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ACCIDENT RESEARCH can be categorized in terms of the general types of data sources employed, regardless of the purposes for which they are used. This approach is particularly useful to the newcomer to the field, because many of the difficult problems in accident research relate to the obtaining of data. In this chapter we shall note some of the various sources of raw materials which may be employed and delineate the skills often used in connection with them. Additional examples will be given in subsequent sections, particularly in Chapter 9.

Aside from its subject matter, there is very little unique about accident research as a form of scientific activity. The same principles and methods apply as in other research areas. Scrupulous care must be exercised in the collection and analysis of data, and conclusions must be based on the careful and unbiased weighing of the best available evidence. There is, furthermore, no substantial evidence that accident research presents problems more difficult than those encountered in related areas of scientific concern, and statements that postulate such difficulty as an apology for work that lacks either rigor or an adequate conceptual basis should be discounted.

It is a very common shortcoming of accident research that the effort expended and the conclusions reached are not justified by the quality of the data employed. Although this has long been a problem, it has become increasingly serious during recent years with the introduction of modern methods of data processing. The availability of such methods, particularly as applied to motor vehicle accident reports, has caused many investigators to lose sight of the fact that no amount of efficient or refined processing can improve the quality of the raw material. Unless a clear-cut distinction is borne in mind between the collection of data and their use, sophisticated analyses are liable to be based on such poor material that the results are of unknown validity. Both this situation and its opposite—the poor use of good data—emphasize the principle that the quality of research cannot be superior to that of its weakest element.

The range of accident research includes purely theoretical work, laboratory and field experiments, and the observation of natural phenomena. It is impossible in a volume of this nature to consider thoroughly the methods of data collection of all of the pertinent disciplines. This chapter is intended, however, to provide a broad introduction to this aspect of the work, leaving for subsequent chapters examples of the many more subtle and complex problems with which the investigator must often be concerned. This chapter, moreover, is concerned only with examples of data sources employed in the counting of accidents and in the study of the factors that lead to their occurrence. This leaves for Chapter 9 the consideration of the somewhat parallel sources of data employed in the study of accidents themselves, and of the unexpected, injurious energy exchanges by which they are defined.

LABORATORY RESEARCH

Little competent accident research has been based on the use of laboratory subjects, and little work of this type is now under way. This is unfortunate, since it is usually only in the laboratory that it is possible to prepare and control the precise conditions under which observations are to be made. In addition, laboratory experimentation can often be designed to avoid the hazards to individuals that

are inherent in accident research of other types. With few exceptions, however, laboratory research has been limited to rather narrow studies in pharmacology, physiology, and experimental psychology. There has also been an increasing amount of laboratory investigation of the ways in which injury per se may be produced or prevented (see the discussion of such research in Chap. 9). Much laboratory work not specifically centered on accidents, however, has produced findings pertinent to this field.

There is a good deal of controversy as to the degree to which it is necessary and desirable to reproduce in individual laboratory experiments, in so far as possible, all the characteristics of accident situations. For example, some investigators strongly favor the development for motor vehicle research of highly sophisticated "simulators"—somewhat like Link Trainers—which would attempt to simulate many of the complex circumstances in which accidents occur.* Others argue that relatively simple experiments concerned with only small numbers of variables can often yield results of equal utility and validity, and there is considerable evidence, particularly from collateral fields, that this is sometimes the case. The fundamental consideration, however, is not the complexity or emphasis of a given approach but rather whether it represents the most scientifically appropriate and efficient means of answering the questions at issue.

It is essential to recognize that, although laboratory research is an effective approach to some types of problems, it is not appropriate for the solution of all research questions. There are classes of problems which cannot usually be solved through laboratory experimentation alone. For example, simulation, of whatever degree of complexity, requires thorough knowledge of that which is to be simulated. This implies the need for observation of accidents as they actually occur.

In addition, it is often desirable to study in the laboratory the effects of circumstances somewhat different from those which have been adequately investigated in relation to actual accidents. When this is done, the relevance of the findings to nonexperimental situations is frequently not firmly known and can only be estimated on the basis of the totality of the available laboratory and nonlaboratory evidence.

