# A Study of Seatbelt Effectiveness Based on a Methodology for Analyzing General Categorical Data With Misclassification Errors

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## ABSTRACT

Most studies examining the effectiveness of seatbelts in reducing injury due to automobile accidents have been based on police-level data. Due to the circumstances surrounding the officer's investigation of the crash, such data generally contain misclassification errors relating to belt use and injury sustained, which can seriously bias any effectiveness estimates derived from that data.

In this paper, a methodology for analyzing general categorical data with misclassification errors is described and the procedure applied to the seatbelt effectiveness question. The technique utilizes an original large sample based on police-reported accidents together with a relatively small supplementary sample that is cross-classified by the police and by a more reliable classification mechanism.

The procedure is illustrated using police-reported North Carolina accidents for the first 8 months of 1975 as the original sample. The true classification of the supplementary sample of accidents is assumed to be obtained through hospital reports for injured occupants and through telephone interviews for the non-injured. Comparisons are then made of the belt-associated relative risks thus obtained.

## INTRODUCTION

The magnitude of the injury-reducing potential of safety belts has long been an issue of controversy. This is due to a variety of problems which make it difficult to obtain a definitive evaluation of safety belt effectiveness. These problems and the difficulties they impose have recently been examined by several researchers (such as Griffin, 1973; Mela, 1974; Kahane et al., 1975; and Hochberg, 1976).

One of the major problems discussed in Mela (1974) and in Hochberg (1976) is the bias in the estimates of safety belt effectiveness resulting from misclassification errors in the police reports of safety belt usage and/or degree of injury. These biases can seriously affect any inference about safety belt effectiveness.

The following discussion explores this problem a little further, based on the theoretical development given in chapter 4 of Hochberg (1976).

Consider a 2 × 2 table of proportions of belt usage (yes or no, say), by injury (yes or no, say). Let U, B, I, and N indicate "unbelted," "belted," "injured," and "not injured," respectively. Denote by $\pi(I,U)$ the true proportion of injured-unbelted occupants and similarly let $\pi(N,U)$, $\pi(N,B)$, and $\pi(I,B)$ represent the other (true) proportions. Theoretically, a total of 12 independent misclassification errors might arise when classifying individuals into such a table. Let $\alpha(I,U|N,B)$ denote the probability that the police will report an actually belted—not injured occupant as being injured and unbelted. Similar notation is used for the other 11 possible misclassification errors. Finally, the observed biased proportions based on police reports are denoted by $\gamma(\cdot,\cdot)$'s instead of $\pi(\cdot,\cdot)$'s.

Formulas that relate the $\gamma(\cdot,\cdot)$'s to the $\pi(\cdot,\cdot)$'s are easily derived; see, for example, Hochberg (1976). In that report, an effort was made to evaluate the resulting biases in estimates of safety belt effectiveness, based on the $\gamma(\cdot,\cdot)$'s corresponding to a range of some educated guesses that were then simulated for the actual values of the $\pi(\cdot,\cdot)$'s and the $\alpha(\cdot,|\cdot,\cdot)$'s. The setup in Hochberg (1976) was further simplified by the following two assumptions on the misclassification errors:

- In no case will an uninjured person be classified as injured.
- Probabilities of simultaneous errors in both characteristics are given by multiplying the corresponding one-way error probabilities, e.g.,

$$\alpha(I,B|N,U) = \alpha(N,B|N,U)\ \alpha(I,U|N,U).$$

Even under that limited setup and the restricted simulated values for the $\pi(\cdot,\cdot)$'s and $\alpha(\cdot,\cdot|\cdot,\cdot)$'s, it was noted (Hochberg, 1976) that:

- The bias in the resulting measures of effectiveness could be as high as 150 percent. For example, this is the case when "injured" indicates fatalities and one assumes:
  - 15-percent belt usage.
  - Probability of fatality when using the belt = .0025 and probability of fatality when not using the belt = .005.
  - $\alpha(N|I,B)$ = .10, $\alpha(B|N,U)$ = .15, $\alpha(U|I,B)$ = .05 and all other error probabilities equal .01.
- The range of values of the biases was very large (from −50 to +150 percent).

Thus, the main impact of that simulated study and of some pilot surveys (see Hochberg, 1976) was the definite need for a more reliable information source than merely the police accident reports for studying the effectiveness of safety belts.

What, then, are the major alternatives?

