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Warning—taboo words ahead! Avoiding attentional capture by spoken taboo distractors

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ABSTRACT

We examine whether the disruption of serial short-term memory (STM) by spoken taboo distractors is due to attentional diversion and unrelated to the underlying disruptive effect of sound on serial STM more generally, which we have argued is due to order cues arising from the automatic pre-categorical processing of acoustic changes in the sound conflicting with serial–order processing within the memory task (interference-by-process). We test whether the taboo-distractor effect is, unlike effects attributable to interference-by-process, amenable to top-down control. Experiment 1 replicated the taboo-distractor effect and showed that it is not merely a valence effect. However, promoting cognitive control by increasing focal task-load did not attenuate the effect. However, foreknowledge of the distractors did eliminate the taboo-distractor effect while having no effect on disruption by neutral words (Experiment 2). We conclude that the taboo-distractor effect results from a controllable attentional-diversion mechanism distinct from the effect of any acoustically-changing sound.

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irrelevant speech; auditory distraction; serial recall; cognitive control; taboo words

Due to the sentinel capacity of hearing, sound is typically processed obligatorily and conveys information even when the organism is not paying attention to it. Whilst this is often highly advantageous (e.g. as an early warning system signalling dangers or opportunities, e.g. Johnston & Strayer, 2001), a highly prevalent negative consequence of the openness of audition is distraction (Hughes & Jones, 2003). Recent work suggests that there are two general mechanisms by which auditory distraction arises, one in which the involuntary processing of the pre-categorical acoustic properties of the sound (e.g. its physical features including pitch and timbre) interferes with processes involved in the focal activity (*interference-by-process*; see Jones & Tremblay, 2000; Marsh et al., 2009; Meng et al., 2020) and another—*attentional diversion*—in which the sound diverts attention from the focal activity, due either to an unexpected change within an otherwise predictable auditory sequence (Hughes et al., 2005) or due to the particular salience of the content of the sound (for an overview, see Hughes, 2014). In the present study, interest centred specifically on the particularly disruptive effect of spoken taboo distractors on serial recall (Röer et al., 2017). Of particular interest here is evidence that attentional diversion—but not the interference-by-process mechanism that underpins the changing-state effect—is amenable to some degree of top-down cognitive control (Bell et al., 2017; Hughes et al., 2013; Hughes & Marsh, 2020; Labonté et al., 2021; Marsh et al., 2018, 2020). We sought in the present study to exploit this difference between the two mechanisms of distraction to examine whether the taboo-distractor effect is caused by attentional diversion and is hence

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functionally unrelated to the interference-by-process mechanism that we argue underpins the changing-state effect.

The cognitive psychology of auditory distraction is often studied using the *irrelevant sound paradigm*. Typically, participants are visually presented with a relatively short list of verbal items (digits, words), presented at a rate of around 1 item per s, which they must then recall in serial order (short-term serial recall). On some trials, to-be-ignored background sound is played (usually over headphones) concurrently with the to-be-remembered items (or in some studies also during a short retention interval between the last to-be-remembered item and a recall cue). Despite the fact the sound is to be ignored, it markedly impairs serial recall (Colle & Welsh, 1976; Jones et al., 1992; Jones & Macken, 1993; Salamé & Baddeley, 1982).

The single most critical characteristic of sound that endows it with the power to disrupt serial recall is its segmentability into temporally discrete elements (e.g. due to silent gaps) and, just as critically, the presence of acoustic change between the resulting successive segments (Jones et al., 1992). For example, the sequence "A,B,A,B,A,B" invariably produces more disruption than "A,A,A,A, A,A". Importantly, this *changing-state effect* is not confined to speech; any kind of sound (e.g. a sequence of tones) disrupts serial recall appreciably if it contains segmentable elements that are changing acoustically (e.g. in fundamental frequency) from one to the next (Jones & Macken, 1993). A prominent account of the changing-state effect posits that it results from *interference-by-process* (e.g. Hughes, 2014; Jones & Macken, 1993; Jones & Tremblay, 2000). In this view, the acoustic changes in the sound are processed preattentively and obligatorily and these changes give rise to information regarding the order of the changing elements as a by-product of the perceptual organisation of sound (cf. Bregman, 1994). This task-irrelevant order information then conflicts with the similar, but this time deliberate, process of serially ordering the to-be-remembered items via articulatory serial rehearsal (Hughes, 2014; Hughes et al., 2007; Jones et al., 2004; Jones & Tremblay, 2000).

The importance of acoustic-level processing in the disruption of serial recall by irrelevant sound is underscored by a number of findings showing that higher-order properties of sound (e.g. when the sound is speech), such as meaning, have little or no effect. For example, spoken continuous English prose is no more disruptive than the same prose reversed (and hence rendered incomprehensible) or a Welsh translation of the same prose for non-Welsh-speaking participants (Jones et al., 1990). In line with the interference-by-process approach, the meaning of speech does, however, have a clear disruptive effect on tasks that themselves, unlike serial recall, involve a strong degree of semantic processing (see Marsh et al., 2008, 2009).

Seemingly at odds with the notion that serial recall is only vulnerable to the pre-categorical, acoustic, properties of irrelevant sound, however, are some studies indicating that the post-categorical, lexical-semantic, properties of speech can indeed influence the degree to which sound disrupts serial recall. Such studies provide evidence for post-categorical (e.g. following lexical-semantic identification) processing of irrelevant sound. For example, Buchner et al. (2004) reported that task-irrelevant spoken words with high valence—that is, words with a positive connotation (e.g. "food") or a negative connotation ("crash")—impaired serial recall more than did neutral words (e.g. "picture"). And of particular relevance to the current study, Röer et al. (2017) reported a taboo-word effect in serial recall: Irrelevant speech comprising taboo words—that is, words representing "the lexicon of offensive emotional language" (Jay, 2009, p. 153; e.g. "asshole"; "climax")—impairs serial recall more than irrelevant speech containing neutral words. Such post-categorical auditory distraction effects have been argued to be problematic for the view that serial recall is only disrupted by pre-categorical properties of irrelevant sound and, by extension, problematic for the interference-by-process account: Röer et al. (2017, p. 741). stated that "This [interference-by-process] view predicts that the semantic processing of irrelevant speech should not interfere with the primary task unless this task requires semantic processing as well." Röer and colleagues espouse the alternative view that all auditory distraction effects, including the changing-state effect, are due to attentional diversion (e.g. Röer et al., 2015, 2017).

However, to challenge the interference-by-process view on the basis of post-categorical distraction effects in serial recall is to overlook the fact that the interference-by-process mechanism has long been incorporated into a duplex-mechanism account of auditory distraction (Hughes, 2014; Hughes et al., 2007). This account makes a strong distinction between: (i) auditory distraction effects in serial recall that are due to interference-by-process and hence specific to the serial processes involved in serial recall; and (ii) effects that are due to a more general attentional diversion mechanism that can be found in the context of serial recall but that are not specific to serial recall (or serial processing more generally). On this account, attentional diversion has, in turn, two main causes: First, stimulus-aspecific attentional diversion occurs when a sound deviates from expectations based on the recent history of auditory input (cf. deviation effect; Hughes et al., 2005, 2007). Second, stimulus-specific attentional diversion can occur when the sound itself is particularly salient or relevant given the participant's long-term knowledge (Hughes, 2014; Marsh et al., 2018). From the standpoint of the duplex-mechanism account, then, whilst we agree with Röer and colleagues that valence- and taboo-distractor effects are due to (stimulus-specific) attentional diversion, we argue that they are unrelated to the classical, changing-state driven, irrelevant sound effect found in serial recall. In support of this view, Marsh et al. (2018) showed that a strong valence effect is found not only in serial recall but also in the context of a task that does not necessitate the processing of serial order (the missing-item task), whilst no changing-state effect is found in this case (unless participants happen to adopt a serial processing strategy; Hughes & Marsh, 2020).

