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No lexical–prelexical feedback during speech perception or: Is it time to stop playing those Christmas tapes?

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ABSTRACT

The strongest support for feedback in speech perception comes from evidence of apparent lexical influence on prelexical fricative-stop compensation for coarticulation. Lexical knowledge (e.g., that the ambiguous final fricative of *Christma?* should be [s]) apparently influences perception of following stops. We argue that all such previous demonstrations can be explained without invoking lexical feedback. In particular, we show that one demonstration [Magnuson, J. S., McMurray, B., Tanenhaus, M. K., & Aslin, R. N. (2003). Lexical effects on compensation for coarticulation: The ghost of Christmash past. *Cognitive Science, 27*, 285–298] involved experimentally-induced biases (from 16 practice trials) rather than feedback. We found that the direction of the compensation effect depended on whether practice stimuli were words or nonwords. When both were used, there was no lexically-mediated compensation. Across experiments, however, there were lexical effects on fricative identification. This dissociation (lexical involvement in the fricative decisions but not in the following stop decisions made on the same trials) challenges interactive models in which feedback should cause both effects. We conclude that the prelexical level is sensitive to experimentally-induced phoneme-sequence biases, but that there is no feedback during speech perception.

Introduction

For spoken-word recognition to succeed, information must flow forward, from lower levels of processing (e.g., prelexical acoustic–phonetic analysis) to higher levels (e.g., the mental lexicon). There is no logical necessity, however, for information to flow in the other direction. That is, there is no need for the lexical level to send information back to earlier stages of processing. Indeed, as Norris, McQueen, and Cutler (2000) have argued, if the prelexical level performs an optimal analysis of the speech signal, lexical–prelexical feedback would be of no benefit to word recognition. Accordingly, many models of speech perception have been proposed in which there is no on-line lexical–prelexical feedback (the Cohort Model, Marslen-Wilson, 1987; Marslen-Wilson & Welsh, 1978; the Fuzzy Logical Model of Perception, Massaro, 1989; Oden & Massaro, 1978; the Race Model, Cutler & Norris, 1979; the Neighborhood Activation Model, Luce, 1986; Luce & Pisoni, 1998; Shortlist, Norris, 1994; Norris & McQueen, 2008; the Distributed Cohort Model, Gaskell & Marslen-Wilson, 1997; Merge, Norris et al., 2000; Lexical Access from Features, Stevens, 2002). We present here new data in support of the claim that there is no on-line feedback in speech perception.

The alternative theoretical view is that there is feedback from the lexical to the prelexical level during word recog-

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nition. This is a key assumption in the influential TRACE model (McClelland & Elman, 1986; see also McClelland, 1991; McClelland, Mirman, & Holt, 2006). Experimental psycholinguists have devoted a considerable amount of energy to trying to distinguish between these two classes of theory. The problem they have faced is that although models with feedback can account for most of the available data, so can models without feedback (see Norris et al., 2000, for review). This is because most of the studies on interaction have simply demonstrated that lexical information can influence decisions about the identity of phonemes. As Norris et al. (2000) showed, however, such findings are fully consistent with feedforward models where lexical and prelexical information is combined in order to make decisions.

Elman and McClelland (1988) appreciated the limitations of the data showing lexical influences on phonemic decision-making. They pointed out that what was needed in support of lexical–prelexical feedback was evidence that lexical knowledge could directly affect the inner workings of the prelexical level. In the TRACE model, for example, feedback of activation from the lexical to the phoneme level results in changes in the activation of phoneme units that are indistinguishable from changes produced by the perceptual input itself. That is, lexical feedback in TRACE alters the prelexical processing of phonemes. Any consequences of activating a particular phoneme should therefore be the same regardless of whether that activation was produced by the input or by feedback. Elman and McClelland presented evidence that seemed to demonstrate a lexical influence on prelexical processing. Their data seemed to show that compensation for coarticulation, which is generally agreed to operate at a prelexical level, could be modulated by information from the lexicon. In this paper, we revisit this critical test of feedback.

Christmas capes

Elman and McClelland's (1988) study focused on perceptual compensation for fricative-stop coarticulation. Mann and Repp (1981) found that listeners' perception of the place of articulation of stop consonants (i.e., a continuum of sounds ranging between [t] and [k]) was influenced by a preceding fricative. Specifically, the same ambiguous stop was identified as [k] more often after [s], and as [t] more often after [ʃ]. Mann and Repp described this perceptual effect as compensation for coarticulation. During speech production, the shape of the vocal tract required for articulation of the fricatives [s] (e.g., spread lips) and [ʃ] (e.g., rounded lips) has acoustic consequences for following stop consonants, making [k] take on more [t]-like qualities after [s], and making [t] take on more [k]-like qualities after [ʃ] (Repp & Mann, 1981, 1982). Mann and Repp's suggestion, therefore, was that the perceptual system compensated for this coarticulation process. There has been considerable debate about the mechanism(s) underlying this and similar types of perceptual compensation effects (e.g., whether they reflect general auditory contrast processes, Lotto, Kluender, & Holt, 1997, or recovery of articulatory gestures, Fowler, 2006, or phonological processes, Mitterer, 2006). There is, however, broad agreement that, whatever the mechanism might be, it has a prelexical locus. One piece of evidence in support of this assertion (albeit from liquid-stop coarticulation rather than fricative-stop coarticulation) is that Japanese listeners show appropriate perceptual compensation behavior in response to [l] and [r] before a stop continuum even when they are unable to identify [l] and [r] accurately (Mann, 1986). This locates the compensation process at a stage prior to that at which explicit decisions about phonemes are made. Another argument is that if the compensation process is prelexical, it can be of benefit in word recognition (i.e., help in the recognition of words beginning with [t] and [k]). A postlexical adjustment process (operating, e.g., at a stage where explicit phonemic decisions are made) would not necessarily provide this benefit. Finally, the demonstration that compensation effects are stronger after nonwords (e.g., *abolis* vs. *arthritish*) than after matched words (e.g., *abolish* vs. *arthritis*; Samuel & Pitt, 2003) also suggests that the underlying process is prelexical (a lexical or postlexical process would surely show larger effects for words).

Perceptual compensation for fricative-stop coarticulation was thus taken by Elman and McClelland (1988), and by many others since, to be a prelexical mechanism. Elman and McClelland were then able to use the effect as a way of testing the prediction of the TRACE model that lexical knowledge could penetrate into the prelexical level and influence the operation of the compensation process. In Mann and Repp's (1981) study the effect of compensation for coarticulation on the perception of a sound ambiguous between a [t] and a [k] was conditioned by the identity of a preceding unambiguous fricative ([s] or [ʃ]). Elman and McClelland asked whether the same effect would occur if the fricative was ambiguous, but when its interpretation was determined by lexical feedback. They presented listeners with stimuli such as *Christma? *apes* and *fooli? *apes*, where the '?' was a sound half way between [s] and [ʃ], and '*' was one of a range of ambiguous sounds between [t] and [k]. If lexical information could feed back to alter prelexical representation of the ambiguous [?] sound, it should be interpreted as [ʃ] in the *fooli? *apes* context and as [s] in the *Christma? *apes* context. This should then trigger compensation for coarticulation such that the [*] sounds should be interpreted as [t] in the context of *fooli? *apes* and as [k] in the context of *Christma? *apes*.

The results were as Elman and McClelland (1988) predicted. They concluded that there was lexical–prelexical feedback. Note that this result is not susceptible to the feedforward explanation that phonemes were being identified by combining lexical and prelexical information (Norris et al., 2000). In the case of compensation for coarticulation, the lexical information about the fricative-final sequence provides no direct information about the identity of the following stop.

Transitional probabilities and a dissociation of effects

Pitt and McQueen (1998) challenged Elman and McClelland's (1988) conclusion, on two grounds. First, they showed that the original *Christmas capes*/*foolish tapes* results were open to an alternative explanation, based on phoneme transitional probabilities. According to an analysis by Cairns, Shillcock, Chater, and Levy (1995) of the London-Lund corpus (Svartvik & Quirk, 1980), [əs] is more likely in English than [əʃ], while [Iʃ] is more likely than [Is]. (The sequence [əs] occurs in *Christmas* and the other [s]-final words in Elman and McClelland's Experiment 1; [Iʃ] appears in *foolish* and the other [ʃ]-final words in that experiment.) Studies of the effects of transitional probabilities and lexical knowledge across a variety of word recognition tasks suggest that knowledge about the sequential dependencies of phonemes is stored outside the mental lexicon (Vitevitch & Luce, 1998, 1999). Pitt and McQueen argued that if the prelexical level were sensitive to these sequential dependencies, the Elman and McClelland results could be explained without lexical–prelexical feedback.

Norris (1993) and Cairns et al. (1995) had previously shown that recurrent networks can predict the apparently lexically-mediated compensation effect without any top–down feedback. The recurrent connections in these networks re-circulate information between the nodes in the hidden-unit level. These networks can thus learn about transitional probabilities, even when they have nothing that corresponds to a lexical level of representation, and hence are given no explicit knowledge about words. Norris showed that when recurrent networks were trained to identify both the words and the phonemes in the input, they could learn about transition probabilities and simulate the contextually-modulated compensation for coarticulation effect. However, these networks' performance could not be attributed to feedback from lexical nodes to prelexical representations because the connections to the lexical output nodes were purely feedforward. There was thus no way that the lexical output units could influence prelexical processing. To the extent that there was any "lexical" knowledge at the hidden-unit level in these networks, it was incomplete, because, in order to recognize words, the networks also required the feedforward connections between the hidden units and output units. The most convincing evidence that the compensation for coarticulation effect in recurrent networks is not lexically mediated, however, was that a recurrent network that had no lexical output nodes and was not trained to recognize words, and so had no lexical knowledge at any level of processing, still produced contextually-modulated compensation for coarticulation. Lexical knowledge is therefore not necessary for sensitivity to transitional probabilities to emerge. Sensitivity to transitional probability at the prelexical level in a recurrent network thus provides an alternative, feedback-free account of the Elman and McClelland (1988) data.

