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How do drivers negotiate intersections with pedestrians? The importance of pedestrian time-to-arrival and visibility

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ABSTRACT

Forward collision warning (FCW) and autonomous emergency braking (AEB) systems are increasingly available and prevent or mitigate collisions by alerting the driver or autonomously braking the vehicle. Threat-assessment and decision-making algorithms for FCW and AEB aim to find the best compromise for safety by intervening at the “right” time: neither too early, potentially upsetting the driver, nor too late, possibly missing opportunities to avoid the collision.

Today, the extent to which activation times for FCW and AEB should depend on factors such as pedestrian speed and lane width is unknown. To guide the design of FCW and AEB intervention time, we employed a fractional factorial design, and determined how seven factors (crossing side, car speed, pedestrian speed, crossing angle, pedestrian size, zebra-crossing presence, and lane width) affect the driver’s response process and comfort zone when negotiating an intersection with a pedestrian. Ninety-four volunteers drove through an intersection in a fixed-base driving simulator, which was based on open-source software (OpenDS). Several parameters, including pedestrian time-to-arrival and driver response time, were calculated to describe the driver response process and define driver comfort boundaries.

Linear mixed-effect models showed that driver responses depended mainly on pedestrian time-to-arrival and visibility, whereas factors such as pedestrian size, zebra-crossing presence, and lane width did not significantly influence the driver response process. Drivers released the accelerator pedal in 99.8 % of the trials and braked in 89 % of the trials. Forty-six percent of the drivers changed their negotiation strategy (proportion of pedal braking to engine braking) to minimize driving effort over the course of the experiment. In fact, 51 % of the of the inexperienced drivers changed their response strategy whereas only 40 % of the experienced drivers did; nevertheless, all drivers behaved similarly, independent of driving experience. The flexible and customizable driving environment provided by OpenDS may be a viable platform for behavioural experiments in driving simulators.

Results from this study suggest that visibility and pedestrian time-to-arrival are the most important variables for defining the earliest acceptable FCW and AEB activations. Fractional factorial design effectively compared the influence of seven factors on driver behaviour within a single experiment; however, this design did not allow in-depth data analysis. In the future, OpenDS might become a standard platform, enabling crowdsourcing and favouring repeatability across studies in traffic safety. Finally, this study advises future design and evaluation procedures (e.g. new car assessment programs) for FCW and AEB by highlighting which factors deserve further investigation and which ones do not.

1. Introduction

Active safety systems are increasingly capable of supporting the driver in critical situations, improving crash avoidance by warning the driver or automatically braking or steering (Brannstrom et al., 2010). However, such interventions need to be timed not to be a nuisance to

the driver (Källhammer et al., 2014; Lees and Lee, 2008). Intervention time, the specific moment in which active safety warns the driver or intervenes, is a crucial design variable. In fact, if set too early, it may upset the driver and, if set too late, it may reduce the system mitigation efficiency (Helmer, 2014; Lubbe, 2015; Sander and Lubbe, 2016). For this reason, threat assessment (i.e., the evaluation of the potential

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danger in relation to the vehicle, the driver, and the environment) should take several factors into account to optimise decision making (i.e., when to issue a warning or take control of the vehicle through automated interventions) (Brännström et al., 2014). Identifying which factors may influence acceptance is critical to the development and evaluation of active safety warnings and interventions (SAE, 2017; Schram et al., 2013, 2015).

Since January 2016, Euro NCAP has been testing automated emergency braking systems (AEB) with crossing pedestrians; more tests for AEB and forward collision warning (FCW) systems for pedestrians and cyclists were implemented in 2018. During the testing, if the intervention times are too short, FCW and AEB may not show significant safety benefits; if too long, then very large safety benefits would be declared at the expense of unrealistic assumptions about driver acceptance. Therefore, it is important for Euro NCAP to set realistic thresholds on intervention timing that are not only consistent and standard but also representative of the factors that may influence system acceptance.

Driver acceptance depends more on the perceived usefulness of an intervention than on its actual usefulness; in other words, a false warning may still be a positive experience if the driver feels it has been useful, whereas a true warning could still be a nuisance if a driver feels that it was not useful (Källhammer et al., 2014). Driver discomfort (Engstrom and Aust, 2011; Summala, 2007) could help draw the line between what is perceived as a useful warning (or intervention) and what is not by relating drivers' responses to their comfort boundaries (Ljung Aust and Engström, 2011). It has been suggested that warnings and interventions outside the driver comfort zone (the subjectively defined region in driver-vehicle-environment state space where the driver experiences no discomfort (Lee, 2006; Vaa, 2014)) are intrinsically more acceptable; empirical evidence comes from different studies such as the one from Puente-Guillen and Gohl (2019), who showed that warnings outside the comfort zone are less accepted by drivers. In fact, warnings and interventions outside the comfort zone typically happen after a missed or delayed response by drivers, which causes them to exceed their comfort zone boundary. By acting after the usual driver response time (Green, 2000; Summala, 2000), an active safety system may prove to the driver that it acted on a mismatch between the traffic situation and the usual driving performance, thus potentially increasing acceptance (Ljung Aust and Dombrovskis, 2013). However, for a system to tune its actions to driver comfort boundaries and responses, it must be aware of the factors influencing the driver response process (how the driver perceives, processes, and acts upon information from the vehicle and the environment over time (Morando et al., 2016)). Although some of these factors, such as speed or proximity to objects, may be obvious because they shape the field of safe travel (Gibson and Crooks, 1938), other factors may be less so, because they depend on human perception as opposed to vehicle dynamics and kinematics. In conclusion, to increase acceptance, system warning and intervention times must be designed to depend on the same factors that influence driver comfort zone and, intrinsically, driver response.

While naturalistic driving data may eventually provide an exhaustive explanation of the factors which influence comfort zones and driver responses (Dozza, 2012; Engstrom and Aust, 2011), these datasets are currently confounded, complex, and limited in size; it would be impossible to disentangle single factors and link them to the intervention time of active safety systems in different driving situations, and in crossing scenarios in particular. Further, the current market penetration for AEB systems capable of preventing collisions with pedestrians is too low to provide enough data for statistically sound results or to enable retrospective studies (Ohlin et al., 2017). Driving-simulator and test-track studies, because they are controlled and repeatable, are useful for evaluating active safety interventions (Chrysler et al., 2015). For instance, Lubbe and Davidsson (2015) used the Toyota high-fidelity simulator to test the extent to which time-to-collision (TTC) at response time was influenced by pedestrian speed when a driver negotiated a

road with a crossing pedestrian. The effect of car speed in a similar scenario was also tested in a test-track experiment (Lubbe and Rosen, 2014). Both studies showed a significant influence of pedestrian speed, and a non-significant influence of a limited range of car speeds, on driver response (i.e., the actions taken by the driver to safely negotiate the intersection, and their timing). However, car speed was hypothesised to have an influence, albeit in a wider range of car speeds than was tested. Both car speed and pedestrian speed should therefore be taken into account in order to increase acceptance of FCW and AEB interventions.

