Comparing accounts of formant normalization against US English listeners' vowel perception

Anna Persson,[1] Santiago Barreda,[2] and T. Florian Jaeger[3]

[1]*Swedish Language and Multilingualism, Stockholm University*[a]

[2]*Linguistics, University of California, Davis*

[3]*Brain and Cognitive Sciences, Data Science, University of Rochester*

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Human speech perception tends to achieve robust speech recognition, despite substantial cross-talker variability. Believed to be critical to this ability are auditory normalization mechanisms whereby listeners adapt to individual differences in vocal tract physiology in vowel perception. This study asks what types of computations are involved in such normalization. Two 8-way alternative forced-choice experiments assessed L1 listeners' categorizations across the entire US English vowel space—both for unaltered and for synthesized stimuli. Listeners' responses in these experiments were compared against the predictions of twenty influential normalization accounts that differ starkly in the inference and memory capacities they imply for speech perception. Listeners' responses were best explained by *extrinsic* normalization accounts, suggesting that listeners learn and store distributional properties of talkers' speech. Of the extrinsic accounts, it was the *computationally least complex* variants that best fit listeners' responses, using a single parameter. These findings have consequences for any research that aims to investigate the perceptual, social, and linguistic information of vowel productions. This includes research in phonetics and phonology, sociolinguistics, and language acquisition. In these fields, it remains common to employ normalization accounts that the present study confirms to be inadequate models of human perception (e.g., Lobanov normalization).

[a]anna.persson@su.se

I. INTRODUCTION

One of the central challenges for speech perception originates in cross-talker variability: depending on the talker, the same acoustic signal can encode different sound categories (Allen *et al.*, 2003; Liberman *et al.*, 1967; Newman *et al.*, 2001). This results in ambiguity in the mapping from acoustics to words and meanings. Research has identified several mechanisms through which listeners resolve this ambiguity, ranging from early perceptual processes, to adaptation of phonetic categories, all the way to adjustments in post-linguistic decision processes (for review, see Xie *et al.*, 2023). The present study focuses on the first type of mechanism, early auditory processes that transform and normalize the acoustic input into the perceptual cues that constitute the input to linguistic processing (for reviews, Barreda, 2020; Johnson and Sjerps, 2021; McMurray and Jongman, 2011; Stilp, 2020; Weatherholtz and Jaeger, 2016). We seek to respond, in particular, to recent calls to put theories of adaptive speech perception to stronger tests (Baese-Berk *et al.*, 2018; Schertz and Clare, 2020; Xie *et al.*, 2023).

Evidence for the presence of early normalization mechanisms comes from neuroimaging and neurophysiological studies (e.g., Oganian *et al.*, 2023; Skoe *et al.*, 2021). These studies have decoded effects of talker identity from subcortical brain areas like the brain stem, and thus prior to the cortical regions believed to encode linguistic categories (e.g., Sjerps *et al.*, 2019; Tang *et al.*, 2017). This includes brain responses that lag the acoustic signal by as little as 20-50 msecs (Lee, 2009), suggesting very fast and highly automatic processes. By removing talker-specific variability from the phonetic signal early, auditory normalization

offers elegant and effective solutions to cross-talker variability, that might reduce the need for more complex adaptation of individual phonetic categories further upstream (Apfelbaum and McMurray, 2015; Xie *et al.*, 2023).[1]

While it is relatively uncontroversial *that* normalization contributes to robust speech perception, it is still unclear what types of computations this implicates. We address this question for the perception of vowels, which cross-linguistically relies on peaks in the distribution of spectral energy over acoustic frequencies (formants). Vowel perception has long been a focus in research on normalization (e.g., Bladon *et al.*, 1984; Fant, 1975; Gerstman, 1968; Johnson, 2020; Joos, 1948; Lobanov, 1971; Miller, 1989; Nearey, 1978; Nordström and Lindblom, 1975; Syrdal and Gopal, 1986; Traunmüller, 1981; Watt and Fabricius, 2002; Zahorian and Jagharghi, 1991; for review, see Barreda, 2020), with some reviews citing over 100 competing proposals (Carpenter and Govindarajan, 1993). Importantly, these accounts differ in the types and complexity of computations they assume to take place during normalization. On the lower end of computational complexity, comparatively simple static transformations of the acoustic signal might suffice to achieve invariance in the mapping from cues to phonetic categories. For example, there is evidence that a transformation of acoustic frequencies (measured in Hz) into the psycho-acoustic Mel-space better describes how listeners perceive differences in the frequency of sine tones (e.g., Fastl and Zwicker, 2007; Stevens and Volkmann, 1940; for a critique, see Greenwood, 1997; Siegel, 1965). It is thus possible that cross-talker variability in vowel pronunciations is effectively reduced when formants are represented in Mel, rather than Hz. Similar arguments have been made about other psycho-acoustic transformations (e.g., Bark, Traunmüller, 1990; ERB, Glasberg

and Moore, 1990; or semitones, Fant *et al.*, 2002). Most of these accounts share that they log-transform acoustic frequencies—in line with neurophysiological evidence that the auditory representations in the brain seem to follow a roughly logarithmic organization, so that auditory perception is (up to a point) more sensitive to differences between lower frequencies than to the same difference between higher frequencies (e.g., Merzenich *et al.*, 1975; for review, see Saenz and Langers, 2014). If such static psycho-acoustic transformations are sufficient for formant normalization, this would offer a particularly parsimonious account of vowel perception as listeners would not have to infer talker-specific properties.

The parsimony of psycho-acoustic transformations contrasts with the majority of accounts for vowel normalization, which introduce additional computations. This includes accounts that normalize formants relative to other information that is available at the same point in the acoustic signal (intrinsic normalization, e.g., Miller, 1989; Peterson, 1961; Syrdal and Gopal, 1986). For example, according to one proposal, listeners normalize vowel formants by the vowel's fundamental frequency or other formants estimated at the same point in time (Syrdal and Gopal, 1986). To the extent that the fundamental frequency is correlated with the talkers' vocal tract size (for review, see Vorperian and Kent, 2007), this allows the removal of physiologically-conditioned cross-talker variability in formant realizations. While such intrinsic accounts arguably entail more computational complexity than static transformations, they do not require that listeners *maintain* talker-specific estimates over time. This distinguishes intrinsic from extrinsic accounts, which introduce additional computational complexity.

According to extrinsic accounts, normalization mechanisms infer and store estimates of talker-specific properties that then are used to normalize subsequent speech from that talker (Gerstman, 1968; Lobanov, 1971; Nearey, 1978; Nordström and Lindblom, 1975; Watt and Fabricius, 2002; for review, see also Weatherholtz and Jaeger, 2016). At the upper end of computational complexity, some accounts hold that listeners continuously infer and maintain both talker-specific means for each formant and talker-specific estimates of each formant's variability (Gerstman, 1968; Lobanov, 1971). These estimates are then used to normalize formants, e.g., by centering and standardizing them (essentially z-scoring formants, Lobanov, 1971), removing cross-talker variability in the distribution of formant values. There are, however, more parsimonious extrinsic accounts that require inference and maintenance of fewer talker-specific properties. The most parsimonious of these is Nearey's *uniform scaling* account, which assumes that listeners infer and maintain a single talker-specific parameter. This parameter (Ψ) can be thought of as capturing the effects of the talkers' vocal tract length on the spectral scaling applied to the formant pattern produced by a talker (Nearey, 1978).[2] Uniform scaling deserves particular mention here as it is arguably one of the most developed normalization accounts, and rooted in principled considerations about the physics of sound and the evolution of auditory systems (for review, see Barreda, 2020).

In summary, hypotheses about the computations implied by formant normalization differ in the flexibility they afford as well as the inference and memory complexity they entail. Considerations about the complexity of inferences—essentially the number of parameters that listeners are assumed to estimate at any given moment in time—arguably gain in importance in light of the speed at which normalization seems to unfold. In the present

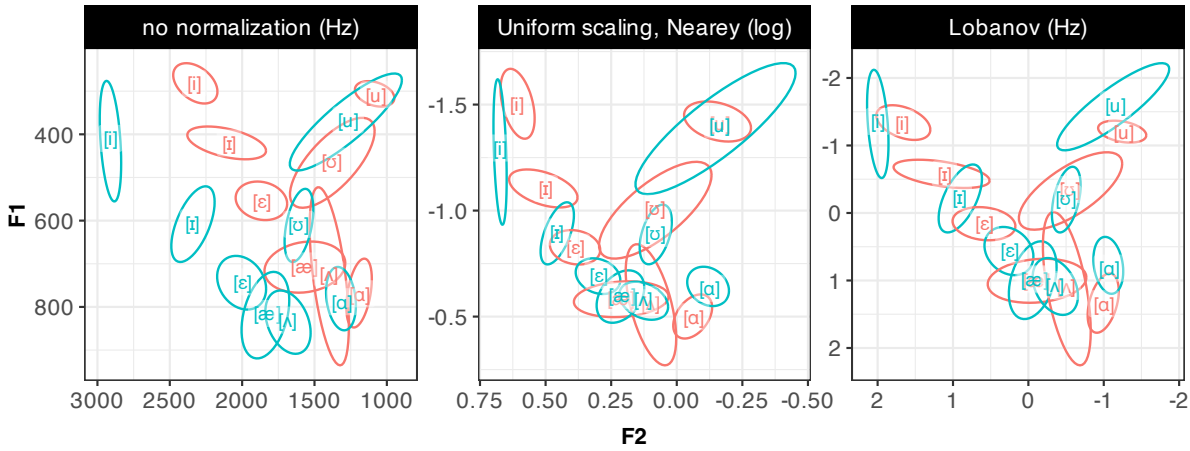


FIG. 1. Illustration of how height, which is positively correlated with vocal tract size, affects vowels' F1 and F2, and how normalization can partially remove this effect. Shown here are realizations of all 8 monophthong vowels of US English by a short (cyan) and a tall native talker (red). **Panel A:** In the acoustic space, prior to any normalization (Hz). **Panel B:** After uniform scaling (Nearey, 1978). **Panel C:** After Lobanov normalization (Lobanov, 1971). The present study compares three of these accounts, along with 17 other normalization accounts.

study, we thus ask whether computationally simple accounts are sufficient to explain human vowel perception.

While previous research has compared normalization accounts across languages, most of this work has evaluated proposals in terms of how well the normalized phonetic space supports the separability of vowel categories (Adank *et al.*, 2004; Carpenter and Govindarajan, 1993; Cole *et al.*, 2010; Escudero and Bion, 2007; Johnson and Sjerps, 2021; Syrdal, 1985). This approach is illustrated in Figure 1. These studies have found that computationally more complex accounts—which also afford more flexibility—tend to achieve higher category separability and higher categorization accuracy (for review, see Persson and Jaeger, 2023). This includes Lobanov normalization, which continues to be highly influential in, for example, variationist and sociolinguistic research because of its effectiveness in removing

cross-talker variability (for a critique, see Barreda, 2021). It is, however, by no means clear that human speech perception employs the same computations that achieve the best category separability or accuracy (see also discussion in Barreda, 2021; Nearey and Assmann, 2007).

A substantially smaller body of research has addressed this question by comparing normalization accounts against *listeners' perception* (Barreda and Nearey, 2012; Barreda, 2021; Nearey, 1989; Richter *et al.*, 2017; for a review, see Whalen, 2016). Interestingly, these works seems to suggest that computationally simpler accounts might provide a better fit against human speech perception than the influential Lobanov model (Barreda, 2021; Richter *et al.*, 2017). For example, Barreda (2021) compared the predictions of uniform scaling and Lobanov normalization against listeners' categorization responses in a forced-choice categorization task over parts of the US English vowel space. In his experiment, listeners' categorization responses were better predicted by uniform scaling than by Lobanov normalization. Findings like these suggest that comparatively simple corrections for vocal tract size—such as uniform scaling—might provide a better explanation of human perception than more computationally complex accounts (see also Johnson, 2020; Richter *et al.*, 2017).

This motivates the present work. We take a broad-coverage approach by comparing the 20 normalization accounts in Table I against the perception of all 8 monophthongs of US English ([i] as in *heed*, [ɪ] in *hid*, [ɛ] in *head*, [æ] in *had*, [ʌ] in *hut*, [ʊ] in *hood*, [u] in *who'd*, [ɑ] in *odd*).[3] We do so for the perception of both natural and synthesized speech. Our broad-coverage approach complements previous studies, which have typically compared a small number of accounts (up to 3) and focused on parts of the vowel inventory,

and thus parts of the formant space (typically 2-4 vowels, Barreda, 2021; Barreda and Nearey, 2012; Nearey, 1989; Richter *et al.*, 2017). The accounts we consider include the most influential examples of psycho-acoustic transformations (Fant *et al.*, 2002; Glasberg and Moore, 1990; Stevens and Volkmann, 1940; Traunmüller, 1981), intrinsic (Syrdal and Gopal, 1986), extrinsic (Gerstman, 1968; Johnson, 2020; Lobanov, 1971; McMurray and Jongman, 2011; Nearey, 1978; Nordström and Lindblom, 1975), and hybrid accounts that contain intrinsic and extrinsic components (Miller, 1989). This broad-coverage approach allows us to assess, for example, whether the preference for computationally simple accounts observed in Barreda (2021) replicates on new data that span the entire vowel space. It also allows us to ask whether accounts even simpler than uniform scaling—such as psycho-acoustic transformations—provide an even better fit to human perception.

Next, we motivate and describe the two experiments we conducted. Then we compare the normalization accounts in Table I against listeners responses from these experiments.

A. Open Science Statement

All stimulus recordings, results, and the code for the experiment, data analysis, and computational modeling for this article can be downloaded from OSF at https://osf.io/zemwn/. The OSF repo also include extensive supplementary information (SI). Both the article and SI are written in R markdown, allowing readers to replicate our analyses with the click of a button, using freely available software (R Core Team, 2023; RStudio Team, 2020). Readers can revisit the assumptions we committed to for the present project—for example, by substituting alternative normalization accounts or categorization models. Researchers can also substitute their own experiments on vowel normalization for our Experiments 1a and 1b, to see whether our findings generalize to novel data. We see this as an important contribution of the present work, as it should make it substantially easier to consider additional normalization accounts—including variants to the accounts we considered—and to assess the generalizability of the conclusions we reach based on the present data.

II. EXPERIMENTS 1A AND 1B

To compare the performance of different normalization accounts against listeners' perception, we conducted two small web-based experiments on US English listeners' perception of US English vowels. The two experiments employ the same 8-alternative forced-choice vowel categorization task (Figure 2), and differ only in the whether they employed 'natural' (Ex-

TABLE I. Normalization accounts considered in the present study. Unless otherwise marked, formant variables (Fs) in the right-handside of normalization formulas are in Hz.

		Normalization procedure	Perceptual scale	Source	Formula
		n/a	Hz	n/a	n/a
trans-formation		—	log		$F_n^{log} = ln(F_n)$
		—	Bark	Traunmüller (1990)	$F_n^{Bark} = \frac{26.81 \times F_n}{1960 + F_n} - 0.53$
		—	ERB	Glasberg & Moore (1990)	$F_n^{ERB} = 21.4 \times \log_{10}(1 + F_n \times 0.00437)$
		—	Mel	Stevens & Volkmann (1940)	$F_n^{Mel} = 2595 \times \log_{10}(1 + \frac{F_n}{700})$
		—	Semitones conversion	Fant et al. (2002)	$F_n^{ST} = 12 \times \frac{ln(\frac{F_n}{100})}{ln}$
intrinsic		Syrdal & Gopal 1 (Bark-distance model)	Bark	Syrdal & Gopal (1986)	$F1^{SyrdalGopal1} = F1^{Bark} - F0^{Bark}$ $F2^{SyrdalGopal1} = F2^{Bark} - F1^{Bark}$
		Syrdal & Gopal 2 (Bark-distance model)			$F1^{SyrdalGopal2} = F1^{Bark} - F0^{Bark}$ $F2^{SyrdalGopal2} = F3^{Bark} - F2^{Bark}$
		Miller (formant-ratio)	log	Miller (1989)	$SR = k(\frac{GMf0}{k})^{1/3}$ $F1^{Miller} = log(\frac{F1}{SR})$ $F2^{Miller} = log(\frac{F2}{F1})$ $F3^{Miller} = log(\frac{F3}{F2})$
extrinsic	centering	Uniform scaling, Nearey	log	Nearey (1978)	$F_n^{Nearey} = \ln(F_n) - mean(ln(F))$
		Uniform scaling, Nordström & Lindblom	Hz	Nordström & Lindblom (1975)	$F_n^{NordströmLindblom} = \frac{F_n}{mean(\frac{F3[F1>600]}{2.5})}$
		Uniform scaling, Johnson	Hz	Johnson (2020)	$F_n^{Johnson} = \frac{F_n}{mean(\frac{F1}{0.5}, \frac{F2}{1.5}, \frac{F3}{2.5})}$
		Nearey's formantwise log-mean	log	Nearey (1978)	$F_n^{Nearey} = \ln(F_n) - mean(ln(F_n))$
		C-CuRE	Hz	McMurray & Jongman (2011)	$F_n^{C-CuRE} = F_n - mean(F_n)$
		—	Bark		
		—	ERB		
		—	Mel		
		—	Semitones conversion		
	standardizing	Gerstman (range normalization)	Hz	Gerstman (1968)	$F_n^{Gerstman} = 999 \times \frac{F_n - F_n^{min}}{F_n^{max} - F_n^{min}}$
		Lobanov (z-score)	Hz	Lobanov (1971)	$F_n^{Lobanov} = \frac{F_n - mean(F_n)}{sd(F_n)}$

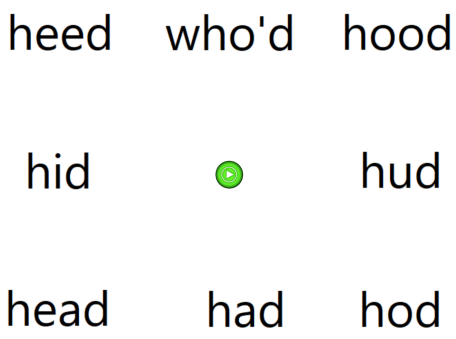


FIG. 2. Screen shot of the eight-alternative forced-choice (8-AFC) task used in both Experiment 1a and 1b.

periment 1a) or synthesized stimuli (Experiment 1b). To the best of our knowledge, these two experiments are the first designed to compare normalization accounts against listeners' perception over the entire monophthong inventory of a language.

Experiment 1a employs recordings of *hVd* word productions from a female talker of US English, these recordings are 'natural' in the sense that they were not synthesized or otherwise phonetically manipulated. One consequence of this is that the formant values of these recordings are clustered around the category means, and thus span only a comparatively small part of the phonetic space. This can limit the statistical power to distinguish between competing accounts. Natural recordings furthermore vary not only along the primary cues to vowel quality in US English (F1, F2) but also along potential secondary cues (e.g., F0, F3, and vowel duration) as well as other unknown properties, which can make it difficult to discern whether the performance of a normalization model is due to the normalization itself or other reasons, e.g., because a normalized cue happens to correlate with another cue that listeners are sensitive to but that is not included in the model.

Experiment 1b thus adopts an alternative approach and uses synthesized vowels. Unlike most previous work, which has used isolated vowels as stimuli (Barreda, 2021; Barreda and Nearey, 2012; Nearey, 1989; Richter *et al.*, 2017), Experiment 1b uses synthesized *hVd* words to facilitate comparison to Experiment 1a. This allowed us to sample larger parts of the F1-F2 space, which has two advantages. First, it allowed us to collect responses over parts of the formant space for which we expect listeners to have more uncertainty, and thus exhibit more variable responses. This can increase the statistical power to distinguish between competing accounts. Second, differences in the predictions of competing normalization account will tend to become more pronounced with increasing distance from the category centers. By collecting responses at those locations, we can thus increase the contrast between competing accounts.

The use of resynthesized stimuli does, however, also come with potential disadvantages. Synthesized stimuli can suffer in ecological validity, lacking correlations between cues, and across the speech signal (e.g., due to co-articulation) that are characteristic of human speech. This raises questions about the extent to which processing of such stimuli engages the same mechanisms as everyday speech perception. Additionally, it is possible that the use of robotic sounding synthesized speech affects listener engagement. This can lead to an increased rate of attentional lapses, and thus a decrease in the proportion of trials on which listeners' responses are based on the acoustics of the speech stimulus rather than random guessing (compare, e.g., Kleinschmidt, 2020; Tan and Jaeger, 2024). By comparing normalization accounts against both natural and synthesized stimuli, we investigate the extent to which

the accounts that best describe human perception depend on the type of stimuli used in the experiment.

A. Methods

1. Participants

We recruited 24 (Experiment 1a) and 24 (Experiment 1b) participants from Amazon's Mechanical Turk. Participants were paid \$6/hour prorated by the duration of the experiments (15 minutes). Participants only saw the experiment advertised, and could only participate in it, if (i) they were located within the US, (ii) had an approval rating of 99% or higher, (iii) met the software requirements (a recent version of the Chrome browser engine), and (iv) had not previously completed any other experiments on vowel perception in our lab. Before the experiment could be accepted, participants had to confirm that they were (i) native speakers of US English (defined as having spent their childhood until the age of 10 speaking English and living in the United States), (ii) in a quiet room without distractions, (ii) wearing over-the-ear headphones. Participants' responses were collected via Javascript developed by the Human Language Processing Lab at the University of Rochester (Kleinschmidt *et al.*, 2021).

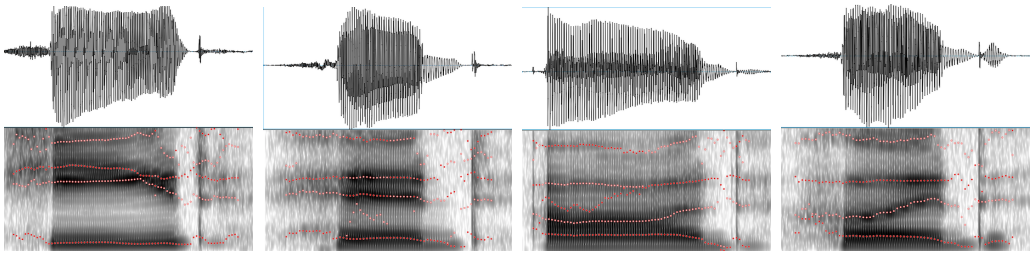
An optional post-experiment survey recorded participant demographics using NIH prescribed categories, including participant sex (Male: 27, Female: 20), age (mean = 35.5 years; SD = 11.4; 95% quantiles = 24-63.25 years), race (White: 36, Asian: 3, Black: 6, multiple:

1, declined to report: 1), and ethnicity (Non-Hispanic: 42, Hispanic: 4, declined to report: 1).

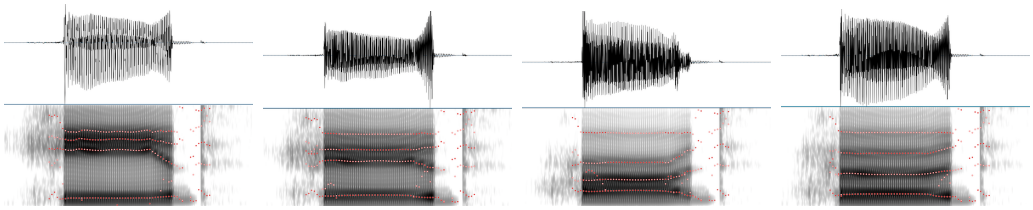
2. *Materials*

Experiment 1a employed *hVd* word recordings by one adult female talker from a phonetically annotated database of L1-US English vowel productions (Xie and Jaeger, 2020). Specifically, we used all 9 recordings of each of the eight *hVd*-words—*heed*, *hid*, *head*, *had*, *hut*, *odd*, *who'd*, *hood* (the use of "hut" and "odd" rather than "hud" and "hod" follows Assmann *et al.*, 2008; but see Hillenbrand *et al.*, 1995).

