RISK HOMEOSTASIS THEORY IN SIMULATED ENVIRONMENTS

THOMAS WILLIAM HOYES

Doctor of Philosophy

THE UNIVERSITY OF ASTON IN BIRMINGHAM

September 1992

The University of Aston in Birmingham

Risk Homeostasis Theory in Simulated Environments

Thomas William Hoyes, M.Sc (Hull)

Submitted for the degree of Doctor of Philosophy
1992

Summary

This thesis has two aims. First, it sets out to develop an alternative methodology for the investigation of risk homeostasis theory (RHT). It is argued that the current methodologies of the pseudo-experimental design and *post hoc* analysis of road-traffic accident data both have their limitations, and that the newer 'game' type simulation exercises are also, but for different reasons, incapable of testing RHT predictions. The alternative methodology described here is based on the simulation of physical risk with intrinsic reward rather than a 'points pay-off'.

The second aim of the thesis is to examine a number of predictions made by RHT through the use of this alternative methodology. Since the pseudo-experimental design and *post hoc* analysis of road-traffic data are both ill-suited to the investigation of that part of RHT which deals with the role of utility in determining risk-taking behaviour in response to a change in environmental risk, and since the concept of utility is critical to RHT, the methodology reported here is applied to the specific investigation of utility. Attention too is given to the question of which behavioural pathways carry the homeostasis effect, and whether those pathways are 'local' to the nature of the change in environmental risk.

It is suggested that investigating RHT through this new methodology holds a number of advantages and should be developed further in an attempt to answer the RHT question. It is suggested too that the methodology allows RHT to be seen in a psychological context, rather than the statistical context that has so far characterised its investigation. The experimental findings reported here are in support of hypotheses derived from RHT and would therefore seem to argue for the importance of the individual and collective target level of risk, as opposed to the level of environmental risk, as the major determinant of accident loss.

Keywords: *Risk homeostasis theory; simulation; accidents; utility; compensation effects.*

Acknowledgements

Sincere thanks go to Dr A Ian Glendon for his supervision of this work. I could not have wished for a better supervisor. Special thanks go also to Dr Lisa Dorn of Leicester University, whose idea it was in the first place that I should do a PhD in risk homeostasis theory, and, as if that were not contribution enough, provided valuable help in gathering data for study 1 and helped with SPSS analysis on studies 1 and 3. Thanks go also to Sue Westwood for her valuable help in study 5 and to Di Haigney for her help in study 3. Thanks too must go to Neville Stanton for his much-appreciated assistance with studies 2 and 4. Neville helped run participants and did all of the programming for the NPPCRS. He also gave valuable help, encouragement and good advice.

Myfanwy Ball provided me with good ideas and useful comments, particularly with the NPPCRS study and the secondary task performance. Thanks go to her too. Liz Murphy, Rachel Hughes and Caryl Anderson gave valuable assistance with the data gathering for the later factor analyses. They helped too in data entry, and for that I am particularly grateful! Thanks also must go to Dr Rob Stammers for introducing me to the many Mac packages, particularly Statview, I needed in preparing this thesis.

I am grateful also to all the participants of the studies reported here, but must give a special word of thanks to Helen Higson, who not only took part in two experiments, but also recruited participants for me from support staff colleagues in the Business School. For that, and for listening to my ideas, problems and gripes, thanks, Helen.

Ray Taylor deserves a paragraph of his own. His computing brilliance in developing the Aston Driving Simulator and in coping with my never-ending requests for programming changes leave me forever in his debt. Thanks, Ray.

Contents

Tables and Figures

"The lapse of time during which a given event has not happened, is, in this logic of habit, constantly alleged as a reason why the event should never happen, even when the lapse of time is precisely the added condition which makes the event imminent. A man will tell you that he has worked in a mine for forty years unhurt by accident as a reason why he should apprehend no danger, though the roof is beginning to sink [...]"

George Eliot, 1861 (from 'Silas Marner').

"If all roads were to be paved with a substance having the same coefficient of friction as ice, the number of people killed on the roads would be substantially reduced. (The performance of the road network as a transportation system might also be reduced - but that is a separate matter.)"

John Adams, 1985.

PART 1 - A REVIEW OF RISK HOMEOSTASIS THEORY AND ITS METHODOLOGY:

In this part of the thesis an attempt is made to review the theory of risk homeostasis together with its methodology. Special attention is given to the conceptual questions surrounding the theory, before applying RHT to the case study of motorcycles. Since Wilde's (1988) paper is taken by this thesis as a definitive account of RHT, a close look is taken at the key propositions of this paper, together with their implications. A discussion of the role of simulation is provided, as applied both to the simulation of the driving process and to simulation as a tool for the investigation of risk homeostasis hypotheses. Finally an attempt is made to place RHT in context by contrasting it with compensation theories and the engineering approach.

General Introduction and Preamble

I wanted to do a PhD in risk homeostasis theory (RHT) for several reasons. A large consideration was that RHT had been my MSc topic, and therefore one with which I was familiar. I became aware during my MSc research that the simulation of physical risk as a methodology for investigating RHT appeared to have escaped attention, and I believed then that such a methodology held many exciting possibilities. So far as these possibilities went, my MSc had, by the time it was completed, barely scratched the surface.

The research tool for my first attempt at research in RHT was crude - it involved a commercial simulation (marketed as a game) of an air traffic control environment. This was limited both in terms of the manipulations I was able to make and in terms of the measurements that were available to me. What I wanted was a more sophisticated simulation device with greater possibilities for the design of experiments and a variety of precise measures. What I wanted, I suppose, was a device purpose-built for research that simulated some aspect of physical risk. Since RHT is postulated almost entirely in terms of road-traffic behaviour, this pointed to a simulation of the driving process.

When I arrived at Aston the perfect research tool was available for me to carry out the sort of research into RHT I had wanted to do in my MSc - The Aston Driving Simulator. The opportunity was too good to miss.

There was a third, less personal, reason for my choosing RHT as a research topic. When I first read about RHT and began to understand what its claims were, I was astonished that such an important assertion could have attracted so little research (at the time of writing this, I am aware of fewer than 60 references to it). If RHT is a true account of how people respond to changes in the intrinsic safety of their environments, then the implications for environmental ergonomics and health and safety are profound. So far as applied psychology is concerned, it would surely be difficult to envisage a theory with more important implications. The investigation of RHT in a new way seemed to me to be as worthy a topic as any from which to derive a doctoral thesis.

In common, I'm sure, with most theses, the aims and focus of work changed during its development. I had planned a questionnaire-based examination of mode migration (changing from one form of transport to another in response to risk changes). However, a pilot study along these lines convinced me that responses were subject to such a restricted range that to make sense of the correlations was all but impossible. In any case, the questionnaire was starting to look out of place with the general theme of simulation, and so, along with a quesionnaire looking at the factor structure of risk-taking, it was abandoned.

The thesis is in three parts. The first (chapters 1-4) is a review of risk homeostasis theory and its methodology. The second part (chapters 5-7) looks at RHT in three simulated physical risk-taking environments. This part of the thesis is intended to answer questions relevant to RHT that do not lend themselves to investigation through the more traditional methodologies. Part 3 of the thesis (chapters 8-10) is concerned with evaluating the success of these simulations.

From this structure two distinct aims arise. First, this thesis is intended to develop an alternative methodology for the investigation of RHT based on the simulation of physical risk-taking. Second, having developed strategies based on simulation with which to investigate RHT, the same strategies are then employed to test those hypotheses derived from RHT that lend themselves particularly to an experimental approach. Although these aims are distinct, their development runs in parallel: most of the studies which follow have some contribution to make to each aim.

Chapter 1 is an introduction to some conceptual and methodological issues associated with RHT. Having set these out the risk homeostasis perspective is illustrated by applying it to the case of motorcycles.

Chapter 2 examines in closer detail Wilde's 1988 paper on risk homeostasis theory. It does this to provide the reader with a more detailed account of what RHT is about, and it closes with some comments and criticisms of the theory. In addition, this chapter seeks to put RHT in context by examining some competing theories that seek to explain risk taking behaviour in a dynamic of environmental risk. To this end, Leonard Evans's 1985 paper is critically examined.

Chapter 3 examines the possibility of simulation as a research tool for the investigation of RHT. It examines very generally the role of simulation in the social sciences before going on to look at simulation of the motor vehicle and simulation as applied to RHT.

Chapter 4 is a summary of the the issues discussed in Part 1 of the thesis.

Chapter 5 is concerned with the question of what role the factor of utility enjoys in the risk homeostasis process. This aspect of the theory is critical to the understanding of risk homeostasis whilst at the same time being an unsuitable area of study for the traditional methodology of risk homeostasis research. Two studies are reported in this chapter, both of which look at the question of whether the factors of utility and environmental risk come together to form a statistical interaction on the dependent measures of risk-taking behaviour. One of these studies involves the Aston Driving Simulator, the other involves a simulated nuclear power plant control room.

Next, chapter 6 looks at risk-taking behaviour in relation to non-specific changes to environmental risk. That is, it is concerned with those changes that affect our intrinsic risk but in a very general sense. This was included so that the behaviours might be contrasted with those in chapter 7 in which highly specific changes to the level of environmental risk took place. Again, the Aston Driving Simulator was used; so too was a low-fidelity simulator involving merely pencil and paper.

The remainder of the thesis attempts to evaluate the simulation exercises described in Part 2. Chapter 8 is given over entirely to structured interviews with participants from the three simulated environments that were used in the earlier studies.

Chapter 9 looks at the factor structure of the simulation devices reported. It starts by examining the factor structure of the Aston Driving Simulator, before going on to look at the factor structure of a low-fidelity simulation device - a pencil-and-paper-based task. Since a number of very specific problems were associated with this low-fidelity device, improvements were made to it before reappraising it in a further, detailed study.

Finally chapter 10 is concerned with conclusions arising from the empirical side of this thesis, and recommendations for future research practice. It is structured around the twin aims of the thesis and involves practical suggestions for researchers concerned with the simulation of physical risk as a research method for investigating risk homeostasis in the future.

Chapter 1 - Introduction to Risk Homeostasis Theory

1.1 Some Conceptual and Methodological Issues

1.1.1 Introduction

The theory of risk homeostasis represents a departure from the engineering view of safety. This sees the level of environmental risk as the basic determinant of accidents. On this view, if we want to make individuals safer, our interventions are likely to be at the level of the environment. Compulsory wearing of safety helmets for motorcyclists would be an example. By making the wearing of safety helmets mandatory, we make the motorcyclists' environment safer. The motorcyclist is now *intrinsically* safer, and we might therefore expect to see improvements in accident loss (many accidents having less severe consequences). With the view that *environments cause accidents*, or that accidents are the result of an interaction of environment and individual, effort should be made first to identify environmental hazards, and then to eliminate them. A certain amount of support for this perspective exists in that some environments are associated with higher levels of accident loss than others.

Some confusion may be said to exist between the terms 'engineering perspective', 'environmental risk' and 'intrinsic risk' in the context of the RHT debate. The engineering perspective (Evans, 1985) suggests that actual risk is directly related to the change in the risk of the environment. It would predict that engineering solutions aimed at making environments safer are successful in reducing actual risk. The confusion here occurs when one talks, for example, of the engineering perspective predicting higher levels of accident loss in poor weather conditions. All this means is changes in behaviour will not occur such as to negate the adverse weather effects, whilst at the same time, the adverse weather is not in any sense an 'engineering' intervention.

The terms 'environmental risk' and 'intrinsic risk' are used synonymously in the context of RHT research. Environmental risk refers to the level of risk inherent in the environment. The term 'intrinsic risk' causes some difficulty in that it is used to mean something different from its everyday meaning. Intrinsic risk changes, suggest RHT proponents, when, all other things remaining the same, we can expect changes in accident loss to follow. Thus, in the absence of behavioural adjustments, we would expect motorcyclists who wear safety helmets to experience lower levels of accident loss than those who do not. Thus, in terms of RHT, this is a change to intrinsic risk, whilst in the every-day sense of the word, intrinsic risk is the same for both groups.

Of course, individuals as well as environments may be looked to in explaining accidents. The popular idea, for example, that some individuals are more accident prone than others has long been explored. Greenwood, Woods and Yule (1919) were perhaps the first to demonstrate this. They gathered data looking at accidents sustained by workers in a munitions factory during war-time. A small minority of workers suffered significantly more accidents than would be predicted by chance alone. Newbold (1926) supported the findings of Greenwood *et al.*, but her study involved workers from a number of different factories, and manufacturing a wide range of different items. Farmer and Chambers (1939) found the same accident proneness effect in a group of drivers. Personality correlates of the tendency to have accidents have also been examined, and will be discussed later (see also Tillman and Hobbs, 1949; Oborne, 1987). One could summarise these findings by saying that whilst the tendency to have accidents is correlated with a number of aspects of personality, there would appear to be no single trait of accident proneness.

Age and experience too are both known to be correlated with accident likelihood. Van Zelst (1954) demonstrated that younger workers are more likely to have accidents even when the factor of experience is partialled out. Hale and Hale (1972) suggest that inattention, indiscipline, and over-estimation of capacity may contribute to the greater likelihood of young people to be involved in accidents. It has been suggested though that older workers, as well as older motorists, are more likely to have accidents (Brouwer, 1987; Murrell, 1962; and OECD, 1985). It is suggested that the likely reason for this is a general perceptuo-motor and attentional deterioration (Planek, 1981; Salthouse, 1982; Summala and Koivisto, 1989; Welford, 1985).

Life stress has been linked with accident causation, although empirical evidence here is limited. Whitlock, Stoll and Rekhdahl (1977) do however provide some evidence here.

The consumption of alcohol too has been linked with accidents. It seems that alcohol consumption increases reaction time (Carpenter, 1959; Jellinek and McFarland, 1940) and reduced skills (Bjerver and Goldberg, 1950; Brenner, 1967; and Trice and Roman, 1972).

On a more cynical note, Hill and Trist (1953) went so far as to produce a theory that what causes accidents is the desire to be given time off work! Indeed their hypothesis does have some limited support. One might predict from it that those workers who take a large number of days off work also have higher rates of uncertified absence. By comparing the uncertified absence rates of 289 workers, 200 of whom had remained accident-free, the remainder having suffered one or more accidents, this link was supported. However, since no matching of workers across the high and low accident groups took place the study does suffer from a possible confounding variable. Moreover, the study fails properly to differentiate between actual and reported accidents, leaving open the possibility that many of the workers lied about their accidents.

On the environment side also research has been undertaken. One perspective here has been to compare factors such as type of industry, size of industry, and geographical region in relation to industrial accident rates (see Davis and Stahl, 1964; Cohen, Smith and Cohen, 1975; and Shafai-Sahrai, 1971 and Simonds, 1973). Attention has been given also to the implementing of expert safety advice (see for example Planek, Drissen and Vilardo, 1967).

What is however clear from the above is that accidents appear to be caused by an interaction of the individual with his or her environment. Some environments are more likely to cause accidents than others (eg Cohen *et al.*, 1975). Some individuals are more likely to be involved in accidents than others (eg Van Zelst, 1954).

Risk Homeostasis Theory (RHT) is a model of risk taking proposed by Wilde (1982a, 1982b, 1984,1985a, 1985b, 1986a, 1986b, 1988, 1989) which takes a very different perspective. Rather than seeing the environment as a cause of accidents, it sees accident loss as arising from the target level of risk experienced by road users. This target is effectively the level of risk that the individual is prepared to accept. It acts as a unique reference variable in comparison with the actual, operational, level of risk to which the individual is exposed. Where the target level of risk is either higher or lower than the actual level of risk, the individual is said to change his or her behaviour such that the difference is, over the long term (and over the entire relevant risk-taking population), eliminated.

Two questions arise from the consideration of the concept of a target level of risk. First, what do we mean be 'prepared to accept'? Do we mean merely tolerate, or do we refer to an active, real choice? Second, at what level does this target risk comparison operate? Is it at the level of the individual, or rather, at the level of the population? On this first question, Wilde's original writings offer little help. By means of a clarification of the term 'target', Wilde suggested that it could be thought of as a preferred, accepted, tolerated, desired, or subjectively optimal level. The problem of course is that these terms mean quite different things, and certainly shed no light on the question of whether the target is an active choice, or merely something that an individual must tolerate. Several authors have made a distinction between accepted risk and acceptable risk (see for example O'Riordon, 1977; Jones and Akehurst, 1980 and the Royal Society report on risk assessment, 1983) . An example of accepted risk is that associated with cigarette smoking, which is at the same time considered to have an unacceptably high risk. The question then is whether Wilde's concept of a target level of risk can be described as an accepted or an acceptable level? The second question is easier to answer. Wilde is quite clear in characterising the individual rather than the population with a target level of risk: he says quite explicitly that any given individual road user is at any moment of time characterised by a target level of risk. The conceptual difficulty this leads to is that whilst the target operates at an individual level, the regulatory mechanism as a whole operates at the level of the whole population.

Although the details of the above belong to Wilde, the more basic ideas behind them of individuals responding to a change in perceived risk by changing their behaviour in the direction of negation of those changes are by no means new. In 1948 Smeed wrote:

"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 the result is the same number of accidents as previously. I think there will nearly 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. Some measures are likely to cause more accidents and others less, and we should always choose the measures that cause less." (p.13).

References to such a mechanism go back even further, to Gibson and Crooks (1938) who said:

"[...] more efficient brakes on an automobile will not in themselves make driving the 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).

One hardly need say that if this perspective really is to explain risk-taking behaviour, improvements at the level of the environment would appear not to offer an effective way of making individuals safer and reducing overall accident loss. If the essential postulates of RHT are true, then making an environment safer will result only in creating a gap between actual risk and target risk (if the environment has become safer, then actual risk may drop below 'target' risk). RHT would predict that individuals will change their behaviour in these circumstances such that actual risk more nearly matches target risk. The exact nature of these behaviour changes is not specified by RHT, but Wilde (1988) does suggest that whatever the behaviour, it will fit into one of three categories. Although he does not label these categories, they could be referred to as: i) behavioural adjustments within the environment (which might take the form of driving at higher speeds, with reduced attention, etc.; ii) mode migration (changing from one mode of transport to another. For example, in conditions of fog and ice one might change the mode of transport from automobile to train); and iii) avoidance (not making the journey in the first place).

The homeostasis process that Wilde describes is said to operate under a population-level closed loop. Because it is population level, the experience of the individual is only important in so far as individuals collectively make up populations. Suppose a road user were to make an error, resulting in his or her death. Whilst this individual can no longer adjust his or her behaviour through a closed-loop process, the population *as a whole* can. Since the theory is based on a closed loop process, it is silent on the role of the mere information of a safety change, but has something to say about the results of the change fed back to the population over the long term. This means that to test the theory a suitable time interval between intervention and testing is required. Short term fluctuations that appear to refute RHT are to be discounted.

For a negation of environmental safety improvements to occur benefits must accrue from compensation. RHT does not posit risk for its own sake. The theory of risk homeostasis is built upon the concept of utility. Wilde suggests that the target level of risk is determined by four relevant utilities: the costs and benefits of relatively cautious and relatively risky behaviour. Where there is no utility attached to a change in behaviour, adjustments in behaviour would not be predicted by RHT.

Since the predicted effects of changes to intrinsic risk are stated in terms of accident loss, it is important to note that Wilde (1988) defines accident loss as "*[...] the sum of the cross products of the frequency of accidents and their costs.*"(Proposition 1, 1988). Target levels of risk, then, can be expected to vary in line with either accident costs, or accident numbers. The probability of an accident, which would not change as a result of some interventions, is not then the only hypothesized factor in behaviour adjustments. This means that a safety improvement can be defined in two ways: an expected reduction in (a) the probability of an accident; or (b) the costs of an accident, given that the accident has already occurred.

Much of what RHT proposes could be summed up by saying that the amount of accident loss accepted by the aggregate of road users in return for those benefits accruing from the given behaviour, will determine the behaviour of the whole population.

Within RHT research, several issues have attracted particular attention. Certainly the issue at the very centre of RHT - does the evidence support or refute the theory? - remains hotly disputed. In this section, attention will be given to five issues related to this question that have attracted interest on both sides of the debate.

1.1.2 Can RHT be falsified?

A criticism often levelled at RHT is that in the terms in which it is stated, it cannot be falsified (eg Adams, 1988). In assessing whether this criticism is fair, several scenarios will be examined. Assume that an unambiguous improvement in environmental safety is made, and....

(a) Measures are taken of driving speeds, headway, number of overtakes, judged risk of each overtake, before and after the intervention. All these measures fail to provide evidence of change.

(b) The total number of people killed on the roads is counted before and after some intervention. The figures are reexamined after 1.5 years (in 1982 Wilde tentatively suggested a time-span of 1 year for the operation of RHT; as recently as 1988, Wilde proposed that the maximum time span for the operation of the homeostasis effect is between 1.5 and 2 years; although in 1989 he reiterated that RHT does not formally specify the time period of its operation). The evidence from this hypothetical study supports a sustained effect of the intervention. Fewer people die.

(c) *Total* accident loss is calculated for the 1.5 years before and the 1.5 years after the intervention for the whole population of drivers. The findings here are that total accident loss falls markedly after the intervention and, 1.5 years later, remains significantly lower. A calculation is then made of the total number of time units of exposure to risk. This average figure, together with the number of people in the jurisdiction, has remained constant across the whole period in which measures were taken.

The first two of these scenarios are typical of attempts to falsify RHT. But do any of the scenarios actually constitute a falsification? The first, along with all related designs,would certainly not amount to a falsification of the theory. Wilde (1988) is clear that RHT does not predict the particular behavioural pathway through which it operates. If the behavioural pathways by which homeostasis occurs are not specified (and they are not, at least not explicitly), then eliminating any number of them, whilst contributing to our understanding of how the mechanism operates, can never constitute a falsification of the theory. On this basis, Lund and Zador (1984) in taking measures of four specific driver behaviours, and failing to find evidence of compensation, have provided findings compatible with the theory. Interestingly, Smith and Lovegrove (1983) who provided limited support for RHT using specific driver behaviours (including one - speed - in common with Lund and Zador) have of course contributed to the evidence *for* RHT. Perhaps then, when looking at specific behaviours, those who find evidence for compensation support the theory. Those who fail to find such evidence, using the same behavioural criteria, have committed some sort of methodological error.

Scenario (b) has taken into account Wilde's time-lagged criterion. Here, even over an extended period, there is a sustained drop in the number of fatalities. There are at least two reasons why this finding too can be comfortably reconciled with RHT. First, RHT refers not specifically to fatalities, but rather, to *total* accident loss: the sum of the cross products of accident costs and their frequency. Perhaps the total number of fatalities has fallen, but if there has been a corresponding rise in the number of serious accidents not resulting in deaths, and trivial accidents with small associated costs, then the *total* accident loss may have been maintained. Second, if the total number of time units of exposure to risk has fallen, then the drop in the number of fatalities may reflect the increased safety per km of exposure; again, perfectly compatible with RHT.

Any examination of scenario (c) must begin with the question of how total accident costs were measured? To date, so far as a literature review was able to determine, no model for such a calculation has been put forward - and with good reason. Accident costs, which form half of the equation of accident loss, are essentially subjective. The price of a dead friend or a broken arm is clearly difficult to calculate. But assuming that some imperfect estimate of total accident loss could be made, would scenario (c) now constitute a falsification of RHT? At this stage (and we are already talking about a level of methodological sophistication that has not yet been reached in any RHT research) the findings would start to embarrass RHT proponents, and would certainly be difficult to fit into the traditional conceptual framework of RHT - difficult, but not impossible. In fighting off this charge, the adherents to RHT would surely call in their defence Proposition 3 from Wilde's 1988 paper. This points out that the target level of risk is determined by four utilities: those benefits and costs associated with relatively risky behaviour, and those benefits and costs associated with relatively safe behaviour. Now, when the target level of risk in the population changes for the better, there will be fewer accidents and less accident loss. But how do we know that the target level of risk in the population has changed? Simple: there will be fewer accidents, and/or less accident loss. The definition is, in other words, as circular as it is unhelpful.

Perhaps this is not quite fair to Wilde, since he has so far only cited this proposition when at the same time he has been able to point to a probable cause of the change in relative utilities, as in changed national economic circumstances affecting the price of petrol and other fuels. But changes in relative utilities do very often take place, since prices, relative incomes and so on are rarely static. In short, if we look hard enough for a confounding variable, we will usually find one. Moreover, when we talk about utilities in risk homeostasis terms we are in fact talking about a perception, or a subjective interpretation. There is no reason why arriving at a destination three minutes sooner should not represent a 'positive' utility for one person, a utility-free outcome for another, and a cost (a negative utility in terms of models such as O'Neill's and the utility-driven model of Janssen, 1990) to a third. Setting out to define in some objective way what we mean by the utility of one behaviour relative to another is, then, doomed from the start. All that is possible is the investigation of population-level perceptions of the value of one outcome relative to another. The generalizability of such findings is another matter altogether.

Is it, then, even *possible* to falsify RHT? Two things seem at present to prevent falsification. The first of these is a definition of accident loss that can never be measured. The implication of this is that so long as *something* remains the same (or changes in the predicted direction) after an intervention, RHT will always have the beginnings of a case. The second is that whenever an intrinsic safety measure brings about a reduction in all measures of accident loss, it is hard to imagine that a case could not be put forward to suggest that a motivational change did not also occur. If it did, of course, then there is no case to answer, and RHT remains unfalsified. This problem is further compounded by the fact that safety interventions are usually accompanied by some attempt to bring about an attitudinal change, such as happens with an advertising campaign. If an accident-loss reduction is brought about, what caused it? Was it the intervention, or was it the graphic reminders of road carnage that went with that intervention, and made everybody *want* to be safer? Until RHT advocates define accident loss and motivational changes in a more objective way, its falsification is indeed hard to envisage. It could be argued that risk homeostasis theory would be greatly advanced if its proponents would spell out in clear and explicit terms the sort of evidence that would constitute its falsification. In the absence of such a contribution it is difficult even to attribute the status of theory to it.

No attention has been paid here to cross-situational homeostasis, although Wilde points to this to as a pathway to homeostasis. He cites the work of Lee (1979) here who, says Wilde, provides us with evidence for cross-situational risk homeostasis in cigarette smoking reductions (note also Wilde's careful use of the word *jurisdiction* in his earlier papers). The implications of this will not be gone into here - there are in fact arguments for suggesting that the Lee paper has little if any bearing on the RHT question. What should be noted is that the cross-situational dimenstion to the RHT postulate does nothing to ease the possibility of a falsification of the theory.

1.1.3. Psychological invisibility and RHT

A second conceptual issue, and one raised by McKenna (1985), is that of psychologically invisible changes in risk. Basically, McKenna's position on this is that environmental safety improvements can be brought about that are impossible for risk-takers to identify, much less to quantify. For example, if a new rear bumper is developed that absorbs rear-end collisions at greater speed, it will look much the same as a rear bumper without these properties. Such an improvement will be psychologically invisible. How, asks McKenna, can a driver compensate for an environmental safety improvement that he or she cannot perceive? The answer on one level is clearly that the driver can do no such thing. If RHT has an open loop component, psychological invisibility must starve it of information. If RHT operates in the short term, its effect must be limited. But RHT is said to work in a closed-loop and over the long term. If what enters that closed loop as its reference variable, or at least as one of its reference variables, is information on accident loss, what effect would this have? Assuming the safety intervention to be effective, fewer people would be killed. Fewer people would suffer serious injury. As total accident loss is reduced, RHT would predict compensatory behaviour.

The question of whether any of this is specific to the drivers and passengers of the vehicles affected by this improvement, or whether it is generalised to the whole of some notional driving population, is open. It may well be the case that the psychological invisibility of the improvement makes for a generalised effect, but a compensation *somewhere* within the driving population may still oçcur. The difficulty, unfortunately, in accepting such a suggestion is in knowing where to look for the effect. On the proposal outlined, it may well be that those drivers and passengers affected by the intervention do enjoy a reduction in total accident loss, but that the driving population on a wider jurisdiction do not. What must be clear, though, is that psychological invisibility does not, in principle at least, provide a way of getting around RHT's essential predictions.

It is also worth recalling Adams's (1988) comments on psychological invisibility. Adams suggests that this whole issue has little practical relevance to risk homeostasis. Before the introduction of an environmental safety improvement, there tends to be a certain amount of public debate about it. In addition, safety is marketable. If one manufacturer develops cars which are intrinsically safer than other cars, why keep quiet about it? Adams's example here - and it is a good one - is that of Volvo, who have *sold* safety. One might go further and ask the question of whether it can even be moral to withhold information concerning the relative safety of one form of transport, one type of road, one car, relative to others? There is, then, evidence to suggest that psychological invisibility is not an issue that needs to be addressed in the abstract and so far as can be determined no empirical evidence exists to settle the issue either way.

1.1.4. Bi-directionality and RHT

Most of the evidence put forward in support of RHT relates to accident loss after an improvement in intrinsic safety. An interesting question therefore arises where intrinsic safety actually falls. Risk homeostasis theory would predict that where the level of risk changes for the worse, a compensation process should follow. Not only should environmental *improvements* be compensated for but reductions in environmental safety too should be negated. In examining this question McKenna points to research findings including those of Codling (1974) and Hawkett (1978) which would seem to show that drivers do not compensate for wet roads. Instead, they make little adjustment in speed to suit their conditions and have more accidents. Can findings like these be reconciled with RHT, or do they, as McKenna suggests, represent a *"refutation of risk homeostasis theory."* (p494)? Several points need making.

1. Wilde's formulation of RHT is, to repeat, about long-, rather than short-term behaviour. Wet roads tend not to stay wet for very long and so the sort of processes that Wilde and others have written about would simply not have time to occur - before road-users have had chance to take account of the change in risk, the road will usually have dried up.

There are, however, two reasons why this does not quite get over the objection. First, there is a sense in which wet roads are not short term at all. Although the driving population may not be in a position to perform some homeostatic operation in risk from a single day's exposure to a wet road, the same population is in a position to compensate the next time a wet road comes along. The number of hours a road stays wet on any given day is now less relevant than the total number of hours on which the driving population is exposed to wet roads and could be brought under closed-loop control. In other words, perhaps a driver may not learn that wet roads are more dangerous than dry roads in the course of a single day, but would learn the same over, collectively, many days. One could get over such a counter by suggesting a generalized rather than specific response to adverse changes in environmental safety. That is to say, it may be the case that wet roads are compensated for, but in the weeks and months *after* drivers have been exposed to them rather than the minutes and hours during which they *are* exposed to them. Second, the only reason why RHT is postulated in the long term is that it is said to be the product of a closed loop process in which feedback influences behaviour. Since feedback occurs in the long term, it is a long term process. But what happens when feedback occurs in the short term? Clearly in the case of wet roads feedback is given immediately in the form of reduced visibility, longer pulling-up distances, etc. Why, then, is there not a short term homeostasis?

2. The question of a compensation to environmental hazards such as wet roads, fog, and ice does however have bearing on the issue of open loop mediation in RHT. In the case of hazards like these drivers may not immediately know through the closed loop processes of their own experience that they are in more danger, but then they hardly need so sophisticated a device to tell them so. Surely the mere information that the ice is there at all should lead to the same conclusion as that arrived at through long-term feedback processes: for the same level of risk, behaviour changes are essential. If there is any open-loop component to RHT (and one is not explicitly ruled either out or in), this much should be obvious.

3. Adams (1988) cites evidence that contradicts McKenna. The case of Ontario is discussed, where snow and ice have been shown to be associated with reductions in both fatalities and severity of accidents (Adams, 1985a, p.43). Adams also cites the case of Sweden in changing from driving on the left to driving on the right. Did this cause a greater number of accidents? Not a bit of it: a 40% reduction in fatalities on normal levels was seen (Adams, 1988, p.45).

How can the evidence cited by McKenna, and that cited by Adams be so far apart? The answer, as one might expect, lies in the choice of denominator. Adams reports total numbers of fatalities and severity, over a given period. What he is saying here is that accident levels are lower in winter months than in summer months. But is it not the case that fewer people are driving in the winter than in the summer? Yes, acknowledges Adams; but if a reduction in environmental safety has changed the behaviour of these drivers - caused them to stay at home or take a train rather than drive their cars - this too is part of the homeostasis process and should be taken into account. One has to be careful here in moving the goalposts: RHT is clear in predicting the compensation to take place at the level of the time unit of exposure. Adams's data has nothing to say about this possibility; unless of course this time unit can include the exposure to hazards in the cognitive sense of being aware that they exist, as well as being, as it were, physically in the midst of the hazard. Also, as Adams points out, there may be a reporting bias to take account of in that in good weather people may be more inclined to wait at the scene of an accident for reporting to the police to take place.

Another reason why there may be fewer accidents in bad weather conditions may simply be that bad weather conditions occur less frequently than good or moderate weather conditions. Thus, when Adams cites the fatality rate associated with different weather conditions, he frequently does not make any correction for weather condition frequency. To be sure, this does leave poor and bad weather conditions as apparently less hazardous than good weather condtions, as shown for the Ontario case, shown in Figure 1.1:

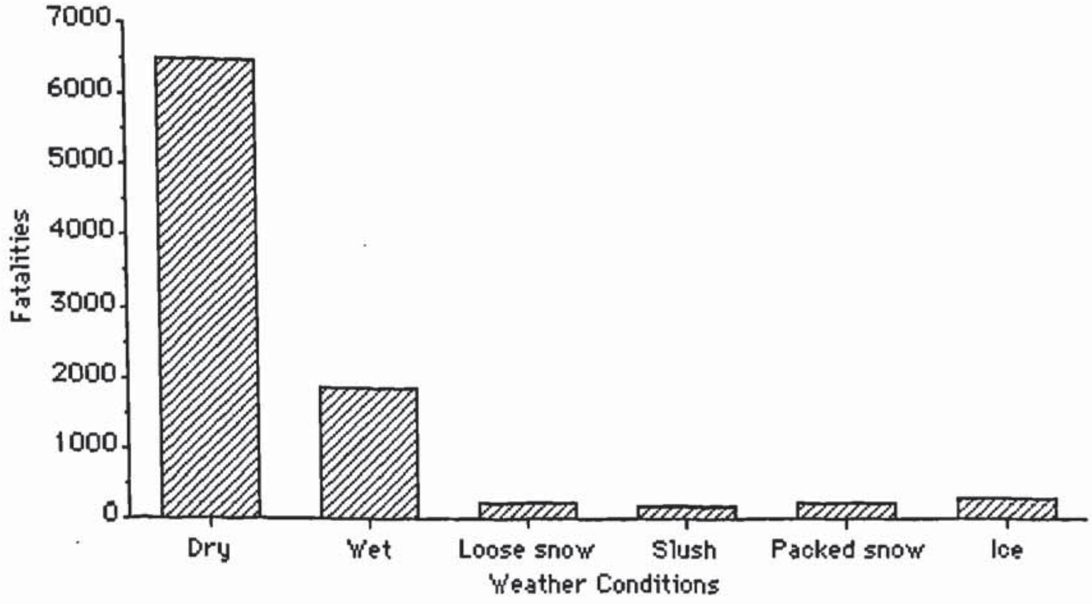


Figure 1.1: Weather conditions and fatalities in Ontario for the period 1974-1980. Source: Motor Vehicle Accident Facts, Ontario Ministry Transportation and Communications, annual; cited by Adams (1985a).

Since the issue of bi-directionality has been linked with the falsification question, it is worth examining these issues together by introducing a new scenario:

Scenario (d): Measures are taken that reflect accident loss per time unit of exposure of dry, clear days and on icy/foggy days. These measures unambiguously point to an increased accident loss on the days with bad weather.

Is this a falsification of RHT? Wilde (1989) answers this question by pointing, albeit indirectly, to the prevailing target level of risk. Who, he asks, are these people who drive their cars in such bad weather conditions? His answer, and one that is hard to argue very much with, is that this *subset* of the population for all we know may differ quite significantly from those people who stay at home as a response to inclement weather. Perhaps they have a higher target level of risk. Perhaps they are just not very good at perceiving the *actual* level of risk that they face. Either way, the possibility that they operate with a higher target level of risk or reduced ability to calculate risk, is not only a real one, but a likely one. If one accepts this, then scenario (d), in common with the first three scenarios, is not one that would falsify RHT.

If Wilde's point about a subset replacing a population after a decrease in intrinsic safety is to be accepted, then logically the same might just as well hold true when there is an *improvement* in intrinsic safety. Suppose that a law is introduced to restrict the speed at which motorcycles can travel on public roads. This law, to reflect the greater vulnerability of motorcyclists relative to other vehicles, requires all motorcyles to maintain a maximum speed of 50% of the speed that applies to all other vehicles. The law, let us suppose, is effective in significantly reducing accident loss. On the same argument of subset versus population, might it not be the case that risk-taking (or, perhaps, sensation-seeking) young motorcyclists have switched to some other form of transport, or, for that matter, to some other form of enjoyment? The population may have changed, and with it, the operational target level of risk.

This would seem to point up a very important lesson for researchers in the field: there really is little point in measuring any component of accident loss resulting from a reduction in intrinsic safety, since it is quite impossible to envisage a finding that would have any significant bearing on the theory, other than those which support it. Again, the goalposts are on the move, for all findings on this issue, as with so many others in RHT research, either support RHT or are irrelevant to it.

1.1.5 Risk Homeostasis Theory and simulation

The issue of whether RHT can be looked at through a simulated environment is, on the surface, very much open to question. Given that RHT is postulated as a closed-loop, population-level phenomenon - a description of how people behave in relation to environmental safety changes in real situations - a critic might well object to the proposition that RHT can be examined via simulated exercises. Such a critic might point out that:

(i) The whole notion of RHT is that compensatory behaviour is only seen in the *long term*. Only where behaviour is observed over a period of months or even years can the homeostasis question be examined. In the case of thirty or so minutes in a simulated risk-taking environment there are no good reasons to suppose that there will be any compensation. In short, the typical experimental time-span is not appropriate.

(ii) RHT is postulated at a *population-level:* the behaviour of a hundred or so participants in a laboratory-based experiment is very nearly irrelevant. What matters is the behaviour of the whole population. The important point here is that no one knows where the compensation process takes place within the population, whether RHT is a small effect from a large proportion of the population, or a large effect from a relatively small number.

(iii) Even assuming that these first two points could be overcome, it must be remembered that RHT is about *real* situations, not simulated ones. Whatever a simulated exercise in RHT might find, we cannot begin to answer this question of whether the participants would have behaved in the same way in a real traffic environment. The possibility that a simulated traffic environment is, for the most part, treated trivially, is a very real one.

Whilst none of the above points could be disputed, this is not quite the whole story and a strong case for examining RHT via simulated work could be put forward.

1. Although a simulation exercise may involve a very different time-span from that envisaged by Wilde and others in formulating RHT, one must remember that the simulated environment holds open the possibility of 'collapsed experience'. Although RHT is something that populations are said to tend towards in the long term, this is in essence because the sort of factors that are said to influence compensatory behaviour tend to take place in the long rather than short term. If these long-term factors (factors such as accidents and near misses) could, through the use of simulations, be collapsed

into the short term, then there are no longer any logical reasons why RHT *must* be examined in the long term.

2. Whilst it is true that RHT is indeed postulated at a population level, this is not by itself a reason not to examine a relatively small number of individuals. Since whole driving populations are made up of individual drivers, to focus attention on the behaviour of those individuals may well be of real value in the modelling of risk homeostasis.

3. The question of how 'seriously' participants take simulations - whether they actually believe that they are driving a car when placed in a simulated driving environment, or treat the experiment more like an arcade game - is of course a real one. All experimenters can do here is to ask those taking part to behave as they would when driving a car, to point out that unless this is done, there is really no point in the experiment at all, and to ask them on completion, whether they felt that their behaviour was realistic. There is evidence to suggest that with behaviour differences arising from the factors of age and sex the Aston Driving Simulator can differentiate in much the same way as in a real road traffic environment (Taylor *et al.* 1990).

It must also be said that the extent to which any given simulation is treated as a real driving experience would be is much less of an issue where an RHT effect is observed. If participants are compensating in any way for changes in environmental safety, then this would seem to indicate that the concepts of safety and risk may be generalized to and from simulated work.

Finally it must be said that the use of simulators in RHT is not about the provision of a definitive answer to the risk homeostasis question. What simulations can do however, is to allow experimental manipulation of those variables which are said to mediate the effect. In this sense the simulator is just another source of evidence aiding our understanding of risk homeostasis, providing benefits that could never be had from either the field work or the theoretical modelling on which the case for RHT rests.

The assertion that RHT has not been examined in a simulated environment may meet with some opposition. Certainly RHT has been examined in an *experimental* way by a number of researchers in the field (Cownie, 1970; Mittenecker,1962; Näätäten and Summala,1975; Veling,1984; and Wilde, Claxton-Oldfield and Platenius, 1985). Taken together, one could say that these studies provide limited support for RHT propositions. But there are reasons to suggest that all of these experimental investigations employed methodologies inappropriate to the investigation of RHT. Rather than simulating traffic and road situations, they were, in essence, games, involving costs and benefits to the participants. The investigators asked the question; would the participants maintain a constant level of risk in these game situations?

So can experimental 'game' conditions be generalized to risk taking on the roads? Wilde *et al.* answer this by pointing out that evidence does exist for what one might term the generalisation of risky behaviour, within subjects, to different conditions, citing Jackson *et al.* (1972) in support. It seems fairly clear that this generalisation argument can not be used in support of this form of investigation for five important reasons. First, the Jackson paper refers to an operationalized concept of risk but one independent of homeostatic behaviour. To verify that *homeostatic* behaviour can be generalized across different risk-taking contexts would be impossible, demanding as it does that the homeostatic behaviour in question - behaviour in which consequences involve physical harm to oneself and others - cannot be individually quantified, and hence, cannot be reconciled with the risk-taking behaviour of the same participants in some other domain, not involving the possibility of physical harm. As to whether we can generalize the findings of RHT from those situations involving the possibility of real physical harm to those involving no such possibility, we do not know. Second, it seems apparent that the absence of the possibility of physical harm, to oneself or to others, leaves any exercise in RHT as little more than an investigation of optimisation strategies. The singular absence of a physical threat leaves words like *risk* and *danger* with altogether different meanings. If real threats do not exist, participants should, it is argued, at least behave *as if they did* - *as if* their behaviour could lead to the death or injury of themselves or others. No-one put this better than Wilde himself, who, in 1982 (three years before his own simulation work) wrote:

"[...] simulation of risk, like a sham duplicating the real thing, is a contradiction in terms."

The 'simulator' investigation undertaken by Veling (1984) illustrates this point. Veling asked participants to maximise their 'points' in a game-type approach where points were allocated for each digit correctly summed in a given period of time. The constant risk hypothesis, by which Veling refers to RHT, predicts, says Veling, that where the computation time is reduced, a constant level of risk will be maintained by participants via behavioural compensation through the control of the number of digits to be added (penalties were made against incorrect additions). In such a design, and Veling's design is fairly typical, there is, to put it bluntly, a right and a wrong response. There is on the one hand a behaviour that will maximise points and minimise penalties, and there is, on the other, a behaviour that will minimise points and maximise penalties. One could therefore argue that in reality Veling's design, and others like it, tested nothing more sophisticated than the hypothesis that participants were bright enough to see all of this for themselves. There was, in an objective sense, a logical course of action, all other courses being, in the same objective sense, simply wrong. Quite how this parallels RHT behaviour is far from apparent. In RHT, risk is held to be a commodity to be traded for objective or subjective benefits. There is never a 'right' course of action, only a chosen course of action, and unless this important distinction is acknowledged in RHT simulations, something other than risk homeostasis is, one could argue, being tested.

There is a third and rather more subtle reason to suggest that perhaps RHT cannot be examined on game-type approaches. Any effect that involves a general optimisation process may well be cognitively mediated in that compensation may be the output of processes at quite a high level of consciousness, the output of hypothetico-deductive processes. The sort of compensation processes that Wilde and others have written about would appear not to be analogous to this. As Wilde (1988, p443) wrote:

"They [the factors that determine individual target levels of risk] *are probably so thoroughly internalised that most individuals are not consciously aware of most of them most of the time."*

He goes on to say (p.444):

"The comparison [between target level of risk and experienced level of risk] *would normally be expected to occur at an intuitive and moderately conscious, i.e. 'pre-attentive' level (Ben-David et al 1972)"*

The proposal that the processing of optimisation strategies within a game-reward experimental design is at this same 'pre-attentive' level is not one for which the present author has been able to find any evidence. Particularly with the overt link between personal performance in these tasks and financial reward made clear at the outset, it does not even seem likely. The possibility therefore that these sort of designs, and attempts at least to mimic real physical risk, may measure processes at different levels of cognitive complexity must be considered.

Fourthly, even if the generalizability question is, as Wilde proposes, to rest on the empirical evidence, there seems even less reason to generalize. To be sure, Jackson *et al.* did provide some evidence for the treatment of risk as a unitary phenomenon, but only in their second order factor analysis. They also obtained four oblique factors of risk corresponding to the facets of monetary, physical, social and ethical risk taking. Moreover, the Jackson *et al.* paper is just one study. As the writers themselves acknowledge, Slovic (1962), Weinstein (1969), and Kogan and Wallach (1964) all found little if any evidence to suggest that risk-taking behaviour can be generalized. Jackson *et al.* offer little explanation as to why their findings differ from earlier results in this way. Even Wilde's own findings - from the very same paper in which he cites the Jackson evidence - throws doubt on the generalizability of risk. Through the use of questionnaires Wilde identified two distinct groups: "risk seekers" and "risk avoiders". Since a response on Wilde's simulation task in excess of 800 ms represents a risk, one would expect the two groups to differ on this criterion. Only a non-significant trend was found. A *post hoc* analysis of between groups differences on the 700 ms threshold then took place; again, only a non-significant trend was found. At 900 ms the same was true. Finally, on 1000 ms threshold, the between groups difference was significant, albeit at the .05 level. The evidence then on empirical grounds for the generalisation of risk-taking findings, from non-physical to physical risk, is, to be charitable, mixed.

Finally, if RHT is cognitively mediated (and about that possibility one can say very little) then an active decision to change behaviour such as to negate an environmental safety improvement should, one hopes, be subject to *ethical* deliberation. One would hope that the conscious negation of the benefits brought about by safety belts would be rejected at least through a realisation that it would contribute unnecessarily to total accident loss, something *morally* unacceptable, whatever one's personal feelings might otherwise be. With a game-type experimental design, such moral questions simply do not enter into the compensation question: to maintain a constant level of risk is, morally at least, perfectly legitimate when one can harm neither oneself nor others. Of course, it may be that RHT is not cognitively mediated; it may be that it is cognitively mediated, but that it is not substantially affected by moral questions. Again, our knowledge is limited, and to assume no moral component in RHT would be dangerous; hence, to explore RHT in a context clearly devoid of moral considerations would be to invite a confounding variable.

But why examine RHT in a simulator at all? If its conceptual roots lie in the road traffic data it seeks to explain, surely its investigation in the laboratory can only represent as step backwards? The reply to this has always been that simulation is something we would all rather avoid, but that it is necessary if manipulative experiments are to be carried out; these in turn are necessary if we are to assert causality. This is true. The employment of a simulator within a laboratory-based experiment has however a further advantage, so far not stressed in the literature. Outside of simulated work, RHT is not uniquely psychological. Certainly it is a theory to which psychologists can make a contribution, but there is a danger of seeing it as a purely statistical phenomenon. So far as the road traffic data are concerned, the central questions (questions like *Does the effect really* *exist?...Is the homeostasis complete?.... How long is the time-period of its operation?*) can be answered entirely by statisticians from any discipline. It might be a psychological phenomenon, but it exists to date without very much in the way of psychological methodology and understanding.

If the above are essentially statistical questions, what would psychological questions look like? Cognitive psychologists would want to know whether the homeostatic operations under discussion are at the pre-attentive level Wilde (1988) suggests, or whether they demand attentional resources. Can the cognitive mediation of the risk information we receive be given an algorithmic description? If so, are there individual differences in this mediation? Is there an attentional reduction following the introduction of an improvement in intrinsic safety? If these questions and others like them are to be tackled at all, a series of controlled laboratory-based experiments involving the simulation of real physical risk would be essential.

1.1.6 RHT research in the future

1. So far, proponents of RHT have been reluctant to specify the behavioural pathways along which RHT might operate. It is the effect alone that is of interest to them. If however, the utility criterion is to be satisfied, then the number of ways in which homeostasis can take place is limited to just a few practical possibilities such as increased speed, increased objective risk of overtaking and attentional reductions. To treat possible pathways to homeostasis as imponderable wonders is neither helpful to researchers in the field, nor necessary. What is more, if opponents of RHT are able to point to possible behavioural pathways that do satisfy the utility criterion, but from which no evidence of compensatory behaviour exists, then the proponents of RHT should explain why that particular pathway was able to escape homeostasis. This is especially so when the very same pathways are conveniently cited when they provide supporting evidence for the theory. Not to provide this explanation would be to leave RHT as a purely statistical description of road behaviour, devoid of any psychological theory.

2. All researchers in risk homeostasis theory would have to agree with the statement that *'behavioural compensation in response to environmental safety changes can occur.'* The two key words that should bring about consensus here are *compensation* and *can*. By *compensation* is meant a change in behaviour in the direction of negation that may or may not represent complete homeostasis. By *can* is meant of course that the operation may not necessarily operate always and everywhere. It is suggested that this statement - *compensation can occur* - should be the starting point for researchers on both sides of the RHT debate. The job then for RHT research should be to map out the scope and limitations of the theory rather than to cite single cases as evidence for or against RHT in the all-or-nothing way that has so far characterised the debate.

3. Much more work in simulated risk taking environments is needed, but in this work, real, physical danger should be simulated. This might take such forms as a simulated air traffic control environment, a simulated car-in-traffic environment, and simulated shipping exercises. Only in this way can RHT break free of the statistical level of explanation in which it has so far existed, and start to enjoy a psychological description in terms of personality trait theory and cognitive modelling.

Risk homeostasis theory has in the past had to fight off charges that it is in some way a pessimistic view of environmental ergonomics. Whilst Wilde (1989) is quite right to point out that the question of whether it is pessimistic or optimistic is irrelevant (it could of course be both pessimistic and true) there are several reasons why it is not necessarily a negative theory. As Wilde has repeatedly pointed out, there is no reason why a motivational intervention should not lead to a reduction in accident loss. Where the target level of risk can be brought down (through, say, incentive schemes), RHT would actually predict such a reduction. If ergonomists devoted more resources to projects aimed at educating road-users and machine operatives, reductions in accident loss could be expected. Moreover, RHT does not posit risk for its own sake: there has to be some benefit arising from a more risky behaviour. RHT would predict that where interventions in environmental safety are extended to include controls limiting specific compensatory behaviours, the safety intervention would be successful. For example, if the compulsory wearing of seatbelts were accompanied by assiduous traffic policing, an increase in speed to compensate for the intervention may well be prevented. In industry, machines may be made intrinsically safer whilst at the same time imposing constraints on the extent to which operatives are able to change their behaviour. Indeed, physical risk may be removed.

It is ironic that the same workers who charge RHT with being pessimistic deny that behavioural compensation can occur such as to negate adverse changes in environmental safety, such as bad weather. If road users made no allowance for ice on the roads, heavy rain, or fog, then we certainly would have something to be pessimistic about.

RHT does not say, either explicitly or implicitly, that accidents are inevitable, that we cannot do anything to improve safety. What it does is to suggest that changes in environmental safety can lead to changes in the behaviour of those affected by them, and that consequently, some interventions are more likely to be successful than others. If it is accepted, RHT provides exciting opportunities to improve *real, operational* safety levels and to reduce accident loss. To suggest that this is pessimistic is not to understand its claims.

One could very nearly summarize this position by saying that both opponents and proponents of RHT agree that improvements in intrinsic safety are a good thing. Opponents of RHT believe that when an environment is made safer, a reduction in accident loss will follow. Proponents of RHT believe that environmental improvements will allow people to gain real benefits (such as arriving at their destinations sooner) without increasing their risk of having an accident. It is then merely the meaning of '*a good thing*' that is in dispute. In that sense, there is nothing left to argue about: the question of whether to improve intrinsic safety does not arise.

1.2 Applying RHT to the case of motorcycles

1.2.1 Introduction

The purpose of this section of the thesis is to demonstrate how the theory of risk homeostasis might be applied to one category of road-users - motorcyclists. This choice of road-user group was made for several reasons. First, perhaps more than any other single group of road-users, motorcyclists have attracted the attention of road-safety scientists by virtue of the very high level of accident loss that has been associated with this group. Any intervention likely to bring about reductions in accident loss for this group would therefore be of particular interest to safety scientists. Second, there have been attempts to bring about improvements in the environmental safety of motorcyclists through the compulsory wearing of safety helmets (in Britain, this change took place in 1973) and through safety education programmes. Both of these (and a number of cases of subsequent deregulation of motorcycle helmet use) provide examples of interventions that should, if the explanation offered by RHT is to be rejected, bring about favourable changes in levels of accident loss. For this reason the motorcyclist has been a key participant in the RHT debate. In the section to follow, however, the emphasis is not on the effect of mandatory helmet use (this has already been explored very thoroughly by amongst others Adams and McKenna). Rather, the focus is on an RHT interpretation of accident loss in general and without reference to particular interventions, although some consideration shall be given to the effect in terms of accident loss, of training courses aimed at improving riders' skills.

1.2.2 The extent of the problem

Although more than 5,000 people are killed on British roads every year, the overall picture is not as gloomy as it first appears. In the mid 1960s for example, that same statistic reached a peak of almost 8,000 - and of course with fewer motor vehicles, the probability of being killed or seriously injured on a British road today is less than at any other time. In terms of cross-cultural comparison too, British roads are safer than any other country in the European Community and compare well with countries outside the Community also (Rutter, Quine and Chesham, 1992).

Turning now specifically to motorcycles, over the period 1980-89, motorcyclists were one of the few groups, as Rutter *et al.*(1992) report (citing Department of Transport figures), to show any significant evidence of a change in accident loss. Road deaths declined from 1,163 in 1980 to 683 in 1989. Serious injuries show a similar fall from 21,543 in 1980 to 11,805 in 1989.

Of course, none of this takes account of either the number of motorcyles on the road or the reduction in the proportion of motorcycles on the road over this period. When these are taken into account, the picture changes. From 1930 to 1989 the number of motorcycles on British roads fell from 31% of total number of registered vehicles to something below 4%. Yet this dramatic reduction in the proportion of motorcycles on the road was not matched by a similar reduction in the number of motorcyclists' deaths over that same period. This figure moved only from 25% to 13% of total deaths on British roads (Rutter, Quine and Chesham, 1992).

As Rutter *et al.* also stress, the period from the 1950s onwards has seen a marked and continuing improvement in the ratio of fatalities for vehicles overall, Britain's motorcyclists have missed out on any improvement.

But the bleakest of all analyses comes when accident frequency data are looked at per km travelled (the number, that is, of registered vehicles in the various categories multiplied by the mean distance each travels). Here the Department of Transport's own figures show that motorcyclists have the worst accident loss figures of all road-user groups. Casualties and deaths per km travelled leaves motorcycles at about twice the rate of cyclists and twenty to thirty times higher than the rates for car drivers.

1.2.3 Individual differences in accident loss between sub-groups of motorcyclists

Is it, one might ask, the case that motorcycles cause accidents, or is it rather an interaction between personality and/or demographic variables that brings about the high rate of accident loss? The answer is a straightforward one: the motorcycling population, certainly in the United Kingdom, cannot be considered a homogeneous whole, but is comprised instead of clearly identifiable sub-groups.

Perhaps the most important single sub-group refers to age (to young riders). The Department of Transport's figures from 1986 show that casualties (per 100 million kilometres travelled) are at a peak for motorcyclists aged 16 (strictly speaking, 16 year olds are allowed by law to ride only mopeds; for the purpose of this discussion, however, no distinction is drawn between mopeds [motorcycles with a power at or below 50cc and speed restricted] and motorcycles above this cc and without speed restriction). For the group of motorcyclists aged 17-18 a large reduction in casualties is seen, made all the more interesting because 17 year olds are entitled by law to ride motorcycles of unrestricted power and speed on passing a motorcycle test and are entitled to ride motorcycles of 125cc power (with an 'L' plate) before passing a motorcycle test. Fatalities between these two age groups is, by contrast, marginally increased. Across age groups 16, 17-18, 19-20 and 21-24, a trend of reduction is seen in the number of motorcyclists seriously injured. With only the exception of the comparison between the age groups of 16 and 17-18 a trend of reduction in fatalities is also seen.

It may well also be that sub-groups of motorcyclists can be identified through social psychological, personality and non-age-related demographic variables. Rutter *et al.* (1992) report such an investigation. Among their major conclusions is the suggestion that the most important behavioural predictor of accident loss in this group is past behaviour which broke the law and which broke rules about safe riding. Since motorcyclists are predominantly young and male, it is perhaps worth considering the accident loss of this group of road users in enclosed motor vehicles. It is known that young drivers speed more often than older drivers (Harrington and McBridge, 1970), that they drive through crossroads at greater speeds (Koneci, Ebbesen and Koneci, 1976), that they adopt shorter headways (Evans and Wasielewski, 1983), and that they are involved in more rear-end collisions than older drivers (Lalonde, 1979). There is also some evidence that young drivers perceive risk differently from older drivers (see Berger and Persinger 1980; Finn and Bragg, 1986; and Matthews and Moran, 1986). It is known too that male drivers take more risks than female drivers (Dorn, 1992). From all of this it seems likely that the group of road users that ride motorcyles would probably be at high risk if they were to change to some other mode of transport.

1.2.4 Motorbikes, accident loss and Risk Homeostasis Theory

How might the perspective of RHT help explain accident loss patterns for motorcyclists? A starting point here might be the efficacy of education and training programmes aimed at reducing accident likelihood for motorcylists. Such a focus has no shortage of either training programmes to look at or attempts to evalutate/validate them (for a more detailed discussion see the contributions of Jonah and Dawson, 1979; Jonah, Dawson and Bragg, 1981, 1982; Prem and Good, 1984; Mortimer, 1984, 1988; McDavid, Lohrmann and Lohrmann, 1989; Rutter, Quine and Chesham, 1992).

Jonah *et al.*. (1981) carried out an analysis of the skills required, as they put it, "*for safe on-street operation of a motorcycle [...]*"(p 307). McPherson and McKnight (1976) developed a test - the Motorcycle Operator Skills Test (MOST) - consisting of nine riding exercises of increasing difficulty. An initial validation exercise with an N of 18 gave support to the test and led to a further validation with an N of 637. The first of these two exercises involved the construct validity as tested on the riders' on-street performance and involved performance judgements by six expert riders.

Jonah *et al.*. (1981) were interested in the question of whether performance on the MOST was capable of predicting accident loss (in other words, does the MOST work?). Three categories of accidents were used in a discriminant analysis - total number of accidents, number of reportable accidents (over a pre-determined threshold of financial damage to the motorcycle), and the number of accidents which appeared in the riders' police files. The MOST had no effect on any one of these measures. Only age of rider and road exposure served as predictors, perhaps pointing to a superiority of on-street practice as opposed to off-road training as a strategy for avoiding accidents.

The concern at this stage shifted, perhaps predictably, to the overall validity of MOST as a measure of skill acquisition. Was it the case, in other words, that MOST was not acting as a predictor because it was not acting as a measure? Prem and Good (1984) therefore devised the *Alternative Skills Test* (AST) as an attempt to bring the test more into line with the real, on-street situation it aimed to improve. There are two problems with the AST, as Rutter *et al.* point out, and they are that: (i) subsequent multiple regression showed it to have virtually identical predictors as MOST; and (ii) no attempt was made to correlate riders' scores on the AST with accident data.

An interesting attempt at evaluating a motorcycle training course (one run by the American Motorcycle Safety Foundation) was carried out by Mortimer (1984). He set up a matched control group to a group of riders who had taken the AMSF course, looking at riding exposure, traffic violations and accident involvement. Partialling out age of rider and years licence held, the AMSF group performed no better than the control group. A follow-up study was carried out in 1988, this time comparing 913 graduates of the AMSF course with a control group of 500 riders. These findings too showed little evidence that the course was in any major sense beneficial for those having taken part. Both the violation rate and the accident rate were not significantly lower for the group having taken the course.

The relevance of Mortimer's work to the RHT question can be seen from Mortimer's own comments:

"Attitudinal or personality variables may also be important in affecting the accident rate, since persons who indicated that they always used seat belts in automobiles had a lower

accident rate when riding motorcycles. This may reflect [...] the possibility that such people do not take as many risks as others and are better able to estimate the actual risks involved in various tasks ." (Mortimer, 1988, p70, cited with omissions by Rutter et al., 1992).

These comments have some bearing on the RHT debate for two reasons. First, the possibility that motorcyclists as a group of road-users, or rather, a sub-set of motorcyclists, have a higher level of target risk would seem to be suggested. Were this to be so, an accident loss differential between these and other road users could co-exist with Wilde's formulation of RHT. Certainly motorcyclists are intrinsically less safe than practically every other group of road users, but if their target level of risk is higher also, then it would seem to serve as a confounding variable to the suggested causal link between the level of environmental safety that exists for motorcyclists and their high (proportionally, that is) level of accident loss.

