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Laboratory and Field Analyses of Decisions Involving Risk

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The effects of the velocity and distance of an oncoming car on decisions by automobile drivers to cross in front of it were examined in a 4 (Velocity) × 5 (Distance) laboratory experiment and in two different field studies. Judgments of the risk of crossing in front of the car were also obtained in the laboratory experiment. The results from the laboratory simulation indicated that while judged risk was an additive function of velocity and distance, normalized crossing probability was described, in part, by the ratio of perceived distance to perceived velocity. Tests of the external validity of the latter finding suggested that real-world crossing decisions are based on a single dimension: the *temporal* distance between the subject's car and the approaching car. Differences between laboratory and field results supported the view that decision strategies may be task and/or procedure specific and that tests of the external validity of decision models should be incorporated into decision-making research.

A number of investigators interested in the role of perceived risk in decision making have assumed that decision alternatives can be conceptualized as multidimensional stimuli (Payne, 1975; Pollatsek & Tversky, 1970; Slovic, 1967, 1975; Slovic & Lichtenstein, 1971). Several research directions have evolved from this multidimensional approach. One of them has attempted to specify the dimensions that are most important in determining perceived risk (e.g., Payne, 1975). Another has been concerned with describing how subjects process the dimensional properties of the alternatives (e.g., Slovic, 1975; Tversky, 1969, 1972).

A review of the research relevant to the multidimensional approach indicates that only a narrow range of dimensions and choice situations have been explored. The probability of winning and of losing, the amount that can be won or lost, and the expected value and variance of monetary gambles have commonly been studied (e.g., Payne, 1975). Occasionally, more representative decision alternatives have been used (e.g., Slovic, 1975), but the consequences of choosing a particular alternative are often minimal. Presumably, the multidimensional approach to decisions involving risk is not restricted to these few decision alternatives and dimensions. Therefore, it seems important to extend the range of decision alternatives in which the role of different dimensions in determining perceived risk and choice are studied. Thus, one of the purposes of the present research was to investigate the external validity of the multidimensional approach.

Situations in which perceived risk is almost certainly an important dimension of decision making are encountered by most adults when driving an automobile. For example, drivers must decide whether to stop or drive through an intersection after the onset of a yellow light (Konečni, Ebbesen, & Konečni, 1976), whether to cross in front of an oncoming car (Ebbesen & Haney, 1973), and the like. Consider the T intersection presented in Figure 1. The driver in car A, the subject, wishes to make a left turn

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from the stem of the intersection into the flow of moving traffic. To do so the driver must cross in front of cars traveling along the lower part of the crossbar of the T (e.g., car B in Figure 1) as well as merge into the flow of traffic along the upper part of the crossbar. Assuming the upper part of the crossbar is clear, the driver of car A can choose to cross in front of car B (and thus save time) or wait until car B passes before turning. Crossing in front of car B clearly involves greater risk than turning after it has passed by, assuming there are no other oncoming cars.

One would expect the amount of perceived (and actual) risk associated with crossing in front of an oncoming car to vary as a function of a number of dimensions. In particular, the velocity and distance of the oncoming car are likely to affect perceived risk. The acceleration characteristics of the driver's own car might be considered as well as the length of the arc that must be traversed to complete the turn. Although one could argue that dimensions such as the velocity of an oncoming car are conceptually equivalent to more traditional dimensions in research on choice and decision (e.g., the probability of losing a bet), it seems more reasonable to examine directly the relationship between such physical dimensions and perceived risk. The same kinds of questions as are typically asked about the probability of winning or losing a bet can be asked about velocity and distance. For example, one can ask how important velocity and distance are in determining the *risk* that individuals associate with crossing in front of an oncoming car as well as how these dimensions combine to affect the crossing *decision*. In short, theoretically important questions about the role of perceived risk in decision making can be answered using a decision situation that also allows the external validity of any proposed model to be examined directly.

Two types of experiments were conducted. A laboratory simulation of the situation presented in Figure 1 was developed to examine the effect of velocity and distance on both the perceived risk and the probability of crossing in front of an oncoming car and also to investigate the relationship between perceived risk and choice probability. Field studies assessed whether the probability-of-crossing functions obtained in the simulation were comparable to the functions that describe the crossing decisions of drivers in real intersections.

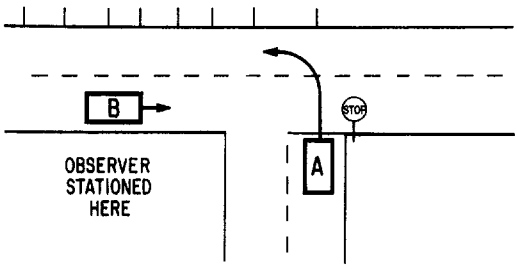

Figure 1. The T-shaped intersection used in the present research. (The rectangle labeled A represents the subject's car, which has come to a full stop and is about to make a left turn. The rectangle labeled B represents an oncoming car.)

Laboratory Experiment

To predict decisions, the multidimensional approach specifies the dimensions that are assumed to be important in the decision and a rule for combining the values of the alternatives on those dimensions. With regard to crossing decisions, we hypothesized that individuals decide whether to cross in front of or behind an oncoming car on the basis of a cut-point rule applied to a subjective risk dimension. If the judged risk of crossing in front of an oncoming car is lower than a specified amount (the cut point), the individual should choose to cross in front of the oncoming car; otherwise, he or she should wait for the car to pass through the intersection before crossing. The risk dimension was related to properties of the crossing situation by hypothesizing that the perceived risk (R) of crossing in front of an oncoming car would be a weighted sum of the perceived velocity (v) and the reciprocal of the perceived distance (d) of the oncoming car, as in the following equation:

$$R = a(v) + b\left(\frac{1}{d}\right) + c, \qquad (1)$$

where a, b, and c are free parameters. It was further assumed that perceived velocity and perceived distance were normally distributed random variables with unit variance.