1. Draw inference on safety belt effectiveness entirely from police reported data. The main motivations for such an approach (that has been the prevailing one until recently) would be:
   - There is a large quantity of such data.
   - Maybe the biases are not large, either because of low probabilities for misclassification errors or because those errors interact in different directions so as to partially cancel one another.
2. Obtain an independent reliable sample by some better classification mechanism and base inference entirely on that sample. That approach is undertaken in Kahane, et al. (1975), where the reader should see some additional motivations for so doing in addition to reducing the effects of misclassification errors.

   One must be aware of the costs involved when adopting approach (2), because if estimates of belt effectiveness are to be entirely based on such a sample, its size must be sufficiently large to reach satisfactory accuracy. The risks of using approach (1) were detailed earlier.
3. Obtain a relatively small (in comparison to (2)) supplementary sample which must then be cross-classified by both police reports and some more reliable source. Then utilize the methodology in Hochberg (in press) (to be described in the sequel) to obtain statistically sound estimates based on the "large" police-reported data in conjunction with the "small" supplementary cross-classified sample.

The section on proposed methodology contains a description of the methodology developed by Hochberg (in press) in conjunction with this final alternative. In the third section, on the effectiveness of safety belts, the procedure is demonstrated utilizing actual data from North Carolina's accidents in the first 8 months of 1975 and a supplementary sample of hospital/telephone data. The fourth section contains a summary discussion.

## THE PROPOSED METHODOLOGY

The methodology for analyzing general misclassification categorical data presented in Hochberg (in press) makes further use of Tenenbein's (1970, 1971, 1972) double sampling scheme originally introduced for estimating the parameters of a multinomial distribution when misclassification errors prevail. The following experimental situation is assumed. There are two classification devices available. (The reader should not adhere to the mechanical connotation of the term "device.") One device is expensive to apply and gives "correct" results, while the other is relatively inexpensive but "fallible." The experimental setup referred to is very often met in reality in problems where the distinction between a true or a false classification device simply relates to making or not making an extra effort to obtain more reliable data.

Such experimental situations are frequently met by researchers in various domains of science. For example, Diamond and Lilienfeld (1962) discuss an experimental situation in public health research where the true classification device is the physician's examination whereas the fallible classifier is a questionnaire completed by the patient.

In real problems, it is often the case that the true classification device uses different scales than those used by the fallible device. The experimenter's knowledge of the degree of correspondence between the levels of two such scales may vary from none to complete. For the first example, a nominal scale for a patient's response to a questionnaire may have four levels, A, B, C, and D, while the physician's report may use some standard scale with levels 1, 2, 3, 4, 5, and 6. The correspondence between the patient's scale and the physician's scale may be clear (for example, A $\leftrightarrow$ (1 or 2), B $\leftrightarrow$ (3 or 4), C $\leftrightarrow$ 5, D $\leftrightarrow$ 6) or, as more often is the case, it may be quite unclear. Note that we refer to correspondence between these scales as implied by the a priori definitions of the scales and their levels. Even in cases where such a relation is completely known, fixed bias errors of misclassification may very well prevail.

The procedures to be discussed here have a double motivation in such experimental situations. First, they can be used to resolve the problems of misclassification errors. Secondly, even when misclassification errors do not exist, the procedures enable one to carry out an efficient study expressing results in terms of the finer scale utilized in the relatively small supplementary sub-sample.

The setup considered in Hochberg (in press) is general in the sense that either some or all the variables under study can be subject to misclassification errors, and the original contingency table can be of any dimensions.

In our specific application, the "fallible" classification device is the police-reported information and the "true" classification derives from hospital reports for injured occupants and intensive telephone interviews of the non-injured. The question of interest is that of the effectiveness of wearing "lap only" versus "none" and of "lap and shoulder" versus "lap only" in U.S. and in foreign cars.

The first sample consists of all police-reported accidents in North Carolina during the first 8 months of 1975. The second, more reliable sample, was cross-classified by the two classification devices on the variables: belt use and injury. All other variables under study (such as, model year of car, sex of injured occupant, and type of accident) were assumed to have been correctly reported by the police.

Let $j_1$ and $j_2$ index the police-reported status of belt use (none, lap only, lap and shoulder) and of injury (not injured, injured), respectively, and let $\underset{\sim}{j} = (j_1, j_2)$. Similarly, let $i_1$, $i_2$ index the "true" levels of belt use and of injury, respectively, as reported by the more reliable source, and put $\underset{\sim}{i} = (i_1, i_2)$. Finally, let $\underset{\sim}{\ell}$ index the specific combination of levels of all the variables under study that are assumed to be correctly reported by the police. In our example of the third section, $\underset{\sim}{\ell}$ assumes only two values, namely, "U.S. cars" or "Foreign cars," due to data limitations. To denote combinations of these multiple indices we simply adjoin them, for example, $(\underset{\sim}{i}\underset{\sim}{j})$ indexes a specific combination of police-reported levels for belt use and injury and of non-police-reported levels for these two variables.

