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The effects of preceding lead-alone and lag-alone click trains on the buildup of echo suppression

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Spatial perception in echoic environments is influenced by recent acoustic history. For instance, echo suppression becomes more effective or "builds up" with repeated exposure to echoes having a consistent acoustic relationship to a temporally leading sound. Four experiments were conducted to investigate how buildup is affected by prior exposure to unpaired lead-alone or lag-alone click trains. Unpaired trains preceded lead-lag click trains designed to evoke and assay buildup. Listeners reported how many sounds they heard from the echo hemifield during the lead-lag trains. Stimuli were presented in free field (experiments 1 and 4) or dichotically through earphones (experiments 2 and 3). In experiment 1, listeners reported more echoes following a lead-alone train compared to a period of silence. In contrast, listeners reported fewer echoes following a lag-alone train; similar results were observed with earphones. Interestingly, the effects of lag-alone click trains on buildup were qualitatively different when compared to a no-conditioner trial type in experiment 4. Finally, experiment 3 demonstrated that the effects of preceding click trains on buildup cannot be explained by a change in counting strategy or perceived click salience. Together, these findings demonstrate that echo suppression is affected by prior exposure to unpaired stimuli.
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I. INTRODUCTION

Sound localization plays an important role in day-to-day communication, particularly in crowded settings such as a cocktail party or meeting (Cherry, 1953). Fortunately, a listener's brain can exploit many acoustic cues, including spatial cues, to segregate a target talker's speech from the background noise (Cherry, 1953; Bregman, 1994; Freyman *et al.*, 1999; Kidd *et al.*, 2005; Shinn-Cunningham, 2008). However, spatial cues are often corrupted in everyday listening environments by echoes from acoustically reflective surfaces such as walls, ceilings, and floors. Echoes not only impact auditory localization (Rakerd and Hartmann, 1985), but also dramatically reduce speech intelligibility for both healthy and hearing impaired listeners (Plomp, 1976; Cranford and Romereim, 1992; Marrone *et al.*, 2008). Despite diminished listening abilities in these circumstances, the auditory system can often "suppress" echoes, making listeners perceptually unaware of the reverberations. In light of its putative importance to human communication in realistic environments, acoustic and crossmodal parameters that affect echo suppression have been studied extensively (Litovsky *et al.*, 1999; Bishop *et al.*, 2011; London *et al.*, 2012; Brown and Stecker, 2013). However, despite decades of rigorous parametric explorations, our mechanistic understanding remains limited.

Echo suppression is a key component of the precedence effect, an umbrella term used to describe several phenomena through which successive sounds (e.g., a "precedent" sound wave and its corresponding echoes from nearby surfaces) interact to yield a listener's spatial experience (Wallach *et al.*, 1949; Litovsky *et al.*, 1999). These phenomena also include localization dominance and discrimination suppression (Litovsky *et al.*, 1999), but the current set of experiments focuses on echo suppression (also referred to as "fusion" or "echo fusion"): that is, when a primary sound wave and any short-latency echoes are fused into a single auditory image. More specifically, echo suppression occurs when an echo's spatial information is combined or "fused" with that of the temporally leading sound. This is often studied in a laboratory setting by presenting listeners with an identical sound from two spatially distinct locations, such as from two free-field loudspeakers or through headphones, and delaying the temporal onset of the second sound (i.e., the echo or "lag sound") relative to the onset of the first (i.e., the primary or "lead sound"). When the two sounds are presented at precisely the same moment in time, listeners report hearing a single sound image located between the two locations (e.g., directly between two loudspeakers) (Zurek, 1980; Shinn-Cunningham *et al.*, 1993; Litovsky *et al.*, 1999; Bishop *et al.*, 2011). As the temporal delay increases from 0 ms to ~1 ms, listeners perceive a single sound image that moves progressively closer to the location of the leading sound source. Listeners continue to hear a single sound image at or near the lead location as the lead-lag delay increases until it reaches a listener's "echo threshold." Once the temporal delay reaches or exceeds echo threshold, listeners begin to report two spatially distinct sounds: one at the lead location and a second closer to or directly at the lag location [but see Brown and Stecker (2013) for a counterexample]. Although echo thresholds vary considerably across listeners, stimuli, and tasks, thresholds for isolated click pairs tend to fall between 2–5 ms. Intriguingly, although the

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echo's spatial information is largely suppressed by the lead sound wave from 1–5 ms, listeners remain perceptually aware of the presence of a second sound through changes in perceived loudness, timbre, and other qualities of the fused image. Thus, echo suppression is a phenomenon through which an echo's spatial information is either fused with or "suppressed" by the leading sound wave while non-spatial information may remain largely intact. Echo suppression is most intimately linked to the relative timing between two sounds—that is, echo suppression is more likely when the lead-lag delay is relatively short and deteriorates at longer delays—but other acoustic attributes contribute as well, including spatial separation, cross-frequency overlap between the lead and lag sounds, and perhaps most importantly, acoustic history (Clifton and Freyman, 1989; Grantham, 1996; McCall *et al.*, 1998; Litovsky *et al.*, 1999; Djelani and Blauert, 2001; Dimitrijevic and Stapells, 2006; Freyman and Keen, 2006; Keen and Freyman, 2009).

In a clear example of the importance of acoustic history, echo suppression "builds up" under laboratory settings with repeated exposure to identical lead-lag pairs. Specifically, when a listener is presented with a train of lead-lag sound pairs with a fixed lead-lag delay she might initially hear both the lead sound and its simulated echo from two spatially distinct locations. However, the echo perceptually "fades out" as the train progresses leaving a single, fused sound image at or near the location of the lead sound (Litovsky *et al.*, 1999). The "buildup" of echo suppression putatively reflects an intrinsic ability of the human brain to create and maintain a model of the current listening environment—a "room acoustics model" that should improve listener performance in real reverberant situations (Clifton and Freyman, 1989; Freyman and Keen, 2006; Keen and Freyman, 2009; Brown and Stecker, 2013). Interestingly, recent evidence has demonstrated that prior exposure to a reverberant listening environment can improve sound localization and speech intelligibility (Brandewie and Zahorik, 2010, 2013; Srinivasan and Zahorik, 2013). Although speculative, these findings may suggest that buildup of echo suppression is behaviorally meaningful and consequently lend support to the room acoustics hypothesis. Thus to date, the room acoustics hypothesis is the most comprehensive conceptual framework of buildup and it has been widely successful in providing intuitive explanations for empirical observations [see Keen and Freyman (2009) for recent review]. Notice that while the central tenet of the room acoustics hypothesis holds that a listener quickly establishes acoustic expectations, an important corollary posits that acoustic information that is *unexpected* (e.g., a change in lead-lag delay) causes the previous acoustic model to quickly break down and a new set of expectations to be established (Freyman and Keen, 2006; Keen and Freyman, 2009). A well-characterized empirical observation that demonstrates these phenomena is how lead-alone click trains affect the buildup of echo-suppression.

Exposing a listener to a lead-alone click train after buildup is established leads to a dramatic reduction in echo thresholds or, equivalently, a "breakdown" of buildup (Keen and Freyman, 2009). Intuitively, this manipulation simulates a listener moving from a reverberant listening environment to an anechoic listening environment. Interestingly, echo thresholds following a lead-alone click train decrease to well below control levels when listeners are presented with a sufficient number of lead-alone clicks [cf. Fig. 3 in Keen and Freyman (2009) and Fig. 8 in Freyman *et al.* (1991)]. This suggests that a lead-alone click train does not only "breakdown" buildup after it is established, but also makes a listener hypersensitive to lag sounds in subsequent lead-lag test pairs. Investigators have suggested that this hypersensitization is due to a change in "contrast" between listening conditions (Freyman *et al.*, 1991; Keen and Freyman, 2009). In other words, the lag sound of lead-lag pairs becomes more perceptually salient or "surprising" when it has been recently absent from the listening environment. A recent investigation into the effects of a lead-alone click train *prior* to establishing buildup—that is, when a lead-alone click train is presented before buildup—has provided additional support for the importance of "contrast" in echo suppression (Sanders *et al.*, 2011). Specifically, Sanders *et al.* (2011) demonstrated that buildup is less robust or "depressed" when a lead-lag click train is presented after a lead-alone click train; this suggests that lag clicks may be more perceptually salient or more "surprising" following a lead-alone click train than with buildup alone. Together, these reports suggest that a lead-alone click train can have a powerful effect on buildup in two ways: (1) a lead-alone click train can breakdown established buildup and (2) buildup becomes less robust following a lead-alone click train, likely due to the high contrast between lead-alone clicks and lead-lag click pairs. Despite the well-documented contributions of lead-alone click trains to buildup, no published studies to date have investigated the role of lag-alone click trains—that is, a click train presented from only what would be the lag location of a lead-lag pair—on this phenomenon [but see Freyman *et al.* (1991)]. Considering the insight gained from studying the effects of lead-alone clicks on buildup and its intuitive explanation within the context of the room acoustics hypothesis, we thought it likely that exploring how a lag-alone click train affects buildup would help refine the conceptual framework and better characterize the importance of acoustic contrast in buildup.

