly, Hale (1998) distinguishes innovation (which he calls 'change') and diffusion, and takes the stance that only the former is a sensible object of study:

> "Diffusion through a population is simply not an I-language phenomenon – 'populations' are irrelevant for those interested in studying the properties of I-language." (Hale 1998: 6)

This stance is compatible with the minimal theory of change, though it is not entailed by it.

Taking the individual seriously – which is at the heart of the minimal theory of change – is not limited to formal or functional perspectives. Inter-individual variation, in particular, has become an important focus of research from a variety of viewpoints in recent years (e.g. Han, Musolino & Lidz 2012, Fonteyn 2017, Petré & Van de Velde 2018). Therefore, whether or not these traditional battlelines are of any use elsewhere, they have no relevance here.

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13 Compare the 'methodological individualism' of Keller (1994: 121) and Enfield (2003: 3). The latter puts it as follows: "the fundamental unit or locus of any social process is the individual, and thus all explanations must be phrased in such terms".

14 Paul is many things to many people, but at times expressed a staunch individualist sentiment. Discussing the concept of *Volksgeist* 'national spirit' in his *Principles of Language History*, he states that "[a]ll mental processes take place in individual minds and nowhere else. ... Therefore, let us get rid of all these abstractions. 'Away with all abstractions' has to be our motto if we want to determine the factors involved in any real event." (Translation taken from Auer & Murray 2015.)

4.2 What's the gain?

Through eschewing mysticism and essentialism, the minimal theory of change presented here should allow us to sharpen our view of what is really going on in linguistic change. Moreover, other than the common-sense conception of language change, which has been shown to be inadequate, there are few well-worked-out theories of what linguistic change actually is. Perhaps the clearest statement of a position diametrically opposed to the minimal theory of change is the diachronic Platonism of Lass (1997).

For Lass, languages are Platonic objects, analogous to a ball rolling across an abstract surface with topographical features (Lass 1997: 294). In a picture like this it is difficult to see any role for individual speaker-hearer-acquirers, and this is a nettle that Lass grasps: "we don't gain anything by invoking them" (Lass 1997: 377 n. 42). More generally:

> "[The fundamental] mistake is considering language change to be something that speakers 'do' (in any real sense, for any reason), rather than something that *happens to their languages*" (Lass 1997: 370; emphasis mine)

Lass is explicitly antireductionist:

> "[T]here are many phenomena in the world that are properties of particular kinds of systems, rather than of the entities that happen to make up the systems. ... The pervasiveness of abstract system types in different domains makes it dangerous to argue ... that a particular sort of propagation ... must necessarily take its origin from the local nature of the population through which it moves, rather than from the nature of a process-type, which may be a piece of 'world-structure' as it were, rather than an attribute of a particular kind of lower-order object in the world." (Lass 1997: 374–375)

He goes on to propose that languages are 'quasi-species' (cf. section 3.1 of this paper).

The most obvious disadvantage to a refusal to invoke any explanatory role for speaker-hearer-acquirers is that it becomes mysterious why events at the individual level – involving a single speaker-hearer-acquirer – seem to be mirrored by events at the population level.[15] For instance, /ki/ is often mis-

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15 This is not to deny the existence of inter-individual variation – far from it. Rather, the point is that any innovation that spreads to a group must have its origin in the usage or grammar of an individual speaker-hearer-acquirer (or more than one).

heard as /tʃi/ at the individual level, and often becomes /tʃi/ at the population level (Ohala 1989). Similarly, morphological regularizations made by child language acquirers are often strikingly similar to the kinds of morphological change we see at the population level, and patterns of inferencing found in communicative interaction are mirrored by pathways of grammaticalization that are attested over historical time Hopper & Traugott (2003). All of these parallelisms must essentially be coincidental if we accept Lass's ontology, since there can be no causal connection in any obvious sense between a 'language' and its speaker-hearer-acquirers. The fact that individuals misperceive /k/ as /tʃ/ before /i/, and the fact that this same change occurs in the history of 'the English language' as well as many others, are simply unconnected events. The nature of languages as Platonic objects under this approach makes any meaningful connection impossible.