Let $N(\underset{\sim}{j}\underset{\sim}{\ell})$ denote the first sample frequency of occupants with levels $\underset{\sim}{j}$ on belt usage and injury and level $\underset{\sim}{\ell}$ on type of car. Similarly, let $n(\underset{\sim}{i}\underset{\sim}{j}\underset{\sim}{\ell})$ denote the second sample frequency of occupants reported as having injury levels $\underset{\sim}{j}$ and car types $\underset{\sim}{\ell}$ by the police while the "true" classification for belt use and injury was given by $\underset{\sim}{i}$. The corresponding unknown population proportions are denoted by $\alpha(\underset{\sim}{j}\underset{\sim}{\ell})$ and $\beta(\underset{\sim}{i}\underset{\sim}{j}\underset{\sim}{\ell})$, respectively. To denote quantities computed from marginal tables (such as, row and column tables), we omit the unnecessary indices: $\alpha(\underset{\sim}{\ell})$ denotes the sum of all $\alpha(\underset{\sim}{j}\underset{\sim}{\ell})$ across all levels $\underset{\sim}{j}$.

The intermediate parameters of interest

are the $\alpha(\underset{\sim}{i\ell})$ that describe the overall true distribution of occupants involved in accidents across the levels of (belt usage) × (injury) × (type of car). The first phase of inference in the methodology of Hochberg (in press) amounts to obtaining efficient estimators of the $\alpha(\underset{\sim}{i\ell})$ and the approximate common distribution of these estimators. In the second phase of inference, these estimators are further analyzed to obtain true estimates of safety belt effectiveness across levels $\underset{\sim}{\ell}$ of car type.

We now outline these two phases of inference. As noted earlier, in the first phase of inference, "efficient" estimators for $\alpha(\underset{\sim}{i\ell})$ are obtained. By that we mean Maximum Likelihood (ML) estimators or some other estimators that are asymptotically equivalent to the ML estimators. One class of estimators that are asymptotically equivalent to the ML's is that based on asymptotic weighted least squares (LS) as in Grizzle et al. (1969).

Since ML estimators are considered most efficient in our setup, and since we have found their use in these problems more convenient than that of the LS principle, we have used only them in our applications.

The ML estimators $\hat{\alpha}(\underset{\sim}{i\ell})$ of the $\alpha(\underset{\sim}{i\ell})$ are obtained by summing across all levels of $\underset{\sim}{j}$ the ML estimators of the $\beta(\underset{\sim}{ij\ell})$ which are given by:

$$\hat{\beta}(\underset{\sim}{ij\ell}) = \frac{N(\underset{\sim}{j\ell}) + n(\underset{\sim}{j\ell})}{N + n} \cdot \frac{n(\underset{\sim}{ij\ell})}{n(\underset{\sim}{j\ell})},$$

where N and n are the total sample sizes of the first and second samples, respectively.

The asymptotic distribution of the $\hat{\alpha}(\underset{\sim}{i\ell})$ is multivariate normal (being ML estimators).

Having obtained the $\hat{\alpha}(\underset{\sim}{i\ell})$, one must be able to produce a consistent estimator of their covariance matrix in order to proceed into the second phase of inference. The determination of the variance matrix of the $\hat{\alpha}(\underset{\sim}{i\ell})$ for both the ML and LS approaches are detailed in Hochberg (in press).

## The "True" Effectiveness of Safety Belts for American and Foreign Cars: An Example

The data for this example is given in tables 1 and 2. The total number of observations for the first sample is 81 617. These were available with no extra effort. (Note that "injury" in table 1 refers to the standard police K, A, B, C, 0 scale.)

Table 2 presents belt use and injury information for the supplementary sample of 2 372 occupants, cross-classified by police and non-police data sources. Note that within each cell of the table, the cases falling along the diagonal (from top left to bottom right) reflect agreement between the two sources of belt information. Those falling above or below the diagonal represent disagreement. The results generally indicate that the police are more likely to report "no belt" and less likely to report "lap belt" or "lap and shoulder belt" in comparison with the response obtained via the hospital or the telephone interview. (Note that the "non-police"-reported injury refers to the AIS scale.)

If only the police-reported data were used for analysis, the resulting estimated risks and effectiveness would be as in table 3. However, if the methodology proposed in the second section of this paper is adopted in order to utilize the true classification of occupants in the "second" sample, then the estimated risks and effectiveness are as displayed in table 4.

