TRANSPORTATION RESEARCH RECORD 1068

Traffic Accident Analysis, Alcohol Involvement, and Evaluation of Highway Improvements

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TRANSPORTATION RESEARCH BOARD
NATIONAL RESEARCH COUNCIL
WASHINGTON, D.C. 1986

Transportation Research Record 1068
Price $15.00
Editor: Edythe Traylor Crump
Compositor: Harlow A. Bickford
Layout: Theresa L. Johnson

mode
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Printed in the United States of America

Library of Congress Cataloging-in-Publication Data
National Research Council. Transportation Research Board.

Traffic accident analysis, alcohol involvement, and evaluation of highway improvements.

(Transportation research record, ISSN 0361-1981 ; 1068)
1. Traffic accidents–United States. 2. Drinking and traffic accidents–United States. 3. Traffic safety–United States. I. National Research Council (U.S.). Transportation Research Board. II. Series.
TE7.H5 no. 1068 380.5 s 86-23555
[HE5614.2] [363.1'251]
ISBN 0-309-04062-0

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Costs of Motor Vehicle Accidents and Injuries

JOHN B. ROLLINS and WILLIAM F. McFARLAND

ABSTRACT

Motor vehicle accident costs are an important component in benefit-cost evaluations of highway safety improvements. A recent study by Miller et al. for the Federal Highway Administration evaluated various approaches to accident cost estimation and presented state-of-the-art societal costs of motor vehicle accidents, based largely on a 1983 accident cost study by the National Highway Traffic Safety Administration. The principal shortcoming of the Miller et al. study is its failure to express accident costs in a form that can be directly used with state accident data in benefit-cost calculations. The objective of this paper is to develop accident costs that can be used directly with state accident data in benefit-cost evaluations of highway improvements. The costs in Miller et al., which were expressed in per-victim and per-vehicle terms, provide the basis for the per-accident costs developed in this paper. These accident costs are based on accident severities and on the A-B-C injury severity scale commonly used in state accident records, rather than on the Maximum Abbreviated Injury Scale (MAIS) used by NHTSA and Miller et al. Accident data from five states are used in deriving the accident costs. Data from the National Crash Severity Study (NCSS) and the National Accident Sampling System (NASS) are used to relate percentage distributions of injury severities by the MAIS and A-B-C scales. The accident costs presented in this paper can be used directly with state accident data, thereby facilitating the use of state-of-the-art accident cost estimates in benefit-cost analyses of highway improvements.

A major problem faced by administrators is how to allocate limited highway safety funds to achieve the maximum reduction in fatalities, injuries, and property damage resulting from motor vehicle accidents. Recognition of this problem has led to the development of advanced benefit-cost techniques for comparing the expected benefits and costs of various funding alternatives. Of central importance in benefit-cost evaluations of alternatives is the accurate estimation of motor vehicle accident costs.

Considerable effort has been devoted to developing accident costs. One of the most recent such studies by Miller et al. for the Federal Highway Administration (1) evaluated various approaches to accident cost estimation and presented what appear to be the best available societal costs of motor vehicle accidents.

The principal shortcoming of this study is its failure to express accident costs in a form that can be directly used with state accident data in benefit-cost calculations. Costs are expressed on a per-victim and per-vehicle basis, rather than on a per-accident basis, and are presented in terms of the Maximum Abbreviated Injury Scale (MAIS). However, benefit-cost analyses often are based on a state's accident data, which typically consist of numbers of accidents per year at various accident locations, with injury severities coded by the A-B-C scale (incapacitating, nonincapacitating, and possible injury, respectively) rather than by the MAIS (0, no injury; 1 to 5, least to most severe nonfatal injury; 6, fatality). Hence, costs such as those presented by Miller et al. (1) cannot be directly applied to state accident data and, therefore, may well be largely ignored in state traffic safety programs.

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The objective of this paper is to develop accident costs that can be directly used in benefit-cost studies with state accident data. Based on the values presented by Miller (1), the accident costs presented here were calculated by using methods previously developed in a study for FHWA (2). During the course of deriving these accident costs, a method for relating MAIS injury severities to the A-B-C scale is presented.

DATA SOURCES

The costs used to develop accident costs in this paper were taken from Miller et al. (1). The costs presented in that report were based largely on societal costs of accidents in an updated report by the National Highway Traffic Safety Administration (NHTSA) (3) and on a study by Hartunian et al. (4). Direct, indirect, and total costs from the study by Miller et al. (1) are summarized in Table 1. Specific components of direct and indirect costs are detailed in the study by Miller et al. (1) and in the NHTSA report (3).

The accident data used in estimating accident costs were compiled from accident records from five states (2): Alabama (90,163 accidents in 1980), Montana (18,185 accidents in 1979), North Carolina (94,366 accidents in 1979-1980), North Dakota (9,340 accidents in 1979), and Texas (627,166 accidents in 1978-1979). These particular states were selected because they responded to a request from FHWA to all states to provide data for a study (2). The data were combined into a single data set on the basis of the annual number of accidents in each state. The data base included such information as the numbers of vehicles per accident (passenger cars and trucks) in Table 2, accident proportions by severity for accidents in rural and urban areas in Table 3, and

TABLE 1 Costs by MAIS Categories (1980 dollars) (*1*, Tables 36-38)

Type of Cost	Cost per Victim (MAIS Categories) 0 (PDO)[a] ($)	1 ($)	2 ($)	3 ($)	4 ($)	5 ($)	6 (Fatality) ($)
Direct[b]	716	1,601	3,442	8,089	18,467	138,684	18,294
Indirect[c]	132	690	1,165	2,217	32,564	122,897	724,227
Total	848	2,291	4,607	10,306	51,031	261,581	742,521

[a]Costs per vehicle in reported property-damage-only (PDO) accidents.
[b]Direct costs include property damage, medical, legal, and funeral costs.
[c]Indirect costs include administrative costs, human capital costs (lost productivity) for injuries, and for a fatality, human capital costs adjusted for individuals' willingness-to-pay to reduce their risk of death or injury.

TABLE 2 Vehicle Involvements per Accident, Five States Combined (*2*)

Area	Accident Severity: Fatal	Injury	PDO	Average
Rural	1.3930	1.4264	1.5307	1.4901
Urban	1.4316	1.5399	1.7918	1.7392

Note: Alabama, Montana, North Carolina, North Dakota, and Texas are combined.

TABLE 3 Accident Proportions by Severity, Five States Combined (*2*)

Area	Accident Severity: Fatal	Injury	PDO
Rural	0.0160	0.3497	0.6343
Urban	0.0045	0.2458	0.7497

Note: Alabama, Montana, North Carolina, North Dakota, and Texas are combined.

numbers of fatalities and A-B-C injuries per accident in Table 4. Of course, to the extent that states differ in how data such as injury severities and rural-urban areas are coded in their accident records, the accuracy of the accident costs developed in this paper may be affected.

TABLE 4 Fatalities and Injuries per Accident, Five States Combined (*2*)

Accident Severity and Area	Number per Accident: Fatalities	A Injuries	B Injuries	C Injuries
Fatal				
Rural	1.1516	0.5315	0.3173	0.1396
Urban	1.0862	0.3528	0.3015	0.1298
All	1.1272	0.4648	0.3114	0.1359
Injury				
Rural	–	0.3457	0.5770	0.6027
Urban	–	0.1883	0.5990	0.6575
All	–	0.2516	0.5902	0.6355

Note: Alabama, Montana, North Carolina, North Dakota, and Texas are combined.

For relating MAIS injuries to the A-B-C scale, data were obtained from the National Crash Severity Study (NCSS) for 1977-1978 and the National Accident Sampling System (NASS) for 1979-1980. These two data sets included injuries cross-classified by the MAIS and A-B-C scales. The NCSS data set was used for injuries in fatal accidents because it had a larger sample of injuries in fatal accidents than did the NASS data. The NASS data set, with a larger sample of injuries in nonfatal injury accidents, was used for injuries in nonfatal injury accidents (2).

COST PER PROPERTY-DAMAGE-ONLY ACCIDENT

The cost per property-damage-only (PDO) accident can be readily calculated from the costs per vehicle involvement in Table 1 and the average number of involvements per PDO accident in Table 2. Direct, indirect, and total costs per PDO accident in rural and urban areas are as follows:

Direct cost = Direct cost per involvement x Involvements per accident = $716 x 1.5307 = $1,096 per rural PDO accident.

Direct cost = $716 x 1.7918 = $1,283 per urban PDO accident.

Indirect cost = Indirect cost per involvement x Involvements per accident = $132 x 1.5307 = $202 per rural PDO accident.

Indirect cost = $132 x 1.7918 = $236 per urban PDO accident.

Total cost = Total cost per involvement x Involvements per accident = $848 x 1.5307 = $1,298 per rural PDO accident.

Total cost = $848 x 1.7918 = $1,519 per urban PDO accident,

or, alternatively, total costs can be estimated as

Total cost = Direct cost + Indirect cost = $1,096 + $202 = $1,298 per rural PDO accident.

Total cost = $1,283 + $236 = $1,519 per urban PDO accident.

The difference in the costs per PDO accident is due to the greater number of involvements per PDO accident in urban areas than in rural areas. To the extent that the costs per involvement in rural and urban areas differ from the average involvement cost of $848 reported by Miller et al. (1), the estimated costs per PDO accident shown here over- or understate the actual cost per involvement by population area. Similarly, all of the accident costs developed here contain some inaccuracy arising from the fact that the source costs reported by Miller et al. (1) are not differentiated by rural and urban areas.

COST PER A-B-C INJURY

Because state accident records typically use the A-B-C scale for coding the severities of nonfatal

injuries, the MAIS scale cannot be used directly with state accident data in benefit-cost analyses. Therefore, a method was devised for relating the percentage distribution of MAIS severities to that of A-B-C severities (2).

This was done by using NCSS and NASS data on injury severities cross-classified by the MAIS and A-B-C scales. Tables 5 and 6 give the percentage distributions of injury severities by the two scales for injuries in fatal accidents and injuries in nonfatal injury accidents, respectively. It can be observed from these two tables that, in the NCSS and NASS sample data, some fraction of injuries coded as A, B, or C by investigating officers turned out to be no injury (MAIS-0) or, in other cases, fatalities (MAIS-6).

TABLE 5 Injuries in Fatal Accidents, Percentages Cross-Classified by A-B-C and MAIS Severities, Based on NCSS Sample

	A-B-C Scale			
MAIS	C (%)	B (%)	A (%)	Total (%)
0	0.30	0.30	0.00	0.60
1	5.86	17.90	14.99	38.75
2	0.75	5.86	13.51	20.12
3	0.60	3.90	19.21	23.71
4	0.30	1.05	9.16	10.51
5	0.00	0.15	5.86	6.01
6	0.00	0.00	0.30	0.30
Total	7.81	29.16	63.03	100.00

TABLE 6 Injuries in Injury Accidents, Percentages Cross-Classified by A-B-C and MAIS Severities, Based on NASS Sample

	A-B-C Scale			
MAIS	C (%)	B (%)	A (%)	Total (%)
0	2.84	0.46	0.07	3.37
1	32.45	30.38	6.08	68.91
2	2.97	7.36	6.67	17.00
3	0.82	2.94	4.70	8.46
4	0.04	0.36	1.25	1.65
5	0.00	0.16	0.42	0.58
6	0.00	0.00	0.03	0.03
Total	39.12	41.66	19.22	100.00

The data in Tables 5 and 6 were used in developing Figures 1 and 2, which can be used for relating MAIS severites to A-B-C severities for any state's percentage distribution of A-B-C injuries. In each figure, the cumulative percent of MAIS injury severities is plotted against the cumulative percent of A-B-C severities. For example, in Table 5, it is observed that C injuries accounted for 7.81 percent of all injuries in fatal accidents in the NCSS sample, with 0.30 percent of all injuries that were coded as C severity turning out to be MAIS-0 severity, 5.86 percent coded as C turning out to be MAIS-1, 0.75 percent coded as C turning out to be MAIS-2, and so forth. In Figure 1, the curves pass through points corresponding to these MAIS values on the ordinate for 7.81 percent C injuries on the abscissa.

Similarly, MAIS cumulative percentages from Table 5 (e.g., for MAIS-1, 17.90 percent + 5.86 percent = 23.76 percent) corresponding to B plus C injuries are plotted on the ordinate in Figure 1 for cumulative B plus C injuries on the abscissa (29.16 percent + 7.81 percent = 36.97 percent of all injuries in fatal accidents). The MAIS cumulative percentages

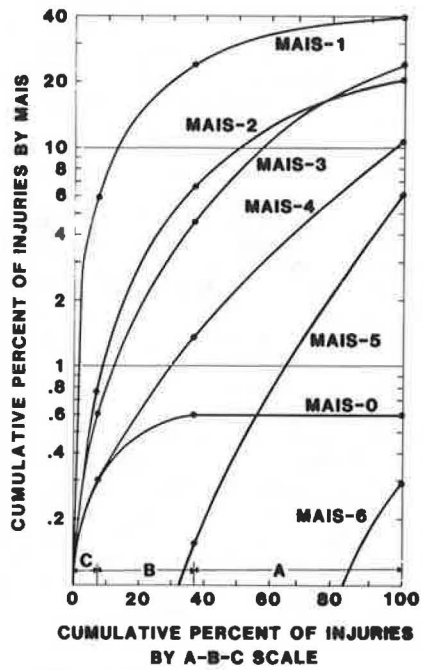

FIGURE 1 Cumulative percent of injuries by MAIS versus cumulative percent by A-B-C scale, injuries in fatal accidents, NCSS sample.

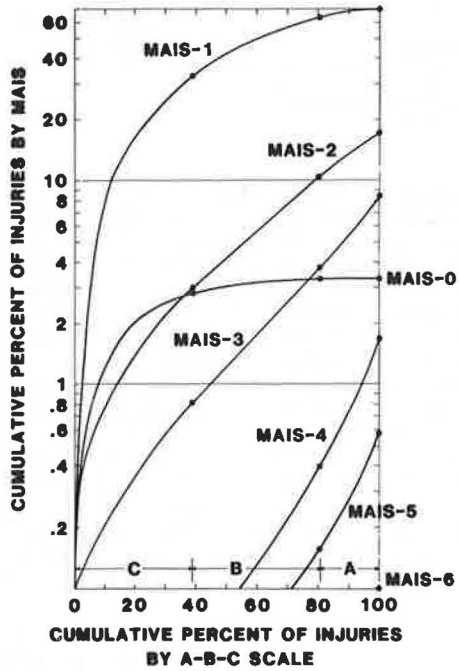

FIGURE 2 Cumulative percent of injuries by MAIS versus cumulative percent by A-B-C scale, injuries in injury accidents, NAS sample.

for all injuries (e.g., for MAIS-1, 14.99 percent + 17.90 percent + 5.86 percent = 38.75 percent) corresponding to A plus B plus C injuries are plotted for cumulative A plus B plus C injuries (63.03 percent + 29.16 percent + 7.81 percent = 100 percent of all injuries in fatal accidents in the NCSS sample).

Figure 2 was developed in a similar fashion for injuries in nonfatal injury accidents, using the NASS data in Table 6. The curves in Figures 1 and 2

were fitted through the four sets of points (origin, percent C, percent B+C, and percent A+B+C) in such a way that the vertical sum of the curves at any point of cumulative A-B-C injuries on the abscissa equals the corresponding cumulative percentage of MAIS severities.

Percentages by MAIS severities can be read from Figures 1 and 2 for any cumulative percentages by A-B-C severities from state accident data, establishing weights to apply to the costs of MAIS injuries and thereby producing the costs of A, B, and C injuries. From data for the five states combined, the percentage distributions of A, B, and C injuries in fatal accidents and in nonfatal injury accidents are given in Table 7. Costs per A, B, and C injury are estimated by obtaining percentages by MAIS severities corresponding to the A, B, and C percentages in Table 7 and then applying these weights to the direct and indirect costs by MAIS category in Table 1 (with adjustments for property damage per accident, as explained in the following paragraph). The costs per injury cannot be calculated separately for rural and urban accidents because this distinction was not available in the NCSS and NASS data used in developing Figures 1 and 2.

TABLE 7 Percentage Distribution of A-B-C Severities, Five States Combined

Accident Severity	Percentage Distribution of Injury Severities		
	A (%)	B (%)	C (%)
Fatal	50.96	34.14	14.90
Injury	17.03	39.95	43.02

Note: Alabama, Montana, North Carolina, North Dakota, and Texas are combined.
Source: Derived from Table 4.

The procedure for estimating the costs of A, B, and C injuries for injuries in fatal accidents and in injury accidents is as follows. From Table 7, the percentage distribution of A, B, and C injuries in fatal accidents is 50.96 percent, 34.14 percent, and 14.90 percent, respectively, whereas that of injuries in injury accidents is 17.03 percent, 39.95 percent, and 43.02 percent. From Figure 1, the MAIS percentages corresponding to 14.12 percent C, 46.22 percent B+C (equal to 32.10 percent + 14.12 percent), and 100 percent A+B+C (equal to 53.78 percent + 32.10 percent + 14.12 percent) for fatal accidents are given in Table 8, with a similar distribution for injuries in injury accidents derived by using Figure 2. For each MAIS category, the percentages of A and B severities are obtained by subtraction (B+C percentage - C percentage = B percentage, and A+B+C percentage - B+C percentage = A percentage). These percentages for A, B, and C severities given in Table 9 constitute weights for the MAIS costs in Table 1 (with adjustments for property damage, as explained in the following paragraphs) to generate costs per A, B, and C injury in fatal accidents and in injury accidents.

The MAIS direct costs per victim in Table 1 include a property damage component expressed as the average amount of property damage per victim (3). However, estimating the amount of property damage per accident necessitates the calculation of property damage on a per-accident basis rather than on a per-victim basis because the average accident includes more than one injury per accident and, in the case of fatal accidents, some injuries as well as fatalities (see Table 4).

Thus, to avoid double-counting of property damage, the direct cost of each nonfatal injury (MAIS-1 to MAIS-5) and fatality (MAIS-6) in Table 1 is adjusted as follows. The average amount of property damage per victim (1) is deleted from each direct cost total to give a net direct cost per MAIS injury as follows:

MAIS Injury	Direct Cost in Table 1 ($)	Property Damage per Victim ($)	Net Direct Cost ($)
1	1,601	811	790
2	3,442	1,354	2,088
3	8,089	2,120	5,969
4	18,467	2,865	15,602
5	138,684	2,845	135,839
6	18,294	3,406	14,888

The direct cost (net of property damage) and the indirect cost per A-B-C injury can be calculated by using these net direct costs for MAIS-1 to MAIS-6 and the indirect costs in Table 1, along with the weights in Table 9. For those MAIS-0 that were coded as injuries on the A-B-C scale, direct and indirect costs of zero are used. (A cost of zero is used for MAIS-0 because no empirical information is available on direct or indirect costs associated with accidents coded as injury accidents but that turn out to be MAIS-0, that is, PDO accidents. Although there may be some costs associated with such accidents, so that positive values should be used with the MAIS-0 weight in Table 9, precisely what values would be appropriate is unclear. In any event, the costs of A, B, and C injuries are not significantly affected by using a zero cost instead of some positive values.) Multiplying the weights by the MAIS costs and dividing the sum of the products by the sum of the weights (expressed as a proportion rather than as a percentage) produces direct and indirect costs of A, B, and C injuries.

TABLE 8 Percentage Distributions of Injuries by A-B-C and MAIS Severities by Accident Severity

A-B-C Cumulative Percentages	MAIS Percentages							
	0	1	2	3	4	5	6	Total[a]
Fatal accident								
C	0.42	10.96	1.80	1.24	0.48	0.00	0.00	14.90
B and C	0.60	29.00	9.50	7.33	2.22	0.39	0.00	49.04
A and B and C	0.60	38.75	20.12	23.71	10.51	6.01	0.30	100.00
Injury accident								
C	2.92	35.76	3.35	0.94	0.05	0.00	0.00	43.02
B and C	3.32	63.80	11.06	4.15	0.46	0.18	0.00	82.97
A and B and C	3.37	68.91	17.00	8.46	1.65	0.58	0.03	100.00

[a]Derived from Table 7.

TABLE 9 Weights for Converting MAIS Costs to A-B-C Costs per Injury

A-B-C Category and Accident Severity	MAIS Percentages (Weights)							
	0	1	2	3	4	5	6	Total[a]
Fatal accident								
A	0.00	9.75	10.62	16.38	8.29	5.62	0.30	50.96
B	0.18	18.04	7.70	6.09	1.74	0.39	0.00	34.14
C	0.42	10.96	1.80	1.24	0.48	0.00	0.00	14.90
Injury accident								
A	0.05	5.11	5.94	4.31	1.19	0.40	0.03	17.03
B	0.40	28.04	7.71	3.21	0.41	0.18	0.00	39.95
C	2.92	35.76	3.35	0.94	0.05	0.00	0.00	43.02

[a]Derived from Table 7.

The procedure can be illustrated by calculating the costs per A injury in a fatal accident. The net direct cost per A injury is estimated as

MAIS Injury	Weight (Table 9)	Net Direct Cost ($)	Product ($)
0	0.0000	0	0
1	0.0975	790	77
2	0.1062	2,088	222
3	0.1638	5,969	978
4	0.0829	15,602	1,293
5	0.0562	135,839	7,634
6	0.0030	14,888	45
Total	0.5096		10,249

Net direct cost = (Sum of products)/(Sum of weights) = ($10,249)/(0.5096) = $20,112 per A injury in a fatal accident.

The indirect cost per A injury is estimated as

MAIS Injury	Weight (Table 9)	Indirect Cost (Table 1) ($)	Product ($)
0	0.0000	0	0
1	0.0975	690	67
2	0.1062	1,165	124
3	0.1638	2,217	363
4	0.0829	32,564	2,700
5	0.0562	122,897	6,907
6	0.0030	724,227	2,173
Total	0.5096		12,334

Indirect cost = (Sum of products)/(Sum of weights) = ($12,334)/(0.5096) = $24,203 per A injury in a fatal accident.

The total cost per injury, net of property damage, is the sum of the indirect and net direct costs:

Net total cost = Net direct cost + Indirect cost = $20,112 + $24,203 = $44,315 per A injury in a fatal accident.

Net direct, indirect, and net total costs per injury are given in Table 10 for A, B, and C injuries in fatal accidents and in injury accidents.

COST PER NONFATAL INJURY ACCIDENT

The total cost per nonfatal injury accident can be estimated in either of two ways. The first approach is to use the net total costs of A, B, and C injuries (C_A, C_B, and C_C) in Table 10 and the average numbers of A, B, and C injuries per accident (A, B, and C) in Table 4, with an adjustment to include the average amount of property damage per injury accident. The net total cost per injury accident is estimated as the costs per injury in Table 10 times the respective numbers of A, B, and C injuries per accident in Table 4:

TABLE 10 Net Costs of A, B, and C Injuries in Fatal and Injury Accidents (1980 dollars)

Accident Severity and Type of Cost	Cost per Injury		
	A ($)	B ($)	C
Fatal			
Direct[a]	20,112	4,303	1,839
Indirect	24,203	4,086	1,876
Total[a]	44,315	8,389	3,715
Injury			
Direct[a]	6,783	2,213	972
Indirect	7,612	1,775	751
Total[a]	14,395	3,988	1,723

[a]Net of direct property damage costs.

Net total cost = $(C_A \times A) + (C_B \times B) + (C_C \times C)$

For injury accidents in rural and urban areas, the net total costs per accident are

Net total cost = ($14,395 x 0.3457) + ($3,988 x 0.5770) x ($1,723 x 0.6027) = $8,316 per rural injury accident.

Net total cost = ($14,395 x 0.1883) + ($3,988 x 0.5990) x ($1,723 x 0.6575) = $6,232 per urban injury accident.

The amount of property damage per injury accident is then added to the net total cost per accident to arrive at the total cost per nonfatal injury accident. The property damage per accident is equal to the average property damage per vehicle involved in injury accidents [$1,632 in 1980 dollars, based on Table VI-1 in the NHTSA Report (3)] times the average number of vehicles involved per injury accident in Table 2. The property damage cost per nonfatal injury accident in rural and urban areas is

Property damage cost = Cost per vehicle x Vehicles per accident = $1,632 x 1.4264 = $2,328 per rural injury accident.

Property damage cost = $1,632 x 1.5399 = $2,513 per urban injury accident.

The total cost per nonfatal injury accident is equal to the sum of the net total cost and the property damage per accident. For injury accidents in rural and urban areas, the total cost per accident is

Total cost = Net total cost + Property damage cost = $8,316 + $2,328 = $10,644 per rural injury accident.

Total cost = \$6,232 + \$2,513 = \$8,745 per urban injury accident.

Alternatively, the total cost per injury accident can be estimated by explicitly calculating the direct and indirect costs per injury accident and then summing these two costs. The indirect cost per injury accident is readily estimated by multiplying the indirect costs of A, B, and C injuries from Table 10 (IC_A, IC_B, and IC_C) by the corresponding numbers of injuries per injury accident from Table 4 (A, B, and C) as follows:

Indirect cost = (IC_A x A) + (IC_B x B) + (IC_C x C) = (\$7,612 x 0.3457) + (\$1,775 x 0.5770) + (\$751 x 0.6027) = \$4,108 per rural injury accident.

Indirect cost = (\$7,612 x 0.1883) + (\$1,775 x 0.5990) + (\$751 x 0.6575) = \$2,990 per urban injury accident.

The net direct cost per injury accident is equal to the sum of the net direct costs of A, B, and C injuries from Table 10 (NDC_A, NDC_B, and NDC_C) times the corresponding numbers of A, B, and C injuries per injury accident from Table 4:

Net direct cost = (NDC_A x A) + (NDC_B x B) + (NDC_C x C) = (\$6,783 x 0.3457) + (\$2,213 x 0.5770) + (\$972 x 0.6027) = \$4,208 per rural injury accident.

Net direct cost = (\$6,783 x 0.1883) + (\$2,213 x 0.5990) + (\$972 x 0.6575) = \$3,242 per urban injury accident.

Net direct cost plus property damage per injury accident gives the direct cost per injury accident:

Direct cost = Net direct cost + Property damage cost = \$4,208 + \$2,328 = \$6,536 per rural injury accident.

Direct cost = \$3,242 + \$2,513 = \$5,755 per urban injury accident.

The total cost per nonfatal injury is equal to the sum of the direct and indirect costs:

Total cost = Direct cost + Indirect cost = \$6,536 + \$4,108 = \$10,644 per rural injury accident.

Total cost = \$5,755 + \$2,990 = \$8,745 per urban injury accident.

COST PER FATAL ACCIDENT

The total cost per fatal accident is derived from cost information reported by Miller et al. (1) for indirect costs and the NHTSA report (3) for direct costs and from the costs of A, B, and C injuries developed earlier. The indirect cost per fatal accident is readily obtained by multiplying the indirect cost per fatality in Table 1 and the indirect costs of A, B, and C injuries in Table 10 (IC_F, IC_A, IC_B, and IC_C) by the numbers of fatalities and A, B, and C injuries per fatal accident in Table 4, as follows:

Indirect cost = (IC_F x F) + (IC_A x A) + (IC_B x B) + (IC_C x C) = (\$724,227 x 1.1516) + (\$24,203 x 0.5315) + (\$4,086 x 0.3173) + (\$1,876 x 0.1396) = \$848,442 per rural fatal accident.

Indirect cost = (\$724,227 x 1.0862) + (\$24,203 x 0.3528) + (\$4,086 x 0.3015) + (\$1,876 x 0.1298) = \$796,670 per urban fatal accident.

The direct cost per fatal accident is estimated as follows. As with the net direct cost per injury, the direct cost per fatality of \$18,294 in Table 1 is adjusted by deleting the average amount of property damage per victim, estimated to be \$3,406 (1), to give a net direct cost per fatality of \$14,888. The direct cost per fatal accident, net of property damage, is then estimated as the sum of the net direct costs per fatality and per A, B, and C injury in Table 10 (NDC_F, NDC_A, NDC_B, and NDC_C, respectively) times the corresponding average numbers of fatalities and A, B, and C injuries per fatal accident from Table 4 (F, A, B, and C, respectively):

Net direct cost = (NDC_F x F) + (NDC_A x A) + (NDC_B x B) + (NDC_C x C) = (\$14,888 x 1.1516) + (\$20,112 x 0.5315) + (\$4,303 x 0.3173) + (\$1,839 x 0.1396) = \$29,457 per rural fatal accident.

Net direct cost = (\$14,888 x 1.0862) + (\$20,112 x 0.3528) + (\$4,303 x 0.3015) + (\$1,839 x 0.1298) = \$24,803 per urban fatal accident.

The amount of property damage per fatal accident is equal to the property damage per vehicle involvement in fatal accidents, which is \$3,760 from Table VI-1 in the NHTSA report (3), times the average number of involvements per fatal accident from Table 2:

Property damage = \$3,760 x 1.3930 = \$5,238 per rural fatal accident.

Property damage = \$3,760 x 1.4316 = \$5,383 per urban fatal accident.

The direct cost per fatal accident, then, is the sum of the net direct cost and the property damage cost:

Direct cost = Net direct cost + Property damage cost = \$29,457 + \$5,238 = \$34,695 per rural fatal accident.

Direct cost = \$24,803 + \$5,383 = \$30,186 per urban fatal accident.

The total cost per fatal accident is equal to the sum of the direct and indirect costs. For accidents in rural and urban areas, the total cost per fatal accident is

Total cost = Direct cost + Indirect cost = \$34,695 + \$848,442 = \$883,137 per rural fatal accident.

Total cost = \$30,186 + \$796,670 = \$826,856 per urban fatal accident.

Direct, indirect, and total costs per fatal, injury, and PDO accident in rural and urban areas are summarized in Table 11. Accident proportions by severity from Table 3 were used to obtain the average cost per rural accident and per urban accident.

UPDATING ACCIDENT COSTS

The accident costs in Table 11 can readily be updated from 1980 by applying appropriate cost indices to the direct and indirect costs. For updating the accident costs to 1985, suitable indices for direct and indirect accident costs are the consumer price index (CPI) for all items (equal to 247.0 in 1980

TABLE 11 Accident Costs by Area and Severity (1980 dollars)

Area and Type of Cost	Accident Cost by Severity			
	Fatal ($)	Injury ($)	PDO ($)	Average ($)
Rural				
Direct	34,695	6,536	1,096	3,715
Indirect	848,442	4,108	202	15,309
Total	883,137	10,644	1,298	19,024
Urban				
Direct	30,186	5,755	1,283	2,581
Indirect	796,670	2,990	236	4,562
Total	826,856	8,745	1,519	7,143

and 323.0 in 1985, third quarter, 1967 = 100.0) and the index of average hourly earnings (IAHE) (equal to 127.3 in 1980 and 165.9 in 1985, third quarter, 1977 = 100.0). The total accident cost for any severity and rural-urban area in Table 11 can be calculated as the sum of the 1980 direct and indirect costs multiplied by their respective increases from 1980 to 1985. For example, the updated average total cost of a rural accident is equal to ($3,715)(323.0/247.0) + ($15,309)(165.9/127.3) = $24,809. Although it would be more precise to first update the MAIS unit costs given by Miller et al. (1) to 1985 dollars and then develop 1985 costs per accident, the described procedure should yield reasonably accurate updates of the 1980 accident costs in Table 11.

SUMMARY AND CONCLUSIONS

In order for states to effectively allocate limited highway safety funds, a method such as benefit-cost analysis must be used. This generally requires accident costs for estimating the expected accident reduction benefits of safety improvements. Among the most recent attempts to provide comprehensive estimates of motor vehicle accident costs is a 1984 study by Miller et al. for FHWA (1) in which the apparently best available estimates were summarized. However, that study did not express accident costs in a form that can be directly used with state accident data in benefit-cost analyses.

In this paper, accident costs were developed from the cost data presented in the study by Miller et al. (1) and accident data from five states, employing methods previously developed in a study by McFarland and Rollins for FHWA (2). A major aspect of this paper was to relate the percentage distributions of injuries by the MAIS and A-B-C severity scales, thereby allowing the MAIS-based costs reported by Miller et al. (1) to be expressed in terms of A-B-C severities. The result of the analysis was a set of costs per accident, in terms of the A-B-C severity scale on which state accident data are commonly based. These accident costs can be used directly with state accident data, thereby facilitating the use of state-of-the-art accident cost estimates in benefit-cost analyses of highway safety improvements.

ACKNOWLEDGMENTS

This paper is partially based on work performed for the Federal Highway Administration, U.S. Department of Transportation, Brenda C. Kragh, contract manager. The NASS Applications Group of the National Highway Traffic Safety Administration provided data on injuries cross-classified by the Maximum Abbreviated Injury Scale and the A-B-C injury scale.

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Publication of this paper sponsored by Committee on Traffic Records and Accident Analysis.

Revised Decision Criteria for Before-and-After Analyses

RICHARD M. WEED

ABSTRACT

Because better experimental designs utilizing control sites are not always feasible, a simple before-and-after analysis is commonly used to analyze accident rates and other counted events. Treating the number of events counted before some experimental change as a known constant rather than as a random variable is a fundamental conceptual error that falsely inflates the confidence level at which the experimental change can be judged to have had a significant effect. For example, a reduction in the number of accidents observed after some improvement has been implemented may be judged to be statistically significant when, in fact, it is primarily the result of the chance occurrence of an unusually high "before" count, a typical manifestation of the "regression-to-the-mean" phenomenon. By properly treating the initial count as a random variable, at least a portion of this problem is avoided. New tables are developed to provide more appropriate decision criteria for applications of this type.

The accident history at a particular site is often the only basis for measuring the effectiveness of a safety improvement. The number of accidents observed during equal periods of time before and after the improvement was implemented are compared to determine whether or not a reduction can be attributed to something other than random chance. This same approach may also be used to judge whether or not an increase in accident frequency at a site warrants remedial action. Although it is highly desirable to incorporate control sites into such analyses to screen out the effects of time, traffic volume, or other extraneous factors, this is not always possible. Consequently, decisions must often be based solely on the "before" and "after" accident counts at a particular location.

Because the typical time and exposure conditions associated with the occurrence of accidents closely approximate the theoretical conditions that give rise to the Poisson distribution, it is usually assumed that accidents are Poisson-distributed for analytical purposes. One method of analysis, presented in graphical form in the Highway Safety Evaluation (HSE) Procedural Guide (1,p.114), treats the before count as a known Poisson mean and indicates the percent change in the after count that must be observed to be judged statistically significant at four selected confidence levels. This graph is shown in Figure 1.

There are at least three things wrong with the method in the HSE Procedural Guide:

1. Unless the before period is quite long, which usually is not the case, it is not appropriate to treat the accident count as a known constant. To be properly evaluated, it must be regarded as a random variable that provides an estimate of the underlying accident potential for that particular site.
2. This conceptual error leads to a second one, the assumption that the same decision criteria can be used to test for either significant decreases or significant increases in accident frequency. This is approximately correct when the before count is truly known but is not correct when it must be treated as a random variable.
3. The Accident Research Manual (2,p.39) states that one of the most important causes of erroneous conclusions in highway-related evaluations is the regression-to-the-mean phenomenon. To illustrate this effect by example, the practice of applying safety improvements only to those locations having the highest accident frequencies--some of which are due in part to random chance and which would have appeared to improve even if nothing were done--tends to falsely inflate the level of significance attributed to the various improvements. That this is not a problem to be casually disregarded is evidenced in a statement by Persaud and Hauer (3,p.44) that this effect is "consistent, real, and nothing short of dramatic." Because the method in the HSE Procedural Guide treats the before observation as a known, rather than recognizing it as a random variable, it is particularly susceptible to this common shortcoming.