According to the duplex-mechanism account, distraction effects that are due to attentional diversion are, unlike the changing-state effect, open to top-down cognitive control. For example, promoting levels of task-engagement by increasing the difficulty of encoding the to-be-remembered items attenuates the disruptive effect of an acoustic deviant (Hughes et al., 2013; Hughes & Marsh, 2020; Marsh et al., 2020) and the disruptive effect of valent words (Marsh et al., 2018) but has no influence on the changing-state effect (Hughes et al., 2013). Similarly, foreknowledge of the content of an upcoming irrelevant sound sequence attenuates attentional diversion by an acoustic deviant as well as that by meaningful spoken sentences but again does not modulate the changing-state effect (Hughes et al., 2013; Hughes & Marsh, 2020; see also Röer et al., 2015). This is in line with the duplex-mechanism account's supposition that the changing-state effect is driven by the automatic processing of the (acoustic features of the) sound sequence and that this processing inevitably disrupts performance so long as participants adopt an articulatory serial rehearsal strategy. In contrast, as attentional diversion effects are not contingent on the nature of the focal-task processing (e.g. Vachon et al., 2017) but rather are due to attention being drawn away from any (attentionally demanding) task, these effects are tempered by top-down factors that enhance attentional focus on that task (Hughes, 2014; Hughes & Marsh, 2020; Marois et al., 2019; Marsh et al., 2020).

Interest in the present study centres in particular on whether the taboo-distractor effect (Röer et al., 2017) is an attentional diversion effect by examining whether it, like other purported attentional diversion effects on serial recall (e.g. acoustic deviation effect, valence effect), is attenuated under high encoding-load (Experiment 1) or/and when foreknowledge of the distractor words is provided (Experiment 2). At the same time, we will examine whether the effect of a sequence of neutral words —which would be expected to produce a changing-state effect but little if any attentional diversion effect—is immune or more resistant to these top-down control manipulations. A dissociation in terms of the impact of encoding-load or foreknowledge on the taboo-distractor effect on the one hand and the impact of these same factors on the pre-categorically driven changing-state effect on the other would favour the duplex-mechanism account (Hughes, 2014) over the unitary account (e.g. Röer et al., 2017) of auditory distraction. In addition, we sought in Experiment 1 to determine whether the taboo-distractor effect is simply a valence effect; that is, we examined the possibility that taboo words are disruptive because such words tend to be valent rather than because they are taboo per se.

Experiment 1

In this experiment, we examined the disruptive effect on serial recall of auditory taboo distractors under two conditions designed to encourage different levels of top-down task-engagement. As noted, other effects attributed in previous work to attentional diversion have been found to be attenuated when the encoding-load within the focal recall task is increased. Specifically, when visual noise is added to the to-be-remembered items—thereby making their encoding more difficult (see Figure 1)—the disruptive effect of an acoustic deviant (Hughes et al., 2013) and that of valent words (Marsh et al., 2018) is attenuated. It has been argued that the increase in encoding-load triggers a voluntary boost in task-engagement level such that the "call for attention" (cf. Schröger, 1997) by the sound(s) can more readily be resisted (Hughes, 2014; Hughes et al., 2013). Based on the duplex-mechanism account, we made the following predictions: The disruptive effect of taboo distractors as compared with that of neutral words (with quiet as a control condition) should be attenuated under high encoding-load. In contrast, the disruptive effect of neutral words compared to quiet—which we would attribute largely to a changing-state effect—should not be modulated by encoding load. In contrast, the unitary account (Röer et al., 2017) posits that all auditory distraction effects are due to attentional diversion and, as such, predicts that the effects of taboo, valent, and neutral words (compared to quiet) should all be attenuated under high load.

We also included a valent-distractors condition in this experiment and predicted that the disruptive effect of valent compared to neutral words (cf. Buchner et al., 2004) would also be attenuated under high encoding-load, thereby replicating a finding first reported by Marsh et al. (2018). But the main reason for including a valent-distractors condition in the present experiment was that previous research has not directly compared the effect of taboo words with that of valent distractors. As such, it remains possible that taboo words—which are valent stimuli (typically negative; Janschewitz, 2008; Jay, 2009)—produce disruption relative to neutral words because they are valent, not because they are taboo per se. Here, therefore, we compared the effect of taboo words that were independently verified as low in valence with that of words verified as being higher in valence but non-taboo. If tabooness per se has disruptive power, then the taboo-but-relatively-low-valent words should be more disruptive than the highly-valent-but-non-taboo words.

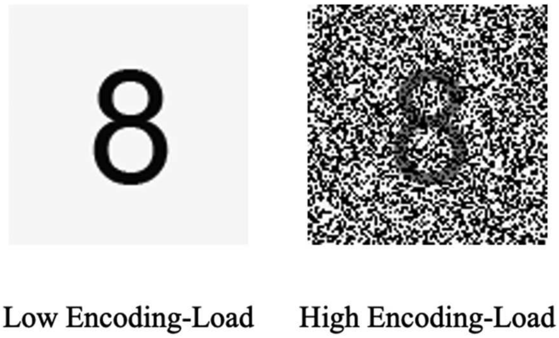

Figure 1. In the low encoding-load condition, all digits in a given list appeared as shown on the left; in the high encoding-load condition, they appeared as shown on the right (cf. Parmentier et al., 2008). See Method for further details.

Method

Participants

We first established a target sample size based on the taboo-distractor effect size reported by Röer et al. (2017). Given $\alpha = \beta = .05$, the assumption that the average population correlation between the two levels of the repeated measures factor is $\rho = .5$, and a taboo-distractor effect size of $dz = .514$, it was determined that a sample size of 43 participants would be adequate to detect the effect with a power of .95. We therefore recruited 50 participants (allowing for a few possible withdrawals or otherwise unusable datasets) who were all students at the University of Central Lancashire (UCLan). Participants took part either voluntarily or for course credits. In the event, all datasets were usable and so the data from all 50 participants were included in the following analyses. All participants reported normal hearing and normal or corrected-to-normal vision. Ethical approval was obtained for both experiments reported in this article via UCLan's ethical procedures.

Apparatus and materials

The serial recall task was executed on a PC running an E-Prime 2.0 programme (Psychology Software Tools) that controlled stimulus presentation and recorded participants' responses.

To-be-remembered items. On each trial, eight digits taken without replacement from the set 1—8 were presented on screen in a sequence. The digits were presented in a pseudo-random order with the constraint that no ascending or descending runs of more than two digits (e.g. 2–3–4 …) occurred in a given sequence. Digits appeared centrally on a white screen in black Times New Roman font, one at a time for 350 ms each with an inter-stimulus interval of 450 ms. Participants sat at a distance of approximately 50 cm from the screen with the digits thereby sustaining a visual angle of about 2.6°.

Encoding load was manipulated by varying the visibility of the digits: High encoding-load was implemented by overlaying a mask of Gaussian visual noise (400%) over each digit, with the transparency of the digit also set to 50% using Adobe Photoshop software (Parmentier et al., 2008; see also Hughes et al., 2013; Marsh et al., 2015, 2018; Vachon et al., 2020). Digits in the low encoding-load condition were shown clearly in black against a white background (cf. Figure 1).