The effects of a lag-alone click train on buildup cannot be easily anticipated based on previous studies or existing conceptual frameworks. For instance, it is unclear what effect a lag-alone click train will have on buildup within the context of the room acoustics hypothesis since a lag-alone train does not relate intuitively to any plausible listening environment. Will a lag-alone click train enhance buildup? An enhancement of buildup might be expected based on the notion of a change in acoustic contrast between listening conditions. Specifically, when listeners are first presented with a lag-alone click train before buildup is established, the lag click in the buildup train could become less perceptually salient or less "surprising" and result in an enhanced buildup effect. Alternatively, does a lag-alone click train result in a depression in buildup similar to a lead-alone click train? While this outcome is inconsistent with predictions based on changes in contrast, it is consistent with previous findings demonstrating qualitatively similar effects of lead- and lag-alone click trains on echo suppression in the absence of buildup [cf. Fig. 8 Freyman *et al.* (1991)]. Or, will the lag-alone click train have no measurable effect on buildup, suggesting that the lag-alone stimulus does not contribute to or interfere with a listener's ability to establish listening expectations? This outcome might be expected if the lag-alone click train is simulating an implausible and therefore uninformative listening scenario (one with just an echo present). Each of these outcomes is plausible based on previous studies and would have significantly different implications for our understanding and theoretical framework of buildup. Consequently, an empirical study is warranted to help situate the contributions of a lag-alone click train within the broader literature on the topic of buildup.

The current experiments therefore explore how unpaired click trains, particularly lag-alone trains, affect subsequent buildup. In experiments 1 and 2, we investigate the role of unpaired click trains on subsequent buildup in free-field and dichotic (over earphones) listening conditions, respectively. In experiment 3, we explore whether the effects demonstrated in experiments 1 and 2 can be explained by a change in task strategy or a generalized change in perceived click saliency (e.g., through a neural or perceptual adaptive process). In experiment 4, we extend observations made in experiment 1 (free field) by exploring the contributions of across-trial effects and the baseline to which lead- and lag-alone conditions are compared, all while providing more precise control over a listener's acoustic and response history.

II. EXPERIMENT 1

The primary goal of experiment 1 was to explore the effects of lead-alone and lag-alone click trains on subsequent buildup in free field. We coupled a condition → probe design with a subjective counting task inspired by a previous study (Clifton and Freyman, 1989).

A. Methods

1. Listeners

In accordance with policies and procedures approved by the University of California Davis Institutional Review Board, a total of 14 listeners [10 female, 21 ± 2.3 (SD) years of age] participated in experiment 1. All listeners were naive to the goals of the study, had self-reported normal hearing, were in good health, gave written consent prior to their participation, and were monetarily compensated for their participation. All listeners met a behavioral cutoff during the training session (see Sec. II A 4); consequently, all results presented here are based on all 14 listeners.

2. Experimental setup

Listeners sat in an acoustically shielded room (height × length × width of 2.64 m × 3.4 m × 2.43 m) in an adjustable chair with a custom-built headrest. The chair was positioned in the approximate center of the room facing a computer monitor placed on a wire rack approximately 120 cm from the listener's brow [Fig. 1(A), left panel]. Two Tannoy Precision 6 free-field loudspeakers were positioned symmetrically along the length of the room ~1.1 m from the listener at 45° to the left and right of the midsagittal plane. The height of the chair was adjusted such that each listener's

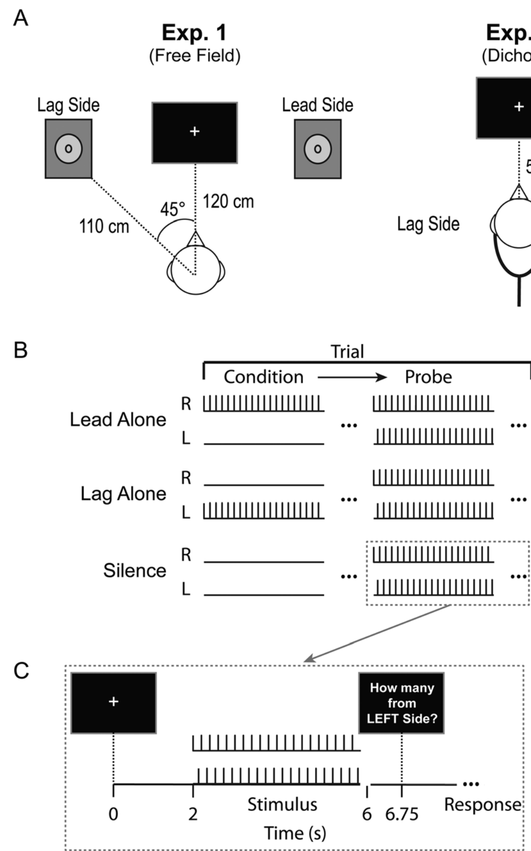

FIG. 1. Setup, trial structure, and stimuli for experiments 1 and 2. (A) Setup. Auditory stimuli were presented either from two free-field loudspeakers (experiment 1) or dichotically through earphones (experiment 2). Free-field loudspeakers were positioned at approximately 45° to the left and right of the midsagittal plane and 110 cm from the listener. Temporally leading sounds were always presented from the right loudspeaker in experiment 1 and right earphone in experiment 2. The second sound (i.e., the lag sound or "echo") was always presented from the left loudspeaker and left earphone. (B) Stimuli. Experiments 1 and 2 employed a condition → probe design to study the effects of lead- and lag-alone click trains on the buildup of echo suppression. The conditioner (lead alone, lag alone, or silence) presented during the condition phase was held constant throughout a session. In contrast, the lead-lag delay of the buildup probe varied from 1 to 18.5 ms across trials. (R = right, L = left). (C) Trial structure and task. Trials followed a condition → probe design. During the condition phase, listeners were presented with one of three conditioners (lead alone, lag alone, or silence). The condition phase was followed by a probe phase during which listeners were presented with a lead-lag "buildup probe." Each phase began with 2 s of fixation, followed by 4 s of stimulus delivery, followed 0.75 s later by a prompt "How many from the LEFT side?". Listeners were instructed to count how many sounds they heard from the left of the midsagittal plane—that is, how many sounds they heard from the same hemifield as the echo—and to enter their response after both phases of the trial. Listeners entered their responses on a keyboard number pad followed by the "enter" key. Responses were immediately posted to the computer monitor for review by the listener. In the event of a typographical error, listeners were allowed to reinitiate the response period by pressing the "n" key followed by "enter." Consequently, the listeners were allowed as much time as necessary to enter their final response. Panel (C) provides a detailed, graphical depiction of events in the probe phase of one trial; the order of events was identical during the condition phase, although different stimuli were presented.

interaural axis was aligned vertically with the center of the loudspeakers. A computer keyboard (Dell SK-8115) was placed on top of a memory foam pillow in the listener's lap with the keyboard number pad used to collect responses (see below) placed slightly to the right of the midsagittal plane. Although the room's walls were covered with acoustic dampening material to minimize acoustic reflections, recordings using a remote microphone (SHURE KSM 44) analyzed using custom scripts in MATLAB revealed a reflection arriving at the location of the listeners' ears ~4.9 ms after the primary wave. Further analysis of the time waveform in MATLAB revealed that the reflection was attenuated by ~12 dB compared to the primary sound wave. Although clearly visible in recordings using the remote microphone, the reflection was unnoticeable even to experienced listeners. Specifically, all authors and several other auditory specialists each sat in the experimental apparatus with the door closed. A single click was presented from one loudspeaker and listeners were asked to indicate the location of all sound sources. All listeners reported a single sound source at the location of the correct loudspeaker, even when pressed to identify a second source. In light of these observations and others reported in several previously published studies in a nearly identical acoustic environment (Bishop *et al.*, 2011; London *et al.*, 2012), we assume that the room acoustics did not contribute to or interfere with a listener's subjective reports.

3. Stimuli, trial structure, and task

Experiment 1 employed a condition → probe design with three different conditioners: lag alone (clicks presented from only the left loudspeaker), lead alone (clicks presented from only the right loudspeaker), or a temporally equated period of silence (no sound presented). Each of these conditioners preceded a "lead-lag" train with a fixed temporal delay between lead and lag clicks; the lead-lag click trains served as "buildup probes" in this and subsequent experiments. The lead and lag clicks were always presented from the right and left loudspeakers, respectively. The conditioners and the probe consisted of 20, 52 μs click or click pairs presented at 5 click(pairs)/s [Fig. 1(B)]. The output of each loudspeaker was calibrated individually to 53.6 dB(A, slow response) prior to each listener's participation using a hand held sound pressure level meter (RadioShack model 33-2055). Peak intensity was measured regularly using a SHURE KSM 44 microphone and custom scripts in MATLAB to ensure that peak intensity remained at 87.4 dB(A).

Each trial consisted of a conditioner (lead alone, lag alone, or silence), a buildup probe, and two, temporally unconstrained response windows [Fig. 1(C)]. Lead-alone clicks and the lead sound of lead-lag click pairs were always presented from the right loudspeaker while the lag alone and lag sound of click pairs were always presented from the left loudspeaker [Fig. 1(A)]. The condition phase of the trial began with a 2.0 s fixation period, followed by a 4.0 s conditioner, and ended with a temporally unconstrained response window. Listeners were prompted for a response 0.75 s after sound offset by a visual prompt reading "How many sounds from the LEFT side?" Following the listener's response, the same pattern of events repeated during the probe phase, but the conditioner stimulus was replaced with a buildup probe [Fig. 1(B)]. Listeners were instructed to count how many sounds they heard from the left of the midsagittal plane (i.e., how many sounds they heard on the lag or "echo" side) during both the conditioner and probe phases of each trial while maintaining fixation on a small white cross presented via the computer monitor. Listeners entered their response (e.g., "20" for a lag-alone conditioner) via the keyboard number pad, followed by the enter key. Immediately after pressing enter, the listener's response posted to the computer monitor to allow listeners to review their response and correct any typographical errors (e.g., typing "200" instead of "20"). If no errors were made, listeners pressed "enter" once more to initiate the probe phase of the trial or begin the next trial. If an error was made and detected, the listener pressed the "n" key followed by "enter." This reinitiated the initial prompt and the process repeated until the listener was satisfied with her response. Importantly, listeners entered their counts for both the condition and probe phases of each trial. This ensured a well-controlled behavioral set during the conditioning. Specifically, we observed in pilot work that listener counts could become inaccurate (typically decreasing) over time. These inaccurate counts typically occurred for lag-alone conditioners, despite the relative ease of the task, and were likely due to inattention. Thus, if counts to the lag-alone conditioner became grossly inaccurate—signaling a waning of attention—the experimenter could reinvigorate the listener through positive reinforcement or a break. Additionally, we deliberately allowed an open-ended response window and response correction in order to gather accurate responses and allow listeners frequent opportunities to rest should they become fatigued. Although open-ended response windows have been used in previous investigations of buildup, unlike the current paradigm, previous studies have typically controlled the timing between conditioner and probe stimuli (e.g., Freyman *et al.* 1991).

