Driving Simulators as Research Tools in Traffic Psychology

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1. INTRODUCTION

Driving simulators are now a major tool, arguably the major tool, for research on driver performance and behavior. Using two major journals—*Transportation Research Part F* and *Human Factors*—as the benchmark, it can be seen that studies based on simulator research constitute a major proportion of the published papers in the driving domain. In 2009, 32% (11 of 34) of the papers published on driving in *Transportation Research Part F* were based on experimental studies conducted in driving simulators, and those papers constitute a far higher proportion of the overall experimental work that was published since a large proportion of the other papers were based on questionnaire studies. In the same year, of 6 papers published in *Human Factors* in the area of "surface transportation," 5 (83%) were drawn from simulator experiments.

This preeminence of the driving simulator for research on driving is relatively new, and hand-in-hand with the growth of simulator studies has been the growth of the experimental approach for studying driving. The tool (simulators) and the method (experiments) are inextricably linked. The simulator is used for the investigation of experimental manipulations; comparison of the efficacy of treatments; what-if scenarios related to new systems and technologies; and the investigation of a variety of impairments, including alcohol, drugs, fatigue, and distraction.

This preeminence of the laboratory over the real world is rather surprising. With millions of drivers and millions of vehicles in the real world, one might well ask why use a driving simulator. Surely, in order to carry out research on driver behavior, road safety, road infrastructure design, the impact of new technologies, driver impairment, and so on, all we need to do is to collect and analyze real-world data. And yet, the number of driving simulators in universities and research establishments is constantly growing, and year-on-year considerable effort is invested in enhancing their capabilities. Indeed, the driving domain is quite unique among transport modes in the focus of its simulators on research as opposed to training. In aviation, maritime transport, and rail, where the vehicles are very costly in relation to the capital cost of a simulator, simulators are mainly used for operator training. In the driving domain, training simulators make only a small contribution compared with in-vehicle training and practice. But in the driving domain, the number of research simulators and the elaborateness of their specification continue to increase.

2. WHAT IS A DRIVING SIMULATOR?

This may appear to be a question with an obvious answer, but it is not really possible to give a precise definition. Since simulators can vary from simple facsimiles of driving using a joystick control with a simplified road environment displayed on a PC screen to multi-million-dollar laboratories providing full-size vehicles mounted on motion systems with up to 9 degrees of freedom and a field of view of up to 360°, there is no straightforward answer to the question.

A simulator has a set of physical features that usually include the following:

- One or more screens to display the scene: The image may be displayed on computer monitors or may be projected onto a flat or curved surface.
- Vehicle controls: The minimum is mouse or joystick control, but more common is a version of normal vehicle controls, either in the form of a steering wheel, pedals, and gearshift from a real car or in the form of a controller made for computer driving games by such companies as Logitech and Microsoft.
- A sound system to deliver road and vehicle noise.
- A dashboard: This may be a virtual dashboard, displayed on a monitor or by projection, or a dashboard from a real car.

with other tasks), there was a substantial increase in lane violations when the motion system was disabled compared to the with-motion condition. Lack of motion also increased heading error—that is, the angular difference of the driven vehicle from the instantaneous road. In a second experiment, drivers had to negotiate a course involving two lane changes laid out by cones. There were four levels of lateral motion scaling: 0 (i.e., no motion), 25, 50, and 70% of real-world acceleration. Heading error decreased with increased motion scaling, and, interestingly, the variability of yaw error also decreased with increasing scale factor, indicating that behavior was more consistent with greater motion capability. The authors concluded that drivers need to pay more attention to heading angle when motion cues are absent or reduced. This is perhaps not surprising because without motion, only visual feedback on vehicle path is available. They also concluded that there is a potential interference between this extra effort and the impact of distracting tasks on driving performance. As indicated previously, they found an interaction between secondary task type and simulator motion in impact of heading error. They stated,

The implications of these interactions for the widespread and growing use of fixed-base simulators to measure distraction caused by secondary tasks are serious. A common rationale for using these simulators is that, while results may not be comparable to actual driving in an absolute sense, relative comparisons of performance metrics across secondary task type are still meaningful. The interactions presented in this paper imply that such relative comparisons are specific to the motion cueing environment provided by the simulator.

