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16. Abstract

This project continues to build upon the foundation provided by the human factors experimentation conducted in the previous Crash Avoidance Metrics Partnership (CAMP) Forward Collision Warning (FCW) system efforts. As in the previous CAMP FCW research, this work was conducted with a surrogate target, test-track methodology, which allows driver behavior to be observed under controlled, real approach, rear-end crash scenario conditions. The surrogate target, test-track methodology involves three vehicles—a lead vehicle, a mock vehicle (or surrogate target vehicle), and a subject vehicle that is driven by the test participant. The real driving conditions created with the surrogate target, test track methodology are likely to increase the chance that the crash alert timing approach developed will generalize to real-world conditions.

The major conclusions from this research are as follows:

- Based on test driver intervention rates during surprise trials, the alert timing approach evaluated, coupled with a single-stage, dual-modality (auditory plus visual) FCW alert, was found to be robust, effective, and judged appropriate across the wide range of conditions evaluated.
- The benefits of the FCW alert during surprise trials were restricted to tasks involving head-down glance activity and were not evident for the eyes-forward distraction tasks examined.
- Results from the time-to-collision (TTC) and first look visual occlusion studies suggest that, provided the driver is looking toward the lead vehicle, the driver can quickly assess TTC and make the appropriate crash avoidance maneuver under the alert timing assumptions evaluated.
- Across all the actual FCW alert or simulated FCW alert (via visual occlusion) conditions examined, there is generally a lack of both age and gender effects. This suggests that FCW alerts may be an effective means of equalizing a driver's abilities to avoid rear-end crashes.
- The "first look" method appears to be a valid, efficient, and promising method for exploring the consequences of later FCW alert timing (e.g., crash avoidance versus crash mitigation).

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Executive Summary

This project continues to build upon the foundation provided by the human factors experimentation conducted in the previous Crash Avoidance Metrics Partnership (CAMP)/NHTSA Forward Collision Warning (FCW) system efforts. As in the previous CAMP FCW research, this work was conducted with a surrogate target, test track methodology, which allows driver behavior to be observed under controlled, real-approach, rear-end crash scenario conditions. The surrogate target, test track methodology involves three vehicles—a lead vehicle, a mock vehicle (or surrogate target vehicle), and a subject vehicle that is driven by the test participant. Two individuals, an experimenter and a trained test track driver, ride in the subject vehicle with the test participant. The experimenter directs the test participant or "driver" through the protocol while the test driver rides in the front passenger seat with access to an override brake pedal and add-on steering wheel to prevent collisions with the surrogate target towed by the lead vehicle. The real driving conditions created with the surrogate target, test track methodology is likely to increase the chance that the crash alert timing approach developed will generalize to real-world conditions.

The first major goal of this research is to address the extent to which a wide range of factors have an impact on the effectiveness of a previously developed CAMP FCW timing approach. This work examined the extent to which alert effectiveness is influenced by driver characteristics, environmental factors, interface design, distraction activity, kinematic conditions, and training/false alarms. In addition, this work examined the degree to which knowledge of these factors would be useful for modifying the alert timing approach, and it investigated the benefits of an FCW alert (or alert presence) with an eyes-forward task versus a task involving head-down activity. To address these issues, a *surprise trial technique* was employed in which the driver is distracted intentionally by the experimenter. The distraction occurs immediately prior to the unexpected lead vehicle braking (or closing) event, which inevitably leads to an FCW alert presentation. Seventeen distinct surprise trial conditions were used. The key driver performance measure was the number of interventions performed by the front-seat, passenger-side test driver, who had access to add-on braking and steering controls.

The second major goal of this work is to use visual occlusion techniques under real approach conditions to further understand the driver's decision-making and avoidance maneuver behavior in rear-end crash scenarios, and to provide a calibration dataset for understanding how driver behavior and judgments compare under on-road versus simulated approach conditions. Simulated approach conditions (i.e., laboratory or driving simulator) involve degraded visual scene properties, which have been shown to influence *time-to-collision (TTC)* judgments, and hence, the driver's perception of crash threat. Two different visual occlusion techniques were employed to address this second major goal. The *TTC judgment occlusion technique* involved occluding the driver's vision during the last phase of an in-lane approach to a lead vehicle, after which the drivers were to press a button the instant they felt they would have collided with the vehicle ahead. The second technique employed, the *first-look occlusion technique,* involved occluding the driver's forward vision (as an extreme form of driver distraction) during the initial phase of an in-lane approach to a lead vehicle, after which the driver's vision was suddenly opened and the driver's task was to avoid colliding with the lead vehicle. The timing of the vision opening provided a test of the alert timing approach across a wide range of kinematic conditions. This technique is intended to simulate a "surprised" distracted driver, who, immediately following an

FCW alert presentation, must quickly decide upon and execute a crash avoidance maneuver. The key driver performance measure was the number of test driver interventions.

The major conclusions from this research are as follows. First, based on test driver intervention rates during surprise trials, the alert timing approach evaluated, coupled with a single-stage, dual-modality (auditory plus visual) FCW alert, was found to be robust, effective, and judged appropriate across the wide range of conditions evaluated. Overall, surprise trial intervention rates in the FCW alert and no-FCW alert conditions were 6.8 percent and 13.2 percent, respectively. The former intervention rate may be reduced if drivers received "valid" FCW alert experience/training, which was not provided here. In addition, results from the TTC and first-look studies provide further support for the alert timing approach evaluated under a wide range of kinematic conditions. Results from the first-look study indicate that under CAMP FCW alert timing conditions, drivers were able to execute an unassisted, successful braking maneuver for over 85 percent of the trials across the approach conditions examined.

Second, the benefits of the FCW alert during surprise trials were restricted to tasks involving head-down glance activity and were not evident for the eyes-forward distraction tasks examined. Furthermore, all test driver interventions occurred when the driver was looking down at the phone at FCW alert onset. Hence, a promising means of improving the CAMP FCW alert timing approach appears to involve sensing driver eye movement location. This sensing capability would not only improve alert timeliness for valid alerts issued when the driver is looking down, but just as important, such a capability would reduce the number of alerts perceived as unnecessary (or as "false alarms") by the driver because they were already looking at the forward scene.

Third, results from the TTC and first-look visual occlusion studies suggest that, provided the driver is looking toward the lead vehicle, the driver can quickly assess TTC and make the appropriate crash avoidance maneuver under the alert timing assumptions evaluated. Experimental manipulations explicitly intended to represent distracted driver conditions (i.e., limiting the forward viewing time to one second and/or introducing a concurrent distraction task) did not adversely impact the driver's TTC judgments. Overall, these results provide support for the view that drivers primarily employ a direct, efficient, and automatic *optic flow heuristic* for making TTC estimations, which may be modified based on speed and relative velocity conditions. Results also suggested that the probability that perceived TTC exceeded actual TTC increased as relative velocity increased, which could be useful for modifying alert timing assumptions.

Fourth, across all the actual FCW alert or simulated FCW alert (via visual occlusion) conditions examined, there is generally a lack of both age and gender effects. This suggests that FCW alerts may be an effective means of equalizing a driver's abilities to avoid rear-end crashes. The general lack of age and gender effects across studies (as well as the lack of viewing time and distraction effects on TTC judgments) suggests that drivers rather uniformly and consistently perceive and act upon low TTC conditions. This suggests that a "one-size-fits all" FCW alert timing approach for closing alerts may be feasible.

Fifth and finally, the first-look method appears to be a valid, efficient, and promising method for exploring the consequences of later FCW alert timing (e.g., crash avoidance versus crash mitigation). Later FCW alert timing may serve to reduce false alarms, and hence, potentially increase the overall "credibility," acceptability, and safety effectiveness of the FCW alert system. Indeed, reducing the number of “cry wolf” false alarms that drivers experience to a level considered acceptable by drivers, while still maintaining effective valid alert timing, remains a formidable challenge for FCW deployment and effectiveness.

Introduction

The current project builds upon the foundation provided by the human factors work conducted in the previous Crash Avoidance Metrics Partnership/NHTSA Forward Collision Warning system program (Kiefer, LeBlanc, Palmer, Salinger, Deering, and Shulman, 1999), as well as Task 1 efforts under the current CAMP FCW Requirements Project (Kiefer, Cassar, Flannagan, LeBlanc, Palmer, Deering, and Shulman, 2003). The experimental work reported here has two major goals. Similar to Task 1, these goals are being addressed under closed-course conditions employing the surrogate target methodology developed in the first CAMP FCW Project. This test track methodology allows driver behavior to be observed under safe, controlled, and realistic rear-end crash scenarios. Attempts to define a crash alert timing approach based on research that places drivers under minimal or no crash risk conditions (e.g., simulator), has the potential to lead to an alert timing approach which is too late or too aggressive (Kiefer, et al., 1999).

The first major goal of this research was to address the extent to which a wide range of factors had an impact on the effectiveness of a CAMP FCW timing approach developed by Kiefer, et al. (1999, 2003). This work examined the extent to which alert effectiveness is influenced by driver characteristics (age, gender), environmental factors (day/night), interface design (e.g., number of alert stages, alert sounds, heads-up displays), distraction activity (e.g., eyes-forward versus tasks involving head-down activity), kinematic conditions, and training/false alarms. In addition, this work also examined the effect of FCW alert presence (relative to a baseline, no alert condition) with an eyes-forward task versus a task involving head-down activity. The alert timing approach employed was based on the required deceleration approach developed in the first CAMP FCW project and slightly revised under Task 1 of the current FCW project (Kiefer, et al., 2003). The *Surprise Trial Methodology Technique* fruitfully employed in the first CAMP FCW project (Kiefer, et al. 1999) was again used here for executing 17 distinct surprise trial conditions. In this technique, the driver is distracted intentionally by the experimenter immediately prior to an unexpected braking event, which inevitably leads to an FCW alert presentation. A key driver performance measure is the number of interventions required from the test driver, who had access to an add-on brake and steering wheel and a “bail out” crash alert (which signaled the test driver to take control over the vehicle). The major impetus for this surprise trial work is to ensure that the CAMP FCW timing approach continues to remain effective under a wider range of conditions than those previously examined by Kiefer, et al. (1999). Furthermore, this data was used to assess the degree to which knowledge of various factors (e.g., eye movement location, time of day) would be useful for modifying the CAMP FCW timing approach.

The second major goal of this work was to use visual occlusion techniques under real approach conditions to further understand a driver’s decision making and avoidance maneuver behavior in rear-end crash scenarios and to provide a calibration dataset for data gathered under simulated approach conditions. With respect to this latter point, since the National Advanced Driving Simulator (NADS) will likely be used as a tool for examining the effectiveness of FCW systems (as well as other crash avoidance systems), it is important to understand how driver behavior and judgments compare under on-road versus NADS conditions. A potential strength of NADS is that it provides a tool for examining driver behavior under more complex traffic and crash scenarios than may be logistically possible under on-road conditions. However, before complex driving scenarios are used in NADS for FCW system evaluations, it is important to gain a more fundamental understanding of the relationship between on-road and NADS data under relatively simple rear-end crash scenarios. These simpler scenarios can be used as a starting point for understanding how a driver's perception of the forward roadway scene in the simulator (which has degraded visual scene properties relative to real-world scenes) impacts the driver's ability to quickly perceive and interpret the scene, project vehicle-to-vehicle kinematic conditions (e.g., time-to-collision), and decide upon the nature, time-course, and aggressiveness of the last-second crash avoidance maneuver.

In order to address this second goal, two different visual occlusion techniques were employed. The *time-to-collision judgment occlusion technique* involved occluding the driver's vision during the last phase of an in-lane approach to a lead vehicle, after which the drivers' tasks were to press a button at the instant they felt that they would have collided with the vehicle ahead (assuming the speeds of both vehicles remained constant). Since the previous Kiefer, et al. (2003) research suggested that inverse TTC plays a key role underlying a driver's perception of normal, versus hard, last-second braking envelopes, TTC judgments and the relationship between actual and perceived TTC are of inherent interest. The second technique, the *first-look occlusion technique,* involved occluding the driver's forward vision (as an extreme form of driver distraction) during the initial phase of an in-lane approach to a lead vehicle, after which the driver's vision was opened and the driver's task was to avoid colliding with the lead vehicle. The timing of the vision "opening" provided a test of the CAMP FCW alert timing approach under a much wider range of vehicle-to-vehicle kinematic conditions than those examined in previous and current CAMP FCW surprise trial research. This technique is intended to simulate a "surprised" distracted driver, who, immediately following an FCW alert presentation, must quickly decide upon and execute a crash avoidance maneuver. As in the surprise trials, the key driver performance measure for this technique is the number of assists, or interventions, provided by the passenger-side test driver.

Surprise Braking Trials

The primary goal of these trials was to assess the roles a wide assortment of factors play in influencing the effectiveness/robustness of the CAMP FCW timing approach. The factors explored in these trials included driver age, driver gender, alert presence, alert sound type, visual alert type, number of alert stages, driver distraction activity, false alarms/training, time of day, vehicle-to-vehicle kinematic conditions, and lead vehicle brakelight presence. The alert timing approach employed was based on the required deceleration approach described in Kiefer, et al. (2003), coupled with a 1.52-second brake reaction time (or 95th percentile brake RT) assumption (Kiefer, et al., 1999). In addition, this research also analyzed the degree to which knowledge of the factors examined would be useful for modifying the CAMP FCW timing (e.g., increasing alert timing ranges when the driver is looking down), and provided a methodology and comparison dataset for guiding similar work involving simulated in-lane approaches.

The *surprise trial methodology technique* previously employed by Kiefer, et al. (1999; Experiments 2, 3, and 4) was used here, employing a *surrogate target* lead vehicle (shown in figure 1). The surrogate target, or lead, subject vehicle (SV) refers to the vehicle immediately ahead of the driver's vehicle during a driving maneuver. The surrogate target consists of a molded composite mock-up of the rear half of a passenger car mounted on an impact-absorbing trailer that is towed via a collapsible beam. The surrogate target is towed by a principle other vehicle (POV). The POV braking level, as well as that of the yoked surrogate target, can be controlled by the experimenter. The surrogate target provides a realistic crash threat to drivers under closed-course conditions, yet is able to absorb impacts of up to 10 mile per hour velocity differential without sustaining permanent damage. Thus, the surrogate target methodology allows experimenters to safely place naive drivers in last-second braking, realistic rear-end crash scenarios on a closed test track and to observe their behavior.

One general method was used across the 17 distinct surprise trial conditions examined involving a total of 260 drivers. At the start of sessions, the drivers were told that they were evaluating an advanced driver information system and then trained on an in-vehicle task while parked. The drivers then followed the lead vehicle at their "normal" preferred headway for either 1½ lengths of track (under no false alarm conditions, in which the drivers were not aware the vehicle was equipped with an FCW alert) or 5½ lengths of track (under false alarm conditions, in which the drivers learned that the vehicle was equipped with an FCW alert). During this drive, the driver performed simple versions of the in-vehicle task prior to the unexpected, surprise braking event (except in the case of the stationary lead vehicle, surprise condition, which is described later). The driver was then intentionally distracted with a complex version of the in-vehicle task while the lead vehicle braked unexpectedly, resulting in the presentation of the FCW alert (except in the no-alert, baseline condition). The driver performance measures included passenger-side experimenter brake assists, driver brake reaction time (RT) to the alert, the (constant) required deceleration level to avoid impact at SV brake onset, time-to-collision at SV brake onset, time headway at POV brake onset, and peak deceleration throughout the maneuver. Subjective measures included alert noticeability and alert timing ratings. Using this general method, various factors of interest were manipulated across the various surprise trial conditions.

Figure 1. *CAMP Surrogate Target Methodology*

Method

Participants

The 260 participants were from three age groups, 20 to 30 years, 40 to 50 years, and 60 to 70 years. (Sampling from select age strata, rather than all possible ages, increased the number of participants in each age group while maintaining a sample representative of the age range in the general population.) The age by gender breakdown of the participants is shown in table 1. Each participant completed one surprise trial. Forty-eight participants from this study also participated in the first-look study (described later in the paper) immediately after completing their single surprise trials. Participants completing just the surprise trial received $125 for participation, whereas surprise trial participants also participating in the first-look study received $150 for their participation. Naive participants were recruited from a database of licensed drivers in the metropolitan area surrounding the test facility via an outside independent market recruiting firm. The recruitment criteria required that participants be licensed drivers who drove regularly and were free of any conditions that may have limited their ability to safely participate in the test.

Table 1. ***Subject sample breakdown***

	Age		
Gender	20 to 30 years	40 to 50 years	60 to 70 years
Female	28	42	62
Male	28	45	55

Subject vehicle, surrogate target lead vehicle, principal other vehicle

The CAMP surrogate target methodology apparatus was identical in nearly all aspects to that employed in previous CAMP FCW work (Kiefer, et al., 1999, 2003). The following vehicle driven by the participant was a 1997 Ford Taurus SHO, referred to as the subject vehicle (or SV). The principal other lead vehicle (referred to as the POV), also a 1997 Ford Taurus SHO, towed the surrogate target lead vehicle assembly. The surrogate target was a three-dimensional mock-up of a Chevrolet Monte Carlo rear end mounted on a lightweight trailer frame. (Previous CAMP FCW work employed a Mercury Sable rear end.) The mock rear end was constructed of polyurethane with a thin, reinforcing fiberglass undercoat, and equipped with working brake lights. A close-up view of the surrogate target from the following driver's perspective is provided in figure 2. The rear end of the trailer assembly was equipped with a high-density Styrofoam and coiled spring bumper. The mock rear end and trailer were attached to a 40-foot (12.2 m) telescoping tow beam capable of collapsing approximately 9 feet (2.7 m). An additional vehicle, a 2002 Chevrolet Suburban, was used as an ancillary lead vehicle in the stationary lead vehicle, surprise trial condition (described below). In this condition, the entire rear surface of the Suburban was covered with a flat black, non-reflective material to make it invisible to the laser-radar ranging device on the SV. This covering ensured that the parked lead vehicle rather than the Suburban triggered the FCW alert.

<u>Figure 2</u>. *CAMP Surrogate Target – Rear View*

Data acquisition, experimenters, and instrumentation

The SV and POV were instrumented to continuously record various measures at 30 Hz, including the range between the two vehicles, and the speed, longitudinal acceleration, and lateral acceleration of both test vehicles. The SV and POV data acquisition systems were networked using a LAN link. Two experimenters rode in the SV with the test participant. The back-seat experimenter instructed participants through the trials and operated the data acquisition system. This data acquisition system allowed the back-seat experimenter to automatically control the POV speed and POV deceleration levels (which could be overridden by the POV test driver). The front-seat passenger-experimenter, a trained test driver, had access to a passenger-side override brake pedal and an add-on steering wheel to prevent collisions with the surrogate target. A curtain strung from the top of the instrument panel to the back of the front passenger seat, hanging to the right of the center stack, blocked the test participant's view of the add-on steering wheel and brake pedal from the driver's seat. The SV experimenters and the POV test driver communicated during the study via digital radio communication.

As shown in figure 3, a Nextel cellular phone secured in a hands-free cradle was mounted to the center stack console of the SV for use in two of the driver distraction tasks, the digit span dialing task and the visual-spatial (mental rotation) distraction task (described below). Participants could easily reach the cellular phone's keypad with their right hand as well as read the lighted display while the phone remained securely mounted. Participants used a headset with an earpiece and microphone connected to the cellular phone to operate the phone for the Tell Me distraction task (described below). For the visual-spatial distraction task, the keypad was marked with the letter N above the 2 key, the letter E above the 6 key, the letter S above the 8 key and the letter W above the 4 key.

<u>Figure 3</u>. *Cell Phone Mounting Location for Digit Span Dialing and Visual-Spatial Distraction Tasks*

An infrared illumination lamp was added to the SV for use in the surprise trials conducted under nighttime conditions. The lamp illuminated the participant's face for videotaping purposes. The lamp was mounted with Velcro to the front edge of the headliner in front of the driver and was adjusted for each participant's seating position.

Experimental Design (Surprise Trial Conditions)

Each participant experienced one of 17 different surprise trial conditions (shown in table 2). The rightmost column with the heading Y/M/O shows the number of participants in the youngest, middle, and oldest age groups, respectively, for each condition. The number of participants in each condition varied depending on the age groups included in the condition and the difficulty of the experimental procedures. These differences are explained further in the description of the procedures for each condition. The surprise trial conditions differed on various factors, each of which will now be described below. For brevity purposes, each of the 17 conditions shown in table 2 will be referred to as conditions C1 through C17 in the discussion below.

Table 2. ***Surprise Trial Conditions***

Cond.	Distractor Task	Kinematic Condition SVspeed/POVspeed/POVDecel.	Time	Alert Sound-Visual	Alert Stages?	False Alarms?	POV Brakelights?	N	Age Groups	Y/M/O
1	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP-HHD	1	No	Yes	25	Y, M, O	7 / 7 / 11
2	Digit Span Dialing	30 / 30 / 0.39 constant	Day	No Alert	NA	NA	Yes	22	Y, M, O	8 / 6 / 8
3	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP-HHD	2	No	Yes	15	M, O	0 / 8 / 7
4	Digit Span Dialing	30 / 30 / 0.39 constant	Night	CAMP-HHD	1	No	Yes	16	M, O	0 / 10 / 6
5	Digit Span Dialing	30 / stationary	Day	CAMP-HHD	1	No	Yes	6	M, O	0 / 3 / 3
6	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	CAMP-HHD	1	No	Yes	17	M, O	0 / 9 / 8
7	"Tell Me"	30 / 30 / 0.39 constant	Day	CAMP-HHD	1	No	Yes	17	M, O	0 / 8 / 9
8	Grammatical Reasoning	30 / 30 / 0.39→0.15 non-constant	Day	CAMP-HHD	1	No	Yes	7	M, O	0 / 4 / 3
9	Grammatical Reasoning	30 / 30 / 0.39→0.15 non-constant	Day	CAMP-HHD	1	No	No	8	M, O	0 / 3 / 5
10	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP-HHD	1	Yes (5)	Yes	12	M, O	0 / 5 / 7
11	Digit Span Dialing	30 / 30 / 0.39 constant	Day	Friendly-HHD	1	No	Yes	13	M, O	0 / 6 / 7
12	Digit Span Dialing	30 / 30 / 0.39 constant	Day	Friendly-HHD	1	Yes (1)	Yes	22	Y, M, O	8 / 7 / 7
13	Digit Span Dialing	30 / 30 / 0.39 constant	Day	None-HUD	1	Yes (1)	Yes	16	Y, O	8 / 0 / 8
14	Digit Span Dialing	30 / 30 / 0.39 constant	Night	None-HUD	1	Yes (1)	Yes	8	O	0 / 0 / 8
15	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	None-HUD	1	Yes (1)	Yes	16	Y, O	8 / 0 / 8
16	Grammatical Reasoning	30 / 30/ 0.39 constant	Day	No Alert	NA	NA	Yes	16	Y, O	8 / 0 / 8
17	Visual-Spatial	30 / 30 / 0.39 constant	Day	CAMP-HHD	1	No	Yes	24	Y, M, O	8 / 8 / 8

Note: For Alert Type column, CAMP=CAMP sound/HHDD visual, Friendly= Friendly sound/HHDD visual, and HUD=flashing HUD only.

Distraction tasks. Participants performed one of four distraction tasks during the surprise braking event. Participants first completed a set of practice trials for the distraction task while the vehicle was parked. Participants then completed distraction task trials while following the lead vehicle as it maintained a constant speed before performing the final task, a complex version of the distraction task, immediately prior to the surprise lead vehicle braking event. A description of each of the four distraction tasks (digit span dialing, grammatical reasoning, Tell Me, and visual-spatial tasks) will now be described.