The contribution of Bragg (1981) too has relevance for RHT. Bragg points out that 25% of motorcycle travel can be described as commuting, compared with the much higher proportion of 40% that exists for car travel. No less than 70% of motorcycle travel is described by Bragg as recreational. The question following from this to be answered from RHT proponents is where an incidence of recreational travel like the one cited by Bragg leaves the concept of utility? RHT has never been about risk-taking for its own sake, but rests instead on the utility that follows from different behaviour options. If 70% of motorcyclists' time is spent on riding *just for fun*, then what is the utility attached to the various behaviour options? Might the motivation for motorcycling in a recreational context be predominantly a sensation-seeking one?

So how might the behaviour of motorcyclists be explained in RHT terms? Before answering this question it is perhaps worth recalling Wilde's theory of how homeostasis might be achieved (from Wilde1988, proposition 2):

Aston University

Illustration removed for copyright restrictions

Figure 1.2: The pathways to the achievement of risk homeostasis (from Wilde, 1988)

By implication from this model, the relative incidence of accidents within a defined and perhaps reduced population is, to put it bluntly, irrelevant. What matters is the crude frequency counts with which the statistical discussions started. This is because leaving the environment and/or changing to an alternative form of transport are both valid pathways to the homeostasis Wilde has in mind.

It will now be recalled that from 1930 to 1989 the number of motorcycles on British roads fell from 31% of total number of registered vehicles to something below 4%. So far as RHT goes therefore, one might almost close the book at this point. The case of motorcyclists would seem, between the above dates, be be explainable in RHT terms through either changing from one mode of transport to another (mode migration) or through avoiding the hazards of road transport altogether and not making the journey that one would otherwise have made. In addition to this, RHT proponents might point to two further sources of evidence from the case of motorcycles in their support. First, there is good evidence to suggest that the high rate of accident loss in this group may be attributable, in part at least, to differences in the target level of risk between this group and other groups of road users, suggesting that to put forward engineering solutions to RHT may be to misinterpret the source of accident loss in this group. It may also be that the high incidence of recreational use plays some part in this. Second, it can be demonstrated that improving the skills of motorcyclists is not sufficient to bring about reductions in their accident rates; yet, when one considers the detailed skill analysis that was undertaken before many of these schemes were widely taken up, it would be difficult to argue that those motorcyclists who took advantage of the course (if *taking advantage* is the right term) were not intrinsically safer through their increased riding skills, skill that, it will be recalled, were demonstrably transferable to on-street situations.

1.2.5 Conclusions

As with all examples, the case of motorcycles does not in any way 'prove' RHT. Opponents of the theory might respond in two ways. First, they might suggest that the emphasis on crude frequency scores in accident loss in misleading. They might argue that if the only way motorcyclists can maintain a constant level of risk is to cease to be motorcyclists at all and become something else instead, then RHT does not amount to very much. So long as motorcyclists *remain* motorcyclists, they are both environmentally and operationally at greater risk than other groups of road users. So far as the case for training efficacy is concerned, they might cite a growing literature that would seem to indicate that, in some circumstances, the training of motorcyclists can reduce their likelihood of being involved in a serious accident (Brooks and Guppy, 1990; McDavid, Lohrmann and Lohrmann, 1989; and Nagayama, 1984). How can it be that these studies appeared to succeed where others have already failed? Although the question remains open Rutter *et al.* raise an interesting point in citing McDavid *et al.* Although McDavid *et al.* found evidence for the efficacy of motorcycle training courses, they were among the first to acknowledge that motorcyclists seeking out training may differ critically from those who do not. An imperfect matching process, or perhaps sampling error, may therefore account for differences between studies. What this criticism and others like it have in common is that they serve to point up the flaws of the quasi-experimental approach. The fact of the matter is, so long as measures of accident loss are taken before and after some intervention, and so long as random allocation to group is not possible, there is hardly such a thing as a truly *independent* variable, and the possibility of confounding variables must always be considered. On this note it is worth pointing out that even the official statistics on the number of people injured in road accidents are unreliable. Zylman (1972) points out that many non-fatal accidents go unreported and therefore unrecorded. Thus, comparisons of official statistics with medical authorities or driver interview show marked differences (Bull and Roberts, 1973; Roosmark and Fraki, 1969). Haddon (1972) suggests that the average number of road traffic injuries in the USA to be in the region of 10,000 per day, a figure about twice as large as that acknowedged by official statistics. Only official statistics of actual fatalities can be regarded as reliable (Dorn, 1992).

But perhaps before this question of experimental design is looked into too closely, another question needs answering: how can it be that following a training course, motorcyclists are, as it were, both safer (having acquired the skills necessary for safe riding) and at the same time, less safe (in terms of their probability of having an accident)? RHT might answer this by suggesting that novice motorcycle riders are perhaps aware of their inexperience and limitations. Risky overtaking, weaving in and out of traffic, and the maintaining of high mean speeds may all come later. Having acquired safe on-street skills, the rider can either *cash in* the safety benefit for the sensation-seeking motive that may well have underpinned his or her decision to purchase a motorcycle in the first place, or may instead choose to be (in an environmental and operational sense) *safer*. Diagrammatically, this might appear as shown in Figure 1.3.

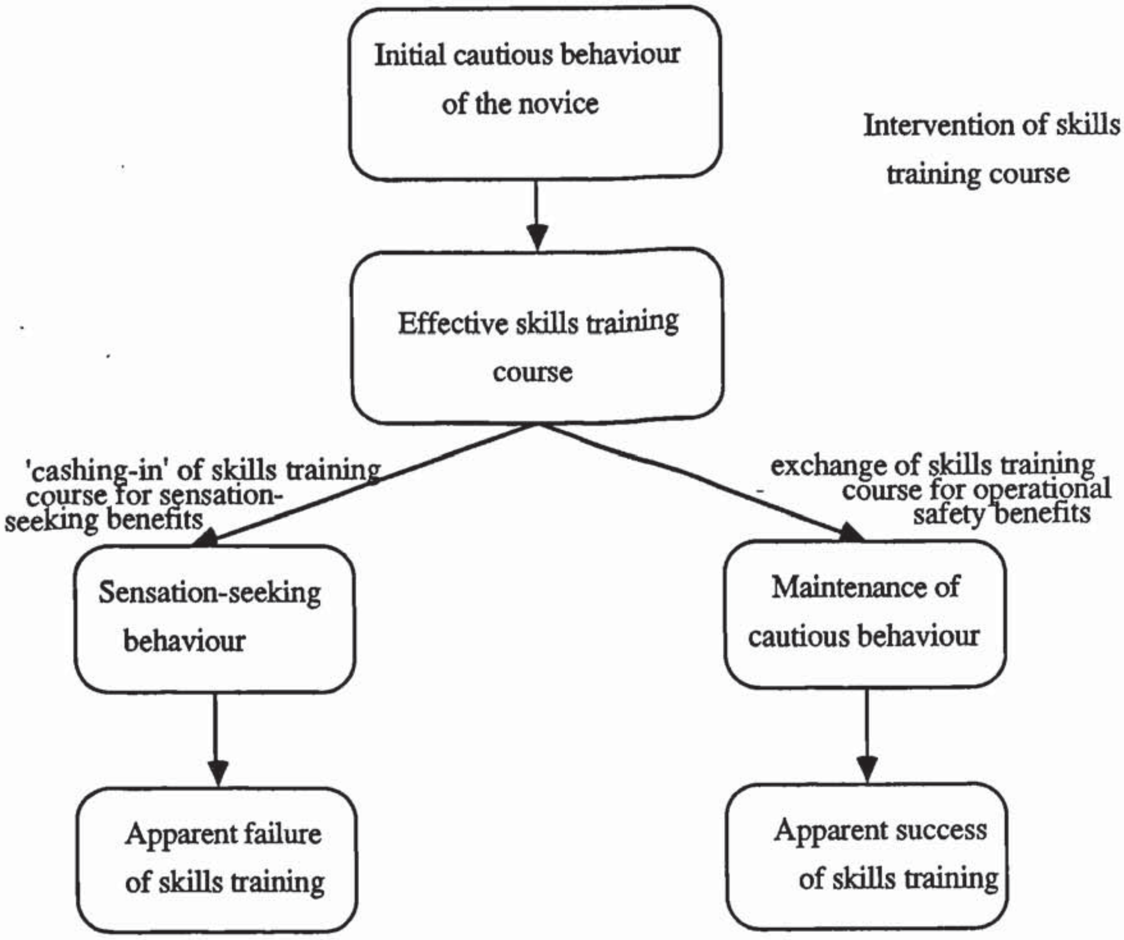


Figure 1.3 : A possible mechanism for risk homeostasis after the intervention of an effective skills training course.

Where a widespread 'cashing-in' of the benefits to be had from the training course takes place, no improvement in the level of operational safety need be seen.

The case of motorcycles and their associated accident loss statistics is at least reconcilable with RHT. The distinct flavour of RHT, its emphasis on the individual's target level of risk as a determinant of accident loss, has intuitive appeal in explaining the failure rate that has been associated with some skills training courses discussed here. That the proportion of road-users travelling by motorcycle has fallen may well indicate that the level of intrinsic risk faced by that group of road-users is so high that the maintenance of a target level of risk within the motorcycling environment is not possible and/or compatible with the relatively high speeds that would be needed for that mode of transport to hold utility either as a sensation-seeking strategy, or a commuting tool. In these circumstances, leaving the environment or switching to another form of transport would seem to be a direct prediction of RHT.

Perhaps what this attempt to explain the behaviour of motorcyclists illustrates most about risk homeostasis theory is the difficulty of falsification of the theory already discussed earlier in this chapter. Suppose first that *all* documented cases of skills training courses had unambiguously led to large improvements in accident loss for those taking up the schemes. Suppose a consensus existed along the lines of *skills training courses really work*.. It has already been acknowledged (by McDavid *et al.*, 1989 and by Rutter *et al.*, 1992) that those motorcyclists who take up the offer of a training course may differ quite significantly from those who choose to ride without taking up any training. Putting this in risk homeostasis terms one could say that the two groups may have, indeed, would appear to have, different *target* levels of risk. This being so, that the two groups should differ in terms of their accident loss levels is not only consistent with RHT, it would actually be *predicted* by it.

But consider next a consensus along the lines that *skills training courses do not work*.. Suppose that all training skills courses, regardless of their quality, effectively fail in reducing accident loss for the group of motorcyclists who take up the training. Would this constitute a falsification of RHT? If it could be demonstrated that the training skills courses had objectively reduced the intrinsic risk faced by motorcyclists through better equiping them to deal with the hazards facing them on the road, and still accident loss were to remain at its pre-intervention level, this would seem to indicate that the benefits to be had from reduced intrinsic risk were 'cashed in' for benefits such as increased speed and increased sensation. The favourable change in intrinsic risk would therefore appear to have been negated through behavioural adjustments. This too would be compatible with RHT.

The case of motorcycles then points up the difficulty of determining whether the target level of risk has altered with the intervention (here, changes in accident loss levels would be predicted), or whether the target level of risk has remained constant at its pre-intervention level after the intrinsic safety intervention of the skills training course (here, changes in accident loss levels would not be predicted). This means that whilst the findings discussed here are compatible with RHT, other findings, diametrically opposed to the ones reported here, could also be reconciled with the theory. The case of motorcycles therefore illustrates in a practical way the rather abstract point made in the first part of this chapter. This constrasts quite sharply with the engineering approach (see Evans, 1985) in which the predictions in terms of accident loss after some environmental intervention are at least clear, even if they are not always empirically supported.

Chapter 2 - Wilde (1988) and RHT : a closer look at key propositions, their implications and their context.

For the purposes of this discussion of risk homeostasis theory, Wilde's (1988) paper entitled "*Risk homeostasis theory and traffic accidents: propositions, deductions and discussion of dissension in recent reactions*" *(Ergonomics, volume 31, number 4, pp 441-468)* will be taken as definitive. This is because Wilde, having formulated and expounded RHT by the early 1980s, used the 1988 paper explicitly to set forth 15 propositions, together with deductions, that both reiterated what RHT was, and tried to remove what ambiguity had hitherto existed. It has then to recommend it that it was written by the formulator of RHT, that it is recent, that it is concise, and that it 'broke-down' RHT into, what could almost be considered to be, fifteen constituent, and clearly identifiable, parts. It has been suggested that Wilde's 1988 paper and his contributions from earlier in the 1980s (eg Wilde 1982a, 1982b) are at times contradictory (McKenna, 1988). Rather than evaluating this claim of inconsistency, it will be assumed that the 1988 paper, through its being more recent, is, from Wilde's perspective, definitive. Because this paper is seen as definitive, all the predictions tested in this thesis are reconcilable with this single paper.

It is, however, important to emphasise that the vast majority of studies relevant to the risk homeostasis question are in fact just as relevant to other theories of risk taking in response to environmental risk changes (such as Pelzman, 1975, and O'Neill, 1977). What these theories have in common is their emphasis on the behaviour changes in the direction of negation that follow interventions aimed at improving intrinsic safety. To that extent, most of this thesis has as much bearing on Gibson and Crooks (1938) as on Wilde (1988). The emphasis on RHT reflects its dominance among these theoretical models, although at times the experimental work reported here tests RHT predictions alone (studies 1 and 2 in particular fit into this category). The detailed discussion of RHT as expounded by Wilde in 1988 that now follows should be seen in this general context.

This chapter, having set out Wilde's view of risk homeostasis theory, attempts to put that view in context by examining other theories that could be said to compete directly with RHT. This is done with particular reference to the paper by Leonard Evans published in 1985.

2.1 Wilde's Risk Homeostasis Theory (1988)

PROPOSITION 1: The aggregate accident loss that has actually occurred in road traffic may be defined as the sum total of the cross-products of the frequency of accidents and their costs.

Under this proposition, but before any deduction, Wilde makes a distinction between accident loss per km of mobility, per time unit of road-exposure, and per head in the entire population in a given jurisdiction, over a given period of time. These three criteria are referred to respectively as *spatial, temporal*, and *per capita*. His main deduction here is that an accident loss reduction on the one criterion (of unit distance of mobility) may not be effective in reducing accident loss with respect to the other two criteria.

The main proposition is of course not a matter for empirical verification, but rather, serves as part of the theory's definition. It does, however, have implications for the empirical falsifications of RHT in that the subjective nature of this definition of accident loss means that it is difficult to calculate. For example, if one were to set about falsifying RHT by referring to accident statistics, and if one were to demonstrate that the absolute number of accidents had fallen after some intervention or other, this would not by itself constitute a falsification of RHT if the severity of those accidents had, on average, increased.

<u>PROPOSITION 2: Any individual is at any moment of time characterized by a target (ie preferred, accepted, tolerated, desired, subjectively optimal) level of transportation accident risk. This is defined as that level of subjective accident risk at which the individual estimates that the balance of all expected benefits minus all anticipated costs (including the perceived danger of accident) of his or her amount and manner of mobility is maximized.</u>

So far as deductions go, Wilde states that the target level of risk that an individual holds can affect the manner in which the individual drives. However, he goes further and suggests that target risk can have two more effects. The first of these is whether to make a trip. Perhaps then, a low target level of risk relative to actual risk can result in a trip not being made, say when road conditions are affected by ice. The second possible consequence is mode change, as from, say, road to rail, or rail to air. In this thesis these three possible pathways to homeostasis are described as, respectively, *behavioural adjustments within the environment*, *avoidance*, and *mode migration*.

<u>PROPOSITION 3: The target level of traffic accident risk is determined by four classes of motivating (subjective utility) factors: (1) the expected advantages of comparatively risky behaviour alternatives, (2) the expected costs of these, (3) the expected benefits of comparatively cautious behaviour alternatives, and (4) the expected costs associated with the latter.</u>

Wilde goes on to point out that some motivating factors are economic in nature, whilst others may be cultural, social and psychological. He states also that:

"They are probably so thoroughly internalized that most individuals are not consciously aware of most of them most of the time." (p.443)

Propostion 3 is particularly significant in that it has implications for the falsification of

RHT. From this proposition it must be clear that the individual's target level of risk is not constant. It can change from journey to journey, and it can change in response to a changed economic outlook. Two implications follow from this proposition:

(1) If accident-loss after some intervention falls, but at the same time there is a change in a cultural, social, psychological or economic variable, relevant to the population in general, then RHT has not been falsified.

(2) Where safety professionals channel their efforts into changing target levels of risk (rather than, as they tend to at the moment, attempting to bring about favourable changes in environmental risk), a fall in per capita accident loss should be seen. Were these efforts to be successful, RHT could not be rightly called a "pessimistic" theory.

So far as the question of exactly how "*thoroughly internalised*" the utility computations are, there is, so far as a literature search was able to find, no empirical evidence one way or the other. However, this part of RHT does not, it must be remembered, form part of any key proposition; nor does it follow logically from any proposition.

PROPOSITION 4: At any moment of time, an individual road user experiences (or expects to experience) a certain amount of subjective accident risk.

This, says Wilde, is determined by such factors as the individual's momentary speed and path in relation to the environment, as well as the behaviour of other road-users. As in the case of a utility calculation, the monitoring of risk is not, says Wilde, necessarily in the individual's awareness. But for the purposes of this particular propostion, Wilde qualifies this with the two exceptions of (i) being asked (when we ask individuals about subjective risk they may then become aware of it) and (ii) sudden, marked changes (here the reader is referred by Wilde to Browning and Wilde, 1977, and Grant, 1985). Therefore, with little qualification, Wilde supports the assertion of Näätänen and Summala (1976) in suggesting that subjective risk experience is at what one might describe as 'psychological zero'.

It is perhaps worth adding that Wilde states quite explicitly that subjective risk, as distinct from objective risk, is not viewed as a multiplicative process of (likely) accident severity and probability of accident. But Wilde is less clear about what exactly then it is, stating only that it is:

"*[...] a much more global notion, reflecting the degree of danger felt by the individual.*" (page 444)

Wilde then goes on to state that factors such as the individual's momentary speed and path influence this calculation.

PROPOSITION 5: At any moment of time, an individual road user compares his or her target level of risk with the level of risk experienced or anticipated, and attempts to reduce any difference - in either direction - to zero.

What is particularly interesting about Proposition 5 is its implication that RHT is bi-directional. Wilde was careful here not to state the behavioural adjustments in just one direction - shifts to risk to compensate for improvements - but leaves the model capable of making predictions where the level of environmental risk changes for the worse. The comparison to which Wilde refers is, he suggests probably at a '*moderately conscious*' or '*pre-attentive*' level. Proposition 5 is used to reiterate the scope of adjustment. It includes speed, direction, trajectory, mode of transport, and, ultimately, the decision not to travel at all. Having specified these pathways to homeostasis Wilde reminds the reader that RHT does not specify the nature of adjustment for any given situation. This means that to identify pathways that would appear not to have been affected by some intervention is not to refute the theory of risk homeostasis. From this limitation of the theory Wilde himself acknowledges that only '*a rather abstract deduction*' (page 444) can follow. He states this in saying that the nature of the adjustment - that is, the behavioural pathway chosen - will be the one that is perceived by the individual as best satisfying the target level of risk. This, says Wilde, will be the behavioural alternative for which overall utility of road user will be at its maximum.

PROPOSITION 6: The ability of individuals to maintain equilibrium between their target levels of risk and their actual accident risk depends on their skill to correctly identify the level of danger to which they are (or anticipate to be) exposed.

Wilde suggests that this ability is made-up of sub-skills. The first of these is an ability to perceive properly the objective risk inherent in the situation in terms of its physical properties (as Wilde puts it, its 'risk-relevant' features). For example, a car travelling at 75 mph is in objective terms a greater threat than a car travelling at 50 mph. Individuals who generally would fail to discriminate between these two speeds might be said to be unskilled in this first ability. The second of these abilities refers to the accuracy in estimating the driver's own skills in making decisions commensurate with the maintenance of matching process between experienced and target levels of risk. Wilde expresses this skill in terms of the degree of over- or under- estimation of the decision-making ability on the part of the individual. This he simplifies to knowing what to do to establish balance between the target and experienced risk.

PROPOSITION 7: Due to interindividual differences in danger-detection skill, some road users incur more actual accident risk than matches their target level and others less.

Three deductions are said to follow from this proposition. First, it would only be the case that perceptual training (and more generally, improvements in danger-detection performance) could reduce accident loss if overestimation were more prevalent than underestimation. Wilde goes on to reject this possibility, at least in the long term (see Propositions 8 and 13). Second, Wilde rejects the notion that improvements in psycho-motor skills will result in increased actual risk. As individuals become more skilled in a particular manoeuvre what happens, says Wilde, is that the individual's target and perceived levels of risk come more into line. As this happens, the individual will be more likely to engage in that manoeuvre, and hence no 'real' benefit will be seen (in terms, at least, of probable accident loss). Third, so far as any individual is concerned, skill acquisition will not change accident likelihood; improved accuracy of self-perception regarding these skills will (here Wilde refers the reader to Proposition 6).

PROPOSITION 8: (With the exception of transient conditions referred to in Proposition 13) the average estimation of risk across all members of the road-user population either equals actual accident risk aggregated across behaviours in relation to traffic situations, or deviates from it by a constant (ie steady-state) error.

This Proposition, says Wilde, is speculative. Wilde says that no data exist to verify it and no methodology can be envisaged that might change this state of affairs. That average estimation of risk and actual aggregated risk might be related was supported by Wilde (1982a, p216) who asked participants to rank-order minute and complex changes in risk. When correlated against true ordinal value, so long as these estimates are pooled over a large number of participants, the correlation is a strong one. But as Wilde himself acknowledges, this leaves the question of a constant error term, analagous to the Y-intercept on a Pearson correlation.

In summary, this Proposition means three things. First, it means that populations (very large groups of individual risk-takers all facing some common risk) are highly skilled at making judgements about the risk of one situation relative to another, even where these situations differ very slightly or in a complex way. Second, and by implication (see also Wilde 1982a), individuals *by themselves*, or small groups of individuals are much less skilled at making such judgements. Like so much of RHT, this is a population-level phenomenon. Third, whilst populations are skilled at making judgements about one risk relative to another, they are, for all we know, less skilled at making absolute judgements about any given risk.

The reader might at this point question what is so complicated or elusive about a methodology aimed at investigating this (possible) constant (collective) error. After all, if situations can be objectively rank-ordered before being subjectively rank-ordered so as to allow correlation coefficients to be calculated, then why, one might ask, can we not correlate objective risk in absolute terms with subjective risk in absolute terms? Would such a design not highlight the point at which the Y-intercept occurs? The answer to this is indirectly answered by Wilde who points out, under Proposition 8, that the real value of accident loss is not known. From this, one imagines, Wilde is saying that since its value exists only subjectively, it can hardly be objectively correlated with anything. This does not however, quite answer the question if only in as much as the term accident loss is missing from Proposition 8 which refers only to actual accident risk. Perhaps though, the search for a methodology which involves precise and absolute estimates, even of accident risk alone, is problematic in that it can usually be expected to interact with those individuals rating the situation.

Finally, if there is a non-zero Y-intercept, Wilde asks the question of whether this intercept will take on a positive or negative value. If actual accident risk is represented on the Y axis, with subjective estimates of the same represented on the X axis, Wilde speculates that the intercept will be positive. This he arrives at via the logic that individuals have tended to overestimate their own skill and have tended to be optimistic and overconfident. The reader is referred by Wilde to Svenson (1981) and to Lichenstein and Fischhoff (1977) and Wright (1984) who all provide evidence of optimism, overconfidence and/or overestimation of skills. Were this to be so one might expect that over N observations with X subjective risk and Y objective risk, a line of best fit might look something like:

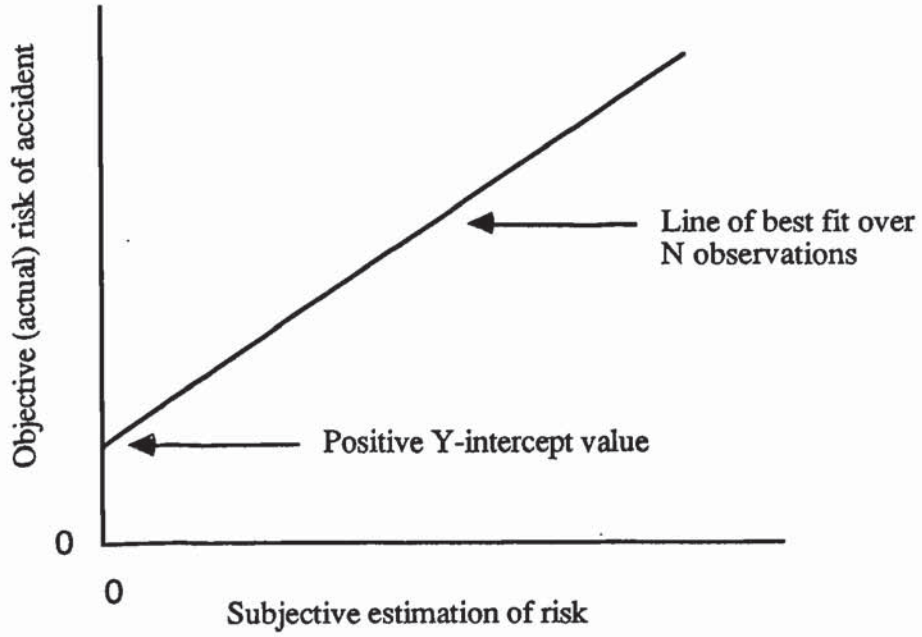


Figure 2.1 - A possible relationship between subjective and objective estimation of risk (from Wilde, 1988).

PROPOSTION 9: Any intentional intervention or fortuitous change in conditions (see Propositions 2 and 3) that is effective in altering the individual's target level of trafffic-accident risk, modifies that individual's actual traffic-accident risk in the same direction.

This amounts to the suggestion that if individuals try to be safer, they will be safer. Since Wilde is careful to avoid any direction to this Propostion (and since he refers to the effect as being in "the same direction" p 445), it follows also that the reverse is true and that when individuals try to be *less* safe, they will succeed also in being less safe. From this Propostion Wilde draws two deductions. The first relies partly on Proposition 3 and states that accident rates (spatial, temporal and per head of population) can be expected to fall for a subgroup of road users, or for the population as a whole, if the utility differential between the fact of having or not having a traffic accident can be increased. The second deduction, one very much on the same theme, states that changes in the economic juncture will increase the utility differential and hence favourably change the target level of risk such as to bring about a reduction in accident loss. A certain amount of empirical support exists for these deductions (see Adams, 1985a, 1985b; Hedlund, 1985; Mercer, 1986; Partyka, 1984; Wilde, 1982b, 1985a, 1986b; Wilde and Kunkel, 1984; Wilde and Murdoch, 1982).

PROPOSITION 10: Any engineering, educational, enforcement or whatever intervention, that encourages (ie enables, recommends, teaches, mandates or rewards) specific cautious behaviours and/or discourages specific risky behaviours, may influence their frequency, but will have no effect upon the population traffic-accident loss, if these interventions do not also affect the target level of accident risk.

Just a single deduction is drawn from this. Since this deduction can be thought of as almost the quintessence of RHT, it is reproduced here in Wilde's own words:

"The population traffic-accident rate will not drop as a result of regulations mandating the use of seat-belts or motor-cycle helmets, limits to speed or blood-alcohol levels, vehicle manufacturing standards, the training or road-user skills, or warrants regarding road-side geometry and signalization, or indeed following any other intervention that does not reduce the target level or traffic accident risk." (p446).

Wilde only qualifies this deduction with the possibility of short-term changes (amplified in Proposition 13).

PROPOSITION 11: Interventions of the type described in Proposition 10, if effective in changing the frequency of specific behaviours in traffic to which they are addressed, lead road users to display other (eg non-regulated) risky behaviours in road traffic at a higher frequency.

This means that interventions that are successful in changing the behaviour/s they set out to change (the wearing of seat-belts in cars and of crash helmets for motor-cyclists, for example) are unsuccessful in their ultimate aim of reducing accident loss statistics. This failure is attributable to changes in other behaviours, changes that might include driving less frequently, driving with greater attention, adopting a less inherently risky following distance, and so on. This, Wilde acknowledges, is popularly termed 'risk compensation', although this term is rejected by Wilde on the basis that compensation, says Wilde, implies a reduction of risk to zero - something that RHT does not specify. Wilde suggests instead the term 'conservation of risk'.

PROPOSITION 12: The traffic-accident loss (and every-day experiences correlated with it) incurred by the members of a population influences the average perceived level of traffic-accident risk among the survivors (see Proposition 4). This in turn influences their road-user behaviour (see Proposition 5), and thus the subsequent accident rate, and so forth.

Wilde points out that this is in essence the homeostasis process (the closed-loop control) to which the term 'risk homeostasis' refers. Quite what Wilde means by *'[...] every-day experiences correlated with it* [accident loss].' he does not spell out. But it is perhaps reasonable to suppose that he refers here to experiences such as seeing an accident take place, seeing the consequences of an accident on some stretch of road, being told of an accident by some road-user who was involved in it, learning of a friend's death or serious injury through a road-traffic accident, and so on. These experiences affect the behaviour of the notional driving population. Perhaps this might be achieved through driving slower or with more attention when learning of the death of another road-user. But without these experiences, or at least without so many of them, the individual can perhaps again begin to take risks.

Wilde draws a deduction from this Proposition. It is that the past accident loss of the collective and the future action of the surviving members of the population, relate to one another in a time-lagged compensatory function. This is described diagrammatically by Wilde, (1988):

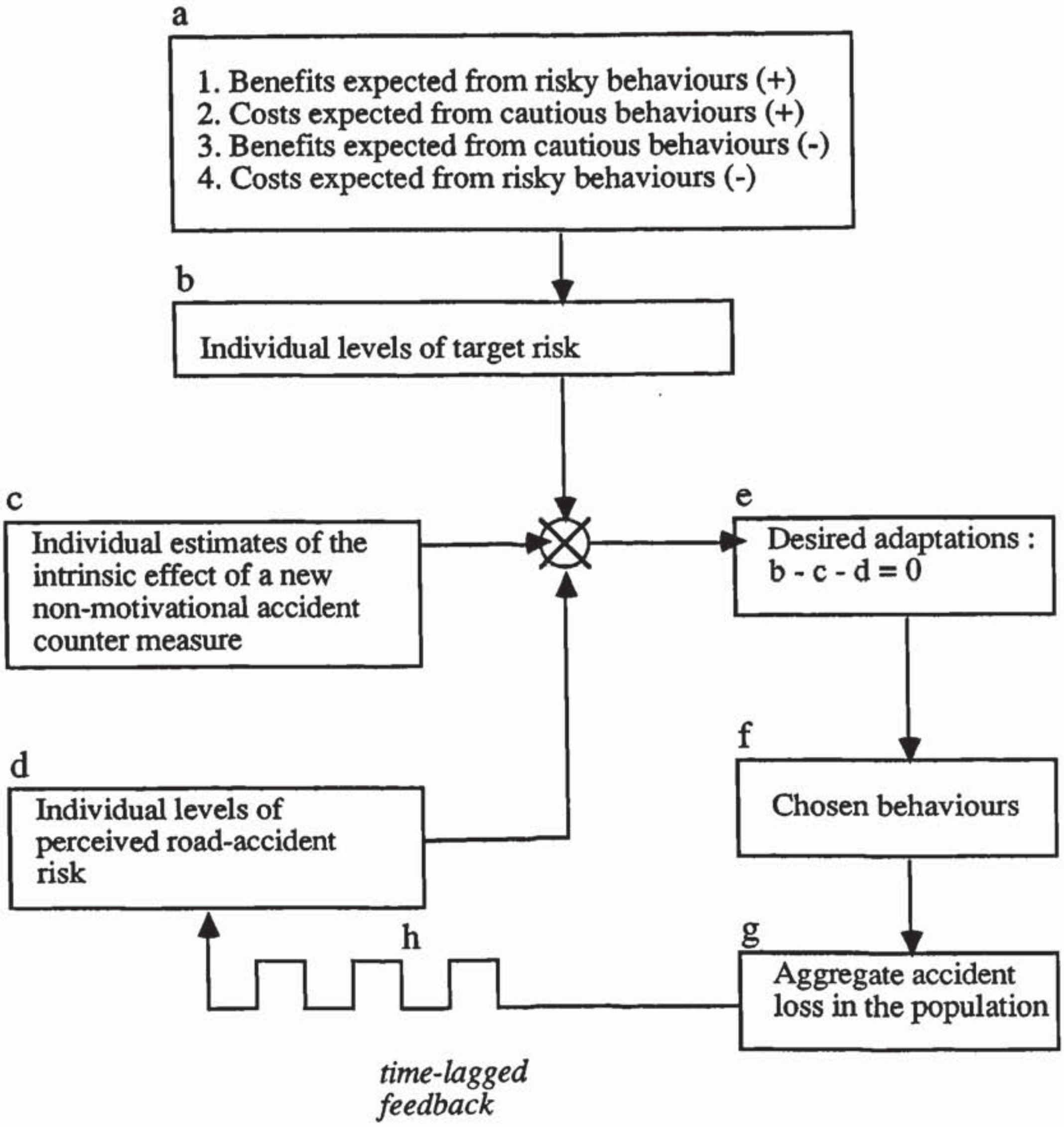


Figure 2.2 - Wilde's model of risk homeostasis theory through time-lagged feedback.

<u>**PROPOSITION 13: Whenever a technical, educational, legislative or whatever intervention - which does not alter the target level of traffic-accident-risk - is introduced, short-term fluctuations in the traffic-accident loss per capita may occur, but these are eventually eliminated through the closed-loop control process postulated in Proposition 12, and the accident rate returns to the pre-intervention level.**</u>

In other words, RHT is a long-term rather than a short-term phenomenon. In order for it to work it requires feedback via the closed loop of accident loss and behavioural adjustments. All of this, says Wilde, takes time, and so any evaluation (any answering of the question of whether RHT holds true) has to take place over the long term.

His first deduction from this proposition is that whether road users initially over-estimate or under-estimate the benefits to be had from a change in, as he puts it, intrinsic safety, is irrelevant *in the long term.* If they initially over-estimate the effect, then accident loss will, in the short term, fall. If they inintially under-estimate the effect, then accident loss will, in the short-term, rise. Over the long term though, accident loss will return to base rate. Wilde adds here that RHT does not specify a direction of initial compensation. It does not say that initial estimates tend on average to be more than or less than that which would be required for complete negation.

A second deduction is drawn. Wilde points out that accident rate will initially rise if road-users are made to believe that the intrinsic benefit is positive, and will initially fall if they are led to believe that the intrinsic benefit is negative. This deduction is significant in that it amounts to a prediction that can be tested in the short term. What Wilde is saying here is that *some* change in behaviour, in the general direction of negation of the intrinsic effect, will be seen *before* the closed-loop operation has a chance to work. But so far as the time-scale of the closed-loop operation is concerned, Wilde takes the opportunity to spell out that:

'[...] *the general principle of RHT contains no information regarding the amount of time necessary for the post-intervention accident rate to return to the pre-intervention level.*' (p 447).

A final deduction is drawn from Proposition 13. Where mass-communication is facilitated (for example, says Wilde, when the intervention is a conspicuous one, concerns few people, or leads to an abrupt change in accident rates), the time interval of the closed-loop mechanism could be expected to be shorter. This has implications for the investigation of RHT in a simulated physical risk taking environment. What Wilde is saying here is that the process of homeostasis is not inherently a long one, but rather, that *it tends* to be long because feedback *tends* to be delayed. Where feedback can be either immediate, or reduced through collapsed experience, it would seem likely that RHT can be investigated over the relatively short time scale that a typical experimental, laboratory-based design would allow.

It is interesting to note that Wilde suggests here that there is some empirical evidence to suggest that the time-span of its operation is between 1.5 and 2 years in a nation of 8 million people (Wilde, 1982a). How can Wilde state as a qualification to one deduction that RHT does not state the amount of time necessary for its closed-loop operation, yet in the next paragraph go so far as to put a figure on it? The answer, it seems, is that the theory of risk homeostasis has nothing to say about any time limit: the figure of 1.5 to 2 years exists merely as an empirical note. Thus, to falsify the above figure would, in the global terms of RHT, not constitue any sort of falsification.

PROPOSITION 14: Any intervention that does not affect the target level of traffic accident risk and pertains to some mode or form of road use, but not to others, may cause a shift in accident loss from the addressed to the non-addressed modes, while the aggregate traffic accident loss per head in the total population remains at the same level.

What this proposition means is that changing the intrinsic safety of one mode of transport may result in a reduced accident loss statistic for that group, but that if individuals leave the mode and move into another, the essential predictions of RHT may still be satistified at some higher level. As a side note here, it is perhaps worth pointing out that the term mode, and the implication of movement from one mode to another, is perhaps not particularly appropriate since as Adams (1988) points out, the migration to which Wilde refers may not merely be from one transport mode to another, but may also be from one transport mode to a quite separate risk-taking behaviour, and one not connected with transportation. An immediate deduction follows from this. Changes (short-term shifts) in accident loss can occur over non-addressed modes of transport as individuals operating in these modes are only slowly becoming aware of the change in behaviour of those people in the addressed mode, and the change in their risk which this implies.

A second deduction Wilde makes is that the proportional contribution that each mode of transport makes to total accident loss, in the long term, will be determined by the relative amount of time that individuals spend using one mode of transport relative to others. This in turn will be determined by how attractive each mode of transport appears, and this 'attraction' can be defined in terms of the motivation factors dealt with in Proposition 3. Wilde goes on here to raise the question of whether total accident loss will be subject to the same homeostatic control. He answers this by pointing out that the purpose of his paper (1988) was to describe RHT in terms of road-traffic safety, interested readers being referred to Wilde (1986b).

PROPOSITION 15: When temporary fluctuations in the pertinent variables (see Proposition 13) are disregarded, RHT may be formulated as follows. Across the entire road network, the entire population, and over an extended time frame (typically one year):

$$A=R.h.N$$

where A=total traffic accident loss in a given jurisdiction and over a specified time (Wilde refers to A as the dependent variable); h=average number of hours spent in traffic by the members of the population in that jurisdiction; R=average level of target risk in traffic across the population; N=the number of individuals making up that population of road-users. What is significant about this equation for accident loss is of course its failure to take any account of intrinsic or environmental risk. So far as accident loss is concerned as it is stated formally above, intrinsic risk is not a factor.

Two deductions are drawn from this proposition. First, Wilde draws what he calls a cross-sectional deduction referring to the behaviour of drivers passing through different road sections which are marked by dissimilar spatial accident rates. This deduction is stated as:

$$km/h=R/[A/(n.km)]$$

The term *km/h* is said to refer to the average space-mean speeds in each section. The term *A/(n.km)* refers to accident loss per km of mobility over each road section travelled by *n* drivers and passengers. Wilde goes on to say under this deduction:

'Thus, to support the notion that the accident rate is constant per time unit of driver exposure, drivers should be seen to choose an average speed that is commensurately lower in those road sections where the aggregate accident loss per km of mobility is relatively high, and vice versa [...]' (p448).

This passage is interesting because in two critical ways it seems to contradict earlier assertions. First, Wilde, not for the first time, uses the term accident *rate* as opposed to accident *loss*. Indeed, by this stage of the paper Wilde seems to be using the terms interchangably, as if they were symonymous. Yet, under Proposition 1 (page 442), Wilde defines accident loss as '[...] *the sum total of the cross-products of the frequency of accidents and their costs* [...]' Under Proposition 15 too (the proposition, that is, from which the deduction is drawn) the term *A* is said to refer to accident loss. The distinction, one hardly need say, is critical. Accident *rate* is just half of the determinant of accident *loss*, as Wilde has defined it. Whereas accident *rates* are objective, measurable phenomena, accident *loss* is not. Since Wilde makes no RHT predictions in terms of accident rates, they would not appear to enjoy any role, except in partly determining accident loss. His shift to this term is unexplained.

A second point too can be seen from the above extract. Here, for the first time, Wilde specifies a behaviour that is involved in the compensation process - namely, km/h, or mean speed. Whilst this is useful in that it would appear to be of help in either supporting or refuting the theory, it would also appear to be in conflict with this earlier statement that the homeostatic process:

'[...] *may take a variety of forms: changes in the frequency of driving, the choice of* *transportation mode, the amount of road use at night or under adverse weather conditions, the amount of mental capacity devoted to the driving task, following distance, driving speed, and so on. However, RHT does not specify in what particular behavoural form this 'equifinal' (Bertalanffy 1968) compensatory adjustment will take place (see the general deductions derived from Proposition 5), but instead that it will take place and that the accident loss per time unit of road-user exposure or per capita (at least in the long run) will not be modified beyond the threshold of the just-noticeable difference in accident loss.' (p446).*

Here Wilde says quite clearly that RHT does not specify, as it were, its route, its behavioural pathway. Wilde seems to be quite clear too in saying that RHT is only about the consequences of intrinsic safety changes in terms of *accident loss*. On how that accident loss is achieved, RHT (as far, at least, as Propostion 11 is concerned) is silent.

Finally a longitudinal deduction follows from Proposition 15: taking the same jurisdiction, but making time-series comparisons of *per capita* spatial mobility across different years, Wilde suggests:

$$KM/N=(R.h)/(A/KM)$$

KM refers to total road-traffic mobility resulting from the entire population with N individuals in a given year. In other words, the average distance travelled by an individual in this given jurisdiction is determined by the product of target risk and mean travelling hours, divided by accident loss divided by total road traffic mobility.

Stated above then are the fifteen propositions of RHT and Wilde's deductions from them. These propositions and the discussion arising from them will be referrred to in this thesis in predicting experimental outcomes.

On the critical side, it can be seen that at times the discussion lacks clarity. Specifically, Wilde should answer two questions : what is the place in RHT for accident rate? Is this statistic important only in that it partly determines accident loss? Or is it that predictions can be derived from RHT that are testable in terms of accident rates alone, assuming that severity can be in some way held constant? Since the term 'accident loss' is a difficult one to measure, a shift in emphasis to a more objective variable would be to many a welcome one. Second, does RHT predict that speed carries the homeostasis effect or not? On this issue Propositions 11 and 15 seem somewhat at odds.

Other inconsistencies too can be seen. In Proposition 11 Wilde says that non-motivational interventions that are successful in changing the target behaviours to which they are addressed, will "lead road users to display other [...] risky behaviours *in road traffic* at a higher frequency." (my italics). Under Proposition 15 he seems to suggest that predictions regarding traffic speed can be derived from changes to target levels of risk. Both of these Propositions would seem to suggest (a) that the negation of intrinsic risk benefits occurs within the environment, and (b) that the specific pathway for this negation is a speed-related one. Yet, under Proposition 2, Wilde suggests that the meeting of target with actual risk need not be brought about through such specific behavioural adjustments within the environment, but may just as well be achieved through changing the mode of transport, or through avoidance of the journey (staying at home). Since Wilde's use of the term 'specific behaviours in traffic' can hardly be meant to include changing mode of transport or journey avoidance, it would seem that Wilde has in mind that in some circumstances behavioural adjustments within the risk-taking environment can be predicted, and in other circumstances they represent only one of three possible outcomes. Quite when the one outcome rather than the other two can be predicted is not made explicit by Wilde, and as the paper stands, Propositions 2, 11 and 15 would seem to be in some conflict.

2.2. Evans (1985) and the theory of risk homeostasis in context

Evans (1985) attempts to put RHT into context by describing it as just one model of driver behaviour with two competitors. These three models he identifies as:

* Engineering
* Economic
* Homeostasis

Without doubt, the engineering model (also known as non-interactive) it qualitatively quite different from economic and homeostatic models. It assumes that behaviour is a function only of the engineering intervention in question. For example, if it were possible (through bio-mechanical and mathematical models) to calculate that a new front bumper should bring about a 20% reduction in fatalities, then this 20% reduction should be seen. Its label of 'non-interactive' comes from the assumption that whatever improvements in environmental safety are brought about, the behaviour of drivers remains the same: it could almost be said to be an "anti-human factors" approach.

One might perhaps say that to label this the engineering approach is slightly misleading, since changes to environmental risk can occur through interventions that have no engineered origin. For example, some changes in the weather affect environmental safety, but they are in no sense engineered. It can be seen from Evans's later formal description of this approach that in all cases where the level of environmental risk changes, a corresponding change in accident loss would be predicted.

Under the term 'economic' models Evans lists the contributions of Adams (1982, 1983) Blomquist (1983), Blomquist and Pelzman (1981), Crandall and Graham (1984), Graham (1984), Graham and Graber (1983, 1984), O'Neill (1977), Orr (1982, 1984) and Pelzman (1985). This perspective sees intrinsic safety as an economic good, like any other, and therefore subject to exchange. When an environment becomes safer a compensation that may or may not be complete could therefore follow depending on the utility of the various behaviour alternatives.

Homeostasis (Wilde, 1982a, 1982b) is the third model identified by Evans. This involves the notion of users maintaining the same level of risk independent of physical or engineering changes in a way analogous to a thermal control system.

One could say that Evans's identification of these three models is slightly misleading, since:

1. Wilde's conceptualisation of RHT makes use of the concept of utility in the same logically necessary way that the so-called economic model does.

2. Risk homeostasis theory allows for the possibility of a less than complete compensation process in exactly the same way that the economic model does. For example, where the associated utility of the change in behaviour is weak, or where a motivational change is introduced at the same time as the change in intrinsic risk, an incomplete compensation would be predicted by RHT.

3. Although Wilde cites the work of O'Neill, and O'Neill cites the work of Wilde, the references are made in mutual support: adherents to Risk Homeostasis Theory do not take issue with other utility-driven models, but rather, tend to confine their disagreements to those existing with the engineering approach.

One might therefore argue that the utility models and RHT can only be separated with diffiulty. Operationally, Evans suggests that the differences between the three theories could be formally described as:

$$\Delta S_{ACT} = (1 + f)\, \Delta S_{ENG}$$

where f characterizes the feedback or interaction within the system. S_{ACT} is actual safety and S_{ENG} is the safety level that would be estimated from the engineering approach, assuming non-interaction. This would lead the three identified models to make predictions as follows:

Engineering:	$f=0$
Economic:	$-1 < f < 0$
Risk homeostasis	$f=-1$

One would therefore like to qualify the risk homeostasis predictions by saying that the f=-1 function would only apply where the relevant utilities were commensurate with full homeostasis and where the intervention is not accompanied by a motivational change. With the same qualifications added, it is hard to see how the economic model would behave any differently.

What Evans does is to fit results from the literature into his model in relation to both increases and decreases in intrinsic safety levels. Increases in intrinsic safety levels are defined as $\Delta S_{ENG}>0$, with decreases defined as $\Delta S_{ENG}<0$. Twenty-six results relevant to the effectiveness of changes in intrinsic safety are then examined. It is suggested that for the $\Delta S_{ENG}>0$ (intrinsic safety increase) results the findings fall into five categories:

1. Safety increased even more than was expected
2. Safety increased as expected
3. Safety increased, but less than expected
4. No safety increase was found *
5. Perverse effect - change led to safety decrease.

For the results following $\Delta S_{ENG}<0$ findings fell into three categories:

1. Perverse effect - the change actually increased safety
2. No safety reduction *
3. Safety reduced, but less than expected

By 'safety' Evans, of course, refers to operational safety levels as reflected in accident loss. Category 4 from $\Delta S_{ENG}>0$ and category 2 from $\Delta S_{ENG}<0$ (marked *) are the predicted outcomes from risk homeostasis theory.

2.2.1. Criticisms of the Evans Paper

Whilst the Evans paper represents a theoretical analysis of accident loss as a function of ΔS_{ENG}, having reported the 26 findings there is no real theoretical analysis of how one intervention leads to one outcome, and another very similar intervention leads to another. For example, painted pedestrian crosswalks are reported to have led to increases in accident loss (Herms, 1972) through, it seems, pedestrians feeling safer than they really were. Yet with the British case of zebra crossings, Howarth and Gunn (1972) report the opposite effect: the drop in accident loss, with an accompanied migration of accident loss to the surrounding road away from the zebra crossing. How did the one intervention lead to an increase, and the other to a decrease, in accident loss, when they were essentially the same intervention? Evans's theoretical contribution seems to amount to the proposition that *anything can happen.* In addition, Evans cites the 26 studies very much on face value and without much consideration of possible methodological flaws:

"In our examination of results in the literature we generally use the quoted author's claim of effectiveness, even though alternative analyses of the same data might lead to different conclusions." p558.

Many of the findings Evans reports are indeed open to alternative interpretation.

Finally, as Evans himself acknowledges, the question of variation over time receives little attention. Yet RHT proposes that it is only in the long term that one would expect to see homeostasis (in the sense of complete negation). As Evans puts it:

"Based only on the evidence presented here, the risk homeostasis approach (Wilde, 1982), which we have characterized by f=-1, could, in principle, be universally correct in the long term. That is, the users could have a desired level of risk taking, and once all the information was finally received and absorbed, the previous risk-taking level could be reestablished." P573

2.3. Conclusions

Wilde has set out a theoretical description of human behaviour in response to changes in intrinsic risk. This model assumes that individual risk-takers are characterised by a target level of risk, and that through a population-level closed loop, risk-takers attempt to match actual risk to target risk. This matching process may or may not be achieved by behavioural adjustments within the risk-taking environment. The concept of utility is important in the theory Wilde sets out in determining where the target level of risk should be set for any given individual.

RHT is just one of a number of theories that attempt to explain behaviour in response to changes in intrinsic risk. The engineering model proposes that changes in intrinsic risk are matched by commensurate changes in accident loss. The economic model is suggested by Evans (1985) to be distinct from RHT, but although economic theories differ in their formulation and detail, much the same predictions would be made by them and by risk homeostasis theory.

Chapter 3 - Simulation and RHT

3.1 Simulation in psychological investigation.

3.1.1 General Background.

The use of simulated exercises in social science research is widespread. Its contribution can be split into several categories.

1. Training. Stammers and Patrick (1975) provide arguments for the use of simulation in training people to do jobs. The advantages here are mostly obvious: simulation can be cheaper; it can allow 'collapsed experience' (allowing a simulated environment to present in the short term what a real environment would present only in the long term, if at all); where physical risk is involved in the real environment, it can be removed from the simulated one (such as would be the case in training pilots); feedback of errors can be more carefully managed. In the field of training simulation is relatively easy to evaluate. Training has as its object the acquisition of skills, and if this object can be shown to be facilitated through simulated exercises, then its utility is demonstrated. An interesting finding to come out of the research in simulation in training (possible extensions of which will be examined in this thesis) is that low-fidelity simulation can in some cases be as effective as high fidelity simulation (see Stammers and Patrick for a full discussion).

2. Simulation as modelling. Some writers use the term 'simulation' to refer to a modelling process. For example Hovland (1962) discusses the advantages of simulating international relations in the hope of determining what actions would bring about what responses in a potential nuclear conflict. By this Hovland is in essence referring to a model of the situation with parameters taken partly from first principles and partly from imperfect empirical evidence. Further examples of modelling (or '*simulating the user*')

are provided by Colbourn and Cockerton-Turner (1989) who simulated peer interaction, Cheepen, Hewitt, Hunter and Monaghan (1989), and Cacciabue, Decortis, Mancini, Masson and Nordvik (1989).

3. Simulation as a tool for investigation. Simulation can be used, as it is in the experiments reported in this thesis, as a tool for the investigation of the phenonena the simulation represents. For example, if we are interested in driving behaviour, we can set up a driving simulator in the hope that the findings from the simulator can be generalised to real driving behaviour.

The first of the three uses can be evaluated empirically: does the simulation aid training on specific skill acquisition? Usually this can be answered in terms of positive and negative transfer of skills. In the second, model building, applications can be evaluated by bringing the model back to the situation it set out to explain, or, since the approach often amounts to a logical interpretation of findings, the conclusions can be challenged on the same logical grounds on which it was built. But the third application for simulation, the drawing of inferences about human behaviour, is much more difficult to evaluate.

Having set up a simulated environment and reported our findings, how then can we answer the question of whether these findings can be extended to the situation they were set up to explain? Chapanis (1967) attempted to answer this very difficult question. Six years earlier Chapanis had said that every laboratory experiment is a model of the real world - an incomplete model and therefore "*always wrong*". In 1967 he moved slightly away from this position in saying that laboratory experiments "*always fail to give us exact solutions to real-world problems*" (p561). Three of Chapanis's objections to the laboratory experiment are particularly relevant to the issue of simulation, and so deserve special consideration here.

1. The findings of laboratory-based experiments lead to predictions that, in the real world, are not always met. Chapanis gives two impressive examples of this, both of which come from research in vigilance - a practical problem to which, one would expect,

a controlled, laboratory-based experiment could make a substantial contribution. The first comes from Elliott (1960):

"Whenever we have studied real watchkeeping tasks, we have achieved some results which could not have been predicted from published test data and, indeed, we have often found the published material quite misleading." (p357)

The second example, again from vigilance research, comes from Jerison and Pickett (1963):

"Although vigilance research received its major impetus from practical problems of sustained visual monitoring of radar displays, it does not, at this time, contribute in clear and unequivocal ways to the solution of similar problems in manned space systems or, for that matter, in any systems that are planned for field operation" (p235)

Empirically then, if laboratory-based simulations set out to explain *'real'* environments, they do not always appear to achieve what they set out to achieve.

2. Independent variables are inappropriately selected. Chapanis explains this by saying that, in the real world, there is a near-infinite number of independent variables with complex interactions between them. Yet in the laboratory, we tend to deal with very few independent variables (IVs) - typically between one and three. This leaves us with the question of the relative importance of those IVs we selected: their importance, that is, relative to those potential IVs we did not select. Chapanis cites an example from his own research. Chapanis and Lockhead (1965) were interested in the effect of sensor lines on performance. These are physical lines that appear between a control and its corresponding display. They found that by giving a cooker sensor lines between hob and controls, performance (reaction time) increased markedly. What inferences, asks Chapanis, can be drawn from this finding? Can we say, he asks, that by putting sensor lines on cookers everywhere, we will be successful in increasing performance figures? Before answering this he reports a second experiment in which the same procedure of

adding sensor lines is repeated for a cooker with very good compatibility (he adds at this point that the first cooker had very poor compatibility). In this second experiment, performance (reaction time) actually increased with the addition of sensor lines. The point of this is that by taking from the near infinite number of independent variables the very narrow experimental design that was produced, results can be misleading. Sensor lines may bring performance up or down; which of these happens depends on how the investigation takes place.

3. Dependent variables too are often selected for the convenience of the experimenter rather than for the extent to which they reflect the 'natural' changes of interest. As Chapanis points out, some dependent measures lend themselves to measuring better than others. (The number of errors participants make and their task completion time are examples of dependent variables that can easily be measured.)

3.1.2 Simulation of the driving process

For those researchers concerned with the driving process, simulation has long been a possibility (see Allen, Hogue, Stein, Aponso and Rosenthal, 1990; Blaauw, 1979; Dorn, 1992; O'Hanlon, 1977; Watts and Quimby, 1979; and Wierwille and Fung, 1975). Early attempts at simulating the driving experience were in fact 'open-loop' simulators. This means that participants' responses did not influence the representation depicted. Increasingly, however, with improvements in hardware and computational speed, the possibility of simulation in a closed-loop system (one in which participants can influence what happens in the situation depicted in much the same way as a real driving environment) is both a real and a practical alternative to field studies. Analogue-to-digital conversion allows the user's input device to take the form of conventional pedal control and a conventional steering wheel; microcomputer technology allows auditory feedback, simulating engine noise; it also controls the simulation and logs performance measures.

3.1.3 Validating driving simulators:

A number of attempts have been made to validate the various forms of driving simulation. Since it could be argued that the acid test of such a device can be stated in terms of learning transfer, it is encouraging to learn that particpants who have used a driving simulator are, it seems, more likely to pass a test of their driving than particpants who have had no simulated experience (see Bishop, 1967; Baron and Willeges, 1975). It is of course important to note that this perhaps says more about what common skills are shared between the simulated and real environment than how practical it would be to train inexperienced drivers using a driving simulator.

Edwards, Hahn and Fleishman (1977) carried out a validation study in which taxi drivers were recorded across 14 categories of driver error. On another occasion the same drivers were invited to take part in a study using an open loop simulation. Would the taxi drivers, asked Edwards, commit the same errors in the simulator as they did on the road? The answer, unfortunately, was an unequivocal no. However, two methodological concerns need mentioning here. First, as Edwards concedes, the low correlations may suggest that the measurements of on-road driver errors were inadequately recorded. Second, since any correlation is ultimately constrained by the reliability of the correlated variables, the non-significant correlations may be a consequence of the low reliability of the measurement of on-road driver errors.

Watts and Quimby (1979), in their validation study of The Transport and Road Research Laboratory Simulation had more success. They found that participants' risk assessments of a real driving exercise and a simulated exercise were significantly correlated. They found too that participants were characterised by a similar emotional response across the real and simulated environments, and they found also that in response to questionnaire items the simulator was rated favourably in terms of its realism.

Dorn (1992) carried out a more detailed questionnaire analysis than Watts and Quimby did. She computed correlations between risk assessment in a real environment and those of the Aston Driving Simulator (ADS). Her finding with a sample of 70 drivers was a correlation of .34 ($p<.01$). There are, however, several difficulties with interpreting this finding. First, as Dorn acknowledges, there is a distinction between perception and behaviour. Participants may have experienced similar perceptions across the two environments, but if they went on to treat the simulated environment trivially, their perceptions become less relevant. Second, the correlation, whilst being significant, may be considered not to be particularly high. Finally, the reliability coefficients of the risk assessments are not reported and hence, just as in the case of Edwards *et al.* (1977), the findings may reflect criterion reliability problems.

3.1.4 Limitations of the Aston Driving Simulator

The technology that enabled the ADS to be developed has its limitations. A major technical problem with it is the number of pixels that are generated from the Acorn Archimedes microcomputer. At high speeds, if any sort of realistic distance perspective is to be achieved, there is a relatively short time scale from seeing an oncoming vehicle to the point at which that vehicle becomes 'parallel' to the ADS. Effectively what this means is that on an open road, participants can pull out to overtake another vehicle when the road ahead is apparently clear, only to find themselves with very little time in which to take avoiding action. How realistic, therefore, is overtaking on the ADS? Dorn (1992) answers this question through her questionnaire study. No fewer than 70% of participants rated fidelity of the overtaking feature of the simulator as either 'low' or 'very low'. Only 8% of participants rated it as either 'high' or 'very high', whilst 22% of participants rated its realism as 'fair'.

3.1.5 Alternatives to the simulation of road traffic

It is worth considering that whilst there remain technical reasons why the simulation of a driving exercise cannot easily be brought within closed-loop control, simulation of physical risk can take place modelling slower-moving environments in which pixel number and screen re-generation rate do not present a problem. In other words, physical risk may be modelled in environments in which computer-generated feedback is slower. Possibilities here include the simulation of an air-traffic control situation (Ball and Jackson, 1988; David, 1969, 1992; Hopkin, 1963, 1965; Hoyes, 1990; Narborough-Hall and Hopkin, 1988), a nuclear power plant control room (Marshall and Baker, 1985; Page, 1983; Stanton, 1992; Whitfield, 1987), and of shipping exercises and shipping manoeuvres (Broome and Marshall, 1980; Cannell 1980). These simulations differ in respect of both their overall physical fidelity and whether they are open- or closed-loop simulations. Whilst the nuclear power plant control room simulation described by Stanton (1992), and used in this thesis, can be described as closed- loop, its physical fidelity is certainly low. David (1969) and Hopkin (1963, 1965) have investigated air traffic control behaviour with both closed-loop simulation, in which the ATC environment is dynamic, and open-loop simulation, in which a static ATC problem is presented to participants (ATC officers) were asked how they would proceed to resolve the problem. Again, it is possible for a simulation to be closed-loop, yet to lack physical fidelity. This point is illustrated well by David, 1969 (cited by Whitfield and Stammers, 1978) who reports a study in which aircraft speeds were over 10,000 knots.

Unfortunately these approaches too may not be without their drawbacks. With simulated driving exercises, the only constraint in recruiting participants is that they are able to drive. Other simulated environments may require more specialised users. In order to simulate effectively some shipping manoeuvres, Cannell found it necessary to require participants to be experienced ship masters. This led to one study involving an *N* of just three participants and a complete absence of any inferential statistics (Cannell, 1980). Hoyes (1990) attempted to overcome this prerequisite skill problem by pre-training his participants in the skills required, but found here that the level of training was such that it became almost prohibitively time-consuming. It too led to a small number of participants (several studies involved an N of 6, and consequently many of the non-significant findings that Hoyes reports may be explained by type II error).

3.2 - The use of simulation in RHT research

3.2.1. Introduction

Risk homeostasis theory is currently being investigated by members of the Human Factors Research Unit (HFRU) at Aston University through the use of simulated physical risk-taking environments. Since this approach to the investigation of risk homeostasis is (so far as an extensive literature search was able to reveal) unprecedented, the purpose of this thesis must be (i) to set out what appear to be its methodological advantages; and (ii) to defend it against what might be seen by some as its shortcomings. Given first are some advantages.