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CORRECTIVE MEASURES

There are several methods by which the before count can be treated as a random variable. Three specific methods that are offered in lieu of that in the HSE Procedural Guide will be referred to as the chi-square, binomial, and modified binomial methods, respectively. An outline of all four methods follows.

Method in HSE Procedural Guide

The before count is taken to be a known Poisson mean. For a series of possible before counts, terms of the Poisson distribution are summed as indicated in Equation 1 to determine the after counts necessary to be judged statistically significant at (or above) the desired levels of confidence. (Alternatively, nearly the same results can be obtained by approximating the Poisson distribution with a normal distribution having $\mu = \sigma^2$ = Poisson mean.) These results are then converted to percentages and used to plot the curves in Figure 1.

$$\alpha = \sum_{x=X_1}^{x=X_2} \lambda^x e^{-\lambda}/x! \qquad (1)$$

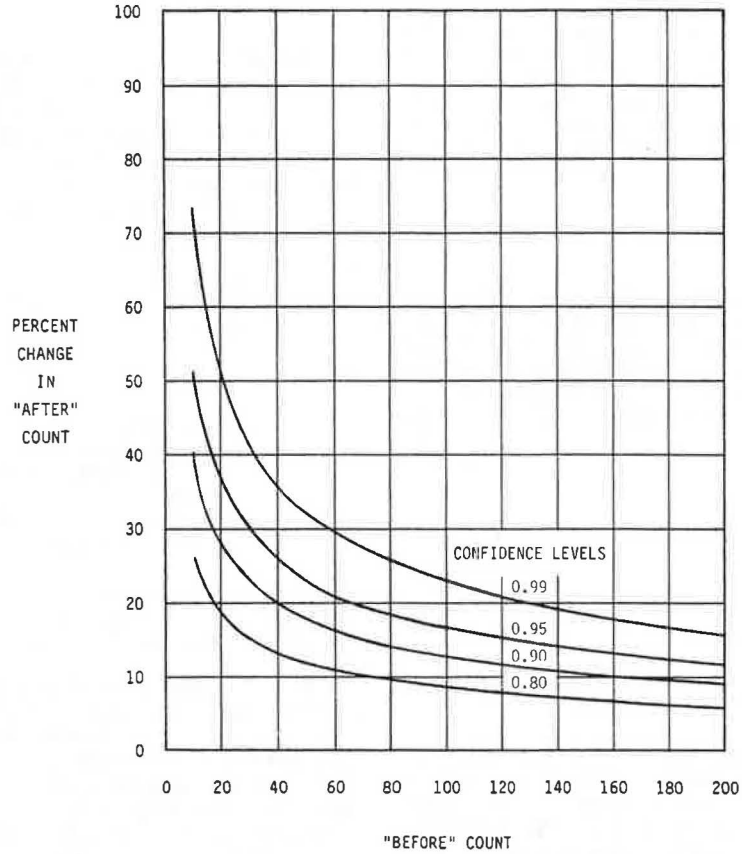

FIGURE 1 Graphical decision criteria in HSE Procedural Guide.

where

α = probability that $X_1 \leq x \leq X_2$, significance level of test;
λ = Poisson mean (assumed equal to before count);
e = base of natural logarithms (2.71828...); and
X_1, X_2 = summation limits.

Let the number of after events be designated by X. To determine whether or not X represents a significant departure from the (assumed) true mean value of λ, the appropriate area under the Poisson distribution is computed. If X is less than λ so that a possible decrease in the number of events is under test, the summation limits are $X_1 = 0$ and $X_2 = X$. If X is greater than λ, the limits are $X_1 = X$ and $X_2 = \infty$. (For practical purposes, the computational procedure is terminated whenever subsequent terms become insignificant.) The value of α obtained in this manner represents the single-tailed significance level at which the observed after count of X can be judged to be significantly different from the (assumed known) before count of λ. To plot the curves shown in Figure 1, each X value is converted to a percent change and the confidence level is taken to be $1 - \alpha$.

For example, suppose that during the 2 years preceding the installation of a skid-resistant overlay, there were 10 accidents in which slipperiness was a factor. In the 2 years following the installation, there were five such accidents. It is desired to know at what level of confidence this degree of reduction can be attributed to anything other than random chance. (It is assumed that traffic volume, an indicator of exposure representing the opportunity for accidents to occur, has remained essentially constant and that no other pertinent factors have changed. The count values used in this example have been chosen to be quite low to simplify the illustration.) The values computed with Equation 1 are presented in Table 1.

Because accident count is a discrete variable, it usually is not possible to match the desired confidence levels in Figure 1 exactly. To be conservative, the critical after counts are selected so that their computed significance levels (α) are less than or equal to those associated with the stated confidence levels ($1 - \alpha$). Although the resulting curves are not strictly continuous, it is a practical expedient to plot them as such in Figure 1.

The previously stated example, in which there were 10 accidents before and 5 accidents after a safety improvement was installed, may now be analyzed. Under the assumption that the before count is

TABLE 1 Illustration of Method Used in HSE Procedural Guide

After Count (X)	Percent Change from Before Count of $\lambda = 10$	Cumulative Probability[a] (significance level, α)	
0	100	0.000045	
1	90	0.000499	
2	80	0.002769	($\alpha \leq 0.01$)
3	70	0.010336	
4	60	0.029253	($\alpha \leq 0.05$)
5	50	0.067086	($\alpha \leq 0.10$)
6	40	0.130142	($\alpha \leq 0.20$)
7	30	0.220221	

[a]Computed with Equation 1 using before count of $\lambda = 10$.

a known constant, it is observed from Table 1 that this particular count combination corresponds to a significance level of $\alpha = 0.067$. If Figure 1 is used, this point falls between the 0.90 and 0.95 confidence lines, a result that might lead the highway agency to conclude that the safety improvement is responsible for a significant reduction in accidents.

CHI-SQUARE METHOD

Of the various methods by which the before count may be treated as a variable, the simplest (but not necessarily the best) is based on the chi-square distribution. It is well known that a variable that can be expressed in the form given by Equation 2 is approximately chi-square distributed (4,p.238) with k - 1 degrees of freedom.

$$\chi^2 = \sum_{i=1}^{i=k} [(O_i - E_i)^2/E_i] \quad (2)$$

where

χ^2 = chi-square statistic,
O = observed count,
E = theoretically expected count, and
k = number of different categories.

For the present application, there are only two possible categories, the before and after counts, which will be designated Y and X, respectively. Under the null hypothesis that X and Y are both estimates of the same underlying Poisson mean, the best estimate of the theoretically expected count is the average of the two. Therefore, $E_1 = E_2 = (X + Y)/2$. Equation 2 then reduces to

$$\chi^2 = (X - Y)^2/(X + Y) \quad (3)$$

where X is the after count and Y is the before count with one degree of freedom. Because there is some difference of opinion in the literature about whether a continuity correction should be applied, no such adjustment has been made.

Using the same example with a before count of Y = 10 and an after count of X = 5, Table 2 has been prepared. These results are substantially different from those in Table 1 where, based on the assumption that the before count can be taken as a known constant, a reduction of 50 percent was required to achieve statistical significance at the $\alpha = 0.10$ level. In Table 2, using the chi-square method to treat the before count as a random variable, a reduction of 70 percent is required to achieve essentially the same level of significance. By this procedure, a reduction in accident count from Y = 10 to X = 5 would not be likely to be judged statistically significant.

TABLE 2 Illustration of Chi-Square Method

After Count (X)	Percent Change from Before Count of Y = 10	Chi-Square Value[a] (χ^2)	Cumulative Probability[b] (significance level, α)	
0	100	10.00	0.002	
1	90	7.36	0.007	($\alpha \leq 0.01$)
2	80	5.33	0.021	($\alpha \leq 0.05$)
3	70	3.77	0.052	($\alpha \leq 0.10$)
4	60	2.57	0.109	
5	50	1.67	0.196	($\alpha \leq 0.20$)
6	40	1.00	0.317	

[a]Computed with Equation 3 using before count of Y = 10.
[b]Obtained from chi-square table (or suitable computer algorithm).

BINOMIAL METHOD

A statistically more efficient method to perform this analysis is based on the binomial distribution (5,p.140). Under the null hypothesis that there has truly been no change, this procedure assumes that a given total number of events will be distributed between the before and after categories as a binomial variable with p = 0.5. The following equation applies:

$$\alpha = 0.5^N \sum_{x=X_1}^{x=X_2} N!/[x!(N - x)!] \quad (4)$$

where

α = probability that $X_1 \leq x \leq X_2$, significance level of test;
N = total count = X + Y;
X = after count;
Y = before count; and
X_1, X_2 = summation limits.

If X is less than Y, the appropriate summation limits are $X_1 = 0$ and $X_2 = X$. When X is greater than Y, the summation limits are $X_1 = X$ and $X_2 = X + Y = N$.

For the example that has been used thus far, the before and after counts are Y = 10 and X = 5, respectively. Using Equation 4, the values in Table 3 are obtained. Although this will not always be the case, it is observed in this example that this method produces slightly different critical values than those obtained by the chi-square method in Table 2. It will be demonstrated in a subsequent section that, in the long run, this procedure tends to produce a slightly greater percentage of correct decisions than the chi-square method.

TABLE 3 Illustration of Binomial Method

After Count (X)	Percent Change from Before Count of Y = 10	Total Count (N)	Cumulative Probability[a] (significance level, α)	
0	100	10	0.000977	
1	90	11	0.005859	($\alpha \leq 0.01$)
2	80	12	0.019287	
3	70	13	0.046143	($\alpha \leq 0.05$)
4	60	14	0.089783	($\alpha \leq 0.10$)
5	50	15	0.150879	($\alpha \leq 0.20$)
6	40	16	0.227249	

[a]Computed with Equation 4 using before count of Y = 10.

MODIFIED BINOMIAL METHOD

Because these methods deal with discrete data, it is seldom possible to control the confidence level $(1 - \alpha)$ at precisely the desired value. Consequently, it is customary to set up decision criteria that are conservative so that the actual confidence level will never be less than the indicated value. If it were desired to have decision criteria that would produce very nearly the stated confidence levels in the long run, a slight modification of the binomial method may be made. Rather than selecting the critical after counts so that the confidence levels are always greater than or equal to the stated values, they can be chosen on the basis of being closest to the stated values, whether larger or smaller. By this procedure, the decision criteria (tables or graphs) would cause individual decisions to be made at confidence levels slightly larger or smaller than the desired values but in a random fashion such that

the averages would tend to be close to the desired values in the long run. If this approach were applied to the values in Table 3, the first three critical values would remain unchanged but, at $\alpha = 0.20$, the critical after count would be taken to be 6, representing a 40 percent reduction. This procedure will be included among those tested in a subsequent section.

SUMMARY OF THE FOUR EXAMPLES

Based on a hypothetical situation in which there was a before count of 10, the percent changes required to achieve statistical significance at the selected confidence levels are given in Table 4. For these examples, the three alternate procedures all require larger percent changes than the HSE method before statistical significance can be claimed. To provide a better impression of the magnitude of the difference over a wide range of possible input data, the $1 - \alpha = 0.95$ curves are plotted for the HSE and binomial methods in Figure 2. It can be seen from this figure that the difference is greater when the test concerns an increase rather than a decrease in the counted data, that there is a larger difference in the realm of smaller counts for both decreases and increases, and that the difference is still fairly substantial even for large counts.

TABLE 4 Summary of Examples

Analysis Method	Percent Reduction[a] Required for Statistical Significance at Selected Confidence Levels			
	0.99	0.95	0.90	0.80
HSE Procedural Guide	90	60	50	40
Chi-square	90	80	70	50
Binomial	90	70	60	50
Modified binomial	90	70	60	40

[a]Based on before count of 10.

Like the binomial method, the chi-square and modified binomial methods exhibit very nearly the same behavior as that shown in Figure 2. In order to judge which of the three alternate methods is best, it is necessary to test their performance in situations in which the null hypothesis is true and also when it is false.

NULL AND POWER TESTS

Computer simulation tests were run to evaluate the performance of the three alternate methods and to compare their performance with that of the HSE method. The first, shown in Figure 3, is a null test but was run primarily to demonstrate that the Poisson random generator was working properly. With the possible exception of the kurtosis, the parameters of the randomly generated distribution are seen to agree very closely with the desired theoretical values.

For this particular run, the four analysis methods were applied to 1,000 different pairs of random Poisson variates and the results (accept or reject the null hypothesis of no difference) were counted. Because the null hypothesis was true (the means of the before and after populations were both equal to 10) and the test was run at the $\alpha = 0.05$ significance level, it would be considered a desirable result if the tests falsely rejected the null hypothesis approximately 5 percent of the time. It is seen

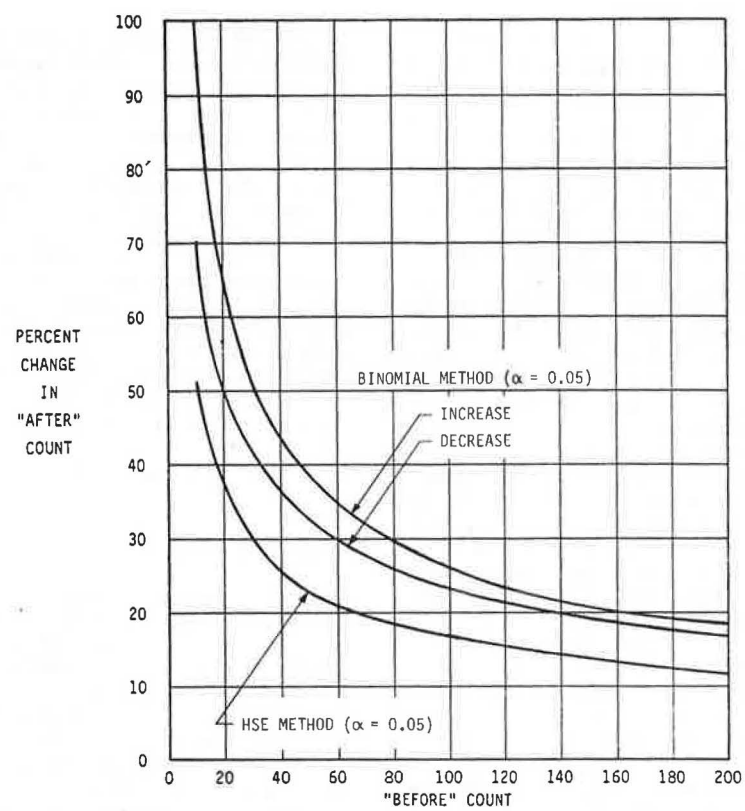

FIGURE 2 Comparison of binomial method with method in HSE Procedural Guide.

```
run poistest
EXECUTION BEGINS...

ENTER'SIGNIFICANCE LEVEL, POISSON MEAN, NUMBER OF REPLICATIONS,
AND RANDOM GENERATOR SEED NUMBER
?
0.05 10 1000 7654321
```

CHECK OF POISSON RANDOM GENERATOR

PARAMETER	DESIRED	OBTAINED
MEAN	10.00	10.11
VARIANCE	10.00	9.86
SKEW	0.32	0.36
KURTOSIS	0.10	0.35
PAIRWISE CORRELATION	0.0	0.03

COMPARISON OF TWO POISSON POPULATIONS

	RELATIVE FREQUENCY OF FALSELY REJECTING THE NULL HYPOTHESIS (BASED ON 1000 REPLICATIONS)	
ANALYSIS METHOD	DESIRED	OBTAINED
HSE PROCEDURAL GUIDE	0.050	0.125
CHI-SQUARE	0.050	0.021
BINOMIAL	0.050	0.028
MODIFIED BINOMIAL	0.050	0.043

FIGURE 3 First computer run to demonstrate simulation concept.

from Figure 3 that the rejection rate for the HSE method is considerably more than 5 percent. This is the result of treating the before count as a constant rather than as a random variable. The rejection rates for the chi-square and binomial methods are both less than 5 percent, an expected result because these methods are known to be conservative. The modified binomial method produces a rejection rate quite close to 5 percent, as intended.

A more complete series of null tests is shown in Figure 4. The same simulation procedure has been used except that, in addition to the empirically derived rejection rates, lower and upper confidence limits have been printed in parentheses to provide an impression of the reliability of the results. The confidence limits are of the equal-likelihood type (6,p.453) and are unsymmetrical.

Figure 4 includes several combinations of significance level and Poisson mean and produces essentially the same results as were observed in Figure 3. The HSE method falsely rejects the null hypothesis much too often whereas the chi-square and binomial methods reject it somewhat less often than probably could be tolerated. The modified binomial method has rejection rates very close to the significance level at which the tests were run.

A series of power tests, all run at a significance level of $\alpha = 0.05$, is shown in Figure 5. For these tests, various combinations of true Poisson means have been used. In every case, the true population means are different and it is desired that the analysis methods be capable of recognizing these differences by rejecting the null hypothesis a large percentage of the time. Obviously, the more pronounced differences will produce higher rejection rates.

At first glance, it might appear that the HSE method is superior because it has rejection rates higher than the other three methods. It must be recognized, however, that this is largely the result of its tendency to reject too often, as demonstrated in Figure 4. Its use would be acceptable only if there were little or no concern about the many times it falsely rejects the null hypothesis. Between the chi-square and binomial methods, the latter appears to be the better procedure. Although the differences are small, it consistently outperforms the chi-square method in both the null tests and the power tests. For the user willing to accept that the modified binomial method will falsely reject the null hypothesis about the proper percentage of time in the long run, still greater power can be obtained, as seen in the last column of Figure 5.

REVISED DECISION CRITERIA

Suitable decision criteria to judge the significance of changes between before and after counts may be derived by any of the three alternate methods--chi-square, binomial, or modified binomial--and may be put in either tabular or graphical form. The critical after values may be presented as percent changes from the before counts (as is presently done in the HSE Procedural Guide) or as direct counts. Because this is believed to have the greatest potential usefulness, the revised decision criteria presented in

```
run nulltest
EXECUTION BEGINS...

ENTER NUMBER OF REPLICATIONS AND RANDOM GENERATOR SEED NUMBER
?
1000 9876543
```

DESIRED RELATIVE FREQUENCY OF FALSE REJECTION	TRUE POISSON MEAN	RELATIVE FREQUENCY OF FALSE REJECTION WITH LOWER AND UPPER ALPHA/2 = 0.025 CONFIDENCE LIMITS: HSE PROCEDURAL GUIDE	CHI-SQUARE	BINOMIAL	MODIFIED BINOMIAL
		(0.036)	(0.000)	(0.001)	(0.003)
0.01	10	0.048	0.003	0.004	0.008
		(0.063)	(0.009)	(0.010)	(0.016)
		(0.048)	(0.000)	(0.001)	(0.003)
0.01	20	0.062	0.002	0.004	0.007
		(0.079)	(0.007)	(0.010)	(0.014)
		(0.041)	(0.001)	(0.001)	(0.003)
0.01	50	0.054	0.005	0.005	0.007
		(0.070)	(0.012)	(0.012)	(0.014)
		(0.129)	(0.014)	(0.017)	(0.031)
0.05	10	0.151	0.022	0.026	0.043
		(0.175)	(0.033)	(0.038)	(0.058)
		(0.126)	(0.013)	(0.020)	(0.032)
0.05	20	0.147	0.021	0.030	0.044
		(0.171)	(0.032)	(0.043)	(0.059)
		(0.113)	(0.019)	(0.027)	(0.033)
0.05	50	0.133	0.028	0.038	0.045
		(0.156)	(0.040)	(0.052)	(0.060)
		(0.173)	(0.035)	(0.046)	(0.073)
0.10	10	0.197	0.047	0.060	0.090
		(0.223)	(0.062)	(0.077)	(0.109)
		(0.191)	(0.035)	(0.060)	(0.087)
0.10	20	0.216	0.047	0.076	0.105
		(0.243)	(0.062)	(0.094)	(0.126)
		(0.187)	(0.036)	(0.064)	(0.078)
0.10	50	0.212	0.049	0.080	0.096
		(0.239)	(0.064)	(0.099)	(0.116)

FIGURE 4 Series of null tests.

Figure 6 are based on the binomial method and have been put in tabular form with the critical after values listed as percent changes.

SUMMARY AND CONCLUSIONS

It is often necessary to use the simple before-and-after analysis of counted events to analyze accident rates or other phenomena. By failing to recognize the before count as a random variable, various safety improvements may be incorrectly judged to be significantly beneficial when, in fact, the apparent benefit may be due only to random chance. The degree to which such misapplications ultimately affect the conclusions of research studies or influence policy decisions is not known, but the potential harm of specifying the wrong material or product, or of establishing a less-than-optimal policy or design, is recognized to be substantial. An error of this type will seldom be an isolated case; it will be repeated with each subsequent application of the product or design standard.

In the case of simple before-and-after analyses, this problem can be alleviated by properly treating the before count as a random variable. Three methods for doing this were presented and one of them, a procedure that uses the binomial distribution to perform a hypothesis test of the equality of two Poisson populations, was used to develop tables of revised decision criteria suitable for applications of this type. It should be noted, however, that this does not correct for the regression-to-the-mean effect, a problem that may forever plague analysts when the test sites are not randomly selected.

The major impact of the new tables is that it will be more difficult to demonstrate that a safety improvement is significantly beneficial. Similarly, it will also be less likely that an apparent increase in accident frequency will incorrectly be interpreted to be real when, in fact, it is due only to chance. In either case, it is important to use the most appropriate analytical tools available. To quote again from the Accident Research Manual (2,p.27), "only with information from rigorous evaluations can sound administrative decisions be made."

AUTHOR'S NOTE

After presenting this paper, I became aware of an extensive set of tables prepared by Hauer (7) at the

```
run powrtest
EXECUTION BEGINS...

ENTER NUMBER OF REPLICATIONS, SIGNIFICANCE LEVEL OF TESTS, AND RANDOM
GENERATOR SEED NUMBER
?
1000 0.05 1234567
```

TRUE POISSON MEANS		RELATIVE FREQUENCY OF CORRECT REJECTION AT ALPHA OF 0.050 WITH ALPHA/2 = 0.025 CONFIDENCE LIMITS			
BEFORE	AFTER	HSE PROCEDURAL GUIDE	CHI-SQUARE	BINOMIAL	MODIFIED BINOMIAL
		(0.434)	(0.229)	(0.258)	(0.342)
10	5	0.465	0.256	0.286	0.372
		(0.496)	(0.284)	(0.315)	(0.403)
		(0.695)	(0.457)	(0.492)	(0.592)
10	3	0.724	0.488	0.524	0.623
		(0.752)	(0.519)	(0.555)	(0.653)
		(0.932)	(0.810)	(0.812)	(0.862)
10	1	0.948	0.835	0.836	0.884
		(0.961)	(0.858)	(0.858)	(0.903)
		(0.291)	(0.099)	(0.131)	(0.172)
20	15	0.320	0.118	0.153	0.196
		(0.350)	(0.140)	(0.177)	(0.222)
		(0.647)	(0.405)	(0.458)	(0.524)
20	10	0.677	0.436	0.489	0.555
		(0.706)	(0.467)	(0.520)	(0.586)
		(0.956)	(0.865)	(0.885)	(0.920)
20	5	0.969	0.886	0.905	0.937
		(0.979)	(0.905)	(0.922)	(0.951)
		(0.242)	(0.056)	(0.089)	(0.113)
50	45	0.269	0.071	0.108	0.133
		(0.298)	(0.089)	(0.129)	(0.156)
		(0.380)	(0.161)	(0.216)	(0.245)
50	40	0.411	0.185	0.242	0.272
		(0.442)	(0.211)	(0.270)	(0.301)
		(0.623)	(0.334)	(0.421)	(0.465)
50	35	0.653	0.364	0.452	0.496
		(0.683)	(0.395)	(0.483)	(0.527)

FIGURE 5 Series of power tests.

EVENTS BEFORE	PERCENT CHANGE IN NUMBER OF EVENTS AFTER					
	CONFIDENCE >/= 0.99		CONFIDENCE >/= 0.95		CONFIDENCE >/= 0.90	
	DECREASE	INCREASE	DECREASE	INCREASE	DECREASE	INCREASE
10	90.0	150.0	70.0	100.0	60.0	80.0
11	90.9	145.5	72.7	100.0	63.6	81.8
12	83.3	133.3	66.7	91.7	58.3	75.0
13	84.6	123.1	61.5	84.6	53.8	69.2
14	78.6	121.4	64.3	85.7	50.0	64.3
15	73.3	113.3	60.0	80.0	53.3	60.0
16	75.0	112.5	56.3	75.0	50.0	62.5
17	70.6	105.9	58.8	76.5	47.1	58.8
18	72.2	100.0	55.6	72.2	44.4	55.6
19	68.4	100.0	52.6	68.4	47.4	52.6
20	65.0	95.0	50.0	65.0	45.0	55.0
21	66.7	95.2	52.4	66.7	42.9	52.4
22	63.6	90.9	50.0	63.6	40.9	50.0
23	65.2	87.0	47.8	60.9	39.1	47.8
24	62.5	87.5	45.8	58.3	37.5	45.8
25	60.0	84.0	48.0	60.0	40.0	48.0
26	61.5	80.8	46.2	57.7	38.5	46.2
27	59.3	81.5	44.4	55.6	37.0	44.4
28	57.1	78.6	42.9	53.6	35.7	42.9
29	55.2	75.9	44.8	55.2	34.5	41.4

FIGURE 6 Revised decision criteria.

EVENTS BEFORE	PERCENT CHANGE IN NUMBER OF EVENTS AFTER					
	CONFIDENCE >/= 0.99		CONFIDENCE >/= 0.95		CONFIDENCE >/= 0.90	
	DECREASE	INCREASE	DECREASE	INCREASE	DECREASE	INCREASE
30	56.7	73.3	43.3	53.3	36.7	40.0
31	54.8	74.2	41.9	51.6	35.5	41.9
32	53.1	71.9	40.6	50.0	34.4	40.6
33	54.5	69.7	39.4	48.5	33.3	39.4
34	52.9	70.6	41.2	50.0	32.4	38.2
35	51.4	68.6	40.0	48.6	31.4	37.1
36	50.0	66.7	38.9	47.2	33.3	36.1
37	51.4	64.9	37.8	45.9	32.4	35.1
38	50.0	65.8	36.8	44.7	31.6	36.8
39	48.7	64.1	38.5	46.2	30.8	35.9
40	50.0	62.5	37.5	45.0	30.0	35.0
41	48.8	61.0	36.6	43.9	29.3	34.1
42	47.6	61.9	35.7	42.9	28.6	33.3
43	46.5	60.5	34.9	41.9	30.2	32.6
44	47.7	59.1	36.4	40.9	29.5	31.8
45	46.7	57.8	35.6	42.2	28.9	33.3
46	45.7	58.7	34.8	41.3	28.3	32.6
47	44.7	57.4	34.0	40.4	27.7	31.9
48	45.8	56.3	33.3	39.6	27.1	31.3
49	44.9	55.1	32.7	38.8	26.5	30.6
50	44.0	56.0	34.0	38.0	26.0	30.0
51	43.1	54.9	33.3	39.2	27.5	29.4
52	42.3	53.8	32.7	38.5	26.9	28.8
53	43.4	52.8	32.1	37.7	26.4	30.2
54	42.6	53.7	31.5	37.0	25.9	29.6
55	41.8	52.7	30.9	36.4	25.5	29.1
56	41.1	51.8	32.1	35.7	25.0	28.6
57	42.1	50.9	31.6	36.8	24.6	28.1
58	41.4	50.0	31.0	36.2	24.1	27.6
59	40.7	50.8	30.5	35.6	25.4	27.1
60	40.0	50.0	30.0	35.0	25.0	26.7
61	39.3	49.2	29.5	34.4	24.6	27.9
62	40.3	48.4	29.0	33.9	24.2	27.4
63	39.7	47.6	30.2	33.3	23.8	27.0
64	39.1	48.4	29.7	34.4	23.4	26.6
65	38.5	47.7	29.2	33.8	23.1	26.2
66	37.9	47.0	28.8	33.3	22.7	25.8
67	38.8	46.3	28.4	32.8	22.4	25.4
68	38.2	45.6	27.9	32.4	23.5	25.0
69	37.7	46.4	27.5	31.9	23.2	24.6
70	37.1	45.7	28.6	31.4	22.9	25.7
71	36.6	45.1	28.2	32.4	22.5	25.4
72	37.5	44.4	27.8	31.9	22.2	25.0
73	37.0	43.8	27.4	31.5	21.9	24.7
74	36.5	44.6	27.0	31.1	21.6	24.3
75	36.0	44.0	26.7	30.7	21.3	24.0
76	35.5	43.4	26.3	30.3	21.1	23.7
77	36.4	42.9	27.3	29.9	22.1	23.4
78	35.9	42.3	26.9	29.5	21.8	23.1
79	35.4	43.0	26.6	30.4	21.5	22.8
80	35.0	42.5	26.3	30.0	21.3	23.8
81	34.6	42.0	25.9	29.6	21.0	23.5
82	35.4	41.5	25.6	29.3	20.7	23.2
83	34.9	41.0	25.3	28.9	20.5	22.9
84	34.5	41.7	25.0	28.6	20.2	22.6
90	33.3	40.0	24.4	27.8	20.0	22.2
91	33.0	39.6	24.2	27.5	19.8	22.0
92	32.6	39.1	23.9	27.2	19.6	21.7
93	32.3	38.7	24.7	26.9	19.4	21.5
94	33.0	38.3	24.5	26.6	19.1	21.3
95	32.6	37.9	24.2	27.4	18.9	21.1
96	32.3	38.5	24.0	27.1	18.8	20.8
97	32.0	38.1	23.7	26.8	18.6	20.6
98	31.6	37.8	23.5	26.5	19.4	20.4
99	31.3	37.4	23.2	26.3	19.2	20.2
100	32.0	37.0	23.0	26.0	19.0	21.0
101	31.7	36.6	22.8	25.7	18.8	20.8
102	31.4	37.3	23.5	25.5	18.6	20.6
103	31.1	36.9	23.3	26.2	18.4	20.4
104	30.8	36.5	23.1	26.0	18.3	20.2
105	30.5	36.2	22.9	25.7	18.1	20.0
106	31.1	35.8	22.6	25.5	17.9	19.8

FIGURE 6 (continued)

EVENTS BEFORE	PERCENT CHANGE IN NUMBER OF EVENTS AFTER					
	CONFIDENCE >/= 0.99		CONFIDENCE >/= 0.95		CONFIDENCE >/= 0.90	
	DECREASE	INCREASE	DECREASE	INCREASE	DECREASE	INCREASE
107	30.8	35.5	22.4	25.2	17.8	19.6
108	30.6	36.1	22.2	25.0	17.6	19.4
109	30.3	35.8	22.0	24.8	18.3	19.3
110	30.0	35.5	22.7	24.5	18.2	19.1
111	29.7	35.1	22.5	24.3	18.0	18.9
112	30.4	34.8	22.3	25.0	17.9	19.6
113	30.1	34.5	22.1	24.8	17.7	19.5
114	29.8	34.2	21.9	24.6	17.5	19.3
115	29.6	34.8	21.7	24.3	17.4	19.1
116	29.3	34.5	21.6	24.1	17.2	19.0
117	29.1	34.2	21.4	23.9	17.1	18.8
118	29.7	33.9	21.2	23.7	16.9	18.6
119	29.4	33.6	21.0	23.5	16.8	18.5
120	29.2	33.3	21.7	23.3	17.5	18.3
121	28.9	33.9	21.5	24.0	17.4	18.2
122	28.7	33.6	21.3	23.8	17.2	18.0
123	28.5	33.3	21.1	23.6	17.1	17.9
124	28.2	33.1	21.0	23.4	16.9	18.5
125	28.8	32.8	20.8	23.2	16.8	18.4
126	28.6	32.5	20.6	23.0	16.7	18.3
127	28.3	32.3	20.5	22.8	16.5	18.1
128	28.1	32.8	20.3	22.7	16.4	18.0
129	27.9	32.6	20.9	22.5	16.3	17.8
130	27.7	32.3	20.8	22.3	16.2	17.7
131	27.5	32.1	20.6	22.9	16.0	17.6
132	28.0	31.8	20.5	22.7	15.9	17.4
133	27.8	31.6	20.3	22.6	16.5	17.3
134	27.6	31.3	20.1	22.4	16.4	17.2
135	27.4	31.9	20.0	22.2	16.3	17.0
136	27.2	31.6	19.9	22.1	16.2	17.6
137	27.0	31.4	19.7	21.9	16.1	17.5
138	26.8	31.2	19.6	21.7	15.9	17.4
139	27.3	30.9	20.1	21.6	15.8	17.3
140	27.1	30.7	20.0	21.4	15.7	17.1
141	27.0	30.5	19.9	22.0	15.6	17.0
142	26.8	31.0	19.7	21.8	15.5	16.9
143	26.6	30.8	19.6	21.7	15.4	16.8
144	26.4	30.6	19.4	21.5	15.3	16.7
145	26.2	30.3	19.3	21.4	15.2	16.6
146	26.7	30.1	19.2	21.2	15.8	16.4
147	26.5	29.9	19.0	21.1	15.6	16.3
148	26.4	29.7	18.9	20.9	15.5	16.2
149	26.2	30.2	19.5	20.8	15.4	16.1
150	26.0	30.0	19.3	20.7	15.3	16.7
151	25.8	29.8	19.2	21.2	15.2	16.6
152	25.7	29.6	19.1	21.1	15.1	16.4
153	25.5	29.4	19.0	20.9	15.0	16.3
154	26.0	29.2	18.8	20.8	14.9	16.2
155	25.8	29.0	18.7	20.6	14.8	16.1
156	25.6	28.8	18.6	20.5	14.7	16.0
157	25.5	29.3	18.5	20.4	14.6	15.9
158	25.3	29.1	18.4	20.3	14.6	15.8
159	25.2	28.9	18.2	20.1	15.1	15.7
160	25.0	28.8	18.8	20.0	15.0	15.6
161	25.5	28.6	18.6	19.9	14.9	15.5
162	25.3	28.4	18.5	20.4	14.8	15.4
163	25.2	28.2	18.4	20.2	14.7	16.0
164	25.0	28.0	18.3	20.1	14.6	15.9
165	24.8	28.5	18.2	20.0	14.5	15.8
166	24.7	28.3	18.1	19.9	14.5	15.7
167	24.6	28.1	18.0	19.8	14.4	15.6
168	24.4	28.0	17.9	19.6	14.3	15.5
169	24.9	27.8	17.8	19.5	14.2	15.4
170	24.7	27.6	17.6	19.4	14.1	15.3
171	24.6	27.5	18.1	19.3	14.0	15.2
172	24.4	27.3	18.0	19.2	14.0	15.1
173	24.3	27.7	17.9	19.7	13.9	15.0
174	24.1	27.6	17.8	19.5	14.4	14.9
175	24.0	27.4	17.7	19.4	14.3	14.9
176	23.9	27.3	17.6	19.3	14.2	14.8
177	24.3	27.1	17.5	19.2	14.1	14.7
178	24.2	27.0	17.4	19.1	14.0	15.2
179	24.0	26.8	17.3	19.0	14.0	15.1

FIGURE 6 (continued)

FIGURE 6 (continued)

EVENTS BEFORE	PERCENT CHANGE IN NUMBER OF EVENTS AFTER					
	CONFIDENCE >/= 0.99		CONFIDENCE >/= 0.95		CONFIDENCE >/= 0.90	
	DECREASE	INCREASE	DECREASE	INCREASE	DECREASE	INCREASE
180	23.9	26.7	17.2	18.9	13.9	15.0
181	23.8	27.1	17.1	18.8	13.8	14.9
182	23.6	26.9	17.6	18.7	13.7	14.8
183	23.5	26.8	17.5	18.6	13.7	14.8
184	23.4	26.6	17.4	18.5	13.6	14.7
185	23.8	26.5	17.3	18.9	13.5	14.6
186	23.7	25.8	17.2	18.8	13.4	14.5
187	23.5	26.2	17.1	18.7	13.4	14.4
188	23.4	26.1	17.0	18.6	13.8	14.4
189	23.3	26.5	21.2	18.5	13.8	14.3
190	23.2	26.3	16.8	18.4	13.7	14.2
191	23.0	26.2	16.8	18.3	13.6	14.1
192	22.9	26.0	16.7	18.2	13.5	14.1
193	23.3	25.9	16.6	18.1	13.5	14.5
194	23.2	25.8	17.0	18.0	13.4	14.4
195	23.1	25.6	16.9	17.9	13.3	14.4
196	23.0	25.5	16.8	17.9	13.3	14.3
197	22.8	25.4	16.8	18.3	13.2	14.2
198	22.7	25.8	16.7	18.2	13.1	14.1
199	22.6	25.6	16.6	18.1	13.1	14.1
200	22.5	25.5	16.5	18.0	13.0	14.0
201	22.4	25.4	16.4	17.9	12.9	13.9
202	22.8	25.2	16.3	17.8	12.9	13.9
203	22.7	25.1	16.3	17.7	12.8	13.8
204	22.5	25.0	16.2	17.6	13.2	13.7
205	22.4	24.9	16.1	17.6	13.2	13.7
206	22.3	25.2	16.5	17.5	13.1	13.6
207	22.2	25.1	16.4	17.4	13.0	13.5
208	22.1	25.0	16.3	17.3	13.0	13.5
209	22.0	24.9	16.3	17.7	12.9	13.9

THIS TABLE APPLIES TO EVENTS THAT ARE POISSON DISTRIBUTED. EACH CONFIDENCE LEVEL REPRESENTS THE ONE-TAILED PROBABILITY THAT PERCENT CHANGES AS EXTREME AS THOSE LISTED WOULD NOT BE EXCEEDED DUE JUST TO CHANCE.