Several testable predictions follow from the above model. Considering risk judgments (R) first, it follows from Equation 1 and the assumption that perceived velocity and distance are normally distributed that perceived risk judgments should also be normally distributed. In addition, velocity and distance should combine in an *additive* fashion to determine mean judged risk. With regard to crossing decisions, if perceived risk is a normally distributed random variable and a cut-point decision rule is used, it follows that some velocity and distance combinations ought to produce such high risk estimates that, for all practical purposes, individuals will never cross; whereas for other combinations of velocity and distance, the risk should be so low that individuals will always cross in front of the oncoming car. More interesting is that the mean perceived risk (for given velocity and distance combinations) should be linearly related to the normalized probability of crossing (for corresponding velocity and distance combinations). Finally, if the postulated cut-point rule and Equation 1 apply, normalized crossing probabilities should also be an additive function of velocity and distance.

The above predictions were assessed by presenting individuals with velocities and distances of an oncoming car in a fully crossed, within-subjects factorial design. The individuals first repeatedly decided whether to cross in front of oncoming cars varying in velocity and distance and then, in a separate series of replications, judged the risk of crossing given the same velocity and distance combinations. The interaction terms in appropriate analyses of variance of these two data sets were examined. Equation 1 predicted that the interaction term should be nonsignificant in both cases.

Method

Subjects. The subjects were 10 male and 10 female undergraduate students at the University of California, San Diego, who participated for course credit. All subjects had a current driver's license.

Apparatus. The apparatus consisted of a 160 × 50 cm table on top of which two model wood cars traveled in specially designed tracks. One track (40 cm long and parallel to the shorter sides of the table) was traversed by the subject's car (a 6 × 10 cm block of wood) at a constant velocity (25 cm/sec) in 1.6 sec. The movement of this car (always away from the subject) was activated by a hand-held switch; however, once activated, the car continued moving, independent of further actions by the subject. The oncoming car (also a 6 × 10 cm block of wood) traveled along another track (120 cm in length) parallel to the longer sides of the table and thus perpendicular to the track along which the subject's car traveled. One end of the longer track bisected the subject's track to form a T in which the side of the crossbar to the subject's left was much longer (110 cm) than the one to his or her right (10 cm). Given this arrangement, the subject's car had to travel 30 cm before passing through the entire intersection (20 cm to reach the intersection and 10 cm more to pass through it).

The movements of both cars were controlled by motors that were concealed from view. Because the torque generated by these motors was considerably greater than the friction produced by the movements of the cars in their tracks, the velocities were, for all practical purposes, reached instantaneously. The velocity of the oncoming car was set at one of four predetermined levels: 24, 42, 60, or 90 cm/sec. The two extreme velocity levels were designed to serve as 0 and 1.0 probability of crossing anchors, since at 90 cm/sec a crossing could not be made safely at even the farthest distances, whereas at 24 cm/sec a safe crossing could be made at all distances. In addition, by randomly interspersing these trials in between the two middle velocities, subjects were discouraged from adopting a decision strategy that was insensitive to the stimulus conditions. Finally, the use of these extreme levels also helped stimulate real-world conditions by presenting the subjects with a range of velocity and distance combinations, which included extremely risky and very safe values.

The starting distance of the oncoming car was set by the experimenter so that the front of the car was 40, 55, 70, 85, or 100 cm away from the intersection of the two tracks. Accidents were signaled by an actual automobile horn. If the oncoming car reached the intersection before the subject's car had traversed the 30 cm required to pass through the intersection, the horn automatically sounded for 2 sec. Subjects reported that this noise was quite aversive. If the subject's car safely crossed the intersection, or if he or she chose not to attempt the crossing on a particular trial, the horn remained silent.

Procedure. Upon arrival in the laboratory, subjects were seated directly in front of the track along which their car traveled, such that their car moved away from them, and the oncoming car ap-

proached from their left. The automobile horn was placed on the opposite side of the table facing the subjects. Subjects were told that the study was concerned with driver behavior, and they were encouraged to think of the objects as real cars and of the apparatus at representing a real T intersection.

The start of a trial was signaled by the experimenter speaking the word READY. The subjects were instructed to look to their left and visually fixate on the oncoming car. After 2 to 3 sec of fixation, the oncoming car began to move at one of the four velocities. The subjects were told to decide whether it was safe enough for their car to cross through the intersection in front of the oncoming car. If, after seeing the distance and velocity of the oncoming car, they felt their car could cross through the intersection without a collision occurring, they were to press the hand-held switch. Otherwise, they were to let the oncoming car go through without responding. Between each pair of succeeding trials, the starting distance of the oncoming car for the next trial was set by reversing the movement of the car and driving it backward along the track to the appropriate starting point. Subjects were told to fixate on the crosspoint of the intersection while these intertrial adjustments were made.

Following a demonstration of the equipment and the hand-held switch, the horn was sounded several times and its purpose explained. Each subject then had 20 practice trials involving all treatment combinations of the 4 (Velocity) × 5 (Distance) experimental design. Upon completion of these practice trials, subjects were exposed to four more replications of the full design. To control for order effects, the cells in the design were presented in different random orders across replications and subjects. For each trial, the experimenter recorded whether the subject had attempted to cross the intersection and, if so, whether an accident had occurred.

After a brief rest, subjects were told that the second half of the experiment was concerned with judgments of the amount of risk associated with driving decisions and that they would be judging the risk involved in crossing in front of approaching cars. Subjects made risk judgments on continuous 200-mm scales anchored by "extremely risky" and "not at all risky." To familiarize subjects with the use of the rating scale, 10 cells of the same 4 × 5 matrix used in the decision task were randomly presented. On each trial, subjects were asked only to judge how risky it would have been to have crossed in front of the oncoming car and were not required to decide whether to cross. Immediately following these practice trials, subjects were presented with two complete replications of the same 4 × 5 design used in the decision part of the study. Again, the various combinations were randomized over replications and subjects. The same trial-by-trial procedures were used in this task as in the previous one with the exception being that subjects judged risk rather than deciding whether to cross.

