RECOMMENDATIONS FOR FUTURE HIGHWAY SAFETY RESEARCH

by

B.J. Campbell, Ph.D.

University of North Carolina
Highway Safety Research Center

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INTRODUCTION

A few thoughts are in order as to how I have approached this task for FHWA in which I am to lay out my ideas concerning research needed in the future. First, it is obvious that I am not equally qualified to write in all subject areas. In this paper I will not even address many specific research areas. Thus, I limit my exposition to areas in which I have thoughts to offer, but even then some of my comments grow out of limited knowledge.

I might also say that I did not find it possible to use reference materials in such a way as to be "led" unerringly to the research areas I am offering for consideration. Rather, the areas in which I think research should be considered grow out of a synthesis of my experience, and I am unable to point to specific "accident facts" as a basis for inclusion.

Next, when I recommend research, I do not confine myself to research that should be sponsored by the federal government. I believe that research should be sponsored by state and federal governments, commercial interests, and also public organizations like foundations. Research topics which are in the nature of policy analysis perhaps should *not* be sponsored by the government since it is usually the policies of government that are being considered in such research. Thus, some of the research areas I mention are candidates for industry or foundation sponsorship.

Finally, I make no claim that these thoughts on needed research are exclusively mine. Some of the matters discussed herein follow closely to the content of the book Traffic Safety: A Global Challenge which I wrote along with seven other authors. Other materials are slightly adapted from a paper I recently prepared for the March 1988 Highway Safety Symposium sponsored by the American Society of Civil Engineers. Still other thoughts are adapted from the various presentations made at conferences called the 2020 Conferences sponsored by the Highway User's Federation for Safety and Mobility. Also, in recent times I have talked with many of my research colleagues from the USA, Australia, and England. In addition, the very process of writing this paper in parallel with seven colleagues similarly engaged under the auspices of FHWA had led to conversations and interactions that have helped to shape the text I am presenting here. The point is that the materials presented here reflect conversations with many people in several countries, and I am most likely presenting research needs here which were initially formulated by others. As I said, I make no claim that these recommendations originated with me.

The topics on which I offer observations are listed below:

A. Statement of the Problem

B. Human Behavior Research

1. Behavior Research-Vigilance
2. Risk Perception
3. Crash Epidemiology of the Elderly
4. Ergonomics of the Elderly
5. Elderly as Research Subjects
6. Policy Analysis Regarding The Elderly
7. Value Research
8. Longitudinal Behavior Study
9. Information Load
10. Driving Simulator Research

C. Vehicle Research

11. Passenger Car Research
12. Truck Standards
13. Truck Aggressivity
14. Biomechanics

D. Highway Research

15. Enhancement of Traffic Flow (Prometheus)
16. Capacity
17. Automatic Highways
18. Relationship of Crashes to Roadway Geometry
19. Utility Poles
20. Critical Literature Reviews

E. System Research

21. Multi-State Enriched Data
22. Calculate a Delta V Simulator from Police Data
23. Vehicle Damage Scale
24. Design Vehicle to Record Roadway Geometrics
25. Roadside Data Collection
26. Relate police-reported injury (KABCO) to Other Trauma Measures
27. Research on Improved Injury Measures
28. Exposure Control
29. Tax Policy Research
30. Research on the Value of Time in Small Packets
31. Standards Rationalization
32. Training for Engineers

F. Conclusion

A. STATEMENT OF THE PROBLEM

I believe we should describe the highway crash losses of our country in comparison to the societal losses attributed to other disease forms. Since highway crashes differentially claim the young, and since they constitute by far the leading cause of death for the young, I think the best way to characterize highway deaths is in terms of the numbers of years of useful life lost from these deaths. From calculations I have carried out, I understand that to be an average of 40 years of life lost per motor vehicle death. Though there are many fewer deaths from motor vehicles than from cancer or cardiovascular diseases, it is likewise true of the latter that they tend to be diseases that strike older people. In fact, when all premature deaths are calculated (presumably in terms of how many years the death occurs before the "normal" end of life), it is found that injuries (of all kinds) account for 40.8% of all the years of premature death. (Injury in America, National Academy Press, Washington D.C., page 5.)

When years of lost life are portrayed, highway crash deaths are seen as much more of a formidable health problem for the country, ranking ahead of cardiovascular diseases in annual costs according to one writer, and 60% of cancer (Hartunian, Smart, and Thompson, 1981, The Incidence and Economic Costs of Major Health Impairments, Lexington Books, DC Heath and Company, Lexington, Massachusetts). When viewed in that light, the research resources available to prevent highway crash losses are revealed as being trivial. Our country presumably spends about 26 federal dollars on highway safety research per year of life lost due to motor vehicle crashes. The comparable research amount relative to years per life lost due to cardiovascular deaths is eleven times as high, and for cancer the figure is 23 times as high. These figures are based on calculations from information in Chapter Seven of Injury in America, cited above. These figures are based on data pertaining to federal research expenditures only (and non-military figures at that). However, the authors of the book cited state their belief that there is no *substantial* source of such research funding outside of the federal budget. Even if we assume that the actual national research budget in highway safety were twice the amount listed here, the funding is still too low by an amount highly disproportionate to the size of the problem.

In view of the above we can legitimately call for greatly increased research funding for highway safety. Based on estimates of travel growth over the next few decades, we can cite an annual death toll of 65-85,000 fatalities by 2010 even if the 100 million mile death rate of 2.5 is a maintained. This is the price of continuing "as is".

B. HUMAN BEHAVIOR RESEARCH

BACKGROUND

Much of highway safety research has been influenced by prevailing theories of human behavior which in turn have reflected conservative and liberal philosophies that have influenced public policy during the half century that automobiles have been a part of national life in the U.S.

It is my belief that during the 1930s, for example, the prevailing philosophy on the part of the highway safety "establishment" was more nearly conservative than liberal in nature. This grew out of the fact that members of the professions that most influenced public policy at that time -- police officers, surgeons, automobile industry officials, highway engineers -- more often tended to be conservative rather than liberal in their personal philosophies. Embodied in this conservative philosophy was a view of human behavior consistent with that philosophy.

At that time the theory of human behavior, as pertained to highway accidents, hinged on emphasis of individual responsibility and the belief that automobile accidents were, by and large, the result of reprehensible if not criminal behavior. The view was, in part, that through research, it would be possible to identify the people that embodied these undesirable characteristics, and such people could either be removed from the highway or their behavior changed through intervention. However, this view of human behavior was so superficial as to be untrue. As a matter of fact, it was not a scientific theory of human behavior at all, but rather a political theory.

Thus, there was a period beginning before the 1930s and up through the 1940s and 1950s wherein a significant amount of the research done in the highway safety field was in effect a search for the "accident-prone driver." There was a common sense view that certain drivers were persistently overrepresented in crashes, coupled with a belief that large inroads on the problem could be made by identifying and changing these few drivers. The first part of this common sense conception is accurate, but the second part is not. It is true that some few drivers have three and even four times as many crashes as does the typical person, but such drivers are so few that even if all were "corrected" completely, the resulting savings would be quite small. This view of human behavior persists today among some people, even though, beginning in the 1930s, research showed that those who are accident repeaters in one period of time account for only a small portion of crashes in the next period of time.

Nonetheless, during that period, the search for "bad" drivers and the search for ways to make drivers better constituted a significant part of the modest amount of research done at that time. There was a decades-long search for variables that would predict bad drivers, along with research on education or re-education through driver licensing and driver license sanctions. The catch

phrase of these "conservative" behavior theories was to "get the bad driver off the road".

During the 1950s and 1960s, however, there began to be a counterpoint to this over simplified politically conservative view of human behavior. The alternate view of human behavior was espoused by people with more nearly liberal political philosophies -- injury control specialists, public health professionals, consumer advocates and the like. The more nearly liberal philosophy also had a catch phrase as the conservative did. The catch phrase of the liberals is, in part, that "education doesn't work" (and presumably therefore we need to achieve highway safety through the instrumentalities of society -- things like air bags, improved highway and vehicle crashworthiness, etc.)

This liberal philosophy proceeded on the basis of research which showed the basic flaw of the accident proneness view, and also was partly an outgrowth of the best research on high school driver education which failed to show much, if any, crash reduction among those who are trained relative to those who are untrained. However, the view that "education doesn't work" likewise over simplified the understanding of human behavior to the point of being inaccurate, and likewise constituted more of a political theory about human behavior than a scientific theory.

It is erroneous on its face to say that "education doesn't work". The people who advocate this view do so in the English language well enough to enunciate the position, and those who hear have been learned to understand and react. It is difficult to say that education doesn't work when people have existed such as Einstein, Shakespeare, or Bach -- or for that matter, any of the people reading this text. The human organism is eminently educable, not only in individual skills but in coordinated behavior among groups.

This is illustrated by considering the collective skills manifested at a busy intersection in Beijing where human and animal drawn carts, bicyclists, pedestrians, trolley buses, etc., and all manner of motorized vehicles ply the area at once. The very fact that the intersection operates with efficiency of somewhere around .999 (or perhaps an order of magnitude better) is testament to individual understanding of the right of way conventions and to the mutually facilitative behavior which people exhibit as they follow the paths they want, perceive the movement of others, and subtly compensate such as to minimize contact. All this comes from "education" -- it isn't inborn.

However, it is clear that, whereas human performance can be made *nearly* perfect, it cannot be made *completely* perfect. Therefore, "accidents" occur in every facet of behavior, whether it be something as benign (normally) as a slip of the tongue during conversation or as catastrophic as forgetting to extend the flaps before a takeoff in a commercial airliner.

One of the obvious characteristics of human behavior is that it is possible to train to a very high level of skill, those behaviors which occur reasonably often and for which there is an opportunity, through reinforcement, to shape that behavior to a high standard. On the other hand, what seems more difficult is a training regime which attempts to suppress behavior that is already very infrequent, e.g., accident behavior.

I believe the country is long overdue for a resumption of human behavior research for the future, not least because there has been so little during the past 25 years as public policy and research was directed toward the needed "catch-up" work in the areas of highways and vehicles. The standstill in behavior research has left the nation ill-equipped to proceed in this area. This needed renaissance in behavior research must not be limited by the superficial expressions of the role of behavior enunciated in the past. Rather it should proceed on the basis of today's understanding of the science of human behavior and performance.

If we believe that defensive driver training programs do not work because we are trying to suppress an already rare event, we might come up with an alternative view of behavior -- to try to strengthen or shape behaviors that are more frequent and not try to deal with the catastrophic failure behavior which is so rare.