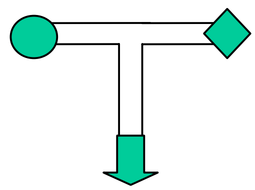
The digit span memory task (described below in the *Experimental Procedure* section) was employed during practice trials for the *digit span dialing* task. The practice trials were completed while the vehicle was parked. Next, while driving, the experimenter asked the participants to dial their home phone numbers with area code on the cellular phone keypad. This request occurred before the vehicles reached the location for the surprise braking event. At the surprise braking event location, the experimenter recited a series of digits while the participant listened. The number of digits in the series for participants was determined by their respective performances in the digit span memory task. After the experimenter finished reciting the list, the participant was required to dial the series of digits in order on the cellular phone keypad. The lead vehicle was signaled to begin the surprise braking event just as the participant's hand began to move to the keypad to begin dialing.

In the *grammatical reasoning* task (Baddeley, 1968), a sentence involving the order of A and B ("B precedes A") was read by the experimenter followed by a letter order ("BA"). The participant's task was to report whether the order conformed to the sentence by stating true or false. For example, if the statement was "B precedes A" followed by the order "BA," the correct response is true. Participants judged seven statement-order pairs during training with the vehicle parked. Participants were given feedback on incorrect answers during training. Next, while driving, the participant judged two more statement-order pairs before reaching the location for the surprise braking event. At the surprise braking event location, the participant was given a particularly difficult statement-order pair (i.e., "B is not preceded by A – AB") involving a negative. The lead vehicle was signaled to brake immediately after this sentence-order pair was presented.

The *Tell Me* task employed the 1-800-Tell-Me free telephone service that provides driving directions, weather, traffic, and sports information. The automated Tell Me system directs callers to navigate through a speech-activated menu to access the various types of available information. The participants in this study first practiced using the Tell Me system while the SV was parked. Participants called the Tell Me system using a hands-free headset that was connected to the Nextel cellular phone. The practice tasks involved retrieving driving directions to a location close to the test facility. While driving, the participant accessed the system again to obtain a traffic report for local roads. This task occurred before the vehicles reached the location for the surprise braking event. At the surprise braking event location, the experimenter asked the participant to retrieve driving directions for traveling from their home address to a fictitious address in Orlando, Florida. The Tell Me system, unable to find the fictitious address, would request that the participant repeat the destination address. The lead vehicle was signaled to begin the surprise braking event as the participant received the error messages and repeated the address.

For the *visual-spatial* (mental rotation) task, the participant first memorized a visually presented figure (shown in figure 4) that was composed of a block-style capital letter T with a different shape – a circle, a diamond, or an arrow -- on each endpoint. For the practice trials, the participant reported from memory the direction of each shape, north, south, east, or west, using the four direction keys marked on the cellular phone keypad. The experimenter then instructed the driver to mentally rotate the imagined figure until the arrow pointed north and report the direction of a specific shape with a key press. The participant completed these practice trials while the vehicle was parked. After the participants started driving, the drivers were asked to press the key for the direction of their homes from the test facility. This task occurred before the surprise braking event location. At the surprise braking event location, the experimenter instructed the participant to again imagine the figure, and mentally rotate it in counter-clockwise fashion until the arrow pointed north, and then report the direction of the diamond with a key press. The lead vehicle was signaled to brake immediately after the instructions were presented.

Figure 4. ***Figure Used for Visual-Spatial (Mental Rotation) Task***



Alert presence/auditory-visual alert types. In trials when an alert was present, participants experienced an FCW alert during the surprise braking event. In trials without an alert (subsequently referred to as no-FCW alert or baseline trials), participants did not receive an FCW alert during the surprise braking event.

Four different FCW alerts were examined. The first alert type consisted of a one-stage, dual-modality alert that combined the CAMP auditory alert sound (Kiefer, et al., 1999) with a red flashing high head-down display (HHDD). The second alert type consisted of a two-stage, dual-modality alert that again combined the CAMP auditory alert sound (Kiefer, et al., 1999) with a flashing HHDD. The second (later) stage of this alert was identical to the first alert type described above. The earlier stage of this alert was generated by preceding the second stage with a one-second steady (non-flashing) presentation of the HHDD. The third alert type consisted of a one-stage, dual-modality alert that combined a "friendlier" alert sound with a red flashing HHDD. This alert sound was a simple 2,000 Hz beep played back at five times per second with a 50 percent duty cycle. The fourth alert type consisted of a one-stage, flashing head-up display (HUD) visual alert. No alert sound was played in this "HUD visual alert only" condition. Illustrations of the HHDD and HUD visual alerts are shown in figure 5. The auditory crash alerts were played through the vehicle's front audio system speakers at 75 dBa.

Figure 5. *High Head-Down Display (HHDD) and Head-Up Display (HUD) Visual FCW Alerts*

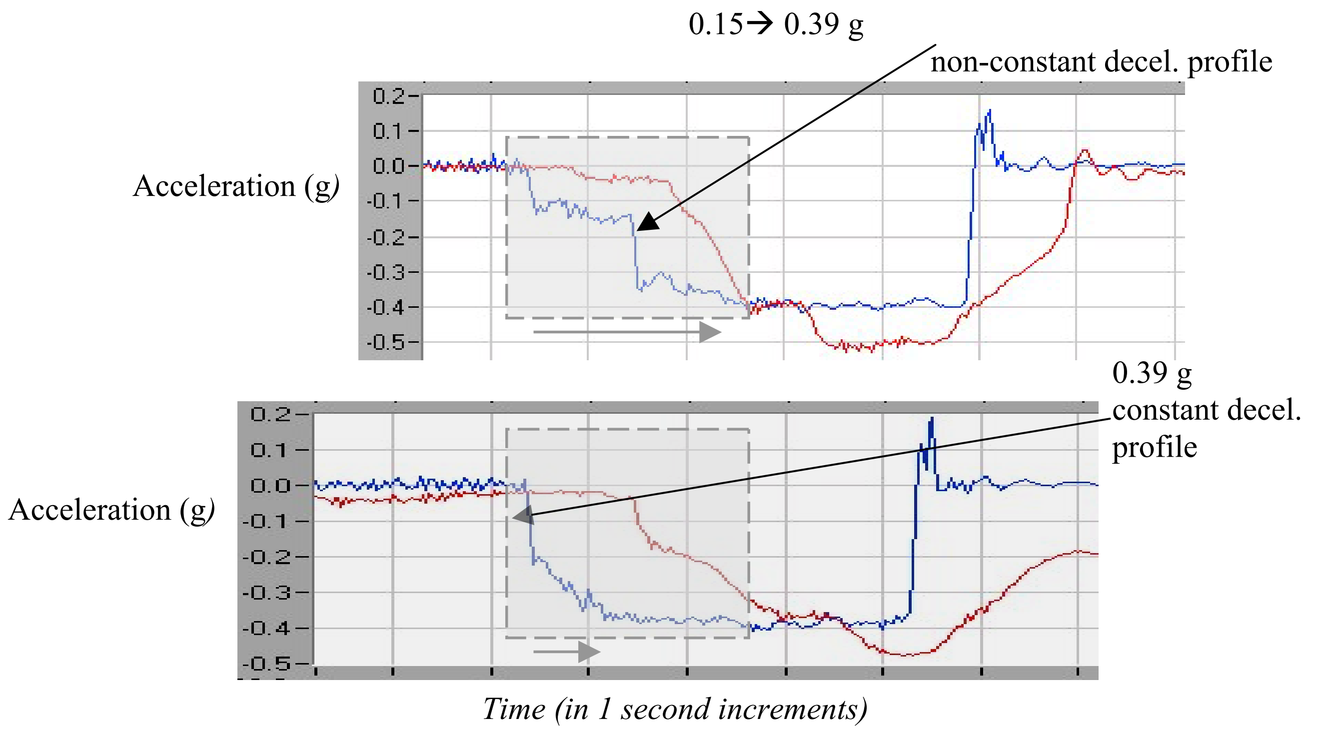
<u>False alarms/training</u>. Participants in the conditions involving false alarms (conditions C10, C12, C13, C14, and C15) experienced the FCW alert prior to the surprise braking event while driving the SV on the test track. Under these false alarm conditions, participants traveled 5 ½ lengths of the track to allow time for the false alarms to occur prior to the surprise braking event. The FCW alert(s) occurred as the participant drove through the turnaround loops at each end of the test track. In the five (high) false-alarm condition, participants experienced five false alarms prior to the surprise braking event (one at each turnaround). In the one (low) false-alarm condition, participants experienced one false alarm during the first turnaround. When the false alarm first occurred in both the five and one false alarm conditions, the experimenter explained to the participant that other engineers who were developing a collision warning system had used the SV previously, that the system could not be turned off, and that the signs and banked roadside on the turn-around loops triggered the alerts. Participants in the no-false-alarm conditions did not experience the FCW alert prior to the surprise braking event (and hence, received no FCW alert training) and traveled 1 ½ lengths of the track.

<u>Brakelight presence</u>. In the one no-brake-lights condition (C9), the lead vehicle decelerated without the brake lights activated during the surprise braking event. The lead vehicle decelerated with brake lights illuminated in all remaining conditions.

<u>Kinematic condition</u>. In 14 of the 17 surprise trial conditions (all but conditions C8, C9, and C5), the surprise braking event involved the lead vehicle braking unexpectedly at a constant, controlled 0.39 g deceleration level while the participant performed a distraction task. Prior to the surprise braking event, the SV followed the lead vehicle while both vehicles traveled at 30 mph. At the surprise trial location, the experimenter presented the participant with a distraction task. While the participant was distracted, the experimenter sent a silent computer message to the POV test driver to initiate the lead-vehicle braking event. The lead vehicle then began braking using a constant 0.39 g deceleration rate. At this point, it was necessary for the participant to perform a driving maneuver to avoid colliding with the lead vehicle.

Of the three remaining surprise trial conditions, two of the conditions (C8 and C9) were identical to the 14 conditions described above with the exception that the lead vehicle braked at a nonconstant deceleration level. In these conditions, the lead vehicle braked automatically at a constant 0.15 g deceleration level for approximately 1.2 seconds followed by the lead vehicle braking automatically at a constant 0.39 g deceleration level until the vehicle came to a stop. A graphic comparison of these two deceleration profiles is shown in figure 6.

<u>Figure 6</u>. *Nonconstant (0.15→0.39 g) versus Constant Lead Vehicle Deceleration Profile (POV deceleration shown in blue; SV deceleration shown in red)*



In the one remaining surprise trial condition (C5), the surprise braking event involved a stationary (or parked) lead vehicle. An illustration of the trial flow for this condition is shown in figure 7. This trial differed from the lead-vehicle braking trials in that the participant only had traveled three-quarters of a mile on the test track when the surprise braking event was introduced. This staging change prevented the participant from seeing the lead vehicle parked in the lane ahead of them. The trial began with the participant following the ancillary vehicle rather than the lead vehicle at the beginning of the test track, ostensibly for the purpose of beginning the actual test at the other end of the test track. The lead vehicle was parked in the driver's lane three-quarters of a mile away from the beginning of the test track. The SV followed the ancillary vehicle at 30 mph. When the participant began dialing their distraction task at the surprise trial location, the ancillary vehicle immediately moved into the left lane and accelerated. At this point, the parked lead vehicle was directly in front of the participant. It was then necessary for the participant to perform a driving maneuver to avoid colliding with the lead vehicle. To discourage the driver from changing lanes and following the vehicle ahead, an additional vehicle followed the driver's vehicle in the left adjacent lane near the driver's blind spot, and drivers were told that they needed to stay in the lane because the data acquisition was using lane marker information.

Figure 7. ***Stationary Surprise Trial Flow (Condition C5)***

Time of day. In 15 of the 17 surprise trial conditions, participants drove under daytime driving conditions. In the 2 remaining surprise trial conditions (C4 and C14), participants drove under nighttime driving conditions.

Experimental Procedure

The study was conducted on a straight, level, two-lane road at the General Motors Milford Proving Ground in Milford, Michigan (shown in figure 1). The lanes were 5,280 feet (one mile, or 1,609 m) in length and 12 feet (3.7 m) wide. All testing was conducted during dry road, daytime conditions.

Each surprise trial began with the SV, POV, and lead vehicle positioned in the right lane at the beginning of the track, with the SV parked directly behind the lead vehicle. The one exception to this initial staging was that in the stationary lead vehicle surprise trial condition (C5), the SV was parked behind the ancillary lead vehicle at the beginning of the track and the POV and lead vehicle were parked three-quarters of a mile ahead in the lane. Participants were escorted to the parked SV as soon as they arrived at the track.

Participants were initially misled about the actual purpose of the study. Participants were told that the aim of the study was to evaluate in-vehicle information systems. The experimenter told the participant that the system they were about to evaluate would provide drivers with information such as telephone numbers or driving directions using one of multiple communication modes such as a visual display or an auditory message. The experimenter then explained that the participant would complete a series of tasks in the vehicle while parked and while driving that would aid the researcher's understanding of a driver's information processing capabilities. If the surprise trial condition involved the HUD FCW alert, the experimenter explained that visual messages would be displayed on the HUD later in the experimental session. The participant then adjusted the HUD so that it was clearly visible in their forward view as the word "CAMP" was displayed.

The first in-vehicle task, the digit span memory task (Lamble, Kauranen, Laakso, and Summala, 1999), began once the participant was comfortably seated, belted, and ready to drive. This task was completed while all of the test vehicles were parked. In this task, the participant first listened to a series of digits then dialed the series on the cellular phone keypad. The first series given contained three digits. If that series was dialed correctly, the next series, which increased in length by one digit, was given. This sequence of, experimenter presenting the digit series incremented by one, followed by participant dialing, was repeated until the participant dialed a digit series incorrectly. Once the participant dialed incorrectly, the length of the next series was decreased by one. If the participant then dialed three series of that length correctly, that length was recorded as the participant's memory digit span. If not, the length of the next series was decreased by one until three correct answers were obtained at a given digit span length.

After the digit span memory task, participants completed training trials for the grammatical reasoning, Tell Me, or the visual-spatial distraction task depending on their surprise trial condition assignment. The test vehicles remained parked during the training trials. Participants in the digit span dialing distraction task condition began the moving trial after completing the digit span memory task.

The moving trial immediately followed the training trials. The experimenter told the participant that the lead vehicle would begin moving and that they should follow it at their normal following distance. No target speed was specified. The lead vehicle accelerated to a 30 mph target speed and the participant followed. If participants maintained a long headway (i.e., >100 ft.) at any point during the moving trial, the experimenter encouraged them to "close up the gap" in order to avoid obtaining data with initial conditions with unrepresentative, long headways (above 2 seconds). The participants in conditions that did not involve false alarms (with the exception of condition C5) followed the lead vehicle for 1 ½ lengths of the track before the surprise braking event occurred. Participants in conditions with 1 or 5 false alarms followed the lead vehicle for 5 ½ lengths of the track before the surprise braking event occurred. Participants in the stationary lead vehicle condition (C5) followed the lead vehicle for three-quarters of the length of the test track before the surprise braking event occurred.

The surprise braking event occurred at the same location, shortly after a turn-around loop, in all of the conditions except the stationary lead vehicle surprise condition (C5). While driving and just before entering the turn-around loop, participants performed a simple version of the distraction task during which the lead vehicle maintained a constant speed. After the simple distraction task and the turn-around loop were completed, a difficult version of the distraction task was presented. As the participant performed the difficult distraction task, the experimenter silently signaled the lead vehicle to begin the surprise braking event. The rapid lead vehicle deceleration triggered the alert presentation in conditions employing an FCW alert. In conditions without an FCW alert, there was no notification to the participant that the lead vehicle was braking.

If an FCW alert was issued during the surprise braking event, the participants were questioned about the noticeability of the alert after they completed the avoidance maneuver, as well their opinions on the timing of the alert. (See Kiefer, et al. (1999) for a description of the alert noticeability and alert timing questionnaire.) These questions were open-ended at first, asking the participants whether they noticed anything in the vehicle. If the participants noticed an alert, they were asked to elaborate about the location and characteristics of the alert. If the participants did not report an FCW alert, they were asked further specific questions to verify that they had indeed failed to notice the alert.

If a participant failed to react to the lead vehicle braking, a computer-calculated, silent signal notified the front-seat passenger test driver to intervene by performing an avoidance maneuver. The frequency of test driver interventions for each condition was recorded. No collisions with the lead vehicle occurred during the surprise braking trials.

Driver performance measures

The driver performance measures examined included passenger-side experimenter brake assists, driver brake reaction time to the alert, the (constant) required deceleration level to avoid impact at brake onset, time-to-collision (TTC) at brake onset, time headway at brake onset, and peak deceleration throughout the maneuver. Subjective measures included alert noticeability and alert timing ratings.

Results and Discussion

With a few exceptions, the focus of the following analysis primarily involved pairwise comparisons of conditions that differed only on the experimental factor of interest. Furthermore, age and gender effects are only reported if there is an interaction with the factor of interest. This analysis is complicated by missing data caused by test driver assists (or interventions). These intervention data were treated as missing data and no attempt was made to substitute “extreme” values to represent these interventions in the analysis of the -nonintervention data. Hence, caution must be exercised when interpreting results from -nonintervention data in comparisons involving “high intervention” rate conditions (e.g., the baseline, no-FCW alert condition). However, there were no instances where the pattern of results in the -nonintervention data was in the opposite direction of the pattern of results observed in the intervention data. All -nonintervention effects reported below are statistically significant at the $p < 0.05$ level. Marginal ($p < 0.10$) effects involving the brake RT measure are also reported because of the importance of this measure for crash alert timing purposes. For brevity purposes, each of the 17 conditions shown in table 2 will be referred to as conditions C1 through C17 in the discussion below. For the nonintervention data analysis discussed below, an analysis of variance (ANOVA) was performed separately on the each of five dependent measures: time headway at POV braking; driver brake RT to alert; TTC at time of SV brake onset (constant); required deceleration at SV brake onset;

and peak deceleration throughout the maneuver. Note the time headway measure is intended to provide a check on initial conditions to ensure that any effects observed with the other four measures cannot be attributed to differences in initial surprise trial conditions. However, it should be noted that previous surprise trial research conducted under similar conditions has demonstrated a low correlation between time headway and the remaining four measures examined here, with R^2 values ranging from 0.03 to 0.20 (Kiefer, et al., 1999).

Subjective results

Alert noticeability. One hundred seventy-eight of the 180 (99 percent) drivers reported noticing the auditory crash alert after the surprise trial event. Visual crash alert noticeablility rates across all 15 conditions in which an FCW alert was presented (i.e., all conditions except C2 and C16) are shown in table 3. Visual alert noticeability rates for the HHDD flashing telltale ranged from 17 percent to 50 percent, and for the HUD flashing telltale ranged from 31 percent to 50 percent. This latter result suggests that a sizable portion of the data gathered in the "flashing HUD only alert" conditions (C13-C15) may be equivalent to a no-alert (baseline) condition for the particular reflected HUD design evaluated. Finally, visual alert noticeability appears to be higher with the digit span and visual-manual distraction tasks involving head-down activity (C1, C3-C5, C10-C14, C17) relative to the corresponding rates observed with the eyes-forward, grammatical reasoning and Tell Me distraction tasks (C6-C9, C15). Visual alert noticeability rates for these tasks involving head-down activity ranged from 21 percent to 38 percent, whereas visual alert noticeability rates for eyes-forward distraction tasks ranged from 29 percent to 50 percent (or from 42 percent to 50 percent if C8 is excluded). Overall, these visual alert noticeability rates provide strong support for the use of nonvisual alert (e.g., auditory and haptic) modalities as part of a multimodality FCW alert approach (Kiefer, et al., 1999). It is also interesting to note that visual alert noticeability rates are in nearly all cases equivalent to (correct) color noticeability rates, and that percent (correct) location noticeability is often lower than these alert and color noticeability rates.

Alert timing ratings. Alert timing ratings across all 15 conditions in which an FCW alert was presented (i.e., all conditions except C2 and C16) are shown in Table 4. Mean alert timing ratings across conditions ranged from 3.3 to 5.0 (on a scale of 1 to 7, with the mid-point 4 = just right), and there were generally very few "extremely early" (defined as a 1 or 2 rating) or "extremely late" (defined as a 6 or 7 rating) ratings. Across conditions, the percent of "extremely early" responses ranged from 0 percent to 17 percent, and the percent of "extremely late" responses ranged from 0 percent to 33 percent (or from 0 percent to 13 percent if C14 is excluded). Overall, these results suggest that drivers perceived the crash alert timing approach adopted in these studies to be appropriate.

Table 3. *Alert Noticeability Rates*

Study	Distractor Task	SV speed/POV Speed/POV Decel. Kinematic Condition	Time of Day	Alert Type	Alert Stages	False Alarms?	Brake lights On?	Percent Visual Alert Noticeability	Percent Color Noticeability	Percent Location Noticeability
1	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	6/25= 24%	5/25= 20%	4/25= 16%
[redacted]										
3	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	2	No	Yes	3/14= 21%	3/14= 21%	3/14= 21%
4	Digit Span Dialing	30 / 30 / 0.39 constant	Night	CAMP	1	No	Yes	5/16= 31%	5/16= 31%	4/16= 25%
5	Digit Span Dialing	30 / 0 / stationary	Day	CAMP	1	No	Yes	2/6= 33%	2/6= 33%	0/6= 0%
6	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	7/17= 42%	7/17= 42%	2/17= 12%
7	"Tell Me"	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	7/17= 42%	6/17= 35%	6/17= 35%
8	Grammatical Reasoning	30 / 30 / 0.39→0.15 non-constant	Day	CAMP	1	No	Yes	2/7= 29%	2/7= 29%	1/7= 14%
9	Grammatical Reasoning	30 / 30 / 0.39→0.15 non-constant	Day	CAMP	1	No	No	4/8= 50%	3/8= 38%	2/8= 25%
10	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	1	Yes (5)	Yes	2/12= 17%	2/12= 17%	2/12= 17%
11	Digit Span Dialing	30 / 30 / 0.39 constant	Day	Friendly	1	No	Yes	3/13= 23%	3/13= 23%	0/13= 0%
12	Digit Span Dialing	30 / 30 / 0.39 constant	Day	Friendly	1	Yes (1)	Yes	5/22= 23%	5/22= 23%	4/22= 18%
13	Digit Span Dialing	30 / 30 / 0.39 constant	Day	HUD	1	Yes (1)	Yes	5/16= 31%	5/16= 31%	5/16= 31%
14	Digit Span Dialing	30 / 30 / 0.39 constant	Night	HUD	1	Yes (1)	Yes	3/8= 38%	3/8= 38%	3/8= 38%
[redacted]										
16	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	No alert	No Alert	No	Yes	n.a.	n.a.	n.a.
17	Visual-spatial	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	6/23= 26%	6/23= 26%	2/23= 9%

<u>Note</u>: For Alert Type column, CAMP=CAMP sound/HHDD visual, Friendly= Friendly sound/HHDD visual, and HUD=flashing HUD only.

