# TO DO

## Priority HIGHEST

* Figure out argument for/against ‘pen as occluder’
* **Get exclusions sorted out** @SHAWN
  + Check all exclusion criteria for Exp 1a-c, 2-2b
    - in add\_exclusions, there is commented out code at the top of the function. Why is it commented out, and how are we removing participants for technical difficult if not there?
  + Finalize exclusion criteria for Exp 3 and add them to the document
* In Exp 4, figure out why we have participants that had exposure to pen-in-hand for critical items (18 out of ~128). @SHAWN

## Priority MEDIUM

* make sure that data formatting \*function\* is in \*this\* repo (raw data should \*not\* be in this repo) @SHAWN
  + let’s make BCS206/7 repo *private*
  + let’s move raw data into that repo
  + run script that:
    - format raw mturk and proliferate files into csv [you could integrate additional format steps here]
    - identifies subjects who had technical difficulty and adds an exclude column that marks that
    - and all other exclusions
    - anonymized subjects
    - writes data into CSV
    - create README.md stating that script for import is in private repo
    - copy script *except* for specific subject IDs into that note file
  + consider adding renaming of variables into formatting function, removing it from early code chunk. i.e., move the code contained in format\_more() into the formatting function.
* consolidate constants.R and functions.R. It's currently unclear what's in which and why. @SHAWN
* fill in all survey questions in procedure section for Exp 1a-c, and provide some summary of participants' responses where relevant. @GEVHER
* edit through introduction @FLORIAN
* Consider pooling exp 1a-c when comparing against Exp 2. This would give us a bit more power (at the potential cost of the monotonicity assumption) @FLORIAN
* For exp 3, make plots that visualize the ‘contextual storage’ hypothesis (and consider rerunning a separate model that code pen-location-exposure == pen-location-test as a factor 🡪 this would make the three-way interaction a two-way interaction in our hypothesis test).

## Priority Low

* Look for places with “XXX” and “REF”
* Change in title?
* Update website and see whether it can be integrated into OSF (i.e., work locally on OSF, ideally with videos etc. being links to the local OSF repo) @GEVHER

Causal inference in audiovisual speech perception and adaptation

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**Abstract**

XYZ

**Keywords**: speech perception; perceptual recalibration; causal inference; compensation for coarticulation; XXX

# Introduction

Spoken language understanding involves a “paradox of perception” (Kraljic, Samuel, & Brennan, 2008, p. 332): listeners must discern miniscule acoustic differences as linguistically meaningful (e.g., the deviations in frequency spectra between English “f”, “th”, “s”, and “sh”) while simultaneously ignoring potentially far greater acoustic differences. For example, one talker’s production of the “s” sound in *sun* can be acoustically identical to a different talker’s “sh” in *shun* (Newman et al., 2001, hereafter, /s/ and /ʃ/, respectively). Similarly, a single talker may generate the same acoustics for an /s/ in one phonetic context as for an /ʃ/ in another (Smits, 2001). What mechanisms allow listeners to overcome the resulting “lack of invariance” across talkers and other contexts (Liberman et al., 1957) continuous to be one of the central questions about speech perception.

Research on this question has been pursued in two largely separate lines of work, sometimes drawing on different theoretical frameworks that make conflicting assumptions about mechanisms and representations underlying speech perception. Research on “compensation” and “normalization” has investigated the *in-the-moment* effects of context, i.e., how listeners process and interpret speech inputs relative to the talker or phonetic context (e.g., Barreda, 2021; Lobanov, 197X; McMurray & Jongman, 2011; Syrdal & Gopal, 19XX; for reviews, see COMPENSATION-REVIEW-REF; Sjerps & Johnson, 2021; Weatherholtz & Jaeger, 2016). For example, XXX. Findings like these suggest that listeners deploy implicit expectations about the effects of specific contexts, using them to “explain away” parts of the observed acoustic input, thereby reducing variability of the input across contexts (Fowler, 2006; McMurray & Jongman, 2016). Listeners employ similar perceptual adjustments for familiar talker contexts. For example, listeners process and interpret the same speech input differently depending on the talker’s inferred height (Nearey, XXX), gender (Strand, 1999; McMurray & Jongman, 2016), age (Walker & Hay, 2011; Hay et al., 2019), or race (Babel et al., 2021; McGowan, 2015; Hanulikova et al., 2021; for review, Brown-Schmidt & XXXX, 2018; Hay, 2018; Weatherholtz & Jaeger, 2016).

Separate lines of research have investigated how listeners acquire the implicit expectations that seem to underlie the types of effects summarized above. This question has attracted particular attention with regard to talker contexts: while listeners might learn the effects of phonetic and phonological contexts during language acquisition, they might continue to encounter unfamiliar talkers throughout their life. Work since the early 2000s has found that less than two minutes of exposure to an unfamiliar second language accent can improve subsequent processing (Clarke & Garrett, 2004; Bradlow & Bent, 2008; Xie et al., 2018, 2021). Even exposure to artificially shifted ‘accents’—for which listeners are less likely to have had prior experience—can sometimes rapidly alter subsequent processing of that accent (Norris et al., 2003; Kraljic & Samuel, 2006; Vroomen et al., 2007; Drouin et al., 2016; Liu & Jaeger, 2019; Cummings & Theodore, 2022; for review, Bent & Baese-Berk, 2021; Xie, Jaeger, & Kurumada, 2023). These adaptations can be talker-specific (REFS) and persist over multiple days even in the absence of additional input from the talker (REFS).

Taken together, these two lines of research raise novel questions about the relation between in-the-moment effects of context and the adaptive mechanisms that allow adult listeners to learn new talker contexts. Relatively little is known about what information listeners store about the speech of an unfamiliar talker, and what factors determine whether this information is maintained.

This is true for differences in production resultant of real-world talker characteristics, such as physiology, sociophonetic traits, and idiosyncratic pronunciation habits, as well as for experimenter manipulation of specific sound categories (e.g. Norris, McQueen, & Cutler, 2003). Anecdotally, however, one can imagine that adjusting expectations towards a talker’s speech is *only* facilitatory to future comprehension if the perceived variation is likely to re-occur. While *characteristic* talker differences should elicit adaptation, the overarching goal of efficient communication is not served via adaptation to transient or incidental variations. A growing body of literature, which the current investigation hopes to contribute towards, has examined the specificity of adaptation with regard to *characteristic* versus *incidental* talker variation (Kraljic, Samuel, & Brennan, 2008; Kraljic & Samuel, 2011; Liu & Jaeger, 2018; Liu & Jaeger, 2019; White & Daub, 2021). Specifically, articulatory obstructors, such as a lollipop or pen in a talker’s mouth, have been found to reduce or block talker-specific learning, posited as potentially resultant of causally attributing the sound shifts to the object rather than to the talker (White & Daub, 2021; Kraljic, Samuel, & Brennan, 2008).

The situation created by studies on adaptation in the context of incidental causes provides a productive but complicated test case. Listeners are simultaneously provided with two variables. On the one hand, they hear a novel talker exhibiting speech deviant from their expectations. This has been found in upwards of 80 manuscripts to elicit adaptation (e.g. Norris, McQueen, & Cutler, 2003; Kraljic & Samuel, 2005; Eisner & McQueen, 2006; van Linden & Vroomen, 2007; Liu & Jaeger, 2018; Cummings & Theodore, 2022; Cummings & Theodore, 2023). On the other, they hear speech within a specific atypical context. To date, studies have not examined listeners’ ability to overcome this contextual variation in the abstract, outside of talker-specific learning. It is well established, however, that listeners *do* account for contextual variation within one talker’s speech**. The current investigation hopes to further understanding of the specificity of learning through acknowledging the potential joint contributions of both talker and context on variability in speech.** To this end, an initial goal is to establish incidental causality as a type of contextual variation that perceptual mechanisms may already be equipped to handle. This is achieved through a brief review of findings on compensation for contextual variation, followed by a perception experiment without a learning component (Experiment 1). Having established this link, Experiments 2 and 3 then extend prior study *within* learning. Extant findings herein, as well as specific theoretical implications and proposals, will be unpacked after Experiment 1.