4. Training procedure

The experiment began with an extensive training procedure designed to familiarize listeners with the stimuli and task while providing the experimenter with objective evidence that listeners could perform the counting task employed. First, listeners were presented with a verbally narrated PowerPoint presentation designed to familiarize the listener with the task, how to enter responses, and the stimuli presented during the experiment. Importantly, the experimenter encouraged listeners to count a sound of any type, quality, or loudness (e.g., a click, pop, or faint echo) heard from the left of the midsagittal plane—that is, any sound heard from the echo's hemifield. These instructions were repeated throughout the experiment to improve listener compliance. Listeners were also told that they would likely hear between 0 and 20 sounds on the left side, but were encouraged to report their counts accurately, even if counts exceeded 20.

Following the PowerPoint presentation, listeners completed 1–4 counting sessions designed to give them practice with perceptually unambiguous stimuli. Trials during the counting sessions were nearly identical to those used during the main experiment (see above). Specifically, each trial was comprised of a conditioner (lead alone, lag alone, or silence), a "probe," and two response windows. However, unlike the buildup probe used in the main experiment, listeners were presented with a "hybrid train" during the probe phase of trials; hybrid trains were designed to provide clear examples of 2–18 sounds originating from the left (lag) side at two loudness levels. Hybrid trains began with 2–18 lead-lag pairs (increments of 2) with a fixed lead-lag delay of 30 ms. A 30 ms lead-lag delay was used to ensure that the delay was well above a typical echo threshold—that is, that listeners were unlikely to experience echo suppression or buildup. The 2–18 lead-lag pairs were followed by lead-alone clicks for a total of 20 clicks or click pairs per hybrid train. Lag clicks were either presented at the same intensity as the lead click or attenuated by 16 dB; two intensity levels were used to encourage listeners to count sounds of any intensity during the main experiment. Each counting session consisted of 18 trials: 6 lead-alone, 6 lag-alone, and 6 silence conditioners each followed by one of eighteen unique hybrid trains. Listeners were provided with informal feedback after each counting session regarding their performance via a computer generated plot of the number of reported sounds vs number of presented sounds (data not shown). If large errors existed, the experimenter encouraged the listener to try a different counting strategy (e.g., counting in groups of 10 or 5, counting every other click, etc.). Listeners were only permitted to continue once they reported being comfortable with the task and their counts were within three of all but two hybrid trains containing 10–18 lead-lag pairs. All listeners satisfied these behavioral criteria in experiment 1 and generally performed remarkably well despite the fast presentation rate of 5 Hz. Following the completion of the counting sessions, listeners completed two practice sessions identical to those used during the main experiment: one lead-alone session followed by one lag-alone session (see below for details). These two sessions allowed subjects to ask for additional clarification prior to the main experiment. The data collected from these practice sessions were omitted from the final analysis.

5. *Experimental procedure*

The main experiment consisted of two instances each of three different session types for a total of six sessions. Trials within each session contained only one of the three conditioners (lead alone, lag alone, or silence) followed by a buildup probe. The lead-lag delay of the buildup probe was held constant within each train, but varied from 1–18.5 ms in 2.5 ms steps within a session; each lead-lag delay was presented twice per session and randomized across trials and listeners. Session order was pseudorandomized such that all three session types were presented in the first three sessions; the same session order was repeated for the remaining three sessions for a total of six sessions. For example, an individual listener might be presented with sessions in the order of silence, lead alone, lag alone during the first half of the experiment and this session order would then be repeated for the second half of the experiment. Following the conclusion of the experiment, the experimenter asked listeners a series of questions designed to qualitatively confirm the presence of buildup during the buildup probe: (1) Did sounds on the left side ever fade out over time? (2) Did sounds on the left side ever fade in over time? (3) Did sounds on the left side ever fade out then in? Listeners were monitored throughout the session via a remote camera and microphone to ensure they were performing the task and maintaining fixation.

B. Results

The percentage of reported lag sounds during the probe phase vs lead-lag delay is plotted in Fig. 2(A). In agreement with previous literature, the percentage of reported lag sounds increased monotonically with increasing lead-lag delay in all three session types (lead alone, lag alone, silence) and most listeners reported that the echo faded out as the buildup probe progressed (see Table I). Additionally,

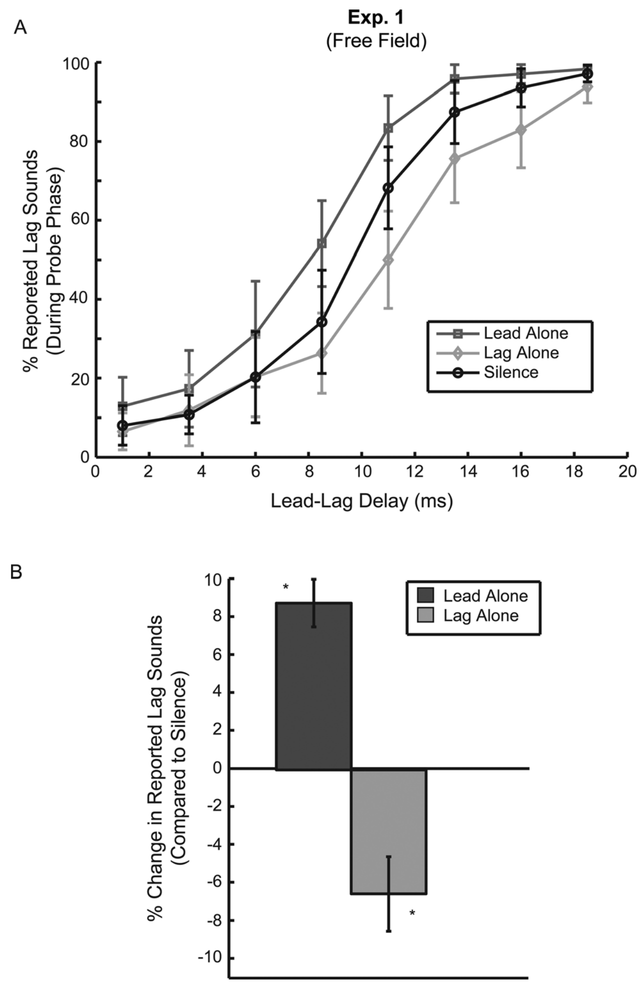

FIG. 2. Experiment 1 (free field) results. (A) The percentage of reported lag sounds during the probe phase is plotted as a function of lead-lag delay for the three session types: lead alone (dark gray squares), lag alone (light gray diamonds), and silence (black circles). (B) The percent change in reported lag sounds for the lead-alone (dark gray) and lag-alone (light gray) sessions compared to silence sessions. The data show a significant increase in lead-alone sessions ($p < 0.05$) and a significant decrease in the lag-alone sessions ($p < 0.05$) compared to silence sessions. Data reflect the mean ± s.e. across listeners ($N = 14$).

TABLE I. Responses to debriefing questions. The number of listeners who responded "yes" to each question is reported in the table below. Listeners responded to three questions following experiments 1, 2, and 4. (1) Did sounds on the left side fade out? (2) Did sounds on the left side fade in? (3) Did sounds on the left side fade out then in?

Experiment	# of listeners	Fade out?	Fade in?	Fade out then in?
1	14	12	1	0
2	13	13	2	1
4	15	15	3	2
Total	42	40	6	3

listener counts during the conditioners were nearly perfect (98.87 ± 0.44% or ~20 for lag alone and 0.63 ± 0.34% or ~0 for lead alone) in this and all subsequent experiments. Most importantly, listener counts of the buildup probe varied considerably depending on the preceding conditioner. Specifically, listeners reported hearing 61.25 ± 4.08% [mean ± standard error (s.e.) across listeners] of lag sounds during lead-alone sessions, 45.93 ± 5.07% during lag-alone sessions, and 52.47 ± 4.23% during silence sessions. In order to quantify the effects of a lead-alone and lag-alone conditioners on buildup compared to silence, the percentage of reported lag sounds was included in a one-factor, repeated measure analysis of variance (ANOVA) with conditioner ([lead alone/lag alone/silence]) as a within listener factor. (Note that data were arithmetically averaged across delays for each session type.) The ANOVA revealed a main effect of conditioner ($F_{(2,26)} = 35.39224$, $p < 0.001$). *Post hoc* tests performed using Fisher's least significant difference (LSD) revealed the following relationship between the percentage of reported lag sounds among the three sessions: lead alone > silence > lag alone. In other words, a lead-alone conditioner reduced buildup by 8.78 ± 1.25% compared to an equivalent period of silence. In contrast, a lag-alone conditioner enhanced buildup by 6.54 ± 1.96% relative to an equivalent period of silence [Fig. 2(B)].

