To: Editorial Board of Cortex,

Dear Dr. Guediche,

We are writing to resubmit our manuscript, CORTEX-D-21-00884 “What we do (not) know about mechanisms underlying adaptive speech perception: A computational review”, to *Cortex*. The manuscript presents a literature review on approaches to adaptive speech perception and two simulation studies to replicate the signature results of empirical studies. Our original submission received highly positive and constructive reviews, and we were given an opportunity to address remaining presentational issues and resubmit the paper. We are grateful for all the suggestions that you and the reviewers provided us with and are happy to submit a revised version of the manuscript for your consideration.

Below, we provide responses to all of the reviewers’ comments. We included the manuscript with track-changes included to highlight the changes made during this round of review.

Thank you very much for your time and guidance. Please feel free to get in touch with us if there is anything else we can clarify.

Xin Xie, T. Florian Jaeger, Chigusa Kurumada

**Major comments from the editor summarizing R1’s and R2’s comments**

1) You and both R1& R2 encouraged us to tighten up the manuscript and increase the readability by potentially moving technical details to an appendix. Also, R1 and you suggested that we provide a clear list of take-home points in a (new) conclusion section. The core message that we gathered from those two pieces of advice is that we need to more clearly/concretely articulate *what researchers can and ought to do* in order to depart form the status quo.

To respond to this, we have implemented the following.

1. We have shortened the introduction by condensing discussions of the two experimental paradigms (*perceptual recalibration* and *accent adaptation*) to Sections 3 and 4. The introduction was XX pages long and now it is XX pages.
2. We have moved some of the technical details to Appendix X.
3. We have revised the paragraphs detailing our recommendations for future studies (now 5.1).

2) Related to 1 above, another criticism raised was about a lack of clarify in what the current model-driven approach ultimately seeks to achieve. Given the empirical indeterminacy illustrated in the current case studies, what should we reasonably assume we can achieve through behavioral experiments? More specifically (as R2 puts it), do we seek to find “any truly discriminant measures” that can reliably rule out one or more mechanism(s) as an explanation of specific empirical data? Or alternatively, do we try to identify regions of a parameter space that allows us to compare the contribution of the models in more relative terms (e.g., relative model fits)?

This is a critical point, and our response is twofold. First, what we envision and lay out in General Discussion is a discriminant method, with the goal of ruling out one or more mechanism(s) based on behavioral results. Second, we additionally argue that:

1. To reliably distinguish between the three mechanisms (normalization, changes of representations, changes of decision-bias), predictions need to be spelled out and tested with respect to **acoustic-phonetic details** of the speech input. The current simulation suggests that it is not possible to develop a discriminant method as far as we continue to make/test our predictions at the level of overall accuracy (or speech) of category recognition (e.g., /d/ or /t/).
2. Computational models are needed to generate competing predictions under the competing mechanisms. More specifically, we need them to formalize roles of the listener’s **prior expectations** and **the current input**, and whether/how the input cumulatively changes the prior expectations. The influences of these factors (along multiple acoustic-phonetic cue dimensions) are highly complex. Unless we have an analytical means to integrate them, our predictions and analyses will be prone to our biases and ad-hoc reasoning.

To this end, it is critical to determine a parameter space for each model that captures human behavior. However, that is not the goal of our approach. Rather, by determining best-fitting models, we will be able to quantitatively predict *where* in the phonetic space and *when* (after how much exposure input) the three mechanisms may make diverging predictions. We will them put these predictions to test in perceptual experiments. We hope the current discussion has made these points clearer.

**Other comments   
  
R1**

1. p.21: the significance of the lapse rate needs to be spelled out
2. p.36: Why are different exposure tokens used between the /d/-biasing vs. /t/-biasing conditions?
3. p.37: Why did the simulation focus on the very early portion of the test phase? What can we say about the gradual changes that can occur throughout the test phase?
4. P.55: The figure showing the decision-making model results need fixing.
5. There were issues with pdf rendering

**R2 (We consolidated some of the comments pointing towards the same source of concerns)**

1. What did we learn from this approach? The literature review made it clear that there is possible empirical indeterminacy. Why do we need simulations? What else can we gain?