Pitt and McQueen (1998) demonstrated that transitional probabilities did indeed influence compensation for coarticulation. If the diphone transitional probabilities between the vowels and the following final fricatives were controlled in words preceding the *tapes–capes* continuum, there was no lexically-mediated shift in stop identification after an ambiguous fricative (i.e., no greater tendency to label stops as [k] after [dʒu?], based on *juice*, than after [bU?], based on *bush*). But if diphone transitional probabilities were manipulated in nonwords preceding the same stop continuum, there was an effect on stop identification, consistent with interpretation of the ambiguous fricative as the more likely sound in that context (e.g., more [k] responses after a [?]-final nonword where the transitional probability bias favored identification of [?] as [s]). The Elman and McClelland (1988) ambiguous-fricative results could thus reflect a prelexical transitional probability bias rather than a lexical bias.

Pitt and McQueen's (1998) finding that there was no sign of lexically-mediated compensation for words when transitional probabilities were equated greatly undermined the case for interaction. But it was also a null result. Perhaps their experiments simply did not produce powerful enough lexical or compensation effects? This seems unlikely. Participants identified not only the stops on the *tapes–capes* continuum, but also the preceding fricatives. Across all three experiments, there was a significant lexical effect on fricative identification (e.g., more [s] responses to [dʒu?] than to [bU?]; cf. Ganong, 1980; McQueen, 1991a). There was also a significant compensation effect with unambiguous fricatives (e.g., after *juice* and *bush*). The two components for a feedback-based explanation (the lexical effect on fricative identification supposedly due to feedback, and the prelexical fricative-stop compensation effect) were therefore present, but there was no lexical compensation effect. Pitt and McQueen argued that this dissociation was damaging for the feedback account. If lexical feedback was the source of the effect on fricative perception, then it should also have affected compensation for coarticulation. Note, however, that this dissociation is not problematic for a feedforward account where the two effects have different loci – the lexical effect on fricative identification at a postlexical decision stage (as, e.g., in Merge, Norris et al., 2000), and the compensation effect at the prelexical level (and hence immune to lexical influences).

Pitt and McQueen's (1998) findings suggested that what had appeared to be lexically-mediated compensation for coarticulation effects were probably due instead to unbalanced transitional probabilities. The dissociation they observed cast substantial doubt on the feedback account. Furthermore, although Elman and McClelland's (1988) question (i.e., is there lexical involvement in a prelexical process?) undoubtedly remains the right one to ask, Pitt and McQueen's results gave an indication of how difficult it is to construct properly controlled stimuli to ask that question. Unfortunately, the picture is even more complicated.

Perceptual grouping and word length

Samuel and Pitt (2003) made a number of important points about lexically-mediated compensation for coarticulation. The first was that fricative-stop compensation effects for both unambiguous and ambiguous fricatives are highly variable (as McQueen, 1991b, also argued). Samuel and Pitt tested eight different pairs of lexical contexts, all followed by a *tame–came* continuum. There was a statistically significant signal-driven compensation effect with words ending in unambiguous fricatives (e.g., *abolish* and *arthritis*) in five pairs, with nonwords ending in unambiguous fricatives (e.g., *aboliss* and *arthritish*) in five pairs (though one pair showed a reversal across two different tests), but an apparently lexically-driven compensation effect in only two pairs with the critical ambiguous fricative (*extinguish–contagious* and *distinguish–consensus*).

Samuel and Pitt (2003) argued that at least two factors were responsible for this variability: perceptual grouping and word length. Their data (and the results of Mann & Repp, 1981) suggested that the likelihood of observing a compensation effect (with both unambiguous and ambiguous fricatives) depends on how the fricative noise is perceived to group with the surrounding speech. Samuel and Pitt suggested that if the fricative is grouped too strongly with the preceding context, the fricative might exert no influence on the interpretation of the following stop. Furthermore, this appeared to occur more for word than for nonword contexts. Word length was found to have two effects. The first was an influence on perceptual grouping: Longer words appeared to attract their final fricatives more strongly than shorter words. This produced more robust compensation effects with unambiguous fricatives for shorter words than for longer words. Second, compensation effects in the critical ambiguous fricative condition were more robust for longer words than for shorter words. The idea here was that longer words might be able to provide more lexical support for their final fricatives (and/or more time for that support to accrue) than shorter words (see also Pitt & Samuel, 2006).

This variability makes studying this critical phenomenon very difficult. As Samuel and Pitt (2003) put it, "finding lexically-mediated compensation requires a rather daunting constellation of events to co-occur" (p. 428). Our own experimental trials and tribulations (see below) suggest that the constellation of necessary events is even greater than Samuel and Pitt envisaged. Nevertheless, Samuel and Pitt (2003) did observe a statistically significant lexical compensation effect in two of their eight pairs. Critically, in response to the Pitt and McQueen (1998) study, they had carefully controlled the vowel-fricative transitional probabilities in their stimuli. Is this convincing evidence of feedback? We suggest not. In part this is because of the variability that we have just discussed: Although there may well be factors such as perceptual grouping and word length which influence the outcome, the critical ambiguous fricative compensation effects are very weak and variable. If feedback from the lexicon had an important functional role to play in speech perception, one would expect that its effects would be both widespread and easy to observe.

Experiment-induced biases?

The present study was based on an experiment by Magnuson, McMurray, Tanenhaus, and Aslin (2003a). They found an apparent lexical effect on compensation with ambiguous fricatives when all transitional probabilities (including, e.g., diphone vowel-fricative dependencies and more complex transitional probabilities) were controlled. In fact, the transitional probability biases were set up to work against the lexical effect. The effect they observed (more [k] responses to a *tapes–capes* continuum after *bli?* than after *bru?*) would thus appear to be strong evidence of lexical–prelexical feedback.

As McQueen (2003) pointed out, however, there is a potential confound in the Magnuson et al. (2003a) design. During the experiment, participants heard only the lexically-consistent fricative contexts (*bliss*, *bli?*, *brush* and *bru?*), and never the lexically-inconsistent sequences with unambiguous fricatives (*blish* and *bruss*). It was thus possible that listeners learned an experiment-internal bias, namely, that [s] was more likely after *bli* and that [ʃ] was more likely after *bru*. If participants interpreted the ambiguous fricatives in line with these learned experimental biases, this would produce a result that looked exactly like lexically-mediated compensation for coarticulation. In response to McQueen (2003), Magnuson, McMurray, Tanenhaus, and Aslin (2003b) performed an analysis of their experiment split by blocks, and showed, in apparent contradiction of the experimental bias account, that the compensation effect did not change over the course of the experiment.

It remains possible, however, that the bias had already been learned in the 16-item practice phase. During the practice phase listeners heard only the unambiguous word contexts (*bliss* and *brush*). We tested that possibility here. Experiment 1 was a close replication of Magnuson et al.'s (2003a) experiment. In Experiments 2 and 3, we changed the composition of the practice trials to test whether the pattern observed in Experiment 1 arose as a consequence of expectancy biases created during the practice phase.

Do lexical effects on fricatives dissociate from those on stops?

An equally important goal of these experiments was to establish whether the dissociation found by Pitt and McQueen (1998) could be replicated. Following the procedure used both by Pitt and McQueen and by Magnuson et al. (2003a), participants in our study were asked to identify both the fricatives and the stops. This allowed us to test for lexical effects on both types of consonant. As we have already noted, a model with feedback such as TRACE predicts that if there are lexical effects on ambiguous fricative identification (due to feedback to the prelexical level) and compensation effects on stops after unambiguous fricatives (due to a prelexical compensation process), this should produce a lexically-mediated compensation effect. Samuel and Pitt (2003) suggest, however, that the latter effect is not a necessary consequence of the former two effects, at least not if there are differences in how the fricative stimuli are grouped perceptually. Specifically, they argued that ambiguous fricatives may group more strongly with the preceding context than unambiguous fricatives. If the ambiguous fricative were grouped too strongly with the first word (perhaps, they argue, precisely because the lexically-consistent interpretation of the fricative causes a word to be formed), then, unlike the unambiguous fricatives, the ambiguous fricatives would exert no influence on identification of the subsequent stop. According to Samuel and Pitt, there could therefore be lexical effects on ambiguous fricative identification in the absence of lexically-mediated compensation for coarticulation.

In our revisitation of the Magnuson et al. (2003a) findings we address this argument by controlling factors influencing the strength of perceptual grouping across conditions. One such factor is the duration of the silent interval between the fricatives and the stops across the unambiguous and ambiguous fricative conditions (Samuel & Pitt, 2003). In the Magnuson et al. study (and the earlier studies on lexical mediation of compensation) the same interval was used across these conditions. With respect to their temporal properties, therefore, the two types of fricative should have grouped equally strongly with their preceding and following contexts and therefore should have been equally likely to induce compensation. However, as Samuel and Pitt argued, the lexical status of the fricative-final sequence might also influence grouping: Words attract their final sounds to themselves more strongly than nonwords do. This lexical influence on grouping should thus be stronger with unambiguous fricatives (i.e., in the real words *bliss* and *brush*) than with ambiguous fricatives (*bli?* and *bru?* are less word-like). With silence duration across conditions in the present experiments controlled, the ambiguous fricatives should tend to group less with the preceding context than unambiguous fricatives, and thus should be more likely to influence stop identification than their unambiguous counterparts. Any failure to observe such an influence (i.e., the Pitt & McQueen, 1998, dissociation) would thus be unlikely to be due to perceptual grouping differences across conditions. In other words, if we were to find such a dissociation, a perceptual grouping account could not provide an alternative explanation for it. This dissociation would pose a serious challenge to models with lexical–prelexical feedback.

The rocky road to compensation effects

We begin with a brief description of a series of pilot experiments which preceded Experiment 1 (see Appendices A and B for further details). Samuel and Pitt (2003) noted that it is extremely difficult to find the "constellation of events" required for apparent lexically-mediated compensation for coarticulation, or indeed even the basic effect with unambiguous fricatives. Our pilot experiments confirm this observation and show, in addition, that even with the same "constellation" (i.e., the same materials and procedure) the effects do not necessarily replicate.