Table 1. Police-reported frequencies of occupants in North Carolina's accidents during the first 8 months of 1975

| | Belt use | | | | | |
|---|---|---|---|---|---|---|
| | U.S. | | | Foreign | | |
| Injury | None | Lap only | Lap and shoulder | None | Lap only | Lap and shoulder |
| Injured | 11 546 | 1 074 | 241 | 1 700 | 189 | 74 |
| Not injured | 52 139 | 6 502 | 1 664 | 5 369 | 691 | 428 |

Table 2. Police and nonpolice cross-classification of injury and belt use,[a] controlling for car type

| Police | | Nonpolice | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | U.S. Cars | | | | | | Foreign Cars | | | | | |
| | | Not Injured | | | Injured | | | Not Injured | | | Injured | | |
| | | U | L | LS | U | L | LS | U | L | LS | U | L | LS |
| Not injured: | U | 1 086 | 130 | 52 | 197 | 26 | 7 | 101 | 11 | 21 | 29 | 2 | 2 |
| | L | 13 | 120 | 24 | 4 | 18 | 1 | 1 | 13 | 6 | 1 | 1 | 0 |
| | LS | 0 | 14 | 55 | 0 | 1 | 6 | 1 | 1 | 21 | 0 | 2 | 4 |
| Injured: | U | 23 | 5 | 1 | 273 | 7 | 3 | 5 | 1 | 0 | 32 | 3 | 2 |
| | L | 2 | 3 | 0 | 3 | 20 | 3 | 0 | 0 | 1 | 0 | 1 | 0 |
| | LS | 1 | 0 | 1 | 1 | 3 | 7 | 0 | 0 | 0 | 1 | 1 | 2 |

[a] U = Unbelted
L = Lap belt
LS = Lap and shoulder belt

Table 3. Estimated risks and effectiveness[a] based on police-reported data only

| Car make | Belt use[b] | | | | | |
|---|---|---|---|---|---|---|
| | U.S. | | | Foreign | | |
| | U | L | LS | U | L | LS |
| Injured (%) | 18.13 | 14.18 | 12.65 | 24.05 | 21.48 | 14.74 |
| STD[c] | .15 | .40 | .76 | .51 | 1.38 | 1.58 |
| Effectiveness | – | 21.81 | 10.76 | – | 10.69 | 31.36 |
| STD[c] | – | 2.31 | 5.94 | – | 6.06 | 8.59 |

[a]Effectiveness of "L" = $100 \times \frac{\text{\% injury for "N"} - \text{\% injury for "L"}}{\text{\% injury for "N"}}$

Effectiveness of "LS" = $100 \times \frac{\text{\% injury for "L"} - \text{\% injury for "LS"}}{\text{\% injury for "L"}}$

[b]U = Unbelted; L = Lap belt; LS = Lap and shoulder belt
[c]STD = Standard Deviation of Estimate

If the estimated risks and effectiveness of table 4 are the true ones, then the bias in the police-based estimates is quite substantial. Hence, the low Standard Deviation of Estimates (STD's) of the estimates in table 3 are not true indicators of accuracy. Rather than looking at the STD's, we must compute the mean square error (MSE) of each estimator (MSE = variance + $(\text{bias})^2$). In doing so for the police-reported data, we find that these are too large and thus do not enable very accurate statistical statements on belt effectiveness.

Unfortunately, due to the small size of our supplementary sample, this limitation also applies when attempting statistical valid inferences from table 4. This is further discussed in the following summary section.

Table 4. Estimated risks and effectiveness[a] based on the two-sample methodology

| Car make | Belt use[b] | | | | | |
|---|---|---|---|---|---|---|
| | U.S. | | | Foreign | | |
| | U | L | LS | U | L | LS |
| Injured (%) | 30.50 | 21.33 | 16.26 | 38.99 | 29.77 | 16.79 |
| STD[c] | .90 | 2.05 | 2.86 | 3.20 | 7.46 | 4.95 |
| Effectiveness | – | 30.10 | 23.77 | – | 23.64 | 43.62 |
| STD[c] | – | 7.11 | 15.66 | – | 20.69 | 22.36 |

[a]Effectiveness of "L" = $100 \times \frac{\text{\% injury for "N"} - \text{\% injury for "L"}}{\text{\% injury for "N"}}$

Effectiveness of "LS" = $100 \times \frac{\text{\% injury for "L"} - \text{\% injury for "LS"}}{\text{\% injury for "L"}}$

[b]U = Unbelted; L = Lap belt; LS = Lap and shoulder belt
[c]STD = Standard Deviation of Estimate

## DISCUSSION

As we saw in the third section, few conclusive statements can be made regarding safety belt effectiveness as a result of the investigation. This is due to the large standard deviations (STD's) of the estimates which, in turn, are partially due to the size of the supplementary sample. As noted earlier, the supplementary sample used to demonstrate the methodology presented in this paper consisted of only 2 372 occupants. As HSRC discovered, it was no little task to collect the supplementary hospital and telephone interview information on even this (relatively) small sample size.