III. EXPERIMENT 2

Experiment 1 demonstrated two key effects of unpaired click trains on buildup in free field. First, the data suggest that buildup is reduced or "depressed" following a lead-alone conditioner compared to an equivalent period of silence; this agrees well with previous observations (Sanders *et al.*, 2011). Second, the data suggest that buildup is enhanced following a lag-alone conditioner compared to an equivalent period of silence. However, it is unclear whether these effects generalize to stimuli presented over earphones. The precedence effect is often studied using earphones in order to isolate the individual contributions of binaural timing (ITD) and level (ILD) differences important in spatial hearing. To date, no study has investigated the effects of lead- or lag-alone click trains on buildup with stimuli presented over earphones despite the potential insight it may provide. Thus, in experiment 2 we investigate if the effects of lead- and lag-alone click trains on subsequent buildup generalize to dichotic listening conditions. We presented lead clicks to the right ear and lag clicks to the left ear only; listeners performed an otherwise identical task.

A. Methods

The methodological approach of experiment 2 was virtually identical to experiment 1, with few exceptions. Consequently, only the methodological differences between experiments 1 and 2 are described here. Stimuli were presented dichotically over earphones in experiment 2 rather than through loudspeakers in free field as in experiment 1. The lead click was always presented to the right ear only while the lag click was always presented to the left ear only [Fig. 1(A)]. As a result, individual clicks did not contain any naturally occurring interaural timing or level cues (ITD or ILD). Also, little effort was made to match sound intensity levels to those perceived in experiment 1.

Fifteen new, naive listeners [10 female, 24 ± 7.5 (SD) years of age] were recruited for experiment 2. Click trains were presented at 80 dB(A, slow response) and 111 dB(A, peak response) via a set of Etymotic ER-4B earphones. Earphone output was calibrated prior to each listener's participation using an earphone coupler, microphone, and custom MATLAB scripts. Listeners sat in a comfortable chair with their chins placed in a chinrest ~50 cm from a computer monitor. The training and experimental procedures were otherwise identical to experiment 1. Two listeners were excused from the experiment; the first was excused because he did not satisfy a behavioral criterion during the training procedure (see experiment 1 for details) and the second was excused because he refused to be monitored via a remote camera used to ensure that subjects maintained fixation throughout sound presentation.

B. Results and discussion

The percentage of reported lag sounds during the probe phase vs lead-lag delay under dichotic listening conditions is plotted in Fig. 3(A). As in experiment 1, the percentage of reported lag sounds increased monotonically with increasing lead-lag delay irrespective of the session type. Listeners reported hearing 74.57 ± 2.73% (mean ± s.e. across listeners) in lead-alone sessions, 60.09 ± 4.21% in lag-alone sessions, and 66.94 ± 2.94% in silence sessions. A one-way, repeated measure ANOVA revealed a main effect of session type ($F_{(2,24)} = 14.91917$, $p < 0.001$) and Fisher's LSD *post hoc* tests revealed the following qualitative relationship: lead alone > silence > lag alone. Quantitatively, the percentage of reported lag sounds increased by 7.64 ± 2.02% following a lead-alone conditioner and decreased by 6.85 ± 2.41% following a lag-alone conditioner compared to an equivalent period of silence [Fig. 3(B)]. Put simply, the data reveal that a lead-alone conditioner reduced while a lag-alone conditioner enhanced buildup compared to the silence baseline. This is precisely the same relationship observed in free field [cf. Figs. 2(A) and 3(A)].

Although lead- and lag-alone click trains had a qualitatively similar effect across free-field (experiment 1) and dichotic (experiment 2) listening conditions, there are several notable differences between the two data sets. For

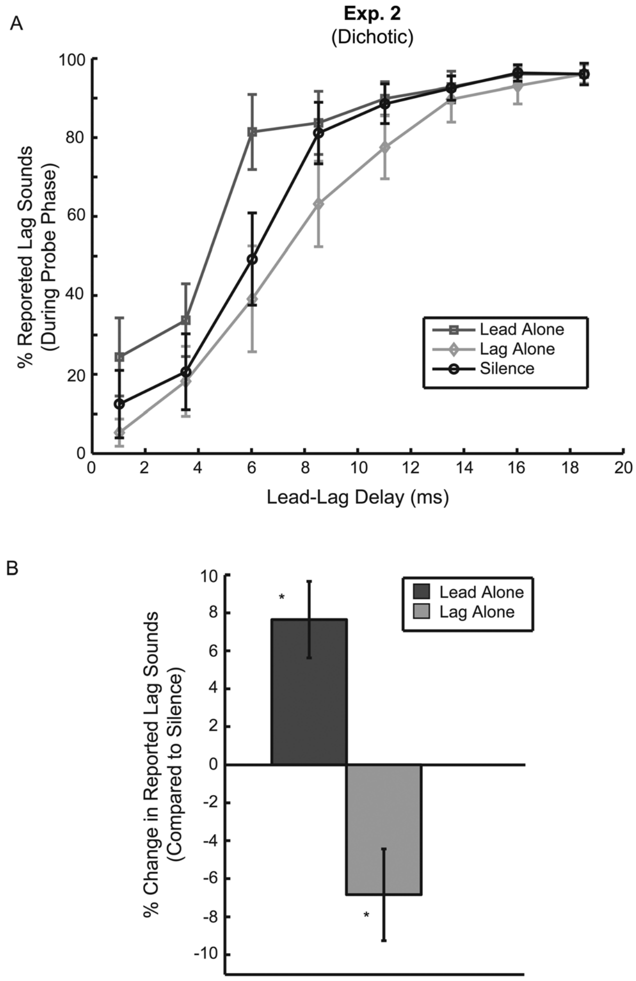

FIG. 3. Experiment 2 (dichotic) results. Data are plotted as in Fig. 2 ($N = 13$).

instance, the psychometric functions under dichotic listening conditions appear to be shifted to the left compared to those in free field [cf. Figs. 2(A) and 3(A)]. We performed an additional analysis to compare our findings across listening conditions. Specifically, we performed a two-factor ANOVA with session type ([lead alone/lag alone/silence]) as a within-subject factor and stimulus delivery ([free field or dichotic]) as a between listener grouping factor. The main effects of session type and stimulus delivery were significant, but the session type × stimulus delivery interaction was not (session type: $F_{(2,50)} = 44.0286$, $p < 0.001$; 67.91 ± 2.49% for lead alone, 53.01 ± 3.32% for lag alone, and 50.07 ± 2.61% for silence; stimulus delivery: $F_{(1,25)} = 6.8062$, $p = 0.015$; 53.21 ± 3.72% for free field and 67.20 ± 3.86% for dichotic; interaction: $F_{(2,50)} = 0.0690$, $p = 0.887$). In other words, buildup was generally less effective in experiment 2 than experiment 1, but there was no statistically significant difference in the modulatory effects of a preceding lead- and lag-alone conditioner on buildup. These similarities are reassuring, especially when one considers the differences in acoustics used in the two experiments; the reader will recall that we made little effort to match the sound levels between free-field (experiment 1) and dichotic (experiment 2) listening conditions and naturally occurring binaural spatial cues were entirely absent from experiment 2, yet similar effects were observed. This suggests that the effects of unpaired click trains on buildup are relatively unaffected by sound intensity levels, occur even in the absence of biologically relevant binaural spatial cues (i.e., ITD and ILD), and do not depend on the method of stimulus delivery (e.g., free field vs dichotic over earphones). Together, the results of experiments 1 and 2 suggest that the modulatory effects of a lead- and lag-alone click train on buildup are remarkably robust.

IV. EXPERIMENT 3

Experiments 1 and 2 demonstrate that a preceding lead- or lag-alone conditioner can reduce or enhance subsequent buildup respectively in both free-field and dichotic listening conditions. However, these experiments cannot rule out two alternative explanations of the observed modulatory effects: neural adaptation and a systematic change in counting strategy across session types. First, considerable evidence demonstrates rapid habituation of cortical and subcortical neural responses with repeated acoustic stimulation, particularly at high presentation rates of identical stimuli (Salamy *et al.*, 1978; Yagi and Kaga, 1979; Prosser *et al.*, 1981; Paludetti *et al.*, 1983; Lasky, 1984; Suzuki *et al.*, 1986; Donaldson and Rubel, 1990; Lasky *et al.*, 1993; Lasky *et al.*, 1996; Lasky, 1997; Polyakov and Pratt, 2003; Stone *et al.*, 2009). Based on these neural observations, we hypothesized that the results reported in experiments 1 and 2 could be driven by a location or ear-specific change in acoustic sensitivity in the peripheral auditory pathway potentially leading to a change in perceptual saliency of the lead or lag click in the buildup train. For example, the addition of a preceding lag-alone conditioner could conceivably render listeners less sensitive to sounds originating from the lag side during the subsequent buildup probe. This would manifest behaviorally as a reduction in the percentage of reported lag sounds in lag-alone sessions compared to silence sessions, much like the changes reported in experiments 1 and 2. Following this line of reasoning, we would expect an increase in the percentage of reported lag sounds following a lead-alone conditioner compared to an equivalent period of silence. Second, listeners may have used different counting strategies during the probe phase depending on the preceding conditioner. In experiment 3, we address the potential contributions of general adaptation and systematic changes in counting strategy across session types by measuring subjective detection thresholds of click stimuli following prior stimulation on the "same side" or "different side." Stimuli were presented dichotically over earphones, as in experiment 2, because this stimulus delivery approach simultaneously tested the hypotheses of ear- and location-specific adaptation effects as well as a change of listener counting strategy. We reasoned that we were most likely to see an effect under dichotic listening conditions and, should we actually find an effect, further tests could be conducted to dissociate the contributions of these potential factors.