Thank you for raising this big picture question. As we summarized in our general response to the editor above, the core take-home point is that empirical indeterminacy will persist so long as we use experimental design and analysis methods that are currently prevalent. The simulation is useful because it does not only confirm that the empirical indeterminacy exists, but it also shows where/how the three mechanisms could be distinguished from each other. We hope that our newly edited general discussion makes these points more clearly than before.

1. The Recommendations in the GD are underspecified

We have extensively edited the general discussion and created a subsection (5.1) dedicated to spelling out 4 concrete predictions.

The current approach uses “complex models with numerous adjustable parameters”, why is it so noteworthy if we found a parameter space (or each model) that can predict comparable perceptual responses?

Indeed, computational models in general can simulate a wide range of behavioral results. One thing we wish to clarify here is that our models are among the simplest in terms of its parameter settings. Note that both prior expectations and the input statistics are derived from human speech data, which leaves only two parameters for each model. All combinations of these parameter settings that are illustrated in the figures in Sections 3 and 4. Nothing else has been determined by us researchers. (To put this in perspective, other major models of speech perception and spoken word recognition often have …..<add more info>.) In sum, the models here are among the simplest, adopting only very standard assumptions (e.g., Luce’s choice rules) with a minimal number of adjustable parameters.

In this light, and more importantly, what makes the “empirical indeterminacy” surprising is not the capacity of the computational models *per se—*As R2 correctly points out, it is not meaningful or noteworthy to find *some* parameter settings of the models that can predict comparable perceptual responses. Rather, what makes is surprising is the fact that *not a single study has thus far* considered these three models as serious contenders of behavioral results. This includes our past work. For instance, we have often simply equated a response shift to an underlying representational shift. To move beyond this

1. Besides the tasks/paradigms we simulated here, there are other adaptation & normalization tasks/paradigms. Is any of them more immune to ambiguity?  
   Thank you for this question. We have discussed a wider range of adaptation/normalization tasks and studies in 5.2. We cannot provide an exhaustive list of such studies. But we are confident that none so far has considered the three mechanisms as well as their combinations as a possible source of perceptual adaptivity. This means that, at the very least, we do not know if they are immune to ambiguity. In this sense, the theoretical and methodological considerations laid out here will provide useful scaffolding to these studies that we did not directly model in this current paper.

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Editor’s comments:

1. Reviewer 1 suggests offering more insight into whether there exist conditions for which any of the proposed mechanisms can be ruled out
2. Reviewer 2 commented on the discussion related to neural effects.
   1. Any neural data related to the proposed mechanisms?
   2. What’s the relationship (implications) between the computational approaches and neural approaches?
3. Make the manuscript shorter
   1. Perhaps moving the formulas into an appendix?
   2. Make the insights accessible to those who are not familiar with computational models or modeling approaches
4. Summarize key take-home points

R1:   
  
Major comments

1. Neural measures
   1. Show a more tangible link or else, cut it completely
2. What happens if all of the mechanisms are at work simultaneously?
3. A clear conclusion needed.

Other comments

1. Clarify the importance of the lapse rate parameters (e.g., p.21)
2. Usability of the pdf doc / figures do not come out well when printed
3. Figure 14A (the exposure tokens used in the perceptual recalibration experiment). Why did the two conditions (/d/-shifted vs. /t/-shifted) used acoustically distinct exposure tokens as “ambiguous” (as is done in other experiments)
4. Time course of changes (throughout the test phase): Why not predicting possible changes happening later in the test phase?
   1. Figure 15 caption: “Predicted categorization responses are shown for the 6 test tokens after /d/- and/t/-shifted exposure, depending on the strength of the prior beliefs in categories means (𝜅0,𝑐) and covariances (𝜈0,𝑐).
5. The decision making model (Accent adaptation). Figure 23. We need to update the figure and explain what was not correctly represented in the submitted version of the figures.

