This is a review of Samuels & Vaux (2019) *Mongolic Dorsals Are Truly Epenthetic*. It concludes that the article presents acceptable evidence that Halh (Khalkha) Mongolian has epenthesis of a consonant that has [dorsal] place of articulation at some point in the derivation, though its exact identity – /ɡ/, an empty root node, or [ɰ̟] – depends on the theory. However, the theoretical proposal that there is a rule that inserts /ɡ/ is not presented in adequate detail. Alternative approaches involving assimilation have not been addressed, particularly those in Svantesson et al. (2005) and Staroverov (2014). Finally, adequate proof that markedness theories such as de Lacy (2006)’s cannot generate Halh’s epenthetic consonant has not been presented.
Introduction

This article is a review of:

Samuels & Vaux (2019) will be referred to as ‘SV’ below.

This document is an unusual review for me in two ways: (1) it is not anonymously written, so I will address potential issues with the author/reviewer (i.e. myself) below; (2) it is written not only for the authors of SV, but for a broader audience. The second point means that I have written this review in a more detailed way than I would have for SV alone. SV know a great deal about phonological theory, markedness, and Mongolian dialects, so this review would have been fairly short – even if telegraphic – if it was just for them. However, as this review may be read by phonologists who may not specialize in the areas under discussion, I have tried to be explicit even when that meant belaboring points that would be obvious to the authors.

Goals

SV has three broad goals:

Goal 1: Theory of /g/-epenthesis
To propose a theoretical mechanism (i.e. a rule) of /g/-epenthesis, set within a particular rule-based framework.
Goal 2: Evidence for /g/-epenthesis rule
To argue that Halh (Khalkha) Mongolian has alternations (called ‘GZA’) that are evidence for the /g/-epenthesis rule, and thereby its larger theoretical framework.

Goals 1 and 2 are discussed and elaborated throughout the article: e.g.

- “Synchronously, GZA in Khalkha Mongolian can be captured with a traditional rule based account in the spirit of Lexical Phonology.” (p.30).
- “[GZA involves] epenthesis of a phoneme /g/ with varied surface manifestations” (p.2)
- “when a vowel-initial suffix is added to a vowel-final stem, /g/ is inserted to avoid hiatus (Svantesson et al. 2005:55).” (p.9).

Goal 3: Alternatives Fail
The third goal is to argue that certain alternative theories are inadequate because they cannot generate Halh’s epenthesis (or, more specifically, the relevant alternations).

SV focus on two kinds of theories: Staroverov (2014)’s splitting theory of epenthesis, and theories with markedness mechanisms in which [dorsal] is marked (e.g. Lombardi 2002, de Lacy 2006).

- “… markedness-based and splitting-based accounts face difficulties with GZA [i.e. Halh’s epenthesis]” (p.29)

For markedness theories, the problem is particularly acute. Some markedness theories consider dorsal place of articulation to be highly marked. So, if epenthesis inserts unmarked consonants, how can dorsals ever be epenthetic?

Reviewer’s note #1: SV discuss Staroverov (2014)’s [Hereafter ‘S14’] theory in detail – I supervised this dissertation (see further comments below).
Reviewer’s note #2: SV discuss markedness theories, specifically mentioning de Lacy (2006) – I wrote this book (see further comments below).

SV propose that both S14’s theory and markedness theories “face difficulties” with Halh (p.29), and that Halh is “unexpected” for those theories (p.29), and additionally that S14’s theory has “theoretical and empirical problems” (pp.1-2). It is not clear to me what exactly these terms mean. However, a Generative theory faces difficulties – in fact, outright failure – when it cannot generate an attested system. So, I will assume that SV’s goal is to show that alternative theories either cannot generate Halh’s alternations or that the additional mechanisms they would need to do so are fatally flawed.

‘Truth’

The title of SV is “Mongolian dorsals are truly epenthetic.”

There are two issues with the title that I encourage the authors to resolve.

One is ambiguity: a possible reading of the title is that all Mongolian dorsals are epenthetic. That’s clearly not so. A title like “Mongolian epenthesizes dorsals” would eliminate the ambiguity.

The other issue is more fundamental, and perhaps reveals something about the substrate of the argument in the article: what does “truly” mean? (I mention this point here because it sets the stage for a good deal of the discussion below.)
For many people, ‘truth’ means something like ‘incontrovertible fact’. So, what is ‘incontrovertible fact’ in a theoretical context? Perhaps ‘truly’ is an appeal to meta-theory of some kind: the idea that there is a theory of theories, and that meta-theory necessitates the interpretation of specific Mongolian data as epenthesis. If so, the authors should explain what this meta-theory is.

I believe it is a better path to define ‘truth’ within a particular theory. For example, if the title was “Mongolian dorsals are epenthetic in SPE”, then the argument would involve showing that the theory presented in SPE (as in Chomsky & Halle 1968) can only generate the data from Mongolian by using rules called ‘epenthesis’. Even more specifically, we can define “epenthesis” in SPE as a rule $\emptyset \rightarrow \alpha \beta \gamma$ (cite page number). So, we would specifically be claiming that all of the SPE rule systems that could generate the Mongolian data involve a rule of the form $\emptyset \rightarrow \alpha \beta \gamma$ (or in this case $\emptyset \rightarrow g/V_V$ – I believe). The title could then be changed to something like “In SPE’s theory, Mongolian necessarily has epenthetic dorsals”.

If the title instead means something like “Mongolian dorsals are epenthetic in all extant phonological theories”, then this assertion is clearly wrong. As the authors point out, in Staroverov (2014)’s theory the Mongolian data is analyzed as splitting, not epenthesis. (See §2.3 below for further discussion).

The other question is whether the title actually accurately encapsulates the authors’ point. One of the authors’ points is that there is a theory in which the only way to generate the Mongolian data is through a computational system that involves epenthesizing dorsals (with ‘epenthesis’ defined in the theory, of course). The authors’ other point is that specific theories that incorporate markedness – like de Lacy (2006)’s (deL) – cannot generate the Mongolian alternations. However, as we see below in §2.5, the mere fact that the
Mongolian data must be analyzed as involving epenthesis in deL’s theory does not actually have any bearing on the validity of the theory itself. What does have bearing is if deL’s theory cannot generate the Mongolian epenthesis. As we shall see in §2.5, the paper hasn’t demonstrated this, and there’s reason to believe that the deL theory can generate the data.

So, the title as it stands doesn’t convey the authors’ key point. Perhaps something along the lines of “No markedness theory can generate Mongolian epenthesis” would be right. As we shall see in §2.4 and §2.5, this title is as yet unproven in SV, but the title would at least convey the article’s goals.

**EVALUATION OF GOALS**

In this section I summarize my evaluation of the article’s goals, as well the article’s novelty.

- **Goal 1: Theory of /g/-epenthesis**
  There are clear steps towards achieving Goal #1, but it requires elaboration to achieve: the article does not yet provide a clear account of either the theoretical framework, or of the specific analysis.

  The framework is said to be “a traditional rule based account in the spirit of Lexical Phonology” (p.29). It is not clear to me what this means—there are a variety of theories that could be described in this way. In addition, the article implies that the proposed theory does not have any markedness mechanisms, even though such mechanisms are ubiquitous in many rule-based theories (e.g. SPE ch.9).

  The article proposes a “rule of /g/-insertion” that is described as “when a vowel-initial
suffix is added to a vowelfinal stem, /g/ is inserted to avoid hiatus” (p.9). However, the rule is not clearly stated (e.g. \( \emptyset \rightarrow g / V+_V \)).

The article should not only provide an explicit statement of the proposed theoretical mechanisms (i.e. the rule) of epenthesis, but of the rules that interact with it. As the article notes, the quality of the epenthetic consonant is influenced by its vocalic environment. A full derivation should be provided, showing how the rules take an input such as /ɔɔ-AAs/ and transform it into [ɔɔʁ̞ ɔs] (p.9).

The reason for such explicitness is that it allows SV’s proposed theory to be evaluated on its own merits: specifically, does the proposed theory generate what it needs to generate? After all, SV proposes that /g/ is epenthesized, yet no observable (i.e. output form) ever has an epenthetic [g] in it – it always spirantizes to [ɰ] or [ʁ̞] depending on the ATR environment.

It is also not clear at this point how the theory accounts for the difference in behavior between underlying stem-final /g/ and the epenthetic consonant: the latter assimilates in ATR, while the former does not (see §2.2 below).

**Goal 2: Evidence for /g/-epenthesis**

I believe that SV meets current standards of evidence for showing that a rule-based framework – and certain other Generative theories – necessarily requires an epenthesis mechanism for accounting for Halh’s alternations.

Specifically, SV argue that the mapping from certain underlying forms to surface forms introduces a consonant in a well-defined environment (in hiatus after stem-final vowels):
e.g. /ɔtɔ-As/ → [ɔtɔʁɔs] involves the insertion of [ʁ] (or something that later turns into [ɣ]).

All the evidence for these claims is present in the article, but it would be clearer if it was consolidated into one place (see comments below).

- **Goal 3a: Against Splitting**

SV focus on two kinds of theory: S14’s theory of splitting, and ‘markedness’ theories such as Lombardi (2002) and de Lacy (2006).

S14’s theory of epenthesis is significantly different from the authors. It is set in a parallelist OT framework with markedness mechanisms, and – crucially – with Correspondence Theory. SV do not argue that S14 fails to generate Halh – in fact, S14:ch.7 explicitly discusses Halh’s consonant epenthesis and how the theory generates it. Instead, SV argue that S14’s theory fails to generate other cases.

However, SV do not provide an adequate proof that S14’s theory fails: i.e. SV does not demonstrate that there is no ranking in S14’s theory that generates the requisite cases, or that such a ranking requires an implausible constraint. Without such a proof, SV’s evaluation of S14’s theory is largely surmise.

In addition, SV miss an opportunity to engage with S14’s more general point: that Halh’s consonant epenthesis involves pervasive assimilation, and this assimilation determines the quality of the epenthetic consonant.

- **Goal 3b: Against Markedness**

SV argue against theories with markedness mechanisms, and specifically those in which [dorsal] is a marked place of articulation, citing Lombardi (2002) and de Lacy (2006) (e.g. p.18).