A. Methods

Experiment 3 was very similar to experiment 2 and only the methodological differences between them are described here. Ten additional naive listeners (6 female, 21 ± 1.3 years of age) were recruited to participate in experiment 3. The training and experimental procedures were virtually identical to those of experiment 2. The only difference between the two experiments is that the buildup probes in experiment 2 were replaced with intensity trains to estimate subjective detection thresholds [Fig. 4(A)]. The intensity trains (ITs) consisted of twenty, monaural clicks of different peak intensity; the first click in a train was the loudest, and each subsequent click's intensity decreased by 1 dB resulting in the 20th click being 19 dB quieter than the first [Fig. 4(A)]. The absolute intensity about which the each IT was centered ranged from 35–69 dB in approximately 4.85 dB steps for a total of eight unique ITs. Importantly, ITs were presented from the left earphone only. Listeners performed the same counting task used in experiments 1 and 2 by counting how many sounds of any type, quality, or intensity they heard from the left of the midsagittal plane.

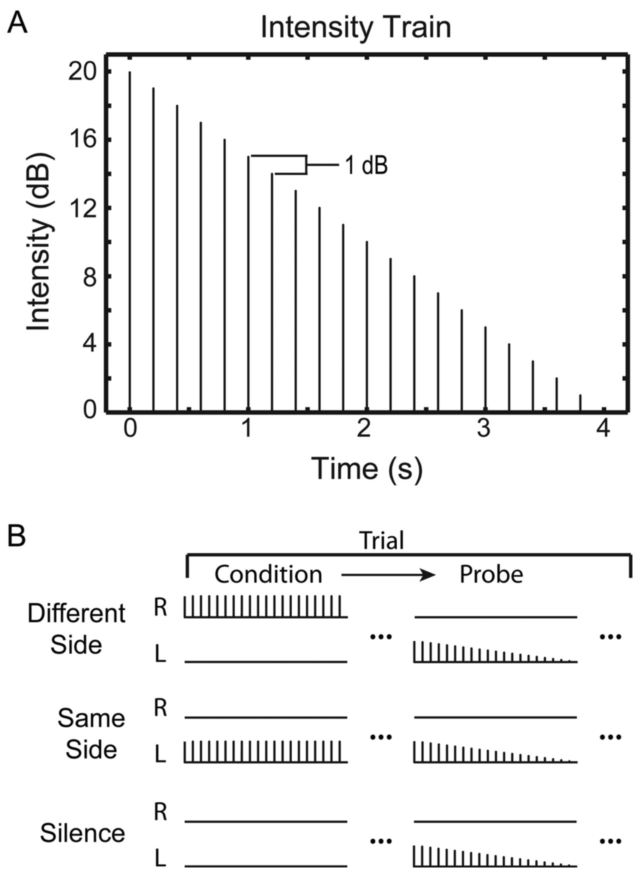

FIG. 4. Stimuli and trial structure for experiment 3. Like experiment 2, experiment 3 employed a condition → probe design and presented sounds dichotically over earphones. During the condition phase, one of three stimuli were presented: a train of 20 clicks in the right ear only (different side), a train of 20 clicks presented in the left ear only (same side), or an equivalent period of silence (silence) [see panel (B)]. During the probe phase of the trial, listeners were presented with one of eight intensity trains (ITs) that differed only in the absolute intensity about which the trains varied. Each IT consisted of a train of 20 clicks; each subsequent click in the train was attenuated by 1 dB relative to the immediately preceding click [see panel (A)]. In other words, an individual IT spanned a 20 dB intensity range. The center intensity of ITs ranged from ~35 to ~69 dB in 4.85 dB increments. ITs were always presented to the left earphone only. Listeners performed an otherwise identical counting task.

Experiment 3 followed a blocked design with one of three conditioners for each session: a train of 20, equal-intensity clicks identical to the "lag-alone" conditioner used in experiment 2 (same side), a train of 20, equal-intensity clicks identical to the "lead-alone" conditioner used in experiment 2 (different side), or an equivalent period of silence. Individual trials followed the same condition → probe design [Fig. 4(B)]. Each session consisted of 16 trials; an IT (eight center intensity levels presented twice per session) was presented during the probe phase of each trial. The center intensity of the ITs was randomized across trials within each session. Session order was pseudorandomized as in experiments 1 and 2.

B. Results and discussion

The percentage of reported sounds during the probe phase is plotted as a function of center intensity of the intensity trains (ITs) in Fig. 5(A). In order to quantify statistically any differences in overall subjective detectability following

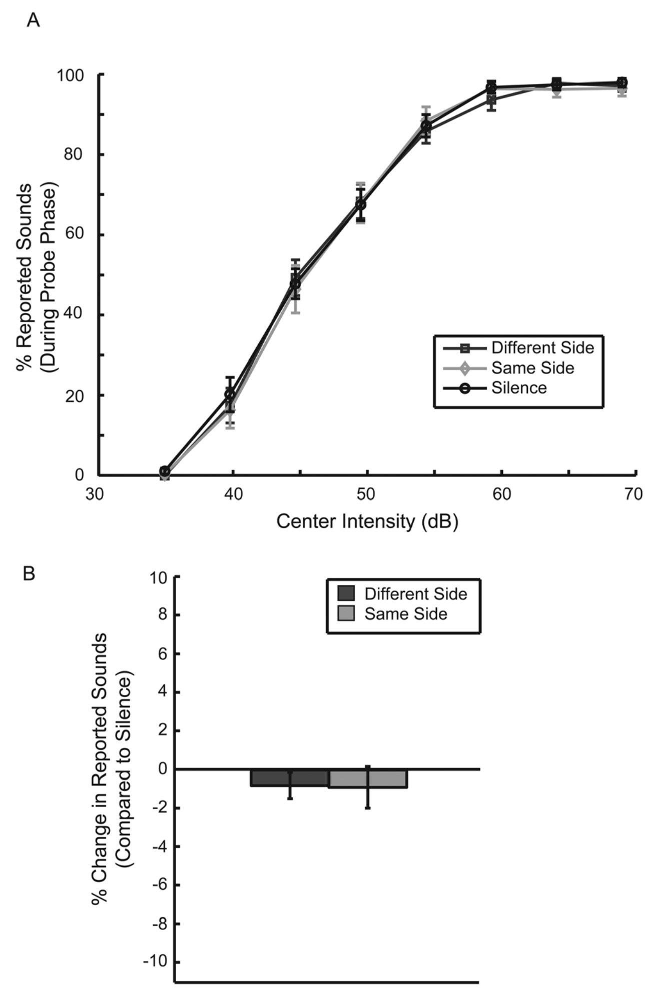

FIG. 5. Experiment 3 results. (A) The percentage of reported sounds during the probe phase is plotted as a function of center intensity for three session types: different side (dark gray squares), same side (light gray diamonds), and silence (black circles). (B) The percentage change in reported sounds during the probe phase during different-side (dark gray) and same-side (light gray) sessions compared to silence. There were no significant differences between session types ($p > 0.05$). Data reflect mean ± s.e. across listeners ($N = 10$).

a same-side or different-side conditioner compared to an equivalent period of silence, the percentage of reported sounds was included as a repeated measure in a one-factor ANOVA with session type ([same side/different side/silence]) as a within-listener factor. The main effect of conditioner was insignificant, suggesting that clicks remained equally detectable and that listeners' counting strategies were not differentially biased by the conditioner used ($F_{(2,18)} = 0.569$, $p = 0.54$; $63.75 \pm 1.88\%$ for same side, $63.65 \pm 2.51\%$ for different side, and $64.54 \pm 1.68\%$ for silence). By extension, these results suggest that the observations made in experiments 1 and 2 cannot be easily explained by a simple ear- or location-specific adaptation effect or a systematic change in counting strategy between session types. However, these data cannot rule out all forms of adaptation, particularly a more central process, and further experimentation involving neural measures are likely required.

V. EXPERIMENT 4 (A AND B)

Experiment 3 lends some specificity to the effects of lead- and lag-alone click trains on buildup by ruling out several alternative interpretations. First, the data suggest that listeners employed the same counting strategy regardless of the session type. Second, the data suggest that the observed modulatory effects cannot be easily explained by an ear- or location-specific adaptive process or a change in perceived saliency. However, several additional design considerations must be considered before the effects of lead- and lag-alone click trains on subsequent buildup can be determined. First, although gathering responses in an open-ended fashion maximized response accuracy and minimized listener fatigue, it also resulted in unpredictable delays between trials as well as between condition and probe phases within a single trial. Additionally, listeners were permitted to review and correct responses in the event of a typographical error; thus, the number of button presses was rarely uniform throughout a session (e.g., there were more button presses when an error was made). In light of the importance of well-controlled acoustic history within the context of these experiments, we were concerned that the variable number of (audible) button presses may have confounded the results reported in experiments 1 and 2. This is of particular concern due to a systematic difference in the number of button presses during the condition phase of each session type; listeners tended to press more buttons following a lag-alone conditioner (e.g., two button presses to enter "20") than following a lead-alone conditioner (e.g., one button press to enter "0"). Second, the effects of a lead- and lag-alone click train were assessed relative to an approximately time-matched period of silence. The rationale for using a time-matched period of silence was to control approximately the rate of buildup probe presentation and to match and remove any latent, across-trial effects of one buildup probe on the next. Previous studies have shown that buildup decays over time and thus it is possible that across-trial contributions would vary depending on the time delay between buildup probes (Djelani and Blauert, 2000). We reasoned that the baseline condition to which lead- and lag-alone session types are compared must contain an equivalent period of silence to account for the spontaneous decay in across-trial effects between buildup probes. However, the silence condition does not control the overall rate of acoustic stimulation—that is, how often listeners are presented with a click train of any type, be it conditioner or probe. In experiment 4, we introduce several key changes to the trial structure and randomization scheme, and include a new "no conditioner" trial type in order to (1) control the delay between trial phases and between trials precisely; (2) better control potential acoustic artifacts due to button presses; (3) determine if across-trial effects are plausible and thus must be accounted for; and as a result, (4) better characterize the effects of lead- and lag-alone click trains on buildup.