The stimuli for Experiment 1b were synthesized from a single *had* recording used in Experiment 1a. Specifically, we used a script (based on descriptions in Wade *et al.*, 2007) in Praat (Boersma and Weenink, 2022) to concatenate the original /h/ with a synthesized vowel and the original /d/ recording. Unlike in Experiment 1a, all eight words thus had an *hVd* context (including "hud" and "hod", rather than "hut" and "odd"). The Praat script first segmented the original *had* token into /h/, /ae/ and /d/ portions. It then filtered the /h/ sound inversely with its LPC, and concatenated this neutral fricative sound with a complex waveform generated from the pitch and intensity patterns of the original vowel, to create a neutral hV-section that did not reflect any vocal tract resonances. The script then created a formant grid that filtered the hV-section to create the intended vowel, and finally concatenated this segment to the final /d/ to create an *hVd* word. For each *hVd* word, the formant grid was populated with the F1, F2 and F3 values that we handed to the script at five time-points transitioning from the /h/ to the vowel, to the final /d/ through



(a) natural *heed* (b) natural *hid* (c) natural *odd* (d) natural *hood*



(e) resynthesized *heed* (f) resynthesized *hid* (g) resynthesized *hod* (h) resynthesized *hood*

FIG. 3. **Top:** Spectrograms of four natural recordings from Experiment 1a. **Bottom:** Same for four synthesized tokens with similar formant values from Experiment 1b.

linear interpolation. Formant bandwidths were 500 Hz at the initial two time-points (the /h/ and beginning of transition to vowel), and then decreased linearly during vowel onset and throughout the final three time-points to 50 Hz (F1), 100 Hz (F2), 200 Hz (F3), 300 Hz (F4), and 400 Hz (F5-F8, following Wade *et al.*, 2007). The bandwidth manipulation implied that formants became stronger as the vowel unfolded (see Figure 3). We used this approach to create synthesized vowels for arbitrary F1-F2 combinations. F3 was set based on those F1-F2 values. Specifically, we ran a linear regression over the natural productions of the talker from Experiment 1a, predicting F3 from F1, F2 and their interaction. We then used that regression to predict F3 values for any F1-F2 combination in Experiment 1b. F4 to F8, as well as vowel duration, were held identical across all tokens (using the same values as Wade *et al.*, 2007).

We generated 146 synthesized *hVd* recordings that spanned the F1 and F2 space. The specific F1-F2 locations chosen were determined by a mix of modeling (using ideal observers described in the next section to predict listeners' categorization responses) and intuition. Specifically, we selected 64 recordings that we expected to fall within the bivariate 95% confidence intervals (CIs) of the eight US English monophthongs, and 82 recordings that we expected to fall between those CIs. Figure 4 under *Results* shows the distribution of stimuli for both experiments. Of note, our procedure also generated formant combinations that are physiologically unlikely to have all been produced by the same talker during 'normal' vowel production (also known as "off-template" instances, Nearey, 1978).

3. Procedure

The procedure for both experiments was identical. Live instances of each experiment can be found at `https://www.hlp.rochester.edu/experiments/DLPL2S/experiment-A/experiments.html`. At the start of the experiment, participants acknowledged that they met all requirements and provided consent, as per the Research Subjects Review Board of the University of Rochester. Before starting the experiment, participants performed a sound check and signed a consent form. Participants were then instructed to listen to a female talker saying words, and click on the word on screen to report what word they heard. On each trial, all eight *hVd*-words were displayed on screen. Half of the participants in each experiment saw the response options organized as in Figure 2 (resembling the IPA representation of a vowel space), half saw the response options in the opposite order (flipping top and bottom and left and right in Figure 2). Each trial started with the response grid on screen, together

with a light green dot centered on screen. After 1000 ms, an *hVd* recording played, and participants indicated their response by a mouse-click. After a 1000 ms intertrial interval, the screen reset, and the next trial started.

In both experiments, participants heard two blocks of the materials described in the previous sections, for a total of 144 trials in Experiment 1a and 292 trials in Experiment 1b. Presentation within each block was randomized for each participant. Participants were not informed about the block structure of the experiment.

After completing the experiment, participants filled out a language background questionnaire and the optional demographic survey. On average, participants took 10.3 minutes to complete Experiment 1a (SD = 6.6) and 18.4 minutes for Experiment 1b (SD = 7.3).

4. Exclusions

We excluded participants who failed to follow instructions and did not wear over-the-ear headphones (as indicated in the post-experiment survey). We also excluded participants with mean (log-transformed) reaction times that were unusually slow or fast (absolute z-score over by-participant means > 3), or if they clearly did not do the task (e.g., by answering randomly). This excluded 6 participants from Experiment 1a and 2 from Experiment 1b (for details, see §2 A).

We further excluded all trials that were unusually fast or slow. Specifically, we first z-scored the log-transformed response times *within each participant* and then z-scored these z-scores *within each trial* across participants. Trials with absolute z-scores > 3 were removed from analysis. This double-scaling approach was necessary as participants' response times

297 decreased substantially over the first few trials and then continued to decrease less rapidly
298 throughout the remainder of the experiment. The approach removes response times that are
299 unusually fast or slow *for that participant at that trial*, while avoiding specific assumptions
300 about the shape of the speed up in response times across trials. This excluded 1.2% of the
301 trials in Experiment 1a and 1.1% in Experiment 1b. This left for analysis 2565 observa-
302 tions from 18 participants in Experiment 1a, and 6354 observations from 22 participants in
303 Experiment 1b.

304 ## B. Results

305 Participants' categorization responses in Experiments 1a and 1b are shown in Figure 4,
306 with larger labels indicating recordings that participants agreed on more.[4] We make two ob-
307 servations. The first pertains to the degree of (dis)agreement between the two experiments.
308 The second observation pertains to the degree of (dis)agreement across participants within
309 each experiment.

310 ### *1. Similarities and differences between Experiments 1a and 1b*

311 Unsurprisingly, participants in both experiments divided the F1-F2 space into the eight
312 vowel categories in ways that qualitatively resembled each other (after taking into account
313 that Experiment 1b covers a larger range of F1-F2 values). Also unsurprisingly, there were
314 some differences between participants' responses across the two experiments, at least when
315 plotted in Hz. For example, [u] rarely was the most frequent response in Experiment 1b,
316 even for stimuli that were predominantly categorized as [u] in Experiment 1a. There are at

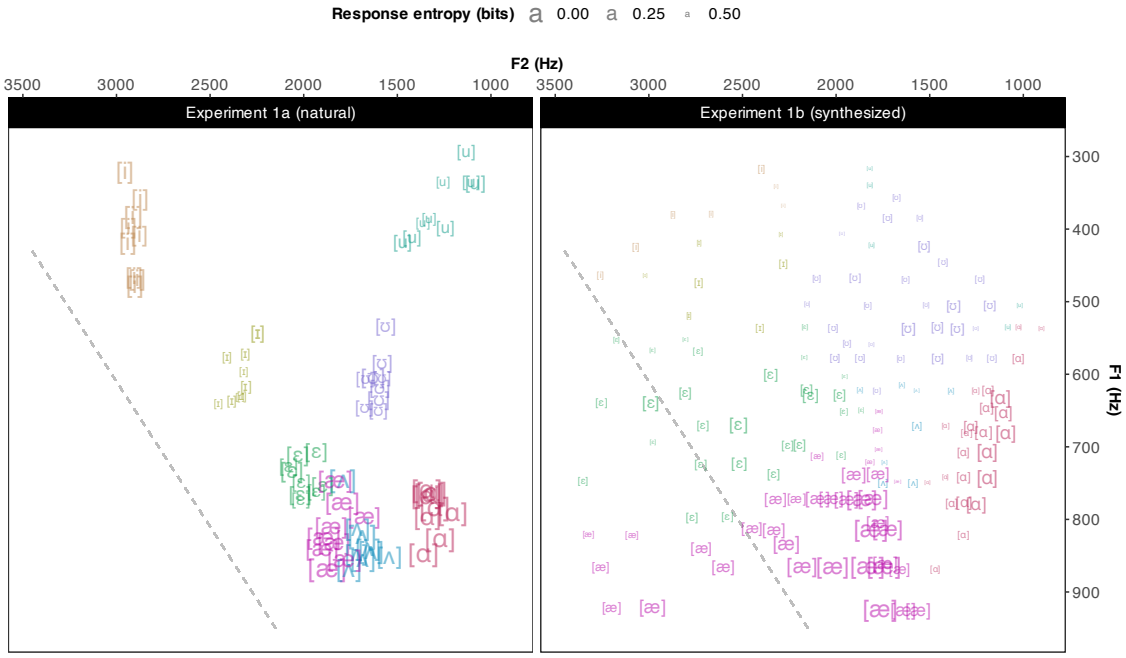


FIG. 4. Summary of listeners' categorization responses in Experiments 1a and 1b in F1-F2 space. The vowel label indicates the most frequent response provided across participants on each test location. Size indicates how consistent responses were across participants, which larger symbols indicating more consistent responses (lower entropy). F1-F2 combinations below the gray dashed line are articulatory unlikely to come from the same talker.

least two reasons to expect such differences. First, stimuli with similar F1-F2 values across the two experiments still differed in other acoustic properties (e.g. vowel duration or F3). These acoustic differences might have affected participants' responses. Second, it is possible that *formant normalization* affected participants' responses—i.e., the very mechanism we seek to investigate in the remainder of the paper. The two experiments differ in the means, variances, and other statistical properties that some normalization accounts predict to affect perception. As a consequence, Hz might not be the space in which we should expect identical responses across experiments.

Auxiliary analyses presented in the SI (§2 C) suggest that *some but not all* of the differences in response entropy between the two experiments were caused by the placement of the stimuli in F1-F3 space: when comparing categorization responses for tokens from the two experiments with similar acoustic properties (differences of ≤ 30 Hz along F1 and F2), response entropies still differed substantially (for N = 40 acoustically similar tokens, mean by-item response entropy for Experiment 1a = 0.18 bits, SE = 0.03; Experiment 1b = 0.39 bits, SE = 0.03). We see two mutually compatible explanations. One possibility is that the differences in listeners' responses across the two experiments originate in *normalization*. The two experiments differ in the means, variances, and other statistical properties of their formant distributions—i.e., in the statistical properties that some normalization accounts predict to affect perception. It is, however, also possible that the relation between formants in the synthesized stimuli or some other unknown acoustic-phonetic differences between the experiments explain the difference in response. For example, the absence of vowel inherent spectral change (VISC) or differences in tilt in the synthesized stimuli might have deprived listeners of information that is actually crucial for establishing phonemic identity (Hillenbrand and Nearey, 1999). This would result in increased uncertainty on each trial, leading to increased entropy of listeners' responses. The computational studies we present below shed some light on these two mutually compatible possibilities.

Similarly, the two experiments differed in the extent to which participants agreed with each other. Participants in Experiment 1b exhibited overall less agreement in their responses (mean by-item response entropy = 0.45 bits, SE = 0.01) than participants in Experiment 1a (mean by-item response entropy = 0.23 bits, SE = 0.02). This was expected given that

Experiment 1b explored the entire F1-F2 space, including—by design—formant combinations located *between* the centers of the natural vowel categories. Experiment 1b therefore achieved its goal of eliciting less categorical response distributions, which is expected to facilitate comparison of competing normalization accounts.[5]

Auxiliary analyses presented in the SI (§2 C) suggest that *some but not all* of the differences in response entropy between the two experiments were caused by the placement of the stimuli in formant space: when comparing categorization responses for tokens from the two experiments with similar acoustic properties (differences of ≤ 30 Hz along F1 and F2), response entropies still differed substantially (for N = 40 acoustically similar tokens, mean by-item response entropy for Experiment 1a = 0.18 bits, SE = 0.03; Experiment 1b = 0.39 bits, SE = 0.03). We see two mutually compatible explanations. First, similar to the differences between experiments in the dominant response pattern discussed above, differences in the degree of agreement between participants might originate in *normalization*. Second, it is possible that the relation between formants in the synthesized stimuli or some other unknown acoustic-phonetic differences between the experiments explain the difference in response. For example, the absence of vowel inherent spectral change (VISC) or differences in tilt in the synthesized stimuli might have made it difficult for listeners in Experiment 1b to reliably estimate the formants (Hillenbrand and Nearey, 1999). This would result in increased uncertainty on each trial, leading to increased entropy of listeners' responses. The computational studies we present below shed some light on these two mutually compatible possibilities.

2. *Similarities and differences between participants*

Since the intended category was known for Experiment 1a, it was possible to calculate participants' recognition accuracy. As also evident in the left panel of Figure 4, participants' most frequent response *always* matched the intended vowel in Experiment 1a. Overall, participants' responses matched the intended vowel on 81.2% (SE = 4.8%) of all trials (Experiment 1b had no such ground truth). This is much higher than chance (12.5%). It is, however, also quite a bit lower than 100%. To better understand the reasons for this, Figure 5A plots the confusion matrix. This suggests that participants' performance was largely affected by confusions between [ɪ]-to-[ɛ] (*hid*-to-*head*), [ɛ]-to-[æ] (*head*-to-*had*), and [ʉ]-to-[ʊ] (*who'd*-to-*hood*).

One plausible explanation for this pattern of vowel confusions lies in the substantial variation that exists across US English dialects (Labov *et al.*, 2006), Differences in the realization of vowel categories, and associated representations, across dialects will directly affect the expected classification for any given token. In addition, listeners might differ in terms of experience with different dialects, or in the dialect they attribute to the talker who produced the stimuli. To test this hypothesis, we calculated the [ɪ]-to-[ɛ], [ɛ]-to-[æ], and [ʉ]-to-[ʊ] confusion rates for each participant in Experiment 1a. These data are summarized in the left panel of Figure 5B. The data in the left panel suggest that most participants in Experiment 1a either heard [ɪ] tokens consistently as the intended [ɪ] (clustering on the left side of the panel) or as [ɛ] (clustering on the right side of the panel). Only a few participants exhibited mixed responses for items intended to be [ɪ]. Tellingly, many of the participants

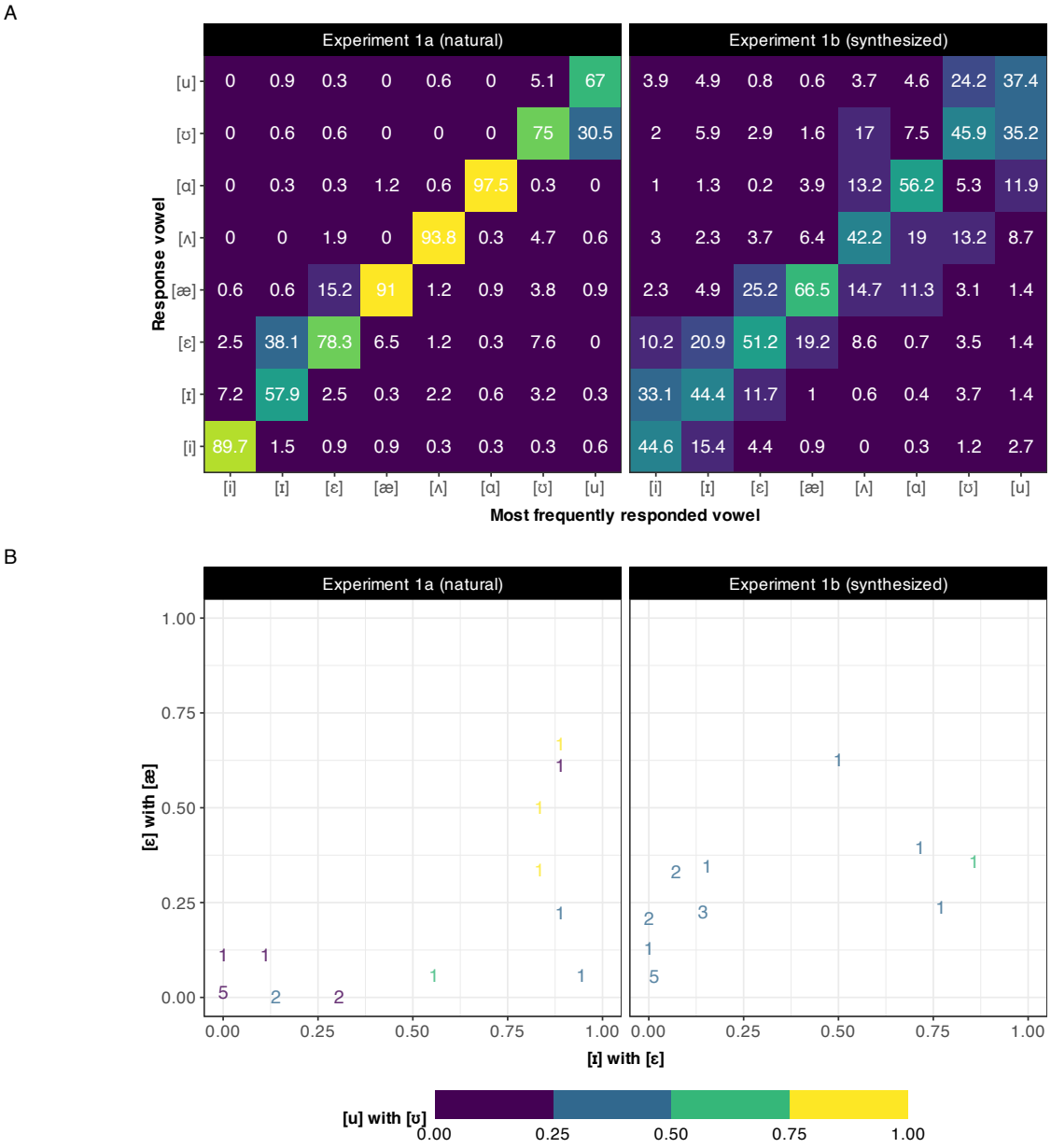


FIG. 5. Category confusability in Experiments 1a and 1b. **Panel A** summarizes the category confusability. Since correct responses were not defined for Experiment 1b, we grouped items along the x-axis based on most frequent response that listeners provided (for Experiment 1a, this was always identical to the intended response). Response percentages sum to 100 in each column, showing the response distribution depending on the most frequent response. **Panel B** summarizes individual differences across listeners, in terms of the listener-specific confusability of [ɪ] with [ɛ] (x-axis), [ɛ] with [æ] (y-axis), and [u] with [ʊ] (color fill).

who exhibited increased [ɪ]-to-[ɛ] confusion *also* exhibited increased [ɛ]-to-[æ] confusion. This

is precisely what would be expected by listeners who assume a dialect in which these vowels are articulated lower (with higher F1) than in the dialect of the talker in Experiment 1a. A similar, but less pronounced, pattern was also found with regard to [ʉ]-to-[ʊ] confusions.[6] Finally, a qualitatively similar relation between [ɪ]-to-[ɛ], [ɛ]-to-[æ], and [ʉ]-to-[ʊ] confusions was also observed in Experiment 1b (right panel of Figure 5B), though the pattern was unsurprisingly less pronounced given that the stimuli in Experiment 1b by design often fell into the ambiguous region *between* vowels. Taken together, Experiments 1a and 1b thus suggest that systematic dialectal differences between participants may be a substantial contributor of the relatively low correct classification rate observed for experiment 1a.

This highlights two important points. First, the data from Experiment 1a demonstrate the perceptual challenges associated with an unfamiliar talker: in the absence of lexical or other context to distinguish between the eight available response options, listeners can only rely on the acoustic information in the input. In such a scenario, even listeners who are in principle familiar with the dialect spoken by the talker have comparatively little information to determine the talker's dialect, making apparent what Matt Winn (2018) aptly summarizes as "speech [perception] is not as acoustic as [we] think". Second, when dialect variability is taken into account, listeners' recognition accuracy improved substantially. After removing 7 listeners who heard more than 50% of the [ɪ] items as [ɛ], *all* vowels were correctly recognized at least 88.3% of the time (overall accuracy = 95.9%). This suggests that dialect differences affected the recognition of all vowels. This aspect of our results serves as an important reminder that formant normalization is only expected to erase inter-talker variability associated with *physiological* differences: variation in dialect, sociolect, or other

non-physiologically-conditioned variation pose separate challenges to human perception, and require additional mechanisms (see discussion in Barreda, 2021; Weatherholtz and Jaeger, 2016). This introduces noise—variability in listeners' responses that cannot be accounted for by normalization—to any comparison of normalization accounts, potentially reducing the power to detect differences between accounts.

III. COMPARISON OF NORMALIZATION ACCOUNTS

In order to evaluate normalization accounts against speech perception, it is necessary to map the phonetic properties of stimuli—under different hypotheses about normalization—onto listeners' responses in Experiments 1a and 1b. Previous work has done so by directly predicting listeners' responses from the raw or normalized phonetic properties of stimuli (Apfelbaum and McMurray, 2015; Barreda, 2021; Crinnion *et al.*, 2020; McMurray and Jongman, 2011; Nearey, 1989). For example, McMurray and Jongman used multinomial logistic regression to predict 8-way fricative categorization responses in US English (see also Barreda, 2021).

Here we pursued an alternative approach by committing to a core assumption common to contemporary theories of speech perception: that listeners acquire implicit knowledge about the probabilistic mapping from acoustic inputs to linguistic categories, and draw on this knowledge during speech recognition (e.g., TRACE, McClelland and Elman, 1986; exemplar theory, Johnson, 1997; Bayesian accounts, Luce and Pisoni, 1998; Nearey, 1990; Norris and McQueen, 2008; ASR-inspired models like DIANA or EARSHOT, ten Bosch *et al.*, 2015; Magnuson *et al.*, 2020). Using a general computational framework for adaptive speech

perception (ASP, Xie *et al.*, 2023) we trained Bayesian ideal observers to capture the expectations that a 'typical' L1 adult listener might have about the formant-to-vowel mappings of US English. We approximated these expectations using a database of L1-US English vowel productions (Xie and Jaeger, 2020)—transformed to reflect the different normalization accounts. We then ask which of the different ideal observer models—corresponding to different hypotheses about formant normalization—best predicts listeners' responses in Experiments 1a and 1b.

A welcome side effect of this is that far fewer degrees of freedom (DFs) are required to predict listeners' responses. For example, using ordinary multinomial logistic regression trained on our perceptual data to predict 8-way vowel categorization as a function of F1, F2 and their interaction would require up to 28 DFs. This problem increases with the number of cues considered. Because the model is trained on data that is independent of our perceptual data, the ASP-based approach we employ instead uses only 2 DFs (i.e., parameters estimated based on our perceptual data) to mediate the mapping from stimuli properties to listeners' responses, regardless of the number of cues considered. Over the next few sections, we describe how this parsimony is made possible through a commitment to strong linking hypotheses motivated by theories of speech perception.

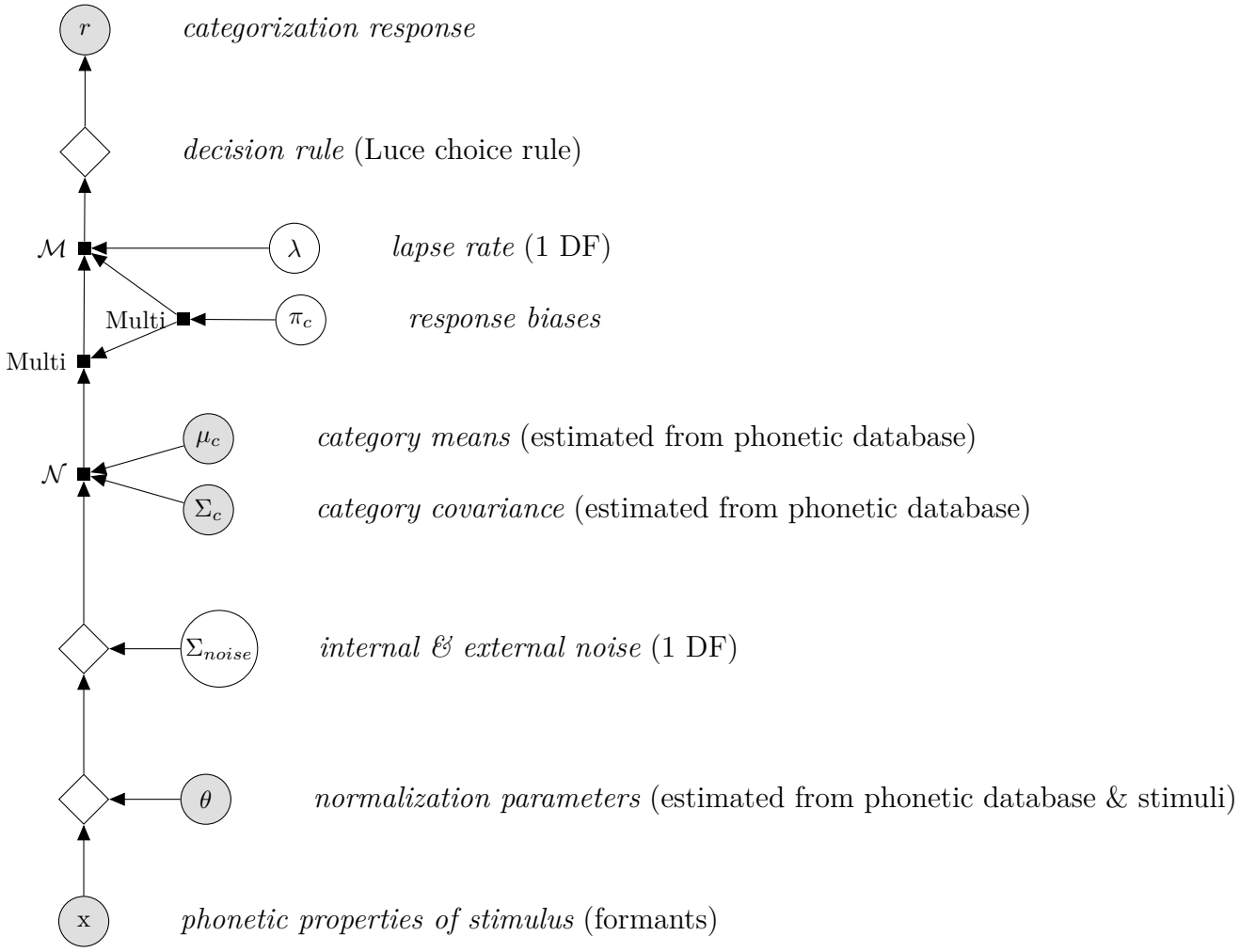


FIG. 6. Graphical model of ASP's general categorization framework (adapted for the current purpose from Xie et al., 2023, Figure 4). Here $J = 8$ (the eight vowel response options in Experiments 1a and 1b). We use this framework to compare normalization accounts against listeners' categorization responses from Experiments 1a and 1b. Filled gray circles represent variables that are known to the researcher. Empty circles represent latent variables that are not observable. Diamonds represent variable-free processes, annotated with the distributions resulting at that level of the model: $\mathcal{N}$(ormal), Multi(nomial), and $\mathcal{M}$(ixture) distributions.

