Are there impossible changes?
θ > f but f ≯ θ

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Abstract

One question that historical phonology should reasonably seek to answer is: *are there impossible changes*? That is: are there plausible changes that we could reasonably expect to occur in the diachrony of languages' phonologies, but which nonetheless do not ever occur? In this paper I seek to spell out what it really means to consider this question and what we need to do in order to answer it for any specific case. This will require a consideration of some fundamental issues in historical phonology, including the distinction between exceptionless and lexically-specific/sporadic changes (which I call 'N-changes' and 'A-changes'), and the connection between that distinction and the 'misperception' model of phonological change. It will involve an analysis of aspects of the phonological history of Pulo Annian, Arabic, Italic, Spanish and several varieties of English. I argue that the current state of evidence indicates that there *are* indeed impossible changes (which I symbolise using 'x ≯ y' to represent that 'x cannot change into y') in a very specific but phonologically real way, and that f ≯ θ *is* one.

1   Introduction

Can any imaginable change occur? This is an important question for historical phonology[1] — if we know that certain changes are impossible, we can confidently weigh up competing phonological reconstructions, we can build phonological theories on a firm basis, and we can establish a set of discoveries that an autonomous discipline of historical phonology can claim as its own. But how can we *know* that a change is impossible? This requires a careful peeling away of distractions in order to define what we mean by 'a change', and then a

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[1] It is only *one* of the many issues that historical phonology should consider, of course — I fully subscribe to the broad range of questions for historical phonology set out in Honeybone, Bradfield, Fruehwald, Iosad, Molineaux & Ramsammy (2016).

cautious interrogation of the evidence. This paper is, therefore, at least in part about the set of factors that we need to consider when we do this. (I restrict all discussion here to the segmental realm but the same issues surely arise in prosodic diachronic phonology.) I argue that we *should* try to answer the question in the paper's title and that we can, in fact, answer it in the affirmative. This means that historical phonology needs the symbol '⇸', with the meaning "the change from one specific type of phonological structure into another specific type of structure cannot occur".

There has been a lot of discussion elsewhere of many of the issues that are considered here, and it would be absurd to try to discuss all relevant previous work. Much of what I discuss here could be seen as 'historical phonological common sense' but I am not sure that it is always recognised as such as fully as it should be (and, in any case, 'common sense' in a dubious notion). I have not seen it all previously brought together and clearly set out in the way that I try to do here.

I touch on a few notable aspects of the historical context to my discussion in section 2 and then go on, in section 3, to set out my case. In order to makes things concrete, I then consider one specific type of change in section 4: the diachronic relationships that are possible between f and θ — some of the attested changes discussed there will require detailed study. Section 5 reconsiders a fundamental matter which is raised in section 3, and which section 4 will show to be crucial: the distinction between exceptionless and sporadic changes, and the link (or lack of it) to misperception. Section 6 concludes.

2   Previous thoughts on impossible changes

The idea that certain phonological changes are expectable and others are not has been discussed since the earliest days of systematic historical phonology. For example, Bredsdorff (1821) wrote that:

> When consonants are pronounced with less effort or more weakly, they commonly change into other consonants, usually as follows:

$$p \text{———} b \left.\begin{array}{c} \\ \\ \end{array}\right\} \text{———} v \text{———} \underset{\circ}{u}$$
$$f$$

This hints in the direction that we are headed: certain types of change are common, while their inverse is not. As Andersen (1982, 21) explains, Bredsdorff's trajectory (from left to right) presents an early understanding of "the typical results of the universal tendency to consonant weakening" in unidirectional diachronic change (see

Honeybone 2008 for a general history of the understanding of such changes).

Murray (2015, 23) shows that Raumer (1837) was also an early thinker in this regard, because he:

> ...argues already in 1837 that one of the best ways of reconstructing phonetic content is to consider the changes the sound undergoes, under the assumption of expected pathways of change; for example, $t > d > ð$.[2]

If there are 'expected pathways of change', then there are also unexpected pathways of change, of course. Ideas like this are now commonplace, and feature in textbooks on historical linguistics. For example, Trask (1996, 53–56) discusses "the commonest types of change", saying that "lenition processes are pervasive" and explaining lenition on the basis of a set of scales rather like those given by Bredsdorff, including those in (1).

(1) geminate > simplex
     stop > fricative > approximant
     stop > liquid

'Unexpected' is not the same as 'impossible', however. To argue that certain types of change are common, and others are just 'uncommon' or 'unexpected' does not make a strong claim. It is only if we hypothesise that certain types of diachronic event are *impossible* that we have something to test, and — if the claim survives the testing — that we can claim to have firm knowledge. One good counterexample is enough to disprove a hypothesis of this type, and this is not the case for claims that certain things are only *unlikely* (this is a well-known aspect of the Popperian scientific method).

If we can establish such knowledge about what is possible in phonological change on the basis of serious testing, then we can seek to explain it on the basis of a theoretical model and we can feel confident to use such knowledge to rule out candidate reconstructions of past synchronic phonological states. While I claim in section 4 to establish a firm basis for one item of such knowledge, I do not move towards theoretical explanation in this paper (although I do consider some pointers in that direction). The impossibility of a particular change could in principle be due to constraints on what is a possible synchronic phonological process, or the comparative markedness of the segments involved, or due to there being no possible diachronic scenario through

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[2] This is Murray's interpretation of Raumer, who does not use the '>' symbol himself.

which such a change could be innovated. That can be considered in future work. Here I simply argue that we should agree with Weinreich, Labov & Herzog (1968, 183) when they say that part of the point of historical linguistics is to address the 'constraints problem' in understanding linguistic change, and that there are ways to do it:

> *The Constraints Problem*. [...] one possible goal of a theory of change is to determine the set of possible changes and possible conditions for change

The idea that there might be constraints on what are possible and impossible changes can be seen as part of diachronic typology, which minimally focuses on "what is thought to be diachronically common" but can go further, so that it "aims at finding linguistic patterns that result from general or even universal factors" (Kümmel 2015, 121–122). Certain worries are sometimes raised in connection with typological claims, as for example Lass (1997, 29) discusses. Lass mentions the observation that "to the best of our knowledge no living language has only rounded vowels" — a typological observation which might be used to place a constraint on both (i) the types of vowel systems that we should be allowed to be reconstruct for past stages of languages and (ii) the kinds of changes that we should think are possible. He writes further:

> Now one possible objection [...] must be taken account of. It goes like this: how good really is 'the best of our knowledge'? Surely nobody has examined all the languages spoken in the world at present (not to mention all the past ones, including those that vanished without trace, and all future ones). Therefore the argument is not based on knowledge at all, but on ignorance; it's only a failure to recognize the obvious (even necessary) limitations of our knowledge of the 'total set of human languages' that allows this smugness.
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> The counterargument is essentially philosophical. All human knowledge is flawed, provisional and corrigible, which is what makes scholarship of any kind worth doing. If somebody reliable found a living language with only rounded vowels next Thursday, I would cheerfully admit it to the canon, and weaken [...] my rejection. Since exhaustive knowledge of any interesting domain is impossible, we can't be faulted for making do with what is, after all, the only knowledge available. [...] With the added proviso (which goes for all scholars) that 'the best of our knowledge' usually means 'the best of my knowledge': in the hopes that I've managed to read widely enough so that the two will (largely anyhow) coincide.

Lass sets out the notion that typological generalisations are falsifiable hypotheses to be tested and argues that it is fair to rely on 'the best of any individual's knowledge' when working them out, setting out a reasonable basis for taking typological generalisations seriously. We can, however, do better than simply relying on any one individual's knowledge: in order to discover whether a hypothesis holds up that a

particular type of imaginable change is impossible, we could positively go out of our way to test it. The two methods in (2) seem reasonable ways to do this:

(2)   (i)   survey a large sample of language histories to see if any examples of a candidate impossible change are attested
      (ii) conduct a survey of experts in the history of all languages, asking if anyone knows of any example of a candidate impossible change

If neither of these turn up any examples of the candidate impossible change, we will have gone far beyond the best of any one individual's knowledge, to reach a truly solid basis for the statement that a specific type of change (which is feasible and imaginable as an event in diachrony) nevertheless never occurs in the history of languages.

2.1   There are *changes* and CHANGES, and we need '>' and '⇸'

It is often not made explicit that the symbol '>' is ambiguous. It is typically described as a central tool of historical phonological description, to represent that one phonological state in the history of a language turned into another. For example, Minkova (2014, 275) writes that in "Late Middle English [-ɛr] > [-ar]", as in *star* and *farm*. Here '>' means 'did change into'. However, the shaftless arrow means something rather different when Cser (2015, 194), discussing the notion of 'conditioned change', writes that "[k] > [tʃ] / __ V[–back], as in Late Latin or several other languages." Here '>' means 'can change into'.

The idea that the same change occurred in Late Latin and several other languages leads us to the idea that there are CHANGES and *changes*, which I distinguish using these two types of typography. That is, there is a set of possible CHANGES (general types of change; 'categories') which can be instantiated as *changes* that occur in the history of languages (diachronic events with a time and space; 'realisations of those categories'). In this way, the affrico-palatalisation of velars driven by the frontness of adjacent segments (the example mentioned by Cser) is one thing that languages can do diachronically (a CHANGE). Late Latin has done it, as have many other languages, including Proto-Slavic (Shevelov 1964), early Old English (Minkova 2014 §4.3) and dialects of Greek (Manolessou & Pantelidis 2013, Lengeris & Kappa, to appear). These are four examples of *changes* which all instantiate the same

CHANGE.[3] This spells out the ambiguity of the '>' symbol: it is used to describe both *changes* and CHANGES.