The other major advantage of the minimal theory of change is that it enables us to make use of tools drawn from other disciplines. Regardless of how domain-specific our theory of language in the individual is, there is no reason to assume that our theory of variation over time in populations should be domain-specific at all.[16] Rather, we can use off-the-shelf tools from disciplines such as complexity science, evolutionary game theory, sociology, and statistical physics – in particular mechanistic mathematical and computational modelling (in the sense of Baker, Peña, Jayamohan & Jérusalem 2018). The next subsection gives a quick example of the kind of approach that the minimal theory of change encourages us to take.

4.3 Case study: S-curves

One of the most robust empirical findings in studies of language change since the last half of the twentieth century is that changes often follow an S-shaped curve: see Blythe & Croft (2012) for a substantial catalogue of changes that behave in this way. Though not all changes behave in this way (see Denison 2000 and Winter-Froemel 2013 for cautionary notes), enough do that it is reasonable to treat the S-curve as "a kind of template for change", following Chambers (2002: 361).

The S-curve is clearly not a property of individuals. If it is a population-level property, it ought to be reducible to the interaction of those individuals. One can then investigate what assumptions about individuals and the relations between them actually give rise to S-curve behaviour as a weakly emergent property at the population level, and this is the aim of Blythe & Croft (2012). Using an extremely general model, the Utterance Selection Model, Blythe &

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16 See Mendívil Giró (2018) on the notion of domain-specificity, which is not as simple as it seems at first glance.

Croft (2012) attempt to show that some scenarios proposed in the literature for the propagation of variants can be ruled out, as they do not give rise to S-curves under plausible assumptions. Specifically, S-curves do not arise under neutral evolution (where both individual language users and linguistic features are unbiased and all users interact equally frequently) or neutral interactor selection (where both individual language users and linguistic features are unbiased and users may interact with unequal frequency), nor do they arise under weighted interactor selection (where there are biases associated with individual language users, but not with linguistic features) except in particularly unrealistic population structures. Only replicator selection – where biases are directly associated with individual linguistic features – robustly gives rise to S-curves.

In Blythe & Croft (2012), the interacting populations are themselves assumed to be static: language changes while populations remain the same. Kauhanen (2017) develops a model in which individual speaker-hearer-acquirers are embedded within a social network which itself evolves over time. Simulations show that this model is able to yield S-shaped trajectories even when no biases are associated with either language users or linguistic features, though this depends on the structure of the social network.

The models in Blythe & Croft (2012) and Kauhanen (2017) are not the only ways of deriving S-shaped trajectories at the population level, of course: see also Niyogi & Berwick (1997) and Yang (2002) for other ways of doing so. What is important, however, is that S-curve behaviour is derived rather than stipulated, and that it can only be derived under specific assumptions about individual speaker-hearer-acquirers and the way they relate to one another. These assumptions can in turn be evaluated against empirical evidence.

5 RECONSTRUCTING REANALYSIS

Back to reanalysis, finally. Recall that the minimal theory of change consists of i. a theory of language in the individual, and ii. a domain-general theory of variation over time in (human) populations. Reanalysis, as a mechanism, doesn't fit neatly into either theory. It is obviously not part of a domain-general theory of variation over time, and it seems to have no place in a theory of language in the individual either. As De Smet (2009: 1731) puts it:

> "Reanalysis ... appears to show no direct correspondence to a principle of synchronic grammatical organisation, [and] it enjoys no privileged status in synchronic model-building"

So can reanalysis be salvaged? I think the answer is yes, but as a purely

descriptive, post hoc label for a (type of) event at the individual level: when "the hearer assigns a parse to the input that does not match the structure assigned by the speaker" (Walkden 2014: 39). In the minimal theory of change it plays no explanatory role.

To see this, consider for instance the model of syntactic acquisition presented in Yang (2002), the Variational Learner. Here the learner comes equipped with various possible grammars and must assess which one provides the best fit for the data they encounter. It does so by associating probabilities with particular grammars (or particular parameter settings). When a sentence is heard, the learner picks a grammar probabilistically to analyse that sentence. Probabilities are updated via reward-penalty learning: if the grammar is successful in analysing the sentence, the probability of that grammar is boosted, otherwise it is penalized. This model – (part of) i. a theory of language in the individual – has been extended to situations of language change by Yang (2002) himself and others (see Kauhanen & Walkden 2018 and references cited there), and derives S-curve trajectories, among other things. As regards ii. the theory of variation over time, most of this work assumes a very simplistic population-level scenario in which populations are well-mixing and have essentially perfect access to the output of the grammar of the previous generation; these assumptions are obviously not true in general, but making them can help us identify what their consequences (predictions) are, and relaxing them under certain conditions may help us to explain more complex situations.