Research 1. Behavior Research-Vigilance

One area for such research would be the area of sustaining driver vigilance (the other side of the coin from driver fatigue -- when driver fatigue is defined more broadly than just sleepiness). It can be shown that the human organism cannot sustain high levels of vigilance indefinitely, and this has been noted as a problem for sailors standing watch, peering to the horizon, watching amidst the nothingness for something that is unlikely to occur, or likewise a radar operator who spends hours waiting for "the blip that never comes".

Too often, when the key event does occur, the vigilance level (or level of arousal) has dropped so low that there is no appropriate reaction. The difficulty is that level of arousal may not be be sustainable without external props to that behavior. As can be seen, there could be analogy in all this to driving, although driving is not a stimulus-poor environment (like that confronting a night lookout on shipboard); driving is, by any measure, a stimulus-rich environment. Even so, it might be possible to embark on a research program to determine whether there are vehicle systems that might be useful to help maintain level of arousal at a suitable state.

One can imagine any of several systems that would keep the person busy and, hopefully, alert in the car. One idea would be light sources distributed around the visual field within the car. These bulbs might intermittently switch on, driven by a computer, at relatively infrequent, irregular intervals. When the bulb

comes on, the driver responds quickly to turn the bulb off. On the basis of the theories of conditioning, it seems possible to train eye movement search patterns such as to keep the person from getting "dull" -- just staring straight ahead.

Other systems might be studied. There might be a way of signalling the driver in response to changes in pressure on the steering wheel, or if there is some unusual movement of the head such as a drooping forward, etc. Also, there might be a programming of auditory or visual events which call for some response. These events should occur sufficiently frequently to sustain the requisite level of vigilance.

There are several aspects to this research.

One research task would be that of determining the best indicator of a decline in level of arousal. Would it be blink rate, blink duration, head motions, postural changes (rate or magnitude), pressure on steering wheel, steering reversals, accelerator pedal use? Defining this surrogate of arousal entails a major research task in itself. It would involve several elements: define the measures, determine the external criterion to which these measures should be related, develop the apparatus.

The next task would be to determine how these measures change with time and level of tiredness (hours on task, subjective reports of tiredness). This is research to define the dependant variable.

The next task is to define some systems designed to counteract the decline in level of arousal (assuming results of the previous research task successfully defines how to measure that). This research could explore the area of introduction of new stimuli or competing tasks to offset the decline in arousal. One possibility would be to introduce stimuli at intervals and to require that the driver quickly respond. Then a research sub-task would be to determine the best schedule for introducing the external stimuli (based on other research, one would suppose a variable interval schedule would be a good starting point).

Subsequent research tasks would be to "exercise" the systems in the lab and on the road to determine the changes that external stimuli may bring about. The important thing is that the interventions bring about changes in the measures designed to approximate level of arousal, but that also, the maintenance of level of arousal be related to the driving task. That is, one doesn't want the subject to be able doggedly to keep responding to the external task, while still being a zombie as far as driving itself is concerned.

The above conception of research is confined to vehicle systems designed to help maintain level of arousal. However, it is equally legitimate to ask, in research terms, whether the road environment itself might play a role in helping (or hindering) the

maintenance of this level of arousal. One can also conceive of research tasks on this subject, and in fact this might be a topic in which a driving and roadway simulator might be useful.

Research 2. Risk Perception

It is of interest to know whether young drivers (in particular) perform the way they do in part because they do not recognize the risk in certain situations, or if they very well recognize the risk, but take the risk anyway (or some combination of both).

As a first step there would be an attempt to define high and low risk takers among both novice and experienced drivers. The way to define risk taking would be based on a questionnaire. The interview could get at matters of life style from which risk taking might be inferred -- such things as history of injury; broken bones, what the person liked to do; hang gliding versus coin collecting, etc. Risk taking scales may already exist. There may be military research or research in other areas that might have previously disclosed information about risk taking. Thus, a significant part of this undertaking would be a search of the literature and considerable care should be taken in creating the survey instruments by which risk takers are to be defined.

Next, one would photograph or video tape traffic scenes of many kinds designed to sample the full range of situations from those that are nominally nearly risk free to those that are highly risky. (This assumes that the "true risk" can be defined by the researcher or at least that a panel of experimenters can set up an hierarchy and can meaningfully rank such situations.) The experimental task would be to force the subject to make a quick judgment about the level of risk depicted in the scene. The subject might be required to press one of five buttons which defined his assessment of risk on a five point scale. The advantage of a still photo or slide of the scene is that the picture could be shown rather rapidly (as by a tachistoscope). The advantage of video would be that the motion could be shown. It may be that it is essentially impossible to rate the apparent risk without seeing the dynamics of the situation as portrayed by motion.

Next, after the subjects have responded to the scenes, an item analysis would be done to see if there were items that differentiated the four groups. Can a metric be defined in which it can be shown that, for example, young risk takers fail to see hazards in scenes that older risk takers perceive? Likewise does an age effect show up for young versus old risk avoiders (or risk takers).

This research might lead to the question of whether there are certain road designs that would reduce the difference in driving behavior between high and low risk takers. The point of this comment is to be reminded that the driver interacts, as a part of a system response, to the vehicle and the roadway, and each can presumably play a role in the performance of the other.

RESEARCH RELATIVE TO ELDERLY DRIVERS

Background

The US population is aging considerably now. As an example, the age group of this writer (just under 60) is forecast to increase 100% during the next 22 years even though overall population will increase only about 15 percent. The median age is now around 31 years now, but will be 38 years old in 2010. The growth among younger age groups, those drivers with the highest fatality rates, will be minimal. On the other hand, the country will, for the first time, be experiencing a large number of old drivers. I assume that old people will be living by themselves even more in the future than today, and the lack of public transportation will continue to make it desirable for them to have access to individual motorized transportation.

The net change in population age distribution should, however, be beneficial for highway safety. I examined the current population motor vehicle death rates by age group and re-calculated today's age distribution into the form it is forecast to have in 2010. I then calculated that today's highway death toll would actually have been reduced by about 800 deaths if the population age distribution were already what it is predicted to be in 2010. Thus, the additional elderly drivers probably will not produce as many deaths as are avoided because of the proportionately fewer young drivers. Nevertheless, the aging of the population sets the stage for increased research on that topic. The paradox of the older driver is that on the one hand, they tend to self-limit their exposure in terms of the number of miles they drive and the times and places they drive, such that when their risk is expressed as the number of crashes per 1000 years licensed, their record is not seen as particularly bad. On the other hand, even when one takes into account the "safer" times and places they drive, their crash rate per mile driven is quite high -- perhaps as high as young drivers.

Research 3. Epidemiology of Crash Injury Involving Elderly Victims

There is need for a research program to define the crash involvement and crash injury problems of elderly drivers in today's (and tomorrow's) environment. One part of that research program would consist of analyses of available data bases for detailed characterization of the crash experience of elderly drivers from many perspectives. First, there should be analysis more nearly characterizing the types of crashes in which the elderly are culpable, other types of crashes in which they may not be culpable, but nevertheless are victims -- in other words, the who, what, when, where and why of crashes involving elderly drivers. It would also be useful to search the literature to see if it is possible to relate this current information to the scattering of studies on elderly drivers from the past -- to learn whether crash

characteristics of elderly drivers are changing.

It is also important to examine the definition of elderly. In the past this may be thought of as those older than 65. I believe however, that the research should be aimed at two or three levels of elderly. Some aging processes are well underway before people reach 60 -- the decline in high frequency hearing for males, and the decline in night vision for both sexes. It might be useful to define one aging group as those 55 to 65; another as those 65-75, and another as those over 75. In some states there are doubtless sharp increases in the number of active drivers even at the age of 80 and more. Aggregating all those 65 and over could obscure important processes.

With respect to crash injuries, it would be useful to conduct detailed studies of NASS, FARS and other crash injury files, to try to characterize the mechanisms of injury for elderly drivers in crashes. There are indications from a variety of sources that elderly drivers are susceptible to injury in a different way from younger drivers. It is said that their bones are more brittle, for example. Thus, it would be useful to seek a thorough understanding of the injury susceptibility of elderly drivers, the frequency with which they suffer broken bones, sustain head injuries, suffer unconsciousness etc. compared to younger drivers in similar crashes. In this way we could gain information as to vehicle and highway design requirements from the standpoint of their appropriateness to the growing population of elderly drivers.

Research 4. Ergonomics of Elderly Drivers

There should be research to look specifically at the vehicle cockpit environment from the standpoint of the normal driving task to learn whether and why it is more difficult for elderly persons. There should be simple studies of the ability to reach and properly activate the controls, adjust the mirrors, work the seat belt, etc. There should also be attention to the ability of the elderly subjects to do these tasks in the context of an unfamiliar car and/or in the context of reduced lighting. It is important to know whether there are specific control functions or specific locations within the car's cockpit that pose differentially greater problems for the elderly.

One aspect of this research should be the impact of such improvements on the driving exposure of the elderly. If such driver aids permit an increase in exposure at no higher level of risk per unit exposure, then it would presumably be an acceptable alternative for society. If, however, the technical aids induced a higher level of exposure at the same higher level of risk, then the increased mobility of the individual might pose a higher level of risk to society.

Also, there should be detailed studies of belt systems from the standpoint of use by the elderly, since it is obvious that fine motor coordination is reduced among the elderly, let alone

the ability to turn and twist and reach the belt. An assessment should be made of the difficulty of reaching around and securing the belt, getting it properly located around the body, and seeing well enough to fasten the belt. This research could lead to alternate designs or optional kits to make it easier for the elderly to use the belts. This is particularly important since the elderly are perhaps less likely to be belted and are more at risk of death from their injuries.

Research 5. The Need to Include Elderly Subjects in Most Research

It is well known that with aging there is a decline in visual acuity, and a differentially higher decline under conditions of reduced lighting. This has implications for highway signing, driver licensing and the whole range of governmental functions related to the driver. Through research, there should be a recalibration of various standard tasks as they apply to elderly subjects. To do this there should be a standard requirement that when human subjects are being studied, the study protocol should include a substantial number of elderly drivers -- specifically, a subset large enough that the data could be analyzed according to age of subject. A brief survey study could be done to determine current practice. It may be that many critical standards are based on measurements of the response of exclusively young subjects.

