Exploring the underlying cognitive mechanisms of driver distraction

By

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Declaration

This thesis is submitted to the University of Warwick in support of my application for the degree of Doctor of Philosophy. It has been composed by myself and has not been submitted in any previous application for any degree. The work presented (including data generated and data analysis) was carried out by the author except in the cases outlined below.

Data collection for Chapter 7 of this thesis was completed by course instructors at Dorset Police Driver Education Unit, UK. For full details of their contribution please see the relevant chapter.

Inclusion of Published Work

Parts of this thesis have been submitted, or are intended to be submitted, for publication by the author.

Chapters 2 and 3 have been combined and are intended to be submitted to the Journal of Experimental Psychology: Human Perception and Performance with the following authors and title:

Gunnell, D. O. A., Kunar, M. A.,& Watson D.G., (In Preparation) Establishing the effect of conversation on memory in visual search.

Dr Watson and Dr Kunar both contributed to the planning of this research and provided helpful comments on the draft manuscript.

Chapter 4 is also in preparation and is intended to be submitted to the Journal of Experimental Psychology: Human Perception and Performance with the following authors and working title:

Gunnell, D. O. A., & Watson D.G., (In Preparation) Naturalistic spatial load and its effect on top down search in the preview benefit.

Dr Watson contributed to the planning of this research and provided helpful comments on the draft manuscript.

Chapter 5 is also in preparation and is intended to be submitted to

Psychological Science by the end of March 2017:

Gunnell, D. O. A., & Watson D.G., (In Preparation) You can't pay attention when I'm talking: Naturalistic conversation narrows how much you see.

Dr Watson contributed to the planning of this research and provided helpful comments on the draft manuscript.

Chapter 6 has been prepared for submission and is intended for submission by the end of March 2017:

Gunnell, D. O. A., & Watson D.G., (In Preparation) Risky conversations: Holding a telephone conversation leads to riskier decision making.

Dr Watson contributed to the planning of this research and provided helpful comments on the draft manuscript.

Gunnell, D. O., Kunar, M. A.,& Watson D.G., (Revise and Resubmit) The Hazards of Perception: Change blindness within a real-world driver education course

Dr Watson and Dr Kunar contributed to the planning of this research and provided helpful comments on the draft manuscript. Melvin Vincent, Laressa Robinson and other driver awareness course instructors from Dorset Police Driver Education Unit, provided helpful feedback on the intervention evaluated in this paper. They also helped us to find a place for the intervention within the driver awareness course and both, implemented the intervention and collected the questionnaire data.

Summary

This thesis explores how naturalistic dual tasks may affect the key processes which underpin everyday tasks. With a focus on processes essential to safe driving, this thesis attempts to establish whether naturalistic conversation has an impact on: intentional time-based visual selection [2], memory in visual search [3], our ability to become alerted and maintain an alerted state, to effectively orient our attention and to executively control attention [5]. I next examine the effect of holding a conversation on another essential element of driving, our ability to make risky decisions [6]. Further, I investigate whether performing a different naturalistic task, following satellite navigation instructions, affects intentional time based visual selection and subsequent search processes [4].

Finally, I present a study which was conducted in collaboration with Dorset Police (UK) Driver Education Unit. This study reports the design and evaluation of a road safety intervention which was included within the driver awareness courses delivered by my collaborators. The intervention was effective in reducing drivers' self-reported overconfidence in their observational abilities and the findings suggest that the intervention has the potential to have a positive impact on subsequent behaviour.

The laboratory work of this thesis demonstrates that naturalistic conversation impacts upon the rate at which we are able to search through the world around us, negatively affects reaction times and limits the amount of information that may be taken in from a single fixation. In addition, I show that we take greater risks and are less likely to experience a physiological response to the consequences of our actions when conversing.

These findings are discussed in the context of their theoretical implications and have wide reaching real world impacts on, for example, vehicle interface design, the use and design of in-vehicle technology and legislation focusing on distracted driving and driver education.

Chapter 1:

General Introduction

In the UK 73% of people hold a driving licence and 84% of people travel by private car at least once or twice a week, either as a passenger or as a driver (Department for Transportation, UK, 2015). It is fair to say that driving is a regular activity for the majority of people. In the modern world, however, drivers' attention is constantly being challenged. Drivers must navigate a world full of potential distractions, from conversations with passengers to in car entertainment systems and, of course, the mobile phone. With the popularity of the smart phone in modern culture, it is not surprising that evidence suggests that their use by drivers is widespread and growing and this trend has been observed both in the United States (Glassbrenner, 2005) and the United Kingdom (Walter, 2009). Many studies point to the dangers of talking on a mobile phone whilst driving (McKnight & McKnight, 1993; for reviews of this area see Collett, Guillot & Petit, 2010; Haigney, & Westerman, 2001; McCartt, Hellinga & Bratiman, 2006). In fact, some of the earliest work in this area estimated that using a mobile phone whilst driving may increase the likelihood of a vehicular collision by four times (Redelmeier & Tibshirani, 1997).

There is some debate as to whether the physical act of holding a mobile telephone to your ear causes a greater impairment than a hands-free conversation (for a review see Ishigmama & Klein, 2009). For example, Consiglio, Driscoll, Witte and Berg (2003) using a driving task, showed that performance, as measured by reaction times, was almost identical across both a hands free and hand held mobile telephone conversation. Whereas Burnes, Parkes, Burton and Smith (2002) demonstrated, using a driving simulator, that participants exhibit poorer driving performance when talking via a handheld device relative to a hands-free device. However regardless of this debate, what is clear is that simply conversing on a hands-free device is able to cause severe detriments to performance (for example see, Barkana, Zadok, Morad & Avni, 2004; Collett, Clarion, Morel, Chapon & Petit, 2009; Kircher, et al., 2003; Kunar et al., 2008). Not least amongst these detriments is the fact that it has been demonstrated that mobile phone use significantly impacts upon participants' reaction times whilst driving (for a meta-analysis see Horrey & Wickens, 2004). In addition, evidence from simulator studies suggest that it may also be causing several other deficits in driving performance such as keeping in lane (Abdel-Aty, 2003), increasing traffic violations (e.g. failing to stop at stop signs and speeding) and attentional lapses (Beede & Kass, 2006).

With the rising acceptance that hands-free mobile telephone conversations are able to impact upon driving performance, researchers are beginning to focus on the conversational component in isolation of the manual components of engaging in a phone call. One such study by Spence, Jia, Feng, Elserafi, and Zhao, (2013) focussed specifically on verbal tasks and their influence on visual attention and found that reaction times were significantly increased under conditions of conversational load. In addition, it has been shown that the content of a conversation is important for establishing what effect it may have on performance. Briggs, Hole and Land (2011) showed that more emotional conversations, specifically conversations about a participant's phobia, caused greater detriments to driving performance and induced visual tunnelling. Dula, Martin, Fox, and Leonard (2011) in a similar study also demonstrated this effect, showing that when participants were asked to drive in a simulator and hold an emotional conversation about a deeply held belief, they were more likely to engage in more dangerous driving behaviours (such as, speeding, crossing the centre line and experiencing collisions), as compared to a no distraction and a mundane conversation condition. However, it is not just the emotional content of a conversation which may cause distraction. Briggs, Hole and Land (2016) showed that participants' performance in a hazard detection task was significantly impaired by holding a concurrent conversation which induces visual imagery.

It could be argued that much of the work which has focused on understanding the interference between cell phone use and driving has, by necessity, been performed at a relatively coarse, global level. When studying a real world behaviour like driving, it is difficult to have both a carefully controlled lab based experiment, in which to test a particular aspect of the cognitive system, and also have the results of that experiment be externally valid and applicable to real world behaviour. However, for a true understanding of the effects of naturalistic conversation on driving behaviour, it is necessary to take a step back from the overall driving task and focus, specifically on fundamental aspects of the task itself such as visual attention and risky decision making. These components can then be surgically dissected in order to better understand how distraction may affect them, in this way the results will be informative in the context of driver distraction but also applicable to a wide range of situations. To that end the lab work reported in this thesis will focus first on understanding how naturalistic tasks, namely conversation and following satellite navigation directions, interact with visual attention. The focus then shifts to understanding how naturalistic conversation may interact with our ability to evaluate the riskiness of a situation, our propensity to make risky decisions and our physiological responses to the outcomes of those decisions. Chapter 1 provides a general introduction to the work of this thesis, however, a specific introduction detailing the relevant literature is given at the beginning of each chapter.

Conversation

Many of the chapters in this thesis focus on the effect that naturalistic conversation may have upon a specific aspect of behaviour or cognition. Therefore, it is important to first discuss what I mean when I use the term naturalistic conversation. Throughout this thesis where the term naturalistic conversation is used it is meant to denote a two way conversation which would be familiar to anyone who has attempted to get to know someone on a university campus. Where conversation is used as an experimental variable it was designed to be as close a proxy to a “normal” conversation as possible. This is particularly important given the literature discussed previously which shows that the content of the conversation being held can have an impact on performance in another task (Briggs, Hole & Land, 2011; Briggs, Hole & Land, 2016; Dula, Martin, Fox, & Leonard, 2011). Therefore, the majority of the conversations held in the experiments presented in this thesis covered some or all of the following topics, life at university, accommodation, food, friends and family, pets and travelling/holidays, please note however that this list is not exhaustive. The experimenter’s role was to ensure, as much as possible, that the conversation flowed 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 goal of this thesis is to better understand whether aspects of the driving task such as visually attending to the world around us, are affected by holding a naturalistic conversation. There are many stages and aspects of the conversational task and while it is not the focus of this thesis to delineate the effects of each of these stages on driving related tasks it is still worth considering their impact where relevant.

For example, the conversation used throughout this thesis was designed to be as natural as possible it is likely, therefore, that the properties of the conversation would conform to the standards of two way communication. In an average conversation which involves turn taking, speakers take on average two seconds per turn and leave only a 200ms gap between each other's turns (Levinson, 2016). The cognitive challenge then is for participants in a conversation to encode what is being said by their counterpart whilst formulating their own response in a very short time, this processes may involve predicting to some extent aspects of the other persons turn so that an appropriate response can be prepared in time (Levinson & Torreira, 2015). If the participant is attempting to complete a visual attention task which also requires key timing of attentional and cognitive resources, then we may observe a deficit in either task when one must be sacrificed for the other. That is unless the timescales of each task allow for efficient task switching to occur so that both the conversation and the visual task can be processed and completed without significant overlap of resources occurring. Indeed Shinohara et al., (2010) found that when participants were required to perform a visual search task a secondary verbal task such as speech production, when performed concurrently, was able to affect search performance. They suggest that this effect may be due to task switching.

In addition conversation requires many different processes such as listening to the other person's speech, comprehending it and then preparing and verbalising a response. In relation to this Kunar et al (2008) used a multiple object tracking (MOT) task to show that conversation has the potential to impact upon visual attention. Of importance, they showed that neither simply listening alone nor listening and vocalising a heard word (in a shadowing task), impacted upon MOT performance. However, holding a conversation and performing a word generation task did interfere with performance. Thus, at least in their experiment they demonstrated that the aspect of conversation which was impacting visual attention was the act of generating an appropriate response. As stated previously one of the aims of this thesis is to better understand how natural conversation is able to impact upon the key elements of the driving task. Therefore, it will be important to keep in mind these general properties of a conversation when interpreting the findings of the work presented in this thesis.

Visual Attention

Aspects of visual attention are often studied in isolation, that is, researchers tend to focus on measuring performance using single task paradigms (e.g. Fan et al., 2002; Posner, 1980; Olivers & Humphreys, 2003; see Carrasco, 2011 for a review). However, in the real world, tasks which rely on visual attention are often performed in conjunction with other tasks, such as conversing with others or simply listening to the radio. The effects of these seemingly mundane and trivial additional tasks should not be overlooked, especially when we consider that they are often performed in conjunction with tasks where response times and decision making are critical to safety, such as when driving.

It is a well-established finding that performing two tasks concurrently can cause a deficit in one’s ability to perform either task (e.g. Humphreys, Watson & Jolicœur, 2002; Kunar et al., 2008; Shinohara, Nakamura, Tatsuta, & Iba, 2010; Spence et al., 2013; Strayer, Drews & Johnston, 2003). One explanation for this is that each task requires a certain amount of resources and when an overlap occurs, performance in one task must be sacrificed for performance in the other (Pashler, 1994). While tasks which require common resources are most likely to interfere with each other (Wickens, 1980, 2002), it is also possible for cross modal tasks to interact (Van der Burg, Olivers, Bronkhorst & Theeuwes 2008; Kunar et al., 2008). For example, Kunar et al., (2008) found that simply holding a naturalistic conversation could impact upon participants' ability to track multiple objects in a scene.

It may not always be obvious what the effect of carrying out two tasks simultaneously will be. Even mundane everyday tasks can be affected if they are performed in conjunction with other tasks. For example, for most people walking is an automatic task which requires little effort, similarly, most people are able to perform simple mental arithmetic with ease. However, it has been known for a long time that attempting to do both concurrently can be challenging (Kahneman, 1973, p. 179). This same interference effect can be found in the visual search literature. For example, in time-based selection, a response time benefit is usually observed when a set of distractors is previewed prior to the presentation of a second set of distractors and a target item (Watson & Humphreys, 1997, 1998). However, this *preview benefit* can be reduced when people have to perform a second competing task at the same time. Humphreys, Watson and Jolicœur (2002) conducted an experiment in which a stream of digits was presented at the onset of the preview items and participants were required to monitor this stream. Irrespective of the modality in which the stream was presented (auditory or visual), this secondary monitoring task interfered with the search task and the preview benefit was disrupted. However, note that if the stream was presented halfway through the preview period, only the visual task caused interference suggesting the existence of a two component inhibitory process. One process (the setup) required general resources and the other component (maintenance) required only visual modality resources. Therefore, it seems that not only is the modality of the secondary task important, but also the timing of the secondary task relative to the primary task.

In the real world many tasks compete for our attention and it is, therefore, necessary to selectively attend to certain tasks over others (for a review see, Driver, 2001 and Petersen & Posner 2012). We can allocate attention intentionally in a top-down goal-driven way, for example, we can visually search a scene to look for a particular target by searching through items that are a particular colour (Kaptein, Theeuwes, & van der Heijden, 1995; Wolfe, 2007) or appear together at a particular point in time (Watson & Humphreys, 1997). However, attention can also be captured via bottom-up mechanisms. A flashing warning light, an abrupt onset, or the appearance of a new object may attract our attention (Remington, Johnston, & Yantis, 1992; Theeuwes, 1994; Yantis & Hillstrom, 1994; Yantis & Jones, 1991; Yantis & Jonides, 1984; Yantis & Jonides, 1990) or we may suddenly find ourselves listening to a new conversation simply because we overheard our name (Conway, Cowan, & Bunting, 2001; Moray, 1959). Whether we voluntarily split our attention between two tasks or not, performance on each task may be worse than if each had been completed independently of the other (Allen, McGeorge, Pearson, & Milne, 2006; Strayer & Johnston, 2001).

For most people conversing is an everyday activity which is often performed in conjunction with other tasks, yet its effects on those tasks are rarely considered. This is concerning, especially in cases in which the negative consequences of conversing can be severe, such as when talking on a mobile phone whilst driving. As mobile phones have become a major part of modern life they are increasingly being used at times which may be dangerous to the user and to others. For example, Trefner and Barrett (2004) demonstrated that holding a hands-free mobile phone conversation whilst driving can cause drivers to exhibit sub-optimal braking behaviour, as well as delay the engagement of critical control actions linked to braking. Strayer, Drews and

Johnston (2003) also investigated the effect of holding a mobile phone conversation on driving and used a simulated driving procedure in which participants either performed a car following task or were asked to follow directions and drive through a sub urban area. They found that using a hands free mobile phone whilst driving impaired driving related visual attention. They propose that this was due to reduced attention to visual inputs based on findings showing that when using a mobile phone: i) drivers showed impaired explicit memory of bill boards, even when they had fixated them, and ii) that implicit memory for words presented at fixation was also impaired. Related to this, McCarley et al., (2004) also found evidence which suggested that participants were paying reduced attention to their visual inputs. Specifically, they found that participants took significantly longer to detect changes in sequentially presented scenes (a change blindness task, Rensink, O'Regan & Clarke, 1997) when they were conversing. More generally it has been shown that talking on a mobile phone may be as detrimental to driving performance as being under the influence of alcohol (Strayer, Drews, & Crouch, 2006).

Research demonstrating the effects of conversation on real world task performance, especially those involving vision, illustrate the need for a greater understanding of why and how visual attention is affected by conversation. In fact, Shinohara et al., (2010, pp. 43) state that "We focused on a visual search task, which is one of the most important cognitive sub-tasks for driving". It has been shown that conversation can have an impact on even the simplest of tasks which require attention, tasks which are essential for safe and effective driving. For example, conversing while searching a visual scene is associated with a significant increase in the time taken to complete the search (Shinohara, Nakamura, Tatsuta & Iba, 2010). Further, Kunar et al., (2008) found that participants' ability to perform a multiple object tracking (MOT) task was significantly reduced when they were required to hold a naturalistic conversation with the experimenter. However, of note, simply listening and shadowing a word sequence did not significantly affect MOT performance. In a typical MOT task participants are presented with a display of identical items, some of these items will be identified as targets, these items will move around the screen independently and the participants must attempt to keep track of the targets for a set duration of time (Wolfe, Place & Horowitz, 2007). The MOT task is expected to require both selective and sustained attention and as such Kunar et al., (2008) took their finding to suggest that mobile phone conversations impaired attention at a central stage (caused by the generation of verbal responses) as opposed to a peripheral stage (verbalisation and listening).

In this thesis I aim to both extend the work outlined above and explore new areas which have thus far been overlooked. It has been established that there is an overall global effect of conversation on our ability to attend to objects in the world around us and that our reaction times are impaired. However, a thorough understanding of the manner in which naturalistic distraction, such as conversation, interferes with visual attention is currently lacking. Therefore, in this thesis I will investigate the effect of holding a naturalistic conversation on several of the fundamental mechanisms that underlie visual search and visual attention. Specifically, I will focus on our ability to benefit from the top down guiding of attention (Chapter 2), memory in visual search (Chapter 3) and our ability to become alerted and maintain an alerted state, orient to meaningful objects and exert executive control over what we attend to in the world around us (Chapter 5). In Chapter 4 I build on the findings of Chapters 2 and 3 and assess whether a different naturalistic task, one which applies a spatial load, is able to affect our ability to benefit from the top down guidance of attention.

Risky decision making under dual task conditions

It is important to note that the driving task does not rely on visual attention alone. Among other key processes, drivers must be able to make decisions effectively in real time. Many of those decisions may involve being able to process and then evaluate information pertaining to the riskiness of an action and then make an appropriate risky decision based on this information. For example, they must be able to decide when it is safe to overtake, when to pull out of a junction and at what speed to enter into a corner.

There are inherent risks attached to most decisions, such as whether or not to buy a lottery ticket, where to invest our savings or if it is safe to overtake a vehicle. Some risks are more obvious or salient than others. For example, the odds of winning on a scratch card are displayed in plain sight and the main cost associated with the risk (the price of the card) is clear. Other risks are not as easily quantifiable, but their outcomes can have extremely serious consequences, for example, making the choice to speed in order to get home from work earlier may result in a speeding ticket or even an automobile accident which could otherwise have been avoided.

There are no steadfast rules for knowing who will take risks and in what circumstances; some individuals may be more inclined towards risk taking than others. In addition, individuals may not be consistently “risky” across all situations and may be motivated towards risk taking for different reasons (Figner & Weber, 2011). For example, adolescents have been found to take more risks than adults (Galvan, Hare, Voss, Glover & Casey, 2007) and young drivers between the ages of

16 and 25 are more likely than older drivers to take risks while driving (Jonah, 1986; Keating & Halpern-Felsher, 2008).

More generally, the propensity to take risks varies between contexts. The Domain Specific Risk Taking (DOSPERT) scale was created to demonstrate these differences and assess attitudes to risky decision making across five different domains: financial, health/safety, recreational, ethical and social (Weber, Blais & Betz, 2002). A shortened version of this task has been used to show that the within-subjects variance in risk taking between domains was around seven times greater than the between-subjects variance (Blais & Weber, 2006). Thus the level of risk that people are prepared to take can vary greatly across different contexts; someone might hold risky attitudes towards recreational activities, but be very risk averse when it comes to making financial decisions.

There are many factors which can influence who might take greater risks and when they are likely to do so. Certain demographic factors have been shown to be related to increased risk taking, for example a participant's age. Younger adults have been shown to make riskier decisions than adults when those decisions engage the affective network and are made dynamically, in real time, as opposed to slower more deliberative decision making (Figner, Mackinlay, Wilkening & Weber, 2009). In fact, in the domain of driving behaviour, it has been found that young males underestimate their risk of having a collision (Finn & Bragg, 1986) and generally perceive driving to be less risky (Rosenbloom, Shahar, Elharar, & Danino, 2008) compared with their peers and older drivers.

These risky behaviours may also be exacerbated by external factors. Simply performing a driving related task in the presence of peers has been shown to impact upon participants' risk taking behaviour in both adolescents (Chein, Albert, O'Brien, Uckert & Steinberg, 2011) and college age participants (Gardner & Steinberg, 2005). Given that young adults are the most likely to use their mobile phones whilst driving (Glassbrenner, 2005) and that the act of talking on the mobile phone could act as a virtual reminder of the presence of peers, it stands to reason that talking on a mobile phone may in fact influence risk taking behaviour. In fact it could be predicted that talking on a mobile phone would influence risk taking behaviour simply because participants may pay less attention to the world around them and so may miss important pieces of information which indicate that a situation is risky (Strayer, Drews and Johnston, 2003). Alternatively, it has been suggested that in some circumstances risk taking behaviour relies on the cognitive control network to inhibit affective impulses in favour of more deliberative processes (Figner, Mackinlay, Wilkening & Weber, 2009). If this is the case then performing the risk task whilst concurrently holding a conversation may result in insufficient resources being left over to inhibit these affective impulses which may lead to increased risk taking.

However, to my knowledge, little research has been performed which focuses specifically on how an externally valid, naturalistic task, such as holding a conversation, may impact upon risk taking behaviour. One piece of work which examines risk taking behaviour under carefully controlled lab based dual load was performed by Turnbull, Evans, Bunce, Carzolio, and O’Connor (2005). These researchers asked participants to complete the IOWA gambling task while also performing one of two additional tasks. The first was an articulatory suppression task in which the participants must recite the number sequence 1 through to 9, a task which was not expected to load executive resources. The second was a random number generation task, which was expected to load executive resources as participants were required to produce random numbers from one to nine, but not produce sequences. In the IOWA gambling task participants are presented with four decks of cards and must choose to turn over cards from these decks until the experiment ends. They are told that their goal is to make as much money as possible. Choosing a card from any deck will usually result in winning a set amount of money, however, turning a card over will occasionally result in a loss. Two decks give large gains but consistently choosing these decks will result in an overall loss, other decks give smaller gains but if consistently chosen result in an overall win (Bechara, Damasio, Tranel & Damasio, 2005). Turnbull et al (2005) found that in their study participants' performance in the gambling task was not significantly affected by the dual tasks. It should be noted that a possible explanation of why these tasks did not interfere with participants' performance on the gambling task is that the IOWA gambling task is thought to rely on emotion based learning. Neither of the additional load tasks applied load which would be expected to overlap with resources necessary for emotion based learning (Turnbull et al., 2005).

Horswill and Mckenna (1999) also conducted research in this area. However, their work was not as strictly lab based as the work by Turnbull et al., (2005) and was more applied in nature. Horswill and Mckenna (1999) asked people to monitor an auditory stream for a target letter whilst also performing a task which would assess their risk taking in a driving context. The driving tasks consisted of video simulations in which a participant viewed a driving scene, filmed from the drivers' perspective, and was required to respond to events in the scene. Two types of simulations were used, one in which the participant was required to watch as the camera vehicle followed another vehicle on the motorway. The camera vehicle gradually approached the vehicle in front and the participants were required to respond when they felt that the car was uncomfortably close. The second simulation was set up so that the camera car was waiting at a junction. Participants were required to respond each time they believed a gap in traffic was large enough that they would usually have pulled out of the junction. Participants in this study took more risks when their attention was split by the dual task compared to a single task condition, indicating that under conditions of attentional competition it is possible that participants' risk taking behaviour can be affected.

Chapter 6 in this thesis attempts to examine risk taking behaviour in a new way by establishing, under carefully controlled lab conditions, whether a dual task which is frequently performed in the real world can affect participants' ability to assess the level of risk and make risky decisions. Talking on a mobile telephone was chosen as the externally valid task due to the fact that, as previously discussed, it is a behaviour which is likely to be engaged in by drivers. In addition, in order to drive safely drivers must be able to effectively evaluate the riskiness of their behaviours and choose to take risks appropriately. If our ability to make risky decisions is impaired then this would have profound implications for driver safety and the law around mobile phone use whilst driving.

Driver education

The final empirical chapter of this thesis focuses on driver education. As this PhD research was a collaborative endeavour with Dorset Police Driver Education Unit, I attempted, as much as possible, to fulfil the practical needs of the collaborating organisation. This resulted in a series of studies being performed. These were designed to provide insights into the effectiveness of the driver awareness courses in achieving their key learning objectives, as well as providing specific data as to attendees' attitudes towards different distracting activities which may be likely to be performed whilst driving. Using this information we then set about designing an intervention which could be used within the driver awareness courses to enhance their effectiveness in a particular learning area.

According to the Driver and Vehicle licencing agency in the UK, drivers must pass a driving theory and driving practical test in order to receive their licence. However, after this basic training they are not required to undertake any additional training or driving assessments. While it has been shown that in some areas drivers gradually improve as they become more experienced (e.g. general vehicle control such as during manoeuvres), it is also the case that bad driving habits can build up over time (Duncan, Williams & Brown, 1991). The average driver is unlikely to receive any additional guidance on their driving, other than feedback from peers, other vehicle occupants and government sponsored mass media safer driving campaigns.

While campaigns which aim to raise public awareness of health related issues, such as smoking, have been shown to be effective in certain circumstances (Hammond, McDonald, Fong, Brown, & Cameron, 2004) there is a growing body of literature which suggests that their effectiveness in the domain of driving can be limited by several key factors. For example, fear campaigns may be actively avoided by drivers (Ruiter, Abraham & Kok, 2001) and research shows that the very people who would benefit the most from general mass media campaigns may be the least likely to be exposed to them (Weenig & Midden, 1997; see Hoekstra & Wegman, 2011 for a review of road safety campaigns). In addition, it has been shown that drivers can watch an educational video designed to exhibit the dangers of a particular driving behaviour and then report an inflated opinion of their own driving skill (Harré, Foster and O'Neill, 2005). One possible explanation for this is that when drivers view campaigns such as these, which show another driver having an accident, it is too easy for drivers to disregard the main message of the campaign as not applying to them because they believe that they are a better driver than the person in the video. This explanation relates to evidence suggesting that drivers show a strong bias when asked to rate their own driving ability; they are significantly more likely to rate themselves as better and less risky than the average driver (Svenson, 1981).

However, if a driver commits a relatively minor road offence, such as speeding, running a red light or talking on their mobile phone whilst driving in the UK, then they may be offered a course designed to educate them as to the unsafe nature of their driving instead of receiving penalty points on their licence. We were given the opportunity by our collaborators to design and implement an intervention which would be delivered as part of the driver awareness course run by Dorset Police Driver Education Unit. Inspired by models of behavioural change which emphasise our capability to perform an action as a motivator for behavioural change (Michie, van Stralen, & West, 2011), I set about to design an intervention which I believed would target participants' overconfidence in their driving related abilities (Svenson, 1981), particularly overconfidence in their observational abilities (Levin, Momen, Drivdahl & Simons, 2000). I based the intervention on a well-known visual attention phenomenon, Change Blindness (Rensink, O'Regan & Clarke, 1997). The task was designed so that it could be easily adapted for use online or as a media campaign. Taking into account what is known about mass media campaigns, it is hoped that given the fact that the task does not rely on fear, was engaging, and most importantly demonstrates directly to participants a flaw in their own beliefs about their observational abilities that it would be effective at changing driver attitudes and the message would not be easily ignored. However, it should be noted that I did not implement it in the context of a mass media campaign, therefore this remains an aim for future work. The design and evaluation, using both quantitative and qualitative methods, of the intervention is presented in Chapter 7.

The structure of this thesis

Clearly, as reviewed above, conversation has been shown to affect performance in certain specific situations. However, a full understanding of when and how conversation will affect performance requires study over a comprehensive range of tasks. In the current work I examine the effect of naturalistic conversation on a range of visual attention tasks and the fundamental mechanisms underlying attention (Chapters 2, 3 and 5). I then focus on a second, key aspect of the driving task, affective risky decision making. The initial empirical chapters examine the effects of conversation on two distinct areas of visual processing, intentional time-based visual selection (Watson & Humphreys, 1997) (Chapter 2) and contextual cueing (Chun & Jiang, 1998) (Chapter 3). More specifically, Chapter 2 examines the extent to which people's ability to prioritise new information is influenced by conversation, whereas Chapter 3 examines the effect of conversation on people's ability to learn and then apply spatial contexts to help guide attention.

Based on a hypothesis formed from the findings of Chapter 2, Chapter 4 deviates from the central theme of this thesis and examines the effect of naturalistic visual and auditory distraction, which has a clear spatial element, on intentional time based visual selection. Having focussed on memory in visual search and time based visual selection, I, in Chapter 5, attempt to establish the effect of conversation on the three fundamental attentional networks (Petersen and Posner, 2012). In this chapter I ask if participants' ability to become alerted and maintain an alerted state, to orient their attention to important aspects of a scene and to apply executive control to attention, is affected when participants are holding a naturalistic conversation.

Chapter 6 is the final chapter which features a lab based experiment. In this chapter I investigate the effect of naturalistic conversation on another fundamental element essential to the driving task, risk taking behaviour. Participants were asked to hold a naturalistic conversation while simultaneously performing an affective risk taking task, the Columbia Card Task (CCT; Figner, Mackinlay, Wilkening & Weber, 2009). In addition to behavioural measures of risk taking, I also collected and analysed participants' physiological responses to key events in the risk task, such as when they took risks and as a result experienced a loss.

The final empirical chapter of this thesis reports field work which was performed in situ within the driver awareness courses run by my collaborator Dorset Police Driver Education Unit UK. Chapter 7 presents an intervention which was designed for use within the driver awareness courses. This intervention or teaching tool was designed with input from my collaborators and aimed to assist in the delivery of a key learning point from the course, the importance of remaining vigilant in one's observation whilst driving.

In the final chapter (Chapter 8) I present a summary of my findings and discuss the real world implications and experimental limitations of my work. This introductory chapter was intended to give an overview of the topic areas that this thesis covers as well as give a general introduction into the issues of driver distraction. Comprehensive reviews of the relevant literature are presented at the beginning of each chapter.

Chapter 2:

Attention while talking: Establishing the effect of naturalistic conversation on the top-down guidance of visual attention

The ability to attend to our environment and efficiently search through it is key in helping us to interpret and then interact with the world around us. Our attention can be automatically drawn to aspects of a scene or guided by our goals and intentions (e.g. see Theeuwes, 2010; Watson & Humphreys 1997; Wolfe, Butcher, Lee & Hyle, 2003). For example, when looking for our keys we are able to apply our top down knowledge of what we are looking for and where it is likely to be. This helps us to actively guide our attention through the complex visual scenes presented to us. However, our attention can also be guided automatically and not necessarily with conscious awareness. This is beneficial as it allows us to become aware of unexpected stimuli such as an angry face looking at us in a crowded bar (e.g. Yantis & Jonides, 1984).

Visual attention although fast and efficient is not infallible, our attentional systems are susceptible to several psychological limitations some of which can be demonstrated through the use of experimental paradigms e.g. the attentional blink (Raymond, Shapiro, & Arnell, 1992) and change blindness (Simons & Levin, 1997). In addition, our ability to attend to a visual scene can be degraded if our attentional resources are being split in order to perform several tasks simultaneously. Pashler (1994) postulated that an attentional bottleneck may form under certain circumstances where one task must be selected for continued processing over another. When this occurs performance in the non-prioritised task may be reduced. Wickens (1980, 2002) presented a multiple resource model which helps to predict and explain the effect that certain additional load tasks will have on a primary task. In this model, tasks which are in the same modality or require similar resources are more likely to impact upon each other. However, it is not always clear what type of load will influence our ability to observe the world around us, or indeed the type of load that is encountered in every-day situations such as talking on a mobile phone.

Therefore, lab based tasks are particularly useful for isolating particular types of load and then dissecting their effects on aspects of visual attention (e.g., Humphreys, Watson & Jolicoeur, 2002; Manginelli, Langer, Klose and Pollmann, 2013). While these tasks may not map onto externally valid tasks in a 1 to 1 way, they do provide useful insights which can be used to better understand dual task performance in the real world.

Various approaches have been taken to assess the mechanisms that facilitate visual search; one such method is preview search (Watson & Humphreys, 1997). In this paradigm one set of distracters is *previewed* by being presented for a period of time (the preview duration) before the onset of an additional set of distractors and a target. The target is never in the previewed stimuli and so it is beneficial for participants to ignore or suppress them. Search efficiency (as measured with RT-display size slopes) in the preview (PRE) condition is typically compared to performance in a full element baseline (FEB) in which all items appear at the same time, and a half element baseline (HEB) in which only the second set of stimuli from the PRE condition is presented. A preview benefit is evidenced by the finding of more efficient search in the preview condition than in the FEB condition, suggesting that fewer items need to be searched when a subset of items is previewed.

One account of this finding is based on the proposal that previewed items are actively inhibited during the preview period via a mechanism called *visual marking*

(Watson & Humphreys, 1997). This process is top-down, memory dependent, resourced-limited and goal driven. The inhibition of a subset of search items (those that are previewed) allows selective search through a new set of stimuli which, when they arrive, are not inhibited. According to the visual marking account, inhibiting irrelevant stimuli produces an increase in the salience of signals associated with the appearance of subsequently appearing new items (such signals could include luminance-, colour-, or object-based representations). The previewed stimuli must be presented for at least 400ms for a full benefit to occur (Watson & Humphreys, 1997). This preview period, it is hypothesized, allows time for the generation of an inhibitory template directed towards the old items (Watson & Humphreys, 1997). More recently it has been shown that with small numbers of items, visual working memory may play an important role in the regulation of such inhibition (Al Aidroos, Emrich, Ferber & Pratt, 2012).

An alternative explanation is presented by Donk and Theeuwes (2001, 2003) who propose that new items automatically capture attention due to luminance changes coincident with their appearance. Therefore, this bottom-up account is not dependent on cognitive resources or memory, but rather, relies on the physical features of an abrupt luminance-onset to capture attention towards the new item locations. This research draws from previous work (Yantis & Jonides, 1984) which has shown that an item which has an abrupt onset experiences a processing advantage relative to items in the same display which lack an abrupt onset (e.g. items gradually revealed through a mask). In a follow up study, Jonides and Yantis (1988) investigated whether other stimuli properties, namely luminance and hue, could gather attention in the same way as an abrupt onset. However, luminance and hue changes were not found to be capable of capturing attention in the same way.

However, it has been argued that although automatic capture by luminance onsets may contribute to generating a preview benefit, they cannot fully account for it. For example, a preview benefit is still found when stimuli that are iso-luminant with their background (and so do not cause a luminance-onset when displayed), are used as search stimuli (Braithwaite, Humphreys, Watson & Hulleman, 2005; Braithwaite, Hulleman, Watson & Humphreys, 2006). In addition, many studies have shown that the preview benefit has a specific time course in that the previewed items must be presented for a minimum duration, usually around 400ms, for a preview benefit to be observed with 1000ms, giving ample time for a robust preview benefit (Watson & Humphreys, 1997; Humphreys, Stalmann & Olivers, 2004; Warner & Jackson, 2009). This is a much larger time interval than is required for new items to automatically capture attention (~100ms) (Yantis & Gibson, 1994). Moreover, probe dot detection methodologies have been used to investigate whether participants are indeed inhibiting old, previewed locations. When a probe was located at the position of an old item, detection was impaired. Of importance, this only occurred when it was advantageous to apply inhibition to the old items (Watson & Humphreys, 2000; Braithwaite, Humphreys & Hulleman, 2005).

Further evidence supporting the top down inhibition explanation of the preview benefit can be found from studies, such as Kunar and Humphreys' (2006) work which used a top-up procedure to investigate the preview benefit, whereby the items to be previewed were presented twice with an offset period in between. Their study suggests that it is necessary for attention to be applied to both the initial onset of the preview items as well as the re-presentation of the items. If attention was not applied to the re-presentation (top up) stage of the preview period then a preview benefit was not found; if attention was not paid to the initial presentation of the items then the preview effect was attenuated. The authors of this work suggest that if participants are able to attend to the early preview stage then a 150ms top up stage may be sufficient to produce a preview benefit as inhibition has already been established. However, if attention is not paid to this initial preview stage, then the top up stage alone is not sufficient to produce a full benefit. Additional evidence is provided from the negative colour carry over effect (Braithwaite and Humphreys, 2003; Braithwaite, Humphreys & Hodsoll, 2003; Braithwaite, Humphreys & Hodsoll, 2004). This shows that when participants must search through a display which is made up of targets of different colours, foreknowledge of the target's colour vastly improved search efficiency in both a FEB and PRE condition. However, if the target shared the same colour as the previewed items then search efficiency was reduced (Braithwaite and Humphreys, 2003). We would not expect these kinds of manipulations to affect the preview benefit if it were purely driven by luminance-onset attentional capture.

In addition to this, the luminance-onset attentional capture account would also not predict that if particular stimuli such as faces are used as search items then the preview benefit would be affected. In fact, this is precisely what is found. Only a partial preview benefit is found with face stimuli and specifically, at short preview durations, negatively valenced faces are particularly difficult to ignore, suggesting that ecological issues have an impact on the mechanisms which produce the preview benefit (Blagrove & Watson, 2010).

Finally, it has also been demonstrated that a (partial) preview benefit remains when the second set of stimuli is added to the display during an eye blink. In this case there are no unique luminance onsets associated with the appearance of the new items. This further suggests that luminance onsets are not crucial for generating a preview benefit and is consistent with the existence of a top-down process that applies inhibition to irrelevant stimuli (von Mühlenen, Watson & Gunnell, 2013). Nonetheless, the attenuated preview benefit in these situations could be consistent with a facilitator role for luminance onsets (or any other signals associated with the appearance of new objects).

Of key importance for the work presented in this chapter and consistent with a role for limited-capacity processes being involved in prioritizing new stimuli are findings in which a controlled secondary load-task has been presented throughout the preview period. The idea here is that if prioritizing new stimuli relies on a limited capacity inhibitory mechanism then reducing the available attentional resources via a competing task should reduce the preview benefit. Examining this possibility, Humphreys, Watson and Jolicoeur (2002) found that if participants had to monitor a stream of digits for a target digit during the preview period, the preview benefit was significantly reduced. This reduction occurred for both visual and auditory secondary tasks provided that they were presented at the onset of the preview display. If presented halfway through the preview period, only the visual secondary tasks caused interference. The authors proposed that the preview benefit consisted of an initial set-up process followed by the maintenance of inhibition towards the old stimuli. The setup stage required ‘general resources’ and so was disrupted by both visual and auditory load tasks. In contrast, the maintenance stage required only visual resources and so was immune to the simultaneous processing of an auditory stream.

Overall, the results on time-based selection suggest that new stimuli can be prioritized by virtue of dynamic signals associated with their presentation which can be augmented by top-down, capacity limited inhibition of irrelevant stimuli already in the field. The intentional, goal-driven, top-down nature of this selection mechanism brings with it adaptable flexibility for coordinating behaviour. However, because it is capacity limited it is also subject to potential interference from other activities that compete for a common pool of resources. From a practical point of view, determining the influence of competing tasks is important because it will allow us to predict when and how the efficient selection of new information might be compromised. This is especially the case for understanding behaviour in the real-world when we might be trying to, or having to, perform multiple activities simultaneously. From a theoretical perspective, assessing the influence of different types of competing tasks can help us to delineate the mechanisms underlying the selection of new information.

To date, no study to my knowledge has assessed the influence of holding a naturalistic conversation on participants' ability to prioritize new objects. As noted above, previous studies have shown that monitoring a simple stream of visual or auditory digits can reduce our ability to select new items efficiently (Humphreys, Watson and Jolicoeur, 2002). However, these relatively simplistic tasks do not tell us much about the efficiency of new item selection when participants take part in a conversation in which they have to listen, comprehend and generate meaningful linguistic responses. Such a task is likely to be much more multicomponent than the simple monitoring tasks examined by prior research. Thus the main goal of the present work was to assess the influence of holding a naturalistic conversation on time-based selection as assessed via the preview paradigm.

EXPERIMENT 1

In the first of the three experiments presented in this chapter I aim to test whether holding a naturalistic conversation while performing a preview search task results in the preview benefit being affected. Based on previous literature I predict that there will be an overall increase in participants' reaction times when they are conversing (Shinohara, Nakamura, Tatsuta, & Iba, 2010; Spence et al., 2013). Of more importance, I expect that holding a naturalistic conversation will reduce our ability to select new items efficiently. This is because the multi-component processes involved in comprehending and generating speech might rely on resources required for time-based selection (Humphreys, Watson & Jolicoeur, 2002). Please note that full ethical approval for the work presented in this chapter and in this thesis as a whole was granted by the Department of Psychology Ethics board, of the University of Warwick.

Method

Participants

Twenty eight University of Warwick students took part (male = 7, mean age = 20.5) in return for course credit or payment of £5 towards their expenses. Participants confirmed that they could easily hear the experimenter in the conversation condition and that they could see the visual display. The sample size was guided by previous literature (Watson & Humphreys, 1997) and was chosen based on the research of Humphreys, Watson and Jolicouer (2002) who found reliable dual task interference in the preview benefit with sample sizes of 12 participants. Investigating the effect of conversation on participants' performance in a visual search task, Shinohara et al., (2010) used a sample size of 20 participants. Therefore, in order to be conservative we tested a minimum of 20 participants in the experiments presented in this chapter. It should be noted that in this chapter and throughout this thesis, the sample size for each experiment was calculated through consideration of previous relevant literature, as described above. In some cases the exact number of participants may vary between each experiment presented in a chapter. This is due to the fact that an ideal sample size was decided upon however the practicalities of recruiting participants sometimes resulted in more participants attending the experiment than had been anticipated. In these cases the participants were allowed to take part. In other cases some participants data had to be removed, for example when a computer error resulted in the experiment no longer being reliable. Despite these minor variations in participant numbers, for all experiments presented in this thesis I ensured that the predetermined minimum sample size was exceeded.

Stimuli and Apparatus

The experiment ran on an IBM compatible PC via a custom written computer program. Displays were presented on a HP191 19 inch LCD monitor at a resolution of 1280×1024 and participants sat approximately 57 cm from the screen. Responses were recorded using a standard QWERTY keyboard. Participants were required to press the < key when the target appeared on the left and the > key when it appeared on the right. On some trials (catch trials) a target would not be present in the display, on these trials the participant was required to press the space bar. The conversation condition required the use of two hands-free phones. These were fitted with an internal speaker and the volume was set so that the experimenter could be heard clearly. The participant’s phone was positioned on their left directly beside the monitor. The experimenter’s phone was located in a separate room.

The stimulus properties were as follows, the fixation dot subtended 0.16° of visual angle horizontally and 0.13° vertically. The search stimuli consisted of H and A box-figure-8 letter stimuli (RGB: Blue = 68,164,176; Green = 11,193,126) which occupied 0.96° vertically and 0.88° horizontally. The stimuli were displayed on a black background within a 6 × 6 grid structure. Each cell in this grid occupied 3.6° of visual angle horizontally and 4.4° vertically, therefore the grid encompassed 21.9° of visual angle horizontally and 26.5° vertically. The target could not fall within the two central columns of the display matrix so as 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 positions were randomly jittered by up to 20 pixels within each cell of the matrix. 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.

Design and procedure

The experiment used a within-subjects 2×2×3 factorial design. The independent variables were conversation (conversation, no conversation), presentation condition (FEB, PRE) and display size (4, 8 and 16 items). The dependent variables were reaction times (RTs) and error rates. In the 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. After a response was made the display turned blank (500ms) after which the next trial began. In the FEB condition all search elements appeared simultaneously with no preview display. Please note that a HEB condition was not included in this experiment as this would increase the time required to complete the experiment by approximately a third and it was felt that this would make the duration of the conversation, and the experiment as a whole, unreasonable.

In the no-conversation condition, participants completed the FEB and PRE conditions without any external distractions. In the conversation condition, the participant held a simulated mobile phone conversation with the experimenter whilst completing their task. The hands-free phone conversation was designed to be as naturalistic as possible. While the experimenter was careful to avoid sensitive and emotionally salient topics, the participants were given freedom to talk about whatever they wished (for a similar methodology see Kunar et al., 2008). 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.

Each participant completed 8 experimental blocks split into two identical sets of 4 blocks: FEB-Conversation, PRE-Conversation, FEB-No conversation and PRE-No conversation. The order of the conversation and search condition was counterbalanced across participants and trial order was randomized within each block. Each block contained 66 trials (Total trials = 528). These were made up of 60 target present trials in which the target could appear either on the left or the right of the screen with equal probability. The remaining trials (6) were target absent trials. On these trials, a target was not present in the display and the participant was required to indicate this by pressing the space bar. This was to avoid participants developing a strategy whereby they only searched half of the display and therefore, by elimination, determined the location of the target. 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 visual feedback in the form of the word “incorrect” being presented in the centre of the screen when they made an error. Before taking part in the experiment and in all of the lab experiments presented in this thesis, participants read through an instruction sheet and signed a consent form.

Results

Reaction times

For the main analysis only RTs on trials in which a target was present were considered. Trials in which the participant made an incorrect response (1.8%), and RTs < 200ms (0.08%) were removed as outliers. Following this, means and SDs for each cell of the design were calculated for each participant and RTs falling further than 3SDs from this mean were discarded (1.4%). The resulting mean RTs were then analysed using a 2(Conversation: No conversation, Conversation) × 2(Presentation Condition: FEB, PRE) × 3(Display Size: 4, 8, 16) repeated-measures ANOVA. Here and throughout this thesis, when the assumption of sphericity was violated, the Greenhouse-Geisser correction was applied.

This analysis revealed significant main effects of presentation condition (FEB, PRE), $F(1,27)= 116.834$, $MSE = 12233$. $p < .001$, $n^2 = .812$, conversation, $F(1,27)= 44.9$, $MSE=30974$, $p < .001$, $n^2=.625$, and display size, $F(1.6,42.5)= 59.7$, $MSE = 3716$, $p<.001$, $n^2=.689$ (Figure 2.1). 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, $F(1.6,42.5)= 59.7$, $MSE = 3716$, $p < .001$, $n^2=.689$, Conversation × Presentation Condition, $F(1,27)= 5.10$, $MSE = 10302$, $p = .032$, $n^2=.159$, and Conversation × Display size interactions, $F(2,54)= 8.80$, $MSE = 1271$, $p < .001$, $n^2=.246$ (Figure 2.1). 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 (29.4ms/item) than in the no-conversation condition (26.2ms/item). These slope values can be seen in Figure 2.2. However, the 3-way interaction was not significant, $F(2,54)= 1.52$, $MSE = 2919$, $p = .229$, $n^2=.053$ (Figure 2.1), indicating that the preview benefit was not affected by the naturalistic conversation. Please note that in all of the figures presented in this thesis, where error bars are displayed they represent the standard error of the mean and play an arelational role (Rouder & Morey, 2005).

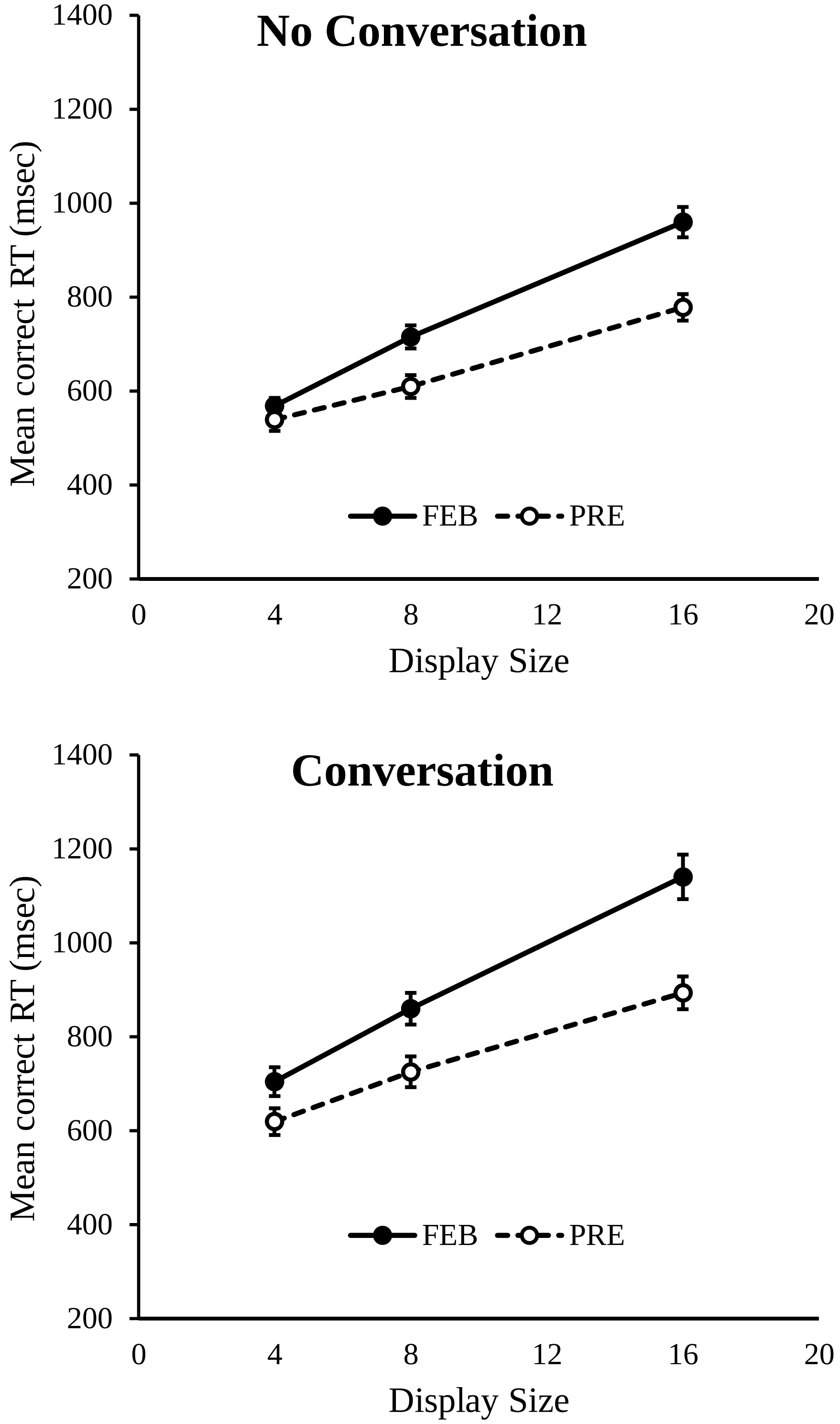

Figure 2.1 – Mean correct RTs as a function of presentation, display size and conversation conditions. Error bars show ±1SE.

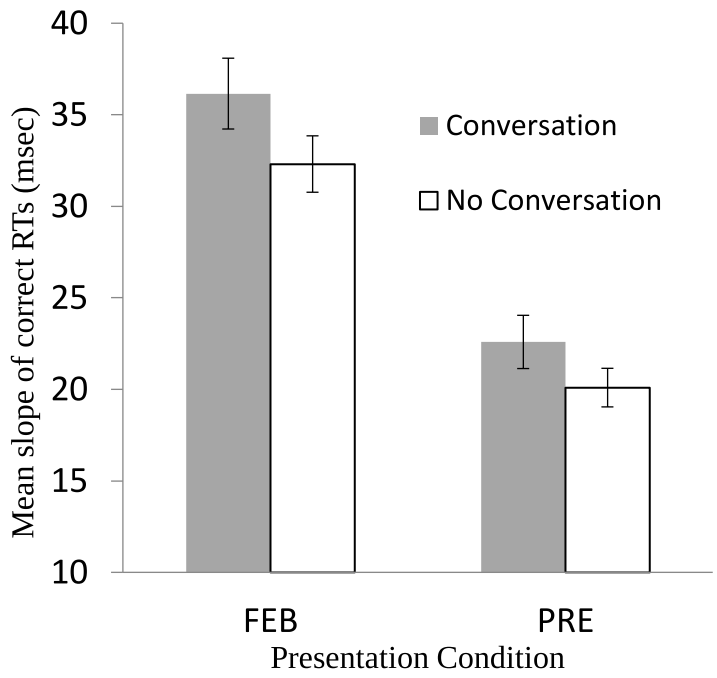

Figure 2.2 – Mean slopes of correct reaction times as a function of the presentation and conversation conditions. Error bars show ±1SE.

Errors

The number of errors was calculated by summing trials which resulted in an error if they met the following conditions. Firstly, the reaction time on the trial must be greater than 200ms and secondly, the trial must not be considered an outlier as outlined at the beginning of this results section. Finally, a target must have been present in the trial. Essentially error trials were only counted if the trial would have been included in the main reaction time analysis had an error not been made. Wherever search trial errors are considered in this thesis this rule applies. The overall mean error rate on search trials was very low (1.0%) and these data were not analysed further (Table B.1).

The overall error rate on target-absent catch trials was also relatively low (5%), suggesting that participants had been searching through the whole display. Due to the small numbers of catch trials, these data were not analysed further.