# Open science statement

All experimental materials—including the original video and audio recordings as well as all audiovisual test stimuli for all experiments—lists, and trial-level data are available as part of the OSF repository at XYZ. The same holds for the JavaScript code for the experiments, and the R code for analyses and visualizations. The latter is made available in the form of a “knittable” R Markdown (REF) document that generates the supplementary information for this article through a single click in a freely available software (R, REF; RStudio, REF). Exact replica of all experiments for demonstration purposes are available at XYZ.

# Experiments 1a-c

The phonetic realization of sound categories is affected by their phonetic context, a process known as co-articulation. For example, English fricatives have a lower spectral center of gravity directly following the vowel /u/ (as in *moon*) compared to the vowel /XXX/ (Soli, 1981; Yeni-Komshian & Soli, 1981). As the spectral center for /ʃ/ is generally lower than that of /s/ in English (Jongman, Wayland, & Wong, 2000), the presence of /u/ serves to make /s/ segments acoustically more similar to typical /ʃ/ segments. Human speech perception is known to compensate for this: for a fricative ambiguous between /s/ and /ʃ/, the presence of a preceding /u/ biases listeners towards /s/ responses (Mann & Repp, 1980; Mann & Soli, 1991). That is, listeners seem to attribute the lowered spectral center of gravity at least in part to the co-articulatory effect of the preceding /u/, rather than an intention to produce a /ʃ/ (Fowler, 2006). This relation between co-articulation in production and compensation in perception is schematically illustrated in Figure XXX. Similar compensation effects have now been documented for a wide range of phonetic contexts (for review, see REF), often under the alternative term normalization (e.g., REFS).

There is also evidence that compensation for co-articulation is not limited to acoustic context. For example, in an effect closely resembling that of preceding /u/, visually presence of lip-rounding preceding audio of an ambiguous /d-g/ blend biases listeners towards perceiving /g/ (Fowler, et al., 2000; Holt et al., 2005). Lip-rounding lowers the third formant (F3). In the absence of this visual context, lower F3 would be more likely to result from producing /d/ rather than /g/. Paralleling compensation for preceding /u/, listeners thus seem to compensate for the visual context of lip-rounding (see also Viswanathan & Stephens, 2016). Results like these have led to the hypothesis that compensation can occur regardless of the type and modality of contextual cues. As Fowler (2006, p. 166) put it: compensation for lip-rounding would be equally expected if a talker “was about to whistle a merry tune or about to kiss a loved one”, as “it does not matter why the lips were rounded; it only matters that they were rounded [prior to articulation of the /d/-/g/ sound] and, therefore, would lower the F3 of the syllable that the gesture overlapped with temporally” (Fowler, 2006).

To the best of our knowledge, it is an open question whether listeners indeed compensate for expected effect on articulation of visually evident non-linguistic causes, such as kissing a loved one, or having a pen in the mouth. A pen in the mouth has two visually evident effects on the articulators during speech. The first is to increase the opening of the jaw and size of oral cavity (as the pen prevents the mouth from closing), and the second is to force lip rounding around the protruding end of the pen. As the size of the oral cavity opening and amount of air constriction are inversely related for fricatives, forced mouth opening is expected to lower spectral center of gravity (McFarland & Baum, 1995). Lip rounding is also expected (in English) to lower the spectral center of gravity for surrounding fricatives. As lower spectral center of gravity is one of the primary cues distinguishing /ʃ/ from /s/ in English (Jongman, Wayland, & Wong, 2000), both of these effects are predicted to make fricatives produced with a pen in the mouth acoustically more ‘/ʃ/-like’. If listeners compensate for either or both of these effects of the pen on articulation, it should bias their perception towards /s/ (against /ʃ/).

Experiments 1a-c test this prediction. All three sub-experiments employ the exact same design and procedure but differ in the specific visual and acoustic stimuli they employ, as well as minor details of the post-experiment survey. Specific differences are described in the methods. Participants were presented audiovisual speech stimuli which formed six steps along a continuum from *ashi* to *asi.* Audio was dubbed onto video of a young female talker holding a pen. During the production of the critical /s/-/ʃ/ fricative, the talker either had the pen in her mouth (Figure 1, left) or rather in her hand outside of the mouth (Figure 1, right). We were interested in whether the presence of the pen—or its visually evident effects on the articulation of /s/ and /ʃ/—affects the interpretation of acoustic cues to the /s/-/ʃ/ contrast. Participants performed an identification (categorization) task, answering whether they thought the talker in the video said *ashi* or *asi*.



Figure 1 Stills of video components of two audio-visual items in Experiments 1a-c, illustrating the critical manipulation. Participants saw and heard audio-visually presented speech stimuli drawn from an acoustic asi to ashi continuum. During the production of the fricative, the talker either had the pen in the mouth (left) or in the hand (right).

The use of audiovisual stimuli comes with unique challenges. While our goal was to investigate how the presence of the pen affects the perception of the acoustic input, the use of audiovisual stimuli entails that participants also had access to visual cues to the /s/-/ʃ/ contrast, such as lip-rounding (Proctor, Shadle, & Iskarous, 2006). Speech perception is well-known to integrate acoustic and visual information to articulation, and identification responses are known to reflect this integration (McGurk & McDonald, 1976; see also Bejjanki et al., 2011; Franken et al., 2017; Lüttke et al., 2018). This raises questions about how the presence of visual cues to the articulation of /s/ or /ʃ/ in the video affect participants’ identification responses. One way to address this question would be to manipulate the video stimuli—either by holding them constant or by gradiently varying the visual cues to /s/ and /ʃ/, independent of the auditory cues. We decided against the second possibility primarily for reasons of feasibility. It is substantially more difficult to create ecologically valid visual manipulations than it is to create ecologically valid acoustic manipulations. The few previous studies that *have* gradiently manipulated visual cues to articulation have either employed animation, rather than real-life videos, or a single ‘ambiguous’ real-life video (e.g., Bejjanki et al., 2011; Kang, Johnson, & Finley, 2016). None of the studies that we are aware of have manipulated video while also modeling the visual consequences of perturbing articulation by a pen is in the mouth.

We thus decided to take an alternative approach. We created the video stimuli by extracting short segments from video recordings of the talker pronouncing words that contained *asi* or *ashi*-like sequences (e.g., *democracy*, which ends in a sound sequence highly similar to *asi*). This means that the audiovisual stimuli in Experiments 1a-c contain visual information that is expected to affect participants’ identification responses. For the test item derived from an original video recording of *democracy,* for example, we would expect responses to be biased towards *asi*. For a video extracted from a video recording of *machinery*, on the other hand, we would expect responses to be biased towards *ashi*. The design of Experiments 1a-c therefore fully crossed the visual /s/ or /ʃ/ bias of the original video clip with the synthesized acoustic *ashi–asi* continuum and the location of the pen. This resulted in a 2 (visual /s/- vs. /ʃ/-bias) x 6 (steps along acoustic /s/-/ʃ/ continuum) x 2 (pen in mouth vs. hand) design, with all conditions being manipulated within participants.

## Methods

Except for the use of audiovisual rather than audio-only stimuli and minor procedural changes reported below, Experiments 1a-c closely followed the norming experiments in Liu & Jaeger (2018).

***Participants.*** Following Liu and Jaeger (2018), participants were recruited from Amazon's crowdsourcing platform Mechanical Turk. Each experiment recruited 64 participants, balanced across two lists that counter-balanced nuisance variables described below. Participants took an average of 22.3 minutes to complete the experiment (SD = 17.3 minutes) and were remunerated $6.00/hour.