R2:   
  
Major comments

1. What do we learn from this approach?
   1. The facts that 1) there are different accounts; and 2) they could predict the same results are clear from the intro. What else did we learn by actually modeling them?
   2. If the only surprising effect is that the decision-making model can predict results, would that justify the 90-page endeavor?
   3. What else can we gain?
2. Recommendations in the GD are underspecified
3. How, in the end, should we separate the models?
4. “If the normalization mechanism leads to a slightly better quantitative fit than the representation mechanism, but does so only in a very small region of parameter space, should we count this as evidence that it's a more likely candidate mechanism?”
5. This is primarily an illustration of the problem
6. We need to provide a more streamlined workflow and logic in GD
7. “Complex models with numerous adjustable parameters”…
   1. This is not the case and we need to clarify that.
   2. “Might a different parameter set have proven better under different assumptions of how input maps to perceptual representations?” We need to provide the rationale behind the approach
8. Do we expect to see any categorical differences between the three models?
   1. If so, where do we see them and why?
9. What’s the goal of the proposed solution?
   1. Are we able to clearly separate the three models (and say e.g., “In this case the empirical results are most likely to stem from Mechanism X)?
   2. Or are we trying to find a parameter space that is likely to distinguish the models?
   3. R2 also says “**Can we fit basically all the same qualitative patterns of data with each mechanism, if we find the right parameterizations?”**
      1. Again, we need to provide the rationale
10. Besides the tasks/paradigms we simulated here, there are other adaptation & normalization tasks/paradigms. Is any of them more immune to ambiguity?
11. “it feels like its **missing the specificity needed to explain next steps.”**
    1. We need to describe the suggested steps more clearly

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Dear Dr. Xin Xie,  
  
Thank you for submitting your work to Cortex. Your above paper has now been reviewed by expert referees, whose comments are enclosed for your perusal. On the basis of these comments, we cannot accept your manuscript in its present form but would like to invite you to revise your paper, taking into account the issues raised by the reviewers. Please note that acceptance is not guaranteed at this stage and any revision is likely to be sent back to the referees for further review.   
  
Both reviewers offered many positive comments regarding the paper, however, a few concerns need to be addressed if you choose to submit a revision. Here, I highlight four important points but the authors should include a point-by-point response to each of the reviewer comments, highlighting each change made in your manuscript and/or providing a suitable rebuttal.   
  
1)      Reviewer 1 suggests offering more insight into whether there exist conditions for which any of the proposed mechanisms can be ruled out or whether it may be more about finding the most optimal solutions. Illustrating such an example with a simple case might add some clarification to the main conclusions.  
  
2)      Reviewer 2 commented on the discussion related to neural effects. Here, the authors can focus on offering a more explicit and clear link on the relationship between the proposed neural mechanisms and the computational mechanisms, rather than expanding the length of this section. Highlighting the main mechanistic arguments made based on specific neural data and drawing parallels to the computational models (or if appropriate, pointing to any neural evidence that distinguishes among or brings together different computational possibilities would be useful). This section will provide a nice connection to other topics addressed in the special issue.  
  
3)      Both reviewers commented on the length of the manuscript. Perhaps some of the mathematical details of the models could be described in detail in an appendix instead? I think this will increase the impact of the main points made for those who are not computationally-inclined. However, it is important to have them readily available in an appendix for those who would like to know more about the specifics and default parameter assumptions.   
  
4)   Finally, both reviewers commented on the need for summarizing the key take-home messages and more specific steps for moving forward, in the conclusions section.   
  
Please remember to update the abstract (if appropriate) to reflect any changes made to the manuscript following review.  
  