Auditory distractor sequences. There were three types of auditory sequence—taboo, valent, and neutral—each containing eight words. The 24 (3 × 8) words were sampled from a set of 168 words used by Tipples (2010) that were in turn selected from those normed by Janschewitz (2008) for imageability, personal use, familiarity, valence, arousal, offensiveness, and tabooness. Although tabooness covers racial epithets and other insults alongside sexual words (see Jay, 1992, 2000), we decided to select only sexual words which included profanities, vulgarities and sexual terms, like those used by Röer et al. (2017). The mean and standard deviation of the seven qualities of words (collected by Janschewitz, 2008) for each of the three word-types are shown in Table 1. Regarding these qualities, Janschewitz (2008) defines "personal use" as the extent to which a participant uses the word themselves, "familiarity" as the extent of exposure to the word (read, heard, or otherwise) in any setting, "offensiveness" as the extent to which participants found the word personally upsetting or offensive, and "tabooness" as the extent to which participants viewed the word as upsetting or offensive to people in general (e.g. in multiple contexts by multiple people). Further, Janschewitz (2008) defines "valence" as the extent to which participants found the word good or bad (smaller values denote a negative valuation and higher values denote a positive evaluation), "arousal" as the extent to which participants found the word attention-grabbing or exciting, and "imageability" as the extent to which participants found it easy to generate a mental image of the word.

Nine one-way ANOVAs were run to test for differences in word length (no. of letters), number of syllables, imageability, personal use, familiarity, valence, arousal, offensiveness, and tabooness between the word types. F and p values for each ANOVA are displayed in Table 1. Significant differences between word-types were found for offensiveness, tabooness, valence, and arousal ratings. Post-hoc testing for offensiveness demonstrated that ratings given to taboo words were significantly higher than for neutral words ($MD = 1.543$, $SE = .345$, $p < .001$, 95% CI [.825, 2.260]) and valent words ($MD = 1.020$, $SE = .345$, $p = .008$, 95% CI [.303, 1.737]). No significant difference was found between neutral and valent words in terms of offensiveness ($MD = .523$, $SE = .345$, $p = .145$, 95% CI [−1.948, 1.240]).

For tabooness, post-hoc tests showed that ratings were significantly higher for taboo words compared to both neutral words ($MD = 3.971$, $SE = .448$, $p < .001$, 95% CI [3.041, 4.902]) and valent words ($MD = 3.048$, $SE = .448$, $p < .001$, 95% CI [2.117, 3.98]). There was a tendency for higher ratings to be given to valent words as compared with neutral words, but this difference did not reach significance ($MD = .924$, $SE = .448$, $p = .052$, 95% CI [−.007, 1.854]).

Unlike the case for offensiveness, tabooness, and arousal, the rating scale for valence used by Janschewitz (2008)—"How positive or negative is the word? Give a 1–9 rating whereby 1 is strongly negative, 5 is not negative or positive, and 9 is strongly positive"—

Table 1. Means and Standard Deviations of the ratings (collected by Janschewitz, 2008) for the neutral, valent, and taboo words used in the present study. Scales were 1–9 with low (1) through medium (5) to high (9) scores (e.g. for "arousal", 1 = not at all arousing, 5 = medium arousing, 9 = very arousing) with the exception of valence (where 1 = strongly negative, 5 = not negative or positive, 9 = strongly positive). F refers to the F value following an ANOVA by condition and p refers to the significance level of the analysis. Post-hoc analyses are presented within the text.

			Neutral Words		Valent Words		Taboo Words	
	F	p	M	SD	M	SD	M	SD
No. of letters	.117	.890	6	1.309	6.125	1.356	5.75	1.982
No. of syllables	.113	.894	2	0.535	1.875	0.641	1.875	0.641
Personal Use	.108	.898	4.313	1.18	4.273	1.656	4.030	1.017
Familiarity	.392	.680	4.88	1.118	5.346	1.385	5.309	0.967
Offensiveness	10.347	< .001	1.024	0.016	1.546	0.512	2.566	1.079
Tabooness	43.130	< .001	1.038	0.032	1.961	0.758	5.009	1.352
Valence	14.948	< .001	0.231	0.275	1.981	0.702	1.295	0.825
Arousal	69.886	< .001	1.381	0.367	3.744	0.722	4.821	0.638
Imageability	1.069	.361	6.528	1.444	5.589	1.569	5.528	1.583

contains a neutral point (5). Therefore, we subtracted 5 from the mean score for each of the words across the three word-types. Furthermore, since valent and taboo words can be rated as negative or positive, we removed the minus sign and therefore computed valence as the difference from neutral regardless of whether the valence was negative or positive (the means computed this way are presented in Table 1). Follow-up post-hoc tests revealed valence ratings to be significantly different for neutral words compared to both valent words ($MD = 1.750$, $SE = .323$, $p < .001$, 95% CI [1.079, 2.421]) and taboo words ($MD = 1.064$, $SE = .323$, $p = .003$, 95% CI [.3930, 1.735]), indicating that the latter two sets of words were more valent. Importantly, valence ratings were significantly different between valent and taboo words ($MD = .686$, $SE = .323$, $p = .045$, 95% CI [.0155, 1.357]), with the valent words being more valent than the taboo words.

Post-hoc tests demonstrated that arousal ratings were significantly higher for valent words compared to neutral words ($MD = 2.363$, $SE = .298$, $p < .001$, 95% CI [1.744, 2.982]), and for taboo words compared to neutral words ($MD = 3.440$, $SE = .298$, $p < .001$, 95% CI [2.821, 4.059]). Arousal ratings were also significantly higher for taboo compared to valent words ($MD = 1.078$, $SE = .298$, $p = .002$, 95% CI [.459, 1.697]).

These analyses support our assignment of the words to the specific types: In particular, in the study by Janschewitz (2008) taboo words were rated as more offensive and taboo than were valent words. The ratings are also consistent with empirical findings showing that subcategories of taboo words (e.g. sexual terms, vulgarities, profanities) are generally more arousing but not of greater valence than subcategories of valent (e.g. negative, positive) non-taboo words (Janschewitz, 2008). Indeed, the valent words sampled from the set Janschewitz (2008) were, according to those norms, rated as significantly higher in valence than taboo words, which allows us to test the notion that the taboo distractor effect is more than a mere valence effect in Experiment 1.

Each word was recorded to 16-bit resolution at a 22-kHz sampling rate using Audacity software. The voice conveying the words was the female English voice, Amy, from the Ivona text-to-speech website (https://www.ivona.com/). The words were spoken at an approximately even pitch. They were normalised to 65 dB(A) and were each edited to last 600 ms. There was a 200 ms inter-stimulus interval between each spoken word. The eight words within each auditory condition were presented in a different random order for each trial. The onset of each word co-occurred with the onset of each visual to-be-recalled digit. Auditory sequences were presented via Sennheiser HD closed-ear headphones at 65 dB(A).

Design

A 4(Auditory condition) × 2(Encoding-load: low, high) within-participant design was adopted. The dependent variable was serial recall performance (see Procedure and Results for more detail). The experiment was split into two blocks of trials, with one containing high encoding-load trials and the other containing low encoding-load trials, with the order of the blocks counterbalanced across participants. Each block contained 32 trials, made up of 8 trials per auditory condition. Within each block, the auditory conditions were assigned to trials in a pseudo-random order with the constraint that no auditory condition was encountered twice in immediate succession. This order was fixed across participants.