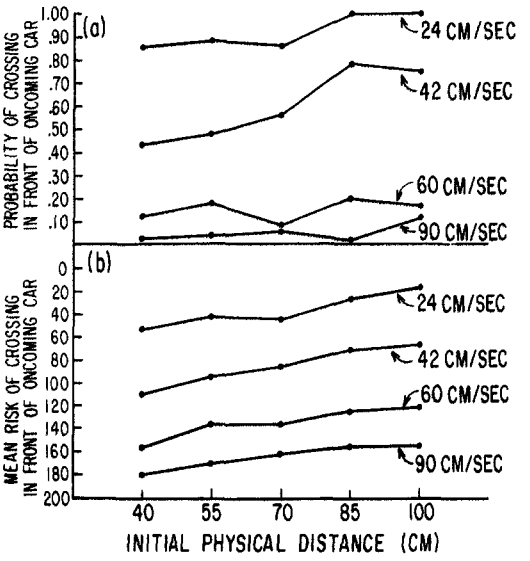

Figure 2. The mean risk and probability of crossing as a function of velocity and distance. (Panel a presents the mean probability of crossing in front of an approaching car as a joint function of the velocity and distance of that car. Panel b presents the mean risk (on a 200-mm scale) associated with crossing in front of the same oncoming car values as in panel a.)

Results and Discussion

Perceived risk. Equation 1 predicts that velocity and distance should combine additively to determine perceived risk. The effects that velocity and starting distance had on the judged risk of crossing in front of the approaching car can be seen in Figure 2b. As expected, both velocity and starting distance affected perceived risk. The faster the approaching car travelled and the closer its starting distance, the greater was the amount of judged risk. Two Sex × Velocity × Distance analyses of variance of the risk ratings were performed. One used data from all four velocity levels, whereas the other—for easier comparison with the crossing results presented later—used only the two middle velocities. In both analyses, the main effects of velocity, $F(3, 54) = 302.06$, $p < .001$, and $F(1, 18) = 97.56$, $p < .001$, and of distance, $F(4, 72) = 46.30$, $p < .001$, and $F(4, 72) = 24.18$, $p < .001$, were highly significant. More importantly, the interaction between these variables was not significant in either

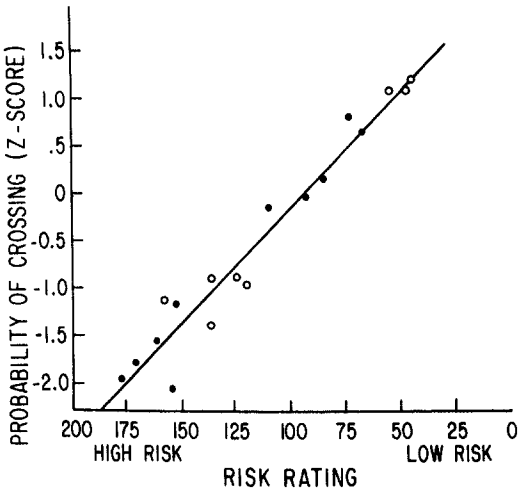

Figure 3. The normal deviate of the crossing probabilities over subjects and replications as a function of the mean risk in each cell of the factorial design. (The two cells in which the mean probabilities were 1.0 are excluded. For ease of inspection, the 90- and 42-cm/sec velocity levels are plotted as filled circles and the 60- and 24-cm/sec levels as open circles.)

analysis, $F(12, 216) = 1.75$, $p = .058$, and $F(4, 72) = .1.20$, *ns,* respectively. In other words, as predicted by Equation 1, perceived velocity and distance seemed to combine *additively* to determine average risk.

The risk ratings were analyzed further to determine whether they satisfied the normality assumption of the model. The mean of each cell of the 4 × 5 factorial design was subtracted from each subject's score in the associated cell. A 10-equal-interval cumulative distribution of the resulting deviation scores for all of the cells was constructed. A chi-square test of fit to a best-fitting normal ogive suggested that the risk data were distributed in an approximately normal fashion, $\chi^2(9) = 7.92$, $p = .46$.

Probability of crossing. The above analyses offer strong support for Equation 1. As predicted, perceived velocity and distance combined in an additive manner to determine perceived risk. A second aspect of the model was that it related crossing decisions to perceived risk via a cut-point rule. To the extent that this latter part of the model is correct, mean risk for the various velocity–distance combinations should be linearly related to the normalized crossing probabilities for the same combinations, and velocity and distance should add within limits to determine those normalized crossing probabilities.

In reference to the first prediction, Figure 3 presents a plot of the normal deviate of crossing probability (computed over subjects and replications) as a function of mean risk for all of the cells in the factorial design (except those two in which the probability of crossing was 1.0). As can be seen, the data are well fit by a linear function, $r(16) = .97$. This analysis supports the interpretation that the crossing decisions were based on a cut-point decision applied to the perceived risk dimension.

The separate effects that velocity and starting distance had on the relative frequency with which subjects decided to cross in front of the approaching car can be seen in Figure 2a. The lower the velocity of the oncoming car and the greater its starting distance, the more likely subjects were to cross. Furthermore, when velocity of the oncoming car was 90 cm/sec, the average probability of crossing was very low at all distances; however, when the speed was 24 cm/sec and crossings could be safely made at all distances, the average probability of crossing was very high. In fact, at distances of 85 and 100 cm all the subjects crossed on every trial. If we consider these two velocities (24 cm/sec and 90 cm/sec) as adequately representing 100%- and 0%-turn boundaries, respectively, the two middle velocities can be used to determine whether velocity and distance combine additively within these boundaries.