A. Methods

1. *A general-purpose categorization model for J-AFC categorization tasks*

Figure 6 summarises ASP's categorization model for a J-alternative forced-choice task (for an in-depth description, we refer to Xie *et al.*, 2023). The model combines Bayesian ideal observers (as used in e.g., Clayards *et al.*, 2008; Feldman *et al.*, 2009; Norris and McQueen, 2008; Xie *et al.*, 2021; for a closely related approach, see also Nearey and Hogan, 1986) with psychometric lapsing models (Wichmann and Hill, 2001). To reduce researchers' degrees of freedom, we adopt all assumptions made in Xie *et al.* (2023), and do not introduce additional assumptions.

Starting at the bottom of the figure, the phonetic input x is normalized. Here, $x =$ the F1 and F2 of our stimuli (the SI, §3 E reports additional analyses that instead employ F1-F3; these analyses support the same conclusion presented here, and we mention them below where relevant). The specific computations applied to the input x depend on the normalization accounts (see Table I). We use θ to refer to the parameters required by the normalization account. For example, for the uniform scaling account (Nearey, 1978), θ is the overall mean of all log-transformed formants. For Lobanov normalization (Lobanov, 1971), θ is a vector of means and standard deviations for each formant (in Hz).

The normalized input is then perturbed by perceptual and environmental noise. Following Feldman *et al.* (2009), this noise is assumed to be Gaussian distributed centered around the transformed stimulus with noise variances that are independent and identical for all formants (i.e., Σ_{noise} is a diagonal matrix, and all diagonal entries have the same value). Next, the

likelihood of the normalized percept under each of the eight vowel categories is calculated, $p(F1, F2|vowel)$. This requires specifying listeners' expectations about the cue-to-category mapping (listeners' likelihood function). We followed Xie *et al.* (2023) and previous work and assume that each vowel maps onto a multivariate Gaussian distribution over the phonetic cues, here bivariate Gaussians over F1 and F2 (cf. Clayards *et al.*, 2008; Feldman *et al.*, 2009; Kleinschmidt and Jaeger, 2015; Norris and McQueen, 2008; Xie *et al.*, 2021). The posterior probability of each vowel is obtained by combining its likelihood with its prior probability or response bias π_c, according to Bayes theorem:[7]

$$p(vowel = c|F1, F2) = \frac{\mathcal{N}(F1, F2|\mu_c, \Sigma_c + \Sigma_{noise}) \times \pi_c}{\sum_{c_i} \mathcal{N}(F1, F2|\mu_{c_i}, \Sigma_{c_i} + \Sigma_{noise}) \times \pi_{c_i}} \quad (1)$$

Up to this point, the model is identical to a standard Bayesian ideal observer over noisy input (Feldman *et al.*, 2009; Kronrod *et al.*, 2016) for which the input has been transformed based on the normalization account. ASP's categorization model adds to this the potential that participants experience attentional lapses—or for other reasons do not respond based on the input—on some proportion of all trials (λ, as in standard psychometric lapsing models, Wichmann and Hill, 2001). On those trials, the posterior probability of a category is determined solely by participants' response bias, which we assume to be identical to the response bias on non-lapsing trials (following Xie *et al.*, 2023). This results in a posterior that is described by weighted mixture of two components, describing participants' posterior on non-lapsing and lapsing trials, respectively:

$$p(vowel = v|F1, F2) = (1 - \lambda)\frac{\mathcal{N}(F1, F2|\mu_c, \Sigma_c + \Sigma_{noise}) \times \pi_c}{\sum_{c_i} \mathcal{N}(F1, F2|\mu_{c_i}, \Sigma_{c_i} + \Sigma_{noise}) \times \pi_{c_i}} + \lambda\frac{\pi_c}{\pi_{c_i}} \quad (2)$$

Finally, a decision rule is applied to the posterior to determine the response of the model, conditional on the input (one of the eight vowels in Experiments 1a and 1b). We followed the gross of research on speech perception and assume Luce's choice rule (Luce, 1959; for discussion, see Massaro and Friedman, 1990). Under this choice rule, the model can be seen as sampling from the posterior, responding with each category proportional to that category's posterior probability.

Next, we describe how we estimated the θs, μ_cs and Σ_cs for each normalization account from a phonetic database. We use this database as a—very coarse-grained—approximation of a the speech input a 'typical' listener might have experienced previously. By fixing θ, μ_c and Σ_c based on the distribution of phonetic cues in the database, we substantially reduce the DFs that are allowed to mediate the mapping from stimulus properties to listeners' responses (following Xie *et al.*, 2023). In addition, this approach naturally penalizes overly complex models by validating these against out-of-sample data. Finally, we describe how we fit the remaining parameters as DFs to participants' responses from Experiments 1a and 1b.

2. *Modeling listeners' prior experience (and guarding against overfitting): θ, μ_c, and Σ_c*

By fixing θ, μ_c, and Σ_c based on a database of vowel *productions*, we impose strong constraints on the functional flexibility of the model in predicting listeners' responses. This benefit is made possible by committing to a strong linking hypothesis—that listeners' categories are learned from, and reflect, the distributional mapping from formants to vowels in previously experienced speech input (e.g., Abramson and Lisker, 1973; Massaro and Friedman, 1990; Nearey and Hogan, 1986). The database we use to approximate listeners' prior experience was originally developed to compare the production of L1 and L2 speakers (Xie and Jaeger, 2020). It contains 9-10 recordings of the 8 *hVd* words from each of 17 (5 female) L1 talkers of a Northeastern dialect of US English (ages 18 to 35 years old). Since Experiments 1a and 1b used recordings of one of these talkers, we excluded that talker prior to fitting training ideal observers on the data. In total, this yields 5842 recordings that are annotated for F0, F1-F3, and vowel duration. The SI (§3 A 1) summarizes the distribution of these cues, and how the different normalization accounts affect those distributions.

To avoid over-fitting the ASP model to the database, we used 5-fold cross-validation: we randomly split the Xie and Jaeger (2020) database into five approximately evenly-sized folds (following Persson and Jaeger, 2023). This split was performed within each vowel to guarantee that all five folds had the same relative amount of data for each vowel category. These splits were combined into five training sets, each containing one of the folds (20% of

the data). This way, each training set was different from the others, increasing the variability between sets.[8]

For each training set and for each normalization account, we then estimated the required normalization parameters θ for all talkers, and normalized all formants based on those talker-specific parameters.This yielded 5 (training sets) * 20 (accounts) = 100 normalized training sets. For each of these normalized training sets, we fit the category means, μ_c, and covariance matrices, Σ_c, of all eight vowels, using the `R` package `MVBeliefUpdatr` (Jaeger, 2024).[9]

This yielded 100 ideal observer models, 5 for each of the 20 normalization accounts in Table I. Of note, the 20 ideal observers fit on each fold differ *only* in the assumptions they make about the normalization that is applied to cues before they are mapped onto the eight vowel categories. Figure 7 visualizes the resulting bivariate Gaussian categories for four of the 20 normalization accounts. This illustrates one advantage of the cross-validation approach: it takes a modest step towards simulating differences across listeners' prior experience (represented by the five different folds).

3. Transforming the stimuli from Experiments 1a and 1b into the normalized phonetic spaces

Next, we transformed the stimuli of Experiments 1a and 1b into the formant space defined by the 20 normalization accounts in Table I. This requires estimating the required normalization parameters θ for each experiment and normalization account. We calculated these θs over all stimuli (of each experiment and normalization account). For example, for the uniform scaling account (Nearey, 1978), we calculated the overall mean of all log-transformed

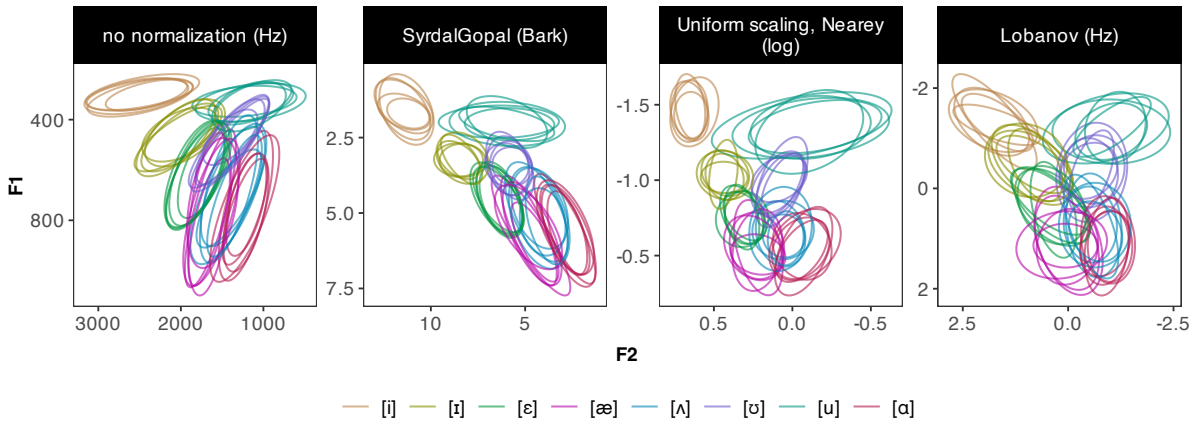


FIG. 7. Visualizing the bivariate Gaussian categories (prior to adding Σ_{noise}) of four example normalization accounts in F1-F2 space. Separate ellipses are shown for each of the five training sets (each set corresponds to one set of eight ellipses). The relative stability of the category ellipses across training sets indicates that the database is sufficiently large for the present purpose.

formants over all stimuli. For Lobanov normalization (Lobanov, 1971), we calculated the mean and standard deviation of each formant (in Hz) over all stimuli. For each combination of experiment and normalization account, we then normalized the stimuli using those parameter estimates.

Combining the 100 normalized training sets described in the previous section with the matching normalized stimuli from each of the two experiments yielded 200 data sets.

4. Noise (Σ_{noise}) and attentional lapses (λ)

Finally, we describe the two parameters of the ASP model that we fit against listeners' responses in Experiments 1a and 1b. These two parameters constitute the only DFs that mediate the link from ideal observers' predictions to listeners' responses, and which are specifically tuned to these. The first DF (Σ_{noise}) models the effects of internal (perceptual) and external (environmental) noise on listeners' perception. While previous work provides

estimates of the internal noise in formant perception, these estimates were obtained under *assumptions* about the relevant formant space. For example, Feldman *et al.* (2009) estimated the internal noise variance to be about 15% of the average category variance along F1 and F2. This estimate was based on the assumption that human speech perception transforms vowel formants into Mel, without further normalization. Since we aim to *test* which normalization account best explains speech perception, we cannot rely on this or other internal noise estimates obtained under a single specific assumption. Additionally, internal noise can vary across individuals and external noise can vary across environments (a point particularly noteworthy, given that we conducted Experiments 1a and 1b over the web). We thus allowed the noise variance Σ_{noise} to vary in fitting participants' responses. Following Feldman *et al.* (2009), we assumed that perceptual noise had identical effects on all formants in the phonetic space defined by the normalization account (see also Kronrod *et al.*, 2016). This reduces Σ_{noise} to a single DF, regardless of the normalization account (for details, see SI §3 A 3).

The magnitude of Σ_{noise} affects the slope of the categorization functions that predict listeners' responses from stimulus properties (here, F1 and F2): higher Σ_{noise} imply more shallow categorization slopes. To facilitate comparison of Σ_{noise} values across normalization accounts, we report results in terms of the best-fitting *noise ratios* (τ^{-1}), rather than Σ_{noise}s. Specifically, Σ_{noise} is best understood *relative* to the inherent variability of the vowel categories (Σ_c). This variability in turn depends on the phonetic space defined by the normalization account. We thus divide Σ_{noise} by the mean of the diagonals of all Σ_cs to obtain the *noise ratio* τ^{-1}. For example, noise ratio of 0 corresponds to the absence of any noise, and a noise ratio of 1 corresponds to noise variance of the same magnitude as the av-

erage category variance along F1 and F2 in the phonetic space defined by the normalization account.[10] Figure 8B illustrates the effects of this noise ratio for Nearey's uniform scaling account.

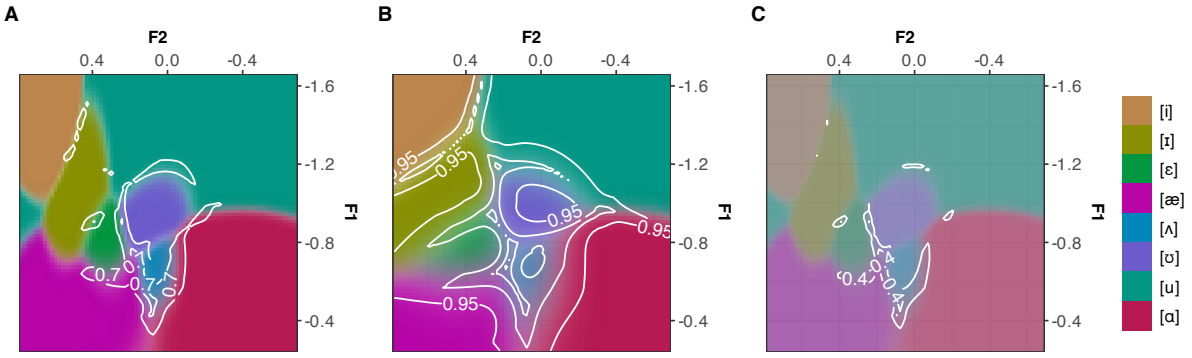


FIG. 8. Illustrating the consequences of perceptual and external noise (Σ_{noise}) and attentional lapse rates (λ) on the predicted posterior distribution of vowel categorizations. Shown are the average predicted posteriors across all five folds for Nearey's uniform scaling account. **Panel A**: Predicted posterior distribution for noise ratio $\tau^{-1} = \lambda = 0$. **Panel B**: Same for $\tau^{-1} = 1$ and $\lambda = 0$. **Panel C**: Same for $\tau^{-1} = 0$ and $\lambda = 0.5$. Transparency of a color is determined by that vowel's posterior probability. Contours indicate the highest posterior probability of any vowel (at .4, .5, .7, .95 probability level).

Second, participants can attentionally lapse or for other reasons reply without considering the speech input. We thus allowed lapse rates (λ) to vary while fitting human responses. This introduces a second DF, which we fit against listeners' responses. Together, the inclusion of freely varying lapse rates and a uniform response bias allows the ASP models to capture that some unknown proportion of listeners' responses might be more or less random, rather than reflecting properties of the vowel stimuli. This is illustrated in Figure 8C.

Finally, participants can have response biases that reflect their beliefs about the prior probability of each category. However, to reduce the DFs fit to participants' responses, we

did *not* fit this response bias against listeners' responses (thus avoiding $J - 1 = 7$ additional DFs). Instead, we assumed uniform response biases—i.e., that listeners believed all eight response options in the experiments to be equally likely ($\forall c\ \pi_c = .125$). This decision implies that our models would not be able to capture any potential non-uniformity in listeners' response biases—including potential effects of additional acoustic differences (the absence of [h] in *odd* or the coda [t], rather than [d] in *hut*) and orthographically particular response options in Experiment 1a ("who'd", "odd", and "hut"). We do, however, see no reasons to expect this decision to bias the comparison of normalization accounts.

5. Fitting normalization accounts to listeners' responses

For each of the 200 combinations of experiment, normalization account, training set, we used constrained quasi-Newton optimization (Byrd *et al.*, 1995, as implemented in `R`'s `optim()` function) to find the λ and τ^{-1} values that best described listener's responses. Specifically, we used the 100 ideal observers described in the previous sections, applied them to the normalized stimuli of the experiment, and determined which λ and τ^{-1} maximized the likelihood of listener's responses (for details, see SI §3 A 3). This procedure yielded five maximum likelihood estimates for both λ and τ^{-1} for each combination of experiment and normalization account—one for each training set. All result presented below were validated and confirmed by grid searches over the parameter spaces (SI, §3 F).

We compare normalization accounts in terms of the likelihood of listeners' responses under these maximum likelihood estimates of λ and τ^{-1}. Comparing accounts in terms of their data likelihood, rather than the accuracy of predicting intended productions (e.g.,

Johnson, 2020; Persson and Jaeger, 2023), or correlations with human response proportions (e.g., Hillenbrand and Nearey, 1999; Nearey and Assmann, 1986), follows more recent work (e.g., Barreda, 2021; McMurray and Jongman, 2011; Richter *et al.*, 2017; Xie *et al.*, 2023) and parallels standard approaches to model comparison in contemporary data analysis. We note that this approach puts normalization accounts to a stronger test. For example, a model can exhibit high correlations with listeners' responses even when its predictions are systematically 'off'. Similarly, a model can achieve high accuracy in predicting listeners' responses simply because it always predicts the most frequent response, and that response accounts for sufficiently much of the data. In contrast, the likelihood of listeners' responses under a model is a direct measure of how well the model captures the distribution of listeners' responses conditional on the stimulus properties. In particular, data likelihood will be maximized if, and only if, the model-predicted posterior probabilities of each vowel for each stimulus are identical to the proportion with which those vowels occur in listeners' responses.

B. Results

We begin by comparing the fit of different accounts against listeners' responses in Experiments 1a and 1b. Given the comparatively large number of accounts compared here, we provide initial conclusions based on the best-fitting accounts along with the description of the results (more in-depth discussion is provided in the general discussion). Following this comparison, we visualize how different normalization accounts predict the formant space to be divided into the eight vowel categories.

631 ***1. Comparing normalization accounts in terms of fit against human behavior***

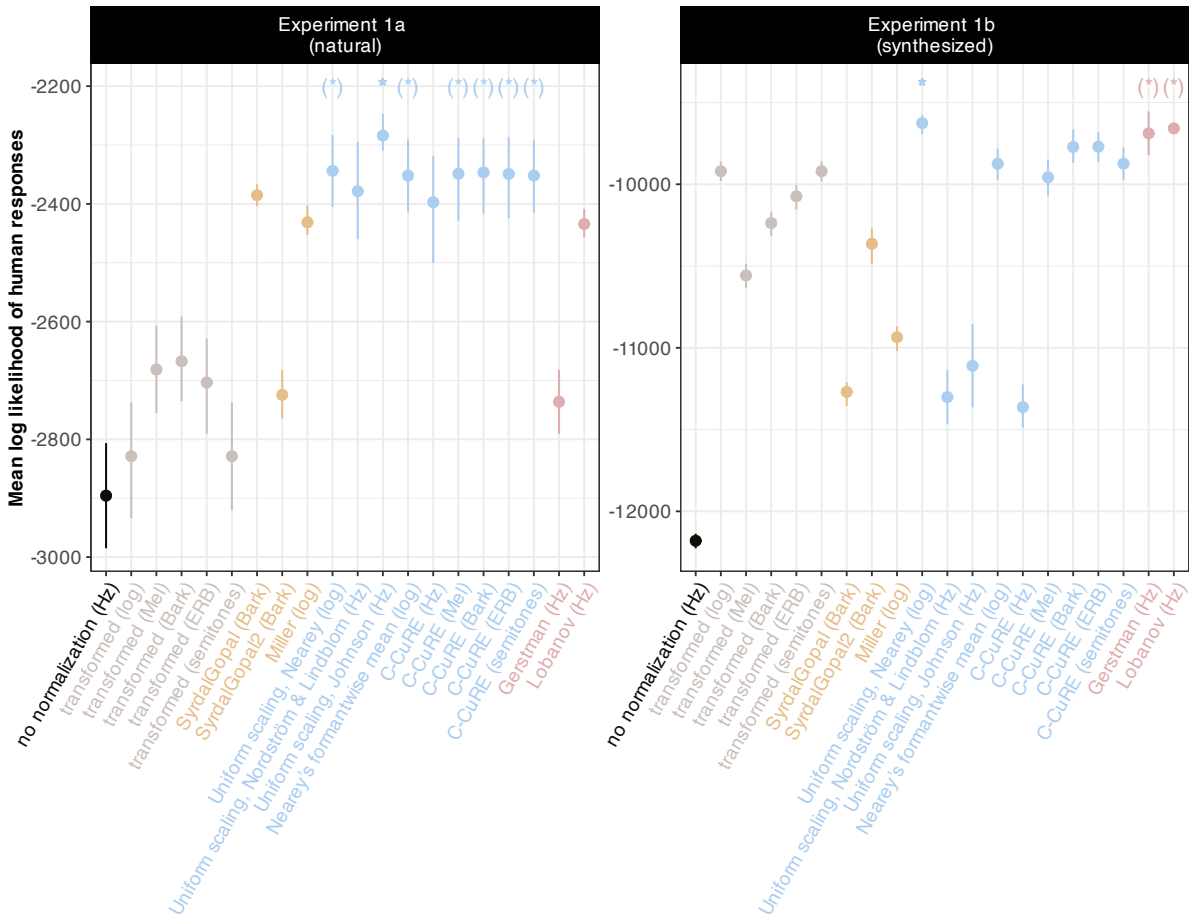


FIG. 9. Comparison of normalization accounts against listeners' responses. Pointranges indicate mean and 95% bootstrapped CIs of the log-likelihood summarized over the five training sets (higher is better). Accounts that fit listeners' responses to an extent that is statistically indistinguishable from the best-fitting account are marked by (*). Note that y-axis range differs across panels, and that it is *not* meaningful to compare the absolute log-likelihood values across the two experiments (just as it is not meaningful to compare the data likelihood of regressions that are fit on two different data sets).

632 Figure 9 compares how well the different normalization accounts fit listeners' responses

633 in Experiments 1a and 1b. All accounts performed well above chance guessing (chance log

likelihood in Experiment 1a: -5334; Experiment 1b: -13213) but also well below the highest possible performance (in Experiment 1a, log-likelihood = -1348, in Experiment 1b: -7225).

Normalization significantly improved the fit to listeners' responses relative to no normalization. This was confirmed by paired one-sided t-tests comparing the maximum likelihood values for each normalization account against those in the absence of normalization (all ps $< .05$; see SI §3 B 1). Not all normalization accounts achieved equally good fits, however: only some extrinsic accounts fit listeners' behavior well across both experiments. This supports two conclusions. First, it suggests that the normalization mechanisms operating during human speech perception involve computations that go beyond static transformations into psycho-acoustic spaces. Second, it suggests that the input to these computations is not limited to intrinsic information—i.e., that the computations draw on information beyond what is available in the acoustic signal *at that moment*. In particular, extrinsic normalization requires the estimation and memory maintenance of talker-specific properties from the speech signal.

While the accounts that achieved the best fit against listeners' responses differed between experiments, both were variants of uniform scaling. For Experiment 1a, Johnson normalization account provided the best fit (log likelihood = -2284, SD = 41 across the five crossvalidation folds), while Nearey's uniform scaling account provided the best fit to Experiment 1b (log likelihood = -9626, SD = 78). Both accounts essentially slide the representational 'template' of a dialect—here the eight bivariate Gaussian categories of an ideal observer—along a single line in the formant space. They differ only in *which* space this linear relation between formants is assumed. The same two accounts still fit listeners' responses

best when F3 was included in the analysis in addition to F1 and F2 (SI, §3 E).[11] This suggests that formant normalization might involve comparatively parsimonious maintenance of talker-specific properties: in its simplest form, uniform scaling employs a single formant statistic to normalize all formants. In contrast, computationally more complex accounts like Lobanov normalization might require the estimation and maintenance of two formant statistics (mean and standard deviation) for each formant that is normalized (e.g., a total of four formant statistics for F1 and F2, or six statistics for F1-F3).

