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Psychological Fidelity: Perception of Risk

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Abstract

The Problem. High-fidelity driving simulators provide a realistic and compelling experience for research participants. However, the credibility of research results from simulator studies continues to be challenged. The fidelity of the driving experience appears insufficient to overcome criticisms concerning the lack of psychological fidelity, defined as the extent to which the risks and rewards of participation in the experiment correspond to real-world risks and rewards. ***Key Results of Driving Simulator Studies.*** Experimental studies eliminate the injury risk associated with driving. They also typically eliminate the trip purpose, which influences all components of real-world driving. Unfortunately, researchers typically give little consideration to this problem, often instructing participants to drive as they normally would. In the absence of a well-defined driving context, such instructions can be confusing to some participants. ***Tools Available to Researchers.*** Well-designed driving simulator experiments eliminate confusion about driving motives by creating constrained situations to elicit specific behaviors. Researchers must identify the driving components that have been eliminated by the simulation and attempt to replace them through the use of instructions and performance incentives. Instructions define the performance space and driving task components; incentives define the relative priorities associated with the task components. The effects of incentives on performance are determined by some combination of (1) the nature of the incentive; (2) task characteristics; (3) aspects of performance selected for measurement; and (4) individual differences. Incentives are likely to improve certain aspects of performance, while degrading others at the same time, implying that care must be taken in matching incentives to performance measures. ***Scenarios and Dependent Variables.*** Reward/penalty schemes are used to incorporate performance incentives into driving simulator studies. Practical issues associated with their use include: simulating the effects of significant negative outcomes (i.e., crashes); multiple crashes; effects of incentives over time; assessment of reward/penalty systems; and non-independence of performance measures. Detailed examples of the use of reward/penalty systems are presented. ***Platform Specificity and Equipment Limitations.*** Problems of psychological fidelity apply to all platforms. Improving psychological fidelity eliminates unwanted variability due to individual differences in driving, which result from uncertainty about the experimenter's priorities.

9.1 Psychological Fidelity: Perception of Risk

The availability of sophisticated driving simulators provides an increasingly realistic driving experience for research participants. The current generation of driving simulators allows researchers to create myriad situations with complex roadway geometry, realistic surrounding traffic, pedestrians and traffic control devices. Among the most advanced simulators, projections of the virtual world completely surround the driver, images from the mirrors are realistic, and drivers feel the effects of their steering or braking inputs. As simulator capabilities have become more affordable, the number of experimental studies using driving simulators has increased. For example, numerous studies have been conducted addressing the question of whether cell phones are sufficiently disruptive to driving to be considered a safety hazard. Despite the fact that these studies provide relatively consistent results (Horrey & Wickens, 2006; Caird, Willness, Steel, & Scialfa, 2008), their findings are often challenged or discounted, relative to those of epidemiological or observational studies. For example, McCartt, Hellinga and Braitman (2006) reviewed 54 studies that utilized driving simulators or instrumented vehicles to assess the impact of cell phone use on driving behavior or performance. Their overall assessment was that the observed "changes in performance of experimental tasks have uncertain implications for real-world driving" (p. 92). While these authors raised a number of methodological issues, they identified one concern that is most relevant to the present discussion—namely, the authors question the lack of realism depicted by experimental studies.

Goodman et al. (1997) discussed the limitations of driving simulators, including lack of realism in the visual display, absence of motion among fixed-base simulators, and most importantly (in the context of cell phone research) the simulator's effects on drivers' priorities. They suggested that drivers may be more inclined to devote an unrealistic amount of attention to the secondary phone task because "there are no serious consequences with driving errors in the simulator" (p. 86). Interestingly, the authors continued by suggesting: "the use of high fidelity simulators such as the National Advanced Driving Simulator (NADS) will greatly enhance our ability to address such concerns." These authors apparently felt that the increased fidelity of the driving experience would lead to a more realistic allocation of attention between driving and secondary (cellular phone) tasks. Implicit in this conclusion is the idea that participants will be drawn into a more compelling experience, which in turn will encourage them, presumably without much thought, to revert to their natural driving behavior.

More than 10 years have passed since that optimistic projection was made and the NADS has been used for a variety of experimental studies, including several studies of drivers' responses to cell phones (Ranney et al., 2005). Unfortunately, there have been no comparative studies addressing the effects of increased fidelity on participants' behavior. Thus, there is no direct way to test whether the assertion made by Goodman et al. (1997) is true; however, according to Caird et al. (2008), the findings of the NADS cell phone research are relatively consistent with those of other studies that have used lower-fidelity simulators. Effect sizes appeared to be in the same range as those derived from studies conducted using simulators with less fidelity. Thus, advanced simulators have not been associated with changes in the patterns of results that would indicate that the compelling driving experience is sufficient to overcome the problems identified by Goodman et al. (1997). Increased fidelity of the driving experience does not address the problem of poor psychological fidelity.

9.2 What Is Psychological Fidelity?

Researchers are typically proud of the fidelity of their driving simulators. However, because they are often intimately involved in their development, they may overlook the peculiarities of their experimental setups and assume that research participants can readily make the leap of imagination necessary to behave in the simulator as they would while driving in the real world. It may be difficult for researchers to consider how the simulator is viewed by members of the community, particularly those removed from the university or research environment. For example, when confronted with an unemployed truck driver, whose participation in a simulator experiment is intended simply to make a few dollars between jobs, or a busy mom whose main concern is that she completes the experiment to be home in time to greet her kids when they get off the school bus, the researcher may conclude success if the participants appear to engage in the simulated driving task without complaint. While the credibility of the driving experience is an important hurdle, it unfortunately ignores the more fundamental problem that is at the heart of psychological fidelity, namely that when we drive in the real world, we do so for a purpose (Duncan, 1990). Even the most sophisticated simulator cannot overcome the fact that driving simulator studies alter the driving task fundamentally, eliminating this most basic component of driving, namely, the trip purpose. Thus, while experimenters may specify a trip purpose, the fundamental artificiality of the experimental setting, including the destination, remains.