Discussion

The main aim of Experiment 1 was to determine the influence of naturalistic conversation on the efficiency of intentional time-based visual selection. The findings showed overall that holding a naturalistic conversation had little, if any, impact on participants' ability to exclude old, irrelevant distractors and prioritize the selection of new stimuli. That is, there was a robust preview benefit in both the no-conversation and conversation conditions. It is thus possible that naturalistic conversation does not impose high enough task demands, or at least overlapping task demands (Wickens, 1980; Wickens, 2002), to interfere with the top-down mechanisms involved in time-based selection (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). The preview benefit consists of two dissociable components, a set-up and a maintenance stage (Watson, Humphreys & Olivers, 2003). Importantly, Humphreys et al., (2002) used dual tasks which could be targeted on a particular component of the preview benefit. Participants were forced to engage in a focused period of attentional activity during the period that they were attempting to set up or maintain inhibition of the previewed items. However, in Experiment 1, presented here, participants were asked to converse naturally with the experimenter while performing the preview search task. In a naturalistic conversation participants can modulate the amount of attention that they pay to the task of conversing and may, therefore, be able to switch their attention between the conversation and the search task when it is beneficial to do so. It is possible then, that participants were able to shift attention back to the search task in order to set up the inhibition of the previewed items. Previous research has shown a preview duration of 400ms is required to establish an optimal preview benefit (Humphreys et al., 2004; Humphreys, Olivers & Braithwaite, 2006; Watson & Humphreys, 1997). In addition, Humphreys et al., (2002) showed that an auditory dual task did not interfere with the preview benefit if it was performed only during the maintenance component of the benefit. In fact as discussed in the introduction of this thesis, conversational structure is fairly well established with participants taking about 2 seconds each turn with only a gap of around 200ms between each turn (Levinson, 2016). As such it is not unreasonable that participants could have shifted attention back to the search task for ~400ms during the setup of inhibition and then shifted attention back to the conversation task without a noticeable lapse in conversation occurring.

That said, holding a conversation did have two important effects. The first, in line with Shinohara et al., (2010), holding a conversation resulted in an increase in overall RTs, however, unexpectedly this increase was greater in the FEB condition than in the PRE condition. One possible explanation for this is that the presentation of the preview stimuli engaged resources required for their inhibition resulting in increased arousal and hence faster engagement with search through the new stimuli when they arrived. This might then result in a smaller overall cost of holding a conversation in the preview condition compared to the FEB. An alternative account is that the act of processing the preview stimuli caused a re-allocation of resources away from the conversation task and towards visual processing. This is especially likely if inhibiting the old stimuli requires items to be detected, localized and a spatial map of them generated (Watson & Humphreys, 1997). This re-allocation of resources might, therefore, have pre-configured the visual system for subsequent search through the new items since many of the processes used in inhibiting the preview stimuli are likely to be re-used when the new stimuli arrived. In this way, inhibiting the old stimuli may have primed or instantiated processes that were then used in the search for the target. In contrast, in the FEB, allocation of resources to the visual search task might have to happen at the onset of the search display which would lead to an additional delay in the onset of search.

The second effect of holding a conversation was that search became less efficient overall as indicated by the overall decrease in search rate throughout the displays. This is in contrast to Shinohara, et al., (2010) who found that verbal tasks interfered with the visual search task, not by affecting search rate (No/Verbal Task by Display Size interaction), but by causing an upward shift in reaction times. A possible explanation for this contrasting finding is that despite using a variety of verbal tasks similar to conversation, naturalistic conversation was not one of the tasks that Shinohara et al., (2010) had participants perform whilst searching. The tasks that Shinohara et al., (2010) used were: an explanation task where the participant was required to answer 20 questions, asked by the experimenter, relating to the operation of the in car SATNAV system; a listening task where the participant simply listened to an audio clip; and finally a recall task where participants answered questions relating to audio clips they had previously listened to. The listening and recall tasks were performed in the same block and recall occurred immediately after each audio clip ended. Also of note was that search was relatively inefficient in Shinohara et al's (2010) study with search rates of between 58.12ms/item and 81.40ms/item when a target was present. This may be due to the fact that their search task was particularly effortful, searching for a complete circle in amongst a display of incomplete circles, in comparison to the search task used in Experiment 1, reported in this chapter. It is possible that the search task that Shinohara et al., (2010) used was so effortful that search efficiency was already very low and as such any slowing of search efficiency bought about by the verbal dual tasks was not able to be detected as it was, relative to the difficulty of the search task, not causing a large detriment in performance.

In Experiment 1, the preview duration was fixed at the typically used, conservative value of 1000ms, previous work has shown that a preview duration of approximately 400ms or longer is required to obtain an optimal preview benefit (Humphreys et al., 2004; Humphreys, Olivers & Braithwaite, 2006; Watson & Humphreys, 1997). It is possible that 1000ms was sufficient time for participants to re-allocate resources from the task of conversing to the task of visual marking which then led to efficient selection. To explore this possibility in Experiments 2 and 3 I assess the influence of conversation on time-based selection when shorter preview durations are used.

EXPERIMENT 2

It is possible that inhibiting old stimuli takes longer when holding a conversation, but that the 1000ms preview duration used in Experiment 1 was sufficiently long to mask this effect (see also Blagrove & Watson, 2010, for a related finding with face stimuli). It is also possible that participants had ample enough time (1000ms) in which to allocate their attentional resources to process the preview items and then switch their attention back to the task of holding a conversation. Accordingly, the main aim of Experiment 2 was to examine the effect of holding a conversation on time-based selection in conditions in which there was less time available (250, 500 or 750ms) to inhibit old, irrelevant stimuli.

Method

Participants

Twenty participants (Male = 10, Mean Age = 23.2) comprising University of Warwick non-academic staff and students took part for payment (£5) or course credit. Participants 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 1. Stimuli consisted of a blue H target with green H and blue A distractors and were presented on a Hanns-G LCD monitor with a resolution set at 1440x900 and 60hz, attached to a i3 RM computer system. The naturalistic conversation was held over SKYPE which was set up on a hands-free mobile phone. The mobile phone was positioned next to the participant so that they could easily and clearly hear what the experimenter was saying and any responses were clearly picked up by the phone's microphone. The experimenter received the call, in an adjacent experimental cubicle.

As stimuli were presented on a slightly different sized monitor in Experiment 2 and 3 the visual angle of the stimuli varied slightly from those presented in Experiment 1. The Fixation dot occupied 0.2° of visual angle both horizontally and vertically. The H and A stimuli occupied 0.7° vertically and 0.65° horizontally. As in Experiment 1, the stimuli were displayed within a 6 × 6 grid structure. Each cell in this grid occupied 3.1 degrees of visual angle horizontally and vertically. Therefore, the grid encompassed 18.6° of visual angle vertically and horizontally.

Design and Procedure

As in Experiment 1, participants performed FEB and PRE search tasks in both conversation and no-conversation conditions. However, within a preview block, the preview duration varied between three different values (250, 500 and 750ms). In order to keep the total number of trials approximately comparable to Experiment 1, two display sizes were used (4 and 12) rather than three. This resulted in a 2 (conversation or 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 across participants. 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

Reaction times

As in Experiment 1, only trials where a target was present were considered and the data were cleaned prior to the main RT analysis. Trials in which the participant made an incorrect response (1.26%), and RTs < 200ms (0.1%) were removed. Following this, means and SDs for each cell of the design were calculated for each participant and RTs falling further than 3SDs from this mean were discarded (1.7% of the remaining trials).

The resulting RTs were initially analysed using a 2 (Conversation: Conversation, No Conversation) × 4 (Presentation Condition: FEB, PRE_{250}, PRE_{500}, PRE_{750}) × 2 (Display Size: 4, 12) within-subject ANOVA. This revealed significant main effects of conversation, $F(1,19)= 22.3$, $MSE = 134878$, $p < .001$, $n^2=.540$, presentation condition, $F(1.56, 29.6)= 28.8$, $MSE = 19807$, $p < .001$, $n^2=.603$, and display size, $F(1,19)=210.4$, $MSE = 22319$, $p < .001$, $n^2=.917$ (Figure 2.3 illustrates these three main effects). There was also a significant Presentation Condition × Display Size interaction, $F(1.66,31.6)= 26.1$, $MSE = 8723$, $p <.001$, $n^2=.578$. No other significant interactions were found (Fs =< 1.51, ps >= .235). As shown in Figure 2.4, search slopes decreased as the preview duration increased and search was least efficient in the FEB condition.

Based on the hypothesis generated from Experiment 1, the effect of preview duration was examined using planned comparisons, a 2(Conversation: Conversation, No Conversation) × 3(Preview Duration: 250, 500, 750) × 2 (Display Size: 4, 12) repeated measures ANOVA was performed. All three main effects were significant, Conversation, $F(1,19)=23.5$, $MSE = 94619$, $p < .001$, $n^2 = .553$, Preview Duration, $F(1.41, 26.7) = 16.87$, $MSE = 6250.5$, $p < .001$, $n^2 = .470$, Display Size, $F(1,19) =$

130.5, MSE = 19077, $p < .001$, $n^2 = .873$. There was also a significant Preview Duration × Display Size interaction, $F(1.42, 26.98) = 3.82$, MSE =3593, $p = .048$, $n^2 = .167$, indicating that the size of the preview benefit decreased as the preview duration decreased with resulting preview slopes of 22.7, 25.6, and 28.2 ms/item for preview durations of 750, 500 and 250 ms, respectively. No other interactions were significant (All Fs =< 0.27). 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}, $F(1,19)= 50.6$, MSE = 4695, $p < .001$, $n^2 = .727$, FEB vs PRE_{500}, $F(1,19)=25.89$, MSE=6784, $p < .001$, n^2=.577, FEB vs PRE_{750}, $F(1,19)=31.81$, MSE = 9812, $p < .001$, n^2 =.626).

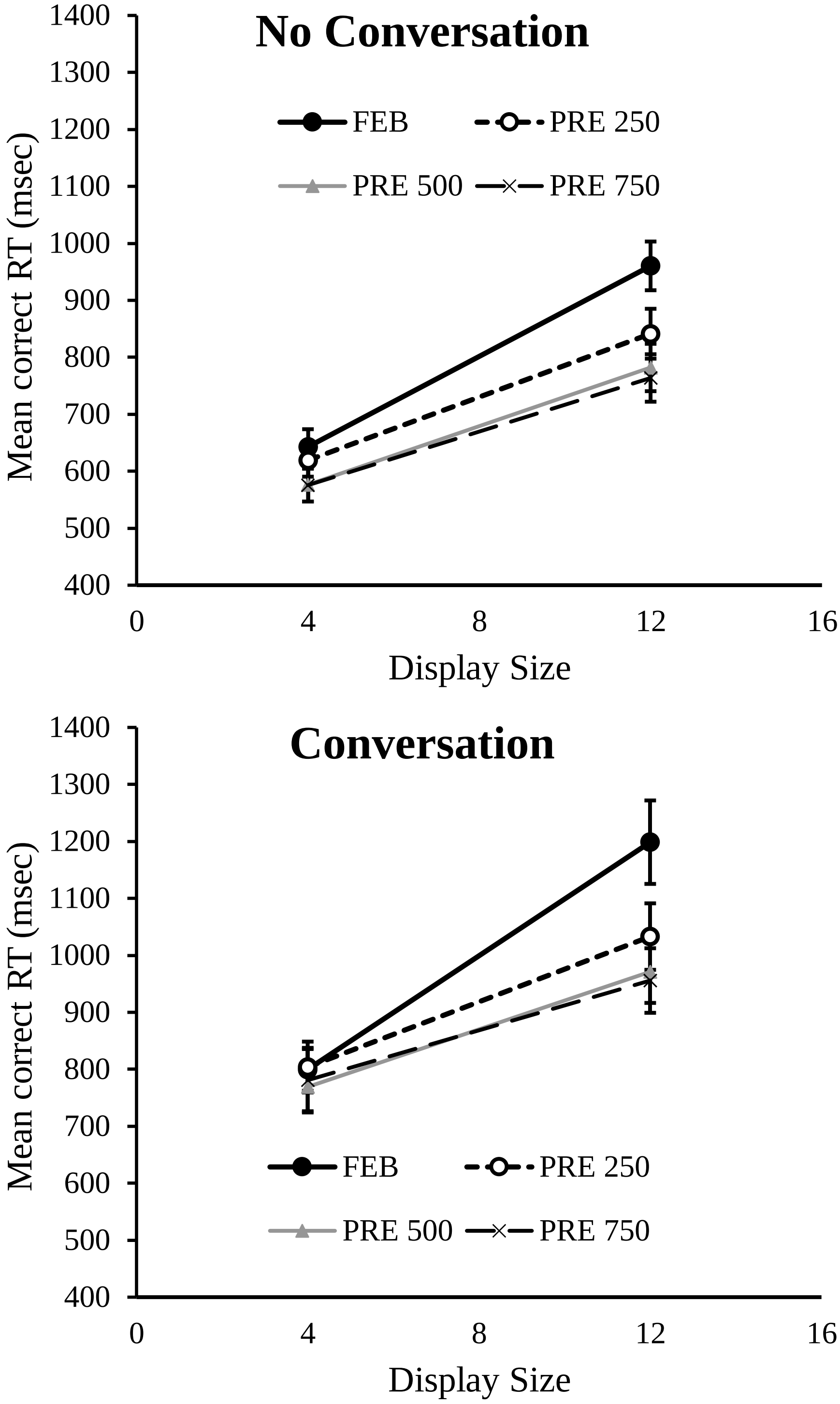

Figure 2.3 – Mean correct RTs as a function of the presentation, display size and conversation conditions. Error bars show ±1SE.

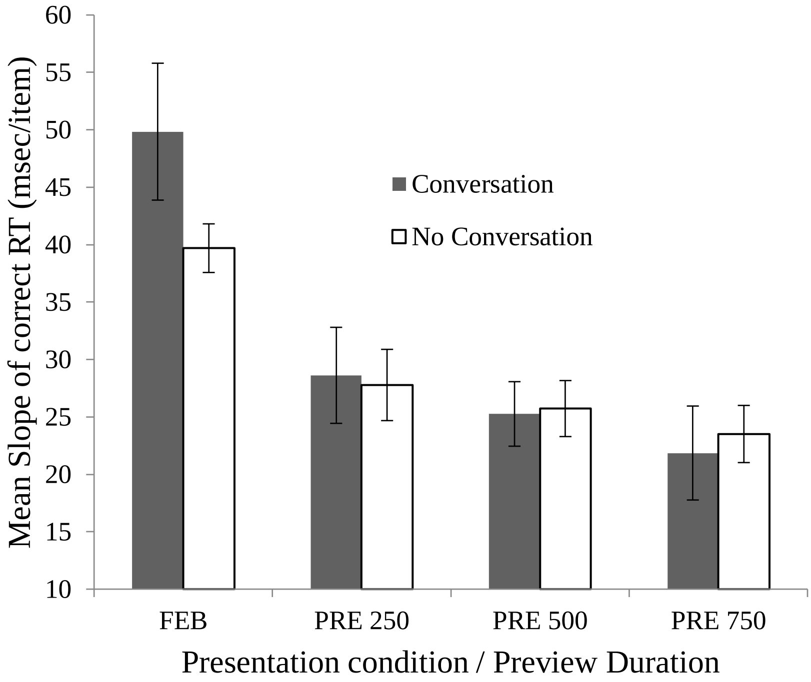

Figure 2.4 – Mean slopes of correct reaction times as a function of the presentation and conversation conditions. Error bars show ±1SE.

Errors

The overall error rate on search trials was extremely low (0.8%) and these data were not analysed further (Table B.2, B.3). The catch trial error rate was also low (3.8%) suggesting that participants had searched both sides of the display.

Discussion

Three main findings emerged from Experiment 2. 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 (see also – Humphreys et al., 2004; Humphreys, Olivers & Braithwaite, 2006; Warner & Jackson, 2009; Watson & Humphreys, 1997). Consistent with this, here I found a gradual decrease in preview search efficiency as the preview duration decreased from 750ms to 250ms, although a robust preview benefit was still present even at the 250ms preview duration. The second finding was that, as in Experiment 1, conversation produced an overall increase in participants' reaction times. Finally, the third finding was that even with reduced preview durations there was no evidence that conversation had a disruptive effect on time-based selection. Given that there was still a robust preview benefit even at the shortest preview duration, it is possible that the preview display was again able to refocus participant's attention on the task of inhibiting the old stimuli and away from the conversation. As in Experiment 1, this would lead to relatively efficient preview search. In Experiment 3 I test further the boundary limits of conversation/time-based selection by reducing the preview duration even more which should make it more difficult to re-allocate attentional resources.

EXPERIMENT 3

The main aim of Experiment 3 was to assess the influence of conversation on time based selection when the opportunity to inhibit old/previewed stimuli was reduced to minimal durations of 75, 150 and 250ms.

Method

Participants

Twenty University of Warwick staff and students (male = 12, mean age = 22.7) took part. Warwick non-academic staff and students took part for payment (£5) or course credit. Participants 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 or equivalent to those used in Experiment 2. However, in Experiment 3 the hands-free conversation took place via a laptop rather than a mobile phone.

Design and Procedure

The design and procedure were identical to Experiment 2, except that preview durations of 75, 150 and 250ms were used in place of 250, 500 and 750ms.

Results

Reaction times

The overall mean RT of one participant was 3.7 SDs away from the overall mean of all participants and so this participant was excluded leaving data from 19 participants available for analysis. As in Experiments 1 and 2, for the main RT analysis, target absent trials and trials in which the participant made an incorrect response (1.7%), and RTs < 200ms (0.04%) were removed. Following this, means and

SDs for each cell of the design were calculated for each participant and RTs falling further than 3SDs from this mean were discarded (1.3% of the remaining trials).

The resulting mean RTs were initially analysed using a 2 (Conversation: Conversation, No Conversation) × 4 (Presentation Condition: FEB, PRE_{75}, PRE_{150}, PRE_{250}) × 2 (Display Size: 4, 12) within-subject ANOVA. This revealed significant main effects of conversation, $F(1,18)=17.41$, $MSE= 343857$, $p<.001$, $n^2=.492$, and display size, $F(1,18)=154.8$, $MSE = 49075$, $p < .001$, $n^2=.896$. There was also a significant Conversation × Display Size interaction, $F(1,18)= 5.89$, $MSE = 19523$, $p=.026$, $n^2=.247$). As shown in Figure 2.5, 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, $F(1.61, 29.04)= 1.69$, $MSE = 22146$, $p = .206$, $n^2=.086$, nor the Presentation Condition × Display Size interaction, $F(3,54)= 1.48$, $MSE = 10576$, $p = .231$, $n^2=.076$, nor the Presentation Condition × Conversation interaction, $F(1.51, 27.14)= 0.37$, $MSE = 19033$, $p = .633$, $n^2 = .02$, were significant. The three way interaction was also not significant, $F(3,54)= 1.34$, $MSE = 8285$, $p = .271$, $n^2 = .069$ (see Figure 2.5 for mean RTs as a function of the conversation, presentation and display size conditions and Figure 2.6 for mean slopes of RTs as a function of the presentation and conversation conditions) .

Based on the findings of Experiment 2, I next examined the potential effect of preview duration using planned comparisons, therefore I performed a 2(Conversation: Conversation, No Conversation) × 3(Preview Duration: 75, 150, 250) × 2 (Display Size: 4, 12) repeated measures ANOVA. Significant main effects of conversation $F(1,18)= 18.25$, $MSE = 237188$, $p < .001$, $n^2 = .503$, and display size $F(1,18) =$

157.80, *MSE* = 33768, $p < .001$, $n^2 = .898$ were found. However, a significant main effect of preview duration $F(2,36)= 0.56$, *MSE* =4200, $p =.577$, $n^2 = .030$ was not found. There was also a significant Conversation × Display Size interaction, $F(1,18)= 5.03$, *MSE* = 15145, $p < .038$, $n^2 = .218$. However, no other interactions approached significance, all *F*s < 1.

When compared individually with the FEB, there was not a reliable preview benefit at any of the preview durations (FEB vs PRE_{75}, $F(1,18) = 1.63$, *MSE* = 7711, $p = .218$, $n^2 = .083$, FEB vs PRE_{150}, $F(1,18)=3.32$, *MSE* = 13201, $p = .085$, $n^2 = .156$, FEB vs PRE_{250}, $F(1,18) = 0.89$, *MSE* = 4414, $p = .358$, $n^2 = .047$), neither were there any significant three-way Conversation × Presentation Condition × Display Size interactions (FEB vs PRE_{75}, $F(1,18)= 0.73$, *MSE* = 5677, $p = .403$, $n^2 = .039$, FEB vs PRE_{150}, $F(1,18)=1.65$, *MSE* = 0.22, $p = .215$, $n^2 = .084$, FEB vs PRE_{250}, $F(1,18)= 0.20$, *MSE* = 7143, $p = .657$, $n^2 = .011$).

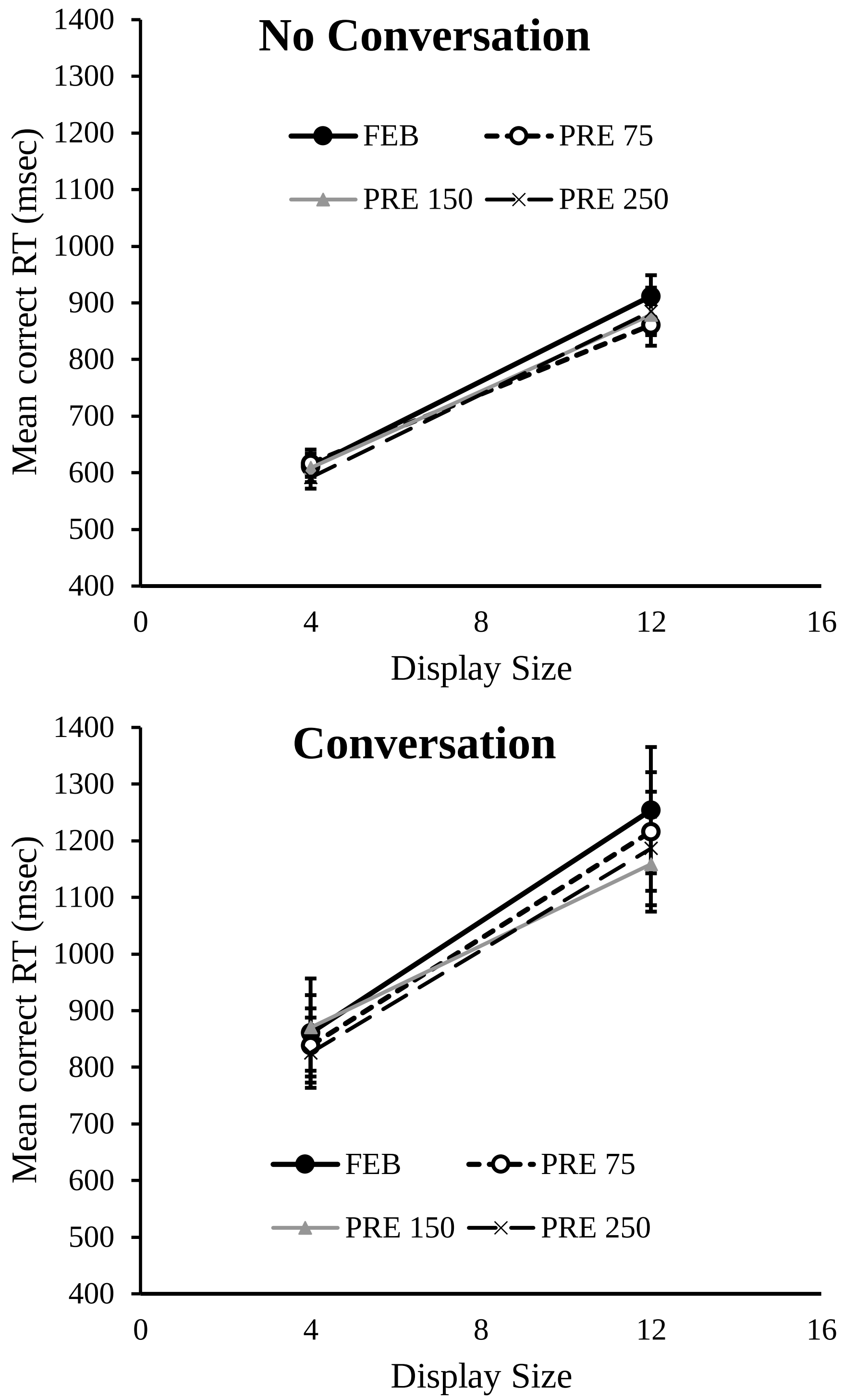

Figure 2.5 - Mean correct RTs as a function of the presentation, display size and conversation conditions. Error bars show ±1SE.

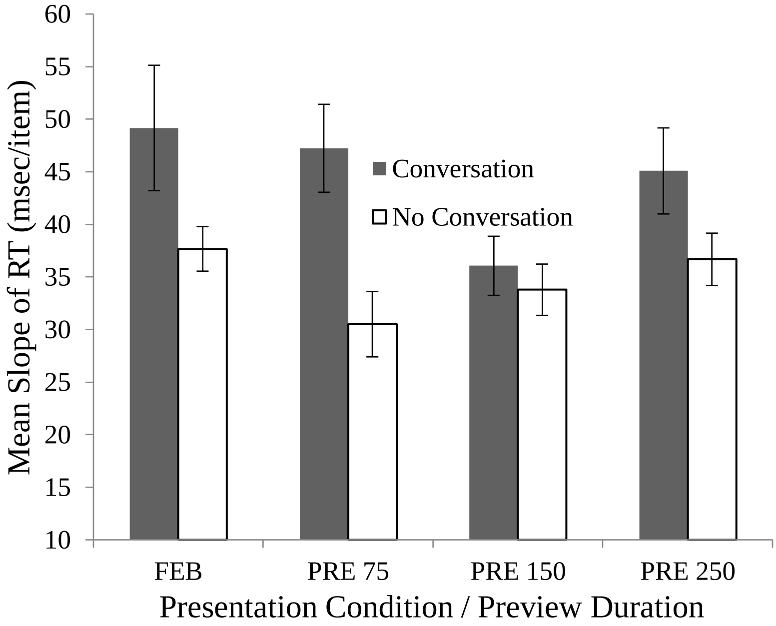

Figure 2.6 - Mean slopes of correct reaction times as a function of the presentation and conversation conditions. Error bars show ±1SE.

Errors

The overall error rate on search trials was 1.1% and was not analysed further (Table B.4, B.5). The catch trial error rate was also relatively low (5.2%) suggesting that participants were searching the full display.

Discussion

The main aim of Experiment 3 was to assess the effect of conversation on time-based selection when the opportunity to inhibit old items was severely shortened. However, surprisingly, 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. A robust preview benefit was found in Experiment 2 when the preview duration was set at 250ms. However, surprisingly, in Experiment 3 a preview benefit was not found even when the preview duration was set at 250ms. In fact a preview benefit was not found in any of the preview conditions in Experiment 3. Hence, there was in fact no time-based selection for the conversation load to disrupt. Nonetheless, a robust effect of conversation on both 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.

The overall effect of conversation on visual search

In order to further determine the overall effect of holding a conversation on the rate of visual search, I calculated and combined the search rates from the FEB conditions of all three experiments[1]. The data deviated significantly from normality and so the Wilcoxon Signed Rank test was performed. This revealed that the median search rate was 24.1% less efficient when a conversation was being held (56.1 ms/item) than when no conversation was held (45.2 ms/item), $Z = 3.567$, $p < .001$, $r = .44$, a difference of 10.95ms/item (95% CI[3.76, 11.60]).

General Discussion

The results of the three experiments reported above indicate, perhaps surprisingly, that naturalistic conversation does not impact upon one's ability to benefit when some distracters are previewed prior to the presentation of further distracters and a target item.

Given that the preview benefit is hypothesised to be driven by a top down resource and memory dependent process (Watson & Humphreys, 1997) and that it has been demonstrated that additional load tasks can attenuate and abolish the preview benefit (Humphreys, Watson & Jolicoeur, 2002), it was hypothesized that conversation would cause an attenuation of the preview benefit. However, at a preview duration of 1000ms it did not appear to do so. In fact, even when the preview duration was reduced, increasing the difficulty of the task, I found no effect of conversation on the preview benefit. Therefore, it appears as though, at least in these experiments, that the preview benefit is not affected by dual task naturalistic conversation.

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[1] We calculated and analyzed 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 1-3.

At first glance it may appear that the results presented here 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 (see e.g., Watson, Humphreys & Olivers, 2003, for a review and the literature discussed earlier in this chapter) points to, at least in part, a top down component to the preview benefit. Among this literature is research showing that under certain conditions it is possible for a dual task to influence the benefit (Humphreys, Watson & Jolicoeur, 2002).

Humphreys Watson and Jolicoeur (2002) demonstrated that an auditory dual task has the potential to interfere with the preview benefit. However, I was not able to find any evidence to suggest that naturalistic conversation can cause such an impairment. However there are several key differences between the current work and the study reported by Humphreys Watson and Jolicoeur (2002). Firstly, I used naturalistic conversation whereas Humphreys et al., (2002) had participants monitor an auditory stream of digits for a target. Therefore, it is possible that this monitoring task required a greater amount of resources or resources which overlapped to a greater extent with those required to perform visual marking, than did naturalistic conversation. In addition, Humphreys et al., (2002) targeted the auditory task so that it occurred during the preview duration. Whereas naturalistic conversation did not allow for this level of control, by its very nature it was flowing, back and forth, which would have required varying amounts of effort from the participant to maintain and respond appropriately. Humphreys et al., (2002) showed that the preview benefit can be split into two components, a set up phase and a maintenance phase. Of note is that only the set up phase was found to be affected by an auditory dual task. Therefore, it is conceivable that the participants were able to divert their attention away from the conversation, so as to adequately encode the preview items, and then switch back to the conversation once inhibition had been set up. These key differences may account for the discrepancy in findings between Experiments 1-3 and the work by Humphreys Watson and Jolicoeur (2002).

In addition, the work in this chapter has demonstrated that aside from increasing reaction times to the target item, participants are also less efficient when searching under dual task conditions, as indicated by the increase in search slopes in the conversation condition. This is in contrast to the recent work of Shinohara et al., (2010) who found that reaction times were inflated in a search task performed in conjunction with a conversational task, but that search efficiency, as measured by the rate at which reaction times rose as display size increased, did not. These findings will all be discussed below.

Strategic Search

Also of interest is the fact that the bottom up attention capture explanation of the preview benefit cannot adequately explain all of the data from Experiments 1-3. Of key importance, a preview benefit was found in Experiment 2 when the preview duration was set as low as 250ms, however in Experiment 3 when the preview duration was also set at 250ms, a preview benefit was not found. I interpret this as evidence of the strategic deployment of visual marking when it appears efficient to employ it. A bottom up account of the preview benefit would expect that given an adequate time interval for the benefit to occur, that 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 2 and 3 when the preview duration was set at 250ms.

The question as to whether attentional mechanisms, such as visual marking, can be applied strategically or are always applied by default has been previously studied (e.g. Watson & Humphreys, 2000; Zupan, Watson & Blagrove, 2015). Aspects of the visual task have been shown to manipulate participants' search strategy. Participants have been shown to adopt a singleton search strategy when targets are consistently unique in a display (Bacon & Egeth, 1994, Leber, & Egeth, 2006). Whereas, when a target shares features with other items in the display, a feature-based strategy is reliably adopted. In the context of visual marking, previous literature has shown conflicting results on this point. Watson and Humphreys (2000) argue that visual marking can be flexibly applied depending on the particular goals of the search task at hand. When participants are asked to find a probe in a classic visual marking display, they are impaired in doing so when the probe appears at the location of an old, previewed stimuli. However, this is only the case when on the majority of trials, they are asked to simply search for a target in a preview search task and only in a minority of cases are they asked to detect a probe. However, when all trials are probe detection trials, participants do not show an impairment in detecting probes when they appear on 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.

However recent research by Zupan, Watson and Blagrove (2015) used a different methodology to test the strategic nature of visual marking. In their design, old, previewed items, "jump" around the display rather than staying static over the course of the preview duration. Results showed that participants uniformly applied marking in difficult search contexts regardless of the efficacy of its application. That is to say marking was applied in trials where it would not be beneficial and even when participants were explicitly made aware of how ineffective marking would be at the beginning of each trial. However, it must also be noted that when search was relatively easy, for example when the target was “obvious”, modest evidence was found that marking was being strategically applied. In summary, they suggest that when the target is highly salient in a majority of trials participants are less likely to apply inhibition, even on the minority of trials where it would be useful. It would appear then 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 previously noted, the data from Experiments 2 and 3 show conflicting results when the preview duration was set at 250ms. One possible explanation of this could be that visual marking was strategically applied in Experiment 2, but not in Experiment 3. The preview duration in Experiment 3 was set so low that participants did not have adequate time to visually mark in 66% of preview trials (when the preview duration was set at 75 or 150ms). Therefore, because visual marking was little to no use in the majority of trials, it is possible a search strategy was adopted which did not incorporate visual marking. As such, even when visual marking would have been a valid strategy in 250ms trials, it was not utilized. However, in Experiment 2 visual marking was a valid strategy to adopt throughout all trials especially when preview durations were longer, e.g. 500, 750ms. Therefore, in this experiment, visual marking was strategically applied across all trials even at 250ms. Further research would be required to validate this interpretation, however this result is interesting when framed in these terms and seems to support Watson and Humphreys’ (2000) finding that visual marking is able to be strategically applied depending on the context of the visual attention task.

The effect of conversation on search

Experiments 1 to 3 show that when the preview duration is sufficiently long (>=250ms) then a preview benefit is found even under conditions of dual task naturalistic conversation. However, what is clear from the data is that while they were conversing, participants showed an overall slowing in their ability to identify a target amongst a field of distracters.

A reaction time deficit when conversing has been demonstrated before in the literature. Shinohara et al., (2010) used verbal tasks to show that participants had significantly slower reaction times when they were performing verbal tasks. McCarley et al., (2004) demonstrated that participants took significantly longer to locate a change in a change blindness paradigm while they were conversing. What is interesting however is that the data from Experiment 1 indicated that reaction times in the PRE and FEB conditions were affected differently by naturalistic conversation. As previously discussed, explanations for this could be that the onset of the preview items caused an increase in arousal or that they allowed for attentional resources to be primed and preallocated in the preview condition. As such, participants may have been able to draw resources away from the conversation in order to better perform on the search task or were simply in a heightened state of attention and, therefore, able to complete the task efficiently. However, Experiments 2 and 3 were not able to replicate this finding and in fact found no difference in how naturalistic conversation impacted on reaction times in the PRE and FEB conditions.

Overall, the results of Experiments 1-3 clearly demonstrate that reaction times in a visual search task are significantly negatively affected by naturalistic conversation. Moreover, our data also point to an effect of naturalistic conversation on participants' search efficiency; participants searched the visual scene less efficiently when they were conversing. Therefore, I can predict that increasing the display size will have a greater effect on participants' performance while they are conversing than when they are not. Experiment 1 provided evidence suggesting that holding a naturalistic conversation impacted upon the rate at which participants were able to search through a display. I followed up this finding by combining the FEB conditions from Experiments 1-3 and performed a Wilcoxon Signed Rank test to compare the search rates across the conversation conditions. The results of which confirmed that search rate was being affected by naturalistic conversation.

This finding, that search rates were affected by naturalistic conversation, contrasts with work by Shinohara, et al., (2010) who demonstrated that verbal tasks, performed concurrently to a visual search task, did not affect the rate at which participants searched through a display. An explanation for this may be that when search is very inefficient as in Shinohara et al., (2010), indicated by the steep search slopes, then any effect on search rate that the verbal task may cause may be too small to be detected. This explanation is supported by the fact that both the verbal tasks (recalling and explaining) used by Shinohara et al., (2010) had numerically, but not significantly, slower search rates when compared to the control condition. However, in our study search was somewhat more efficient and as such the effect of conversation on participants' search may have been more easily discernible. It may also be the case that the tasks used by Shinohara et al., (2010) were not sufficiently similar to naturalistic conversation to have the same effect on search performance as was found in the current work (Experiments 1-3). Indeed Shinohara et al., (2010) suggest that the deficit in performance associated with the verbal task may be due to task switching. The verbal task used in the current work was specifically designed to be as naturalistic as possible. Relatively constant monitoring of the conversation would have been required in order for the participants to respond appropriately and, as such, may not have allowed participants to switch between focusing on the conversation and the visual task as readily as the verbal tasks used by Shinohara et al., (2010).

Another possible explanation of our result is that whilst conversing participants are not able to, as efficiently, inhibit their return to items in the display which they have previously searched (for a review see, Klein, 2000). If this is the case then they may be exhibiting a more erratic search pattern where they are re-examining items which they have previously classified as not being a target. This would act to decrease their search efficiency on each trial and would also explain why the deficit in their search performance increases as more items are added to the display. However, it should be noted that previous work has found that visual spatial working memory tasks can eliminate IOR, but other tasks despite involving memory and attention did not (Castel, Pratt & Craik, 2003).

In any case, it is clear from the work of Chapter 2 that naturalistic conversation does not appear to attenuate participants' ability to experience a preview benefit. However, an overall effect of conversation on search was found, namely an increase in reaction times and a decrease in search efficiency. Therefore, it is clear that at some level naturalistic conversation is impacting visual attention. With that in mind, in Chapter 3 I continue my exploration of how memory in visual search may be affected by dual task naturalistic conversation by turning to a measure of implicit learning and memory expression in visual attention, the contextual cueing paradigm (Chun & Jiang, 1998).

Chapter 3:

Establishing the effect of naturalistic conversation on implicit memory in visual search.

Memory of visual scenes is a key facilitator of visual search (Chun & Jiang, 1998). It can be implicit, outside of awareness, or explicit where we actively try to remember information and are conscious of doing so (see Strayer, Drews & Johnston, 2003 for examples of experiments examining these types of memory). Implicit processes have been shown to play a role in the driving task, for example Lewis-Evans and Charlton (2006) showed that while participants did not explicitly notice that a road had been narrowed, they had implicitly processed this change as measured by their behavioural adaptation, driving slower, and their increased ratings of the risk. In addition, an increasing body of literature speaks to the role that implicit memory plays within the control of visual attention (eg, Chun & Jiang, 1998; Johnson, Woodman, Braun & Luck 2007). Implicit memory has been shown to be able to guide spatial attention (Chun & Jiang, 1998), and influence the adaptive deployment of attention across scenes, aiding in the effective processing of a scene and its key details (Chun & Nakayama, 2000). Given this, and the fact that visual attention is a key component of the driving task (Shinohara et al., 2010), it is important to investigate and understand how implicit learning and memory may be affected by a concurrent task which drivers are likely to undertake whilst driving, namely talking on a mobile phone.

One way to accomplish this is to use the contextual cueing task which is specifically designed to investigate participants' implicit learning and expression of spatial contexts (Chun & Jiang, 1998). This task is usually split into two distinct phases: a learning phase and a test phase. In the learning phase participants are exposed to a series of displays and must find and respond to a target in each display. Unbeknownst to participants, some displays are presented many times so that they may begin to implicitly learn them. Next there is a test phase in which participants see the previously learnt displays and other completely novel displays. These displays are usually presented several times and in a random order. Typically, the old, learnt displays are responded to more quickly than the novel displays (Chun & Jiang, 1998). By comparing reaction times to these old and new trials we can see if participants have successfully learnt and then expressed the repeated displays.

Contextual cueing (Chun & Jiang, 1998), it is hypothesised, is the implicit learning of repeated displays so that at a later point they may act as spatial cues which expedite the search time for those specific displays. The question, is contextual cueing affected by increased task demands and the splitting of attention, has recently received attention in the academic literature. For example, Vickery, Sussman and Jiang (2010) investigated this and manipulated participants' visual working memory (VWM) load during a contextual cueing task. VWM was loaded using a variety of techniques including dot patterns, and multiple potential targets. This load was applied during the training phase of the experiment. The test phase contained no additional tasks. Search was facilitated by repeated exposure to spatial contexts in both the control group (no VWM load group) and the VWM load group. There was no effect of increasing load, suggesting that implicit learning of spatial contexts is not affected by VWM load. However, as no load manipulations were made during the test phase of the experiment, this study did not test the effects of VWM load on the recall and utilisation of the learnt spatial contexts.

Manginelli, Langer, Klose and Pollmann, (2013) took these findings a step further and tested whether a specific form of VWM, visual spatial working memory had an impact on contextual cueing. Of key importance was that they tested the effects of load in both the training and the test phase of contextual cueing. Participants completed a standard contextual cueing paradigm. In addition, participants also completed a visuo-spatial or visuo-non-spatial working memory task. Participants were presented with a working memory display which they were required to memorise before viewing each search display. After searching for the target in the search display, the participants were then required to perform a memory test. Visuo-spatial working memory was loaded in two different ways. At the beginning of a trial four black squares were presented to participants, the positions of these squares were chosen at random from 8 possible locations (the 8 positions were arranged in an imaginary circle). In the memory test, after the search display had been presented, participants were presented with a single black square and asked if it occupied the same location as any of the black squares they had seen at the beginning of the trial. Visuo-spatial working memory was also loaded using Gabor patches. Participants were shown two Gabor patches which each could be oriented by either 0, 45, 90 or 135 degrees. After searching, participants were shown a single patch and asked if its orientation matched either of the previously displayed patches. Visuo-non-spatial working memory was loaded using a colour memory task and a Klingon letter task (for another example of the use of a Klingon letter task see, Mecklinger, Bosch, Gruenewald, Bentin, & von Cramon, 2000). In the colour memory task participants were presented with 3 differently coloured squares arranged in the same way as the grey squares were in the visuo spatial task. Then after searching, participants were given a single coloured square, presented at fixation, and asked if its colour matched any of the previously viewed squares. The Klingon letter task consisted of an initial display of two artificial letters which could not be easily reconciled with the Latin alphabet. Later participants were presented with a single letter and asked if it matched either of the two letters they were given to memorise.

Manginelli et al., (2013) tested the effects of both visuo-spatial and visuo-non-spatial working memory load in both the training and the test phase of contextual cueing. Their results showed that contextual cueing was immune to visuo-nonspatial working memory load, finding no effect in either the training or test phase. Visuo-spatial working memory load, on the other hand, did impact upon participants' ability to search old displays more efficiently than new displays. However, an effect was only found when the visuo-spatial working memory task was performed in the test phase. This suggests that visuo-spatial working memory is not required for the learning of spatial contexts, but is necessary for the expression of these contexts.

Annac et al., (2013) made the case that the reduction in contextual cueing due to dual task visuo-spatial WM load may not necessarily be due to the loading of specific spatial resources, but may be due to a central executive bottleneck. Through a series of experiments they attempted to disentangle these two possibilities and concluded that the expression of learned spatial information in contextual cueing is attenuated by spatial WM resources and not by executive WM load.

Thus, there is a growing body of evidence suggesting that the expression of learnt spatial contexts can be inhibited by WM load, particularly spatial WM load, but that the learning of spatial contexts remains unaffected. However, research by Travis, Mattingley and Dux (2013) may challenge these findings. Travis et al., (2013) conducted several experiments in which participants were asked to complete a contextual cueing task while concurrently performing a spatial-WM task. The participants were subjected to one of three levels of WM load (No load, Low load, High Load) whilst they completed the training phase of a contextual cueing task. Their results showed that spatial-WM load can also negatively impact upon the learning of spatial contexts.

As it seems possible for an additional task to apply a high enough load to impact upon implicit learning and memory, it cannot, therefore, be assumed that contextual cueing will be immune to other more generalizable forms of dual task interference. Additionally, there are two distinct phases of contextual cueing which may be affected by this dual task interference and the above research shows varied results as to which might or not might not be affected by different loads. For example, Vickery, Sussman and Jiang (2010) showed that VWM load has no effect on participants' ability to learn spatial contexts, however they did not study the test phase of contextual cueing. Whereas, Travis et al., (2013) were able to find an effect of load on the learning of spatial contexts. In addition, Manginelli, Langer, Klose and Pollmann, (2013) and Annac et al., (2013) demonstrated that by increasing visuo-spatial working memory load in the test phase, the expression of old spatial contexts was interrupted. While these studies used specific tasks targeted at attention in different ways, using an overall more externally generaliseable task, such as naturalistic conversation, would provide a valuable insight into visual detriments in the real world and ways to overcome them. This is especially poignant as there is precedent for an effect of naturalistic conversation on an implicit task.

Strayer, Drews and Johnston, (2003) demonstrated that it is possible for naturalistic conversation to impact upon implicit memory of words. The work presented in Chapter 3 aimed to build on this research and explored whether naturalistic conversation was able to impact upon participants' ability to implicitly learn and express spatial contexts. It should also be noted that the visuo WM tasks outlined above required that participants process the WM stimuli and then hold them in memory until after the search trial. These tasks were likely to be effective at loading memory resources, but did not require constant monitoring or processing during the search trial. Whereas, a task such as naturalistic conversation may not specifically load spatial WM resources, but does require almost constant monitoring and processing from the participants while they are concurrently trying to search the display. Therefore, participants were required to switch their attention between the conversation and the search task. This is a key difference to those studies which loaded WM resources while participants performed the contextual cueing task. If sufficient resources are required to monitor the conversation, generate appropriate responses and switch between the two tasks then an effect on contextual cuing may be observed, despite the fact that conversation is not loading visuo-spatial WM.

In this Chapter I present three experiments which aim to investigate the effect of naturalistic conversation on participants' ability to implicitly learn and then express spatial contexts. In Experiment 1 I focused on participants' ability to learn spatial contexts. As such, participants were required to hold a naturalistic conversation with the experimenter during the training phase of the experiment. The aim of Experiment 2 was to investigate the effect of conversation on the expression of learnt spatial contexts, therefore participants held the conversation during the test phase. Experiment 3 had two aims, the first was to investigate if increasing the difficulty of the contextual cueing task would lead to greater interference from the naturalistic conversation. The second aim was to establish if holding a conversation in both the training and the test phase of the experiment impacted upon the contextual cueing effect observed.

EXPERIMENT 1

As previously discussed (Annac et al., 2013; Manginelli, Langer, Klose and Pollmann, 2013; Vickery, Sussman and Jiang 2010; Travis et al., 2013), there is conflicting evidence as to whether an additional load task, when completed during the training phase of a contextual cueing experiment, can influence participants' ability to learn spatial contexts. Therefore, it cannot be assumed that participants' learning of spatial contexts will not be affected by a more naturalistic task, such as conversation. Experiment 1 was designed to examine this.

Method

Participants

Sixteen University of Warwick non-academic staff and students took part in this experiment (4 male, Mean Age = 19.7). Participants confirmed that they could easily hear the experimenter in the conversation condition and that they could see the visual display. I based the sample size for Experiments 1-3 on previous studies which found a robust contextual cueing effect (Chun & Jiang, 1998; Kunar, Flusberg, Horowitz & Wolfe, 2007) and investigated contextual cueing specifically under dual task conditions (Manginelli, Langer, Klose, & Pollmann, 2013; Vickery, Sussman and Jiang, 2010).

Stimuli and Apparatus

Displays were generated and displayed on a Sony CRT 45cm monitor with a resolution set at 800x600 pixels. The participant sat approximately 57cm from the monitor. The experiment ran on an IBM compatible PC. Participants responded using a standard QWERTY keyboard.

The conversation condition required the use of two hands-free phones. These were fitted with an internal speaker and the volume was set so that the experimenter could be heard clearly without the need for the participant to change position or listen intently. The participant's phone was positioned on the participant's left directly beside the monitor. The experimenter's phone was located in a separate laboratory room.

The participants' task was to search for a target within a visual field of 12 distracting stimuli. 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 where the horizontal stem of the T had been offset from either corner of the vertical bar. The distracters were randomly orientated in the same possible orientations as the target. Participants were required to locate and 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 occupied 1.5° of visual angle both horizontally and vertically. The stimuli consisted of lines which were 3 pixels thick (0.17°). The fixation dot occupied 0.6° both horizontally and vertically. The stimuli were arranged within a 6 × 6 cell matrix. Each cell in this matrix occupied 4.7° horizontally and 3.5° vertically. Therefore, the matrix encompassed 27.4° horizontally and 20.8° vertically. All stimuli were white and where presented on a black background.

There were two types of displays which could be presented to a participant in a trial. Old, repeated displays which the participant should implicitly learn and New displays. Every display contained 12 stimuli, one of which would be a target. These stimuli were placed within a 6 × 6 matrix, when placing the stimuli, noise was added to the location coordinates so as to ensure that the stimuli were jittered inside each cell of the matrix. Old displays were randomly generated at the beginning of the experiment and stimuli positions did not then change throughout all 10 epochs. New displays, were generated slightly differently, the target positions were defined at the beginning of the experiment however the distracters were randomly positioned on each trial. The targets orientation was randomised on every trial.

Design and Procedure

A 2 (Conversation Condition: Conversation, No Conversation) × 2 (Spatial Context: Old, New) × 10 (EPOCH: 1-10) within-subjects design was used. There were two phases: a training phase consisting of 224 trials (Epochs 1-7); a test phase consisting of 192 trials (Epochs 8-10). Each Epoch in the training phase (1-7) consisted of 32 trials in all of which Old, to be learnt spatial contexts, were presented. Epochs in the test phase consisted of 64 trials and contained both completely novel spatial contexts (New) and spatial contexts which were presented in the training phase (Old), in equal numbers (32 of each). Each Epoch consisted of two blocks of trials. In the training phase blocks consisted of 16 Old spatial context trials. As there were 4 Old contexts these were repeated in a random order 4 times within each block. In the test phase blocks consisted of 32 trials. Each block contained an equal number of each of the Old and New contexts. As there were 4 Old contexts these were each repeated twice within a test block. The remaining 16 trials in each test block consisted of New spatial contexts. Participants were asked if they would like to take a short break in between the training and test phases of the experiment. After each trial participants were told if they gave the correct response or not.

Participants completed a training phase followed by a test phase in both the conversation condition and the no conversation condition (e.g. Conversation-Training followed by Conversation-Test, then No Conversation-Training followed by No Conversation-Test). 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. In order to reduce noise in the results caused by participants improving at the contextual cueing task over time the distraction condition order was counterbalanced across participants.

As previously stated, the conversation took place over two hands-free phones. The experimenter's role in the conversation was to ensure that the conversation flowed continuously and that there were no extended periods of silence. The conversation was steered towards mundane topics such as the participants experiences of university and their subject of study. However, the experimenter allowed the participant to guide the conversation to any topic they wished as long as it was not highly emotive or distressing. For a similar methodology see Kunar et al., (2008).

Before beginning the experimental trials the participants took part in a demonstration phase and then a practice phase. The demonstration phase introduced participants to the task and consisted of 3 randomly generated trials. In two of these trials the target and distracters would be in the same place but the target would be oriented differently, thus demonstrating a repeated or Old display. The practice phase consisted of 4 old spatial contexts which were each shown three times and 4 novel spatial contexts which were each only shown once. Therefore, participants completed 16 practice trials. Completing a practice phase allowed the participants to familiarise themselves with their task.

Results

As in Chapter 2 all trials in which the participant responded in less than 200ms were removed (0 trials) next all trials in which the participant made an error were removed (1.3%). An outlier removal procedure was then performed whereby the mean and standard deviation was calculated for each participant for each cell of the experimental design. Then any reaction times which deviated by more than 3SD from the mean of their respective cell were removed. This resulted in 1.6% of the remaining trials being removed. The number of errors made by participants on trials which would have otherwise been included in the reaction time analyses detailed below was very low (1.3%) and as such the error data was not analysed further (Table C.1, C.2).

Training Phase

For each participant their mean correct reaction times on Old displays were calculated for each of the Epochs 1-7. These reaction times were then compared across the conversation condition, in order to establish whether naturalistic conversation impacted upon participants' ability to learn the Old displays. Mean correct RTs were analysed using 2(Conversation Condition: Conversation, No conversation) × 7 (Epoch: 1-7) repeated measures ANOVA. A significant difference was found between participants RTs in the conversation and the no-conversation condition $F(1,15)=15.1$, $MSE = 310667$, $p < .001$, $n = .501$, participants on average responded 288.2msec more slowly when they were conversing. A significant main effect of epoch was also found, $F(3.2,47.5)=19.9$, $MSE = 44436$, $p < .001$, $n = .570$. As the participants progressed through the epochs they reacted faster to the stimuli, suggesting that either, spatial context learning was taking place or that participants were becoming more efficient at performing the task (the effect of epoch and the difference caused by conversation is shown in Figure 3.1). However, the Conversation × Epoch interaction did not approach significance, $F(2.0, 30.2)=0.244$, $MSE = 106441$, $p = .787$, $n = .016$.

