5 *BEHAVIORAL RESEARCH ON ACCIDENTS*

THE READINGS:

BEFORE WE EMBARK upon the first of four chapters dealing with behavioral approaches to accident prevention, it would be well to attempt a clarification of the role of behavior in accidents, a role that is often as ambiguous to the research worker as it is to the general public.

In the compiling of accident data it has become almost traditional to use a classification system which dichotomizes accidents due to human behavior and those in which human behavior apparently played no part. Thus an accident attributed to "driver error" is regarded as clearly due to human behavior, whereas an accident resulting from the failure of a mechanism—the tire of an automobile, the fuel system of an aircraft, an automatic alarm system—is classified as having no human cause.

Such a scheme is misleading for several reasons. First, it implies that enough is known about accident causation to permit accurate attribution to human or nonhuman factors. In the present state of our knowledge such an implication is not warranted. Secondly, what we do know about accident causation leads to the conclusion that behavioral elements, far from being discretely present or absent, are present to a greater or smaller extent in virtually every accident. Even the most obvious case of "driver error" may involve a nonhuman environmental situation in which "error" was greatly favored if not, indeed, inevitable. Similarly, even an accident that seems due to a mechanical failure[1] involves the human element of failure to inspect the device adequately or to provide it with fail-safe features,* just as an accident due to a hurricane or other "act of God" involves the behavioral element of the victims, who failed to predict the occurrence of the damaging winds or to remove or otherwise protect themselves from the effects.

Even when the magnitude of the human element in a certain kind of accident can be accurately assessed, it is important to note that its sheer magnitude does not inevitably make it the most appropriate target for preventive measures. The amputations and other injuries common in industry during the late nineteenth century, for example, could be clearly attributed to "human error," but the most effective countermeasure-proved to be not a program to change human behavior—that is, to make the worker "more careful"—but rather the installation of machine guards which made the human error noninjurious to the person involved. On the other hand, such largely "nonhuman" kinds of accidents as those due to the failure of electric wiring, boilers, and elevators, and to spontaneous combustion have been effectively reduced by altering human behavior—through legislation requiring building codes, periodic inspection, and the provision of police and fire department personnel to prevent or reduce damage and injury.

Yet another difficulty stems from the use, in the literature, of the term "human factors" to cover a wide range of meanings. (See McFarland, Chap. 2.)[2-6] Vascular accidents, the physical dimensions of the human body, reaction time, color blindness, drunkenness, attitudes toward the police, emotional disturbances—in short, a wide variety of elements, modifiable and nonmodifiable, close to and remote from accidents—have at one time or another been labeled as "human factors." The ambiguity of this term can be a major obstacle, both to the research worker who uses it loosely and to the reader seeking to identify the objectives, the point of view, and the purposes of the studies in which it is thus used.

* See reference 5 for a discussion of fail-safe devices.

Within the limits delineated by the foregoing caveats, however, there exists a vast array of accident phenomena that lend themselves to study by behavioral scientists from various disciplines. The psychologist may examine the personalities, perceptions, and response characteristics of those involved in accidents; the sociologist, the characteristics of social groups which influence exposure to danger; the anthropologist, cultural values concerning safety; the economist, the relative costs of safe and less safe industrial processes; the historian, the development of safety legislation and other safeguards; and the political scientist, the political forces that influence safety legislation. Accident research thus oriented has not yet made great advances, but the causes and results of many types of accidents can be studied as productively from the behavioral-sciences point of view as other aspects of human experience.

Because of the several disciplines and numerous approaches represented in the behavioral sciences, we have devoted four chapters to this aspect of accident research, and additional examples are included in Chapter 3. The present chapter begins with excerpts from four papers that reflect the general emphasis and present level of development of various behavioral-science approaches to accident research. These indicate some of the current theoretical and methodological problems and provide background for the selections in the next three chapters.

SOME COMMENTS ON ACCIDENT RESEARCH

—Anatol Rapoport, Ph.D.

Although it does not present a systematic analysis of behavioral approaches, this paper is a refreshing departure from most of the behaviorally oriented accident literature by virtue of its sophistication and its wide-ranging concern with aspects of accident research and causation (such as the role of cultural factors) which have received insufficient attention from behavioral scientists. It is especially noteworthy for its avoidance of the narrow and exclusive emphasis on personality factors, which has long dominated the accident literature. Although concerned primarily with childhood accidents, its points and emphasis are of general relevance.

