

**Examining How Different Methodological
Approaches Impact Safety Outcome Effects in
Child Pedestrian Research: Comparing Typical
and Novel Measurement Approaches in Virtual
Reality**

by

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ABSTRACT

EXAMINING HOW DIFFERENT METHODOLOGICAL APPROACHES IMPACT SAFETY OUTCOME EFFECTS IN CHILD PEDESTRIAN RESEARCH: COMPARING TYPICAL AND NOVEL MEASUREMENT APPROACHES IN VIRTUAL REALITY

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There has been a great deal of academic research aimed at understanding and preventing child pedestrian injury. Many varied methodologies have been employed with the goal of designing simulations that produce rigorous assessment of children's behaviours without putting children at risk of pedestrian injury. Most research has assessed children's pre-crossing decision making and extrapolated crossing outcome measures from estimates of mean walking speed. This study explores the nature and extent of measurement bias that is introduced when average walking speed is used to produce estimates of outcomes versus measuring actual in-road behaviour directly. Using a within-subjects design and a highly immersive virtual reality pedestrian simulator, both measures were taken. Comparisons based on regression models revealed the extent of differences in results produced by measurement bias. Results indicated that measurement bias is produced when average walking speed is used such that hits and high risk crossings are over-estimated and missed opportunities are under-estimated, resulting in an overall over-estimate of children's risk for pedestrian injury. Results are discussed in relation to the importance of considering evasive action in child pedestrian research and critical perceptual-motor processes that have been under-emphasized in much of the child pedestrian literature.

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Introduction

Pedestrian injury represents a serious threat to the health and wellbeing of people across much of the world. Indeed, more than one fifth of all people that die on roads each year are not traveling in a vehicle of any kind, motorized or otherwise (World Health Organization, 2013). In Canada, pedestrian injuries are the fourth leading cause of death and a leading cause of injury for children 0-14 (Public Health Agency of Canada, 2012). In the United States, it is estimated that, on average, a pedestrian is killed every 2 hours and a pedestrian is seriously injured every 8 minutes (Borse & Sleet, 2009). Although child pedestrian injuries and deaths have seen a decline in the last decade, recent US data indicate that 8,000 children were injured and 207 were killed as pedestrians or cyclists in 2014 (NHTSA, 2014). Although children under 15 represent only about 20% of the population of the US, one large scale study ($n = 5000$) demonstrated this group accounts for 38% of pedestrian injuries (Peng & Bongard, 1999). In fact, in one of the only studies that has adjusted for traffic exposure, the number of pedestrian injuries affecting 5 to 10 year-olds was estimated to be more than four times higher than that for adults (Thomson, Tolmie, Foot, & McLaren, 1996). Thus, children constitute a 'high risk' group for pedestrian injury. In light of this, there has been considerable research interest in identifying factors that contribute to child pedestrian risk to aid in the development of effective interventions.

Risk Factors for Child Pedestrian Injury

Although there has been long-standing debate about the relative importance of built environment factors (e.g., crosswalks, traffic calming measures, etc.) versus child behavioural factors (Thomson et al., 1996), it is generally agreed that both are important determinants of pedestrian risk (Cross & Hall, 2005). Research on both of these types of risk factors are considered below.

Environmental Risk Factors

Elementary school-aged children are typically injured when travelling to and from school or other locations close to home (Lightstone, Dhillon, Peek-Asa, & Kraus, 2001; Macpherson, Roberts, & Pless, 1998). Although child pedestrians are injured both at intersections and midblock locations, evidence suggests that a large proportion of injuries occur at midblock suburban/residential locations (Desapriya et al., 2011) where no traffic controls (e.g., lights,

signs, crosswalks) are present (Mayr et al., 2003); this is typical of pedestrian injuries at all ages (NHTSA, 2008, 2009), however, in childhood, younger children (5-9) are more likely than are older children (10-15) to cross at midblock with no traffic controls (DiMaggio & Durkin, 2002). It has been suggested that these injuries are partly due to roads in residential neighborhoods being poorly designed for child pedestrians because they are often wide and straight, allowing for cars to park on both sides. This road design encourages higher speeds and prevents drivers from seeing children emerging from behind parked cars (Schieber & Vegega, 2002).

Although vehicle speed is associated with increased injury severity (Rosén & Sander, 2009), the majority of child pedestrian injuries occur when drivers are travelling straight, not speeding, and typically when travelling at speeds of 50 km/hr or less (DiMaggio & Durkin, 2002; Mayr et al., 2003; Peng & Bongard, 1999). A small proportion of these injuries are fatal; these typically occur at vehicle speeds of 50km/hr or greater (Desapriya et al., 2011). In the majority of cases, visibility seems to play a major role in child pedestrian injuries. Although the majority of these injuries occur during the day, in clear and dry conditions (DiMaggio & Durkin, 2002; Mayr, et al., 2003), a large proportion of child/vehicle collisions result from the driver not seeing the child in time to avoid collision or the child not seeing the vehicle, or both; two major behavioural causes of this are children emerging from between parked cars or darting out quickly into traffic (DiMaggio & Durkin, 2002; Wills, et al., 1997).

Child Demographic Risk Factors

Children's age is an important factor to consider in understanding injury risk. Toddlers (1-2 years of age) are typically injured in driveways (Winn, Agran, & Castillo, 1991) whereas teen pedestrians are injured at night, often while intoxicated or distracted (Sleet, Ballesteros, & Borse, 2010). During the elementary school years (i.e., ages 4-12), children are at particularly high risk of pedestrian injuries because they are more often allowed to navigate pedestrian environments on their own or with peers, and are more likely to be injured in these contexts than when supervised by adults. Both boys and girls are at increased risk when compared to adults although, in the 0-14 year age range, males are somewhat over-represented (i.e., approximately 7 to 8 percent more in medically attended pedestrian injuries; Public Health Agency of Canada, 2012). Thus, middle childhood has been consistently identified as a period of elevated pedestrian

risk (Barton & Schwebel, 2007; Congiu et al., 2008; Wills et al., 1997) as exposure increases with children crossing many more times on average as they age (Macpherson et al., 1998) .

Lower family socioeconomic status also has been consistently shown to be an important risk factor for child pedestrians. For example, Pless and colleagues (1987) showed that child pedestrian and bicycle injuries were four to nine times higher in lower income than more affluent neighborhoods. Several researchers have pointed to higher traffic exposure in lower SES communities rather than parent or child behavioural factors to explain this disparity (Laflamme & Diderichsen, 2000; Macpherson et al., 1998; Schwebel, Davis, & O’Neal, 2012).

Cognitive Risk Factors

There is a small but growing body of epidemiological and experimental evidence linking child cognitive development to pedestrian risk. Indeed, younger age is a risk factor; children 5-9 years of age have been shown to have accident rates that are four times that of adults (Thomson et al., 1996). When relatively low traffic exposure of children is taken into account, the disparity is even greater (Demetre et al., 1992). In a study assessing the safety of crossing decisions, six year olds were 12 times more likely to make a risky decision (e.g., that could have resulted in a collision) than ten year olds (Congiu et al., 2008). A number of early researchers in this area have suggested that Piaget’s developmental theory provides a good explanation for increased risk in the 5-9 age range. Specifically, as children develop concrete operations they have difficulty understanding the interrelationships between time, distance and velocity simultaneously and instead tend to focus on distance alone (Demetre et al., 1992). Several researchers have explored the role of the various cognitive skills required to navigate a pedestrian environment (Thomson et al., 1996) and have produced a growing body of evidence that many of these skills are not fully developed in childhood, which may help to explain decreasing pedestrian risk as children age and develop (e.g., Barton & Schwebel, 2007; Demetre, 1997; Pitcairn & Edlmann, 2000).

Thomson and colleagues (1996) have identified major areas of cognitive-perceptual skill and ability that are critical for safely navigating a street crossing: 1) *Detecting the presence of traffic* involves selective attention to appropriate visual stimuli in coordination with auditory information; 2) *Visual timing judgments* require the ability to accurately determine vehicle speed and distance in order to judge time-to-contact; 3) *Coordinating perception and action* involves

coordinating perceptions of multiple moving objects and the time available to cross with time required to cross, and plan when to initiate a crossing based on this information.

One of the key factors influencing the safety of a road crossing is the ability to select a safe and appropriate gap based on one's ability to clear the gap before it closes and then acting on this decision as soon as it is safe to do so. A few studies have demonstrated that young children can be less efficient at crossing than older children or adults. Specifically, although children have been shown to choose crossing gaps between oncoming vehicles of a similar length to that of adults, they have been shown to hesitate longer before entering the roadway after deciding to cross into a safe gap, referred to in the pedestrian literature as a *start delay* (Pitcairn & Edlmann, 2000; Plumert, Kearney, & Cremer, 2004); this can decrease the available time to cross, increasing risk of injury; children also tend to hesitate more than adults resulting in missing safe opportunities to cross (Demetre et al., 1992).

