The Effects of Simulated Cellular Phone Conversation on Road-Crossing Safety

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ABSTRACT

The effects that cellular (cell) phone conversation may have on pedestrian road-crossing performance is unknown. A series of experiments was conducted using a virtual reality road crossing simulator to examine this issue. The participants were primarily university students aged between 18 and 24 years old, although one study compared a group aged 18 to 24 to a group between 50 and 67 years old. Two experimental situations were used: a gap-choice situation, in which the participants had to choose a gap to cross through; and an infrequency situation, where vehicles were present on only 10% of the trials. Participants were impaired by a simulated phone conversation task when compared to no-conversation task, as evidenced by longer reaction times, slower walking speeds, poorer gap choices, and more cautious behaviours. Most importantly, conversation was related to a decrease in the mean margins of safety, and the participants were hit or nearly hit by vehicles more often when talking. The general performance of the older participants did not differ from that of the younger participants, and both groups were impaired to a similar extent by the conversation task. Participants were found to use irrelevant distance information to inform their gap-choice decisions, a strategy associated with a decrease in safety as the distance between the vehicles increased. It was also found that their use of time-to-arrival information was impaired when engaged in the conversation task. Overall, talking on a cell phone while crossing a road may represent an unnecessary increase in risk; therefore, care should be taken if these two acts are being conducted concurrently.

CHAPTER 1: GENERAL INTRODUCTION

In 2005, 943 pedestrians were injured on New Zealand roads, with 32 pedestrians being killed (M.o.T, 2006). Most the accidents occurred on urban roads, with only 5% of the injury causing accidents and 22% of the fatal accidents occurring on rural roads. Although pedestrian fatalities and injuries make up only a small proportion of the road toll in New Zealand there is a need to understand how cellular (cell) phone use may affect pedestrian behaviours. Pedestrians can be distracted, and this can be a contributing factor in accident causation (Gordon, 2005), but very little information is available on the effects of a secondary, potentially distracting task on pedestrian behaviour. For instance, while a search of the Crash Analysis System (CAS; LTSA, 2005a) found only six recorded crashes involving pedestrians with cell phones for the 10-year period between 1995 and 2005 (Craig Gordon, 2007, personal communication) it is possible that other crashes were not reported.

Road crossing is a well practised behaviour, and so it is conceivable that a secondary task will not affect it (Wickens & Hollands, 2000). Rumar (1990) notes that driving, compared to walking, occurs at speeds faster than we have had the time to adapt to. Also, as the speeds and masses involved are lower when walking, the consequences of a detection failure are not so great. This may be true in the general environments we walk in, such as around home or the office, but the potential negative consequences are greater when humans and vehicles coexist in the same environment. For instance, a pedestrian who does not notice a tree in their path may end up bruised. On the other hand, a pedestrian who steps in front of an oncoming car may end up dead. A pedestrian hit by a vehicle travelling at 40 km/h has approximately a 30% chance of being killed, whereas if they are hit by a vehicle travelling at 60 km/h this increases to around 90% (A.C.C., 2000). The chance of being killed also increases with age, a 50 year-old having twice the risk of being killed as a 20 year-old male for an equivalent collision (A.C.C., 2000). With the potential consequences of an attention lapse being severe, it is important to know how phone conversation affects pedestrians.

The current experiments examine two aspects of pedestrian behaviour that may be affected by cell phone conversation; gap-choice behaviours and the direction of attention. The first study examines how simulated cell phone conversation affects gap-choice behaviours. The second study builds upon the first but also includes an investigation of potential age differences in how phone conversation impairs performance. The third study focuses on the direction of attention, using a simulated quiet road with vehicles present very infrequently. A forth experiment investigates an alternative explanation for a finding from Experiments 1 and 3.

Until recently the main focus of study in the distraction literature to date has been driving behaviours, with good cause. Out of 16 listed factors that contributed to crashes, *inattention or diversion of attention* was third for open road crashes and fifth for urban crashes in terms of social cost[1] (LTSA, 2005b). Only *driving too fast for the conditions* and *alcohol involvement* were consistently higher for both environments, but in urban situations *failure to give way* and *not seeing other party* were also higher (LTSA, 2005b). However, both of these can, in part, be considered in terms of attention or distraction: Why did the driver fail to give way, and why did they not see the other party? For instance, Rumar (1990) argues that late detection is the basic driving error, and part of this may come down to "internal" distractions such as worrying about a personal problem.

A large number of people own cell phones, approximately 1.5 billion worldwide at the end of 2004 (CellularOnline, 2006). In New Zealand, Vodafone claims a customer base of over 2

[1] "Social cost calculations include loss of life or life quality, loss of output due to injuries, medical and rehabilitation costs, legal and court costs and property damage" (LTSA, 2005b p. 1)

million customers and 53% of the market (Vodafone, 2006), while Telecom claims over 1.6 million connections (Telecom, 2006). Given that the current population of New Zealand is estimated at approximately 4.1 million (Statistics New Zealand, 2006), this means around 88% of New Zealanders own a cell phone.

Ownership is also on the rise. In the United Kingdom, ownership increased across all age groups between 2001 and 2003 (National Statistics, 2004). Older adults were less likely to own a cell phone overall, but had the greatest increase in ownership. For instance, while the increase for people between 15 and 54 was 5.5 percentage points on average, for adults older than 54 the increase was 11.33 percentage points (National Statistics, 2004). The greatest increase was for the 75+ age group; their usage almost doubling from 13% to 24% (National Statistics, 2004). (Note, however, that the lower base rate of ownership for older adults in respect to younger adults means any increase in ownership will result in a higher percentage increase for older adults than younger adults.) Newer distractions are also being introduced into vehicles, such as navigation systems and cellular (cell) phones (Sheridan, 2004), making it more important to study the effect these new distractions will have on behaviours.

A substantial number of drivers report using a cell phone while driving, such as 68% (Lamble, Rajalin, & Summala, 2002) to 81% (Pöysti, Rajalin, & Summala, 2005) for Finland and 47% of cell phone owners in England (White, Eiser, & Harris, 2004). Usage while driving is higher in specific subgroups; for instance, 86% of US college students (Seo & Torabi, 2004) and 99% of heavy vehicle drivers in Denmark (Troglauer, Hels, & Christens, 2006) reported using a cell phone while driving.

Estimates for the number of drivers talking at any given time have also been made, although only for the use of hand-held phones. In Western Australia 1.5% of drivers were observed talking on a phone (Horberry, Bubnicha, Hartley, & Lamblea, 2001), while the figure was 2.2% in New York just prior to the ban on driving while using hand-held phones (McCartt, Braver & Geary, 2003). In New York this value dropped to 1.1% following the ban, but whether this decrease was maintained was not certain (McCartt et al., 2003).

A fairly large proportion of those who use phones while driving have experienced dangerous situations. For instance, 21% of students who reported having been in an accident, or near accident, situation also reported that a phone was being used by at least one of the drivers (Seo & Torabi, 2004). In one Finnish study 50% of phone users reported having experienced a dangerous situation related to using their phone while driving (Lamble et al., 2002), while in another study phone related hazardous situations had been experienced by 44% of respondents in the six months prior to being polled (Pöysti et al., 2005). For heavy vehicle drivers in Denmark, 0.5% reported that their phone use had caused an accident and 6% reported it had caused a dangerous situation, while 66% reported they had experienced a dangerous situation due to the phone use by other road users (Troglauer et al., 2006).

Younger drivers seem more likely to talk on a phone while driving than older ones, males more than females, and people with higher annual kilometres driven more than those with less (Pöysti et al., 2005). Drivers who talk on a phone while driving may also be more likely tolerate other risks, since only 75.8% of the drivers observed talking on a phone were wearing a seatbelt compared to 82.8% of the non-phone users (Eby & Vivoda, 2003). White et al. (2004) examined risk perceptions in three groups of people: those who would use a cell phone while driving, those who would not do so, and those who could not do so (i.e. did not own a cellular phone, did not drive, or did not own a car). Group differences for risk perception were found for 5 out of 16 activities. Four of these related to phone use; the group who were willing to use a phone while driving rated the act of making (or receiving) a phone call on a hand-held (or hands-free) phone as being less risky than did the other two groups. The only other activity where this risk-perception trend was evident was for the use of seatbelts. Hands-free phones were also perceived as being less risky to use by all groups than hand-held phones (White et al., 2004), a finding replicated in a New Zealand sample (Gordon, 2005). Phone use has been seen as being less risky and less prevalent than other potential accident causes, such as drinking and driving or driving too fast (Vanlaar & Yannis, 2006). The only potential accident cause which was seen as less risky than phone use was traffic congestion.

In Chapter 2 the Ecological Approach (EA), the overall theoretical context for this research, will be discussed. Both the benefits and potential issues of virtual reality (VR) simulation will be examined in Chapter 3, while pedestrian road-crossing research will be detailed in Chapter 4. Chapter 5 will cover some of the relevant theoretical concepts related to dual-task performance, such as attention and situation awareness, as well as a review of research into dual-task performance. (The majority of the research reviewed focuses on how secondary tasks affect driving, but there is a small body of work examining how secondary tasks affect walking.) Chapters 6 through 9 will detail the experimental work, with a summary of the findings provided at the end of each chapter. Finally, Chapter 10 will contain an overall discussion spanning all of the experiments.

CHAPTER 2: THE ECOLOGICAL APPROACH

The following research was conducted from an ecological approach to psychology. The term EA used without a qualifier is ambiguous, the general term applying to the seminal work of four different psychologists (James J. Gibson, Egon Brunswik, Roger Barker, and Urie Bronfenbrenner) Each had a different perspective (Heft, 2001), but all take the ecosystem – the person, task, and environmental support – as the unit of analysis for understanding cognition and behaviour. The author follows the Gibsonian approach, which will be outlined in this section to provide a theoretical context for the research.

Introduction to the Ecological Approach

In his 1979 book J. J. Gibson states "It is not true that 'the laboratory can never be like life. The laboratory *must* be like life!" (p. 3, italics in original). This is a guiding principle of the approach, to ensure that the constraints of an experiment match as closely as possible the constraints of the world. The overall goal of the approach is "…nothing less than a complete understanding of the complex and everchanging relationship of person-as-knower to the environment-as-known" (Shaw & Bransford, 1977, p. 6).

One of the central tenets of the EA is that the individual is an active perceiver (Michaels & Carello, 1981) – "…we must perceive in order to move, but we must also move in order to perceive" (J. J. Gibson, 1979/1986, p. 223). For example, studying perceptual errors using a distorted room by only allowing the participants one stationary vantage point and the use of only one eye, unusual constraints in the everyday situations, may well produce interesting findings. However, if the constraints are modified to be more realistic, perhaps by enlarging the eye-hole so two eyes can be used, the distorted room can be seen for what it is (J. J. Gibson, 1966/1983; Katz, 1987). Related to this is the view that perception is not a passive process. For instance, a movie set may be believed to be real by an observer, but if they are able to move closer or walk around it they will detect that it is fake (Shaw & Bransford, 1977).

Direct Perception and Information

Indirect theories of perception are based on the assumption that the information provided to the senses is impoverished. Information about the world, therefore, must be processed by the brain to make sense of it (Michaels & Carello, 1981). In other words, although there is a real world "…we have no direct, *im*mediate access to the world, nor to any of its properties" (Neisser, 1967, p. 3, italics in original). Following Koffka (1935), Neisser (1967) drew a distinction between *distal* and *proximal* stimuli. A distal stimulus is one in the environment, such as a tree in a field. However, the sensory input, assumed to be the basis of our perceptions, is taken to be the proximal stimulus. In the case of vision the proximal stimulus is the image on the retina, so we would not perceive the tree directly but indirectly via the image of the tree on the retina.

In contrast, J. J. Gibson argued that perception is direct, with all the information needed for perception being available in the environment without a need for internal processing (J. J. Gibson, 1979/1986). According to this approach, information about the environment is not considered to be impoverished, but rather rich and accurate (Michaels & Carello, 1981). We perceive the world directly, not the retinal image of the world, a point J. J. Gibson made explicit in an article entitled *The Information Contained in Light* (J. J. Gibson, 1960). Reinforcing this point is the fact that many animals have compound eyes, which do not produce retinal images, but such animals are still able to operate in the environment (J. J. Gibson, 1960), and may use the same information humans do, as in the case of optical looming (J. J. Gibson, 1979/1986). This suggests that different biological systems can be sensitive to the same information. In other words animals perceive structured energy in stimulation, rather than images on the retina, similar to how bats perceive using echolocation (Michaels & Carello, 1981).

Information, as described within the EA, differs from the information processing approach's definition of information (Mace, 1977). Within the information processing approach information is defined as the reduction of uncertainty (e.g. Proctor & Dutta, 1995). Within the EA information *specifies* the environment (J. J. Gibson, 1966/1983; Michaels & Carello, 1981). The information about the environment is contained in ambient arrays, such as the optic array (J. J. Gibson, 1960) which "is the light that surrounds an observer" (J. J. Gibson, 1966/1983, p. 12). For instance, the absence of texture specifies open air, whereas texture specifies a solid or liquid surface (J. J. Gibson, 1960).

The information we need to attend to specifies the invariant properties of the environment (J. J. Gibson, 1950, 1966/1983, 1979/1986). To rephrase an example given by J. J. Gibson (1950), the retinal image we have of our back garden is continually changing as we move. To receive the same image twice we would need to sit in the same place both times, and have our head in exactly the same position. As this is unlikely, it could be said that we will never have the same image of our back garden twice. However, we always perceive that it is the same environment. This means there must be invariant properties that inform us this is our back garden rather than our neighbour's. For another example, the absence of texture, which specifies open air, may also be viewed as an invariant (J. J. Gibson, 1979/1986)[2].

Following Gibson, Michaels and Carello (1981) state that the "useful dimensions of an animal's sensitivity are to the structured energy that invariantly specifies properties of the environment

[2] In the case of the visual cliff or a glass door the absence of texture does not specify open air. Visual perception may not be veridical if optical information is missing, in this case information specifying the presence of a sheet of glass.

of significance to that animal" (p. 156, italics added). (An example of an optical invariant that specifies time-to-arrival, *tau*, will be discussed shortly.) A question that could be asked then is: What is significant? This has to be taken in relation to the animal and its current task, as what is significant to a hungry fish may not be significant to a hungry bird. What is acquired by an individual is information about invariants specifying what behaviours are possible, what the environment *affords* the individual (J. J. Gibson, 1979/1986) This concept will be expanded in the following section.

Affordance Theory

Individuals have different capabilities. Cohen, Dearnaley, and Hansel (1955) note that

> …in a given situation, such as jumping a beam, if one raises the height of the beam, the task becomes more difficult to some extent for all subjects. But for each person there will be a particular height of the beam he will succeed in jumping, say, in 70 percent of his attempts. (p. 120)

A person who is taller is likely to be able to jump over a higher beam than a shorter person can. It makes sense, then, to consider a task such as the jumping of a beam in terms of an individual's capabilities rather than in absolute terms. Although there will be absolute differences in the beam heights a shorter and a taller person can jump, these differences may disappear when the individuals' heights or leg lengths are taken into account. These concepts will be expanded upon in the following section, and the relevance of affordance theory to road-crossing research will be outlined on page 13.

The concept of an affordance has been around for over 50 years (E. J. Gibson, 2000). J. J. Gibson discussed his reasons for developing the concept and gave an elementary definition of the term in his 1966/1983 book:

> I have coined this word as a substitute for *values*, a term which carries an old burden of philosophical meaning. I mean simply what things furnish, for good or ill. What they *afford* the observer, after all, depends on their properties (p. 285, italics in original).

An entire chapter was devoted to affordances in his last book, but the basic concept was the same. Simply: “The affordances of the environment are what it *offers* the animal, what it *provides* or *furnishes*, either for good or ill” (J. J. Gibson, 1979/1986, p. 127, italics in original). Affordances are always measured in relation to an individual (J. J. Gibson, 1979/1986). For instance, knowing a seat will hold 70 kg of weight does not tell us about the sitting on or standing on affordance of the object. Only when we combine this knowledge with the fact the person who wishes to sit weighs 50 kg can we say that the seat affords sitting on *for that person*. The seat does not afford sitting on for a person who weighs 90 kg, at least not for very long.

An additional concept has been introduced into affordance theory by Shaw, Turvey, and Mace (1982), an *effectivity*. They describe a specific effectivity as being complimentary to a specific affordance, specifically that “an effectivity of an animal (or human) *is a specific combination of the functions of its tissues and organs taken with reference to an environment”* (p. 197, italics in original). Where an affordance can be seen as what an environment will allow a specific individual to achieve, an effectivity can be seen as what behavioural properties of a specific individual support achievement in that environment.

The concept of an affordance has been described as “one of the most important and revolutionary in ecological science; it is also intuitively clear and appealing” (Michaels, 2003, p. 135), but there are still issues regarding the exact characterisation of an affordance. Some examples of a number of these disagreements are provided by Chemero (2003) and Michaels (2003), and can also be seen in the discourse between various writers in the journal *Ecological Psychology* (e.g. Chemero, 2001; Kotchoubey, 2001; Michaels, 2000; Smeets & Brenner, 2001; or Chemero, Klein, & Cordeiro, 2003; Stoffregen, 2000a, 2000b, 2003). It is generally agreed that affordances are properties of the environment relative to properties of an individual (Chemero, 2003), and for the purposes of this work this definition will be sufficient.

Affordance Research

Affordance theory has been examined within and applied to a variety of experiments, most often investigating geometric properties and the perception thereof. An early study examined stair climbing in three samples of shorter and taller college students (Warren, 1984). Using a set of adjustable stairs and an adjustable stair-climber, both of which allowed the stair riser height to be adjusted, Warren conducted three experiments investigating: 1) the critical riser height at which participants felt they could no longer climb the stairs without using their hands; 2) the optimal riser height for energy use, and; 3) the participant’s perceptions of the optimal riser height. He determined that although there were absolute differences between the groups, when the ratio of riser height to leg length was substituted for riser height these differences disappeared. For example, although the critical riser height differed for the two groups (67.13 cm for the shorter group and 81.33 cm for the taller group) the ratios were almost identical, (88% of leg length for the shorter group and 89% for the taller group). This value had been predicted a priori by examining maximum human leg flexion. In a similar fashion, optimal energy efficiency occurred for both groups at a ratio of 26% of leg length. This value was similar to that found for the perception of the optimal riser height, a ratio of 25% for both groups.

Warren's (1984) experiments illustrate the point that affordances are perceivable, and not simply in a dichotomous *does afford / does not afford* way. Participants were able to perceive the maximum stair height they could climb as well as the optimal stair height to climb, two different points within the affordance of *climbability.* These experiments also illustrated the utility of a *pi* number, described by Warren (1984)) as "a dimensionless (i.e. unitless) number that uniquely expresses a particular individual-environment fit" (1984, p. 686).

Pi numbers are created when an environmental property E, such as riser height, and an individual property A, such as leg length, are both measured in the same units (e.g., cm) and a ratio is taken of the two: (Warren, 1984).

$$\pi = E/A \qquad \text{(Eq.1)}$$

Pi numbers have also been investigated in relation to other tasks, such as walking through an aperture (Warren & Whang, 1987). Warren and Whang (1987) found that participants, whether broad- or narrow-shouldered or walking normally or quickly, turned sideways to pass through an aperture that was approximately 1.3 times wider than their shoulders. However, when asked to make judgements on passability, the ratio was only 1.16. This was possibly due to instructional differences. Participants in the first task were asked to move freely, while in the second experiment they were asked to judge if they could, rather than would, walk through the aperture without turning their shoulders (Warren & Whang, 1987). Given that a gap only needs to be slightly wider than a person's shoulders to allow them to pass without turning, it seems the participants in the first study were allowing themselves a 30% margin of safety (Warren & Whang, 1987). This again suggests that not only are affordances perceivable, but that people can perceive what is possible and what is optimal.

As well as continued research on stair climbing (e.g. Cesari, Formenti, & Olivato, 2003; Mark, 1987), further research has investigated the numbers of fingers or hands required to pick up different sized objects (Cesari & Newell, 2000), stepping or jumping over obstacles (Cornus, Montagne, & Laurent, 1999), sitting on (Hirose & Nishio, 2001; Mark, 1987), and stepping over objects (Hirose & Nishio, 2001).

The study of affordances has not been limited to the physical properties of objects such as stairs. For example, research has been conducted into the affordances of smiles (Miles, 2005), a study of social affordances. More germane to the current research, temporal affordances have been studied in the context of road-crossing. Oudejans, Michaels, van Dort, and Frissen (1996) investigated how locomotion affected pedestrians' critical gap choices. Using an observation method they compared the critical time gap, the size of the gap (in seconds) where people swap from crossing to not crossing, for pedestrians who initiated crossing from a stationary position or while walking. They found that while both groups of pedestrians took similar times to cross the road, 2.78 s and 2.6 s for stationary and walking respectively, the critical gap for the stationary group was over 1.5 times longer than the walking group, 4.63 s and 3.02 s respectively. The authors suggest that pedestrians may make more accurate judgements about when it is safe to cross when in they are in motion (Oudejans et al., 1996).

Using a virtual reality (VR) road-crossing environment, Simpson and Owen (2002) examined the utility of a pi number for a temporal affordance. The number they used was termed the *safety ratio*, and was defined as the ratio of the available crossing time to the time taken to cross (see Eq. 2).

$$\text{safety ratio} = \text{time-to-arrival} / \text{time-to-cross} \qquad \text{(Eq. 2)}$$

A safety ratio greater than 1 implies a safe crossing, while 1 or less than 1 suggests an unsafe crossing. The safety ratio can be easily converted into a *margin of safety* using the following formula:

$$\text{margin of safety (\%)} = (\text{safety ratio} - 1) * 100 \qquad \text{(Eq. 3)}$$

For example, if a person crossed in a gap that was 3 s long, and they took 2.5 s to do so, they would have a safety ratio of 1.2 and a margin of safety of 20%. They found that the margin of safety was a sensitive performance measure; mean margins of safety were lower when vehicles began further away, regardless of the time the vehicles would take to reach their position. This indicated that participants used distance information in part to make their crossing decisions, a finding consistent with other pedestrian studies (e.g. Connelly, Conaglen, Parsonson, & Isler, 1998; Connelly, Isler, & Parsonson, 1996). Later road-crossing experiments have shown the margin of safety to be sensitive to mobility impairment (Murray, 2003) and to differences in safety between children with and without attention deficit / hyperactivity disorder (ADHD; Clancy, Rucklidge, & Owen, 2006). The observational data from Oudejans et al. (1996) can also be converted into margins of safety, and this point will be returned to in the more detailed review of road-crossing research in Chapter 4.

Affordances are not static. What the environment affords an individual changes as the individual's effectivities change, such as a pedestrian carrying a heavy load (Young & Lee, 1987) or age-related changes in walking speed (Ketcham & Stelmach, 2001). For example, in Murray (2003) the participants' walking speeds were reduced using a leg brace. As they could no longer cross the road as quickly, gaps that would have previously afforded safe crossing became unsafe. Effectivities can also be enhanced as well as reduced, such as in Hirose and Nishio (2001). By having their participants wear *geta*, Japanese shoes that increased the wearer's height by 10 cm, the participants were able to step over or sit on greater heights. Participants in both of these studies took some time before they adapted to their new capabilities, suggesting that adaptation in this case was a result of learning.

Along with physical changes in effectivities there may also be cognitive ones. The argument can be made that cellular phone conversation may change a person's capabilities, and if they can not identify that their capabilities have changed they may not modify their behaviour to take the change into account. The impact of phone conversation on a person's abilities will be addressed further in Chapter 5.

The Optical Specification of Time-to-Arrival

Along with invariants specifying physical properties of the environment there are also invariants that specify temporal properties. Time-to-arrival[3] (T_A) is important information to detect for both pedestrians and drivers, and is specified by the optical invariant *tau (*see below). However, whether people detect and use tau information is disputed. This section contains an outline of tau and some of the evidence for and against it. It also includes a description of research which suggests alternate information that may be used by people for T_A judgements.

[3] The term time-to-arrival is sometimes used synonymously with time-to-contact or time-to-collision. Time-to-arrival was chosen to indicate that in road-crossing the important information is when a vehicle arrives at the pedestrian's position. Schiff and Oldak (1990) view time-to-contact events as a subset of T_A events. For them T_A describes "impending contact between self and object, two or more objects, or no contact at all, as in miss-path trajectories" (p. 304)

To mathematically calculate the time-to-arrival (T_A) of a vehicle we can use two pieces of information, the distance of the vehicle from the observer and its velocity[4]. However, although there is evidence that we can estimate distance and velocity, individually or together, this may not be how humans estimate T_A (McLeod & Ross, 1983). Lee (1976) proposed a theory for detecting $T_{A,}$ specifically for the purposes of braking, from a higher order optical invariant he named tau. He defined tau at time t using the following formula (Eq. 4; originally from Lee, 1976, p 441):

$$\text{Tau}(t) = \frac{(\text{angular separation of any two image points of the obstacle})}{(\text{rate of separation of the image points})} \qquad \text{(Eq. 4)}$$

It is, then, the rate of change in the optical angle subtended by two points on an object that specifies T_A, e.g. the angle subtended between the brake lights on a slowing vehicle. The direction of change is also important. If the angle is decreasing over time the object is receding, whereas if it is increasing the object is approaching. Tau applies specifically when there is a constant closing velocity (Lee & Reddish, 1981). Time-to-arrival, as described, can be perceived directly from tau, and therefore there is no need for the information to be processed.

As tau only specifies T_A for a given moment in time, which is of little use if we want to know what the T_A will be in the future, or was in the past. Lee (1976) specified the time derivative of tau, tau-dot, which is the rate of increase of tau. Holding this value steady at approximately -0.5 (the negative indicating deceleration) when approaching a stationary object will mean that motion will stop at the object (Lee, 1976). If it is less than -0.5 then a collision will occur, whereas if it is greater than -0.5 no collision will occur (Andersen & Sauer, 2004).

Since its conception tau has undergone much scrutiny (Cavallo & Laurent, 1988) and has inspired many researchers (Hecht & Savelsburgh, 2004). Lee and Reddish (1981) demonstrated

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[4] Velocity, rather than speed, is used here since velocity has an associated direction. Knowing the speed of a vehicle does not help if we do not know which way it is traveling.

that tau may be used by gannets when they are diving for food. Their data suggested that the gannets did not fold in their wings at a specific height, after a certain amount of diving time, nor at a specific T_A value. It appeared that the gannets were preparing to enter the water at a certain tau margin (Lee & Reddish, 1981), although there is disagreement with their conclusion (Wann, 1996).

There has been additional support for the theory. The tau-dot hypothesis was tested in relation to driver braking by Yilmaz and Warren (1995), along with optical expansion and the computation of the necessary deceleration to avoid a collision from spatial variables. They found that the mean value of tau-dot was -0.51, very close to Lee's (1976) value for ideal braking. McLeod and Ross (1983) found that viewing time did not affect the accuracy of T_A judgements, suggesting that tau was being used rather than a cognitive method requiring computation from distance and velocity. (They presumed that if the cognitive method took more time to use performance should improve as viewing time increased.)

However, others disagree with elements of the theory (e.g. Tresilian, 1991), or disagree with its utility (e.g. Wann, 1996). Smeets, Brenner, Trebuchet, and Mestre (1996) argued against tau and suggested that T_A judgements are based on a ratio of the perceived distance and velocity. They also noted that since T_A is based on the ratio of the actual distance and velocity any variables that describe this ratio will correlate with performance, so results that appear to support tau may be due to this relationship. This assumes, of course, that their hypothesis is true: If another variable, such as tau, is used for T_A judgements it is likely that the ratio of distance and velocity would correlate with it, suggesting their findings may be due to the relationship.

Smith, Flach, Dittman, and Stanard (2001) examined the optical constraints on collision control. Their task was designed to meet Tresilian's (1997) conditions for the successful use of the tau strategy:

> Condition A. The experimental task actually involves performance of a fast timing interceptive task with requirements for very precise timing.
> Condition B. The action is a natural interceptive action (such as a catch or a hit) or closely approximates a natural interceptive action, and the participant is a skilled performer.
> Condition C. The task constraints are such that the use of [T_A] information is implied and the tau margin is an adequate source of such information. (Tresilian, 1997, p. 1274).

In a simulated environment, participants were required to judge when to release a pendulum so that it hit a ball back along the ball's path of travel. They manipulated the approach speed and the size of the ball, and also examined differences in performance for different approach speed ranges (slower speeds, between 4- and 16-m/s, and faster speeds, between 12- and 24- m/s). Participants for the last study (different approach speed ranges) were only tested in one condition (slower or faster), and for a final study Smith et al. examined the transfer of learning by retesting the same participants in the alternate condition.

They found that the participants responded earlier to slower and larger balls, findings that would not be expected if a tau strategy was being used. These errors were reduced with practise, potentially indicating a greater use of the tau strategy, but even at the end of testing these tendencies remained. For the two differing approach speed studies they found that participants in the slower condition performed the task with greater accuracy than those in the faster condition, and these performance differences were maintained when the groups switched to the other speed condition.

Drawing on these findings they proposed that people use the optical angle (the numerator for the tau equation) and optical expansion (the denominator for the tau equation) independently to judge T_A, rather than using tau by itself. They examined the information that was being used through a state space analysis, comparing the actual performance of the participants with what would be expected if the participants were using each form of information (optical angle, optical expansion, or tau; see Flach, Smith, Stanard, & Dittman, 2004 and Smith et al, 2001 for more details on their analyses).

Flach and colleagues believe that this theory helps explain results troubling for tau theory, such as velocity (Andersen, Cisneros, Atchley, & Saidpour, 1999) and size (DeLucia & Warren, 1994) affecting T_A judgements, and can model both individual differences and skill gain (Flach et al, 2004). The theory is similar to distance / velocity theories, with optical variables being used to estimate distance (from the optical angle) and velocity (from optical expansion). However, the actual values for distance and velocity do not need to be known.

Summary

The EA proposes that information about an environmental event is directly acquired from the stimulation by an active perceiver without a need for internal processing. The information specifies invariant properties of the event that the individual may find significant. Hence, the information is about affordances, and specifies the actions an individual may perform in its environment. The concept of invariants is not limited to physical properties of the environment as there are invariants that may specify temporal events, tau being an example of a temporal invariant that specifies T_A. However, tau may not actually be used by people when they estimate T_A, with Flach and colleagues suggesting alternative optical information that may be used.

In terms of pedestrian research the EA provides an appropriate framework. The focus on ensuring that the experimental task closely matches the real-world task improves the generalisability of the experimental task to the real world. Within affordance theory the road-crossing task can also be conceptualised as the detection of the *crossability* affordance of a gap, and cell phone conversation can be characterised as impairing the ability of a pedestrian to detect this affordance. The next chapter outlines VR experimentation, a way to more closely match the constraints of the experimental task to the real-world constraints of the task.

CHAPTER 3: VIRTUAL REALITY AND VIRTUAL ENVIRONMENTS

The current experiments utilise VR to provide a high level of ecological validity for the task. This chapter provides a basic background into VR, including both the benefits and drawbacks of the method. It begins with a general overview of the area, and then provides a more in-depth examination of *presence*, an important but ill-defined topic for VR research. Finally, one of the potential side effects of VR use, simulation sickness (SS) will be discussed.

General Overview

The terms VR and *Virtual Environment* (VE) are both used to refer to the same concept, a simulated environment that is stored on and produced by a computer (Loomis, Blascovich, & Beall, 1999). For current purposes the term VR will be used to refer to the experimental method, while VE will be used when referring to the environment that is navigated. A VE can be presented in various ways. With one method, sometimes known as desktop VR, the environment is presented on a computer monitor (Loomis et al., 1999). An example of this is the first experiment presented in Simpson (2002), where participants, seated in front of a monitor, used a joystick to control the braking of a vehicle which was following a bus. Immersive systems can involve surrounding a participant with multiple screens and speakers or, more commonly, using a head-mounted display (HMD) combined with a motion tracking system (Loomis et al., 1999).

Virtual reality simulations provide a number of benefits. Using them allows a participant to actively participate in the environment rather than to be a passive observer (Rose & Foreman, 1999). They also allow potentially risky activities to be performed safely, such as training

(Wickens & Hollands, 2000) and experiments in driver distraction (Haigney & Westerman, 2001). From an ecological perspective this is ideal as it enables the participants to be active information gatherers, as "the actions of the perceiver have a significant impact on what is perceived" (Zebrowitz, 2002, p. 143), and to make the laboratory more like life, which J. J. Gibson (1979/1986) deemed necessary.

There have been numerous actual and proposed uses for VR. Some of the more common uses have been flight and driving simulators (Brooks, 1999), but it has also been used for research and training in other domains where the potential consequences of accidents are high. These include helicopter piloting (Owen, 1996), astronaut training (Brooks, 1999; Lamb, 2002), shipping (Brooks, 1999), and crossing roads (e.g. Clancy et al., 2006; Simpson, Johnston, & Richardson, 2003). Due to their flexibility, the use of VR for social psychology research has also been proposed (Blascovich et al., 2002).

Virtual reality has also been used to treat phobias, including spider phobia (Carlin, Hoffman, & Weghorst, 1997; Garcia-Palacios, Hoffman, Carlin, Furness, & Botella, 2002) and a fear of flying (Brooks, 1999). The suitability of VR for treating driving phobias has also been examined (Walshe, Lewis, O'Sullivan, & Kim, 2005). Walshe et al. (2005) questioned whether VR was realistic enough to be usable as a treatment for driving phobia, i.e. would the participants' immersion in the simulation be sufficient to elicit behavioural changes. They measured immersion through the driving-phobic participants self-reports of the realism of the driving experience, as well as through self-reported distress ratings and heart-rate measures (an increase in heart rate indicating more distress). Most of the participants (10 out of 11) scored highly on all three measuring, suggesting their simulation was immersive. This concept of immersion is generally referred to as presence and will be discussed in the following section.

Presence

Presence[5] is a complex concept, one that has nuances and implications that are difficult to capture (Floridi, 2005). In an early issue of the journal *Presence* presence was defined to be “experienced by a person when sensory information generated only by and within a computer and associated display technology compels a feeling of being present in an environment other than the one the person is actually in” (Sheridan, 1992, p. 274). Another author has argued that presence consists of three aspects, paraphrased as: (1) a sense of being in the generated environment; (2) the extent to which participants respond to events in the VE compared to the real world; and (3) whether the participants feel like they have visited a place compared to simply seeing computer generated images (Slater, 1999). Gilkey and Weisenberger (1995) provide a more detailed summary of some of the suggested design features that may be used to maximise presence.

A number of alternatives for measuring presence have been suggested. For example, Witmer and Singer (1998) proposed a questionnaire measure consisting of seven-point Likert scales with questions such as “How much were you able to control events?” and “How quickly did you adjust to the virtual environment experience?” Another questionnaire consisted of lines anchored by two opposing statements, such as “completely there” and “not there at all” for the *presence* scale (Freeman, Avons, Meddis, Pearson, & Ijsselsteijn, 2000). Participants were required to place a mark on the line indicating where their experience lay in comparison to the two statements.

[5] The term *telepresence* was coined in 1980 by Marvin Minsky to “emphasize the possibility that human operators could feel the sense of being physically transported to a remote work space via teleoperating systems (Lee, 2004, p. 29)”. Telepresence is “considered central to teleoperations and virtual reality endeavour (Zahorik & Jenison, 1998, p. 78)”, with the general term *presence* being used to refer to both teleoperations and VR applications among other things (Lee, 2004).

The utility of questionnaires for measuring presence has been questioned. Slater (2004), in his criticism, used the example of a questionnaire measuring how colourful a particular day was. Although respondents attributed consistent meanings to the questions, and relationships were found with other behaviours, Slater argued that there is no evidence that the mental concept of a day being "colourful" existed during the day being asked about. The only way the respondents could describe their experiences was in terms of the colourful questions, so although they may look back on the day and view it as colourful after the fact they may not have viewed it that way at the time. This is the crux of the argument: We cannot judge whether people felt present in the simulation using these questionnaires as the questions only elicit specific responses. This was not meant to be a criticism of all questionnaires, and the example of "anxiety" questionnaires was used to illustrate the problem. Respondents to an anxiety scale will have past experiences that can be used for comparison purposes, and physiological measures can be taken to give an overall picture of the level of anxiety felt; "In other words, questionnaires can be useful in circumstances where there is a stock of experience against which to judge a given experience, and where comparisons can be made about a specific behavioral outcome (p. 487)". Slater's conclusion was that the only way for presence research to progress was to reduce the dependency on questionnaires as a way of measuring presence. Some alternatives to questionnaires will be outlined shortly.

Self-report measures may have other issues. Stappers, Flach, and Voorhorst (1999) describe an anecdote regarding a participant in a VE who complained about a table in the environment. Although he said it looked nice, it did not look real. However, he tried to use the table for support when standing from a kneeling position and stumbled. Although he reported a lack of belief in the table, his actions implied something else. It is possible then, that what a person *does* in a VE may be a better indicator of the level of presence they feel?

A suggested objective measure like this is task performance, presence being measured by how well someone performs a specific task (Schloerb, 1995). An example of this given by Schloerb (1995) is of throwing a ball in a basket. If a person can throw a ball into a basket half of the time the person's objective presence is given a rating of ½. However, looking at performance measures in this way overlooks actual-world performance. If the same person can only throw a ball into a basket half of the time in the actual environment simulated, meaning their actual and virtual performances are of a comparable level, is presence then rated as 1? Slater (1999) notes that even in the actual world people fail at tasks, for reasons such as poor design or lack of experience. A person being unable to fix a car engine in VR may not indicate a lack of presence if such a task is also beyond them in the actual environment (Mantovani & Riva, 1999). Expecting perfect performance in a simulation to indicate presence, when performance in the actual environment is not perfect, seems to be asking too much.

An Ecological Approach to Presence

Another way to look at presence is in terms of the actions the simulation supports, some authors concluding that "*Presence is tantamount to successfully supported action in the environment*" (Zahorik & Jenison, 1998, p. 87, italics in original). Successfully supported action, as described by the authors, is when a person in an environment acts and the environment reacts in the expected way. For instance, if the environment is simulated, and if the reaction from the environment matches what would be expected in the actual environment, our expectations have been met (Zahorik & Jenison, 1998).

This is congruent with the EA's view of the coupled nature of perception and action (Flach & Holden, 1998; Zahorik & Jenison, 1998). This view focuses on the functionality of the simulation rather than its appearance (Flach & Holden, 1998). Higher presence should be felt if the dynamics of the simulation closely match the actual world (Mantovani & Riva, 1999; Zahorik & Jenison, 1998). This means that the actor should not only be able to complete tasks in the simulation that they can complete in the actual environment, but they *should not* be able to complete tasks that they cannot complete in the actual environment. An affordance-based design approach may facilitate this (Gross, Stanney, & Cohn, 2005). If the affordances of the simulation match those of the actual environment then participants will be able to perform tasks within the VE that they usually can outside of it, and only those tasks.

Stappers et al. (1999) examined the use of pi ratios for measuring presence under this approach. Participants walked through apertures of various widths in the actual environment, with or without the VR equipment on, and in a VE[6]. In the first two conditions, in the actual environment, there was a definite initiation of shoulder rotation at 1.5 shoulder-widths, while in the VE participants always rotated their shoulders. They were sensitive to the size of the gaps, indicated by greater degrees of rotation as the gaps became smaller, but the lack of a critical ratio suggested they could not relate gap size to shoulder size. It could be argued, then, that there was information missing from the simulation that the participants would usually use to inform their decision of when to rotate.

In a separate experiment regarding ball throwing under different simulated gravities, participants reported that a gravitational acceleration of 5 m/s /s felt most real, while the actual value for gravity (9.8 m/s /s) felt too fast (Stappers et al., 1999). Performance was also better at the lower value. Presence, they concluded, may not relate to how well the VE matches the actual environment, and even having high fidelity matches between the environments may not mean that the experiences are equivalent.

[6] It seems likely that their experiment was been based on Warren and Whang (1987), regarding shoulder turning when walking through apertures, but this is not specifically stated.

If we want to measure how well a simulation represents the actual environment, comparing the actual behaviours of individuals in both environments makes sense. Using performance measures like these, rather than ones like those suggested by Schloerb (1995), may be a good way to proceed (Stappers et al., 1999). An example of a measure like this for the current research is outlined on page 37, with some of the participants (specifically one child and one adult) in previous road-crossing research indicating that their behaviour in the VE matched their real-world behaviour.

