

A SIMULATED DRIVING TEST OF WILDE'S RISK HOMEOSTASIS

THEORY

by

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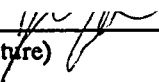
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A Simulated Driving Test of Wilde's Risk Homeostasis Theory

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Abstract

Risk Homeostasis Theory is one of the few models of driver behavior that can account for fluctuations in the population traffic accident loss. In essence, the theory predicts that reductions in the overall net gain associated with risky alternatives will be effective in reducing the total population accident cost, while restrictions on specific risky behaviors, such as speeding or seatbelt wearing, will not. The challenge that this view presents to the established ideology of traffic legislators has precipitated a good deal of scientific study. Unfortunately, research investigations have been plagued with a variety of problems including poor experimental control, inadequate data, misinterpretations of the theory's main postulates, and experimental tasks with poor ecological validity. The present research addresses a number of these problems with the use of an interactive driving simulator. Thirteen male and eleven female subjects drove in four ten-minute sessions. In each session, subjects were fined for exceeding either a high or low speed limit, and were penalized either fifty cents or two dollars for causing an accident. Subjects received all four combinations of the two bi-level factors. If Risk Homeostasis Theory is correct, drivers would be expected to cause a greater number of accidents in the low versus high accident cost condition, but would be expected to show similar accident involvement in the two speed limit conditions. Results were consistent with these expectations. Some conclusions are drawn regarding the implications of these findings for future tests of the theory and for traffic legislation in general.

Dedication

It is difficult to express my gratitude strongly enough for the concern, help, and preparation that my parents, Dr Stewart Jackson and Mrs Joyce Jackson have given me in this endeavor. They have made my dream of higher education possible, and I thank them deeply.

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Table of Contents

| | |
|---|------|
| Approval | ii |
| Abstract. | iii |
| Dedication | iv |
| Acknowledgments | v |
| Table of Contents | vi |
| List of Tables | viii |
| Introduction | 1 |
| Early References To Risk Compensation | 3 |
| Risk Homeostasis Theory | 5 |
| Correlational Investigations of RHT. | 9 |
| Research on The Effects of Seatbelt And Safety Device Legislation | 9 |
| Research on The Effects of Changes To Specific Vehicle Design Characteristics. | 22 |
| Studies of Risk Compensation at Traffic Intersections | 24 |
| Other On-The-Road Studies of Risk Compensation. | 27 |
| Experimental Tests of RHT. | 32 |
| A Simulated Driving Test of RHT. | 41 |
| Method | 43 |
| Subjects. | 43 |
| Apparatus. | 43 |
| Design | 45 |
| Procedure. | 45 |
| Results | 49 |
| Assumptions and Practice Effects | 54 |
| Discussion | 60 |

Risk Compensation to Changes in Accident Cost 60
Risk Compensation to Manipulations of Nonmotivational Variables 64
Migration of Risk Taking From Restricted to Unrestricted Behaviors 65
Other Findings and Speculation on Their Relevance To Traffic Safety Regulation. . . . 67
Recommendations for Future Research 69
Summary and Conclusions 70
References 72

List of Tables

| | |
|---|----|
| Table 1. Means and standard deviations for the cost of an accident, the speed limit, the cost of a speeding violation, for each of the four accident loss variables | 50 |
| Table 2. Means and standard deviations for the cost of an accident, the speed limit, the cost of a speeding violation, for each of the nonaccident loss variables. | 50 |
| Table 3. Cell means for both levels of speed and fine for the variables average moving speed, average speed 30, steering corrections, time over 40, time over 50, and fastest speed. | 52 |
| Table 4. Overall and within condition effects of order of presentation on the total number of accidents, total number of errors, number of stop lights run and number of crosses of the yellow. | 56 |
| Table 5. Analysis of variance summary table of the effects of treatment position and condition on the number of errors. | 57 |
| Table 6. Analysis of variance summary table of the effects of treatment position and condition on the number of errors. | 58 |
| Table 7. Analysis of variance summary table of the effects of treatment position and condition on the number of crosses of the center line | 58 |
| Table 8. Analysis of variance summary table of the effects of treatment position and condition on the number of runs of the stop lights | 58 |

INTRODUCTION

Since the United States introduced automobile safety regulations in 1968, a great deal of attention has been focused on the effectiveness of traffic safety legislation (Peltzman, 1975; Joksch, 1976; Robertson, 1981; Orr, 1984; Adams, 1985). Proponents of the legislation argued that the benefit of the new safety devices could be calculated without reference to intervening human factors. For them, the reduction in accident loss associated with the introduction of a safety device could be determined purely on the capacity of the measure to limit injuries. However, others have argued that human reactions to the introduction of safety legislation will influence the extent to which the legislation is effective in reducing the accident loss. This view is held by risk compensationists who argue that drivers compensate for the reduction in risk that accompanies the introduction of a safety measure. The compensation results in an increase in risk-taking behavior that tends to offset or counteract the risk reduction that triggered the change.

The subject of risk compensation in driver behavior has attracted a great deal of attention from governments, legislators, the general public and academic science. The attention has often taken the form of heated debate as interest groups of all types have struggled to have their voices heard (Adams, 1985). The issue is an important one because it not only reflects on the way we understand behavior on the roads, but on the way we control that behavior. The traditional approach to traffic regulation places the responsibility for controlling the accident rate on traffic legislators rather than on the driver. Drivers are seen as manipulable, mechanical units moving about in an environment that must be externally controlled and restricted for their own safety. There is little faith in the driver's ability to regulate their own behavior, or control their natural tendency toward risk-taking behavior. In general, those people I will call noncompensationists can be characterized by this view. While I believe that noncompensationists have a tendency toward each one of these ideologies, they are more appropriately described as people who reject the view that motorists are capable of regulating the amount of risk they expose themselves to by compensating for changes in the traffic environment that influence their level of perceived risk.

By this view, counter-measures that restrict specific behaviors, such as speeding or belt wearing, are fully effective - in principle, at least - in reducing the accident loss by amounts that could be expected from engineering calculations alone.

Compensationists, on the other hand, believe that drivers constantly adjust their behavior in order to accommodate for situational fluctuations that influence the amount of risk they experience. From this perspective, counter-measures that are aimed at specific behaviors are certain to result in a reduction in the accident loss smaller than would be expected from engineering calculations alone. This is because drivers transfer the risks associated with the regulated behavior to unregulated behaviors. In doing this, motorists are said to "spend" some of the legislation's safety benefits by changing other non-regulated behaviors in a way that increases the risk associated with them. Although the debate between these two factions still rages (Haight, 1986), a third type of compensationist has attracted even more attention. Those who believe that drivers compensate fully for the reduction in risk that accompanies safety measures support a more radical view that is best represented by the principles of driver behavior outlined in Wilde's (1982) Risk Homeostasis Theory (RHT). This view has radical implications for most modern forms of traffic legislation because the notion that drivers compensate fully for safety counter-measures is tantamount to the claim that most legislated safety measures have not been effective in reducing the total population traffic accident loss.

The purpose of this paper is to review a sample of studies that have a bearing on RHT, and to present a research project that was undertaken to address some of the issues the review uncovers. In addition, a description of Risk Homeostasis Theory and its implications will be given. I shall argue that empirical attempts to determine the effects of safety legislation and the implications of these regulations for RHT have been inadequate.

Early References To Risk Compensation

An early reference to the role of compensation in driver behavior was made by Gibson and Crooks (1938) in their analysis of the driving task. Invoking some well known theoretical constructs used today, they argue that drivers perceive the threat associated with objects in the driving environment with the purpose of avoiding those objects that are perceived as dangerous (known as threat avoidance in Fuller's (1984) model). At the same time, drivers attempt to maintain a field of safe travel that, by definition, has a positive valence (is not threatening). In a specific example, they suggest that the ratio of a driver's field of safe travel to his or her minimum stopping distance is a constant, and therefore the ability to stop more quickly results merely in the reduction of the driver's field of safe travel. They argue:

Except for emergencies, more efficient brakes on an automobile will not in themselves make driving an automobile any safer. Better brakes will reduce the absolute size of the minimum stopping zone, it is true, but the driver soon learns this new zone and, since it is his field zone ratio which remains constant, he allows only the same relative margin between field and zone as before. (p. 458)

Here, Gibson and Crooks hint that "homeostasis" is taking place by suggesting that the field zone ratio is constant, but they qualify the remark by suggesting that no compensation is made for emergencies. Thus, as far as the total accident loss is concerned, better brakes would lead to a reduction, since some number of emergency accidents would be avoided.

A debate concerning the extent to which compensation is complete was born in these rather fleeting comments (Gibson and Crooks made this argument in a footnote to the main body of their paper). From this point on, researchers can be distinguished by the extent to which they believe the mechanism controlling risk-taking in driving behavior is homeostatic. Smeed (1949) noted:

There is a body of opinion that holds that the provision of better roads, for example, or the increase in sight lines merely enables the motorist to drive faster, and results in the same number of accidents as previously. I think there will always be a tendency of this sort, but I see no reason why this regressive tendency should always result in exactly the same number

of accidents as would have occurred in the absence of active measures for accident reduction. (p. 24)

Here, Smeed espouses the compensationist's view by allowing for behavioral reactions to counter-measures, but rejects the notion that the behavior of motorists is homeostatic (i.e., fully compensatory) with respect to the accident rate.

In fact, few traffic researchers would accept the view that no behavioral compensation exists in the traffic environment (Adams, 1985). What divides the camps can be framed as a disagreement concerning the utility function that determines driver behavior. Those who view behavioral compensation to the introduction of a counter-measure as incomplete with respect to the accident loss are suggesting that, as utility maximizers, drivers are attempting to maximize the value of a utility function that includes factors other than safety. Thus, while it is true that drivers may "spend" some of the added safety benefit with an increase in risk taking, they will also spend some of the benefit as it was intended, by increasing safety. On the other hand, those that believe compensation is homeostatically controlled argue that drivers are using a utility function that maximizes the similarity between a target level of safety and the perceived level of safety. Since the target level of safety is not expected to change upon the introduction of a safety measure, the utility function does not allow for an increase in safety following the introduction of a counter-measure.

In the review that follows, a significant amount of attention will be paid to the compatibility of the results with the homeostasis hypothesis. In addition, a number of studies will be included that have specific implications for RHT. In order to better appreciate the theoretical relevance of this research, the review of the empirical evidence will be preceded by a description of RHT and its essential postulates.

Risk Homeostasis Theory

Wilde (1982, 1985, 1988) has proposed a model of driver behavior that accounts for risk taking by assuming that drivers are continually attempting to reduce the difference between a subjectively perceived level of risk and a motivationally determined target level of risk. The target level of risk is determined by the outcome of a utility function that serves to maximize the net gain associated with the expected costs and benefits of both risky and safe alternatives. The target level of risk is significant because it defines, at any given time, any single driver's optimal level of risk exposure under any particular set of circumstances. Wilde has proposed that the following function relates the population accident loss to the population average target level of risk;

$$A = R * H * N \dots \dots \dots (1)$$

where A = the total population accident loss due to all causes (i.e., physical harm, economic costs or personal factors), R = the average population target risk level, H = the population average number of hours exposure and N = the size of the population.

It is clear from the rudimentary deduction that A and R are directly related, that the traffic safety measures that influence R should be the only counter-measures capable of permanently reducing the total population accident loss. Wilde argues that interventions that are aimed at specific risky behaviors that do not influence the driver's desire to be safe will not lead to lasting reductions in R. This is because the limiting of specific risky behaviors, such as speed restrictions or seatbelt wearing, is associated with a reduction in accident risk that is eventually neutralized by changes in other behaviors that act to restore the perceived level of risk to the target level. Thus, risk homeostasis is said to occur if factors that limit specific risky behaviors are fully compensated for by concomitant changes in other factors. The form of the behavioral compensation is such that it operates to maintain the perceived level of risk as near as possible -

theoretically equal - to the target risk level. Thus, interventions that limit specific driving behaviors should not influence the population accident loss because risk homeostatic mechanisms will serve to maintain risk levels at the motivationally determined target level.

Since the homeostatic mechanism relies on feedback concerning the total population accident loss, temporary reductions in accident loss may occur. The duration of these fluctuations is dependent on feedback to individual drivers concerning changes in the population accident loss that result from the introduction of counter-measures. Once individual drivers recognize that the effect of the counter-measure has served to reduce, for example, the subjective probability of loss, compensations that serve to increase the subjective probability of loss to the target level will occur.

In contrast to the effects of changes in non-motivational counter-measures, Wilde argues that the manipulation of motivational variables should result in changes in R . The target level of risk (R) is arrived at in an intuitive manner, and results from the optimization of the expected net benefits that derive from participating in the traffic system. The expected net benefits are said to be a function of four classes of motivating factors: (1) the expected gains associated with risky alternatives, (2) the expected losses associated with risky alternatives, (3) the expected gains associated with safe alternatives and (4) the expected costs associated with safe alternatives. Two important motivational variables that Wilde (1988) outlines are incentives for accident free driving and penalties for accident involvement. Changes in the accident rate (A) can be predicted from fluctuations in R which increases as factors 1 and 4 increase and decreases as factors 2 and 3 decrease. Thus, manipulation of motivational variables should cause an immediate and permanent change in A , while the manipulation of nonmotivational variables should cause a short term increase in A that diminishes as the effect of the nonmotivational counter-measure becomes known.

A number of consequences follow from the formulation of RHT given above. First, since A is a function of R and the two exposure variables, H and N , reductions in the accident loss *per*

capita hour of exposure are the only reductions that should result from a decrease in R ¹. Reductions in R do not imply reductions in the accident loss *per km of road travel*. Second, since the population accident loss is defined as a multiple of the frequency of accidents and their costs, reductions in other factors such as the death rate or the number of injury accidents do not imply a reduction in R . Third, since access to accurate accident loss data is usually not available to road users, individual variability in danger detection skill could cause road users to be exposed to more or less actual accident risk than is perceived. Thus, fluctuations in R may occur to the extent that perceived and actual risk do not coincide. Unfortunately, since the actual traffic accident loss in the natural traffic environment is a function of a number of currently unmeasurable factors (Wilde, 1988), the valence of the difference between perceived and actual risk, if in fact a difference exists, can not be determined. Fourth, since RHT applies to all road users in a given jurisdiction, a reduction in the accident loss in any one, or subgroup, of transportation modes does not imply a reduction in the overall population accident loss.