Although only a few studies have addressed this issue, poorer or underdeveloped attention-based and executive functioning (EF) abilities appear to influence child pedestrian pre-crossing behaviour and decision making (Barton & Morrongiello, 2011; Congiu et al., 2008). Executive functioning is a construct comprising a number of mental capacities/processes that are involved in goal directed behaviour including, planning, monitoring, impulse control, working memory and attentional control (see Goldstein & Naglieri, 2013, for a review). Stavrinou and colleagues (2011) found that executive functioning fully mediated the relationship between children having an ADHD diagnosis and heightened pedestrian risk (i.e., children who scored higher on executive *dysfunction* chose tighter gaps and had less time to spare as a consequence). Congiu and colleagues (2008) also found an association between executive dysfunction and choosing tighter gaps that could result in increased risk for children. Greater cognitive efficiency (e.g., speed and accuracy in cognitive performance) has been shown to be a protective factor, associated with more observation of traffic prior to crossing and more time remaining to cross a selected gap when they enter the road. Children's metacognitive ability to monitor and assess their own behaviour has been associated with gap size selection differentially depending on traffic volume: under low traffic volume children with lower monitoring abilities chose riskier gaps and chose larger gaps under high traffic volume (Barton & Morrongiello, 2011).

Attending to appropriate cues in a pedestrian environment has been consistently cited as an important factor in child pedestrian safety. Children are notorious for failing to “look both ways” before entering the street. Thomson and colleagues’ (1996) review of empirical work including naturalistic and experimental studies showed that more than 50% of children in the age range of 4-14 did not look both ways before crossing. Even when younger children do look before crossing, it has been shown that their attention may be misplaced. Indeed younger children, under 10 years of age, have been shown to focus on the presence of a vehicle (e.g., if they can see a car then it is dangerous, if they cannot see one because their view is occluded then it is safe), and focus more on the distance and not the speed of the vehicle (e.g., if it is far away it is safe; Morrongiello, Corbett, Milanovic, & Beer, 2015). Young children aged 3 to 6 also have been shown to watch a car pass in the close lane and initiate a crossing without looking back to see if another car is coming (Briem & Bengtsson, 2000; Congiu et al., 2008; Connelly, Conaglen, Parsonson, & Isler, 1998; Foot, Tolmie, Thomson, McLaren, & Whelan, 1999; Thomson et al., 1996; Tolmie et al., 2005). Thus, failure to attend and/or sustain attention to appropriate cues in a pedestrian environment elevates injury risk, with younger children being more likely to make these errors.

Perceptual Mechanisms of Risk

Gibson’s ecological theory of visual perception (Gibson, 2014) also has been used to offer an alternative explanation for child pedestrian risk. The pertinent tenant of this theory is that, in order to survive in their environment, animals must have immediate access to cues about the movement of objects in relation to their own body movement and have an evolved ability to extract time-to-contact information *directly* from the visual system without the need for higher order cognitive functions, as proposed by a Piagetian perspective. This has been supported by a variety of research including a study demonstrating that the retinal image of an approaching object varies inversely with the time to collision of on approaching object (Lee, 1976). Furthermore, research with adults has shown that collision time can be anticipated from expanding images when no distance cues are present (McLeod & Ross, 1983; Schiff & Detwiler, 1979) According to Gibson, therefore, the developmental task of the child pedestrian is to gain sufficient experience with perceiving the movement of objects in the environment to allow them direct access to time-to-contact information directly from perceptual cues (Demetre, 1992).

Several researchers have examined perceptual abilities of pedestrians and how this influences judgments of time to contact; early work often included presenting participants with two-dimensional simulations of traffic and asking participants to indicate how long it would take for a vehicle to reach them (Hancock & Manster, 1997; Manser & Hancock, 1996; McLeod & Ross, 1983). The general finding in these perceptual studies is that people tend to *underestimate* time to contact, hence, researchers concluded that participants would have crossed the road safely. Connelly et al. (1998) assessed child judgments of time to contact simultaneously with judgments about their ability to cross safely by asking children to indicate, at the roadside, the point at which a vehicle was too close for them to cross safely. They found that when vehicles approached at speeds above 60km/h then 5-6 and 8-9 year olds made more unsafe judgements than 11-12 year olds. However, when such judgments are coupled with some sort of action (walking across a pretend or contrived road) children of younger age have been shown to be more cautious, allowing more safe opportunities to pass before choosing a gap (Demetre et al., 1992; te Velde, van der Kamp, Barela, & Savelsbergh, 2005) Thus coupling of motor behaviour with perception appears to produce more cautious pedestrian behaviour in younger children than when they are asked to make a perceptual judgement and estimate their own motor abilities.

Methodological Issues in Child Pedestrian Research

Although the evidence-base for understanding child pedestrian risk is growing, the research behind it has been fraught with methodological challenges. The majority of these challenges stem from the necessity of balancing the need for methodological rigor with the safety of child participants. Research examining the determinants of child pedestrian injury has typically employed passive methods such as table top simulations (e.g., Thomson, 1997), video presentation of traffic (e.g., Pitcairn & Edlmann, 2000), or more interactive road-side (e.g., Connelly et al., 1998) or “pretend road” methods (e.g., Barton & Schwebel, 2006; Demetre et al., 1992) For example, table top models of city blocks may be used to assess child pedestrian route selection, while videos of approaching traffic have been used to assess inter-vehicle gap choices and time to contact estimation. Of course, these methods do not measure pedestrian behaviour directly and, therefore, their generalizability to real world contexts is limited. Hence, researchers have sought methods that put children in situations that are as close to a real crossing as is possible.

Several approaches have been developed that put children close to a real road and have them make judgments and decisions based on approaching vehicles. For example, studies have asked children to stand on the curb of a real road and classify oncoming vehicle gaps as safe or unsafe (Van Schagen, 1988). Similarly, the “two-step task” involves children standing beside a real road approximately 60cm away from the curb and indicating the decision to cross by taking two steps toward the road while imagining that they would be traversing beyond a protective barrier and across the street (Demetre, 1992). Another approach that has been used extensively in research, was originally designed as a training tool for children and is referred to as the ‘pretend road’ method (Lee, Young, & McLaughlin, 1984; Young & Lee, 1987). Using this method, children stand a distance away from a one or two lane street that is the same as the width of the street; at the curb of the real street there is a short barrier that stops children from crossing into traffic (See Figure 1). Children are asked to pretend that for each vehicle approaching in the real lanes there is one in the pretend lane and they are instructed to cross the pretend road safely. This approach has the advantage of requiring very little equipment and can be done anywhere there is an unobstructed view of traffic in both directions. Demetre (1992) has criticized this method for having “distorting characteristics,” in that children are displaced from the locus of action. This displacement could have two potential impacts on conclusions that can be drawn from findings. First, the task is potentially much more cognitively complex than a real road crossing and therefore may underestimate child road crossing abilities. Second, the task involves children focusing on a space that is twice the width of the actual road and this may influence their judgment of how long it takes to cross, resulting in an overestimation of risk indices, such as hits and tight fits/close calls.

In addition, there are several challenges with these approaches, including: data collection is weather and time of day dependent; road-side studies lack experimental control (i.e., traffic exposure is different for each child, measures are created using video/audio coding which is labor intensive) and, importantly, children’s actual crossing behaviour cannot be measured directly but rather must be estimated based on the child’s typical walking speed. In-road behavioural measures are extrapolated from these estimates (e.g., *if* the child had crossed when they indicated, they would have been hit or would have come some distance from being hit). Even in the pretend road methodology, safety indices are often extrapolated, though not always (Demetre et al., 1992), from the time that children enter the pretend road, based on their average

walking crossing time rather than for their actual crossing behaviour (e.g., Barton & Morrongiello, 2011; Barton & Schwebel, 2006).

Using virtual reality (VR) has been shown to be an innovative and effective way to address some methodological limitations such as weather dependence and experimental control (e.g., McComas, MacKay, & Pivik, 2002; Plumert et al., 2004; Schwebel, Gaines, & Severson, 2008; Tolmie et al., 2005). Nonetheless, the large majority of VR applications to pedestrian research to date have employed desktop computers and/or three screen displays in which the participant's average walking speed is assumed to be the speed at which they would cross and is used to *estimate* their in-road crossing behaviour (Byington & Schwebel, 2013; Congiu et al., 2008; Schwebel et al., 2008; Schwebel, Stavrinos, et al., 2012; Thomson et al., 2005). Although these approaches provide important information about pre-crossing behaviour, the most critical drawback of these systems is the same as in roadside, pretend road and video based methodologies—namely they do not measure *in-road behaviour* directly and thus may provide poor estimates of child pedestrian safety outcomes.

Given the safety implications for child participants it has not been possible to fully validate a VR simulation to real world crossings in the same way it has never been possible to validate any previous methodology. The validation that has been done has been indirect. For example, adult gap choice in pretend road methods has been validated against real world gap choice in adults (Barton & Schwebel, 2007; Schwebel et al., 2008) and some child pedestrian pre-crossing behaviours in virtual pedestrian environments have been validated against pretend road methodologies, the purpose of this research is to compare the validity of findings using different measurement approaches and VR offers a great deal of experimental control in service of this aim. Studies using these methodologies and drawing conclusions linking pre-crossing decisions to in-road safety outcomes do so under the tenuous assumption that children cross at a constant speed and do not adjust their movement speed to traffic conditions resulting in a failure to capture children's ability to coordinate perception and action. This is not to say that researchers actually believe this is a valid assumption. Rather they are forced into this assumption by the limitations of their virtual reality systems that cannot measure children's actual street crossing behaviours. Therefore, while common sense and evolutionary psychology approaches would suggest that a car approaching on a hit course should evoke self-preservation reactions to try to get out of the way, these virtual reality systems are not taking this possibility

into account. These systems, therefore, likely underestimate children's abilities and introduce measurement bias into the interpretation and understanding of what constitutes safe crossing behaviour.