University of Toronto. Although both the format and the derivation are different from that used for the tables presented in this paper, where a comparison is possible the agreement appears to be exact. I highly recommend these tables.

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Publication of this paper sponsored by Committee on Traffic Records and Accident Analysis.

An Overview of Selected Computer Programs for Automotive Accident Reconstruction

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ABSTRACT

Seven computer programs that have been extensively used by the authors and others for reconstruction of automobile accidents are discussed. These programs are CRASH, EES, IMPAC, VTS, TBS, SMAC, and HVOSM. Some programs have become well established in the last 10 years whereas others are new. They provide simulations of collision and vehicle trajectory to varying levels of complexity, sophistication, and ease of use.

It is a common inclination to adopt a "broad-brush" attitude toward complex subjects, leaving the details to others. This inclination is widespread among computer-program users, especially in crash reconstruction applications. It must be emphasized, however, that computer-accident reconstruction programs do not reason or evaluate; they are simply computational robots that carefully follow detailed instructions. The instructions embedded in the program, together with the input data, combine to determine the result. Programs written for one purpose may not be expected to yield accurate estimates in other situations. Programs validated for one case or series of cases may not always yield valid results in other (even similar) cases, for a variety of reasons.

It is the authors' experience that people too often lend unwarranted authority to computer program results for myraid reasons, such as

1. Programs sanctioned or distributed by government;
2. Intricate, detailed, sophisticated models are incorporated;
3. Impressive visual output graphics are produced; and
4. Results have been validated by selected application.

Often black-box programs are accepted because a personal evaluation of the innards of the box is too difficult or time consuming. This appears to be particularly true of large programs that have gained a substantial following from the government and users.

No mathematical approximation can ever represent reality exactly. Programs cannot be substituted for experience or judgment; they can only assist the analyst by doing calculations. Model limitations, coding errors, and program bugs will always plague the computer program user; it is not possible to wait for that utopia when all of these drawbacks are resolved. The informed user must be willing to understand the program in its current, if imperfect, condition and must carefully prepare and edit its input. Further, he must be ready to admit the limitations that any computer program process has, and he must use it in combination with other methods to reach educated conclusions.

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Although myraid individually styled computer routines undoubtedly exist to aid in the calculations related to automobile crash reconstruction, research sponsored by the U.S. Department of Transportation (DOT) has resulted in three major routines (SMAC, CRASH, and HVOSM) quite widely known in the scientific community. These were developed in a series of contract research projects under DOT auspices at Cornell Aeronautical Labs (CAL) (later Calspan Corporation) and are the result of considerable effort by R.R. McHenry. This pioneering work of 10 to 15 years ago resulted in substantial contributions to computer-assisted reconstruction, and because of the substantial government funding and effort involved, it also resulted in an attitude of awe and infallability that may have tended to discourage individual competitive efforts. Only recently have other computer program efforts appeared that are effective competitors for CRASH and SMAC in some applications. The authors are aware of no HVOSM competitors, other than some locally altered versions.

Most of the computer programs discussed here have evolved as use has suggested shortcomings and improvements. This evolution has often left a poorly marked trail of reasoning and documentation. In this paper an attempt is made to touch only the high points, and some insight gained from the authors' limited experience and study is shared. It is not all-inclusive, even by identification of programs, let alone descriptive or evaluative. It is hoped that this paper can assist in providing a referenced overview as an aid to further study and in inspiring participative interaction that will result in the long-range improvement of the seven programs mentioned previously, the development of better ones, and the overall utility of the computer in automobile crash reconstruction. A summary comparison of pertinent features and limitations of the seven programs is presented in Table 1.

CRASH--CALSPAN RECONSTRUCTION OF ACCIDENT SPEEDS ON THE HIGHWAY PROGRAM

CRASH in the form of CRASH3 (C1) and its predecessor, CRASH2 (C2) has probably been utilized more times than any other reconstruction program. The "damage only" option of this program has been the basis for establishment of accident severity (vehicle delta-V) in the National Crash Severity Study (NCSS) accident data base (C3) and is currently be-

TABLE 1 Summary Comparison of Reconstruction Programs

	CRASH3	EES-ARM	IMPAC	VTS	TBS	SMAC	HVOSM
Developed by	CALSPAN	D-Benz	CSE	CSE	UMTRI	CALSPAN	CALSPAN
Dimensions	2	2	2	2	2	2	3
Initial/final value problem	F	F	I	I	I	I	I
Time steps/impulsive	I	I	I	T	T	T	T
Number vehicles	2	2	2	1	1+1	2	1
Trajectory model	Yes[a]	No	No	Yes	Yes	Yes	Yes
Tire model	No	No	No	Yes	Yes	Yes	Yes
Collision model	Yes	Yes	Yes	No	No	Yes	Yes
Computer time required	Medium	Low	Low	High	High	High	Very high
Degree of input difficulty	Medium	Low	Low	Medium	Medium	High	Very high
Trajectory and tire model features							
Steering control	No	–	–	Yes	Yes	Yes	Yes
Braking control	No[a]	–	–	Yes	Yes	Yes	Yes
Traction control	No	–	–	Yes	No	Yes	Yes
Tire force model	Table	–	–	CS	CS	CS	CS
Friction limit	Circle	–	–	Elipse	Circle	Circle	Elipse
Dynamic tire normal force	No	–	–	Yes	Yes	No	Yes
Articulated vehicle	No	No	No	No	Yes	No	No
Graphics output	No	No	No	Yes	No	Yes	Yes
Trajectory parameter plot	No	–	–	DTPLOT	No	DTPLOT	No
Collision model features							
Common velocity point	Yes	Yes	Yes	–	–	No	No
Preimpact rotation	No[a]	No	Yes	–	–	Yes	Yes
Tire forces during collision	No	No	No	–	–	Yes	Yes
Multiple collisions	No	Yes[a]	Yes[a]	–	–	Yes	No
Sideswipe type collision	No	No	Yes[a]	–	–	Yes[a]	No
Crush stiffness parameters	2-linear	Tests	–	–	–	1-linear	1-linear
Stiffness varies with width	No[a]	Yes	–	–	–	No	Yes[a]
Crush profile usage	Input	Zones	–	–	–	Generate	Yes[a]
Number of points in crush profile	Six max	–	–	–	–	100 max	Many
Occupant trajectory	No	No	Yes[a]	–	No	PLOTTK	No
Crush energy output	Yes	Yes	Yes	–	–	No	No
Delta-V output	Yes	Yes	Yes	–	–	Yes	Yes

Note: CS = cornering stiffness model. DTPLOT and PLOTTK are supplemental programs used at CSE to present graphical output. PLOTTK also contains a point occupant trajectory feature; dash = not applicable.

[a] Indicates limited capability if Yes, or partial capability if No. For an elaboration of this point refer to the text describing the program.

ing used in the National Accident Sampling System (NASS) accident data collection program (C4), which is ongoing. About 45 percent of the accidents investigated in NCSS were assigned an accident severity measure by CRASH. The others were either not suitable for the CRASH algorithm or there were infulficient input data (C3).

The major advantage of using the CRASH algorithm for assessment of vehicle delta-V from damage is that it is completely independent of traditional reconstruction methods that use skid distances and momentum. The CRASH algorithm, which is based on the method proposed by Campbell (C5), requires comparative crash test data and crush measurements taken from the accident vehicles or estimated from photographs.

Measurements of impact and rest positions taken at the scene have been known to be greatly in error. In addition, it is often difficult if not impossible to adequately estimate drag factors (average vehicle deceleration from impact to rest). For example, drag factor estimation is subjective when the automobile traverses multiple surfaces with widely different friction coefficients; when it is not known if tires are braked or locked by damage; when the automobile spins to rest; or when no tire marks are left on the pavement because of wet conditions. With the increased use of antiskid brakes, traditional reconstruction methods will be even less applicable. Hence, the damage method of reconstruction, as in the CRASH3 program or by other means, is gaining in importance.

The central disadvantage of damage analysis is that it can only yield information about speed change (delta-V) or the relative approach speed of the two colliding vehicles. Road speed or speedometer speed cannot be obtained by the damage method alone. However, vehicle speed change has been found to be an important measure of injury exposure for unrestrained occupants, hence its use in the accident data and its importance in assessment of crashworthiness.

In addition to the damage method for reconstruction, CRASH3 contains a version of the more traditional reconstruction method referred to in the program documentation as Spinout Trajectories and Conservation of Linear Momentum. This method will be referred to hereinafter as Spin2+CLM. The trajectory part, Spin2, calculates postimpact velocities based on the distance between the point of impact and the point of rest, surface friction, average rolling resistance of the tires, direction of rotation, number of revolutions, and the curvature of the center-of-gravity (CG) path. The trajectory calculation is not a time-step procedure as in SMAC (C6). Instead, the programmers of CRASH have devised a complex multivariable interpolation algorithm using a matrix of coefficients based on 18 SMAC runs. This may or may not produce acceptable results. In its present form, the CRASH program does not display the result of the Spin2 procedure. An optional correction feature is present that performs five iterative runs of the TRAJ routine from SMAC to refine the Spin2 result. This is an unfinished option that usually does not converge and should not be invoked.

The momentum calculation uses the assumption that there is a common velocity at one point in the mutual crush zone (or common CG velocity in the case of colinear collision). The centroid of the crush volume of each car is selected as the common point. The collision force is directed along the line of action, which passes through the common point and has the direction specified by the user (Figure 1). Thus the user must determine the principal direction of force (PDOF) from an examination of the damaged vehicle. The user must also specify heading and slip angles of the two vehicles at impact. These three angles are combined by the program to define the

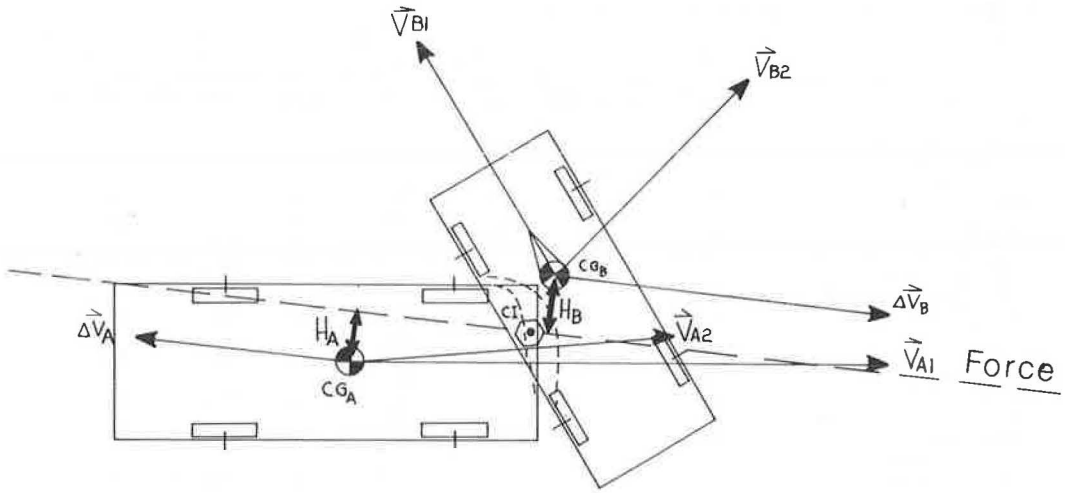

FIGURE 1 Delta-V, force line of action, and impulse moment arms.

force direction relative to the surface. The program then checks to see if the force on each car is oppositely directed, per Newton's third law. If the forces are not opposite within ±15 degrees, the computation terminates, if they are within this range they are then averaged to be made opposite.

The momentum formula is apparently coded in such a way that it has a singularity when the impact velocities are aligned (there is no singularity in the principle of conservation of momentum). In order to avoid this problem, CLM is abandoned if the impact velocities are aligned within ±10 degrees. In such cases the result of the damage option is used in combination with the Spin2 result to obtain preimpact speeds.

In practice, prespecification of the PDOF angles is difficult at best. Displacement of metal parts is an indication of PDOF, assuming that the analyst can find a suitable reference and properly take into account the complex buckling pattern of the vehicle structure. However, calculation is greatly simplified by requiring this input. Because by Newton's second law the direction of the impulse (time integral of the force) is parallel to the momentum change, the direction of the delta-V vector is specified by PDOF. This required input is one-half the desired answer, the other one-half being the magnitude of delta-V as shown in Figure 1.

The Spin2+CLM method produces good results when Spin2 is able to develop a good estimate of separation conditions and when the user accurately specifies vehicle heading at impact. Too often this is not the case. Furthermore, if the result of Spin2+CLM is not close to that of the damage method, the analyst is forced to choose one method over the other. Judging by the exclusive use of the damage method in the NCSS and NASS studies, the damage option of CRASH3 is preferred. The damage option has also been studied more extensively by the National Highway Traffic Safety Administration (NHTSA), which funded the development of CRASH, via comparison with crash tests to establish accuracy and sensitivity of the damage option for use as a statistical averaging tool (C7).

For the previously stated reasons, reference to the CRASH program reconstructions generally refers to the damage option results. The cornerstone of this success is the data base of staged crash tests on which crush energy correlation coefficients are based. Tables 8-1 and 8-2 of the CRASH3 users manual (C1) present nine categories of vehicles for which test data have been correlated. These coefficients, A, B, and G, are tabulated for frontal, side, and rear impacts for each of the nine categories. The test data on which A, B, and G depend are primarily for 1970s vintage cars.

It is important to note that the calculation algorithm used by the damage option of the CRASH program can be readily accomplished with a programmable calculator. There are three steps involved:

1. Integrate the damage profile over the crush width using appropriate crush energy coefficients, A, B, and $G = A^2/2B$. CRASH uses six equally spaced points over the crush width. Trapezoidal rule integration with unequally spaced points is easily programmed.

2. Correct the crush energy for oblique crush (PDOF at an angle to the front, side, or rear) and other nonbarrier effects. CRASH corrects for oblique crush by multiplying the integral by the factor $1+\tan^2\alpha$, where alpha is the angle between the crush direction and the surface normal. This correction factor has a physical basis when alpha is small, for example, less than 20 degrees, but becomes outlandish as alpha approaches 90 degrees. CRASH3 arbitrarily cuts off the correction factor at 45 degrees based on the recommendation by Monk and Guenther (C8) who also recommended as an alternative that the correction factor be eliminated. At 45 degrees the factor doubles the value of the integral. The analyst must also make allowances for crush damage that does not correspond to flat-face barrier crush damage from which the data are taken. Currently there are no general guidelines for dealing with such problems as underride and override, large induced crush, offset crashes, and crashes with either substantially more or substantially less crush than that of the crash tests (generally 30 or 35 mph fixed-barrier frontals, 30 mph moving barrier for rears, and 20 mph moving barrier into the door region for sides). These would appear to be fruitful research areas.

3. The final step in the procedure is the calculation of speed. For essentially colinear impacts with little or no rotation, the closing speed or the vehicle delta-Vs may be calculated knowing only the crush energy, as previously determined, and the vehicle weights. For noncolinear collisions, the vehicle delta-Vs may be found if the analyst is also able to estimate the PDOF relative to the road surface, the point of application of the force resultant on the cars, and the distance offset between the force and the CG of each vehicle (C1). CRASH

does this by interpreting vehicle heading, sideslip angle, crush profile, and PDOF relative to each vehicle. As noted previously, determination of these angles with precision is difficult. The analyst may only be able to give a wide range of possibilities that will produce a range of delta-V. When the accuracy and sensitivity of the damage option in crash was studied by the NHTSA (C7), it was reported that estimation of PDOF was the most critical measurement reported by field investigators, accounting for 18 percent error in vehicle delta-V.

Whether the damage analysis is completed by the CRASH program or by other means, it is appropriate to check the results for viability by computations based on Newton's laws either as an impulse model, such as IMPAC, or as a time-stepping model such as SMAC.

Program output for the CRASH3 program is given in the following paragraphs for the number 2 staged crash test reported by Smith and Noga (C9). All 16 of these NHTSA-sponsored tests have been reconstructed by the CRASH3 program using both the damage and Spin2+CLM options (C10). The performance chart obtained for just the damage option is shown in Figure 2, wherein Test 2 appears as cars B and b on the figure. The input for Test 2 with abbreviated output is shown in Figure 3.

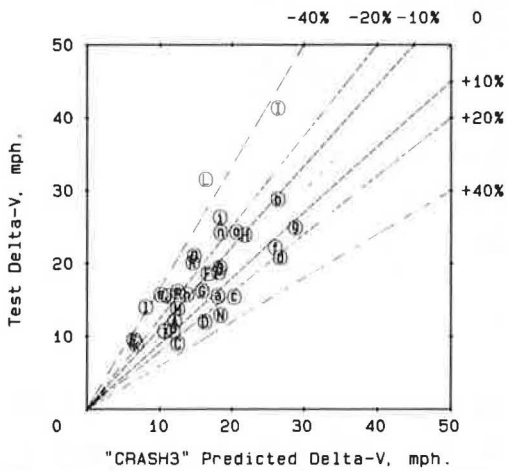

FIGURE 2 CRASH3 predicted delta-V using damage.

NHTSA is pursuing a modest development and updating program for CRASH3. Preliminary results from application of a microcomputer version called MICROCRASH were presented in October 1985 at the Volvo Delta-V workshop in Washington, D.C. Also exhibited at that time were the results of a planar graphics program driven by MICROCRASH. It is the authors' understanding that this graphics version was based on a substantial degree of interpolation of vehicle planar position, based on the three positions employed in the SPIN2 subroutine within CRASH. The accuracy of these graphics is only a rough approximation of what may have happened (oral communication of Nick Tsongas, NHTSA, October 1985).

NHTSA representatives also clarified an important issue in the same meeting. The CRASH program was never intended for litigation applications, nor does it have the accuracy needed for such application. On the other hand, it was NHTSA's intent that CRASH3 be applied primarily to provide information about crash severity that could be summarized statistically in the NCSS and NASS files, the hope being that the inaccuracies resulting from individual application of CRASH3 would balance each other and result in a reasonable estimate of statistical distributions of severity (statement by Carl Nash, NHTSA Office of Research, at Volvo Delta-V workshop, Washington, D.C., October 1985). Whether this balance is actually achieved has not been proven.

The source code Fortran listing for CRASH3 is available on tape from the National Center for Statistics and Analysis, NHTSA, U.S. Department of Transportation. Operational versions may be run via modem at 1200 baud using the computer facilities at the University of Michigan Transportation Institute, or at the Boeing Computer Services Company, Vienna, Virginia. The CRASH3 program has also been ported to personal computers (PCs) with runtime modules offered for sale for several popular PCs (C11).

A summary of crash test data is currently being compiled by the NHTSA and presently contains nearly 1,000 tests (C12). [Note that the authors' research indicates that source documentation is not available to address apparent physical inconsistencies observed in some of the tests reported.] For purposes of accident reconstruction (as contrasted with statistical data gathering), these test data provide a basis for the determination of stiffness coefficients for the accident vehicles along lines recommended by Strother et al. (C13). A summary of the equations needed to program the damage option on a calculator or a personal computer is provided in the appendix of "Crush Energy in Accident Reconstruction" (C13).

EES-ARM--EQUIVALENT ENERGY SPEED-ACCIDENT RECONSTRUCTION PROGRAM

The EES-ARM program has been widely used in Europe for speed reconstruction in automobile crashes. It is designed to evaluate the collision phase relationships, using physical principles and approximations customarily used in hand calculations (E1-E3). The method is based on the graphical Drive Balance procedure derived by Slibar (E4) in 1973 as an aid to hand-calculated reconstruction.

Like the crush damage calculations in CRASH, EES-ARM automates the methods normally usable for hand calculation of collision-phase speeds based on the principles of momentum and energy. Common to CRASH3, it requires that the user provide quality input regarding the angular relationships in the collision. This requirement is clearly stated in the documentation (E1) as contrasted with some confused claims made in the documentation for CRASH3 (C1,C10). The energy inputs are required in the form of an "energy equivalent speed" (EES) for each car, based on interpretation of crash test data and adjustments for test and vehicle mass variations. The user is fully responsible for the EES inputs and attendant crash-related analysis and stiffness evaluations. The calculation procedure also allows for solution without the EES inputs, using an alternate computation based on more complete specifications of inlet trajectory angles. The angular momentum theorem is used in the EES-ARM method only to provide a check on the calculations made independently from runout-skidmark-rest position evidence. The program itself does not calculate the runin or runout trajectory processes.

Zeidler has developed regression equations for EES values based on crash tests of three series of Mercedes Benz vehicles. The equations are two-parameter regressions based on Equivalent test deformation [(ETD), millimeters] and equivalent overlap degree [(EOD), percent overlap], with the result presented in kilometers per hour. An example, for the 201 series, is $EES = 0.491(ETD)^{0.758}(EOD)^{0.369}$,

```
CRASH3:  Calspan Reconstruction of Accident Speeds on the Highway
 NHTSA version 3, Jan 1982

 Enter a question mark (?) for help
 (Complete, Abbrev., Rerun, Print, Document, SMAC, File, Get, End)
 Which option? (first letter is fine!)                a
  1. TITLE?   NHTSA Staged Crash Test # 2 by Smith & Noga (RICSAC-2).
  2. CLASS/WEIGHTS?   4 4710 1 3261
  3. CDC/PDOF # 1?    11fdew2 -32.5
  4. CDC/PDOF # 2?    02rdew4 35.1
  5. VEHICLE 1 AND VEHICLE 2 STIFFNESS CATEGORIES?   4 1
  6. REST & IMPACT? (Y OR N)    n
 38. DAMAGE DIMENSIONS? (Y OR N)  y
 42. END DAMAGE WIDTH #1   75.5
 43. END DAMAGE DEPTH #1   .5 2.4 3.7 6.9 12.0 16.5
 44. END DAMAGE MIDPOINT OFFSET #1    0
 45. SIDE DAMAGE WIDTH #2    118.5
 46. SIDE DAMAGE DEPTH #2   6.8 22.8 23.5 21.3 10.0 0.0
 47. SIDE DAMAGE MIDPOINT OFFSET #2    13.7

                    CRASH INPUT COMPLETED

            S U M M A R Y   O F   C R A S H 3   R E S U L T S
NHTSA Staged Crash Test # 2 by Smith & Noga (RICSAC-2).
    Speed change (Damage)
          total        long.        lat.         ang.
Veh#1   18.1 mph    -15.3 mph     9.7 mph    -32.5 deg.
Veh#2   26.1 mph    -21.4 mph   -15.0 mph     35.1 deg.
Energy dissipated by damage  Veh#1  47733.9 FT-LB  Veh#2 139914.7 FT-LB

SUMMARY OF DAMAGE DATA                (* indicates default value)
            Vehicle # 1                       Vehicle # 2
Stiffness---category 4                Stiffness---category 1
Weight------  4710.0 lb.              Weight------  3261.0 lb.
CDC---------11FDEW2                  CDC---------02RDEW4
L-----------    75.5 in.              L-----------   118.5 in.
C1----------     .5 in.               C1----------     6.8 in.
C2----------    2.4 in.               C2----------    22.8 in.
C3----------    3.7 in.               C3----------    23.5 in.
C4----------    6.9 in.               C4----------    21.3 in.
C5----------   12.0 in.               C5----------    10.0 in.
C6----------   16.5 in.               C6----------      .0 in.
D-----------     .0                   D-----------    13.7
RHO---------   1.00       *           RHO---------    1.00       *
ANG---------  -32.5 deg.              ANG---------    35.1 deg.
D'----------   15.0 in.               D'----------     5.6 in.
```

FIGURE 3 CRASH3 input for RICSAC-2.

as given by Zeidler (E1). Hence a full overlap crash of a 201 Mercedes that resulted in a 20-in uniform frontal crash would be predicted to result from a 55.2 km/hr (34.0) mph barrier crash. By comparison, the CRASH3 category 3 frontal crush parameters A = 317 lb/in, B = 56 lb/in^2, and G = 901 lb predict a total crush energy of 108,000 ft-lbs, or a barrier test speed of 31.4 mph for a 70.3-in wide 3,265-lb$_f$ Mercedes 200D, neglecting restitution (C5).

It is unclear how or whether restitution effects are included in the EES method. Characteristically, in a 30-mph barrier crash the delta V felt by the occupants is 32 to 34 mph, due to restitutions of the order of 0.1. This restitution is probably somewhat higher than that observed in car-to-car impacts, however, because flat barriers do not allow intermingling yield of the stiff load-carrying structures, and hence tend to involve stiffer springback.

The input and output data tables for a typical EES-ARM application are given in Table 2 as taken from Zeidler's recent SAE paper, which also contains a more complete presentation, including a program listing (E1).

Zeilder's EES methods are complemented by several compilations of crash tests readily available to European users (E5,E6). His program is reported to be available through DEKRA in Stuttgart (E7).

HVOSM--HIGHWAY VEHICLE OBJECT SIMULATION MODEL

The first chronology of the U.S. Government-sponsored reconstruction programs was the HVOSM developed under Federal Highway Administration Contract CPR-11-3988 between 1966 and 1971. This model is intended to describe the three-dimensional motion of an automobile in space, including interaction with roadway, shoulders, ramps, berms, and the like. It is supplied in two versions emphasizing either highway design or vehicle dynamics as given in Table 3, taken from the HVOSM User's Manual (H1).

The first HVOSM version (Roadside Design: HVOSM-RD) makes provisions for simplified modeling of collisions with fixed objects. The collision deformation force is modeled by a classic linear force-deflection characteristic as $dF = KA(x)\,dx$, where x represents deformation associated with a given area A(x) over an isotropic, weightless layer surrounding a point mass approximation for the sprung mass of the vehicle. This represents a generalization to three dimensions of the crush layer model used in the planar representation of SMAC. HVOSM-RD also provides for representation of two "hard-points" within the layer, modeled by localized $F_i = k_i x$ load paths. The use of this version in actual fixed object modeling is not documented in the HVOSM Users Manual, nor is the modeling of the impact partner elucidated (H1).

TABLE 2 Input Data for EES-ARM

	Vehicle 1	Vehicle 2	Unit
Vehicle Data			
Vehicle length	L1 = 4.42	L2 = 4.96	m
Wheelbase	R1 = 2.67	R2 = 2.87	m
Mass	M1 = 1325.0	M2 = 1895.0	kg
Running-Out Conditions			
Running-out velocity	V1′ = 41.60	V2′ = 28.40	km/h
Running-out angle	Ny1′ = 350.0	Ny2′ = 171.0	Degree
Angle of rotation[a]	Fi1′ = +82.0	Fi2′ = +78.0	Degree
Coefficient of friction rotation	MyR1 = 0.15	MyR2 = 0.10	–
Running-In Conditions			
Yaw angle	Psi1 = 0	Psi2 = 181.0	Degree
Angular velocity[a]	Om1 = 0	Om2 = 0	1/s
Angle of running-in velocity	Ny1 = 0[b]		Degree
Using EES Values			
Energy equivalent speed	EES1 = 48.0	EES2 = 42.0	km/h
Equivalent test deformation	ETD1 = 0.9	ETD2 = 1.0	m
Not Using EES Values			
Angle of running-in velocity		Ny2 = -.-	Degree
Check Calculation by Theorem of Angular Momentum			
Impact force lever arm	SHA1 = 1.1	SHA2 = 1.5	m
Angle of direction[a]	Rho1 = +35.0	Rho2 = 32.0	Degree

[a](–) means clockwise rotation; (+) means counterclockwise rotation.
[b]Defined by coordinate system, in every case Ny1 = 0.

TABLE 3 Summary of HVOSM Capabilities

	HVOSM-RD Roadside Design Version	HVOSM-VD Vehicle Dynamics Version
Degrees of freedom		
Sprung mass	6	6
Unsprung mass	4	4
Steer	1	1
Wheel spin	–	4
External forces		
Tire forces	Friction circle	Friction ellipse
Impact forces	Yes	–
Aerodynamic forces	–	Yes
Rolling resistance	–	Yes
Road roughness	Yes	Yes
Terrain	Rigid-five tables	Rigid-five tables
Curbs		Yes
Suspension stops	Asymmetric-energy absorbing	Asymmetric-energy absorbing
Control inputs		
Steer table	Yes	Yes
Wheel torque table	Yes	–
Brake system pressure	–	Yes
Throttle setting and transmission ratio	–	Yes
Closed-loop driver	–	Yes

Note: Dash = not applicable.

The primary use of the vehicle dynamics version of HVOSM is for the evaluation of vehicle trajectories due to launch, vault, or handling maneuvers. HVOSM-VD includes a detailed model of the suspension, from the tire interface through the geometry to the body mass. As such, it requires that measured or assumed data be supplied for detailed inputs such as spring rates, damping rates, rear axle inertia, and even aerodynamic drag. It also contains built-in models for engine torque and drag, hydraulic brake pressure versus brake torque at a given wheel, and so forth. The HVOSM-VD version also allows for assumptions about driver control inputs. This version of HVOSM has been used successfully to predict vehicle dynamics in complex roadway design situations. The example cited most often arose from the design of ramps for a barrel-roll stunt used successfully by an automobile stunt troupe and used in a James Bond movie (H2,H3). Applications of HVOSM to real-world rollovers is limited by the inability of either standard HVOSM program to tolerate ground contact by any vehicle component other than the tires, although its ability to predict pretouchdown kinematics appears to be quite good. An improved version for touchdown and roll applications is reportedly under development at the Texas Transportation Institute (personal Communication from Donald Ivie, July 1985).

Because much of the HVOSM input data are not readily available in the open literature, and are often somewhat difficult and always tedious to measure, an auxiliary preprocessing program has been developed to predict these inputs from measurements of six 1971-1973 production automobiles including a 1971 Volkswagen Beetle and a 1973 Ford Galaxy 4-door (H1). Even with the preprocessor, the required input is voluminous and tedious. Formatting requirements are typical 1968-vintage, which makes the program accessible only to those with extraordinary patience. For those who persevere, however, HVOSM presents a timewise output of minutely detailed tables of predicted vehicle dynamic information. A postprocessor program formerly proprietary to Calspan corporation is now available to produce three-dimensional graphics (private Communication, McHenry Consultants, Cary, North Carolina).

HVOSM, in the Roadside Design version, has 11 degrees of freedom (6 for sprung mass, 4 for unsprung masses, and one steering input). It employs a friction circle tire interface model, and allows a basic road roughness to be specified over a rigid terrain specified by five input tables. It allows the specification of curb geometries and models suspension stops by asymmetric energy-absorbing relations. It allows tabular steering and wheel torque controls.

Gross distinctions between the HVOSM-RD version and HVOSM-VD are that while impact forces are missing, the VD version includes 4 more degrees of freedom (15 total) for tire spins, provisions for aerodynamic and rolling restrictive forces, and brake system modeling. It requires inputs for brake system pressure, throttle setting, transmission ratio, and closed-loop driver controls actions. A summary of advertised HVOSM capabilities is given in Table 3. Examples of the graphic output is shown in Figures 4 and 5.

A more complete but still somewhat sketchy documentation of the HVOSM models is contained in an FHWA report (H1). The program is available for use, either through DOT contract computer auspices, or it may be obtained on tape from the FHWA. A list of related references is supplied for those interested in further reading (H6-H16).

IMPAC--IMPACT MOMENTUM OF A PLANAR ANGLED COLLISION PROGRAM

The IMPAC program (I1) is intended to provide a straightforward and simple analysis of angled collisions, providing something that allows the user to at once avoid the tedious hand calculations of momentum and the complexity of SMAC (S1) while achieving a useful technical result. The conceptual model is similar to that used in the CLM part of CRASH3 (C1) in that one point within the crush zones of the two planar collision partners has the same velocity

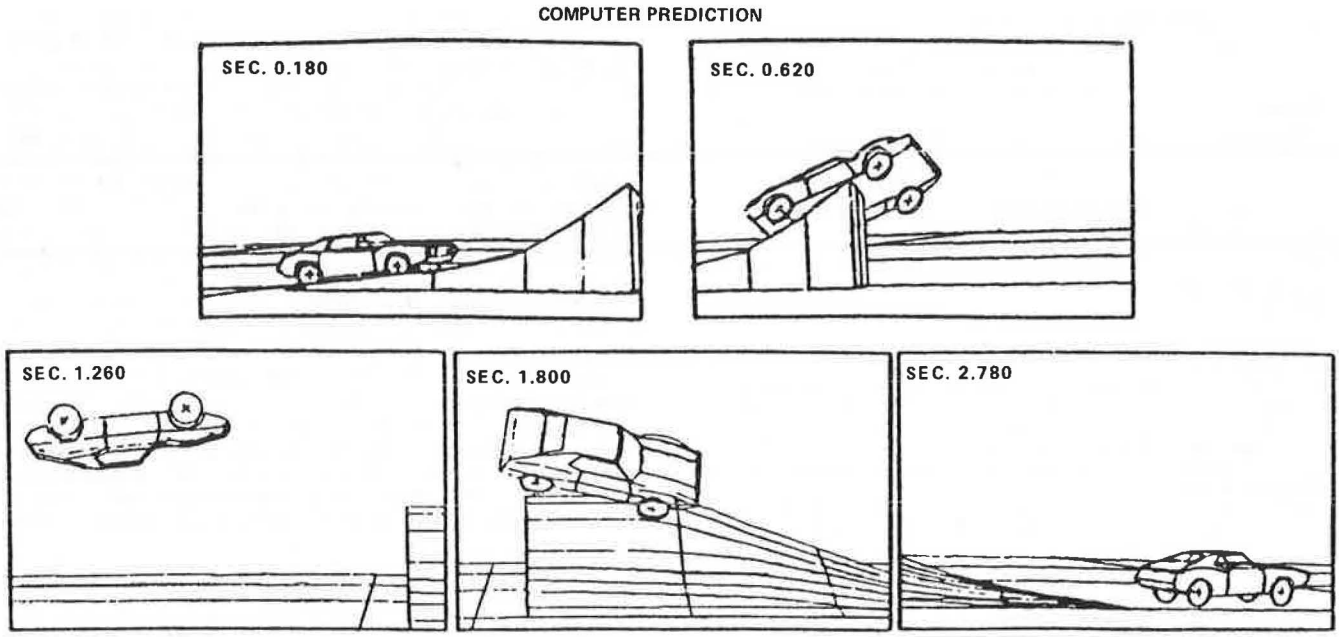

FIGURE 4 HVOSM graphics of car stunt.