The final consequences that follow from RHT can be deduced mathematically from Equation 1. Consider two separate jurisdictions within a single traffic population at some single point in time. If both sides of Equation 1 are multiplied by the km driven in a single jurisdiction, and small n is substituted to represent the number of road users passing through that jurisdiction (as opposed to large N which signifies the total number of people in the population as a whole), algebraic manipulation gives Equation 2:

$$\text{km/h} = R/(A/n*\text{km}). \dots\dots\dots (2)$$

If the accident rate is constant per time unit of driver exposure, then sections marked by higher accident rates per person km (given by the size of the term $A/n*\text{km}$) should also be sections in which the average speed (given by the term km/h) is lower. This equation represents what Wilde (1988) terms the cross-sectional deduction from RHT. The prediction of lower speeds in sections

¹ This can be clearly seen by multiplying both sides of equation one by the reciprocal of $H*N$.

of road with higher accident rates per person km is correct for sections of road compared at a single point in time but should not be confused with the longitudinal prediction made in the following equation. The second mathematical deduction involves comparisons of single jurisdictions at different points in time. Multiplying each side of Equation 1 by the total number of KM² driven during a specified period of time, algebraic manipulation gives:

$$KM/N = (R*h)/(A/KM) \dots\dots\dots (3)$$

Equation 3 shows that if the product of R and h remains constant over time, the average population spatial mobility should increase as the population accident loss per KM decreases.

It is of interest to note that the formulation of RHT given in Equation 3 shares a great deal of conceptual similarity with another well known, but much earlier, law of traffic accident loss. Smeed (1949) proposed that:

$$D/N = a(N/P)^P \dots\dots\dots (4)$$

where D represents the total number of road accident deaths in a given population at a given time, N is the number of registered motor vehicles, and P is the number of individuals in the population. If we permit the assumption that as the number of vehicles per person increases, an increase in the number of KM driven per person should also occur, then by Smeed's law the per person population mobility is directly related to the per vehicle population accident loss. But, since we have assumed that an increase in the number of vehicles should be related to an increase in the number of KM driven, Smeed's law predicts that the population accident loss per KM should be directly related to the per person population mobility. This, with a few caveats, is conceptually equivalent to the view given by Equation 3.

² Large KM refers to the total number of KM driven in the population while small km refers to the total number of km driven in a single jurisdiction or by a subset of the population.

Correlational Investigations of RHT

A large number of studies have been conducted for the purpose of testing RHT. These studies incorporate a number of methods including analyses of archival accident statistics, field studies using various dependent variables as criteria of safety or risk taking, and quasi experimental designs (Wilde, Claxton-Oldfield and Platenius, 1985). In general, most of these studies assess the influence of a particular counter-measure, or nonmotivational variable, in a single jurisdiction over a varying period of time. In the following section, studies on the effects of a number of different types of counter-measures will be reviewed separately.

Research On Seatbelt and Safety Device Legislation

In a seminal paper on the effectiveness of automobile safety devices, Peltzman (1975) reported an analysis of American vehicle safety regulations that became effective in 1968. The major vehicle design changes required by the legislation were as follows: (1) seatbelts for all occupants, (2) energy-absorbing steering columns, (3) penetration-resistant windshields, (4) dual braking systems and (5) padded instrument panels. Peltzman attempted to assess the effects of these safety devices by predicting the levels of accident loss that would have occurred if they had not been introduced. These predictions were then compared to the actual accident loss that occurred in the post regulatory period. Predicted rates that exceeded actual accident rates would provide evidence that the safety legislation had been effective in reducing the accident loss. Death rates were predicted by regressing them on a set of explanatory variables for the preregulatory period of 1947 to 1965. The explanatory variables were the cost of an accident (a weighted average of the consumer price indexes for hospital and physician costs and auto repair costs, deflated by the all-items consumer price index and multiplied by an insurance loading charge), income (real earned income per 15 years or older person), a linear trend variable,

alcohol intoxication (the average consumption of distilled spirits per 15 years or older person), vehicle speed (the estimated average speeds of vehicles travelling on noninterstate roads at off-peak hours), and the number of young drivers (the ratio of the number of 15-25-year-olds in the population divided by the number of older members of the population). In addition, three dependent variables were analyzed: (1) the vehicle occupant death rate per mile, (2) the nonoccupant death rate per vehicle mile, and (3) the total death rate per vehicle mile.

Peltzman found that for the years between 1965 and 1971, the predicted and actual total death rates differed by between $-.6$ and 2.4% . On average the projected death rates were *lower* than the actual death rates, indicating a "perverse" effect of safety regulation. This effect was largest for the year 1972 when the predicted death rate was 4.7% lower than the actual death rate. Interestingly, the pattern of differences between the predicted and actual death rates varied for occupants and nonoccupants. It was found that the occupant death rate showed a reduction greater than predicted, while the nonoccupant death rate rose considerably higher than was predicted. Peltzman concluded that the results indicated an increase in driving intensity following the introduction of the safety counter-measures. He suggested that the increase in driving intensity was not sufficient to fully offset the safety benefits of the legislation for occupants, while nonoccupants experienced an increased risk that resulted from exposure to less cautious drivers.

Joksch (1976) reported a number of criticisms of Peltzman's analysis. First, he objected to the validity of the variates that Peltzman chose. It was argued that the inclusion of the cost of an accident and the income per capita variables was arbitrary, and that the cost of an accident variable probably had no relation to the actual accident cost experienced by the driver. It was also suggested that since the dependent variable is a direct function of the number of miles driven, spurious correlations between the dependent variable and the independent variables could exist if any of the independent variables correlated with the number of miles driven. Since the number of miles driven is known to have an approximately exponential time trend, spurious correlations can be expected between the linear time trend variable and the dependent variable.

A further problem with Peltzman's analysis resulted from collinearity between the independent variables. Since income, speed, time and the cost of an accident are all time dependent, they were probably all highly correlated. The tendency for collinearity to inflate the standard errors of the regression coefficients probably resulted in very poor predictions of the dependent variables beyond the period from which the regression coefficients were derived.

The criticisms Joksch makes that reflect on the validity of the regression coefficients are valid but not particularly damaging to Peltzman's conclusions. The logic of Peltzman's procedure does not rely upon the nature of the specific variates used to predict the post-regulatory accident loss. Since the purpose of the analysis was not to provide theoretical rationale for the structure of the relationship between the predictors and the death rate, the variables used are only of value to the extent that they are reliable predictors. Thus, any set of variables that could reliably predict the total death rate beyond the data they derive from, would be sufficient to fulfill the purpose of the analysis. In my view, the weakness of Peltzman's data lies in three main areas: (1) the collinearity of independent variables causes unstable predictions beyond the data, (2) the lack of a control sample seriously limits the extent to which the observed changes in death rates can be viewed as a function of the introduction of the counter-measures, and (3) the dependent variables used by Peltzman are not necessarily the most important or relevant indicators of accident loss.

Given these weaknesses, it would be imprudent to place too much faith in the implications of these data for RHT. A naive reading of the results would lead to the conclusion that homeostasis with respect to the total population accident loss took place in the United States during the period following the introduction of safety legislation. However, I have shown here that the results are not reliable from either a statistical or methodological perspective.

Other attempts have been made to determine the effects of the 1968 U.S. safety regulations. Robertson (1981) used data that became available after Peltzman's analysis in order to reassess the effects of the U.S. regulations. Regression analyses were performed in which the introduction of federal and state safety legislation (expressed as dummy variables with values of 0 or 1), vehicle type (car or truck), vehicle age and vehicle age squared and cubed, were regressed

against the death rate per km. Regressions were run separately for occupants, pedestrians, motorcyclists, cyclists and overall. It was shown that both federal and state safety legislation was significantly influential in reducing the occupant death rates. As well, death rates were significantly related to the introduction of the federal law for all categories of road users, but were not correlated with the introduction of state law for motorcyclists and cyclists. Robertson concluded that 37,000 fewer deaths occurred in the 1975-1978 period than would have occurred without the instigation of safety regulations.

Unfortunately, there is reason to believe that the structure of Robertson's model may have biased his conclusions. Cooley and Lohnes (1971) argue that regression weights are highly unstable if collinearity exists among the predictors, and that extreme caution should be taken when attaching practical significance to the regression coefficients. The inclusion of three vehicle age terms in Robertson's model makes it likely that, unless the variables were centered (there is no indication by Robertson whether this was done or not), large correlations existed between the three age variables (Howell, 1982). It has also been shown that large correlations existed between the car/truck and regulation dummy variables (Orr, 1984). Since Robertson used the regression weights to estimate the effects of the safety legislation, instability of the regression weights would likely render his estimates unreliable.

There are also a number of criticisms of Robertson's design that mirror those made of Peltzman's work. There was no control group, the dependent variable was a measure of the death rate per km, and the choice of vehicle age as the lone nonregulatory independent variate was not supported by theory or empirical evidence. But more troubling were the effects Robertson found for vehicle safety regulation on nonregulated road users. Under the no compensation hypothesis, one would expect that the effects of regulation should be highly influential on the group of road users they apply to, the occupants, and have little effect on other groups of road users such as pedestrians, cyclists and motorcyclists. Curiously, however, the effect of federal safety legislation on pedestrians was found to be large and highly significant. As well, federal legislation was found to significantly reduce the number of fatalities per vehicle mile for cyclists

and motorcyclists. This apparently perverse result suggests that safety devices such as energy absorbing steering wheels, safety belts, padded instrument panels and penetration resistant windshields were effective in reducing the fatality rates of motorcyclists, cyclists and pedestrians. Given this result, it should be clear that Robertson's technique and methods are capable of producing anomalous results and should therefore be interpreted with caution.

Orr (1984) isolated a further problem with Robertson's analysis. He suggested that the inclusion of truck data was inappropriate because none of the mandated safety regulations applied to trucks. It was argued that the inclusion of truck data would have had the effect of causing the life-saving benefits of the safety measures to be too optimistic. Using only data derived from car death rates, Orr estimates that the safety regulations were responsible for a reduction in occupant deaths of between 700 and 5,900, while the reduction in nonoccupant deaths was predicted to be between -3,600 and 5,400. These estimates are considerably different from the estimates of 26,500 and 10,600 made by Robertson. Rather than settle the debate over the American safety regulation data (see Robertson, 1984), Orr's analysis underlines a fundamental problem with time series predictions of accident rates that go beyond the data they are based upon. The exclusion of a single variate from Robertson's model leads to an enormous difference between the two studies in the death rate predictions. Even excluding the problems associated with the explicit assumption that all variables related to the dependent variable but not included in the model are required to stay constant past the developmental data, the problem of prediction sensitivity to the set of variates chosen for the model sheds clear doubt on the usefulness of these designs for accurate prediction.

Despite these problems, time series analyses have been used on a number of occasions to assess the effects of traffic safety regulation. Lindgren and Stuart (1980) reported an analysis of the effects of traffic safety legislation introduced in Sweden between the years of 1965 and 1973. They used a method that was as similar to Peltzman's as could be achieved with the data they had available to them. Projections of the total population deaths for occupants and nonoccupants were obtained using data from the period between 1947 and 1964. Predictions were made for the

period beginning in 1965 and ending in 1973. The independent variables used were as similar as possible to Peltzman's, with the exception of the accident cost variable which was not included due to its lack of contribution to the prediction. In this analysis, the regression weights obtained for the occupant death rates differed in size considerably from Peltzman's. As well, the regression weights for both the trend variable and income variable were of opposite sign to those found by Peltzman. As might be expected, the death rate predictions were also not consistent with Peltzman's. In fact, the predicted values for occupant death rates were considerably higher than the actual values, leading to the conclusion that the safety measures had been effective and homeostasis had not occurred.

Adding to the already convoluted analysis of the American data, Graham (1984) reported a study in which he used the time series forecasting approach originated by Peltzman. He regressed the calendar year, model year, age and weight of cars on the occupant death rates for cars of each given type. It was hypothesized that a negative regression weight for the model year variables would indicate that as the model year increased, the occupant death rate decreased. But, since an increase in safety related equipment accompanied an increase in the model year, the negative regression weight for model year would imply that safety equipment had served to reduce the death rate. The results were consistent with this hypothesis. Curiously, however, the regression weights for cars of model years 1974 to 1981 were all larger and of the same sign as those for the earlier model years in which safety equipment first became available. Since no new safety equipment was introduced in those years, it appears that something other than the introduction of safety equipment was responsible for the pattern of data reported by Graham.

On October 1, 1985, a seatbelt restraint law came into effect in North Carolina. In a 15-month period following the enactment of the law, warning tickets were issued to violators, while as of January 1, 1987, violators were subject to a \$25 fine for noncompliance. A structural time series analysis was used by Reinfurt, Campbell, Stewart and Stutts (1990) to predict the effects of the introduction of both warnings and citations. Forecasts were made of death and injury rates for 1985 to 1988, on the basis of the pattern of death and injury rates in the 1981 to 1985 period.

The two intervention dummy variables entered in to the analysis were the introduction of warnings, and the instigation of a \$25 fine for belt violators. Both the percentage reduction in the number of deaths and the statistical significance of the interventions were analyzed. For front seat occupants of vehicles covered by the law during the warning phase, a 1.4% increase in fatal injuries occurred, while a 5% decrease in moderate and serious injuries was found. Each of the three effects was statistically significant at either the .05 or .01 significance level. For all other groups of road users, small increases and some nonsignificant small decreases in injuries and fatalities were found. During the citation phase the patterns were similar with the effects being approximately twice as large as in the warning phase.

These results do not suggest any consistent form of compensation. In general, for road users who were not targeted by the law (i.e., nonoccupants, rear seat passengers) the results were inconsistent with respect to whether increases or reductions in fatalities occurred, and, overall, were statistically nonsignificant³. In contrast, occupants targeted by the law experienced a reduction in fatalities of approximately 11% following the introduction of citations. Thus, from a naive perspective, the North Carolina seatbelt law appears to have reduced the overall accident death rate.

Conybeare (1980) conducted a similar analysis on the Australian experience with seatbelt legislation. He used data on the number of casualties and injuries per vehicle mile for the period between 1949 and 1971. A time series analysis was used to predict the number of casualties and injuries per vehicle mile in a 5-year period following the 1972 introduction of seatbelt legislation. The independent variables in the equation were the cost of risky driving behavior, income, age, alcohol consumption, automobile power and a time trend⁴. For each year following

3 In an analysis of this sort, statistical significance should properly carry little weight in comparison to the concerns raised by the heavy assumption load of the design. This is because a change in a single variable at or near the time of regulation causes the statistical results to be invalid.

4 The following is a more accurate description of the independent variables: (1) The cost of risky driving was expressed as the proportion of final consumption expenditure on motor vehicles and all medical hospital and funeral services in the previous year; (2) Income was expressed as the previous year's disposable income per person 15 years or older; (3) Age was determined by the proportion of the population between 15 and 24; (4) Alcohol consumption was an index of the

1971 the predictions for occupant death rates exceeded the actual values, while the predictions for nonoccupant death rates were lower than the actual values. The results followed the same pattern as Peltzman's findings, adding support for the view that some form of risk compensation occurred in vehicle occupants. In a second analysis, a dummy variable was included in the prediction equation and was given a value of either 0 for the pre-legislation years and 1 for the post-legislation years. As expected, the coefficients were negative for equations with occupant deaths or injuries as the dependent variables, and positive for equations with nonoccupant deaths or injuries as the dependent variables.