*JUST AS THE STUDY of childhood diseases can*not be meaningfully separated from general problems of health and disease, so the study of childhood accidents must be related to a general investigation of accident phenomena.

The big question is where, to begin, because the different backgrounds and predilections of people interested in accidents make themselves strongly felt. Fortunately, research workers have become sophisticated enough not to permit their special interests to distort their views. Most dedicated workers have come to realize that the approach which they see most clearly because of their special interest or special training is only one of several and that ideally the several approaches should complement one another instead of struggling for primacy in matters of attracting researchers and public support and implementing proposed corrective measures.

However, a proper integration of effort is easier asked for than accomplished. Even aside from competition for effort, even if

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funds and manpower were unlimited, we would still be divided by differences of opinion about what to do first, because of differing notions of what constitutes a logical sequence. The situation is the same in any large-scale planned activity. For instance, when a society is industrialized, a certain rhythm must be preserved in expansion of capacity, expansion of market, training of cadres, and changes in patterns of living. When resources are as strictly limited as they are in research and action in the field of accident phenomena, organization of effort becomes a crucial task if any sort of effectiveness is to be expected.

The question, then, is where to begin. There are those who will not feel comfortable unless they have precisely defined the problem area. Thus we have energetic searches for a definition of "accident." The various points of view from which "an accident" can be defined have been ably summarized by Dr. Suchman. I think it should be granted that in some problem areas precision of definition is prerequisite to the effectiveness of measures undertaken to solve the problems. Thus, in medicine, nosology is of prime importance, and the reason is not far to seek. Most spectacular advances in medicine crystallized around the discovery of "specifics"—as in chemotherapy and in the discovery of the importance of vitamins and trace elements in diets. Here, of course, precise classification and identification of diseases were of supreme importance to the extent that very fine distinctions had to be introduced before progress could be made. Obviously the very nature of the specific requires that the situation in which it applies be specifically recognized.

Nevertheless it should be borne in mind that equally dramatic advances in man's war against disease were made with the discovery of the importance of nonspecific factors—*e.g.*, public hygiene and general levels of nutrition. In this connection it should be emphasized that factors not directly connected with medicine or even with the general problems of health and disease played an equal or a more important role in improving the general level of man's health and longevity. It is possible to argue that Euclid may have contributed more to medicine than all the medical discoveries from Hippocrates to Pasteur. For Euclid systematized geometry; systematized geometry made possible the science of optics; optics gave us the microscope; and the microscope led to the discovery of microorganisms and eventually to the first major breakthrough in medicine and hygiene.

We have, then, in medicine, two types of advances: dramatic discovery of specifics and a gradual, rather uneven amelioration of *conditions* in which man can safeguard his health and can come to grips with specific problems of health and disease. And while we speak of "conditions," we should certainly not forget the general philosophic orientation, the world view, the mood in which man faces the problems confronting him. In this respect the activist mood, characterizing Western society, was certainly more conducive to medical advances than the fatalist mood; naturalistic philosophy was more favorable than an animistic one, and so forth.

In dealing with accidents we can also distinguish between specific and global approaches. The discovery and removal of a structural defect in an airplane, improvements in road engineering, industrial safety devices, etc. are all obvious examples of specific measures. Their importance is by no means to be discounted, especially since the "yield" of these measures in terms of lives saved, even if small, is usually clearly demonstrable. The situations allow for "before and after" tabulations with the consequent identification of "causes." This specificity makes the fight against the accident clear and attractive. I venture to say that it taps a particularly regrettable but on occasions useful facet of human motivation—namely, the identification of a "culprit." I shall have more to say on this subject a little later.

