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Having a phone conversation delays but does not disrupt cognitive mechanisms

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Public Significance Statement

This study suggests that engaging in a phone conversation does not prevent people from performing a cognitive task. People remain able to learn spatial layouts and filter out irrelevant information when conversing and performing a visual search task. However, having a conversation leads to a consistent delay in their responses. This delay has consequences for applied tasks where the ability to respond quickly is important (e.g. driving).

Abstract

Previous research has shown that talking on a mobile phone leads to impairments in a number of cognitive tasks. However, it is not yet known whether the act of conversation disrupts the underlying cognitive mechanisms (the *Cognitive Disruption* hypothesis) or leads to a delay in response due to a limit on central cognitive resources (the *Cognitive Delay* hypothesis). We investigated this here using two cognitive search tasks that investigate spatial learning and time-based selection: Contextual Cueing and Visual Marking. In Contextual Cueing, responses to repeated displays are faster than those to novel displays. In Visual Marking, participants prioritise attention to new information and deprioritise old, unimportant information (the ‘Preview Benefit’). Experiments 1 to 3 investigated whether Contextual Cueing occurred while people were engaged in a phone conversation, whereas Experiments 4 to 6 investigated whether a Preview Benefit occurred, again while people were engaged in conversation. The results showed that having a conversation did not interfere with the mechanisms underlying spatial learning or time-based selection. However, in all experiments there was a significant increase in response times. The results are consistent with a Cognitive Delay account explaining the dual-task cost of having a phone conversation on concurrent cognitive tasks.

Key Words: Conversation, Dual-task, Contextual Cueing, Visual Marking, Preview Search

Introduction

The ability to attend to our environment and search through it efficiently is key in helping us to interpret and interact with the world around us. However, our attention is limited, leading to dual-task deficits and task switching costs if we have to use more cognitive resources than are available in order to complete our goals (e.g., Rogers & Monsell, 1995; Monsell, 2003, Kahneman, 1973). One clear example of this is the dual-task cost observed as a result of talking on a mobile phone (Strayer & Johnston, 2001) whilst also trying to perform other important cognitive tasks (such as driving or paying attention to the world around us).

Although people frequently converse via mobile phones, previous research has reported severe costs of such actions in a number of applied social and cognitive tasks (e.g., Breim & Hedman, 1995, Strayer, Drews, & Crouch, 2006; Strayer & Johnston, 2001). For example, it has been shown that the act of conversing on a mobile phone has led to participants showing no memory for road signs that they have directly fixated (Strayer & Drews, 2007) and to miss highly salient events which were easily noticeable when participants were not talking on a mobile phone (Hyman et al., 2009). There is also substantial evidence showing that phone conversations lead to deficits in driving performance (McKnight & McKnight, 1993; for reviews of this area see Collet, Guillot & Petit, 2010; Haigney, & Westerman, 2001; McCartt, Hellinga & Bratiman, 2006) and that having a phone conversation may increase the likelihood of a vehicular collision by four times (Redelmeier & Tibshirani, 1997). Not only does having a phone conversation lead to deficits in tasks like driving but it can also lead to impairments in other real-world tasks such as pedestrian situational awareness (leading to an increase in injuries of pedestrians who talk on a mobile phone, e.g. Nasar & Troyer, 2013) and attention failures in information processing (e.g. Steinborn & Huestegge, 2017). Given that mobile phone use is ubiquitous in many everyday situations it is important to understand how having a phone conversation interferes with concurrent tasks that we might be engaged with.

Recent research has started to look at how results from laboratory experiments can be used to benefit real-world driving issues (Gunnell et al., 2019). For example, laboratory experiments have shown that a dual-task deficit of having a conversation is not limited to the motor conflicts resulting from physically holding a hand-held device but is also observed in conditions in which the conversation takes place on a *hands-free* device (Strayer & Johnston, 2001, Kunar et al, 2008, Kunar et al., 2018, Strayer, Drews, & Crouch, 2006). This finding is important given that the current legislation bans phone conversations using hands-held devices while driving, whereas the use of hands-free devices has not been prohibited. Recently, Kunar et al. (2018) examined how conversation using a hands-free phone affected people's ability to perform a visual search task. In this task they had participant's complete an 'easy' task in which participants searched for a red letter among green distractors. This type of search is known to be highly efficient, whereby the Reaction Time x Display Size function is close to 0 ms/item (e.g., Treisman & Gelade, 1980, Wolfe et al., 2005). They also completed a difficult search task where they were asked to search for a letter 'T' among distractor 'L's. In this type of task, search is inefficient, typically producing a steep Reaction Time x Display Size function which is greater than 0 ms/item. The results showed that conversation led to a delay in response times regardless of task difficulty: Reaction Times (RTs) increased in both the easy and difficult search task. Of note, despite this delay in response there was no observable effect of conversation on the search slopes – even in the difficult search task. The results suggested that the efficiency of the overall spatial search mechanism was not impacted by holding a conversation.

It is clear that talking on a mobile phone causes impairments in a variety of cognitive tasks. However, what is not known is why this impairment occurs. From an applied perspective, as the popularity of the smart phone (and their use by drivers) is growing (Glassbrenner, 2005) it is important to investigate whether having a mobile phone conversation interferes with the underlying mechanism behind a task or leads to a delay in doing a task. As mentioned above Kunar et al. (2018) suggested that conversation leads to a *delay* in response rather than a *disruption* of cognitive mechanisms, however, this has not been directly tested. One reason why Kunar et al. (2018) only found a delay in responses rather than a disruption of the search task might have been that they only investigated the dual-task impact of conversation in a 'standard search' It is possible that even though the more difficult search task demanded additional resources, there remained sufficient capacity for a conversation to be held without affecting the search processes themselves (i.e., the rate of visual scanning and the processes involved in selecting and rejecting items). The overall RT cost due to conversation might then reflect a relatively fixed delay in the initial onset of search or to making a manual response. We investigate this here by examining the effect of conversation on visual search tasks in which *additional* mechanisms are involved beyond those present in 'standard' spatial search tasks: Contextual Cueing (involving associative learning) and Visual Marking (using the 'Preview Search' paradigm to investigate temporal selection). If conversation leads to a disruption of search mechanisms, we would expect to find it here, given that the mechanisms underlying these search tasks are susceptible to dual task-interference (Manginelli, Langer, Klose and Pollmann, 2013, Travis et al., 2013, Humphreys, Watson and Jolicœur, 2002, Kunar Shapiro & Humphreys, 2006).

<u>Contextual Cueing and Preview Search: An Overview</u>

Contextual Cueing and Visual Marking have been demonstrated numerous times and are known to produce robust effects. From an applied point of view, both search tasks demonstrate mechanisms that are used in real-world behaviour. Contextual cuing relates to our ability to learn and benefit from predictive spatial layouts, helping us identify, orient to, search and navigate familiar environments. Visual Marking relates to our ability to filter out irrelevant information allowing us to detect and prioritise the selection of new and important information.

Contextual Cueing reveals that we are faster to respond to displays that we have seen before as the repeated displays either guide attention to the target (Chun & Jiang, 1998) or facilitate response selection (Kunar et al., 2007). In Contextual Cueing experiments, participants are asked to typically search for a letter T among letter Ls (e.g., Chun & Jiang, 1998[1]). Across the experiment, some of the displays are repeated. Chun and Jiang (1998) found that RTs to find the target in repeated ('Old') displays were faster than RTs to find targets in the unrepeated ('New') displays that had not been seen before – a benefit known as the Contextual Cueing (CC) Effect.

Visual marking, based on the preview search paradigm, demonstrates that attention can be selectively applied to new information, at the expense of old information in order to aid search (Watson & Humphreys, 1997). In Preview Search a visual search task is presented across time so that half of the distracters were presented for a 'preview' period before the onset of the other half of the distractors and the target item, if present (Watson & Humphreys, 1997). The target was never in the previewed stimuli meaning that it was beneficial for participants to ignore or suppress them. Search efficiency (as measured with RT x Display Size slopes) in the preview condition was compared to performance in a Full Element Baseline (FEB) in which all items appeared at the same time. The results showed that search in the preview condition was more efficient than search in the FEB condition; suggesting that participants were only searching the new items and had deprioritised attention to the old items. Please note the preview period had to be at least 400ms for a full benefit to occur (Watson & Humphreys, 1997, Kunar et al., 2003). Watson and Humphreys (1997) argued that the previewed items were actively inhibited during the preview period – a process known as Visual Marking. However, alternative explanations have also been proposed. For example, Donk and Theeuwes (2001) argued that

[1] Although other types of cues, such as backgrounds, can be used as predictive contexts (e.g. Brockmole & Henderson, 2006, Kunar et al., 2006, 2014).

the preview effect occurred as new items automatically captured attention via bottom-up processes (a ‘New Onset’ account), whereas, Jiang et al (2002) suggested attention could be selectively applied to the new items as they had formed a ‘temporal group’ that separated them from the old items.

Although on the face of it Contextual Cueing and Visual Marking represent different abilities of our visual system, they share a number of common factors. First, both mechanisms require attentional resources, without which the effects are reduced or do not occur (Jiang & Chun, 2001, Watson & Humphreys, 1997, Humphreys, Watson and Jolicœur, 2002, Kunar, Shapiro & Humphreys, 2006). Second, both mechanisms involve a two-stage procedure. In Contextual Cueing the initial stage requires the learning of display regularities whereas the second phase involves the expression of the effect. Recent CC studies have shown this by adapting the study design to include a ‘Training’ stage, where participants are given the opportunity to implicitly learn the repeated information and a ‘Test’ phase, which is used to test how this knowledge facilitates the overall search process (e.g., Brady & Chun, 2007; Kunar & Wolfe, 2011; Makovski & Jiang, 2010, Kunar et al., 2014). In Preview Search, in order to become visually marked, the old items have to undergo an initial encoding and inhibitory set up stage, before a second stage which maintains the inhibition once established (Humphreys, Watson and Jolicœur, 2002, Kunar, Shapiro & Humphreys, 2006).

Finally, and of relevance to the studies in the current paper, both mechanisms are susceptible to dual-task interference. Although Vickery Sussman and Jiang (2010) initially found no interference of visual working memory when it was applied to the training phase of CC, Manginelli, Langer, Klose and Pollmann, (2013) found that dual-tasks involving visual spatial working memory interfered with the *expression* of Contextual Cueing (see also Annac et al., 2013). Furthermore, Travis et al., (2013) suggested that Vickery et al.’s (2010) working memory (WM) manipulation may not have been strong enough to interfere with contextual cueing. When they included novel displays throughout the experiment and changed the WM load to a more spatial based (rather than object based) task they found a dual-task interference on CC.

With respect to Visual Marking, Humphreys, Watson and Jolicœur (2002) introduced a WM task in which participants had to attend to and find a target in a centrally presented digit stream that appeared at the onset of the preview. After responding to the preview display, participants were asked to report if a target digit was present or not. The digit stream could either be presented in the auditory or visual modality. In both modalities, the dual-task of attending to the digits presented during the initial part of the preview display disrupted preview search. However, if the digits were presented halfway through the preview period, only the visual task disrupted preview search. This is consistent with an initial visual marking setup phase which requires general modality resources, followed by a maintenance stage which relies on visual modality resources.

Current Research

The experiments presented in this paper examined the dual-task effect of having a phone conversation in both Contextual Cueing and Preview Search tasks. As discussed, mobile phone conversations lead to a number of dual-task decrements. However, the locus of conversational interference is not yet known. One hypothesis is that the act of conversation simply causes a fixed, additive delay in response, similar to the Psychological Refractory Period (Welford, 1952). This *Cognitive Delay* hypothesis suggests that one task must be selected for continued processing at the expense of another due to restrictions of a central attentional bottleneck (Pashler, 1994). This fixed delay would suggest that the interference point from the conversation either occurred at the pre-attentive, perceptual processing stage (involving processes such as segmentation, grouping etc.) or added a fixed delay to the response stage in visual search (see Figure 1a). Importantly a fixed delay would not prevent a cognitive mechanism from eventually operating. Instead, such a delay would predict that a CC Effect and Preview Effect would still be observed, however search times would be longer overall compared to when no conversation takes place. Conversely, an alternative account suggests that conversation interferes with the underlying cognitive mechanisms that enable us to process and attend to the world. In this case we may no longer be able to perform certain cognitive tasks. In terms of Contextual Cueing, this would mean interference to the associative learning process so that predictive contexts were no longer able to be learned (see Figure 1b), whereas in Preview Search this would mean interference to the inhibitory process so that old items would no longer be deprioritised from search (see Figure 1c). This *Cognitive Disruption* hypothesis predicts that there would be an attenuation, if not complete removal of CC and Preview Effects when participants were conversing.