It now appears that, in order to make statistically significant statements on safety belt effectiveness using this two-sample methodology, one should probably have (roughly speaking) a three-fold or four-fold size supplementary sample. Thus, the data presented in this paper should be regarded primarily as a tool to demonstrate a new technique, rather than as decisive evidence of safety belt effectiveness.

It should be noted, at this point, that the sample size also limited the extent to which the data could be broken down during analysis. For the purposes of this paper, the data on injury level and belt use were broken down by only one variable—car type (U.S. versus foreign). The small sample size precluded the possibility of studying the effects of any two or three of these variables simultaneously.

Similar problems of low accuracies for belt effectiveness when the data are broken down by several factors of interest were encountered in the Restraint Systems Evaluation Project (RSEP) (Reinfurt et al., 1976). In this study, seatbelt effectiveness was evaluated based on a single sample of 15 818 weighted occupants involved in towaway crashes. The data were of a "Level II" nature, with special emphasis placed on obtaining accurate measures of safety belt use.

While increasing the size of the supplementary sample will improve the accuracy of the belt effectiveness estimates based on the two-sample methodology, additional research is needed to further improve upon the technique. More specifically, research is needed to incorporate smoothing models for the entries in the supplementary sample, based on (hopefully) only a few parameters for the misclassification errors. The methodology as it now stands does not allow for using model-predicted estimates of the frequencies in the supplementary cross-classified sample prior to "merging it statistically" with the original sample.

It is very reasonable to expect that the very large number of misclassification errors (that introduce too many degrees of freedom in the procedures described) could be structured by an appropriate statistical model, resulting in lower STD's for the predicted frequencies. The author hopes to be able to carry out this research in the near future.

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# SEMINAR THREE
## ACCIDENT AVOIDANCE

## A New Approach to Vehicle Dynamic Analysis of Severe Steering and Braking Inputs

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### INTRODUCTION

In the United States in 1974 3.9 percent of all accidents involving 14 percent of all fatal accidents concerned vehicles overturning without a preceding collision on the roadway [1]. This demonstrates that such accidents belong to that group characterized by potentially severe occupant injuries.

A high percentage of these occurrences transpired after the vehicle had left the road, whereas approximately one-sixth were caused by forces generated at the tire/road interface. It is probable that a large percentage of such accidents can be attributed to tripping mechanisms of various types.

The low frequency of vehicle dynamics-related upsets indicate that upset resistance requirements must not be permitted to detrimentally affect other dynamic properties of a motor vehicle, such as responsiveness and accident avoidance capabilities.

This paper describes possible procedures for the evaluation of vehicle behavior prior to an upset due to the friction between the tire and road surfaces. Suitable test methods are also described, including a closed-loop method and an open-loop method. The results are merely intended to demonstrate the practicability of the test procedures described and not to analyze the performance of a specific vehicle or vehicle type. Both open- and closed-loop methods were used because each approach has its particular advantages. With the aid of the closed-loop method, it is possible to obtain information about operating functions such as steering, braking and accelerating which can cause the upset, without major test expenditures. The open-loop method offers, in addition to this, the opportunity for a systematic investigation of vehicle reactions to reproducible operating functions developed in the closed-loop tests.

### SELECTION OF UPSET-RELATED DRIVING MANEUVERS

Vehicle upsets on the road are usually caused by collisions with other vehicles or obstacles, or by mechanical tripping. The least frequent cause is the effect of tire side forces.

The test procedures for vehicle upset resistance related to vehicle handling characteristics should not include on-road collision situations with subsequent upset. Furthermore, steering and/or brake maneuvers in upset resistance tests should be within the space limits given by the dimensions of roads, and should not be disproportionate to prudent driving practices under given conditions.

Three types of steering/braking input maneuvers are felt to be useful because they are assumed to reflect realistic situations [2]. At this time there are no relevant statistics available.

- Severe lane change maneuver with sinusoidal steering input
- Sinusoidal steering input maneuver during cornering at a given lateral acceleration

- Drastic steering and braking input maneuvers

## Severe Lane Change Maneuver

The severe lane change maneuver with sinusoidal steering input is a sudden maneuver to avoid obstacles on a straight course. The individual phases in this maneuver are:

- Driver recognition of the situation;
- Driver steering input to initiate avoidance maneuver;
- Corrective steering input to resume previous course and vehicle directional control.