It is currently unknown to what extent factors other than pedestrian and car speeds may influence driver response and comfort boundaries when negotiating an intersection with a pedestrian. In particular, factors such as pedestrian conspicuity (Tanaka and Teraoka, 2014), pedestrian trajectory (Tanaka and Teraoka, 2014; Varhelyi, 1998), lane width (Shawky et al., 2014), and zebra-crossing presence (Varhelyi, 1998) have been suggested to influence driver response. In addition, Seiniger et al. (2014) suggest including factors such as crossing side and overlap when testing AEB, and distinguishing between adult and child pedestrians. Non-verbal communication between road users is also essential for infrastructure negotiation (Björklund and Åberg, 2005; Kitazaki and Myhre, 2015), although it is hard to sense for active safety systems. Finally, factors related to the drivers themselves, such as driving experience, may also influence driver response (Summala et al., 1998). Testing the potential influence of so many factors on driver comfort zone would, however, require many studies. Fractional factorial design (FFD) (Box et al., 2005) is one of the advanced statistical methods often used to minimise the number of tests while maximising the number of factors tested (Massumi et al., 2002; Nejad et al., 2010; Poorna and Kulkarni, 1995). This method has been used in the field of traffic safety (Machado-León et al., 2016; Wing-Gun and Ka-Hung, 1999), particularly in driving simulator experiments (Belz et al., 1998; de Ruyter, 2016), possibly because the design requires repeated exposures to similar situations. Notably, these repetitions may favour expectancy and adaptive behaviours (Engstrom and Aust, 2011).

This study employed an open-source driving simulator and an FFD to determine the extent to which seven different factors (namely, car speed, pedestrian speed, pedestrian size, crossing angle, crossing side, zebra-crossing presence, and lane width) may affect the comfort zone of drivers (and therefore acceptance to FCW and AEB) when negotiating an intersection with a pedestrian. Empirical evidence that these factors may influence driver behaviour came from a number of studies (Seiniger et al., 2014; Shawky et al., 2014; Tanaka and Teraoka, 2014; Varhelyi, 1998). From the literature on comfort zone and driver behaviour, this study hypothesised that drivers would react earlier when: speeds are higher, because of higher urgency (Green, 2000; Wang et al., 2016); the pedestrian is a child, or comes from the near side, or crosses the road perpendicularly (because of the larger risk (Wilde, 1982)); a zebra crossing is present; or the lane width is narrow (because of the more stringent constraints on the field of safe travel and the traffic rules that may have triggered a more precautionary and self-regulating behaviour; (Dozza et al., 2015; Norris, 1997)). This study also compared the response process between participants who often drove a car and participants who seldom drove to devise a model that can improve the design and evaluation of active safety systems that take driving experience into account.

2. Methodology

2.1. Participant recruitment, selection, and demographics

Selection criteria for the participants included having a valid driver's license and being between 22 and 60 years old. Before the experiment, all participants signed a consent form and were informed about their rights in accordance with the ethical application for this study (Dn:146-16). Participants were naive to the driving simulator and

were instructed to behave as they normally would in traffic, as if they were driving back from work. Before the experiment, all participants practiced in the driving simulator until they felt confident that they could master its controls (steering wheel, gas pedal, and brake pedal). During the practice, participants were asked to drive as close as possible to a brick wall (and, eventually, to hit it). This test was intended to confirm that the participants could estimate distances correctly in the virtual environment. In fact, in driving simulators distances are notoriously hard to estimate, especially without any practice (Baumberger et al., 2005).

2.2. Data collection: OpenDS set up and experimental protocol

In this study, the participants were asked to drive in a driving simulator and negotiate a virtual scene of an urban intersection with a pedestrian. The simulated road environment was built using open source software. The pedestrian models were created using the application MakeHuman¹ (Fig. 1). The models were manipulated and further developed using Blender² (3D-modelling software). Blender was also used to create the environment, which included streets, buildings, and trees, to make the urban intersection as realistic as possible. This modelling effort is not trivial and more information can be found in Jaber and Thalya 2020. The environment and pedestrian models were then combined into complete driving tasks in MATLAB and exported to OpenDS³ (version 3.5) to run the experiment. OpenDS is an open-source driving simulator based on jMonkeyEngine (jME)⁴. jME is a Java-based game development tool. For the simulator experiment, different driving conditions were loaded into the OpenDS driving simulator. Each driving condition defined the simulation environment (including pedestrian appearance and behaviour) in which the participant drove.

OpenDS was used along with the basic set-up of Logitech G27 steering wheel and pedals. A Windows 7 Enterprise computer with an Intel Core i7-3770 3.40 GHz processor, NVIDIA GeForce GT640 graphic card and 16 GB RAM rendered the simulation, visualised with a Hitachi CP X1 LCD projector (1024 × 768 resolution). A Python script automatically loaded new driving tasks as the experiment progressed to prevent human errors (manually loading the wrong driving task).

In each task, the participants accelerated a car from a standstill on a straight road either up to 20 or 60 km/h (depending on the experimental conditions, which were modelled on Euro NCAP scenarios), then approached an intersection where a pedestrian crossed their path; the intersection was 130 m away from the starting point. A video from a representative run is linked below⁵. A speed limiter made sure the driver never exceeded the set speed for each trial. Seven binary factors were manipulated in this experiment (Table 1). These seven factors were selected based on evidence from the literature (Seiniger et al., 2014; Shawky et al., 2014; Tanaka and Teraoka, 2014; Varhelyi, 1998), which indicated that they might have influenced driver response. Each factor was assigned one of two values (low or high) according to their expected effect on comfort boundaries suggested from the literature. In other words, we hypothesised that when the factors were assigned the higher values, participants would show smaller comfort zone boundaries. To test all seven factors within one hour, thus minimising spurious effects from fatigue and boredom (J. D. Lee, 2017), we used an FFD. This statistical tool has many applications, such as optimisation problems in chemistry (Massumi et al., 2002; Nejad et al., 2010) and

medicine (Poorna and Kulkarni, 1995). FFD excludes repetitious combinations of factors, reducing the number of trials necessary to understand the effects of factors with a fixed number of levels (Box et al., 2005). The full fractional design in our study would have required 128 different combinations for seven factors; with FFD only 32 were needed. An FFD 2^{7-2} resolution IV (Box et al., 2005) was implemented in this study to assess the influence of the seven factors. The drawback of the FFD is that not all possible interactions among factors can be statistically tested and, although FFD tests the individual effect of each factor, some interactions may be confounded. We accepted this compromise because it greatly reduced the testing time. The order of the 32 tasks was randomised for each participant to control for order effect.

