MEASURING THE EXTENT OF THE DRINKING AND DRIVING PROBLEM

Article Accid. Anal. & Prev. 15 (1983) 6 : 407-414

R-83-45

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Leidschendam, 1983
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1. INTRODUCTION

Drinking and driving is considered a problem because of its relationship to traffic accidents. This problem exists in most countries and has captured the attention of researchers and policy-makers alike. However, the accurate measurement of the problem has not been easy.
As early as 1974, Zylman wrote that many of the statements about the number of traffic fatalities caused by drinking are not justified and often exaggerated. Such statements are based on the results of studies on the Blood Alcohol Content (BAC) of traffic fatalities. According to Zylman these "have been misinterpreted, misread and sometimes misrepresented".
More recently Wilde (1981) criticized the measures used to evaluate the effects of alcohol countermeasures. As a rule, indirect measures are used, for instance a so-called surrogate measure such as nighttime crashes: the "relevance (of which) remains in doubt". The BAC-distribution of traffic fatalities is considered to be "another dubious measure", roadside BAC-measurements are "not necessarily relevant", and DWI-arrest rates have "more mundane methodological flaws".

The aim of this paper is to discuss and illustrate a number of methodological issues involved in the measurement of the drinking-driving problem. There are several methods to measure the problem. The advantages or disadvantages of these depend on the purpose for which the measure is being used. In this paper a distinction will be made between:
- measuring the extent of the problem;
- monitoring changes over time;
- evaluating the effects of countermeasures.

2. MEASURING THE EXTENT OF THE PROBLEM

The extent of drinking and driving as a road safety problem should be expressed as the number of accidents caused by alcohol. The most direct method to determine this is by means of in-depth accident investigations such as that reported by Storie (1975). In this study, a sample of about 2000 (mostly minor) accidents was investigated by a team of experts and it was found that one or more of the drivers had been drinking in about 25% of the accidents. Drinking could be established as a contributory factor in 9% of all accidents, 14% of single-vehicle accidents and as much as 30% of nighttime accidents.
The percentage of accidents with drinking established as a contributory factor is an overestimation of the percentage of accidents caused by alcohol. Yet this kind of in-depth study is nevertheless the most direct method to measure the extent of the problem.

On the other hand, the number of accidents caused by drinking can be indirectly estimated by means of a method described by Hurst (1970). First, the relation between BAC and accident risk is estimated by means of a case-control study. Secondly, the number of alcohol-related accidents is estimated by combining this knowledge with the BAC-distribution of accident-involved road users taken from the case-control study. Actually, the estimate can also be based on the BAC-distribution of the control sample. Hursts' method can be further extended by using BAC-distributions of other investigations.
Investigations into the BAC-distribution of accident-involved road users, roadside surveys and case-control studies will be discussed in separate paragraphs.

BAC-measurement in accidents

Investigations into the BAC of accident-involved road users are usually restricted to those seriously or fatally injured. This may lead to the problem that in multi-vehicle accidents the injured road user may have been sober whereas an uninjured road user in the same accident had been drinking. In this case, the accident would not be registered as being alcohol-related. Apart from this, there are other causes of missing data in this kind of investigations, which will be shown in the following examples.

The Traffic Injury Research Foundation of Canada (TIRF) constructed a data base from coroners' records in five Canadian provinces (TIRF, 1975). The two criteria for inclusion of a province were: coroners records should cover at least 85% of officially known traffic fatalities; at least 60% of driver fatalities aged 16 and over should have been tested for BAC. In 1973, 78% of the fatally injured drivers included in the data base had in fact been tested. BAC-testing seems to be dependent on collision type, age and sex of victim, and interval between time of crash and time of death (with 24 hours used as a limit for the definition of a traffic fatality). Under the assumption that drivers who had not been tested had a negative BAC, 38% of driver fatalities were over the legal limit of 80 mg%, with 27% in the case of multi-vehicle accidents and 53% in the case of single-vehicle accidents.

In another investigation by TIRF (Warren et al., 1982), BAC-measurements were obtained from a sample of 2500 road users reporting to a hospital for injury treatment. In this case, missing data are a more serious problem. A BAC-measurement was obtained for only 45% of the total sample. Reasons for exclusion included: serious injury requiring immediate treatment (10.6%), age under 16 years (9.2%), time interval between crash and arrival at hospital over three hours (12.6%), refusals to be tested (9.7%), and miscellaneous reasons (12.8%). Drivers had a higher refusal rate (19%); of the 488 tested drivers 28% had been drinking (BAC > 20 mg%), and 21% exceeded the legal limit of 80 mg%.