Please note that Cortex is a signatory to the Transparency and Openness Promotion (TOP) guidelines (see [https://www.elsevier.com/\_\_data/promis\_misc/Cortex\_TOP\_FAQs.pdf](https://urldefense.com/v3/__https:/www.elsevier.com/__data/promis_misc/Cortex_TOP_FAQs.pdf__;!!CGUSO5OYRnA7CQ!L5roUGmDktPh9Z42mxg0-se9ZMc37weIoECnLCXSAJ7Z2djpK7rZRy045nb_bAwl7PF11bQ$) for further details). Upon submitting your revised manuscript, you will be required to complete a brief checklist and provide information in the manuscript about the use (or otherwise) of open practices including citation standards, availability of data, materials, and analysis code, study preregistration (where applicable) and a declaration confirming that the manuscript reports how sample size was determined, all data exclusions (if any), all inclusion/exclusion criteria, whether inclusion/exclusion criteria were established prior to data analysis, all manipulations, and all measures in the study. For an example of this checklist, please see [https://www.elsevier.com/\_\_data/promis\_misc/CORTEX\_TOP\_Example\_Checklist.pdf](https://urldefense.com/v3/__https:/www.elsevier.com/__data/promis_misc/CORTEX_TOP_Example_Checklist.pdf__;!!CGUSO5OYRnA7CQ!L5roUGmDktPh9Z42mxg0-se9ZMc37weIoECnLCXSAJ7Z2djpK7rZRy045nb_bAwlhG6GGtY$)  
  
  
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I look forward to receiving your revised manuscript.  
  
Yours sincerely,  
  
Sara Guediche  
Guest Editor  
Cortex  
  
  
Comments from the Reviewers:  
  
  
  
Reviewer #1: Review of : What we do (not) know about mechanisms underlying adaptive speech perception: A computational review  
  
SUMMARY  
The authors introduce a computational framework that characterizes how recent exposure can impact pre-linguistic signal normalization, linguistic representations, and decision-making.  They use this model to raise questions about whether prior accounts of adaptive change necessarily relate to changes in linguistic representations, or whether they could be attributable to other levels of representation, across two case studies.  They conclude by offering targeted guidance for how future work could leverage this framework to improve experimental practice and better determine the locus (or loci) of adaptive change in speech perception.  
  
ASSESSMENT  
Overall, I thought this was a very strong paper.  The computational framework introduced in this work and illustrated in the two case studies makes a strong argument that attributing adaptive change to changes in linguistic representations is premature --- both pre- and post-linguistic adaptation can also potentially explain these results.   Incorrectly oversimplifying the potential contributions of other cognitive systems (e.g., the decision system when studying the lexical system, or vice versa) clearly has the potential for misattributing the locus of a range of empirical effects, but direct comparisons of the potential contributions of each system are lacking in the literature, which this work aims to address.  This work also very clearly demonstrates the utility of models not only in describing extant empirical results but in generating targeted predictions that can delineate between competing (or partially complementary) theoretical accounts, which seems especially  
useful in the present case.

I also appreciate how the current framework can be extended to capture an even broader set of phenomena, which speaks to the generality of the approach.  Furthermore, the introduction of a formal, quantitative method appears to have had the added benefit of revealing how, sometimes by chance, a set of stimuli could have more variation on a dimension other than the typically expected one, and how that could alter the results of the experiment (in section 4.2.1).

On another front, the authors went to great lengths to integrate this paper with a wide body of prior empirical and theoretical work.  Although this does make for a long read, it does help illustrate the breadth of issues that this work connects to as it is presented and could be connected through in future extensions.  I have gone back and forth on suggesting that the length of the paper be reduced, which might increase its uptake, but I do think that there is value in having all of this  
content available in one place.  The authors' commitment to open-science principles is also truly exemplary, in that they provide literally all materials needed to replicate their work, including the paper itself, in their materials.  In so doing, they have increased the accessibility of this computational approach to other researchers and made it easier to test other related hypotheses or model other related data sets in the future.  I found the extensive set of figures very helpful in fleshing out a number of points and in illustrating how various model parameters operate, and the model description to be reasonably accessible, although the complexity of some aspects of the math may still pose some accessibility challenges for a wide audience.    
  