Unfortunately, the independent variables employed in this study were highly intercorrelated, with correlations among them ranging between .76 and .95. Conybeare correctly deduced that this problem could (and in fact did) lead to erratic changes in the coefficients with small changes in the model. However, he was not correct in the assumption that since the purpose of the model was not to specify the structural relationships between the variables, collinearity was not detrimental to the forecasts generated by the equations. Dillon and Goldstein (1984) show that since the size and signs of the regression weights determine the value of the dependent variable, unstable regression weights can cause unstable predictions.

Sivack (1987) expresses many of the methodological concerns raised by these types of studies in his analysis of the reasons for the NHTSA's grossly erroneous predictions of the 1985 U.S. fatality rates. He cites data that show the NHTSA's forecasts to be almost twice as pessimistic as the actual outcome. On the basis of these errors, he outlines two general issues that traffic safety forecasters must face when attempting to predict the future. First, the prediction equation in any forecast necessarily assumes that no variables other than the ones included in the model will ever have an effect on the dependent variable. Second, the extent to which an independent variable predicts fluctuations in the dependent variable can be solely a function of the other variables that reside in the equation. He goes further by suggesting that even the form

amount of spirits, wine and beer sold per person 15 years or older and (5) Automobile power was expressed as the horsepower rating of the most powerful car sold in that year.

of a given variable will determine its effectiveness as a predictor (an example is given in which the unemployment rate was found to be an important predictor in one model, while the number of unemployed had little predictive value in another).

In a statistically less complex approach, MacKay (1985) assessed the effects of the January 31st, 1983 introduction of Great Britain's mandatory seatbelt wearing law. He compared the number of fatal and serious casualties in two 8-month periods before and after the introduction of the safety belt legislation. The results showed a 25% reduction in casualties in the period following the January 31st introduction of the law. Since the wearing rates after the introduction of the law were found to be approximately 91%, the British data offered an excellent opportunity to test the influence of seatbelt legislation. In a number of other cases, relatively low wearing rates may have allowed for selective migration of the non-wearers before the law to wearers after the law. A tendency for the more cautious non-wearers to comply with the law would, in these cases, tend to reduce the effectiveness of the legislation.

Adams (1985) presents a strong criticism of MacKay's data and the conclusions it spawned. Using a prediction equation with the independent variables of time, expenditure on alcohol adjusted for inflation, petrol consumption and unemployment, Adams produced forecasts of the ratio of the death rate to the national traffic index. His forecasts agreed closely with the actual death rates following the introduction of the belt law. Adams also looked at the trend in accident deaths in Britain over a longer period of time than the short 8-month periods used by MacKay. The graphs he presented clearly indicated that while large seasonal and transient fluctuations in death rates did occur, the reduction in deaths following the legislation disappeared when viewed over an extended time frame. In fact, MacKay's data is fully consistent with RHT. Wilde admits that the imprecision of the feedback loop is such that temporary fluctuations in the accident loss may occur following the introduction of a nonmotivational counter-measure. But, as Adams showed, these fluctuations should, over more extended time frames, disappear as homeostasis acts to restore the preregulation accepted traffic accident risk.

In Canada seatbelt laws became effective in Ontario, Quebec, Saskatchewan and British Columbia between January, 1976 and October, 1977. Since not all Canadian provinces enacted seatbelt legislation during this period, the non-law provinces offer excellent control data for the law provinces. Jonah and Lawson (1984) used linear regression methods to project occupant fatality rates in the post-law period between 1977 and 1981, for both the law and no-law provinces. The projections were based on data for the period beginning in 1960 and ending at the date of legislation. The forecasts were found to significantly overestimate the number of occupant fatalities in both the law and no-law provinces. Counter to expectations, however, the no-law provinces were over-predicted by a greater amount than the law provinces for 1977, 1978 and 1981.

Due to the existence of a control group, the data from this study are far more reliable than the data from the studies discussed so far. In this case, the use of a control group has revealed two potentially serious problems with the studies reviewed to this point. Most importantly, it shows that the forecasting method can produce false estimates in cases in which interventions are present, and in cases in which they are not. It also shows that conclusions based on data for which there is no control can be opposite to the conclusions that would have been made in the presence of controls. It is surprising that a large part of the literature on safety legislation has relied and speculated upon data that are demonstrably untrustworthy.

In this section we have looked at a number of studies on the effectiveness of motor vehicle engineering safety legislation. It should be clear from the criticisms given that a considerable amount of this literature is specious in its approaches and conclusions. The failure of archival data to conform to the basic requirements of empirical study has been ignored and overshadowed by the use of statistically complex procedures. In many cases, the analyses are fundamentally flawed by inappropriate specifications of the models or by the unstabilizing effects of inadequate data structure. In other cases, the disregard for appropriate experimental control is foremost. With the exception of the study based on the Canadian data, the results of this research are so unreliable that they should be given little weight as indicators of the validity of the homeostasis

hypothesis or RHT. The element of control in the Canadian study, however, makes its results the most compelling of the group. But, since it assessed the effects of only one of the two essential variables implicated by the theory - the nonmotivational manipulation of seatbelt wearing - and the dependent measure of total deaths is not the factor expected to remain stable with the manipulation of nonmotivational variables, even the Canadian data do not provide firm support for RHT.

In a different approach, a number of researchers have attempted to determine the source of compensatory behavior that occurs in response to the introduction of a counter-measure. Since RHT suggests that the effect of limitations on specific behaviors is to cause increased risk-taking in other unregulated behaviors, one would expect the introduction of safety measures to cause changes in behaviors exempt from the safety legislation. The remaining studies discussed in this section have attempted to assess changes in behaviors to which risk-taking may have migrated as a result of its reduction in the regulated behavior of seatbelt use.

Evans, Wasielewski and Von Buseck (1982) measured the headway (following latencies) of 4,812 belted and unbelted Ontario drivers both before and after the introduction of a seatbelt law. The headways of belted and unbelted drivers in the unregulated, neighboring state of Michigan served as controls. The authors hypothesized that since, under voluntary wearing conditions nonusers were known to have smaller average headways than users, the difference between the two groups should decrease or even reverse after the introduction of the law. This result was expected because, following the introduction of the law, the user group would be expected to contain pre-law nonusers who had a propensity to accept smaller headways. It was further hypothesized that the reduction in the difference between the two groups should have been more than expected due to mere migration, because the influence of danger compensation should have caused the new users to accept smaller headways than they did as nonusers. Data from Michigan drivers were used in order to determine the difference between the user and nonuser groups that should have resulted from migration alone. The results indicated that the reduction in headway

of 16% that was found for the Ontario drivers was less than the reduction of 17.8% expected by migration alone, and thus, danger compensation did not occur.

The logic of the analysis conducted by Evans et al, assumes that the migration of nonusers to users following the introduction of the law was not selective. If only those nonusers that used the largest headways buckled up, the difference between the user and nonuser groups could have actually increased as a result of the legislation. Since the post-legislation wearing rates were only 50% - 60% (Robertson, 1978), it seems highly possible that selective migration could have taken place.

Lund and Zador (1984) observed the driving behavior of motorists in Newfoundland and Nova Scotia, both before and after the July 1, 1982 introduction of Newfoundland's seatbelt legislation. The measures of driving performance observed were: (1) vehicle speed on curves, signalized intersections, unsignalized T - intersections, two-lane highways and four-lane highways, (2) the percentage of drivers not stopping on a yellow signal, (3) the percentage of vehicles turning in front of oncoming traffic, and (4) the tenth percentile headway latency. No statistically significant differences were found between the behavior of the drivers in the two provinces on any of the dependent measures.

The results of this study indicate that the introduction of seatbelt legislation in Newfoundland had no measurable effect on a wide range of objective measures of driver risk taking. Since an increase in risk taking on unregulated behaviors should result from the regulation of seatbelt use, it was concluded that these data were not consistent with RHT. However, since homeostasis is expected to occur only after time-lagged feedback of the effect of the safety measure becomes available, one should not be surprised that no behavioral changes were observed only 3 to 6 weeks after legislation was introduced. For this reason, it could be argued that these results are not incompatible with RHT.

O'Neill, Lund, Zador and Ashton (1985), conducted a replication of the Canadian study in Birmingham, England. Speeds on two-lane straight roads, four-lane straight roads, and on curved two-lane roads were measured, along with headways on two and four-lane roads. As well,

information on the age and sex of the driver and the age and type of car, was taken from the driving records of the owners of the cars observed in the study. Observations were made 5 months prior to and 7 - 8 months after seatbelt use became mandatory. With adjustment for the covariate effects of age, sex, car age and car type, the results were as follows: (1) speeds on curved two-lane roads were significantly slower after the law than before; (2) speeds on two-lane straight roads were faster after the law than before; (3) speeds on four-lane straight roads were the same after the law as before, with the exception of the outside lanes in which the speeds were slower after than before the law; (4) a nonsignificant increase in following headways occurred on two-lane roads after the law; and (5) a nonsignificant decrease in following headways on all other roads occurred after the law. The authors argued that their study was "the most carefully controlled to date" and concluded that "Certainly, the pattern of findings does not support the risk compensation hypothesis." These conclusions are surprising given the obvious lack of a control group, and a pattern of results that showed compensation in some behaviors but not in others.

Although criticisms of each of these studies have already been made, a more fundamental criticism of their general approach seems warranted. By attempting to assess the migration of the expression of risk taking from regulated to nonregulated behaviors, these studies miss a fundamental tenet of RHT in particular and compensation theories in general. RHT proposes that compensation occurs with respect to the total population accident loss, and as such, the specific behaviors that change as a result of a nonmotivational regulation are not specified. As well, the amount that behaviors must change is also not specified. As a result, a 5 mile per hour increase in average speed on two-lane roads, for example, is no more likely to indicate compensation than a .01 mile per hour decrease. For this reason, none of these studies should be taken as evidence for or against RHT.

In this section, studies of the effects on driver behavior of vehicle modifications will be reviewed. In general, the hypotheses of these experiments are that behavioral compensation should result from modifications to, or differences between cars. These studies are particularly relevant because, as the earlier quote by Gibson and Crooks suggests, the notion of risk compensation was first expressed with particular reference to changes in vehicle handling and control characteristics.

In one of the most often cited studies on the risk compensation hypothesis, Rumar, Berggrund, Jernberg, and Ytterbom (1976) investigated the influence of studded and non-studded tires on cornering speeds. Driver's speeds were observed on two types of corners in both dry and icy conditions. It was expected that, since studded tires offer greater resistance in icy conditions and similar resistance in dry conditions, cars with studded tires would be driven more quickly around corners in icy conditions but at similar speed in dry conditions. Such a result would indicate that drivers were increasing their speed in order to compensate for the added safety afforded by studded tires in the icy conditions in which they are most effective.

As expected, the results indicated that on dry roads, there was no difference in cornering speeds between cars with studded and nonstudded tires. However, in icy conditions, cars with studded tires drove an average of 2.5 km/h faster than cars without studded tires, indicating some form of compensation. In order to determine the extent to which compensation was taking place, Rumar et al used an estimate of the increase in the coefficient of friction made available with studded tires to calculate the increase in cornering speeds the tires should have allowed. When differences in the proportion of side friction used between the studded and non-studded groups were considered, no significant effect was found. However, when the amount of risk taken was measured as the percentage of maximum speed used, a statistically significant difference was found between the two groups for corner 1 only. The difference was due to drivers with studded tires using less of the maximum available speed than drivers with non-studded tires.

These results have commonly been cited as inconsistent with RHT (see for example MacKay, 1985), even though, of the three main conclusions, only a portion of one is not fully compatible with the theory. However, the use of the maximum speed available in a given corner is not an accurate measure of risk taking. If complete compensation was taking place, since the consequences of exceeding the maximum allowable speed increase as the speed of the vehicle increases, one would expect drivers to use less of the maximum speed available to them in order to offset the increase in the negative consequences associated with an accident at higher speed. Thus, complete compensation with respect to the maximum available speed would not be expected under RHT. This criticism, coupled with the heavy reliance Rumar et al place on their estimate of the average increase in the coefficient of friction allowed by studded tires (that was arrived at in the absence of any empirical evidence relating to this difference) suggests that the single result they obtained that was not fully consistent with RHT should be interpreted with skepticism.

Wilson and Anderson (1980) studied the effects of tire type on driving speed, on public roads and in a test track situation. They had drivers make a lane change maneuver on a test track and drive a 16-mile long section of road. Half of the time the car was fitted with radial ply tires and the other half of the time cross ply tires were fitted. Drivers were also asked to provide a measure of subjective risk for each tire type. The results showed a nonsignificant difference in driving speed on the rural road section between cross ply and radial tires, indicating that no compensation occurred to the change in tire type. However, subjects drove 1.7 meters per second faster on the test track when radial tires were fitted. Unfortunately, and for reasons difficult to understand, the authors did not interpret this effect with respect to the compensation hypothesis. A further result showed that a measure of the subjective risk experienced by drivers on the test track was equivalent for the radial and cross ply conditions. It was concluded that this effect was consistent with the constant risk hypothesis.

The results of this study are quite similar to the results found by Rumar et al. In conditions under which the relative capabilities of the tires used were not salient (i.e., the dry roads in

Rumar's study and the rural driving in Wilson's study), no difference in driving speeds were found in either study. However, when tire characteristics became particularly influential determinants of driver's subjectively experienced risk (i.e., the icy corners in Rumar's study and the test track maneuver in Wilson's study), drivers did increase their speed when using tires with increased grip. In addition, the experience of equal amounts of subjective risk in the test track maneuver supports the view that compensation was acting to maintain subjectively experienced risk at a predetermined target level.

Evans and Herman (1976) conducted a test of the effects of adapting the starting acceleration of a vehicle on the gap acceptance functions of 7 male drivers. They modified a vehicle in a manner that reduced the time taken to clear the lane of an oncoming car by .5 seconds. Measurements of the 50% gap acceptance (the size of the gap as likely to be accepted as rejected) of the modified and unmodified cars showed that drivers of the modified car increased their gap acceptance by .37 seconds. Thus, drivers did not compensate fully for the reduction in acceleration of the modified car. Although complete compensation with respect to the timed gap acceptance did not occur here, the results are not inconsistent with RHT. As previously mentioned, compensation is expected to operate in accordance with the population accident loss and not with respect to specific behaviors. In this study it is possible that the target level of risk was maintained by, for example, an increase in vigilance while driving the modified car.

Studies of Risk Compensation At Traffic Intersections.