Our initial attempts to replicate Magnuson et al. (2003a) (Pilots 1a and 1b) used natural British English recordings of their materials and native British English participants. We failed to replicate the critical compensation effect with ambiguous fricatives, or even to find a baseline compensation effect with unambiguous fricatives. In Pilots 2a and 2b, we used the original Magnuson et al. recordings, with American English participants. We again failed to observe compensation effects with unambiguous and ambiguous fricatives. These failures led Magnuson and McMurray (Magnuson, personal communication) to attempt their own replications with the original materials. While they succeeded in two experiments (one in Connecticut by Magnuson and one in Iowa by McMurray) to find compensation with unambiguous fricatives, they also failed in both experiments to replicate the effect with ambiguous fricatives. A new set of materials were then constructed by Magnuson and colleagues, for which they found reliable compensation effects with American English listeners for both unambiguous and, critically, ambiguous fricatives. It is these materials that were used in Experiment 1.

In addition to the perceptual grouping and word length factors identified by Samuel and Pitt (2003), but controlled in all of the present experiments, the pilot experiments (both ours and those of Magnuson et al.) suggest that stimulus quality (the nature of the fricative and stop sounds) is also an important factor. They also indicate that there may be considerable inter-participant variability. Nevertheless, the effect with ambiguous fricatives was observed in the original Magnuson et al. (2003a) study, and in their final experiment in this preliminary series of pilots using new materials. In Experiment 1 we sought to replicate the effect once again, but with these new materials and British English listeners, and hence to set up a baseline for our examination of the effects of changes in the make-up of the practice trials.

Experiment 1

Experiment 1 was a close replication of the design used by Magnuson et al. (2003a). Critically, during practice, participants were exposed only to the word endpoints of the fricative words and never to their nonword endpoints (see Table 1). That is, all practice trials contained either *bliss* or *brush* but never *blish* or *bruss* (nor *bli?* or *bru?*).

Method

Participants

Twenty-four students from the participant pool of the MRC Cognition and Brain Sciences Unit, Cambridge were tested. All participants were native speakers of British English and reported no hearing or language disorder.

Stimuli

All materials were constructed by McMurray (personal communication). Materials were based on three 21-step continua: *bliss-blish*, *brush-bruss*, and *tape–cape*. All three

Table 1
Design overview.

Experiment	Practice	Main experiment	
		Unambiguous fricatives	Ambiguous fricatives
1	*bliss*, *brush*	*bliss*, *brush*	*bli09*, *bru09*, *bli11*, *bru11*, *bli13*, *bru13*
2	*blish*, *bruss*	*bliss*, *brush*	*bli09*, *bru09*, *bli11*, *bru11*, *bli13*, *bru13*
3	*bliss*, *brush*, *blish*, *bruss*	*bliss*, *brush*	*bli09*, *bru09*, *bli11*, *bru11*, *bli13*, *bru13*

were constructed with the KlattWorks (McMurray, in preparation) interface to the Klatt and Klatt (1988) synthesizer. The cascade branch of the synthesizer was used to create voiced and aspirated portions, and the parallel branch was used for fricated portions. Synthesis was based on a single recording for each of the endpoints *bliss*, *brush*, and *tapes*, spoken by a male native speaker of American English. Formants and pitch tracks were extracted from these recordings using Praat (Boersma & Weenink, 2005) and loaded into KlattWorks. The raw formant and pitch values were fitted to a series of smooth logistic functions. The remaining parameters (amplitudes of frication, aspiration, and voicing, the bandwidths of each formant, and the amplitudes of each formant in the parallel branch) were set by hand in order to create synthetic tokens that best matched the natural recordings (as established by a comparison of spectrograms). The model for the *tape* endpoint was based on a recording of *tapes*. A *tape–cape* continuum was used in order to avoid any contrast effects between the final [s] of *tapes–capes* and the critical fricative in the preceding word, but this decision was made after *tapes* had been recorded and modeled. To make the *tape* model, the frication at the end of the *tapes* model was excised and a small amount of aspiration was added after the release burst of the [p].

The three continua were then made. For the *bliss-blish* and *bruss-brush* continua, the *bli-* and *bru-* portions came from the synthetic models of the *bliss* and *brush* recordings, respectively, the [s] endpoint from the *bliss* model, and the [ʃ] endpoint from the *brush* model. The fricative continuum in *bliss-blish* and *bruss-brush* was thus identical. A number of parameters of the fricative were constant across both endpoints. The amplitude envelope (the amplitude of frication parameter) rose at fairly slow rate as voicing offset and fell off 210 ms later. The 3rd and 4th formants also received some excitation (7 and 17 dB, respectively), and their frequencies were the same across both continua (2050 Hz and 2350 Hz, respectively). Some broadband noise was added using the AB parameter. The [s] was constructed by setting the frequency of the 5th formant to 4300 Hz, after a drop from its steady state (4480 Hz for *bli-*, 4700 Hz for *bru-*). Its amplitude was set to 58 dB. The 6th formant had a frequency of 4900 after a drop from its steady state (4990 Hz for both), and its amplitude was set to 55 dB. To change this [s] into [ʃ], the frequencies of the 5th and 6th formants were lowered, across 21 different steps, from the [s] endpoint values to 2500 Hz (for F5) and 4000 Hz (for F6). Their amplitudes and bandwidths were held constant. This resulted in a continuum that varied only in the frequency of the center of frication. This is in contrast to the sample-averaged stimuli used in earlier studies (e.g., Pitt & McQueen, 1998; Magnuson et al., 2003a; Samuel & Pitt, 2003).

This resulted in two 21-step continua ranging from *bliss* to *blish* and from *bruss* to *brush*. Pilot studies were then conducted at the Universities of Connecticut and Iowa by Magnuson and McMurray respectively (Magnuson, personal communication). Subjects were asked to categorize the final consonants of both continua as either "s" or "sh". The categorization responses were used to select a range of three ambiguous fricatives that were close to the [s]/[ʃ] category boundary (Steps 9, 11 and 13).

The *tape–cape* continuum was constructed from the *tape* synthetic model by manipulating three features of the initial stop consonant. First, the onset of F2 was manipulated. For *tape*, F2 started at 1450 Hz and rose to a steady state of 1790 Hz. The onset frequency was changed to 2150 Hz (based on measures taken from a natural recording of *capes* made by the same speaker) in 20 equal increments of 35 Hz. Second, F3 at onset was 3050 Hz for *tape* and fell to a steady state of 2570 Hz. F3 onset was adjusted, across 20 decrements of 37.5 Hz, to an endpoint value of 2300 Hz (for *cape*). This resulted in a *tape* endpoint with lower F2 and higher F3, and a *cape* endpoint with a "velar pinch". Third, the spectrum of the release burst (which lasted two frames) was manipulated by adjusting the A2, A3, A4, and A5 parameters (amplitude of the 2nd, 3rd, and 4th formants in the parallel branch). For *tape*, A5 was set to 50 dB, and A2-4 was 0 dB. For *cape*, A5 was reduced to 15 dB, A2 and A3 to 45 dB, and A4 to 20 dB. The spectrum of the release burst for *tape* thus contained only high-frequency (5th formant) components, while the spectrum of *cape* was more distributed.

This 21-step continuum was also piloted at the Universities of Iowa (by McMurray) and Connecticut (by Magnuson; Magnuson, personal communication). Participants were asked to categorize the initial sound on the continuum as either "t" or "k". Steps were selected to equalize the number of stimuli on each side of the pilot participants' group category boundary: Steps 1 ([t]), 8, 10, 11, 12, 13, and 21 ([k]).

Finally, the seven selected tokens from the *tape–cape* continuum were each spliced onto the *bliss* and *brush* endpoints, and onto the three ambiguous tokens of each context (*bli9*, *bli11* and *bli13*; *bru9*, *bru11* and *bru13*). There were thus 56 items in total. In all cases, there was 24.7 ms between the offset of the fricative and the onset of the stop release burst.

Procedure

The procedure was the same as in the final experiment by Magnuson and McMurray in the preliminary series of pilots (Magnuson, personal communication). Participants were tested individually in a quiet room. They were instructed to identify the last sound of the first word as either [s] or [ʃ] and the first sound of the second word as either [t] or [k] in word pairs such as *brush tape* or *bliss cape*. Responses were given by pressing one of four labeled keys ('sT', 'sK', 'shT', 'shK') on a keyboard (positioned over the letters Q, E, I, and P, respectively). An example of each endpoint combination and the correct response key was provided in the instructions, which were identical to those in Magnuson et al. (2003a) and Pilot 2b (see Appendix B) except that the instructions used here were more explicit about the possibility that participants could take a break.

Critically, the experiment started with a practice block that contained only *bliss* and *brush* endpoint tokens combined each with the *tape* and *cape* endpoints (see Table 1). Practice consisted of four repetitions of these items, that is, altogether 16 trials. The main part of the experiment consisted of six blocks. In each block, the participant was presented with all 56 items once (i.e., the two word contexts with each of the ambiguous fricatives and with the lexically-consistent unambiguous endpoint fricative, followed by all seven levels of the *tape–cape* continuum). Presentation order within a block was randomized for each participant. A break could be taken after each block. On each trial, the four response alternatives were presented on the screen 300 ms after trial onset. Stimulus presentation followed after 500 ms. Responses could be given starting 150 ms after stimuli onset. Once a response was given or 1500 ms passed, the next trial began. The experiment was controlled by PsyScope 1.2.5. Stimuli were presented at a comfortable listening level over Sennheiser HD250 linear II headphones. Participants were allowed to adjust the volume if they felt that that was needed.

Results

The data from four participants were excluded from the data analysis, in two cases because of equipment failure, and in the other two cases because the participants did not comply with the instructions. All critical results (i.e., lexical effects on fricative labeling, compensation for coarticulation effects on stop labeling after unambiguous fricatives, and, most importantly, compensation for coarticulation effects on stop labeling after ambiguous fricatives) are reported in Table 2. As can be seen, there were significant lexical effects on fricative labeling (more [ʃ] than [s] responses to *bru?*). This is an essential precondition for determining whether there might be lexically-mediated compensation for coarticulation on the identification of the following stop. We also observed the standard compensation for coarticulation effect following unambiguous fricatives (more [k] responses after *bliss* than after *brush*). There was also a compensation effect after one of the ambiguous fricatives (more [k] responses after *bli?* than after *bru?*), as would be expected if compensation for coarticulation could be lexically mediated.

Fricative labeling

Table 3 shows the average proportions of [s] responses to the unambiguous and ambiguous fricatives in each of the lexical contexts (*bli-* and *bru-*). There was a strong lexical bias for all three ambiguous fricatives.