Several different methods were used to test the additivity prediction. In one analysis, each subject's probability of crossing in a given cell was transformed to its normal deviate.[1] A 2 (Sex of Subject) × 2 (Middle

[1] With four trials per subject per cell, the probabilities were 0, .25, .50, .75, and 1.00. Since normal deviates for 0 and 1.00 are $-\infty$ and $+\infty$, respectively, a strategy for selecting analyzable z scores had to be devised. In one strategy, two-decimal place precision was assumed, and the values .01 and .99 were arbitrarily substituted for 0 and 1.00, re-

Velocities) × 5 (Distance) analysis of variance (the latter two factors being within subjects) was then performed on these transformed data. The main effects of distance, $F(4, 72) = 8.50$, $p < .001$, and of velocity, $F(1, 18) = 130.04$, $p < .001$, were both highly significant. Contrary to the prediction, however, the interaction between these two factors was also significant, $F(4, 72) = 4.29$, $p < .01$. No other effects were significant.

The significant interaction reported above could be explained if the subjects were dividing perceived distance by velocity rather than adding the two subjective quantities.[2] If so, a Linear × Linear contrast (Winer, 1971, pp. 388–391) based upon the spacing of the main effect normal deviate means (Anderson, 1974) should account for all of the interaction variance in the above analysis, and this was indeed the case, $F(1, 72) = 9.92$, $p < .01$. The residual was negligible, $F(3, 72) = 1.86$, $p = .14$.[3]

In a somewhat different approach to the analysis of the crossing decisions, probabilities were generated for each cell of the design by computing the frequency of crossing over all subjects of the same sex. A 2 (Sex of Subject) × 2 (Middle Velocities) × 5 (Distance) analysis of variance with one observation per cell was then performed on the normal deviate transformation of these cell probabilities. Using the three-way interaction as an estimate of error, velocity and distance were again found to interact significantly, $F(4, 4) = 7.00$, $p < .05$. A Linear × Linear contrast based on the main effect means accounted for a significant ($p < .05$) portion of that variance, residual $F(3, 4) = 5.98$, *ns*.

Several interpretations of the failure of Equation 1 to account for the choice data can be generated. Given the excellent fit of the dividing rule, one interpretation is that the decision to cross may be based on the perceived *temporal* gap (i.e., the perceived ratio of distance to velocity) between the oncoming car and the intersection. That is, the subjects may have been applying a cut-point decision rule to a subjective temporal-gap dimension rather than to a risk dimension. Another possibility is that the subjects may have been generating separate estimates of distance and velocity and then combining these according to a dividing rule.[4] These interpretations will be examined further after the results from the field studies have been presented.

Risk and crossing decisions. Despite the high correlation between judged risk and normalized crossing probability shown in Figure 3, crossing decisions and risk judgments do not seem to be affected by velocity and distance in exactly the same way.[5] Subjects seem to have decided whether to cross by combining distance and velocity in an

spectively. Their z scores (±2.33) were then used in the analysis. In another strategy, the lowest and highest probability of crossing over in the 20 cells being analyzed (namely, .05 and .80) replaced each subject's 0 and 1.00 scores, respectively. In a third strategy, extremely large z values were substituted for 0 and 1.00 scores (implying precision of probability estimates greater than 10^{-100}), namely ±30. Since the conclusions were identical for these different analyses, only the F values for the first are reported in the text.

[2] This dividing rule should not be confused with one that assumes that subjects divide the *actual* distances and velocities to arrive at a decision. In the present case, the transformation from actual distance and velocity to the subjects' subjective representations is left unspecified. Therefore the ratio of actual distance to velocity should not necessarily predict crossing probability.

[3] For the case in which the 0 and 1.0 scores were assigned z values of ±30.0, $F(4, 72) = 3.71$, $p = .01$. In addition, the Linear × Linear contrast also accounted for a large portion of this variance, the residual $F(3, 72) = 1.41$, $p = .24$. A Linear × Linear contrast also accounted for the significant interaction when the z values for .05 and .80 were substituted for the 0 and 1.0 probabilities, respectively.

[4] These two explanations differ in that the former assumes subjects directly perceive temporal gap, while the latter assumes subjects first estimate distance and velocity and then divide these separate estimates to form the relevant decision dimension. These two views predict the same results only when the transformations between perceived and actual distances and velocities are assumed to be at least proportional ones.

[5] In the present case, this can be seen by plotting separate linear functions for each velocity level in Figure 3 and noting that the separate slopes differ even though the overall correlation is large.

interactive fashion, whereas they estimated the risk of crossing by combining the perceived velocity and distance in an additive manner. While Equation 1 was supported, the results did not support the idea that subjects decided to cross by determining whether the subjective risk of crossing (as defined by the risk ratings) was lower than a cut point.

It is possible to explain the differences between the risk and crossing results in several ways. The decision model can be generalized in a manner similar to that in which Tversky (1969) has generalized utility models. In particular, we can assume that the probability of crossing is some unspecified function of the weighted sum of perceived velocity and distance. To account for these results, we need merely assume that the unspecified function is logistic when subjects are deciding whether to cross and linear when subjects are judging the associated risk. Another interpretation suggests that the risk response scale should be transformed. For example, an exponential transformation would make the risk results more compatible with the decision results.

While both of the above explanations allow us to retain the original risk–cut-point model, it is equally plausible that the risk ratings were determined by one decision strategy and the crossings by another. There are several reasons to prefer this interpretation over the previous ones. First, the two tasks differed in a number of potentially important respects. The ratings were made on a continuous scale under little or no time pressure *after* the oncoming car had traveled through the entire intersection. On the other hand, the crossing decisions were of a go–no-go variety and had to be made under extreme time pressures *before* the oncoming car even reached the intersection. Thus, the cues upon which the risk rating was based could well have been different from the cues used in the decisions to cross.