A related difficulty involves what may be referred to as the "ranking problem" (see "Alcohol in the Single Vehicle Fatal Accident," Chap. 4). It is seldom recognized that a common type of research problem is not readily susceptible to either laboratory or field experiments. Although these approaches may be suitable to determine whether a given factor (or combination of factors)—for example, the use of barbiturates—*can* lead to the occurrence of accidents, they cannot usually be used to determine the frequency with which that factor *does* lead to accidents. Thus, although the production of accidents in relation to the administration of barbiturates might be simulated in the laboratory, such work usually contributes little to knowledge of either the prevalence of the pertinent barbiturate concentrations in the actual nonlaboratory population at risk or their importance as a cause of accidents. Questions that at present can be investigated only by epidemiologic methods.

A related problem derives from the fact that variables not known or thought

* These are now being developed.¹

to be pertinent in connection with accidents as they actually occur are not likely to be studied in the laboratory. This is but another reason why laboratory research should be substantially based on careful study of the actual phenomena with which it is concerned (see "On the Natural History of Falls in Old Age," Chap. 4).

Experiments may also vary greatly in their complexity, content, and emphasis, since the situations which they attempt to duplicate show similar variation. As a result, no one laboratory approach is best for all problems, and, as in all research, the first step should be the definition of the problem rather than the decision to use a given approach. This is commonly overlooked in accident research, to the detriment of the field.

Objective choices between laboratory and other approaches, however, involve many considerations in addition to those just noted. In theory, the method selected should be the most pertinent, reliable, and efficient available; in practice, questions of financing, the availability of competent and interested personnel, and many other factors influence the choices made. Consequently, it is not possible to provide rules of thumb for choice of method in specific cases. Experience has shown, however, that certain questions are answered far more easily in some ways than in others,[†] and that some approaches cannot answer certain types of questions. For example, one would not expect an approach based exclusively on interviewing to differentiate reliably between fatal accidents resulting from "heart failure" and those resulting from the rupture of various arteries in the brain. Nonetheless, such limitations are often overlooked by those who consistently apply only one method and viewpoint to all problems.

SOME EXPERIMENTAL BIASES

Whenever accident research involves the use or identification of specific individuals, it is essential that their selection be free from pertinent biases. For example, if one wished to learn the effects of a pharmacologic agent on individuals of highly varied characteristics, such as occur in the general public, it would be inappropriate to use as subjects healthy young engineers or laboratory technicians or those who, through previous experience with the same drug, had developed tolerance to it.[‡] In addition, in laboratory research the circumstances in which the experiment is carried out must not, if the research is to have validity, pertinently influence the results to be obtained. Thus, physical and social characteristics of the experimental situation—for example, noise, vibration, and the presence of observers—are among

[†] "Student" has cited a classical example of the cost of a poorly planned experiment. In it, 20,000 subjects were used when 100, appropriately chosen, would have been sufficient.²

[‡] Many examples of the inappropriate choice of experimental subjects could be cited, particularly with respect to the testing of safety equipment. One tinted windshield, for example, was tested with a small group of young adult males who, in addition to being atypical of much of the driving public, particularly with respect to vision and related characteristics, also knew in advance the location of the targets they were to identify while driving under low illumination. Those who come to accident research from backgrounds in engineering and other fields in which the high degree of variability characteristic of human and other biological material is not usually present appear to overlook consistently the problems which this presents, not only in the choice of experimental subjects and samples but also in the design of equipment.

the many factors which must also be controlled. Finally, as in all experimentation with living subjects, it is necessary that placebo and learning effects be either eliminated or rigorously controlled.

One of the common shortcomings of laboratory research has involved failure to familiarize the experimental subjects with the equipment and circumstances in which they are to be tested, with the result that studies of the effects of changes in the variables under investigation have been grossly contaminated by learning effects. The performance of human subjects on driving simulators, for example, has been measured before and after the administration of beverage alcohol despite the fact that the subjects were still learning how to operate the devices used. Although the error of this approach would appear obvious, it has been overlooked by many investigators unfamiliar with the sophisticated methods developed especially by psychologists and pharmacologists to overcome biases of this type. It is to indicate the nature of these safeguards that the following selection is included.

THE INFLUENCE OF ALCOHOL ON AUTOMOBILE DRIVING ABILITY

—*T. A. Loomis, Ph.D., M.D., T. C. West, Ph.D.*

This report indicates the extreme care that must be taken in any such experiment. It demonstrates also the substantial magnitude of the shifts in quality that may occur during the learning phase of a subject's performance with a new and complex situation or device.