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 taken any of the other experiments in this study or in Liu and Jaeger (2018, 2019). Before the experiment could be accepted, participants had to confirm that they were (i) native speakers of US English (defined as XXX), (ii) in a quiet room without distractions, (ii) wearing over-the-ear headphones.

***Materials.*** To create the audiovisual stimuli, we combined audio and video recordings.

*Audio recordings.* The acoustic stimuli for all three experiments were selected from the same 31-step acoustic continuum from *ashi* to *asi* created by, and used in, Liu and Jaeger (2018). This continuum was created with FricativeMakerPro (McMurray, Rhone, & Galle, 2012) based on recordings of typical *ashi* and *asi* pronunciations by a female talker in her twenties—the same recordings elicited and employed in Kraljic et al. (2008). The audio recordings for the present study thus come from the same female talker used in previous research on perceptual recalibration (Kraljic & Samuel, 2005; Kraljic et al., 2008; Kraljic & Samuel, 2011, Liu & Jaeger, 2018). As in Liu & Jaeger (2018), we selected six steps along the 31-step continuum. These steps were selected with the goal to maximize the ability to detect effects of both the acoustic continuum and our other manipulations. The latter includes the location of the pen, but also the perceptual recalibration effects that we investigate in Experiments 2 and 3: as mentioned above, the secondary purpose of Experiments 1a-c was to norm the test phase for those perceptual recalibration experiments. To detect effects of the acoustic continuum, it is important for the test locations to span a sufficiently large range along the continuum. However, the power to detect other effects—including the location of the pen—is highest at test steps that elicit close to 50% *ashi* and 50% *asi* responses. Similar to experiments on perceptual recalibration, we thus aimed to select one step that, across all other manipulations, would yield approximated 25% *ashi* responses, four steps that would yield close to 50% *ashi*-responses, and one step that would yield 75% *ashi* responses (e.g., Kraljic et al., 2008 and later work).The six acoustic continuum steps selected differed between Experiments 1a-c, with the goal to increase power for this and subsequent experiments (for details, see SI:XXX).

*Video recordings.* The videos for the test stimuli were extracted from the exposure videos employed in the perceptual recalibration experiments in Liu and Jaeger (2018). These videos were recorded for Babel (2016) and generously provided by Dr. Molly Babel, as the original video stimuli from Kraljic et al. (2008; Kraljic & Samuel, 2011) are no longer available. The videos show a female talker of similar age as the one employed in the audio and video recordings of Kraljic et al. (2008; Kraljic & Samuel, 2011), providing a highly plausible match for the voice of the talker in audio recordings. Previous study has used this exact combination of the videos recorded by Babel and the audio recorded by Kraljic and found perceptual recalibration for audio-only test stimuli from the audio recorded by Kraljic (Liu & Jaeger, 2018).

The stimuli created by Babel and colleagues (Babel 2016) did not contain video recordings of the *ashi-asi* nonce-words, and the talker recorded by Babel and colleagues was no longer available (Molly Babel, p.c. July 17, 2020). For Experiment 1a, we thus identified twelve video recordings of exposure stimuli with the required sound sequence similar to *ashi* (e.g., *m[achi]nery*) or *asi* (e.g., *democr[acy]*) (for full list, see supplemental materials). Care was also taken to select recordings for which the relevant sequence matches the duration of the acoustic stimulus. We used the open-source video editing software Shotcut (shotcut.org) to extract the relevant video sequence out of the original recordings. Following the procedure used in previous work to create the exposure videos (Babel 2016), we added a fade-in and fade-out (each of 300 msecs) to the beginning and end of the new video segments. This resulted in videos of, on average, 1361 msecs duration (SD = 54 msecs).

Half of the twelve videos were extracted from video recordings of the talker pronouncing a word with an *asi* sequence (e.g., *leg[acy]*, henceforth visual /s/-bias). The other half were extracted from video recordings of the talker pronouncing a word with an *ashi* sequence (e.g., *gl[aci]er*, henceforth visual /ʃ/-bias). For each of those six videos, half showed the talker with the pen in the mouth and half showed the talker with a pen in the hand. The presence of a visual bias towards /s/ or /ʃ/ and the location of the pen were thus fully crossed between the twelve video items.

Experiments 1b and 1c employ eleven of these twelve videos. The twelfth video was replaced with a video in kind because, as described below, the results of Experiment 1a indicated a particularly strong visual bias for that video (see SI:XXX).

*Audio-visual stimuli.* The audio and video recordings were combined into audiovisual stimuli following the same procedure used in Liu and Jaeger (2018). Care was taken to ensure that the audio and video recordings aligned. We fully crossed the six steps along the acoustic continuum with each of the 12 video items, resulting in 72 audiovisual stimuli for each of the three experiments.

***Procedure.*** Our web-based procedure closely followed Liu and Jaeger (2018). The experiment consisted of (1) instructions, followed by (2) a test phase and (3) an exit survey.

*Instructions.* The first page of instructions informed participants “This HIT is a psychology experiment about how people understand speech. Your task will be to listen to words, and to press a button on the keyboard to tell us what you heard.” Participants were informed that "It is extremely important that you use over-the-ear headphones of good sound quality for this experiment. If your headphones cost less than $30, then it is likely that they do not fulfill our criteria.” Participants were informed of the duration of the experiment, payment, eligibility, then completed a sound check, and gave consent. Following all previous experiments in our lab, these steps were all available prior to accepting the experiment, but in order to start the experiment, participants had to accept the experiment.

*Test phase.* At the beginning of the test phase, participants were instructed:

*You will see and hear videos of a female speaker producing words. Your task is to decide whether the speaker is saying “asi” or “ashi”. We appreciate your attention to this task. Please answer as quickly and accurately as possible, without rushing. You may hear similar sounds several times. As a form of quality control, you may sometimes see a white dot in the video. If it occurs, it is easy to see. If you see a white dot, please press “B” instead of answering. Do not press “B” unless you see a white dot. This helps us distinguish you from a robot.*

The instructions about the catch trial were included for the sake of comparability with Experiments 3 and 4. None of the trials during the test phase actually contained a white dot. Participants then completed 72 trials of an 2AFC identification task. Participants could respond *asi* or *ashi* (via the X and M keys on their keyboard) only after the video had finished playing. Catch trial responses could be registered at any point during the video and caused the video to stop and the next trial to start. A progress bar indicated how many trials had been completed and how many remained, and the key binding was indicated at the top of the screen (see Figure 3). Key binding was counterbalanced across participants. This was the only nuisance variable, resulting in two between-participant lists. Each trial ended by the participant pressing M, X, or B (to indicate a catch trial). Both the response and the response time were recorded.

A child holding a pen

Description automatically generated with low confidence

Figure 2. Screenshot of 2AFC identification trial during Experiments 1a-c, identical to test phase of Experiments 2 and 3. The progress bar (experiment was roughly 30% completed at time of screenshot) and key bindings were visible throughout the experiment.

The order of test stimuli was determined separately for each participant through constrained randomization that grouped stimuli into blocks and then randomized the order within and across blocks (Kraljic et al., 2008; Liu & Jaeger, 2018). Specifically, the 72 audiovisual stimuli were grouped into six blocks of 12 stimuli so that each of the 12 video items occurred exactly once within each block. Each block further fully crossed the two pen locations (pen in hand vs. mouth) with the two visual bias conditions (/s/ vs. /ʃ/), resulting in 3 video items each for each of these four conditions. Each block of 12 stimuli further consisted of two instances each of each of the six audio conditions (steps along the *asi – ashi* continuum). One of these two instances occurred with the pen in the mouth and one occurred with the pen in the hand. Across the six blocks all 72 combinations of the 12 video items and the six audio conditions occurred exactly once. The order of the 12 test stimuli within each block was fully random.