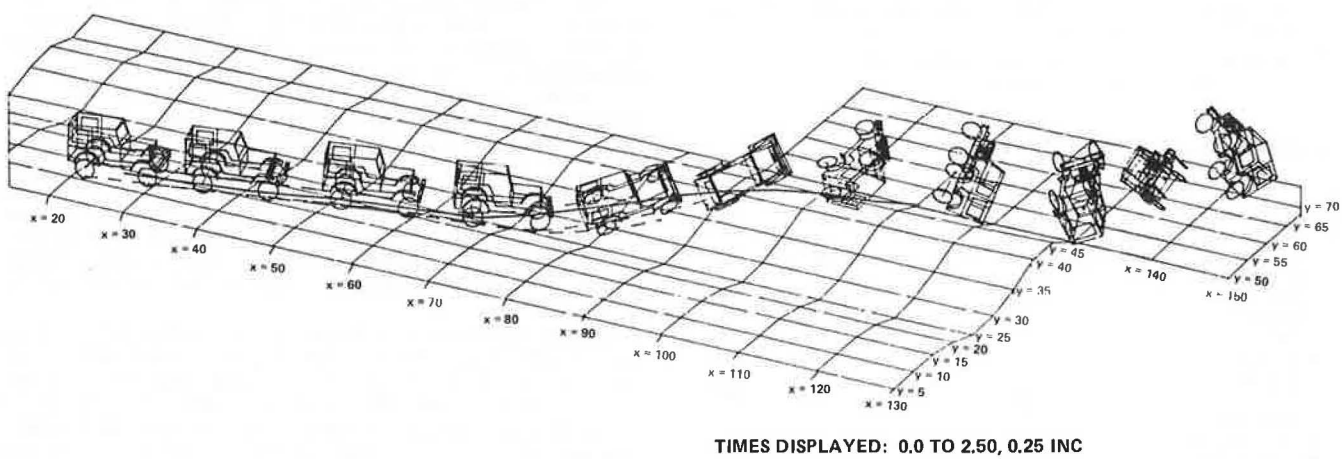

FIGURE 5 HVOSM graphics of rollover.

at the end of the momentum exchange. The similarity ends there because IMPAC is posed as an initial value problem rather than as a final value problem (the user specifies pre-impact conditions and the program calculates post-impact conditions). The math model and coded equations are also quite different because the six simultaneous equations that are solved come from four vector equations (I1). The governing equations for this planar model are (a) conservation of linear momentum, (b) conservation of angular momentum, and (c) the constraint condition of a common velocity at the center of impact.

Program IMPAC provides a simple, easily used collision model to reconstruct accidents that are beyond hand calculation. It has also been used in combination with vehicle trajectory simulation (VTS) for trajectory analysis, as a preprocessor for SMAC to reduce the number of runs required to obtain a reconstruction, and to study sensitivities in proposed crash test alignments, both car-to-car and car-to-barrier.

Because each collision is analyzed individually, cases involving multiple impacts can be examined. The output of one impact can be entered into a trajectory simulation, such as VTS, or directly back into IMPAC if the time interval is small as in a side slap. Use of the IMPAC program to reconstruct side-slap collisions has been compared to three crash tests with good results (I1,I2).

IMPAC also contains one feature that is as yet not validated because of the absence of test data, the sideswipe algorithm. Sideswipes, which imply no lockup in the crush zone, are modeled via replacement of the common velocity constraint condition with a sideswipe constraint. A slip interface plane is defined relative to one of the cars at the common contact point. Along this plane the cars may slide past one another but the velocity of each car normal to the plane must be identical and system momentum must be conserved. The user specifies the relative velocity of sliding, attempting to match the length of the contact damage for the prescribed collision time interval.

The simplicity of the IMPAC program is illustrated by the sample run that follows in which case number 2 of the NHTSA test data is analyzed (C9)

(Figure 6). Figure 7 shows the simplified geometry required by the program. Only dimensional information about the position of the centers of impulse relative to the CGs is needed. The exterior dimensions are not required.

The procedure for solution of the reconstruction problem with program IMPAC is as follows:

1. Collect all available scene data from the accident site.
2. Measure the crush deformation on the vehicle(s).
3. Estimate the total crush energy dissipated by the impact (by comparing with crash test data, comparing to the CRASH program data correlations via hand computations, or by running the damage-only option of CRASH).
4. Estimate the total runout energy dissipated from impact to rest (by hand calculations or trajectory simulation programs, etc.).
5. Estimate the runout angles, rotational directions, and an order of magnitude for the rotational rate postimpact.
6. Estimate the one point in each vehicle within the crush zone that most closely represents the point of lockup during the impact (or a representative contact point on the slip interface plane). This point is representative of the time-averaged center of impulse for the collision.
7. Estimate the vehicle weight and radius of gyration for each vehicle.
8. Make a first approximation for the preimpact speeds and heading of each vehicle.
9. Iterate by changing preimpact conditions until a solution is obtained. By solution it is meant that the predicted postimpact conditions and crush energy all correspond to their estimated values within a reasonable tolerance.
10. Perform additional computations with input perturbed from the solution run to obtain an understanding regarding the sensitivity of the result.

One minor feature of IMPAC is the output of postimpact velocity at two selected points within each vehicle. This information may be used as an aid in analyses of occupant kinematics if the points correspond to occupant contact areas such as the dash in a frontal impact or the opposite door in a side impact. In accidents with substantial postimpact rotation, the contact point may have a substantially greater or lesser velocity than the vehicle CG. The motion of the occupant contact point during the short collision interval should also be in agreement

```
Staged Crash Test #2.  NHTSA, Smith & Noga, "Examples of Staged Collisions..."
 Vehicle A:                PROGRAM IMPAC                    Vehicle B:
74 Chevelle Malibu.        Case #2 or RICSAC #2        74 Ford Pinto - 2 door
    4710.0 lbm             Vehicle mass                        3261.0 lbm
      60.9 inches          Radius of gyration                    49.5 inches
      89.0 inches          Impulse center, fwd from veh CG       13.0 inches
        .0 inches          Impulse center, lat from veh CG       23.0 inches
     -30.0 degrees         Vehicle heading at impact             90.0 degrees
      31.3 mph             Forward speed at impact               31.3 mph
        .0 mph             Lateral speed at impact                 .0 mph
        .0 deg/sec         Pre-impact rotational velocity          .0 deg/sec

      24.0 inches          Selected point-1, fwd from CG           .0 inches
     -18.0 inches          Selected point-1, lat from CG         24.0 inches
      24.0 inches          Selected point-2, fwd from CG         30.0 inches
      18.0 inches          Selected point-2, lat from CG         24.0 inches

        .0 deg             Angle of slip plane (relative to vehicle A)
        .0 % slip          Slip velocity, % of approach vel along slip plane.
       80. millisec        Time duration of impulse       version: R85L01W25.

Staged Crash Test #2.  NHTSA, Smith & Noga, "Examples of Staged Collisions..."
A: 74 Chevelle Malibu.     PRE-IMPACT CONDITIONS   B:  74 Ford Pinto - 2 door
              -30.0 deg    Vehicle Heading                          90.0 deg
  31.3 mph @ -30.00 deg    CG Velocity                 31.3 mph @  90.00 deg
    0. deg/sec             Rotational Velocity                   0. deg/sec
 154254.        0.ft-lb   Linear & Rotational KE     106799.        0.ft-lb

------------------------   IMPACT CONDITIONS          ---------------------
  19.6 mph @ 135.27 deg    Crash Severity Index  (^V)  28.2 mph @ -44.73 deg
         -4060. lbf-sec    Longitudinal Impulse               -2954. lbf-sec
          1067. lbf-sec    Lateral Impulse                    -2983. lbf-sec
      2.4 ft &  -1.0 ft    X & Y Impact Motion of CG       1.2 ft &   2.5 ft
                           Approximate Mutual Crush                  3.18 ft
                           Impulse Duration                     80 milli-sec
                           Approach Velocity  - CG's   54.2 mph @ -60.00 deg
                           Separation Velocity- CG's   15.0 mph @-117.23 deg
                           Slip Velocity along plane     .0 mph @ -27.14 deg
Approach Vel @ IC's:  45.5 mph tangential &  -29.4 mph normal to slip plane.
                           Crush Energy of Collision          161010. ft-lbf

------------------------   POST-IMPACT CONDITIONS     ---------------------
              -24.3 deg    Vehicle Heading @ runout                 93.6 deg
  13.3 mph @  -8.14 deg    Runout velocity             23.1 mph @  29.66 deg
  143. deg/sec             Runout rotational speed      90. deg/sec
   8.4 ft                  Runout Work / Weight        18.5 ft
  21.1 mph @  26.21 deg    Velocity @ impulse-center   21.1 mph @  26.21 deg
  17.0 mph @   -.09 deg    Velocity @ point-1          22.2 mph @  24.76 deg
  12.7 mph @  10.44 deg    Velocity @ point-2          19.7 mph @  27.79 deg
  28056.    11735.ft-lb    Linear & Rotational KE      58122.     2129.ft-lb
```

FIGURE 6 Sample run from IMPAC program.

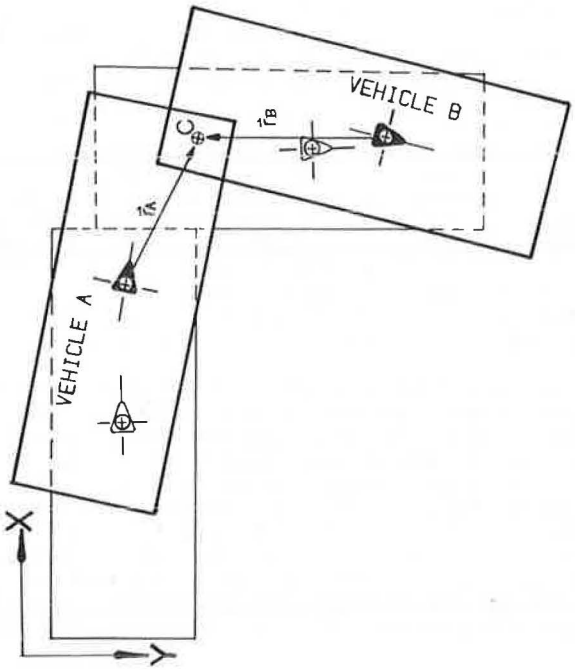

FIGURE 7 Center of impulse, point C.

with or evidence of car heading and alignment pre-impact.

Program IMPAC has been compared with 16 staged crash tests that were performed by the NHTSA for such purposes (C9). Figure 8 shows graphically the result of this comparison as tabulated in the introductory documents (I1,I2).

IMPAC may be accessed at CSE via telephone or modem at 1200 or 2400 baud from a variety of terminals or personal computers. The program operates on an HP-9000 series 500 computer under the UNIX operating system (HP-UX). Those who wish to experiment with the IMPAC or VTS programs may do so by contacting the authors to establish a dial-up connection.

SMAC--SIMULATION MODEL OF AUTOMOBILE COLLISION PROGRAM

The SMAC computer program was developed by McHenry for the U.S. Department of Transportation (DOT)

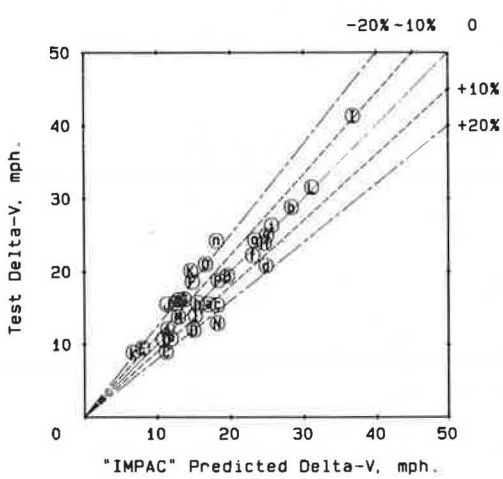

FIGURE 8 IMPAC predicted delta-V.

(S1-S6). SMAC represents each of (up to) two cars as a rectangular planar chassis with four contact points (wheels) on a planar ground surface. Each vehicle mass is surrounded by a crushable layer (body) characterized by a linear force-deflection relationship, whereby deflection is measured parallel to the longitudinal or lateral axis of the vehicle, depending on impact location. Intervehicular normal crush forces are generated by interacting crush zones subjected to local static force balance. Tangential crush forces are calculated from a frictional force model based on a circumferentially uniform friction coefficient. Residual crush depths and separation velocities are calculated from a rebound model that is based on a concept that uses energy rather than velocity as a separation criterion.

Pavement tire forces are calculated from a friction circle model that encompasses constant normal forces, tabulated wheel torques and pre-set cornering stiffnesses from each individual tire, and pre-set roadway friction, which may be represented differently on either side of a friction boundary along the planar roadway surface.

To conduct SMAC runs, vehicle geometry, mass, yaw inertia, and tire properties, together with time-dependent braking and steering, data are tabulated in program input for each vehicle; intervehicle friction and restitution are selected, and one or two roadway friction regions are described. Initial conditions of velocity and position in two rectangular and one angular coordinate are described for each automobile, and simulation control inputs are inserted to initiate and terminate the computer run (see Figures 9 and 10). The computer calculates individual tire forces and intervehicle crush forces for this initial-value problem at preassigned time steps by a Runge-Kutta integration scheme. The result is a time-based tabulation of position, velocity, and acceleration of the mass centers, planar outlines, and wheel contact locations of each vehicle in rectangular coordinates. Basic graphic subroutines exhibit tire tracks and residual crush profiles (see Figure 11).

DOT contract research has been devoted in efforts to upgrade the fidelity and efficiency of SMAC. James et al. (S7) introduced modifications designed to improve narrow-object crash simulations, to model a sloping terrain, and to include hard spots in the vehicle crush layer. Chi et al. (S8) revised the numerical integration scheme. Another DOT project has introduced optimization logic to semiautomate the process of matching input conditions and rest positions (S9).

The authors' experience with SMAC is common with that of Jones (S10,S11) with respect to spinout trajectory. The authors, too, have found it necessary to alter cornering stiffness values to effect appropriate trajectories in some cases involving shifting tire normal forces. This is cumbersome because it requires stopping and restarting the entire SMAC program. The problem was treated with a first-approximation simulation to the pitch and roll degrees of freedom. This simulation has since been abandoned with the advent of VTS as described in another section of this paper.

In earlier papers, the authors identified some programming errors and conceptual problems with SMAC that arose from early phases of industry-sponsored research with the program (S12-S15). Changes were proposed and implemented in the model features dealing with restitution, tire-force calculations, crush layer, and integration techniques. The revised model was identified by the name PRED (S14,S15). In addition, an input-preparation/editor program called SMACED (S14,S15), was developed to ease the task of preparing an input file for SMAC or PRED.

```
                    Simulation Model of Automobile Collisions (SMAC)
                    Example Problem

                         INITIAL CONDITIONS                                    CALCULATION CONSTANTS
     VEHICLE NO. 1                          VEHICLE NO. 2

XC10'    = 140.400 INCHES              XC20'    =  22.900 INCHES              DELPSI   =   3.000 DEGREES
YC10'    =  -4.000 INCHES              YC20'    = 120.000 INCHES              DELRHO   =    .200 INCHES
PSI10    =  82.500 DEGREES             PSI20    =   2.500 DEGREES             LAMBDA   =  12.000 LB/IN,PRESSURE ERROR
PSI1D0   =    .000 DEG/SEC             PSI2D0   =    .000 DEG/SEC             ZETAV    =   5.001 IN/SEC,MIN.FOR FRICT
U10      = 470.000 IN/SEC              U20      = 425.000 IN/SEC
V10      =    .000 IN/SEC              V20      =    .000 IN/SEC

            DIMENSIONS AND INERTIAL PROPERTIES                              DEFORMABLE LAYER
A1       =  52.000 INCHES              A2       =  54.450 INCHES             KV1      =  50.000 LB/(IN**2)
B1       =  42.500 INCHES              B2       =  66.550 INCHES             KV2      = 124.500 LB/(IN**2)
TR1      =  52.500 INCHES              TR2      =  63.500 INCHES             MU,FRICT =   1.000
I1       =  12751. LB-SEC**2-IN        I2       =  46972. LB-SEC**2-IN       C0       =    .000 RESTITUTION
M1       =   5.311 LB-SEC**2/IN        M2       =  10.622 LB-SEC**2/IN       C1  =   .10000E+00   VERSUS
PSIR10   =    .000 DEGREES             PSIR20   =    .000 DEGREES            C2  =   .50000E+02 DEFLECTION
XF1      =  79.100 INCHES              XF2      =  93.850 INCHES
XR1      = -79.500 INCHES              XR2      =-122.150 INCHES
YS1      =  30.500 INCHES              YS2      =  39.900 INCHES

                                          TIRE PROPERTIES
                    CORNERING STIFFNESS                                       TIRE-TERRAIN COEF AND TERRAIN ZONES
C(1)     =  -8580. LB/RAD              C(5)     =  -8580. LB/RAD             XB1' = 900.000 IN.     YB1' = 252.000 IN.
C(2)     =  -8580.  ''                 C(6)     =  -8580.  ''                XB2' =-900.000 IN.     YB2' = 252.000 IN.
C(3)     =  -8580.  ''                 C(7)     =  -8580.  ''                XMU1 =    .700
C(4)     =  -8580.  ''                 C(8)     =  -8580.  ''                XMU2 =    .700
                                                                            CMU  =   .00000E+00

     PSIB RANGE TESTS                     PSIBI FOR RHOBI TESTS              PROGRAM CONTROL DATA
     COLLISION CRITERIA                   COLLISION CRITERIA
PSILIM1  =  70.000 DEGREES             PSILIM5  =  10.000 DEGREES           TO       =    .000 SEC.,BEGIN
PSILIM2  = 110.000   ''                PSILIM6  = 170.000   ''              TF       =    .500  ''  END
PSILIM3  = 250.000   ''                PSILIM7  = 190.000   ''              DTTRAJ   =    .050  ''  INTEG. INTVL,TRAJ
PSILIM4  = 290.000   ''                PSILIM8  = 350.000   ''              DTCOLL   =    .001  ''  INTEG.INTVL,COLL
                                                                            DTCOLT   =    .010  ''  INTEG.INTVL,CPOS
                                                                            DTPRNT   =    .010  ''  PRINT INTERVAL
                                                                            UVMIN    =    .000 IN/SEC STOPPING TEST
                                                                            PSIDOT   =    .000DEG/SEC STOPPING TEST
                                                                            NO.OF VEHICLES = 2.
                                                                             FMOVIE =  0.(ZERO,FINAL DAMAGE TABLE TAPE
                                                                                       (NON-ZERO,DAMAGE HISTORY TAPE
                                                                                       (ALSO WRITTEN ON FORTRAN 2.
                                                                                          (TAPE IS ALWAYS FORTRAN 1)
```

FIGURE 9 SMAC sample input file—example problem.

The source code Fortran listing for SMAC is available on tape from the National Center for Statistics and Analysis, NHTSA, U.S. Department of Transportation. Operational versions may be run via modem at 1200 baud using the computer facilities at the University of Michigan Transportation Institute or at the Boeing Computer Services Company, Vienna, Virginia.

A combined collision and trajectory model that in many ways is similar to SMAC although independently designed and coded has recently been developed by the Japan Automobile Research Institute (S16). A released version of the "J2DACS" program (JARI 2 Dimensional Automobile Collision Simulator) is anticipated in mid-1986.

TBS--TRACTOR BRAKING AND STEERING SIMULATION

The TBS simulation was developed at the Highway Safety Research institute (HSRI) under the sponsorship of the Motor Vehicle Manufacturers Association

```
                    Simulation Model of Automobile Collisions (SMAC)
                    Example Problem

((PSIFI(I,K),I= 1, 2),K=1,   7) STEER TABLES ALL ZERO FOR VEHICLE NO. 1

((PSIFI(I,K),I= 3, 4),K=1,   7) STEER TABLES ALL ZERO FOR VEHICLE NO. 2

VEHICLE NO. 1
            TRACTIVE OR BRAKING FORCE LB                TRACTIVE OR BRAKING FORCE LB                TRACTIVE OR BRAKING FORCE LB
     SEC      RF       LF       RR       LR       SEC      RF       LF       RR       LR       SEC      RF       LF       RR       LR
    .000 -322.90 -322.90 -394.60 -394.60       .200 -322.90 -322.90 -394.60 -394.60       .400 -322.90 -322.90 -394.60 -394.60
    .100 -322.90 -322.90 -394.60 -394.60       .300 -322.90 -322.90 -394.60 -394.60       .500 -322.90 -322.90 -394.60 -394.60

VEHICLE NO. 2
            TRACTIVE OR BRAKING FORCE LB                TRACTIVE OR BRAKING FORCE LB                TRACTIVE OR BRAKING FORCE LB
     SEC      RF       LF       RR       LR       SEC      RF       LF       RR       LR       SEC      RF       LF       RR       LR
    .000     .00      .00 -102.50 -102.50       .200     .00      .00 -102.50 -102.50       .400     .00      .00 -102.50 -102.50
    .100     .00      .00 -102.50 -102.50       .300     .00      .00 -102.50 -102.50       .500     .00      .00 -102.50 -102.50
```

FIGURE 10 SMAC input file—continued.

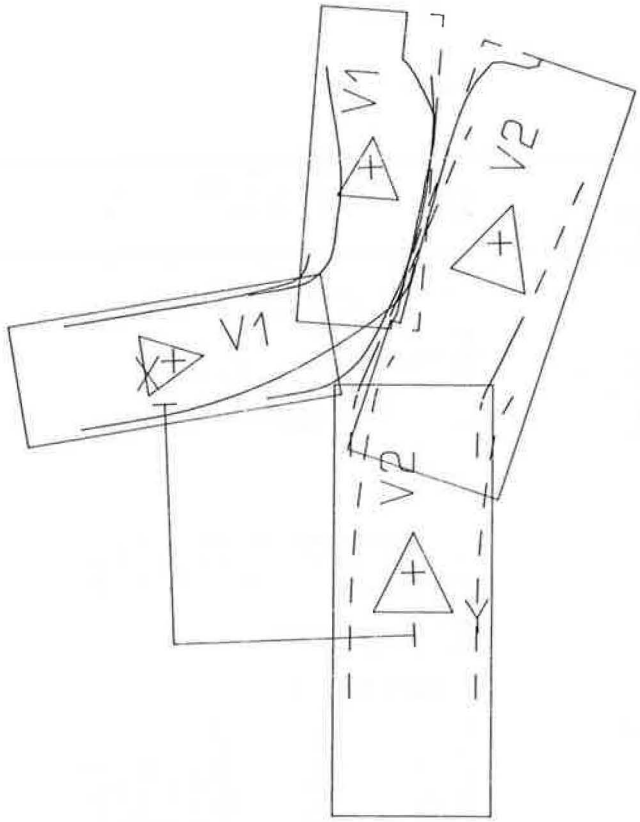

FIGURE 11 SMAC graphics—example problem.

(MVMA) in addition to earlier more comprehensive programs (T1-T3). During the course of development of the earlier programs, it became apparent that there was a need for less complex simulations that could be run interactively using minimal I/O. The BRAKES2 simulation was developed to simulate the straight-line response of commercial vehicles to a step brake input (T4). Following BRAKES2, the TBS simulation was developed. This simulation contains a simplified vehicle model for predicting the directional response of commercial vehicles to braking or steering inputs, or both. The simulation consists of two interactive computer programs--one for a straight truck (TBSTR) and the other for a tractor-trailer (TBSTT).

The mathematical model for TBS was constructed using the model developed by Leucht (T5) as a starting point. Additions and changes, particularly with respect to the tire model, were made to produce the present simulation.

The TBS simulation was formulated and programmed to describe the directional dynamics of a tractor-trailer. A similar model was then developed for a straight truck by simplifying the tractor-trailer model. The following discussion treats the tractor-trailer model only because the truck model is a simple derivative of the tractor-trailer model.

The vehicle model consists of two rigid bodies: one for the tractor and the other for the trailer. The model has four degrees of freedom, namely, the longitudinal velocity and the lateral velocity of the tractor, the yaw rate of the tractor, and the articulation angle of the trailer relative to the tractor. There are no roll or pitch degrees of freedom. Load transfers, both longitudinal and lateral are computed quasistatically.

In the simulation the hitch is assumed to transmit a yaw moment (but not a roll or pitch moment) through the hitch due to friction. The normal load on each wheel is equal to the sum of the static load on that wheel and the load transfer (both longitudinal and lateral) taking place at any instant of time. The load transfer at the trailer wheels is based on the trailer CG height, the hitch height, the forces on the trailer at the hitch and the road, the track width of the trailer, and the distance between the fifth wheel and the trailer axle.

The load transfer on the tractor wheels is not quite so straightforward. The apportionment of the lateral load transfer between the front and rear axles of the tractor depends on the properties of the suspension system, which are not included in the simple TBS simulation. The user must input the parameter that defines the fraction of the total lateral load transfer that takes place at the front axle of the tractor. The remaining fraction of the lateral load transfer takes place on the rear axle (or axles) of the tractor.

A simplified model for tandem axles is included. The properties of all the tires at both axles in the tandem pair are assumed equivalent and are specified for one tire. A quasistatic interaxle load transfer is specified by entering a load transfer coefficient for the tractor tandem axles and the trailer tandem axles.

The simulation incorporates the "friction circle" model for computing tire forces. An antilock model is included by supplying lateral and longitudinal antilock effectiveness coefficients. Dual tires are treated as two single tires, each sharing the vertical load on them equally and each yielding the same longitudinal and lateral forces.

Braking is handled in the model by specifying the time history of attempted brake force for the brakes on each side of each axle. This allows brake imbalance to be simulated. For a tandem axle pair, the two sets of brakes on one side of the tandem axles are assumed equivalent. Steering inputs are also entered as a table consisting of the time followed by the average steer angle for the front wheels.

The program is designed so that the user answers questions or enters data in response to questions or commands from the computer. In addition, data for the program may be optionally input from a file. A sample set of input parameters is shown in Figure 12. There are 83 output variables for the articulated vehicle and 52 for the straight truck. Each of these may be displayed as a function of time. A list of the available output variables is shown in Figure 13. The user specifies the number of output variables (six maximum), their identifying numbers, and the time step on which the output file is to be printed. After this output has been echoed, the user may demand an additional six output variables in the same manner and repeat until the desired output variables are obtained. There is no graphical output for the TBS simulation in its present configuration.

UMTRI has two additional tractor-trailer simulations for use in reconstructions that require use of more complex simulations: PHASE4 (T6) and YAW/ROLL (T7). PHASE4 was developed in 1980 for the MVMA as a consolidation of previous models. It is a nonlinear, time domain simulation of a tractor with an optional semitrailer and up to two additional full trailers. PHASE4 is applicable in directional response studies in which the influence of braking parameters such as brake pads, hysteresis, proportioning, antilock logic, stopping distance, brake timing, effect of split friction surfaces, and other braking performance parameters are to be considered. When used to study cornering performance behavior, the program provides a more realistic simulation of understeer and oversteer properties of articulated ve-

```
0,0,0          41 White tractor, Fruehauf trailer.  Page 23, 41 of manual.
1,1,1,1        42 Tandem/dual key: tractor tandem,dual; trailer tandem,dual
14970.         01 GVW1   Wt of tractor, (lbs).
11160.         02 GVW2   Wt of trailer, (lbs).
241636.        03 IZZ    Tractor yaw inertia, (in-lb-sec**2).
736983.        04 ITZZ   Trailer yaw inertia, (in-lb-sec**2).
54.4           05 AA     Dist. between tractor tandem axles, (in).
49.3           06 AAT    Dist. between trailer tandem axles, (in).
63.9           07 A1     Dist. from tractor CG to front axle, (in).
78.1           08 A2     Dist. from tractor CG to rear axle, (in).
261.2          09 A3     Dist. from trailer CG to 5th wheel, (in).
104.8          10 A4     Dist. from trailer CG to trailer rear axle, (in).
0.             11 BB     Tractor rear axle to 5th wheel, (in) (aft is neg).
40.            12 TRA1   One-half the tractor front track width, (in).
36.            13 TRA2   One-half the tractor rear track width, (in).
36.            14 TRA3   One-half the trailer track width, (in).
48.            15 Z0     Height of 5th wheel above ground, (in).
39.9           16 Z1     Height of tractor CG above ground, (in).
55.5           17 Z2     Height of trailer CG above ground, (in).
.05            18 MU5    5th wheel friction coefficient.
19.            19 RAD5   Equivalent radius of 5th wheel, (in).
.16            20 GAM1   Tractor front axle lateral load X-fer coef.
-.375          21 GAM3   Tractor tandem axle load X-fer coef.
-.375          22 GAM4   Trailer tandem axle load X-fer coef.
26.8           23 VEL    Initial forward velocity, (mph).
6.             24 TIMF   Max simulation time for run, (sec).
30.            25 IQUIT  Max articulation angle allowed, (deg).
467.           29 CALF(1) Cornering stiffness of front tires, (lb/deg).
208.           30 CALF(3) Cornering stiffness of tractor rears (lb/deg).
200.           31 CALF(7) Cornering stiffness of trailer rears (lb/deg).
n                 Do all tires have the same friction curve?
.942           32 MUP(1) Peak friction coef, tractor front.
.939           33 MUP(3) Peak friction coef, tractor rear.
.960           34 MUP(7) Peak friction coef, trailer rear.
.895           35 MUS(1) Sliding friction coef, tractor front.
.895           36 MUS(3) Sliding friction coef, tractor rear.
.895           37 MUS(7) Sliding friction coef, trailer rear.
.11            38 SP(1)  Slip corresponding to peak friction - tractor front.
.11            39 SP(3)  Slip corresponding to peak friction - tractor rear.
.11            40 SP(7)  Slip corresponding to peak friction - trailer rear.
0.0            41 PW     Payload weight, (lbs).
7                 number of lines in brake force table. (15 max)
0.000, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0,     time,F(1),F(2),F(3),F(4),F(7),F(8)
2.190, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0,
2.215,77.4,77.4, 0.0, 0.0, 0.0, 0.0,
2.315,387.,387.,420.,420., 0.0, 0.0,
2.385,604.,604.,713.,713.,598.,598.,
2.410,682.,682.,713.,713.,812.,812.,
2.443,682.,682.,713.,713.,1094.,1094.,
2                 Number of lines in steering table.  (25 max)
0.,0.,                                   time, average steer angle.
1.,4.62,
```

FIGURE 12 TBS sample input file.

hicles, tandem-axle effects, jackknife prediction, and suspension effects.

The Constant Velocity Yaw/Roll program simulates the turning and rolling behavior of motor vehicles in constant speed maneuvers. Turning may be controlled either by defined steering versus time or by a driver model following a prescribed trajectory. In the absence of a brake model, YAW/ROLL features an expansion of axle and articulation arrangements for prediction of stability and turning behavior of articulated vehicles. A truck with up to three trailers may be examined with multiple-axle configurations and different types of hitching mechanisms between units.

The source code Fortran listing for TBS (and also PHASE4 and YAW/ROLL) is available on tape from the UMTRI (C12). Operational versions of TBSTT and TBSTR may be run via modem at 1200 baud using the computer facility at UMTRI.

VTS--VEHICLE TRAJECTORY SIMULATION PROGRAM

The VTS program (V1) simulates the trajectory of one vehicle on a horizontal surface. Its application to accident reconstruction is to study the preimpact and postimpact motion of a vehicle (automobile or two-axle truck, no trailers). The information obtained may then be used as parametric data for separate collision programs, using the modular approach to accident reconstruction.

Preimpact motion is studied to define vehicle capabilities in time, such as steering and braking vehicle responses based on assumed driver inputs. VTS is also useful in studying the vehicle response to sudden changes in surface friction (patches) when undergoing a maneuver such as cornering or braking.

The fictitious example VTS run of Figures 14 and 15 illustrates this capability whereby an unladen pickup encounters a patch of black ice when rounding a corner at 55 mph on an unbanked turn. The cause of the sudden rotation and loss of control is seen to be the change from a low friction to a high friction surface coupled with partial braking. Partial braking for this unladen pickup leads to rear brake lockup on the ice patch and exaggerates the small but highly significant rotation that takes place as the pickup slides across the patch. As the front tires leave the ice patch they have a slip angle because of the rotation and develop a large cornering

Variable #	Variable Name
*** POSITION VARIABLES ***	
1	XO-COORD
2	YO-COORD
3	PSI
4	GAMMA
*** VELOCITY VARIABLES ***	
5	U-VEL
6	V-VEL
7	PSIDOT
8	GAMMADOT
***	
9	TURN RAD
10	SIDESLIP
*** TIRE SLIP ANGLES ***	
11	ALFA 1+2
12	ALFA 3+4
13	ALFA 5+6
14	ALFA 7+8
15	ALFA9+10
*** ACCELERATION VARIABLES ***	
16	U-DOT
17	V-DOT
18	PSI-DDOT
19	GAM-DDOT
20	LONG ACC
21	LAT. ACC

Variable #	Variable Name
*** TIRE-ROAD INTERFACE FORCES ***	
*** BRAKE FORCES: FX(I), SIDE FORCES FY(I) ***	
22-31	FX(1-10)
32-41	FY(1-10)
*** LOAD TRANSFERS, LONG. DFX(I), LAT. DFY(I) ***	
42-51	DFX(1-10)
52-61	DFY(1-10)
*** INSTANTANEOUS LOAD FORCES ***	
62-71	FZ(1-10)
*** PROGRAMMED BRAKE FORCES ***	
72-81	FSX(1-10)
*** HITCH FORCES ***	
82	XH
83	YH

FIGURE 13 TBS output variables.

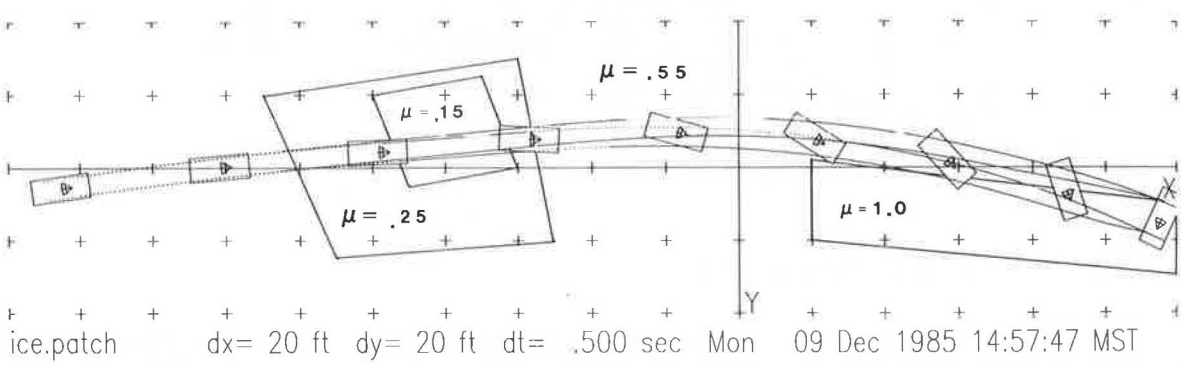

FIGURE 14 VTS graphic for ice patch run.

force. The result is similar in effect to that of a large and rapid steering input. VTS also reveals that the timing of this event is less than normal reaction times. This combination of factors illustrates one way in which a vehicle may leave the inside of a curve sideways or rearward. When rerun with full locked wheel braking (not shown here) VTS predicts that the vehcle will slide off the outside of the curve.