In addition, a fixed task—in which the car speed was 20 km/h, the pedestrian was an adult with a speed of 1 m/s who entered the intersection from the near side at a 90° angle at a zebra crossing, and the road was three meters wide—was repeated four times during the experiment to monitor the potential order effect (participants learning or adapting to the environment). It is worth noting that crossing side necessarily changed the time at which the pedestrian was visible: the pedestrian became visible four seconds prior to the potential collision for the near-side condition and eight seconds for the far-side condition. (The other factors had no effect on this time interval.) Four seconds of visibility was enough time to ensure that the drivers did not need to panic brake, and eight seconds were necessary for the pedestrians to cover the extra distance to the collision point.

The experimental design also included three additional tasks similar to those of previous studies (Lubbe and Davidsson, 2015; Lubbe and Rosen, 2014) for comparison purposes. In the first, the car speed was 30 km/h, the pedestrian speed was 1 m/s, and the pedestrian was an adult crossing the car path orthogonally; there was no zebra-crossing and the pedestrian came from the near side and crossed a 3-m road. The second task was identical, except the pedestrian's speed was 2 m/s. In the third task, the car speed was 50 km/h, the pedestrian speed was 1 m/s and all other factors were the same as in the other two. Thus, each participant performed 39 tasks during the experiment with a short break of 5–10 min after 19 tasks (see Fig. 2).

Participants were instructed to drive as they normally would while travelling home from work. After the experiment, the participants filled in a questionnaire about their simulator experience. This information was used to compare the behaviour of experienced drivers with less experienced drivers. Additionally, a negative response to the question, “Did you react like you normally would in a real traffic situation?”, was an exclusion criterion.

2.3. Data analysis: measures, response analysis, statistics

The positions of the car and the pedestrian, as well as the controls (steering wheel and pedals), were recorded in each trial. Time stamps for the start of gas pedal release and its full release were extracted from the gas pedal position. The time stamp for brake onset was extracted from the brake pedal position. These time stamps were used to describe the participant response process. Comfort boundaries were quantified with five metrics: TTC (SAE, 2015; option B; pg. 54), lateral and longitudinal distances to the collision point (from the car position), minimum TTC (mTTC; SAE, 2015; option B pg. 56), and time-to-arrival at intersection centreline (TTA). (TTA was defined as the time necessary for the pedestrian to reach the centre of the intersection.) All metrics have been used in previous studies (Lubbe and Davidsson, 2015; Lubbe and Rosen, 2014) except mTTC. This basic metric for threat assessment in longitudinal support systems was added as a general indicator of criticality of each trial (Van der Horst, 1990). It is worth noticing that mTTC can only have a single value for each trial. All other metrics, on the other hand, are continuous time series, and the value to be included in the analysis depends on the definition of response time. In fact, each of these four metrics was calculated using the three different actions representing different aspects of the response process: the start of gas

¹ MakeHuman Open Source tool for making 3D characters. Available: <http://www.makehumancommunity.org/> (visited on 2020-02-20).

² Blender. Available: <https://www.blender.org> (visited on 2017-02-08)

³ OpenDS the flexible open source driving simulator. Available: <http://www.opensds.eu> (visited on 2017-02-08)

⁴ jMonkeyEngine, a cross-platform game engine for adventurous Java developers. Available: <http://jmonkeyengine.org> (visited on 2017-02-20).

⁵ <https://youtu.be/G-0f5Q4ilt4> & https://youtu.be/at3_vrpIF6E (factors are indicated in the video descriptions).

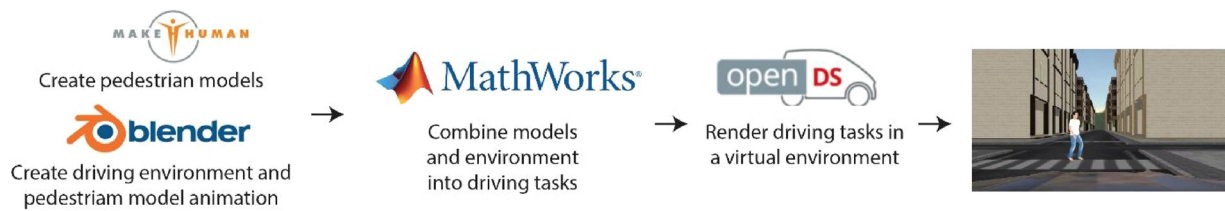


Fig. 1. Schematic representation of the software used to create the driving simulator and run the experiment.

Table 1
Factors investigated.

Factors	Low level	High level
Crossing side	Far side	Near side
Car speed	20 km/h	60 km/h
Pedestrian speed	1 m/s	2 m/s
Crossing angle	90°	45°
Pedestrian size	Adult (176 cm)	Child (115.2 cm)
Zebra-crossing presence	Yes	No
Lane width	3 m	2.5 m

Morando et al., 2020). Two-tail *t*-tests ($\alpha = 0.05$) compared the individual results of Trials 1, 3, and 4 to results from previous studies (Lubbe and Davidsson, 2015)(Lubbe and Rosen, 2014). Effect size for *t*-tests was computed according to Cohen (1998).