Drivers' motives are significant determinants of on-road driving behavior (Näätanen & Summala, 1976; Duncan, 1990; Ranney, 1994). Although empirical studies typically do not address this issue directly, drivers may change their on-road behavior depending on the purpose of their trip. Getting to work on time may evoke different driving behavior than embarking on a recreational trip with no time constraints. Drivers' motives may also change within a given trip, leading to changes in driving behavior. For example, some drivers may increase their speed and alter their decision-making when they realize that they will be late for an appointment or planned event. Emergent driving situations may also influence drivers' momentary motives and their driving behavior. For example, drivers may alter their attention entirely when they find themselves trapped behind a slow-moving vehicle in dense traffic. Together, the global trip purpose and the driver's momentary motives play a significant part in determining much of their on-road behavior, including speed, following distance, and decision-making in accepting gaps in passing and entering traffic. In the context of Michon's (1985) hierarchical model, motives at the strategic level (e.g., trip purpose) combine with those at the tactical (e.g., gap acceptance) and vehicle control (e.g., speed selection) levels to influence driving behavior. As a particular example, we can look to the effect that teen passengers have on the behavior of teen drivers. It has been observed that teen drivers in the presence of male passengers drive faster than the general traffic and have shorter headways (Simons-Morton, Lerner, & Singer, 2005). Here, both the tactical and vehicle control levels are being influenced by the driver's motives.

Well-designed driving simulator experiments eliminate confusion about driving motives by creating constrained situations to elicit specific behaviors. An example is the slow-moving lead vehicle scenario described above; however, without a defined trip purpose, there is no reason to expect that drivers will consistently experience the momentary frustration assumed to motivate them to give high priority to extricating themselves from these situations in real-world driving. Removing the trip purpose leaves a void in the participant's motivation that must either be defined by the experimenter or left to the participants to fill. Depending on the experimental objectives, ignoring this potential problem can serve to introduce a significant amount of unwanted variability due to individual differences in priorities (Edwards, 1961). Unfortunately, according to Zeitlin (1996), who examined 106 published research simulation studies, researchers have given little consideration to this potential problem.

Current trends in research funding provide another way of considering the importance of psychological fidelity to driving behavioral research. Recently, the "100-Car Naturalistic Driving Study" was conducted by Virginia Tech Transportation Institute (VTTI) (Klauer, Dingus, Neale, Sudweeks, & Ramsey, 2006). This study represents an emerging research methodology in which drivers are observed in their own vehicles during everyday driving. Based on the success of this and other naturalistic studies, the second Strategic Highway Research Program (SHRP 2) has committed a significant portion of their Safety Program resources to the implementation of a large-scale naturalistic study. The stated rationale for this undertaking derives from the desire to better understand pre-crash behavior (Transportation Research Board, 2008). But instead of focusing on crashes per se, the study will follow drivers for several years on the assumption that some of them will become involved in crashes during this period. The inefficiency of this approach for studying pre-crash behavior, relative for example to one that utilizes a high-fidelity driving simulator (such as the NADS), becomes evident when one considers the fact that on average an injury crash occurs less than once per million vehicle miles of vehicle travel (National Highway Traffic Safety Administration, 2008). In a simulator, over the course of a year researchers can expose thousands of drivers to carefully calibrated crash-imminent situations. Situational dynamics can be systematically varied to evaluate their effects on the likelihood of crashes. In contrast, a naturalistic observational study will discard thousands of hours of driving data for each crash and the circumstances of each crash will likely be so different as to challenge conclusions about the generality of pre-crash behaviors. If one accepts the validity of the NADS's realism, it makes little sense to devote relatively huge amounts of research resources to an undertaking as relatively inefficient as a naturalistic study. Why then is this being done? Sadly, we must conclude that validity of the NADS realism is not accepted among policymakers and that naturalistic observation is preferred, even given the significant associated inefficiencies, because it involves real rather than artificial driving. The fact that the funding pendulum is swinging strongly in this direction can be viewed as an endorsement of the importance of (naturalistic) realism in the study of driving behavior. Clearly, to compete in this environment, researchers using simulated driving environments must address this credibility issue by means other than improving the fidelity of their simulators.

9.3 How to Improve Psychological Fidelity

The fact that researchers trade realism for experimental control is a cornerstone of the scientific method. Making this trade is necessary to test specific hypotheses, but the loss of realism implies a loss of psychological fidelity. There are tools at the researcher's disposal that can help improve the psychological fidelity of the simulator experiment. Most generally, this requires identifying the components of driving that have been eliminated by the simulation and attempting to replace them through the combination of instructions and performance incentives. The focus of the remainder of this chapter is on the use of these tools.

9.3.1 Simulation of Performance Versus Behavior

Researchers are sometimes imprecise in describing their experimental objectives. For example, they may use the terms driving *behavior* and *performance* indiscriminately or interchangeably. However, these terms have different meanings in the context of driving behavioral research (Evans, 1991; Näätanen & Summala, 1976; Ranney, 1994) and understanding this distinction will help determine how best to improve the psychological fidelity of the simulation. Performance refers to drivers' responses at the limits of their ability, or what the driver can do; behavior refers to the typical unconstrained on-road driving, most of which involves a level of effort significantly below the driver's limiting ability. Driving simulators have been used for both purposes, yet researchers typically do not refer to this distinction, nor discuss its implication for the generalizability of their results. The results of driving performance studies generalize most directly to critical on-road situations, which typically occur when task demands increase unexpectedly and drivers are required to respond at or near the limits of their abilities to avoid a crash. Examples include vehicles unexpectedly encroaching into the travel lane or unexpected patches of slippery roads. Early theories of crash causation assumed that crashes were primarily caused by such failures and this led to an emphasis on identifying the limits of drivers' skills (Ranney, 1994). However, the convergence of theoretical models that emphasize motivational factors (e.g., Näätanen & Summala, 1976) with research results demonstrating an inverse relation between driving skill and crash involvement (Williams & O'Neill, 1974) led to increased interest in the study of errors in non-critical situations (e.g., inadequate safety margins) as a means to better understand and develop theories of driving behavior (e.g., Brown, 1990). To improve psychological fidelity, researchers must first understand whether they want to study crash-avoidance performance in limiting situations or more typical behavior in routine non-limiting situations. This distinction has significant implications for the design of experimental protocols and for developing participant instructions.