Table 3
Proportion of [s] responses for each unambiguous fricative ([s] after *bli-* and [ʃ] after *bru-*), and each ambiguous fricative (Steps 9, 11, and 13) in each lexical context.

Experiment	*bli-* context				*bru-* context			
	[s] (%)	Step 9 (%)	Step 11 (%)	Step 13 (%)	[ʃ] (%)	Step 9 (%)	Step 11 (%)	Step 13 (%)
1	97	74	78	84	2	3	3	6
2	95	54	66	79	3	6	10	17
3	94	52	69	80	4	8	13	22

The proportions of [s] responses to the ambiguous fricatives were analyzed as a function of lexical context, level of ambiguity, and step on the [t]–[k] continuum (as in the analysis of stop labeling below, this analysis was restricted to trials involving the five most ambiguous stops). Lexical context had an effect (see Table 2). The level of ambiguity on the [s]–[ʃ] continuum also affected fricative labeling ($F(2,38) = 6.88$, $p < .05$): At more s-like steps, the proportion of [s] responses increased. There was a trend for the lexical effect to vary across the three fricatives ($F(2,38) = 2.47$, $p = .10$). There were also more [s] responses for the more ambiguous levels of the [t]–[k] continuum than for its endpoints ($F(4,16) = 3.68$, $p < .05$). The three-way interaction was marginally significant ($F(8,152) = 2.00$, $p = .05$). None of the other interactions was significant (all p's > .05). An analysis of the unambiguous endpoints alone confirmed that participants reliably identified the unambiguous fricatives correctly ($F(1,19) = 5151.67$, $p < .001$).

Stop labeling

Fig. 1 plots averaged proportions of [k] responses as a function of lexical context and [t]–[k] level separately for the unambiguous fricative endpoints and for each ambiguous fricative. There was a compensation for coarticulation effect for most of the stops after unambiguous fricatives (more [k] responses after *bliss* than after *brush*), but only weak effects for some stops after ambiguous fricatives.

The analyses on the proportion of [k] responses were restricted to the five ambiguous levels of the continuum (i.e., the unambiguous [t]–[k] endpoints were excluded, as in Magnuson et al. (2003a). These analyses were done sepa-

Table 2
Results overview.

Experiment	Measures	Lexical effect on fricatives	Compensation effect after unambiguous fricatives	Compensation effect after ambiguous fricatives		
				Step 9	Step 11	Step 13
1	$F(1,19)$	129.39	13.96	.28	8.71	1.24
	p	<.001	<.001	.60	<.01	.28
	Mean difference	.75	.10	.01	.06	.02
	±95% CI	.14	.05	.05	.04	.05
2	$F(1,19)$	69.69	16.25	.62	.97	4.73
	p	<.001	<.001	.44	.34	<.05
	Mean difference	.55	.08	.01	.02	−.05
	±95% CI	.14	.04	.04	.05	.05
3	$F(1,19)$	64.09	11.54	.19	1.16	.53
	p	<.001	<.01	.67	.30	.47
	Mean difference	.53	.11	.01	.03	.02
	±95% CI	.14	.07	.04	.05	.05

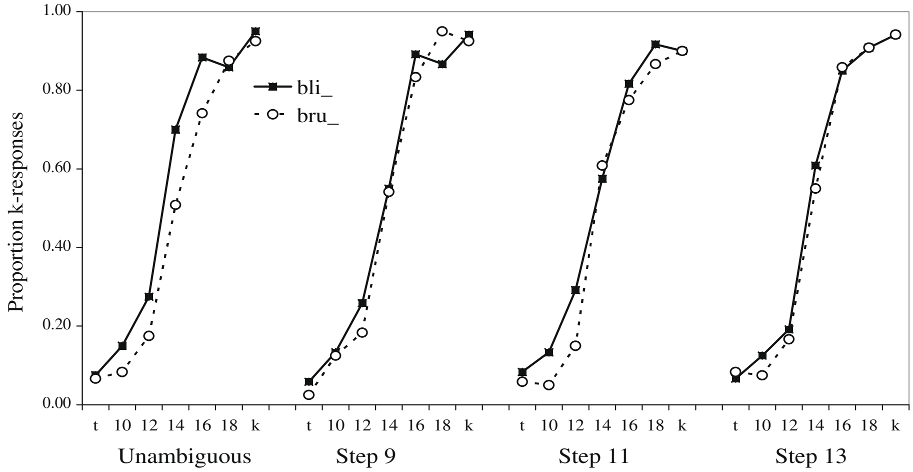

Fig. 1. Experiment 1: Proportion of [k] responses for each step of the [t]–[k] continuum after the unambiguous fricative-final words (*bliss* and *brush*) and for each ambiguous fricative (Steps 9, 11 and 13) in each lexical context (*bli_* and *bru_*).

rately for the fricative endpoints and for the ambiguous fricatives. For the fricative endpoints, compensation for coarticulation was found (see Table 2): More [k] responses were given after [s] than after [ʃ]. The [t]–[k] continuum also had an effect on stop labeling ($F(4,16) = 34.20$, $p < .001$). Unsurprisingly, more [k]-like sounds received more [k] responses. Continuum also interacted with the fricative factor ($F(4,16) = 4.53$, $p < .05$; the compensation effect was larger for some of the ambiguous stops than for others).

For the ambiguous fricatives, the effect of lexical context (*bli*- vs. *bru*-) was compared across the three fricative steps (Steps 9, 11 and 13). There was no main effect of step ($F < 1$), a marginal effect of context ($F(1,19) = 4.11$, $p = .057$), and no interaction ($F(2,38) = 1.30$, $p = .280$). There was therefore only weak evidence of a lexically-guided compensation for coarticulation effect, and no strong evidence that this effect varied across the ambiguous fricatives. Nevertheless, given the established fragility and variability of these effects, and in order to follow the analysis procedure in Magnuson et al. (2003a), separate analyses were performed for each ambiguous step. A statistically significant lexical compensation for coarticulation effect was found for Step 11, but not for Steps 9 and 13 (see Table 2). That is, for Step 11, more [k] responses were given when the ambiguous fricative was presented in a *bli*- than in a *bru*- context. An effect of [t]–[k] continuum was found for each of the ambiguous fricatives (Step 9: $F(4,76) = 141.13$, $p < .001$; Step 11: $F(4,16) = 75.80$, $p < .001$; Step 13: $F(4,16) = 82.61$, $p < .001$). The context factor did not interact with the [t]–[k] continuum in any of the analyses (Step 9: $F(4,16) = 2.34$, $p = .10$; Step 11: $F(4,16) = 2.10$, $p = .13$; Step 13: $F(4,16) = .58$, $p = .68$).

Discussion

These results replicate the Magnuson et al. (2003a) findings of an apparently lexically-driven compensation for coarticulation effect, at least for one of the ambiguous fricatives. More [k] responses were given when the lexical context favored an [s] interpretation than when it favored an [ʃ] interpretation. It would thus appear that stop identification can be influenced by lexical knowledge about the identity of an otherwise ambiguous fricative, even in the absence of a transitional probability bias. Having replicated Magnuson et al. (2003a), albeit rather weakly, we could finally examine whether this apparent lexical compensation for coarticulation effect is due to an experimentally-induced bias. We did so in Experiment 2.

Two other aspects of the results of Experiment 1 should be noted first, however. One is the variability of the compensation effect after ambiguous fricatives: It was statistically significant only for the middle fricative (Step 11), and, in the combined analysis, only marginally significant. This underlines the fragility of the effect, as noted for example by Samuel and Pitt (2003). The second point is that, for the other two ambiguous fricatives, we found a lexical effect in fricative judgments combined with an absence of a lexical effect on the following stops, that is, we replicated the dissociation found by Pitt and McQueen (1998). It is not plausible that the dissociations for these two conditions reflect differences across conditions in perceptual grouping (cf. Samuel & Pitt, 2003): Stimulus durations, timing, and lexical contexts were identical across all conditions. These dissociations thus challenge the view that there is lexical–prelexical feedback.

Experiment 2

As we noted in the Introduction, the apparent lexical effect on stop labeling is potentially due to the statistical properties of the practice trials. During practice, listeners were only given lexically-consistent fricative items (*bliss* and *brush*). Lexically-inconsistent fricative endpoints (*blish* and *bruss*) were not presented. This distribution of fricative items during practice could have induced participants to label ambiguous fricatives in a way that was consistent with the bias. For example, an [s] interpretation of an ambiguous fricative in the *bli*- context was predicted by the practice trials as well as favored by lexical knowledge.

If the distribution of fricative items during practice caused the effect, then presenting only lexically-inconsistent fricative endpoints in the practice block should reverse the bias and should therefore reverse the "lexical" compensation for coarticulation effect. Note that this manipulation provides a strong test of the experimentally-induced response bias account, since if the apparent lexical effect is indeed at least partially due to lexical feedback, then the experimentally-induced bias has to overcome the lexical influence to reverse the effect.

Experiment 2 thus tested the experimental bias explanation by reversing the probabilities of fricative tokens during practice. Participants were exposed only to nonword endpoints (*blish* and *bruss*) during practice. The test phase was identical to that in Experiment 1, in that only lexically-consistent and ambiguous-fricative endpoints were included (i.e., *blish* and *bruss* were once again not presented in the test phase). We predicted that if the apparent lexical compensation effect observed in Experiment 1 (and by Magnuson et al., 2003a and by Magnuson and colleagues in their subsequent replication) is due to an experimental bias, then the results should reverse for the ambiguous fricative conditions. More [k] responses should be found after the *bru?* context than after the *bli?* context. If, however, the previously found lexical compensation for coarticulation effect is not due to an experimental bias, and is a true lexical effect, then the results of Experiment 1 should be replicated.

Method

Participants

Twenty-seven participants from the same population as tested in Experiment 1 took part. None had participated in the previous experiment.

Stimuli and procedure

The materials, procedure, and design of the experiment were identical to those in Experiment 1. The only difference was that here only the *blish* and *bruss* endpoint items were presented during practice, each combined four times with each of the endpoints of *tape* and *cape* for a total of again 16 practice trials (see Table 1).