Research 6. Policy Analysis With Respect to Mobility of the Elderly

It would be appropriate to carry out additional research of an economic or policy analysis nature as to the presence of the elderly driver in the U.S. over the next thirty years. What are the societal tradeoffs of encouraging continued high mobility to elderly drivers? Will it be possible for the system to retain, in the future, the policy of allowing elderly drivers unrestricted access to the roads as we have in the past? Or must society consider restricting access of elderly drivers to the highway through license control? Would restricting mobility of the elderly be a good deal for society or would it constitute such a burden on the other support systems as to rule out such considerations (access to shopping, medical care)?

Research 7. Value Research

This section perhaps illustrates a class of research that would be considered inappropriate for federal funding and therefore may be better funded by a foundation or some other private source.

Much of what society is willing to do about highway crashes -- the resources we are willing to invest and the countermeasures society will tolerate -- depends on societal values and how those values can be modified. A program of research might be undertaken

to try to understand better what the public (and, separately, public officials) value the most -- what programs they value, what would lead them to act in the "public interest," and what would not.

For example, action has been taken by the federal government on the subject of All Terrain Vehicles (ATVs). A consent decree was reached between the Consumer Product Safety Commission and the manufacturers of ATVs. Also, at least one state is considering legislation on all these vehicles. It seems that some value research might be done to try to understand when, how, and why the public is willing to act relative to ATVs -- how the public perceives the threat from ATVs on the one hand versus (let us say) motorcycle crash helmets on the other.

What would be required is a series of surveys involving a series of judgment processes in which subjects are asked to rate the threat they perceive to society and their willingness to act with regard to a range of program options. They would be asked to consider what would be an appropriate response to various threats to society -- possession of hand guns, freedom to use ATVs on private property, necessity for bicycle helmets, necessity for motorcycle helmets, appropriateness of seat belt laws and the like.

Another aspect of the value research would be to try to understand the degree of agreement between perceived risk and actual risk. This would be based on a series of comparisons and subjective judgments in which subjects might be queried to get at questions like the estimated per trip danger of fatality in an airplane, versus a car, versus a snowmobile etc. and to compare these respective risks with the perceived risk of being struck by lightening, eaten by a shark, etc.

This would indicate what people perceive to be the hierarchy of risk for the various events, and it might be possible through research to find the ways in which those rankings vary as a function of demographics, socioeconomic status, private citizen, public official, etc. It would also be possible to compare the perceived risk with the true risk. This might indicate the basis on which people are willing to take individual or collective action in one case compared to others.

Another subset of this research would be to try to determine why society seems willing to put so much more of its resources into health measures than it does safety measures. What are the elements of the willingness of society (or the willingness of public officials) to devote a great deal more of society's resources into research on heart disease, cancer, and AIDS than for highway safety.

As a part of this research there could be an attempt to determine whether an important factor is the way the public perceives the degree of professionalism of the research community in highway safety versus that in the areas of heart disease, cancer,

or AIDS. Thus, what is the degree of hostility versus friendliness toward highway safety initiatives proposed by the government or the auto industry as opposed to health initiatives proposed by medical or pharmaceutical interests.

The purpose of this research would be to gain greater understanding about how the public views the problem of highway accidents compared to other problems, and would have a goal of identifying some of the barriers that exist to greater public and financial support for research and program activities in highway safety.

Research 8. Longitudinal Behavior Study

At a recent conference a speaker noted in passing that he thought drivers were doing a worse job nowadays than in the past -- that they are paying less attention to courtesy, signaling, etc. Of course, he had to speculate as to the truth of that proposition because no one knows whether drivers are now better or worse in such regards than they were in the 1940s or 1950s. That is an unfortunate situation. We need a formal longitudinal study in which a long-term commitment is made to gather driver behavior data every few years at the same locations to see whether long-term trends are indeed underway.

I have in mind, for example, a situation in which driver behavior at, let us say, 50 roadway locations is observed in terms of the percent of drivers who signal for a left turn, the percent that violate stop signs, the headway distances maintained, acceptance or rejection of gaps, etc. This would require that the same behaviors be measured over a period of time, and I think the results would be instructive.

Perhaps one can discern some of the previous trends from existing literature. In the July 1958 issue of Traffic Quarterly, a study was reported by O.K. Normann in which measurements were carried out at identical sites during two time periods separated by some years (1938 and 1957). The acceptance of opportunities to pass was compared. During the later period drivers were more careful about accepting opportunities to pass even though vehicle acceleration capabilities were greater than in the earlier years. Obviously, one cannot make too much of that single study, but one interpretation would be that driver behavior had, if anything, become somewhat more prudent over the years. Yet we are left only to speculate about this rather basic matter.

Such research would involve (1) defining the behaviors to be measured and development of measurement protocols, (2) selection of sites at which the measures are to be conducted, and (3) beginning of a procedure by which these measures are taken at intervals over a period of many years.

Research 9. Information Load

Another area of behavioral research would be continuing research to address the attempt to ease competing tasks in driving. The human is reasonably effective in handling competing tasks, but can get overloaded and when that happens some of the responses tend to drop out. Usually, the more peripheral responses are the ones that drop out, but one does not like to have such an overload situation in any case. If there is a situation in which the driver is having to read signs and maneuver the car, all in a short time, there can be difficulties. All of us have experienced the situation in an unfamiliar setting where the signs, requiring a route change, "sneak up on us too quickly." This is relevant to the question of how to define a rate of information load that is reasonable.

It is not necessarily clear to me how this research should proceed, thus an initial undertaking might be a project to define the research task and methodology. One part could be a review of research in aircraft and aerospace areas to look at what is known about divided attention and juggling attention among competing tasks. Then, it might be possible to define some high and low accident intersections or stretches of roadway, and to study driver decision characteristics and information load related to that stretch of roadway. Thus, an initial research task in this area is a review of past work (including areas outside highway safety), and a problem definition effort.

Research 10. Driving Simulators for Research

The issue of driving simulators for research has a long and somewhat spotted history. For many years -- even decades -- there has been a view by some that there is at least some need for such simulators. Others have had an uneasy feeling that such a simulator was, in effect, a capability looking for a research reason to exist.

It has always been understood that the lack of a high fidelity visual field in such a simulator was an extreme limiting factor. A close second to that deficiency was the lack of a sufficiently high fidelity motion platform. Some continuing simulator research is being done. FHWA continues to use a low fidelity simulator, forced by budget considerations to utilize technology that is perhaps 20 years old.

I am not entirely sure that a major simulator research effort is called for, but I do believe that research should be undertaken to keep track of changes in the state-of-the-art that might alter the likelihood that a high fidelity simulator can be manufactured.

Daimler-Benz (D-B) has a simulator with a six degree-of-freedom motion platform (certainly sophisticated compared to that seen previously). The D-B simulator also has what appears to be a

rather high fidelity computer-generated computer display for the visual field. This is perhaps not surprising. It is very obvious that computing power has increased by orders of magnitude during the last 25 years and, the computer driven visual field seems an obvious choice.

Any video arcade now has computer games in which the visual display goes well beyond that contemplated even for a research device a few years ago. Increased computing power and solid advances in computer graphics open up the possibility that a high fidelity driving scene can be presented accompanied by computers powerful enough to update the visual scene the required several times per second.

It seems at the very least that research should be undertaken which allows examination of the state-of-the-art. This would include examination of simulators used in other areas. Obvious candidates would be the latest versions of commercial aircraft flight simulators, the latest tactical air force combat aircraft simulators, and the latest for tank and submarine warfare. Also aerospace simulators at NASA as well as the driver simulator at D-B should be assessed.

The other part of the research on this capability would be a rather hard-nosed attempt to define research problems that might be appropriately and uniquely addressed by the simulator. I am recommending at least enough research to update the studies of simulator feasibility conducted about twenty-five years ago.

C. VEHICLE RESEARCH

PASSENGER CARS

Background

Passenger cars have received the most government regulation of any highway vehicle, which is appropriate for the USA. Nevertheless, consideration should be given to the next round of standards for these vehicles (as well as extending these standards to other vehicles).

Research 11. Optimizing Restraint Systems to Reach More Users

In the past (prior to the seat belt law era) restraint use was confined to a fairly small number of people who voluntarily used the belt. Presumably a substantial portion of these volunteer users had significant knowledge about how belts should be worn. With the introduction of belt laws, however, belt use is being induced among many who formerly were not users. Among these are an increased number of elderly, obese, or pregnant drivers or occupants. Also among these are an increased number of wearers who know little about correct use, and may not be particularly motivated to learn.

Research 11a. Obese occupants

Misuse of the belt restraint appears to be a particular problem for obese occupants, based on roadside observations. Therefore, it is proposed that research be undertaken, aimed at understanding the particular usage problems of obese occupants. The research project would involve systematic roadside observations aimed at belt misuse in general, and special problems of the obese in particular. When the frequency and nature of misuse patterns is uncovered, it would then be possible to undertake an experimental phase in which obese subjects would be instructed as to the particular dangers of their misuse. This would help to determine the extent to which the misuse problems among the obese can be overcome by public information. Beyond that, however, the research should also examine the feasibility of design changes that will make it easier for obese persons to use the belt systems properly. This could include after-market devices, but should also consider whether there are basic design elements at the manufacturers level that could be incorporated.

Research 11b. Pregnant occupants

A subset of the above would be the special study of pregnant occupants from the same standpoint.

Research 11c. Elderly occupants

As has been said before, elderly occupants appear to have more difficulty than younger people in reaching the seat belts and

fastening them properly. The shoulder belt is located around behind them and requires a fair amount of twisting and turning to reach the belt. They also may have difficulty drawing the belts around and additional difficulty with the eye-hand coordination involved in fastening the belt. In part this may be a simple matter of reduced illumination at the point where the belt receiver is located, and the fact that the buckle may be at a distance at which neither part of bifocal glasses are "in focus".

An analysis should be made of the the range of belt use problems faced by the elderly. This analysis should consist of a roadway observation phase, and also an experimental phase in which the performance of the elderly (compared to younger subjects) can be observed. Based on such analysis, some of the design difficulties with existing belt systems could be determined, and consideration could be given to easing these problems either with after-market devices or in the original design.

Research 11d. Comfort and convenience features

The above research issues are part of the larger question as to the general comfort and convenience of belts in cars. It seems fair to say that, with the advent of belt laws, there are now literally tens of millions of belt users who formerly were not. There are probably many of these new users (as well as old users) who are impatient if not enraged with the perceived inadequacies of the system. Comfort may not be a stand-alone inducement to belt use, but for a person looking for an excuse not to wear the belt, any annoyance or discomfort may provide a sufficient rationale for rejecting the belts.