DRIVING AN AUTOMOBILE has become a necessary part of the daily activities of the average American adult. Multiple physiological and sociological mechanisms must function properly if the privilege of automobile driving is to be a safe and helpful factor in everyday life. Because the automobile is a complicated and sometimes a lethal instrument, all drivers are obligated to function to the best of their normal ability in any situation that may arise. It is therefore proper that penalties be imposed through suitable legal channels on those who drive at any time without due regard for the rights of others. The driving of an automobile after drinking alcoholic beverages presents a major and important phase of this problem.

Since the consumption of alcoholic beverages by adults is legal everywhere in this country, it is necessary to have a suitable mechanism for determining the limit of effect from alcohol beyond which ability is impaired.

The two methods in common use for determining the extent of functional impairment resulting from the ingestion of alcohol are (a) clinical evaluation of the individual and (b) chemical estimation of the blood alcohol concentration. Neither method provides direct evidence of the degree of functional impairment in a given individual. The first method assumes that average normal function always exists in the absence of alcohol. There is no estimation of the con-

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trol ability of a given suspect that would establish the basis on which to evaluate the decreased ability attributable to alcohol. Consequently, impairment resulting from alcohol must be extensive and obvious before such clinical tests can be interpreted positively. Must the state of impairment of function from alcohol be obvious and excessive before any person shall be considered as being under the influence of alcohol or before his driving ability is impaired?

The second method of evaluating the extent of impairment of function from alcohol involves the use of chemical tests for measuring the concentration of alcohol in the blood. Rationale for the use of this indirect method is based on reports made by formal committees of the National Safety Council and the American Medical Association. These reports are the opinions of experts who were acquainted with the experimental evidence indicating a direct relationship between the effect of alcohol and the concentration of this compound in the blood. A number of studies have dealt directly with the relationship between the amount of alcohol in the blood and the degree of functional impairment in actual or simulated driving conditions. Many other studies, as summarized by Greenberg utilized laboratory techniques to determine the direct relationship between depressed function of the central nervous system and blood alcohol concentration.

In 1955, 46 states were using chemical tests to help determine the degree of intoxication in suspected drinking drivers. Twenty-three states have chemical-test laws which are generally modeled after Sec. 11-902 of the Uniform Vehicle Code. A significant feature of that section of the Code is the provision that if the concentration of alcohol in an individual's blood is 0.15 per cent or more, he shall be presumed to be under the influence of alcohol. Therefore, the value 0.15 per cent (sometimes expressed as 150 mg. per cent) has attained considerable legal prestige as the single value that determines whether or not an individual is "under the

influence." Pharmacologically, alcohol would indeed be a peculiar drug if it produced no significant effect at a blood concentration of 0.14 per cent but a significant effect at 0.15 per cent. Yet this is essentially the stand taken by the legal profession with the establishment of such laws. Pharmacologists recognize that alcohol has, among its several actions, a progressive central nervous system depressant effect. This implies that there must be a minimum concentration of alcohol below which the effect is not measurable and therefore is not significant. The question is not how high but rather how low the concentration of alcohol in blood may be if driving ability is not to be influenced by the presence of alcohol.

METHOD

In order to evaluate critically the blood alcohol level at which driving ability is measurably impaired, an experimental procedure was devised. A simulated automobile driving device was designed for laboratory use. A conventional automobile steering wheel, brake and accelerator apparatus could be operated in a manner identical to that of driving an automobile. The steering apparatus was arranged to operate a miniature automobile as it passed over a moving belt. On the surface of the belt was a continuing road bed 50 yards in length which moved irregularly but smoothly from side to side. A photo-electric cell mounted beneath the "auto" was in the beam of a light source below the belt. The light source moved with the auto. When the auto was not directly above the opaque road, the time off the road was accurately recorded and a signal light above the auto was illuminated. Above and in the center of the belt were three colored lights (green, amber and red) covered with a translucent plastic plate. Timed illumination of these traffic lights was controlled automatically from an electrically driven cam-switching mechanism. Cumulative interval timers were arranged to record the time interval between the appearance of the red light and application of pressure on the brake

pedal, and between the appearance of the amber light and the release of pressure on the accelerator pedal. Adjacent to the traffic lights was an additional clear light which was illuminated only when the auto was "off the road" thereby providing a visual indication to the driver that he was "off the road."

