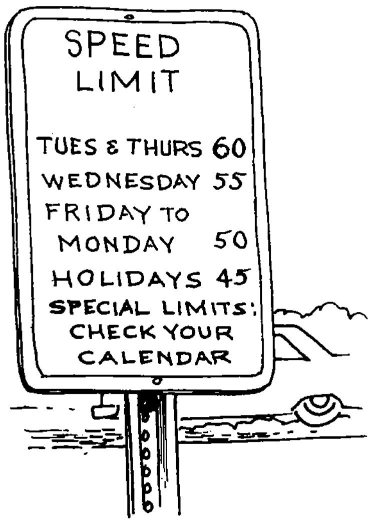

DO SPEED LIMITS REDUCE TRAFFIC ACCIDENTS?

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It is surprisingly difficult to discover what factors cause increases or decreases in traffic accidents. Sometimes changes are made in speed limits, in highway design, in driver-licensing standards, or in vehicle specifications in an attempt to decrease accidents. Yet, it can sometimes happen that the number of accidents will actually increase. These surprising increases may come merely from increases in population, or from increases in number of vehicles on the road, or perhaps from greater distances traveled.

MEASURING ACCIDENT RATES

So it is sensible to measure accident *rates* rather than the raw number of accidents. Of course, there is a variety of such rates; among the most common are *accidents per person in the population, accidents per registered vehicle,* and *accidents per vehicle mile traveled.* For example, if along a certain highway there were 500 accidents in 1970 and 10 million vehicle

miles, then the last rate would be 50 accidents per million vehicle miles. Often these rates are measured in terms of fatalities rather than in terms of all accidents.

The behavior of these rates in the U.S. since the late thirties has been approximately as follows: (1) the fatality rate per person is increasing slightly; (2) the fatality rate per registered vehicle is decreasing; and (3) the fatality rate per vehicle mile is decreasing, rather quickly during the forties and fifties and less quickly later. From 1963 to 1970 it may even have been stationary. This combination of rates would be consistent with a trend of proportionately more cars but relatively safer ones. This is roughly true also for other places in the world where road traffic plays an important part in the society: Western Europe, Australia, and New Zealand.

The total size of the population considered affects the stability of all such accident rates. For a large country like the United States there is very little fluctuation from year to year or even from month to month because we are dealing with averages based upon large numbers of cars and large numbers of accidents. In a small town, to take the other extreme, it may be nearly impossible to "see the forest for the trees" since a very few accidents may have the effect of changing the average greatly.

This tendency is shown clearly by the data of Figure 1 from the National

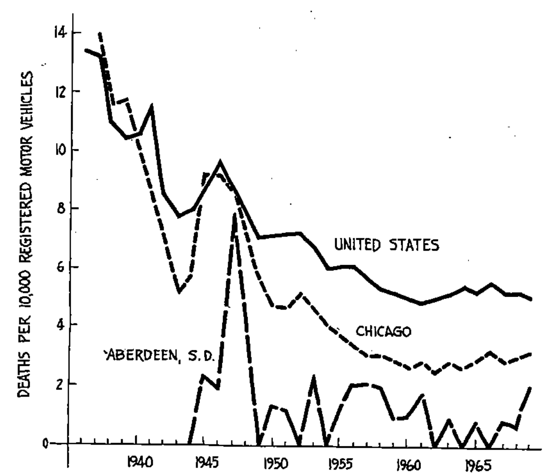

FIGURE 1
Deaths per 10,000 registered motor vehicles, 1936–69, U.S. and selected localities. Source: Data from National Safety Council

Safety Council. It should be noted that the rate given in Figure 1 is *fatalities per ten thousand registered vehicles*. A similar graph based on the rate of *fatalities per hundred million vehicle miles driven* would be even more informative, but this rate is not known for subdivisions smaller than whole states. The reason is that the estimate of vehicle miles traveled is based on gasoline consumption and records of this type are not usually kept by municipalities. (Thus, if you buy a gallon of gasoline and throw it away without using it in your car, you will actually lower the fatality rate per vehicle mile for that year as much as if you had used the gas safely in your car!) Because the averages for the country are so stable but those of small towns so unstable, predictions of the number of fatalities expected over holiday weekends are usually given for the country as a whole rather than for smaller geographical divisions.