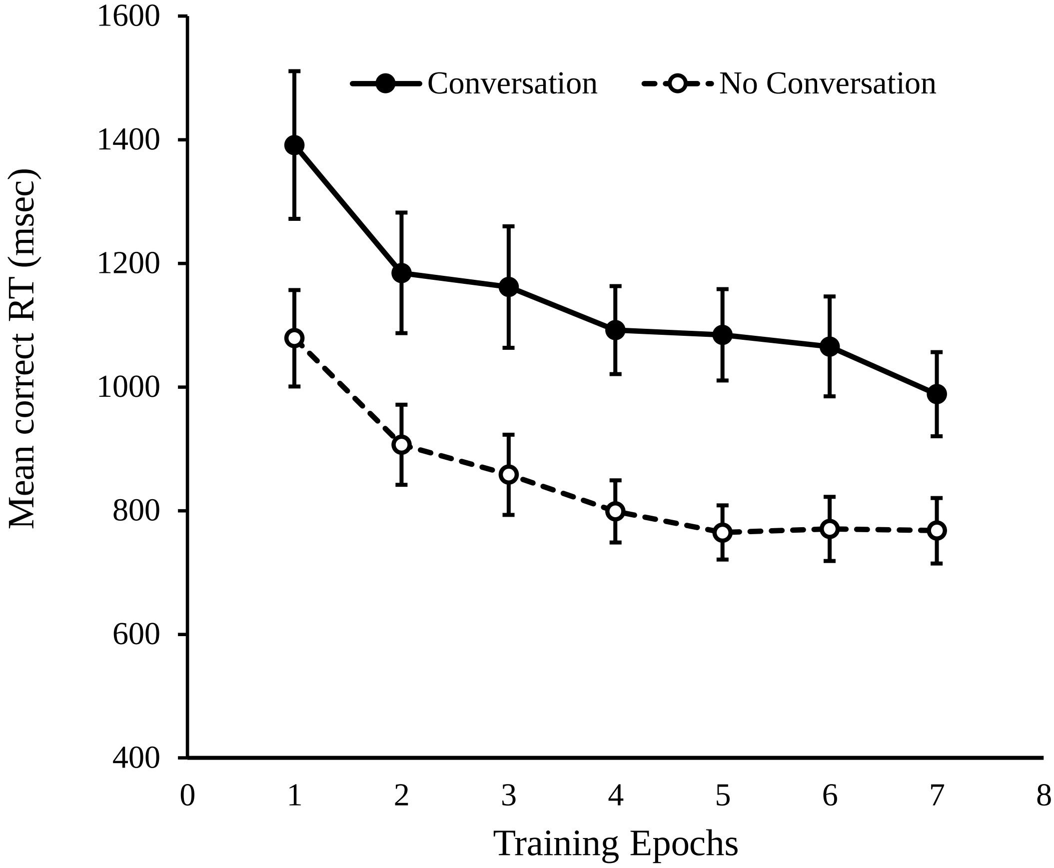

Figure 3.1 – Mean correct reaction times as a function of the conversation condition and training phase Epochs (1-7). Error bars show ±1SE.

Test Phase

The next stage of the analysis aimed to establish whether holding a conversation in the training phase of the experiment impacted upon participants' performance in the test phase (Epochs 8-10). Conversing while learning the old displays could have resulted in a smaller contextual cueing effect being found in the test phase. To test this, a repeated measures ANOVA was performed, this time with a 2 (Conversation Condition: Conversation, No Conversation) × 2 (Spatial Context: Old, New) × 3 (Epoch: 8-10) design. When participants held a conversation during the training phase, RTs in the test phase were not significantly different to when no conversation was held, $F(1,15) = 0.015$, $MSE = 75763$, p = .903, $n = .001$, the average difference between conversation and no conversation conditions across the three epochs was 4.6msec. However, significant main effects of spatial context (old versus new trials), $F(1,15)=27.3$, $MSE = 85612$, p < .001, $n = .645$, and epoch $F(1.4,21.6)= 11.4$, $MSE = 16033$, $p = .001$, $n = .432$ were found. Participants took longer to react when the displays were Novel as compared to when they were Old. Additionally, participants' RTs became shorter as they progressed through the epochs. The mean correct RT data is presented in Figure 3.2. No significant Conversation × Spatial Context interaction was found $F(1,15)=2.12$, $MSE = 48794$, p = .166, $n = .124$, neither were any other significant interactions found (All Fs =< 2.457, ps => .103). For reference, the average contextual cueing effect, calculated for each participant by averaging the spatial context conditions across the test phase epochs and then taking old trials away from new trials, was numerically higher in the no conversation condition (M = 263.2, SE = 52.5) than the conversation condition (M = 170.4, SE = 52.1).

Despite the fact that the conversation condition was counterbalanced throughout the experiment it is still important to consider whether the order of the conversation conditions impacted upon the size of the contextual cueing effect observed. For example, it is conceivable that in the no conversation condition the contextual cueing effect is large whether or not the participant performs the no conversation first or second. However, order may be important for the conversation condition. Participants may show a large contextual cueing effect when they converse first but a small one, or no effect at all, when they converse second. Therefore, the order of conversation blocks was added as a between subjects factor and the ANOVA was run again as above. As the contextual cueing effect relies on the difference between old and new trials I will only consider the interactions which involve the spatial context condition, conversation condition and the order condition. Neither the three way interaction between the Conversation, Spatial Context and Order conditions ($F = 1.330$, $p = .268$), nor the four way interaction between Conversation, Spatial Context, Epoch and Order ($F = 0.450$, $p =.642$) was found to be significant. This suggests that the order that the participants completed the conversation conditions in did not impact upon the observed contextual cueing effect.

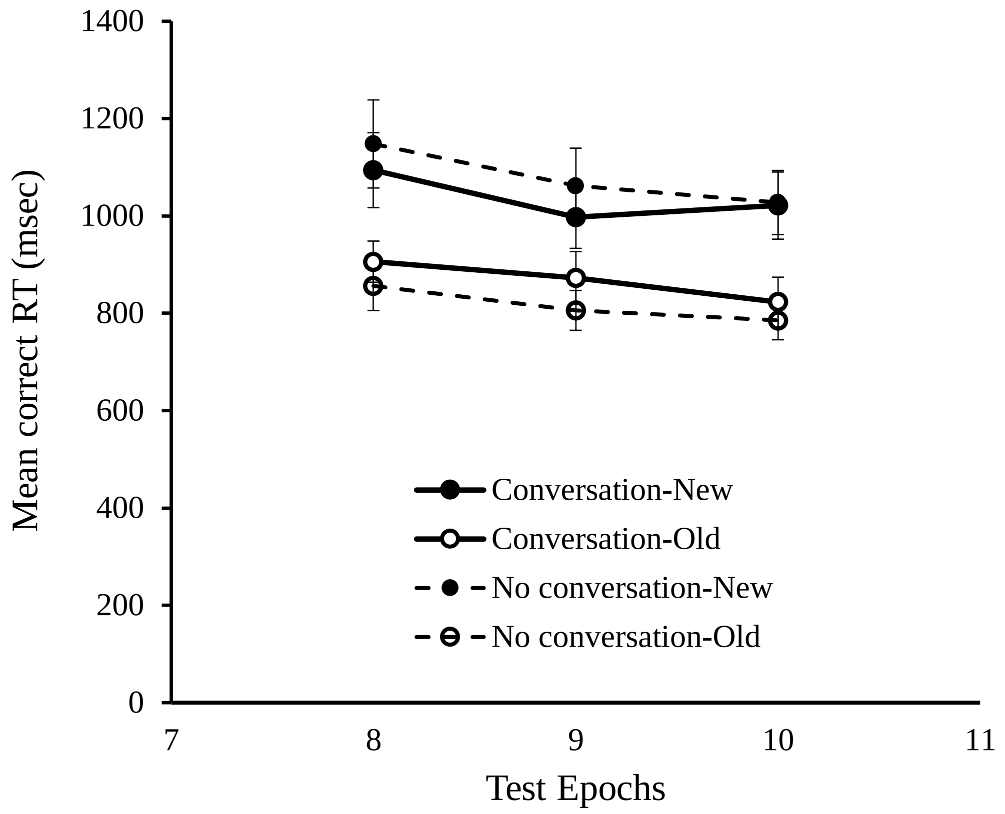

Figure 3.2 – Mean correct reaction times as a function of the conversation condition, spatial context condition and the test phase Epochs (8-10). Error bars show ±1SE.

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 (Chun & Jiang, 1998). Conversation did not significantly affect participants' reaction times to Old displays in the training phase. Neither was a difference found between the contextual cueing effect observed in the test phase of the experiment when participants were conversing compared to when they were not. These findings suggest that participants' ability to learn spatial contexts implicitly was not affected by conversation. This is consistent with research by Vickery, Sussman and Jiang, (2010) which found that when participants' VWM load was manipulated while they completed the contextual cueing task a significant effect on participants' ability to implicitly learn spatial contexts was not found.

The results of Experiment 1 suggest that participants are able to learn spatial contexts whilst conversing. However, what remains to be seen is if participants' ability to express what they have learnt is impaired, or attenuated if they are required to converse whilst attempting to do so. Experiment 2 addressed this question by following a similar methodology to Experiment 1 except that participants held the naturalistic conversation with the experimenter in the test phase rather than the training phase.

EXPERIMENT 2

As previously discussed, additional load has been shown to have a different effect on contextual cueing dependent on the phase at which the load is applied (e.g. Annac et al., 2013; Manginelli, Langer, Klose and Pollmann, 2013; Vickery, Sussman and Jiang 2010; Travis et al., 2013).Therefore, despite that 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, Experiment 2 aimed to investigate this. Experiment 2 was identical to Experiment 1 except that participants held the naturalistic conversation with the experimenter in the Test phase only.

Method

Participants

Eighteen university of Warwick non-academic staff or students, took part in return for payment or course credit, as in Experiment 1 (Male = 5, Mean Age = 21.7). Participants 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 also procedurally equivalent 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

As in Chapter 2 prior to analysis all trials where reaction times were < 200 msec were removed (0.13% of trials) as were all trials which resulted in an error (1.54%). Next the outlier removal procedure was performed whereby the mean and standard deviation was calculated for each participant for each cell of the experimental design. Then any reaction times which deviated by more than 3SD from the mean of their respective cell were removed. This resulted in 1.6% of the remaining trials being removed. The number of errors made by participants on trials which would have otherwise been included in the reaction time analyses detailed below was very low. Only 1.53% of trials resulted in an error and so these data were not analysed any further (Table C.3, C.4).

Training Phase

Participants held the conversation with the experimenter during the test phase of the experiment. Therefore, the training phase in the conversation condition and the training phase in the no conversation condition should not significantly differ. To test whether this is the case a 2(Conversation Condition: Conversation, No conversation) × 7 (Epoch: 1-7) repeated measures ANOVA was used to analyse participants' mean correct reaction time data. No significant difference was found between participants' reaction times in the conversation condition (M = 1076.6, SE = 103.8) compared to the no conversation condition (M = 944.9, SE = 66.4), $F(1,17)=1.46$, $MSE = 752909$, $p = .244$, $n = .079$. A significant main effect of Epoch was found ($F(2.3, 39.7)= 18.9$, $MSE = 107536$, $p < .001$, $n = .526$) with participants generally responding more quickly as they progress through the Epochs. This can be observed in Figure 3.3. A significant Conversation by Epoch interaction was not found, $F(2.5,41.9)= 0.342$, $MSE = 132606$, $p = .756$, $n = .020$.

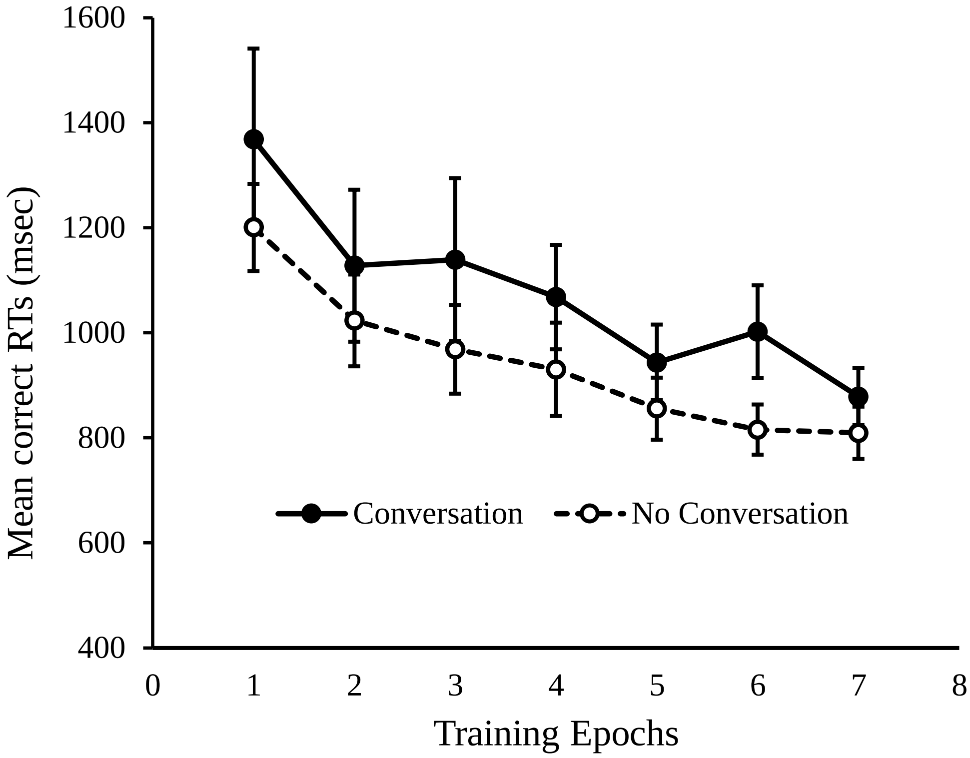

Figure 3.3 – Mean correct reaction times as a function of the conversation condition and training phase Epochs (1-7). Error bars show ±1SE.

Test Phase

Analysis of the mean reaction time in each condition, using a 2 (Conversation Condition: Conversation, No Conversation) × 2 (Spatial Context: Old, New) × 3 (Epoch: 8-10) repeated measures ANOVA showed that when participants held a conversation (M = 1346.6, SE = 132.0) during the test phase reaction times were significantly longer than when no conversation (M = 1038.9, SE = 57.9) was held, $F(1,17)= 7.70$, $MSE = 657774$, $p = .013$, $n = .312$. In addition a significant main effect of spatial context (old vs new trials), $F(1,17)=32.9$, $MSE = 126613$, $p < .001$, $n = .659$, and epoch $F(2,34)= 8.41$, $MSE = 15699$, $p < .001$, $n = .331$ was found. Overall, participants responded faster to old displays than new and reaction times were shorter in later epochs (Figure 3.4). However, the Conversation by Spatial Context interaction was not found to be significant, $F(1,17)=0.05$, $MSE = 117838$, $p = .828$, $n = .003$, and neither were any other significant interactions found (All *F*s =< .683). For reference, the contextual cueing effect, calculated for each of the conversation conditions as in Experiment 1, was numerically higher in the conversation condition (M = 288.1, SE = 73.1) than in the no conversation condition (M = 267.5, SE = 60.9).

As with Experiment 1, I was interested to discover whether or not the order in which participants completed the conversation conditions had an effect on the contextual cueing effect that was observed. Again order was added as a between subjects factor and the above ANOVA was rerun and only interactions involving spatial context and conversation were considered. The four way interaction between Conversation, Spatial Context, Epoch and Order was not significant ($F = 0.017$, $p = .944$). However the Conversation × Spatial Context × Order interaction was found to be significant, $F(1,16) = 7.864$, $MSE = 83945.8$, $p < .001$, $n = .329$. This interaction suggests that conversation order was having an effect on the contextual cueing effect observed. Examining the interaction further we can see that the contextual cueing effect was indeed numerically larger in the first block compared to the second block. This was true for both the Conversation (Conversation First (Order 1): M = 427.0, SE = 120.3; Conversation Second (Order 2): M = 148.84, 57.2) and the No conversation condition (No Conversation First (Order 2): M = 349.0, SE = 89.7; No Conversation Second (Order 1): M = 185.5, SE = 77.2).

An independent samples t-test was used to compare the contextual cueing effect observed from participants who completed the conversation condition first with those who completed the conversation second, no significant difference was found $t(16)= 2.088$, $p = .053$, however it should be noted that this comparison only narrowly missed significance at the .05 level. Neither was there a significant difference found when the participants who performed the no conversation condition first were compared with those who completed the no conversation condition second $t(16)= 1.378$, $p = .187$.

I next used paired samples t-tests to compare the size of the contextual cueing effect across conversation conditions. First, I compared the contextual cueing effect observed in each of the conversation conditions for only those participants who completed the conversation condition first. I found that the difference was marginally significant $t(8)= 2.305$, $p = .050$, however, given the number of comparisons being made here this must be considered with care. Next, I only considered data from participants who completed the no conversation condition first and again I compared the contextual cueing effect across conversation conditions. The contextual cueing effects observed did not differ significantly in this case $t(8)= 1.844$, $p = .102$.

Given the results of the t-tests reported above a follow up ANOVA was performed on the data from the first section of the experiment only. Therefore, I compared the performance of participants who conversed in the first section of the experiment with those who did not hold a conversation in the first section of the experiment. A 2 (Spatial Context: Old, New) × 3 (Epoch: 8-10) repeated measures ANOVA was performed with Conversation (Conversation, No Conversation) as a between subjects factor. The results mirror the full analysis presented above except that the between subjects effect of conversation did not reach significance, $F(1,16) = 2.431$, $MSE = 1546000$, $p < .139$, $n = .132$. Significant main effects of spatial context, $F(1,16) = 26.590$, $MSE = 153710$, $p < .001$, $n = .620$, and of Epoch, $F(1,16) = 3.715$, $MSE = 16311$, $p = .035$, $n = .178$, were found. No significant interactions were found (all *F's* <= 1.141, *p's* >= .332).

While I cannot, based on this analysis alone rule out the fact that order effects are impacting upon the results presented in this chapter, it is encouraging that when only the reaction times of trials from the first section of the experiment are considered, that the contextual cueing effect follows the same pattern as can be found when the full counterbalanced design is considered. However, it is important to keep in mind when interpreting the results presented in this chapter that a by-product of the within subjects design may have made it difficult for an effect of conversation on contextual cueing to be observed.

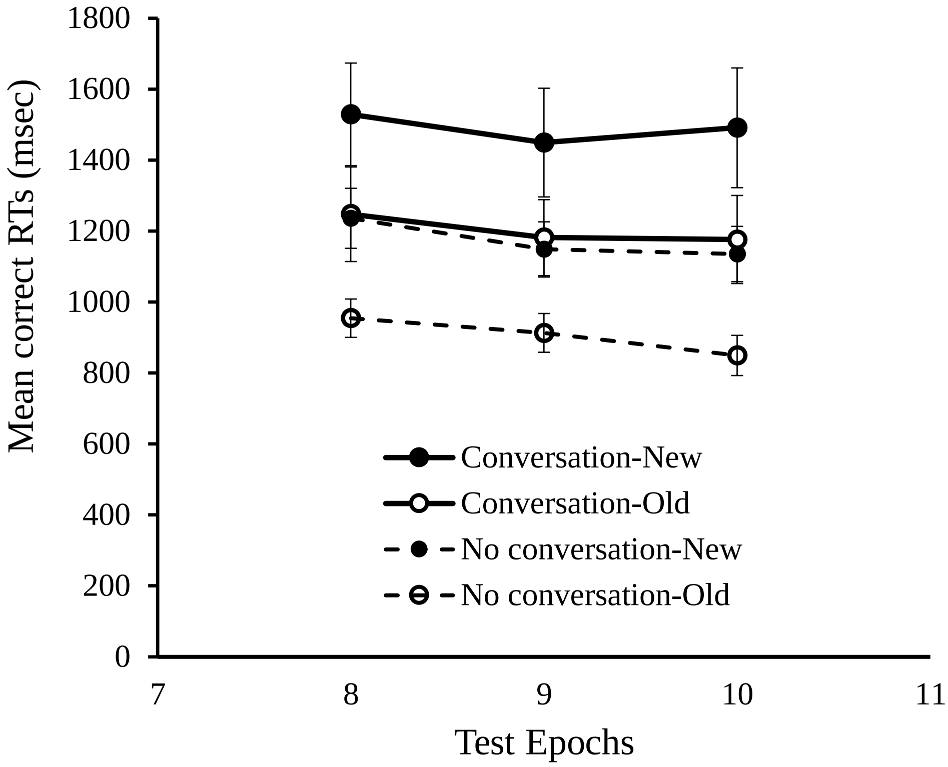

Figure 3.4 – Mean correct reaction times as a function of the conversation condition, spatial context condition and the test phase Epochs (8-10). Error bars show ±1SE.

Discussion

Participants in Experiment 2 held the conversation with the experimenter only in the test phase of the experiment. In the test phase of the experiment participants were exposed to Old and New spatial contexts. If the Old spatial contexts were successfully learnt in the training phase then they may be expressed in the test phase resulting in faster reaction times to Old as compared to New spatial contexts. The significant difference between the spatial context conditions and the lack of a Spatial Context by Conversation interaction, found in the test phase of Experiment 2 shows that participants were able to access and express learnt spatial contexts while holding a conversation. Manginelli et al., (2013), showed that certain load conditions were sufficient to impact upon the contextual cueing effect exhibited by participants in the test phase. However, it is important to note that only when a visuo-spatial working memory load task was used as a dual task did they find that the contextual cueing effect was attenuated. No effect on contextual cueing was found when visuo-nonspatial working memory was loaded.

Naturalistic conversation did not interfere with the contextual cueing effect in either Experiment 1 or 2. In the context of work by Manginelli et al., (2013) and Annac et al., (2013) the lack of any effect could be explained by naturalistic conversation not specifically exerting a visuo-spatial working memory load. Indeed Wickens' (1980, 2008) multiple resource model predicts that tasks which require similar resources or operate in the same modality are more likely to interfere with each other (in addition, for a review see Wickens, 2002). However, it must also be considered that it was simply too easy for participants to learn the Old displays. It is possible that participants had enough resources available to both learn the spatial contexts and hold a conversation with the experimenter. Experiment 3 aimed to test this by increasing the difficulty of the contextual cueing task by changing the training phase of the experiment so that it consisted of both Old and New spatial contexts and reducing the number of trials participants were given to learn the spatial contexts. In addition, participants in Experiment 3 were required to converse with the experimenter in both the training and the test phase of the experiment. These manipulations were made in order to create a situation in which it was more likely that insufficient resources would be available to converse and perform the contextual cueing task optimally. Therefore, if conversation were able to interfere with the cueing effect then this would increase the likelihood of an effect being detected.

EXPERIMENT 3

In Experiments 1 and 2 I found that conversation did not affect the learning or the expression of spatial contexts. However, participants only held the naturalistic conversation in either the test phase or the training phase of the experiment. In addition, the training phase in Experiments 1 and 2 consisted only of Old trials and as such it is possible that participants had ample time to learn these spatial contexts and so the additional task demands which the naturalistic conversation induced were not sufficient to interact with the learning task. This explanation is leant support by Travis et al., (2013) who asked participants to perform a contextual cueing task and simultaneously a working memory (WM) load task (during the training phase). When the training phase consisted of only old, repeated displays, they found no effect of WM load. However, when the contextual cueing task was made more difficult, by including both novel and repeated displays in the training phase, an attenuated contextual cueing effect was observed.

Experiment 3 was performed to answer two questions. The first is, if I make the spatial contexts more difficult to learn, by interspersing new trials amongst old throughout the whole experiment, and by reducing the number of trials overall, do participants find it more difficult to learn spatial contexts when conversing with the experimenter? Second, will the contextual cueing effect, experienced in the test phase (the final three Epochs), be significantly attenuated if participants converse throughout both the training and the test phase of the experiment? The rationale here is that holding a conversation may be interfering with both the learning and expression of spatial contexts but to such a small degree that it was not detectable when the conversation was held for only one of the stages of the experiment. By forcing participants to converse throughout both stages of the experiment it is conceivable that these small effects could combine and a detectable effect of holding a conversation on the contextual cueing effect may be found.

Method

Participants

Participants (Mean Age = 18.9, Female = 19) were an opportunity sample of 20 University of Warwick non-academic staff and students. Participants confirmed that they could easily hear the experimenter in the conversation condition and that they could see the visual display.

Stimuli and Apparatus

Displays were presented on a HP191 LCD monitor with a resolution set at 1440x900 pixels. The participant was positioned approximately 57cm from the monitor. The experiment ran on an IBM compatible PC. Participants responded using a standard QWERTY keyboard.

The naturalistic conversation took place in a similar fashion to Experiments 1 and 2. However the equipment used differed, participants conversed via a hands-free mobile phone (Samsung Galaxy S4) which was on speaker phone and placed next to them. The conversation was held over SKYPE and was recorded to allow checking that a “natural” 2 way conversation was held. The experimenter received the SKYPE call on a Laptop computer (Toshiba Satellite Pro with an Intel Celeron processor) in an adjacent experimental cubicle.

The stimuli were designed to closely replicate those used in Experiment 1. As in Experiment 1 the stimuli were placed within a 6 × 6 matrix and their exact position was jittered within each cell by up to 20 pixels horizontally and vertically.

Design and Procedure

Experiment 3 differs in three key ways from Experiments 1 and 2. Firstly, participants were required to converse with the experimenter throughout both the training and the test phase of the experiment. Whereas, in Experiments 1 and 2 participants were required to converse in only one of the two phases. Secondly, the training phase consisted of both Old and New spatial contexts. The training phase of Experiments 1 and 2 contained only Old displays. Finally, the number of trials was reduced so that participants did not have as long to learn the Old spatial contexts. These changes resulted in a design where there were no clearly defined training and test phases and where participants took part in fewer Epochs (8). Therefore, for analysis purposes and in order to be consistent with Experiments 1 and 2 the final three Epochs (6-8) were chosen to form the Test phase and Epochs 1-5 formed the training phase. These changes led to a 2 (Conversation: Conversation, No conversation) × 2 (Spatial Configuration: Old, New) × 8 (Epochs: 1-8) within subjects design being used.

The participants’ task was also equivalent to that used in Experiment 1. They were required to search for a T shape amongst a display of T’s where the central stem of the T had been offset from centre, and then respond based on the targets orientation. Participants completed eight Epochs containing both Old and New trials. As in Experiments 1 and 2, four Old displays were randomly generated at the beginning of the experiment. Four new displays were also generated, however, as before, the target would remain in the same position and the distracters would be randomly placed each time the display was presented. Each Epoch contained 32 trials, with each of the four Old displays and each of the four new displays equally represented. This resulted in a total of 256 trials (Old = 128, New = 128).

Before beginning the experimental trials participants completed 8 randomly generated practice trials so that they could become familiar with the task. Next, as in Experiments 1 and 2 participants completed a training and then a test phase in both the conversation and no conversation conditions. The conversation condition order was counterbalanced across participants. The naturalistic conversation was held in an identical manner to the conversation in Experiments 1 and 2.

Results

As in Experiment 1, the data were cleaned prior to analysis. All RTs < 200ms (0.02%) and all errors were removed (1.64%) then the outlier procedure was followed as in Experiment 1 with all RTs greater than 3 standard deviations from the mean for a respective cell in the design being removed (0.84%). The number of errors made by participants, on trials which would have otherwise been included in the reaction time analyses detailed below, was again very low. Only 1.58% of these trials resulted in an error and as such the error data were not analysed any further (Table C.5, C.6).

Experiments 1 and 2 had clearly defined training and test phases. Only Old displays were presented in the training phase and both New and Old displays were presented in the test phase. However, all of the Epochs in Experiment 3 contained both Old and New displays. Therefore, in order that the analysis of Experiment 3 was consistent with Experiments 1 and 2 the first 5 Epochs were allocated as training phase Epochs and the final three Epochs were considered the test phase.

In order to get an overview of the data from this experiment a 2 (Conversation condition: Conversation, No Conversation) × 2 (Spatial Context: Old, New) × 8 (Epoch: 1-8) repeated measures ANOVA was performed. A significant main effect of Spatial Context, $F(1,19)=41.747$, $MSE = 226631$, $p < .001$, $n^2 = .687$, and of Epoch, $F(2.832,53.806)=12.508$, $MSE = 530529$, $p < .001$, $n^2 = .397$, was found (see Figure 3.5). When all 8 epochs are considered together and the conversation condition is ignored, participants responded, on average 243ms more quickly to Old displays than new and overall RTs decreased by 556ms from Epoch 1 to Epoch 8. However, neither a main effect of conversation nor any significant interactions were found (all *Fs* =< 2.587, *ps* => .115).

It is important to note here that as with Experiments 1 and 2 the effect on the contextual cueing effect of the order in which the participants completed the conversation conditions was considered. Experiment 3 differed from experiments 1 and 2 in that both old and new trials were present throughout all of the experimental Epochs therefore, a 2 (Conversation condition: Conversation, No Conversation) × 2 (Spatial Context: Old, New) × 8 (Epoch: 1-8) repeated measures ANOVA was performed with order added as a between subjects factor. As before, only interactions which included the three conditions order, spatial context and conversation were considered. Neither the Conversation × Spatial Context × Order interaction, $F(1,18)=0.253$, $MSE = 384783$, $p = .621$, $n^2 = .014$, nor the 4 way interaction between Conversation, Spatial Context, Epoch and Order were found to be significant, $F(3.924,70.633)=0.259$, $MSE = 167365$, $p = .900$, $n^2 = .013$. This suggests that in this experiment, that order did not significantly affect the contextual cueing effect observed across conversation conditions.

Next I focused specifically on the training phase of the experiment in order to investigate if holding a conversation had an impact upon participants' ability to learn spatial contexts, a 2 (Conversation condition: Conversation, No Conversation) × 2 (Spatial Context: Old, New) × 5 (Epoch: 1-5) repeated measures ANOVA was performed. A significant main effect of Spatial Context, $F(1,19)=30.2$, $MSE = 237953$, $p < .001$, $n^2 = .614$, and of Epoch, $F(2.45,46.62)=10.7$, $MSE = 268111$, $p < .001$, $n^2 = .360$, was found (see Figure 3.5, note that this figure displays all epochs, 1-8). Overall participants responded more quickly to Old displays than new and RTs decreased from Epoch 1 to Epoch 5. In addition, a significant three way interaction was found between the conversation, spatial context and epoch conditions, $F(4,76)=2.88$, $MSE = 103748$, $p = .028$, $n^2 = .132$. However, neither a main effect of conversation nor a Conversation × Spatial Context interaction, $F(1,19)= 0.439$, $MSE = 369650$, $p = .516$, $n^2 = .023$, nor any further significant interactions were found (all *Fs* =< 2.587, *ps* => .115).

To investigate the three way interaction further a 2 (Conversation condition: Conversation, No Conversation) × 2 (Spatial Context: Old, New) × 2 (Epoch: 1-2) repeated measures ANOVA was performed. The rationale for this analysis was that, it appears from the lack of a significant spatial context by epoch interaction that the difference between Old and New spatial contexts does not significantly differ between epochs. It has been shown previously that Old displays can be learnt in only 5 repetitions (Chun & Jiang, 1998). In addition, the data from Experiment 3 indicate that Old spatial context learning happened very early on. Therefore, if conversation is able to affect the speed at which participants are able to learn spatial contexts then this effect is most likely to be found in the first two epochs. However, while a significant main effect of spatial context, $F(1,19)=24.5$, $MSE = 103719$, $p < .001$, $n^2 = .563$, and of epoch, $F(1,19)=20.7$, $MSE = 133826$, $p < .001$, $n^2 = .522$, were found as well as a significant interaction between Conversation and Epoch, $F(1,19)=6.58$, $MSE = 133030$, $p = .019$, $n^2 = .019$. Only marginal evidence was found for a three way interaction, ($F = 4.135$, $p = .056$). No other main effects or interactions were found to be significant (All Fs <= 1.674, *ps* >= .211).

It is important to note that the significant three way interaction found when all five epochs were included in the repeated measures ANOVA does not appear to be systematic (Figure 3.5). In addition, when only the first two blocks were included in the analysis, where we would expect to find the effect of conversation on learning, no significant effect was found. Based on this evidence alone it is not possible to conclude that conversation impacted upon the rate at which participants learnt spatial contexts. In fact, taken in the context of the findings of Experiments 1 and 2 of this chapter, it appears as though holding a conversation does not have a significant effect on spatial context learning.

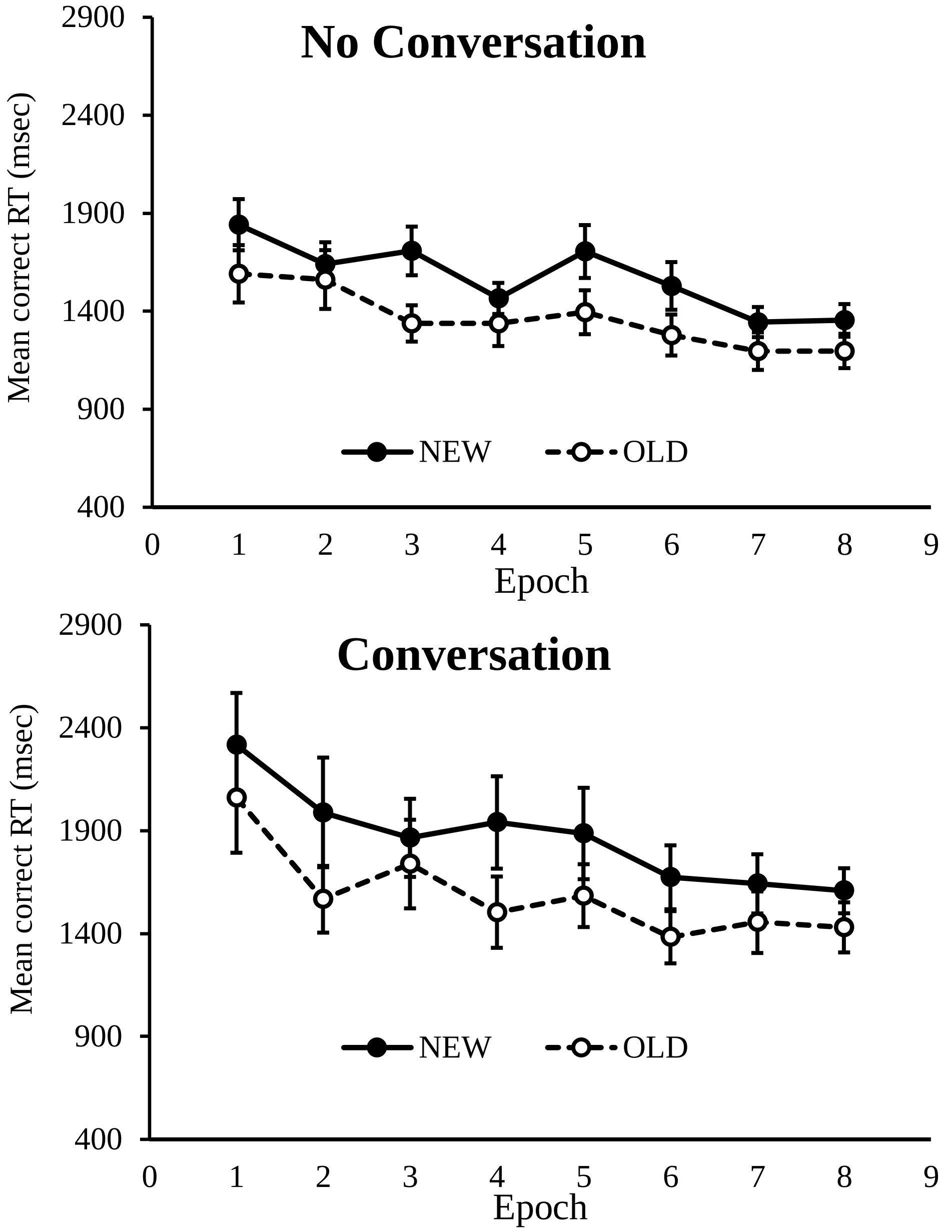

Figure 3.5 - Mean reaction times as a function of the conversation and spatial context conditions and Epoch (1-8). Error bars show ±1SE.

Next I performed a 2 (Conversation condition: Conversation, No Conversation) × 2 (Spatial Context: Old, New) × 3 (Epoch: 6-8) repeated measures ANOVA to investigate the effects of conversation and spatial context in the final three epochs where any contextual cueing effect should be firmly established. A significant main effect of spatial context was found, $F(1,19)=42.570$, $MSE = 57359$, $p < .001$, $n = .691$, with participants again reacting more quickly to Old displays. A main effect of conversation was found to be borderline significant, $F(1,19)=4.277$, $MSE = 649211$, $p = .053$, $n = .184$. However, as one would predict that conversation would significantly lengthen participants' reaction times (e.g., Kunar et al., (2008) and the data presented in Chapter 2 and in the first two experiments of this chapter) a one tailed test might be appropriate. Under these new parameters the main effect of conversation reaches significance, with participants taking longer to react whilst conversing than when they were not conversing. No other main effects or interactions were significant (all $Fs =< 1.334$, $ps => .276$). Therefore, although participants' reaction times were significantly different in the spatial context conditions, naturalistic conversation did not significantly affect the extent to which Old spatial contexts were learnt and then expressed, producing the contextual cueing effect. The mean data from this analysis is displayed in Figure 3.6. For reference the average contextual cueing effect, calculated as in experiments 1 and 2 looking only at the final three epochs, was numerically higher in the conversation condition (M = 218.6, SE = 44.8) compared to the no conversation condition (M = 184.9, SE = 50.9).

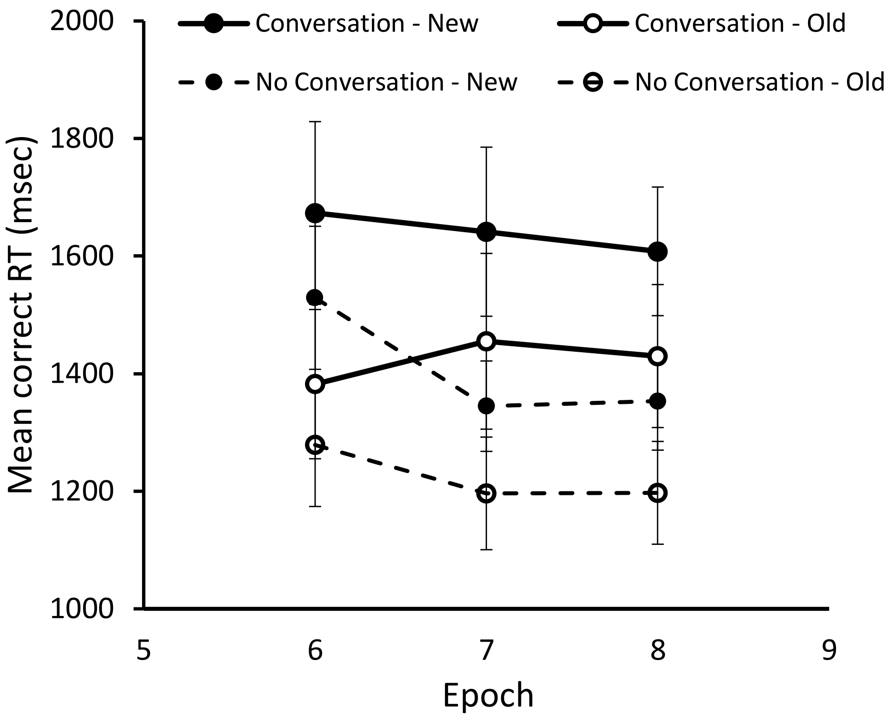

Figure 3.6 - Mean correct reaction times as a function of the conversation condition, spatial context condition and the test phase Epochs (6-8). Error bars show ±1SE.

Discussion

Holding a conversation while performing the contextual cueing task did not have a significant effect on participants' reaction times except when the final three epochs are looked at in isolation (this difference was significant in the test phase when a one tailed test was used). However, despite this overall increase in reaction times, a significant effect of spatial context was found in both distraction conditions. Participants responded faster to spatial contexts which they had previously been exposed to, indicating that contextual cueing was taking place even whilst they were conversing.

Thus Experiment 3 shows that even when the contextual cueing task is made more difficult, by adding new spatial contexts throughout the training phase, reducing the overall number of trials in which the spatial contexts can be learnt and requiring that participants converse throughout the training and test phase, contextual cueing still occurs.

General Discussion

Experiments 1-3 aimed to examine how conversation may affect the participants' implicit coding, and then use of, context when searching a visual scene. They achieved this aim in that all three experiments show the same pattern of results. Contextual cueing, it would appear, is immune to the additional load which naturalistic conversation applies. However, the visual search task itself, the finding of a target amongst a field of distractors, was significantly slowed relative to the no conversation condition. This general finding that participants' reaction time performance is attenuated adds to the literature around naturalistic conversation and visual attention (Spence et al., 2013, Kunar et al., 2008) and in fact supports the preposition that conversation induces an overall slowing of participants' interaction with visual scenes. Whereas, participants' ability to implicitly learn effectively and apply that learning at a later stage is not affected.

Strayer, Drews and Johnston's (2003) found that participants' implicit memory for words that had been presented at fixation, was worse when they were conversing than when they were not. The three experiments presented in this chapter aimed to extend this finding by investigating whether the implicit memory for spatial contexts could also be affected by load induced by holding a naturalistic conversation. However, no such evidence for an effect of holding a conversation on implicit memory for spatial contexts was found. An explanation as to why an effect on implicit memory was found for words but not for spatial contexts may be that the implicit processing of a word is not prioritized to the same extent as the implicit encoding of a spatial context or simply that as conversation and implicit word learning are both verbal tasks they require similar resources and so interfered with each other to a greater extent.

Also of note is that, the participants in the current work were repeatedly exposed to the old stimuli and so had a relatively long time in order to implicitly learn them. This extended period of learning may be the root of the difference between the data presented here and the work by Strayer Drews and Johnston (2003) especially when we consider that their participants were given only a single opportunity to implicitly learn each of the words.

The findings presented in this chapter highlight the need for future work looking into this area. Of interest is that the deficit in performance caused by conversation in everyday driving tasks may not be as great as previous research has suggested. Although it has been shown that holding a conversation impacts upon implicit word learning (Strayer Drews & Johnston, 2003), the experiments presented in this chapter have shown that implicit learning and expression of spatial contexts remains unaffected. Therefore, it appears that at least with respect to spatial context learning, even whilst they are conversing participants are able to make use of attentional mechanisms which can aid the way in which they attend to their visual environment. However, it may be the case that varying the nature of the conversation could result in it impacting upon our ability to benefit from the implicit learning of spatial contexts. It is possible that in an effort to make the conversation as natural as possible, the conversation was not emotionally salient, or challenging enough, to induce a “high” attentional load. If higher load interferes with implicit memory tasks to a greater extent than lower attentional load, as in Travis et al., (2013), then we may expect that more challenging conversation may have a greater effect on the contextual cueing effect. Related to this, Rakausakas, Gugerty and Ward (2004) demonstrated that holding a conversation while driving impacted on driving behaviour. However, the deficit in driving performance was not changed by varying the level of load between “Hard” conversation and “Easy” conversation. Therefore, where conversation is concerned it is not simply the case that increasing the secondary tasks difficulty will cause greater impairment to the primary task.

While increasing the cognitive difficulty of the conversation task may not affect participants’ ability to encode and express spatial contexts, it is possible that changing the content of the conversation could affect task performance. For example, conversations which are high in visual imagery have been shown to affect driver’s performance in driving simulation studies (Bergen, Medeiros-Ward, Wheeler, Drews, & Strayer, 2013). In addition, conversations with highly emotional content, that are anxiety provoking, cause greater cognitive working load demands, and may therefore have more of an impact on people's ability to process visual scenes and respond appropriately in their environment (Briggs, Hole & Land, 2011). Finally, perhaps conversation of a spatial nature may interfere with the contextual cueing task just as visual-spatial WM load was able to but visual-non spatial WM load was not (Annac et al., 2013; Manginelli et al., 2013; Travis et al., 2013).

Previous studies have demonstrated that when participants are engaged in a verbal-spatial task, such as mental rotations, while driving they show marked deficits in driving behaviour such as gaze freezing where participants make longer fixations and a lower glance frequency to mirrors and speedometer (Recarte & Nunes, 2000). In addition, it has been shown that conversations which focus on visuo-spatial mental imagery, such as "How do I get from here to the Mall?" and "How many windows do you have in your house?", impact significantly on driving behaviour compared to when no conversation is held (Beede & Kess, 2006). Therefore, it is possible that conversation which focused on spatial imagery may affect participants' ability to perform the contextual cueing task as the spatial imagery may interrupt the spatial context learning and expression necessary to complete the contextual cueing task. Indeed using conversation of a spatial nature as a dual task may also shed light on the finding that implicit word learning was affected by naturalistic conversation (Strayer, Drews and Johnston, 2003) but that spatial context learning was not. It is possible that as word learning and holding a conversation are both verbal tasks that they drew from a similar pool of resources resulting in dual task interference. Therefore, it could be predicted that a conversation which was spatial in nature may draw on the same pool of resources that are required to implicitly learn spatial contexts and result in a reduction of contextual cueing.

In summary, Chapters 2 and 3 indicate that naturalistic conversation does not impact upon participants' ability to visually mark or to learn and express spatial contexts. A preview benefit was found when participants were conversing even at a preview duration of 250ms. In addition, when participants were required to hold a conversation while performing the contextual cueing task, regardless of the stage at which conversation took place, participants responded faster to old spatial contexts compared to novel contexts. Holding a conversation caused a significant increase in participants' reaction times to the target compared to trials where no conversation was held. However, the increase in reaction times to novel and old spatial contexts did not significantly differ from one another. Therefore, while conversation appears to cause an overall slowing of reactions to target stimuli the implicit encoding and expression of spatial contexts and participants' ability to benefit from previewing a subset of stimuli prior to the full set, is not affected.

These results should not be misinterpreted as conversation having no effect at all on the visual search task. In fact, our results indicate that participants search a visual scene less efficiently, take longer to indicate that they have found a target item and furthermore this deficit in reaction time performance is likely to get larger as the visual scene becomes more and more complex.

In addition to the theoretical impact of this work, Chapters 2 and 3 have important real world implications. Given the complex nature of everyday visual scenes it is, and should be, concerning that visual attention is so impaired by such a simple dual task as holding a conversation. For example, my data show that in the FEB condition, in the displays with the fewest search stimuli, reaction times to find the target were slowed by 165ms and in the most cluttered by 247ms, when participants were conversing. This additional time, if added to the response time to stop a car in an emergency, would increase the distance that a car would travel by 18 feet, assuming that the car was travelling at 50 miles per hour and 25 feet if the car were traveling at 70 miles per hour.

Therefore, despite the fact that our attentional system may be able to rely on mechanisms such as visual marking and contextual cueing to aid it in processing the world around us, we can very easily handicap ourselves by holding a conversation. When we do this our reaction times are slowed and we experience a decrease in our search efficiency. In the lab this results in participants taking longer to complete an experiment, in the real world this delay could be the difference between life and death.

Having established that the preview benefit can survive even under conditions of dual task naturalistic conversation, I designed a further series of studies to investigate the effect of a second task which is often performed whilst driving, namely receiving and processing spatial directions. This addresses two key aims, the first is widening the applicability of this research by investigating a further task which is prevalent in driving scenarios. Second, these experiments will allow for the investigation of how a naturalistic task may affect the preview benefit, specifically by applying resource demands in the same domain as those predicted to be used by the visual attention task (Wickens, 1980, 2002). This is especially poignant given previous research, albeit using a different visual attention task, which indicates that in order for a visual working memory task to interfere with the contextual cueing effect it is necessary for this task to have a spatial component (Manginelli, et al., 2013; Travis et al., 2013). With this in mind, Chapter 4 focuses on assessing the effects of a naturalistic spatial load task on the preview benefit. This is of particular interest as visual marking is hypothesised to require that a spatial representation of the previewed items be generated in order that a preview benefit is achieved (Watson, Humphreys & Olivers, 2003). This process of generating spatial representations may be particularly susceptible to interference from a task which induces a spatial load.

Chapter 4:

Naturalistic spatial load and the preview benefit

As discussed in the introduction to Chapter 2 visual attention is of key importance in a variety of tasks. However, we are often required to perform other tasks while attempting to visually attend to the world around us. In these situations our attentional resources may be taxed to the point that performance in one task must be sacrificed for performance in another (Pashler, 1994, Wickens, 1980, 2002). In Chapter 2 I investigated how holding a naturalistic conversation may be affecting our ability to visually attend to the world around us using the preview search paradigm. Chapter 4 extends this work by investigating whether another task which is often performed whilst driving, listening and remembering satellite navigation commands, is able to affect participants' ability to prioritise new visual stimuli over old stimuli using the preview search paradigm (Watson and Humphreys, 1997). In this paradigm participants search for a target amongst other distracter stimuli. In some of the trials half of the distracters are presented first and then after some time the remaining distracters are presented along with the target item. This is called a preview trial (PRE). In other trials, known as Full Element Baseline (FEB) trials, the target and distracters all onset together. The classic finding is that search rates are improved in the PRE condition relative to the FEB condition (Watson & Humphreys, 1997).

While the general preview benefit finding is robust, the mechanisms behind it are still under dispute. A prevalent account of the preview benefit was first posited by Watson and Humphreys (1997). They state that the benefit is due to the intentional top down inhibition of the old items, during the preview period, so that new items may be prioritised and searched more efficiently. As this hypothesised process is top down it is able to be goal directed. However, it is also memory and resource dependent. As such for a full preview benefit to occur a sufficient preview duration of at least 400ms must be used (Humphreys et al., 2004; Humphreys, Olivers & Braithwaite, 2006; Warner & Jackson, 2009; Watson & Humphreys, 1997). This ensures adequate time for the old stimuli to have inhibition applied to them (Watson & Humphreys, 1997) or for them to be encoded into visual short term memory (Al Aidroos, Emrich, Ferber & Pratt, 2012). Please see chapter two for an overview of the wealth of literature supporting the top down, resource dependent explanation of the preview benefit. However, despite this evidence, alternative explanations for the preview benefit have been suggested which focus upon bottom up mechanisms, whereby physical features of the new items, such as their abrupt onset, automatically capture attention (see Donk & Theeuwes 2001, 2003; Jonides & Yantis, 1988; Yantis & Jonides, 1984). See Chapter 2 for a more in depth look at the various explanations of the preview benefit.

It has been shown that, our ability to experience a preview benefit can be attenuated by an additional dual task (Humphreys, Watson & Jolicoeur, 2002, for details see Chapter 2). However, in Chapter 2 I did not find an effect of naturalistic conversation on participants' ability to benefit from previewing a subset of distracter items before the onset of further distracters and a target. A possible explanation of this finding may be that naturalistic conversation did not apply a level of load which was dependent on the same resources required by the preview benefit. Alternatively, it is possible that conversation did not impact upon the preview benefit as participants were able to flexibly divert attention away from the task of conversing and apply it, when necessary, to process the preview items.

As previously discussed, Wickens' (1980, 2002) multiple resource model, predicts that tasks which share overlapping resources are more likely to interfere with each other. Visual marking is a visual attention task which, it is hypothesised, requires that a spatial representation of the old items be generated and an inhibitory marker be applied to them (Watson & Humphreys, 1997) or for these items to be stored within visual short term memory (Al Aidroos, Emrich, Ferber & Pratt, 2012). Therefore an additional dual task which loaded similar resources such as a visuo-spatial task might interfere with our ability to experience a preview benefit. In fact this has been demonstrated in a different attentional task, contextual cueing (Chun & Jiang, 1998). Manginelli, Langer, Klose and Pollmann, (2013) showed that our ability to express spatial contexts, which have been previously learnt, was impeded by a dual task which applied a visual spatial WM load but not one which applied a more general load.

Given the focus of the lab based work in this thesis, the investigation of the effects of naturalistic load on mechanisms vital for safe and efficient driving (as well as other related real-world tasks), I endeavoured to find a load task which fit these criteria but which applied a spatial load. Therefore, I chose to manipulate load through a naturalistic task akin to listening to, viewing and remembering satellite navigation directions. By designing the task in this way I was also afforded the opportunity to investigate how holding a piece of information in working memory can impact upon visual attention.

Recent research has shown that what we hold in working memory is able to interact with and impact upon, the way in which we apply attention to the world around us (see Soto, Hodsoll, Rotshtein & Humphreys, 2008, for a review). For example, Soto, Heinke, Humphreys and Blanco (2005) used a visual search task in which participants searched for and responded to a target item. The target and distracters were each contained within a coloured shape. Participants were also given a cue at the beginning of each trial which could match one of the shapes in the display. When the visual cue matched the shape which contained the target, search was expedited, however when it matched a distracter search was less efficient. However, of key importance, this was only the case when the cue was held in memory; no effect of the cue was found when the cue was simply observed. It is interesting to note, that the cues held in working memory still affected search even when they were never valid, in that they never indicated the position of a target.

In addition, Soto and Humphreys (2007) showed that it is not only visual cues which are able to generate this effect but that verbal primes are also sufficient. In their study search items were positioned inside coloured shapes and the participant must search through these to find a target item. Verbal primes, which were two word descriptions of a coloured shape (eg “red square”), were given to participants who were then required to search through the display. As with visual primes, when verbal primes were committed to memory they impacted upon participants search performance.

Experiments 1-4 of this chapter are similar to the work of Soto et al., (2005) and Soto and Humphreys (2007) but have several key differences. Firstly, participants were given a spatial direction at the beginning of each trial (in the form of an arrow oriented either vertically upwards, rotated 90 degrees to the left or rotated 90 degrees to the right) rather than being presented with defining colour or shape features e.g. “red square”. I will refer to these spatial directions as cues and primes as these are the terms typically used in the literature. However, strictly speaking the spatial directions were not designed to act as cues and were not predictive of the targets location in the proceeding search trial. Participants were specifically informed of this lack of a relationship between the spatial direction and the targets location. While the spatial direction that was presented to participants did not identify a particular target or distracter it is possible that presenting a direction in this way may act to guide attention to a particular area of the display (either the left or the right). Therefore, it might be the case that trials in which the target appears on the same side of the display as the spatial direction that they are remembering (e.g. Direction = “Left”, Target appears on the “Left”) are responded to faster than those in which the target and direction are incongruous. Of particular interest is that our task, as discussed, was designed to be naturalistic and so the prime and target/distracters were not linked. As previously stated, the prime did not indicate a particular target or distracter item and participants were made aware of this.