Figure 1 about here

In Experiments 1 to 3, we examined CC under conditions in which people were engaged in a naturalistic phone conversation. In Experiment 1, participants conversed in the 'Training' phase of the CC task – the phase in which participants implicitly learn the spatial layout of the displays (see also Manginelli et al., 2013, and Travis et al., 2013, for similar designs). In contrast, in Experiment 2 participants conversed in the expression or the 'Testing' phase of the CC task. In Experiment 3 we manipulated the difficulty of learning by including novel displays from the outset (Travis et al., 2013) and also engaged participants in conversation throughout the experiment. In Experiments 4 to 6, we examined Preview Search while people were engaged in a phone conversation. In Experiment 4 participants were given a preview period of 1000ms, which previous research has shown to be ample time to establish a preview benefit.

In Experiments 5 and 6, the preview periods were reduced so that there was less time to set up and deploy the processes required for a preview benefit.

Experiment 1: Contextual Cueing with Conversation in the Training Phase

Method

Participants

Sixteen participants took part in this experiment (4 male, Mean Age = 19.7 years). All confirmed that they could easily hear the experimenter in the conversation condition and that they could see the visual display. In all experiments the sample size was guided by previous literature (e.g., Chun & Jiang, 1998; Kunar, Flusberg, Horowitz & Wolfe, 2007, Watson & Humphreys, 1997) and had a minimum of 14 participants per experiment. A power analysis (F-test, repeated measures, effect size = 0.25, alpha = 0.05) indicated that this is the minimum number needed to achieve a power of 0.95. However, please note that the exact number of participants varies between each experiment. Despite an ideal sample size being established, the practicalities of recruiting participants sometimes resulted in more participants completing the experiment than had been anticipated. In these cases, for ethical reasons, the participants were allowed to take part. Despite these minor variations in participant numbers the predetermined minimum sample size was exceeded for all experiments. Full ethical approval for this work was granted by the Department of Psychology Ethics board, of the University of Warwick.

Stimuli and Apparatus

Displays were generated and responses recorded by custom written computer programs running on a PC. The conversation condition required the use of two hands-free phones, fitted with an internal speaker. The participants' phone was positioned to the left of the participants' monitor. The experimenter's phone was located in a separate laboratory room.

Participants were asked to search for a target among distractor items. Each display contained 12 stimuli, one of which would be a target. Stimuli were placed within a 6 × 6 matrix. When placing the stimuli, random noise was added to the location coordinates so as to ensure that the stimuli were jittered inside each cell of the matrix. The target was a T shape which was orientated either 90 degrees clockwise from vertical or 90 degrees anticlockwise from vertical. Distracters were L shapes which contained a small offset at the line junction to make search more difficult (e.g. Russell & Kunar, 2012). The distracters were also orientated either 90 degrees clockwise from vertical or 90 degrees anticlockwise from vertical. Participants were required to respond to the orientation of the target, pressing the 'M' key for a clockwise orientation and the 'Z' key for an anticlockwise orientation. All stimuli were white and subtended 1.7° x 1.7°, at a viewing distance of 57 cm. The background of the display was black. Example displays can be seen in Figure 2.

Figure 2 about here

There were two types of display: Old (repeated displays) and New (unrepeated displays). For the Old displays, stimulus positions were randomly generated for each participant at the beginning of the experiment and remained unchanged throughout the experiment. For the New displays, the target positions were generated at the beginning of the experiment, however, the distracters were randomly positioned on each trial. The target and distractor orientations were randomised on every trial.

<u>Design and Procedure</u>

There were two conditions: A Conversation condition and a No conversation condition. Both conditions consisted of a Training Phase, which established learning, and a Test Phase, which measured CC (see also Brady & Chun, 2007, Makovski & Jiang, 2010, Kunar & Wolfe, 2011, Kunar, John & Sweetman, 2014). The Training Phase consisted of seven epochs (Epochs 1-7). Each epoch had four repeated displays which were shown eight times per epoch. Thus, over 7 epochs, each repeated display was shown 56 times. Previous work has shown that by increasing the number of repetitions and decreasing the number of repeated displays, stronger contextual learning occurs, resulting in large contextual cueing effects (e.g., Kunar et al., 2006, 2007, 2008). This is important when it comes to comparing contextual cueing across conditions as it is crucial to demonstrate strong contextual learning to start with, before measuring any detrimental effect, resulting from the experimental manipulation (e.g., Kunar et al., 2014). The Test Phase was presented straight after the training phase and contained three epochs (Epochs 8 – 10). In each epoch the four repeated trials from the training phase were shown eight times per epoch alongside 32 unrepeated trials which had never been seen before. Following past studies, the number of possible target locations in the New trials matched those of the Old trials to show that in Old trials participants were learning the contexts and not just the target locations (e.g., Chun & Jiang, 1998, Kunar et al., 2007, Kunar & Wolfe, 2011).

In the conversation condition participants were required to hold a hands free telephone conversation with the experimenter only in the training phase of the task. Throughout this study when conversation was used as an experimental variable it was designed to be as close a proxy to a 'normal' conversation as possible (see also Tillman, Strayer, Eidels, & Heathcote, 2017, Sanbonmatsu, Strayer, Biondi, Behrends, & Moore, 2016, Kunar et al., 2018 for similar methods). Therefore, the majority of the conversations covered some or all of the following topics: life at university, accommodation, food, friends and family, pets and travelling/holidays. This list was not exhaustive and the conversation was designed to flow naturally throughout the experiment. Our goal was for the participant and experimenter to have a 'normal' conversation - therefore it was not rigidly scripted and was framed as a two way conversation which would be familiar to anyone who has attempted to get to know someone on a university campus[2]. The experimenter's role was to ensure that the conversation was as smooth as possible and that the participant and experimenter contributed approximately equally. For the most part the participant was allowed to guide the conversation should they wish to, however the experimenter would change topics when necessary, for example, steering the conversation away from potentially highly emotive themes. The main role of the experimenter in the conversation was to encourage equal participation from the participant and to make sure that there were no periods of silence. Should the conversation reach its natural end then the experimenter would begin a new conversation based on the topics outlined above.

Conditions were counterbalanced and RTs and error rates were recorded. Participants were asked to respond as quickly but as accurately as possible and completed a short practice session prior to the experimental blocks.

Results

The overall error rate was low at 1.3% and errors were not analysed further. All trials where RTs were less than 200ms were removed (0 trials). An outlier removal procedure was then performed to remove any RT which deviated by more than 3SD from the mean of their respective cell (1.6% of the data). Figure 3 shows the mean correct RTs. In addition to standard frequentist statistics, we also report the Bayes Factors from Bayesian t-tests (calculated with a Cauchy prior width of 0.707 using JASP version 0.9.2) for the most critical comparisons. Following the guidelines of Jeffreys (1961), a BF_{10} (which compares evidence of the alternative hypothesis to evidence for the null hypothesis) of 1 to 3 provides *anecdotal* evidence for the alternative, a BF_{10} of 3 to 10 provides *substantial* evidence for the alternative and a $BF_{10} > 10$ provides *strong* evidence for the alternative, with the inverse of these numbers (BF_{01}) providing support for the null hypothesis (Jarosz & Wiley, 2014). Given that determining Bayes factors

[2] Because we were interested in the effects of natural conversation on attention the conversations were by definition variable across participants (e.g., for each conversation the topics could vary and evolve in different ways across time).

for repeated measures designs is still a challenging and ongoing topic of research (Wagenmakers, et al. 2018) we used Bayesian t-tests to compare the CC Effects in Experiments 1-3 and Preview Effects (using a Preview Effect Ratio) in Experiments 4-6 between Conversation and No Conversation conditions.

Figure 3 about here

Training Phase

Mean correct RTs were analysed using a 2 (Conversation Condition: Conversation vs No conversation) × 7 (Epoch: 1-7) repeated measures ANOVA. A significant difference was found between RTs in the Conversation and the No-conversation condition $F(1,15) = 20.02$, $p < 0.01$, $\eta_p^2 = .572$, with participants slower to respond in the Conversation condition. A significant main effect of epoch was also found, $F(6, 90) = 22.13$, $p < 0.01$, $\eta_p^2 = .596$, where RTs decreased across the training phase. The Conversation × Epoch interaction was not significant, $F(6, 90) = 1.72$, $p = .13$, $\eta_p^2 = .103$.

Test Phase

To investigate the extent of cueing, mean correct RTs in the Test Phase were entered into a 2 × 2 × 3 repeated measures ANOVA with factors of Conversation[3] (Conversation vs No Conversation) × Context (Context: Old vs New) × Epoch (Epoch 8-10). There was a main effect of Context, $F(1,15)=28.0$, $p < 0.01$, $\eta_p^2 = .651$, where RTs were shorter for the Old compared to the New displays and of Epoch $F(2, 30)= 11.1$, $p < 0.01$, $\eta_p^2 = .424$, where RTs decreased across epoch. However, there was no significant main effect of Conversation, $F < 1$. The Context x Epoch interaction was significant, $F(2, 30) = 2.20$, $p < 0.05$, $\eta_p^2 = 0.257$.

[3] Please note that there was no conversation held in the test phase. The conversation factor as used here relates to whether or not a conversation had been held in the associated training phase of the experiment.

However, importantly the Conversation × Context interaction was non-significant, $F(1,15) = 2.33$, $p = 0.15$, $\eta_p^2 = .134$. None of the other interactions were significant (All Fs < 1).

To compare the CC Effects in each experiment we calculated the difference between Old and New RTs collapsed across the Test Epochs for both the Conversation and No Conversation conditions (e.g. see Kunar, John & Sweetman, 2014, Chun & Jiang, 1998; Kunar et al., 2006, 2007, 2008a; Kunar & Wolfe, 2011, who used similar methods). The results indicated that there was no significant difference in the strength of CC between the Conversation and No-Conversation conditions, $t(15) = 1.5$, $p = 0.15$, with anecdotal evidence in support of the null, $BF_{10} = 0.67$.

Discussion

Participants in Experiment 1 held a naturalistic conversation with the experimenter in the Training phase, the stage at which Old spatial contexts are implicitly learnt. However, despite this, participants still showed a reliable CC Effect in the Test phase. Specifically, RTs to the Old displays were faster than those to the New Displays in both the Conversation and No Conversation condition Importantly, the magnitude of the CC Effect did not differ between the Conversation and No Conversation conditions suggesting that participants' ability to learn spatial contexts was not impaired by having a conversation. This finding does not match the prediction from the Cognitive Disruption hypothesis, according to which having a conversation would interfere with the mechanism underlying spatial learning. Instead, the results fit with a Cognitive Delay account which predicts a general slowing of RTs as a result of holding a conversation.

Despite the fact that Experiment 1 found no effect of naturalistic conversation on participants' ability to learn spatial contexts, it remains to be seen if participants are able to express these learnt contexts, as efficiently, if they are concurrently holding a conversation. Manginelli, et al. (2013) found no dual-cost of a working memory task in the Training phase of a Contextual Cueing task but interference effects were found in the Test Phase. Therefore, it is possible that conversation might well have an effect in the Test Phase. This was assessed in Experiment 2.

Experiment 2: Contextual Cueing with Conversation in the Test Phase

Method

Participants

Eighteen participants took part in return for payment or course credit (Male = 5, Mean Age = 21.7 years). All confirmed that they could easily hear the experimenter in the conversation condition and that they could see the visual display.

Stimuli and Apparatus

The stimuli and materials used in this experiment were identical to those used in Experiment 1.

Design and Procedure

Experiment 2 was similar to Experiment 1, except that participants conversed with the experimenter during the Test phase of the experiment (Epochs 8-10) and not during the Training phase.