Historically, only two systems have measured behaviourally-based safety outcomes (e.g., hits and near-misses) in children in a virtual pedestrian environment. One was a bicycle simulator system (e.g., Plumert et al., 2004; Plumert, Kearney, Cremer, Recker, & Strutt, 2011) and the other a pedestrian system that was developed in the early 2000's when computing power did not afford a high degree of immersion or realistic experience of a traffic environment (Simpson, 2003). Recently, Morrongiello and colleagues (2015) developed a more advanced VR pedestrian simulator, used in the current study and discussed in more detail below. This system uses state of the art technology and not only measures in-road behaviour precisely but also can be used to assess how these measures compare with methodologies assuming static in-road behaviour (i.e., a constant walking speed); the latter assessment is done by measuring participants' average walking speed and computing estimated safety measures based on these data, while at the same time generating safety measures derived from vehicle/participant position and velocity data (i.e., actual in-road behavioural data).

There are a number of findings in the literature that a comparative analysis of outcomes derived from different methodologies is informative to aid in the interpretation of inconsistencies in past findings. For example, greater child hesitation after choosing a gap to cross into the road (*start delay*) has been considered a risk factor in children that is not seen in adulthood. Some evidence suggests that children tend choose similar gaps to cross into as adults but tend to delay entry more than adults, reducing the available time to cross and resulting in reduced time to spare at the end of the crossing; these findings are often considered when attempting to explain the disparity between child and adult pedestrian injury risk (Pitcairn & Edlmann, 2000). However, gap choice and hesitation outcomes have been shown to be sensitive to the methodology employed and the impact of start delay on the safety of the crossing has been brought into question. One study for example, using videos of traffic and crossing behaviour, measured by children pressing a button on a keyboard, found significant age differences in gap size choice, with 7 year olds choosing larger gaps than adults and also positive correlations between start delay and gap size, suggesting that children are strategic in gap choice and take into account their own tendency to delay (Pitcairn & Edlmann, 2000). Young and Lee (1987), using a pretend road

method, showed that 5 year olds rejected 45% gaps of adequate duration to cross (i.e., a *missed opportunity*) compared with only 10% rejection by adults, suggesting children may not be as skilled at using temporal information but that they adjust for this by having a wider safety margin for gap acceptance. However, a series of three experiments conducted by Demetre and colleagues (1992) compared the pretend road method with a “two-step” method (i.e., children stand close to the curb of a real road and take two steps toward it to indicate their intention to cross) and showed that the pretend road method overestimated missed opportunities, but not estimated measures of risk (i.e., child-vehicle collisions or close calls). Risky gap choices on the other hand, did not vary across methodologies or between children and adults. Most relevant to the current discussion, te Velde and colleagues (2005) conducted an experiment showing that gap choice is sensitive to the methodology employed. They compared children ages 5-7, 10-12 and adults making crossing decisions either verbally, and not crossing, or simply crossing if they felt it was safe to do so. Traffic was generated in a lab consisting of a moving bicycle on a track and participants were told to look at the bike at different distances and make their decision verbally or walk across the path of the approaching bike. The study employed a within-subjects design to compare conditions and showed that verbal judgments resulted in more unsafe crossing decisions than actually crossing. No age differences were found in gap choice, but younger children tended to show greater start delay than older children and adults. Thus, it is clear that child age, gap selection and start delay are interrelated but the nature of this relationship is not consistent across studies. The methodology used has implications for the resultant findings and the conclusions that can be drawn from them.

The fact that different methodological/measurement approaches can yield different results on the *same* outcomes highlights the need for research to examine the nature and extent of influence of differing methodologies on outcomes. Only in so doing, can one attempt to untangle and interpret inconsistencies across the literature. Unfortunately, pioneering studies such as that of Demetre et al. (1992) and Velde et al. (2005) that compare methodologies are extremely rare in the child pedestrian safety literature. The current research addresses this gap by comparing statistical models predicting safety outcomes derived through different methods (e.g., estimated versus real-time, in road behaviour measurement) and using the same participants.

Current Study: Estimated Versus In-Road Crossing Behaviours

Given that differing methodologies can produce inconsistencies in the same pre-crossing decision and behavioural measures, it is important to consider what kinds of differences may arise when comparing methodologies that estimate in-road behaviour versus actually measuring this behaviour precisely. A recent study using the fully immersive VR system used in the current study showed that children's average walking speed is a poor proxy for children's motor behaviour when in the path of a vehicle (Morrongiello, Corbett, Milanovic, Pyne, & Vierich, 2015). Children in this study responded to increased risk (i.e., tighter inter-vehicle gaps) by increasing their speed while in the car path. Importantly, it was not only that children entered the road at a higher rate of speed but they also showed greater increases in speed while in the car path under greater threat of being hit. Furthermore, using average walking speed to calculate pedestrian-vehicle collisions resulted in estimating approximately three times more hits than the same children experienced in the virtual crossings. Thus these findings provide the first evidence of 'evasive action' in child pedestrian crossing behaviour. This brings into question whether the assumption that children walk at a constant pace while in road is a reasonable one and whether past research has potentially over-estimated the impact of pre-crossing decisions on safety outcomes by ignoring the potentially strong influence of evasive action. The current study extends these findings by comparing the relationship between start delay, gap choice, and safety indices using estimated crossing behaviour versus actual crossing behaviour to determine how these different measurement approaches may influence research findings.

It is important to consider also that the potential influence of estimating in road behaviour may have implications beyond measures of safety outcomes alone. Indeed, any measure that relies on average walking speed in its derivation is likely to be affected, introducing measurement bias into research findings. For example, a *missed opportunity* is defined in the literature as any rejected temporal gap in vehicles that is 1.5 times or more greater than the time it would take the child to clear the lane (e.g., a child does not enter a gap that is 6 seconds when their estimated time to cross the lane is 3 seconds). Importantly, this measure has generally been derived from the child's average walking speed as measured before the testing session. Given that children tend to speed up in the road, using actual walking speed as measured in the testing trials should clearly provide a more unbiased estimation of whether a missed opportunity occurred. The current study explores the possibility of measurement bias by calculating missed opportunities using average walking speed as measured before the testing trials and comparing

results to using the actual in-road mean walking speed for the trial that missed opportunities are counted.

In summary, the aims of the current study were to determine the nature and extent of measurement bias associated with estimating pedestrian safety outcomes by comparing regression models predicting these outcomes with outcomes derived from measuring in road behaviours directly from known pre-crossing measures associated with pedestrian risk (i.e., gap choice and start delay).

Method

Power Analysis

A power analysis was conducted using G*Power (Erdfelder, Faul, & Buchner, 1996) to estimate a reasonable sample size for the proposed analyses. Given that effect sizes are likely to be influenced by the methods employed, previous findings using this VR system were used to estimate the effect sizes that could be expected for the current study. Specifically, Morrongiello and colleagues (Morrongiello, Corbett, Milanovic, Pyne, et al., 2015) showed that the effect of increasing levels of risk on walking speed adjustments made by children were in the medium to large range and the effect of using estimated versus actual crossing behaviour on hits was in the medium range. Although the traffic conditions in the current study were quite different, the effect size resulting from comparing estimated vs actual crossing behaviour was deemed a reasonable basis for predicting the effect size of similar comparisons in this study. Results of the power analysis indicated that to detect a small to moderate effect size $f^2 = 0.10$ at a power of .80, $p = .05$ (Cohen, 1992) using a regression analysis with up to 4 predictors, a sample size of 125 would be required. Based on this a conservative sample size of 130 was chosen to account for the impact of different traffic conditions and different measures being compared in the current study (e.g., missed opportunities derived from mean walking speed has never been compared to the same measure derived from actual walking speed).

Participants

A total of 130 children, ages 7 through 12, were recruited from the local community, including 62 girls ($M = 9.88$ years, $SD = 1.82$ years) and 68 boys ($M = 10.17$ years, $SD = 1.87$ years). This age range was chosen because it includes children at ages at high risk for residential

pedestrian injuries, includes older children who have attained a greater degree of pedestrian skill through crossing streets independently (Thomson et al., 1996), while not including children too young to understand instructions and work with the VR hardware (i.e., previous unpublished research in our lab indicates that 7 years is the minimum age for using our head mounted display). Children were from predominantly white (94%), well-educated (61% had some university or college and an additional 27% had post-graduate training), and medium to high income families (19% had incomes between \$40,000 and \$79,999 and 72% had family incomes above \$80,000 per year).

Measures

Demographic/SES information was collected from parents upon arrival to the VR lab.

Screening Questionnaire.

The Simulator Sickness Questionnaire was completed over the phone at the time of booking an appointment to ensure the child was not at an increased risk for physical symptoms (e.g., nausea, dizziness) while wearing the virtual reality headset. The Simulator Sickness Interview Questionnaire specifically assesses the child's history of migraine headaches, claustrophobia, motion sickness, and dizziness/nausea (Kennedy, Lane, Berbaum, & Lilienthal, 1993).

Traffic Exposure

A modified version of the *Exposure to Traffic* questionnaire (Congiu et al., 2008) was used to assess how much supervised and unsupervised practice a child had acquired crossing streets.