The second main difference is that Experiments 1-4 were set up to not only look at standard search but also to investigate if holding directions in WM has an effect on time based selection and if this effect is different under various display size conditions. It may be that holding a direction in memory biases search towards one side of the display or another when all the targets onset together (FEB) but when some stimuli are previewed relative to others (PRE), the effect is not found. Finally, it must be considered that the spatial directions may have a different effect on visual search and the preview benefit depending on which modality they are presented in. To investigate this, the modality in which spatial directions were presented was varied between Experiments 1-4. In Experiment 1 participants were presented with the spatial directions both visually and via audio. In Experiment 2 and 3 the directions were only presented visually. Experiment 3 differed from Experiment 2 in that three directions were given at the beginning of each trial rather than one, in order to increase WM load. Spatial directions in Experiment 4 were only presented via audio. The search task conditions remained constant throughout Experiments 1-4. Designing the experiments in this way allowed for the evaluation of whether modality is important when considering the effect of WM load on search performance.

In summary, the four experiments presented in this chapter had two overall aims. The first was to establish whether holding a spatial direction in working memory interferes with the preview benefit. The hypothesis being that loading spatial resources in this way may interfere with the generation of a spatial map of the previewed stimuli, which is believed to be a necessary component of time based selection (Watson & Humphreys, 1997). The second aim was to determine whether holding a spatial direction in memory has an influence on subsequent search processes and whether the modality that the direction is presented in varies this effect.

EXPERIMENT 1

This experiment was designed to investigate how being required to remember a spatial direction whilst taking part in a preview search paradigm may impact upon the preview benefit and any subsequent search processes. The direction was presented to participants both visually and via audio.

Method

Participants

23 participants[2] were sourced from the University of Warwick research pool. The mean age of participants was 20 and 13 were female. Participants confirmed that they could easily hear the auditory stimuli and see the visual display. The sample size for this study was based on the experiments conducted in Chapter 2 of this thesis.

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[2] While 23 participants took part in this experiment 2 participants were removed because of non-compliance with the task instructions.

Stimuli and Apparatus

The experiment was run on an Intel i7 Computer attached to a 24 inch IIyama Prolite B2480H5 monitor running at a resolution of 1920x1080.

Visual search trials consisted of a fixation cross that occupied 0.24 visual degrees both horizontally and vertically. As well as a blue H target, blue A distracter (RGB: 68,164,176) and green H distracter (RGB: 11,193,126) which all subtended 0.67° vertically and 0.53° horizontally. All stimuli were presented on a black background. All of the letter stimuli were box figure-8 type stimuli. The distracters and the target were placed within a 6 × 6 grid which subtended 18.0° vertically and 17.9° horizontally (see von Mühlenen, Gunnell and Watson 2013, for a similar methodology). The target could not appear in the central two columns of the grid to ensure that the position of the target to the right or left of the fixation was always easily discernible. The targets positions were jittered by the addition of random noise (up to 20 pixels) to their x and y coordinates. The number of stimuli presented on each trial was either 4, 8 or 16.

Directional stimuli consisted of a right facing and a left facing arrow each subtending 1.15° vertically and 1.72° horizontally as well as an arrow pointing upwards with the same dimensions transposed (all arrows had RGB values of: 180, 180, 180). Arrows were presented at the fixation point and were surrounded by a black box with borders which were 0.1° (RGB: 180, 180, 180).

Design and Procedure

In this study, the participants took part in a search task similar to that used in Chapter 2. In the search task participants were required to search through a visual field of distracting stimuli in order to locate a target item. The target could appear on either the left or the right hand side of the display. Participants were asked to respond to the position of the target in the display, for example, pressing the left mouse button if it appeared on the left.

The participants took part in a PRE condition and a FEB condition. In the preview condition half of the distracters were presented followed by the remaining distracters and a target, with a preview duration of 1000ms. In the FEB trials all of the stimuli onset simultaneously. A minority of trials (7%) were target absent trials. Their purpose was to stop participants from being able to develop a strategy whereby it is only necessary to search half of the visual display to determine where a target is located.

The experiment also consisted of a spatial load condition and a no load condition. Participants completed both search conditions (FEB and PRE) in each of the spatial load and no load conditions. The order in which these blocks of trials were completed was counterbalanced across participants. For example, if a participant started with the FEB-Spatial load condition then the next block would be the PRE-Spatial load condition. The participant would then go on to complete the FEB and PRE search in the no load condition in the same order. The participant would always complete both FEB and PRE search in one load condition before moving on to the next. In the spatial load phase, before each trial, participants were presented with a visual cue in the form of a directional arrow (Up, Left, Right). Along with the visual cue the participants were also given the direction of the arrow in the form of a verbal cue, for example, the word “left” would be played through headphones. Their task was to remember the direction whilst they performed the search task, then at the end of the trial they were asked to indicate which direction they had been given. In the no load condition participants completed the search tasks without being presented with a direction at the beginning of each trial.

Before every block of trials, instructions for the block were displayed to the participant to ensure that they knew what to expect in the block and to remind them of their task. Participants performed 4 blocks of 10 practice trials, one block for each level of the spatial load condition and the presentation condition. Next participants completed four experimental blocks in the same order as the practice blocks. Two Full Element Baseline (FEB) blocks, one with spatial load and one with out and two Preview (PRE) blocks, one with spatial load and one without. Each block consisted of 84 trials 6 of which were catch trials. Participants therefore, each completed 336 experimental trials. The remaining 78 trials were equally split so that the target location fell equally often on the right and left of the screen and each display size condition was equally represented. The search tasks were identical in both the spatial load and no load condition. However, preceding each visual search trial in the spatial load condition participants were presented with an arrow and a congruous auditory direction (Either, Ahead, Left or Right). The arrow was presented for 1000ms; and the corresponding direction was spoken out loud by the computer once. After the visual search trial participants were presented with the three directional arrows positioned in the centre of the screen from left to right in the order “Left”, “Ahead”, “Right” and were asked to recall which direction they had been given at the start of the trial by clicking on the corresponding arrow. The experimenters intended for the spatial directions to be equally represented amongst the experimental trials. However, due to a programming error the proportion of directions given in each block were as follows (Left = 29.5, Right = 30.8, Ahead = 39.7).

This methodology resulted in a 2 (Load: Direction, No Direction) × 2 (Presentation Condition: FEB, PRE) × 3 (Display Size: 4, 8, 16) repeated measures design. Participants took approximately 50 minutes to complete the experiment.

Results

Before the data were analysed all trials in which the participant responded in less than 200ms, made an error, trials in which a target was not present and trials in which participants were not able to correctly recall the direction given at the beginning of the trial, were removed (a total of 8.6% of trials). Next an outlier removal procedure was performed whereby for any given cell of the design, any reaction time which was greater than 3SD away from the mean of that cell was removed (90 trials, 1.4%).

With the remaining data I performed a 2 (Load: Direction, No Direction) × 2 (Presentation Condition: FEB, PRE) × 3 (DS: 4, 8, 16) on participants' reaction time data. Significant main effects of load, $F(1,20)=5.82$, $MSE = 54574$, $p=.026$, $n^2=.226$, presentation condition, $F(1,20)=63.23$, $MSE = 33512$, $p < .001$, $n^2 = .760$, and display size, $F(1.25,24.91)=282.4$, $MSE = 25022$, $p < .001$, $n^2 = .934$, were found. Reaction times were on average higher in the load condition (M = 924.3ms, SE = 47.9ms) than the no load condition (M = 851.9ms, SE = 31.2ms), participants also took longer to respond as display size increased and participants responded faster in the PRE condition compared to the FEB condition. In addition, there was a significant interaction between Presentation Condition and Display size, $F(2,40)=36.37$, $MSE = 7715$, $p< .001$, $n^2 = .645$ (Figure 4.1). However, none of the other interactions were significant (All F's <= 1.94, *p's* >= .158).

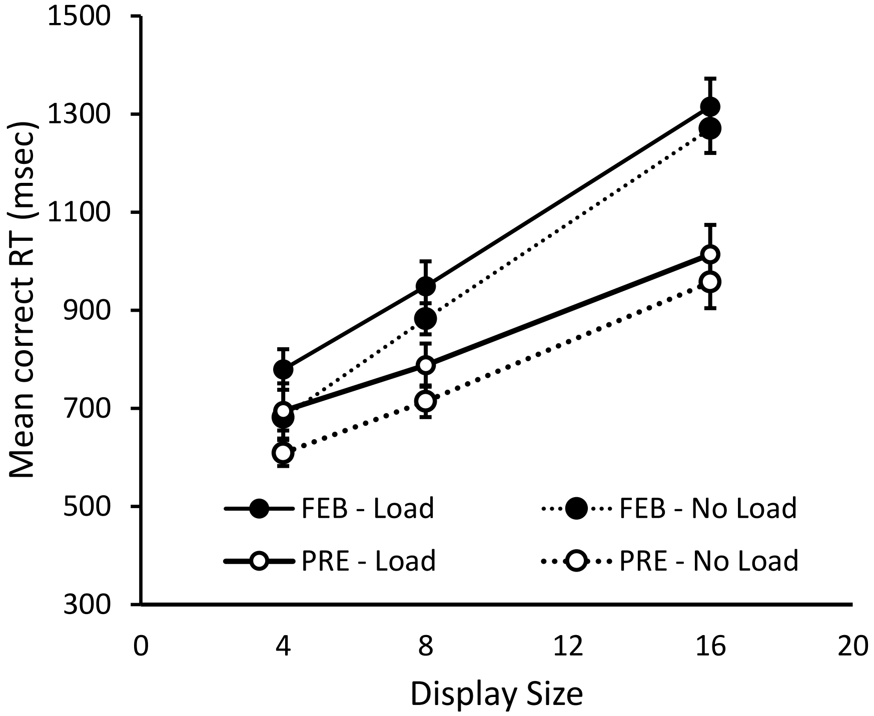

Figure 4.1 – Participants' mean correct reaction times as a function of the spatial load, presentation and display size conditions. Error bars show ±1SE.

I was also interested if the orientation of the arrow presented at the beginning of the trial would have an impact on participants' reaction times to find the target item. The data was recoded so that I could identify any trials in which the directional arrow was orientated in the same direction as the target appeared on the screen (e.g. a left arrow and the target appeared on the left), I termed these congruent trials. Incongruent trials consisted of trials in which the direction given was oriented opposite to the position of the target in the display. Trials in which the directional arrow was not oriented left or right were excluded from the analysis. I then compared the participants reaction times in congruent trials and incongruent trials using a 2 (Congruence: Congruent, Incongruent) × 2 (Presentation Condition: FEB, PRE) × 3 (Display Size: 4, 8, 16) repeated measures ANOVA. Significant main effects of congruence, $F(1,20) = 16.7$, $MSE = 25443$, $p < .001$, $n^2 = .454$, presentation condition, $F(1,20) = 46.7$, $MSE = 34415$, $p < .001$, $n^2 = .700$, and display size, $F(1.4, 28.0) = 282.4$, $MSE = 40108$, $p < .001$, $n^2 = .934$, were found (Figure 4.2). In addition a significant interaction between Presentation Condition and Display Size was found $F(2,40) = 36.4$, $MSE = 18761$, $p = .002$, $n^2 = .276$. However, no other interactions were found to be significant (all F's <= 1.33, *p's* >= .277). It is worth mentioning however that the Congruency by Presentation condition interaction narrowly missed significance $F(1,20) = 4.1$, $MSE = 5939$, $p = .056$, $n^2 = .170$.

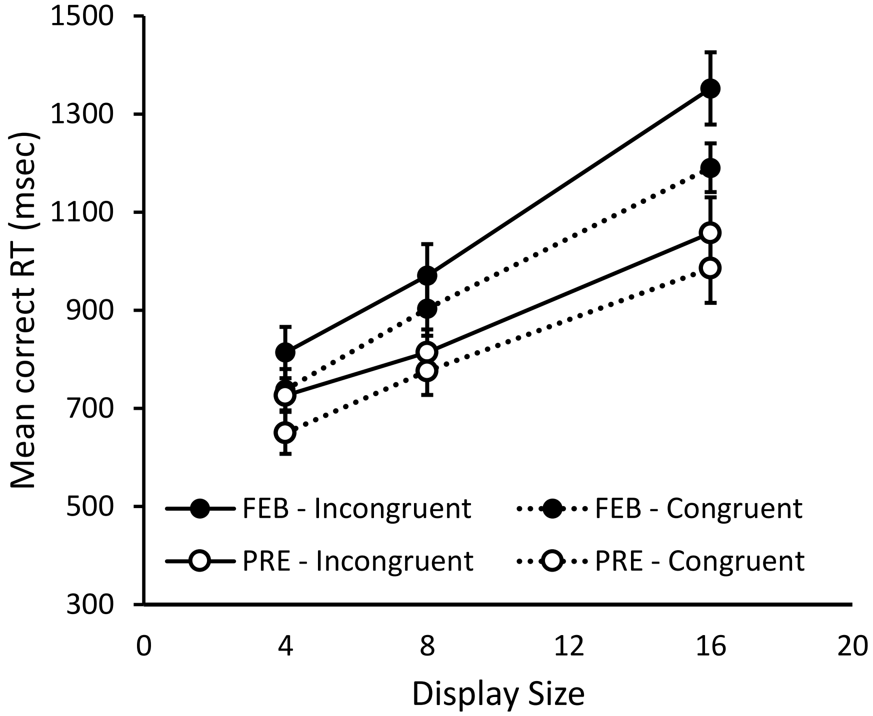

Figure 4.2 – Participants' mean correct reaction times as a function of the congruency, presentation and display size conditions. Note that only data from congruent and incongruent trials, as defined above, is included in this figure. Error bars show ±1SE.

Errors

The number of target location errors made by participants, on trials which would have otherwise been included in the main analysis detailed above, was calculated. Therefore, Target absent trials, trials with a reaction time <200ms, trials which were outliers and trials in which the participant made a direction recall error or did not respond correctly in the search task were not counted. Overall participants made an error on only 0.89% of trials and therefore the error data was not analysed any further (Table D.1).

On average, participants made an error on 5.6% of target absent trials. This indicates that participants were not simply guessing the targets location based on having searched only half of the visual display. No participants were removed from the analysis based on their performance in target absent trials.

In addition to the error types discussed above, it was also important to establish that participants were in fact paying attention to and attempting to memorise the spatial direction they were given at the beginning of each trial. Participants on average made an error on 1.1% of trials[3].

Discussion

Experiment 1 showed that our design was sensitive enough to detect a preview benefit, shown by the presentation condition by display size interaction. However what is also clear is that there was no effect of spatial load on the preview benefit. Participants showed a statistically similar effect of previewing a subset of stimuli in both the spatial load and no load conditions.

From a visual marking perspective, this is surprising as we might have expected that a process which is resource dependent (Watson & Humphreys, 1997;

[3] All trials in which a direction was given were included in the calculation of spatial errors.

Humphreys Watson & Jolicoeur, 2002) would be affected by a dual task load. Particularly, if that load relied on similar resources and was delivered in the same modality as the search task (Wickens 1980, 2002). Visual marking is proposed to be (Watson & Humphreys, 1997; Humphreys Watson & Jolicoeur, 2002) a visuo-spatial task which is resource and memory dependent. Our load task was designed to rely upon similar resources by requiring that participants remember a spatial direction for the duration of a trial. However, it is possible that as the direction was delivered both visually and via audio, that participants simply relied on the auditory cue to encode the spatial direction. This, according to Wickens (1980, 2002) would be less likely to interfere with the visual marking task. It may, therefore, simply be the case that the spatial load task was not taxing enough to impact the preview benefit. The fact that participants incorrectly recalled the spatial direction on only 1.1% of experimental trials indicates that this may indeed be the case.

This explanation finds support from previous work by Humphreys, Watson and Jolicoeur (2002) who showed that participants' performance on the preview task was affected when they were also required to monitor a stream of digits. When the digits were presented visually, the preview benefit was affected regardless of whether the digits were presented throughout the entire preview period or were only presented half way through the preview period. However, when the digits were delivered via audio the preview benefit was affected only when they were presented throughout the entire preview period. Therefore, to better determine whether naturalistic spatial load can interfere with the preview benefit I would need to remove the auditory cue in order to force participants to rely solely on the, potentially more distracting, visual cue.

I also hypothesised that participants may show an effect of target, spatial direction congruence. Posner (1980) in his classic work on the orienting of attention and cueing, showed that when participants are exposed to a cue, such as an arrow, which can indicate the likely position of the target in a search task. Participants show a benefit in reaction times when the cue is valid and a deficit when it is invalid. In my experiment the target could only appear on the left or on the right of the display and so when analysing my data to investigate congruence I only included trials in which the spatial cue was also oriented left or right. It should be noted that the trials I used were more complex in nature than the original experiments presented by Posner (1980) and in addition the cues in Experiment 1, if they indicated the left or right hand side of the screen, were approximately equally likely to be valid or invalid. Despite this, a main effect of congruency was found indicating that participants reacted faster in the search task, when the target appeared on the same side of the screen as the directional arrow indicated at the start of the trial.

Given the differences between the design of Experiment 1 and that used by Posner (1980), outlined above, it is perhaps surprising that an effect of congruence was found. This is especially true when it is considered that participants were explicitly told that the spatial load task had no relation at all to the search task. However, previous research has shown that information which we hold in working memory can, in some circumstances, impact upon the way in which we apply our visual attention to the world around us (Soto, Hodsoll, Rotshtein & Humphreys, 2008). In fact it has even been suggested that this occurs in a fairly automatic manner. As discussed in the introduction to this chapter, when participants were asked to remember a piece of visual or verbal information such as “red square”, their attention was guided towards a congruent object in a subsequent visual search task (Soto &

Humphreys, 2007). Therefore, it is possible that the spatial directions which were presented to participants in the current work acted to guide participants' attention to the corresponding half of the visual display. Using a different experimental design Flanagan, McAnally, Martin Meehan and Oldfield (1998) showed that visual attention can be guided equally well by both auditory and visual cues. If visual attention can indeed be guided in this way this could have important implications in the real world. For example, in the context of driving, simply following satellite navigation directions may impact on the way in which we attend to the world around us. Holding a direction in mind may focus our attention on the corresponding area of our visual field, whether we intend for that to happen or not, and as a result we may be able to more quickly identify the junction that we are looking for but, may be less likely to identify hazards which are located outside of the area in which our attention has been cued. Given the importance of understanding the congruency effect Experiments 2 and 4 of this chapter will examine this further.

It could be predicted that the preview benefit was not affected by spatial load in Experiment 1 because both auditory and visual directions were given. If, as the research by Humphreys Watson and Jolicoeur (2002) would predict, the auditory directions were interfering to a lesser extent with the set-up of visual marking than the visual directions, participants may have relied solely on the auditory directions to complete the task. In order to test this prediction Experiment 2 was performed in which participants received the directions visually but not via audio.

Overall effect of spatial load

While spatial load was not found to affect the preview benefit an overall effect on participant's reaction times was found. This mirrors the effect of conversation found in Chapter 2. However, what is surprising is the fact that naturalistic spatial load, which I would predict would be more taxing on the search task than naturalistic conversation, did not impact upon participants' search efficiency. This was indicated by the lack of a spatial load by display size interaction. Whereas in Chapter 2 I show that another naturalistic task, namely conversation does have an effect upon participant's search efficiency. This will be investigated further in Experiment 2 below.

EXPERIMENT 2

As no significant effect of Spatial Load was found on participant's ability to experience a preview benefit in Experiment 1 I attempted to investigate this further in Experiment 2. It is possible that the results of Experiment 1 were driven by the fact that both an auditory and visual cue were given to participants. Wicken's (1980) multiple resource model states that tasks are more likely to interact if they share the same modality and resources. In Experiment 1 participants may have ignored the visual spatial load in favour of the auditory cue as the auditory cue may not have interfered with the preview task to the same extent as a visual load. Research by Humphreys, Watson and Jolicoeur (2002) demonstrated that a distracting auditory dual task was only able to interfere with the preview benefit if the auditory load was delivered throughout the preview duration. Whereas, a visual dual task was able to disrupt the benefit if presented half way through the preview period or throughout the entire preview period. Therefore, in Experiment 2 I tested this possibility by eliminating the auditory cue, thereby forcing participants to rely solely on the visual cue for the spatial information.

Method

The methodology for Experiment 2 was identical to Experiment 1 except that the participants did not receive an auditory cue and so only received the direction visually. Nineteen University of Warwick non-academic staff and students took part in this experiment, the mean age was 23 and 10 were female. Participants confirmed that they could easily hear the auditory stimuli and see the visual display.

Results

Before the RTs were analysed trials in which a participant responded in <200ms were removed (0.06%), as were trials which resulted in an error (0.8%) and target absent trials. The same outlier removal procedure that was used in Experiment 1 was used again here in Experiment 2 resulting in a further 1.3% of trials being removed. Finally, trials in which the participant did not correctly recall the direction they were given at the beginning of a trial were removed. This resulted in 5.4% of spatial load trials being removed.

The remaining RT's were analysed using a 2(Spatial Load: Direction, No Direction) × 2(Presentation: FEB, PRE) × 3(Display Size: 4, 8, 16) repeated measures ANOVA. A main effect of Spatial Load was found, $F(1,18) = 20.6$, $MSE = 23519$, $p < .001$, $n^2 = .533$. Overall participants were slower to respond in the spatial load condition (M = 956ms, SE = 40) than in the no load condition (M = 864, SE = 40). A main effect of presentation condition was also found $F(1,18) = 59.1$, $p < .001$, $n^2 = .767$, reaction times were generally longer in the FEB condition (M = 984, SE = 42) relative to the PRE condition (M = 833, SE = 37). In addition, reaction times increased significantly as the number of items in the display was increased (DS: 4 M = 713, SE = 25, DS: 8 M = 867, SE = 39, DS 16 M = 1149, SE = 56), $F(1.2, 21.3) = 138.5$, $MSE = 45696$, $p < .001$, $n^2 = .885$. A significant interaction between Presentation Condition and Display Size was also found indicating that search efficiency was significantly different between the two groups, $F(2,36) = 15.1$, $MSE = 6020$, $p < .001$, $n^2 = .456$ (Figure 4.3). Search was more efficient in the PRE condition

(31ms per item) than the FEB condition (42ms per item). However, no other significant interactions were found (All F's <= .439).

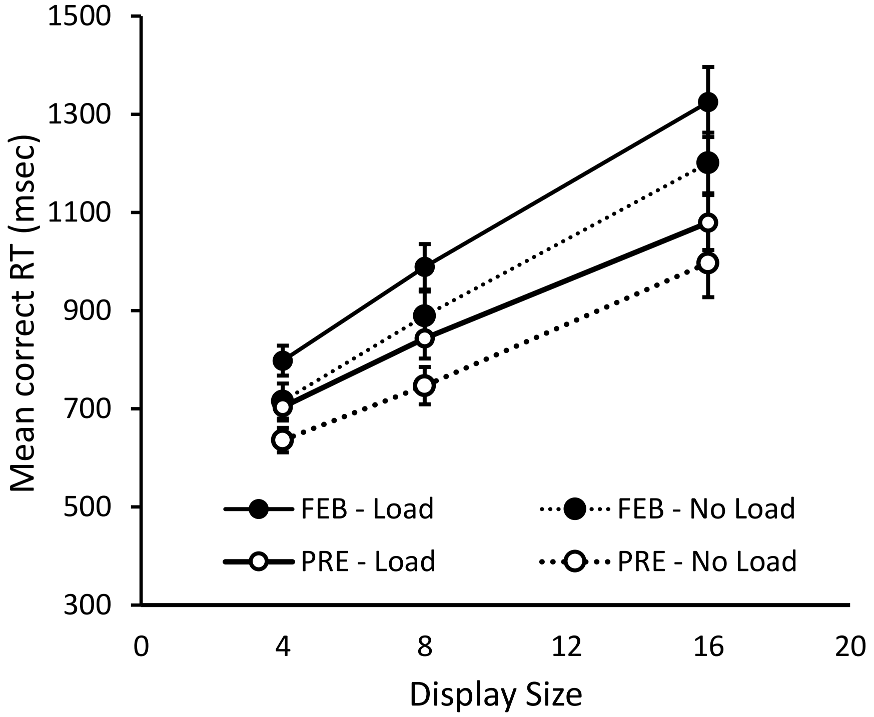

Figure 4.3 – Participants' mean correct reaction times as a function of the spatial load, presentation and display size conditions. Error bars show ±1SE.

As in Experiment 1, I was also interested if congruence between the direction given and the position of the target in the display would have an effect on participants' search rates for the target. I therefore recoded the data as in Experiment 1 and then compared reaction times in congruent trials and incongruent trials using a 2 (Congruence: Congruent, Incongruent) × 2 (Presentation Condition: FEB, PRE) × 3 (Display Size: 4, 8, 16) repeated measures ANOVA. Significant main effects of presentation condition, $F(1,18) = 24.7.2$, $MSE = 62426$, $p <.001$, $n^2 = .579$ and display size, $F(1.3,24.3) = 77.4$, $MSE = 75733$, $p <.001$, $n^2 = .785$ were found, but not of congruence $F(1,18) = 2.26$, $MSE = 28487$, $p =.150$, $n^2 = .112$ (Figure 4.4). A significant interaction between Condition and Display Size was found, $F(1.3, 23.9) = 6.81$, $MSE = 14924$, $p =.010$, $n^2 = .274$. However, none of the other interactions were significant (All F's <= 2.529, *p's* >= .114).

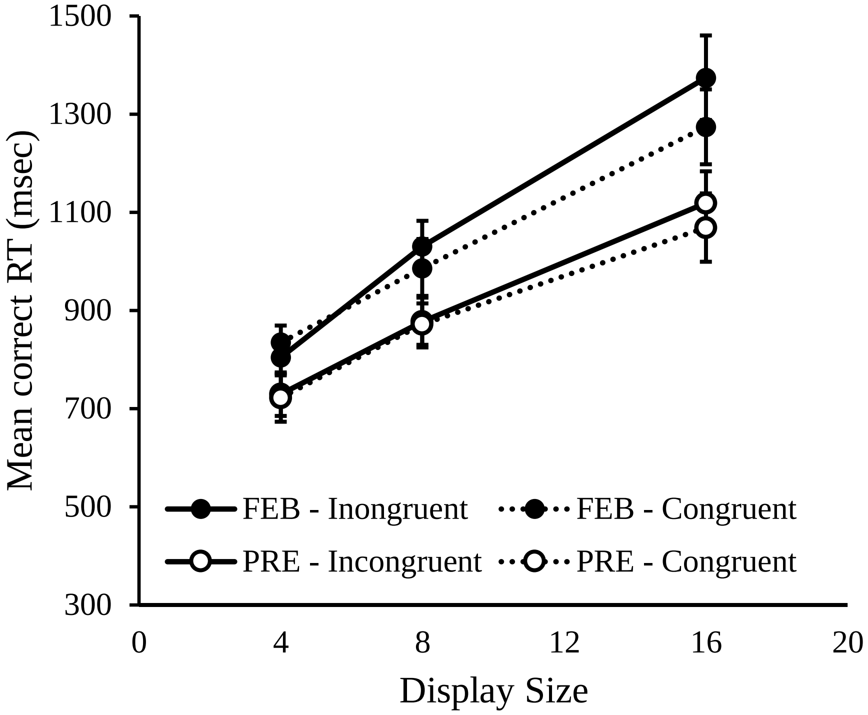

Figure 4.4 – Participants' mean correct reaction times as a function of the congruency, presentation and display size conditions. Note that only data from congruent and incongruent trials is included in this figure. Error bars show ±1SE.

A comparison between the observed congruency effect in Experiment 1 and 2

As an effect of congruency was found in Experiment 1 but not in Experiment 2 I investigated this difference further using a 2(Congruency: Congruent, Incongruent) × 2(Presentation Condition: FEB, PRE) × 3(Display Size: 4,8,16) mixed ANOVA with Experiment (1,2) as a between subjects factor. A main effect of congruence $F(1,38) = 15.1$, $MSE = 24576$, $p = .001$, $n^2 = .268$, presentation condition $F(1,38) = 44.4$, $MSE = 65698$, $p < .001$, $n^2 = .539$ and display size were found $F(1.4,53.2) = 180.0$, $MSE = 58855$, $p < .001$, $n^2 = .825$. A significant interaction between Presentation Condition and Display Size was also found $F(1.7, 62.7) = 15.0$, $MSE = 18785$, $p < .001$, $n^2 = .283$. In addition, the interaction between Congruency and Display Size narrowly missed significance $F(1.7, 62.8) = 2.87$, $MSE = 20552$, $p = .074$, $n^2 = .069$.

A significant between subjects effect of Experiment was not found $F(1,38) = 0.764$, $MSE = 555429$, $p = .388$, $n^2 = .020$. However the interaction between Experiment and Congruency $F(1,38) = 3.37$, $MSE = 24576$, $p = .074$, $n^2 = .060$, narrowly missed significance at the .05 level. Given that we could predict the direction of this effect from the previous analyses, there is some justification for considering this a directional test which would be significant. No other significant interactions were found (All F's <= 1.951, *p's* >= .171).

Errors

The number of target location errors made by participants, on trials which would have otherwise been included in the main analysis detailed above, was calculated as in Experiment 1. Overall, only 0.56% of trials in which a target was present resulted in an error (Table D.2). This provides too few data points to perform a reliable analysis and therefore this data was not analysed any further.

As before target absent trials were used solely to ensure that participants completed the search task as they were expected to. The number of errors made by participants on all target absent trials (except those where the reaction time was less than 200ms) was 4.2%. Therefore, it is reasonable to conclude that participants were searching through the display and only responding once they had either located the target or were convinced that a target was not present. No further analysis was performed on this data.

In addition, participants in general were able to correctly state which arrow they had been presented with at the beginning of each trial. Only 5.4% of trials resulted in an error.

Discussion

The analysis of the data from this experiment showed a clear preview benefit, with improved search rates in the preview condition relative to the FEB condition. In addition, it is evident that spatial load in the form of remembering the orientation of an arrow, had the effect of increasing the time taken for participants to find the target in the search task. However, the results show no effect of spatial load on the preview benefit itself.

The reason for this may be that the spatial load applied did not interact with the preview benefit because the preview benefit is purely driven by bottom up factors. However, given the wealth of evidence suggesting that top down factors play a role in generating the preview benefit this does not seem likely (e.g. Kunar & Humphreys, 2006; von Mühlenen, Watson & Gunnell, 2013; Watson & Humphreys, 2000). Alternatively, as in Experiment 1, the load may not have been sufficient to attenuate the preview benefit. However, participants made approximately five times as many spatial errors in Experiment 2 as in the first experiment. While this is indicative that the participants were finding the additional load task more challenging. It is possible that it was still not sufficient enough load to cause attenuation of the preview benefit. It must also be considered that the participants may have been re-encoding the visual directions so that they were being stored verbally. If this is the case then this may explain why the visual directions did not have an effect on the preview benefit. Spatial resources may have been required to initially process the directions and then re-encode them. However, maintaining the directions verbally in memory is unlikely to have required spatial resources. It may, therefore, be the case that participants may be bypassing the need for spatial resources to be loaded while they completed the search task and as such participants' ability to generate a spatial representation of old items (Watson, Humphreys & Olivers, 2003) was unimpeded.

In addition, I also failed to find any effect of the congruency of the target position and the orientation of the arrow presented at the beginning of the trial. This indicates that unlike in Experiment 1 the participants were not being primed to begin their search in the spatial direction indicated by the arrows. This is an interesting finding as previous research has demonstrated that a visual cue alone is sufficient to impact upon the way in which participants visually attend to a display (Soto et al., 2005). This will be discussed further in the General Discussion below.

Experiment 3 addresses whether increasing the number of directions which participants were required to remember on each trial is able to induce an effect on the preview benefit.

EXPERIMENT 3

The next logical step was to increase the complexity of the spatial load task in the hope that any attentional limitations would be more pronounced and therefore easier to detect (Kahneman, 1973; Norman & Bobrow, 1975). Experiment 3 differs from Experiment 2 in one key aspect. Participants were required to remember three directional arrows presented to them at the beginning of each trial. In this way it was possible to investigate if the preview benefit would be attenuated by an increased level of spatial load.

Method

Participants

30 participants took part with a mean age of 21 and 17 were female. Participants confirmed that they could easily hear the auditory stimuli and see the visual display. One participant's data was removed for non-compliance with the task instructions leaving 29 participants whose data were suitable for analysis. The sample size was increased in this experiment as it was difficult to anticipate how participants would perform on the now considerably more challenging, spatial load task. As error rates could have increased a great deal in this final experiment it was conceivable that it would be necessary to remove more participant's data prior to our analysis.

Equipment and materials

The same equipment materials and stimuli that were used in Experiment 2 were used again in Experiment 3.

Procedure

Experiment 3 followed closely the methodology of Experiment 2 except for the increased number of directional arrows presented to the participants. However, one key change was instigated, as it was noted that a programming error had caused a minor error in the proportions of various direction arrows in Experiments 1 and 2 (see the method section of Experiment 1 for details), this was rectified in Experiment 3. Participants were required to remember the orientation and the order of three arrows which were presented one above the other in a line down the centre of the display. The arrows were equally spaced apart. The first arrow was presented in between the fixation point and the top of the screen, the second arrow was presented at the fixation point and the third arrow was presented between the fixation and the bottom of the screen. The directions were presented to participants for 2 seconds. After the search trial participants were asked to state which directions they were given at the beginning of the trial. The three possible arrows were presented and the participant was asked to select which arrow was at the top of the display, after they made a selection they were asked to do the same for the central arrow and the bottom arrow.

Experimental blocks consisted of 90 trials, 24 of which were target absent trials. The remaining 66 trials in each block were equally split between the display size conditions. In the spatial load condition three random directions were chosen at the beginning of each trial from the following options “Ahead”, “Left”, “Right”. At the beginning of each trial three arrows were chosen at random, with replacement, from the three possible orientations.

Results

As in Experiment 1 the data were cleaned prior to analysis. All reaction times which were less than 200ms were removed (0.1%), target absent trials and trials where the participants made an error were removed (2.0%). In addition, all trials where the participant did not correctly recall the orientation of all of the direction arrows and in the correct order were removed (6.4%). All outliers which fell 3 standard deviations away from the mean for each cell in the design were removed (1.1%). One participant was removed prior to all analyses. This was due to the fact that 77% of their target absent trials resulted in an error and in addition they made an error 39% of the time when asked which directions they were given at the start of a trial.

As before a 2 (Spatial Load: Directions given, No directions given) × 2 (Presentation Condition: FEB, PRE) × 3 (Display Size: 4, 8, 16) repeated measures ANOVA was performed on participants' reaction time data. The results mimic those found in Experiment 2. Main effects of spatial load, $F(1,27) = 15.6$, $MSE = 80644$, $p < .001$, $n^2 = .366$, presentation condition, $F(1,27) = 69.9$, $MSE = 35955$, $p < .001$, $n^2 = .721$ and display size, $F(1.2,32.0) = 210.0$, $MSE = 26556$, $p < .001$, $n^2 = .886$, were found (Figure 4.5). In addition there was a significant interaction between presentation condition and display size, $F(1.6,42.8) = 29.2$, $MSE = 7489$, $p < .001$, $n^2 = .519$. No other main effects or interactions were significant (All Fs <= 0.809). Please note, Experiment 3 aimed to investigate if increasing the difficulty of the spatial load task affected the preview benefit. Emphasising load in this experiment by increasing the number of directional arrows resulted in a design which did not easily lend itself to an analysis of congruence. As such, a congruency analysis was not performed.

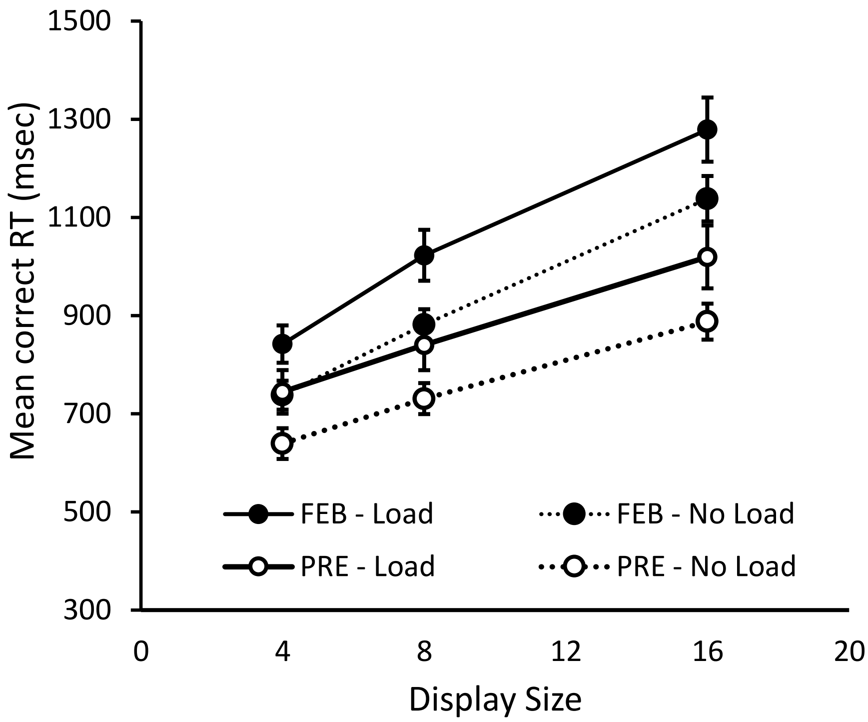

Figure 4.5 – Participants' mean correct reaction times as a function of the spatial load, presentation and display size conditions. Error bars show ±1SE.

Errors

The same procedure used in Experiment 2 was again used here. Overall 1.8% of trials resulted in an error (Table D.3). This again provided too few data points to perform a reliable analysis and therefore these data were not analysed any further. On average 2.8% of target absent trials resulted in an error. This again indicates that it is reasonable to conclude that participants were searching through the display and only responding once they had either located the target or were convinced that a target was not present. No further analysis was performed on this data.

In addition to the search errors I was also interested in how many errors participants made when asked to recall the three arrow orientations they were given at the beginning of each trial. An error was recorded if participants did not correctly recall all three orientations, in the correct order. On average participants made 6.4% recall errors and as such I can conclude that participants were not simply guessing the orientation of the arrows.

Discussion

The results of Experiment 3 mimicked those of Experiment 2. This was not expected as it was predicted that by increasing the spatial load upon participants while they performed the preview task would cause an attenuation of the preview benefit. This is certainly what could have been predicted based on previous research which has examined preview performance under different load conditions (Humphreys, Watson and Jolicoeur, 2002). An explanation for this is that my efforts to increase the complexity of the task did not result in a sufficient increase in spatial load. Therefore, participants were able to inhibit old items and search only through new items in the display. It must also be considered that it is possible that the load task was not inducing spatial load at all, in fact it is possible that participants were simply re-encoding the direction stimuli verbally. In either case, it is reasonable to assume that the stimuli would be stored in working memory. However, as visual marking is hypothesised to also be dependent on memory (Watson & Humphreys, 1997) we would also expect that a memory load would have the potential to impact upon the preview benefit.

EXPERIMENT 4

Given that an effect of congruency has only been found when both auditory and visual directions were used, in Experiment 4 I investigated this further by only delivering the directions via audio. The methodology was identical to that used in Experiment 2 except that directions were delivered via audio (in the same manner as in Experiment 1) and not visually. 20 participants took part in this experiment in return for course credit, with a mean age of 18.6 and 18 were female. Participants confirmed that they could easily hear the auditory stimuli and see the visual display.

Results

As in Experiment 1 the RT data were cleaned prior to analysis. Trials in which the participant responded in <200ms were removed (0.03%), as were trials which resulted in an error (1.4%) and all target absent trials. Next the outlier removal procedure described in Experiment 1 was performed this resulted in 1.2% of trials being removed. In addition, all trials in which the participant did not correctly recall the spatial direction were removed (1.4%).

As before RTs were then analysed using a 2(Spatial Load: Direction, No Direction) × 2(Presentation: FEB, PRE) × 3(Display Size: 4, 8, 16) repeated measures ANOVA. The results mirror those of the previous three experiments. As before, participants responded more slowly in the spatial load condition (M = 884ms, SE = 32ms) than in the no load condition (M = 806ms, SE = 34ms), $F(1,19) = 15.0$, MSE =

24408, p = .001, n^2 = .441. Again, reaction times were generally longer in the FEB condition (M = 944ms, SE = 39ms) relative to the PRE condition (M = 746ms, SE = 27ms), $F(1,19) = 68.6$, $p <.001$, n^2 = .783, and reactions times increased as display size increased (DS: 4 M = 659ms, SE = 22ms, DS: 8 M = 795ms, SE = 30ms, DS 16 M = 1085ms, SE = 47ms), $F(1.2, 22.6) = 163.5$, MSE = 38958, $p <.001$, n^2 = .896 (Figure 4.6). Search was also significantly more efficient in the PRE condition (28ms per item) relative to the FEB condition (43ms per item) as indicated by the interaction between Presentation Condition and Display Size. $F(2,38) = 39.3$, MSE = 4289, $p <.001$, n^2 = .674. No other significant interactions were found (All F's <= 1.3, *ps* =< .284).

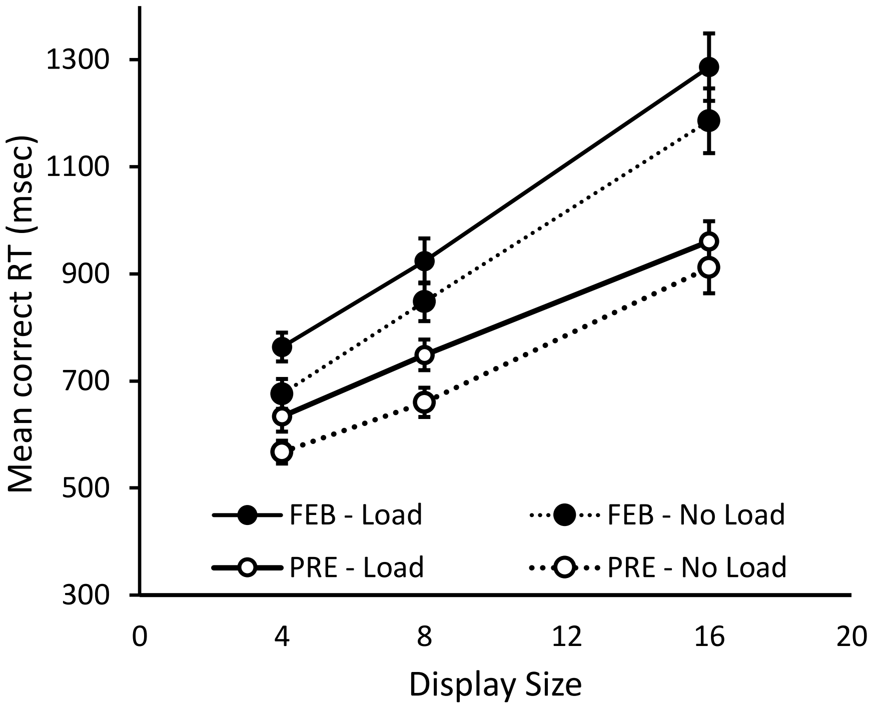

Figure 4.6 – Participants' mean correct reaction times as a function of the spatial load, presentation and display size conditions. Error bars show ±1SE.

A congruency analysis was then performed as in Experiments 1 and 2, main effects of congruency, $F(1,19) = 4.95$, $MSE = 35711$, $p = .038$, $n^2 = .207$, presentation condition, $F(1,19) = 46.8$, $MSE = 57017$, $p < .001$, $n^2 = .711$, and display size, $F(1.4,26.0) = 189.2$, $MSE = 31053$, $p < .001$, $n^2 = .909$, were found (Figure 4.7). On average, participants were significantly faster when directional arrows were congruent with the search tasks target position (M = 864ms, SE = 31ms) than when they were incongruent (M = 921ms, SE = 38ms). A significant interaction was also found between the Presentation Condition and Display Size, $F(1.5,28.8) = 17.5$, $MSE = 19528$, $p < .001$, $n^2 = .479$. No other interactions were significant (All Fs <= 1.98, *ps* >= .176).

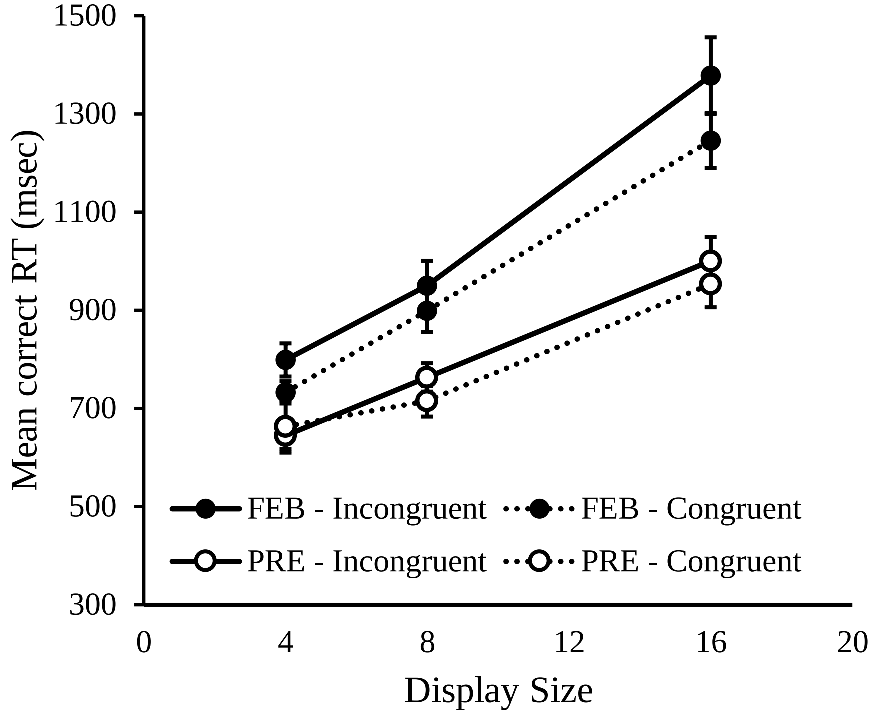

Figure 4.7 – Participants' mean correct reaction times as a function of the congruency, presentation and display size conditions. Note that only data from congruent and incongruent trials, as defined above, is included in this figure. Error bars show ±1SE.

Errors

Errors were calculated as in the previous three experiments. Overall 1.1% of trials resulted in an error (Table D.4). As before this provided too few data points to perform a reliable analysis and therefore these data were not analysed any further. On average 6.0% of target absent trials resulted in an error, indicating that participants were not only searching through half of the display. No further analysis was performed on these data. In addition, participants made on average 1.4% direction recall errors and as such we can conclude that participants were not simply guessing the orientation of the arrows.

Comparing the Direction Recall Errors from Experiments 1, 2 and 4

An interesting pattern emerged between Experiments 1 to 4. Participants appeared to make fewer recall errors when the directions were delivered via audio as compared to when they were delivered visually. In order to test if these differences between experiments was significant I performed 3 separate independent samples t-tests comparing the percentage error data from experiments 1 (M = 1.1%, SE = 0.33%), 2 (M = 5.4%, SE = 1.30%) and 4 (M = 1.4%, SE = 0.23%). The percentage errors made in Experiment 2 (visual directions) differed significantly from the percentage errors made in both Experiment 1 (visual and audio), $t(20.4)=3.194$, $p = .004$, $d = 1.033$, and Experiment 4 (audio only), $t(19.2)=3.043$, $p = .007$, $d = .987$. Whereas, Experiments 1 and 4 did not differ significantly from each other, $t(39)=0.652$, $p = .518$, $d = .205$. Overall participants made approximately 4 times as many errors when only visual directions were presented.

Discussion

As in Experiments 1-3 no effect of spatial load was found on the preview benefit. This is not surprising given the fact that auditory load has been shown to interfere less with the preview benefit than visual load (Humphreys et al., 2002). Therefore, as no effect of spatial load was found when the load was delivered visually or when it was delivered visually and via audio, it could have been predicted that using audio alone would not affect the preview benefit.

However, the main aim of Experiment 4 was not to establish if spatial load delivered via audio can affect the preview benefit. Rather it was performed to investigate whether delivering the directions via audio resulted in the directions impacting upon subsequent search processes or whether a combination of visual and auditory directions is necessary to achieve this, as in Experiment 1. It could have been predicted that using both auditory and visual cues ensured that the cues were reinforced and therefore processed to a greater extent than when visual cues alone were used. This then may have resulted in the congruency effect observed in Experiment 1. Alternatively, it may be that delivering the directions via audio is sufficient for a congruency effect to be observed. Experiment 4 was able to differentiate between these hypotheses.

A significant effect of congruency was found in Experiment 4 indicating that holding the auditory directions in memory impacted upon subsequent search processes. Previous research has shown auditory information can be used to guide visual attention (Flanagan, McAnally, Martin Meehan & Oldfield, 1998; Ho, & Spence, 2005) and that this auditory information and then subsequent visual information from the search task may be integrated within occipito-parietal regions and then used to guide visual attention (Fu et al., 2001). While it is not possible to conclude from Experiments 1-4 the exact mechanism as to why auditory cues were able to impact upon subsequent search processes and why visual cues were not, it is an important and interesting finding. One possible explanation is that the auditory cues were encoded verbally but that the visual cues were not. Soto and Humphreys (2007) demonstrated that remembering verbal cues in the form of written descriptions, impacted upon subsequent visual search. If the auditory directions were encoded verbally but the visual directions were stored in a different manner, then this may account for the differences observed in Experiments 1, 2 and 4.

Alternatively, identifying the visual cue, and then encoding this visual information may have required similar resources to those required by the subsequent search task. It is possible then, that the processing of verbal cues required resources which were also necessary for the successful completion of the search task and as such, the search task itself was prioritised. This may have resulted in the visual directions not reaching a high enough level of processing to guide participants' subsequent search behaviour.

Regardless of the underlying mechanisms, these findings have important implications in the real world. For example, satellite navigation devices usually deliver instructions both visually and via audio. Intuitively we may choose to use the auditory information over the visual guidance as it may appear to be less distracting and because we are then able to keep our eyes on the road. However, Experiments 1-4 have shown that delivering directions via audio may have a surprising effect on the way in which we go on to search the world around us. If we are told to turn right and are remembering this piece of information, then we may show a bias towards our right hand visual field and then potentially miss important visual information on our left.

General Discussion

While not a direct challenge to the theory of visual marking it is interesting to note that certain load conditions appear to have little to no impact on the preview benefit. This is surprising given the copious number of studies which add evidence in support of visual marking relying on resources and memory. For example, those studies showing that an adequate preview duration is necessary to ensure a preview benefit (Humphreys et al., 2004; Humphreys, Olivers & Braithwaite, 2006; Warner & Jackson, 2009; Watson & Humphreys, 1997); and that additional load can impact upon participants' ability to benefit when a subset of stimuli are presented before the onset of the full set (Humphreys, Watson & Jolicoeur, 2002).

I suggest three possible explanations as to why the additional load task in our experiments did not affect the preview benefit. First it cannot be overlooked that the load task did not affect the preview benefit because the preview benefit can be generated purely from bottom up mechanisms, as suggested by Donk and Theeuwes (2001, 2003). However, given the wealth of evidence which suggests that the benefit derives from top down mechanisms (see above for examples) this seems unlikely.

Second, the preview benefit may not have been affected by spatial load because the load task utilised separate resources to those required by the preview benefit. However, this explanation also seems unlikely given that, according to the theory of visual marking, top down inhibition of old items is resource and memory dependent. Even if it were not the case that visuo-spatial resources are necessary for producing a preview benefit, the task relies on memory and as such a dual task which loads memory resources could be predicted to impact upon it (Watson & Humphreys, 1997). Humphreys et al., (2002) also showed that simply monitoring a stream of digits while performing a preview search task was able to impact upon the preview benefit, particularly if the load is delivered via the visual modality.

However, there are several key differences between the study performed by Humphreys et al., (2002) and the work presented in this chapter. Humphreys et al., (2002) used a load task which required participants to monitor a stream of digits while concurrently inhibiting the preview stimuli. This load task would likely have required resources throughout the entire preview period. In contrast, in Experiments 1-4 participants were simply required to attend to the spatial directions once and then commit them to memory. In addition to this it is possible that participants were re-encoding the spatial directions verbally. If so the resources required to hold these verbal cues in memory may not overlap with the resources required to benefit from previewing a subset of stimuli. This then may account for why a preview benefit was observed in all of Experiments 1-4. Tasks which place high demands on attention have the potential to interfere with visual marking, however, at least in these experiments it appears as though tasks which specifically load memory resources do not.

Finally, it may simply be the case that my spatial load task was not sufficiently taxing enough to divert participants' attentional resources away from the preview search task. I attempted to address this by increasing the difficulty of the spatial load task, first by forcing participants to rely on visual resources to perform the spatial load task and then by increasing the number of directions which must be remembered. However, while, numerically, participants' error rates increased as the load task was made more taxing, still no effect on the preview benefit was found.

However, as with naturalistic conversation in Chapters 2 and 3, an overall effect of the additional load task was found. Participants were significantly slower overall. However, unlike naturalistic conversation, the spatial load task did not impact upon participants' overall search efficiency. The displays used in this experiment are highly simplistic when compared to the visual scenes in the real world. As such the finding that search efficiency is not impeded by tasks akin to my spatial load task implies that the deficit in participants' reaction time will not get worse as the scenes become more and more complex. This is reassuring when taken in a real world context such as driving with a satellite navigation device, for example.

The effect of target-direction congruency

The experiments presented here are also able to add to the literature around the influence of items held in working memory on visual attention. The results of Experiment 1 are consistent with research which shows that if we are asked to hold information in memory while performing a visual attention task, our performance on the task can be affected (for a review see, Soto et al., 2008). For example, Soto et al., (2005) and Soto and Humphreys (2007), showed that holding a particular colour and shape in mind can guide attention to congruent items within a visual search display. What the experiments in this chapter have demonstrated is that holding spatial directions in memory, which do not specify features of the target but rather indicate its general position on the screen, are also able to guide attention. What is important here is that I only found evidence of my spatial directions guiding attention when they were delivered via audio alone or via both an auditory and a visual medium. Soto and Humphreys (2007) demonstrated that visual and verbal (as in written descriptions) were able to guide attention, however in my experiments when visual cues alone were used congruence did not impact upon performance.