Smith and Lovegrove (1983) conducted a study in which they observed driver behavior both before and after the placement of a stop sign at an uncontrolled intersection. An intersection on a parallel road served as a control for the signalized intersection. The authors reasoned that if drivers were constantly assessing the amount of subjective risk present in their driving environment, the reduction in risk caused by the introduction of a stop sign would cause drivers

to increase their risk level at a subsequent intersection (test intersection) by approaching it more quickly. For this reason, it was expected that after the installation of a stop sign at the uncontrolled intersection, drivers would approach the following intersection more quickly than they had done prior to the installation of the stop sign. In addition, the approach speeds of frequent and nonfrequent commuters were measured. The results showed that infrequent commuters increased their approach speed to the test intersection after the installation of the stop sign, while frequent commuters showed no increase in speeds at the test intersection. The authors concluded that risk compensation occurred for nonfrequent commuters as evidenced by their increased risk taking at the test intersection, but that since there was a reduction in approach speed for regular commuters, they showed no tendency towards risk compensation.

While there may be some gross translation of risk compensation theory that is consistent with the reasoning used by Smith and Lovegrove, their hypothesis is certainly not correct with respect to RHT. The aspect of RHT that is relevant to this study is the prediction that drivers should choose slower speeds on sections of roads marked by dissimilar accident rates. Since the accident rate at the test intersection would not be expected to change upon the introduction of a stop sign at a previous intersection, no reduction in speed would be expected at the test intersection. Furthermore, RHT does not suggest that drivers are risk *averagers* in the sense described here. In fact, drivers are believed to be continually attempting to maintain perceived risk at a predetermined target level. Therefore, the perception of an increased risk at Time 1 does not cause drivers to attempt to average out their perceived level of risk by reducing their risk exposure below the target level at Time 2.

Hakkert and Mahalel (1978) reported a study in which they analyzed the effect of traffic signalization on the number and severity of accidents at intersections. Both the effects of the introduction of a blinking green phase and the installation of traffic lights were studied. For each of 34 urban intersections, assessments were made of the number of casualty accidents occurring in two 38-month periods both before and after the installation of a traffic signals. As well, a single road section on which lights with blinking green phases were introduced was used. A

second road section in which no blinking green was introduced served as a control. The results showed that the introduction of traffic signals was associated with a significant decrease in the total number of accidents at those intersections with over 14 accidents in the period before signalization. As well, a small and nonsignificant increase in the number of accidents was observed for intersections with less than 15 accidents in the period before signalization. The combined effect of signalization was an overall reduction in the total number of accidents. However, the relative severity of accidents did not change after signalization. In contrast, the introduction of a blinking green phase was associated with an increase in accidents. This escalation, however, was a result of a large increase in the number of rear end accidents and a lesser decrease in the number of right-angle accidents. Since rear end accidents tend not to have consequences as severe as right-angle accidents, the overall accident loss (which was assessed by a weighted measure of severity) did not change after the introduction of a blinking green phase.

Unfortunately, the lack of a control condition for the introduction of signalization severely limits the strength of Hakkert's findings. Even more damaging to the credibility of these results, however, was the failure of the authors to notice the possible effects of statistical regression in their data. What Hakkert and Mahalel interpreted as indicating the effectiveness of traffic signals at high, but not low volume intersections, could merely have resulted from statistical regression. Specifically, those intersections with high accident volumes in the first period of assessment would be likely to regress toward lower numbers of accidents in the second assessment period with the opposite expected trend for low accident volumes. Given the lack of sufficient experimental control and the failure of the authors to notice an important statistical confound, these data should be generalized with great caution. However, the results from the blinking green intersections are perhaps more reliable due to the use of control intersections.

Hermes (1972) reported the results of a study that he conducted in order to determine the safety effects of painted cross walks. In order to determine the relative safety benefits of painted and unpainted crosswalks, 400 intersections which had one painted and one unpainted crosswalk were studied over a 5-year period. For each intersection, the numbers of fatal and nonfatal

accidents were recorded. A second set of intersections was also used to determine the number of pedestrians using each type of crosswalk. It was found that when an adjustment was made for the relative volume of users at each of the two types of crosswalks, twice as many pedestrian accidents occurred in painted crosswalks. The authors suggested that pedestrian's erroneous beliefs about their safety in painted crosswalks caused them to take less care in painted crosswalks than they took in unpainted crosswalks. On the surface, these results present a problem for RHT since compensation was not equivalent on the two types of crosswalks.

In fact, RHT does make a specific prediction of the outcome in this situation. From Equation 2, if we assume that painted and unpainted crosswalks represent two different traffic jurisdictions, one would expect pedestrians to walk at half the rate on painted crosswalks relative to the speed they choose on unpainted crosswalks⁵. Although we do not have any data that would allow us to test this hypothesis accurately, I ask the reader to reflect on his/her own experience in the traffic environment. How many times have you seen a pedestrian running on a painted crosswalk, and how many times have you seen a pedestrian running across the road in an unpainted crosswalk? If your experience is anything like mine, you will have noticed that pedestrians actually choose faster speeds in unpainted crosswalks.

Other On-The-Road Studies of Risk Compensation

The remaining correlational studies that will be reviewed do not integrate well into the three categories we have just considered. They are, however, important to consider because they have all been cited as reflecting heavily on the predictions made by RHT. I will begin with a paper that has often been referred to as the seminal study in compensation research.

Taylor (1964) conducted a study in which he measured the Galvanic Skin Response (GSR) of subjects driving predetermined routes in suburban areas. He hypothesized that if drivers

⁵ This expectation is valid only if the populations using each type of crosswalk are identical, and if driver's behavior remains constant over the two types of crosswalks. While the latter may be a reasonable assumption, it seems likely that the former is probably not true.

continually attempt to maintain their perceived risk at a constant level, GSR rates should remain constant over sections of road that present differing levels of accident risk. Thus, GSR rates served as the dependent variable while the accident risk variables of rate per km in varying road sections, number of side roads per km, traffic flow, and number of lanes of traffic served as independent variables. The results indicated that the GSR rates of subjects remained constant irrespective of changes in the independent variables measured. Thus, it appeared that drivers were adjusting their behavior to external indicators of accident risk in order to maintain constant GSR rates. On the basis of these results Taylor concluded that driving is a "self-paced" task in which drivers attempt to maintain experienced risk at a constant level. The notion that homeostasis operates to maintain perceived risk at some motivationally determined target level, is precisely the view put forward in RHT.

An important result of this experiment was that the constancy of GSR rates was only with respect to vehicle time. That is, GSR rates remained constant over time from the perspective of an observer placed inside the car. In road sections that had a higher density per mile of accident risk factors (i.e., side roads), drivers reduced their speed in order to maintain a constant GSR rate per unit time. Equation 2 predicts exactly this result. That is, in jurisdictions marked by higher accident rates per person mile, drivers should be seen to choose lower speeds.

Williams and O'Neill (1973) conducted a study in which they investigated a more enduring characteristic of the drivers that served in their study. The on-the-road driving records of 447 Sports Car Club of America competition racing licensed drivers were compared to the records of 1,053 matched controls. The authors analyzed the results in order to determine the validity of the commonly held belief that more skill implies greater safety. Not surprisingly, licensed race drivers had more speeding violations than non-licensed drivers, but counter to intuition, the racing drivers had more accidents than their matched controls. Although the results suggest that drivers with greater skill compensate by driving more dangerously, some critics might complain that the compensation shown by licensed racing drivers was not sufficient to maintain their accident risk at the level chosen by the controls. However, it seems obvious that licensed racing

drivers represent a portion of the population that accepts greater than average risk, and as a result, would be involved in a greater number of accidents. Given this, it seems likely that even if compensation for skill level was complete, licensed racing drivers would incur a greater number of accidents than other members of the population.

Before leaving this study, I would like to comment on the authors' conclusion that their results do not support the introduction of special licenses and privileges for more highly trained drivers. From the results presented in the body of the report, I obtained the following ratios of the number of race driver accidents to the number of control driver accidents in the states of Florida, New York and Texas: 2.00, 1.52 and 1.18. The same ratios calculated for speeding violations were 2.91, 3.03 and 1.85 respectively. From these data it is clear that race drivers have a higher rate of speeding violations per accident than normal comparison drivers. If we assume that the purpose of issuing penalties for speeding violations is to control the accident risk associated with excessive speed, it appears that race drivers are not penalized equally to normal drivers. This is because race drivers had more speeding tickets per accident than normal drivers. If the purpose of the law is to limit those behaviors that are related to accident involvement, then perhaps the law should make concessions to more skilled drivers in order to achieve parity for all.

In a study done to determine the effects of the introduction of a mandatory crash helmet law in Nigeria, Asogwa (1980) compared the number of deaths and injuries occurring in two-year periods before and after the introduction of the law. In the two-year post-law period, the number of injuries increased from the pre-law value of 70 to 145 and the number of deaths increased from 5 to 18. The rise in the number of registered motorcycles, from 5303 before the law to 7071 after the law, was not large enough to account for the more than twofold increases in the injury and death rates. The lack of external controls limits the strength of this finding, but does emphasize the problem of over-interpreting results based on these types of analyses. Acceptance of these results with no regard for the lack of control would force us to maintain the untenable hypothesis that safety legislation was responsible for a doubling of accident injury and death

rates. It is interesting to note, however, that historically many traffic researchers have been much more willing to accept empirical results based on poorly controlled designs when the results are in favor of legislation than when they are counter to the goals of legislation (Adams, 1985).

The final study that will be considered in this section is particularly interesting to those who support enforcement and legislation as the appropriate tools for accident control - that is, to the critics of RHT. In February 1976, the Finnish police went on strike for approximately two weeks, during which time the enforcement of speed laws was severely limited. Summala, Naatanen and Roine (1979) conducted a study in which they observed drivers' speeds on highways during the two-week strike and for 1 week after the strike ended. As well, speeds were checked on city streets on 3 days during the strike and 4 days after the strike. A third experiment was also conducted in which a combination of archival speed data and measured speeds were used from observations made on two-way main roads. The authors made three main conclusions with respect to the speed data they collected: (1) the mean speed of travel was 2-3 km per hour higher on city streets during the strike but did not change on highways; (2) the standard deviation of speeds on highways increased by 20%; and (3) the number of drivers exceeding the speed limit by more than 10 km per hour increased by between 50 and 100% on both city streets and highways. The authors results suggested that only relatively few drivers significantly changed their behavior in response to the absence of enforcement.

These results are clearly contrary to the commonly accepted view that without enforcement anarchy would reign on the roads. As well, the small increase in the average speeds driven clearly indicate that something other than speed legislation motivates driver's speed choice. There is the clear suggestion that the gains in accident reduction made by speed legislation may be very small in comparison to the gains that might be made by influencing the motivating factors that maintained the Finnish driver's speed choices in the unenforced period.

Although the correlational studies discussed here by no means exhaust the research conducted on the issue of compensation, they do illustrate some fundamental problems associated with testing RHT in the natural driving environment. It has been shown that

forecasting approaches are unreliable and have suffered from poor experimental control and faulty statistical analyses. In the proceedings of the 1984 General Motors symposium on Human Behavior and Traffic Safety, Warner (1985) commented on the empirical validity of a number of the studies presented here:

This is just a general comment on all of these presentations. I don't know this field and I'm hoping that what I've heard here today is simply a method of trying to present it so that it's easy to understand. I am very concerned about the methodology here.... And regarding the whole notion of trend lines, anybody who has ever worked with them knows what they are worth, and that you can get them going any direction you want them to. (p. 281)

In my inspection of the outcomes of the forecasting done in the studies that have been reviewed here, none seem to do anything much different than would be achieved by visually extending the existing trends. It is curious that, while few of us would be so bold as to use visually generated trends as valid indicators of the future, complex statistical techniques that do little better have gained so much favor in traffic research. If we are to avoid faulty conclusions and make sound legislative judgments, I believe that it is extremely important that we do not overlook sound empirical techniques in the search for sophisticated statistical evidence.

Other more statistically simple pre-test post-test studies have been shown to be advantageous, in the sense that they do not suffer from the introduction of error due to the failure of data to meet the structure required by the statistical techniques. However, many of the authors of these studies have tended to give too much weight to data that want for control. In a number of other studies reviewed here, the lack of appropriate data has caused researchers to test hypotheses that are not directly derivable from RHT, and yet to treat the results as if they did reflect on the validity of the theory. As we have seen, RHT is explicit with respect to its fundamental assumptions; research that purports to test the theory should be similarly explicit with respect to the variables that are studied.

Wilde (1985) has argued against the use of correlational data for the purpose of testing RHT. He argues that in most cases, the problems encountered in correlational, quasi-

experimental studies are associated with an inability to control accurately the variables that pertain to the theoretical statements made in the theory. This is especially problematic in the determination of the total population accident loss. Wilde believes that in uncontrolled environments like the traffic system, total accident loss is impossible to assess accurately. Available statistics, he suggests, are incomplete in that they account for only some of the factors that are related to the total population accident loss. As such, present data are not sufficient to provide an accurate test of RHT in the traffic system. For this reason, Wilde et al (1985) suggest that RHT can be reliably tested only in an experimental context. In the following section, I discuss a number of experimental tests of RHT.

Experimental Tests of RHT

There have been a number of attempts to test RHT in experimental settings. The best examples of these are the studies reported by Cownie (1970), Naatanen and Summala (1975), Veling (1984), Wilde (1985) and Streff and Geller (1988). For each of these studies I will show that, as tests of RHT, they suffer from three weaknesses. In general, these studies are inadequate because they are tests of risk compensation and not RHT, the designs are between subjects rather than within, and the experimental settings lack ecological validity.

In one of the earliest experiments that has consequences for RHT, Cownie (1970) had seven subjects play a game in which they attempted to make monetary gains under varying levels of risk. Over a series of trials, subjects were able to make money by incrementing their scores in amounts of their own choosing. A letter, A to J, indicated the probability with which the chosen increment would be higher than a critical amount. Thus, subjects could base their selection of the appropriate increment on the likelihood that the critical value would be lower than the increment chosen. However, the ten probabilities of exceeding the critical value were not given explicitly to subjects. Over a series of trials, subjects were expected to learn the level of probability that was associated with each letter. On trials in which the critical value was lower than the increment, an

"accident" was said to have occurred. Once involved in an accident, subjects had a fixed probability of 1 in 50 of receiving the punishing consequence.

Cownie suggests that his operational game is an analogue for most real-life hazardous activities in which participants are able to choose between differing levels of risk-taking. He gives five properties that the operational game shares with real-life hazardous events: (1) subjects are motivated to take part by the potential to make money at the game; (2) the threat of a penalty ensures that subjects will be inhibited from acting unreasonably; (3) the activity is hazardous in the sense that an increase in the rate of utility extraction is made at the expense of greater risk exposure; (4) information is presented to the subject that allows for the recognition of different hazards; and (5) information is presented to the subject that allows them to determine the stochastic properties of each hazard.