Results

The data from seven participants were excluded from the analysis, in six cases because of equipment failure, and in the remaining case because the participant did not comply with the instructions. All critical results are reported in Table 2. As in Experiment 1, there was a significant lexical bias on fricative identification (more [ʃ] than [s] responses to *bru?*) and a compensation for coarticulation effect following unambiguous fricatives (more [k] responses after *bliss* than after *brush*). However, following ambiguous fricatives, the only shift in stop labeling was in the opposite direction from that expected if compensation for coarticulation were lexically mediated (more [t] responses after *bli?* than after *bru?*).

Fricative labeling

Table 3 shows the averaged proportions of [s] responses. For the ambiguous fricative conditions, the proportion of [s] responses varied as a function of lexical context, with more [s] responses after *bli?* than after *bru?* (see Table 2). The ambiguity of the fricatives also affected responses ($F(2,18) = 17.88$, $p < .001$): As the ambiguous fricative approached [s], more [s] responses were given. There was also an interaction of fricative step with the lexical effect: As the ambiguous sound became more [s]-like, the lexical effect became stronger ($F(2,18) = 5.97$, $p < .01$). Posthoc directional pairwise comparisons with a Bonferroni-adjusted alpha level of .017 showed that the effect was larger for Steps 13 ($t(19) = 3.25$, $p < .017$) and 11 ($t(19) = 3.14$, $p < .017$) than for Step 9. The effect was only marginally larger for Step 13 than for Step 11 ($t(19) = 1.75$, $p = .05$). There was no difference in fricative labeling as a function of step on the [t]–[k] continuum ($F(6,76) = .68$, $p = .6$), and this factor marginally interacted with fricative step ($F(8,152) = 1.36$, $p = .07$), but not with lexical context or both (all p's > .05). An analysis on responses to the unambiguous fricative endpoints showed that participants again correctly identified the fricative endpoints ($F(1,19) = 1860.94$, $p < .001$).

Stop labeling

As in Experiment 1, responses to only the five ambiguous [t]–[k] steps were analyzed. Fig. 2 shows the mean proportions of [k] responses as a function of lexical context and [t]–[k] level for each fricative. For unambiguous fricative endpoints, an influence of fricative context on proportions of [k] responses was found (see Table 2): There were more [k] responses after *bliss* than after *brush*, that is, there was compensation for coarticulation. In addition, there was a main effect of [t]–[k] continuum step ($F(4,16) = 63.15$, $p < .001$; more [k]-like sounds were identified as [k] more often), as well as an interaction of this factor with fricative ($F(4,16) = 7.22$, $p < .01$; the size of the compensation effect varied across the stop continuum).

For the ambiguous fricatives, there was no main effect of lexical context ($F < 1$), no effect of fricative step ($F(2,38) = 2.86$, $p = .069$), and a significant interaction of these two factors ($F(2,38) = 3.63$, $p = .036$). Separate analyses for each of the three fricatives showed that there was a difference in proportions of [k] responses between lexical contexts for Step 13 ($F(1,19) = 4.73$, $p < .05$), but not for Step 9 ($F(1,19) = .62$, $p = .44$) or Step 11 ($F(1,19) = .97$, $p = .34$). Critically, the lexical effect for Step 13 was in the opposite direction to that observed for Step 11 in Experiment 1. For Step 13, the context effect varied across the [t]–[k] continuum ($F(4,76) = 2.80$, $p < .05$). This was not the case for Step 9 ($F(4,76) = .76$, $p = .55$) or Step 11 ($F(4,76) = 1.37$, $p = .25$). An effect of [t]–[k] continuum level was found for all three fricative steps (Step 9: $F(4,16) = 72.06$, $p < .001$; Step 11: $F(4,16) = 68.00$, $p < .001$; Step 13: $F(4,16) = 69.27$, $p < .001$): More [k] responses were given to more [k]-like stops.

Discussion

In Experiment 2, for one of the ambiguous fricatives, there were more [k] responses following a *bru?* context

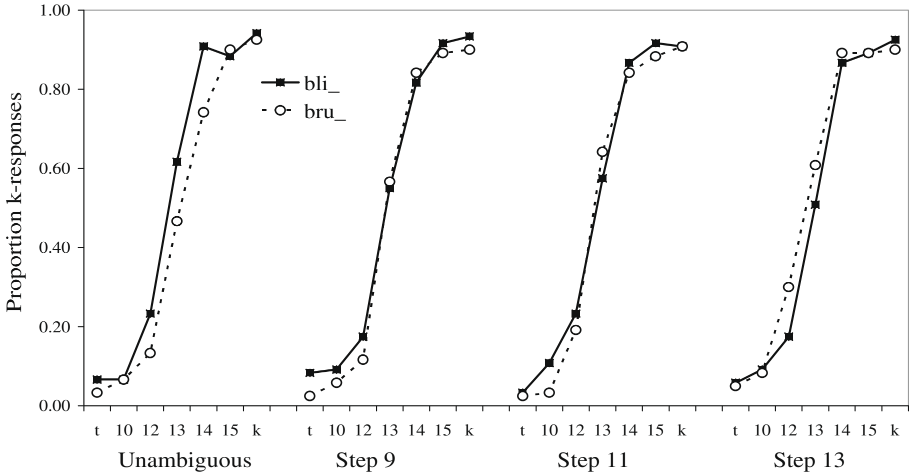

Fig. 2. Experiment 2: Proportion of [k] responses for each step of the [t]–[k] continuum after the unambiguous fricative-final words (*bliss* and *brush*) and for each ambiguous fricative (Steps 9, 11 and 13) in each lexical context (*bli_* and *bru_*).

than following a *bli?* context. This is exactly what is expected if performance with ambiguous fricatives in the test phase is driven by the contextual probabilities of [s] and [ʃ] in the practice block. This effect is the reverse of that predicted by an account based on lexical feedback. It therefore suggests that the "lexical" compensation effects found in Experiment 1 and by Magnuson et al. (2003a) were caused by a bias that was induced during practice.

Experiment 2 also presents further evidence on the fragility and variability of the compensation effect with ambiguous fricatives. For Step 9, there was no effect in either experiment, but for Step 11 there were more [k] responses after *bli?* than after *bru?* in Experiment 1 but not in Experiment 2, and for Step 13 there was no effect in Experiment 1 and more [k] responses after *bru?* in Experiment 2. Whatever the mechanism(s) responsible, these shifts are very difficult to find.

The dissociation first observed by Pitt and McQueen (1998) was replicated again in Experiment 2. Lexical knowledge influenced identification of the ambiguous fricative Steps 9 and 11 but, contrary to the predictions of a feedback model, there was no consequent shift in the identification of the following stops. As already noted, this dissociation is unlikely to be due to differences in perceptual grouping. Comparisons across experiments support this conclusion. For Step 11 (i.e., the same materials, with presumably the same perceptual grouping), the dissociation was observed in Experiment 2, but not in Experiment 1. If the lack of a lexical effect on the stops in Experiment 2 were because the ambiguous fricative grouped too strongly with the preceding context, then there should have been no effect in Experiment 1. It is clear that perceptual grouping is one of the many factors that can determine whether there will be compensation for coarticulation, but grouping cannot explain the absence of a compensation effect for Step 11 in Experiment 2.

Furthermore, a new type of dissociation was observed for Step 13: There was a lexical effect on the fricative judgments, but the reverse of a lexical effect on the stop judgments. The lexical effect on fricative labeling, however, was stronger for Step 13 than for Steps 9 and 11. This means that if there were a bias in stop identification driven by lexical restoration of the ambiguous fricative, then it ought to have been seen most strongly for Step 13. But instead of a strong lexically-mediated compensation effect, an effect in the opposite direction was observed. This reversal suggests once again that there is no lexical mediation of compensation for coarticulation.

Experiment 3

The results from Experiment 2 suggest that the so-called "lexical" compensation for coarticulation effect found in the Magnuson et al. (2003a) study, and in our replication in Experiment 1, is due to a bias introduced during practice. It is possible, however, that there was a lexical effect, but that it was confounded by the experiment-induced bias in Experiment 1, and masked by the opposite bias induced in Experiment 2. The final experiment therefore tested whether a lexical compensation for coarticulation effect would be found when there was no bias in the practice block. Participants were now exposed equally often during practice to the word and nonword endpoints (*bliss*, *brush*, *blish*, and *bruss*). With this kind of balanced exposure, the lexical compensation for coarticulation effect should be found, if indeed lexical knowledge can influence compensation for coarticulation via feedback connections.

Method

Participants

Twenty-two participants from the same population as before took part. None of them had participated in the previous experiments.

Stimuli and procedure

Materials, design, and procedure were once again the same. The only change in the design was that, during practice, all endpoint fricative tokens (*bliss*, *blish*, *bruss*, and *brush*) were presented with the stop endpoints *tape* and *cape* (see Table 1). The overall number of practice trials was sixteen trials as before, so two instead of four repetitions of each item combination were presented.

Results

The data from two participants were excluded from the analysis due to equipment failure during data collection. Table 2 summarizes the critical results. As in Experiments 1 and 2, there was a significant lexical bias on fricative identification (more [ʃ] than [s] responses to *bru?*) and a compensation for coarticulation effect following unambiguous fricatives (more [k] responses after *bliss* than after *brush*). However, contrary to the predictions of an interactive model, there was no shift in stop labeling following ambiguous fricatives. That is, there was no lexically-mediated compensation for coarticulation.

Fricative labeling

In the ambiguous fricative conditions, fricatives once again tended to be labeled as lexically consistent with the context: More [s] responses were found for the *bli?* than for the *bru?* context (see Tables 2 and 3). Furthermore, more [s] responses were given to more [s]-like sounds ($F(2,18) = 12.79$, $p < .001$). Again, these two factors interacted ($F(2,38) = 10.57$, $p < .001$): The lexical effect was stronger for Steps 13 ($t(19) = 3.71$, $p < .001$) and 11 ($t(19) = 4.41$, $p < .001$) than for Step 9. There was no difference between Step 13 and Step 11 ($t(19) = .42$, $p = .16$). The proportion of [s] responses did vary as a function of stop step ($F(4,76) = 1.88$, $p = .12$). This factor did not interact with any other variable (all p's > .05). Participants identified the fricative endpoints correctly ($F(1,19) = 413.05$, $p < .001$).