Reanalysis is something that happens in this model, to be sure. However, it is in no sense a primitive. We can use the term 'reanalysis' to describe the situation in which a hearer successfully analyses an incoming sentence using a grammar different from the one that the speaker used to generate it. Thus, reanalysis is purely epiphenomenal. A full specification of how parsing works, like this one, obviates the need for reanalysis as a core theoretical notion. A model like this has other advantages, too: in particular, it specifies under what conditions reanalysis is more or less likely to happen. The explanatory power here is part of the model, with reanalysis events as explanandum rather than explanans.

Returning to the typology of uses of the term 'reanalysis' in section 2, repeated in table 2, what we are dealing with here is reanalysis[2]: a change event, that takes place at the individual level. As argued in section 3, mechanisms aren't real, so reanalysis[3] is ruled out. If we wanted to, we could still use the term to refer to a category (of change events), as in reanalysis[1], or the cause or result of such events (reanalysis[4] or reanalysis[5]), though for the sake of disambiguation it might be better not to. The same goes for population-level

Reanalysis itself	Object of reanalysis	Scale of reanalysis
category (reanalysis[1])	grammars	individual-level
change event (reanalysis[2])	utterances	population-level
mechanism (reanalysis[3])	structures	
cause (reanalysis[4])	constructions	
result (reanalysis[5])		

Table 2   Understandings of reanalysis

interpretations of reanalysis: most agree that it is useful to distinguish between innovations on an individual level and the spread of those innovations at the population level.[17] As a consequence, I would suggest that we avoid using the term in the latter sense (particularly since there is no reason to believe that the spread of innovations that have arisen through reanalysis behaves any differently than the spread of other innovations).

Finally, as regards the object of reanalysis, this will to a large extent be dependent on one's theory of language in the individual, and it is not appropriate to be prescriptive here – especially since different objects of reanalysis may turn out to be one and the same way of looking at the same change event. From a Construction Grammar perspective, for instance, Traugott & Trousdale (2013: 16–17) talk about 'constructs', which are utterance tokens that instantiate constructions. This relation of instantiation means that although it is the construct/utterance that is locally involved in any change event at the individual level, it is also simultaneously reasonable to speak of the construction being the object of the change event. And if utterances instantiate grammars, then it is not unreasonable to speak of either an utterance or a grammar being the object of a change event. The object of reanalysis is thus left open.

The point of this section is not to argue that the Variational Learner of Yang (2002) is the correct way of thinking about syntactic acquisition and change – it almost certainly isn't. But on a broader level it represents, I would argue, the best way of thinking about diachronic mechanisms: namely, by reducing them to things that actually exist.

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17 This is what is intended in Hale's (1998) distinction between 'change' (individual innovation) and 'diffusion', in Croft's (2000: 4–5) distinction between 'innovation' and 'propagation', and in Traugott & Trousdale's (2013: 2) distinction between 'innovation' and 'change' (diffusion).

6   CONCLUSION

In this paper I have argued that reanalysis as a mechanism of language change, and all other mechanisms, should be eliminated. Mechanisms, insofar as they have a role in our diachronic narratives at all, are epiphenomenal: they're after-the-fact descriptions of particular events, or types of events.

In the first part of the paper, I have argued that the term 'reanalysis' is used in a plethora of ways (section 2), and that this may be due to the fact that mechanisms of change are otiose and ontologically weird (section 3). The second part of the paper outlines a diachronically reductionist 'minimal theory of change' according to which population-level events and properties should be derived from a specification of the properties of individuals and the ways in which they relate and interact with one another (section 4) – really a blueprint for theory construction rather than a predictive theory in its own right. Although we can reconstruct reanalysis as a (type of) change event occurring at the individual level (section 5), it's questionable how much that helps us in understanding linguistic change.

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