Experiment 4 was similar to experiment 1, but differed in several key ways to address the issues above: (1) the time between sound offset and onset between trials and phases within a trial was equated using a fixed response window; (2) responses were only gathered after the probe phase of each trial and response entries required precisely two button presses (e.g., "20" in experiment 4 instead of "'20' then 'enter'" in experiments 1–3); (3) the lead-lag delay randomization scheme was either blocked (all instances of the same lead-lag delay presented on sequential trials) or changed between trials (changing delay). This was done to qualify the possibility of across-trial effects between buildup probes. (4) A no-conditioner trial type was introduced that, in contrast to silence sessions in experiments 1 and 2, controls for the overall rate of acoustic presentation. Together, these changes lend significant interpretational power to the findings reported in experiments 1 and 2. Stimuli were presented in free field, as described in experiment 1.

A. Methods

1. Listeners

Seventeen naive listeners (13 female, 22 ± 3 years of age) were recruited for experiment 4. Two listeners failed to satisfy a behavioral cutoff during training and were excused from the study (see experiment 1 for details). Thus, the data reported here are based on the remaining 15 listeners (12 female, 22 ± 3 years of age).

2. Experimental setup

The experimental setup was nearly identical to that of experiment 1, except that a quieter keyboard was used (Thinkpad UltraNav USB keyboard) and care was taken to place the keyboard's number pad used to collect responses directly in front of the listener. These changes substantially reduced, but did not entirely eliminate, key press noise.

3. Stimuli, trial structure, and task

In order to maintain precise timing control throughout the experiment, sounds in experiment 4 were presented at a rate of 48 kHz instead of 96 kHz; the lower sampling rate reduced computational demands that led to timing imprecisions with click trains sampled at 96 kHz during early pilot testing (data not shown). The change in sampling rate resulted in slightly longer individual clicks in experiment 4 (62.5 μs) than in experiments 1–3 (52 μs). Conditioners (lead alone, lag alone, and silence) and the buildup probe were otherwise identical to those described in experiment 1. Experiment 4 employed two types of trials: those consisting of both a condition and probe phase (lead-alone, lag-alone, silence sessions) and those consisting of only a probe phase (no conditioner). Specifically, trials in lead-alone, lag-alone, and silence sessions began with 2.5 s of fixation, followed by a 4.0 s conditioner. A buildup probe was presented precisely 5.5 s after the end of the conditioner. Listeners were cued to begin counting by the appearance of a visual cue (white fixation cross turning to green) 0.75 s prior to probe onset. The reader will note that, unlike previous experiments, listeners did not count during the condition phase and thus only one count was gathered per trial. The buildup probe lasted 4.0 s and was followed immediately by the same prompt used in all other experiments ("How many sounds from the LEFT side?"). Listeners were instructed to begin counting after the visual cue and were permitted 3.0 s to input their response. The prompt remained on the screen throughout the response period and was removed at the end of the response window. Responses were discarded if listeners failed to enter a two-digit response within the response window. The same counting task was employed, but response input differed in several key ways: (1) listeners were explicitly instructed to press the keys quietly; (2) listeners were instructed to press two keys for all responses (e.g., "01" instead of "1"); (3) a fixed response window of 3.0 s was enforced; (4) listeners were instructed not to count during the conditioning train; and (5) listeners were not allowed to correct their mistakes. These changes ensured that the acoustic contamination from button presses was minimal and well-controlled throughout.

In addition to the three conditioners used in experiment 1, a "no conditioner" control was included. No-conditioner sessions did not contain an explicit conditioner of any kind, and instead consisted solely of a probe phase. Consequently, the trial structure was slightly different for the no-conditioner sessions. Trials lasted only 9.5 s, again beginning with 2.5 s of fixation, followed by a 4 s buildup probe and a 3.0 s response window [Fig. 6(B)]. The visual cue to initiate counting preceded the probe by 0.75 s. This trial structure maintained an equivalent stimulus presentation rate of 1 conditioner or probe/9.5 s in all but the silence session type; the overall presentation rate was 1 probe/19 s in the silence session type.

4. Training procedure

The training procedure was virtually identical to experiment 1 and only the differences are described here. First, during the PowerPoint presentation, listeners were instructed to press keys quietly, press two keys for all responses (e.g., "01" instead of "1"), and to count only after the cross turned green. The presentation and instructions were otherwise identical. Second, the condition phase was removed from the counting sessions in an effort to reduce training time. Thus, each counting session contained 16 trials following the no conditioner

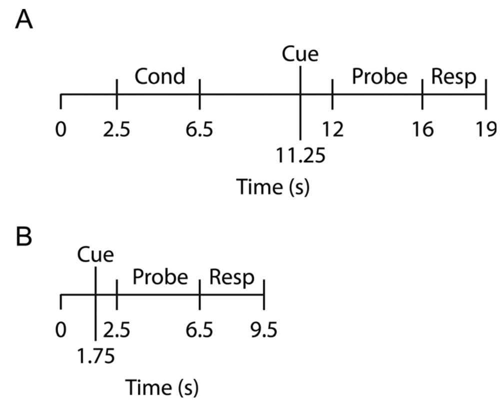

FIG. 6. Experiment 4 trial structure. Experiment 4 employed a condition → probe design and presented sounds via free-field loudspeakers. Listeners performed the same counting task used in experiments 1–3. In contrast to previous experiments, responses were only gathered after the probe phase and two distinct trial structures were employed: one for lead-alone, lag-alone, and silence sessions and a second for "no conditioner" sessions. (A) Lead-alone, lag-alone, and silence trials began with 2.5 s of fixation, followed by a 4 s conditioner. The conditioner was followed by 5.5 s of fixation prior to the presentation of the buildup probe (20 lead-lag click pairs). The response window was limited to 3.0 s. (B) A modified trial structure was used for "no conditioner" sessions to match the overall sound presentation rates across all session types. No-conditioner trials began with 2.5 s of fixation, followed by a buildup probe. The buildup probe was immediately followed by a 3.0 s response window. A visual cue (cross changing from white to green) prompted subjects to begin counting 0.75 s prior to the start of the buildup probe in all trials. (Cond = conditioner; Resp = response).

trial structure [Fig. 6(B)]. Finally, listeners completed one lead-alone session and one lag-alone session identical to those used during the main experiment as practice; session order was counterbalanced across listeners.

5. Experimental procedure

The experiment consisted of eight sessions: two lead alone, two lag alone, two silence, and two no conditioner. The lead-lag delay of buildup probes was again 1–18.5 ms in 2.5 ms steps. Each lead-lag delay was repeated three times per session. (Note that lead-lag delays were repeated twice in experiments 1 and 2.) Experiment 4 was divided into two halves. Each half contained one of each session type for a total of four sessions per half. In one half of the experiment (experiment 4A: "changing delay"), the lead-lag delay was pseudorandomized to ensure that the lead-lag delay of the buildup probe was never the same on two consecutive trials within a session. In the other half of the experiment (experiment 4B: "blocked delay"), all three instances of the same lead-lag delay were presented on consecutive trials. This was done to allow direct measurement of across trial buildup effects—that is, whether or not buildup on one trial carried over and affected buildup on the next trial. Experiment order (4A/4B) was counterbalanced across listeners.

B. Results and discussion

The percentage of reported lag sounds during the probe phase vs lead-lag delay is plotted for experiment 4A (changing delay) and 4B (blocked delay) in the top and bottom panels of Fig. 7(A), respectively. The data collapsed across both randomization schemes are plotted in Fig. 7(B) (top panel).