As a minimum, it seems that research should be done to put some parameters on comfort and convenience issues. I can envision research in which a variety of sizes, shapes, and ages of users are asked to wear belts and to rate their comfort. For what percent of the population is there a problem of the shoulder belt chafing across the neck. What percent cannot wear a belt properly in the rear seat by virtue of their body size? Are particular car configurations more of a problem than others? Part of this research might be a matter of trying to obtain internal research presumably done by the auto industry to evaluate their own designs (such auto industry research probably would be regarded as proprietary). After the problem definition part of the work, the next task would be to formulate new design concepts aimed at increasing the degree to which shoulder belts can fit properly, not coming across the neck or face, and to which lap belts can be worn snugly.

Research 11e. Vehicle systems to increase belt use

Through analysis of the effects of belt laws, it has been demonstrated that belt use can be increased and held at a reasonably high level through an appropriate mix of enforcement and public information. However, it may be that vehicle systems could

also provide a boost to belt use. Research should be done to develop and test ideas of vehicle systems designed to remind people to use seat belts.

One such approach would be use of the voice synthesizers already on some car models. The voice synthesizer that already informs the person that the door is ajar, or the fuel is low, could also deliver a seat belt reminder message, could then repeat the message several times as long as the belt is not fastened, and could deliver a "thank you" message when the belt is fastened.

This voice synthesizer could presumably be "smart" enough to distinguish between the driver and the right front occupant. A "please and thank you" although amusing to hear from a voice synthesizer, and perhaps regarded only as a "trick," nevertheless embodies sound principles of behavioral reinforcement. Calling attention to the desired behavior and giving a verbal reinforcement when that correct behavior occurs has been shown to be successful in scores of contexts.

Research 11f. Interlocks

In addition, research could address other kinds of reminders of the sort available in the 1970s when ignition-belt interlocks were tried abortively. A variety of "bells and whistles" as belt use reminders could be tried to see which is most effective. Some of this research was presumably done in the 1970s, but it seems appropriate to reexamine the issue now because the status of restraint use is different today from what it was in the 1970s.

Although Congress ruled out ignition interlocks as a way of enforcing the use of belts, other types of interlocks could be tried. It would be perfectly possible, for example, to design a system such that when the vehicle is in motion the radio could not be turned on except that the belt is fastened. Also, it may be time to conduct new research on the ignition interlock. The reliability problems may be less severe now, and public acceptance may be greater. Also, in about two years 100% of new cars must be equipped with passive restraints. There is a very sober question of whether there will be wholesale disengaging of these passive device by users who are less than enthusiastic about belt use. Because of this possibility, it might be appropriate to consider vehicle systems to prevent driving with the automatic restraint system disengaged, or else to set up some vehicle system to encourage the person not to disengage the system.

In the initial part of this research there would need to be surveys to determine the scope of the problem; then some focus group or survey work to determine public attitudes toward various measures; then a system design phase; then trial use of the devices in fleets.

Research 12. Extend Standards to Trucks

Certainly the federal government should conduct the necessary research and analysis preparatory to extending crashworthiness standards, already developed for passenger cars, to the widest number of vehicles feasible from the standpoint of technology and politics. An obvious candidate is trucks. Clearly, the vehicle market has changed over the last few decades such that the market penetration of non-passenger cars is now higher than before. A great variety of light trucks are on the market and for much of their exposure, are being used in the same way as a passenger car. Therefore, it seems appropriate to carry out the necessary research to analyze, standard by standard, the appropriateness of the standard and the modifications necessary to apply these standards to trucks.

Research 13. Heavy Truck Aggressivity

Heavy trucks constitute an important part of traffic deaths in the U.S. However, a disproportionate number of the deaths that occur in connection with heavy truck crashes are sustained by drivers in other vehicles rather than the truck drivers themselves. It seems to me that a research program should be undertaken, having both public policy and crash mechanics implications, dealing with the heavy truck aggressivity.

First, with respect to characteristics of the truck itself, a research program should be undertaken with the goal of defining changes in truck design to increase the chances that occupants of other vehicles would survive a crash with a heavy truck. This research process should include studies of underride contact from the rear in which passenger cars ride up under the rear of large trucks with severe consequences for the underriding vehicle. Once the problem definition studies are done, the next step would be to consider designs that could be added to the truck to reduce the consequences when such underride events occur. There should also be continuing research on side structures, again to prevent underride or contact between the wheels of truck or tractor and other vehicle.

The front-end structure of heavy trucks should be examined through research from the standpoint of adding energy absorbing material such as to give the struck vehicle a better chance in the case of a truck crash with a smaller vehicle. Research could examine the engineering of existing front end structures from the standpoint of how to make them more energy absorbing in a crash and should also consider such design approaches as adding to the front-end (let us say) a few feet of material whose sole purpose is to absorb crash energy. The research could determine how effective and how costly such an approach would be.

Such research would also have implications for weight and length standards to which the truck is subjected by state law.

Thus, there should be public policy research too, ascertaining ways to make the truck less aggressive in a crash, relative to financial or other penalties that would be imposed on the manufacturer or operator (the cost ultimately to be borne, of course, by the consumer-taxpayer). Incidentally, there are likely other areas of truck research that might be considered. One wonders whether there may still be useful research to be done in the area of truck conspicuity. Better light reflectance and light emission from the rear of a truck might also help eliminate some of the underride problems.

Further, there should be public policy research as to where the costs should be distributed. There should be research to analyze the financial and societal costs of large trucks. What is the situation with respect to the revenue they produce? What is the situation with respect to the societal costs they impose? If it is concluded that the societal costs they impose (through the injury and death to occupants of other vehicles) is sufficiently large, then consideration might be given to a tax policy with respect to heavy trucks such that the damages they inflict could be offset by the funds generated by a tax policy shift of this sort.

Research 14. Biomechanics of Impact

There is continuing need for better means to relate events that take place in staged crash tests using dummies to events that occur in real highway crashes in which people are exposed to harm. In a recent paper, Dr. David Viano of the General Motors Research Laboratories wrote on the needed research, and, with his permission, several extended excerpts from his work are presented here:

> The broad goal of injury biomechanics research is to understand the injury process and to develop ways to reduce or eliminate the structural and functional damage that can occur in an impact environment. To achieve this goal, researchers must identify and define the mechanisms of impact injury, quantify the responses of body tissues and systems to a range of impact conditions, determine the level of response at which the tissues or systems will fail to recover, develop protective materials and structures that reduce the level of impact energy and force delivered to the body, and develop test devices and computer models that respond to impact in a human like manner, so that protective systems can be accurately evaluated.
>
> #### Gaps in Current Biomechanical Knowledge
>
> The status of current knowledge about injury mechanisms, the understanding and quantification of biomechanical response and tolerance

to impact, and the availability of meaningful injury evaluation criteria and technology are summarized in Table 1. Although the head and spine house the organs that control life itself, and there is a reasonable understanding of the processes that generate structural injury, very little is known about the mechanisms of functional damage to the brain and spinal cord. Likewise, the mechanical parameters associated with anatomical alterations of thoracic organs are relatively clear, but the disruption of cardiac function after thoracic impact, without apparent structural damage, is only now being determined.

Research to improve our understanding of and ability to quantify biomechanical response to impact, as well as to establish impact tolerance thresholds, has always been handicapped by the obvious necessity of using human surrogates in impact experiments, except at very low severity levels. Human cadavers have provided a reasonable experimental model for skeletal response, especially for the skull, rib cage, and femur, and cadaver research continues to emphasize the response characteristics of the thorax and lower extremities. These studies are supplemented by the use of anesthetized animals for thoracic and abdominal impact experiments, the results of which can be scaled to human dimensions. There is an urgent need, however, to quantify head/neck impact response, because of the high incidence of brain and spinal injury in motor vehicle crashes and the serious and usually irreversible consequences of brain and spinal cord damage.

Test dummies are the primary tool for predicting injury, yet only a few measures of potential injury are assessed with current techniques during impact tests. The most common ones are the acceleration response of the head and chest and the forces on the femur. Although the measurement of femoral forces is well accepted and used, the mechanisms of femoral injury is not axial compression, but rather bending. The head also has a well publicized criterion for evaluation of injury risk (HIC), but the criterion is based on limited experimental verification and has not been correlated with the risk of brain damage. Some experimental data are available for the evaluation of neck injury, but they do not assess functional changes associated with risk of paralysis. Clearly, the two most significant body regions, the head and neck, are not

Table 1
STATUS OF KNOWLEDGE IN IMPACT BIOMECHANICS

Body Region	Injury Mechanisms	Impact Response	Impact Tolerance	Assessment Technologies
HEAD				
Skull	****	***	***	***
Face	***	**	**	**
Brain				
Structure	**	*	*	*
Function	**	*	*	*
SPINE				
Vertebrae	**	**	*	**
Spinal Cord				
Structure	**	*	*	*
Function	**	*	*	*
THORAX				
Rib Cage	****	****	****	***
Heart, Great Vessels				
Structure	***	***	**	**
Function	***	**	**	**
Lungs	***	**	*	*
ABDOMEN				
Solid Organs	***	***	***	**
Hollow Organs	**	**	*	*
EXTREMITIES				
Femur	****	****	****	***
Other Long Bones	***	**	**	**
Joints	***	**	**	**
Muscle	**	*	*	*
SENSORY ORGANS				
Skin	***	***	**	***
Other	***	**	**	*

* Unknown/Unavailable
** Hypothetical/Inadequate
*** Somewhat Understood and Verified/Useful
**** Well Known/Adequate

adequately evaluated using current testing technology, yet these are the regions most frequently and severely injured in motor vehicle crashes.

The above material and the table was quoted from: Injury Biomechanics Research: An Essential Element of the Prevention of Trauma, by D.C. Viano, J.W. Melvin, A.I. King, K. Weber, General Motors Research Lab Publication GMR-4951, Warre
n, Michigan, October 1987.

I have also received some input from Dr. Kennerly Digges, a senior level scientist in the Research and Development arm of NHTSA, and he has granted me permission to quote from some of his writings as well. Dr. Digges has written regarding the need for an improved crash test dummy, and has also proposed a research program on the mechanisms of head injury.