For both experiments, there were several accounts that fit listeners' responses similarly well as the best-fitting accounts (ps $> .065$). All of these were extrinsic accounts, though the specific accounts differed between experiments. Notably, only Nearey's uniform scaling either provided the best fit (Experiment 1b) or achieved performance statistically indistinguishable from the best fit *for both experiments* (for Experiment 1a: $p > 0.08$, log likelihood = -2344, SD = 84). Beyond the performance of Nearey's uniform scaling, there was little evidence of a correlation in relative ordering of accounts between experiments (Spearman rank $r = 0.09$, $p = 0.72$). Some accounts fit listeners' responses well for Experiment 1a, but not for Experiment 1b, and vice versa. Of note is the particularly variable performance of the centering accounts operating in Hertz space, i.e., C-CuRE Hz, Nordström & Lindblom and Johnson normalization. Similar variability across the two experiments is also observed for the two standardizing accounts, both of which operate in Hz space.

That an account operating over log-transformed formants—Nearey's uniform scaling—fits human behavior better should not be surprising. While questions remain about the exact organization of auditory formant representations, it is uncontroversial that the perceptual

sensitivity to acoustic frequency information is better approximated by a logarithmic scale than by a linear scale (see Moore, 2012). As a result, a 30 Hz difference in an F1 of 300 Hz (a 10% change) is expected to be perceptually more salient than a 30 Hz change in an F2 of 2500 Hz (a 1.2% change). In line with this reasoning, additional tests not reported here found that Johnson normalization would provide the best fit to *both* experiments if it was applied to log-transformed formants (instead of Hertz). In summary, variability in how well different accounts predict human behavior across the two experiments highlights the importance of psycho-acoustic transformations for human speech perception. This also highlights the importance of comparing normalization accounts against multiple types of data.

2. Visualizing the consequences of different normalization mechanisms

Before we turn to the general discussion, we briefly visualize how different normalization mechanisms affect vowel categorization. This sheds light on *why* the accounts differ in how well they fit listeners' responses. Figure 10 visualizes the categorization functions predicted by four different normalization accounts, using the best-fitting λ and τ^{-1} values for each account (i.e., the values that lead to the fit shown in Figure 9). Figure 10 highlights three points. First, a comparison across rows of Figure 10 shows how much the choice of normalization can affect how the acoustic space gets carved up into vowel categories: a comparison of the first (no normalization), third (Johnson), and fourth row (Lobanov) shows that even normalization accounts operating over the same space can yield very different categorization behavior.

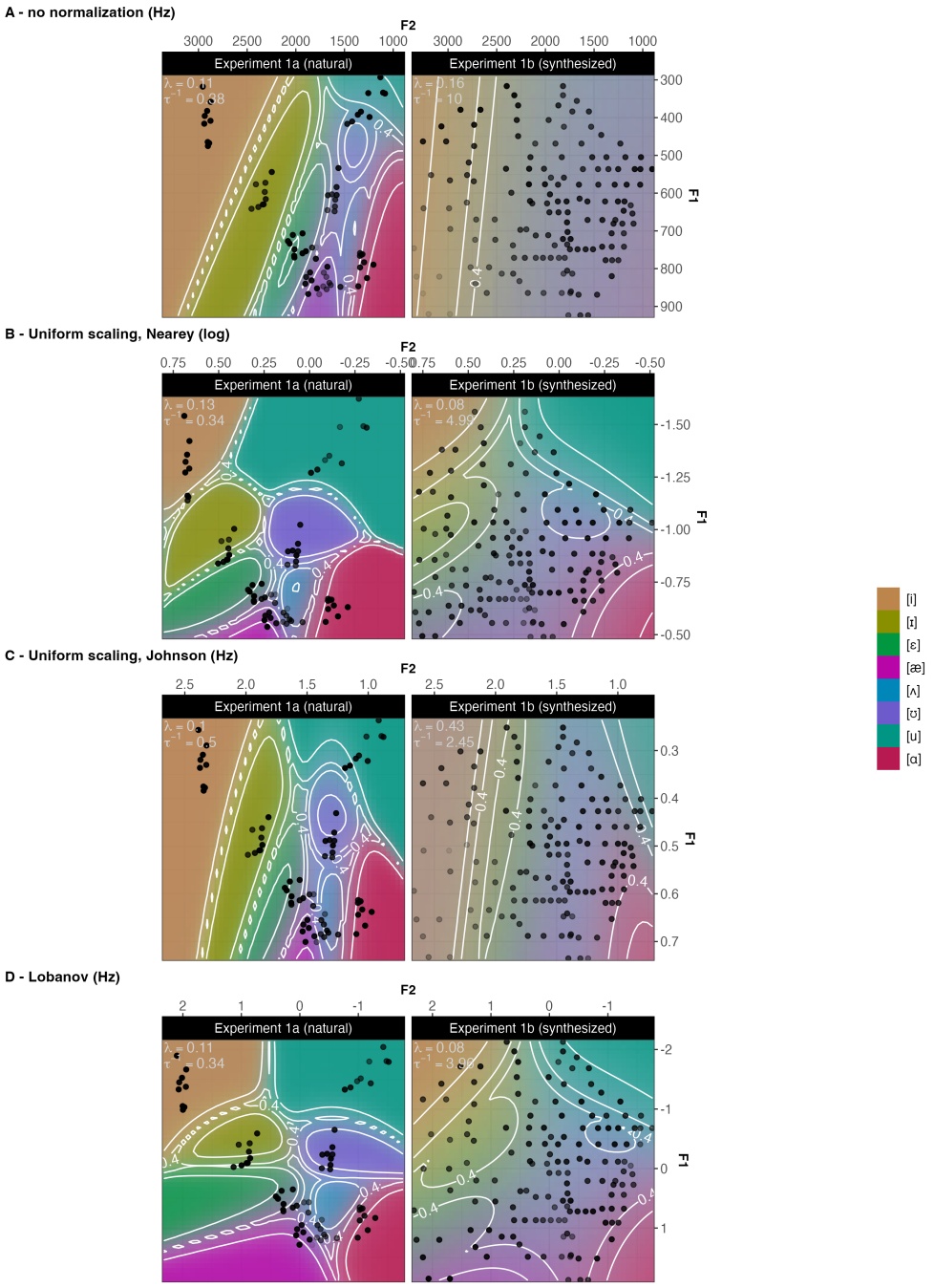


FIG. 10. Predicted categorization functions over the F1-F2 space under four different normalization accounts. For each account, we show the predicted posterior probabilities of all eight vowels obtained by averaging over the maximum likelihood parameterizations (of λ and τ^{-1}) for the five training sets (shown at top of each panel). **Top:** absence of normalization shown for reference. **2nd row:** the best-fitting account for Experiment 1a. **3rd row:** the best-fitting account for Experiment 1b (and second best for Experiment 1a). **Bottom:** the second best-fitting account in Experiment 1b. Contours indicate the highest posterior probability of any vowel. Points indicate location of test stimuli. The increasing opacity of points indicates a worse fit by the account (see text for detail).

Second, the best-fitting parameters (shown at the top of each panel) were relatively comparable across accounts but differed more substantially across experiments. Specifically, the best-fitting estimates of lapse rates λ were generally comparable across the two experiments (with the exception of Nordström & Lindblom and Johnson normalization, which exhibited substantially higher lapse rates in Experiment 1b; SI §3 B 2). This suggests that participants in both experiments were about equally likely to pay attention to the stimulus. The best-fitting noise ratios τ^{-1}, however, differed substantially across experiments, and were 9 times larger for Experiment 1b (mean $\tau^{-1} = 4.74$, SD $= 2.57$ across normalization accounts) than for Experiment 1a (mean $\tau^{-1} = 0.52$, SD $= 0.49$). This difference most likely reflects the fact that the synthesized stimuli in Experiment 1b left listeners with substantially more uncertainty about the intended category, as discussed during the description of the experiments.

Since noise is assumed to be independent of category variability (see also Feldman *et al.*, 2009; Kronrod *et al.*, 2016), differences in noise ratios can substantially change the categorization function. This is particularly evident for the accounts that had more variable performance across the two experiments. For example, both Johnson (third row) and Lobanov normalization (fourth row) resulted in very different best-fitting categorization functions for Experiments 1a and 1b.

Third and finally, Figure 10 also shows how well accounts fit listeners' responses for each test stimulus (opaqueness of the black points). This begins to explain *why* some accounts fit listeners' responses in Experiment 1b less well. For example, the Johnson normalization account (third row) predicts the responses to the test stimuli in Experiment 1a well, but

fails to predict the responses to the test stimuli in Experiment 1b. This drop in performance seems to be primarily driven by stimuli that are unlikely to be articulated by the same talker (lower left, cf. dashed line in Figure 4). This might suggest that this account was over-engineered to explain naturally occurring productions—the type of data, it was originally tested on (Johnson, 2020). A plausible account of normalization, however, should be able to explain human perception to any type of stimulus, including synthesized stimuli. The SI (§3 B 3) presents more detailed by-item comparisons of normalization accounts that might be of interest to some readers.

IV. GENERAL DISCUSSION

Research on vowel normalization has an influential history. Cognitive scientists have long aimed to understand the organization of frequency information in the human brain (Siegel, 1965; Stevens and Volkmann, 1940), and how it helps listeners overcome cross-talker variability in the formant-to-vowel mapping (e.g., Fant, 1975; Joos, 1948; Nordström and Lindblom, 1975). Auditory processes that normalize speech inputs for differences in vocal tract physiology are now recognized to be an integral part of speech perception (Johnson and Sjerps, 2021; McMurray and Jongman, 2011; Xie *et al.*, 2023). Here, we set out to investigate what types of computations are implicated in the normalization of the frequency information that plays a critical role in the recognition of vowels.

Our results support three theoretical insights. First, human speech perception draws on more than psycho-acoustic transformations or intrinsic information, in line with previous research on normalization (Adank *et al.*, 2004; Ladefoged and Broadbent, 1957; Nearey,

742 1989). Rather, formant normalization seems to involve the estimation and storing of talker-
743 specific formant properties. Second, computationally simple uniform scaling accounts pro-
744 vide the best fit to listeners' responses, suggesting comparatively parsimonious maintenance
745 of talker-specific properties. This replicates and extends previous findings that uniform scal-
746 ing or similarly simple corrections for vocal tract size provide a better explanation for human
747 perception than more complex extrinsic accounts (Barreda, 2021; Richter *et al.*, 2017). It is
748 impossible to rule out more complex approaches to perceptual normalization given the large
749 number of possible alternatives. However, given that uniform scaling provides a parsimo-
750 nious explanation for human formant normalization, and the current absence of empirical
751 evidence for more complex computations, we submit that researchers ought to adapt uni-
752 form scaling as our working hypothesis. Third, the psycho-acoustic representation assumed
753 by different normalization accounts matter, as indicated by the comparison of otherwise
754 computationally similar accounts (e.g. Nearey's vs. Johnson's uniform scaling).

755 These results contribute to a still comparatively small body of work that has evaluated
756 competing normalization accounts against listeners' perception, whereas most previous work
757 evaluates accounts against intended productions. Complementing previous work, we took
758 a broad-coverage approach: the present study compared 20 of the most influential nor-
759 malization accounts against listeners' perception of *hVd* words with all eight US English
760 monophthongs in both natural and synthesized speech. This contrasts with previous work,
761 which has typically focused on subsets of the vowel system, either using natural *or* synthe-
762 sized speech, and considering a much smaller subset of accounts (typically 2-3 at a time). By
763 considering a wider range of accounts, a wider range of formant values and vowel categories,

764 and multiple types of speech, we aimed to contribute to a more comprehensive evaluation
765 of competing accounts.

766 Next, we discuss the theoretical consequences of these findings for research beyond for-
767 mant normalization. Following that, we discuss limitations of the present work, and how
768 future research might overcome them.

769 A. Consequences for theories of speech perception and beyond

770 Understanding the perceptual space in which the human brain represents vowel categories—
771 i.e., the normalized formant space—has obvious consequences for research on speech percep-
772 tion. To illustrate how far reaching these consequences can be, we discuss a few examples.
773 For instance, research on *categorical perception* has found that vowels seem to be per-
774 ceived less categorically than some types of consonants. Recent work has offered an elegant
775 explanation for this finding: the perception of formants—relevant to the recognition of
776 vowels—might be more noisy than the perception of the acoustic cues that are critical to
777 the recognition of less categorically perceived consonants (Kronrod *et al.*, 2016). This is
778 a parsimonious explanation, potentially preempting the need for separate explanations for
779 the perception of different types of phonemic contrasts. Kronrod and colleagues based their
780 argument on estimates they obtained for the relative ratio of meaningful category variability
781 to perceptual noise (τ, the inverse of our noise ratios, $\tau^{-}1$). Critically, this ratio depends
782 both on (i) the perceptual space in which formants are assumed to be represented (Kronrod
783 at al used Mel-transformed formants), and on (ii) whether the meaningful category variabil-
784 ity is calculated prior to, or following, normalization (Kronrod et al assumed the former,

785 which increases estimates of category variability). Our point here is not to cast doubt on
786 the results of Kronrod *et al.* (2016) —the fact that the best-fitting noise ratios in our study
787 were relatively similar across accounts (while varying across experiments) suggests that the
788 result of Kronrod and colleagues are likely to hold even under different assumptions about
789 (i) and (ii)—but rather to highlight how research on the perception and recognition of
790 vowels depends on assumptions about formant normalization. For example, similar points
791 could be raised about experiments on statistical learning that manipulate formant or other
792 frequency statistics (e.g., Chládková *et al.*, 2017; Colby *et al.*, 2018; Wade *et al.*, 2007; Xie
793 *et al.*, 2021). Such experiments, too, need to make assumptions about the space in which
794 formants are represented. If these assumptions are incorrect, this can affect whether the
795 experimental manipulations have the intended effects, increasing the chance of null effects
796 or misinterpretation of observed effects.

797 Understanding the perceptual space in which the human brain represents vowel cate-
798 gories also has consequences for research beyond speech perception, perhaps more so than is
799 sometimes recognized. For instance, in sociolinguistics and related fields, Lobanov remains
800 the norm for representing vowels due to its efficiency in removing cross-talker variability
801 (for review, see Adank *et al.*, 2004; Barreda, 2021). However, as shown in the present study,
802 removing cross-talker variability is not the same as representing vowels in the perceptual
803 space that listeners actually employ. Here, we do *not* find Lobanov to describe human
804 perception particularly well. On the contrary, we find no support for the hypothesis that
805 human speech perception employs these more complex computations that have been found
806 to perform best at reducing category variability. This should worry sociolinguists. In order

807 to understand how listeners infer a talker's background or social identity, it is important
808 to understand the perceptual space in which inferences are actually rooted. Critically, the
809 representations resulting from formant normalization presumably form an important part of
810 the information that listeners use to draw social and linguistic inferences. It should thus be
811 obvious that the use of normalization accounts that do not actually correspond to human
812 perception can both mask real markers of social identity, and hallucinate markers that are
813 not actually present. For example, in order to determine how a talker's social identity influ-
814 ences their vowel realizations, it is important to discount *all and only* effects that listeners'
815 will attribute to physiology, rather than social identity (Disner, 1980; Hindle, 1978).

816 Similar concerns apply to dialectology, research on language change, second language
817 acquisition research, etc. For example, the perceptual space in which vowels are represented
818 is critical to well-formed tests of hypotheses about the factors shaping the organization of
819 vowel inventories across languages of the world (Lindblom, 1986; Stevens, 1972, 1989). It is
820 essential in testing hypotheses about the extent to which the cross-linguistic realization of
821 those systems is affected by perceptual processes (Flemming, 2010; Steriade, 2001), or by
822 preferences for communicatively efficient linguistic systems (e.g., Hall *et al.*, 2018; Lindblom,
823 1990; Moulin-Frier *et al.*, 2015). Similarly, tests of the hypothesis that vowel *articulation*
824 during natural interactions is shaped by communicative efficiency do in obvious ways depend
825 on assumptions about the perceptual space in which talkers—by hypothesis—aim to reduce
826 perceptual confusion (cf. Buz and Jaeger, 2016; Gahl *et al.*, 2012; Scarborough, 2010; Wedel
827 *et al.*, 2018). The same applies to any other line of research that aims to understand the
828 perceptual consequences of formant variation across talkers, including research on infant- or

child-directed speech (Eaves Jr *et al.*, 2016; Kuhl *et al.*, 1997), and research on whether non-native talkers are inherently more variable than native talkers (Smith *et al.*, 2019; Vaughn *et al.*, 2019; Xie and Jaeger, 2020). In short, the perceptual space in which vowels are represented is a critical component of understanding the structure of vowel systems, the factors that shape them, and the ways in which they are used in natural language.

B. Limitations and future directions

The present work shares a few limitations with previous work. Here we focus on limitations that follow from the assumptions we made in our computational framework. While theories and hypotheses often contain substantial vagueness, *quantitative tests* of those theories—as we have done here—require assumptions about *every* aspect of the model. Here, this included all the steps necessary to link properties of the stimuli to listeners' responses. For this purpose, we adopted the ASP framework (Xie *et al.*, 2023), and visualized the graphical model that links stimuli (x) to responses (r) in Figure 6.

Many of the assumptions we made should be quite uncontroversial—e.g., the decision to include both external (environmental) and internal (perceptual) noise in our model. While these noise sources are often ignored in modeling human behavior, it is uncontroversial that they exist. Other assumptions we made were introduced as simplifying assumptions for the sake of feasibility—e.g., we expressed the effect of both types of noise through a single parameter that related the average within-category variability of formants to noise variability in the transformed and normalized formant space. In reality, however, environment noise can have effects that are independent of internal noise, and internal noise likely affects

information processing at multiple (or all) of the steps shown in Figure 6. Such simplifying assumptions are both inevitable, and not necessarily problematic: as long as they do not introduce systematic bias to the evaluation of normalization accounts, they should not limit the generalizability of our results.

Some of our assumptions, however, might be more controversial. For example, we assumed that category representations can be expressed as multivariate Gaussian distributions in the formant space. This assumption, too, is a simplifying assumption—it simplified the computation of likelihoods—rather than a critical feature of the ASP framework we employed. While human category representations are unlikely to be Gaussians, the alternative, e.g., exemplar representations, would come with its own downsides, such as increased sensitivity to the limited size of phonetic databases and substantial increases in computation time (exemplar representations afford researchers with much larger degrees of freedom). For researchers curious how this and other assumptions we made affect our results, our data and code are shared on OSF. This includes the R markdown document that generates this PDF, making it comparatively easy to revisit any of our assumptions to then regenerate the entire study with a click of a button in RStudio.

Like previous work, we further assumed that all listeners in our experiments use the same underlying vowel representations—the same dialect template(s). However, as already discussed, it is rather likely that not all of our listeners employed the same dialect template(s). An additional analysis reported in the SI (§3 D) thus compared normalization accounts against only the subset of listeners who employed the dialect template used by the majority of participants (see lower-left of Figure 5B). This left only 11 participants for

Experiment 1a (61.1%) and 14 for Experiment 1b (77.8%), substantially reducing statistical power. Replicating the main analysis, uniform scaling accounts again fit listeners' behavior well across both experiments. The best-performing accounts did, however, differ from the ones obtained for the superset of data (see SI, §3 D).

A related assumption was introduced by the use of a phonetic database to approximate listeners' vowel representations. This deviates from most previous evaluations of normalization accounts (McMurray and Jongman, 2011; Barreda, 2021; but see Richter *et al.*, 2017), and reflects our commitment to a strong assumption made by most theories of speech perception: that listeners' representations reflect the formant statistics previously experienced speech input. By using a phonetic database to estimate listeners' representations, we *substantially* reduced the degrees of freedom in the evaluation of normalization accounts, reducing the chance of over-fitting to the data from our experiments. Our approach does, however, also introduce two new assumptions.

First, our approach assumes that the mixture of dialect template(s) used by talkers in the database sufficiently closely approximates those of the listeners in our experiments. Some validation for this assumption comes from the additional analysis reported in the preceding paragraph: when we subset listeners to only those who used the majority dialect template, this improved the fit of all normalization accounts—as expected, if the category representations we trained on the phonetic database primarily reflect those listeners' representations (see SI, §3 D). Future work could further address this assumption in a number of ways. One the one hand, dialect analyses like the ones we presented for our listeners (in Figure 5B) could compare listeners' templates against the templates used by talkers in the database.

Alternatively or additionally, researchers could see whether our results replicate if ideal observers are instead trained on other databases that have been hypothesized to reflect a 'typical' L1 listeners' experience with US English.

Second, we made the simplifying assumption that listeners' category representation—or at least the representations listeners' drew on during the experiment—are talker-*independent* (we trained a single set of multivariate Gaussian categories, rather than, e.g., hierarchically organized set of multiple dialect templates). While this assumption is routinely made in research on normalization and beyond, it might well be wrong (see e.g., Xie *et al.*, 2021).

Finally, the evaluation of normalization accounts in the present work shares with all previous work (e.g., Apfelbaum and McMurray, 2015; Barreda, 2021; Cole *et al.*, 2010; McMurray and Jongman, 2011; Nearey, 1989; Richter *et al.*, 2017) another simplifying assumption that is clearly wrong: the assumption that listeners *know* the talker-specific formant properties required for normalization. Specifically, we normalized the input for each ideal observer using the maximum likelihood estimates of the normalization parameters for the respective experiment. For example, for the evaluation of the ideal observer trained on Lobanov normalized formants against listeners' responses in Experiment 1a, we used the formant means and standard deviations of the stimuli used in Experiment 1a to normalize F1 and F2. While this follows previous work, it constitutes a problematic assumption for the evaluation of extrinsic normalization accounts. For these accounts, the approach adopted essentially assumes the ability to predict the future: even on the first trial of the experiment, the input to the ideal observers were formants that were normalized based on the maximum likelihood estimate of the normalization parameters given the acoustic properties of *all* stimuli. Lis-

teners instead need to *incrementally infer* talker-specific properties from the speech input (Nearey and Assmann, 2007; Xie *et al.*, 2023). The development and testing of incremental variants of formant normalization strikes us an important avenue for future research.

C. Concluding remarks

We set out to compare how well competing accounts of formant normalization explain listeners' perception of vowels. We developed a computational framework that makes it possible to compare a large number of different accounts against multiple data sets. The code we share on OSF makes it possible to 'plug in' different accounts of vowel normalization, different phonetic databases, and different perception experiments. This, we hope, will substantially reduce the effort necessary to conduct similar evaluations on other datasets, dialects, and languages.

Comparing 20 of the most influential normalization accounts against L1 listeners' perception of US English monophthongs, we found that the normalization accounts that best describe listeners' perception share that they (1) learn and store talker-specific properties and (2) that they seem to be computationally very simple—taking advantage of the physics of sound generation to use as few as a single parameter to normalize inter-talker variability in vocal tract size. While the number of studies that have compared normalization accounts against *listeners'* behavior remains surprisingly small, these two results confirm the findings from more targeted comparisons that were focused on 2-3 accounts at a time (Barreda, 2021; Nearey, 1989; Richter *et al.*, 2017). Overall then, we submit that it is time for research in

speech perception and beyond to consider simple uniform scaling the most-likely candidate for human formant normalization.

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AUTHOR CONTRIBUTIONS

AP designed the experiments and collected the data, with input from TFJ. TFJ programmed the experiments with input from AP. AP analyzed the experiments, with input from TFJ. AP and TFJ wrote the code to implement and fit the normalization models, with input from SB. AP developed the visualizations within input from SB and TFJ. AP wrote the first draft of the manuscript with edits by SB and TFJ.

SUPPLEMENTARY INFORMATION FOR *PERSSON, BARREDA & JAEGER (2024). COMPARING ACCOUNTS OF FORMANT NORMALIZATION AGAINST US ENGLISH LISTENERS' VOWEL PERCEPTION*

§1. REQUIRED SOFTWARE

Both the main text and these supplementary information (SI) are derived from the same R markdown document available via https://osf.io/zemwn/. It is best viewed using Acrobat Reader. The document was compiled using `knitr` in RStudio with R:

```
##                _
## platform       aarch64-apple-darwin20
## arch           aarch64
## os             darwin20
## system         aarch64, darwin20
## status
## major          4
## minor          3.2
## year           2023
## month          10
## day            31
## svn rev        85441
## language       R
## version.string R version 4.3.2 (2023-10-31)
## nickname       Eye Holes
```

Readers interested in working through the R markdown, and knitting it into a PDF will also need to download the IPA font SIL Doulos and a Latex environment like (e.g., MacTex or the R library `tinytex`).