9.3.2 Instructions

Participation in experimental research studies differs from everyday experience; participants typically enter the experimental situation with very few expectations. For this reason, participants are usually very attentive and actively attempt to construct expectations by observing experimenters' behaviors. Researchers who study driving performance typically want participants to perform as well as possible. To accomplish this, they may rely on the participant's inherent desire to do well, or the assumption that participants bring an attitude of deference into what they perceive to be a "testing" environment, as sufficient motivation to ensure peak performance. Alternately, they may instruct the participants to perform "as well as possible without making errors." While this instruction is widely used in laboratory tests, particularly those involving a speed-accuracy tradeoff, it is fundamentally a contradictory combination of instructions, as pointed out by Pachella (1974). Specifically, it is not possible to know precisely how well one can perform "without making errors," unless some are actually made. Edwards (1961), although reviewing research from a different era, noted that ambiguous or internally contradictory instructions are not uncommon in psychological experimentation.

In contrast, researchers who study driving behavior may instruct participants to "drive as you normally would." Excluding naturalistic situations, the use of this instruction is generally an indication that little consideration has been given to the psychological fidelity of the experimental protocol. As discussed above, drivers may alter their "normal" driving behavior depending upon the trip purpose and their momentary motives. If the experimental protocol fails to address these critical determinants of driving behavior, it is not surprising that some participants may be confused by such an instruction. Researchers would do well to consider the generic instruction to "drive as you normally would" as being the first part of an instruction that requires further specification. Examples of more complete instructions would include:

"Drive as you normally would when you are late for a job interview" or

"Drive as you normally would when you just finished your last exam."

Finally, an implicit part of the instruction to "drive as you normally would" contains a corollary problem, namely the questionable assumption that drivers normally drive in a consistent manner across days or across situations in a given drive. The anticipated emergence of accessible naturalistic data bases may provide an opportunity to examine this assumption, particularly given designs that provide data over an extended time period for each participant.

Many studies combine these two approaches, asking participants to drive normally and subsequently surprising them with unexpected situations requiring avoidance maneuvers (e.g., lead-vehicle braking). Several assumptions are implicit in this scenario. First, researchers assume that participants' inherent desire to avoid crashes will transfer intact from the roadway to the experimental situation, resulting in realistic crash-avoidance behavior. To the extent that scenarios elicit drivers' immediate and automatic crash-avoidance responses, this may be true. However, it is also possible that some participants may want to take advantage of the fact that driving in a simulator will ensure that there are no serious consequences for their driving errors. These adventuresome drivers, although representing a small minority of typical research participants, may adopt unusually risky behaviors to increase the likelihood of experiencing a crash. Second, if the researchers are primarily interested in the drivers' responses to the unexpected events, they may fail to consider whether the strategies adopted by drivers during the (normal) driving influenced their subsequent responses to the unexpected events (e.g., Brown, Lee, & McGehee, 2001). For example, if the researcher fails to define the overall context of the simulated trip, it is reasonable to expect the drivers to adopt different speeds and headways for a variety of unknown reasons. These differences can be expected to influence drivers' responses to the unexpected event, adding variance to the experimental design that may reduce statistical power for addressing the questions of primary concern. Although researchers may argue that leaving the context undefined preserves realism by allowing for individual differences in driving styles, this argument is based on the untested and probably erroneous assumption that drivers' real-world driving practices transfer intact to the simulated driving environment. At a minimum, drivers in experimental studies need enough information to guide their speed selection. However, providing the speed limit may not be enough information, drivers must also understand how it will be enforced and the consequences of exceeding the speed limit.

Many contemporary driving simulator experiments utilize a dual-task paradigm, in which participants perform secondary tasks while driving. A significant issue for this type of study with respect to psychological fidelity is the drivers' allocation of attention between primary and secondary tasks, which requires some judgment as to the relative importance of the two tasks. Edwards (1961) pointed out that when the implicit instruction for an experiment is to "do the best you can," this essentially is an instruction to maximize or minimize some mathematical function defined by the components of the experiment. However, in a dual-task situation, such as driving while performing a secondary task, this instruction becomes troublesome because it implies that the participant is intended to maximize or minimize two functions simultaneously. It is very unlikely, Edwards continues, that two functions will have maxima or minima that can be jointly achieved, which makes it impossible for the subject to do their best possible on both components simultaneously. In this situation, it is incumbent on the researcher to provide specific information concerning the relative importance of the concurrent tasks in this paradigm. Edwards (1961) advocated providing a complete payoff matrix to participants to avoid misinterpretation of instructions. Thus, instead of simply instructing the driver concerning the relative priorities of the primary and secondary tasks, the researcher would present a matrix that defines all possible outcomes based on the combinations of task (primary or secondary) and level of task performance (e.g., good, acceptable, poor) and defines the specific rewards associated with each task combination.

9.3.3 Incentives and Driving Simulation

Incentives are fundamental to learning theory and there is much, albeit mostly older, research on how they affect performance or behavior. Zeitlin (1996) distinguished among three categories of incentives that can influence performance or behavior in simulators: (1) *Consequential incentives* are real-world consequences associated with simulator performance, such as a certification or job licensing. These incentives relate primarily to the use of simulators for testing and it is generally assumed that because the test outcome has real-life implications the participants will strive to perform to the best of their ability. (2) *Intrinsic incentives* are features of the simulation that inherently motivate participants to perform at a desired level. These incentives include the entertainment value or challenge associated with the simulated events. According to Eysenck (1983), intrinsic motivation to perform a task will be present to the extent that performing the task increases an individual's feeling of competence and self-determination. Increasing intrinsic incentives is typically not compatible with research or training objectives because the specific elements that must typically be incorporated into driving simulations to increase the intrinsic motivation are likely to be features of games that are not typically found in real-world driving (e.g., real-time scoring system, exciting scenario events). (3) *Extrinsic incentives* are rewards and penalties associated with different aspects of performance or behavior, including monetary rewards and penalties, praise, or food. As will be discussed below, this category offers an opportunity to motivate behavior that approaches on-road driving in research studies using driving simulators. However, the use of extrinsic incentives will serve to restrict the participant's performance or behavior to the activities associated with the delivery of contingent rewards (Eysenck, 1983).