The steering input is the simplified configuration of a sine wave. Figure 1 shows the individual phases of such a manevuer within a sine wave.

## Sinusoidal Steering Input During Cornering

The sinusoidal steering maneuver during cornering is an obstacle avoidance maneuver performed in a steady-state turn, with the avoidance course being directed toward the inside of the turn. The individual phases are similar to those in the obstacle avoidance maneuver conducted on a straight course with the exception of the prevailing steering input when the vehicle entered the turn. It is assumed that the driver applies steering input during the steady-state constant radius driving pattern without having changed the position of his hands on the wheel. This assumption, however, does not necessarily reflect driver behavior under actual operating conditions on the highway.

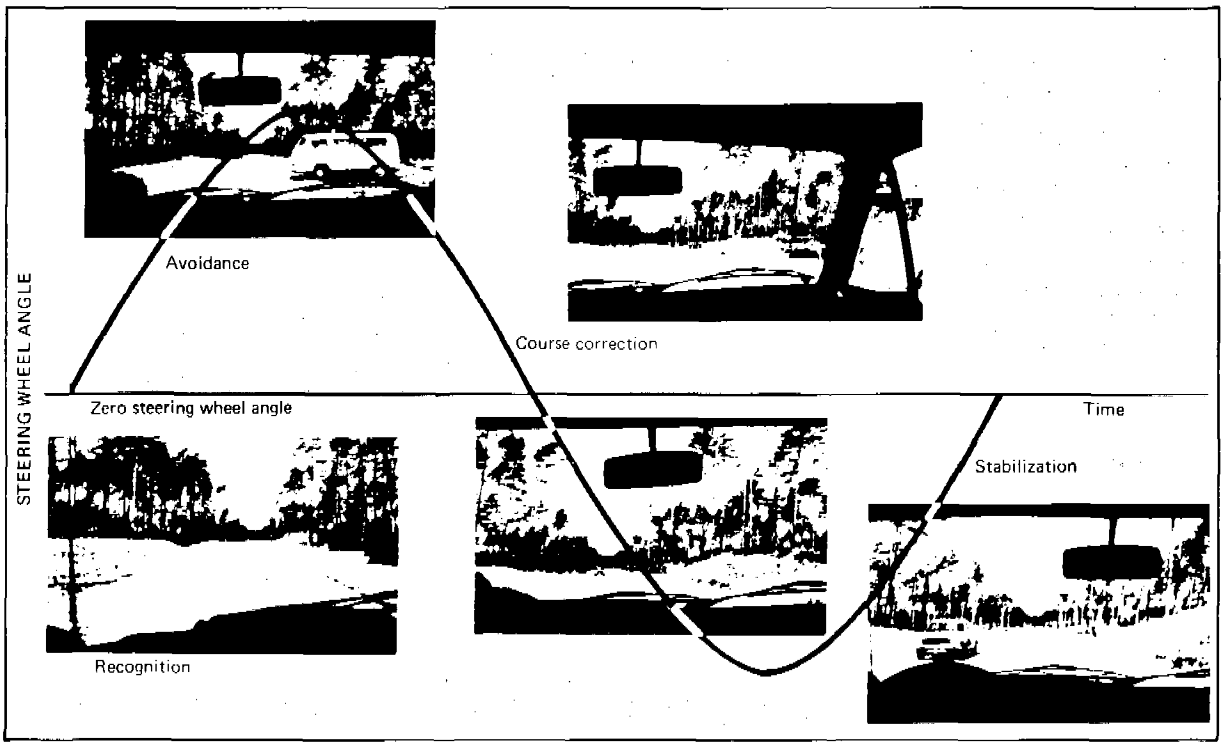
Figure 2 shows the individual phases and the simplified steering-angle input during such a maneuver.

This type of maneuver was conducted only in open-loop testing within the scope of this study.

## Drastic Steering and Brake Maneuver

The drastic steering and braking input maneuver is a sudden obstacle avoidance maneuver that may have to be performed on highways. This maneuver requires sudden reduction of the vehicle velocity to avoid a

Figure 1. Phases of avoidance maneuver on straight road, simplified steering input.