SV asserts that such theories cannot generate (“have difficulties with”) Halh’s
consonant epenthesis. However, Staroverov (2014:150) asserts the opposite: “this markedness theory [de Lacy 2006] predicts that epenthetic dorsals must arise through assimilation... These formal mechanisms provide a possible account of Mongolian dorsal/uvular epenthesis.”

However, neither SV nor Staroverov (2014) provide any proof. To show that the Lombardi (2002) or de Lacy (2006) theory cannot generate Halh’s consonant epenthesis requires demonstrating that there is no ranking in the theory that can generate it, or any putative ranking would require an undesirable constraint.

In fact, a brief evaluation of the de Lacy (2006) theory suggests a path towards generating Halh’s consonant epenthesis.

• **Novelty**

Articles must present something new, so here I evaluate SV’s novelty.

• Regarding **empirical novelty**, SV is not alone in claiming that Khalkha (Halh) Mongolian has an epenthetic dorsal consonant. Outside the language-specific literature, Staroverov (2014:ch.7) argues the same empirical point. Within the language-specific literature, it seems the usual view that Halh has consonant epenthesis (e.g. Svantesson et al. 2005: 55).

However, the value of SV is in providing a focused, detailed account of Halh consonant epenthesis that not only discusses synchronic alternations, but discusses diachrony as well. I therefore consider SV to make a valuable contribution, though perhaps previous work that has agreed with their assessment that Halh has an epenthetic dorsal could be further highlighted.

• Regarding **theoretical novelty**, I believe SV could make a significant contribution. It would be valuable if they specifically laid out a rule-based account of Halh, particularly
since there is a competing rule-based account in Svantesson et al. (2005) that does not involve insertion of /g/.

SV is not alone in claiming that certain markedness theories (e.g. de Lacy 2006’s) either have difficulties (Staroverov 2014§7.5) or simply cannot generate Halh epenthesis (e.g. Anderson 2016 – “[de Lacy & Kingston 2016] suggest, we should find instances of epenthetic [t,d] but not of [k,g]”). However, SV go further than other discussions that I have seen in examining a variety of different alternative analyses of Halh epenthesis, and so I see their contribution as valuable and welcome.

In conclusion, I recommend that the article be revised to address the theoretical issues raised in this review. On receiving a satisfactory response, I see no impediment to its publication.

STRUCTURE AND THEORY NEUTRALITY

SV is a very ‘theoretical’ – as opposed to ‘descriptive’ – article: it makes theoretical proposals and provides detailed discussion of alternative theories, and its goal is to make an explicit theoretical argument: that theories that employ markedness mechanisms fail.

However, the current structure of the article obscures its theoretical nature. A major reason is the presentation of Halh’s epenthesis in section 1 and 2. I believe these sections are an attempt by SV to state the empirical evidence in as theory-neutral a way as possible. For example, SV refer to the evidence as ‘/g/-zero alternations’ (p.14) rather than ‘/g/-epenthesis’.

Unfortunately, any attempt at theory-neutrality, or atheoretism, is inherently doomed (Medawar 1969). Discussion of any linguistic data is heavily theory-laden, and consonant
Epenthesis provides a good illustration. For SV, the Halh alternations are evidence of a rule of /g/-insertion. However, for S14:ch.7, they are not evidence of insertion, but of a mechanisms which a vowel breaks into a consonantal and vocalic piece, both preserving elements of the underlying segment. For de Lacy (2006)’s theory, on the other hand, the alternations are evidence that epenthetic segments can be affected by assimilation as well as default features.

Similarly, SV’s theory forces (I assume) the use of a rule of /g/-epenthesis to account for Halh. However, for S14 and de Lacy (2006), the alternations do not provide evidence of /g/ epenthesis, but rather of insertion of [ɰ/ʁ̞]. In other words, the serialist, Lexicalist nature of SV’s theory requires a fundamentally different description than in output-oriented theories.

The present point is therefore structural: evidence for a theory should not be presented before the theory itself. So, I recommend that SV start by stating the theory (i.e. that there is a rule of /g/-epenthesis in a particular framework) and then present the evidence for the theory (i.e. the Halh alternations).

The article could then turn to evaluating other theories in a similar way: by discussing the theory, then the Halh alternations as potential evidence (or anti-evidence).

There are a variety of ways to structure a paper to achieve these points. However, placing discussion of the Halh alternations before the theoretical proposal makes such an approach difficult, and is liable to inadvertently lead to backgroundering of crucial aspects of the theoretical proposals.

If I was writing the article, I would take the following tack:

1. Introduction
   • Sketch of the theoretical proposal
   • Sketch of the evidence
   • Outline the major problems with other theories

2. Theory
Introduce the theoretical framework

Show how a rule of /g/ epenthesis is admissible in this framework

3. Evidence

Argue that the Halh alternations provide evidence for the /g/ epenthesis rule

4. Other theories

4a. S14’s Splitting Theory

Argue that S14 cannot generate the Halh alternations.

4b. Markedness Theories (with marked dorsality)

Take a specific markedness theory – e.g. de Lacy (2006) – and show that it cannot generate the Halh alternations.

5. Conclusions

I realize that in this structure there is no room for the current section 3 “Diachronic Development of GZA”. I believe it would be valuable to write a separate paper based on this section. The article’s arguments are about synchronic theories, so at the moment the section is a long detour, and diverts attention from the article’s synchronic goals.

PROPOSED REVISIONS

I believe that SV could make a valuable contribution to theoretical phonology. I recommend the following steps be taken:

(1) Provide adequate detail about the proposed theoretical framework and derivational mechanism.

-- i.e. state the rules, and show derivations (see more discussion below).

(2) Consolidate the Halh alternations and arguments about underlying forms and mappings.
(3) Rethink the spirit of S14’s proposal: that the quality of the Halh epenthetic consonant is due to assimilation.

(4) Focus on a specific markedness theory (e.g. de Lacy 2006’s), and provide a proof that it cannot generate Halh.

I provide more discussion in the Options section (§2.6) below.

If the issues above were addressed, I see no impediment to publication.

THE REVIEWER

I (Paul de Lacy) wrote this review alone.

• SV argues against the theories in de Lacy (2006) and de Lacy & Kingston (2013), which I (co-)authored.
• I supervised Staroverov (2014) (but not Staroverov 2016), which SV also argues against.

To some extent, I also disagree with the de Lacy (2006) theory (especially in regard to sonority-driven stress – Shih 2018). However, there may be a residual emotional connection to those works which precludes my complete objectivity. Consequently, the authors (and other readers) may reasonably infer that my arguments could unconsciously lean towards supporting the de Lacy (2006) and Staroverov (2014) theories, and so adopt an appropriately skeptical stance.

THE LANGUAGE’S NAME

SV refer to the language in question as “Khalkha Mongolian”. This is the name most
often used in phonological literature. However, in accord with Staroverov (2014:140), I suggest that it be noted that the language’s alternative names are “Halh (Mongolian)”, and “Central Mongolian”. Svantesson et al. (2005) (S05) initially refers to the language as “Halh (Khalkha) Mongolian”. S05 is more specific in identifying it as “Standard Mongolian, the Halh (Khalkha) dialect as spoken in Ulaanbaatar, the capital of the Republic of Mongolia” (S05:xvii). S05 generally refers to it as “Halh” throughout, as does Staroverov (2014).

So, I recommend for SV and all future work in phonology on Khalkha (Halh) Mongolian that it be mentioned on first reference as “Khalkha (Halh) Mongolian” or “Halh (Khalkha) Mongolian”. It would also be useful to be as specific as S05 about where this particular variety is spoken (for reasons in e.g. de Lacy 2009), and that it is probably also about the formal speech register (cf., for example, Karlsson 2005).

### 1 Halh Consonant Epenthesis

#### 1.1 Overview

One of the central questions of SV is “Does Halh have an epenthetic dorsal consonant?” It is not possible to answer this question outside of a particular theoretical framework. However, the theories that SV focus on – all transformational Generative ones – share properties that allow us to address the issue in general terms. Theory-specific issues are addressed below.

So, in this section I ask, for (most) Generate theories:

(a) Have SV provided adequate evidence that there is consonant epenthesis in Halh?
(b) Have SV adequately identified the featural content of the epenthetic consonant?

For (a), I conclude that for most Generative theories (particularly for the ones that have
an epenthesis operation), the answer is that SV has provided adequate evidence.

For (b), I conclude that SV have adequately identified some features of the epenthetic consonant. Importantly, the Place of Articulation of the output segment is dorsal – either velar or uvular depending on the vocalic (ATR) environment. However, the Manner of Articulation is still murky. It is possible that SV’s inserted /g/ is a sonorant (Svantesson 1995 and S05). From an output-oriented theoretical point of view, I believe that SV establish that the epenthetic consonant is either a fricative or – more likely – an approximant. However, further clarity is desirable.

1.2 Epenthesis

In many Generative theories, an epenthetic segment is one that has no counterpart in its preceding derivational form (for definition, e.g. de Lacy & Kingston 2013; cf. Staroverov 2014 for an alternative theory). In 2-level Optimality Theory with Correspondence Theory, an epenthetic segment is an output segment that has no correspondent (whether in the input, reduplicative base, derivational base, and so on) (McCarthy & Prince 1999). In rule-based theories, an epenthetic segment is one that is introduced into the derivation by a rule with the form $\emptyset \rightarrow \alpha/\beta_\gamma$.

So, evidence for Halh epenthesis requires phonological forms that lack a consonant that is not present in the preceding derivational form. Alternations provide evidence for input→output mappings in all Generative theories (non-alternating forms also provide evidence to varying extents depending on the theories – see de Lacy 2009 for discussion). So, alternations are needed to prove that Halh has consonant epenthesis.

The alternations provided in SV§2 are therefore crucial: they all provide alternations which suggest this exact pattern.