9.3.3.1 Incentives and Performance

Generally, incentives are considered to have the same effects as arousing stimuli (Eysenck, 1983) and the Yerkes-Dodson model (1908) is used to describe the effects of incentives on performance. According to this model, there is an inverted U-shaped relationship between arousal or motivation and performance, with intermediate levels of motivation being optimal for performance. Within this general framework, Eysenck (1982, 1983) has suggested that the measured effects of incentives on performance are determined by some combination of the following factors: (1) nature of the incentive; (2) task characteristics; (3) aspects of performance selected for measurement; and (4) individual differences. The nature of the incentive includes the amount of the incentive and the probability of attaining the incentive. The interaction of these two factors may be most important in determining the level of motivation. For example, a very large incentive combined with a near-zero probability of success is likely to provide a relatively low level of motivation. Task characteristics include task complexity and duration. Most generally, according to Eysenck (1982), performance on complex tasks, including those involving problem-solving or requiring creativity, is more likely to be adversely affected by incentives than performance on simple, particularly speed-based tasks, such as response time. This generalization reflects the conclusion that incentives encourage relatively narrow, focused thinking, which may not be optimal for tasks that require cognitive flexibility. Using multiple performance measures is preferable when feasible for assessing effects of incentives. According to Eysenck (1982), when the effects of incentives on two or more aspects of performance are considered, it is common to find improvement on one measure and impairment on another. Finally, individuals differ in the extent to which they are motivated by incentives or frustrated by non-reward. Relevant individual characteristics include both personality traits (e.g., introversion/extraversion) and transient states or moods. Participants may also differ in their response to a particular incentive. For example, the perceived value of monetary rewards may vary according to socio-economic status.

Among the paradigms used to study incentive effects, the dual-task studies most closely resemble the task demands of driving. Most generally, incentives lead to reallocation of attentional resources in dual-task studies, with greater resources invested in the task designated as the main task, and a corresponding decrease in resources to the task designated as subsidiary or secondary (Eysenck, 1983). Incentives are thus likely to improve certain aspects of performance, while degrading others at the same time (Eysenck, 1982). Among studies that consider the interaction between incentive and stressor effects, incentives are found to enhance performance more for sleep-deprived than for non-sleep-deprived individuals (Broadbent, 1971). Finally, for tasks in which participants can trade speed for accuracy, incentives are likely to increase speed and reduce accuracy.

Several experimental studies have shown that transient changes in drivers' motivation can produce changes in driving behavior. In a series of experiments in which participants provided speed choice decisions while viewing static pictures of driving situations, Delhomme and Meyer (1997) examined the effect of transient motivational state on speed selection. They manipulated motivation by deceiving participants about their performance on tasks unrelated to driving. They hypothesized that earlier task failure would lead to perceptions of transient control loss, which in turn would increase participants' motivation to regain control. In turn, the increased control motivation would limit risk-taking, which would be reflected by limited speed choice. Their data supported this hypothesis: Participants in the "failure" group (high-control motivation) chose slower speeds than those in the "success" group (low-control motivation). Delhomme and Meyer (1997) also explored the interaction between transient motivational factors and driving experience. They hypothesized that motivational effects related to loss of control should affect inexperienced drivers in situations with a heavier cognitive load. They found the novices' performances were more dependent on their motivational state than the more experienced drivers, who based their decisions on a more detailed analysis of the available visual information. However, their data were insufficient to determine whether this effect reflected differences between experienced and inexperienced drivers either in the degree to which driving task components are automatized or in the generally greater difficulties novice drivers have regulating their behavior.

Desmond and Matthews (1997) examined the interaction between drivers' motivational state and fatigue-related performance decrements. They manipulated motivational state by providing instructions that the participants' driving was being evaluated during certain portions of a simulator drive and measured the effects of these instructions on driving performance. They found changes in driving performance (heading error) associated with the motivating instruction, but only in the latter part of the driving task and only on straight road (i.e., low-demand) segments. Interestingly, the motivating instruction influenced performance more for subjects in their task-induced fatigue condition.

9.3.3.2 Reward/Penalty Systems in Driving Simulation

Stein, Allen and Schwartz (1978) provided a rationale for the use of monetary reward/penalty systems in driving simulation. Based on their conceptualization, reward/penalty parameters and computational algorithms were incorporated into the STISIM driving simulator software (Rosenthal, Parseghian, Allen, & Stein, 1994). Central to their model is a reward for completion time, which either adds or subtracts a prorated amount of money depending upon the amount of time required to complete a driving scenario, relative to a pre-established criterion. Faster completion times yield rewards, while slower times result in penalties. At the same time, speeding is discouraged by monetary penalties associated with speeding tickets, which are issued based on a probabilistic scheme that can be varied by the experimenter. Crashes are also associated with monetary penalties, which are larger than those associated with tickets. Crashes also influence completion time, because there is a delay during which the simulator is reset and the vehicle must start from a stop.

The conceptual model underlying this reward/penalty system represents speed selection as a tradeoff between two motives: (1) the desire for timely arrival, and (2) the desire to avoid speeding tickets and crashes. Specifically, providing a monetary reward for timely arrival can create a sense of urgency associated with the simulated drive, which can motivate the driver to increase speed. In contrast, monetary penalties for speeding, crashes, or other violations can simulate the costs that deter speeding. Together, these monetary rewards and penalties have the potential to effectively simulate the tension that motivates speed selection in real-world driving. The credibility and effectiveness of such reward/penalty systems would be increased by empirical studies that examined the effects of different parameter values on driving behavior.

Because extrinsically-motivated participants can be expected to restrict their performance to the activities associated with rewards and penalties (Eysenck, 1983), reward/penalty systems will determine the driver's allocation of attentional resources among driving task components. Reward/penalty systems must therefore be sufficiently comprehensive to represent all aspects of the driving task, which the experimenter expects participants to monitor. Michon's (1985) three-level hierarchy, together with the assumption that drivers actively decide how to allocate resources among strategic, tactical, and operational levels of control (Ranney, 1994), can serve as a useful starting point. To maximize ecological validity, this implies that researchers should assign extrinsic incentives to each of the hierarchical levels in proportions that reflect experimental objectives. The speed-selection model described above is consistent with this recommendation. Adjusting the reward for timely arrival addresses the strategic motivation (i.e., trip purpose), while the issuance of speeding tickets motivates tactical (e.g., adequate gap acceptance as in passing) and operational (speed selection) decision-making. Embedding visible enforcement targets into the scenario could be a way to elicit the automatic responses made by speeding drivers when they see a patrol car alongside the road.

9.3.3.3 Issues Relating to the Use of Reward/Penalty Systems

Incentives and automatic behaviors. Aspects of driving behavior or performance that are highly automatic may not be amenable to the effects of performance incentives. The amount of time available between stimulus and response is a key determinant; the more time that drivers are given to decide how to respond to a situation, the more malleable their responses are likely to be. For example, one would not expect a driver surprised by the sudden intrusion of a vehicle to respond any differently in a simulator than in real driving. In contrast, passing decisions, which drivers may have considerable time to plan and execute, would likely be more amenable to such manipulation. Thus, to the extent that operational-level behaviors operate in consistently shorter time frames than either tactical or strategic behaviors, they may generally be less susceptible to the effects of incentives or instructions.