*Exit survey.* The survey for Experiment 1a was identical to that of Liu and Jaeger (2018). All questions are listed in the SI (XXX). Questions assessed the quality of the audio equipment and whether participants experienced stalling of audio or video (to help us catch code problems). The survey also contained a catch question, asking about the gender of the talker shown during the test phase. In Experiments 1b and 1c, we made minor changes to the wording of the exit survey, and removed some questions that had been found to be uninformative (for details, see SI XXX).

Following the exit survey, a final survey collected demographic information using the gender, age, race, and ethnicity categories required for NIH reporting. All responses in the demographic survey were indicated as optional.

***Exclusions.*** We removed participants who (1) experienced technical difficulties or did not complete the experiment, (2) reported to not have used headphones, or (3) did not answer the catch question about the talker's gender correctly. Or (4) has unusually fast or slow reaction times (XXX). We also excluded participants who (5) had swapped the response keys, as determined by their responses. For this purpose, we considered participants with significant slopes in the opposite of the expected direction as likely having swapped the response keys[[1]](#footnote-2) Table 1 summarizes the exclusions for all experiments. After participant exclusions, 58 trials (0.5%) were missing observations due to (incorrect) catch trial responses, leaving for analysis 12,608 observations from 179 participants across the three experiments.

Table Exclusions for all experiments reported. Total exclusions can be less than the sum of all individual exclusion criteria since some participants failed multiple criteria. The SI (XXX) contains additional visualizations, showing exclusions relative to the mean and variability of participants’ reaction times.

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| **Experiment** | **1a** | **1b** | **1c** | **2** | **3** |
| *Recruited* | *64* | *64* | *64* | *64* | *257* |
| Technical difficulty | - | - | 2 (3.1%) | - | - |
| Ignored instructions | - | 3 (4.7%) | 1 (1.6%) | 1 (1.6%) | 4 (1.6%) |
| Lexical decision accuracy | NA | NA | NA | NA | 1 (0.4%) |
| Catch question/trials | 1 (1.6%) | 2 (3.1%) | 1 (1.6%) | 1 (1.6%) | 8 (3.1%) |
| Swapped keys | - | 1 (1.6%) | 1 (1.6%) | 2 (3.1%) | 2 (0.8%) |
| Outlier RT | 3 (4.7%) | - | - | 2 (3.1%) | 3 (1.2%) |
| Missing trials | - | - | - | - | - |
| *Total* | *4 (6.2%)* | *6 (9.4%)* | *(7.8%)* | *6 (9.4%)* | *18 (7.0%)* |

## Results

Experiments 1a-c served two purposes, the first being theoretical—to test whether listeners compensate for the (visually evident) effects on articulation of a pen in the mouth—and the second methodological—to identify audiovisual test tokens suitable for the remaining experiments, i.e., stimuli that are centered around 50% *ashi* responses, ranging from about 25-75% *ashi* responses. .

***Analysis approach.*** We use Bayesian generalized linear mixed models with a Bernoulli (logit) link—mixed-effects logistic regression—for the analysis of identification responses (for an introduction to mixed-effects logistic regression, see Jaeger, 2008).

Responses (1 = *ashi* vs. 0 = *asi*) were regressed against pen location (effect coded: .5 = in mouth vs. -.5 = in hand), visual bias (effect coded: .5 = /ʃ/-bias vs. -.5 = /s/-bias), acoustic continuum, and test block as well as all their interactions. The six continuum steps and the six test blocks were codes as monotonically ordered categorical predictors (REF-Buerkner & Charpentier). This avoids the linearity assumption made in most previous analyses of perceptual recalibration experiments, allowing changes across continuum steps or from test block to test block have non-linear effects, while still constraining effects to be monotonic.[[2]](#footnote-3) We included test block and its interactions primarily as baseline for Experiments XXX-XXX. These experiments assess changes in categorization responses after exposure (“perceptual recalibration”), and there is now evidence that those effects decrease with continued testing over a uniform acoustic continuum Liu & Jaeger, 2018, 2019; Tzeng, Nygaard, & Theodore, 2021; Zhang & Samuel, 2023; Hodges, Cummings, & Theodore, in prep.).

All analyses further contained the full random effect structure for the three design variables pen location, visual bias, and acoustic continuum (by-participant intercepts and slopes for all population-level predictors). No random slopes for test block were included since our studies were not designed to test this nuisance effect, leading to convergence problems for some experiments.

We followed recommended practice and use weakly regularizing priors to facilitate model convergence, specifically the exact same as in our previous work to reduce researchers' degrees of freedom (e.g., Hörberg & Jaeger, 2021; Xie, Liu, & Jaeger, 2021). For fixed effect parameters, we used Student priors centered around zero with a scale of 2.5 units (following Gelman et al., 2008) and 3 degrees of freedom. For the monotonic predictors, we used a Dirichlet prior with the default = 1. For random effect standard deviations, we used a Cauchy prior with location 0 and scale 2, and for random effect correlations, we used an uninformative LKJ-Correlation prior with its only parameter set to 1 (Lewandowski et al., 2009), describing a uniform prior over correlation matrices. Model diagnostic indicated convergence (e.g., all ). All analyses were fit using the library *brms* (Bürkner, 2017) in R version 4.3 (REF).

***Hypothesis tests.*** The SI lists the full model summary for all analyses. In the main text, we present Bayesian hypothesis tests over the fitted GLMMs for the questions of interest. Additionally, we report whenever the bidirectional 95% credible interval for any other effects does not contain 0. This was not the case for any effects in Experiments 1a-c. Table 1 summarizes those tests for all three experiments.[[3]](#footnote-4) Figure 4 shows participants responses depending on the pen location, acoustic continuum and visual bias.



Figure 4 Summary of participants’ responses in Experiments 1a-c, depending on pen location and acoustic continuum step (Panel A) or visual bias (Panel B). Points show means of by-participant averages. Intervals show bootstrapped 95% CIs over those by-participant means.

[INSERT TABLE]

Table 1 Summary of hypothesis tests based on GLMM analyses for Experiments 1a-c. Hypotheses for which we had not strong expectations are shown with shaded backgrounds.

Of primary interest, participants in all three experiments were less likely to respond *ashi* if the pen was in the mouth (BFs > 19.6), as predicted by the compensation hypothesis. There also was evidence that this effect increased for stimuli that were acoustically or visually more *ashi*-like. This evidence was strongest for Experiment 1c (BFs > 22.7), potentially because the effect of compensation—decreases in the probability of *ashi* responses—is more difficult to detect for audio-visual stimuli for which *ashi*-responses are unlikely to being with. The same trend was, however, present across all three experiments.

Beyond the effect of primary interest, all three experiments exhibited the expected effects of the acoustic continuum (BFs > 3999) and visual bias (BFs > 18.2), with increasing probabilities of *ashi* responses when the audio-visual articulatory evidence biased towards *ashi*. These two effects seem to be independent of each other, suggesting additive effects of acoustic and visual evidence (BFs > 8, in line with models of ideal cue integration, REF). Finally, all three experiments suggest that the effects of pen location, visual bias, and the acoustic continuum were rather stable across blocks (BFs > 17).

## Discussion

Experiment 1a-c tested whether presence of a pen in a talker’s mouth affects listeners’ perception of an audiovisual /s/-/ʃ/ continuum. All three experiments find this to be the case. Specifically, listeners were more likely to categorize an audiovisual input as *ashi* when the talker in the video had a pen in the mouth, compared to when the talker held the pen in the hand. This effect was larger when for tokens that were acoustically or visually more *ashi*-like, closely resembling findings for compensation for preceding phonetic context (Kang et al., 2016).