Crossing Measures

The crossing behaviours that were measured or derived and/or used in calculation of measures are described in Table 1. All of these measures have been thoroughly tested through a process called unit testing (See Appendix A for a complete description) wherein thousands of possible scenarios are tested for each measure to ensure that the expected value (based on pre-determined parameters) matches the actual value that is output by the system.

The main dependent variable assessed in this study requires some additional rationale beyond what is provided in Table 1. There are two main safety outcome measures used across

the child pedestrian literature: hits and close calls or near misses. As described above, these measures are estimated in the large majority of studies. Near misses or close calls are most often defined as any crossing wherein the child has 1 second or less between themselves and the approaching vehicle when they exit the vehicle's path or the lane. Many researchers combine these into one risk composite (e.g., Schwebel, McClure, & Severson, 2014; Stavrinou et al., 2011; Stavrinou, Byington, & Schwebel, 2009). For the current study we assessed the impact of using different kinds of safety outcomes (see Results) on the difference between estimated and actual measures and then chose a measure representing hits in our system. Hits in the current system are registered any time the position of any part of a vehicle intersects with the participant's position. As described below the participant's position is tracked by tracking the position of an infra-red light located in the head-mounted display. This light is positioned at approximately the centre of their body if they are standing still. If they are moving forward and especially if they are accelerating, their body is leaning in a forward direction and therefore their head is slightly in front of their body, because of this, when the child is "almost" hit, the system may not register a hit because their head was out of the path, but in reality their body would have been hit (i.e., a leg or their torso). Therefore, the number of hits is slightly underestimated¹. The goal of this outcome measure was to represent injury risk, because the goal of the study is to estimate the effect of measurement bias on estimating injury risk and thus needed to include all trials in which an injury is likely to have occurred; to ensure a measure that included all hits, the measure High Risk Time Left to Spare (HRTLs) was used in all analyses; it is defined as the proportion of trials in which the participant had 0.25 seconds or less between the car and themselves when they exited the car path (i.e., a TLS of 0 was a system confirmed hit and between 0 and 0.25 seconds a very high probability of a hit occurring).

Virtual Reality Pedestrian Simulation and Motion Tracking System

The VR system has been constructed in an 8m X 5m room using an 8-camera optical-motion tracking system (PPTH by Worldviz) to feed position data to specialized software (Vizard), using a high-level scripting language (Python) to accomplish many low-level graphics and hardware interfacing actions.

¹ Future research with additional measurement apparatus may be able to adjust for this but this not currently a capability of our system.

Participants wear small infra-red lights on a headset that is tracked by the cameras such that any movement fed back to the system changes the view the participant sees through the headset virtually instantaneously (i.e., very low latency). Importantly, the scaling of movement in the real world to the virtual is 1:1 such that a step in the real world changes their position in the virtual world by precisely the same distance.

Participants view the virtual environment through a Virtual Research Systems 1280 X 1024 resolution stereoscopic head-mounted display (HMD). Mounted on the HMD is an Inertia Cube 3, which measures head orientation up to 360 degrees on all axes (i.e., X, Y and Z coordinates) using accelerometers, and gyroscopic and magnetic sensors such that changes in head orientation change the participant's view of the virtual environment with negligible latency (4ms). All movement and orientation data are captured at a rate of 60 times per second. The virtual environment is a two-lane street with sidewalks.

The virtual pedestrian environment's realism is enhanced visually by houses, trees, shadows and realistic road, sidewalk and grass textures. Realistic sounds of traffic movement (e.g., engine sound becoming louder and changing pitch as car gets closer; i.e., Doppler Effect) as well as some faint ambient sounds such as birds chirping are presented to participants via high fidelity ear phones. Participants control the direction they walk, their speed of movement, and if they make a poor crossing decision, they have the ability to step back to the curb or speed up to evade the approaching vehicle.

Procedure

Two trained and closely supervised research assistants (RAs) ran the testing sessions, with one overseeing the operation of the computer that controls the VR system (i.e., the presentation of specific types of trials), and the other remaining in the test room and available to ensure the safety of the participant and assist if needed during completion of the trials.

Prior to testing in the VR environment participants were fitted with a small LED light attached to their head with a strap (e.g., much like a smaller version of a head lamp used for camping). They were then asked to walk as though they were crossing a street from one X on the floor to another X about 3m away, representing the distance from the curb in the VR environment to just across the yellow line of the two-lane VR street. The LED light was tracked by the optical motion tracking system to provide velocity data on each of 10 'crossings.' These

data were then used to provide an average walking speed to be used in the calculation of the measures employing estimation and extrapolation typical of pedestrian research as explained herein.

Each participant completed two phases in the VR pedestrian simulation. In the familiarization phase, children were introduced to the virtual environment. First, an RA demonstrated how to cross the street while wearing the headset as the participant viewed what the RA could see in the headset on an external computer monitor in the room; this included demonstrating what would happen if one were to be hit by a car (i.e., all vehicles disappear and a siren plays).

The participant was then fitted with the VR headset (HMD). To ensure the HMD is fitted properly and the participant can see clearly, a test screen is presented initially with letters arranged in asterisk formation and s/he is asked to correctly identify the letters before s/he is shown the street environment; knob adjustments allow the viewer to adjust the headset to improve fit and visual acuity.

The familiarization phase began with the participants' view changing to the virtual pedestrian environment, wherein they were positioned to stand on the curb facing a two-way street (i.e., one lane each way); to prevent possible tripping hazards, all curbs are visually presented but the child does not have to step down/up on these. The participant was then instructed to walk across to the first lane of the road and across the yellow line in the centre, to turn around and walk back to the sidewalk and into a large translucent green cylinder, triggering the start of the next trial. The street is the size of a fairly narrow two-lane residential street (i.e., 5.5m across). Children were then instructed to cross back (no traffic) to reach the curb they had started from and proceeded to repeat this process 10 times with no cars appearing; pilot testing showed that children were walking at the same speed with the HMD on across the rest of the trials after 10 practice crossings. This phase gave participants exposure to the VR environment, experience walking with the headset on, and provided them an opportunity to ask any questions and make any necessary adjustments before traffic is presented.

In phase 2 (Test Trials), participants were introduced to traffic and asked to cross the street when they deemed it safe to do so. Vehicle gaps were presented in an incremental and increasing order, with two gaps of each size (i.e. 2, 2, 2.5, 2.5, 3, 3, 3.5, 3.5, 4, 4, 4.5, 4.5, 5, 5, 6,

6 seconds). This gap ordering was used because it provides an assessment of the child's 'risk threshold' (i.e., the smallest gap the child considers to be safe enough to cross through). Because children are operating at their highest tolerance for risk, their behavioural and decision making skills are more likely to be taxed like in a real world high risk pedestrian situation. Initial gap presentations increase in smaller increments than those at the end of the sequence in order to increase the sensitivity of measurement in the range that children at these ages are likely to choose. A previous study with the same system showed that children, regardless of age, typically choose gaps of around three seconds ($M = 2.99$, $SD = 0.56$; ($M = 2.99$, $SD = 0.56$; Morrongiello & Corbett, 2015). Also, the smaller gaps at the beginning of the sequence come much quicker than those at the end, so this sequence gives children more time to assess the danger of each gap. These temporal gaps were presented for 5 crossings at three speed conditions: 30, 50, 70km/hr (i.e., a total of 15 trials).

Analytic Approach and Data Reduction

The purpose of the analyses was to determine the nature and extent of measurement bias introduced by estimation of pre-crossing (i.e., missed opportunities) measures and safety outcomes (i.e., High Risk TLS) using average walking speed and to determine the effect of this bias on regression models that have typically been used to understand how pedestrian risk arises.

Pre-screening of the data was done on a trial by trial basis first to remove any extreme univariate outliers (i.e., above 5 standard deviations). Univariate outlier removal occurred for approximately 0.01 percent of trials. All pedestrian variables (gap size, start delay, missed opportunities, high risk TLS) were computed as aggregates across trials, and were assessed for normality violations and transformed where appropriate. Independent and dependent variables were pre-screened for violation of assumptions for ANOVA and regression (i.e., multivariate normality, linearity, homoscedasticity, independence of residuals) and paired t-test (i.e., normality of differences) on an analysis by analysis basis. During the analyses, models were screened for undue influence of multivariate outliers using Cook's distance values above 3 standard deviations above the mean. Potential outliers were removed individually to determine their impact on the results of the analysis (Tabachnick & Fidell, 2001). Outliers were removed if

they showed a disproportionate impact on the effect size². For repeated measures ANOVA when the sphericity assumption was violated Greenhouse-Geisser adjusted tests of significance are reported.

For all regression and ANOVA models, Age, Sex and Pedestrian Experience were first correlated with the dependent variable (See Appendix B); significant correlations resulted in inclusion of these variables as covariates.

Results

What is the impact of using average walking speed captured at pre-testing versus in-trial walking speed to calculate missed opportunities?