It is in the latter kind of sense that we need the symbol '≯'. The question *are there impossible changes?* makes sense if there is a finite set of CHANGES which can be set out in as in (1), and which we can hope to delimit. Trask (1996) is describing CHANGES in the passage cited above, as is Bredsdorff (1821). Non-existent CHANGES can be written with '≯', with the symbol meaning 'cannot change into'. For example, Fox (2015, 61), while discussing methodology in comparative reconstruction, considers a case where two possible scenarios need to be weighed up against each other, writing that:

> A solution which requires a change k > s, for example, is unexceptionable, since such a change is widely attested, whereas one which requires s > k is highly suspect.

If we abandon Fox's caution (which might be reasonable if we have investigated the case and assured ourselves that we have a truly solid basis to make such claims), we could reformulate his words as follows:

> A solution which requires a *change* k > s, for example, is unexceptionable, since this CHANGE is widely instantiated, whereas one which requires s > k is rejectable because s ≯ k.

So: *are* there impossible CHANGES? This is an easy question to ask, but a difficult one to answer. What kinds of things do we need to bear in mind as we investigate a candidate impossible CHANGE to assure ourselves that it really is impossible? How exactly can we implement the methods set out in (2)? Section 3 considers the first of these questions, and section 4 considers the second. While I have just spent a section introducing a typographical distinction between *changes* and CHANGES, I do not always implement it below because, as long as we are aware of the conceptual ambiguity of the term 'change', it should be clear what is meant.

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[3] As implication of this is that the four diachronic events mentioned here are in some real sense four cases of literally *the same thing*, in the same way that monozygotic quadruplets are in some sense the same. In most useful senses quadruplets are obviously *not* the same, and they occur in different places and have different personalities, but they are also equally obviously four instantiations of the same genetic combination.

3   What do we need to bear in mind when we consider whether there are impossible changes?

My central question is this: are there plausible changes that we could reasonably expect to occur in the diachrony of languages' phonologies, but which nonetheless do not ever occur? One thing that we need to know in order to answer it is what 'plausible' means. I address that point in section 3.1. Elsewhere in this section, I consider a number of other issues that may seem quite obvious, but are nonetheless not always brought together in discussions of these things.

We first need to establish which candidate changes it is appropriate to consider (those which are plausible but not well attested) and then we need to set about testing whether or not they have actually ever occurred in the history of any language. While candidate impossible changes are empirical hypotheses which can be disproven by one counterexample, it is important to be sure that those counterexamples are *true* counterexamples. There can be diachronic events in the history of languages which are *not* changes in the same sense as the kinds of things that we need to consider. In order to work out if there are indeed impossible change, we need to be sure that we are comparing like with like and that we are only considering relevant data. In order to explain what I mean, I need to reflect on some of the basics of historical phonology.

For example, if we are considering an unconditioned ('spontaneous', 'isolative', 'context-free') CHANGE, which affects every occurrence of a segment (or other phonological structure) in all of the environments in which it occurs — which could arise from latent possibilities within a segment or system in itself — we should be sure to consider only other unconditioned changes for the purposes of comparison and contrast (and when we are considering conditioned changes, which are driven by some aspect of the environment in which a segment occurs, we should only consider similarly conditioned changes). It is possible for one diachronic correspondence between segments to have different types of causes, so we need to be wary of this. For example, in the 'Northern Fronting' of Middle English, oː > øː occurred spontaneously, as an unconditioned change unconnected to the segment's environment — this has led to various front reflexes (after further subsequent changes) in contemporary Northern English and Scots traditional dialects, with the original ø-type output retained (in words like *boot* and *root*) in several present-day Roxburghshire and Dumfrieshire traditional dialects (see, for example, Jordan 1974 and Mather, Speitel & Leslie 1986). That change was a fundamentally different kind of thing to the conditioned oː > øː that was part of pre-Old English *i*-umlaut, due to the imposition of a feature specification on oː

from without, involving harmony triggered by a following palatal vocoid, and leaving such forms as *foet* føːt 'feet' < Proto-West-Germanic foːtiz, and *groene* grøːnə 'green' < Proto-West-Germanic groːniː in Northumbrian Old English (Ringe & Taylor 2014, 227). In section 4, I focus on an unconditioned change, and so I need to set aside changes which may *look* like the change that we are considering because they involve the right input and output, but which are actually irrelevant because they were conditioned.

In the remainder of this section, I address a number of similar points which need to be considered when we investigate whether or not there are plausible phonological changes which do not ever occur.

3.1   We need to know what might be a plausible change

It is not interesting to make a claim like t͡ʃ ⇸ œ. A change like that would involve a vast number of alterations in features at once, and that fact by itself can legitimately be assumed to make such a thing impossible. Because of this, while it is trivially true that t͡ʃ ⇸ œ, it is not an 'impossible change' in a theoretically interesting way because it is not a plausible change. This leaves a problem: if something is indeed impossible, how can we know that it is a thing? How can we decide what the candidate plausible impossible changes are?

One sensible approach to this question is to consider the directionality of diachronic correspondences: if x > y is firmly attested in the history of languages, we can reasonably assume that y > x should also be possible — unless, that is, something (interesting) prevents it. If x > y is found in a range of languages, it is likely that the gap between x and y is not great, and is bridgeable in one change, such that if x can turn into y, y should be able to turn into x. Thus, for every well-established change, its inverse is a candidate plausible impossible change. Another way of putting this is that the central question at issue here could (at least in part) be rephrased as: *are any phonological changes unidirectional*?

One aspect of this is that we should consider the bridgeability of a diachronic gap in terms of the number of changes involved. We should only consider candidate impossible changes that involve one quantum — a term that Lass (1997) uses for the unit of segmental phonological change. It is possible to imagine a number of diachronic stages which would actually allow for a diachronic correspondence of the type t͡ʃ > œ in the history of a language, but that would involve a long time-depth and a large number of separate quanta. We are not interested here in whether there are constraints on how changes can be telescoped — there may be, but that takes us beyond the kind of change that we are

really considering. Telescoping can produce processes which are not innovatable in one quantum, and which we should put aside both when we consider what are candidate impossible changes and when we consider whether the historical record actually shows examples of them occurring. Hualde (2011) gives an example of this from Ondarroa Basque, shown in in the data in (3). In that language, the normal way to form the absolute singular is to add -a, as in *gixona* and *sagarra*. However, absolutives like *neski*, whose uninflected forms end in -a, seem to involve something like aa → i / _#,[4] and a naïve consideration of the case might assume that this is due to a diachronic development of the type aa > i /_#.

(3) uninflected   absolutive singular
     gixon   gixona   'man/the man'
     sagar   sagarra   'apple/the apple'
     neska   neski   'girl/the girl'

There was no such change, however, as Hualde shows. The aa → i in *neski* is in fact due to the telescoping of four changes. There was a stage at which the absolutive was *neskaa*, but a series of changes gave *neskea > neskia > neskie > neski*. As evidence for this, Hualde (2011, 2217) writes that "[a]ll the intermediate forms are attested in other Basque dialects." Unsurprisingly, aa > i / _# is not a possible change, and if we bear in mind the criterion of monoquantality, we can recognise that this kind of situation is not relevant to our search for changes that weigh on our question.

3.2   We need to distinguish between 'N-changes' and 'A-changes'

When searching the record of attested changes in order to discover what it possible, it is important to distinguish between two distinct fundamental types of change (both of which can be monoquantal). The basic distinction in question has long been recognised and has been discussed under a number of terminological traditions. I describe it here as a difference between 'N-changes' and 'A-changes', prompted by the italicised words in (4).

(4) N-changes = those which the *neogrammarians* called 'sound
                   change'; that is, those which are often seen as
                   '*natural*' changes, with exceptionless patterning

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[4] Hualde (1991) actually uses a (small) number of synchronic rules to account for this.

A-changes = those which are due to *analogy* or to a (re)*analysis* involving underlying forms, which have lexically-specific patterning[5]

The type of distinction set out here was first established most firmly by the neogrammarians, with Osthoff & Brugmann (1878) famously setting out fundamental assumptions about exceptionlessness and analogy, for example. I return to the issue of exceptionlessness later in this section and (because it will be crucial in the argumentation in section 4) also at some length in section 5, where it is reinforced and linked to the manner of implementation of a change.

The two basic types of change in (4) have been recognised and rerecognised many times since Osthoff & Brugmann, with a number of developments and disagreements concerning their precise characteristics. They are still widely assumed to be distinct types of thing, so we should not expect them to pattern similarly, and we should not mix them up if we are investigating what is possible in change. If we are really asking 'are there impossible N-changes?' (which is what I have tacitly been doing up till now), then we need to exclude A-changes as potential evidence, because A-changes, just like telescoping (as discussed in the last section), can lead to diachronic correspondences which are unknown from N-changes.