3. Results

Of the 103 volunteers who signed up for the study, 94 participated, and 88 provided data for the analysis. (Two individuals were unable to complete the experiment due to simulator sickness, and four were ex-

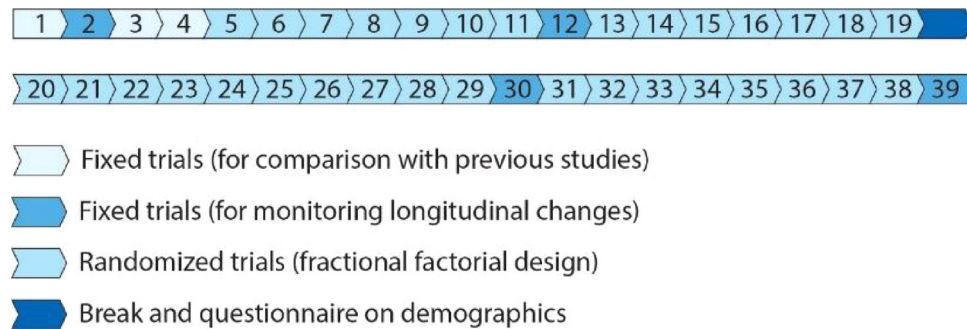


Fig. 2. Experimental protocol.

pedal release (gRT; SAE, 2015, 7.2.1), full gas pedal release (rRT; SAE, 2015, 7.2.4), and the start of braking (bRT; SAE, 2015, 7.2.2). For TTA, it is particularly important to verify the extent to which drivers may have based their response on the field of safe travel of the pedestrian instead of on their own (Gibson and Crook, 1938), because this has important implications for the design of active safety. It is worth noticing that TTA is similar to TTC, as defined by the traffic conflict technique (Laureshyn et al., 2010) and used by Várhelyi (Várhelyi, 1998). However, TTA does not take into account the vehicle's width. A linear regression model with mixed effects quantified (and statistically verified) the effects of the seven factors on the five metrics. The model fit was verified by calculating the coefficient of determination (R^2). Two-tail paired *t*-tests ($\alpha = 0.05$) compared the results of Trials 2 and 39 to assess behavioural adaptation (among the four fixed trials, we expected the most change between Trials 2 and 39 because they were the farthest apart in time). By behavioural adaptation we refer to the evidence that, during an experiment and especially when the experimental protocol entails the repetition of the same task, the participants may change their behaviour over time because of motor learning or of the increasing level of automation in the response processes (Engström et al., 2010;

cluded because they stated that they had not behaved in the simulator as they would in real traffic.) Trials in which the participants crashed (10) or did not reach top speed (75) were excluded from data analysis.

The age range for the 88 participants retained in the study was 22–59 years old ($M = 33.8$, $SD = 10.8$). Of all participants, 19.3 % were females (age 25–51, $M = 32.5$, and $SD = 9.1$). Based on their driving frequency, the participants were divided into two groups, experienced drivers and inexperienced drivers. Experienced drivers drove three to seven days per week (48 participants: age 25–59 year, $M = 37.8$, $SD = 11.5$). Inexperienced drivers drove once or less than once per week (40 participants: age 22–52 year, $M = 29.0$, $SD = 7.6$).

A total of 3171 trials were analysed to investigate the following: 1) the driver response process, 2) behavioural adaptation, 3) the effect of the seven factors on the response metrics, and 4) comparability with results from previous studies.

3.1. Response process

During the experiment, the participants accelerated the vehicle, reached top speed (either 20 or 60 km/h), and then started releasing the

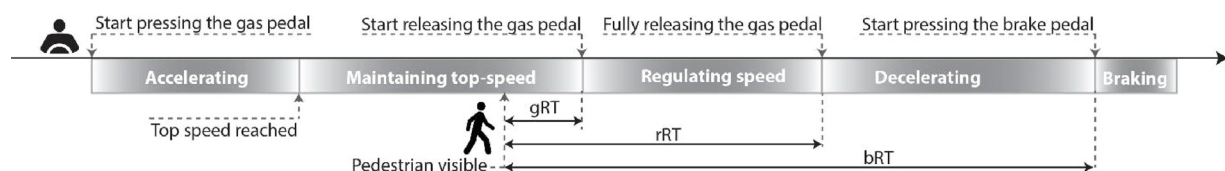


Fig. 3. Response process during the experiment. Response times are calculated from the moment the pedestrian becomes visible to 1) start of gas pedal release (gRT), 2) full gas pedal release (rRT), and 3) start of braking (bRT).

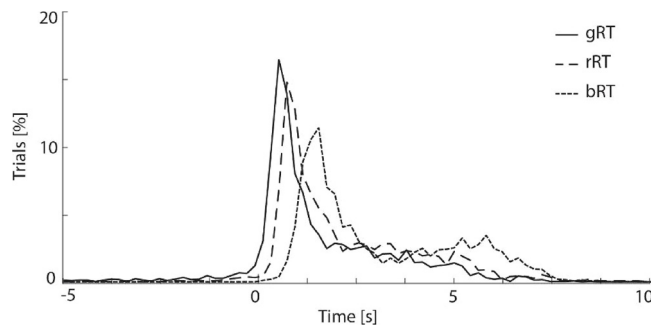


Fig. 4. Distribution of the response time at different points in the response process. The pedestrian became visible at 0 s.

gas pedal. Eventually, they fully released the gas pedal and started slowing down (in most cases by braking), to negotiate the intersection with the pedestrian (Fig. 3). Because it was possible for the drivers to decrease speed by simply releasing the gas pedal, braking was not necessary to avoid a collision with the pedestrian and, in fact, the brake pedal was used in only 89 % of the trials. Nevertheless, in all trials considered for analysis, drivers reached the top speed and initiated the response process by releasing the gas pedal. In 99.8 % of the trials the drivers also reached full gas pedal release. Fig. 3 shows how the gRT, rRT, and bRT were calculated from the point in time when the pedestrian was visible. Fig. 4 compares their distributions from the experimental data. gRT, rRT, and bRT were shorter for experienced participants (by approximately 0.4, 0.2, and 0.05 s, respectively); these results were statistically significant for gRT and rRT ($p < 0.05$ from t -test; effect sizes were 0.19 and 0.14, respectively).

3.2. Behavioural adaptation

Paired t -test verified that gRT did not significantly change between the first and the last repetition of the fixed trials (Trial 2 vs. Trial 39; Fig. 2) for both experienced and inexperienced drivers. However, rRT and bRT increased across the same trials (by 0.82 s and 0.90 s, respectively); this result was statistically significant only for the experienced group ($p < 0.05$; effect sizes were 0.43 and 0.89, respectively). As for braking behaviour, 53 % of the participants showed the same response strategy (Fig. 3) in both trials, 7 % of the participants braked in Trial 39 but not Trial 2, and 40 % braked in Trial 2 but not Trial 39 (Fig. 2). Interestingly, a larger proportion of the inexperienced drivers (60 %) kept the same strategy across the whole experiment compared to the experienced drivers (49 %).

3.3. Factor analysis

Although experienced drivers reacted slightly earlier than inexperienced drivers and showed a somewhat larger behavioural adaptation, the two groups were combined for the factor analysis after verifying that individual groups' responses were affected to a similar extent by the same factors. Because of the large sample size, most of the factors and interactions in the linear mixed-effect models were statistically significant; to help the reader appreciate effect size, which becomes more important than statistical significance itself because of the large dataset, Fig. 5 reports the percentage effect of each factor in relation to the intercept of the linear model. All linear effect models scored an R^2 between 0.82 and 0.94 in the goodness-of-fit test. Results from the models were similar for gRT and bRT. For simplicity, we will now focus on the gRT and bRT results, because rRT results are similar to gRT.