Missed opportunities were calculated based on average walking speed measured at pre-testing (missed opportunities estimated) and based on the mean walking speed for crossing in the trial wherein the missed opportunities occurred, yielding two separate variables representing different measurement approaches. Screening indicated that the difference score between these variables was significantly positively skewed and thus the variables were LOG transformed to correct for this. A paired-samples t-test³ was conducted on the log transformed data, however, raw means (i.e., the mean number of missed opportunities per trial, means are less than 1 because, for many trials, there were no missed opportunities) are reported for ease of interpretation. The difference between the two measures was substantial with approximately 2.5 times more missed opportunities per trial resulting from using actual mean walking speed ($M = 0.44$ missed opportunities/trial, $SD = 0.78$) than when using estimated walking speed ($M = 0.18$, $SD = 0.43$). This difference was statistically significant, $t(129) = 5.40$, $p < .001$. Given that missed opportunities is a measure often considered a proxy for cautiousness, this finding suggests that the traditional derivation of this variable may produce underestimates of how cautious children are when deciding on a safe gap in which to cross.

What is the impact of using average walking speed measured before trials versus actual walking speed during trials to determine High Risk Time Left to Spare?

² Specifically, if the removal of an outlier or group of potential outliers increased or decreased the effect by a proportion of more than 5 times the proportion of the sample represented by the outliers then they were removed.

³ A Wilcoxon Signed Rank test was also conducted on untransformed data and resulted in the same effects.

High risk TLS variables were calculated using average walking speed measured at pre-testing (HRTLS-estimated) or measuring time left to spare on exiting the car path directly (HRTLS-actual). HRTLS was operationalized as the proportion of trials that a participant was hit or came within 0.25 seconds of being hit, so higher scores indicate greater risk. A paired-samples t-test was applied to the data and revealed a significant difference between the two variables [$t(129) = 8.01, p < .001$], with HRTLS-estimate returning approximately 51% more high risk crossings than HRTLS-actual ($M = 0.56$ and $0.37, SD = 0.30$ and 0.22 , respectively). Thus, using average walking speed to calculate this safety outcome *over*-estimated child pedestrian risk when compared to measuring walking speed directly. Recent research discussed herein indicating evidence for evasive action, in the form of children speeding up during high risk crossings, suggested that greater crossing velocity during actual crossings than in pre-crossings would likely explain this difference and affirm that children show more capabilities to manage risk than suggested when an estimated walking speed measure is used.

To confirm this, mean pre-trial velocity was compared to mean in-car-path velocity using a paired-samples t-test. Results indicated that average child velocity while in the path of a vehicle ($M = 1.39$ m/s, $SD = 0.26$) was approximately 25% greater than the mean walking speed measured at pre-trial ($M = 1.11$ m/s, $SD = 0.15$). To determine if this difference was due to changes in velocity while in the car path (evasive action), the difference between the velocity on entering the car path and the maximum velocity was calculated and showed a significant increase ($M = 0.35$ m/s, $SD = 0.14$) when a t-test was applied with a reference value of 0, $t(129) = 29.13, p < .001$. Furthermore, when a hierarchical regression was applied, controlling for gender, gap choice and start delay (age and pedestrian experience were not correlated with either HRTLS variable and were therefore excluded from all subsequent analyses), then evasive action (i.e., the velocity change from the point of entering the gap to maximum velocity) emerged as a significant predictor of HRTLS-actual, $\beta = -.374, t(122) = -3.00, p < .005$, accounting for 4.1 percent of unique variance in HRTLS-actual, $F(1, 122) = 9.01, p < .005$. Thus the greater the increase in velocity while in the car path, the lower the proportion of HRTLS trials. As expected, evasive action was not a significant predictor of HRTLS-estimated because evasive action was assumed to be zero in the derivation of this outcome.

What is the impact of the proportion of high risk trials included in HRTLS on the difference between estimated and actual safety outcomes?

Safety outcomes across the child pedestrian literature include both near misses/close-calls and hits and many researchers combine these measures (e.g., hit/close call, tight fit) (Demetre et al., 1992; Schwebel, Barton, et al., 2014; Schwebel & McClure, 2014; Stavrinou et al., 2009). What is considered a near/miss close call seems somewhat arbitrary and varies across researchers. For example, Demetre et al. (1992) defines a “tight fit” as a crossing in which the child would have been hit or narrowly escaped but does not give a definition of narrow. Clancy, Rucklidge and Owen (2006) define a near miss as the participant being within 0.5 seconds of being hit whereas Schwebel and colleagues (2014) combine close calls with hits and define this measure as the proportion of trials that the child came within 0 (hit) to 1 second of being hit. Given that safety outcomes vary across researchers in terms of the degree of risk included it is important to determine how measurement bias (i.e., estimating versus measuring directly) may interact with the degree of risk inherent in the outcome. If evasive action is the primary mechanism driving the difference between estimates and actual measures then, measuring/estimating higher risk outcomes such as hits should produce a larger difference in the measures than measures that include lower risk crossings such as near misses at 1 second. To examine this relationship HRTLS measures were calculated based actual position data and estimates based on average walking speed for 4 levels of risk, including hits and near misses of varying time-frames. All safety outcomes were operationalized as the proportion of trials in which the participant was hit and/or came within a certain amount of time of being hit. All measures included hits.

A repeated measures ANOVA was conducted with age and sex as covariates and two within subject factors, Measure Derivation (2:Estimated, Actual) X Risk (4: Hits, TLS < 0.25 seconds, TLS < 0.5 seconds and TLS < 1.0 second). Results revealed a marginal main effect of Risk, $F(1.36, 162.86) = 3.44, p < .06, \eta_p^2 = .03$, a significant main effect Measure Derivation, $F(1, 120) = 13.29, p < .001, \eta_p^2 = .10$ and a marginal interaction of these two factors, $F(1.81, 217.30) = 2.97, p < .06, \eta_p^2 = .02$. Post hoc analyses using Wilcoxon Signed Rank tests (See Table 2) revealed significant differences between estimated and actual measures at all levels of risk, as can be seen in Figure 3.

What are the safety consequences (HRTLS) of long start delay and poor gap choice and to what extent do regression models differ when actual versus estimated safety measures are used as dependent variables?

To determine if gap choice and start delay differentially predicted HRTLS-actual versus HRTLS-estimated two initial hierarchical regressions were conducted predicting each dependent variable separately, controlling for *gender* in step one and entering *gap choice* and *start delay* in step 2 (See Table 3). Gap choice and start delay were entered into the same model because they were correlated, $r(129) = .61, p < .01$ such that children show a strong tendency to exhibit less start delay after they decide to cross into a smaller gap. Thus it was important to determine their unique effect as predictors, controlling for each other. These two models were then compared using *net regression* (Cohen, Cohen, West, & Aiken, 2013).

Net regression was originally designed to compare sets of risk factors to different outcomes in the same sample to determine if regression models predicted significantly more variance in one outcome than another and to determine which factors were significantly stronger/weaker predictors of one outcome than another (Cohen, Brook, Cohen, Velez, & Garcia, 1990). The procedure involves first conducting OLS regressions for each outcome (i.e., Y and Z) with the same predictors, to determine beta weights and intercepts for each regression equation. These parameters are then used to generate a variable with the predicted value of Y (\hat{Y}) for each participant. This variable is then subtracted from Z to generate a new variable ($Z - \hat{Y}$) that is then regressed on the original set of predictors. The overall R^2 and its associated F-test from this analysis is used to determine how much more variance is accounted for in one model than the other; each regression coefficient and their corresponding t-tests indicate whether one independent variable is a significantly better predictor in one model than the other. Thus in this HRTLS-estimated was subtracted from the predicted value of HRTLS-actual so a significant R^2 would indicate that in the model with HRTLS-estimated as the outcome, gap choice and start delay together predict significantly more variance in HRTLS than when actual HRTLS is used as the outcome.

Results, presented in Table 4, indicated that start delay and gap choice together accounted for 36% more variance in HRTLS-estimated, than HRTLS-actual; the net regression indicated that this difference between models was significant, $F(2, 122) = 30.21, p < .001$. Therefore, although

gap choice and start delay together are strong predictors of high risk crossings regardless of the measurement approach employed, when estimates of crossing time are used, the relation between these predictors and risk is significantly overestimated when compared to measuring the outcome directly. An examination of the individual predictor coefficients in the net regression revealed that the start delay coefficients were not significantly different in the two models but gap choice coefficients were significantly different suggesting that gap choice was the driving force behind the large difference between the models. It is important to consider that the size of the gap chosen was exactly the same for both models, the only parameter that varied was how HRTLS was calculated. Therefore, when a dangerous gap was chosen the participant was required to accelerate and walk faster to avoid being hit. Measuring HRTLS directly based on the participants' position in relation to the vehicle at the end of the crossing captures this evasive action in that a successful evasion will mean that the participant had more than 0.25 seconds between themselves and the vehicle and that trial will not contribute toward the total number of HRTLS trials. When the safety outcome for this same trial is estimated HRTLS, based on their average walking speed, measured before the trials, evasive action is not accounted for in the measure in that the participant is assumed to walk at their average walking speed throughout the crossing, resulting in them being hit on the trial and contributing to the total number of HRTLS trials. In essence, a dangerous gap choice is much more likely to result in a hit when there is no evasive action and evasive action is not captured when safety outcomes are estimated based on average walking speed.