The discovery and implementation of "global" measures analogous to public hygiene or to cultural attitudes is, in the prob

lem area of accidents as elsewhere, a much more difficult matter. Here precise definitions will not help us, because we are not dealing with a search for precise measures. To resort once more to the analogy with medicine, the value of eliminating superstition, for example, although it was probably very great in improving public hygiene, could never be ascertained exactly. We still do not know how to measure the "amount" of residual superstition or misconceptions in a population or to correlate this amount with the level of attainable hygiene. Results in such an area, then, are not bound to be dramatic, nor even precisely demonstrable. Yet their importance would be difficult to deny. I, for one, am firmly convinced that our cultural climate, our aspirations and myths, find definite reflection in the prevailing attitude toward the automobile and that these contribute in no small degree to a basic accident rate which would remain, even if all the specific measures of accident prevention were realized. This idea lurks in the paper by McFarland and Moore, *Youth and the Automobile*. Other factors contributing to accident rates, as officially recorded, factors having to do with deep-seated attitudes rather than with explicit exercise of safety measures, were soberly discussed by Dr. Foote. These matters are difficult to demonstrate, and even if some of them could be proved, we would be at a loss as to what to do about them now.

We have, then, these two extremes. On the one hand, there are the demonstrable specific accident-conducive situations. The only problem here is to induce people to recognize the situation and to do something about it. This is sometimes formidably difficult, as Dr. Freedman has shown so convincingly in his discussion of a specific type of accident—namely, lead poisoning in children eating paint in poor homes. But at least we know where to direct our efforts when we face a situation of this kind. At times such efforts are rewarded, particularly when the removal of the situation depends on people who can be made targets of campaigns: landlords, industry, common carriers, government, etc. Witness the speed with which action was taken in the polio vaccine deaths of a few years ago.

At the other extreme, we have diffuse factors, difficult to identify and even more difficult to demonstrate as relevant, but which may nevertheless be of great importance. I shall return to these factors later, but first I would like to ask this question: Do we have intermediate situations? In other words, do we have factors of importance in the area of accident phenomena which could in principle be demonstrated but which have not yet been demonstrated? In the light of the history of medicine, which, I agree with several of the participants, is entirely relevant to our topic of discussion, the answer should certainly be "yes." In fact the history of medical discovery is overwhelmingly a record of identifying factors specifically relevant to health or disease which had not been previously recognized. What had been a vague notion that low, marshy places are conducive to malaria became very specific knowledge about just what it was about these places that gave man malaria—namely, not *mal aria* (bad air) but certain definite species of mosquito, and not even these directly, but rather the protozoa parasitic on the insect.

It is axiomatic that scientific research is the discovery of causal chains, the establishment of the form "*If* so, *then* so." Now we have been amply warned, and rightly so, against the facile singling out of causes. We are shown innumerable instances of how the same alleged cause produces different effects in different contexts. However, illegitimate extrapolations of the causality principle and unwarranted simplifications do not by their failure invalidate the principle. Is not the whole notion of controlled experiment a successful circumvention of this pitfall? Wherever it is possible to reproduce essentially identical conditions except for the value of one independent variable, the "effect" of that variable on some other variable of interest can always be studied. This is not, of course, falling into

the trap of single-cause single-effect fallacy. The end result of a controlled experiment is not the answer to a question of the form "What causes X?" but rather to a question of the form "To what extent does the variation of Y, other things being equal, effect variations in X?"

Now this is known as the analytic method, time-honored in the classical physical sciences of the nineteenth century. There has been a reaction in twentieth-century philosophy against this method of inquiry. We are told that "the whole is greater than the sum of its parts" and that it is a mistake to regard the entire situation as additively composed of its components, that the entire situation has emergent properties of its own which we cannot discern as long as we contemplate only the parts, and so on. I suppose the position of Kurt Lewin must be so understood.

Now, I usually lend a sympathetic ear to the holists' objections to the analytic approach. Indeed, some entirely convincing examples can be given. Our music has a vocabulary of 12 tones—call it 72 tones if we multiply by the six-octave range of the orchestra. It is ludicrous to insist that a Beethoven symphony is essentially represented by such and such frequency of occurrence of each of these 72 tones. We know, of course, that wholes are composed of parts *arranged in patterns*. Analysis presumably destroys the pattern and is therefore irrelevant in revealing the nature of the "whole."

This criticism is valid, I believe, when it is applied to attempts to explain perception or integrated action in terms of isolated units, be they stimuli or muscular actions. These attempts fail because they ignore patterns which mold the parts into the whole. I have serious doubts, however, that this criticism is relevant when applied indiscriminately against analysis of complex situations just because the situations are complex. Analytic methods can be much misused. But this does not mean that they cannot be used to advantage if used properly.