Notwithstanding these strengths, there were a few points that I thought might warrant some revision.  In no particular order: I found the section on neural correlates to be somewhat underwhelming and non-specific.  For this paper to have substantial weight, I think it would be necessary to spell out how specific neural measures could be tied, in a quantitative way, to specific parameters of the computational framework.  I would encourage the authors to consider a simple case study to illustrate this point, otherwise, I would suggest that this section could be tightened considerably or eliminated.  In a different vein, the current case studies attempt to illustrate the separate and independent effects of manipulating parameters at three different levels of processing; however, I would imagine that there is a good case to argue that adaptation could occur at all three levels simultaneously, at least to some degree.  If that is the case, I think it would be useful to explain  
how change could be modeled at all three levels simultaneously, even if the main focus here is on these clear contrasts that are possible through independent manipulations of each parameter.  Finally, I found that a clear integrative conclusion was missing for this paper, which seemed to end fairly abruptly.  I think it would be very helpful for a reader of a 60+ page paper to have the authors state what they think the key take-home points of the work are to facilitate retention of those points and to avoid losing them among the dense package of content that this paper provides.    
  
Other comments:  
  
  
-when introducing the lapse rate parameter on p. 21, I was not initially sure of why this parameter would receive such prominent treatment in the paper, although the case was nicely made later on in the paper.  Given the importance of lapses was not discussed in detail earlier in the paper, it could be helpful to foreshadow the importance of this parameter earlier on.    
  
-I had several issues using the pdf document, including generating a printed copy.  I suggest the journal and the authors be mindful of this if this paper is moved to production.  I was on windows 10 using the current version of Adobe Acrobat when I encountered these issues.    
  
-likewise, a number of the figures do not print out well, potentially because of the default state of the figures in the interactive figures.  Although I appreciate that the figures are best appreciated on a computer, it would be useful, I think, if they could work in a basic sense in a printed copy of the document.    
  
-on p.36: Examining Figure 14A, my impression was that different stimuli were used in the /d/-shifted vs. /t/-shifted panels of the figure.  Would the tightest control not contain the same base stimuli shifted in either direction?    
  
        p. 37: The focus on the simulations is on the beginning of the test phase; however, should the model not also be able to account for performance throughout the test phase?  If not, why not?  Is this reflective of some additional parameter not included in the model (e.g., a reluctance to keep changing beyond a certain point?), particularly in the face of repeated stimuli?  
  
p.55 the authors state "the highest accuracy is obtained for the fastest changes, and it matches that observed for changes in decision making."  Looking at the data, I am not sure that the match is especially strong, but I may be misinterpreting the data being referenced here or the level of "match" that the authors are referring to.  Perhaps this could be clarified?    
  
  
Reviewer #2: Summary  
The manuscript presents a model of phonetic adaptation effects that aims to better investigate the linking functions between behavioral tasks and the mechanisms of speech adaptation. In particular, the model distinguishes between processes of altering representation, establishing response biases and normalizing input. The authors then simulate typical talker adaptation and accent adaptation effects, exploring the parameter space of the model for these three different processes. They find that certain regions of parameter space can recreate the qualitative patterns of these tasks for all three types of process. They interpret this as a major challenge to prominent accounts of talker and accent adaptation, which regularly posit a locus of effect in altered representations. They then suggest that use of their approach to simulating behavioral adaptation tasks can help better identify the mechanisms of adaptation, particularly when paired with careful stimulus and design  
decisions to maximize how well patterns of data can discriminate between the different processes.  
  
Review  
There's a lot to like about this manuscript. The general approach is timely and important - the authors emphasize the need to carefully think about the way we operationalize the constructs we care about, and provide a mathematical model to do so. I applaud them for this approach - many of our theories are built on a backbone of methods that haven't received this kind of careful methodological treatment, and instead rely heavily on face validity without adequate skepticism. Assessing the validity of our operationalizations is an important next step to overcome to the replication crisis and strengthen our theories.  
  