We used the following R packages to create this document: R (Version 4.3.2; R Core Team, 2023) and the R-packages *assertthat* (Version 0.2.1; Wickham, 2019), *brms* (Version 2.21.0; Bürkner, 2017, 2018, 2021), *Cairo* (Version 1.6.2; Urbanek and Horner, 2023), *cmdstanr* (Version 0.5.3; Gabry and Češnovar, 2022), *dplyr* (Version 1.1.4; Wickham *et al.*,

2023), *ellipse* (Version 0.5.0; Murdoch and Chow, 2023), *forcats* (Version 1.0.0; Wickham, 2023a), *furrr* (Version 0.3.1; Vaughan and Dancho, 2022), *fuzzyjoin* (Version 0.1.6; Robinson, 2020), *ggforce* (Version 0.4.2; Pedersen, 2024a), *ggh4x* (Version 0.2.8; van den Brand, 2024), *ggnewscale* (Version 0.4.10; Campitelli, 2024), *ggplot2* (Version 3.5.1; Wickham, 2016), *ggtext* (Version 0.1.2; Wilke and Wiernik, 2022), *knitr* (Version 1.47; Xie, 2024), *linguisticsdown* (Version 1.2.0; Liao, 2019), *lubridate* (Version 1.9.3; Grolemund and Wickham, 2011), *magrittr* (Version 2.0.3; Bache and Wickham, 2022), *mgcv* (Version 1.9.1; Wood *et al.*, 2016; Wood, 2003, 2004, 2011), *modelr* (Version 0.1.11; Wickham, 2023b), *MVBeliefUpdatr* (Version 0.0.1.10; Jaeger, 2024), *nlme* (Version 3.1.164; Pinheiro *et al.*, 2023), *patchwork* (Version 1.2.0; Pedersen, 2024b), *phonR* (Version 1.0.7; McCloy, 2016), *phonTools* (Version 0.2.2.2; Barreda, 2023), *plotly* (Version 4.10.4; Sievert, 2020), *purrr* (Version 1.0.2; Wickham and Henry, 2023), *Rcpp* (Version 1.0.12; Eddelbuettel *et al.*, 2024), *readr* (Version 2.1.5; Wickham *et al.*, 2024a), *remotes* (Version 2.5.0; Csárdi *et al.*, 2024), *RJ-2021-048* (Bengtsson, 2021), *rlang* (Version 1.1.4; Henry and Wickham, 2024), *stringr* (Version 1.5.1; Wickham, 2023c), *tibble* (Version 3.2.1; Müller and Wickham, 2023), *tidybayes* (Version 3.0.6; Kay, 2023), *tidyr* (Version 1.3.1; Wickham *et al.*, 2024b), and *tidyverse* (Version 2.0.0; Wickham *et al.*, 2019).

If opened in RStudio, the top of the R markdown document should alert you to any libraries you will need to download, if you have not already installed them. The full session information is provided at the end of this document.

A. Interested in using R markdown do create APA formatted documents that integrate your code with your writing?

A project template, including R markdown files that result in APA-formatted PDFs, is available at `https://github.com/hlplab/template-R-project`. Feedback welcome. We aim to help others avoid the mistakes and detours we made when first deciding to embrace literal coding to increase transparency in our projects.

§2. ADDITIONAL INFORMATION IN EXPERIMENTS 1A AND 1B

A. Participant exclusion

We adopted the following exclusion criteria: participants would get excluded if the failed to pay attention to the instruction to wear over-the-ear-headphones, if they had unusually slow or fast RT-means compared to other participants, or if they clearly did not do the task (e.g., randomly clicking on different response options).

N=1 participant in Experiment 1a was excluded based on the first criteria, as s/he used external speakers instead of head set (based on response in post-experiment questionnaire). This participant was also more than 3 standard deviations faster in her/his mean log-RTs than other participants (second criteria), as were another participant in Experiment 1a. We decided to exclude participants who were more than 3 standard deviations faster or slower in their mean (log-transformed) RTs compared to other participants. We further excluded *all trials* with RTs more than 3 standard deviations faster or slower than expected. This was determined by first z-scoring the log-transformed RTs *within each participant* (by subtracting the participants' mean from each observation and dividing through the participants standard deviation) and then z-scoring these z-scores *within each trial* across participants. This double-scaling approach was necessary as participants' RTs decreased substantially over the first few trials and then continued to decrease less rapidly until converging against a participant-specific minimum. This criterion did not remove just the first few trials but rather removed RTs that were unusually fast or slow *for that participant at that trial*. And, unlike more complicated methods (like developing a model of cross-trial decreases in RTs), the approach employed here does not make any assumptions about the shape of the speed up in RTs across trials. In total, N=117 trials were excluded, however, no participant was excluded based on too high proportion of missing trials. Figure S1 summarizes participant exclusions due to reaction times and not wearing headphones.

The experiments did not contain independent catch trials. We therefore looked into participants' individual responses in order to identify participants that seem to have randomly answered, independent of stimulus. Figure S2 suggests that participants 13, 15, 21, 22, and 24, have not performed the task. Their responses are indicating that they have not payed attention to the stimuli but rather randomly selected responses irrespective of the vowel they heard, hence, e.g., *had* responses for all different locations in the vowel space (e.g., partici-

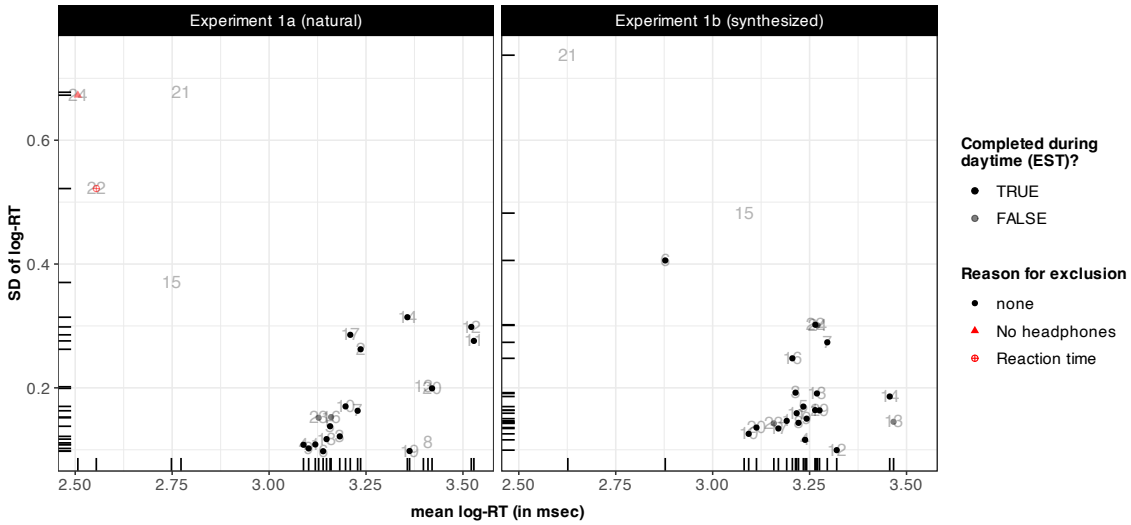

FIG. S1. Participant exclusions in Experiment 1a and Experiment 1b. Two participants in Experiment 1a (participants 22 and 24) were excluded based on their log-transformed RTs and/or for not wearing headphones.

pants 15, 21, 22, 24), or *heed* responses for the low back vowels (e.g., 24), or *odd* responses for high front vowels (e.g., participants 13, 21). Participant 8 seem to have performed the task but clearly used the phonetic space in ways different from everyone else, as s/he seems to have inverted the *who'd-hood* categories. This behaviour more likely indicates different dialect patterns, we still however, decided to exclude participant 8 as well. Participants 22 and 24 had already been excluded based on unusual RT-patterns (22) and not wearing over-the-ear headphones (24).

Excluding participants because of unusual vowel responses is more complicated for Experiment 1b as the stimuli is synthesized and more difficult to categorize in general. Figure S3 nevertheless indicates that most participants made use of the phonetic space in similar ways (and in line with where natural categories fall), besides participant 15 and 21. Participant 15 often responds *who'd* for tokens in the high front part of the space, and *heed* for tokens in the high center and back parts, while participant 21 is overall more random in responses, but often selects *heed* for high back tokens and several times selects *hod* for front tokens.

Unusual vowel responses is not an objective criterion. There are many participants that gave unexpected responses on some occasions, e.g., participants 10 and 12 in Experiment 1a, or participants 5 and 24 in Experiment 1b, however, we decided not to exclude them

as they were not systematic in their response patterns, i.e., there were no indications of a definite dialect shift (as with participant 8 in Experiment 1a), or systematic randomness in selecting any kind of vowel for any kind of stimuli (as with participants 13, 15, 21, 22, 24, in Experiment 1a, and to some extent, participants 15 and 22 in Experiment 1b).

B. Distribution of stimuli F1-F3 in Experiments 1a and 1b

Figure S4 visualizes the stimuli in Experiments 1a and 1b in F1-F3 space.

C. Auxiliary analysis of participant responses in Experiments 1a and 1b

Participants in Experiment 1b showed overall less agreement in their responses to the stimuli than participants in Experiment 1a, as indicated by the higher response entropy in Experiment 1b. In order to assess the extent to which this was a result of the placement of the tokens in the F1-F3 space, we compared linear regression models that predicted response entropy from experiment, to models that employed residuals from a general additive model including response entropy and the tokens placement in the phonetic space (F1, F2) as response variable, and experiment as predictor. We furthermore compared against models based on the full data set to models that excluded all *hut* and *odd* responses from Experiment 1a in order to assess effects of lexical context.

When adding effects of lexical context to the model, the difference between experiments is reduced by 23.9%. Adding a nonlinear model with F1-F2 values of the tokens, the difference is reduced by an additional 35.6%, while adding F3-values reduces the difference by 50.9%. In sum, the result suggest that approximately two-thirds of the difference in response entropy between experiments can be attributed to the placement of stimuli in the formant space, while the remaining one-third is influenced by other factors, most likely the synthesized stimuli sounding highly unnatural.

As stated in the main paper, response entropies differed even for tokens that overlap in Hertz space. Figure S5 visualizes differences in categorization behaviour for these tokens. For many of these tokens, the most frequent response is the same category across experiments, however, with substantially higher disagreement for tokens in Experiment 1b. In the bottom part of the acoustic space, participants in Experiment 1b seem to respond *had* disproportionally often.

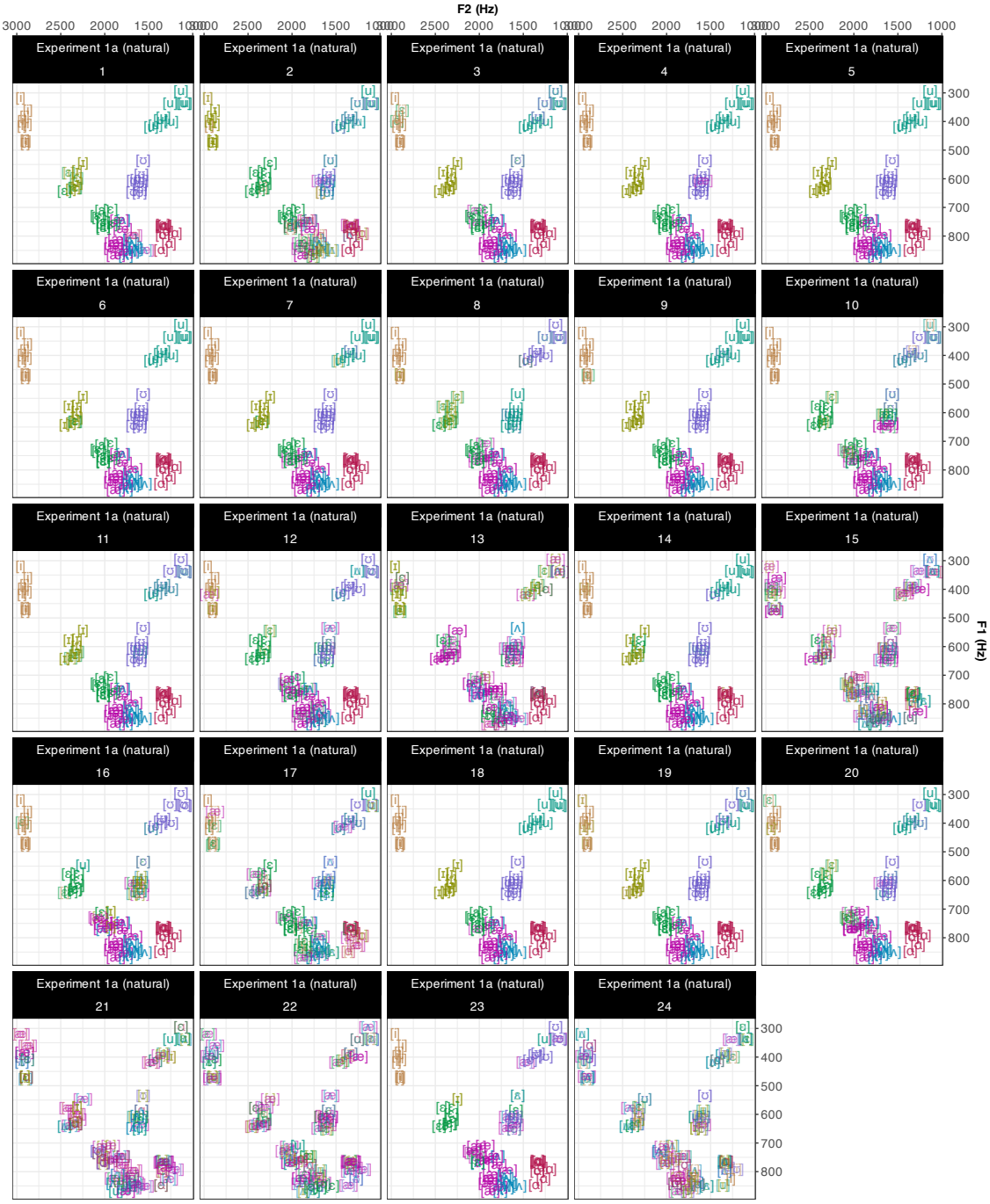

FIG. S2. Participants' categorization responses in Experiment 1a, shown in F1-F2 space. Color and vowel label indicate response provided by participants on each test location. Each vowel was repeated twice.

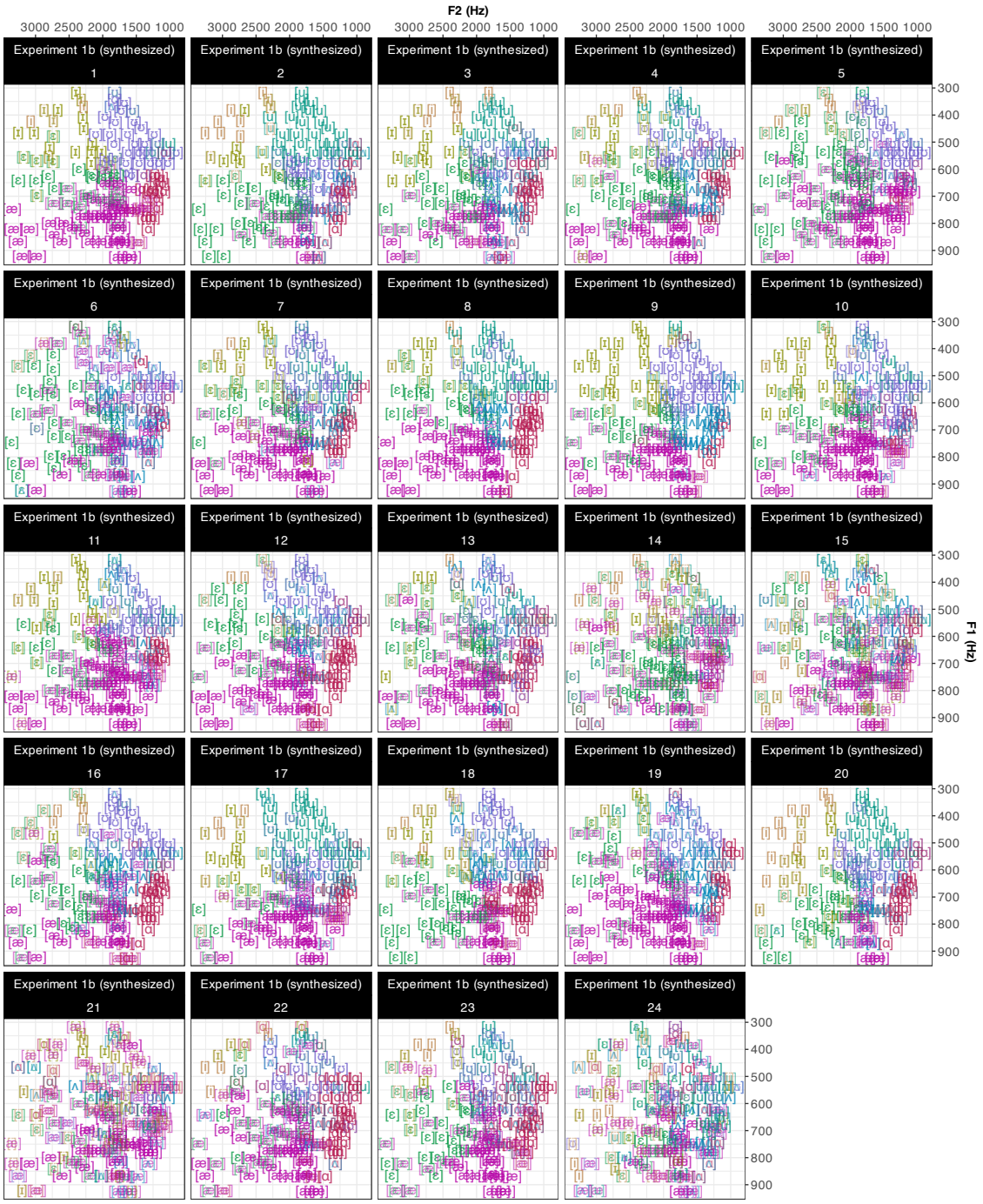

FIG. S3. Participants' categorization responses in Experiment 1b, shown in F1-F2 space. Color and vowel label indicate response provided by participants on each test location. Each vowel was repeated twice.

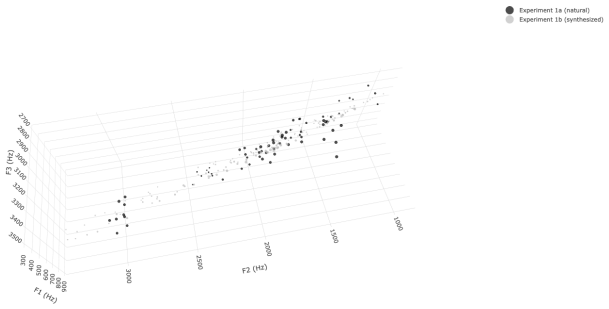

FIG. S4. Stimuli of Experiments 1a and 1b in F1-F3 space. Point size indicates response entropy: larger points represent higher listener agreement, and vice versa.

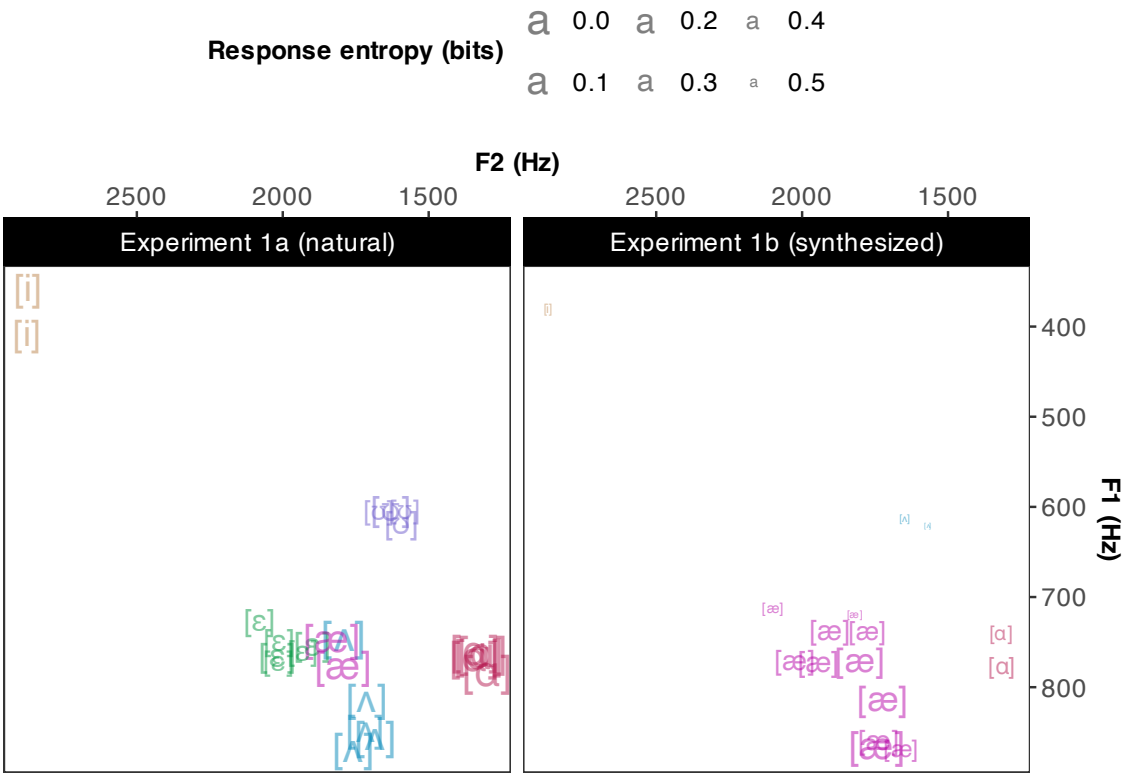

FIG. S5. Listeners' categorization responses in Experiments 1a and 1b, for comparable tokens in Hertz space. The vowel label indicates the most frequent response provided by participants on each test location. Size indicates how consistent responses were across participants, which larger symbols indicating more consistent responses (lower entropy).

§3. ADDITIONAL INFORMATION ON THE COMPUTATIONAL COMPARISON OF NORMALIZATION ACCOUNTS

A. Methods

1. Vowel data used to train ideal observers (Xie and Jaeger, 2020)

The Xie and Jaeger database consists of N=1168 *hVd* word recordings from 17 (5 female) L1 talkers of a Northeastern dialect of US English (ages 18 to 35 years old). The talkers were recorded reading a list of 180 English monosyllabic words, a list of short sentences, and a list of ten *hVd* words—the eight US English monophthongs as well as *aid* and *owed* (for further information, see Xie and Jaeger, 2020). For each talker, the database contains 9-10 recordings of each *hVd* word. An automatic aligner [Penn Phonetics Lab Forced Aligner; Yuan and Liberman (2008)] was used to obtain estimates for word and segment boundaries.[12]

The first author manually corrected the automatic alignments for all vowel segmentations. We then used the Burg algorithm in Praat (Boersma and Weenink, 2022) to extract estimates of the first three formants (F1-F3) at three points of the vowel (35, 50, and 65 percent into the vowel). The following parameterization of the Burg algorithm was used:

- Time step (s): 0.01
- Max. number of formants: 5
- Formant ceiling (Hz): 5500 (5000 for the male talkers)
- Window length (s): 0.025
- Pre-emphasis from (Hz): 50

In addition to F1-F3, we automatically extracted vowel duration and the fundamental frequency (F0) across the entire vowel. These are the data that we used in the cross-validation procedure to train ideal observers, as described in the main text. Figure S6 visualizes the vowel data from the Xie and Jaeger (2020) for all pairwise combinations of F0, F1, F2, F3 and vowel duration, in raw Hertz. Figure S7 shows the distribution of F1 and F2 in the different normalization spaces used in the main study.