The most common motion platform for driving simulators is the so-called Stewart platform (it was actually invented by Eric Gough) or hexapod, which uses six strut-type actuators linking a base platform and a simulator platform to provide motion in six degrees of freedom—that is, x, y, and z plus roll, pitch, and yaw. The potential to present realistic longitudinal and lateral accelerations as experienced in real cars is quite limited with a hexapod, and it is even more limited with the now quite common mini-hexapods. The driver is tricked into feeling such accelerations by means of tilt. To provide at least some extent of the true accelerations, the more elaborate simulators, such as the U.S. NADS (National Advanced Driving Simulator), mount the hexapod on an x–y table, which is able to surge both longitudinally and laterally (Figure 7.1). In some cases, a yaw table is also included.

But no matter how large the motion system, it will not be feasible to provide, for example, the continued lateral accelerations that a driver would feel in real-world negotiation of a long curve. Accelerations are generally scaled down substantially from what would be felt in real driving. Even with an x–y table, surge motion needs to be blended

FIGURE 7.1   The motion system in the University of Leeds Driving Simulator.

at some point with tilt, and the motion system must be returned to its neutral point (known as "washout"). With a hexapod, almost all the accelerations are unreal, and too rapid onset of tilt can result in participants becoming aware of the unnatural motion. In addition, there are transport delays in motion systems. All of these factors mean that there is an art as well as a science to selecting the algorithm to be used by the motion system, and the evaluation of one algorithm over another tends to rely on subjective responses from drivers (Dagdelen, Reymond, Kemeny, Bordier, & Maizi, 2009). There is a substantial literature on the advantages of motion, the type of motion system to implement, and the choice of motion cueing strategy.

5. WHAT KIND OF SIMULATOR TO USE

What level of simulator is required for a given study? The U.S. National Research Council committee of experts that was tasked with estimating demand for using the U.S. NADS (Transportation Research Board, 1995) was unable to produce a clear scientific justification for the large-scale motion system proposed for NADS:

There may be a scientific justification for a large motion base to simulate crash-avoidance maneuvers in NHTSA-sponsored research. The need for a motion base in other applications, however, is less apparent and cannot be specified with confidence unless and until a simulator with a large motion base is built and tested. Even so, past assessments of potential uses of driving simulators have found that most research can be performed satisfactorily on simulators without the range of motion that NADS will provide. A large motion base probably would be useful in vehicle design applications, but, as noted, representatives of the automobile industry have indicated very limited interest in using the simulator. Nevertheless, this does not mean that others would not use it. NADS is intended to be the most advanced driving simulator in the world. If it functions as designed, there are likely

to be users willing to pay a premium for the additional realism that its motion base will provide. Certainly the most advanced simulators built for space and aviation have been used heavily, often for applications that the builders could not have imagined. (p. 5)

Of course, a justification can always be made for using the most elaborate tool for scientific research. But cost is also a major consideration. Funding agencies and sponsors have limited budgets, and very high costs will result in fewer studies being performed. Researchers should carefully consider whether using a low-cost simulator can be justified, and the case for doing so will depend on the focus of the study, the participant numbers required, the practicalities of using a high-end simulator, and so on. Nevertheless, over time, there is a consistent trend of technologies becoming cheaper in real terms, and the advent of small and relatively low-cost hexapods may mean that motion systems become more commonplace.

In terms of image projection, once again, more tends to be better. There are clear arguments for providing a large field of view (360° horizontally is the ideal), rearview and side mirrors, high graphics resolution, as well as high contrast and brightness. But on a small monitor, very high resolution is pointless.