When used properly, the analytic method does not presuppose that all causative factors are linear or that they are additive. Naive elementalism is not inherent in the analytic approach. Admittedly the analytic approach emphasizes the study of one component of the situation at a time. How the components are to be put into the whole picture is a separate problem, but the two problems should not be confused with each other.

In a controlled experiment, components are studied one at a time by a deliberate structuring of the experimental situations. The handicap under which social science must operate is the well-known fact that experiments involving the society *in vivo* are often impossible for practical or ethical reasons. The social scientist, then, must select from naturally occurring situations those aspects which best approximate controlled experiment. This is possible wherever the gross situation is composed of so many minute components that the law of large numbers has a chance to operate.

The law of large numbers is to social science what deterministic causality is to physical science. Indeed, many of the so-called deterministic laws of physical science have been definitely shown to be simply instances of statistical regularity resulting from the operation of the law of large numbers. Although the numbers involved in man's behavior are, of course, nowhere near the magnitude of numbers of molecular events involved, say, in chemical reactions, nevertheless the regularities of accident statistics are respectable, to the extent that we can speak of an operation of quasi-causal relations.

We are, then, in principle, enabled to single out differential causative factors of accidents. To do so we must select for comparative study classes of events with as much care as a sophisticated experimenter controls the conditions in his laboratory. Accidents are, to be sure, rare events in the life of a single individual. But there are so many of us that the absolute number of accidents is prodigious. The 100,000 fatalities alone are sufficient to provide statistically respectable samples for comparing

populations of events statistically identical with one another except for a selected factor treated as an independent variable. In this way a first approximation tabulation could be made of what could be expected (on the basis of a naive additive model) if the factor in question were to be changed alone in a given direction.

Of course, several such tabulations already exist. However, they are only a first step. Two other things are to be ascertained in connection with any attempt to induce action toward the reduction of accidents. First the marginal costs in appropriate units of each type of change must be ascertained. For example, it may be found on theoretical grounds from the analysis above that a reduction of speed limits on certain types of highways by five miles per hour can be expected to save so many lives, prevent so many injuries, and save so many dollars. This finding is not sufficient. One must also estimate the effort which can be expected to succeed in actually effecting the reduction of speed. Only after comparing the expected "yield" of effort in various directions would one have an idea of whether it is feasible to put out effort in a particular direction. Already on the basis of present data, one could make estimates of what one can reasonably expect from different types of ordinances, restrictions on driving licenses, campaigns for more safety in playgrounds, etc., etc. One could sharpen these estimates by more careful analysis and extend their range. Then one would have a basis for projected action.

But only a basis. This brings me to the second qualification of the analytic method. The theoretical marginal costs do not necessarily ensure that the actual costs will stay within the limits or that the expected marginal effects will be realized. For we must remember that our model of *additive* causative factors was only a first approximation. In practice the factors may be intimately intertwined. A change in one may bring unavoidable changes in others, sometimes counteracting the first changes. It is well known, for example, that safer highways tend to bring about faster driving and perhaps greater risk-taking.

When it comes to these interaction effects, our simple analytic approach may be revealed to be inadequate. It is then that the need for more holistic approaches may come to be felt. It may very well be, for example (although I have not at present the slightest evidence to support this conjecture), that accidents are indeed necessary consequences of aggressive tendencies in the population at large. What some pre-industrial cultures achieve by the ritual of blood sacrifice, including human, we may be "achieving" by our yearly slaughter of innocents. We would shrink from the idea of drawing lots to decide which 100,000 men, women, and children were to be killed each year. But in effect, we are essentially doing just that, and our explanations of what actually occurs—*i.e.*, the notion of "accident," where the events are attributed to the will of chance (just as in other cultures blood sacrifices are rationalized as pleasing to the gods)—could well be mere rationalizations.

Stated so blandly, the idea smacks of mysticism or of misanthropic defeatism, and I do not intend for a moment to ask you to take it seriously. But there is only one way to test the hypothesis to the effect that we cannot do anything about our accident fatalities (*i.e.*, that these events are inherent in the very fabric of our culture). That is to undertake an analysis of contributory factors, an estimate of the marginal effectiveness of various courses of action and of the associated marginal costs, and to see whether we *can* do anything about them.