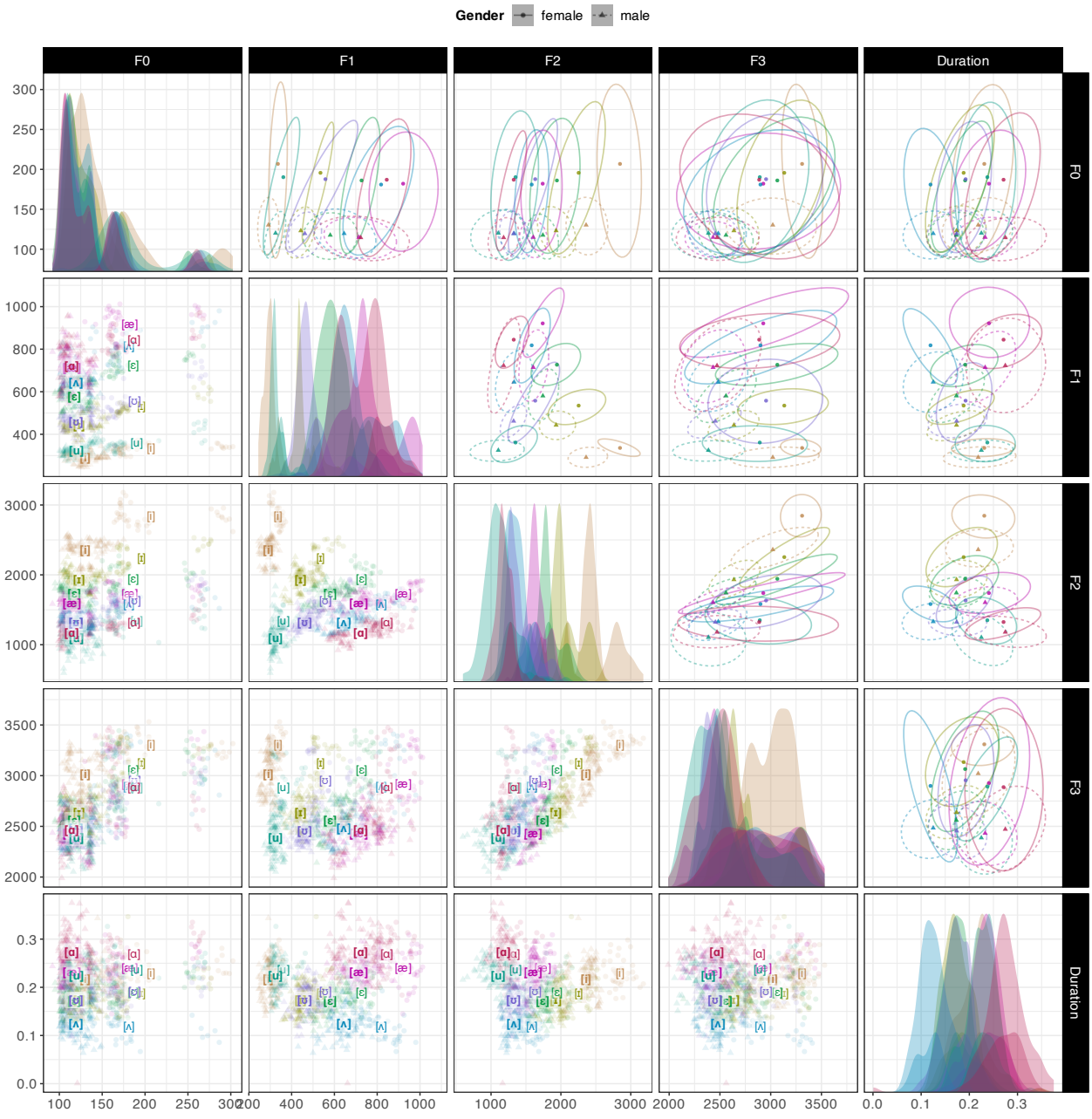

FIG. S6. The pairwise distributions of F0, F1, F2, F3, and duration for all 1168 recordings of the eight monophthong *hVd* words in Xie and Jaeger (2020). Note that axis directions are not reversed. **Panels on diagonal:** marginal cue densities of all five cues. **Lower off-diagonal panels:** each point corresponds to a recording, averaged across the three measurement points within each vowel segment. Vowel labels indicate category means across talkers. Male talkers' vowels are boldfaced. **Upper off-diagonal panels:** Same data as in the lower off-diagonal panels but showing bivariate Gaussian 95% probability mass ellipses around category means.

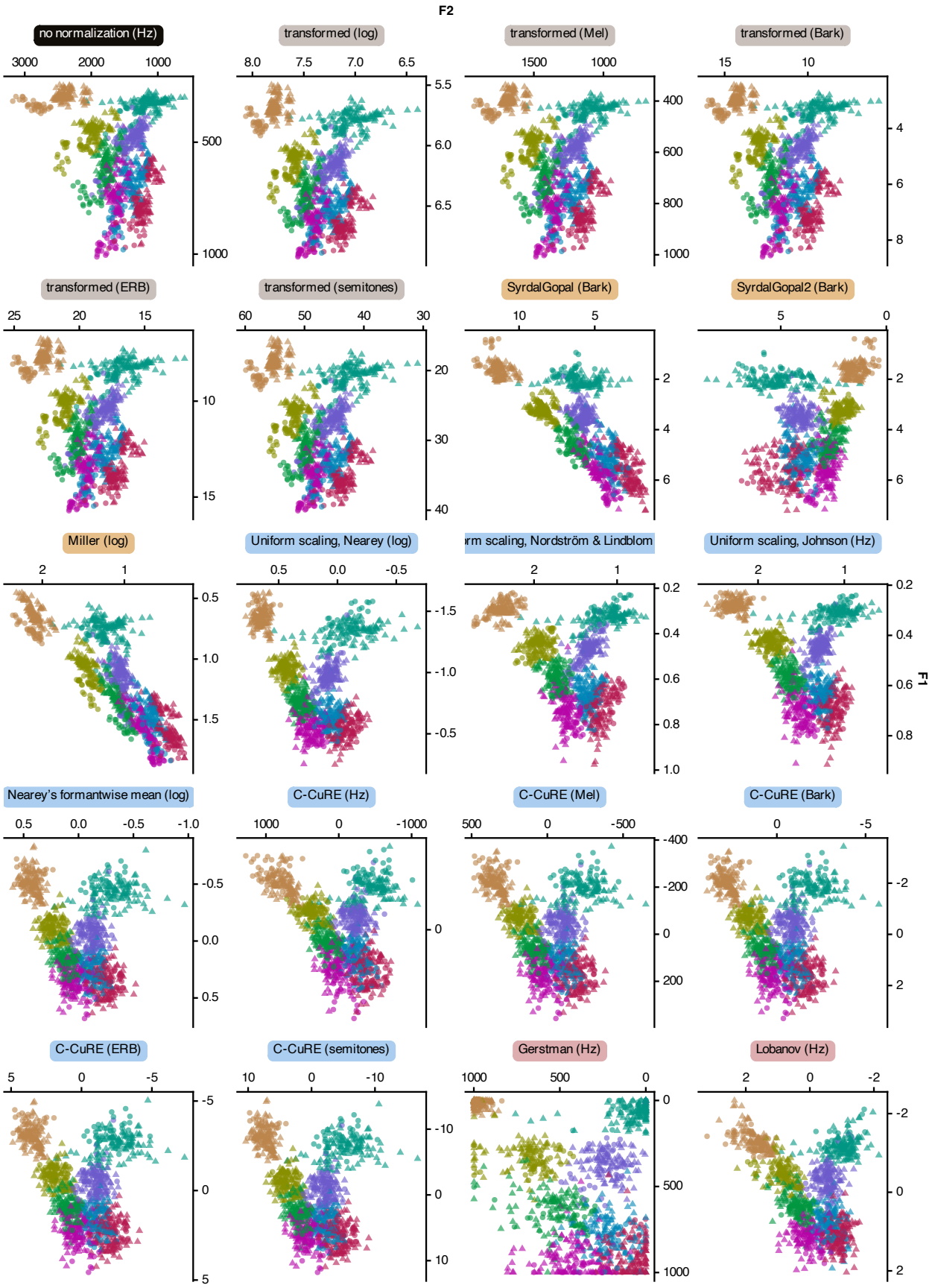

FIG. S7. The 8 monophthong vowels of US English from the Xie and Jaeger (2020) database when F1 and F2 are transformed into a perceptual scale (**grey**), intrinsically normalized (**yellow**), or extrinsically normalized through centering (**blue**) or standardizing (**purple**). Each point corre-

2. *Normalization parameters θ*

Figure S8 relates the normalization parameters θ obtained for each experiment to those found for the five training sets of the Xie and Jaeger (2020) database. This servers two purposes. First, by comparing the θ of Experiment 1a, which was based on natural productions, to the θ obtained from Xie and Jaeger (2020), we can assess the extent to which the talker used for Experiment 1a is 'typical' relative to the other talkers of that database. Second, by comparing the range and variability of the θ across normalization accounts and experiments, we can assess the volatility of different types of parameters, and assess the difference between the beliefs the ideal observers have about the parameters and the parameters in the experiment. How reliably the statistics of the input is established for the same amount of data seems to depend on the space. For instance, parameters in Hertz space display more variability. Within a given scale, we also note that some parameters are more difficult to estimate than others, for instance, mean estimates display less variability than SD, and range values (min and max).

3. *Optimization process to fit models to human responses*

We used constrained quasi-Newton optimization (Byrd *et al.*, 1995) to determine the best-fitting values for the two degrees of freedom—lapse rate (λ) and noise ratio (τ^{-1}). Optimization was performed separately for each of the 200 combinations of normalization account, experiment, and cross-validation fold. Specifically, we maximized the *likelihood* of the human categorization responses in each experiment under the categorization model conditional on the model's lapse rate and perceptual noise, $\Sigma_i^N \log p(response_i|F1_{i,\theta}, F2_{i,\theta}, M_{\theta,\lambda,\Sigma_{noise}})$, where $response_i$ is the ith categorization response, $F1_{i,\theta}, F2_{i,\theta}$ are the F1 and F2 values for the ith observation after normalization (with parameters θ being estimated based on the distribution of phonetic cues across the stimuli in the experiment). $M_{\theta,\lambda,\Sigma_{noise}}$ is the categorization model in Figure 6, with normalization parameters θ fixed based from the prior cue distribution in the phonetic database (Xie and Jaeger, 2020), and λ and Σ_{noise} as the only free parameters to maximize the likelihood. The best-fitting parameterizations were determined by means of the `optim()` function in `R`'s `stats` package (R Core Team, 2023). The starting value of lapse rates and perceptual noise were set to 0.1 and 0.15, respectively. We set the lower and upper bounds to $10^{-10} \geq$ lapse rate ≥ 1, and $10^{-10} \geq$ perceptual noise

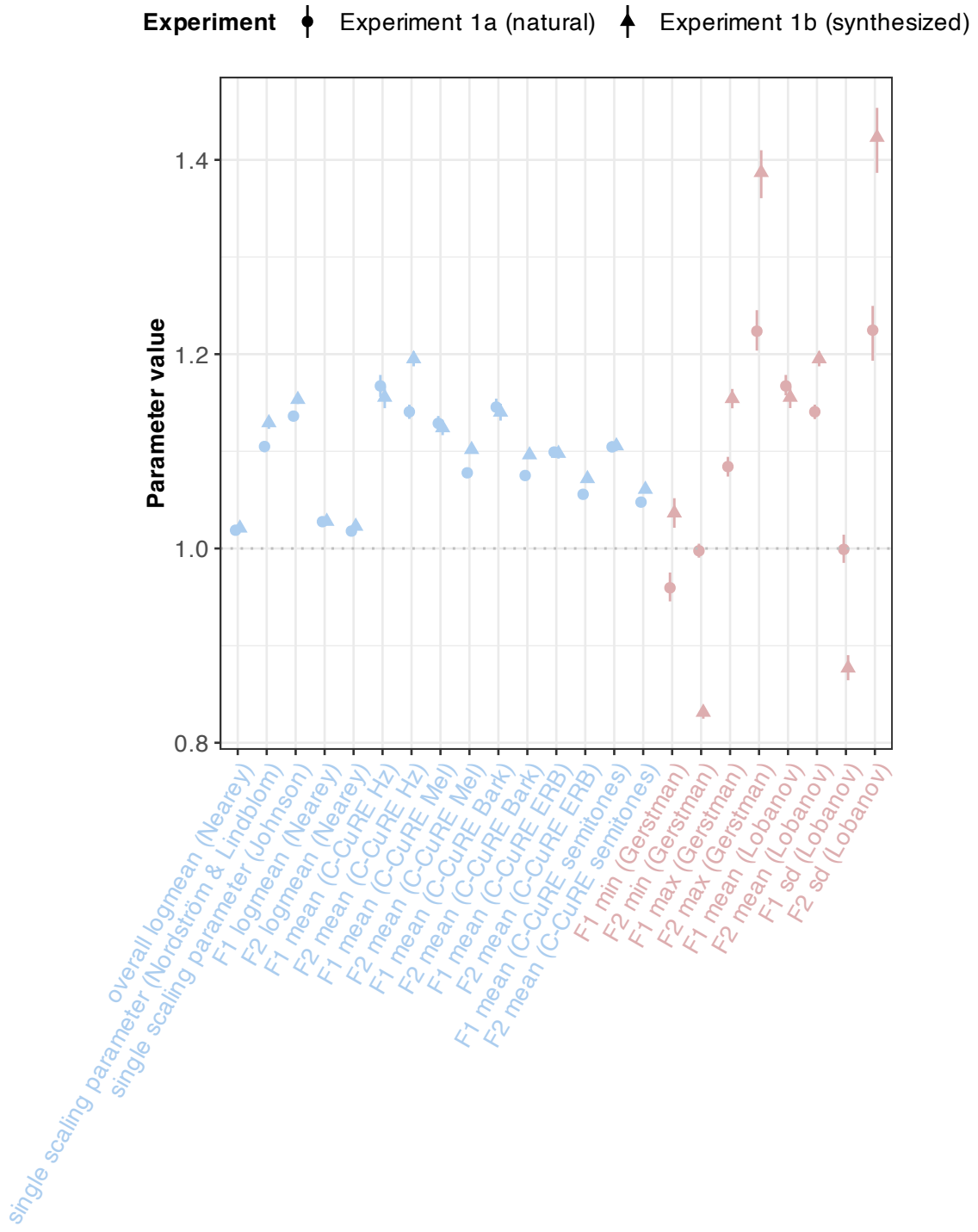

FIG. S8. Comparing normalization parameters θ across the phonetic database used to estimate listeners' prior experience (Xie and Jaeger, 2020) and Experiments 1a and 1b. Only accounts that assume talker-specific normalization parameters are shown.

≥ 10 (well above previously observed estimates for perceptual noise in Kronrod *et al.*, 2016, p. 1698).

Section §3 F presents additional analyses that instead used a grid search over the parameter space. These analyses confirm the results presented in the main paper.

B. Results for F1-F2

1. Significance test of model performance

Tables S1 and S2 present the results from the paired one-sided t-tests conducted, predicting model log likelihood as a function of normalization account for Experiment 1a and 1b (dummy coded with no normalization model as reference model). The log likelihoods are averaged across the five cross-validation folds and ordered by best-fitting models.

2. Parameter estimates for best-fitting models

In this section, we provide the estimates found for the two degrees of freedom—noise (Σ_{noise}) and attentional lapses (λ)—when fitting the models to human behaviour. This will provide insights into the relative contribution of these factors to explaining the variability found in the behavioral data between the two experiments, and to understanding the relative performance of the different normalization accounts as models of human behavior.

Figure S9 visualizes the parameter estimates for each account, averaged across the five training sets (see also Tables S3, S4 for summary of fitted values, and S10 for an illustration of how the fitted noise affects the bivariate Gaussian categories). The Figure indicates that fitted λs are very similar across experiments. In Experiment 1a, the mean λ across models was 0.1 (sd = 0.04), and for Experiment 1b, 0.12 (sd = 0.14). These estimates can in part confirm what was hypothesized with regard to listeners' categorization accuracy—that the performance of listeners in inferring the category intended by the talker in part reflected attentional lapses (mean accuracy in Experiment 1a = 81.2% (SE = 4.8%); Experiment 1b had no such ground truth).

What is perhaps more obvious from Figure S9 is that Σ_{noise} estimates clearly differ between experiments. In Experiment 1a, the best-fitting Σ_{noise} estimates are comparable to what Kronrod *et al.* (2016) found (mean Σ_{noise} in Experiment 1a = 0.52 (sd = 0.49). In

TABLE S1. T-test predicting the model log likelihood as a function of normalization account for Experiment 1a

Normalization account	Statistic	Estimate mean	Diff. in means	p_value
Uniform scaling, Johnson (Hz)	-15.085	-2523.406	611.644	0.000
Uniform scaling, Nearey (log)	-9.100	-2523.406	551.676	0.000
C-CuRE (Bark)	-13.229	-2523.406	549.192	0.000
C-CuRE (Mel)	-12.722	-2523.406	546.779	0.000
C-CuRE (ERB)	-11.847	-2523.406	546.472	0.000
Nearey's formantwise mean (log)	-10.428	-2523.406	543.718	0.000
C-CuRE (semitones)	-10.428	-2523.406	543.718	0.000
Uniform scaling, Nordström & Lindblom (Hz)	-7.791	-2523.406	517.075	0.001
SyrdalGopal (Bark)	-9.381	-2523.406	510.360	0.000
C-CuRE (Hz)	-10.169	-2523.406	498.443	0.000
Miller (log)	-7.466	-2523.406	464.305	0.001
Lobanov (Hz)	-8.970	-2523.406	461.405	0.000
transformed (Bark)	-13.415	-2523.406	228.190	0.000
transformed (Mel)	-12.472	-2523.406	214.317	0.000
transformed (ERB)	-10.291	-2523.406	192.156	0.000
SyrdalGopal2 (Bark)	-3.673	-2523.406	171.154	0.011
Gerstman (Hz)	-2.104	-2523.406	159.477	0.052
transformed (log)	-2.617	-2523.406	66.928	0.029
transformed (semitones)	-2.617	-2523.406	66.928	0.029

Experiment 1b, this is not the case (mean $\Sigma_{noise} = 4.74$ (sd=2.57). However, there is no a priori reason to expect internal perceptual noise to differ between experiments, which is why these high noise ratios likely reflect external noise. Given what was shown for the human data (cf. discussion on differences in response entropy between experiments, Section II B),

TABLE S2. T-test predicting the model log likelihood as a function of normalization account for Experiment 1b

Normalization account	Statistic	Estimate mean	Diff. in means	p_value
Uniform scaling, Nearey (log)	-64.722	-10372.71	2553.594	0.000
Lobanov (Hz)	-82.707	-10372.71	2521.647	0.000
Gerstman (Hz)	-34.270	-10372.71	2491.092	0.000
C-CuRE (ERB)	-35.509	-10372.71	2409.974	0.000
C-CuRE (Bark)	-33.226	-10372.71	2408.381	0.000
Nearey's formantwise mean (log)	-35.495	-10372.71	2305.669	0.000
C-CuRE (semitones)	-35.495	-10372.71	2305.669	0.000
transformed (log)	-63.629	-10372.71	2259.438	0.000
transformed (semitones)	-63.629	-10372.71	2259.438	0.000
C-CuRE (Mel)	-28.933	-10372.71	2221.903	0.000
transformed (ERB)	-51.487	-10372.71	2106.368	0.000
transformed (Bark)	-46.651	-10372.71	1942.912	0.000
SyrdalGopal2 (Bark)	-29.866	-10372.71	1816.140	0.000
transformed (Mel)	-41.633	-10372.71	1622.458	0.000
Miller (log)	-26.334	-10372.71	1244.978	0.000
Uniform scaling, Johnson (Hz)	-8.574	-10372.71	1069.757	0.001
SyrdalGopal (Bark)	-20.657	-10372.71	910.361	0.000
Uniform scaling, Nordström & Lindblom (Hz)	-11.293	-10372.71	878.721	0.000
C-CuRE (Hz)	-10.441	-10372.71	817.140	0.000

this is perhaps not surprising. The stimuli in Experiment 1b were clearly more noisy and presumably left listeners with more uncertainty about the true value of the formants, and how to best make use of previous experience. Even if the task itself was identical across experiments, the nature of the stimuli in Experiment 1b likely contributed to making the

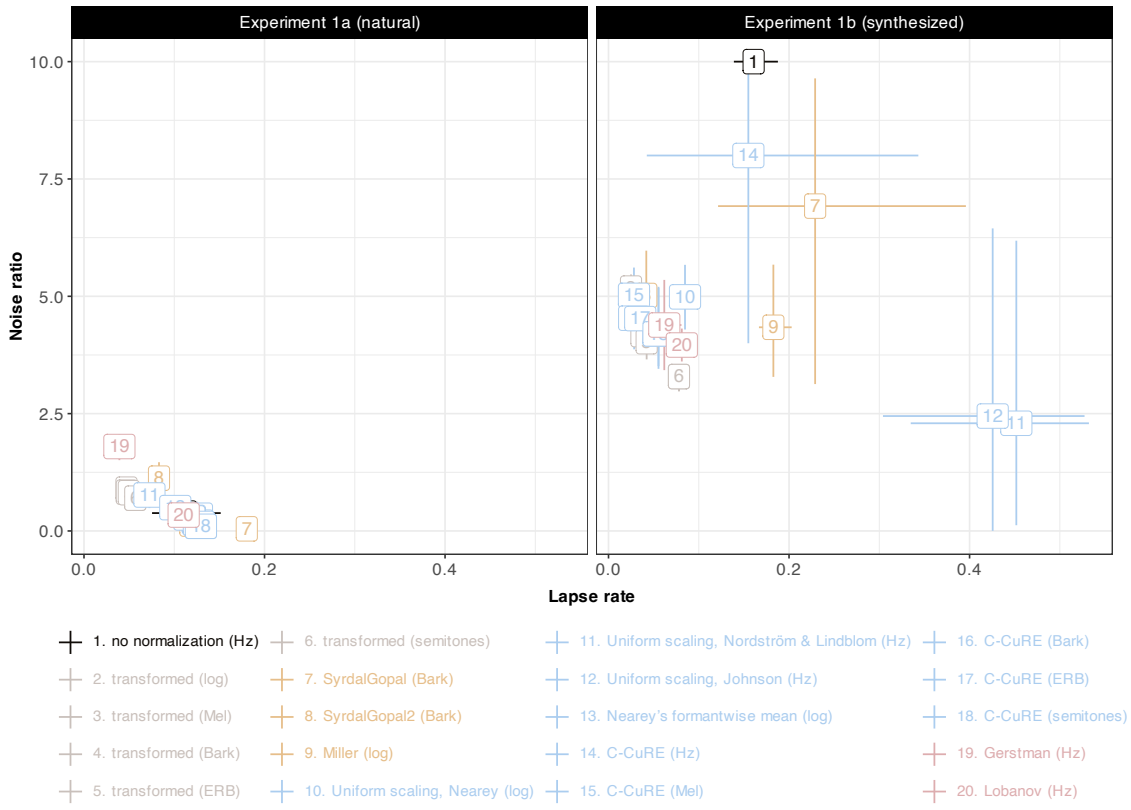

FIG. S9. Best-fitting estimates obtained for λ and Σ_{noise}. Numeric label is placed at the mean across the five folds, line ranges represent the 95% CIs.

experiment overall more demanding. In addition, Experiment 1b was longer (N=74 more trials, and took on average 8.1 more minutes to complete), both of these aspects might also affect the amount of attentional lapsing.

Finally, for the majority of accounts, there is little variability in parameter estimates—and likelihoods—across training sets (Tables S3 and S4). This suggests that models achieved their maximum likelihood fit to human data on similar estimates for the two degrees of freedom, which provides a sanity check of the modelling approach adopted.