Subjects were instructed with regard to operation of the test driving apparatus in the following manner: When the light was green the accelerator pedal was to be depressed. This actuated movement of the road belt. When the light turned amber the accelerator was to be released. This procedure did not influence movement of the road belt. When the light turned red the brake pedal was to be depressed. This procedure stopped the road belt. The auto could be "steered" from side to side whether the road belt was stationary or moving and each subject was instructed to keep the auto centered over the opaque road strip at all times. Each test consisted of the time (approximately 4½ minutes) required to traverse the entire length of the road belt. During each test period the amber or red light was on 8 to 10 times, each of 3 seconds' duration.

The following data could be obtained at each test period:

1. Reaction time to amber light in milliseconds. This is the interval between the appearance of the amber light and release of pressure on the accelerator pedal.

2. Reaction time to red light in milliseconds. This is the interval between the appearance of the red light and the application of the brake.

3. The accumulated time during which the auto was not on the road.

4. The time required to operate the auto over the entire length of the road.

5. The time that the road belt was in motion.

Figure 1 [omitted] consists of two views of the test apparatus. This apparatus differs from the American Automobile Association "auto trainer" in the following respects: the A.A.A. auto trainer consists of a continuous belt (approximately 10 feet in circumference) which rotates several complete revolutions

in a single test, thereby exposing the driver repeatedly to the same segments of road; the road bed is relatively wide compared to the width of the auto; the accuracy of maintaining the auto on the road is determined by electrical contacts which are made between the car and metal staples embedded every 3 inches in the road and each such contact advances a counter 1 unit; the "traffic lights" are illuminated intermittently by the test operator. The apparatus used in the present experiments contains a 50-yard nylon belt with a 1-inch wide road bed and a 4-inch wide auto; each portion of the road is encountered only once in a given test and the identical distance and road are traversed in each individual test; accuracy of maintaining the auto on the road is photoelectrically timed, resulting in a continuous recording of the time that the auto is off the road; all signal lights are automatically timed and are illuminated for identical intervals during and between tests. The test is sufficiently difficult to perform so that even the most "expert" operators are not able to maintain the car on the road throughout the entire test.

Acquainting the subjects with the test apparatus required a learning period. Following the learning period, 10 adult subjects, in 2 groups of 5 each, were scheduled for experimental tests at weekly intervals. On each test day subjects ate a light breakfast and no food until 11 A.M. at which time they were given a standard lunch consisting of ½ pint of milk and an egg or meat sandwich. Between 11 A.M. and noon a control test run was made on the apparatus. Between noon and 1 P.M. the subjects ingested by mouth a priming dose of alcohol. Each hour thereafter for the next 5 hours they received an additional maintenance dose of the alcoholic beverage. The alcohol was ingested in the form of conventional Martini or Manhattan cocktails or straight whisky. Repeated test runs on the driving apparatus were conducted at 1:30 P.M., 3 P.M. and 5 P.M. Following each test run a blood sample was obtained from an antecubital vein for alcohol

ship between the output required in such circumstances and the capacity of such elderly individuals to respond (see "On the Natural History of Falls in Old Age," Chap. 4). Until studies of this kind are undertaken for many accident situations, key items of information will be unavailable to those concerned with understanding their causation and prevention.

THE VISUAL CLIFF*

—*Eleanor J. Gibson, Richard D. Walk*

The next selection, which summarizes experiments concerned with the accident-avoiding behavior of the newborn of several species, including man, is an excellent example of laboratory-based research. It is of interest not only because it is one of the very few examples of accident research dealing with nonhuman species but also because it demonstrates the importance of avoiding a narrowly anthropocentric point of view. It is noteworthy also because it demonstrates the pertinence of characteristics which are biologically innate, an area that has been ignored by most accident research workers.

The authors were concerned, as they state in the first paragraph, with understanding the fact that "human infants at the creeping stage are notoriously prone to falls from more or less high places."† In view of our emphasis in the preceding chapter on the hazards of basing assumptions on "common sense," the following passage is of particular interest. "Common sense," they state, "might suggest that the child learns to recognize falling-off places by experience. . . . But is experience really the teacher? Or is the ability to perceive and avoid a brink part of the child's original endowment?" The data reviewed, in supporting the latter conclusion, offer an additional example of the need for objective experimentation rather than unsupported speculation.