For example, intervocalic sibilant rhotacism (of the type z > r / V_V) has been claimed to be an N-change in a number of languages, such as North and West Germanic (leaving remnants in alternations of the type *was~were*) and Latin (giving alternations of the type *flos~floris* 'flower NOM~GEN'). If we want to know if such a change is unidirectional, we should not be distracted, for example, by the change that has occurred from Old English *coren* to Modern English *chosen*, which looks like it is a case of r > z / V_V. This diachronic correspondence of r and z is not due to an N-change at all. It is due to an (inherently lexical) analogical levelling on the model of the (non-initial) consonant in the base form *choose* — and this analogy was, of course, an A-change. Similarly, consonant loss, of the type n > Ø, is well attested as an N-change, and its opposite (spontaneous, arbitrary epenthesis) is a good candidate for an impossible change. The appearance of initial n in the change from Middle English *ewt* to *newt* and *ekename* to *nickname* might at first sight seem to be due to a change of the type Ø > n, and if it were, that would show that this type of change is possible after all. But

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[5] The 'A' could also stand for *alles andere* — 'everything other' than N-changes. If so, then the 'N' could stand for 'normal' — that is, the type of change that phonologists and phoneticians normally consider when they discuss historical phonology.

we know that this was not the case. This change is taken (see, for example, Fertig 2013, 34) to be due to a reanalysis in which the final n in a determiner preceding such vowel-initial nouns (such as *an* in the common collocation *an-ewt*) was (mis-/re-)analysed by learners as being initial in the base (giving *a-newt*) — this was possible because *a* was also a potential form of the determiner. Again, this is an inherently lexical A-change (and occurred in only a few vowel-initial words).

Changes of the types discussed in section 2 are all N-changes, and that focus remains the primary one of this paper (although the existence of A-changes will also be important). This means that I am primarily investigating whether particular N-changes are possible, and so need to be sure not to be confused by A-changes — they are subject to their own set of constraints (equally worthy of separate study). This will become quite crucial later in the paper, and so it will be vital that we can reliably distinguish between N-changes and A-changes. A central criterion here is the degree to which a change has affected a language's lexicon: the exceptionlessness issue. This has been contentious in historical phonological debate, as is well known (see, for example, Wang 1969, Labov 1981, Phillips 2015), and the details cannot all be considered here.[6] I assume, following Labov (2006), Bermúdez-Otero (2015) and Kiparsky (2016), that it has been shown beyond doubt through the investigation of change in progress that neogrammarian-type exceptionless change does occur, and that N-changes can thus indeed be expected to show this kind of regularity because they involve lexicon-independent phonological structures (such as segments). I set out a basis for this, with reference to the precise types of changes considered in this paper, in section 5.

3.3   We need to distinguish between endogeny and exogeny

One final distinction that we need to bear in mind is the difference between changes with endogenous ('internal') causes and those with exogenous ('external') causes. There are surely constraints on how contact can affect phonological systems, but there is no reason to expect that they are the same as those that determine how endogenous changes can pattern. Endogenous change is thought to be due to such things as system-internal pressures (as considered by Martinet 1955, for example), the realisation of pathways allowed by constraints on

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[6] This assumption about exceptionlessness is commonly claimed (e.g. Foley 1986, 207) to be of fundamental importance in historical phonology because "[i]t is the regularity of sound correspondences which provides the comparative method with its rigour."

phonological representations (as considered by Anderson & Jones 1977, for example) and the phonologisation of phonetic biases (as considered by Garrett & Johnson 2013, for example). Exogenous change is thought to be due to such things as cross-language or cross-dialect borrowing in bilinguals, whole-scale language shift, dialect levelling, second language acquisition effects and new-dialect formation, all of which can provide new phonological forms which learners (or other speakers) can adopt. The default when discussing the patterning of phonological change is to focus on endogenous change (as in textbooks such as Trask's 1996 chapters on 'Phonological Change' and Hock & Joseph's 1996 chapter on 'Sound Change'), and I reflect that bias here. My question is thus really: are there changes which are impossible to innovate endogenously?

It does seem that exogeny can do things that endogeny cannot. For example, the *Survey of English Dialects*, conducted between 1948 and 1961 (see Orton *et al.* 1962–71), records that traditional dialect speakers in West Yorkshire at survey localities like Wibsey (*SED* locality number Y22) and Golcar (Y29) produced absolutely no occurrences of h in any of the 104 words which can feature h in other varieties of English. The *SED* fieldworkers thus recorded transcriptions like [ɔt] for *hot*, [ʊŋəɹɪ] for *hungry*, and [ɛdʒɔg] for *hedgehog* at those localities, for example. This is clearly due to a total absence of h in these varieties at that time that the *SED* was conducted. The presence of h in such words is well attested throughout Old English, however, so we know that it was there at an earlier stage, and the state recorded in the *SED* is straightforwardly ascribable to an endogenous change of the type h > Ø (see, for example, Minkova 2014, 101). The total loss of a segment (such as h > Ø) is unexceptional, but its opposite — a segment arising spontaneously and arbitrarily from nowhere — would be quite surprising and is a fair candidate for the status of impossible change in the sense developed here. Can Ø > h occur, with an h widely epenthesised from nowhere? If we do not discount exogenous changes, then we might think that it can. For example, Petyt (1985) investigated 106 speakers from across the social scale in exactly the same areas just mentioned, including Bradford (of which Wibsey is a part) and Huddersfield (which is a couple of miles from Golcar) and found many occurrences of h, to the extent that every speaker produced *some* occurrences of h (for example in a minimal pair list, featuring pairs like *otter* and *hotter*) and most age groups had around 50% occurrence of h, in exactly those environments where other varieties can have h. There seems to have been a change Ø > h in the few decades between the *SED*'s fieldwork and Petyt's fieldwork. Of course, however, this is not an endogenous change — it has exogenous causation, as it is due to dialect levelling in the direction of varieties of

English which did not ever lose h, such as RP. Ø > h remains a candidate impossible (endogenous) change in the light of this data.

The motivations for endogenous and exogenous changes are ontologically different things, so it is perfectly likely that they are able to do different things. It is in principle equally interesting to investigate what constraints exist on exogenous change, but I set this aside here because the changes that I consider in section 4 show every sign of being endogenous.

While the distinction seems obvious in the case just mentioned, it is not always so straightforward. One aspect of Blevins (2006) shows how careful we need to be in order to avoid mixing the two types of change discussed in this section. That article connects closely with the kinds of things under discussion here: it argues for a distinction between 'natural' *vs* 'unnatural' changes, which equates to 'possible' *vs* 'impossible' in the terms used here (assuming that 'unnatural' changes cannot be innovated endogenously through N-changes).[7] I am agreeing with Blevins in this section — she writes that "if we are interested in discovering the [...] origins of a particular sound change, we must filter out contact-induced change" (2006, 9). Blevins considers in that article some "sound changes with clear phonetic bases [which] are recurrent in the history of Modern English" (2006, 10), two of which are 'dental fricative stopping' (e.g. θ > t) and 'dental fricative fronting' (e.g. θ > f). She brings together a number of such changes, as reproduced here in table 1.

Dialect/Variety	Sound Change	Complete?
Shetland	ð > d, θ > t	yes
West Ireland	ð > d, θ > t	yes
Southeastern England	ð > d/#_, θ > f ð > v elsewhere	yes
Newfoundland	ð > dð, d, θ > tθ, t	yes
Maori English	ð > dð, θ > tθ	variable
Gullah	ð > d, θ > s, t	yes
Fiji English	ð > d, θ > t	yes
New Zealand, Australia	ð > v, θ > f	variable

Table 1: examples of the loss of dental fricatives
in Modern varieties of English from Blevins (2006, 11)

[7] More generally, Blevins has done important work on the diachronic typology of phonological change (eg, 2004, 2015), which has thoughtfully pushed along our conceptual space in this area.

Dental fricative fronting is the change that I consider in detail in section 4, so I focus here on dental fricative stopping. The data in table 1 imply that θ > t is a common type of endogenous N-change, but my point here is that this may be overhasty. There are reasons to believe that at least some of the cases grouped together in table 1 are exogenously-driven.

To consider the first change listed: there have been several waves of language and dialect contact on Shetland, in part because the North Germanic language Norn was the normal language of Shetland for centuries and in part because of the contact that occurred on Shetland between the dialects of Scots that gradually replaced Norn. As Maguire (2012, 64) explains, Norn

> ...survived at least until the eighteenth century, so there was contact between Norn and Insular Scots in the Northern Isles for up to 300 years. As a result, various features of the divergent Insular Scots dialects have been attributed to Norn influence, including ... *th*-stopping ([t] and [d] for historical /θ/ and /ð/), assumed to be the result of Norn lacking dental fricatives.

Knooihuizen (2009) argues that simple take-over from Norn does not explain the whole story for Shetland stopping, but a contact account for a change does not require an absolute and overwhelming causative explanation — it only requires an exogenous source for the variant that becomes embedded in the system (in this case, stops as possible realisations of what were fricatives in the varieties of Scots that were brought to Shetland), and this is clearly plausible here. Knooihuizen (2009) argues (in part following Millar 2008) that Shetland Scots was formed through a process of new-dialect formation, that a Norn-influenced L2 variety was one of the dialects involved, and that this accounts for the 'dental fricative stopping' found in Shetland.

The second case in table 1 — Irish English — is also dubious as an endogenous change (and Irish English had a major role in the development of Newfoundland English, which is also in table 1, as among others Clarke 2010 explains). Hickey (1998, 226) writes that:

> ...the dental fricatives of British English correspond in the main to dental stops in Irish English. There is general consensus in the relevant literature on why this is the case. The standard wisdom on the subject runs as follows. The Irish who were first confronted with English at the beginning of the early Modern English period ... used for the fricatives of English the nearest equivalents which they had in Irish. These were the dental allophones on the non-palatal stops of Irish."