> One of the principal research tools for measuring the effectiveness of safety systems is the crash dummy. These dummies have instruments to measure the safety level when motor vehicles are crash tested. Safety engineers need better knowledge of how these dummy measurements relate to injuries and their consequences. This is a field in critical need of interdisciplinary research.
>
> A key question is how to calibrate crash dummies to measure the injuries that a human would receive under the same circumstances. In the past, the interpretation of dummy instrumentation data has been based on biomechanics data that are extremely expensive to obtain and limited in extent. Recently, computer simulation has provided a new basis for obtaining biomechanics information.
>
> Today, vehicle crashes can be simulated inexpensively by computer, so that a wider community of public health researchers can study the mechanisms of injuries and the potential for injury prevention and mitigation.
>
> Proposal For Head Injury Prevention Studies
>
> The outcome of a head injury is the result of the interaction of mechanical, medical, and psycho social variables over a period of time, often measured in years. The process begins with the mechanical insult and extends through the injured's biologic and psychological/emotional response, and finally to rehabilitation of the victim into family and society.

To address the total head injury problem, we recommend the research agenda address all phases and their interactions. This research should include the mechanisms which cause the injury, the resulting lesions and their symptoms, the benefits of the various alternatives for management, treatment and rehabilitation, and finally the societal costs of specific common head injuries.

The physical mechanisms that cause head injury are poorly defined. Understanding the causal mechanisms is crucial to good preventative medicine. It would also contribute to clinical medicine through improved management of the trauma victim.

To better understand injury mechanisms we need to study fundamental relationships between the mechanical conditions imposed on the brain and any resulting physiological sequela. Studies of this type require merging disciplines of mechanics and physiology to determine the relationships that link the mechanical state of brain tissue to its physiological capacity to function.

The head injury research should include projects to address the following needs:

1. Constitutive relationships for brain material, both neural and vascular, under dynamic loading conditions.

2. Failure criteria, both functional and physical, for both types of brain material.

3. Relationships of translational and rotational kinematics on the functional and physical state of the brain.

4. Relationships of specific brain lesions to physiological disfunction, and to probable outcome, both physiological and psychosocial.

5. Relationships of specific brain lesions, head injury symptoms (i.e., period of unconsciousness), and the magnitude and duration (dosage) of the phenomena which caused the injury.

6. Relationships of the injury causal phenomena, the resulting symptoms, alternative treatments, and the final outcome in terms of costs, the nature and magnitude of impairment and its effect

on quality of life.

7. Definition of the magnitude of the head injury problem in terms of specific injuries, their frequency, and total cost to the victim and society.

The above material was taken in part from a presentation Dr. Digges made at the Centers for Disease Control first annual Injury in America Conference, and in part from writings he personally communicated to me. The former was entitled Technological Approaches to Injury Prevention, Kennerly Digges, PhD, Deputy Associate Administrator for Research and Development, National Highway Traffic Safety Administration, 400 7th Street, SW, Washington, DC 20590.

Suffice it to say that future progress in vehicle crashworthiness depends heavily on further research progress in the biomechanics of injury, and that topic should be a major national priority.

D. HIGHWAY RESEARCH

Research 15. Use of Computers to Enhance Traffic Flow and Safety

This is a research area in which the United States may need to stake out a research initiative almost as a matter of a defensive ploy from a political standpoint. Within the last two years, the European automobile and research community has undertaken a major project called Prometheus and that effort may have, almost overnight, put the United States ten years behind.

Under the leadership of Daimler-Benz, an eight to ten year research and development activity is being funded in Europe involving a level of support of about 50 million dollars a year. Called Prometheus, it involves all the European automobile companies (with the significant exception that U.S. makers in Europe have been excluded). In addition to European car makers, the project is said to involve 50 or more research centers around Europe.

The idea of the research program is to develop means of communication between vehicles in order to improve the efficiency of traffic flow, reduce route uncertainty, avoid crashes, etc. The project envisions development of computer hard and software, networking and analysis capabilities such that there will be greater car to car communication, greater car to driver communication, and also greater communication between multiple drivers and a central computer.

It is anticipated that this would provide computer enhanced information to the driver to aid in passing maneuvers; to avoid running up on crash sites in reduced visibility situations; to give route information; to sort out cars that are interacting through an intersection, etc.

It seems to me that the United States should not let this European initiative go unchallenged, especially since this type of research really plays to the strength of the United States. I propose a research undertaking which at the very least critically reviews and assesses the Prometheus program and which outlines a strategy by which the United States could undertake this type of effort in the future.

It may be that some of the technology developed in the Prometheus undertaking will be in the open literature, for indeed, some of the basic research is apparently to be carried out by universities. But while the purpose is improved traffic flow and traffic safety in Europe, a logical add-on will be to give European automobile manufacturers a common advantage in all markets.

Some of the advance information about Prometheus is stated rather glibly in their literature, I must say. Not surprisingly, there appears to be a certain amount of press agentry in all this. Nevertheless the brochures indicate, for example, that an on-board

computer combined with radio technology could be used to indicate to the driver the fact that a car has stopped abruptly up ahead as in a crash. The signal would be emitted to all trailing vehicles in the hope of avoiding chain-reaction rear end collisions involving scores or even hundreds of vehicles such as occur once in a while in the fog in Europe (or the US for that matter).

Another approach envisions that on-board computers would calculate headway and speeds with other vehicles in the immediate vicinity and thus could aid the driver in judging whether whether a gap is acceptable for passing or entering the traffic stream. Through the same sort of communication system with a central computer, route directions could be provided on-board.

The reported level of funding is quite substantial and the United States probably should not ignore the effort particularly since it is in an area in which some research has already begun in this country. Therefore, I propose research which, at the barest minimum, would make an independent assessment of the promise that lies in this approach, and would carefully monitor and assess what is going on in Europe and elsewhere around the world.

Research 16. Highway Capacity

One of the future problems that deserves research attention is the competition among increasing numbers of vehicles for access to the existing highway system. In the absence of the ability to build many additional highways to meet this need, a research program should be undertaken to examine factors that now limit highway capacity and factors that might increase capacity.

Research 16a. Operations

I can imagine research on high capacity highways that might consider the extent to which congestion on the ramps spills out onto the main road, the extent to which sorting out traffic entering and leaving the main road reduces capacity on the through lanes, the extent to which better signing might be demonstrated to smooth out traffic such as to increase capacity.

Research 16b. Dedicated lanes

Within the context of the desire to increase capacity, there is perhaps room for additional research on the possibility of dedicated lanes for trucks on certain roadways having high truck traffic. One purpose of the research would be to inquire whether a high proportion of trucks in the traffic stream does in fact cause a disproportionate reduction in capacity, and whether a case can be made that special lanes for trucks could increase capacity overall (perhaps the trade would merely be an increase in capacity for non-trucks at the expense of reduced capacity for trucks. If so, would that be good or bad for society?)

Even if the question of dedicated lanes for trucks were not regarded as appropriate in terms of increasing capacity, there might be a safety benefit, and the research should consider that issue. If there were dedicated lanes for trucks, would this create a reasonable opportunity for a differential speed limit whereby cars could go 65 mph and trucks perhaps only 55 (and would that be desirable?).

Finally, in terms of cost, would special lanes for trucks be good for society in the sense that these special lanes could be built with increasing bearing strength perhaps reducing the damage to highways imposed by trucks.

Research 17. Automatic Highways

In view of the above, is it time to re-examine the future possibility of automatic guidance and headway control in light of technology changes that have happened since the round of such studies in the 1960s (and in anticipation of foreseeable advances during the next twenty years)? Technology relating both to tracking and headway control has already advanced since the automatic highway studies of the 1960s. Buried cable is now used as a way of guiding robot vehicles in factories and offices (though high speed applications may not yet be sufficiently proven). Automatic throttle control devices already are present in many vehicles as a convenience feature. Sensing technology has advanced sharply, perhaps re-opening the possibility of headway control through radar, infrared or laser sensing.

All these advances, combined with the fact that certain vehicles repeatedly use the same routes, increases the desirability of re-examining the issue. I do not know whether this would be fruitful. It doesn't seem like much of a safety gain if we merely make it easier for drivers to sleep behind the wheel. The point is that the research must consider not only the technology, but the operational factors including anticipated driver performance is such a setting.

Research 18: The Relationship of Safety to Roadway Geometry

There has been a great deal of research, too much of it poorly executed, on the relationship between various roadway characteristics and crashes. Indeed, three or four reviews of the literature have been published over a period of the last twenty-five or thirty years. However, these compilations of research tend to be based on uncritical reviews; therefore, the poor research has been mixed in with however much good research exists on the subject.

One result apparently is that the known relationships between safety and roadway geometry are not necessarily made a routine part of roadway design. We are told that, whereas roadway

designers must consider and forecast the cost of a design, the environmental impact of a design, the noise impact of a design, the travel time impact of a design, nevertheless there is no such requirement or practice of estimating the safety effects of a design.

This situation should be corrected, and a program of research should be aimed in that. This would involve still another attempt to estimate the relationship between safety and various elements of roadside geometry. This effort should, however, be a rather large scale, comprehensive and critical review of the literature, whereby the large amount of chaff is separated from the small (but hopefully existent) amount of wheat. If it is found that we basically just don't know enough about the relationships between roadway geometry and safety to design safe roads, then that should lead to a major research program of studies to close the knowledge deficit. If this requires the creation of major new data bases, then so be it (indeed the question of upgrading the data base is discussed elsewhere in this paper).

Research 19. Utility Poles

Among lethal roadside objects, utility poles constitute one of the major classes. Yet there is still research to be done in coming up with designs for making newly placed utility poles more crashworthy and also finding ways of modifying existing utility poles so as to increase their crashworthiness. There appears to be a lack of political will to do something about the problem. As long as there are significant gaps in the research, it will be much easier to avoid tackling the countermeasure problem. Therefore, a two-fold research process is in order.

The first is an extensive research process to come up with safer initial designs and initial placement strategies for utility poles, and second, ways to modify existing utility poles (at least high risk ones) to decrease their lethality when struck. A second research undertaking would be the matter of deciding which utility poles to modify. While it might eventually be possible to require crashworthiness in newly placed poles, it would seem clearly not feasible to require conversion of all existing poles. Surely, however, some poles are more at risk of being struck than others. There is recent research in Australia that purports to show a way of rating poles as to the likelihood of crash involvement and the research evidently showed that a modest portion of poles might be expected to account for quite a large portion of crash involvements.

Third would be research of a policy analysis nature relating to societal costs and benefits -- a sort of societal balance sheet with respect to utility poles, who pays for the utility poles; who pays for the casualty losses resulting from striking unyielding utility poles. There should be legal and public policy research to look into whether it would be in the interests of society to re-allocate some of that burden. At present, utility poles by the

millions are placed so close to the roadway as to constitute a hazard of considerable magnitude. Thus, the cost of current placement policies are obvious, and the costs are borne by the victims. The other question is to ascertain who benefits from that policy? Is it the utility companies, and is there the political will to do something about it? These are questions for policy research and analysis.