The results indicated that the amount of risk taken by subjects was independent of the probability of loss. It appeared that subjects were able to compensate for the increase or decrease in risk that was indicated by the letter of the alphabet shown at each trial. Specifically, as the level of hazard subjects were presented with increased or decreased, so did the size of the increment subjects were willing to attempt. Cownie argues that the results suggest that:

improvements in the physical properties of the activity-such as improvements in roads, brakes, road holding etc. are not likely to reduce the overall accident rate. (p. 9)

This conclusion is consistent with RHT because motorists are viewed as belonging to a closed loop system in which feedback concerning the amount of risk associated with a given set of behaviors is used in order to maintain perceived risk at a constant level. However, although the conclusions of this study are consistent with RHT, the study itself is not a complete test of the theory. While the motivational variable of accident cost/likelihood was manipulated by having subjects increment their earnings at varying levels of accident likelihood, no nonmotivational variable was manipulated. Thus, the results support one of the major propositions of RHT but do not support both.

Naatanen and Summala (1976) conducted a study in which they had subjects toss darts at a cork board. Towards the right side of the board two separated vertical lines indicated a "near accident" area, while to the right of this rectangular space an "accident" area was defined. To the left side of the "near accident" zone a score gradient was used that increased as the horizontal distance from the "near accident" area decreased. Two gradients were used that represented differing amounts of reward and differing levels of reward change. One gradient was characterized by moderate overall rewards and a moderate increase in the rewards as the "near accident" area was approached, while the other gradient was characterized by large overall rewards and a steep increase in the rewards as the "near accident" area was approached. The subject's task was to score 1000 points per session. The motivation to complete the task was to avoid the increased time to completion resulting from low scores per throw. The penalty for accident involvement was a deduction of 1000 points from the subject's current score.

Since this experiment was not designed as an explicit test of RHT, the authors did not test hypotheses specifically derived from the theory. However, RHT would predict that the increase in the expected gain experienced for the "high reward" gradient would cause subjects to take more risks with this gradient in order to restore net gain to its maximum level. As predicted, subjects did reduce the horizontal distance of their throws from the "near accident" zone as the reward gradient increased. Unfortunately, over the course of the experiment only 3 penalties (accident losses) were incurred, and thus, the extent to which compensation occurred could not be reliably determined.

Wilde, Claxton-Oldfield and Platenius (1985) reported a study in which they manipulated the expected probability of loss for exceeding a predetermined response latency to a light stimulus. The subject's task was to press a button as close as possible to 800 milliseconds after the onset of a red light. Increasing rewards were given as the response latencies approached the 800 ms time limit. Responses occurring after the 800 ms time limit were penalized with either a .3 or .7 probability. A second manipulation consisted of the presence or absence of trial-by-trial feedback. Of the 110 subjects selected to participate in the study, the highest and lowest twenty

scorers on a subset of Zuckerman's Sensation Seeking Scale and Wilde, Cannon and O'Neill's Concern for Road Safety questionnaire were used. In addition, an individual measure of skill on the button pressing task was obtained for each of the subjects.

Six main research predictions were made: (1) The intercorrelations between the two stimulation seeking scales and the concern for road safety questionnaire should be positive; (2) risk seekers, as indicated by the three measures of risk taking, should score in the upper ranges on the behavioral measure of risk taking - that is, press the button closer to the 800 ms time limit; (3) as RHT predicts, the average response latency to the light should be smaller when the probability of loss is .7 than when the probability is .3; (4) as deduced from RHT, the average loss under the .7 probability of loss condition should be equal to the average loss under the .3 probability of loss condition; (5) performance feedback should lead subjects to make comparatively safe responses following a response in which a loss occurred; and (6) skill should have no effect on the rate of loss. The results with respect to these six predictions were as follows: (1) the correlations between the three risk taking scales ranged between .21 and .38; (2) risk seekers were found to make significantly more responses in excess of 1000 ms than risk avoiders, although all other differences between their risk taking propensities were nonsignificant; (3) as expected, subjects responded significantly more quickly in the .7 probability of loss condition than in the .3 probability of loss condition; (4) counter to expectations, subjects experienced considerably greater losses in the .7 probability of loss condition than in the .3 probability of loss condition; (5) while the removal of feedback did not significantly influence response times, earnings were significantly reduced under conditions of no feedback; and (6) consistent with expectations, performance on the skill test was not related to average response times, earned benefits, or to the frequency of responses below or at 800 ms.

The authors concluded that the results of the study were consistent with RHT, with the important exception that subjects did not show equal losses in the two differing probability of loss conditions. If RHT were true, the monetary loss (total accident loss in the theory) should have been equal in the two probability of loss (cost of an accident in the theory) conditions. This

is because, upon encountering, for example, an increase in the accident loss (trials in which the probability of loss was .7), subjects should reduce their perceived risk to the target level in a manner that would ensure an equal amount of accident loss under the .7 probability of loss condition. In an attempt to explain these findings the authors argued that the motivation to make money may have been secondary to the motivation to succeed in the experimental task. If this was the case, they argue, one might expect to see greater losses in the .7 probability of loss condition than would be expected from RHT. This conclusion is supported by the roughly equal gains that subjects made in each of the probability of loss conditions, but is not borne out by the smaller latencies found for the .7 probability of loss condition.

In an interesting test of the constant risk hypothesis, Veling (1984) unearthed some subtle ways in which subjects take risks. In a simple digit adding task, Veling had subjects choose the number of digits they thought they could add correctly in a known but variable time period. In one experiment, subjects chose the number of digits they thought they could add from a known set of six digits (1 to 6) in time periods of 2, 4 and 8 seconds. In a second experiment, subjects chose the number of digits they thought they could add correctly from 3 different sets of digits. The first set of digits (set 3) consisted of the numbers 8, 9 and 10; the second set of digits (set 7) consisted of the numbers 4 to 10; and the third set of digits (set 11) consisted of the numbers 0 to 10. In the third and final experiment, the number of digits in a set was varied between subjects, in contrast to the within-subject manipulation conducted in experiment 2. In all three experiments, subjects were motivated to solve equations with as many digits as possible by being encouraged to make as many points as possible. Points were awarded for each digit in a correctly solved equation. Penalties for incorrectly solving an equation consisted of a reduction in the total score by the number of digits in the incorrectly solved equation, and of performing a digit typing task before continuing with the experiment.

The rationale for the manipulation in Veling's Experiment 1 was that compensation should cause subjects to choose the number of digits to add such that the probability of making an error remained constant over the three differing time periods. Specifically, subjects were expected to

compensate for the increased availability of computation time by attempting to solve equations with a larger number of digits. In Experiment 2, the input uncertainty of the problem to be solved was varied by using sets of numbers that consisted of differing numbers of digits. It was argued that sets with more digits would cause a greater uncertainty with respect to the problem to be solved than sets with fewer digits (i.e., if you choose to solve a 3-digit equation when the set has three digits in it (set 3), you can be certain of which three digits will appear; the same can not be said for the 11-digit set). On the basis of this reasoning it was predicted that the influence of compensation would result in no changes in the error probability for the time allotted or the input uncertainty. Finally, Experiment 3 was conducted to determine if between-subjects changes in the input uncertainty would influence the error probability.

The results of Experiment 1 were consistent with RHT. Subjects varied the number of digits they attempted to add such that the probability of incorrectly adding the digits remained the same for varying calculation intervals. The results of Experiment 3 were also consistent with RHT. The size of the digit set, and thus the uncertainty of the difficulty of the task (input uncertainty), did not influence the probability that subjects would make an error. As in Experiment 1, complete compensation occurred to a change in accident risk. However, the results of Experiment 2 were more difficult to interpret. It was found that as the input uncertainty of the task increased, subjects made less errors. If complete compensation to the input uncertainty had occurred, subjects should have made an equal number of errors in all the input uncertainty conditions. A second result in Experiment 2 was that when subjects were less certain of their ability to complete the addition task, they made more errors. Again, if complete compensation was taking place equal numbers of errors should have occurred irrespective of the subject's certainty in their ability to complete the task. These results suggest that there are circumstances in which subjects' abilities to judge the amount of risk present in their environment can cause them to take more or less actual risk than they wish to accept.

In an attempt to determine the effects of seatbelt wearing on risk taking and risk perception, Streff and Geller (1988) conducted an experiment in which they manipulated seatbelt wearing

both within and between subjects. Subjects drove 15 laps of an oval track in a go-kart on two different occasions (Phase 1 and Phase 2). Four conditions were determined by the driver's seatbelt use on each of the two occasions. The order of seatbelt use in each of the four conditions was as follows: seatbelt - no seatbelt, seatbelt - seatbelt, no seatbelt - seatbelt and no seatbelt - no seatbelt. The authors predicted that drivers who were unbelted in Phase 1 and belted in Phase 2 would increase their speed in order to compensate for the decreased cost of having an accident while using a seatbelt. It was also expected that compensation would operate in the opposite direction for drivers who were belted in Phase 1 but unbelted in Phase 2. Lastly, no change in lap latencies was expected when no change in belt use occurred. The authors were also interested in the extent to which the effect of safety interventions could be shown in within-subject versus between-subject designs. It was expected that since compensation occurs in response to feedback concerning environmental changes, no between-subjects difference in lap latencies would be found.

The results of this study indicated that compensation did occur when seatbelt use was analyzed as a within-subject factor, but that no compensation occurred when seatbelt use was analyzed as a between-subjects factor. This result indicates that studies that have assessed the effects of a safety measure between groups of subjects might not have found compensation where compensation actually existed. Unfortunately, the results were not as clear when seatbelt use was analyzed as a within-subject factor. Although lap latencies decreased most when subjects experienced a change from no belt use to belt use, the reduction of lap latencies when subjects experienced a change from belt use to no belt use was no smaller than the reductions that occurred in the other two conditions. Thus, compensation took place when subjects experienced an increase in safety but did not exist for a reduction of safety.

Unfortunately, the data presented by Streff and Geller are confounded by differences in performance between the four experimental groups. Although in the first phase of the study there was no clear difference in lap latencies for the belted drivers, an apparently large difference in lap latencies existed between the two groups of unbelted drivers. This difference was due to

significantly slower lap latencies for the unbelted - belted condition. Two sorts of analyses were performed in an attempt to assess this problem. First, independent groups t-tests were conducted for each mean difference observed for each of the fifteen laps. Second, condition was entered in to an analysis of variance as a between-subjects factor. Since no significant differences were found for any of these tests, the authors concluded that no difference in lap latencies existed between any of the four experimental groups. However, neither of these analyses is appropriate for testing for mean differences between the conditions. The t-test approach results in a considerable lack of power because the pattern of effects over the fifteen laps is ignored. The second approach ignores the influence of the important covariate of practice. Since mean lap times improved significantly over the 30 laps driven in each condition, the effect of condition was washed out by the influence of practice. One simple approach to the analysis that accounts for the consistency of the effect and partials out the influence of practice is to conduct a difference scores t-test on the mean differences in lap times for each lap. If in Phase 1 of the experiment (the first 15 laps driven) difference scores were consistently larger for the unbelted - belted condition than for the unbelted - unbelted condition, the hypothesis of no mean differences in lap latencies in Phase 1 for the two unbelted groups would be rejected. If this analysis is performed, an enormous and highly significant difference between the two Phase 1 unbelted groups results. These results show that the experimental groups used in Streff and Geller's study were not equivalent with respect to their baseline scores on the dependent variable. As such, the outcome of the experiment should be interpreted with great caution.

As tests of RHT, the experimental studies considered here have three main weaknesses. The first and most important weakness is their failure to incorporate the manipulation of both motivational and nonmotivational variables. While Cownie did manipulate the probability of loss and therefore the accident loss, he did not place any arbitrary or specific limitations on subject's behavior. A similar omission occurred in Naatenen and Summala's study. Since the study was not designed as an explicit test of RHT, the failure to manipulate a nonmotivational variable is understandable. Wilde, on the other hand, did manage to measure what might be considered a

nonmotivational variable. In addition to manipulating the cost of an accident, Wilde and his colleagues assessed each subject's skill at performing the light extinguishing game.

Unfortunately, skill level was a between-subjects factor, and given the results obtained by Streff and Geller, might not be expected to show compensation in the same way as a within-subject variable. The Veling and Streff and Geller studies, on the other hand, were limited to the manipulation of nonmotivational variables. Streff and Geller manipulated seatbelt wearing, while Veling manipulated time and input uncertainty. Since none of the studies conducted within-subject manipulations of both motivational and nonmotivational variables, none of them constitutes a complete test of RHT.

A second problem with these studies is their general lack of ecological validity. It is not at all clear that the determinants of dart tossing and button pressing in the face of risk will carry over to the natural driving environment. Finally, these studies differ with respect to the ways in which variables have been manipulated across subjects. The only study in which both a motivational and nonmotivational variable were manipulated lacked a within-subject manipulation of the nonmotivational variable.

In summary, a significant problem in risk compensation research is revealed. Correlational field studies do not offer sufficient control for the manipulation of variables or for the assessment of total accident loss. Experimental studies, on the other hand, sacrifice ecological validity for control. Although these problems operate against each other, a compromise between them could be reached with the use of an ecologically valid method of simulating real road driver behavior that allows for the control and measurement of the variables relevant to RHT. The study reported here strikes this compromise by making use of an interactive driving simulator.

A Simulated Driving Test of RHT

The purpose of this study is to conduct a test of Wilde's risk homeostasis theory that includes within-subject manipulations of both motivational and nonmotivational variables. This was made possible by the use of an interactive driving simulator. The simulator used in this study consists of a video screen on to which animated road traffic environments are presented, and a driver's compartment, complete with controls from a Chevrolet Sprint. Routes can be driven that include right and left turns, traffic lights, route indicators, pedestrians, and other cars. The design of the simulator allows the driver to choose any route or speed he/she wishes. These features make the simulator an excellent platform on which to observe and control behavior while the subject is involved in a task functionally similar to real road driving.

The general approach of this study was to have subjects drive a single route under differing sets of experimental conditions. Conditions were determined by a high and low cost of an accident and a high and low speed limit. Since each subject drove at both levels of each of the two variables, both factors were manipulated within subjects. As well, since it was possible to measure all accident involvement, the dependent measure specified in RHT was assessable. It was hypothesized that fewer accidents would occur in the high than low accident cost condition (Prediction 1), while no difference in the number of accidents would be found between the high and low speed limit conditions (Prediction 2). The theoretical rationale for each of these predictions is as follows:

Prediction 1:

If only those factors that influence the target level of risk have an influence on the accident loss, and since changes in the cost of an accident will affect the target level of risk in an opposite direction to the change, the accident loss should vary as an inverse function of the cost of an accident. Therefore, an increase in the cost of an accident should result in a decrease in the number of accidents.