Fig. 5 shows the effect of the seven factors on the five metrics for gRT and bRT. Crossing side was the predominant factor showing the largest influence on all metrics, independent of the definition of response time. Car speed had a significant effect (i.e., its effect in relation

to the intercept of the linear model was larger than 5 %) on all metrics but TTA and lateral distance. Crossing angle had a significant effect on TTC and lateral and longitudinal distances. Pedestrian speed had a significant effect only on lateral position (Fig. 5). There was no significant effect from the 2-factor interactions, except for sporadic exceptions where the effect was as high as 7.8 % of the intercept value and crossing side was one of the interacting factors. Interestingly, TTA was only affected by crossing side. Because crossing side had such a large effect independent of metrics and response time definition, we took a closer look at how the response process was affected by near- and far-side crossing. Fig. 5 makes possible a comparison across all factors on all metrics for both gRT and bRT, which was the main aim of this paper. A drawback is that Fig. 5 does not include the models' details, such as the actual effect sizes and p . For completeness we report more details from the model in Table 2.

Because the influence of crossing side was surprisingly evident in Fig. 5, we performed some more analyses to better understand why this factor had such a large effect compared to the others. Fig. 6 shows that when the pedestrian crossed from the far side, the distribution of the response time (gRT, rRT, and bRT) spread and shifted, exhibiting a larger variability and mean value than when the pedestrian crossed from the near side.

3.4. Results of comparison to previous studies

For direct comparison with Lubbe's work (Lubbe and Davidsson, 2015; Lubbe and Rosen, 2014), the TTCs at the start of braking for Trials 1, 3, and 4 are shown in Fig. 7 as cumulative distributions. The results of the t -tests proved that, as car speed increased, TTC significantly ($p < 0.05$) decreased (by 0.3 s on average; effect size was 0.53). However, pedestrian speed did not significantly influence TTC.

4. Discussion

4.1. Design of active safety systems

Using an open-source driving simulator and applying FFD, this paper investigated the driver's response process when negotiating an intersection with a pedestrian. The point in time when the pedestrian first became visible proved to be crucial for explaining how different factors may influence the response process. Pedestrian speed also proved to be important; in fact, TTA, the relation between pedestrian distance to the car's centre line and pedestrian speed, offered a general description of how six of the seven factors could influence driver responses. Analysis of the seventh factor, i.e. whether the pedestrian crossed from the near or far side, showed a relationship between response variability and proximity to collision. In the latter condition, the distribution of driver responses appeared to be lognormal. This finding is consistent with previous literature showing that a lognormal distribution fits driver response time well (Dozza, 2012; Green, 2000; Summala, 2000). Although the time difference between the two conditions was only four seconds, the distribution of driver responses changed substantially (Fig. 6); in the far-side condition the distribution became more spread out and no longer resembled a lognormal distribution. Supporting this result, previous research has reported that the later a pedestrian becomes visible, the smaller the variability in driver response (Olson and Sivak, 1986), and that, as a situation becomes more critical, response variance decreases (Engström et al., 2010). In contrast, in the far-side condition, drivers had more time to negotiate the intersection, so they could rely on a different strategy—speed modulation with the gas pedal instead of braking—to safely negotiate the intersection; hence, the response process became more complex, including more frequent control and speed adjustments, possibly exerted in a satisficing fashion (Summala, 2007). In any case, this study's TTA results suggest that, when negotiating an intersection with a pedestrian, driver comfort boundaries might depend more on the

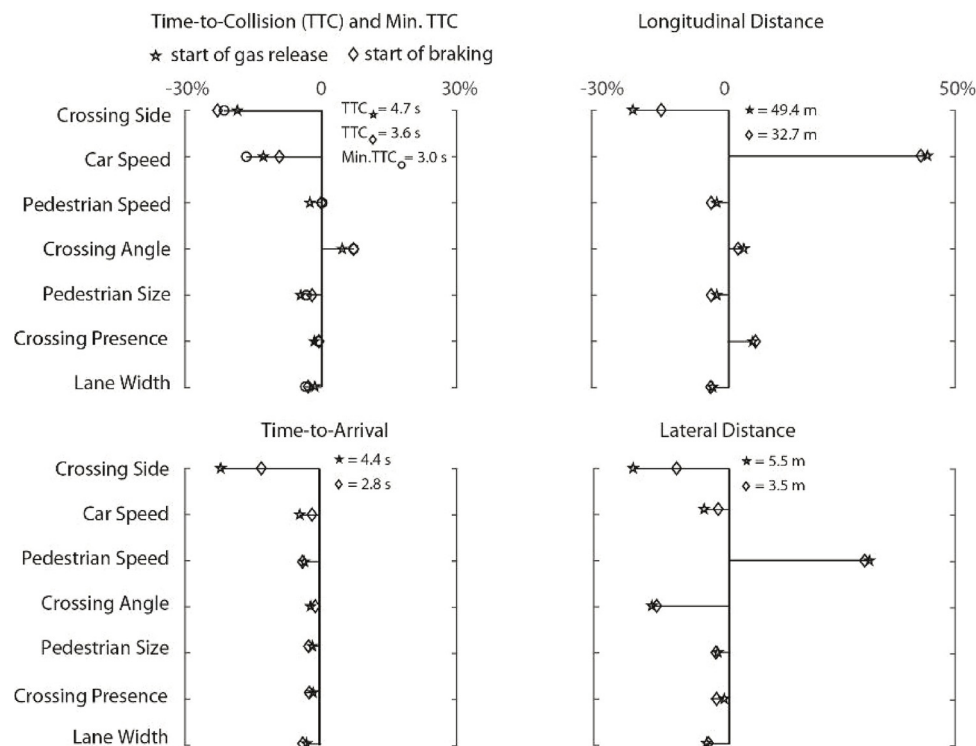


Fig. 5. Main factor effects from mixed effect models presented as percentage of the intercepts. Intercepts are also reported for start of gas pedal release and start of braking.

Table 2

Linear mixed-effect models coefficients. Estimates for time-to-collision (TTC), longitudinal distance, lateral distance and time-to-arrival (TTA) are reported as well as standard error (SE) and p-values (* indicates $p < 0.01$ and ** $p < 0.001$).