Results

The overall error rate was low at 1.5% and errors were not analysed further. All trials in which RTs were less than 200ms were removed (0.1 % of trials). An outlier removal procedure was then performed to remove any RT which deviated by more than 3SD from the mean of their respective cell (1.6% of the data). Figure 4 shows the mean correct RTs.

Figure 4 about here

Training Phase

Mean correct RTs were analysed using a 2 (Conversation Condition: Conversation vs No conversation) × 7 (Epoch: 1-7) repeated-measures ANOVA. There was a significant main effect of Epoch, $F(6, 102) = 18.9$, $p < 0.01$, $\eta_p^2 = .526$, where RTs decreased across the training phase. There was no significant main effect of Conversation, $F(1,17) = 1.47$, $p = .242$, $\eta_p^2 = .079$. Neither was the Conversation × Epoch interaction significant, $F < 1$.

Test Phase

Analysis of mean correct RTs in the Test Phase, using a 2 (Conversation: Conversation vs No Conversation) × 2 (Context: Old vs New) × 3 (Epoch: 8-10) repeated measures ANOVA revealed a main effect of Conversation, $F(1,17) = 7.70$, $p = 0.01$, $\eta_p^2 = .312$, where RTs were slower in the Conversation compared to the No Conversation condition. There was also a main effect of Context, $F(1,17) = 32.9$, $p < 0.01$, $\eta_p^2 = .660$, where participants responded faster to old displays than to new, and a main effect of Epoch $F(2, 34) = 8.54$, $p < 0.01$, $\eta_p^2 = .334$, where RTs decreased across time. However, crucially the Conversation x Context interaction was not significant, $F(1, 17) = 0.05$, $p = .824$, $\eta_p^2 = .003$. None of the other interactions were significant, (all Fs < 1).

Comparing the CC Effects over the Test-Phase we see no significant difference in CC between the Conversation and No Conversation condition, $t(17) = 0.23$, $p = 0.82$, with the Bayes Factor analysis providing substantial evidence in favour of the null, $BF_{10} = 0.25$. Given that there were baseline differences in RTs across the New displays in the Conversation and No Conversation conditions we also compared the percentage of facilitation that occurred in each condition (using the average CC Effect and RTs for the New contexts across the Test Phase). The results again showed that there was no effect of conversation on percentage facilitation (17.5% vs 19.5% in the Conversation vs No Conversation condition, respectively), $t(17) = 0.34$, $p = 0.74$, with substantial evidence for the null, $BF_{10} = 0.26$.

Discussion

Participants in Experiment 2 engaged in a conversation whilst completing the Test phase of the experiment. The Test phase was used to determine whether spatial learning in the Training phase was expressed while people were engaged in conversation. The results showed that in both the Conversation and the No Conversation conditions a robust CC Effect occurred. Moreover, there was no statistical difference in the magnitude of the CC Effect across conversation conditions. The findings showed that the spatial learning that had occurred in the Training Phase could be expressed, without detriment, whilst participants were maintaining a conversation. Similar to the results of Experiment 1, this goes against the Cognitive Disruption account which predicts a reduction (at the very least) of the CC Effect in the Conversation condition. Despite this there was a general slowing of overall RTs in the Test phase when participants were conversing. This is consistent with the Cognitive Delay account of dual-task conversation costs.

Experiments 1 and 2 suggested that the dual-task cost of conversation only caused a delay in responding rather than specific interference with the mechanism underlying spatial learning. However, in both these experiments the learning of the context occurred without the presence of interleaving Old displays. Travis et al. (2013) showed that having novel displays present during the learning phase of Contextual Cueing led to an overall increase in task demands. This increase in task difficulty led to observed dual-task costs (using a WM task) and a reduction in the CC Effect which did not occur when spatial learning was generated without the interspersing of novel displays. Therefore in Experiment 3 we increased the difficulty of the Contextual Cueing task by having Old and New spatial contexts appear throughout the experiment. In addition, participants in Experiment 3 were required to converse with the experimenter across the entire experiment. These manipulations created a situation in which it was more likely that insufficient resources would be available to converse and perform the contextual cueing task optimally. Therefore, these changes should maximise the possibility of finding a dual-cost effect of conversation on the mechanisms responsible for learning and expressing spatial contexts.

Experiment 3: Contextual Curing with Conversation in the Training and Test Phase

Method

Participants

Twenty participants took part in return for payment or course credit (Male = 1, Mean Age = 18.9 years). All confirmed that they could easily hear the experimenter in the conversation condition and that they could see the visual display.

Stimuli and Apparatus

The stimuli and apparatus were similar to those in Experiment 1, however, the conversation was held over SKYPE using a hands-free phone[4]. The experimenter received the SKYPE call on a Laptop computer in an adjacent experimental cubicle.

Design and Procedure

The design and procedure were similar to those of Experiment 1 except that participants were required to converse with the experimenter throughout the whole experiment. Furthermore, there was no clearly defined 'Training' and 'Test' phase. Instead, all epochs contained both Old and New displays, following the design of the original experiments by Chun and Jiang (1998). Participants completed eight Epochs in total. Each Epoch contained 16 old trials (each containing four old displays that were repeated four times per epoch) and 16 New trials. Similar to the previous experiments, the number of target locations in the New trials

[4] Although we changed the conversation 'platform' in this experiment to take place o SKYPE, rather than using a phone, the different mode of conversation transmission should have little impact on the results. In both platforms the conversation was clear. The only difference between these two technologies was how the initial connection was made. However, this was completed prior to the visual search task being initiated by the experimenter and so did not affect the results.

matched those of the Old trials allowing us to show that in the Old trials participants were learning the contexts and not just the target locations. This resulted in a total of 256 trials per participant.

Results

The overall error rate was low at 1.6% and errors were not analysed further. All trials in which RTs were less than 200ms were removed (0.02 % of trials). An outlier removal procedure was then conducted to remove any RTs which deviated by more than 3SD from the mean of their respective cell (0.84% of the data). Figure 5 shows the mean correct RTs.

Figure 5 about here

Mean correct RTs were entered into a 2 (Conversation: Conversation vs No Conversation) × 2 (Context: Old vs New) × 8 (Epoch: 1-8) repeated measures ANOVA. There was a significant main effect of Context, $F(1,19) = 41.75$, $p < 0.01$, $\eta_p^2 = .687$, where RTs in Old displays were faster than those in New displays and of Epoch, $F(7, 133) = 12.51$, $p < 0.01$, $\eta_p^2 = .397$, where RTs decreased across epoch. However, there was no main effect of Conversation nor any significant interactions (all *Fs* < 2.59, *ps* > .12).

Given that there was no formal Test phase in this experiment we cannot use this to measure CC. Instead, Chun and Jiang (1998) suggested that the CC effect should be measured as the difference between New and Old configurations across the last three epochs (see also Kunar, Flusberg & Wolfe, 2008, Kunar et al., 2007, Kunar, Watson, Cole & Cox, 2014). This procedure focuses on the asymptotic benefit for having learned a predictive context over a non-predictive one. Following their reasoning, we examined the data across the last three epochs (here Epochs 6 to 8). A 2 (Conversation: Conversation vs No Conversation) × 2 (Context: Old vs New) × 3 (Epoch: 6-8) repeated measures ANOVA showed that there was a main effect of

Context, F (1,19) = 42.57, $p < 0.01$, $\eta_p^2 = .691$, with participants reacting more quickly to Old displays compared with the New. Conversation was found to be marginally significant, F (1,19) = 4.27, $p = 0.053$[5], $\eta_p^2 = .184$, where there was a trend for participants to respond slower in the Conversation condition compared to the No Conversation condition. However, no other main effects or interactions were significant (all $Fs < 1.34$, $ps > 0.27$).

Comparing the CC Effect over the last three epochs the analyses showed there was no significant difference in CC between the Conversation and No Conversation condition, $t(19) = 0.46$, $p = 0.65$, with substantial support for the null, $BF_{10} = 0.26$. Similarly when we converted the CC Effect into the percentage of CC facilitation, the results again showed there was no disruption of CC by conversation (13.4% vs 13.0% facilitation effect in the Conversation vs No Conversation condition, respectively) $t(19) = 0.08$, $p = 0.94$, with substantial support for the null, $BF_{10} = 0.23$.

Discussion

Experiment 3 was used to investigate the effects of conversation when the difficulty of learning the spatial contexts was increased. This was achieved by interspersing novel displays in the learning phase (see Travis et al., 2013). Despite this, there was little difference in Contextual Cueing across the Conversation and No Conversation conditions. The data counter a strong version of the Cognitive Disruption account which predicts a reduction in CC in the conversation condition. In contrast, there was a trend for RTs to be slower in the Conversation condition compared to the No Conversation condition. Taken together with the results of Experiments 1 and 2, the current findings add weight to the Cognitive Delay account of dual-task interference. We consider this finding further in the General Discussion.

[5] Note that since there was a main effect of conversation in the first two experiments, there is some justification for considering that this should be a directional test which would be significant at the $p < .05$ level.

Please note that we did not measure whether the CC Effects obtained in these experiments were due to implicit or explicit learning (see Smyth & Shanks, 2008, for evidence of explicit awareness in CC). It could therefore be possible that participants were aware of the repetitions of the Old displays (although this would be less likely the case in Experiment 3 where Old displays were interspersed with New throughout the experiment). Nevertheless, regardless of the type of learning involved, our data consistently showed there was little interference to CC of having participants converse.

In Experiments 1 to 3 we investigated whether conversing interferes with the mechanisms underlying spatial learning. Experiments 4 to 6 investigate the effect of conversation on the processes underlying another search mechanism: namely, those involved in temporal selection. Recall that in Preview Search participants are presented with an initial search display which contains only distractors. Following a 'preview' period of those items, an additional set of items is presented. The target only ever appears in the new set of items and never in the preview set. Participants have to ignore the old, previewed items in order to attempt to prioritise the newly arriving stimuli (e.g., Watson & Humphreys, 1997). In the remaining experiments we examined the extent to which holding a conversation interferes with participant's abilities to ignore old and prioritise new stimuli.

Experiment 4: The Effect of Conversation on Preview Search

Method

Participants

Twenty-eight participants took part in return for payment or course credit (Male = 7, Mean Age = 20.5 years). All confirmed that they could easily hear the experimenter in the conversation condition and that they could see the visual display.

Stimuli and Apparatus

The apparatus was the same as that used in Experiment 1, however the search stimuli consisted of H and A box-figure-8 letter stimuli[6] (RGB: Blue = 68, 164, 176; Green = 11, 193, 126) which measured 0.96° vertically and 0.88° horizontally. The stimuli were displayed on a black background within a 6 × 6 grid structure with stimulus positions randomly jittered by up to 20 pixels within each cell of the matrix. The target could not fall within the two central columns of the display matrix to ensure that the target was always clearly either to the left or right of the display centre (see von Mühlenen, Watson & Gunnell, 2013, for a similar design). The stimuli were evenly distributed between the left and right side of the display, with an equal number of green and blue items presented on each side. Example displays are shown in Figure 6.

Figure 6 about here

Design and Procedure

The experiment used a within-subjects 2×2×3 factorial design. The independent variables were Conversation (Conversation vs No Conversation), Presentation condition (Full Element Baseline vs Preview) and display size (4, 8 and 16 items). In the preview (PRE) condition a trial started with a fixation dot (1000ms), followed by either 2, 4 or 8 green Hs (the preview display). After 1000ms, 2, 4 or 8 blue letters were added to the display to give total display sizes of 4, 8 or 16 items. The blue letters consisted of one blue H target and the remainder were blue letter A distractors. This display remained visible until participants indicated whether the blue H target was to the left or right of the display centre. If the target

[6] These stimuli were used to match those in the original Visual Marking study by Watson and Humphreys (1997).

was on the left participants pressed the left arrow key and if the target was on the right participants pressed the right arrow key. After a response was made the display turned blank (500ms) after which the next trial began. In the Full Element Baseline (FEB) condition all search elements appeared simultaneously with no preview display.