For example, [ɔtɔ] ‘now-NOM’ cf. [ɔtɔʁɔs] ‘now-NOM+ABL’ (5c) suggest the input
The two crucial parts of the argument are (1) that the root is not */ɔtɔʁ̞/ (or */ɔtɔɡ/ or */ɔtɔɡ/, depending on one’s theory), and (2) the suffix is not */-ɔs/. SV explicitly address these points in SV§4.1:

- The root cannot be */ɔtɔɡ/ underlingly because the nominative would presumably surface as *[ɔtɔɡ] (crucially, [ɡ/ɡ]-final roots are grammatical – e.g. [pag], [zaɡ], [uːɡ] – SV§4.1).
- The suffix cannot be */-ɔs/ because in C-final roots it appears as [ɔs] (e.g. [nɔm-ɔs], *[nɔm.ɔs]), and – crucially – [C.ɡ/ɡ] clusters are permissible (e.g. [pɔsɔχ] – SV§4.1).

There are other important parts to the argument, as discussed by de Lacy & Kingston (2013) and de Lacy (2006), all of which SV address. One is in providing a clear definition of the phonological environment. SV do this on p.9: “when a vowel-initial suffix is added to a vowel-final stem”. It is of course possible that other descriptions of the environment would work depending on the theory (e.g. ‘syllable onsets’), but SV’s description seems accurate and to the point.

SV also observe that the appearance of the Halh consonant is found in almost all vowel-initial suffixes (23 of them). Similarly, Staroverov (2014:280) observes: “dorsal epenthesis is not morphologically restricted”. This argument means that a suppletive analysis – where all relevant suffixes have two forms, one with a dorsal consonant and one without – is highly unlikely (i.e. not likely to be acquired by a learner). SV give more details regarding suppletive allomorphy in SV§4.2. The only exception is the infinitive/future participle, which surfaces as [-x] after vowels and [-ɔx] after consonants; it is possible that this suffix is has phonologically-conditioned suppletive allomorphs as SV argue that it can neither be /ɔx/ or /x/ (p.14).

1 Note that I use Times New Roman font for IPA symbols – the journal’s recommended font Liberation Serif misaligns diacritics, as is the case in SV’s PDF file.
At this point, I consider the evidence that Halh has synchronic consonant epenthesis (for Generative theories that allow it) compelling: there are underlying forms lacking a consonant that map to surface forms that have one in a well-defined environment. There are no restrictions: the epenthesis is phonologically general.

What about productivity? SV’s point that all (but one) suffix induces epenthesis suggests that the process is productive. SV also identify loanword roots that undergo epenthesis: e.g. pitssta-gaa ‘pizza-ABL’ (p. 12), and generally note “The [epenthetic] /g/ appears in hiatus configurations regardless of whether the stem is native or borrowed.” (p.14). Loanwords are useful because they possibly behave like a wug test (or at least at some point in their adoption they were effectively wug words).

In fact, SV also discuss wug tests. Novel vowel-final roots should undergo consonant epenthesis when suffixed, just as novel words in English are suffixed with the appropriate plural allomorph (Berko 1958). SV’s discussion focuses on Staroverov (2016)’s results from such a wug test for Buryat, a related language (SV p.21-22). Staroverov (2016) reports the results of a wug test to argue that g-epenthesis is not productive in Buryat. SV provide a variety of objections to the form of Staroverov (2016)’s experiment. However, SV’s conclusion is that Staroverov (2016)’s results “says … nothing at all about GZA [g-epenthesis] in Khalkha.” (p.22).

It is of course correct to point out that the results of a wug test for one language does not say anything about another language. However, if this is SV’s conclusion, then it seems to me that they could dismiss Staroverov (2016)’s results by simply pointing out that they are not about Halh.

The relevant issue for SV is whether a wug test for Halh would show consonant epenthesis. SV do not address this issue, and it is not clear to me why not. It would be worthwhile for SV to state clearly whether they think a wug test might be probative (perhaps not, as suggested by the reference to Pierrehumbert 2006 on p.22). I am not suggesting that SV actually perform a wug test – that is not (yet) a standard evidential procedure for epenthesis in phonology. However, as it stands, it is unclear what the article is
advocating: i.e. are SV asserting that wug tests do not ever provide evidence for underlying forms? Or that wug tests on Halh would be pointless for other reasons?

SV also discuss the diachronic development of Halh epenthesis. It is controversial whether diachronic change necessarily informs us about synchronic processes (e.g. de Lacy 2009, de Lacy & Kingston 2013). In any case, SV’s synchronic arguments are adequate to establish the claims. So, I feel that the diachronic section could be removed and developed into its own article without detriment to the current argument. The downside of keeping it is mainly expository: in its current form, the diachronic section is a large detour in the overall argument.

In short, I believe SV have provided evidence for Halh epenthesis from a Generative perspective that accords with the usual standards found in the literature. In particular, they successfully addressed the evidentiary pitfalls described in de Lacy (2006) and de Lacy & Kingston (2013). In short, I believe this paper makes a valuable contribution in laying out a solid case for Halh consonant epenthesis, with appropriate acknowledgments to previous work on the language which make the same assertion (in particular S05 and Staroverov 2014).

### 1.3 Featural Content of the Halh Epenthetic Consonant

Have SV adequately identified the featural content of the epenthetic consonant?

A confusing part of the discussion of Halh epenthesis is exactly what is inserted in terms of phonological features. Some say that Halh inserts a [g] (or [c] depending on the environment) (e.g. Anderson 2016; Staroverov 2014). However, SV are clear that such statements are incorrect.

Certainly, what is inserted phonologically is highly theory-dependent. For a rule-based theory with a relatively early (i.e. lexical stratum) insertion of the epenthetic segment, it seems potentially defensible to argue that a /g/ is inserted, as SV do. However, for output-
oriented theories, SV’s work indicates that an approximant [u̯] (or [ʁ̞]) is inserted.

So, I ask two questions:

1. Have SV provided adequate evidence that the epenthetic consonant is /g/ in their rule-based theory?
2. What is the phonological output form (i.e. phonological features) of the epenthetic consonant?

The second question is important because SV discuss several alternative theories, including those that focus on the output form of the epenthetic consonant inserted.

- Is the epenthetic consonant /g/ or something else?

SV are careful to state that the epenthesis involves “a phoneme /g/”. SV then point out that subsequent phonological processes change the realization of /g/ (“this phoneme surfaces intervocically as a spirant” – i.e. [u̯/ʁ̞]).

In other words, a linguist will always hear the realization of the epenthetic consonant as an approximant (spirant) [u̯/ʁ̞], and never as a stop [g]. So, how do SV come to propose that the epenthetic consonant is /g/ at its earliest point in the derivation?

I can made an educated guess about how SV came to this conclusion based on the statement that SV’s proposal is “a traditional rule-based account in the spirit of Lexical Phonology” (p. 29). However, SV should spell out exactly how the proposed theoretical precepts necessitate a rule of /g/ epenthesis.

For example, if one adopts Lexical Phonology (e.g. Kiparsky 1982), then one could reason that [u̯/ʁ̞] are allophones, not phonemes, so cannot be introduced into the Lexical stratum. One would then argue that epenthesis was a Lexical (not post-Lexical) process (perhaps because it is word-bounded). If so, then [u̯/ʁ̞] cannot be inserted directly – only some phoneme can, leading to the proposal that /g/ is inserted.

It is possible that SV assume that the reader is familiar with rule-based theories and Lexicalist precepts to make these links on their own. However, it would be valuable to
be explicit and explain why exactly it is /g/ that is inserted.

After all, there is an extant counter-proposal, also set in a Lexical Phonology rule-based framework: i.e. S05:55-56. S05 propose that consonant epenthesis “consists of the addition of a consonant without place features”. S05’s proposal is built on the reasoning that “The feature [velar] is redundant for the Halh vowels, and it is possible to specify the consonants as well without referring to it, so that velars are unmarked consonants without any place feature.” (s05:55).

SV do not discuss S05’s proposal. However, I recommend that they do so because hopefully it will explain why SV’s theory necessitates that Halh epenthesis cannot involve the insertion of a placeless consonant. S05’s counter-proposal also directly challenges SV’s claim that it is a dorsal that is inserted in Halh; S05’s proposal is that an empty consonant is inserted, and its dorsality is added/realized later in the derivation.

In conclusion, SV should provide a clearer statement of the theoretical precepts that lead them to propose that /g/ is inserted, rather than some other segment (or an empty segment, as in S05). I do not believe this needs to be a long or onerous discussion – a simple statement about which theory they adopt would be sufficient.

- **Is the epenthetic consonant an obstruent or sonorant?**

S95 (and S05) makes observations about Halh /g/ and /ɡ/, concluding that “these consonants function as sonorants”. The evidence is that:

1. [g ɡ] form codas with a following obstruent, while otherwise only sonorants do (e.g. [gtʰ gʃʰ gtsʰ ɡt ɡʃ gts ɡʃ ɡs] are possible codas, as as [mtʰ mtʃʰ mtsʰ mp mt mtʃ ms mx], and so on for [mC nC nC ʃC ʃC rC rC wC wC jC] (S05:66)).
2. [g ɡ] almost never appears as the second member of a coda cluster, while other obstruents do (e.g. [mp mt mtʃ mtʃʰ mtsʰ] but *[mg]). [g] does appear after [ŋ] (i.e. [ŋg]), but this may be ascribed to assimilation to the dorsal nasal – S95:759).
What makes the significance of these comments somewhat uncertain is whether \( /g\ g/ \) remain stops in codas, or spirantize. As SV note (p.10), “In the\ V\_C context underlying \( /g/\) can also surface as a stop or fricative”.

Another relevant observation about \( [g\ g] \) is in Halh’s native segment inventory (e.g. S05:25):

(1) *Halh contrastive consonants from S05:25*

\[
\begin{array}{cccccc}
(p^{h})(p^{i}) & t^{h} & t^{i} & g^{i} & g & g \\
p & p^{i} & t & t^{i} & g^{i} & g \\
\text{ts} & \text{ts}^{h} & t^{i} & t^{i} & g^{i} & g \\
n & n^{i} & n^{i} & n^{i} & n^{i} & n^{i} \\
l^{j} & l^{j} & l^{j} & l^{j} & l^{j} & l^{j} \\
r & r^{i} & r^{i} & r^{i} & r^{i} & r^{i} \\
w & w^{i} & j \\
\end{array}
\]

One immediately notices that \( [g\ g^{i}\ g] \) are somewhat featurally isolated, having no voiceless counterparts (i.e. \([k\ k^{i}\ q]\) – except in some loanwords).