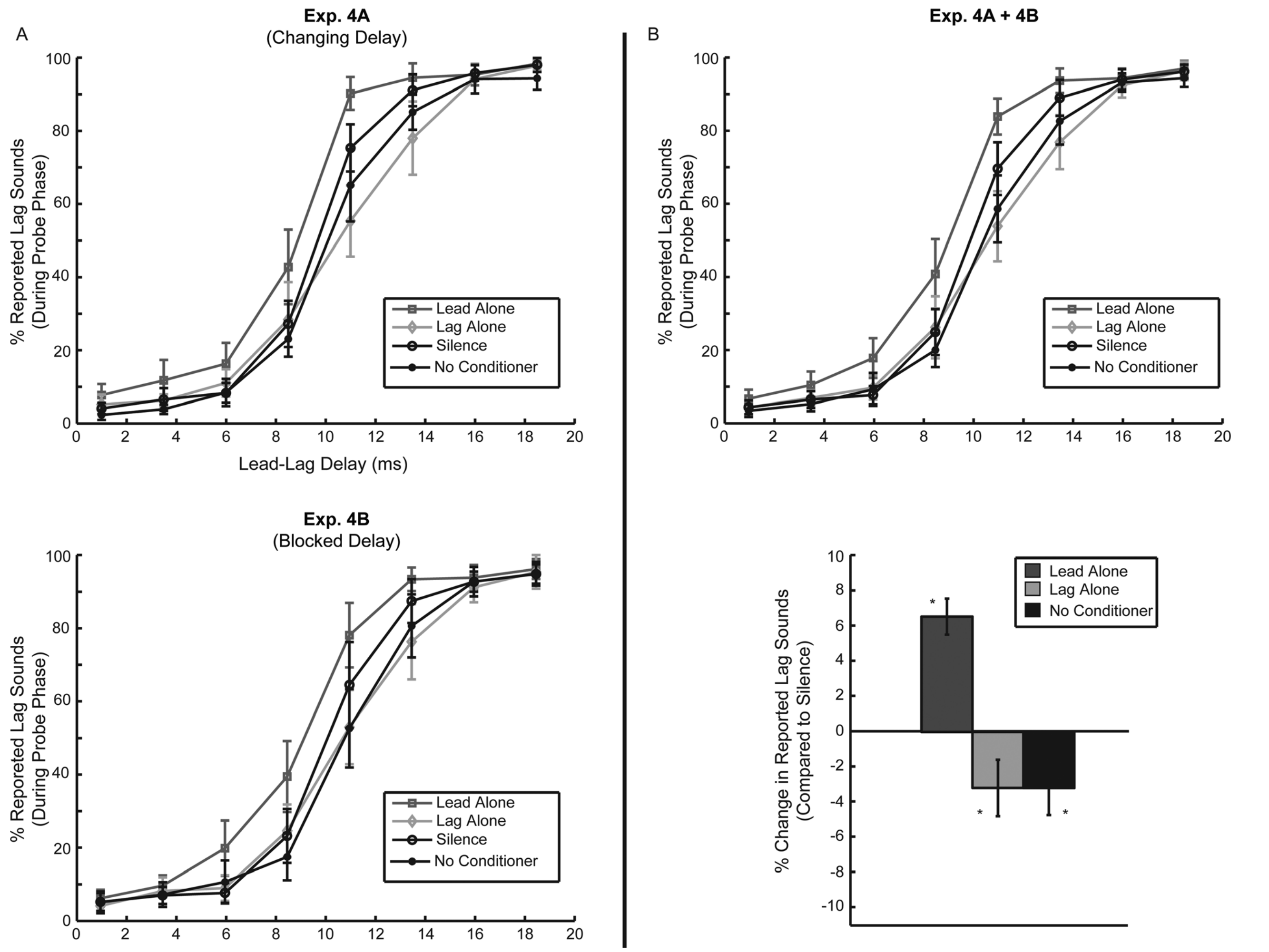

FIG. 7. Experiment 4 results. Experiment 4 consisted of two smaller experiments that differed only in the randomization of the lead-lag delay of the buildup probe across trials. Experiment 4A (changing delay) ensured that the lead-lag delay during the buildup probe varied from trial to trial. In contrast, experiment 4B (blocked delay) ensured that all three instances of the same lead-lag delay were presented on consecutive trials before changing the delay. (A) The percentage of reported lag sounds during the probe phase is plotted as a function of lead-lag delay for the lead alone (dark gray squares), lag alone (light gray diamonds), silence (black open circles), and no conditioner (black closed circles) session types in experiments 4A (top panel) and 4B (bottom panel). An ANOVA using the percentage of reported lag sounds during the probe phase as a repeated measure identified a mean difference between the two randomization schemes [experiment 4B (blocked delay) > experiment 4A (changing delay)], but no interactions ($p > 0.05$). Consequently, the data were collapsed across randomization schemes and replotted [(B), top panel]. The percentage change in reported lag sounds during the probe phase in the lead-alone (dark gray), lag-alone (light gray), and no-conditioner (black) sessions are plotted relative to the silence in the lower panel of (B) (*$p < 0.05$). Data reflect mean ± s.e. across listeners ($N = 15$).

As in experiments 1 and 2, the percentage of reported lag sounds increases monotonically with increasing lead-lag delay. In order to assess the effects of lead- and lag-alone conditioners compared to an equivalent period of silence, the percentage of reported lag sounds was included in a two-factor, within-listener ANOVA with factors session type ([lead alone/lag alone/silence/no conditioner]) and randomization scheme ([changing delay/blocked delay]). As in previous experiments, data were arithmetically averaged across delay for each session type. The main effects of session type and randomization scheme were significant, but the session type × randomization scheme interaction was not (session type: $F_{(3,42)} = 23.65$, $p < 0.001$; 55.76 ± 2.01% for lead alone, 46.02 ± 2.64% for lag alone, 49.21 ± 1.83% for silence, and 46.01 ± 1.98% for no conditioner; randomization scheme: $F_{(1,14)} = 6.5444$, $p = 0.023$; 50.50 ± 1.89% for changing delay and 48.00 ± 2.17% for blocked delay; session type × randomization scheme: $F_{(3,42)} = 0.1772$, $p = 0.903$). *Post hoc* tests (Fisher's LSD) revealed the following relationships between the four session types: lead alone > silence > lag alone, no conditioner. That is, a lead-alone conditioner reduced buildup by 6.55 ± 1.02% and a lag-alone conditioner enhanced buildup by 3.19 ± 1.61% compared to an equivalent period of silence. To put it simply, despite introducing a host of methodological changes, we find precisely the same qualitative effects of lead-alone and lag-alone click trains on buildup when compared to a silence baseline. Furthermore, these findings suggest that latent, across-trial effects are possible despite exposure to numerous lead-lag pairs within a buildup probe and intervening unpaired click trains or periods of silence. Thus, these data demonstrate the potential for across-trial effects and emphasize the importance of matching the rate of buildup probe presentation.

Despite the qualitative similarities between experiments 1 and 4, the data do suggest that the methodological changes resulted in several quantitative differences. For instance, we noticed that the magnitude of the modulatory effects of lead- and lag-alone conditioners tend to be somewhat smaller in experiment 4 than in experiment 1 when compared to a silence baseline [cf. Figs. 3(B) and 7(B), bottom panel]. In order to assess whether or not these differences were statistically meaningful, we included the percentage of reported lag sounds in a two-factor ANOVA, with session type ([lead alone/lag alone/silence]) as a within-listener factor and experiment ([experiment 1/experiment 4]) as a between-listener grouping factor. (Note that data were collapsed across randomization schemes in experiment 4.) Although the main effect of session type was significant, the main effect of experiment and the session type × experiment interaction did not reach significance (session type: $F_{(2,54)} = 63.8345$, $p < 0.001$; $58.51 \pm 2.23\%$ for lead alone, $45.97 \pm 2.80\%$ for lag alone and $50.84 \pm 2.25\%$ for silence; experiment: $F_{(1,27)} = 0.3749$, $p = 0.545$; $53.21 \pm 3.37\%$ for experiment 1 and $50.33 \pm 3.27\%$ for experiment 4; session type × experiment: $F_{(2,54)} = 3.1582$, $p = 0.062$). In other words, the reduction in buildup following a lead-alone conditioner and the putative enhancement of buildup following a lag-alone conditioner were not statistically different between experiments.

Interestingly, while the decrease in buildup following a lead-alone click train does not depend on the baseline measure selected (e.g., silence vs no conditioner), the conclusions drawn regarding the contributions of a lag-alone click train to subsequent buildup differ depending on the baseline to which the data are compared. For instance, lag-alone click trains appear to enhance buildup when compared to a silence baseline, but seem to have no measurable effect on buildup when compared to a no-conditioner control [Fig. 7(B), bottom panel]. As discussed in the Introduction, these outcomes have significantly different implications on our understanding of buildup and the importance of acoustic contrast. We consider a silence baseline to be most appropriate for two key reasons. First, experiment 4 demonstrates the possibility of across-trial effects between buildup probes. Specifically, buildup increases across subsequent trials when the lead-lag delay is held constant (blocked delay) vs not (changing delay). These across-trial effects are likely time dependent, as others have shown that buildup decays over comparable or shorter time periods (Djelani and Blauert, 2000). Consequently, the potency of across-trial effects may differ significantly based on the time delay between buildup probes. This specific property, the rate of buildup probe presentation, is matched across lead-alone, lag-alone, and silence sessions but *not* no-conditioner sessions. Second, the rate of response entry and the acoustic history associated with button presses may impact buildup. Specifically, although additional safeguards were taken to minimize the sound of button presses and variance in the number of buttons pressed across trials, button presses remained audible within the testing chamber. Thus, it is important to compare lead- and lag-alone sessions to a baseline with a matched rate and number of button presses. This is an additional property that is matched between lead-alone, lag-alone, and silence sessions but *not* no-conditioner sessions. It is for these two key reasons that the silence condition is the more appropriate baseline to which the lag-alone data are compared. However, further study on the topic of appropriate baseline would help clarify the matter.

VI. GENERAL DISCUSSION

In a series of four experiments, we explored the effects of preceding unpaired click trains on buildup of echo suppression. In experiment 1, we coupled a subjective counting task with a condition → probe design to characterize how lead- and lag-alone click trains affect subsequent buildup in free field. The data suggest that a preceding lead-alone click train reduces buildup while a preceding lag-alone click train enhances buildup compared to a silent baseline condition. Experiment 2 demonstrated that lead- and lag-alone click trains had modulatory effects on buildup under dichotic listening conditions similar to those observed in free field. In experiment 3, we demonstrated that the effects of a lead- and lag-alone click train on buildup cannot be explained by a simple adaptive process or a change in listener counting strategy. Finally, experiment 4 extended our free-field observations in experiment 1 by providing additional control over a listener's acoustic experience and adding a "no conditioner" session type. In the following sections, we discuss our findings and their mechanistic implications within the broader literature. In the final section, we consider some of the advantages and disadvantages of the counting task employed here over established subjective condition then test paradigms (Litovsky *et al.*, 1999).