A second reason to prefer the separate strategy interpretation comes from a comparison of the results for the *actual* risk of an "accident" occurring with the results obtained from the risk ratings and crossing decisions. The probabilities of an accident actually occurring given that a crossing was attempted were .90, .93, .71, .88, and .83 at each of the five distances (from 40 to 100 cm) when the oncoming car was traveling at 60 cm/sec, and they were .91, .62, .42, .19, and .08 when the oncoming car was traveling at 42 cm/sec.[6] Note that the slope is much steeper in the latter velocity condition than in the former. Thus, while the mean rated risk estimates were parallel in these two velocity conditions (see Figure 2b), the actual risks of an accident occurring were diverging. Apparently, rated risk was not a simple linear function of actual risk. On the other hand, the pattern of the crossing data was similar, although not identical, to the pattern of the actual risk data. This similarity is consistent with the idea that the crossing decisions were based on cues related to the actual probability of an accident occurring, such as the perceived distance and velocity, rather than on the subjective risk estimates obtained from the risk-rating study. In other words, while perceived risk may have been a cue used in the crossing decisions, it seems likely that the risk ratings were not representative of the perceived risks in the decision part of the study. Different decision rules seems to have been used for risk ratings and crossing decisions.

The idea that minor, theoretically unimportant features of a task may influence the results one obtains has been suggested by others. Olson (1976) reinterpreted Kahneman and Tversky's (1972) "representativeness" findings by suggesting that their results may have been due to features of their tasks which were not specified a priori by their model. Furthermore, Payne (1975) found that the dimensions subjects use to judge the risks associated with monetary gambles are different from the dimensions they use to choose between gambles. In short, there is independent evidence to support the idea that slight variations in irrelevant task features may cause the subjects to employ different decision or judgment strategies.

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[6] The probabilities of an accident given a crossing attempt were all 1.00 in the 90 cm/sec velocity condition and .06, .00, .00, .00, and .00 in the 24 cm/sec condition.

Field Experiments

A major reason for selecting the T-intersection decision task rather than, for example, a monetary or a hypothetical social decision task, was that the decisions in this situation could be easily studied in naturalistic settings. While it is often assumed by researchers interested in decision making that their models have direct applicability to the way in which decisions are made in the real world, few have actually attempted to assess the generality of their conclusions in extralaboratory settings. Yet, it is quite reasonable to require that the same stringent standards used to evaluate the internal validity of laboratory experiments be applied when evaluating their external validity (Campbell & Stanley, 1963). A second study was therefore conducted in order to determine the generality of the findings obtained in the decision part of the laboratory experiment.

Method

The data for this study were obtained by unobtrusive observation of the behavior of drivers making left turns and of the velocity and distance of the relevant oncoming cars. By measuring both the velocity and the distance of an oncoming car and recording whether a subject turned in front of it, it was possible to determine the effects of velocity and distance on turning probability in the field.

Subjects. The subjects were 2,058 male and female drivers who made left turns into the flow of traffic at four different T-shaped intersections in San Diego county.

Procedure. Two different procedures were used to collect relevant data. One employed a between-intersections design and the other a within-intersection design. In the between-intersections procedure, three T-shaped intersections (all with stop signs for drivers in the stem of the T, but not for drivers in the crossbar of the T) were selected because they differed with regard to the average velocity of the flow of traffic along the crossbar of the T. An obvious problem with this design was that driver characteristics might have been confounded with intersections and therefore with the velocity factor. In order to control for this problem, a within-intersection procedure was also used; here we studied a single T-shaped intersection in which there was considerable variability in the velocity of vehicles moving along the crossbar of the T. In this procedure, naturally occurring velocities and distances were classified into several categories.

1. Between-intersections procedure. The mean velocities of traffic along the crossbar of the T in the three intersections were 25, 38, and 45 mph, respectively. The standard deviations around these means were quite small, the largest value for a given day of observation being 4.8 mph. Thus, there was little overlap of the velocities across the three intersections.

A highly trained observer started a stopwatch at the moment a subject's car (always a left-turning car) came to a full stop at the crossbar of the T. The observer stopped the timer when the first car coming from the subject's left reached the middle of the intersection. This time interval was defined as the temporal gap. The observer also noted whether the subject crossed in front of the oncoming car and whether another car had been approaching the intersection from the subject's right. Only those cases in which no car was approaching from the right were used. In this way, only one temporal gap and turn–no-turn observation was obtained per subject. Distances were derived from the temporal-gap values and the average velocity for a given intersection. A total of 1,507 usable observations were obtained.

2. Within-intersection procedure. In the within-intersection procedure, distances were measured directly, while the velocity of each oncoming car was derived. White sticks were positioned unobtrusively next to the roadway along the crossbar of the T in a manner that controlled for the effects of parallax. The sticks were placed so that a correctly positioned observer would see them as being 0, 15, 30, 45, 60, 75, 90, and 105 yd. (0, 13.7, 27.4, 41.2, 54.9, 68.6, 82.3, and 96.0 m) away from the middle of the intersection. The observer assigned oncoming cars to the midpoints of the above distance intervals (e.g., 7.5 yd. for the 0–15-yd. interval).

The observer was positioned so as to have a clear view of both the stem and the crossbar of the T (see Figure 1). When a subject's car came to a halt at the stop sign in the stem of the T, the observer started a stopwatch and immediately scanned the left side of the crossbar, noting the distance of the oncoming car nearest to the intersection. The moment the front bumper of that car reached the 0-yd. mark, the observer stopped the timer. If the subject had not turned in front of this oncoming car, a second stopwatch was activated at the same moment the first was stopped. The observer again examined the left part of the crossbar of the T and noted the distance of the next oncoming car that was nearest to the intersection. This cycle continued until the subject made the left turn. In this way, both the distance and the temporal gap were recorded for each oncoming car, and several observations were sometimes obtained for a particular subject. As with the between-intersections procedure, when a car was approaching the intersection from the subject's right, the observation was not used. In addition to temporal gap and distance, the observer also recorded the sex of the driver. The velocity of each oncoming car was derived from the temporal-gap and distance measurements. A total of 843 usable observations were obtained.