Table 4. *Alert Timing Ratings*

Study	Distractor Task	SV speed/POV Speed/POV Decel. Kinematic Condition	Time of Day	Alert Type	Alert Stages	False Alarms?	Brake lights On?	Mean Alert Timing Ratings	Percent "Extremely Early" (1 or 2 rating)	Percent "Extremely Late" (6 or 7 rating)
1	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	4.0	3/25= 12%	1/25= 4%
[redacted]										
3	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	2	No	Yes	4.0	1/14= 7%	0/14= 0%
4	Digit Span Dialing	30 / 30 / 0.39 constant	Night	CAMP	1	No	Yes	4.1	0/16= 0%	1/16= 6%
5	Digit Span Dialing	30 / 0 / stationary	Day	CAMP	1	No	Yes	3.8	1/17= 6%	0/17= 0%
6	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	3.3	1/6= 17%	0/6= 0%
7	"Tell Me"	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	4.2	0/17= 0%	1/17= 6%
8	Grammatical Reasoning	30 / 30 / 0.15→0.39 non-constant	Day	CAMP	1	No	Yes	4.4	0/7= 0%	0/7= 0%
9	Grammatical Reasoning	30 / 30 / 0.15→0.39 non-constant	Day	CAMP	1	No	No	4.4	1/8= 13%	1/8= 13%
10	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	1	Yes (5)	Yes	4.4	1/12= 8%	0/12= 0%
11	Digit Span Dialing	30 / 30 / 0.39 constant	Day	Friendly	1	No	Yes	4.2	0/13= 0%	1/13= 7%
12	Digit Span Dialing	30 / 30 / 0.39 constant	Day	Friendly	1	Yes (1)	Yes	4.1	1/22= 5%	1/22= 5%
13	Digit Span Dialing	30 / 30 / 0.39 constant	Day	HUD	1	Yes (1)	Yes	4.2	0/5= 0%	0/7= 0%
14	Digit Span Dialing	30 / 30 / 0.39 constant	Night	HUD	1	Yes (1)	Yes	5.0	0/3= 0%	1/3= 33%
15	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	HUD	1	Yes (1)	Yes	4.3	0/8= 0%	0/8= 0%
[redacted]										
17	Visual-spatial	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	4.1	1/21= 5%	0/21= 0%

<u>Note</u>: For Alert Type column, CAMP=CAMP sound/HHDD visual, Friendly= Friendly sound/HHDD visual, and HUD=flashing HUD only. Shaded rows denote distraction tasks involving head-down looking activity.

Driver performance results

It should be stressed that the primary measure of interest in the following analysis was test driver assists (or interventions), which provides a relatively gross, discrete measure of driver performance. In contrast, results from the nonintervention data were considered potentially useful for more detailed effects of driver performance.

Test driver assists (or interventions). Test driver intervention rates for each of the 17 conditions are shown in table 5. Test driver intervention rates were higher under no-FCW alert (baseline) conditions relative to FCW alert conditions, demonstrating the potential benefit of FCW alerts. For baseline trials with no FCW alert (i.e., conditions C2 and C16), the test driver intervened in 13.0 percent (5 of 38) of the trials conducted, and all interventions occurred with the digit span dialing task (in condition C2) where the test driver intervened in 23 percent (5 of 22) of the trials conducted.

For trials in which an FCW alert was presented (i.e., all conditions except C2 and C16), the test driver intervened in 6.8 percent (15 of 222) of the trials conducted, providing evidence for the robustness of the CAMP FCW alert timing approach. This intervention rate observed with an FCW alert can be compared to the 3.7 percent (4 of 108) test driver intervention rate observed by Kiefer, et al. (1999) under similar FCW alert conditions. This comparison suggests the driver tasks employed here (particularly the digit span dialing task) were more distracting. Once again, all interventions during FCW alert trials occurred with the tasks involving head-down activity (i.e., digit span dialing and visual-spatial), where the test driver intervened in 9.5 percent (15 of 157) of the conducted trials.

Furthermore, all test driver interventions during FCW trials occurred under conditions where the driver was looking at the phone while their foot was on the accelerator when the crash alert was presented. This effect was examined in more detail via a frame-by-frame video analysis of intervention and nonintervention data obtained from a subset of conditions (C1, C3, C4, C10, C11, and C12) employing the digit span dialing distraction task and a dual-modality crash alert. In this sample of eight test driver interventions, the average time between when the alert was presented and when the eyes “landed” (or fixated) on the forward view (referred to as the “alert onset-look up” delay) was 1,505 milliseconds (ms), and ranged between 933 and 1,933 ms. For corresponding nonintervention trials (where the drivers were looking at the phone while their foot was on the accelerator when the crash alert was presented) the corresponding average alert onset-look up delay time was 566 ms (including intervention data raised this average to 685 ms). Hence, it appears that the underlying cause for interventions during FCW alert trials was due to long alert onset-look up delays for some drivers. Since drivers experiencing interventions were not highly trained or experienced with the FCW alert capabilities on the vehicle other than a few that received incidental training through false alarm exposure, further training or experience with the FCW alert may reduce the alert onset-look up delay and thereby increase FCW alert effectiveness.

Table 5. ***Test Driver Intervention Rates***

Study	Distractor Task	SV speed/POV Speed/POV Decel. Kinematic Condition	Time of Day	Alert Type	Number of Alert Stages	False Alarms Provided?	POV Brakelights On?	Percent Interventions
1	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	2/25 = 8%
2	Digit Span Dialing	30 / 30 / 0.39 constant	Day	No Alert	No Alert	No Alert	Yes	5/22= 23%
3	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	2	No	Yes	1/15= 7%
4	Digit Span Dialing	30 / 30 / 0.39 constant	Night	CAMP	1	No	Yes	1/16= 6%
5	Digit Span Dialing	30 / 0 / stationary	Day	CAMP	1	No	Yes	0/6= 0%
6	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	0/17= 0%
7	"Tell Me"	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	0/17= 0%
8	Grammatical Reasoning	30 / 30 / 0.15→0.39 non-constant	Day	CAMP	1	No	Yes	0/7= 0%
9	Grammatical Reasoning	30 / 30 / 0.15→0.39 non-constant	Day	CAMP	1	No	No	0/8= 0%
10	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	1	Yes (5)	Yes	1/12= 8%
11	Digit Span Dialing	30 / 30 / 0.39 constant	Day	Friendly	1	No	Yes	2/13= 15%
12	Digit Span Dialing	30 / 30 / 0.39 constant	Day	Friendly	1	Yes (1)	Yes	3/22= 14%
13	Digit Span Dialing	30 / 30 / 0.39 constant	Day	HUD	1	Yes (1)	Yes	1/16= 6%
14	Digit Span Dialing	30 / 30 / 0.39 constant	Night	HUD	1	Yes (1)	Yes	2/8= 25%
15	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	HUD	1	Yes (1)	Yes	0/16= 0%
16	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	No alert	No Alert	No	Yes	0/16= 0%
17	Visual-spatial	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	2/24= 8%

Note: For Alert Type column, CAMP=CAMP sound/HHDD visual, Friendly= Friendly sound/HHDD visual, and HUD=flashing HUD only. Shaded rows denote distraction tasks involving head-down looking activity.

Generally, this pattern of results provides support for the utility of sensing head-down eye movement activity in order to improve the timeliness of the FCW alert. For example, the driver brake reaction time assumption associated with an FCW alert timing approach could be increased (e.g., by 685 ms) if the driver was detected looking down. Conversely, if the driver was detected with their eyes looking forward, perhaps the driver brake RT could be reduced, which could substantially reduce the number of FCW false alarms.

Effects of age and gender on the number of test driver assists. The age by gender breakdown for test driver intervention rates during no-FCW alert (baseline) and FCW alert trials are shown in table 6 and table 7, respectively. These limited results suggest that under no-FCW alert conditions, older drivers experienced higher intervention rates than younger or middle-aged drivers, and that female drivers experienced higher intervention rates than male drivers. However, these trends disappear under FCW alert conditions, where the FCW alert may serve to equalize drivers' abilities to effectively respond to rear-end crash scenarios.

Table 6. ***Test Driver Intervention Rates During Baseline (no-FCW alert) Trials as a Function of Age and Gender***

	20 to 30 yrs.	40 to 50 yrs.	60 to 70 yrs.	*OVERALL*
Female	1/8= **12.5%**	0/2= **0%**	3/9= **33.3%**	4/19= **21.1%**
Male	0/8= **0%**	0/4= **0%**	1/7= **14.3%**	1/19= **5.3%**
OVERALL	1/16= **6.3%**	0/6= **0%**	4/16=**25.0%**	5/38= **13.2%**

Table 7. ***Test Driver Intervention Rates During FCW Alert Trials as a Function of Age and Gender***

	20 to 30 yrs.	**40 to 50 yrs.**	**60 to 70 yrs.**	***OVERALL***
Female	2/19= **10.5%**	0/35= **0%**	3/55= **5.5%**	5/109= **4.6%**
Male	2/20= **10.0%**	4/43= **9.3%**	4/50= **8.0%**	10/113= **8.8%**
OVERALL	4/39= **10.3 %**	4/78= **5.1%**	7/105= **6.7%**	15/222= **6.8%**

Effects of FCW alert presence. The effects of FCW alert presence were explored by individually comparing four different FCW alert conditions (C1, C6, C13, and C15) to the corresponding (or matched) baseline, no-FCW alert condition employing the same distraction task (C2 for the digit span dialing task, and C16 for the grammatical reasoning task).

In the first comparison, the effect of alert presence was examined under conditions that employed the digit span dialing distraction task and the CAMP sound/flashing HHDD alert approach. The baseline (no-FCW alert) condition C2 and the FCW alert condition C1 were compared in this analysis. For these conditions, as noted above, 23 percent and 8 percent intervention rates were observed in the baseline and FCW alert conditions, respectively. For the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (20-30, 40-50, or 60-70 years old), gender, and alert presence (yes or no). Results indicated main effects of alert presence for each measure except time headway. With the FCW alert relative to no-FCW alert condition, brake RTs were faster (0.90 versus 1.15 sec), TTC values were higher (2.72 versus 2.38 seconds), required decelerations were lower (0.31 versus 0.37 g's), and peak decelerations were lower (0.53 versus 0.63 g's). (Note that substituting a 2-second brake RT value for intervention trials would have increased the observed 250 ms benefits in brake RT to 360 ms.) In addition, an Age x Gender x Alert Presence interaction was observed for both the TTC and required deceleration measures. The three-way interaction found with these two measures indicate positive benefits of alert presence for all age by gender groups except for the 20-30 female group. Overall, these results provide clear support for the benefits of FCW alert presence under conditions involving the digit span dialing distraction task and the CAMP sound/flashing HHDD alert approach.

In the second comparison, the effect of alert presence was examined under conditions that employed the grammatical reasoning distraction task, the CAMP sound/flashing HHDD alert approach, and older drivers. The baseline (no-FCW alert) condition C16 and FCW alert condition C6 were compared in this analysis. There were no interventions in these conditions. For the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of gender and alert presence (yes or no). There was a main effect of alert stages on TTC values, under which TTC values were lower in the FCW alert versus baseline condition (2.46 versus 3.09 seconds). Although this pattern of results suggest a potential cost of the FCW alert under these conditions (eyes-forward grammatical reasoning distraction task, the CAMP sound/flashing HHDD alert approach, and older drivers), it should be noted that intervention rates remained unaffected, as well as the brake RT, required deceleration, and peak deceleration measures. In addition, it should be noted that drivers in these conditions received no training with the FCW alert (i.e., no false alarms were issued under these conditions). Training may eliminate or mitigate any potential costs of the FCW alert with an eyes-forward task. This issue will be discussed further in the fourth comparison below.

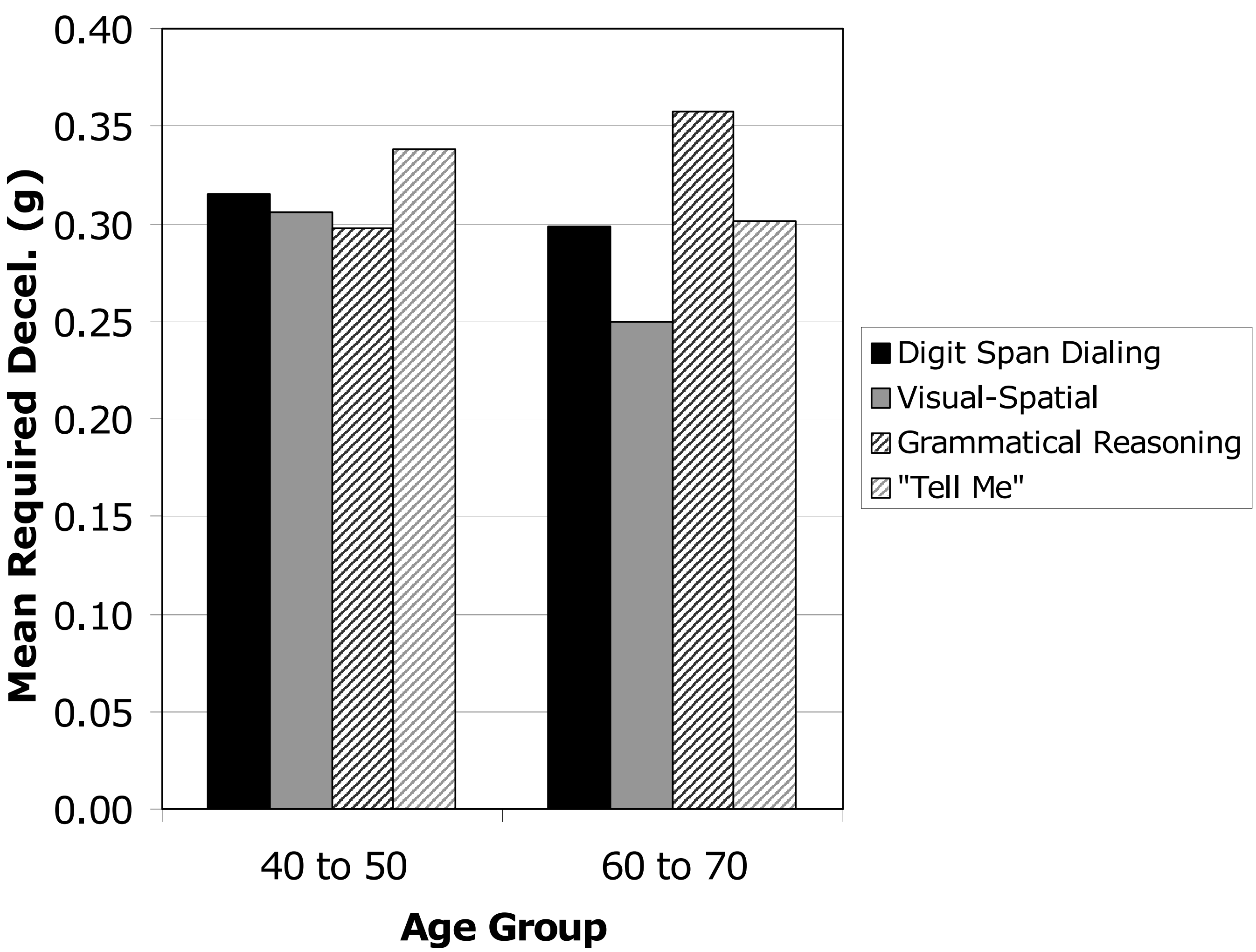
In the third comparison, the effect of alert presence was examined under conditions that employed the digit span dialing distraction task and the flashing HUD only alert approach. The baseline (no-FCW alert) condition C2 and the FCW alert condition C13 were compared in this analysis. For these conditions, as noted above, there were 23 percent and 6 percent intervention rates in the baseline and FCW alert conditions, respectively. However, the flashing HUD visual alert noticeability in condition C13 of only 31 percent suggests that this condition may be more akin to a baseline (no alert) rather than an FCW alert condition, or alternatively, that the flashing HUD may have automatically attracted the driver to attend to the scene ahead without their awareness. In any case, for the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (20-30 or 60-70 years old), gender, and alert presence (yes or no). Results indicated an Age x Gender x Alert Presence interaction for the peak deceleration measure, under which peak decelerations were markedly lower for the female 60-70 group in the FCW alert condition than in the baseline condition (0. 59 versus 0.90 g's). Although these nonintervention results do not suggest a robust FCW alert benefit under these conditions, the intervention results do provide tentative support for the benefits of a flashing HUD only approach in these digit span dialing distraction task conditions. However, the underlying mechanism for this benefit remains unclear.

In the fourth comparison, the effect of alert presence was examined under conditions that employed the grammatical reasoning distraction task and the flashing HUD only alert approach. The baseline (no-FCW alert) condition C16 and the FCW alert condition C15 were compared in this analysis. There were no interventions in these conditions. However, flashing HUD visual alert noticeability in condition C15 was only 50 percent, once again suggesting this condition cannot be strictly considered an FCW alert condition, or alternatively, that the flashing HUD may have automatically attracted the driver to attend to the scene ahead without their awareness. In any case, for the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (20-30 or 60-70 years old), gender, and alert presence (yes or no). No significant effects were observed. Hence, these results, along with the intervention data, do not provide evidence for the benefits of FCW alert presence under conditions involving the grammatical reasoning distraction task and the flashing HUD only alert approach. However, these results are also not consistent with the potential cost of an FCW alert discussed in the second comparison above, in which the same grammatical reasoning distraction task was used with a different alert approach (CAMP sound/flashing HHDD). Rather, these findings lend support to the argument that the observed cost in the second comparison may have been due to lack of experience/training with the FCW alert, since no cost was observed here under conditions with a flashing HUD in which the driver received a single false alarm.

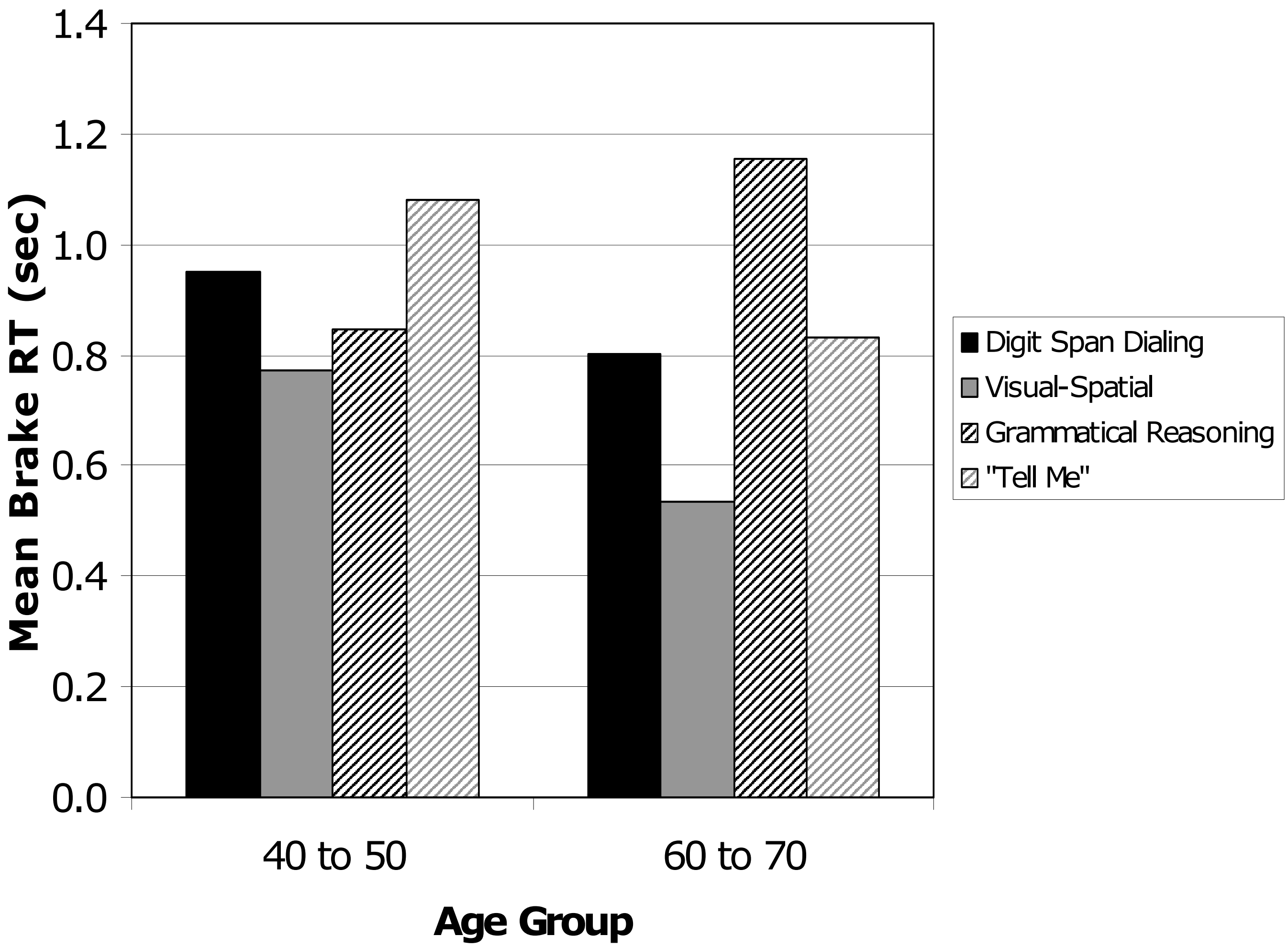
Overall, these results support the benefits of an FCW alert with the digit span dialing task involving head-down activity, however, these benefits were not evident with the eyes-forward, grammatical reasoning task. Results from the flashing HUD only alert condition are considerably less straightforward to interpret (including the FCW alert benefit observed with the digit span dialing task), given the low percentage of drivers reporting the presence of this alert coupled with the possibility that the driver's attention may have been automatically attracted to the forward scene (without their awareness). In any case, these flashing HUD only approach results do not suggest that this approach interfered with the driver's ability to respond to a rear-end crash scenario. Finally, these results suggest that the single nonintervention effect suggesting a cost for the FCW alert with the eyes-forward, grammatical reasoning task may be due to drivers' lack of any previous experience with the FCW visual alert. Nonetheless, note that test driver interventions were not observed with the grammatical reasoning task.

Effects of distraction task. The effects of distraction task were examined separately under no-FCW alert (baseline) and FCW alert conditions. For the no FCW (baseline) alert analysis, conditions C2 and C16 were compared. These conditions differed only in the distraction task variable (digit span dialing versus grammatical reasoning task). For these conditions, 25 percent and 0 percent intervention rates were observed with the digit span dialing task and the grammatical reasoning task, respectively. For the nonintervention data under these no-FCW alert conditions, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (20-30 or 60-70 years old), gender, and distraction task (digit span dialing or grammatical reasoning). There was a main effect of task for required deceleration and peak deceleration, as well as a Distraction x Age x Gender interaction for each of these measures. The pattern of results for the required deceleration and peak deceleration were similar, as shown in figure 8. Higher required decelerations were observed with the digit span dialing relative to the grammatical reasoning task for the 20-30 male and 60-70 female groups.

Figure 8. ***Required Deceleration at Brake Onset as a Function of Distraction Task and Age***



<u>Figure 9</u>. *Brake RT as a Function of Distraction Task and Age*



To analyze the effects of distraction task in the presence of an FCW alert, conditions C1, C6, C7, and C17 were compared. These conditions differed only in terms of the distraction task variable. Results indicated interventions only occurred with the digit span dialing and visual-spatial tasks, with an 8 percent intervention rate for both these tasks. For the nonintervention data under these FCW alert conditions, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (40-50 or 60-70 years old), gender, and distraction task (digit span dialing, grammatical reasoning, Tell Me, or visual-spatial). There was a main effect of task for both the brake RT and TTC measures, under which faster brake RTs and higher TTC values were observed in the visual-spatial task condition. The Age x Distraction Task interaction was also significant for the brake RT measure, which is shown figure 9. This interaction appears to be due to older drivers yielding slower brake RTs compared to those of middle-aged drivers in the grammatical reasoning task condition. These results suggest that the grammatical reasoning task may be particularly well suited as an eyes-forward distraction task for older drivers.

Table 8 provides 85th and 95th percentile brake RTs for nonintervention (i.e., unassisted braking) trials across these four driver distraction tasks, as well as for tasks used in the Kiefer, et al. (1999) surprise trial research conducted under similar conditions. The 85th percentile values remain remarkably stable across the seven tasks compared, ranging between 1.03 and 1.22 seconds. As might be expected, the 95th percentile brake RT values across these tasks tend to vary more widely, ranging from 1.10 to 1.73 seconds.