There will, of course, continue to be a philosophical barrier against action to make utility poles safer as long as the sentiment prevails that, "after all, it is the 'fault' of the drivers who hit the utility poles" and that neither the utility companies nor the highway officials have responsibility for pole design and placement to reduce injury. It may be that some of those political barriers to action can be overcome to some extent by research of the sort outlined above. One of the outcomes of the research may be to illuminate the existing situation by which utility companies and their poles have not shared proportionately in the "clean up" of the roadside.

Research 20: Literature Reviews

A great deal of research has been conducted over the past 20 years -- much of it is flawed, and some of it is good. However, the distribution of this information -- the technology transfer -- is lacking. Research contracts frequently call for a review of the literature as a part of the effort, but normally only a small amount of time is allotted to such a task, certainly not enough time for a comprehensive or critical review.

I think a part of any large scale national research program should include a process of systematic, critical reviews of the research literature. I am thinking of a task in which researchers are asked to make critical reviews of a focused part of the sub - literature in which they have maximum expertise. This would require that significant amounts of time be given to the reviewers. These reviews of the literature could be published and it might become somewhat easier to define areas of knowledge deficiencies and to transfer the knowledge available.

E. SYSTEM RESEARCH

BACKGROUND

Throughout my tenure in highway safety research, there has been a continuous lament as to inadequacies in the quality and quantity of accident data available for research. However, things are better than they were 35 years ago when I first began working in highway safety. More large data bases are computer accessible, and we see the beginnings of file linkage projects by which a richer array of crash data is made available.

Traffic records activities have been a priority area for NHTSA for some time, and in the very earliest years of NHTSA large amounts of 402 dollars were spent on traffic records hardware. Curiously, however, there has been relatively little research aimed at improving the quality and usability of the data itself. In the absence of such research, it is my view that a good deal of the money spent on hardware was less than optimally expended. The initial grouping of topics below aims at creation of a large scale data base of sufficient quality such as to be usable for research.

Research 21. Multi-State Project

This research would be a series of efforts leading to creation, updating, and use of a multi-state data bank of police reported highway crash data. The data from the states would be sufficiently similar to warrant combining the data into a pool of crashes amounting to approximately one million new cases per year.

For years, researchers have lamented the lack of quality and comparability across state lines that characterizes most of the nation's state crash data bases. Nevertheless, despite the obvious difficulties various users have now begun to pursue state crash data bases for an increasing variety of research tasks and have even tried to amalgamate such files across state lines because there is now sufficient "computer muscle" to do so. (This process of attempting to combine data across state lines is indicated by the CARD FILE efforts of NHTSA.)

CARD FILE and other such laudable efforts have, however, essentially taken the data as they exist. What I am proposing is research on the feasibility of creating a much superior data base of about a million cases a year. My belief in the need for this grows out of my perception of the inadequacies of the NASS data system. NASS is valuable and should be retained at some level, just as FARS should, but neither system produces enough cases annually to be usable for the kind of brand-name research and "early warning" research that needs to be done.

My view is that if NHTSA and FHWA were willing to work with a few states, they should be able to find a way to create such a data system. The reason such a thing has not happened before is that no one has ever been willing to put money into the states in return for the necessary actions to create a research-usable multi-

state mass data base.

Step 1: Survey existing crash report forms from relatively populous states and assess the quality and comparability of the data. This would involve a detailed look at what data is present on the form, what is coded onto the computer, what crashes are typically reported by the police etc.

Step 2: Recommend a group of possibly ten states that would be logical candidate states, such that with some relatively minimal changes in their data collection forms and procedure, a resulting source document would be created which had some degree of comparability among these several states.

Step 3: Negotiate with those states to determine what sort of financial considerations would be necessary for them to embark on a procedure of modifying their form to bring about this degree of comparability. I am not speaking here of a completely uniform accident report form among the six or seven states. Rather, I am thinking of some relatively modest alterations in the form, the addition of VIN coding, and a deformation rating scale, such that some key variables would be present in suitably common form.

In addition, the financial considerations to the state would be used in part to finance a newsletter and training procedure such that officers could be instructed, praised, admonished etc. in an attempt to keep the records and reporting system at a level such that the data would be usable.

Step 4: Finally, would be a compilation of data from these several states and a beginning of the long process accumulating and trying to use the data. Implicit in the use of the data would be discovery of many shortcomings. This would call for continuing feedback and training for the police officers, modification of coding procedures, discovery of computer processing errors, and in short, all the elements of the continuing process necessary to maintain a usable data base. This is all the more challenging since the data bases in most states have never been used at the level of rigor implicit in a research undertaking. Thus, there has not existed in the past a means by which the consequences of error came back to the part of the system that created the error.

Most of the above text has been addressed to crash data. However, along with that information is the continuing need for exposure data. If this kind of research program were undertaken, then an obvious and necessary part of the effort would be the acquisition of exposure data. One example of this is presented elsewhere in the call for a vehicle to record geometric characteristics of the entire road system -- mile by mile and foot by foot.

All of this would imply a long term federal commitment perhaps requiring a million dollars per year to each of the six or

seven states to guarantee that the data would continue to be forthcoming in the necessary quality. In addition, there would be a headquarters function where the data are compiled, stored and made available to users. This would also include a group of programmers and system analysts to study the data, compile it etc.

It seems to me that this sort of thing could and should be done even if it required some reduction in funds available to NASS.

Research 22. Research Project: Construct a Delta V From Police Data

One line of research that would be a part of the above multi-state data system would be analysis and modeling to seek a better indicator of the level of crash forces involved in a given real world highway crash. In almost any analysis of real world crash data aimed at either vehicle or roadway factors in crash injury, it is deemed desirable to characterize the level of physical forces involved in crashes.

One is ordinarily interested in severity of the crash if for no other reason than to use it as a control variable. For example, in an analysis of the effectiveness of seat belts, one would wish to control for crash severity when comparing the injury of restrained versus unrestrained occupants.

In some of the "in-depth" accident investigation projects sponsored by NHTSA, information is collected, including deformation measurements, such that a Delta V value can be calculated, hopefully with some degree of accuracy.

There are two lines of research that should be carried out here. One would be aimed at a validation of the ability of investigators at the scene to calculate a Delta V in the first place. In this regard, it would be useful to carry out a research project in which vehicles are crashed on a proving ground under controlled conditions with telemetry and other measurement procedures including photography, such that the Delta V can be accurately calculated.

Then, after the crash test is conducted (staged away from the view of investigators), a group of investigators could view the vehicles without their having been moved, make the required calculations and estimate Delta V based on their reconstruction. It would then be possible to "calibrate" the accuracy of the investigators ability to make these calculations.

A second and more important research project, however, would be to create a better indication of crash severity based on mass police data -- that is, the severity of the crash forces that happened. In North Carolina, for example, the accident report form routinely contains information including the Vehicle Identification Number. From this, the weight of the vehicle can be

calculated. In addition, officers apply a seven point deformation rating scale to the vehicle based on the TAD rating booklet. Even further, the report delineates the location of the damage, the manner of vehicle engagement, and the nature of the object struck if another vehicle is not involved. Also the officer gives at least a crude measure of skid marks both before and after the impact. The resulting injuries are also known.

It seems that a worthwhile project would include construction of a computer calculation model based on such physical parameters as those mentioned above. It would presumably be possible to construct a procedure for combining this information on mass, deformation, angle of impact, movement of vehicles after impact, in such a way as to calculate a number representing increased crash severity.

There are two ways this could be one done. One would be a strictly statistical combination of the above named predictor variables using a regression technique. The manner in which the variables were combined could be completely empirically based -- whatever happened to give the best prediction of resulting injury.

Another approach, however, would be a combination of these values based on the physical principles of the mechanics of the real world crash, giving appropriate consideration to vehicle mass, vehicle deformation, vehicle movement after impact, etc. These values could be combined in a manner consistent with the forces and motions implicit in such an impact. The resulting number would be a characterization of the severity of the crash, and would presumably be proportional somehow to the severity of the accelerations expected to be present in such a crash. Once a "DV simulator" were constructed, it would be possible to do some further research on the relationship between the behavior of that scale and the resulting injury. The advantage of the latter approach is that the predictive model would be based on physical principles of collision mechanics.

If such an algorithm could be developed it would be possible to computerize the process completely such that, as a part of the routine processing of police crash report forms, it would be possible to read in the information variables from the report form, process those values against the computation model, and then write on the computer record of that particular crash the DV simulator that was calculated. In North Carolina alone, this could be done for well over 100,000 vehicles each year.

Once a data base were created with such information thereon, further research would be possible to answer the empirical question as to whether or not the DV simulator had utility. Does it account for more injury variance than other crash severity indicators? If not, then empirically derived variables would have just as much utility. However, if the DV simulator were as good or better than any other predictor then it would have added value

in that it had meaning in the physical world derived from the matters of mass, crash engagement, deformation and the like.

Research 23. Vehicle Damage Scale for Police Officers

At present, at least two vehicle damage rating scales are used in the field by on-scene crash investigators. One is the CDI (Collision Damage Index) used by NASS investigators and the other is the TAD (Traffic Accident Damage) scale used by police officers in a few states. These scales give the reporting officer or investigator the means to characterize the degree of vehicle deformation in a way that can be coded into a computer. It is believed that degree of vehicle deformation is the best overall indicator of the severity of the crash forces that occurred within the collision event.

I am unaware of any calibrating research that has ever been conducted with respect to the CDI scale, but with respect to the TAD scale, research has been completed in which the inter-rater reliability of the ratings were examined. This research showed that properly trained police officers can rate crashed cars with suitably small amounts of classification disagreements between officers. Also, it has been shown that the scale accounts for more injury variance than does officers estimates of speed.

The scale was created out of a perceived need by researchers at the University of North Carolina. What I am proposing here is a research process embodying a more rationale construction of a pictorial damage rating scale for use by officers at the crash scene. It would be useful to create the pictorial scale using photographs of cars which were crashed at known speeds, and in which suitable measurements were taken so that the crash forces would be known. If photographs such as that were used as the basis for the scale used by officers, then their damage ratings would be made against a base line of crashes of known severity. Damage rating results from such a source would than have more meaning when fed into the computer-based DV simulator described above.