In other words, if even lexical \( /g\ g^{i}\ g/ \) are phonological sonorants, what implications does this have for SV’s epenthesis rule? In other words, what exactly does SV’s epenthesis rule insert? A \([+\text{sonorant}]\) velar, or a \([-\text{sonorant}]\) velar? If it is \([+\text{sonorant}]\), is it correct to refer to the epenthetic consonant as a “\(/g/\)”, implying that it is an obstructuent?

Of course, a possible rejoinder could be that SV’s goal is to show that the Halh epenthetic consonant is *dorsal*, so its manner of articulation is not really relevant. However, there are two good reasons to address the issue of manner of articulation. One is for the sake of precision: it would be useful to know exactly what SV are proposing for Halh. More importantly, the manner of articulation of the epenthetic segment turns out to be a
significant issue in the evaluation of other theories – Staroverov (2014)’s and OT markedness theories like de Lacy (2006)’s. I will discuss this point further below in section 2.

- **What is the output form of the Halh epenthetic consonant?**

I believe that one of the most valuable contributions of SV’s paper is in making headway on the issue of the output form of the Halh epenthetic consonant. As they succinctly state: “when we speak of [g-epenthesis] we refer to the phoneme /g/, which … is typically realized as a dorsal fricative or approximant by virtue of being intervocalic.” (p.11).

Knowing the epenthetic consonant’s output form is crucial for both rule-based and parallelist theories. For rule-based theories, it is crucial to know what post-insertion rules the epenthetic consonant undergoes to see whether its featural content could be ascribed to those rules. For output-oriented theories, it is crucial to know what the surface form of the epenthetic consonant is because this is the form that is evaluated for well-formedness.

The Halh epenthetic consonant only shows up intervocalically: i.e. “when a vowel-initial suffix is added to a vowel-final stem” (SV p.9). SV observe that:

“Stuart & Haltod (1957) note that /g/ becomes a voiced dorso-velar lenis fricative intervocalically… Our native speaker consultant also spirantizes under certain conditions in this context…” (p.10).

SV’s summary in table 6 states that the epenthetic consonant would be realized as [ɣ] or [ɰ] in [+ATR] contexts, and [ʁ] or [ʁ̞] in [−ATR] contexts.

Of course, it is still not ideal that we do not know whether the epenthetic consonant surfaces as a fricative or approximant, or whether there is inter-speaker variation. It seems that SV lean towards the idea that it is an approximant because the allophonic transcriptions have the approximant forms (e.g. [tsaʁəs] – p.9).
The question, then, is what phonological manner features the epenthetic consonant has in the output. Several authors, e.g. Gonzalez (2009:198), analyze the spirantized stops in various Spanish dialects as [+sonorant, +continuant] approximants – i.e. [β̞ ð̞ γ̞]. So, it would therefore be unsurprising if the phonological MoA of the Halh epenthetic segment was [dorsal, +sonorant, +continuant].

So, in most of the following discussion, I will assume that the epenthetic consonant is an approximant in the output, returning to the possibility that it is a fricative later on.

In short, the Halh epenthetic consonant certainly has the phonological feature [dorsal] in the output. However, it is not clear that it must have a [dorsal] feature when it is introduced into the derivation in rule-based theories – S05 propose that it does not.

The manner of articulation of the epenthetic consonant is less clear. S95 and S05 argue that it is [+sonorant] at all stages in the derivation. The output epenthetic form is either a fricative or an approximant – i.e. [+continuant], and perhaps [+sonorant,+approximant].

I believe that SV make a significant contribution by providing details about the epenthetic consonant’s realization. This information is important regardless of one’s phonological theory. However, SV should go deeper in discussing the featural content of the inserted segment – must it be [dorsal] when it is first introduced into the derivation? And what is its manner of articulation, both when it is introduced and in the output form?

1.4 SUMMARY

I believe that SV present reasonable evidence that the majority of Generative theories must model Halh as having an epenthetic consonant. However, I believe that there are still steps to make to conclude that there is epenthesis of /g/ in the proposed theoretical framework, as opposed to some other segment. I also believe it would be rather straightforward to identify those properties of SV’s rule-based Lexical Phonology theory that
necessitate a /g/-epenthesis rule for Halh. Discussing S05’s proposal that a placeless segment is inserted would further clarify SV’s theoretical proposals.

The epenthetic consonant’s manner of articulation is less certain. It may be [+sonorant] at all points of the derivation in rule-based theories, after S95/S05. It is important for SV to discuss S95/S05’s proposal, even if only to explain how it does not affect the argument.

SV makes a particularly valuable contribution in laying out the surface realizations of the epenthetic consonant. In the output, it is at least [+continuant], but may be [+approximant], too. I recommend keeping this discussion, and perhaps adding a note on its importance for output-oriented theories.

2 THEORIES AND EVIDENCE

2.1 OVERVIEW

SV have (at least) three theoretical goals:

1. SV’s theory
One theoretical goal is to demonstrate that there is a particular theory that can generate Halh epenthesis: namely, a rule-based serialist theory with epenthesis of a phoneme /g/ “in the spirit of Lexical Phonology.” (p.29). This goal is discussed in section 2.2 below.

2. Epenthesis, not splitting
A further theoretical goal is to show that Halh consonant epenthesis is not splitting, but insertion. This goal focuses on Staroverov (2014)’s theory of consonant epenthesis as splitting: “We further detail … the theoretical and empirical problems with treating consonant-zero alternations as splitting rather than insertion” (pp.1-2). This goal is discussed in section 2.3.
3. Not markedness

A third theoretical goal is to show that Hallh’s /g/ epenthesis presents particular problems for theories with markedness mechanisms. Hallh’s “dorsal stop epenthesis … is considered in some theories to be highly marked or even impossible (see e.g. Lombardi 2002, de Lacy 2006, Ufmann 2014)” , and “epenthetic /g/ is particularly unusual and difficult to explain through either markedness or faithfulness…” (p.4).

This goal is discussed for markedness theories in general in section 2.4, and for de Lacy (2006)’s theory specifically in section 2.5.

Recommendations for revision are given in section 2.6.

It is important to note that SV refer the reader to Vaux & Samuels (2017), particularly for the evaluation of markedness theories: “These [constraint-based markedness] approaches encounter a number of difficulties, which we largely set aside here as they are discussed at length by Vaux & Samuels (2017).” (p. 3) and “Vaux & Samuels (2017) provide an overview of why [g epenthesis] is unexpected in theories that consider epenthesis to produce ‘emergence of the unmarked’.

Reviewers are presented with a conundrum when articles refer to other work in order to make a crucial part of the argument: should the review focus on the article alone, or on both the article itself and the article(s) it refers to? In this case, should this review refer to SV alone, or about SV and Vaux & Samuels (2017) (or at least the part of Vaux & Samuels 2017 that are directly relevant to SV’s arguments)? A further question also arises: is it appropriate for the article to move the burden of exposition onto other work, or should it make a complete argument in the article itself?

Personally, I understand why SV did not incorporate Vaux & Samuels (2017)’s (hereafter ‘VS’) arguments – they did not want to repeat unnecessarily. However, SV’s article is an ideal opportunity to gather both theoretical arguments and empirical evidence
about Halh in one place. Having established the generalizations about the Halh alternations, the moment is ripe to ask “Which theories can generate Halh’s consonant epenthesis?”

So, I suggest that SV directly incorporate the theoretical arguments into this paper rather than refer to Vaux & Samuels (2017). Specifically, SV would ideally directly address the three theoretical goals in terms of analyses of Halh in various theories: (1) their own, (2) Staroverov (2014)’s Splitting Theory, and (3) de Lacy (2006)’s markedness theory. The goal of (1) would be to demonstrate that they indeed have a theory that can generate Halh consonant epenthesis. The goals of (2) and (3) would be to show that a splitting theory and a markedness theory are unable to generate it.

In the rest of this review, I lay out some of the challenges with addressing SV’s three goals. It is not within my remit to review Vaux & Samuel (2017) here, so I will focus on SV’s points and arguments. I point out that demonstrating (1) is far from trivial; SV seem to have misconstrued the import of S14, and SV mischaracterize deL. In other words, SV have not shown that either S14 or deL fail to generate Halh epenthesis.

2.2 SV’S THEORY

SV’s theoretical account of Halh consonant epenthesis states:

- “Synchronically, GZA in Khalkha Mongolian can be captured with a traditional rule based account in the spirit of Lexical Phonology.” (p.30).
- “[GZA involves] epenthesis of a phoneme /g/ with varied surface manifestations” (p.2)
- “when a vowel-initial suffix is added to a vowel-final stem, /g/ is inserted to avoid hiatus (Svantesson et al. 2005:55).”
I found SV’s analysis somewhat opaque and limited. I recommend that SV expand the discussion of the proposed theory, both in terms of specifics and general details about the theoretical framework.

In terms of specifics, it would be better if SV presented an actual rule in addition to the prose description. For example: $\emptyset \rightarrow g/V+_V$ (or $\emptyset \rightarrow g/$_$V$, etc.). It would also help to situate this rule within the other rules responsible for the epenthetic segment’s final form. A reason for being formally explicit is that it allows the reader to see the pros and cons of the analysis.

For example, SV’s analysis suggests that the epenthetic consonant should behave like lexical /g/ in terms of the phonological processes it undergoes because the epenthetic consonant has the same representational form as lexical /g/. However, there seem to be differences between the behavior of lexical and epenthetic /g/.

The epenthetic consonant’s PoA varies between velar and uvular depending on the vocalic environment (e.g. SV:11). In contrast, S05:29 notes that underlying morpheme-final velars remain velar regardless of the vowels involved (e.g. [pag-as] ‘team-RFL’, [pag-ig] ‘team-ACC’) and contrast in morpheme-final onset position (e.g. [pag-as] ‘small-RFL’, [pag-ig] ‘small-ACC’).

In other words, the epenthetic consonant undergoes a process of ATR assimilation that underlying lexical /g/ does not. SV should provide an account of this in their theory. At first blush, it seems problematic: if the epenthesis rule introduces a /g/ that is indistinguishable from lexical /g/, then why should an ensuring vowel assimilation rule not apply to both? (The situation is even more complicated – it turns out that lexical /g/ does undergo vowel assimilation, but only in word-initial position.)