Potential Issues for VR Research

Simulation Sickness

One of the problems that may occur with VR exposure is simulator sickness (SS). Simulator sickness is similar to motion sickness, but tends to occur less frequently and less severely than motion sickness (Kennedy, Lane, Berbaum, & Lilienthal, 1993). Symptoms of SS include eye strain, sweating, disorientation, and vertigo (Ebenholtz, 1992), although these are not the only symptoms. Extreme symptoms, such as vomiting, are uncommon (DiZio & Lackner, 1992; Kennedy et al., 1993). A questionnaire developed by (Kennedy et al., 1993) measures three SS symptom clusters; *oculomotor* (e.g. eyestrain, headaches), *disorientation* (e.g. vertigo), and *nausea* (e.g. stomach awareness, burping). They provide norms based on research with the United States Navy personnel (Kennedy et al., 1993; Kennedy, Lane, Lilienthal, Berbaum, & Hettinger, 1992), but did not provide any other psychometric properties of their scales.

Although occurring in a smaller proportion of the general population than motion sickness, there are other risks to sufferers. For example, disorientation may cause problems for a driver (McCauley & Sharkey, 1992; Pausch, Crea, & Conway, 1992). It is therefore desirable to minimise risk and to ensure participant safety by alerting them to the symptoms of SS and, if necessary, allowing them time to recover. Other users may suffer from fatigue post-exposure (Pausch et al., 1992). There may also be issues related to training, with simulators being used sub-optimally, if sickness interferes with learning (Kennedy et al., 1992). Behaviours in the VE that are intended to reduce SS may also prove maladaptive to real world performance (Pausch et al., 1992).

The specific causes of SS are unclear, although they are related to a functioning vestibular system since a person with a non-functioning vestibular system does not experience motion sickness (Ebenholtz, 1992). It may be due to intersensory conflict (Biocca, 1992), such as when the observer is not moving but there is optical motion in their visual field (Hettinger & Riccio, 1992). This has been called *neural mismatch*, when the signals from the visual system and vestibular system conflict (Howarth & Finch, 1999). The technology itself may exacerbate problems with errors related to position tracking. The updating of body position may lag, the VE body position may not match the actual position of the body, or the image may vibrate (Biocca, 1992).

Using an HMD may also cause problems. People viewing a game of chess they were playing through an HMD reported more SS symptoms than a group using a monitor (Howarth & Costello, 1997). This was thought to be due to the stable image presented to the participants; although their heads could move freely the image never changed, the opposite situation to the observer being still while the visual field is moving. This configuration was related to a greater increase in symptoms than a previous VR study, suggesting that technological problems such as tracker-lag are not required for SS (Howarth & Costello, 1997). The opposite situation, global optical motion in the visual field while the participant does not move, also seems to cause problems. In a comparison of a static versus dynamic (via treadmill) method of navigating in a virtual environment, higher levels of SS were found for the static simulator (Jaeger & Mourant, 2001). Both cases seem to be examples of neural mismatch, as mentioned before. Weight of the helmet may also exacerbate problems, both by causing muscle strain with prolonged use (Howarth & Costello, 1997) and by increasing the weight of the head. DiZio and Lackner (1992) mention previous studies where SS was made more severe by attaching weights to the participants' heads, and suggest this is due to the change in inertia that the extra weight causes.

In terms of who is more susceptible, it seems that males are at less risk than females (Jaeger & Mourant, 2001; Pausch et al., 1992; Stanney, Kennedy, Drexler, & Harm, 1999). Children and young adults (up to age 21) have the highest risk of developing SS, with susceptibility decreasing with age until it almost vanishes at 50 (Biocca, 1992; Pausch et al., 1992). Pre-existing illnesses, such as colds and ear infections, may also increase the risk of developing SS (Pausch et al., 1992).

Postural Stability

Exposure to a VE does seem to cause subsequent problems for postural stability, but this impairment is only short-term, lasting for about 10 minutes after exposure (Cobb, 1999; Cobb, Nichols, Ramsey, & Wilson, 1999). Simulation sickness, then, appears to be the biggest issue relating to the use of VR.

Summary

Ecologically, VR provides a good framework for the current research. Virtual environments allow participants to be active explorers, and allow the constraints of the simulated task to more closely match the constraints of the actual task. It is also possible to examine behaviours and situations in VR that may be too dangerous to study in the actual environment, such as road crossing behaviours. The general level of presence supported by the VE may be measured by comparing how people act in the VE with how they act in the actual environment, and also by how well the constraints of the simulation match those of the actual world. One potential problem stemming from the use of the VR, SS, may cause issues for research, but it seems that SS may be more detrimental for training situations. Overall, then, VR appears to be a good way to study how cell phone conversation affects pedestrian behaviours.

CHAPTER 4: PEDESTRIAN RESEARCH

As the primary task for the current experiments involves road-crossing it is necessary to outline some of the previous research into pedestrian behaviour. This chapter will begin with a review of real-world experimental studies into pedestrian behaviours, followed by an outline of various types of laboratory research. The last part of the chapter will detail observational studies, and how their findings compare to the real-world and laboratory studies.

Real-World Studies

The main focus of most road-crossing research has been children, given that they have a high risk of being killed or injured (Connelly et al., 1998; Connelly et al., 1996). This research has often been conducted using simulated crossing tasks in the real world, such as the *pretend road* (Demetre, Lee, Pitcairn, & Grieve, 1992; Lee, Young, & McLaughlin, 1984; Young & Lee, 1987), in which the child is positioned on the footpath, one lane-width from the side of the road, and crosses this when they feel it is safe. A similar method is the *two-step task* (Demetre et al., 1992), where the child begins 60 cm from the edge of the road, signaling their crossing decision by taking two steps forward when they feel they could cross the lane safely. Two other tasks that have been used are the *shout task* (Demetre et al., 1992) and the *yes-no task* (Connelly et al., 1998; Connelly et al., 1996). Both of these tasks require the participant to verbally indicate when they would cross, rather than to physically move. With the shout task the children said *now* when they felt they could cross safely, whereas for the yes-no task the children said *yes* as long as they felt it was safe to cross, switching to *no* when the gap was no longer perceived as safe.

One of the more interesting findings from this research is that the participants' decisions seemed to be influenced by the distance the vehicles were from the participants, with riskier decisions being made when the vehicles were further away (Connelly et al., 1998; Connelly et al., 1996). Distance information is useful when all the vehicles are traveling at relatively similar speeds. When this is the case then greater distances will specify larger temporal gaps, given that if speed is constant distance must vary to determine T_A. If vehicle speeds vary, however, attending to distance information becomes potentially dangerous. On urban Canterbury roads in 2004 the average speed was 52.1 km/h, but 15% of vehicles were found traveling at 58 km/h or higher (M.o.T, 2004). If we take an initial distance of 50 m then a vehicle traveling at the average speed will reach an observer in 3.45 s, whereas a vehicle traveling at 58 km/h will reach an observer in 3.1 s. This may not be a large absolute difference, but depending on a pedestrian's crossing speed it may be enough to turn a safe crossing into a potentially dangerous one. While it is unlikely that people would attend to distance information alone, especially given some of the additional experimental findings outlined below, the fact that it influences people's decisions at all is concerning**.**

There are some issues with these methods. The pretend road offers different optical information than the actual road as the participant views the cars from a different angle (Simpson et al., 2003; Simpson & Owen, 2002; te Velde, van der Kamp, Barela, & Savelsbergh, 2005). Although the two-step task, the shout task, and the yes-no task do provide the same optical information as the real task, they are also limited as they do not require the participant to actually perform the task. There is evidence that there is a qualitative difference between verbal judgments of safety and actual task performance (te Velde et al., 2005). The participant is also unable to reassess their crossing decision partway through, perhaps to modify their speed if they are walking too slowly (Simpson & Owen, 2002). Two alternative tasks that do not suffer from these problems are laboratory and VR simulations.

Laboratory and Virtual Reality Simulation Studies

Laboratory Simulations

One form of laboratory simulation that has been used is a mock road and bicycle (te Velde, Savelsbergh, Barela, & van der Kamp, 2003; te Velde et al., 2005). In this simulation a bicycle, pulled by a cable along a track, represents a vehicle and can travel at any speed up to 1.8 m/s. Given that the bicycle could be stopped by the experimenters at any time, actual crossings could occur without any risk to the participants. As well as investigating differences between children with cerebral palsy and control children (te Velde et al., 2003), this simulation has also been used to compare verbal judgments of gap safety with the participant's road-crossing performance (te Velde et al., 2005). Three age groups (5-7 years old, 10-12 years old, and adults), 4 initial distances, and 2 T_A were used. The youngest participants tended to be more cautious, and the adults had more unsafe crossings, but these differences did not reach significance. Participants also appeared to use both distance and the velocity of the bicycle to inform their crossing decisions. Interestingly, they found that participants made more unsafe crossing choices when they were making verbal judgments than when they were actually crossing. This finding of interference has particular relevance to the issue of cell phone distraction, since it suggests that addition of a verbal task turns road crossing into a dual-task situation. This issue will be addressed in detail in Chapter 5.

Oxley, Ihsen, Fildes, Charlton, and Day (2005) conducted an experiment using a task that closely followed the shout task. Using a simulated road, which was projected onto a screen, participants were asked to make two judgments about various traffic gaps; whether they would try crossing in the gap and how safe they thought it was. Three velocities (40, 60, and 80 km/h) and 5 T_As (1, 4, 7, 10, 13 s) were used, distance being varied to specify the gaps. There was an increase in the frequency of gap acceptance with an increase in the temporal length of the gaps, but participants also seemed to use distance information. Within a specific temporal gap size there were more gap acceptances when the vehicle started further away (i.e. was traveling faster). There were some specific cases where a longer T_A and a slower speed produced a shorter initial distance than a shorter TA and higher speed. In these cases there were more yes responses for the longer T_As than for the longer distances, indicating that T_A was also being used to inform the gap acceptance decisions. In a second experiment they determined that allowing more time for a decision did not change the trend noticed in the first experiment for the younger group, but did have an effect for the older participants. When given 5 s to make their judgments, distance had less of an effect on their decisions. When given 1 s, however, there was a greater use of distance information, with a longer distance and a shorter T_A producing a higher proportion of gap acceptances than a shorter distance and a longer T_A.

Video simulation has been used in an attempt to train children to cross roads more safely (McKelvey, 1984). A variety of two-vehicle gap combinations were filmed, with T_As varying between 4 and 9 s, in 1-s increments, and speeds varying between 20 and 80 km/h, in 10-km/h increments, producing 42 different combinations. Distance between the two vehicles was varied to produce each gap. Performance improved with increase in age of the children, and performance was also better overall in the group that was given feedback on their gap decisions. However, one possible issue was that they chose 6 s as the border between an unsafe gap and a potentially unsafe gap, with gaps longer than 7 s being classified as safe. Crossing times for a different sample of children, gained by observing children crossing, varied between 3 and 6 s. This means that it is likely some children accepted gaps that would have afforded safe crossing for them but that would have been scored as unsafe.

Virtual Reality Simulations

Other experimenters have used VR simulations. Plumert, Kearney, and Cremer (2004) investigated gap choice in children and adults. Participants rode a stationary bicycle through a VE consisting of six intersections, the environment being projected onto three screens. At each intersection a continuous stream of cars approached in the near lane, with the cars traveling at a speed of either 25 mph (40.2 km/h) or 35 mph (56.3 km/h), and with temporal gaps between vehicles varying between 1.5 and 4 s in 0.5-s increments. As the speed of the cars was held constant the distance between vehicles was varied to create each temporal gap, as with Oxley et al. (2005). The speeds of the vehicles were counterbalanced, with half of the sample encountering 25-mph vehicles for the first three intersections and 35 mph for the last three, and vice versa for the second group. They found that the main difference between the children and adults was in how long the children waited before deciding to cross and in the time they took to get the bicycle moving. Both age groups chose the same sized temporal gaps. One interesting result was that although the waiting times for the 25-mph group steadily decreased across the experiment, the waiting times at the fourth intersection increased for the 35-mph group. This marked the change in vehicle speed from 35 mph to 25 mph, and hence a decrease in the distance between vehicles. The authors suggested this may have occurred because the change in distance was more salient than the change in velocities.

A number of fully immersive VR studies have been conducted in the Department of Psychology at the University of Canterbury. These experiments have used an HMD to present the VE to the participants. The intent of the earliest of these was to investigate whether road-crossing behaviours could be examined in a VR simulation (Simpson et al., 2003). This experiment used a gap-choice paradigm, where participants were faced with various temporal gaps produced by a line of 10 vehicles and crossed when they though it was safe. Gaps varied between 4 and 10 s, in 2-s intervals. Either the velocity of the vehicles or the initial distance[7] between the vehicles was held constant, velocity at 40, 50, or 60 km/h and distance at 65, 75, or 85 m. In a constant-speed trial distance was varied to produce the T_As of the vehicles, and vice versa. Fewer unsafe crossings were made in the constant-velocity trials, suggesting that distance information was being used to inform the participant's gap choices, or was at least more salient to the participants.

A later experiment (Simpson & Owen, 2002) used a forced-choice paradigm. Participants were faced with a single oncoming vehicle and had to choose how fast to cross, compared to choosing when to cross for the gap-choice design. In addition, rather than having set T_As that were consistent across participants, the T_As for this experiment were *individuated*. Walking speeds were sampled at the beginning of the experiment, with the participant walking across the virtual road at either a normal pace or as if rushing, with no vehicles present. These values were used to determine the T_As of the vans, so that if a participant, when rushing, crossed the road in 2 s the shortest T_A would be 2.5 s (2 s, plus 0.5 s to account for reaction times (RT)). Three van velocities were used, 30, 45, and 60 km/h, distance being varied. Shorter T_As, or slower velocities, would produce shorter distances. As mentioned in Chapter 2, this was the first experiment to use the safety ratio as a dependent variable. Although not studied explicitly, they commented that participants did seem to be using distance information in part to inform their decisions.

Subsequent gap-choice experiments (Clancy et al., 2006; Lamb, 2004; Murray, 2003) have also been individuated using much the same method as Simpson and Owen (2002). These experiments have all used approximately the same design as Simpson et al. (2003), with a line of 10 vans approaching a participant's position. However, as individuation was used, the gaps

[7] The initial distance between vehicles was the distance between the back of a van as it passed the participants position and the front of the following van.

were unique to each participant. For these experiments the shortest T_A was determined by the shortest road-crossing in the trials where no vehicles were present. The longer gaps were calculated by adding a certain percentage (15 or 17.5%) of the shortest gap's T_A to the preceding gap. For example, if the shortest gap was 2 s and 15% percent was being added, the next shortest gap would be 2.3 s, then 2.6 s and so on.

Although these studies all investigated different road-crossing issues, namely mobility impairment (Murray, 2003), ADHD (Clancy et al., 2006), and attempting to train people to attend to optical variables (Lamb, 2004), they all found crossings were less safe when the distance between the vehicles was greater, This is, then, a consistent finding across a variety of participant groups and methods. Note, however, that an unpublished study, with the testing conducted by the author, found that the size of the vehicles also affected mean margins of safety, with larger vehicles producing safer crossings (see Table 1). This may indicate that the distance effect might in part be explained by the vehicles appearing larger when they are closer.

Table 1. The mean margins of safety from an unpublished study which examined road-crossing behaviours in relation to the initial distance vehicles started in relation to the participants, and their apparent optical size. Note that the margins of safety increase as the optical size increases, and decrease as the initial distance increases.

	Optical Size		
Initial Distance	Small	Medium	Large
Close	24.49	58.61	81.61
Medium	16.43	45.54	60.32
Far	12.73	29.55	55.11

Another consistent finding was what has been termed a *cautious crossing* (Simpson et al., 2003). A trial was defined as a cautious crossing if a participant waited for all of the vehicles to pass before attempting to cross the road. If a participant always crossed after all the vehicles had passed they were dubbed an extremely cautious crosser. All of the gap choice experiments mentioned above found that some participants had cautious crossings. Some participants also had to be excluded because most or all of their trials were cautious crossings (Clancy et al., 2006; Lamb, 2004; Murray, 2003; Simpson et al., 2003). For Simpson et al. (2003) it was noted that a 6-year-old participant who was an extremely cautious crosser, had been instructed by her parents to only cross when there was no traffic in sight. This suggests that the simulation provides a reasonable level of immersion (Simpson et al., 2003), which is supported by anecdotal evidence from the other experiments. For example, the extremely cautious adult crosser from the Murray (2003) study commented that she did not feel safe crossing, even though she knew that vans could not hurt her.

Observational Studies

As well as empirical studies, there have also been observational studies. One early study, Cohen et al. (1955), examined the gap sizes that were accepted by pedestrians. They determined that a critical gap size for their sample was 4.6 s, half of the sample not crossing if the gap was smaller than that. Females tended to be more cautious (i.e. chose longer gaps) than the males, although males and females aged between 31 and 45 years old chose similar gaps. No one crossed when the T_A was 1.5 s or shorter.

Oudejans et al. (1996), as touched upon earlier, investigated the effect of locomotion on road-crossing, comparing the gaps that pedestrians chose when they were moving to those they chose when they had stopped before crossing. They filmed 311 crossings from a standstill and 499 from a walk and looked for the T_A values for the critical T_A, the time when crossings occurred 50% of the time. Although crossing times were almost identical in both situations (2.6 s from a walk and 2.78 s from standstill), the critical T_A was 3.02 s when they were walking and 4.63 s when they were at a standstill. These equate to margins of safety of 16% from a walk and 66% from standstill. The standstill number is similar to the mean values found for the VR gap-choice experiments mentioned earlier: 63% for Clancy et al. (2006), 71% for Murray (2003;

first six and last six trials only), and 76% for Lamb (2004). The mean value for one experiment, Simpson and Owen (2002), was lower (approximately 41%), although this experiment used a forced-choice design. This design may have reduced the potential margins of safety as participants had to cross the road in front of the van during each trial rather than being able to choose when to cross. Also interesting is that the standstill gap for Oudejans et al. (1996) value is similar to the critical gap value of 4.6 s found by Cohen et al. (1955), although Cohen and colleagues did not report whether the pedestrians they observed crossed from a standstill.

Another study compared the relative safety of younger and older pedestrians crossing on either one-way or two-way streets (Oxley, Fildes, Ihsen, Charlton, & Day, 1997). These authors found that for two-way streets older pedestrians chose much riskier gaps, especially those whose walking speeds were slower. A greater number of the older pedestrians had margins of safety close to the minimum safe margin, where their crossing time was equal to or greater than the T_A of the vehicle. They also delayed at the curb for longer before crossing. However, on a one-way street many of these differences disappeared, with no differences in curb delays or in general crossing trends.

Gap acceptance, and some of the factors moderating it, had also been explicitly studied. (Harrell & Bereska, 1992) examined the road crossings of 75 individuals and groups. They concluded that groups containing at least one infant chose more conservative gaps (defined as gaps longer than 5.6 s), while the estimated mean group age was related to the choice of risky gaps (defined as gaps under 2 s in length), such that the higher the mean age the less likely a risky gap would be chosen. They found no relationship between gaps chosen and the sex ratio of the group or the traffic volume at an intersection.

Summary

One of the major findings is that people use irrelevant, and potentially dangerous, distance information in part when making their road-crossing decisions, consistent across a variety of real-world and simulation studies. Given that across New Zealand 15% of motorists travel at 55 km/h or more (M.o.T, 2005), and given the exponential increase in likelihood of death as speeds increase (from 10% at 30 km/h to 70% at 50 km/h to almost 100% at 70km/h; A.C.C., 2000), the consequences of misjudging a gap can be high. The distance between vehicles is not the only information used, with some studies finding participants' judgments also being influenced by the T_A or the size of the vehicles.

The results from the studies conducted at the University of Canterbury are consistent with those from other studies, especially in relation to the use of distance information and possibly in relation to mean margins of safety. While the collision rates in the simulated experiments per participant were higher than actual collision rates (Simpson et al., 2003), these similarities suggest that the simulated task does match the real-world task, although not perfectly. It does seem that simulation is a valid way of assessing pedestrian behaviour, especially as participants are able to cross the road. Through the use of VR, the simulated task more closely matches the actual road-crossing task, and therefore meets J. J. Gibson's (1979) imperative that the laboratory must be like life, thereby increasing the generalisability of the results.

CHAPTER 5: DUAL-TASK RESEARCH

This chapter will outline some of the concepts related to dual-task performance. These include: attention (notable for the relationship between attention and dual-task performance; Wickens, 2002); mental workload (included in part to provide a basic background to some of the experimental work reviewed); multiple resource theory, a theoretical model of dual-task performance; and situation awareness, a reasonably new and controversial concept. Note that the current experiments were not intended as tests of these concepts, and therefore these sections are included primarily to provide a background for the research area. The bulk of this chapter will consist of a review of dual-task performance studies: Much, but not all, of this research has been focused on how secondary tasks affect driving performance, with no experiments studying how secondary tasks affect pedestrian behaviours. Some of the methods used will be discussed first, both in terms of the primary and secondary tasks, and then the findings will be reviewed. Also included in this section are epidemiological studies which attempt to determine the actual risk to drivers caused by cell phone use.

Attention

About attention, William James (1891) claimed: "Everyone knows what attention is. It is the taking possession by the mind, in clear and vivid form, of one out of what seem several simultaneously possible objects or trains of thought" (p.403-404). Since then the concept of attention has remained fairly static. Some recent introductory textbooks have defined attention as "the process of directing and focusing certain psychological resources…to enhance perception, performance and mental experience" (Bernstein, Clark-Stewart, Roy, Srull, & Wickens, 1994, p. 204) and "the process of focusing conscious awareness, providing heightened sensitivity to a limited range of experience requiring more extensive information processing" (Westen, 1999, p. 395).

Attention has been conceptualised in a number of ways. One famous conceptualisation is the *spotlight metaphor* (Posner, Snyder, & Davidson, 1980). Another conceptualisation is that attentional resources[8] are limited (Kahneman, 1973; Moray, 1967), dual-task impairment occurring as the limited pool of resources is expended. Finally, there is the bottleneck theory (Müller, Jennings, Redfern, & Furman, 2004), where two stimuli are presented and one 'wins' the competition for processing resources (Fernandez-Duque & Johnson, 2002). These will be discussed in slightly more detail below. However, as the current experiments were not designed to test these theories of attention they will not be discussed in depth. More thorough reviews can be found in Heuer (1996) and Neumann (1996).

It needs to be noted first that *fixated on* does not mean *attended to* (Posner et al., 1980; Strayer, Drews, & Johnston, 2003). For example, if a person scanning the pages of a book begins thinking of something unrelated to the book they may stop perceiving the text (Gippenreiter & Romanov, 1974). Ceasing to acquire task-relevant information is potentially hazardous if it occurs during driving or road crossing. Having the eyes fixated on the road does not mean that attention is focused on the primary task.

The Attentional Spotlight

The original conception of the attentional spotlight seems to have been as a detection enhancer, events within the 'beam' being detected more efficiently than those outside it (Posner et al., 1980). Conceptualising attention in this way poses the problem of determining how large an

[8] Terms other than resources have been used to describe this concept, including effort, capacity, and attention (Kahneman, 1973; Navon & Gopher, 1979).

area the spotlight covers (also known as the bandwidth) (Wijers, Mulder, Gunter, & Smid, 1996). The original analogy has been reformulated, one such version being the zoom lens model (Eriksen & St James, 1986). Under this formulation the size of the spotlight can vary, like a zoom lens (Eriksen & St James, 1986; Wijers et al., 1996). Increasing the size of the spotlight reduces how densely spread the attentional resources are (Wijers et al., 1996).

Posner et al. (1980) conducted an experiment examining whether the spotlight 'beam' could be split. Using a stimulus-response method they found that cuing the participants to the location of the second-most frequently occurring stimulus (occurring 25% of the time, compared to 65% for the most frequent) did not improve RTs to that stimulus unless it was located in an adjacent position to the most frequently occurring stimulus. This seems reasonable, given that it is an attempt to divide attention within a modality (i.e. vision). However, dividing attention between modalities complicates matters. For instance, someone who is driving should intend to attend to the driving situation. In a similar way, someone talking on a phone wishes to attend to the person they are conversing with. Under the basic conception of the spotlight (i.e. Posner et al., 1980), determining how the tasks *interact* seems difficult. However, more recent research indicates that attention can be divided between objects, regardless of their spatial locations (e.g. Cepedia, Cave, Bichot, & Kim, 1998; Davis, Driver, Pavani, & Shepherda, 2000; Driver & Baylis, 1989; Zemel, Behrmann, Mozer, & Bavelier, 2002). Other research has indicated that visual processing can be affected by task-irrelevant auditory objects (Turatto, Mazza, & Umiltà, 2005), so considering the two tasks in terms of objects, both visual and auditory, may be more productive.

Limited Resource Theory

Another theory proposes that dual-task impairment is due to both tasks attempting to draw upon a single pool of resources. On the surface the limited resources analogy seems reasonable given that performance of two tasks performed concurrently is generally poorer than when the tasks are performed independently (Tombu & Jolicœur, 2005; Tsang, Shaner, & Vidulich, 1995). This could be explained by both tasks competing for the same limited pool of resources (Fernandez-Duque & Johnson, 2002; Kahneman, 1973; Moray, 1967). However, results from dual-task studies have not always supported this. For instance, there may be no impairment when impairment would be predicted (Wickens, 1992). Different secondary tasks of a similar level of difficulty may also cause different levels of impairment on the same primary task (Wickens, 1992). Because of these discrepancies (Navon & Gopher, 1979) theorized that rather than there being only a single resource pool, there are multiple resource pools. Wickens developed this idea further, evolving what he called multiple resource theory (i.e. Wickens, 1980; Wickens, 2002). As this model includes both attention and workload components it will be discussed after workload.

The Bottleneck Theory

According to bottleneck theory, two RT tasks cannot be performed concurrently (Kahneman, 1973; Ruthruff, Johnston, Van Selst, Whitsell, & Remington, 2003; Wickens & Hollands, 2000). A proposed reason for this is that certain processing stages cannot be occur concurrently, so the processing for one task must be complete before the second task can be processed, while another is based on the limited resource theory described above (Pashler, 1984). Using bottleneck theory to conceptualise how talking on a cell phone affects driving performance seems troublesome. One issue is that research suggests human attention is more flexible than would be expected assuming the theory is correct (Kahneman, 1973). The experimental tasks used to test the bottleneck pose another problem. Pashler (1984) describes the overlapping tasks paradigm, where two stimuli are presented and each is responded to in the order of occurrence. The presentation of the second stimulus is such that the response to the first stimulus will generally not have occurred (Pashler, 1984). This time-difference in presentation will tend to be under 500 ms (Neumann, 1996). This generally results in both stimuli being responded to more slowly than when they are presented individually (Pashler, 1984).

The difficulty comes when relating these experiments to how drivers may react when talking on cell phones. For instance, while it is fairly clear that braking in response to a leading vehicle braking is a RT task, it is less clear whether a phone conversation is also a RT task. It is possible to measure RTs in relation to a phone conversation, such as the time taken to respond to a question, but this does not seem to be the same sort of RT task as is used in bottleneck experiments. There is also no guarantee that the braking task and the phone task occur concurrently. A driver may also be distracted simply by listening to a phone conversation, a situation that can not be explored in this experimental paradigm. It may be the case that if a RT task, such as braking, occurs concurrently with a verbal response task, there may be differences in levels of impairment depending on how close together the events occur. This could be expected given the finding that as the event-presentation separation increases, the RT impairment decreases (e.g. Heuer, 1996; Pashler, 1994). Overall, it seems as though the theory is too limited to explain how a cellular phone conversation may impair a driver or road crosser, although it might prove to be a useful conceptual model in some cases.

An Ecological Approach to Attention

J. J. Gibson (1979/1986) disagreed with the idea that attention was “strictly… narrowing-down and holding-still” (p. 246). Instead he proposed that the invariant information in an optic array was scanned widely. Ecologically, attention is seen as “the control of detection” (Michaels & Carello, 1981, p. 70), the individual choosing which information to focus on.

The individual is also a knowing agent, one with intentions and goals, and it is these that constrain the information to which the individual attends (Michaels & Carello, 1981). Combining this with the concept that we detect information about affordances, our goals and intentions will determine which affordances we detect. For example, while a chair affords sitting on if we wish to sit, if we wish to reach an object on a high shelf it also affords standing on. Although both affordances are present concurrently, which one we attend to depends on our desired goal. However, misperception of an affordance is also possible (J. J. Gibson, 1979/1986). Although there may appear to be a passable gap in our path of travel, our movement may be blocked by a glass door (J. J. Gibson, 1979/1986). It is possible that cell phone conversation may set the occasion for misperception and / or a lack of perception, neither of which is desirable for an individual. A gap between vehicles may be perceived to afford crossing through safely, but if it does not (for instance, if the pedestrian has attended to irrelevant distance information) an accident may occur. The same may be said if the vehicles are not detected at all.

Mental Workload

Workload is a complex concept, one that is difficult to define (Jex, 1988). However, there are some similarities among definitions. For instance, workload can be discussed in terms of an individual's spare resources (Zeitlin, 1995). Maximum capacity can be seen as the highest level of workload that can be accepted before task performance decreases (Zeitlin, 1995). Jex (1988) defines workload in a similar way, but assumes that people know their spare capacity and can optimise performance. While discussing workload in relation to workplace demands, Wickens, Gordon, and Liu (1998) defined workload as a ratio of the time remaining to complete a task to the time available (also termed *time pressure;* Hendy, 1995). If this ratio exceeds 1 then it is likely that overload will occur (Wickens, 2002; Wickens et al., 1998). All of these conceptions take into account both the task demands and the individual's ability to cope with the demands. For instance, training designed to automate an activity should reduce the workload of that activity (Wickens et al., 1998). Also of note is that performance may be affected by *underload*, where there is too little arousal (Kahneman, 1973; Wildervanck, Mulder, & Michon, 1978), such as in the case of a vigilance task (Wickens et al., 1998).

Various methods have been used to measure workload. The main ones are primary task measures, secondary task measures, physiological measures, and subjective measures (Verwey & Veltman, 1996; Wickens et al., 1998). Primary task measures, as the name suggests, examine performance of the primary task. Higher workload is assumed to impair performance, but this may not always be the case; depending on task complexity good primary task performance may only be obtained with high levels of workload (Wickens et al., 1998). Secondary task measures examine the reserve capacity of an individual by having them perform a low priority task in conjunction with a high priority primary task (Verwey & Veltman, 1996; Wickens et al., 1998; Zeitlin, 1995). Physiological measures can include heart rate variability (Verwey & Veltman, 1996; Wickens et al., 1998), visual scanning (Wickens et al., 1998), pupil dilation (Recarte & Nunes, 2003), and eye-blinks (Hancock, Wulf, Thom, & Fassnacht, 1990; Verwey & Veltman, 1996). Finally, subjective measures involve a questionnaire being administered to the participants, such as the Subjective Workload Assessment Technique (Reid & Nygren, 1988) and the NASA Task Load Index (NASA-TLX; Hart & Staveland, 1988).

Differences between subjective and performance measures of workload may occur due to differences in task demands (Yeh & Wickens, 1988). If a task is not particularly resource intensive an increase in task demands may not affect performance, since the individual has more resources to invest, hence suggesting that a subjective measure may be most appropriate (Yeh & Wickens, 1988). Conversely, if a task is resource intensive, with subjective workload already high, then the individual may not be able to counteract additional task demands by using more resources. Performance is likely to suffer due to the lack of resources but there will be no change in measured subjective workload as it is already at a maximum (Yeh & Wickens, 1988). Meshkati and Loewenthal (1988) provide an in-depth critique of these workload methods, but as workload is not being studied specifically in the current research there is no need to examine the issues in more detail.

Multiple Resource Theory

According to Heuer (1996), there are two types of theory that postulate multiple resources; "a more formal version that has very little concern for the question of what types of resources do exist, and a less formal version that is mainly concerned with the identification of types of resources and neglects the formal requirements of this type of model (p. 134)". Wickens' multiple resource theory falls into the latter category.

His theory is a four-dimensional model of attention and workload (Wickens, 2002). These can be defined by "2 stage-defined resources (e.g. early versus late processing), 2 modality-defined resources (e.g. auditory versus visual encoding), and 2 resources defined by processing codes (e.g. spatial versus verbal)" (Wickens, 1992, p. 375), as well as an additional division in visual processing (focal and ambient processing; Wickens, 2002; Wickens & Hollands, 2000). According to this model there will be more interference between tasks if both draw upon the same resource pool, but less interference if the tasks draw on different pools. The basic dichotomy is between verbal and spatial resources, although they may also be described as auditory and visual, or vocal and manual (Wickens, 1992). Any task involving verbal components, such as listening to speech, doing mental arithmetic, or responding vocally, constitute parts of the verbal pool. Tasks involving spatial components, such as scanning the road ahead, mental rotation, or typing a response, constitute parts of the spatial pool. This model helps to explain why some tasks, such as playing the piano or typing while shadowing speech, can be time-shared perfectly (Wickens et al., 1998).

The theory does have some limitations, such as not accounting for all of the structures that may influence dual-task performance (Wickens, 1984); although in the same book he notes it was not designed to). It does not provide a full account of how the resources are allocated to each task, rather just a general framework of how they may be allocated (Wickens, 2002). He gives an example of how this may be a problem from Strayer and Johnston (2001). Some of their participants seemed to stop attending to a driving task in order to perform a conversation task, even though the two tasks would seem to utilise different resources. One other issue is that, if taken literally, the model would suggest that there should be no interference if the tasks use different resources (Strayer, Drews, & Johnston, 2003; Strayer & Johnston, 2001; Wickens, 1984). Research does not support this stance (see the second part of this chapter), with this situation perhaps better thought of as causing *less,* rather than no, interference (Lamble, Kauranen, Laakso, & Summala, 1999). Limitations aside, multiple resource theory does seem to provide a good framework for considering how a phone task may affect a road-user's performance (Haigney & Westerman, 2001; Lamble et al., 1999; Patten, Kircher, Östlund, & Nilsson, 2004).

Situation Awareness

Situation awareness (SA) appears to be an ill-defined construct (Gaba, Howard, & Small, 1995; Sarter & Woods, 1991; Uhlarik, 2002). Smith and Hancock (1995) defined it as "adaptive, externally directed consciousness" (p. 138), viewing it as the aspect of consciousness that is directed at the world in much the same way introspection is directed at the self. In contrast, it has also been defined as "knowledge relevant to the task being performed" (Gawron, 2000; p.

155). Although both of these definitions place SA in the individual, exactly how it is expressed differs; is it a searching strategy as per the first definition, or knowledge about a task? Others have disagreed with whether we should look for SA within the person at all, saying that we should focus more on the situation rather than the awareness (Flach, 1994). By this he means that we should develop a theory of situations, and from there attempt to understand humans as adaptive beings, not to discover why we may lose SA but rather how we maintain it when completing a task successfully.

Endsley (1995) has proposed a three-stage model of SA. Level 1 SA is the perception of the elements in the environment, Level 2 is the comprehension of the current situation, and Level 3 is the projection of future status (Endsley, 1995). The three stages can be summarised as follows: to be aware of the situation, to understand the situation, and to act in an informed anticipatory manner in the situation. For example, to have full SA a driver would need to know the location of the vehicles around them (Level 1), how the location of the vehicles relates to their goals (such as changing lanes or passing while avoiding an accident; Level 2), and to project the current situation forward in time (Level 3).

This last point can be viewed in light of the field of safe travel defined as the *field of possible paths which the car may take unimpeded* (J. J. Gibson & Crooks, 1938, p. 454). The length of the field is limited by frontal obstacles (i.e. other vehicles), while the width is affected by obstacles present to the sides of the vehicle, such as parked cars or pedestrians . It should also be noted that the field exists optically regardless of whether it is perceived, given that it is the area in which movement can take place without impediment, and it changes continuously as the environment being travelled through changes. Level 3 SA could be characterised in part as knowledge of your personal field of safe travel. It would be difficult to know your field of safe travel if you were not aware of your current situation or did not comprehend elements of it, both of which are required for Level 3 SA (Endsley, 1995). Also, as the field of safe travel helps specify movement outcomes (safe travel if movement occurs within it, a dangerous situation if it does not), then it can be seen as a projection of future status.

It should be noted that under this characterisation a loss of SA does not necessarily mean a loss of action; losing SA does not affect your ability to act but rather to act appropriately. A driver who drives through a red light while tuning their radio has acted, albeit in a dangerous fashion. Similarly, loss of SA does not mean an accident will occur, but it can set the occasion for one to occur. Using the above example, if there are no other cars at the intersection then there is no risk of a crash, the vehicle's movement occurring within the field of safe travel even if the field was not perceived.

In addition to disagreements over the definition of SA, concern has been voiced about the distinction between SA and workload. Hendy (1995) compared the NASA-TLX subjective workload inventory to a SA measure, the Situation Awareness Rating Technique, and concluded that the SA measure seemed to be tapping into workload rather than a distinct construct. Flach (1994) has also expressed concern over the relationship between workload and SA, as well as the difficulty of trying to separate SA from skill or expertise. One of the major issues that Flach (1995) saw with SA is that it is used causally, such as saying that an accident was *caused* by loss of SA. The evidence that it was lost is that there was an accident, leading to circular reasoning (Flach, 1995). That said, SA may be useful when viewed as the description of a phenomenon rather than as something causal (Flach, 1995).

Smith and Hancock's (1995) characterisation of SA may inform a useful ecological method for measuring changes in SA. As described previously in this section, they defined SA as "adaptive, externally directed consciousness" (p. 138), but they also characterise it as the invariant at the core of Neisser's (1976) Perception-Action cycle in terms of supporting skilled performance. It is, then, a characteristic of both an individual and their environment, rather than an attribute of an individual alone. Under this characterisation performance changes can index reductions in SA. Specifically for the current experiments, changes in SA may be seen in the difference between the safety margins for a participant when they are engaged in the conversation task compared to when they are not. More directly, the third experiment examines whether a conversation task affects the likelihood of a pedestrian crossing the road without looking for traffic first: Failing to look for traffic will result in an obvious reduction in SA. Impairment in either of these two outcomes would appear to indicate that the participant was not as aware of their situation while engaged in the secondary task. As Flach (1995) notes, however, care must be taken not to enter into circular reasoning.

Cellular Phone and Dual-Task Research

As the experiments described in Chapters 6 to 9 did not use a driving simulator the overview of the simulations used is intentionally general. More specific details of the conversation tasks used and the findings are presented as these topics are of greater relevance for the current studies.

Experimental Studies

This review of experimental studies will begin by examining the driving tasks (simulated or real-world) that have been utilised. Following this will be an examination of some of the findings from these studies, organised into four categories: Pedestrian-relevant findings, for studies where the tasks are directly related to pedestrian behaviours; pedestrian-analogous findings, for results from driving studies that may have pedestrian analogues (e.g. RT and SA results); driving-specific findings, which will outline other ways in which a secondary task has been found to affect driving performance; and comparison studies, for findings from studies comparing different impairment-causing behaviours (e.g. cell phone conversation compared to driving under the influence of alcohol).

Driving Situations

Research on the effects of cell phone conversation on task performance has dealt predominantly with driving tasks, although there have been some exceptions. These have included a video cassette recorder programming task (Monk, Boehm-Davis, & Trafton, 2004), responses to stimuli via a keyboard (García-Larrea, Perchet, Perrin, & Amenedo, 2001; Golden, Golden, & Schneider, 2003), a visual scanning task (Pomplun, Reingold, & Shen, 2001), a physical pointing task (Lyda, Osborne, Coleman, & Rienzi, 2002), and a cursor tracking task (Strayer & Johnston, 2001). It may be difficult to relate findings from studies like these to the real world given the disparity between the studied task and the behaviour of interest (driving while using a cell phone). Consequently, most of the research uses some form of driving task. The driving tasks can be divided into four categories: Actual road driving, closed track driving, lower fidelity laboratory simulation, and higher fidelity laboratory simulation. The distinction between the last two categories will be made clear shortly.

Actual road driving tasks have the highest ecological validity, the experiment being conducted in the same environment as the actual task. A variety of different situations have been examined, including car following (Brookhuis, de Waard, & Mulder, 1994; Lamble et al., 1999); heavy and light traffic conditions (Brookhuis, de Vries, & de Waard, 1991); different width roads (Wikman, Nieminen, & Summala, 1998); city and non-city (open road) driving (Brookhuis et al., 1991; Törnros & Bolling, 2005); and general open road driving (Matthews, Legg, & Charlton, 2003; Patten et al., 2004; Recarte & Nunes, 2002; Recarte & Nunes, 2003). There may be ethical issues involved, however, as there is generally an expectation of driving impairment. This means experimenters may be putting participants at risk (Gugerty, Rakauskas, & Brooks, 2004). Another issue is that while the participants may give informed consent other road users cannot (Haigney & Westerman, 2001). Experimenters may also be asking their participants to break the law if local laws forbid the use of a cell phone while driving, or if by using a phone the driver is not considered to be in full control of the vehicle (Haigney & Westerman, 2001).