Discussion

In light of recent evidence that children can implement evasive actions to increase their safety as pedestrians, the goal of this study was to determine the nature and extent of the measurement bias resulting from ignoring evasive action in the derivation of pedestrian measures (both predictors and outcomes). The findings of this study offer some insight into this issue and support for the hypothesis that using estimated walking speed to derive both pre-crossing decision making measures as well as safety outcome measures is problematic and has likely produced biased results in studies of child pedestrian behavior. This has implications for our understanding of the mechanisms of child pedestrian injury because this measurement bias tends to artificially inflate the risk associated with poor *pre-crossing* decision-making and initiation of

the crossing and fail to consider post-initiation stages of crossing. Some indirect implications for this are that a biased understanding of the risk associated with curbside decision-making may be leading child pedestrian injury researchers to emphasize cognitive mechanisms of injury in their investigations, rather than fully appreciate the perceptual-motor mechanisms that operate during street crossing.

The results of this study offer rigorous and compelling evidence that measurement bias is introduced when mean, pre-testing, walking speed is used to derive pedestrian measures, as opposed to measuring children's crossing behaviours directly. These two measurement approaches produce findings that differ at a statistically significant level. The novel approach of using different methods to measure the *same* outcomes in the *same* children serves to strengthen confidence in the results of this study because the differences found cannot be attributed to sample characteristics or any other methodological differences. All children were tested in exactly the same pedestrian simulation and received the same gap sizes and vehicle speed testing conditions. Because the different measures were calculated on the same children for the same trials, all aspects of the design, including gap choice, and start delay were held constant, providing a highly controlled experimental condition for accurate comparison.

Missed opportunities is a pedestrian measure that is often considered to be associated with risk because more missed opportunities before a crossing means that children are more cautious in their decision making (e.g., they took longer to make a decision and allowed potentially safe opportunities to pass; Demetre et al., 1992; Stevens, Plumert, Cremer, & Kearney, 2013; Tolmie et al., 2005). The results of this study suggest that how this measure is calculated has a substantial impact on the proportion of gaps that are considered safe. The findings also show clearly that this difference is predominantly attributable to evasive action. When missed opportunities are calculated using average walking speed and a missed opportunity is 1.5 times the time it takes to cross, this results in an under-estimation of the number of missed opportunities when compared to using the velocity captured during the crossing because children are actively attempting to evade being hit and crossing faster. Thus more gaps are potentially safe than missed opportunities would suggest.

There are a number of implications of this finding. First it suggests that children are more cautious than previous studies have suggested because they allow more potentially safe gaps to

pass before crossing. This has implications for interpreting past findings. For example, Young and Lee (1987) found that young children rejected 45% of gaps that would have been of adequate duration to cross, in comparison with only 10% rejection by adults. Demetre and colleagues (1992) showed that the pretend road methodology overestimates missed opportunities when compared to a method wherein children take two steps toward a real road when they deem it safe to cross. Both of these methods, however, used mean pre-testing walking speed to derive the missed opportunities measure. The current findings suggest that both of these methods underestimate missed opportunities. This is important because the difference between children and adults may actually be greater than the Young and Lee (1987) finding suggests. These findings show that missed opportunities are underestimated for children ages 7-12 but say nothing about adults. Although it is likely that adults change their velocity in response to risk, meaning adults' missed opportunities may have been underestimated as well, it could be that children rely on evasive action to a larger degree because they are not as effective as adults at making safe crossings. They may not only be more cautious than adults in terms of allowing gaps that they could cross through safely to pass by, but they also may cross the street more quickly to help ensure their safety.

The finding that the proportion of high risk crossings (HRTLS: High Risk Time Left to Spare) is much greater when walking speed is estimated rather than measured directly is consistent with past work showing that evasive action resulted in approximately three times fewer hits than when average walking speed was used to calculate hits, for children in the same age range (Morrongiello, Corbett, Milanovic, Pyne, et al., 2015). The current study differs from this previous study, however, in that children *chose* the gaps they wanted to cross into based on their risk tolerance. In the 2015 study, gap sizes were uniform in each trial so they did not have to judge the safety of one gap over another. The current finding that evasive action resulted in approximately 51% fewer high risk crossings when compared to not measuring evasive action extends our current understanding of how children manage risk. In this study, gap sizes were presented in larger and larger increments starting at 2 seconds and children were asked to cross when they deemed it safe to do so. So their decision to cross can be interpreted as representing their judgement of what was a tolerable risk level for them or their *risk threshold*. In other words, children were given the opportunity to simply wait for a safe gap and cross at a leisurely pace, reducing the need for evasive action. Children did not do this, however, but instead chose

riskier gaps and managed this by crossing faster and increasing speed as needed during the crossing. Hence evasive action was used as *part of* a risk management strategy even when safer strategies were readily available. Although HRTLS-estimated over-estimated risk to a large degree, the finding that using evasive action as a strategy resulted in children being hit or almost hit (e.g., 0.25 seconds from being hit) on 37 percent of trials suggests that this strategy employed by children is quite ineffective; they showed relatively poor judgment of what level of risk they could handle effectively through evasive action. This finding is consistent with past findings that children tend to over-estimate their physical abilities (Plumert, 1995). The pattern of the current findings indicate, therefore, that although children used evasive action to reduce their risk significantly, they overestimated their ability to clear the gap, resulting in very dangerous crossings over a third of the time. Importantly, had a typical measurement approach been employed this over-estimation of their abilities would have been artificially inflated, suggesting that it is a more critical factor in safety than it actually is.

It is important to note that this new information could not have been revealed using typical methodologies, such as pretend road or other road-side methods, because these methods do not allow for the measurement of evasive action. The current findings suggest that evasive action is a potentially critical component of child pedestrian risk management that has been previously overlooked due to methodological limitation. Indeed, when comparing regression models in the current study we found that gap choice emerged as a significantly stronger predictor of risk when evasive action was ignored (i.e., when HRTLS-estimated was used as a dependent variable). This is important because it suggests that the majority of past findings have over-estimated the variance accounted for by gap choice in risk measures such as hits and close calls. Although gap choice is clearly a very important predictor of risk, overestimating its impact on risk may lead to a negatively biased understanding of how children manage risk. A review of past literature on individual difference factors would suggest a strong focus on child pedestrian risk assessment (e.g., see recent review by Schwebel, Davis & O'Neil, 2012) but almost no attention has been paid to how children actively manage this risk until recently (Morrongiello et al., 2015). This is largely because there has been no way of measuring how children manage *in-road* risk until recently.

Gibson's ecological theory of perception and more recent extensions of this theory are relevant to understanding the importance of in-road behaviors. One of the major assumptions of the Gibsonian perspective and ecological psychology is that cognitive processes should be analyzed as relations between agents and other systems. Rather than understanding cognitive processes as those that can be attributed to an individual, the processes are better understood as an interaction of the agent and their environment (Greeno, 1994). Gibson invoked the concept of *affordances* to describe the specific things about the environment that determine what kind of interaction occurs. For example, the concept of *dynamic affordances* has been applied in the pedestrian literature to describe how the perceptual-motor abilities of a pedestrian, to initiate and adjust their movement across a road, interacts with an approaching vehicle. The process is considered dynamic because the possibilities for action are constantly changing as the vehicle approaches (Plumert & Kearney, 2014). This concept is important for understanding different forms of visual information processing and how it may impact research findings in the pedestrian literature.

Milner and Goodale (1995, 2008) have proposed a theory, now supported with a great deal of evidence including neuroimaging and neurophysiology research, that proposes two anatomically distinct visual streams in the cerebral cortex (i.e., dorsal and ventral). Both streams process visual information about physical characteristics of objects and their locations in space and both are driven by the attention of the observer. The authors propose that the two streams process and transmit information in quite different ways as they eloquently explain:

“The ventral stream transforms visual inputs into perceptual representations that embody the enduring characteristics of objects and their spatial relations. These representations enable us to parse the scene, and to think about objects and events in the visual world. In contrast, the dorsal stream's job is to mediate the visual control of skilled actions, such as reaching and grasping, directed at objects in the world. To do this, the dorsal stream needs to register visual information about the goal object on a moment-to-moment basis, transforming this information into the appropriate coordinates for the effector being used... The key contribution of the perceptual mechanisms in the ventral stream is the identification of possible and actual goal objects-and the selection of an appropriate course of action to deal with those objects. But the subsequent *implementation* of that action is the job of the dorsal stream” (Milner and Goodale (2008, p. 774)

For example, if one observes some object on a desk such as a pen, the ventral stream processes information about the size and shape and location in space relative to the desk and other objects

on the desk. These are all things that can be described verbally. When one then initiates the action of picking up the pen, there is a perceptual-motor loop that occurs through interaction of visual information passing through the dorsal stream that guides the hand to the pen via motor adjustments and picks it up. This is visual information that is inherently non-verbal and cannot be described in concrete terms like the information processed by the ventral stream. Milner and Goodale (2008) refer to this as “vision for action” or the “use of visual information ...in the detailed programming and real-time control at the level of elementary movements” (p. 776). Importantly the authors provide evidence that the dorsal stream is responsible for calibrating all aspects of the movement including its initiation and, thus, it is activated just prior to the movement initiation.