Roadside surveys

The sample of road users for a roadside survey is drawn from the traffic flow at selected locations and times. The road users are interviewed and tested for BAC. Most roadside surveys are conducted on a voluntary basis and therefore suffer from non-response.
In a series of roadside surveys in The Netherlands, the non-response rate ranged between 10 and 20% (Noordzij, 1981). Various publications contain suggestions for handling this problem (Wolfe, 1973; Hurst & Darwin, 1977;

Carlson, 1979; Jonah, 1982), however, the only satisfactory solution is to keep the non-response rate low.

In a number of Scandinavian roadside surveys the problem of non-response does not seem to exist (Persson, 1978; Päntillä et al., 1981), probably because these surveys were conducted in close co-operation with the police, or as part of police enforcement activities. This, however, introduces other problems. The sampling procedure of roadside surveys is meant to produce a random sample of drivers, while police sampling may be aimed at detecting a high number of drivers with illegal BAC, resulting in biased samples. Alternatively, the police may want to be highly visible in their enforcement activities. If they succeed in doing so, normal drinking and driving patterns will be disturbed and the results of the BAC-distribution in the sample will not represent the normal situation. (This may even be a problem with roadside surveys conducted on a voluntary basis, which may attract public attention and be perceived as an enforcement activity.) In addition, once a driver is selected from the traffic flow and stopped, the police may be hesitant to demand a breath test if the driver does not show any signs of impairment. This again may result in biased sampling.
A secondary problem involves the type of breath testing device that police officers are authorized to use. Some of the screening breath test devices do not provide an accurate BAC-measurement (Noordzij & Mulder, 1978).

<u>Case-control studies</u>

In case-control studies a sample of accident-involved road users is matched to a control sample of road users. BAC is measured in both samples, and relative accident risk is calculated. Hurst (1970) presented a formula for this purpose:

$$RP(C/B) = \frac{P(C/B)}{P(C/B_o)} = \frac{P(B_o)\ P(B/C)}{P(B)\ P(B_o/C)}$$

where:

P = the probability

RP = the relative probability (accident risk)

C = the accident

B = a positive BAC category

B_o = a negative BAC category

Hurst applied the formula to data obtained from various investigations, and found that in all cases the relative accident risk started to increase at BAC-levels between 50 and 100 mg%, and increased rapidly at higher levels. At higher BAC-levels, however, the magnitude of the relative accident risk varied considerably between investigations.

Investigations of this kind are rare. One such investigation has recently been carried out in Australia (McLean & Holubowycz, 1981). The results closely resemble those obtained in the well known study by Borkenstein et al. (1974) with regard to the relation between BAC and accident risk. Other investigations, however, revealed considerably higher values for the relative accident risk (Warren, 1976; Christensen et al., 1978).

Differences in results may be related to differences in the design of the investigations.

First, the results are likely to depend on sample characteristics, e.g., time and location of the accidents, road user characteristics, accident seriousness, and whether the accident sample included all road users involved or only those injured. Borkenstein et al. and McLean & Holubowycz analysed mostly minor accidents, Christensen et al. used seriously injured persons and Warren had a sample of traffic fatalities. The limited scale of most of the investigations does not allow differentiation of the relative accident risk, e.g. between road user characteristics. Warren, however, differentiates between driver age groups and finds considerable variation in relative accident risk (at BAC > 100 mg%).

Secondly, the results can be affected by the accuracy of the BAC-measurement.

Thirdly, the level of aggregation of accident and control sample matching may vary between investigations. Borkenstein et al. and McLean & Holubowycz matched their samples on a per individual basis with respect to accident time and location. Warren and Christensen et al., on the other hand, matched samples on a group basis with respect to period and area.

Fourthly, the treatment of missing BAC-data may influence the results. Warren & Simpson (1980) found that, in their case, the relative accident risk varied by a factor two, depending upon the way missing data in the accident sample was treated.

Finally, the relative accident risk is calculated per road user rather than per accident. The flaw of this approach becomes evident in the case of multi-vehicle accidents. Namely, the risk for a sober road user is increased by the presence of drinking road users.