Procedure

Participants were informed orally and via an on-screen instruction that any sound heard through the headphones was irrelevant to the task and that it should be ignored. Participants placed their headphones on prior to beginning the task. Before the first block of 32 experimental trials, participants received three "quiet" trials. To begin each trial, participants clicked on a "begin trial" button on the screen. Following presentation of the final to-be-remembered digit and a retention interval of 10 s, participants were presented with a screen displaying an order-reconstruction task. Here, the eight digits were re-presented at random positions within a circular array. Below the array, eight horizontally arranged boxes were shown that corresponded to each position in the to-be-remembered list. Participants were required to recreate the serial order of the to-be-remembered list by selecting the digits in a forward serial order using a mouse-driven pointer. Upon selection, a digit disappeared for 50 ms before a duplication of the digit appeared in the response window in the current recall position. Once selected, a response could not be changed.

Results

Serial recall performance was scored according to the standard strict serial recall criterion: an item was only scored as correct if it was recalled in the same absolute serial position as that in which it was presented. Figure 2 shows the proportion of items correctly recalled in order in the four auditory conditions as a function of encoding load. It is evident that whilst the three types of sound sequence disrupted serial recall compared to quiet, disruption was greatest in the taboo condition, followed by the valent condition, followed by the neutral condition. However, it is also clear that, regardless of distractor-sequence type, encoding load had no influence on the extent of disruption.

An initial 2 (Block-order) × 4 (Auditory condition: Quiet, Neutral, Valent, Taboo) × 2 (Encoding load: Low, High) mixed ANOVA revealed no between-participants main effect of Block-order, $F(1, 48) = 1.777$, $MSE = 0.206$, $p = .189$, $\eta_p^2 = .26$, nor an interaction between Block-order and Auditory condition, $F(3, 144) = 0.310$, $MSE = 0.009$, $p = .818$, $\eta_p^2 = .006$, and so it was not included in the following analysis. Supporting our impression of the pattern of results in Figure 2, a 4 (Auditory condition) × 2 (Encoding load) repeated-measures ANOVA revealed a main effect of Auditory condition, $F(3, 147) = 45.561$, $MSE = 0.009$, $p < .001$, $\eta_p^2 = .482$. Pairwise comparisons (Least Significant Differences; LSD) revealed that, compared to quiet, performance was significantly poorer in the neutral ($p < .001$, 95% CI [.031, .093], Cohen's $d = 0.565$, $BF_{01} = 0.010$), valent ($p < .001$, 95% CI [.085, .136], Cohen's $d = 1.222$, $BF_{01} < 0.001$), and taboo ($p < .001$, 95% CI [.113, .177], Cohen's $d = 1.293$, $BF_{01} < 0.001$) conditions. Furthermore, compared to the neutral condition, performance was significantly poorer in both the valent condition ($p < .001$, 95% CI [.026, .071], Cohen's $d = 0.610$, $BF_{01} = 0.004$), and in the taboo condition ($p < .001$, 95% CI [.059, .107], Cohen's $d = 1.293$, $BF_{01} < 0.001$). Finally, performance with taboo distractors was significantly poorer than with valent distractors ($p = .003$, 95% CI [.013, .056], Cohen's $d = 0.450$, $BF_{01} = 0.099$). However, there was no main effect of Encoding load, $F(1, 49) = 0.066$, $MSE = 0.025$, $p = .79$, $\eta_p^2 = .001$, nor an Encoding load × Auditory condition interaction, $F(3, 147) = 0.59$, $MSE = 0.007$, $p = .623$, $\eta_p^2 = .012$.

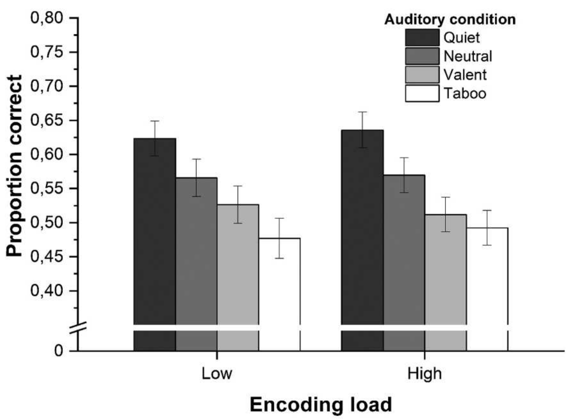

Figure 2. Proportion of items recalled in the correct serial position as a function of task encoding-load and auditory condition in Experiment 1. Error bars show the standard error of the mean.

Post-hoc across-trials analysis

One of the hallmarks of attentional diversion is the habituation of the attentional response with repeated exposure to the same material (Röer et al., 2017; Sokolov, 1963; Vachon et al., 2012). In the present setting, then, having been exposed to, and had attention drawn to, the taboo words, their effect on subsequent trials may be expected to be smaller due to their now greater familiarity. In contrast, given our view that the effect of neutral words compared to quiet is driven mainly by a changing-state effect rather than attentional diversion, no such systematic dimunition of that effect would be expected. We should sound a cautionary note here, however, that whilst we agree with an anonymous reviewer that examining a possible habituation effect in the present context was of potential value, the experiment was not designed for such an analysis and the data were likely to be noisy due to the small number of data-points per cell in the design.

A 4(Auditory condition: Quiet, Neutral, Valent, Taboo) × 2 (Encoding load: Low, High) × 8(Ordinal trial position: 1 through 8) revealed a main effect of Ordinal trial position, $F(7, 343) = 2.486$, $MSE = 0.060$, $p = .017$, $\eta_p^2 = .048$, as well as an interaction between Auditory condition and Ordinal trial position, $F(21, 1029) = 1.942$, $MSE = 0.055$, $p = .007$, $\eta_p^2 = .038$. There was no main effect of Encoding load, $F(1, 49) = .066$, $MSE = 0.201$, $p = .799$, $\eta_p^2 = .001$, no interaction between Encoding load and Ordinal trial position, $F(7, 343) = 1.519$, $MSE = 0.055$, $p = .160$, $\eta_p^2 = .030$, and no three-way interaction, $F(21, 1029) = 1.069$, $MSE = 0.052$, $p = .376$, $\eta_p^2 = .021$. It is evident from Figure 3, which shows the (predictably noisy) data collapsed across the Encoding load factor, that the taboo-word effect was particularly

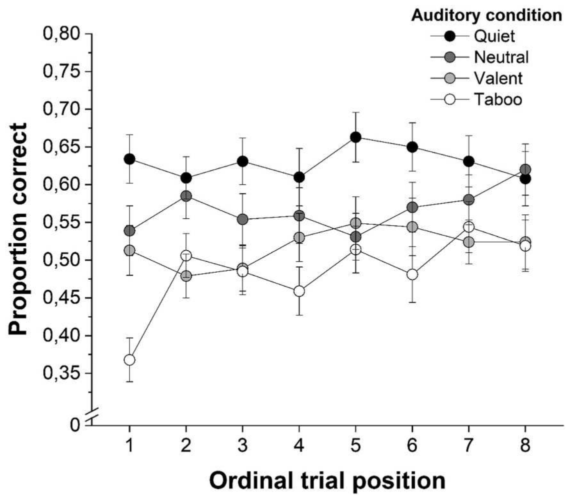

Figure 3. Proportion of items recalled in the correct serial position as a function of auditory condition and ordinal trial position in Experiment 1. Error bars show the standard error of the mean.

marked at Trial position 1. Indeed, there was a significant effect of Ordinal trial position in the taboo condition, $F(7, 343) = 5.573$, $MSE = 0.026$, $p < .001$, $\eta_p^2 = .102$, but not in any other auditory condition ($Fs < 1.44$, $ps > .187$). Thus, whilst there was some evidence of fast-acting habituation of the attentional-diversion response to the taboo distractors, there was no such evidence of habituation in relation to neutral or valent words.