In other words, it is not clear to me that SV have yet proposed an analysis that can generate Halh’s epenthetic segment and account for its output form.

In this regard, I also recommend that SV compare their theory’s predictions for Halh with those of other extant theories: i.e. S05’s and S14’s. In S05’s analysis, consonant epenthesis “consists of the addition of a consonant without place features” (S05:55). So,
the epenthetic consonant is (at least initially) representationally distinct from the lexical /g/. The lack of place features allows the epenthetic consonant to take its features from its environment, so S05’s theory offers a ready explanation for why epenthetic consonants assimilate in ATR while the lexical one does not. Similarly, for S14, the epenthetic consonant corresponds to an underlying vowel while lexical [g] correspond to underlying consonants (i.e. /g/); S14 shows how this formal distinction allows the epenthetic consonant to undergo vowel assimilation.

In other words, it is crucial for SV to prove that they in fact do have an analysis of Halh consonant epenthesis. At this point, it is not clear to me whether their account will actually work. In any case, if one asserts that one has a theory that generates a particular phonological system, one must prove it: in SV’s framework, that would involve identifying a set of rules, a rule order, and showing how the derivations work.

As mentioned above, it is also desirable for SV to compare their epenthesis rule to S05’s one, and show why the epenthetic consonant must have velar PoA features, as opposed to a segment with no place features. After all, SV’s theoretical point is that there are rules of epenthesis that insert velars. Given that S05’s proposal denies this claim, SV should address the issue of why they believe S05’s proposal to be incorrect.

A final issue that I would like to see addressed is the issue of markedness in SV’s theoretical framework. SV contrast their theory with theories that incorporate markedness mechanisms, so the reader could infer that SV’s theoretical framework lacks markedness mechanisms. It is important to know whether this is the case, and what it means, exactly.

To explain, in de Lacy (2006) [hereafter deL], ‘markedness’ refers to mapping asymmetries. A grammar can be conceived broadly as a set of input→output mappings, and then one can ask ‘Are all grammars possible?’ (given a particular representational definition of inputs and outputs). If the answer is ‘no’, then we can refer to the missing grammars as showing ‘markedness’. For example, deL claims that grammars in which /t→[k]
(in codas outside of assimilatory contexts) do not exist – i.e. markedness. Consequently, I assume that SV are claiming that their theoretical framework can have any grammar.

However, this is a tough claim to make. A great deal of care must be given in defining ‘anything’. In rule-based theories, I believe this might mean that there can be rules of the form $\alpha \rightarrow \beta / \gamma$ where $\alpha$, $\beta$, and $\gamma$ can be any bundle of features. However, even with such freedom in $\alpha$, $\beta$, and $\gamma$, can such a theory do anything, where ‘anything’ means ‘can generate all possible input→(intermediate forms...)→output mappings’? It would be surprising if the theory could because it is still (presumably) limited by its rule definition language (e.g. discussion in de Lacy 2011): there may be limits on the form of rules – e.g. on the distance between elements (e.g. is $\alpha \rightarrow \beta / ... \gamma$ a possible rule?). For example, SV seem to imply that Halh epenthesis involves insertion of a phoneme /g/ because at the crucial point in the derivation, it is not possible to insert an allophone – i.e. If so, this Lexicalist restriction is a major limit on what the theory can do. Even rule ordering itself imposes inherent limits on what the theory can do, depending on whether rules can be repeated in the derivation, or cyclicity is possible, and so on.

In short, I am not sure what SV’s criticism of markedness theories means for the theoretical proposal they espouse. It would help to have explicit statements about the theoretical framework in terms of what it can and cannot generate, and what it means for it to not have any markedness mechanisms, and whether there are limits on the grammar restrict input-output mappings.

In short, SV provide an explicit account of their theoretical framework, and the analysis of Halh consonant epenthesis and related processes (e.g. ATR assimilation). The analysis should be accompanied by explicit derivations, showing how we get from underlying forms such as /nɔm-AAs/, /ɔtɔ-AAs/, /pag-AAs/, and /paɡ-AAs/ to their output forms.
2.3 Splitting Theory (Staroverov 2014)

“We further detail … the theoretical and empirical problems with treating consonant-zero alternations as splitting rather than insertion.” (SV pp.1-2).

SV:§4.3 address S14’s analysis of Halh consonant epenthesis as splitting in some detail. ‘Splitting’ (also called ‘breaking’, and for vowels ‘diphthongization’) refers to a concept in Correspondence Theory (McCarthy & Prince 1999) where a single input element corresponds to multiple output elements. For example, in the mapping /i1/ → [j1i1], the underlying /i/ corresponds to both the output [j] and [i]; the segment underlying [j] is /i/, and the segment underlying [i] is also /i/. Splitting is distinct from insertion/epenthesis: e.g. /i1/ → [ji1] – here the output [j] has no underlying correspondent.

SV state that “[S14’s] ranking may be able to generate GZA in Khalkha…” (p.24). SV then continue to argue that S14’s theory of consonant epenthesis is wrong on general grounds.

Apart from S05, S14 provides the only other formal analysis of Halh consonant epenthesis. In addition, SV wish to bolster their theory by showing that only it can generate Halh epenthesis. So, I understand why SV discuss S14’s proposal in such detail.

However, I believe it would be worthwhile for SV to point out that S14’s analytical mechanism is simply not available in their (i.e. SV’s) theory: if one rejects Correspondence Theory, as SV do, then splitting is simply not a possible operation. In extant rule-based theories, there is no indexing of input and output segments, so there is no formal difference between Ø →j/#_i and i → ji/#_. Splitting in such theories is simply a combination of insertion followed by assimilation. In other words, Halh’s epenthesis cannot be splitting in SV’s theory because there is no operation of ‘splitting’ in their theory.

However, I believe there are a couple of important points to take from S14’s theory here. S14’s leading observation is that if one’s theory allows splitting (as in Correspondence Theory), then in a mapping /α/ → [αβ], one must consider the possibility that /α/ has
split into $[\alpha\beta]$). This possibility has been available from the beginning of Correspondence Theory (McCarthy & Prince 1995:124). The problem is that until S14, very few analysts ever considered that at least some cases of epenthesis could actually be splitting. At the minimum, then, S14’s contribution is to remind analysts that what looks like epenthesis could actually be analyzed as splitting. Certainly, S14 goes further in arguing that there is no epenthesis process – only splitting. However, any Correspondence-based theory must contend with the splitting analysis whenever anything like epenthesis is encountered; this includes de Lacy (2016)’s theory, discussed below.

However, the point that is most important for SV is in the translation of S14’s proposal into a rule-based framework. In a rule-based frameworks, splitting involves insertion+assimilation/dissimilation. For example, /i/ ‘splits into [ji] in two steps: /i/ $\rightarrow$ epenthesis [Ci] $\rightarrow$ assimilation [ji]. Diphthongization involves epenthesis, followed by assimilation, then dissimilation; for example, /e/ $\rightarrow$ [ai] might undergo the steps /e/ $\rightarrow$ epenthesis [eV] $\rightarrow$ assimilation [ee] $\rightarrow$ high dissimilation [ei] $\rightarrow$ low dissimilation [ai].

So, taking S14’s insight, we are then led naturally to the question: can Halh consonant epenthesis be explained as insertion+assimilation? This analysis contrasts with SV’s which involves direct insertion of a /g/ – i.e. the epenthetic consonant starts with its features, rather than starting featurally impoverished and adopting its features from the environment.

As mentioned above, this option was addressed in part by S05. S05 inserts a placeless consonant, and then pharyngeal features spread from pharyngeal vowels to produce a uvular consonant. For S05, placeless consonants are interpreted with velar PoA. In other words, S05 recognizes that the quality of the epenthetic consonant is at least partially determined by assimilation.

In fact, adapting S14’s analysis to a rule-based framework means that all the epenthetic consonant’s features are entirely determined by assimilatory mechanisms. S14 adopts Halle (1995)’s proposal that all vowels have the feature [dorsal]. For S14, then, the Halh
epenthetic consonant is [dorsal] because it gets this feature from the adjacent vowel (through splitting, but a rule-based theory could get it through epenthesis).

The following derivation sketches out such a process. An empty consonant is inserted (after S05). It then assimilates in place of articulation to adjacent vowels (i.e. assimilates to [dorsal]). Some mechanism then imposes its manner of articulation. This mechanism could be assimilation to the vocalic manner of articulation (i.e. [+approximant]), or due to the independent spirantization process that turns intervocalic stops into approximants.

\[
\begin{align*}
\text{UR:} & \quad /\ddot{o}\ddot{t}\ddot{o}-\text{AAs/} \\
\text{V Harmony:} & \quad \ddot{o}\ddot{t}\ddot{o}\text{-s} \\
\text{Insert empty C:} & \quad \ddot{o},\ddot{t},\text{C}s \\
\text{Assimilate V dorsal:} & \quad \ddot{o},\ddot{t},\text{C}^{\text{DORSAL}}\text{s} \\
\text{Assimilate ATR:} & \quad \ddot{o},\ddot{t},\ddot{\text{A}}^{\text{DORSAL,PAR}}\text{s} \\
\text{Manner:} & \quad \ddot{o},\ddot{t},\ddot{\text{s}}\text{s}
\end{align*}
\]

So, for SV, S14’s analysis raises a straightforward analytical question: why not ascribe the PoA of the epenthetic consonant to assimilation to vocalic features? Instead of inserting a /g/, why not insert a placeless consonant, then have an ensuring rule of C-V place assimilation result in the velar/uvular PoA?

After all, S05 and others make it very clear that Halh has assimilatory processes involving dorsals and vowels: “The phonemes /ŋ, x/ are realized as velars [ŋ, x] in non-pharyngeal words [those with the vowels a, ʊ, ɔ], and as uvulars [n, χ] in pharyngeal words [those with the vowels [i e u o]]” (S05:28). Similarly, “In word-initial position [g] behaves like the other velars and occurs only in non-pharyngeal words, alternating with ɢ in pharyngeal words...” (S05:29).