Research 24. Development of a Vehicle to Record Highway Geometrics and Features

With the increased computer capability now readily available to many users, it is now feasible to represent the state or local road network on a computer and, in fact, most states do this. However, in some states (North Carolina), a good many characteristics of the roadside and the roadway geometry itself is not represented on this computerized map. I am proposing research to develop a vehicle with the necessary number of sensors, inertial platforms, and event recorders such that the vehicle could be driven along the highway at modest speed and automatically record characteristics of the terrain over which the vehicle is passing.

This would imply an inertial platform to characterize

horizontal and vertical translations, and a precise odometer to record longitudinal travel. The operator could also have a keyboard so that events could be entered such as the beginning and end points of guardrails, the presence of sign posts, and such other roadside appurtenances as should be inventoried.

If one regards this only as a research tool, it is perhaps not necessary to have this capability in every state. It may be that this capability need exist only in the six or seven states alluded to in the proposed multi-state data base. Even if this capability were essential for the operations of every state, it is possible that fewer than one vehicle per state would be required. One of the things to be determined would be what is to be measured and how quickly per mile could the data be recorded.

At this point the technology with various on-board sensing devices, inertial platforms, etc. seem well within the available technology. Some versions of such vehicles already exist. The research question would be not only -- "Can such a vehicle be developed?", but "Can it be applied to create usable information?"

Research 25. Roadside Data Collection

As a part of collecting exposure data, some research attention might be given to techniques by which data collectors along the roadside could obtain information without undue interference with the traffic stream. An example of that, already in common use, is the practice by which observers count the presence of shoulder belts in moving traffic. However, other information is collectable from the passing traffic stream as well.

One way to do this would involve use of low powered radio station at 1,610 megahertz. (Presumably this frequency is designated for use with low powered transmitters to give strictly local information.) A sign could be placed along the roadway in which people would be alerted to a survey ahead and would be asked to tune to 1610. Then a particular research question could be asked -- one that involved a yes/no answer and people could then be asked to turn on their lights if the answer was "yes" and sound their horns if the answer were "no" when they passed a designated sign up ahead. (It might even be possible to record the data automatically).

It would be easy enough to carry out this research to determine what proportion of the people responded and then, of course, to ask these questions one at a time. It would also be possible to record the license plate number and through linking of data, find out certain characteristics about the make and model of the car, the registered owner, the zip code of the owner's address, etc. (To assess the "lie" factor, one could ask a question that could be independently verified, such as by reference to information obtainable from the license plate number.)

Thus, it might be possible to collect a considerable amount of data, one question at a time, through this rather inexpensive and non intrusive procedure. Clearly, this approach would not suffice for the multiple questions of an origin and destination survey, but where single questions could be asked, it might be worthwhile to examine the feasibility of this approach. For example, questions could be asked about trip length, trip purpose, time since trip origin, time since last stop, estimated time to next stop or to destination.

Research 26. Research on Relationships Between the KABCO Scale and Related Trauma Measures

Police officers in nearly all states use the KABCO scale to rate injury and everyone seems to recognize its imprecision. However, some research would probably be worthwhile to assess the ability of the scale to detect the effects of certain interventions design to reduce injury severity. Thus, is the KABCO scale sensitive enough to detect the benefit of seat belts? Is it effective enough to detect the difference between an effective head restraint and one that is ineffective one in terms of reduced neck injuries?

It would be useful to carry out a research project in which a certain number of police crash reports were followed up by reference to other sources of injury information such that one would not only know the police injury rating (KABCO scale), but one would also know the injuries to that same person in terms of medical diagnosis, nature of treatment, cost of treatment, etc. It would then be possible to set up a double analysis -- an independent variable versus two dependent variables.

One could then analyze some sort of intervention, such as presence or absence of head restraint, or the presence or absence of a child restraint seat. One could then analyze the data to ask whether intervention was detected or quantified better with the one dependent variable or the other. If one were lucky, it might turn out that the KABCO scale is sensitive enough to detect the effects of most major interventions. For example, one might find that if the injury reducing benefit of a particular device appears to be of the order of 30 percent with respect to some sophisticated injury scale, that it also might be to be about 30 percent with the KABCO scale.

On the other hand, if one is not lucky then there would be significant disagreements between the two injury scales when analyzed against the same intervention. This kind of information is needed because even though more sophisticated injury scales are being developed for some specialized research, still the large bulk of injury characterizations relative to highway crashes are made by police. We need to know the strengths and limits of what we have so as to have a platform from which to try to move on to better injury data in the future.

Research 27. Research on Improved Injury Data

In view of the weakness of the KABCO scale I think it is worthwhile to explore ways of obtaining better injury data. I can think of two research approaches: One would be to bypass the officer altogether and obtain injury information directly from the hospital. The other would be to improve injury reporting by officer's themselves.

As to the first approach, one might visualize a situation in which the officer at the scene identifies the vehicles, the time and place of the crash, where the persons were seated and their identity. Then, at the hospital, the victims would be identified as to time, day and place of treatment, their names would be collected, and some sort of "origin code" would give an idea of the event that caused the trauma. Then, through a central processing system, it might be possible to link the two files. Since these two systems were designed for totally different purposes it seems unlikely that data linking could be done without some procedural changes made on both sides, probably involving considerable research.

It should be instructive to understand the process and outcome of the effort in New Zealand to link the respective national data files on hospital-treated injury and highway crash data. Likewise, an NHTSA effort is underway in the US, on a trial basis in the State of Maryland, to link the police-generated crash report with hospital data describing the injuries.

One of the many problems associated with this kind of undertaking is the linking of public data files versus those that are confidential. In North Carolina, for example, most records of the Division of Motor Vehicles are public records under the governing statutes, and this includes the officer's crash report form. On the other hand, hospital data on injuries are confidential, and the hospitals often must obtain patient consent before they release information. This poses a formidable problem in obtaining permission to link such data bases, and poses a question as to the status of resulting records. Does the public nature of some of the contents or the private nature of the remainder hold sway in defining the legal status of the resulting record?

The second approach to enriching injury data would still rely on police-reported data but would change the concept by which the police records said data. Under this procedure, the officer would more nearly *describe* the injuries he sees rather than *rate* their severity. When an officer is asked to rate injury severity (as in KABCO), the implicit assumption is that he has observed the injury and has a knowledge base allowing him to categorize one injury as being more severe than another.

There is perhaps a reporting scheme that could rely less heavily on this inferential process. I visualize an accident form

that depicts mannequins with a three-quarter forward and rearward view with a grid superimposed. (By having two three quarter perspective views, one is able to represent all the body surface. In contrast, for a straight-on view, there must be a front, back, and one or perhaps two side views.) The officer would mark the area of principal injury and then use words he deemed appropriate to describe the injury. (Training and information newsletters would help to reduce the variation among officers as to their means of characterizing the injuries).

Coding could be done at headquarters, and with the superimposed grid the coordinate "address" of the injury could be entered into the computer. The coder could also type in, *verbatim*, the words the officer used to describe the injury. These words and this body area location could be processed relative to a computer algorithm programmed into the computer which would assign a candidate severity code. The coder could read the candidate code and agree or change it. The few coders at headquarters could be trained more readily and more comprehensively than could the entire reporting police force. Also quality control would be more feasible.

Obviously, research would have to be done to derive the severity code which would be a joint weighting of the body area location and the nature of the injury. However, with the ability for computers to handle words and with the reference to a "dictionary," it might be possible to come up with an algorithm to recognize and classify a goodly percent of the officers' injury reports. Considerable research in this area would be required. Validation of this procedure could be accomplished by obtaining injury descriptions of the same data set from medical professionals. Then, analysis could be done to ascertain the words officers use to describe the injuries in questions. This phase of the research would also help to define training requirements for officers.

Research 28. Exposure Control

A. Introduction

Gains in road safety might be accomplished by shaping public policy in a way that actually reduces the amount of travel, or that substitutes safer modes of travel for less safe modes. This approach to road safety has not been widely employed up to now. Our highway system has developed in the context of a relatively free market characterized by plentiful, cheap fuel and a *laissez faire* public policy regarding highway transportation development.

This has led to dominance by the highway transportation sector because of the great flexibility of travel thus given to individual, business, and government interests. Indeed, highway transportation apparently has a high priority world-wide. Many nations seem to be emphasizing individual transportation as the key to their economic and social development.

Despite the undeniable appeal and the obvious benefits of free access to highway transportation, exposure control deserves to be considered for the mid-term future because sheer growth may push the death toll to increasingly unacceptable levels even in the face of major safety initiatives already in place. The death rate has reached an all time low of about 2.5 fatalities per 100 million driving miles. Even if that rate holds until the year 2010, the annual fatality toll will reach 80,000 assuming a four percent annual growth in travel -- a growth level that has been realized in the past.

To hold total fatalities at their present level given that growth rate, it will be necessary to reduce the USA mileage death rate to about 1.5. This would mean a reduction of 40% in the present rate, and would mean achieving a rate for the whole nation that has so far been experienced only on freeway systems with superior design. It is not by any means certain that a gain of this magnitude will be possible through continuing only those approaches that seek to lower the death rate per mile exposure. Thus, it may be increasingly necessary to consider unprecedented approaches to exposure control. However, extensive reliance on exposure control sets up a major conflict with other important values in our society, touching on such basic issues as the ready ability to choose where to work and live, the very lay-out of cities, and the freedom of movement considered a keystone of our political system. Clearly a great deal of public policy research would have to be done to point the way for consideration of exposure limiting options.

B. Vehicle restrictions

One way of gaining the safety benefits of exposure control is through vehicle regulation, and such regulation can be implemented in a variety of ways. A rather extreme case is the direct control of vehicle sales. In one large province in China, officials are convinced that motorcycles are a safety problem, and have restricted sales and licensing of motorcycles to 1,000 per year. However, in the US it is more probable that tax policy could be used than outright control of sales, and an application of tax policy was discussed elsewhere herein.

C. Roadway restrictions

Exposure control through regulation of roadway use is a common practice. Already many countries do not allow pedestrians or bicyclists on high speed freeways. Also, it is not uncommon to restrict truck traffic from residential streets except for purposes of a specific delivery. Further, trucks are sometimes forbidden in central business districts during daylight hours. Some of these restrictions are primarily for congestion control, but they also have safety implications.

D. User restrictions

There are a number of ways in which exposure control can be achieved through regulation of road users. Perhaps the most common method is the control of the age at which user licenses are granted. Since there is essentially universal data showing that crash rates are much higher for young drivers, it follows that control of licensing age is relevant to safety. If other considerations allow it, the later the age of licensing, the greater the gain in safety.