	TTC gas release			TTC start braking			min TTC			Long dist. gas release			Long dist. start braking		
	Estimate	SE	p	Estimate	SE	p	Estimate	SE	p	Estimate	SE	p	Estimate	SE	p
Intercept	4.90	0.106	**	3.64	0.083	**	3.01	0.078	**	52.00	1.140	**	32.10	0.764	**
Crossing side	-0.98	0.026	**	-0.81	0.019	**	-0.56	0.017	**	-9.44	0.285	**	-4.81	0.170	**
Car speed	-0.28	0.026	**	-0.61	0.019	**	-0.35	0.017	**	23.60	0.287	**	14.00	0.171	**
Pedestrian speed	0.00	0.026	0.98	0.01	0.018	0.499	-0.08	0.017	**	-0.85	0.285	*	-0.32	0.169	0.06
Crossing angle	-0.31	0.026	**	-0.25	0.018	**	-0.12	0.017	**	-2.78	0.285	**	-1.96	0.167	**
Pedestrian size	0.12	0.026	**	0.13	0.018	**	0.15	0.017	**	1.30	0.285	**	1.21	0.167	**
Crossing presence	-0.04	0.026	0.14	-0.05	0.018	**	-0.02	0.017	0.23	-1.16	0.285	**	-0.72	0.167	**
Lane width	0.14	0.026	**	0.13	0.018	**	0.05	0.017	**	1.62	0.285	**	1.24	0.169	**

	TTA at gas release			TTA at braking			Lat. dist. at gas release			Lat. dist. at stat braking		
	Estimate	SE	p	Estimate	SE	p	Estimate	SE	p	Estimate	SE	p
Intercept	4.68	0.101	**	2.79	0.068	**	5.86	0.121	**	3.53	0.085	**
Crossing side	-0.78	0.025	**	-0.34	0.015	**	-1.02	0.029	**	-0.41	0.021	**
Car speed	-0.09	0.025	**	-0.04	0.015	*	-0.19	0.029	**	-0.08	0.021	**
Pedestrian speed	-0.25	0.025	**	-0.09	0.015	**	1.67	0.029	**	1.08	0.021	**
Crossing angle	-0.11	0.025	**	-0.02	0.014	0.10	0.90	0.029	**	0.58	0.020	**
Pedestrian size	0.12	0.025	**	0.08	0.014	**	0.18	0.029	**	0.12	0.020	**
Crossing presence	-0.07	0.025	*	-0.03	0.015	0.02	-0.14	0.029	**	-0.04	0.020	0.03
Lane width	0.12	0.025	**	0.11	0.015	**	0.22	0.029	**	0.17	0.021	**

pedestrians' field of safe travel (Gibson and Crooks, 1938) than on the drivers'.

This study did not replicate the results from Lubbe et al. (Lubbe and Davidsson, 2015; Lubbe and Rosen, 2014), namely that there was no significant effect of car speed (Lubbe and Rosen, 2014) and there was a small but significant effect of pedestrian speed (Lubbe and Davidsson, 2015) on TTC. Nevertheless, our study does not disprove the hypothesis that pedestrian speed and car speed are important factors for the design of FCW and AEB. Response time has been calculated in several ways across studies (Green, 2000); studies testing warnings (Engström et al.,

2010; Lubbe, 2017) typically calculated it as the time from the warning to the start of braking. However, when warnings are not available, other starting cues, such as a precipitating event, have been used (Dozza, 2012). When braking was not part of the reaction, other responses, such as hovering over the brake pedal, have been adopted (Morando et al., 2016). In practice, the driver actions making up the response process vary depending on the context, constraining the definition of response time to the timing of context-specific actions (Markkula et al., 2016). This study calculated the response time at three different steps of the response process and compared the results to find

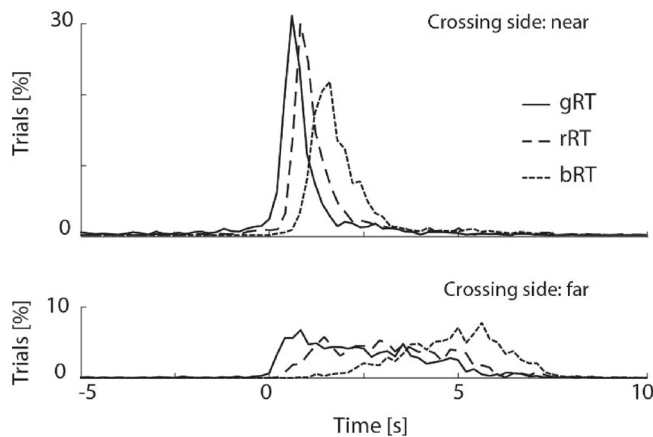


Fig. 6. Distribution of the response time at different points in the response process for near-side (top) and far-side crossing (bottom). The pedestrian became visible at 0 s.

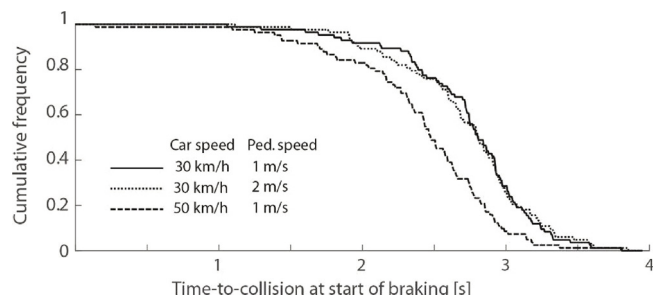


Fig. 7. Cumulative distributions of time-to-collision for comparison to previous studies.

that, although times can differ greatly (from a median 2.3 s for gRT to 0.9 s for bRT) across the steps of the response process, their relation to the seven factors under analysis was similar. This similarity shows that different steps of the same response process may be highly correlated, possibly as a consequence of driver expectancy due to trial repetition. In fact, in 7 % of the trials, participants reacted early, before the pedestrian was visible (Fig. 4), clearly showing that task repetition created expectancy. Further, most early responses occurred in near-side entry trials (Fig. 6). Nevertheless, when we ran the factorial analysis without the trials with anticipated responses, we found the same pattern of influence of the seven factors on the five metrics, confirming that adaptation alone cannot explain our results. Finally, late responses are often more informative than early responses for the design and evaluation of interventions (Lubbe and Rosen, 2014).