In the No Conversation condition, participants completed the FEB and PRE conditions without any external distractions. In the Conversation condition, similar to Experiment 1, the participant held a phone conversation with the experimenter whilst completing the task. Each participant completed 8 experimental blocks split into two identical sets of four blocks: FEB-Conversation, PRE-Conversation, FEB-No conversation and PRE-No conversation. The order of the conversation and search condition was counterbalanced and trial order was randomized within each block. Each block contained 66 trials (resulting in a total of 528 trials per participant). Each individual block was made up of 60 target present trials in which the target could appear either on the left or the right side of the screen with equal probability. The remaining 6 trials were catch trials in which the target was not present and the participant was required to indicate this by pressing the space bar. The catch trials were included so that participants could not develop a strategy whereby they searched only half of the display and therefore, by elimination, determined the location of the target (see Watson & Kunar, 2010 for a similar methodology). Display size was distributed evenly throughout each block with 20 trials for each level of display size (2 for each level in the target absent trials). The experiment took approximately 50 minutes to complete and participants received feedback when they made an error. Participants completed a short practice session before the experiment began.

Results

The overall error rate was low at 1.8% and errors were not analysed further. All trials in which RTs were less than 200ms were removed (0.08 % of trials). An outlier removal procedure was then performed to remove any RTs which deviated by more than 3SD from the mean of their respective cell (1.4% of the data). Figure 7 shows the mean correct RTs and search slopes for all experiments can be found in Table 1.

Figure 7 and Table 1 about here

A 2 (Conversation: No conversation vs Conversation) × 2 (Presentation Condition: FEB vs PRE) × 3 (Display Size: 4, 8, 16) repeated-measures ANOVA on mean correct RTs revealed significant main effects of presentation condition, $F(1,27) = 116.8$, $p < 0.01$, $\eta_p^2 = .812$, conversation, $F(1,27) = 44.9$, $p < 0.01$, $\eta_p^2 =.625$, and display size, $F(2, 54) = 501.7$, $p < 0.01$, $\eta_p^2 = .949$. Overall, RTs were longer in the FEB condition than in the PRE condition, were longer in the Conversation than in the No Conversation condition and increased with display size. These main effects were qualified by significant Presentation Condition × Display Size, $F\ (2, 54) = 59.7$, $p < 0.01$, $\eta_p^2 =.689$, Conversation × Presentation Condition, $F(1, 27) = 5.10$, $p = 0.032$, $\eta_p^2 =.159$, and Conversation × Display size interactions, $F(2,54) = 8.80$, $p < 0.01$, $\eta_p^2 =.246$. The increase in overall RTs as a result of holding a conversation was larger in the FEB condition (M = 153.1ms SD =123.2) than in the PRE condition (M = 104.9ms, SD =110.9), $t(27)=2.21$, $p=.036$, $d=.417$, and RTs increased more with display size in the Conversation condition than in the No Conversation condition. However, the 3-way interaction was non-significant, $F(2,54)= 1.52$, $p = 0.23$, $\eta_p^2=.053$, indicating that the preview benefit was not reliably affected by the naturalistic conversation. This was confirmed by comparing the slope values (Table 1) using a 2 (Conversation: No conversation vs Conversation) × 2 (Presentation Condition: FEB vs PRE) repeated-measures ANOVA. This revealed a main effect of Condition, $F(1, 27) = 78.35$, $p < 0.01$, $\eta_p^2 = 0.744$, in which slopes were shallower in the Preview condition compared to the FEB condition and a main effect of Conversation, $F(1, 27) = 14.93$, $p < 0.01$, $\eta_p^2 = 0.356$, in which slopes were shallower in the No Conversation compared to Conversation condition. However, the Condition × Conversation interaction was not significant, $F < 1$.

In order to determine the strength of the preview benefit across conversation conditions we took the ratio of the Preview to FEB search slopes (Table 2). If a perfect preview benefit occurred we would expect the search slope in the Preview condition to be half that of the FEB baseline because participants would only be searching the newly presented search set and not the old preview items (Watson & Humphreys, 1997). Thus a Preview Effect–Ratio (PE-Ratio) of 0.5 (or below) would indicate a full preview benefit and a ratio of 1 (or above) would indicate no preview benefit. If conversation interfered with preview search we would expect the PE-Ratio in the conversation condition to be larger than in the No Conversation condition. However, this was not the case. Instead there was no significant difference between PE-Ratios, $t(27) = 0.092$, $p = 0.927$, with substantial support for the null $BF_{10} = 0.201$.

Discussion

Experiment 4 examined the influence of naturalistic conversation on Preview Search. The results showed that a robust Preview Effect occurred in both the No Conversation and in the Conversation conditions. Furthermore, there was no reliable difference in the size of the Preview Benefit across conditions. Similar to the results of Experiments 1 to 3, which examined spatial learning, the results of Experiment 4 do not follow the predictions from the Cognitive Disruption account of dual-task interference. However, they do concur with the Cognitive Delay account of conversation interference, that is, RTs were slower overall when participants were conversing.

Please note that although conversation did not appear to affect the preview benefit there was nonetheless, an effect of search efficiency overall. Specifically, holding a conversation led to search becoming less efficient for both the preview and FEB display conditions. These findings contrast with those of Kunar et al. (2018) who found that dual-task costs of verbal tasks and conversations did not interfere with the search rate but did cause an upward shift in RTs (see also Shinohara, et al., 2010, who also found no effect of verbal or listening tasks on search slopes). We consider this difference further in the General Discussion.

The findings from Experiment 4 showed that holding a conversation had little, if any, impact on participants' ability to exclude old, irrelevant distractors and prioritize the selection of new stimuli. This is in contrast to other work showing that auditory tasks interfere with the preview benefit if they co-occur with the onset of preview display (Humphreys, Watson & Jolicoeur, 2002). What should be noted, however, is that our experiment differed in at least one key dimension to the experiments presented by Humphreys, Watson and Jolicoeur (2002). As mentioned before, research has shown that the preview benefit consists of two dissociable phases: a set-up and a maintenance stage (Watson, Humphreys & Olivers, 2003, Kunar et al., 2006). Importantly, Humphreys et al., (2002) used dual tasks that specifically targeted each particular stage. When participants were given the auditory dual-task in the set-up stage no preview benefit occurred. Although in Experiment 4 participants were asked to converse with the experimenter throughout the experiment, having a preview period of 1000ms might have allowed participants to shift attention back to the search task in order for the preview effect to be set up before returning to the conversation (given that the preview effect only needs 400 ms to be established, Watson & Humphreys, 1997, Kunar et al., 2003). To investigate this, in Experiment 5, we reduced the preview period to examine the effect of holding a conversation on time-based selection in conditions in which there was less time available for task switching.

Experiment 5: The Effect of Conversation on Preview Search with Preview Durations of 750, 500 and 250 ms

Method

Participants

Twenty participants took part in return for payment or course credit (Male = 10, Mean Age = 23.2 years). All confirmed that they could easily hear the experimenter in the conversation condition and that they could see the visual display.

Stimuli and Apparatus

The stimuli and apparatus were similar to those of Experiment 4, except the conversation was held over SKYPE (similar to Experiment 3).

Design and Procedure

As in Experiment 1, participants completed FEB and PRE search tasks under both Conversation and No Conversation conditions. However, within the preview block, the preview duration could be 250, 500 or 750 ms (PRE_{250}, PRE_{500}, PRE_{750} respectively). All preview durations were mixed within one block. In order to keep the total number of trials comparable to Experiment 1, two display sizes were used (4 and 12) rather than three. This resulted in a 2 (Conversation vs No Conversation) × 4 (Presentation Condition: FEB, PRE_{250}, PRE_{500}, PRE_{750}) × 2 (Display size: 4 or 12 items) within-subjects design.

Each participant completed three blocks of trials in an ABA design (a block of 36 FEB trials followed by a block of 108 PRE trials followed by a block of 36 FEB trials) for the no-conversation and again for the conversation condition (six blocks in total: $ABA_{conversation}$ and $ABA_{no\text{-}conversation}$). The Conversation/No Conversation condition order was counterbalanced. In the PRE block participants were prompted to take a break after every 36 trials. Eleven percent of trials were ‘no-target’ catch trials in which participants were required to press the space bar to continue.

Results

The overall error rate was low (1.3%) and errors were not analysed further. All trials in which RTs were less than 200ms were removed (0.1 % of trials). An outlier removal procedure was then conducted to remove any RT which deviated by more than 3SD from the mean of their respective cell (1.7% of the data). Figure 8 shows the mean correct RTs.

Figure 8 about here

Mean correct RTs were analysed using a 2 (Conversation: Conversation vs No Conversation) × 4 (Presentation Condition: FEB, PRE_{250}, PRE_{500}, PRE_{750}) × 2 (Display Size: 4, 12) repeated measures ANOVA. This revealed significant main effects of Conversation, $F(1,19)= 22.3$, $p < 0.01$, $\eta_p^2=.540$, Presentation Condition, $F(3, 57)= 28.8$, $p < 0.01$ $\eta_p^2=.603$, and Display Size, $F(1,19) = 210.4$, $p < 0.01$, $\eta_p^2 =.917$. Overall, RTs were longer in the FEB condition than in the PRE conditions, were longer in the Conversation than in the No Conversation condition and increased with display size. There was also a significant Presentation Condition × Display Size interaction, $F(3, 57)= 26.1$, $p < 0.01$, $\eta_p^2 =.578$. No other significant interactions were found (Fs < 1.52, ps > 0.22). As shown in Table 1, search slopes decreased as the preview duration increased and search was least efficient in the FEB condition.

Based on the hypothesis generated from Experiment 4, the effect of preview duration was examined using planned comparisons. A 2 (Conversation: Conversation vs No Conversation) × 3 (Preview Duration: 250, 500, 750) × 2 (Display Size: 4, 12) repeated measures ANOVA revealed that all three main effects were significant, Conversation, $F(1,19)=23.5$, $p < 0.01$, $\eta_p^2 = .553$, Preview Duration, $F(2, 38) = 16.9$, $p < 0.01$, $\eta_p^2 = .470$, and Display Size, $F(1,19) = 130.5$, $p < 0.01$, $\eta_p^2 = .873$. There was also a significant Preview Duration × Display Size interaction, $F(2, 38) = 3.82$, $p = 0.031$, $\eta_p^2 = .167$, indicating that the size of the preview benefit decreased as the preview duration decreased with resulting preview slopes. No other interactions were significant (all Fs < 1).

When compared individually with the FEB, there remained a reliable preview benefit (Presentation Condition × Display Size interaction) at all three preview condition durations (FEB vs PRE_{250}, $F(1,19)= 25.9$, $p < 0.01$, $\eta_p^2 = 0.577$, FEB vs PRE_{500}, $F(1,19)= 50.6$, $p < 0.01$, $n^2= 0.727$, FEB vs PRE_{750}, $F(1,19)=31.81$, $p < 0.01$, $\eta_p^2 =.626$).

To measure differences in the Preview Effect for both the Conversation and No Conversation conditions we calculated the PE-Ratio for each Preview Duration using their respective FEB (Table 2). Similar to Experiment 4, there was no significant difference in the magnitude of the Preview Effect between the Conversation and No Conversation conditions and substantial support for the null (for 750 ms: $t(19) = 0.54$, $p = 0.60$, $BF_{10} = 0.265$; for 500 ms: $t(19) = 0.86$, $p = 0.40$, $BF_{10} = 0.322$; for 250ms: $t(19) = 0.36$, $p = 0.73$, $BF_{10} = 0.246$).

Discussion

Three main findings emerged from Experiment 5. First, as might be expected from prior research, shortening the preview duration resulted in a reduced preview benefit. In earlier work, Watson and Humphreys (1997) showed that for an optimal preview benefit, a preview duration of approximately 400ms was required. Consistent with this, here we found a gradual decrease in preview search efficiency as the preview duration decreased from 750ms to 250ms, although perhaps surprisingly, a robust preview benefit was still present even at the 250ms preview duration. Second, as in Experiment 4, conversation produced an overall increase in RTs, consistent with a Cognitive Delay account. Finally, in contrast to the Cognitive Disruption account, even with reduced preview durations there was no evidence that conversation had a disruptive effect on the mechanism underlying time-based selection.

Given that there was still a robust preview benefit even at the shortest preview duration, it is possible, although unlikely, that there was enough time in the preview period to task switch between the setting up of the preview effect and the conversation. To investigate this possibility, in Experiment 6 we further tested the boundary limits of conversation on time-based selection by reducing the preview duration to periods which would make it even more difficult to re-allocate attentional resources. Thus, in Experiment 6 the preview period was reduced to minimal durations of 75, 150 and 250ms.