So, if assimilatory processes involving vowel and consonant PoA are necessary anyway, what is the justification for claiming that it is /g/ that is inserted, as opposed to some other consonant (e.g. a placeless one) whose PoA is then determined by assimilation? In
most general terms, this is the issue that S14 raises.

In short, I see why SV wish to show that S14’s theory cannot be correct because it competes with their account. However, SV do not address the core insight of S14’s analysis of Halh – that the PoA of the epenthetic consonant is determined by assimilation.

As for SV’s critique of S14’s theory itself, I have a few notes:

- “splitting theory violates “freedom of analysis””
  
  Freedom of analysis means that GEN applies all available operations. However, S14 proposes that consonant insertion is not an operation available to GEN. There are formal limits on GEN; for example, GEN doesn’t insert trisyllabic feet, or produce candidates with line-crossing, and so on (see articles in Blaho, Bye, and Krämer (2008) Freedom of Analysis? De Gruyter Mouton).

  I also think that SV make too much of the idea that V-insertion is a possible GEN operation. S14 focuses on consonant epenthesis, but essentially remains agnostic about whether vowel epenthesis involves insertion or splitting. (By the way, I think I should publicly take the blame here – I recall telling Staroverov that he shouldn’t broach the subject of vowel epenthesis because his dissertation would take too long to finish).

- [S14] “runs into problems with GZA in the closely related Mongolic language Dagur… Uyghur…poses a related set of difficulties”

  I recommend that SV take another approach in showing that S14’s theory cannot account for specific cases. The way to prove that S14’s theory cannot generate these cases is to demonstrate that there is no constraint ranking in the theory that can generate the requisite mappings, or that the constraints needed to avoid a contradiction are pathological. Unfortunately, any other method is liable to error as it relies on intuition (see multiple papers by Alan Prince and his collaborators – e.g. Samek-Lodovici & Prince 1999, Prince 2006, and most subsequent papers by Prince on the Rutgers Optimality Archive).
In the Dagur epenthesis case, for example, SV state that [j] is inserted in the nominal declension, regardless of its vocalic environment, and that this is a problem for S14’s theory. Is it? S14’s theory permits emergence of the unmarked, and the invariant [coronal] PoA of glide epenthesis here could be seen as a TETU effect. We won’t know until SV provide a proof that S14’s theory cannot generate Dagur epenthesis: i.e. show that there is no ranking that can generate it. The same goes for Uyghur.

In short, a claim that a theory fails to generate a specific case requires a formal proof using the logic of the theory itself. SV should provide such proofs for their claims about S14’s theory’s failure to generate Dagur and Uyghur, otherwise the claims made are essentially just suspicions, or suggestions for future research.

2.4 Markedness Theories in General

“… epenthetic dorsals are unexpected on the basis of markedness. In most Optimality Theoretic (OT) approaches, an epenthetic segment is inherently unfaithful and must therefore be relatively unmarked to emerge. However, [dorsal] is considered quite marked as a C-place (see, e.g., Lombardi 2002, de Lacy 2006)…” (SV p.18).

I suspect that SV’s primary goal is to argue against theories that have markedness mechanisms, like de Lacy (2006)’s (deL). When SV is considered together with Vaux & Samuels (2017), this goal is stated in clearer terms. For example, Vaux & Samuels (2017:69):

“the range of consonants which are chosen for insertion cross-linguistically cannot be accounted for in terms of markedness…. [we] critique several markedness-based approaches to consonant-epenthesis, including Lombardi (2002), de Lacy (2006), and Steriade (2009), concluding that a viable theory of synchronic phonology must allow for epenthesis of any segment, no matter how marked.”
SV’s exploration of these claims would be better served by asking which property of markedness theories they believe is responsible for the theories’ putative failure.

For example, SV could discuss whether their critique applies to markedness hierarchies or markedness mechanisms. SV specifically mention theories in which “[dorsal] is considered to be quite marked as a C-place” (SV p.18), so it is possible that SV are objecting to the form of a specific PoA markedness hierarchy here. After all, there are many theories that consider velar as being an unmarked PoA: Trigo (1988:90ff), Rice (1996), Causley (1999), and many others (see de Lacy 2006§8.4.2ff for discussion). For example, Rice (1996)’s theory has representational mechanisms to express markedness, with velars as lacking a Place node dependent. For this theory, Halh consonant epenthesis would involve the insertion of a placeless consonant (as in S05’s account) (followed by assimilation to pharyngeal vowels to produce contextual uvulars). So, I suspect that rather than see Halh consonant epenthesis as a problem, advocates of Rice (1996)’s theory would joyfully hail it as validation.

Other than markedness hierarchies, there are computational and representational mechanisms that implement those hierarchies. For example, deL’s theory has a specific way of implementing both output and faithfulness constraints for any markedness hierarchy, involving both conflation of markedness categories and a homogeneity of category reference across different constraint types. It is not clear whether SV believe that Halh’s consonant epenthesis also presents a problem for such mechanisms.

In short, SV’s idea that markedness theories cannot generate (or ‘have difficulties with’) Halh consonant epenthesis should be more carefully examined. Exactly which properties of markedness theories are responsible for the theories’ putative inability to generate Halh? Is the source of the problem the particular PoA markedness hierarchy where dorsals (including velars) are most marked? Or are SV arguing that having any formal expression of markedness in a theory will mean that it is unable to deal with Halh?
2.5 de Lacy (2006)’s theory and Halh epenthesis

SV (and Vaux & Samuels 2017) mention a number of markedness theories. One of them – de Lacy (2006)’s theory (along with de Lacy & Kingston 2013) – recurs in SV’s (and VS’s) critiques:

- “epenthesis of these consonants is considered in some theories to be highly marked or even impossible (see e.g. Lombardi 2002, de Lacy 2006, Uffmann 2014).”
- “numerous constraint-based approaches have attempted to limit the range of possibilities and predict the identity of epenthetic Cs based on markedness (e.g., Lombardi 2002, de Lacy 2006).”
- “[dorsal] is considered to be quite marked as a C-place (see, e.g., Lombardi 2002, de Lacy 2006)”
- “epenthetic dorsal Cs in the Mongolic languages have been met with some scepticism (e.g., de Lacy & Kingston 2013; Staroverov 2014, 2016).”

So, I take deL’s theory as a proxy for ‘markedness theories’ in general. This allows us to ask the specific question: does deL’s theory fail to generate Halh consonant epenthesis? I conclude that SV do not adequately demonstrate that deL’s theory fails. This failure is a problem because SV’s focus on Halh epenthesis seems to be motivated by the desire to show that all markedness theories are empirically inadequate.

*Why think that deL’s theory can’t account for Halh epenthesis?*

There are many assertions in the literature that the deL theory cannot produce an epenthetic [g] (or any epenthetic dorsal). De Lacy (2006:81) has a chart of possible epenthetic consonants, reproduced below, and [g] (or any other dorsal) is not marked as being potentially epenthetic.
(2) Possible ‘default’ epenthetic consonants (From de Lacy 2006:81)

<table>
<thead>
<tr>
<th></th>
<th>labial</th>
<th>coronal</th>
<th>retroflex</th>
<th>velar</th>
<th>uvular</th>
<th>pharyngeal</th>
<th>glottal</th>
</tr>
</thead>
<tbody>
<tr>
<td>oral</td>
<td>p</td>
<td>t</td>
<td>t</td>
<td>c</td>
<td>k</td>
<td>q</td>
<td>?</td>
</tr>
<tr>
<td>stops</td>
<td>b</td>
<td>d</td>
<td>ď</td>
<td>j</td>
<td>g</td>
<td>G</td>
<td></td>
</tr>
<tr>
<td>affricates</td>
<td>βf</td>
<td>ts</td>
<td>tʃ</td>
<td>kx</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>nasals</td>
<td>m</td>
<td>m̅</td>
<td>n̄</td>
<td>ñ</td>
<td>n̄</td>
<td>N</td>
<td></td>
</tr>
<tr>
<td>fricatives</td>
<td>Øσ</td>
<td>s̃</td>
<td>s̃</td>
<td>ç̅</td>
<td>ch̄</td>
<td>h̄</td>
<td>h̄</td>
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<tr>
<td>lateral</td>
<td>ɬ</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>fricatives</td>
<td>ß̌</td>
<td></td>
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<td></td>
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</tr>
<tr>
<td>l. approximants</td>
<td>ɭ̅</td>
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</tr>
<tr>
<td>rhotics</td>
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</tr>
<tr>
<td>glides</td>
<td>ɯ̄</td>
<td>ȭ</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

More directly, de Lacy & Kingston (2013) state “We argue that synchronic … epenthesis of [k] (and dorsals generally) is unattested”. Also, deL (p.108) says “even in a … language where there is no [t], labials and dorsals can never be epenthetic.”

Quite apart from de Lacy’s own work, the idea that deL’s theory excludes dorsals from being epenthetic is apparently a common understanding. For example, Anderson (2016:10) comments on deL and deL&K, critiquing the theory by noting “On the other hand, at least one instance of general epenthesis of a velar does appear to exist [i.e.
Halh]… We must conclude, therefore, that a phonological regularity involving epenthetic stops other than coronals is not in fact excluded by any general principles of the Language Faculty.”

More directly, deL’s theoretical mechanisms involve a PoA markedness hierarchy \( \text{dorsal} \succ \text{labial} \succ \text{coronal} \succ \text{glottal} \) (where ‘\( \succ \)’ means “is more marked than”). If dorsals are the most marked PoA, then how could epenthetic consonants – whose features are entirely determined by output constraints – ever be dorsal?

So, SV’s interpretation that the deL theory cannot produce an epenthetic dorsal in Halh seems eminently reasonable, based on what others have said. Nonetheless, we shall see that is based on a misinterpretation of the theory.

• **The dangers of trusting de Lacy**

I should mention that it is potentially dangerous to trust assertions about a theory – even by the theory’s creator – without proof. I mean ‘proof’ in a formal sense – so, has it been demonstrated that deL’s theory cannot generate an epenthetic dorsal?