A possible explanation for this is that the auditory channel required fewer resources to monitor than the visual channel. As such, in Experiment 1 participants were able to rely solely on the auditory channel to process the spatial direction. This auditory information was then able to be processed to a greater extent than visual information as more of the necessary resources were available to complete the processing of this information. The finding that auditory information can be used to guide visual attention is not unusual (Flanagan, McAnally, Martin Meehan &

Oldfield, 1998; Ho, & Spence, 2005) and indeed specific to this context it has been shown that information delivered via different sensory modalities can be integrated within occipito-parietal regions and this information can then be used to guide visual attention (Fu et al., 2001). Humphreys, Watson and Jolicoeur (2002) showed that a secondary task delivered via audio was only able to affect the preview benefit if the task was delivered throughout the entire preview duration. However, the auditory task was not able to affect the preview benefit if delivered half way through the preview duration. Whereas, a visual secondary task, caused attenuation of the preview benefit regardless of at which point during the preview duration it was presented. This fits with what may be expected from Wickens' (1980, 2002) multiple resource model, which would predict that a secondary task which overlaps to a greater extent with the modality and resources required to complete the primary task would cause a greater impairment of performance. The auditory directions in Experiments 1 and 4 may have been able to be processed in parallel to the search task or were encoded in a different way to the visual directions and were therefore able to affect performance to a greater extent than the visual directions alone. Whereas when the directions were delivered visually, both the spatial load task and the search task required overlapping resources and so it is possible that a bottleneck was reached (Pashler, 1994) whereby one task was given priority over the other. Therefore, if the search task was given priority then the spatial load task may not have reached the same level of processing as when auditory directions were given, resulting in a reduced effect of congruency. I would also expect then that a larger number of errors would be made when visual directions are given in the absence of auditory directions. This is indeed what was found, participants made approximately 5 times as many errors in Experiment 2 compared to Experiment 1 and 4 times as many errors as in Experiment 4. However, it is interesting to note that Mondor and Amirault (1998) showed that cues that are delivered in the same modality as the target in a target identification task show a greater cue validity effect than those cues that are delivered via a different modality.

What is clear is that the results of Experiments 1-4 demonstrate that the effect of auditory processing on participants' visual attention cannot and must not be overlooked. Dalton, Agarwal, Fraenkel, Baichoo and Masry (2013), in a recent study, investigated the effects of delivering Satellite Navigation instructions via audio on simulated driving behaviour. They found that complex instructions significantly impacted upon several driving performance measures and that memory for directional information was improved when it was presented via audio rather than as visual arrows. They conclude that the use of audio to deliver navigational instructions is likely to be inducing a significant demand upon drivers. The experiments presented in this chapter have demonstrated that even simplistic instructions that involve a spatial element, have the potential to impact on the way in which we allocate our attention in the world.

Conclusion

The main aim of this work was to investigate the effect that a naturalistic spatial load task may have upon participants' ability to benefit from previewing a subset of items in a search task. The data show that the preview benefit was immune to the additional load applied by our spatial direction task. However, I was able to demonstrate that, as with naturalistic conversation, the load task did have an overall, significant, impact on the time taken for participants to find and respond to the target in the display. This finding has implications within the real world, for example within the realms of driving behaviour. Satellite navigation systems are becoming common place to the extent that they are built into cars, available as smart phone applications and can be purchased as stand-alone gadgets. However, the work which I present here indicates that there are inherent safety implications that come with their use. It is reassuring that holding directions in memory does not appear to affect our ability to benefit from key attentional mechanisms such as the preview benefit. It should however not be over looked that any task which negatively impacts upon drivers' reaction times can have severe, if not fatal implications.

Finally, as discussed above these results add to the literature around the effect of holding information in working memory whilst performing a visual task. I have shown that my spatial cues were able to guide participants' attention, seemingly automatically. However, this was only found to be the case when the cues were delivered via audio or via both an auditory and visual medium. At least in the experiments presented here, presenting the cues visually was not enough to invoke an effect on the guidance of visual attention. This is perhaps due to the fact that visual cues required overlapping resources to those required for completion of the search task and therefore as a result the cue was not processed to the same extent and so was not able to guide attention. Alternatively, the auditory directions may have been encoded in a different manner to the visual directions. If auditory cues were encoded verbally then they may have been able to influence subsequent search processes, as was demonstrated by Soto and Humphreys (2007). Whereas visual cues may have been stored in a different manner, one which was not able to interact with subsequent search performance.

Chapters 2-4 allowed for the investigation of how the top down guidance of attention and memory in visual search may be affected by naturalistic tasks which are likely to be performed whilst driving. These results are encouraging as they provide evidence that people who are talking on their mobile phone or following satellite navigation directions are still able to benefit from previewing visual information. In addition, even whilst talking on a mobile phone participants were able to implicitly learn and then express spatial contexts. However, what should be noted is that a very clear overall effect of conversation on participants' reaction times was found. Holding a conversation and processing satellite navigation directions resulted in slower reaction times and of key importance in the case of naturalistic conversation, this was compounded by the displays complexity. This suggests that the effect of conversation would be exacerbated as visual scenes become more and more complex. Given the simplistic nature of the scenes used in these experiments and the relatively complex nature of natural visual scenes this finding has very clear and potentially dangerous implications.

Having investigated the top down guiding of attention and aspects of memory in visual search I next turned my attention to the three fundamental attentional networks, the alerting and orienting responses and executive control of attention.

Chapter 5:

Naturalistic conversation and its effect on the three attentional networks

The ability to search through a visual scene efficiently and effectively is a vital function that has implications for the most primal hunter to the most advanced fighter jet pilot. As discussed in previous chapters, much of the research in this field has focused on single task paradigms in which a participant is required to look for a target item within a field of distractors (e.g., Chung & Jiang, 1998; Kunar & Watson, 2011; Wolfe, 1998; Wolfe, Brunelli, Rubinstein & Horowitz, 2013; Treisman & Gelade, 1980). However, search in the real world is rarely this focused. Multiple tasks constantly compete for attentional resources and our attention may need to be split in order to perform multiple tasks simultaneously. Such situations can be as complex as an air traffic controller being required to track multiple aircraft whilst relaying important information; or as simple as listening to music whilst cooking.

Dissecting the effect of naturalistic conversation on a complex every-day task such as driving is a daunting task. However, we can use lab-based studies to examine individual aspects of the driving task rather than the task as a whole. Attending to one's surroundings is clearly a major part of driving, and previous research (e.g., Kunar et al., 2008; Shinohara, Nakamura, Tatsuta, & Iba, 2010; Strayer Cooper & Drews 2004; Strayer & Drews, 2007) has shown that conversation can impair our ability to do this. However, although these studies show the overall detrimental influence of conversation on visual performance, they do so at a relatively global/coarse level. In contrast, in the current study I examined the influence of conversation on three elemental, distinct and fundamental processes involved in the control of visual attention.

It has been proposed that there are three distinct networks which are involved in applying and maintaining attention. Each of these attentional networks has a different primary function (Petersen & Posner, 2012; Posner & Petersen 1990). The first is the *alerting* network, which responds and maintains a state of readiness. This network allows us to become "alerted" to the fact that our attention is required and then maintains a high level of attention in order to perform a task efficiently. The second network is the *orienting* network, which focuses on the prioritisation of inputs, directing attention to, for example, the appropriate sensory modality or spatial location. This is necessary because given resource and capacity limitations, it is important to be able to allocate our attention to specific areas of the visual field which may be of interest and not waste time and resources searching irrelevant areas or locations. Finally, the *executive* network is involved in target selection, error detection, and conflict resolution. This network ensures that our visual system is able to filter out conflicting information and establish if visual inputs are important for the completion of a task or if they should be ignored. For reviews of these three attentional networks see Petersen and Posner (2012), Posner (2008) and Fan, McCandliss, Fossella, Flombaum, and Posner (2005).

Fan et al., (2002) developed the Attention Network Task (ANT) to examine the three proposed attentional networks. The ANT has several conditions each designed to demonstrate and assess the activity of one of the three networks. Some aspects of ANT are constant across all conditions. For example, a target arrow oriented either 90 degrees clockwise or anticlockwise from north, will always be presented flanked by two distractors on either side. On each trial, participants indicate the direction of the central arrow by pressing one of two response keys. Several aspects of ANT trials are manipulated in order to examine the operation of a specific attentional network. For example, the executive control network is examined by manipulating the identity of the flankers. In a typical ANT, in one third of trials the flankers are congruent with the direction of the central target arrow, in a further third of trials the flankers are incongruent with the target and in the remaining third they are neutral. These manipulations allow the measurement of a participant's ability to filter out irrelevant information and to resist making competing responses driven by the irrelevant flankers (see Eriksen, 1995, for a review of similar "Flanker Task" designs). The influence and operation of the *executive control network* on attention can be observed by comparing RTs and error rates across these flanker conditions.

On each trial the target is also preceded by one of four different visual cues. These cue conditions, when compared, allow for the investigation of the effects of the participants' orienting and alerting networks on their attentional control. In the *double* cue condition, a cue appears both above and below fixation at the two possible locations that the target can appear. Participant responses on double cue trials are compared to those on trials in which there was not a cue. This allows the involvement of the *alerting network* to be measured and hence the ability of participants to maintain an alerted state of attention (Posner, 2008).

The *orienting network* is investigated in a similar way by comparing performance on trials containing a central (non-target) cue with those on which the cue indicates the likely target location. Fan et al., (2002) suggest that comparing these two cue conditions allows for the participant's ability to orient attention to a cued location to be measured. A participant who is able to orient their attention effectively, should produce faster responses in conditions in which the target location is cued relative to conditions in which the display centre is cued. Thus in summary, differences across ANT trials allows three separate aspects (alerting, orienting and executive control) of attentional control to be assessed.

How might conversation impact the three attentional networks?

The main aim of this chapter was to investigate the effect of conversation on the three attentional networks. This was achieved using the ANT in order to allow us to determine in much greater detail and specificity the reasons why and how conversation influences attentional performance than has been possible in previous studies which used more global tasks. It is not easy to predict what the effect of naturalistic conversation will be on the three attentional networks. The first possibility is that the attentional networks will not be affected at all and participants will show just as strong an alerting, orienting and executive control effect in both conversation and no conversation conditions. However, given that previous research has shown that conversation can impair attentional performance (Shinohara, et al., 2010; Kunar, et al., 2008), albeit with more global/complex tasks, this possibility seems unlikely. Previous findings would, however, predict that overall reaction times in the conversation condition would be slower than in the no conversation condition.

Given that it is thought that the attentional networks are distinct from each other and rely on different underlying systems (Petersen & Posner, 2012), it is also possible that each attentional network could be affected differently by conversation. As discussed earlier, certain stimuli may capture our attention involuntarily (Remington, Johnston, & Yantis, 1992; Theeuwes, 1994), or whilst our attention is engaged elsewhere (Conway, Cowan, & Bunting, 2001; Moray, 1959), therefore it might be expected that alerting cues will be effective even under conversation conditions. What is unclear is whether we would expect an increase in the alerting response when participants are conversing, a decrease, or no difference between the conversation conditions.

We might expect that participants would show a greater alerting effect in the conversation condition than the no conversation condition as the cue may act to alert participants to the fact that they should be concentrating on the visual attention task and not on the conversation. That is, if response times to stimuli are slower when a participant is conversing (Shinohara et al., 2010; Kunar et al., 2008) then the alerting cue may cause the participant to refocus their attention away from the conversation and apply it to the visual task. Alternatively, the act of conversing with the experimenter may result in the alerting cue not producing as strong an alerting response in the conversation condition simply because less attention is being paid to visual inputs (Strayer, Cooper, & Drews, 2004) and as such the alerting network is not activated to the same extent. Finally, the cue may be observed and processed equivalently in both the conversation and no conversation conditions as might be expected from other visual attention results in which the task was completed successfully but a general slowing was observed (Spence et al., 2013, Shinohara et al., 2010). Therefore, we may find a general increase in reaction times but that the underlying alerting response (Posner, 2008) is still present.

Similarly, the orienting network may not be as effective in the conversation condition because the cue might be missed or not processed to the same extent, or the network itself may not be as efficient under dual task conditions. Indeed, holding a conversation has been shown to disrupt older adult’s ability to orient to meaningful objects in a change detection task (McCarley, Vais, Pringle, Kramer, Irwin, & Strayer, 2004). Unlike the alerting network, we may not expect that the orienting network would show a greater benefit in the conversation condition compared to the no conversation condition simply because the orienting network benefit is calculated by subtracting the central cue condition reaction times from those made in the spatial cue condition. As there is a cue in both conditions we would expect participants to be equally alerted. Finally, it is possible that naturalistic conversation (at least if it is not specifically spatial in nature), would not impact on the orienting network (Wickens, 2002) to such an extent that a deficit in participants' ability to orient to stimuli would be found in the conversation condition.

The potential effect on the executive control network is even more challenging to predict. There are different ways in which resources can be loaded. For example, some tasks specifically load working memory resources (Baddeley, 1986; Jaeggi et al., 2003; De Fockert, 2013); others apply a cognitive load while others cause an effect which is attributed to perceptual load (Lavie, Hirst, De Fockert & Viding, 2004). Participants' performance on a task is likely to change depending on which resources are being loaded. Lavie, Hirst, De Fockert and Viding (2004) demonstrated that irrelevant flankers are processed differently under cognitive load compared to perceptual load. Under high *cognitive* load, participants are not able to deploy cognitive resources to control the allocation of attention to a specific target and as such flankers are processed. However, under high *perceptual* load participants do not process the flankers due to the narrowing of the attentional spotlight. Therefore, if conversation increases cognitive load I would predict greater interference from incongruent distracters in the conversation condition and perhaps an increase in the number of errors made in these trials.

Alternatively, conversation may act to narrow the spread of spatial attention (Lavie & Tsal, 1994; Lavie, 1995) creating a bottleneck in attentional resources. This attentional bottleneck may not allow for incongruent flankers to be processed at all, if the flankers are not perceived then they cannot affect participants' responses. Here we might expect a greater effect on reaction times in congruent/incongruent trials in the no-conversation condition compared with the conversation condition. If this is the case, we would also expect more errors to be made on incongruent trials in the no-conversation condition.

In summary, in the present study I aim to assess the impact of holding a conversation on the elemental and yet fundamental processes involved in the control of visual attention. By doing so I will characterise the influence of conversation in a much finer grained way than previous studies which have used more global tasks. Specifically, the ANT was used to assess the effect of holding a conversation on participants' ability to be alerted and maintain an alerted state, to orient to specific spatial locations and to intentionally control the allocation of their attention. These processes are fundamental to many complex tasks and so understanding how conversation may or may not affect each one of them will allow us to predict and understand when and how conversations influence performance in a wide range of settings.

Method

Participants

There were 41 participants (35 female, Mean age = 20.7) recruited from the undergraduate psychology and the decision research participant pools at the University of Warwick. Participants received course credit or £5 for taking part and confirmed, prior to the experiment that they could clearly and easily see the information presented on the computer screen.

Stimuli and Apparatus.

A custom written computer program was used to present the ANT task, following the specifications given by Fan et al., (2002). The program ran on an i3 RM Computer attached to a Hanns-G LCD monitor running at a resolution of 800x600. Participants responded using a standard QWERTY keyboard. The conversation took place over a Samsung Galaxy S4 smart phone running SKYPE. The experimenter received the call on a Toshiba Satellite Pro Laptop computer with an Intel Celeron processor, running SKYPE.

Design

A 2(Conversation condition: Conversation, No Conversation) × 4(Cue type: no cue, central cue, double cue, spatial cue) × 2(Target locations: above or below fixation) × 2(Target direction: Left, Right) × 3(Flanker conditions: Incongruent, Congruent, Neutral) within-subjects design was used. The dependent variables were the participants' RTs to identify the target and error rate. Participants completed six blocks of 96 experimental trials and one block of 24 full feedback practice trials. In half of the experimental blocks participants held a naturalistic conversation with the experimenter. Conversation blocks alternated with no-conversation blocks in a counterbalanced manner. The trial structure replicated that of Fan et al., (2002) and used a visual alerting cue so as to avoid auditory interference from the conversation.

Each trial consisted of a 400-1600ms central fixation phase followed by one of four possible cues. In the central cue condition an asterisk replaced the fixation cross. In the double cue condition an asterisk appeared above and below the fixation cross in the two possible target locations. In the spatial cue condition a single asterisk appeared at one of the target locations and always indicated the proceeding target location. The cue remained visible for 100ms in all cue conditions. In the no-cue condition the fixation remained until the target appeared (Figure 5.1).

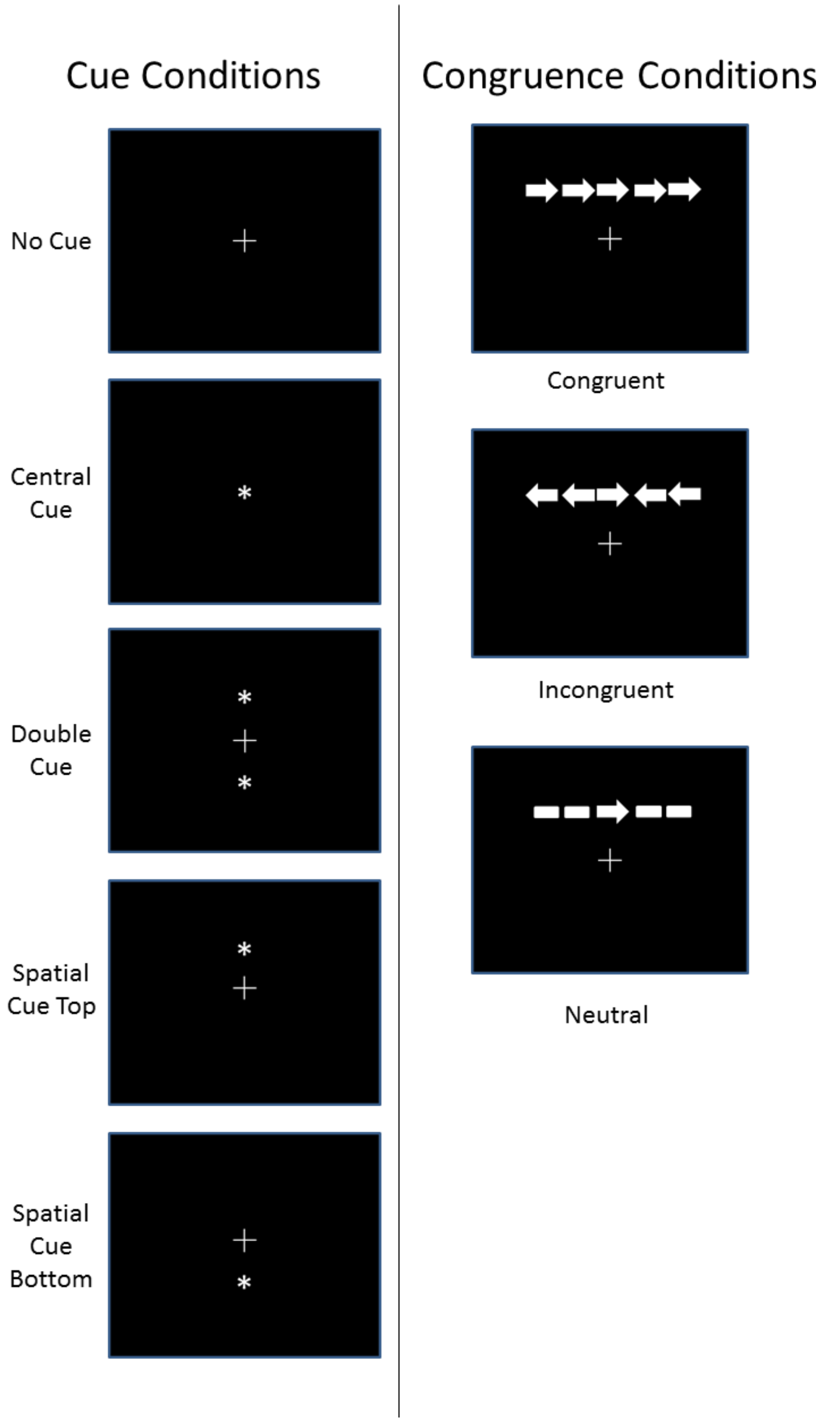

Figure 5.1 – The individual cue conditions and congruence conditions.

Following the cue, the central fixation cross remained alone for a further 400ms after which the target appeared either above or below the central fixation cross accompanied by two horizontally placed flankers. The flankers were either congruent (arrows oriented in the same direction as the target), incongruent (arrows oriented opposite to the target) or neutral (dashed lines) (Figure 5.1). The target remained on the screen until either the participant made a keyboard response to indicate the direction of the target arrow or 1700ms had elapsed. At this point the target offset and a central fixation cross was presented on the screen until the trial duration reached 4000ms (Figure 5.2). The stimuli measurements in visual degrees were as follows. The arrow target and flankers were 1.21° horizontally and 0.7° vertically, the neutral flankers were 1.21° horizontally but were 0.2° vertically. Each item was separated by 0.3° of blank space. The cue was 0.4° both vertically and horizontally. The fixation cross was 0.3° in both dimensions. The line of stimuli, together occupied a total of 7.2° horizontally. The distance between the closest point of the target and the fixation cross was ~1° of visual angle.

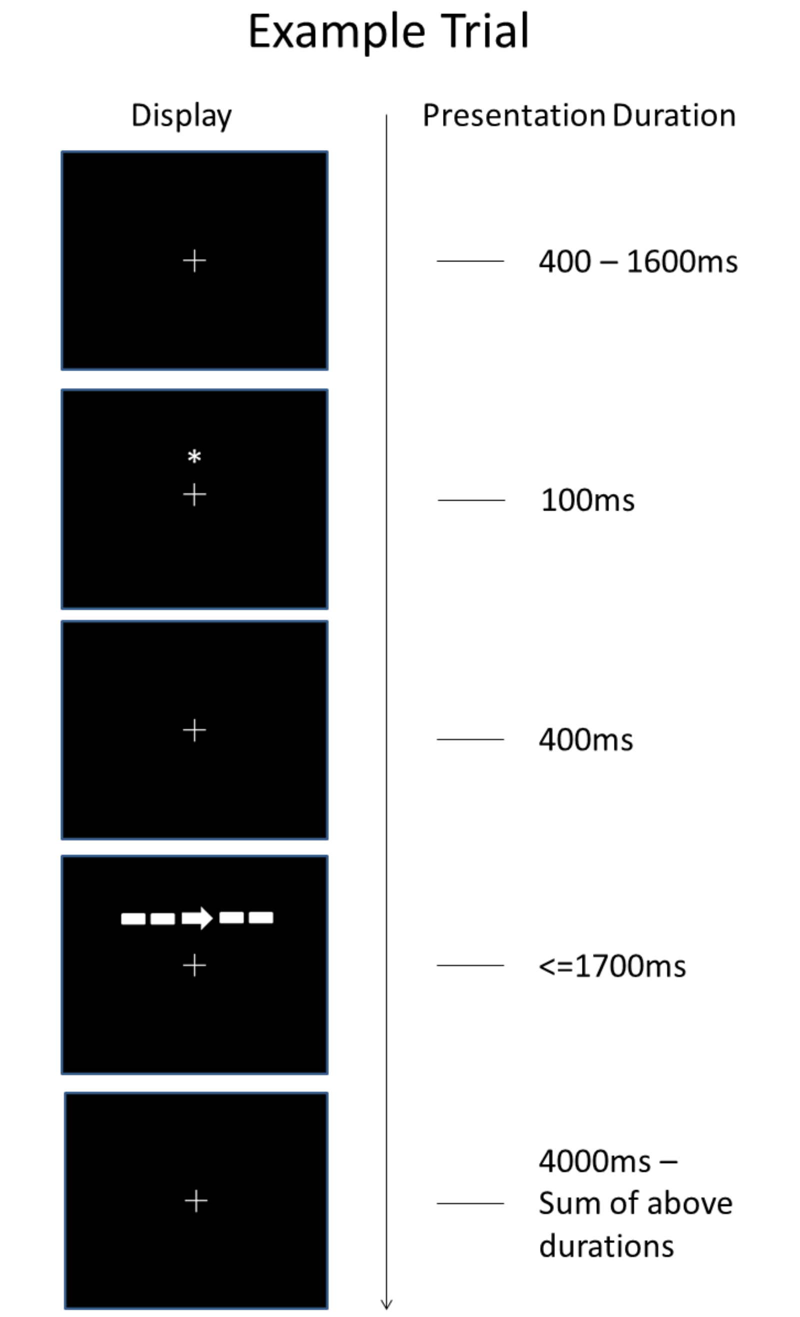

Figure 5.2 – An example trial. The first display is presented for a random duration between 400 and 1600ms, so that the targets appearance is not temporally predictable from the beginning of the trial. The participants' RT is naturally variable. Therefore, the final display duration is calculated by taking the sum of the durations for all previous displays away from 4000ms. This ensures that each trials duration is equal to 4000ms.

Procedure

Participants sat approximately 57cm from the computer screen, read through the task instructions and gave informed consent. They then completed 24 practice trials which were randomly selected without replacement from the 48 possible trial variations. After this the participants were invited to ask any questions that they had and were reminded that for half of the experimental blocks they would be holding a naturalistic conversation with the experimenter and that the conversation would be recorded. The conversation took place over a hands-free mobile phone using SKYPE VOIP software. The experimenter received the call on a laptop computer in an adjacent experimental cubicle.

Participants then completed six blocks of 96 trials. Each block contained every possible combination of conditions, twice. Participants were instructed to look at the central fixation point until the target appeared on the screen and respond to the orientation of the target arrow by pressing the < key if the target was pointing to the left and the > key if it was pointing to the right. If participants did not respond within 1700ms the trial automatically moved on to the end phase and a "no response" was recorded.

Results

I provide first an overall analysis of the data before considering each of the attentional networks individually. Trials on which participants made an incorrect response (1.38%) or failed to respond in time (0.23%) were excluded from the main analysis, as were responses which occurred in less than 200ms (0.03%). Mean correct RTs were analyzed using a 2(Conversation: Conversation or No Conversation) × 4(Cue: No cue, Central cue, Double cue, Spatial cue) × 3(Target/flanker congruence:

Congruent, Incongruent, Neutral) within-subject ANOVA. When the assumption of sphericity was violated the Greenhouse-Geisser correction was applied.

The three main effects of conversation, cue and congruence were all significant, $F(1,37)=40.421$, $MSE = 15028.4$, $p<.001$, $\eta^2 = .522$, $F(2.4,90.2)=129.961$, $MSE = 1411.7$, $p<.001$, $\eta^2 = .778$, and $F(1.5,53.7)=179.006$, $MSE = 2770.4$, $p<.001$, $\eta^2 = .829$ respectively. There were also two significant interactions, Conversation × Congruence $F(2,74)=14.639$, $MSE = 653.0$, $p<.001$, $\eta^2 = .283$, and Cue × Congruence, $F(6,222)=7.131$, $MSE = 634.4$, $p<.001$, $\eta^2 = .162$. No other significant interactions were found (all Fs < 1.315, ps > .263).

As shown in Figure 5.3A, overall RTs across all three congruency conditions were longer in the conversation condition than in the no-conversation condition. RTs were also longer in the incongruent condition than in the congruent and neutral conditions and this slowing was greater when no conversation was being held. In terms of the effects of cue type, RTs were shortest in the spatial cue condition and longest in the no-cue condition and the effect of cue type was similar regardless of whether a conversation was held or not (Figure 5.3B). Regarding the Cue × Congruence interaction (Figure 5.3C), RTs in the incongruent condition were longer than in the congruent and neutral flanker conditions. However, this difference was smaller in the no-cue condition than in the other cue conditions. A possible explanation of this pattern is that the incongruent flankers may already have slowed RTs down to a sufficient extent to reduce any additional effects of the presence or absence of a spatial cue.

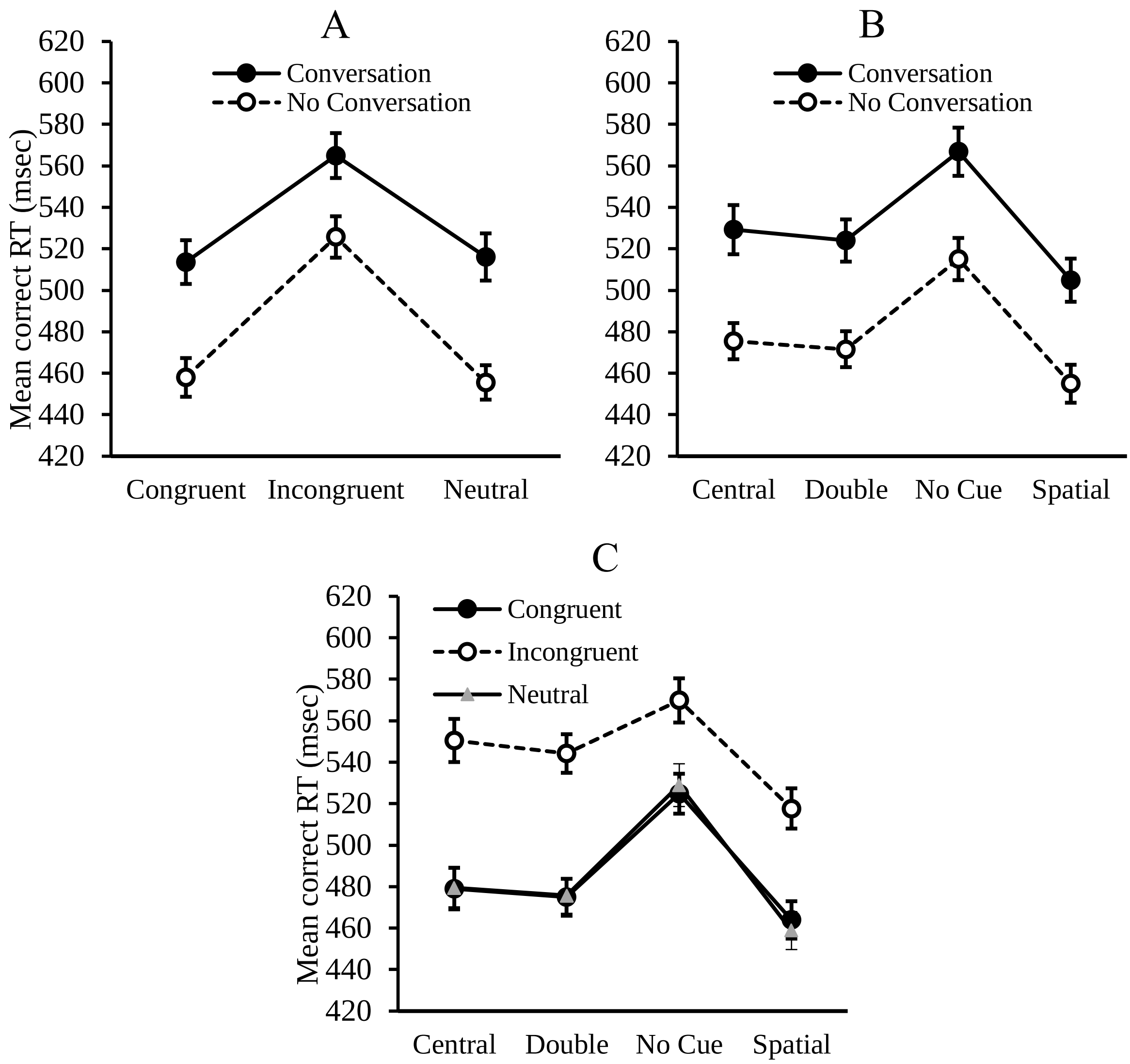

Figure 5. 3 – Mean correct RTs as a function of target-distractor congruence and conversation (Panel A), cue condition and conversation (Panel B), and congruence and cue condition (Panel C). Error bars show ±1SE.

Error rates

Participants in the ANT can produce two types of errors, incorrect responses (response errors) and failures to respond within 1700ms (timeout errors).

Response errors: On average 1.38% of trials resulted in an error[4] (Table E.1). As the original hypothesis was concerned with the number of errors made in each of the conversation conditions I analysed this data further. However, note that caution should be taken when interpreting this analysis given the very low overall error rate. First, mean percentage errors were calculated for each participant for each cell of the design, these were then arcsine transformed (Cohen & Cohen, 1983) and analysed using a 2(Conversation: Conversation; No Conversation) × 4(Cue: No-; Central-; Double-; Spatial-Cue) × 3(Congruence: Congruent; Incongruent; Neutral) within-subject ANOVA. This revealed a significant main effect of conversation, $F(1,37)=12.104$, $MSE = 0.003$, $p=.001$, $\eta^2 = .246$ and congruence, $F(1.26,46.6)=50.225$, $MSE = 0.009$, $p<.001$, $\eta^2 = .576$, and a significant Cue × Congruence interaction $F(4.1,151.4)=3.028$, $MSE = 0.004$, $p=.017$, $\eta^2 = .077$ (Figure 5.4C). There was little effect of cue type in the congruent and neutral condition. However, in the incongruent condition participants made more errors when the cue was central or double. Neither the Conversation × Congruence interaction, $F(1.3,49.6)=2.93$, $MSE = 0.005$, $p=.082$, $\eta^2 = .073$, nor any other main effects or interactions reached significance (all Fs < 2.301, ps $> .081$).

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[4] Trials in which the participant responded in less than 200ms were removed prior to Error analysis as were trials in which the participant did not respond within the allotted time.

As the hypotheses were concerned with the difference in the number of errors made in each of the congruency conditions I performed 3 separate paired samples t-tests in order to compare the number errors made in each level of distracter congruence across the conversation condition. This analysis showed that participants did not make a significantly different number of errors when the distracters were neutral, $p = .268$, or congruent, $p = .07$ (Wilcoxon signed rank as the data violated normality), with the target. However, a significant difference was found when the distracter was incongruent with the target, $t(37)=2.764$, $p = .009$, $d = .448$. Participants made significantly fewer errors when conversing (M = 2.32%) than when they completed the task under single task conditions (M = 3.66%).

Timeout errors: The number of timeout errors was extremely low (0.23%) and were not analyzed further.

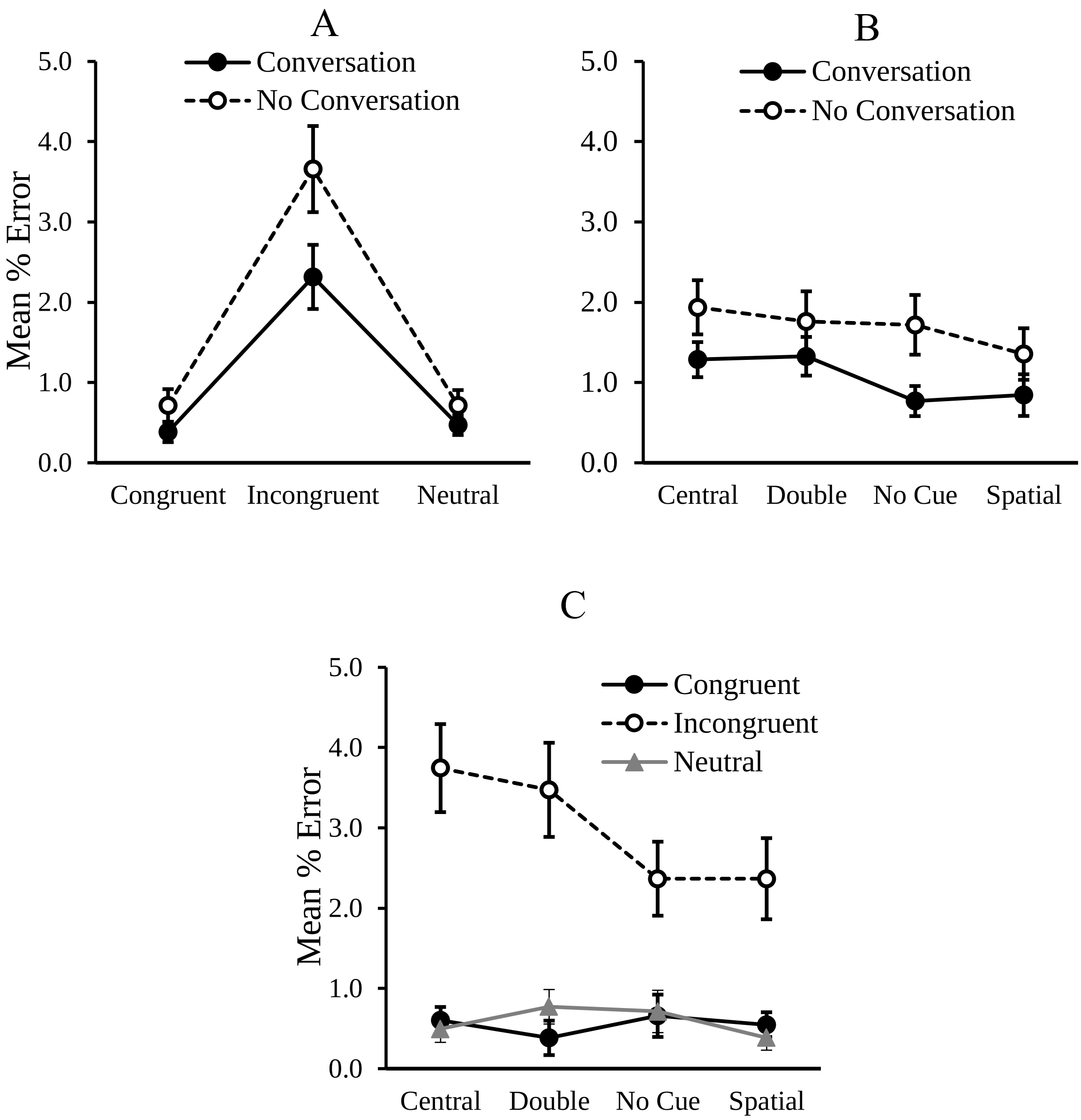

Figure 5.4 – Percentage errors as a function of target-distractor congruence and conversation (Panel A), cue condition and conversation (Panel B), and congruence and cue condition (Panel C). Error bars show ±1SE

Analysis at the level of individual attentional networks

I next consider the impact of conversation on each of the three attentional networks.

Alerting Network. Following Fan et al., (2002) the activity of the alerting response network was calculated by computing, on a per participant basis, a mean RT for all trials in which a double cue was presented and subtracting it from the mean RT on no-cue trials (Figure 5.5A). A paired-samples t-test showed that the difference in alerting response between the conversation (42.81ms) and no-conversation (43.47ms) conditions did not approach significance, $t(37)= 0.141$, $p=.889$, $d = .023$.

Orienting Network. The orienting response was calculated by averaging RTs from the spatial cue conditions (cue above or below fixation) and subtracting from mean RTs for the central cue trials (Figure 5.5B). This showed that the orienting response was numerically larger in the conversation condition (Conversation Med=29.05ms vs. No Conversation Med = 22.77ms), however, this difference did not approach significance, $z = 404$, $p = .636$[5].

Executive Control. The role of executive functions was calculated by taking participants' mean RT for congruent target/flanker trials and subtracting this from their mean for incongruent trials. The resulting value was the effect of flanker congruence on reaction time (Figure 5.5C). The results showed that the effect of flanker congruence on RT was considerably smaller in the conversation (51.4ms) than in the no conversation condition (67.7ms), $t(37)= 3.813$, $p<.001$, $d = .618$.

[5] A Wilcoxon Signed Ranks test was used as the data in this analysis were skewed.

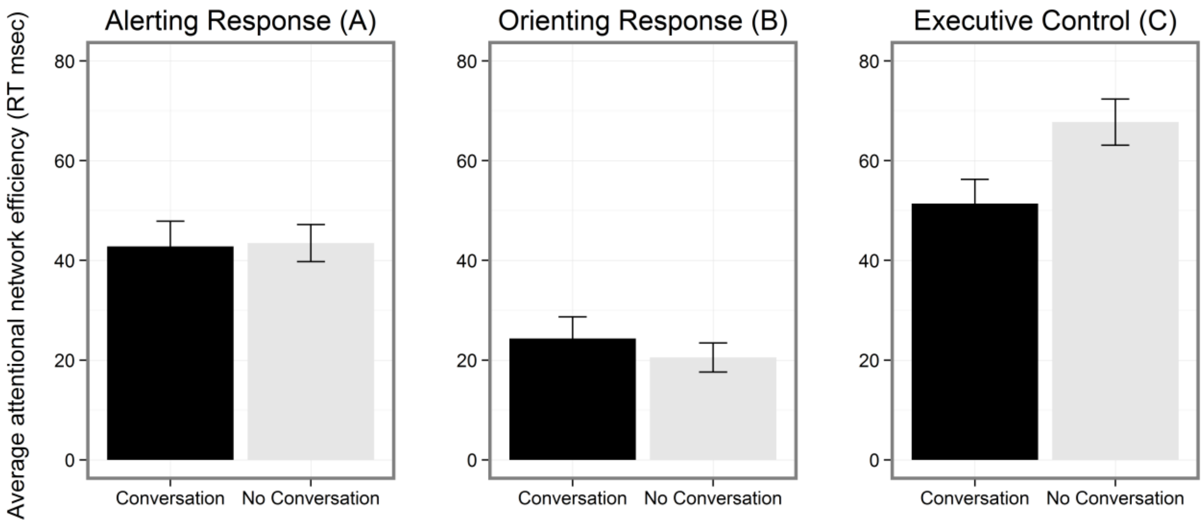

Figure 5.5 – The average efficiency of each attentional network in milliseconds, from left to right, the alerting response, the orienting response and executive control all as a function of conversation. Error bars show ±1SE.

Discussion

Consistent with some of the previous literature (e.g., Spence et al., 2013; Shinohara, Nakamura, Tatsuta, & Iba, 2010) holding a conversation produced a general and robust increase in RTs across all conditions. However, of most interest was the extent to which conversation influenced processes associated with each of the three individual attentional networks.

In this respect, the findings provided no evidence for an effect of naturalistic conversation on either the altering response or the orientation response. One possible account of these findings is that the abrupt luminance onset of the cues might have operated in an automatic and relatively resource-free manner to both orient attention to a spatial location and provide an alerting or warning signal.

In support of this possibility are previous findings that suggest luminance onset cues can capture attention automatically (see, Posner, 1980, Yantis & Jonides, 1984; Jonides & Yantis, 1988). Posner (1980) demonstrated this in his classic cueing paradigm in which a target could appear either to the left or right of a fixation point, this target was preceded by either a valid (cue at the target location) or invalid (cue at a non-target location) luminance-defined spatial cue. Participants showed a reduced RT when their attention was guided by a valid cue and an increased RT when an invalid cue was present. In a similar way, abruptly appearing objects can capture attention automatically at the expense of other, changing items in a visual field (Yantis & Jonides, 1984; Jonides & Yantis, 1988).

In contrast to the Alerting and Orienting networks, the Executive network was affected by conversation. Specifically, participants showed a much greater effect of target-distractor congruence in the no-conversation condition than when a conversation was being held. Thus, perhaps counterintuitively, in this particular task, holding a conversation *improved* performance rather than made it worse.

Note that I do not propose that conversation in some way enhanced the function of the executive network. Rather I propose that under attentional load (i.e., conversation) fewer resources were available to process irrelevant and extraneous information. This finding and account might be predicted on the basis of the perceptual load theory of attention (Lavie, 1995). According to this theory, when perceptual load is high, more resources are required to process the target and therefore attention becomes more focused. As a result of this, surrounding extraneous information is less likely to be processed. However, under low perceptual load, 'spare' attentional resources are left over and so more of the scene, and therefore extraneous information, enters our awareness.

Lavie (1995) showed that when under high perceptual load participants do not process irrelevant distracters which they are able to process when under low perceptual load (for a review see, Lavie, 2005; Lavie & Torralbo, 2010). The explanation given for this finding is that perceptual load affects processing capacity by either focusing it solely on one specific aspect or freeing capacity and enabling a broader spotlight of attention. There is some dispute in the literature as to whether or not perceptual load theory or the theory of the dilution of attention (Tsal & Benoni, 2010ab) best explains the effects of irrelevant distracter interference in some task conditions but not others. However, for the purposes of the current study both theories provide valid explanations for the observed results.

As discussed earlier, I could predict two different outcomes in the congruency/flanker task based on the type of load that conversation applied to the participants' attentional system. Lavie, Hirst, De Fockert and Viding (2004) showed that when working memory load and cognitive load were manipulated during the flanker response-competition task (Eriksen & Eriksen, 1974), greater distracter interference was found under conditions of high load. This suggests that cognitive control resources are necessary for the focusing of attention and the filtering of irrelevant information. However, in contrast to cognitive load, perceptual load had a different effect. When perceptual load was increased, spatially separate irrelevant stimuli were not processed to the same extent. Given that I found reduced interference in the conversation condition, in the context of Lavie et al., (2004), it appears as though naturalistic conversation may be exerting a perceptual load rather than a cognitive or working memory load. According to this view, holding a conversation reduced the perceptual resources available for processing which in turn meant that fewer resources were available to process extraneous, irrelevant information (here the flanking distractors). As a result, incongruent flankers were less likely to interfere with identifying the central target.

I am not alone in making this claim. Atchley and Dressel (2004) demonstrated that when participants were required to perform a task similar to conversation, in which they were given a word and they must respond with another word which began with the last letter of the word which was supplied, they showed a large reduction in their functional fields of view. Another study in the area of optometry by Maples DeRosier, Hoenes, Bendure and Moore (2008) showed that hand held cell phone conversations were able to artificially constrict participants' peripheral awareness. In their study participants' visual field area showed a reduction of between 10.1 and 6.9 percent, depending on target size. It should be noted that both Atchley and Dressel (2004) and Maples et al., (2008) by their own report used tasks which were similar to conversation but may not be valid representations of "real-world" conversation. In addition, both of these studies show that field of view is attenuated at the periphery whereas in the work presented in this chapter the target and distracters were located side by side with the closest distracter being only 0.3° of visual angle away from the target. As such the findings that I present here are able to extend their initial findings by confirming that naturalistic conversation does appear to be sufficient to reduce the amount of information that can be attained from a single fixation. Importantly the work of this chapter has also shown that even visually very close items, which appear in central vision and not peripheral vision, may not be processed when a participant is conversing.

Our findings also add to those of Lee, Lee and Boyle (2009) who asked participants to perform a task akin to interacting with in vehicle technology, the task involved listening to information about restaurants located nearby and then verbally responding based on this information. Participants performed this task while executing a driving task in a simulator. The driving task was based on a modified Posner cue-target paradigm (Posner, 1980) such that elements within the driving scene were set up to cue participants' attention. The secondary task was predicted to induce a cognitive load and therefore increase the extent to which exogenous stimuli distracted the participant. However, the opposite effect was observed. Lee et al., (2009) interpreted this effect in terms of perceptual load theory (Lavie, 1995) and suggested that perhaps their task involved a perceptual load element. Our results provide converging evidence for Lee et al's (2009) explanation for their results and also show that a task as seemingly mundane as holding a conversation is able to produce a similar effect to that observed in their study.

Additional evidence in support of this explanation can be found in recent literature which explores how perceptual load can influence cross modal tasks

(Macdonald & Lavie, 2011) and real world tasks such as driving behaviour (Murphy & Greene, 2016). Macdonald and Lavie (2011) varied perceptual load in a visual discrimination task and demonstrated that participants were significantly more likely to fail to notice that a particular tone had been played, when under high visual load compared to low visual load. They coined this phenomenon inattentional deafness. However, it should also be noted that Murphy, Fraenkel and Dalton (2013), using a different design, were not able to find an effect of auditory perceptual load on auditory distractor processing. They suggest that this may be because the auditory system acts as an early warning system and so maintains surplus capacity in order to be able to process auditory information from other streams other than the one which is currently being monitored. However, given that visual perceptual load has been shown to affect auditory processing and it has been shown that auditory stimuli can affect our visual processing of a scene (Strayer, Cooper & Drews, 2004; Strayer, Drews & Johnston, 2003; Van der Burg, Olivers, Bronkhorst & Theeuwes 2008), one might expect that an auditory task, which implies a high load on the participant, may impact upon their ability to visually process a scene.

The current findings mesh with arguments which propose that part of the observed detriment in driving performance, caused by naturalistic conversation, can be attributed to less attention being given to visual inputs. For example, according to the inattentional blindness theory of driver mobile phone use, attention is drawn away from the visual task by the conversation task and, as such, even directly fixated objects may not be properly visually encoded (Strayer, Cooper & Drews, 2004). Using a driving simulator methodology, Strayer, Drews and Johnston (2003) found that participants had reduced explicit memory for road signs which they had nonetheless observed whilst holding a naturalistic conversation. In addition, participants also showed reduced implicit memory for objects which they had directly foveated during the simulator task. In a related study, McCarley, et al., (2004) found that participants were impaired in their ability to search for changes in visual scenes when they were required to converse at the same time. They argued that this was due to a reduction in the amount of visual encoding during each fixation.

The results of the current study are consistent with the inattentional blindness theory of driver mobile phone use because here, the flankers, despite being easily observable and positioned directly adjacent to the target, had a reduced influence when a conversation was being held. That is, conversation reduced the amount of visual information that could be processed. In the current task, this resulted in an increase in performance because the reduction in processing capacity had the effect of reducing interference from distracting information. However, clearly, in many tasks, limiting the perception of visual inputs is likely to have mal-adaptive effects. In previous work holding a conversation has been shown to reduce the ability to track multiple identical objects (Kunar et al., 2008). One general reason suggested for this deficit was that conversation impacts on sustained visual attention via an 'amodal central bottleneck'. The current work shows specifically that conversation impacts on visual attention in such a way as to limit the amount of information that is able to be processed from a fixation (rather than reducing attention capture or orienting). Given that the MOT task requires parsing of multiple objects (the refreshing of visual indexes; Pylyshyn, 1994) it is likely that reducing the amount of information that can be processed in each fixation would lead to a reduced ability to track multiple objects. In this case my results suggest that the 'amodal bottleneck' was bought about by an additional perceptual rather than cognitive load.

Practical implications

The results of this study have clear implications for many areas of everyday life. To take driving as just one example, the results add to the results of other studies which show that conversation has a large overall impact on RTs and this alone would very likely increase braking distances and slow down the initiation of avoidance responses (e.g., Kunar et al., 2008; Shinohara, Nakamura, Tatsuta, & Iba, 2010; Spence et al., 2013). However, although there was a general slowing of RTs in the conversation condition, participants did appear to be basically responsive to both alerting and spatial orienting cues – at least within the given parameters of this study. This suggests that rapid luminance changes (such as emergency vehicle lights, or instrument warnings) within the currently viewed field might still be effective in some driving situations even when cognitive resources are devoted to other tasks. However, as noted before, the overall speed with which such events are perceived or acted upon might well be delayed. In addition, conversation might also negatively affect the extent to which such stimuli are processed within the viewed field, if for example, conversation reduces the frequency of mirror checking. In fact, studies in which participants have been asked to perform concurrent tasks while driving have shown that spatial imagery tasks (Recarte, & Nunes, 2000) and difficult cognitive tasks (double digit addition) (Harbluk, Noy, Trbovich & Eizenman, 2007), when delivered verbally, can reduce participants' glance frequency towards their mirrors and more generally find that gaze is confined to a smaller central area.

However, of more importance, my findings suggest that increasing driver load, here holding a conversation, might well reduce the amount of information that drivers are able to process. In the present work this resulted in an increase in performance because the processing of distracting information was reduced. However, in a more real-world context, the results suggest that when focussing on, for example, the car in front, holding a conversation might well impair the driver's ability to detect visual stimuli that appear more peripherally, such as a pedestrian on the pavement or a vehicle located at a junction. Similarly, a narrowing of attention around the vehicle directly in front could lead to a failure to notice that cars further away have braked, leading to the increased possibility of a collision. This is in line with research by Murphy and Greene (2016) who demonstrated that perceptual load theory is applicable to real world, applied scenarios such as participants' awareness whilst driving. Manipulating a visual driving task so that it could either apply high or low perceptual load, they found that in conditions of high perceptual load participants were significantly less likely to be aware of an unexpected pedestrian or animal present on the side of the road. Therefore if, as our findings suggest, conversation causes an increase in driver load the resultant narrowing of spatial attention could lead to a reduction in the perception of the general context of a scene or situation, which will have important implications in both driving tasks and a range of other situations.

Chapter 5 marks the final chapter in which I aim to investigate the effect of a naturalistic task such as conversation on our ability to visually attend to the world around us. Chapter 5 specifically investigated the effect of conversation on participants' ability to exhibit an alerting response, orienting response and apply executive control to attention. The key finding from this chapter is that the data indicated that, while conversing participants' ability to process the wider picture of the world around them was significantly reduced compared to single task conditions. I present an explanation of this effect in terms of conversation applying a perceptual load which acts to reduce the attentional spotlight and therefore excess resources were not available to pick up and process distracter stimuli.

Given that the data from chapters 2, 3 and 5 are able to add to the literature suggesting that conversation is causing interference through an amodal central bottleneck (Kunar, 2008) it stands to reason that this effect may not be limited to visual attention alone. Indeed it is possible that other, key driving related tasks may be affected, for example by drawing attention away from key elements of the task, or reducing the amount of information which may be processed in a particular scenario. Therefore, in Chapter 6 I turn my attention to another key component of the driving task, our ability to make risky decisions.