Prediction 2:

Since factors that do not change the target level of risk do not influence the accident loss, and since limits on speed do not influence the target level of risk, increases or decreases in the speed limit should not affect the accident loss.

METHOD

Subjects

Subjects were 11 female and 13 male undergraduate and graduate students at Simon Fraser University. The ages of subjects ranged from 18 to 37. All subjects were volunteers and had current British Columbia drivers licences. Subjects were paid 10 dollars for their participation in the study.

Apparatus

An interactive driving simulator was used that consists of a video screen, video projector, driver's compartment from a Chevrolet Sprint, an Epson 386/20 personal computer and an Iris Version 4 personal work station. The driving environment generated by the simulator consists of a predefined, square and flat world. As the driver maneuvers through the world by manipulating the car's controls, his/her position is continuously monitored by the Epson pc. At a rate of once every .03 seconds, information on the position of the car in the predefined world is relayed to the Iris, which then draws a picture of what the world would look like from the position of the car. This image is then projected on to the video screen in front of the driver. The position, size, color etc, of objects in the predefined world are known to the Iris and can be drawn by it. However, the properties of those objects with respect to a car travelling amongst them are not known. For example, while the Iris can draw a street sign, it does not know that contact with it will cause damage to an automobile. As a result, information concerning the eventualities of striking solid objects must be relayed from the pc to the Iris. In a sense, two different worlds are known by the Iris and the pc. For the pc, the world consists of a square, flat, plain with solid objects at certain points throughout the plain. For the Iris, a world of "visual scenes" exists.

Although the Iris can draw a number of scenes including buildings, bridges and tunnels, and knows of a number of different routes incorporating such road features as two-lane roads, curves, and highway on-ramps and off-ramps, the pc knows of only one route of solid objects. As the simulator is developed, the solid objects seen by the Iris will be programmed in to the pc so that a greater number of driving environments will be available. At present, however, only one object route is available.

This route consists of a grid of interconnected blocks. Each block is made up of two sections of road at 90 degrees to each other that cross at their respective midpoints. From a driver's perspective, the roadway consists of a straight road with intersections spaced equal distances apart. The route the driver follows, however, is not straight. A driver is instructed by route indicators to turn either left or right at certain intersections. Upon turning, the driver again sees a straight road with a series of equidistant intersections. By snapping blocks together in line or at right angles, and using route indicators to instruct the driver where to turn, a route with a series of straight sections and right-angle left or right turns was created. Straight sections varied between four and seven blocks in length.

There are 12 different blocks available, each of which was used at least once in the route. Blocks vary in the existence of traffic lights at the intersection - some blocks have traffic lights and others do not; the existence and direction of route indicators - some blocks have no route indicators while others have either a left or right route indicator; the existence of pedestrians - some blocks have a single pedestrian and others have no pedestrian; and the existence of light triggers - some of the traffic lights operate on a random onset basis, while others are triggered by the car passing a predetermined point in the road. All of the blocks are the same size and all show two-lane roads.

As the driver negotiates the route, data on a number of behavioral measures are collected and stored in a binary file at a rate of once every .03 seconds. A program was written that selected the most important behavioral measures from the binary file and wrote them in to an ASCII file. The behavioral measures written to the ASCII file were as follows: (1) the position

of the car in the two-dimensional square world; (2) the speed of the car in km/h; (3) the braking force given on a scale of 0 to 12000; (4) a dummy variable that indicated 0 when the car was not over the center line and 1 when the car was over the center line; (5) three dummy variables with values of 0 or 1 that indicated the type of solid object hit; (6) the steering wheel position in degrees to the right of center; and (7) the gear currently being used. A FORTRAN program was written in order to create the following behavioral measures from the data in the ASCII file: (1) the average cornering speed in km/h (*corner speed*); (2) the average speed in km/h for the entire session (*average speed*) when the car is not stopped (*average moving speed*) and when the car is travelling at greater than 30 km/h (*average speed 30*); (3) the amount of time spent driving over 30 km/h (*time over 30*), 40 km/h (*time over 40*), 50 km/h (*time over 50*) and 60 km/h (*time over 60*); (4) the maximum speed driven at any point in the session (*fastest speed*); (5) the number of times the steering wheel passed zero degrees (*steering corrections*) and the amount of time spent over the center line (*time over line*).

Design

The design used was a 2x2x2 mixed factorial with conditions presented in a latin square order. The within-subject independent variables were the cost of an accident (*cost*) and the speed limit (*speed*). In addition to the within-subject manipulation of speed limits a between-subjects nonmotivational variable was also manipulated. For reasons discussed later in this report, the severity of punishment for speeding (*fine*) was manipulated between subjects.

Procedure

The subject's task was to operate the simulator on five successive "trips" or sessions. The first session was a 10-minute practice run and the four subsequent sessions were 10-minute experimental runs. Conditions based on the speed limit and the cost of an accident were set for

each of the experimental runs. On each run, the cost of an accident was a monetary penalty of either 50 cents or 2 dollars and the speed limit was either 40 km per hour or 60 km per hour. The monetary penalty for exceeding the speed limit and the probability of passing through a speed check-point were manipulated between subjects. For half of the subjects the probability of passing through a single speed check in each route was .25, and the fine for being found over the limit was 1 dollar for infractions of between 0 and 10 km/h over, and 2 dollars for being found more than 10 km/h over the limit. For the other half of the subjects, the probability of passing through a single speed check in each of the four routes was 1, and the fines for being found between 0 and 10 km over, and more than 10 km/h over, were 1 dollar and 4 dollars respectively. The motivation for subjects to place themselves at risk of an accident and at risk of being caught speeding was a monetary incentive for driving the route more quickly than the average speed achieved by other drivers. This incentive remained constant over all four trials and amounted to 5 cents for every second the subject was faster than the average time.

Although subjects were given the foregoing description of the experimental manipulations, a number of the penalties and incentives were not actually determined in this way. In fact, although subjects believed that they would be checked for speeding, their speed was never checked, and no fines were ever issued. As well, incentives were not contingent on subjects' performance. Incentives of a predetermined amount were issued in an order that was counterbalanced with respect to the presentation of experimental trials. Therefore, neither the incentives earned nor speeding penalties varied systematically over the levels of the independent variables. As well, subjects believed that each route they would drive was exactly 10 km in length. In fact, subjects were timed and told that they had completed the route when they had been driving for 10 minutes. Post-participation questioning indicated that subjects were not aware of the discrepancies between the advertised and actual conditions.

Approximately 15 minutes was required to describe the experimental task to each subject. The order in which instructions were given was as follows:

- (1) Subjects were told that they would be paid \$10 for their participation, and that their performance would determine whether or not this amount would increase or decrease. Subjects were also ensured that losses of more than 10 dollars would not be charged against them.
- (2) The independent variables, and the costs associated with them were described.
- (3) The incentive of 5 cents for every second they were faster than the average time done so far by experimental subjects in each of the four conditions, was described.
- (4) The subject was taken through a short driving session in which the experimenter indicated the types of hazard that would be encountered, the operation of the route indicators, and the driving errors that would constitute an accident. Accidents were described to subjects as constituting either three crosses of the center line (*crossed yellow*) - defined as the center line being to the right-hand side of the bottom left corner of the video screen; as running a red light (*run stop*) - defined as crossing the white stop line at the edge of the intersection when the light was red; or as hitting any solid object, including the curb (*crashes*).
- (5) Subjects were asked to read a short description of the experimental task and indicate that they understood the instructions by signing the form.
- (6) Subjects were seated at the controls of the simulator, and after being given a few short instructions on the operation of the controls, the 10-minute practice session began. At the beginning of the practice session, subjects were asked to drive as they would if the accident penalties were in effect. During the session, subjects were given oral warnings by the experimenter each time they made an error that constituted an accident.
- (7) Before the beginning of each experimental session, subjects were informed of the accident cost and the speed limit for the following session, as well as the incentives that were available for driving quickly.
- (8) Following each experimental session, subjects were informed of the number of accidents they had been involved in. They were not told if they had been caught for speeding, or the total amount of incentives they had earned, until all four experimental sessions had been completed.

Unfortunately, the behavioral data collected from the sessions could not be used to evaluate whether or not an accident had occurred. While data were collected on the number of lane crossings, it was not possible to have the computer indicate to subjects when they had crossed the center line. Since, for the computer, the point at which the car crossed the center line was not the point at which subjects were instructed a cross had occurred, the data collected by the computer did not reflect the actual number of lane crossings that occurred. This problem also existed for crossings of the white line during red lights. To accommodate for this problem, each incidence of line crossings and red light infractions were recorded by the experimenter during the experimental sessions. Even though the experimenter was not blind with respect to the experimental conditions, bias was not considered to be a factor. Since subjects were fined for each accident they were involved in, it is unlikely that they would have accepted a penalty that clearly was falsely assessed by the experimenter. In fact, the determination of a line crossing was not based on ambiguous or perceptually demanding dimensions. In all cases, it appeared obvious to both the experimenter and the subject that an infraction had occurred.

RESULTS

In addition to the dependent measures of crashes, crossed yellow and run stop, two composite measures of accident involvement were created. A measure of the number of accident related errors (*errors*) was generated by forming a linear combination of the number of crashes, the number of stop lights run, and one third of the number of crosses of the yellow line. This particular linear combination assumes that each cross of the center line represents an equal increment in the accident risk. Another possibility, however, is that subjects perceived a nonlinear (most likely increasing) increment in accident risk with each successive cross of the yellow line. If this were true, the number of errors made would not accurately represent the actual perceived accident loss. For this reason, a variable that represents the total actual accident loss (*accidents*) was created. The algorithm used was as follows:

$$\text{accidents} = \text{solid} + \text{stop} + \text{integer}(1/3 * \text{yellow})$$

where accidents = the total number of accidents, solid = the number of times a solid object was hit, stop = the number of times a stop light was run, yellow = the number of crosses of the center line and integer rounds the bracketed value down to the nearest whole number.

Means and standard deviations for the two levels of each of the three factors and for each of the four accident involvement dependent variables are given in Table 1. The factors of speed and cost are within-subject factors and the fine variable is the between-subjects factor. Table 2 gives means and standard deviations of the three independent variables for the remaining dependent measures.

Table 1. Means and standard deviations for the cost of an accident, the speed limit and the cost of a speeding violation, for each of the four accident loss variables.

| | | Cost | | Speed | | Fine | |
|----------------|---|-------------------|------|-------|------|------|------|
| | | mean | sd | mean | sd | mean | sd |
| accidents | L | 1.29 ^d | 1.02 | 0.81 | 0.90 | 1.08 | 1.05 |
| | H | 0.58 | 0.76 | 1.06 | 0.88 | 0.79 | 0.85 |
| errors | L | 1.56 ^d | 1.03 | 1.08 | 1.05 | 1.32 | 1.12 |
| | H | 0.84 | 0.85 | 1.33 | 0.95 | 1.08 | .87 |
| run stop | L | 1.02 ^d | 0.96 | 0.62 | 0.91 | 0.81 | 0.94 |
| | H | 0.43 | 0.71 | 0.83 | 0.86 | 0.65 | 0.84 |
| crossed yellow | L | 1.31 ^a | 1.27 | 1.04 | 1.03 | 1.15 | 1.37 |
| | H | 0.83 | 0.91 | 1.10 | 1.22 | 1.00 | 0.83 |

a means are different at alpha < .05
b means are different at alpha < .01
c means are different at alpha < .005
d means are different at alpha < .001

Table 2. Means and standard deviations for the cost of an accident, the speed limit and the cost of a speeding violation, for each of the nonaccident dependent variables.

| | | Cost | | Speed | | Fine | |
|----------------------|---|-------|------|--------------------|------|-------|------|
| | | mean | sd | mean | sd | mean | sd |
| average speed | L | 25.79 | 3.67 | 24.52 ^c | 3.36 | 26.42 | 3.76 |
| | H | 25.71 | 3.33 | 26.98 | 3.19 | 25.08 | 3.09 |
| average moving speed | L | 35.13 | 5.49 | 32.73 ^d | 4.87 | 36.13 | 5.16 |
| | H | 34.39 | 4.72 | 36.79 | 4.53 | 33.40 | 4.72 |

| | | | | | | | |
|----------------------|---|--------|-------|---------------------|-------|--------|--------|
| average speed | L | 45.36 | 6.74 | 42.10 ^d | 6.76 | 47.31 | 6.93 |
| | H | 45.17 | 6.37 | 48.41 | 4.47 | 43.21 | 5.42 |
| time over 30 | L | 283.09 | 44.55 | 282.98 | 46.19 | 276.44 | 38.44 |
| | H | 278.18 | 41.06 | 278.29 | 39.22 | 284.83 | 46.58 |
| time over 40 | L | 158.27 | 82.85 | 115.77 ^d | 83.02 | 171.58 | 74.03 |
| | H | 157.06 | 77.91 | 199.56 | 49.58 | 143.75 | 84.02 |
| time over 50 | L | 85.17 | 70.42 | 49.36 ^d | 63.60 | 99.46 | 67.25 |
| | H | 80.27 | 66.86 | 116.09 | 55.89 | 65.98 | 65.93 |
| time over 60 | L | 29.94 | 35.90 | 20.75 ^c | 34.36 | 45.42 | 39.86 |
| | H | 29.06 | 34.73 | 38.25 | 34.04 | 13.58 | 19.72 |
| fastest speed | L | 71.58 | 17.22 | 63.89 ^d | 18.63 | 76.35 | 18.50 |
| | H | 70.86 | 17.63 | 78.54 | 12.29 | 66.08 | 14.55 |
| corner speed | L | 17.29 | 5.26 | 17.02 | 5.03 | 16.79 | 3.09 |
| | H | 16.81 | 3.76 | 17.08 | 4.07 | 17.31 | 5.68 |
| steering corrections | L | 407.02 | 95.05 | 392.79 | 93.69 | 375.63 | 100.48 |
| | H | 401.69 | 97.55 | 415.91 | 97.54 | 433.08 | 82.36 |
| time over line | L | 29.23 | 5.47 | 29.46 | 5.30 | 30.63 | 7.45 |
| | H | 30.23 | 6.61 | 30.01 | 6.76 | 28.83 | 4.11 |

a means are different at alpha < .05

b means are different at alpha < .01

c means are different at alpha < .005

d means are different at alpha < .001

Interaction effects were not included in Tables 1 or 2 because the majority of interaction tests were not significant. There were no significant three-way interactions, and no significant two-way interactions between cost and speed or between cost and fine. In fact, for all 45 of these tests, only two p-values were below .2. However, significant two-way interactions were found between speed and fine for the average moving speed ($p=.043 < .05$), the average speed over 30 ($p=.004 < .005$), the number of steering corrections ($p=.017 < .05$), the time spent over 40 ($p=.004 < .005$), the time spent over 50 ($p=.003 < .005$), and the fastest speed driven ($p=.011 < .05$). The origin of these interactions can be seen from the patterns of the speed and fine cell means presented in Table 3.