A. Effects of unpaired click trains on buildup

Our findings join and complement a growing corpus of reports exploring the effects of lead-alone click trains on echo suppression (Freyman *et al.*, 1991; Keen and Freyman, 2009; Sanders *et al.*, 2011). First, data reported by Freyman *et al.* (1991) demonstrated that echo thresholds of isolated lead-lag pairs decreased following a lead-alone conditioner, albeit in the absence of any explicit attempt to elicit buildup. Second, Keen and Freyman (2009) extended these findings in two key ways: (1) they demonstrated that established buildup can be disrupted and reduced by exposure to a lead-alone click train and (2) that the reduction in buildup is highly dependent on the number of lead-alone clicks presented. For example, buildup is only partially reduced following the presentation of a single lead-alone click but is completely undone following repeated exposure [cf. Fig. 3 in Keen and Freyman (2009)]. Finally, Sanders *et al.* (2011) demonstrated that buildup can also be reduced or "depressed" by a preceding lead-alone click train; the authors harnessed the depression in buildup to explore the neural correlates of echo suppression in human listeners. In every instance, a lead-alone click train reduces echo suppression and buildup, regardless of whether the lead-alone click train precedes or follows buildup. The data reported here corroborate these observations generally by again demonstrating a clear reduction in echo suppression—reflected as an increase in the percentage of reported lag sounds—when a lead-lag train is preceded by a lead-alone conditioner. In addition to qualitative agreement, there is considerable quantitative similarity between the reduction in buildup reported here and in Keen and Freyman (2009). Specifically, Keen and Freyman report that a lead-alone conditioner can reduce echo thresholds following buildup by ~4 ms (cf. left-most empty diamond in Fig. 5 left panel and filled triangles in Fig. 5, right panel of Keen and Freyman). The data reported here depict a similar reduction in echo perception. For instance, the data reported in Fig. 2 show that ~20% of lag sounds are reported in the silence baseline condition at a lead-lag delay of ~6 ms. By comparison, 20% of lag sounds are reported in the lead-alone condition at a lead-lag delay of ~4 ms, resulting in a change of ~2 ms. Note that comparisons between studies should be made cautiously since psychometric functions from different tasks may not correspond numerically to identical perceptual abilities or subjective experience. Nevertheless, this degree of quantitative similarity is remarkable considering the numerous differences in methodology between the reports, perhaps most notably the relative positioning of the lead-alone click train before (current study) or after [Keen and Freyman (2009)] buildup is established. In contrast to the handful of studies investigating the effects of a lead-alone click train on buildup, no other studies to date have investigated the effects of a lag-alone click train on buildup. Thus, there are no studies to which our findings can be suitably compared. However, we should note that a single previous report has demonstrated that a lag-alone click train is not sufficient to elicit buildup (Freyman *et al.*, 1991).

B. The role of contrast in echo suppression

In the current study, we hoped to improve our understanding and theoretical framework of echo suppression by investigating the effects of a preceding lag-alone click train on buildup. As discussed in the Introduction, the outcome of this manipulation is unclear based on existing literature and our understanding of the room acoustics hypothesis. Although our conclusions regarding the effects of a lag-alone click train on buildup depend on the selection of an appropriate baseline (silence vs no conditioner; see Sec. V B for details), we cautiously concluded that a lag-alone click train leads to an enhanced buildup effect. In contrast, a preceding lead-alone click train results in a reduced or "depressed" buildup effect, similar to previous reports (Sanders *et al.*, 2011). These effects are consistent with predictions made based on the importance of acoustic contrast between listening conditions. Specifically, moving from an environment with only a lead click to an environment with both lead and lag clicks results in a reduction in buildup. By comparison, moving from an environment with only a lag click to an environment with a lead-lag pair seems to enhance buildup, possibly due to the increased perceptual salience of the lead click in the following buildup probe; further experimentation is necessary to test this hypothesis. Although data in experiments 1, 2, and 4 are consistent with these effects, experiment 3 suggests that the role of contrast in buildup must be qualified. Specifically, experiment 3 demonstrates that repeated exposure to a click train from the same location does *not* result in a decrease in perceptual salience of subsequent, unpaired stimuli presented from the same location. Consequently, if contrast is indeed an important aspect to buildup, the mechanisms for establishing acoustic contrast do not generalize to all listening scenarios; instead, they may be limited to lead-lag pairs, suggesting some level of specificity to buildup and the precedence effect generally. An additional qualification to the role of contrast in buildup can be found in the data from experiments 1, 2, and 4. Specifically, the lead-lag delays over which lead- and lag-alone click trains affect buildup differ. Lead-alone and silence psychometric functions diverge at all but the longest delays (<16 ms), suggesting that lead-alone click trains affect echo suppression below, at, and well above echo thresholds. In contrast, lag-alone and silence functions only diverge at longer lead-lag delays (~9–18 ms), suggesting that lag-alone contributions to buildup are a suprathreshold phenomenon. It is these differences in the domain of the effects that casts doubt on the generalized role of contrast in buildup. In light of these two nuances to our data, the notion of contrast as an explanation for changes in buildup and echo thresholds cannot be applied without qualification, nor can it be dismissed without exception. Instead, the data suggest that the idea of contrasting listening environments may only apply to lead-lag delays over which buildup can occur, and even then will depend on the nature of the change in contrast (e.g., buildup following a lead- or lag-alone click train). Further experimentation is warranted to explore this phenomenon.

C. Task and paradigm

Although the counting task used here was inspired by previous work (Clifton and Freyman, 1989), it deviates considerably from other tasks used to study echo suppression and buildup [see Litovsky *et al.* (1999) for a review]. The counting task provides several advantages over more commonly used condition → test paradigms. First and perhaps most importantly, the counting task assays buildup directly rather than indirectly through the use of a single test pair following a buildup conditioner. Consequently, the behavioral measure (number of sounds counted or a percentage thereof) is more indicative of the phenomenon of interest (buildup) and arguably less susceptible to latent factors. Second, using a buildup probe is potentially more informative than using a single test pair following buildup. For instance, a single counting trial can be used to measure the outcome of a manipulation on buildup (e.g., a change in conditioner), while many trials are necessary using a single test pair and a binary response (e.g., yes or no) to derive the same information. These aspects of the paradigm may prove to be advantageous in many experimental circumstances and dramatically reduce the time required to conduct studies.

Despite its advantages, the counting task is not appropriate in all experimental circumstances. First, it does not allow the experimenter to determine precisely when the echo is heard or not heard within the buildup probe. For example, a hypothetical response of "10" does not provide unambiguous evidence of which ten echoes were heard. Although listeners often report that echoes "fade out" over time, suggesting that the first ten echoes were perceived, some listeners report that sounds fade in or out-then-in throughout the buildup probe on occasion (see Table I). Thus, experimenters must be cautious in assigning a discrete perceptual outcome to a specific lead-lag pair within the buildup train. Second, the counting task is necessarily limited by how quickly and accurately listeners can count. Although listeners tend to perform quite well at rates at or below 5 Hz, performance is far more variable at faster rates (data not shown). Consequently, this would not be a suitable task to investigate the influence of presentation rate on buildup [*à la* Clifton and Freyman (1989)]. Third, the specifics of the across-condition effects reported here may depend on the number of lead-lag pairs in the buildup probe, particularly at longer lead-lag delays. For example, the data are qualitatively similar across all conditions with a lead-lag delay of ~18 ms in experiment 1 (cf. Fig. 2). However, if the buildup probe were modified to contain an additional twenty click pairs (thus expanding the response range) we would expect to see psychometric functions diverge at longer lead-lag delays. Thus, this paradigm would not necessarily be well-suited to study the effects of the number of lead-lag pairs on buildup or other common avenues of research. Finally, as with any task that assays a subjective measure, the counting task employed here is susceptible to changes in listener strategy or decision criterion (λ). There are established, objective measures (e.g., d') that have been used to study the precedence effect, but these measures are rarely applied to studies of the buildup of echo suppression (Litovsky *et al.*, 1999). Consequently, an objective alternative to subjective measures might not exist at present. Taken together, the counting task may be a useful tool, but its use should be carefully considered in each circumstance.

VII. CONCLUDING REMARKS

In the current report, we explored the effects of lead- and lag-alone click trains on the buildup of echo suppression in three independent listener pools and two listening conditions (free field and dichotic). While the data show unequivocally that a preceding lead-alone click train reduces or "depresses" subsequent buildup, the putative enhancement of buildup following a lag-alone click train depends on the selection of an appropriate baseline (silence but not the no-conditioner baseline; see Sec. V B). The depression and enhancement of buildup are qualitatively identical in free-field and dichotic listening conditions. Additionally, we demonstrate that the effects of unpaired click trains on buildup cannot be explained by a simple adaptive process, a change in perceptual salience, or a systematic change in counting strategy. Furthermore, findings in experiment 3 suggest that the importance of acoustic contrast between listening conditions to buildup must be qualified. Together, these data help clarify the phenomena of buildup and echo suppression, thereby deepening our understanding of communication in reverberant environments.

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