VTS's use in simulation of postimpact motions helps to define postimpact velocities and rotation rates or to determine the average deceleration of a damaged rotating automobile. These parameters are needed for subsequent collision calculations. Given an estimate of the point of impact and point of rest, VTS is used in an iterative manner to estimate launch conditions that produce plausible trajectories to rest. This application, though obviously tedious if the surface frictional conditions are not well established, is facilitated by graphical display of the output as it develops and by the ease in making changes to the input for the next iteration.

The VTS model is two-dimensional (planar) except that normal tire loads are adjusted for quasistatic weight shift associated with acceleration. Steering and traction (or braking) may be applied to any or all of the four wheels. All forces acting on the vehicle come from the tire-surface interaction.

The simulation surface is a horizontal flat plane with a uniform friction coefficient except for quadrilateral patches of user-specified size that may be assigned a different friction.

Tire longitudinal forces are set by a table of requested traction (braking) coefficients versus time, which may be viewed as requested friction coefficients (longitudinal force = traction coefficient * static equilibrium tire normal load).

Tire lateral force is defined by cornering stiffness and slip angle. The cornering stiffness model assumes that cornering stiffness for a given tire can be described as a parabolic function of tire load given the unloaded, peak, and overload values of cornering stiffness (V1). The limiting frictional condition is either the friction circle, as used in

```
! file - ice.patch  Unladen Pickup on a Curve.
RUN  Ice patches at 0.25 & .15 on road of 0.55 friction.
CASE Black Ice Study
USER RLW - CSE
!        xmin, xmax, ymin, ymax, friction
SURFACE -200., 120., -40., 40., .55
!        x1,   y1,   x2,   y2,   x3,   y3,    x4,  y4, friction
PATCH  -130., -20., -60., -30., -50.,  20., -110., 25., 0.25
PATCH  -100., -20., -70., -25., -60.,   0., -90.,   5., 0.15
PATCH    20.,  20.,  20.,  -2., 120.,  10., 120.,  30., 1.00
!           wb,  ftw,  rtw, CG%, weight, yaw_rg, CG_height
VEHICLE  117.8, 64.5, 65.5, 40.,  4400.,  61.3,     24.
!    loc, tire#, stiffness, offset_x, offset_y, offset_steer
WHEEL rf,   1,     1.0,        0.0,     0.0,      0.0,
WHEEL lf,   1,     1.0,        0.0,     0.0,      0.0,
WHEEL rr,   2,     1.0,        0.0,     0.0,      0.0,
WHEEL lr,   2,     1.0,        0.0,     0.0,      0.0,
!  tire#, model, cs_none, cs_nom, cs_over, nom_load, overload, frx, fry
TIRE   1,circle,   48.8,   164.2,  181.5,    1320.,    1720.,  1.0, 1.0
TIRE   2,circle,   48.8,   135.6,  181.5,     880.,    1720.,  1.0, 1.0
!         start_time, end_time, minimum_step, output_step, max_error
SIMULATION    0.0,      5.0,       .001,          .1,         .01
!   tick_x, tick_y, plot_interval, foh, roh, width
PLOT   20.,   20.,       .5,       32.5, 43.8,  81.,
!         xo,    yo, heading,  uo,  vo, yaw_speed, system
INITIAL -184.,  5.5,  -8.0,    88.,  0.,   4.8,VEHICLE
STEERING @steer.curve
TRACTION @brake.on.off
END

! file - steer.curve
! Constant radius turn of 1040 feet.
!-----------------------------------
! Time   Steer-angles (degrees cw)
!(sec)   (lf)   (rf)   (lr)   (rr)
!-----  --------------------------
0.0, 0.934, 0.934, 0.0, 0.0
1.8, 0.934, 0.934, 0.0, 0.0
2.5,-10.00,-10.00, 0.0, 0.0

! file - brake.on.off
! Brake slowing on ice patch (rear lock).
! Unladen pickup with brake apply, then release.
!Traction coefficients are dimensionless.
!Traction coefficients may be viewed as requested friction coefficients.
!Traction force - traction coefficient * static equilibrium tire load.
!-------------------------------------------------------------------
! Time  Traction coefficients (braking coefficients are negative)
!sec., lf,   rf,   lr,   rr
0.0,  0.0,  0.0,  0.0,  0.0
0.5,  0.0,  0.0,  0.0,  0.0
0.8, -.21, -.21, -.44, -.44
1.8, -.21, -.21, -.44, -.44
2.1,  0.0,  0.0,  0.0,  0.0
3.0,  0.0,  0.0,  0.0,  0.0
3.3, -.70, -.70, -.93, -.93
```

FIGURE 15 VTS input for ice patch run.

the SMAC program (V2), or the friction ellipse as used in the HVOSM program (H1), which provides for a different friction for side force.

Wheel position may have a damage offset from that defined by wheelbase and tread width. Each wheel may also be oriented to simulate damage and may be associated with one of several tire models.

Tire forces are calculated in the coordinate system of each individual tire, then transformed to the vehicle coordinate system where they are summed at the vehicle CG. The resultant force and torque are transformed to the surface coordinate system where the vehicle accelerations are defined by means of Newton's second law of motion. The resulting system of three second-order ordinary differential equations are solved as a system of six first order differential equations using a variable step, variable order Adams predictor corrector method.

VTS may be accessed at Collision Safety Engineering (CSE) via modem at 1200 or 2400 baud from a variety of terminals or personal computers. Graphics is supported only on HP terminals at present. The program operates on an HP-9000 series 500 computer under the Unix (HP-UX) operating system. Those who wish to experiment with the VTS or the IMPAC programs may do so by contacting the authors to establish a dial-up connection.

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Publication of this paper sponsored by Committee on Traffic Records and Accident Analysis.

Efficacy of Jurisdiction-Wide Traffic Control Device Upgradings

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ABSTRACT

An evaluation of several jurisdiction-wide traffic control device upgradings in Michigan was undertaken. The upgradings included only signs. A "before-after with control" experiment design was employed in the examination of general accident distributions along with a more detailed distribution of vehicle-vehicle accidents. Results of assessing the overall effectiveness of traffic control device (TCD) upgradings on a jurisdiction-wide basis were mixed at best. The general variability of accident statistics and because most sites in a jurisdiction have only minor, if any, problems, potential positive results tend to be overwhelmed at sites where there may be significant improvements. It is suggested that safety-effectiveness studies are more appropriate at lower levels of aggregation.

Federally supported programs for inventorying and subsequently upgrading traffic control devices (TCDs) within specific local jurisdictions have long been considered effective investments in highway safety. The purpose of such upgrading is to bring all TCDs and their placement into compliance with the Manual on Uniform Traffic Control Devices (MUTCD) published by the FHWA, U.S. Department of Transportation. Although there are numerous studies on the effectiveness of specific devices at specific locations, few have been explicitly concerned with the evaluation (in terms of safety measures) of jurisdiction-wide programs. The study described here was undertaken with the objective of quantifying the safety-related impacts of comprehensive TCD upgradings in several jurisdictions of varying size in Michigan. The upgradings that were included in this program were concerned with signs only.

Michigan has reasonably extensive and reliable machine-accessible accident and related files. Regardless of the jurisdiction, a common accident report form is filed with the Michigan Department of State Police (MSP), coded, and entered in a central system. The report contains a variety of information about the physical description of the accident itself, the involved vehicles, the accident site, the drivers and passengers, as well as other descriptive information. Approximately 12 years of data were used (1972 through 1983).

METHODOLOGY

The basic approach was to select several jurisdictions that had undertaken TCD upgradings and identify their safety-related impacts. The study was based on a "before and after with modified control" experiment design. The use of the modified control consisted of making parallel comparisons for treated and untreated streets within each jurisdiction--the latter being state trunklines (i.e., numbered state routes) ineligible for TCD upgrading project funds. Thus, a control is provided that has the advantage of being internal. Notwithstanding that somewhat different kinds of accidents might occur on the two systems, the impacts of other confounding factors are avoided by using streets that have the same variations in, for example, weather conditions over the analysis period. For each jurisdiction in the analysis, one or more before periods and an after period (all of equal duration) were identified.

The measures of effectiveness (MOEs) were concerned with the distributions and actual numbers of accidents in each jurisdiction. More specifically, the MOEs addressed the following:

1. Distribution of accidents by general type. For example, is there a shift between vehicle-vehicle collisions and vehicle-fixed object collisions? Examination of the before-after statistics for the control streets (state trunklines) would generally establish whether there were shifts among general accident categories. Having established this baseline, the shifts on the treated streets could be examined.
2. Distribution of vehicle-vehicle collisions. For example, is there a shift between multivehicle rear-end and angle collision accident types?

Evidence of the preceding shifts for treated and control streets within jurisdictions is useful information in itself, but there are at least two other aspects of the shift that are important.

3. Total number of accidents that occurred. Given that equal duration before-and-after periods were defined for each jurisdiction, absolute comparisons of the total number of accidents and the number of accidents in various categories can also be made.
4. Severity of accidents. The foregoing information is supplemented by a consideration of accident severity.

The use of trunklines as a "true" control has obvious disadvantages when the treated streets are local; for example, trunklines carry different kinds of traffic, the applicable design standards are different, and vehicle speeds are different. The purpose was to establish a baseline for the more inter-

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esting before-and-after comparisons of the treated streets. The real utility of the control in this experiment, however, is to establish a general trend in accidents between similar time periods. For example, if there was an increasing trend in accidents on state trunklines within a city an increasing trend could be expected in accidents on the local system; if there is more travel on trunklines there should be proportionately more travel on the local system as well; that is, the trends in increasing or decreasing exposure should be similar. Accident types could be expected to be different on the two types of streets--and indeed they were. So, the comparison between the "treated" and "control" groups was a loose one--an attempt to establish a very general trend. As will be demonstrated, the comparison between the before-and-after periods for the trunklines and, separately, for the treated streets was useful.

APPROACH TO DATA ANALYSIS

The approach to data analysis was straightforward and consisted of three basic stages. The first stage was to identify all jurisdictions to be studied and to identify one jurisdiction as a test case to be examined in detail before undertaking the analysis on all jurisdictions. Albion, Michigan, was chosen as the test case based on the average number of accidents occurring there in a year and because 100 percent of the local system had been treated during an upgrading project.

The second stage of analysis was concerned with the examination of the distributions of different characteristics of the accidents occurring in Albion, for example, what age groups were involved. This level of analysis was also directed to identifying any basic differences between accidents on trunklines (control) and those on the local street system (treated). This analysis also provided, in part, the basis for defining different groups of motorists and accidents for which the differential effects of TCDs might be apparent; for example, one group of accidents consisted of those occurring during the day in good weather where the driver of vehicle number 1 (the "at-fault" vehicle) was unimpaired.

The third stage consisted of the actual before-after comparison for treated streets and the comparison between control and treated groups as outlined earlier.

SELECTION OF JURISDICTIONS AND DATA COLLECTION

The selection of which jurisdictions would be analyzed was based on several criteria: the percentage of local streets (i.e., treated as part of the upgrading project); whether the project completion date allowed an adequate after period for analysis; and whether the set of jurisdictions provide for reasonable mixes of population and geographical representation.

Eventually, 13 jurisdictions were chosen ranging in size from Kaleva, which has a population of 450 and less than a dozen accidents per year, through Albion, which has a population of 11,000 and more than 300 accidents per year, to Pontiac, which has a population of 77,000 and 5,000 accidents per year. The percentage of eligible local streets that were treated was 100 for most cities although for Pontiac, the largest city, the percentage was 86.

Although several kinds of data were collected for each jurisdiction, by far the most important was the accident records. Other data, such as project beginning and end dates, jurisdiction population, and so forth, were primarily used in the selection of the sample of jurisdictions to be analyzed. Once the jurisdictions had been identified, all of the accident records for each jurisdiction over the entire time period 1972 to 1983 were obtained from files maintained by the Michigan Department of Transportation (MDOT) and the Michigan Department of State Police (MSP).

Several problems were encountered with the data, including differentiating the effects of the TCD upgrading from general background accident trends across the state, isolating accidents that could realistically be expected to be affected by TCD upgradings, identifying an appropriate control for each jurisdiction, and accounting for general occurrences such as seasonal variation in accidents and user volumes.

DATA ANALYSIS AND RESULTS

The data analysis was done in two fundamental phases: the first phase was concerned with an exploration of the data for Albion, the test city, and the second was concerned with applying the knowledge gained from the Albion investigation to the 12 other cities.

General Description of the Data

The initial examination of Albion data started with a review of the frequency distributions of the several variables available in the accident files. The rationale for this review was to make a basic determination of which "confounding" variables were of concern in order to either (a) eliminate some accidents from the analysis (e.g., accidents occurring in a construction zone) or to (b) provide the basis for data stratification.

The stratification of accidents was achieved by assigning group designations (which were not necessarily mutually exclusive). The purpose of the group designations was not to eliminate accidents, but to stratify them according to certain common characteristics of interest. For example, the reaction of drunk drivers to TCDs may be different from the reaction of nonimpaired drivers, that is, impaired and nonimpaired drivers provide one dimension for group definition.

The final step before beginning the analysis in earnest was the identification of before-and-after periods for each of the test cities. After some experimentation with longer and shorter periods, a basic length of three years was selected.

Although the 3-year periods are equal in absolute length, they do not contain data for the same precise time periods for each city (because of different project time periods). The advantage of the equal before-and-after periods is that both relative and absolute comparisons of the number of accidents occurring can be made.

Basic Analytical Approach

The fundamental analytical approach taken was to compare accident statistics before and after the project was undertaken. The basic statistical technique was chi-square testing to evaluate whether the before-and-after distributions by, for example, general accident type were the same. This was augmented with other testing as appropriate. There was also a before-after comparison for the control (untreated) streets. In general, the analysis proceeded as follows: for a specific variable, a before-after comparison was made for the control streets (state

TABLE 1 Before-After Comparison for MSPAT, All Cities Combined

	STL System[a]				LOC System[b]			
Category	Before	Percentage	After	Percentage	Before	Percentage	After	Percentage
Overturned	10	0.6	10	0.6	26	0.7	14	0.4
Train	(Combined with others)				11	0.3	14	0.4
Parked vehicle	65	3.7	43	2.7	612	16.5	568	16.1
Another vehicle	1,528	87.2	1,412	88.6	2,638	71.0	2,529	71.8
Pedestrian	18	1.0	14	0.9	72	1.9	59	1.7
Fixed object	112	6.4	91	5.7	287	7.7	272	7.7
Bike	17	1.0	18	1.1	69	1.9	65	1.8
Other categories (combined)	3	<1.0	5	<1.0	3	<1.0	2	<1.0
Total	1,753		1,593		3,718		3,523	

Notes: All cities except Pontiac are combined for this analysis. Absolute number of accidents are given and percentage of total in category. Chi-square comparisons of before and after periods: STL: chi-square (6 X 2) = 3.906; p = .563. Chi-square (7 X 2) = 4.619; p = .594. LOC: chi-square (8 X 2) = 4.664; p = .701.

[a]State trunkline (control).
[b]Local (treated).

trunklines, ineligible for treatment) followed by the same comparison for the treated streets. If the data were "well-behaved" and the TCD upgrading was effective, the following results could be expected: for the state trunklines, a net decrease in accidents in all categories would be observed although the before-after distribution would be proportionately the same; for the city streets (the treated group), larger decreases would be accompanied by shifting among the categories. Note that strict comparisons of "treated to control" were not made other than to verify that, indeed, the types of accidents occurring on the two systems were different.

The initial results for Albion were somewhat promising in that overall accident decreases were observed. Subsequently, the analysis was expanded to all cities. In the following discussion the analysis in the second phase is described, beginning with an analysis of the aggregated accidents for all cities except Pontiac.

Much of the discussion refers to two key variables: Michigan State Police accident type (MSPAT) (see Table 1 for categories) and highway accident type (HWYAT), basically types of vehicle-vehicle collisions (see Table 2 for categories).

General Results--All Cities Combined

The first step was to examine all of the cities collectively for the trends in MSPAT and HWYAT. (The exception was Pontiac, which was examined separately due to cost.) The overall analysis provides the broadest possible view of the potential TCD impact. The one shortcoming is that although all time periods have a common time length, the overall before data, for example, will contain data from different "real time" periods. No group stratifications are reported here.

Table 1 gives the overall results for MSPAT. Qualitatively, there appears to be little difference in the percentages of the different types of accidents (shown in parentheses). However, the chi-square statistics indicate that the MSPAT distributions are different on both the local (LOC) or treated system and the state trunklines (STL) or control system, which is counter to the result that would lead to a straightforward interpretation of the TCD upgrading effect. Indeed, based on the relative p-values, the before-after distributions are more similar for the LOC system than for the STL system--the opposite result from one indicating that the TCD upgrading had any effect. It should be noted that the changes in the total number of accidents are somewhat less pronounced than was initially observed for Albion alone.

A note on the use of frequencies rather than rates is appropriate here. The use of some measure of exposure to normalize the comparisons is always desirable. However, taken in the aggregate, as is the case here, an accurate exposure rate for an entire city (plus a breakdown by street types) is difficult at best. Further, the assumption is that ex-

TABLE 2 Before-After Comparison for HWYAT, All Cities Combined

	STL System				LOC System			
Category	Before	Percentage	After	Percentage	Before	Percentage	After	Percentage
Other	277	18.1	243	17.2	435	16.5	417	16.5
Head-on	27	1.8	20	1.4	63	2.4	62	2.5
Sideswipe (same direction)	60	3.9	25	1.8	61	2.3	38	1.5
Sideswipe (opposite direction)	10	0.7	11	0.8	24	0.9	19	0.8
Angle	348	22.8	409	29.0	1,106	41.9	1,147	45.4
Left turn	254	16.6	223	15.8	243	9.2	248	9.8
Right turn	46	3.0	33	2.3	96	3.6	66	2.6
Rear end	442	28.9	413	29.2	456	17.3	392	15.5
Back into	37	2.4	16	1.1	82	3.1	105	4.2
Parking	27	1.8	19	1.3	72	2.7	35	1.4
Total	1,528		1,412		2,638		2,529	

Notes: All cities except Pontiac are combined for this analysis. Absolute number of accidents are given and percentage of total in category. Chi-square comparisons of before and after periods: STL: chi-square (10 X 2) = 32.965; p = .0001; LOC: chi-square (10 X 2) = 30.800; p = .0003.

posure on the trunklines and local system would vary in a similar fashion. Hence, to at least some degree, if exposure increases on the trunklines and there is a corresponding increase in the accident frequency (a constant overall rate), it would be expected that the exposure and the frequency would increase on the local system as well (without TCD upgradings). Therefore, if the upgradings had an impact the increase in frequency should be less (a lower rate). (Although the impact might also be a shift in type or severity of accident as well.) Hence, although the before-after comparison is based on frequency distributions there is a consideration of exposure when the trend is compared with the trends observed on the trunklines.

Furthermore, much of the testing is done with chi-square, which is relatively insensitive to the overall frequency per se. The greater sensitivity is to shifts among, for example, accident categories, which are independent of the absolute number of accidents (overall frequency) or the exposure (rate).

The next variable to be examined was HWYAT, the vehicle-vehicle collision category of MSPAT. It is in this category of accidents that the TCD upgradings could be expected to be most likely to have a positive effect.

Table 2 gives a before-after comparison for all cities (except Pontiac) for HWYAT. A qualitative examination shows that for the STL (control) system, the major shifts in vehicle-vehicle accident types are (a) a decrease in same-direction sideswipes, (b) a relatively sizable increase in angle accidents, and (c) a relatively small decrease in left-turn accidents. This is in the context of an overall decrease in vehicle-vehicle accidents--from 1,528 to 1,412. On the LOC (treated) system, the qualitative review of the percentage changes show a small decrease in same-direction sideswipes (similar to the STL results); a moderate increase in angle accidents (again, similar to STL results); a small increase in left-turn accidents (opposite of and somewhat less than the STL results); a decrease in rear-end accidents (STL had increased very slightly); and an increase in "backing" accidents. This occurred with an overall decrease of from 2,638 to 2,529 vehicle-vehicle accidents. The chi-square comparison of the before-after distributions showed that they were different for both the LOC and STL systems.

The overall results are not particularly enlightening in terms of the effects of the TCD upgrading. There were changes on the LOC system, as well as changes on the STL system. Moreover, the shifts that took place between categories on the two systems were of similar magnitudes, again making it difficult to isolate TCD effects.

The problem just cited was avoided in the next set of analyses, which were concerned with a general examination of selected individual cities.

Results for Selected Cities

Before-After Comparison of General Accident Types (MSPAT)

Table 3 gives the results for Albion, Dundee, East Tawas, Hudsonville, Mackinaw City, Mt. Pleasant, and Pontiac using the MSPAT variable. Several other small cities were not explicitly considered because of the extremely low number of accidents that occurred.

Looking first at Albion, it can be seen that the results displayed in Table 3 indicate that the MSPAT distributions vary for both the LOC and STL systems although the vehicle-vehicle collisions on both decreased between the before-after periods. On a percentage basis, the LOC system experienced a somewhat larger decrease.

Dundee, which is somewhat smaller than Albion, showed somewhat different results. Although there was a difference in the before-after distribution for the STL system, there was less of a difference for the LOC system. The absolute and percentage decreases in vehicle-vehicle accidents reflected this; they were more pronounced for the STL system. Most of the statistics were not calculated for East Tawas, Hudsonville, and Mackinaw City, but the absolute and percentage decreases can be examined. All three towns showed decreases for both systems: for East Tawas the percentage decrease on the LOC system was approximately the same as for the STL system; the percentage decrease was similar for Hudsonville and less (LOC versus STL) for Mackinaw City.

Mt. Pleasant is substantially larger than Albion and, more important, exhibited substantially different results, although the results for the chi-square were similar (distributional differences for both systems); vehicle-vehicle accidents increased.

Pontiac, the largest city in the study, showed results that were similar to the results shown in Albion and Mt. Pleasant as far as the statistical comparison was concerned, but the results were somewhat more favorable in terms of the changes in accidents. Approximately the same number of vehicle-vehicle accidents occurred on the STL system whereas there was a decrease in the number that occurred on the LOC system.

Based on an examination of the MSPAT distributions for the several cities, little consistent evidence exists that the TCD upgradings had either a positive or a negative effect. The results are inconsistent in general.

TABLE 3 Summary of Before-After Comparisons for MSPAT, Individual Cities

	STL				LOC			
	Statistic		Absolute		Statistic		Absolute	
City	Chi-Square	p-value	Change	Percent Change	Chi-Square	p-value	Change	Percent Change
Albion	4.870	.182	256-171	-33	5.232	.156	304-161	-47
Dundee	3.983	.263	109-88	-19	0.970	.809	44-41	-7
East Tawas	–	–	79-58	-27	0.697	0.874	115-83	-28
Hudsonville	–	–	74-67	-9	–	–	122-108	-11
Mackinaw City	–	–	44-27	-39	1.845	.605	69-44	-7
Mt. Pleasant	4.601	.331	876-913	+4	14.685	.012	727-747	+3
Pontiac	17.189	0.16	3,106-3,104	<1	18.862	.009	4,483-4,019	-10

Notes: Absolute change in vehicle-vehicle accidents and percent change; minus sign denotes a decrease. Chi-square is calculated on all possible cells of MSPAT distribution; one cell is a combination. Dash denotes inadequate number of cells with high enough frequency for chi-square calculation.

TABLE 4 Summary of Before-After Comparisons for HWYAT, Individual Cities

City	STL Chi-Square Statistic	STL p-value	LOC Chi-Square Statistic	LOC p-value	Comments
Albion	0.522	.991	8.803	.117	STL: accidents decrease 256-171; LOC: accidents decrease 304-161, higher percent angle, lower percent left turn
Dundee	0.366	.985	0.027	.871	Generally low frequencies; STL: accidents decrease 109-88; LOC: accidents decrease 44-41
East Tawas	0.809	.847	1.283	.257	STL: accidents decrease 79-58, higher percent left turn; LOC: accidents decrease 115-83, higher percent angle, lower percent left turn, lower percent rear end
Hudsonville	6.561	.087	7.341	.290	STL: accidents decrease 74-67, lower percent left turn, higher percent rear end; LOC: accidents decrease 112-108, higher percent "other," lower percent angle
Mackinaw City	2.628	.105	3.625	.459	STL: accidents decrease 44-27, fewer rear end and "other," percents not meaningful; LOC: accidents decrease 69-64, higher percent angle, lower percent left turn, higher percent backed into
Mt. Pleasant	17.982	.021	16.000	.067	STL: accidents increase 876-913, higher percent angle; LOC: accidents increase 727-747, higher percent angle
Pontiac	23.340	.005	38.348	.000	STL: accidents decrease 3106-3104 on a percentage basis, distributions very similar; LOC: accidents decrease 4483-4019 on a percentage basis, distributions very similar

Notes: Chi-squares calculated on distributions of values in HWYAT categories, before and after project. In comments, only shifts on the order of 5 percent or more are noted.

Before-After Comparison of Vehicle-Vehicle Accidents (HWYAT)

HWYAT is a more important variable that allows a more detailed evaluation of vehicle-vehicle accidents. Table 4 gives a summary of the before-after comparisons for the several cities for HWYAT. Examining the chi-square information, it appears that a shift occurred in the before-after distributions for the STL systems for Hudsonville, Mackinaw City, Mt. Pleasant, and Pontiac, whereas there was no shift for Albion, Dundee, and East Tawas. With the exception of Dundee (and to a lesser extent, Mackinaw City), the cities generally show changes in the HWYAT accident distributions for the LOC system.

It is important to note between which categories the shifts in accidents actually occurred; in Albion, for example, there was a higher percentage of angle accidents and a lower percentage of left-turn accidents on the LOC system between the before-and-after systems.

One of the interesting results is that for several of the cities, higher percentages of angle accidents were noted on the LOC system (the exception being Hudsonville where the percentage was lower). In two of the smaller cities and Albion, this was accompanied by a lower percentage of left-turn accidents. This was seen as a potential result of the TCD project.

On the basis of the finding just cited, a review of the shifts in the accident categories was undertaken using a different technique. If the TCD upgradings have a consistent effect (regardless of whether it is favorable or unfavorable) in terms of preventing some types of accidents (and possibly encouraging others), a pattern of categorical shifts should emerge from a review of the different cities. The data in Table 5 represent a summary of HWYAT accident type shifts for the five cities that were studied in some depth. The table is divided into two sections. The first section is a summary for the LOC system in which the table entries are either plus (+), minus (-), or zero (0). A plus indicates that the percentage of accidents in the category increased by 1.5 percent or more between the before-and-after periods; a minus indicates that there was a decrease of 1.5 percent or more; and a zero indicates that the before-after shift was between -1.5 percent and +1.5 percent. Note that these percentages are relative and have no implications for the absolute number of accidents in any category. Angle accidents can then be seen to have increased in the LOC system in four of the five cities analyzed. The only other categories to show such consistent results were "sideswipe-opposite direction" and "right turns," which experienced very little proportional change--all entries were zeroes. Taken alone, this finding would indicate that the TCD effect was a proportionate increase in angle accidents.

TABLE 5 Summary of Proportional Shifts in HWYAT Categories

HWYAT Category	Pontiac	Albion	East Tawas	Hudsonville	Mt. Pleasant
LOC system (criterion = change > 1.5 percent)					
Other	0	0	-	+	-
Head-on	0	0	0	-	0
Sideswipe—same direction	0	-	-	+	0
Sideswipe—opposite direction	0	0	0	0	0
Angle	+	+	+	-	+
Left turn	0	-	-	+	0
Right turn	0	0	0	0	0
Rear end	+	-	-	+	-
Back into	0	+	+	0	0
Parking	0	-	+	0	0
STL system (criterion = change > 1.5 percent)					
Other	0	+	-	0	-
Head-on	0	0	0	0	0
Sideswipe—same direction	-	-	-	-	-
Sideswipe—opposite direction	0	0	0	+	0
Angle	0	+	+	+	+
Left turn	0	0	+	-	0
Right turn	0	0	0	-	0
Rear end	0	0	-	+	0
Back into	0	-	0	0	0
Parking	0	0	-	0	0

Note: Given stated criterion: if percent increases +, if percent decreases -, and if no change 0.

The second part of the table is the same type of comparison for the STL system. Again, it is observed

that angle accidents increased in four of the five cities (although the city without the increase is different). Other consistent trends for the STL system include a decrease in the sideswipe-same direction category for all cities and little change in head-ons, sideswipe-opposite direction, right turns, backing, and parking.

The increase in the angle category for the STL system as well as for the LOC system indicates that the change was not attributable to the TCD upgrading (or anything else that is characteristic of the LOC system).

Review of the table reveals no consistent trends on one system that are not present on the other. Further, in most instances the results vary from city to city for any given accident category. In short, the systems are consistent only in their inconsistency of shifts among accident categories.

Before-After Comparison of Accident Severity

The last phase of the analysis was the examination of the severity of accidents occurring on the systems in the various cities. Regardless of whether the shifts in accident types could be tracked and attributed to the TCD upgradings, changes in the severity of accidents might be attributable to them. There are confounding factors that must be considered as well; for example, motorists becoming more safety conscious and vehicles becoming safer.

A comment about the coding of accidents by severity is appropriate. An accident can result in a serious injury, property damage, or a fatality, and different numbers of each. However, each accident was assigned a category according to its most serious outcome; for example, if there were three incapacitating injuries and a fatal accident, the accident was recoded as a fatal accident.

Given the preceding discussion, comparisons of all vehicle-vehicle accidents in all cities other than Pontiac were made. The overall indication was that, in general, the before-after severity distributions tend to be different on the STL system--most explicitly when all vehicle-vehicle collisions are considered and somewhat less so when angle or left-turn accidents are considered. They also tend to differ for the LOC system, although they are reasonably similar when only left-turn accidents are considered.

The same type of comparison was made on an individual basis for three cities: Albion, Mt. Pleasant, and Pontiac (Table 6). In each instance all vehicle-vehicle accidents were examined followed by angle and left-turn accidents. For the Albion STL system, it is observed that within the context of an overall decrease in the number of vehicle-vehicle accidents, there is a shift to somewhat more severe accidents [PDOs decrease proportionately (4 percent) while B and C accidents increase] (see table notes for accident severity description). On the LOC system there is a more pronounced shift to more severe accidents in an overall context of a decreasing number of accidents. The trend is similar, but somewhat more pronounced when only the angle accidents are considered. For left-turn accidents there is a decrease in number on both systems, with the STL accidents becoming somewhat more serious and the LOC accidents becoming less serious. It should be noted that sample sizes are quite small for the angle and left-turn separations, and the percentages can vary greatly with only a few accidents.

The results for Mt. Pleasant are somewhat different. For all vehicle-vehicle accidents, the numbers of accidents on both the STL and LOC systems remained nearly constant between the before-and-after periods, whereas on the STL system they became somewhat more serious and on the LOC system they became somewhat less serious (although the latter shift was between the two least serious categories).

The Mt. Pleasant angle accidents increased on both systems, becoming less serious on the STL system and more serious on the LOC system (again the major shift was between PDO and C categories in both instances). STL left-turn accidents decreased whereas LOC left-turn accidents remained the same. However, there was a positive shift on the LOC system in terms of severity and a negative shift on the STL system. Again most shifting was between the less severe categories and sample sizes were small.

Review of the situation in Pontiac is somewhat more definitive in the sense that all of the sample sizes are greater. For total vehicle-vehicle accidents, there was a shift toward more severe accidents on both systems in the context of an overall decrease in accidents on the LOC system. For angle and left-turn accidents, on the LOC system the shift is not great but clearly toward more severe accidents within an overall decrease in the numbers of both types of accidents. The shifts on the STL system were toward more severe accidents in the angle category and less severe accidents in the left-turn category, with little change in the numbers of accidents in both categories. For Pontiac, the chi-square statistic and p-value indicate that the shifts for the LOC system are (a) highly significant for all vehicle-vehicle accidents, (b) moderately significant for angle accidents, and (c) insignificant for left-turn accidents.

SUMMARY AND DISCUSSION

The analysis of several cities in Michigan for the efficacy of jurisdiction-wide traffic control device upgradings yielded inconsistent results. In summary, there is no substantive evidence that TCD upgradings have a consistent, measurable (positive or negative) impact on safety on a jurisdiction-wide basis as measured by a variety of safety (accident) measures.

A summary of the results for each of the several parts of the analysis undertaken follows:

1. Trends in background and descriptive statistics. Accident distribution (by type of crash) were somewhat different for the STL and LOC systems; the biggest difference was in the proportion of the vehicle-vehicle crashes in the angle category. In general, background information was similar for LOC and STL systems, for example, demographic characteristics of the drivers and weather conditions.

2. General trends in accident frequencies. Some city-to-city variation existed in the trends in the numbers of accidents occurring on the LOC and STL systems. For example, in Albion there was a general decreasing trend on both systems, whereas in Pontiac the trend was increasing and then decreasing.

3. Trends in changes in general and specific accident types (MSPAT and HWYAT). Changes occurred on both systems; that is, between the before-and-after periods on both systems changes occurred in the MSPAT and HWYAT distributions. This result was expected on the LOC system but unexpected on the STL system. This points to the general variability of the accident statistics over time, which makes isolation of the effects of specific changes on either system (i.e., the TCD upgrading) problematic.

4. Absolute and proportional changes in the number and type of vehicle-vehicle accidents. Neither absolute nor proportional changes in the overall

TABLE 6 Summary of Severity of Vehicle-Vehicle Accidents, Selected Cities

Accident Severity	STL Before Absolute	STL Before Percentage	STL After Absolute	STL After Percentage	LOC Before Absolute	LOC Before Percentage	LOC After Absolute	LOC After Percentage
Albion: All Vehicle-Vehicle Accidents[a]								
PDO	215	84	136	80	258	85	126	78
C	27	11	22	13	23	8	19	12
B	11	4	11	6	16	5	11	7
A	3	1	2	1	7	2	5	3
Albion: Angle Accidents[b]								
PDO	40	80	27	71	95	81	55	71
C	8	16	8	21	10	8	11	14
B	2	4	3	8	9	8	7	9
A	0	–	0	–	4	3	4	5
Albion: Left Turn Accidents[c]								
PDO	26	79	15	71	19	70	5	83
C	4	12	2	10	2	7	0	–
B	2	6	3	14	5	19	1	17
A	1	3	1	5	1	4	0	–
Mt. Pleasant: All Vehicle-Vehicle Accidents[d]								
PDO	219	75	211	73	193	76	200	81
C	53	18	44	15	39	15	28	11
B	14	5	25	9	14	6	16	6
A	6	2	9	3	7	3	4	2
Mt. Pleasant: Angle Accidents[e]								
PDO	35	61	60	67	80	67	92	54
C	12	21	12	13	26	22	19	15
B	7	12	12	13	9	8	11	9
A	3	5	5	6	4	3	3	2
Mt. Pleasant: Left Turn Accidents[f]								
PDO	50	81	41	73	29	78	31	84
C	9	15	12	21	5	14	3	8
B	3	5	3	5	1	3	3	8
A	0	–	0	–	2	5	0	–
Pontiac: All Vehicle-Vehicle Accidents[g]								
PDO	2,230	72	2,080	67	3,201	71	2,767	69
C	577	19	633	20	802	18	762	19
B	202	7	265	9	314	7	332	8
A	92	3	123	4	155	3	153	4
F	5	–	3	–	11	–	5	–
Pontiac: Angle Accidents[h]								
PDO	433	62	379	53	731	68	669	65
C	134	19	179	25	201	19	208	20
B	87	13	96	14	105	10	102	10
A	38	5	52	7	40	4	56	5
F	2	–	3	–	5	–	2	–
Pontiac: Left Turn Accidents[i]								
PDO	347	65	376	70	330	61	284	59
C	115	22	88	16	115	21	110	23
B	47	9	51	10	64	12	51	11
A	23	4	21	4	33	6	32	7
F	1	–	0	–	1	–	2	–

Notes: PDO = property damage only, C = possible injury, B = non-incapacitating injury, A = incapacitating injury, and F = fatal. Chi-square calculations did not include the fatal cell.