Table 8*. 85TH and 95th Brake Reaction Time During Successful (Nonintervention) Surprise Braking Trials as a Function of Distraction Tasks Employed in the Current Studies and Kiefer, et al. (1999) Research*

Study	Distractor Task	Kinematic Condition	Time of Day	Alert Sound Type	Number of Alert Stages	False Alarms Provided?	POV Brakelights On?	*85th Percentile "Successful" Brake RT*	*95th Percentile "Successful" Brake RT*	*Highest "Successful" Brake RT*
1	Digit Span Dialing	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	1.12	1.29	1.47
6	Grammatical Reasoning	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	1.13	1.73	1.83
7	"Tell Me"	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	1.17	1.51	1.53
17	Visual-Spatial	30 / 30 / 0.39 constant	Day	CAMP	1	No	Yes	1.03	1.10	1.40
Old CAMP FCW Study 2	Natural Conversation	30 / 30 / 0.39 constant	Day	CAMP	1	No	No	1.22	1.38	2.03
Old CAMP FCW Study 3	Background Question & Answer	30 / 30 / 0.39 constant	Day	CAMP	1	No	No	1.12	1.24	1.45
Old CAMP FCW Study 4	Phantom Head-Down Telltale	30 / 30 / 0.39 constant	Day	CAMP	1	No	No	1.19	1.52	1.69

Note: For Alert Type column, CAMP=CAMP sound/HHDD visual, Friendly= Friendly sound/HHDD visual, and HUD=flashing HUD only.

Overall, these results indicate that the type of distraction activity may have a relatively minor effect when an FCW alert is presented, and hence, the FCW alert may serve to neutralize the effect of distraction task during rear-end crash scenarios. In addition, consistent with the driver intervention rate data discussed earlier, the pattern of nonintervention results from baseline (no-FCW alert trials) suggest that the digit span dialing task (which involves head-down activity) is more problematic for the driver than the eyes-forward grammatical reasoning task during rear-end crash scenarios.

Effects of number of alert stages. For this analysis, conditions C1 and C3 were compared. These conditions differed only in terms of the number of alert stages. A HHDD visual alert was employed in each of these conditions. Nearly identical driver intervention rates were observed across the visual alert stage levels (6 percent and 7 percent rates for 1- and 2-stage alerts, respectively). For the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (40-50 or 60-70 years old), gender, and visual alert stages (1 or 2). There was a main effect of alert stages, under which peak decelerations were lower in the 1-stage relative to 2-stage condition (0.52 versus 0.57 g's). Results also indicated a marginal ($p<0.10$) main effect of alert stages under which brake RTs were faster in the 1-stage relative to 2-stage condition (1.08 versus 0.87 sec). Hence, given that the FCW visual alert was provided via a HHDD, these results provide support for a 1-stage over a 2-stage visual alert for situations in which the driver is closing in on the vehicle ahead.

Effects of alert sound. For this analysis, conditions C1 and C11 were compared. These conditions differed only in terms of the FCW alert sound. No false alarms (or training) were provided in either of these conditions. Lower intervention rates were observed in the CAMP sound relative to the friendly alert sound condition (6% versus 15%). For the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (40-50 or 60-70 years old), gender, and alert sound (CAMP or friendly). Results indicated main effects of alert sound for each measure except time headway. With the CAMP relative to the friendly alert sound, brake RTs were faster (0.87 versus 1.33 sec), TTC values were higher (2.76 versus 2.24 sec), required decelerations were lower (0.31 versus 0.40 g's), and peak decelerations were lower (0.52 versus 0.60 g's).

Several higher-order interactions were also observed. A Gender x Alert Sound interaction was observed for both the required deceleration and time headway measures. For the former measure, this interaction indicated that the effect of lower required deceleration in the CAMP sound condition was more pronounced for males. For the time headway measure, the interaction indicated that time headways were noticeably shorter in the males in the friendly alert sound condition relative to the other three gender by alert sound combinations. In addition, an Age x Gender x Alert Sound interaction was observed for the peak deceleration measure, which indicated that the effect of lower peak decelerations found in the CAMP alert sound condition was restricted to middle-aged males.

Given the driver has no experience with an FCW alert, which may be somewhat analogous to a driver who has not experienced an FCW alert for a prolonged period of time or an FCW alert system which rarely produces false alarms, these results provide support for the CAMP alert sound over the friendly alert sound. For an FCW system that produces false alarms, which is currently the more realistic, practical assumption, the CAMP alert sound could be considered by drivers to be overly annoying. The effects of FCW alert experience/training, and how this may benefit a "friendlier" (less urgent sounding) alert sound are addressed in the next section.

Effects of false alarms/training. The effects of false alarms/training were explored in two different comparisons, both of which involved the digit span dialing distraction task. In the first comparison, the effectiveness of a dual-modality FCW crash alert (CAMP sound/flashing HHDD) was compared under conditions with zero and five false alarms (conditions C1 and C10, respectively). Nearly identical driver intervention rates were observed across these two conditions (6 percent and 8 percent rates for the zero and five false-alarm conditions, respectively). For the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (40-50 or 60-70 years old), gender, and false alarms (zero or five). There was a main effect of false alarms under which brake RTs were faster in the zero relative to five false-alarm condition (0.87 versus 1.08 sec). In addition, results indicated a Gender x False Alarms interaction for peak deceleration. Under this interaction, lower peak decelerations were observed for males in the zero relative to five false-alarm condition (0.52 versus 0.64 g's). Hence, under those conditions that employed the CAMP sound/flashing HHDD approach, a relatively high false-alarm rate (or large amount of negative training on the alert) slowed driver brake RT and for males, increased peak decelerations.

In the second comparison examining false alarm/training effects, the effectiveness of a dual-modality FCW crash alert (friendly sound/flashing HHDD) was compared under conditions with zero false alarms and one false alarm (conditions C11 and C12, respectively). Lower intervention rates were observed in the one versus zero false alarm condition (6% versus 15%). For the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (40-50 or 60-70 years old), gender, and false alarms (zero or one). There was a main effect of false alarms on TTC, required deceleration, and peak deceleration. Under the one false alarm relative to the no false alarm condition, TTC values were higher (2.76 versus 2.24), required decelerations were lower (0.34 versus 0.40 g's), and peak decelerations were lower (0.52 versus 0.60 g's). There was also a marginal ($p<0.10$) main effect of false alarms on brake RTs under which brake RTs were faster in the one relative to zero false alarm condition (1.00 versus 1.33 sec). In addition, results indicated a Gender x False Alarms interaction for the TTC and required deceleration measures, which indicated that the effects of a single false alarm (or training) lengthening TTC and lowering required deceleration values was restricted to males.

In order to further explore the positive effects of a single false alarm exposure on driver behavior under these conditions, an additional analysis was conducted in which the effectiveness of a friendly sound/flashing HHDD alert approach with one false alarm exposure was compared to the effectiveness of a CAMP sound/flashing HHDD alert approach with no false alarms (and hence, no training). These two conditions correspond to conditions C1 and C12, respectively. Results indicated lower intervention rates in the CAMP sound/flashing HHDD alert approach with no false alarms condition than in the friendly sound/flashing HHDD alert approach with one false alarm exposure (8% versus 14%). For the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (20-30, 40-50 or 60-70 years old), gender, and alert approach/false alarm exposure (friendly sound/flashing HHDD alert with one false alarm, CAMP sound/flashing HHDD with no false alarms). There was also a main effect of alert approach/false alarm exposure on brake RT and required deceleration, and a three-way Age x Gender x Alert Approach/False Alarm Exposure interaction. These results indicated that the benefits found in the CAMP sound/flashing HHDD with no false alarm condition of lowering brake RT and lowering required decelerations were restricted to the 40-50 female and 20-30 male groups. Overall, these results favor the CAMP sound/flashing HHDD with no false alarms condition over the friendly sound/flashing HHDD alert with one false alarm,

Overall, these results indicate that under these surprise trial conditions, the presence of false alarms can either positively or negatively impact driver performance depending on false alarm behavior. A single false alarm had an impact on driver performance in a positive manner, and may have essentially served as (relatively effective) FCW alert training under these conditions. In contrast, repeated false alarms over a relatively short period of time (without any "valid" alerts) had a negative impact on driver performance. Since it is difficult at best to relate the rate (and nature) of false alarms examined in this experiment to an FCW system operating under real world driving conditions over a much longer period of time, it is unclear the extent to which these false alarm effects (including the gender effects reported above) would generalize to real world driving conditions. In any case, these results do underscore the important effects false alarms may have on driver behavior with an FCW system, which are best understood under in-traffic, field operational test conditions.

Effects of time of day. The effects of time of day were explored in two different comparisons, both of which involved the digit span dialing distraction task. In the first comparison, the effectiveness of a dual-modality FCW crash alert was compared under daytime versus nighttime conditions (conditions C1 and C4, respectively). There was a 6 percent intervention rate in each of these comparison conditions. For the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (40-50 or 60-70 years old), gender, and time of day (day or night). There was a main effect of time of day on the brake RT, TTC, and required deceleration measures, and an Age x Time of Day interaction for the brake RT and required deceleration measures. These interactions indicated that the time of day effects were restricted to older drivers, who under nighttime (relative to daytime) conditions exhibited slower brake RTs (1.35 versus 0.80 sec) and higher required decelerations (0.39 versus 0.30 g's).

In the second comparison examining time of day effects, the effectiveness of a flashing HUD only approach was compared under daytime versus nighttime conditions (conditions C13 and C14, respectively) with older drivers only. Higher intervention rates were observed in the nighttime relative to daytime conditions (25% versus 12%). However, flashing HUD visual alert noticeability in conditions C13 and C14 were 25 percent and 38 percent, respectively, suggesting this condition may be more akin to a baseline (no alert) rather than an FCW alert condition. Indeed, the relatively high level of interventions in these conditions provides support for this suggestion.

Overall, these results suggest that nighttime conditions were more problematic than daytime conditions for drivers performing the digit span dialing task, which involved head-down activity. It remains unclear the extent to which this effect would generalize to an eyes-forward task. Further analysis suggested that the nighttime effect observed above for older drivers was not due to a time of day effect, but instead due to differences in the probability with which drivers were observed to be looking at the cell phone at the time of the FCW alert onset. For middle-aged drivers, the percent of drivers who were looking at the phone at the time of alert onset was 57 percent and 60 percent in daytime (condition C1) and nighttime (condition C4) conditions, respectively. For older-aged drivers, the corresponding percentages were 36 percent and 83 percent, respectively. This pattern of "caught looking down" results is entirely consistent with results from the first time of day comparison discussed above in which nighttime effects were restricted to older drivers, and suggests that these effects are more consistent with a look-down behavior rather than nighttime effect interpretation. The possibility that older drivers may be more willing to look down under nighttime conditions is at least a plausible (though not particularly compelling) interpretation of this data. In any case, it appears an FCW system that modifies alert timing based on sensing driver eye movement behavior, as discussed earlier, may largely (if not fully) address the observed nighttime effects for older drivers.

Effects of brakelight presence. For this analysis, conditions C8 and C9 were employed, which differed only in terms of the lead vehicle brakelight presence. (Note these conditions used the eyes-forward grammatical reasoning distraction task along with non-constant lead vehicle deceleration).

There were no interventions in these comparison conditions. For the nonintervention data, a separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (40-50 or 60-70 years old), gender, and brakelight presence (on or off). There was a main effect of brakelight presence on brake RT, under which brake RTs were faster in the brakelights-on relative to the brakelights-off condition (0.55 versus 0.90 sec). These results support activating brakelights in surprise trial research (which was true here for all but condition C9), in order to provide a fair test of FCW alert effectiveness.

Effects of lead vehicle behavior. The effects of lead vehicle behavior were explored in two different comparisons. In the first comparison, results from the 30 mph/0.39 constant deceleration condition were compared to those found under the 30 mph/0.15→0.39 non-constant deceleration condition (conditions C6 and C8, respectively). (Note these conditions used the eyes-forward, grammatical reasoning distraction task.) There were no interventions in these comparison conditions. A separate ANOVA was performed for each of the five dependent measures using the between-subjects variables of age (40-50 or 60-70 years old), gender, and POV deceleration constancy (constant or non-constant). There was a main effect of POV deceleration constancy on brake RT, under which brake RTs were faster in the POV non-constant (relative to the constant) deceleration condition (0.55 versus 0.90 sec).

In the second comparison examining lead vehicle behavior, results from a 30 mph/0.39 constant deceleration condition were compared to those found under the 30/0 condition where the lead vehicle was parked (conditions C1 and C5, respectively). (Note these conditions used the digit span dialing distraction task, which involved head-down activity). The 30/0 surprise trial proved to be logistically very difficult to choreograph. Ten of the 16 trials conducted were eliminated because of short reaction times suggesting drivers were not responding to the alert, and consequently, an ANOVA was performed on these limited data. However, results indicated higher intervention rates in the 30/0.39 relative to the 30/0 condition (6% versus 0%), and the pattern of nonintervention data also suggested the 30/0.39 g condition was more problematic.

Overall, these results (albeit tentative for the 30/0 lead vehicle stationary condition) provide support for the robustness of the CAMP FCW alert timing approach for these two different lead vehicle behavior profiles. In addition, a method for executing surprise trials under lead vehicle stationary conditions was demonstrated to feasible, although it proved to be logistically difficult to choreograph and relatively inefficient.

General Discussion

The primary goal of this work was to assess the impact of a wide range of factors on the effectiveness/robustness of the CAMP FCW alert timing approach. Both objective and subjective results provide support for the effectiveness/robustness of this approach. In terms of objective data, 0 percent-15 percent test driver intervention rates (and hence, high, unassisted successful braking rates) were observed across the 12 dual-modality FCW alert conditions examined. Overall, across all FCW alert modality conditions examined, the surprise trial intervention rate was 6.8 percent. This intervention rate can be compared to the 13.2 percent intervention rate observed under baseline (no-FCW alert) trials, which provides support for the overall utility of FCW alerts. Furthermore, the higher intervention rates observed for female drivers and older drivers under no-FCW alert conditions were not found under

FCW alert conditions, where intervention rates remained uniformly low across the age (20-30, 40-50, and 60-70 years old) and gender combinations examined. This suggests that FCW alert information may be an effective means of equalizing (or neutralizing) drivers in their ability to avoid rear-end crashes. Subjective data indicated that drivers perceived the crash alert timing approach adopted to be appropriate.

The potential benefit of FCW alerts (in terms of lower test driver intervention rates relative to a matched baseline, comparison condition) was demonstrated for the digit span dialing distraction task that involves head-down activity, but was not evident for the eyes-forward, grammatical reasoning distraction task. The digit span dialing task required the driver to memorize and dial an unfamiliar string of numbers (the length of this string was based on the individual's memory span) on a center counsel-mounted cellular phone. Hence, this task required the driver to alternate glances to the in-cab phone and the outside forward scene, whereas the grammatical reasoning task allowed the driver to maintain a forward glance direction. Further examination of these digit span dialing data indicated that test driver interventions appear to be strongly linked to cases where the driver was looking down at the phone at the time of the alert, and there was a long delay between FCW alert onset and the driver looking toward the forward scene. Further training or experience with the FCW alert relative to what drivers experienced here may serve to reduce this alert onset-look up delay, and thereby increase FCW alert effectiveness provided false alarms do not undermine the "credibility" of the FCW alert system.

More generally, all test driver interventions during FCW alert trials occurred for distraction tasks involving head-down glance activity (i.e., the digit span dialing and visual-spatial tasks), and conversely, no interventions occurred for the eyes-forward distraction tasks employed (i.e., the grammatical reasoning and "Tell Me" tasks). Furthermore, test driver interventions only occurred when the driver was looking down at the phone at the time the FCW alert was issue. Look-down behavior at the time of alert is also believed to explain the decreased performance for older drivers under nighttime relative to daytime conditions. This pattern of results suggests that knowledge of driver eye movement location (e.g., eyes-down versus eyes-forward) could be particularly useful for modifying FCW alert timing assumptions (e.g., by modifying the driver brake RT assumption). More specifically, if the driver was looking down (or away from the forward scene), the FCW alert range assumption could be increased. Conversely, if the driver was looking toward the forward roadway scene, the alert range could be decreased, which could prove to be a very effective method for reducing the rate of FCW false alarms.

Results also indicated that visual crash alert noticeability was relatively low across FCW alert conditions (50% or less), even in the flashing HUD conditions. In contrast, auditory alert noticeability was near perfect. These results provide strong support for the use of non-visual alert (e.g., auditory and haptic) modalities as part of a multimodality FCW alert approach (Kiefer, et al., 1999). This pattern of low visual alert noticeability also confounds clear interpretation of results from the flashing HUD only alert condition. At a minimum, results from this condition suggest that this approach did not negatively interfere with driver's ability to respond to the rear-end crash scenario. On the contrary, these data provide some very tentative support that the flashing HUD may have benefited drivers under the digit span dialing distraction task conditions, perhaps by automatically driving the driver's attention forward.

Finally, results provided no support for a two-stage over a one-stage alert for warning drivers when they are approaching a vehicle ahead too rapidly (i.e., for a closing alert). In addition, for an FCW alert system that rarely issues false alarms, which is not representative of current production FCW systems, the CAMP alert sound increased driver performance relative to the friendly alert sound evaluated. However, it should be noted that the CAMP sound might be considered by drivers to be unduly annoying under false alarm conditions. On a related note, although both negative and positive effects of false alarms were observed under these experimental conditions, it is extremely difficult to generalize these experimental conditions to the rich and varied nature of drivers' experiences with valid FCW alerts and alerts considered to be a nuisance (or false) under actual, naturalistic driving conditions.

From a methodological perspective, these results suggest that a task involving head-down eye movements, such as the digit span dialing task, provides a more appropriate and stringent test of FCW alert effectiveness. More precisely, these results indicate that examining FCW alert effectiveness when the driver is looking down at the time of the FCW alert onset should be the focus of subsequent experimental work. Furthermore, this work demonstrated that surprise trial work involving a stopped lead vehicle is feasible, albeit logistically difficult to choreograph.

Time-to-Collision (TTC) Judgments

For the rear-end crash scenario, time-to-collision refers to the time it would take for a collision to occur at the prevailing speeds, distances, and trajectories associated with the driver's vehicle and the closest lead vehicle (van der Horst, 1990). Hence, time-to-collision (TTC) provides a measure of crash risk, that is, the time before impact if prevailing conditions continue. TTC can be kinematically defined as the range between a following and lead vehicle divided by the relative velocity (alternatively referred to as delta velocity or $\triangle$V) between these two vehicles (i.e., range/$\triangle$V). Alternatively, TTC can be visually (or perceptually) defined as the angular size of the approaching object divided by its angular speed (Lee, 1976; Summala, Lamble, and Laakso, 1998). Hence, TTC is directly tied to the visual looming or angular expansion of the lead vehicle, which gradually increases in visual angle during an approach before becoming "optically explosive" immediately prior to a collision (Groeger, 2000; Schiff and Detwiller, 1979).

Task 1 work under this project suggests that inverse TTC (i.e., 1/TTC) is a key element of the underlying mental process drivers use in deciding when to brake hard (i.e., when they are in a hard versus normal braking envelope) (Kiefer, et al., 2003). This suggestion is also supported by previous in-traffic research which found inverse TTC to be the most robust measure for describing drivers' judgments about whether they were closing or "opening" distance relative to the lead vehicle under extremely small relative speed/acceleration conditions (Evans and Rothery, 1974). Hence, drivers' perception of the instant they feel that they would have collided with the vehicle ahead, and the relationship between this perceived TTC and actual TTC are of inherent interest.

There were four major goals associated with this experimentation. The first goal was to gather TTC judgment data under actual driving conditions over a wide range of in-lane approach scenarios, and to assess the roles age and gender play in these TTC judgments. There is clearly a strong need for gathering of test track data under realistic rear-end crash scenario conditions. With one notable exception discussed below (Cavallo and Laurent, 1988), all previous TTC judgment studies intended for automotive application have been conducted under simulated approach (laboratory or simulator) rather than real approach situations (Groeger, 2000; van der Horst, 1990). Across these simulated approach studies, there are inconsistencies with respect to the effects of numerous variables, including viewing time of scene prior to the TTC judgment, approach speed, and driving experience (Groeger and Comte, 1999). These inconsistencies may be due to differences in the properties of the visual scenes employed across these studies. Gathering TTC judgment data under real approach conditions provides a means of completely circumventing these artificial visual scene effects.

Relative to real-world scenes, simulated approach scenes have distinctly different visual properties, including reduced peripheral vision, degraded binocular distance cues, and artificial scene texture gradients. Furthermore, both peripheral vision, which plays a role in speed perception, and binocular vision were shown to influence TTC judgments in the only previous TTC judgment study reported under real approach conditions (Cavallo and Laurent, 1988). Hence, the absolute levels and patterns of TTC estimation observed in simulated approach studies may be of questionable utility for crash avoidance timing purposes and for understanding the relationship between TTC estimations and real-world crash avoidance behavior.

The Cavallo and Laurent (1988) test track study involved a front-seat passenger making TTC judgments to a flat, stationary mock-up of a vehicle under approach speeds of 19 and 56 mph (or 30 and 90 km/hr). The current study examined TTC estimation under both lead-vehicle-stationary and lead-vehicle-moving conditions to a realistic visual representation of a vehicle (i.e., the surrogate target shown in figures 1 and 2) under 12 combinations of driver speed and relative velocity.

A second goal of this study was to assess whether a shortened viewing time of the forward scene alters TTC judgments. Shortened viewing times can occur, for example, when a driver is performing a visual-manual in-vehicle task and rapidly alternating glances between the forward scene and the in-vehicle task. Note that for a head-down task, 1 second corresponds loosely to the time available to assess the forward scene after an FCW alert prior to initiating any crash avoidance maneuver response. The adequacy of shortened viewing times for making TTC judgments was assessed by comparing judgments under 1-second glimpse versus normal (continuous) viewing conditions.

A third goal of this experimentation was to explore whether a concurrent distraction task influences TTC judgments. If a concurrent distraction task increases TTC judgments (particularly at the low TTC values examined here), this may in turn increase the probability of TTC overestimation, that is, where perceived TTC exceeds actual TTC. TTC overestimation could play an important underlying role in collision causation. This pattern of results was observed by Groeger and Comte (1999) under laboratory conditions, so it is of considerable importance to examine whether such a pattern would be observed under real approach conditions. If such a pattern of results was observed, it would provide evidence that the impact of a task on concurrent TTC judgments may be an important criterion for deciding the potential consequences of an in-vehicle task on a driver's ability to effectively respond to a rear-end crash scenario.

The fourth and final goal of this research was to provide a methodology and comparison dataset for guiding any subsequent laboratory or simulator calibration/validation work. As argued earlier, simulated scenes have distinctly different visual properties than real-world scenes that may impact TTC judgments, and hence, driver's perception of crash threat conditions. Since TTC appears to play an important role in driver's judging and responding to rear-end crash scenarios, it is important to understand the relationship between perceived and actual TTC under simulated versus real approach conditions.

Method

Participants

The 51 test participants were from three different age groups. The age x gender x distraction task breakdown of the participants is shown in table 9. Each driver was tested individually in a single one-hour testing session and paid $100 for participating. Naive participants were recruited from a database of licensed drivers in the metropolitan area surrounding the test facility via an outside independent market recruiting firm. The recruitment criteria required that participants be licensed drivers who drove regularly and were free of any conditions that may have limited their ability to safely participate in the test.

Subject vehicle, surrogate target lead vehicle, and principal other vehicle

The CAMP surrogate target methodology apparatus (SV, surrogate target, and POV) used in the previous surprise trial study was again employed in this study.