3.2.2. Operationalising the factor of utility

Wilde (1982, 1988) is quite clear about the role of utility in RHT: homeostasis is only to be hypothesised in cases where a shift in risk-taking behaviour can be expected to bring about some benefit. Other models that point to the same compensatory mechanisms also hold that utility is logically necessary for compensation to occur (eg O'Neill, 1977). Indeed, one could almost go so far as to suggest that in so far as risk homeostasis theory is a theory at all, it is one that rests entirely on the concept of utility. When evidence is put forward supporting RHT, its proponents provide their explanations in terms of this factor. When evidence appears to contradict the theory, again utility is used to explain why, in that particular case, the relative utilities involved did not occasion behavioural adjustments.

Unfortunately for a factor so central to the explanation of risk homeostasis, the central role that utility is said to enjoy has the status only of an assumption, existing to date without anything in the way of empirical support. Accident loss statistics by themselves can obviously not allow one to determine whether, for example, a perceived benefit caused the accident to happen. Whether the individual having suffered an automobile accident would have behaved differently in different conditions of environmental risk cannot be determined. Whether the individual placed any utility on the behaviour changes also cannot be answered. Utility, for those individuals who have already suffered an accident, cannot, in short, be measured.

When RHT is investigated in a simulated environment the role of utility can be investigated through operationalising it as a factor in an experimental design. Two examples illustrate this method:

Hoyes (1990) investigated RHT in a simulated air-traffic control environment using a microcomputer. Intrinsic risk was operationalised on three levels using a computerised conflict avoidance system (CCAS). This device was said to intervene to prevent collisions by over-riding the operator's instructions in cases where those instructions by themselves could be expected to result in collision. The first of these levels served as a control in which participants were given no information regarding its use. The second level was known as CCAS10. On this level participants were told that the CCAS would intervene to avoid collisions on about 10% of occasions. The third level was known as CCAS90, on which participants were informed that 90% of errors would be corrected by the computer. In this way the relationship between a quantifiable change in risk and behavioural adjustments was investigated.

Utility was operationalised on two levels of mental workload: high and low. It was suggested that the monitoring of aircraft on the low mental workload condition required attentional resources of such a low order of magnitude that there was no benefit to be had from behavioural compensation in response to the change in intrinsic safety. On the high mental workload condition however it was assumed that the much greater levels of attention required meant that participants now did have a distinct utility attached to behavioural compensation. The analysis of results could take place on this two-factor design with the logic that if utility really is necessary for behavioural adjustments to be made then the factors should act in a multiplicative way to bring about a statistical interaction between intrinsic risk and utility. Only mixed support for this interaction was found.

Experimental designs that involve the simulation of physical risk can then also allow an investigation of the utility criterion. What is more important, although only two experiments to date have tested utility in this way (see Hoyes, 1990), the evidence from these does little to support the assertion that utility is logically necessary for risk homeostasis to occur. One hardly need say that if utility were to be shown to be anything other than central to risk homeostasis, RHT would require considerable re-writing.

3.2.3. Repeated-measures designs and psychological correlates

A second advantage to the employment of simulators in experimental work is that they hold open the possiblity of correlating individual homeostasis results with psychological variables. For example, in a simple one-factor design with two levels of intrinsic risk, where participants are run under each level measures can be taken, not only of the absolute value of, say, speed or accelerator use, but also of the value of speed on one condition of intrinsic risk *relative to another*.

Suppose on such a design that significant differences existed between speed, and suppose that participant 1 recorded a mean speed of 55 mph on one level of risk and 70 mph on the second, less-risky level. This difference of 15 mph can now be correlated against a variety of psychological measures. This leaves open the possibiltiy of identifying an RHT 'type'. Since extraverts are known to have more accidents than introverts (Fine, 1963; Mackay, DeFoneka, Blair and Clayton, 1969; Shaw and Sichel, 1971), and since there is some evidence that aggression pre-disposes individuals to accidents (see Suchman, 1965 and Wrogg, 1961) an interesting line of enquiry might be to investigate whether those personality types that tend to have more accidents than others tend also to compensate more for changes in environmental risk levels. Since the theory of risk homeostasis involves assumptions of quite sophisticated information processing skills on the part of risk-taking populations, the question might also be asked of whether 'homeostats' differ from the rest of the population with respect to cognitive skills. In connection with this it is worth recalling Wilde's (1988) speculation that RHT occurs at a pre-attentive level. Is it the case that individuals knowingly compensate for changes in the safety of their environments, or are they blissfully unaware of their adjustments? An RHT awareness questionnaire is being developed by members of the HFRU to examine this question.

The employment of the repeated measures design holds other implications besides correlational ones. So far risk homeostasis has been investigated only at a population level. The question must therefore remain of individual differences in risk homeostasis: what proportion of people account, one might ask, for the effect? Is a perfect homeostasis operation carried out by all of us, or do most of us not compensate for changes to our environmental safety, leaving relatively few individuals to over-compensate? A repeated measures design would allow questions like this to be examined. It would allow us to gauge, for perhaps the first time, the absolute size of any effect for any given sample.

3.2.4. The analysis of behavioural pathways

So far as the consequences of changes in intrinsic safety go, RHT is postulated purely in terms of accident loss; it has nothing to say about *how* possible changes will be compensated for. One could of course put forward many behavioural candidates for the job of carrying the homeostasis. These candidates would not, moreover, be mutually exclusive. Homeostasis may, for example, be carried by making intrinsically riskier overtaking manoeuvres. It may be achieved simply by driving faster, or by driving with less attention. It may be brought about by a combination of all of these behaviours and more besides. Although some researchers in RHT have concerned themselves with the question of the behavioural pathways that carry the effect, RHT has, for the most part, been investigated in the terms set out in the theory, in terms, that is, of accident loss. The important assumption here made by Wilde, is that the homeostasis will be achieved come what may: if one pathway to homeostasis were to be blocked, then the homeostasis would manifest itself somewhere else. The clear implication is that the identification of behavioural pathways to homeostasis is a trivial exercise.

But should investigation of RHT be confined to accident loss, rather than the pathways to this loss? At least two arguments exist in support of the analysis of pathways. First, the assumption that pathway identification is trivial, since the blocking of one pathway will result in the opening up of another, has the status only of conjecture; it certainly exists without anything in the way of empirical support. The question then must be: if pathways can be identified and then blocked, will the effect of homeostasis (assuming it existed in the first place) be 'killed', or will some alternative pathway open up to carry the effect? Supplementary to this is the question of whether there is a finite number of pathways to the effect, which, when blocked, would kill the effect. Second, the mere fact that RHT is silent on the issue of behavioural pathways is not by itself a reason not to examine them. It may well be that pathway identification would lead to a greater theoretical understanding of how the mechanism of homeostasis works. The nature of the identified pathways might, for example, be reconcilable with the concept of utility. It may not be reconcilable with the notion that risk-takers are making compensation judgements at relatively high cognitive levels of processing. It is difficult to see how theoretical insights of this nature could be achieved without some analysis of the particular behavioural pathways to the effect.

A further theoretical question which has not so far been examined is whether homeostasic behaviours are local or general. This issue is best illustrated by an example. Suppose an environment is made intrinsically safer by increasing a given car's acceleration. In such an event less time would be required when overtaking another vehicle and if the same target level of risk were to be maintained, then this could be achieved through adjusting the time margin required to overtake other vehicles. If this specific adjustment were to take place in the absence of other changes, then the homeostasis could be said to be local to the change in intrinsic safety. But the maintenance of a given target level of risk could be achieved in other, more general ways. It would be possible to leave local behaviours unadjusted and 'make up' for this improvement in other ways, say, by driving faster or with less attention. Such adjustments might mean that whilst overtaking manoeuvres become operationally safer, other behaviours become operationally riskier, and the same overall operational level of risk is maintained. Quite why risk-takers might favour general rather than local strategies is not clear, but it should be remembered that the theory is said to operate at a pre-attentive level, perhaps leading to a more general maintenance of risk.

If there is some basis for examining RHT through specific behaviours, a simulated environment has obvious and immediate advantages. The Aston Driving Simulator, for example, allows over twenty specific behaviours to be constantly monitored. Research using the ADS has focussed on a variety of speed- and attention-related behaviours. On speed, as well as allowing means to be calculated, maxima and minima over the experimental sessions have been of interest (in some cases maximum speed showing an effect of intrinsic risk where the less-sensitive measure of mean speed escaped the effect, at least so far as statistical significance went). Attentional changes may be monitored through two measures. The first of these is position-on-road relative to the centre white line on the right, and the kerb on the left. This could be seen as, in essence, a tracking task. Thus, the measure may allow the inference of changes in attention to be inferred. The second measure is an auditory secondary task, tape recorded for subsequent analysis. The secondary task employed in previous research (Hoyes, 1990) involves the alternate articulation of letters of the alphabet and corresponding numbers (A,1,B,2,C,3, etc.) This task has shown itself to be particularly sensitive to changes in intrinsic safety. In as much as attentional changes on a primary task may be inferred from differential performance on a secondary task, the early conclusion to this work would appear to be that at least one behavioural pathway to homeostasis is attention-related: when an environment holds less intrinsic risk, we tend to reduce our levels of concentration. Such a finding can only come through the sort of detailed pathway analysis that can only take place in a relatively sophisticated simulated environment.

3.2.5. *"...But it isn't real..."* : an argument against simulation

One argument that is very often put to members of the HFRU group is that *People don't* *behave the same in simulation exercises.* It is said that participants in studies that make use of the Aston Driving Simulator drive faster, more carelessly, and with much less regard for other road-users than they would when driving a real car. All of this results in an 'accident rate' that serves only to confirm the fact that participants knew very well that they were not exposed to any real, physical risk.

Whilst none of this could be disputed, it is far from certain that it need matter very much from the point of view of investigating risk homeostasis theory. One could argue that the only assumption that is needed if simulations are to parallel 'real' situations is that participants be characterised by a target and actual level of risk. If, as seems likely, the target level of risk for participants in simulated environments is very much higher than it would be if real physical risk were involved, this by itself need not matter. All that does matter is that a target exists to be maintained. In as much as participants clearly try to avoid accidents when using the Aston Driving Simulator, there is no reason to suppose that they were not characterised by individual target levels of risk.

3.2.6. The use of simulation in this thesis

This thesis seeks to examine RHT in three simulated environments. In the order in which they appear, they are: the Aston Driving Simulator (ADS), the nuclear power plant control room simulator (NPPCRS), and the low fidelity driving simulator. A few words is needed on each.

The ADS has already been referred to as a research tool in the general discussion on simulation. It was developed by members of the Human Factors Research Unit at Aston University specifically to examine sex and age differences in driver behaviour. It is a closed-loop driving simulator in the sense that the operator (driver) can influence the simulation through his or her actions. The simulator is modelled on an automatic car (the speed and acceleration of the simulator make it roughly analogous with a Jaguar 4.2); the controls consist of an accelerator pedal, a brake pedal, and a steering wheel. The road representation is presented on a motitor screen in front of the driver (no side or rear views are provided). The simulation is driven by an Acorn Archimedes with analogue to digital conversion of the three controls. Display information is given to the participant, but is restricted to an analogue speedometer, a clock, and a graphical fuel guage.

The simulated driving experience is based on a fairly busy single carriageway 'A' road with cars that may be overtaken (the speed of which is variable by the experimenter) and on-coming cars. The simulation covers the event of collisions, both with other vehicles and with the kerb.

The ADS now allows the experimental designs to be menu-driven by the experimenter. The experimenter designs experiments by defining number of runs, maximum user speed, length of each run, other traffic speed and speed variance, and whether British or Continental conditions hold (driving on the left or right hand side of the road). A range of driver behaviours (described in study 1) are recorded by the simulator every half second. This results often in an experiment with literally millions of data points, and so some simplification of the data is required. This can be achieved by 'blocking' the runs - taking an average of the measures, say for every minute, and thus compressing every 120 data points into just one. Once data have been assembled into blocks, the blocks can then be used as a repeated measures factor. The difficulty here is that some blocks are inherently different from others, for example, the first block always involves moving from rest and often incorporates a sharp bend. Therefore to compare one block with another can be misleading. Although the effects of blocks are reported in this thesis, they are provided only for completeness and in order to look at interactions between it and other factors. Simplification can also be achieved by taking factored scores of the behaviours. It was felt that a simulation of the driving process should be included, since RHT is postulated primarily in terms of driver behaviour.

The NPPCRS is in essence an alarm handling task. Again it is based on a micro computer - a Macintosh IIsi, and again it was developed by members of the Human Factors Research Unit at Aston University. It is intended to mimic a control room environment, and the messages that are handled were taken from an observational study of a real control room (Stanton, 1992). In fact, the actual alarm messages were taken from a coal-fired station, not a nuclear one, but that was felt not to detract substantially from those aspects of the fidelity of the simulation relevant to the topic under investigation. Stanton argues that this gives the simulation ecological validity. The speed at which the operator is asked to handle alarms is variable, as is the proportion of target alarms to non-target alarms. Unlike the ADS, neither of these experimental variables is menu-driven.

In common with the ADS, the NPPCRS was not developed as a research tool for the investigation of RHT hypotheses. It was developed rather to examine the effect of presentation rates on user errors. A number of specific measures are provided by the simulation, described fully in study 2. When this simulator was being designed, a modification was made to allow the experimenter to record responses to questions relating to the likelihood that the user would remain in the environment at different salary levels. This was to test the 'avoidance' pathway - removing oneself from an environment in order to adjust actual risk to target risk. This avoidance pathway was not measured on the ADS. The NPPCRS was used since in order to assess whether a homeostatic process operates outside of driver behaviour in other conditions of environmental risk. It was included also, since it enabled conditions of very high mental workload to be examined. In such conditions, behavioural adjustments within the risk-taking environment of the magnitude needed to restore previously existing levels of environmental risk are not always possible. Behaviour in these circumstances was of interest (would any homeostatic tendencies manifest themselves through extrinsic compensation, through, that is, notional avoidance, or would the compensation process simply not be evident?). Behavioural pathways within the NPPCRS environment were passive and active errors.

The low fidelity simulator was included to see whether static conditions can also be successful in testing RHT hypotheses. Its reason for inclusion in this thesis was that if low fidelity conditions can be successful in examining RHT, a range of physical risk-taking environments could be developed at low cost, anwering ostensibly the same questions as those currently under investigation in relatively high fielity environments.

The low fidelity simulator could therefore be described as 'open loop'. The behavioural pathways of these simulators (two different versions were developed) were broadly those of the Aston Driving Simulator.

3.2.7. Conclusions

So long as a target level of risk is maintained, there seems good reason to suppose that simulation may be a valid way of examining RHT hypotheses. Evidence suggests that the absolute values of behaviours such as speed, steering control and number of other-vehicle collisions cannot be generalised from a simulated environment to a real one. However, so far as the testing of RHT hypotheses is concerned, these differences are not relevant.

In adopting a simulated environment in preference to the *post hoc* analysis of official accident data and the quasi-experimental approach (the 'before and after' design) advantages are seen that can aid the understanding of possible RH mechanisms, and what is more, can do so in psychological terms. So long as the role of utility remains central to RHT, it is difficult to see how controlled experiments can be avoided. In bringing physical risk-taking into the laboratory it is anticipated that the further understanding of the RH mechanisms that will inevitably be gained can be brought back to those environments they set out to explain. The simulation of physical risk has therefore a positive contribution to make to the RHT debate. This contribution has not so far been fully acknowledged and its development forms one of the two aims of this thesis.

Chapter 4 - Summary of Part 1.

As a model of risk taking in the dynamic of environmental safety, RHT offers an explanatory framework at the level of motivation rather than the environmental safety level. In doing so it would appear to contradict the logic of making improvements to environmental safety such as the compulsory wearing of safety belts, although close attention should be paid to the denominator of accident loss before concluding that the theory is pessimistic (see Hoyes and Glendon, 1992 and Wilde, 1988). In addition, in as much as RHT suggests that improvements in accident loss can be brought about through motivational interventions, it may reasonably be described as an optimistic theory.

RHT can provide a model to explain the behaviour of road users in specific circumstances such as the compulsory wearing of safety belts and, in the case of motorcycles, crash helmets. However, rather than achieving any sort of consensus here, the accident loss data have become something of a battleground for those who put forward and those who oppose the theory of risk homeostasis.

Unfortunately it is difficult to attribute the status of theory to risk homeostasis so long as it fails to hold open the possibility of falsification. Since no finding can be envisaged that would serve to falsify RHT, the theory does not predict one outcome whilst at the same time excluding another. It follows that if RHT is to enjoy fully the status of theory, its proponents should spell out in clear terms what would constitute a falsification of it.

The opponents of RHT also face conceptual problems. The link between RHT and the theory of utility is a strong one, and indeed some would say that RHT is no more than an extension of utility theory. In opposing RHT then, are its opponents rejecting utility theory also? If they are, what is to be its replacement? If they do not oppose utility theory *per se*, why do they claim that it fails to operate in a dynamic of environmental risk? Until these questions are answered, the position of the opponents of RHT too must be a state of conceptual uncertainty.

It is suggested that a sensible step forward would be for the proponents and opponents of RHT to answer the above questions and then to set out to explore the circumstances in which a change in environmental risk might be met with some degree of behavioural adjustment.

PART 2 - AN EXPERIMENTAL EXAMINATION OF RHT IN THREE SIMULATED PHYSICAL RISK-TAKING ENVIRONMENTS

In this, the second part of the thesis, RHT is examined experimentally in three different simulated environments: a high-fidelity driving simulator, a low-fidelity driving simulator, and a simulated nuclear power plant control room. Over the five studies that appear in this part of the thesis, essentially three questions are asked. First, what is the role of utility in determining behavioural changes in conditions of dynamic environmental risk? Second, what is the relationship between the specificity of change to environmental risk and the specificity of behavioural adjustment to that change? Third, can a low-fidelity simulation be successfully employed to test RHT predictions? Findings together with their implications are discussed.

0.1 Introduction to experiments

The two aims of this thesis - to develop a methodology for RHT based on the simulation of physical risk, and to employ that methodology to examine those RHT predictions that cannot easily be tested through existing methodologies - underpin the studies reported here. Whilst the studies are all reconcilable with these aims, they do retain a certain amount of independence from each other. The extent to which any one study follows logically from preceding ones can at times be lost in the considerable scope of these two aims. For this reason, a short section entitled *links with previous studies*, is provided after the introductions from study 2 onwards.

0.2 Behavioural Pathways

Throughout this thesis many references are made to the behavioural pathways of RHT. The term pathways can be used to mean two slightly different things. Under proposition 2, Wilde (1988), the term can mean just one of three things: behavioural adjustments within the environment, and mode migration from and avoidance of the environment. However, under the first of these terms, behavioural adjustments within the physical risk-taking environment, one could ask *specifically* how accident loss stability is being achieved. Candidates here, in a road-traffic environment, might include driving speed adjustments, attention changes, increasing or decreasing the temporal leeway allowed for overtaking, etc. Although Wilde suggests that these are trivial questions since RHT does not specify the exact pathways through which homeostasis will be achieved, for the reasons already outlined, the question of what pathways carry (or are associated with) the effect will be given special attention in this thesis. The following section serves to define what these pathways are.

Strictly speaking, measures *of* accident loss, both direct and indirect, should not be referred to as behavioural pathways to homeostasis; rather, they are *manifestations* of any risk homeostasis. However, since they are often inexorably linked with specific behaviours and hold clues as to how homeostasis may be achieved, they are, albeit slighly incorrectly, categorised as pathways for the purposes of this thesis.

0.3 The Aston Driving Simulator.

For the first two studies involving the ADS, the behavioural pathways on which measures are taken are:

mean speed; maximum speed; speed variability (the SD of speed over the experimental session); accelerator variability; accelerator mean; brake mean; brake variability; 'kerb collisions' (hitting the curb); 'other vehicle collisions' (hitting another vehicle, either from behind or through 'cutting in' from the side); overtakes (the number of times the driver overtook another vehicle); position mean (position relative to centre white line); position variability (the SD of position relative to centre white line over the experimental session); distance (distance to car in front); distance variability (SD of distance to car in front).

Subsequently, the ADS was improved to allow more detailed measures of overtaking behaviours. This improved ADS was employed in study 5. The new measures associated with overtaking behaviour were:

aborted overtakes (the number of); other vehicle collisions whilst overtaking, risky overtakes ('risky' being defined as an overtake initiated when an oncoming vehicle was visible from the driver's position when the overtake was initiated); successful overtakes; overtakes ending in collision with the kerb; mean temporal leeway of overtake (time-to-spare between pulling back after overtaking and the point at which a collision would otherwise have occurred).

In one study, attention is examined indirectly via secondary task output.

0.4 The nuclear power plant control room

Pathways to homeostasis here fall broadly into four categories: active and passive errors, and primary and secondary task performance. Active errors are those which involve taking incorrect action; passive errors are those which arise through taking no action. The measures taken from primary task performance were:

errors including missed targets; errors excluding missed targets; the absolute number of targets missed; and the proportion of targets missed to incoming targets.

Attention was examined indirectly via secondary task performance. Here the number of errors, the proportion of errors, and the absolute level of output were all examined.

The nuclear power plant control room environment is the only simulation reported here to attempt to take measures of notional avoidance. This took the somewhat crude form of asking participants to give a probability rating of their likelihood to remain in the environment at different salary levels. (These salary levels were intended to provide utility to the decisions.)

0.5 The low-fidelity driving simulators

Two attempts to examine RHT in a low fidelity environment are reported in this thesis. Together these involve measures being taken of the following pathways:

mean speed, mean number of overtakes, risk of overtakes; estimated concentration when driving, relative likelihood of stopping on seeing an amber light; likelihood of adhering to mandatory speed restriction; accelerator pressure; collision likelihood; temporal leeway; position on road; brake pedal pressure; attention (estimated incidence of taking eyes off road); collisions whilst overtaking; kerb collision estimated likelihood; clutch pedal pressure; aborted overtakes; steering control; collision with kerb whilst ovetaking.

Chapter 5 - The role of utility in risk homeostasis theory

5.1. Introduction to utility and RHT

Risk homeostasis theory relies on the concept of utility (Wilde, 1988). Certainly utility is at the very heart of the mechanism by which RH is said to work, and writers such as Graham (1982) and Summala and Näätäten (1988) have gone further and suggested that RHT amounts more or less to a utility maximisation model. This view states that RHT is just a single case of, or a single prediction from, utility theory. Others, for example, van der Molen and Botticher (1988) and Tränkle and Gelau (1992) have suggested that RHT is, whilst being related to utility theory, not identical with it. Van der Molen and Botticher (1988) suggest that RHT is not a pure utility maximization theory in that is has a different 'decision rule'. The suggestion here is that once the 'target' level of risk is set, further calculations of utilities and probabilities do not take place. What happens instead, say van der Molen and Botticher, is that the choice of behaviour is selected on the basis of avoiding a change in expected losses (which translates to risk). Yet, if RHT were purely utility-driven, the behaviour alternative with the maximazation of overall expected benefits would be selected.

Tränkle and Gelau (1992) in their empirical look at RHT in a simulated risk-taking environment see RHT and the subjective expected utility (SEU) model as competitors. Subjective expected utility is a model dealing with situations in which a decision maker experiences some degree of uncertainty of outcome. The SEU-model, proposed by Edwards (1954, 1955), states that in determining which of several competing behaviour options to select, the individual calculates the expected utility of each and selects the behaviour with the greatest expected utility. This calculation is a product of the probability of the outcome and the consequences, if the (uncertain) outcome is achieved. A problem with the model, as Tränkle and Gelau point out, has been that situations where decision making does not conform to a utility rule appear to exist, as highlighted in the work of Tversky and Kahneman (1981) and Kahneman, Slovic and Tversky (1982).

Certainly, if the SEU-model and RHT prove to be distinguishable, SEU would appear to be a competitor to RHT. Both operate in road traffic situations, both are by implication generalizable to other cases of dynamic physical risk, both hold predictions for cases where the level of environmental risk changes, either for the better, or for the worse. The question then is whether SEU and RHT are distinguishable forms of utility-driven behaviour.

Tränkle and Gelau say that they are. They point first of all to the different decision rule highlighted by van der Molen and Botticher (1988), and they suggest also a difference in the level of cognitive processing. As they put it:

"[...] we have to keep in mind that keeping the level of risk constant may be observable as an empirical fact without it being actually desired by the subjects: risk constancy may be an epiphenomenon of the maximisation of the expected utility [...]" (p9).

Putting to one side the fact that RHT is not about risk constancy at all, the point the authors make is a fair one, and one that sees an echo of Wilde (1988) when he said that:

"They [the factors that determine individual target levels of risk] *are probably so thoroughly internalised that most individuals are not consciously aware of most of them most of the time.* (Wilde,1988, p443)."

Perhaps then, one difference between the SEU-model and RHT is that SEU seems to suggest a calculation at quite a high level of consciousness, whilst RHT suggests that the calculation is something that individuals are probably not aware of making. However, so far as the two models are concerned on the matter of utility-derived predictions, the two would appear to amount to the same thing, operationally, at least.

As already stated, the simulation of physical risk allows those predictions of RHT that are relevant to the concept of utility to be tested. With such a simulation, the concept of utility, whilst still escaping any practical measurement, can at least be operationalized as an independent variable. The question of whether this variable has any effect on risk-relevant behaviour, either as a main effect or in interaction with environmental risk, either in direct compensation behaviours within the simulated environment or through the pathway of avoidance, is the subject of the two experiments which follow.

5.2 Study 1 - Utility, RHT and the Aston Driving Simulator

5.2.1. Introduction

To understand what it means to make an environment safer, a distinction is required between two quite separate concepts of safety. *Intrinsic* safety refers to interventions at the level of the environment that, *ceteris paribus,* would be expected to reduce some measure of accident loss. Seat belts are an example of improvements in the intrinsic safety of motor vehicles: assuming all other things remain the same, safety belts make the environment *intrinsically* safer and accident loss can be expected to fall. The second concept of safety is a purely operational one. It refers to actual, operating safety levels. Operational safety changes may or may not occur because of some change in intrinsic risk.

These two concepts of safety, it has been argued, do not amount to the same thing. Wilde (1982, 1988) has argued that an improvement in intrinsic safety is not logically necessary in order to reduce accident loss and that motivational changes by themselves can bring about a reduction in loss. Moreover, an improvement in intrinsic safety is held not to be logically sufficient for a reduction in accident loss to be seen. A number of writers (eg O'Neill, 1977; Peltzman, 1975; Wilde, 1982, 1985, 1986, 1988, 1989) have suggested possible mechanisms by which a change in intrinsic safety can be negated. Wilde calls this suggestion 'Risk Homeostasis Theory' (RHT), whilst O'Neill refers to the phenomenon as 'Danger Compensation'. The essential postulates of the suggestion differ only slightly from writer to writer, and whereas Wilde's model of RHT is built around empirical findings, O'Neill's contribution takes the form of an essentially mathematical description of the compensation process. In common, they have the proposal that where a benefit (or utility) is attached to a shift in behaviour to a more risky strategy, behaviour will change such that in one sense (ie, on the accident reduction criterion) the change in intrinsic safety might just as well not have happened. If this suggestion holds true, one hardly need say that the implications for workers concerned with improving the level of intrinsic safety are profound.

It is important to reiterate that behavioural changes in response to improvements in intrinsic safety are said to be dependent on there being a utility attached to the behaviour change in question. RHT is not about risk *for its own sake*, but rather, states that an associated utility is logically necessary before any effect can take place. O'Neill's mathematical model is also built on several utility terms. (The preposition *on* rather than *around* is used to imply that in the light of any falsification of the role of utility, the model necessarily falls down.)

How might one determine whether suggestions of this kind need to be taken seriously? How might one determine whether risk homeostasis theory is supported? The traditional approach in answering these questions has been the simple before-and-after design. Periodically improvements are made in the intrinsic safety associated with a group of road users. This happened when motorcycle crash helmet laws were introduced: the riders of motorcycles were intrinsically safer. It happened again when front-seat passengers of cars became obliged under law to wear seatbelts; they too were intrinsically safer. There are, however, three major problems associated with this approach.

To recap then, these are: first, since they are in essence uncontrolled pseudo-experimental designs, one never knows what has changed in addition to environmental safety levels. When an environmental safety improvement is associated with accident reduction, proponents of RHT can point to other factors that might have brought about the effect. Equally, when accident loss remains at its previous level after an intervention, opponents of RHT are able to point to environmental factors that might have prevented what would otherwise have been a reduction in accident loss. This has the consequence of leaving the methodology of RHT in a state conceptually akin to the correlational approach.

Second, as a methodology it is best suited to the examination of *effects* of the change in intrinsic safety. How those effects were brought about (through attentional reductions, increased speed, riskier overtakes, etc.) is, to say the very least, difficult to determine. So far, only very crude measures such as speed and headway have so far been examined in this way. Wilde (1988) has argued that this should not be viewed as a problem since it is *only* the effects of environmental safety changes that should be of interest to investigators. How the effects come about is, according to Wilde, irrelevant to the theory of risk homeostasis. This position is arrived at by the logic that if one behavioural pathway to homeostasis were blocked, the homeostasis would simply manifest itself somewhere else. This much of the theory though has not so far been supported by empirical evidence: whether blocking a behavioural pathway *can* eliminate the homeostasis is not known.

One difficulty with concentrating on the effects of the change in intrinsic safety rather than its behavioural pathways is, as Brindle (1986) and McKenna (1986) point out, that it does seem to leave RHT in the position of predicting the null hypothesis; and of course, in any empirical science, we can only with any confidence *reject* the null hypothesis. A change of emphasis towards the identification of the behavioural pathways that carry the homeostasis would unambiguously bring the theory back into line with conventional empirical science: by looking at specific behaviours, it would no longer predict *nothing* (no change in accident loss), it would predict *something* (changes in specific behaviours).

Third, the part of risk homeostasis theory that deals with the *effects* of environmental safety improvements can certainly be tested (albeit crudely) with the before-and-after design. But RHT makes a second assertion: that the homeostasis is an entirely utility-driven phenomenon. The extent to which behavioural compensation depends on the factor of utility cannot, it would seem, be answered via these simple and rather crude methodologies.

If these problems are to be overcome, an experimental approach to RHT would be essential. Through a simulated environment the role of utility and the question of which behavioural pathways are and are not implicated, could be examined. Without laboratory-based experimental work, it is difficult to see how the role attributed to utility can ever be properly examined.

So far as one can determine from the RHT literature, the logically necessary status of utility in bringing about the homeostasis effect amounts to no more than an unverified assumption. The assumption is present, indeed it is necessary, because the theory can be seen as more or less an extension of economic utility theory; Graham (1982) goes so far as to suggest that RHT is no more than a single application of utility theory. One could say then that the central role given to utility in risk homeostasis is crucial to its conceptual standing: in so far as RHT represents a theoretical description of risk-taking in relation to environmental safety levels, utility *is* that theory, and without it RHT can be given only a statistical description, devoid of any explanatory framework, certainly devoid of one postulated in psychological terms.

The present study then, attempts, through a simulated driving environment, to examine both the role of utility and the particular pathways that might carry the effect. It does so without the awarding of points for different behaviours (see chapter 1 for a discussion of the problems associated with this procedure), but instead rewards participants through the same intrinsic rewards that exist in the driving situation RHT was formulated to explain.

Most, if not all, of the experimental work reported in this thesis tests in a very general way those models of risk taking that see the individual's accepted level of risk as the major determinant of accidents, rather than the level of environmental safety. However, more specifically the experimental work reported here tests RHT as defined by Wilde (1988). From this paper, this study can be seen to test a number of specific predictions. From Wilde's proposition 3 it is predicted that the target level of risk is determined by the relevant utilities associated with relatively risky and relatively cautious behaviour. In that this study attempts to operationalise utility and measure its effects over a range of dependent variables, this prediction is tested. Wilde's proposition 13 states that interventions that do not change the individual's target level of risk but do change some other measure of environmental risk will not serve to reduce accident loss in the long term. His proposition 5 states that at any given moment, an individual is characterised by a target level of risk. This study attempts to operationalise environmental risk via the pulling-up distance of the simulator vehicle. Although target level of risk is operationalised through the utility factor, utility does not confound the environmental risk factor; therefore, RHT, through propositions 5 and 13, would predict then that accident loss correlates should be roughly equal under different conditions of environmental risk.

5.2.2. Method

Participants

100 participants took part in the study. All participants were aged between 19 and 31 and possessed a full driving licence. 50 of the participants were male and 50 were female. Participants were recruited from staff and students at Aston University and Aston Science Park.

Equipment

The Aston Driving Simulator was used for this study.

Design

The factor of utility was operationalised over two conditions: time and distance. On the *time* condition participants were asked to drive for ten minutes irrespective of what happened during that time. On this condition there was no obvious incentive to engage in risk-taking behaviour. The *distance* condition involved participants being asked to drive for a given distance (7.2 miles). On this condition there was assumed to be instrumental value attached to risk taking behaviour, assuming time to be of value to participants. This factor of utility was counter-balanced within participants.

Participants were run under one of three conditions of intrinsic safety (a between-participants factor). On condition I participants were informed correctly that an advanced braking system would be in operation. On condition II participants were informed *incorrectly* that an advanced braking system was in operation. Condition III was included as a control in which no information regarding the braking system was provided, participants being given standard brakes. This allowed the role of utility in RHT to be experimentally examined. It was assumed that if utility were logically necessary for the homeostasis effect to occur, a statistical interaction between the two main factors of intrinsic risk and utility would be observed. In addition, an analysis of the particular behavioural pathways through which RHT manifests itself could be undertaken to determine whether these pathways represent any instrumental value to drivers. If utility is not logically necessary for homeostasis to occur one would predict that (a) the factors of intrinsic risk and utility to be statistically independent, and (b) the profile of changed behaviours to be unrelated to utility.

Procedure

Participants were randomly allocated to one of three groups. The first of these groups were told that they would be driving a car simulator fitted with an advanced braking system. The braking system on the simulator program was advanced in the sense that the braking system was operational when the brake pedal was applied 8 degrees as opposed to the standard 30 degrees. A pre-test showed that participants were able to discriminate between these levels of braking efficiency. The second group were given the same instructions as the first experimental group although this information was in fact false and the simulation program with the standard braking system was employed. Finally, a control group were given no information about the braking system. Thus, the only feature discriminating the first and second experimental groups was the efficiency of the braking system. It was believed that a difference between these two experimental groups would point to a closed-loop mediation of the information regarding changes in intrinsic safety. A difference between the second experimental group and control would indicate that open-loop processes can bring about risk homeostasis, whilst evidence of this difference in the absence of a difference between experimental groups would indicate that the effect need operate at *no more* than at an open loop level, at least so far as initial compensation is concerned.

A second factor appeared within participants: the utility of the driving exercise. This had two levels: *distance* and *time*. On the *distance* condition participants were instructed to drive a distance of 7.2 miles. On the *time* condition participants were asked to drive for a period of ten minutes. On the *distance* condition, a positive utility was assumed to be attached to risky behaviour, since the taking of risks would allow participants to cover the distance in a shorter period of time and hence finish the experiment sooner. On the *time* condition it was assumed that no such utility would exist for risky behaviour, since this session ended at the same point regardless of the extent to which participants took risks during the experimental session. RHT would predict an interaction between these two variables, utility and environmental safety, on the dependent measures associated with risky behaviour such as speed, overtaking and headway. Equal numbers of males and females were used in the experiment.

The experiment involved three separate sessions lasting 10, 15 and 15 minutes respectively. The first session was intended as a practice run for participants to familiarise themselves with the simulator. Although data from these practice sessions were gathered, no analysis of them took place.

The information given to participants regarding the driving of a fixed distance of 7.2 miles then was untrue. The programming changes required to allow termination of the run after a fixed distance could not be made in the time scale available. However, it is suggested that this inadequacy did not represent a serious problem for three reasons. Firstly, most participants drove at a speed roughly in line with the distance said to be covered. For these participants the information appeared to be true. Secondly, those participants who drove at a speed outside of this range believed for the majority of the trial that the information given to them was correct. Therefore the two trials did differ even if participants were able to refute the information. Thirdly, on the occasions where participants questioned the information, they tended to do so in terms of the *precise* distance covered, rather than questioning whether any fixed distance was being covered. That is, they suggested that really the distance was not 7.2 miles, but was much further. On this belief state too a utility was present. Before the practice session, participants were given the following instructions:

In this experiment you will be required to drive this simulator as you would a normal automatic car. The road ahead will appear on the screen in front of you and you will need to press your foot firmly on the accelerator in order to travel along the road. The brake is situated as it would be in a normal car, but there is no clutch. The steering wheel will respond to your actions in the normal way.

When the road appears on the screen in front of you, you should start driving immediately for a 10-minute practice session at your preferred speed (the maximum possible speed is 70 mph). During this session you should familiarise yourself with the feel of the simulator whilst driving. If you touch the kerb an alarm will sound and you will slide around the track. If you hit another vehicle your windscreen will smash and you will be re-positioned further up the track in order for you to continue driving. It is important that you try as much as possible to avoid hitting either the kerb or another vehicle.

If there are any questions, ask the experimenter now.

The ten minute practice trial then took place.

Those participants allocated to the "no advanced-braking system" condition were given the following instructions on trials 2 and 3 (counterbalanced):

In this part of the experiment your task is to drive a distance of 7.2 miles. The simulation will end when this distance is covered. The car is now ready to start.

(trial 3)

In this part of the experiment your task is to drive for a period of 10 minutes. The simulation will end when this period is over. The car is now ready to start.

Participants in the "advanced braking system" group, and participants in the "advanced braking system group false" (participants told of the advanced braking system, but given standard brakes) were given the following instructions:

In this part of the experiment your task is to drive a distance of 7.2 miles. The simulation will end when this distance is covered. The car has been fitted with an Advanced Braking System and will be able to stop very quickly, should you need to. The car is now ready to start.

(trial 3)
In this part of the experiment your task is to drive for a period of 10 minutes. The simulation will end when this period is over. The car has been fitted with Advanced Braking System and will be able to stop very quickly, should you need to. The car is now ready to start.

5.2.3. Results

The Aston Driving Simulator allowed 14 possible behavioural pathways of RHT to be examined. These were: *mean speed; maximum speed; speed variability* (the SD of speed over the experimental session); *accelerator variability; accelerator mean; brake mean; brake variability; 'kerb collisions'* (hitting the kerb); *'other vehicle collisions'* (hitting another vehicle, either from behind or through 'cutting in' from the side); *overtakes* (the number of times the driver overtook another vehicle); *position mean* (position relative to centre white line); *position variability* (the SD of position relative to centre white line over the experimental session); *distance* (distance to car in front); *distance variability* (sd of distance to car in front).

ANOVAs were performed on all of these dependent measures for intrinsic risk (advanced braking system, no advanced braking system, or the false information that an advanced braking system was in operation), and utility (a *distance* condition under which utility was assumed, or a *time* condition under which utility was assumed not to exist). In addition, a third factor was analysed: *block*. By the end of the experiment the Aston Driving Simulator had generated somewhere in the region of half a million data points. To simplify subsequent analysis the data for each 15-minute run were broken down into twenty 45-second blocks. This enabled *block* to be entered into the analysis as a factor reflecting a change in the given behaviour during the run. In other words, an effect of *block* means that the behaviour changed as a function of time throughout the experimental session.

It was predicted from RHT that those behavioural pathways tending towards homeostasis would interact with the factor of utility. A series of utility x intrinsic risk interactions were therefore to be expected if RHT were to be supported.

For speed variability there was a significant effect of intrinsic risk, $F[2,95]=4.05$; $p<.025$. There was greater variability on the ABS true than the ABS false condition and greater variability on the ABS false condition than the control condition. The factor of utility and the intrinsic risk x utility interaction both produced F values of <1, *NS; df* $[1,95]$ and $[2,95]$ respectively. A block effect was seen, $F[19,1672]=13.84$, $p<.001$, together with a block x utility interaction ($F[19,1672]=30.32$, $p<.001$).

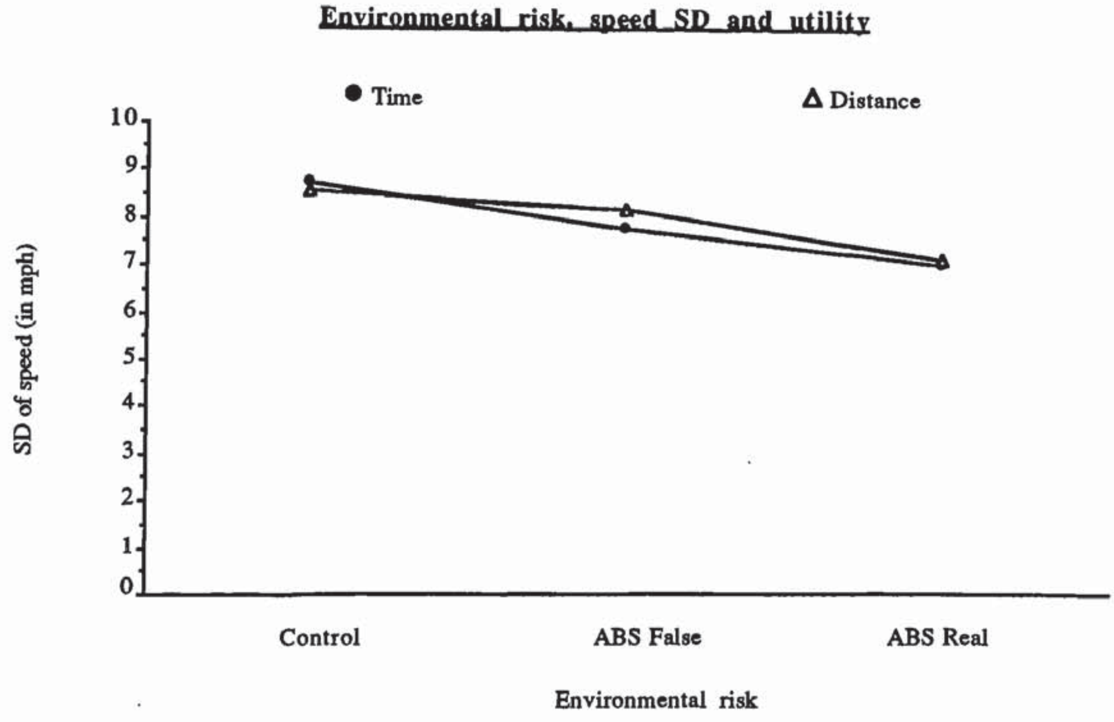


Figure 5.1 - Environmental risk, SD of speed (mph) and utility

For the measure of mean speed, a measure on which one would expect to see compensation since its utility is clear, there was no effect of intrinsic safety, $F[2,95]=1.38$, *NS*. Utility and intrinsic risk x utility were also not significant, respectively producing $F[1,95]<1$ and $F[2,95]<1$.

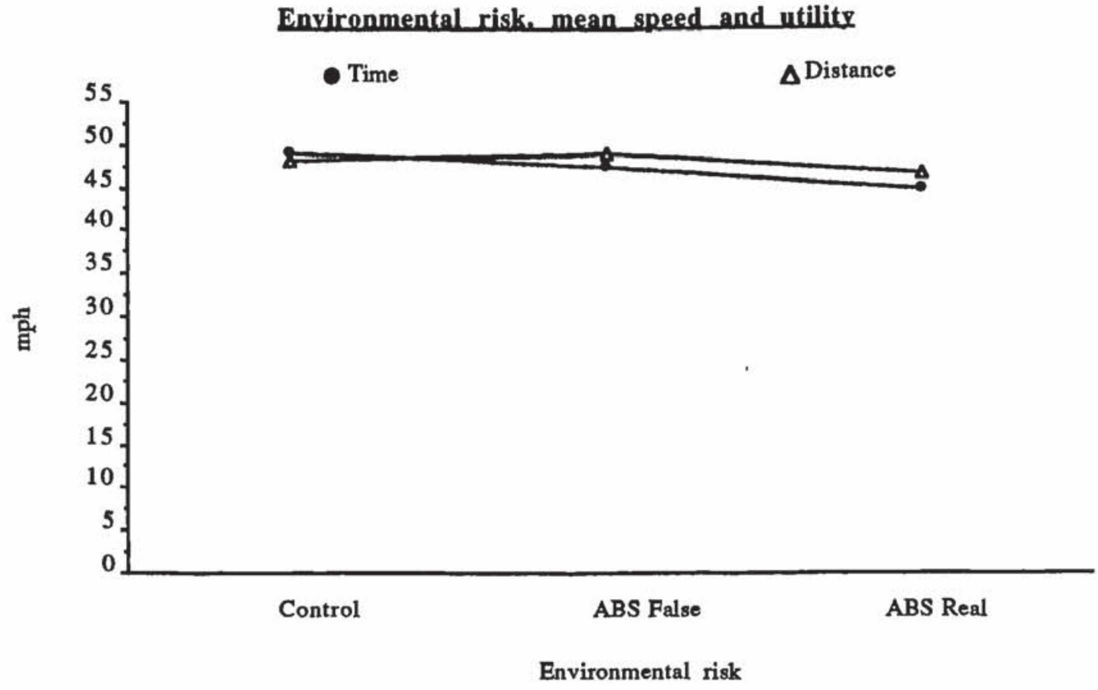


Figure 5.2 - Environmental risk, mean speed, and utility.

Surprisingly however, there was an intrinsic risk effect for mean accelerator angle, obviously strongly correlated with speed, $F[2,95]=3.53, p<.05$. Here the angle was greater on ABS true than ABS false, but greater on ABS false than control. On this measure neither utility ($F[1,95]<1$) nor intrinsic risk x utility ($F[2,95]=1.39$) were significant. Speed changed as the runs progressed with block producing an $F[19,1672]=4.52, p<.001$. It is, however, difficult to interpret this finding in as much as speed between one block and another could change for a variety of reasons, including traffic and road variability. Observation of participants revealed that in most cases an increase in speed throughout the run occurred. The block x utility interaction was also significant with $F[19,1672]=12.67, p<.001$.

Further support for a more subtle behavioural compensation in speed comes from the measure of maximum speed. Here intrinsic risk produced an effect of $F[2,95]=3.58$, $p<.05$ (participants on the ABS real condition recorded greater maximum speeds than control participants), and an interaction of intrinsic risk x block of $F[38,1672]=1.49$, $p<.05$.

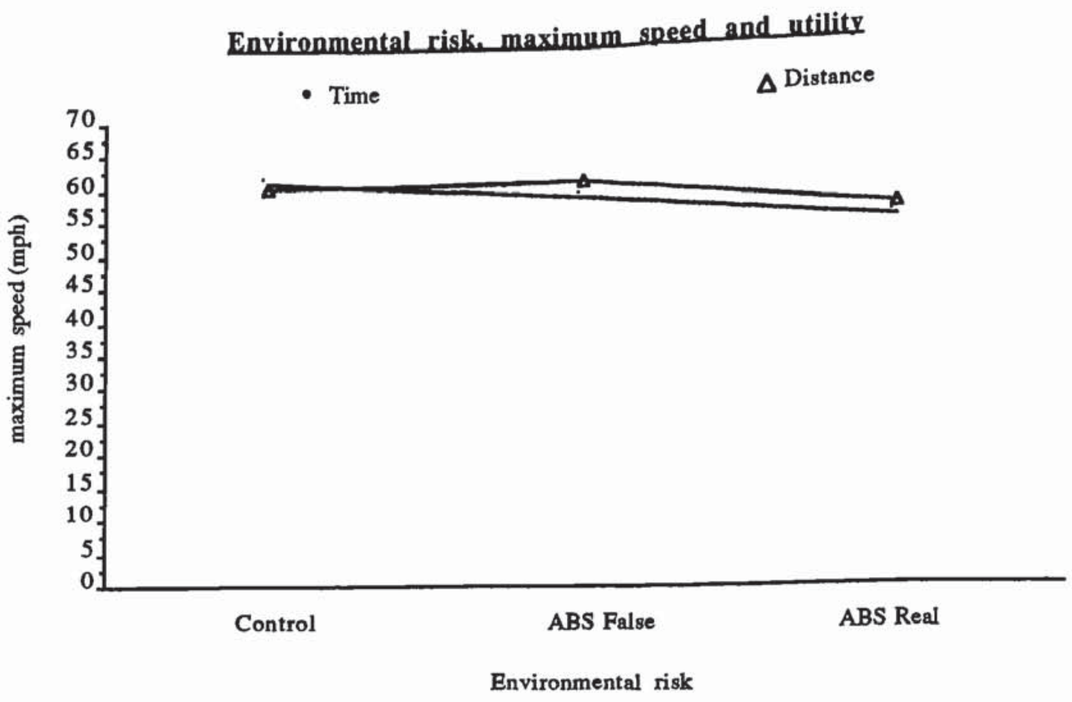


Figure 5.3 - Environmental risk, maximum speed over the whole experimental condition, and utility.

Accelerator variability, by contrast, did not produce an effect of intrinsic risk *(F[2,95]=2.17, NS)*. It did however show a strong effect of utility *(F[1,95]=9.84, p<.0025)*, and again, the intrinsic risk x utility interaction was not significant, *F[2,95]<1*. Block was however significant: accelerator variability differed from block to block, *F[19,1672]=3.50, p<.001*, with block x utility interaction significant also, *F[19,1672]=1.68, p<.05*. In addition intrinsic risk x block x utility was significant, *F[38,1672]=1.78, p<.01*.

The variable of mean braking did produce an intrinsic risk effect, *F[2,95]=3.28, p<.05*. But here utility did not exert an influence, *F=2.67, NS*. It should of course be remembered that the nature of the change in intrinsic safety, the shifting of the initiation threshold, makes the effect of mean braking inevitable as a purely mechanical effect and should perhaps be interpreted in this way rather than in psychological terms. The interaction of intrinsic risk with utility was also not significant with *F[2,95]<1*. Block too was significant, *F[19, 1672]=2.50, p<.001*.

Interestingly, variability in braking behaviour did not bring about an effect of intrinsic risk, *F[2,95]=2.55, NS*. However, on the same measure utility was significant with *F[1,95]=5.64, p<.025*. No interaction was seen between factors, *F[2,95]<1, NS*, but block was again significant, *F[19,1672]=3.83, p<.001*.

The Aston Driving Simulator provides two very direct measures of performance: other vehicle collisions and kerb collisions. An other vehicle collision involves hitting another vehicle (head-on, cutting in from the side, or hitting the vehicle in front). A kerb collision occurs when the simulator vehicle hits the curb on either side of the road. RHT would of course predict that for both of these criteria there should be no effect on intrinsic risk. On the factors of utility *(F[1,95])* and intrinsic risk *(F[2,95])* all *F* values were *<1, NS*. The interaction between these two factors was also not significant for both criteria *(F[2,95]<1* for kerb collisions; *F[2,95]=1.31* for other vehicle collisions).

For overtaking behaviour there was no effect of intrinsic risk *(F[2,94]=1.64, NS)*. Utility produced *F[1,94]<1, NS*; and intrinsic risk x utility produced *F[2,94]=1.37, NS*. Block produced a significant effect of *F[19,1672]=3.28, p<.001*. The interaction of block x utility too was significant, *F[19,1672]=2.02, p<.01*.

Position mean, the measure of tracking performance, provided no evidence for an effect of intrinsic risk, *F[2,95]<1, NS*. Utility gave an *F[1,95]* value of *<1, NS* and the utility x intrinsic risk produced *F[2,95,]<1, NS*. So far, then, as attention can be inferred from tracking performance (and so far as tracking performance can be inferred from position mean), there was no risk homeostasis effect. However, the factor of block was significant, *F[19,1672]=5.42, p<.001*, as was the interaction of block x utility, *F[19,1672]=1.64, p<.05*. The related measure of position variability showed only an effect of block *(F[19,1672]=2.88, p<.001)*.

On the measure of distance-to-car-in-front, intrinsic risk also failed to show any effect, $F[2,95]<1, NS$. Utility ($F[1,95]=1.71$) was also not significant, with the utility x intrinsic risk interaction producing $F[2,95]<1, NS$. Block was significant, $F[19,1672]=12.7, p<.001$. Again, there was a block x utility interaction, $F[19,1672]=6.24, p<.001$.

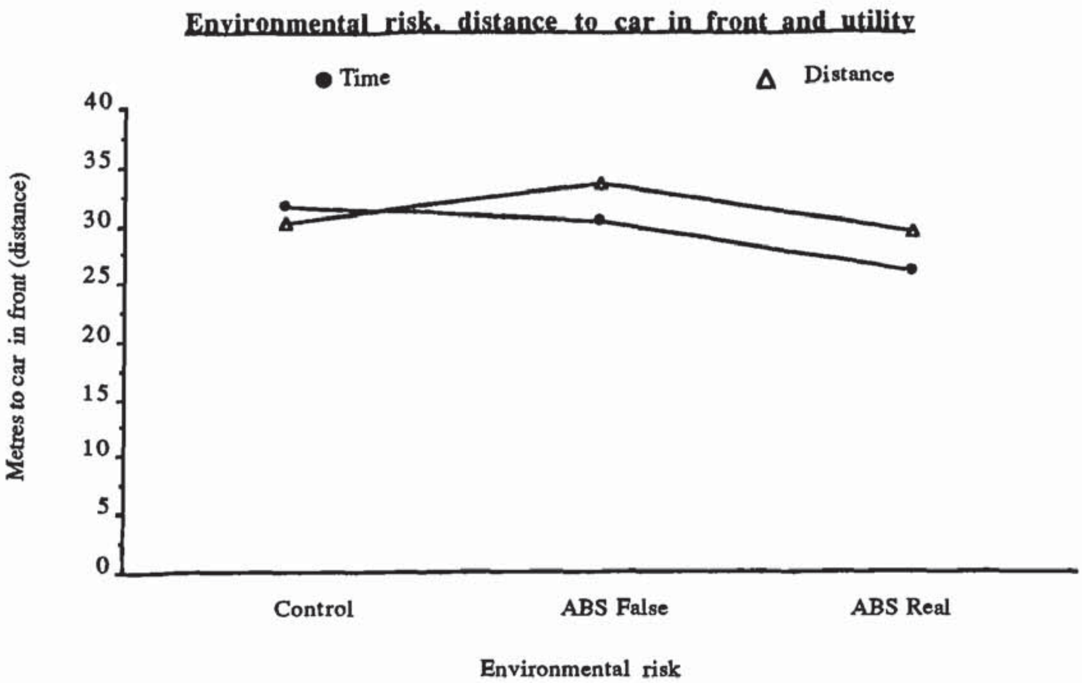


Figure 5.4 - Environmental risk, distance to car in front (in simulated metres) and utility.

Distance variability showed a similar absence of effect with $F[2,95]=1.23, NS$ for intrinsic risk; $F[1,95]=1.40, NS$ for utility, and $F[2,95]=1.08, NS$ for utility x intrinsic risk interaction. To some extent, this reflects the nature of the criterion as operationalized in the Aston Driving Simulator. There were relatively few oncoming cars and so rather than stay behind a car drivers were free to overtake. In addition to this, some drivers maintained a speed below that at which cars in front of them were travelling. For these drivers, distance from car in front increased throughout the run. Since only headways of below 110 m were recorded by the simulator, these drivers did not really have a headway at all. In spite of these factors, however, there was an interaction of intrinsic risk x block, $F[38,1672]=1.57, p<.025$. Utility x block was significant also, $F[19,1672]=6.69, p<.001$, as was the factor of block, $F[19,1672]=23.91, p<.001$.

Post hoc analyses of the effect of intrinsic risk

Tukey's HSD *post hoc* analyses were carried out on all significant effects of intrinsic risk to determine whether the mere information of a safety change is sufficient to bring about compensation, or whether closed-loop feedback of the change is necessary (that is, to determine between which of the three groups of intrinsic risk the effect was significant). Only the dependent measures asssociated with brake use were not analysed in this way, since the real advanced braking system (ABS real) condition involved changing the initiation threshold. Consequently effects of brake use and brake variability could be attributed to this mechanical intervention rather than to human factors. For all *post hoc* analyses harmonic means were used.

First, speed was analysed. Here the significant effect of speed variability could be entirely attributed to the control versus ABS real difference (Q_{OBT}=3.86, $p<.05$). For maximum speed (that is, the maximum speed recorded over the experimental session) the same was true: only the ABS real versus control comparison was needed to account for the effect (Q_{OBT}=3.57, $p<.05$).

For mean acceleration, however, the only effect lay between the two conditions of intrinsic risk, the information of a safety change and the actual safety change (Q_{OBT}=3.47, $p<.05$).

These results would seem to indicate that whilst the factors of intrinsic risk and utility are capable of producing main effects, they are, for the most part, statistically independent.

5.2.4. Discussion

Given that Wilde's RHT and O'Neill's danger compensation models both suggest that an attached utility is necessary for compensation to take place after any change in environmental safety, any experimental design incorporating utility and intrinsic risk should, according to these models at least, show evidence of a statistical interaction. Across the behavioural pathways examined, only one interaction was significant - speed variability. How could this be reconciled with RHT or danger compensation?

It would not appear to be the case that the utility factor was not properly operationalised . On no fewer than six criteria, utility was significant as a main effect. It must therefore be clear that the utility conditions were differentiated by participants. More risks were taken when more stood to be gained.

Neither was it the case that the differences across the three intrinsic safety conditions were not recognised, for in the context of this experiment, evidence for behavioural compensation was seen in relation to intrinsic risk. The factors, then, of intrinsic safety and utility both provide evidence for some sort of behavioural compensation, but, except in the single case of speed variability (a measure itself that is difficult to reconcile with the concept of utility), the factors appeared to behave independently. RHT's essential postulate - that behavioural compensation does not occur in the absence of utility - would appear, in the context of this work, to be refuted.

In addition, it is interesting to look at the behavioural pathways through which the homeostasis manifested itself. These were maximum speed, speed variability, accelerator mean and brake mean. It is considerably harder to reconcile these pathways with the concept of utility than it would be for the pathways of mean speed and number of overtakes, yet those potential pathways showed no evidence of homeostatic behaviour. If one particular compensation pathway is cognitive in the sense of an attentional reduction, a contingency that could be reconciled with utility, then one would expect performance on a tracking task to be negatively affected. In essence, *position mean* provides a measure of tracking performance. Whilst there was a block x utility interaction (as the run progressed, participants' position mean dropped when they were performing on the utility (distance) condition, $F=1.44, p<.05$), there was no effect of intrinsic risk. In other words, participants did not compensate for the change in intrinsic risk by a reduction in attention, at least so far as can be determined by their tracking behaviour.

Not only then is it the case that utility and intrinsic risk seem to be statistically independent across the behaviours reported here, but moreover, the particular behavioural pathways through which risk homeostasis seems to operate are not particularly good candidates for utility provision. It is hard to see any obvious benefit derived from the changing of speed variability behaviour, but easy to see a benefit to be exchanged for driving faster, or with reduced attention. Yet the first of these behaviours tended towards homeostasis whilst the two remaining did not.

The important question now must be whether the independence of utility and intrinsic safety observed in the present experiment is generalizable. Empirically there is not very much to say on the matter: the importance of utility as a determinant of risk-related behaviour changes has the status of an all-pervasive assumption. In O'Neill's (1977) model of danger compensation for example, behaviour is assumed to be a product of the utility per hour of driving without accident, the utility (a negative utility, of course) of actually having an accident, together with accident probability and trip length in units of distance.

It stands to reason that if two of these four terms enjoy no causal role in determining risk-taking behaviour, the model's assumptions of utility are manifestly unwarranted. How then, is one to interpret utility as a *main* factor? Does the experimental finding not indicate its importance in the homeostatic process? The answer would seem to be no. A change in behaviour arising from a change in utility cannot be related to a homeostatic process since, in the absence of changes in intrinsic risk, the operational level of risk has *risen*. The factor of utility might then be reconciled with RHT in changing the target level of risk. The change in risk was not, however, of sufficient magnitude to influence the criteria of other vehicle collisions and kerb collisions, which correspond broadly to accident loss.

If the factors of utility and intrinsic risk are both capable *by themselves* of bringing about the change in risk-taking behaviour, where might this leave risk homeostasis theory? One possible answer, and perhaps the most likely, would be that the phenomenon of risk homeostasis should be seen in a sensation-seeking context. Risk homeostasis might then have to be explained in the very terms that have always been so fiercely avoided: *risk for its own sake*. To see risk homeostasis in this explanatory framework would not, however, be to leave the question of behavioural pathways as a trivial matter, as Wilde believes them to be under a utility-driven account. If the effect is primarily about sensation-seeking, then the restriction of the effect to very specific behavioural pathways would be be very nearly inevitable.

5.3. Implications following from study 1

The main question about the observed behavioural compensation arising from study 1 is whether the manipulation of intrinsic risk was appropriate. The effect of the advanced braking system was a relatively subtle one, and over a time span of thirty minutes a more effective strategy might have been to provide a very sharp contrast of intrinsic risk, as in, say driving on the left and on the right hand side of the road - a particularly interesting experimental question after the experience of Sweden.

Study 1 was successful in identifying several behavioural pathways through which homeostatic behaviour was evident. It was successful too in showing that other behavioural pathways, through which one might expect to see homeostatic behaviour, showed no such tendency. Careful and lengthy examination of these findings failed to show any real pattern so far as utility is concerned. Certainly the findings cannot in any meaningful way be reconciled with the traditional utility-driven account. The question then arises of whether these pathways *reliably* carry the effect. It must be remembered that several of the intrinsic risk effects were significant at relatively modest alpha levels, .05 or thereabouts. One should therefore demand replication of the pathway findings in order to reject the alternative explanation of the results, that of Type I error.