3. By-item analysis

To provide further insight into model performance, we visualize model fits against human behavior on a by-item level for three of the best-performing models across experiments, Nearey's uniform scaling, Johnson's uniform scaling and Lobanov. This allows us to as-

TABLE S3. The best-fitting estimates obtained for noise ratios and lapse rates in Experiment 1a (averaged across the five cross-validation folds and ordered by best-performing models)

Normalization account	mean log likelihood	noise percentage	lapse rate
Uniform scaling, Johnson (Hz)	-2214.93	mean=0.5 (SD=0.28)	mean=0.1 (SD=0.01)
Uniform scaling, Nordström & Lindblom (Hz)	-2219.16	mean=0.77 (SD=0.39)	mean=0.07 (SD=0)
C-CuRE (Bark)	-2261.27	mean=0.28 (SD=0.26)	mean=0.12 (SD=0.02)
C-CuRE (Mel)	-2261.61	mean=0.29 (SD=0.27)	mean=0.12 (SD=0.02)
C-CuRE (ERB)	-2262.53	mean=0.16 (SD=0.23)	mean=0.13 (SD=0.02)
C-CuRE (semitones)	-2264.28	mean=0.1 (SD=0.17)	mean=0.13 (SD=0.01)
Nearey's formantwise mean (log)	-2264.28	mean=0.1 (SD=0.17)	mean=0.13 (SD=0.01)
Uniform scaling, Nearey (log)	-2273.68	mean=0.34 (SD=0.36)	mean=0.13 (SD=0.02)
C-CuRE (Hz)	-2305.16	mean=0.19 (SD=0.15)	mean=0.13 (SD=0.02)
SyrdalGopal (Bark)	-2357.42	mean=0.05 (SD=0.04)	mean=0.18 (SD=0.01)
Miller (log)	-2374.96	mean=0.15 (SD=0.06)	mean=0.12 (SD=0.01)
Lobanov (Hz)	-2390.23	mean=0.34 (SD=0.27)	mean=0.11 (SD=0.02)
transformed (Bark)	-2569.87	mean=0.83 (SD=0.2)	mean=0.05 (SD=0)
transformed (Mel)	-2587.65	mean=0.89 (SD=0.18)	mean=0.05 (SD=0)
transformed (ERB)	-2598.16	mean=0.81 (SD=0.24)	mean=0.05 (SD=0)
Gerstman (Hz)	-2647.38	mean=1.8 (SD=0.33)	mean=0.04 (SD=0)
SyrdalGopal2 (Bark)	-2661.45	mean=1.12 (SD=0.39)	mean=0.08 (SD=0.01)
transformed (semitones)	-2682.60	mean=0.7 (SD=0.34)	mean=0.06 (SD=0.01)
transformed (log)	-2682.60	mean=0.7 (SD=0.34)	mean=0.06 (SD=0.01)
no normalization (Hz)	-2779.13	mean=0.38 (SD=0.26)	mean=0.11 (SD=0.05)

TABLE S4. The best-fitting estimates obtained for noise ratios and lapse rates in Experiment 1b (averaged across the five cross-validation folds and ordered by best-performing models)

Normalization account	mean log likelihood	noise percentage	lapse rate
Gerstman (Hz)	-9474.08	mean=4.39 (SD=1.24)	mean=0.06 (SD=0.03)
Uniform scaling, Nearey (log)	-9551.73	mean=4.99 (SD=0.87)	mean=0.08 (SD=0)
C-CuRE (Bark)	-9595.30	mean=4.53 (SD=0.9)	mean=0.03 (SD=0.02)
C-CuRE (ERB)	-9601.26	mean=4.52 (SD=0.93)	mean=0.04 (SD=0.02)
Lobanov (Hz)	-9608.45	mean=3.96 (SD=0.47)	mean=0.08 (SD=0.01)
Nearey's formantwise mean (log)	-9702.20	mean=4.2 (SD=1.08)	mean=0.06 (SD=0.03)
C-CuRE (semitones)	-9702.20	mean=4.2 (SD=1.08)	mean=0.06 (SD=0.03)
C-CuRE (Mel)	-9772.00	mean=5.02 (SD=0.71)	mean=0.03 (SD=0.01)
transformed (semitones)	-9815.97	mean=3.29 (SD=0.37)	mean=0.08 (SD=0.01)
transformed (log)	-9815.97	mean=3.29 (SD=0.37)	mean=0.08 (SD=0.01)
transformed (ERB)	-9956.82	mean=4.03 (SD=0.42)	mean=0.04 (SD=0.01)
transformed (Bark)	-10123.34	mean=4.18 (SD=0.42)	mean=0.04 (SD=0.01)
SyrdalGopal2 (Bark)	-10231.19	mean=5.02 (SD=1.14)	mean=0.04 (SD=0.01)
transformed (Mel)	-10431.06	mean=5.17 (SD=0.39)	mean=0.02 (SD=0.01)
Uniform scaling, Johnson (Hz)	-10791.39	mean=2.45 (SD=4.33)	mean=0.43 (SD=0.14)
Miller (log)	-10848.88	mean=4.34 (SD=1.54)	mean=0.18 (SD=0.02)
Uniform scaling, Nordström & Lindblom (Hz)	-11085.94	mean=2.29 (SD=4.32)	mean=0.45 (SD=0.13)
C-CuRE (Hz)	-11098.93	mean=8 (SD=4.47)	mean=0.15 (SD=0.21)
SyrdalGopal (Bark)	-11187.23	mean=6.92 (SD=4.17)	mean=0.23 (SD=0.19)
no normalization (Hz)	-12118.49	mean=10 (SD=0)	mean=0.16 (SD=0.03)

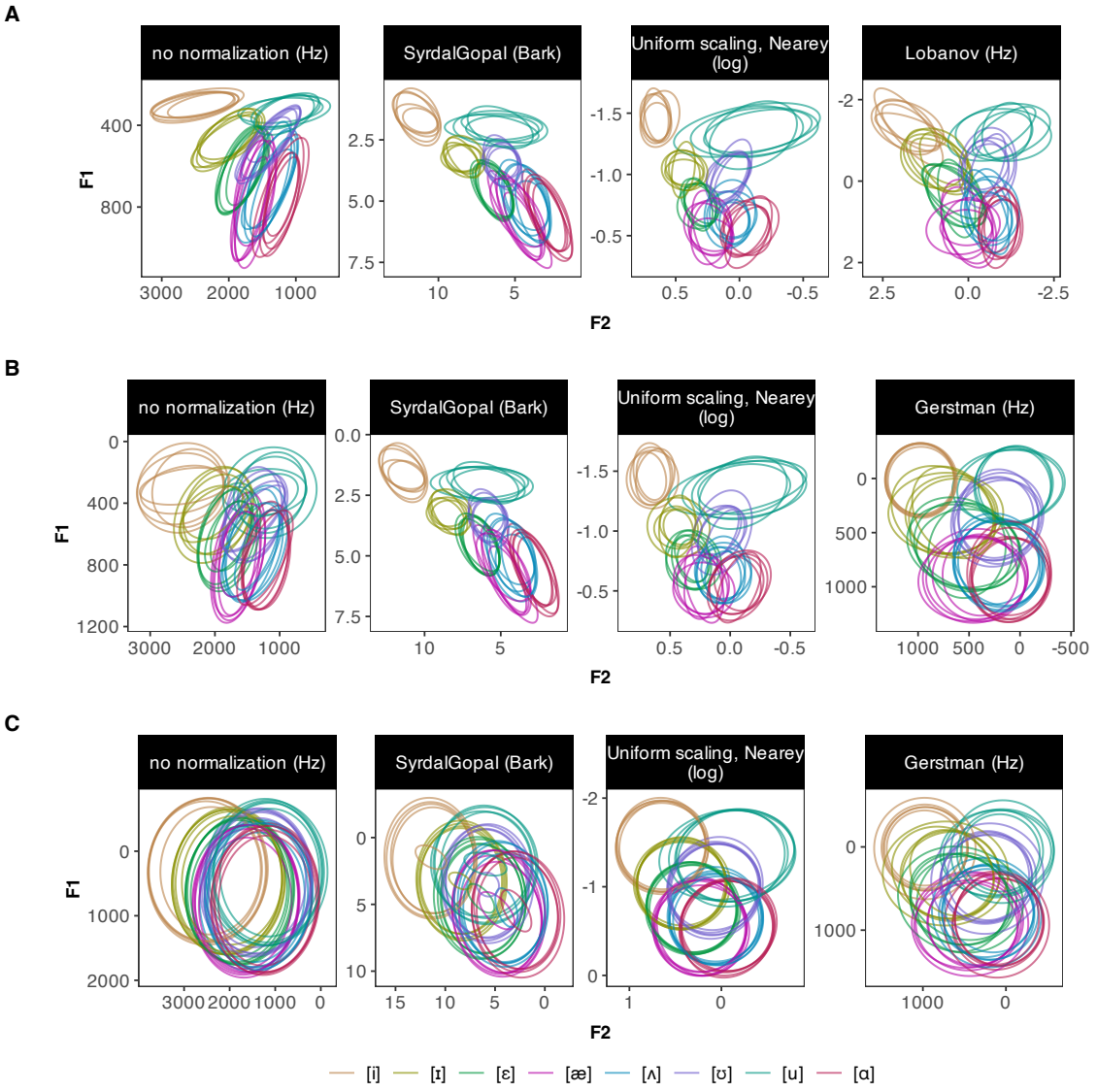

FIG. S10. Visualizing the bivariate Gaussian categories of four example normalization accounts for each of the five cross-validation folds (each fold corresponds to one set of eight ellipses). **Panel A** prior to adding Σ_{noise}, **Panel B** with added noise from best-fitting models in Experiment 1a, **Panel C** with added noise from best-fitting models in Experiment 1b. For most of the accounts in Panel B and C, noise ratios adds so much category variability that models could presumably only make correct predictions at the outer range of the ellipses. If allowing for separate noise estimates for F1 and F2, this might however not be the case.

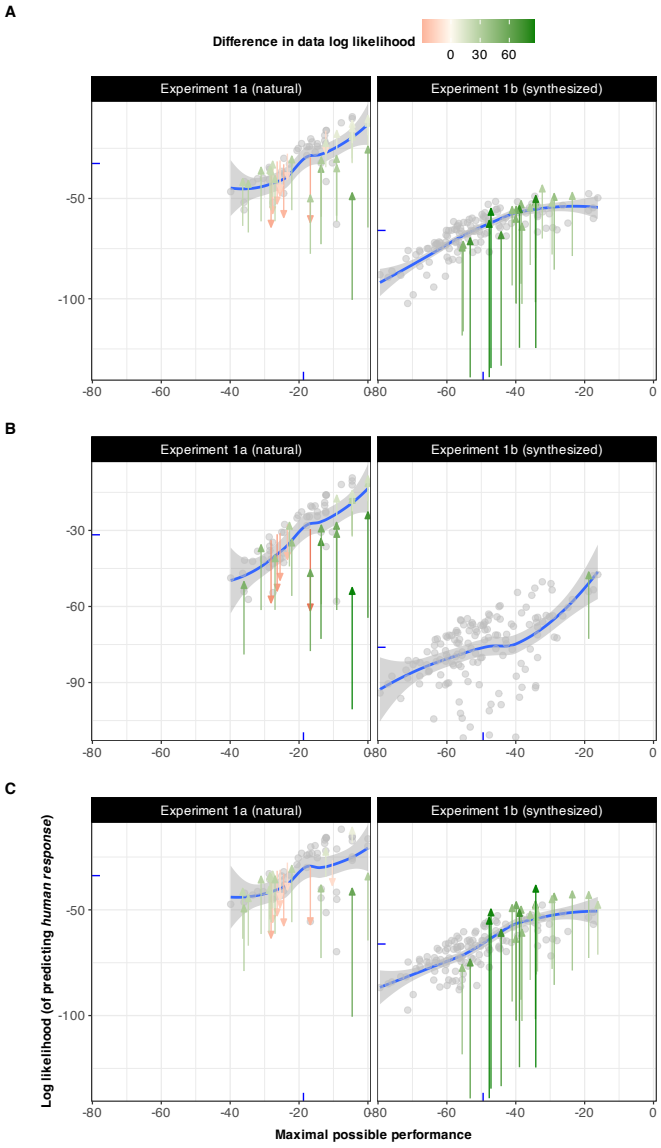

FIG. S11. By-item model improvement from no normalization, relative to the maximum possible performance (predicting human responses from human responses). Maximum log likelihood across items indicated by ticks on axis. Arrows indicate change from no normalization to Nearey's uniform scaling (**panel A**), Johnson (**panel B**), and Lobanov (**panel C**), for items with a change of more than 35%. Points represent items for which change is less than 35%. Color and arrow head indicate decrease or increase in log likelihood.

sess whether normalization always improves model fit in absence of normalization, and if normalization models perform equally well in different parts of the acoustic-phonetic space.

Figure S11 indicates a general tendency for increased model performance as humans' predictions about human behavior become stronger, even though models' improvements are not limited to items for which humans have strong predictions. Normalization does not, however, improve model fit across the board. Relative to no normalization, all three accounts both increase and decrease in performance on a by-item level. The advantage of Nearey's uniform scaling relative to no normalization seems to be driven by smaller improvements (<35% change) on many items in Experiment 1a (proportion of items with increase in performance = 76.4%, mean improvement in likelihood by item = 13.92 (sd = 11.87), mean likelihood by item for items where there is *no* improvement = -12.59 (sd = 10.81)), whereas for Experiment 1b, Nearey's uniform scaling improves substantially on many items (proportion = 93.2, mean improvement in likelihood by item = 19.32 (sd = 15.38), mean likelihood by item for items where there is *no* improvement = -7.37 (sd = 4.62)). Johnson follows the same pattern for Experiment 1a only (proportion = 80.6%, mean improvement in likelihood by item = 13.16 (sd = 11.36), mean likelihood by item for items where there is *no* improvement = -10.84 (sd = 9.91)), while for Experiment 1b, improvements are less pronounced (proportion = 75.3%, mean improvement in likelihood by item = 11.9 (sd = 7.74), mean likelihood by item for items where there is *no* improvement = -6.65 (sd = 4.47)). Lobanov seems to follow the same pattern as Nearey (for Experiment 1a, proportion = 73.6%, mean improvement in likelihood by item = 12.67 (sd = 12.08), mean likelihood by item for items where there is *no* improvement = -11.06 (sd = 9.82); for Experiment 1b, proportion = 87%, mean improvement in likelihood by item = 20.59 (sd = 17.07), mean likelihood by item for items where there is *no* improvement = -4.92 (sd = 4.62)).

To explore whether differences in model performance are related to where in the acoustic-phonetic space items are located, we plot the likelihood of the unnormalized model in the acoustic-phonetic space, along with likelihood differences between the best-performing models (see Figure S12).

Figure S12 suggests that normalization does not improve things universally across the acoustic-phonetic space. Overall, model performance is better for items for which human predictions are stronger, that is, models perform better in parts of the acoustic space where humans can easier predict human behavior (Figure S12, *Panel A*). To the extent that this is not the case, it seems that normalization in general can adjust for this, improving model performance on many tokens where the maximum performance is high but the unnormalized

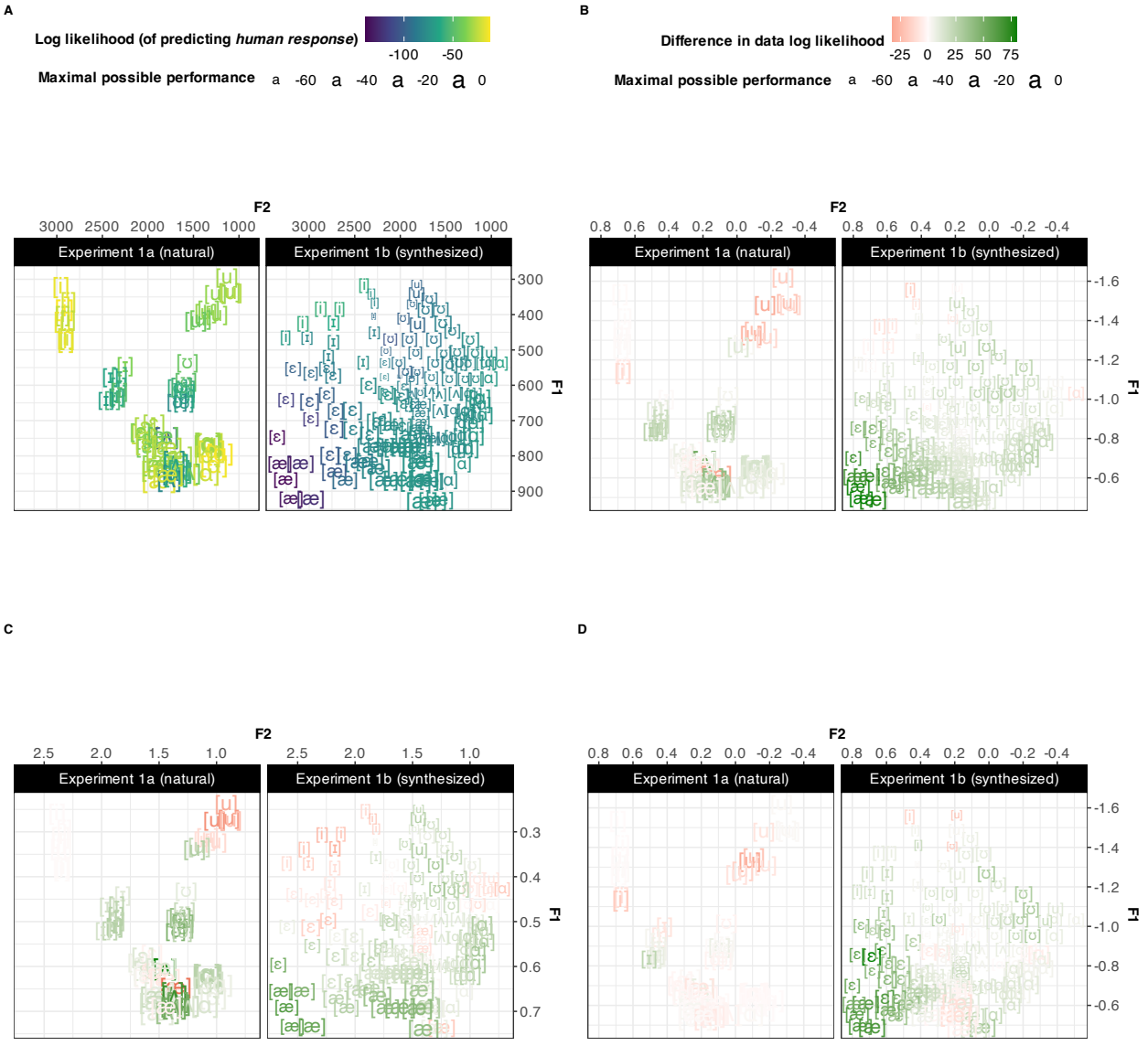

FIG. S12. In which part of the acoustic-phonetic space does normalization fail to improve fit against human responses? For each test location, the vowel label indicates the most frequent response provided by participants. Size of vowel label relates model performance to maximum performance (predicting human responses from human responses). **Panel A** shows the likelihood of the unnormalized model in predicting human responses to both experiments. **Panels B-D** shows difference in likelihood between models, Nearey's uniform scaling vs. no normalization (**panel B**), Johnson vs. no normalization (**panel C**), Nearey's uniform scaling vs. Johnson (**panel D**).

model's predictions are low, e.g., in the left bottom and center part of the acoustic space (*Panels B-C*). There is overall less improvement in Experiment 1a, presumably because models are already performing well predicting human behavior in the first experiment. Both Nearey's uniform scaling and Johnson clearly perform worse relative to the unnormalized model in the upper right part of the space, more specifically for the [u] category (*Panels B-C*; left), which could indicate that models are overly categorical in a part of the space where humans are less categorical. Possible reasons to this, could be 1) the stimuli sounding more like a neighbouring category to many listeners, or 2) potential effects of orthography, making humans less inclined to select the [u] category. The potential effect of the infrequent non-word response option *who'd* could have been checked against the synthesized stimuli in Experiment 1b. If there was indeed an effect of orthography, we should have observed a better model fit and larger between-account differences in predictions in this part of the acoustic space. Unfortunately, we under-sampled that part, which is an important caveat for Experiment 1b. For the items closest to the area in question, participants however often responded *hood*, which might indicate that items in this part of the space for this talker overall sounded more like *hood* and not *who'd* for many listeners (c.f., discussion on listeners' dialect templates in Section II B).

Comparing the two best-performing models across experiments (*Panel D*), there are no evident patterns of improvement in one model relative to the other. In Experiment 1a, Johnson provided the best fit to listeners' responses and appears to improve the fit relative to Nearey across the entire space (with the exception of one [ɪ] token). For Experiment 1b, Nearey overall improves the fit relative to Johnson, with the exception of some locations in the mid part of the phonetic space (including high, center and low vowels).

C. Results for F1-F2 (subsets of Experiments 1a and 1b)

To evaluate two potential concerns with our stimuli, we decided to compare the 20 normalization accounts against a subset of the data from Experiment 1a and 1b. For Experiment 1a, we excluded listeners' responses to the two *hVd* stimuli that differed in phonological context from all other words: *odd* and *hut*. For Experiment 1b, we excluded responses to stimuli that were presumed physiologically implausible under the assumption of a single talker (all stimuli below the diagonal dashed line in Figure 4).

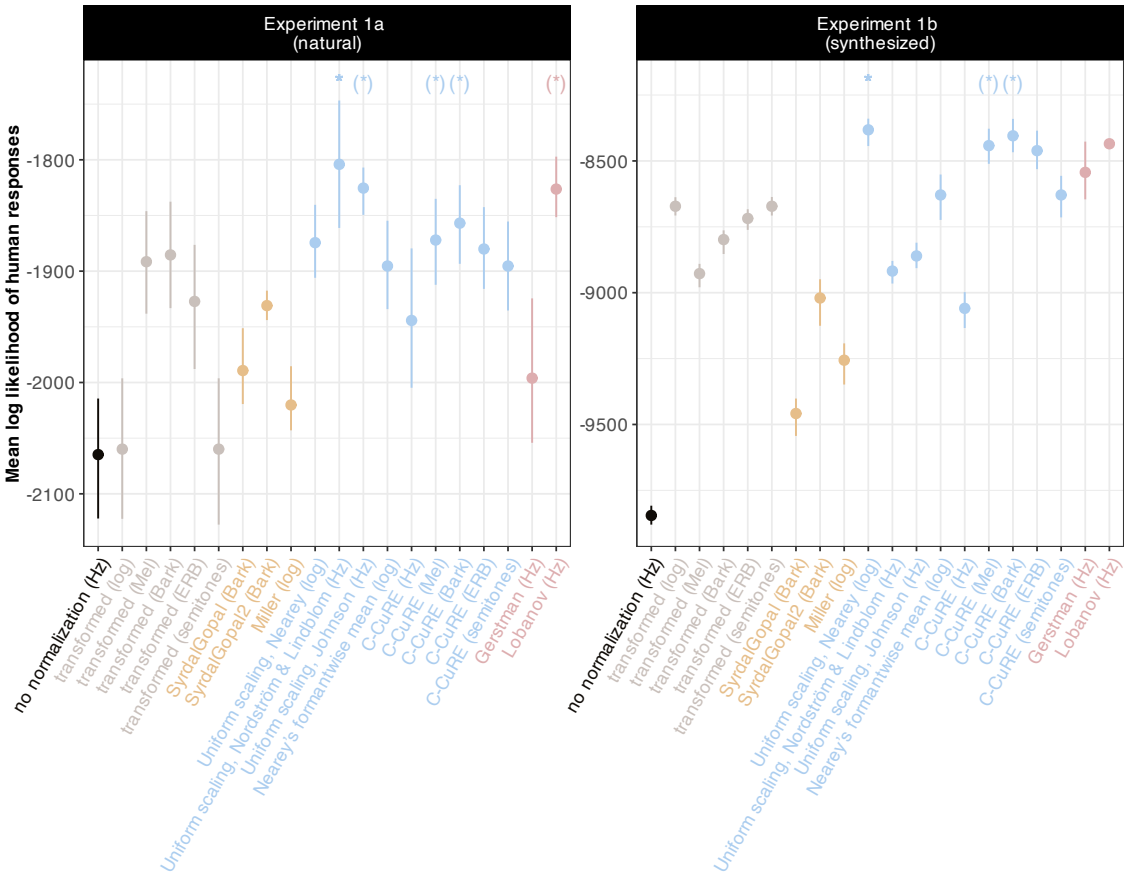

FIG. S13. Results of model fit to subset data. Pointranges indicate mean and 95% bootstrapped CIs of the log-likelihood summarized over the five training sets (higher is better). Accounts that fit listeners' responses to an extent that is statistically indistinguishable from the best-fitting account are marked by (*).

This analysis overall replicates the results from the main analysis: uniform scaling accounts again provide the best fit against listeners' responses in both experiments (Figure S13). For Experiment 1a, Nordström & Lindblom achieved the best fit (log likelihood = -1804, SD =), while Nearey's uniform scaling again provided the best fit to Experiment 1b (log likelihood = -8382, SD = 71).

D. Results for F1-F2 (subset of listeners sharing dialect template)

Analyses in the main paper suggested that not all listeners in Experiment 1a and 1b shared dialect template (Section II B). To investigate the effect of excluding listeners that likely did not use the same underlying vowel representations for categorization, we compared

the 20 normalization accounts against a subset of listeners who employed the dialect template used by the majority of participants (see lower-left of both panels in Figure 5B). This left 11 participants for Experiment 1a (61.1%) and 14 for Experiment 1b (77.8%). Under the assumptions that 1) our model of listeners is adequate, that 2) the subset group of listeners now share dialect template and that 3) the priors, the phonetic database, can approximate this template, we would expect all model to increase their likelihood fit to listeners' responses (c.f., Section IV).

As expected, Figure S14 suggests that all models overall provide higher likelihood fits against human responses in both experiments compared to the main model. To increase comparability to the results of the main model, we scaled the log likelihood of models in the subset data to those of the main analysis by multiplying the model log likelihoods with the ratio of the number of observations in the main model over the number of observations in the subset model. This suggested that the improvement in likelihood for the dialect subset model to the original dataset was 41.1%.