At the extremes, there is no question about the answers. We could abolish automobile deaths by banning automobiles and accidental deaths from firearms by banning privately owned firearms. It is usually argued that the banning of automobiles would result in such crippling dislocations in our lives that the costs would be absolutely prohibitive even if the ban could be put into effect. The argument is certainly reasonable. But it does not apply to firearms. No great dislocations in our economy would result

from an absolute prohibition of firearms in private hands. The cost in terms of losses of opportunities for self-defense would be trivial: cases in which a person saved his own or someone else's life because he had a gun are, I submit, exceedingly rare. The only genuine costs involved would be the loss of opportunities for hunting and the costs of enforcement. The former could be circumvented by allowing hunters to rent hunting weapons and thus to keep the slaughter of humans by firearms almost entirely within their own ranks.

Let us further grant that there are limits on enforcement. Certain sectors of the population in the underworld or on its fringes would probably keep firearms even if it were against the law. This means we can expect deaths from firearms which result from crimes of greed and passion to continue, perhaps undiminished. But an enforcement of a law against private possession of firearms would probably be quite effective among ordinary, law-abiding citizens, those who keep revolvers in drawers against burglars, and this would mean that fatal accidents involving firearms would be drastically reduced. In particular most of the 500 children's lives lost annually as a result of shootings would be saved. The question to be put, then, is a straightforward one: does our society consider it a good bargain to trade the privilege of owning private firearms for 500 children's lives per year? The answer to such a question can be interpreted only from what society actually does in this and in similar matters, not from what people *say* ought to be done.

The importance of such questions is that they point up peculiar attitudes of a society toward the value of human life. It is well known that the worth of a human life as reflected in what society will undertake to do to save a life varies over a tremendous range. In fact, one could almost assert that the marginal value of a human life is inversely related to the rate at which lives are lost in given situations. The callousness of the public toward automobile fatalities compared with its responsiveness to dramas involving very rare diseases is a well-known illustration of this principle. But of course much more is involved. Rare events are much more widely publicized. For this reason, there is much more apprehension about plane travel than about automobile travel, although the former is considerably safer than the latter, statistically speaking. Also, efforts to save lives apparently doomed are usually of the utmost energy and ingenuity. Witness the rescue activity following a mine accident or even in connection with attempts to save a trapped animal. Doubtless it is drama that increases the popular valuation of life. When loss of life is routine, we witness a callousness which we deplore when we see it in other cultures in other contexts.

However, perhaps the most important reason differentiating the amount of effort which will be expended in saving lives is the lack of comprehension in the general population of the role that the probability of an event ought to play on the scale of social value. The predominant way of thinking is in terms of individual experience. An event happens or does not happen *to me*. I cannot experience (except in the vaguest way) the probability of an event. Now, on the social scale, global action aimed at reducing accidents is action aimed at reducing the probability of accident for an individual. This probability is not felt when it is reduced. That is, it is not felt in the same way that explicit events are directly experienced. To some extent on the grossest level, we do perceive the difference between clearly dangerous situations (*i.e.*, those in which probabilities of injury or death are high) and clearly safe situations (in which the probabilities are low). But we do not feel anything personally if the accident fatality rate is reduced from one per hundred million passenger miles to one per two hundred million passenger miles or, on the contrary, raised by a comparable ratio.

I think that this absence of a direct experience of probability (except among professional gamblers) is primarily responsible for the gross inconsistencies in the value placed upon life in various situations. When

some miners are trapped in a mine, everything humanly possible is done to rescue them even if the chances are all but nil. This is because the misfortune has happened to *specific* people and hence our empathy can be enlisted directly. The potential victims exist for us as individuals. But when efforts are enlisted to save many more lives needlessly lost, even lives of children, on which supposedly an extremely high value is placed in our society, nothing can be done on nearly the same scale. The potential victims do not exist for us as individuals. The deaths and maimings which are spread as "probabilities" over the entire population, with only a dim shadow of danger falling on each real individual, are not seen as misfortunes happening to members of our society.