An important aspect of the intersection used in this procedure was that a driver's view of cars coming from his or her left was obstructed by a building until the driver's car reached the stop line painted on the roadway. Thus, like the subjects in the simulation, the drivers had little prior information about the cross traffic until they reached the intersection and a decision would be made. The intersections studied in the between-intersections procedure did not have this property.

Results and Discussion

Of major interest in this study was the way that the velocity and distance of approaching cars combined to determine real-life turning decisions. If subjective estimates of velocity are divided into subjective estimates of distance to determine the normalized probability of turning (as the results of our laboratory study implied), and the functions relating the physical scales to their subjective representations are proportional, then a normal ogive function relating probability (or a linear one relating normalized probabilities) and temporal gap should be obtained.

Figure 4 presents the probability of turning as a function of temporal distance. In the left panel are the results of the within-intersection procedure. Those data points connected by a line represent oncoming cars that were in the same physical distance category but that differed in velocity and therefore temporal gap. The most important features of these results are the high degree of overlap among the curves and the fact that they approximate a normal ogive. When the probabilities were normalized (leaving out undefined values of 0 and 1.0), they were well fit by a single linear function, $r(17) = .94$. The right panel of Figure 4 presents the results from the between-intersections procedure. In this case, the data points connected by a line represent oncoming cars that were in the same velocity category (i.e., the same intersection) but that differed in initial physical distance and therefore temporal gap. Once again, the two most notable features of the results are the high degree of overlap among the curves and the fact that a single linear function fits the normalized data very well, $r(13) = .97$.

It is important to note that although these two procedures were quite different in a number of respects, the patterns of results are remarkably similar. Whether categorized by distance or velocity, the results indicate that the probability of turning in front of an oncoming car in the real world is consistently and simply related to the duration of the temporal gap.

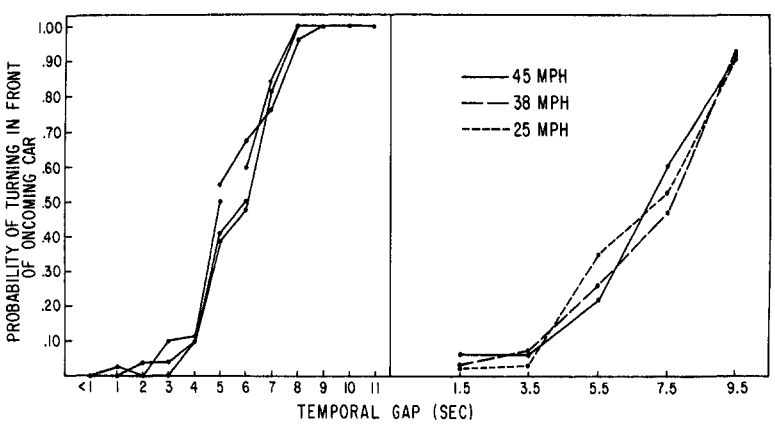

Figure 4. The probability of turning as a function of temporal distance. (The left panel presents the probability of turning in front of an approaching car [the within-intersection procedure] as a function of temporal-gap categories. Each line connecting a set of data points represents the data obtained for oncoming cars that were at the same initial distance, but that differed in velocity. The right panel also presents turning probabilities as a function of temporal-gap categories [the between-intersections procedure]; however, each line connecting a set of data points represents cars at different intersections.)

Although the above analyses are quite consistent with the laboratory results, they do not test the fit of the dividing rule that was found to hold for the laboratory data. Therefore, Figure 5 presents the normalized probability of turning as a function of the distance to the nearest oncoming car for several levels of velocity and for both data-gathering procedures. The functions clearly form a diverging fan, which is exactly the pattern predicted by the dividing rule. (The nature of the data made a quantitative test unwise.) In summary, the form of the velocity and distance effects and the extremely high degree of overlap between the curves in Figure 4 are both consistent with the view that temporal distance (or a simple dividing rule) determines the probability of turning.

Comparison of Field and Laboratory Results

Up to this point, the crossing results from the laboratory and the turning results from the field studies have converged. In both cases, the data seem to be well described by a model that assumes that subjects made decisions by estimating whether the perceived temporal gap was above or below some cut point. However, it is possible that subjects were forming separate estimates of velocity and distance and then combining the two quantities in a manner isomorphic with estimating temporal gap directly. Although velocity and distance can be thought of as separate dimensions, it is unclear which perceptual dimensions subjects were using in deciding whether to cross in front of an oncoming car.

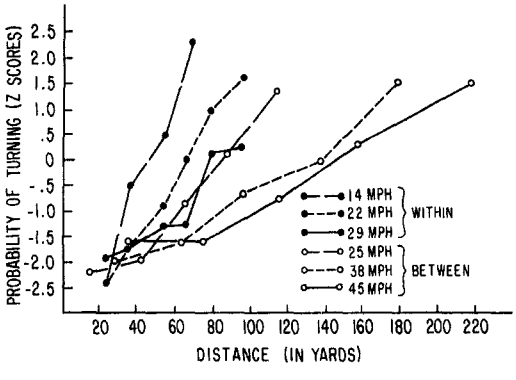

Figure 5. The normal deviate of the probability of turning as a function of the velocity and distance of an oncoming car. (Data for the within-intersection and between-intersections procedures are combined.)

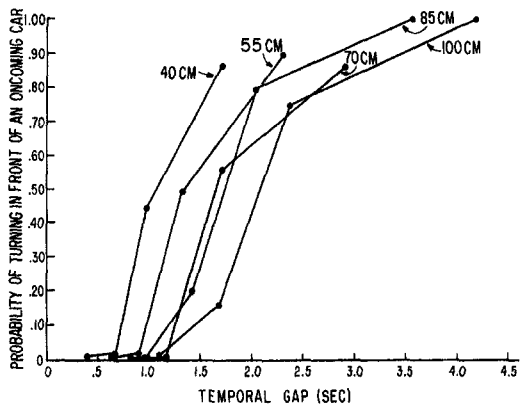

Figure 6. Probability of crossing as a function of temporal gap for the laboratory experiment. (Each set of connected data points was obtained from a given initial distance of the oncoming car in the laboratory simulation. The results should be compared to the left panel of Figure 4.)