Comparisons of lower cost with more elaborate simulators do not invalidate the cheaper alternative. Instead, they indicate that simulator quality is a continuum. Santos, Merat, Mouta, Brookhuis, and de Waard (2005) compared a "laboratory"—that is, a very simple fixed-base simulator that used a 21-in. monitor and a low-cost games-style steering wheel and pedals—with a more elaborate fixed-base driving simulator in which the driver sat in a full-sized car and that used five-channel front projection on a curved screen to give a 230° horizontal field of view as well as a back view that enabled using the central review mirror. Both systems ran the identical software. The study investigated the impact of visual distraction from an in-vehicle information system (IVIS) on driving performance. The results indicated broadly similar effects in both the laboratory and the simulator. In both environments, a difference in lateral position variation and in lane exceedences was found between driving without and with the visual distraction. However, in the simulator, but not in the laboratory, differences in lateral position variation could also be observed between the levels of visual distraction. A parallel real-world (field) study found effects that were similar to but generally less powerful than those observed in the simulator. The authors concluded,

A simple, low-cost laboratory simulator setup is able to provide a first-shot test-facility to the automotive industry for assessing the impact of an IVIS under design or development. For more detailed analyses of the nature and seriousness of the influence of IVIS-type systems, a (medium cost) simulator is indicated, whereas

some of the earlier established problems with field studies in an instrumented vehicle have been confirmed. (p. 145)

Engström, Johansson, and Östlund (2005) compared the impact of both visual and cognitive task load in a fixed-base simulator, a moving-base simulator, and real-world driving in an instrumented vehicle. The road environment in each case was a motorway. The simulated and real roads had a similar layout. The fixed-base simulator used a full-size car and had a horizontal field of view of 135° to the front with no rear projection. The moving-base simulator had the front part of a full-size car and presented a horizontal field of view of 120°, again with no rear view. The motion system provided large linear motion via a track, as well as tilt and vibration. The study found generally consistent results between the two simulators. However, lateral variation was substantially greater in the fixed-base simulator. This is consistent with the results of Greenberg et al. (2003). What is not known is the extent to which greater effort is required to control the vehicle in a static simulator as opposed to a moving-base one. If there is a substantial increase in primary task workload because of the increased difficulty of steering in the absence of the vestibular cues, then some experimental results obtained in static simulators—particularly on secondary task interference from, for example, mobile phone use—might be partially invalidated. Certainly, drivers' subjective rating of workload has been reported to be higher in a driving simulator than in real-world driving (De Waard & Brookhuis, 1997). How much this effect is caused by the extra effort of steering without vestibular feedback and to what extent this effect is mitigated with a motion system have not been investigated.

However, it should be remembered that many factors affect simulator quality, not just the presence or absence of a motion system. Any driving simulator, however elaborate, has its limitations. Even the most capable motion systems scale down real-world accelerations, and even the largest motion systems are not able to sustain a longitudinal or lateral acceleration for very long before they run out of track. Also, the art of motion cueing in simulators is a significant field of study in its own right.

Other notable simulator qualities, apart from motion, include the following:

- Display capabilities: Field of view, pixel resolution, brightness, contrast, and capability for blending the images from more than one projector.
- Delay: Scene display lag and motion system lag following a driver input.
- Scene animation: The provision of textured (as opposed to flat-shaded) graphics, number of objects in the scene, and lighting algorithms.
- The physical models used to calculate vehicle dynamics: These can vary from a simple "bicycle" model of vehicle dynamics (Segel, 1956) to complex

multibody models that simulate the linkages in a vehicle's power train, steering, and suspension systems as well as the interaction between tires and road surface.

- The vehicle interface: Games controller, vehicle mockup or real vehicle, and the engineering of that interface (e.g., the provision of steering feel).
- Sound provision in terms of both hardware and software.
- The programming environment and its capability to deliver a wide variety of road layouts and traffic environments, the ability to create ambient traffic with appropriate behavior, and a capability to script scenarios to order.

It is clear that the quality of the software is as crucial as the quality of the physical environment. The capability of electronics hardware is now such that many of the low-cost simulators in use or being sold by simulator providers use the identical software as that used by their larger brethren. It is no longer the case that low cost means dumbed down.