[a] STL: chi-square = 1.635; p = .441. LOC: chi-square = 3.356; p = .340.
[b] Statistics not calculated, small cell frequencies.
[c] Statistics not calculated, small cell frequencies.
[d] STL: chi-square = 4.671; p = .198. LOC: chi-square = 2.056; p = .357.
[e] STL: chi-square = 1.441; p = .486. LOC: chi-square = 1.921; p = .383.
[f] Statistics not calculated, small cell frequencies.
[g] STL: chi-square = 20.781; p = .000. LOC: chi-square = 8.404; p = .038.
[h] STL: chi-square = 16.569; p = .001. LOC: chi-square = 4.742; p = .192.
[i] STL: chi-square = 4.994; p = .172. LOC: chi-square = 0.900; p = .825.

number of accidents, vehicle-vehicle accidents, or specific categories of vehicle-vehicle accidents yielded any consistent results for either the STL or LOC systems. Indeed, one qualitative comparison of trends in the specific accident categories showed that the trends were the same on both systems.

5. Trends in accident severity. Overall the trends in severity showed that minimal changes occurred among the different accident types, and there was some contradictory information, for example, a trend toward more severity for one type of accident and less severity for another for the LOC system with some STL trends being the same and some opposed, and in addition to city-to-city differences.

A review of this information indicates that the most striking result is the overall lack of consistency in the results whenever a detailed analysis was attempted; this is especially important in view of a general similarity in broad background characteristics.

Does the lack of results (either positive or negative) mean that TCD upgradings should not be undertaken? The answer is at least twofold. First, from the point of view of a jurisdiction's liability for damage suits, and so forth, TCD upgradings are quite important. The relative success or failure here to identify and quantify systemwide changes does not necessarily mitigate against the efficacy of improved TCDs at specific sites.

Otherwise, the failure to arrive at definitive quantitative results is seen as being due to general variability in accidents per se and a host of confounding variables for which no control was possible. Looking at a jurisdiction as an analysis unit has inherent drawbacks, although the TCD upgrading is indeed jurisdiction-wide, many intersections, for example, would probably experience no change in either the placement or the type of TCD present. Additional intersection-related changes might be concerned with relatively minor placement modifications. These are modest changes unlikely to be picked up in a general analysis. What is left then is relatively few changes in a jurisdiction that might be termed changes of substance. The changes in accident frequency at these relatively few intersections are then lost within (confounded by) the overall lack of change at other sites. An additional factor is that many of the TCD changes may be concerned with noncritical signs such as no parking, and so forth.

In summary, it would appear that safety analyses would be better directed toward the consideration of key problem sites in a jurisdiction. Procedures for this type (level) of analysis are well-defined and accepted within traffic engineering. Although the idea of being able to make a sweeping generalization about the efficacy of TCD upgradings for different jurisdictions is appealing, and would indeed be helpful from an agency viewpoint (in terms of resource allocation, for example), the overall variability of the data appears to overwhelm detectable changes at the jurisdiction level.

ACKNOWLEDGMENT

The work described in this paper was funded by a grant from the Office of Highway Safety Planning (OHSP) of the Michigan Department of State Police. The comments of the reviewers on the original draft of this paper are also acknowledged and appreciated.

Publication of this paper sponsored by Committee on Methodology for Evaluating Highway Improvements.

Modeling the Relationship of Accidents to Miles Traveled

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ABSTRACT

Consideration of highway safety studies in a time-space domain is used to introduce the concept that different study designs result in different underlying probability distributions describing accident occurrence. Poisson regression is proposed as a superior alternative to conventional linear regression for many safety studies because it requires smaller sample sizes and has other desirable statistical properties. Models are estimated using accident, travel mileage, and environmental data from the Indiana Toll Road. A pooled model including all accidents revealed that accident occurrence increases with automobile vehicle miles of travel (VMT), truck VMT, and hours of snowfall. Segmentation of the data into subsets that describe different types of collisions revealed that automobile accidents are much more sensitive to environmental conditions than are truck accidents. Use of the segmentation technique allowed a much clearer understanding of the effects of travel mileage on accident occurrence than could have been obtained from the pooled data alone.

It is generally recognized that the occurrence of accidents results from the complex interaction of characteristics of the driver, vehicle, roadway, and environment. The number of accidents (accident frequency) is also clearly related to the amount of travel that occurs. Quantity of travel may be measured in any of several ways including hourly volume, average daily traffic, or vehicle miles of travel (VMT), among others. These measures of quantity of travel can be used to describe traffic conditions that exist during exposure to accident risk. A more precise definition of exposure is, "...the amount or opportunity for accidents which the driver or traffic system experiences" (1). This broader interpretation of exposure has led some researchers to explore the effects on accident occurrence of environmental conditions during which the driving occurred (2).

Previous studies relating accident occurrence to level of traffic have used a variety of measures of travel quantity. Belmont (3) found the accident rate (accident per million VMT) for two-lane sections almost linear with hourly traffic flow during daylight. For four-lane divided sections, Leutzbach (4) and Gwynn (5) found that a U-shaped relation exists between accident rate and hourly traffic flow, where the minimum values of the accident rates happened at approximately 600 to 1300 vehicles/hr per two lanes. In another study the accident rate increased rapidly when the traffic volume was below 550 vehicles/hr per two lanes, but showed little variation beyond this flow value (6).

Smeed (7) considered the problem on a much broader scale, studying national yearly accident rates. He found that the total accident rate showed little variation with annual traffic volumes. When he separately considered single-vehicle and mutiple-vehicle crashes he found that the single-vehicle accident rate decreased with annual traffic while the multiple-vehicle rate increased.

Ceder and Livneh (8) used both time-sequence analysis and cross-sectional analysis to study single- and multivehicle accidents for a series of eight roadway segments over an 8-year period. Ceder (9) expanded on this work by considering accident rates in conjunction with free-flow and congested-flow conditions. He found that the total accident rate versus hourly flow curve followed a U-shaped configuration for the free flow, which is the result of a convex downward and a convex upward curve for single- and multivehicle accidents, respectively. For congested flow, the accident rate for multivehicle accidents increases rapidly with hourly traffic flow.

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A TIME-SPACE PERSPECTIVE ON ACCIDENT OCCURRENCE

These studies can be considered as representing different areas in a space-time plane (see Figure 1). Smeed's research represents a very large area because he used national statistics on an annual basis (7). The use of horizontal lines indicates that the analysis was cross-sectional, comparing the accident experiences of different countries (a spatial analysis). Gwynn considered only one route but conducted his comparisons on an hourly basis in the time domain, so there are vertical lines within the domain defining his study (5). These areas are not drawn to scale but are used to illustrate how these two studies would be represented in the space-time plane.

Different types of accident studies result in different shapes in the space-time plane. Each of these shapes can be linked to particular probability distributions that describe the probability of accident occurrence. For example, with a long time period and large study section it may be reasonable to approximate the occurrence of accidents by a normal distribution. If the time and space domains are large, there is a very small likelihood of zero accidents in a time interval. The normal distribution will then have a large mean and a comparatively small variance that will make zero or negative values unlikely. This appears to be a reasonable distribution to use in this context.

The spatial and temporal aggregation required by these large time-space areas makes it difficult if not impossible to isolate the influence of driver and environmental characteristics. The normal distribution that is assumed in linear regression and analysis of variance (ANOVA) may be appropriate for

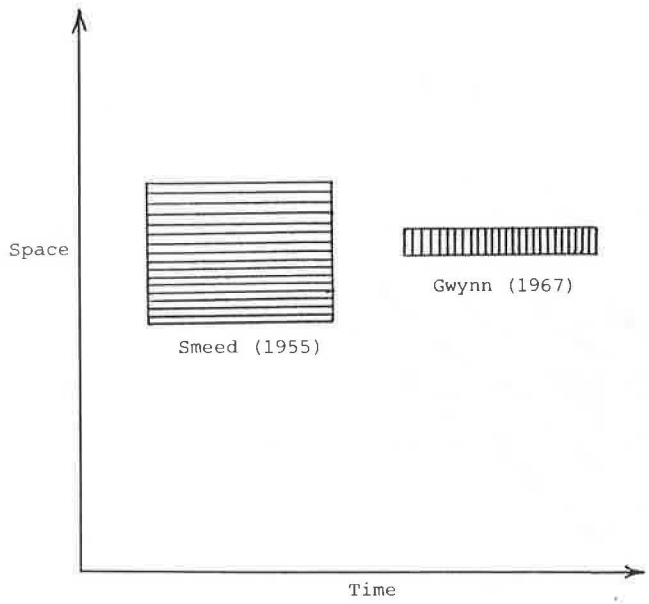

FIGURE 1 Space-time plane for highway accident study.

some specific comparisons (e.g., accident frequency on particular roadway segment) but controls for driver and vehicle factors must be carefully developed particularly for cross-sectional studies. Although vehicle and driver characteristics may be known for accidents, these attributes may be very difficult to obtain for nonaccidents.

At the other extreme of the time scale are analyses that seek to explore accident occurrence as a series of "success" or "failure" trials in which success is the safe completion of a trip and failure is an accident. Over a period of time accidents can be considered as governed by this Bernoulli process (10). At the level of individual trip success and failure trials, much detail can be retained about the driver and the vehicle. Although roadway and environment data are available at the accident scene, it is more difficult to obtain these data for the other portion of the trip. Data collection would have to be carefully managed as very large sample sizes may result.

One extension of this representation as a Bernoulli process is the use of survival theory to predict the probability of having an accident at a particular time, given that the driver has survived until that time (11). Survival theory also provides for inclusion of successful trips that are completed without an accident as exposure data. Although more research needs to be conducted to improve variable specifications, this model holds great promise in allowing improved representation of driver, vehicle, roadway, and environment in a consistent framework. A major potential disadvantage of the Bernoulli approach is that data collection and assembly may be expensive, particularly for exposure data.

Intermediate between these two representations is the Poisson distribution, which allows for discrete outcomes that are strictly nonnegative. If the time and space domains are carefully defined, and the assumption about independence of events is satisfied, the Poisson distribution may be reasonable.

OBJECTIVES

The discussion of time-space domains for accident analysis introduces the concept that different study designs can lead to different underlying probability distributions describing accident occurrence. The objective of this paper is to further explore this issue. If the true causes (and potential countermeasures) of accidents are to be identified, then statistical procedures must be used that accurately describe accident occurrence. In the remainder of the paper a model of accident occurrence that offers important advantages over conventional linear regression methods is developed and tested.

In the next section of the paper particular properties of accident occurrence that can complicate analysis are discussed. Poisson regression is suggested as a means to overcome many of these complications. The analytical structure for Poisson regression is described in a later section. Results and discussion of a model application are presented followed by conclusions.

DISCRETENESS, NONNEGATIVITY, AND HOMOSCEDASTICITY

Regression analysis has been widely applied in studies that seek to relate accident occurrence to traffic volume (8-9). Three particular properties of accident occurrence argue for care in the application of linear regression to road safety studies. These properties are the existence of relationships between the mean and variance of accident frequency, nonnegativity of the dependent variable (either accident frequency or accident rate), and occurrence of nonnormal error term distributions. Each of these issues is discussed in detail in the paragraphs that follow.

It is common to think of accident occurrence as a process that follows a Poisson or possibly a Bernoulli process (10). Both of these processes imply that the variance of accident frequency is functionally related to the mean (e.g., in Poisson processes the variance is equal to the mean). If an attempt is made to regress accident frequency by vehicle miles of travel for an accident process that is actually Poisson, the results obtained might be similar to Figure 2. Because more accidents are generally likely to occur at higher traffic volumes (due to increased conflicts), a linear positive relationship would be expected to fit through such data. It can readily be seen from Figure 2, however, that as VMT increases so does the variance of accident frequency (the dependent variable). This condition clearly violates the homoscedasticity assumption (error term has equal variance for the entire range of the predictor variables) of linear regression (12).

Violation of the assumption of equal variance of the error terms will not affect the estimated parameters; it does affect the confidence intervals of the estimators, invalidating any hypothesis tests concerning the significance of the parameters. If the objective of a study is to determine the influence that particular predictor variables have on accident occurrence, the failure to properly test for parameter significance is a serious flaw.

The use of accident rates (accidents and quantity of traffic) in the regression analyses may appear to overcome the problems with functionally related means and variances. Figure 3 shows a comparable regression line for accident rate regressed against VMT. Despite the transformation to a continuous dependent variable, one can still sketch in contours of equal accident frequency per unit time (whatever the time dimension of exposure). This estimation still results in a violation of the homoscedasticity assumption of linear regression.

Assume that accident frequency for a study section is governed by a Poisson process (mean = variance = λ) and that the frequency will increase with in-

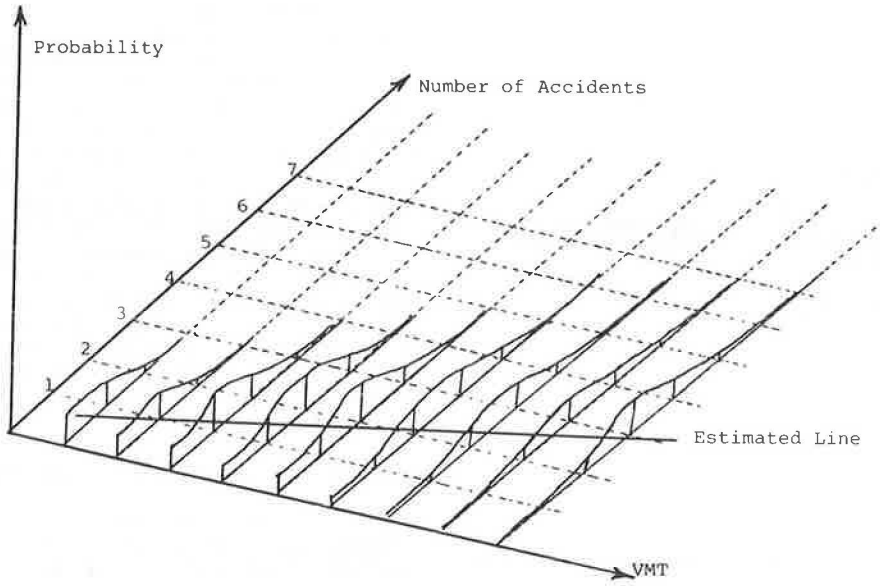

FIGURE 2 Regressing accident frequency and VMT.

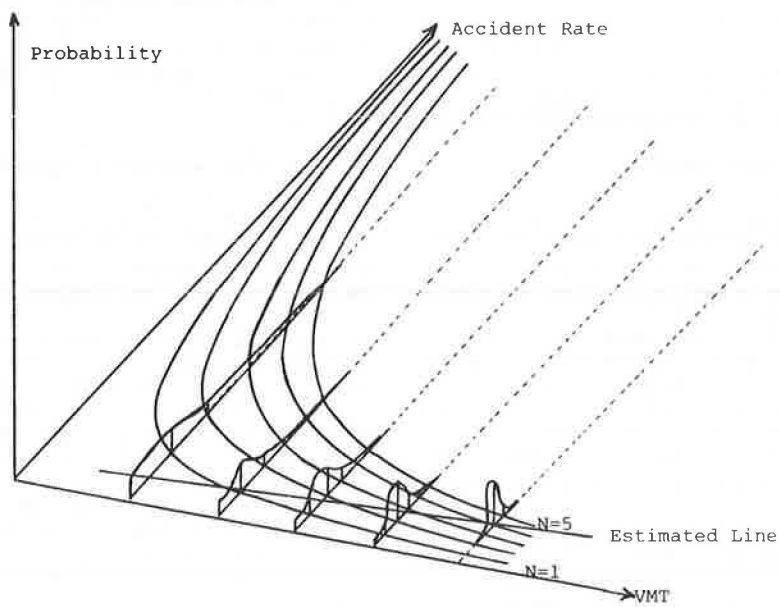

FIGURE 3 Regressing accident rate and VMT.

creasing VMT. The regression relationship represented in Figure 2 can be written as

$$y_t = ax_t + u_t \tag{1}$$

where

y_t = accident frequency in period of time, t;
x_t = VMT during time t;
u_t = error term, assumed to be distributed normal (o, σ_u^2); and
a = regression parameter.

If accident occurrence is a Poisson process, then the variance of u_t will be related to the VMT through the Poisson parameter λ, which will increase with increasing VMT. Therefore

$$Var(u_t) = x_t\sigma_u^2 \tag{2}$$

This problem can be corrected by using a variance stabilizing transformation (13); specifically, dividing Equation 1 by $(x_t)^{1/2}$. This yields

$$y_t/(x_t)^{1/2} = a(x_t)^{1/2} + u_t^* \tag{3}$$

$$u_t^* = u_t/(x_t)^{1/2} \tag{4}$$

then

$$Var(u_t^*) = E[(u_t^*)^2] = E(u_t^2/x_t) = (1/x_t)E(u_t^2) = (1/x_t)(x_t)\sigma_u^2 = \sigma_u^2 \tag{5}$$

Therefore error terms of equal variance will result for the case when Equation 1 is divided by $(x_t)^{1/2}$. If y_t/x_t (accident rate) is regressed with x_t, the result will be narrower variance at higher levels of VMT. This is shown in Figure 3 by a smaller variance as VMT increases.

These variance-stabilizing transformations are useful, but when the response variable has been re-expressed, the predicted values are in the transformed scale. It is often necessary to convert the predicted values back to the original units. Unfortunately, applying the inverse transformation directly to the predicted values gives an estimate of the median of the distribution of the response instead of the mean (14).

The second problem, nonnegativity of accident occurrence, also imposes restrictions on the application of linear regression. The restriction is apparent in Figures 2 and 3 for both discrete and continuous dependent variables. If either regression is conducted for a set of data with high accident frequency or accident rate, respectively, then the prediction of "negative" accidents is much less likely. The requirement for either high frequency or high rate carries obvious implications for study design (particularly in the time-space plane of Figure 1). Restricting cases to those with high accident frequency may also increase study costs by requiring a larger sample of data.

A number of analytic methods are available to deal with these estimation problems. The method of least squares subject to a priori constraints can overcome the problem of negative-value prediction, but it will lead to biased estimates of model coefficients (15). Nonlinear models are also used to avoid the negative-value prediction problem, and a least squares estimation procedure based on the linearization of the nonlinear form (such as logarithm) can be applied to estimate the parameters. However, the logarithm of zero is not defined, and a zero accident observation therefore cannot be included in the investigation. One alternative for dealing with this problem is to omit the zero observations, but this is undesirable because the traffic situations in which no accidents occur are obviously important. The other alternative is to add a small number (e.g., 0.04) to all observations of the dependent variable (16). Such pretreatment of observations can greatly affect the estimation and is therefore undesirable.

A third problem occurs when the error terms are not normally distributed due to the characteristics of nonnegativity and small value of discrete dependent variable (see Figure 2). Under these conditions the correct confidence intervals will not be obtained for estimated parameters, and tests of parameter significance are again invalid.

The Poisson regression model, which assumes that the occurrence of the dependent variables follows the Poisson distribution, can effectively overcome most of the problems caused by discrete and nonnegative values of observations in normal linear regression analysis. Poisson regression techniques were used for the analyses of accidents in The Netherlands (17). Each accident was assigned to a category defined by different ranges of independent variables. For example, Hamerslag specified 4 classes for motor vehicle volume and 4 classes for motor bicycle volume, obtaining 16 categories of accidents through the combination of those two independent variables. The expected annual accident frequency for each category was assumed to be a function of its respective classes of independent variables. The number of accidents that occurred within a given time interval for each category was assumed to be Poisson-distributed with the mean equal to the predicted accident frequency for that category.

Aggregating information in the specification of the independent variables by range hinders the ability to explore the risk factors for traffic operation. Furthermore, if the number of independent variables is large, a huge sample size of data is required in order to obtain statistical power. The authors' research model is an attempt to apply the Poisson model to a more disaggregate analysis of highway accidents, to better identify some of the factors contributing to highway operating risk.

POISSON REGRESSION MODEL

The Poisson distribution was first considered in the context of regression analysis about two decades ago (18). It assumes that the dependent variables in a regression analysis are counts that follow the Poisson distribution, and that the observations are independent with the expectation as defined by the following equation:

$$E(y_{ij}) = f(\underline{x}_i,\underline{\beta}) \qquad i = 1,2.....,n. \qquad j = 1,2.....,m_i. \tag{6}$$

where $\underline{x}_i = (x_{i1},x_{i2},.....,x_{ik})$ is the ith set of values of the k independent variables, m_i is the number of replications of the ith experimental condition, $\beta = (\beta_1,\beta_2,.....,\beta_p)'$ is a p-dimensional vector of unknown parameters, and (y_{ij}) is a particular realization of the experiment (15). It is further assumed that some general form of the model is known and $f(\underline{x},\underline{\beta})$ is a differentiable function of $\underline{\beta}$. Then n values of the independent variables are selected by the experimenters or specified by the situation. The number of n is supposed to be sufficiently greater than p to ensure estimability of the parameters. Three different methods are available to estimate the parameters of the Poisson regression model. They are maximum likelihood, weighted least squares, and minimum chi-square estimation. Maximum likelihood estimation has been widely accepted in past applications because of its convenience.

The occurrence of highway accidents can be reasonably described by the nonstationary Poisson process if the study system (or area) is adequately selected. According to the basic assumptions of the Poisson process, it is assumed that the number of accidents occurring within each observed time interval is independent, with the expectation defined as in Equation 6. This expectation of the number of accidents in each time interval is a function of traffic volume, road and weather conditions, and so forth. Hence the expected values of accidents are different from time interval to time interval, and this is the so-called nonstationary Poisson process. The model is set as

$$\lambda_i = f(\underline{\beta},x_i) \tag{7}$$

where

λ_i = expected value of accident frequency for ith time interval,
$\underline{\beta}$ = the vector of parameters to be estimated, and
$\underline{X}_i$ = the vector of independent variables for ith time interval.

The probability of k accidents occurring in t intervals is represented as

$$P_i(k) = (e^{-\lambda_i t})(\lambda_i t)^k/k! \tag{8}$$

However, because only the accidents occurring in each time interval (t=1) are considered, Equation 8 becomes

$$P_i(k) = e^{-\lambda_i}(\lambda_i)^k/k! \tag{9}$$

Then a set value of $\hat{\underline{\beta}}$ that maximizes the following likelihood value (L) is sought:

$$L(\beta) = \prod_{i=1}^{n} \prod_{k=0}^{\infty} P_i(k)^{D_{ik}} \quad (10)$$

where D_{ik} is the dummy variable for the number of accidents that occur in the ith time interval:

D_{ik} = 1, if k accidents occurred in ith time interval,
D_{ik} = 0, otherwise.

For convenience, a logarithm transformation of Equation 10 is taken and called the log-likelihood value $[LL(\hat{\underline{\beta}})]$:

$$LL(\hat{\beta}) = \sum_{i=1}^{n} \sum_{k=0}^{\infty} D_{ik} \log [P_i(k)] \quad (11)$$

LL(c) is also defined as the log-likelihood value of the model in which only the constant term is used. The value of $2[LL(\hat{\underline{\beta}})-LL(c)]$ is distributed as χ^2-distribution with p-1 degrees of freedom. It is a statistic for testing the significance of all explanatory variables included in the model. ρ^2, defined as $1-[LL(\hat{\underline{\beta}})/LL(c)]$, is an informal goodness-of-fit measure and is analogous to R^2 used in regression.

DATA COLLECTION AND MODEL FORMULATION

Data Collection

The data used in this study were collected on the Indiana Toll Road in 1978. The Indiana Toll Road is an east-west road 157 mi long. It transverses mostly open flat country. There are no steep grades or sharp horizontal curves. Daily VMT data were derived from the toll collection system. All drivers entering the Indiana Toll Road receive a ticket that records the designation of the particular interchanges of entry and the date on which the vehicle entered the toll road. The toll is collected when the vehicle leaves the tollway, and the vehicles are then classified according to the toll schedules' list of vehicle classes. Because a vehicle can only enter and leave the tollway at a limited number of interchanges, these facilities are closed systems where VMT can be easily calculated and recorded by mechanized card-reading procedures. Thus, the precise daily automobile and truck VMTs are available.

Data describing all toll road main line accidents were obtained from records of the Indiana State Police Toll Road Headquarters. Accidents occurring at toll booths, access roads, service areas, and ramps were excluded because they are likely to be influenced by geometric design and other operational characteristics that are site-specific. By studying only main line accidents, the authors hoped to obtain a clearer relationship between accidents and exposure for a well-designed, four-lane freeway facility. After screening non-main line accidents, the data set included more than 700 accidents and 1,023 vehicle involvements (19).

The weather data were obtained from the National Oceanic and Atmospheric Administration's Environmental Data and Information Service at the National Climatic Center in Asheville, North Carolina. There are six stations recording hourly precipitation and amount of daily snowfall along the Indiana Toll Road. The toll road is within 5 mi of all the stations, which are roughly evenly spaced. The hours of snow and hours of rain were derived by Delleur (19); an average of the weather conditions of those six survey stations is used as the hours of snow and hours of rain for the toll road overall.

Independent Variables

A regression model does not imply a cause-and-effect relationship between the variables. To establish causality, the relationship between the regressors and the response must have a basis outside the sample data; for example, the relationship may be suggested by theoretical considerations. Regression analysis can aid in confirming a cause-effect relationship, but it cannot be the sole basis.

Traffic volume and traffic composition affect traffic speed, variation of vehicle traveling speeds, and drivers' psychological condition. For example, automobile drivers may feel uncomfortable when they join a traffic stream that has a high truck volume. Hence, the increase of traffic volume will not only increase the number of accidents because of more exposure, but it will also increase traffic conflict and friction. VMT is used to represent the traffic volume in the study system. Daily traveling miles of automobiles, small trucks, and large trucks are separated not only for exposure considerations, but also for distinguishing their effects on different accident patterns. Small trucks include six-tire vehicles with two axles, commercial vehicles with three axles, and two-axle tractors with one-axle trailer.

The second factor tested in this model is the weather condition, which influences the friction of roadway pavement and driver's sight distance. These effects will affect the safety of high-speed operation on the highway. The hours of snowing and raining in the study system are considered to reflect the effect of daily weather condition on daily accident occurrence. An average of the data collected from the survey stations along the toll road is used to represent the daily weather condition.

The last regressor considered in this study is the effect of different driving populations on accident occurrence. Travel is derived by people's activities, which are generally controlled during weekdays by work trips and related travel. These drivers are likely to be frequent travelers of the toll road, familiar with its relatively high mix of automobiles and trucks. Weekend travelers may be less frequent users who are less able to cope with traffic conditions on the road. In order to help capture the influence, if any, of different driving populations, a dummy variable--labeled WEND--is included in the model.

Functional Form of the Model

There is no reason to prefer one functional form over any other for Equation 7. A linear additive form was initially tested but failed to result in valid model estimates. It appeared that some sets of possible parameter estimates caused $\hat{\lambda}$ to be negative, violating the assumed conditions of the Poisson distribution. A multiplicative specification was also tested and yielded valid parameter estimates. The multiplicative form of Equation 7 is as follows:

$$\lambda = \beta_0(VMTa)^{\beta_1}(VMTlt)^{\beta_2}(VMTst)^{\beta_3}(1+HSNOW)^{\beta_4}(1+HRAIN)^{\beta_5}(1+WEND)^{\beta_6} \quad (12)$$

where

λ = expected accident frequency per day;
VMTa = daily VMT of automobiles (10^6 vehicle miles);
VMTlt = daily VMT of large trucks (10^6 vehicle miles);
VMTst = daily VMT of small trucks (10^6 vehicle miles);
HSNOW = hours of snow in the study system;
HRAIN = hours of rain in the study system; and
WEND = dummy variable for weekend; WEND=1 for weekend, and WEND=0 otherwise.

One has been added to each of the last three predictor variables to prevent zero values for estimated λ's, which would result in the logarithm of zero (which is undefined) occurring in Equation 6. Notice that this formulation does allow for zero values of the dependant variable so that it is not subject to the criticisms described in the section on Discreteness, Nonnegativity, and Homoscedasticity.

MODEL RESULTS AND INTERPRETATION

Preliminary empirical results indicated that the parameters of the weekend and small truck VMT were not significant. The dummy variable, WEND, was removed from the original model in order to improve the model structure. A χ^2-test, revealed that deleting weekend caused no significant difference in the explanatory ability of the restricted model compared to the original model. The small truck VMT also had no significant effect on accident occurrence. The small truck VMT was small relative to automobile VMT and large truck VMT, and it also was positively correlated with the large truck VMT. In order to deal with the issue of collinearity between independent variables, the small truck and large truck VMTs were combined into one variable, truck VMT.

Therefore, only four independent variables were included in the model: (a) automobile VMT, (b) truck VMT, (c) hours of snow, and (d) hours of rain. The empirical results of the final model are given in Table 1, along with the log likelihood and goodness-of-fit measures discussed in the section titled Poisson Regression Model.

In addition to the pooled model, which includes all accidents, several additional models are estimated for different types of collisions. Separate models are estimated for single-vehicle crashes (both automobile and truck) and for two-vehicle crashes differentiated as automobile-automobile, truck-truck, and truck-automobile. In addition, separate models were estimated for single vehicle and multiple vehicle crashes. Crashes with three or more vehicles were extremely rare, so more than 95 percent of the accidents were included in these categories. The different models were separately estimated to determine if VMT and weather conditions may have had different effects on different types of vehicle crashes.

Pooled Model

The estimates for the pooled model indicate that all parameters are significant except for hours of rainfall. The χ^2-test for the entire model strongly rejects the null hypothesis that the full model has explanatory power equal to that of the model with the constant term only. Consistent with previous results (2), hours of snowfall is strongly positively associated with accident occurrence as are automobile and truck VMT.

Although the pseudo goodness-of-fit measure is small (ρ^2 = .06), this is an indication of the additional variation in accident frequency explained by the four predictors compared to the constant term alone. The magnitude of the additional variation explained by the predictor variables is not incon-

TABLE 1 Empirical Results for Poisson Regression Models

Parameter	Accident Type		Pooled Model	Single Auto	Single Truck	Auto-Auto	Truck-Truck	Auto-Truck	Auto Involved	Truck Involved	Single Vehicle Accident	Multiple Vehicle Accident
Constant		β_0	1.775	0.572	0.481	0.128	0.293	0.359	1.038	1.117	1.047	0.735
		t	(17.20)	(10.33)	(8.48)	(5.26)	(5.80)	(7.25)	(13.74)	(12.54)	(13.43)	(10.75)
VMTauto	(10^6)	β_1	0.255	0.513	-0.066	0.984	-0.929	0.146	0.502	-0.135	0.300	0.183
		t	(3.49)	(-4.99)	(-0.44)	(4.25)	(-3.84)	(0.79)	(5.40)	(-1.32)	(3.19)	(1.57)
VMTtruck	(10^6)	β_2	0.229	-0.059	0.674	-0.412	1.769	0.556	0.005	0.751	0.184	0.311
		t	(2.48)	(-0.40)	(3.12)	(-1.68)	(4.41)	(2.28)	(0.05)	(4.94)	(1.56)	(2.07)
Snow	(hrs)	β_3	0.626	0.569	0.683	0.741	0.377	0.673	0.634	0.624	0.617	0.636
		t	(9.90)	(5.32)	(5.41)	(4.22)	(1.85)	(4.47)	(8.12)	(7.18)	(7.55)	(6.39)
Rain	(hrs)	β_4	0.025	0.180	0.097	-0.186	-0.277	-0.252	0.037	-0.076	0.150	-0.244
		t	(0.39)	(1.93)	(0.74)	(-0.87)	(-1.06)	(-1.35)	(0.48)	(-0.77)	(1.98)	(-1.97)
Average Daily Accidents			1.995	0.789	0.466	0.230	0.178	0.332	1.351	0.975	1.255	0.740
Number of Accidents in 1982			728	288	170	84	65	121	493	365	458	270
LL(c)			-714.29	-455.46	-339.00	-222.19	-184.21	-278.15	-605.67	-516.44	-566.55	-451.57
$LL(\hat{\beta})$			-668.77	-434.41	-321.82	-200.16	-164.81	-266.01	-565.37	-478.63	-539.42	-428.06
$\rho^2 = 1 - \frac{LL(\hat{\beta})}{LL(c)}$			0.064	0.046	0.051	0.099	0.105	0.044	0.067	0.073	0.048	0.052
$2(LL(\hat{\beta}) - LL(c))$			91.0	42.1	34.4	44.1	38.8	24.3	80.6	75.6	54.3	47.0

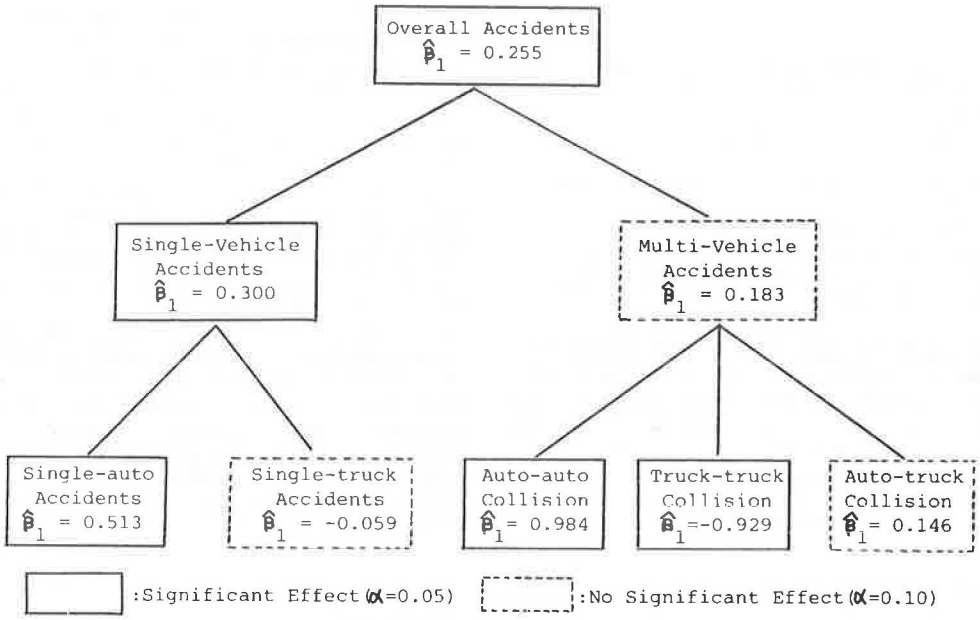

FIGURE 4 Effects of automobile VMT on the accident occurrence for different accident patterns.

sistent with results at disaggregate models in the travel demand literature. It must be remembered that the dependent variable is daily accident frequency. A significant amount of random variation might be expected with such a variable.