Table 3. Cell means for both levels of speed and fine for the variables average moving speed, average speed 30, steering corrections, time over 40, time over 50 and fastest speed.

| | Fine | | | |
|----------------------|--------|--------|--------|--------|
| | Low | | High | |
| | Speed | | Speed | |
| | Low | High | Low | High |
| average moving speed | 34.87 | 37.38 | 30.58 | 36.21 |
| average speed 30 | 45.54 | 49.08 | 38.66 | 47.75 |
| steering corrections | 379.79 | 371.46 | 405.79 | 460.37 |
| time over 40 | 152.54 | 190.62 | 79.0 | 208.50 |
| time over 50 | 85.33 | 113.58 | 13.37 | 118.61 |
| fastest speed | 72.33 | 80.38 | 55.46 | 76.71 |

With the exception of steering corrections, an inspection of the simple main effects of the speed limit at each level of the severity of the speeding fine shows larger values for the high speed limit condition than for the low speed limit condition. However, it is clear for all of the variables that this trend is larger at the high level of speeding fine than at the low level of speeding fine. Thus, for the variables that are measures of the speed driven, the significant interactions appear to be due to subjects paying more attention to the speed limits as the severity of the fine for speeding increased.

An examination of Table 1 shows that for both the number of errors and the number of accidents, large and highly significant reductions occurred with an increase in the cost of an accident. As would be expected, large and significant decreases also occurred in the number of crosses of the center line and the number of stop lights run. However, no significant mean differences between the high and low speed limit conditions were found for any of the four accident variables. Therefore, both of the major predictions are supported by these data.

Inspection of Table 2 indicates the lack of significance for any of the variables not directly related to the accident loss. No significant speed or wheel correction differences existed between the high and low cost of an accident conditions. As well, the number of crosses of the yellow collected by the computer showed no significant difference between the two cost of an accident conditions. These findings indicate that subjects are extremely discriminating with respect to the compensations they make in response to changes in the cost of an accident.

Table 2 also shows that the manipulation of speed limits had large and highly significant overall effects on subject's speed choices. With the exception of cornering speed, large and highly significant reductions in speed were found with reductions in the speed limit, for all the speed variables. It is interesting to note that higher speed limits did not lead to faster cornering speeds. This result, in combination with the lack of significant differences in the number of accidents for the two speed limit conditions, suggests that drivers do not use an increase in speed limit as an opportunity to take increased risk. Further support for this conclusion can be found in the marginally significant differences in the number of steering corrections. In the high speed

limit condition drivers were apparently maintaining a higher level of control on the wheel by making more frequent adjustments to the wheel position.

Assumptions and Practice Effects

In order for the statistical tests performed here to be valid, a number of assumptions concerning the structure of the data must be met. For within-subject analyses of variance with a between-subjects factor, the assumptions are as follows:

- (1) normality of population distributions;
- (2) homogeneity of within-subjects treatment variances;
- (3) equal variance of the differences between treatment scores within each level of the non-repeated factor; and
- (4) homogeneity of dispersion matrices.

While plots of the speed, steering corrections and time-over-line variables all appear symmetrical and unimodal, plots of the sample distributions of the accident variables indicate that the populations probably have Poisson distributions. Although, in general, the violation of assumptions are not equally damaging for repeated measures and factorial designs, the violation of the normality assumption in repeated measures designs leads to conclusions similar to those for factorial designs (Keppel, 1982). Koopman (1992) suggests that as long as the within-cells population distributions have similar skew, no real reduction in the validity of the F ratios results from the normality violation. Since all of the within-cell distributions for the accident variables have positive skew, the normality violation is almost certainly of little consequence for the validity of the F ratios.

Inspection of Tables 1 and 2 indicates that while, in most cases, the within-cells sample variances show quite high levels of homogeneity, there does appear to be a tendency for the variances for the accident variables to be lower in the high accident cost condition. Although the violation of this assumption has little impact on the between-subjects factor, the repeated factors

F ratios may be influenced by departures from within-cells homogeneity of variance (Collier, Baker, Mandeville and Hayes, 1967). However, Collier et al show that even gross departures from homogeneity will influence F probabilities by between only 2 to 3 percent. Thus, the effects of the small departures found here are probably too modest to have any measurable effect on the calculated p values.

The assumption of equal variances of the differences between scores for pairs of treatment levels is not applicable here. Since there are only two levels of each repeated factor, there is only one off-diagonal covariance, and thus no inequalities are possible. However, Box's M tests for the homogeneity of dispersion matrices were conducted. The results indicated that no statistically significant differences between dispersion matrices existed for any of the four accident loss dependent measures. However, with the exception of the number of steering wheel corrections and the fastest speed driven, statistically significant differences between dispersion matrices were found for all the other dependent measures.

The analysis of the data structure suggests that there were no influential violations of the four mixed factorial analysis of variance assumptions for any of the four accident loss variables. Therefore, the p values indicated in Table 1 are not biased for these variables. However, most of the behavioral measure variables suffered from heterogeneity of dispersion matrices problems. Nevertheless, inspection of the p values for the cost variable indicates that the smallest p value was .174 (for the average moving speed variable), while none of the other p values was less than .4. Since major violations of this assumption lead to only small percentage changes in the calculated p values, none of the statistical conclusions would be reversed as a result of this problem. The same conclusions can be made with respect to the statistical tests of the effects of speed limits. Since all of the dependent variables for which dispersion matrices were heterogeneous were significant at between .004 and .0001, the violation of this assumption is not sufficient to reverse any of the statistical conclusions.

The remaining set of effects for which statistical analyses were conducted represent the influence of practice and learning in the driving task over trials. The tests presented in Table 4

represent the effects of the order in which subjects received conditions, over all conditions and within individual conditions. Since the effects of order are most important for the accident loss variables, only the tests for the four accident loss measures are presented in Table 4.

Table 4. Overall and within-condition effects of order of presentation on the total number of accidents, total number of errors, number of stop lights run and number of crosses of the yellow.

| Dependent variable by condition tested | F | significance of F |
|--|------|-------------------|
| NUMBER OF ACCIDENTS | | |
| Overall | 1.17 | .327 |
| Low cost, low speed limit | .88 | .468 |
| Low cost, High speed limit | .44 | .728 |
| High cost, low speed limit | .81 | .503 |
| High cost, high speed limit | .91 | .454 |
| NUMBER OF ERRORS | | |
| Overall | 1.04 | .377 |
| Low cost, low speed limit | .65 | .590 |
| Low cost, High speed limit | .37 | .772 |
| High cost, low speed limit | .74 | .539 |
| High cost, high speed limit | 1.52 | .239 |
| RUNS OF STOP LIGHTS | | |
| Overall | 1.22 | .306 |
| Low cost, low speed limit | 1.15 | .354 |
| Low cost, High speed limit | 1.17 | .344 |
| High cost, low speed limit | .89 | .464 |
| High cost, high speed limit | 3.41 | .037 |

NUMBER OF LINE CROSSES

| | | |
|-----------------------------|-------|------|
| Overall | 2.214 | .092 |
| Low cost, low speed limit | .53 | .670 |
| Low cost, High speed limit | .48 | .702 |
| High cost, low speed limit | .08 | .972 |
| High cost, high speed limit | .93 | .446 |

Under the null hypothesis that the order of presentation has no effect on each of the four dependent variables presented in Table 4, one p value of less than .05 would be expected by chance alone in these twenty tests. Since only one p value of less than .05 was obtained, we can conclude with confidence that the order of presentation of conditions had no effect on subject's scores.

A second analysis was undertaken in order to determine whether or not differential practice effects existed. This analysis amounts to a test of the interaction between presentation position and condition. Anova summary tables for the differential practice effects analyses are presented in Tables 5, 6, 7 and 8. Again, only analyses for the four accident loss variables are reported.

Table 5. Analysis of variance summary table of the effects of treatment position and condition on the number of accidents.

| Source of Variation | Sum of squares | DF | Mean square | F | Significance of F |
|-----------------------------------|----------------|----|-------------|------|-------------------|
| Condition | 13.71 | 3 | 4.57 | 5.48 | .002 |
| Treatment Position | .88 | 3 | .29 | .35 | .789 |
| Position by condition interaction | 6.37 | 9 | .71 | .85 | .573 |

Table 6. Analysis of variance summary table of the effects of treatment position and condition on the number of errors.

| Source of Variation | Sum of squares | DF | Mean square | F | Significance of F |
|-----------------------------------|----------------|----|-------------|------|-------------------|
| Condition | 14.39 | 3 | 4.80 | 5.23 | .002 |
| Treatment Position | .94 | 3 | .31 | .34 | .796 |
| Position by condition interaction | 7.33 | 9 | .81 | .89 | .541 |

Table 7. Analysis of variance summary table of the effects of treatment position and condition on the number of crosses of the center line.

| Source of Variation | Sum of squares | DF | Mean square | F | Significance of F |
|-----------------------------------|----------------|----|-------------|------|-------------------|
| Condition | 6.45 | 3 | 2.15 | 1.73 | .168 |
| Treatment Position | 1.61 | 3 | .54 | .43 | .730 |
| Position by condition interaction | 12.93 | 9 | 1.44 | 1.15 | .335 |

Table 8. Analysis of variance summary table of the effects of treatment position and condition on the number runs of stop lights.

| Source of Variation | Sum of squares | DF | Mean square | F | Significance of F |
|-----------------------------------|----------------|----|-------------|------|-------------------|
| Condition | 9.21 | 3 | 3.07 | 4.33 | .007 |
| Treatment Position | 2.78 | 3 | .90 | 1.27 | .289 |
| Position by condition interaction | 6.37 | 9 | .71 | 1.00 | .447 |

None of the interaction effects in Tables 5 to 8 is large or significant. Therefore, the effects of differential practice can confidently be ruled out. As well, assessment of the position main effects indicates that no significant treatment position effects were present.

Discussion

Risk Compensation to Changes in Accident Cost

The approximately two-fold reduction in the number of accidents and errors that occurred as a result of the increase in accident cost supports the view put forward in RHT that the manipulation of motivational variables does cause a reduction in the total frequency of accidents. However, consistent with Wilde et al's (1985) finding, subjects failed to compensate fully for the change in accident cost. In order to maintain a constant level of accident risk, accident losses should have been similar in the two accident cost conditions. Since the monetary cost of an accident was four times greater in the high accident cost condition, subjects should have caused four times fewer accidents in that condition. There appear to be four possible reasons for the failure to find complete compensation in the present experiment:

- (1) compensation does not necessarily occur only with respect to the *monetary* losses associated with accidents;
- (2) Wilde's 1985 deduction that compensation should occur only with respect to accident losses is a false deduction from RHT;
- (3) this experiment did not provide the appropriate feedback for compensation to occur; or
- (4) RHT is false.

Although this experiment does not provide explicit evidence for or against the first of these problems, it is likely that losses other than monetary penalties were indeed present. The assumption that the 2 dollar penalty represents four times the actual accident loss as the 50 cent penalty amounts to the claim that a monetary penalty of 0 cents represents no accident loss. Observations of subjects' behavior in pilot trials suggest that this assumption is probably false. For the first few pilot subjects there was no explicit accident penalty, yet they showed an obvious motivation to avoid accidents. Perhaps in these trials subjects were motivated to avoid accidents by a need to perform well for the experimenter, or in comparison to others, or merely

for their own satisfaction. Although this effect was not explicitly studied, these observations suggest that some level of accident loss probably existed in the absence of monetary penalties. Since this problem would essentially raise the baseline accident loss, the four-fold increase in the monetary accident penalty almost certainly represented an increase in accident loss of less than a factor of four. Therefore, even though homeostasis (complete compensation) did not occur with respect to the monetary accident loss, it still may have occurred with respect to the *total* accident loss. A way in which future experimentation may attempt to ameliorate this problem will be discussed at the end of this section.

The second reason for the failure to find complete compensation to changes in accident cost may be that RHT does not in fact predict that a four-fold reduction in accident frequency should occur. Wilde et al (1985) deduced that subjects in their experiment would compensate only with respect to the losses associated with accident involvement. The influence of the gains associated with risky behavior on the propensity of subjects to incur losses was ignored. However, in a subsequent publication Wilde (1988) clearly states that:

Each individual chooses a configuration of behaviors ... such that the positive difference between the prevailing expected gains and losses associated with that set of behaviors is maximized. The amount of subjective accident risk at which this maximization occurs defines the target (optimal) level of accident risk. (p. 452)

This quote clearly shows that, since the target level of risk determines the accident loss, variations in the gain structure of the environment will affect the accident loss. Since, in the present experiment, subjects believed that they could gain incentives through faster driving, there is reason to believe that their behavior was a function of the relative gains associated with risky driving. For example, suppose that each 10-second decrease in driving time was associated with a unit increase in the accident frequency. In the high accident cost condition, the expected net benefit associated with a 10-second decrease in driving time would be $10 * (\$.05) - \$2.00 = -\$1.50$. However, in the low accident cost condition, the expected net benefit associated with the same 10-second decrease would be $10 * (\$.05) - \$.5 = \$0$. These calculations show that for equivalent configurations of behaviors, the differences between the prevailing expected gains

and losses in each of the two accident cost conditions are not equal. In the low accident cost condition, for example, faster driving would not be expected to result in equally severe losses as in the high accident cost condition. Therefore, if R is determined in the way Wilde (1988) suggests, one would expect subjects in this experiment to drive more quickly under lower accident penalties. Although the effect was nonsignificant, subjects did indeed drive more slowly in the high accident cost conditions.

Although it appears that complete compensation should not have been expected, a further problem may have influenced the actual accident loss. It is conceivable that the feedback concerning the accident loss to which drivers were exposed was not sufficient to fully influence their behavior. RHT is clear with respect to the requirement that drivers can only be expected to adjust their behavior in the presence of feedback concerning the population accident loss. In fact, Wilde has argued that in the natural traffic environment feedback is usually only sufficient after periods of at least one year. Presumably, Wilde requires periods this long because feedback concerning the effect of a counter-measure on the accident loss is often ambiguous. In fact, in the studies reviewed here, it was shown that feedback concerning the population accident loss is necessarily flawed, inexact, and inaccessible. On a number of occasions we saw that data relating to the population accident loss are unavailable, and that other more accessible data such as the population death rate are not sufficient to determine the actual accident loss. It is difficult to imagine how a normal commuter could reliably estimate the population accident loss when the vast resources of scientific research have been unable to do so.