These results are unexpected if listeners simply integrated visual and acoustic evidence of articulation, without discounting the *causes* for that evidence. The presence of a pen is expected to increase lip rounding and oral cavity opening. Either of these would result in lower center of gravity (similar to the effects of a bite-block, McFarland & Baum, 1995; Baum et al. 1996), making a sound acoustically more /ʃ/-like. If listeners naively integrated this visual evidence with the acoustic evidence, listeners should be *more* likely to respond *ashi* when the pen is in the mouth—the opposite of what we observed in all three experiments. Similarly, if listeners ignored the pen, or if the effects of the pen on articulation, were not sufficiently visually evident, we should have failed to find *any* effect of pen location. This was not the case. Instead, the results of Experiments 1a-c are predicted by the hypothesis that listeners expect and ‘explain away’ the effect of the pen, paralleling compensation effects previously documented for surrounding phonetic context (REF).

One alternative explanation would be that the pen partially or completely obscures some of the visual cues to /ʃ/—i.e., rather than causingmore lip rounding or a more open oral cavity, the pen might obscure the presence of lip rounding and cause the oral cavity to be more closed. This would explain the observed direction of the effect of pen location. This alternative explanation is addressed in Experiments 2.

Before we turn to those experiments, we note that Experiments 1a-c also validate the audiovisual test tokens we developed. We found clear effects of both the acoustic continuum and visual bias (due to the /s/ or /ʃ/ sound the talker produced in the original videos we excerpted the test stimuli from). The slope of the acoustic effects is comparable across both experiments ( in Experiment 1a-c vs. in LJ18),[[4]](#footnote-5) and responses for the audio-only test token fell approximately half-way between the two visual bias conditions of Experiments 1a-c, as would be expected given that acoustic and visual evidence to /s/-/ʃ/ had additive effects in Experiments 1a-c

# Experiment 2

The materials and procedure of Experiment 2 were identical to Experiment 1c, except that the talker’s mouth was occluded by a black rectangle during the production of the /s/-/ʃ/ fricative (see Figure 6). The rectangle was absent at the start and end of the video, appearing XXX msecs before the start of the fricative and disappearing XXX msecs after the end of the fricative. This left it rather clear *that* the pen was in the mouth during the production of the fricative, while occluding most direct evidence of the effect of the pen on the specific state of the articulators (lip rounding, oral cavity opening) during the production of the fricative.

A picture containing screenshot, person, design

Description automatically generated

Figure 6 Schematic of an audio-visual item in Experiment 2, during the production of the fricative. As in Experiments 1a-c, the talker either had the pen in the mouth (left) or in the hand (right) during the production of the fricative. Unlike in Experiments 1a-c, a black box occluded the talker’s mouth during the production of the fricative.

This manipulation served two purposes. First, by assessing the effect of pen location in Experiment 2, we can test whether the presence of a pen in the mouth was sufficient to cause the effect observed in Experiments 1a-c or whether listeners need to have more direct evidence of the *articulatory consequences* of the pen in the mouth. For example, if listeners only compensate if they observe that the pen indeed causes more lip rounding or larger opening of the oral cavity, then we expect the effect of the pen—replicated three times in Experiments 1a-c—to be no longer observed in Experiment 2. This latter possibility strikes us to be what compensation accounts like that advanced by Fowler would predict since “it does not matter why the lips were rounded; it only matters that they were rounded” (Fowler, 2006, p. 166).

Second, Experiment 2 allows us to test whether the decreased rate of *ashi* responses when the pen was in the mouth in Experiments 1a-c was due to visual obstruction of articulatory evidence, rather than compensation. Under this alternative hypothesis, both pen conditions (pen in mouth vs. hand) of Experiment 2 should yield rates of *ashi* responses comparable to the pen-in-the-mouth condition in Experiment 1c (since Experiment 2 occludes most direct visual evidence of fricative articulation). Thus, Experiment 2 aimed to distinguish three hypotheses, two of which are elaborations of the compensation hypothesis: (1a) that listeners compensate for the visually evident presence of a cause that is known to affect the production of the fricative (pen in the mouth), (1b) that listeners compensate based on the visually evident state of the articulators just prior to the fricative, (2) that the effects of Experiments 1a-c were due to visual occlusion of articulatory cues, rather than compensation.

## Methods

***Participants.*** We again recruited 64 participants, using the same approach, payment, etc. as in Experiment 1c. Participants took an average of 24.4 minutes to complete the experiment (SD = 20.0 minutes).

***Materials.*** All materials were the same as in Experiment 1c, except for the addition of a black rectangle to the video, as described above. The black rectangle was positioned such that vertically, the area from the bottom of the talker's nose to the bottom of her chin were blocked from view. Horizontally, the entire width of the face was occluded. This was intended to occlude visually specified articulation, including lip rounding, mouth aperture, and tongue position. In cases where the talker moved during production, the size of the rectangle was increased such that the above criterion always applied. This gave rise slightly different dimensions between different video frames. The occluder appear during the video frame after the talker's maximum mouth aperture for the preceding vowel. The occluder disappeared at word offset. The vowel after the fricative was therefore also visually occluded. This window was intended to balance the competing constraints of giving subjects maximum opportunity to see the pen in the talker's mouth, while blocking the entirety of the fricative segment.

***Procedure.*** The procedure was identical to Experiment 1c, with the exception that the phrase “with a black box occluding the speaker's mouth” was added to instructions where relevant.

***Exclusions.*** We applied the same exclusion criteria as in Experiments 1a-c, removing four participants (6.2%; see Table 1). After participant exclusions, 11 trials (0.3%) were missing observations due to (incorrect) catch trial responses, leaving for analysis 4214 observations from 60 participants.

## Results

We used the exact same analysis approach as in Experiments 1a-c.The SI lists the full model summary for all analyses. Table 1 summarizes the hypothesis tests, Figure 4 shows participants’ responses with those from Experiment 1c shown in the background for comparison. In contrast to Experiments 1a-c, we found no notable evidence for an effect of pen location (BF = 1.1), though the effect still went in the same direction as in Experiments 1a-c. Similarly, the effect of visual bias was also substantially reduced, though still present in the same direction as in Experiments 1a-c (BF = 2.2). Participants continued to be strongly affected by the acoustic continuum (BF > 4000), the effect of which was similar in magnitude () to Experiments 1a-c ().



Figure 4 Summary of participants’ responses in Experiments 2, depending on pen location and acoustic continuum step (Panel A) or visual bias (Panel B). For comparison, the results from Experiment 1c are shown in the background. The two experiments were identical except for the presence of the black rectangle during the production of the fricative in Experiment 2.

[INSERT TABLE]

Table 1 Summary of hypothesis tests based on GLMM analyses for Experiment 2. Hypotheses for which we had not strong expectations are shown with shaded backgrounds.

## Discussion

These results suggest that participants in Experiment 2 paid attention to the stimuli, and yet failed to exhibit any effects of pen location. In the SI (XXX), we report Auxiliary Experiment 2b with N = 64 participants. This experiment was identical to Experiment 2, except that participants had to *additionally* press the SPACE bar whenever the pen was in the talker’s mouth. This was intended to (and did successfully) direct participants’ attention towards the location of the pen. Experiment 2b replicated all effects of Experiment 2—including the substantially reduced effect of pen location. These findings suggests that a pen in the mouth of the talker is *not* sufficient to elicit the effect observed in Experiments 1a-c.

The comparison of Experiment 2 against Experiment 1c in Figure 4A further suggests that the effects of pen location in Experiments 1a-c are unlikely to be exclusively due to the pen obstructing visual cues to /s/-/ʃ/: at least for the two most *ashi*-like audio stimuli (for which the effect of pen location was strongest in Experiments 1a-c), responses in Experiment 2 seem to group with the pen-in-hand (no occlusion), rather than pen-in-mouth, condition in Experiment 1c. There is, however, also some evidence that visual occlusion might explain *part* of the effects in Experiments 1a-c. For the two steps in the middle of the acoustic continuum, responses in Experiment 2 fall half-way between the pen-in-hand and pen-in-mouth conditions of Experiment 1c (for the remaining two steps, the effect of pen location was too small even in Experiment 1c to draw meaningful conclusions about Experiment 2).