HUMAN INFANTS at the creeping and toddling stage are notoriously prone to falls from more or less high places. They must be kept from going over the brink by side panels on their cribs, gates on stairways and the vigilance of adults. As their muscular coordination matures they begin to avoid such accidents on their own. Common sense might suggest that the child learns to recognize falling-off places by experience—that is, by falling and hurting him-

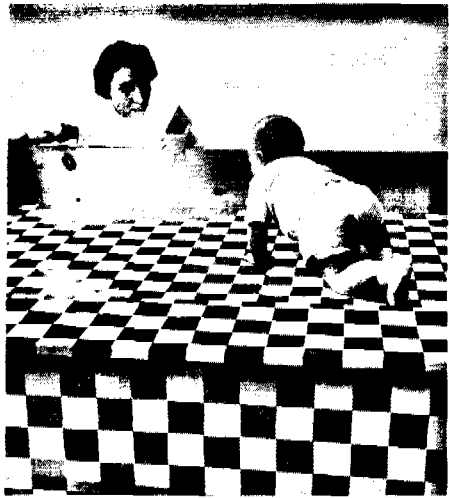
self. But is experience really the teacher? Or is the ability to perceive and avoid a brink part of the child's original endowment?

Answers to these questions will throw light on the genesis of space perception in general. Height perception is a special case of distance perception: information in the light reaching the eye provides stimuli that can be utilized for the discrimination both of depth and of receding

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William Vandivert.]

* See also a related paper by James J. Gibson in Chap. 6.

† The selection from Rowntree in this chapter documents this on a population basis. These two reports provide an excellent example of the complementary nature of the results of laboratory and nonlaboratory research concerned with related subject matter.

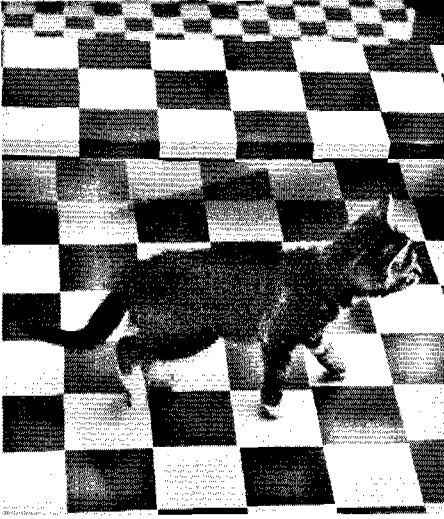


Child's depth perception is tested on the visual cliff. The apparatus consists of a board laid across a sheet of heavy glass, with a patterned material directly beneath the glass on one side and several feet below it on the other. Placed on the center board (*top left*), the child crawls to its mother across the "shallow" side (*top right*). Called from the "deep" side, he pats the glass (*bottom left*), but despite this tactual evidence that the "cliff" is in fact a solid surface he refuses to cross over to the mother (*bottom right*).

distance on the level. At what stage of development can an animal respond effectively to these stimuli? Does the onset of such response vary with animals of different species and habitats?

At Cornell University we have been investigating these problems by means

of a visual cliff. The cliff is a simulated one and hence makes it possible not only to control the optical and other stimuli (auditory and tactual, for instance) but also to protect the experimental subjects. It consists of a board laid across a large sheet of heavy glass which is supported



Kitten's depth perception also manifests itself at an early age. Though the animal displays no alarm on the shallow side (*top*), it "freezes" when placed on the glass over the deep side (*bottom*); in some cases it crawls aimlessly backward in a circle.

a foot or more above the floor. On one side of the board a sheet of patterned material is placed flush against the undersurface of the glass, giving the glass the appearance as well as the substance of solidity. On the other side a sheet of the same material is laid upon the floor; this side of the board thus becomes the visual cliff.

We tested 36 infants ranging in age from six months to 14 months on the visual cliff. Each child was placed upon the center board, and his mother called him to her from the cliff side and the shallow side successively. All of the 27 infants who moved off the board crawled out on the shallow side at least once; only three of them crept off the brink onto the glass suspended above the pattern on the floor. Many of the infants crawled away from the mother when she called to them from the cliff side; others cried when she stood there, because they could not come to her without crossing an apparant chasm. The experiment thus demonstrated that most human infants

can discriminate depth as soon as they can crawl.

The behavior of the children in this situation gave clear evidence of their dependence on vision. Often they would peer down through the glass on the deep side and then back away. Others would pat the glass with their hands, yet despite this tactual assurance of solidity would refuse to cross. It was equally clear that their perception of depth had matured more rapidly than had their locomotor abilities. Many supported themselves on the glass over the deep side as they maneuvered awkwardly on the board; some even backed out onto the glass as they started toward the mother on the shallow side. Were it



not for the glass some of the children would have fallen off the board. Evidently infants should not be left close to a brink, no matter how well they may discriminate depth.