6. HOW VALID ARE DRIVING SIMULATORS AS RESEARCH TOOLS?

The issue of the validity or nonvalidity of driving simulators for the purposes of research on driving is a contentious one. In the aviation and maritime domains, simulators are used largely for training rather than for research. They have to meet minimum performance requirements, but in general the justification for their use is a combination of the huge cost of the airplane or vessel and the potential for simulators to provide training in the handling of specific scenarios, particularly hazardous or emergency events.

In driving, simulators have been used mainly, but not exclusively (there are low-cost training simulators on the market), as platforms for research studies. There is a long history of such studies, dating back to the 1960s. But if simulators do not elicit normal or real-world behavior, then it can be argued that such studies lack validity and should properly be performed on real roads or on specialized test tracks.

Certainly, the arguments of critics of driving simulators are both forceful and plausible. Leonard Evans, in his influential book *Traffic Safety and the Driver* (1991), drew a distinction between driver performance, which represents an individual's capabilities and skills, and driver behavior, which refers to how an individual chooses to drive, given his or her skills. An example of performance is reaction time, whereas examples of behavior are speed choice and chosen time headway. Evans argued that driving simulators were appropriate tools for the investigation of performance but not of behavior:

As driver performance focuses on capabilities and skills, it can be investigated by many methods, including laboratory tests, *simulator experiments, tests using instrumented vehicles and observations of actual traffic. As driver behavior indicates what a driver actually does, it cannot be investigated in laboratory, simulator, or instrumented vehicle studies. (p. 133)*

Not content with this blanket prohibition of simulator studies for behavioral investigation, Evans also impugned the validity of simulators for the investigation of performance:

The discussion … on reaction time showed the primacy of expectancy; even in real-world experiments, reaction times of participating subjects are substantially shorter than unalerted drivers. Thus, any estimate of reaction times using a simulator, no matter how realistic, would be suspect unless the subject drove for many hours to establish arousal and anxiety levels characteristic of normal driving, thus limiting data collection rates to a few per day. (p. 126)

He continued with some scornful remarks about the lack of progress in addressing research topics such as the impact of alcohol and fatigue, the design of road markings and signs, and reduced visibility on driving: "Can the lack of progress [over the previous 20 years] be traced specifically to insufficient realism in the simulator, thus justifying a more sophisticated simulator?" (p. 127). He commented that simulators lacked the ultimate element in eliciting realistic behavior, namely giving drivers the fear that crashing could result in real damage or injury.

These opinions were repeated in Evans' later book, *Traffic Safety* (2004): "It is exceedingly unlikely that a driver simulator [sic] can provide useful information on a driver's tendency to speed, drive while intoxicated, run red lights, pay attention to nondriving distractions, or not fasten a safety belt" (p. 188). Perhaps we may concede on drink driving and belt wearing, but the other phenomena have all been investigated in simulator studies that produced meaningful results. These criticisms of simulator studies have been echoed by Olson, Hanowski, Hickman, and Bocanegra (2009) in their report on truck driver distraction as observed through naturalistic driving studies carried out at Virginia Tech Transportation Institute (VTTI):

It is important to highlight that some results of the current study and other recent naturalistic driving studies … are at odds with results obtained from simulator studies … and future research should be conducted to explore the reasons why such study results often differ from studies conducted in actual driving conditions (i.e., the full context of the driving environment). It may be, as Sayer et al. (2007) note, that controlled investigations cannot account for driver choice behavior and risk perception as it actually occurs in real-world driving. If this assessment is accurate, the generalizability of simulator findings, at least in some cases, may be greatly limited outside of the simulated environment. (p. xxvi)

The press release from VTTI that accompanied the release of the report by Olson et al. (2009) went even further. Discussing "the disconnect between naturalistic and simulator research," it stated,

It is important to keep in mind that a driving simulator is not actual driving. Driving simulators engage participants in tracking tasks in a laboratory. As such, researchers that conduct simulator studies must be cautious when suggesting that conclusions based on simulator studies are applicable to actual driving. (VTTI, 2009, p. 2)

It is interesting to note that when faced with a disjuncture between the findings from simulator studies and those based on naturalistic driving, these researchers do not consider the possibility that the analysis techniques adopted for the real-world studies might be faulty. The particular finding that was most out of line with simulator-based evidence was the conclusion in Olson et al. (2009) that talking on a handheld mobile phone did not increase risk, whereas talking on a hands-free mobile phone actually reduced risk. The authors do not appear to have considered the possibility that there was a methodological flaw in their analysis, for example, regarding the identification of distracted and nondistracted episodes for comparison purposes. Nor do they discuss other real-world studies that point in the opposite direction.