CONTINUE HERE WITH REGRESSION RESULTS OF COMPARISON ACROSS 2 vs. 1c. THIS LOOKS POTENTIALLY QUITE COMPATIBLE WITH SIMPLE OBSTRUCTION OF CUE, EXCEPT FOR THE RIGHT END OF THE CONTINUUM.

We thus tentatively conclude that at least some of the results in Experiments 1a-c are due to compensation for visually evident effects of the pen on the configuration of articulators that are relevant to the /s/-/ʃ/ contrast. This suggests that listeners can normalize or ‘explain away’ some effects of the pen (if they are sufficiently visually evident). Equipped with this insight, we turn to the second question we seek to address: how is listeners’ adaptation to unexpected pronunciations affected by the presence of an incidental (visually evident) cause for those pronunciations?

# Experiment 3

As outlined in the introduction, previous work has proposed a number of competing hypotheses. XXX

The perceptual recalibration effect is generally robust and does not seem to require much or any conscious attention; subjects adapt even given an environment full of distractions, other cognitive load-bearers, or explicit instruction that the speaker or tokens are unreliable or altered (Drouin & Theodore, 2018; Liu & Jaeger, 2019). As few as 4 shifted tokens has been found to elicit significant shifts in categorization responses (Cummings & Theodore, 2023; Liu & Jaeger, 2019).

However, despite the effect’s rapid and robust nature, it does not appear indiscriminate or unable to be blocked. Kraljic, Samuel, & Brennan (2008, Experiment 1b) exposed listeners to 10 shifted tokens which co-occurred with video including a pen in the mouth of the talker. These were followed by 10 unshifted tokens co-occurring with video of the talker without the pen in her mouth. This second section of exposure served to allow listeners to infer that the shift in production was in fact resultant of the pen; productions became normal as soon as the pen was removed. In subsequent audio-only test, participant groups between biasing conditions did not show a shifted /s/-/ʃ/ categorizations from one another. This is taken as support that the presence of a plausible alternative explanation for an altered pronunciation blocks perceptual recalibration. Specifically, the authors posit that “The system integrates available cues about whether a variation is characteristic of the speaker who is producing it or an incidental consequence of some other factor. If the variation seems characteristic, the appropriate phonemic representation is restructured to accommodate it; if the variation seems incidental, no such restructuring occurs.” (Kraljic, Samuel, & Brennan, 2008).

The results and interpretation of Kraljic, Samuel, & Brennan (2008) leave open at least two alternatives. The most succinct is perhaps a framework wherein all audio input that co-occurs with atypical visual input (such as a pen in the mouth or any other incidental cause) is discarded entirely by the listener. This postulates that perceptual adaptation is driven only by prior perception in typical contexts and should only be seen experimentally when exposure includes these contexts. This hypothesis parsimoniously explains the lack of learning but is unlikely given the finding of the current Experiment 1 that visual evidence of incidental causes is accounted for in perception. An alternative is that phonetic restructuring may be specific to the environment in which it occurs, a hypothesis furthered below.

In a follow-up study, Kraljic & Samuel (2011, Experiment 2) found that learning is preserved when shifted pronunciations continue in the second section of exposure (wherein the pen is no longer in the talker’s mouth). This remained the case when visual evidence was entirely absent from the second section of exposure, which importantly suggests an assumption by listeners that speech evidence in absence of video is produced without an incidental cause. Learning was also found when a talker’s speech was atypical only during the second section of exposure, but normal when the pen was in the talker’s mouth (Kraljic & Samuel, 2011, Experiment 3). These findings are taken as evidence that the effect of the pen on learning (or lack thereof) follows from the way humans store and retrieve previously experienced speech input. Specifically, it’s well documented that speech input is stored along with the context in which it occurs, and that memory traces of speech ‘exemplars’ or ‘episodes’ are used to categorize subsequent input (Johnson, 1997; Pierrehumbert, 2002; Kleinschmidt & Jaeger, 2015). Under this account, perceptual adaptation is driven by the linked storage of auditory and visual (typical or otherwise) input. Retrieval of these memory traces should result in perception of subsequent auditory input being colored by previous experience within the same visual context.

Liu & Jaeger (2018, Experiment 1b) provides the first study wherein causal ambiguity between the talker and the pen was not disambiguated during exposure. Shifted sounds co-occurred with video of the talker with the pen in her mouth for the entirety of exposure. In subsequent audio-only test, learning was found to be blocked only for the “sh”-labelled group (those hearing words ambiguous between, e.g., *publisher* and *publiser*). This provides the first evidence for an effect of compensation in exposure towards learning exhibited in test. Liu & Jaeger (2018) identify that this asymmetry “could be because a pen might plausibly disrupt lip-rounding, which is involved in the articulation of /ʃ/ but not /s/. This would mean that listeners have some degree of sensitivity to the articulatory gestures used in the production of specific sounds”.

These studies collectively, while providing a wealth of intriguing findings, are all crucially limited by their audio-only testing phases. If learning is indeed linked to retrieval of previously stored audio-visual exemplars, then the modality shift from visually informed exposure to non-visually informed test is expected to require generalization and constrain learning. Robust support of context-specific learning cannot be found when context changes between the phase of the experiment designed to elicit learning and the phase which measures it. Finally, a crucial test of context-specific learning is to determine whether shifts encountered in context of the pen in the mouth, which have been found not to elicit adaptation in audio-only context, may still elicit adaptation in subsequent matching context (being, test to percepts including the pen in the mouth).

Experiment 2 of the present study therefore seeks to replicate Experiment 1b of Liu & Jaeger (2018), with the addition of visual information in test.

Experiment 2

Having established that the visual presence of an incidental cause affects listeners’ perception of an unfamiliar talker’s speech, prior to any informative exposure to the talker, our second goal was to examine how this alteration in perception manifests itself in adaptation. To this end, we ran three exposure-test Experiments (2a-c). These experiments employ the exact same design and procedure but differ in the specific visual and acoustic stimuli they employ, as well as minor details of the post-experiment survey, and one significant shift in the pre-experiment practice phase. Specific differences are described in the methods.

Participants first underwent an exposure phase, wherein they were presented audiovisual speech stimuli of both English words and nonwords. A lexical decision task was used to ensure attention. Crucially, words typically containing either /s/ or /ʃ/ (split between-participant groups) were shifted in this phase to contain instead an ambiguous sound between /s/ and /ʃ/. During production of the critical /s/-/ʃ/ fricative, the talker either had the pen in the mouth (Figure 1, Panel A) or in the hand (Panel B), again split between-participant groups. This exposure phase was paradigmatically identical to that of Liu & Jaeger (2018; Experiment 1). All subjects then underwent a test phase identical to that of Experiment 1c.

## Methods

Except for the use of audiovisual rather than audio-only test stimuli and minor procedural changes reported below, Experiment 3 closely followed Experiment 1a in Liu & Jaeger (2018).

***Participants.*** We recruited 257 participants over the crowdsourcing platform Prolific. Participants took an average of XXX.X minutes to complete the experiment (SD = XXX minutes) and were remunerated $XXX.00/hour.

We used Prolific's pre-screening to limit the experiment to participants (1) of US nationality, (2) who reported to be English speaking monolinguals, and (3) had not previously participated in any experiment from our lab on Prolific. Before the experiment could be accepted, participants had to confirm—as in all previous experiments—that they were (i) native speakers of US English, (ii) in a quiet room without distractions, (ii) wearing over-the-ear headphones. An additional XXX participants loaded the experiment but did not start or complete it.