One method for reducing risk is to use a closed circuit track, although this does not appear to be a particularly common method. Brown, Tickner, and Simmonds (1969), probably the first experimenters to investigate the use of a telephone while driving (Brookhuis et al., 1991), used this method, as have a few others (Tashiro et al., 2005; Tijerina, Parmer, & Goodman, 2000; Treffner & Barrett, 2004). However, most experiments use some from of laboratory simulation.

For current purposes, *lower fidelity* refers to simulations which do not reasonably match real-world constraints in respect to information or control. For example, while McKnight and McKnight (1993) provided participants with vehicle controls and showed footage of actual driving situations, the participants had no actual control over the simulation. Moving the wheel would indicate that they had detected a hazard, but the vehicle would continue on its predetermined path. Other tasks have consisted of controlling a triangle representing a car on a curved line or blue strip representing a road (Briem & Hedman, 1995; Graham & Carter, 2001), having to respond to a red lamp (simulating a brake light) by releasing an accelerator and applying a brake (Consiglio, Driscoll, Witte, & Berg, 2003; Irwin, Fitzgerald, & Berg, 2000), a Playstation console game (Jenness, Lattanzio, O'Toole, Taylor, & Pax, 2002), and a change detection task (McCarley et al., 2001).

Higher fidelity, as used here, refers to simulations where there *is* a reasonable match between the simulation and the actual task, meaning the information provided and motion controls are similar. This includes simulations that may not be generally considered high fidelity, ranging from a desktop VR simulation (Beede & Kass, 2006; Gugerty et al., 2004), through to a full-motion simulator with temperature and sound controls (Alm & Nilsson, 1994, 1995). Most higher fidelity VR studies simulate situations similar to those mentioned under actual road situations, including city driving (Hunton & Rose, 2005; Strayer, Drews, & Johnston, 2003), motorway or open road driving (Charlton, 2004; Engström, Johansson, & Östlund, 2005; Haigney, Taylor, & Westerman, 2000), easy or difficult roads (Alm & Nilsson, 1994, 1995; Liu, 2003), a closed-circuit track (Rakauskas, Gugerty, & Ward, 2004), and car following on a motorway (Strayer, Drews, & Crouch, 2003). Driving a truck at night has also been simulated (Drory, 1985), but no examples of this task being used in a real-world dual-task study were found at the time of this review.

Secondary Tasks

As well as a variety of driving tasks, numerous secondary tasks have been utilised. These can be loosely separated into three categories: visual/manual tasks, cognitive tasks, and conversation tasks. Although conversation can also be viewed as a sub-category of cognitive tasks, the distinction will be made clear in the following discussion. It is also important to note that tasks can overlap categories. For example, conversing on a hand-held cellular phone while crossing a road may involve tasks from all three categories. Finally, some of the tasks used for comparisons between manual and voice dialling, as well as for speech-recognition systems will be discussed.

Visual / Manual Tasks

Visual/manual tasks involve having the participants complete some form of physical task, generally requiring attention to be focused on something inside the vehicle. They include tuning a radio or changing a cassette (McKnight & McKnight, 1993; Tijerina et al., 2000; Wikman et al., 1998) and the physical manipulation of the phone, such as manually dialling a number (Graham & Carter, 2001; Jenness et al., 2002; Lamble et al., 1999; Reed & Green, 1999; Salvucci, 2001; Tijerina et al., 2000; Wikman et al., 1998). Other in-vehicle systems, such as route-guidance systems, have been examined as they also require attention to be focused away from the road (Horberry, Anderson, Regan, Triggs, & Brown, 2006; Jahn, Oehme, Krems, & Gelau, 2005; Santos, Merat, Mouta, Brookhuis, & de Waard, 2005). In terms of ecological validity these tasks seem fairly sound as they reproduce actual tasks that may be performed while driving.

Cognitive Tasks

Cognitive tasks are often used to simulate conversation. One of the benefits of many of these tasks is that they can be scored, allowing secondary task performance to be measured, although this may be at the expense of ecological validity (Haigney & Westerman, 2001). One basic cognitive task has the participants simply listening to the radio or music, with no response required (Briem & Hedman, 1995; Consiglio et al., 2003). More intensive is a *shadowing* task (Charlton, 2004; Patten et al., 2004; Spence & Read, 2003), with participants repeating words or numbers they are given by an experimenter or from a pre-recorded source. Memory tasks, again for numbers or words, are also used (Engström et al., 2005; Pomplun et al., 2001; Recarte & Nunes, 2002; Recarte & Nunes, 2003), as are word-production (Charlton, 2004; Gugerty et al., 2004; Recarte & Nunes, 2002; Recarte & Nunes, 2003; Strayer & Johnston, 2001) and mathematical tasks (Lamble et al., 1999; McKnight & McKnight, 1993; Patten et al., 2004;

Tashiro et al., 2005). Other tasks have included odometer checking and a vigilance task (Drory, 1985), word legibility (Matthews et al., 2003), a general knowledge test (Horberry et al., 2006), the answering of visuo-spatial questions (Beede & Kass, 2006), and information reduction (Treffner & Barrett, 2004).

Specific tests have also been used, such as the paced serial addition task (Brookhuis et al., 1991; Törnros & Bolling, 2005), a combined memory and addition task. The Baddeley grammatical reasoning task (Baddeley, 1968) has been used a number of times (Alm & Nilsson, 1994, 1995; Brown et al., 1969; Haigney et al., 2000). The test involves a participant being given a statement such as "A follows B", followed by the two letters, i.e. BA. If the statement is true given the order of the letters they respond 'true', otherwise they respond 'false'. Modified versions of this test, such as using sentences rather than letters, have also been used (Briem & Hedman, 1995).

As noted earlier, the biggest problem with these methods is their lack of ecological validity. While being easy to measure and score there is the risk that any impairment caused may not accurately represent the impairment that would be result from actual conversation.

Conversation Tasks

The main distinction between conversation tasks and other cognitive tasks is that most others are one-way. That is, the participants perform a specific type of task, such as answering mathematics questions, with no two-way interaction between the participant and the experimenter or confederate. Conversation tasks are intended to allow a dialogue, more accurately modelling actual conversation. The topics of conversation have varied, some being based more specifically on the participants and their interests (Consiglio et al., 2003; Hunton &

Rose, 2005; McCarley et al., 2001; McKnight & McKnight, 1993; Strayer, Drews, & Crouch, 2003; Strayer, Drews, & Johnston, 2003; Tashiro et al., 2005). Others topics were more general, relating to participants' opinions of various current events or controversial topics, as well as closed-style questions (Briem & Hedman, 1995; Irwin et al., 2000; Liu, 2003). One aspect investigated is the intensity of conversation. While many of the studies just mentioned may use more ecological forms of conversation for low-intensity conversations, there is a tendency to use cognitive-style tasks for more intense conversations (e.g. Briem & Hedman, 1995; Liu, 2003; McKnight & McKnight, 1993). Rakauskas et al. (2004) attempted to avoid this by having various questions rated on difficulty, then using those questions rated least and most difficult for the two levels of conversational intensity [9]. However, some of the questions rated as difficult, such as asking the participants if they thought the world would be better or worse in 100 years time, seem on the surface to be of a similar level of difficulty to rated as simple by Briem and Hedman (1995), such as discussing child prostitution in Thailand.

Voice Dialling and Speech Recognition Tasks

As well as investigating of the effects of manual dialling on driving, some experimenters have compared manual to voice dialling (Graham & Carter, 2001; Jenness et al., 2002; Salvucci, 2001). Voice dialling could require the participant to speak each digit in the phone number individually or to say a name to call a number that was stored in the phone. Similar to these studies was one investigating how speech-based e-mail systems affected a driver's attention (Lee, Caven, Haake, & Brown, 2001). The task in this experiment was more difficult than in the voice dialling experiments, given that the simplest system involved three menu levels, each

[9] This method has been discussed here as there was some attempt to create a dialogue between the experimenter and the participant (i.e. "What is your major? Why do you find that interesting?"), although this technique was not as open-ended as the previously described conversation tasks.

with two options, while the complex system had four to seven options per level. These tasks seem reasonable to use given that they are modelling actual tasks that may be used by drivers.

Pedestrian-Relevant Findings

Very few dual-task studies have direct relevance for pedestrian behaviours. However, two behaviours have been studied that do have some relevance; obstacle avoidance and postural stability.

Performing a secondary-task has been shown to affect obstacle avoidance while walking. Chen et al. (1996) found that participants were more likely to step on an obstacle, presented as a beam of light across their walking path, when they were verbally responding to the activation of a light. This impairment was more pronounced when the participants had less time to respond, and for older participants. A 10.8% increase in failure rates for obstacle avoidance was found under dual-task conditions by Weerdesteyn, Schillings, van Galen, and Duysens (2003), They also found that participants who lengthened, rather than shortened, their stride to avoid an obstacle took longer to bring their foot down while performing a secondary task.

Secondary tasks have also increased age differences in postural stability. Maylor and Wing (1996) found group differences in stability between a group of 50-year-olds and a group in their 70s and 80s. Notably, the performance of two cognitive tasks decreased stability to a greater extent for the older group than for the younger group (Maylor & Wing, 1996). Performing three other cognitive tasks also reduced stability, but did so to a similar extent for both groups. Interference was generated between an auditory RT task and a postural RT task, the second measure being how long participants took to regain their balance after the platform below them moved (Müller et al., 2004). They found that for easy tasks the interference was short-lived, and also found no difference in effects between their younger and older groups (in their 20s versus 70s to early 80s respectively).

Pedestrian-Analogous Findings

This section is devoted to studies where results may be analogous to findings related to pedestrian behaviours. For instance, RT and gap-choice behaviours may both be impaired for both drivers and pedestrians. However, behaviours such as lane maintenance and car following do not have analogous pedestrian behaviours and therefore will be discussed in the next section,

Gap Judgements

This section includes findings on judgement of both physical and temporal gaps. Brown et al. (1969) examined drivers' judgements of which physical gaps they could drive a car through. Five different gaps were used. The smallest was 3-in smaller than the test car, and the gaps increased in 3-in steps up to 9 in wider than the car. When engaged in a secondary task participants attempted to drive though more gaps that were impossible (0-in clearance or less) and slightly fewer that were possible. Actual steering through the gaps did not seem to be impaired, although there was a tendency towards worse performance when the gap was only 3 in.

Temporal gap judgements were studied by Cooper and Zheng (2002). A left-turn situation (equivalent to a right-turn in New Zealand) was created by positioning the participants at the side of a closed track while eight vehicles drove past them in a continuous circle. They found that less risky gap acceptance was based upon longer gaps, lower speeds, shorter decision times and the participant being younger. However when not distracted, participants also took into account the condition of the pavement (wet or dry), whereas they did not do so when distracted. Since a vehicle cannot decelerate as quickly in wet conditions as it can in dry conditions, this is an important finding (Cooper & Zheng, 2002). For Horswill and McKenna (1999) participants indicated when they would join a stream of traffic presented via video (the participants were not actually driving). Participants in the dual-task condition chose smaller (i.e. riskier) gaps than those in the single-task condition (Horswill & McKenna, 1999).

Reaction Time

Reaction time is a fairly common measure of a driver's performance, although what the participants are reacting to can vary depending on the particular experimental method. For instance, RT in a visual search task was increased by an additional task, either a memory or auditory task (Pomplun et al., 2001). More relevant to driving is the RT to initiate vehicle braking, often studied via driving simulator or simulated brake light (e.g. a red lamp turning on; Irwin et al., 2000). Results from these studies indicate that RT is increased in dual-task conditions (Alm & Nilsson, 1995; Irwin et al., 2000; Lamble et al., 1999; Lee et al., 2001; Liu, 2003; Strayer & Drews, 2004; Strayer, Drews, & Crouch, 2003; Strayer, Drews, & Johnston, 2003), although in some cases this is limited to more difficult driving tasks (Alm & Nilsson, 1994). Studies comparing conversation to listening to a radio have found a greater impairment from the conversation task (Consiglio et al., 2003; Strayer & Johnston, 2001). Hazard detection RT has been impaired by conversation (Gugerty et al., 2004; Horswill & McKenna, 1999), as has RT while performing a peripheral detection task (Amado & Ulupinar, 2005; Patten et al., 2004; Törnros & Bolling, 2005). For Tashiro et al. (2005) cell phone conversation slowed RTs, and also increased the RT impairment caused by using sedating antihistamines.

Not all studies show clear impairment. The effect on RT for peripheral signals varied for Shinar, Tractinsky, and Compton (2005) depending on the day of experimentation, RTs being shorter with a distraction task on the first three (of five) days, and shorter without the distraction task on the last two. Parkes and Hooijmeijer (2000) found a significant impairment for the first of three presented stimuli in a series, but not for the second or third. However, for all stimuli RTs were increased with the phone task, even if only slightly. Small improvements in RT have also been found in the presence of a phone task, but only when it was paired with a signal detection task (Beede & Kass, 2006).

Speed

Average speed and speed variability have generally been affected by a secondary task. Generally, average speeds decrease with a distracter task (Alm & Nilsson, 1994; Engström et al., 2005; Haigney et al., 2000; Rakauskas et al., 2004; Törnros & Bolling, 2005), while at the same time there tends to be greater variability in speeds (Rakauskas et al., 2004; Reed & Green, 1999). More specifically, variability may decrease for a simple conversation, but increase for a difficult conversation, when compared to listening to a radio (Briem & Hedman, 1995), and greater variance in longitudinal acceleration has been found in short compared to long conversations (Liu, 2003). For Horberry et al. (2006), both an in-vehicle entertainment system and conversation task increased variability around a posted speed limit, but only the entertainment system reduced average speeds. Strayer and Drews (2004) found small, statistically insignificant decreases in speed relating to conversation, but did find that time to regain lost speed increased significantly with a conversation task. This was consistent with an earlier finding (Strayer, Drews, & Crouch, 2003). Dialling has also reduced average speeds (Törnros & Bolling, 2005).

There are almost no incidents of speeds being higher in the dual-task condition, but there are some studies where this has happened. For a simulator study, greater curve approach speeds for less severe curves (i.e. suggested curve speeds of 65 or 85 km/h rather than a suggested speed of 45 km/h) were found when participants were engaged in a secondary task while driving compared to driving alone (Charlton, 2004). However, this could be explained as the secondary task impairing the drivers' braking RTs for the curves. For the 65- and 85-km/h curve conditions the lowest speeds appeared to be at the middle of the curve for the distraction conditions, compared to the entry of the curve for the non-distracted condition. In a car following task, driving speeds were higher for both math and conversation tasks compared to no secondary task (Shinar et al., 2005). In two other conditions of the same study which involved maintaining driving speed at 50 or 65 mph, no speed increases were found. Overall, then, it seems likely that a secondary task reduces driving speeds.

Task Completion Time

Task performance times have been found to increase with a secondary task. This has been found for a pointing task (Lyda et al., 2002), as well as for dialling while driving (Jenness et al., 2002) and for driving times in a closed-circuit study (Brown et al., 1969). Given the evidence that a secondary task slows driving speeds and RTs, this is not an unexpected finding.

Undesirable Driving Behaviours and Outcomes

For driving behaviours, Beede and Kass (2006) found that phone conversation increased the number of traffic violations (speeding, failing to stop for stop signs and traffic lights, or movement outside the boundaries of the lane) and attentional lapses (not scanning at a stop sign, stopping when there is no stop sign or the traffic light is green, and entering an intersection when the light is red). Drivers also took longer to begin driving after stopping at a stop sign or red light when conversing, although this delay was less when the phone task was paired with a signal detection task.

Unsafe driving outcomes include crashes, unsafe incidents, and leaving the road. The findings seem split, with some studies finding no increase in unsafe outcomes (Briem & Hedman, 1995; Rakauskas et al., 2004; Shinar et al., 2005). Other studies have found that phone use increases the number of crashes in a tracking task, especially if the phone is hand-held (Graham & Carter, 2001) and that hands-free phones resulted in no increase but that hand-held phones do (Haigney et al., 2000). Conversation, both with passengers and on a phone, has also been related to increases in both crashes and unsafe incidents, although phone conversation produced more such incidents (Hunton & Rose, 2005). One interesting aspect of this last study was a comparison between pilots and non-pilots. Given that pilots are trained in radio communication (Hunton & Rose, 2005), it was hypothesised that conversation would have less impact on them, and this was the case. Pilots showed no increases in crashes or incidents when talking to passengers, and smaller increases compared to non-pilots when using a phone. They suggested that training may be a way to reduce the impairment from conversation (Hunton & Rose, 2005).

Self-report data from students at five United States universities found that of the 64% of students who reported accidents or near accidents, 21% reported that at least one driver was using a cell phone (Seo & Torabi, 2004). There is also evidence that drivers may not be aware that their performance has been impaired, especially older females (Lesch & Hancock, 2004).

Situation Awareness

There has been evidence suggesting that SA is impaired by phone conversation. Gugerty et al. (2004) used moving scenes presented on a computer monitor, but the participants had no control over these scenes. Situation awareness was measured by asking the participants questions during a pause in the scene, after the scene, or by having the participants indicate how they would react to a specific situation using the arrow keys on a keyboard. They found impairment across most of their measured variables, but found no difference in hazard detection. They determined that for easy-to-detect hazards there was no effect of a secondary task, but if the hazard was more difficult to detect there was impairment. During a pause in the simulation Parkes and Hooijmeijer (2000) asked questions about the current position of other vehicles on the road, and found that participants gave more correct answers when not engaged in conversation.

Gaze and Detection

A radio-tuning task was found to increase glances away from the road more than a phone dialling task or audio-cassette changing task (Wikman et al., 1998). The phone task did produce a greater difference between experienced and novice drivers, the glances of the experienced drivers being of the optimal duration (described as 0.5 to 2 s) more often. Manual dialling has been found to be accompanied by more glances away from the driving scene than voice dialling, and both more so than no dialling (Jenness et al., 2002), but glances away were not related to the number of lane-keeping errors. A secondary task did not reduce mirror checking significantly for Brookhuis et al. (1991), but did interact with the road condition used (easy or difficult). Mirror checking was only reduced for the easy road, a minimum level of attention already being paid to the mirrors on the busier, hard road. Recarte and Nunes (2003) found general reductions in both glances at the mirrors, as well as glances at the speedometer, during dual-task performance. Glances at the speedometer were reduced 3-fold in the presence of a distracter task, and this effect was more pronounced when participants had to travel at a restricted speed compared to a free choice of speed (Recarte & Nunes, 2002).

Charlton (2004) found that the detection of symbols presented on road signs or the dash-board of the vehicle was impaired by two phone tasks, while there was only a slight impairment for symbols presented on the road for one of the phone tasks. For Recarte and Nunes (2003), fewer targets were detected when a secondary task was performed compared to a single task. Strayer, Drews, and Johnston (2003) examined memory for billboards, as well as glance fixations and duration. They found that not only was recall of billboards reduced by a secondary task, but that this could not be attributed to fixation. There was no significant difference between conditions for the probability of fixating on a billboard, nor for the duration of the fixation. As well, there was twice the probability of recalling a billboard if it had been fixated in the single-task compared to the dual-task condition. This suggests that a secondary task can impair the recognition of objects in the environment, even if they are fixated (inattention blindness; Strayer, Drews, Crouch, & Johnston, 2005).

Workload

There have been consistent findings of increased workload in dual task situations. These include subjective increases measured by the NASA-TLX (Alm & Nilsson, 1994, , 1995; Graham & Carter, 2001; Horberry et al., 2006; Lee et al., 2001; Matthews et al., 2003), the Rating Scale Mental Effort (Rakauskas et al., 2004; Törnros & Bolling, 2005), the AFFTC Revised Workload Estimate Scale (Charlton, 2004), as well as a general measures of effort (Brookhuis et al., 1991; Recarte & Nunes, 2003). An objective measure, heart rate, also indicated increased workload during a phone call (Haigney et al., 2000) and with conversations in different driving situations (Brookhuis et al., 1991). Pupil dilation has also increased with a secondary task, indicting higher workload (Recarte & Nunes, 2003).

Driving-Specific Findings

This section is devoted to findings that have no analogous pedestrian behaviours. Although not directly relevant to pedestrian behaviours these findings illustrate other ways in which cellular phone conversation may affect task performance.

Steering Deviations

Evidence for the effect a secondary task may have on a driver's steering is mixed. Overall increases in deviations have been found in relation to a secondary task (Reed & Green, 1999), and in relation to specific parts of a phone conversation (dialling or answering; Brookhuis et al., 1991). However, others have found decreases in deviations (Shinar et al., 2005) or no change (Briem & Hedman, 1995).

Lane Maintenance

The findings for lane maintenance, and similar variables such as lateral position and lateral speed, are not consistent in regards to dual-task impairment. There is evidence that lane maintenance is impaired by a secondary task (Alm & Nilsson, 1994; Jenness et al., 2002; Reed & Green, 1999), that shorter conversations produce less variation than longer ones (Liu, 2003), that longer glances at an in-car task produce greater displacement (Wikman et al., 1998), and that there can also be impairment in tracking tasks (Briem & Hedman, 1995; Engström et al., 2005; Graham & Carter, 2001). Other research has found no effect of a secondary task (Alm & Nilsson, 1995; Briem & Hedman, 1995; Engström et al., 2005; Graham & Carter, 2001; Rakauskas et al., 2004; Shinar et al., 2005), a decrease in variability for conversation but an increase for dialling (Törnros & Bolling, 2005), and less variability when driving on a quiet motorway (Brookhuis et al., 1991). Lane maintenance was also improved for (Beede & Kass, 2006), but this seemed to be due to participants intentionally changing lanes less often when conversing. Manual dialling seems to increase lateral deviations slightly over voice dialling and no dialling, while these last two did not differ (Salvucci, 2001).

Following Distance

Although some research has indicated that participants will increase their following distance when performing a secondary task (Strayer & Drews, 2004; Strayer, Drews, & Crouch, 2003; Strayer, Drews, & Johnston, 2003), this is not always the case. Alm and Nilsson (1995) found that participants did not increase their following distance to compensate for their reduced RT. Other research has found that participants have a 600-msec delay in adapting to speed changes in a leading vehicle (Brookhuis et al., 1991). Horswill and McKenna (1999) found that participants performing a secondary task chose a riskier ‘normal’ following distance, as well as a riskier ‘close’ following distance, than those not performing the secondary task. It should be noted, however, that the participants were just indicating their preferences and were not actually driving.

Comparison Studies

This section includes the findings from studies where different potentially-impairing behaviours have been compared. For instance, studies have examined differences in impairment induced by hand-held versus hands-free and passenger conversations, and between cellular phone conversation and alcohol consumption. One study has also compared how different tasks, including a conversation task, may reduce fatigue-related impairment in truck drivers.

Comparisons between Hand-Held, Hands-Free, and Passenger Conversations

When compared directly there are few differences in the impairment produced by hand-held or hands-free phones (Consiglio et al., 2003; Matthews et al., 2003; Patten et al., 2004; Strayer, Drews, & Crouch, 2003; Törnros & Bolling, 2005). Given that studies using only hands-free phones have also found impairment (e.g. Alm & Nilsson, 1994; Strayer & Drews, 2004) this is not a completely surprising finding. However, some studies have found little or no difference between passenger and cell-phone conversation (Consiglio et al., 2003; Golden et al., 2003; Strayer et al., 2005), suggesting that even talking to a passenger could be hazardous. Although this may be the case there is evidence that passengers may reduce risky driver behaviours (Vollrath, Meilinger, & Krueger, 2003) and relative accident risk (Laberge-Nadeau et al., 2003), although this may depend on the particular driver / passenger combination (Baxter et al., 1990). There may also be problems regarding the regulation of conversation over a cell phone which do not occur, or are less of a problem, with passenger conversation (Crundall, Bains, Chapman, & Underwood, 2005; McKnight & McKnight, 1993).

The types of conversation held between drivers and passengers may also be of a different type than those that drivers hold over cell phones. Drews, Pasupathi, and Strayer (2004) found that 50% of drivers missed their correct exit from a motorway when talking on a phone, compared to 12.5% who were talking to a passenger and 4% when driving alone. They attributed this better performance in part to shared SA between the driver and the passenger; although the topic of conversation was not about the driving conditions, these conditions were discussed. More references to the driving conditions were made in the passenger condition, and both the driver and their conversational partner had more turns talking in this condition.

Comparisons with Alcohol

Hands-free phone conversation has been found to affect the peripheral visual system to a similar extent as a low dose of alcohol[10] (Langer, Holzner, Magnet, & Kopp, 2005), approximately the legal limit. Strayer, Drews, and Crouch (2003) found that talking on a cell phone (hand-held or hands-free) caused similar levels of impairment to being legally intoxicated, although when drunk participants drove more aggressively and while conversing drove more sluggishly.

A Comparison of Different Fatigue Reduction Techniques

Secondary tasks may be beneficial in some situations. In an investigation of how secondary tasks and rest affect truck driver fatigue levels, it was found that a limited conversation task was more effective at countering fatigue-induced performance impairment than a vigilance task or providing the drivers with an extra rest period (Drory, 1985). However, the conversation task also produced higher levels of reported fatigue. This seems to be an example of the situation described by Wickens et al. (1998), where better performance occurred with higher workload, or perhaps an alerting effect similar to the one found by Brookhuis et al. (1991) on a quiet motorway.

Non-Experimental Studies

This section details two forms of non-experimental study: Epidemiological studies, which examine the accident risk associated with cell phone use while driving; and benefit-cost studies,

[10] The authors defined a low dose as being a blood / alcohol level of about 4-5 g of alcohol per 100 ml blood. This seems too high to be described as a low dose, especially given that the legal limit in New Zealand is 80mg / 100 ml (LTSA, 2005a), 1 / 50th of the level described by the authors. The performance impairment produced by a dose this high would be far greater than that produced by using a cell phone while driving, given that the individual would be dead. (The median lethal dose of alcohol is between 400 and 500 mg / 100 ml (Medsafe, 2004), 1 / 10th of the supposed low dose.)

which weigh up the benefits of banning cell phone use while driving against the financial costs of a ban. These studies give some indication of the actual risk posed by cell phone use while driving as well as the feasibility of banning their use.

Epidemiological Studies

A number of epidemiological studies have been conducted to ascertain the actual risk of driving while using a cellular phone. Violanti and Marshall (1996) compared 60 people who had been involved in an accident to 77 people who had not. They found that 13% of the accident group reported that they used a cellular phone while driving, compared to 9% of the no accident group (seven people from each group). When analysing the data from these 14 people they found a 5.59-fold increase in risk for those using their phone for more than 50 minutes a month (not necessarily while driving).

Violanti (1997) compared the characteristics of accidents where a phone was present, or in use, with those where there was no phone present. The presence of a phone was associated with accidents where there were higher rates of unsafe speeds, inattention, driving on the wrong side of the road, striking a fixed object, vehicles overturning, swerving before the accident, entering the other lane, and running off the road. There was a 10.9-fold increase in the rate of fatalities when a phone was present at the accident and a 2.5-fold increase if it was in use.

Phone presence and use were also associated with increased odds of an accident producing a fatality for Violanti (1998): 2.11-fold for presence and 9.29-fold for use. The results suggested that phone use was twice as risky as speeding, three-times riskier than alcohol and drug use, and six-times riskier than inattention, but this did not consider event duration (Violanti, 1998). For example, while a phone call may only last minutes alcohol can stay in the blood for hours (Redelmeier & Tibshirani, 1997b).

Using a case-crossover technique with 699 drivers, where each person is also their own control, Redelmeier and Tibshirani (1997a) found a 4.3 times increase in the relative risk of accident if a telephone was in use 10 minutes before a collision compared to when they were not talking. This increase is comparable to driving at the legal blood alcohol limit. They also found no difference between hand-held and hands-free phone use, but they note that this was based on a small sample (only 21% of the sample reported using a hands-free phone), so this finding may be due to a lack of power. This small sample has been seen as a limitation of the study, especially for informing potential laws (Maclure & Mittleman, 1997). It should also be noted that this increase in risk is on top of other distracting in-car behaviours that the sample drivers may have engaged in, such as talking to passengers or listening to the radio, not compared to a no-distraction condition (Redelmeier & Tibshirani, 2001).

Sagberg (2001) found that drivers responsible for accidents were 2.2 times more likely to have been using a cell phone when the accident occurred compared to innocent drivers. The total proportion of accidents that could be attributed to cell phone use, over and above general exposure, was 72% higher than expected and was significant when driver demographics were taken into account. Hand-held phones seemed to be associated with a greater increase in risk, but as with the Redelmeier and Tibshirani (1997a) study the sample size was too small to draw any definitive conclusions. In terms of the types of accidents that occurred, Sagberg (2001) found that the most frequent accident was a rear-end collision. One general caution he offers is that activities that occur more frequently, such as conversing with a passenger, may cause more accidents in total, even if the relative risk of the less frequent activities is higher (Sagberg, 2001).

Small but statistically significant increases in risk for both male and female phone users were found by Laberge-Nadeau et al. (2003), relative risks of 1.1-fold for males and 1.21-fold for females. They also found an association between number of calls made and relative accident risk, but no difference between phone types. This is similar to the association found by Violanti and Marshall (1996) regarding time spent talking, although the increases found by Laberge-Nadeau et al. (2003) were not as pronounced as for Violanti and Marshall (1996). Violanti and Marshall (1996) found that the relative risks were generally increased between 1.5-fold and 2.5-fold times as the number of calls increased, with infrequent phone users having a similar level of risk those people without cell phones.

Benefit / Cost Analyses of Restrictions

A number of studies have examined whether the financial benefits produced by legally restricting the use of cellular phones while driving are greater than the costs produced by restricting use. The benefits are measured in the reduction of lives lost, injury costs, and property damage, while the costs are taken in relation to the phone users (for example, delays due to stopping to take a call) and to cell phone providers (Hahn, Tetlock, & Burnett, 2000). Two studies concluded that the benefit / cost ratio is close to zero (Cohen & Graham, 2003; Hahn et al., 2000), while a third has found that the cost for each quality-adjusted life year saved may vary widely, from US$50 000 to US$700 000 at the time of publication (Redelmeier & Weinstein, 1999). The authors concluded that a ban would appear to be an expensive way to save lives, and that education may be a better way to proceed.

Implications

This body of evidence suggests that driving while engaged in a secondary task is potentially dangerous. Although there are some mixed findings concerning vehicle control (i.e. steering deviations, lane maintenance, following distance) the more consistent findings are troubling. For instance, the general impairment found for RT may cause more serious problems. In an examination of a variety of collision situations a 300-ms delay in braking was associated with a 38.5% increase in collisions and an 80.7% increase in collision velocity, for vehicles travelling at 56.4 km/h (Lee et al., 2001).

Also of concern are the findings for SA and for gaze and detection. J. J. Gibson (1961) states that "So long as the margin [of safety] is *perceived* and the behavior is controlled, one is safe" (p. 84, italics added). Secondary tasks may impair people's ability to perceive their current situation, a Stage 1 SA error. Although failing to perceive your margin of safety does not mean there will be an accident, it does suggest that it may be more difficult to avoid one.

As mentioned earlier, it has been argued that late detection is the basic driver error (Rumar, 1990), and anything which increases the likelihood of this should be avoided if possible. These findings also suggest that cell phone conversation may increase the risk of *errors*[11], defined by Reason et al. (1990) as "…the failure of planned actions to achieve their intended consequences" (p. 1315). Given that "(e)rrors may be understood in relation to the cognitive function of the individual" (Parker et al., 1995, p. 1036), anything interfering with this cognitive function may increase the chance of errors occurring, although as noted in the section on undesirable driving outcomes there are mixed results from the experimental work. However,

[11] The other types of undesirable driving behaviour have been called *violations* (Parker, Reason, Manstead, & Stradling, 1995; Reason, Manstead, Stradling, Baxter, & et al., 1990). Violations, compared to errors, are deliberate risky behaviours (Parker et al., 1995).

the epidemiological evidence does suggest that interference may be the case, given the association between phone use and accident involvement.

For pedestrians the findings that a phone conversation can cause inattention blindness (e.g. Strayer, Drews, & Johnston, 2003) is a very important one. As described earlier, the consequences of not seeing an oncoming car are likely to be serious to a vulnerable pedestrian. Given that there is evidence that a secondary task can impair obstacle avoidance, and there is no reason to suspect that the attentional impairment is limited to driving tasks. As secondary tasks have impacted on non-driving tasks, this topic deserves further investigation.

Overall Summary of the Pedestrian and Dual-Task Research

This section will summarise the current state of knowledge in the pedestrian and dual-task research areas relevant to the current research. Each area will be addressed separately, and following each summary of the main research findings, some general predictions based on the findings will be made.

Summary of the Pedestrian Research

There are two main findings from the pedestrian literature that are relevant to the current research. The first is that pedestrians make riskier gap judgements when the distance between vehicles is greater, suggesting that people use inter-vehicle distance as a source of information for T_A. This is not the only source of information used by pedestrians however, given the second finding that pedestrians are less likely to cross the road if the T_A of a vehicle is too short, It appears, then, that pedestrians decisions will be influenced by how far away a vehicle is, both in distance and in time, The cause of the distance effect is difficult to determine, as it is possible it is in part or mostly due to closer vehicles appearing larger.

The findings of the pedestrian experiments enable two main predictions to be made. The first is that the further apart the vehicles are the riskier the participants' gap-choice decisions will be. The second is that participants will be more likely to chose a gap if it has a longer $T_{A.}$

Summary of the Dual-Task Research

The overall finding from the dual-task literature is that a secondary task affects task performance negatively. Directly relevant to the current research is that a secondary task affects obstacle avoidance and postural stability, indicating that walking may be impacted by a conversation task. Findings from studies which used driving tasks may also be relevant for pedestrian behaviours. Importantly, drivers' gap judgements have been impaired in the presence of a secondary task. In one experiment (Horswill & McKenna, 1999) participants in the dual-task condition accepted riskier gaps than participants in the control condition, while Cooper and Zheng (2002) found that participants took the condition of the road surface (dry or wet) into consideration when not distracted, but did not pick longer gaps when the road was wet while distracted. There has also been a generally consistent finding that there are more unsafe or undesirable outcomes in the presence of a secondary task compared to no secondary task. Both of these findings are important for the current research, as pedestrians crossing a road have to make gap judgements and the cost of an unsafe crossing can be very high.

More generally, drivers' RT and speeds have been slowed by a secondary task, while the primary task takes longer if a secondary task is present. There have been consistent findings that workload, measured subjectively or objectively, is increased by a secondary task.

Participant SA has been affected by a secondary task, and participants have been generally found to have more glances away from the road and to examine their mirrors less often when engaged in a secondary task. In terms of glance fixation a secondary task has reduced the recall of billboards while not affecting the length of time participants fixated on the billboards, an example of looking but not seeing.

What is not known at this stage is how a secondary task affects pedestrian behaviours. However, if the effect of a secondary task on pedestrian behaviours is similar to the effect on driving behaviours then a few general predictions can be made for the current experiments. Pedestrians' gap choices should be negatively affected by a conversation task, which will most likely result in more unsafe crossing outcomes. If driving speeds are reduced as a way of compensating for the additional demands of a secondary task it is plausible that participants' will reduce their walking speeds for the same reason. It is also possible that the secondary task will affect how the participants direct their attention, and may affect the information the participants use to inform their crossing decisions.

The effects of the simulated conversation task on SA and workload is more difficult to predict. For the current experiments the main evidence for impairment in either SA or workload would be impairment in performance. Since the main theories for why a secondary task impairs performance involve impairment in SA and/or an increase in workload this becomes circular reasoning (Flach, 1995). Therefore no predictions regarding SA or workload will be made for these experiments.

The Current Experiments

The following experiments address different aspects of the road-crossing task that may be affected by cell phone conversation. Experiments 1 and 2 examine the gap-choice decision that pedestrians make when crossing through traffic. For each trial participants are presented with a line of 10 vans and their task is to choose which gap to cross through. The vans have variable T_As and for half of the trials the participants are engaged in a simulated cell phone conversation. While the participants for Experiment 1 are only 18 to 24 years old, for Experiment 2 a younger group aged 18 to 24 years old are compared to and older group aged 50 to 67 years old.

Experiment 3 examines how cell phone conversation affects the direction of attention. For this experiment participants are positioned on the edge of a road that was empty 90% of the time, vehicles being present on the remaining 10% of the trials (4 out of 40 trials). Their task is to look for vehicles and to cross the road if **no** vehicles are approaching.

The final experiment is an examination of an alternative explanation for a specific effect found for most of the preceding experiments, a decrease in participant walking speed. This study does not involve any road crossing decisions. Instead participants simply have to walk 3 m (the width of half the simulated road) while engaged in simulated conversation or in silence, and in the actual environment or the VE.

Chapter 6: Experiment 1

This experiment was designed as an initial exploration of the effects of the simulated cellular phone conversation on pedestrian behaviours.

Hypotheses

Simulated Cellular Phone Conversation

Given the lack of previous research into the effects of cell phone conversation on pedestrian behaviours the hypotheses for this experiment are based on analogous findings from the driving studies reviewed in Chapter 5.

1. Participant safety will be impaired when they are engaged in the simulated cell phone conversation. This will be demonstrated through a decrease in their margins of safety and an increase in near misses and collisions.
2. The participants' road crossing behaviours will be negatively affected by the simulated conversation, so they will pick smaller temporal gaps to cross in and will delay for longer before crossing in their chosen gap, They may also walk slower if the finding from the driving literature that people drive slower when engaged in a secondary task represents a coping mechanism for the increased task demands.
3. The participants may become more cautious, as indicated by allowing all of the vehicles to pass before trying to cross more often, or allowing more potentially safe gaps to pass, when engaged in the conversation task.

1. Safety will reduce as the distance between the vans increases, with longer initial distances being associated with lower margins of safety and an increase in near misses and collisions.
2. Participants will chose smaller gaps and use less of each gap as the distance between the vans increases; however, walking speeds will not vary across distances (Clancey et al, 2006; Murray, 2003).
3. Participants will be more cautious, allowing more safe gaps to pass and having more cautious crossings, when the initial distance between the vans is shorter.
4. Participants will be more willing to cross in the gaps with the longer T_As than in the gaps with the shorter T_As.

Method

Participants

Fifty-five participants were recruited for Experiment 1, 27 male and 28 female, aged between 18 and 24 (mean age = 21.2 years, SD = 1.98). All participants were students at the University of Canterbury, predominantly from the Department of Psychology, and all reported having normal or corrected normal vision and hearing. Five participants were removed from all but one of the analyses due to absent or insufficient data, defined as fewer than 3 out of 5 usable trials for at least one of the six experimental cells, leaving 25 males and 25 females with a mean age of 21.24 years (SD = 1.94). The one exception will be detailed under the dependent variable section. Seven of the included participants, and one excluded participant, reported never having driven a car in a city, while one included participant had not driven a car but had driven a motorcycle. As the bottom of the shelf holding the VR transmitter was 195 cm from the floor, and in the path of the participants, only participants under 190 cm in height were used (the 5 cm difference is accounted for by the height added by the HMD). There was one exception to this, a participant who was 201 cm tall. However, this participant commented that he had lived in a house with short doorways and had learned to walk in such a way as to avoid colliding with the doorways. As he walked the same way for both sets of trials this was not considered to cause any problems. Participants were given a $5 food voucher for their time.

Materials and Apparatus

The Actual Environment

The VR laboratory was 8.05 m wide by 8.17 m long, and was approximately 2.95 m high./ The VR laboratory was organised so that there were no obstacles in the path of the participants.

The Virtual Environment

Custom in-house software was used to generate the VE, which had a straight, two-lane section of road with traffic present in the near lane only. The centre line of the road consisted of a broken white line which divided the road into two 3-m lanes. There was a continuous white edge line along both sides of the road. A tree was located directly behind the participant's starting position and a street light was located directly opposite. See Figure 1 for a bird's eye view of the environment. The vans used to represent the traffic were 174 cm wide by 438 cm long.

Hardware

The VE was generated on a 2.8 GHz Pentium 4 PC with 512-Mb of RAM and a 128-Mb GeForce4 TI 3D graphics accelerator card. The VE was viewed through a Virtual Research Systems V8 HMD containing two full-colour 3.3-cm 640-by-480-pixel active matrix liquid crystal displays with a refresh rate of 60 frames per second, presenting a 48-degree horizontal by 60-degree diagonal field of view to each eye. The virtual world was represented stereoscopically.