The paper is also written extremely clearly. The model is described well, the theoretical backing is easy to follow, and the authors show a very comprehensive mastery of the relevant literature.   
  
Despite these clear pros, the paper is a bit challenging to review. It's long, thorough and detailed, but I'm left at the end wondering what, exactly, we've gained. The authors nicely identify that the interpretations given for past studies could arise because of different mechanisms, but I'm not sure that a 90-page technical modeling paper is necessary for this. Much of the introduction highlights the theoretical reasons that different mechanisms might explain the extant behavioral data. The fact that the model confirms this is reassuring of their logic, but also makes me wonder what we learn from the model itself. The primary surprising finding from the model seemed to be that response biases can do more than we might expect, but this feels a little underwhelming given the scale of the paper.  
  
In the General Discussion, the authors suggest ways to use this approach to better investigate mechanisms of adaptation, but this felt fairly underspecified. For example, I agree that quantitative model fit might prove more important than qualitative fit in some cases, but relying on quantitative fit is easier said than done. In the case of the present model, there are multiple parameters for each mechanism that can be manipulated in search of best fitting models, and it's not clear what the best way to adjudicate between these might be. If the normalization mechanism leads to a slightly better quantitative fit than the representation mechanism, but does so only in a very small region of parameter space, should we count this as evidence that it's a more likely candidate mechanism? This issues of model selection on the basis of quantitative fit are quite contentious, and seem particularly challenging for complex models with numerous adjustable parameters, like the present  
one. In addition, the way the model accommodates training input depends on how it establishes and represents its priors about category information. Some aspects of this are included as adjustable parameters in this model, but others aren't - for example, the way that the model maps acoustic input into perceptual features relies on assumptions about how input is processed and normalized. It's necessary to make some assumptions like these, but they then pose challenges to interpretations that rely on quantitative fit comparisons. Might a different parameter set have proven better under different assumptions of how input maps to perceptual representations?   
  
This concern points to a benefit for situations where predictions of qualitative distinctions are meaningful. In particular, it would be helpful if the authors could identify conditions that can't be accounted for by some of the mechanisms, no matter the parameter choice. For example, are there certain types of stimuli or training regimens that would only predict an effect if representations change, but can't be explained by normalization or response bias? Can the authors point to any truly discriminant measures by which we can rule out a mechanism as incapable of explaining a pattern of results, rather than just offering a poorer quantitative fit? Or is the whole enterprise here a question of finding the specific region of parameter space that best accommodates whatever data can be collected? This isn't necessarily disqualifying - ideas like parameter space partitioning have proven a useful tool for comparing simulations - but it raises questions about whether the model is  
just overly flexible. Can we fit basically all the same qualitative patterns of data with each mechanism, if we find the right parameterizations? And if so, is it worth doing a more formal parameter space partitioning analysis to see if some of the approaches more stably predict this?  
  
There's a wide array of other speech adaptation and/or talker normalization tasks out there beyond those simulated here. Are there any of these for which the model offers qualitatively discriminant predictions?   
  
Overall, I think the approach taken in this paper is intriguing and potentially valuable, but it feels like its missing the specificity needed to explain next steps. The ability of the model to simulate patterns of data from all three mechanisms is problematic for previous accounts - so what should we do? The paper would be much more powerful if it had a clearer path forward to help us discriminate between these mechanisms. There's a short paragraph on Page 62 that starts down this path, but it quickly reverses into more conceptual discussion of how this could be done.   
  
I find I'm left wanting more of something from this paper, despite it already being lengthy. I love the perspective about a need for more careful consideration of linking functions, but think that much of the heavy lifting for this perspective can be carried by the introduction and lit review, before the model has been formally described, let alone the simulations have been run. I like the premise of using this modeling approach to adjudicate between different mechanisms of adaptation, but didn't feel like we got a clear answer of how to do so with this model. We certainly need a better understanding of the link between behavior and adaptation tasks and theories of the processes that allow this. The issue is that this paper just points out what's wrong, without enough thought for how to make it right.