On the view that the valence effect (valence vs. neutral) is also attributable to attentional diversion, we may also have expected the valence effect to show a habituation effect. The fact that it did not may have been due to the relatively low degree of sensitivity of the design to habituation effects alluded to earlier, especially in relation to an attentional diversion effect that was relatively small from the outset (compared to the taboo-distractor effect).

Discussion

Experiment 1 replicated the auditory taboo-distractor effect in serial recall: Performance in the presence of irrelevant taboo words was significantly poorer than in the presence of neutral words (cf. Röer et al., 2017). Moreover, the disruption produced by taboo distractors was significantly greater than that produced by valent distractors even though the valence of the latter, as validated through norming studies (Tipples, 2010), was higher. This suggests that the taboo-distractor effect is not merely a valence effect: some other property of taboo words, such as their "offensiveness", is responsible for their additional disruptive power over and above that of valent words. The results of an additional across-trials analysis suggested that this greater effect of taboo words compared to valent words was confined to the first time the taboo words were encountered (Trial position 1) due to fast-acting habituation of the attentional response to the taboo distractors (for a similar finding, see Marsh et al., 2014, Experiment 2. These results contrast with those of Röer et al. (2017) who observed no evidence of habituation of the taboo-distractor effect. However, this inconsistency across studies is most likely attributable to the fact that Röer et al. (2017) presented different taboo words on each trial whereas we presented the same words on each taboo trial. When taken together, the two studies suggest that habituation to taboo distractors might only operate at the level of the particular words and not at the level of tabooness as an abstract category.

High encoding-load did not, contrary to predictions, reduce the taboo-distractor effect or the valence effect. The latter finding is at odds with the attenuation of the valence effect by high encoding-load reported by Marsh et al. (2018). This failure to replicate the effect of encoding load on the valence effect raises doubts about whether we can infer much if anything from the absence of an impact of the load manipulation on the taboo-distractor effect. It may be the case that the manipulation simply did not serve to increase the difficulty of encoding the to-be-remembered items on this occasion or that even if it did increase encoding difficulty this did not for some reason boost task-engagement levels. For example, there was no main effect of encoding load. Whilst this was the case in previous studies too, in those cases the critical reliable interaction between encoding load and the distraction effect of interest was sufficient to indicate that the load manipulation had been successful (Hughes et al., 2013; Marsh et al., 2018). Here, the absence of both an interaction and a main effect of encoding load makes it difficult to tell whether the manipulation had the desired effect (we return to speculate as to why this may have been the case in the General Discussion). If it did not, then clearly the experiment does not speak to whether or not the taboo-distractor effect is amenable to cognitive control nor therefore to the duplex- and unitary-mechanism accounts of auditory distraction.

In light of the difficulty in interpreting the null effect of the encoding load manipulation on the taboo-distractor effect in Experiment 1, we turn in Experiment 2 to use an arguably stronger manipulation of top-down cognitive control, namely, to provide participants with foreknowledge—via a brief forewarning—as to the content of the imminent auditory distractor sequence.

Experiment 2

The disruption produced by an acoustic deviation in an irrelevant sound sequence (e.g. a single change of voice conveying a sequence of speech tokens) is abolished when participants are presented with a visual forewarning that the irrelevant sequence in the impending trial will contain a deviant (Hughes et al., 2013). Similarly, later studies demonstrated a reduction in the particularly disruptive impact of meaningful spoken sentences on serial recall when participants are provided with either a transcript of the impending irrelevant sequence or/and pre-exposed to the spoken sentence itself (Bell et al., 2017; Hughes & Marsh, 2020; Röer et al., 2015). Hughes and Marsh (2020) suggested that the additional disruption caused by a meaningful sentence—compared, for example, to that produced by a relatively meaningless sequence of discrete letter tokens—is due to its meaning holding "relevance" or "interest" for the participant. In this view, foreknowledge renders the sentence more familiar and hence less salient or interesting to the participant thereby reducing its disruptive effect. More generally, top-down information (as prior knowledge) alters a subsequent attentional response to otherwise distracting material.

Given that, on the duplex-mechanism account, taboo words are also particularly disruptive due to features associated with their semanticity, it follows that foreknowledge of their content should also attenuate the taboo-distractor effect. However, we also predict a dissociation whereby, as shown in previous studies (Hughes & Marsh, 2020; Röer et al., 2015), the effect of relatively meaningless and hence relatively "uninteresting" material (here a sequence of neutral words) will, in contrast, be immune to foreknowledge.

On the unitary account, it has been argued that the disruptive effect of a meaningful sentence is reduced by foreknowledge because the foreknowledge reduces the unpredictability of the auditory sequence hence reducing the likelihood of attentional capture (Röer et al., 2015). The observation that distraction from a neutral-words sequence, in contrast, is not modulated by foreknowledge (Röer et al., 2015; see also Hughes & Marsh, 2020) has been explained on this account by supposing that it is relatively difficult to build a stable mental representation (during the forewarning) of such a sequence because, unlike a sentence, it lacks grammatical and syntactical structure. As such, it should also be difficult to form a mental representation of a (non-sentential) sequence of taboo words and therefore this account predicts that there should be little or no effect of foreknowledge on disruption by taboo (as well as neutral) words.

As interest in the present study centres mainly on the taboo-distractor effect, we did not include a valent-words condition in this experiment and we also removed the retention interval[1] implemented in Experiment 1, the inclusion of which constituted a possibly important difference from the method of Marsh et al. (2018) (see General Discussion).

Method

Participants

Given $\alpha = \beta = .05$, the assumption that the average population correlation between the two levels of the repeated measures factor is $\rho = .5$, and a taboo-distractor effect size of $dz = .679$ obtained from Experiment 1, it was determined that a sample size of 25 participants would be adequate to detect the effect with a power of .95. Due to better than anticipated recruitment success we were able to test 44 students at UCLan. All participants reported normal hearing and normal or corrected-to-normal vision. Participants took part either voluntarily or for course credits. All participants spoke English as their first language. None had taken part in Experiment 1.

Apparatus and materials

The apparatus and materials were identical to Experiment 1 except for the following:.

[1]We included a 10-s retention interval in Experiment 1 because the intention was originally to concurrently record heart-rate variability and galvanic skin responses in a subset of the participants. Unfortunately, following the recruitment of participants for the "behavioural-only" part of the study, we encountered difficulty recruiting participants for the psychophysiological part of the study due to the onset of the COVID-19 pandemic.

Auditory distractor sequences. This experiment used two of the three types of auditory distractor sequence used in Experiment 1 (taboo and neutral) and again included a quiet control condition. In the with-foreknowledge condition, before each to-be-remembered list, participants were visually and auditorily presented with the words that were about to be presented as the distractor-sequence on that trial. Specifically, the exact same distractor sequence—same words in the same order with the same timings—to be used on that trial was presented over headphones before each to-be-remembered list that would be accompanied by a distractor-sequence. At the same time, a transcript of each distractor-word—displayed in 32 pt Times New Roman font—was presented on screen for 800 ms each to coincide with its spoken counterpart. The first word was presented on the far left of the screen, the second further towards the centre, and so on until the eighth word appeared on the rightmost side of the screen. Before each to-be-remembered list in the no-foreknowledge condition (for all auditory conditions), the words "No information" appeared on the screen in 32 pt Times New Roman font for the same duration as the foreknowledge information on with-foreknowledge trials.