To provide an adequate proof requires engaging deeply with the theory (e.g. Prince 2006). For Classical OT, there have been many articles on how to produce an adequate proof (e.g. many articles by Prince and collaborators on the ROA). In general, to show that a Classical OT theory (such as deL’s) cannot generate an epenthetic dorsal, we would have to show that there was no possible constraint ranking which generated epenthetic dorsals.

One might object by saying that we don’t know all of the constraints, so we can’t possibly prove or disprove anything. However, we can make headway by taking one of two options. One is to limit ourselves to generally accepted constraints – more strictly in deL’s case, perhaps even only the constraints mentioned by deL. Another is to use the proof to discover which constraints are required to generate an epenthetic dorsal; we can then evaluate such necessary constraints and see if they contradict theories of constraint
families we know we have, or are otherwise unlikely. Luckily, there is even software that can help with the task (e.g. Hayes 2017, Prince et al. n.d.).

It is tempting here to undertake such a task. However, I will refrain from doing so for the simple reason that it is the task that SV (and VS) have taken on for Halh and deL’s theory, so for me to address this issue would be for a reviewer to pre-empt a reviewee’s paper before it is published – something that feels unethical to me.

However, I will suggest some avenues of exploration to the authors.

My first point is that SV (and VS) do not provide a proof of the claim that deL’s theory cannot generate an epenthetic dorsal. Specifically, there is no demonstration that no ranking in deL’s theory can generate the Halh grammar.

So, my comments below will focus on factors to consider in undertaking such a demonstration.

- **What is the epenthetic consonant in deL’s theory?**

The first important point to consider is what the theory *should* generate for Halh. In SV’s rule-based serialist theory, the epenthesis rule inserted a /g/ – a dorsal stop. In contrast, Classical OT is an output oriented theory: an epenthetic consonant is an output segment that has no input correspondent (putting aside Staroverov 2014’s theory).

So, from a Classical OT perspective, the epenthetic consonant in Halh is *not* [g], but rather [ɥ/ɿ] (after SV p.10) – a velar/uvular approximant, with the realization of velar/uvular determined by vocalic context (see section 1.3 above for discussion). The question then, is whether the deL theory can generate epenthetic [ɥ/ɿ]. (It is possible that SV:10 suggest that the epenthetic consonant is actually a *fricative* [ɥ/ɿ], citing Stuart & Haltd (1957). However, the description is a ‘lenis fricative’, suggesting that it has less frication than a fricative, and is more approximant-like. I will consider the implications of the epenthetic consonant’s potential fricativeness below, but assume (as SV seem to do) that it is really an approximant.)
• What should the empirical scope be?

It is common in OT research to focus on snippets of grammars – i.e. just those mappings that seem relevant to the issue at hand. For example, de Lacy (2006) discusses a number of cases, but only provides alternations and rankings specific to consonant epenthesis. Is this enough for Halh?

A problem with the ‘snippet’ approach is that it can miss important properties of the grammar in which the alternation occurs. In Halh, for example, we also know that vowels affect the epenthetic consonant’s PoA. We also know that the epenthetic consonant behaves differently from underlying /g/. Ideally, then, the success of any theory in regard to Halh consonant epenthesis should be considered in the context of Halh’s larger phonological system. I made this point for SV’s theory in §2.2, and it should certainly apply to deL’s theory. S14 also makes a point of not only providing a ranking for Halh epenthesis, but also for Halh’s consonant-vowel assimilation. A practical question is where we should stop – should we model the entire phonological system? Ideally, yes, but in practice that would be onerous. For Halh, at least, we have identified parts of the system that should be considered as interacting with consonant epenthesis: the allophony of /g/, and the assimilation of PoA to vowels. So, these issues should be considered in any evaluation of whether a theory ‘works’ for Halh consonant epenthesis.

• What does deL’s theory say?

deL divides constraints into three types: faithfulness, ‘default’ markedness, and assimilatory/dissimilatory. For consonant epenthesis, deL states that faithfulness constraints are irrelevant (“preservation is irrelevant” – p.79).\(^2\) That leaves two kinds of output constraint: those for markedness, and those for assimilation/dissimilation.

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\(^2\) S14 points out that this claim is wrong, and that faithfulness can be relevant through splitting; however, we will ignore splitting here because it doesn’t affect the rest of the argument. Howe & Pulleyblank (2014) also argue that this claim is wrong, but only if there are constraints that penalize the insertion of specific features (DEP-F constraints). See deL p.85 and §8.7 for discussion.
For assimilation/dissimilation, deL states:

“Processes like assimilation and dissimilation can interfere with markedness reduction’s influence on epenthesis. For example, epenthetic elements can assimilate in PoA and manner to adjacent segments” (p.79).

deL cites Dakota’s consonant epenthesis as an example; it involves [j] before coronal (front) vowels and [w] before dorsal (back) vowels (p.80). In this case, the epenthetic consonant’s features are determined by assimilation in both PoA and manner – the epenthetic [j/w] is as vowel-like as it can possibly, and as close in PoA as it can be.

Interestingly, deL then cites a case that looks very similar to the Halh case:

“Similarly, the epenthetic consonant in Brahui assimilates to low back vowels in dorsality, voice, and continuancy, resulting in [ɣ]” (deL p.80).

This quote is of interest here because it states clearly that epenthetic dorsals – i.e. the velar [ɣ] – are possible in the deL theory. In the Brahui case, deL claims they arise through assimilation.

The general point is that deL’s theory contains constraints that provoke assimilation/dissimilation, so such constraints need to be included in any proof involving Halh consonant epenthesis. In addition, in keeping with general properties of Optimality Theory, whether an epenthetic element assimilates or shows an unmarked value is not a binary choice – the epenthetic consonant might assimilate in some features, but have other default features.

- **Halh, assimilation, and markedness**

Halh clearly has assimilatory processes. For one, vowels must harmonize in ATR. In
addition, lexical /g/ becomes uvular when adjacent to [−ATR] vowels (in certain contexts). Also, the epenthetic consonant is also uvular with [−ATR] vowels, and velar with [+ATR] ones. (I adopt Vaux (1999:12)’s proposal that “uvulars are dorsals with a [−ATR] specification”, so the consonant velar-uvular alternation can be seen as assimilation to ATR.)

The manner of articulation of Halh’s epenthetic consonants is also clearly assimilatory from deL’s theory’s point of view. The epenthetic consonants are approximants [u̯/y̯], and so effectively as similar to vowels as consonants can be in terms of MoA. In deL, constraints on onsets favor low sonority elements, so assimilation to MoA must outrank all such markedness constraints in Halh. A relevant ranking is given in deL:102, where AGREE[approx] outranks constraints on PoA and MoA markedness. A sketch of a ranking is provided below:

The requirement that epenthetic consonants agree in MoA with an adjacent vowel (AGREE[APPROX]) outranks constraints that pressure output PoA to be least marked. So, coronals such as [t] are eliminated as competitors because they are not vocalic enough even though they are ideal in terms of onset sonority (*ONS≥x). Labials like [m] are similarly ruled out because they are not approximants, as are other dorsals, like [x].

But what about other approximants? There are also liquids [ɮ ɮ̃ ɭ ɭ̃], and glides [w w̃ ɭ]. The liquids [ɮ ɮ̃ ɭ ɭ̃] have the virtue of being coronal (well, at least [ɮ ɭ]). However,
they have the problems of not being approximants (Walsh Dickey 1997:11, 14). [ɮ] is a fricative ([+continuant, −approximant]) as it has a non-sonorant-like “tendency to de-voice” (S05:19). The rhotic is a trill [ɾ] or tap [ɾ̝], and for some speakers a “fricative [ɹ]” (S05:20). So, the only available approximants in Halh are [w j] and [ɰ/ʁ̞].

So, supposing we are left with [w], [j], and [ɰ/ʁ̞]. Why does [ɰ/ʁ̞] beat [w] and [j]?

S14 argues that the glides [w] and [j] are consonantal, and this disqualifies them. However, another approach is to consider the PoA of these approximants. [w] and [j] arguably have two PoAs each, and so are ‘complex’ segments (Danis 2017): [w] is [labial, dorsal], and [j] is [coronal, dorsal] (Hall 2007). So, the reason why [ɰ/ʁ̞] is preferable to [w j] is that the former has a single PoA, while the latter have two.

Interestingly, Danis (2017) observes that segments with complex PoA in deL’s theory can incur a superset of violations of segments with simplex PoA. In other words, while [w] is dorsal, it is also labial, and that complexity makes it more ‘marked’ in terms of the PoA markedness constraints than either a plain labial or dorsal. The following tableau illustrates this observation; see Danis (2017) for extensive discussion.

(4) **Emergent Anti-Complex PoA**

<table>
<thead>
<tr>
<th>/e-e/</th>
<th>AGREE (approx)</th>
<th>*DORSAL</th>
<th>*DORSAL, LABIAL</th>
<th>*DORS,LAB, COR</th>
</tr>
</thead>
<tbody>
<tr>
<td>(a) ewe</td>
<td>*</td>
<td>*</td>
<td>*!</td>
<td>*</td>
</tr>
<tr>
<td>(b) eje</td>
<td>*</td>
<td>*</td>
<td>*</td>
<td>*!</td>
</tr>
<tr>
<td>ɰ̝ (c) eュー</td>
<td>*</td>
<td>*</td>
<td>*</td>
<td></td>
</tr>
</tbody>
</table>

[w], [j], and [ɰ] all have the feature [dorsal], so they violate all of the PoA markedness constraints. However, [w] also has the feature [labial], so it incurs two violations of *{DORSAL,LABIAL} – one for its [dorsal] feature, and one for its [labial] feature. Similarly, [j] has the feature [coronal] as well as [dorsal], so it violates *{DORS,LAB,COR} twice. In this way, the PoA markedness constraints favor segments with simplex PoA over those
with complex PoA (under certain conditions of feature intersection – see Danis 2017).

So, why is [ui] ‘better’ than [w] and [j] for Halh? [ui] has one PoA feature, while [w] and [j] both have two, and so [ui] violates a subset of the PoA markedness constraints.