***Materials.*** The audiovisual materials for the exposure phase were identical to those of Liu & Jaeger (2018, Exp. 1a). However, unlike LJ18, our test phase also employed audiovisual materials—specifically, the same stimuli as in Experiment 1c.

The exposure materials consisted of …[DESCRIBE HERE]]

Although not reported or analyzed in Liu and Jaeger (2018), the experiment employed catch trials during exposure. The materials for those catch trials were identical to filler word and non-word videos with one exception. While the video was playing, a white dot appeared roughly in the center of the video, close to the talker's mouth—the region of the video in which the pen appeared when it was in the talker’s mouth. Because of changes we made to the list generation, we use 10 of the 12 catch videos developed in LJ18. Half of the catch videos contained filler words recordings and half contained filler non-word recordings. The pen was in the talker’s mouth on half of the catch trial videos, and in the talker’s hand on the other half.

***Procedure.*** Our web-based procedure closely followed, and largely looked identical, to Experiment 1a in LJ18. The experiment consisted of (1) instructions, followed by (2) a practice phase, (3) exposure phase, (4) a test phase, and (5) an exit survey.

*Instructions.* The instructions were identical to LJ18. Participants were informed that [DESCRIBE HERE] Participants were also informed that the experiment contained catch trials that were intended to distinguish human participants from robots.

*Practice.* Participants started with six practice trials that exposed them to the lexical decision task for the exposure phase, except that participants received feedback during practice. Participants were instructed to press “X” or “M” to indicate whether the recording they heard was a word or not. Key bindings—which key corresponded to a word response and which corresponded to a non-word response—were balanced across participants, and held constant throughout the practice and exposure phase. [DESCRIBE TRIAL COMPOSITION HERE]One of the two videos during the practice phase was a catch trial and thus contained a white dot. Participants were instructed to press “B” when they noticed the white dot (instead of “X” or “M”).

*Exposure*. We used 100 exposure trials, consisting of 10 words with typical fricatives, 10 words with shifted fricatives, 30 filler words, and 50 filler non-words. This is the same proportion of typical, shifted, and filler trials used in other previous studies (Kraljic et al., 2008; Kraljic & Samuel, 2011; Liu & Jaeger, 2018, 2019). Unbeknownst to participants, exposure was divided into ten blocks, each consisting of one word with a typical fricative (/s/ or /sh/ depending on the exposure condition), one word with shifted fricative (/sh/ or /s/ depending on the exposure condition), three filler words, and five filler non-words. Catch trials were evenly distributed across the ten blocks, with one catch trial per block. For each block, a random coin flip determined whether the catch trial was on a filler word or non-word, and whether the pen was in the talker’s mouth or hand.

The assignment of the specific videos to each block—for example, which of the 10 words with a typical fricative was assigned to the first block—was randomized for each participant. The order of the 10 stimuli in each block was also randomized under the constraint that two typical or two shifted words could never occur in adjacent list positions. The exposure list was obtained by concatenating the ten blocks, resulting in the 100 exposure trials.

*Test.* The task and list generation during the test phase of Experiment 3 was identical to Experiment 1c.

Exit *survey.* The survey was identical to that of Experiment 1c.

***Exclusions.*** We applied all exclusion criteria from Experiments 1a-c. Additionally, we excluded participants with a less than 85**%** lexical decision accuracy in exposure (calculated over all XXX non-catch trials without shifts). Following LJ18, we OTHER EXCLUSIONS This removed XXX participants (XXX.X%; see Table 1). After participant exclusions, XXX test trials (XXX.X%) were missing observations due to (incorrect) catch trial responses, leaving for analysis XXX observations from XXX participants.

## Results

We used the exact same analysis approach as in Experiments 1-2.The SI lists the full model summary for all analyses. Table 1 summarizes the hypothesis tests, Figure 4 shows participants’ responses.

* (equally) robust learning in all conditions

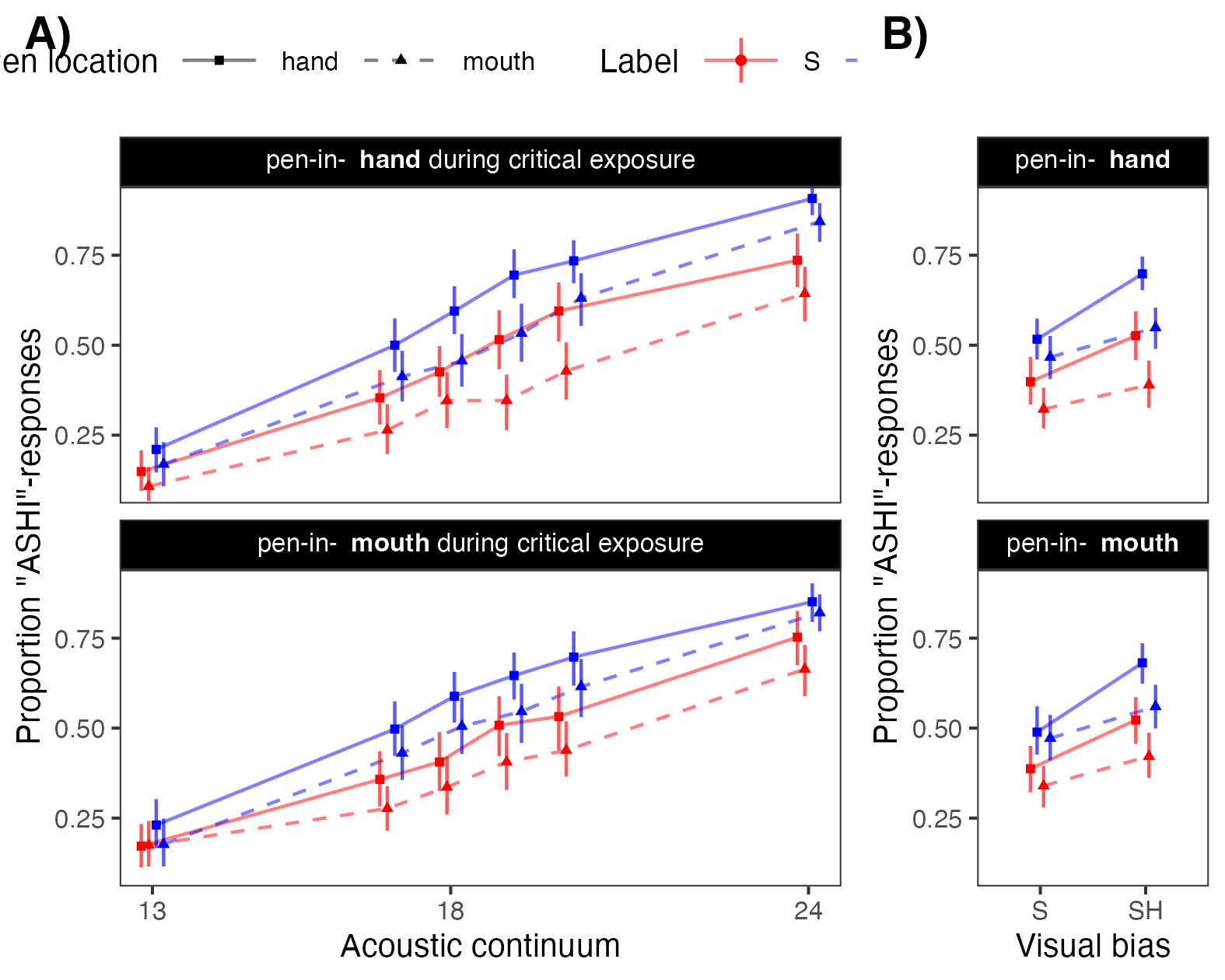


Figure 4 Summary of participants’ responses in Experiments 3, depending on pen location and acoustic continuum step (Panel A) or visual bias (Panel B). Points show means of by-participant averages. Intervals show bootstrapped 95% CIs over those by-participant means.