The summaries in Table 1 include values for LL(c), LL($\hat{\beta}$), ρ^2 and 2[LL($\hat{\beta}$) - LL(c)] for each of the models of the individual accident types. These statistics can be used to test the improvement in model fit that is obtained when a more detailed analysis is conducted (i.e., when one moves from a pooled model to single-vehicle crashes to separate single automobile and single truck collisions as is shown in Figure 4). The findings were consistent: the detailed models always achieved a statistically significant improvement in goodness of fit compared with the less-detailed models.

Effect of Automobile VMT

Figure 4 is an overview of the parameters for automobile VMT for each of eight separate but related models. The tree structure sequentially separates accidents into the more detailed categories. By estimating this sequence of models and comparing the significance and magnitude of a parameter, the influence of automobile VMT on different types of accidents can be determined. In addition to the parameter value, the figure also indicates statistical significance. As would be expected, automobile VMT is significant for the pooled model (overall accidents) as well as single-automobile and automobile-automobile collisions.

In order to better understand the interaction of the types of collisions, Figure 5 was constructed.

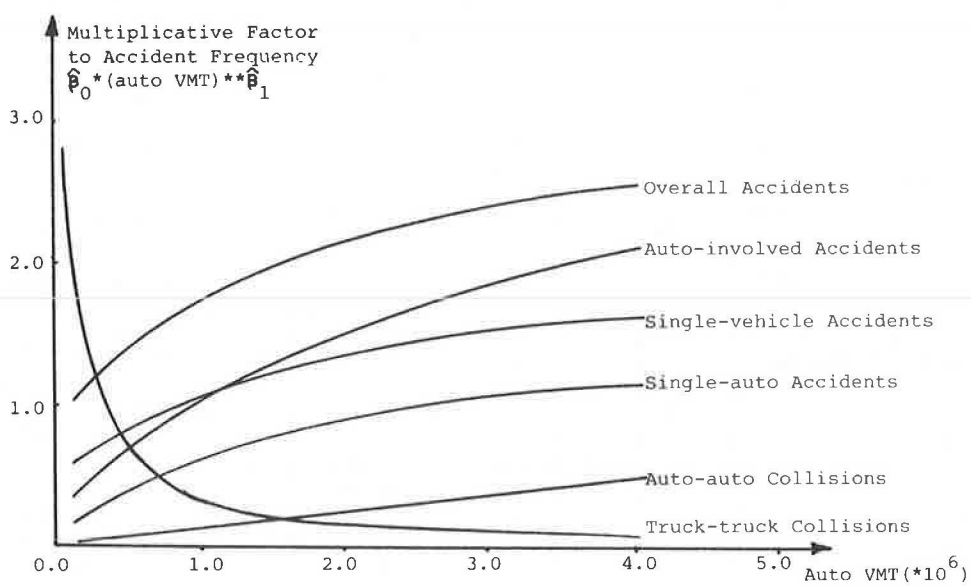

FIGURE 5 Effects of automobile VMT on accident occurrence within the range of available data.

For comparison purposes, the constant term, $\hat{\beta}_0$, is combined with the effect of automobile VMT and called a multiplicative factor. The number of overall accidents increases at a decreasing rate ($0 < \hat{\beta}_1 < 1.0$) as the automobile VMT increases. This means that increases in automobile traffic will increase the number of overall accidents when the other factors are fixed, but the accident rate, which can be determined by (VMTauto)**($\hat{\beta}_1$-1), will decrease.

This increase of overall accidents is mainly attributed to the increase of automobile-involved accidents, especially the single-automobile accidents.

From Figure 5, it can be seen that the curves for single-vehicle accidents and single-automobile accidents are parallel over the range of available automobile VMT data. This implies that the automobile VMT has no effect on the single-truck accidents. The number of single-automobile accidents increases at a decreasing rate, whereas the number of automobile-automobile collisions increases approximately linearly ($\hat{\beta}_1 \doteq 1.0$). Hence, the proportion of automobile-automobile collisions to automobile-involved accidents will increase as the automobile VMT increases.

The number of truck-truck collisions sharply decreases when the automobile VMT increases. This decrease might be expected to be compensated by an increase in truck-automobile collisions. However, neither a significant decrease of single-truck accidents nor a significant increase of truck-automobile collisions can be found. The effect on total multiple vehicle accidents is not significant because of the compensating effects on truck-truck and automobile-automobile crashes (this is also shown in Figure 4). This is a clear example of how this market segmentation approach can be used to gain information about the influence of predictor variables on different types of accidents.

Effect of Truck VMT

Truck VMT also has a significant effect on overall accidents (see Figures 6 and 7). The number of overall accidents increases at a decreasing rate ($0 < \hat{\beta}_2 < 1.0$) as the truck VMT increases (Figure 6). This increase of overall accidents is mainly attributed to the significant increase of truck-involved accidents, which include the single-truck, truck-truck, and truck-automobile accidents. The single-truck accidents and truck-automobile collisions increase at a decreasing rate, whereas the truck-truck collisions increase at an increasing rate. From Figure 7, it can be seen that the number of truck-truck collisions will occupy a significant proportion of truck-involved accidents when truck traffic is high. The number of automobile-automobile collisions marginally decreases as the truck VMT increases (Figure 6). The increase of truck-automobile collisions and decrease of automobile-automobile collisions when the truck VMT increases verify the hypothesis that automobile-automobile collisions shift to truck-automobile collisions as truck VMT increases.

Effects of Environmental Variables

The hours of snow have a significant effect on accident occurrence for all the accident patterns. The values of the parameter of snow hours are similar for all the accident patterns except the truck-truck collisions. The parameter indicates that more snow hours will increase accident occurrence at a decreasing rate ($< \hat{\beta}_3 < 1.0$), and the magnitude of this effect is quite similar for all the accident patterns except truck-truck collisions. The hours of snow have a lesser effect on truck-truck collisions than on the other accident patterns.

The hours of rain have a significant effect on single-automobile accidents, single-vehicle accidents, and multivehicle accidents. As the hours of rain increase, single-vehicle accidents increase at a decreasing rate, whereas multivehicle accidents decrease at a decreasing rate. However, overall accidents do not significantly increase. It appears that increases in rain hours tend to shift multivehicle accidents to single-vehicle accidents. The increase of single-vehicle accidents results from an increase in single-automobile accidents. In general, rainfall has much less of an effect on accident occurrence than snowfall.

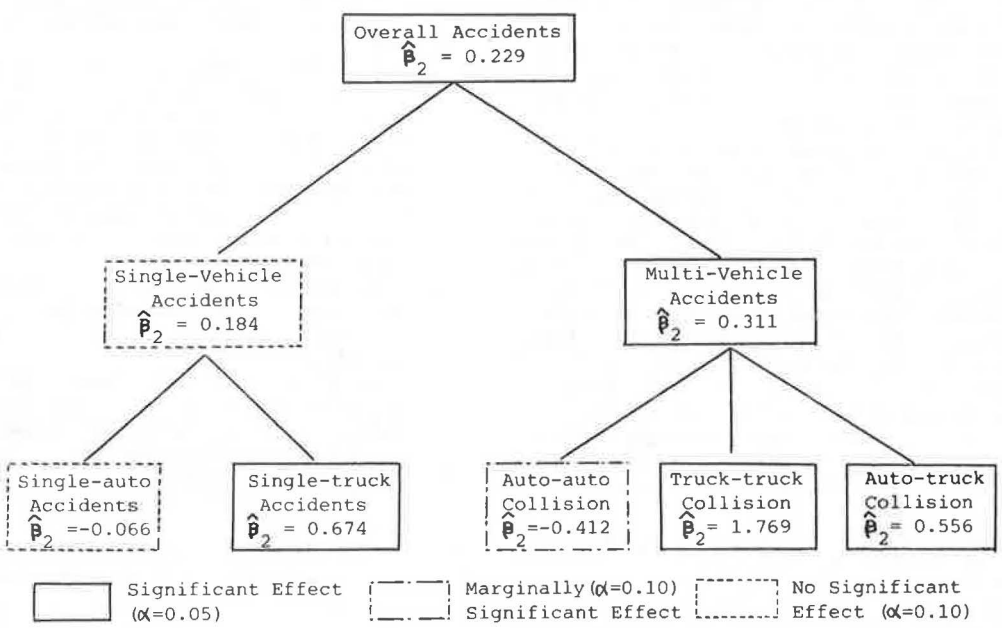

FIGURE 6 Effects of truck VMT on the accident occurrence for different accident patterns.

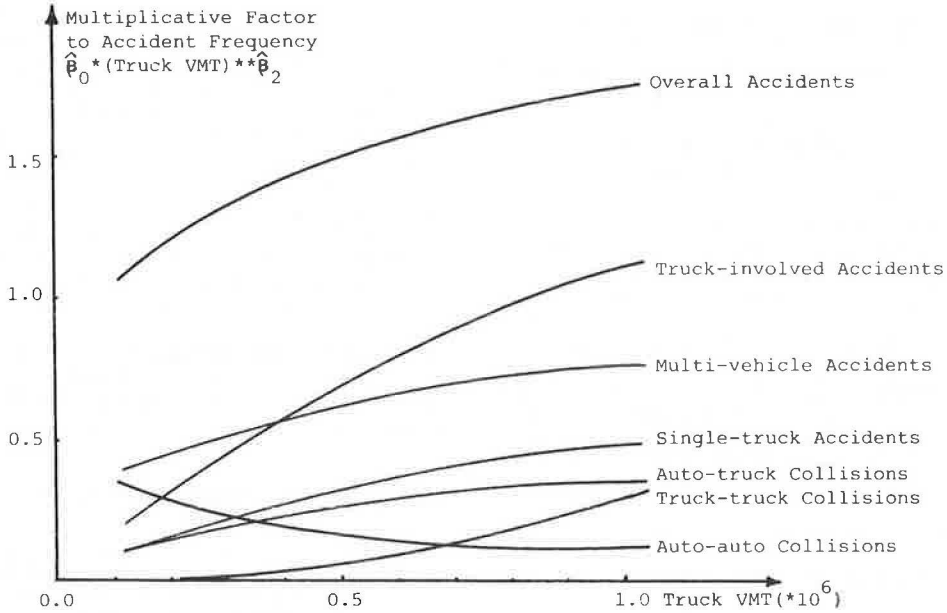

FIGURE 7 Effects of truck VMT on accident occurrence within the range of available data.

SUMMARY AND CONCLUSIONS

The analysis of highway accidents and identification of factors contributing to their occurrence is a complex process. A time-space framework is presented to facilitate a review of the literature and introduce the use of various probability distributions to model accident occurrence.

The normal distribution, which underlies traditional linear regression and hypothesis testing methods, should be used with caution because of problems associated with nonnegativity and error terms with unequal variance. If the underlying accident process is one in which the mean accident frequency is functionally related to the variance (e.g., Poisson distribution), parameters in a linear regression model will be unbiased but will have incorrect confidence limits. If the objective of the regression is to identify factors that significantly affect accident occurrence, incorrect confidence limits invalidate hypothesis tests of parameter significance--a serious shortcoming. Regressing accident rates rather than accident frequency may still result in unequal error variances, particularly when the underlying process is Poisson.

Poisson regression applied directly to accident data is proposed as a method to overcome many of these shortcomings. A Poisson regression model is applied to daily accident, travel mileage, and environmental data from the Indiana Toll road. Market segmentation is used to study whether VMT and weather conditions have different effects on different types of vehicle crashes. The models reveal that automobile and truck accidents are directly related to automobile and truck travel (as expected). As truck VMT increases, there is also a marginal reduction in automobile-automobile collisions and an increase in automobile-truck collisions. Snow strongly affects all accident types, whereas rainfall primarily increases the mean automobile accident frequency and has no effect on trucks.

Poisson regression has superior statistical properties for many potential applications to highway safety. In addition, it can be used with generally smaller sample sizes than linear regression. In conjunction with the use of segmentation, it can yield important insights about the significance of factors in accident occurrence.

ACKNOWLEDGMENT

The authors gratefully acknowledge the financial support provided by Yellow Freight Systems, Inc., United Parcel Service Foundation, and the Gar Foundation of Roadway Express, Inc.

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The findings and conclusions as well as any errors and omissions are solely the responsibility of the authors, not the project sponsors.

Publication of this paper sponsored by Committee on Traffic Records and Accident Analysis.

Identification of Accident Factors on Highway Segments: A Method and Applications

T. CHIRA-CHAVALA and KING K. MAK

ABSTRACT

An algorithm was developed to provide traffic engineers a means to identify factors or combinations of factors that cause accident overrepresentation at a given highway location relative to some average. The output from this algorithm can be used for further site investigation to develop accident countermeasures that may be responsive to the problems at the site. It also provides quantitative measures of the degree of overrepresentation for each factor at that site. Available mainframe computer programs to facilitate the computation involved are given. An example of application of this algorithm to a highway location in Texas is described in full detail. Comparison of the output from this full algorithm with that from an automated microcomputer program developed at the Texas Transportation Institute is also discussed.

The objective of this paper is to develop an algorithm to identify accident factors that are overrepresented at a selected roadway location relative to some "average," as well as to determine the magnitude of the overrepresentation. The output from such an analysis can thus be used as input for

1. Developing specific accident countermeasures for that location; and
2. Establishing priority locations within a city, county, or district where accident remedies may be more urgently needed.

INTRODUCTION

Traffic engineers and planners are often faced with the tasks of having to identify roadway locations that are deemed accident hazardous and to determine effective remedies to alleviate the problems. The task of identifying locations with high accident history has been greatly facilitated by available computerized accident data, roadway inventory files, and computer programs to isolate highway segments that show high accident rates, frequency, and severity.

To determine appropriate accident countermeasures, the causes of the problems at the site must first be understood and identified. Once this is accomplished, engineers or accident investigators can conduct in-depth site investigations and then develop appropriate remedies. Procedures to reliably determine causative accident factors and their magnitude of overrepresentation at a given site have not been developed or well documented.

Accidents are complex phenomena, and the problems at different locations are likely to be different or site specific. When properly conducted, analyses of past accident records for a specific site of interest can help illuminate possible causes of accidents at that location.

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CONCEPTUAL BASIS

The procedure developed here is aimed at identifying, from computerized accident data, characteristics of accidents (factors) or combinations of factors that are overrepresented at the site relative to some average. This average may be accidents on a similar road class within a city, county, or district. The algorithm is based on the principles of discrete-multivariate models and is capable of simultaneously analyzing a number of potential variables. In this way, both independent (or main) effects and interactions of these variables with one another can be systematically determined. Effects due to confounding variables, which may jeopardize the results, can therefore be minimized or avoided.

The number of accidents on a given segment of highway 2 to 3 mi long is not likely to be very large to permit a simultaneous analysis of an unlimited number of variables of interest. Fortunately, most accident variables usually correlate and interact with one another in such a way that only some, but not all, will be required in the analysis. This procedure therefore incorporates a variable-selection step that takes place before the model estimation. The two-stage algorithm is fully described in the following section.

ALGORITHM

The algorithm consists of two stages: variable selection and modeling. Variable selection involves the systematic selection of variables that may be significantly overrepresented at the site and elimination of those that are statistically nonsignificant. Only the significant variables are further analyzed at the modeling stage. The modeling involves determining specific factors or combinations of factors that are overrepresented at the site, as well as the magnitude of such overrepresentation. Figure 1 shows a flow chart for the entire algorithm.

Variable Selection

This is a sequential procedure based on two measures of statistical association in contingency table

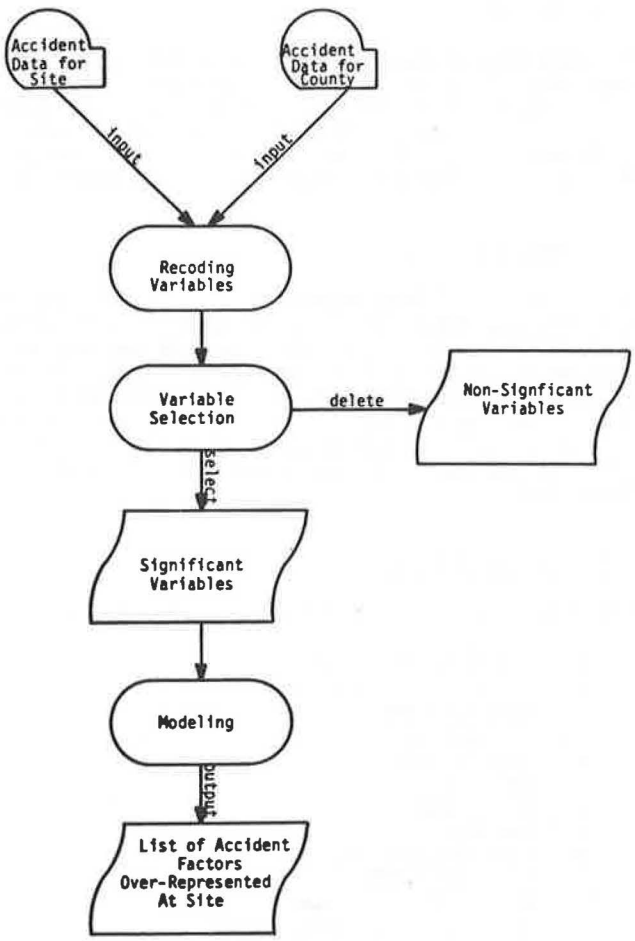

FIGURE 1 Flow chart for the algorithm.

analyses: Q_T and Q_{CMH} (1). In each step of the variable selection, one independent variable that is the most significant is selected after examining the effects of all (unselected) variables on accident overrepresentation at a site. The dependent variable for variable-selection analyses is site/average. The null hypothesis associated with the tests of Q_T and Q_{CMH} can be stated as follows (1):

> H_o: for each level of the independent variables, the accidents are distributed at random between the site and the county (assuming county as the comparison average) for all levels of the covariable(s).

The variable selection algorithm follows these steps (2):

1. A two-way contingency table of accident frequency is formed between the dependent variable (site/county) and each of the potential independent variables. A Pearson chi-square is then computed. The variable selected is one that has the highest value of chi-square per degree of freedom.
2. For each of the variables not yet selected, a contingency table of accident frequency is formed among this variable, the dependent variable, and the variable previously selected. Q_T is calculated, which reflects both the main effect of this variable, as well as its interaction with the previously selected variable. The variable selected in this step is the one that has the highest Q_T value per degree of freedom. On the other hand, those variables showing statistically nonsignificant Q_T values will be eliminated from further analysis.

In this context, Q_T expresses the extent of "total association" of the variable with the dependent variable, having accounted for the previously selected variable. The derivation of Q_T is given by Landis et al. (1) and its formula is given as

$$Q_T = \sum_{h=1}^{q} G_h' \, [\mathrm{Var}\,\{G_h \mid H_o\}]^{-1} G_h \qquad (1)$$

where

$h = 1, 2, \ldots, q$, is the levels of the previously selected variable(s);
G_h = a matrix of the differences between observed and expected frequencies under H_o; and
G_h' = a transposed matrix of G_h.

The degrees of freedom for Q_T is $q(s - 1)(r - 1)$, where s and r are the levels of the independent variable under investigation and the dependent variable, respectively.

3. The variable selection process then continues in this manner until completion. After the first few steps of the variable selection, however, the cell frequencies of the contingency table may thin out considerably, and the degrees of freedom may increase so rapidly that Q_T may become less effective. In this situation, Q_{CMH} will be used as a selection criteria instead. Q_{CMH} is not as sensitive to small cell size as is Q_T, and its test of significance is based on degrees of freedom of $(s - 1)(r - 1)$. Q_{CMH} is capable of capturing a weak but consistent effect of a variable although it does not reflect the total contribution, which includes interactions with other variables as does Q_T (2).

The derivation of Q_{CMH} is also reported by Landis et al. (1) and its formula is given as

$$Q_{CMH} = G' \, [\mathrm{Var}\,\{G \mid H_o\}]^{-1} G \qquad (2)$$

where $G = \sum_{h=1}^{q} G_h$.

The variable selected using Q_{CMH} is one that has the highest Q_{CMH} per degree of freedom.

The variable selection process is completed when either of the following is met: (a) the list of potential variables is exhausted, or (b) the data thins out so much that neither Q_T nor Q_{CMH} are appropriate.

Modeling

Accident characteristics or combinations of these characteristics that are overrepresented at a site relative to the county can be isolated and the magnitude of their overrepresentation quantified using the following model estimation technique.

For illustration, assume that three independent variables were selected by the variable selection process. The following procedure applies, which remains unchanged for any number of the independent variables:

1. Accident frequency for county only is cross-classified by these three variables.
2. A log-linear model that best describes this county contingency table is estimated. A log-linear model expresses the cell probabilities as a function

of significant main effects of and interactions among the variables. Such a model can generally be expressed as

$$\ln(P_{ijk}) = u + u_1 + u_2 + u_3 + U_{12} + u_{13} + \ldots \quad (3)$$

where

P_{ijk} = estimated cell probability for the (i,j,k)th cell,
u = overall mean,
u_1 = main effect of variable 1,
u_2 = main effect of variable 2,
u_3 = main effect of variable 3, and
u_{12} = interaction between variables 1 and 2, and so on.

The goodness-of-fit for a log-linear model is a likelihood ratio statistic (3):

$$X^2 = -2 \sum \text{(observed) log (estimated/observed)} \quad (4)$$

3. A contingency table of expected accident frequency for the site, if sites and county are not different, is constructed. The following formula is used to compute the expected cell counts for the site. This contingency table is cross-classified by the same variables as those in Step 1.

$$E_{ijk} = N \times P_{ijk} \quad (5)$$

where N is the total number of accidents at the site.

4. For each cell of the site contingency table, compute the overrepresentation indicator or the Freeman-Tukey deviate (3):

$$Z_{ijk} = (X_{ijk})^{1/2} + (X_{ijk} + 1)^{1/2} - (4E_{ijk} + 1)^{1/2} \quad (6)$$

where X_{ijk} is the observed accident frequency of the (i,j,k)th cell for the site. The overrepresentation indicator (Z_{ijk}) reflects the extent to which the actual observed number of accidents in any one cell of the site contingency table differs from the expected number of accidents in that cell, if the site is indeed no different from the county. A large positive value of Z_{ijk} indicates that the observed number of accidents at the site is higher than expected, and therefore an overrepresentation is indicated for that cell. A negative value of Z_{ijk} indicates that the author did not observe as many accidents as expected at the site for that cell. When the observed and the expected number of accidents are similar, Z_{ijk} will be a small positive number that is less than 1.

One property of this indicator that is particularly useful here is that its magnitude is a function of both (a) the extent to which the observed accident frequency differs from the expected frequency and (b) the cell size. That is, a larger value of either (a) or (b) will result in a larger positive Z_{ijk}. Therefore, a cell that has higher accident counts will display a higher overrepresentation ranking indicator than a cell that has lower accident counts, even if both indicate identical percent differences between observed and expected frequencies.

5. The cells that show Z_{ijk} values larger than, for example, 1.5 are listed. These cells represent combinations of accident factors that are found to be significantly overrepresented at the site relative to the county. The selection of 1.5 as the cutoff point is based on the fact that when the observed and the expected frequencies are similar, Z_{ijk} will be less than 1. Its value will be even smaller for increased cell size.

APPLICATIONS

The algorithm developed in the Algorithm section has been applied to an analysis of accident data at a number of sites on urban Interstate and urban non-Interstate systems in Texas. As an example for a case study, the analysis carried out for one of these sites is fully described in the next paragraph.

Description of Site

The site is a 2.4-mi segment of a U.S. highway in San Antonio, Texas. It is a six-lane divided urban freeway, full access control, with both straight and sharp-curve segments. From 1980 through 1982, 254 accidents were reported between milepoints 23.8 and 26.1. These were fatal, injury, or property-damage-only accidents. The comparison average for this site are the accidents on all urban U.S. highways in Bexar County.

Independent Variables

The following 12 variables were initially considered:

1. Degree of curvature
2. Weather and surface condition
3. Accident time
4. Accident type
5. Vehicle type
6. Severity
7. Driver age
8. Speeding
9. Driving while intoxicated (DWI)
10. Light condition
11. Vehicle damage scale
12. Driver license status

The levels of these variables are given in Table 1. Two different levels for these variables were defined: one for variable selection purposes and the other (more detailed) for modeling. This was to minimize the number of overly small cells that would adversely affect variable selection more than modeling. The first five variables are important because they are directly applicable to developing traffic engineering-related countermeasures; they are called primary variables. The other seven variables are mostly driver related and they help to further illuminate the causes of accidents; they are called secondary variables. Their usefulness in traffic engineering-related countermeasures is probably more limited.

To ensure that the five primary variables will have a good chance of being evaluated before the data hopelessly thin out, the variable selection will be applied to these variables first. Once this is done, the secondary variables will then be examined.

Result of Variable Selection

The variable selection yielded the following outcome step by step:

Step 1: Table 2 gives the values of the Pearson chi-square, X_p^2, obtained for the five primary variables evaluated: degree of curvature, weather and surface condition, accident time, accident type, and vehicle type. Degree of curvature, which shows the largest X_p^2 per degree of freedom, was selected.

Step 2: Table 3 gives the values of Q_T for the

TABLE 1 Levels of Potential Independent Variables

Variable	Level for Variable Selection	Level for Modeling
Degree of curvature	Straight Curve	Straight Less than 2 degrees Greater than 2 degrees
Weather/surface condition	Dry Wet	Dry Wet
Accident time	Weekday, rush hour Weekday, non-rush hour; or weekend, day Evening or night	Weekday, rush hour Weekday, non-rush hour Weekend, day Evening or night
Accident type	Single-vehicle Multivehicle Rear-end, sideswipe Other	Single-vehicle: barrier/object Single-vehicle: other Multivehicle: rear-end side-swipe angle, head-on
Vehicle type	Passenger cars only At least one pickup or van At least one heavy truck or bus	Passenger cars only At least one pickup or van At least one heavy truck or bus
Severity	Fatal or injury Property damage only	Fatal Injury Property damage only
Driver age	At least one over 55 or at least one under 21 21 to 55	At least one over 55 At least one under 21 21 to 55
Speeding	At least one speeding No speeding	At least one speeding No speeding
Driving while intoxicated (DWI)	At least one DWI No DWI	At least one DWI No DWI
Light condition	Daylight Other	Daylight Other
Vehicle damage scale	Front Rear Side Other	Front Rear Side Other
Driver license status	At least one out of state Other	At least one out of state Other

TABLE 2 Result of Variable Selection: Step 1

Independent Variable	X_p^2	Degrees of Freedom	p-value
Degree of curvature	228.0	1	0
Weather/surface condition	31.2	1	0
Accident time	8.8	2	0.012
Accident type	14.0	2	0.001
Vehicle type	1.6	2	0.437

TABLE 3 Result of Variable Selection: Step 2

Variable	Q_T	Degrees of Freedom	p-value
Curvature x surface condition	25.5	2	0
Curvature x accident time	17.9	4	0.001
Curvature x accident type	12.7	4	0.013
Curvature x vehicle type	1.3	4	0.856[a]

[a]Eliminated.

four primary variables not selected in Step 1. Weather and surface condition, which shows the largest Q_T per degree of freedom, was selected. On the other hand, vehicle type with a nonsignificant Q_T value was eliminated from further analysis.

Step 3: Table 4 gives the values of Q_T for the primary variables not yet selected or eliminated. Q_{CMH} values were also computed for these variables. Although the Q_T values for both variables were not highly significant, the Q_{CMH} value for accident time was.

TABLE 4 Result of Variable Selection: Step 3

Variable	Q_T	Degrees of Freedom	p-value	Q_{CMH}	Degrees of Freedom	p-value
Curvature x surface x accident time	16.9	8	0.032	11.8	2	.003
Curvature x surface x accident type	15.2	8	0.055	8.7	2	.013

This indicates that accident time had consistent main effect on accident overrepresentation at the site. This variable was, therefore, selected. Accident type was retained because its Q_{CMH} showed a p-value of 0.013, indicating that this variable may have consistent (though relatively weak) main effect.

Step 4: A cross-classification of accident frequency by degree of curvature, weather and surface condition, accident time, and accident type, resulted in 32 percent of the cells having fewer than four accidents. Accident type was therefore regarded as a "sparse" variable, and no variable selection analysis was performed for this variable. Sparse variables are those associated with too many small (less than four accidents) or empty cells to warrant meaningful variable-selection analyses. Such variables have neither been selected nor eliminated as significant variables because of the sample size limitation. As an option, they can be further investigated in the modeling stage.

Step 5: Having exhausted the primary-variable list, the selection process continued with the secondary variables. Each secondary variable was first cross-classified with the dependent variable and all those variables already selected. Only severity, driver age, speeding, and driver license status showed cells with reasonable sample size to justify variable-selection analyses. All other secondary

TABLE 5 Result of Variable Selection: Step 4

Variable	Q_T	Degrees of Freedom	p-value	Q_{CMH}	Degrees of Freedom	p-value
Selected variables x speeding	22.4	12	.034	1.16	1	.281
Selected variables x driver age	14.0	12	.302[a]	0.78	1	.378
Selected variables x severity	11.2	12	.511[a]	0.43	1	.510
Selected variables x license status	9.7	12	.641[a]	2.33	1	.127

[a]Eliminated.

variables were sparse. The values of Q_T and Q_{CMH} for severity, driver age, speeding, and driver license status are given in Table 5. Of these, only the Q_T for speeding showed a p-value less than 0.05; all four showed nonsignificant Q_{CMH}s. Therefore, speeding was selected while the other three variables were eliminated from further analysis.

The independent variables that were found to be significant from the variable selections were

- Degree of curvature
- Weather and surface condition
- Accident time
- Speeding

The independent variables that were sparse were

- Accident type
- Light condition
- Vehicle damage scale
- DWI

The independent variables that were found to be nonsignificant and thus eliminated from further analysis were

- Vehicle type
- Driver age
- Severity
- Driver license status

Modeling Result

The modeling result is described next, step by step:

1. A contingency table of accident frequency for county, cross-classified by the selected variables (a) degree of curvature, (b) weather and surface condition, (c) accident time, and (d) speeding is given in Table 6.

TABLE 6 Number of Accidents for County

			Speeding (V1)	
Curvature (V4)	Condition (V3)	Time (V2)	Yes	No
Straight	Dry	Weekday, rush hour	42	82
		Weekday, non-rush	25	59
		Weekend, day	7	21
		Evening or night	90	179
	Wet	Weekday, rush hour	15	17
		Weekday, non-rush	9	10
		Weekend, day	6	7
		Evening or night	40	28
Less than 2 degrees	Dry	Weekday, rush hour	3	6
		Weekday, non-rush	3	6
		Weekend, day	2	3
		Evening or night	10	21
	Wet	Weekday, rush hour	0	2
		Weekday, non-rush	0	2
		Weekend, day	2	0
		Evening or night	2	6
Greater than 2 degrees	Dry	Weekday, rush hour	0	4
		Weekday, non-rush	0	3
		Weekend, day	0	1
		Evening or night	9	9
	Wet	Weekday, rush hour	0	2
		Weekday, non-rush	2	1
		Weekend, day	0	2
		Evening or night	3	4

2. A log-linear model that best describes the data in Table 6 was found to be

$$\ln (P_{ijkl}) = u + u_2 + u_4 + u_{13} \quad (7)$$

This model states that the probability of accidents for the county is influenced by the main effects of accident time and degree of curvature, as well as the interaction between speeding and weather and surface condition. The chi-square goodness-of-fit test was 44.14 for 39 degrees of freedom (a p-value of 0.263), indicating a very good fit.

3. If the site and the county were similar in accident characteristics, the expected number of accidents at the site, cross-classified by degree of curvature, weather and surface condition, accident time, and speeding, could be obtained (using Equations 5 and 7) as given in Table 7. Also given in Table 7 are the actually observed number of accidents at the site.

4. The magnitude of accident characteristics that was overrepresented at the site relative to the county was computed by using Equation 6. The result is given in Table 8. Only those characteristics associated with the overrepresentation indicator (z) of greater than 1.5 and the observed accidents of at least 7, are given in Table 7. These are cells that show a significantly higher than expected number of observed accidents at the site. Thus accident overrepresentation is indicated by these cells. The following findings can be drawn from Table 8:

(a) The curve section with curvature greater than 2 degrees was a major cause of accident overrepresentation, almost regardless of time of day, weather and surface condition, or presence or absence of speeding.

(b) The combination of this sharp curve, wet conditions, and speeding was particularly serious in causing accident overrepresentation, as indicated by consistently high values of the overrepresentation indicator (z).

(c) Accidents on this sharp curve were found to be especially overrepresented in the evening or at night (very high values of overrepresentation indicator are given).

(d) To a lesser extent than evening and nighttime, accidents on this sharp curve tended to be overrepresented during rush hours of week days.

NOTE: The accident overrepresentation analysis can conclude at this point or the analyst may choose to further examine the four sparse variables. As mentioned earlier, sparse variables may not necessarily be nonsignificant. Their statistical significance was not tested because of the sample size limitation. Analyses in the modeling stage to incorporate these sparse variables can help illuminate causes of accident overrepresentation at the site even further, if these variables are indeed significant. Sparse variables can be analyzed in the modeling stage by one of two methods:

1. Replacement of the last independent variable selected with each of the sparse variables, and Steps 1 through 5 are repeated. Because there were four sparse variables, four modeling analyses would be required.

2. Incorporation of each of the four sparse variables into a modeling analysis together with the four independent variables already selected, and Steps 1 through 5 are repeated. Again, four analyses would be required for the four sparse variables.

An advantage of the second method is that the final analysis result of accident-overrepresentation causes would be more complete. However, the smaller cell size due to an additional variable may make the result less interesting for practical purposes.

COMPUTER SOFTWARE

The algorithm as described can be performed by using computer programs that are currently available. For variable selection, PARCAT (4), which is a mainframe program, can be used. For modeling, BMDP (5), ECTA (6), or any other standard log-linear model program will be satisfactory. In order to perform the analysis using these computer programs and to reach the final outcome given in Table 8, users are required to have sufficient familiarity with the statistics involved to make statistical decisions and to se-

TABLE 7 Expected and Observed Number of Accidents for Site

Curvature (V4)	Condition (V3)	Time (V2)	Speeding (V1) Yes		Speeding (V1) No	
Straight	Dry	Weekday, rush hour	(12.7)	7	(26.1)	9
		Weekday, non-rush	(8.8)	2	(18.2)	8
		Weekend, day	(3.7)	0	(7.7)	7
		Evening or night	(29.4)	8	(60.6)	11
	Wet	Weekday, rush hour	(5.3)	3	(5.4)	6
		Weekday, non-rush	(3.6)	4	(3.7)	6
		Weekend, day	(1.5)	6	(1.5)	3
		Evening or night	(12.2)	5	(12.5)	6
Less than 2 degrees	Dry	Weekday, rush hour	(1.3)	2	(2.8)	3
		Weekday, non-rush	(.9)	2	(1.9)	1
		Weekend, day	(.4)	0	(.8)	2
		Evening or night	(3.1)	1	(6.5)	7
	Wet	Weekday, rush hour	(.6)	1	(.6)	2
		Weekday, non-rush	(.4)	0	(.4)	0
		Weekend, day	(.2)	0	(.2)	1
		Evening or night	(1.3)	4	(1.3)	2
Greater than 2 degrees	Dry	Weekday, rush hour	(.8)	7	(1.6)	9
		Weekday, non-rush	(.6)	2	(.1)	11
		Weekend, day	(.2)	1	(.5)	6
		Evening or night	(1.8)	17	(3.8)	28
	Wet	Weekday, rush hour	(.3)	8	(.3)	1
		Weekday, non-rush	(.2)	11	(.2)	1
		Weekend, day	(.1)	7	(.1)	0
		Evening or night	(.8)	12	(.8)	9

Note: Numbers in parentheses are expected numbers of accidents.