Simulator driving is by definition an attempt at convincing participants that they are engaged in an analogue of real-world driving. The success with which that is achieved will determine the validity of a given simulator. A common distinction (Blauw, 1982; Wang et al., 2010) is between physical validity and behavioral validity. Physical validity refers to the physical components and subsystems of a simulator, whereas behavioral validity refers to how close the experience of the participants and the driving elicited approximates that in a real vehicle on real roads. The two are not necessarily aligned: It is possible for a simple static simulator with a visual display on a single monitor and a gaming-style vehicle interface to produce driving that is close to real-world behavior, whereas a very elaborate simulator does not necessarily produce "real" behavior. But it is reasonable to suppose that a more elaborate environment will be more realistic and more immersive.

Physical validity can be further broken down into various components:

- The accuracy of the underlying software representing vehicle dynamics.
- The capability of the visual system in terms of brightness, contrast, resolution, field of view, and size of the projected world (note that virtually all simulators use two-dimensional projection to display a three-dimensional world).

- The fidelity and elements of the sound system—road noise, engine noise, etc.
- The elaborateness of the physical vehicle controls and displays with which the driver interacts—more capable simulators generally use a real vehicle cab—and the accuracy with which pedal feel, steering wheel feel, and gearshift feel (where this is provided) are conveyed.
- In a simulator with a motion base, there are the numbers of degrees of freedom (up to nine) provided, the scaling factor relative to real-world forces used for the direct motion cues (surge in the x axis, sway in the y axis, and heave in the z axis), the strategies used for tilt coordination (there is a quasi-standard here in the form of the classic motion drive algorithm as described by Nahon and Reid (1990)), and the inertia and mechanical delays imposed by the motion platform.

Behavioral validity is also not a single construct. One can refer to the basic levels of driving performance such as speed and lateral position, or one can consider more demanding tasks, such as the control of deceleration in approaching a stop line or the ability to carry out a smooth lane change or lateral positioning in fast negotiation of curves. In addition and in accordance with the previous discussion on whether simulators can provide accurate studies of the impact of driver distraction, one could examine task prioritization between the primary task of driving and potential distracters such as mobile phone use.

Another distinction that has been made in the literature on simulator validity is between absolute and relative validity (Blauw, 1982; Kaptein et al., 1996). Blauw's distinction between the two is as follows:

All methods [of validation] give parameters describing validity by comparing conditions of driving in the simulator in relation to driving under the same road conditions. A modification of this approach is to compare performance differences between experimental conditions in the simulator with performance differences between similar conditions in the car. When these differences are of the same order and direction in both systems, then the simulator is defined to have relative *validity. If, in addition, the numerical values are about equal in both systems, the simulator can be said to have* absolute *validity as well. (p. 474)*

It would perhaps be more accurate to state that, in order to achieve relative validity, a simulator should not only produce the same ordering of effects as would occur in the real world but also not induce any spurious interactions between conditions, participant groups, and rank ordering of effects. One would not want one group of participants, such as young males, to be differently affected from another group, such as older females, in terms of the reproduction in a simulator of real-world orderings.

What do the simulator validation studies that have been carried out generally indicate? First, not every type of

simulator has been validated. Not surprisingly, validation studies have been concentrated on mid-level and top-end simulators. Also, for driving simulators, unlike training simulators for flight, there is no standard set of evaluation tests.