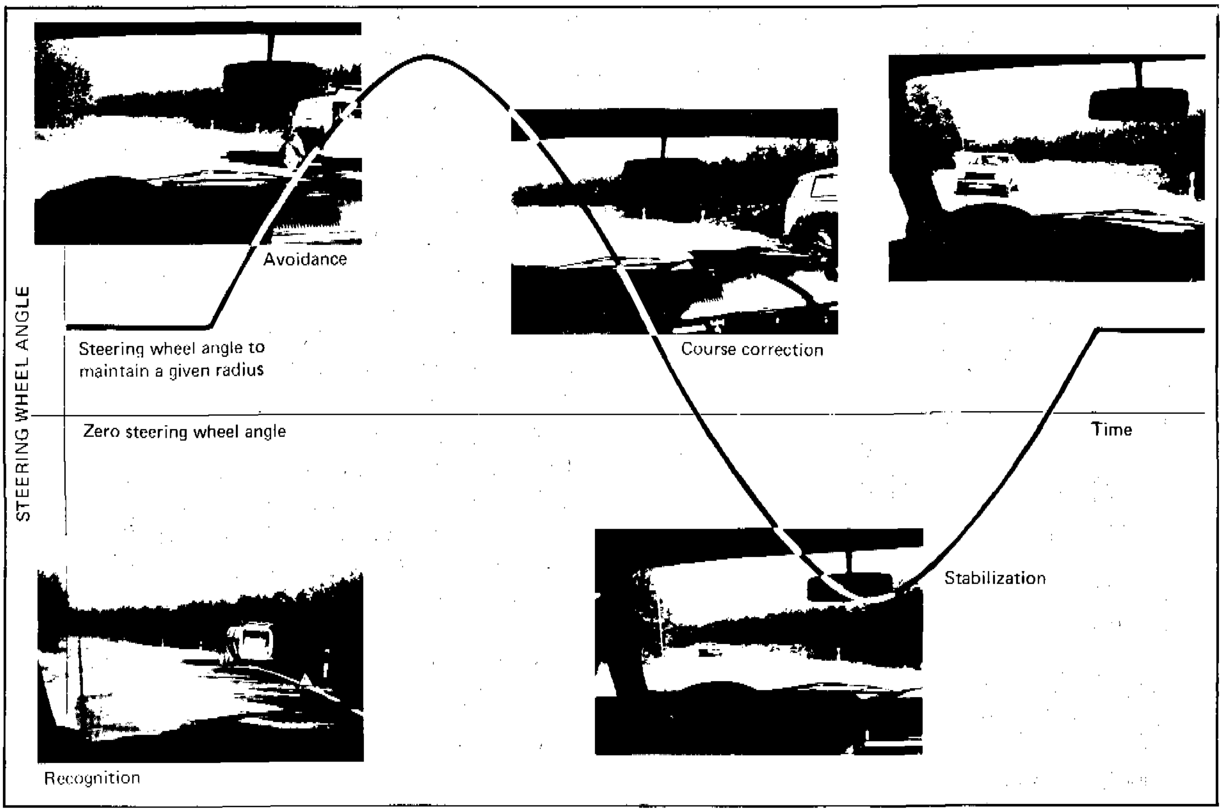


Figure 2. Phases of avoidance maneuver while cornering, simplified steering input.

collision. The vehicle's intended course is obstructed by obstacles or other vehicles. Figure 3 shows the individual phases of the maneuver with the simplified time histories of input functions.

The closed-loop tests have been performed in such a way that the brakes were applied and released during a sinusoidal sequence of steering inputs.

## METHODS FOR ANALYZING STEERING/BRAKING MANEUVERS

Two basic methods can be employed for the experimental evaluation of the maneuvers described previously:

- The closed-loop method, in which the driver translates information according to the driving task and situation into satisfactory driving inputs
- The open-loop method, in which vehicle reaction is evaluated in light of definitely reproducible driving inputs

### Closed-Loop Method

Closed-loop tests producing upsets were performed with vehicles of various types and sizes by experienced test engineers. The test cars were in standard condition except for some modifications made for driver protection. All vehicles were equipped with outriggers. Two typical maneuvers were performed:

- Straight approach, followed by a sinusoidal sequence of steering inputs (steering oscillation), similar to the method described above
- Straight approach, followed by a sinusoidal sequence of steering inputs with brake-lock and release during the second half of the sine wave, as described above

### Open-Loop Method

**Programmable Driving Machine.** A programmable driving machine was developed for the experimental verification of the open-