The driver licensing process itself could be set up such as to introduce the driver more gradually to the roadway environment. There could be a multi-step process -- a graduated license -- in which, for the first period of time, the driver would be accompanied by another person. Next, the driver could be allowed to drive alone but be restricted to daylight hours, since night driving constitutes a higher risk. Finally, the person could drive in the full range of situations.

A concept similar to regulation of age of licensure is regulation of the age at which alcohol can be purchased. Recent experiments with raising the "drinking" age have produced gains in road safety, since young persons are both inexperienced drivers and inexperienced drinkers. Another common exposure control method is the setting of high motor vehicle insurance premiums for drivers who have manifested a bad driving record.

Finally, curfew policies exist in many places around the world, applied in a variety of ways. In a few towns in the USA local authorities have imposed night-time curfews on young people. Though such curfews are imposed for multiple and frequently controversial reasons, one of their main benefits is their crash prevention value.

E. Communication as an alternative

Improved methods of electronic communication might constitute a realistic alternative for some forms of transportation now being used as a means to achieve communication. One such category is travel related to business. For some years now the idea of teleconferencing has been making steady gains, though the costs are as high or higher than the comparable travel costs. Also it is apparently conceded that there is little evidence that teleconferencing has, in fact, replaced travel to any measurable amount. Nonetheless, it is well that trends in this area be monitored from time to time to see if there are developments that may become significant.

Another aspect of communication in lieu of travel is the notion that increasing numbers of people will work in their homes using computer terminals. This is an idea so futuristic in nature as to have much less immediacy than some other items. However,

one purpose of research is to spotlight the future; therefore some analysis and monitoring of this trend may be useful.

Research 29. Tax Policy Research

The following is a discussion of research that might shift public policy regarding highway safety more in the direction of using tax policy as a way of achieving highway safety. This may be outside of mission of FHWA or even the Department of Transportation. It may be that this sort of research would have to be funded by a foundation or the like. However, the research I propose can be illustrated in two areas: motorcycle safety and drunk driving.

In the area of motorcycle safety, it would appear that motorcycles moreso than other vehicles or road users do not generate enough revenue to offset the societal costs they impose. Tax revenues and user fees generated by the lifetime contribution of motorcycles may fall short of the societal costs for motorcycle crashes including the long-term rehabilitation of people permanently injured. That same situation may be true of other vehicle classes, but is exaggerated in the case of motorcycles because the casualty rate for this vehicle is considerably higher per mile driven than for other vehicles.

One wonders whether motorcycles, in that financial light, constitute a "good deal" for society. I do not know exactly what that balance sheet would look like and, that is part of the policy research I propose. I also propose research proceeding the next step -- some public policy research to look into the feasibility of a tax structure on motorcycles such as to generate revenue and possibly ways to dedicate that revenue to rehabilitation and treatment programs. An outcome of that research might be recommendations that motorcycle users more nearly pay their way (or perhaps to reveal that they already do).

The research could include an analysis of whether tax policy would discourage the use of motorcycles and thus perhaps force some motorcycle users into other, presumably safer, forms of transportation. An adjunct of this undertaking would be further crash research into the evidence regarding the new breed of extremely high-powered motorcycles, almost designed for racing competition, but ridden on the streets. Some of these motorcycles, manufactured in Japan, are sold widely in the U.S. As I understand it, the Japanese do not allow them to be sold in their own country.

I am not necessarily suggesting that the use of tax policy is appropriate in the regulation of the entire array of transportation options, but I think it is worth considering such research for this particular vehicle since it is one of the most dangerous vehicles on the highway when the casualties are measured in terms of the revenue they produce and the exposure on which this toll is based.

A second area for research along this line is in the area of alcohol, and particularly beer consumption. Beer drunkenness is said to be the most common form of drunk driving in the U.S. Beer is sold and dispensed widely and there are many brands. It is not uncommon to find that the pricing of a six pack of beer is less than that of a six pack of soft drinks. Various brands of beer have some variation in the percent of alcohol. Some research would be appropriate to see if there is any way of getting at the relationship between the level of alcohol in beer and the relationship to crashes. For example, if one person spends an evening drinking 3% beer and another person spends an evening drinking 6% beer, will the latter person have more alcohol in the blood? Or will the alcohol be relatively constant because the former person consumes twice as many beers with all the logistical aspects implied -- higher cost, longer time, more fluid for the body to process, etc.

The question would then be whether a lower percent of alcohol in beer, forced by some legislative or tax consideration, would indeed reduce the amount of drunkenness. Research could be done and, in fact, some has been done on the question of the behavior of people in a controlled setting when they have access to one or the other kind of beer. Research could be done in which unlabeled beers at two levels of alcohol could be served to learn whether there is a preference for high alcohol beer.

The research could include a survey of beer manufacturers to ascertain whether there are incentives to make higher alcohol beer versus lower alcohol beer. Then there would be the public policy issue of whether it is useful for society to opt for sale of beer with a lower percent alcohol.

Research 30. Research on the Value of Time in Small Packets

Many of the benefits currently ascribed to highway design changes come from benefits in terms of time savings and in many cases these time savings are quite small units of time savings, enjoyed by large numbers of people. Thus, if signal phasing can save fifteen seconds for each of 10,000 motorists per day, simple arithmetic is done to calculate the benefit of 7.6 man years saved per year. Benefits are then ascribed in terms of the value of that many man years of time.

On the other hand it seems to this writer that a legitimate philosophical question can be raised as to whether such an approach is legitimate. One needs to consider the value of time in small packets. Thus, if a person's time is valued at $70 per day, does that mean that an hour's worth of that person's time is worth $8.50. If so, does that mean that one minute of that person's time is worth 14 cents. Does that really mean that ten seconds of that person's time is worth literally, two cents? Previous research has already indicated that people do not ascribe a

pro rata value to time when it is in quite small packets.

The manner of ascribing value to time saved becomes crucial when the time benefits are weighed against societal costs in terms of death and injury. For example, one might ask whether it is a good for society to have a seat belt law wherein several tens of millions of people have to buckle up in order to save one life?"

Let us suppose, for example, that about 75 to 100 million bucklings or unbucklings of belts were required to save one life. The lost time associated with the fatality can easily be calculated. On the average, each motor vehicle fatality represents a loss of about 40 years of life (crashes primarily claim young people). On the other hand, the 75 to 100 million "clicks" of buckling and unbuckling is distributed over literally millions of people and occurs in small repetitive packets of between five and fifteen seconds.

Is it a good bargain for society to ask 90 million people to spend fifteen seconds buckling up so that one person can avoid spending 40 years being dead. What are the implications for cost and benefits if those two time values turn out to be approximately the same? Suppose it requires about 40 man years of buckling and unbuckling (distributed into millions of small packets of time on a population wide basis) in order to save one person from being continuously dead for 40 years. That trade certainly sounds to me like a bargain for society and it seems to me that those two concepts of time are rather different.

Another example is the debate that, in fact, took place before the recent change from a 55mph sped limit to the 655 mph limit. In this context it was easy to calculate the time savings of a few minutes here and there that would result from the increased speed limit. By summing these small individual savings it was possible to show that the societal savings from faster travel were considerable indeed. However, it was also shown that that an equal time was estimated to be lost through premature death. The real issue is how to weigh small individual savings enjoyed by a large number of people versus large individual losses through death or disability suffered by a comparatively few people. Thus, when the benefit of highway measures is in terms of reduced death and injury and the cost is in terms of increased time, it seems to me that the traditional way of calculating time benefits should be subject to reconsideration, and should be examined through a research program. Perhaps we drastically over value the savings of small amounts of time by a large number of people.

Research 31. Standards Rationalization

In a recent paper, Professor Ezra Hauer lamented the lack of a rational basis for the highway design standards that are central to safety. As an example, he traced sight distance and curve requirements and pointed out that there is no empirical

demonstration that such standards values are related to safety, or, if related to safety, that the level at which the standard is set, is the "right one."

Such standards too often represent crude ideas for quantifying a desired end-goal, and too often were set up at a relatively early stage in the development of knowledge. No one really intended that the early, crude standards were to remain in effect indefinitely, but once in place the standards take on a life of their own. Indeed, there quickly come to be barriers to changes because a way of doing business between the government and the private sector develops around the process building products to meet these standards and the certification thereof.

However, I think it worth considering that research be done in the area of standards rationalization. I am not qualified to set forth the details of the needed research (Professor Hauer would be such a person), but it does seem to me that, we should include research to examine the basis for the highway design standards. We should examine the safety implications of the standards themselves and ask whether, in view of "tomorrow's traffic," these standards are right.

Research 32. Better Research Training for Highway Engineers

A certain portion of transportation engineering graduates end up having to do "research" at some point in their careers. This is true of transportation engineers that wind up in city traffic engineering offices, in state traffic engineering offices, at universities, at consulting firms, etc. One has only to observe the continuing use of simple (and flawed) before and after studies, and the continuing ignorance of regression to the mean to realize that many highway engineers graduate with little understanding of research design.

Poor research continues to plague this field in both a tactical and strategic sense. On the tactical level, it simply means that bad decisions are made with an unknown frequency. On the strategic level poor research influences the way the society regards the field of highway safety. There is not perceived to be a scientific, professional research community dealing with highway safety as there is in the case of other major health problems society faces. The lack of a perceived look of professionalism may be one of the barriers to increasing funding for highway safety research.

It seems to me that a research undertaking would be useful in which an appraisal is made of the curriculum of engineering schools with respect to research design and statistical analysis, and an appraisal of what would be the minimum course requirements necessary to provide such training. This sort of inquiry would be fairly threatening to Universities because it would deal with the adequacy of higher education in this particular respect. Thus,

the research should be done by academic types and within the framework of self-examination by universities.

If it is concluded that this cannot be done within an undergraduate engineering curriculum, then the research should consider when and how it *should* be done.

F. CONCLUSION

As a concluding thought, I would say that as I have worked in highway safety research over the years, I have had increasingly strong feelings that so much of our research policy is driven by the penny-ante research environment in which this field has always operated. Too often, for lack of money, we have not addressed important research problems at all, or we have not replicated research, or we have been unable to follow up research findings.

The need is not merely to define the next round of research for the country, but to address the fact that highway safety progress has been captive to and has been sharply limited by the strictures of funding levels that are at least an order of magnitude too small considering the size of the national health problem that highway crashes constitute.