Experiment 6: The Effect of Conversation on Preview Search with Preview Durations of 250, 150 and 75 ms

Method

Participants

Twenty participants took part in return for payment or course credit (Male = 12, Mean Age = 22.7 years). All confirmed that they could easily hear the experimenter in the conversation condition and that they could see the visual display.

Stimuli and Apparatus

Stimuli and apparatus were the same as those used in Experiment 5.

Design and Procedure

The design and procedure were identical to those of Experiment 5, except that the preview durations were shorter at 75ms, 150ms and 250ms.

Results

The mean RT of one participant was 3.7 SDs away from the overall mean of all the participants, therefore the data from this participant were removed. The overall error rate was low (1.7%) and errors were not analysed further. All trials on which RTs were less than 200ms were removed (0.04% of trials). An outlier removal procedure was then performed to remove any RT which deviated by more than 3SDs from the mean of their respective cell (1.3% of the data). Figure 9 shows mean correct RTs.

Figure 9 about here

Mean correct RTs were analysed using a 2 (Conversation: Conversation vs No Conversation) × 4 (Presentation Condition: FEB, PRE_{75}, PRE_{150}, PRE_{250}) × 2 (Display Size: 4, 12) repeated-measures ANOVA. This revealed significant main effects of conversation, $F(1,18)=17.41, p < 0.01, \eta_p^2=.492$, and display size, $F(1,18)=154.8$, $p < 0.01, \eta_p^2=.896$. There was also a significant Conversation × Display Size interaction, $F(1,18)= 5.89$, $p = 0.026$, $\eta_p^2=.247$. As shown in Figure 9, RTs were longer in the conversation condition than in the No Conversation condition, increased with display size and the increase with display size was greater when a conversation was being held (i.e. search was less efficient) than when not. Neither the main effect of Presentation Condition, $F(3, 54) = 1.69$, $p = .181$, $\eta_p^2=.086$, the Presentation Condition × Display Size interaction, $F(3, 54)= 1.48$, $p = 0.231$, $\eta_p^2 =.076$, nor the Presentation Condition × Conversation interaction, $F(3, 54)= 0.37$, $p = 0.772$, $\eta_p^{22} = .02$, were significant. The three way interaction was also non-significant, $F(3, 54)= 1.34$, $p = 0.271$, $\eta_p^2 = .069$.

The effect of preview duration was examined using planned comparisons. A 2 (Conversation: Conversation vs No Conversation) × 3 (Preview Duration: 75, 150, 250) × 2 (Display Size: 4, 12) repeated measures ANOVA revealed significant main effects of conversation $F(1,18)= 18.25$, $p < 0.01$, $\eta_p^2 = .503$, and display size $F(1,18) = 157.80$, $p < 0.01$, $\eta_p^2 = .898$. However, there was no significant main effect of Preview Duration, $F(2,36) = 0.56, p = 0.577, \eta_p^2 = .030$. The Conversation × Display Size interaction was significant, $F(1,$

18)= 5.03, $p < 0.038$, $\eta_p^2 = .218$. However, no other interactions were significant (all *F*s < 1.7, ps > 0.2).

Planned comparisons also showed that when compared individually with the FEB, there was not a reliable preview benefit (Presentation Condition × Display Size interaction) at any of the preview durations (FEB vs PRE_{75}, $F(1,18) = 1.63$, $p = 0.218$, $\eta_p^2 = .083$, FEB vs PRE_{150}, $F(1,18)=3.32$, $p = 0.09$, $\eta_p^2 = .156$, FEB vs PRE_{250}, $F(1,18) = 0.89$, $p = 0.358$, $\eta_p^2 = .047$), neither were there any significant three-way Conversation × Presentation Condition × Display Size interactions (FEB vs PRE_{75}, $F(1,18)= 0.73$, $p = .403$, $\eta_p^2 = .039$, FEB vs PRE_{150}, $F(1,18)=1.65$, $p = .22$, $\eta_p^2 = .084$, FEB vs PRE_{250}, $F(1,18)= 0.20$, $p = 0.657$, $\eta_p^2 = .011$).

To measure differences in the Preview Effect across conversation conditions we calculated the PE-Ratio for each Preview Duration using their respective FEB (Table 2). The results again showed that there was no significant difference in the magnitude of the Preview Effect between for the Conversation and No Conversation condition (for 250 ms: $t(18) = 0.01$, $p = 0.99$, for 150 ms: $t(18) = 0.53$, $p = 0.60$, for 75ms: $t(18) = 1.57$, $p = 0.14$). A Bayes analysis provided substantial evidence in favour of the null for preview durations of 250ms ($BF_{10} = 0.24$) and 150ms ($BF_{10} = 0.27$) and anecdotal evidence for the null at a duration of 75ms ($BF_{10} = 0.67$).

Discussion

The main aim of Experiment 6 was to assess the effect of conversation on time-based visual selection when the opportunity to establish a preview effect was severely reduced. There was no evidence that a preview benefit occurred at any of the preview durations; search slopes in the preview conditions were statistically equivalent to those in the FEB. In Experiment 5, we observed a reliable preview benefit with a preview duration of 250ms. However, in Experiment 6, a 250ms preview did not produce a reliable benefit; indeed, a preview benefit was not found in any of the preview conditions. Hence, there was no time-based selection for the conversation to disrupt. Nonetheless, a robust effect of conversation on overall RTs and on the slope of the search functions was observed. Holding a conversation both increased overall RTs and reduced the rate of search through the display.

General Discussion

In six experiments we examined the dual-task cost of conversation on two important search mechanisms: Contextual Cueing and Preview Search. The results showed that in all experiments there was a general slowing of response times while the participants were engaged in conversation. However, there was little impairment of the mechanisms underlying the effects: when the CC or Preview Effect was present in the No Conversation condition it was also firmly established in the Conversation condition.

The findings provide insight into how conversation affects our ability to complete other tasks. Specifically, the data do not support a Cognitive Disruption account of conversational costs. Experiments 1 to 3 showed robust CC Effects in all conditions, while Experiments 4 to 6 showed that in conditions where a Preview Effect was likely to occur (Experiments 4 and 5) it was also observed while participants were engaged in conversation. Instead the results point to a generalised slowing of responses consistent with the Cognitive Delay account. Given that conversation relies on auditory and motor processes (in terms of speech production) and search tasks rely on visual processes, this slowing is likely to be due to a competition for processing at the level of a central amodal bottleneck (where processing from different modalities compete for the same central resource, e.g. Wickens et al., 1984, 2002, Kunar et al., 2008). That is, while conversation is being processed, there is a fixed and additive delay in the ability to either initiate the search of a display or generate a response. Initiating search or responding to the target can only then occur when participants have either finished conversing, or more likely, in our experiments (given that the conversation was ongoing) found time to task-switch and re-allocate attention between the conversation and search process. It is well known that task switching leads to a delay in response and that this switch cost also leads to long-term, as well as transient deficits (e.g. see Monsell, 2003, for a review). One reason for this task switching cost is due to 'Task Set Reconfiguration' (TSR), where attention is shifted between two goal-states (Rogers & Monsell, 1995) – in the case of our experiments, switching between holding a conversation and completing a competing visual search task. TSR is needed to suppress responses to the 'switched-from' task and activate responses to the 'switched-to' task. In our experiments, as the two tasks are continuous, requiring constant switching between conversation and visual search, TSR would occur multiple times, leading to the observed delay in response times.

One could argue that the locus of the delay in conversation might have occurred at a pre-attentive stage, perhaps at the initial perceptual processing stage. Unfortunately, from our data we are unable to identify whether the interference occurred at the perceptual or response stage. However, in contrast to previous work (Kunar et al., 2018, Shinohara, et al., 2010) our data also point to a possible effect of conversation on participants' search efficiency. This is true for Experiments 4 and 6 here (although not Experiment 5) in which search was less efficient when participants were conversing compared to the No Conversation conditions. It could be that conversation leads to a small, fixed cost in the time it takes to select each search item (or a group of items) for further processing in the visual search task (without interfering with spatial learning or inhibition). If this were the case then the locus of interference is likely to have occurred after the initial perceptual stage (involving processes such as grouping and segmentation). Given the difference in the results across these (and previous) experiments, in order to determine whether there was an overall effect of holding a conversation on search efficiency, we calculated and combined the search rates from the FEB conditions of all three preview experiments (Experiments 4 to 6)[7]. The data deviated significantly from normality and so the Wilcoxon Signed Rank test was performed. This revealed that search was less efficient when a conversation was being held (43.9 ms/item) than when no conversation was held (36.0 ms/item), $Z = 3.39$, $p < 0.01$, with strong evidence for the alternative hypothesis (BF_{10}=10.38). This contrasts with work by Kunar et al. (2018) and Shinohara, et al., (2010) who demonstrated that verbal tasks, performed concurrently to a visual search task, did not influence the rate at which participants searched through a display.

It is not clear why these differences occurred. However, one explanation may be that search was less efficient overall in the work by Kunar et al. (2018) and Shinohara, et al., (2010) as indicated by the steep search slopes (around 80 ms/item). If search was already particularly effortful, then any slowing of search efficiency bought about by conversation may not have been so easy to detect (see for example Watson, Maylor & Bruce, 2005, for a similar argument related to the effect of old age on enumeration efficiency). Future research is needed to establish the exact conditions whereby conversation leads to a change in search efficiency. However, for now it is important to note, that under certain conditions, the rate that we search through the world can also be impaired whilst talking on the phone, however, the use of spatial context and the ability to select new information appears to be relatively spared.

The way in which we process and attend our visual environment is not only theoretically important but has implications for how we navigate through the world. Although, our results suggest that cognitive mechanisms might be retained while talking on a mobile phone, conversation led to a significant delay in responding. This is important for tasks where it is crucial to respond in an immediate and timely manner. Take the example of driving.

[7] We calculated and analysed at the level of search rates because of the differing display sizes across the three experiments. Data were cleaned prior to analysis, outliers, trials resulting in an error and target absent trials were removed as in the main analysis sections from Experiments 4-6.

Experiment 1 showed that conversation in the Training phase slowed responses by 283 ms, on average. If a person were driving at a speed of 60 miles/hour a delay of 283 ms would lead to an additional 25 feet being travelled before response (e.g. pressing the brake pedal). However, for some participants the delay could be even longer. For example, one participant showed a conversational delay of 712 ms, which would lead to an additional 63 feet being travelled before response. While in some circumstances this delay may have no consequences, in other situations (e.g. if a child unexpectedly ran into the road) the repercussions could be very serious. Please also note that in all our experiments, the conversations took place on *a hands-free* device. This also has relevance for legislation concerning the use of mobile phones while driving. Currently talking on a mobile phone while driving is only illegal if a hands-held device is used. However, given these results, legislative committees should also take measures to readdress policies involving conversation on a hands-free device while driving.

In our experiments the conversations were set up to be as naturalistic as possible and to largely cover non-emotive topics. However, it could be argued that more complex and emotional conversations would lead to different results. Although Kunar et al. (2018) found little effect of conversation difficulty on visual attention, Briggs, Hole and Land (2011) showed that more emotional conversations (e.g., about a participant's phobia) caused greater detriments to driving performance and induced visual tunnelling. Dula, Martin, Fox, and Leonard (2011) also demonstrated that when participants were engaged in an emotional conversation about a deeply held belief, they were more likely to participate in dangerous driving behaviours (such as speeding, crossing the centre line and experiencing collisions). Future research is needed to investigate whether more emotional conversations would lead to cognitive disruption. Nevertheless, our research demonstrates that even an everyday, potentially 'mundane' conversation leads to consistent delays in response.

It could also be the case that there was a dual-task detriment to conversation, when participants were engaged in visual search. Although we did not measure this directly there is some evidence to suggest that the dual-task cost involving conversation is bi-directional. For example, Becic et al. (2010) found that speech production, language comprehension and the encoding of stories into memory was impaired when participants were concurrently driving in a driving simulator. Furthermore, Drews et al. (2008) found that conversation complexity decreased as driving-simulator demands increased. We predict a similar cost to conversation would be found when people were concurrently performing a visual attention task, although further research would be needed to confirm this. Nevertheless, the studies by Becic et al. (2010) and Drews et al. (2008) highlight the practical considerations that people should make when planning important phone-calls, namely that they will be impaired when performing a secondary dual-task.