Chapter 6:

Risky conversations: Talking on the phone leads to riskier decision making

As discussed in the general introduction of this thesis (Chapter 1), visual attention is just one of the key elements essential to safe driving. Another highly important factor is our ability to assess the level of risk inherent to a particular situation and then make appropriate decisions that might involve an element of risk. It is not trivial to predict who will take risks and under what circumstances. Therefore, it is difficult to predict how dual tasks, which might be performed whilst driving, will interact with participants' ability to effectively evaluate and take risks.

Researchers have developed tools which allow us to quantify participants' attitudes towards risk and measure actual risk taking behaviour. One such tool is the Domain Specific Risk Taking Scale the DOSPERT (Weber, Blais & Betz, 2002). This self-report scale is used to measure participants' attitudes towards risk over five different domains. In addition, lab based tasks have also been developed in which participants take part in a particular exercise and their risk taking behaviour is observed. In one procedure, called the Balloon Analogue Risk Task (BART, Lejuez, et al., 2002), participants decide how many times to inflate a virtual balloon. Money is earned for each addition of air but all the money for a given trial is lost if the balloon bursts. The amount of air that a participant adds to the balloon indicates the level of risk that they are prepared to take. Other examples include the IOWA gambling task (Bechara, Damasio, Tranel, & Damasio, 1997) which measures decision making under uncertainty and the Game of Dice task (Brand et al., 2005) which is specifically designed to assess decisions under risk.

More recently, Figner, Mackinlay, Wilkening and Weber (2009) have developed a novel computer-based risk assessment procedure called the Columbia Card Task (CCT). One particular benefit of this task is that it can be used to measure both *hot* and *cold* decision making processes, corresponding to affective and deliberate risk taking respectively. In the hot version, on each trial, participants are presented with 32 virtual cards shown face down. The majority of these cards are reward cards and if selected/turned over earn the participant a certain number of points. However, some of the cards are loss cards which, if selected, end the trial and result in a substantial loss of points. During each trial, participants have the option of ending the trial and banking the points so far earned rather than risk turning over a loss card. This hot version of the task relates to emotion-based decision making (Metcalfe & Mischel, 1999; Weber, Shafir & Blais, 2004) in which participants make quick, triggered responses rather than more deliberate, cold and considered choices. Each card selection can elicit an emotional response from participants by either being positive (win) or negative (loss) and as such can influence future decision making. Thus this task relates to situations in which people make real-time decisions based, in part, on emotions and impulses, and where the level of risk changes over time.

In contrast to the hot version, in the cold CCT participants simply have to report how many cards they would like to turn over but they do not actively turn any cards over and they do not get to see the outcome of their decisions on any of the trials. This version is designed to assess the *cool system* of decision making which is based on self-control and is reflective in nature (Metcalfe & Mischel, 1999; Weber, Shafir & Blais, 2004). In addition to a measure of general risk taking propensity, both versions allow assessment of more specific attributes, including sensitivity to loss, gain, and differing probabilities of loss (Figner, Mackinlay, Wilkening & Weber, 2009; Schonberg, Fox, & Poldrack, 2011). This is achieved by manipulating the number of points gained for each gain card, the number of points lost as a result of turning over a loss card, and the number of loss cards present in the deck. In both versions of the task, the number of cards that participants choose to turn over provides a measure of their willingness to take risks.

Changes in the likelihood of risk taking

As noted in Chapter 1, age can influence the level of risk taking with younger adolescents making riskier hot decisions than adults (Figner, Mackinlay, Wilkening & Weber, 2009). However, the amount of risk taking behaviour can also vary within an individual depending on a number of both internal and external factors.

One such external factor is the presence or absence of peers. Chein, Albert, O'Brien, Uckert and Steinberg (2011) assessed participants of three different age ranges in a driving simulator task that was designed to measure risk taking behaviour. Risky behaviour in this task increased when peers were thought to be observing and was associated with increased activity in reward-related brain regions (specifically the ventral striatum and orbitofrontal cortex). This pattern of results was only found for adolescent participants and is consistent with findings from an earlier study in which college age participants were found to take 50% more risks in a video-based driving task when they were in the presence of peers (Gardner & Steinberg, 2005).

Risk taking in conditions of attentional competition

As discussed briefly above, there exists a rich set of findings relating to risk taking behaviour and the factors that influence it. However, one area of risk taking that appears to remain relatively unexplored relates to the effects of secondary tasks on people's willingness to take risks. This is despite the fact that in everyday life there are often multiple behavioural goals and stimuli competing for our attention whilst we make moment-to-moment decisions. Given the limits of human attention and processing resources (e.g., Marois & Ivanoff, 2005; for an overview see Shapiro, 2001) it might be expected that performing secondary tasks would influence risk taking. With this in mind Turnbull, Evans, Bunce, Carzolio, and O'Connor (2005) examined the effects of different secondary load tasks performed whilst completing the Iowa Gambling Task (IGT). Participants completed a random number generation task which was designed to load executive resources, or an articulatory suppression task, which did not load executive resources. These conditions were compared with a control condition in which no load task was performed.

Perhaps surprisingly, the results showed that, compared with the control condition, neither load task had a reliable influence on the rate of learning in the IGT. This outcome was explained by the potential lack of overlap between the resources required to complete the IGT and the resources required to perform the two types of secondary load tasks. That is, the IGT is claimed to be dependent on emotion based learning (Bechara, Damasio, & Damasio, 2000) and neither load task required the commitment of emotion-related resources.

In a more applied piece of work Horswill and Mckenna (1999) investigated whether auditory distraction, in the form of a verbal task where the participant was required to monitor a stream of letters for a target letter, affected dynamic risky decision making in a driving context. The results showed that participants who were required to perform the verbal task alongside the risky decision making task took more risks than those who performed the risky decision making task in isolation.

Purpose of the current study

The main purpose of the present work was to examine the impact of a different kind of concurrent load task which was ecologically valid and similar to real life scenarios, namely holding a naturalistic conversation, on dynamic, real-time risk taking behaviour. Applicable to real-world performance, the general distracting effects of conversation have been demonstrated clearly in the field of, for example, driving. Holding a mobile telephone conversation has been linked to an increase in accidents and performance changes such as greater variability in accelerator pedal position (Rakauskas, Gugerty, & Ward, 2004). Other negative consequences include a general slowing of reaction times (Caird, Willness, Steel, & Scialfa, 2008) and deficits in tracking multiple objects (Kunar, Carter, Cohen & Horowitz, 2008). Indeed, driving performance while conversing on a mobile phone has been claimed to be as impaired as driving whilst under the influence of alcohol (Strayer, Drews, & Crouch, 2006).

In the present work I consider the potential impact of holding a conversation on risk taking behaviour using the hot version of the CCT. The CCT is clearly a laboratory-based task, however, it shares many features relevant to real-world risk taking activities. For example, it consists of making a series of dynamic, real-time risky decisions in which actions and outcomes need to be continuously evaluated. There is also the need to plan and execute a series of manual responses in order to act on the current decisions. As such, compared to some other risk measuring tasks, it is likely to be more susceptible to secondary tasks that require constant reorienting or switching between the secondary and main tasks or tasks that have some emotional/social element (Figner, Mackinlay, Wilkening & Weber, 2009). These aspects suggest that the results will also be applicable to general real-world decision tasks which share similar features (see also Kunar, Carter, Cohen & Horowitz, 2008 for an earlier example of generalizing controlled lab-based tasks to the field of driving behaviour).

Predicting the results is less than straightforward. One possibility is that there is insufficient overlap in the resources required to hold a conversation and those required to assess and respond to the risk task optimally. In this case we would expect to find no influence of conversation. This would be a similar finding to that of Turnbull et al's., (2005) study which reported little influence of a random number generation task or an articulatory suppression task on the IGT. Alternatively, the resources involved in holding a naturalistic conversation might overlap with those required for assessing risks, resulting in a performance impact. However, the direction of any potential impact is also not obvious. On the one hand, it might be the case that reducing the resources available for assessing risk leads to participants acting relatively conservatively because they realise that their judgements might be impaired. Alternatively, reducing the available resources might lead people to behave less conservatively because they process and evaluate less well the potential risks that they are encountering.

Furthermore, the distracting effects of holding a conversation might act to reduce the negative impact/emotion that would occur as a result of making a decision which resulted in a negative outcome (here encountering a loss card and the associated loss of points, or related to driving behaviour, encountering a near-miss or realizing the need to correct a positioning error). This potential reduction in the impact of loss outcomes/errors to risky decisions would also act to make people more liberal in their risk taking.

We assessed these possibilities in two experiments by having participants complete the hot version of the CCT. In one condition they completed the standard CCT. In another condition they completed the same task whilst simultaneously holding a naturalistic conversation (see Kunar, Carter, Cohen & Horowitz, 2008, for a similar approach with a multiple object visual tracking study).

In addition to the behavioural measurements of performance, I also recorded electrodermal activity (EDA; Boucsein, 2012; Figner & Murphy, 2011) in order to detect any differences in emotional/physiological activity between the conversation and no conversation conditions. The main two measures of interest were Skin Conductance Level (SCL) and Skin Conductance Response (SCR). The SCL is a measure of the tonic conductivity of the skin over a relatively extended period of time. The SCR is a measure of phasic, discrete transient changes and can occur spontaneously – non-specific SCRs (NS-SCR), or in response to a particular stimulus event – specific SCRs (Boucsein, 2012; Figner & Murphy, 2011).

Both the SCR and SCL are products of eccrine sweating caused by sympathetic neuronal activity (Boucsein, 2012). Nagai, Critchley, Featherstone, Trimble and Dolan, (2004) provide evidence for two separate neural pathways for the control of the SCL and SCRs. They suggest that the SCL is related to ventromedial prefrontal cortex and orbitofrontal cortex activity, while SCRs are correlated with activity in a range of brain areas including the lateral prefrontal cortex, the hypothalamus, thalamus, striate cortex, extrastriate cortex, anterior cingulate and insular cortices. However, the exact mechanisms of central nervous system control of EDA are still under debate (see e.g., Boucsein, 2012, for an in depth review). The limbic and paralimbic areas, including the amygdala and hypothalamus, have also been linked to EDA. This is of particular interest to the current study because the Hot CCT is argued to rely on affective processes; the limbic and paralimbic areas are critical regions for processes involving emotion (Figner & Murphy, 2011). By measuring participants' skin conductance across all the experimental trials the conversation and no conversation conditions can be compared and increases or decreases in emotional or affective arousal can be determined.

I present two experiments both of which were designed to investigate how holding a naturalistic conversation might influence affective decision making.

EXPERIMENT 1: EFFECTS OF CONVERSATION ON RISK TAKING BEHAVIOUR

Method

Participants

Twenty participants (14 female), aged 18 to 50 (*M*=24[6]), recruited from the University of Warwick, Decision Research at Warwick participant panel, took part in return for payment. Part of the payment was contingent on performance in the CCT task.

Stimuli and apparatus

Columbia Card Task. Participants completed the hot version of the Columbia Card task which was delivered via a custom written program running on a Pentium-II based personal computer. Displays were presented on an HP191 LCD monitor at a

[6] One participant declined to provide their age. It should also be noted that in total 26 participants took part in this experiment however three had to be removed for technical/procedural reasons. In addition, as the conversation condition order was counterbalanced across participants, it was important that the two groups be of equal size. The three participants who were removed were all part of the same group and as such three additional participants were removed from the second group. The last three participants tested were removed. These participants were all removed prior to any data analysis procedures.

resolution of 800 × 600 pixels. On each trial participants were presented with a display consisting of 32 face down cards arranged in a 4 × 8 configuration. The number of loss cards in the deck (1 to 3), the loss amount associated with turning over a loss card (250 or 750 points) and the reward for each gain card selected (10 or 30 points) were displayed at the top of the screen (see Figure 6.1). Participants were asked to sequentially turn over (by making a mouse click on them) as many cards as they wished on each trial. A trial stopped when: i) the participant decided that they did not want to turn over any more cards, which they indicated by clicking on the *STOP/ turn over* button, which ended the trial and then displayed the face of all of the remaining cards, or ii) when the participant turned over a loss card. If a loss card was selected, the loss amount (250 or 750 points) was deducted from the participant's points for that round and the trial ended. For example, if the loss amount for a round was 750 points and a participant had collected 900 points in the round before clicking a loss card, their round total would be 150 points. It is therefore possible for participants to end a round with a negative points total if they clicked on a loss card and the loss amount exceeded the number of points they had collected in that current round. A hands-free speaker phone was placed to the side of the computer monitor to enable participants to talk freely with the experimenter who was in a different room.

Design and procedure

A single block of CCT trials consisted of 48 trials with each combination of the number of loss cards (1 or 3), the number of points associated with a loss card (250 or 750 points), and the number of points associated with a gain card (10 or 30 points) represented equally often (see, Figure 6.1 for an example trial). Participants completed two blocks of trials, a no-conversation block and a conversation block (counterbalanced across participants). In the no-conversation block participants completed the CCT task in silence. In the conversation block participants completed the CCT whilst holding a conversation, via the speaker phone, with the experimenter who was situated in a different room (as in Kunar, Carter, Cohen & Horowitz, 2008). Participants completed a five-trial practice block before the first full block of experimental trials began. After completion of the experiment three random trials were selected and scores on those trials were converted into monetary payment at a rate of 1 point = £0.01. Participants also received a fixed payment of £4 in addition to their winnings. This resulted in a possible range of payments between £4.00 and £32.80.

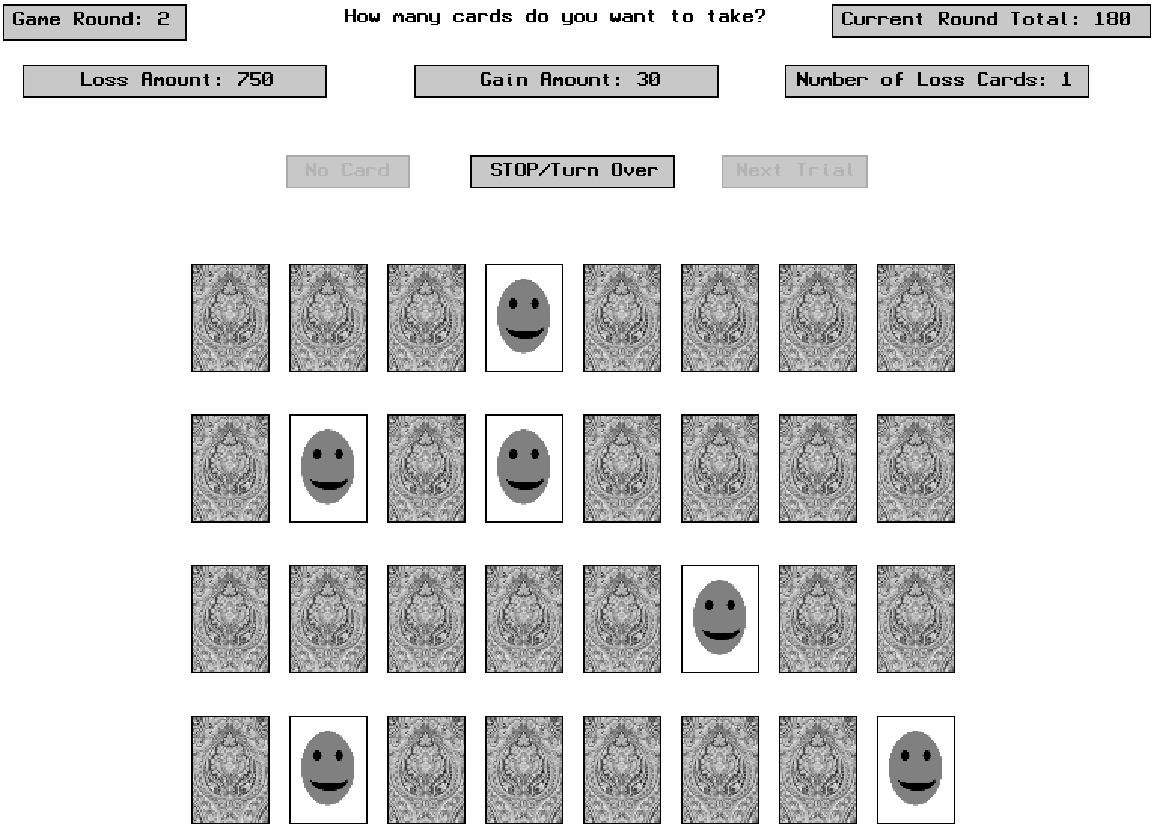

Figure 6.1. Example screen from the Columbia Card Task version used in this study (in grey scale).

Results

I examined the influence of conversation on risk taking behaviour using a number of measures including the overall number of cards selected by participants and the influence of the three pieces of information participants were given at the beginning of each trial to indicate the riskiness of the trial.

Number of cards selected

I first consider the average number of cards participants chose to select collapsed across the various conditions. Next I break this down by examining the influence of the number of loss cards present in the trial, the loss amount and the gain amount[7]; higher numbers indicate greater risk taking behaviour. For these analyses, only trials in which participants ended the trial (banked their points) were considered. Trials on which a loss card was selected were not included because it is not clear on those trials how many further cards the participant would have gone on to select. Hence the number of cards selected on loss trials is not a reliable measure of the level of risk taking.

Overall effect of conversation: In order to determine the overall effect of conversation on risk taking, the average number of cards selected in the conversation and no conversation conditions of the CCT were calculated for trials on which participants intentionally ended the trial (banked their points). This analysis revealed that significantly more cards were selected in the conversation condition (M = 14.21, SD = 4.86) than in the no-conversation condition (M = 12.39, SD = 4.39),

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[7] It should be noted that the number of data points was insufficient to allow the inclusion of all factors in a single analysis. As such 3 separate repeated measures ANOVA were performed to investigate the influence of the number of loss cards present in a trial, the loss amount and the gain amount separately.

$t(19)=2.428$, $p=.025$, $d = 0.543$, hence participants' level of risk taking was higher when they were holding a conversation.

Influence of the number of loss cards present: The mean number of cards selected (on non-loss trials) was analysed using a 2 (conversation / no-conversation) × 2 (number of loss cards: 1 or 3) within-subjects ANOVA. This revealed a significant main effect of conversation, $F(1,18)=8.592$, $MSE= 9.499$, $p=.009$, $\eta^2 = .323$, number of loss cards, $F(1,18)=60.244$, $MSE = 5.262$, $p<.001$, $\eta^2 = .770$, and a significant conversation × number of loss cards interaction, $F(1,18)=10.017$, $MSE = 6.707$, $p=.005$, $\eta^2 = .358$. As shown in Figure 6.2 (pp191), more cards were selected in the conversation condition than in the no-conversation condition, fewer cards were selected when there were three loss cards in the deck than when there was only one loss card, and this reduction as a function of the number of loss cards was greatest in the no-conversation condition.

Influence of the loss amount: As expected from the above analysis, a 2 (conversation / no-conversation) × 2 (loss amount: 250 or 750 points) within-subjects ANOVA revealed that more cards were selected in the conversation condition than in the no-conversation condition, $F(1,19)=5.498$, $MSE = 11.223$, $p=.030$, $\eta^2 = .224$. However, neither the main effect of loss amount, $F(1,19)=2.383$, $MSE = 2.956$, $p = .139$, $\eta^2 = .111$, nor the conversation × loss amount interaction were significant, $F(1,19)=1.055$, $MSE = 1.017$, $p = .317$, $\eta^2 = .053$.

Influence of the gain amount: A 2 (conversation / no-conversation) × 2 (gain amount: 10 or 30 points) within-subjects ANOVA revealed a significant main effect of conversation, $F(1,19)=6.512$, $MSE =11.212$, $p=.019$, $\eta^2 = .255$. However, neither the main effect of gain amount, $F(1,19)=.520$, $MSE = 2.573$, $p = .480$, $\eta^2 = .027$, or the conversation × gain amount interaction were significant, $F(1,19)=1.759$, MSE = 2.218, p = .200, η^2 = .085.

Discussion

The main aim of Experiment 1 was to examine the possible impact of holding a conversation on risk taking behaviour. Predicting the outcome was not straightforward. As described in the Introduction section, the effect of holding a conversation could have resulted in more risk taking, less risk taking or had no effect at all. Nonetheless, the results showed a clear pattern in terms of risky behaviour. Participants turned over significantly more cards per trial in the conversation condition than in the no-conversation condition. Therefore, by this measure of risk (Figner, Mackinlay, Wilkening & Weber, 2009) participants were making riskier decisions when they were cognitively distracted by naturalistic conversation.

Using information to assess risk

In addition to simply making a greater number of card selections, participants in the conversation condition appeared to be less able to effectively evaluate risk information compared to when they were not holding a conversation. When there was only one loss card present in the display, the number of cards selected in the conversation and no conversation conditions was similar. In contrast, when three loss cards were present, participants reduced the number of cards that they selected suggesting that they were using information about the level of risk to modulate their behaviour. As in previous work, this finding provides evidence that participants were generally sensitive to the probability of a loss card being found (Figner, Mackinlay, Wilkening & Weber, 2009; Schonberg, Fox, & Poldrack, 2011) and were using this information to modulate the level of risk that they were willing to tolerate. However, the present results are particularly interesting because they show that holding a conversation reduced the extent to which people evaluated and made use of this type of information. That is, the reduction in the number of cards selected when more loss cards were available was significantly smaller when holding a conversation than when no conversation was being held. In Experiment 2, I provide a replication of Experiment 1 and also examine physiological responses. It is important to note that in Experiment 1 I also recorded EDA data. However, I did not present this data because approximately 14% of the data were lost due to a programming error. An initial analysis of this EDA data indicated that one of the potentially most interesting effects was that participants appeared to be less likely to show an SCR to a loss card during the conversation condition than during the no conversation condition. Typically, SCRs to specific events are analysed during a time window of 0.5 to 4 seconds (Pashler, 2012). However, because there was no 'blank' period between trials (e.g., after a loss card had been turned over) it was possible that any EDA following a loss card was contaminated by responses to events occurring in the next trial (for example, the onset of the next trial, or even the experience of turning over a second loss card). Experiment 2 was designed to investigate this finding further under conditions which would allow for EDA data to be more accurately associated with specific events in a trial.

EXPERIMENT 2: EFFECTS OF CONVERSATION ON RISK TAKING AND PHYSIOLOGICAL RESPONSES

Experiment 2 was designed to confirm the behavioural results from Experiment 1 and to examine potential differences in physiological responses (with a focus on activity following a loss card) to risk taking during conversation. The design was essentially the same as that of Experiment 1, except that a 6000ms delay was present between each trial. This allowed me to assess EDA responses as a result of turning over a loss card without contamination by events from the next trial.

Method

Participants

Forty participants[8] (24 female), aged 18 to 32 (*M*=21.5), recruited from the University of Warwick, Decision Research at Warwick participant panel, took part in return for payment. Part of the payment was contingent on performance in the CCT task. Experiment 1, was a first investigation of the effects of conversation on affective risk taking using the Columbia card task, it provided several key insights whilst, in addition, acting as a pilot of the experimental design. Experiment 2 was designed to replicate and validate the results of Experiment 1 but with the addition of reliable and valid physiological measurements. In order to achieve this, two key methodological

[8] In total 42 participants were tested, however one did not complete the experiment due to time constraints. One participant's data was removed because they reported to the experimenter at the end of the experiment that they had not complied with the task instructions. Two participants declined to provide their age. In addition, when performing the physiology analysis, it was necessary to remove a further 2 participants due to errors in the physiological data acquisition process.

changes were made. The first was to adjust the risk task in order to more accurately measure physiological responses and the second was to increase the sample size. The sample size chosen was based on previous literature (Figner, Mackinlay, Wilkening & Weber, 2009, Turnbull et al., 2005).

Stimuli and apparatus

Columbia Card Task. The experimental design was identical to that used in Experiment 1 with three differences: i) The computer used to run the task was a Dell XPS 420 with an intel core 2 Duo processor, this was connected to a Viewsonic monitor running at a resolution of 800x600 pixels, ii) Instead of a hands-free telephone, a laptop computer with an external microphone was placed to the side of the computer monitor to enable participants to talk freely with the experimenter who was in a different room, iii) a screen with text indicating to participants that the next trial was loading was presented for six seconds between each trial. This was to allow for physiological responses to return to baseline before the next trial began and to allow responses to be accurately matched to end-of-trial events such as turning over a loss card.

Electrodermal activity. Skin conductance was measured via disposable pre-gelled 1cm diameter AgCL electrodes (Biopac EL507) attached to the distal phalanges of the first and middle fingers of the non-dominant hand. The electrodes were connected to a Biopac MP36-R unit (Biopac Systems Inc., Goleta, CA) which transmitted data to a Samsung NP550P5C Laptop-PC running an i5 3210M Processor at 2.60GHz via a USB connection. Recordings were digitized (24-bit) at a rate of 1 KHz with an amplification gain of ×2000 via the Biopac AcqKnowledge v4.2 software. Electrodes were attached for approximately five minutes before data collection began. The CCT stimulus computer program signalled relevant events (the beginning of a trial, the end of a trial, presentation of a reward card, presentation of a loss card and clicking of the *STOP/ turn over* button) via an 8-bit digital signal connecting the parallel port of the stimulus PC to the MP36-R unit. These digital signals were recorded simultaneously, and were synchronized with the skin conductance measurements.

Design and procedure

The design and procedure was similar to that of Experiment 1 except that the participant conversed via a laptop computer running SKYPE and an external microphone, with the experimenter who was situated in a different room. In addition, participants completed an eight-trial practice block before the first full block of experimental trials began.

Results

I consider the behavioural measures first followed by the physiological data. As in Experiment 1, the data were collapsed across two or more variables where necessary.

Behavioural measures

Number of cards selected

As in Experiment 1, I determined the average number of cards participants chose to select across the various conditions with higher numbers indicating greater risk taking behaviour. Three participants were removed from this analysis because they had skipped over (i.e. chose not to turn over any cards) on an abnormally high number of trials (50% or more trials in a single condition).

Overall effect of conversation: In contrast to Experiment 1, there was no significant difference in the average number of cards selected in the conversation condition (M = 11.42, SD = 4.93) compared with the no-conversation condition (M =

10.64, *SD* = 4.91), *t*(36) = 1.585, *p* = .122, *d* = .261. Below I consider the possible effect of conversation when interactions between the other factors are taken into account.

Influence of the number of loss cards present: The mean number of cards selected (on non-loss trials) was analysed using a 2 (conversation / no-conversation) × 2 (number of loss cards: 1 or 3) within-subjects ANOVA. There was no significant main effect of conversation, $F(1,34) = 1.687$, $MSE = 7.733$, $p = .203$, $\eta^2 = .792$, however, as in Experiment 1, there was a significant main effect of the number of loss cards, $F(1,34) = 129.601$, $MSE = 6.343$, $p < .001$, $\eta^2 = .792$, and a significant Conversation × Number of loss cards interaction, $F(1,34) = 12.920$, $MSE = 2.912$, $p =.001$, $\eta^2 = .275$. As shown in Figure 6.2 (pp191), fewer cards were selected when there were three loss cards in the deck than when there was only one loss card, and this reduction was greatest in the no-conversation condition. Exploring this interaction further showed that when there was only one loss card there was no significant difference between conversation conditions, $t(36) = 0.976$, $p = .335$, $d = .161$ Conversation: $M = 12.8$, $SD = 4.4$; No conversation: $M = 13.4$, $SD = 5.2$). However there was a significant difference when there were three loss cards present, $t(34) = 3.218$, $p = .003$, $d = .544$ (Conversation: $M = 9.0$, $SD = 5.1$; No conversation: $M = 6.7$, $SD = 4.0$).

Influence of the loss amount: A 2 (conversation / no-conversation) × 2 (loss amount: 250 or 750 points) within-subjects ANOVA revealed a significant main effect of loss amount, $F(1,36) = 33.32$, $MSE = 7.690$, $p < .001$, $\eta^2 = .481$, but not of conversation ($F(1,36)=2.115$, $MSE = 9.157$, $p = .155$, $\eta^2 = .055$). The Conversation × Loss amount interaction approached significance, $F(1,36)=3.311$, $MSE = 2.672$, $p = .077$, $\eta^2 = .084$ (Figure 6.2, pp182). Examining this interaction further revealed a significant difference in the average number of clicks made between conversation conditions when loss was high (750) *t* (*36*) = 2.188*, p* = .035*, d* =.360, (Conversation: M = 10.5, SD= 5.1; No conversation: M= 9.3, SD=5.1), but not when loss was low (250) (*t* (*36*) = 0.407*, p* = .687 *, d* =.067, (Conversation: M = 12.7, SD= 5.1;No conversation: M= 12.5, SD=5.6).

Influence of the gain amount: A 2 (conversation / no-conversation) × 2 (gain amount: 10 or 30 points) within-subjects ANOVA revealed a significant main effect only of gain amount $F(1,36)=9.217$, $MSE = 10.728$, $p = .004$, $\eta^2 = .204$. There was no main effect of conversation, $F(1,36)= 1.310$, $MSE = 9.703$, $p = .260$, $\eta^2 = .035$, nor a significant Conversation × Gain amount interaction, $F(1,36)=0.216$, $MSE = 4.136$, $p = .645$, $\eta^2 = .006$.

Physiological measures

The raw skin conductance data were first trimmed to include only the period starting from the first card selected to the end of the final trial for each conversation condition, resulting in two files per participant (one for conversation and one for no-conversation conditions). EDA-related measures were then determined using the analysis functions within the Biopac acqKnowledge 4.2 software and custom scripts were written using the acqKnowledge scripting add on to expedite the analysis process. Prior to analysis the EDA data was smoothed by eye to remove artefacts (see Braithwaite, Broglia & Watson, 2014, for use of a similar approach).

Tonic shifts in SCL: The skin conductance measurements for each participant were split into 10 equal epochs for each conversation condition. The minimum SCL value was then determined for each epoch and an average of those values taken (see Braithwaite, Broglia & Watson, 2014, for use of a similar approach). The resulting average SCL level for the conversation condition (*M*= 12.73, SD= 3.81) was numerically larger than that for the no-conversation condition (*M*= 12.41, SD= 3.69), however this difference was not significant, $t(37) = 1.257$, $p = .217$, $d = .204$. .

NS-SCR frequency and amplitude: Here I define NS-SCRs as all SCRs regardless of their association with a particular stimulus event. The raw SCL data were first processed using a 0.05Hz highpass filter. NS-SCRs were then identified using an amplitude threshold of 0.03 microsiemens and a rejection threshold of 10%. The number of NS-SCRs that occurred in each block was then divided by the duration of the block to give a rate of NS-SCRs per minute. The rate of NS-SCRs was numerically larger in the conversation condition (*M* = 3.37 events per minute, SD= 1.40) than in the no-conversation condition (*M* = 3.32 events per minute, SD= 2.00), however this difference did not approach significance ($t(37) = 0.242$, $p = .810$, $d = .039$).

SCR amplitudes were first transformed into z-scores individually for each participant before being compared (Ben-Shakhar, 1985). A paired t-test on the transformed scores revealed no significant difference in the size of the response in the conversation (*M* = -.043, *SD* = .534) compared with the no-conversation condition (*M* = -.053, *SD* = .755), $t(37) = 0.480$, $p = .634$, $d = .078$.

Stimulus specific SCRs to loss cards. The number and size of stimulus-specific SCRs that occurred within a window of 500ms to 4000ms following the selection of a loss card were identified and compared between the conversation and no-conversation conditions. This analysis showed that the z-transformed amplitudes of specific SCRs following a loss card did not differ between the conversation (*M* = -.076, *SD* = .360) and no-conversation conditions (*M* = .115, *SD* = .341), $t(35) = 1.733$, $p = .092$, $d = .289$. However, the probability of an SCR occurring in response to a loss card being selected was significantly greater in the no-conversation condition (*M*

= .41, *SD* = .18) than in the conversation condition (*M* = .34, *SD* = .18), $t(37) = 2.508$, $p =. 017$, $d = .407$.

Discussion

In contrast to Experiment 1 I found that conversation did not result in a significant increase in the overall number of cards turned over - although the difference was in the same direction numerically. Furthermore, unlike in Experiment 1, participants were now sensitive to the loss and gain amounts. That is, more cards were selected when the gain amount was high and fewer cards were turned over when the loss amount was high. A likely account of this difference is that the addition of a six second delay between trials in Experiment 2 allowed participants more time to consider aspects of the task (such as the potential consequences of loss and gain amounts between trials). This opportunity for deeper processing could in turn have led to a greater impact of the information available on each trial, and appreciation of the risks involved with each action. Nonetheless, holding a conversation did have a robust influence on some aspects of performance related to risk taking. In particular, as in Experiment 1, participants were not as sensitive to the level of risk indicated by the probability of encountering a loss card during the conversation condition. That is, the reduction in the number of cards selected when more loss cards were available was significantly smaller when holding a conversation than when no conversation was being held. This suggests that holding a conversation reduces the amount of information considered when deciding whether to take a risk which in turn here led to greater risk taking. There was also some, albeit relatively weak, evidence that the amount of loss was also considered less well during a conversation.

Physiological measures and reaction to loss events

Following Figner, Mackinlay, Wilkening and Weber (2009), in addition to the behavioural measures provided by the CCT I also recorded participants' EDA in order to provide an objective measure of emotional reactivity to the task. Figner, Mackinlay, Wilkening and Weber (2009) found an increased magnitude of NS-SCRs whilst completing the hot version of the CCT compared with the cold version. This was attributed to a greater involvement of affective based decision making in the hot version of the task. In the current study I also measured EDA to assess emotional reactions during the hot CCT under conditions of conversation and no-conversation. Overall, there was no evidence for a difference in tonic SCL levels, frequency of NS-SCRs or the amplitude of the NS-SCRs in the conversation versus no-conversation conditions. This might be because general NS EDA was already at a maximum level in the hot version of the CCT. Alternatively, reduced activity due to distraction from the risk task might have been negated by an increase in activity as a result of holding a conversation. The current data cannot discriminate between these two possibilities.

In contrast, for specific-SCR EDA events, I found that encountering a loss card was significantly less likely to generate an associated SCR in the conversation condition than in the no-conversation condition. Interestingly, the amplitude of loss card related SCRs did not differ across the conversation and no-conversation conditions. This suggests that when a loss outcome was processed it had a similar emotional impact on the observer whether a conversation was being held or not. However, the probability of obtaining an emotional response to a loss was lower during a conversation.

One account of this finding is that conversing with the experimenter distracted participants from the CCT in such a way that they did not notice having clicked a loss card and as a consequence did not consciously attend to the event. However, this seems unlikely given the design of the experiment. When a loss card was encountered all the remaining cards in a trial were turned over and the loss amount was taken away from the round total. The participant was then required to press the “next” button and then wait for 6 seconds until the next trial began. An alternative explanation is that the loss might have been attended to but participants may not have evaluated the consequences of losses as strongly because attentional resources were more heavily focused on the conversation and not on the card task (see O’Regan, Deubel, Clark & Rensink, 2000, for evidence that people can also fail to process visual changes that they are nevertheless fixating).

Related to these possibilities, McCarley et al., (2004) observed that when participants held a naturalistic conversation their performance in a change detection task declined. It was suggested that this was due to visual encoding being degraded. Therefore, in the current study it is possible that the loss events, although observed, were not properly encoded and as such did not result in a physiological response. This explanation is also supported by results from Strayer, Cooper and Drews (2004) showing that, compared to single task conditions, participants’ memory for objects, at which they had been directly gazing, was attenuated when they were simultaneously holding a naturalistic conversation. This suggests that simply directing one’s gaze at an object is not enough to ensure durable memory encoding.

Finally, according to the Somatic Marker Hypothesis (Bechara, Damasio, & Damasio, 2000) people can develop an anticipatory response to situations/actions that cause a future event to occur. The anticipatory response can in turn cause people to change their behaviour so as to avoid (or encourage) the triggering of the event. A reduction in the likelihood of experiencing an emotional response to a loss card in the conversation condition might have led to loss events being treated as less negative. Hence there might have been less inclination to avoid the actions that could lead to the appearance of a loss card (i.e. the risky selection of another card). Related to this, Carter, Hofstötter, Tsuchiya, and Koch, (2003) found that increasing the cognitive demands on an individual while they are attempting to associatively learn reduces the chance that conditioning will occur. Similarly, successful emotional regulation of loss aversion is correlated with a reduction in amygdala activity in response to loss (Sokol-Hessner, Camerer & Phelps, 2013), and the amygdala has been shown to be a key neural component in producing SCR's in response to losses (Bechara, Damasio, Damasio & Lee, 1999).

Experiment 1 and 2 Combined Behavioural Analysis

In order to increase power and test the robustness of my findings I first pooled the data from both experiments to examine the overall effect of conversation on risk taking in terms of the number of cards turned over. Following this, to consider possible differences between the results of Experiments 1 and 2 as a result of the change in methodology (the inclusion of a six second interval between trials in Experiment 2) I conducted a further series of tests which mirrored the earlier analyses but included Experiment (1 and 2) as a between-subjects factor.

Overall effect of conversation on the number of cards turned over: The pooled data from Experiments 1 and 2 relating to the average number of cards turned over in the conversation and no-conversation conditions were analysed with a paired t-test. This revealed that participants turned over significantly more cards in the conversation condition (M = 12.40, SD = 5.04) than in the no-conversation condition (M = 11.25, SD = 4.76), $t(56) = 2.758$, $p = 0.008$, $d = .365$.

Influence of the number of loss cards present: The mean number of cards selected (on non-loss trials) were analysed using a 2 (Experiment: 1 or 2) × 2 (conversation / no-conversation) × 2 (number of loss cards: 1 or 3) mixed ANOVA. This revealed significant main effects of conversation, $F(1,52) = 10.62$, $MSE = 8.344$, $p = .002$, $\eta^2 = .162$, number of loss cards, $F(1,52) = 164.57$, $MSE = 5.969$, $p < .001$, $\eta^2 = .756$, and experiment, $F(1,52)=4.311$, $MSE = 50.71$, $p = .043$, $\eta^2 = .07$. There was also a significant Conversation × Number of loss cards interaction, $F(1,52) = 24.80$, $MSE = 4.226$, $p < .001$, $\eta^2 = .314$. No other interactions were significant, Conversation × Experiment, $F(1,52)= 3.155$, $p =.082$, Number of Loss cards × Experiment interaction $F(1,52)= 1.197$, $p =.279$, the three way interaction, $F(1,52)= 2.075$, $p =.156$. As shown in Figure 6.2, overall, more cards were turned over in Experiment 1 than in Experiment 2. In addition, participants turned over fewer cards when the number of loss cards was higher, but this reduction was significantly smaller in the conversation condition than in the no-conversation condition.

Influence of the Loss amount: A 2 (Experiment: 1 or 2) × 2 (conversation / no-conversation) × 2 (loss amount: 250 or 750) mixed ANOVA revealed significant main effects of conversation, $F(1,55)=8.089$, $MSE = 9.871$, $p < .006$, $\eta^2 = .125$, and loss amount, $F(1,55)=22.298$, $MSE = 6.055$, $p < .001$, $\eta^2 = .259$, but not of experiment, $F(1,55)=2.533$, $p = .117$. There was also a significant Experiment × Loss amount interaction, $F(1,55)=8.905$, $MSE = 6.055$, $p = .004$, $\eta^2 = .103$. As shown in Figure 6.2, more cards were selected in the conversation condition in both Experiments 1 and 2. In addition, participants selected fewer cards when the loss amount was larger, but this effect was significantly greater in Experiment 2 than in Experiment 1. No other two-way interactions were significant, Conversation × Loss amount, $F(1,55)=0.409$, p

< .525, Experiment × Conversation, $F(1,55)=1.404$, $p < .241$, nor was the three-way interaction, $F(1,55)=3.210$, $p < .079$, $\eta^2 = .055$.

Influence of the gain amount: A 2 (Experiment: 1 or 2) × 2 (conversation / no-conversation) × 2 (gain amount: 10 or 30) mixed ANOVA revealed that more cards were turned over in the conversation condition in Experiment 1 and in Experiment 2, $F(1,55)=7.92$, $MSE = 10.224$, $p < .007$, $\eta^2 = .122$. There was also a significant Experiment × Gain amount interaction, $F(1,55)=46.540$, $MSE = 7.911$, $p < .019$, $\eta^2 = .092$. As shown in Figure 6.2, in Experiment 1 gain amount had little influence, however, in Experiment 2, participants turned over more cards when the gain amount was larger (30 points vs. 10 points). No other main effects or interactions were significant, experiment, $F(1,55)=2.90$, $p=.094$, gain amount, $F(1,55)=3.12$, $p=.083$, Experiment × Conversation, $F(1,55)=2.23$, $p=.141$, Conversation × Gain amount, $F(1,55)=0.31$, $p=.582$, the three-way interaction, $F(1,55)=1.33$, $p=.253$.

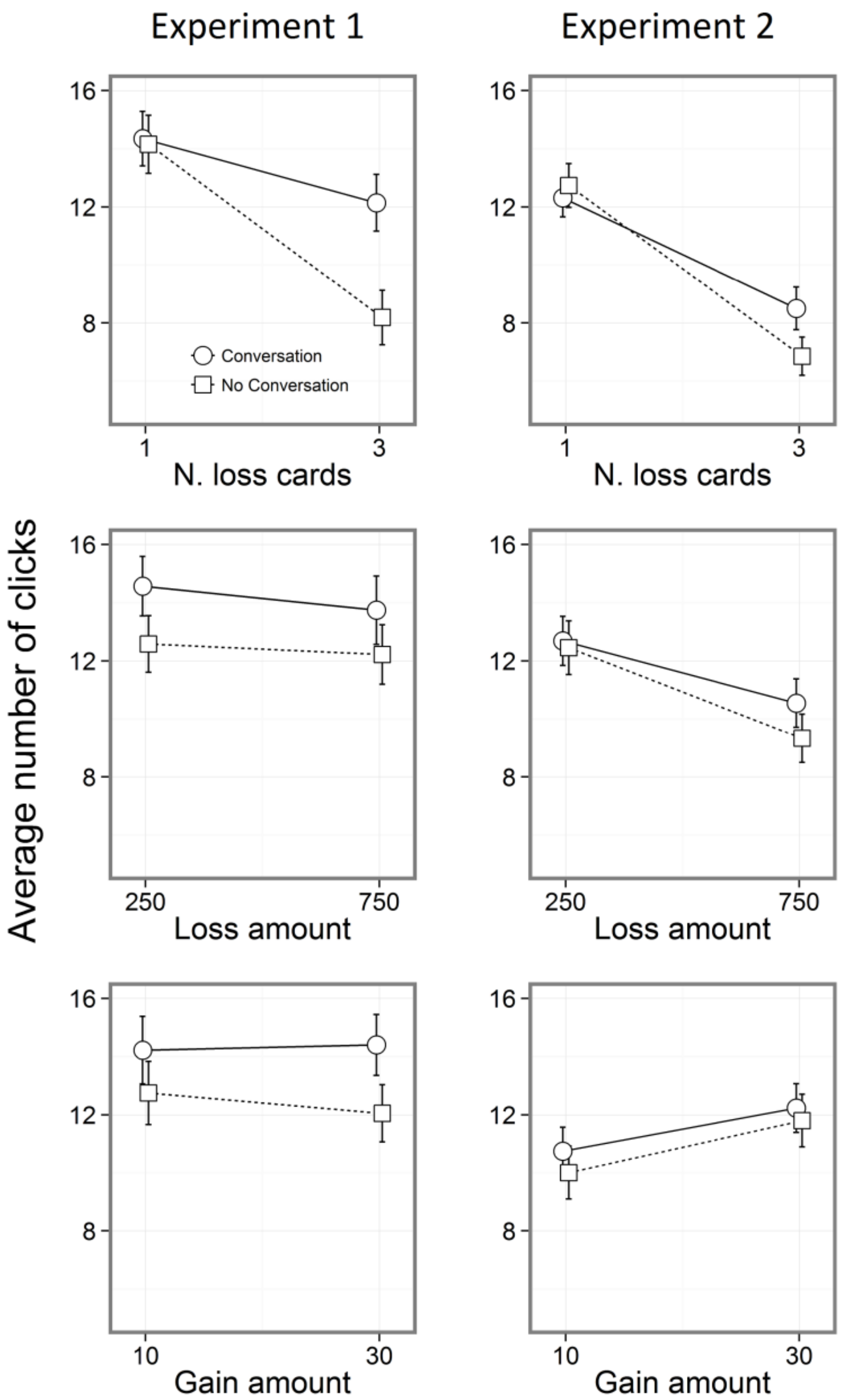

Figure 6.2 – The average number of cards selected on non-loss trials as a function of conversation condition and (Top) number of loss cards present, (Middle) loss amount and (Bottom) score amount. Data from Experiment 1 is presented in the first column and data from Experiment 2 in the second column. Error bars show ±1SE.

General Discussion

The main aim of this study was to examine the impact of holding a conversation on risk taking. Specifically, does the level of risk taking increase when holding a conversation? The overall answer appears to be yes it does. The main findings were: i) overall, participants turned over significantly more cards when holding a conversation than when not holding a conversation, which means that they were taking more risks, ii) participants reduced the number of cards they were willing to turn over when more loss cards were present. However, this reduction was smaller when a conversation was being held (Experiment 2). This suggests that less information regarding the level of risk was being processed or taken account of during a conversation, and iii) the frequency of showing a physiological response (SCR/orienting response) was reduced when holding a conversation in comparison to a no-conversation condition.

I also found that changing the amount of 'risk-free' time during the task seemed to influence risk taking behaviour. Specifically, the addition of the six-second delay between trials in Experiment 2 (required for the physiological measurements), may account for the fact that participants took fewer risks overall and were sensitive to the loss amount present within the task. I suggest that these blank delays allowed people more time to process the implications of the information presented to them in order to evaluate the risks present. In addition, it allowed a greater opportunity to evaluate the consequences of their actions following loss trials. Nonetheless, even with these delays between trials, there remained an influence of conversation on risk taking. Specifically, participants reduced their level of risk taking less when the loss amount was high during a conversation than when no conversation was being held.

Accounting for the effect of conversation on risk taking behaviour

Whilst the Hot and Cold versions of the CCT generally assess two different aspects of risk taking (deliberative and affective processes respectively; Figner, Mackinlay, Wilkening & Weber, 2009) there remains some overlap between the two tasks. Participants use predominantly affective processes in the Hot CCT, although deliberative processes also appear to be utilised to some extent (Figner, Mackinlay, Wilkening & Weber, 2009; Weber, Shafir & Blais, 2004). The results of the current study indicate that participants in both conversation conditions used the probability of encountering a loss card and the amount that they would lose in order to adapt their behaviour to some degree. However, this information appears to be used less effectively when a conversation is held. There are two possible reasons why this may arise. First, conversation might reduce participants' capacity to attend to and detect the loss information (what we might call an early processing level explanation), and second, the information might be attended to but the content not processed with respect to consequences for decision making (a higher level processing explanation). These two possibilities need not be mutually exclusive.

With relevance to the first suggestion, previous work (Kunar, Carter, Cohen & Horowitz, 2008) has shown that holding a conversation reduces performance in a task in which multiple moving objects have to be attended and tracked amongst a set of moving distractor items. Similarly, Humphreys, Watson and Jolicoeur (2002) found that monitoring an auditory stream for a target reduced people's ability to process and ignore (Watson & Humphreys, 1997, 1998) visual information already present in a scene. Thus auditory processing can have a negative impact on attention and the processing of visual information. Of note, the current task presented participants with a relatively complex visual scene (e.g., numerous cards to select and numerous pieces of textual information) which would require focussed attention to process the various elements. Hence it is possible that holding a conversation reduced the participants' attentional capacity to pick up risk related information (such as the number of loss cards present) and therefore this information had a reduced impact on their decision making.

Response inhibition, working memory and risk taking

Previous research has demonstrated a link between inhibition and the CCT. Participants who exhibited stronger inhibition in a go/no-go task, showed greater cognitive control and took fewer risks in the hot CCT (Figner, Mackinlay, Wilkening & Weber, 2009). Further, Knoch and Fehr (2007) applied low frequency repetitive transcranial magnetic stimulation to disrupt the right dorsal lateral prefrontal cortex, and suggested that diminished self-control results in riskier behaviour. Therefore, if deliberative processes are effortful (Weber, Shafir & Blais, 2004) and self-control or behavioural inhibition requires top-down control, dual task interference from a conversation might reduce the cognitive resources available to inhibit affective impulses, thus resulting in an increase in risk taking behaviour.

In terms of memory capacity, Figner, Mackinlay, Wilkening and Weber (2009) showed that for adults, working memory span (specifically backward digit span), was positively correlated with the extent of information use in the Hot version of the CCT. Furthermore, adults' working memory span was significantly negatively correlated with an increase in risk taking because of a hypothesised reduction in the ability to engage deliberative processes. Therefore, it is possible that conversation also increases general risk taking behaviour by increasing working memory demands which in turn reduces the involvement of deliberative processes (Turnbull et al., 2005). As a consequence, greater weight might be given to more affective decision making processes. With relevance to this, it has been suggested that the higher risk taking exhibited by adolescents in comparison to adults results from their greater reliance on affective processes due to an under developed cognitive control network (Figner, Mackinlay, Wilkening & Weber, 2009).

My physiological data indicated that holding a conversation whilst performing the CCT impacted upon the likelihood that participants would respond emotionally to negative events, in this case loss events. This finding alone cannot draw a distinction between the low or high level processing explanation of my results. However, it does provide corroborating evidence that elements of the risky task are not being processed to the same extent whilst participants are conversing. In single task conditions participants were more likely to respond emotionally to loss events, therefore it is possible that when an emotional response occurs it reinforces that turning over a loss card has negative consequences. However, whilst conversing, participants were less likely to experience an emotional response and therefore may not experience as much reinforcement, leading them to show less behavioural adaptation in later trials.

Whatever the cause of the heightened risk taking exhibited by participants in this study, it is a particularly troubling finding. While it is yet to be established how well the Columbia Card Task maps onto real world risk performance (Schonberg, Fox, & Poldrack, 2011), the present findings suggest that the consequences of holding a conversation could be severe. In fact, a study by Horswill and McKenna (1999) explored how interference in the form of an auditory monitoring task could affect how participants make dynamic judgements of risk. They were interested in the extent to which dynamic risk judgements may become automatic over time and if, therefore, auditory interference would affect them. They chose driving as a proxy for examining well-practised dynamic risk judgements made in the real world. They found that participants who had to monitor an auditory stream for a target item took more risks in a driver video simulation task. In contrast, in the present work I have demonstrated that a more common everyday task - holding a naturalistic conversation - can also impact upon dynamic risk taking behaviour. Much of the research in the area of driver distraction via mobile phones has focused on the effect of conversation on visual aspects of the driving task (Strayer, Cooper & Drews, 2004; Kunar et al., 2008; Horswill & Mckenna, 1999). The results of the current study suggest that another essential aspect of safe driving, our ability to make and evaluate risky decisions, might also be compromised by concurrently holding a naturalistic conversation.

Conclusion

The overall finding from this study is that holding a naturalistic conversation appears to increase risk taking behaviour. To the extent that the CCT is applicable to real-world behaviour the results suggest that holding a conversation could have numerous negative consequences whenever risk information has to be accessed and acted upon. To take just one example, applied to the field of driving, my results suggest that drivers might evaluate information less thoroughly and take more risks whilst holding a conversation whilst also being less aware of the consequences of their decisions.

In summary, Chapter 6 established that when investigating the effect of dual tasks on driving behaviour it is critically important to investigate beyond the domain of visual attention. While visual attention is of key importance to safe driving, our ability to perform efficiently in other domains such as making risky decisions should not be overlooked. Where naturalistic conversation is concerned we can see that people are likely to exhibit riskier behaviour whilst conversing and are significantly less likely to experience a physiological response to the negative consequences of their risky decisions.

Having investigated, via lab studies, naturalistic conversation in two key domains that are highly relevant for driving, I now move my attention to the field work conducted during this PhD project. As this project was a collaborative endeavour with Dorset Police, Driver Education the field work conducted in this thesis was highly influenced by the goals of my collaborators. I worked closely with my collaborators to design and conduct an intervention for use within their driver education courses. The intervention was designed to target and reduce participants' overconfidence in their visual abilities whilst driving. In order to do this I drew from a classic visual attention paradigm and applied it in a driver education context. I chose to base the intervention on Change Blindness (Rensink, O'Regan & Clarke, 1997). The interventions construction and evaluation is the focus of Chapter 7.

Chapter 7:

The Hazards of Perception: Change blindness within a real-world driver education course

Flaws and limitations in visual processing are well known within the psychological literature and yet because everyday perception *seems* so natural and complete to us, the layperson can be unaware of such weaknesses. People have the impression that vision is seamless, continuous and unlimited, and because of this they may overestimate their ability to perceive the world around them whilst performing everyday tasks. With relevance to the topic of driving behaviour, failing to notice or see objects can have catastrophic consequences for the driver and for other road users. Thus, making people aware of their attentional limits might encourage them to consider such limitations whilst driving, leading to safer driving behaviour. For example, being more aware of the limits of vision and attention might make people look just a little bit longer and more carefully, before pulling out of a junction, to make sure that they haven't missed something.

This general approach is not without precedence; demonstrations of visual flaws have been used successfully in other domains to improve safety. For example, a phenomenon named Motion Induced Blindness (Bonneh, Cooperman & Sagi, 2001), has reportedly been used to demonstrate to aircraft pilots the importance of moving their head and eyes around when scanning the environment, preventing the pilot from focusing on one particular spot which can cause a failure to notice stationary objects. This phenomenon is easily demonstrated by superimposing a global moving field of stimuli, such as a pattern of small blue dots, onto several high contrast visual stimuli, such as larger yellow dots, which do not move in the field (Bonneh, Cooperman &

Sagi, 2001). Typically the stationary stimuli will be observed disappearing for several seconds at a time. Accordingly, with the view that many psychological phenomena rarely make it out of the lab, in the present work I developed a domain specific demonstration of change blindness based around driver education. This demonstration was then evaluated within a real-world UK driver education course using both quantitative and qualitative methods.