It is important to bring together *changes*, as in table 1, in order to establish the set of possible endogenous CHANGES, but it is also

important to be sure that they represent the same kind of thing. It may be that some of the cases of dental fricative stopping in table 1 are endogenously driven, but it is likely that some are not (and it may even be that none are). Contact explanations for change should not be adopted without good reason, but where there *is* good reason, we should be suspicious that endogeny is the cause, and it is important when compiling sets of changes in typological work of this kind, that we interrogate the changes involved to a reasonable extent. There will always be a tension between on the one hand the impetus to search a large number of grammars and histories of languages in order to find as many cases of particular changes as exist, and on the other hand the impetus to be sure that we understand the changes concerned in real detail, and it is unlikely to be possible to always properly do both, but we must at least try to bear both in mind. Section 4 attempts to do this.

To conclude this section, the question that I am really asking in this article is this: are there any impossible plausible endogenously-innovatable monoquantal N-CHANGES? That involves a lot of qualifiers, but I think it is what is commonly meant when the issue is discussed, and we do need to bear all these things in mind. Data might look like it is relevant to the testing of a claim about the possibility of a particular change, but on closer analysis turn out not to be relevant at all, as I show in the next section.

4 Case study: possible diachronic relationships between θ and f

In order to make the discussion concrete, I consider in this section the possible diachronic relationships between θ and f. Can one turn into the other? If so, is the change bidirectional? The narrow focus on only two segments will mean that the issues are relatively straightforward to investigate. Donegan & Nathan (2015, 446) highlight why this precise case is worth considering: "while there are numerous cases of /θ/ > /f/, there are virtually no known cases of the reverse, /f/ > /θ/." Virtually no cases? Or not a single one? Garrett & Johnson (2013, 72) think it is the latter: "a [f] > [θ] change is unknown." In the light of the discussion above, the key questions to consider are: is θ > f really safely attested as an endogenous N-change; and is f > θ? Or is it in fact the case that f ≯ θ? I address this for the putative f ≯ θ in section 4.2, after first considering θ > f.

4.1 Are there cases of θ > f?

We have already seen in table 1 that cases of θ > f have been identified. But are they robust? Are they endogenous N-changes? Are they found in a range of languages? The methods set out in (2) are relevant here: they

call for a systematic study of the history of languages in order to find out how well changes are attested. The best available systematic survey of this kind is that in Kümmel (2007).[8] This surveys the histories of around 200 languages, which have been sampled in a systematic (if not fully comprehensive) way, and catalogues the changes that are known to have affected their consonants. It is not the final word on the matter as it does not claim to survey *all* languages' histories (it only considers the Indo-European, Uralic and Semitic families) or to consider the changes involved in detail — the presentation of each individual change is brief, inviting us to investigate it in detail along the lines set out in section 3. The volume is nonetheless a massive undertaking, and offers us an important testing ground to discover if particular changes have been attested.

Kümmel (2007, 193) identifies several cases of changes like θ > f, as reproduced directly here in (5), with the abbreviations for language names translated.

(5)   θ > f; ð > v /_   Modern English dialects
       θ > f; ð > v; ðˤ > vˤ /_   South Anatolian Arabic dialects
       *θ > f /(#)_   Bahrain Shiite Arabic dialects
       *θ > f /(#)_   Proto-Italic, Venetian
       θ > f /_   Albanian dialects (sporadic)
       *ð > v <f> /V_   Faliscan
       *ð > v <f> /_   Proto-Sabellic

The first of these cases is also presented in table 1 above — the well-known case from British English. Some of the changes in (5) can be discounted for our purposes: as we are focusing on θ > f, we should set the last two aside as they do not obviously involve the fortis fricative. If the Albanian case is sporadic, then it cannot be included in consideration here, given the criteria discussed in section 3 — if it was not exceptionless, it does not count as an N-change.

There are thus at least three firm cases of θ > f from Kümmel's survey, which are highly unlikely to be related to each other through contact: in Arabic, Italic and English. This might lead us to agree with Garrett & Johnson's (2013) claim that the change is not very common (although Garrett & Johnson themselves mention another case in Athabaskan, and Blevins 2004 mentions a case in Rotuman), but this claim needs to be seen in the context of the fact that θ itself is not very

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[8] Cser (2003) is a congruous undertaking, but only considers changes that could be considered to be cases of lenition and fortition (in around 100 languages), as do Lavoie (1996), Kirchner (1998) and Gurevich (2004).

common — it occurs in just 3.99% of the UPSID 451 language sample[9] (which aims to genealogically balanced), and just 4% of the 2155 segmental inventories included in the PHOIBLE database[10] (which claims to cover a sizable proportion of the world's languages) — so changes affecting θ could not common be, either.

If we take the discussion in section 3 seriously, we need to probe beneath the surface of the changes in (5) to be sure that they are indeed what the brief formulae there imply. One relevant question is whether it is it right to assume that these changes involve just one quantum. This is commonly assumed (e.g. Blevins 2004, Schleef & Ramsammy 2013), but Garrett & Johnson (2013) propose that θ > f involves an intermediate stage of a labialized dental fricative θ^w on the basis of a reconstruction of an intermediate stage of θ^w in Athabaskan and a description of similar forms in a study of variation in the currently occurring θ > f change in Glasgow (Stuart-Smith, Timmins & Tweedie 2007). This seems ill-founded to me. The Athabaskan intermediate reconstruction is not based on any attested form, so cannot be taken as evidence by itself, and the numbers of such forms in the Glaswegian data are small — they make up only 16 out of 2370 tokens of all realisations of /θ/, contrasting with 132 realisations as f. This 'in-between' form is unlikely to be a half-way point on the way from θ to f, especially as the change is very new in Glasgow (with only 7 f-like forms in older speakers and 141 in younger speakers). It is more likely a compromise form, produced as speakers aim to produce both θ and f at the same time — this is also suggested by the facts that Stuart-Smith et al (2007) themselves transcribe it as '[θ/f]', and that almost all cases of [θ/f] (12 out of 16) occur in the reading of wordlists, when speakers are more likely to be monitoring their speech (so may well aim for multiple targets for multiple reasons). Garrett & Johnson's idea predicts that there should be more clear cases of unconditioned θ > θ^w in the history of languages than of θ > f (because the former would involve less changes), but I am not aware of any (Kümmel 2007 does not list any, either). Other studies of θ > f in progress (e.g. Clark & Trousdale 2009, Schleef & Ramsammy 2013) do not describe in-between forms, and so from all this it seems most likely that θ > f is monoquantal.

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[9] The UPSID sample is described in Maddieson (1984), and the percentage was calculated using Henning Reetz's UPSID interface, which is available here: http://web.phonetik.uni-frankfurt.de/upsid.html.

[10] The details of the inventories and languages included in PHOIBLE are described in Moran (2012), and the percentage is from Moran, McCloy & Wright (2014), which is available here: http://phoible.org.

We still need to interrogate the three cases taken from (5) to some extent to be sure that they really are cases of the kind of change defined in section 3. The English case is one where the evidence is plentiful and unambiguous and has been subject to detailed study. This is possible because it is currently in progress in a large number of areas, spreading through the speech communities, starting with the youngest members, and currently showing the kind of variation that we would expect as a change occurs. The consensus of several studies is that this is an exceptionless change, which affects all words which can feature θ in English, in any environment. Several studies have specifically searched for a frequency effect (of the type predicted for lexically diffusing changes by exemplar models of phonology, as in work such as Bybee 2001, Pierrehumbert 2001, Phillips 2006) and have failed to find one. Nielsen (2010) investigated the data from Glasgow discussed in Stuart-Smith et al (2007) and shows that there is no lexical frequency effect in terms of how common [f] realisations of /θ/ are — once phonological environment is controlled for, all words are changing in lockstep. Clark & Trousdale (2009) perceive only an inconsistent interaction with frequency in data from Fife, and Schleef & Ramsammy (2013) find no frequency effect in data from London and Edinburgh. The English θ > f change is clearly an N-change. There is no reason to assume that the initial innovation of this change was driven by exogeny (and I know of no claim that it was) so on this basis θ > f does indeed show every sign of being an (unconditioned) endogenously-innovatable monoquantal N-CHANGE.

The two other cases in (5) provide support for this: although θ > f is not a common change (because θ is not common), it has also been innovated elsewhere. With reference to the Arabic case, Watson (2002, 15, translating Fischer & Jastrow 1980, 50) confirms that, "[i]n southern Anatolian Siirt, the original interdentals have become labiodentals /f, v, ɣ/, as in: *faᶜlab* 'fox' (< *ṯaᶜlab*)" (the symbol ṯ is an Arabist convention to represent θ). Jastrow (2005, 88) also describes this case in the same way, giving other examples, such as *ba'af* 'he sent' (< *ba'aṯ*), which show that the change is unconditioned, like the English case. Fischer & Jastrow (1980, 50) also mention other dialects, such as Tunis Arabic, where θ has become f in "isolated cases", that is, in a lexically specific, sporadic way (and given the distance between Anatolia and Tunisia it is unlikely that these developments are connected through dialect contact). The implication of this is clearly that the change in Siirt Arabic is exceptionless, as in the English case.

Anatolian Arabic is spoken in "linguistic islands" in Turkey, surrounded by speakers of other languages, and "[m]ost of the speakers also know Kurdish (the regional trade language) and Turkish (the

official language of the state)" (Jastrow 2005, 88), so there clearly has been language contact in the area, and it is therefore possible that the loss of the dental fricatives through θ > f in Siirt Arabic was due to exogeny, but I am not aware of any argument that there *was* an exogenous cause to this change, and we should not assume one simply because there has been some contact in the history of the language. The kind of linguistic contact in Siirt is different from that described for the loss of θ in Shetland and Ireland in section 3.3. The latter involved speakers failing to acquire θ when starting to speak a language which had θ; the former would involve the loss of θ in speakers' native language when they came into contact with speakers of languages without θ, or through the influence of non-native learners of Arabic, which are less likely scenarios.