In terms of the performance of the simple vehicle control task of speed maintenance and lateral control, the validation studies on mid-level simulators are not in full agreement with each other, perhaps because the simulator designs differ and because there is no proper control for the contribution of the various design elements across the studies. Kaptein et al. (1996) reported on a study of the TNO Human Factors simulator carried out in the early 1990s that examined the impact of road width and curve layout on speed. In the simulator and in driving on the real road, speed reduced with decreased road width and with sharper curves. However, in the simulator, speeds were generally higher, including on sharp curves. By contrast, Blana (2001) found in her study of the then similarly configured Leeds Driving Simulator that speeds on straights were generally higher than in real road traffic, but that speeds on sharp curves were in line with those observed on the real road. In terms of lateral position, correlations with real-road traffic was less good: Less curve straightening (corner cutting) was observed in the simulator (perhaps not surprising in a static simulator), and there was a smaller lateral shift away from opposing traffic in the simulated environment. Also, variation of lateral position was higher than for real-road traffic (Blana & Golias, 2002).

Kaptein et al. (1996) examined the impact of research question on validity. For example, they found absolute validity in using a mid-level simulator for the study of driver route choice. They also concluded that the provision of a moving base substantially reduced variation in lateral position and could lead to absolute validity for that measure. Overall, they concluded,

Tasks that depend on estimation of speeds and time duration may be affected by image resolution limitations. Yet, a number of experimental results in simulators with limited image resolution and without a moving base have been validated satisfactorily, indicating that such limitations are not important to all driving tasks. (p. 35)

Perhaps the most thorough behavioral validation of a single simulator has been carried out by Wang et al. (2010). They compared performance in a typical midrange simulator, the MIT AgeLab driving simulator, with performance data collected in an instrumented vehicle. The study data used were on secondary task load from three different input devices used for destination entry in a surrogate navigation system. They noted that relative validity becomes more complex in a situation in which, in the real world, no significant difference is found between

some conditions (here, devices) for a particular performance indicator. They also proposed that relative validity is good when not only the rank ordering of effects is similar but also there is correspondence in the relative magnitude of the effects—that is, there is no interaction between experimental condition and the environment in which the data are collected (simulator or real-world driving) in terms of effect.

Wang et al. (2010) used two groups of participants—one for on-road driving in an instrumented vehicle and another in the simulator. They analyzed a wide range of dependent variables, examining response time to initiate the task, task completion time, glance frequency, total glance time, eyes on-the-road time, and maximum glance duration. They also examined a number of parameters of driving performance—mean speed, standard deviation of speed, and standard deviation of lane position. They found that the measures of task time and visual attention indicated both relative and absolute validity of the simulator. On the other hand, the driving performance measures were problematic because there was generally no differentiation in either environment between the devices tested in terms of these measures, although the standard deviation of speed measure did meet the criteria for both relative and absolute validity, They concluded,

Fixed-based driving simulation is a safe method of assessing basic task performance and visual distraction for purposes of comparing manual user interface designs and provides valid estimates of these behaviors on-road for the type of in-vehicle interface interactions examined in this study. (p. 419)

7. PROBLEMS IN USING SIMULATORS: SIMULATOR SICKNESS

One major problem encountered in simulator studies is that of simulator sickness. This is not just an issue with research driving simulators but also with simulators for other applications, such as the training of tank drivers by the military. Simulator sickness is a form of motion sickness caused by a mismatch between the visual perception of acceleration or deceleration and vestibular sensation of the same motion. Clearly, there is no vestibular feedback in static simulators, but even the most elaborate motion platforms employ trickery in the form of tilt to maintain the illusion of sustained acceleration, and in any case there will be transport and other delays in a motion system so that even "true" motion cues will not be totally accurate.

The issue for research is whether simulator sickness is just an inconvenience for researchers and participants or whether it causes more profound problems. One issue is that not all types of participants are affected at an equal

rate. In one of the experiments conducted for the HASTE European project on driver distraction, we attempted to conduct an experiment with a group of elderly drivers (older than 60 years) to study the impact of visual distraction on driving performance. The visual distraction was created by means of a task displayed on an LCD screen positioned close to the driver. However, the proportion of elderly participants who experienced simulator sickness was so large that we had to abandon using that group of participants. The consequence was that we were unable to investigate the impact of visual distraction on elderly drivers, although we were able to successfully perform an experiment on the impact of cognitive distraction using an auditory memory task.