Data acquisition and experimenters

The data acquisition system and the experimenter roles were identical to the approach taken in the previous surprise trial study.

Table 9. ***Subject Sample Breakdown***

	GENDER		
AGE	Female	Male	*Total*
20-30	8 (4,4)*	8 (4,4)	*16*
40-50	9 (4,5)	8 (4,4)	*17*
60-70	10 (6,4)	8 (4,4)	*18*
Total	*27*	*24*	*51*

**First and second numbers in parentheses indicate number of subjects that did and did not perform mental addition task, respectively.*

Visual occlusion device

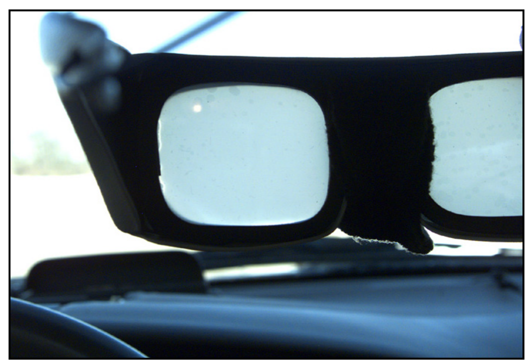
The vehicle was instrumented with a pair of liquid-crystal, occlusion glasses and a finger button switch for recording TTC judgments. (See Milgram [1987] for a description of these occlusion glasses.) The occlusion glasses allowed experimenters to block the driver's view of the forward scene (closed state). The driver's peripheral vision was blocked when wearing the occlusion glasses by the thick, wrap-around arms. The finger button switch was secured around the subjects' index fingers on their right hands using a Velcro strip in a manner that did not interfere with the drivers assuming their normal steering position with their hands.

Experimental procedure

This study was conducted under dry-road, daytime conditions on the closed-course test track shown in figure 1. Upon entering the vehicle, participants were instructed to make themselves comfortable for driving by adjusting the seat, steering wheel, and mirrors as necessary. They were also required to securely fasten their seatbelts. Participants were then told about the general nature of the trials they were about to perform. They were also informed that the surrogate target was designed to allow impacts, and of the add-on steering wheel and brake available to the passenger-side, experimenter test driver. Subjects were also familiarized with the open and closed states of the liquid-crystal spectacles used for visual occlusion purposes (see Figure 10).

Test subjects performed in-lane approaches to the surrogate target ahead under various constant △V conditions that are described below. The SV traveled at a constant speed (i.e., the target speed), and the surrogate target was towed by the POV at a constant speed lower than that of the SV. During an approach, the driver was asked to quickly reach the designated target speed while the spectacles were in the open state (i.e., the driver could see clearly ahead). Once they reached this speed, drivers were instructed to set the cruise control. In the normal viewing condition, after the driver reached the target speed, the spectacles remained in the open state for approximately 10 seconds before changing to the closed state so that the driver could not see ahead. The spectacles closed at either 3.6 or 5.6 seconds time-to-collision. In the one-second glimpse viewing condition, upon reaching the target speed, the spectacles were placed in the closed state until they were opened for a 1-second glimpse beginning at either 4.6 of 6.6 seconds time-to-collision (and hence, the spectacles were placed in the closed state at either 3.6 or 5.6 seconds time-to-collision, respectively). In both viewing conditions, after the spectacles were placed in the closed state for the latter phase of the in-lane approach (at either 3.6 or 5.6 seconds TTC), the driver was instructed to press the TTC finger button at the instant they felt that they would have collided with the vehicle ahead assuming that the speeds of their vehicle and the vehicle ahead did not change. This was the driver's TTC judgment.

Figure 10. ***Open and Closed States of the Occlusion Glasses from the Driver's Perspective***



When the spectacles were placed in the closed state during this TTC judgment decision interval, drivers were instructed to keep their eyes facing forward and to keep their hands on the steering wheel, while the front seat, passenger-side experimenter took over braking and steering control of the vehicle. In addition, during this TTC judgment decision interval the POV driver swerved into the left lane out of the driver's path as quickly as possible in order to allow the SV to maintain the target speed and continue traveling in the driving lane. After the driver passed the lead vehicle (which was brought to a stop after swerving into the left lane) and provided a TTC response, the passenger-side test driver braked the SV gradually to a stop and the trial ended.

Experimental design

The between-subjects independent variables were age (20-30, 40-50, or 60-70 years old), gender, and distraction (none or mental addition task). The within-subjects variables manipulated included driver speed (30, 45, and 60 mph), △V (10, 15, 20, and 30 mph), actual TTC (3.6 and 5.6 sec), and viewing condition (1-second glimpse and normal). Hence, the 30-mph driver speed by 30-mph △V combination involved an approach to a stationary vehicle, whereas the remaining 11 combinations of the driver speed by △V combinations involved approaches to a moving vehicle.

Each subject experienced 48 trials formed by crossing driver speed, △V, actual TTC, and viewing condition variables. Driver speed, △V, and actual TTC were randomized from trial to trial. The viewing condition variable was blocked, such that half of the drivers experienced 24 glimpse viewing condition trials followed by 24 normal viewing condition trials, and the remaining half experienced the viewing conditions in reverse order. Four random orders of trials were employed.

In the mental addition distraction task, once the driver attained the target speed, the experimenter read a string of numbers one at a time and the driver was asked to verbally sum the last pair of numbers presented. An example set of stimuli and responses are shown in table 10. This task required drivers to forget the answer they just gave and recall the previous number, and then add it to the new number. This proved to be a demanding task, and usually participants could only continue for about 5 or 6 addition problems before they made a mistake. If the driver made a mistake, a new pair of numbers were provided, followed by a presentation of numbers one at a time, until the next mistake was made.

Table 10. ***Mental Addition Task Stimulus and Correct Response Sequence***

Stimulus Presented Verbally – Paced by Experimenter	*Response Provided Verbally By Test Subject*
"5"	
"2"	*"7"*
"4"	*"6"*
"5"	*"9"*
"1"	*"6"*
"2"	*"3"*
"9"	*"11"*
"6"	*"15"*

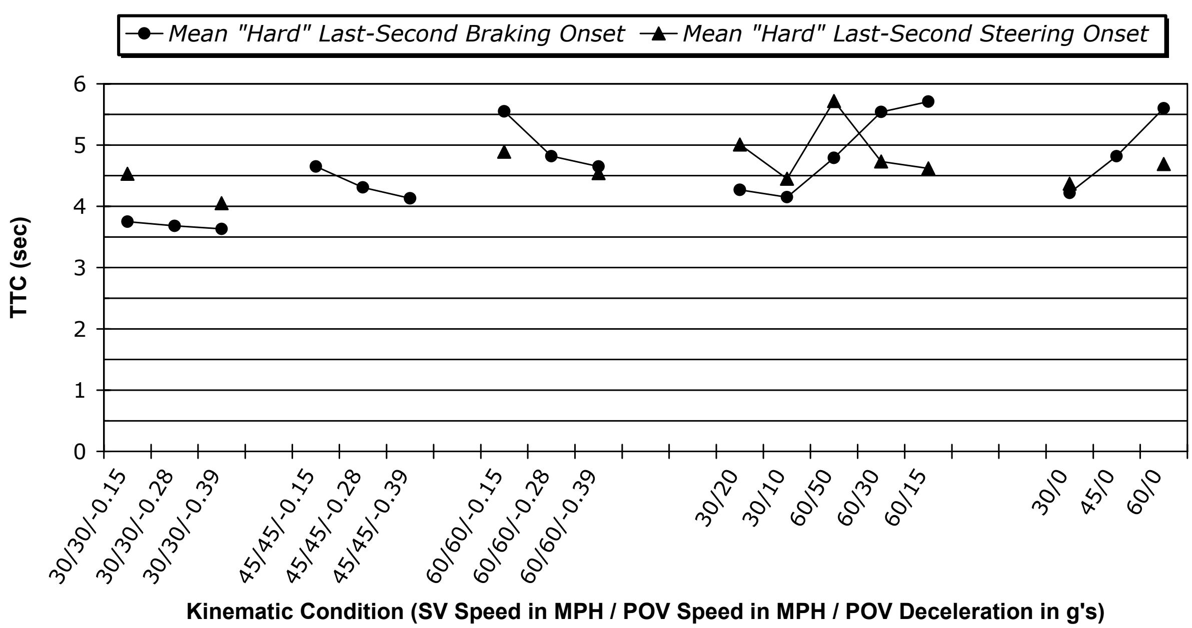
The mental addition distraction task was chosen for a number of important reasons. First, the task could be administered continuously in a rapid-fire fashion during the in-lane approach without lengthy pauses between each stimulus and response, and hence, provided for a relatively steady driver workload by placing ongoing demands on working memory. Second, the experimenter could pace the rate of stimulus presentation to the limit of the subject's abilities, and consequently, the relatively high workload demand of this task could be held relatively constant across drivers. Third, the driver could perform the task while their vision was occluded. Fourth, this task allowed drivers in the glimpse condition to keep their eyes steadily directed to the forward scene ahead in preparation for the brief spectacle opening. Fifth, the mental addition task has been shown to decrease TTC at brake onset when drivers (under test-track conditions) were instructed to brake whenever they detected small decelerations of a lead vehicle (Lamble et al., 1999). However, it should be noted that these results were found under relatively high (> 10 sec) TTC conditions, where TTC judgment accuracy becomes increasingly suspect and the role and importance of TTC estimation may be diminished (McLeod and Ross, 1983; Schiff and Detwiller, 1979).

The 3.6 and 5.6 TTC values were chosen because this range effectively brackets the range of observed TTC values for "idealized" FCW alert timing based on "hard" last-second braking judgments across 17 vehicle-to-vehicle kinematic conditions (Kiefer, et al., 1999; Kiefer, et al., 2003). This idealized timing, shown in figure 11, was created by adding 1.72 seconds to the mean observed last-second braking onset. This 1.72 seconds assumption includes 1.52 seconds for the driver brake RT assumption and 0.20 seconds for the brake delay time assumption (Kiefer, et al., 1999).

Driver performance measures

TTC was defined as the range between the two vehicles divided by the difference in speeds between these two vehicles (i.e., range/$\triangle$V). This measure is sometimes referred to as "momentary TTC". The first dependent variable analyzed was the TTC ratio, defined as perceived TTC divided by actual TTC. The TTC ratio measure is considered a more valid measure than a measure employing the difference between actual and perceived TTC because it eliminates effects due to minor actual TTC variations across trials.

Figure 11. ***Mean TTC at Last-Second Braking Onset for Each Kinematic Condition, Plus the Distance Traveled During a 1.72-Second Assumed Delay (a 0.20 brake system delay combined with a 1.52 driver brake reaction time)***



The second dependent variable analyzed was the number of TTC overestimations, defined as the cases where perceived TTC exceeds the actual TTC value. Previous TTC work indicates that the vast majority of TTC judgments involve TTC underestimations (or TTC ratios which fall below 1). Hence, the infrequent cases of TTC overestimations provides a convenient "break point" for examining extreme TTC judgments which may enhance understanding of rear-end collision causation. It should be emphasized that a TTC overestimation does not imply a collision would have occurred, for a variety of reasons. First, drivers are likely to continue updating TTC judgments throughout an in-lane approach, and hence, may compensate for TTC overestimations throughout the maneuver. Second, in an attempt to explain the consistent pattern of TTC underestimations in this commonly employed "disappearance paradigm," it has been argued that drivers during TTC judgments may be prone to estimating the moment of action rather than the moment of collision (van der Horst, 1990), or that drivers may incorporate a safety margin into these prospective time estimates (i.e., estimating when a particular time interval has elapsed) (Cavallo and Laurent, 1988; Cavallo, Mestre, and Berthelon, 1997). Furthermore, Cavallo et al. (1997) argue that TTC ratios obtained under real-approach conditions have particular importance by providing an ecologically valid reference for interpreting TTC underestimation. For all the reasons discussed above, the trend in the number of TTC overestimations as a function of the variables examined is of more relevance than the absolute level of TTC overestimations observed under these real approach conditions.

Results and Discussion

TTC Ratio

A repeated-measures ANOVA was performed on the TTC ratio measure. The between-subjects variables were age (20-30, 40-50, or 60-70 years old), gender, and distraction (none or mental addition task), and the within-subjects variables were driver speed (30, 45, and 60 mph), △V (10, 15, 20, and 30 mph), actual TTC (3.6 and 5.6 sec), and viewing condition (1-second glimpse and normal). All effects discussed below met at least the $p < 0.05$ criterion level of statistical significance.

As has been seen in previous TTC judgment research, drivers generally underestimated TTC, which resulted in mean TTC ratios falling well below 1. Results indicated a complete absence of age and gender effects, and only a single (three-way interaction) significant effect involving the distraction variable (discussed further below). Results did indicate main effects of driver speed ($F(2, 50) = 27.33$, $p < 0.0001$) and △V ($F(3, 75) = 32.20$, $p < 0.0001$), as well as a Driver Speed x △V interaction ($F(6, 150) = 2.59$, $p < 0.05$). These effects are shown in figure 12, which shows that the TTC ratio (and hence, TTC accuracy) increased as △V increased, decreased as driver speed increased, and that this driver speed effect was not stable for the 45 mph driver speed condition. In addition, △V x Viewing ($F(3, 75) = 3.40$, $p < 0.05$) and Driver Speed x △V x Viewing ($F(6, 150) = 2.84$, $p < 0.05$) interactions were observed. The latter three-way interaction is shown in figure 13, which suggests that the Driver Speed x △V interaction is less stable in the glimpse viewing condition, particularly under the 45 mph speed condition.

Results also indicated an Actual TTC x Viewing interaction ($F(1, 25) = 5.09$, $p < 0.05$). As shown in figure 14, TTC ratios were slightly lower in the glimpse-3.6-second actual TTC condition relative to the other three Viewing x Actual TTC condition combinations. Hence, under limited viewing time, close actual TTC conditions, TTC estimates became slightly more conservative. Results also indicated a Driver Speed x Viewing x Distraction interaction ($F(2, 50) = 3.40$, $p < 0.05$), shown in figure 15. This effect can be attributed to the lower (more conservative) TTC ratios found in the distracted-glimpse condition under 30 mph conditions relative to the other condition combinations at this speed condition. Note that of the conditions examined, this distracted-glimpse combination is most representative of distracted drivers who have lost visual and/or cognitive contact with the lead vehicle ahead, and upon returning their gaze to the forward scene, must make rapid decisions (within 1 second) with respect to the urgency of the crash threat (i.e., make a TTC judgment).

Overall, these TTC ratio results suggest the following about driver's TTC estimates under real approach conditions. First, drivers generally underestimate TTC, with underestimates increasing as actual TTC increases. This pattern of results is consistent with previous TTC judgment research. Second, speed and △V have important and opposite effects on TTC estimates. Increases in driver speed decreased TTC estimates, whereas increases in △V increased TTC estimates. Third, given the general lack of difference between TTC judgments under 1-second glimpse versus normal viewing conditions, these results suggest that drivers pick up TTC very quickly when assessing the forward scene after a period of losing visual and/or cognitive contact with the vehicle ahead. Fourth, since the concurrent distraction task examined (i.e., mental addition task) did not lengthen TTC judgments, this suggests that the previous TTC lengthening effect observed by Groeger and Brown (1999) under laboratory conditions may be restricted to substantially higher TTC conditions than the relatively low TTC values examined here. Fifth, age and gender effects were not found in this analysis (however, note the age effect reported below).

<u>Figure 12</u>*. Mean TTC Ratio as a Function of Driver Speed and Delta Velocity (△V)*

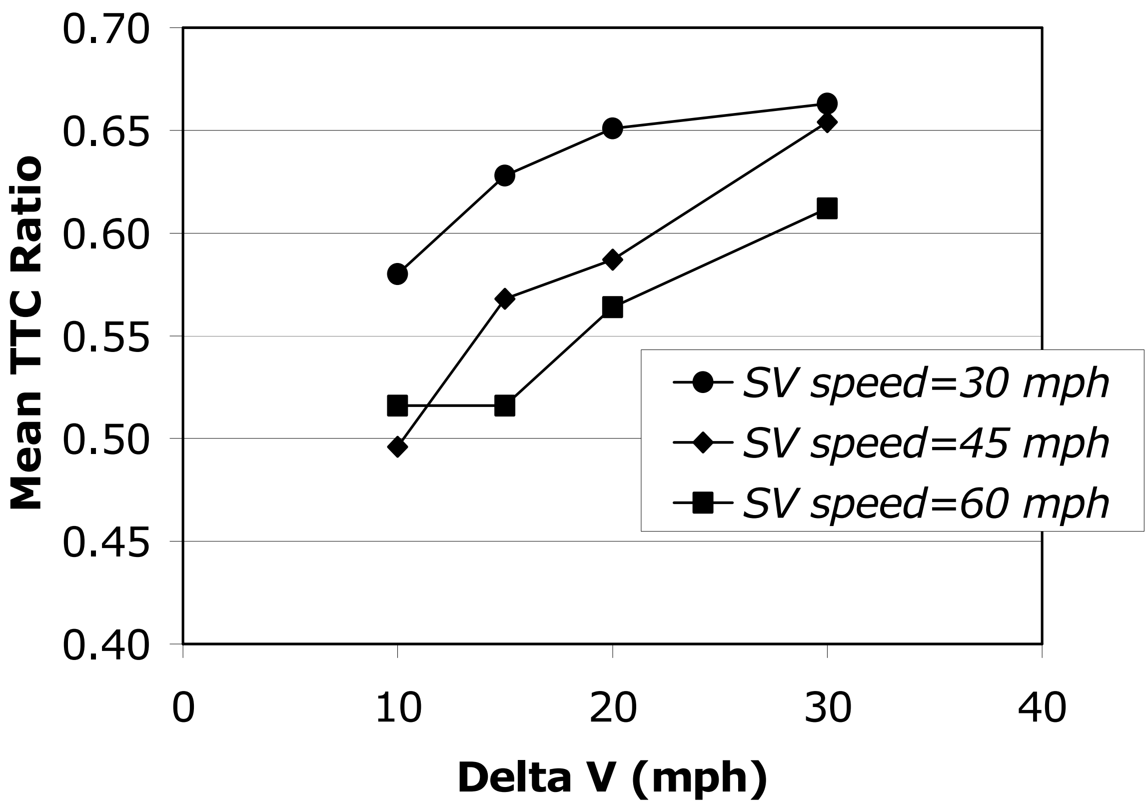
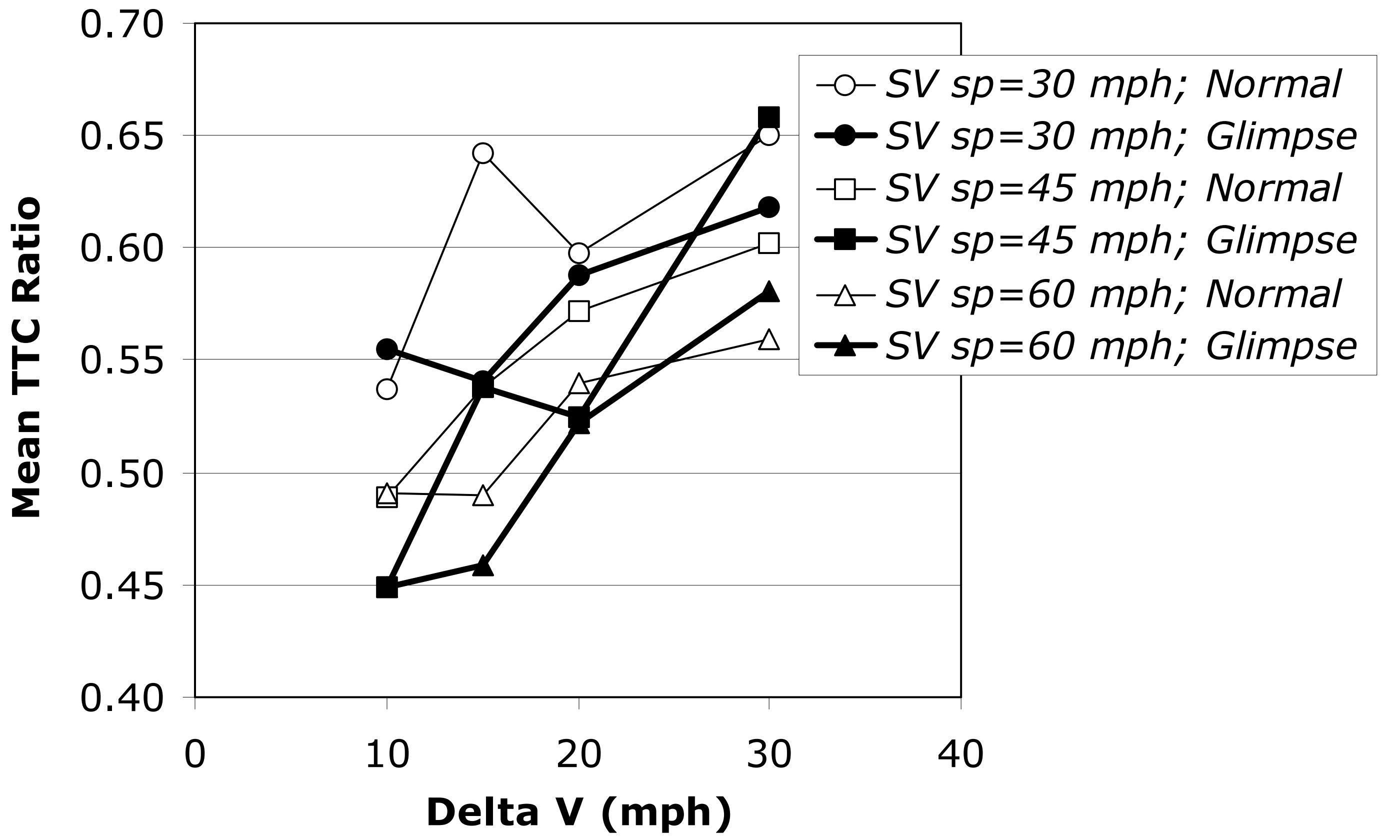
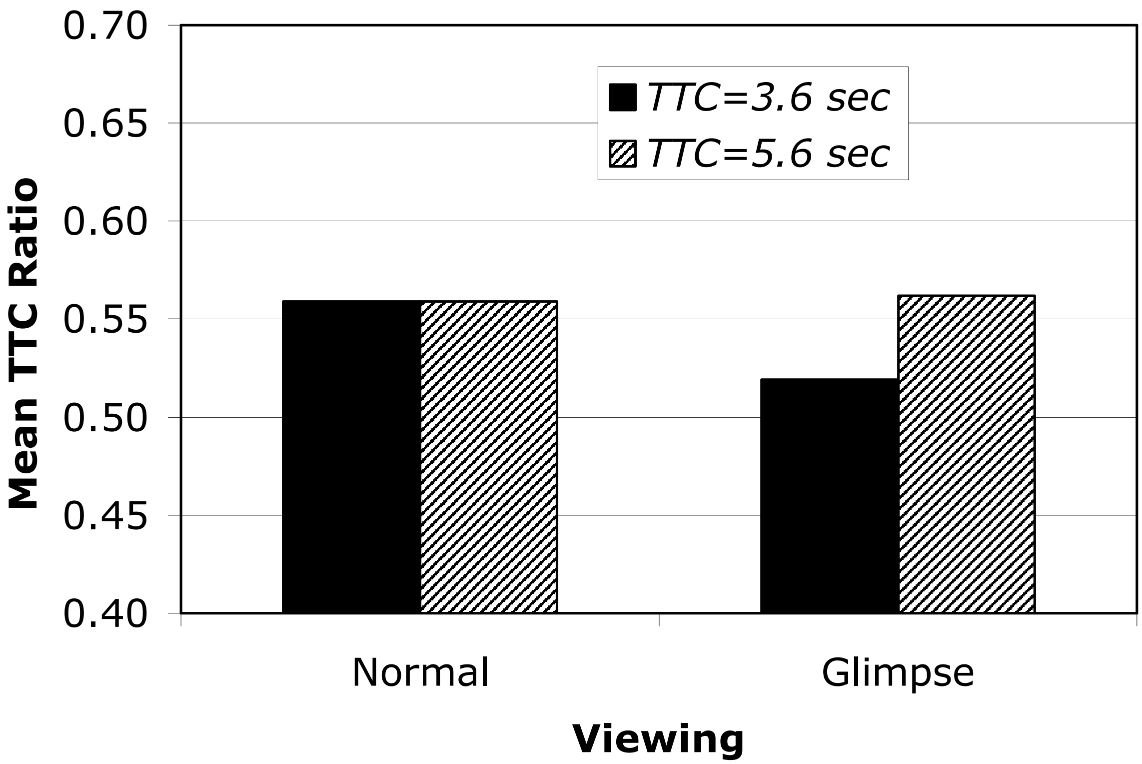