A standard way to quantify driver comfort boundaries is still lacking in traffic safety research; in an effort to define the safety zone and find a surrogate for “distance from crash” (Summala, 2007), metrics such as TTC (Lee, 1976; Lubbe and Rosen, 2014; Van der Horst, 1990), mTTC, and time-to-lane-crossing (Godthelp et al., 1984; Mulder et al., 2008) have been used. In this study, we compared more traditional metrics, TTC and mTTC, with heuristics (longitudinal and lateral distances) and a metric—TTA—that took the pedestrian’s field of safe travel into account (because it measured the time-to-arrival at the intersection from the pedestrian’s point of view). We found very little difference in the results between TTC, mTTC, and longitudinal distance. Further, simple geometry alone may explain the slightly different effects on lateral distance. Thus, although standardised comfort boundaries would improve inter-study repeatability and comparison, the choice of the specific metric to quantify longitudinal comfort boundaries does not seem that crucial, at least in the context investigated in this experiment. Interestingly, the metric based on pedestrian safety (TTA) was the one which described the driver responses in the simplest way, suggesting

that, when negotiating an intersection with a pedestrian, drivers may project their own comfort boundaries on the pedestrians, prioritizing the pedestrian’s field of safe travel over their own. This study shows that it is important for longitudinal interventions such as FCW and AEB to keep pedestrian behaviour into account to improve acceptance. Both FCW and AEB traditionally rely on TTC and required deceleration for their threat-assessment and decision-making algorithms (Brannstrom et al., 2010; Montgomery et al., 2014), possibly because the systems were initially developed to avoid rear-end collisions. As these systems address new scenarios including pedestrians and incorporating lateral conflicts, adapting the algorithms to model how other road users interact with drivers becomes increasingly important. In conclusion, including models for driver and pedestrian comfort zones in threat-assessment algorithms may improve the acceptance of FCW and AEB intervention times.

4.2. Evaluation of active safety systems within Euro NCAP

This study computed a model able to predict TTC at brake onset depending on seven factors. The TTC from this model makes predictions within the driver comfort zone, and therefore when a system warning or intervention may be acceptable (M Ljung Aust and Dombrovskis, 2013). Predictions from this model can be used for specific scenarios, in order to inform the design and evaluation of AEB and FCW—and, specifically, the design of Euro NCAP’s test scenarios. Euro NCAP currently assesses AEB performance in four types of pedestrian crossings. Fig. 8 shows our predicted earliest acceptable intervention time, expressed as TTC at brake onset. For comparison, two previously suggested brake onsets are also included in Fig. 8: the first one (Seiniger et al., 2014) is based on pedestrian proximity, indicating the point in time after which a pedestrian is not able to come to a complete stop before getting within one meter of the driving corridor. The second one (Lubbe and Kullgren, 2015) is based on driver comfort boundaries modelled as a function of pedestrian speed only. Fig. 8 shows that, while our brake onset times at 60 km/h are in line with (Lubbe and Kullgren, 2015), they exceed the limits proposed by (Seiniger et al., 2014), indicating that additional time is available for FCW and AEB activations when considering driver comfort boundaries rather than pedestrian kinematics.

Our brake onsets also argue for substantially earlier activations for low car speeds and far-side crossings (tested in CPFA-50; see Fig. 8). The first finding is of limited relevance, as it does not take a long time to come to a full stop at low speed, but the latter suggests that far-side crossing should be the prioritized test scenario and that asymmetric

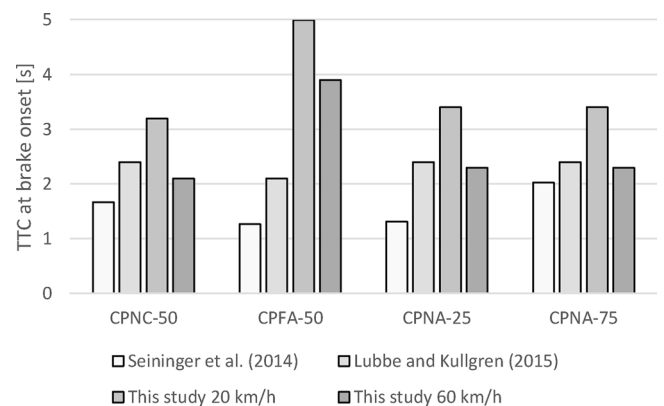


Fig. 8. Comparison of time-to-collision (TTC) at brake onset across four Euro NCAP scenarios. CPNC-50: an (obstructed) child pedestrian entering the road from the nearside at 5 km/h colliding with 50 % vehicle width overlap; CPFA-50: an adult pedestrian entering from the far side at 8 km/h with 50 % overlap, CPNA-25: an adult pedestrian entering the road from the nearside at 5 km/h with 25 % overlap; and CPNA-75: an adult pedestrian entering the road from the nearside at 5 km/h with 75 % overlap.

intervention times (for far- and near-side)—a concept not implemented in AEB and FCW so far—could increase acceptance and thus may be further investigated. Our brake onsets (Fig. 8) also indicate that there is sufficient time for a driver to react to a warning. Hence, for all Euro NCAP crossing scenarios, assessment of FCW in addition to AEB becomes relevant and should be considered in future protocols.

4.3. Experimental methodology

The ecological validity of driving simulator studies has been widely discussed, including the importance of deceleration cues that are absent in stationary simulators such as the one used in this study (Engstrom and Aust, 2011; Mullen et al., 2011; Summala, 2007). In addition, perception of speed and distances may be different in a simulator than in real-world driving and this limitation may constrain the applicability of our results to Euro NCAP scenarios. Nevertheless, A recent study from Boda et al. (2018) compared results from the same experimental protocol performed on a test-track and in a stationary simulator: braking response time was not influenced by the test environment (but braking profiles were). Because the experimental protocol and the driving simulator presented in this paper are very similar to the ones in the study by Boda et al., we believe that the lack of deceleration cues probably did not influence braking initiation. Nevertheless, the moment when drivers started to release the gas pedal may have been anticipated because of the lack of deceleration cues (Boda et al., 2018) and because the Logitech G27 pedals are spaced differently and provide different feedback than the pedals of a real car. Simulator experiments are often criticized because of the reduced risk perception in simulators compared to the real world; we acknowledge that this may be yet another limitation of this study. However, because the driving experience was very low-risk (we did not expose drivers to critical situations and we closely controlled their speed) the effect of reduced risk perception on our results may have been limited.

This study is also conditioned by a repetition issue: in the real world, drivers would seldom negotiate 39 intersections in a row with a pedestrian. In addition, despite our best effort to realistically animate the pedestrian, it was still clear to the drivers that the pedestrian was a virtual animation. Nevertheless, because the negotiations are not critical and do not necessarily require highly optimised control, the potential for our results to be applicable in the real world is high (Boer, 1999; Summala, 2007). Further, task repetition in a simulator environment is a common procedure (Cummings et al., 2007; Lee et al., 2002; Ljung et al., 2007; Scott and Gray, 2008) and encourages drivers to act within their comfort boundaries (Engstrom and Aust, 2011; Räsänen and Summala, 1998). Finally, although our longitudinal analysis showed some adaptive behaviour over time, these changes were not sufficient to significantly change gRT.