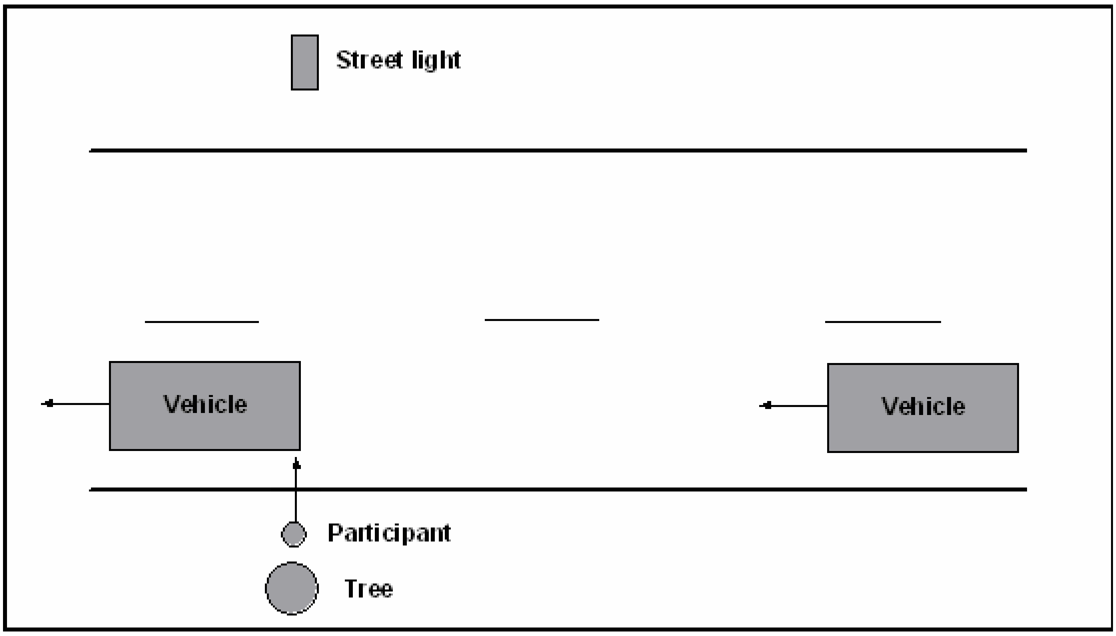

Figure 1. A bird's eye view of the central portion of the virtual environment. The participant is shown at the starting position at the side of the road with one vehicle just past and another approaching the intended crossing path.

The system included a 6-degree-of-freedom head tracker (Ascension Technology Flock of Birds with extended range transmitter) with an orientation and position sample rate of 60 Hz. The position of the participant was recorded from a receiver on the top of the HMD. Movements of either the head or the entire body changed the *camera viewpoint*, i.e. what the participant saw. The camera viewpoint was used to determine whether a crossing was unsafe (i.e. resulted in a near miss or collision; see the dependent variables below).

A hands-free cellular phone was simulated using the headphones attached to the HMD and a microphone that the experimenter used to talk to the participant.

The Simulated Cellular Phone Conversation

Conversation consisted of the experimenter verbally administering the activity section of the Activity Risk Taking Questionnaire[12] (ARTQ; see Appendix A) to the participant, the last page of the ARTQ being completed post-experiment. This ensured that the nature of the conversation did not vary between participants. Before starting the experiment the format of this section of the ARTQ was explained to the participants. During the experiment they were given the name of an activity and had to report the following information: whether they had done the activity before and if so how many times; whether they would not, may, or would do the activity; and how risky they considered the activity to be by rating it on a 10-point scale. For instance, if they were given the activity *bungy jumping* a participant may reply with; “once”, “would not do it again”, and “7” out of 10 for risk.

It was stressed that their answers should be as accurate as possible, and the participants were given the opportunity to change their answers later if they wished. If they forgot what they needed to report they were prompted by the experimenter. Activities were presented to the participants continuously during the *conversation* trials to more accurately represent conversation. This also ensured that the simulated conversation was occurring at the beginning of the appropriate trials. No talking occurred during the *no-conversation* trials unless absolutely necessary (i.e. to prevent the participant from walking into an object or a wall).

[12] The ARTQ was developed by Dean Owen and the author. The original intent of the questionnaire was to investigate whether there was a relationship between road-crossing behaviours and willingness to participate in risky activities.

This format was chosen to ensure that the participant would do the majority of the talking during the conversation trials, and that the focus of their attention would be on the conversation task. Task elements included recall for how many times they had done the activity, a judgement of how risky the activity was, and whether they would consider doing it. Under the previous characterisation (see page 55) this was a cognitive task, given that the same specific information was requested for each activity. However, some participants chose to go into greater detail than was asked for, but this was not common.

The information collected by the ARTQ was not used in the following analyses as it would likely have added a needless level of complexity, especially given the limited findings from Murray (2003). The ARTQ scales correlated poorly with the dependent variables used in that experiment (these variables were similar to those described below), with the main finding being that participants who had at least one cautious crossing also reported having performed more of the activities in the past than those participants with no cautious crossings. As risk-taking is not a focus of this study it will not be discussed further.

Independent Variables

The independent variables were *Conversation* condition (conversation or no-conversation) and the *Initial Distance between Vehicles* (the distance between the rear of a lead vehicle and the front of a following vehicle as the lead vehicle passes the participant's position; 40-, 50-, or 60-m). For a given trial only one initial distance between vehicles was used (i.e. within a trial vans could be 40 m apart, or 50 m apart, but not both).

Time-to-Arrival

While some experiments have manipulated T_A as an independent variable (e.g. Oxley et al, 2005; Simpson et al, 2003), that was not directly possible for this experiment. While T_A has been included as a dependent variable (the total time available to cross) there was no direct experimental control over T_A. Each participant had their own specific T_As (e.g. one person's shortest gap may be 1.5 s while it may have been 2 s for another), and the T_As gaps were randomly assigned within each trial. In terms of gap selection, it is possible for some participants to cross through all 10 T_A gaps during the experiment, while others may not. For instance, if a participant only chose the five longer gaps there would be no data for the five shorter gaps. Also, if a certain gap only occurred later in a trial (for instance, if the longest temporal gap was always one of the last five gaps presented), and a participant always crossed within the first four gaps presented to them they would never encounter the longest gap. This lack of control over when the gaps were presented also means that there is no guarantee that all gaps would be used for both conversation conditions.

Although these limitations mean T_A cannot be treated as an independent variable it is still possible to examine the effect of T_A on the participants' willingness to cross the road. The following steps were performed on each participant's experimental data to enable an analysis of the likelihood of a gap being selected in relation to the other gaps. Extremely cautious crossers were excluded from this analysis, as were any trials where a cautious crossing occurred. These steps were performed for the Conversation and No-conversation conditions separately. Table 2 provides an example for steps 2 to 8 for one participant for the no-conversation condition only.

1. The T_A gaps were converted into their ordinal ranking, 1 being the shortest gap and 10 being the longest.
2. The total number of times each ordinal gap was available for the participant to cross in was calculated. This is equivalent to 15 minus the number of times the ordinal gap

occurred, within a trial, after the gap that was selected. For each ordinal gap this total could vary between 0 and 15. This value will be referred to as *presented*.

3. The total number of gaps that were presented to the participants was calculated. This is simply the sum of the *presented* values for each ordinal gap.
4. The total number of times each ordinal gap was selected was calculated. This could vary between 0 (the gap was never selected) to 15 (the gap in which the participant always crossed) for each ordinal gap, but was capped at the *presented* value for that specific gap (e.g. if Gap 3 was presented 10 times, by definition it could not be selected 11 times). This value is referred to as *selected*.
5. The total number of gaps selected was calculated. This value was 15 minus the number of completely cautious crossings for that participant in the specific conversation condition.
6. The proportion of times each ordinal gap was presented was calculated.
7. The proportion of times each ordinal gap was selected was calculated.
8. A ratio was taken of the percentage of times each ordinal gap was selected by the percentage of times each ordinal gap was presented. A value higher than 1 indicated that the ordinal gap was preferred, while a value less than 1 meant that it was avoided.

Table 2. An example of the steps used to calculate the likelihood ratio of a gap being selected for one participant (no-conversation condition only)

	Ordinal Gap Number									
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Selected (total=15)	0	3	2	1	1	2	2	1	2	1
Presented (total=62)	3	7	6	3	8	5	7	7	8	8
Proportion Selected	0.00	0.20	0.13	0.07	0.07	0.13	0.13	0.07	0.13	0.07
Proportion Presented	0.05	0.11	0.10	0.05	0.13	0.08	0.11	0.11	0.13	0.13
Selected/Presented	0	1.77	1.38	1.38	0.52	1.65	1.18	0.59	1.03	0.52

Dependent Variables

The dependent variables are presented in Table 3. Dependent variables for Experiment 1. While most of these variables are sufficiently explained under the description heading, three require more explanation. The *percentage of the gap available to use* indexes how much of the chosen gap was used by a participant. It was calculated using the following formula (Eq 5):

$$\text{Percentage of gap available to use} = \frac{T_A \text{ of van when participant begins to cross}}{\text{Total } T_A \text{ of the van}} * 100 \quad \text{(Eq. 5)}$$

The last two variables were included to examine the hypothesis that conversation may result in participants becoming more cautious, i.e. rejecting gaps that would have afforded safe crossing or not crossing at all. The *number of safe gaps rejected* was calculated by dividing the T_As of all the vans for a trial by the participant's time to cross, less the time they waited before crossing. This created a list of *safety ratios* (see Equation 2; page 14) for each trial, which were converted into *margins of safety* (see Equation 3; page 14). A cut-off margin of safety was determined for each participant which was based on the lowest margin of safety obtained that did not result in a near miss or collision (see Table 3). The number of gaps before the chosen gap which had margin of safety greater than the cut-off score was calculated for each trial. For example, if a participant crossed in the 5th gap and only gaps 2 and 3 would have afforded safe crossing, this value would be 2. On the other hand, if Gap 5 was the first safe gap this value would be 0.

A *cautious crossing* was said to have occurred when a participant waited for all of the vans to pass before crossing the road. These were the main cause of missing data for the other variables, with computer errors[13] being a minor secondary cause. Given that it is possible that

[13] Occasionally at the beginning of a trial the participant would be placed behind the tree, rather than next to the road. This seemed to occur when the participant was taller (over 180 cm tall) and was also standing too far behind

cell phone conversation may increase the likelihood of cautious crossing occurring, they were included as a dependent variable. Because data from those participants who were excluded from the other analyses for having too many cautious crossings may have been informative (i.e. for comparing if they had more cautious crossings in the Conversation condition than the No-conversation condition) they were included for this analysis.

Table 3. Dependent variables for Experiment 1.

Variable	**Description**	**Unit**
Safety Ratio	The available crossing time from when the participant moved 0.5 m from the starting point divided by the time taken to cross to the far edge of the van.	-
Margin of Safety	The safety ratio expressed as a percentage (1 – safety ratio) * 100	
Near Misses	Crossings in which the participant was within 0.5 s of being hit by a van	-
Collisions	Crossings in which the participant was hit by a van	-
Walking Speed	The speed with which the participant crossed from 0.5 m to the far edge of the lane.	m/s
Percentage of Gap Available to Use	The percentage of the available gap used by the participant. (If the gap is 2 s long and the participant waited for .5 s before crossing, this value would be 75%.)	-
Total Time Available to Cross	The time-to-arrival of the van that the participant crossed in front of.	
Number of safe gaps rejected	The number of gaps that would have afforded safe crossing but that the participant rejected.	-
Cautious Crossings	Whether or not the participant waited for all of the vehicles to pass before crossing the road.	-

These dependent variables can be divided into three categories. The first is the *measures of safety*, consisting of the margin of safety, near misses, and collisions. The margin of safety is a fine-grained measure of the safety of a crossing, whereas the other two are coarser measures. A

the red line on the floor (see Procedure). Other issues included vehicles being present but invisible, and on occasion a trial would end just after it commenced. These were all very infrequent.

decrease in the margin of safety always indicates a decrease in safety, but will not necessarily be associated with an increase in near misses or collisions.

The second category is the *potential components of safety*. They are walking speed, the percentage of the gap available to use, and the total time available to cross. Each of these variables may be related to the safety of a crossing. For instance, a crossing where the participant's walking speed is reduced, where they use less of a gap, and where they chose a smaller gap is likely to be less safe than if they walk faster, use more of the gap, and pick a larger gap to cross through.

Finally, the third category consists of the *measures of caution*. These are the number of safe gaps left and cautious crossings. As the gaps are randomised within each trial there is no consistent benefit from waiting longer; the largest gap could be the first gap, the last gap, or anywhere in between. It is possible that the number of safe gaps left may relate to overall safety in a general way. If a participant leaves 5 safe gaps then, unless all of the gaps afford safe crossing, it is less likely that the gap they chose will be safe. However, it is likely that at least one of the remaining gaps will afford safe crossing given that the shortest gap is based on their individuated walking speed. A cautious crossing is a safe crossing: If a participant waits for all of the vehicles to pass before crossing then the crossing has to be safe. However, as a general crossing method, waiting for a clear road may not be desirable, especially in areas with high traffic flow. Although waiting ensures a safe crossing, it may also mean a long wait.

Procedure

The experiment consisted of two sessions held sequentially. Each session consisted of 38 trials. The first eight trials were referred to as *individuation* trials, and no traffic was present for these trials. For the first two individuation trials the participants walked with the HMD on their head but not covering their eyes. For these trials they were asked to walk in the actual environment to the centre of where the road would appear in the virtual environment (marked with a line of white tape on the floor of the laboratory) at a normal or a rushing walking speed. The remaining six individuation trials were conducted in the VE, with three trials performed at a normal walking speed and three at a rushing walking speed. A trial ended when the participant reached the centre of the road. For the experimental trials (described shortly) all of the vehicles disappeared when a trial ended, so that the road was empty when the participant returned to the starting point.

There were two purposes for the individuation trials. The first was to gain a measure of the participants' normal and rushing walking speeds in the both the AE and the VE. This data was used for a later analysis (see the Results section). Second, each participant's shortest crossing time was used to calibrate the T_As for the experiment. For instance, if a participant crossed to the centre of the road in 2 s the shortest T_A for their vehicles would be 2 s. The remaining T_As were calculated by adding 17.5% of the first gap onto the following gaps. Unlike Simpson and Owen (2002) no additional time was added to the T_As of the vans. An example of the van velocities across the three initial distances and ten T_As this situation is given in Figure 2. The horizontal axis of Figure 2 indicates the range of T_As that would be produced. These gaps were randomly assigned within each trial, so the first gap presented to a participant could be the shortest gap, the longest gap, or one of the eight intervening gaps.

All of the experimental trials were conducted in the VE. The first session was a familiarisation session and was intended to provide the participants with experience moving in the virtual world as well as with the road-crossing task. Previous research using the VR road-crossing simulation has found that participants' walking speeds in the VE increase with continued exposure to the simulation (Murray, 2003; Simpson & Owen, 2002). As a participant's initial

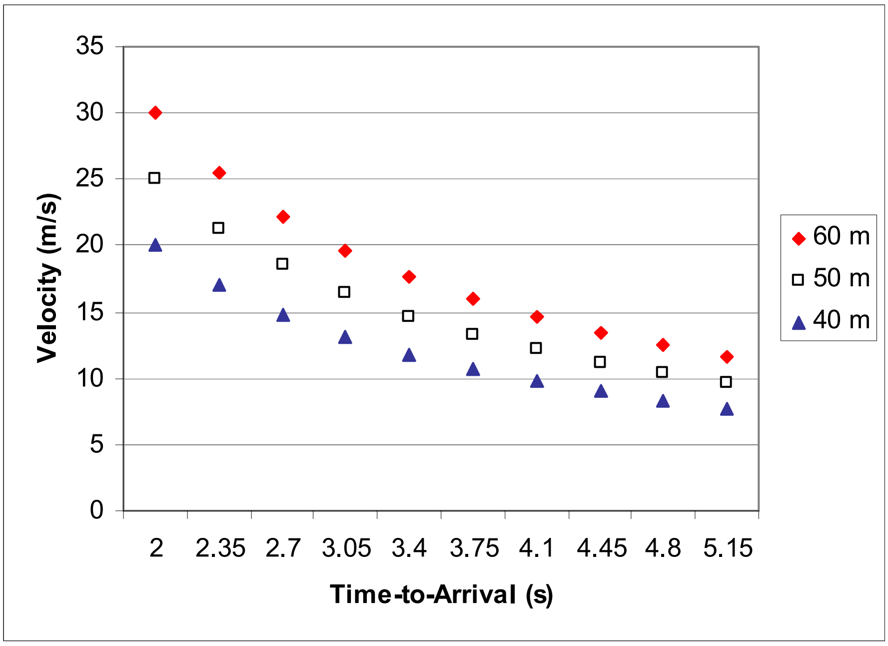

Figure 2. The vehicle velocities across the three initial distances given a shortest crossing time of 2 s and adding 17.5%. of the first gap to each subsequent gap.

walking speed in the VE was used to individuate the experiment, this means that the relative safety of the gaps between the vehicles would increase (e.g. the relative safety of a 2-s gap differs if it takes the participant 1.8 s to cross originally but only 1.5 s to cross by the end of the experiment).

The 30 experimental trials in the second session were divided into 10 blocks of 3 trials. For half of the blocks the participant was engaged in conversation with the experimenter (described earlier), and for the other half there was no conversation. Conversation trials were counterbalanced (see Appendix B for the counterbalanced design). A summary the types of trials used, the instructions associated with each trial type, and the order the trials were presented in is given in

Participants first read the information sheet and signed a consent form, then read the instruction sheet and filled in the pre-test SSQ (see Appendix C for the information sheet, instruction sheet,

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Table 4. A summary of the types of trials used, the instructions given for the trials, and their order of presentation for Experiment 1.

Trial Number(s)	Description of the trial(s)
1	The participant was asked to cross the laboratory room at a normal walking speed (in the actual environment)
2	The participant was asked to cross the laboratory room as if they were in a rush (in the actual environment)
3-5	The participant was asked to walk towards the street light at a normal walking speed in the first 3 virtual environment individuation trials
6-8	The participant was asked to walk towards the street light as if they were in a rush in the second 3 virtual environment individuation trials
9-38	In the remaining virtual environment familiarisation trials the participant was asked to cross the road when they felt it was safe. For the experimental trials the task was the same but a conversation distraction task was either present or absent for each trial.

and consent form, and Appendix D for the SSQ). Before the individuation trials commenced the participants were positioned on a red strip of tape on the floor. This strip could be used by the participants to reposition themselves at the beginning of each trial[14]. They were instructed to walk towards the street light then turn around and return to the tree (see Figure 1). At the end of each trial a black screen with white text instructed them to prepare for the next trial. For each session they first completed the 8 individuation trials followed immediately by the 30 experimental trials. After the first session was completed they were given the option of having a short break.

[14] Although the participants were wearing the HMD there was generally a small gap through which they could see the ground. This was not considered to be a serious problem; to complete the task successfully the participants had to focus their attention on the VE which precluded attending to the floor while crossing.

As described earlier, for the second session the participants were engaged in the conversation task for half of the experimental trials. Most of the participants responded with the basic information (i.e. “I have done the activity 4 times, I would do it again, and I think it is 5 out of 10 on the risk scale”), but a few went into greater details about their activities and willingness, or lack of willingness, to do the activity. In general only one activity was given per trial, but on occasions when participants crossed late in a trial two or three activities were used. Even less frequent was one activity being used for more than one trial, and this only occurred when the participant went into greater depth about the activity. Any activities not provided during the experiment were completed by the participant following the experimental trials, as was the general information section of the questionnaire (the demographics on the first page and general questions on the last page).

Following the second session participants were asked to fill in the post-test SSQ and completed the ARTQ.

Results

As Session 1 was only intended to give the participants’ experience moving in the VE, data from that session was not analysed further. The only exception to this is the walking speed comparison between the individuation trials for Sessions 1 and 2. All post-hoc analyses were conducted using the Tukey Honest Significant Difference test (all reported differences post hoc were significant at $p < .05$).

Before the analyses were conducted each participant’s data was averaged for each cell (e.g. no-conversation, 40-m initial distance; or conversation, 60-m initial distance). Depending on the amount of missing data for each participant this meant that each individual’s cell means were based on an average of 3, 4, or 5 data points. This had the effect of replacing the missing data with the each participant's mean for that particular cell. Replacing missing data this way helped retain individual variability.

ANALYSES

Eight 2-way (2 conversation condition by 3 initial distance) repeated measures analyses of variance (ANOVAs) were conducted. These analyses tested the hypotheses related to the effect of the simulated cellular phone conversation and the initial distance between the vehicles. There were no significant interactions across any of the variables. While the non-significant interactions are not reported in text, full ANOVA tables, including effect sizes[15], have been provided in Appendix E. Means, standard deviations, and ranges are included in Appendix F. An additional ANOVA conducted for walking speed will be described in the walking speed section. Finally, to examine whether the participants' gap choices were affected by the T_A of the gaps, trend analyses were conducted on the ratio data for each conversation condition across the 10 levels of T_A (see page 92 for specific details on this ratio).

Assumptions

The assumptions for the ANOVAs generally held. As the experiment used a completely within-participant design there was no need to test the homogeneity of variance assumption, as this only applies to between-groups analyses. The assumption of sphericity was not always met, but none of the significant results became non-significant after correcting for the increase in Type 1 error by using degrees of freedom adjustments (e.g. Greenhouse-Geisser or Huynh-Feldt corrections).

[15] The effect size used was Cohen's f (Cohen, 1988) and was calculated using the formula $f = (F/N)^{0.5}$.

The assumption of normality held for the margin of safety, walking speed, the percentage of gap available to use, and total time available to cross. However, there were violations of this assumption for near misses, collisions, safe gaps left, and cautious crossings. This is most likely due to the nature of these variables; with the exception of safe gaps left they indexed infrequent events, while for safe gaps left there was a general ceiling effect (i.e. on average participants crossed in the second or third gap, meaning that there could only be one or two safe gaps before the one chosen).

There are two reasons why these violations are not seen as problematic. The first is that ANOVAs are robust to violations of the normality assumption when sample sizes are equal. The second is that the patterns of the violations were consistent across cells, so there were no cases where the data was positively skewed for some cells and negatively skewed for others. Overall, then, the data seems suitable for the following analyses.

Margin of Safety

The margin of safety was higher in the no-conversation condition than in the conversation condition (means of 73.54% and 64.45% respectively), $F(1, 49) = 12.11$, $p < .01$. This suggests that conversation had an overall negative impact on participants' road crossing safety. There was also a main effect of distance, $F(2, 98) = 43.90$, $p < .001$. Post-hoc analyses indicated that all 3 distances differed significantly, suggesting that as the initial distance increased the safety of the crossing decreased (see Figure 3). Possible reasons for these decreases in safety will be discussed shortly.

Near Misses

Near misses were more prevalent when participants were engaged in conversation compared to when they were not (means of 25.19% and 20.23% respectively), $F(1, 49) = 4.36$, $p < .05$. Greater initial distances were also associated with more near misses, $F(2, 98) = 4.26$, $p < .05$ (see Figure 4), consistent with the findings for the margin of safety. The only difference post-hoc was between the 40- and 60-m distances.

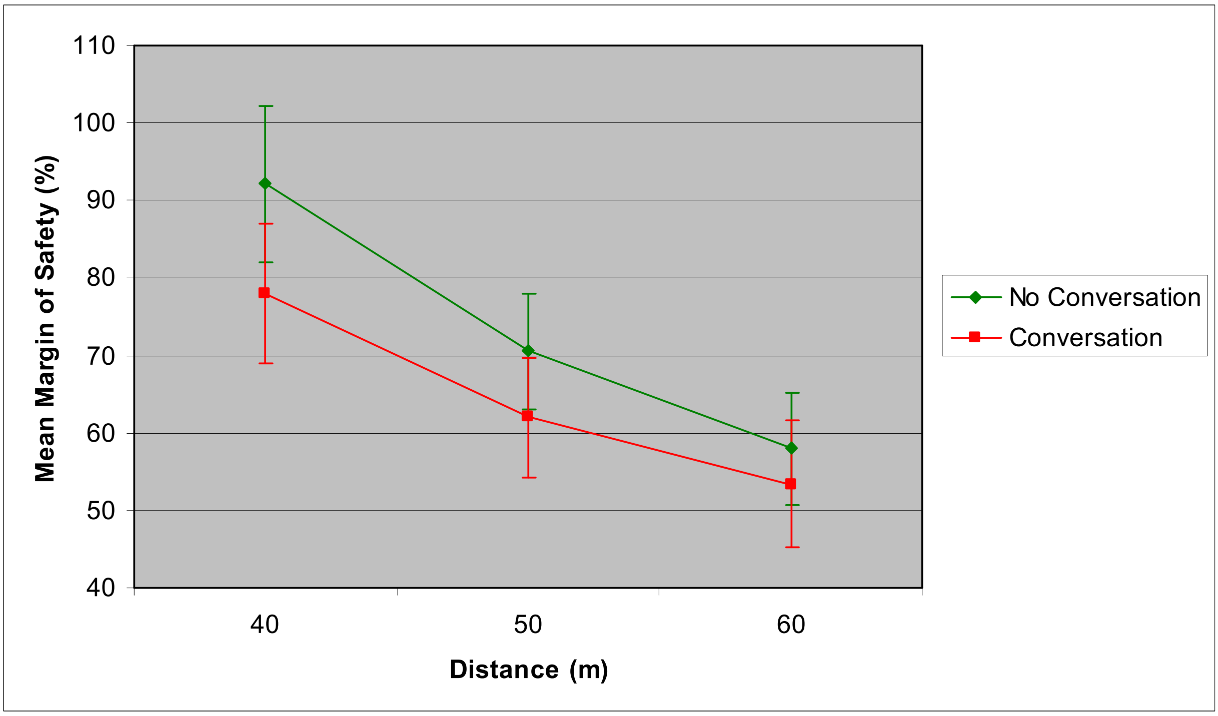

Figure 3. The main effects of Conversation and Distance on the margin of safety. The interaction effect was not significant. The bars around the means represent the 95% confidence intervals.

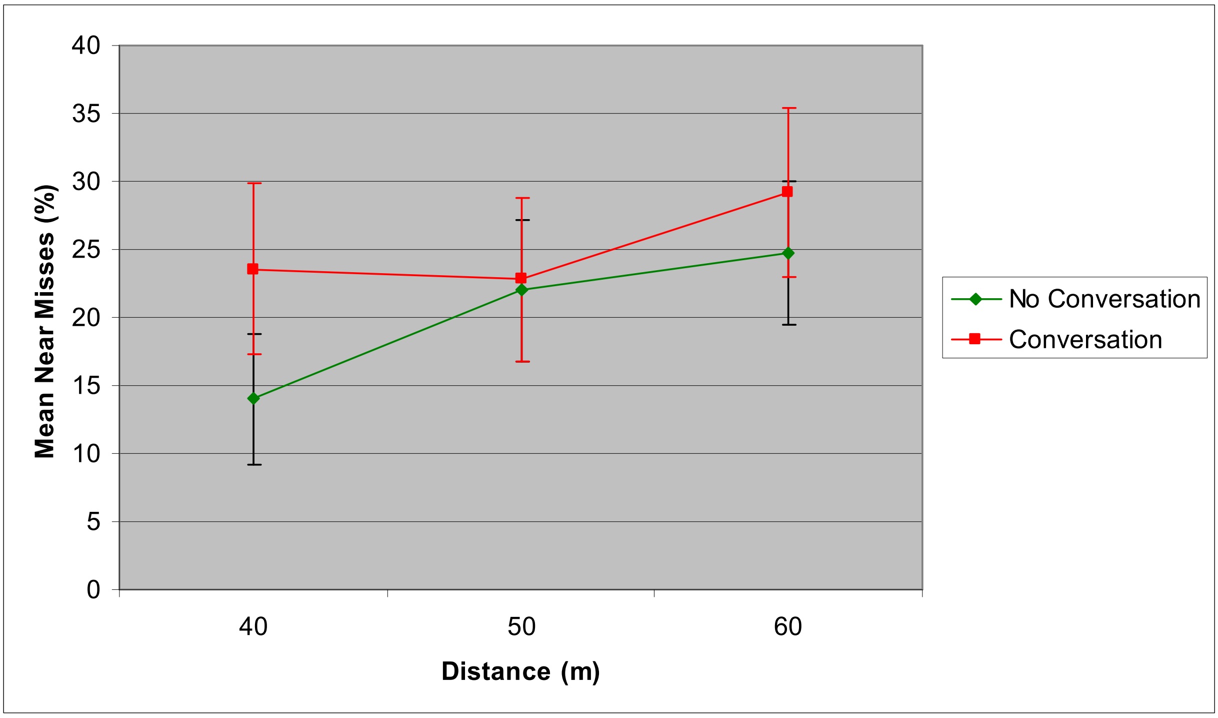

Figure 4. The main effects of Conversation and Distance on the mean frequency of near misses. The interaction effect was not significant. The bars around the means represent the 95% confidence intervals.

Collisions

Along with decreased margins of safety and an increased rate of near misses, there were more collisions when participants were talking (8.4% compared to 5.73% when not talking). This difference, however, only approached significance[16], $F(1, 49) = 3.92$, $p = .053$. This finding, combined with the finding for near misses, indicates that the gaps selected by the participants when conversing were less likely to afford safe crossing. Distance had a greater effect, $F(2, 98) = 15.21$, $p < .001$, with the percentage of collisions increasing as the initial distance increased (see Figure 5). There was no difference between the 50- and 60-m distances, but the 40-m distance differed significantly from both.

[16] See (Hauer, 1983) for a description of the issues relating to the assumption that a non-significant result is equivalent to no effect, and (Pollard, 1993) for a treatment of why it may be advisable to treat the .05 significance level as a soft, rather than hard, cut-off.

Walking Speed

There was a small but statistically significant decrease of 0.04 m/s in walking speed when the participants were engaged in conversation (1.97 m/s compared to 2.01 m/s in the no-conversation condition; $F(1, 49) = 6.66$, $p < .05$; see Figure 6). This is equates to the 3 m crossing taking an extra .03 s, which seems to be only a small increase. This may still contribute to the overall decrease in safety, especially when considered along side the following findings for the time available to cross and the percentage of gap available to use. The main effect of distance was not significant, $F(2, 98) = 2.7$, $p = .10$, although there was a tendency for longer initial distances to be associated with slower walking speeds.

As well as the main analysis, walking speeds were also compared between the individuation trials for Sessions 1 and 2 (the normal speed and rushing trials in both environments), and to the

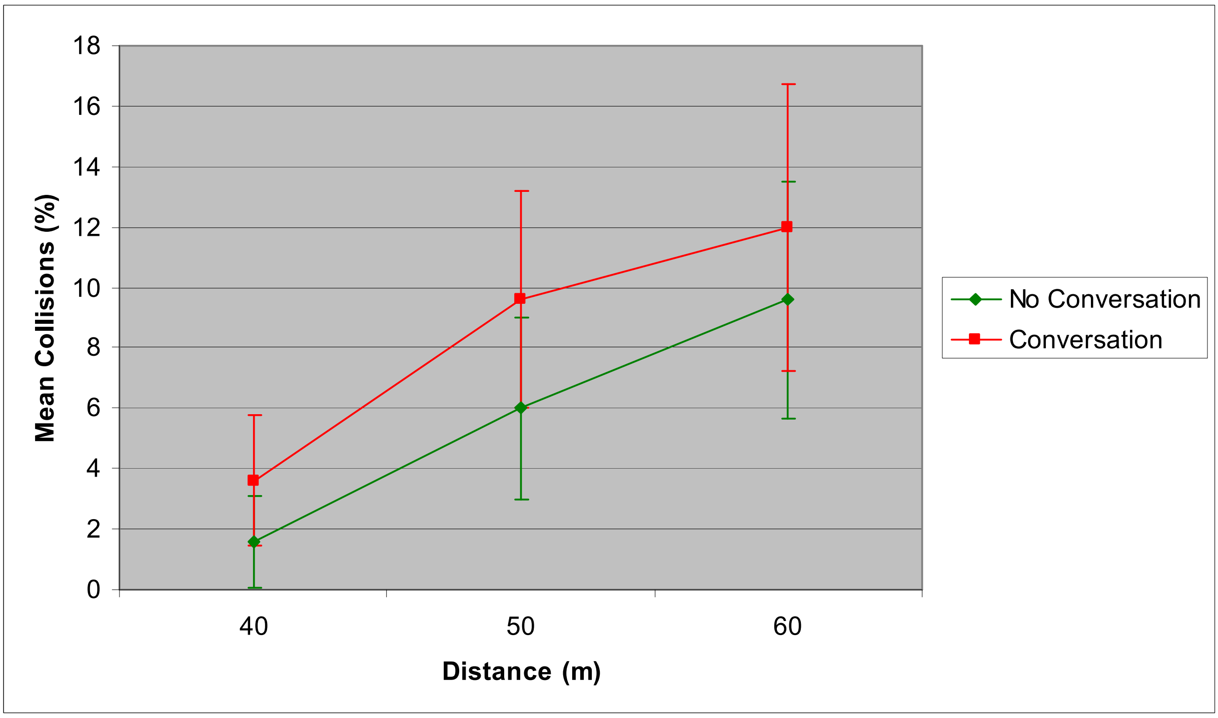

Figure 5. The main effects of Conversation and Distance on the mean frequency of collisions. The interaction effect was not significant. The bars around the means represent the 95% confidence intervals.

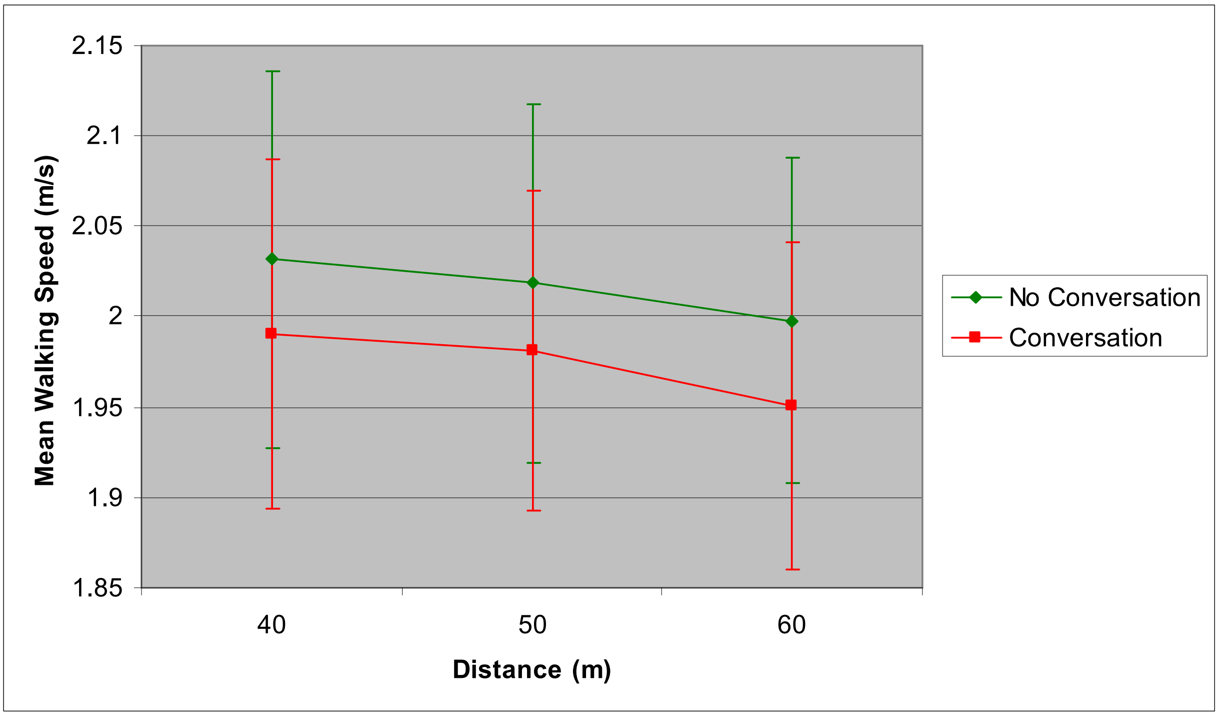

Figure 6. The main effects of Conversation and Distance on mean walking speeds. The interaction effect was not significant. The bars around the means represent the 95% confidence intervals.

maximum speed attained in the vehicle trials for each session. This was tested using a 2-way (2 sessions * 5 walking speed conditions) repeated-measures ANOVA. As predicted, participants walked faster in the second session, $F(1, 49) = 96.46$, $p < .001$, walking on average at 1.77 m/s across the 5 conditions for Session 1 and 2.01 m/s across the 5 conditions for Session 2. There was also a main effect of condition, $F(4, 196) = 227.83$, $p < .001$, as well as an interaction between the two, $F(4, 196) = 3.78$, $P < .01$ (see Figure 7). Post-hoc analyses indicated that the means for each condition differed between the sessions. Within each session there were differences between means for all of the conditions, with three exceptions: between actual walk and virtual walk for both Session 1 and Session 2, and between actual rush and virtual rush for Session 2.

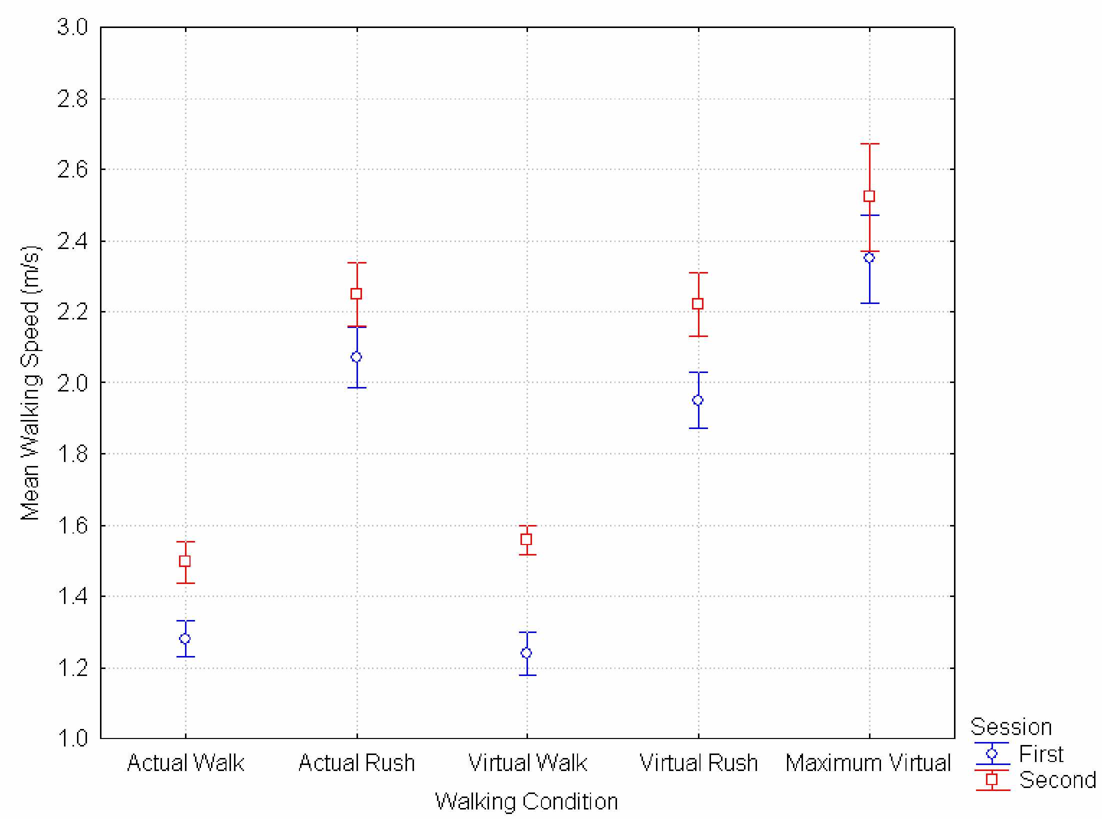

Figure 7. The main effects of Walking Condition and Session, and the interaction between the two, on mean walking speeds. The first four conditions refer to the individuation trials while the fifth (Maximum Virtual) is based on the maximum speed reached by each participant in the experimental trials. The bars around the means represent the 95% confidence intervals.

Total Time Available to Cross

There was a significant difference between conversation conditions for the temporal size of the gaps the participants chose, $F(1, 49) = 8.16$, $p < .01$, with participants choosing shorter gaps when they were talking (2.33 s, compared to 2.43 when not talking). Shorter gaps were also chosen when the distance between vans increased, $F(2, 98) = 3.75$, $p < .05$ (see Figure 8), although the only post-hoc difference was between the 40- and 60-m distances.