Design

A 3 (Auditory Condition: quiet, neutral, taboo) × 2 (Foreknowledge: With-foreknowledge, No-foreknowledge) within-participant design was used, with serial recall performance as the dependent variable. The experiment was split into two blocks: a with-foreknowledge block and a no-foreknowledge block, with block-order counterbalanced across participants. Each block comprised 24 trials, 8 for each auditory condition. Within each block, no auditory condition was encountered more than twice in immediate succession.

Procedure

The procedure was the same as Experiment 1 with the exception that the instruction screen before each block informed participants that they would be pre-exposed auditorily and visually to the upcoming auditory distractor sequence (with-foreknowledge block) or see the words "No information" (no-foreknowledge block) before the onset of the to-be-remembered list. In both foreknowledge blocks, the foreknowledge period was followed by an 8-s delay (during which no stimuli were presented) before the to-be-remembered list commenced. The 10-s retention interval between the last to-be-remembered item and the order reconstruction display included in Experiment 1 was removed.

Results

Figure 4 shows the proportion of items correctly recalled in order as a function of foreknowledge and auditory condition in Experiment 2. First, it is clear that without foreknowledge, there was a marked disruptive effect of neutral words and a still larger disruptive effect of taboo words compared to quiet. Critically, whilst the provision of foreknowledge had little or no influence on the disruptive effect of neutral words, it brought.

performance in the taboo condition up to broadly the same level as that with neutral words. In other words, as predicted by the duplex-mechanism account, foreknowledge selectively attenuated the taboo-distractor effect.

An initial analysis revealed no between-participants main effect of Block-order, $F(1, 42) = 0.557$, $MSE = 0.104$, $p = .46$, $\eta_p^2 = .013$. Nor did Block-order interact Auditory condition, $F(2, 84) = 0.421$, $MSE = 0.011$, $p = .658$, $\eta_p^2 = .010$, or Foreknowledge, $F(2, 84) = 1.467$, $MSE = 0.010$, $p = .233$, $\eta_p^2 = .034$. There was also no Block-order × Auditory condition × Foreknowledge interaction, $F(2, 84) = 1.069$, $MSE = 0.007$, $p = .348$, $\eta_p^2 = .025$, and so it was dropped from the following analysis. A 3(Auditory condition: Quiet,

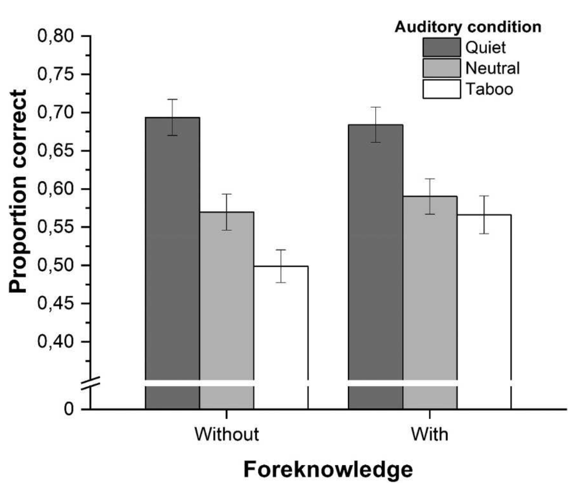

Figure 4. Proportion of items recalled in the correct serial position as a function of foreknowledge and auditory condition in Experiment 2. Error bars show the standard error of the mean.

Neutral, Taboo) × 2 (Foreknowledge: No-foreknowledge, With-foreknowledge) repeated measures ANOVA confirmed the pattern of effects evident in Figure 4: There were main effects of Auditory condition, $F(2, 86) = 53.679$, $MSE = 0.011$, $p < .001$, $\eta_p^2 = .555$, and Foreknowledge, $F(1, 43) = 6.757$, $MSE = 0.007$, $p = .013$, $\eta_p^2 = .136$, that were qualified by a reliable Foreknowledge × Auditory condition interaction, $F(2, 86) = 5.002$, $MSE = 0.007$, $p = .009$, $\eta_p^2 = .104$. A decomposition of this interaction revealed a significant difference between quiet and the neutral words condition in the no-foreknowledge condition ($p < .001$, 95% CI [.087, .161], Cohen's $d = 1.007$, $BF_{01} < 0.001$) and in the foreknowledge condition ($p < .001$, 95% CI [.054, .134], Cohen's $d = 0.716$, $BF_{01} = 0.001$). There was also a significant difference between quiet and taboo conditions in both the no-foreknowledge ($p < .001$, 95% CI [.152, .237], Cohen's $d = 1.397$, $BF_{01} < 0.001$) and foreknowledge conditions ($p < .001$, 95% CI [.069, .167], Cohen's $d = 0.735$, $BF_{01} = 0.001$). However, whilst there was a reliable difference between taboo and neutral words in the no- foreknowledge condition ($p < .001$, 95% CI [.036, .105], Cohen's $d = 0.624$, $BF_{01} = 0.007$), this difference disappeared in the foreknowledge condition ($p = .156$, 95% CI [−.010, .058], Cohen's $d = 0.218$, $BF_{01} = 3.132$). Finally, there was no difference between the neutral with-foreknowledge condition and the neutral no-foreknowledge condition ($p = .224$, 95% CI [−.054, .013], Cohen's $d = 0.186$, $BF_{01} = 4.078$).

Post hoc across-trials analysis

As for Experiment 1, we explored whether the auditory taboo-word effect diminishes over the course of an experiment and whether this interacts with the presence of foreknowledge. Although, again, the experiment was not designed for this purpose, one might reasonably hypothesise that the foreknowledge effect would be most evident early in the experiment before participants have gained foreknowledge via the repeated exposure (i.e. across trials) to the taboo words as irrelevant sound and from previous forewarnings.

A 3(Auditory condition: Quiet, Neutral, Taboo) × 2 (Foreknowledge: No-foreknowledge, With-foreknowledge) × 8(Ordinal trial position: 1 through 8) revealed a main effect of Ordinal trial position, $F(7, 301) = 2.619$, $MSE = 0.043$, $p = .012$, $\eta_p^2 = .057$, indicating that, overall, recall tended to decrease across trials. Foreknowledge interacted with Ordinal trial position, $F(7, 301) = 2.132$, $MSE = 0.042$, $p = .040$, $\eta_p^2 = .047$, but there was no interaction between Auditory condition and Ordinal trial position, $F(14, 602) = .793$, $MSE = 0.043$, $p = .677$, $\eta_p^2 = .018$. However, this was a consequence of the fact that Auditory condition and Ordinal trial position entered into a three-way interaction with Foreknowledge, $F(14, 602) = 2.032$, $MSE = 0.040$, $p = .014$, $\eta_p^2 = .045$. Further investigation showed that, as evident in Figure 5, this 3-way interaction arose because the effect of Foreknowledge across Ordinal trial position (i.e. the two-way