Replicating the results from the main analysis, uniform scaling accounts again fit listeners' behavior well across both experiments. While Nearey's uniform scaling provided the best-fit in Experiment 1b (log likelihood = -5591, SD = 47), Syrdal & Gopal now achieved the best fit to Experiment 1a (log likelihood = -618, SD = 19). Only one additional account performed within the range of the best-performing accounts: for Experiment 1a, all $ps < .0341$; for Experiment 1b, Lobanov achieved likelihood fits statistically indistinguishable from Nearey's uniform scaling ($p > .15$, log likelihood = -5612, SD = 30).

While Nearey's uniform scaling displayed relatively stable performance across experiments, Syrdal & Gopal varied drastically, achieving one of the worst fits to listeners' responses in Experiment 1b (log likelihood = -6684, SD = 75). As mentioned in Section III B 2, a possible explanation to large fluctuations in model fits between experiments, is that this account has been over-engineered on specific types of natural vowel productions. Given that formant normalization is a pre-linguistic mechanism, it ought to be able to explain listeners' responses to any type of data, including data that does not follow correlations in natural data. This would suggest that Syrdal & Gopal might not be a plausible account of normalization.

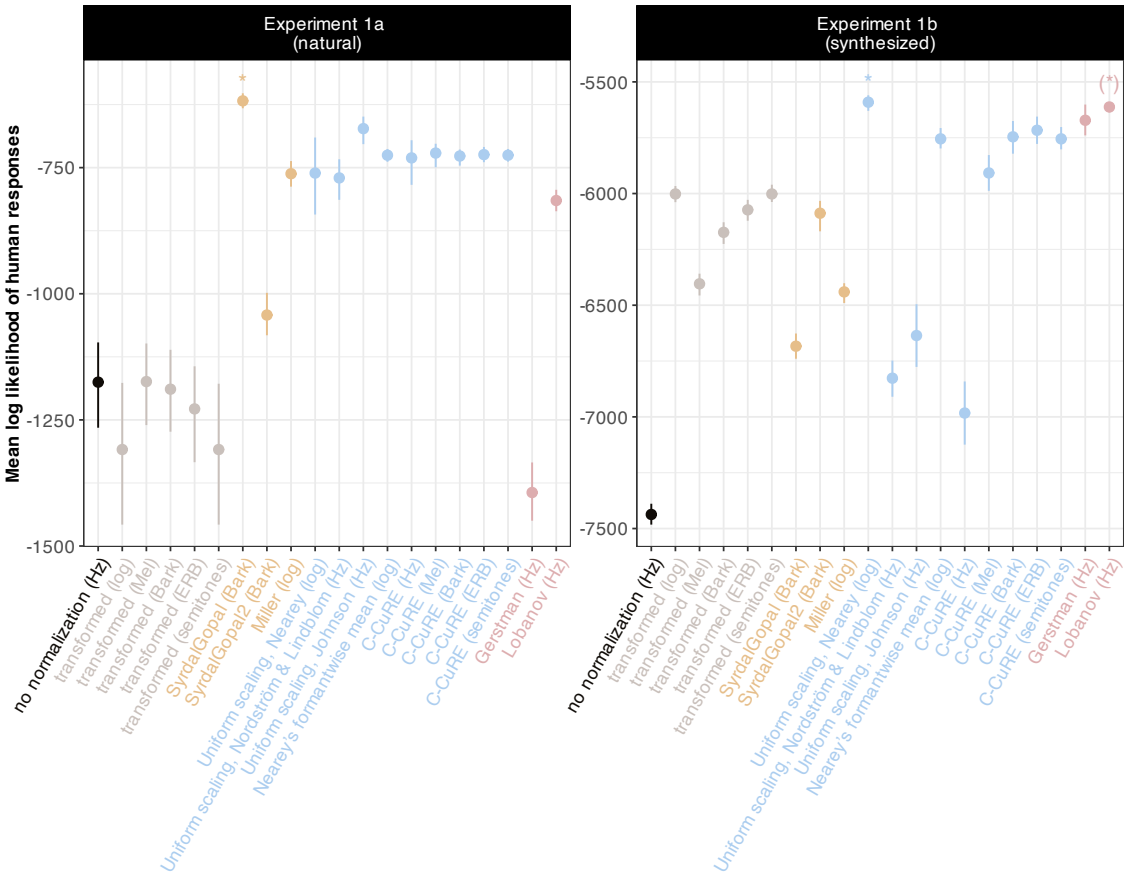

FIG. S14. Results of model fit to data excluding listeners that do not seem to share dialect template. Pointranges indicate mean and 95% bootstrapped CIs of the log-likelihood summarized over the five training sets (higher is better). Accounts that fit listeners' responses to an extent that is statistically indistinguishable from the best-fitting account are marked by (*).

E. Results for F1-F3

To investigate whether the inclusion of F3, a cue known to be important for vowel category distinctions, would improve the model fit to human behavior, we trained ideal observers on multivariate (F1-F2-F3) categories from the same database as in the main study. Here, we first report the results of the F1-F3 model and qualitatively compare them to the results in the main text for F1-F2. This will highlight that the results are largely similar and support the same conceptual conclusions, but there are some differences in model fit. To understand these differences better, we then also directly compare the results quantitatively to see for which accounts the inclusion of F3 improved the fit against listeners' responses and for which accounts it decreased the fit.

Figure S15 summarizes how well the different accounts fit listeners' responses in Experiments 1a and 1b when assuming F1-F2-F3 multivariate category representations. Many aspects replicate the F1-F2 results reported in the main text. First, normalization significantly improved the fit relative to no normalization. Second, the same uniform scaling accounts again achieved the best fit against listeners' responses: for Experiment 1a, Johnson normalization account provided the best fit (log likelihood = -2345, SD = 23), while Nearey's uniform scaling account provided the best fit to Experiment 1b (log likelihood = -9610, SD = 76). However, we note that the inclusion of F3 does not improve the fit to listeners' responses for several accounts (compare *squares* and *circles* in Figure S15). In fact, with the exception of the raw Hertz, scale transformations, and intrinsic accounts, most extrinsic accounts seem to decrease their fit, more so in Experiment 1a than 1b. This includes the overall best-performing account in the main text, Nearey's uniform scaling, that no longer achieves a statistically indistinguishable fit from Johnson in Experiment 1a. At first blush, this is puzzling given that the model now has access to more information of a type that is broadly believed to be informative for US English vowel recognition (Hillenbrand *et al.*, 1995; Nearey, 1989; Peterson and Barney, 1952). What might be underlying the apparent lack of improvement, and why does it appear as if some accounts actually achieve worse fits?

One possible explanation is that listeners were only exposed to one talker in Experiment 1a. According to some theories, F3 is expected to contribute to vowel recognition when there are multiple talkers, acting as a sort of normalizer for vocal tract length (Nearey, 1989). In the absence of other talkers, this advantage might instead introduce noise to the models—an additional source of information that are not of use for listeners in this context. It is also possible that this particular talker has a pattern of F3 use across categories that is atypical given the other talkers in the database. This might explain why the raw Hertz model improves the fit with F3-inclusion. We checked for additional outliers along F3 for this talker, and also inspected the talker's categories in 3D-space (S4), but we could not find that outliers would be a likely explanation. To gain further knowledge into this talker's use of F3 compared to other talkers in the database, we used the same models to predict the ground truth, i.e., the category the talker actually intended to produce. These models patterned with the other prediction results, again indicating that F3-inclusion did not improve model performance. We take this to suggest that the F1-F3 results is not about how our model uses F3, but rather about how this specific talker uses F3. The results might thus link back to the potential dialect differences between talkers in the database, reported in Section II B.

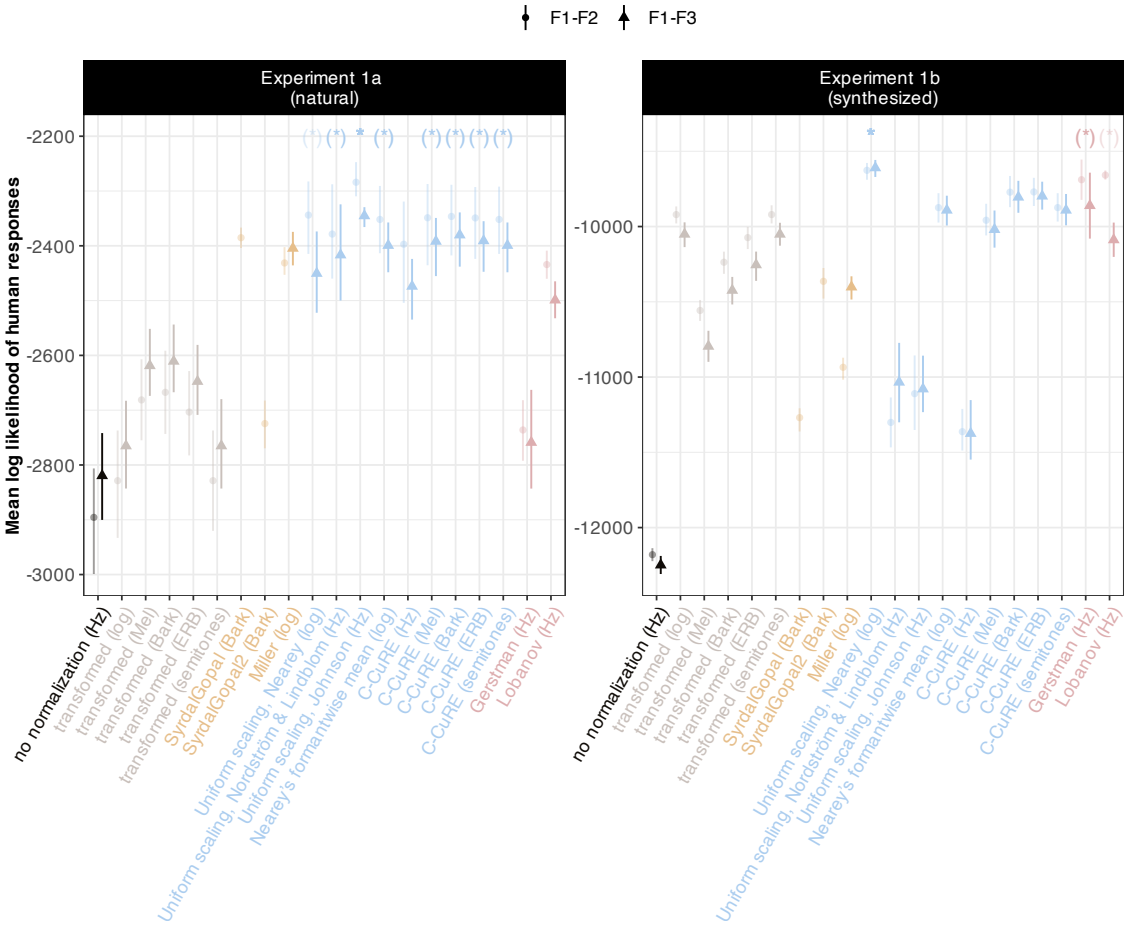

FIG. S15. Results of ideal observer models trained on F1, F2 and F3 as cues to vowel identity. As in Figure 9 in the main text, pointranges indicate mean and 95% bootstrapped CIs of the log-likelihood summarized over the five training sets (higher is better). For comparison, results from the F1-F2 models are included (more transparent circles).

F. Grid search over parameter space for F1-F2 and F1-F3

As an alternative to the quasi-Newton optimization presented in the main text, we also conducted a grid search over the space defined by the two parameters lapse rate and noise ratio. Figure S16 summarizes the results for a grid of lapse rates $\in$ 0, .02, .06, .18, .36, .72 and noise ratios $\in$ 0, .3, .6, 1.25, 2.5, 5 for Experiment 1a. For Experiment 1b (Figure S17), the range of noise ratios explored was $\in$ 0, 1.5, 3, 6, 12.5, 25.

This search confirmed the pattern described in the main text. Additional grid searches confirmed this pattern held beyond the values shown here. For all normalization accounts, all combinations of cues, and both experiments, the goodness of fit of the ideal observers

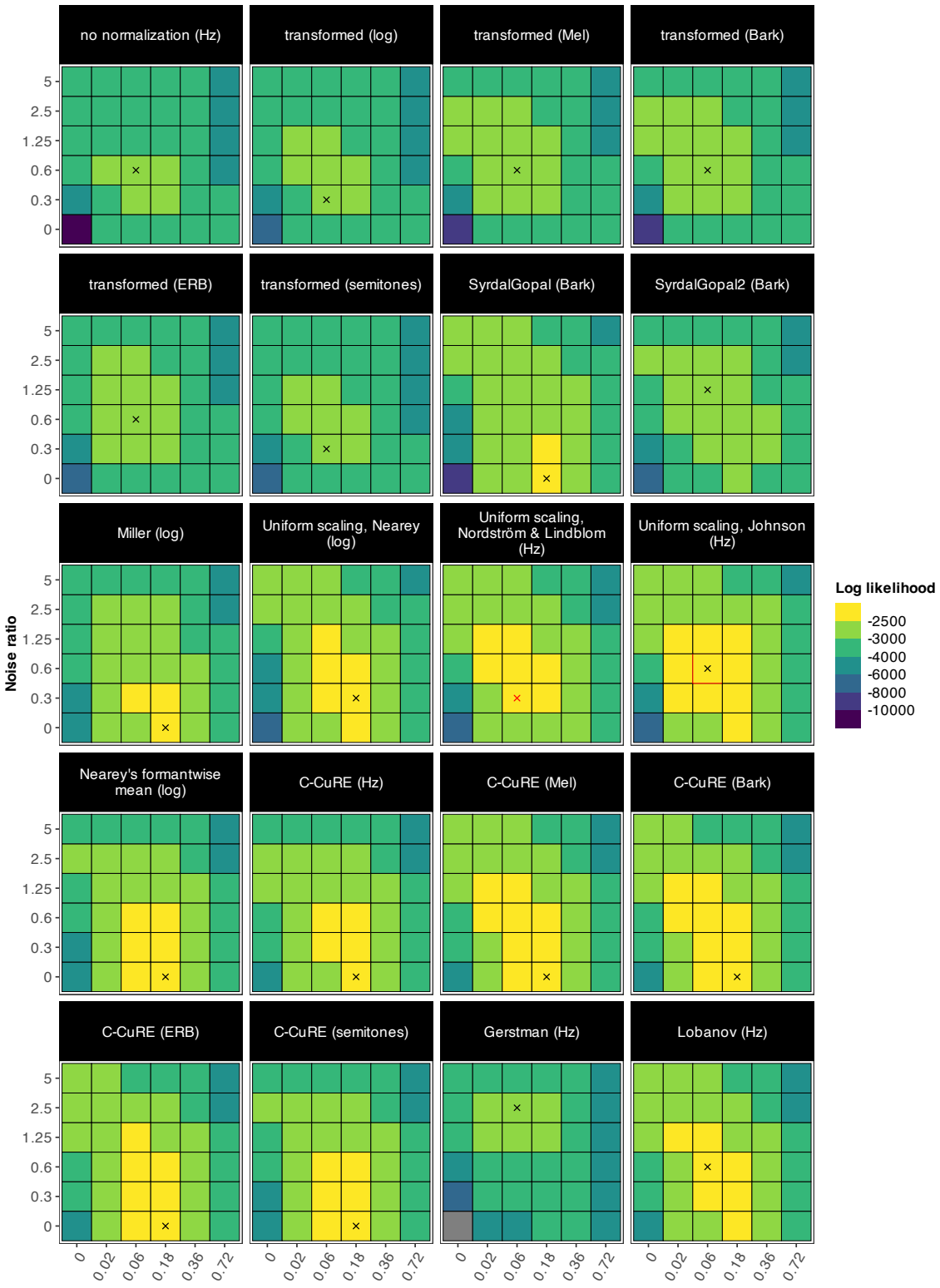

FIG. S16. Predicted likelihoods of ideal observer for human vowel responses in Experiment 1a, under different normalization accounts, different λs and different τ^{-1}s. Likelihood is aggregated across vowels. Crosses are placed at the combination of parameters for which the maximum likelihood for an account and a cross-validation fold was found. The red cross indicates the maximum likelihood across all accounts and folds.

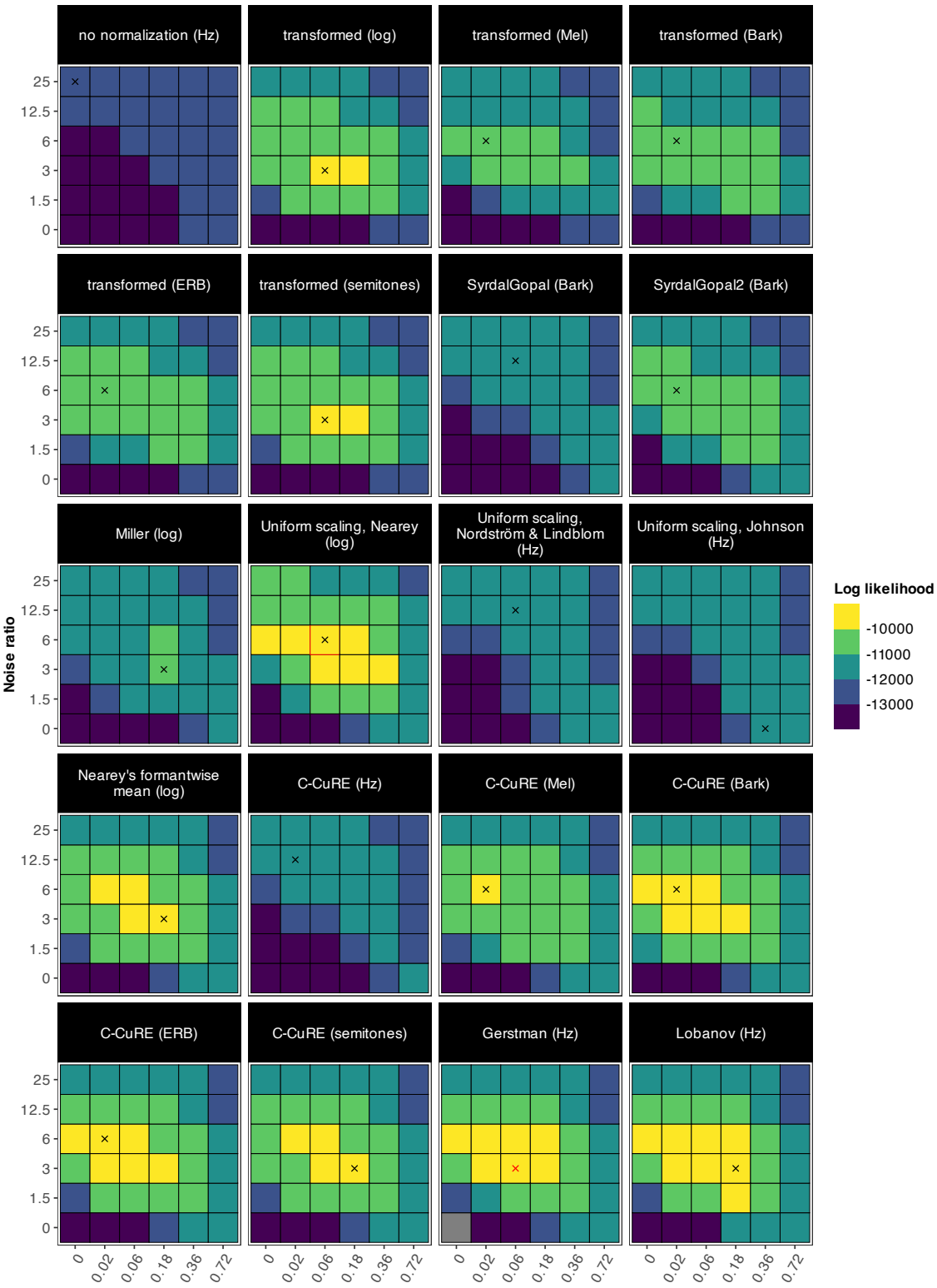

FIG. S17. Predicted likelihoods of ideal observer for human vowel responses in Experiment 1b, under different normalization accounts, different λs and different τ^{-1}s. Likelihood is aggregated across vowels. Crosses are placed at the combination of parameters for which the maximum likelihood for an account and a cross-validation fold was found. The red cross indicates the maximum likelihood across all accounts and folds.

initially improved with increasing lapse rate and increasing noise ratios, and then decreased once lapse rates or noise ratios reached the best-fitting values (which depended on the combination of normalization account, cues, and experiment). It further indicated that Nearey's uniform scaling, together with the other uniform scaling accounts and some of the C-CuRE accounts (Experiment 1a) and Gerstman (Experiment 1b), improved faster and performed consistently well for a good range of parameters, even for high τ^{-1}. Many of the other models were less consistent and only performed well for a smaller range of estimates.

§4. REFERENCES

[1]Normalization does not necessarily imply that *only* talker-normalized auditory percepts are available to subsequent processing. There is ample evidence that subcategorical information can enter listeners' representations of sound categories (e.g., Hay *et al.*, 2017, 2019; Johnson *et al.*, 1999; McGowan, 2015; Walker and Hay, 2011), in line with episodic (Goldinger, 1996) and exemplar theory of speech perception (Johnson, 1997; Sumner, 2011).

[2]Under uniform scaling accounts, listeners essentially 'slide' the center of their category representations (e.g, the 'template' of vowel categories for a given dialect) along a single line in formant space, with Ψ determining the target of this sliding. Later extensions of this account maintain its memory parsimony but increased its inference complexity by allowing both intrinsic (the current F0) and extrinsic information (the talker's single mean of log-transformed formants) to influence the inference of Ψ (Nearey and Assmann, 2007). We return to this extension in the general discussion.

[3]We use Johnson's (2020) implementation of Nordström and Lindblom (1975). We group both Nordström and Lindblom (1975) and Johnson (2020) with the centering accounts, as they are essentially variants of uniform scaling, differing in their estimation of Ψ. We also include both versions of Syrdal & Gopal's Bark-distance model. The two versions differ only in their normalization of F2, and have not previously been compared against human perception.

[4]Shannon (1948) response entropy is defined as $H(x) = -\sum_{i=1}^{n} P(x_i) \log P(x_i)$. The maximum possible response entropy for an 8-way response choice is 3 bits, which means that all eight vowels are responded equally often. The minimum response entropy = 0 bits, which means that the same vowel is responded all the time.

[5]Note that participants in Experiment 1a exhibited high agreement on [ʌ], [æ], and [ɑ], despite the close proximity between, and partial overlap of, these vowels in F1-F2 space. To understand this pattern, it is

important to keep in mind that the recordings for [ʌ] and [ɑ] differed from the recordings for other stimuli in their word onset ("odd" for [ɑ]) or offset ("hut" for [ʌ]).

[6][u] has been undergoing changes in many varieties of US English. Whereas the talker in Experiment 1a produces [u] with low F1 and F2 (high and back), other L1 talkers of US English produce this vowel considerably more forward (higher F2).

[7]For Gaussian noise and Gaussian category likelihoods, the resulting noise-convolved likelihood is a Gaussian with variance equal to the sum of the noise and category variances (Kronrod *et al.*, 2016).

[8]We intentionally did *not* split the data within talkers since normalization accounts are meant to make speech perception robust to cross-talker variability. Further, splitting the data by speaker rather than by vowel category avoids the potential for biases in the normalization parameter estimates for different speakers in the case of missing or unbalanced tokens across vowel categories, see (Barreda and Nearey, 2018). Additional analyses not reported here confirmed that the same results are obtained when splits are performed within talkers and within vowels (except that this lead to smaller CIs, and thus *more* significant differences, in Figure 9). These analyses can be replicated by downloading the R markdown document this article is based on from our OSF (see comments in our code).

[9]Alternatively, it would be possible to treat these parameters as DFs in the link to listeners' responses, and infer them from the responses in Experiments 1a and 1b (cf., Kleinschmidt and Jaeger, 2016). This approach would afford the model with a high degree of functional flexibility, regardless of which normalization approach is applied (similar to previous approaches that have employed, e.g., multinomial logistic regression).

[10]This ratio is a generalization of the inverse of the "meaningful-to-noise variance ratio (τ)" used in Kronrod *et al.* (2016). However, whereas Kronrod and colleagues committed to the simplifying assumption that all categories have identical variance (along all formants), we allowed category variances to differ between vowels, and between F1 and F2 (matching the empirically facts). We merely assume that the *noise* variance is identical across all formants (in the phonetic space defined by the normalization account, e.g., log-Hz for uniform scaling and Hz for Lobanov).

[11]Additional analyses reported in the SI (§3 C) replicated this result for subsets of Experiments 1a and 1b. For Experiment 1a, we excluded responses to the two *hVd* stimuli that differed from the other stimuli in the preceding (*odd*) or following phonological context (*hut*). For Experiment 1b, we excluded responses to any stimuli that were physiologically implausible for the talker (stimuli below the diagonal dashed line in Figure 4).

[12]We thank Xin Xie and Leslie Li for providing us with the recordings and aligned Praat textgrids.

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