A proper formula to calculate the relative accident risk for multi-vehicle accidents has yet to be developed, and should be a function of the BAC-levels of all road users involved in the same accident.

This paragraph has shown that the relation between BAC and accident risk is known only by approximation. This implies that an estimate of the number of accidents caused by drinking is also an approximation. The estimate is further complicated by the difficulties involved in obtaining accurate BAC-distributions.

3. MONITORING CHANGES OVER TIME

A change in the extent of the problem can be discovered by repeated application of the methods described in Section 2. However, these methods do not seem feasible for frequent or continuous monitoring, due to the cost of data collection. For this reason, an alternative method has been developed which does not even require BAC-measurements. For example, Ross (1982) used the number of seriously and fatally injured road users during weekend nights to study the effects of the 1967 British Road Safety Act. The use of this so-called surrogate measure for alcohol-related accidents seems to be reasonable since a relatively large portion of these accidents involve alcohol. Nevertheless, the choice and use of a surrogate measure is complex, as will be demonstrated in the following paragraphs.

The choice of a surrogate measure

To begin with, combinations of accident characteristics should be found which are closely correlated with drinking as the cause of accidents. Alternatively, a correlation with drinking by accident-involved road users should be found. Of course, such characteristics must be easily measurable and should preferably be included in standard accident statistics or police records. On the basis of these characteristics, an accident sub-group comprising a relatively large number of alcohol-related accidents can be selected. The size of this sub-group is called a surrogate alcohol measure. A change in the extent of the drinking and driving problem will be reflected by the surrogate alcohol measure. Conversely, a change in the surrogate alcohol measure should be interpreted as a change in the extent of the drinking and driving problem. The validity of a statement about a change in the extent of the problem which is based on a change in the surrogate alcohol measure, will be reduced if a portion of the alcohol-related accidents caused by alcohol is not included in this surrogate measure.

It is conceivable that changes in the surrogate alcohol measure are caused by factors other than drinking, which affect the (exposure to) the accident risk but are not specific to the drinking and driving problem. In order to detect and correct for these changes a reference accident

sub-group has to be set up as well. This reference sub-group must react to these factors in the same way as the surrogate alcohol measure.
The size of this reference sub-group is called a surrogate non-alcohol measure.
A portion of the alcohol-related accidents may unintentionally be included in the surrogate non-alcohol measure. In that case, the comparison between the surrogate alcohol measure and the surrogate non-alcohol measure will be misted.
In practice, a number of different surrogate measures is used, but the chosen surrogate alcohol measure is often some sub-group of serious nighttime accidents/casualties, and the surrogate non-alcohol measure is some group of serious daytime accidents/casualties.

<u>The use of surrogate measures</u>

In a previously mentioned study, Ross (1982) compared changes in the surrogate alcohol measure to the trend of the number of seriously injured and fatalities during daytime commuting hours. This surrogate non-alcohol measure, in contrast to the surrogate alcohol measure, did not change significantly immediately after the introduction of the Road Safety Act. This finding lends support to the interpretation of the change in the surrogate alcohol measure as caused by the introduction of the Act.

The use of surrogate measures is further complicated, if the surrogate alcohol measure and the surrogate non-alcohol measure are differentially affected by factors other than drinking. Such a possibility is far from academic. Noordzij (1981) reports on the effects of the introduction of a legal BAC-limit in The Netherlands during late 1974. The situation was complicated due to the fact that the country was confronted with an energy crisis in 1973. The 1974 accident data show a considerable drop in the number of fatal daytime accidents involving moving passenger cars (the surrogate non-alcohol measure). Such a drop was hardly noticable in the number of fatal nighttime accidents involving passenger cars. This surrogate alcohol measure did not decrease considerably until after the introduction of the legal BAC-limit. At the same time, the surrogate non-alcohol measure started to rise again, indicating that the energy crisis had little to do with the decrease of the surrogate alcohol measure.

This interpretation of the Dutch figures is supported by USA-data. According to Monaco (1977), "the energy crisis and the 55 mph speed limit had a greater impact on the surrogate measure for non-alcohol related accidents than on surrogate measures for alcohol-related crashes". Monaco studied which accident sub-groups could be used as surrogate measures in the evaluation of Alcohol Safety Action Projects (ASAP's) in various North American states. Consequently, the impact of ASAP's was studied on the basis of fatal nighttime accidents as compared to fatal daytime accidents (Levy et al., 1978). By establishing control areas, it was also possible to check whether the changes in the surrogate alcohol measure were the result of ASAP's or whether they might have been caused by other factors.