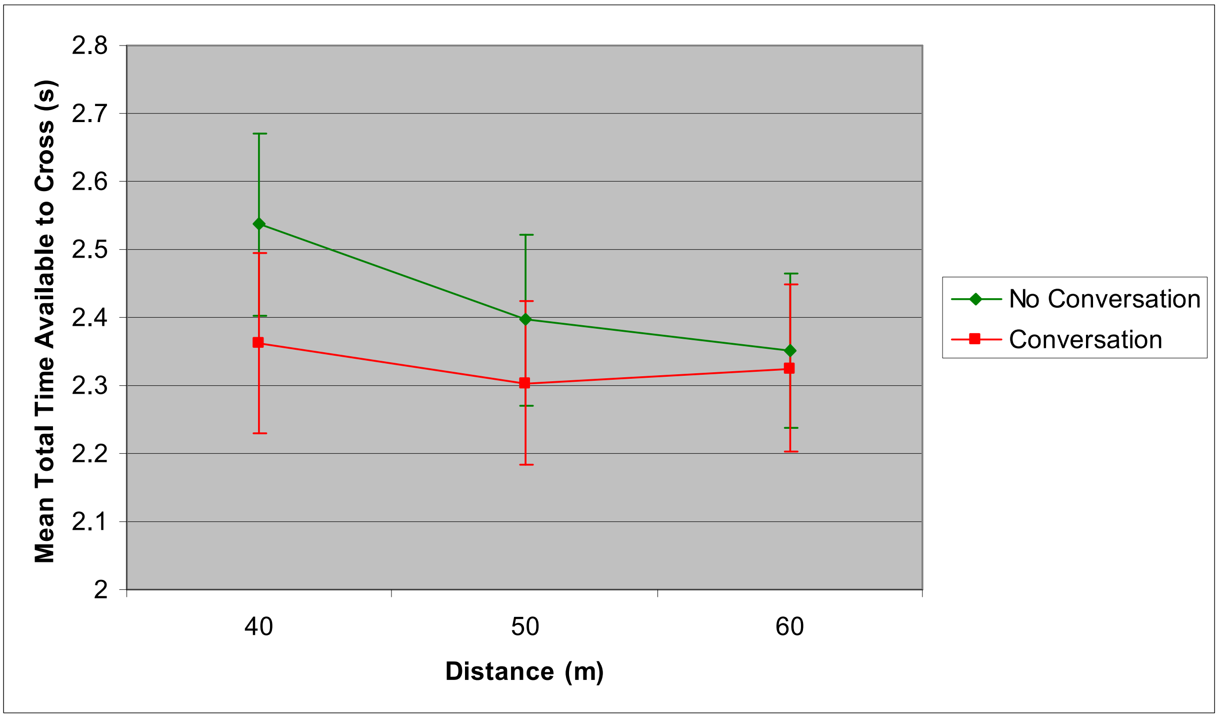

Figure 8. The main effects of Conversation and Distance on the mean total time available to cross. The interaction effect was not significant. The bars around the means represent the 95% confidence intervals.

Percentage of Gap Available to Use

There was a small difference in the percentage of gap available to use between conditions, $F(1, 49) = 5.27$, $p < .05$, with participants using 73.7% of the gap when talking and 75.1% when not. This is only a small decrease, but this finding, combined with the reduced walking speeds and shorter gaps chosen, would seem to account for the overall reduction in safety. Distance was again significant, $F(2, 98) = 88.08$, $p<.001$, with less of the gap being used as distance increased (see Figure 9). All of the distances differed significantly.

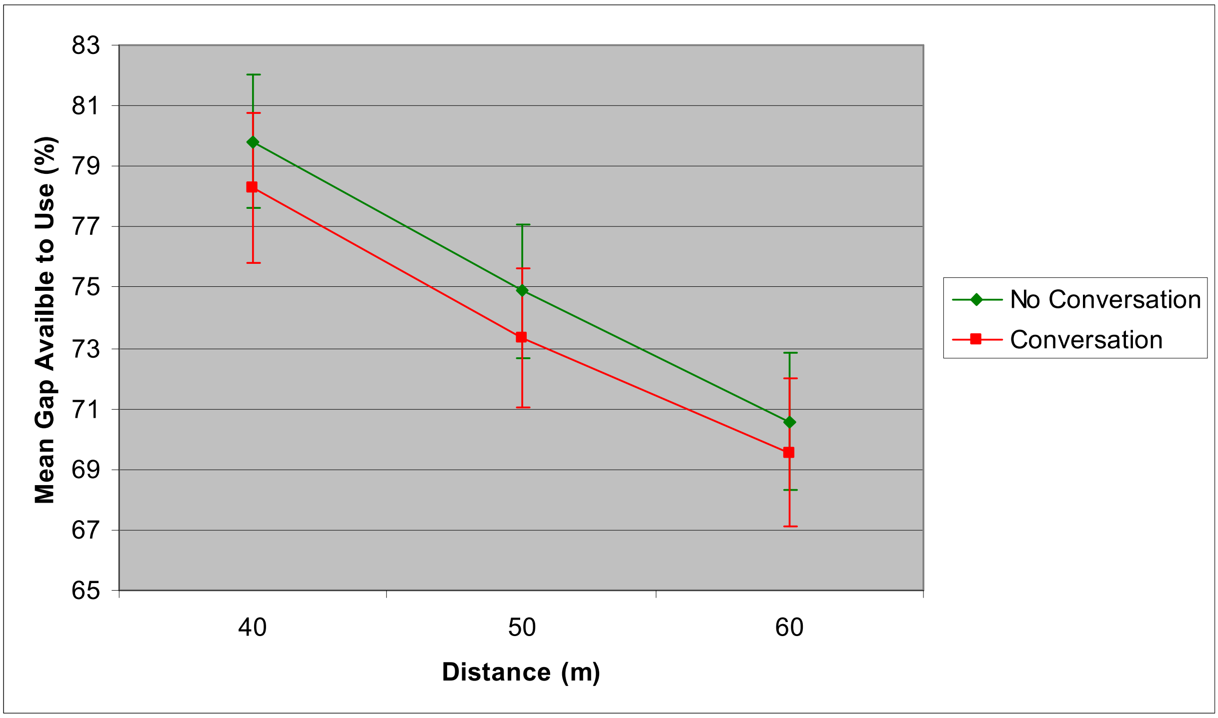

Figure 9. The main effects of Conversation and Distance on the mean percentage of gap available to use. The interaction was not significant. The bars around the means represent the 95% confidence intervals.

Safe Gaps Left

Conversation also affected how many safe gaps were left, with an average of 1.48 safe gaps being left in the conversation condition compared to 1.13 safe gaps in the no-conversation condition, $F(1, 49) = 14.69$, $p < .001$. Fewer safe gaps were left when the initial distance was larger, $F(2, 96) = 39.89$, $p < .001$, again suggesting that the participants perceived that an increased distance between the vans represented an increase in safety (see Figure 10). All of the distances were significantly different from each other.

Cautious Crossings

Cell phone conversation did not appear to affect the number of cautious crossings that occurred, $F(1, 54) = 0.0003$, $p = .99$, with approximately 6% of the trials in each conversation condition being cautious crossings. Distance, however, did affect cautious crossings, $F(2, 108) = 8.76$, $p < .001$ (see Figure 11). Cautious crossings were more frequent at short initial distances. Post-

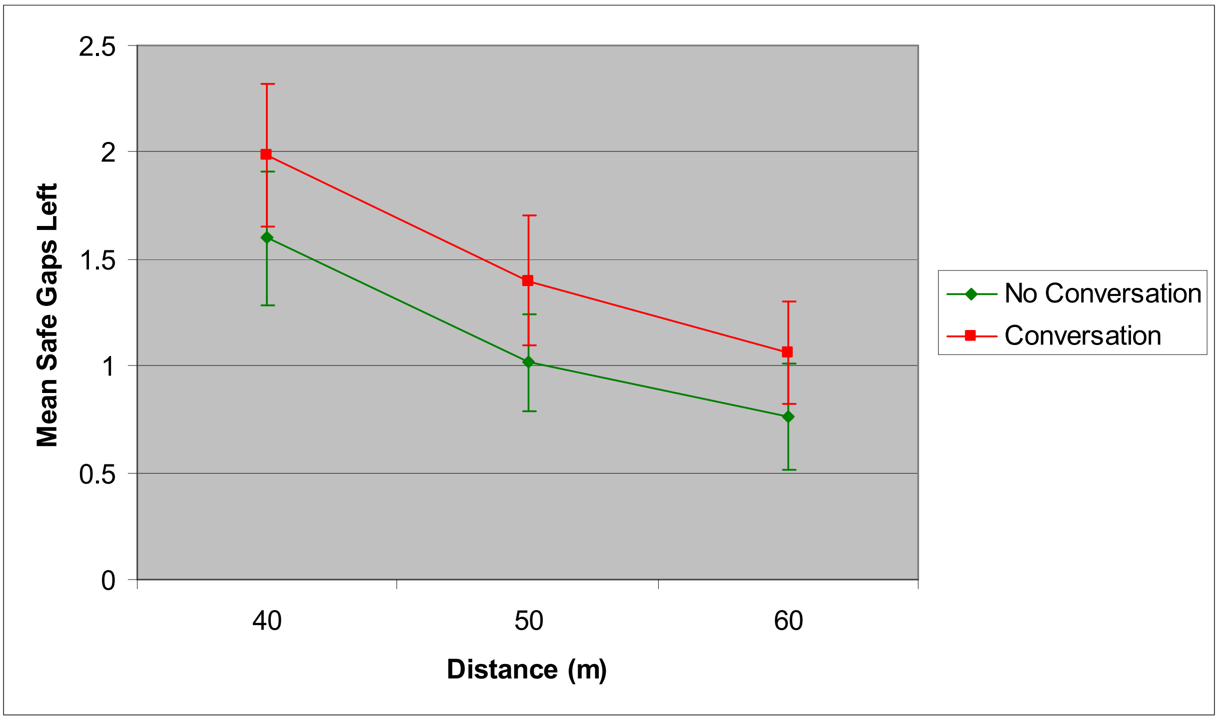

Figure 10. The main effects of Conversation and Distance on the mean number of safe gaps left. The interaction effect was not significant. The bars around the means represent the 95% confidence intervals.

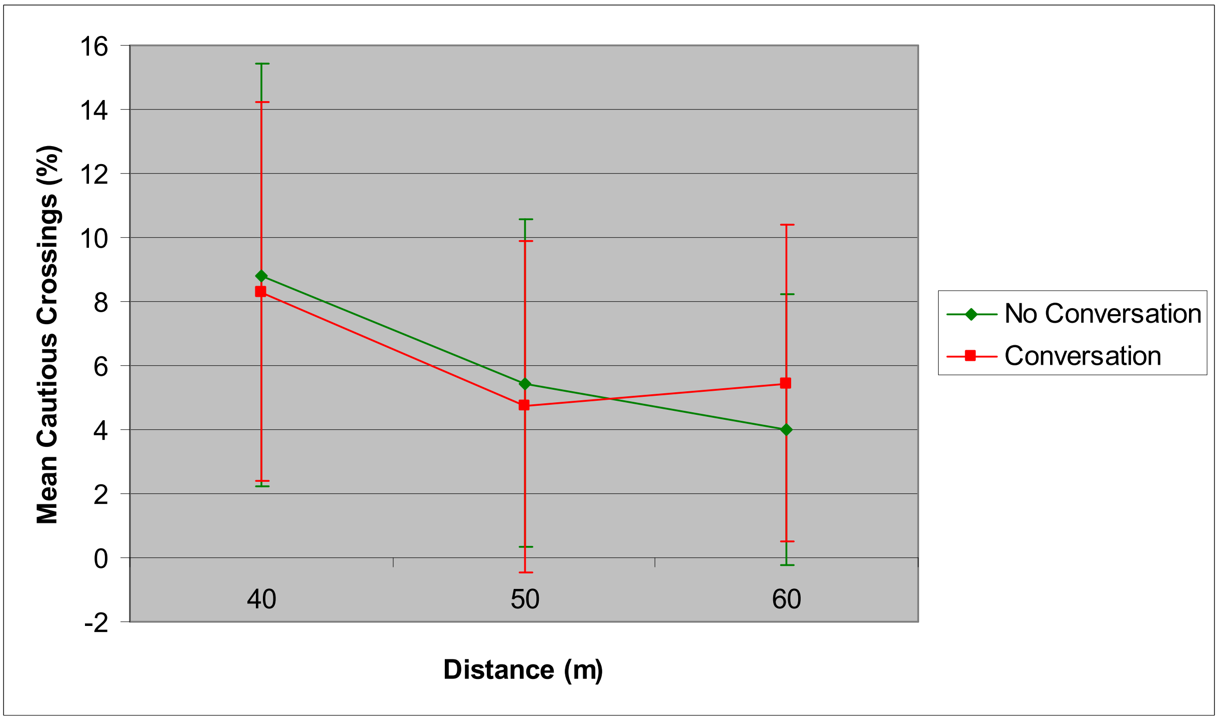

Figure 11. The main effects of Conversation and Distance on the mean frequency of cautious crossings. The main effect of conversation and the interaction were not significant. The bars around the means represent the 95% confidence intervals.

hoc testing indicated that while the 40-m initial distance differed from both the 50- and 60-m distances, the latter two did not differ.

Across the experiment 41 participants did not have any cautious crossings for any condition, meaning that only 9 of the retained participants did. To examine if those participants who had at least one cautious crossing performed differently to those that had none a series of t-tests were conducted. The data for the other dependent variables were averaged across distances within each conversation condition, meaning that 14 tests were conducted. Of these, only two were significant, both for the number of safe gaps left. Participants who had at least one cautious crossing let more safe gaps pass in both the no-conversation condition, $t\ (48) = 4.66$, $p < .001$, and in the conversation condition, $t\ (48) = 3.75$, $p < .001$. This was not surprising, as this is the other measure of caution. Overall, though, the two groups did not differ and hence can be considered equivalent for the majority of the analyses.

Time-to-Arrival

A visual examination of Figure 12 indicates that the participants were tending to prefer the longer gaps over the shorter gaps, but only in the no-conversation condition (compare the top and bottom panels of the figure). The linear contrast was not significant for the no-conversation condition, $F(1, 49) = 2.31$, $p = .14$, or for the conversation condition, $F(1, 49) = .14$, $p=.71$ (see the straight lines in Figure 12), which may indicate that participants were not favouring the longer gaps in either condition. However, the cubic contrast was significant for the no-conversation condition, $F(1, 49) = 4.54$, $p < .05$. Only the 5th order contrast was significant for the conversation condition, $F(1, 49) = 8.25$, $p < .01$. Examining Figure 12 it can be determined that the four longest gaps all had likelihoods over 1 for the no-conversation condition, while the remaining six gaps all had likelihoods less than 1. In contrast, for the conversation condition two of the first five gaps had likelihoods over 1 and three of the second five had likelihoods under five. There is, then, a definite trend towards longer gaps being favoured in the no-conversation condition but not in the conversation condition.

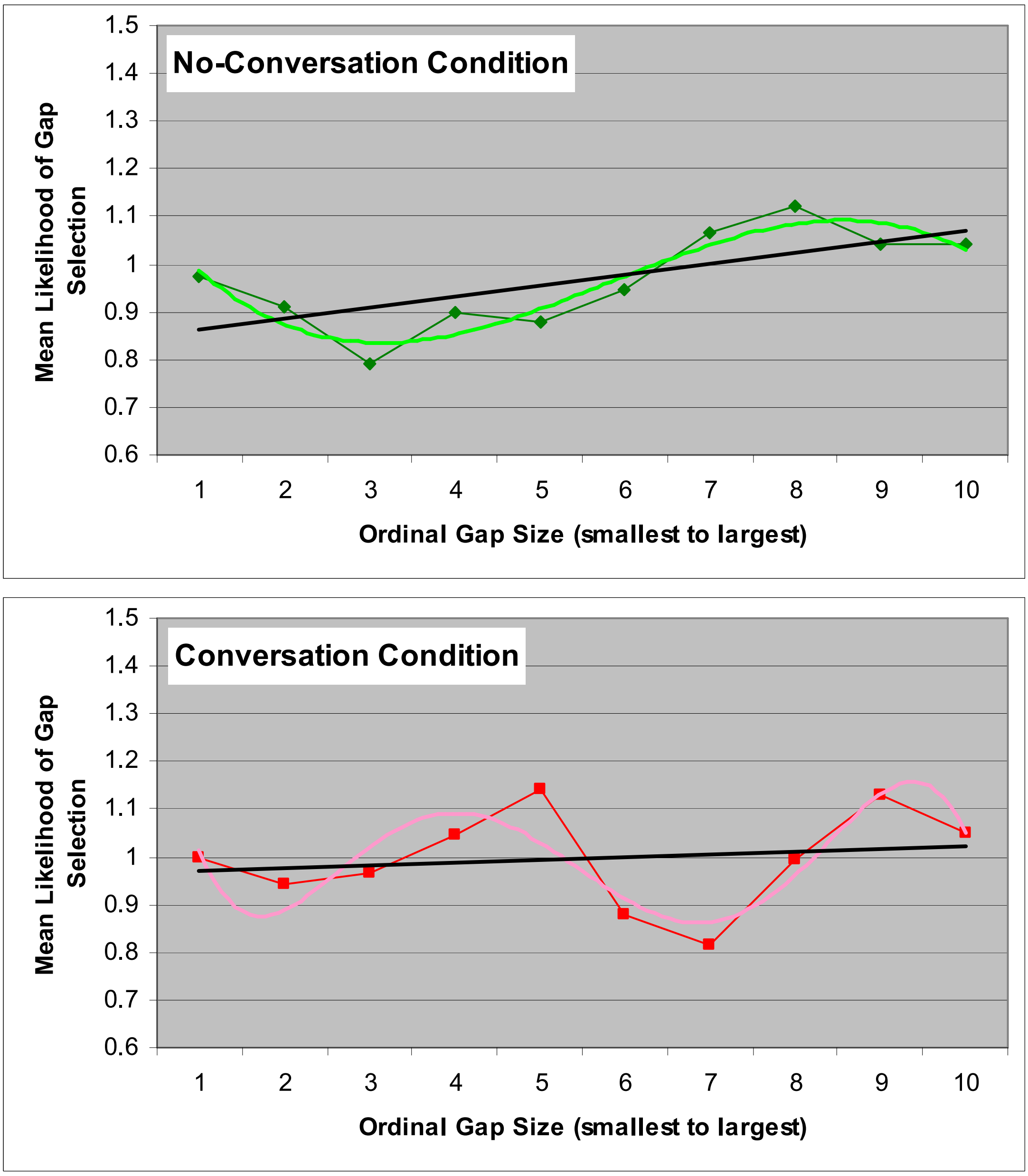

Figure 12. The effect of ordinal gap size on the likelihood that a gap is selected for the no-conversation condition and the conversation condition. The straight lines are a linear fit to the data, while the smoothed curved lines are the lines of best fit for each condition. Values above 1 indicate that the gap is picked more often than would be expected, and values less than 1 indicate the gap is picked less often than would be expected.

Summary

Overall the results suggest that the participant's road-crossing ability was impaired by the conversation task. Safety, as measured by the margin of safety, was reduced by conversation.

Collisions and near misses, two coarser but more critical measures of safety, occurred more frequently when the participants were engaged in the conversation task. Gap selection, then, was poorer in the conversation condition as the gaps selected were less likely to afford a safe crossing. This decrease in safety may be related to decreases in walking speeds, smaller temporal gaps being chosen, and less of the gap being used for each crossing in the presence of the conversation task.

Participants allowed more safe gaps to pass when they were talking, indicating a general level of distraction. However, there was no increase in cautious crossings. This suggests that participants, although exhibiting impaired performance while talking, were generally still willing to cross the road rather than to wait for all the vans to pass.

The initial distance between the vehicles affected participant behaviours. As the initial distance between the vehicles increased there was a decrease in safety, indicated by a reduction in the margin of safety and more frequent collisions and near misses. Participants chose smaller temporal gaps at longer initial distances, and used a smaller percentage of the gap they chose. Fewer safe gaps were left and fewer cautious crossings were made when the vans were further away. Participants tended to walk slower as the initial distance increased, but this was not statistically significant. Participants favoured the longer gaps in the no-conversation condition, and did not in the conversation condition, suggesting that the conversation task also impaired their ability to use T_A to inform their crossing decisions.

CHAPTER 7: EXPERIMENT 2

Introduction

Amongst the older population cell phone use is becoming more common (Pöysti et al., 2005). A question that needs to be examined, then, is whether dual-task impairment is greater for older people compared to younger. This section outlines some of the age-based changes relevant to road-crossing and cell phone use. Following this is a brief review of some of the literature relevant to the above question.

Age-Related Changes in Task Performance

For road-crossing, one important change related to aging is that older adults tend to walk slower (Ketcham & Stelmach, 2001). This means they need to select temporally larger gaps to ensure safe road-crossings. Other motor control decrements noted in older adults are reduced RTs, and longer movement times in tasks such as grasping and pointing (Ketcham & Stelmach, 2001).

For the current research, it is important to note that older adults perform worse in postural stability tasks than younger adults when given no visual feedback, and maintaining balance requires greater attentional resources (Ketcham & Stelmach, 2001). Older adults are also at a greater risk of loss of balance if they are required to divide their attention (Ketcham & Stelmach, 2001), another finding pertinent to this research. Given that the participants are unable to see themselves in the VE, and two tasks must be performed in addition to walking (choosing a gap to cross in and the conversation task), this may be a problem. Walking has also increased RTs to various stimuli for older adults compared to younger adults, suggesting that walking required a greater level of attentional resources for the older group (Sparrow, Bradshaw, Lamoureux, & Tirosh, 2002).

An observational study examining whether the road-crossing behaviours of older differed from those of younger pedestrians found that older pedestrians tended to wait longer before crossing, to delay for longer at the curb, to take longer to cross the road, and to make more head movements before and while crossing (Wilson & Grayson, 1980). The tendency for older people to wait for longer at the curb has been noted elsewhere (Oxley et al., 1997), but in this case the differences were quite small, suggesting that older pedestrians did not form a distinct sub-group within the general pedestrian population (Wilson & Grayson, 1980).

In a review of the visual function literature, Johnson and Choy (1987) determined that visual function remains fairly constant up until around the age of 50 when it begins to decline, although the actual age of decline may vary between 45 and 60 years of age. Older adults may be impaired in their ability to detect impending collisions (Andersen, Cisneros, Saidpour, & Atchley, 2000). They also exhibit different tendencies to younger adults when it comes to estimating velocities; they overestimate velocity at lowers speeds, and underestimate it at higher speeds (Scialfa, Guzy, Leibowitz, Garvey, & Tyrrell, 1991). For Schiff, Oldak, and Shah (1992) older women made the most conservative (i.e. underestimated) judgments of T_A, although all participants tended to be conservative in their judgments.

There may also be an increase in performance variability for older people. This has been noted in general movement control, such as movement velocity and duration (Ketcham & Stelmach, 2001), performance on a tracking task (Naveh-Benjamin, Craik, Guez, & Kreuger, 2005), as well as in driving behaviours such as speed variability (Shinar et al., 2005). In relation to variability in choice RTs, Alm and Nilsson (1995) note that even if RTs for people over 60 tend to be slower than RTs for younger adults, they will not necessarily be slower for a given older individual.

Divided Attention and Aging

Whether older adults are more impaired by dividing their attention than younger adults is unclear. Although some research supports this hypothesis (Brouwer, Waterink, Van Wolffelaar, & Rothengatter, 1991; McDowd, 1986; McDowd & Craik, 1988; Monk et al., 2004; Naveh-Benjamin et al., 2005), this is not the only finding. Other studies have found no difference between older and younger in terms of magnitude of impairment (Fernandes & Moscovitch, 2003; Salthouse, Fristoe, Lineweaver, & Coon, 1995), or that older adults adopt different strategies to cope with the additional attentional demands (Kemper, Herman, & Lian, 2003). (Also see McDowd and Birren (1990) for a short review of this area, mentioning this uncertainty, and Verhaeghen and Cerella (2002) for a review of meta-analyses suggesting that older adults are more affected by dual-task distraction.)

These mixed findings are repeated for cell phone conversation studies. In an epidemiological study, Lam (2002) found no increased relative risk for injuries or deaths related to hand-held cell phone use except for the 25-29 year-old age group. Most age groups also had similar relative risks for death or injury in regards to general in-car distractions, although those above 70 had an increased risk and those between 40 and 49 had a decreased risk. A number of other studies have found age-related differences indicating worse performance in older adults, but with no interactions with the distracter variables, for various driving tasks (Alm & Nilsson, 1995; Horberry et al., 2006; Strayer & Drews, 2004) and a visual scanning task (McCarley et al., 2001). This indicates that, for these experiments, while the older participants had worse overall task performance they were not more impaired by the distraction task than the younger participants. In other studies the interaction has also been significant, such as for responding to driving situations (McKnight & McKnight, 1993), people over 60 being more impaired by a driving task than those between 20 and 30 (Reed & Green, 1999), and older women being less aware that their driving has been impaired by a conversation task (Lesch & Hancock, 2004).

The age ranges used for the older groups have varied quite considerably, the youngest starting point being 50 years old (McKnight & McKnight, 1993), while others have only used people over 70 years old (Kemper et al., 2003). For the other studies the initial age for the older group tends to fall between these values, with the mean age generally being between 60 and 70 (e.g. Horberry et al., 2006; McCarley et al., 2001; Monk et al., 2004). Most of the participants in the younger groups tend to be in their 20s, although there are some studies where some participants are in their 30s (Reed & Green, 1999; Sparrow et al., 2002), or where the mean age is around 30 (Lesch & Hancock, 2004). In terms of comparisons, the most extreme age difference was for Strayer and Drews (2004), with mean ages of approximately 20 years for the young group and 70 years for the older. The smallest difference was for Alm and Nilsson (1995), who divided their participants into 2 groups; those above and those below 60 years of age.

Hypotheses

Cellular Phone Conversation

1. Crossing safety will be reduced by the simulated cell phone conversation, with a decrease in the margin of safety and an increase in near misses and collisions when the participants are engaged in conversation.
2. Participant performance will be impaired by conversation, with walking speeds being reduced, smaller gaps being chosen, and less of each gap being used when compared to no-conversation
3. Participants will become more cautious when conversing, with the participants allowing more potentially safe gaps to pass when conversing compared to no-conversation.

Age Differences

Compared to younger participants, older participants;

1. will have lower margins of safety, and more near misses and collisions;
2. will walk slower, chose smaller gaps, and use less of each gap;
3. will be more cautious, exhibited by allowing more safe gaps to pass on each trial and having more cautious crossings.
4. will have greater variability in their performance
5. may also be more impaired than the younger participants by the conversation task, and if so this will be shown in the interaction term.

Information Used to Inform Gap Choices

1. Safety will reduce as the distance between the vans increases, with longer initial distances being associated with lower margins of safety and an increase in near misses and collisions.
2. Participants will chose smaller gaps and use less of each gap as the distance between the vans increases; however, walking speed will not be affected by distance.
3. Participants will be more cautious, allowing more safe gaps to pass and having more cautious crossings, when the initial distance between the vans is shorter.
4. Participants may be more likely to select larger gaps to cross in, but only when not engaged in the simulated conversation task.

Postural Stability

1. Postural stability may be negatively affected by VE exposure

2. If postural stability is negatively affected by VE exposure the effect will be greater for the older participants

Method

Participants

Forty participants were tested, 20 younger and 20 older adults. There were 13 younger and 12 older females, and 6 younger and 8 older males. The younger adults' ages ranged between 18 and 27 (a mean of 21.58 years, SD = 2.22), while the older adults' ages ranged from 50 to 67 (a mean of 55.45 years, SD = 5.13). One of the younger participants did not record their sex or age. All of the younger participants were students at the University of Canterbury. The older participants consisted of students or staff from the Department of Psychology at the University of Canterbury, or were friends of the experimenter. None of the participants had any mobility issues, and all but one of the participants reported having normal or corrected normal vision. One of the older participants was blind in one eye, but this was not seen as a problem as this was how they interacted with the world in general. While most of the participants reported no hearing impairments there was one exception. An older participant reported that they were deaf in one ear, but as this was not the ear to which sounds were presented this was not a cause for concern. Three of the participants (two younger and one older) reported never having driven a car or a motorbike.

As with Experiment 1 some participants needed to be excluded due to insufficient data. The criterion was loosened for this experiment as retaining the criterion used for Experiment 1 would have resulted in the loss of too many participants. Participants were excluded if they were missing data from more than 2 trials per cell (i.e. only 1 or 2 usable trials), but only if this was the case for two or more cells. One cell could have only 2 usable trials, but the remainder had to have 3 or more. After excluding those who did not meet this criterion 19 younger adults (12 female, 6 male, and 1 unknown) and 16 older adults (11 female and 5 male) remained. The age range for the younger sample remained unchanged (a new mean of 21.5, SD = 2.26) while the range for the older sample changed to 50-64 (a new mean of 54.5, SD = 4.16). The full sample was used for the analysis of cautious crossings. Participants were given a $10 petrol voucher for their participation.

Materials and Apparatus

These were essentially identical to Experiment 1. The only changes were that the pre-recorded instructions were recorded in the experimenter's voice and the initial instruction was changed to "Please get ready to cross". These changes were made due to an issue that was discovered with the pilot study for Experiment 3 and are explained fully on page 135. The end-of-trial instruction was the same as Experiment 1, "Turn to your right and walk back to the tree", but was also recorded in the experimenter's voice.

The Simulated Cellular Phone Conversation

The simulated conversation was identical to Experiment 1

Independent Variables

The within-group variables of *conversation* condition and *initial distance* were used (see Experiment 1, page 84), as was the between-group variable *age group* (younger and older).

Dependent Variables

The dependent variables were identical to Experiment 1 (see Table 3. Dependent variables for Experiment 1.on page 37), with one addition. *Postural stability* was added for this experiment. This was measured twice, once before the experiment commenced and once immediately after the last experimental trial. Measurements were taken using the HMD in the left-right, up-down, and forwards-backwards axes. Standard deviations were taken over the first 1.4 s[17] of measurement for each axis, and were pooled[18] across the three trials. This was to obtain a measure of the variability in postural stability. It should be noted, however, that this is not a standard way to measure postural stability, and was chosen as it was a convenient method that could be easily used before and after the experimental trials.

Procedure

Rather than using a two-session design as per Experiment 1, the practice and experimental trials were conducted as one session. Preceding the individuation trials were 3 postural stability trials. The initial 8 individuation trials, used to configure the van T_As, were identical to Experiment 1. Following these trials were 45 experimental trials. The first 15 of these were familiarisation

[17] The original intent was to use the first 2 s of each trial, but unfortunately the data for some participants did not allow this. In some cases the recording of the data began up to .7 seconds after the trial was started, meaning 2 s worth of data was not collected. It was decided to keep the length of measurement and the period of measurement (i.e. the beginning of each stability trial) as consistent as possible across participants, 1.4 s being the minimum usable amount of data collected.

[18] The SDs were converted to variances before pooling, and then converted back to SDs for analysis.

trials, comparable to the first block of 30 trials from Experiment 1 (see below for an explanation of why 15, rather than 30, practice trials were used). The remaining 30 trials were experimental, and followed the same format as the experimental trials from Experiment 1. A summary of the types of trials, and the instructions to the participants, is given in Table 5. A summary of the types of trials used, the instructions given for the trials, and their order of presentation for Experiment 2.

The design was modified for a few reasons that related to the older sample. There was the potential risk that older participants would be more cautious than their younger counterparts. This assumption was tentatively supported by a small pilot study consisting of a 67–year-old female and a 69-year-old male. Given that there was a reasonable rate of cautious crossings in the Experiment 1, and as time and resources were limited, it was decided that the difficulty level of the experiment should be reduced to ensure fewer cautious participants (younger and older). One way of doing this was to remove the re-individuation of the experiment after the practice trials. As noted in the design for Experiment 1, participant's walking speeds in the VE tend to increase throughout the experiment, resulting in a change in the relative safety of each gap. This was also supported by the second walking speed analysis (see page 98). By not recalibrating the experiment to the participant's walking speed partway through, the experimental gaps would be effectively safer overall than the practice gaps.

The reduction in trial numbers was to reduce the total time of the experiment. As older people tend to walk slower (Ketcham & Stelmach, 2001) it was thought that there would be a risk of the total time of the experiment exceeding an hour. An examination of the data for the first session of Experiment 1 indicated that mean walking speeds seemed to asymptote after about 15 trials, so this was chosen as the number of practice trials. The number of experimental trials was not changed.

Table 5. A summary of the types of trials used, the instructions given for the trials, and their order of presentation for Experiment 2.

Trial Number(s)	Description of the trial(s)
1-3	For the 3 pre-experiment stability trials, the participant was asked to stand as still as possible with the HMD covering their eyes.
4	The participant was asked to cross the laboratory room at a normal walking speed (in the actual environment),
5	The participant was asked to cross the laboratory room as if they were in a rush (in the actual environment),
6-8	The participant was asked to walk towards the street light at a normal walking speed in the first 3 virtual environment individuation trials,
9-11	The participant was asked to walk towards the street light as if they were in a rush in the last 3 virtual environment individuation trials,
12-26	For the familiarisation trials, the participant was asked to cross the road when they thought it was safe.
27-56	In the remaining virtual environment experimental trials, the participant was asked to cross the road when they felt it was safe. Conversation was either present or absent for each trial.
57-59	For the 3 post-experiment stability trials, the participant was asked to stand as still as possible with the HMD covering their eyes.

After reading the instruction sheet (see Appendix G) and the individuation trials, but before beginning the familiarisation trials, the participants were informed that there would be at least 1 or 2 safe gaps each trial. The reason for this was to attempt to reduce the number of cautious crossings that occurred for both groups, hopefully without seriously affecting the participants' road crossing behaviours. After the 15 familiarisation trials participants were asked if they would like a short break. The 30 experimental trials were then conducted in an identical manner to Experiment 1 (see page 91). Following the post-experiment postural stability measurement the participants completed the ARTQ and the SSQ, as per Experiment 1.

Results

The hypothesis that there would be greater variability of performance in the older group was tested using the Levene's test for homogeneity of variance. This hypothesis will be addressed following the ANOVAs. Post-hoc analyses were conducted as per Experiment 1 (see page 93). The results from the postural stability study will conclude the main results section.

ANOVAs

Eight 3–way (2 age group by 2 conversation condition by 3 initial distance) ANOVAs, with repeated measures on the last two factors, were conducted. These analyses tested the hypotheses for age-related differences in performance and impairment, the effect of the simulated conversation task on performance, and the effect on performance of the initial distance between vehicles. Only one of the interactions, for near misses, was significant and this is the only interaction reported in text. Full ANOVA tables, including effect sizes, are included in Appendix E. Means, standard deviations, and ranges are included in Appendix H. An additional walking speed analysis was also conducted, which will be detailed in the walking speed section. The examination of the participants' gap choices in relation to the temporal size of the gaps was conducted as per Experiment 1 (see page 85).

Assumptions

The same comments that were made about assumptions for Experiment 1 apply to this experiment. The only additional assumption is that of homogeneity of variance. This assumption was not always met, but as predictions were made regarding this assumption the violations will be discussed in a later section. Overall, however, the assumption was generally met and therefore the described analyses are deemed to be appropriate for the data.

Margin of Safety

There were only two statistically significant effects for the margin of safety, the main effects of condition and distance. Mean margins of safety were smaller when participants were conversing (65.80%) than when they were not (74.00%), $F(1, 33) = 8.2$, $p < .01$. These values are quite similar to those found in Experiment 1 (64.45% and 73.54% for conversing and not conversing respectively) suggesting that this is a robust effect. Greater initial distances generally produced smaller safety margins, $F(2, 66) = 28.92$, $p < .001$ (see Figure 13). Post-hoc testing indicated that the 40-m distance differed from the 50- and 60-m distances. However, the two greater initial distances were not significantly different. There was no significant difference between the age groups, $F(1, 33) = 0.95$, $p = .34$.

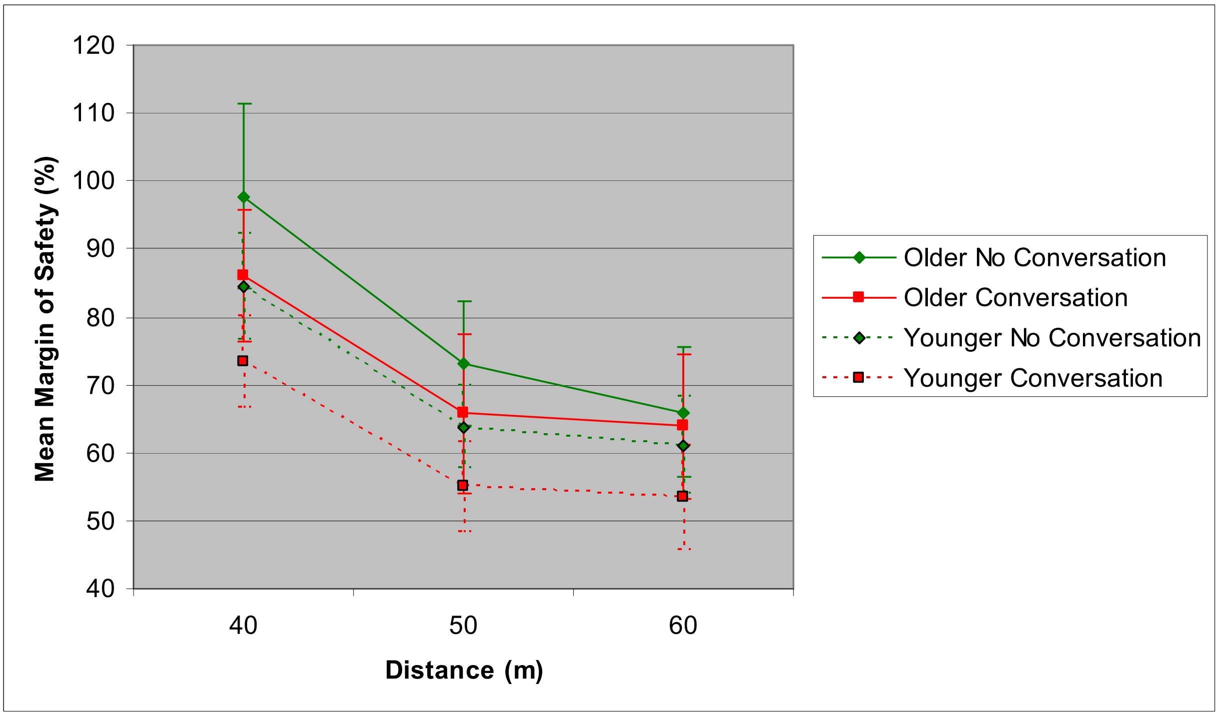

Figure 13. The main effects of Conversation and Distance on the mean margins of safety. The main effect of Age Group and the interaction effect were not significant. The bars around the means represent the 95% confidence intervals.

Near Misses

There were no significant main effects found for near misses, although the main effect of conversation did tend towards significance, $F(1, 33) = 3.69$, $p = .063$. The number of near misses increased from 15.03% when conversation was not present to 19.01% when it was present. Unlike Experiment 1 the main effect of distance was not significant, $F(2, 66) = 2.12$, $p = .12$, although the effect size found for this experiment ($f = .25$) was of a similar magnitude to that for Experiment 1 ($f = .29$). The groups did not differ significantly in the frequency of near misses, $F(1, 33) = 0.79$, $p = .38$. The 3-way interaction was significant, $F(2, 66) = 3.82$, $p < .05$ (see Figure 14). Examining the graph it appears that near misses did not increase for either age group at the 40-m initial distance. However, they did increase for the older group when the initial distance was 50-m and for the younger group when the initial distance is 60-m. The results for the younger group partially match those found for Experiment 1. There was little effect of conversation on the frequency of near misses at the 50-m distance, but a greater effect at the 60-m distance, much like the present experiment. However, conversation was associated with an increase in near misses at the 40-m distance for Experiment 1, a finding which was not replicated here. Overall, however, the interaction appears to be uninterpretable.