As a third point, it is worth noting that although the behavioural pathways need further analysis and replication, their failure to be reconciled with utility in conjunction with the absence of interactions between utility and intrinsic risk do point to a need to re-interpret the explanatory mechanism of RHT. If utility does not drive the effect, it might be profitable to examine possible 'competing' hypotheses. At the forefront of these would be (a) a general sensation-seeking account, stressing the importance of balancing a need for excitement with a need for intrinsic safety, and (b) a personality-based account, perhaps taking into account the sensation-seeking needs of (a), but not generalized to the whole of some notional driving population. This hypothesis would place emphasis on individual differences in personality. So far as further experimental designs go, these hypotheses could be better tested with a shift from independent subjects design of study 1 to repeated measures. This would allow individual homeostatic behaviours to be measured and correlated against, say, personality measures.

A fourth consideration following from study 1 was the low volume of traffic. When participants overtook cars under conditions in which overtaking was unsafe, the probability of their meeting another car was relatively low. Unless the measures of other vehicle collisions and kerb collisions are sensitive to changes in risk taking behaviour, it would, of course, be methodologically quite improper to place any importance on, as it were, the significance of *non*-significance.

For this reason the number of cars to be overtaken and the number of on-coming cars was doubled from approximately one on-coming car per minute for subsequent experiments. This, it was felt, would provide a more sensitive measure of risk taking, in as much as unsafe overtakes would now be more likely to be recorded in the form of crashes.

Some participants complained that the simulator was unrealistically slow. The 0-60 mph time was later tested and found to be approximately 15 seconds. This was reduced to 6.6 seconds for subsequent experiments. More importantly, observation of participants during the experiment led to the conclusion that the maximum speed of 70 mph may have resulted in a ceiling effect. That is to say, when participants were driving fairly consistently at 70 mph under one condition of intrinsic risk, there was not, as it were, anywhere left for speed-related compensation to take place. For this reason the program was changed following study 1 so that the maximum speed was raised to 120 mph.

5.3.1. A demand characteristics explanation

Since the design of study 1 involved telling some participants that a change was to take place in intrinsic risk and then observing their behaviour, the issue of demand characteristics must be considered. However, at the outset, two possible outcomes were envisaged: either RHT is supported, or it is not. If RHT is supported, then the interaction between intrinsic risk and utility will strongly suggest that demand characteristics were not causally important in the essence of the RHT evidence. This is because if demand characteristics existed for the design, they presumably existed across both levels of utilty. If RHT is not supported, there will be no demand characteristics problem to discuss. What was not anticipated were the main effects of the design without interactions; yet, if demand characteristics were operating in the experiment, this is exactly what one would expect to see.

All that can be said in defence of this is that debriefing of participants did not reveal evidence to suggest that the design together with intructions served to cue the observed behaviour. An implication from this would appear to be that designs should seek to manipulate intrinsic risk in some way that is not obvious from the experimental design. For example, where intrinsic risk is manipulated through asking participants to drive on the right rather than left hand side of the road, or where pulling-up distances are manipulated subtly without informing participants, it would be much more difficult to see how demand characteristics could present the same kind of problem. At the same time, the debriefing of participants remains important to monitor the success of this strategy.

Study 1 then, in showing an apparent independence of intrinsic risk and utility, can be interpreted in at least two ways. It may be that the factors really are independent in that what governs risk homeostasis is a sensation-seeking or personality-related mechanism, or at any rate, some mechanism that is not related to the concept of utility. Utility may then have no place in RHT.

This first explanation is difficult to accept when one considers (a) that utility is almost the whole of the theoretical underpinning of RHT, and (b) that utility by itself was shown to influence participants' behaviour. A second explanation then is one of demand characteristics. Intuitively at least, the experimental design of study 1 leaves itself open to the demand characteristics charge. Only participant debriefing can be cited in defence here.

The next step would appear to be an attempt at deciding between these two competing hypotheses. An independent subjects design with no information given regarding the absolute or relative level of intrinsic safety would appear to control against the possibility of demand characteristics confounding results. On the repeated measures side personality correlates with homeostasis (or perhaps more correctly compensation behaviours) could be looked at.

A further implication of experimental work involving the modelling of physical risk is that one form of compensation (a way of moving towards homeostasis) would be through the behavioural pathway of mode migration or avoidance. For example, if one were to judge a motorcyle as being intrinsically more risky than one's target level of risk, one may compensate through reduced speed, increased attention, making fewer overtakes, etc., but then again, one does not have to use that particular mode of transport in the first place. The same journey could be made by an alternative form of transport or one could simply not make the journey, but stay at home. In an experimental design involving simulation and based on the ADS, these two possible pathways to homeostasis are effectively 'blocked'. If the experimental data are to be included, the participant must engage in the simulated activity that he or she is being asked to engage in. This has obvious implications. It may be that a simulated excercise involving changes in intrinsic risk is capable of producing differential accident loss measures that would not be reflected in the environments the simulation seeks to explain.

There is a defence to the above objection. Wilde (1988) suggests that the question of the particular behavioural pathway through which RHT manifests itself is a trivial matter since the effect will manifest itself come what may. If a single pathway is blocked, the effect will come from somewhere else. However, the concept of behavioural pathways can be extended to include mode migration and avoidance (respectively, making the journey by the means of an alternative form of transport, or not making the journey at all), then blocking these pathways should only change *the way* in which homeostasis is brought about: the homeostasis should still be achieved, so long as pathways remain open that are capable of carrying it.

5.4 - Study 2: Risk homeostasis in a simulated nuclear power plant control room (NPPCRS) environment

5.4.1. Introduction

Whilst study 1 was successful in providing evidence for RHT in a simulated environment, the evidence comes entirely from behavioural adjustments within the physical risk-taking environment. However, it is important to recall that in Wilde's (1982a, 1982b, 1988) model of risk homeostasis theory, it is suggested that the mechanism by which an equilibrium state of accident loss is said to take place involves three separate behavioural choices (Wilde, 1988, proposition 2). When a change is made to the level of environmental risk, the risk-taker may respond first by behavioural adjustments *within* the risk-taking environment. In a road traffic environment, this may involve driving faster or slower, overtaking less frequently, reducing the marginal temporal leeway at which an overtake will be attempted, increasing or decreasing attention, and so on. A second route to the achievement of homeostasis is what one might term 'mode migration' - changing from one form of transport to another. For example, a motorcyclist may decide, in the light of inclement weather, to take a train into work rather than risk collision on his or her motorcycle. Finally, if the level of target risk and the level of actual risk cannot be reconciled either within the risk-taking environment, or through changing from one mode of transport to another, the individual may elect to stay at home and not to undertake any journey. This possibility, for the purposes of this thesis, will be referred to as 'avoidance'.

So, the achievement of risk homeostasis can, according to its originator, be brought about in three ways. These can be labelled behavioural adjustments within the environment, mode migration, and avoidance.

Out of this comes a realisation that all of the attempts to examine RHT in simulated environments, including study 1, have in fact looked only at one possible pathway to homeostasis: behavioural adjustments *within* the environment. Interesting though this question is it would appear to answer just part (perhaps only one third) of the risk homeostasis question.

Before going any further, some discussion of the human reliability in nuclear regulatory control should be provided. In the United Kingdom, the safe running of nuclear installations is the responsibility, primarily, of HM Nuclear Installations Inspectorate (NII). Although it is not the job of the NII to impose specific regulations in design and operation of installations (their safety is a matter for the individual installations), the NII has the power to provide or withold a licence. A nuclear plant has to be shut down for annual or biennial maintenance; NII consent is required before the plant can start up again. Although no regulations are imposed by the NII with respect to any given plant, the NII can set specific conditions or operating requirements for any individual licensee (Whitfield, 1987).

The general expectations of the NII, so far as safety standards are concerned, have been set out for the purpose of promoting safety in power reactors and nuclear chemical plant (NII, 1979; NII, 1983a). As Whitfield says, the purpose of these guidelines is not to serve as design guides, but they do serve to help the licensee reduce risks so far as is reasonably practicable. This much the NII does in the name of intrinsic safety.

But the NII has for some time recognized that human error also is important in plant safety (NII, 1982; NII, 1983b; Whitfield, 1987). Whitfield (1987) lists the major topics considered by the NII under the heading of ergonomics as: allocation of operator tasks; control room design; maintenance facilities and procedures; ergonomics of quality assurance procedures; definition of operating goals; staffing levels; selection and training; operating procedures and documentation; monitoring of personnel performance; justification for omission of human actions from fault and event trees; operators as initiators of events; cognitive/conceptual errors; maintenance errors; and task analysis for safety actions. Whitfield suggests that many of these areas were specifically addressed in the NII audit of part of the Selafield nuclear reprocessing plant (HSE, 1986; cited by Whitfield, 1987). Other writers too have discussed human factors relating to safety in the nuclear industry without reference to any compensatory actions on the part of operatives (Marshall, Duncan and Baker, 1981; Marshall and Baker, 1985). Marshall and Baker, for example, see the contribution of human factors to nuclear safety in terms of automation (taking decision-making responsibility from the operators), information presentation (within which they discuss the alarm reduction question), and training. Other researchers have focused attention on the processing of alarm information, modelling alarm-initiated activity (Stanton and Booth, 1990).

Safety targets for nuclear regulatory control are reported by Whitfield. It is noted, for example, that, for a station such as Sizewell 'B', the frequency of a limited release of radioactivity into the environment should be no more than 1 in 10,000 per reactor year.

What is clear from the research activity of Whitfield, Marshall and Baker, Stanton and Booth, and others, is that the emphasis in nuclear regulatory control is very much on the intrinsic side. Nowhere in these discussions is RHT or the possibility of behavioural compensation for intrinsic risk improvements, even mentioned. This is not to say that the human factors perspective is ignored, for it is not; rather, through selection and training, system design, operator-plant interface, alarm reduction and automation, effort is made to reduce operational as well as intrinsic risk.

Given then that the nuclear industry has not appeared to have taken on board the major tenets of RHT, and instead, tried to reduce accident risk through intrinsic improvements, have these efforts worked, or, as RHT would perhaps predict, have they been negated through behavioural adjustment? The question is impossible to answer. Not only does accident loss arising from human error in nuclear regulatory control suffer from the same difficulties as the road traffic situation discussed in chapter 1, but with the nuclear plant, accident loss, at least in terms of frequency, does not occur often enough to draw from it any pattern that might support or refute RHT. The simulation of the nuclear power plant control room has already been undertaken within several research projects (Marshall and Baker, 1985; Page, 1983; Stanton, 1992; Whitfield, 1987). These simulations vary both in their development costs, and, perhaps consequently, in their physical fidelity. Whilst Marshall and Baker proudly report that their simulation is '*vitually indistinguishable from the real plant*', the simulation developed by Stanton, and used in this study, has relatively low physical fidelity, and mimics just a small part of the operators' responsibility. Never before has it been used to test RHT predictions. Just as in the Aston Driving Simulator, the NPPCRS reported here attempts, then, to increase the probability of accident loss, thus enabling the analysis of 'collapsed experience'.

The study reported here then has several aims. First, it seeks to examine once again the possible interaction between utility and intrinsic risk. But rather than investigating the interaction within the risk-taking environment, it seeks to examine evidence for it through the pathway of avoidance. Second, the study is concerned with risk-taking behaviour in an environment in which intrinsic risk reaches such high levels that behavioural adjustments within the risk-taking environment are not capable of eliminating accidents, and in which quite high levels of accident loss are inevitable. In study 1, the high risk condition did not inevitably lead to high levels of accident loss, but only did so in interaction with specific behavioural decisions, such as electing to carry out a high-risk overtake. Third, the study aims to extend RHT research beyond the road-traffic environment and into a more general physical risk-taking environment.

The study attempts to operationalise intrinsic risk in two ways: through the presentation rate of alarms, and through the ratio of alarms to non-alarms. Initial exploratory work showed that very high levels of accident loss correlates were associated with the higher levels of intrinsic risk. This would indicate that restoration of an acceptable level of risk can only be achieved through avoidance of the risk-taking environment altogether. In examining this possibility, proposition 2 from Wilde's 1988 RHT paper is being tested.

Links with previous study

Study 1 involved making a relatitvely subtle change to environmental risk. It provided some support for RHT over specific behavioural pathways within the risk-taking environment of simulated road traffic. It found no evidence for an interaction between environmental risk and utility. Building on this, study 2 aims to examine whether the interaction of environmental risk and utility might exist, not within the environment, as study 1 tested, but outside it, on the pathway of notional avoidance. In addition, it seeeks to extend the evidence for RHT in a road traffic situation, to another specific case of physical risk-taking - the nuclear power plant control room.

A criticism of study 1 was that different levels of environmental risk were not sharply differentiated. Addressing this criticism, study 2 operationalises environmental risk in two ways, each sharply differentiated.

Study 1 took only one measure of attention - postion on road. This is open to criticism in that position relative to centre-white line is not quite equivalent to a tracking task. For one thing, frequent overtaking can affect the measure, whilst tracking performance remains unaffected. For another, style of driving can affect the measure; different styles can produce different results on this measure, whilst not violating the rule of not crossing the centre-white line. Since attention is of obvious interest as a behavioural/cognitive pathway to homeostasis, its effective measurement is desirable. Study 2, though a spatial decision task, addresses this shortcoming of study 1.

5.4.2. Method

Participants

Forty-five participants took part in this study. All were first year psychology students from The University of Aston and were aged between 18 and 39 years.

Equipment

45 Macintosh IIsi microcomputers were used in the NPPCRS. Each had a program simulating a nuclear power plant control room. This program was originally coded in Supercard 1.5.

Design and Procedure

Two tasks were performed - a primary and a secondary task.

The Primary Task

This was a matching/categorization task. To the right of the screen a number of alarms were presented. To the left of the screen four target boxes and a non-target box were shown. The participants' task was to categorize the top, highlighted alarm to the right of the screen as either one of the targets, or as a non-target. This was achieved by moving a cursor by a mouse control to the appropriate selection box and clicking the mouse control.

The Secondary Task

In addition to the primary task, participants were asked to carry out a secondary spatial decision task. For this, a stick-figure holding an object in one hand was presented to the left of the screen. To the right of the screen a second figure was shown, but on a different rotation from the first. The task was to make a decision as to whether the figure matched or did not match. The direction buttons of the Macintosh were labelled 's' and 'd' (same and different). After pressing one of these buttons a new rotation was presented, and so on. The secondary task could therefore be categorized in terms of number of attempts and task accuracy.

General

In addition to the instructions reproduced below from the screens, participants were told that the alarm to which they should refer at all times is the top highlighted alarm. This information was given three times: before the primary task practice, before the combined task practice, and before the combined task proper. Participants were also told that the primary task should, at all times, be given priority.

Participants were informed that a prize of £5 would be given to the best score in each condition, though they were not informed what criteria would be applied to determine 'best' performance. The reason for not disclosing the way in which the performance on the relative measures was converted into a a single measure for comparison was that no objectively correct course of action could be said to exist at the outset. The pathway of avoidance was not included in this prize. The reason for offering a prize at all was to maximise the probability that individuals would be characterised by a target level of risk. That is, it was assumed that individuals would actively seek to avoid accident loss and accident loss correlates when some incentive for high levels of performance is provided.

As shown in the reproduced screen displays, a practice was given for the primary task, the secondary task, and the combination of primary and secondary tasks. During the experimental session two experimenters remained to answer questions arising from the simulation.

The Experimental Design

This was a two factor design, each factor reflecting environmental risk. The primary risk factor was temporal probability of target, which can be thought of as presentation rate. It had three levels: one alarm per 1, 4 and 8 seconds. The second factor was ratio probability of target. This too had three levels: 2%, 6% and 10% of alarms being targets. Presented alarms were categorized as: correct target, correct non-target, incorrect target, incorrect non-target, and missed alarm (a missed alarm is one that scrolled off screen without being processed by the participant).

The NPPCRS was simulated on a Macintosh IIsi microcomputer, the program being coded in Supercard II, but running as a stand-alone package. The following represents the screen displays that appeared during the simulation:

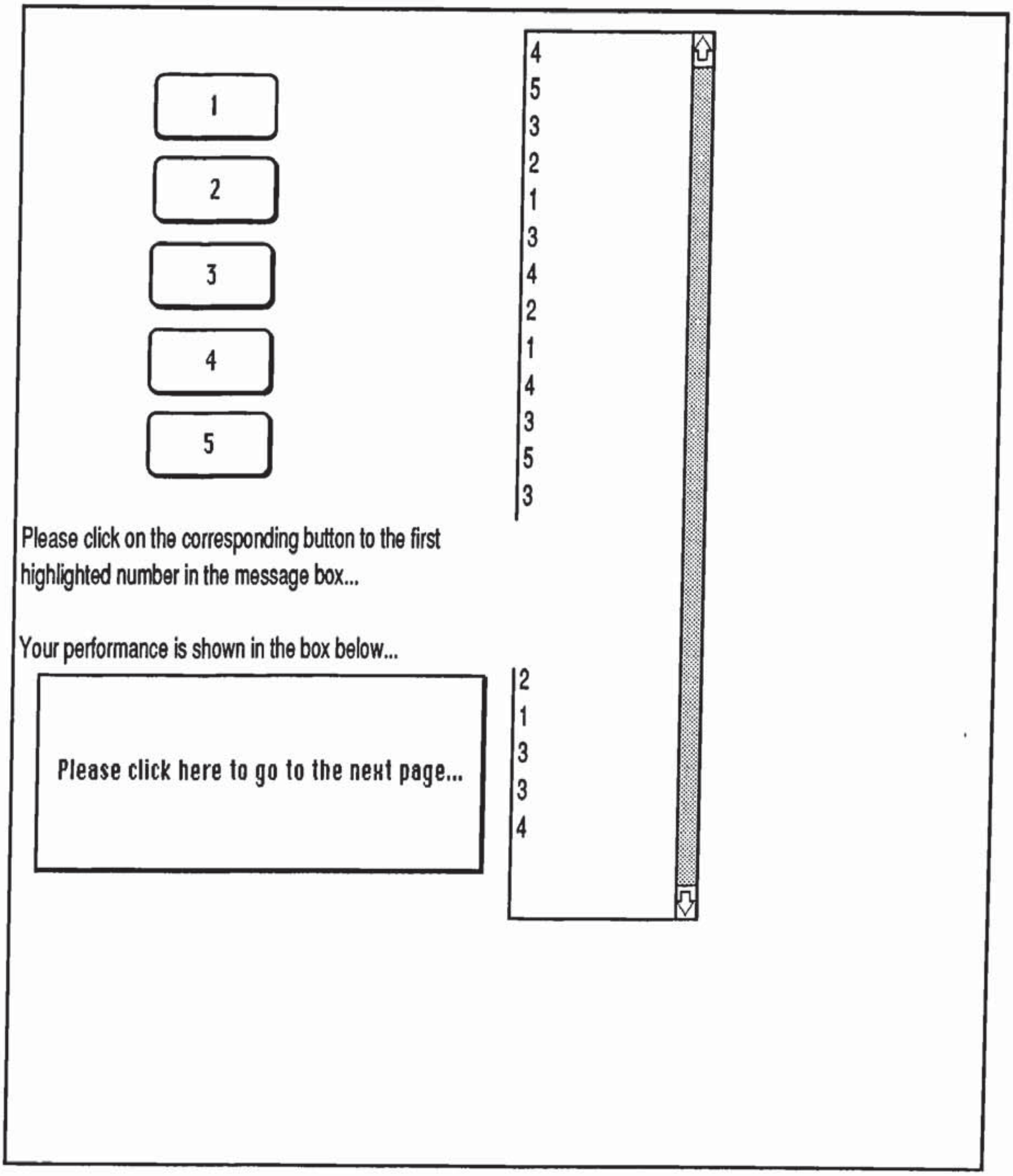
Introduction

In the following experiment you will be required to deal with alarms in a simulated nuclear power plant. The task is to assign appropriately the alarm presented as either a non-target or a target alarm. Missing target alarms in a real nuclear power plant would result in a major incident such as in Three Mile Island or Chernobyl. Please try to behave as if you are in the environment described. You will also be required to attend to a secondary task when the primary task allows you to. Please do this experiment to the best of yor ability. There will be a £5 first prize in each of the nine groups taking part for the best performance.

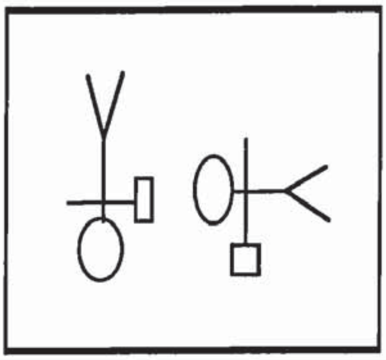
You do not have to remember the instructions at this stage, just understand them.

When you happy that you have understood this introduction click on the button below to go to the next page...

next page...


1
2
3
4
5
4
5
3
2
1
3
4
2
1
4
3
5
3
Please click on the corresponding button to the first highlighted number in the message box...
Your performance is shown in the box below...
Please click here to go to the next page...
2
1
3
3
4

Please press on the SAME or DIFFERENT keys on the keyboard for this task....Your performance is shown in the box above...



Please click here to go to the next page...

Instructions

You will now be required to do both tasks together as part of an experiment into alarms in a simulated nuclear power plant. The task is to assign appropriately the alarm presented as either a non-target or a target alarm. Missing target alarms in a real nuclear power plant would result in a major incident such as in Three Mile Island or Chernobyl. Please try to behave as if you are in the environment described. You will also be required to attend to a secondary task when the primary task allows you to. Please do this experiment to the best of your ability. There will be a £5 first prize in each of the nine groups for the best performance.

Do you understand the instructions? If not please call the experimenter.

When you happy that you have understood this introduction click on the button below to go to the next page...

next page...

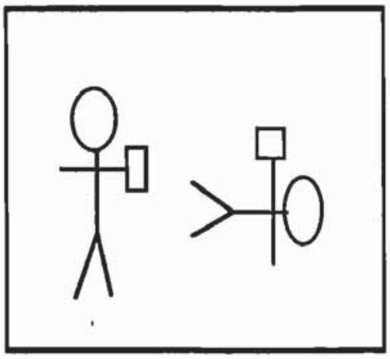
NON TARGET

CORE TEMPERATURE HIGH

LP HTR 1/2 DANGEROUSLY HIGH

PRIMARY CIRCUIT LEAKAGE

DRUM LEVEL HIGH EMERGENCY



DAMPER INTERTRIP INHIBITED
SEAL AIR DIFFERENTIAL LOW
THRUST BRG OIL FLOW LOW OR STR BLKD
DAMPER INTERTRIP INHIBITED
SEAL AIR DIFFERENTIAL LOW
DAMPER INTERTRIP INHIBITED
LUBE OIL PRESS LOW
TURNING GEAR FAILED
GAS IN COOLANT AUTO VENTING
GAS IN COOLANT AUTO VENTING
GAS IN COOLANT AUTO VENTING
HYDROGEN DRYER FAIL
GAS IN COOLANT AUTO VENTING
PRIMARY CIRCUIT LEAKAGE
HIGH PRESSURE GLAND STEAM TEMP LOW
H2 SEAL OIL SYSTEM FAULT
GAS IN COOLANT AUTO VENTING
GAS IN COOLANT AUTO VENTING
GAS IN COOLANT ABNORMAL
PERM MAGNET GENERATOR 71 FAILURE
PERM MAGNET GENERATOR 72 FAILURE
GEN TRANS OIL LEVEL LOW
GAS IN COOLANT AUTO VENTING
TURBINE CONTROL OIL PS FAULTY

Imagine that the task you have just done is your full-time job. On a scale of 1 to 10, how likely is it that you would carry on doing the job for the following rates of pay?

	Not very likely									Very likely
£6000 per annun	1	2	3	4	5	6	7	8	9	10
£15000 per annun	1	2	3	4	5	6	7	8	9	10
£28000 per annun	1	2	3	4	5	6	7	8	9	10

Next page..

Thank you for taking part in this investigation....

Please call experimenter....

Save RESULTS>>>

5.4.3. Results

Of primary interest in this study was the question of whether an interaction would be found between environmental risk and utility on the measure of avoidance. Surprisingly, environmental risk did not produce an effect of avoidance by itself ($F[2,36]=1.227$, NS). Just as in Study 1, although through a different pathway, it would seem that utility and environmental risk do not behave in any multiplicative way. Figure 5.5 shows the effect of environmental risk on the measure of avoidance:

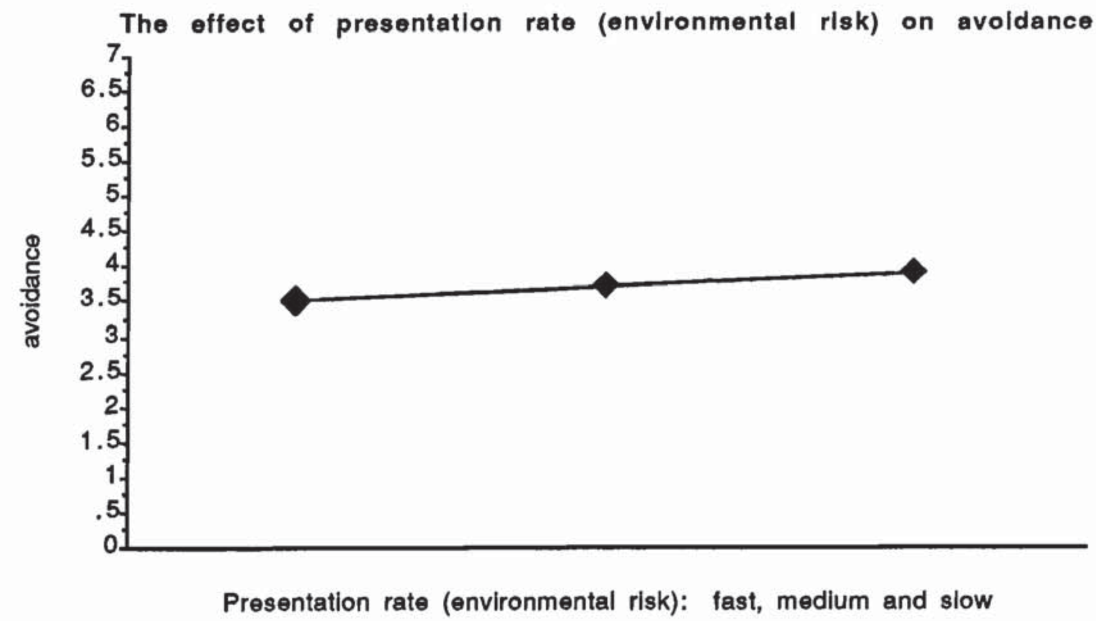
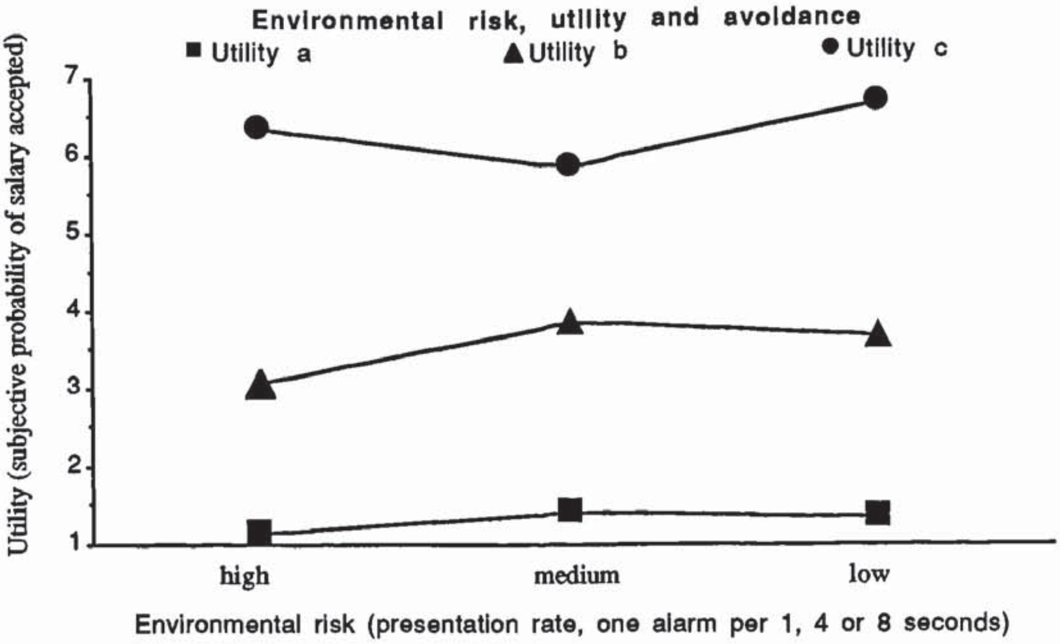


Figure 5.5 : The effect of presentation rate (environmental risk) on the measure of avoidance.

Utility, by contrast, did produce an extremely large effect of avoidance ($F[2,36]=91.4$, $p<.0001$). However, the hypothesised interaction between environmental risk and utility was, just as in study 1, not significant ($F[4,72]<1$, NS). It would seem then, so far as findings from this experiment go, that environmental risk does not in any sense determine a participant's likelihood of removing him/herself from the environment. Figure 5.6 shows the relationship between environmental risk, the utility of remaining in the environment, and avoidance. Figure 5.7 shows avoidance and the effect of utility. In interpreting figure 5.7 it should be remembered that high ratings on the likelihood measure represent low judged probabilities of avoidance. In other words, participants characterised by high scores would be unlikely to remove themselves from the environment on the relevant utility condition.



Utility a: likelihood of remaining in the environment for salary of £6000 pa

Utility b: likelihood of remaining in the environment for salary of £15000pa

Utility c: likelihood of remaining in the environment for salary of £28000pa

Figure 5.6 : Environmental risk, utility and avoidance (low scores represent high likelihood of avoidance).

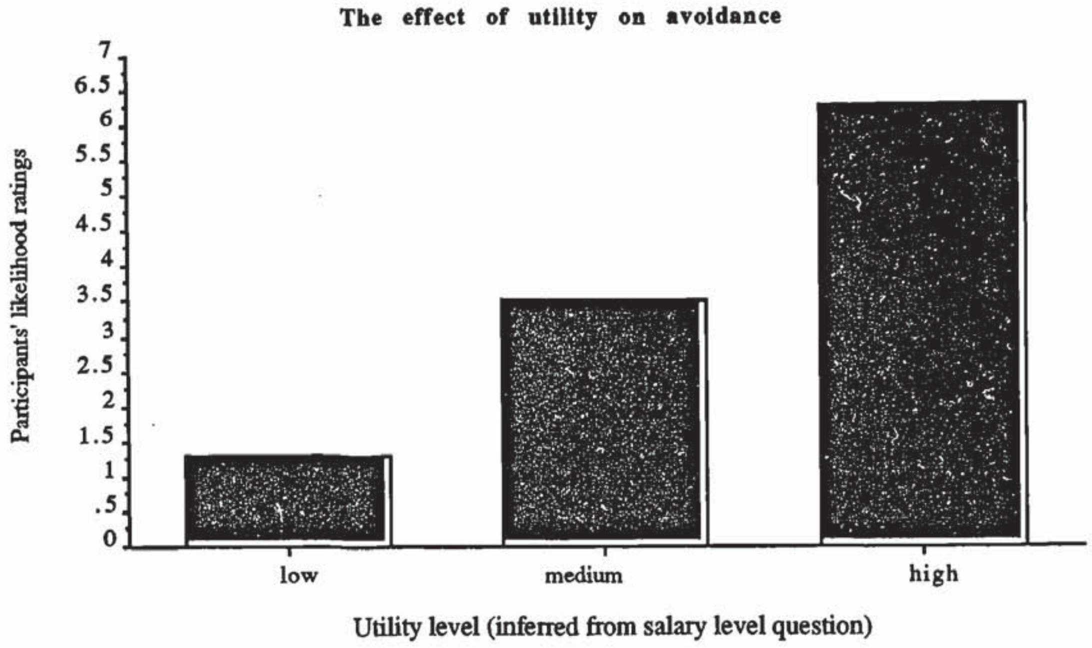


Figure 5.7: The effect of utility (salary for simulated job) and participant rating of likelihood of remaining in the environment.

Before rejecting altogether the possibility that a relationship might exist between environmental risk and utility on the measure of avoidance, a series of correlations were carried out, *post hoc*, between avoidance scores on each of the three levels of utility and four estimates of accident loss: errors including missed targets, errors excluding missed targets, the absolute number of target missed, and the proportion of targets missed to incoming targets. This would appear to indicate that however accident loss is measured, and whatever utility level is examined, there is no relationship between errors (the indirect measure of accident loss) and rated probability of avoidance. The correlations are produced in Table 5.1.

Utility Level (Salary decision)			Accident loss measure
A	B	C	
-.072	-.209	-.081	errors including missed targets
.237	.108	.170	errors excluding missed targets
-.117	-.234	-.107	number of targets missed
-.121	-.238	-.117	proportion of targets missed

Table 5.1 : Correlation coefficients between estimate of accident loss and notional avoidance (none is significant).

One might at this stage ask whether the above finding can be explained in terms of an *operational* negation of environmental risk. Was it the case that all participants were equally safe within the simulated environment?

To answer this question, a measure corresponding to probable accident loss is required. Three possibilities exist. First, the proportion of errors (incorrect hits plus incorrect misses divided by the total of incoming alarms) could be examined to provide an indication of likely accident loss. The problem with this measure is that it does not reflect errors of omission (missed alarms that scrolled off the screen without being acknowledged as either target or non-target). For this reason a second possibility for measuring likely accident loss would be to add missed targets to incorrect hits plus incorrect misses, dividing this total by total incoming alarms. Finally the proportion of missed alarms to total incoming alarms could be examined.

On the first measure of accident loss, the measure excluding missed alarms from the error criterion, the ratio probability factor did have an effect ($F[2,36]=3.274$, $p=.0494$). But the factor of temporal probability, deemed to be the main factor of environmental risk, showed no effect ($F[2,36]=2.316$, NS). The interaction between factors was not significant.

Interestingly, when one includes missed alarms in the error criterion, the significance of temporal and ratio probability are reversed. Ratio probability is now non-significant (F[2,36=2.614, NS). Temporal probability moves from non-significance to F[2,36]=12.902, p<.0001. Again, there is no statistical interaction. Proportionately far more errors are recorded on the one-second presentation rate (the highest level of environmental risk) than on the four second and eight second rates.

When looking at the proportion of missed alarms there is again no ratio effect (F[2,36]=1.505, NS), but again there is a very large effect of temporal probability (F[2,36]=10.214, p=.0003). Once more there is no evidence for a statistical interaction of the two factors. Figure 5.8 shows the three estimates of accident loss in relation to presentation rate (environmental risk):

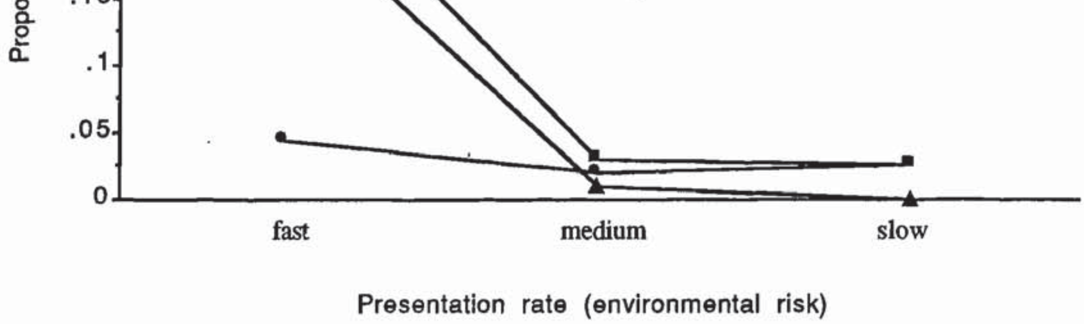


Figure 5.8 : Presentation rate (environmental risk), proportion of errors and the three estimates of accident loss: errors excluding missed targets, proportion of missed targets

to total messages, and errors including missed targets.

Secondary task performance

There are two ways of examining secondary task performance: in terms of the number of secondary tasks attempted or in terms of the error rate on those which were attempted. For the total secondary tasks attempted there was no effect of either ratio or temporal probability ($F[2,36]=1.431$, NS and <1, NS respectively). For secondary task error rate the same was true with ratio probability giving ($F[2,34]=1.942$, NS), and temporal probability giving ($F[2,34]=1.862$, NS). In terms then of both error rates and total output, it seemed that participants were as accurate and as productive in the secondary task whatever the level of primary task demands made of them. The implications of these findings and a possible explanation of them is given in the discussion.

5.4.4. Discussion

This is the first study to find evidence that is, on the surface at least, *against* risk homeostasis theory. In it, participants neither adjusted their behaviour within the environment such as to negate the adverse level of environmental risk, nor did they report that they would be more likely to leave the environment when it was more hazardous. On all relevant measures, when environmental risk was at its greatest, expected accident loss was at its worst. Therefore one could expect that improvements in environmental safety would result in commensurately reduced accident loss statistics.

The results of this experiment then would seem to point to one of two possibilities. Either this particular simulation was, for whatever reason, inappropriate as a tool for the investigation of RHT (these findings, in other words, have no *practical* relevance), or, and again for whatever reason, this experiment, though accurate in its relationship with the real environment it set out to model, represents a situation in which risk homeostasis does not usually occur.

To start with the first of these possibilities, one must recall that the ADS simulation was successful in finding evidence for RHT : was it the case then that the NPPCRS in some way failed as a research tool where the ADS had succeeded? There are at least two reasons why this possibility, though it cannot be completely rejected, seems unlikely. First, feedback given by participants after carrying out experiments was by and large more positive on the NPPCRS than on the ADS. Whereas the ADS was said to be "*.....not like a car at all....*" and whilst participants claimed it was "*.....quite unrealistic*", no such comments where given when using the NPPCRS. Perhaps this was because whilst all the participants concerned with the ADS studies had experience of driving a car and were therefore in a position to make meainingful comparisons, none of the participants in the NPPCRS study had experienced a nuclear power plant control room, and were therefore unable to make any sort of comparison. Second, in the ADS studies, risk homeostasis was only examined through behavioural adjustments *within* the environment. It may be that some participants were unable to compensate within the environment and would, given a choice, which under experimental conditions they did not have, remove themselves from the environment all together, or travel by some alternative means of transport. In the NPPCRS study such notional compensation *external* to the environment was, for the first time, possible and measurable. In that sense one could say that the NPPCRS represented a more comprehensive and realistic investigation tool. To these two reasons one might add a third. In as much as physical risk was simulated, the NPPCRS represented the simulation of *more* physical risk in that accident loss here could be in any quantitative sense much greater than anything one might envisage in a driving environment following from errors in the behaviour of a single driver. If in both situations some element of that belief was simulated, then in the NPPCRS one might expect the exercise to have been taken more seriously.

Might it then have been the case that the NPPCRS study failed to uncover evidence for RHT simply because the environment simulated here is not characterised by a risk homeostasis process, at least in the short term? Although this is the first study to uncover what is, at first sight at least, evidence that could be interpreted as being against RHT, it is also the first study deliberately set up to prevent a complete negation of the environmental safety change from within the environment. The reason then, that no negation took place from within the simulated environment is, one imagines, that such negation was not possible. (To put it bluntly, participants carried out either a *possible* task, or one of two *impossible* tasks.) This only leaves the measure of avoidance to be explained in which a negation of the environmental risk change was possible by participants. This notional 'avoidance' pathway would be equivalent to participants saying, in effect, that whilst they could not maintain a constant level of accident loss *within* the environment, they could and would get out of that environment. This did not happen. On the measure of notional avoidance participants did not significantly react to the level of environmental risk, and when the correlations between the various measures of accident loss and avoidance scores were examined, these correlations too failed to reach significance. In other words, participants who had made a large number of errors were not significantly more likely to report that they would leave the environment than participants making fewer errors. To some extent this might be explained in terms of removal of feedback, for feedback of false negatives and false positives (active errors) was not given. Perhaps then participants were not aware that they were making errors. However, this explanation is inadequate in as much as feedback of *passive* errors was given. Missed targets were highlighted as they scrolled off the screen, making participants aware that a passive error had occurred. Even when the error criterion includes passive errors (or, for that matter, consists *entirely* of passive errors as in the dependent measure of missed targets), the correlation between the error criterion and avoidance, across all three levels of utility, is not significant.

The conclusion from this would seem to be that where behavioural compensation within the environment is not possible, participants show a marked reluctance to engage in external compensation by removing themselves from the environment. Tentatively, then, one might conclude that where environmental safety standards fall to a level at which participants are unable to change their behaviour within the environment to negate the change (examples here might include particularly unfavourable weather conditions, snow and ice etc., or, as simulated here, increases of a certain magnitude in mental workload demands), the change in environmental risk will be matched by a commensurately large increase in accident loss. The other side of the coin, of course, would be to say that where the level of environmental safety is such that accident loss inevitably characterises the environment (the same examples as above applying), and environmental safety then changes such that the pre-existing level of accident loss is no longer inevitable, then the environmental improvement will lead to a *decrease* in accident loss. Perhaps then, in these circumstances at least, an engineering solution to the operational safety question would be possible.

Another interesting feature of this experiment is the complete failure of the secondary task to differentiate between any levels of the ratio probability factor or temporal probability factor. This was true both in terms of total output (putting aside accuracy) and in terms of accuracy (putting aside total output). This finding was intitially difficult to reconcile with the design, since it would seem to imply that the attentional demands made in the primary task do not affect performance, in any way, on the secondary task. It would seem, by extension, to indicate that an alarm presentation rate of one per second demands no more resources than an alarm presentation rate of one per eight seconds. Even over the N of 45, there was no evidence even of a non-significant trend on any comparison.

What is in all probability the answer to this enigma was put to one of the experimenters sometime after the experiment by a participant, and at a time when re-analysis and a checking of raw data was actually taking place. It was suggested, from the participant's own experience, and from talking to other participants, that the higher levels of demand in the primary task were just too difficult. Rather than attempt to do the impossible, to cope with a presentation rate of, say, one alarm every second, participants stopped even trying to do the primary task and put all of their efforts into the secondary task, which was, after all, considerably easier. This, it was suggested, allowed participants at least to salvage something from the study (there was of course, the possibility of a prize to consider). In view of this, it would in all probability be inappropriate to consider further the meaning of the secondary task results. They were, it now seems, an artifact of the experimental design.

It has already been said in chapter 1 that it is difficult to envisage a finding that would serve to falsify RHT. But if RHT can reconcile itself with any finding, in any conditions of dynamic environmental risk, how might it be reconcilable with this finding, which on the surface at least would appear to contradict it? What is clear from this study, it must be remembered, is only that a constancy of *actual* risk was not observed. But RHT does not necessarily predict risk constancy, and in cases where the target level of risk of those individuals affected by the environment changes also, then a change in actual risk would in fact be predicted by the theory. So could it be argued that the target level of risk across the differing conditions of environmental risk was not constant? In answering this question it will be recalled that Wilde (1988) suggests that the target level of risk comes from four relevant utilities: costs and benefits of relatively cautious behaviour and costs and benefits of relatively risky behaviour. Perhaps then it could be argued that on the pathway of avoidance - leaving the environment all together - the costs of relatively cautious behaviour have risen, and stand at much higher levels than those that would be associated with behavioural adjustments within the environment. This being so, one might predict that the target level of risk would change such that individuals would be prepared to accept higher levels of accident loss, precisely what has happened here! This point is discussed further in the conclusions of this thesis.

5.4.5. The importance of an avoidance measure

The findings from study 1 and study 2 do have an important implication for RHT research as applied to the effects of age. In Part 1, it was noted that older workers and road users are at a greater risk than their younger counterparts. In fact, this is only partly true. To be sure, the skills that are prerequisite to safe driving do deteriorate with age -(Planek, 1981; Salthouse, 1982; Summala and Koivisto, 1989; Welford, 1985). However, per distance unit travelled, older drivers face a fatality risk anywhere between 2 and 5 times that of younger adults (OECD, 1985; Van Wolffelaar, Rothengatter and Brouwer, 1987). But on other measures such as time unit of exposure, there is evidence to suggest that accident rates for older drivers are either only slightly greater than, or not at variance at all with, younger adult drivers (Evans, 1987; Summala and Koivisto, 1989). It has been suggested that the latter can be accounted for by a compensation mechanism of either avoidance or mode migration (Summala and Koivisto, 1989; Van Wolffelaar *et al.*, 1987). Clearly, if the older road user (or older worker) cannot perform a homeostatic operation via behavioural adjustments within their risk-taking environment, the examination of compensatory behaviour in a simulated environment in which avoidance and mode migration are not possible would be inappropriate. Designs such as the NPPCRS, with some measure of notional avoidance, may then be of real value in testing RHT hypotheses in simulated environments.

Chapter 6 - An examination of RHT in relation to non-specific changes in the level of environmental risk.

6.1. Study 3

6.1.1. Introduction

This study is concerned with the environmental safety relating to the side of the road on which participants drive. It takes as its starting point the Swedish experience of September 1967 in which a change from driving on the left hand side to driving on the right hand side of the road was not matched by an increase in accident loss. Both Adams (1985a) and Wilde (1982a) report the Swedish experience as support for the RHT case. In fact, when the Swedes changed from driving on the left hand side to driving on the right hand side of the road, this initially led to a 40% decrease in road accident fatalities, relative to the average level up until that time. Unfortunately, over the following period, accident loss crept up until by the end of a two year period, road fatalities no longer differed significantly from their pre-intervention level (Alexandersson, 1972). In fact, the precise time-lag for the readjustment is the subject of some debate. Adams (1985a) argues that some considerable movement in the direction of return to baseline conditions occurred in the space of just a month. On the measure of fatalities, accident loss had risen rapidly, even by 1968. The remarkable adjustment to this change in environmental risk led Adams to make the rather tongue in cheek statement:

"If all roads were to be paved with a substance having the same coefficient of friction as ice, the number of people killed on the roads would be substantially reduced." (Adams, 1985a, p45).

So perhaps on the surface, the Swedish experience represents clear and unequivocal support for RHT. The level of environmental risk had, it seems, increased, yet the level of accident loss, which, from the engineering model, would be predicted to rise (using the term 'engineering' quite loosely), showed an initial and significant decrease (over-compensation, it would seem), before before eventually returning to its pre-intervention level.

However, a certain amount of caution is required in interpreting this pseudo-experimental design, for it suffered from something of a confounding variable. Along with the changeover of the side of road on which road users were to drive came new speed regulations. In some cases this reduced traffic to just 40 km/h. There is also evidence to suggest that the level of enforcement of these change-over speed laws differed from that associated with the pre-intervention situation (see Summala, 1985; McKenna, 1988).

The side of the road on which participants are asked to drive, continental versus British conditions, represents a special case of environmental risk. First, it points up the importance of the distinction between environmental risk, which can be thought of as a *ceteris paribus* assumption of accident loss, and intrinsic risk, in which an environment is objectively less safe unless behavioural changes are made. It would be difficult to argue that the side of the road on which participants are asked to drive has anything to do with their intrinsic safety, since the left hand side holds no more *intrinsic* risk than the right hand side of the road. Yet, used as we are to driving on one side of the road rather than the other, it is a reasonable *ceteris paribus* assumption that accident loss will increase unless behavioural adjustments are made. Certainly this has been the logic on both sides of the debate. Second, the nature of the compensation that might match this change in environmental safety is far from clear and would not appear to be local to any specific feature of the driving environment. However, one might hypothesise that compensation to this change would be at least attentionally-related (it may also include speed adjustments, but perhaps these would appear only as a consequence of changes in attention). To investigate these hypotheses the Aston Driving Simulator was again employed. Attentional pathways to homeostasis were investigated on two measures:

position on road and secondary task.

Once more, proposition 3 (stating that individuals are characterised by a target level of risk) and proposition 13 (stating the interventions that do not change that target can be expected in the long term to have no effect on accident loss) from Wilde (1988) are being tested.

Links with previous studies

Studies 1 and 2 could both be described as involving intrinsic changes to environmental risk. That is, in purely objective terms, different levels of intrinsic risk existed within the first two studies. Study 3 served two purposes not met by either study 1 or study 2. First, it examines responses to an environmental risk change in which no change to intrinsic safety has been made. Second, it exists to clarify a debate that already exists in RHT through the Swedish experience and that has been answered by imperfect accident loss data, contaminated by other factors such as speed restrictions.

Study 1 failed to take any effective measure of attention. Study 2 addressed this shortcoming through the employment of a secondary task, but in the light of the excessive primary task demands of this study, this study too failed to measure effectively the attentional differences of participants across different levels of environmental risk. Study 3 attempts once again to take measures of secondary task performance in order to estimate attentional differences between environmental risk conditions. Two possible reasons might explain the failure of study 2 to detect attentional differences via secondary task performance. One is that the very high levels of workload associated with the high risk conditions were in fact too great and should be reduced if attention is to be measured effectively. Another reason might be that the particular secondary task chosen - a spatial decision task - was not suited to the inferential measurement of attention. These two possible flaws in study 2 were taken account of in study 3. First, the high levels of mental workload were reduced. This was achieved by moving out of the alarm-handling environment back to the ADS. Second, the spatial decision task was replaced by a *secondary task that has in the past shown itself to be sensitive in measuring attention in RHT research (Hoyes, 1990). It is therefore hoped that study 3 might detect any attentional differences existing across different levels of environmental risk.*

6.1.2. Method

Participants

Forty-one participants took part in this study. They were students at Aston University.

Equipment

The Aston Driving Simulator was used for this study. In addition, a cassette recorder was used, placed by the side of the simulator to record participants' secondary task performance.

The secondary task

Since the primary task involved driving the ADS the secondary task to be chosen should not interfere with the local or mechanical demands of this (see Broadbent, 1987; Ogen, Levine and Eisner, 1979). For this reason an auditory task was given to participants: articulating the letters 'a' to 'z' with their corresponding alphabet positions (eg A,1,B,2,C,3......etc). The task was also chosen because it had in the past proved to be a particularly sensitive measure of attention in simulated RHT research (Hoyes, 1990). Participants were asked to say these out loud into a cassette recorder placed by the side of the simulator.

Design and Procedure

The design was a simple repeated measures with two variables: side of road and block.

For this study, the ten minute runs were divided into five blocks of equal duration. Since the factor of block by itself was of little interest here, but rather, its interaction with side of road, its employment was felt to be justified; it did bring with it the advantage that the developmental nature of behavioural adjustments could be examined.

Participants were informed that they would be required to drive for a distance of 7.2 miles. This was to provide a positive utility for any risk-taking behaviour; the choice of the operationalisation of utility was taken from study 1.

After being informed of the secondary task requirement, those participants who failed to engage in the task were reminded that they should do so.

Instructions to participants

The following instructions were read out to participants:

"You are seated in front of a driving simulator which is intended to mimic an ordinary automatic car. The left pedal will function as the brake, the right pedal as the accelerator, and the view through the windscreen will be displayed upon the monitor in front of you. The steering wheel will function in the same manner as an ordinary car.

Your task is to drive (as you would normally drive a real car) along the side of the road upon which you have been placed, that is, either within a continental driving condition or a British driving condition, for a distance of 7.2 miles.

The car will automatically stop when this distance has been covered. Should you crash, or veer off the road, you will be re-positioned on the road and you may continue driving.

Whilst driving, you will simultaneously perform a secondary task. Speaking clearly, you are required to recite letters of the alphabet paired with a number representing that letter's place in the alphabet. For example: A1, B2, C3, D4.

When you reach the end of the alphabet (ie Z,26), repeat the task by beginning again at A1. Continue to do this until the simulation is complete.

Priority ought to be given to driving the simulator safely rather than the successful completion of the secondary task. Please treat the experiment seriously and drive, so far as is possible, as you would normally drive a real car.

If you have any queries regarding the experiment, please ask the experimenter before you begin. OK?"

6.1.3. Results

Two-variable, repeated measures ANOVAs were carried out for all the dependent measures provided by the ADS. Additionally, a number of related t-tests was calculated for participants' secondary task performance. Effects of block are reported here for completeness, since their interactions are reported, but care should be taken in interpreting block as a main effect (see earlier discussion).

Speed variability showed a relatively large effect of risk ($F[1,40]=9.59$; $p=.004$). However, block x risk did not produce an effect ($F[4,160]<1$; NS). The same was true of block as a main effect ($F[4,160]=1.52$; NS).

For mean speed, the picture is slightly different. Here, risk by itself did not produce a main effect ($F[1,40]=1.1$; NS). Block too did not produce an effect ($F[4,160]=2.05$; NS). But on this measure a very large effect of block x risk was seen ($F[4,160]=7.09$; $p<.0001$). Figure 6.1 illustrates this:

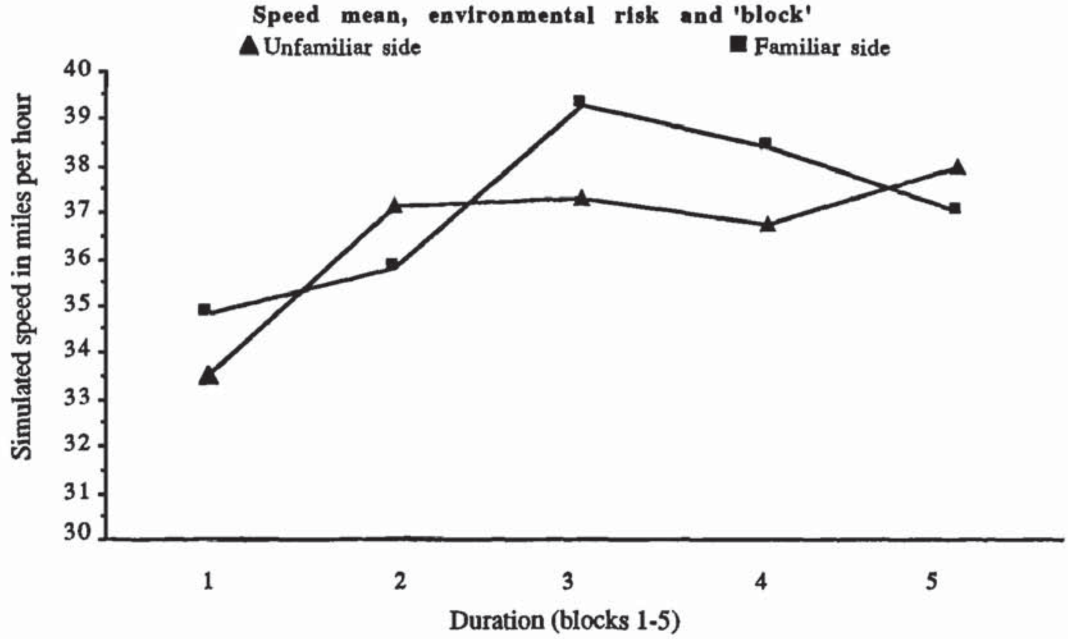


Figure 6.1: Mean speed, duration (blocks 1-5) and environmental risk (side of road on which participants were asked to drive).

NB - Suppressed origins were used for all graphs comparing different conditions of environmental risk in order to show, as clearly as possible, the interaction of environmental risk and behavioural compensation over duration.

For accelerator variability, a related measure, risk and block are again both non-significant, each producing F ratios of <1. The block x risk interaction is, however, only marginally significant ($F[4,160]=2.48$; $p=.046$), as illustrated in Figure 6.2:

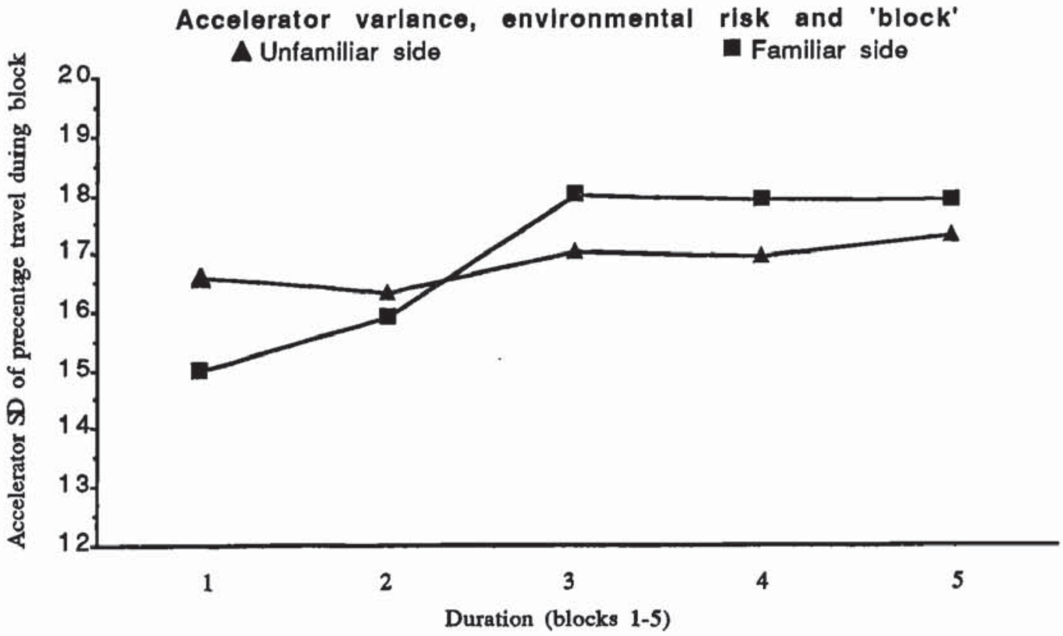


Figure 6.2: Accelerator variability, duration (blocks 1-5) and environmental risk (side of road on which participants were asked to drive).

All comparisions of accelerator mean are non-significant.

Mean braking does not show any effect of risk or block x risk interaction. Only block by itself had an effect (F[4,160]=3.36; p=.011).

Brake variability showed no effect of risk (F[1,40]=2.02, NS). Again, there was a main effect of block (F[4,160]=4.36, p=.002). There was also a block x risk interaction (F[4,160]=4.15, p=.003), shown in Figure 6.3:

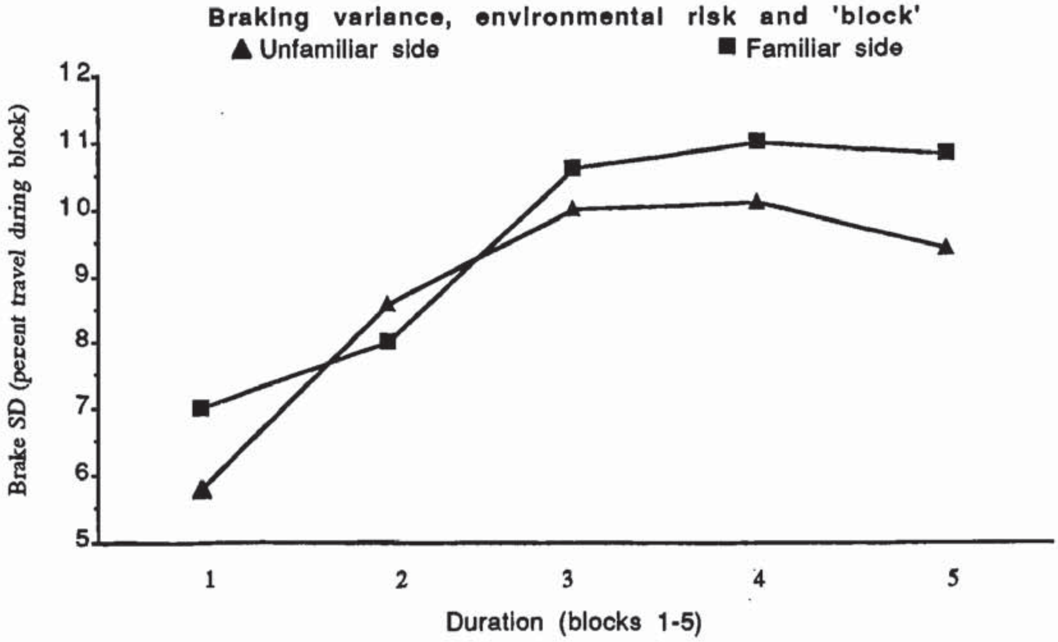


Figure 6.3: Braking variability, duration (blocks 1-5) and environmental risk (side of road on which participants were asked to drive).

Kerb collisions also showed a main effect of block ($F[4,160]=7.34$, $p<.0001$). As RHT would predict, risk and block x risk interaction were both non-significant.

The main measure of accident loss, other-vehicle collisions, showed no effect of risk, block, or block x risk interaction. This would be consistent with RHT predictions, since individuals would appear to have adjusted their risk-taking, and consequently, their accident loss, so that the difference between operational risk across the two levels of intrinsic risk tends towards zero.