The results of the Preview experiments reported above indicate, perhaps surprisingly, that conversation does not interfere with the mechanism behind Preview Search. Given that the preview benefit is hypothesised to be driven by a top-down resource (Watson & Humphreys, 1997) and that it has been demonstrated that additional load tasks can attenuate and abolish the preview benefit (Humphreys, Watson & Jolicoeur, 2002), one might have predicted that conversation would cause an attenuation of the preview benefit. At first glance it may appear that the results are consistent with the bottom up explanation of preview search (Donk & Theeuwes, 2001, 2003). If an abrupt onset is all that is required to automatically guide attention to new items and induce the preview benefit then an additional load task would not be expected to interfere with the benefit. However, a wealth of literature points to, at least in part, a visual marking account of preview search involving top-down inhibitory control (e.g., Emrich, Ruppel, Al-Aidroos, Pratt & Ferber, 2008; Kunar, Shapiro & Humphreys, 2006; Kunar, Thomas & Watson, 2017; Olivers & Humphreys, 2002; Braithwaite et al., 2005; Kunar, Humphreys & Smith, 2003a, 2003b,; Watson & Humphreys, 1997; Watson, Humphreys & Olivers, 2003; Watson & Kunar, 2010; Watson & Kunar, 2012; von Mühlenen, Watson, & Gunnell, 2013; Kunar, Humphreys, Smith & Hulleman, 2003, Kunar, Humphreys, Smith & Watson, 2003, Zupan, Watson, Blagrove, 2015).

Furthermore, the bottom up attentional capture explanation of the preview benefit cannot adequately explain all of the data from Experiments 4 - 6. Of key importance, a preview benefit was found in Experiment 5 when the preview duration was set as low as 250ms, however, in Experiment 6 when the preview duration was also set to 250ms, a preview benefit was not found. A bottom up, onset capture account of the preview benefit would predict that given an adequate time interval for the benefit to occur, it should occur automatically. As such, if abrupt onsets alone are sufficient to induce a benefit then one should have been found in both Experiment 5 and 6 when the preview duration was set at 250ms. Instead, we interpret this as further evidence for the strategic deployment of visual marking depending on task demands and the context in which the displays are presented.

The question as to whether attentional mechanisms, such as visual marking, can be applied strategically or are always applied by default has been previously examined (e.g. Watson & Humphreys, 2000; Zupan, Watson & Blagrove, 2015, Kunar, Thomas & Watson, 2017). For example, Watson and Humphreys (2000) argued that visual marking can be flexibly applied depending on the particular goals of the search task at hand. When participants were asked to find a probe dot in a classic preview search display, they were impaired in doing so when the probe appeared at the location of an old, previewed stimulus. However, this was only the case when on the majority of trials, they were asked to search for a target in a preview search task and only in a minority of cases were they asked to detect a probe. However, when all trials were probe detection trials, participants did not show an impairment in detecting probes when they appeared at the location of old items. This was taken as evidence that visual marking can be applied flexibly, only when the task conditions make it a viable strategy to adopt. Similarly, Zupan, Watson and Blagrove (2015) reported that that the strategic use of visual marking may depend on several factors such as the type of stimulus and the complexity and composition of the task.

As noted previously, the data from Experiments 5 and 6 showed conflicting results when the preview duration was set at 250ms. One possible explanation could be that visual marking was strategically applied in Experiment 5, but not in Experiment 6. The preview durations in Experiment 6 was set so low that participants did not have adequate time to visually mark in 66% of preview trials within a block (i.e. when the preview duration was set at 75 or 150ms). Therefore, because visual marking was little to no use on the majority of trials, it is possible that participants chose not to apply inhibitory visual marking. As such, even when visual marking would have been a valid strategy on 250ms trials, it was not utilized. However, in Experiment 5 visual marking was a valid strategy to adopt on all trials especially when preview durations were longer, e.g. 500, 750ms. Therefore, in Experiment 5, visual marking may have been strategically applied across all trials including those with a preview duration of 250ms[8]. Further research would be required to validate this interpretation, however this result is consistent with Watson and Humphreys' (2000) finding that visual marking is able to be strategically applied depending on the context of the visual attention task.

[8] Note that at the onset of a preview display participants would not know whether the duration of the preview display would be 250, 500 or 750ms and hence it would be advantageous to apply inhibition at the onset of all preview displays.

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Table 1: Search Slopes (ms/item) for conditions in Experiments 4 -6. Standard Errors are reported in the parentheses.

Condition	Conversation	No Conversation
<u>Experiment 4</u>		
Full Element Baseline	36.16 (1.93)	32.31 (1.54)
Preview	22.59 (1.45)	20.09 (1.05)
<u>Experiment 5</u>		
Full Element Baseline	49.84 (5.96)	39.69 (2.12)
Preview – 250 ms	28.62 (4.18)	27.78 (3.10)
Preview – 500 ms	25.26 (2.81)	25.73 (2.44)
Preview – 750 ms	21.85 (4.09)	23.51 (2.49)
<u>Experiment 6</u>		
Full Element Baseline	49.16 (6.43)	37.66 (3.24)
Preview – 75 ms	47.23 (6.92)	30.50 (2.39)
Preview – 150 ms	36.06 (3.90)	33.78 (2.32)
Preview – 250 ms	45.08 (8.36)	36.67 (3.16)

Table 2: Preview Effect – Ratios (PE-Ratio) for Experiments 4 – 6. A PE-Ratio of 0.5 indicates a full preview benefit, whereas a ratio of 1 or above indicates no preview benefit. Standard Errors are reported in the parentheses.

Condition	Conversation	No Conversation
Experiment 4		
Preview	0.65 (0.04)	0.64 (0.04)
Experiment 5		
Preview – 250 ms	0.64 (0.10)	0.69 (0.07)
Preview – 500 ms	0.58 (0.07)	0.65 (0.05)
Preview – 750 ms	0.53 (0.09)	0.58 (0.04)
Experiment 6		
Preview – 75 ms	1.05 (0.11)	0.86 (0.07)
Preview – 150 ms	0.92 (0.12)	1.02 (0.13)
Preview – 250 ms	1.03 (0.13)	1.03 (0.10)

Figures

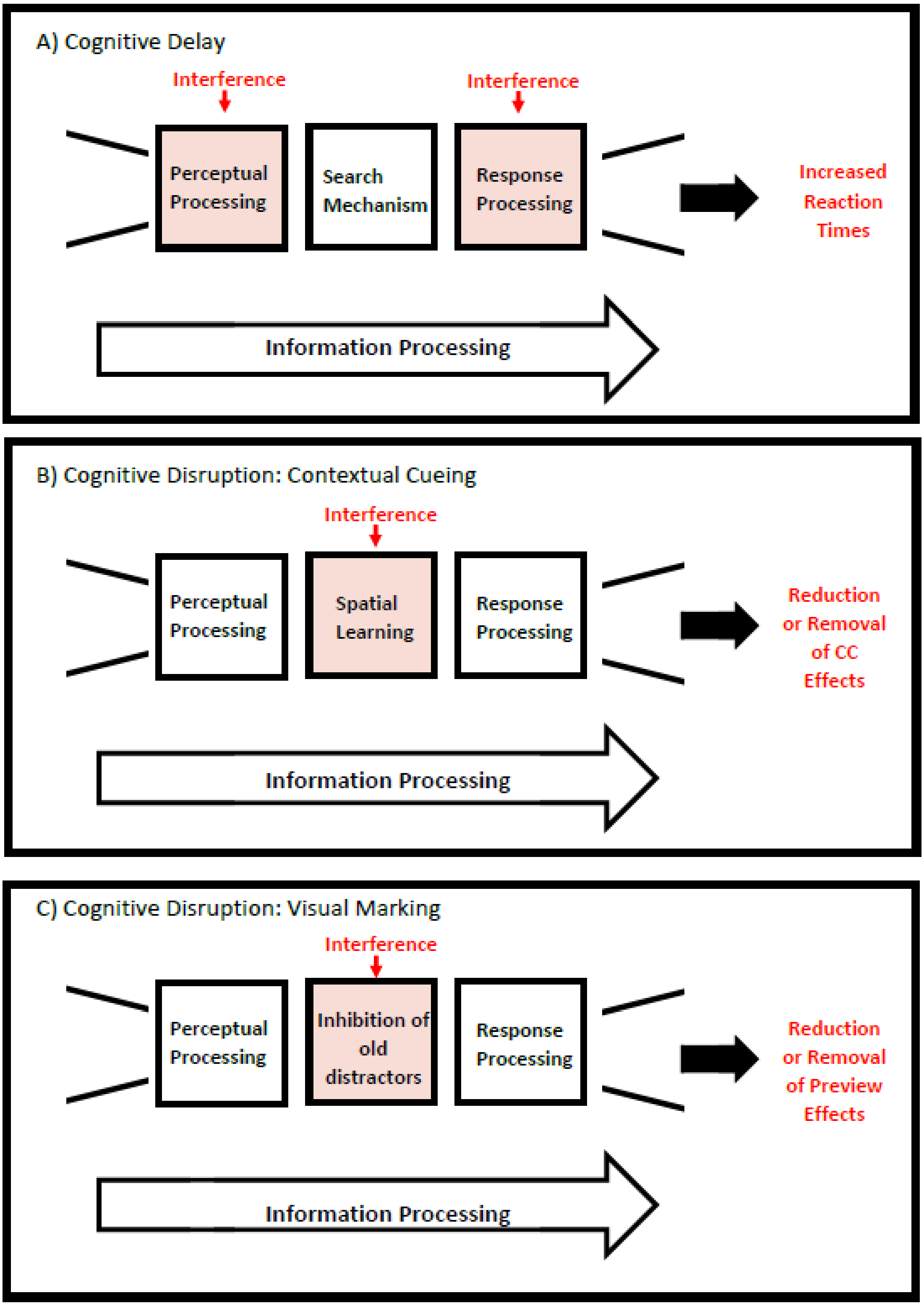

A) Cognitive Delay
Interference
Interference
Perceptual Processing
Search Mechanism
Response Processing
Increased Reaction Times
Information Processing
B) Cognitive Disruption: Contextual Cueing
Interference
Perceptual Processing
Spatial Learning
Response Processing
Reduction or Removal of CC Effects
Information Processing
C) Cognitive Disruption: Visual Marking
Interference
Perceptual Processing
Inhibition of old distractors
Response Processing
Reduction or Removal of Preview Effects
Information Processing

Figure 1. A schematic representation of the ways in which conversation could affect search. Inputs here refer to both the phone conversation and the visual search display. The Cognitive Delay hypothesis states that dual-task interference would occur at either the perceptual or response processing stage, where there will be a bottleneck in processing time leading to an overall delay in response. The Cognitive Disruption hypothesis suggests that the dual-task interference would occur either at the spatial associative learning stage for Contextual Cueing or during the inhibitory stages in Visual Marking. This disruption would lead to a reduction or complete removal of the Contextual Cueing and Preview Effects.

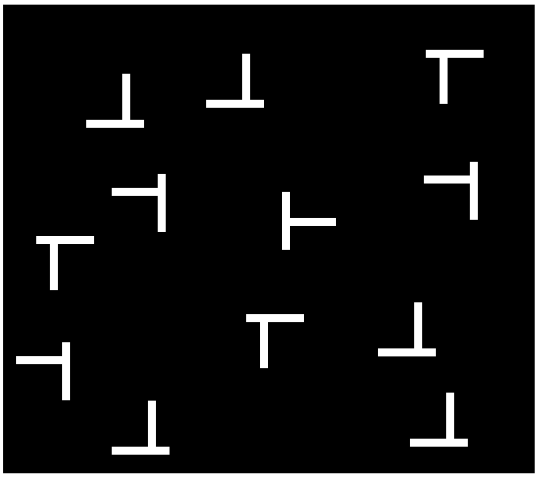


Figure 2. Example displays used in Experiments 1 to 3.