Collisions

Both phone conversation and the initial distance between the vehicles had a significant effect on the mean percentage of collisions. While the mean percentage of collisions was 6.43% when there was no conversation, this increased to 10.43% when conversation was present, $F(1, 33) = 6.14$, $p < .05$. No group difference was found, $F(1, 33) = 1.19$, $p = .28$, but the main effect of distance was significant, $F(2, 66) = 9.72$, $p < .001$ (see Figure 15). Post-hoc testing indicated

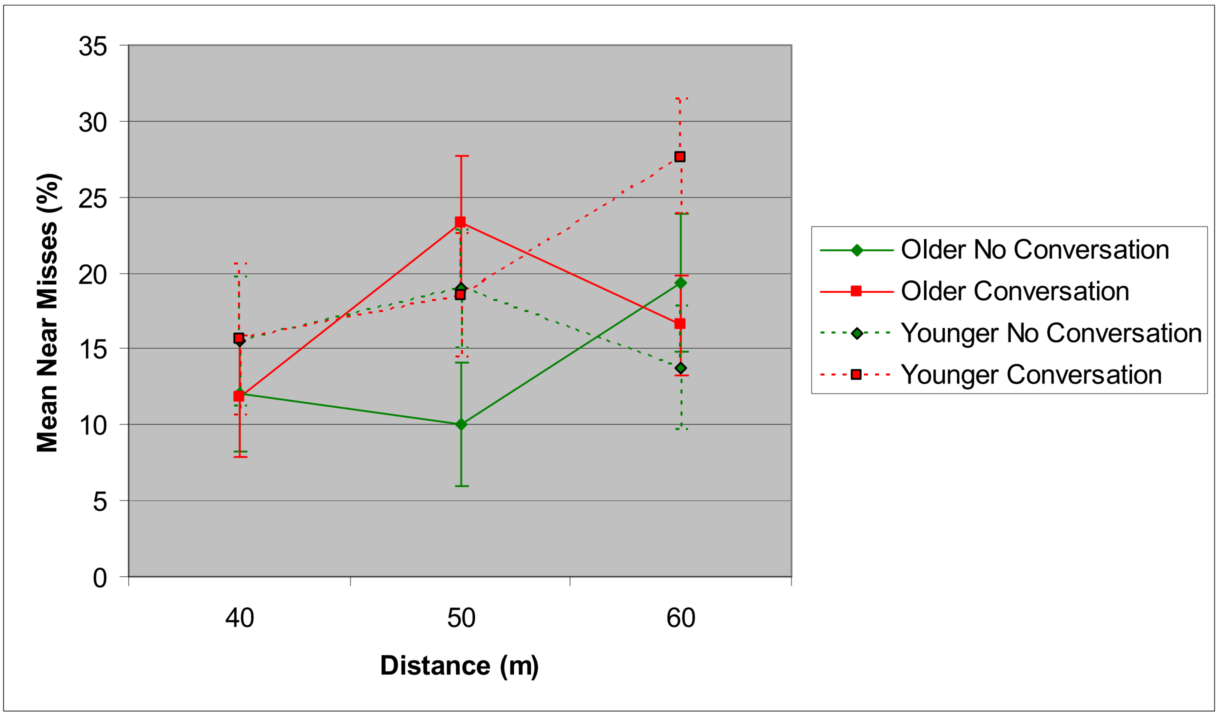

Figure 14. The significant 3-way interaction between Age Group, Conversation, and Distance for the mean frequency of near misses. None of the main effects or lower-level interactions were significant. The bars around the means represent the 95% confidence intervals.

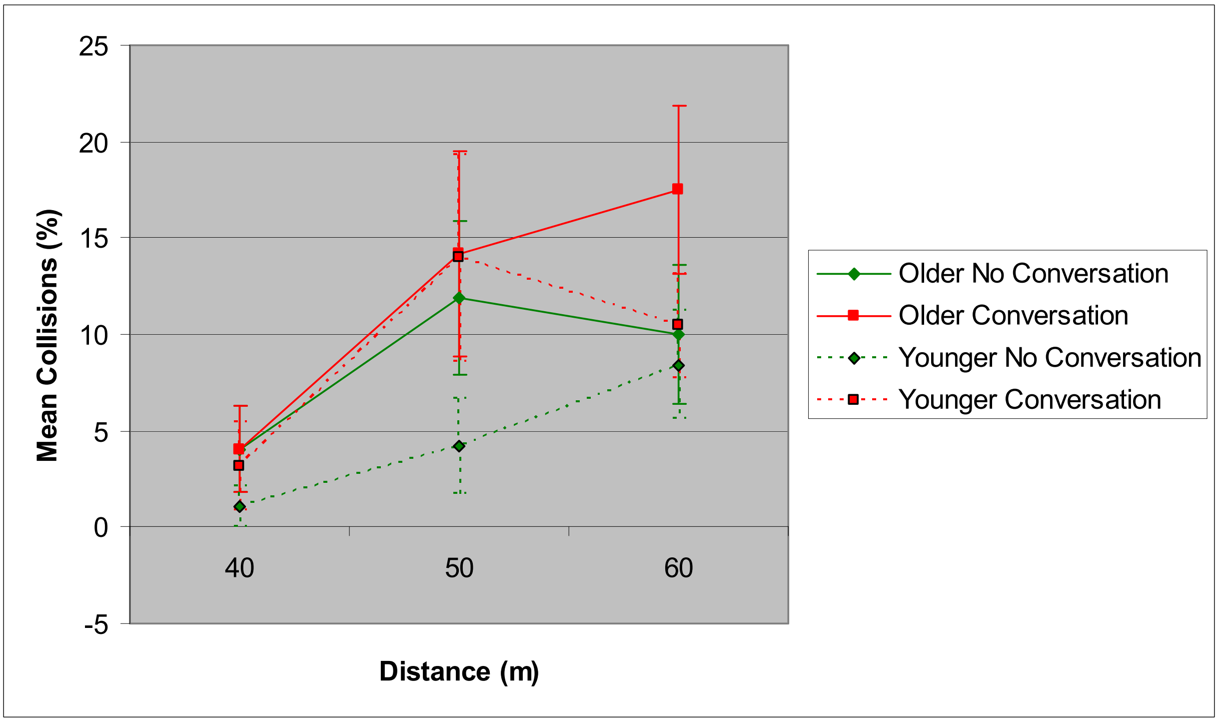

Figure 15. The main effects of Conversation and Distance on the mean frequency of collisions. The main effect of Age Group and the interaction effect were not significant. The bars around the means represent the 95% confidence intervals.

that while the 50- and 60-m initial distances did not differ significantly the 40-m initial distance differed from both.

Walking Speed

Unlike Experiment 1, walking speeds were not affected by the simulated phone conversation, $F(1, 33) = 0.017$, $p = .90$. In contrast to Experiment 1, walking speeds were affected by the initial distance between the vehicles, $F(2, 66) = 6.68$, $p < .01$, with a slight slowing in speed occurring as the distance increased (see Figure 16). However, the only post-hoc differences were between the 40-m initial distance and the two longer distances. One other unpredicted result occurred for age group. There was a significant difference between the two groups, $F(1, 33) = 5.33$, $p < .05$, but it was not in the hypothesised direction. The older group walked faster on average than the younger group (mean speeds of 1.93 m/s and 1.72 m/s respectively), which meant the younger group took, on average, 0.19 s longer to cross to the centre of the road. The mean walking speed for the older group was close to the walking speed found for Experiment 1 (approximately 2 m/s), which means it is possible that the younger group was walking slower than expected and not that the older group walking faster.

As with Experiment 1 (see page 55), an additional walking speed analysis was conducted. A 2-way (2 age group by 5 walking speed conditions) ANOVA with repeated measures on the last factor was conducted. Both main effects were significant, $F(1, 33) = 4.31$, $p < .05$ for age group and $F(4, 132) = 178.38$, $p < .001$ for walking speed condition. As can be seen in Figure 17 the older group walked consistently faster than the younger group (1.58 m / s and 1.45 m / s respectively, averaged across the five conditions). The largest difference was for the maximum speed attained in the VE, a difference of 0.32 m / s. The other differences were between one sixth and one third of this value. Post-hoc testing did not indicate any significant age

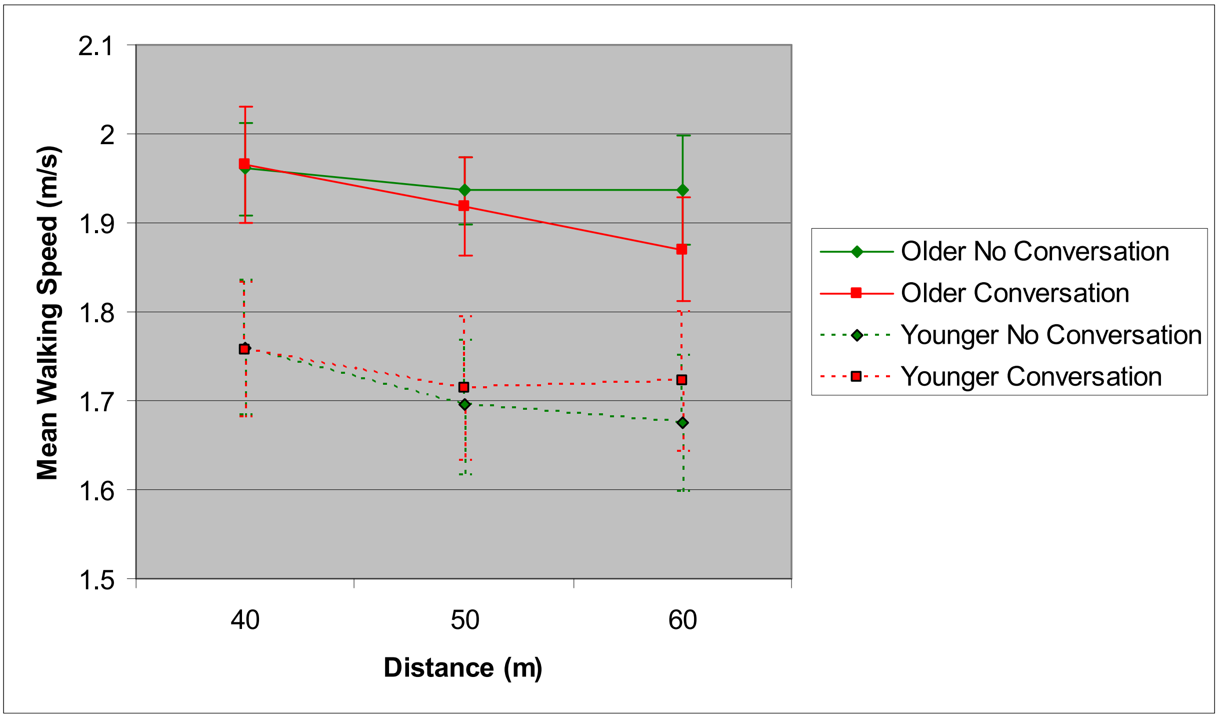

Figure 16. The main effects of Age Group and Distance on mean walking speeds. The main effect of Conversation Condition and the interaction effect were not significant. The bars around the means represent the 95% confidence intervals.

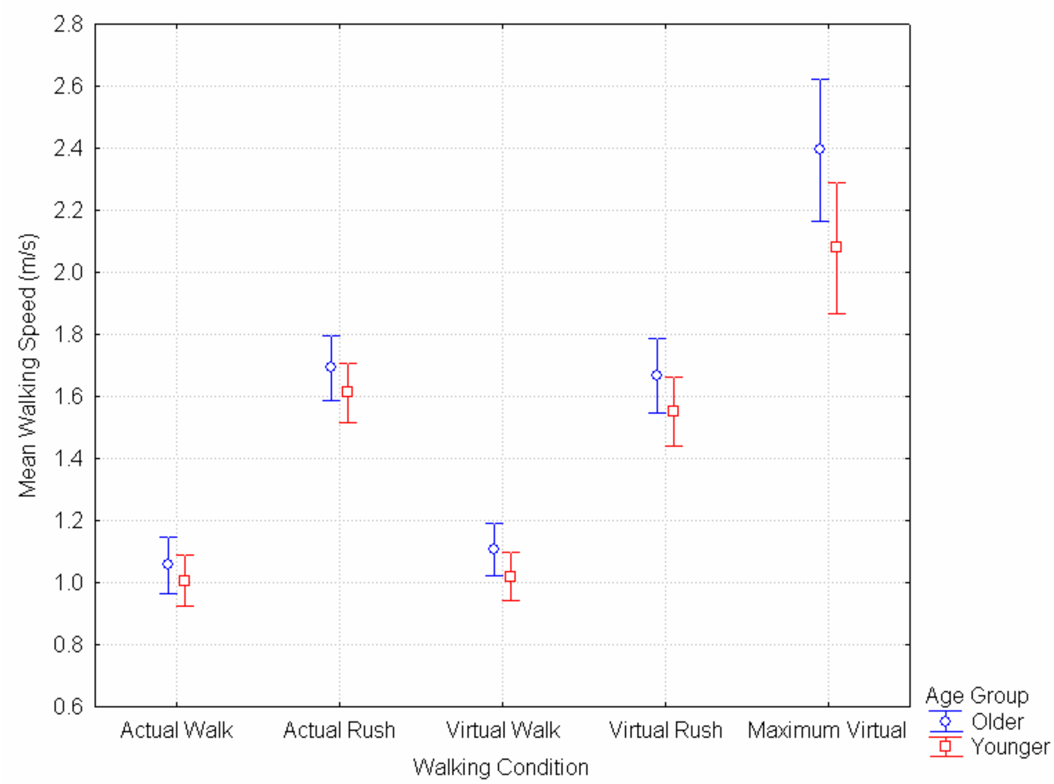

Figure 17. The main effects of Walking Condition and Age Group on mean walking speed. The first four conditions refer to the individuation trials while the fifth (Maximum Virtual) was based on the maximum speed reached by each participant in the experimental trials. The interaction was not significant. The bars around the means represent the 95% confidence intervals.

differences; however, the p value for the maximum virtual condition was half the size of the others (approximate p of .5 compared to a p of .99 for the other 4 conditions). The general trend across conditions replicated Experiment 1, although unlike Experiment 1 not all of the means differed significantly from each other. Participants walked (rushed) at about the same speed in the AE and in the VE. This was consistent within each group, as was the finding that the maximum virtual condition differed from all of the other conditions. The interaction was not significant, $F(4, 132) = 2.06$, $p = .089$.

Total Time Available to Cross

Participants chose shorter gaps when engaged in the conversation task, $F(1, 33) = 9.36$, $p < .01$, the average length decreasing from 2.95 s when not talking to 2.8 s when talking (see Figure 18). The main effect of distance was not significant, $F(2, 66) = 2.6$, $p = .081$, and as can be seen in Figure 18 the trend does not even appear to be the same as was found in Experiment 1 (compare Figure 18 to Figure 8). The two age groups did not differ significantly, $F(1, 33) = 1.41$, $p = .24$.

Percentage of Gap Available to Use

The results for the percentage of gap available to use were consistent with Experiment 1. Less of the gap was used when participants were talking, $F(1, 33) = 6.48$, $p < .05$. Participants used 71.5% of the gap when not talking compared to 69.90% of the gap while talking, and this was consistent across both age groups, $F(1, 33) = 0.02$, $p = .89$. Distance was also significant, $F(1, 33) = 48.59$, $p < .001$. Less of the gap was used as the initial distance between vehicles

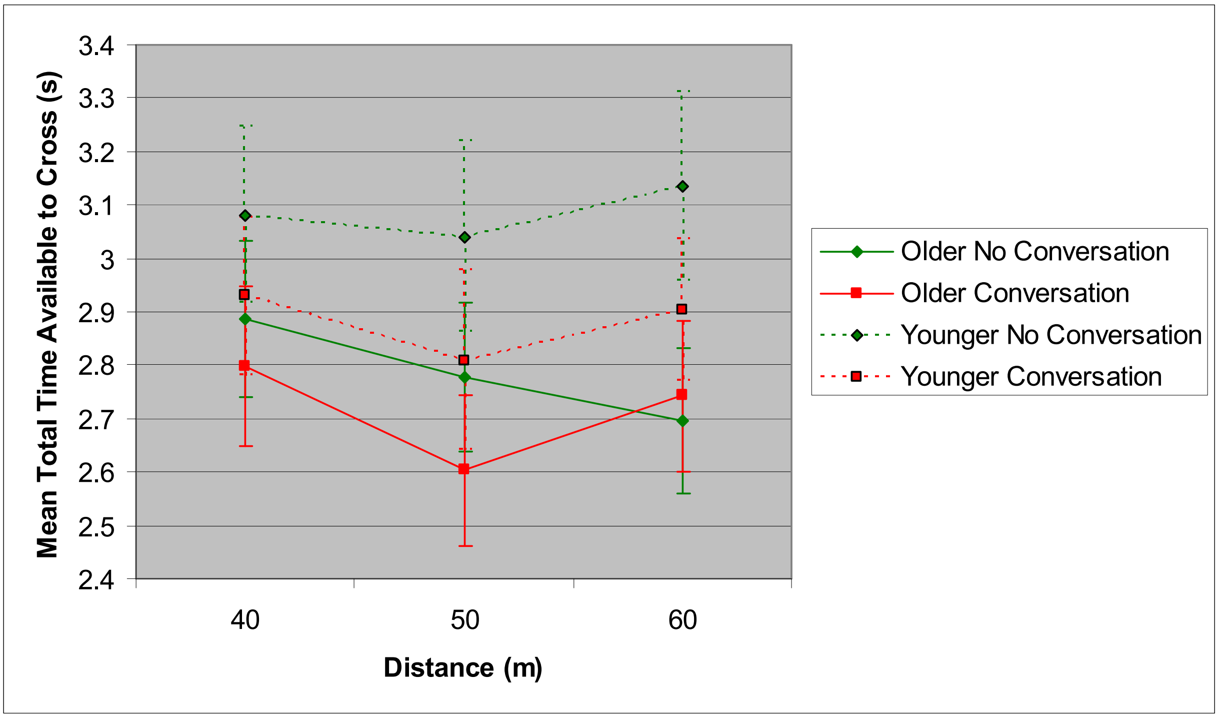

Figure 18. The main effects of Conversation and Distance on the mean total time available to cross. The main effect of Distance and the interaction effect were not significant. The bars around the means represent the 95% confidence intervals.

increased (see Figure 19), post-hoc testing indicating that all of the distances differed from each other.

Safe Gaps Left

Unlike Experiment 1 the simulated phone conversation did not have a significant effect on the mean number of safe gaps left, $F(1, 33) = 1.66$, $p = .21$, although distance was significant, $F(2, 66) = 26.77$, $p < .001$. Participants left fewer safe gaps as the initial distance increased, all of the distances differing significantly from each other (see Figure 20). Participants in the older group tended to leave fewer safe gaps (a mean of 1.29) than those in the younger group (a mean of 1.99), although this did not quite reach significance, $F(1, 33) = 4.02$, $p = .053$.

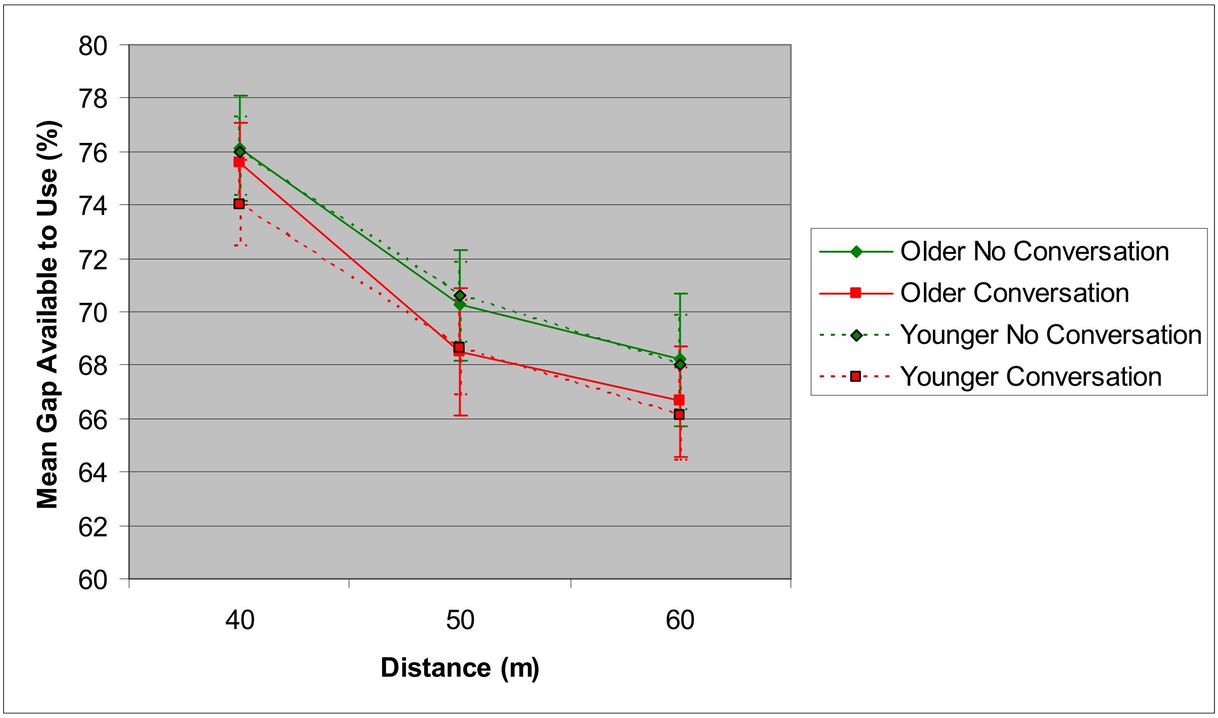

Figure 19. The main effects of Conversation and Distance on the mean percentage of gap available to use. The interaction is not significant. The bars around the means represent the 95% confidence intervals.

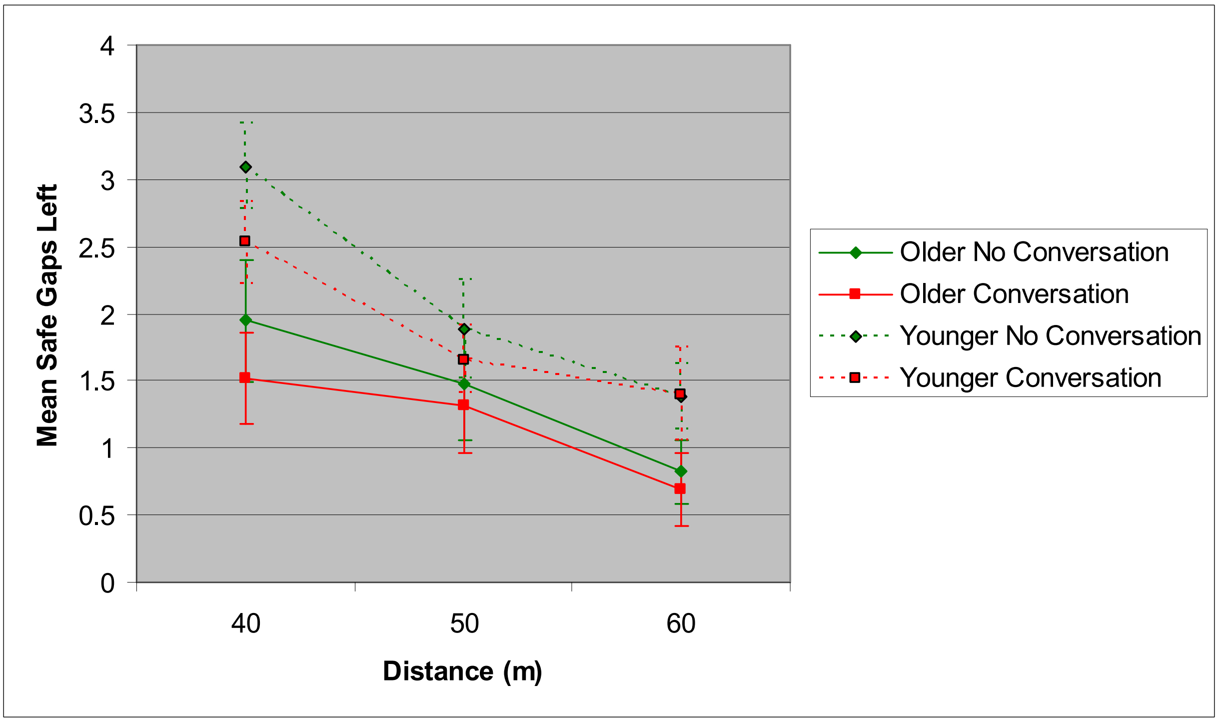

Figure 20. The main effects of Age Group and Distance on the mean number of safe gaps left. The main effect of Age Group and the interaction effect were not significant. The bars around the means represent the 95% confidence intervals.

Cautious Crossings

Unlike Experiment 1 the simulated phone conversation did appear to have an effect on cautious crossings, $F(1, 38) = 6.33$, $p < .05$. More cautious crossings occurred when participants were engaged in the conversation task (14.17%) compared to no conversation (9.83%). The main effect of distance was again significant, $F(2, 76) = 8.37$, $p < .001$, reproducing the findings from Experiment 1. Post-hoc testing indicated that more cautious crossings occurred at the 40-m initial distance than the other two distances (see Figure 21), the two longer distances not differing significantly. The frequencies of cautious crossing did not differ between the age groups, $F(1, 33) = 2.21$, $p = 1.44$. As well, cautious crossers were fairly evenly distributed across age groups, with half of the older group and eight of the younger group having had at least one cautious crossing.

As with Experiment 1, t-tests were conducted to compare those participants with at least one cautious crossing to those with no cautious crossings. The findings were the same as Experiment 1, with the only significant differences between the groups occurring for the number of safe gaps left, for both the conversation trials, $t(33) = 2.22$, $p < .05$, and the no-conversation trials, $t(33) = 3.51$, $p < .01$. Again this suggests that, in general, the two cautious crossing groups do not differ in any substantive way.

Time-to-Arrival

The same visual trend that was evident for Experiment 1 was repeated here, with participants preferring the longer gaps when not engaged in the conversation task (see Figure 22). Unlike

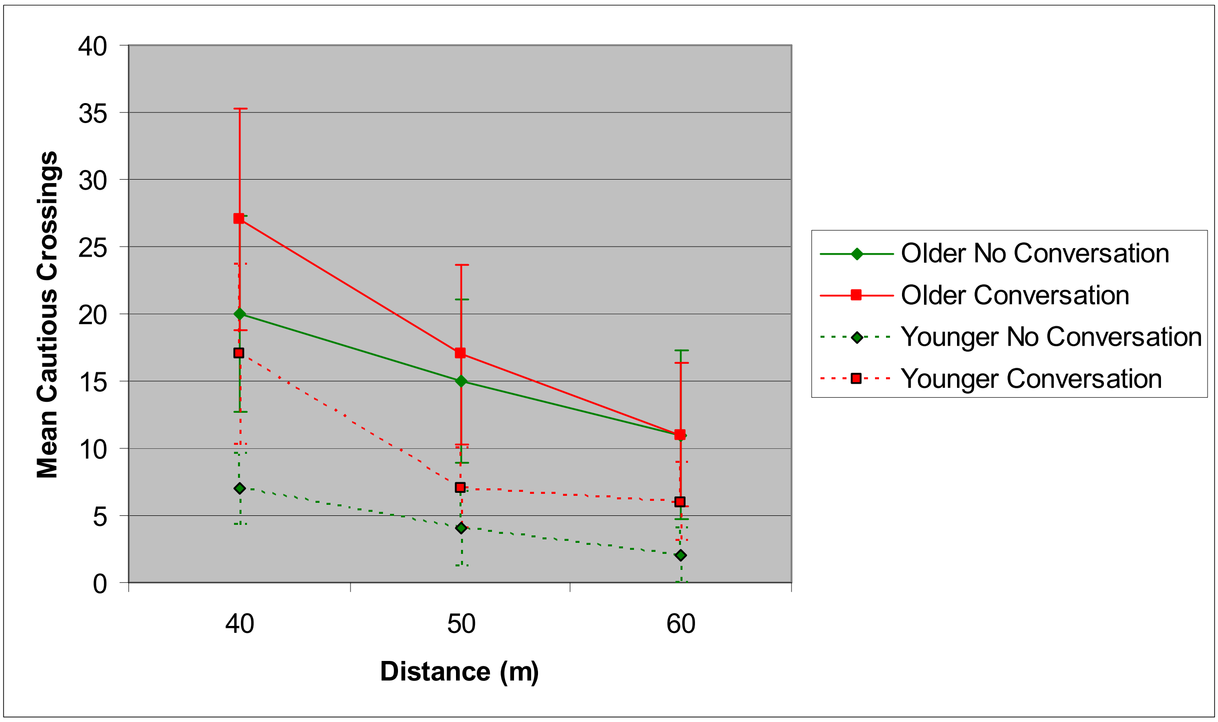

Figure 21. The main effects of Conversation and Distance on the mean frequency of cautious crossings. The interaction effect was not significant. The bars around the means represent the 95% confidence intervals.

Experiment 1 the linear contrast for the no-conversation condition was significant, $F(1, 33) = 9.16$, $p < .01$, although the cubic trend was again significant, $F(1, 33) = 8.05$, $p < .01$. The linear contrast for the conversation condition remained non-significant, $F(1, 33) = 1.13$, $p = .30$, although two other trends were significant for this experiment; the quadratic trend, $F(1, 33) = 8.05$, $p < .01$, and the 8^{th} order trend, $F(1, 33) = 5.00$, $p < .05$. The four largest gaps were again preferred by the participants when they were not conversing, but as was found for Experiment 1 the participants showed no consistent preference for the longer gaps when conversing. These analyses were also conducted for each age group independently, and as the results were consistent with the pooled data only the overall analysis was reported.

Age-Based Differences in Variability

The hypothesis that there would be greater variability in the older group than in the younger

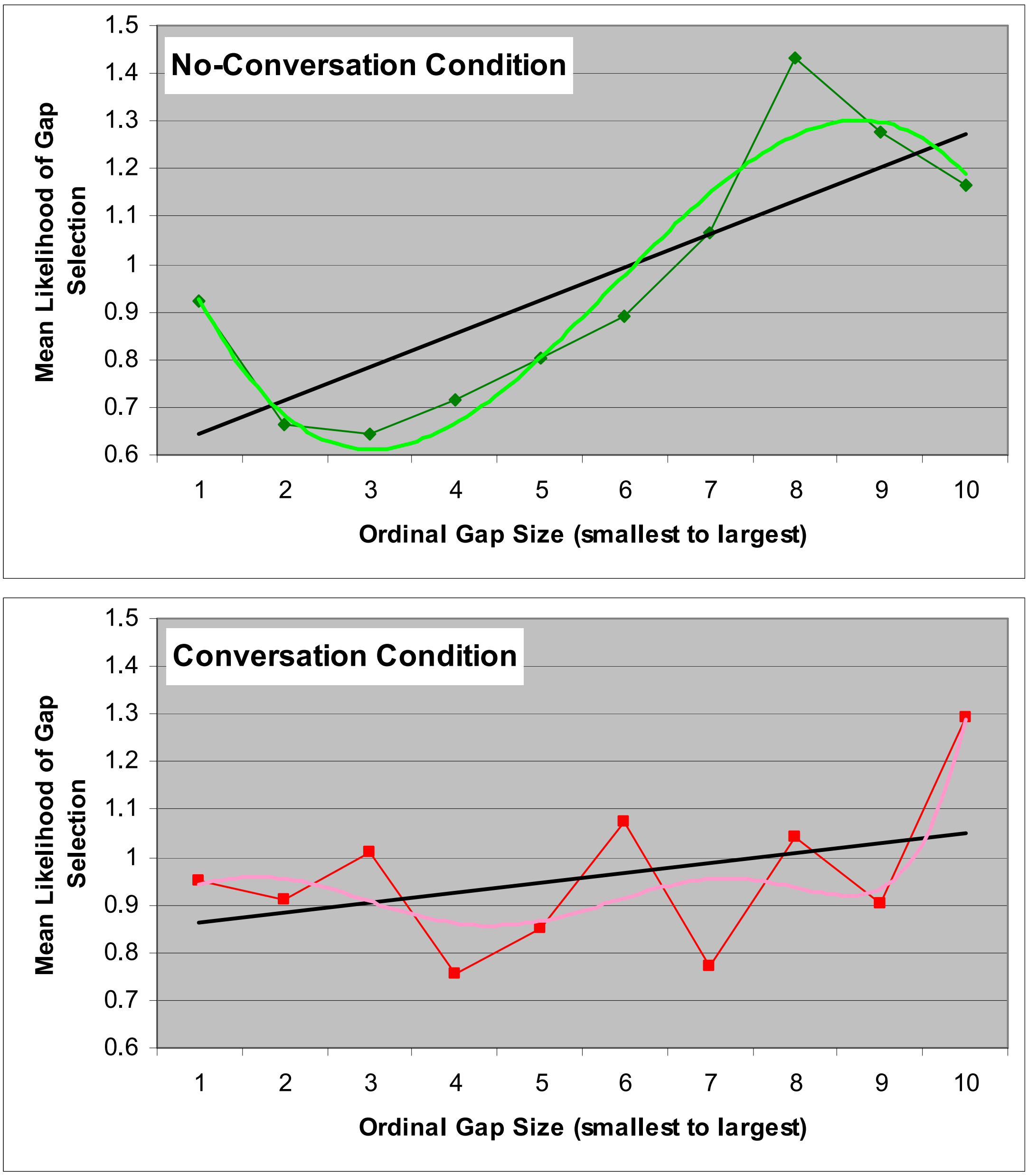

Figure 22. The effect of ordinal gap size on the likelihood that a gap is selected for the no-conversation condition and the conversation condition. The straight lines are a linear fit to the data, while the smoothed curved lines are the lines of best fit for each condition. Values above 1 indicate that the gap is picked more often than would be expected, and values less than 1 indicate the gap is picked less often than would be expected.

group was largely unsupported. Of the 48 Levene’s tests that were conducted (6 conditions by 8 dependent variables) only 11 were significant (see Table 6. Conditions in which the Levene's test was significant.for specific details). In nine of the cases the difference was in the hypothesised direction. However, a general examination of the group variances indicated that the older group had the larger variance just over half the time (26 instances, compared to 22 instances of the younger group having the larger variance). When only looking at which group had the greater variance, and not considering whether the

Table 6. Conditions in which the Levene's test was significant.

Dependent Variable	Phone Conversation	Initial Distance (m)	Hypothesis Supported?
Margin of Safety	No	40	Yes
	Yes	50	Yes
Collisions	No	40	Yes
	No	50	Yes
Walking Speed	No	40	No
	No	50	No
Percentage of Gap Used	No	50	Yes
Cautious Crossings	No	40	Yes
	No	50	Yes
	No	60	Yes
	Yes	50	Yes

difference was statistically significant, some general trends can be noted. For instance, the older group exhibited greater variability for all conditions for the margin of safety and cautious crossings, for five out of six conditions for the percentage of the gap used, and for four out of six for collisions and the number of safe gaps left. However, for walking speed and the time available to cross the younger group always exhibited greater variability, and for five out of six conditions for near misses.

It was possible that, in general, the group with the higher (or lower) mean may consistently have had the higher variance. In most cases where the older group had the higher variance they also had the higher mean score on that variable, with the exception of the number of safe gaps left. This was generally the case for the younger sample too, although for walking speeds they had greater variance but the lower mean. It is possible, then, that in general the higher the mean the greater the variance. This makes intuitive sense as the potential scores around a lower mean are more constrained than the scores around a higher mean (assuming, of course, that there is a minimum score, which is the case for all of the dependent variables for this experiment). Nothing definitive can be said about age differences in variance based on these results.

Postural Stability

As mentioned earlier, postural stability was measured pre- and post-exposure to the VE. Three 2-way (2 age group * 2 pre- and post-exposure) ANOVAs with repeated measures on the last factor were conducted. Data from all participants was used for this analysis as there was no missing data.

Stability was not affected as predicted. Across the three axes, there were no significant differences between the age groups and no interactions with exposure to the VE. There was one significant main effect of exposure, for the up-down axis, $F(1, 38) = 5.77$, $p < .05$. But rather than increasing, stability decreased. This was indicated by the mean variability decreasing which can be seen in the reduction of the 95% confidence intervals (see Figure 23). This figure shows the age groups separately to indicate that this change was consistent between the two groups. Slight, non-significant, decreases in variability were also found for the other two axes.

Summary

This experiment generally supports the findings from Experiment 1. Participant safety was reduced, with more collisions and near misses occurring when the participants were engaged in the conversation task. Conversation also produced lower margins of safety, a decrease which was of a similar magnitude to that found in Experiment 1 (Cohen's fs of approximately 0.49 for each experiment). Participants chose smaller gaps, and used less of the gap that they did choose, again consistent with Experiment 1. However, unlike Experiment 1 conversation did not

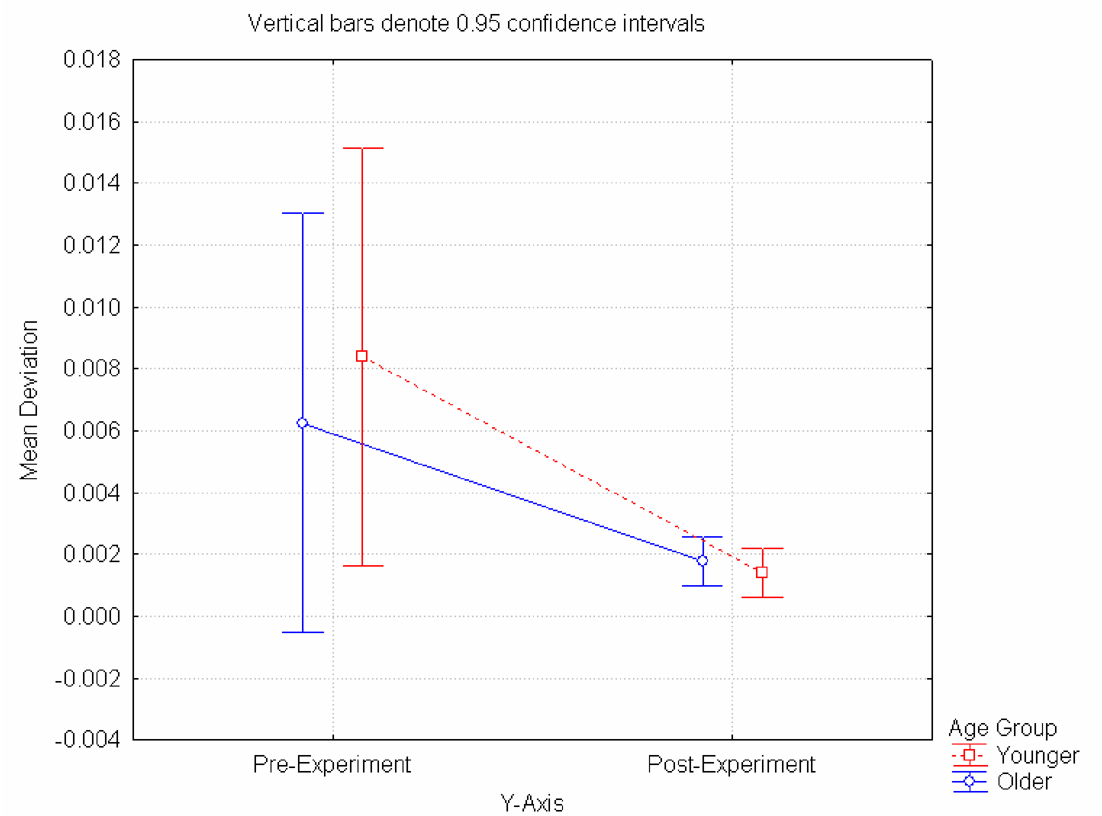

Figure 23. The main effect of VE exposure on mean postural stability scores in the Y-axis. The main effect of age group and the interaction effect were not significant. The bars around the means represent the 95% confidence intervals.

significantly increase the number of safe gaps that were left. Walking speeds, which were slowed by conversation for Experiment 1, were almost unchanged for this experiment. Phone conversation affected the frequency of cautious crossings for this experiment, more occurring when conversation was present, whereas there was no effect of conversation for Experiment 1. Overall this suggests that the finding of reduced safety is robust, and indicates that the reduction in walking speed that was found for Experiment 1 may not have had a major effect on participant safety.

The hypotheses for age differences were not supported. Older participants had slight, non-significant, increases in collisions and cautious crossings compared to the younger group, but otherwise performed as well as or better than the younger group. This was most notable for walking speed. While it was predicted that older participants would walk slower than the younger participants, the opposite was found. The results of the additional walking speed analysis supports this, with the older participants walking significantly faster in all of the walking speed conditions than the younger group. Potential reasons for this finding will be discussed in Chapter 10. The performance variability analysis was inconclusive. The increase in variation could be explained in part by which group had the higher overall mean for the cell rather than by which age group they were in.

The results for the postural stability analysis were also inconclusive, both groups exhibiting similar stability levels. A curious finding was a decrease in the variability of the participants' postural stability in the post-exposure test. It is plausible that the unfamiliar weight of the HMD was the reason for the greater variation in stability pre-exposure. The participants may have adapted to the weight by the end of the experiment, hence the apparent improvement in their stability.

The initial distance between the vehicles again affected the behaviour of the participants. The participants were less safe, walked slower, and used less of the gap at longer initial distances. When the vans started further away, greater distances were also associated with a decrease in cautious behaviours, the participants allowing fewer safe gaps to pass, and having fewer cautious crossings. While smaller gaps were chosen at longer distances this effect did not reach significance. The trend for this variable also differed from the one found for Experiment 1, the smallest gaps being chosen at the 50-m initial distance. As well, the findings regarding the participants' preference for longer gaps was replicated, with a definite preference for the longer gaps revealed only when they were not engaged in the conversation task.