Simulating significant negative outcomes. Human subjects' committees (Institutional Review Boards), responsible for ensuring the safe and ethical treatment of human participants in research studies, will typically not approve of studies that allow participants to sustain significant negative outcomes. This creates a problem for researchers studying driving behavior, who would like participants to attempt to avoid simulator crashes as they would avoid real-world crashes. Attempting to simulate this real-world expectation with a monetary reward/penalty system would require a penalty of significantly greater magnitude than penalties associated with all other errors, such that the unfortunate crash victims would be required to pay significant sums of money to the experimenter. This creates a significant obstacle for the simulation of realistic consequences, because it is highly unlikely that experimenters would be permitted by Institutional Review Boards to collect significant sums of money from participants in research studies. A specific example of an attempt to simulate and enforce realistic consequences associated with crash involvement is presented in the final section.

Multiple crashes. In the real world, a single collision will either terminate a trip or create a delay, the duration of which will depend upon the level of injury and/or property damage. In contrast, many experimental simulation protocols allow multiple collisions, with minimal delay. The rationale for continuing experiments following collisions typically is based on the experimenter's desire to complete all planned data collection, thus avoiding the need to compensate for unbalanced data sets. However, when multiple collisions are allowed in an experimental protocol, it is certainly possible that the subject's behavior following a crash will be different from the behavior observed before the crash. This can create difficulties for data analyses, if the statistical procedure is based on the assumption that behavior during one part of a driving run is independent from behavior during another part of the run. Clearly, the most realistic way of addressing this problem is to terminate the experimental session following a crash. Short of that, researchers can explicitly test for differences before and after crashes; however, if differences are found, alternative analytical strategies may be required. For example, we have used the "time into the run before the first collision" as a summary measure of overall driving alertness. This measure eliminates the effects of the potentially unrealistic behavior resulting from portions of runs in which drivers accumulated a large number of crashes. We found this measure to be more sensitive than crash frequency as a measure of impairment due to fatigue (Ranney, Simmons, Boulos, & Macchi, 2000).

We also found that when participants became significantly impaired, they would sustain multiple crashes within a relatively short time interval. We defined a criterion, based on a certain number of crashes within a specified time interval, and interpreted clusters of crashes satisfying this criterion as reflecting the point in time at which the driver would have stopped driving, either voluntarily or involuntarily, in a real-world setting.

Coercive completion bonus. Human subjects' considerations require that participants be able to terminate participation at any point in an experiment and that no coercion be used to encourage participation when a participant wants to stop. Excessive completion bonuses may be coercive to a participant who faces the conflict between wanting to terminate participation and realizing that perseverance for another hour will yield a significant monetary payoff. Institutional Review Boards must determine what amount of completion bonus is potentially coercive for a given experimental protocol and participant population.

Effects of incentives over time. In an experimental protocol in which participants were required to participate for several days, we observed fairly consistent changes in drivers' attentiveness over time (Ranney & Pulling, 1989). Upon first arrival, the participants were highly attentive and generally appeared to feel that they were in a "testing" environment. This may have been because many of the subjects were elderly and may have been sensitive to the possibility that their skills were being evaluated. However, on the second or third day of participation, participants had clearly determined that there was no significant threat and that the task requirements were more tedious than challenging. At this point, we began to observe increases in apparent lapses of attention. As the participants became more comfortable with the task requirements, they abandoned the hyper-vigilance they initially brought to the experiment. We concluded that the changes in attentiveness were due at least in part to the lack of specificity of the reward/penalty system. Specifically, participants were paid an hourly rate plus a relatively small increment for "acceptable performance." In this study, the incentive increment was most likely too small and ill-defined, since it was not tied to specific outcomes. One possible solution to avoid changes in performance over time is to design experiments that require only a single session.

Assessment of reward/penalty systems. Monetary rewards influence performance, but how does an experimenter know when the rewards and penalties are having the desired effect on driver behavior? The most direct approach is to include reward/penalty parameter values as independent variables in the experimental design and compare performance at different levels. While this will increase the required data collection, the resulting accumulation of information concerning the effects of incentives on driving behavior will help experimenters better understand the ecological validity of their research paradigms. A body of research supporting their use will also improve the credibility of reward/penalty systems.

Non-independence of performance measures. Components of reward/penalty systems may not be independent, which can create problems for statistical analysis. For example, if a delay is associated with penalized outcomes such as crashes, completion time may be correlated with crash frequency. Similarly, analyses using the total amount of money received as a dependent measure may preclude additional analyses of component measures that comprise the overall measure. In this situation, researchers must choose which measure is more consistent with study objectives.

9.3.3.4 Representing Rewards and Penalties With Decision-Making Models

Although classical decision theories may not be appropriate for describing the processes involved in real-world decision-making (e.g., Beach & Lipshitz, 1993), they are useful for structuring simple driving decisions required of participants in experimental contexts. Decision representation requires specification of four basic elements (Lehto, 1997), including: (1) the potential actions (A_i) to choose among, (2) the events (E_j) that occur as a result of the decision, (3) the consequences (C_{ij}) associated with each combination of action and event, and (4), the probability of occurrence (P_{ij}) associated with each combination of action and event. For example, consider the decision whether to stop or continue through an intersection when the traffic signal changes from green to yellow. The potential actions include braking to a stop or continuing through the intersection. There are essentially four categories of events that may occur as a result of these actions, based on a matrix that crosses the decision (stop or go) with the outcome (success or failure). A successful "stop" occurs when there is insufficient time to clear the intersection before the traffic signal changes to red. When there is sufficient time to clear the intersection, the "stop" is unsuccessful, because the driver's progress is delayed unnecessarily. A successful "go" decision occurs when there is sufficient time to clear the intersection. When there is insufficient time to clear the intersection the "go" decision is unsuccessful, because of the possibility of receiving a ticket on being involved in a crash. The consequences associated with the successful decisions include continued progress toward the destination without adverse consequences.