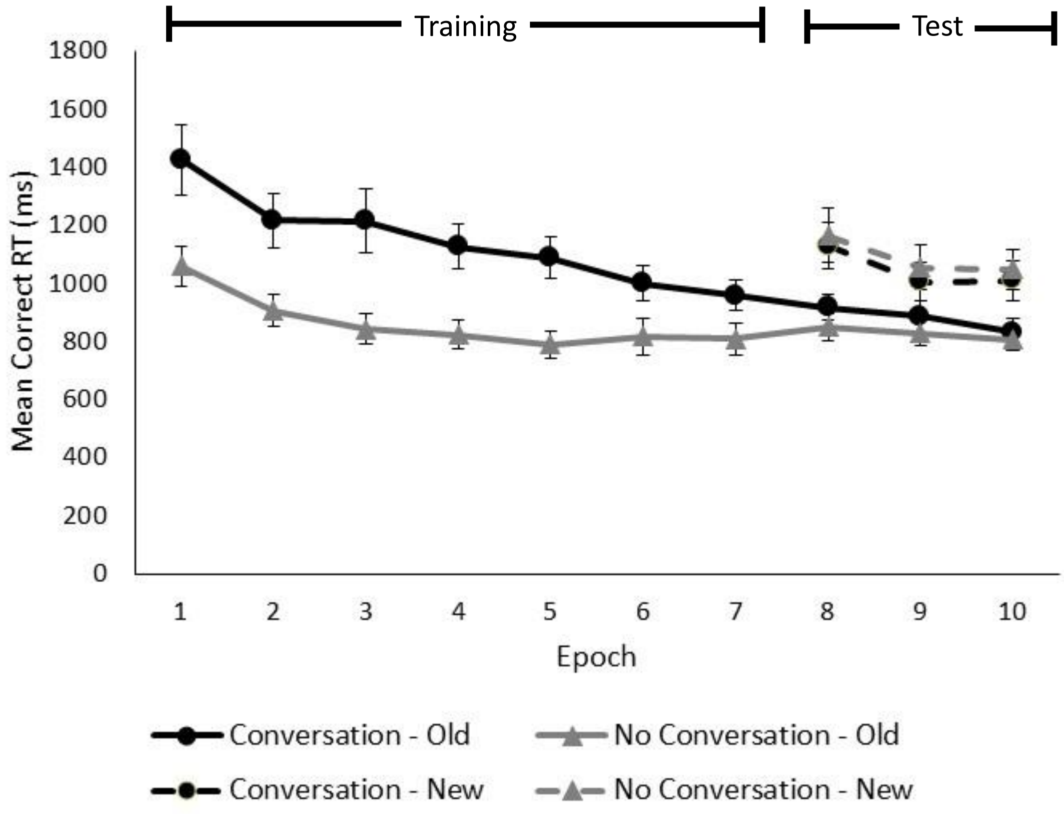


Figure 3. Mean correct RTs as a function of the conversation conditions and Epochs (1-10) in Experiment 1. Error bars show ±1SE. change figure legend to reflect new graph

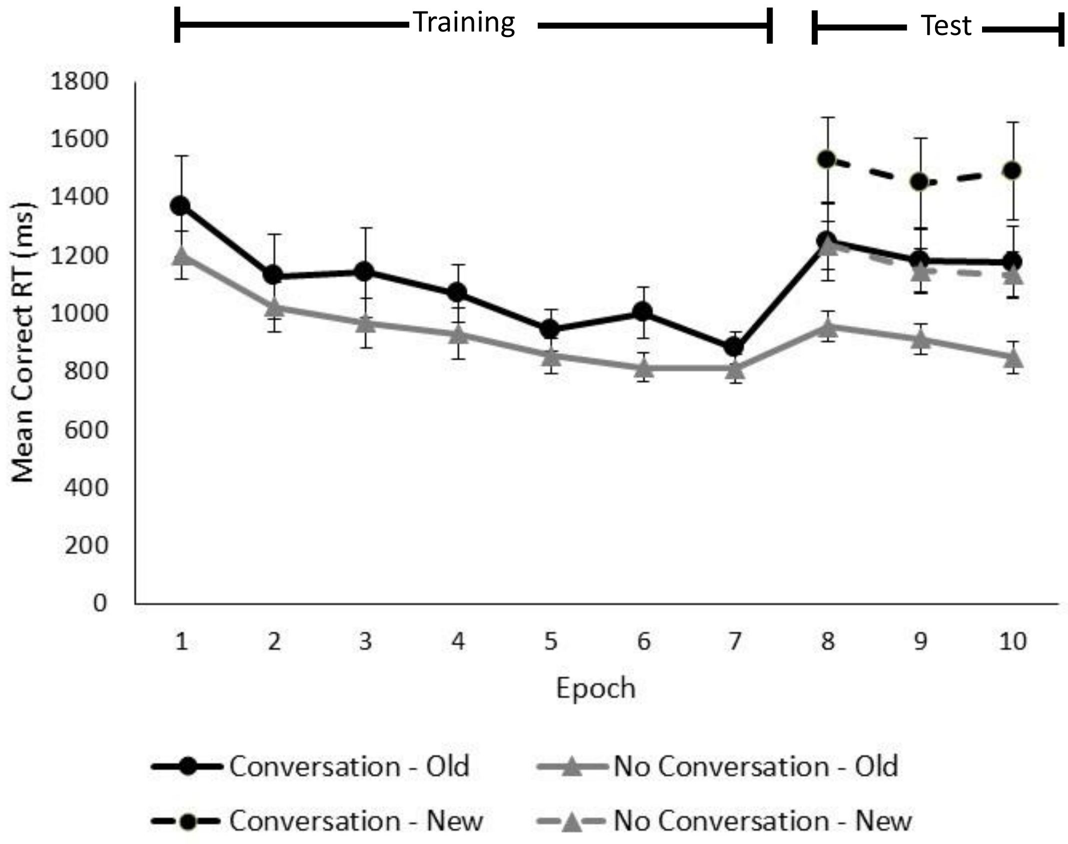


Figure 4. Mean correct RTs as a function of the conversation conditions and Epochs (1-10) in Experiment 2. Error bars show ±1SE.

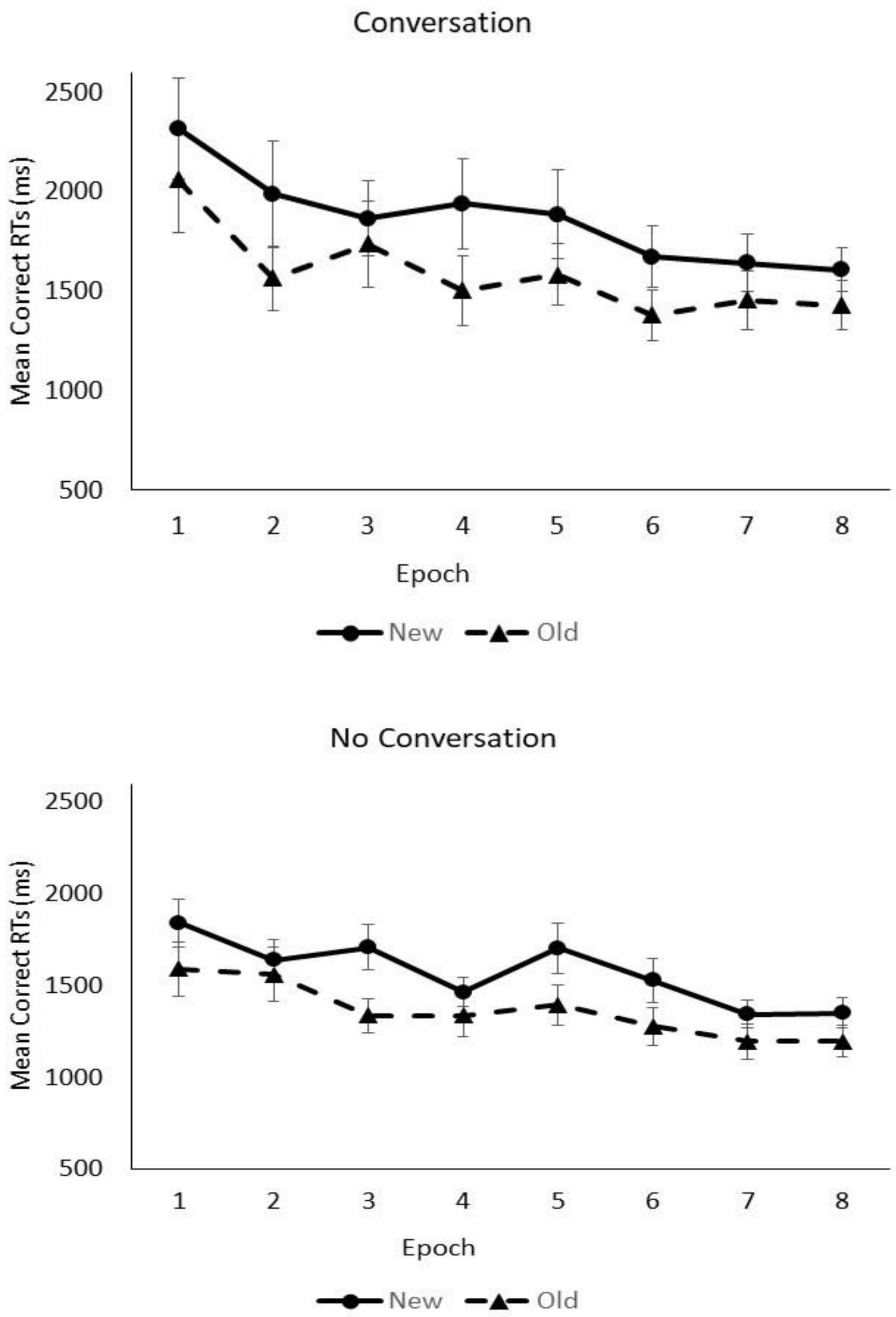
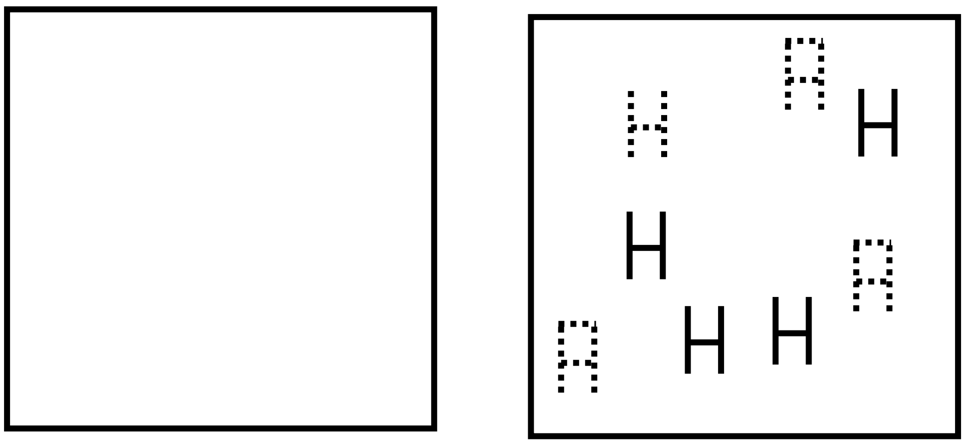


Figure 5. Mean RTs as a function of the conversation and spatial context conditions and Epoch (1-8) in Experiment 3. Error bars show ±1SE.