Stop labeling

Again, only the five ambiguous levels of the [t]–[k] continuum were considered in these analyses. Fig. 3 shows averaged proportions of [k] responses for lexical contexts over the [t]–[k] continuum, separately for each of the fricatives. Proportions of [k] responses were affected by the preceding unambiguous fricative (see Table 2). That is, unambiguous fricatives led to a compensation for coarticulation effect on the labeling of the subsequent stop consonants (more [k] responses after [s] than after [ʃ]). Furthermore, proportions of [k] responses varied across the [t]–[k] continuum ($F(4,16) = 37.09$, $p < .001$), with more [k] responses to more [k]-like stops. These two factors did not interact ($F(4,76) = .85$, $p = .50$).

In a combined analysis of all three ambiguous fricative steps, there was no effect of context ($F(1,19) = 1.19$, $p = .290$), no effect of step ($F(2,38) = 1.83$, $p = .185$), and no interaction ($F < 1$). As in the previous experiments, the results for each fricative were analyzed separately. A lexical compensation for coarticulation effect was not found for any of the ambiguous fricatives (see Table 2). There was an effect of [t]–[k] continuum on stop labeling for all three ambiguous fricatives (Step 9: $F(4,16) = 61.15$, $p < .001$; Step 11: $F(4,16) = 30.79$, $p < .001$; Step 13: $F(4,16) = 63.24$, $p < .001$). Furthermore, context never interacted with the stop effect (Step 9: $F(4,76) = .99$, $p = .42$; Step 11: $F(4,76) = .17$, $p = .96$; Step 13: $F(4,76) = .65$, $p = .63$).

Discussion

When the bias in the practice trials was removed, there was no trace of a lexically-mediated compensation effect after ambiguous fricatives. The absence of such an effect is striking, especially since there was a lexical effect on fricative identification and compensation effects after unambiguous fricatives. If the lexical effect were due to feedback, and perceptual grouping factors were such as to allow a compensation effect after unambiguous fricatives, there ought to have been a lexical compensation effect after the ambiguous fricatives.

General discussion

Over the past 30 or so years, dozens of papers have addressed whether there is feedback from lexical to prelexi-

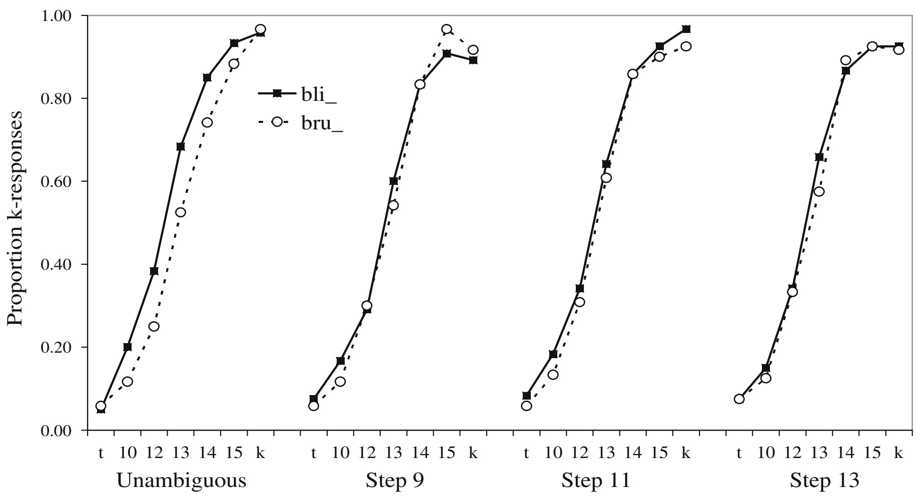

Fig. 3. Experiment 3: Proportion of [k] responses for each step of the [t]–[k] continuum after the unambiguous fricative-final words (*bliss* and *brush*) and for each ambiguous fricative (Steps 9, 11 and 13) in each lexical context (*bli_* and *bru_*).

cal processing. In their review of this literature, Norris et al. (2000) pointed out that most studies have simply shown that lexical information can influence the way listeners make metalinguistic judgments, such as phoneme categorizations. These results can all be explained by assuming that listeners make these decisions by combining information from lexical and prelexical sources. There is no need to assume that the lexical information feeds back down to influence the inner workings of prelexical perceptual processes. The data on lexically-mediated compensation for coarticulation first reported by Elman and McClelland (1988), however, is not susceptible to this kind of explanation. The critical results from their experiments do not depend on identification of the lexically-altered phoneme itself, but on the influence that phoneme has on the perception of the following phoneme. Compensation for coarticulation has therefore come to be seen as the critical test of the existence of feedback during speech perception. Unfortunately, it has proven difficult to design experiments that successfully control for the wide range of potential confounds. To date, the most convincing evidence of lexically-mediated compensation for coarticulation comes from the study by Magnuson et al. (2003a). But in the present series of experiments we have shown that even that study has a fatal confound: Statistical biases induced by the composition of the practice trials can modulate the effect. When using the same word-endpoint practice items used by Magnuson et al. (*brush* and *bliss*), we succeeded in replicating their effect (Experiment 1). However, when we replaced these practice items with the nonword endpoints (*blish* and *bruss*), the effect reversed (Experiment 2). In our final experiment we eliminated the bias by including both word and nonword endpoints in the practice, and the effect disappeared.

How does the composition of the practice trials alter perception?

These data show that both the presence and the direction of the compensation for coarticulation effect after ambiguous fricatives can vary as a function of the composition of the practice trials. In contrast, the lexical effect on fricative identification was unaffected by the nature of the practice trials: There was a word bias in fricative identification in all three experiments. How can we explain this dissociation?

One possible explanation is that the nonwords in the practice trials effectively become lexicalized. So, in Experiment 2, the initial exposure to *blish* and *bruss* would make them function as words in the experiment. If this were the case, and the exposure were sufficient to overcome the competing effects of the words *bliss* and *brush*, then these lexicalized 'nonwords' might trigger compensation in the 'wrong' direction. However, because this analysis implies that the nonwords effectively behave more like words than do the real words, it also implies that the nonwords should reverse the lexical bias on fricative identification. But this was not the case. Just as in Experiment 1, listeners in Experiment 2 identified more ambiguous fricatives as [s] in the *bli*- context than in the *bru*- context. So the nonwords cannot have been operating as real words. Whatever participants were learning in the practice block, it cannot have been that the practice stimuli were to be treated as words.

The alternative explanation is that the learning is occurring at the prelexical level. What participants are learning about is the probability of sequences of phones. In Experiment 2, participants heard *blish* and *bruss* eight times each in the practice block, and then heard unambiguous *bliss* and *brush* and ambiguous *bli?* and *bru?* during the experiment proper. When they began the experimental trials they had already revised their expectation of the probability of hearing [ʃ] following [blI]. In accordance with this revised probability, *bli?* would therefore tend to be analyzed, at the prelexical level, as ending in [ʃ] rather than [s]. Note that *bliss*, however, contained a clear [s] and so would be relatively immune to any bias (i.e., its final sound would be analyzed as [s]). It is thus possible that repetitions of *bliss* during the main experiment could slowly alter the listener's expectations about the most likely sound to follow [blI]. But this could only happen slowly as there were only seven occurrences of *bliss* during the first block of trials. Crucially, these trials were intermixed with three times as many occurrences of *bli?* (collapsing over the three ambiguous fricative steps). What listeners could learn on these ambiguous trials was that, at a prelexical level, [*?*] was to be interpreted as [ʃ] in the context of [blI]. That is, listeners were not just learning what follows [blI] but how to interpret [blI?]. The practice trials teach the prelexical level that [ʃ] is the most likely phoneme to follow [blI]; this causes the prelexical level to identify [blI?] as *blish* in the main experiment.

Note that explicit identification of the final fricative of [blI?] would involve combining this prelexical knowledge, modified by the composition of the practice trials, with lexical knowledge. In order to explain the pervasive lexical bias on fricative identification, therefore, we have to assume that the lexical bias is larger than the statistical bias that changes stop identification. This is certainly consistent with the fact that while the lexical bias is extremely robust, the compensation for coarticulation effect is very fragile.

The listeners in Experiment 2 also heard [*?*] in *bru?*, and they heard *bruss* eight times in the practice block. Following the same reasoning as before, the prelexical level should come to identify the [*?*] in *bru?* as [s]. This differential learning across contexts, we argue, resulted in the reversed compensation effect observed in Experiment 2. We suggest that the same prelexical learning argument holds for the other two experiments. In Experiment 1, the practice trials encouraged transitional probability learning about [*?*] that happened to coincide with lexical biases, and hence an apparently lexically-mediated compensation effect. In Experiment 3, there was no bias in the practice block, so there was no prelexical learning about [*?*], and hence no compensation for coarticulation effect after the ambiguous fricatives.

This account is based on two assumptions. First, prelexical processing must be open to change, and those changes must occur quickly. A number of studies of perceptual learning in speech (e.g., Bertelson, Vroomen, & de Gelder, 2003; Norris, McQueen, & Cutler, 2003) indeed suggest that listeners can use lexical or visual constraints to readjust their perception of ambiguous phones, and can do so very rapidly (within as few as 10 trials; Kraljic & Samuel, 2007). The second and only other assumption required to explain the present data is that listeners can also adjust their perception of ambiguous speech sounds on the basis of the transitional probabilities set up in the practice phase.

Transitional probability

Several studies have shown that listeners are sensitive to phoneme-sequence constraints (McQueen, 1998; Onishi, Chambers, & Fisher, 2002) and to phonetic transitional probabilities (van der Lugt, 2001; Pitt & McQueen, 1998; Vitevitch & Luce, 1998, 1999), and critically that this latter sensitivity has a prelexical locus. For example, Pitt and McQueen showed that transitional probability biases on ambiguous fricatives in nonword contexts could induce a compensation for coarticulation effect on following stops. While these effects are based on exposure to the listener's language as a whole rather than a short practice block, they do suggest that the prelexical level can respond to differences in sequential dependencies. Prelexical adjustments based on lexical knowledge is fast (e.g., Kraljic & Samuel, 2007), as is learning about experiment-internal phoneme-sequence constraints (Onishi et al., 2002). It is thus reasonable to assume that adjustments based on practice-internal biases are also rapid. Furthermore, the idea that the prelexical level is sensitive to changing sequential probabilities is consistent with recent Bayesian accounts of speech recognition (Clayards, Tanenhaus, Aslin, & Jacobs, 2008; Feldman & Griffiths, 2007; Norris & McQueen, 2008).