Following Lehto (1997), the expected value (EV) of each action A_i can be calculated by weighting the various consequences C_{ij} over all events j, by the probability associated with each event which follows action A_i. A value function $V(C_{ij})$ is used to transform the consequences C_{ij} into values, which in our example are monetary values. The expected value for a given action then becomes:

$$EV[A_i] = \sum_j P_{ij} V(C_{ij})$$

Subjective expected utility (SEU) theory provides a normative model to represent decision-making under uncertainty (Lehto, 1997). SEU theory emphasizes the distinction between the (objective) value of an outcome, typically expressed in currency, and the (subjective) utility, which reflects the usefulness of the outcome to the individual. While the relationship between value and utility is generally monotonic, it may not be linear (e.g., Tversky & Kahneman, 1981). However, in an experimental situation in which subjects are assumed to be motivated to maximize the amount of monetary reward, their behavior will likely be neither risk-aversive nor risk-seeking with respect to the types of decisions required. In this situation, the value function is linear. Moreover, when the utilities are defined in terms of monetary values, utilities can be assumed to be equivalent to values and therefore SEU is equivalent to expected value (EV) theory. Allen, Stein and Schwartz (1981) represented decision-making at a yellow traffic signal using EV theory, as shown below:

$$\text{SEV(Go)} = V(F|\text{Go}) * SP(F|Go) + V(S|\text{Go}) * SP(S|\text{Go})$$

$$\text{SEV(Stop)} = V(F|\text{Stop}) * SP(F|\text{Stop}) + V(S|\text{Stop}) * SP(S|\text{Stop})$$

where

V is the value of the outcome
SP is the subjective conditional probability of the outcome
F is the outcome fail
S is the outcome success

Allen et al. (1981) used this model to develop risk acceptance functions, which they found to predict driver decision-making at a signalized intersection in an experimental study. The model structure demonstrates the importance of incorporating rewards and penalties for predicting decision-making behavior in driving experimentation. Clearly, if there are no consequences (values) associated with the various decision outcomes, there is no basis for predicting decision-making behavior and thus no reason to expect subjects to prefer one choice over another.

In a separate study, Stein et al. (1978) varied the monetary reward/penalty values associated with tickets for speeding or red-light violations and found that drivers responded by adopting slower speeds and modifying their decision-making at signalized intersections. These results show how variations in reward/penalty structures can influence driver decision-making in experimental settings.

9.4 Examples of Effects of Reward/Penalty Systems in Simulator Studies of Driving Behavior

This section includes two examples of driving simulation experiments that incorporated reward/penalty systems. These studies had slightly different objectives, which necessitated slightly different reward/penalty parameters. Both studies were conducted on a fixed-base driving simulator, based on STISIM simulation software (STISIM, v. 7.03), which was developed by Systems Technology Inc. (STI). Drivers manipulated standard vehicle controls while sitting in a mock-up truck cab. The roadway scene was projected onto a wall-mounted screen. Scenario events and performance measures were different for each experiment. What is instructive about these two examples is just how difficult it can be to identify a reward/penalty system in the driving simulator that corresponds to the system which is functioning in the real world.

Example 9.1: Adaptive Warnings for Collision-Avoidance Systems

The first experiment (Lehto, Papastavrou, Ranney, & Simmons, 2000) examined the effects of different warning system thresholds and visibility levels on drivers' decisions whether or not to pass slow-moving vehicles ahead. Fifteen subjects completed three sessions, consisting of a control run (no warning) and two one-hour driving runs (different warning thresholds). During each run, participants encountered a number of passing opportunities, some of which, if attempted, were very likely to result in a crash, due to the presence of an oncoming vehicle. At the point in time at which the driver was required

TABLE 9.1 Components of Passing Decision-Making Task

		STATE OF THE WORLD	
		Oncoming Vehicle	No Oncoming Vehicle
DRIVER RESPONSE	Attempt to Pass	Miss (M)	Correct Rejection (CR)
	No Attempt	Detection (D)	False Alarms (FA)

Source: Reproduced with permission from Lehto, M. R., Papastavrou, J. D., Ranney, T. A., & Simmons, L. A. (2000). An experimental comparison of conservative versus optimal collision avoidance warning system thresholds. *Safety Science, 36*, 185–209.

to make the passing decision, the information available to the driver concerning the presence of the oncoming vehicle was incomplete, as it might be in a real-world situation with restricted visibility, such as in heavy fog. In particular, the driver could see an object of varying brightness in the distance, but was uncertain about whether or not the object was an oncoming vehicle (three levels of visibility: dim, medium, and bright). A warning system, when present, provided information to the driver concerning the probability that the object was actually a vehicle. The warning display was either a red bar plus an auditory signal, indicating that the warning system had concluded that it was not safe to pass, or a green bar, indicating that it was safe to pass. The driver was required to combine the information available visually with the information provided by the warning system to decide whether or not to pass the slower vehicle ahead.

Participants were instructed to make their passing decisions as if they were driving a real vehicle and to avoid risky passing attempts. There were 51 passing events (the lead vehicle slowed from 60+ mph to 30 mph in a straight section). The probability of an oncoming vehicle was set at 19/51; that of no oncoming vehicle at 32/51. A system of performance incentives and penalties was implemented to elicit behavior that approached realistic driving. Specifically, each participant was given an hourly base pay ($6.00 per hour) for each hour of participation in the study. This amount was not influenced by performance on the task. In addition, each participant was given a daily allotment of $20.00 at the beginning of the session. To this allotment, $0.20 was added for each successful pass and $0.10 was subtracted for each missed passing opportunity. We subtracted $10.00 for each unsafe passing attempt, which typically resulted in a crash. A siren was sounded to indicate issuance of a ticket for unsafe passing and crashing noises (breaking glass, screeching tires) were sounded if a crash occurred, either with an oncoming vehicle or if the vehicle ran off the roadway. The participants were instructed that they would be allowed to keep the total of the daily allotment and the driving performance rewards and penalties, and that this amount would vary between $0.00 and $30.00. Therefore, in effect, their hourly pay could increase from $6.00 per hour to approximately $16.00 per hour. Moreover, because repeated tickets or crashes could result in a negative balance, it was stipulated that, if the subject had a negative balance at the end of any session, the subject would not be allowed to participate further in the experiment. This stipulation was added to the protocol in an attempt to simulate the significant negative consequences associated with a collision in the real world, including the disruption or termination of travel plans.