The temporal-gap model and the separate-estimates model yield the same results (when plotted against temporal gap) only if the functions relating velocity and distance to their subjective counterparts are proportional. Therefore, it would be possible for subjects to take the ratio of *subjective* estimates of distance and velocity without the resulting data being well described by a single function relating actual temporal gap and turning probability. Figure 4 has already demonstrated that this does *not* occur in the field: A single temporal-gap function fit all of the physical distance and velocity data. However, when separate probability-of-crossing by temporal-gap functions were plotted (in Figure 6) for each initial distance used in the laboratory study, the resulting curves were not as well described by a single function, $r(16) = .82$, as were the field data. The correlations for the field data from both the within-intersection and the between-intersections procedures were significantly larger ($p < .05$) than that for the laboratory data. Apparently, in the laboratory simulation the main effects of velocity and/or dis-

tance were larger than would be expected if subjects were deciding on the basis of temporal gap alone.[7] Even though the more general dividing model seemed to fit the data from both studies, the present analysis (Figure 6) suggests that the subjects in the laboratory simulation were computing separate estimates of velocity and distance and then combining them to make a decision, while subjects in the field were basing their decisions directly on perceived temporal gap. Thus, the field turning results were not in complete agreement with the laboratory crossing results.

Additional differences between the laboratory and field results emerged when the sex-of-subject effects were examined. Sex of subject had no effect on the crossing functions in the simulation (all Fs < 1). On the other hand, in the field experiments, male drivers consistently turned at shorter temporal gaps than female drivers. An analysis of variance of the arcsine-transformed proportions yielded a significant main effect of sex for the within-intersection data, $F(1, 7) = 14.59$, $p < .01$. The tendency for females to be more conservative than males in various real-life driving situations has been found in a number of studies (e.g., Ebbesen & Haney, 1973; Konečni et al., 1976; Leff & Gunn, 1973; McNeil & Morgan, 1968). In short, something about the simulation eliminated an otherwise strong sex effect. Although the age of subjects might provide a reasonable explanation, the sex-of-driver effects have been found to be even more pronounced with young drivers than with older ones (Konečni et al., 1976).

General Discussion

The findings of the laboratory simulation indicated that judgments of the amount of risk involved in crossing in front of an oncoming car were affected differently by the velocity and distance of the car than were decisions to cross or not to cross in front of it. The former appeared to be based upon an additive combination of the two factors, whereas the latter were based upon a dividing rule. This difference in combination strategies existed even though the two measures were highly correlated overall. The possibility that a high correlation can be found even though different combination strategies are involved makes salient the inadequacy of tests of fit that rely exclusively on correlational procedures (cf. Birnbaum, 1973, and Ebbesen & Konečni, 1975, for additional discussion of this point).

The fact that subjects used different combination rules on the two response dimensions can be explained in several ways. As we pointed out earlier, the hypothesis that the crossing decisions were mediated by risk can be retained by invoking the necessity for additional transformations of the risk data. Alternatively, the original model can be rejected in favor of a view that assumes that the crossing decisions and the risk judgments were generated by independent, task-specific strategies. Even though velocity and distance were important dimensions in each task, the crossing decisions did not seem to be based upon the risk estimates obtained from the rating task. Instead, the subjects seemed to have made their decisions by taking the ratio of perceived distance and perceived velocity and applying a cut-point rule to the result. This strategy made the decisions more closely related to the actual risks than the rated risks.

To the extent that the several procedural differences between the risk and crossing tasks caused subjects to use different decision strategies (see Slovic, 1972, for similar results), it is possible that the laboratory simulation results were a reflection of theoretically unimportant features of the two tasks rather than more basic, underlying decision processes. If minor features of more commonly used decision tasks (e.g., pie-shaped diagrams or bar graphs) also play a

[7] One way this can happen is if the transformation from actual to perceived velocity or distance involves an additive, as well as or instead of, a proportional constant. In such a case, a plot of the actual ratios against the subjective ones would take on a form similar to that in Figure 6. In other words, the subjects in the simulation seemed to compute separate subjective estimates of velocity and distance, which were approximately linearly but not proportionally related to the actual velocities and distances.

role in the decision strategies that subjects use, then our theories must be revised to specify a priori which of the many task features are of theoretical importance (Olson, 1976), and we should require that our conclusions from studies of decision making be externally validated by conducting field research in appropriate settings.

In our attempts to assess the generality of the findings from the laboratory simulation, we found that even though a dividing rule could be used to describe both the laboratory and field results (see Figures 2a and 5), the real-life crossing decisions were, in addition, well described by a model that assumed that individuals decided whether to cross on the basis of the actual temporal gap (see Figure 4), whereas the laboratory crossing decisions could not be described by such a model (see Figure 6). Instead, the laboratory subjects seemed to generate separate subjective estimates of distance and velocity and then combine them according to a dividing rule. Thus, the field results did not completely validate the laboratory findings.

The fact that the temporal-gap model, which is a single-dimension rather than a multiple-dimension decision model, fits the results from both field studies may mean that unidimensional decision strategies are commonly employed by drivers. By attending to one real or perceptual dimension that incorporates or is correlated with several physical dimensions, drivers may be able to save time by avoiding decision strategies that require that several separate dimensions be cognitively combined together to make a decision. Obviously, the time taken to decide whether to act is of considerable importance in most real-world driving decisions. For example, Konečni et al. (1976) found that drivers often made wrong decisions (those decisions increasing the likelihood of an accident) by failing to take into account the time required to *decide* whether to stop for the yellow light. If their decisions had taken less time, the probability of crossing through a red light would have been reduced.