But there are still other factors operating in the determination of public attitudes toward preventable misfortunes. I have already mentioned the recent uproar over the polio vaccine deaths. Here, statistically speaking, the danger was infinitesimal, far smaller than the danger associated with the disease itself. Yet the public reaction reached almost panic proportions. This seems to belie the earlier statement that danger, manifested as a probability, is not clearly felt. We must now qualify this statement by noting that even very small probabilities are sometimes quite dramatically felt, provided one can focus on an agent who is assumed to have the power to remove the danger. Food and drugs are made by specific agents. These agents can be made to bear the brunt of anger if laxity in safeguarding the consumer can be shown. The situation is quite different in the case of diffuse, universal dangers, when there is no specific outside agent in whose power it is to reduce the danger. Indeed, the agent, if there is one, is ourselves. It is we who must change our ways to improve the situation. Accordingly much more sophistication, awareness, and social-mindedness are needed to bring about improvements in these areas.

To summarize, the degree to which action can be effective to combat accidents in general and, of course, accidents to children in particular depends on a variety of factors. We can utilize the knowledge of these factors in guiding any planned actions of this sort. It is easiest to instigate action against accidents under the following conditions:

1. The accidents are sufficiently rare as to be well publicized. This apparent negative correlation between public awareness and accident toll is, of course, most unfortunate. But it seems real and must be taken into account.
2. The action required is specific—*e.g.*, the removal of a drug from the market or of a type of airplane from scheduled flights.
3. The actor who is to take specific action is well identified and preferably one upon whom aggression can be vented and who is vulnerable to pressure.

All these are actions aimed at the discovery and the use of what are the analogues of "specifics" in medicine. In medicine, too, specifics were usually the easiest measures to introduce to the population. This is bound up with the relative ease with which direct, lineal cause-effect relations are understood and with the well-known fact that it is easier to insist that someone else, such as the physician, do something about the situation rather than oneself.

In the next category are actions against accidents whose causes and remedies are discoverable only statistically. Here the problem is first to ascertain the causes, which are by no means obvious. Research is difficult and expensive, and the results are often at variance with preconceived notions and therefore unconvincing to the public. It is hard to accept results which de-emphasize the role of a favorite scapegoat or point an accusing forefinger at established practices.

We all have our pet villains. For example, I would love it if so many accidental shootings and hangings of children per year could be definitely pinned on the children's television diet. Dr. MacIver, in his discussion of cultural factors making for what one might call "social accident-proneness," has pointed explicitly to this possibility. But whether this is actually so or not has nothing to do with

my desire to vent my own aggressions on what I consider the seedy aspects of our mores.

Here, then, is the most obvious and the most extensive but also the most difficult area of research. The first objective of such research is to establish criteria of theoretical marginal effectiveness of some proposed measures. This figure can only be theoretical for reasons which I have mentioned and will reiterate in a moment. Marginal effectiveness of a proposed change is a theoretical figure, because we do not know in advance the interrelation of the variable in question with other variables and the side costs of implementation programs. Suppose, for example, that in very careful studies, in which every attempt is made to control selected samples so that only one component to be studied is varied, say speed limit, it is established that a reduction of speed limit by five miles per hour ought to result in a saving of so many lives, limbs, and dollars. Assuming that the reduction can be carried out, it is by no means certain that such a prognosis will be vindicated, because the speed variable may be linked with another one, which we do not know about. Moreover, the implementation may have side effects which the original analysis did not reveal.

It seems, then, that a great deal of research is needed in the field of accident analysis and prevention before all-out action campaigns can be undertaken that do not stand the danger of fizzling out or backfiring or raising the public's immunity to exhortations. This research should be on two levels—namely, statistical analysis, to determine promising areas of pay-off, and on the level of action, to determine the costs and the actual consequences of making marginal changes.

Eventually certain specific courses of action, distinguished by large expected pay-offs and feasibility of implementation, will be singled out. At this point, the propagandist, the agitator, or—to use the more polite but somewhat repulsive term—the public relations man comes into his own.

What should he sell and how should he sell it? I know next to nothing about the techniques of manipulating public opinion, especially since my immediate, instinctive reaction to pressure campaigns is a shrinking reflex. Nevertheless, we must recruit the image peddlers to good causes, if only for the reason that this will to some extent drain them away from bad ones. Armed with reliable findings of carefully conducted research, the public relations man and the lobbyist, enlisted to the cause of combating accidents, can sell lives at market prices. "In order to save 100 lives which otherwise will certainly be lost in the following manner, here is a choice of things you can do." So a brochure can be worded, giving several alternative procedures, plans of action on community, state, or national levels which can be reasonably expected to save so many lives. These brochures can have specially displayed price lists on children's lives and limbs. Such an approach has the advantage of harmonizing culturally with our deep regard for free enterprise and consumer choice. It may also capitalize on the statistics and score-mindedness of our society. Pride of accomplishment is often manifested with us in quoting figures—incomes, heights of skyscrapers, Hooper ratings, popularity indices, etc. Possibly a portion of this preoccupation with quantitative indices can be harnessed to efforts to increase properly calculated and widely publicized safety ratings. Besides having a potential for instigating action, such campaigns may have a definite value for behavioral science research.