Describing the Italic case in some detail, Stuart-Smith (2004, 206) writes that "*[θ-]>[f-]; *[-ð-]>[-v-]: In word-initial and word-internal position the reflexes of the PIE dental/alveolar voiced aspirate merge with /f/." The implication is that Proto-Indo European d^h first became θ, in Proto-Italic, and then became f (with intersonorant lenisation accounting for the medial situation). The relevant segments are not transcribed as existing word-finally (Stuart-Smith 2004, 109 writes: "PIE did not permit word-final voiced aspirates", meaning that the fricatives would not have emerged there in Proto-Italic), so it seems that this was also an unconditioned change, affecting all examples of the relevant segment, in all environments (initial and medial) in which it occurred. As a change that is reconstructed, from the Proto-Italic θ to attested f, this does not provide the strongest evidence for the θ > f change by itself, but the case makes sense in this reconstruction, and — together with the other cases — adds up to show that θ > f is a reasonably well attested change which fulfils the criteria for being counted an endogenously-innovatable monoquantal N-CHANGE. This now allows us to pose the crucial question: is it bidirectional?

4.2   Are there cases of f > θ?

Given that θ > f is a possible change, f > θ is a plausible change, too. We should thus be able to find examples of it in the history of languages. We have, however, seen a claim above that f > θ is an impossible change (that is, it is f ≯ θ). In order to consider whether it really is the case that f ≯ θ, we need to search for counterexamples to it — are there in fact any instantiations of the candidate impossible change? If we make every effort to find them and fail, we would have a firm basis for the claim that the change is impossible. In order to make every such effort,

following the points set out in section 2, we need to do the two things set out above in (2), repeated here:

(2)   (i)   survey a large sample of language histories to see if any examples of a candidate impossible change are attested
       (ii) conduct a survey of experts in the history of all languages, asking if anyone knows of any example of a candidate impossible change

Handily, both of these have already been done for f > θ.

Kümmel (2007), described in the last section, has surveyed the histories of 200 languages, looking for all kinds of changes. Kümmel lists no occurrences of f > θ from any of the language histories that he considered. He clearly searched for them, and lists a number of changes which involve change in the place of articulation from labial/labiodental to coronal, but these are mostly all examples of p > t or m > n (with a range of environmental conditionings). Only two changes of this broad type are listed as affecting f, and both of these are f > s, in the environment of adjacent coronals.

Kümmel (2007) is a vast testing ground, but is of course limited in terms of the number of languages that it covers. This is where (2ii) becomes important, and Bennett (2010a) is precisely the kind of survey of experts that we need to help answer our question. Bennett posted the following query to the Linguist List:

> I'm curious whether anyone knows of any examples of a diachronic /f/ > /theta/ change, in any phonological environment. I would also be interested to hear about any synchronic /f/ ~ /theta/ dialect variation, where /f/ has become /theta/ in the innovative dialect.

Bennett reached the most linguists possible by posting this query to the Linguist List, as it is the most well known place for linguistic discussion. Bennett received 16 responses from other linguists, which he summarised in a posting which also appeared on the list, which I refer to here as Bennett (2010b). These responses mentioned a number of putative cases of f > θ, but many of the responders themselves acknowledged that their change was not a phonological change in the 'normal' sense (which I defined as 'N-changes' in section 3.2). Three changes are mentioned which involve segmental conditioning (f > θ / __ t in Albanian dialects, f > θ before from high vowels in Tsakonian Greek, and f > θ / __ l in part of the Gothic lexicon). These are thus *not* the inverse of the unconditioned θ > f. The conditioned f > θ changes seem likely to involve assimilations to the coronality of the following segments, and (as argued in section 3) that is a fundamentally different

kind of thing to an unconditioned change like θ > f. Two cases are mentioned (from Yazghulami and neighbouring Sanglichi and from Occitan) which involve f > θ in one single lexical item in a language, leaving all other cases of f in the language untouched, and both of these are also described as potentially tied to specific phonological environments (pre-m and pre-j) — these changes can thus clearly be discounted: they do not represent the inverse of θ > f, both on the grounds that they may have been conditioned (although this cannot be known with any certainty), and on the sure grounds that they were not exceptionless N-changes.

Bennett's summary of responses also mentions a reconstructed series of changes from the history of Italic along these lines: d^h > d > θ > f > θ > d, the penultimate stage of which could fit the bill (this is also suggested for this change in McGuire & Babel 2013, perhaps following Bennett 2010b). This series of changes is proposed on the basis of forms in Latin with medial d which correspond to attested forms in the related and earlier Sabellic with medial f. However, as the relevant stages are based on unattested reconstructions, this is dubious as evidence, partly given that it is a diachronic duke-of-york derivation, and especially given that Latin is not a daughter of Sabellic, so there is no evidence that Latin ever had an f stage in the medial environment. Bennett's (2010b) summary (following the suggestion of one of those who responded to his query) claims that this chain of changes is suggested in Stuart-Smith (2004), but this is a misinterpretation of what is proposed there (Stuart-Smith, personal communication) — Stuart-Smith does not reconstruct any voiceless fricatives in medial position, and does not reconstruct medial f > d in the history of Latin. This case seems to be based on a misreading of specialist literature and does not provide anything like a compelling example of f > θ.

One other case came to light in response to Bennett's query, and this one invites more serious consideration: in Pulo Annian, "Proto-Micronesian *f became /ð/". As stated, this results in ð (not θ) but it would not be surprising if this were due to a subsequent lenisisation of θ. This case has indeed entered the literature (perhaps following Bennett 2010b) as a 'potential case of f > θ' — it is cited as such by McGuire & Babel (2013), for example — so it requires our attention. I deal with it in section 4.2.1. I also consider, in the remainder of this section, two other potential cases of f > θ, because they have also come to my attention. We should be sure to consider every possible counterexample to a claim. One of these is mentioned by Blevins (2015, 491), who is commenting on the claims mentioned at the start of this section. She writes that:

> The existence of f > θ sound change is disputed (e.g. Garrett & Johnson 2013, Donegan & Nathan, this volume [=2015]); however, there appears to be at least one clear case in Peninsular Spanish, as reported in Fernández (1996: 216).

Is the Peninsular Spanish case compelling? I consider it in section 4.2.2. In section 4.2.3, I consider another possible case of f > θ that has come to light from the history of English.

4.2.1 Pulo Annian

Pulo Annian is a Micronesian language from the small island of Pulo Anna, which is part of the Republic of Palau. It is most closely related to the other Chuukic languages (also called Trukic languages). The Ethnologue (Lewis, Simons & Fennig 2016) sees it as a dialect of Sonsorolese but most specialist work sees Sonsorolese and Pulo Annian as closely related sister languages. The Ethnologue estimates that there are currently 10 speakers, none of whom now live on Pulo Anna, so it is highly endangered. It is well documented in one source, however, from which all evidence seems to be derived: Oda (1977). This is a detailed and trust-inspiring work, with a structural description in early sections and a dictionary as an appendix.

In life's-work-summarising material, Bender, Goodenough, Jackson, Marck, Rehg, Sohn, Trussel & Wang (2003a,b) give a detailed account of phonological correspondences for Micronesian languages (including Pulo Annian), a reconstruction of the segmental inventory of Proto-Micronesian and of intermediate stages between Proto-Micronesian and the attested Micronesian languages, such as Proto-Chuukic (from which Pulo Annian and Sonsorolese descend), and also a reconstructed lexicon for Proto-Micronesian, Proto-Chuukic and other related proto-languages. This material can give us a good understanding of the phonological history of Pulo Annian, if we consider it carefully.

The correspondence in question here is exceptionless: every single word that is reconstructed by Bender et al (2003a) with an f in Proto-Micronesian (and hence has an inherited f in Proto-Chuukic), and which has not been lexically lost in Pulo Annian, contains a corresponding segment of the ð type in Pulo Annian. The f that is reconstructed for Proto-Chuukic (the stage before Pulo Annian) is on solid ground: it is attested as [f] in all other present-day Chuukic languages (and is assumed to be inherited unchanged from Proto-Micronesian, for which there is also other evidence). There is thus every indication of N-change like behaviour, and the output in Pulo Annian is attested, so a serious case can thus be made that f > ð in Pulo Annian on an N-change-like

basis, and it would not be surprising if such a change turned out to have involved a θ stage.

It has been suggested (for example in McGuire & Babel 2013, following others) that this change could be due to contact with Palauan, which is also spoken in the Republic of Palau, and which has dental fricatives. However, Palauan and Pulo Annian are only distantly related (so a new-dialect formation scenario is unlikely) and it is not clear why Palauan should influence Pulo Annian and not any other Chuukic language (not even Sonsorolese, which is also spoken in Palau). This exogenous explanation, if it were persuasive, would mean that this potential case of f > θ could be easily discounted, but we should not reach for exogenous explanations unless there is some real evidence that contact occurred of a type that is likely to lead to phonological change. In the absence of such evidence, we should take seriously the idea that the correspondence in question here is due to endogenous change.