In contrast to the ambiguity present in the natural traffic environment, drivers in this experiment had direct and explicit feedback concerning the accident loss. Both the accident frequency and the cost per accident were relayed to subjects at the end of each of the 10-minute driving sessions. As well, the uncertainty associated with accident costs in the natural traffic environment was not a factor in the present experiment. While in normal driving situations, an accident could result in anything from minor damage to serious personal injury, the cost of an accident in this experiment was fixed. Therefore, although limited time was available to estimate

the accident loss, a tendency would exist for this problem to be offset by the precision and availability of feedback. Further research should assess this likelihood by exposing drivers to the experimental conditions for longer periods of time.

The final reason for the failure to find complete compensation in the present experiment is that RHT is false. Although this possibility may appeal to the critics of RHT, in my view, the present results suggest that such a conclusion would be inappropriate. Until further research can incorporate the modifications that have been suggested, the effects of each of the three problems discussed above will be unknown. Since each of these problems could have a significant impact on the extent to which subjects compensate to changes in the accident cost, it would be premature to attack the theory on the basis of these data.

To this point, I have reported the aspects of these results that are not compatible with RHT. This representation of the data, however, does under-emphasize the support they provide for the theory. Although RHT does suggest that homeostasis must take place with respect to the accident loss, it does not require equivalent monetary accident losses in each of the two accident loss conditions. There are two reasons for this. First, losses other than monetary fines would operate to reduce the extent to which compensation should have occurred. As a result, the observed two-fold reduction in the accident frequency is almost certainly an under-representation of the actual relative amount of behavioral compensation. Second, a four-fold reduction in the accident frequency would only be expected under conditions in which the target level of risk (R) remained constant over the varying levels of accident cost. However, since it has been shown that R was not constant over the two accident cost conditions, an accurate determination of the expected accident frequency can not be made. These two problems reveal a significant difficulty in measuring the effects of any motivational manipulation. Since the total accident loss can not be accurately assessed, it can never be known exactly how much R will change as a result of a motivational manipulation. For this reason it may never be possible to determine the size or direction of changes in R that result from changes in the accident cost. It should be noted, however, that this problem exists only for the manipulation of motivational

variables. Since no change in the cost of an accident results from the manipulation of a true nonmotivational variable, changes in the accident frequency do imply changes in R. And since changes in R should not result from fluctuations in nonmotivational variables, the validity of the theoretical effects of nonmotivational variables can be empirically established.

Risk Compensation to Manipulations of Nonmotivational Variables

While changes in the accident frequency are expected to occur with variations in accident cost, RHT predicts that manipulation of nonmotivational variables should not influence accident loss. In the present experiment, nonmotivational variables were manipulated both within-subjects and between-subjects. The results indicate that the within-subject speed limit manipulation did not significantly affect the accident frequency. Although fewer accidents did occur in the low speed limit condition, the effects were not large or significant. A similar pattern was found for the between-subjects manipulation of the speeding fine.

Although we are unable to reject the hypothesis that there is no effect of speed limits or speeding fines on the accident loss, there does appear to be a tendency toward fewer accidents in the low limit and low fine conditions⁶. In order to establish the reliability of these patterns, future research should incorporate two design features that were absent in this study. First, longer periods of exposure to the counter-measure should be considered. In the present experiment, no time was allowed for subjects to become familiar with the effects of the counter-measure. Therefore, accidents that occurred early in each session, could have resulted from insufficient feedback concerning the effect of the counter-measure. Future research should not assess the effects of the counter-measure until subjects have been exposed to the safety-measure

⁶ In the following discussion, my arguments will focus on the effect of the speed limit manipulation. This is because the smaller number of accidents in the low speed limit condition is a considerably more reliable result than the smaller number of accidents in the low fine condition. There are two reasons for this. First, the within-subject speed limit manipulation was measured over twice as many subjects as the between-subjects manipulation of speed fines. Second, since the manipulation of speed fines was essentially conducted as a second experiment, subjects were not randomly assigned to the two levels of the speeding fine variable.

for a period sufficient to allow complete feedback. Second, since the effect size for speed limits appears to be relatively small, a larger number of subjects may be required in order to reliably establish the efficacy of speed restrictions.

In summary, the results suggest that at least as far as speed limits are concerned, the manipulation of nonmotivational variables is not effective in reducing the accident loss. In addition, the effect appears to be consistent for both within-subject and between-subjects manipulations. Further research, however, should attempt to establish the reliability of these findings by increasing the statistical power of the manipulation of the nonmotivational variable.

Migration of Risk Taking From the Restricted to Unrestricted Behaviors

Although Predictions 1 and 2 are primary to this research, a number of the other observed data patterns are relevant to RHT. Previous research (see for example the studies cited in the introduction by Evans et al, Lund et al, O'Neill et al, Rumar et al, Wilson et al, Evans and Herman, Smith et al, Hakkert et al and Naatanen and Summala) has attempted to measure changes in specific driver behaviors that were not restricted by the introduction of a safety device or counter-measure. The assumption is that restrictions on risk taking in the regulated behavior should manifest themselves in increased risk taking on the unregulated behaviors. In this study, a number of measures were taken that can be used to assess behavioral risk migration. In addition to a number of straight-line speed measures, assessments were made of cornering speed, the number of steering corrections, and the time spent "near" the yellow line.

Table 2 shows that the speed subjects drove varied only very slightly between the two accident cost conditions. Although lower speeds were driven in the high accident cost condition, none of the differences was statistically significant. This result suggests that an increase in accident cost does not cause a reduction in driving speed. In addition, no significant differences were found for the cornering speed, the computer assessed crosses of the yellow, or the number of steering corrections. In combination with the speed data, these results suggest that specific

behavioral measures change very little with variation in the accident cost. But, since the manipulation of accident cost had a large effect on the accident frequency, it appears that specific behavioral measures change little upon significant reductions in accident risk.

This finding is important for two reasons. First, it suggests that a considerable amount of traffic safety research may have focused on behaviors that are unrelated to accident risk. Jonah and Dawson (1986), for example, reviewed a number of studies that have attempted to relate the increased accident involvement of young drivers to a variety of risk-taking behaviors (e.g., speeding, impaired driving, following too closely). Other research links these behavioral choices with accident records (Evans and Wasielewski, 1982; Garretson and Peck, 1981; Wilson and Greensmith, 1983) and violation history (Peck and Kuan, 1983; Smith and Kirkham, 1982). The assumption of these studies is that the associations of accident risk with age and accident risk with speeding, for example, indicate a link between speeding and accident risk. That is, that the accident risk of young drivers is a function of their speed choice, because older drivers that choose lower speeds have a lesser accident risk. These data suggest that this conclusion is false because driver's speed choice was unaffected by changes in their accident risk. In light of these findings, a more benign interpretation of the above studies seems warranted. It could be argued that although risk-takers do choose higher speeds, the cause of their increased accident risk is not the speed they choose but their willingness to take risks. In a sense, these data indicate that specific risky behaviors such as speed choice may exist as mere epiphenomena to the whole process of accident causation.

The second important aspect of these findings is related to our understanding of the mechanism of risk compensation. Although fluctuations in the accident frequency clearly show that subjects compensated for changes in accident cost, there was no significant compensation on any of the specific behavioral measures of risk taking. In addition, the manipulation of both nonmotivational variables resulted in no significant changes in cornering speed, the computer assessed crosses of the center-line, or steering corrections. It appears that the migration of risk-taking from the restricted to unrestricted behaviors and the reduction of accident risk

accompanied by changes in accident cost are not easily assessed by the analysis of specific risky behaviors. Future research should consider these findings before constructing tests of risk migration. If the results of this research are valid, researchers can expect to encounter small, if any, effect sizes for specific behavioral measures of risk migration. As well, compensation to less obvious or accessible factors can also be expected.

Other Findings and Speculation On Their Relevance To Traffic Safety Regulation

In contrast to the three previous sections in which the theoretical implications of these findings were discussed, this section will deal with some of the more practical issues revealed by these results. Although the theoretical issues do have enormous practical importance, a discussion of the theoretical relevance of the findings is necessarily limited to the language and premises unique to RHT. Therefore, while most of the issues discussed in this section will have direct theoretical application, some may go beyond the scope of RHT.

In an earlier footnote, I hinted at an aspect of the design that was responsible for the nonrandom assignment of subjects to the between-subjects speeding fine factor. In fact, subjects were not randomly assigned to the levels of this factor because the experiment was run in two stages. In the first phase of the experiment, subjects served in the low speeding fine condition. Although a number of pilot subjects were run in order to determine sufficiently punitive speeding fines, early in the experiment it became apparent that subjects were breaking the speed limit with great regularity. While speeds were significantly slower in the low limit condition, I was concerned that critics would discard the data on the basis of a weak manipulation of the nonmotivational variable. In response to this problem, the monetary value of speeding fines and the frequency of speed checks were increased for the final twelve subjects.

The reaction to this problem is important because it simulates the popular approach to accident safety regulation. It is quite common for government bodies, legislators and enforcement communities to "clamp down" on speeders by raising fines and initiating

enforcement campaigns. Their claim is that since speed kills, larger fines and more excessive enforcement will undoubtedly result in fewer traffic deaths. In the present study speeding fines for infractions of greater than 10 km/h were doubled, and enforcement (the probability of passing through a speed check) was quadrupled. Although these measures were almost certainly more punitive than would ever be sanctioned in the natural traffic environment, it is notable that their introduction here resulted in no significant reduction in accidents.

Most damaging to advocates of the legislative approach, however, was the *relative* effectiveness of the two types of counter-measure. While an eight-fold increase in the expected monetary speeding fine had no significant effect on the accident rate, a smaller four-fold increase in the accident cost reduced the accident rate by half. Although I do not believe that the results of this experiment compel a significant restructuring of our traffic legislation system, they should cause legislators and researchers to re-evaluate their methods of accident control. Against the uncontrolled, inaccurate and confounded evidence that the correlational methods have produced, these data represent a rare combination of high internal and external validity.

There is another slant on these results that indicates the relative ineffectiveness of speeding fines. Inspection of Table 3 indicates the relatively minor effects of speeding fines that were similar in value to the highest level of accident cost. Only a 2.51 km/h decrease in average speed was observed when speed limits were lowered by 20 km/h. Even more informative of speed choice, however, was the small 38.12 second increase in the over 40 km/h driving time, when the speed limit was raised to 60 km/h. These data suggest that even though speed fines in the low fine condition were high relative to the cost of an accident, subjects failed to make substantial adjustments to their speed. But, since the cost of an accident in the real road environment is far larger than the cost of a speeding ticket, we would expect the relative effects of speeding fines and accident penalties to be even smaller on the roads than in this experiment. In fact, in order to obtain any significant gains in the ability to control speed choice, it was necessary to raise speeding fines to an amount four times larger than the average cost of an accident. And, even under these conditions, only small and statistically nonsignificant reductions in the accident loss

were achieved. Once again, it appears that the much more cost effective way to reduce the frequency of traffic accidents is to increase their cost rather than increase fines for speeding violations.

Recommendations for Future Research

Although I do believe that this design can be improved, there are, in my judgment, few other designs that could provide a better test of RHT. For the first time, all the essential variables in RHT have been systematically manipulated and measured in an ecologically valid environment. Besides the unfeasible option of testing the theory in the real traffic environment, no technology currently exists that would allow a more ecologically valid platform.

Although representing the best strategy, some of the tactics used in the present experiment could be improved. It was mentioned earlier in this section that the baseline accident loss (the accident loss when the monetary cost of an accident was 0 cents) was almost certainly not zero. Since compensation should occur in direct relation to the proportional change in accident cost, large baseline accident losses would tend to mask the effect of the manipulation of accident cost. However, for a given baseline accident loss, larger monetary accident costs would deflate the influence of the nonmonetary or baseline losses. For example, suppose that the nonmonetary baseline accident loss could be expressed in monetary units of 1 dollar. The proportional increase in accident cost for fines of 50 cents and 2 dollars would then be $(\$2 + \$1)/(\$0.5 + \$1) = 2$. This increase is considerably smaller than the four-fold increase in the monetary accident cost. However, suppose that the monetary accident penalties were raised from 50 cents and 2 dollars to 10 dollars and 40 dollars. In this case the proportional increase in the accident cost would be $(\$40 + \$1)/(\$10 + \$1) = 3.72$. Although the proportional increase in the total monetary and nonmonetary accident losses is not exactly equivalent to the proportional increase in the monetary accident cost alone, there is considerably better agreement in this second case. These examples show that the influence of nonmonetary accident losses could be minimized by an

increase in the monetary cost of an accident. The increase, however, would have the effect of reducing the frequency of accident involvement because subjects are motivated to maintain the total accident loss (i.e., the product of accident costs and their frequency). In order to maintain reasonable distributions of accident frequency, either more subjects would be required, or each subject would be required to serve for longer periods of time.

In my view, the second of these two options should be incorporated in to future research. It is essential to RHT's predictions concerning the accident loss, that sufficient time passes between the onset of a counter-measure and the assessment of its effect on the accident loss. Although feedback in this experiment was far superior to the feedback available in the real traffic environment, a greater amount of time between the manipulation of the nonmotivational variable and the measurement of the accident loss could only serve to increase the effectiveness of the manipulation. Thus, future research should increase the monetary costs associated with accidents and increase the amount of time each subject serves under each of the levels of the manipulated factors.

Summary and Conclusions

Earlier in this report, a number of correlational field studies of RHT were reviewed. Taken individually, most of the studies suffered from inadequate control, faulty predictions based on erroneous deductions from RHT, and in many cases poor statistical analyses. Taken together, the results of these studies are unreliable, weak and inconsistent. Unfortunately, the impact of correlational research is unlikely to be improved in the foreseeable future because the data required to provide valid test of RHT are not available. Other research has attempted to overcome these problems by conducting experimental laboratory tests of the theory. Although these studies offer greater control over the manipulation and measurement of variables, they have tended to lack ecological validity. In addition, none of the studies reported in this paper has included within-subject manipulations of the variables relevant to RHT. The present study

responds to these problems by manipulating the relevant variables in a within-subject fashion, and in an ecologically valid laboratory setting.

Although the results of the present experiment are not in precise agreement with RHT, the proposed elimination of some of the weaknesses of the design would be expected to provide more compatible results. However, these data should not be devalued because of their failure to fully support the theory. It should be recognized that since the perceived gains and losses associated with a given set of behaviors can never be precisely known, the extent to which compensation should occur will also never be accurately assessable. As a result, the failure to find complete compensation is not necessarily evidence that the theory is false. The challenge is to design experimental manipulations that allow the measurement of as many of the perceived gains and losses as possible. Only then can we begin to develop a reliable understanding of the mechanisms that control compensatory behavior.

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