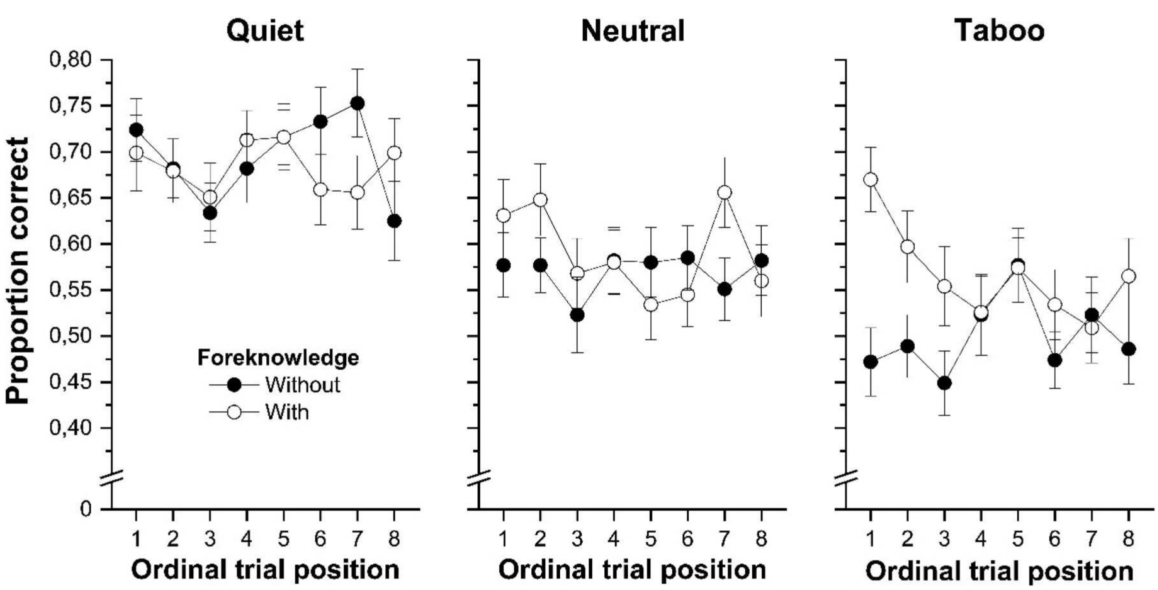

Figure 5. Proportion of items recalled in the correct serial position as a function of foreknowledge and ordinal trial position for each auditory condition in Experiment 2. Error bars show the standard error of the mean.

interaction between Foreknowledge and Ordinal trial position) was not significant for the neutral condition, $F(7, 301) = 1.585$, $MSE = 0.043$, $p = .139$, $\eta_p^2 = .036$, or the quiet condition, $F(7, 301) = 1.856$, $MSE = 0.036$, $p = .077$, $\eta_p^2 = .041$, but it was for the taboo condition, $F(7, 301) = 2.753$, $MSE = 0.041$, $p = .009$, $\eta_p^2 = .060$: the beneficial effect of foreknowledge was significant for the first 3 trials of the taboo condition ($ps < .031$)—where the taboo effect was relatively large—but not for the subsequent 5 trials ($ps > .078$).

Discussion

Experiment 2 demonstrated unambiguously that the auditory taboo-distractor effect (taboo words versus neutral words) in serial recall disappears when foreknowledge of the distractors is provided shortly before the to-be-remembered list whereas foreknowledge has no effect on the disruptive impact of neutral words. An across-trials analysis showed further that that the forewarnings only had an impact early during the experiment. This might be explained by supposing that the potential impact of the forewarnings becomes diluted by the fact that the taboo words are becoming increasingly familiar due both to forewarnings about those words given on previous trials and to the repeated exposure to the taboo words as irrelevant sound. However, an odd aspect of the across-trials results that does not cohere well with this interpretation is that receiving forewarnings on successive early taboo trials (trial-positions 1–4) appears to hurt recall. Before speculating any further based on the present (rather noisy) across-trials data, however, we suggest that a study that is specifically designed to have the power to clearly reveal the nature of such detailed effects is needed.

The effect of foreknowledge on the taboo-distractor extends that previously reported in the context of the acoustic deviation effect (Hughes et al., 2013) and the disruptive effect of meaningful sentences (Hughes & Marsh, 2020; Röer et al., 2015). Just as importantly, as in those previous studies, the effect of foreknowledge was selective: Whilst foreknowledge eliminated the particularly disruptive effect of taboo words it did so only to the level of that produced by neutral words and the disruption produced by neutral words as compared with quiet was unaffected by foreknowledge. This pattern is entirely in line with the duplex-mechanism account of auditory distraction (Hughes, 2014; Hughes et al., 2007): As a sequence of neutral words is likely to have little or no stimulus-specific (nor stimulus-aspecific) attention-diverting power, the disruption produced by such a sequence is likely to be attributable to its acoustic changing-state quality. As such, the disruption is not, on the duplex-mechanism account, expected to be modulated by prior knowledge or by top-down factors more generally.

On the unitary account, the (replicated) absence of a foreknowledge effect on disruption by the neutral-words sequence (compared to quiet) can be explained on the grounds that it is difficult to form a mental representation of such a sequence due to its lack of grammatical or syntactical structure (Röer et al., 2015). However, by the same token, it then becomes difficult for the account to explain why there was indeed an effect of foreknowledge on distraction from the taboo-distractor sequence, as this sequence also lacked grammatical or syntactical structure. Doubts have been raised in any case as to whether the unitary account's explanation of foreknowledge effects is logically consistent with its account of auditory distraction (Hughes & Marsh, 2020): Given that the sequential structure inherent in a sentence makes its elements more predictable than those in a neutral-words sequence—hence why it is supposed on this account that a forewarning about a sentence but not a neutral-words sequence is helpful—then the more predictable sequence (the sentence) should (in the absence of foreknowledge) be less disruptive than the less predictable sequence (the neutral-words sequence). The opposite is in fact the case (Hughes & Jones, 2020; Röer et al., 2015), in line with our view that it is the meaning of a sentence, not the predictability of its successive elements, that makes it both more disruptive than a neutral-words sequence but also more amenable to the mitigating effect of foreknowledge.

General discussion

The findings of the present study advance our understanding of the various ways in which cognitive performance is susceptible to auditory distraction and particularly the disruption caused by spoken taboo words (cf. Röer et al., 2017). Experiment 1 demonstrated that the taboo-distractor effect is not simply a distractor-valence effect (cf. Marsh et al., 2018): The taboo words deployed in Experiment 1 were of lower valence than the valent words, and yet the taboo words were more disruptive. Of interest therefore is the fact that the taboo words differed from valent words not only in terms of their "tabooness" but also in terms of their "offensiveness" and in terms of their degree of "arousal", according to the norms collated by Janschewitz (2008). As such, any or all these properties might drive their additional disruptive potency. Offensiveness and tabooness are both measures of (in)appropriateness but nevertheless are thought to be distinct: Whereas the offensiveness of a word is measured from the rater's personal point of view, tabooness relates to how the rater perceives the word's inappropriateness in relation to society as a whole (Janschewitz, 2008). Arousal refers to the capacity of a word to produce excitement or grab attention. The strong emotionality of taboo words may derive from their high arousal level (e.g. Jay, 2000; Kensinger & Corkin, 2003, 2004). However, there is an obvious circularity here since implying the taboo words may have caused attentional capture because they were high in "arousal" does not say anything if the definition of arousal includes "attention grabbing". Future work should aim to break this circulatory and systematically determine which psycholinguistic property is responsible for the greater disruption produced by taboo over valent words by controlling for their arousal ratings (Tipples, 2010). Furthermore, since arousal is typically measured physiologically, measurements of galvanic skin response and heart-rate variability could be used to independently validate the arousal-provoking nature of different distractor sequences (e.g. Harris et al., 2003; Huang & Nicoladis, 2020; Lang et al., 2005; Manning & Melchiori, 1974; McGinnies, 1949; Siddle et al., 1979).