Full Element Baseline



Preview Search

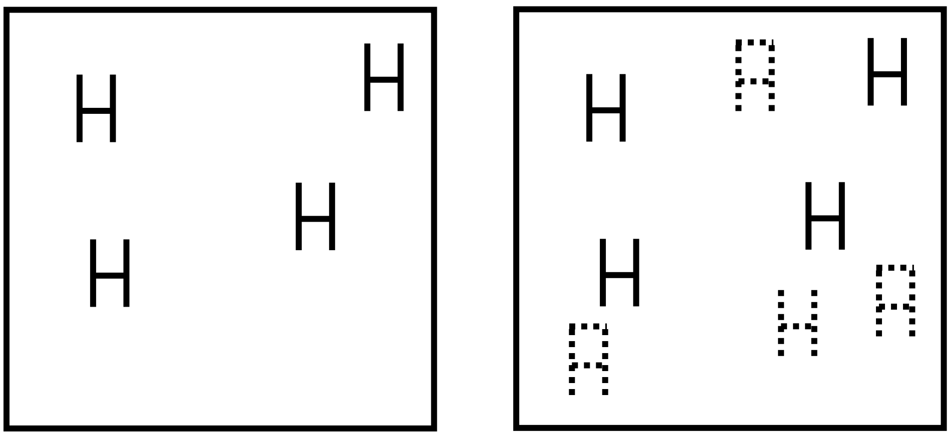


Figure 6. Example displays of Experiments 4 to 6. Solid lines represent green stimuli. Dotted lines represent blue stimuli

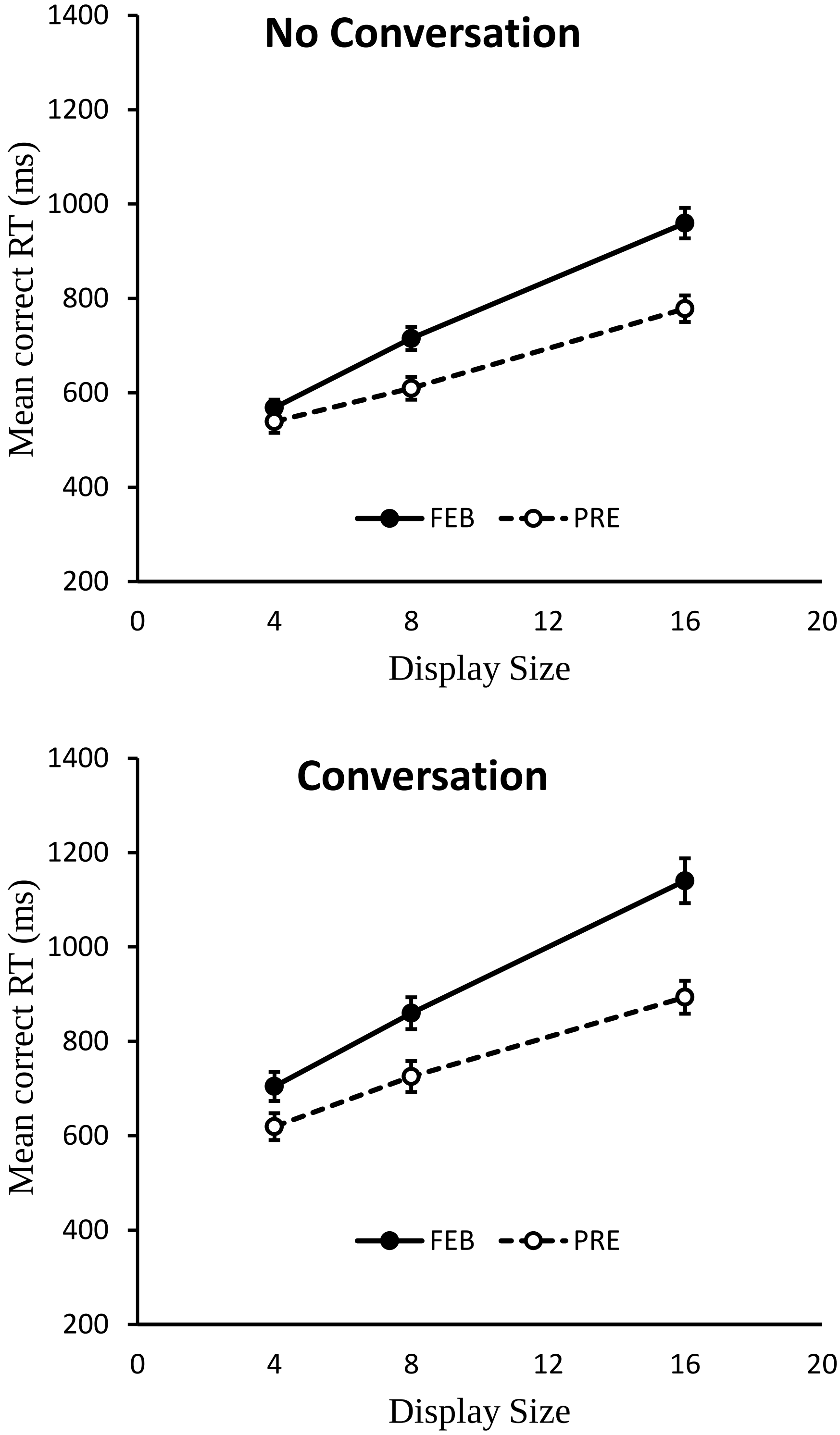


Figure 7. Mean correct RTs as a function of presentation, display size and conversation conditions in Experiment 4. Error bars show ±1SE.

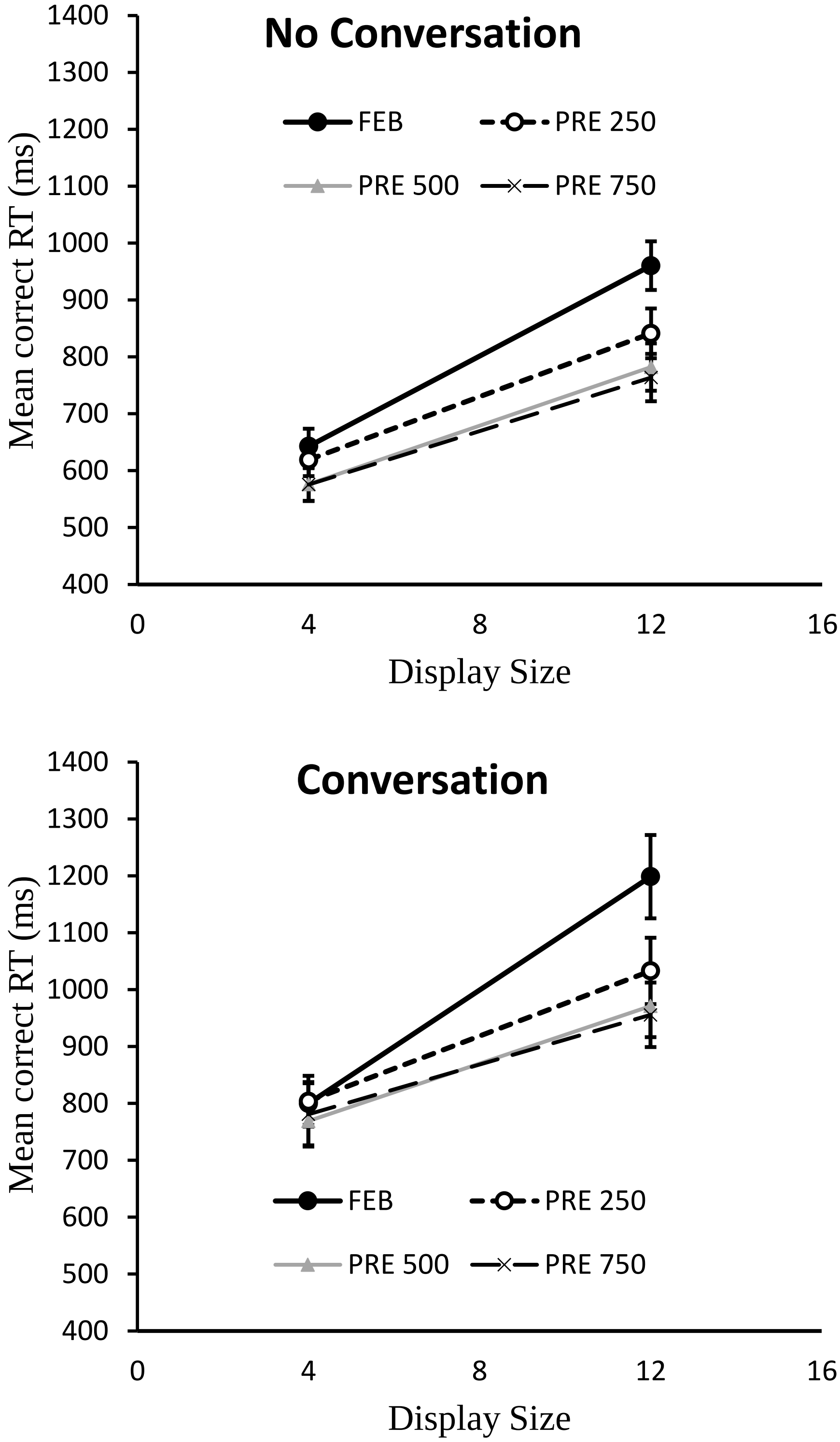


Figure 8. Mean correct RTs as a function of the presentation, display size and conversation conditions in Experiment 5. Error bars show ±1SE.

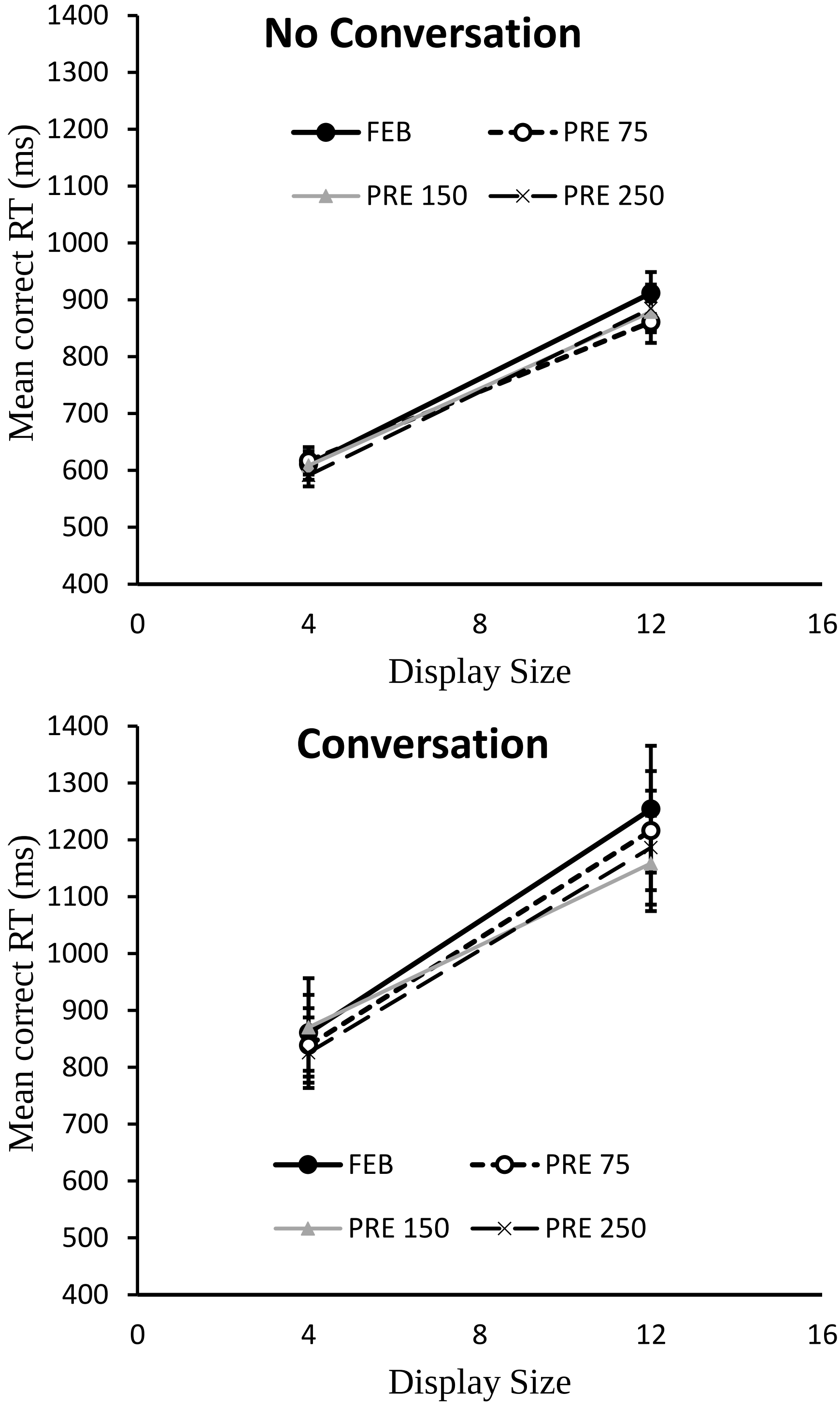


Figure 9. Mean correct RTs as a function of the presentation, display size and conversation conditions in Experiment 6. Error bars show ±1SE.