The examples presented in this paragraph demonstrate the complexity of the use of surrogate measures. The complexity may be reduced by a careful choice of the surrogate measures. The number of alcohol-related accidents must be established beforehand for both the candidate surrogate alcohol and non-alcohol measures, as well as for the group of accidents not falling into either category. Once the extent of contamination has been established and the surrogate measures have been chosen, data collection may be restricted to these measures only.

4. EVALUATION OF COUNTERMEASURES

The evaluation of a countermeasure can be seen as a special case of monitoring changes over time. A typical evaluation study involves repeated measurements of the drinking-driving problem before and after the introduction of a countermeasure. The interpretation of a change as caused by the countermeasure under study is a particular problem of evaluation studies. More sophisticated evaluation studies use a number of various measures in order to delineate the process by which the effect of the countermeasure was achieved. These two related subjects (interpretation of changes and process evaluation) will be introduced in the following paragraphs.

Interpretation of changes

A discussion of all the design features of an evaluation study is outside the scope of this paper. The interested reader may refer to the technical literature (e.g. Cook & Campbell, 1979).
The design of an evaluation study will enable the researcher to differentiate between the effects of the specific drinking-driving countermeasure and those of other, more general factors. To this end the number of alcohol-related accidents must be compared to the number of non-alcohol-related accidents occurring under similar circumstances. On a practical level this leads to a comparison of changes in the number of alcohol- and non-alcohol-related accidents for both nighttime and daytime hours, separately. See Section 3 for detailed discussion of the issue of a reference group.

Wilde (1981) raised an interesting point concerning the interpretation of a change in the BAC-distribution of accident-involved road users. He suggested that a drinking-driving countermeasure may have a positive effect on drivers with a moderate BAC, but no effect on drivers with a high BAC-level. As a result the total number of accidents may decrease, while the proportion of accidents involving high BAC-levels increases. If only high BAC-levels are recorded, such a change may be incorrectly interpreted as a deterioration of the original situation.

Process evaluation

An evaluation study is more enlightening if the results not only show the overall impact of a countermeasure, but also delineate the process by which the impact was achieved as well. For this purpose, a study must include measures of different stages of the accident generation process. In the case of drinking and driving, this could mean the inclusion in the same evaluation study of BAC-measurement in roadside surveys, BAC-measurement of accident-involved drivers, a direct measure of accidents caused by alcohol and of non-alcohol-related accidents, or the use of surrogate measures.
The process may be studied in more detail if additional data are also gathered from roadside or home interviews (on knowledge, attitude and behaviour) and from behavioural observations. This, however, will not be considered in this paper. For small-scale evaluation studies, the accident numbers are too small to permit statistical analysis. In those cases, the evaluation is restricted to other measures representing various stages of the accident generation process. The use of this measures will not permit a straightforward statement concerning the effect of the countermeasure upon accidents.
Three examples concerning three changes in drinking-driving legislation are presented below.

The effects of the 1967 British Road Safety Act have been studied by means of a surrogate measure (Ross, 1982) and by BAC-measurement of traffic fatalities (Codling & Samson, 1974). Both measures showed similar results: a sharp decrease immediately after the introduction of the Act was followed by a gradual recovery. Ross indicates an immediate drop of 66% in serious and fatal injuries during weekend nights. The BAC-measurements of drivers over 16 years of age dying within 12 hours of accident showed an immediate decrease of BAC's above the legal limit of 80 mg% from 27 to 17%. Remarkably, the percentage of illegal BAC's showed no regular trend for nighttime fatalities only (see Table 1). If both studies are correct, it should be concluded that the Road Safety Act had a strong and immediate positive effect on the nighttime (exposure to) accident risk for both sober and drinking road users.

Ross also presents a somewhat similar phenomenon in connection with the 1978 French alcohol legislation. The number of serious accidents/casualties decreased temporarily after the implementation of new legislation, while a series of roadside surveys carried out by the Organisation Nationale de Securité Routière (ONSER) did not indicate a change in nighttime drinking, not even during weekend nights. In this case, the legislation may again have affected the (exposure to) accident risk of both sober and drinking drivers. Alternatively, the (exposure to) accident risk of only drinking drivers may have been affected, even though they kept drinking (they may have been driving more carefully).