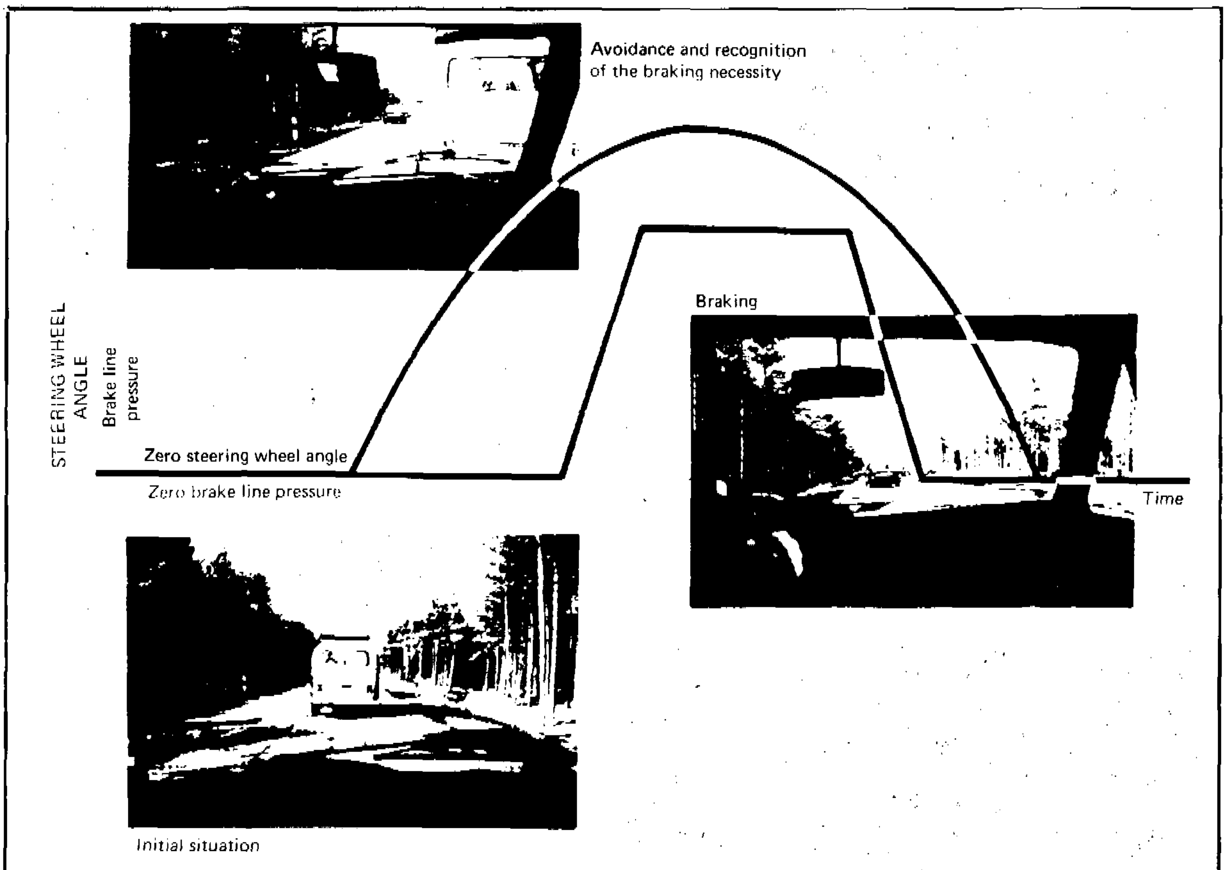



Figure 3. Phases of a simulated drastic steer and brake maneuver, simplified steering and braking input.

loop method. The use of the driving machines in motor vehicle driving tests is useful and indicated in the following situations:

- When driver influence is to be eliminated as a factor, or when driver input, perception, or behavior is not necessary for the evaluation of vehicle performance
- When good test reproducibility by drivers cannot be achieved for the formulated test purpose
- When driver safety can be compromised

As the driving machine replaces the driver, the machine must be able to drive (accelerate), steer, and brake the vehicle. Furthermore,

- The weight of the driving machine should not substantially exceed the weight of a driver plus seat
- The configuration, dimensions, and adjustment devices of the machine must be so designed that the machine can be exchanged quickly and easily between vehicles
- The performance capability (operating forces and speeds) should exceed those of a driver under the circumstances of the test
- The vehicle velocity should be fairly variable between 0 and 100 km/h, and the machine should be able to maintain a constant vehicle velocity during the actual vehicle dynamics test. The adjustment speed of the throttle should be adapted to the longitudinal vehicle dynamics.

All these requirements are met by the system developed by Volkswagenwerk AG. Figures 4 and 5 show the entire test apparatus consisting of vehicle, on-board equipment, and control stand.

The driving machine is remote-controlled by radio to start and maneuver the vehicle. Once a given test velocity has been reached,