So: is the Pulo Annian case evidence that f > θ is a possible endogenous N-change? I do not think it is. If we assume that only endogenous changes were involved but investigate the case in detail, then it does not seem likely that the diachronic developments involved were due to a simple N-change of the type f > θ (with voicing somewhere along the way to give an attested ð). There are a number reasons to think this. Firstly, it is not completely clear what the current segment is in Pulo Annian — that is, what output the change(s) produced; and secondly, the changes involved in producing the present-day Pulo Annian obstruent system seem to have involved large-scale mergers.

Although Bennett's (2010b) sources use the symbol <ð> to describe the segment (and other discussions do, too), the original (and only) source for Pulo Annian (Oda 1977) does not make it clear that this involves /ð/ (that is, an underlyingly voiced dental fricative). The convention proposed by Oda is to use <d> to transcribe the segment involved at the underlying level. Oda describes the full consonant system of Pulo Annian in a table reproduced here as (6).

(6)	labial	labio-velar	alveolar	velar
fricative			d, s	
stop	p	pʷ	t	k
nasal	m	mʷ	n	ŋ
lateral			l	
glide		w	y	

In terms of features, Oda analyses 'd' as [+anterior], [+coronal] and as unspecified for any other place feature, which is the same set of specifications given to the other obstruents that are described as 'alveolar' in the consonant table. In terms of manner features, 'd' is analysed as [–syllabic], [–sonorant], [+consonantal], [+continuant], which is the same as the specifications that are given to 's'. The pair 'd, s' are distinguished by [±strident], with 'd' specified as [–strident] and 's' as [+strident]. Further relevant information is Oda's proposal for underlying to surface mappings for obstruents, which is reproduced here as (7). This contains an UF ('underlying form') for each segment, its SFs ('surface forms') and a symbol proposed for it in the orthography of the language (given in the third column).

(7)   UF   SF   Symbol
         /p/   [p]   p
         /pʷ/   [pʷ, βʷ]   pʷ
         /t/   [t]   t
         /k/   [k, x, ɣ]   k
         /d/   [ð]   d
         /s/   [s]   s

From all this, it seems clear that the segment which is transcribed as <d>, /d/ and [ð] is not a canonical voiced dental fricative. Oda is unambiguous in describing the segment as alveolar and it seems that [voice] is not active at all in underlying specifications in Pulo Annian — it would be very surprising typologically to have a voicing contrast in fricatives but not in stops, and Oda's use of [±strident] to characterise the contrast makes it clear that the segment in question is not fundamentally voiced, but is simply less noisy than [s]. The fact the Oda does not use 'z' to transcribe the segment in question shows that it is not a sibilant (grooved-tongue) fricative; on the other hand, we know that it *is* alveolar. The most likely candidate is therefore an unvoiced alveolar fricative with a flat (or 'slit') cross-sectional tongue shape, of the type that Pandeli, Eska, Ball & Rahilly (1997) propose should be best transcribed as θ̠, using the θ base symbol to show that the segment is a flat-tongue fricative, and the 'alveolar' diacritic from the extended IPA for disordered speech to represent its place of articulation. Such 'slit fricatives' occur in Irish English (as Pandeli et al describe) and also in Liverpool English (Honeybone 2001, Watson 2007), as a realisation of underlying /t/, where they are less noisy than and hence auditorily distinct from the groove-tongue s, which also occurs in both systems. It makes sense to use <d> to write θ̠ in Pula Annian if there is an impetus to use only single roman letters for segments, as <t> is needed for t and

<s> for s, leaving only <d> available of the single letters that are used to represent alveolar obstruents (if we discard <z> as clearly sibilant). Although 'd' is typically a 'lenis' symbol, it need not be that it is intended to imply that the segment is phonologically 'voiced' — Oda does not specify it as [+voice], as mentioned above, but rather distinguishes it from the other alveolar fricative in terms of stridency, so I use the θ̠ symbol for the segment here (rather than ð̠) as it is standard practice to use the 'fortis' symbols to stand for either a segment which is positively specified as fortis, or for plain segments which are laryngeally unspecified.

The rationale for assuming in (6) and (7) that the segments which have fricative realisations (other than 'd' and 's') are underlying stops is that they appear as stops in classically 'strong' environments, and as fricatives in classically 'weak' environments (for a discussion of such strength and weakness see Ségéral & Scheer 2008). Given this, Oda follows common practice and assumes that the fricative forms are derived through lenition processes. Pulo Annian has a singleton~geminate contrast and, unsurprisingly (as discussed in Honeybone 2005 and much else), the obstruents typically surface as stops in gemination (a classical 'strong' environment). Oda writes (1977, 12) that:

> Phonetically the difference between a geminate and a single p̱ and t̠ is a matter of fortis vs. lenis pronunciation, that between a geminate and a single p̱w and k̠ is a matter of stop vs. fricative, that between a geminate and a single m̱, m̱w, and ŋ̱ is a matter of duration, that between a geminate and a single n̠ is a matter of nasal vs. flap, and other geminate continuants tend to be pronounced with a slight affricate-like stoppage at the onset followed by longer duration than single continuants.

The segment that is of interest for our purposes (identified as θ̠ above) therefore presumably surfaces as slightly affricate-like when geminate, and purely fricative-like when a singleton, but this is not stop-like enough to lead to an analysis of the segment as an underlying stop. In contrast, /pʷ/ and /k/ are seen as underlying stops because they are unambiguously stops in gemination, and Oda derives their singleton fricative realisations through a rule of 'consonant weakening', represented as in (8).

(8)
$$\begin{matrix} k \\ p^w \end{matrix} \rightarrow \begin{matrix} x \\ \beta^w \end{matrix} \quad / \quad \left\{ \begin{matrix} \# \_ V \\ V \_ V \\ V \_ \# \end{matrix} \right\}$$

It is clear from (8) that Pulo Annian allows considerable spirantisation, even in initial position (when a singleton). We return to the implications of this below.

Given all this, it seems that the change that caused us to consider this case is not, in fact, of the type f > θ. Rather, it seems to be f > θ̱. This would still be a somewhat surprising change for a language to undergo, however, and I show now that a detailed consideration of the phonological history of Pulo Annian suggests that something rather different was involved.

Bender et al (2003a) reconstruct the consonant system of Proto-Chuukic as in (9), in which I have fitted Bender et al's segmental symbols into a table like that in (6), for the sake of comparison with Oda's analysis of Pulo Annian.

(9)

	labial	labio-velar	alveolar	palatal	velar
fricative	f		s[11]		
stop	p	p^w	t,d	c	k
nasal	m	m^w	n	ñ	ŋ
lateral			l		
rhotic			r		
glide		w	y		

I focus henceforth only on obstruents, as sonorants do not interact with the relevant changes. (10) sets out an overview of the changes that derive the obstruents of Pulo Annian's (6) from Proto-Chuukic's (9), as implied by Bender et al's full lists of correspondences (but using my interpretation of the segments s and θ̱). Henceforth I abbreviate Proto-Chuukic and PCk and Pulo Annian as PuA, following Bender et al.

(10) PCk   d   t   s   f   c   k   p p^w

PuA   t   θ̱   s   k   p p^w

It is clear that considerable changes are involved here, and that (10) hides several diachronic stages, so that the development of PCk f cannot

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[11] Bender et al (2003a) in fact use the symbol 'T' here to show that a distinct segment must be reconstructed, but to remain somewhat vague about its features. I follow Goodenough (1992) in assuming that it was s — this seems most likely as 6 of the 10 Chuukic languages now have s, and two others have h, which could easily be derived from s through debuccalisation.

be understood in isolation from the other changes. If we consider the details, we can also reconstruct much of the relative chronology of these changes on language-internal grounds. As an explanation of (10), I propose the changes and chronology set out in (11).

(11) (i) t > d / __ a
    – this is on the basis of the split that Bender et al (2003a) describe in what was t in PCk, given that PCk t corresponds to PuA t only before a
    – this involves a partial merger with inherited PCk d, the result of which is later subject to (v)

   (ii) s, f > t
    – this involves a total loss of fricatives, leaving only stops in pre-PuA obstruent system
    – it is a merger with what remained of PCk t, 'replenishing' that segment when it had mostly been lost through (i)
    – the two PCk fricatives thus merged with the least marked PCk obstruent (i.e. t), retaining their laryngeal state
    – this must have occurred after (i), or else the outputs of (ii) would have undergone (i)

   (iii) c > s
    – once s had been lost through (ii), fricatives were reintroduced into the system through the spirantisation of c, producing the typologically most common single fricative, s
    – this must have occurred after (ii), or else the outputs of (iii) would have undergone (ii)

   (iv) t > θ̲
    – once fricatives were back in the pre-PuA system after (iii), a further spirantisation occurred
    – the result of this spirantisation of t is typologically plausible: it is the same as in Irish English and Liverpool English, where spirantisation of t also produced the slit alveolar fricative θ̲
    – PuA clearly allows considerable spirantisation, even in initial position, as shown in (8)
    – it is possible that this spirantisation in (iv) was originally part of the same change that introduced (8), and it was maybe even the same change as (iii), so that they can all be collapsed as the introduction of a context-specific spirantisation which affected all fortis stops which had any

specification for tongue activity (t, c, k, pʷ), leaving only p unaffected; if so, we simply need assume that the spirantisation remained as a synchronic process in k and pʷ, but that the fricative forms spread to all environments in coronals, so that the fricative output became lexicalised into the language's URs (producing θ̲ and s)

(v)   d > t
– this change can be seen as filling the 't-gap' in the obstruent system that had emerged after (iv)
– this must have occurred after (iv), or else the outputs of (v) would have undergone (iv)

For clarity, this can all be set out in terms of underlying forms as in (12), where each stage of pre-PuA that existed after one of the changes in (11) is indicated by the small roman number given to that change. The final stage in (12) is present-day PuA, as set out in (6).

