



Most of the concerns raised in my previous review have been adequately addressed with an exception of an important one: the paper draws a too strong conclusion in favor of temporal adjustment hypothesis given the data while dismissing other possibilities with subjective language such as “seems more natural” and “what it is claimed here”. Two paragraphs in the discussion section need revision due to this subjective language and speculative hypotheses.

Moreover, I would like to add that, if misplacement of the V-C boundary is ¹³⁵ due to random error (which is a neutral assumption to make), the mea- ¹³⁶ sured displacement from the ‘actual’ boundary will approximately follow a normal distribution with mean 0, so that negative and positive misplace- ments would cancel each other out.



This is not true. Even though the misplacement is due to random error, the negative and positive misplacements would *not* cancel each other out, because the vowel vs. closure measurements are not independent. If only one annotator annotates the data, the measurements are dependent and the results would not cancel each other out, but would result in negative correlation.

However, what it is claimed here is that the stability of the interval is a logical antecedent, while the different

The CV of the release to release duration is 0.203, while that of the vowel onset to release duration is 0.232. The CQV is 0.127 for the release to release and 0.136 for the vowel onset to release. Lower values mean less dispersion/more cohesion.

And

“This interpretation

635 seems more natural in light of accounts of gestural phasing like the one
636 proposed”



It is not relevant what authors claim. And what “seems more natural”. The authors should avoid such subjective judgments and stick to empirical facts. It is true that the results are compatible with the compensatory temporal adjustment, and the body of the paper makes only this claim. The discussion section, however, is too strong in its conclusions. The results are also compatible with the effects of the laryngeal features. A possible articulatory explanation (Tilsen 2013) is not evidence in favor of the proposal, because the laryngeal feature effect has also a possible articulatory explanation which the authors do not mention.



Even when multicollinearity between predictors is minimal, statistical
659 significance of multiple terms cannot unequivocally inform us on the actual
660 contribution of those terms, since it is possible that unknown relations be-
661 tween terms mask underlying mechanisms (for a discussion see McElreath
662 2015). For example, it is possible that, through time, different phonation
663 categories (like ejective, voiceless, and voiced stops) can develop differ-

664 ent closure duration sub-distributions (Sóskuthy 2013).

This is true, but presence of significant predictors is still more informative than the absence of any empirical evidence.



This problem is not an argument against the laryngeal features effect either. The same problem can work in the opposite direction. Why can't the same argument go in the opposite direction, where initially the "causal" effect is only that of laryngeal features? This is in fact what many scholars believe, where secondary durational differences arise due to perception. This paragraph is based on pure speculation without any empirical evidence. If the authors have any empirical evidence, please state so.

633. his can result in
634. ⁶⁶⁵ the ability for the phonation predictor to capture variance in vowel dura-
635. ⁶⁶⁶ tions, even if the original mechanism directly involved closure durations
636. ⁶⁶⁷ only

But why in the expected direction? This is not very elaborate (beyond being a possibility).

Abstract:

While other factors (like perceptual biases and laryngeal ef-
¹⁹ fects) could also play a role in the development of the voicing effect, the
²⁰ data discussed here sheds new light on a possible production account of
²¹ voicing-related differences in vowel durations.

Laryngeal effect is also compatible with a production account, not only the temporal compensation approach.

Conclusion

The conclusion is adequate.

745. Such pluralist view has already been pro-
746. ⁷⁴⁹ posed, for example, for incomplete neutralisation in [Winter & Röttger](#)
²⁰¹¹
747. ⁷⁵⁰ (for a review of explanatory pluralism in the cognitive sciences, see [Dale](#)
748. ⁷⁵¹ [et al. 2009](#) and references therein).



Pluralist views are already proposed for the very problem discussed, not only for incomplete neutralization or in general in cognitive science.