Simulator sickness is more than just an inconvenience. In a study carried out on a driving simulator with a small motion base, Bittner, Gore, and Hooey (1997) confirmed a significant interaction between age and display type in the prediction of sickness as indicated by a factor "faintness" calculated from participant comfort questionnaires. In a further step, the same study carried out an analysis of driving performance data with and without simulator sickness as a covariate. The dependent variable was reaction time in an emergency situation. Inclusion of faintness and vehicle speed as covariates resulted in a substantial increase in the number of independent factors that were significant ($p < 0.05$) and near significant ($p > 0.055$) in the analysis of variance. In other words, simulator sickness affected performance in the emergency task. The authors concluded, "It is strongly recommended that researchers explore and control the potential confounding effects of simulator sickness to assure meaningful performance assessments" (p. 1092).

Thus, discomfort can both prevent studies from being completed and affect the results obtained in driving simulators. Anecdotally, it can be stated that the rate of simulator sickness is reduced with a motion system, and especially with a large-scale motion system, but as far as we are aware, this has not been investigated systematically.

8. EXPERIMENTAL DESIGN

No particular experimental design can be considered as standard, although within-subject designs have major advantages in terms of experimental power. However, they can also have disadvantages, both in terms of the time required for participants to experience all the required conditions and because repeated-measures designs tend to induce familiarity with the scenarios included in the experiment and may therefore make surprise events nonviable.

Similarly, counterbalancing of conditions can be considered as the norm because of the ability to control for learning effects. But the disadvantage is that counterbalancing makes it very difficult to investigate learning and ordering effects when these might be considered to be important.

Thus, in experimental designs, as in other aspects of the setting up of simulator experiments, there is no right way and no wrong way. Experimenters should be guided by the research questions and hypotheses that they wish to address, and they should carefully weigh the advantages and disadvantages of alternative designs.

Another experimental design issue relates to the amount of control over scenarios. There is a strong impetus to create scenarios that are equal in severity for all participants. Thus, it may be considered desirable to have a car-following scenario in which the lead vehicle is controlled in terms of a give time headway to the driven car. Then an event such as a sudden braking of the lead vehicle can be triggered such that all participants have to respond to an event of equal severity. However, participants cannot be forced to drive at a given speed (unless speed control is automated), and a given participant may find that the chosen time headway is too close for comfort. The participant will react by slowing down, the preceding vehicle will come closer, the participant will slow down more, and so on. This phenomenon of participants trying to "override" the scenario design has been observed in the University of Leeds Driving Simulator. It is also discussed by Donmez, Boyle, and Lee (2008), who carried out an analysis of such a scenario using the inverse of actual headway distance (rather than time headway, which was preset) at the time of accelerator release as a covariate. The finding from this analysis was that the experimental results changed depending on whether the covariate was taken into account: Without consideration of the covariate, distraction of various types appeared to improve reaction time, but once the covariate was considered, it was found that distraction resulted in longer reaction times.

9. CONCLUSIONS

Simulators provide the opportunity to investigate driving under controlled conditions in a manner that is unparalleled by the alternatives. Real-world studies lack the equivalent control element, whereas test tracks offer a very depleted and inflexible driving environment. Simulator capability, particularly in terms of the graphics performance of PC-based systems, has grown very fast in recent years, and the advent of small-scale and relatively low-cost motion systems means that it may soon become standard for a midrange simulator to be equipped with six degrees of freedom of motion. The number of research simulators worldwide continues to increase, and simulator studies constitute an increasing proportion of the research literature on driving performance and behavior. Simulators may not be total replicates of the real world, and indeed they cannot be. But they offer the researcher of driver behavior an

advantage that real-world studies cannot match: the ability to control experimental conditions and create prescripted scenarios.

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