(12) PCk   d   t   s   f   c   k   pʷ   p

after (i)   d   t   s   f   c   k   pʷ   p

after (ii)   d   t   c   k   pʷ   p

after (iii)   d   t   s   k   pʷ   p

after (iv)   d   θ̲   s   k   pʷ   p

after (v)   t   θ̲   s   k   pʷ   p

The above changes and correspondences are exemplified in the data in (13), which is adapted from Bender et al. (2003b), and relies on their reconstruction of PCk and Oda's description of present-day Pulo Annian. (13) is given exactly as Bender et al. represent the words, apart from the analyses discussed above: where Bender et al use 'T' in PCk, I use 's' following Goodenough's (1992) realist reconstruction, and where they use 'd', following Oda (1977), I use 'θ̲', as argued for above.

(13)   PCk   PuA
         dakua   takua   'yellow-fin tuna'
         tafe(y)a   ta<u>θ</u>ea   'medicine, to medicate'
         makoto   mako<u>θ</u>o   'finger, broken stick'
         sooŋa   <u>θ</u>oŋa   'be angry'
         fakafaka   <u>θ</u>aka<u>θ</u>aka   'coughing, to cough'
         ciwa-ni   súwe-ni   'still, yet'

If all the above is anywhere near right, then the diachrony of the segments in focus here is: f > t > <u>θ</u>. There was no f > θ change in Pulo Annian.

4.2.2 New Castile Spanish

When Blevins (2015, 491) writes (as mentioned above) that there appears to be a case of f > θ in Peninsular Spanish, she is taking seriously the requirement to consider every possibly relevant attested *change* when assessing whether a CHANGE is bidirectional. It is important that we should investigate this case to see if it really does fulfil the criteria set out in section 3. If it does, then f > θ is clearly not impossible.

The reference that Blevins gives as evidence for the case leads to Moreno Fernández (1996), a chapter which describes the dialect of Spanish spoken in New Castile, in central Spain. The full passage dealing with this aspect of the dialect (Moreno Fernández 1996, 216) is as follows:[12]

> The phoneme /f/ is realised as bilabial in much of the region. In speakers with little education, there are acoustic equivalences of the type *celipe* 'Felipe' ['Philip'], *cinca* 'finca' ['estate'], *escalazón* 'escalafón' ['scale'].

Moreno Fernández's data gives orthographic transcriptions for the New Castile Spanish forms (in italics), making use of the fact that <c> before <e, i>, and <z> before <o> represent θ in Spanish orthography. It is clear that there are words with θ in New Castile Spanish which correspond to forms with f in Standard Spanish. There are, however, two reasons to doubt that this is due to an N-change of the type f > θ. The first is that, as Moreno Fernández explains in the first sentence quoted above, that the labial fricative (in speakers not affected by the

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[12] My translation, with glosses into English added. The original reads: "El fonema /f/ se realiza como bilabial en buena parte de la región. En hablantes con pocos estudios se encuentran equivalencias acústicas del tipo *celipe* 'Felipe', *cinca* 'finca', *escalazón* 'escalafon'."

change under discussion) is actually [ɸ] in much of the region, so the change may have involved ɸ > θ. There is no way of knowing this, however — it could be that an intermediate f stage was the direct input to the change. More important is the fact that the forms with θ are not due to a change that had the characteristics required in section 3. Moreno Fernández (personal communication) writes further that:[13]

> My interpretation is that this has to do with a phonetic phenomenon, but it does not necessarily imply a change [...] You don't hear *zuera* [ie, [θ]*uera*] for *fuera* ['outside'], for instance. Therefore it doesn't affect every word, as a regular phonetic change.

It is clear that the change involved here is lexically-specific and has not affected a common word like *fuera*. The change involved is certainly interesting and does produce θ, but it is an A-change, presumably due to the sporadic reanalysis of certain words, so it does not fit the bill: there is still no evidence that f > θ is possible as an N-change.

4.2.3 Traditional dialect English from Whitwell, Isle of Wight

The only other candidate for f > θ of which I am aware shows up in the material collected during the 1950s and 60s for the *Survey of English Dialects* (also mentioned in a different connection in section 3.3, above). I am grateful to Pavel Iosad for bringing it to my attention. One of the questions in the *SED* questionnaire about cartwheels asked "what do you call these sections of the wooden rim?" The Standard English expected response was *fellies*, the plural of *felly*, a word with an established Germanic etymology which refers to "the curved pieces of wood which, joined together, form the circular rim of a wheel" (OED). The fieldworker transcription for this word from Whitwell on the Isle of Wight is [θɪlɪz] (Orton *et al*. 1962–71), and so presumably the singular would be [θɪlɪ]. This is no mistake — the form [ðəɫiːz] is recorded for Kingston in Dorset, a *SED* locality which is very close to Whitwell, showing that the dental place of articulation is a dialect feature of at least a small area around the south coast of England and the neighbouring Isle of Wight. The Kingston form also shows the effects of the Southern English Fricative Weakening (SEFW — see, for example, Honeybone 2012), a regular change in much of the south-west of England, which made all fortis fricatives lenis.

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[13] The first sentence in the quotation is my translation of "Mi interpretación es que se trata de un fenómeno fonético, sin que ello implique necesariamente un cambio" from Moreno Fernández's email to me, and I have italicised the data and added the gloss and transcription in square brackets.

The [θɪlɪ] form is mentioned by Wakelin & Barry (1969), who are primarily interested in the SEFW and so consider all words with initial fricatives in the *SED* materials for the English south-west. They draw attention to these surprising forms with dental fricatives as a side-point. No other word which has a labiodental in Standard English (for example *furrow, farm, fleas, flowers, friends, faint, pheasants*) has a dental in their data, so it is clear that only *felly* has undergone this change. The change could perfectly reasonably be due to f > θ (the result of which has also undergone the SEFW in Kingston), but this makes this case from Whitwell (and Kingston) English just like those from Yazghulami/Sanglichi and Occitan mentioned from Bennett's (2010b) summary, and also like the case in New Castile Spanish: it is a sporadic, lexically specific A-change.

4.3 Summary: possible diachronic relationships between θ and f

To summarise, θ > f is well attested as an N-change, given the general cross-linguistic rarity of θ. All the cases of f > θ that have been thrown up, however, by surveys, queries and related literature, have been shown to be A-changes — f > θ is clearly is possible, but, it seems, only as an A-change. Indeed, despite claims to the contrary, f > θ does not seem extremely rare as an A-change: there is evidence that it has occurred in New Castile Spanish, Whitwell English and possibly also in Yazghulami/Sanglichi and Occitan. There is no evidence, however, that it is possible as an N-change.

5 Sporadicity, exceptionlessness, misperception and phonology

In the above, it might appear that sporadic changes have been too easily dismissed as irrelevant to the issue of whether f > θ is possible as an N-change. I do not think so. There is good reason to believe that N-change-like diachronic correspondences and A-change-like diachronic correspondences are due to completely different mechanisms. This basic claim is nothing new: Labov (1981), Kiparsky (1988) and Bermúdez-Otero (2007), for example, have argued that 'neogrammarian' and 'lexically diffused' changes are both possible, but involve different types of diachronic event. Certain issues arise in connection with the specific changes considered in this paper, however, which are not those typically discussed in this connection.

It has been common to point out since at least Ohala (1981, 1993),[14] that [θ] and [f] are "very close auditorily", and hence offer something akin to "inherent ambiguity in the speech signal" (1981, 178), and that this could be the key factor that drives the change between them. This is typically linked to the fact that experimental investigations into the confusability of consonants show that "[t]he distinctions between [f] and [θ] and between [v] and [ð] are among the most difficult for listeners to hear" (Miller & Nicely 1955, 347). Such studies imply that [θ] and [f] are inherently confusable, which means that, while there are phonetic cues which differentiate between the two, they are quite subtle and could be missed by listeners for a range of reasons (noise while listening, fast or mumbled articulation, or maybe even simply lack of attention while listening). If the cues that differentiate between two sounds are missed, one sound could be misperceived as the other. Ohala (e.g. 1993) sees this as able to lead to one possible type of listener-based phonological change: the 'confusion of acoustically similar sounds'. Blevins (e.g. 2004) describes this as 'CHANGE' in her CHANGE-CHANCE-CHOICE typology of phonological change. This kind of model is often set out in diagrams like that in (14), which represents how the change in Whitwell English discussed in section 4.2.3 works under these assumptions.