The components of the rewards and penalties associated with the various decisions and outcomes are represented in a decision matrix in Table 9.1. The expected monetary value (EV) associated with different decision-making strategies under the control condition (no warning) and two warning system conditions (with different thresholds) was calculated using the following equation:

$$EV = (N_M \times R_M) + (N_{CR} \times R_{CR}) + (N_D \times R_D) + (N_{FA} \times R_{FA})$$

where N is the frequency associated with each respective outcome [Miss (M), Correct Rejection (CR), Detection (D), and False Alarm (FA)] and R is the monetary reward/penalty associated with the particular outcome (Table 9.2). For example, with the control condition the optimal decision is to pass only when there is a dim stimulus (no oncoming vehicle) since when there was a medium stimulus a car was present on 2 of 17 trials. This means that there were no misses and 19 detections (the 19 trials in which there was an oncoming car all had a medium or bright stimulus). Of the remaining 32 trials in which there was no oncoming car, there was a dim stimulus

TABLE 9.2 Reward/Penalty Scheme

Driver Behavior	Outcome Category	Amount of Reward/Penalty ($)
Safe no-pass decision	Detection (D)	0
Fail to pass	False Alarm (FA)	−.10
Unsafe passing attempt	Miss (M)	−10.00
Safe Pass	Correct Rejection (CR)	+0.20

Source: Reproduced with permission from Lehto, M. R., Papastavrou, J. D., Ranney, T. A., & Simmons, L. A. (2000). An experimental comparison of conservative versus optimal collision avoidance warning system thresholds. *Safety Science, 36*, 185–209.

in 17 cases (correct rejection; driver passes) and a medium stimulus in 15 cases (false alarm; driver does not pass). Thus, the expected value is:

$$EV = (0 \times -10.00) + (17 \times 0.20) + (19 \times -0.10) + (15 \times -0.10)$$

$$= \$1.90$$

The expected value under the optimal policy gains with the other two warning systems are, respectively, \$4.30 and \$6.40. The larger point here is that individuals could come away with either \$27.90 (\$6.00 + \$20.00 + \$1.90), \$30.30 or \$32.40 if they followed the optimal policy.

Despite these monetary incentives, 5 (33%) out of 15 participants were not permitted to participate beyond the first session, due to their inability to meet the performance criterion of maintaining a positive monetary balance at the end of each session. Discussions with these participants revealed that they typically had adopted decision strategies which would have resulted in disastrous consequences in real-world driving. Specifically, even though they knew there was a chance they could crash in a specific condition and that this might lead to the loss of all incentive pay, they risked this loss in an attempt to earn greater monetary rewards. This finding led us to consider the expectations that participants brought to the experiment. In particular, the population from which participants for this study were drawn tended to include a significant proportion of unemployed people, who were motivated to earn money. One might have expected them to be particularly cautious, given that individuals are usually risk averse in the domain of gains. Yet, they were more willing to take risks. Moreover, we have found that individual participants' situations may change day-to-day, leading some participants to drop out in the middle of a multiple-day study because they have found a longer-term job. Some participants were thus clearly motivated to maximize their single-day pay. This is quite different from the traditional use of young undergraduate students, who are motivated to participate either because of their enthusiasm for psychology or as a course requirement. Nevertheless, one would again expect participants in the current experiment to be risk averse.

This example raises important questions concerning the effects of reward/penalty systems on drivers' simulator behavior or performance. In particular, for those drivers who adopted excessively risky passing strategies we do not know to what extent their behavior reflects an attempt to take advantage of the inherent artificiality of the simulator setting versus the effects of the reward/penalty system alone. One might argue that the reward/penalty system elicited unrealistic behavior; however, it could also be argued that the lack of significantly negative consequences associated with simulator driving motivated the unexpected decision-making. There is no way to know without additional experimentation and unfortunately the funding to address such methodological issues is virtually non-existent. What can be concluded is that anomalous behavior does occur, at least among some participants in simulator experiments. Moreover, the use of the reward/penalty system provided not only a mechanism to identify such behavior, but also a model within which to shape participants' behavior to conform more closely with expectations based on real-world decision-making models. Finally, the elimination of drivers from the protocol provides a means of simulating the significantly negative consequences of a crash with some amount of face validity.

Example 9.2: Effect of an Afternoon Nap on Driving Performance

The second study (Ranney et al., 2000) evaluated the effects of an afternoon nap on overnight driving performance. Eight professional drivers completed two replications of a two-day (43–47 hour) protocol, each including eight hours of overnight driving following a truncated (five-hour) sleep period on the previous night. One replication included a three-hour nap on the afternoon before the overnight driving; the other replication involved overnight driving with no preceding nap. The overnight driving consisted of four two-hour runs, separated by half-hour breaks. The driving task included vehicle control on straight and curved roads, detection of pedestrians appearing alongside the roadway and targets in the mirrors, and avoidance of obstacles and oncoming vehicles.

The monetary reward/penalty system used in this experiment was a two-tiered hierarchical system, with macro and micro components. The macro reward/penalty system had three components. First, the participants were paid a daily rate for their participation in the experiment, including their sleeping time and time when not driving. Second, because the experimental design required participants to complete the (40+ hour) protocol twice, they were given a significant bonus for completing both parts of the experiment. Third, a micro reward/penalty system was in effect during each driving simulator run. Specifically, drivers were rewarded for timely arrival at the destination, which was defined as a specific number of miles. Based on pilot testing, we developed a reference time for the pre-established distance. Drivers were then given \$1.00 per minute (pro-rated) for each minute or portion thereof faster than the reference time in which they completed the drive. They were penalized the same amount for each minute, or portion thereof, they arrived after the deadline. In addition, drivers were penalized for crashes and speeding tickets and were rewarded for each target detected.

The experimental protocol was intended to stress the participants to the point of psychological fatigue and to elicit micro-sleep episodes while driving. The micro reward/penalty structure was intended to motivate truck drivers to complete each two-hour drive under nighttime conditions. The effects of the (micro) reward/penalty structure alone were assessed in a series of pilot studies conducted in preparation for this work. Specifically, we began pilot studies without a reward/penalty structure, using alert drivers not subjected to sleep deprivation. We found that most drivers became drowsy after approximately 30 minutes of simulated nighttime driving; however, we were surprised by how quickly they recovered their alertness when the two-hour drive ended. We then implemented the micro reward/penalty structure, which included the elements described above and found that the pilot drivers maintained their alertness during the entire drive. We concluded that the reward/penalty structure was effective in motivating realistic nighttime driving. We also concluded that the drowsiness observed among the initial pilot drivers had nothing to do with fatigue; rather, it reflected the combination of the uneventful nighttime scenario and the lack of a trip purpose.