Overtaking (the number of overtakes observed) provides perhaps the most interesting finding of this study, certainly in terms of strength of effect. Block was significant ($F[4,160]=13.33$, $p<.0001$). Risk too was significant ($F[1,40]=10.32$, $p<.003$). The interaction of these two factors was also significant ($F[4,160]=4.21$, $p<.003$). What this means is that when driving on the usual side of the road participants carried out more overtakes. Once again, to reconcile this with the concept of utility is problematic, since there were no obvious benefits to be had from carrying out fewer overtakes when driving on the unfamiliar side of the road. Figure 6.4 illustrates the environmental risk/duration interaction:

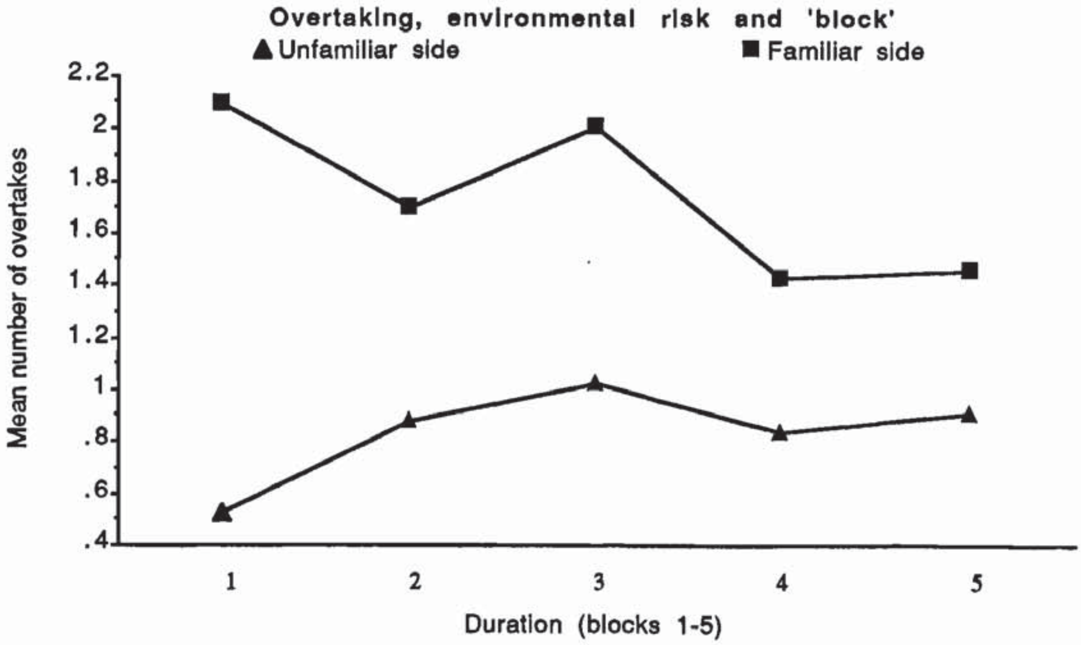


Figure 6.4: Mean number of overtakes, duration (blocks 1-5) and environmental risk (side of road on which participants were asked to drive).

The variable of maximum speed showed no block effect or block x risk interaction. It did, however, show evidence for risk as a main effect ($F[1,40]=5.59$, $p=.023$). When driving on the familiar side of the road, maximum speed is greater than when driving on the unfamiliar side of the road.

Distance mean (distance to car in front) showed an effect of block ($F[4,160]=3.67$, $p<.007$). Neither risk nor the block x risk interaction were significant.

However, the related measure of distance variability did not show a block effect. Risk by itself did not show a main effect but did show a block x risk interaction ($F[4,160]=3.4$, $p=.011$ - see Figure 6.5:

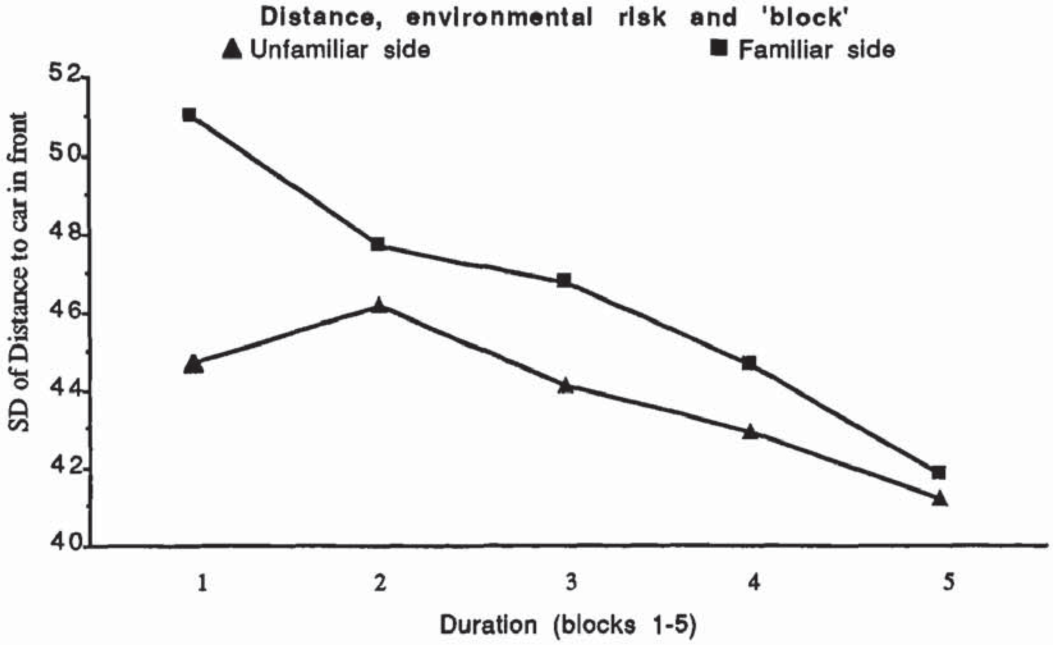


Figure 6.5: Distance variability, duration (blocks 1-5) and environmental risk (side of road on which participants were asked to drive).

6.1.4. Discussion

In as much as a change in the side of the road on which participants are required to drive represents a change to their physical risk, this experiment provided no evidence to contradict RHT hypotheses. RHT would predict a rough constancy of accident loss across these conditions together with some evidence to show significantly different driving strategies through which to maintain this constancy. Both these predictions were met. Several interesting points do, however, deserve special mention.

First, there is relatively little evidence here to suggest that the behavioural pathway of speed is particularly important in carrying the homeostasis effect. Indeed, across the two conditions of environmental risk, there was no significant main effect of risk on the pathway of mean speed. This suggests that the emphasis on speed as a means of possible refutation of the essential hypotheses of RHT (see, for example, Lund and Zador, 1984) is unwarranted.

Second, although accident loss, as reflected on the errors criterion, is more or less constant across the two levels of environmental risk, and although this is perfectly compatible with RHT, it does not parallel the Swedish experience. Were it to do so, one would, over such a short time scale, expect to see significantly reduced accident loss on the high environmental risk condition, rather than roughly equal accident loss. A number of possbilities exist to explain this. It may be that the Swedish experience saw accident loss reductions through the behavioural pathways of avoidance and/or mode migration. These pathways of course had no parallel in the simulated exercise presented here in which there was no avoidance option. Alternatively, it may be that the imposition and assiduous policing of the speed restrictions in Sweden served to reduce accident loss. Again, and by design, this was not a feature of the simulated environment of the ADS. Another possiblity may be that the ADS exercise was limited in the road traffic environments it presented to participants. For most of the run, participants were faced with driving on a stretch of road with no intersections and light traffic. It may be that only at the margin is there strong compensatory behaviour that perhaps goes further than merely restoring the previously existing level of accident loss. For example, particularly difficult situations such as dealing with a roundabout or a left hand turn into traffic may bring about overcompensation.

Third, it is interesting to note that the attentional demands of the driving task, as reflected in the secondary task performance, would seem to be roughly equivalent across different conditions of environmental risk. Once again, this may reflect the fact that the more difficult aspects of 'cognitive reversing' were not included in the task and that for the most part the task did not make high attentional demands of participants. The alternative interpretation, that the secondary task was not particularly effective in measuring attentional resources seems unlikely in that the task has in the past proved particularly sensitive in measuring attention in simulated physical risk-taking (Hoyes, 1990), and that no apparent local or mechanical interference between primary and secondary tasks would appear to have been present.

Finally, just as in study 1, whilst mean speed escaped the main effect of environmental risk, the associated measure of maximum speed did not. At the margin, therefore, participants were prepared to reach, if not maintain, greater speeds where environmental risk was low. Once again this points up the difficulty of examining RHT hypotheses in the field with the crude measure of mean speed or momentary speed and suggests that to understand the nature of the compensation process, a simulated environment is perhaps a more appropriate research strategy.

6.2 - Study 4 - An examination of a low fidelity simulation device

6.2.1. Introduction

In occupational psychology, in the field of training, the question of fidelity of simulation has attracted much attention. One conclusion from this debate is that low-fidelity simulation can be as effective as high fidelity simulation so far as transfer of skills goes (see Stammers and Patrick, 1975). Expensive apparatus and state of the art electronics might look very much better than a simple pencil and paper exercise, and in a functional sense might have very much more to offer. But if the functional benefits are not relevant to the skill transfer in question, the extra expense may very well be wasted. There may be a parallel in the investigation of physical risk-taking behaviour in that risk homeostasis hypotheses might potentially be examined in a low fidelity context. Studies 4 and 2 represent attempts to investigate RHT hypotheses outside the relatively high-fidelity environment of the simulated driving experience captured on the Aston Driving Simulator.

Study 4 attempts to examine, in essence, very similar dependent measures to those taken on the ADS. But rather than measuring directly those variables that might be expected to respond to changes in environmental risk, study 4 attempts to examine the pathways by asking participants: *"what would you do if......"* and then providing a behavioural checklist for participants to complete.

There are two questions one would want to ask of such an experiment. The first of these is simply answered empirically, and it is: will the experiment provide evidence of specific behavioural adjustments in response to changes in environmental safety? Secondary to this might be questions of what exactly the behavioural pathways are, whether they are reconcilable with the concept of utility, whether they correspond to the pathways identified in the ADS studies, and so on.

A further question that could not easily be answered is: to what extent does this low fidelity simulation parallel the actual environment it sets out to simulate? All one can do here is to take the evidence from low and high fidelity studies and construct working models of the processes, and then test those models in environments of real physical risk-taking. Later in this thesis, attention will be given to the factor structure of this low-fidelity approach in comparison with the factor structure of the Aston Driving Simulator. Confirmation that a low-fidelity simulation is characterised by a similar factor structure would serve to indicate that the two devices may measure, in essence, the same responses. Attention will also be given to the question of whether a low-fidelity simulation can differentiate betweeen the same groups of road-users as the Aston Driving Simulator (young and old drivers and male and female drivers).

Study 4 has another aim. In addition to exploring RHT in a low-fidelity environment, it seeks also to examine responses to a special case of intrinsic risk, and one which is qualitatively different from those examined thus far - ambiguity. This factor is significant in that if behavioural adjustments are for the most part local to the nature of the change in risk, then ambiguity (a special case of physical risk) should be met with general compensation.

Consequences of ambiguity in design are well documented (see, for example, Bever, Garrett and Hartig, 1973; Chapanis, 1965; and Sinaiko and Brislin, 1973). In the area of alarm-initiated activity too, ambiguous displays have been associated with disastrous consequences. Stanton, Booth and Stammers (1992) report and give commentary on a transcript from a recent aircraft disaster. Given below are extracts from that transcript:

21.24	pilot:	That keeps ... that's come on
22.20	co-pilot:	So we passed transition altitude one zero three
22.30	pilot:	What's it say in there about that ...?
25.19	co-pilot:	Shall I ask the ground staff?
25.22	pilot:	What's that?
25.23	co-pilot:	Shall I ask the technical men?
25.26	pilot:	You can tell 'em about it just it's it's it's just - ah no, ah, it's probably just moisture or something because it's not, it's not just on, it's coming on and off.
25.39	co-pilot:	Yeah
25.40	pilot:	But, ah, you know it's - it doesn't really, it's just an advisory thing.
30.27	co-pilot:	Ah, reverser's deployed.
30.41	pilot:	Jesus Christ!
30.44		[sound of four caution tones]
30.47		[sound of siren warning]
30.48		[sound of warning stops]
30.53	pilot:	Wait a minute.
30.58	pilot:	Damn it!
31.05		[sound of bang]

From the selection of dialogue above it is clear that the pilot and co-pilot initially failed to comprehend what the alarm meant. At this stage, additional information was sought, before incorrectly identifying the alarm (as Stanton *et al.* say, a trivial problem is allocated to the alarm activation). The category to which the alarm belongs - advisory rather than urgent - was next misunderstood, before finally the true meaning of the alarm is realized. Unfortunately, that final stage came rather too late for the crew.

Rather than attempt a laboratory-based simulation along the lines of the above, the present study employed symbolic information from a previous study (Langley, Baber and

Wankling, 1992) which was used to operationalize the factor of ambiguity. The symbols from this study fell into three categories: symbol in the absence of text; symbol with text used to identify the symbol's meaning, and symbol, together with text used both to identify the symbol's meaning and to inform the user of the action required, if any. Clearly it was necessary before this study began to establish that the different levels of this variable really did represent different levels of ambiguity. Fortunately a pre-test to establish this was not necessary, since Langley, Baber and Wankling (1992), using the same symbolic information, had shown that the different levels were indeed associated with different ambiguities. In fact, the researchers also showed that variation in the amount of information, and hence, ambiguity, gave rise to differences in participants' urgency ratings.

Since ambiguity of symbolic information changes the intrinsic risk of the environment, propositions 2, 3 and 13 are once again tested. The primary purpose of this exercise, however, is not to test new hypotheses derived from Wilde (1988), but rather to examine the possibility that the testing of such hypotheses can take place with the aid of a low-fidelity simulation. Additionally, this study set out to test Wilde's assertion that movements in the direction of the negation of an intrinsic risk change are so thoroughly internalized that most individuals are not conscious of them.

Links with previous studies

Studies 1, 2 and 3 all examined RHT in closed-loop simulation. Although they were to some extent useful in answering RHT issues, the studies leave open the question of whether such sophisticated closed-loop simulation is necessary in order to look effectively at RHT predictions. Study 4 therefore aims to look at the notional behavioural responses of participants in a low-fidelity open loop simulation.

In addition, studies 1, 2 and 3 all contain clearly definable changes in environmental risk. They do not therefore tell us anything about how individuals respond in cases where the change to the level of risk is ambiguous, as in the appearance of an unfamiliar alarm.

Study 4 aims to address this question.

6.2.2. Method

Participants

150 participants were involved in this study. All were undergraduate psychology students, from different course years. A driving licence was not required to take part in the experiment. Participants were aged between 18 and 39 years.

Equipment

Only the questionnaire (see appendix 1) was used for this study.

Design and Procedure

The study involved interpretation of symbols taken from an experimental set under consideration by Jaguar Cars (UK). In all, twelve different symbols were used. These twelve symbols were presented together with questions concerning whether participants would stop their car immediately and whether they would shorten their journey. Questions on eight different behavioural pathways were then asked before the next symbol was presented. Participants were asked not to turn each questionnaire page over until all the responses requested had been given. They were also told not to flick forwards through the questionnaire booklet or flick backwards to make alterations.

After the presentation of all twelve symbols, the symbol presentation was repeated in the same order and with the same questions. On this second presentation a textual aid accompanied the symbol (examples of these include *Anti lock brake failure* and *Ice warning*). The third presentation involved textual aid and required action.

The addition of text and required action served to remove any ambiguity that may have existed with the presentation of the symbol on its own. For this reason, it would have been impossible to counterbalance the design. For example, if some participants had been given symbols together with text and their required action and then given those symbols by themselves, what would otherwise have been an incomplete interpretation would have been confounded by the earlier complete full interpretation. This leaves the question of a potential order effect. It was felt that this need not be a methodological problem since any sort of practice effect on a task involving no motor or problem-solving skills would be hard to envisage. Extensive pilot work indicated that no fatigue effect on concentration existed.

It was hoped that the symbols presented by themselves were, very often, ambiguous (a pilot study indicated that this was very much the case). Environmental risk was therefore operationalized in terms of alarm ambiguity. When an alarm was presented, the meaning of which was unclear, an environmental risk could be said to be present in the form of a state of uncertainty. The removal of this uncertainty, either partially or completely (respectively, symbol and text and symbol, text and action), might therefore be said to be a reduction in environmental risk.

6.2.3. Results

Repeated measures ANOVAs were computed across the three levels of symbol/text information with Scheffe's F test applied *post hoc* to comparisons within this variable. All figures that follow represent responses on the behavioural scale of the questionnaire, across the range of -7, through zero, to +7. A response of zero always indicates no change in the behaviour in question. Table 6.1 shows the mean adjustment in behaviour across the eight different behaviours sampled.

Behaviour	Symbol	Symbol + T	Symbol + T + A
1	-1.927	-2.804	-2.804
2	-2.07	-3.143	-2.838
3	-2.412	-3.458	-3.067
4	.024	-.214	.114
5	1.887	2.55	2.41
6	.336	.372	.619
7	-.639	-1.381	-1.206
8	1.052	1.707	1.522

Table 6.1 : Mean change in behaviour as indicated on the eight 15 point scales.

Key: 1=relative speed, 2=relative number of overtakes, 3=risk of overtakes, 4=use of brake pedal (much less use to much more use), 5=relative concentration on driving, 6=relative likelihood of stopping on seeing an amber light, 7=relative likelihood of momentarily taking eye of road, 8=relative likelihood of adhering to mandatory speed restriction. 'Symbol + T' refers to symbol plus text. 'Symbol + T + A' refers to symbol plus text plus action.

The first of the eight behaviours, relative speed, failed to provide evidence of an ambiguity effect in that the difference in stated behaviour between symbol and symbol and text conditions was not in the predicted direction. This is shown in figure 6.6. With greater ambiguity (symbol condition) participants stated that they would drive faster. However, the comparison between the symbol and text conditon and the symbol, text and action condition was both significant and in the predicted direction (which was behaviour changes in the direction of compensation for the change in environmental risk, as reflected in ambiguity). Support for the hypothesis from this behaviour then would appear to be mixed (overall $F[2,3566]=81.36$, $p<.0001$; the *post hoc* comparison between symbol and text and symbol, text and action gave Scheffe F-test at 41.217, $p<.05$).

NB - For simplification, all y axes for this study are labelled 'behavioural adjustment'. This refers to the mean response to the behavioural adjustment question, a value between -7, through zero, to +7. Appendix 1 should be consulted for more detail.

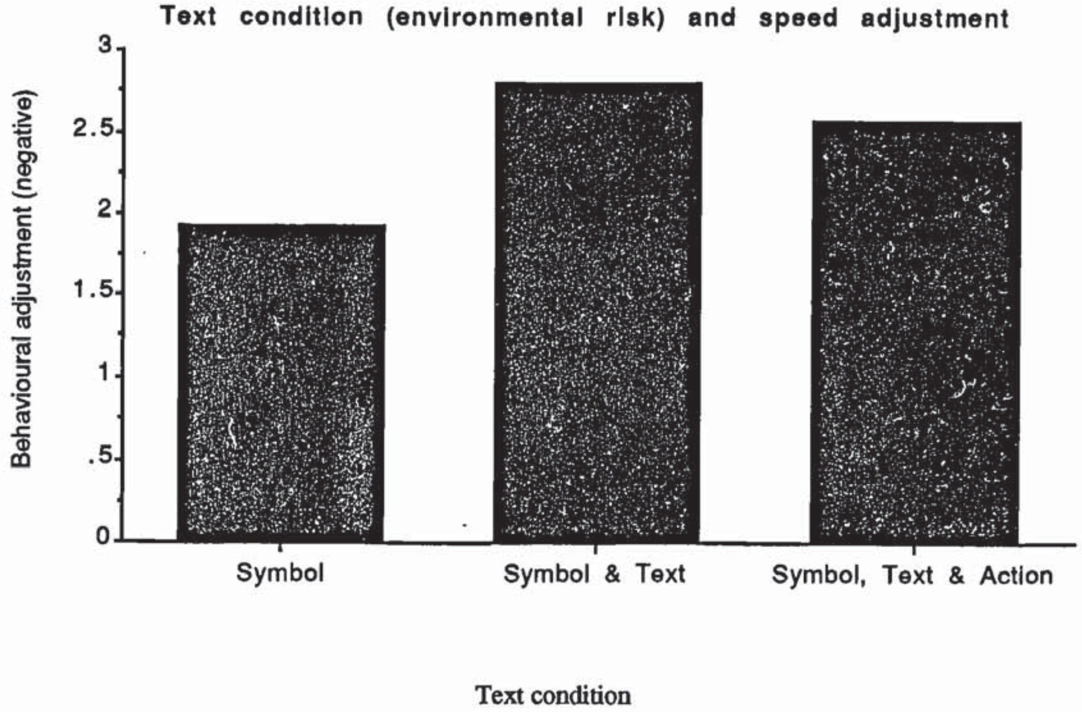


Figure 6.6 : Behavioural adjustment (in the negative direction of the questionnaire) of speed in relation to text condition (environmental risk).

The second behaviour, relative number of overtakes carried out, showed much the same pattern as the first behaviour. This is shown in figure 6.7. Whilst observations across the three levels of ambiguity were significant, the provision of text along with symbol resulted in more cautious rather than less cautious behaviour. The provision of required action did however result in a shift to risk. Overall $F[2,3564]=113.15$; $p<.0001$. For symbol versus symbol plus text, Scheffe's F test gives 106.536; $p<.05$. For symbol versus symbol, text and action, Scheffe's F is 54.583; $p<.05$; and for symbol and text versus symbol, text and action, Scheffe's F is 8.606; $p<.05$.

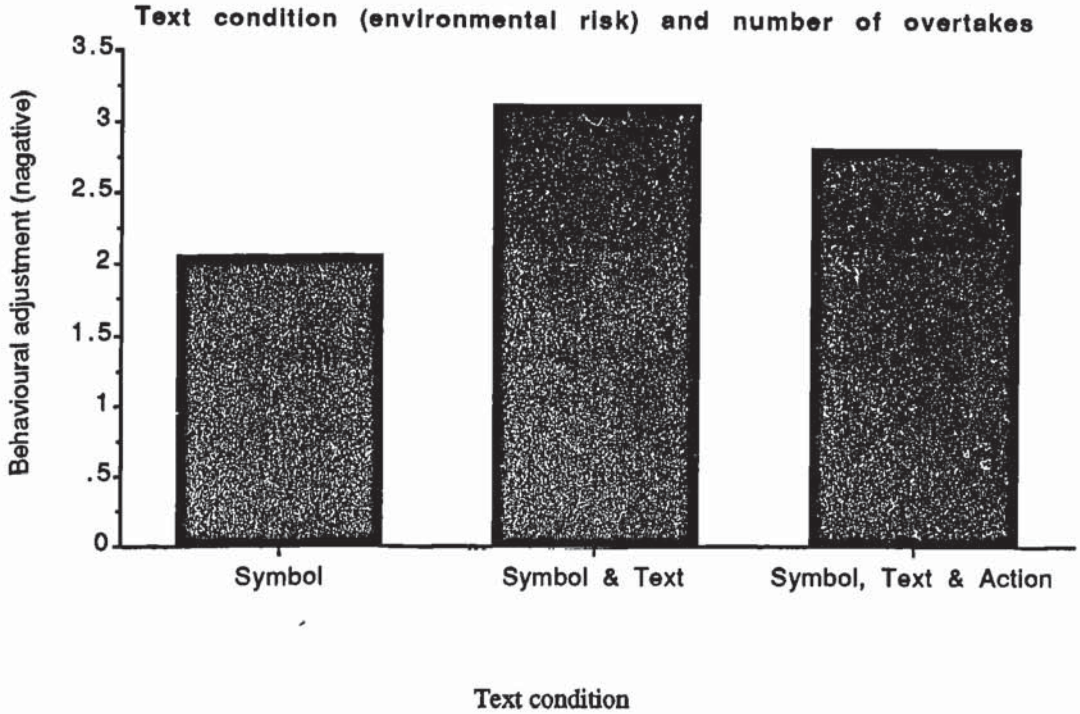


Figure 6.7 : Behavioural adjustment (in the negative direction of the questionnaire) of relative number of overtakes in relation to text condition (environmental risk).

The third behaviour, carry out relatively less risky overtakes, showed the same pattern as the first two behaviours: an initial shift to risk was seen when text was introduced, but a return to caution appeared when required action was introduced. This is shown in figure 6.8. Overall $F[2,3562]=102.598$; $p<.0001$. All *post hoc* comparisons were significant (Sheffe's F for symbol versus symbol and text=100.463, $p<.05$; for symbol versus symbol, text and action $F=39.403$; $p<.05$ and for symbol and text versus symbol, text and action, $F=14.032$; $p<.05$.

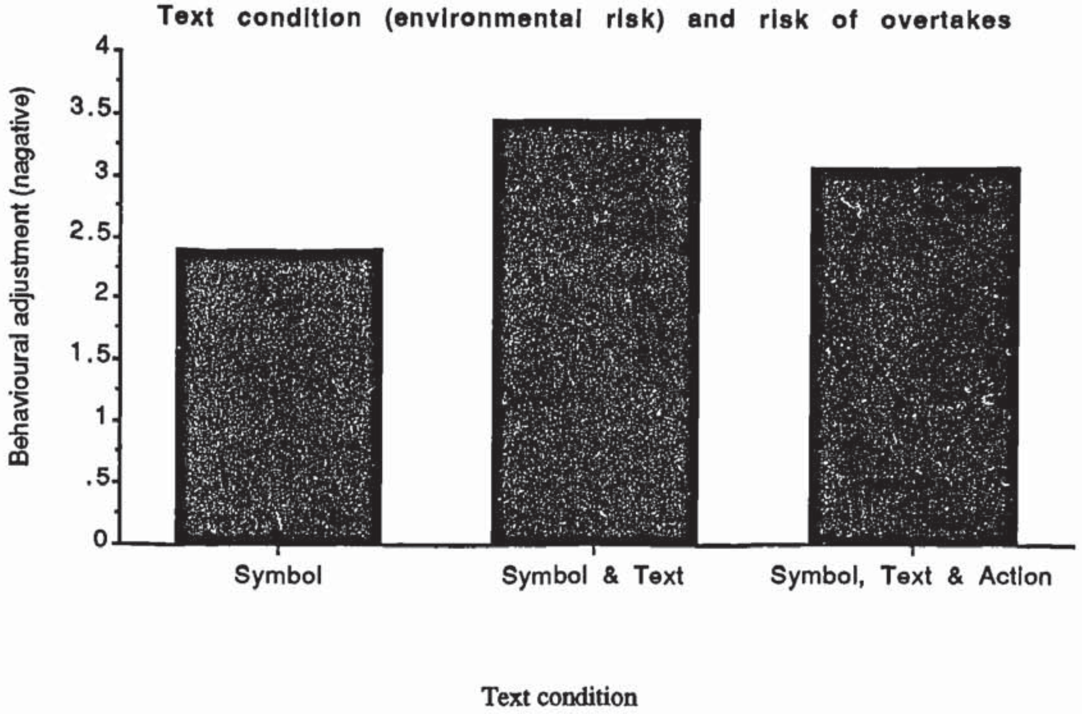


Figure 6.8 : Behavioural adjustment (in the negative direction of the questionnaire) of relative risk of overtake in relation to text condition (environmental risk).

This now common pattern was again repeated for the comparisons of the fourth behavioural pathway - use of brake pedal. (No further bar charts are therefore included.) Again, the overall comparison was significant with $F[2,3554]=10.334$; $p<.0001$. For this behaviour, however, not all of the *post hoc* comparisons were significant. The *post hoc* comparison of symbol versus symbol and text was significant (Sheffe's $F=5.081$; $p<.05$). For the comparison of symbol versus symbol, text and action, Scheffe's $F<1$; NS. The comparison of symbol and text versus symbol, text and action, however, was significant (Scheffe's $F=9.684$; $p<.05$). This fourth pathway is of great interest since the responses, in terms of their meaning in relation to risk and caution, are reversed. That is, a score of negative 7 on the first three behaviours would represent an extreme score of caution. On the fourth behaviour, however, the same score of negative 7 now represents an extreme score of risk. One would therefore expect the general pattern of reponses to be reversed for behaviour 4. It was not, suggesting a response set.

The fifth behaviour to be examined was concentration on driving. It too showed the same pattern of responses. Overall $F[2,3562]=46.862$; $p<.0001$. The *post hoc* comparison between symbol and symbol and text is significant (Scheffe's $F=42.181$; $p<.05$) The comparison of symbol versus symbol, text and action is also significant (Scheffe's $F=26.225$; $p<.05$). Interestingly however, the comparison of symbol and text versus symbol text and action fails to reach significance (Scheffe's $F=1.887$; NS). Just as in behaviour 4, this behaviour had its sign reversed so far as caution and risk were concerned. Since it too showed the same general pattern of behaviour, one might say that it too provided evidence for a general response set among participants.

The sixth behaviour to be examined was relative likelihood of stopping on seeing an amber light. Here there was at least the start of a linear trend. Unfortunately for RHT though, the trend was going in the wrong direction. Participants claimed that they were more likely to stop on seeing an amber light when more information was given to them in the form of text and text and action. Scheffe's $F<1$, NS for the comparison of symbol versus symbol and text. For the comparison on symbol versus symbol, text and action, Scheffe's F gives a value of 6.85, $p<.05$ and for the comparison of symbol and text versus symbol, text and action, Scheffe's F gives a value of 5.221, $p<.05$. Again, the behaviour was rated in the reverse direction from the first four such that positive numbers were associated with relatively safe behaviour.

The penultimate behaviour to be examined, likelihood of momentarily taking one's eye off the road returned to the convention established over the first four behaviours of allowing positive number responses to be associated with relatively risky behaviour. The addition of text resulted in safer behaviour ("*less likely to momentarily take your eye off the road*"), but with the addition to text of symbol and action, a very slight but significant shift to risk was seen. Scheffe's F for the comparison of symbol versus symbol and text gave a value of 55.019, $p<.05$. The comparison of symbol versus symbol, text and action gave an F value of 32.144, $p<.05$, whilst the comparison of symbol and text versus symbol, text and action gave an F value of 3.055, $P<.05$.

The relative likelihod of adhering to a mandatory speed restriction was next examined. On seeing the symbol participants reported that they would be more likely to adhere to a mandatory speed restriction. When text accompanied this symbol they reported that they would be more likely still to adhere to the restriction, but when action was added to the message, a slight but significant return to relative risky behaviour ("less likely to adhere to a mandatory speed restriction") was seen. Scheffe's F tests were carried out on all comparisons and on all comparisons significance was seen (43.773, $p<.05$; 22.532, $P<.05$; and 3.494, $P<.05$ for the comparisons of symbol versus symbol and text, symbol versus symbol, text and action, and symbol and text versus symbol, text and action respectively).

Finally, two further measures of avoidance were included: the questions of whether the journey would be either stopped or shortened. In cases were the symbol referred to a warning, 47 participants said they would stop their journey with symbol only, 36 with symbol and text, and 136 with symbol, text and action. For the question of whether the journey would be shortened, again with warning symbols, 109 participants claimed they would shorten their journey with just symbols appearing, 126 said the same with symbol and text, and 142 with symbol, text and action. For caution symbols and the 'stop' question, 47 participants said they would do so with symbol only, 65 with symbol plus text, and 14 with symbol, text and action. For the shorten question and caution symbols, those values are, respectively, 80, 118, and 118. Since these data would appear not to support RHT, and since the relationship between ambiguity and avoidance measures is neither linear, nor systematic, no inferential statistics were performed on these data.

6.2.4. Discussion

There can be little doubt that this study was successful in that it served to operationalize environmental risk across at least two levels of ambiguity. Feedback from participants confirmed this beyond question. The study though provides only very mixed support for RHT: the general compensation in the predicted direction was absent. A more important conclusion to emerge from this design would seem to be that the simple pencil-and-paper-based methodology, as reported here, cannot be said to be adequate in simulating physical risk.

There is, however, considerable room for improvement in any subsequent work.

(i) The study could be said to be too long for participants to maintain either interest or concentration to the required degree. In future research, fewer responses might be required of participants.

(ii) The interdependence of the different behaviours might, in any future work, be stressed. For example, participants should perhaps be made aware that if they decide to overtake less frequently, their mean speed can be expected to fall.

(iii) A more carefully restricted participant sample might be used in future research; specifically, participants might be required to hold a current driving licence.

Observation of participants led to the question of whether they were characterised by a target level of risk whilst completing the questionnaire and whether the responses were arrived at through an appreciation of this risk. This points up the importance of evaluation of new types of simulation, a point that shall be returned to in chapters 8 and 9.

Part of the reason for carrying out this study was to test the hypothesis that individuals are not aware, most of the time, of their behavioural adjustments towards the negation of an intrinsic risk benefit. To some extent therefore, this finding might be considered to support this: different levels of environmental risk were not associated particularly with different stated behaviours in the direction of negation of the change to risk.

Chapter 7 - Behavioural responses and specific changes to the level of environmental risk.

7.1 - Study 5 -Specific changes in environmental risk

7.1.1. Introduction

This study, making use again of the Aston Driving Simulator, has two distinct aims. First, it seeks to establish whether a highly specific change to environmental risk will lead to equally specific and 'local' behavioural adjustments, or whether a more general pattern of behavioural adjustments might result. Its second aim is to assess whether the time span of the closed-loop regulatory mechanism can operate over a relatively short time period (a ten-minute run time) where immediate feedback of results is provided. It is hypothesised that in this short time scale, individuals will have time to learn effectively the change in environmental risk, but will not have time to learn exactly how much more or less safe the environment has become. Consequently it is expected that the experiment will provide evidence of an incomplete homeostatic operation with significant differences over the range of dependent measures that may include criteria reflecting accident loss. It is further hypothesised that this incomplete change may differ in either direction. Either accident loss will be significantly greater in the condition of high environmental risk, or it will be significantly lower on the condition of high environmental risk. Significant differences across a number of relevant behavioural pathways are also hypothesised.

The study of overtaking behaviour is not new (see Crawford, 1963). Much attention has been given to the issue of overtaking margin and type of vehicle. It has, for example, been shown that drivers of small cars often adopt riskier headways (Wasielewski, 1981), although Evans and Rothery (1976) provide evidence which suggests that findings like that of Wasielewski might be at least partly explained by differences between driver groups. Whether a correlational fallacy or not, it is known that drivers of powerful cars tend to make fewer errors when overtaking (Kaukinen, 1967).

Harris, Brindle and Muir (1986) point out that the power of a vehicle is of less interest than the power to weight ratio. They found that HGV drivers specifically, and generally drivers of low power to weight ratio vehicles, adopt riskier overtaking strategies. By contrast, drivers of high power to weight ratio vehicles were shown to adopt relatively safe overtaking strategies. Unfortunately, Harris *et al.* were unable, just as previous researchers, to eliminate the possiblity that overtaking differences may have been attributable to driver-related, rather than vehicle-related, factors. An advantage of a counterbalanced, repeated-measures design, such as the one reported here, is that the same 'drivers' can be asked to participate under different overtaking conditions, thus eliminating driver-related overtaking factors.

So far as Wilde's RHT is concerned, this study has particular relevance to proposition 12 from Wilde (1988) in which it is stated that traffic accident loss, and the everyday experiences associated with it, influence road-user behaviour. The purpose of this study was to establish (via initial pilot work) conditions in which a specific aspect of intrinsic risk contrasts so sharply over two conditions, that an intrinsic risk difference can be learnt over a relatively short time-scale. By doing this it is expected that a sharp contrast in driving experience should exist. If this difference leads to significant behavioural compensation, then proposition 12 could be said to enjoy some support.

In that the target level of risk should not, in this study, change (owing to no differences being present in the utility of risk taking in different intrinsic risk conditions), the study tests also the aspects of propositions 2, 3 and 13 already described in previous studies.

<u>Links with previous studies</u>

The previous ADS studies, study 1 and study 3, involved general changes to intrinsic *risk. Both of these studies are limited in that, by virtue of the general change in intrinsic risk, no reconciliation of qualitative risk change with behavioural pathway is possible. The present study therefore seeks, for the first time, to make a highly specific change to intrinsic risk, via the temporal leeway of overtaking. This was done with the intention of comparing the behavioural adjustment arising from a general change of risk (as in studies 1 and 3) with the behavioural adjustment arising from a specific change of risk, as in the present study.*

7.1.2. Method

Participants

70 participants took part in this study. Thirty-five participants were aged between 18 and 30 years; the remaining thirty-five participants were aged between 45 and 60 years.

Equipment

The Aston Driving Simulator was used for this study.

Design and Procedure

All participants were given two experimental trials. On condition 1, the cars in front of the simulated driving position were moving at speeds of between 20 mph and 30 mph. On condition 2, the cars in front of the simulated driving position were moving at speeds of between 30 mph and 40 mph. Condition order was counterbalanced. Given that the speed of on-coming cars was held constant across these two conditions, condition 2 can be considered environmentally the riskier condition in that overtaking times (together with error margins) would be reduced. If overtaking behaviour were to remain unchanged between the two conditions, and if no other behavioural compensation were to occur, one would predict greater accident loss on condition 2. However, this can be considered a very local change to environmental risk and one that could be compensated for, in theory at least, by changes to the overtaking-decision threshold, commensurate with the change in risk. Measures were therefore taken to establish whether the changes in behaviour were indeed either local in themselves or consequences of local changes to environmental risk (knock-on effects of local adjustments). Measures were also taken to establish the extent to which a more general compensation process might be pursued, along with the usual measures reflecting accident loss - other vehicle collisions and kerb-collisions.

7.1.3. Results

1. The overtaking (specific) measures

Where the environment was characterised by higher levels of environmental risk, this was associated with fewer other-vehicle collisions, fewer successesful overtakes, and fewer end-pull-backs (aborted overtakes). It was associated with more kerb collisions whilst overtaking, though not significantly more.

Related *t*-tests were carried out on all the above comparisons. Risky overtakes occured on significantly fewer occasions on the condition of high environmental risk, t=2, p=.024. This is shown in figure 7.1. For the purpose of this experiment, a risky overtake was defined as one in which an oncoming car was visible from the driver's position when the participant pulled out to overtake. The comparison of aborted overtakes produced t=3.15, p=.0012. This comparison is shown in figure 7.2. For the number of 'other vehicle collisions whilst overtaking' comparison the t value of 1.70 was significant (p=.0464). This is shown in figure 7.3. Kerb collisions whilst overtaking produced a t value of <1, NS, as did the comparison of total overtakes.

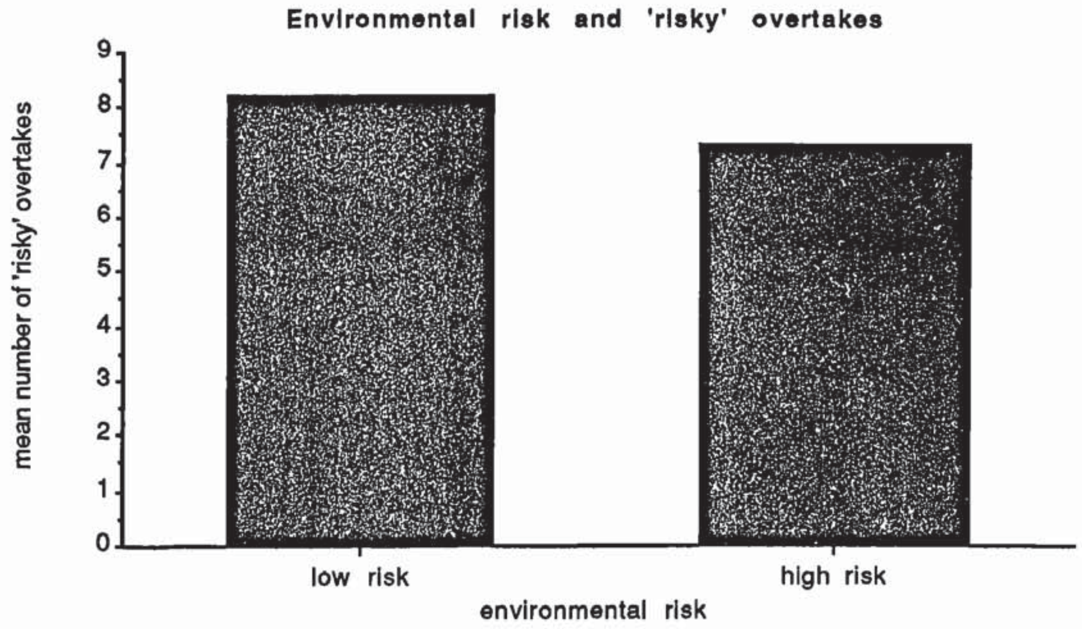


Figure 7.1 : Environmental risk and mean number of 'risky' overtakes

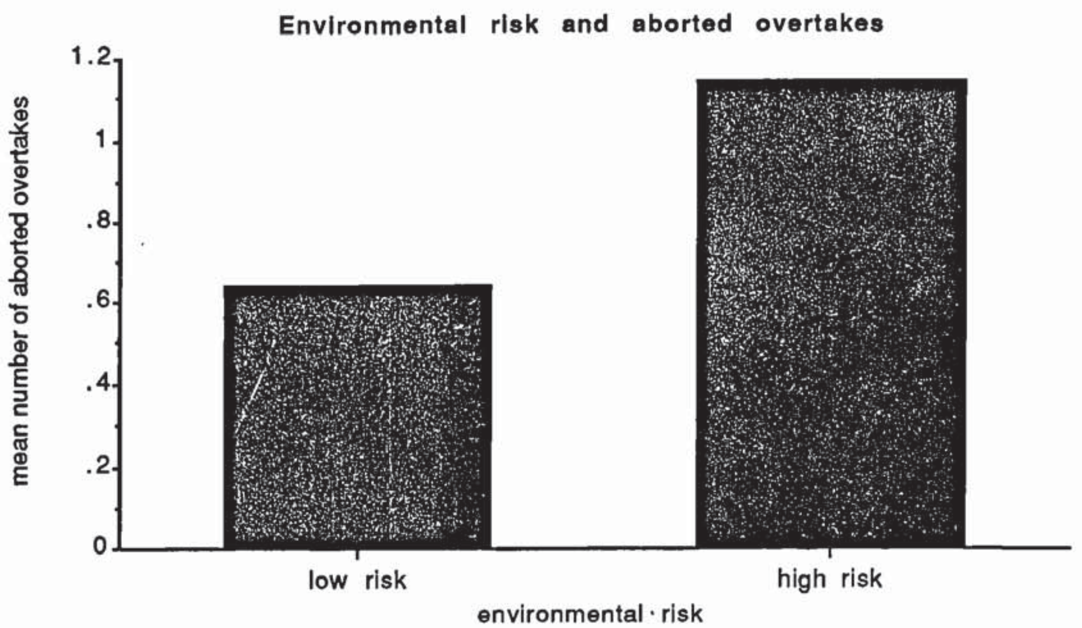


Figure 7.2 : Environmental risk and mean number of aborted overtakes

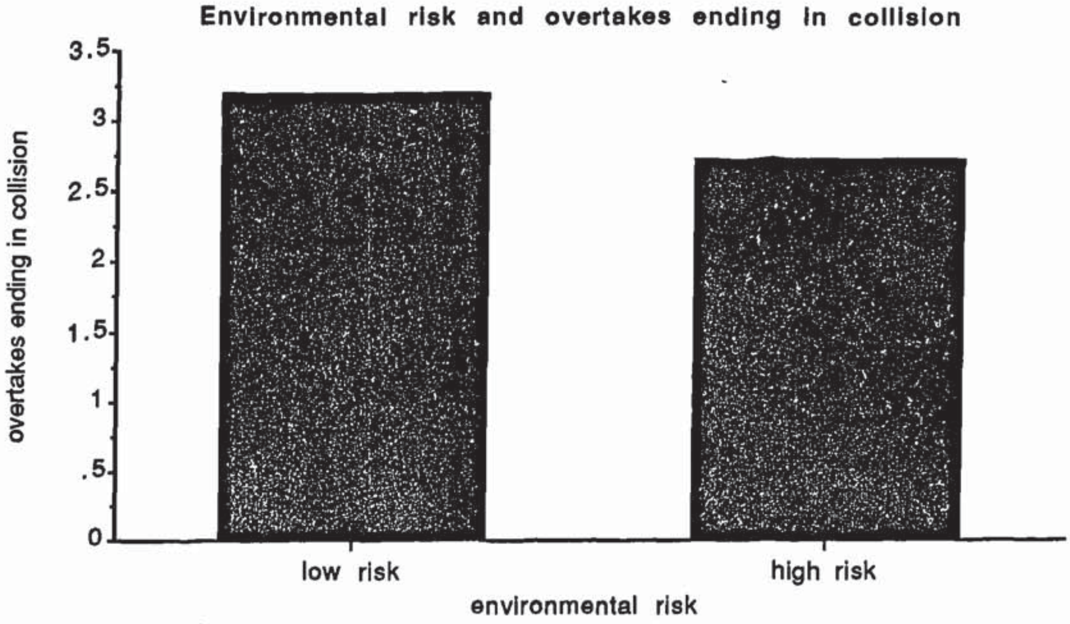


Figure 7.3 : Environmental risk and mean number of overtakes that ended in collision with another vehicle

Successful overtakes too occured on fewer occasions on the condition of high environmental risk, t=1.957, p=.0272. Finally, the comparison of mean leeway was on the margin of significance with t=1.594, p=.0577, in the direction of over-compensation.

2. The non-overtaking (general) measures

Related t-tests were carried out for a range of general indicators of driver behaviour, across the two conditions of environmental risk. First, the comparison of road position (position relative to centre white line and kerb) across conditions was not significant (t<1, NS). This is shown in figure 7.4.

It has been argued previously (study 1) that road position can be considered a measure of tracking performance, and therefore, by inference, an indirect measure of attention and specific motor skills. Since benefits may well be gained from maintaining reduced levels of attention, one could look upon this as a general measure of compensation, not local to the change in environmental risk. From this it would seem that arguments for a more general form of compensation related to attention would not be supported.

Accelerator mean position, correlated with speed, of course, showed no effect of environmental risk (t=1.038, NS). In fact, the correlation between accelerator travel and mean speed, whilst strong, perhaps does not quite reach the strength one might imagine - r=.505 (SD of mean speed across conditions = 6.55; SD of mean accelerator travel across conditions = 14.21; covariance of xy = 47.04). Again, this general pathway moved away from, rather than supporting, homeostasis.

Brake was significantly different across conditions (t=4.53, p=<.0001). This effect is shown in figure 7.5. Again, this result is perhaps best seen as a mechanical consequence to changes in local environmental risk. Where overtaking is made more difficult, drivers will have more often to abandon an overtake, and this may well involve greater use of the brake.

The mean position of the steering wheel also differed between conditions (t=-3.97, p<.0001). On the surface this might reflect either nothing more complicated than a greater number of overtakes on one condition rather than another, or a reflection of the relative attention level. Since differences in the number of overtakes and attempted overtakes must necessarily be reflected in steering wheel position, and since *position on road* did not provide evidence of attentional differences across conditions, the former suggestion has perhaps more appeal.

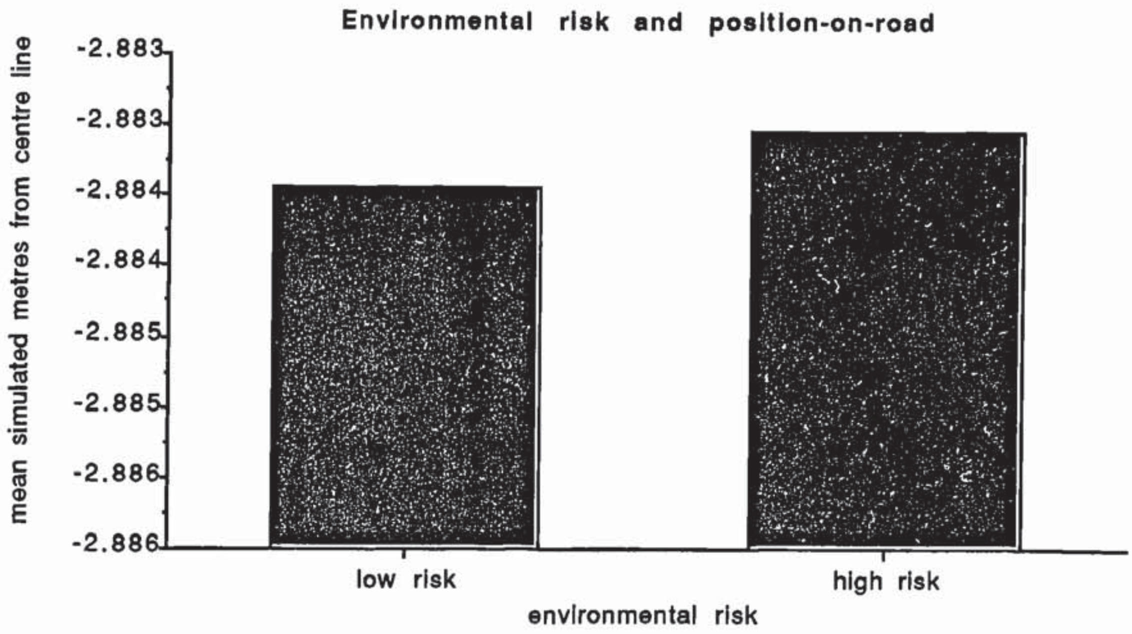


Figure 7.4 : Environmental risk and position on road (metres from centre white line).

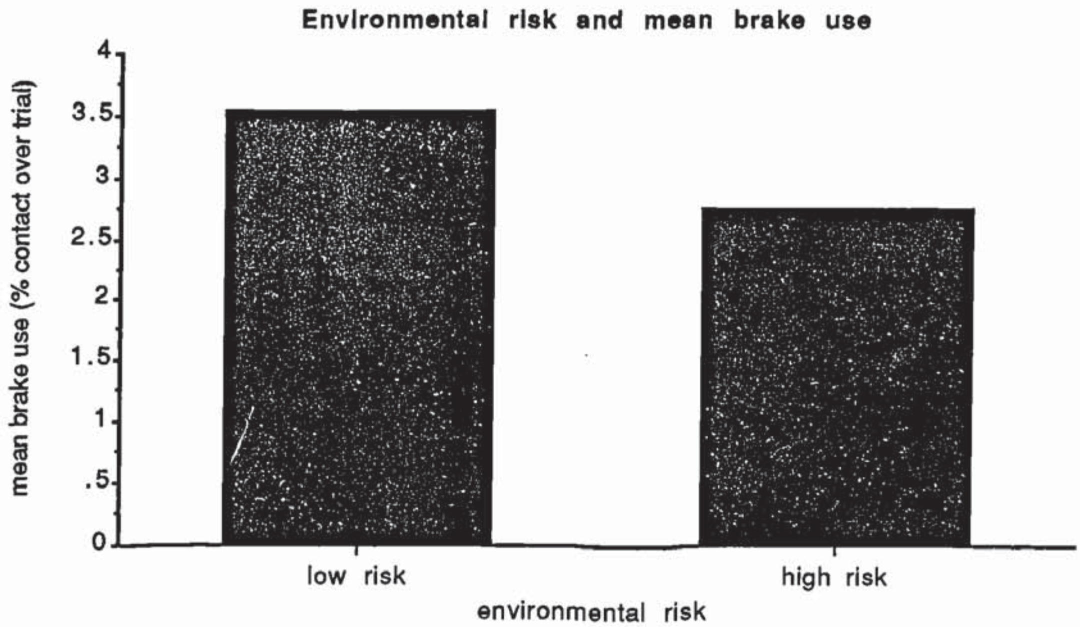


Figure 7.5 : Environmental risk and mean use of brake (percentage use over the experimental trial).

Speed too was examined and proved significant with a *t* value of 8.98, $p<.0001$. The effect of environmental risk on this measure is shown in figure 7.6. Although one might again imagine that this finding would be difficult to interpret (speed adjustments might reflect a general compensation, or may just be a trivial consequence of not being able to overtake so frequently, or having to overtake at higher speeds) the direction of the difference is able to settle the matter. The condition of high environmental risk was associated with greater speeds. Far from compensating through this pathway, participants actually made things worse.

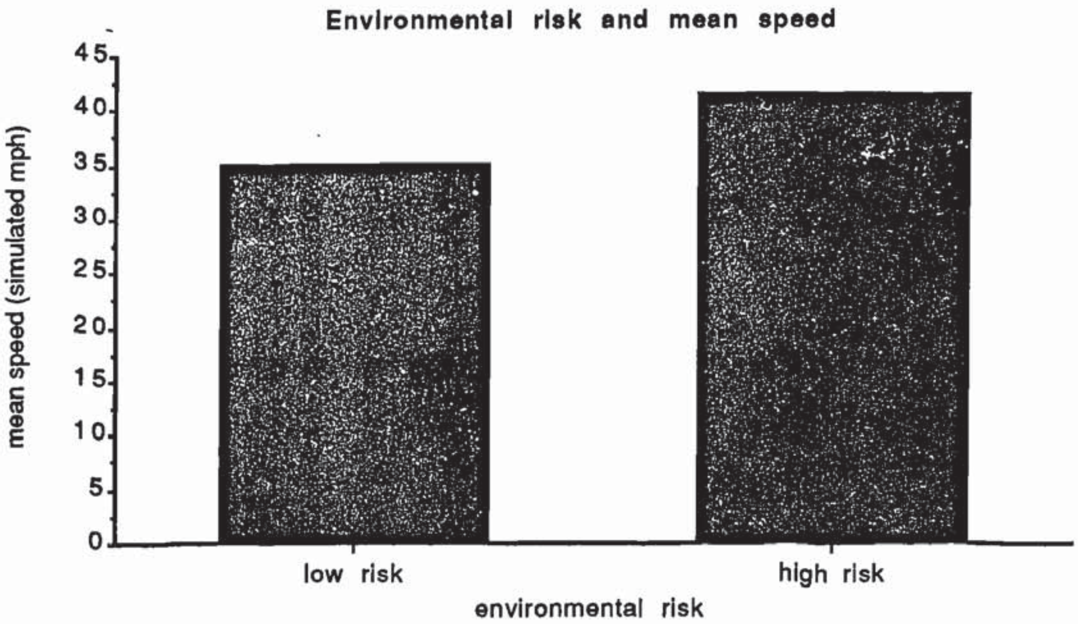


Figure 7.6 : Environmental risk and mean speed in simulated mph.

The next measure to be examined was *distance from car in front*. (see figure 7.7). This gave a related-t value of 2.67 ($p=.0024$). This finding is difficult to interpret. On the one hand, the behavioural adjustment of pulling closer to the car in front does not facilitate the local need for adjustment. However, when one looks more closely at the behaviour one sees that the finding is in fact the reverse of that which would be predicted by RHT.

Rather than respond to the change in environmental risk by increasing the 'safety' gap, drivers were, it would appear, making an already riskier situation (environmentally speaking), more risky still (behaviourally speaking) by moving closer to the car in front. Perhaps then this finding can be explained in terms of its being an almost mechanical consequence of the change to environmental risk, or rather, its specific manifestation: when drivers are prevented from overtaking a car, they tend to move closer to the car preventing the manoeuvre. This behaviour can therefore not be considered a general case of behavioural compensation, but is probably best explained outside of any compensation framework.

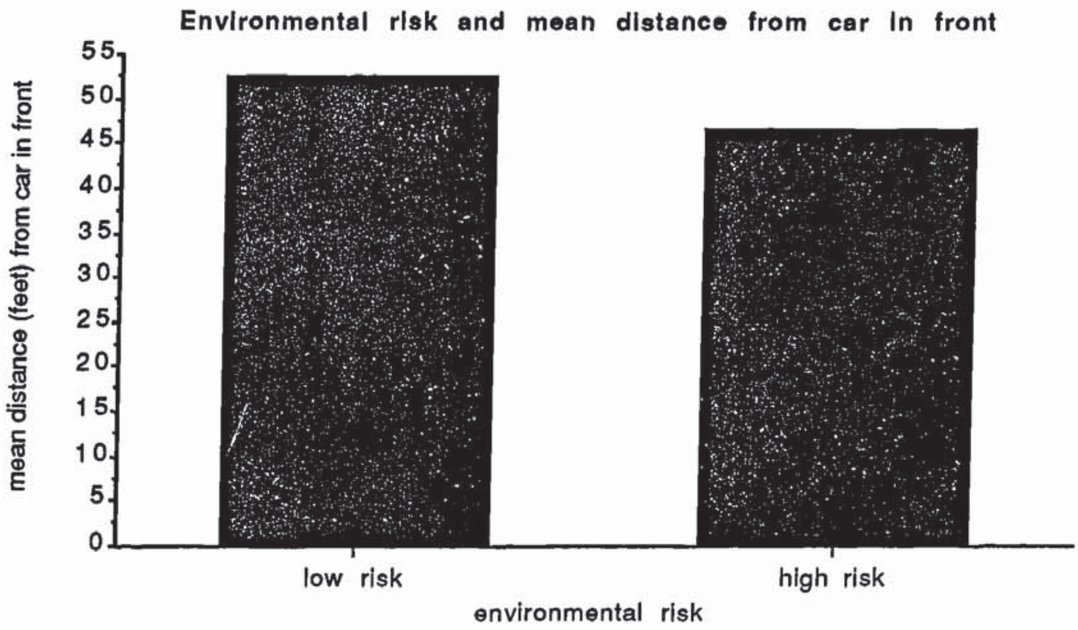


Figure 7.7 : Environmental risk and mean distance from car in front.

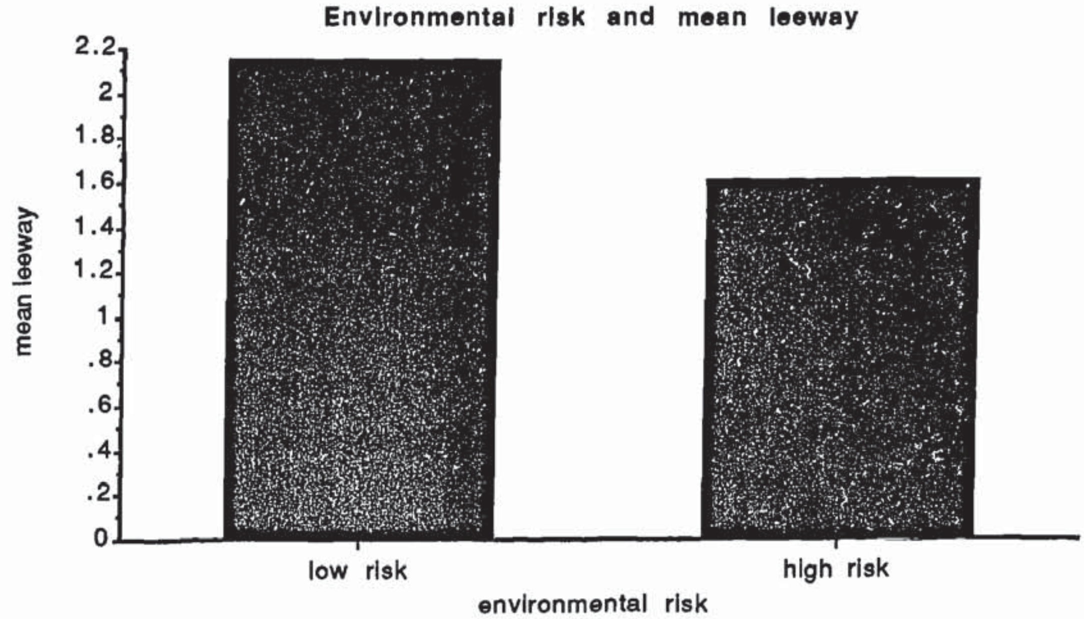


Figure 7.8 : Environmental risk and mean leeway in overtaking.

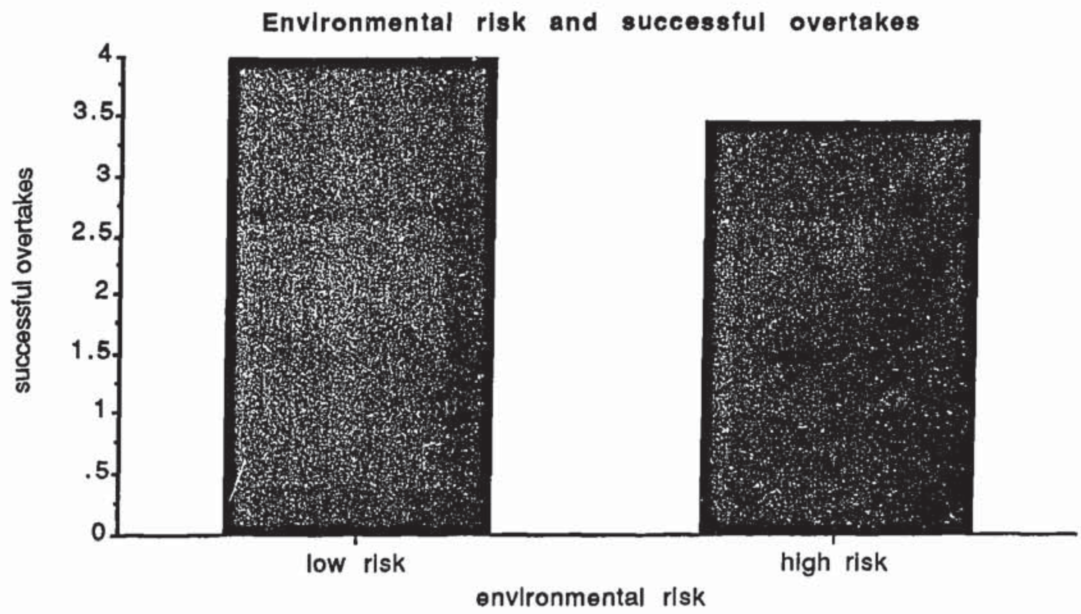


Figure 7.9 : Environmental risk and mean number of successful overtakes

Finally the two measures reflecting accident loss should be considered. On the condition of greater environmental risk there were fewer other-vehicle collisions (hitting another car) and fewer kerb collisions (hitting the kerb) than on the condition of low environmental risk. In the case of other-vehicle collisions (the more serious measure of accident loss) this difference was significant (t=3.66; p=.0002). This is shown in figure 7.10. The effect of environmental risk on kerb collisions is shown in figure 7.11.