Once a "market" is established in which lives, children's lives in particular, can be bought at fairly stable prices, we can study the market behavior of our public with respect to buying lives at current prices. This behavior can then be considered as an objective index of how lives are valued in our society. I am sure that we will find some surprising differentials. Actions will be taken, I am sure, in some areas of combating accidents, and other areas will be grossly neglected. We can then have firsthand knowledge of where the implementation pay-offs are. This is market research, or call it motivation research. This is the study of public reactions with the express purpose of future manipu-

lation of such reactions. But I think it would be in a worthy cause.

I find it extremely regrettable to be obliged to end on a note which may be interpreted as one of pessimism. Nevertheless, there is one possibility for which we must be prepared. Perhaps if we admit this possibility in advance, we can do something about it in the long run. It may well happen that even the grandest and best-directed efforts to reduce accident mortality and morbidity will produce an extremely meager yield. This may well happen if the various statistically determined "causes" of accidents which we isolate in well-designed research studies turn out to be only pseudo-causes, like the surface environmental causes of some neuroses are sometimes said to be.

In other words, it *may* turn out that the analytic approach is largely futile, that the isolation of specific correlations in accident research is an entirely spurious matter. To illustrate what I mean, suppose a Martian who knows nothing about the game of bridge undertakes to investigate bridge-playing behavior and does so by studying ever so carefully and intensively the muscular motions of the players. He will get nowhere from our point of view, no matter how many stable correlations he establishes in his study. What we know as the game of bridge—that is, its essentials—cannot be put together from a description of muscle twitches and how they are put together. The rules of the game and its strategies are described in concepts which are in an entirely different universe of discourse from that in which physiological events are described. Suppose, now, that our bridge game is played with some gruesome rules—namely, the winners shoot the losers. Our Martian tries to understand this outcome by looking for a chain of events of which the outcome is a consequence. As long as he analyzes the bridge game in terms of muscle twitches, he will get nowhere. He will understand what is happening only if he understands the rules of the game itself and the peculiar rule governing the behavior of the winners.

The example is admittedly far-fetched, but an analogue in the accident situation is not to be discounted. In fact, I believe that this is what the holists and the depth psychologists and others who seek beyond the surface causations of events are trying to point to. In the extreme instance, an understanding of the physical chains of events which lead to accidents *may* not be very relevant if accidents are manifestations, as some suggest, of global cultural factors—that is, of social analogues of destructive drives. When stated on a priori grounds, this view seems unduly mystical. Therefore, I would like to see a de-emphasis of this view *for the time being*. But we may be forced to concede some validity to it as we gather more evidence.

For example, if we should find that in spite of clear determination of the marginal effectiveness of certain measures and the actual implementation of these measures with the resulting reduction of accident mortality and morbidity in one area, the *over-all* accident incidence of morbidity or mortality remains unaffected—*i.e.*, if stopping up one wound we only help to open another, as sometimes happens in superficial cures of conversion hysterias—then we will know that something more basic is wrong with us. We will then be forced to attack the problem on other levels. In a small way, we certainly have some evidence of the operation of deeper causes of accidents. I have already mentioned the well-known finding that improvement of road safety has gone hand in hand with an increase of road speeds which counteracts to some extent the reduction of accident mortality brought about by the improvement. This is an indication that the degree of risk-taking tends to be maintained at a certain level. In an extreme case, the risk-taking could even increase sufficiently to completely offset the safety improvement. In that case the fatality rate would be kept at a constant level and conjectures related to aggression, the death wish, etc. will have to be taken more seriously.

But I insist that such pessimistic hypotheses should be entertained only on a posteriori grounds. As Dr. Deutsch has pointed out, it is not parsimonious to see accident repeaters as victims of unconscious self-destructive drives. I submit it is also not