When the micro reward penalty structure was used with the sleep deprivation protocol, we found that the macro reward/penalty structure, which included the completion bonus, was successful in simulating the conflicting incentives that motivate truck drivers to continue driving after the point at which they should have stopped driving. As participants became significantly drowsy, they sustained an increasing number of crashes, sufficient to ensure that no monetary reward would be received for a particular run. However, most participants decided to persevere, despite sustaining penalties for crashes and tickets, because they were motivated by their desire to complete the experiment and receive the larger hourly rate and completion bonus.

This example allows several conclusions about the use of reward/penalty systems. First, while the simple (micro) reward/penalty structure was sufficient to motivate attentive driving among alert drivers in a simple protocol, it was insufficient for this purpose among sleep-deprived participants when a longer more complex protocol was used. The macro reward/penalty structure, which combined the micro structure with the significant completion bonus, was necessary to motivate the perseverance that pushed drivers beyond the limits at which normally they might stop driving. Researchers must therefore consider how a reward/penalty system interacts with the other manipulations in the experiment. Second, the behaviors elicited from drivers that were beyond their apparent limits led us to conclude that some portions of the driving trials were not representative of real-world driving. Specifically, we searched for a way to define the point in time at which a driver would have been unable to continue safely, so that we could eliminate behaviors that were not representative of actual on-road driving. For example, we determined the point in time at which the first crash occurred in each two-hour drive and used it as a performance measure. Thus, when reward/penalty systems are in use, researchers need to be vigilant to the possibility that some portion of the recorded behavior will be confounded by multiple crashes or other events that are not sufficiently independent for analytical purposes. Some portion of a balanced design may need to be sacrificed to increase the psychological fidelity of the overall protocol.

9.5 Conclusions

Some amount of artificiality is inherent in all experiments that use driving simulators. An often overlooked component of this artificiality is psychological fidelity, defined as the extent to which the risks and rewards of participation in the experiment correspond to real-world risks and rewards. Experimental studies typically eliminate the trip purpose, which influences all components of real-world driving. It is therefore incumbent on researchers to provide direct guidance to participants concerning their expectations about the simulated trip that they are engaged in. Without such guidance, research participants may have considerable difficulty interpreting the often-used instruction to "drive as you normally would," lacking an appropriate frame of reference. However, providing an artificial trip purpose is likely to be insufficient unless accompanied by specific details and consequences.

In dual-task situations, in which participants are asked to engage in secondary tasks such as a phone conversation or destination entry, researchers must provide clear guidance to help participants determine the relative importance of the component tasks. Failure to provide such guidance can be expected to introduce significant unwanted variability into performance data due to individual differences in the assignment of priorities.

The main tools available for improving psychological fidelity are task instructions and reward/penalty systems. Specification of rewards and penalties in relation to a target completion time can be used to manipulate the urgency of trip completion. Specification of penalties for speeding and other violations using a probabilistic delivery schedule can simulate the potential costs of violating traffic laws. Together, these two mechanisms can create the tension that exists in real-world driving between timely arrival and avoidance of tickets. Institutional Review Boards make it difficult to realistically simulate the consequences of the ultimate negative outcome, namely a crash. The increase in psychological fidelity gained through use of specific instructions and reward/penalty systems may create problems that are inconsistent with balanced designs. Examples include termination of participants who perform below a specified criterion or elimination of portions of a protocol due to multiple crashes.

Data pertaining to the effects of instructions and reward/penalty systems are sorely needed. The increasing preference for naturalistic methods makes it essential that laboratory researchers acknowledge the importance of psychological fidelity and address it directly. The incorporation of psychological fidelity into experimental designs would facilitate the demonstration that it can be manipulated and thus better understood. The resulting accumulation of a body of research results on this topic would better define the tools needed to reduce unwanted variability from research designs while improving the credibility of simulator studies. Approaching psychological fidelity directly would allow researchers to confidently assert the role of driving simulator studies in the quest to understand driving behavior and performance.

Key Points

- Driving simulator studies continue to be criticized for lack of realism, despite significant advancements in the fidelity of the driving experience.
- Some amount of artificiality is inherent in all experiments that use driving simulators. An often-overlooked

component of this artificiality is psychological fidelity, defined here as the extent to which the risks and rewards of participation in the experiment correspond to real-world risks and rewards.

- Experimental studies typically eliminate the trip purpose, which influences all components of real-world driving. Researchers must therefore provide direct guidance to participants concerning their expectations about the simulated trip.
- Failure to define the driving context is likely to result in a significant amount of unwanted variability in performance measures, reflecting a combination of confusion and different assumptions made by participants about the priorities in the experiment.
- Instructions and performance incentives are the main tools available to researchers for improving psychological fidelity of their experiments. Instructions define the performance space; incentives provide guidance about priorities.
- Reward/penalty systems can be used to represent the real-world tradeoff between the desire for safe and timely arrival and the desire to avoid speeding tickets and crashes. Their credibility would be increased if researchers would incorporate reward/penalty system components as independent variables in experimental designs and examine their effects on driving behavior.
- Institutional Review Boards, responsible for ensuring the safe and ethical treatment of human participants in research studies, will typically impose limits on researchers' use of performance incentives and penalties. Excessive rewards (e.g., completion bonuses) may be considered coercive while limits on penalties make it virtually impossible to accurately simulate the significant negative consequences associated with crash involvement.

Keywords: Driver's Motives, Instructions, Psychological Fidelity, Performance Incentives, Risk Perception, Rewards and Penalties

Glossary

Extrinsic incentives: Rewards and penalties associated with different aspects of performance, including money, praise, or food. Extrinsic incentives are typically used in driving simulator studies to motivate the behavior desired by experimenters.

Hierarchical model of driving behavior: Driving behavior consists of concurrent activity at strategic (e.g., trip purpose), tactical (e.g., gap acceptance) and vehicle control (e.g., speed selection) levels. Instructions provided to participants of simulator studies should provide guidance for decision-making at all levels in this model.

Psychological fidelity: Realism of the simulator experience includes the extent to which the risks and rewards of participation in the experiment correspond to real-world risks and rewards.

Reward/Penalty systems: In a driving simulator study, drivers are typically rewarded for timely arrival at a destination and penalized for speeding tickets and crashes. The combination of these rewards and penalties comprises the reward/penalty system.

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