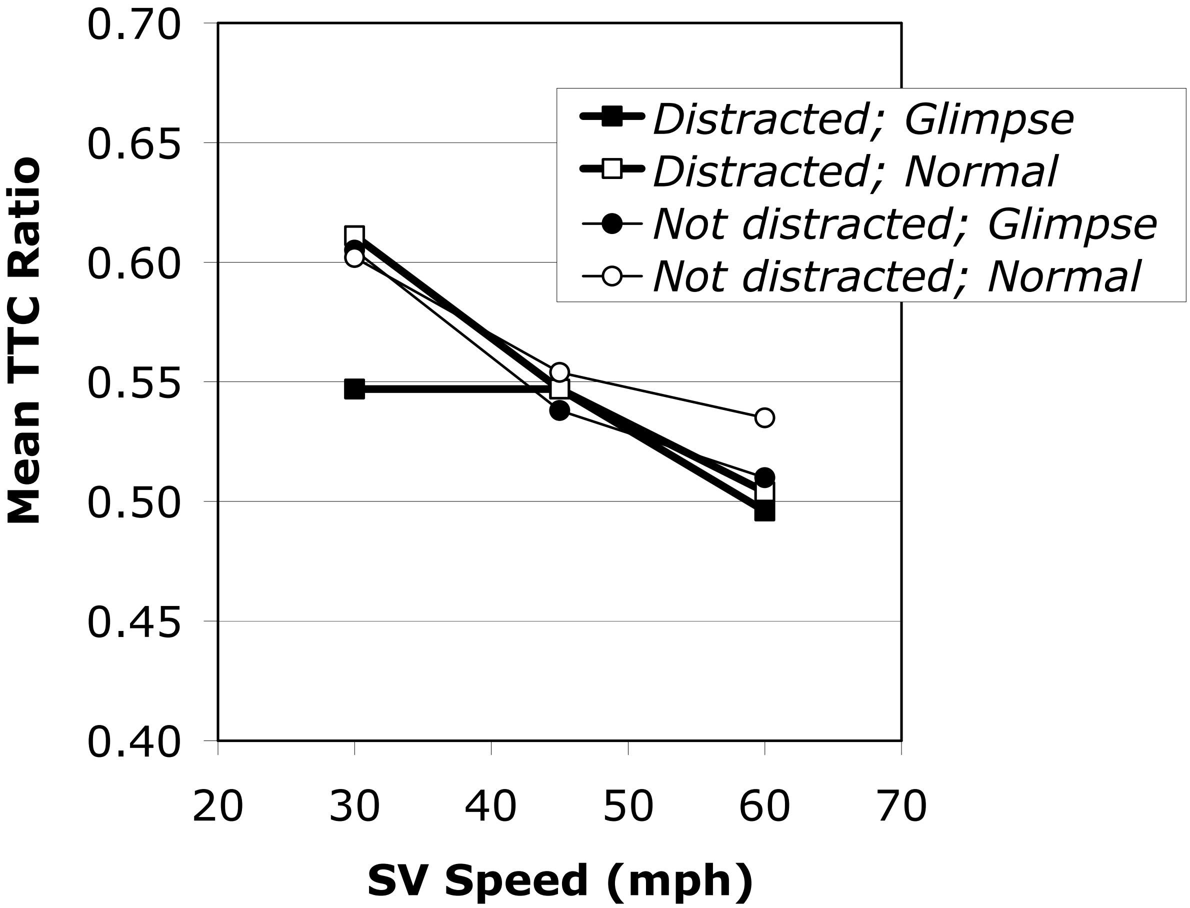
Figure 13. ***Mean TTC Ratio as a Function of Driver Speed, Delta Velocity (△V), and Viewing***



<u>Figure 14</u>. *Mean TTC Ratio as a Function of Actual TTC and Viewing*



<u>Figure 15</u>*. Mean TTC Ratio as a Function of Driver Speed, Viewing, and Distraction*



Number of TTC Overestimations

Logistic regression was used to predict the probability of TTC overestimation, which provides a measure of tendencies toward more extreme TTC judgments, which may be suggestive of underlying collision causation. The following predictors were examined; age, gender, distraction presence, driver speed, lead vehicle speed, △V, actual TTC, and viewing condition. The only significant predictors resulting from this analysis were age and △V. The observed and modeled probability of TTC overestimation employing these predictors, as well as the logistic regression model itself, are shown in figure 16. The Hosmer and Lemeshow χ^2 test indicated a nonsignificant deviation ($\chi^2 = 11.126$, df=8, p=0.195) between the observed data and the modeled data (Hosmer and Lemeshow, 2000), which provides support for the model. These results indicate the probability of overestimation increases with age, and as △V increases. Note that the model shown in figure 16 extrapolates beyond the range of **△V** values examined in the current study (i.e., 10-30 mph).

Figure 16*. Observed and Modeled Probability of TTC Overestimation as a Function of Age and Delta V (△V)*

General Discussion

The current data set provides the most extensive set of TTC judgment data ever gathered under real approach conditions. Hence, these data are most applicable to real-world conditions, and provide a useful tool for validating/calibrating TTC data gathered under less realistic simulator and laboratory approach conditions. Further TTC research involving simulated approaches should consider examining lower actual TTC values than were logistically possible to execute under these test track conditions.

Drivers generally underestimated TTC, and increases in driver speed decreased TTC estimates, whereas increases in △V increased TTC estimates. Results also suggested that TTC judgments were not adversely impacted by either a limited, 1-second viewing time (such as that which might be available following an FCW alert) or by a concurrent distraction task (i.e., mental addition). Indeed, TTC judgments actually became shorter, and hence, more conservative under some limited viewing and distraction conditions. These results suggest that drivers pick up TTC information very rapidly, and that TTC judgments under low actual TTC conditions may by largely unaffected by ongoing distraction activity. This latter result may be due to the salient nature of looming (or rapid changes in inverse TTC values) occurring under low (3.6-5.6 second) TTC conditions. Finally, a logistic loglinear model of this data suggests that increases in age and relative velocity (or delta velocity) lead to higher probabilities of TTC overestimation (i.e., where perceived TTC exceeds actual TTC). These model trends may prove useful for deepening understanding of rear-end collision causation.

These results also provide a test of two competing models of TTC estimation (McLeod and Ross, 1983). Under the optic flow method of TTC estimation, which is akin to the visual or perceptual definition of TTC, it is hypothesized that drivers estimate TTC by operating directly on the visual scene and associated looming properties of the lead vehicle. This higher-order heuristics method may be characterized as direct, efficient, and involving rapid automatic processing. Under the cognitive method, which is akin to the kinematic definition of TTC, drivers estimate both their speed and distance to the vehicle ahead, and integrate this information for TTC estimation purposes. This lower-order heuristic method may be characterized as indirect and involving slower, computational processing relative to the optic flow method. As argued by Cavallo and Laurent (1988), results from a study under actual driving conditions provide a fairer test of these two competing TTC estimation models than can be achieved with simulated approach studies, since the cognitive method is highly dependent on the quality and amount of speed and distance information available.

The general lack of viewing time, distraction, age, and gender effects on TTC estimation under these low TTC conditions provides evidence for a robust, immediate pick-up of TTC information, and hence, strongly favors the optic flow method. The cognitive method would predict that longer viewing times would increase the accuracy of speed and distance (and hence, TTC) estimation (McLeod and Ross, 1983), and that a concurrent task would interfere with TTC judgments (Groeger and Comte, 1999). On the other hand, the influence of driver speed and △V on the observed TTC estimations is inconsistent with a pure form of the optic flow method, which would predict no effects of these variables. The driver speed effect observed here is inconsistent with previous test track findings reported by Cavallo and Laurent (1988) under 3- and 6-second TTC approach conditions, which found no effect of speed (in the binocular-normal field of view condition) while approaching a stationary object. The inconsistency across studies may be due to different processes or strategies underlying TTC estimation when approaching a stationary versus moving lead vehicle. (Note that only one lead vehicle stationary condition was examined here.)

Together, these results suggest that driver speed and $\triangle$V may play a role in modulating TTC judgments primarily driven by optic flow, which provides an efficient, elegant, lower-order heuristic for drivers to use in approach situations. Furthermore, test track results from Cavallo and Laurent (1988) lend support to the possibility that the optic flow TTC estimation heuristic may be less dominant for beginner than experienced drivers, since TTC estimations under limited field-of-view conditions that reduced speed information available were more influenced by speed for beginner drivers.

Finally, it should be stressed that these results do not directly assess the distinct possibility that the cognitive method of TTC estimation may play a larger role under higher TTC (e.g., near looming threshold) conditions, where TTC accuracy has been shown to decline substantially (McLeod and Ross, 1983). Indeed, Schiff and Detwiller (1979) suggest TTC may not be used much beyond 10-second TTC values.

First-Look Maneuvers

The first-look technique was aimed at quantifying a surprised driver's reaction to an FCW alert. After receiving an alert, the surprised driver must decide as quickly and accurately as possible whether to perform an aggressive crash avoidance maneuver, what type of maneuver to perform (braking and/or steering), and the time-course and aggressiveness of this maneuver. In this study, in order to create an extreme form of driver distraction (i.e., a surprised driver) in which the driver has lost all visual and/or cognitive contact with the vehicle ahead, a portion of the driver's central vision was blocked with an occlusion window during the initial phase of an in-lane approach such that the driver could not see the lead vehicle. During the last phase of this approach, the driver's vision was suddenly "opened" at a point in time intended (based on surprise trial data reported earlier) to correspond to when a driver would get a "first look" at the vehicle ahead after receiving an FCW alert. The timing of the vision opening was based on the CAMP FCW "minimum" alert timing approach (Kiefer, et al., 2003), coupled with the assumption that at the time of the alert onset, the driver was dialing and looking at a cellular phone mounted on the center console while a foot was on the accelerator. A driver is presumed to be in an alerted state shortly after an FCW alert is issued, which in this case corresponds to the time of the window opening.

Following the vision opening, drivers had to quickly decide the type of crash avoidance maneuver to perform (if any) and execute this maneuver. Drivers were encouraged to brake if at all possible unless they were not closing on the vehicle ahead (referred to as "catch trials"), in which case they were instructed to refrain from either braking or steering. If the driver was closing in on a vehicle ahead after vision opening, two steps were taken to prevent the driver from adopting a strategy of either always braking or always steering. To discourage the driver from adopting a strategy of always braking, trials were included with very late window opening timing, where a steering avoidance response was predicted to be favored over braking based on Kiefer, et al. (2003) last-second maneuver data. To discourage the driver from adopting a strategy of always steering, a trailing vehicle was present which passed in and out of the driver's blind spot and discouraged a steering response. Overall, it is felt this first-look method may be a promising technique for generating decision-making and maneuver behavior representative of that which would be obtained from drivers under real-world, rear-end crash scenarios.

There were five major goals of this experimentation. First, to assess the robustness of the CAMP FCW timing approach across a wide range of vehicle-to-vehicle kinematic conditions that were intended to require increased decision-making complexity relative to that experienced by drivers in previous and current CAMP FCW surprise trial research (Kiefer, et al., 1999). Test driver assists (or interventions) were used as the key measure to assess the effectiveness of this timing approach. A second goal of this study was to quantify drivers' ability to quickly decide upon and execute a crash avoidance maneuver. A third goal was to begin to explore the relative effectiveness of later FCW timing approaches, which was accomplished by using very late window opening timing. A fourth goal was to assess the validity of this method for gathering "surprise, trial-like" data by comparing current results to those found with a matched subset of actual surprise trial data. Assuming that this first-look technique is validated, this technique has the distinct advantage of allowing experimentation where repeated observations can be obtained from a given subject. This can be compared to the relatively inefficient surprise trial approach described earlier, in which only a single observation can be obtained per subject. The fifth and final goal of this research was to provide a methodology and comparison dataset for guiding any subsequent simulator calibration/validation research. As was argued earlier, it is important to understand the relationship between a driver's decision-making and maneuver behavior under simulated versus real approach conditions.

Method

Participants

The 48 participants in the first-look study also completed one surprise trial for the surprise trial study reported earlier. Half of the 48 participants were 20 to 30 years old (*mean* = 26.83, *SD* = 2.14) and the other half were 60 to 70 years old (*mean* = 65.42, *SD* = 3.03), with equal numbers of males and females in each age group. Drivers were tested individually in a 1 ½ hour testing session and paid $150 for their participation. Naive participants were recruited from a database of licensed drivers in the metropolitan area surrounding the test facility via an outside, independent, market recruiting firm. The recruitment criteria required that participants be licensed drivers who drove regularly and were free of any conditions that may have limited their ability to safely participate in the test.

Subject vehicle, surrogate target lead vehicle, and principal other vehicle

The CAMP surrogate target methodology apparatus (SV, surrogate target, and POV), shown in figure 1, was identical to that employed in the previous studies.

Visual occlusion device

A Polytronix Polyvision privacy glass window was added to the driver-side hood of the SV for visual occlusion purposes, as shown in figure 17. The window was vertically mounted on the front hood (perpendicular to the road) within a 37.5" wide by 33.5" high aluminum frame (the frame itself was 1.5" wide). Polyvision glass windows are constructed of laminating polymer-dispersed liquid-crystal film sandwiched between two layers of glass. When electrically charged, the liquid-crystal film can be rapidly changed from translucent white (in the "off" state) to optically clear (in the "on" state). The Polyvision window was used to occlude the driver's view of the central portion of the forward scene. A two-wire waterproof connector connected the window to a controller in the trunk of the vehicle, which could be switched on and off by a computer-operated relay switch.

Data acquisition and experimenters

The data acquisition system and the experimenter roles were identical to the approach taken in the previous studies.

Experimental procedure

This study was conducted under dry road, daytime conditions on the road that can be seen in figure 1. Upon entering the vehicle, drivers were instructed to make themselves comfortable for driving by adjusting the seat, steering wheel, and mirrors as necessary. They were also required to securely fasten their seatbelts. They were informed that the surrogate target was designed to allow impacts, and of the add-on steering wheel and brake available to the passenger-side experimenter test driver. Drivers then experienced an unexpected lead vehicle braking, surprise trial. Results from these surprise trials were reported earlier. Drivers were then familiarized with the open and closed states of the occlusion window used for visual occlusion purposes as shown in figure 18. This was followed by 4 practice and 21 experimental visual occlusion test trials. A few drivers required additional practice trials in order to feel comfortable with the "driving blind" procedure described below.

Figure 17. ***Occlusion Window***

Figure 18. ***Closed and Open States of the Occlusion Window from the Driver's Perspective***

Drivers were told that the trials were designed to examine whether distracted drivers could choose and execute safe driving maneuvers near the time a crash warning would be given. The trial procedure for the initial phase of the various in-lane approaches explained below varied somewhat based on the speed and deceleration profile of the lead vehicle. However, all in-lane approaches involved the driver being instructed to accelerate to and maintain a target speed, and the occlusion window being placed in the closed state (which prevented the driver from seeing the lead vehicle). As mentioned earlier, this closed window state was intended to represent an extreme form of driver distraction in which the driver has lost all visual and/or cognitive contact with the vehicle ahead. Once the window was placed in the closed state, the drivers were instructed to maintain the target speed with a foot on the accelerator and keep their hands on the steering wheel, while the passenger-side experimenter assumed steering control and maintained the lane position of the vehicle by using the add-on steering wheel. Note that drivers still had visual information available through the side windows and portions of the front windshield, which is important since non-central visual information plays an important role in speed perception.

During some point during the final phase of this in-lane approach, the occlusion window instantly changed from the closed state to the open state. Thus, this technique provided an experimentally-controlled, surprise-like situation. At the point of window opening, drivers were instructed to choose and execute one of three driving maneuvers as necessary to avoid the lead vehicle—do nothing at all, brake, or steer (i.e., change lanes to the left to avoid colliding with the lead vehicle). If drivers were closing on the vehicle ahead, they were told to brake if at all possible, and to use steering only as a last resort. Drivers performed these in-lane approaches under the vehicle-to-vehicle kinematic conditions described in table 11.

Table 11. ***Timing and Kinematic Conditions for In-Lane Approaches***

	KINEMATIC CONDITION		
TIMING	SV Speed (mph)	POV Speed (mph)	POV Decel.
Catch	30	30	0
	60	60	0
CAMP FCW Alert	30	30	-0.15g
	30	30	-0.39g
	60	60	-0.15g
	60	60	-0.39g
	30	20	0
	60	30	0
	30	stat	0
85th Percentile Steering	60	60	-0.15g
	60	30	0
	30	stat	0

Each trial also involved a third vehicle in addition to the SV and POV, as shown in figure 19. The third vehicle, a black 2004 Pontiac Vibe, traveled in the lane adjacent to the SV while the driver accelerated to the target speed and while the occlusion window was closed. During these phases of the in-lane approach, the third vehicle's position varied between being completely parallel with the SV to dropping back into the participant's left blind spot. The third vehicle continued with an irregular forward-backward positioning pattern relative to the SV until the occlusion window was opened. At this point, the third vehicle fell back into one of two positions. On all catch trials and half of the CAMP FCW alert timing trials, the third vehicle stayed in the lane adjacent to the SV but decreased speed until clear of the driver's blind spot. On the other half of the CAMP FCW alert timing trials and on all steering trials (described below), the third vehicle decreased speed until clear of the participant's blind spot and then changed lanes behind the SV. The presence of this trailing third vehicle was intended to increase the complexity and real-world validity of the driver's maneuver decision-making process. Indeed, drivers often complained that they found the third vehicle annoying.

Figure 19. ***Configuration of Vehicles and Varying Behavior of Trailing Vehicle***

For trials involving lead vehicle braking (i.e., POV deceleration), both the participant driver and the POV driver accelerated to the target speed (either 30, 45, or 60 mph). The participant drivers were instructed to let the experimenter know when they had stabilized at their normal following distances so that the experimenter knew they were ready for the occlusion window to be placed in the closed state. After the driver had stabilized the SV speed after the window closed, a computer signal was sent to the POV to begin the lead vehicle braking event. When the timing criteria for placing the window in the open state was met, the window was cleared, and the driver quickly selected and executed a maneuver (i.e., do nothing, brake, or steer) to avoid colliding with the lead vehicle. The staging for the catch trials, in which the driver was not closing on the vehicle ahead at window opening and both vehicles traveled at the target speed, was identical to these lead vehicle braking trials with the exception that the lead vehicle never decelerated.

For trials in which the lead vehicle traveled at a constant speed which was slower than the following vehicle (i.e., constant ΔV trials), the trial began with the SV stopped in the lane directly behind the lead vehicle and the occlusion window was placed in the closed state. The POV driver then was given a "head start" and accelerated to the target speed (either 20 or 30 mph). Soon after, the driver was instructed to quickly accelerate up to and maintain the target speed (either 30 or 60 mph) while their forward view was blocked and the front-seat experimenter maintained lane position with the add-on steering wheel. As in the lead vehicle braking trials, the driver's forward view remained blocked until the timing criteria for the window opening was met, at which time the driver had to quickly decide upon and execute the appropriate maneuver.

In some instances, the front-seat passenger test driver was forced to assist participants with braking and/or steering in order to avoid collisions with the surrogate target. No collisions with the lead vehicle occurred. The frequency of test driver assists in the various conditions is reported in the results section.

Experimental design

The between-subjects independent variables were age (20-30 or 60-70 years old) and gender. The within-subjects variables were the timing of the window opening (catch, CAMP FCW alert timing, and steering) and the in-lane approach or vehicle-to-vehicle kinematic conditions (SV speed/POV speed/POV deceleration profile). The combinations of timing and kinematic conditions examined are shown in table 11.

The two catch trials (in which an avoidance maneuver was not necessary) were conducted at 30 and 60 mph, and involved both the lead and following vehicles remaining at the target speed after window opening. Consequently, the driver was not closing in on the vehicle ahead, and they were instructed to refrain from steering or braking.

The seven CAMP FCW alert timing trials included four lead vehicle braking scenarios (30/30/0.15, 30/30/0.39, 60/60/0.15, and 60/60/0.39), two constant ΔV scenarios (30/20 and 60/30), and one lead vehicle stationary scenario (30/0). Each of these seven trials was experienced twice by the subjects. These trials were chosen to span a wide range of vehicle-to-vehicle kinematic conditions. The window opening timing for the CAMP FCW alert timing trials was intended to correspond to the "first look" to the forward scene after an FCW alert, at which time the driver is assumed to be in an alerted state. The window opening timing for these trials was based on CAMP FCW alert braking onset assumptions (Kiefer, et al., 2003) coupled with a 1,041-millisecond delay time. This delay time was based on assuming a (95th percentile) 1,520-ms driver brake RT delay (Kiefer, et al., 2003), a 140-ms delay corresponding to the delay between requesting window opening and actual window opening, and a 66-millisecond delay in range information. Furthermore, a 685- millisecond FCW alert onset-look-up delay was subtracted from the composite sum of these three delays (1,726 ms), to yield the 1,041-millisecond delay time assumption used for window opening timing purposes. The alert onset-look up delay corresponds to the time between when drivers receive an FCW alert while looking down and when they first fixate on the scene ahead. The 685-millisecond assumption for this delay value is based on the average value for surprise trial data discussed earlier, which met the following conditions at the time the FCW alert was issued: The drivers were performing the digit span dialing task, looking directly at the (center-console-mounted) cellular phone, dialing the phone, and pressing the accelerator with their right foot.

These seven different CAMP FCW alert timing trials formed the basis for evaluating whether the CAMP FCW alert timing approach would allow subjects sufficient distance to perform unassisted, in-lane braking maneuvers under these decision-making conditions. The catch trials and steering timing trials (described below) were included primarily to increase the complexity of the "last-second" maneuver decision, as well as to discourage subjects from developing a response set strategy, such as always braking or always steering on every trial.

The three steering timing trials included one lead vehicle braking scenario (60/60/0.15), one constant ΔV scenario (60/30), and one lead vehicle stationary scenario (60/0). Note that these three scenarios were also run under corresponding CAMP FCW alert timing conditions. Previous CAMP FCW last-second maneuver data indicated that driver's last-second steering onsets occurred later than their last-second braking onsets for these three in-lane approach scenarios (Kiefer, et al., 2003). Hence, the participant had less time to avoid colliding with the lead vehicle under these steering timing relative to the matched CAMP FCW alert timing conditions. It was expected that the steering timing would encourage a steering maneuver and discourage subjects from automatically deciding to brake if they were closing on a vehicle ahead (i.e., during a non-catch trial), particularly since the steering timing was based in part on 85th percentile (aggressive) last-second hard steering onsets. For the 30/0/0, 60/60/0.15, and 60/30/0 conditions, this 85th percentile hard steering onset assumption corresponds to observed 75th, 90th, and 95th hard last-second braking onset values, respectively, based on the data of Kiefer, et al. (2003). Hence, these steering trials provided preliminary data addressing the consequences of later FCW alert timing assumptions. This 85th percentile hard steering onset assumption was coupled with a 526- millisecond delay time assumption to create the window opening timing for steering timing trials. The 526- millisecond delay was based on assuming a 320- millisecond steering onset delay, a 140- millisecond delay corresponding to the delay between requesting window opening and actual window opening, and a 66- millisecond delay in range information.