When we eliminated the bias in the practice phase of Experiment 3, there was no sign of a lexically-mediated compensation effect. In other studies, however, there was no bias in the practice trials and local sequential probabilities (e.g., diphone transitional probabilities) were controlled, and yet there was apparent lexical mediation of compensation (Elman & McClelland, 1988, Experiment 3; Samuel & Pitt, 2003). It is possible that these are genuine lexical effects, but they are also open to an alternative explanation: The materials had longer-range transitional probability biases. In the [s]-final lexical contexts in the Elman and McClelland materials, the quadraphone sequences ending in [s] are much more frequent than those ending in [ʃ], and the reverse is true for the [ʃ]-final lexical sequences (see Pitt & McQueen, 1998). A stronger quadraphone bias occurs for the pairs of words which produced significant lexical compensation effects in the Samuel and Pitt study (*extinguish–contagious* and *distinguish–consensus*). According to the CELEX database (Baayen, Piepenbrock, & Gulikers, 1995), the sequences of four sounds at the end of *contagious* and *consensus* occur in these and other words, but the equivalent sequences ending in [ʃ] (e.g. [nsəʃ]) do not occur in English. Likewise, the quadraphone sequence at the end of *extinguish* and *distinguish* occurs in these and similar words, but the [s]-final equivalents do not. The apparent lexical effects in both studies (Elman & McClelland, 1988; Samuel & Pitt, 2003) may thus be due to long-range transitional probability biases. Until this possibility has been ruled out it would seem unwise to attempt to build a case for feedback on such weak foundations, especially given the evidence against feedback presented here.

It is important to note, however, that the computation of transitional probabilities (and hence the design of experiments examining or controlling for transitional probabilities) is fraught with problems. Magnuson et al. (2003a), for example, make the case that the diphone probabilities in the original Elman and McClelland (1988) materials were not biased in the way that Pitt and McQueen (1998) and Cairns et al. (1995) had claimed that they were. One problem here is that analyses of different speech corpora can lead to differences in estimates of transitional probabilities. Another problem concerns how the corpora have been transcribed (see Magnuson et al., 2003a, 2003b, and McQueen, 2003, for further discussion). A third problem is that speech corpora (no matter how they are transcribed or how large they are) can only provide estimates of listener knowledge. Ideally, one would want measures of the sequencing constraints that individual listeners have acquired. Fourth, there is the issue of how the estimates are computed (e.g., forward vs. backward conditional probabilities, or sequence frequencies). Cairns et al. (1995), for example, computed vowel-fricative probabilities conditional on fricative probabilities (i.e., backward transitional probabilities). They did so in order to normalize for the fact that [s] occurs much more frequently in English than [ʃ]. Magnuson et al. (2003a) argue, however, that backward transitional probability is the incorrect statistic. Finally, and most importantly, not enough is known about what kinds of sequential dependencies listeners are actually sensitive to.

All of these problems make it difficult to be fully confident that all transitional probability biases have been controlled (or appropriately manipulated) in the experiments on lexical effects in compensation for coarticulation. These problems also make it unclear whether transitional probability sensitivity can consistently explain both the presence of apparent lexical effects in some studies (e.g., in Elman & McClelland, 1988, Experiment 3) and their absence in others (e.g., in Pitt & McQueen, 1998; see Magnuson et al., 2003a, 2003b, for discussion). Further research is thus required to establish what kinds of sequential constraints (e.g., short- vs. long-range transitional probabilities) listeners are sensitive to, and whether such sensitivities can explain the full range of data on compensation for coarticulation without the need to postulate any lexical feedback.

No lexical–prelexical feedback

We have presented three arguments in support of the conclusion that there is no lexical–prelexical feedback. First, we have shown that the Magnuson et al. (2003a) results are likely to be due to an experiment-internal bias, acquired during the practice phase of their experiment. Therefore neither the Magnuson et al. (2003a) findings, nor the more recent Magnuson et al. replication mentioned in the introduction, nor those from the present Experiment 1, can be taken as support for feedback. Second, unless it can be shown that the results of Elman and McClelland

(1988, Experiment 3) and Samuel and Pitt (2003) are not due to long-range transitional probabilities, there is thus no data from the compensation for coarticulation paradigm that can be taken as unambiguous evidence of lexical–prelexical feedback.

The third and strongest argument against feedback, however, is based on our repeated demonstrations of dissociations in lexical effects, of the sort first observed by Pitt and McQueen (1998). Samuel and Pitt (2003) argued that that original dissociation could be explained through the effects of perceptual grouping. They argued that if the ambiguous fricative were perceived as grouping with preceding context to form a word, then there could be a strong lexical effect on interpretation of the fricative itself (e.g., hearing *bli?* as *bliss*), and, for the very same reason, no influence of the fricative interpretation on stop identification. We agree that perceptual grouping is an important influence on compensation effects; it was thus controlled here. Gap duration was identical for the words ending with unambiguous fricatives (where compensation effects were consistently observed) and for the stimuli ending with ambiguous fricatives (where compensation effects were far from consistent). One might argue, however, that grouping will not be the same for ambiguous and unambiguous stimuli. Indeed, as Samuel and Pitt showed, fricatives are more likely to group with the preceding contexts (and thus modulate compensation less) in words than in nonwords. The unambiguous fricatives in the present experiments (which always formed words) should thus, if anything, group more with their lexical contexts than the ambiguous fricatives (which form less word-like sequences) and be less likely to lead to compensation. Given this, and the same gap durations, there are no grounds to dismiss the absence of compensation effects with ambiguous fricatives as being the result of perceptual grouping.

The current dissociations are in fact stronger evidence than those observed in Pitt and McQueen (1998). One of the ambiguous fricatives (Step 11) was associated with an apparent lexical compensation effect in Experiment 1, but in Experiments 2 and 3 it was associated with dissociations. Another (Step 13) was associated with dissociations in Experiments 1 and 3, but with a reverse lexical compensation effect in Experiment 2. Since exactly the same stimuli were presented in all three experiments (only the practice sessions differed), and those stimuli sometimes produced compensation effects, the dissociations found with them cannot be due to failures of the fricatives to group sufficiently with the following stops.

The weight of evidence from fricative-stop compensation for coarticulation is therefore currently against there being feedback. This conclusion is supported by other recent findings (Frankish, 2008; Mitterer, 2007). It is also supported by theoretical arguments. Lexical–prelexical feedback does not benefit word recognition, and can harm phoneme recognition (Norris et al., 2000). Although feedback can make the TRACE model perform better (Magnuson, Strauss, & Harris, 2005), this is because the standard implementation of the model does not recognize words optimally (Massaro, 1989). There is no way that lexical–prelexical feedback can make a feedforward model perform better because the feedforward architecture is already exactly what is required for optimal recognition performance. Hence, from the perspective of a feedforward model, the only thing feedback can do is make speech recognition suboptimal (see the exchange between McClelland et al., 2006 and McQueen, Norris, & Cutler, 2006a, and, for further elaboration, Norris & McQueen, 2008).

Feedback for learning

There is, however, more than one way in which information might feed back from the lexical to the prelexical level. The primary debate in the literature has been the one that we have addressed here: Is there on-line feedback? That is, is there a continuous bidirectional flow of information between lexical and prelexical levels that alters the way the current input is processed? There is, however, another possibility: feedback for learning (Norris et al., 2003). Listeners can use information from the lexicon to adjust their prelexical categories over time. In contrast to on-line feedback, this kind of feedback can assist word recognition in that it can help listeners retune their perceptual categories to adjust to unusual input (e.g., from speakers of an unfamiliar regional accent). This form of feedback alters *future* processing but does not alter the perception of the *current* input. Norris et al. demonstrated feedback for learning by presenting listeners with an ambiguous sound, midway between [f] and [s], in lexical contexts which indicated for one group that the sound should be interpreted as [f] and for a second group that it should be interpreted as [s]. Listeners used the lexical information to adjust the way they analyzed the ambiguous sound. Those in the first group identified more sounds on an [f]–[s] continuum as [f] than those in the second group. McQueen, Cutler, and Norris (2006b) have subsequently shown that this kind of perceptual learning generalizes to the recognition of words that did not form part of the training. This indicates two things: That the learning does indeed benefit subsequent word recognition (i.e., the recognition of new words spoken by the same speaker), and that the learning has a prelexical locus (adjustments at the prelexical level will influence all words containing the adjusted sounds).

The possibility of feedback for learning adds another dimension of complexity to the interpretation of data purporting to support on-line feedback. Two studies by Samuel (1997, 2001) investigated on-line feedback using a selective adaptation paradigm (where repeated presentation of a speech sound causes a shift in identification of similar sounds away from the adapting sound; Eimas & Corbit, 1973). The logic of these studies was the same as that in the compensation for coarticulation case: If the lexicon influences selective adaptation, which is assumed to be prelexical, there must be feedback. However, Norris et al. (2003) and McQueen et al. (2006a) have argued that the selective adaptation effects reported by Samuel were mediated by perceptual learning, and not by on-line feedback. A reanalysis of the Samuel (2001) data by Vroomen, van Linden, de Gelder, and Bertelson (2007) indeed suggests that there was a perceptual retuning process in the first block of the experiment (i.e., after 32 adaptor trials), and a selective adaptation effect (contingent on the perceptual learning effect) arising thereafter. In the Samuel (1997) study there were 84 adaptor trials prior to the first test block, so there was ample time for selective adaptation to develop based on prior perceptual learning. Thus, while the Samuel (1997 and 2001) studies appeared, like the original Elman and McClelland (1988) study, to be critical tests of on-line feedback, they too are open to an alternative explanation.

Feedback for perceptual learning is motivated because, unlike on-line feedback, it is of benefit for spoken-word recognition. This leads to a different way of thinking about purported evidence for on-line feedback (Norris et al., 2003; McQueen et al., 2006a). It appears that no convincing data for feedback during speech perception are currently available. If such data were to be found, however, then this should best be taken as evidence that the mechanism responsible for feedback for speech learning also influences on-line processing. That is, on-line feedback (which itself is of no benefit to speech recognition) might arise as an epiphenomenon of the useful (indeed necessary) perceptual learning process. If so, then things would come full circle, since feedback in TRACE was originally motivated in part to serve a perceptual learning function (McClelland & Elman, 1986).