[INSERT TABLE]

Table 1 Summary of hypothesis tests based on GLMM analyses for Experiment 3. Hypotheses for which we had not strong expectations are shown with shaded backgrounds.

## Discussion

At first blush, robust learning in all conditions seems counter to theories of context-specific learning. Important to consider here, however, is the overall contextual similarity between exposure and test for the current study. Prior work has established that evidence of shift provided in visual pen-in-mouth episodes does not seem to generalize to audio only test. Two crucial shifts occur herein: causality and modality. Recall the 2nd experiment of Kraljic & Samuel, 2011, establishing that audio-only tokens are assumed to be characteristic of the talker, given no evidence to the contrary. Visual evidence without incidental cause, in contrast, is able to generalize to audio only test, as causality is held constant through the change in modality. The conditions of the current Experiment 2 always maintain modality across exposure and test, and therefore require generalization across at most the single dimension of causality. Recall additionally, via the results of Liu & Jaeger (2018, 1b), that unresolved causal ambiguity only blocked one of two learning conditions. This provides plausibility to at least some generalization across cases where causality is ambiguous. Experiment 2, wherein modality never changes and causality is never strongly disambiguated in either direction, appears to meet criterion for generalization of learning in all four conditions.

This explanation, reliant on at least partial generalization across ambiguous contexts, makes a strong prediction for a case wherein modality is held constant and causality is disambiguated in exposure. Specifically, while some amount of learning may occur across conditions, more learning is expected in cases where the disambiguated causality of exposure matches the presented context of test. Experiment 3 examines this hypothesis via an audiovisual replication of Kralic, Samuel, & Brennan (2008, Experiment 1b). Subjects first experience shifted tokens in context of a pen in the mouth, then have causality disambiguated through a second section of exposure with unshifted tokens and the pen no longer in the mouth. Test is then provided to both pen in mouth and pen in hand percepts, with context-specific learning predicting more evidence of adaptation present for pen in mouth test.

# Experiment 4

methods:

results:

Discussion:

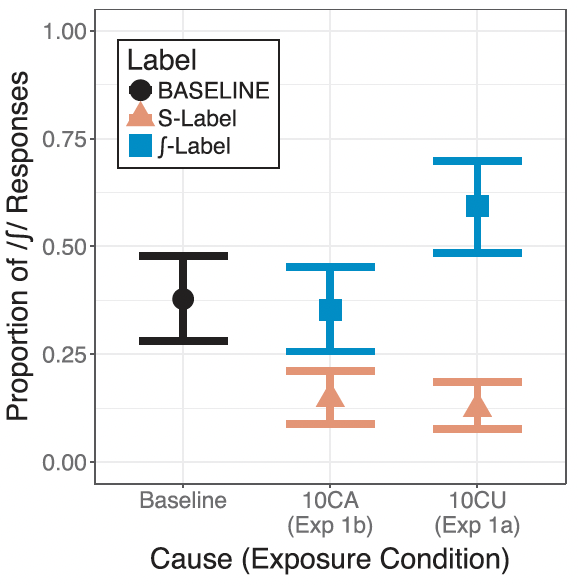
HYPOTHESIS TREE:

1. PIM percepts ignored:

* Really unlikely based on LJ18 and our E1 & E3

1. Pre-categorization normalization (Causal CfC) drives the effects; PIM has no effect on adaptation but just on perception:

* Works for E1-2
* E3:
  + Exposure Predictionsː Causal compensation predicts all PIM tokens to behave as if they are higher spectral CoG than they really are. In s-bias (red) conditions, this could inhibit adaptation; in sh-bias (blue) conditions, it is expected either to have no effect or to increase adaptation. The ‘double’ shift of the acoustic shift and compensation being directionally the same in sh-bias conditions could potentially move those tokens out of the ‘goldilocks’ zone (Babel et al. 2019), but this is a priori unlikely and doesn’t pattern out in the data. Since the effect of the pen is handled through in the moment compensation, and no evidence is provided that e.g. the talker *only* exhibits a shift while PIM, causal compensation is not expected to fully block adaptation.
  + NOTEː Causal compensation predicts that s-bias conditions should not induce adaptation when pen is in moth (as there isn’t a difference between expectations and results, while ʃ-bias conditions should continue to induce adaptation. This seems to be the opposite of what Liu & Jaeger (2018; Exp 1) found... (LJ18, Fig 6), though the pattern obtained by LJ18 might be due to a floor effect for the s-bias condition (in which case, it’s impossible to tell for which bias condition adaptation was blocked more strongly when the pen was in the mouth):



* + Test Predictionsː As in Experiment 1a-c, PIM tokens should be lower in /ʃ/ responses than their PIH counterparts. Learning (as a difference from baseline/exp1) should be exhibited by the ʃ-groups more so than the s-groups.
  + Resultsː Overall patterning is that all conditions learn about the same amount. For PIH test, s- vs ʃ-bias conditions seem to move about the same distance from baseline, rather than the s-bias staying put (as most predicted by the Causal Compensation hypothesis), but neither condition significantly differs from baseline. For PIM test, the s-bias condition does seem visually to align more with the PIM test in Experiment 1, but again no differences from baseline are significant. Overall, no glaring issues with a causal compensation hypothesis!
* E4:
  + First Exposure predictionsː Same as Experiment 3; learning in sh-bias but not s-bias groups.
  + Second Exposure predictionsː Causality is disambiguated! For sh-bias group, perhaps ‘unlearning’ is predicted, but perhaps not (see Cummings & Theodore, 2022; consistency of input doesn’t seem to matter as much as quantity). For s-bias group, causal compensation gains support (as it predicts the pattern of Amb-PIM to clear-PIH). IF uncertainty maintenance were at play to begin with (and perhaps listeners were only compensating for a portion of lip rounding’s expected effect), then perhaps this support will strengthen the effect.
  + Test predictionsː If causal compensation becomes more robust (resultant of its confirmation via exposure 2), then the s-bias condition in PIM test is expected to have even fewer /ʃ/ responses than experiment 2.
  + Resultsː Both conditions learn, but the PIM condition exhibits significantly *more* learning (driven by the s-bias condition). While the paragraphs above explain how Causal Compensation *could* predict this pattern, it’s admittedly tenuous and HARK-ey...

1. Pre-categorization happens during perception, but percepts are ADDITIONALLY stored along with their context:
   1. Passive context-based storage/retrieval
   2. More sophisticated causal reasoning

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**Author contributions:** SNC proposed the experiment. SNC, GK, and MY designed the experiment and developed the hypotheses, with input from TFJ. SNC created the audiovisual stimuli from the source audio and video files. TFJ and GK programmed the web-based experiments. GC created a webpage with links to all experiments for demonstration purposes. MY and TFJ conducted data visualization and organization, with input from SNC. TFJ conducted the statistical analyses. All authors jointly interpreted the results. SNC and TFJ wrote paper, with input from all authors.

# Appendices

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1. This differs somewhat from the approach taken in Liu and Jaeger (2018), which did not require *significant* reversal of the slope for participant exclusion. We consider the approach taken here more conservative, and decided on this change prior to analysis. [↑](#footnote-ref-2)
2. For the present experiments, the analyses of all experiments strongly supported linear effects of acoustic continuum and non-linear effects of test blocks. The results we report below replicate when standard linear effects used for continuum and block. [↑](#footnote-ref-3)
3. Here and for all other experiments, all ‘main’ effects were assessed in the center of the acoustic continuum for the first test block (paralleling Liu & Jaeger, 2018, 2019). [↑](#footnote-ref-4)
4. The LJ18 data is available on OSF XXX. We downloaded it and fit the same analysis to it as for Experiments 1a-c (except that pen location, visual bias, and their interactions were not manipulated in the audio-only LJ18 experiments and thus not included in the analysis. [↑](#footnote-ref-5)