It should be noted that the small group sizes are a limitation of this study. The pattern of means would stabilise with a larger sample size and hence be more interpretable, while the increase in power may have resulted in significant group differences.

CHAPTER 8: EXPERIMENTS 3

This experiment was designed to examine the effect of conversation on the direction of attention, specifically whether conversation would make it less likely for participants to look for oncoming traffic. As participants always looked before crossing in Experiments 1 and 2, for this experiment it was decided that the vehicle events should be less frequent, simulating a quiet road. Out of a total of 40 trials vehicles were present for four of them, two trials with vehicles in each conversation condition (conversation / no-conversation).

A previous study using 50 participants did not find any effect of conversation on whether participants looked before crossing. This was thought to be due to an instruction given to the participants before the experiment began ("Please look to your right, and if no vehicles are coming please cross the road"). These instructions were changed to "Please treat the road as an actual road" for this experiment. The findings from the pilot experiment, in relation to walking speeds and reaction times (see below), were nearly identical to the findings of this experiment, both for mean differences and standard deviations; therefore the pilot study has not been reported in full.

Hypotheses

Conversation

1. Conversation will reduce the likelihood that participants will turn to look for oncoming traffic.
2. Conversation will reduce the participants' walking speeds.
3. Conversation will reduce the participants' reaction times.

Method

Participants

The participants consisted of 11 females and 9 males between the ages of 18 and 24 (mean age 20.8 years; SD = 2.14). They were students from the University of Canterbury, primarily from the Department of Psychology. All participants reporting normal or corrected normal vision and hearing, and none had any form of mobility impairment. They were given a $10 petrol voucher for their participation. No information was collected on whether they were drivers.

Materials and Apparatus

These were identical to Experiment 2 with two exceptions. The positions of the tree and the street light in the virtual environment were swapped, i.e. in the starting position the street light was behind the participant, while the tree was opposite them. The reason for the difference between the VE is related to the original program that was modified for this experiment. The VE had not been standardized for the preceding research (e.g. Simpson et al, 2003; Owen, 2002) and so the positions of the tree and street light were not consistent. The objects were simply reversed for this experiment compared to Experiments 1 and 2, with no underlying theoretical reason for the change.

Second, the initial instruction was changed from “Turn to your left and get ready to cross” to “Please get ready to cross”. During the pilot study it was noted that some participants looked to the left, rather than the right, before crossing the road. As it was likely that this instruction was causing the confusion it was changed. The new message was recorded in the experimenter’s voice, and the end message, “Turn to your right and walk back to the tree”, was rerecorded in the experimenter’s voice for consistency.

Simulated Cellular Phone Conversation

The cellular phone conversation was the same as Experiment 1.

Independent Variable

The independent variable was *conversation* condition (conversation or no-conversation).

Dependent Variables

The dependent variables are presented in Table 7. Note that looking to the right was based on whether a participant turned far enough to see the first of the line of vans, as was measured by computer via the position of the HMD.

Table 7. Dependent variables for Experiment 3.

Variable	**Description**	**Unit**
Direction of Attention	Whether the participants looked to the right before initiating a crossing	-
Walking Speed	The speed with which the participant crosses from 0.5 m from the starting point to the far edge of the lane	m/s
Reaction Time	The time from initiation of a trial to the participant initiating their crossing	s

Procedure

The experiment consisted of 40 trials, in 4 of which vehicles approached a participant's starting position from the right. Participants were asked to treat the road as an actual road. This instruction was not repeated during the course of the experiment. The first vehicle began 15 m from the participant's position and had a T_A of 1.5 s (with a velocity of 36 km/h). The remaining vehicles were spaced 20 m apart and had T_As of 1.3 s (with velocities of 55.38 km/h). These values were chosen to reduce the likelihood of a participant crossing if they were attending to the approaching vehicles as none of the gaps were likely to be safe to cross in.

The conversation trials were arranged using a Latin square design (see Appendix I) to avoid order effects. The vehicle trials were randomly assigned within each set of 10 trials using Microsoft Excel, with two provisions: there had to be vehicles in 2 of the conversation trials and in 2 of the no-conversation trials, and the vehicle trials had to be separated by at least 4 no-vehicle trials, so that vehicle trials did not occur too close to each other. A list of the trials in which vehicles were present is given in Appendix J.

The pre-trial procedure was the same as Experiment 1 regarding instructions and questionnaires (see page 92; the instructions for Experiment 3 are in Appendix K), although there were no individuation trials before the experiment. Participants were instructed to treat the road as if it were an actual road. If vehicles were present participants were instructed to wait until the vehicles had passed before crossing. A message that was recorded in the experimenter's voice was presented at the beginning of each trial ("Please get ready to cross"). At the end of each trial there was a black screen with white text instructing them to prepare for the next trial. When this screen appeared they were instructed to look to the front.

In general only one activity from the ARTQ was given per trial, although occasionally one activity would be used for more than one crossing. The nature of this experiment, in contrast to Experiment 1, made it unlikely for more than one activity to be used for a given trial. As the decision for this experiment was cross/don't cross there was less time spent waiting before crossing for each trial, as compared to the gap-choice used in Experiments 1 and 2. The only trials where multiple activities per trial were used were those where vehicles were present, although not in all instances.

At the end of the experiment participants were asked to fill in the post-test SSQ and complete the reduced version of the ARTQ which excluded the items used for the conversation task (see Appendix L).

Results

Three t-tests were conducted, one for each dependent variable. Since the predictions were directional 1-tailed tests were used.

Assumptions

While the assumption of normality did not hold for this data t-tests are robust to normality violations when the sample sizes are equal. As this is a completely within-participants design, and the repeated-measures factor only has two-levels, there were no possible violations of the other assumptions.

T-Tests

Direction of Attention

Participants tended to look less often during the simulated conversation (93% of the time, SD = 13) compared to no conversation (97% of the time, SD = 5.7), $t(19) = -1.76$, $p < .05$, Cohen's $d = 0.43$. This effect was mostly limited to the first 10 trials. Across the first 10 trials there were 35 instances of not looking compared to only 3 instances across the remaining 30 trials. The majority of these instances also came from three participants who accounted for 26 of the 38 trials where a participant did not look before crossing.

Walking Speed

Simulated conversation again impaired the participants' walking speeds, $t(19) = -3.51$, $p < .01$, Cohen's $d = 0.50$ with participants walking 0.075 m/s slower when engaged in conversation (a mean speed of 0.98 m/s, SD= 0.17) compared to no conversation (a mean speed of 1.06 m/s, SD = 0.13). This impairment is very similar to the one found in the pilot study (0.08 m/s).

Reaction Time

Consistent with the pilot study, RTs were slowed by 0.49 s during simulated conversation compared to no conversation, $t(19) = 2.45$, $p < .05$, Cohen's $d = 0.49$. Mean reaction times were 3.26 s (sd = 1.1) during conversation trials and 2.77 (sd = 0.9) during no-conversation trials.

Summary

Participants were less likely to look for traffic when conversing, but this was limited mostly to the first 10 trials and to 3 participants. Conversation, then, may affect some people's tendency to look, but it did not appear to be a consistent effect across participants. However, the increase in RT and decrease in walking speed were consistent with the results for the pilot study, suggesting that they are robust effects.

CHAPTER 9: OTHER FINDINGS

This chapter includes findings that are relevant for more than one of the studies or compare across studies. It is divided into three sections: (1) Experiment 4, an experiment designed to test an alternate explanation for the general finding that walking speeds were impaired by conversation; (2) an examination of the internal consistency of the dependent variables across Experiments 1 and 2, and; (3) the results from the SSQ for the reported experiments.

Experiment 4

Introduction

Participants walked slower when conversing for two of the three reported studies, as well as the pilot study for Experiment 3. Although this could be solely attributable to the added distraction of the conversation task there was another possibility. The decrease in walking speeds was potentially due to a combination of the conversation task *and* the visibility differences between the VE and the AE. Given that the participants had no visible virtual body (i.e. they could not see their own bodies in the VE), there may have been an increased fear of tripping in the VE. When there was no secondary task participants may have had sufficient cognitive resources to cope with walking while being unable to see their bodies. However, the increased demand on resources that was produced by the secondary task may have resulted in the reduced walking speeds as a way of coping with this increase. If this was the case there would be no walking speed decrease in the AE. This experiment was designed to test the hypothesis that it was the conversation, and not an interaction between the conversation and the reduced visibility in the VE, that caused walking speed to be impaired.

Hypotheses

1. Simulated conversation will reduce the participants' walking speeds regardless of the environment (VE or AE) that they are in
2. Participants will walk slower in VE than in the AE

Method

Participants

The sample for this experiment consisted of 7 females and 5 males aged between 20 and 41 years old (mean = 25.2 years, sd = 5.98). All but two were post-graduate students in the Department of Psychology at the University of Canterbury. The remaining two were friends of the experimenter. All of the participants reported having normal or corrected normal vision and hearing, and none had any form of mobility impairment. No information was collected on whether they were drivers.

Materials and Apparatus

These were identical to Experiment 2, with one exception. As with Experiment 3 the position of the tree and the lamp post were transposed, the lamp post being behind the starting position and the tree opposite it.

Variables

There were two independent variables: *environment* and *conversation* condition. The first refers to the environment the participants were walking in, the virtual or actual environment, while the conversation conditions have been described previously (see page **Error! Bookmark not defined.**). The dependent variable was walking speed.

Procedure

The experiment consisted of 20 trials. Half of these were in the AE, with the HMD positioned on the head of the participant but not covering their eyes, and half were in the virtual environment. For each environment there were 5 trials with conversation and 5 trials without. These were counterbalanced to ensure that each combination of environment and conversation occurred 3 times in each of the four possible blocks (see Appendix M). The road-crossing task for this experiment was simply walking to the centre of the road and back. In the actual environment the approximate centre was marked with a strip of white tape.

Participants first read the information sheet (see Appendix N) and signed the consent form. The design of the experiment, as outlined above, was then explained verbally. In the VE trials participants were instructed to begin walking when the trial started. In the AE conversation trials participants were instructed to begin walking once the activity name had been given, while in the no-conversation trials they began walking after the experimenter said “go”.

Results

Assumptions

As this was a completely within-participant design, and neither of the repeated-measures had more than two-levels, there were no issues with the assumptions for this experiment.

ANOVA

The results were analysed using a 2-way (2 conversation by 2 environment) repeated-measures ANOVA. There was a significant main effect of condition, $F(1, 11) = 27.95$, $p < .001$, Cohen's $f = 1.53$, participants walking 0.17 m/s slower when engaged in the phone task (means of 1.02 m /s and 0.85 m /s for not conversing and conversing respectively). This meant, on average, that participants took 0.59 s longer to cross to the centre of the road. The main effect of environment was also significant, $F(1, 11) = 4.84$, $p = .05$, Cohen's $f = 0.64$. Participants walked slower in the VE (0.88 m/s) than in the AE (0.99 m/s). The interaction was not significant, $F(1, 11) = 4.21$, $p = .065$, Cohen's $f = 0.59$ (see Figure 24). Overall these findings suggest that it is primarily the conversation task that reduces walking speeds.

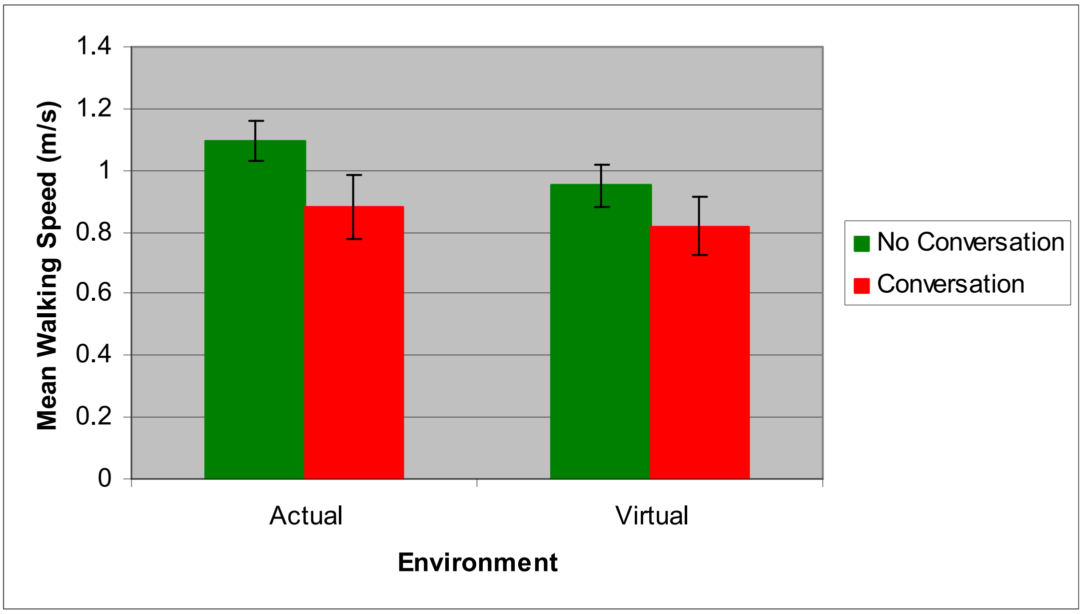

Figure 24. The main effects of Conversation and Environment on walking speeds. The interaction was not significant.

Consistency of Performance for Experiments 1 and 2

To examine the consistency of the participants' performance across the different conditions for Experiments 1 and 2 Chronbach's alphas were calculated for each of the dependent variables (see Table 8 for full alpha tables). Most of the alphas were high (generally over .8), indicating that the participants' performance was consistent across conversation conditions and initial distances. The only variables where the alphas were generally low were near misses and collisions. As noted earlier these two variables are gross measures of safety. This means that participants who have a near miss or collisions in one condition may not have them in the other conditions. It is therefore not surprising that the alphas are lower for these variables.

Table 8. Chronbach's alphas for each dependent variable. Alphas are provided for Experiments 1 and 2, and for the younger and older groups from Experiment 2 separately.

	Experiment 1	Experiment 2		
		All	Younger	Older
Near Misses	0.36	0.48	0.61	0.22
Collisions	0.52	0.73	0.67	0.76
Margin of Safety	0.82	0.92	0.84	0.94
Total Time Available to Cross	0.87	0.95	0.95	0.94
Safe Gaps Left	0.89	0.85	0.74	0.91
Percentage of Gap Available to Use	0.92	0.88	0.83	0.92
Cautious Crossing	0.97	0.93	0.81	0.95
Walking Speed	0.98	0.98	0.99	0.93

Simulation Sickness

The SSQ results for pre- and post-VE exposure for Experiments 1, 2, and 3 are presented in Table 9. Also included in this table are the means and SDs from Kennedy et al. (1993) for comparison. As can be seen in the table the post-exposure means are higher than those found by Kennedy et al. (1993), and remain so even if the pre-scores are taken into account. It can also be seen that the total score increases as the length of exposure to the simulation increases. Experiment 1, with 60 trials, produced the highest overall score, followed by Experiment 2 (45 trials) and Experiment 3 (40 trials). Length of exposure, then, did seem to have an effect on reported SS symptoms. Note, however, that this is a purely visual analysis which does not take into account variability. While there appear to be differences these may merely be artefacts of the data.

It is possible that the specific simulation used for these experiments may produce higher levels of SS than is desirable, which may confound experimental findings. Another possibility is that the samples used for this experiment differ in some way to the samples used for the norms. The samples used by Kennedy et al. (1993) were naval personnel, people who may have less susceptibility to motion or simulation sickness than the general population.

Table 9. Simulation Sickness Questionnaire means and standard deviations for pre- and post-VE exposure for Experiments 1, 2, and 3, and for post-exposure for Kennedy et al (1993).

		Pre-exposure				Post-exposure			
		N	O	D	TS	N	O	D	TS
Experiment 1	**Mean**	**10.93**	**8.54**	**13.16**	**12.04**	**29.31**	**25.63**	**39.48**	**34.75**
	SD	17.65	12.72	21.83	17.87	27.41	25.54	39.89	32.07
Experiment 2	**Mean**	**7.63**	**6.25**	**10.09**	**9.35**	**18.60**	**14.59**	**24.01**	**25.06**
	SD	12.82	10.28	18.38	14.12	26.19	19.07	30.88	23.84
Experiment 3	**Mean**	**13.36**	**9.10**	**16.01**	**14.03**	**16.70**	**17.81**	**24.36**	**21.88**
	SD	17.89	9.07	25.66	17.24	21.19	15.99	28.16	21.24
Kennedy et al (1993)	**Mean**					**7.70**	**10.60**	**6.40**	**9.80**
	SD					15.00	15.00	15.00	15.00

Note. N = Nausea, O = Oculomotor, D = Disorientation, TS = Total Score.

The counterbalanced design for the experiments should have spread any potential performance detriment induced by SS evenly throughout the data, but the loss of some participants for Experiments 1 and 2 may have affected the counterbalanced design for these experiments. As can be seen in Table 10, following the removal of the extremely cautious crossers the experiments were no longer fully counterbalanced. This is not surprising for Experiment 3, as the uneven number of participants meant it was not possible to have an equal number of participants in each conversation condition for each block. The counterbalancing could have held for Experiment 1, but this was not the case for any block except the first. For the remaining blocks there was either a 24 to 26, or a 23 to 27 participant split across conversation conditions. However, there was no systematic bias in the spread of these unbalanced blocks, although slightly more of the conversation blocks occurred within the first five blocks (127 blocks of conversation trials, compared to 123 blocks of conversation trials for the last five blocks of the experiment). Given this spread, any impairment produced by SS would have reduced the differences between the conversation conditions. Therefore it can be concluded that the findings for these two experiments were not due to an unbalanced design.

Table 10. The counterbalanced designs of Experiments 1 and 3 after the extremely cautious crossers were removed. The numbers indicate how many participants had each type of trial for each 3-trial block.

	Block									
	1	2	3	4	5	6	7	8	9	10
Experiment 1										
Conversation	25	23	27	26	26	24	23	23	27	26
No Conversation	25	27	23	24	24	26	27	27	23	24
Experiment 3										
Conversation	18	17	17	18	18	17	17	18	18	17
No Conversation	17	18	18	17	17	18	18	17	17	18

CHAPTER 10: GENERAL DISCUSSION

The Effects of the Phone Conversation Task

The cell phone conversation task affected participant performance across all of the experiments. The results from Experiments 1 and 2 (the gap-choice experiments) will be summarised first, followed by a summary of Experiments 3 (the infrequent vehicle experiment). The implications of the findings will be discussed following the summaries.

Experiments 1 and 2

Although initially it seems more reasonable to compare the younger participants' data from Experiment 2 with the Experiment 1 data, an examination of the graphs for the older and younger participants, as well as the general lack of significant age-related main and interaction effects, suggests that the groups were fairly consistent. Therefore, for simplicity, Experiment 1 will be discussed in relation to the combined data from Experiment 2.

The phone task had an impact on participant safety. Conversation reduced the margin of safety by approximately 8.5 percentage points for both Experiments 1 and 2. Near misses and collisions increased for both experiments when the participants were engaged in the phone task. However, near misses for Experiment 2 and collisions for Experiment 1 only neared significance. For Experiment 1 near misses increased by 5%, while collisions increased by about 2.7%. Both near misses and collisions increased by about 4% for Experiment 2. Combined, these findings indicate that the conversation task reduced the safety of the participants' road crossings for both experiments.

Three behavioural variables were measured that may have related to the safety of a crossing. These were the participants' walking speeds, the temporal size of the gap the participants' crossed in, and how much of each gap they used. Walking speeds were slightly, but significantly, reduced (0.04 m/s) by phone conversation for Experiment 1, but were not significantly reduced for Experiment 2. This finding for Experiment 2 was unexpected, given that walking speeds were affected by the conversation task for all of the other experiments. This discrepancy will be addressed shortly.

Participants also chose significantly smaller gaps to cross in while talking for both experiments. This impairment was smaller for Experiment 1 (0.1 s) than for Experiment 2 (0.29 s). One other difference between the two experiments was that participants in Experiment 1 chose smaller gaps in general (a mean of 2.38 s versus about 2.88 s for Experiment 2[19]). The likely cause of this was way the experimental trials were set up for each experiment. For Experiment 1 the vehicle trials were recalibrated, using the individuation trials, after the practice trials. This was not done for Experiment 2. This meant that the available gaps were shorter for Experiment 1 than for Experiment 2. This explains why smaller gaps were chosen for Experiment 1 overall, and may help explain why the secondary-task impairment for Experiment 2 was larger than for Experiment 1.

Significantly less of the chosen gap was used across both experiments. The decrease was only small, about 1.5% for both experiments. However, considering this result in light of the finding that participants chose smaller gaps to cross in seems to explain the reduction in safety. Although the reduction in walking speed would also be expected to reduce safety this did not seem to be the case: Walking speeds were not significantly reduced by conversation in

[19] This difference was statistically significant, $t(81) = 4.91$, $p < .001$.

Experiment 2 but safety was still affected. It seems likely, then, that the reduction in safety was due to the size of the gap chosen and the amount of the gap used rather than the participants walking slower while talking.

Two general measures of impairment were also recorded; the number of potentially safe gaps left prior to the one chosen, and the number of cautious crossings. Significantly more safe gaps were left when participants were conversing for Experiment 1 (a mean of 0.36 more safe gaps left), although there was no difference for Experiment 2. Conversely, phone conversation was associated with more cautious crossings for Experiment 2 but not Experiment 1. Approximately 6% of the trials for Experiment 1 were cautious crossings, consistent across both phone conditions. For Experiment 2 cautious crossings increased from 9.8% for no conversation to 14.2% for phone conversation. Taken together these findings suggest that participants tended to become more cautious when engaged in the conversation task. However, how this caution was exhibited differed between the two experiments.

Finally, no significant interactions were observed between the phone task and the distance between the vans. This indicates that the secondary task does not affect the participants' use of distance information to inform their road-crossing decisions. However, the participants' use of T_A information was affected by the simulated conversation. While participants tended to favour the larger temporal gaps when not conversing they did not do so when engaged in the conversation task. The effect the simulated conversation has on the information people use when deciding when to cross a road could be further examined using the kind of state space analysis used by Flach and colleagues. While this analysis will be conducted for any future publications, for present purposes the finding that people's attention to T_A information is impaired while engaged in the conversation task is sufficient.

Experiment 3

As with Experiment 1 walking speeds were affected by the simulated conversation for Experiment 3, participants walking 0.075 m/s slower when engaged in the conversation task compared to no conversation. Reaction times were also affected, with the participants reacting .5 s slower when engaged in conversation. The degree of impairment was almost identical to the pilot study, suggesting that these were robust findings.

Experiment 3 was designed to fix a flaw in a pilot experiment. For the pilot study the participants were given the instruction to turn to the right before crossing. This may have overridden the effect of the simulated conversation on the participants' direction of attention, as no conversation-related difference was found for the participants' tendency to look for traffic. For Experiment 3 the instruction was changed to "Please treat the road as an actual road". With this instruction participants did tend to look less often when engaged in conversation, looking for traffic on 97% of the no-conversation trials but only 93% of the conversation trials. A closer examination of the data indicated that this effect was due to a small number of the participants, most of the no-looking events coming from three participants. The no looking trials were predominantly found early in the experiment, 35 of the 38 occurrences being recorded in the first 10 trials. Combined, these findings indicated that the simulated conversation also negatively affected the participant's crossing behaviours for this crossing situation.

Implications

It should be noted that these are possibly the first experiments to examine the effect of cell phone conversation on pedestrian behaviour. The majority of the research into cellular-phone related impairment has involved driving behaviours. This means that the results of the current experiments are not directly comparable to the previous body of research. However, some of the findings from these studies may be seen as analogous to the findings from the driving studies and will be discussed as such.

Combined, the results from these experiments indicate that the cell phone conversation task had a negative impact on participant performance. In terms of safety, participants had smaller safety margins and more unsafe crossings when engaged in the conversation task. This is consistent with the studies that suggest there is an increase in unsafe outcomes (e.g. Graham & Carter, 2001; Haigney et al., 2000; Hunton & Rose, 2005). This is also the most important finding. If people's road-crossing behaviours in the VE are related to their behaviours in the actual environment then talking on a phone while walking may place them at risk. As noted earlier, the potential consequences of a lapse of attention for pedestrians are severe; being hit by a vehicle incurs a high chance of serious injury or death, especially given the general speed of vehicles on New Zealand roads, even urban roads. The main conclusion that can be drawn from this research, then, is that a secondary task negatively affects road-crossing behaviours. It would be recommended, then, that pedestrians should exercise extra caution if they are attempting to cross a road while using their cell phone, or preferably they should avoid conversing at all while crossing a road.

Participants were also slightly more likely to fail to look before crossing the road for Experiment 3. This could be characterised as impairing the attainment of Level 1 SA (Endsley, 1995), the participants failing to be aware of their environment and the situation. However, given the nature of this specific finding no firm conclusions can be drawn about how the conversation affected the participants' tendency to look for traffic.

Gap choice was poorer in the conversation condition, demonstrated by participants choosing smaller temporal gaps. This seems to replicate the finding of Horswill and McKenna (1999), who found that riskier gaps were selected by participants in a dual-task condition, albeit with a different task (road-crossing for the current research, merging with traffic for Horswill and McKenna). Cooper and Zheng (2002) also found that participants' gap-choices were impaired, but in a more specific way than was found in the current research. Participants in the dual-task condition when the road surface was wet failed to take the road surface into account, choosing gaps when the road surface was wet that were equivalent to those that were selected when the surface was dry. This is similar to the finding from Experiments 1 and 2 regarding the participants' use of T_A information for informing their gap choice decisions, with the participants preferring the larger gaps only when not conversing. This point will be addressed again in the next section, as well as in the design extensions section.

As well as choosing smaller gaps while conversing, the participants also used less of the gap they did choose. This may indicate that not only was their choice of gap poorer while engaged in the conversation task but that their decision process was also impaired; a greater proportion of the gap passed before their decision to cross was made. However, it is also possible that the two findings are related in a more straight-forward way. Given that the chosen gaps were smaller, taking the same amount of time to make the decision to cross in the gap would produce a similar effect. For instance, if a person took 0.5 s to decide to cross in a specific gap and the gap they chose was 2 s long, their fraction of gap used would be 75%. With the same decision time and a smaller gap, say 1.8 s, this value would be 72.2%, a smaller value with no increase in decision time. An examination of the correlations[20] between these two variables suggests that, although this explanation may account for some of the apparent impairment it probably

[20] The correlations, within each cell, for both experiments varied from weak (r = .07, from Experiment 1) to quite strong (r = .51, from Experiment 2), but tended to be approximately r = .2.

does not account for all of it. Overall it seems fairly safe to say that participants' gap choice judgements were impaired by the conversation task.

Walking speed was the only variable measured across all of the experiments, and showed clear conversation-related impairment for all but one of the experiments. For Experiment 2 there was no difference in walking speeds between the two phone conditions. The exact reason for why Experiment 2 was different is unclear. A closer examination of the data indicated that for the older group walking speeds decreased by 0.026 m/s for the phone task, whereas there was a 0.021 m/s increase for the younger group while they were engaged in the conversation task. This may explain in part why no effect was found, given that the increase and decrease nearly cancel each other out. There does not seem to be an obvious answer for why only the older group slowed down, nor why the younger group walked faster, when talking. The age difference will be discussed in more detail shortly, but currently this finding appears to be inexplicable.

While the effect of the phone task on walking speeds for the remaining three experiments seemed clear, another possibility was considered. The impairment may not have been due solely to the phone conversation but rather could have been due to an interaction between the phone task and VR exposure. The additional demands of the phone task, combined with the limited visibility of the VE, may have reduced the participants walking speeds if the participants were worried about tripping. The results from Experiment 4 indicated that the walking speed impairment was primarily due to the conversation task as participants' walking speeds were also reduced when they were not immersed in the VE.

Walking is a well practised task and mostly automatic task (Wickens & Hollands, 2000), and therefore it is plausible that it would not be affected by a secondary task. It does not seem unreasonable to assume that most people will have practised walking to a greater degree than they have practised driving. While caution is required when extrapolating the results from these experiments to driving tasks, it is feasible that a less well-practised task, such as driving, may be more affected by a secondary task. Also related to this point is that, evolution-wise, humans have had much longer to adapt to the demands of walking then we have to the demands of driving (Rumar, 1990). These findings, though, do not give any indication of the magnitude or type of impairment that could be produced.

Participants probably reduced their walking speeds as a way of compensating for the increased task demands induced by the phone task, possibly analogous to the speed reductions that have been found in driving studies (e.g. Alm & Nilsson, 1994; Engström et al., 2005; Haigney et al., 2000; Rakauskas et al., 2004). Walking slower, as with driving slower, reduces the consequences of a mistake, any potential impact with a moving or stationary being lessened. Alternatively, the type of conversation may affect how walking speeds are affected. For instance, a conversation that requires more intense concentration may slow walking speeds. A conversation that the participants are enthusiastic about may increase walking speeds. For instance, Brodsky (2002) found that people drove faster in a simulator when listening to fast-paced music, so it is possible that other activities that increase arousal may also increase speeds. Using two different forms of conversation in future studies would assist in determining if this is the case.

The Information Used to Inform Gap Choices

This section details Experiments 1 and 2 only. The potential information used by the participants to inform their crossings was not studied for Experiments 3 and 4.

Consistent with previous experiments (Clancy et al., 2006; Connelly et al., 1998; Connelly et al., 1996; Lamb, 2004; Murray, 2003; Owen, Simpson, & Murray, 2002; Oxley et al., 2005; Plumert et al., 2004; Simpson & Owen, 2002) participants seemed to use irrelevant distance information when making their road-crossing judgements. In terms of the measures of safety, road-crossings were safest at the 40-m initial distances for both experiments, with the longer initial distances producing lower margins of safety. For near misses there was no significant distance effect for Experiment 2, although there was a trend towards more near misses occurring at longer distances and the effect sizes were similar between Experiments 1 and 3 (Cohen's fs of 0.29 and 0.25 respectively). This effect was significant for Experiment 1. The same trend was evident for the percentage of collisions for both experiments.

There was a tendency for walking speeds to be slower at longer initial distances for Experiment 1, although the effect did not reach significance. This effect was significant for Experiment 2, with walking speeds being faster at the 40-m distance than at either the 50- or 60-m distances. This is congruent with the findings for the margin of safety, faster walking speeds most likely producing safer crossings. However, as noted earlier correlations between the margin of safety and walking speeds for each distance / conversation combination were not consistently strong. This would suggest that other factors were more instrumental in the decrease in safety.

Participants tended to choose smaller gaps when the initial distance between the vehicles was longer. This effect only tended towards significance for Experiment 2, but both experiments had a similar magnitude for the effect (Cohen's $f \approx .27$). Although this suggests the effect was reasonably robust, it should be reiterated that although there was a consistent trend for smaller gaps to be chosen as the distance increased for Experiment 1, for Experiment 2 the 50-m initial distance was associated with the smallest gaps selected. Further, less of the gap was used for both experiments as the distance between the vehicles increased. Both of these variables correlated consistently well with the margin of safety, suggesting that changes in the size of the gap selected and how much of that gap is used determine the safety of a crossing.

The effect of distance was also evident in the two measures not related to safety. Fewer safe gaps were left, and fewer cautious crossings occurred, at longer initial distances. This was consistent across both experiments. These findings indicate that participants used information about the distance between the vehicles to judge the safety of a gap; participants were more willing to cross when the vans were further apart.

However, as noted earlier (page 37), it is possible that any effect of distance is confounded with the apparent size of the vehicle, closer vehicles appearing larger. What seems clearer is that the participants did attend to T_A information when making their crossing decisions, and that their attention to this information was reduced or eliminated when they were conversing. Further analyses are required to gain a deeper understanding of the information people attend to when crossing, and as previously mentioned these can take the form of state space analyses.

Implications

The finding that participants use distance information to inform crossing indicates that the greater the variability of speeds on a road the less safe the road is likely to be to cross. Within the EA learning is characterised as the education of attention (J. J. Gibson, 1966/1983). Using the heuristic that the further away the vehicle is the safer the crossing will be is likely to be appropriate in situations where vehicle speeds have little variability. For instance, 85% of the vehicles on urban roads in Canterbury were travelling at less than 58 km / h (M.o.T., 2005). With some knowledge of this, and having a conception of how long it will take to cross the road, a pedestrian would probably be able to make safe road-crossing judgements using only distance information. However, the participants also attended to T_A information, but only when they were not distracted. In contrast, the effects found for the initial distance between the vehicles were not affected by the simulated conversation. This may indicate that the people will attend to both types of information, but that distance information can be acquired in a very brief time compared to T_A information.

The findings for the measures of safety highlight the utility of VR for this kind of research. For the real-world studies where the effect of distance was examined (Connelly et al., 1998; Connelly et al., 1996) the children did not actually cross the road. This means it is difficult to determine how safe their decisions were. For the yes / no task, used by Connelly and colleagues, once the decision to accept a gap had been made there was no way to change it. For Experiments 1 and 3 participants would occasionally begin to make a crossing, then reconsider their decision and wait for a different gap. It is also possible that the verbal judgements required for the yes / no task would have been less safe than their actual crossing behaviours would have been (te Velde et al., 2005). However, given that the accident rates in the VR experiments (i.e. (Clancy et al., 2006; Murray, 2003) are higher than actual accident rates (Simpson et al., 2003), a similar argument could be made about the current research. Overall, though, VR seems to be a good way to study road-crossing behaviours as it provides a fairly naturalistic, and safe, environment for experimentation.

Age Effects

This section refers to Experiment 2 only as none of the other experiments used different age groups.

The hypotheses relating to age differences were unsupported overall. Generally, the performance of the older adults did not differ significantly from the younger adults. When it did differ, the older participants performed arguably better than the younger ones. The older group tended to allow fewer safe gaps pass before crossing and, surprisingly, also walked significantly faster than the younger participants (c.f. Ketcham & Stelmach, 2001). When just examining the differences the only area where the younger group outperformed the older group was for collisions, the younger participants being hit less often overall. Otherwise all of the differences pointed towards superior performance in the older sample. The reasons for this are unclear. One possibility is that the older participants felt they were in competition with the younger participants and attempted to do better at the task. (It is expected that most of the older participants would have known they were part of a separate group as a need for two age groups was advertised with the Department of Psychology, and the need for a specific age range may have also been suspicious.) However, when examining the mean walking speeds for the groups it appears that the older group was walking nearly as quickly as the participants in Experiment 1 (means of 1.93 m /s for the older group and 1.99 m / s for Experiment 1, pooling over conditions). In contrast, the younger group was walking slower, only 1.72 m / s on average. This may indicate that the older group was not performing better than expected, but rather that the younger group was performing worse. One of the older participants also noted that she did not feel that she was in competition with the younger group, so there may be another reason for this finding.

It was possible that the participants who were excluded from most of the analyses for having too many cautious crossings may have been poorer performers overall. Four older adults were excluded, compared to one younger adult. If the excluded participants were poorer performers this may have advantaged the older group compared to the younger group. For instance, if the excluded older participants had slower mean walking speeds than the included older participants then, by excluding them, the overall older group mean would be inflated. A visual comparison of the means for the included and excluded older adults indicates that the excluded older adults did walk slower than the remaining older participants. However, the mean for the older participants (excluding the one completely cautious crosser) was still higher than the mean of all of the younger participants, although the difference did decrease. For the number of safe gaps left the difference between the groups actually increased, indicating that the excluded older participants were allowing *fewer* safe gaps to pass than the included older participants. The excluded older adults also had higher margins of safety than the included older adults, suggesting that the excluded older participants were not performing worse overall than the included older adults.

In terms of dual-task impairment, Experiment 2 partially supports the argument that older adults are not necessarily more impaired than younger adults by a secondary task (e.g. Fernandes & Moscovitch, 2003; Salthouse et al., 1995). However, the lack of group differences in the levels of phone task impairment does not mean that such differences do not exist. For instance, it is possible that the older age group used for this experiment was too young to exhibit greater impairment than the younger group, although using an older sample may not mean that these differences will be found. The age ranges used for studies that found no additional performance cost for the dual-task performance of older adults (e.g. Fernandes & Moscovitch, 2003; Kemper et al., 2003; McCarley et al., 2001; Strayer & Drews, 2004) were similar to those studies which found differences (e.g. Lesch & Hancock, 2004; McDowd & Craik, 1988; McKnight & McKnight, 1993; Monk et al., 2004), generally around 60 to 70 years of age for the older sample and with the younger sample up to 50 years younger.

Another possibility, then, was that the phone task was not taxing enough to produce differential impairment. An examination of the distraction tasks used indicated that both groups of experiments (i.e. those that found greater impairment for older adults and those that did not) used similar tasks overall, such as general conversation-style tasks and cognitive tasks. It did not appear that the tasks used for the experiments that found increased impairment for older adults were more complex than were those used by the other studies. Future research into how age relates to dual-task impairment should use different tasks, as well as the different age groups, to examine how task complexity affects performance.

There is some evidence that older people have trouble inhibiting task-irrelevant information when performing a task (e.g. Cohn, Dustman, & Bradford, 1984; Panek, Rush, & Slade, 2001; Phillips & Lesperance, 2003; Plude & Hoyer, 1986; however, also see Kramer, Humphrey, Larish, Logan, & Strayer, 1994 for evidence that the impairment is task specific). Adding task-irrelevant information to the VE may produce a greater level of impairment in the older participants.

Older participants were also predicted to have greater variability of performance, but this hypothesis was not well supported. There was a tendency for variability in the older sample to be greater than that for the younger sample, but only in select cases. It was possible that the differences in the variances could be explained by which group had the higher mean score for the specific variable. Overall, this means that no firm conclusions can be drawn from this data set regarding how age affects the variability of performance.

Implications

The findings from Experiment 2 suggest that the two age groups did not differ in the level of impairment produced by the phone task, nor in terms of general performance. Overall, it seems that the main concern in relation to pedestrians is whether they are using a phone or not, not how old the phone user is. Different tasks, an older age group, or increased sample sizes may produce different results, so this is only a tentative conclusion.

Presence

A number of the findings suggest that the participants had some degree of presence. First, there are the findings for cautious crossings from Experiments 1 and 2. If the participants did not feel immersed in the simulation, at least to an extent, it is unlikely that any cautious crossings would have occurred. An unwillingness to cross suggests an unwillingness to be hit, even though the only negative outcome from a virtual collision was the sound of glass breaking.

The average walking speeds across the experiments also provides an indication of presence. Pooling across conditions, the mean walking speeds for Experiments 3 and 4 were 1.02 m / s, and 0.94 m / s respectively. The mean walking speeds for Experiments 1 and 2 were 2.00 m / s and 1.83 m / s respectively, considerably higher than for the other experiments. For Experiments3 and 4 the participants simply walked across the road. There were no cars coming, and hence no need to rush. For the other experiments, however, the participants had to cross the road without being hit. The higher walking speeds mean that the participants were taking the situation seriously. If they were not there is no reason to suspect the participants in the gap choice experiments would walk faster than those in the other studies.

As mentioned in Chapter 3, pi numbers may provide a method for measuring presence (Stappers et al., 1999). The margins of safety found for Experiments 1 and 2, both while conversing (64.45% and 66.25% respectively) and not conversing (73.54% and 74.36% respectively), are similar to those found for the ‘standstill’ value from Oudejans et al. (1996), 66%. This, as suggested earlier (see page 34), may mean that participants are behaving in the VE as they would when crossing an actual road. If this is the case it would seem to indicate that the simulation promoted a good level of presence, with the information provided in the simulation matching that provided in the AE. However, further experimentation is needed to confirm if this was the case.

Both the walking speed finding and the finding for cautious crossings may only indicate that the participants wanted to do well on the task, rather than indicating that they experienced presence. However, in a previous forced-choice experiment, in which the current author was the experimenter (Owen et al., 2002), it was noted that one participant’s walking speed did not vary at all across conditions. The trend was for participants to walk faster when the vehicles were closer or larger. This suggests that this participant did not experience presence, and did not take the simulation seriously. This is the only example of this kind of behaviour that has been observed by the author, supporting the argument that the remaining participants were likely to have been experiencing some degree of presence.

One final piece of evidence is anecdotal. On occasion the vans would not disappear at the end of a trial (gap-choice only). This would mean that there were occasions where the participants’ path back to the start would be blocked by a vehicle. In this situation participants were hesitant to walk through the van, even though they knew that there was nothing physically blocking their path in the room. This is similar to the anecdote mentioned by Stappers et al. (1999), where a participant who claimed a VR table did not look real then attempted to use it for support when standing. Although the participants in the gap choice *knew* the van was not real, they behaved in the same way they would if it were real. When these findings are taken together it seems reasonable to say that the participants did experience presence.