I should add a quite note on feature theories. S14 adopts Halle (1995)’s proposal that all vowels have the feature [dorsal]. In contrast, de Lacy (2006) agrees with Clements & Hume (1995)’s approach. In fact, the deL theory does not work well with Halle (1995)’s feature theory – if all vowels have a [dorsal] feature, it should be possible to produce an epenthetic [k] (an undesirable result – see de Lacy & Kingston 2013). However, notice that even without vowels all being [dorsal], it is still possible to get epenthesis of [ui/ʁ̞] in all environments, as suggested above.

At this point, have we proven that deL’s theory can generate Halh consonant epenthesis? Not at all. Proof would require considering more of Halh’s phonological system, and other relevant constraints. However, we have started to suggest a way forward in exploring the theory and how it might be used to analyze Halh.

Even if the idea that [w] and [j] have complex PoA and [ui] does not is rejected, the broader point is that other output constraints with emergent effects should be examined to see whether they favor [ui] over [w] and [j].

Part of the fuller analysis would involve showing why [ui] and [ʁ̞] appear in different environments. As other velars vary in their velar-uvular place depending on vocalic ATR, presumably agree[ATR] plays a role, and so it would be worth exploring its position in the ranking, and how it might interact with the other constraints that determine epenthetic form.
Also, lexical /g/ and /ɡ/ and the epenthetic consonant behave differently: as mentioned above, morpheme-final lexical /g/ and /ɡ/ retain their PoA pre-vocalically. However, epenthetic consonants agree in ATR with following vowels in the same position. In the deL analysis, it would be worth exploring whether a faithfulness constraint was responsible for this difference: e.g. a constraint like IDENT[ATR], preserving underlying PoA, would outrank AGREE[ATR]. IDENT[ATR] would not apply to epenthetic elements, as they have no underlying correspondent (cf. S14’s theory).

One final important issue in regard to proving whether deL’s theory can generate Halh is its inventory. As in Classical OT, deL claims that the output inventory is determined by constraints. Is it possible to have a ranking that generates Halh’s output inventory and generates the right epenthetic consonant? This is not obviously true. I have not shown that the deL theory can generate Halh’s phonological system – that would be interloping on SV’s analysis. At this point, though, SV have not proven that the deL theory is incapable of doing the job. To do so would require more detailed analysis, and ideally a proof.

The more general point about the deL theory is that it is rather dangerous to assume that it can or cannot epenthesize a particular segment. The theory predicts that the best available consonant is inserted, where ‘best available’ is defined by the grammar’s output constraints and their ranking, with some effects being emergent – either emergently assimilatory/dissimilatory, or emergently unmarked, or some mixture of the two. In Halh, [ɰ/ʁ̞] is ‘best’ because it is a very vowel-like consonant (i.e. assimilatory), and has the
least marked PoA of the vowel-like consonants. It thus presents an interesting case where a segment with the most marked PoA (dorsal) can be epenthetic, because the alternatives are so terrible (i.e. they have complex PoA). To some degree, this finding is unsurprising: deL discusses how glottal is the ideal PoA in terms of the PoA markedness hierarchy, yet epenthetic consonants can be coronal, labial, or palatal – or ‘promoted’ to ‘least-marked’ – by interacting markedness constraints and assimilation. Halh presents a situation where non-complex dorsals are similarly promoted.

• **What if the Halh epenthetic consonant is a fricative?**

SV mention the possibility that the epenthetic consonant is a fricative [ɣ/ʁ] rather than an approximant. It is still possible that the deL theory could generate epenthetic [ɣ/ʁ]. On the one hand, [ɣ/ʁ] is better than the Halh sonorants because it is less sonorous (i.e. *CONSREF > nasal favors [ɣ/ʁ] over [m n r w j] etc.). However, [ɣ/ʁ] is better than other Halh obstruents because it is [+voice], like vowels (i.e. AGREE[voice] favors [ɣ/ʁ] over [p t s x] etc.).

In other words, [ɣ/ʁ] hits a sweet spot of relative unmarkedness, but relative vocalic similarity. I should point out that its success is due to it being the *only* [+voice] obstruent intervocally in Halh.

(6) **Fricative epenthesis**

<table>
<thead>
<tr>
<th></th>
<th>/e/-e/</th>
<th>AGREE (voice)</th>
<th>*CONSREF</th>
<th>*DORSAL</th>
<th>*DORS, LABIAL</th>
<th>*DORS, LAB, COR</th>
</tr>
</thead>
<tbody>
<tr>
<td>(a) ete</td>
<td>*!</td>
<td></td>
<td></td>
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<td>*</td>
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<tr>
<td>(b) eye</td>
<td></td>
<td></td>
<td>*</td>
<td>*</td>
<td></td>
<td>*</td>
</tr>
<tr>
<td>(c) ene</td>
<td></td>
<td></td>
<td>*!</td>
<td></td>
<td></td>
<td>*</td>
</tr>
<tr>
<td>(d) eje</td>
<td></td>
<td></td>
<td>*!</td>
<td></td>
<td>*</td>
<td>*</td>
</tr>
</tbody>
</table>

PAGE 46
MANUSCRIPT AVAILABLE IN RADICAL
• *Is the deL theory restrictive?*

Supposing that SV demonstrate that the deL theory can generate Halh’s consonant epenthesis, it would then be worth exploring whether the deL theory is adequately restrictive. In other words, if the deL theory can generate Halh’s epenthetic [ɰ/ʁ̞], can it actually generate *any* epenthetic consonant?

As we have seen above, the answer to this question is “it depends”. For any language, the ranking that generates the epenthetic consonant is part of the overall ranking for the language. That overall ranking influences the form of the epenthetic consonant, so the theory’s predictions are best couched in statements such as “Given specific rankings R, possible epenthetic consonants are x,y,...z”. Representation also matters. If we adopt the feature theory of Halle (1995), for example, where all vowels are [dorsal], then the deL theory would predict that epenthetic dorsals like [k] are possible. So, as with any complicated theory, the answer is complicated.

However, if we focus on Halh, we can see that its epenthetic [ɰ/ʁ̞] is remarkable. It the only sonorant in Halh’s inventory with a single Major PoA feature that does not carry other potentially undesirable featural baggage – [rhotic], [lateral], [+nasal], or dual PoA features. This remarkable nature is what allows it to be promoted to epenthetic status, in spite of its highly marked dorsality. In other words, the epenthetic [ɰ/ʁ̞] might be bad in terms of PoA, but the alternatives are worse.

### 2.6 Options

At this point, it strikes me that a full account of SV’s theory *and* adequate critiques of alternative theories would expand SV’s article significantly. The nature of this journal (*Radical*) means that there is no impediment to length. However, SV may not wish to devote the time to responding in the way I have outlined above.

So, I suggest options for revision:
(1) Focus on the SV theory
SV could focus on presenting a detailed theoretical account of their own theory. This would involve identifying the theoretical framework’s relevant properties, proposing a specific rule system, and demonstrating that it generates the requisite derivations. The idea that /g/ is epenthesized could be discussed in more detail, as well as explaining why the lexical /g/ behaves differently from the epenthetic [g]. It would be insightful to contrast this approach with S05’s rule-based theory, and to discuss why the epenthetic consonant cannot be ascribed to assimilation processes (against S14’s claims).

At this point, SV would have shown that there is a viable analysis of Halh in their theory. It is not strictly speaking necessary to address other theories, then – it would be up to others to present a working account in those theories.

Of course, this approach would mean that any claims about the viability of ‘markedness’ theories based on Halh would be unsupported. What SV could claim, though, is that there is a viable rule-based account of Halh, but as yet no accounts of Halh based in markedness theories, apart from S05’s and S14’s.

(2) The “all in” approach
The most interesting, and challenging, option would be to both present a theory that works, and carefully analyze other theories with respect to Halh. However, as we have seen above, showing that Halh poses an insurmountable problem for all theories that incorporate markedness in them is a great challenge. It would be better to focus on one theory and show why that theory can’t generate Halh, and then argue that the theory fails because it has markedness mechanisms. The deL theory is complex enough to be interesting in this regard, so I suggest focusing on it. I feel that a definitive demonstration that a specific theory fails is of more value than a surmise that theories with a particular property fail.
SV is not alone in claiming that Halh (Khalkha) Mongolian has an epenthetic dorsal, or in providing an analysis of it (e.g. S05, S14). However, SV focus specifically on this one case, and so the article is valuable and should see publication in some form.

However, it is not clear that SV’s analysis works, or what its details are. As the concept of epenthesis is highly theory-dependent, any argument about epenthesis must proceed from a theory: the theory must be clearly stated, and then epenthesis can be defined succintly. What’s missing in this paper is a clearly defined theory.

It is also not clear that SV’s claims that certain other theories – i.e. S95/S05’s, S14’s and deL’s – cannot generate Halh, or are otherwise flawed, are correct. It is also not clear how they determined that all theories that have markedness mechanisms are flawed, particularly with regard to theories that regard velar as default (e.g. Rice 1996 et seq.). Theory evaluation must proceed from the theory itself. The discussions of the theories here are at an ‘intuitive’ level – they attempt to discern what the theories seem to say about epenthesis rather than using the logic of the theories to determine what they actually can say.

In short, I recommend publication if the authors’ own theory is developed in adequate detail to address the issues raised above. The authors may choose to not discuss other theories. However, if they do, the issues outlined above should be addressed.

**A note for future research**

To make a final point clear, while I accept that the evidence presented for Mongolian epenthesis is perfectly in line with current methodology and practice in the field of theoretical phonology, I do not believe that current methodology and practice is at all adequate. For specific discussion, see de Lacy (2009), and for discussion of a specific case Shih (2018a, 2018b).

Grammars are not necessarily reliable sources. It’s striking that even though there are
several grammatical descriptions of Halh, there is still a lot of uncertainty about the allophony of the epenthetic segment. Is it a glide, or a fricative? Can it appear as a stop? These questions are rather basic, and I suspect that theoretical issues cannot be addressed without addressing such a fundamental question first: i.e. what is the phonetic quality of the epenthetic segment? And from that, what can we determine its phonological features to be?

For future researchers, I suggest that any significant advance for Halh will have to involve systematic phonetic and phonological investigation directly involving native speakers of Halh. I hasten to add that this belief does not affect the issue of whether SV should be published – SV’s grammar-only evidentiary approach is entirely consistent with current practices in the field.

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