This well supported theory is important when one considers the differences in pedestrian research methodologies that elicit only verbal indications of crossing decision versus actual crossing from participants. Te Velde and colleagues (2005) designed a carefully controlled experiment to determine how tapping these different perceptual pathways may influence pedestrian safety outcomes in children aged 5-7 years, 10-12 years, and adults. In a large laboratory, they designed a road-way that had a bicycle on a track that travelled toward the participant in the near lane from the right side. On the left side of the participant was a light that they were asked to look at and then turn their head to the right when it turned on to view the approaching bicycle. They were asked to indicate verbally whether they would cross at the instant they turned their head, following this they were asked to do the same thing but to actually cross in front of the bike if they deemed it safe to do so. Finally, they repeated the verbal condition. Thus, the two verbal conditions differed in that one was done before they actually crossed and one was done after to control for the experience gained by crossing that may influence judgement of one's ability to cross safely. This was done over a number of trials blocked on speed and starting distance and randomized across participants. The results indicated a large difference in the percentage of unsafe judgements in both verbal conditions (i.e., approximately 17% in both) versus the performance condition (i.e., approximately 7%). Thus participants made significantly safer judgments when they crossed the road than when they indicated their decision verbally and there was no transfer of knowledge from the performance task to the second verbal task. The authors suggest that in the performance task, as in a real road crossing, the dorsal visual stream was primarily activated and the ventral stream was activated in

the verbal task; thus, in order to achieve valid assessments of road crossing abilities, perception *must* be coupled with movement (further support for Gibson's theory).

Given that there are different perceptual pathways that are activated depending on whether or not perceptual judgments are coupled with movement, and their activation may have implications for pedestrian safety outcomes, it is not difficult to see why there may be inconsistencies in results reported in the child pedestrian research literature. As described above, some pedestrian methodologies elicit verbal judgements only, some ask participants to take one or two steps toward a real or virtual road, and some have participants walk across a pretend road that is physically displaced from the road in which they are attempting to make perceptual judgements (resulting in not fully accurate perceptual information being provided as the car approaches). This varying level of perceptual-motor coupling across methodologies likely explains the variety of, and inconsistencies in, research results about child pedestrian abilities. Not surprisingly, therefore, children's greatest abilities were obtained in the current study in which a fully immersive VR system was used and children experienced the highest level of perceptual-motor coupling.

In summary, the current study suggests that risk management is a very important consideration in research evaluating pedestrian risk. Ignoring it by choosing methodologies that do not measure in-road behaviour has likely resulted in underestimating how cautious children are before crossing, allowing many more adequate gaps to pass (missed opportunities) and have overestimated the prevalence of high risk crossings (hits and near misses). Additionally, ignoring this important perceptual/motor process may have the effect of biasing researchers to focus on curbside decision making and crossing initiation as opposed to considering how these factors interact with behavioural crossing measures. These findings suggest that future research should begin to focus more attention on understanding how risk management varies across different pedestrian contexts and whether reliance on evasive action as a risk management strategy is a factor that differentiates children from adults. If this is the case, targeting evasive action (i.e., teaching children to rely less on evasive action) may be an effective strategy for intervention. If we can find such large differences in the same children in the same study based solely on different measurement approaches, what is the impact of the multitude of differing research paradigms across studies and how readily can we compare or amalgamate findings? The current

study provides some important insight into the degree of bias introduced by estimating crossing behaviour that should aid in comparisons between past findings and future studies that use similar VR approaches. Many have concluded that pre-crossing decision-making and crossing initiation are critical determinants child pedestrian safety because these determine the time available to cross and if children have less time to cross then this clearly puts them at greater risk. This is a reasonable argument but it is one that is, in light of the current findings, based on biased results that over-estimate the influence of decision making on road safety. This is not to say that these are unimportant determinants of safety but how important they are needs to be reconsidered in light of these findings to help inform future directions for research and aid in the design of effective, behaviourally-based interventions that will lead to real world injury prevention.

Study Limitations and Future Research Directions

There are several limitations to this study and important research questions to address in future research. First, the sample used in this study was limited in terms of socioeconomic status, age and the degree of urbanization that the children had experienced. It would be informative to look at crossing skills in different SES and cultural groups. Evasive action, for example, may be much more important in highly populated heavily urbanized regions with less well developed traffic control systems, such as large cities in India or China.

Second, the study did not include an adult sample, but it would be informative in future studies to assess how evasive action differs between adults and children. Reliance on evasive action as a risk management strategy may be something that adults do less than children (because they make better initiation decisions) or that they are simply better at (i.e., children have been shown to overestimate their abilities). This may help to explain some of the discrepancy between adults and children in terms of their pedestrian skills. Furthermore, future research should assess what are the underlying mechanisms of evasive action. For example, we have seen in this study that evasive action is commonly employed and largely ineffective as a safety strategy when vehicles are approaching at a constant rate. How might reliance on evasive action change if vehicles slowed down in response to the child entering the road. Specifically, what role would a child's expectation that a car will slow down have on the crossing strategies they employ? Studies using cars that slowdown could be very informative in terms of looking at child x driver

behaviour interactions. For example, what are the consequences of differing levels of driver distraction on child safety when children have some expectation that vehicles will slow in response to them crossing?

Third, the current study did not explore how individual difference factors influence children's crossing skills but it would be informative to examine what cognitive and/or personality factors may predict reliance on evasive action as a risk management strategy. This is important because the current study showed that reliance on evasive action resulted in poor safety outcomes for children. For example, factors that predict poor pre-crossing decisions may in turn predict reliance on evasive action and children with certain personality characteristics (e.g., impulsive) may actively plan to use evasive action and choose riskier gaps as a consequence. Answers to these questions would be informative for the development of interventions aimed at promoting safer pre-crossing decisions.

Finally, the current findings on measurement bias have extended the findings from past studies that have looked at methodological issues in pedestrian research (Demetre et al., 1992; te Velde et al., 2005) and suggest that much more research is needed in this area. Not only is there measurement bias introduced when average walking speed is used to derive safety outcomes such as hits and initiation measures such as missed opportunities but the findings of te Velde and colleagues (2005) suggest that there are likely important perceptual differences at the neurological level between having children actually cross versus verbally reporting that they would or would not cross. What is currently unknown is if these perceptual differences influence pedestrian safety outcomes when there are varying degrees of perceptual-motor coupling. For example, the two-step technique wherein children indicate their crossing decision by taking two steps toward a real road may produce less biased measures of safety outcome because there is some perceptual-motor coupling in that the crossing is physically initiated. The VR system used in the current study offers a flexible and rigorous platform for extending our understanding of methodological issues in child pedestrian research. For example, a study could be done in which all major methodologies are compared in a within-subjects design. A number of experimental conditions could be randomized to assess for differences between children reporting their crossing decision verbally, by taking one or two steps toward the road, by crossing the close lane with no cars and pretending they are crossing the far lane with traffic and finally actually

crossing the road. This would help answer the question of how much perceptual-motor coupling is necessary to get valid estimates of safety outcomes. Additionally, measures could be calculated in a number of different ways including using pre-crossing average walking speed, pre-crossing running speed or some aggregate. Given that very few researchers currently have access to state of the art VR pedestrian simulators it is critically important that other methods such as pretend road, the two step technique or even verbal reports of decisions be improved to produce more valid findings and increase consistency across studies. For example by identifying specific measurement biases inherent in specific methodologies, steps may be taken to reduce these by making changes to the methodologies or statistical adjustments during analyses. There are a great many opportunities for improving pedestrian research and with the rapidly increasing availability of ever improving Virtual Reality applications more and more researchers will be able to take advantage of this critical tool for research.