	Speaker		Listener/Learner
(14)	*felly*		'*thelly*'
	/fɪlɪ/		/θɪlɪ/
	↓		↑
	[fɪlɪ]	→	[θɪlɪ]
		misperception	
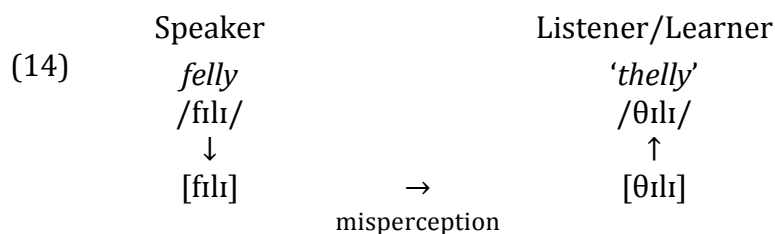
I assume in (14) that the listener is engaged in acquisition at the time of the misperception on the assumption that it is unlikely that a mature speaker-hearer, with a fully established phonology, would assume that they had mis-set their underlying phonology on the basis of one misperception event. The event in (14) must represent an important datum in acquisition that a listener/learner used to fix their underlying representation for this word — one time when they misperceived the intended [f] as [θ] due to the inherent confusability of

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[14] The basic observation is much older. Ohala points back to Sweet (1888), who discusses "[s]uch isolative changes as ᴜ to ﹥ and ω to ɛʃ" under the heading of 'acoustic changes' (1888, 43), showing an awareness of the acoustic similarity of the sounds, and the implication that this similarity can drive such changes. Sweet is using his revised version of Bell's (1867) 'Visible Speech', in which ᴜ = θ, ﹥ = f, ω = ɾ, ɛʃ = ʀ.

the two. This listener/learner then naturally turns speaker and produces cases of [θ] for this word, which will be the input for other listener/learners, who, in this case, must have followed suit and used the [θ] data to set their underlying form, ignoring the cases of [f] from other speakers, so that the θ-ful form could spread to a whole speech community (or, just possibly, other listener/learners coincidentally made the same misperception at the same time).

This model is a compelling way of understanding certain kinds of change. If we assume that the listener/learner has established that the language they are acquiring has both θ and f, then they need to determine which words have θ and which have f. In the Whitwell English traditional dialect described in 4.2.3, listeners adopted the same UR as previous generations for words like *farm* and *pheasant*, but not for *felly*, and misperception of the type set out in (14) explains this perfectly well. It is notable, however, that this model is inherently lexically specific. The misperceptions involved are misperceptions of individual words and there is no reason why a listener/learner who has misperceived *felly* to fix its UR as /θɛlɪ/ should also misperceive *pheasant* to fix its UR as /θɛznt/

This model is therefore appropriate for sporadic changes such as that in traditional dialect Whitwell English, and also for the change in New Castile Spanish which saw *Felipe* and *finca* be misanalysed, but not *fuera*. It is presumably down in part to chance which words are reanalysed in this way. This is just like the kind of reanalysis that was discussed in section 3.2, in which the final n in a preceding determiner (such as *an*) was misperceived by listener/learners as being initial in the base of a few vowel initial words, as in *ewt > newt*, which was possible because *a* was also a potential form of the determiner. This is shown in (15), which assumes that *a~an* involves phonologically controlled allomorph selection (Mascaró 1996, Nevins 2011), and represents the fact that there were likely subtle phonetic cues (involving milliseconds of segmental duration, for example) that could differentiate between the form intended by the speaker and that perceived by the listener/learner (and that these cues were not always clear, leaving a situation ripe for reanalysis). There are certainly such cues in present-day English — Hoard (1966) showed, for example, that listeners can reliably tell the difference between casual speech realisations of the phrases *an aim* and *a name* (for 3 out of 4 speakers) — and I assume here that this can be projected back to the earlier stage of English when the reanalysis was made. This issue is often discussed under the heading of 'juncture' (see Scheer 2010 for a recent detailed discussion), and I follow an old practice in (15), in representing juncture with the symbol '+'.

(15)   Speaker   Listener/Learner
              an ewt   *a newt*
              /#an##ɛut#/   /#a##nɛut#/
                  ↓   ↑
              [an+ɛut]   →   [a+nɛut]
                              misperception

This change is also inherently sporadic, as an A-change — there is no reason why the reanalysis should occur throughout the vowel-initial lexicon. It did happen a few times (in *newt*, *nickname* and *nonce*), and it is a bidirectional change, as would be expected: a sequence of *an+vowel-initial-word* sounds like a sequence of *a+n-initial-word*, just like a sequence of *a+n-initial-word* sounds like a sequence of *an+vowel-initial-word*, and indeed *adder*, *apron* and *umpire*, which all originally had initial n, have undergone the inverse change.

Given this, we would expect that, if [f] sounds like [θ], then [θ] should sound like [f]. Assuming that the distinction between [f] and [θ] is difficult to hear, then we would predict that, if f > θ is possible as an A-change, then θ > f should be possible as an A-change, too. It seems that it is. As mentioned in section 4.1, Fischer & Jastrow (1980, 50) report that in Tunis Arabic the word *ṯamma* 'there, there is' has become *famma*. Tunis Arabic otherwise retains θ, so this change must have been an A-change, along the lines of (16). We can conclude, therefore, that θ > f is bidirectional as an A-change.

(16)   Speaker   Listener/Learner
              ṯamma   *famma*
              /θamːa/   /famːa/
                  ↓   ↑
              [θamːa]   →   [famːa]
                              misperception

It may be uncontroversial, but it is worth being explicit that there is no reason why the same diachronic correspondence cannot be due to both A-changes and N-changes. I showed in section 4.1 that θ > f can be an N-change, and it is clear from the Tunis Arabic case that it can be an A-change, too. The case of θ > f which is currently in progress in English (and hence allows detailed investigation), for example, has been shown to be classically neogrammarianly exceptionless, and is thus unlike the changes considered in this current section. It seems to me that the Ohalaesque, listener-based, misperception account of change discussed

in this section does not allow us to appropriately model θ > f when it is an N-change (or indeed to model any N-change at all). It would be a remarkable coincidence if all lexical items were subject to the same misperception at the same time. There is thus reason to believe that N-changes are implemented by a different mechanism (and are thus fundamentally different things). As Scheer (2015, 313) argues, while discussing naturalness and diachrony:

> Regularity in linguistic patterning is the result of grammatical computation: it is due to the fact that lexically stored pieces are run through a computational system (made of rules or constraints) before they reach the surface. What we see, then, are the traces that grammar leaves on the lexical ingredients, and these traces are regular.

While the misperception analysis can model A-changes well, it is more likely that regular, exceptionless N-changes are due to changes in phonological computation over segments (and other phonological structures), not words. This can presumably involve the innovation of phonological processes (the outputs of which could be subsequently lexicalised into URs). If this is right, then N-changes and A-changes are indeed fundamentally different things, and if we are searching for the inverse of θ > f as an N-change, we should be sure to discount cases of f > θ that are A-changes — they are simply not the same kind of thing.

From all the above, it seems that we have a firm basis to claim that f ≯ θ (if we restrict ourselves to N-changes). It is difficult not to ask: *why*? We can only explain why this might be the case from within a theoretical, predictive model (of phonology and/or of phonological change) — an explanation of this type could give us a truly satisfying proof that certain changes are impossible (because they cannot be modelled in a particular framework). For example, a model might be able to explain why θ is more marked than f, and why a less marked segment cannot turn into a more marked one. This current paper cannot seek to answer such questions (or indeed to provide a straightforward definition of markedness). There have been attempts to model this, however: θ is more marked than f in certain featural theories because it has a more complex representation. From a different perspective, McGuire & Babel (2013) argue that θ is more marked because there is considerable variability in whether speakers give visual cues for it or not (because θ can be interdental — that is, some but not all speakers articulate θ with the tongue visible between the teeth). This may play a role in change, but McGuire & Babel assume a model which relies on misperception on the part of the listener-watcher/learner, of the type set out in (14–16), with visual cues playing a role in the misperception as well as audible cues. As we have seen,

however, this kind of model leads to A-changes, and in A-changes f > θ does not seem to be uncommon: examples from New Castile Spanish, Whitwell English and possibly also in Yazghulami/Sanglichi and Occitan are discussed above; only one case of θ > f as an A-change has been identified in this paper, from Tunis Arabic, although it is likely that the Albanian case of θ > f mentioned in (5) is also an A-change (as it is described as being sporadic). The numbers involved here are small but they do not demonstrate an asymmetry of occurrence of either direction in A-changes, against what McGuire & Babel's approach seems to predict. The cause for the directionality of θ > f as an N-change may yet lie in the possibilities of phonological computation.

6   Conclusion

Given all the above, I argue that we *can* define a very specific sense in which there *are* impossible changes. If we restrict ourselves to plausible endogenously-innovatable monoquantal N-CHANGES then we can hope to interrogate the collective knowledge of the field in search of candidate examples of relevant changes from the history of languages, and if we fail to find any after a serious attempt to do so, we may reasonably conclude that they are indeed impossible. We therefore need the symbol '≯'.

I hope that the investigations above also show that, if we restrict ourselves to N-changes, θ > f is attested to a reasonable extent (given the rarity of θ), but f ≯ θ. Indeed, if both θ > f and f > θ were possible as N-changes we might expect the latter to be much more common than the former in phonological change, because f is much more common in languages than θ. While θ occurs in just in just 3.99% of the UPSID 451 language sample, f occurs in 39.91% of those languages (and f occurs in in 49% of the languages in PHOIBLE), so we would expect to find more cases of the possible changes that can affect f in the history of languages than we would cases of the changes that can affect θ. I have shown here, however, that both a systematic survey of language histories and a survey of language experts have turned up no convincing case of N-change f > θ. There are some cases where what once was f is now in diachronic correspondence with θ or ð (or <u>θ</u>), but these are not due to monoquantal N-changes. It does seem that both θ > f and f > θ are possible as A-changes, but N-changes and A-changes are fundamentally different things: as sporadic, lexically specific changes, A-changes are good candidates for misperception models of change, but that kind of model does not predict the properties of N-changes.

All knowledge is provisional, and future study may find a counter-example to this claim, but on our current state of knowledge, f ≯ θ.

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