The participants were naive to the driving simulator; however, we do not think their inexperience in the driving environment significantly affected the results because 1) the participants practiced in the virtual environment until they felt confident they could master the vehicle and 2) we verified that the participants were able to longitudinally control the vehicle and accurately estimate distances before collecting data. Nevertheless, we do not know to what extent our results apply to the real world, where expectancy may be lower—but still present in some situations. For instance, a zebra-crossing at a school on a commuting route may, over the years, create an expectancy similar to the experimental protocol in this study. Repeating this experiment in a field environment and comparing our results to those using naturalistic data may help us understand the current discrepancy between this study and Lubbe et al.'s previous studies (Lubbe and Davidsson, 2015; Lubbe and Rosen, 2014) as well as clarifying under which circumstances (and the extent to which) the results from driving simulators may be ecologically valid for specific research questions and experimental protocols.

Thanks to the FFD, this study compared several TTA factors in one experiment revealing the importance of pedestrian TTA for calculating

driver response. More traditional experiments, addressing one or two factors at a time, would have missed the general conclusion about TTA and instead directed the reader's attention to statistically significant (but tiny) effects of the factors on different metrics of the response process. Thus, a clear benefit of the FFD, or other screening designs (Box et al., 2005; Diamond, 2001), is providing a bird's-eye view comparing multiple factors at once (Belz et al., 1998; de Ruyter, 2016; Machado-León et al., 2016; Wing-Gun and Ka-Hung, 1999). We chose FFD as it is a common design, and it provided valuable insights for our study, possibly because of the high resolution we chose (IV). On a less positive side, FFD did not allow us to appreciate potential interactions across factors. For instance, we could not (legitimately) divide the dataset into far-side and near-side trials once we understood the large effect that crossing side had on the data, although doing so would have deepened our analysis. In conclusion, FFD appears to be a useful tool for exploratory pilot work and, when combined with OpenDS, provides a promising platform for the rapid prototyping of experimental protocols. The more promising protocols could then be incorporated into the design of more expensive test track experiments to optimise testing resources.

It is possible to overlook driving experience in driving simulator studies, because there is no legal requirement to own a driver's license for participating. In addition, because they are more available, students often participate in simulator experiments despite being less experienced than average drivers (Saffarian et al., 2015). This study compared participants based on their driving experience, to determine whether the extra effort of recruiting experienced drivers is worthwhile. Our results suggest that, indeed, experienced drivers may respond earlier to threats and adapt their behaviour more than inexperienced ones; nevertheless, all participants reacted similarly and were influenced similarly by the factors under investigation, independent of driving experience. In conclusion, although response times in absolute numbers may be influenced by driving experience, the necessity of including only experienced drivers in a simulator study depends on the actual research questions. It is also worth noting that age is a possible confounder in this analysis, because more experienced drivers also tend to be older and students are less likely to own a car or commute to work. Future studies may also address the extent to which personal traits such as sensation seeking may influence the results from our experimental protocol.

This study explored the potential of open-source software (OpenDS) for traffic safety research. Although this study is not the first using OpenDS (Bouhoute et al., 2015; Golestan et al., 2014; Isnainiyah et al., 2014), it is the first to demonstrate the flexibility of this driving environment by including human models. The flexibility and the potential to share models across studies on a common open-source platform offer a unique opportunity for traffic safety research to leverage crowdsourcing and coordinate studies. Unfortunately, today's driving simulators and their virtual environments are often created ad-hoc and differ greatly across studies, limiting the validity of result comparisons even when experimental protocols and research questions are similar. Sharing environments, human models, and experimental protocols using open-source code may prove to be an efficient, economical, and effective way to harmonise simulator studies and improve their repeatability and comparability (Collaboration, 2015).

5. Conclusions

This study shows that, when drivers negotiate an intersection with a pedestrian in a highly repetitive driving scenario, their response process greatly depends on the relation between the time the pedestrian becomes visible and the pedestrian's speed, suggesting that the drivers' response might depend more on the pedestrian's field of safe travel than on their own. Thus, by modelling how a driver controls the vehicle in relation to the first point in time a pedestrian becomes visible, a threat-assessment algorithm may be able to take driver comfort boundaries

into account, improving the acceptance of its warning and intervention time. Unfortunately, because of the nature of this experiment, it is not possible to generalise this finding to situations with low expectancy or with an active interaction between the driver and the pedestrian.

In relation to the Euro NCAP pedestrian AEB 2018 test scenarios⁶, we found driver comfort boundaries to be reached substantially earlier than the latest point in time when an automated brake intervention would need to act to avoid the collision. Consequently, FCW may also provide safety benefit in these scenarios and may be introduced in future NCAP protocols.

Driving experience influenced how drivers behaved in this study, including their behavioural adaptation; however, whether an experienced driver behaves more realistically than an inexperienced driver in a driving simulator is still an open question. Nevertheless, the seven factors under investigation influenced the response process of both experienced and inexperienced drivers similarly.

This study employed FFD in a driving simulator experiment, confirming the design's strength for exploratory analyses and its weakness for deeper analyses. When combined with OpenDS, FFD provides a simple, flexible, and economical framework for exploring and piloting different experimental protocols. This framework may help plan more effective studies on test tracks or in larger driving simulators by identifying promising protocols, both of which are more expensive and time-consuming than experiments in fixed-base simulators such as ours.

OpenDS showed as a promising environment for traffic safety research and a potential platform for sharing models and experimental protocols across studies, leveraging crowdsourcing and boosting repeatability across driving simulator experiments.

CRedit authorship contribution statement

Marco Dozza: Conceptualization, Methodology, Funding acquisition, Supervision, Project administration, Resources, Visualization, Writing - original draft, Writing - review & editing. **Christian-Nils Boda:** Formal analysis, Methodology, Software, Visualization, Validation, Writing - review & editing. **Leila Jaber:** Data curation, Formal analysis, Methodology, Software, Investigation, Writing - review & editing. **Prateek Thalya:** Data curation, Formal analysis, Methodology, Software, Investigation, Writing - review & editing. **Nils Lubbe:** Conceptualization, Supervision, Project administration, Funding acquisition, Writing - review & editing.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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⁶ Euro NCAP, 2019. Test Protocol – AEB VRU systems, Version 2.0.4

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