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Figures

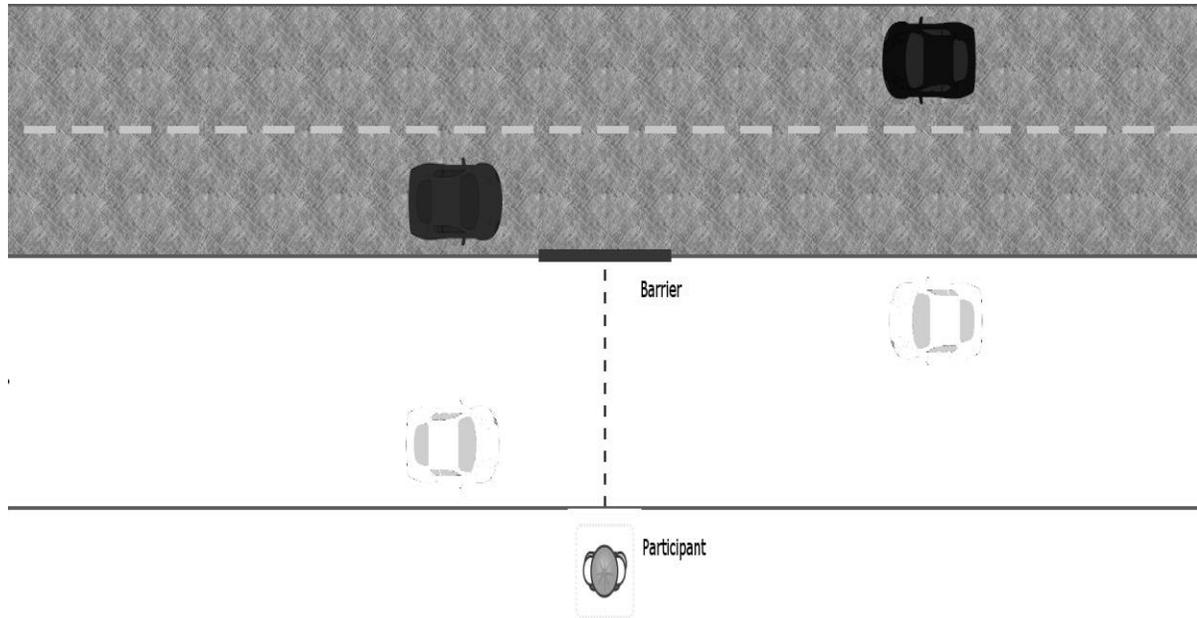
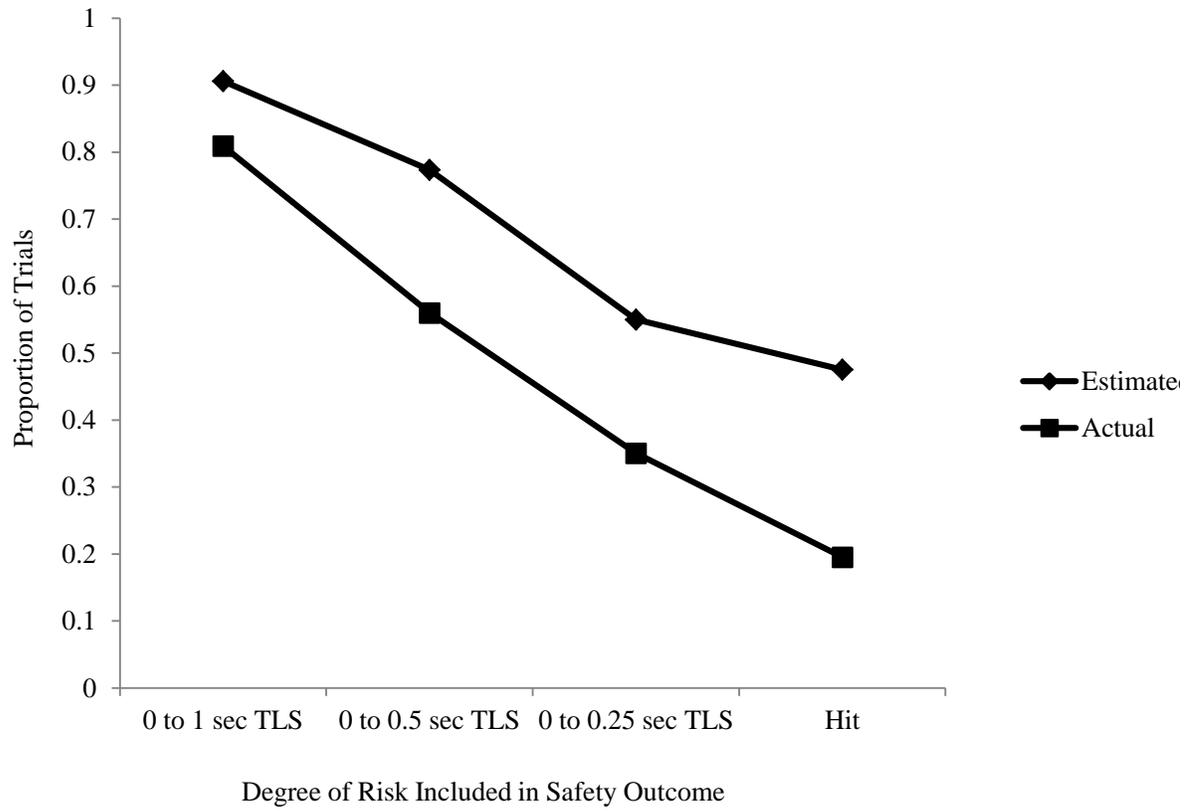
Figure 1. *An illustration of the pretend road methodology.*Figure 2. *Grey-scaled image of pedestrian simulator environment as seen through the head mounted display.*

Figure 3. The effect of the degree of risk included in the safety outcome on differences between actual and estimated time left to spare (TLS).



Tables

Table 1. *Pedestrian measure and concept definitions.*

Measure/Concept	Definition
Missed Opportunities	The number of inter-vehicle gaps that were rejected that were 1.5 times the length of the child's actual crossing time on that trial.
Missed Opportunities (Estimated)	The number of inter-vehicle gaps that were rejected that were 1.5 times the length of the child's estimated crossing time based on mean walking speed.
Start Delay	The time between the rear bumper of the first car in the chosen gap crossing the participants' path and the time when they step off the curb
Time Left to Spare (TLS)	The time remaining for the approaching car to intersect with the child's path. Calculated as the time left (in seconds) between the child and the approaching car when the child exits the path of the approaching car. When TLS = 0, the child was hit by the vehicle. Safety
Time Left to Spare (Estimated)	Time Left to Spare calculated using Walking Speed (Estimated) When TLS (estimated) = 0, the child would have been hit by the vehicle.
High Risk TLS/Estimated TLS	The proportion of trials with a TLS ranging from 0 (hit) to .25 seconds (very close call or possible hit)
Gap Choice	The inter-vehicle gap size (for the gap that the participant entered) measured in seconds rather than distance units. Calculated as the time in seconds from the rear bumper of the first car in the gap passing the participant and the arrival of the front bumper of the second car at the same point.
Walking Speed	Calculated at various points in the crossing as the distance travelled (in meters) / 0.05 seconds
Walking Speed (Estimated)	The mean walking speed as measured by the participant walking the same distance as the width of the street, without VR equipment, ten times.

Evasive Action

The difference in velocity from the point of entering the gap to the maximum velocity while in the path of the car

Table 2. *Repeated measures simple effects comparing safety outcomes derived through actual measurement versus estimates based on average walking speed.*

Estimated versus Actual TLS	Mean Difference	SD	95% CI		<i>t</i>	<i>df</i>
			<i>Lower</i>	<i>Upper</i>		
0 to 1 seconds	0.10	0.12	0.08	0.12	9.12	122
0 to 0.5 seconds	0.21	0.19	0.18	0.25	12.16	122
0 to 0.25 seconds	0.20	0.27	0.15	0.25	8.08	122
0 (Hits) seconds	0.28	0.26	0.23	0.33	11.97	122

All comparisons significant at $p < .001$

Table 3. Summary of hierarchical regression analysis predicting high risk time left to spare HRTLS measured directly (Actual) versus estimated based on average walking speed at pre-trials. ($N = 128$)

Variable	Step 1			Step 2		
	<i>B</i>	<i>SE B</i>	β	<i>B</i>	<i>SE B</i>	β
Outcome: HRTLS-actual						
Gender	-.02	.04	-.04	-.01	.03	-.03
Gap Choice				-.31	0.04	-.77***
Start Delay				0.74	0.10	.64***
R^2		.002			.41	
<i>F</i> for change in R^2		.21			42.89***	
Outcome: HRTLS-estimated						
Gender	-.12	.09	-.20*	-.05	.04	-.08
Gap Choice				-.53	0.04	-.91***
Start Delay				0.47	0.11	.31***
R^2		.04			.61	
<i>F</i> for change in R^2		5.00*			92.48***	

* $p < .05$. ** $p < .01$. *** $p < .001$

Table 4. *Net Regression Comparing Regression Analysis Predicting High Risk Time Left to Spare HRTLS Measured Directly (Actual) Versus Estimated Based on Average Walking Speed at Pre-trials. (N = 126)*

Variable	Step 1			Step 2		
	<i>B</i>	<i>SE B</i>	β	<i>B</i>	<i>SE B</i>	β
Outcome: HRTLS-actual Predicted minus HRTLS-estimated						
Gender	.09	.04	.20*	.03	.04	.07
Gap Choice				.23	0.04	.51***
Start Delay				0.13	0.11	.10
R^2		.04			.36	
<i>F</i> for change in R^2		4.88			30.21***	

* $p < .05$. ** $p < .01$. *** $p < .001$

Appendix A

Unit Testing of Virtual Reality Measures

The VR pedestrian simulator collects data in real time and saves these raw data (data consisting of the positions and head orientation of the participant as well as car position data) to the hard drive. Prior to analysis, the files are loaded and the measures are computed from the raw data. Processing raw data after the trials are complete also allows for more complete testing of the measures and more efficient use of computer hardware when the system is running trials. Importantly, data can be generated and used to test the individual measures without having to run a participant through the trials.

The measures were tested using a unit testing framework built on Python's pyunit (Python is a programming language). In software development, unit testing is a widely-used method in which the smallest parts of an application are tested for their correctness. We have created unit tests for each measure which ensure that the measures are computed correctly under all common scenarios, as well as a wide range of other possible, less likely, scenarios.

Each unit test ensures that a measure calculation outputs exactly as is expected. Raw data are generated with known outputs for the measures (based on their definitions). After we generate these data, we can use it to test our calculations. If the calculations produce the expected result we know that the calculation is correct. Each unit test does the following: mathematically generates car data based on a trial configuration (i.e., car velocity and behaviour, gaps, etc.); moves a participant around the cars so that we know exactly what's happening; saves this raw car and participant data to disk; loads the data and computes the measure we're testing (as if it was real participant data); indicates if the calculated value is equal to the expected value.

Appendix B

Correlations of covariates with dependent variables.

Measure	HRTLS Actual	HRTLS Estimated
Age (years)	.00	.03
Gender	-.21*	-.05
Traffic Exposure	.04	.10

* Significant at $p < .05$; Gender was correlated using a Spearman correlation.