It is possible that people employ unidimensional decision strategies in all situations where decisions involving important consequences must be made under time pressure. Several studies have been reported that suggest that time pressure and/or arousal reduces the number of dimensions or cues that enter into a decision process (Konečni & Sargent-Pollock, 1976; Wright, 1974). Given the results from the present research, however, it is likely that time pressure is not the only factor contributing to the unidimensional nature of the decision strategy that was apparently used by the subjects in the field. The laboratory subjects were under greater time pressure than the field subjects, yet the former seemed to use a strategy with more dimensions than the latter. It may be that unidimensional decision strategies are more common in real-world settings than previously suspected, regardless of the time pressures imposed on the decision maker. For example, in a study on legal decision making, Ebbesen and Konečni (1975) found evidence indicating that even though judges reported taking many different dimensions of a case into account when setting bail, approximately 90% of the variance in actual bail-setting decisions could be accounted for by one dimension. Although the judges intuitively believed they were using a multidimensional strategy, they were primarily responding to only one dimension. While additional field research in new settings is clearly required, the prevalence of unidimensional, rather than multidimensional, decision strategies may be underemphasized by the current theoretical interest in the multidimensional approach.

One explanation for the presence of what appeared to be a multiple-dimension decision strategy in the laboratory simulation and a single one in the field is that subjects in the simulation were exposed to a full factorial design, whereas those in the field were not. It is conceivable that the laboratory subjects were forced to form separate estimates of velocity and distance in deciding whether to cross, because the design created an artificial orthogonal relationship between the two variables (Ebbesen & Konečni, 1975). Information about the level of one variable could not help in the estimate of the level of the second variable. This independence of distance and velocity may not exist in real-world driving situations. Drivers of fast-

moving cars may tend to space their cars differently than drivers of slow-moving cars, which would cause a confounding between distance and velocity. Even if such a confounding were small or nonexistent, it is still true that drivers are simply not exposed to the distance–velocity combinations in complete factorial designs, especially close in time. Such a bias in exposure could well produce the impression of a correlation between the two variables (cf. Brunswik, 1956). In short, it is possible that within-subjects factorial designs actually *create* decision strategies rather than investigate the authentic cognitive processes involved in real-life decisions.[8]

The laboratory and field tasks differed in other ways besides the degree of orthogonality between distance and velocity. Subjects in the laboratory study were much more willing to risk an "accident" (accidents occurred on 9% of all trials in the laboratory, whereas no accidents were observed in the field); they had to respond under greater time pressure (the average temporal gap was shorter in the simulation than the field); the control over the speed of the crossing car was more restricted in the laboratory simulation; the number of distractors was probably greater in the field; the amount of prior information about the flow of traffic was different; the subjects were drawn from different populations; and so on.

Any one of these or other task differences may have contributed to the lack of convergence of the results. It is quite possible, therefore, to generate alternative, post hoc explanations for the differences in results by focusing on one or more of these task differences. The tendency for laboratory subjects to take greater risks, for example, may mean that the subjects were not taking the task as seriously as real drivers and were therefore less veridical in their estimates of distance and velocity. Alternatively, the probable presence of many more distractors in the field, requiring that the subjects divide their attention, might explain the use of a unidimensional strategy on the part of real drivers (Wright, 1974). Regardless of the particular differences one points to, however, it is still possible to ask whether the results from the simulation tap basic processes or create decision strategies peculiar to the exact procedures being used. Simulations will always differ from the situations that they simulate on some dimensions, and when the results from a simulation differ from those obtained in the field, the argument that the simulation taps cognitive processes that are somehow basic loses much of its impact. Unless we construct theories that tell us, a priori, which features of a simulation task will be irrelevant when we generalize to various field settings, we will always be façed with the problem of having multiple post hoc explanations for whatever differences emerge between simulation and field results.

While it would be possible to conduct research isolating the crucial differences between our particular simulation and the field settings, in the long run, the utility of such research seems limited. Presumably, decision research is conducted to discover the rules people use when making various types of decisions. In the present case, the concern was with the decisions of drivers. Since we have already discovered the rules drivers use from our field research, attempts to discover why the laboratory simulation failed to replicate the field results would seem unlikely to add to that understanding. They might tell us about subjects in the simulation, but they would add little to our understanding of drivers in the field.

We might have been able to increase the correspondence between the results from the laboratory simulation and the naturalistic studies by making the temporal and visual properties of the task more similar to the field setting. It is important to note, however, that we originally believed these differences to be unimportant ones. We designed our

[8] It is interesting to note that information integration theory (e.g., Anderson, 1974), with its emphasis on factorial designs, may overestimate the extent to which subjects combine values on several independent dimensions to reach decisions. Not only might the consistent use of factorial designs create task-specific strategies, but even if they do not, the decisions may be based on a single dimension which is highly correlated with the result of the assumed combination rule, as is the case for temporal gap and the ratio of distance and velocity in the present study.

procedures the way we did because we believed they were sufficient to tap what we assumed was a basic decision process. We fully expected the results from the field studies to replicate those from the laboratory simulation. While it can be argued that our failure to anticipate the differences in results stems from naiveté on our part, it should be noted that the same naiveté is probably common to other laboratory researchers (especially those who employ even less naturalistic decision tasks than ours) and that our decision theories (e.g., Slovic & Lichtenstein, 1971) do not help us overcome such naiveté because they do not specify which differences in task features can be ignored (cf. Olson, 1976, for a similar argument, but in reference to other decision theories and tasks).

Regardless of the reasons for our failure to replicate the field results in the simulation, it should be noted that the knowledge that the laboratory findings did not generalize could only be obtained by studying decision making in the appropriate real-world settings. The generality of a model or result is an empirical issue that cannot be overcome simply by employing simulations with greater face validity. Therefore, to avoid making similar mistakes in the future, it seems important to require that claims that a given laboratory task provides direct access to a basic process or decision strategy be buttressed by some evidence about the ability of the postulated strategy or model to predict behavior in relevant naturally occurring situations.

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