Conclusions

We draw three conclusions. First, the compensation for coarticulation paradigm may have outlived its usefulness in the on-line feedback debate. Is it perhaps time to stop playing the *Christmas tapes* and move on? It would be a brave researcher indeed who would attempt to control short- and long-range transitional probabilities, perceptual grouping, stimulus quality, word length and possible experiment-internal biases in order to then probably fail to find replicable lexical mediation of compensation for coarticulation. While we would much prefer that researchers accept our arguments that lexical feedback has no useful function in on-line processing, if they really do want to pursue this question empirically, we strongly recommend that they follow the lead of Samuel (1997, 2001) and try to do so by testing for lexical influences on a prelexical mechanism other than compensation for coarticulation.

Second, we have shown that speech perception is influenced by experiment-internal biases that can arise from only 16 practice trials. This is a striking demonstration both of the flexibility of the perceptual system and of its sensitivity to phone sequence probabilities. In the context of other recent work on perceptual learning, sequence probabilities are simply another form of training signal (beyond the signals provided by the lexicon, Norris et al., 2003, or by visual input, Bertelson et al., 2003) that can rapidly be used to retune phonetic perception.

Third, the dissociations we observed between the compensation for coarticulation effect and the lexical effect on fricative identification challenge interactive models of speech perception. According to TRACE (McClelland & Elman, 1986), the lexical bias on fricative identification comes about through top–down activation of the lexically-consistent phoneme nodes representing the word-final fricative. This, in turn, should trigger compensation for coarticulation by altering the activation of the following word-initial stop consonant. That is, the lexical effect on the fricative and the compensation effect on the stop should always go hand in hand. But they do not. In our experiments there was always a lexical effect on fricative identification, but the compensation effect could go in either direction, as determined by the composition of the practice trials. Not only was compensation not triggered by a lexical influence on the fricative, but it was quite independent of any lexical influence. Compensation for coarticulation thus appears to be an autonomous prelexical process that is not subject to modification by top–down feedback. The present results therefore do much more than remove the main pillar of support for interaction, they provide compelling evidence that prelexical processes are immune to the influence of the lexicon. That is, there is no feedback during speech perception.

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Appendix A. Pilot studies 1a and 1b

A.1. Method

A.1.1. Participants

Twenty-four paid native speakers of British English from the participant pool of the MRC Cognition and Brain Sciences Unit, Cambridge, were tested: 15 in Pilot 1a and 9 in Pilot 1b. All reported having normal hearing and no language disorders.

A.1.2. Stimuli

A male native British speaker was recorded saying the precursor contexts *bli* and *bru*, the words *tapes* and *capes*, and the fricatives [s] and [ʃ]. Recordings were downsampled to 22.05 kHz. A pair of *bli* and *bru* tokens was cut to be of approximately similar duration (239 ms). Similarly, selected [s] and [ʃ] sounds were cut to be of similar length (233 ms) and adjusted to be of similar amplitude (58 dB). A 39-step continuum was created from these endpoints using the waveform averaging technique (see Pitt & McQueen, 1998). A pilot phonetic categorization test determined that Steps 20 (44% [s] responses) and 22 (53% [s] responses) were the most ambiguous. These two steps were spliced onto the *bli*- and *bru*- contexts.

A pair of *tapes* and *capes* tokens was altered to be of same amplitude (70 dB). As in Magnuson et al. (2003a), [teI] and [keI] fragments were cut out of the *tapes* and *capes* tokens. For [keI] that consisted of the stop plus the initial four vocalic pitch periods (82.99 ms). [teI] (82.95 ms) was cut to be of similar length to [keI]. A 39-

step continuum was made by digital averaging. A phonetic categorization pilot showed that the steps with 45%, 47.5%, 50%, 52.5%, 55%, 57.5%, and 60% [k] were best for creating an ambiguous range of test items. These seven steps, plus the two endpoints [teI] and [keI], were concatenated with the remaining [eIps] from the *capes* token, before being attached to the *bliss*, *brush*, *bli20*, *bli22*, *bru20*, and *bru22* precursors to make a total of 54 stimuli. There was no gap between the precursors and the *tapes–capes* tokens. For Pilot 1b, the only change was that the onset of the [t]–[k] tokens (i.e., from the end of closure silence) was ramped from 0% to 100% over the first 30 ms.

A.1.3. Procedure

The experiment was controlled by DMDX software (Forster & Forster, 2003). Participants were tested individually in a quiet room. They were instructed that on each trial they would hear over headphones word pairs where the first word ended in either [s] as in *mess* or [ʃ] as in *mesh*, and the second word started with either [t] as in *toll* or [k] as in *coal*. Their task was to indicate as quickly and as accurately as possible which sound combination they heard by pressing one of four buttons, labeled ‘s t’, ‘s k’, ‘sh t’, and ‘sh k’. The spatial arrangements of response labels were counterbalanced across participants. Each trial began with the presentation on a computer screen of the four labels for 800 ms, followed by stimulus presentation. Once a response was given or 4 s had passed, a black screen was presented for 700 ms before the next trial began. The 14 practice trials consisted of a random order of the two precursors *bliss* and *brush* each combined with all seven steps of the *tapes–capes* continuum. The main experiment consisted of 324 trials in six blocks, each containing all 54 stimuli. Presentation order within each block was randomized for each participant individually. The procedure was identical for Pilots 1a and 1b.

A.2. Results

The mean proportion of [s] responses over trials containing the five mid steps of the stop continuum were analyzed as a function of lexical context (see Table A1). The unambiguous fricatives were reliably identified in both pilots. Furthermore, the ambiguous fricatives were identified as lexically consistent in both experiments (i.e., more [s] responses were given in the *bli*- than in the *bru*- context). There was no evidence for compensation for coarticulation in stop labeling in either pilot, following neither unambiguous nor ambiguous fricatives. That is, in spite of the lexical effect on ambiguous fricative labeling, there was no lexically-mediated compensation. There was a small reverse trend in Pilot 1a (i.e., more [k] responses after a *bru*- than after a *bli*- context). For Pilot 1b, a lexically-driven compensation effect on stop identification was found after one of the ambiguous fricatives (Step 1).

Table A1
Pilot experiments: mean percentage differences in fricative ([s]) and plosive ([k]) labeling in unambiguous and ambiguous fricative contexts

Experiment	Fricative labeling			Plosive labeling		
	Unambiguous fricatives (%)	Ambiguous fricatives (%)		Unambiguous fricatives (%)	Ambiguous fricatives (%)	
		Step 1	Step 2		Step 1	Step 2
Pilot 1a	93	71	68	−4	−5	−5
Pilot 1b	90	66	70	−3	7	1
Pilot 2a	91	79	76	−2	−1	−4
Pilot 2b	91	73	75	−1	−9	−5

Notes: Because the differences are the mean responses for the *bli*- context minus those for the *bru*- context, larger positive numbers indicate a stronger tendency for lexically-consistent labeling. For Pilots 1a and 1b, Steps 1 and 2 correspond to Steps 20 and 22 in the original 39-step fricative continuum. For Pilots 2a and 2b, Steps 1 and 2 correspond to Steps 50 and 60 in the continuum made by Magnuson et al. (2003a).

Appendix B. Pilot studies 2a and 2b

B.1. Method

B.1.1. Participants

Twenty-seven undergraduates from the University of California, Santa Cruz, participated for course credit (12 in Pilot 2a, 15 in Pilot 2b). All participants were native speakers of American English and reported no hearing or language problems.

B.1.2. Stimuli

The materials were those presented in Magnuson et al. (2003a).

B.1.3. Procedure

This was almost identical to that in Pilots 1a and 1b. One difference was that a keyboard rather than a button box was used for response collection. Red stickers indicated the response keys (A, S, K, and L). Labels above those keys indicated response choices. The only difference between Pilots 2a and 2b were the instructions. Pilot 2a used the same instructions as Pilots 1a and 1b. Pilot 2b used the original instructions of the Magnuson et al. (2003a) study. Specifically, they were: ‘‘In this experiment, you will hear pairs of words, such as ‘‘*brush tapes*” or ‘‘*bliss capes*”. After you hear each pair, your task is to identify the last sound of the first word and the first sound of the second word. For example, if you hear ‘‘*brush tapes*”, you should press the key marked ‘‘sh t”, since ‘‘*brush*” ends with ‘‘sh” and ‘‘*tapes*” begins with ‘‘t”. If you hear ‘‘*brush capes*”, you should press ‘‘sh k”, since ‘‘*capes*” begins with a ‘‘k” sound. If you hear ‘‘*bliss tapes*”, press ‘‘s t”. If you hear ‘‘*bliss capes*”, press ‘‘s k”. Some of the items may not be pronounced clearly. After each pair, just press the key that corresponds best to what the last sound of the first word and first sound of the second word sounded like. The best thing to do is to put one finger on each key. The labels will also appear on the screen, so you won’t have to look at the keys. By keeping one finger on each key, you will be able to finish the experiment much more quickly. After you press a key, the next trial will begin immediately. About every 5 to 7 min, you will have a chance to take a break. A message will appear on the screen telling you to take a break. Breaks are rather short so please don’t take off the headphones. You will be listening through the headphones. You may adjust the volume using the volume control on the speaker or amplifier. Please tell the experimenter you have finished the instructions. Feel free to ask any questions".

B.2. Results

In Pilot 2a, one participant was excluded due to equipment failure. In Pilot 2b, two participants were excluded (one did not show sensitivity to the [t]–[k] continuum; the other did not show evidence of lexical involvement in fricative labeling). The results replicated those found in the previous pilots (see Table A1). Unambiguous fricatives were reliably identified. Ambiguous fricatives tended to be identified as lexically consistent. Despite this lexical involvement in fricative labeling, the lexicon did not seem to influence plosive identification. Instead there was a trend towards a reverse lexical effect in both pilots. As in Pilots 1a and 1b, there were again trends towards reverse compensation effects after unambiguous fricatives (i.e., more [k] responses after *brush* than after *bliss*).

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