EVALUATING CHANGES IN ACCIDENT RATES

From the point of view of accident prevention there are two important consequences of this variability. The first is that since the most commonly used rate is generally going down, its decrease after some specific change is made does not by itself prove that the change was beneficial. Second, if the change takes place over a small population—even as small as several million—it may be well nigh impossible to separate the effect of this change from the general chance fluctuation. The statistical point is not really the population size, but the general level of the numbers of accidents. The numbers of accidents do not vary much percentagewise when the general level is large, but they do when it is small (see the essay by Campbell).

An added difficulty is that most changes in traffic patterns, laws, or law enforcement are expensive: rebuilding highways, eliminating grade crossings, building safer cars, and enforcing safety rules are typically costly items. Some of these factors are, in addition, nearly permanent; once done it is not easy to undo them. For example, if an expensive bridge is built, it cannot readily be relocated. Hence, there is a tendency to introduce improvements in the road system on the basis of good sense and experience rather than on empirically demonstrated improvements in highway safety; many claims of benefit are to be taken in a general rather than specific sense—the accident rate *is* decreasing although it is hard to pinpoint the exact causes.

This may in some ways be a gratifying situation, but it is not helpful in planning new measures. Given a fixed budget, should investment be made in driver training, in better enforcement of traffic regulations, in alcohol testing, in road reconstruction, in speed limit changes, or in some completely different approach such as subsidizing public transit? Furthermore, safety and budget are not the only considerations; convenience counts too. How can we find out if each of these proposed measures is effective?

EXPERIMENTS TO EVALUATE PROPOSED CHANGES

It would seem that a logical method would be to set up an experiment. But, in field after field of public policy, responsible authorities object to carefully controlled experimentation because they feel they cannot tamper with such important and expensive arrangements. Although there is much to discuss on this point, we shall not debate the rights and wrongs of it here, but report only that road authorities are usually unwilling to have the road transport system play the role of a guinea pig. They regard it as expensive and confusing to the traveling public and likely to be inconclusive.

Nevertheless, let us think about how such an experiment to test the effect of an improvement in traffic control on accident fatalities should be designed. Because fatal accidents are rare (less than six fatalities per 100 million vehicle miles in the U.S., for example), a large sample of driving is needed for such an experiment to give a clear answer through the mist of chance fluctuation. Furthermore, to allow for a general decreasing trend in rates the statistician would like to have the proposed improvement operate only on alternate days of the week or be "turned on and off" according to some reliable, recognizable program. The policeman will usually not agree to have traffic regulations change so often, although he may be willing to experiment for a week or two along a few miles of highway.

THE SCANDINAVIAN EXPERIMENT

An exception to this usual official reluctance to experiment has been in progress in Denmark and Sweden for nearly ten years. The governments of these countries have introduced *periodic speed limits* (speed limits that stay the same for long periods of time over long stretches of road, and then are changed for other long periods).

We must realize that speed limits of any sort are bitterly opposed in many European countries and are considered to be justified only if their reduction of accident rates can be strictly demonstrated. The American idea that speed limits are favorable for all aspects of road usage (for example, maximum flow), not merely safety, is in general not accepted.

In Sweden, a royal commission was established in 1961 to investigate various aspects of speed control; the commission members were statisticians and other scientists from the country's foremost technical university. The experiment for the first year involved the comparison of a 90-kilometer-per-hour limit (about 56.7 miles per hour) against no speed limit over 71 different road sections, chosen to represent a variety of road conditions. In that first year the speed limitation was in force from Friday, May 19, through Wednesday, May 24, from Thursday, June 22, through Wednesday, July 12, and from Friday, September 1, through Monday, September 11.

In subsequent years, the experimental program has been more ambitious and more complicated, involving speed limits of 90 kph, 110 kph, and 130 kph, as well as larger road networks; in 1968, all the roads of the European highway system in Sweden were included, together with some of the principal national main roads. Also the speed limits (or lack of limits) were maintained for longer periods of time, sometimes several months or a year, in order to obtain larger samples of accidents.

ACCIDENT RATES AND THE POISSON PROCESS

With the immense quantity of data obtained (7000 accidents during the 1968 experiment, for example) many different types of statistical analysis were performed. This article deals with one analysis which evoked some theoretical problems: before-and-after comparisons of accident rates.