Experiment 1 also sought to examine a possible dissociation, whereby, in contrast to the effect of neutral words, the effect of both taboo and valent words would be amenable to top-down cognitive control. That is, regardless of the specific property of taboo words that endows them with particular disruptive power, we posit that the disruption caused by both taboo and valent words over and above that caused by neutral words is due to a (stimulus-specific) attentional diversion mechanism that is under at least some degree of top-down control (Hughes, 2014; Marsh et al., 2018). In contrast, we suggest that most, if not all, of the disruptive effect of a neutral word sequence (e.g. compared to quiet) can be attributed to an acoustically-driven changing-state effect (cf. Jones et al., 1992), which appears to be resistant to top-down control (e.g. Hughes et al., 2013). As such, we predicted that high task-encoding load within the focal task—thought to increase levels of focal task-engagement (Hughes, 2014; Sörqvist & Marsh, 2015)—would attenuate disruption by both valent and taboo distractors but have no effect on the disruption produced by neutral words compared to quiet. In the event, encoding load had no effect on the disruptive effect of any of the different types of sound-sequence. Our tentative and admittedly not altogether satisfactory conclusion was that the encoding manipulation may simply have failed to make encoding more demanding on this occasion or that, even if it did increase encoding load, this did not boost task-engagement levels. If so, then clearly Experiment 1 was impotent with respect to providing a test of the duplex- and unitary-mechanism accounts of auditory distraction. One speculative possibility as to why load may not have had the intended effect could be based on the fact that the design of the current Experiment 1 included a 10 s retention interval between the last to-be-remembered item and a recall cue, unlike the study of Marsh et al. (2018) and, to the best of our knowledge, all other studies that have shown an effect of encoding load on auditory distraction (e.g. Hughes et al., 2013; Marsh et al., 2018). It seems possible that the meta-cognitive knowledge of there being more time to cycle through and consolidate the articulatory plan for output following the list (i.e. during the retention interval) reduced the need to boost task-engagement levels during list presentation in the face of the visual degradation of the to-be-remembered items. More specifically, high encoding load may boost task-engagement levels in the context of serial recall because it affects the timely "pick-up" of the items into an articulatory plan, which is in competition with the need to start cycling through and consolidating the order of items already assembled into that plan (cf. Hughes et al., 2016). Having a retention interval may delay somewhat the need to cycle through the plan hence allowing greater focus on the pick-up process, thereby weakening or eliminating the impact of a high encoding load. One way to examine this possibility in a future study would be to examine the effect of co-manipulating encoding load and the presence and length of a retention interval.

In Experiment 2, we took a different approach to testing the prediction of the duplex account of a dissociation between the taboo-distractor effect and the underlying changing-state effect (as operationalised in the present case as an effect of neutral words compared to quiet). Specifically, we examined whether the taboo-distractor effect is, in contrast to the effect of neutral words (i.e. the changing-state effect), attenuated by a forewarning about the nature of the upcoming distractor-sequence, as has previously been found for the acoustic deviation effect (Hughes et al., 2013) and the effect of meaningful sentences (Hughes & Marsh, 2020). And this was indeed what we observed: foreknowledge selectively abolished the disruption produced by taboo as compared with neutral distractors, leaving the effect of neutral words compared to quiet (i.e. the changing-state effect) relatively unaffected.

Whilst the results of Experiment 1 afford little theoretical leverage, then, Experiment 2 provides clear support for the duplex-mechanism account. In this view, the taboo-distractor effect is caused by stimulus-specific attentional diversion (cf. Hughes, 2014). In this sub-type of attentional diversion, the attention-diverting power of the sound arises from its specific content or quality; the stimulus diverts attention because it has some sort of relevance or interest for the organism (Hughes, 2014; Hughes & Marsh, 2020). From this standpoint, we suggest that a forewarning generated an expectancy for the particular taboo words to be heard during the upcoming trial thereby reducing their shock/arousal value and hence their disruptive effect. Consistent with this suggestion, Vanderhasselt et al. (2009) found that the left dorso-lateral prefrontal cortex (DLPFC)—which plays a key role in top-down attentional control—is activated when participants are able to prepare attentionally for an upcoming conflict. The left DLPFC is also involved in the intentional down-regulation of emotional responses, which might reduce the negative appraisal of taboo words (Clarke et al., 2020). One way to further examine the action of foreknowledge on the taboo-distractor effect would be to investigate the psychophysiological correlates of the attentional orienting response (cf. Sokolov, 1963; e.g. slowed heartrate, increased galvanic skin response, increased pupil dilation) or arousal responses to taboo compared to neutral distractors during foreknowledge and no-foreknowledge trials: We would predict that any differences in orienting/arousal response to taboo compared to neutral distractors would diminish or disappear in the foreknowledge condition. Finally, the fact that the taboo-distractor effect was strongest at early trial positions, before much exposure to the taboo words, is also consistent with the suggestion that their shock/arousal value is key.

A more general implication of the taboo-distractor effect is that it clearly shows that the meaning of to-be-ignored speech presented during serial recall is processed and that such processing can contribute to the disruption of performance (see Vachon et al., 2020). However, the present study reinforces our view (cf. Marsh et al., 2018) that such effects of meaning do not challenge the veracity of the interference-by-process account of distraction, contrary to arguments made by proponents of the unitary account (e.g. Röer et al., 2017). The duplex-mechanism account posits that, in the context of serial recall, interference-by-process is (only) responsible for the disruption caused by the obligatory processing of acoustic changes within a sound sequence. Any additional effect of meaning on serial recall—such as that from taboo words—is attributable to (stimulus-specific) attentional diversion, not interference-by-process (Hughes, 2014; Marsh et al., 2018).

Finally, we take the opportunity here to correct a mischaracterisation by Röer et al. (2017) of the interference-by-process component of the duplex-mechanism account. Röer et al. (2017) stated that we (e.g. Hughes, 2014; Marsh et al., 2009) assume an "automatic account of interference which construes the disruptability of working memory performance by irrelevant speech as a shortcoming of the cognitive system" and that "interference is … the result of pre-attentional processing of stimulus features that occurs because the cognitive system is leaky and cannot block off the processing of to-be-ignored stimuli completely … processing may 'spill over' to the auditory distractors, because the routines that are necessary to process task-relevant information also inadvertently process task-irrelevant information" (p. 741). Contrary to this description, the duplex-mechanism account is founded on a functionalist approach rather than the structuralist principles that this attribution implies. Specifically, it is not that the processes involved in the focal task inadvertently spill over to task-irrelevant information; rather, the nature of the processing involved in the focal task determines which properties of the (possibly fully processed) sound will interfere (Jones, 1995; see also Jones et al., 2012; Marsh et al., 2008, 2009).

To conclude, both attentional diversion and interference-by-process result from the functional, preattentive, processing of the auditory scene, but the specific way in which that processing disrupts performance differs. In the case of spoken taboo words presented during serial recall, for example, the acoustic-based processing of their serial order intrudes into the serial motor planning that supports the reproduction of the to-be-remembered list (interference-by-process). In addition, the preattentive processing of their meaning further disrupts performance by causing an involuntary attentional switch away from that motor planning process (attentional diversion).

Disclosure statement

No potential conflict of interest was reported by the author(s).

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Data availability statement

The data from this study are available from the corresponding author, JEM, upon request.

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