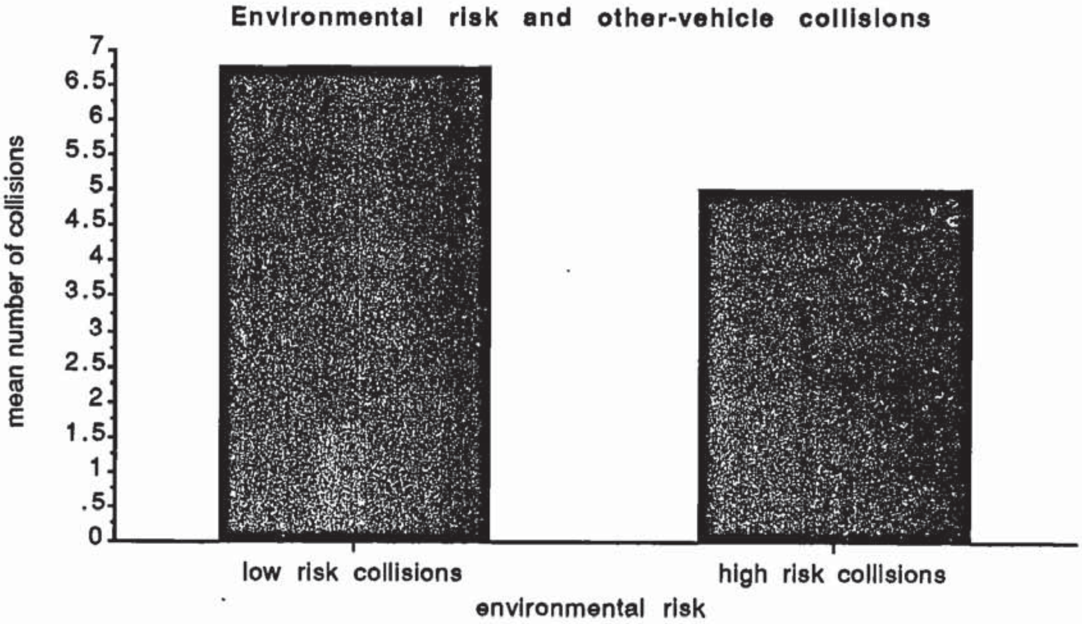


Figure 7.10 : The effect of environmental risk on mean number of other-vehicle collisions

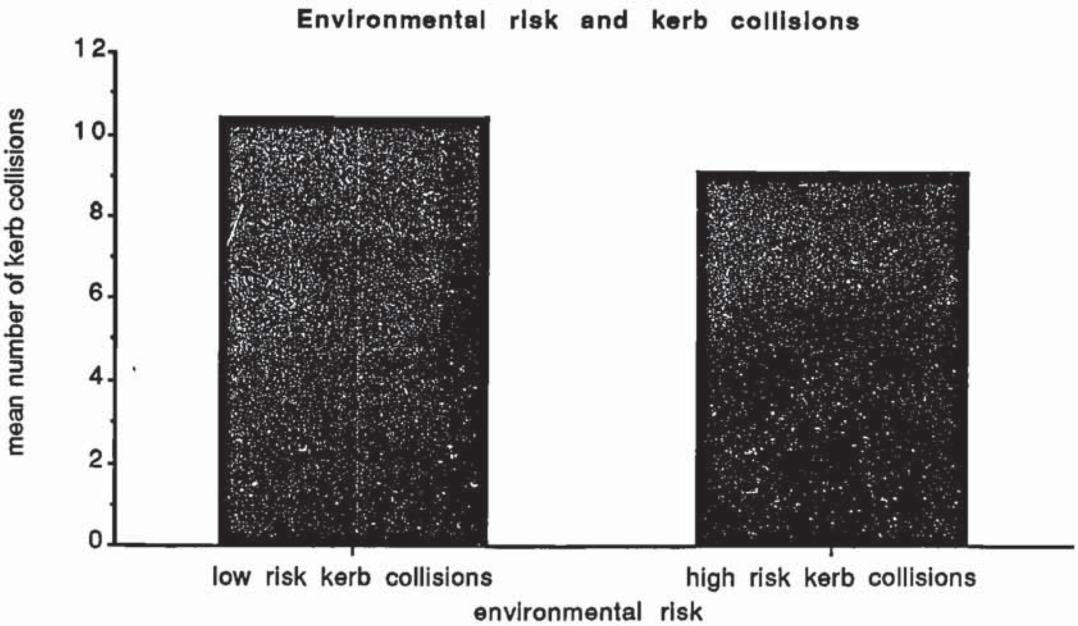


Figure 7.11 : The effect of environmental risk on mean number of kerb collisions.

The strong implication from the above findings on accident loss reflecting variables would seem to be that behavioural compensation can occur in the short term, and that initial adjustments may be characterised by their tendencies towards being over-compensatory. In predicting that initial compensation can be perfect, imperfect surplus or imperfect deficit, RHT really says only that in the short term anything can happen. In other words, any finding could have been reconciled with RHT. The findings on other-vehicle collisions however are far more satisfactory than the open prediction in that they (i) provide active support for compensation and; (ii) do so at very low significance levels.

7.1.4. Discussion

This study is arguably the most significant of those so far reported for a number of reasons. First, in providing evidence of significant initial over-compensation, the study has provided the clearest findings so far in support of RHT. In direct opposition to the engineering perspective, this study showed that on the condition of high environmental risk, there were fewer collisions, both with the kerb and with other vehicles. Whereas RHT has evidence to support it from field studies involving crude before-and-after designs, this study provides evidence for RHT in a tightly-controlled, laboratory study with possible confounding variables removed.

Second, the study shows that the investigation of RHT hypotheses in a simulated physical risk-taking environment can be successful to the extent of bringing about an effect. If Wilde is correct in his assertion that road-users are characterised by a target level of risk, which is maintained when changes in environmental safety are introduced, then this study would indicate that this same process can be successfully reproduced in a simulated environment.

Third, the study is interesting in that it provides evidence for a movement towards homeostasis within a very short time span - when environmental risk changed, participants drove for a period of just ten minutes. Within this period a significant adjustment took place. This leaves the question of how participants were able to compensate so quickly. Two, not necessarily mutually exclusive, explanations could account for this. The first of these is the possibility that participants recognised and compensated for the risk change as soon as that change was apparent - as soon, that is, as they realized that overtaking would be more or less hazardous if relevant behaviours were maintained at their previous level. The second possibility is that information regarding accident loss, such as other-vehicle collisions, kerb collisions, or near-misses, must be given to participants before compensation can occur. These two possibilities could be crudely labelled as, respectively, open- and closed- loop explanations.

A fourth aspect of this study that makes it interesting is its apparent justification of the attention given to the particular behavioural pathways that carry the effect. It would seem from these that where a change in environmental risk is highly specific, such as the time margin allowed to overtake vehicles, this specific risk change is responded to by participants by equally specific behaviours - behaviours, in other words, that could be said to be *relevant* to the environmental risk change.

The experiment has implications too regarding the aim of this thesis concerning the development of an alternative methodology for RHT. The evidence reported here would seem to suggest that although the time-scale of homeostasis in real physical risk-taking situations spans months or even years (see Wilde 1988 and Wilde 1989), this may be nothing more than a trivial consequence of delayed feedback. Just so long as immediate feedback of errors and of the change in intrinsic safety can be provided, a simulated environment may be entirely appropriate for investigating compensatory behaviours over a very short time span. In other words, collapsed experience must be possible for effective investigation to take place.

A word or two needs saying about the way in which environmental risk was operationalised in this study. In order to make the risk change specific to some particular behaviours, the environmental risk factor of temporal leeway was operationalised. However, as stated in the method section, this manipulation was not direct, as would have been the case, say, had the simulator's acceleration capabilities been altered, but was instead indirect through the manipulation of the average speed of cars on the simulator's driver-side of the road. When the average speed was increased, the temporal leeway was, indirectly, reduced. But can such an indirect manipulation really be said to change environmental risk? A critic might point out that when cars in front of the participants were moving at greater speeds, the driver had a much reduced *need* to overtake them, and thus, it may even be that the direction of the change in environmental risk is the opposite to that reported here. A number of arguments can be used to rebut this criticism and suggest that the indirectly reduced temporal leeway condition really did represent a reduction in environmental risk:

1. Two participants were run and interviewed informally in a pilot study. Both of these participants agreed that the reduced temporal leeway condition was in their view the more hazardous. In the case of the Swedish experience in changing the side of road on which road-users were asked to drive, it must be remembered that one side of the road is *intrinsically* no more dangerous than the other. What made the Swedish case relevant to the RHT debate was that those affected by the change had some *subjective* experience of risk change. Since workers in RHT from both sides of the debate are in agreement that the Swedish experience is relevant to the homeostasis question because of this subjective experience, the agreement by the participants in the pilot study that the reduced temporal leeway condition did represent higher risk might be said to be justification enough.

2. For the indirect manipulation of leeway to fail in changing environmental risk in the predicted direction, participants must have a reduced need to overtake at the higher speeds. Extensive pilot work from other studies involving the ADS suggests that this tendency is subject to floor effects. So long as the maximum speed of the car being followed is equal to or less than 40 mph (as was the case in this study) participants will be characterised by overtaking behaviour.

3. The reduced temporal leeway condition allows greater speeds to be reached for the same level of overtaking. Since speed is known to be correlated with risk, this too should lead to the conclusion that the reduced temporal leeway condition is environmentally the more hazardous.

4. The measure of mean leeway is independent of the number of times participants overtook other vehicles. If speed increases in the vehicles being followed led to reductions in the number of overtakes, this measure takes this reduction into account. The measure of mean leeway is on the margin of significance (p=.0577).

In fact the indirect nature of this risk change was deliberate. To have manipulated leeways directly through acceleration capability would have led to delayed feedback and a number of inevitable collisions during learning. The indirect manipulation was an attempt to provide almost concurrent feedback. Moreover, it was felt that this indirect approach had greater ecological validity. We often find ourselves with less time to overtake other vehicles because the vehicles ahead of us are driving faster; we rarely suffer a reduced temporal leeway through a failure of our vehicles to accelerate to the same speed in the same time that they did a few moments ago. In essence, a direct manipulation of temporal leeway would have involved just this.

A further criticism might be made of this study - that the operational definition of a 'risky overtake' was inadequate. It will be recalled that a risky overtake was defined as one in which an oncoming car was visible at the point at which the participant pulled out to overtake a vehicle. Of course, there are times when such a practice might be quite legitimate, such as when the oncoming vehicle is a considerable distance from the overtaking vehicle, or when the oncoming car is visible, but clearly travelling slowly. Equally, pulling out to overtake when no oncoming car is visible, but when visibility is severely restricted, could be considered risky, yet on the criterion adopted here, it would escape categorisation as such. There were in fact technical reasons why a risky overtake was defined in terms of the visibility of oncoming cars. Moreover, it was felt that the inclusion of the measure of mean temporal leeway went some way to compensate for the deficiencies of the measure of risky overtakes.

PART 3 - AN EVALUATION OF THE SIMULATION OF PHYSICAL RISK-TAKING ENVIRONMENTS AS A METHODOLOGY FOR THE INVESTIGATION OF RHT.

In this, the final part of the thesis, an attempt is made to evaluate the extent to which the simulation of physical risk successfully tested the predictions of RHT. Three approaches are taken here. First, structured interviews take place with participants from all three simulated environments. Second, an improved version of the low-fidelity driving simulator is tested for its ability to differentiate between the groups of road users that both the real driving situation and the Aston Driving Simulator are able to differentiate between (young and old and male and female drivers). Finally, this improved simulator is factor analysed. Factor analyses of the other simulators reported here are carried out too. Mixed support for the methodology is reported together with suggestions for improvements and general conclusions.

CHAPTER 8 - Structured interviews with participants.

8.1 - General introduction

One of the aims of this thesis was to develop a methodology, based on the simulation of physical risk-taking to investigate risk homeostasis. This having been done, attention should now be turned to the question of *how well* it was done. Whilst part of the answer here must come from the data gathered (the extent to which risk homeostatic behaviour was observed, for example, and the factor solutions of the data observed from the simulation devices), there is the wider question of how successful the simulations were in recreating the environments they set out to investigate.

The purpose of the study reported here was to interview, in depth and with open questions, six participants from each of the three simulation exercises used in this experimental work.

Of particular interest in this study was the question of whether participants were really characterised with a target level of risk, necessary generally to test RHT predictions, and necessary specifically to test propositions 2, 3, 5, 6, 7, 9, 10, 12, 13, 14, and 15 from Wilde (1988).

Links with previous studies

All previous studies have involved one of three different simulated physical risk-taking environments. The purpose of this study is to examine how these simulation-based studies were perceived by the participants.

Study 6.1 - The Aston Driving Simulator

8.1.1. Method

Participants

Six participants took part in this study from each of the simulation exercises used, opportunity sampled from those participants who had taken part.

Equipment

For all the interviews a cassette recorder was used. Copies of the interviews are available from the Experimenter.

Design and Procedure

The design and procedure of this study centred around the following open questions:

1. In what ways do you think your behaviour was different in the experiment from what it would have been in the corresponding real environment?
2. What were you thinking about while you were carrying out the experiment?
3. Some people have said that they treated the experiment like a game. How fair do you think this criticism is?
4. What do you/did you think the experiment was about? What do you/did you think we

were trying to find? *

* question 4 was included to assess possible demand characteristics (see implications from study 1).

Opportunity was given for participants to answer the questions in small groups. All of these sessions were tape recorded for later analysis and part transcription. The interviews were planned to take approximately 20-25 minutes per group of 2-3 participants.

To avoid unintended biases, the interviews were conducted by two assistants who were blind to the Experimenter's hypotheses associated with this work.

8.1.2. Results and Discussion

Reproduced below from all of the interviews is a discursive analysis of what was said, based on many hours listening to the tape recordings of the interviews. The selected comments are intended to give a flavour of the many thousands of remarks that were made. For several reasons, a content analysis of these interviews was not undertaken. First, a preliminary attempt at content analysis revealed that most of the comments were difficult to code in any way that might advance our understanding of how participants perceived these simulated environments. A much more representative 'flavour' of the interviews was felt to be provided by reference to the comments made. Second, for several of the questions put to participants, responses were remarkably idiosyncratic. The preliminary content analysis therefore had many categories with frequency counts between the values of zero and two. Third, a comprehensive content analysis of the hours of recordings taken would have been extremely time-consuming, and, arguably, unnecessarily so.

1 . In what ways do you think your behaviour was different in the experiment from what it would have been in the corresponding real environment?

Statements such as "I knew I couldn't really hurt anyone" were common among the comments made about the ADS. This would appear to confirm the concern that target levels of risk were lower in this simulated environment. Participants characteristically claimed to have tried to avoid accidents, but agreed that the extent to which they tried to avoid them could not be generalized to the environments which the simulators were held to represent.

One participant claimed that the experimental environment was associated with more, not less attention. As he said:

"It was an experiment so I knew it was serious. When I drive a car I look out of the window a lot and I talk to people in the car, I mean, I'm always taking my eye off the road ....you know. But the simulator was sort of serious because it was a proper experiment. I tried really hard not to hit other cars and I don't think I took my eyes off the road at all. I think I was much safer in the simulator than in a car."

It is interesting to note that statements such as "I didn't care", or "...it's only an experiment so I didn't try." were not made by participants. All the indications point to the maintenance of a target level of risk, albeit at a lower level than that which is likely to exist in any real physical risk-taking environment.

2. What were you thinking about while you were carrying out the experiment?

For one participant, this led to the response that :

"I was thinking about what I was doing - just driving the car simulator"

Another participant claimed:

"Well at first I was taking it really seriously and concentrating a lot. Then I got a bit fed

up and just wanted the thing to be over. I started driving faster then but I tried not to hit the other cars."

None of the participants questioned claimed to have been thinking about driving an automatic car. No-one was, it seemed, trying to imagine him or herself in the environment described, but rather, saw it very clearly in terms of the experiment that it was. This question provided further support for the notion that individuals were actively seeking to avoid accidents.

3. Some people have said that they treated the experiment like a game. How fair do you think this criticism is?

All those participants questioned agreed that the simulation had a game-like appearance to it and some thought that at times the simulation may have been treated rather like an arcade game. Three things need saying here. First, this was, as one might expect, more of a problem for male participants than for female participants. In fact, no female participants suggested that they treated the simulation as an arcade game. Second, treating the simulation as an arcade game seemed to occur more towards the end of the experimental session than at any other time. Thus, for the vast majority of the experimental period, it seems that participants did not treat the simulator as a 'game'. Third, further, informal discussions with those participants who claimed that the simulator may have been treated like an arcade game, revealed that cognitively what had occurred was a failure to consider fully the consequences of an accident, were one to occur, but that the maintenance of active accident avoidance still characterised participants. This is interesting to bring back to the two-part formulation of accident loss (Wilde, 1988) as *the sum of the cross products of accident frequency and their costs*. On this bifurcation only the former term was apparently of interest to some participants. This has an implication so far as future research is concerned: in cases where physical risk is being simulated, that risk should refer to accident probability, not accident costs, given that an accident has occurred.

This can perhaps best be illustrated by several practical examples. An environmental intervention such as the compulsory wearing of safety belts for car users, or crash helmets for motorcyclists, all have no bearing on the probability of an accident. Yet, they both serve to reduce intrinsic risk of those road users affected by them through a 'cost' reduction in cases where an accident has occurred. These types of intervention would appear, from this finding, not to be suitable for simulated exercises. By contrast, interventions such as a new and advanced braking system with reduced pulling-up distances, or a choice between low or high available temporal leeway in overtaking, are, it would seem, suitable for examination in an experimental context. This leaves the experimental work reported in this thesis conceptually still intact. All the interventions experimentally manipulated represent changes to the probability of an accident, rather than to its cost, given that the accident has already occurred.

4. What do you/did you think the experiment was about? What do you/did you think we were trying to find?

The most common reply to this question was given along the lines of a reaction time study. Some participants placed the context of the experiment in even broader terms. As one said:

"I thought it was an experiment looking at how I drive, what sort of a driver I am, and things like that."

Another participant said that she believed the study had something to do with '*attention and road sense*'. None of the participants interviewed in this part of the study said anything to indicate that the ADS study suffered in any way from demand characteristics. None, it seemed, knew what the study had set out to investigate.

Study 6.2 - The NPPCR

8.2.1. Method

The design and procedure for this study, together with the participants, is given in the main section of this study, above. Although the same *number* of participants were involved in this study, those participants were in fact different.

8.2.2. Results and Discussion

1. In what ways do you think your behaviour was different in the experiment from what it would have been in the corresponding real environment?

There was of course an obvious reply to this question. It was articulated by one participant as follows:

"Well since I've never been in a real nuclear power plant control room, I haven't got a clue!"

When pressed on how the participants would be likely to behave, were they ever to find themselves in such an environment, most agreed that the simulation did at least *feel* like the real thing. It would seem that there was nothing in the environment to falsify the hypothesis that participants were in a nuclear power plant control room. As one put it:

*"I think that nuclear stations are really like the experiment. It all seemed real. I think the messages helped too, I mean, they sort of seemed realistic. I suppose that in a control room there wouldn't be any silly stick men * going round in circles"*

* The 'stick men' is a reference to the secondary task.

All participants agreed that they had tried, at least at the start of the experiment, to avoid errors. One participant who carried out the experiment under the one-second presentation condition, claimed that she found the experiment impossible. As she said:

"It seems a daft question. I couldn't have been in the real environment, and if I had been I would have just left. The whole thing was impossible and I couldn't do it."

The above participant, it seems from a subsequent interview, was not in any real sense, characterised by a target level of risk. She was not, it seems, particularly concerned with the number of errors she was making as soon as it was obvious that she was unable to operate effectively within the environment.

2. *What were you thinking about while you were carrying out the experiment?*

This question led to remarkably similar answers to the corresponding question from study 6.1. Participants did not believe, nor did they make any attempt to believe, that they were in a nuclear power plant control room environment. They carried out the tasks required of them as if they were carying out an experiment. What they were thinking about whilst they carried out the experiment *was* the experiment.

3. *Some people have said that they treated the experiment like a game. How fair do you think this criticism is?*

All participants agreed that the experiment was not treated like a game. One participant said:

"What do you mean like a game? It was too boring to be a game! It started off all right, but it soon got really boring and I just wanted the thing to be over. You should have made the program more interesting."

4. What do you/did you think the experiment was about? What do you/did you think we were trying to find?

Once more it seems that the experiment was successful in not communicating its real purpose to participants. One imagines that the experimental design employed by the NPPCRS study had some part to play here. Because the design was one of independent participants, and not one of repeated measures, the participants were not aware that either the ratio of alarm to non-alarm messages, or the temporal presentation rate, was being manipulated. Again, there was a similarity with the ADS study in that two participants claimed to have thought that the study was concerned with reaction time.

Study 6.3 - The pencil-and-paper-based simulation

8.3.1. Method

The design and procedure for this study, together with the participants, is given in the main section of this study, above. Although the same *number* of participants were involved in this study, those participants were in fact different. For the purposes of this study, question 3 from studies 6.1 and 6.2 was omitted from study 6.3, since it did not apply to the type of simulation under investigation.

8.3.2. Results and discussion

1. In what ways do you think your behaviour was different in the experiment from what it would have been in the corresponding real environment?

This first question was a cause of some considerable difficulty for participants to answer. The reason for this was that in no sense did participants perceive that they were ever in a simulated environment, in spite of having been told to imagine themselves in the situation described. When this was explained to participants most agreed that they had tried to answer the questions as if they really were in the environment described, but that this was difficult in view of the absence of any context in which to place the symbols.

2. What were you thinking about while you were carrying out the experiment?

All participants were in agreement in answering this question. They were thinking of completing the exercise as soon as possible. All claimed that it was an extrememely unintersting exercise. Participants said that they had not imagined themselves driving the car and that answers had been given in a fairly perfunctory way. One participant spoke for many when he said that:

"It was a really stupid experiment and it was impossible to do. There was no point in any of it. Right, to be honest I couldn't answer some of the questions because I didn't know what the symbols meant - so I asked someone sitting next to me. He wasn't sure, but he told me what he thought the answer was and what you should do about it, driving slower and all that. That was the only way I could do it because otherwise I didn't have a clue about any of it. It wasn't just me though, no-one else was taking it seriously, a lot of the others were laughing and not taking it seriously at all. I don't blame them, it was really stupid."

3. What do you/did you think the experiment was about? What do you/did you think we were trying to find?

There seemed to be genuine confusion here in that participants appeared to believe that the study was intended to examine their ability in recognising the symbols. This could perhaps be considered something of a success, except in so far as it appeared to lead to intentional biases. The true purpose of this study, together with the ADS studies and the NPPCRS study was, it would seem, not recognised by participants. This would seem to indicate that the face validity of the low fidelity simulator could be seriously questioned.

8.3.3. General comments:

In addition to answers to the structured interview, participants in this study provided comments outside the range of these set questions. Comments on the symbol study were dominated by a concern that the instructions were not clear and that it was felt that the study took as its aim the investigation of 'correctness of interpretation', even though the experimenters repeatedly stated that '*[...] we are not interested in what you think these symbols mean or whether your interpretations of them are correct; we are only interested in what you would do if you saw them.*' These statements clearly did not succeed in taking from participants worries about correctness of interpretation and would have to be looked at again if further low-fidelity research were to take place. The question of how the behaviours of participants in the symbol study were different from what one might have expected in the environment they represented brought an interesting reply from one participant:

"The problem was, you were asking us about whether we'd drive slower or faster and all those kind of things, but you didn't tell us things like how fast we were going in the first place."

The possibility of providing baseline behaviours in the simulation was not considered when the simulation was put together. Neither was it picked up by the six participants who took part in the pilot stage of the symbol study. The criticism then, coming as it did at the end of the experimental work, was something of a surprise. It would be tempting to say that in future the problem could be overcome by providing participants with baseline measure such as '*[...] you are driving on a motorway at 70 mph [...]*' This approach would however, whilst getting over one problem, probably create another. In telling participants that they are driving on a motorway at given speed, and then asking them: would they or would they not change their behaviour if some change in the environment were to take place, participants may reply by saying that they would never drive on in the conditions described at such slow speeds, unless they were in some way constrained. If they were constrained, the question of behaviour changes would no longer be bi-directional. Similarly, other groups of road users may reply by saying that they would never drive so fast in the conditions described. One might thererfore say that the issue of the provision of baseline behaviours is a limitation to the low fidelity approach whether they are in fact given or not. There is also the question of whether individuals in such low fidelity work can properly be said to select a meaningful target level of risk. This question will be returned to in chapters 9 and 10.

8.2. General conclusions from study 6.1

Both the ADS and the NPPCRS were successful in that participants were characterised with active accident avoidance and were therefore, it seems fair to conclude, characterised by a meaningful target level of risk that was subject to alteration as a function of environmental risk. The conclusion would, however, appear to be unwarranted in the case of the pencil and paper-based simulation. The ADS and NPPCRS are therefore, on the basis of this study, considered appropriate tools for the investigation of RHT hypotheses, with the qualifications set out above.

It is worth defending the pencil and paper-based simulation in that it may have been the case that participants were at least capable of treating the environment with greater respect than that observed. The critical failing of this simulation would appear to be that participants rejected the device as being without any merit and behaved accordingly. That being so, its actual and intrinsic merit become irrelevant. It must be clear that the investigation of RHT hypotheses through such low fidelity simulation devices would not, at present, seem to be worthwhile.

Chapter 9 - An evaluation of the factor structure of the driving simulators.

9.1. The factor structure of the ADS

9.1.1 Introduction

On completion of the data analysis of study 4, Mathews, Dorn and Hoyes submitted a paper (Matthews *et al.*, 1992) reporting a factor analysis of the ADS from the results reported from study 1. The authors reported having extracted three factors from this earlier experiment: acceleration, steering control, and pedal control. These extracted components were rotated to oblique simple structure.

The results from study 5 offered an excellent opportunity to carry out a further factor analysis, since to the original analysis of study 1, further measures corresponding to overtaking behaviour were now available (adding a further seven measures); a second analysis would serve to cross-validate the first and the possibility of a rotated orthogonal solution could be examined.

9.1.2. Procedure and Results

The factor extraction rule for this analysis was taking eigenvalues greater than one. On both the low and high physical risk conditions there was no clear scree on roots greater than one, and so the scree test was not used as a method of determining the number of factors to extract. The high risk and low risk conditions of study 5 were given separate analyses for two reasons. First, on *a priori* grounds there were no reasons to suppose that the two conditions of environmental risk would not result in different factor structures. If this were so, the simplification of measures through a factor solution would probably be inappropriate. Second, the separate analysis allowed a further cross-validation of the factors extracted. This meant that only those factors common to both analyses need be considered. Both analyses will, however, be discussed together.

The dependent measures that were entered into the analysis were: *overtakes ending in collision with another vehicle; aborted overtakes; overtakes ending in collision with the kerb; total number of overtakes, number of risky overtakes (defined as overtakes in which an oncoming car was visible at the point at which the driver of the simulator pulled out to overtake); successful overtakes; mean temporal leeway (time between returning to the correct side of the road after an overtake and the point at which another vehicle (oncoming) would have collided with the simulated car had the driver not pulled back); distance to car in front; position to centre white line (in metres); mean speed; accelerator angle; brake (% of run spent in use); steering (a direct measure from the steering wheel); number of collisions with another vehicle; number of collisions with the kerb*. Although it might seem that many of these measure were so nearly measuring the same thing that a factor analysis could not be carried out, examination of the intercorrelation matrix showed this not to be the case: measures were, it seemed, independent enough to allow a factorial solution to be revealed.

The method of factor extraction - taking eigenvalues greater than one - tends to produce a less robust solution than a simple scree test. The reason for using this method is twofold. First, the purpose of this work is exploratory, and so all factors of potential interest justify examination. Those factors that might be extracted on the basis of the eigenvalues greater than one rule, that would not be extracted on the basis of a scree test may be eliminated after initial examination. Second, these experimental data appeared over two trials, thus allowing the second trial to serve as a pseudo cross-validation of the first. It was felt that if trivial and uninterpretable factors are to be extracted on the first factor analysis, they will not be repeated in the second and the problem of robustness will thus be taken into account. In fact, since there is no clear scree over the roots greater than 1 in both conditions, the scree test would lead to the same factors being extracted.

Both conditions were analysed using "Statview" (a package for Apple Macintosh). Unlike Mathews *et al.* (1992), this analysis extracted five orthogonal factors. These were, for the low risk condition:

<u>Factor order and interpretation</u>	<u>Variance proportion(%)</u>
Factor 1 - Tendency to overtake;	23.0
Factor 2 - General risk-taking tendency;	18.2
Factor 3 - Steering control;	11.8
Factor 4 - Pedal control;	10.3
Factor 5 - Overtaking errors.	7.6

A sixth factor was extracted on which only mean temporal leeway had a factor loading of more than .204. Since this factor was not extracted on the high risk condition, since mean temporal leeway was, on the high risk condition, strongly loaded on factor 1, and since its logical place in factor one is beyond question, this factor will not be considered further, except to say that it accounted for just 7.1% of observed variance and had a loading on that factor of .948.

Factor extraction from the high risk condition resulted in much the same structure, although the order of extraction was reversed for overtaking errors and for pedal control.

<u>Factor order and label</u>	<u>Variance proportion (%)</u>
Factor 1 - Tendency to overtake;	25.0
Factor 2 - General risk-taking tendency;	22.1
Factor 3 - Steering control;	11.6
Factor 4 - Overtaking errors;	8.6
Factor 5 - Pedal control.	7.2

Since the factor analyses reported in this chapter are used primarily as an exploratory device, the relationship between the measures and factors are discussed. However, to aid understanding of the factor solution, table 9.1 and table 9.2 are provided to show the measures making up the extracted factors. Table 9.1 shows the factor solution for the high-risk condition. Table 9.2 shows the factor solution for the low-risk condition.

Factor analysis for high risk condition	
Factors and measures	Factor loadings
Factor 1 - Tendency to overtake	
risky overtakes	.85
successful overtakes	.84
total overtakes	.83
mean leeway of overtakes	.66
Factor 2 - General risk-taking	
mean speed	.87
distance to car in front	.86
other-vehicle collisions	.80
Factor 3 - Steering control	
kerb collisions	.87
position on road	.55
steering	.51
Factor 4 - Overtaking errors	
aborted overtakes	.76
overtakes with vehicle collision	.68
overtakes with kerb collision	.56
Factor 5 - Pedal control	
brake use	.83
accelerator angle	.72

Table 9.1 - Factor solution for the high-risk condition of study 5

Factor analysis for low risk condition	
Factors and measures	Factor loadings
Factor 1 - Tendency to overtake	
risky overtakes	.96
successful overtakes	.88
total overtakes	.67
overtakes with vehicle collision	.67
Factor 2 - General risk-taking	
mean speed	.89
distance to car in front	.89
other-vehicle collisions	.87
Factor 3 - Steering control	
kerb collisions	-84
position on road	.78
steering	.41
Factor 4 - Pedal control	
brake use	.92
accelerator angle	.84
Factor 5 - Overtaking errors	
overtake with kerb collision	.71
aborted overtakes	.68

Table 9.2 - Factor solution for low-risk condition of study 5

9.1.3. Factor loadings and the ADS behaviours

Overtakes ending in collision with another vehicle Just two measures appeared to be loaded on different factors on the two different conditions: overtaking ending in collision with another vehicle is the first of these. On the condition of high risk it appears to be loaded (a positive loading) on factor 4 - *overtaking errors* (it has a factor loading here of .676). On the condition of low risk it appears to be loaded on factor one - *tendency to overtake* (here it has a factor loading of .668). For this condition the item loading with factor 4 is just .109; on the condition of high environmental risk the item loading with factor 1 is .295. There would then seem to be some confusion over how best to interpret this item on the basis of the factor solutions presented here. However, it could be said that logically it would make good sense to interpret the measure either way. The measure of overtakes ending in collision with another vehicle could, in other words, be seen as both part of a tendency to overtake (if this tendency is not present in a participant the value for that measure will, after all, be zero) and at the same time as part of a wider category of overtaking errors. A certain amount of caution should of course be exercised in interpreting factors that were extracted on the basis of Eigen values greater than one, rather than the more reliable methods.

A further question relating to this measure is that of why migration should occur from the one factor to the other in different conditions of environmental risk. Perhaps some of the answer to this comes when one considers that overtaking errors in the high risk condition are more likely to be met with collisions, given that they occur at all. Put simply, in the low environmental risk condition, there is a lower probability that participants will pay for an error in overtaking by a collision with another vehicle.

<u>Aborted overtakes</u> This measure would appear unambiguously to be loaded on the factor labelled *overtaking errors*. On the high environmental risk condition this factor was the fourth to be extracted and here aborted overtakes was loaded at .758 (again, a positive loading on both conditions). On the low environmental risk condition the *overtaking errors* factor was the fifth to be extracted and on this factor aborted overtakes was loaded slightly lower at .679. Aborted overtakes then could be said to be just one kind of overtaking error.

<u>Overtakes ending in collsion with the kerb</u> This measure too would appear to be part of the factor *overtaking errors*. For the high environmental risk condition it was loaded at .558, and on the low environmental risk it was loaded at .712 (again, a positive loading on both conditions). Why it should have a higher loading on the low environmental risk condition is uncertain; perhaps it results from reduced variance arising from fewer

overtakes taking place in this condition.

Total number of overtakes This measure was associated with high loadings across the two environmental risk conditions on the factor of *tendency to overtake*. Loadings appeared as .833 and .668 for high and low environmental risk conditions respectively (again, a positive loading on both conditions).

Number of risky overtakes The measure of number of risky overtakes also appears to be loaded on the factor of *tendency to overtake* (again, a positive loading on both conditions). The loadings here were .849 for high environmental risk and .961 for low environmental risk.

Successful overtakes Successful overtakes too appeared under *tendency to overtake* on both conditions (a positive loading on both conditions). With the high environmental risk condition the loading appeared as .882; with the low environmental risk the loading was .836.

Mean temporal leeway (time between returning to the correct side of the road after an overtake and the point at which another vehicle (oncoming) would have collided with the simulated car had the driver not pulled back) This was the second measure that was apparently loaded on different factors on the two conditions of environmental risk. On the high environmental risk condition it was loaded on the first factor - *tendency to overtake* at .66. Yet on the condition of low environmental risk the measure was loaded on the sixth factor to be extracted at .948 (a positive loading on both conditions). The next highest loading on this factor is just .204 (with the measure of aborted overtakes), leading to the conclusion that for this second analysis mean leeway would appear not to be reducible to an element of general overtaking behaviour. Although mean leeway is, more than any other, the measure on which one would expect to see a difference across the two conditions of environmental risk, the question of how one should interpret its migration from factor 1 to its independence is far from clear. Perhaps it indicates a strategy change from making judgements dependent on other overtaking behaviours to making them independently.

Distance to car in front This measure was loaded on *general risk-taking tendency* on the high environmental risk condition at .857. On the low environmental risk condition its loading appeared as .892 (a positive loading on both conditions). This would seem to suggest that distance to car in front reliably forms part of a more general factor of risk-taking and would perhaps best be interpreted in this way.

Position to centre white line (in metres) This measure was loaded on what was, on both analyses, the third factor to be extracted: *steering control.* Its loading on the condition of high environmental risk was .549; on the condition of low environmental risk that loading changed to .784 (a positive loading on both conditions). Together with kerb collisions and steering this measure forms what seems to be the same steering control factor that Matthews *et al.* extracted as their second factor on an oblique rotation.

Mean speed Speed was highly and unambiguously loaded on the factor of *general risk-taking tendency* (a positive loading on both conditions). On high environmental risk its loading was .869; on low environmental risk it appeared with a slighly higher loading of .886

Accelerator angle This measure is loaded on the pedal control factor (a positive loading on both conditions). For the high environmental risk condition its loading was .716 (on this condition the factor was the fifth to be extracted); on the low environmental risk condition the loading was .839 (but here the factor was the fourth extracted).

Brake (% of run spent in use) Brake use formed the other half of the *pedal control* factor. Its loadings were .833 and .918 for the high and low environmental risk conditions respectively (a positive loading on both conditions).

Steering (a direct angular measure from the steering wheel) This measure had relatively low loadings on the *steering control* factor, although the loadings were relatively reliable across the two environmental risk levels (.507 and .413 for low and high environmental risk respectively). It too had positive loadings on both conditions.

Number of collisions with another vehicle This measure appears under the *general risk-taking tendency* factor on both conditions. Its loading on the high environmental risk condition is .797 and under the low environmental risk condition that figure is increased to .869 (a positive loading on both conditions).

Number of collisions with the kerb The number of collisions with the kerb is loaded on the factor of *steering control* for both analyses. This measure is the only one to appear with a negative loading. For the high environmental risk its loading is -.867; for the low environmental risk condition its loading is -.844.

9.1.4. Conclusion

Three important conclusions seem to follow from this analysis. First, the measures extracted from the ADS appear to come together into a logical and coherent factor solution. This by itself would seem to lend weight to the argument that the ADS has some construct validity with the on-street behaviour it seeks to mimic. Of course, this does assume that these extracted factors would be obtained from the natural observations of road traffic - an unverified assumption - and so perhaps content validity might be a better term. Second, the orthogonal solution presented here comprises significant and sizable factors accounting for a large proportion of the variance within measures. The solution, therefore, is not a trivial one. Third, the factors extracted would appear to be stable over the two experimental sessions. However, in the strict sense, the second solution cannot be considered a cross-validation of the first: a useful exercise in the future might be to repeat the analysis on a different participant population.

9.2. Factor analysis of study 4

9.2.1 Introduction

The purpose of factor analysing measures taken on the behavioural pathway list of study 4 was twofold. First, would the factor structure of the ADS derived from study 5 data be mirrored in the pattern of responses on the pencil-and-paper approach of study 4? Were this so, some basis might exist for supposing that the two forms of simulation were in essence measuring the same thing. Second, in the absence of a *similar* factor solution, can the factor solution to emerge be given a logical interpretation which can be reconciled with the driver behaviour it seeks to simulate? The analysis which follows was intended to evaluate further the suitability of the low-fidelity approach in RHT simulation and should be seen as part of the methodological development with which this thesis is partly concerned.

9.2.2. Procedure

A principal components analysis was carried out. The factor extraction rule for this analysis was that followed for study 5: eigenvalues >1 being required for extraction. Although this experiment does not allow for the same internal cross validation, and hence potentially suffers from the reliability problem associated with this decision rule of factors to extract, the same rule was applied in order to make a valid comparison of factor structure. The point, in fact, is academic, since, just as in study 5, a scree test would also justify the same number of extracted factors (see figure 9.1):

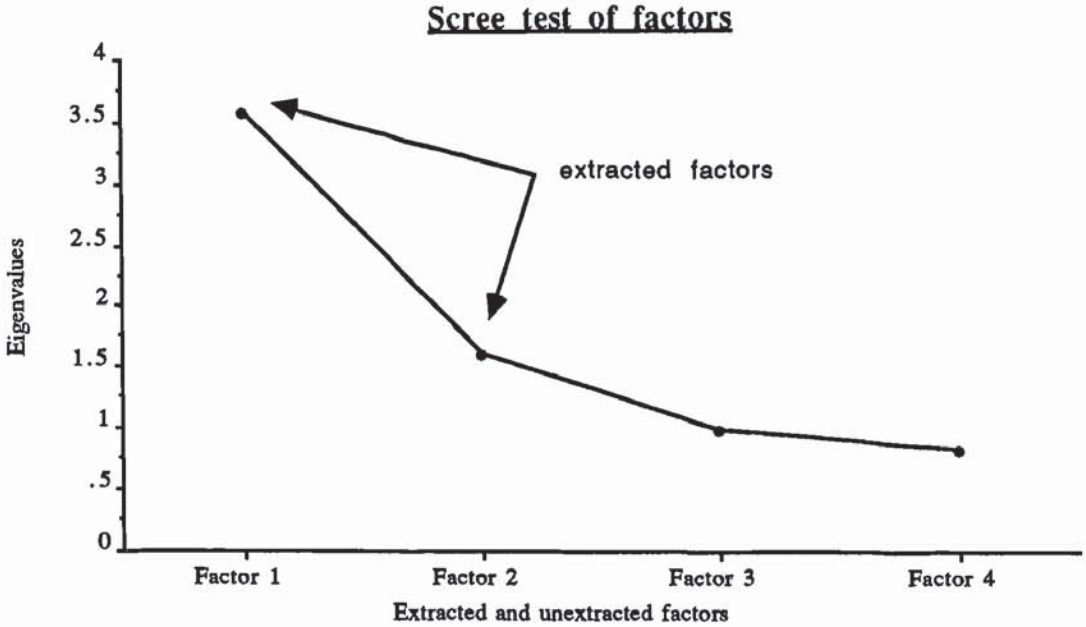


Figure 9.1: Scree test of factors to be extracted.

Although study 4 involved taking measures of the eight behavioural pathways on 36 different stimuli items, only responses to the first of these 36 stimuli were entered. This was because debriefing of participants revealed that the task was, to cite just one comment, *"utterly boring..."* It was suggested that most attention and serious consideration was given to items earlier on in the questionnaire. For this reason it was felt that to factor analyse responses to the first stimulus item to appear would be to look at the factor structure of the device at its most powerful. Only if this analysis proved useful in pointing up evidence for a factor structure that might validate the approach would further analysis be needed of subsequent responses.

9.2.3. Results and Discussion

The dependent measures from the eight behavioural pathway choices given in study 4 were entered for analysis. Question responses appeared on a scale of -7 through zero (no change) to positive 7.

1.Drive slower; Drive faster.

2.Carry out fewer overtakes; Carry out more overtakes.

3.Carry out relatively less risky overtakes; Carry out relatively more risky overtakes.

4.Use brake pedal very much less; Use brake pedal very much more.

5.Concentrate less on your driving; Concentrate more on your driving.

6.Less likely to stop on seeing an amber light; More likely to stop on seeing an amber light.

7.Less likely to momentarily take your eye off the road; More likely to momentarily take your eye off the road.

8.Less likely to adhere to a mandatory speed restriction; More likely to adhere to a mandatory speed restriction.

The first factor to be extracted accounted for 44.3% of original variance. It has an eigenvalue of 3.544. Only one other factor was extracted with the factor rule of eigen value>1 (justified also from the scree test shown above); that had an eigen value of 1.583 and accounted for 19.8% of original variance. The factor loadings are given below for each of the eight behavioural pathways. Again, the loadings are described under the heading of the measures. The factor solution for study 4 is shown in table 9.3:

Factor analysis of Study 4	
Factor	Factor loading
Factor 1	
number of overtakes	.96
'risky overtakes'	.94
speed	.90
attention	-.54
Factor 2	
brake use	.81
'stopping on amber'	.70
taking eye off road	-.61
adhering to mandatory speed restriction	.57

Table 9.3 - Factor solution for study 4

Item number 1.Drive slower... Drive faster. This has a loading of .90 on factor 1.

Item number 2.Carry out fewer overtakes... Carry out more overtakes. This has a loading of .964, also on factor 1.

Item number 3.Carry out relatively less risky overtakes...Carry out relatively more risky overtakes. This has a loading of .937, again on factor 1.

Item number 4.Use brake pedal very much less... Use brake pedal very much more. This item is loaded on factor 2 at .805.

Item number 5.Concentrate less on your driving... Concentrate more on your driving. This has a loading of -.541 on factor 1.

Item number 6.Less likely to stop on seeing an amber light... More likely to stop on seeing an amber light. This item has a factor loading of .698 on factor 2.

Item number 7.Less likely momentarily to take your eye off the road... More likely momentarily to take your eye off the road. This item too is loaded on factor 2 at -.605.

Item number 8.Less likely to adhere to a mandatory speed restriction... More likely to adhere to a mandatory speed restriction. This has a loading of .566 on factor 2.

Factor 1 then is associated with responses to the questions regarding speed, overtaking (both their frequency and their risk), and concentration.

Factor 2, by contrast shows association with responses to the questions regarding use of brake pedal, likelihood of stopping on seeing an amber light, likelihood of momentarily taking one's eye off the road (the 'attention' question) and likelihood of adhering to a mandatory speed restriction.

These loadings do not offer any immediately intuitive factor solution. The previously extracted factors of tendency to overtake; general risk-taking tendency; steering control; pedal control and overtaking errors are not mirrored in this analysis and hence it must be concluded that the factor structure of the ADS and of this low fidelity simulation are quite different, if indeed the low fidelity simulation presented here can be said to have any factor solution at all. It is suggested that the factor structure observed above is probably an experimental artifact and cannot meaningfully be given the logical interpretation that would be required for labelling the extracted factors.

9.3. Study 7 - Re-designing and reapraising the low fidelity driving simulation

9.3.1. Introduction

Reported here is an attempt to improve the low-fidelity simulation as a method for the investigation of RHT. The failings of the earlier attempt at low-fidelity simulation (study 4) are discussed in the discussion section of that experiment. For the purpose of improving the simulation, the following changes were deemed necessary:

(1) The length of the questionnaire should be reduced. Study 4 consisted of no fewer than 36 stimuli, each multiplied by 8 behavioural pathways. In addition, participant data including sex, whether a driving licence was held, etc were also gathered. All of this combined to mean that the completing of the questionnaire took some 20-35 minutes of what was described by one participant as "*utter boredom*". (The six participants who piloted the study were 'kind' enough not to mention this feature of the procedure!) The response set to emerge from these data led to the hypothesis that the questionnaire was answered with something less than the attention it required to consider it a valid measure. It was therefore hoped that more attention might be paid to a questionnaire that involved fewer questions. For this reason a between-participants design was considered more appropriate for future research with each participant responding to sixteen pathway questions for just a single scenario. It should, however, be remembered that the factor solution to study 4 took place using 'first symbol' measures (those responses to the first of the 36 questions) and so whilst boredom and fatigue may have had some detrimental effect so far as the ANOVAs were concerned, they cannot be said to have played any part in the disappointing factor solution to emerge from that experiment.

(2) Following also from study 4 was the issue of including non-drivers in the participant sample. During Study 4, it was felt that the questions did not depend on any motor or perceptual skills that existed in drivers but not in non-drivers and so non-drivers may just

as well be included in the experiment. After this experiment it was felt that the specific nature of the behavioural pathways may well mean that the use of non-drivers would be inappropriate. Perhaps, then, participants can better answer the pathway questions where only drivers are selected.

(3) It might be said that the search for a factor structure similar to the ADS in the low-fidelity approach adopted in study 4 was doomed from the start in that the questionnaire approach of that experiment involved slightly different pathways from those employed in the ADS. Whilst this puts to one side the criticism that study 4 failed to provide any evidence at all for *any* sort of logical factor solution, a more useful approach in comparing the two methods would be to mirror, as far as possible, the ADS behaviours.

(4) With study 4 the emphasis had been on "*....what would you do if you were to see the following symbol?*" rather than "*....what does the following symbol mean?*" The focus of attention, in other words, was on what participants would do rather than on the meaning of the symbols. Indeed, a certain amount of confusion here was required in order to change the level of environmental risk through the manipulation of ambiguity. It should, however, be acknowledged that the requirements made of participants were not satisfactorily communicated to them. The objection of "*....but I don't know what the symbols mean, so how can I answer the questions?*" was a common one and was not in any sense allayed by the experimenter's telling them that it was this very state of confusion that was of interest in the experiment. It seems therefore likely that a somewhat perfunctory set of responses were given. A useful change to any subsequent work might therefore involve a qualitatively different independent variable to represent environmental risk.

(5) Through necessity, study 4 involved a repeated measures design (the initial plan had been to correlate compensation scores with an awareness questionnaire) without counterbalancing. Had the design been counterbalanced, the presentation of, say, symbol text and action before the presentation of the corresponding symbol should have

confounded the ambiguity effect. A change to subsequent work might therefore either allow for the possibility of counterbalancing (or some other device to control for order effects) or be independent in design.

Again, though, it should be pointed out that the factor solution to study 4 made use only of first symbol responses and so the less than perfect design of this experiment cannot have affected the factor structure.

(6) One likely contributory cause to the factor solution of the Aston Driving Simulator may have been the mechanical interdependence of the measures. It is, for example, impossible to increase mean speed beyond a given point without overtaking other vehicles. It is impossible also to carry out more risky overtakes (overtakes in which an oncoming vehicle was visible at the time of pulling out to overtake) without also reducing the mean temporal leeway. In addition, a consequence to more risky driver behaviour is (usually) an increase in accident loss. In these examples and others like them, certain kinds of driver behaviour tend to cluster whether drivers, when asked, imagine they do or not.

With study 4 however, this was not the case. The driver behaviours were chosen such that they could theoretically exist independently of one another. In order to avoid this, participants may have needed to have been reminded that their responses to some questions should affect their responses to others. Subsequent experimental work might therefore attempt to make two changes to the behavioural pathways included. First, there should be some mechanical interdependence between the behaviours on which measures are taken to bring the simulation more in line with the ADS in this respect. Second, that some of the measures may not be interdependent should be stressed to participants.

A further purpose of this study is to examine whether the simulation device reported here is capable of differentiating between 'drivers' in terms of their age and sex. The rationale for this comparison is derived from a number of studies (eg, Hagen, 1975; Dorn, 1992) that have demonstrated sex differences in driving. Amongs other things, males drove closer to the centre line, they drove faster, and pressed harder on the accelerator. Other research (eg Jonah, 1986; Dorn, 1992; Dorn, Glendon, Hoyes, Matthews, Davies and Taylor, 1991) has shown that older drivers differ from younger drivers (the cut-off point between old and young being at the age of 30) in a similar way such that younger drivers drive faster. If the simulator reported here is capable of differentiating between male and female and old and young drivers in a similar way across the same behaviours, this would serve to validate the device.

So far as Wilde's RHT is concerned, this study tests proposition 13 (1988) in which Wilde states that interventions that do not change the target level of risk will, through closed loop control, be unsuccessful in reducing accident loss. Here, the intervention chosen was the level of environmental risk as reflected in the efficiency of the brake system.

Links with previous studies

Study 4 represents what was to some extent an unsuccessful attempt to simulate a physical risk-taking environment for the purpose of testing RHT hypotheses. Some of the limitations of study 4 are addressed here in the final study of this thesis. In addition, a further attempt is made to examine the validity of this low-fidelity simulation. This involves the comparison of the factor solution of the ADS with the low-fidelity simulator's factor solution. It involves too the examination of sex and age differences in the low-fidelity environment. Since the ADS has been shown to be sensitive to these differences across a range of dependent measures, if the low-fidelity simulator in not capable of mirroring these difference, it could be argued that it is not measuring the same constructs.

Study 1 attempted to manipulate intrinsic risk via the efficiency of the brakes fitted to the Aston Driving Simulator. This is a closed-loop simulation, and therefore leaves the question of whether an open-loop simulation would produce the same findings at reduced costs and in less time. For this reason, Study 7 involved the same intervention to *intrinsic safety.*

9.3.2. Method

Participants

98 participants took part in this study. All had a full British driving licence.

Equipment

The following questionnaires were given to the 98 participants. Forty-nine copies of version 1 were given to a random allocation of the forty-nine participants; forty-nine copies of version 2 were given to the remaining participants. All participants held a full driving licence.

Version 1:

Specific driver behaviour perceptions

Suppose you are driving on a busy 'A' road. There are lots of cars on the road; some are ahead of you and moving quite slowly, so if you are to maintain a reasonable speed, you may find yourself wanting to overtake. There are quite a few on-coming cars. The weather conditions and the light are both good. You are aware that your brakes are slightly inefficient resulting in longer pulling-up distances.

Please answer the following questions as honestly as you can. There is no right or wrong answer. When answering the questions, remember that your responses to some questions should affect your answers to others. For example, if you say that you would be unlikely to overtake, you must, as a consequence, drive slower. In each case you should circle a number between one and ten. The number one represents, *in your view*, the minimum *realistic* value that could be assigned to the behaviour in question. The number ten represents, *again in your view*, the maximum *realistic* numerical value that could be assigned to the behaviour in question.

example: if you were asked about what percentage of time you would be likely to take your eyes off the road for, you may think that 0.5% would be the realistic minimum and that 2% would be the realistic maximum. If you were then faced with a response on the following scale:

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

you might let the number 1 represent 0.5% and the number 10 represent 2%. There would be equal intervals between the points.

IF YOU HAVE ANY QUESTIONS, PLEASE ASK THE EXPERIMENTER NOW, OTHERWISE YOU MAY PROCEED.

1. How many times would you overtake other vehicles?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

2. How hard would you press down on the accelerator?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

3. How likely do you think you would be to collide with another vehicle?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

4. How big a time gap would you allow in overtaking vehicles?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

5. How often do you think you would accidentally cross the centre-white line of the road?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

6. How often would you apply pressure on the brake pedal?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

7. How likely do you think you would be to take your eye off the road?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

8. How likely would you be to make an error whilst overtaking?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

9. How likely would you be to overtake a vehicle when an on-coming vehicle is visible in the distance?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

10. How large (on average) would be the 'safety' distance between you and the car in front of you?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

11. How likely would you be accidentally to hit the kerb?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

12. How often would you apply pressure to the clutch pedal?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

13. How fast would you be likely to travel?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

14. How likely do you think you would be to to pull out to overtake a car and then have to pull back without completing the overtake?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

15. How likely would you be to hit the kerb whilst overtaking another vehicle?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

16. How erratic would your steering wheel control be?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

Version 2:

Specific driver behaviour perceptions

Suppose you are driving on a busy 'A' road. There are lots of cars on the road; some are ahead of you and moving quite slowly, so if you are to maintain a reasonable speed, you may find yourself wanting to overtake. There are quite a few on-coming cars. The weather conditions and the light are both good.

Please answer the following questions as honestly as you can. There is no right or wrong answer. When answering the questions, remember that your responses to some questions should affect your answers to others. For example, if you say that you would be unlikely to overtake, you must, as a consequence, drive slower.

In each case you should circle a number between one and ten. The number one represents, *in your view*, the minimum *realistic* value that could be assigned to the behaviour in question. The number ten represents, *again in your view*, the maximum *realistic* numerical value that could be assigned to the behaviour in question.

example: if you were asked about what percentage of time you would be likely to take your eyes off the road for, you may think that 0.5% would be the realistic minimum and that 2% would be the realistic maximum. If you were then faced with a response on the following scale:

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

you might let the number 1 represent 0.5% and the number 10 represent 2%. There would be equal intervals between the points.

IF YOU HAVE ANY QUESTIONS, PLEASE ASK THE EXPERIMENTER NOW, OTHERWISE YOU MAY PROCEED.

1. How many times would you overtake other vehicles?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

2. How hard would you press down on the accelerator?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

3. How likely do you think you would be to collide with another vehicle?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

4. How big a time gap would you allow in overtaking vehicles?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

5. How often do you think you would accidentally cross the centre-white line of the road?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

6. How often would you apply pressure on the brake pedal?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

7. How likely do you think you would be to take your eye off the road?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

8. How likely would you be to make an error whilst overtaking?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

9. How likely would you be to overtake a vehicle when an on-coming vehicle is visible in the distance?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

10. How large (on average) would be the 'safety' distance between you and the car in front of you?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

11. How likely would you be accidentally to hit the kerb?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

12. How often would you apply pressure to the clutch pedal?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

13. How fast would you be likely to travel?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

14. How likely do you think you would be to to pull out to overtake a car and then have to pull back without completing the overtake?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

15. How likely would you be to hit the kerb whilst overtaking another vehicle?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

16. How erratic would your steering wheel control be?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

Design and procedure

This study had two distinct aims: first, to test RHT hypotheses on a single factor, two level between-participants design, operationalizing environmental risk through the information that the brakes in a simulated driving environment were operating on one of two levels of efficiency; second, it aimed to validate further the methodology of the low-fidelity simulation of the pencil-and-paper-based approach through an additional factor analysis. Third, it aimed to examine possible age and sex differences in an attempt to validate the simulation device through its construct validity with the ADS. One could, however, say that the purpose of the study was singular: to test further the usefulness of this low-fidelity approach in simulating physical risk-taking. The extent to which the design might or might not be successful in providing envidence for RHT was seen as a valid part of this issue.

The reason for using a self-referenced scale was that study 4 had resulted in confusion over the reference anchors of the dimensions. It was therefore felt that the maximum and minimum values should mean something concrete, and that their meaning be understood by participants. Earlier informal pilot work suggested that the provision of absolute values here led to the response that answers were impossible because behaviour such as specific speed would depend on a great many other factors. What was needed, said the pilot sample, was a scale taking these other factors into account. On being shown the scale employed in this study, the sample suggested that this was appropriate and now possible to answer.

Participants were randomly allocated to one of the two experimental conditions: high and low environmental risk. The condition of high environmental risk contained the additional information in the instructions to participants that :

"You are aware that your brakes are slightly inefficient resulting in longer pulling-up distances."

The conditon of low environmental risk contained no information regarding the efficiency of the brakes.

The factor solution was arrived at through an oblique analysis, since there was no *a priori* basis for supposing the extracted factors to be independent. The number of factors to be extracted was determined by a scree test.

NB - The inclusion of an experimental design relevant to the RHT question leaves this study somewhere between the two joint aims of this thesis. It is concerned with both the development of a methodology for testing RHT predictions in that it seeks to improve the earlier low-fidelity study. Yet at the same time it operationalizes environmental risk. It appears under the former category only because the methodology development was its principal aim, the testing of an RHT hypothesis being something of a convenient add-on.

One could however say that this study aimed to test the effectiveness of this low fidelity approach to simulation in RHT by attempting to answer three questions:

1. Does the simulator differentiate between behaviours across different levels of

environmental risk? Does it, in other words, provide evidence for risk homeostasis theory?

2. Does the simulator differentiate between the same groups of road users as the ADS (which itself differentiates successfully between young and old drivers and male and female drivers in the same way as 'real' road traffic situations)?

3. Finally, is the simulator characterised by the same factor solution as the ADS? If not, one might ask whether the factor solution is at least reconcilable with, or related to, the ADS solution, and whether the factor solution to emerge from the present simulation device has any logical basis.

9.3.3. Results

The results here will be structured around the three questions listed above.

1. Support for RHT?

First then, did the simulator differentiate between behaviours across the two levels of environmental risk? The answer here is an unqualified no. Across the sixteen specific driver behaviours there were no significant differences. Since the precise *F* values and *p* values are therefore of limited interest, they are provided in appendix 2.

It is, however, interesting to look at the difference in mean scores across behaviours that might be expected to show some effect, namely, the average safety distance from the vehicle in front (question 10), and the mean speed (question 13). Both of these behaviours would certainly be relevant to compensation for the inefficiency of a car's brakes. Their observed differences can be seen in figures 9.2 and 9.3.

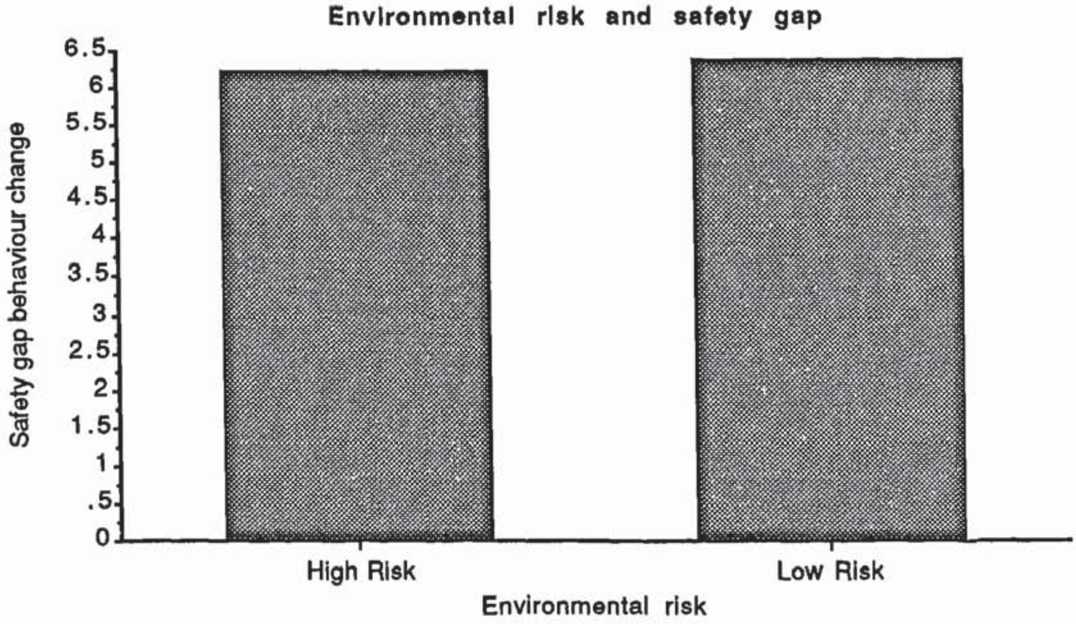


Figure 9.2 : Environmental risk and safety gap response (F<1, NS).