Long study of the traffic accident phenomenon shows that a simple random process which well describes the instant at which accidents occur is the so-called *Poisson process*. A Poisson process is a quantitative way of expressing the fact that the accidents are indeed *accidental,* that is, that each accident occurs at a moment in time which is completely independent of the moments when other accidents occur—the times of the accidents are perfectly scrambled. This specification is incomplete, however, unless it also includes a so-called mean value that expresses how often accidents occur *on the average.* There can be a Poisson accident process on a main highway with, for example, 100 accidents per year on the average, or on a small rural road with only one accident per year on the average. In such a case the accident record for the rural road for 100 years would, if compressed into a year, duplicate the statistical properties of the single year on the main road. Thus, if we let the letter m denote the mean number of accidents observed for a section of road, we find that m will depend (among other things) on the traffic volume characteristic of the road sections. In the Swedish experiments, the volumes ranged from less than 150 vehicles per day up to values in excess of 5000 per day.

This variability in the traffic volume typical of a road section was complicated further by variability in volume over the days of the year. Some roads have heavy traffic during the summer, others in the winter. On certain days there are special events such as football games or national holidays to increase traffic flow. The weather influences both the volume of traffic and the risk of an accident, and even if the record is limited to the same day in consecutive years, there may have been a blizzard on one of these days and not on the other.

Therefore, the problem of comparing values of m without the speed limit to values of m with a speed limit seems to break down into thousands of special tests, one for each bit of road and bit of time. Fragmentation of the problem in this way reduces the sample sizes and seems to nullify the

whole purpose of the experiment. Also, the conclusions would be far less reliable because of the small samples, and in the majority of cases, it might be impossible to come to any conclusion whatsoever.

It is clearly desirable to group the road segments and days into larger groups that will be homogeneous, but the factors involved are so varied as to make this nearly impossible. How can we compare the effect of a rain-shower with that of a construction project, or of a national holiday with an extra-wide shoulder? Is there any quantitative equivalence between a road in the far north experiencing wintry conditions for long periods and one near a ferry terminal serving many foreign tourists?

The solution to this problem was found to lie in grouping together road segments having the same number of accidents during the total period of the study, and for each group examining the proportion of accidents that occurred in the "before" period, and the complementary proportion that occurred during the "after" period; these time periods are of equal length. Thus, for example, we group together all road segments having ten accidents over the total experimental period; these road segments might be very different, yet their qualitative differences balance out to the extent that they experienced the same overall number of accidents.

To get an intuitive idea about the reasoning, let us suppose that there are but two conditions: having a speed limit and not having one. If we suppose that having a speed limit is effective in improving safety, then when we collect all the road segments we should find that more accidents occur under the "no limit" condition than under the "speed limit" condition. We have many segments, so we can look at the results for many similar segments, and thus pile up a considerable record. Furthermore, in some segments, the "speed limit" condition would have come first, and in others second, and we can check whether the order mattered. We can also see whether "speed limits" matter more to safety in segments with high accident rates or with low, and so on.

In any case, the key statistical technique was to put together those road segments having the same total number of accidents in the "before" and "after" periods, even though the segments grouped together might have nothing in common beyond their accident experience. With this approach, it was found that deciding whether the Poisson mean value m had changed as a result of the speed limit could be reduced to a simpler statistical problem involving the ratio of the m value "before" to the m value "after," given that the total accidents "before-and-after" was the same.

SOME RESULTS

One general result of the Swedish and Danish analyses is especially interesting. It appears that speed limits were more effective in Sweden than in Denmark! The reason behind this is not at all clear.

We can only speculate on the value of a similar experiment in the U.S. Probably it would not be useful to choose "speed limit" as the experimental variable because most of our communities have roughly similar attitudes towards speed restriction, with relatively small local variations. A more interesting variable might be vehicle inspection systems, which vary greatly from no compulsory inspection at all in California to very rigorous periodic checks in Pennsylvania. There has been recent discussion in technical journals about the effect of inspection laws on accident experience. How could we design an experiment that would test this factor in isolation from others? We could, perhaps, trace the accidents which Pennsylvania vehicles have on California roads and vice versa. Or we might begin an inspection system which applies only to blue cars and trace the proportion of blue cars in accidents. Either of these, or some other design, would give some clear indication of the usefulness of various inspection systems.