Change Blindness

Change blindness (Rensink, O'Regan & Clarke, 1997) is the name given to the finding that it is incredibly difficult to notice changes that occur in a scene if those changes occur whilst one's vision is temporarily disrupted – e.g., during an eye blink (O'Regan, Deubal, Clark & Rensink, 2000) or even an eye movement, (Grimes, 1996; Henderson & Hollingworth, 1999). Examples of the consequences of change blindness can be found in many real world situations. For example, in one study an experimenter engaged the attention of a pedestrian and began a conversation. Halfway through the conversation workmen walked between the participant and experimenter, blocking the participants view with a door, while a confederate swapped places with the original experimenter (Simons & Levin, 1998). In this study only 50% of participants noticed the switch.

Demonstrating change blindness

The change blindness phenomenon can be demonstrated easily by repeatedly presenting two pictures, let's call them picture A and picture B, one after the other. Picture B is the same as picture A except that a single change has been made to it. For example, in a driving scene, picture B might be the same as picture A except that a car or pedestrian has been removed from the image. In a typical change blindness demonstration, picture A will be presented followed by a blank screen and then picture B followed by a blank screen and so on (e.g., see Rensink, O'Regan & Clarke, 1997). The interleaved blank screens simulate and have the same effect as making an eye blink (or eye movement, see earlier) by masking the transients between the two images that would normally indicate the location of a change. This A-blank-B-blank sequence repeats and the task is to try to find the difference between the two pictures. Typical findings show that people are exceptionally bad at spotting the difference between the two images even with prolonged viewing times (Rensink, O'Regan & Clarke, 1997).

Numerous factors influence how difficult a change is to observe in a change blindness task. For example, certain changes are easier to see than others. If a change is made to an image that is semantically inconsistent with the original image (Stirk & Underwood, 2007) it is detected more readily. Furthermore, not all change blindness paradigms require a visual occlusion, change blindness can still occur when something changes gradually in a scene (Simons, Franconeri, & Reimer, 2000). When changes occur in this way, older adults find it more difficult to detect these changes than younger adults (Batchelder, Rizzo, Vanderleest, & Vecera, 2003). In addition, cognitive load, bought about by trying to complete two tasks simultaneously, can also modulate change detection performance. For example, when participants hold a naturalistic conversation while viewing change blindness images, they are less likely to observe the changes in the images (McCarley et al., 2004). McCarley's study also showed that the context of a change is important; meaningful changes to a scene were detected more quickly than less-meaningful changes. Finally, cultural variation in change detection performance has also been reported. Masuda and Nisbett (2006), found evidence which suggests that East Asians, when compared to Americans, are more likely to notice contextual changes and are faster to detect these changes.

Whereas changes to objects were noticed equally as quickly, however, East Asians detected fewer object changes than did Americans.

The facts regarding how change blindness occurs and under what circumstances changes are harder to detect paint a stark picture for driver safety. It is possible that change blindness plays a major factor in road safety. Given that change blindness can occur under single task lab conditions (Rensink, O'Regan & Clarke, 1997) and that simple tasks such as holding a conversation can exacerbate the change blindness effect (McCarley et al., 2004), it stands to reason that a complex task such as driving may also make change detection more difficult. This might be especially the case given that a driver must continually update their representation of the world as they move through it at speed. Moreover, Cohen (1981) found that people make more saccades whilst driving than under 'normal' conditions. It follows that if saccades also lead to change blindness (Grimes, 1996; Henderson & Hollingworth, 1999) then drivers might also be at an increased risk of missing important events due to this increased frequency of driving-related eye movements.

Considered in this way it is clear that change blindness demonstrations could play an important role in driver education– which was the main aim of the present work.

Driver capability and over confidence

Overconfidence in one's ability has been found in a variety of domains (for a review see Moore & Healey, 2008), from academic study (Clayson, 2005) and financial decision making (Statman, Thorley & Vorkink, 2006) to driving ability (Svenson, 1981). Indeed in one study (Svenson, 1981), 70-90% of drivers reported that their driving was better and less risky than that of the average driver. Similarly, participants usually overestimate their ability to detect changes in a visual scene.

When participants were asked to state whether they would see certain changes if they were to occur within a scene, they stated confidently that they would. However, when other participants were tested via a change blindness technique, the actual detection rate for those changes was low (Levin, Momen, Drivdahl & Simons, 2000). Although it does not appear possible to teach participants to see changes more efficiently (Rensink, O'Regan & Clarke, 1997), it is possible that by increasing awareness of visual limitations, drivers will be more vigilant overall and perhaps more likely to avoid engaging in distracting tasks.

Overconfidence can be dangerous and can lead to risky decision making and potentially hazardous consequences. Sandroni and Squintani (2004) performed a literature survey focusing on health, driving risk and overconfidence. They concluded that overconfidence in one's driving may result in poor decisions such as not purchasing adequate insurance. The review also presents evidence suggesting that traditional driver education and training designed to educate drivers about the risks and hazards of driving may in fact be exacerbating the overconfidence problem by increasing participants' confidence in their own abilities. For example, Katila, Keskinen and Hatakka (1996) performed an evaluation of special courses run in several countries designed to make drivers safer on slippery roads. The authors suggest that one of the reasons these courses are not effective is that they may be making drivers feel more capable and are increasing driver's confidence in their ability to handle loss-of-control situations. This is because the courses promote practicing routines in controlled situations but these routines may not transfer well to real life scenarios. One consequence is that drivers who have greater confidence in their own abilities may feel comfortable driving more dangerously. Although more recent work from Katila, Keskinen, Hatakka and Laapotti (2004) suggests that this may be an oversimplification of the link between overconfidence and accidents and that higher confidence alone may not predict safety but rather one's skills and how those skills are used is of critical importance.

Such undesirable effects are not without precedence; it has been shown that adding safety measures to vehicles is not always effective at reducing accidents on the road. For example, Peterson, Hoffer and Millner (1995) examined data on the effects of introducing airbags into cars. They concluded that drivers compensated for the addition of airbags by adopting a more aggressive driving style, which negated the benefit for the driver and increased the risk to other road users. There are many explanations for these kinds of effects where the addition of a safety measure is not met with the expected increase in driver safety, for example, Hedlund's compensation index (Hedlund, 2000) and the controversial theory of risk homeostasis (Wilde, 1982). The general consensus of these theories is that in many cases the introduction of a safety feature or procedure results in the perception that the individual is now safer and so can offset this perceived increase in safety by taking greater risks (Vrolix, 2006).

There are a variety of models which can be used to try to understand and then change behaviour. One such model is the theory of planned behaviour which focuses on participants' attitudes, subjective norms and their perceived behavioural control (Ajzen, 1991). Whereas, the COM-B model of behavioural change highlights the importance of *capability*, *opportunity* and *motivation* for influencing behaviour (Michie, van Stralen, & West, 2011). Of particular relevance to the work of this Chapter is *capability* which is defined as "the individual's psychological and physical capacity to engage in the activity concerned. It includes the participant having the necessary knowledge and skills." (Michie, van Stralen, & West, 2011, p. 4).

Behavioural change interventions can, therefore, target a particular level or multiple levels, of the COM-B framework in order to affect a target behaviour. When behavioural change interventions focus on capability it is usually the case that they focus on improving capability or giving participants the capacity and the means to perform a behaviour such as encouraging healthy eating (Atkins, & Michie, 2013) and medical adherence (Jackson, Ecksonliasson, Barber, & Weinman, 2014). However, the capability component of the COM-B model could theoretically be applied to reduce an individual's confidence in their own knowledge or skill in performing an action in order to modify their behaviour.

Demographic differences in overconfidence

Overconfidence, in certain contexts, has been found to be related to both age and gender. Men have been found to be generally more overconfident than women in a variety of different domains, such as confidence in their academic test answers (Bengtsson, Persson & Willenhag, 2005), particularly when the answers are incorrect, (Lundeberg, Fox & Punćcohaŕ, 1994), when making investment decisions (Barber & Odean, 2001) and in competitive tasks (Niederle & Vesterlund, 2007). However, in the field of driving behaviour the data is not so clear. Young male drivers are more likely to underestimate their chance of having an accident (Finn & Bragg, 1986) and perceive driving as less risky (Rosenbloom, Shahar, Elharar, & Danino, 2008) relative to their peers and older drivers. In contrast, research which has focused on assessing driver capability and comparing this to self-report ratings of confidence of driving ability has shown that male drivers may not be any more overconfident in their abilities than female drivers (Mynttinen et al., 2009). The current work also touches on another variable that may interact with overconfidence, a person's age.

Studies examining the relationship between age and overconfidence have shown seemingly conflicting results. Menkhoff, Schmeling and Schmidt (2013), in an investment context found that older participants were more overconfident, whereas, in a different domain, Pliske and Mutter (1996) found that older adults were more accurate in their judgements of their own performance on a general knowledge test. In addition, as previously discussed, younger drivers are more likely to be overconfident than older drivers (Finn & Bragg, 1986; Rosenbloom, Shahar, Elharar, & Danino, 2008). Given this set of conflicting findings I also assessed the effects of age and gender in the present work.

A method to reduce overconfidence

Previous work, although not explicitly COM-B based, has shown that it is possible to reduce participants' overconfidence in their abilities. For example, although people are generally overconfident in their answers to general knowledge questions, it is possible to reduce such overconfidence (Arkes, Christensen, Lai & Blumer, 1987). One way to achieve this is to present questions which appear to be easy but are in fact challenging and then provide the participants with feedback on their answers. Arkes, Christensen, Lai and Blumer (1987) applied this method and showed that individuals were not as confident in their answers to subsequent general knowledge questions once they had been made aware of their performance on questions they had thought were easy. Pulford and Coleman (1997) showed that it is not the feedback which is important in this case but rather the mis-match between the perceived and actual difficulty. In the current study I adapt this approach for use in driver education by demonstrating to drivers how seemingly easy to spot changes, in a visual scene, may go unnoticed.

Overview of the current work

In the current study I evaluated the feasibility and effects of introducing a driving related, change blindness task into a real-world driver education course. Of particular interest was the effect that the change blindness demonstrations might have on participants' self-reported confidence in their observational abilities. In outline, participants first completed a pre-test questionnaire to obtain their baseline views of how observationally skilled they were. A series of change blindness demonstrations were then shown, followed by a post-test questionnaire that re-assessed participants' views. The questionnaires were designed to provide both quantitative and qualitative data. This mixed-methods approach allowed for a quantitative analysis of participants' confidence regarding their observational and other abilities while the qualitative data added detail and complementary information.

Method

Participants

160 participants (61 female, 96 male, 3 declined to answer) (Age = 18 to 85, Mean = 44.1, SD = 14.7) attending Police Driver Awareness courses (Dorset Police, UK) took part. Participants were told that their participation was completely voluntary and that any answers they gave would be treated anonymously and would have no bearing on their successful completion of the course. Participants were tested in groups of approximately 20.

Materials and stimuli

Six change blindness demonstrations were created. When designing the demonstrations several considerations were taken into account: i) it was important that the images related to driving scenes to match the content of the course (see Lees, Sparks, Lee and Rizzo, 2007), ii) it was necessary to minimize any potential cues that might lead to an artificial improvement in change detection across repeated demonstrations (e.g., ensuring that the change was not always in the same location and was not always the same type of object), and iii) changes that might be particularly easy to detect (e.g., changing many parts of the scene, whole regions, or introducing semantic inconsistences, Stirk & Underwood, 2007) were avoided to ensure an adequate demonstration of the change blindness phenomenon.

The images were all of typical driving situations and depicted varying traffic conditions and environments. In total 12 images were created, I then piloted these images and asked participants to rate how difficult the change was to observe and the relevance of the change to a driving scenario. The final six images were chosen to balance both difficulty to observe the change and driving relevance. For all images, identifying features such as number plates and road names were blurred out. Part of one of the images from each image pair was modified in order to create a difference between the two images. During a change blindness demonstration the two associated image pairs were presented sequentially with a blank grey screen interleaved between them. Each image was presented for 750ms and the intervening blank screen for 250ms. The total duration of each demonstration was 6 seconds (see Figure 7.1).

Side-by-side images of the six pairs were also created with a red outline circle highlighting the difference. Two additional side-by-side image pairs were also generated which were used when introducing the change blindness demonstrations to illustrate the type of changes that might occur in the sequentially presented displays. The two examples were chosen from pilot work and were rated the highest on relevance to driving, and perceived difficulty in seeing the change. Two questionnaires (pre- and post-demonstration) were designed to elicit responses relating to participants' confidence in their own observational abilities, and those of others whilst driving. The questions used a variety of scales, for example, confidence was measured on a scale from 1-7 which was anchored with “Not confident at all” (1) at one end and “Totally confident” (7) at the other. Whereas, perceived difficulty of spotting important changes was measured by asking participants to choose from five options, Strongly Disagree, Disagree, Neither Agree nor Disagree, Agree, Strongly Agree. For a full list of questions please see Appendix A. The demonstrations were presented on a 42in screen which was easily visible to all participants. The questionnaires were delivered in the form of multi-section paper booklets given to each participant.

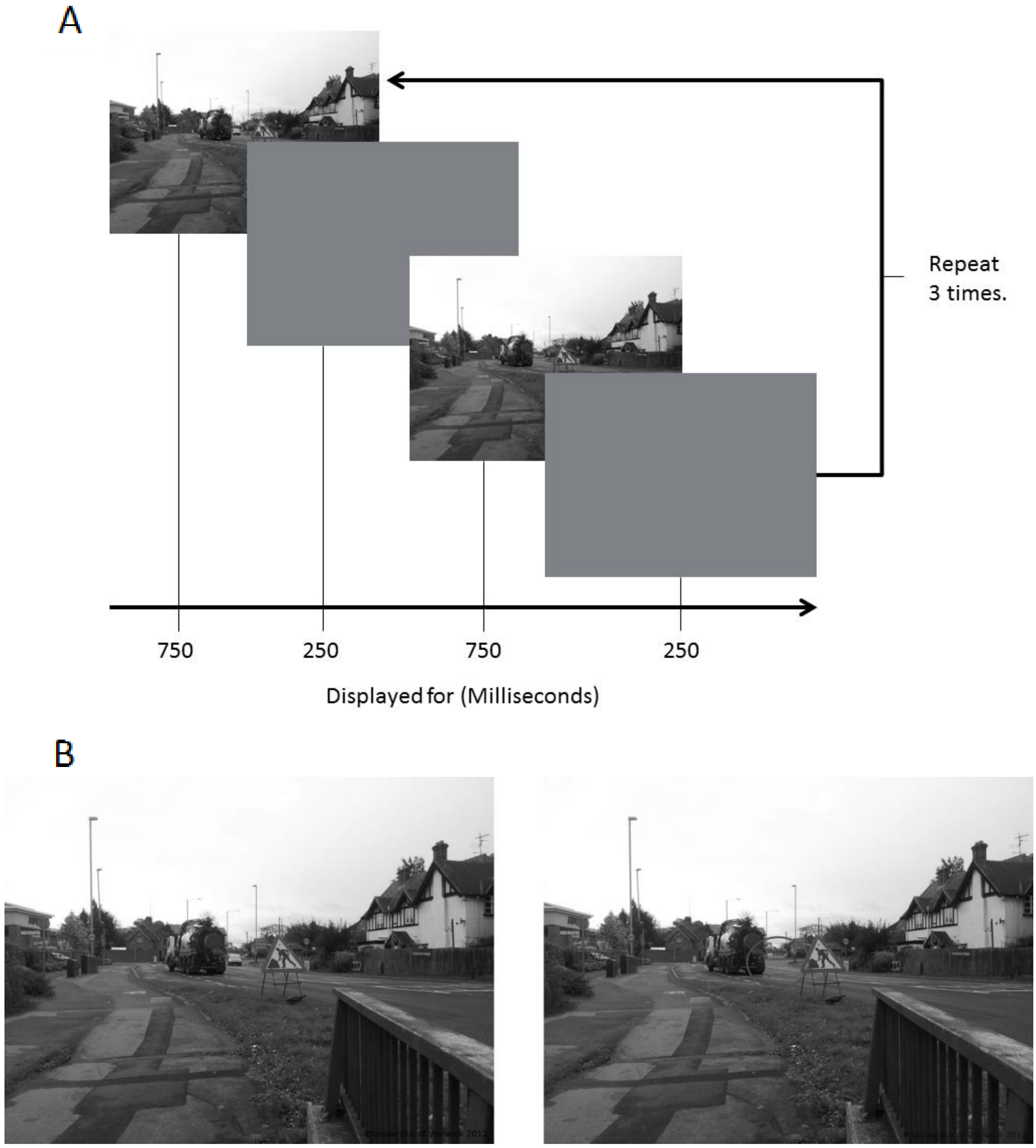

Figure 7.1 – An example of the procedure for a single change blindness demonstration. Participants viewed the interleaved images for a total duration of 6 seconds (Panel A). Following this, the instructor clicked on a “display the change” button which presented the two images side-by-side with the change highlighted by an outline circle (Panel B). During the change blindness task images were presented in colour however, here, they are shown in greyscale.

Design and Procedure

The demonstration was presented approximately halfway through the Driver Awareness Course run by Dorset Police (UK) Driver Education Unit and was delivered by the course instructors. In order to introduce and explain the study to the course instructors, the lead researcher made a site visit to go through the images that would be used and explain the purpose of the study. This also provided useful feedback from the course practitioners. In addition, an instruction sheet was provided to instructors which gave detailed information regarding obtaining informed consent from participants, when to deliver flashing imagery warnings and, importantly, outlined the procedure that the instructors should follow when delivering the change blindness demonstrations.

At a predetermined point in the Driver Awareness Course instructors handed out the booklets to all participants, the study was then explained and the participants were asked if they would like to volunteer to take part. Participants were informed that they would not be adversely affected in any way if they did not take part and that their answers would be analyzed anonymously. These two points were emphasized strongly to attempt to reduce biases caused by participants' potential concerns that instructors might be made aware of their responses. Booklets were collected from those who did not wish to take part. Warnings stating that the demonstrations contained flashing imagery were given at various times throughout the procedure and participants were advised not to take part if they thought that they might be sensitive to this.

Instructors asked participants to open their booklets and fill in the Pre-test questionnaire. Participants were then shown two examples in the form of side-by-side images to demonstrate the types of changes they might expect in the change blindness task. The instructors then explained the change blindness task to the participants and that they should try to identify what was changing in each scene. After each demonstration the two images from that demonstration were presented in a side-by-side format and the change was highlighted for the participants. After all six demonstrations had been presented participants completed the Post-test questionnaire, followed by debriefing and continuation of the Driver Awareness course.

Results

I first compared answers to questions that were present in both the pre- and post-demonstration questionnaires. I then considered responses to questions that were present in only the post-test questionnaire, followed by the open question responses.

Comparisons between the Pre- and Post-demonstration questionnaires

The answers to questions pre- and post-demonstration were analyzed using mixed ANOVAs with time-point (pre- or post-demonstration) as the within-subjects factor and age and gender as between-subject factors. Gender comprised two categories male (N = 96) and female (N = 61) and age was split into three categories, used previously (Shinar, Schechtman, & Compton, 2001), in driving related research 18-25 (N = 20), 26-50 (N=79), 51 and above (N=51).

Spotting important changes: A 2 (pre-/post-demonstration) × 2 (gender) × 3 (age) mixed ANOVA revealed that having seen the demonstrations, participants reported that *Spotting important visual changes…* was more difficult than they had previously thought, pre-test (Estimated Marginal Mean = 2.5, SE = 0.10), post-test (Mean = 2.1, SE= 0.08), $F(1,137) = 12.29$, $MSE = .418$, $p = .001$, $n^2 = .082$. However, no other main effects or interactions were significant (all Fs <= 2.30, ps >= .132).

Confidence in own and in others abilities: A 2 (Time-point: pre-/post-test) × 2(You or Others) × 2 (Gender) × 3(Age) ANOVA showed that ratings of confidence that *you/others see everything whilst driving* decreased between the pre- (Estimated Marginal Mean = 3.79, SE = .11) and post-demonstration questionnaires (Estimated Marginal Mean = 3.06, SE = .11), $F(1,141) = 65.69$, $MSE = .865$, $p < .001$, $n^2 = .318$. In addition, participants gave significantly higher ratings of confidence in their own ability (Estimated Marginal Mean = 4.03, SE = .12) relative to their ratings of others (Estimated Marginal Mean = 2.82, SE = .17) $F(1,141) = 88.01$, $MSE = 1.74$, $p < .001$, $n^2 = .384$. A significant Time-point × You or Others interaction, $F(1,141) = 22.33$, $MSE = .446$, $p < .001$, $n^2 = .137$ was also found. As shown in Figure 7.2, the demonstration produced a greater reduction in participants' confidence in their own abilities (mean reduction = 1.01, SE = .099) than in the confidence of others' abilities (mean reduction = .378, SE = .092), $t(147) = 5.807$, $p < .001$, $d = .48$. No other main effects or interactions were significant (all $Fs <= 3.45$, $ps >= .065$).

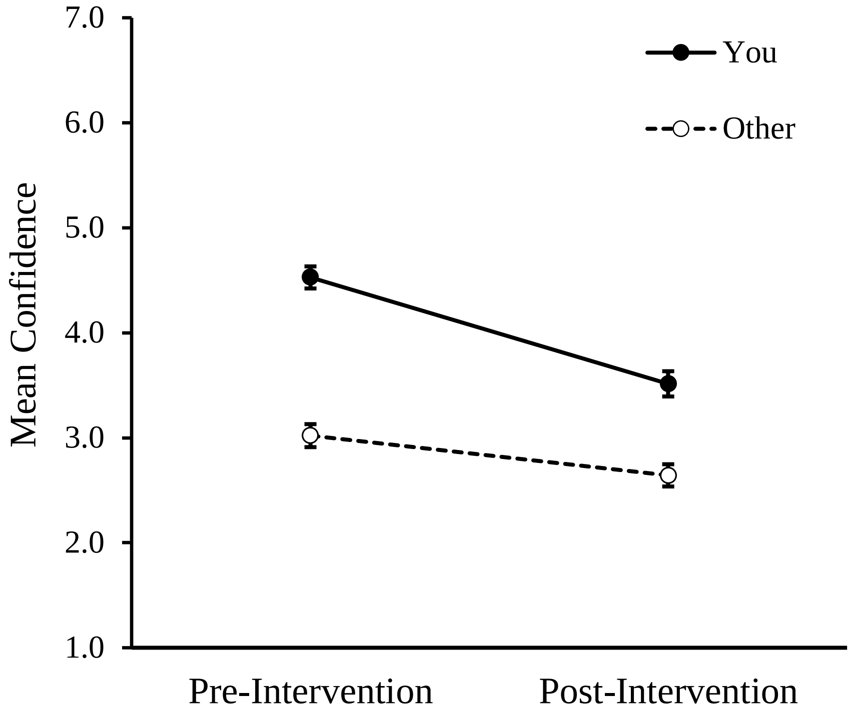

Figure 7.2 – Mean self-reported confidence in own and others' ability to *see everything whilst driving* measured pre- and post-intervention. Error bars show ±1SE.

Failing to see information: A 2(pre-/post-test) × 2 (Gender) × 3 (Age) mixed ANOVA revealed that participants' concern that they might *miss important visual information* significantly increased between the pre- (Estimated Marginal Mean = 4.5, SE = 0.15) and post-demonstration (Estimated Marginal Mean = 4.9, SE = 0.15) questionnaires, $F(1,141) = 9.347$, $MSE = .770$, $p = .003$, $n^2 = .062$. However, no other main effects or interactions were significant (all *F*s <= 2.62, *ps* >= .107).

At the end of the questionnaire participants were asked to state whether they agreed or disagreed with three statements. Participants answered on a 5-point scale from strongly disagree, disagree, neither agree nor disagree, agree, strongly agree, each response was given a numeric value from 1-5 respectively. Chi squared analyses were used to determine if the observed distribution of responses on each question, differed significantly from what could be expected if participants were equally likely to choose any of the 5 items on the scale. On average participants agreed (114 out of 151 participants selected agree or strongly agree, $\chi^2(4) = 118.172$, $p < .001$, (Mean = 3.9, SD= 0.9, Median = 4) that they were surprised by *how difficult it is to see/observe visual changes*. In response to the statement *Spotting important changes in a visual scene is easier than I expected it to be,* 99 out of 150 participants selected that they disagreed or strongly disagreed $\chi^2(4) = 72.533$, $p < .001$, (Mean = 2.3, SD = 1.1). Finally, 117 out of 151 participants stated that they agreed or strongly agreed that *I am now more aware of my visual limitations* ($\chi^2(4) = 105.921$, $p < .001$) (Mean = 4.0, SD = 0.9).

Univariate ANOVA were used to investigate these final questions further. Gender and Age were included as fixed factors. No main effects or interactions were found for any of the final questions (all *F*s <= 2.15, *ps* >= .135).

Open question analysis

In the post-demonstration questionnaire, I asked two open ended questions: 1) *Did you find the demonstrations useful?* and 2) *Do you think that the general public would benefit from viewing the demonstrations?* Thematic analysis (Boyatzis, 1998) was used to group and evaluate the responses given to these questions. The data were first split by question and then further split depending on whether the participant answered yes or no to each question respectively. The data were sorted into themes by the experimenter and an independent coder who was blind to the purpose and design of the experiment. The two coders worked independently at first and then discussed the results of their individual thematic categories. The results presented below are the combined agreed upon themes which were identified in the open response data.

Did you find the demonstration useful?

The majority of participants stated that they found the demonstration useful (130 out of 155, $\chi^2(1) = 71.13$, $p < .001$). All 25 people who answered “No” gave an open response to explain why they had answered no. Open responses to this question were grouped into two central themes. Please note where quotations are presented the participant’s age bracket and gender is also reported. First participants had concerns relating to the purpose or general applicability of the change blindness demonstration. In particular, some participants believed that the demonstration lacked relevance to driving behaviour while others claimed that the types of changes were not representative of changes which are likely to occur whilst driving.

“I didn’t think it resembled real life. I don’t look for observations like those when I am driving or out.”

-18-25, Female

"I don't think the test is a fair reflection of how well people concentrate and observe at the wheel"

-26-50, Male

The second theme concentrates on the design of the change blindness demonstration. Comments from participants which contribute to this theme indicate that they were finding the flickering distracting and that the flicker was too fast and made the changes difficult to see.

"A lot of the time the flashes were too quick and wouldn't actually put you in a genuine situation"

-18-25, Female

A reason given by some participants for not finding the demonstration useful was that they did not see the point of it. It is conceivable that these responses are related to the first theme that the changes were not seen as strictly relevant to driving behaviour.

One participant did not find the demonstration useful because of a lack of realism, but did make a suggestion regarding how it could be improved. The recommendation given below is useful for the future development of the demonstration and could be considered when using Change Blindness demonstrations in a road education context.

"I didn't find it a very realistic model of what happens on the road. A film or moving image where certain elements are suddenly removed would be better."

-26-50, Female

I next considered the themes that arose from those participants who answered "Yes", they found the demonstration useful. Of the 130 participants who answered yes, 120 gave an open response when asked to explain why it was useful. The reasons given by participants in this condition can be grouped into four central themes. The first and most regularly mentioned theme was that the demonstration raised awareness that it is important to maintain concentration whilst driving and continually be observant of your surroundings.

"It teaches you how hard you have to concentrate if you are going to see the whole picture" -26-50, Male

"Yes- because it makes you more aware. When most of the time you do not concentrate sufficiently."

-51+, Female

Of particular importance to this theme is that participants made reference to the fact that they were now aware that they can easily miss visual information and that they need to be looking at the whole picture rather than focused on just one spot.

"Made me realise I need to be more observant. It's easy not to see things that can be quite important."

-26-50, Female

"It has made me realise that I don't see everything in my fields of vision with a single glance so I do need to spend more time looking."

-26-50, Female

Another theme which is closely linked to the raising awareness of observation and concentration theme is that participants stated that the change blindness task actually demonstrated how different people see the world. Importantly they were able to see how people have different perceptions and focus on different things when looking around the world.

"It's interesting to see just how different people perceive different situations"

-26-50, Male

The third theme focused on the perceived applications of the change blindness demonstration. Responses which fell into this theme would consistently mention factors relating to the demonstration being useful as a training/re-education tool and that it was informative about aspects of road safety.

"Alerts you to potential dangers which will affect road safety" – 26-50, Male

"I answered yes because it made me think more about the things around me, even though I considered I was road aware."

-51+, Female

The final theme was confidence. Participants stated that the demonstration had made them question how confident they are in their ability to observe everything in the world around us. Participants expressed surprise that they had not seen the changes and stated that perhaps they may previously have been overconfident in their observational abilities.

"Made me realise how over confident I am at noticing things."

-26-50, Female

In addition to these very positive responses it should also be noted that one participant also noted that the demonstration was interesting but that it may not be completely relevant to road use. This will be considered alongside the themes that arose from the no responses presented earlier. Finally, one participant stated that the demonstration showed that they were quite observant. While this was not the intentional aim of the demonstration, the possibility that some people will notice many of the changes must not be overlooked and is a consequence of balancing the difficulty of the change detection with the relevance of the material.

Do you think that the general public would benefit from viewing the demonstrations?

Participants overwhelmingly stated that they believed that the general public would benefit from seeing the demonstration (128 out of 151, $\chi^2(1) = 73.01$, $p < .001$).

Open responses to this question were again split initially by whether or not the participant thought that the general public would benefit from viewing the demonstration. 21 of the 23 participants who did not think that the demonstration would benefit the public, gave open responses which grouped into two main themes. As might be expected, the themes were very similar to those provided by participants who did not find the demonstration useful. The first theme identified illustrates that for some participants the aim of the demonstration was unclear so they did not feel they learnt anything.

"*needs to be observed with a specific objective*" – 51+, Male

The second theme, as before, focused on the extent to which the demonstration was not realistic enough, or representative of real driving scenarios.

"Is not set in real time and made in same situation and condition as if you were driving ie. House disappearing."

-26-50, Male

Of the 128 participants that believed that the demonstration would be beneficial for the general public to see, 117 of them provided an open response when asked to explain why. These responses grouped into three main themes. As before, participants thought that the demonstration would help to show the general public the importance of maintaining concentration on the roads and how difficult it can be to observe in detail a visual scene.

"The exercise enables one to think about the fact that on first sight you are not gathering all the available or important information. It makes it clear that you need more time than you imagine to assess road risk."

-26-50, Male

A second theme identified was that the exercise demonstrated how easy it is to miss important information and that people in general are over confident in their ability to observe changes in the world around them.

"To many people believe and think they are observant. To much confidence."

– 26-50, Male

"In general it may be a perception that we believe we notice everything but reality is we don't and this exercise proved it."

-51+, Male

The third theme mimicked what was found in the open responses given to whether or not the demonstration was useful. Participants noted that the demonstration was effective at illustrating the differences between the participant and others ability to detect changes and importantly that not everyone views a scene in the same way.

"Raises awareness of lack of observational skills in self + other road users."

– 26-50, Female

"Show people how other people see things." – 26-50, Male

Some participants also noted that they believed that the demonstration should be presented to the general public as it would help to improve road safety.

"It would help give a greater understanding of road awareness" – 51+, Male

However, it was also noted that non-drivers, such as pedestrians would benefit from seeing the demonstration as it would raise awareness of how difficult it is to be observant whilst driving.

"To make all drivers and road users aware of how alert you need to be when using roads"

-26-50, Female

Finally, several of the participants made comments regarding how they thought it would be best to show this demonstration to the general public, some suggested a prime time television advertisement, another said that they believed it should be part of the national driving test. However, what should be mentioned is that it was also noted that the demonstration should be delivered in a controlled way. While it is not clear exactly what is meant by this, it is clear from some of the negative comments that the context that the demonstration is presented within and the explanation of the effect is very important in order for as many participants as possible to gain valuable insights from the demonstration.

Discussion

The main aim of the current study was to develop and assess the feasibility and effectiveness of presenting a change blindness demonstration within a driver education course. I predicted that demonstrating to participants that their visual system is not infallible might reduce unfounded confidence in their observational abilities and those of others. A necessary first step for my analysis was establishing if one of the key learning points from the intervention had been achieved. This was whether participants had been made aware of how difficult it can be to detect important changes in our immediate environment. My results suggest that this message was indeed delivered successfully. Participants, on average, stated that it is more difficult to "spot important visual information" after they had viewed the demonstrations.

Simple knowledge of the negative effects or risks of performing a particular behaviour can play an important role in affecting how likely someone is to perform said behaviour. For example, a factor found to motivate people to give up smoking was the bringing of smoking related risks into the public domain via clear and powerful package labelling (Hammond, McDonald, Fong, Brown, & Cameron, 2004). However, it was important that the demonstration also impacted on participants' attitudes, while increasing their knowledge base.

Attitudes play an important role in behavioural change and feature prominently in many models of behaviour (Ajzen, 1991; Michie, van Stralen, & West, 2011). Our analysis demonstrated that not only did participants state that spotting important changes was harder after the experiment, but also suggested that confidence in their own observational abilities had been reduced. I offer converging evidence for this point. First, participants reported that they were significantly less confident in their ability to see everything whilst driving, and second, participants showed greater concern that they may miss important visual information. Moreover, a qualitative analysis provided a wealth of examples of participants stating that they were surprised at how difficult it is to see changes like those demonstrated and that they are not as observant as they had previously thought.

This is a very encouraging finding and answers the main question posed by this study, whether attitudes towards observation and concentration, namely overconfidence in said abilities, can be attenuated by exposure to change blindness demonstrations. In addition, although the impact of this study on participants' real world driving behaviour was not investigated, the theme, identified from the qualitative data, regarding the need for greater observation and concentration while driving implies that participants may be critically evaluating the need to change their behaviour as a result of the adjustment in confidence brought about by the change blindness demonstration.

There are many examples of studies which have shown that attitudes are linked to intentions to perform certain behaviours (e.g., speeding: De Pelsmacker, & Janssens, 2007; texting while driving: Nemme, & White, 2010). Therefore, it is not unreasonable to predict behavioural change as a result of the attitudinal/confidence changes brought about by the demonstration. However, this was not explicitly tested for, as the main focus of the demonstration was lowering overconfidence, which has been suggested to be a contributing factor to traffic accidents (Harré, Foster, & O'Neill, 2005; Deery, 2000).

My findings suggest that participants consistently overestimated their ability to detect changes in a visual scene. This meshes with findings from previous change blindness studies (e.g., Levin, Momen, Drivdahl and Simons, 2000) and driving skill literature (Svenson, 1981). Stevenson, Palamara, Morrison and Ryan (2001) found that drivers who had medium to high ratings of confidence-adventurousness were around twice as likely as those with lower ratings to have a vehicular collision. In fact this “overconfidence” has been suggested as a major factor in road safety and driving related decisions by a variety of sources (Deery, 2000; Harré, Foster, & O'Neill, 2005; Katila, Keskinen & Hatakka, 1996; Sandroni & Squintani, 2004; see also, Vrolix (2006) for related work) and therefore it is very encouraging that a change blindness intervention was able to reduce overconfidence in a key driving related ability, at least in the short term. However, of course, future research will need to determine the robustness of this change.

Factors that might influence the effectiveness of change blindness demonstrations

In an attempt to be as effective as possible, the change blindness images were all of driving related situations (McCarley et al., 2004). They were also designed to cover a range of perceived difficulties so that some demonstrations contained “obvious changes” that participants would expect to notice easily but were, in reality, difficult to detect. Overall, participants were surprised by how challenging the changes were to see and stated that it was more difficult to observe the changes than they had originally thought. It is not possible to draw any firm conclusions about whether or not these design factors played a key role in the current study. However, given previous literature (Levin, Momen, Drivdahl & Simons, 2000), the perceived difficulty of change blindness images used in driver education and the asynchrony between their perceived and experienced difficulty (Arkes, Christensen, Lai & Blumer, 1987) should be carefully considered.

A flaw in many interventions designed to affect behaviour is that they can be avoided or their message denied by the individual who is being targeted. Ruiter, Abraham and Kok (2001) reviewed the literature on interventions which induce fear in their target audience in an attempt to influence future behaviour. Among the potential problems with fear inducing campaigns is that participants may deliberately avoid the campaign as a defence mechanism to control their own fear level. Therefore, the message will not be delivered successfully. Other studies of mass media road interventions have demonstrated that they may not be effective at reaching certain sectors of the population, such as people with lower degrees of education who are less likely to pay attention to a campaign (Weenig & Midden, 1997, see Hoekstra & Wegman, 2011 for a review of road safety campaigns). In addition, Harré, Foster and

O'Neill, (2005) found that when their participants viewed short films designed to demonstrate the dangers of drink driving they reported inflated opinions of their own driving skill. The authors suggested that this may be due to the fact that participants may judge others as having poor driving skills and therefore consider their own skill level to be higher.

The change blindness demonstration implemented in the current study may benefit from the fact that it was delivered in a group setting which allowed discussion and encouraged engagement with the material, while at the same time engaging participants and simultaneously demonstrating to each of them flaws in their visual awareness. By demonstrating the flaw rather than simply describing a behaviour and presenting examples of how others are affected by it, the participants could not as easily dismiss it as something that was not relevant to them and assume that the message was meant for other people. In fact, personalising the message has been suggested as a worthy pursuit for driver education campaigns (Hoekstra & Wegman, 2011). Furthermore, because change detection performance does not appear to improve with practice (Rensink, O'Regan & Clarke, 1997) the limits of visual processing could be experienced repeatedly by each group member further reinforcing the message.

Framing of the demonstration

As noted earlier, some participants reported that the procedure was not representative of real world driving scenarios, was unrealistic or that the flicker was artificially distracting. I note that the demonstrations were not solely designed to be completely representative of changes that may occur whilst driving. Rather, they were designed to demonstrate more generally, how easy it is for even relatively large changes to occur and yet not be perceived within a driving-related context. This does appear to have been well received by the majority of participants and was clearly identified as a theme in the responses by those participants who reported that they found the demonstration useful. However, for a minority this point appears to have been missed, perhaps as a result of variation in the presentation style of individual instructors. Although the instructors were briefed on the procedure, it is to be expected that there would be some variation in presentation style and emphasis. Although effective for the majority of participants in this study, it seems that ensuring that participants understand the general point of the demonstration will maximize its benefits. One way to achieve this might be to make participants aware of research findings, such as that holding a mobile phone conversation disrupts people's ability to detect changes in traffic scenes (McCarley et al., 2004). This may indicate to participants why they should think carefully about how observant they are whilst driving. At the same time the results would demonstrate a clear link between change blindness task performance and driving scenarios.

Own versus others' abilities

Participants reported higher confidence in their own ability to see everything whilst driving than in the ability of others. This self-superiority is consistent with previous work showing that drivers are likely to rate their own hazard perception ability as better than others (Horswill, Waylen, & Tofield, 2004). As discussed earlier, overconfidence in one's ability can have negative consequences (Stevenson, Palamara, Morrison and Ryan, 2001; Katila, Keskinen & Hatakka, 1996; Sandroni & Squintani, 2004). Conversely, assuming a lack of competence in others may be beneficial in making drivers more cautious of other road users. That is, you might be less confident that other road users have seen you. With this in mind, our findings suggest that both self-confidence and confidence in the ability of others were reduced by viewing the demonstrations. Participants appeared to realise that the task not only demonstrated their own lack of observational ability, but those of others as well. This effect might be driven by performing the task as part of a medium sized group in an interactive classroom environment, where participants were aware of how others were performing. Indeed, as reported earlier, one participant noted that they believed that all road users, including pedestrians, would benefit from viewing the demonstration as it would show them how difficult it can be to be observant of the whole visual scene whilst driving.

Demographic differences in responses

Contrary to what could have been predicted from previous literature (Barber & Odean, 2001; Bengtsson, Persson & Willenhag, 2005; Lundeberg, Fox & Punćcohaŕ, 1994; Niederle & Vesterlund, 2007), including specifically, studies which have found gender differences in how driving risk is estimated (Finn & Bragg, 1986; Rosenbloom, Shahar, Elharar, & Danino, 2008) I did not find a significant difference between male and female participants' responses.

Given that previous studies have indicated differences in overconfidence between different age groups (eg, Menkhoff, Schmeling & Schmidt (2013); Pliske & Mutter, 1994) in driving related judgements (Finn & Bragg, 1986; Rosenbloom, Shahar, Elharar, & Danino, 2008), I also examined the effect of age on confidence and the influence of the demonstration. I found no evidence for an effect of age on confidence judgements nor did age interact with the reported effectiveness of the demonstration. One possible explanation for this is the fact that all the participants had been offered the course as a result of committing a driving-related infraction. This might have acted to level any differences in confidence across the age groups. Driver awareness courses are populated by a diverse set of attendees, both in terms of age and gender. As such it is critically important that an intervention such as the change blindness task presented in this chapter, which is designed for use within a driver awareness course, is not biased to affect one subset of drivers over another. It is encouraging then that the results indicate that the change blindness intervention which I present here, is likely to be effective across a large and diverse range of participants irrespective of age and gender.

The reliability of drivers' self-reported data

It is well known that self-report data can be subject to biases such as participants being untruthful in their responses and answering in such a way as to conform to social expectations (Nederhof, 1985). However self-report questionnaires are nevertheless a useful tool for the assessment of interventions and participant attitude change that can be very difficult to measure effectively in other ways. In addition, there is precedent for the validity of self-report data in the driving domain. Lajunen and Summala (2002) asked two groups of people; applicants to a driver instructor training course, and students on the course, to fill in the driver behaviour questionnaire (DBQ; Reason, Manstead, Stradling, Baxter, & Campbell, 1990) and a scale designed to measure the extent to which participants were trying to give socially desirable answers. The applicants completed the questionnaires in public and the students completed them in private. There were few differences between the two groups but those who completed the questionnaires in public reported negative behaviours less frequently. Overall DBQ responses showed only a relatively small bias towards socially desirable responding.

This is encouraging for the current study as although participants completed the questionnaire in a group setting they were assured that their answers would be anonymous. This inspires confidence that the answers given were not simply a result of participants answering questions in what they perceived to be the socially desirable way. Evidence from the current study which supports this assumption is that not all answers followed what might have been perceived as a correct or desirable response.

Conclusion

The main aim of this study was to examine the feasibility and potential benefits of implementing a change blindness demonstration in a national driver safety course. The overall findings showed that such a task was effective in reducing the majority of participants' over confidence in their own observation abilities and those of other road users. The majority of participants reported that the demonstration was useful and that it would be valuable to present to other road users and the general public. Examining the longer term effects beyond the local context of the driver awareness course in comparison to other parts of such courses was beyond the scope of the current study and will be a goal for future research. Nonetheless at this stage I am confident that the change blindness task was effective in raising the awareness of observational limits, changing driver's self-reported attitudes, particularly overconfidence, and has the potential to cause positive behavioural change.

Chapter 8:

General Conclusion

A detailed discussion pertaining specifically to each individual chapter can be found at the end of each chapter. However, below is a general summary of the main findings from this thesis. In addition, the limitations of the work will be discussed as well as the implications and expected impact of the work.

Conversation and the top down guidance of attention

Through Chapters 2, 3 and 5 I attempted to dissect the effect that naturalistic conversation may have on our ability to perform one of the key elements of the driving task, visually attend to the world around us (Shinohara et al., 2010). In Chapter 2 I examined the effect of naturalistic conversation on our ability to benefit from previewing a subset of search items prior to searching for a target. As previously discussed, there is some dispute in the literature as to how a preview benefit is achieved (e.g. Donk & Theeuwes ,2001, 2003; Watson & Humphreys, 1997), however a convincing body of literature does point towards, at least in part, a role for top down inhibition of old stimuli (see Chapter 2 for a summary). This work supports the view of visual marking which states that top down mechanisms, reliant on memory and cognitive resources, are responsible for the preview benefit. Therefore, dual tasks which apply an additional load and so use up available cognitive resources, could be predicted to impact upon our ability to perform visual marking. However, the results showed that participants were able to experience a preview benefit even when asked to hold a naturalistic conversation whilst performing the search task. This may come as a surprise to those who argue for a top down, resource dependent explanation of the preview benefit (e.g. Watson & Humphreys, 1997; Watson & Humphreys, 2000). However, there is evidence to suggest that the preview benefit may, at least in part be bought about via bottom up mechanisms (Donk & Theeuwes ,2001, 2003, von Mühlenen, Watson & Gunnell, 2013).

Regardless of the fundamental theory, or indeed theories, underlying the preview benefit, the finding that it is not attenuated under a naturalistic dual load such as conversing is extremely positive. It suggests that if a person is conversing while simultaneously performing a task which relies heavily on visual attention and search, such as driving, then they will be able to benefit from the efficiency boosting mechanism, that is, visual marking, or the preview benefit. However, my findings also have another clear message, participants show a marked increase in their reaction times when they are conversing. This on its own is not new and in fact the studies presented in this thesis are able to add to a growing literature which indicates that simply holding a hands-free mobile phone conversation whilst driving has severe safety implications (e.g. Kunar et al., 2008; Shinohara et al., 2010; Strayer, Drews, & Crouch, 2006).

Shinohara et al., (2010) performed an experiment in which they asked participants to perform a visual search task while at the same time they were required to perform an auditory dual task, some of which required verbal responses from the participant. They observed that the participants showed a marked increase in their reaction times but that they showed no significant slope differences between load conditions, indicating that search efficiency was not affected by the dual load. However, the experiments performed in Chapter 2 of this thesis contradict these claims. I found that participants' search efficiency was significantly affected by dual task conversation. That is, the effect of conversation on reaction times increased as display size increased. This should not be taken lightly as the displays used in these studies were relatively simplistic and as they grew more complex participants' performance got markedly worse, relative to single load conditions. In the real world visual scenes are likely to be much more complex than those used in the lab and so we may expect this effect to be exaggerated even further. Therefore, while I can conclude that the preview benefit survives dual task conversation, drivers search efficiency and reaction times are very likely to be severely compromised by said conversation.

In addition to the key findings of this chapter, the data also added to the debate around the strategic deployment of visual attention. Whether or not attention and indeed attentional mechanisms, can be strategically deployed has been scrutinized by many researchers (e.g. Bacon & Egeth, 1994, Leber, & Egeth, 2006; Zupan, Watson & Blagrove, 2015; and the general discussion in Chapter 2 for an overview). My visual marking experiments where I altered the length of the preview duration can add to this debate. I performed two experiments, in each experiment the preview duration was varied between three possible durations. In the first experiment the preview durations were 750, 500, 250ms. The data from this experiment clearly indicated that participants were experiencing a preview benefit. However, when I reduced the preview durations in the next experiment to 250, 150, 75ms, I found no evidence of a preview benefit occurring. As the duration of 250ms appeared in both experiments I was able to analyze this difference further. A preview benefit was found in the first experiment when the preview duration was set at 250ms but not in the second. An explanation of this finding is that when on the majority of trials the top down inhibition of old items is a valid strategy to adopt, as in the first experiment, then a preview benefit is found. However, when on the majority of trials the top down inhibition of old items is not a valid strategy to adopt as the preview duration is too short for the old items to be fully encoded (Humphreys et al., 2004; Humphreys, Olivers & Braithwaite, 2006; Warner & Jackson, 2009; Watson & Humphreys, 1997), as in the second experiment, I find no evidence of a preview benefit. I therefore interpret this as evidence for the strategic deployment of attention within the preview paradigm.

While these results do indeed point towards a conclusion that attentional mechanisms such as visual marking, can be strategically deployed, this is just a beginning. Further work will be needed to investigate this further. For example, the whole picture may not be this clear, recent work by Zupan, Watson and Blagrove (2015) provides mixed evidence that visual marking can be strategically applied. Under certain conditions, when search was relatively easy moderate evidence for strategic deployment was found. However, overall their research suggested that visual marking was applied uniformly regardless of its efficacy in a particular situation. Therefore, while my work certainly adds to this debate, it still remains to be conclusively shown whether attentional mechanisms such as visual marking can be strategically deployed and under what conditions.

The findings of Chapter 2 provide a strong basis for the exploration of the effects of naturalistic conversation on the top down guidance of attention. However, that is not to say that there is not room for improvement. In these experiments I was able to show that overall participants were able to experience a preview benefit. However, I was not able to definitively assess whether this preview benefit was attenuated whilst the participant was conversing. This is because I did not include a half element baseline (HEB) condition in which participants search through a display which contains half the number of items presented in the FEB condition. Participants' performance in this HEB condition can be compared to their performance in the preview condition. If performance is statistically similar in both of these conditions then we can say that participants experienced a full preview benefit (Watson &

Humphreys, 1997). However, including a HEB condition significantly increases the amount of time required to run the experiment. A key concern when designing the Experiments presented in Chapter 2, was that participants should not be required to take part in the experiment for an extended period of time. Each experiment took around 50 minutes to complete and participants were required to converse for approximately half of that time. Including a HEB condition would likely have increased the duration of the experiment by a third. In order to balance participants' fatigue with collecting additional data the decision was made to not include the HEB condition. With that said, future experiments in this area could include this condition in order to further differentiate any effects of conversation on the preview benefit. However, they must carefully consider the impact on participants' time and perhaps attempt to reduce the length of the experiment in different ways such as by reducing the number of trials each participant completes in each condition.

In addition, I attempted in Experiments 2 and 3 of chapter 2 to increase the difficulty of the preview paradigm in order to increase the likelihood of detecting an effect of naturalistic conversation. The method used to accomplish this was to decrease the preview duration. However, it would also be interesting to manipulate other aspects of the preview task. For example, I found that participants' search efficiency was affected by naturalistic conversation, as such increasing the display size across all presentation conditions could help us to investigate this finding further. Additionally, it may be that as suggested in von Mühlenen, Watson and Gunnell (2013), the preview benefit is able to rely on both bottom up and top down mechanisms. Therefore, the stimuli themselves could be altered to reduce the effect of bottom up mechanisms and increase participants' reliance on top down mechanisms in order to experience a preview benefit. Of relevance, it has been shown that even when stimuli are iso-luminant to their background, a preview benefit can be obtained (Braithwaite, Humphreys, Watson & Hulleman, 2005; Braithwaite, Hulleman, Watson & Humphreys, 2006). Using stimuli like these and, therefore, forcing participants to rely on top down processes may provide more optimal conditions for naturalistic conversation to impact upon preview search.

Conversation and implicit memory

Moving on from top down guidance of attention, in chapter three I focused on establishing what effect naturalistic conversation may have upon participants' ability to implicitly learn and then express spatial contexts. As discussed in the chapter, the ability to learn the layout of familiar visual scenes allows us to efficiently and effectively process these scenes and guide our attention to relevant aspects of the scene (Chun & Jiang, 1998). This ability is especially critical when we are driving; being able to implicitly learn from our environment and then express that implicit learning could enhance our ability to identify hazards and locate important stimuli necessary for the completion of the driving task, such as road signs and markings.

Three experiments were performed in this chapter. The participants were asked to take part in a contextual cueing paradigm (Chun & Jiang, 1998) while either holding a conversation with the experimenter or performing no additional task. All three experiments show the same pattern of results. Naturalistic conversation did not significantly affect participants' ability to implicitly learn or express spatial contexts. After performing Experiments 1 and 2 I hypothesized that perhaps participants were finding it too easy to learn the spatial contexts and as such the additional load applied by the conversation task was not sufficient to cause any attenuation in performance. In Experiment 3 I attempted to address this by including both novel and repeated spatial contexts in the training phase of the experiment, where previously participants were only exposed to repeated displays in the training phase. However, I found that participants were still able to learn the repeated displays very quickly and so this remains a criticism of the current work. An effect of conversation may not have been found simply because the spatial contexts were learnt so readily that any difference caused by the conversation was not detectable. Future work should therefore look to confirm the findings of this chapter, but with spatial contexts that are harder for participants to learn perhaps by increasing the number of repeated displays that must be learnt, or by changing the structure of the displays to less easily remembered stimuli or larger noisier displays.

Despite this limitation, the results of the study are encouraging in that they suggest that we may still be able to rely on key attentional mechanisms which assist us in visually attending to world around us. This may be surprising given previous research that has shown that our implicit memory for words is impaired under conditions of dual task conversation (Strayer Drews & Johnston (2003). However, as discussed in Chapter 3, an explanation for this may be that implicit processing of visual scenes requires less resources than the processing of words. Alternatively the nature of my experiment, with displays being repeated many times may have provided ample opportunity for the scenes to be implicitly encoded, especially when we consider that displays can be learnt after only 5 repetitions (Chun & Jiang, 1998). It should be pointed out, however, that my work fits within the current research literature which suggests that only a specific working memory load, visual spatial load, is able to influence participants' ability to express learnt spatial contexts (Annac et al., 2013; Manginelli, Langer, Klose & Pollmann, 2013) and in some cases the learning of spatial contexts (Travis et al., 2013). In the context of this research it would appear that the load induced by naturalistic conversation does not overlap sufficiently with the resources required to benefit from contextual cueing (Wickens, 1980, 2002). Finally, it may be that participants to some extent began to explicitly learn the spatial contexts, however this was not mentioned by any of the participants at the end of the experiment. To ensure that this is not the case, if further experiments are to be run they would benefit from an explicit memory test of the spatial contexts being run at the end of the experiment (such as in, Chun & Jiang, 2003). This would allow us to ensure that the participants were implicitly and not explicitly remembering the spatial contexts.

In any case what is also clear from my results is that, like the results of Chapter 2, they add to the literature which shows that talking on a mobile phone causes an overall general increase in reaction times (Shinohara, Nakamura, Tatsuta, & Iba, 2010; Spence et al., 2013). As discussed previously, this is of vital consequence in the real world and has the potential to cause accidents and even fatalities if mobile phone use whilst driving is not addressed.