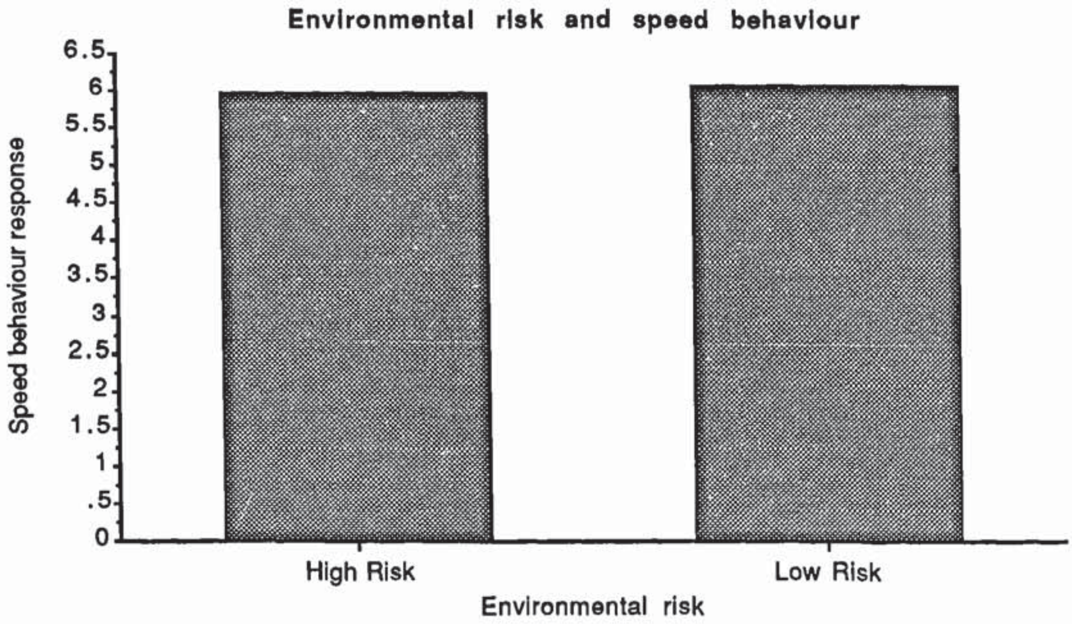


Figure 9.3 : Environmental risk and speed behaviour response (F<1, NS).

The simulator was therefore unable to differentiate between different levels of environmental risk. Only the NS results on the accident loss items provide any support for RHT at all. What participants were saying, it seems, was that accident loss would be maintained at a constant level, but as for how this could be achieved, they had no idea!

2. Differentiating across age and sex groups

Since the ADS has already been shown to be capable of differentiating between individuals in terms of their age and sex in much the same way as the real environment on which the ADS is modelled, the question of how well this low fidelity simulation can distinguish between these same groups is of key interest. For the purposes of this comparison, young drivers were defined as those aged 29 years or younger; older drivers were those aged 30 years or over. The age of 30 was selected because experimental evidence suggests that this cut off point differentiates successfully between the two groups in terms of a number of specific driver behaviours, including those examined below (Dorn, 1992).

Question 1 was concerned with the number of overtakes and here one would expect to see both age and sex differences. For sex differences, only non-significant findings appeared ($F[1,85]<1$, NS). For age differences the same was true ($F[1,85]<1$, NS).

Question 2 measured the pressure on the acelerator that the participants stated they would apply. Again there was no significant sex differnce ($F\ [1,85] <1$, NS). However, the age difference was significant ($F\ [1,85] =7.84$, $p=.0063$).

Question 3 reflected the subjective probability of collision with another vehicle. The ADS showed evidence for both sex and age differences here, but this study showed neither. For the factor of sex the $F\ [1,85] <1$, NS; for the factor of age $F\ [1,85] =2.83$, NS.

Question 4 was intended to measure the temporal gap for overtaking. Here again there was neither a sex nor an age difference (respectively: $F\ [1,85] <1$, NS; $F\ [1,85] <1$, NS).

Question 5, the crossing of the centre white line in the road, also failed to provide evidence for either a sex or an age difference (respectively: F [1,85] =1.43, NS; F [1,85] <1, NS).

Question 6 was the brake pedal pressure question. Again there was no evidence for either a sex or an age difference (respectively: F [1,85] =1.03, NS; F [1,85] <1, NS).

Question 7 was included as an attentional question and asked participants how many times they would take their eyes off the road. Again there was no evidence for either a sex or an age difference (respectively:F [1,85] <1, NS; F [1,85] =1.74, NS).

Question 8 asked participants how likely they were to make an error whilst overtaking. Again there was no evidence for a sex difference (F [1,85] <1, NS), although there was on this measure some evidence for an age difference (F [1,85] =4.78, p=.0315).

Question 9 was concerned with the probability of making a risky overtake. For the purposes of this question the term 'risky' was defined in the same was as the ADS - pulling out to overtake a vehicle when an oncoming vehicle is in sight. No evidence was found here for a sex or an age difference (F [1,85] =1.458, NS; F [1,85] =2.08, NS, respectively).

Question 10 was concerned with the safety gap between vehicles. Again there was no evidence for either a sex or an age difference (respectively: F [1,85] <1, NS; F [1,85] <1, NS).

Question 11 was intended to measure the hitting of the kerb. Here too there was no evidence for either a sex or an age difference (respectively: F [1,85] <1, NS; F [1,85] <1, NS).

Question 12 was a pedal control question, this time looking at frequency of clutch use. This is an interesting measure in that it does not appear on the ADS, since that is modelled on an automatic car. However, again there was no evidence for either a sex or an age difference (respectively: F [1,85] <1, NS; F [1,85] <1, NS).

Question 13 is perhaps the most interesting of all measures as a candidate for sex and age diffences, since the purpose of this question was to measure (or rather, reflect) mean speed. This is known to be a measure on which evidence exists for both sex and age differences, yet, and once again, there was no evidence for either a sex or an age difference (respectively: F [1,85] <1, NS; F [1,85] =1.978, NS).

Question 14 was included as another measure intended to mirror an ADS measure - that of aborted overtakes (pulling out to overtake another vehicle, and then pulling back without that overtake being completed). It provided no evidence for a sex difference (F [1,85] <1, NS), but did provide evidence for an age difference (F [1,85] =6.28, p=.0141).

Question 15 reflected the hitting of the kerb whilst overtaking another vehicle. Again there was no evidence for either a sex or an age difference (respectively:F [1,85] <1, NS; F [1,85] <1, NS).

Question 16 was a measure of erratic steering. It too provided no evidence for either a sex or an age difference (respectively: F [1,85] <1, NS; F [1,85] =2.81, NS).

Taking all of these behaviours together there is very little evidence that the present simulator is capable of differentiating between groups of different ages, and between male and female drivers in the way that the ADS is capable of doing. Whilst Dorn (1992) found considerable evidence for sex differences in driving behaviour on the ADS, only age differences were found here. These were confined to accelerator pressure, overtaking errors, and aborted overtakes. In the light of the evidence *against* sex and age differences, these are probably not worth further consideration.

Only five graphs are provided to illustrate age and sex differences: graphs for the three measures on which an age difference was significant, and graphs for the non-significant but hypothesised age and sex differences for chosen relative speed. The three significant age differences are shown in figures 9.4, 9.5 and 9.6. Sex and age differences would be predicted on the measure of mean speed if the simulator is capable of distinguishing between older and younger, and male in female, drivers in the same way as the ADS. For completeness, therefore, the actual age and sex differences in chosen relative speed are provided (see figures 9.7 and 9.8).

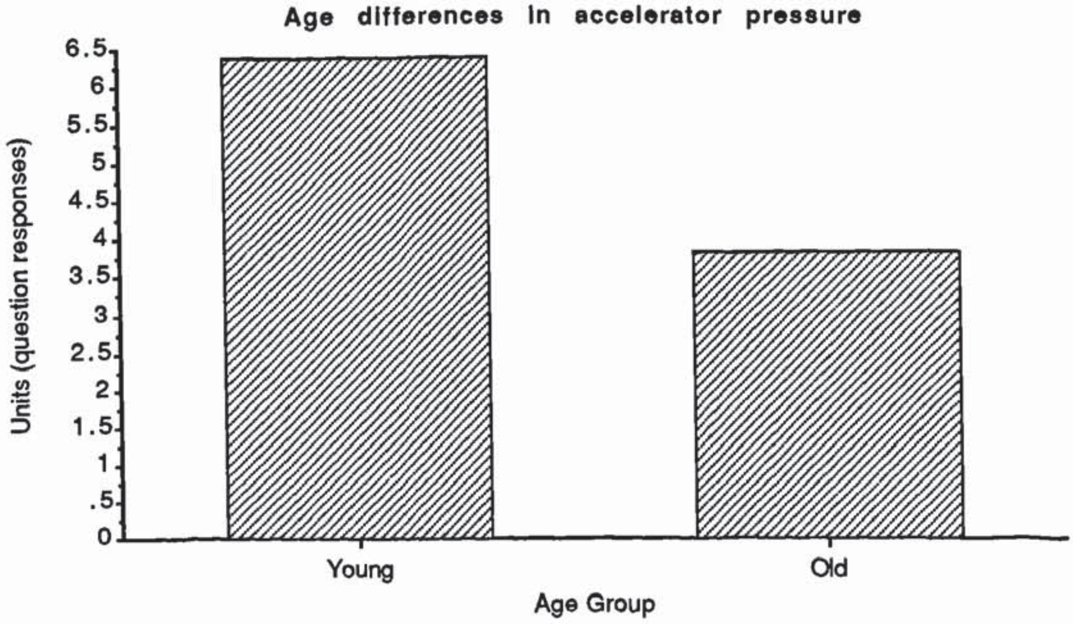


Figure 9.4 : Age differences in accelerator pressure.

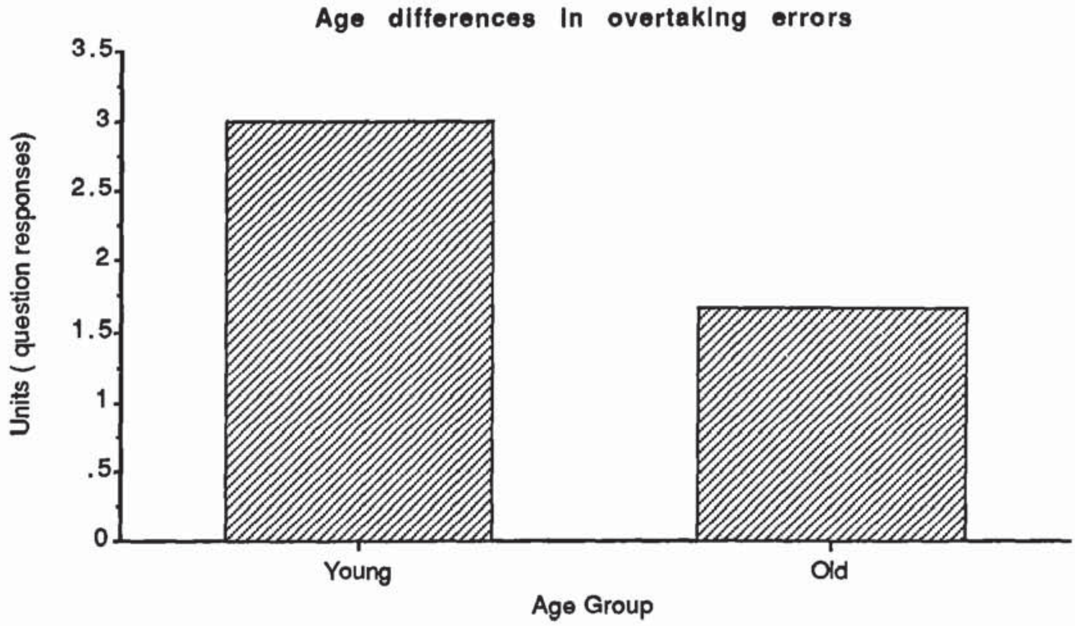


Figure 9.5 : Age differences in overtaking errors.

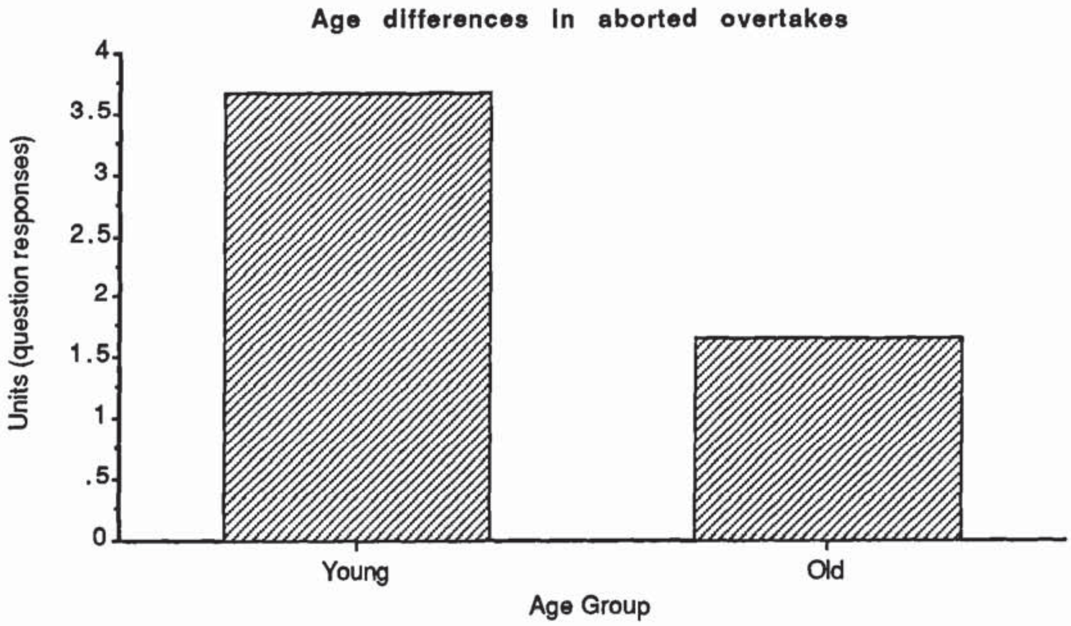


Figure 9.6 : Age differences in aborted overtakes.

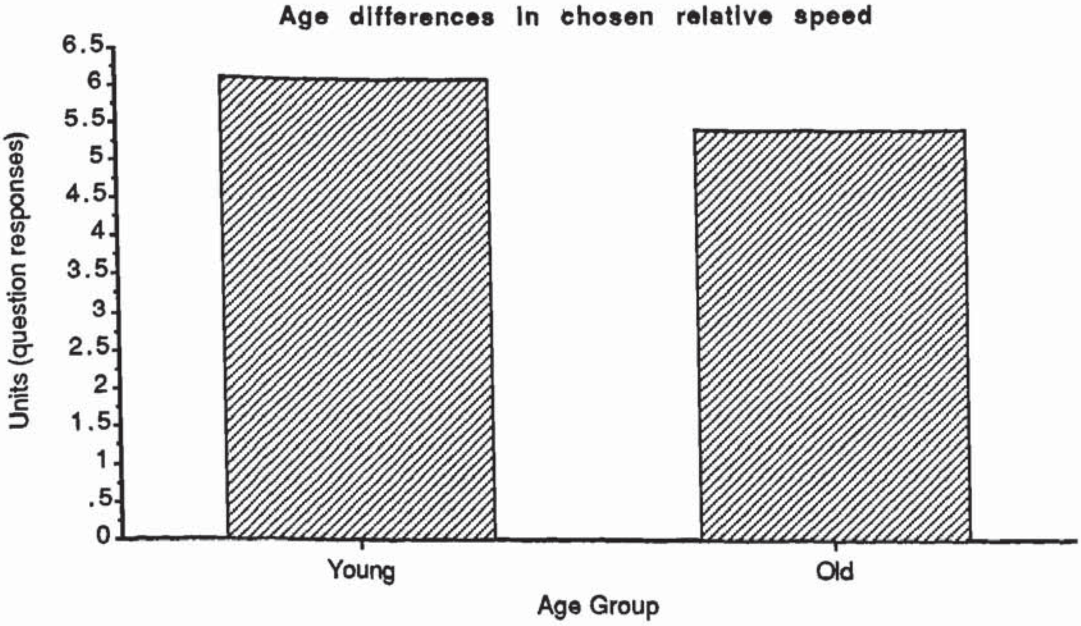


Figure 9.7 : Age differences in chosen relative speed.

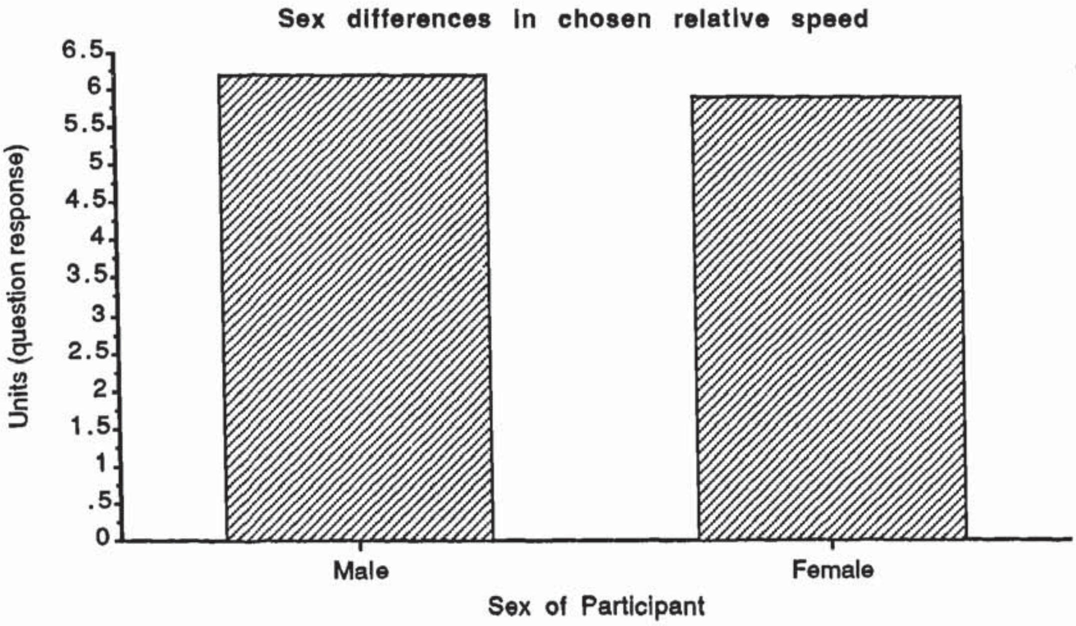


Figure 9.8 : Sex differences in chosen relative speed.

3. An improved factor solution?

This time the measures were given an oblique rotation with a scree test to determine the number of factors to extract. The scree test is shown in figure 9.9:

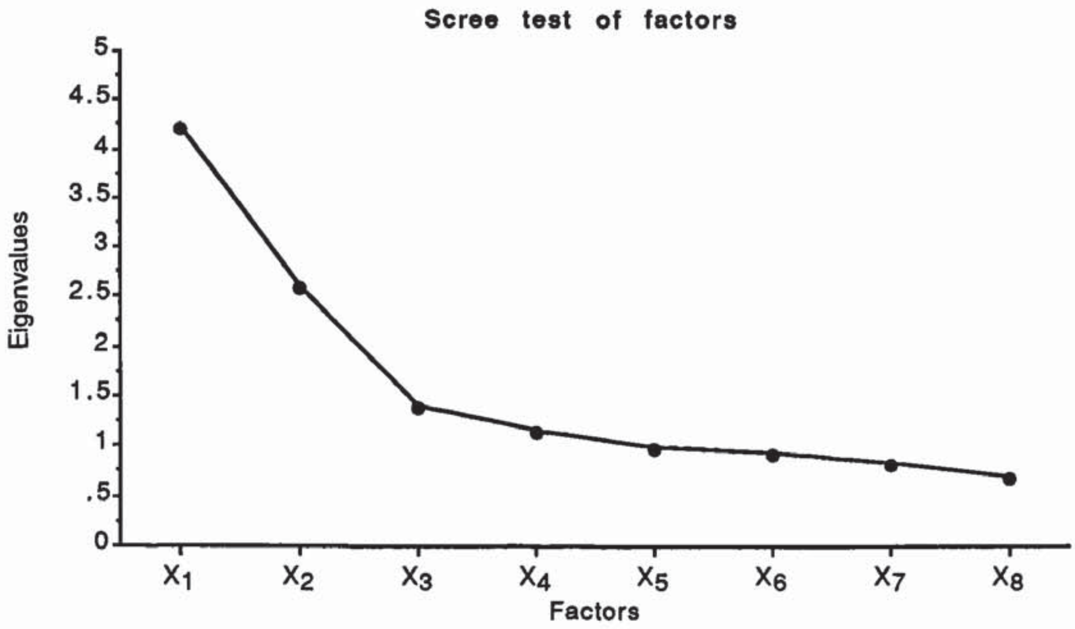


Figure 9.9 : Scree test to determine the number of factors to extract.

A scree test was chosen as a valid method of determining the number of components to retain (Zwick and Velicer, 1986). It would seem from the test that the number of factors to be extracted is two, possibly three. It was decided to extract the third factor for two reasons. First, the scree by itself could justify the extraction of a third factor. Second, when one examines the relevant loadings on that factor, it can be given a logical interpretation and one that appears to be identical with a factor extracted from the ADS - pedal control. It is therefore suggested that the present simulation is characterised by a three factor oblique solution along the following lines:

Factor analysis of Study 7	
Factor	loading
Factor 1 General risk taking	
accelerator pressure	.78
mean speed	.78
number of overtakes	.67
probability of risky overtake	.57
Factor 2 Driving errors	
probability of kerb collision	.82
probability of kerb collision in overtake	.80
erratic steering	.62
probability of collision with other vehicle	.60
Factor 3 Brake and Clutch use	
clutch use	.72
brake use	.64

Table 9.4 - Factor solution for study 7

This leaves the following specific driver behaviours that fell outside this factor solution:

1. Time gap required in overtaking (question 4)
2. Crossing the centre white line (question 5)
3 Attention (question 7)
4. Overtaking errors (question 8)
5. Safety distance from the car in front (question 10)
6. Aborted overtakes (question 14)

Of the above, it is difficult to understand how the 'time gap required in overtaking' measure should not be loaded on to the *general risk taking* factor, perhaps along with the 'attention' question. Certainly one would expect to see the 'safety distance from the car in front' item loaded on to the general risk taking factor, and overtaking errors should, one imagines, be loaded on the *driving errors* factor. It is therefore perhaps not surprising to learn that the proportion of original variance accounted for by the first three factors is just 51.3%.

9.3.4. Discussion

It would seem likely that a pencil and paper based simulation is not well suited for testing RHT predictions. It seems that the improvements made in this study had little effect on the suitability of this low fidelity simulation as a research tool for the investigation of RHT predictions. Perhaps the reason for this is that the prerequisite of an effective RHT simulation - that individuals be characterised by active accident loss avoidance, thus maintaining a target level of risk - is not particularly well met with such a low fidelity approach.

One possible criticism of this study arises from its use of the adapted and individually-referenced dimensions. For example, so far as mean speed was concerned, rather than enquire about a specific speed in mph, participants were instead asked:

13. How fast would you be likely to travel?

1 2 3 4 5 6 7 8 9 10

minimum value maximum value

the values attached to minimum and maximum were defined by the individuals through the instruction:

"The number one represents, *in your view*, the minimum *realistic* value that could be assigned to the behaviour in question. The number ten represents, *again in your view*, the maximum *realistic* numerical value that could be assigned to the behaviour in question.

example: if you were asked about what percentage of time you would be likely to take your eyes off the road for, you may think that 0.5% would be the realistic minimum and *that 2% would be the realistic maximum. If you were then faced with a response on the following scale:"*

This method of referencing was selected because the previous pencil and paper-based experimental work had led to the criticism that answers to the questions were impossible in absolute terms and that instead participants should be asked how they would change their driving behaviour relative to the constraints that might operate in the road traffic environment at the time in question. Unfortunately it suffers from a problem in that these constraints could be said to move when manipulations to environmental risk took place. Thus, the maximum and minimum values would both be higher where low environmental risk operated and lower where high environmental risk operated. The problem of what scale to place the behaviours on then is far from clear. It is however suggested that since this would seem to be an inappropriate methodology for the investigation of RHT hypotheses, the question does not, in any real sense, arise.

Whilst the factor structure of this simulator is not by any means identical with that of the ADS, it is perhaps reconcilable with it, and has, it must be said, a factor structure that is logically interpretable. The present simulator is, however, incapable of differentiating between young and old drivers and between male and female drivers in the way that the ADS does. Whilst RHT does not specify any sex and age differences in its operation, it was felt that if the present simulation is not capable of differentiating between the same driver groups, then it is probably not measuring the same thing. The comparisons of age and sex were therefore used to answer the validity question, rather than any RHT issues. Its practical benefit in the investigation of RHT must therefore be subject to a number of qualifications. One question that will be returned to is that of the extent to which participants on this simulator were characterised by a target level of risk - essential if RHT hypotheses are to be tested. Another question is the suitability of this simulator when it is capable only of showing constant accident loss across different conditions of environmental risk, but is at the same time, it seems, incapable of identifying the particular behavioural pathways that carry that effect.

Chapter 10 - Conclusions and recommendations for future research practice.

10.1. Conclusions following from the development of simulation as a research tool for the investigation of RHT predictions

This section is concerned with the evaluation of the suitability of the simulation of physical risk as methodology for investigating RHT hypotheses. Before setting out the conclusions from this thesis however, a number of axioms will first be given on this matter.

Axiom 1: In order to investigate RHT in a simulated physical risk-taking environment, individuals must be characterised by a target level of risk. Since Wilde's formulation of RHT (1988) suggests that this 'target' risk acts as a unique reference variable, its replication in an experimental context is essential and only research methodologies that first of all satisfy this requirement can be employed.

Axiom 2: Where the investigation of RHT involves the awarding of points, rational participants will, if the awarding of points is reinforcing, act in such a way as to maximise their points. The basis of this axiom is dealt with in chapter 1.

Axiom 3: Since RHT invokes the concept of subjective utility, simulation exercises involving only objective utility are inappropriate. In other words, RHT is about the exchange of one subjectively valued good for another. This is so even when one acknowledges that objective utility has subjective correlates, for with objective utility and its subjective correlates there is a 'right' and a 'wrong' course of action, and this does not in any way parallel the RHT model.

Axiom 4: Where physical risk is being simulated and individuals are characterised by active accident avoidance, the level of accident loss (or its errors correlate) associated with any given level of environmental risk is not relevant to the testing of RHT hypotheses, so long as different behavioural options are available to participants that are able to restore previously existing levels of accident loss (or its errors correlate). In other words, so long as a negation of an environmental risk change is possible through behavioural adjustments (either within, or outside, the simulated physical risk-taking environment), the absolute level of accident loss (or its errors correlate) need not be of concern to the experimenter.

A number of conclusions now follow, both from the axioms given above, and from the research discussed here. Most of these conclusions take the form of practical steps that should be taken into account at the design stage of any RHT simulation exercise.

<u>Conclusion 1: When designing RHT simulations there should be some *a priori* reason to suppose that individuals are characterised by a target level of risk.</u> Practically what this means, it is suggested, is that the Experimenter should first satisfy him/herself that individuals are engaged in active accident avoidance. Usually this is not a particularly difficult criterion to verify and observation of participants in a pilot run should be sufficient. All the evidence here would suggest that the ADS was successful on this criterion. Participants, whilst accepting a higher level of accident loss than they would in a 'real' situation, did at the same time try to avoid accidents. Unfortunately, there is very little evidence to show that the low fidelity approach of pencil-and-paper-based tests met this requirement, and so they are probably not suitable for the investigation of RHT hypotheses. So far as the NPPCR study was concerned, there is good evidence to suggest that participants sought to minimize accident loss through attempting to avoid errors.

Conclusion 2: A reduced level of attention to the simulated task, such as occurs through boredom, jeopardises the maintenance of any target level of risk. Participants are probably only characterised by a target level of risk, and thus only try to avoid accidents, so long as their attention, and therefore their interest, is maintained. A practical lesson to be learnt here is to make the environment as intrinsically interesting as possible. The ADS, in this context, is an example of success. The environment was held by those who took part in the studies, to be '*good fun*', and was, they said, an environment in which they were able to concentrate for long periods. The NPPCR is probably less successful. Participants here complained that the experimental procedure was boring. The danger in creating uninteresting experiments is that participants will 'turn off' to the environment and fail to pay any attention to the task. When this happens, RHT hypotheses are no longer being tested, for participants fail to behave as if they were subject to any physical risk. They fail, in other words, to be characterized by a target level of risk. The first attempt at the pencil-and-paper-based task was, in terms of its intrinsic interest, an unqualified failure. The study took a long time and requested many answers to the same questions, with the stimuli differing very little. The second attempt failed too in capturing the imagination of participants, although this was probably less of a problem with the reduced time scale.

Conclusion 3: If behavioural adjustments within the simulated environment are of direct interest to the Experimenter, accident loss (or errors) should be, to at least a limited extent, within the control of the participant. In the case of the NPPCR study, and even then, probably on only one condition, the task was, for many participants, effectively impossible. This left the participants with very low motivation and in some cases this resulted in no attentional resources being given to the simulated exercise. Just as in the case of boredom, this probably removed any target level of risk and left the simulation testing something other than the RHT hypotheses it set out to test.

Conclusion 4: The feedback given to participants should be dictated by the time scale of the simulation exercise. The justification for the simulation of physical risk as an alternative to the examination of accident data is that collapsed experience is made possible. It must be obvious that where the feedback, by its nature and/or quantity, can be expected to influence behaviour over a time scale of several days, then the failure of an experimental session lasting only fifteen minutes to bring about behavioural changes in no way refutes RHT. The very subtle change to intrinsic risk reported here as study 1, in conjunction with its limited time scale, could probably be said to be in danger of violating this criterion.

Conclusion 5: Just as in any experimental work, care should be exercised in reducing or eliminating the demand characteristics of the task. Two strategies present themselves following from lessons learned in the relatively high fidelity simulation exercises described here. Study 1, involving giving information to participants regarding the efficiency of the braking system, ran the risk of possible demand characteristics. To some extent this was not anticipated since main effects in the absence of an interaction was not regarded as a likely finding. Additionally, the informal debriefing of participants after the experimental sessions led to the conclusion that if demand characteristics were present, they did not present a significant problem. Study 3 (involving side of road) could also be criticized as having potential demand characteristics. To some extent this was inevitable if that particular research problem was to be addressed at all. Two experiments that appeared to escape from the demand characteristics problem were the NPPCR simulator (study 2) and the ADS study looking at temporal leeway (study 5). These two studies, it is suggested, were significant in that they each represented a strategy for overcoming the demand characteristics problem. In the case of the NPPCR simulator the design was one of independent participants. The structured interview studies would seem to indicate that where participants remain for the whole of the experimental session under one condition of environmental risk, they tend to look for other hypotheses to confirm! By contrast, the ADS studies all involved at least one within participants factor and in moving from one condition of environmental risk to another, the possibility that behaviour in response to environmental risk levels was manifestly of interest to the Experimenter cannot be ignored.

The second study (the ADS study involving different temporal leeways) involved no instructions regarding the level of environmental risk or the nature of the risk change. Allowing participants to discover this difference for themselves seemed to be successful in masking the true aim of the study from the particpants. Informal debriefing even indicated that in some cases participants were not fully aware that intrinsic risk (temporal leeway) had changed.

Conclusion 6: Manipulations of environmental safety should be relevant to the probability of an accident, rather than the costs of the accident, given that it has already occurred. This conclusion follows from the evidence from study 6.2 in which participants agreed that in some circumstances they were unconcerned with the consequences of an accident, but were always concerned with avoiding one. Unfortunately this would seem to indicate that many of the central questions of RHT such as the effectiveness of safety-belt legislation, crash helmet legislation, and so on, are not particularly well-suited to investigation in a simulated physical risk-taking environment and will therefore perhaps always suffer from the methodological problems associated with the pseudo-experimental design already discussed.

Conclusion 7: Where RHT is investigated entirely within the closed-loop of a simulated environment, the inferences drawn from the investigation should be restricted to that part of RHT concerned with behavioural adjustments within the environment. Since RHT offers two additional pathways to homeostasis - avoiding the environment altogether and changing the mode of transport (or, on a more general level, changing the form of physical risk-taking) - it would be incorrect to conclude from investigations that fail to provide evidence for behavioural adjustments within the environment sufficient to restore previously existing levels of actual safety, that the theory of risk homeostasis has in any way been refuted. This leaves two options. Either the research tool of simulation should be used only to investigate specific behaviours within a physical risk-taking environment, and not concern itself with either mode migration or avoidance, or that further effort should be directed to developing a methodology to investigate these external pathways. Whether indeed they lend themselves to investigation in a simulated environment at all is still unknown, and consequently the research findings relevant to these pathways, reported in study 2, should be interpreted with care.

Conclusion 8: There are at present technical reasons why the simulation of a driving exercise, brought within closed-loop control, lacks realism in overtaking exercises. This has resulted, in the case of the ADS, in it being possible for participants to score highly on the measures associated with overtaking errors even though, at the time of pulling out to overtake, there was little or no perceived risk. To a large extent this problem is inherent in any simulation of such a fast-moving environment. This apparent loss of control on the part of the participants may even have resulted in a reduction in attention and the abandonment of any target level of risk.

This fidelity problem is serious and can be dealt with in three ways. First, the simulation of the driving process can be put on hold until the technology has improved such that the problems discussed no longer apply. Perhaps the development of 'virtual reality' systems holds some promise here. Second, since the problem is one that presents itself only when high speeds are simulated, the simulation of the driving process can be restricted to the relatively low speeds on which the technical problems of the screen regeneration rate and pixel number do not manifest themselves. Unfortunately, in cases where speed compensation is of interest to the researcher, such a strategy may be problematic in as much as it creates a possible ceiling effect. Even where the top possible simulated speed is as high as 70 mph, (from study 1) there is evidence to suggest that ceiling effects exist. Third, the simulation of physical risk can take place, for the time being at least, using environments other than the road-traffic situation. Candidates here are the simulation of sea-traffic in shipping exercises and the simulation of air-traffic control. Although these too suffer from practical problems (see chapter 3), these may be a price worth paying so long as the driving process remains technically constrained.

10.2. Conclusions following from the experimental contribution

This section is concerned with the generation of conclusions based on evidence from the experimental work of this thesis. In arriving at these statements it is assumed that the first aim of this thesis has been met and that the simulated physical risk-taking environments described here were successful enough to allow generalisations to be drawn from them to the real physical risk-taking situations they sought to explain. Clearly this is problematic in as much as some of the experimental work described here would appear not to have been successful in these terms (work, that is, involving the low-fidelity, pencil-and-paper approach). Perhaps it is problematic too in that the case of any generalisation from simulated work has not yet been established. It is provided nevertheless in the form of a working hypothesis. If the attempts at simulating physical risk were unsuccessful, the hypotheses, when tested against statistics in real physical risk-taking environments will obviously not be supported. But that hardly represents a cogent argument not to test any hypotheses derived from this work, and indeed not to do so would to a large extent render it wasted. So far as the specific limitations of the low-fidelity experimental work go, these will be taken into account in formulating conclusions and propositions wherever possible. None of the following conclusions is derived from the low fidelity experimental work reported in this thesis.

Conclusion 1: Utility serves, at least partly, to determine risk-taking (and risk-avoiding) behaviour. For this, the experimental work reported here gave unequivocal support.

In all cases where utility was manipulated as a factor, behaviours measured showed significant changes across its different levels. This is shown in studies 1 and 2. The experimental work reported here looked at both behaviour changes within the environment and measures of avoidance. In all cases the utility manipulation led to a significant change in relevant behaviours, with a large strength of effect. From this a deduction can be drawn that has some bearing on the earlier challenge to opponents of RHT on their position on the utility issue.

To oppose RHT at a conceptual level it seems that one of two positions is necessary. Either one could say that risk-taking behaviour is not in any sense the product of a utility calculation, or one could say that utility is a determinant of risk-taking behaviour, but that in a dynamic of environmental risk, utility no longer completely explains behaviour. This is best clarified by an example, and it is a variation of the example given earlier. Suppose that a motorist drives through a suburban street at a speed of 40 mph. Why is this speed selected? First, why is it that the motorist is not travelling at 20 mph? If this more cautious alternative speed were adopted by this motorist and others, a dramatic reduction in accident loss could be expected, for drivers would have more time to react to dangers, thus reducing the number of accidents, and, given that an accident has occurred, the lower speeds will lead to severity reductions. Given such benefits then, why is the driver not travelling at 20mph? Equally, why is he or she not travelling at 75 mph? If time is of value to the driver, such an increase in speed can be expected to result in large time savings. If the driver derives any sensation-seeking benefits from high speed, these too could be expected to increase. Given these benefits then, why is the driver not travelling at 75 mph? The utility explanation for this is simple. The chosen speed reflects a trade off between safety (risk-taking benefits and costs), and other benefits, such as arriving at the chosen destination sooner. It could equally well have been applied to attention, number of overtakes, mean leeway of overtakes, and just about any other driving behaviour. That this factor of utility plays an important role in determining the level of risk taking is the very strong suggestion from this thesis (see studies 1 and 2). This being so, the job of the opponent of RHT *is, it would seem, to explain why utility* no longer offers an explanation of risk-taking behaviour in dynamic conditions of environmental risk.

Assuming the level of environmental risk has changed, the utility calculation will now be different. There will be greater benefits (or fewer benefits, depending on the direction of the change) arising from more risky behaviour, and so behaviour, if it is a product of a utility calculation, can be expected to change. The deduction from conclusion 1, then, would appear to be that to oppose RHT effectively, the starting point has to be a recognition of the utility-driven explanation of driving behaviour. The job of RHT opponents would therefore be to answer the second question: given that utility would appear to determine many driver behaviours (and, in a non-driving context, avoidance), why is it that this mechanism falls apart in a dynamic of environmental risk?

Conclusion 2: Changes in environmental risk are capable, in given circumstances to be discussed, of bringing about behaviour changes in those affected by them. These behaviour changes may, at least in the short term, more than compensate for the change in environmental risk. This finding alone would seem to indicate that many of the central claims of RHT have been supported. Study 5 supports this conclusion well. Interventions at the level of environmental safety may not bring with them reductions in accident loss.

Conclusion 3: Whilst utility and environmental risk are both capable of bringing about changes in the behaviour of those affected by them, they do not appear to produce any statistical interaction. Again, this was seen from studies 1 and 2.

A deduction from conclusion 1 and conclusion 3 would seem to be that whilst utility is logically sufficient in bringing about changes to risk-taking behaviour, it is not logically necessary.

Conclusion 4: Where a change in environmental risk takes place in which behavioural adjustments within the environment are not capable of restoring the previously existing *actual* level of risk, there is a marked reluctance on the part of those affected by the environmental risk changes to compensate for the change via the pathway of avoidance (leaving the environment to eliminate the risk).

This conclusion of course requires some qualification. The only evidence for it comes from study 2. Here participants were faced with such high levels of environmental risk that no behavioural adjustments within the environment could bring accident loss correlates down to the levels existing on other conditions. Yet when asked how likely they would be to leave the environment, the level of environmental risk had no effect on judgements. It may be that this was due, partly at least, to the relatively short time span of the experiment. Perhaps if participants could see more clearly the high accident levels they would be faced with if they remained in the environment, they would be more likely to choose to leave the environment in conditions of high environmental risk. Additionally, the finding takes no account of possible mode migration (the analogy to which, in this case, would be changing jobs). Perhaps participants responded to the avoidance question in the way they did to indicate only that they would not, as it were, walk out of the environment. It takes no account either of the fact that the high levels on the accident loss correlates associated with the conditions of high environmental risk would not be tolerated by either the employers of such workers, were they to exist, or society as a whole. In short, whether the participants said they would leave the enviornment or not is arguably of little importance or interest if the decision would not be theirs to make. If individuals or groups other than the agents themselves can bring about avoidance, then the behaviour of the agents is perhaps not the only area worth studying.

But perhaps in spite of all this, the reluctance of individuals who cannot compensate for risk changes within an environment to leave the environment altogether is of real interest, and perhaps the finding from study 2 is of genuine interest. Poor weather conditions, certainly at the margin, represent just such a case. When rain, snow, fog or ice reach given intensities, accidents may become unavoidable if those conditions are faced by road-users. In this way behavioural adjustments within the environment cannot restore previously existing levels of accident loss. In these situations, do individuals stay at home and avoid the conditions (compensation through avoidance) or do they face them anyway and accept higher levels of accident loss? The answer to this question has been the object of some debate (eg see McKenna, 1988; Wilde, 1989), and perhaps here this simulation approach has something useful to offer.

There are, however, and as already discussed, arguments for suggesting that this finding is not only compatible with RHT, it would in fact be predicted by it. If the environment does not allow behavioural adjustments sufficient to restore some previously existing level of accident loss, and the only way that this (previous) level may be maintained is through the pathway of avoidance, the costs of relatively cautious driving will now have changed. Behavioural adjustments within the environment tend to have low costs (costs such as paying more attention to the road, driving slower, etc.), yet the pathway of avoidance - not making a journey at all - will tend to have a large cost. From Wilde (1988) one could predict that such a change would serve to increase the target level of risk, and thus, increase accident loss. This is because the target level of risk is said to be determined by four utilities. So long as three of these remain unchanged and one - the costs of relatively cautious driving - increases, accident loss can be expected - indeed would be predicted - to increase.

Conclusion 5: Where a change in environmental risk takes place in which behavioural adjustments within the environment *are* capable of restoring the previously existing *actual* level of risk, there is no evidence here for the level/s of high environmental risk mapping on to higher levels of accident loss correlates than conditions of low or lower environmental risk. Indeed, there is some evidence to suggest that in the short term, there may be a compensation process that serves to reverse actual and environmental safety.

A deduction could now be drawn from conclusion 4 and conclusion 5. The challenge set out in the earlier review to specify when and where RH operates and when and where it fails to operate might tentatively be answered by saying that RHT is always capable of explaining behaviour changes where a change in the level of environmental risk occurs that can be completely negated within the environment. It cannot always explain behaviour in cases where one environment is intrinsically so much riskier than another that, assuming the population remains in that environment, a change in accident loss would be inevitable in spite of possible attempts to change behaviour.

10.3 - General Conclusions

The simulation of physical risk as a method of investigating RHT predictions would appear to be both desirable (for the reasons already set out) and possible (with the qualifications discussed above). It is therefore suggested that further attempts to simulate physical risk should be undertaken, and the methodology of simulation in connection with physical risk investigated further, in an attempt to develop a greater understanding of risk homeostasis.

All of the evidence reported here is compatible with risk homeostasis. Whilst the conceptual state of RHT is such that this conclusion was almost inevitable before the experimental work began, some of the findings provide very active support for the predictions of RHT, hitherto unreported. Of key importance here are the findings on the factor of utility as a main effect.

10.4. Possibilities for further research

10.4.1. Cognitive Compensation

This thesis set out to answer two questions - is RHT true, and can it be investigated through simulated environments? One approach that might be adopted in the future with a view to answering both of these questions is the employment of the simulated methodology to investigate the likely behavioural and cognitive effects of an engineering intervention aimed at improving environmental safety. If the findings from such activity can be generalised to the situation on which the simulation is modelled (and that is an empirical question), then the validity of the approach may be assessed. The example here can already be given. Hoyes (1990) carried out research in which behavioural and cognitive responses to a decision support system in a simulated air traffic control environment were investigated. Members of the human factors research unit at Aston University are now improving this simulator and intend to examine further the possiblity of a risk homeostasis effect in operators. They propose to validate their findings from observational studies in the air traffic control environment on which their simulation is modelled.

10.4.2. Towards Positive Confirmation - a further study in utility

It is also worth recalling an issue first raised in chapter 5 - RHT tends to be associated with confirming the null hypothesis. To a certain extent, evidence is provided here which supports RHT by the rejection of the null hypothesis. Studies 1 and 2 certainly provide strong evidence that the factor of utility is capable of producing a strong main effect, and study 5 provides strong evidence for, at least an initial, overcompensation in relation to a change in environmental risk. What is needed now is perhaps further work that could serve to provide more active support for RHT.

One possibility along these lines would be a testing of Wilde's description of the determination of the target level of risk. It will be recalled that Wilde suggests that the target level of risk is arrived at through the values of four utilities: the costs and benefits of relatively risky behaviour, and the costs and benefits of relatively safe behaviour. Again using the Aston Driving Simulator, these could be manipulated in order to reward differentially relatively risky and relatively cautious behaviour. Accident loss correlates across different levels of reinforcement (target levels) could then be examined. Such research could compare target risk manipulations with environmental risk improvements as strategies for reducing accident loss. An interesting possibility here would be that the utility manipulations are subject to ceiling effects. Were this to be the case, the benefits of relatively cautious behaviour might be increasing, whilst the accident loss figure remains constant. Such a study, then, would serve to provide active support for the part of RHT concerned with the setting of a target level of risk and would serve also perhaps to satisfy the conditions necessary for a falsification of the theory. An experiment looking at this is at present being undertaken.

10.4.3. Blocking the behavioural pathways that carry negation effects

Another possiblity for future research would be the blocking of the behavioural pathways than can be shown to carry the compensation effect. This possibility has already been discussed in chapter 3, but no empirical work concerning it has been carried out. Unfortunately, a difficulty with it at present is that arguably few pathways on any of the three simulators reported here have been shown reliably to carry the effect.

10.4.4. Direct manipulations to temporal leeway.

The strongest evidence for RHT from the empirical work reported in this thesis arguably comes from study 5 (the temporal leeway study). This work might be futher advanced in the future by making direct manipulations to the acceleration of the simulated vehicle. Such manipulations could enable investigators to be more confident about which behavioural pathways, within the risk-taking environment, carry the effect, and which are merely correlated with the intervention.

10.4.5. Validation of the notional avoidance measure of extrinsic compensation

In study 2 it was noted that without the pathways of avoidance and mode migration, there are severe limitations to the simulation of physical risk as a methodology for the investigation of RHT hypotheses. The only attempt to look at avoidance came with study 2. In fact, the measure of notional avoidance reported there has not been validated. What one can say in its favour is that when using it as a measure, it seems capable of responding to the manipulations in utility in much the same way as behavioural adjustments within the risk-taking environment. However, what one has to say against it is that it does not, from study 2 at least, seem capable of responding to manipulations in environmental risk in the same way as more direct, intrinsic measures. Clearly, if RHT is to be examined in simulated environments, some measure of extrinsic compensation such as avoidance is needed, especially when the level of environmental risk is high, and hence, behavioural compensation within the environment, is either difficult or impossible. This being so, further research sould be concerned with developing reliable and valid measures of such extrinsic compensation.

10.4.6. Further cognitive questions - validation of the pre-attentive model

This thesis has been concerned primarily with the behavioural effects of changes to environmental risk and utility. This leaves open several cognitive questions. One might for example ask whether it is possible for an individual to achieve a homeostatic operation behaviourally, without actually being aware of this. Wilde, in suggesting that the process is 'pre-attentive', leaves open just this possibility. Another cognitive question might be that of how the closed-loop operates with collapsed experience? What kind of information about risk, likely accident loss, and so on, does the individual process and what kind of information is filtered? Cognitive questions of this kind are currently being investigated, again, by members of the Human Factors Research Unit at Aston, with the aid of verbal protocol analysis (Hoyes and Baber, manuscript in progress). A further advantage that this approach brings is the possibility of comparing verbal protocols from 'real' environments with verbal protocols from simulated ones. Again, work is already underway, and early indications are that in terms of cognitive processes in decision making in driving, the Aston Driving Simulator and the road traffic environment are indeed similar.

10.4.7. Risk homeostasis in a non-transportational domain

Little distinction in this thesis has been made of transportational and non-transportational risk. The reason for this is that although the theory was originally proposed to explain road-traffic behaviour, it is by implication generalisable to other forms of physical risk, and this generalisation was made explicit by Wilde in 1986. Even so, perhaps this failure to distinguish between transportational and non-transportational risk could be considered a mistake, for several reasons. First, the concept of utility, so key to the risk homeostasis debate, transfers only with some difficulty from the transportational to the non-transportational domain. When driving a car or flying an aircraft, we tend to wish to arrive at our destinations as soon as possible. There is therefore a time-referenced utility attached to the negation of an intrinsic risk benefit. When faced with, say, a monitoring task, it is difficult to see just what utility is attached to negating a similar benefit to intrinsic risk. Most such tasks involve a shift of a fixed duration - time and speed would appear under these conditions to lose their utility-providing properties. Several questions need to be asked here. Do other benefits replace the time-benefit, such as, say, attention reductions? In the absence of clear time-utility, does the homeostatic process break down? And on the issue of closed-loop feedback too, there are marked differences to be taken into account. Whereas accident loss, near misses, and so on are, at a population level, relatively common events in road-traffic, in the non-transportational domain, can the same necessarily be said? Accident *rates* in the nuclear industry are relatively low. Now, if Wilde can tentatively suggest that the closed loop applied to road-traffic can take up to 2 years to bring accident loss to its pre-intervention level, and if it is some closed-loop process that determines accident loss, say, in the nuclear industry, with accident rates in nuclear power plants at just a small fraction of those of road-traffic, we would, at first sight at least, be justified in suggesting that the negation of an intrinsic risk benefit to the nuclear industry might take hundreds, if not thousands, of years to negate. Finally, whatever the theory might have to say on the matter, the empirical evidence relevant to RHT in the non-transportational domain at present does little to support very much in the way of homeostasis. Much of the empirical work of Hoyes (1990) suggests that, although individuals do decrease attention when an improvement is made to intrinsic risk in a monitoring task (ATC), their adjustments do not bring about a negation of the change to intrinsic risk. This finding would appear to underline that of an ealier study carried out by Whitfield, Ball, and Ord (1980) in which a computer assisted approach sequencing system was used in an air traffic control simulation, and was successful in reducing errors. Study 2 from this thesis would seem to provide, albeit limited, support for the same principle. Perhaps therefore a line of future research might be to examine operators' behaviour in response to changes in the intrinsic risks of non-transportational environments. Stager and Hameluck (1990) provide what looks at first sight like support for RHT in a real air traffic control environment: they analysed 301 operating irregularities made by air traffic control operators over a period of one year and found that no fewer errors occurred in conditions of low or moderate workload than in conditions of high workload. This would appear to indicate that extra attentional resources are brought in to cope with high workloads. Unfortunately the paper does not report the relative incidence of low, moderate and high workload, and so whether this is really what happens is open to question. Moreover, the researchers used subjective ratings of workload without providing objective measures such as number of aircraft, number of aircraft on same headings, and so on. Work is currently underway at Aston to bring this workload variation into the laboratory, controlling exposure times to each objective workload condition.

10.4.8. The role of feedback in RHT

Wilde's model of RHT is built on the concept of a utility-driven, population-level, closed-loop regulatory mechanism. Whilst this thesis has attempted to test some predictions of that part of the RHT formulation concerned with utility, the role that feedback enjoys has not been examined; rather, that feedback occurs in the way Wilde describes has been its assumption in testing other RHT predictions. This, of course, leaves more work to do. Studies in which feedback of accident loss and accident loss correlates are systematically manipulated, perhaps in interaction with the environmental risk might be a useful start here. In addition, quite how individuals change their behaviour in relation to qualitatively different forms of feedback could be examined by interview and content analysis. This would have the further advantage of aiding our understanding of whether RHT really is 'pre-attentive'. Beyond this are further questions. The transfer of feedback from one environment of physical risk, to behaviour changes in another, could, for example, be examined, as could be the distinction of personal and population-level feedback. The first question, of whether feedback affects accident loss correlates, and the last question, of whether this effect is population level and/or personal, would serve to interrogate the theory of risk homeostasis in a way that is difficult to envisage outside of the simulated environments endorsed in this thesis. Just as in the case of utility, the role of feedback in RHT at present seems only to have the status of an unwarranted assumption.

10.4.9. Engineering versus RHT : a laboratory-based comparison.

A good many of the empirical findings reported in this thesis serve to test RHT against the null hypothesis that behavioural adjustments of risk-takers do not negate changes to the level of intrinsic risk. Equally (perhaps, one might say, the other side of the coin), one could test the hypothesis that *engineering solutions aimed only at reducing intrinsic risk are successful in reducing actual risk* against the null hypothesis that the intrinsic benefits are negated. But perhaps a more illuminating line of study, and one that would represent a move away from the *all-or-nothing* approach, would be an experimental analysis in which changes in the level of actual risk are operationalised as one independent variable, and changes in the relevant utilities are manipulated as another. Such a study might begin to answer the question of which of the two strategies can be expected to have most effect of accident loss levels.

10.4.10. Simulator versus simulandum

The advantages of investigating RHT through simulated environments are so overwhelming that is is at times difficult to lose sight of their disadvantages. A fairly immediate implication for future research to come out of this thesis is that the findings reported here should, wherever possible, be verified in the real environments they set out to model. Certainly so far as those predictions of RHT which are confined to accident loss are concerned, there is no reason why investigation should not continue. The evidence from simulated environments should perhaps be considered as no more than a single piece in the jigsaw of RHT.

10.4.11. A Sociological level of explanation

Perhaps an alternative, although somewhat radical, approach to the conceptualisation of risk homeostasis would be at a sociological, rather than psychological level. What all sides of the RHT debate have emphasised is a model of risk-taking behaviour at the level of the individual risk-taker. But, particularly outside of the road-traffic domain, social and political mechanisms may be just as important. In Britain, much political debate has surrounded the operational safety levels of physical risk-taking environments when taken from public into private control. As long ago as 1980 Wiener conceptualised midair collisions as, in part, a consequence not of individuals' actions, but rather, as a consequence of political decisions. Perhaps political structures too have their target levels of risk?

Appendix 1 - Symbols from first low-fidelity study.

The symbols that were used for the low-fidelity simulation are reproduced below. After the symbols, the simulator text and the behavioural checklist are reproduced. The symbols appear in their final form of symbol plus text plus action.

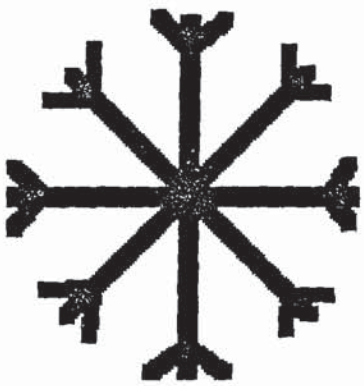
Brake pads worn

Service
Change pads soon

Anti lock brake failure

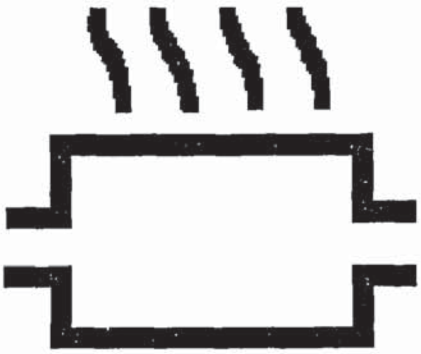
Caution
Drive to dealer

Ice warning



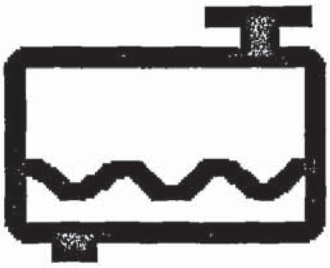
Status
No action needed

Catalyst temp. high



Stop immediately
Turn off engine

Coolant level high



Service
Fill up soon

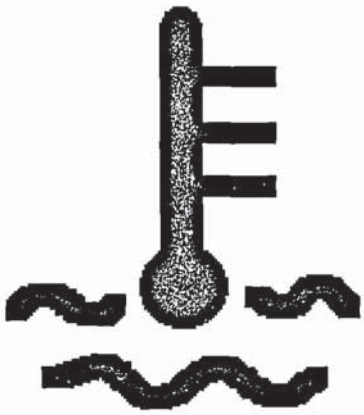
Fuse failure

Caution
Drive to dealer

Hazard light

Status
No action needed

Coolant temp. high

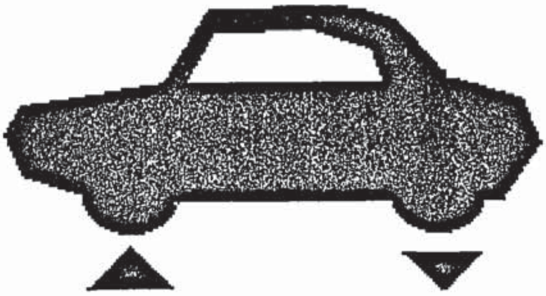


Stop immediately
Turn off engine

Screenwash level low

Service
Fill up soon

Suspension system fault

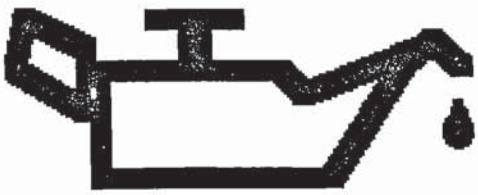


Caution
Drive to dealer

Cruise control on

Status
No action needed

Oil pressure low



Stop immediately
Turn off engine

Following the presentation of each symbol, the participant was asked:

If this symbol appeared on your dashboard display how would you change your driving behaviour?

1. Would you stop your car immediately? YES / NO (please circle as appropriate)
2. Would you shorten your journey? YES / NO (please circle as appropriate)

If your answer to 1 is NO then please circle the scales below.

If your answer to 1 is YES then please imagine that it is not possible to stop (for example you are in a tunnel or in a single file contra-flow system) and continue to circle the scales below.

The following behavioural checklist was also given to each participant after the presentation of each symbol:

-7	-6	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5	+6	+7
Drive Slower							No Change							Drive Faster

-7	-6	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5	+6	+7
Carry out fewer overtakes							No Change							Carry out more overtakes

-7	-6	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5	+6	+7
Carry out relatively less risky overtakes							No Change							Carry out relatively more risky overtakes

-7	-6	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5	+6	+7
Use brake pedal very much less							No Change							Use brake pedal very much more

-7	-6	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5	+6	+7
Concentrate less on your driving							No Change							Concentrate more on your driving

-7	-6	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5	+6	+7
Less likely to stop on seeing an amber light							No Change							More likely to stop on seeing an amber light

-7	-6	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5	+6	+7
Less likely to momentarily take your eye off the road							No Change							More likely to momentarily take your eye off the road

-7	-6	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5	+6	+7
Less likely to adhere to a mandatory speed restriction							No Change							More likely to adhere to a mandatory speed restriction

Appendix 2 - Statistical analyses from Study 7.

NB - In the analyses of variance that follow, the age group variable appears in the incidence tables under the variable name 'A'. The sex of participant appears in the incidence table under the variable name of 'B'. The level of environmental risk appears as 'C'.