Theoretical Implications

Overall, the theoretical implications for this research regarding dual-task impairment are limited as these experiments were not designed to test any of the theories of divided attention. However, it is possible to make some general observations. This research provides more support to the finding that task performance is worse under dual-task conditions (Tombu & Jolicœur, 2005; Tsang et al., 1995), although it does not assist with understanding the underlying mechanisms. It also highlights some of the issues of multiple resource theory; as noted earlier, when taken strictly as written the theory would predict no task interference if the two tasks utilise different modalities (Strayer, Drews, & Johnston, 2003; Strayer & Johnston, 2001; Wickens, 1984). The *verbal* phone task and *spatial* road-crossing task used here would seem to access two separate modalities. Although multiple resource theory does provide a reasonable framework for conceptualising how two tasks may interact, it is limited when it comes to making fine-grained predictions (Heuer, 1996). For example, while it is possible to predict that a voice-controlled in-car system and a cell phone conversation (verbal) will have less effect on driving behaviours than trying to operate a laptop (spatial), the theory does not assist in making predictions about which of the two verbal tasks will be more impairing.

One interesting anecdotal finding may relate to the bottleneck theory. It was observed that some participants would wait until they had finished giving the requested information before crossing the road, and would then cross the road almost immediately after they finished speaking. Although further experimentation would be needed to see how this finding relates to the general concept of a bottleneck, it at least suggests that, for some participants, there was a preference for performing only one task at a time.

In terms of affordance theory the impairment effect can be characterised as a change in the participants' effectivities. Participants' decision times increased, shown by the change in the percentage of the gap used for Experiments 1 and 2, and the increased RTs for 3, indicating a change in their cognitive effectivities. Conversation also affected a physical effectivity property, their walking speed. This indicates that a cognitive task may not simply affect a person's cognitive capabilities but may also affect their physical capabilities.

Limitations

Technical Limitations

The field of view provided by the HMD was quite limited compared to what is generally available to people. The field of view was only 48 degrees horizontal, compared to the general field of view available which is over 160 degrees horizontal. The HMD provides a similar field of view to that available when wearing a sweatshirt, jacket or anorak hood, and meant that the participants had reduced field of view (FOV). One of the effects of the reduced FOV is that it was impossible to have both the lamppost and the line of vehicles in view and the same time. If participants wished to look at the vehicles as they crossed they could not see the lamppost, and would hence tend to cross the road on a diagonal path rather than straight across. In this situation they would have spent more time in the line of traffic than was necessary, meaning their crossing may have been less safe. Correcting this problem would require purchasing a HMD with a greater field of view, which was prohibitively expensive. As this issue was consistent across all of the experimental conditions it seems better to simply acknowledge the limitation rather than trying to remove it.

There was no sound in the simulation aside from the noises for near misses or collisions. One possible impact of this was a reduction in participant presence (Gilkey & Weisenberger, 1995). While this may have reduced the maximum level of presence felt, participant behaviours do seem to indicate that some degree of presence was felt. One other possible problem relating to the lack of sound was that the information available to the participants was reduced. However, there is evidence that people make more accurate judgements of T_A when given only visual information compared to audio only or visual and audio information (Schiff & Oldak, 1990). Overall, the lack of sound, although possibly causing some issues, may not be a major issue given the programming requirements for adding realistic sound to the simulation.

The SSQ scores were higher than desirable. According to the norms published by Kennedy et al. (1993) this simulation produced average scores that were generally above the 90th percentile from their samples. The exact effect this may have had on the results of the experiments is unknown, but as the trials were counterbalanced it is assumed that any impairment caused by sickness should have been consistent across conditions. Although excluding some participants for Experiments 1 and 2 did affect the counterbalancing for those experiments any effect would have resulted in better performance in the conversation condition, and worse performance in the no-conversation condition. This would mean that, assuming that SS impaired performance, the differences found between the conversation conditions would be smaller than if the experiments had remained perfectly counterbalanced.

Most of the participants did not appear to suffer too badly from exposure. Only one participant requested any breaks from the VE exposure due to sickness, although a small number did mention feeling unwell post-exposure. Therefore the main concern was for participant wellbeing rather than for any effect on the results of the experiments.

Design Limitations

The phone task was not particularly naturalistic. Unlike the tasks used in some experiments (e.g. Consiglio et al., 2003; McKnight & McKnight, 1993; Strayer, Drews, & Crouch, 2003; Strayer, Drews, & Johnston, 2003), this phone task did not allow easy two-way interaction between the experimenter and participant. In general participants only responded with the information requested (i.e. how many times had they done the activity, were they willing to do the activity, and how risky they felt it was) without going into more detail. Even when they did offer more information than was strictly required there was not much opportunity for the experimenter to respond in a natural way. One major limitation, then, is that the level of impairment may not match the impairment that may occur in the natural environment. This may mean that the results from these experiments over- or underestimate the effect phone conversation would have on pedestrians.

The trial-based nature of the experiment may have affected how the conversation task altered the participants' road-crossing behaviours. The main effect of this for Experiments 1 and 2 was to limit how freely the conversation could occur; in general a new activity would be used for each trial. The effect on Experiment 3 is of more concern. The decision task for this experiment was simply go or do not go, and with each new trial being a separate instance of this task. Compare this situation to the natural situation: A person may be walking along the pavement talking on their phone and potentially fail to attend to the upcoming road. In this situation it may be less likely for a person to turn to look for traffic if they are more deeply involved in the conversation compared to the experimental task. For the experimental task they were already at the side of the road, and hence would be expected to be more aware of the potential danger than if the phone task had been in progress in a safer environment (e.g. walking along the pavement). This was not such a concern for Experiments 1 and 2; for these experiments it was presupposed that the participants were aware of the road, the behaviour of interest being their gap decisions rather than whether they attended to the oncoming vehicles.

As noted earlier, because only pedestrian behaviour was studied the findings from these experiments cannot be directly related to previous driving studies. This limits the use of these studies for informing decisions that may be made regarding any impairment in driving performance produced by concurrently talking on a cell phone, and means they provide no new information about how risky such behaviour is. The reason for not using a driving simulator was pragmatic; the simulator that was under development was unlikely to be useable within the timeframe required for this dissertation. However, the experiments do provide the first known experimental examination of how phone conversation may affect pedestrian safety.

Finally, the small sample size for Experiment 2 may have concealed between-groups differences. Larger sample sizes would have stabilised the means, as well as increasing the experiment’s power. However, due to time constraints and the difficulty in obtaining the older sample increasing the sample size was not a feasible option.

Design Extensions

There are two possible paths for further extensions from this research. The first is to continue studying pedestrian behaviours under dual-task conditions. Future research would use a more naturalistic form of conversation and, if feasible, a more natural road-crossing task. A dynamic simulator, such as used by Jaeger & Mourant (2001), may allow a more continuous flow of conversation compared to the discrete trial-based experiments described here. This may also enable experiments similar to Experiment 3 to be conducted with more success. The VE could also be varied, perhaps using an urban environment to more closely model the environment most of the participants would be used to. A variety of cars could be used rather than just vans. While using only one type of vehicle means that vehicle type is eliminated as a potential confound there is the risk that it may reduce participant immersion. How the type of vehicle affects road-crossing behaviours could also be studied (for example, testing whether participants would be more willing to cross in front of a mini compared to a sports car or a truck). Environmental sounds would also be beneficial. Even though audio cues may impair T_A judgements (Schiff & Oldak, 1990) it seems important to try to ensure that the information available in the VE matches the AE the task occurs in.

Although it would be desirable to conduct dual-task research in the real world for comparison with the simulation studies the risks are too high to justify it. However, some form of validation should be possible. A variation on the method used by McComas, MacKay, and Pivik (2002) may be possible. McComas et al. (2002) used coloured tags attached to children's backpacks to identify which training group they were in, and observed their real-world behaviours. Although time-consuming, research like this would assist in the understanding of how generalisable the results from these studies are. If a person's real world behaviours closely match their VE behaviours then we can be confident in generalising from the experimental findings, and less confident if their behaviours are not congruent.

The second path is to utilise the gap-choice task in a driving situation. Gap selection has been infrequently studied explicitly (e.g. Cooper & Zheng, 2002; Horswill & McKenna, 1999; Plumert et al., 2004), although it is arguably one of the most dangerous manoeuvres a driver needs to perform. Choosing a gap that does not afford crossing through means there is a risk of being struck side-on, where there is less protection for the car passengers. The basic task used for Experiments 1 and 2 would translate easily into a driving simulation, and could be made more difficult by including traffic travelling in both directions. This was a limitation for the current studies: due to the limited range of the tracking equipment only one lane could be used. A driving simulator would also allow for a more naturalistic task and conversation flow, a variety of different tasks being imbedded into each trial. Turning across traffic, merging into traffic, and car following could be combined together into a continuous driving situation.

Conclusions

Cellular phone conversation was found to alter participants' road-crossing behaviours in such a way as to reduce safety. Of most importance was the finding that the percentage of near misses and collisions increased when participants were engaged in the phone task, indicating that the participants were selecting gaps that did not afford safe crossing. Participants becoming more cautious while talking did not produce safer crossings. Conversation, however, did not appear to have a strong effect on whether participants looked before crossing the road.

The decrease in safety was most likely due to changes in the participants' cognitive effectivities, indicated by participants choosing smaller gaps to cross in and using less of the chosen gap. The increase in RTs for Experiment 3 also suggests that the participants' decision times increased due to the conversation. Walking speeds were also affected by the conversation task for all but one of the experiments, a surprising finding for two reasons. First, walking is a well practised behaviour that would be expected to occur automatically; and second, it was not expected that a cognitive task would affect a physical behaviour in such a consistent way. The change in walking speeds, however, only seemed to be a small part of the decrease in safety. It is also likely that their safety was affected by their reduced use of T_A information when engaged in the conversation task compared to the no-conversation condition.

Participants' inappropriate use of distance information to inform their road crossing decisions was again evident. This indicates that pedestrians may have a greater risk of misjudging the affordance of a gap in areas where vehicle speeds vary greatly, especially if distracted by a secondary task.

There did not seem to be any major differences between the two age groups used. The older participants' performance was generally as good as or better than the performance of the younger adults, and both groups were affected by the conversation task to a similar degree. Although suggesting that phone use affects performance at all ages equally, it is possible that adults who are older than the participants in this study may be affected more severely than younger adults.

Overall, in can be concluded that the performance of the participants in these experiments was significantly impaired by the simulated conversation, in three different situations, and this impairment was exhibited in a variety of different ways. While the magnitude of the effect of a cell phone conversation task on pedestrian behaviours in the world cannot be determined from these experiments, it would seem prudent to avoid combining conversing while trying to cross a road.

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APPENDECIES

Appendix A: The Activity Risk Taking Questionnaire

Instructions

For each activity please tick the one box out of the first three boxes that most applies to you now (Would not do, May consider doing, Would do). After this fill in the number of times you have done this activity, and how risky you perceive it to be. This means that you can be unwilling to repeat an activity having done it previously; hence you may tick “would not do” as well as putting ‘1’ under “have done”.

For the questions at the end please answer as accurately as possible. If there are multiple options please circle the most appropriate answer. If there are spaces please fill in the numbers if you know them. All answers will remain confidential and will not be linked to your name. If you are uncomfortable answering any of the questions please leave them blank.

Thank you for taking the time to help with this experiment.

Information

Age:

Circle those that apply:

Sex: Male Female

Highest Level of Study: Secondary Tertiary Postgraduate PhD

Other (specify): ______________________________

How much time per week do you spend using a computer? ___________________

How much time per week do you spend talking on a cellular phone? ___________

Glossary

River Surfing: Travelling down a river lying on a specially designed board

Sky Scraper Viewing Platform: A viewing platform that looks over the edge of a sky- scraper. Also includes glass floors, such as the one in the Sky Tower that is over the edge of the building.

Girder Crossing: A guided tour over the walkways on a harbour bridge, such as Auckland or Sydney Harbour Bridges.

Abseiling: Walking backwards down a vertical surface using ropes.

Rap jumping: Walking forwards down a vertical surface using ropes.

Activity	Have done (insert number of times)	Would not do	May consider doing	Would do	How risky is this activity? Low (1-10) high
White water rafting					
White water kayaking					
Sea kayaking					
Jet boating (passenger)					
Jet boating (driver)					
Surfing					
Wind surfing					
River surfing					
Diving/ snorkelling (shallow waters)					
Shark cage diving					
Guided shark diving at feeding time					
Looking over the edge of a sky scraper viewing platform					
Girder crossing on a harbour bridge					
Hang-gliding (tandem with instructor)					
Hang-gliding (alone)					
Sky-diving (tandem with instructor)					
Sky-diving (alone)					
Abseiling down a vertical surface					
Rap jumping down a vertical surface					
Bungy jumping					

Activity	Have done (insert number of times)	Would not do	May consider doing	Would do	How risky is this activity? Low (1-10) high
Tramping (established track)					
Caving (guided)					
Climbing Mount Taranaki					
Climbing Mount Cook (guided)					
Climbing Mount Everest (guided)					
Horse trekking					
Cycle touring					
Mountain biking					
Downhill cycle riding (streets)					
Bicycle riding (transport)					
Motorcycle riding (passenger)					
Motorcycle riding (driver)					
Car driving (general city transport)					
Go-cart driving					
Skate boarding (transport)					
Inline skating (transport)					
Helicopter flights					
Scenic flights (small aeroplane)					
Hot air ballooning (passenger)					
Skiing (controlled fields)					
Snow boarding (controlled fields)					
Luge (wheeled, not ice)					

Activity	Have done (insert number of times)	Would not do	May consider doing	Would do	How risky is this activity? Low (1-10) high
Playing contact sports					
Playing non-contact sports					
Running with the bulls in Spain					
Burnham Assault Course					
Snake Handling					

Circle those that apply:

Highest diving board used: Do not use 1 meter 3 meter 10 meter

Do you wear a seatbelt while driving? Never Sometimes Always

How often do you use a cell phone while driving?

Never Less than once a week More than once a week but not daily Daily

Have you had any tickets for speeding? Yes No

How many? __________ How much over the limit? __________km/h

Have you ever driven under the influence of a legal drug that may cause drowsiness? Yes No

Have you ever driven under the influence of alcohol? Yes No

How much over? __________

Have you ever driven under the influence of an illegal drug? Yes No

Have you had any citations for driving under the influence? Yes No

How many? _________

Have you ever been involved in an accident as a pedestrian? Yes No
as a cyclist? Yes No
as a motorist? Yes No

Are there any activities that you have done that are not listed? Please write them below.

____

____

____

Are there any other activities not listed that you feel should be listed? Please write them below.

____

____

____

Appendix B: Counterbalanced Design for Experiments 1 and 2

			Trial									
Participant			1-3	4-6	7-9	10-12	13-15	16-18	19-21	22-24	25-27	28-30
1	25	49	N	C	N	C	N	C	N	C	N	C
2	26	50	C	N	C	N	C	N	C	N	C	N
3	27	51	N	N	C	C	C	N	N	N	C	C
4	28	52	C	C	N	N	N	C	C	C	N	N
5	29	53	N	N	N	C	C	N	N	C	C	C
6	30	54	C	C	C	N	N	C	C	N	N	N
7	31	55	N	N	N	N	C	N	C	C	C	C
8	32		C	C	C	C	N	C	N	N	N	N
9	33		N	N	N	N	N	C	C	C	C	C
10	34		C	C	C	C	C	N	N	N	N	N
11	35		C	N	N	N	N	C	C	C	C	N
12	36		N	C	C	C	C	N	N	N	N	C
13	37		N	N	C	N	N	C	C	N	C	C
14	38		C	C	N	C	C	N	N	C	N	N
15	39		N	C	N	N	N	C	C	C	N	C
16	40		C	N	C	C	C	N	N	N	C	N
17	41		N	N	C	C	C	N	N	C	C	N
18	42		C	C	N	N	N	C	C	N	N	C
19	43		C	C	C	N	N	N	N	N	C	C
20	44		N	N	N	C	C	C	C	C	N	N
21	45		N	N	C	C	C	C	C	N	N	N
22	46		C	C	N	N	N	N	N	C	C	C
23	47		N	N	C	C	C	C	N	N	N	C
24	48		C	C	N	N	N	N	C	C	C	N

Notes:
N = No-conversation trial, C = Conversation trial
For Experiment 2 only 40 participants were testing in total, compared to 55 for Experiment 1

Appendix C: Information Sheet, Consent Form, and Instructions for Experiment 1

INFORMATION SHEET

Cellular Phone Impairment Road Crossing Study

You are invited to participate in an experiment that forms part of a wider program of research investigating human performance in virtual environments. The project is being carried out by Stephen Murray (phone 3642987, extension 3409; e-mail sjm144@student.canterbury.ac.nz) under the supervision of Dr Dean Owen, who can be contacted on extension 6166. He will be pleased to discuss any concerns you may have about participation in the project. In this experiment, I am interested in studying how cellular phone conversation affects the safety of an individual's road crossing behaviour. Therefore, the following experiment consists of a road crossing simulation in which you will cross one lane of a two-lane virtual road which has traffic in the near lane only. The virtual road appears in a head mounted display that you will wear for the duration of the experiment. The experiment will take about an hour, with between 35 and 45 minutes spent in the virtual world.

***Note*: Some virtual reality users experience a condition known as *simulator sickness* which is somewhat similar to motion sickness. Symptoms are variable but may include general discomfort, fatigue, headaches, dizziness, eyestrain or nausea. If you experience mild discomfort please attempt to continue. If you feel you are unable to continue then please let me know and I will stop the experiment immediately. If you feel unable to travel on your own an alternative form of transport will be arranged, at my expense.**

It is understood that by signing the attached consent form you have agreed to participate in this project and assented to publication of the findings. You are assured that in any such publication your anonymity will be preserved. It is also understood that you may withdraw from the experiment at any time, including the withdrawal of any information you have provided.

This research has been reviewed by the University of Canterbury Human Ethics Committee.

CONSENT FORM

Cellular Phone Impairment Study

I have read and understood the description of the above named project. On this basis I agree to participate as a subject in the project, and I consent to the publication of the results of the project with the understanding that anonymity will be preserved. I understood also that I may at any time withdraw from the project, including withdrawal of any information I have provided.

Signed ……………………………………

Date ……………………………………

Road Crossing Instructions: Experiment 1

You are about to take part in a road crossing simulation. You will wear a virtual reality helmet that displays a straight, flat stretch of road. You can look around by turning your head and move around by walking.

Before the experimental trials, there will be 2 trials where you will walk while wearing the helmet on the top of your head but not over your eyes. Following this there will be a block of 6 trials to familiarize you with walking around in the virtual environment, with the helmet over your eyes. There will be no traffic in these trials. When you have crossed the first lane, you will hear a verbal instruction to turn around and return to your starting position. It is important that once you have begun walking towards the tree, that you **keep walking until you hear the instruction to turn around**. Do not stop walking until you hear the instruction. At this point you should turn to your right and walk back across the road towards the street light that you will see in front of you. You can walk at whatever speed you like on the way back.

There will then be a session of 30 experimental trials consisting of a line of 11 vehicles approaching from your right creating 10 gaps of differing safety. At the beginning of each trial turn to your right to look at the approaching vehicles. Your task is to choose a safe gap to cross to the center of the road. At the end of each trial you will hear an instruction to turn around and return to the start. You may walk back to the tree at a speed that is comfortable to you. There will be no traffic on the way back. If you are nearly hit by a vehicle you will hear a horn honk, and if you are hit you will hear the sound of breaking glass.

The second session will proceed in the same manner as the first except that on half of the trials you will be engaged in conversation with the experimenter. The conversation will take the form of a questionnaire. You will be given the name of an activity and you respond with how many times you have done the activity; whether you would be willing, might be willing, or would not be willing to do the activity; and how risky you think the activity is on a scale of 1-10, 10 being very risky. You will be given some example activities before commencing the experiment for practice.

Appendix D: Simulation Sickness Questionnaire

Simulator Sickness Questionnaire

Please indicate how much each symptom is affecting you ***right now***.

General Discomfort	*None*	*Slight*	*Moderate*	*Severe*
Fatigue	*None*	*Slight*	*Moderate*	*Severe*
Headache	*None*	*Slight*	*Moderate*	*Severe*
Eyestrain	*None*	*Slight*	*Moderate*	*Severe*
Difficulty focusing	*None*	*Slight*	*Moderate*	*Severe*
Increased salivation	*None*	*Slight*	*Moderate*	*Severe*
Sweating	*None*	*Slight*	*Moderate*	*Severe*
Nausea	*None*	*Slight*	*Moderate*	*Severe*
Difficulty concentrating	*None*	*Slight*	*Moderate*	*Severe*
Fullness of head[1]	*None*	*Slight*	*Moderate*	*Severe*
Blurred vision	*None*	*Slight*	*Moderate*	*Severe*
Dizzy (eyes open)	*None*	*Slight*	*Moderate*	*Severe*
Dizzy (eye closed)	*None*	*Slight*	*Moderate*	*Severe*
Vertigo[2]	*None*	*Slight*	*Moderate*	*Severe*
Stomach awareness	*None*	*Slight*	*Moderate*	*Severe*
Burping	*None*	*Slight*	*Moderate*	*Severe*

[1]Fullness of head refers to an awareness of pressure within the head.

[2]Vertigo refers to a loss of orientation with respect to vertical or upright.

Appendix E: ANOVA Results and Effect Sizes (Experiments 1 and 2)

Table 11. ANOVA tables and effect sizes for Experiments 1 and 3. The main effect of Group, and any interaction between Group and Phone and / or Distance, only applies to Experiment 2.

Margin of Safety

	Experiment 1				Experiment 2			
	df	F	p	Cohen's f	df	F	p	Cohen's f
Group (G)					1	0.95	0.336	0.16
Error					33			
Phone (P)	1	12.11	0.001	0.49	1	8.21	0.007	0.48
P*G					1	0.15	0.705	0.06
Error	49				33			
Distance (D)	2	43.90	0.000	0.94	2	28.92	0.000	0.91
D*G					2	0.29	0.748	0.09
Error	98				66			
P*D	2	1.14	0.324	0.15	2	0.29	0.748	0.09
P*D*G					2	0.07	0.929	0.05
Error	98				66			

Near Misses

	Experiment 1				Experiment 2			
	df	F	p	Cohen's f	df	F	p	Cohen's f
Group (G)					1	0.79	0.381	0.15
Error					33			
Phone (P)	1	4.36	0.042	0.30	1	3.69	0.063	0.32
P*G					1	0.07	0.794	0.04
Error	49				33			
Distance (D)	2	4.26	0.017	0.29	2	2.12	0.128	0.25
D*G					2	0.04	0.962	0.03
Error	98				66			
P*D	2	1.38	0.255	0.17	2	0.82	0.447	0.15
P*D*G					2	3.82	0.027	0.33
Error	98				66			

Collisions

	Experiment 1				Experiment 2			
	df	F	p	Cohen's f	df	F	p	Cohen's f
Group (G)					1	1.19	0.283	0.18
Error					33			
Phone (P)	1	3.92	0.053	0.28	1	6.14	0.018	0.42
P*G					1	0.18	0.673	0.07
Error	49				33			
Distance (D)	2	15.21	0.000	0.55	2	9.72	0.000	0.53
D*G					2	0.17	0.842	0.07
Error	98				66			
P*D	2	0.14	0.872	0.05	2	0.95	0.391	0.16
P*D*G					2	1.51	0.229	0.21
Error	98				66			

Walking Speed

	Experiment 1				Experiment 2			
	df	F	p	Cohen's f	df	F	p	Cohen's f
Group (G)					1	5.33	0.027	0.39
Error					33			
Phone (P)	1	6.66	0.013	0.36	1	0.02	0.898	0.02
P*G					1	1.33	0.257	0.19
Error	49				33			
Distance (D)	2	2.37	0.099	0.22	2	6.68	0.002	0.44
D*G					2	0.19	0.831	0.07
Error	98				66			
P*D	2	0.06	0.944	0.03	2	0.08	0.927	0.05
P*D*G					2	1.85	0.166	0.23
Error	98				66			

Total Time Available to Cross

	Experiment 1				Experiment 2			
	df	F	p	Cohen's f	df	F	p	Cohen's f
Group (G)					1	1.41	0.244	0.20
Error					33			
Phone (P)	1	8.16	0.006	0.40	1	9.36	0.004	0.52
P*G					1	2.11	0.155	0.25
Error	49				33			
Distance (D)	2	3.75	0.027	0.27	2	2.60	0.082	0.27
D*G					2	0.91	0.406	0.16
Error	98				66			
P*D	2	1.37	0.259	0.17	2	0.47	0.626	0.12
P*D*G					2	0.56	0.576	0.13
Error	98				66			

Percentage of Available Gap Used

	Experiment 1				Experiment 2			
	df	F	p	Cohen's f	df	F	p	Cohen's f
Group (G)					1	0.02	0.886	0.02
Error					33			
Phone (P)	1	5.27	0.026	0.32	1	6.48	0.016	0.43
P*G					1	0.26	0.614	0.09
Error	49				33			
Distance (D)	2	88.08	0.000	1.33	2	48.57	0.000	1.18
D*G					2	0.21	0.814	0.08
Error	98				66			
P*D	2	0.08	0.927	0.04	2	0.07	0.935	0.04
P*D*G					2	0.06	0.940	0.04
Error	98				66			

Safe Gaps Left

	Experiment 1				Experiment 2			
	df	F	p	Cohen's f	df	F	p	Cohen's f
Group (G)					1	4.03	0.053	0.34
Error					33			
Phone (P)	1	14.69	0.000	0.54	1	1.66	0.206	0.22
P*G					1	0.00	0.972	0.01
Error	49				33			
Distance (D)	2	39.89	0.000	0.89	2	26.78	0.000	0.87
D*G					2	2.34	0.104	0.26
Error	98				66			
P*D	2	0.27	0.764	0.07	2	1.53	0.224	0.21
P*D*G					2	0.16	0.849	0.07
Error	98				66			

Cautious Crossings

	Experiment 1				Experiment 2			
	df	F	p	Cohen's f	df	F	p	Cohen's f
Group (G)					1	2.22	0.145	0.24
Error					38			
Phone (P)	1	0.00	0.985	0.00	1	6.33	0.016	0.43
P*G					1	0.60	0.444	0.12
Error	54				38			
Distance (D)	2	8.76	0.000	0.40	2	8.37	0.001	0.49
D*G					2	0.43	0.655	0.10
Error	108				76			
P*D	2	0.72	0.489	0.11	2	1.87	0.162	0.23
P*D*G					2	0.08	0.920	0.05
Error	108				76			

Appendix F: Means, standard deviations, and ranges for Experiment 1

Table 12. The means, standard deviations, and ranges for the dependent variables and conditions Experiment 1

Dependent Variable	Conversation Condition	Initial Distance	Mean	SD	Range
Margin of Safety	No	40	92.14	36.26	156.56
	No	50	70.51	27.14	114.63
	No	60	57.96	26.00	138.43
	Yes	40	77.99	32.29	144.55
	Yes	50	62.02	27.74	119.96
	Yes	60	53.33	29.66	148.15
Near Misses	No	40	14	17.26	60
	No	50	22	18.76	75
	No	60	24.7	18.91	75
	Yes	40	23.57	22.66	80
	Yes	50	22.8	21.76	100
	Yes	60	29.2	22.57	80
Collisions	No	40	1.6	5.48	20
	No	50	6	10.88	40
	No	60	9.6	14.14	40
	Yes	40	3.6	7.76	20
	Yes	50	9.6	12.93	40
	Yes	60	12	17.14	60
Walking Speed	No	40	2.03	0.38	1.59
	No	50	2.02	0.36	1.54
	No	60	2.00	0.32	1.29
	Yes	40	1.99	0.35	1.55
	Yes	50	1.98	0.32	1.37
	Yes	60	1.95	0.33	1.43
Percentage of Gap Available to Use	No	40	79.82	7.91	33.80
	No	50	74.88	8.04	33.16
	No	60	70.58	8.25	34.39
	Yes	40	78.31	8.94	43.10
	Yes	50	73.34	8.34	28.55
	Yes	60	69.54	8.86	37.59
Total Time Available to Cross	No	40	2.54	0.48	2.39
	No	50	2.40	0.45	1.83
	No	60	2.35	0.41	1.62
	Yes	40	2.36	0.48	1.95
	Yes	50	2.30	0.43	1.94
	Yes	60	2.32	0.44	2.00
Safe Gaps Left	No	40	1.60	1.12	4.4
	No	50	1.02	0.83	4.4
	No	60	0.77	0.90	4.75
	Yes	40	1.99	1.21	5.2
	Yes	50	1.40	1.09	5.4
	Yes	60	1.06	0.88	3.67
Cautious Crossings	No	40	0.42	1.12	5
	No	50	0.25	0.99	5
	No	60	0.29	0.94	5
	Yes	40	0.45	1.24	5
	Yes	50	0.29	0.98	5
	Yes	60	0.2	0.80	5

Appendix G: Experiment 2 Instructions

Road Crossing Instructions: Experiment 2

You are about to take part in a road crossing simulation. You will wear a virtual reality helmet that displays a straight, flat stretch of road. You can look around by turning your head and move around by walking.

Before the experimental trials, there will be 2 trials where you will walk while wearing the helmet on the top of your head but not over your eyes. Following this there will be a block of 6 trials to familiarize you with walking around in the virtual environment, with the helmet over your eyes. There will be no traffic in these trials. When you have crossed the first lane, you will hear a verbal instruction to turn around and return to your starting position. It is important that once you have begun walking towards the lamppost you **keep walking until you hear the instruction to turn around**. Do not stop walking until you hear the instruction. At this point you should turn to your right and walk back across the road towards the street light that you will see in front of you. You can walk at whatever speed you like on the way back.

There will then be 15 practice trials consisting of a line of 11 vehicles approaching from your right creating 10 gaps of differing safety. At the beginning of each trial turn to your right to look at the approaching vehicles. Your task is to choose a safe gap to cross to the centre of the road. At the end of each trial you will hear an instruction to turn around and return to the start. You may walk back to the tree at a speed that is comfortable to you. There will be no traffic on the way back. If you are nearly hit by a vehicle you will hear a **horn honk**, and if you are hit you will hear the **sound of breaking glass**.

Following these will be an additional 30 trials, and on half of these trials you will be engaged in conversation with the experimenter. The conversation will take the form of a questionnaire. You will be given the name of an activity and you respond with how many times you have done the activity; whether you would be willing, might be willing, or would not be willing to do the activity; and how risky you think the activity is on a scale of 1-10, 10 being very risky. You will be given some example activities before commencing the experiment for practice.

Appendix H: Means, standard deviations, and ranges for Experiment 2

Table 13. Means, standard deviations, and ranges for the dependent variables, conditions, and age groups for Experiment 2.

Dependent Variable	Conversation Condition	Initial Distance	Younger Mean	Younger SD	Younger Range	Older Mean	Older SD	Older Range
Margin of Safety	No	40	84.43	33.61	136.39	97.72	54.69	200.51
	No	50	63.77	26.52	96.99	73.12	36.88	139.93
	No	60	61.13	31.34	120.62	65.97	37.99	160.51
	Yes	40	73.45	29.49	119.82	86.09	38.89	146.81
	Yes	50	54.96	28.68	107.70	65.76	46.76	169.95
	Yes	60	53.38	33.75	157.72	63.89	42.25	180.80
Near Misses	No	40	15.53	18.48	50	12.08	15.44	40
	No	50	18.95	16.96	40	10	16.33	60
	No	60	13.68	17.70	60	19.38	18.06	50
	Yes	40	15.61	21.49	66.67	11.88	16.01	50
	Yes	50	18.51	17.54	60	23.33	17.51	50
	Yes	60	27.63	16.53	60	16.56	13.26	40
Collisions	No	40	1.05	4.59	20	4.06	8.80	25
	No	50	4.21	10.71	40	11.88	16.01	50
	No	60	8.42	12.14	40	10	14.61	40
	Yes	40	3.16	10.03	40	4.06	8.80	25
	Yes	50	13.95	23.25	80	14.17	21.34	66.67
	Yes	60	10.44	11.70	33.33	17.5	17.32	50
Walking Speed	No	40	1.76	0.33	1.22	1.96	0.21	0.82
	No	50	1.70	0.31	1.17	1.94	0.15	0.62
	No	60	1.68	0.33	1.15	1.94	0.24	1.03
	Yes	40	1.76	0.33	1.10	1.97	0.26	1.04
	Yes	50	1.71	0.35	1.29	1.92	0.22	0.84
	Yes	60	1.72	0.34	1.17	1.87	0.23	0.84
Percentage of Gap Available to Use	No	40	75.96	5.85	19.29	76.10	7.87	26.46
	No	50	70.64	5.31	18.57	70.25	8.26	27.77
	No	60	68.04	7.86	29.08	68.21	9.98	40.58
	Yes	40	74.03	6.98	31.69	75.55	6.04	21.15
	Yes	50	68.63	7.74	30.88	68.49	9.57	33.59
	Yes	60	66.15	7.43	31.45	66.64	8.31	26.48
Total Time Available to Cross	No	40	3.08	0.72	3.33	2.89	0.58	2.17
	No	50	3.04	0.78	3.13	2.78	0.56	2.38
	No	60	3.13	0.78	2.83	2.70	0.54	2.21
	Yes	40	2.93	0.65	2.55	2.80	0.60	2.05
	Yes	50	2.81	0.74	2.74	2.60	0.56	2.12
	Yes	60	2.90	0.58	2.23	2.74	0.57	2.48
Safe Gaps Left	No	40	3.10	1.38	5.2	1.95	1.81	6.5
	No	50	1.88	1.60	6.2	1.47	1.64	6
	No	60	1.38	1.06	3.6	0.82	0.96	3.25
	Yes	40	2.53	1.32	4.4	1.52	1.36	5.5
	Yes	50	1.66	1.08	3.6	1.32	1.41	4.67
	Yes	60	1.40	1.50	6	0.69	1.09	4.25
Cautious Crossings	No	40	0.4	0.68	2	1	1.62	5
	No	50	0.2	0.62	2	0.75	1.37	5
	No	60	0.1	0.45	2	0.55	1.39	5
	Yes	40	0.85	1.50	5	1.35	1.84	5
	Yes	50	0.35	0.67	2	0.85	1.50	5
	Yes	60	0.3	0.66	2	0.55	1.19	5

Appendix I: Experiment 3 Counterbalanced Design

Participant			**Trials**							
			1-5	6-10	11-15	16-20	21-25	26-30	31-35	36-40
1	9	17	C	N	C	N	C	N	C	N
2	10	18	N	C	N	C	N	C	N	C
3	11	19	C	C	N	N	C	C	N	N
4	12	20	N	N	C	C	N	N	C	C
5	13		C	C	N	N	N	N	C	C
6	14		N	N	C	C	C	C	N	N
7	15		C	C	C	C	N	N	N	N
8	16		N	N	N	N	C	C	C	C

Notes:
N=No-Conversation trial, C = Conversation trial

Appendix J: Experiment 3 Vehicle Trials

	Van Trial			
Participant	First	Second	Third	Fourth
1	5	15	26	37
2	4	12	28	39
3	3	11	23	37
4	9	14	28	40
5	2	17	22	35
6	2	15	24	36
7	6	20	25	38
8	2	16	21	34
9	8	13	29	35
10	5	16	23	40
11	5	12	29	39
12	2	14	22	36
13	6	15	23	33
14	5	14	22	35
15	6	12	25	40
16	8	13	23	34
17	4	16	29	35
18	3	19	24	39
19	5	15	27	35
20	3	20	25	38

Appendix K: Experiment 3

Road Crossing Instructions: 3

You are about to take part in a road crossing simulation. You will wear a virtual reality helmet that displays a straight, flat stretch of road. You can look around by turning your head and move around by walking.

The experiment will consist of 40 road crossing trials. Treat the road as you would a real road. In some trials a line of vehicles will be present and in others there will be none. Cross the road immediately if there are no vehicles, or wait until the vehicles have passed. When you reach the centre of the road you will hear an instruction to turn around and return to the start. Please keep walking until you hear this instruction. There will be no traffic on the way back and you can walk at whatever speed you like. It is important that once you have begun walking towards the tree you **keep walking until you hear the instruction to turn around**. Do not stop walking until you hear this instruction. At this point you should turn to your right and walk back across the road towards the street light that you will see in front of you.

On some trials you will be engaged in conversation with the experimenter. The conversation will take the form of a questionnaire. You will be given the name of an activity and you respond with how many times you have done the activity; whether you would be willing, might be willing, or would not be willing to do the activity; and how risky you think the activity is on a scale of 1-10, 10 being very risky. You will be given some example activities before commencing the experiment for practice.

Appendix L: Experiment 3 Demographic Sheet

Information

Age:

Circle those that apply:

Sex: Male Female

Highest Level Studied: Secondary Tertiary Postgraduate PhD

Other (specify): ______________________________

How much time per week do you spend using a computer? ________________

How much time per week do you spend talking on a cellular phone? ________

Do you wear a seatbelt while driving? Never Sometimes Always

How often do you use a cell phone while driving?
Never Less than once a week More than once a week but not daily Daily
Do not drive

Have you had any tickets for speeding? Yes No How many? __________
How much over the limit? __________km/h

Have you ever driven under the influence of a legal drug that may cause drowsiness? Yes No

Have you ever driven under the influence of alcohol? Yes No
How much over? __________

Have you ever driven under the influence of an illegal drug? Yes No
Have you had any citations for driving under the influence? Yes No
How many? _________

Have you ever been involved in an accident as a pedestrian? Yes No
as a cyclist? Yes No
as a motorist? Yes No

Appendix M: Counterbalanced Design for Experiment 4

Participant	**Trials**			
	1-5	6-10	11-15	16-20
1	**A / C**	**A / N**	**V / C**	**V / N**
2	**A / N**	**A / C**	**V / N**	**V / C**
3	**A / C**	**A / N**	**V / C**	**V / N**
4	**V / N**	**V / C**	**A / N**	**A / C**
5	**V / C**	**V / N**	**A / C**	**A / N**
6	**V / N**	**V / C**	**A / N**	**A / C**
7	**A / N**	**A / C**	**V / N**	**V / C**
8	**A / C**	**A / N**	**V / N**	**V / C**
9	**A / N**	**A / C**	**V / C**	**V / N**
10	**V / C**	**V / N**	**A / C**	**A / N**
11	**V / C**	**V / N**	**A / N**	**A / C**
12	**V / N**	**V / C**	**A / C**	**A / N**

Note: A = Actual Environment, V = Virtual Environment, C = Conversation Trial, N = No-conversation Trial.

Appendix N: Information Sheet for Experiment 4

INFORMATION SHEET

Cellular Phone Impairment Road Crossing Study

You are invited to participate in an experiment that forms part of a wider program of research investigating human performance in virtual environments. The project is being carried out by Stephen Murray (phone 3642987, extension 3409; e-mail sjm144@student.canterbury.ac.nz) under the supervision of Dr Zhe Chen, who can be contacted on extension 7179. She will be pleased to discuss any concerns you may have about participation in the project. In this experiment, I am interested in studying how cellular phone conversation affects the safety of an individual's road crossing behaviour. Therefore, the following experiment consists of a road crossing simulation in which you will cross one lane of a two-lane virtual road which has traffic in the near lane only. The virtual road appears in a head mounted display that you will wear for the duration of the experiment. The experiment will take about 15 minutes, with between 5 and 10 minutes spent in the virtual world.

***Note*: Some virtual reality users experience a condition known as *simulator sickness* which is somewhat similar to motion sickness. Symptoms are variable but may include general discomfort, fatigue, headaches, dizziness, eyestrain or nausea. If you experience mild discomfort please attempt to continue. If you feel you are unable to continue then please let me know and I will stop the experiment immediately. If you feel unable to travel on your own an alternative form of transport will be arranged, at my expense.**

It is understood that by signing the attached consent form you have agreed to participate in this project and assented to publication of the findings. You are assured that in any such publication your anonymity will be preserved. It is also understood that you may withdraw from the experiment at any time, including the withdrawal of any information you have provided.

This research has been reviewed by the University of Canterbury Human Ethics Committee.

Appendix O: Glossary of Abbreviations

ADHD	Attention Deficit Hyperactivity Disorder
AE	Actual Environment
ANOVA	Analysis of Variance
ARTQ	Activity Risk Taking Questionnaire
EA	Ecological Approach
FOV	Field of View
HMD	Head-Mounted Display
NASA-TLX	NASA Task Load Index
RT	Reaction Time
SD	Standard Deviation
SS	Simulation Sickness
SSQ	Simulation Sickness Questionnaire
T_A	Time-to-Arrival
VE	Virtual Environment
VR	Virtual Reality