The introduction of a legal BAC-limit late in 1974 in The Netherlands was evaluated by means of fatal nighttime accidents (with moving passenger cars) and by a series of roadside surveys of weekend-night drivers (Noordzij, 1981). At the end of 1975 the BAC-levels of weekend-night drivers were reduced from 15 to 9% for BAC's above 50 mg% and from 5 to 2% for BAC's above 100 mg% (as compared to 1973). Three years after the introduction of the new legislation the BAC's were still lower than before the introduction (see Table 2).
The results of the roadside surveys suggest a modest effect as compared to the accident data. The surrogate alcohol measure decreased by 34% the year after the legislation was introduced, and stabilized at a reduction of about 20% for several years thereafter. The corresponding surrogate non-alcohol measure (fatal daytime accidents involving moving passenger cars) went up slowly during this period (see Table 3). In view of the knowledge about the relation between BAC and accident risk these results of the roadside surveys and the accident data do not necessarily conflict: a small change in positive BAC's may produce a considerable change in accidents.

The British and French examples demonstrate that different measures may lead to seemingly contradictory results. Actually, the use of multiple measures would present a more refined image of what actually occurred. Yet an unfortunate choice of a single evaluation measure may mislead rather than enlighten. Even so, the Dutch example demonstrates that this need not be the case.

5. CONCLUSIONS

In-depth accident investigation is the most direct way to measure the extent of the drinking and driving problem. The results are presented as the portion of accidents in which alcohol was a contributory factor. The number of accidents caused by alcohol can be indirectly estimated from the results of a case-control study. In addition, the results of such a study can be used to estimate this number from a BAC-distribution of accident-involved road users or of a random sample of road users. There is a need for more and better case control studies to improve the accuracy of such estimates.

If the opportunities for data collection are limited, a change in the extent of the drinking and driving problem may be studied by using surrogate measures for alcohol-related and non-alcohol-related accidents. The surrogate alcohol measure is usually some sub-group of serious nighttime accidents/casualties and the surrogate non-alcohol measure is some subgroup of serious daytime accidents/casualties. Better documentation of the choice of surrogate measures in future studies will hopefully lead to more uniformity.

Any important drinking-driving countermeasure should be evaluated on the basis of accident data in combination with other measures in order to delineate the process through which an effect on accidents was achieved. An evaluation study which does not include accident data, will not permit a straightforward conclusion about the effect the countermeasure has on accidents.

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Year*	1967	1968	1969	1970	1971
<u>Daytime</u>					
BAC > 9 mg%	22 %	13 %	16 %	18 %	20 %
BAC > 80 mg%	11 %	6 %	8 %	10 %	11 %
n	358	296	352	323	300
<u>Nighttime</u>					
BAC > 9 mg%	76 %	64 %	70 %	59 %	72 %
BAC > 80 mg%	52 %	40 %	53 %	44 %	58 %
n	181	100	107	125	143

* 1967 = December 1966 - September 1967, etc.

<u>Table 1.</u> BAC-distribution of motor vehicle drivers killed in England and Wales within 12 hours of an accident (Codling & Samson, 1974).

Year	1970	1971	1973	1975	1977
BAC > 20 mg%	22 %	28 %	31 %	20 %	23 %
BAC > 50 mg%	13 %	17 %	15 %	9 %	11 %
BAC > 100 mg%	5 %	8 %	5 %	3 %	4 %
non-response	14 %	13 %	18 %	11 %	15 %
n	1341	3417	2617	4039	3690

<u>Table 2.</u> BAC-distribution of weekend night drivers of passenger cars in The Netherlands (Noordzij, 1981).

Year*		1971	1972	1973	1974	1975	1976	1977	1978
<u>Daytime</u>	a	1679	1630	1612	1181	1247	1277	1327	1351
	b	142	138	136	100	106	108	112	114
<u>Nighttime</u>	a	432	480	489	448	295	360	370	332
	b	96	107	109	100	66	80	83	74

* 1971 = October 1970 - September 1971, etc.

<u>Table 3.</u> Number (a) and index (b; 1974 = 100) of fatal accidents with moving passenger cars in The Netherlands (Noordzij, 1981).