Anova table for a 3-factor Analysis of Variance on Y_1: Question 1

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	.122	.122	.026	.8712
male/female (B)	1	.126	.126	.027	.8693
AB	1	2.2	2.2	.478	.4914
Experimental conditio...	1	4.72	4.72	1.024	.3143
AC	1	.12	.12	.026	.8724
BC	1	1.212	1.212	.263	.6094
ABC	1	3.333	3.333	.724	.3974
Error	85	391.585	4.607		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_1: Question 1

	male/female:	level 1	level 2	Totals:
age group	level 1	32 4.969	49 4.612	81 4.753
	level 2	9 4.333	3 4.667	12 4.417
Totals:		41 4.829	52 4.615	93 4.71

The AC Incidence table on Y_1: Question 1

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 4.447	43 5.023	81 4.753
	level 2	7 4.286	5 4.6	12 4.417
Totals:		45 4.422	48 4.979	93 4.71

The BC Incidence table on Y_1: Question 1

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 4.5	19 5.211	41 4.829
	level 2	23 4.348	29 4.828	52 4.615
Totals:		45 4.422	48 4.979	93 4.71

The ABC Incidence table on Y_1: Question 1

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 4.529	15 5.467	21 4.381	28 4.786	81 4.753
	level 2	5 4.4	4 4.25	2 4	1 6	12 4.417
Totals:		22 4.5	19 5.211	23 4.348	29 4.828	93 4.71

Anova table for a 3-factor Analysis of Variance on Y_2: Question 2

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	36.762	36.762	7.835	.0063
male/female (B)	1	1.17	1.17	.249	.6188
AB	1	1.483	1.483	.316	.5754
Experimental conditio...	1	.711	.711	.152	.698
AC	1	.428	.428	.091	.7634
BC	1	1.986	1.986	.423	.5171
ABC	1	1.386	1.386	.295	.5883
Error	85	398.829	4.692		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_2: Question 2

male/female:		level 1	level 2	Totals:
age group	level 1	32 6.875	49 6.041	81 6.37
	level 2	9 4.222	3 4	12 4.167
Totals:		41 6.293	52 5.923	93 6.086

The AC Incidence table on Y_2: Question 2

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 6.368	43 6.372	81 6.37
	level 2	7 4.143	5 4.2	12 4.167
Totals:		45 6.022	48 6.146	93 6.086

The BC Incidence table on Y_2: Question 2

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22	19	41
		6.318	6.263	6.293
	level 2	23	29	52
		5.739	6.069	5.923
Totals:		45	48	93
		6.022	6.146	6.086

The ABC Incidence table on Y_2: Question 2

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17	15	21	28	81
		6.882	6.867	5.952	6.107	6.37
	level 2	5	4	2	1	12
		4.4	4	3.5	5	4.167
Totals:		22	19	23	29	93
		6.318	6.263	5.739	6.069	6.086

Anova table for a 3-factor Analysis of Variance on Y_3: Question 3

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	6.515	6.515	2.826	.0964
male/female (B)	1	.009	.009	.004	.9515
AB	1	.1	.1	.043	.8357
Experimental conditio...	1	.987	.987	.428	.5146
AC	1	.602	.602	.261	.6106
BC	1	.001	.001	3.246E-4	.9857
ABC	1	.19	.19	.082	.7748
Error	85	195.954	2.305		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_3: Question 3

	male/female:	level 1	level 2	Totals:
age group	level 1	32 2.219	49 2.286	81 2.259
age group	level 2	9 1.444	3 1.333	12 1.417
	Totals:	41 2.049	52 2.231	93 2.151

The AC Incidence table on Y_3: Question 3

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 2.316	43 2.209	81 2.259
	level 2	7 1.714	5 1	12 1.417
Totals:		45 2.222	48 2.083	93 2.151

The BC Incidence table on Y_3: Question 3

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 2.091	19 2	41 2.049
	level 2	23 2.348	29 2.138	52 2.231
Totals:		45 2.222	48 2.083	93 2.151

The ABC Incidence table on Y_3: Question 3

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 2.176	15 2.267	21 2.429	28 2.179	81 2.259
	level 2	5 1.8	4 1	2 1.5	1 1	12 1.417
Totals:		22 2.091	19 2	23 2.348	29 2.138	93 2.151

Anova table for a 3-factor Analysis of Variance on Y_4: Question 4

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	1.094	1.094	.237	.6274
male/female (B)	1	.104	.104	.023	.8807
AB	1	1.378	1.378	.299	.5859
Experimental conditio...	1	20.404	20.404	4.426	.0383
AC	1	14.336	14.336	3.11	.0814
BC	1	6.087	6.087	1.32	.2537
ABC	1	2.191	2.191	.475	.4924
Error	85	391.832	4.61		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_4: Question 4

male/female:		level 1	level 2	Totals:
age group	level 1	32 6.75	49 7.02	81 6.914
	level 2	9 6.889	3 7	12 6.917
Totals:		41 6.78	52 7.019	93 6.914

The AC Incidence table on Y_4: Question 4

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 7.079	43 6.767	81 6.914
	level 2	7 7.857	5 5.6	12 6.917
Totals:		45 7.2	48 6.646	93 6.914

The BC Incidence table on Y_4: Question 4

	Experimental ...	High Risk	Low Risk	Totals:
male/fem...	level 1	22 6.909	19 6.632	41 6.78
	level 2	23 7.478	29 6.655	52 7.019
Totals:		45 7.2	48 6.646	93 6.914

The ABC Incidence table on Y_4: Question 4

	male/female:	level 1		level 2		Totals:
	Experimental ...	High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 6.706	15 6.8	21 7.381	28 6.75	81 6.914
	level 2	5 7.6	4 6	2 8.5	1 4	12 6.917
Totals:		22 6.909	19 6.632	23 7.478	29 6.655	93 6.914

Anova table for a 3-factor Analysis of Variance on Y_5: Question 5

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	1.278	1.278	.268	.606
male/female (B)	1	6.793	6.793	1.425	.236
AB	1	1.002	1.002	.21	.6479
Experimental conditio...	1	9.345	9.345	1.96	.1652
AC	1	6.719	6.719	1.409	.2385
BC	1	.006	.006	.001	.9726
ABC	1	1.518	1.518	.318	.574
Error	85	405.329	4.769		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_5: Question 5

	male/female:	level 1	level 2	Totals:
age group	level 1	32 3.188	49 3.776	81 3.543
	level 2	9 2.333	3 3.333	12 2.583
Totals:		41 3	52 3.75	93 3.419

The AC Incidence table on Y_5: Question 5

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38	43	81
		3.474	3.605	3.543
	level 2	7	5	12
		1.857	3.6	2.583
Totals:		45	48	93
		3.222	3.604	3.419

The BC Incidence table on Y_5: Question 5

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22	19	41
		2.591	3.474	3
	level 2	23	29	52
		3.826	3.69	3.75
Totals:		45	48	93
		3.222	3.604	3.419

The ABC Incidence table on Y_5: Question 5

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 2.882	15 3.533	21 3.952	28 3.643	81 3.543
age group	level 2	5 1.6	4 3.25	2 2.5	1 5	12 2.583
Totals:		22 2.591	19 3.474	23 3.826	29 3.69	93 3.419

Anova table for a 3-factor Analysis of Variance on Y_6: Question 6

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	2.624	2.624	.812	.37
male/female (B)	1	3.332	3.332	1.031	.3127
AB	1	3.175	3.175	.983	.3243
Experimental conditio...	1	1.73	1.73	.535	.4664
AC	1	1.447	1.447	.448	.5051
BC	1	8.972	8.972	2.777	.0993
ABC	1	1.67	1.67	.517	.4741
Error	85	274.615	3.231		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_6: Question 6

male/female:		level 1	level 2	Totals:
age group	level 1	32 5.156	49 5.143	81 5.148
	level 2	9 3.889	3 5.667	12 4.333
Totals:		41 4.878	52 5.173	93 5.043

The AC Incidence table on Y_6: Question 6

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 5.237	43 5.07	81 5.148
	level 2	7 4.429	5 4.2	12 4.333
Totals:		45 5.111	48 4.979	93 5.043

The BC Incidence table on Y_6: Question 6

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 4.591	19 5.211	41 4.878
	level 2	23 5.609	29 4.828	52 5.173
Totals:		45 5.111	48 4.979	93 5.043

The ABC Incidence table on Y_6: Question 6

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 4.882	15 5.467	21 5.524	28 4.857	81 5.148
	level 2	5 3.6	4 4.25	2 6.5	1 4	12 4.333
Totals:		22 4.591	19 5.211	23 5.609	29 4.828	93 5.043

Anova table for a 3-factor Analysis of Variance on Y_7: Question 7

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	7.338	7.338	1.735	.1913
male/female (B)	1	3.997	3.997	.945	.3337
AB	1	.203	.203	.048	.8269
Experimental conditio...	1	.009	.009	.002	.9638
AC	1	.134	.134	.032	.8593
BC	1	.282	.282	.067	.7968
ABC	1	.645	.645	.153	.6971
Error	85	359.448	4.229		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_7: Question 7

	male/female:	level 1	level 2	Totals:
age group	level 1	32 3.75	49 3.184	81 3.407
age group	level 2	9 2.889	3 2	12 2.667
Totals:		41 3.561	52 3.115	93 3.312

The AC Incidence table on Y_7: Question 7

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 3.474	43 3.349	81 3.407
	level 2	7 2.571	5 2.8	12 2.667
Totals:		45 3.333	48 3.292	93 3.312

The BC Incidence table on Y_7: Question 7

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 3.773	19 3.316	41 3.561
	level 2	23 2.913	29 3.276	52 3.115
Totals:		45 3.333	48 3.292	93 3.312

The ABC Incidence table on Y_7: Question 7

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17	15	21	28	81
		4.059	3.4	3	3.321	3.407
	level 2	5	4	2	1	12
		2.8	3	2	2	2.667
Totals:		22	19	23	29	93
		3.773	3.316	2.913	3.276	3.312

Anova table for a 3-factor Analysis of Variance on Y_8: Question 8

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	12.708	12.708	4.784	.0315
male/female (B)	1	.172	.172	.065	.7998
AB	1	.303	.303	.114	.7362
Experimental conditio...	1	.007	.007	.003	.959
AC	1	2.501	2.501	.942	.3346
BC	1	.585	.585	.22	.6401
ABC	1	.811	.811	.305	.5819
Error	85	225.771	2.656		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_8: Question 8

male/female:		level 1	level 2	Totals:
age group	level 1	32 3.25	49 2.857	81 3.012
	level 2	9 1.667	3 1.667	12 1.667
Totals:		41 2.902	52 2.788	93 2.839

The AC Incidence table on Y_8: Question 8

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 3.289	43 2.767	81 3.012
	level 2	7 1.429	5 2	12 1.667
Totals:		45 3	48 2.688	93 2.839

The BC Incidence table on Y_8: Question 8

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 3.273	19 2.474	41 2.902
	level 2	23 2.739	29 2.828	52 2.788
Totals:		45 3	48 2.688	93 2.839

The ABC Incidence table on Y_8: Question 8

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 3.824	15 2.6	21 2.857	28 2.857	81 3.012
	level 2	5 1.4	4 2	2 1.5	1 2	12 1.667
Totals:		22 3.273	19 2.474	23 2.739	29 2.828	93 2.839

Anova table for a 3-factor Analysis of Variance on Y_9: Question 9

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	8.953	8.953	2.079	.153
male/female (B)	1	6.28	6.28	1.458	.2305
AB	1	.31	.31	.072	.7891
Experimental conditio...	1	4.414	4.414	1.025	.3142
AC	1	.079	.079	.018	.8925
BC	1	.328	.328	.076	.7832
ABC	1	.054	.054	.013	.911
Error	85	366.006	4.306		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_9: Question 9

	male/female:	level 1	level 2	Totals:
age group	level 1	32 3.531	49 2.735	81 3.049
	level 2	9 2.667	3 1.667	12 2.417
Totals:		41 3.341	52 2.673	93 2.968

The AC Incidence table on Y_9: Question 9

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 3.474	43 2.674	81 3.049
	level 2	7 2.714	5 2	12 2.417
Totals:		45 3.356	48 2.604	93 2.968

The BC Incidence table on Y_9: Question 9

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 3.545	19 3.105	41 3.341
	level 2	23 3.174	29 2.276	52 2.673
Totals:		45 3.356	48 2.604	93 2.968

The ABC Incidence table on Y_9: Question 9

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 3.706	15 3.333	21 3.286	28 2.321	81 3.049
	level 2	5 3	4 2.25	2 2	1 1	12 2.417
Totals:		22 3.545	19 3.105	23 3.174	29 2.276	93 2.968

Anova table for a 3-factor Analysis of Variance on Y_{10}: Question 10

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	.21	.21	.05	.8229
male/female (B)	1	.98	.98	.235	.6294
AB	1	4.879	4.879	1.168	.2829
Experimental conditio...	1	2.652	2.652	.635	.4279
AC	1	5.067	5.067	1.213	.2739
BC	1	8.089	8.089	1.936	.1677
ABC	1	7.867	7.867	1.883	.1736
Error	85	355.14	4.178		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_{10}: Question 10

male/female:		level 1	level 2	Totals:
age group	level 1	32 5.938	49 6.408	81 6.222
	level 2	9 6.889	3 6.333	12 6.75
Totals:		41 6.146	52 6.404	93 6.29

The AC Incidence table on Y_{10}: Question 10

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 6.079	43 6.349	81 6.222
	level 2	7 6.857	5 6.6	12 6.75
Totals:		45 6.2	48 6.375	93 6.29

The BC Incidence table on Y_{10}: Question 10

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 6	19 6.316	41 6.146
	level 2	23 6.391	29 6.414	52 6.404
Totals:		45 6.2	48 6.375	93 6.29

The ABC Incidence table on Y_{10}: Question 10

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 5.824	15 6.067	21 6.286	28 6.5	81 6.222
	level 2	5 6.6	4 7.25	2 7.5	1 4	12 6.75
Totals:		22 6	19 6.316	23 6.391	29 6.414	93 6.29

Anova table for a 3-factor Analysis of Variance on Y_{11}: Question 11

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	.849	.849	.63	.4295
male/female (B)	1	.634	.634	.47	.4947
AB	1	1.552	1.552	1.151	.2863
Experimental conditio...	1	1.856	1.856	1.377	.2439
AC	1	.462	.462	.343	.5599
BC	1	.419	.419	.311	.5787
ABC	1	1.945	1.945	1.443	.2329
Error	85	114.555	1.348		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_{11}: Question 11

	male/female:	level 1	level 2	Totals:
age group	level 1	32	49	81
		1.812	1.633	1.704
	level 2	9	3	12
		1	2	1.25
Totals:		41	52	93
		1.634	1.654	1.645

The AC Incidence table on Y_{11}: Question 11

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 1.816	43 1.605	81 1.704
	level 2	7 1.429	5 1	12 1.25
Totals:		45 1.756	48 1.542	93 1.645

The BC Incidence table on Y_{11}: Question 11

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 1.818	19 1.421	41 1.634
	level 2	23 1.696	29 1.621	52 1.654
Totals:		45 1.756	48 1.542	93 1.645

The ABC Incidence table on Y_{11}: Question 11

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 2.059	15 1.533	21 1.619	28 1.643	81 1.704
age group	level 2	5 1	4 1	2 2.5	1 1	12 1.25
Totals:		22 1.818	19 1.421	23 1.696	29 1.621	93 1.645

Anova table for a 3-factor Analysis of Variance on Y_{12}: Question 12

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	.089	.089	.016	.9006
male/female (B)	1	1.344	1.344	.238	.6272
AB	1	.776	.776	.137	.712
Experimental conditio...	1	.454	.454	.08	.7777
AC	1	1.832	1.832	.324	.5707
BC	1	3.431	3.431	.607	.4382
ABC	1	.035	.035	.006	.9372
Error	84	474.948	5.654		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_{12}: Question 12

male/female:		level 1	level 2	Totals:
age group	level 1	31 4.935	49 4.939	80 4.938
	level 2	9 4.444	3 5.333	12 4.667
Totals:		40 4.825	52 4.962	92 4.902

The AC Incidence table on Y_{12}: Question 12

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	37 5.405	43 4.535	80 4.938
	level 2	7 4.429	5 5	12 4.667
Totals:		44 5.25	48 4.583	92 4.902

The BC Incidence table on Y_{12}: Question 12

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	21	19	40
		4.762	4.895	4.825
	level 2	23	29	52
		5.696	4.379	4.962
Totals:		44	48	92
		5.25	4.583	4.902

The ABC Incidence table on Y_{12}: Question 12

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	16	15	21	28	80
		5	4.867	5.714	4.357	4.938
	level 2	5	4	2	1	12
		4	5	5.5	5	4.667
Totals:		21	19	23	29	92
		4.762	4.895	5.696	4.379	4.902

Anova table for a 3-factor Analysis of Variance on Y_{13}: Question 13

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	7.302	7.302	1.977	.1633
male/female (B)	1	2.655	2.655	.719	.3989
AB	1	.469	.469	.127	.7226
Experimental conditio...	1	1.080E-4	1.080E-4	2.923E-5	.9957
AC	1	.928	.928	.251	.6175
BC	1	.156	.156	.042	.8378
ABC	1	3.684	3.684	.997	.3208
Error	85	313.953	3.694		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_{13}: Question 13

	male/female:	level 1	level 2	Totals:
age group	level 1	32 6.312	49 5.98	81 6.111
age group	level 2	9 5.667	3 4.667	12 5.417
	Totals:	41 6.171	52 5.904	93 6.022

The AC Incidence table on Y_{13}: Question 13

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38	43	81
		6	6.209	6.111
	level 2	7	5	12
		5.714	5	5.417
Totals:		45	48	93
		5.956	6.083	6.022

The BC Incidence table on Y_{13}: Question 13

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22	19	41
		5.955	6.421	6.171
	level 2	23	29	52
		5.957	5.862	5.904
Totals:		45	48	93
		5.956	6.083	6.022

The ABC Incidence table on Y_{13}: Question 13

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 5.882	15 6.8	21 6.095	28 5.893	81 6.111
	level 2	5 6.2	4 5	2 4.5	1 5	12 5.417
Totals:		22 5.955	19 6.421	23 5.957	29 5.862	93 6.022

Anova table for a 3-factor Analysis of Variance on Y_{14}: Question 14

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	34.788	34.788	6.282	.0141
male/female (B)	1	.146	.146	.026	.8714
AB	1	.711	.711	.128	.721
Experimental conditio...	1	1.782	1.782	.322	.572
AC	1	1.566	1.566	.283	.5963
BC	1	.666	.666	.12	.7295
ABC	1	.167	.167	.03	.8625
Error	85	470.697	5.538		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_{14}: Question 14

	male/female:	level 1	level 2	Totals:
age group	level 1	32 3.562	49 3.735	81 3.667
	level 2	9 1.778	3 1.333	12 1.667
Totals:		41 3.171	52 3.596	93 3.409

The AC Incidence table on Y_{14}: Question 14

	Experimental ...	High Risk	Low Risk	Totals:
age group	level 1	38 3.658	43 3.674	81 3.667
	level 2	7 2.143	5 1	12 1.667
Totals:		45 3.422	48 3.396	93 3.409

The BC Incidence table on Y_{14}: Question 14

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 3.364	19 2.947	41 3.171
	level 2	23 3.478	29 3.69	52 3.596
Totals:		45 3.422	48 3.396	93 3.409

The ABC Incidence table on Y_{14}: Question 14

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 3.647	15 3.467	21 3.667	28 3.786	81 3.667
	level 2	5 2.4	4 1	2 1.5	1 1	12 1.667
Totals:		22 3.364	19 2.947	23 3.478	29 3.69	93 3.409

Anova table for a 3-factor Analysis of Variance on Y_{15}: Question 15

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	.157	.157	.177	.6748
male/female (B)	1	.726	.726	.822	.3672
AB	1	1.415	1.415	1.603	.209
Experimental conditio...	1	1.845	1.845	2.089	.1521
AC	1	.467	.467	.529	.469
BC	1	.743	.743	.841	.3617
ABC	1	1.392	1.392	1.576	.2127
Error	85	75.075	.883		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_{15}: Question 15

	male/female:	level 1	level 2	Totals:
age group	level 1	32 1.594	49 1.449	81 1.506
age group	level 2	9 1	3 2	12 1.25
	Totals:	41 1.463	52 1.481	93 1.473

The AC Incidence table on Y_{15}: Question 15

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38 1.632	43 1.395	81 1.506
	level 2	7 1.429	5 1	12 1.25
Totals:		45 1.6	48 1.354	93 1.473

The BC Incidence table on Y_{15}: Question 15

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22 1.591	19 1.316	41 1.463
	level 2	23 1.609	29 1.379	52 1.481
Totals:		45 1.6	48 1.354	93 1.473

The ABC Incidence table on Y_{15}: Question 15

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 1.765	15 1.4	21 1.524	28 1.393	81 1.506
	level 2	5 1	4 1	2 2.5	1 1	12 1.25
Totals:		22 1.591	19 1.316	23 1.609	29 1.379	93 1.473

Anova table for a 3-factor Analysis of Variance on Y_{16}: Question 16

Source:	df:	Sum of Squares:	Mean Square:	F-test:	P value:
age group (A)	1	7.228	7.228	2.807	.0975
male/female (B)	1	.638	.638	.248	.6199
AB	1	.084	.084	.033	.8569
Experimental conditio...	1	1.66	1.66	.645	.4242
AC	1	.119	.119	.046	.8302
BC	1	.003	.003	.001	.9718
ABC	1	1.313	1.313	.51	.4771
Error	85	218.841	2.575		

There were no missing cells found. 4 cases deleted with missing values.

The AB Incidence table on Y_{16}: Question 16

male/female:		level 1	level 2	Totals:
age group	level 1	32	49	81
		2.219	2.388	2.321
	level 2	9	3	12
		1.111	1.667	1.25
Totals:		41	52	93
		1.976	2.346	2.183

The AC Incidence table on Y_{16}: Question 16

Experimental ...		High Risk	Low Risk	Totals:
age group	level 1	38	43	81
		2.447	2.209	2.321
	level 2	7	5	12
		1.429	1	1.25
Totals:		45	48	93
		2.289	2.083	2.183

The BC Incidence table on Y_{16}: Question 16

Experimental ...		High Risk	Low Risk	Totals:
male/fem...	level 1	22	19	41
		2.273	1.632	1.976
	level 2	23	29	52
		2.304	2.379	2.346
Totals:		45	48	93
		2.289	2.083	2.183

The ABC Incidence table on Y_{16}: Question 16

male/female:		level 1		level 2		Totals:
Experimental ...		High Risk	Low Risk	High Risk	Low Risk	
age group	level 1	17 2.588	15 1.8	21 2.333	28 2.429	81 2.321
	level 2	5 1.2	4 1	2 2	1 1	12 1.25
Totals:		22 2.273	19 1.632	23 2.304	29 2.379	93 2.183

Factor Analysis of behavioural pathways follows:

Factor Analysis for Specific driver behaviour: $X_1 \ldots X_{16}$

Summary Information

Factor Procedure	Principal Component Analysis
Extraction Rule	Method Default
Transformation Method	Orthotran/Varimax
Number of Factors	4

Note: 1 case deleted with missing values.

Correlation matrix

	Question 1	Question 2	Question 3	Question 4	Question 5	Question 6	Question 7	Question 8
Question 1	1							
Question 2	.482	1						
Question 3	-.17	.133	1					
Question 4	-.343	-.135	.015	1				
Question 5	.166	.175	.188	-.135	1			
Question 6	-.206	-.091	.08	.241	.08	1		
Question 7	.328	.344	.212	-.256	.164	.079	1	
Question 8	.059	.325	.344	-.061	.44	.222	.428	1
Question 9	.538	.38	.102	-.211	.156	-.234	.404	.193
Question ...	-.307	-.262	-.046	.416	-.277	.175	-.412	-.259
Question ...	.021	.101	.416	-.006	.145	.247	.291	.302
Question ...	.055	.157	.035	-.129	.15	.268	.164	.189
Question ...	.583	.686	.078	-.22	.216	-.133	.348	.279
Question ...	.254	.299	.291	-.036	.371	.1	.247	.269
Question ...	-.018	-.047	.315	.018	.134	.087	.141	.197
Question ...	-.01	.18	.365	.011	.267	.176	.364	.416

Correlation matrix

	Question 9	Question...	Question...	Question...	Question...	Question...	Question...	Question...
Question 9	1							
Question ...	-.33	1						
Question ...	.056	-.119	1					
Question ...	.176	-.056	-.054	1				
Question ...	.438	-.294	.086	.133	1			
Question ...	.278	-.126	.238	.164	.261	1		
Question ...	.106	-.131	.634	.004	-.109	.237	1	
Question ...	.106	-.341	.493	.119	.155	.307	.436	1

Partials in off-diagonals and Squared Multiple R in diagonal

	Question 1	Question 2	Question 3	Question 4	Question 5	Question 6	Question 7	Question 8
Question 1	.582							
Question 2	.166	.53						
Question 3	-.33	.075	.357					
Question 4	-.212	.104	-.07	.315				
Question 5	.094	-.099	.041	-.045	.32			
Question 6	.012	-.08	-.089	.216	.022	.352		
Question 7	.121	.028	.056	-.122	-.158	.117	.433	
Question 8	-.155	.168	.123	.007	.347	.162	.243	.446
Question 9	.329	-.015	.126	.116	-.038	-.226	.207	.046
Question ...	-.004	-.052	.113	.32	-.165	.107	-.189	-.026
Question ...	.04	.019	.211	-.08	-.029	.26	.131	.01
Question ...	-.116	.091	-.038	-.211	.046	.348	.027	.018
Question ...	.317	.465	.052	-.008	.045	-.072	-.01	.076
Question ...	.146	.112	.179	.043	.264	.08	.028	-.055
Question ...	.079	-.074	.038	.047	-.005	-.131	-.111	.035
Question ...	-.113	.011	.086	.153	.039	.022	.139	.105

Partials in off-diagonals and Squared Multiple R in diagonal

	Question 1	Question 2	Question 3	Question 4	Question 5	Question 6	Question 7	Question 8
Question 1	.582							
Question 2	.166	.53						
Question 3	-.33	.075	.357					
Question 4	-.212	.104	-.07	.315				
Question 5	.094	-.099	.041	-.045	.32			
Question 6	.012	-.08	-.089	.216	.022	.352		
Question 7	.121	.028	.056	-.122	-.158	.117	.433	
Question 8	-.155	.168	.123	.007	.347	.162	.243	.446
Question 9	.329	-.015	.126	.116	-.038	-.226	.207	.046
Question ...	-.004	-.052	.113	.32	-.165	.107	-.189	-.026
Question ...	.04	.019	.211	-.08	-.029	.26	.131	.01
Question ...	-.116	.091	-.038	-.211	.046	.348	.027	.018
Question ...	.317	.465	.052	-.008	.045	-.072	-.01	.076
Question ...	.146	.112	.179	.043	.264	.08	.028	-.055
Question ...	.079	-.074	.038	.047	-.005	-.131	-.111	.035
Question ...	-.113	.011	.086	.153	.039	.022	.139	.105

Measures of Variable Sampling Adequacy

Total matrix sampling adequacy: .748

Question 1	.733
Question 2	.804
Question 3	.742
Question 4	.64
Question 5	.735
Question 6	.547
Question 7	.836
Question 8	.808
Question 9	.787
Question 10	.769
Question 11	.682
Question 12	.499
Question 13	.786
Question 14	.827
Question 15	.651
Question 16	.83

Bartlett Test of Sphericity- DF: 135 Chi Square: 528.599 P: .0001

Eigenvalues and Proportion of Original Variance

	Magnitude	Variance Prop.
Value 1	4.21	.263
Value 2	2.595	.162
Value 3	1.403	.088
Value 4	1.155	.072
Value 5	.975	.061
Value 6	.924	.058
Value 7	.81	.051
Value 8	.695	.043

Eigenvectors

	Vector 1	Vector 2	Vector 3	Vector 4	Vector 5	Vector 6	Vector 7	Vector 8
Question 1	-.262	-.367	.055	-.165	.158	.226	-.305	.101
Question 2	-.31	-.21	-.187	-.327	.026	-.248	.007	.172
Question 3	-.192	.311	.069	-.175	-.218	-.13	.655	.209
Question 4	.166	.241	-.249	-.502	-.016	-.111	-.157	-.608
Question 5	-.245	.077	-.174	.216	-.634	.202	-.281	.008
Question 6	-.009	.329	-.478	.109	.332	-.02	-.326	.168
Question 7	-.332	-.011	.024	.154	.318	-.26	.068	-.191
Question 8	-.304	.181	-.221	.132	-.196	-.337	6.712E-6	-.083
Question 9	-.29	-.234	.079	-.13	.145	.258	.255	-.46
Question ...	.279	.123	-.275	-.427	.068	.16	.144	.239
Question ...	-.225	.373	.262	-.159	.244	.01	-.172	.314
Question ...	-.13	.005	-.498	.345	.312	.343	.355	-.021
Question ...	-.315	-.276	-.143	-.273	.014	-.202	-.071	.215
Question ...	-.272	.095	-.154	-.234	-.246	.486	-.003	.003
Question ...	-.168	.36	.37	-.069	.175	.355	-.086	-.02
Question ...	-.278	.309	.094	.074	.037	-.152	-.101	-.246

Unrotated Factor Matrix

	Factor 1	Factor 2	Factor 3	Factor 4
Question 1	.537	-.592	-.065	.177
Question 2	.636	-.338	.222	.352
Question 3	.393	.502	-.081	.188
Question 4	-.34	.388	.294	.54
Question 5	.502	.125	.206	-.233
Question 6	.019	.53	.566	-.118
Question 7	.682	-.018	-.028	-.166
Question 8	.623	.292	.261	-.142
Question 9	.595	-.377	-.093	.14
Question 10	-.572	.198	.326	.459
Question 11	.462	.6	-.31	.171
Question 12	.267	.008	.589	-.371
Question 13	.646	-.444	.17	.293
Question 14	.559	.152	.182	.252
Question 15	.345	.58	-.438	.074
Question 16	.571	.498	-.112	-.08

Communality Summary

	SMC	Final Estimate
Question 1	.582	.674
Question 2	.53	.692
Question 3	.357	.448
Question 4	.315	.644
Question 5	.32	.364
Question 6	.352	.615
Question 7	.433	.493
Question 8	.446	.562
Question 9	.45	.524
Question 10	.413	.683
Question 11	.568	.699
Question 12	.233	.556
Question 13	.595	.729
Question 14	.317	.432
Question 15	.508	.653
Question 16	.461	.593

Orthogonal Transformation Solution-Varimax

	Factor 1	Factor 2	Factor 3	Factor 4
Question 1	.749	-.103	-.1	-.304
Question 2	.815	.063	.154	-.001
Question 3	.085	.643	.128	.105
Question 4	-.117	.106	-.016	.787
Question 5	.189	.229	.471	-.231
Question 6	-.235	.166	.645	.34
Question 7	.382	.318	.283	-.408
Question 8	.237	.413	.564	-.134
Question 9	.653	.09	-.028	-.297
Question 10	-.21	-.183	-.089	.773
Question 11	.036	.835	.006	-.009
Question 12	.081	-.152	.719	-.099
Question 13	.84	-.003	.113	-.107
Question 14	.451	.382	.271	.096
Question 15	-.1	.79	-.099	-.099
Question 16	.069	.692	.281	-.175

Oblique Solution Primary Pattern Matrix-Orthotran/Varimax

	Factor 1	Factor 2	Factor 3	Factor 4
Question 1	.748	-.132	-.154	-.184
Question 2	.863	-.003	.095	.168
Question 3	.075	.648	.032	.149
Question 4	.066	.141	-.001	.829
Question 5	.092	.132	.446	-.192
Question 6	-.238	.067	.689	.345
Question 7	.269	.248	.212	-.348
Question 8	.151	.307	.515	-.07
Question 9	.63	.061	-.1	-.19
Question 10	-.017	-.143	-.027	.786
Question 11	-.004	.873	-.127	.017
Question 12	.003	-.308	.781	-.071
Question 13	.871	-.068	.057	.056
Question 14	.468	.329	.199	.211
Question 15	-.162	.85	-.225	-.112
Question 16	-.029	.662	.18	-.15

Oblique Solution Reference Structure-Orthotran/Varimax

	Factor 1	Factor 2	Factor 3	Factor 4
Question 1	.671	-.123	-.142	-.168
Question 2	.775	-.003	.088	.152
Question 3	.067	.607	.03	.136
Question 4	.06	.132	-.001	.754
Question 5	.082	.123	.413	-.175
Question 6	-.213	.063	.638	.314
Question 7	.242	.232	.196	-.317
Question 8	.135	.287	.476	-.063
Question 9	.565	.057	-.093	-.173
Question 10	-.015	-.134	-.025	.715
Question 11	-.004	.817	-.118	.016
Question 12	.003	-.288	.723	-.065
Question 13	.781	-.064	.052	.051
Question 14	.42	.307	.185	.192
Question 15	-.145	.795	-.208	-.102
Question 16	-.026	.619	.167	-.137

Primary Intercorrelations-Orthotran/Varimax

	Factor 1	Factor 2	Factor 3	Factor 4
Factor 1	1			
Factor 2	.146	1		
Factor 3	.203	.343	1	
Factor 4	-.411	-.104	-.132	1

Variable Complexity-Orthotran/Varimax

Variable	Orthogonal	Oblique
Question 1	1.404	1.278
Question 2	1.083	1.1
Question 3	1.171	1.139
Question 4	1.082	1.071
Question 5	2.356	1.658
Question 6	1.984	1.765
Question 7	3.713	3.499
Question 8	2.37	1.878
Question 9	1.446	1.255
Question 10	1.295	1.07
Question 11	1.004	1.043
Question 12	1.155	1.322
Question 13	1.069	1.029
Question 14	2.737	2.675
Question 15	1.096	1.255
Question 16	1.487	1.261
Average	1.653	1.519

Proportionate Variance Contributions

	Orthogonal	Oblique		
	Direct	Direct	Joint	Total
Factor 1	.314	.311	-.004	.307
Factor 2	.299	.302	.019	.321
Factor 3	.191	.193	-3.563E-5	.192
Factor 4	.196	.194	-.015	.179

Factor Score Weights for Oblique Transformation Solution-Orthotran/Varimax

	Factor 1	Factor 2	Factor 3	Factor 4
Question 1	.302	-.049	-.14	.013
Question 2	.422	-.033	-.005	.277
Question 3	.058	.265	-.071	.138
Question 4	.271	.097	-.033	.585
Question 5	-.076	-.05	.272	-.115
Question 6	-.093	-.093	.46	.186
Question 7	-.016	.035	.09	-.182
Question 8	-.026	.012	.288	-.015
Question 9	.236	.022	-.12	-.007
Question 10	.232	-.014	-.006	.531
Question 11	-.002	.386	-.198	.041
Question 12	-.112	-.292	.556	-.067
Question 13	.399	-.058	-.021	.201
Question 14	.238	.093	.05	.242
Question 15	-.098	.394	-.246	-.077
Question 16	-.097	.228	.036	-.089

Factor Score Weights for Orthogonal Transformation Solution-Varimax

	Factor 1	Factor 2	Factor 3	Factor 4
Question 1	.274	-.056	-.12	-.034
Question 2	.346	-.019	.01	.193
Question 3	.035	.245	-.031	.117
Question 4	.133	.08	-.025	.516
Question 5	-.023	.003	.257	-.11
Question 6	-.092	-.019	.418	.177
Question 7	.038	.058	.101	-.179
Question 8	.008	.066	.282	-.025
Question 9	.221	.014	-.095	-.045
Question 10	.103	-.025	-.016	.474
Question 11	-.007	.341	-.137	.036
Question 12	-.055	-.186	.492	-.062
Question 13	.338	-.044	-.007	.124
Question 14	.188	.105	.07	.185
Question 15	-.077	.339	-.184	-.059
Question 16	-.055	.229	.066	-.079

References

Adams J G U (1982) The efficacy of seat belt legislation (SEA Paper No. 820819). Warrendale, PA: Society of Automotive Engineers.

Adams J G U (1983) Public safety legislation and the risk compensation hypothesis: The example of motorcycle helmet legislation. Environment and Planning C: Government and Policy, 1, 193-203

Adams J G U (1985a) Risk and Feedom: The record of road safety regulation (Transport Publishing Projects, London).

Adams J G U (1985b) Smeed's law, seat belts, and the emperor's new clothes. In: Human Behaviour and Traffic Safety (Edited by L Evans and R C Schwing) (Plenum Press, New York), 193-248.

Adams J G U (1988) Risk homeostasis and the purpose of safety regulation, Ergonomics (UK), 31,4, 407-428.

Alexandersson (1972) Some data about traffic and traffic accidents. Swedish Road Safety Office, Stockholm.

Allen R W, Hogue J R, Stein A C, Aponso B Land Rosenthal T J (1990) Low cost, real time simulation based on microcomputers. Proceedings of the Human Factors Society, 34th Annual Meeting

Ball R G and Jackson A (1988) We'll say it my way: a user-oriented approach to direct voice input, In Contemporary Ergonomics, Edited by E D Megaw, Taylor and Francis.

Baron M L and Williges R C (1975) Transfer effectiveness of a driving simulator.

Human Factors 17(1), 71-80

Bever T G, Garrett M F and Hartig R (1973) The interaction of perceptual processes and ambiguous sentences, Memory and Cognition, 12, 29-33.

Bishop R W (1967) Evaluating simulation instruction for accomplishing driver education objectives. Research Review 11, 12-17

Blaauw G J (1979) Simulation and road design. Institute for Perception TNO, report 12F 1979-C21. Soesterberg, The Netherlands

Blaauw G J (1982) Driving experience and task demands in simulator and instrumented car: A validation study. Human Factors 24(4), 473-486.

Ben-David G, Lewin G, Haliva Y and Tel-Nir L (1972) the influence of personal communication on the driving behaviour of private motorists in Israel. Accient Analysis and Prevention, 4, 269-301.

Bertalanffy L (1968) General system theory (G. Braziller, New York).

Bjerver K and Goldberg L (1950) Effects of alcohol ingestion on driving ability, Quarterly Journal on the Study of Alcohol, 1, 1-30.

Blomquist G (1983, October) The role of behavioural response in traffic safety regulation. Paper presented at the Fifth Annual Research Conference of the Association for Public Policy Analysis and Management, Philadelphia, PA.

Blomquist G C and Pelzman S (1981) Passive restraints: An economist's view. In R W Crandall and L B Lave (Eds) The scientific basis of health and safety regulation (pp 37-42). Washington D C : Bookings Institution.

Bragg B W E (1981) Risk of accident involvement for motorcyclists. In 'Road Safety' Praeger Publishers.

Brenner B (1967) Alcoholism and fatal accidents, Quarterly Journal of Studies of Alcohol, 28, 517-528.

Brindle L (1986) Some further evidence refuting risk homeostasis theory, Proceedings, ATEC 86, The lack of road safety (Paris, June 9-13).

Broadbent D E (1987) Skill and Workload. In: Psychology at Work, edited by P. Warr, Pelican Books, 9-29.

Brooks P and Guppy A (1990) The social context of driver error in accidents invovling motorcycles : a theoretical framework and empirical exploration. Driving Behavior in a Social Context. Caen, Paradigme, 1990.

Broome D R and Marshal L (1980) Assessment of ship course-keeping performance with variation in environmental disturbance, In: Human Factors in Transport Research Volume 1, (Eds D J Oborne and J A Lewis), Academic Press. pp 31-39.

Brouwer W H (1987) Bejaarden in het verkeer. In Sociale Verkeerskunde edited by C W F van Knippenberg, J A Rothengatter and J A Michon (Assen: Van Gorkum)

Browning J J and Wilde G J S (1977) The effect of beverage alcohol on perceived risk under realistic and simulated traffic conditions. Proccedings, seventh International Conference of Alcohol, Drugs and Traffic Safety (Melbourne, Australia, 23-28 January).

Cacciabue P, Decortis F, Mancini G, Masson M and Nordvik J P (1989) Cognitive modelling as a simulation technique for the development of intelligent user interfaces, In: Simulation in the Development of User Interfaces pp 40-49.

Cannell W P (1980) Maritime collision avoidance, In: Human Factors in Transport Research Volume 1, (Eds D J Oborne and J A Lewis), Academic Press. pp 22-30.

Carpenter J A (1959) The effect of caffeine and alcohol on simple visual reaction time, Journal of Comparative and Physiological Psychology, 52, 491-496.

Chapanis A (1965) Words, words, words, Human Factors, 7, 1-17.

Chapanis A (1967) The Relevance of Laboratory Studies to Practical Situations, Ergonomics, 10, 557-577

Chapanis A and Lockhead G R (1965) A test of the effectiveness of sensorlines showing linkages between displays and controls, Human Factors, 7, 219-29.

Cheepen C, Hewitt J, Hunter C and Monaghan J (1989) User application simulation in the evolution of an intelligent speech interface, In: Simulation in the Development of User Interfaces, pp 50-64.

Codling P J (1974) Weather and road accidents. In: Climatic Resources and Economic Activity (Edited by J A Taylor)(Newton Abbot: David & Charles)

Cohen A, Smith M and Cohen H H (1975) Safety program practices in high versus low accident rate companies (HEW Publication No. NIOSH 75-185). Washington, DC, US Government Printing Office, June 1975.

Colbourn C J and Cockerton-Turner T (1989) Simulating peer interaction via computer-based "HELP" facilities, In: Simulation in the Development of User Interfaces, pp 65-74.

Cownie A R (1970) An operational game of the study of decision making in a hazardous activity, Accident Analysis and Prevention, volume 2, 1-10

Crandall R W and Graham J D (1984) Automobile safety regulation and off-setting behaviour: Some new empirical estimates. American Economic Association Proceedings, 74, 328-330.

Crawford A (1963) The overtaking driver, Ergonomics, 6, 153-170.

David H (1969) Human factors in air traffic control: a study of the ability of the human operator to predict dangerously close approaches between aircraft on simulated radar displays. Unpublished PhD thesis, Loughborough University of Technology.

David H (1992) Evaluation of two advanced ATC display systems. In Contemporary Ergonomics, Edited by E J Lovesey, Proceedings of the Annual Conference of the Ergonomics Society, Aston University, Birmingham, 7-10 April. Taylor and Francis, pp 47-52.

Davis R T and Stahl R W (1964) Safety organization and activities of award-winning companies in the coal-mining industry (US Department of the Interior Information Circular 8224) Washington DC, US Department of the Interior.

Dorn L (1992) Individual and Group Differences in Driver Behaviour, unpublished PhD thesis, The University of Aston in Birmingham.

Dorn L, Glendon A I, Hoyes T W, Matthews G, Davies D R and Taylor R G (1991) Group Differences in Driving Performance, Paper presented to the 3rd annual Meeting of the Road User Behaviour Group, Manchester University, 17-18 September 1991.

Edwards D S, Hahn C P and Fleishman E A (1977) Evaluation of laboratory methods for the study of driver behaviour: Relations between simulator and street performance, cited by L Dorn in 'Individual and Group Differences in Driver Behaviour', unpublished PhD thesis, The University of Aston in Birmingham.

Edwards W (1954) The theory of decision making, Psychological Bulletin, 51, 380-417.

Edwards W (1955) The prediction of decisions among bets, Journal of Experimental Psychology, 51, 201-214.

Elliot E (1960) Perception and Alertness. Ergonomics 3, 357-364.

Evans L (1985) Human behavior feedback and traffic safety, Human Factors, 27, 555-576

Evans L (1986a) Risk homeostasis theory and traffic accident data, Risk Analysis, 6, 81-94

Evans L (1986b) Comments on Wilde's notes on 'Risk homeostasis theory and traffic accident data'. Risk Analysis, 6, 103-107

Evans L (1987) Fatal and severe crash involvement versus driver age and sex. General Motors Research Laboratories, Publication GMR-5721.

Evans L and Rothery R (1976) The influence of foreward vision and target size on apparent inter-vehicular spacing, Transportational Science, 10, 85-101.

Farmer E and Chambers E G (1939) A Study of Accident Proneness Amongst Motor Drivers. Industrial Health Research Board Report No. 84, London: IHRB.

Fine B J (1963) Introversion, extraversion and motor driver behaviour, Perceptual and Motor Skills, 16, 95.

Gibson J J and Crooks L E (1938) A theoretical field-analysis of automobile driving. The American Journal of Psychology, 51, 453-471.

Graham J D (1984) Technology, behaviour and safety: An empirical study of occupant-protection regulation. Policy Sciences, 17, 141-151.

Graham J D and Graber S (1983) Automotive safety regulation : Is rational driver response a critical factor? Paper presented to the Fifth Annual Research Conference of the Association for Public Policy Analysis and Management, Philadelphia, October 1983.

Grant B A (1985) Empirical investigations of the theory of risk homeostasis and the study of risk perceived by automobile drivers. PhD Thesis, Queen's University, Kingston, Ontario, Department of Psychology.

Greenwood M, Woods, H M and Yule G U (1919) A report on the incidence of industrial accidents upon individuals with special reference to multiple accidents, Report No.4, Industrial Health Research Board. In W Haddon, E A Suchmann and D Flein (eds) Accident Research (1964) New York: Harper.

Hagen R E (1975) Sex differences in driving performance. Human Factors 17(2), 165-17.

Hale A R and Hale M (1972) A Review of the Industrial Accident Research Literature, Committee on Safety and Health at Work Paper. London HMSO

Harris D, Brindle L R and Muir H C (1986) Driver overtaking behaviour - an observational study, In Comtemporary Ergonomics, Edited by D J Oborne, Proceedings from the Annual Ergonomics Society's Conference, held at The University of Durham, UK, pp 98-102.

Health and Safety Executive (1986) HM Nuclear Installations Inspectorate: Safety Audit of BNFL Sellafield 1986, (HMSO).

Hedlund J H (1985) Recent US traffic fatality trends. In: Human Behaviour and Traffic Safety (Edited by L Evans and R C Schwing) (Plenum Press, New York) 7-19.

Herms B F (1972) Pedestrian crosswalk study accidents in painted and unpainted crosswalks. Highway Research Record, 406, 1-13.

Hill J M M and Trist E L (1953) A consideration of industrial accidents as a means of withdrawl from work situation, Human Relations, 6, 357-380.

Hopkin V D (1963) Laboratory studies in conflict detection II. Track velocity, angle of approach and distance between tracks. Flying Personnel Reseach Committee, FPRC/1208.

Hopkin V D (1965) Laboratory studies in conflict detection III. Turning tracks. RAF Institute of Aviation Medicine, IAM report no. 328.

Hovland C I (1962) Computer simulation of thinking. In Simulation in social science: readings. Edited by Guetzkow H. pp16-29, Prentice-Hall International, Inc.

Howarth C I and Gunn M J (1982) Pedestrian safety and the law. In A J Chapman, F M Wade and H C Foot (Eds), Pedestrain Accidents, London: Wiley, 265-289,

Hoyes T W (1990) Risk homeostasis in a simulated air-traffic control environment, Unpublished Masters Thesis, The University of Hull.

Hoyes T W and Baber C (1992) Risk homeostasis theory in a simulated driving environment: a verbal protocol analysis. Manuscript in progress.

Hoyes T W and Glendon A I (1992) Risk homeostasis theory: issues for future research. In press (Safety Science).

Jackson D N, Hourany L, and Vidmar N J (1972) A four-dimensional interpretation of risk taking, Journal of Personality, volume 40, 483-501

Janssen W (1990) The economy of risk; or, what do I get for my error?, Ergonomics, volume 33, no.10/11, 1333-1348.

Jellinek E M and McFarland R A (1940) Analysis of psychological experiments on the effects of alcohol, Quarterly Journal on the Study of Alcohol, 1, 272-371.

Jerison H J and Pickett R M (1963) Vigilance : a review and re-evaluation. Human Factors, 5, 211-238.

Jonah B A (1986) Accident risk and risk-taking behaviour among young drivers. Accident Analysis and Prevention 16, 255-271.

Jonah B A and Dawson N E (1979) Validation of the motorcycle operator skill test. Accident Analysis and Prevention, 11, 163-171.

Jonah B A, Dawson N E, and Bragg B W E (1981) Predicting accident involvement with the motorcycle operator skill test. Accident Analysis and Prevention, 13, 307-318.

Jonah B A, Dawson N E, and Bragg B W E (1992) Are formally trained motorcyclists safer? Accident Analysis and Prevention, 14, 247-255.

Jones E E and Akehurst R L (1980) Risk assessment: an outline. Bull. Institue of Maths Appl., 16, 252-258.

Kahneman D, Slovic P and Tversky A (eds) (1982) Judgement under Uncertainty: Heuristics and Biases (Cambridge University Press, New York).

Kaukinen R (1967) Overtaking at nighttime, Report no. 14, Department of Psychology, University of Tampere.

Kogan N and Wallach M A (1964) Risk-taking: A study in cognition and personality. New York: Holt.

Langley D, Baber C and Wankling J (1992) Reconfigurable displays for in-car information systems, Contemporary Ergonomics 1992 (Proceedings of the Ergonomics Society's 1992 Annual Conference, Brimingham, England, Taylor and Francis, 7-10 April 1992. pp 29-34.

Lee P N (1979) Has the mortality of male doctors improved with the reduction in their cigarette smoking? British Medical Journal, 2, 1538-1540.

Lichtenstein S and Fischhoff B (1977) Do those who know more also know more about how much they know? Organisational Behaviour and Human Performance, 20, 159-183.

Lund A K and Zador P (1984) Mandatory seat belt use and driver risk taking, Risk Analysis, 4, 41-53.

McDavid J C, Lohrmann B A and Lohrmann G (1989) Does Motorcycle Training Reduce Accidents? Evidence from a Longitudinal Quasi-experimental Study. Journal of Safety Research, 20, 61-72.

McKenna F P (1985) Do safety measures really work? An examination of risk homeostasis theory, Ergonomics, Vol28, No. 2, 489-498

McKenna F P (1986) Does risk homeostasis theory represent a serious threat to ergonomics? In Comtemporary Ergonomics, Edited by D J Oborne, Proceedings from the Annual Ergonomics Society's Conference, held at The University of Durham, UK, pp 88-92.

McKenna F P (1988) What role should the concept of risk play in theories of accident involvement?, Ergonomics (UK), 31,4, 469-484

McPherson K and McKnight A J (1976) A task analytic approach to development of a motorcycle operator licence skill test. Human Factors, 18, 351-360.

Mackay G M, DeFoneka C P, Blair I and Clayton A B (1969) Causes and Effects of Road Accidents. Department of Transportation, University of Birmingham.

Marshall E and Baker S (1985) Strategies for the prevention of control room based accidents in nuclear plants, In Contemporary Ergonomics, Edited by D J Oborne, Proceedings from the Ergonomics Society's Annual Conference held in Nottingham, UK, 27-29 March, 1985. pp 111-116.

Marshall E C, Duncan K D and Baker S M (1981) The role of withheld information in the training of process plant fault diagnosis. Egonomics, 24, pp 711-724.

Matthews G, Dorn L and Hoyes T W (1992) Individual Differences in Driver Stress and Performance, Paper presented to the Annual Ergonomics Society Conference at Aston University.

Mercer G W (1986) Influences on paggenger casualty accidents: unemployment driver gender, driver age, drinking driving and restraint device use. (Ministry of the Attorney General, British Columbia, Canada, February).

Mittenecker E (1982) Methoden und Ergebnisse der psychologischen Unfaallforschung. Vienna, Austria: Deuticke.

Mortimer R G (1984) Evaluation of the motorcycle rider course, Accident Analysis and Prevention, 16, 63-71.

Mortimer R G (1988) A Further Evaluation of the Motorcycle Rider Course, Journal of Safety Research, 19, 187-196.

Murrell K F H (1962) Industrial aspects of ageing, Ergonomics, 5, 147-153.

Näätänen R and Summala H (1975) A simple method for simulating danger-related aspects of behavior in hazardous activities, Accident Analysis and Prevention, Volume 7, 63-70

Näätänen R and Summala H (1976) Road User Behaviour and Traffic Accidents (Norht-Holland, Amsterdam).

Nagayama Y (1984) An analysis of accidents involving motorcycles and suggestions for drivers' education. IATSS Research Report, 8, 28-39.

Narborough-Hall C S and Hopkin V D (1988) Human factors contributions to air traffic control evaluations, In Contemporary Ergonomics, Edited by E D Megaw, Taylor and Francis.

Newbold E M (1926) A Contribution to the Study of the Human Factor in the Causation of Accidents. Report 34, Industrial Health Research Board. London: IHBR.

NII (1979) Safety assessment principles for nuclear pwer reactors. HSE - Report HA5, (HMSO)

NII (1982) Ergonomics/ human factors in the design and safe operation of pressurised water reactors. Report NII - ONSWG (92) P.1.

NII (1983a) Safety assessment principles for nuclear chemical plant. Health and Safety Executive.

NII (1983b) Sizewell 'B' : A review of HM Nuclear Installations Inspectorate - Supplement 13: Human Factors. HII 01, SUPP 13.

O'Hanlon J F (1977) Directory of research laboratories operating driving simulators and research vehicles in North America. Washington DC:Transportation Research Board, Committee report No A3B06

O'Neill B (1977) A Decision-Theory Model of Danger Compensation, Accident Analysis and Prevention, 9,3, 157-165.

O'Riordan T (1977) Environmental ideologies, Environment and Planning, 9, 3-14.

OECD (1985) Traffic safety of elderly road users (Paris: OECD).

Oborne D J (1987) Ergonomics at Work, 2nd Edition, John Wiley and Sons, 300-316.

Ogden G D, Levine J M and Eisner E J (1979) Measurement of workload by secondary tasks. Human Factors, 21, 529-548.

Orr L (1982) Goals, risks and choices, Risk Analysis, 2, 239-242.

Orr L (1984) The effectiveness of automobile safety regulation: Evidence from the FARS data. American Journal of Public Health, 74, 1384-1389.

Page S J (1983) A study of control room team activities during simulated nuclear power plant disturbances, In Contemporary Ergonomics, Edited by Karenna Coombes, p135.

Partyka S C (1984) Simple models of fatality trends using employment and population data. Accident Analysis and Prevention. 16, 211-222.

Peltzman S (1975) The effects of automobile safety regulation. Journal of Political Economy, 83, 677-725.

Planek T W (1981) The effects of ageing on driver abilities, accident experience and licensing. In Road Safety edited by H C Foot, A J Chapman (Eastbourne: Praeger), pp 171-180.

Planek T W, Driessen G and Vilardo F J (1967) Industrial safety study. National Safety News, 96, 60-63.

Prem H and Good M C (1984) Motorcycle Skills Assessment. Australian Office of Road Safety, Melbourne, Australia.

The Royal Society (1983) Risk Assessment: A study group report. ISBN 0 85403 2088.

Rutter D R, Quine L and Chesham D J (1992) Social Psychological Factors in Road User Behaviour: An Examination of Motorcycling, Final unpublished report prepared by the Institute of Social and Applied Psychology for the Centre for Research in Health Behaviour.

Salthouse T A (1982) Adult Cognition, New York: Springer.

Shaw L and Sichel H S (1971) Accident Proneness: Research in the Occurrence, Causation, and Prevention of Road Accidents, Oxford: Pergamon.

Shafai-Sahrai Y (1971) An inquiry into factors that might explain differences in occupational accident experience of similar size firms in the same industry (Doctoral Dissertation, Michigan State University) Dissertation Abstracts International 32, 2247A (University Microfilms No. 71-23, 242).

Simonds R H (1973) OSHA compliance: "Safety is good business.", Personnel, 50, 30-38.

Sinaiko H W and Brislin R W (1973) Evaluating language translations: experiments on 3 assessment methods, Journal of Applied Psychology, 57, 328-334.

Slovic P (1962) Convergent validation of risk-taking measures. Journal of Abnormal and Social Psychology, 65, 60-71

Smith R G and Lovegrave A (1983) Danger compensation effects of stop signs at intersections, Accident Analysis and Prevention, Vol. 15, No. 2, 95-104

Smeed R (1948) Some statistical aspects of road safety research, Journal of the Royal Statistical Society, Series A, 112, 1-23.

Stager and Hameluck (1990) Ergonomics in air traffic control, Ergonomics, 33(4), 493-499.

Stammers R B and Partick J (1975) The psychology of training, Essential Psychology Series, Mathuen and Co, Ltd.

Stanton N A (1990) The psychology of alarms, In Contemporary Ergonomics, Edited by E J Lovesey, Taylor and Francis, pp 378-383.

Stanton N A (1992) Human Factors in Alarms, Unpublished PhD thesis, The University of Aston in Briminham.

Stanton N A and Booth R T (1990) The Psychology of Alarms. In Contemporary Ergonomics, Edited by E J Lovesey, Taylor and Francis, 378-383.

Stanton N A, Booth R T and Stammers R B (1992) Alarms in human supervisory control: a human factors perspective, International Journal of Computer Integrated Manufacturing, Vol 5(2) : 81-93.

Suchman E A (1965) Cultural and social factors in accident occurrence and control, Journal of Occupational Medicine, 7, 487.

Summala H (1986) The deterministic man in a stochastic world: risk management on the road. In New directions in research on decision making (Edited by B Brehmer, H Jungermann, P Lourens and G Sevon) (Elsevier, Amsterdam).

Summala H and Koivisto I (1989) Unaltered drivers' brake reaction times: older drivers compensate for their slower reactions by driving more slowly, Paper presented at International Symposium on Driving Behaviour in a Social Context, Paris, 16-18 May, 1989).

Summala H and Naatanen R (1988) The zero-risk theory and overtaking decisions. In T Rothengatter and R de Bruin (eds), 'Road User Behaviour: Theory and Research (Van Gorcum, Assen), 82-92.

Svenson O (1981) Are we all less risky and more skillful than our fellow drivers? Acta Psychologica 47, 143-148.

Taylor R G, Dorn L, Glendon A I, Davies D R and Matthews G (1990) Age and sex differences in driving performance: Some preliminary findings from the Aston Driving Simulator. Paper presented at the 2nd meeting of the Road User Behaviour group. University of Nottingham. September

Tillman W A and Hobbs G E (1949) The accident-prone automobile driver, American Journal of Psychiatry, 106, 321-331.

Tränkle U and Gelau C (1992) Maximization of subjective expected utility or risk control? Experimental tests of risk homeostasis theory, Ergonomics, vol 35, no.1, 7-23.

Trice H M and Roman P M (1972) Spirits and Demons at Work: Alcohol and Other Drugs on the Job. New York: New York School of Industrial and Labor Relations, Cornell University.

Tversky A and Kahnemann D (1981) The framing of decisions and the psychology of choice, Science, 211, 453-458.

Van der Molen, H H and Botticher A M T (1988) A hierarchical risk model for traffic participation, Ergonomics, 31, 537-555.

Van Wolffelaar P, Rothengatter T and Brouwer W (1987) Compensation strategies of elderly car drivers. In Comtemporary Ergonomics, Ed E D Megraw, Taylor and Francis.

Van Zelst R H (1954) The effects of age and experience upon accident rate, Journal of Applied Psychology, 38, 313-317.

Veling I H (1984) A Laboratory Test of the Constant Risk Hypothesis. Acta Psychologica, 55, 281-294.

Watts G R and Quimby A R (1979) Design and validation of a driving simulator for use in perceptual studies. Transport and Road Research Laboratory. DOT report 907.

Weber S J and Cook T D (1972) Subject Effects in Laboratory Reseach: An examination of subject roles, demand characteristics and valid inference. Psychological Bulletin, Vol 77, no. 4, pp273-295.

Weinstein M S (1969) Achievement motivation and risk preference. Journal of Personality and Social Psychology, 13, 153-172

Welford A T (1985) Changes of performance with age: an overview. In N Charness (Ed) Ageing and Human Performance, New York : Wiley

Whitfield D (1987) Human reliability from a nuclear regulatory viewpoint, Contemporary Ergonomics, Edited by E D Megaw, Taylor and Francis, 58-63.

Whitfield D, Ball R G, and Ord G (1980) Some human factors aspects of computer-aiding concepts for air traffic controllers, Human Factors, 22(5), 569-580.

Whitfield D and Stammers R B (1978) The air-traffic controller. In The study of real skills: volume I; The analysis of practical skills. Edited by W T Singleton (MTP Press Ltd). pp 209-234.

Whitlock F A, Stoll J R and Rekhdahl R J (1977) Crisis, life events and accidents, Australian and New Zealand Journal of Psychiatry, 11, 127-132.

Wiener E L (1980) Midair collisions: the accidents, the systems, and the Realpolitik, Human Factors, 22(5), 521-533.

Wierwille W W and Fung P P (1975) Comparison of computer-generated and simulated motion picture displays in a driving simulation. Human Factors 17(6), 577-590

Wilde, G J S (1982a) The theory of risk homeostasis: Implications for safety and health, Risk Analysis, Vol 2, No. 4, 209-225

Wilde G J S (1982b) Critical Issues in risk homeostasis theory (Response), Risk Analysis, Vol.2, No. 4, 249-258

Wilde (1984) Evidence refuting the theory of risk homeostasis? A rejoinder to Frank P. McKenna, Ergonomics, Vol. 27, No. 3, 297-304.

Wilde G J S (1985a) The use of incentives for the promotion of accident free driving. Journal of Studies of Alcohol, Supplement No. 10, 161-167.

Wilde G J S (1985b) Assumptions necessary and unnecessary to risk homeostasis, Ergonomics, vol. 28, No. 11, 1531-1538.

Wilde G J S (1986a) Notes on the interpretation of traffic accident data and of risk homeostasis theory - a reply, Risk Analysis, 6,1, 95-101.

Wilde G J S (1986b) Beyond the concept of risk homeostasis: suggestions for research and application towards the prevention of accidents and lifestyle-related disease. Special Issue: Risk, Accident Analysis and Prevention, Volume 18(5), 377-401.

Wilde G J S (1988) Risk homeostasis theory and traffic accidents: Propositions, deductions and discussion of dissension in recent reactions, Ergonomics (UK), 31,4, 441-468.

Wilde G J S (1989) Accident countermeasures and behavioural compensation: The position of risk homeostasis theory, Journal of Occup. Accidents, 10,4, 267-292

Wilde, G J S, Claxton-Oldfield, Stephen P, and Platenius, Peter H. (1985) Risk homeostasis in an experimental context, in Human Behavior and Traffic Safety, edited by Leonard Evans and Richard C. Schwing (Plenum Publishing Corporation)

Wilde G J S and Kunkel E (1984) Die begriffiche und empirische Problematik der Risikikompensation. Zeitschrift fur Verkehrssicherheit, 30, 52-61.

Wilde G J S and Murdoch P A (1982) Incentive systems for accident-free and violation-free driving in the general population. Ergonomics, 25, 879-890.

Whitfield D, (1987) Human reliability from a nuclear regulatory viewpoint, Contemporary Ergonomics, Edited by E D Megaw, Taylor and Francis, 58-63.

Wright G (1984) Behavioural decision theory: an introduction (Sage Publications, Beverley Hills, Calif.).

Wrogg S G (1961) The role of emotions in industrial accidents, Archives of Environmental Health, 3, 519.

Zwick W R and Velicer W F (1986) Comparison of five rules for determining the number of components to retain, Psychological Bulletin, 99, 432-442.