Naturalistic spatial load and the top down guidance of attention

In Chapter 2 I showed that naturalistic conversation, despite impacting upon participants' search efficiency and overall reaction times to find targets, did not appear to affect the preview benefit. One possible explanation of this was that conversation did not apply sufficient load, which overlapped in the same resource and modality as those required by the preview search task (Humphreys, Watson & Jolicoeur, 2002; Wickens, 1980). Therefore, in Chapter 4 I adjusted my focus in order to investigate whether a second naturalistic task, which, again, was likely to be performed whilst driving, would impact upon the preview benefit. I chose satellite navigation as the naturalistic distraction and designed an experiment which I believed would load similar resources to following satellite navigation directions. The task involved the encoding and then later recall of spatial directions. As previously discussed, a leading explanation of the preview benefit, visual marking, posits that the preview benefit arises by the top down inhibition of old items (Watson & Humphreys, 1997). Importantly this process is resource dependent and requires memory. Therefore, I hypothesized that the spatial load task, if performed concurrently with the search task, would attenuate the preview benefit.

However, the results showed that the spatial load task was not sufficient to induce an effect on the preview benefit. This was true even when visual-spatial load was isolated, as in Experiment 2 and the complexity and therefore level of load induced by the spatial task was increased in Experiment 3. It would appear then, that the naturalistic spatial-load task, as with naturalistic conversation, was unable to impact upon participants' ability to experience a preview benefit.

This has been covered elsewhere in this thesis, however, it should be mentioned that I now have two studies offering converging evidence that the preview benefit is not affected by additional load. This does therefore, warrant further investigation as it can be seen to add support to those who argue for a purely bottom up, automatic attention capture explanation of the preview benefit (Donk & Theeuwes, 2001, 2003). While my findings are in no way conclusive, it is certainly interesting that more naturalistic tasks are not able to impact upon the preview benefit whereas, more contrived, lab based tasks can (Humphreys, Watson & Jolicoeur, 2002). Further work, which aimed to extend this research, could look at a middle ground between these dual tasks in order to dissect the preview benefit further and find specifically under which conditions it is able to be attenuated.

A clear limitation of this work is that the spatial load task, although intuitively relying on spatial and memory resources, has not been conclusively shown to be dependent on those resources. The task was chosen because of its relation to the real world task of listening to and following satellite navigation directions. However, it is possible that the experiments presented in this chapter are limited by the fact that the task may not have been loading the resources that I expected. Further work should, therefore, look towards assessing the extent to which the load task impacted upon spatial and memory resources. In order to remedy this point and the point above regarding finding an alternative spatial load task with which to investigate the preview benefit, a task such as that used by Manginelli, Langer, Klose and Pollmann, (2013) could be used. Their task was shown to influence participants' ability to express learnt spatial contexts by exhibiting a spatial load and therefore it would be interesting to see if the preview benefit is similarly affected.

What is clear, however, is that my spatial-load task, like the conversation task in Chapters 2 and 3, produced an overall increase in participants' reaction times. However, unlike the conversation task, it did not impact upon participants' search efficiency. This is a reassuring finding for those who rely on satellite navigation whilst driving. However, it clearly indicates that naturalistic conversation is a potent distraction which should not be taken lightly.

Conversation and the three attentional networks

In Chapter 5 I extended the work of the previous three chapters by examining the effect of naturalistic conversation on the three fundamental and distinct attentional networks (Petersen and Posner, 2012; Posner 2008). I used the ANT which enabled me to dissect the effect of conversation on the orienting, alerting and executive control networks (Fan et al., 2002). In so doing I was able to investigate conversation in a much finer grained way than previous lab based attentional studies have achieved (e.g. Kunar et al., 2008; Shinohara, Nakamura, Tatsuta, & Iba, 2010; Spence et al., 2013; Strayer Drews & Johnston, 2003). The results showed that conversation did not have a statistically significant effect on either the alerting network or the orienting network. I argue that this may be due to the alerting and orienting cues acting to gather attention in a relatively resource free, automatic manner (for example see Jonides & Yantis, 1988; Posner, 1980; Yantis & Jonides 1984). Therefore, these networks are able to provide a benefit even while attentional resources are being strained by dual load conversation. However, participants did show a significant difference in the performance of the executive control network. In addition, as in Chapters 2 and 3 participants showed a marked increase in their reaction times when they were conversing.

The effect of the executive network was calculated by comparing trials in which the flanking distracters were congruent with the target with trials in which they were incongruent. Reaction times in these trials differed to a greater extent when the participant was not conversing, indicating that incongruent flankers were impacting upon participants' ability to quickly process and respond to the target. This also resulted in the participants making significantly more errors in incongruent trials when not conversing. When interpreting this finding in the context of load theory I find evidence to suggest that conversation may be applying a perceptual rather than a cognitive load (Lavie, Hirst, De Fockert & Viding, 2004). It is possible to make this distinction because we would expect cognitive and perceptual load to have different effects on distracter processing. According to perceptual load theory (Lavie, 1995; Lavie, 2005; Lavie & Torralbo, 2010), when perceptual load is low, attentional resources are left over and so additional stimuli such as distracters can be processed. However, under conditions of high perceptual load, resources are strained and so there is no attentional capacity left over and therefore distracters are not processed.

Whereas cognitive load would have the opposite effect; high load would equal more distracter interference. As less distracter interference, a smaller increase in reaction times and fewer errors to incongruent trials, was found when participants were conversing this indicates that conversation is applying a perceptual load.

This has relevance to and may extend research by Kunar et al., (2008) who found that participants' ability to perform a multiple object tracking task was impaired by conversation. The researchers suggest that this may be due to an "amodal central bottleneck" caused by conversation. The work presented in Chapter 5 has shown that this bottleneck may be brought about by a perceptual rather than a cognitive load.

In the experiment presented in Chapter 5, the additional load which conversing bought about actually caused a positive outcome. The number of errors made decreased and participants showed less distracter interference. However, in the real world, the effects of increased perceptual load are likely to cause several problems. Perceptual load can be thought of as narrowing the attentional spotlight and so reducing the amount of information that can be processed from a fixation. Murphy and Greene (2016) showed that perceptual load is directly relevant to the driving task and in fact, in conditions of high perceptual load, participants are much less likely to be aware of a pedestrian or animal located on the side of the road.

Considering how important being able to identify hazards and efficiently process visual scenes is to the task of driving. Any task, such as conversation, which reduces our ability to visually attend to a scene should not be performed if we wish to maximize driver safety.

However, it should be noted that the evidence presented in Chapter 5 is not on its own definitive. Further studies will need to be undertaken in order to confirm or refute the hypothesis that conversation induces an effect akin to high perceptual load. Further work could take the form of an additional study, one which attempted to replicate the findings of Lavie (1995) or Lavie, Hirst, De Fockert and Viding (2004). Applying naturalistic conversation as a dual load while participants attempt to perform a classic perceptual load paradigm may provide converging evidence that conversation is acting at least in part as a perceptual load.

The experiment presented in Chapter 5 was designed to mimic closely that used by Fan et al., (2002). As such, the orienting cue locations were fixed at a set distance from the fixation. It is important to consider what may happen if the distance between the target and the orienting cue were increased. This is because, it is possible that participants were experiencing a narrowing of the attentional spotlight due to the conversation but that the orienting cues used in Chapter 5 were appearing within this narrowed spotlight. Therefore, if this distance was increased, an effect of conversation on orienting may be more likely to be found.

Another potential limitation of this study was the overall very low rate of errors made by participants. While this is an unusual limitation it is a poignant one for this piece of work. In this study I found that participants made significantly more errors in the incongruent condition and even more so when they were not conversing. Despite this, the overall error rate was very low and as such it was necessary to perform an arcsine transformation on the data before it could be analyzed. Given the low statistical power of this analysis the findings must be taken with a large degree of caution and as such cannot add the weight required to solidly support the conclusion that conversation is, at least in part, causing a high perceptual load. Therefore, if the work was to be replicated, as suggested above, then it may also be beneficial to emphasize to participants the importance of responding as quickly as possible.

Emphasizing speed over accuracy in this way may provide more errors and therefore the analysis based on this data may be more convincing.

Conversation and risk taking

In Chapter 6 I deviate from the main theme thus far of investigating the effects of naturalistic distraction on visual attention. Instead I refocus on another essential aspect of the driving task, our ability to evaluate and make risky decisions, dynamically and in real time. I was interested in whether holding a naturalistic conversation while performing a risky task would increase participants' risk taking. To evaluate this possibility I asked participants to perform the Hot version of the Columbia Card task (Figner et al., 2009) while also conversing with the experimenter. When we are driving we must constantly evaluate the risks associated with our actions and select which action we are going to take. For example, should we overtake the car in front or, do I have enough time to pull out of this junction? These decisions are made quickly, in real time. The hot version of the CCT is well suited to investigate risk taking behaviour that is affective, dynamic and performed in real time (Figner, et al., 2009).

The findings presented in this chapter show that particularly when the level of risk was high, participants took greater risks when conversing than when they performed the CCT without any additional distraction. In addition, information pertaining to the "riskiness" of the current trial was always presented and visible at the top of the screen. I found that participants were using at least two of the three pieces of information when they were not distracted. However, when they were required to converse with the experimenter my data indicates that their level of information use actually decreased. I suggest both an early processing explanation of this effect and a late processing explanation. The early processing explanation is related to the visual attention literature discussed previously (e.g. Kunar, Carter, Cohen & Horowitz, 2008) and evidence from chapters 2, 3 and 5 of this thesis, showing that concurrently performed auditory dual tasks, such as naturalistic conversation, can affect the extent to which visual stimuli are attended to and processed. For example, the data in Chapter 5 indicate that conversation reduces the amount of information that we can acquire in each fixation. Therefore, the risk information may simply have not been prioritized and so as a result, was not detected or attended as it fell outside of the participant's attentional spotlight. If this is the case then it would not be able to be a factor in participants' assessment of how risky to behave in the trial. The late processing explanation, by contrast, hypothesizes that the naturalistic conversation is applying a load which allows for the risk information to be attended to, but which effects participants' ability to process this information effectively with respect to the evaluation of risk. Conversation may be, in this case, applying a cognitive load which is affecting participants' ability to apply adequate cognitive control to inhibit their actions. Both cognitive control and participants' ability to inhibit their actions have been shown to relate to risk taking in the CCT (Figner, Mackinlay, Wilkening & Weber, 2009).

Finally this chapter also showed that participants were less likely to experience a physiological or emotional response towards loss events. This finding offers converging evidence towards the idea that conversation is diverting attention away from the primary risk task and so participants are not processing aspects of that task, such as risk information and the outcome of their decisions, to the same extent. Additionally, if participants are less likely to fully process and recognize a loss event as a negative outcome, to the same extent whilst conversing, then they may not be cued to change their behaviour. The somatic marker hypothesis relies on anticipatory responses to actions or situations that lead to certain events occurring (Bechara, Damasio, & Damasio, 2000). As discussed in Chapter 6, if participants are not experiencing losses in the same way whilst conversing they may not learn to adapt their behaviour in future trials.

In the context of driving, our results indicate that people who concurrently converse on the phone may be repeatedly making riskier decisions than usual and not processing information pertaining to the risks of their behaviours. In addition, they may also not process the negative consequences of their actions to the same extent and so may continue to perform risky behaviours that they may otherwise have learnt to avoid.

One possible criticism of the study presented in Chapter 6 is that, despite intuitively overlapping with risk behaviour relating to driving, it still remains to be seen how externally valid the Hot CCT is to real world risk taking behaviour (Schonberg, Fox, & Poldrack, 2011). However as mentioned in chapter 6, a study was performed by Horswill and Mckenna (1999) which investigated dynamic risk taking in a driving context (video-simulations) and the influence that an additional auditory task may have upon it. They found that monitoring an auditory stream for a target resulted in greater risk taking. This study demonstrates that dynamic risk taking, specifically in a driving context, is susceptible to interference from an auditory task and as such, provides precedence for the findings of the study presented in Chapter 6 being applicable to the real world. A possible extension of this work could be to assess the extent to which the CCT maps onto driving behaviour by asking participants to take part in a driving simulation in which they encounter risky situations and either before or after the simulation they would also complete the CCT.

As a follow up to this point, future research in this area could investigate whether the findings presented in Chapter 6 can be replicated using another lab based measure of risk taking such as the Balloon Analogue Risk Task (BART Lejuez, et al., 2002). The BART has a wealth of academic literature supporting it and has been, to some extent, validated to real world risk behaviour (Schonberg, Fox, & Poldrack, 2011). Therefore, demonstrating that naturalistic conversation also impacts upon risky decision making in the BART would provide both converging evidence for the reliability of the findings of Chapter 6 and add weight to the assumption that the findings are ecologically valid.

The work of Chapter 6 provides a strong basis from which naturalistic conversations effect on risk taking behaviour can be explored. Although it was not possible to achieve a full understanding of this effect within the confines of this thesis, it would be beneficial to perform several additional experiments such as with other risk taking tasks and with clear theoretical questions in mind. For example, the study could be repeated, but with clearly defined age groups so that I could consider age as a factor. This is because of the fact that it has been hypothesized that adolescents may rely to a greater extent on their affective processes as their cognitive control network is not as developed as it is in older adults (Figner, Mackinlay, Wilkening & Weber, 2009). This may account for the fact that adolescents are likely to take greater risks. Therefore, we may find that the effect of conversation on risk taking performance varies for younger adults than older adults. In addition to this, it would be interesting to perform additional work which asked participants to complete the Cold CCT whilst conversing. According to Figner et al., (2009) this task relies on deliberative processes and as such conversation may have an increased effect on performance in this task due to the fact that deliberative processes are effortful

(Weber, Shafir & Blais, 2004) and so additional load may affect participants' cognitive control. In the real world it is possible that a reduction in cognitive control could lead to more affective and impulsive decision making (Figner et al., 2009) which could result in increased risk taking. Alternatively, conversation may have a reduced effect on performance because participants' decision making is slower and more calculated, participants are able to take their time to calculate and respond in a trial and so distraction may be less likely to influence their decision making. Assessing both the affective and deliberative processes behind risk taking behaviour is important as in the real world it may be difficult to say with certainty that one process is being used and not the other, or in fact a combination of the two. Therefore, for a thorough understanding of how naturalistic conversation may impact real world risky decision making, it will be necessary for future work to take into account both of these key processes. Further work of this nature not only has the potential to add to the literature around who makes risky decisions, when and how by establishing the specific aspects of risky decision making that are affected by dual task naturalistic load. It also has the potential to provide key insights into real world risk behaviours such as reckless driving, which if better understood could aid in forming interventions to tackle these behaviours.

While every effort was made to carefully design and conduct the lab experiments presented in Chapters 2-6, with the benefit of hindsight there are changes which could have been made to improve the reliability and validity of the findings. If the effects of naturalistic conversation are going to be investigated further in a similar manner to the above then it may be beneficial to introduce an external measure of participants' performance in the conversation and a subjective measure of the level of load that the conversation is applying. This could be achieved through applying a memory test at the end of the conversation stage of the experiment to assess the extent to which participants engaged with and can remember the content of the conversation which they participated in. In addition participants could be asked to fill in a subjective measure of workload such as the NASA-TLX (Hart & Staveland, 1988). This would allow us to better understand and dissect the effects of conversation on visual attention and risky decision making.

Change Blindness as an educational tool

In Chapter 7 I present an educational tool that I developed with my collaborators to be used within driver awareness courses. This work was completed in situ with Dorset Police Driver Education Unit. The tool was built with a specific learning objective in mind, reducing driver's overconfidence in their observational abilities, thereby, hopefully, leading to safer driving behaviours.

The tool was based on the visual attention paradigm Change Blindness (Rensink, O'Regan & Clarke, 1997). Participants filled in two questionnaires, one before the change blindness demonstration and one at the end. These questionnaires were designed to gather information about the perceived usefulness of the demonstration as well as to measure participants' attitudes and confidence towards their observational abilities whilst driving. The change blindness demonstrations were all still images of road situations where something in the image would change. As discussed in the chapter, I piloted several images and selected images in order to balance relatedness to the driving task and difficulty of detecting the change.

The data from this study showed that the demonstration was effective at raising awareness of observational limitations and caused a significant change in participants' attitudes, particularly their overconfidence in their observational abilities. In addition, I was able to show that participants were biased towards higher confidence in their own abilities relative to their confidence in others. This is in line with previous literature which has shown that there is a general trend for participants to believe themselves to be better than average, specifically in relation to driving (Horswill, Waylen, & Tofield, 2004; Svenson, 1981).

The general finding, presented in Chapter 7 that participants gave high ratings of confidence in their observational abilities before viewing the demonstrations and that these ratings were reduced after the demonstration finds precedence elsewhere in the change blindness literature. For example Levin, Momen, Drivdahl and Simons, (2000) found that participants were typically overconfident in their ability to detect the changes in change blindness trials.

The fact that my study was able to show that participants' confidence and attitudes towards overconfidence were affected by the change blindness demonstration is a very encouraging finding. Simply improving participants' knowledge of the danger of their actions has been shown to have positive effects on said behaviour, for example in the case of cigarette packaging warning messages (Hammond, McDonald, Fong, Brown, & Cameron, 2004). It is a reasonable assumption to make that altering attitudes or in this case confidence is also likely to lead to behavioural adaptation in this context. This is because it has been suggested that overconfidence has a strong link to road safety. Stevenson, Palamara, Morrison and Ryan (2001) found that drivers who had medium to high ratings of confidence-adventurousness were around twice as likely as those with lower ratings to have a vehicular collision. In fact this "overconfidence" has been suggested as a major factor in road safety and driving related decisions by a variety of sources (Deery, 2000; Harré, Foster, & O'Neill, 2005; Katila, Keskinen & Hatakka, 1996). In addition, we can find evidence that attitudes link to real world behaviours through frameworks such as the theory of planned behaviour (Ajzen, 1991). This has also been specifically demonstrated in the field of driving behaviour (De Pelsmacker, & Janssens, 2007; Nemme, & White, 2010).

The work presented in Chapter 7 was an initial exploration of the effectiveness of a change blindness demonstration as a tool for driver education. It, therefore, fell outside of the remit of the work to investigate whether the demonstration was effective at changing real world driving behaviours. Future work, aiming to extend these findings, should look towards frameworks such as the theory of planned behaviour to enable us to start to build theoretically strong links between participants' attitude changes and their intentions to change their behaviour. In addition, it would be beneficial to include items from the Driver Skills Inventory (Lajunen & Summala, 1995) which would allow us to assess whether key driving related skills are being affected by the demonstration such as observational abilities (e.g. hazard perception) and whether the demonstration also impacts upon other areas assessed by the DSI. More longitudinal work using questionnaires completed over a longer time period (eg, six months), could also look at detecting whether the demonstration had a lasting effect on participants' attitudes, but also importantly, assess whether any driving related changes had been observed by the participant, utilizing both the DBQ (Driver Behaviour Questionnaire; Reason, et al., 1990) and DSI to achieve this.

It should also be noted that it cannot be ruled out that participants' answers were due to demand characteristics and the fact that they were taking part in the questionnaire in a group setting, for example, the participants may have assumed that they were supposed to say that they were more concerned with their observational abilities after taking part in the course. However, I attempted to eliminate this by ensuring participants of the anonymity of their answers. In addition, participants were made aware that their answers would have no bearing on their successful completion of the course. I hoped that this would increase participants' motivation to give truthful answers. Research by Lajunen and Summala (2003) supports this assumption as it showed that people's answers to driver related questions exhibited only a small social desirability bias.

Finally, future work would benefit from extending the demonstration to make its applicability to real world driving scenarios more intuitive to participants. This could be done by using a short video which incorporates change blindness. For example, if the film was shot from a driver's perspective and items in the scene changed when the driver looked down to check their phone, were distracted by a passing pedestrian or simply made an eye blink. This was raised as a possible improvement in the qualitative data collected in the study and is a constructive point. Making these changes to the delivery of the demonstration would, it is hoped, make the demonstration more engaging and may make its application to driving behaviour more obvious to participants.

As it stands this demonstration has been very well received by Dorset Police Driver Education and has been included as a tool which they are able to deploy as part of their driver awareness courses. The demonstration has also been presented to key academic and emergency services personnel as well as a representative of the national driver awareness courses (e.g. at an impact day held at the University of Warwick which was held to showcase the work of this PhD thesis on the 21st of July 2016). It is hoped that in the future the demonstration, or a later version, will be shown more widely. In fact a website is currently under development which will host this demonstration online.

Conclusion

This thesis has had two distinct, but related, aims. Working with my collaborators, Dorset Police Driver Education Unit I identified key areas of academic interest and real world impact. Therefore, one of the key aims of the thesis was to better understand the way in which distraction may influence driving related behaviours. To that extent, I have shown that naturalistic conversation, a key behaviour which is often undertaken whilst driving, impacts upon not only the way in which we perceive the world around us but the way in which we behave in this world. I have presented evidence which indicates that holding a mobile telephone conversation whilst driving is likely to reduce how efficiently we can search through the visual world, slow our reaction times in response to events in the world and that the information which we are able to gather from a single fixation is significantly reduced. In addition, I have demonstrated that we are likely to take greater risks, especially in situations where it is unadvisable to do so. More worrying still, when negative events occur, we are less likely to experience a physiologically indexed emotional response which may affect our ability to learn from such events.

However, what I have also shown is that several key attentional mechanisms are, perhaps surprisingly, robust to the influence of dual task naturalistic conversation. Specifically participants were able to experience a preview benefit, even at very short preview durations, whilst holding a conversation. In addition, participants in my experiments were able to learn and then express spatial contexts as efficiently under dual task conditions as under single task conditions. On a more fundamental level I demonstrated that participants' ability to orient their attention and become alerted and maintain an alerted state whilst conversing was, at least in my experiments, not impaired. This is reassuring as it indicates that despite the negative impacts outlined above, we are still able to benefit from key efficiency boosting attentional mechanisms even whilst distracted by dual task conversation.

In addition to the lab based work of this thesis, I aimed to introduce a tool which could be adopted by my collaborators for use in the driver awareness courses which they deliver. To that end I designed, from a visual attention and behavioural change perspective, a driver education demonstration. The demonstration, based on change blindness, and designed to reduce driver's overconfidence in their observational abilities was extremely well received and the data collected indicates that it was successful in significantly reducing participants' self-reported confidence in their observational abilities. In addition, the qualitative data which was collected supported the quantitative findings and suggested that the significant changes that I detected have the potential to transfer into real world behavioural change.

It is my hope that the combination of the lab based and field work that has contributed to this thesis will not only have a strong academic impact, but will also be applicable to, and have an impact in, the real world. The findings have key implications in the discussion around the safety implications of using a mobile telephone whilst driving. In addition, the findings have clear implications for designers of in vehicle technology and safety features. With my collaborators in mind, my findings have the potential to impact upon driver education, drawing drivers' attention to the dangers of their behaviour and equipping course providers with theory driven evidence to support their arguments and an educational tool able to support key learning objectives. In summary, this thesis adds weight to the arguments for banning mobile phone use while driving and points to the dangers of in-vehicle distraction which, as I have demonstrated, can be unintuitive and severe.

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Appendix A

Below is a copy of the Questionnaire that was used in the study presented in Chapter 7. Part 1 is the Pre-demonstration questionnaire and part 2 is the Post-demonstration questionnaire.

Questionnaire: Part 1

Please circle your Gender.   Male   Female   Other ____________

Please indicate your age in years ___________

Please select the phrase which you believe best fits in the gap in the sentence below.

Spotting important visual changes, such as a child running out from behind a parked car, is ___________.

Very Difficult	Difficult	Neither difficult nor easy	Easy	Very Easy

How confident are you that you see everything whilst you are driving? (please circle a number on the scale below)

Not confident at all						Totally confident
1	2	3	4	5	6	7

How confident are you that other people see everything whilst they are driving? (please circle a number on the scale below)

Not confident at all						Totally confident
1	2	3	4	5	6	7

Are you concerned that you might miss important visual information? (please circle a number on the scale below)

Not concerned at all						Extremely concerned
1	2	3	4	5	6	7

Please stop here and wait for further instructions.

Questionnaire: Part 2

Now that you have seen the demonstrations please answer the questions below.

Did you find the demonstrations useful?

Yes/ No

In the box below please tell us why you have answered either Yes or No.

Do you think that the general public would benefit from viewing the demonstrations?

Yes/ No

In the box below please tell us why you have answered either Yes or No.

Please select the phrase which you believe best fits in the gap in the sentence below.

Spotting important visual changes, such as a child running out from behind a parked car, is ___________.

Very Difficult	Difficult	Neither difficult nor easy	Easy	Very Easy

How confident are you that you see everything whilst you are driving? (please circle a number on the scale below)

Not confident at all						Totally confident
1	2	3	4	5	6	7

How confident are you that other people see everything whilst they are driving? (please circle a number on the scale below)

Not confident at all						Totally confident
1	2	3	4	5	6	7

Are you concerned that you might miss important visual information? (please circle a number on the scale below)

Not concerned at all						Extremely concerned
1	2	3	4	5	6	7

Now that you have seen the demonstrations please indicate in the table below how much you agree with each statement.

Having seen the demonstrations….	**Strongly Disagree**	**Disagree**	**Neither Agree nor Disagree**	**Agree**	**Strongly Agree**
I am surprised by how difficult it is to see/observe visual changes.	**1**	**2**	**3**	**4**	**5**
Spotting important changes in a visual scene is easier than I expected it to be.	**1**	**2**	**3**	**4**	**5**
I am now more aware of my visual limitations.	**1**	**2**	**3**	**4**	**5**

Appendix B

Table B.1 – Data from Chapter 2, Experiment 1. Mean percentage errors for each participant as a function of the conversation, presentation and display size conditions.

Conversation						No Conversation					
FEB			PRE			FEB			PRE		
DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16
0	0	0	0	0	0	0	0	2.5	0	0	0
0	2.5	2.5	2.6	5.1	2.5	0	7.7	7.7	2.6	5.1	2.5
0	0	5.1	0	2.6	0	0	0	0	2.6	0	5.1
0	2.6	5.1	0	2.5	7.7	2.6	2.5	5.1	0	2.5	7.7
0	0	2.6	0	0	2.6	2.5	0	0	2.6	0	0
0	2.6	5.1	0	0	0	5.1	0	2.6	0	0	2.6
0	0	2.5	0	0	0	0	0	0	2.6	0	0
0	0	0	0	0	0	0	2.6	0	0	0	2.6
0	0	0	2.6	0	0	0	0	0	0	0	0
0	0	0	0	0	2.6	2.5	0	0	2.6	7.7	0
0	0	0	0	2.6	0	0	0	0	2.6	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	2.6	0	5.0	5.3	2.5	0	2.5	0	2.5	0
0	0	0	0	0	2.6	0	0	0	0	0	2.6
0	0	2.5	2.6	0	0	0	0	0	2.5	0	0
0	0	0	2.6	0	0	0	2.6	0	0	2.5	0
0	0	2.6	0	0	2.5	0	0	2.6	0	0	2.5
0	0	0	0	0	5.0	2.5	0	0	0	0	0
0	0	0	0	0	2.5	0	0	0	0	0	0
0	2.6	0	0	0	2.5	0	0	5.0	2.6	2.6	0
2.5	0	0	2.6	0	0	0	0	0	0	0	2.5
2.6	0	2.5	5.3	2.6	0	0	0	0	2.6	5.0	2.6
0	2.6	2.6	0	0	0	0	0	2.6	0	5.1	0
0	0	0	0	2.6	0	2.6	2.6	0	0	0	2.6
0	0	2.5	0	2.6	0	5.1	5.0	0	0	0	2.6
0	5.1	2.6	0	2.6	0	5.0	2.6	2.5	2.5	5.3	5.1
0	0	0	0	0	0	0	0	0	0	0	0
0	2.6	0	0	0	0	0	0	10.3	0	0	0

Table B.2 – Data from Chapter 2, Experiment 2. Mean percentage errors for each participant, data is presented from the no conversation condition as a function of the presentation and display size conditions.

No Conversation							
FEB		PRE (250ms)		PRE (500ms)		PRE (750ms)	
DS 4	DS 12	DS 4	DS 12	DS 4	DS 12	DS 4	DS 12
3.2	3.1	0	0	0	3.1	0	3.1
0	6.3	3.3	0	0	0	3.2	0
0	0	0	0	0	3.1	3.1	0
0	0	0	0	0	3.1	3.2	0
0	0	3.3	0	0	0	0	3.2
6.7	3.1	0	3.2	0	0	0	0
0	0	0	3.1	0	0	0	0
6.7	0	3.2	9.7	3.2	3.3	6.3	3.2
0	0	0	0	0	3.2	0	6.5
0	0	0	3.1	0	3.1	0	0
3.2	0	0	0	0	3.1	0	0
0	3.1	0	0	0	0	0	0
6.3	3.2	0	3.2	0	3.2	0	0
0	0	0	0	0	0	0	0
0	0	0	0	0	3.3	0	0
0	0	0	3.1	0	3.2	0	0
0	3.1	0	0	0	3.1	0	0
0	0	0	0	0	0	0	0
3.2	0	0	0	0	0	0	0
0	0	6.3	0	0	0	0	0

Table B.3 – Data from Chapter 2, Experiment 2. Mean percentage errors for each participant, data is presented from the conversation condition as a function of the presentation and display size conditions.

Conversation							
FEB		PRE (250ms)		PRE (500ms)		PRE (750ms)	
DS 4	DS 12	DS 4	DS 12	DS 4	DS 12	DS 4	DS 12
3.2	0	0	3.2	3.2	0	0	0
6.5	3.1	0	0	0	0	0	0
0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	3.1
0	3.1	0	0	0	0	0	3.1
0	3.2	0	0	3.2	0	0	0
0	0	0	0	0	0	0	0
6.3	0	0	9.4	0	3.2	6.5	6.5
0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0
0	0	0	0	0	0	3.3	0
0	0	0	0	0	0	0	0
0	3.1	0	0	0	0	0	3.2
0	0	0	0	0	0	0	0
0	3.3	3.3	6.5	0	0	0	0
0	3.2	0	0	0	0	0	0
0	0	0	3.2	0	0	0	0
0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0

Table B.4 – Data from Chapter 2, Experiment 3. Mean percentage errors for each participant, data is presented from the no conversation condition as a function of the presentation and display size conditions.

No Conversation							
FEB		PRE (75ms)		PRE (150ms)		PRE (250ms)	
DS 4	DS 12	DS 4	DS 12	DS 4	DS 12	DS 4	DS 12
0	0	0	0	0	0	0	3.1
0	0	0	0	0	0	3.2	0
0	3.1	3.2	0	3.1	6.3	0	0
0	0	0	0	0	0	0	0
0	0	0	3.2	0	0	0	0
0	0	6.3	0	0	0	0	0
3.2	0	3.2	0	3.1	0	0	6.5
0	0	12.5	6.3	12.5	9.4	21.9	12.9
0	0	0	0	0	0	0	3.1
0	0	3.1	0	0	3.1	0	0
0	0	0	0	0	3.1	0	0
0	0	0	0	0	0	3.1	3.1
3.1	0	0	0	0	0	0	0
0	0	0	0	3.2	0	0	6.3
3.1	0	3.2	0	3.1	0	0	0
0	3.2	0	0	0	3.2	3.1	0
0	3.2	0	6.5	3.1	3.1	9.4	0
0	0	0	6.3	3.1	0	0	0
0	0	0	3.1	3.1	0	0	3.2

Table B.5 – Data from Chapter 2, Experiment 3. Mean percentage errors for each participant, data is presented from the conversation condition as a function of the presentation and display size conditions.

Conversation							
FEB		PRE (75ms)		PRE (150ms)		PRE (250ms)	
DS 4	DS 12	DS 4	DS 12	DS 4	DS 12	DS 4	DS 12
0	0	0	3.2	0	3.1	0	0
0	0	0	0	3.1	0	0	0
0	0	0	3.1	0	3.1	3.1	6.3
0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0
0	0	0	0	6.5	0	0	0
0	3.2	0	0	12.5	3.2	6.3	6.3
0	0	0	0	0	0	0	0
0	0	0	0	3.1	0	0	6.5
0	0	3.2	0	0	3.2	3.1	0
0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0
0	0	0	0	3.1	3.1	0	0
0	3.2	0	0	0	0	0	0
0	0	0	0	6.5	6.3	0	3.1
3.2	0	0	3.2	3.1	0	0	0
0	0	0	0	0	0	0	3.1

Appendix C

Table C.1 – Data from Chapter 3, Experiment 1. Mean percentage errors for each participant, data is presented from the no conversation condition as a function of the spatial context and epoch conditions.

No Conversation												
Old										New		
Epoch												
1	2	3	4	5	6	7	8	9	10	8	9	10
0	0	3.1	0	0	3.2	0	0	3.1	0	0	0	0
3.2	6.3	3.1	3.2	6.5	15.6	0	3.1	9.7	3.1	16.1	3.1	9.4
6.3	0	0	3.2	3.2	0	0	0	3.1	0	0	0	3.1
0	3.1	0	3.1	0	0	0	0	0	3.1	3.2	0	0
0	3.3	0	0	0	0	0	0	0	0	3.1	3.1	0
0	0	0	0	0	3.1	0	0	0	0	0	0	0
3.2	0	0	0	0	0	3.1	3.2	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0	0
6.5	6.5	0	3.2	6.3	0	0	0	0	3.1	0	3.1	3.1
0	0	3.1	0	0	3.2	0	0	3.2	0	0	0	0
0	0	0	0	3.1	0	0	0	0	0	0	0	0
3.2	0	0	0	3.1	0	0	3.2	0	0	0	3.2	0
0	0	0	0	0	0	3.1	0	0	0	0	6.3	0
0	3.2	0	0	0	0	3.2	0	0	0	0	0	0
0	3.1	0	0	3.2	0	3.1	9.7	0	0	3.2	3.1	0
0	6.3	0	0	6.5	0	6.5	0	6.3	0	0	0	0

Table C.2 – Data from Chapter 3, Experiment 1. Mean percentage errors for each participant, data is presented from the conversation condition as a function of the spatial context and epoch conditions.

Conversation												
Old										New		
Epoch												
1	2	3	4	5	6	7	8	9	10	8	9	10
3.1	0	0	3.2	3.1	0	0	0	0	0	0	0	3.2
34.4	6.5	0	6.3	0	12.9	3.2	12.9	6.5	9.4	12.9	3.2	12.5
0	0	0	0	0	0	0	0	3.1	0	3.1	6.5	0
0	0	0	0	0	0	0	0	3.2	0	0	0	0
0	0	0	0	0	0	0	3.1	0	0	0	0	0
0	0	0	0	0	3.1	0	0	0	0	0	6.3	0
0	3.2	0	0	0	0	3.1	0	0	0	0	3.1	0
0	0	0	0	3.1	0	0	0	0	0	0	0	0
6.5	0	0	0	0	0	0	0	0	3.2	3.1	9.4	0
0	0	0	3.2	0	0	0	0	0	0	0	3.2	3.1
0	3.1	0	0	3.1	0	0	3.2	0	0	0	0	3.1
0	0	0	0	3.1	0	0	0	3.1	3.1	3.2	0	0
0	0	0	0	0	3.2	0	0	0	0	0	0	0
0	3.1	0	0	0	0	0	0	0	0	0	0	0
3.2	0	3.1	0	6.3	0	0	6.5	6.3	3.3	6.3	3.1	6.3
0	0	0	0	0	3.1	0	0	0	0	0	0	3.2

Table C.3 – Data from Chapter 3, Experiment 2. Mean percentage errors for each participant, data is presented from the no conversation condition as a function of the spatial context and epoch conditions.

No Conversation												
Old										New		
Epoch												
1	2	3	4	5	6	7	8	9	10	8	9	10
3.1	0	0	3.1	0	3.1	6.5	0	0	0	0	0	0
0	3.1	3.2	3.1	6.5	3.2	0	0	0	3.1	6.9	3.3	0
0	3.1	0	0	0	6.3	3.1	3.2	0	0	0	0	3.1
0	0	0	3.2	0	0	0	0	3.1	0	0	0	0
3.1	0	3.1	0	3.1	0	6.7	9.7	0	10.0	6.3	0	12.5
0	0	0	3.2	0	0	0	0	0	0	0	0	0
0	0	0	0	3.1	3.1	0	0	0	0	0	0	0
3.2	3.2	3.2	0	0	3.1	0	0	0	0	0	0	0
3.1	0	6.5	0	0	0	6.5	0	0	3.1	3.2	0	3.2
0	0	0	3.2	3.1	3.2	6.5	0	3.1	3.1	3.1	6.5	0
0	0	0	0	0	0	0	3.1	6.5	3.1	0	0	0
0	0	0	9.4	0	0	3.1	0	3.2	6.5	0	0	3.1
0	0	0	0	3.2	0	0	3.2	0	0	0	0	0
0	0	0	0	0	0	0	0	0	3.1	0	3.1	3.1
0	0	3.1	0	3.1	0	3.2	3.2	0	6.3	3.1	0	0
3.1	0	0	0	0	3.2	0	0	0	0	0	0	0
3.2	3.1	3.1	6.3	6.5	9.7	6.5	0	9.7	9.7	18.8	6.7	9.4
0	0	6.5	0	0	3.2	3.2	3.1	0	0	0	0	0

Table C.4 – Data from Chapter 3, Experiment 2. Mean percentage errors for each participant, data is presented from the conversation condition as a function of the spatial context and epoch conditions.

Conversation												
Old										New		
Epoch												
1	2	3	4	5	6	7	8	9	10	8	9	10
3.2	0	0	3.1	0	0	0	0	0	3.1	0	0	0
3.1	0	0	0	0	3.1	0	0	0	3.1	0	0	0
3.2	3.1	6.3	0	3.3	3.2	3.1	0	0	0	0	3.1	0
0	3.1	0	0	0	3.1	0	0	0	3.1	0	0	0
6.3	3.2	10.0	3.3	0	6.5	3.2	0	0	0	0	0	0
0	0	0	0	0	3.2	3.2	0	0	0	0	0	0
3.1	0	9.4	6.5	0	3.2	6.9	0	0	0	0	0	3.2
0	0	3.2	0	0	0	0	0	0	0	0	0	0
0	0	0	0	3.2	0	3.1	0	0	0	0	3.2	0
3.1	0	0	0	0	0	3.1	0	3.1	3.1	0	0	0
0	0	3.1	0	0	3.1	3.2	0	0	0	0	0	3.1
0	3.1	3.2	3.1	3.1	0	0	0	9.4	6.3	9.7	3.1	6.3
0	0	0	6.3	3.1	3.2	0	0	0	0	0	0	0
0	0	3.1	0	0	3.2	0	0	0	0	0	0	0
3.1	0	0	0	0	0	0	3.2	0	3.2	3.2	0	0
0	0	0	0	0	0	0	0	0	0	0	0	0
6.3	12.9	3.2	6.3	3.1	12.5	3.2	0	0	3.1	0	0	3.2
3.1	0	0	0	0	0	6.5	0	0	6.5	0	3.1	3.2

Table C.5 – Data from Chapter 3, Experiment 3. Mean percentage errors for each participant, data is presented from the no conversation condition as a function of the spatial context and epoch conditions.

No Conversation															
New								Old							
Epoch								Epoch							
1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
0	0	0	0	6.3	0	0	6.7	0	12.5	0	0	0	6.3	0	0
6.7	0	0	6.3	6.3	6.3	6.7	0	6.7	0	0	6.3	6.3	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
12.5	0	0	6.3	0	0	0	0	0	6.3	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	6.3	0	0	0	0	6.7	6.3
0	0	6.3	0	0	0	6.3	12.5	0	0	0	0	12.5	0	0	0
0	0	0	0	6.3	0	0	0	6.3	6.3	0	0	6.3	0	6.3	0
6.3	6.3	0	0	0	0	0	0	0	0	0	0	6.3	6.3	0	0
0	0	0	0	0	0	0	13.3	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
12.5	12.5	6.3	0	0	0	6.3	6.3	6.3	6.3	0	0	6.3	0	0	0
0	6.3	0	6.7	0	0	6.7	0	6.3	0	0	6.7	0	0	6.7	0
0	0	0	0	0	0	6.3	0	6.3	0	0	0	0	0	6.3	0
0	6.3	0	0	0	6.3	0	0	0	0	0	0	6.3	0	0	0
0	0	0	6.3	0	0	6.3	6.3	0	0	0	0	0	6.3	0	6.3
0	0	0	6.3	0	0	0	0	0	0	0	0	0	0	0	0
0	12.5	0	0	6.3	0	6.7	0	0	0	0	0	6.7	0	0	0
0	0	0	0	0	0	0	0	6.3	0	0	0	0	0	0	0
12.5	6.3	0	0	0	0	6.3	0	0	6.3	6.3	6.3	0	6.3	0	6.7
0	0	12.5	0	0	0	6.3	18.8	12.5	0	6.3	0	0	6.3	0	0

Table C.6 – Data from Chapter 3, Experiment 3. Mean percentage errors for each participant, data is presented from the conversation condition as a function of the spatial context and epoch conditions.

Conversation															
New								Old							
Epoch								Epoch							
1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
0	12.5	0	0	0	6.3	0	6.3	0	0	0	0	0	6.3	0	0
25.0	12.5	6.3	0	0	0	0	0	0	12.5	0	0	0	6.3	0	0
0	0	0	0	0	0	0	6.3	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	6.7	0	0	0	0	0
0	0	0	0	0	0	0	6.3	6.3	0	6.3	6.3	6.3	0	6.3	0
0	0	0	6.7	0	0	0	6.7	6.3	0	0	0	0	0	0	6.7
0	0	0	0	0	0	0	0	0	0	0	0	0	6.3	6.3	0
0	0	6.3	0	0	0	0	0	0	0	0	0	6.3	6.3	0	0
0	0	0	6.3	0	0	0	0	0	0	0	0	0	6.3	0	0
0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
12.5	0	0	6.3	0	0	12.5	12.5	6.3	0	0	6.3	6.3	0	0	12.5
0	6.3	6.3	6.3	0	0	0	0	6.3	0	0	0	0	0	0	0
0	0	0	0	0	0	6.3	0	0	0	0	6.3	0	0	0	0
0	0	0	0	0	0	0	0	0	0	6.3	0	0	0	0	0
0	0	0	6.3	0	0	0	0	0	0	0	0	6.3	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0	6.3	0	0	0
0	0	0	0	0	0	0	0	0	0	0	12.5	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	6.7	0	0	0	0
6.3	6.3	0	0	0	6.3	13.3	0	0	0	6.3	0	0	12.5	0	6.7
0	0	0	0	0	0	0	0	0	0	6.3	0	0	0	6.3	0

Appendix D

Table D.1 – Data from Chapter 4, Experiment 1. Mean percentage errors for each participant as a function of the spatial load, presentation and display size conditions.

No Load						Spatial Load					
FEB			PRE			FEB			PRE		
DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16
0	8.0	3.8	4.2	0	0	3.8	0	3.8	0	0	3.8
0	0	0	4.0	0	0	0	0	0	0	0	4.0
0	0	0	0	0	0	0	0	7.7	0	0	0
0	0	7.7	0	4.2	0	0	0	4.0	0	0	0
0	0	3.8	0	8.0	4.0	3.8	0	0	4.5	0	4.5
0	0	3.8	0	0	0	4.0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	4.0	3.8	0	0	7.7	0	0	0	0	0	0
0	3.8	0	0	0	0	8.0	0	8.0	0	4.2	3.8
0	0	0	0	0	0	3.8	0	0	0	0	4.0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	11.5	0	3.8	4.0	0	8.0	0	0	0	4.0
3.8	0	0	0	0	0	0	0	0	4.0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	3.8	0	0	0	0	0	0	4.0	0	0
0	0	0	0	0	7.7	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	3.8	4.0	0	0	0	0	0	0
0	0	3.8	0	3.8	3.8	0	3.8	0	0	0	0

Table D.2 – Data from Chapter 4, Experiment 2. Mean percentage errors for each participant as a function of the spatial load, presentation and display size conditions.

No Load						Spatial Load					
FEB			PRE			FEB			PRE		
DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16
0	0	0	0	4.0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
3.8	0	0	0	3.8	3.8	0	0	0	0	0	0
4.0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	4.0	0	0	0
0	0	3.8	0	0	3.8	0	0	0	4.2	0	0
0	0	0	0	0	0	0	0	0	0	4.2	0
0	0	4.0	0	0	4.0	0	0	0	0	4.5	0
0	0	0	0	4.2	0	0	0	0	0	4.2	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	3.8	0	0	0	0	0	4.2	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	3.8	3.8	0	0	3.8	0	0	14.3	0	0	0
0	0	3.8	0	0	0	0	0	0	8.7	0	0
0	0	3.8	3.8	0	4.0	0	0	3.8	0	4.0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	3.8	0	0	0	0	0	0

Table D.3 – Data from Chapter 4, Experiment 3. Mean percentage errors for each participant as a function of the spatial load, presentation and display size conditions.

No Load						Spatial Load					
FEB			PRE			FEB			PRE		
DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16
4.5	0	4.5	0	0	4.5	4.8	0	0	0	0	9.5
0	0	0	0	4.5	0	0	0	0	0	0	0
0	4.5	13.6	0	4.5	0	0	0	0	0	0	0
0	0	0	0	0	14.3	4.8	0	5.0	0	5.0	0
0	0	0	0	0	0	0	0	4.8	0	0	5.3
0	0	13.6	0	0	4.8	4.8	0	5.0	4.8	0	9.5
0	0	0	4.5	0	0	0	0	4.5	0	0	0
0	9.1	0	4.8	4.5	0	0	5.0	9.5	0	0	0
0	0	0	0	4.8	4.5	0	0	0	0	0	0
4.5	0	4.5	0	4.5	4.5	5.0	0	9.1	0	4.5	4.8
0	0	9.1	0	0	9.5	0	4.5	4.8	0	0	5.0
4.5	0	0	0	0	4.5	0	0	4.5	0	0	0
0	0	0	0	4.5	0	0	0	0	0	4.5	9.5
9.1	0	0	0	0	4.8	0	4.5	4.5	0	0	5.0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	4.5	4.8	11.8	4.8	0	0	0	0
9.1	0	4.5	0	0	4.5	0	4.5	4.8	0	0	4.5
0	4.5	13.6	0	4.8	9.1	0	0	0	0	0	6.3
0	9.5	0	0	0	4.8	0	0	0	0	0	10.0
0	0	4.5	0	0	9.5	0	0	0	5.0	0	0
0	0	4.8	4.5	0	0	4.8	0	4.8	0	0	4.8
0	4.5	0	4.8	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	5.0	9.5	0	0	5.0
0	0	9.1	0	0	0	4.8	5.0	0	0	0	5.3
0	4.5	0	0	0	0	0	0	0	0	0	5.0
0	0	4.5	0	0	0	0	0	4.5	0	4.5	0
0	4.5	4.5	4.5	0	0	0	4.8	0	0	0	4.8
0	0	0	0	0	0	4.8	0	0	0	0	0

Table D.4 – Data from Chapter 4, Experiment 4. Mean percentage errors for each participant as a function of the spatial load, presentation and display size conditions.

No Load						Spatial Load					
FEB			PRE			FEB			PRE		
DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16	DS 4	DS 8	DS 16
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	3.8	0	3.8	0	0	0	0	0	0	0
0	0	0	0	0	0	4.2	0	0	0	0	0
0	3.8	0	0	3.8	3.8	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
8.0	7.7	3.8	0	4.0	0	0	0	0	0	3.8	0
3.8	0	11.5	0	8.0	7.7	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	11.5	0	3.8	3.8	0	0	0	0	0	0
4.0	3.8	13.0	3.8	0	12.5	0	0	8.3	0	3.8	0
0	0	0	0	0	3.8	0	0	4.0	0	0	0
0	0	3.8	0	0	3.8	0	0	0	0	0	0
0	0	7.7	0	3.8	0	0	0	0	3.8	0	0
0	0	0	0	0	0	4.0	0	0	0	0	0
0	3.8	0	0	3.8	7.7	0	0	4.3	0	0	0
0	0	0	0	0	4.0	0	3.8	3.8	0	0	0
8.0	3.8	12.0	0	3.8	3.8	0	0	0	4.2	0	0
0	0	0	0	0	0	0	0	0	4.0	0	0

Appendix E

Table E.1 – Data from Chapter 5, Experiment 1. Mean percentage errors for each participant, data is presented from the no conversation condition as a function of the flanker congruence and cue conditions.

No Conversation											
Congruent				Incongruent				Neutral			
Central Cue	Double Cue	No Cue	Spatial Cue	Central Cue	Double Cue	No Cue	Spatial Cue	Central Cue	Double Cue	No Cue	Spatial Cue
12.5	0	0	8.3	4.2	8.3	0	8.3	0	0	0	0
4.2	4.2	0	0	4.2	0	0	0	0	0	0	0
0	0	0	4.2	4.2	0	0	0	0	0	4.2	0
4.2	0	0	0	12.5	4.2	4.2	4.2	0	0	0	4.2
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	4.2	4.2	0	0	0	0	0	0
0	4.2	0	0	8.3	12.5	12.5	0	0	0	0	4.2
0	0	0	0	8.3	0	4.2	4.2	0	0	0	0
0	0	0	0	0	0	4.2	0	0	0	0	0
0	0	0	0	0	0	8.3	0	0	0	0	0
0	0	4.2	0	4.2	12.5	12.5	0	0	0	8.3	4.2
0	0	0	0	0	0	4.2	0	0	0	0	0
0	0	0	0	4.2	4.2	0	4.2	0	0	0	0
0	4.2	0	0	0	4.2	0	0	0	4.2	4.2	0
0	0	0	0	4.2	29.2	0	4.2	4.2	4.2	0	0
0	0	0	0	0	0	0	0	0	0	4.2	0
0	0	0	0	0	0	8.3	0	0	0	0	0
0	0	0	0	20.8	0	4.2	4.2	0	0	0	0
0	0	4.2	0	0	0	0	0	0	0	0	0

0	0	0	0	4.2	8.3	4.2	4.2	0	0	0	0
0	0	0	0	0	4.2	0	4.2	0	0	0	0
0	0	0	0	4.2	0	0	0	0	0	0	0
4.2	0	8.3	8.3	12.5	12.5	12.5	8.3	0	4.2	8.3	4.2
0	0	4.2	0	0	4.2	0	0	0	0	0	0
0	0	0	0	0	0	4.2	4.2	0	0	0	0
0	0	0	0	8.3	0	12.5	0	4.2	8.3	0	0
0	4.2	0	0	8.3	13.0	4.3	25.0	4.2	0	0	0
0	0	0	0	8.3	0	0	0	0	0	0	0
0	0	0	0	0	4.2	0	0	0	0	0	0
0	0	8.3	0	0	4.2	4.2	4.2	0	4.2	8.3	4.2
0	0	0	0	12.5	0	0	8.3	0	0	0	0
0	0	0	0	4.2	0	0	0	0	0	4.2	0
4.2	4.2	0	0	0	0	8.3	4.2	0	0	0	0
0	0	0	0	8.3	4.2	0	0	0	0	0	0
0	0	4.3	0	12.5	8.3	0	4.2	0	0	0	0
0	0	0	0	0	8.3	0	0	0	0	0	4.2
4.2	0	0	0	8.3	4.2	8.3	13.0	4.2	0	0	0

Table E.2 – Data from Chapter 5, Experiment 1. Mean percentage errors for each participant, data is presented from the no conversation condition as a function of the flanker congruence and cue conditions.

Conversation											
Congruent				Incongruent				Neutral			
Central Cue	Double Cue	No Cue	Spatial Cue	Central Cue	Double Cue	No Cue	Spatial Cue	Central Cue	Double Cue	No Cue	Spatial Cue
0	4.2	0	4.2	0	0	0	0	4.2	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	4.2	0	0	0	0	4.2	4.2	0	0
0	0	4.2	0	8.3	0	4.2	4.2	0	0	0	0
0	0	0	0	0	0	4.2	0	0	0	0	0
0	0	0	0	0	4.2	0	0	0	0	0	0
0	0	0	0	0	4.3	4.2	0	0	0	0	0
0	0	0	0	4.2	8.3	0	0	0	0	0	0
0	0	0	0	0	0	4.2	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	4.3	8.7	0	0	0	0	0	0
0	0	4.2	4.2	0	4.2	4.3	4.2	4.2	0	4.2	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	4.2	0	0	0	0
0	0	0	0	0	4.2	4.2	4.2	0	0	0	0
0	0	0	0	4.2	0	0	4.2	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	4.2	8.3	0	0	0	0	0	0	0
0	0	0	0	4.3	4.2	4.2	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	4.5	4.3	0	0	0	4.8	0	0
0	0	0	0	0	0	0	0	0	4.2	0	0
0	0	0	0	4.3	4.2	0	0	0	0	4.2	0

0	4.2	4.2	4.2	12.5	8.3	8.3	16.7	0	4.2	0	4.2
0	0	0	0	8.3	0	4.2	4.2	0	0	0	0
0	0	0	0	8.3	0	0	4.3	0	0	0	0
0	0	0	0	8.3	16.7	4.2	12.5	0	0	0	0
0	0	0	0	4.2	4.2	0	0	0	0	0	0
0	0	4.2	0	0	4.2	0	0	0	0	0	0
0	0	0	0	0	4.2	0	0	0	8.3	0	0
8.3	0	0	0	4.2	8.3	0	4.2	4.2	0	4.2	0
0	0	0	0	0	0	4.2	0	0	0	0	0
0	0	0	0	4.2	4.3	0	0	0	0	0	0
0	0	0	0	8.3	0	0	0	0	4.2	0	0
0	0	0	0	4.2	4.2	0	0	0	4.2	0	0
0	0	0	0	4.2	0	0	0	0	0	0	0
0	0	0	0	4.2	4.3	0	8.3	0	0	0	0
4.2	0	0	0	0	4.3	8.3	0	4.2	0	0	0