Each driver completed 4 practice trials followed by 21 experimental trials. The first practice trial was a catch trial conducted at 30 mph. The second and third practice trials employed the CAMP FCW alert timing with a condition in which both vehicles traveled at 45 mph and then the lead vehicle decelerated at -0.28 g's. The fourth practice trial employed the steering timing with a condition in which both vehicles traveled at 30 mph and then the lead vehicle decelerated at -0.15 g's. Each of the four random orders of the experimental trials was created so that the same kinematic condition never occurred on consecutive trials and that equal numbers of each trial type occurred in the first, second, and third portions of the trial sequence. Trials were also balanced across the four random orders such that each timing/kinematic combination condition occurred in the first, second, and third portions of the trial sequence.

Driver performance measures

The dependent measures analyzed included test driver assists (or interventions), maneuver choice (brake, steer, brake and steer, nothing), maneuver onset reaction time (defined as the time between window opening and maneuver onset), the (constant) required deceleration at brake onset, and the peak deceleration value throughout the maneuver. In terms of evaluating the robustness of the CAMP FCW alert timing approach, the number of test driver assists is considered the key measure of this analysis.

Results and Discussion

Test Driver Assists and Maneuver Choices

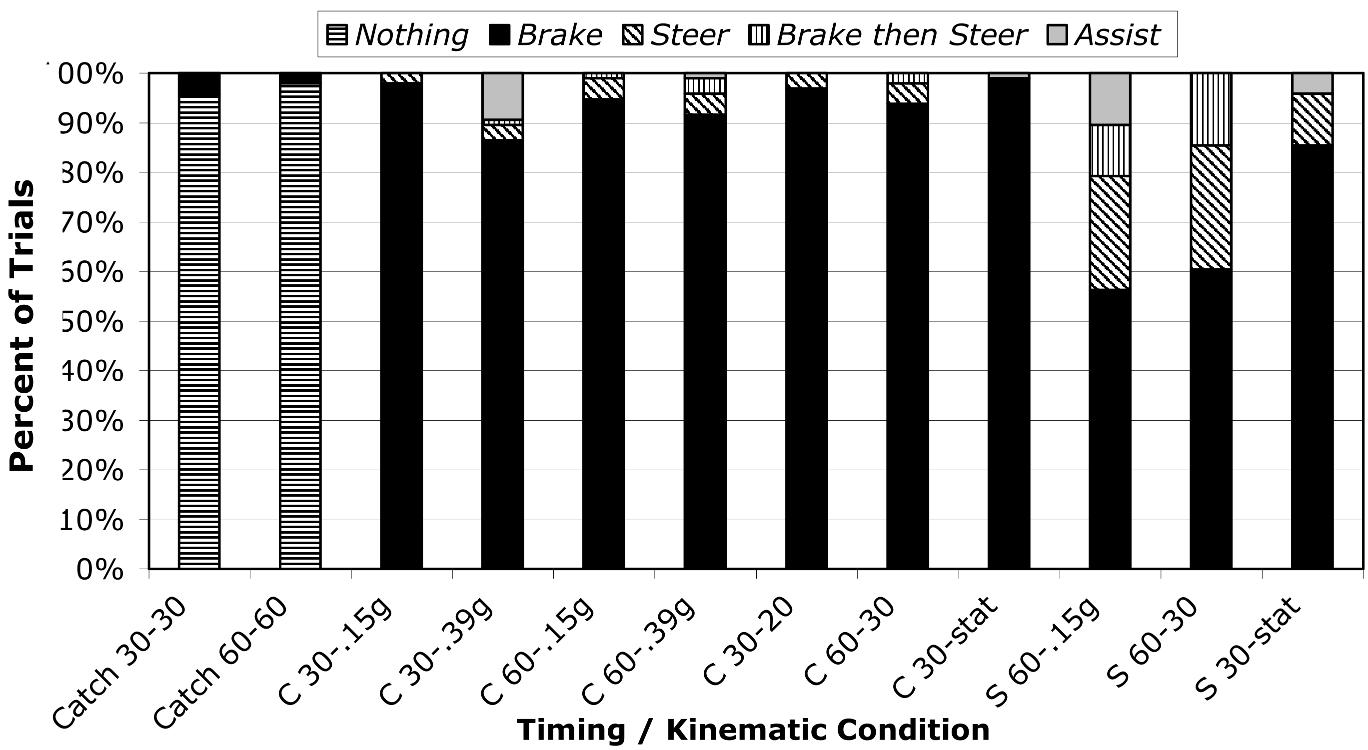
The main results from this study are shown in figure 20, which provides the percent of test driver assists (or interventions) and maneuver choices as a function of the various timing/kinematic conditions examined. The following results are noteworthy. First, the lack of avoidance maneuver responses during catch trials suggests these trials were extremely effective at discouraging drivers from reflexively performing a braking or steering response immediately after vision opening. Second, unassisted, successful braking was observed for 86 percent-99 percent of trials across the various CAMP FCW alert timing/kinematic conditions examined (shown in the middle portion of figure 20). This finding provides strong support for the robustness of the CAMP FCW alert timing approach. It is worth noting that the lowest percentage of unassisted, successful braking (86 percent) occurred in the 30/0.39 g condition, which provides support for the continued use of this condition for FCW surprise trials. (Note this kinematic condition has been extensively used in both the current research, as well as in the previous CAMP FCW surprise trial research [Kiefer, et al., 1999]). Third, braking was the predominant response when drivers were closing on a vehicle ahead, even under steering timing conditions. This tendency to brake may have been inflated by the instructions ("brake if at all possible") and the presence of the trailing vehicle. Fourth, as one might expect, a comparison of maneuver choices under matched kinematic conditions (i.e., the 60/0.15 g, 60/30, and 30/0 conditions) suggest a systematic relationship whereby steering maneuver attempts will increase as higher percentile last-second braking values are used for window opening timing. Of particular interest were results from the 30/0 condition, which indicated that the 75th percentile braking assumption used for the steering timing yielded a high percentage (85%) of unassisted, successful braking maneuvers. More generally, given that the percent of unassisted, successful braking responses was uniformly high across these CAMP FCW alert timing/kinematic conditions, this pattern of results provides support for the merit of exploring the trade-offs of later FCW alert timing in further research (discussed further below).

The effects of age and gender on the overall percent of test driver assists and maneuver choices are shown in figure 21. Due to the low percent of test driver assists across these groups, it remains difficult to reach any statistical conclusions with respect to the effects of age or gender on this measure. For trials not involving a test driver assist, results suggest that maneuver choices were largely consistent across the age by gender groups, with perhaps a diminished tendency for older (particularly male) drivers to perform a "steer only" maneuver (or increased tendency to perform a "brake only" maneuver).

Maneuver Onset Reaction Time

Three separate repeated measures ANOVAs were performed on maneuver onset RT for trials in which an unassisted, successful maneuver was performed. (In addition, catch trials were excluded from this analysis.) The first ANOVA focused on examining the effects of maneuver choice, age, and gender on maneuver onset RT. The within-subjects variable was driver maneuver choice (brake only, steer only, and brake then steer) and the between-subjects variables were age (20-30 or 60-70 years old) and gender. There was a main effect of age ($F(1, 782) = 3.90$, $p < 0.05$) and an Age x Maneuver Choice interaction ($F(2, 782) = 3.74$, $p < 0.05$), which is shown in figure 22. This interaction indicates that when the maneuver choice was "brake then steer" (which as indicated in figure 21 was a very low probability maneuver), younger drivers had significantly faster maneuver onset RTs than older drivers (538 ms versus 709 ms).

<u>Figure 20</u>. *Test Drivers Assists and Maneuver Choices Across the Various Timing/Kinematic Conditions*



<u>Note</u>: For the horizontal axes labeling, "C" corresponds to CAMP FCW alert timing trials and "S" corresponds to 85th percentile last-second steering timing trials.

Figure 21. ***Test Drivers Assists and Maneuver Choices as a Function of Age and Gender***

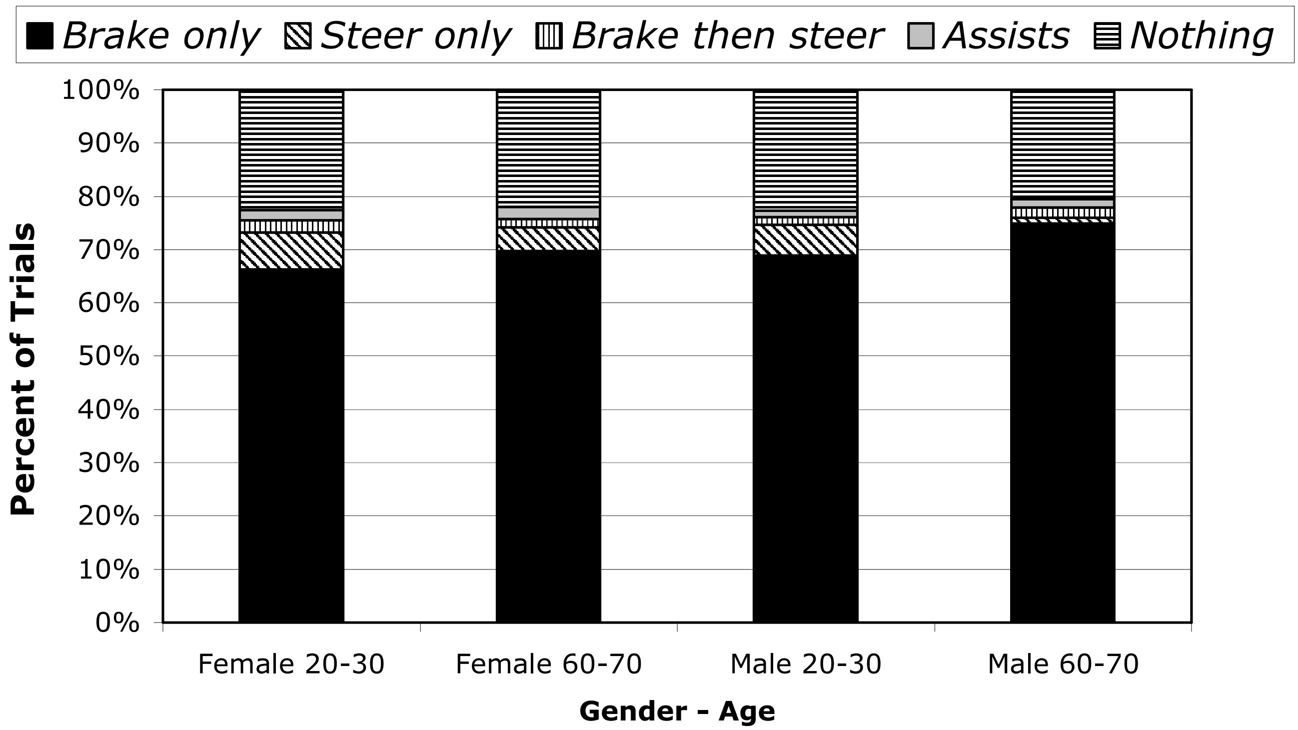
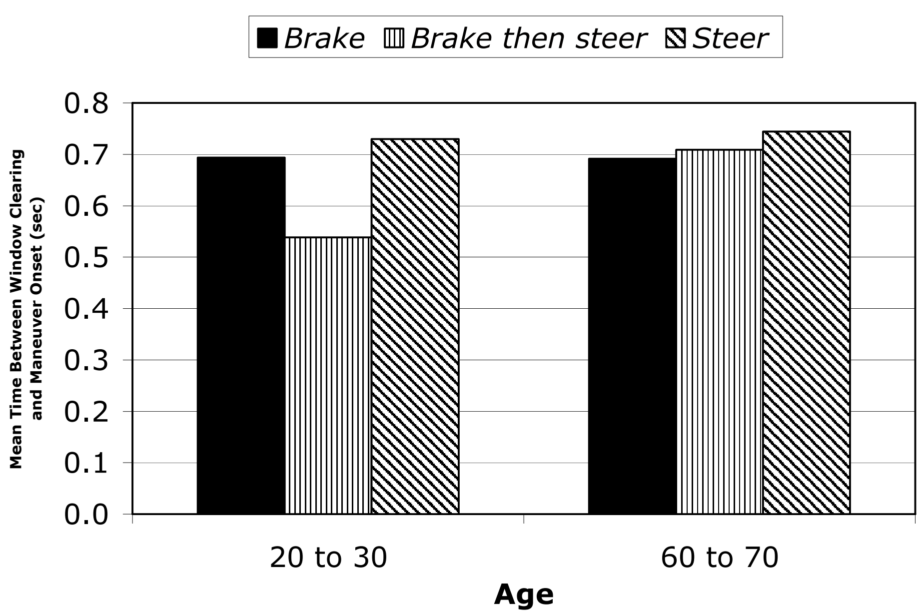


Figure 22. ***Maneuver Onset Reaction Times as a Function of Age and Maneuver Choice***



The second ANOVA focused on examining the effects of speed, lead vehicle deceleration profile, age, and gender on maneuver onset reaction time. This analysis employed data from the following CAMP FCW alert timing/kinematic conditions: 30/0.15, 30/0.39, 60/0.15, and 60/0.39. The within-subjects variables were speed condition (30 and 60 mph) and lead vehicle deceleration profile (0.15 and 0.39 g's), and the between-subjects variables were age (20-30 or 60-70 years old) and gender. There was a main effect of speed ($F(1, 44) = 30.88$, $p < 0.001$), under which maneuver onset RTs were slightly faster in the 30 mph versus 60 mph condition (635 versus 726 ms, respectively). These results can be observed in the left portion of Figure 23 for the timing/kinematic conditions employed in this analysis.

The third ANOVA focused on examining the effects of timing, kinematic condition, age, and gender on maneuver onset reaction time. This analysis employed data from the 60/0.15, 60/30, and 30/0 kinematic conditions, which were matched kinematic conditions across the two timing levels examined (CAMP FCW and steering). The within-subjects variable was timing (CAMP FCW alert and steering) and kinematic condition (60/0.15, 60/30, and 30/0), and the between-subjects variables were age (20-30 or 60-70 years old) and gender. There was a main effect of timing ($F(1, 43) = 156.30$, $p < 0.001$), condition ($F(2, 42) = 10.09$, $p < 0.001$), and a Timing x Kinematic Condition interaction ($F(2, 42) = 5.38$, $p < 0.01$). This latter interaction can be observed in figure 23 (focusing on the conditions used in this analysis), and indicates that the maneuver onset RTs were faster in the (later) steering relative to CAMP FCW alert timing condition, and that this difference ranged from 193 to 334 milliseconds faster across the three matched kinematic conditions. Within both timing conditions, maneuver onset RTs were fastest in the 60/0.15 kinematic condition.

Hence, overall, these results indicate that maneuver onset RTs were fastest under higher speed and later window opening (i.e., steering) timing conditions. Furthermore, these effects were largely uninfluenced by age or gender, particularly when considering the predominant brake only and steer only responses.

Required Deceleration at Maneuver Onset

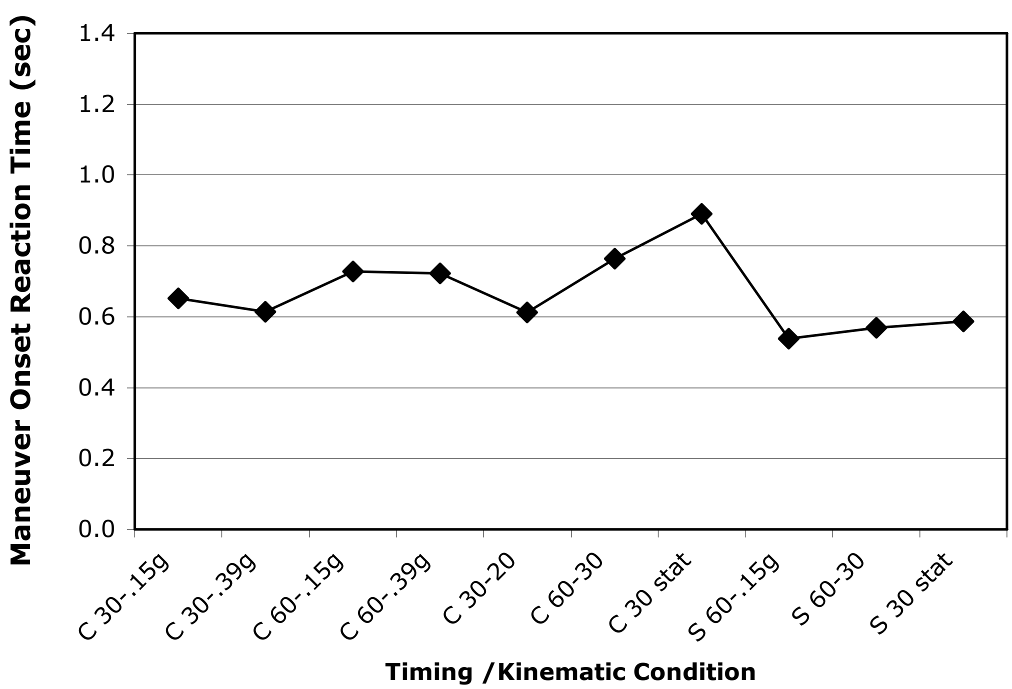
The corresponding ANOVA described above for the maneuver onset RT variable was performed on the required deceleration at maneuver onset variable. The first ANOVA focused on examining the effects of maneuver choice, age, and gender on required deceleration, and indicated a main effect of maneuver choice ($F(1, 781) = 16.05$, $p < 0.001$). Mean required decelerations at brake onset for trials involving brake only, steer only, and brake then steer maneuver choices were 0.31, 0.38, and 0.43 g's, respectively. This pattern of results is largely dictated by the timings employed in this study, and indicate that as levels of required deceleration at brake onset increase, the likelihood of the driver transitioning from brake only responses to steer only responses (with a "brake then steer" point intervening between these maneuver choices) will increase.

The second ANOVA focused on examining the effects of speed, lead vehicle deceleration profile, age, and gender on required deceleration indicated a 3-way Age x Speed x Lead Vehicle Deceleration Profile interaction ($F(1, 43) = 6.88$, $p < 0.05$). This interaction is shown in the leftmost portion of figure 24 (for the timing/kinematic conditions employed in this analysis), which provides mean required deceleration at braking onset as function of timing/kinematic condition and age. For three of the four timing/conditions examined in this analysis, required decelerations were slightly higher for the older age group.

The third ANOVA focused on examining the effects of timing, kinematic condition, age, and gender on required deceleration. There was a main effect of age ($F(2, 44) = 5.97$, $p < 0.05$) that indicated slightly higher required decelerations for older relative to younger drivers (0.35 versus 0.33 g's, respectively). Results also indicated a Timing x Condition interaction ($F(2, 36) = 15.82$, $p < 0.001$) similar to the pattern of results observed with the maneuver onset RT variable. Required decelerations were higher in the steering relative to the CAMP FCW alert timing condition (as expected), and this difference ranged from 0.18 to 0.24 g's across the three matched kinematic conditions (i.e., 60/0.15, 60/30, and 30/0).

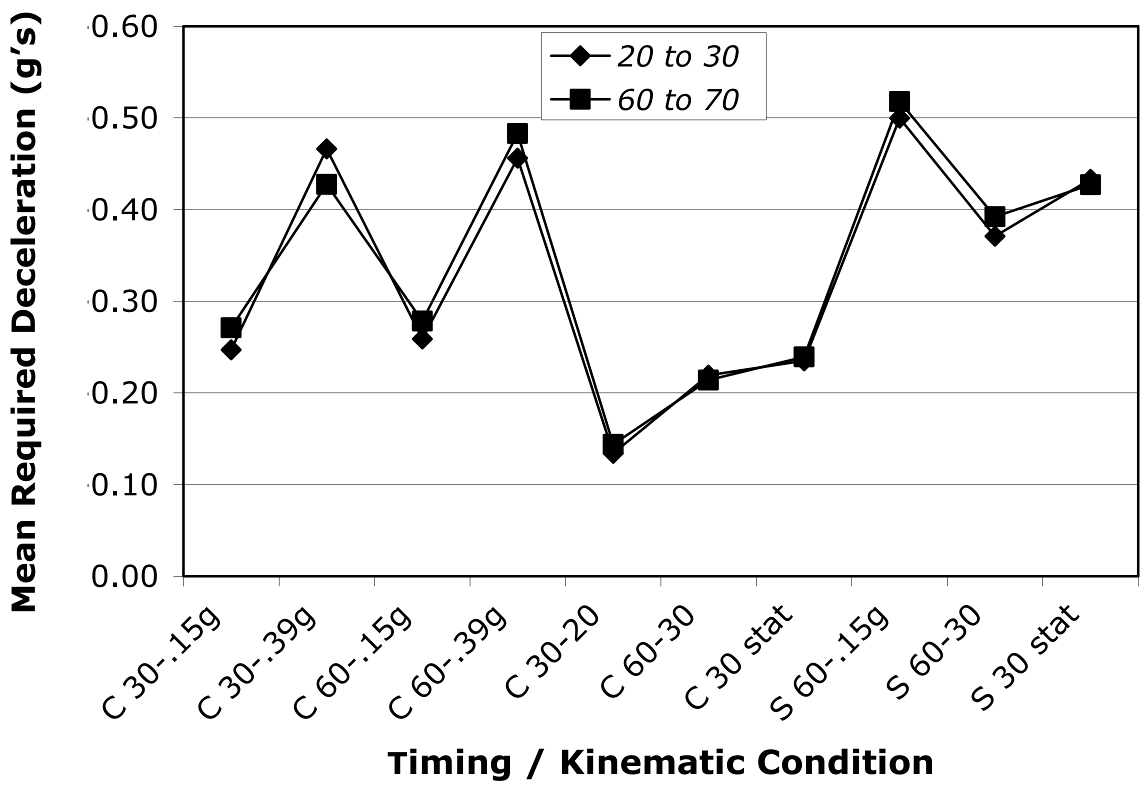
Hence, overall, these data suggest a trend for slightly higher required deceleration at braking onset for older drivers. In addition, and as intended under the experimental design, the later alert timing (i.e., the steering timing) resulted in higher required deceleration at braking onset, and increased the tendency for drivers to choose a "brake only" response over a "steer only" response.

Figure 23. ***Maneuver Onset Reaction Times as a Function of Timing and Kinematic Condition***



Note: For the horizontal axes labeling, "C" corresponds to CAMP FCW alert timing trials and "S" corresponds to 85th percentile last-second steering timing trials.

<u>Figure 24</u>. *Mean Required Deceleration at Braking Onset as a Function of Age and Timing/Kinematic Condition*



<u>Note</u>: For the horizontal axes labeling, "C" corresponds to CAMP FCW alert timing trials and "S" corresponds to 85th percentile last-second steering timing trials.

Peak Longitudinal Decelerations Throughout Maneuver

Two separate, repeated measures ANOVAs were performed on the peak longitudinal deceleration measure. Note that since maneuver choice has a direct impact on peak longitudinal deceleration values, the analysis below was focused on “brake only” response data.

The first ANOVA focused on examining the effects of speed, lead vehicle deceleration profile, age, and gender on peak longitudinal decelerations. This analysis employed data from the following CAMP FCW alert timing/kinematic conditions: 30/0.15, 30/0.39, 60/0.15, and 60/0.39. The within-subjects variables were speed condition (30 and 60 mph) and lead vehicle deceleration profile (0.15 and 0.39 g’s), and the between-subjects variables were age (20-30 or 60-70 years old) and gender. There was a main effect of age ($F(1, 40) = 9.66$, $p < 0.01$), as well as Age x Lead Vehicle Deceleration Profile ($F(1, 40) = 17.19$, $p < 0.001$), Age x Speed x Lead Vehicle Deceleration Profile ($F(1, 40) = 13.84$, $p < 0.001$), and Gender x Speed x Lead Vehicle Deceleration Profile ($F(1, 40) = 24.37$, $p < 0.05$) interactions. (These effects will be discussed shortly.)

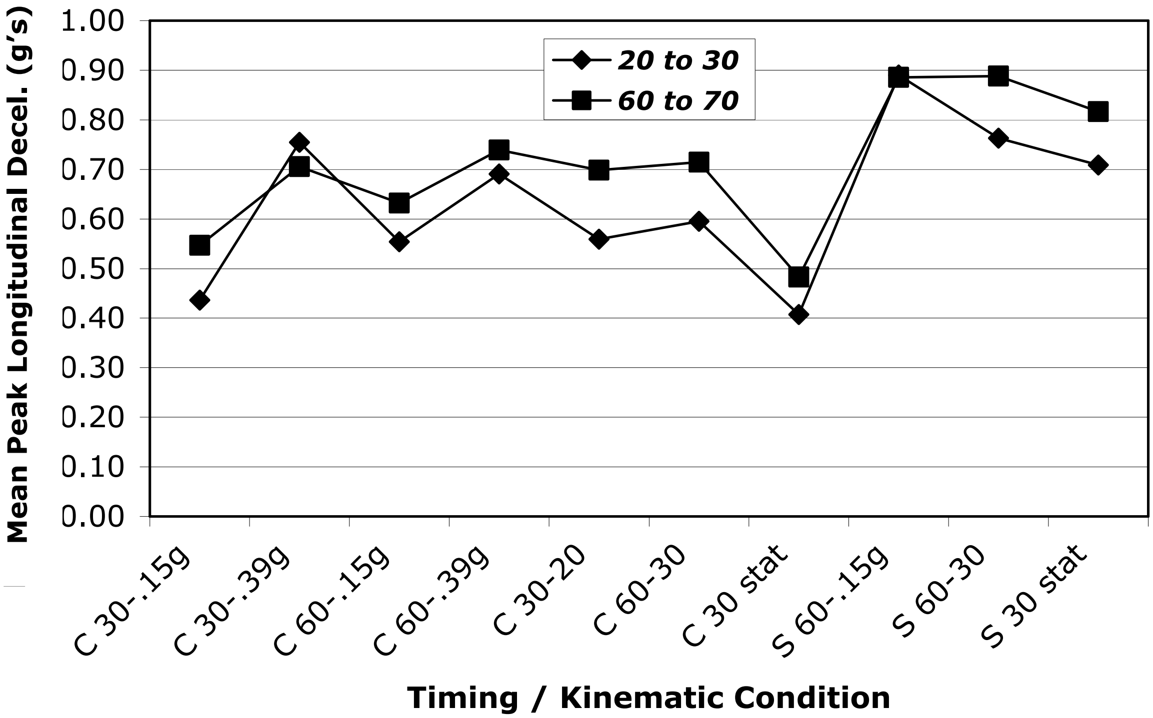
The second ANOVA focused on examining the effects of timing, kinematic condition, age, and gender on peak longitudinal decelerations. This analysis employed data from the following CAMP FCW alert timing/kinematic conditions: 60/0.15, 60/30, and 30/0. The within-subjects variables were timing (CAMP FCW alert and steering) and kinematic condition (60/0.15, 60/30, and 30/0), and the between-subjects variables were age (20-30 or 60-70 years old) and gender. Results indicated main effects of age ($F(1, 44) = 16.79$, $p < 0.01$), timing ($F(1, 20) = 408.58$, $p < 0.001$), and kinematic condition ($F(2, 19) = 74.69$, $p < 0.001$). In addition, there were Timing x Condition ($F(2, 19) = 9.42$, $p < 0.001$) and Age x Condition ($F(2, 19) = 5.02$, $p < 0.05$) interactions.

The effects from both of the ANOVAs described above primarily involve the age and timing/kinematic condition-related variables (i.e., speed, lead vehicle deceleration profile, kinematic condition, and timing). Hence, these effects can be largely illustrated in figure 25, which provides mean peak decelerations as a function of age and timing/kinematic condition. These results indicate that there was a robust effect of age, under which peak decelerations were higher for the older relative to the younger group. The only notable counterexamples to this age effect were for the CAMP FCW timing-30/.39 and steering timing-60/0.15 g timing condition combinations. Second, as expected, peak decelerations were higher in the steering relative to CAMP FCW timing conditions for matched kinematic conditions (60/0.15, 60/30, and 30/0).

Peak Lateral Accelerations Throughout Maneuver

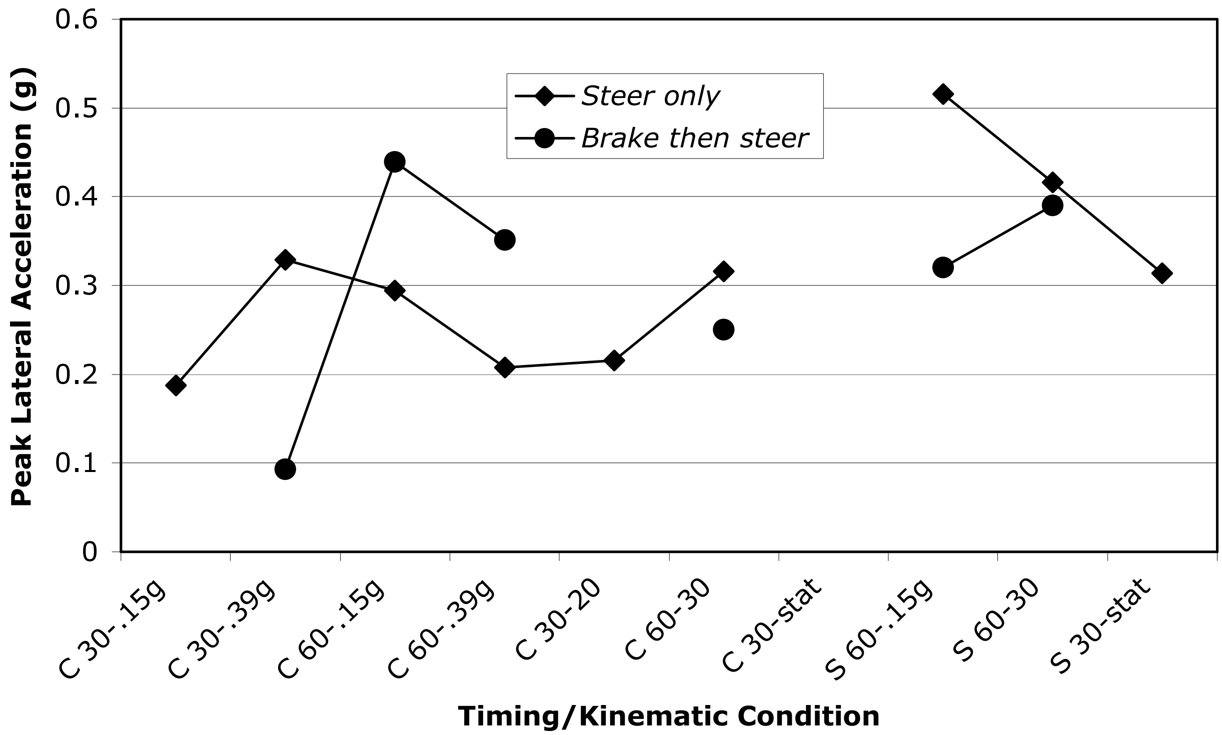
Due to the dominance of “brake only” maneuver choices, no statistical analyses were performed on the remaining “steer only” or “brake then steer” maneuver choice data. Hence, these data involving steering are considered as exploratory in nature. Figure 26 provides mean peak lateral accelerations as a function of maneuver choice (steer only, brake then steer) for each of the timing/kinematic conditions examined. For two of the three kinematic conditions, which were matched across CAMP FCW and steering timing trials (60/0.15, 60/30), peak lateral accelerations during steering only trials were higher with (later) steering timing relative to the CAMP FCW alert timing condition.

<u>Figure 25</u>. *Mean Peak Longitudinal Decelerations Throughout the Maneuver as a Function of Age and Timing/Kinematic Condition*



<u>Note</u>: For the horizontal axes labeling, "C" corresponds to CAMP FCW alert timing trials and "S" corresponds to 85th percentile last-second steering timing trials.

<u>Figure 26</u>. *Mean Peak Lateral Accelerations Throughout the Maneuver as a Function of Timing/Kinematic Condition*



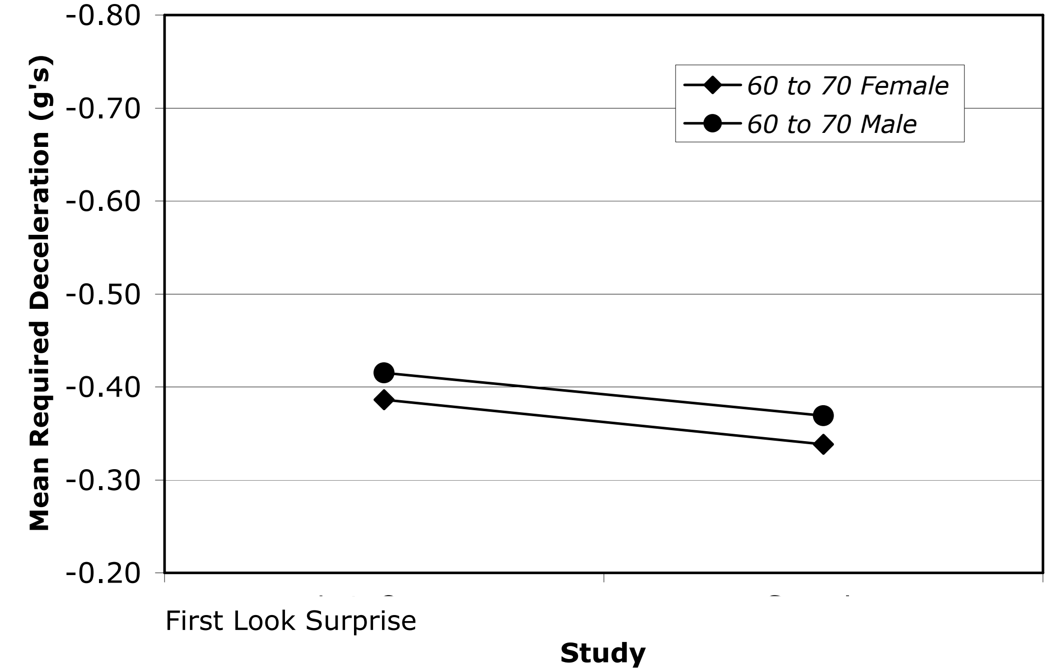
<u>Note</u>: For the horizontal axes labeling, "C" corresponds to CAMP FCW alert timing trials and "S" corresponds to 85th percentile last-second steering timing trials.

Comparison of First-Look and Surprise Trial Results

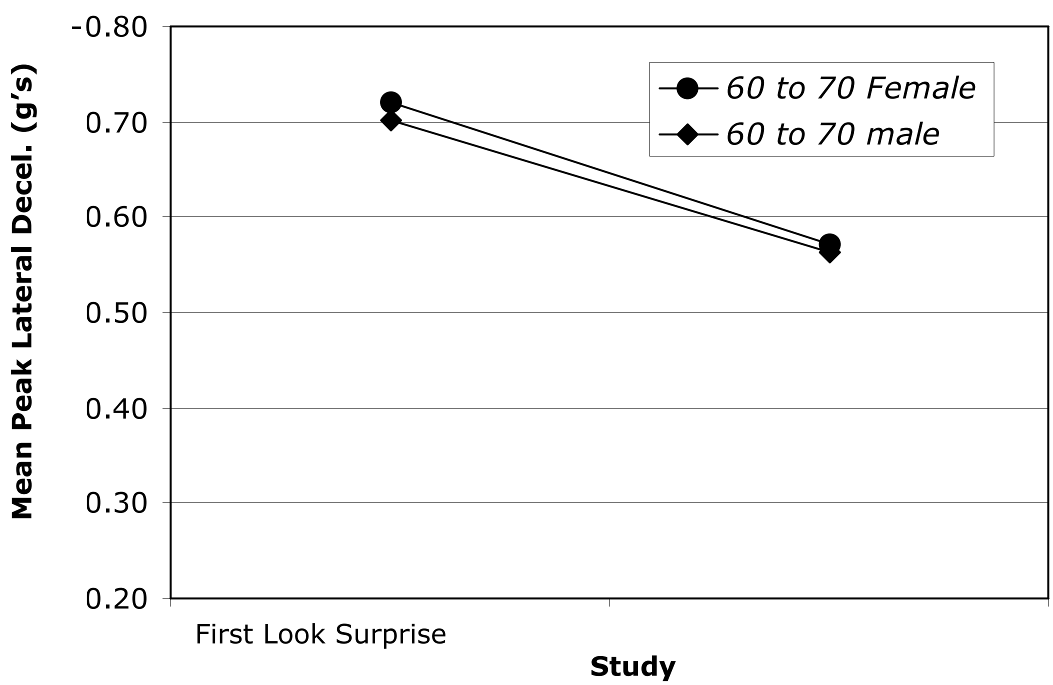
In order to determine the extent to which these results compare to those found under actual (rather than simulated) surprise trial conditions, "matched" data from the surprise trial study reported earlier were selected based on applying two filters (described below). Recall that the alert-look up delay used in the window opening timing employed in the first-look study for the CAMP FCW alert timing conditions was based on surprise trial data that met the following conditions at the time the alert was issued: the driver was performing the digit span dialing task, looking directly at the (center console-mounted) cellular phone, dialing the phone, and pressing the accelerator with their right foot. Hence, the first filter was applied to selected surprise trials that met these same conditions. Since the application of this filter resulted in trials which largely involved older drivers tested under daytime conditions with the 30/0.39 kinematic condition, these age, time of day, and kinematic conditions were used as a second filter. These two filters resulted in a set of 24 "matched" surprise trials.

These matched surprise trial data were compared to first-look data for older drivers under the 30/0.39 kinematic condition for both the required deceleration at braking onset and peak deceleration measures. Results for the required deceleration measure are shown in figure 27, which provided mean required decelerations as a function of age, gender, and methodology (or study). These results indicate that mean required decelerations were slightly higher for the first look relative to the matched surprise trial data set. Corresponding results for the peak deceleration measure are shown in figure 28, and indicate that peak decelerations were markedly higher in the first look relative to this matched surprise trial data set. Together, these results suggest that the first-look method represents a rather extreme form of driver distraction, and hence, this method may provide a conservative estimate of FCW alert effectiveness from a crash avoidance perspective.

<u>Figure 27</u>. *Mean Required Decelerations as a Function of Study and Gender*



<u>Figure 28</u>. *Mean Longitudinal Peak Decelerations Throughout the Maneuver as a Function of Study and Gender*



General Discussion

The predominance of unassisted, successful braking maneuvers in this study across the various CAMP FCW alert timing/kinematic conditions examined provides clear support for the crash avoidance effectiveness of this timing approach. The robustness of this approach was observed under conditions that attempted to increase the complexity of the driver's decision-making process. Together with the results from the previous TTC judgment study, these results suggest that drivers are capable of picking up and acting upon TTC information very rapidly, and that age and gender do not play a dominant role in last-second crash avoidance judgments and maneuvers. With respect to this latter point, it should be noted that older drivers did exhibit higher longitudinal peak decelerations. Although other interpretations are certainly plausible (e.g., older drivers are more risk aversive), this pattern of results coupled with an increased tendency of TTC overestimation for older drivers (observed in the TTC judgment study), suggests older drivers may brake harder to compensate for initial TTC overestimations. It is also worth noting that the time between the vision (or window) opening and the initiating of the crash avoidance maneuver (or maneuver onset RT) was faster under later timing (i.e., lower TTC) conditions, slower under high-speed conditions, and once again, relatively uninfluenced by age and gender.

A comparison of this data with matched surprise trial data (gathered in the first study reported here) suggests that the first-look method may provide a conservative test of FCW alert timing effectiveness, since peak decelerations and to a lesser extent required decelerations at braking onset were higher with the first look relative to surprise trial methodology. In addition, the current methodology, which provides an extreme form of driver distraction in that the driver has lost visual and/or cognitive contact with the lead vehicle, provides an efficient means of gathering surprise-trial like data. Hence, this method could be extremely useful for exploring the implications and trade-offs involved by employing later FCW alert timing (e.g., crash avoidance versus crash mitigation). Later FCW alert timing is advantageous from the standpoint of potentially reducing both in-path and out-of-path nuisance alerts. Nuisance alerts are likely to diminish the acceptability and credibility of the FCW system and the effectiveness of valid alerts signifying a true crash threat. Indeed, reducing the number of "cry wolf" false alarms drivers experience to a level that is acceptable to drivers while still maintaining adequate timing for valid alerts is a formidable challenge for FCW deployment and effectiveness.

From a methodological perspective, it is important to note that the number of interventions during CAMP FCW timing trials was highest in the 30/0.39 kinematic condition, which support the continued use of this vehicle-to-vehicle condition for surprise trial studying rear-end crash scenarios. This condition has been the dominant surprise trial kinematic condition not only for the current research, but also for previous CAMP FCW surprise trial research (Kiefer, et al., 1999). Furthermore, the current data also provide a useful tool for validating/calibrating data gathered under simulated (e.g., driving simulator or laboratory) approach conditions, which could potentially augment the current data set by exploring in-lane approaches that are not logistically possible to execute under test track conditions.

Final Conclusions and Recommendations

These results from the surprise, time-to-collision (TTC) judgment, and first-look trials continue to methodically build upon the foundation provided by previous CAMP FCW research efforts (Kiefer, et al., 1999, 2003). As in the previous CAMP FCW work, this work was conducted with a surrogate target, test track methodology, which allows driver behavior to be observed under real-approach, rear-end crash scenario conditions, and thereby increases the likelihood that the crash alert timing approach developed will generalize to real-world conditions (Kiefer, et al., 1999).

The major goals of this research were to continue to assess the robustness of the CAMP FCW alert timing approach, and to explore the use of visual occlusion techniques for enhancing understanding of a driver's decision making and avoidance maneuver behavior under rear-end crash conflict conditions. The major conclusions from this research are discussed below.

First, based on test driver intervention rates during surprise trials, the CAMP FCW alert timing approach, coupled with a single-stage, dual-modality (auditory plus visual) FCW alert was found to be robust, effective, and judged appropriate across a wide range of driver characteristic, environmental, interface design, distraction, kinematic, and training/false alarm conditions. Overall, surprise trial intervention rates in the FCW alert and no-FCW alert conditions were 6.8 percent and 13.2 percent, respectively. The former intervention rate may be reduced if drivers receive further "positive" training/experience with the FCW alert. (It should be noted that many drivers received no training or experience with the FCW alert). Alternatively, as was demonstrated, significant "negative" training/experience with the FCW alert can also degrade alert effectiveness. The results from the TTC and first-look studies (discussed in more detail below) also provide support for the appropriateness and robustness of the CAMP alert timing approach under a much wider range of vehicle-to-vehicle kinematic conditions than have been examined under surprise trial conditions.

Second, the benefits of the FCW alert during surprise trials (relative to no alert conditions) were restricted to tasks involving head-down glance activity, and were not evident for the eyes-forward distraction tasks examined. Indeed, all test driver interventions occurred when the driver was looking down at the phone at FCW alert onset. Hence, a promising means of improving the CAMP FCW alert timing approach appears to involve sensing driver eye movement location, and more precisely, sensing when the driver is looking down instead of looking forward at the scene ahead. An FCW alert timing modification based on driver eye movement location would not only improve alert timeliness for valid alerts issued when the driver is looking down (when the driver is unable to make TTC judgments), but just as importantly, such a modification would reduce the number of alerts perceived as unnecessary (or as "false alarms") by the driver because they were already looking at the forward scene and purportedly aware of the vehicle ahead. Although this research cannot address the relative merits of added driver eye movement location precision, these results suggest that even a coarse determination of eye-movement location (head-down or eyes-forward) may be extremely useful for improving the effectiveness and acceptance of an FCW alert system.

Third, results from the visual occlusion studies suggest that provided the driver is looking toward the lead vehicle (and hence, fully capable of making TTC judgments), the driver can quickly assess TTC and make the appropriate crash avoidance maneuver under CAMP FCW alert timing assumptions. Experimentally limiting viewing time (to one second) and/or introducing a concurrent (mental addition) distraction task, which were experimental manipulations explicitly intended to represent distracted driver conditions, did not have an adverse impact on drivers' TTC judgments. Overall, the TTC results provide support for the view that TTC estimations are primarily driven by an optic flow heuristic (at least under these low-TTC conditions), which may be modified based on speed and relative velocity conditions. Results also suggested that the probability of TTC overestimation, that is when perceived TTC exceeds actual TTC, increased as the relative velocity increased. This finding indicates another potential means of improving the CAMP FCW alert timing approach. More specifically, the CAMP FCW timing assumptions could be modified to increase the FCW alert range as delta velocity increased. Results from the first-look study indicated that under CAMP FCW alert timing conditions (which determined when drivers received their "first look" to the scene ahead) and with perhaps increased decision-making complexity relative to the surprise trials, drivers were able to execute an unassisted, successful braking maneuver for over 85 percent of the trials across the approach conditions examined. Overall, these TTC and first-look visual occlusion results may be particularly useful for validating/calibrating results found under simulated approach (i.e., laboratory or driving simulator) conditions, which have degraded visual scene properties shown to influence TTC judgments. This modification/alteration of TTC estimation under simulated approach conditions is of paramount importance for proper interpretation of simulated approach results particularly if the experimental goal is to assess alert effectiveness, since TTC appears to play a key role underlying drivers' perception of normal versus hard last-second braking envelopes under real approach conditions (Kiefer, et al., 2003), and hence, perception of crash threat.

Fourth, across all these various experimental approaches, there is generally a lack of both age and gender effects under actual FCW alert or simulated FCW alert (via visual occlusion) conditions. Indeed, this research suggests that the FCW alert information may be an effective means of equalizing (or neutralizing) drivers in their ability to avoid rear-end crashes. The general lack of age and gender effects across studies (and additionally the lack of viewing time and distraction effects on TTC judgments) suggests that drivers rather uniformly and consistently perceive and act upon low TTC conditions, perhaps due to the extreme salience of the lead vehicle looming behavior under these conditions. This also suggests that the process for perceiving TTC and acting upon TTC information is direct, efficient, and involves rapid automatic processing which is consistent with the optic flow heuristic method of TTC estimation described above. Finally, these results also suggest that a "one size fits all" FCW alert timing approach for closing alerts may be feasible.

Fifth, and finally, the first-look method appears to be a valid, efficient, and promising method for exploring the consequences of later FCW alert timing (e.g., crash avoidance versus crash mitigation). Later FCW alert timing may serve to reduce false alarms, and hence, increase the overall "credibility" and acceptability of the FCW alert system. Indeed, reducing the number of "cry wolf" false alarms drivers experience to a level that is acceptable to them, while still maintaining adequate timing for valid alerts, continues to be a formidable challenge for FCW deployment and effectiveness.

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