TABLE 8 Accident Overrepresentation Indicators for Site

Curvature	Condition	Time	Speeding Yes	Speeding No
Straight	Dry	Weekday, rush hour	a	a
		Weekday, non-rush	a	a
		Weekend, day	a	a
		Evening or night	a	a
	Wet	Weekday, rush hour	a	a
		Weekday, non-rush	a	a
		Weekend, day	a	a
		Evening or night	a	a
Less than 2 degrees	Dry	Weekday, rush hour	a	a
		Weekday, non-rush	a	a
		Weekend, day	a	a
		Evening or night	a	a
	Wet	Weekday, rush hour	a	a
		Weekday, non-rush	a	a
		Weekend, day	a	a
		Evening or night	a	a
Greater than 2 degrees	Dry	Weekday, rush hour	3.43	3.44
		Weekday, non-rush	a	5.58
		Weekend, day	a	a
		Evening or night	5.50	6.66
	Wet	Weekday, rush hour	4.35	a
		Weekday, non-rush	5.44	a
		Weekend, day	4.26	a
		Evening or night	5.02	4.11

[a]Overrepresentation indicator (z) less than +1.50 or observed number of accidents less than 7.

lect, evaluate, and interpret data at every intermediate step of the analysis.

The Texas Transportation Institute (TTI) has developed an automated microcomputer program for the entire algorithm (7). The program is part of a study conducted for the Texas State Department of Highways and Public Transportation. Because the entire analysis procedure is fully automated, it does not require intervention by the users at any of the intermediate steps. Once the subset of accident and roadway data are specified by a user, the program will automatically initiate the variable-selection analysis, carry out the modeling, and finally report the accident overrepresentation factors for that site. This program was written in turbopascal for IBM PC-XT or compatible systems.

This automated microcomputer program was based on the algorithm described in this paper but was especially modified for use on microcomputers. The decision for such a simplification was made for practical reasons: manageable run-time and storage memory of microcomputers. The analysis output from the full algorithm and that output from the automated program are compared in the following section for the site mentioned in the Applications section.

COMPARISON OF ANALYSIS RESULTS FROM FULL ALGORITHM AND AUTOMATED MICROCOMPUTER PROGRAM

For the site presented here, there were no differences in the result of variable selection as far as the independent variables selected or eliminated or the order for which the variables were selected. For modeling, the factors or combination of factors that caused accident overrepresentation at the site were found to be the same for both algorithms. However, the magnitude of the accident overrepresentation indicators (z) from the two algorithms was slightly different. Table 9 gives the values of z obtained from the automated microcomputer program.

Generally, the result provided by the automated microcomputer version is not expected to be very different from that provided by the full algorithm unless it concerns "borderline" cases. These borderline cases may be variables indicating p-values close to 0.05 in the variable selection stage or the overrepresentation factors associated with values of the indicator (z) close to +1.5. For the purpose of accident countermeasures, these borderline cases are likely to be less interesting, and thus they may not affect subsequent actions taken by engineers.

One advantage of the full algorithm that has not been developed for the automated microcomputer version is that for a certain site, Step 2 of the modeling may yield the result such that the levels

TABLE 9 Accident Overrepresentation Indicators for Site (from Automated Program)

Curvature	Condition	Time	Speeding Yes	Speeding No
Straight	Dry	Weekday, rush hour	[a]	[a]
		Weekday, non-rush	[a]	[a]
		Weekend, day	[a]	[a]
		Evening or night	[a]	[a]
	Wet	Weekday, rush hour	[a]	[a]
		Weekday, non-rush	[a]	[a]
		Weekend, day	[a]	[a]
		Evening or night	[a]	[a]
Less than 2 degrees	Dry	Weekday, rush hour	[a]	[a]
		Weekday, non-rush	[a]	[a]
		Weekend, day	[a]	[a]
		Evening or night	[a]	[a]
	Wet	Weekday, rush hour	[a]	[a]
		Weekday, non-rush	[a]	[a]
		Weekend, day	[a]	[a]
		Evening or night	[a]	[a]
Greater than 2 degrees	Dry	Weekday, rush hour	4.47	3.64
		Weekday, non-rush	[a]	4.54
		Weekend, day	[a]	[a]
		Evening or night	4.76	7.07
	Wet	Weekday, rush hour	4.83	[a]
		Weekday, non-rush	4.86	[a]
		Weekend, day	4.47	[a]
		Evening or night	4.83	3.64

[a]Overrepresentation indicator (z) less than +1.50 or observed number of accidents less than 7.

of some independent variables can be collapsed to form fewer levels. Collapsing the levels of variables, when statistically justified, is particularly desirable because it may allow additional variables to be incorporated in the modeling without sample size problems that may have arisen otherwise.

CONCLUSION

The algorithm reported here was developed to provide traffic engineers a powerful means to identify factors or combinations of factors that cause accident overrepresentation at a site relative to some average. In this way, engineers can use the output from this algorithm to develop remedial options that may be responsive to the problems at that site. Engineers and planners can also use the output to develop an areawide traffic safety improvement plan for the area's highway network by analyzing a number of potential sites and examining the values of accident overrepresentation indicator. Sites and/or factors that show higher values of such indicator may suggest more serious safety problems and a stronger need for safety improvement measures.

Currently, there are available mainframe computer programs to facilitate the statistical computation involved in carrying out the analysis. However, this requires the analysts to have sufficient knowledge of the statistical methods used. An automated microcomputer program has been developed by the Texas Transportation Institute for the Texas State Department of Highways and Public Transportation to eliminate this user requirement. Although the microcomputer program is a modified version of the full algorithm, its applications to date have suggested a comparable outcome to that provided by the full algorithm.

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Publication of this paper sponsored by Committee on Traffic Records and Accident Analysis.

Automated Analysis of High-Accident Locations

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ABSTRACT

A procedure was developed to identify high-accident locations on urban freeways, to analyze the accident experience at these locations, and to determine and evaluate appropriate remedial measures. The procedure consists of (a) a mainframe computer program to identify and rank highway sections by number of injury and fatal accidents per 100 million vehicle miles of travel, (b) a microcomputer program to identify factors overrepresented in accident occurrence at these locations relative to the average for similar highways in the area, (c) a multidisciplinary approach to identify accident causative factors and to devise appropriate remedial measures, and (d) evaluation of remedial measures actually implemented. The procedure is currently being field tested.

Identification of high-accident locations and associated accident causative factors as well as determination and evaluation of appropriate remedial measures at these sites are continuing functions of transportation engineers. This process is time consuming and tedious, requiring extensive compilation and analysis of accident data. Computerized accident data have long been used to identify high-accident locations, but the analyses of accident data to identify causative factors have not been as well developed or automated.

A study is being conducted by the Texas Transportation Institute (TTI) for the Texas State Department of Highways and Public Transportation (SDHPT) to develop a procedure to aid engineers in performing this task in a more systematic and efficient manner. Although the procedure is designed for use with urban Interstate highways and urban non-Interstate freeways, it can easily be modified for use with other highway types. The major components of the procedure are as follows:

1. A mainframe computer program to rank highway sections by using accident rate,
2. A microcomputer program to analyze accident data at selected high-accident locations,
3. A multidisciplinary approach to identify accident causative factors and to devise appropriate remedial measures, and
4. Evaluation of remedial measures actually implemented.

Only the first three steps of the procedure are reported in this paper, with emphasis on the microcomputer program for automated analysis of accidents.

The key steps for the two computer programs and their interactions are illustrated in the schematic diagram as shown in Figure 1. Brief descriptions of the two computer programs are presented as follows.

WINDOW PROGRAM

A mainframe computer program previously developed by TTI for the Texas SDHPT, known as the "WINDOW" program, is used to determine the accident frequency/rate of highway segments and to rank the segments according to the accident frequency/rate. The program utilizes a "window," that is, a highway segment of specified length, which is then moved along the highway network in 0.1 mi increments. For each window, the accident frequency/rate is calculated and compared to that of other windows. Those windows with the highest accident frequency/rate are identified.

The WINDOW program was designed with numerous built-in options to accommodate user-specified inputs, including

1. Years of accident data (1 to 5);
2. Accident selection (subsetting) criteria, for example, county, highway type, accident type, accident severity;
3. Length of window (0.1 to 10 mi);
4. Ranking by accident frequency or rate; and
5. Output format, for example, number of roadway segments to be ranked, reports to be generated.

For this specific application, the latest 3 years of accident data are used. The accidents are subset by county (only one county is studied each time); highway type (urban Interstate highways and urban non-Interstate freeways); accident type (excluding construction zone accidents); and accident severity (injury and fatal accidents only, excluding property-damage-only accidents). A 2-mi long window is used and the roadway segments are ranked by accident rate per 100 million vehicle miles of travel.

Construction zone accidents are excluded from consideration because traffic operating conditions, and hence the accident characteristics, are very different in construction zones when compared to normal highway conditions. The determination of accident frequency/rate is based on injury and fatal accidents only in an attempt to include accident severity in the identification of high-accident locations. Also, this will minimize the impact of differing accident reporting thresholds between various law enforcement agencies within the study area (i.e., county). Some large urban police departments in Texas have adapted the policy of reporting only injury and fatal accidents as opposed to the statewide reporting threshold of injury accidents or accidents involving more than $250 in property damages. It should be noted, however, that all accidents, including property-damage-only accidents, are used in the accident analysis of the procedure.

Traffic volume and other roadway-related data are obtained from the computerized roadway inventory

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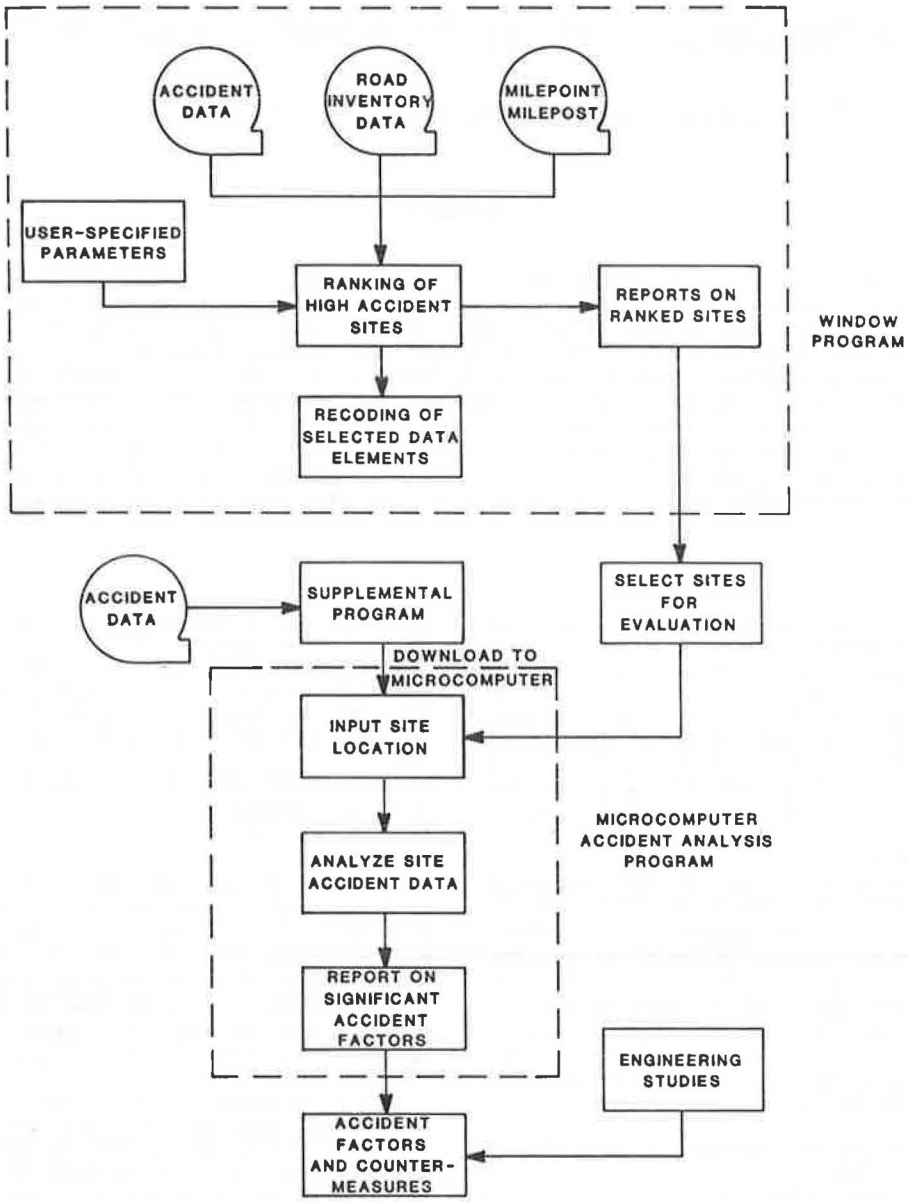

FIGURE 1 Schematic diagram illustrating key steps for the WINDOW program and the automated accident analysis programs.

file. The milepoint-milepost equivalency file establishes a track in going-down-the-highway order. The window is then moved along this track and takes snapshots every 0.1 mi to find the most hazardous locations with the highest accident rates.

The WINDOW program then outputs a user-specified number of 2-mi highway sections ranked by accident rate, as given in Table 1. The section length of 2 mi is selected arbitrarily and can be changed as appropriate. Other reports can also be generated, such as listing of highway sections sorted by highway number and accident counts by 0.1 milepoints.

The user then selects specific locations for evaluation from the list of high-accident locations generated from the WINDOW program. For evaluation purposes, minor changes can be made in the beginning and ending milepoints of the locations to coincide with identifiable landmarks, such as interchanges and bridge structures. These changes, if necessary, are accommodated by the microcomputer program before analysis of the accident data. Each of the high-accident locations selected is then analyzed individually using the microcomputer accident analysis program.

A supplemental mainframe computer program is used to create an accident data file from the state master accident data file for use with the microcomputer accident analysis program. The data file includes all accidents within the study area (which is a county for the purpose of this study) that meets the subsetting criteria used with the WINDOW program, except for accident severity (i.e., property-damage-only accidents are also included in the data file).

Because storage space is limited on the microcomputer, only selected data elements are included in the output data file, a list of which is given in Table 2. Also, many of the data elements are recoded to fewer levels for use with the microcomputer accident analysis program. The subsetting and recoding of the data elements are also handled by the supplemental computer program. The output acci-

TABLE 1 Example Output from WINDOW Program

Rank	Highway District	Highway	Beginning Milepoint County	Beginning Milepoint Control Section	Beginning Milepoint MPT	Ending Milepoint County	Ending Milepoint Control Section	Ending Milepoint MPT	Accidents	Rate (accidents/ 100 MVM)	Fatal Accidents	Fatalities	Injury Accidents	Injuries	PDO Accidents
1	12	US 0059	Harris	177-7	4.5	Harris	177-7	6.5	38	718.49	2	2	36	54	0
2	12	US 0059	Harris	177-11	5.1	Harris	177-11	7.1	282	343.23	12	13	270	377	0
3	12	SH 0146	Harris	389-5	1.1	Harris	389-5	3.1	54	333.06	2	2	52	98	0
4	12	SH 0225	Harris	502-1	7.1	Harris	502-1	10.7	93	319.69	3	3	90	125	0
5	12	US 0059	Harris	27-13	6.2	Harris	27-13	8.2	391	262.24	4	4	387	526	0
6	12	US 0059	Harris	177-11	2.3	Harris	177-11	4.3	197	230.26	8	9	189	283	0
7	12	US 0059	Harris	27-13	8.4	Harris	27-13	10.4	307	227.02	5	5	302	418	0
8	12	SH 0225	Harris	502-1	11.1	Harris	502-1	13.1	54	224.84	4	6	50	78	0
9	12	SH 0225	Harris	502-1	14.4	Harris	502-1	16.4	39	207.07	2	2	37	55	0
10	12	US 0059	Harris	177-11	7.5	Harris	27-13	1.0	187	190.74	5	5	182	263	0
11	12	US 0059	Harris	177-7	9.5	Harris	177-11	1.7	141	190.67	3	3	138	192	0
12	12	US 0059	Harris	27-13	4.1	Harris	27-13	6.1	249	181.05	7	7	242	337	0
13	12	SH 0146	Harris	389-5	3.2	Harris	389-5	5.2	18	170.17	1	1	17	31	0
14	12	US 0059	Harris	27-13	2.0	Harris	27-13	4.0	207	167.05	3	3	204	287	0
15	12	SH 0146	Harris	389-12	9.7	Harris	389-5	0.6	31	161.77	2	2	29	43	0
16	12	SH 0225	Harris	502-1	1.2	Harris	502-1	3.2	104	146.95	6	6	98	139	0
17	12	US 0059	Harris	27-13	12.0	Harris	27-13	14.0	93	114.46	4	5	89	130	0
18	12	US 0059	Harris	177-7	7.1	Harris	177-7	9.1	83	107.21	3	3	80	133	0
19	12	SH 0225	Harris	502-1	3.4	Harris	502-1	5.4	54	105.52	3	5	51	85	0

Note: 1980-1982 Texas on-system accidents—non-Interstate urban freeway. Rank 30, 2-mi segments, main lane Harris County. Subset excludes PDO and construction accidents. Segments sorted by rank for rate.

TABLE 2 List of Primary and Secondary Variables

Variable	Level
Primary	
Accident type	Single vehicle (fixed object)
	Other
	Multivehicle
	Rear-end
	Sideswipe
	Other
Accident time	Weekday, rush hour
	Weekday, nonrush hour
	Weekend, daytime
	Evening/night
Weather/surface condition	Adverse
	Not adverse
Degree of curve	Straight
	Less than 4 degrees
	Greater than 4 degrees
Vehicle type	Passenger car
	Pickup truck/van
	Truck/bus
Secondary	
Accident severity	Fatal and injury
	Property damage only
Driver age	Under 21
	21 to 55
	Over 55
Speeding	Yes
	No
DWI or DW drugs	Yes
	No
Driver license status	Out-of-state or military
	In-state

dent data file is then downloaded onto the microcomputer.

MICROCOMPUTER ACCIDENT ANALYSIS PROGRAM

The microcomputer accident analysis program (MAAP) is designed to provide users a list of accident factors and their interactions that are significantly overrepresented at the location under consideration in comparison to an average. The program is written in turbo-pascal for use with IBM PC-XT or compatible microcomputers with MS-DOS version 2.1 or above. The program has more than 2,300 lines of code and requires 150K of memory. A minimum configuration of 256K memory and a hard disk drive is required to use the program.

The accident analysis methodology is based on the simple concept of overrepresentation. The assumptions are that certain accident characteristics (factors) or combinations of factors, or both, are overrepresented at a high-accident location when compared to the average of similar highway types within the study area (note that a different baseline of comparison can be used as appropriate for other applications), and that these overrepresented accident factors and/or combinations of factors are indicative of accident causative factors at the high-accident location.

The accident analysis is based on a discrete-multivariate algorithm. A two-staged procedure is used: variable selection and modeling. The first stage selects a set of significant variables or factors for further analysis in the second stage. This intermediate step is required because the number of variables that can be simultaneously analyzed in the modeling stage is restricted by the number of accidents at a given site. It is therefore desirable to reduce the number of variables to only those that are statistically significant to minimize the problem of insufficient sample size in the modeling process.

The algorithm for the entire analysis, variable selection and modeling, is completely automated. Users' intervention at any of the intermediate steps is not required. Once a site is specified by the user, the algorithm will start with the variable selection process and automatically proceed to modeling at the end of variable selection. The output of overrepresented accident factors for that site is then printed.

Variable Selection

The purpose of the variable selection process is to narrow down the list of 13 potential variables to only those with significant influence on accident overrepresentation at the high-accident sites. The significant variables are then analyzed in the modeling process while the nonsignificant variables are eliminated from further consideration.

The 10 variables, as given in Table 2, are categorized as either primary or secondary. The primary variables (1 through 5) are considered to be more important because they are directly applicable to

the development of traffic engineering-related countermeasures. The secondary variables (5 through 10) contain mostly driver-related factors and are useful for law enforcement-related countermeasures.

A step-by-step description of the algorithm is presented as follows:

1. Each of the primary variables is cross-classified with the dependent variable (i.e., site versus average) to form a two-way table with accident counts as entries in the cells. Pearson chi-square statistic is calculated for each of these tables. The variable with the smallest p-value (i.e., highest level of significance) is then selected in this initial step.

2. For each of the remaining primary variables, a three-way contingency table is formed among this variable, the dependent variable, and the variable selected in Step 1. A statistic, Q_T, is then calculated (1-3), which reflects both the main effect of this variable and its interaction with the previously selected variable. The variable with the smallest p-value for the Q_T statistic is then selected as the second variable. Also, variables with nonsignificant p-values in the Q_T statistic are eliminated from further consideration.

3. The process in Step 2 is repeated for the remaining primary variables, with the addition of one more selected variable at each step. The process will continue until all primary variables have been either selected or eliminated, or until the data are exhausted. In other words, the data may have thinned out so much that the sample size for a large number of cells in the contingency table becomes too sparse for proper analysis. In such a case, the last entered significant primary variable is dropped and the process as described in Step 2 is repeated with each of the sparse variables. If the Q_T statistic is significant, the sparse variable will be included in the modeling process. If the Q_T statistic is not significant or if the data remain sparse, the variable will be dropped from further consideration.

4. After all primary variables have been evaluated, the selection process is continued for the secondary variables. The process described in Steps 2 and 3 are repeated until all the secondary variables are either selected or eliminated, including the sparse variables.

An intermediate program output, which summarizes the results of the variable selection process, is provided. Each variable is listed as significant, sparse but significant, or nonsignificant. Only variables found to be significant, or sparse but significant, are evaluated in the modeling process.

Modeling

The purpose of the modeling process is to identify and to isolate combinations of levels within the significant variables that contribute to accident overrepresentation at the high-accident location, relative to the average. A step-by-step description of the modeling algorithm is presented as follows:

1. A contingency table on accident frequency (or counts) for the county is created, including all the significant primary and secondary variables previously identified, but excluding those sparse variables that are significant. The cell probabilities for all the cells in the contingency table are then computed. There are a number of ways that these cell probabilities can be obtained (1). The method chosen for this microcomputer program is as follows. For the (i,j,k)th cell, the cell probability, P_{ijk}, is determined by dividing the accident count in the cell (Y_{ijk}) by the overall total ($\sum_{ijk} Y_{ijk}$), that is

$$P_{ijk} = Y_{ijk} / \sum_{ijk} Y_{ijk}$$

The subscripts i, j, and k denote the levels of the selected significant variables.

2. A contingency table for the expected accident frequency of the site under evaluation, E_{ijk}, is then computed based on the cell probabilities of the county determined under Step 1, that is,

$$E_{ijk} = N \times P_{ijk}$$

where N is the total number of accidents for the site under evaluation.

3. Cell residuals are then computed by comparing the actual or observed accident frequencies at the site under evaluation, X_{ijk}, to the expected accident frequencies, E_{ijk}, determined under Step 2. The Freeman-Tukey residuals (4), Z_{ijk}, are then calculated for all the cells of the contingency table:

$$Z_{ijk} = (X_{ijk})^{1/2} + (X_{ijk} + 1)^{1/2} - (4E_{ijk} + 1)^{1/2}$$

Those cells with Z_{ijk} greater than +1.5 are considered to be significantly overrepresented; that is, the observed accident counts are significantly higher than expected frequency based on the countywide average. The value of +1.5 is chosen arbitrarily and can be changed as appropriate. These cells are then printed out in descending order of magnitude for the Z_{ijk}'s.

4. This modeling process, as described in Steps 1 through 3, is then repeated for each of those variables that are sparse but significant. Recall that these sparse variables are tested without the last entered significant variable. Thus, the last entered significant variable is also excluded in the modeling process for the sparse variables.

Program Output

The output from the program is illustrated using a study site in San Antonio, Texas. The study site is on a six-lane divided U.S. highway with full access control. The end points of the study site have been adjusted to coincide with interchanges, and the total section length is 2.4 mi. A total of 254 accidents were reported at this site in the 3-year period from 1980 to 1982. Results from the variable selection process are as follows:

Selected	Sparse	Rejected
	Primary Variables	
Degree of Curve	Accident Type	Vehicle Type
Weather/Surface Condition		
Accident Time		
	Secondary Variables	
Speeding	DWI Involvement	Accident Severity
	Driver License Status	Driver Age

The results obtained from the modeling process are summarized in Figure 2. The first four variables (from left to right): degree of curve, weather/surface condition, accident time, and speeding, are those identified as statistically significant on accident overrepresentation and selected by the variable selection algorithm. These significant variables were analyzed first.

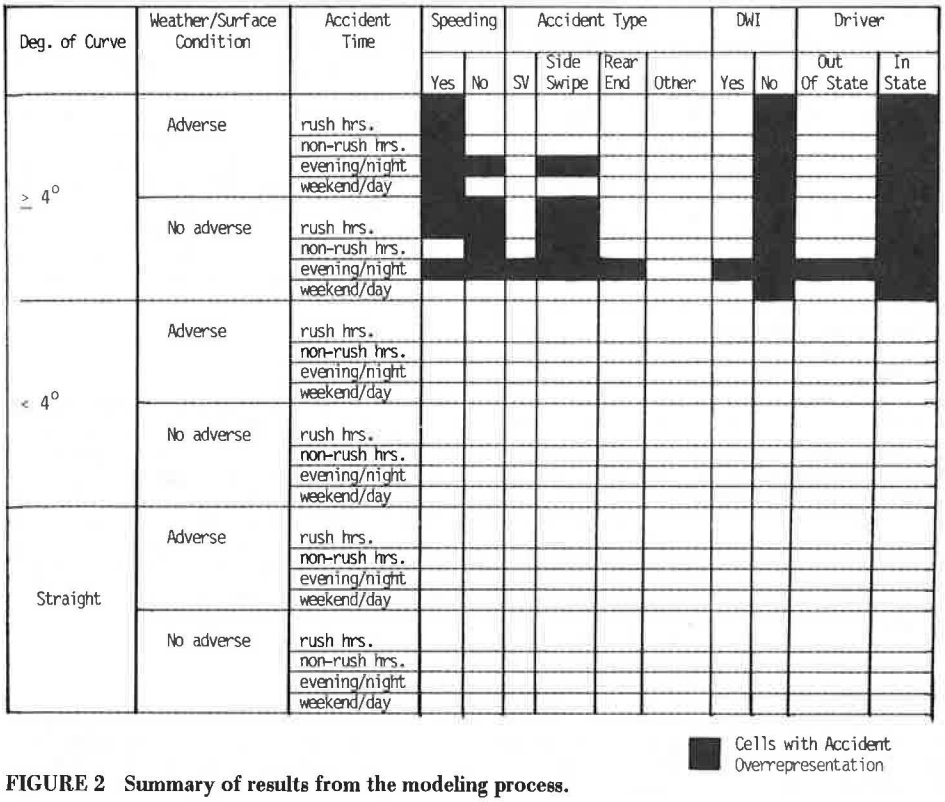

FIGURE 2 Summary of results from the modeling process.

The analysis was then repeated for each of those variables that are sparse but significant by replacing the significant variable that was selected last (i.e., speeding) with one of the sparse but significant variables. For example, accident type replaced speeding as the fourth variable and the analysis was repeated for the following variables: degree of curve, weather/surface condition, accident time, and accident type.

The analysis results indicate the following factors as causes of accident overrepresentation at this site relative to the average for the county:

1. Curve section with curvature greater than 2 degrees;
2. Combination of adverse (wet) weather/surface condition speeding on curve section;
3. Accidents are overrepresented during the time period of evening and night on curve section; and
4. Single vehicle accidents, especially those involving median barriers and rollovers, are overrepresented in the evening or at night on curve section as are sideswipes.

The accident analysis results were then combined with field observations and engineering studies to determine accident causative factors and applicable remedial countermeasures.

FIELD EVALUATION

It should be borne in mind that the results from the MAAP program are only indications of accident factors and combinations of factors that are significantly overrepresented at the location under evaluation. The program cannot and should not replace detailed field studies and sound engineering judgment in the effort to determine potential causative factors and possible remedial measures.

A multidisciplinary team approach is used for the field evaluation. The multidisciplinary team consists of an accident analyst, a traffic engineer, and an analyst with human factors or law enforcement expertise, or both, to provide a broad spectrum of expertise to the evaluation process. Results from the MAAP program and other available information, such as as-built plans, traffic counts, and so forth, are first analyzed to identify potential accident causative factors and remedial measures. The team then visits the location under evaluation to observe and assess the physical and traffic characteristics at the site and to identify potential problem areas and appropriate remedial measures. The site is also videotaped for future reference and further evaluation in the office.

Again using the San Antonio site as an illustrative example, the results of the accident analysis suggest that sharp horizontal curves, low skid resistance, speeding, and night visibility, are candidate accident causative factors. A review of the as-built plans and site visits confirm these potential problem areas.

Because of restrictions in available right-of-way and environmental impact concerns, the design speed of the highway was reduced from the typical 70 mph to 50 mph for the highway section under evaluation. Several sharp horizontal curves are present in the section, with high degrees of curvature. The curve at the beginning of the section is particularly troublesome. First, it is at the end of a long straight section with a downgrade approach. Also, it is a compound curve and the apex of the curve is not evident from the straight approach. Unfamiliar drivers could easily misjudge the sharpness of the curve and fail to respond properly.

Despite a reduction from 55 to 50 mph in the speed limit, speeding appears to be a problem at the site with a median speed of approximately 60 mph. Drivers are actually accelerating when they enter the curve because of the downgrade approach.

The concrete pavement surface is polished, but not slick. Also, the pavement surface is grooved and

the drainage appears good. However, under adverse weather or surface conditions, the demand for skid resistance may be fairly high at the sharp horizontal curves.

Night visibility at the site does not appear to be a problem. The section is lighted and well-delineated with raised pavement markers. Chevron panels have been erected on top of the concrete median barrier to better delineate the curve. Overrepresentation of accidents during evenings and nights may be attributable to other factors, such as increase in speed, alcohol involvement, and so forth.

After conferring with the SDHPT district personnel, a number of remedial measures have been implemented or planned for the site. First, an overhead warning sign with flashing beacons and accompanying advance curve warning sign were installed at the problem curve to forewarn drivers of the curve. The pavement surface was recently rotomilled to increase skid resistance and to improve drainage. Another planned countermeasure is the installation of transverse striping in an attempt to reduce the speed of traffic before it enters the curve. The effectiveness of these countermeasures will be evaluated as they are implemented.

Increased law enforcement at the site was also considered, but not implemented. Previous efforts in increased law enforcement at the site resulted in only temporary improvements. Also, the city police department has limited resources in terms of funding and manpower, and speed enforcement is not necessarily a high priority item. The Selective Traffic Enforcement Program (STEP) would be a good source of funding for this type of activity, but, unfortunately, the city does not participate in this program.

SUMMARY

Two computer programs developed by TTI for the Texas SDHPT have been reported in this paper. The first program, known as the WINDOW program, is designed for use on mainframe computers to identify and rank high-accident locations. This program has been fully operational for some time. An effort is currently underway to incorporate several minor changes into the program to improve its capabilities and flexibility.

The microcomputer program, MAAP, is being field tested with a small number of sites in Fort Worth, Houston, and San Antonio, Texas. A number of improvements are planned for the program and other changes may be identified from the field tests. Most of the planned improvements are in the areas of program output and reporting in an effort to make the program more user-friendly or to improve on the execution time. It is anticipated that the program will be ready for field operation some time in 1987.

Analysis results from these computer programs are then used with field evaluation and sound engineering judgment to determine candidate accident causative factors and remedial measures. This entire process provides a systematic and efficient means of analysis and evaluation in the effort to improve safety at identified high-accident locations.

ACKNOWLEDGMENTS

The authors gratefully acknowledge the support and assistance of the Texas State Department of Highways and Public Transportation and the guidance and direction of Herman Haenel, project manager for the study. Thanks are also due to numerous TTI staff, especially Robert Streckfus, Rebecca Yette, Edward Wever, Stephen Chan, and Mark Andrews, for their contributions to the programming and debugging of the computer programs at various stages.

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Publication of this paper sponsored by Committee on Traffic Records and Accident Analysis.

Alcohol Involvement in Texas Driver Fatalities: Accident Reports Versus Blood Alcohol Concentration

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ABSTRACT

The extent of alcohol involvement among driver fatalities has been difficult to estimate from subjective, nonquantitative sources such as accident reports. Compared in this paper are estimates from two data sources of the proportion of driver fatalities in which the driver is legally intoxicated: accident reports and toxicological reports [i.e., reports of blood alcohol concentration (BAC)]. On the basis of 1,260 driver fatalities in Texas for which BAC test results were available, 51 percent of the drivers were legally intoxicated as defined by a BAC greater than or equal to 0.10 percent blood alcohol by volume. Accident reports for these same driver fatalities reported alcohol as a contributing factor in the accident in only 20 percent of the fatalities. Of the legally intoxicated driver fatalities identified by the BAC tests, 68 percent of the corresponding accident reports did not indicate alcohol as a contributing factor in the accident. Descriptive statistics based on BAC results by age and sex of the driver and time and date of the accident are reported. The underreporting rate of alcohol involvement is also described by age and sex of the fatally injured driver and by investigating officer [i.e., local police versus department of public safety (DPS)]. The findings emphasize the need for better quality data on alcohol involvement in traffic accidents.

Although alcohol involvement in fatal accidents has been shown to be related to driver blood alcohol level [it has been estimated that more than 50 percent of all fatal collisions involve alcohol (1)], the extent of this involvement has been difficult to quantify. Previous studies that have relied on accident report data have been criticized for potentially low estimates of actual alcohol involvement (2). These studies have relied on data that are basically subjective, that is, the police officer's assessment of whether or not alcohol was involved. Further, these assessments suffer because they are categorical (yes or no) and may lack consistency as a result of the format of the accident report. Also, a fear of involvement in civil suits may result in reluctance on the part of the investigating officer to cite alcohol as a contributing factor unless a driving while intoxicated (DWI) charge is filed.

A highly reliable estimate of the extent of alcohol involvement can be obtained from blood alcohol concentration (BAC) measured on the drivers of all vehicles involved in accidents. This variable is nonsubjective and quantitative. Unfortunately, this information is not easily attainable. It is seldom available for the driver who survives the accident and it is not always available for the fatally injured driver. Several reasons for the lack of BAC data on fatally injured drivers are (a) lack of legislation requiring a postmortem BAC test on fatally injured drivers, (b) lack of facilities and medical examiners to perform BAC tests, or (c) an excessive amount of time having elapsed between the time of the accident and the time of death or autopsy.

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In the United States, 35 states currently have laws that require postmortem BAC tests to be conducted on drivers who are fatally injured in motor vehicle accidents (3). Table 1 gives some general information on the number of drivers tested for blood alcohol content and the results of these tests for selected states, as well as a nationwide estimate. Two of the states listed in Table 1, New Jersey and Rhode Island, require postmortem BAC tests, whereas in Virginia and Maine the tests are often performed, but are not mandatory. Even in those states where such tests are required, testing falls short of 100 percent; however, the percentage of drivers tested in each state is higher than the national rate. Therefore, the data from these states should reflect a much more realistic picture of the extent of alcohol involvement in traffic fatalities.

The higher percentages of positive BAC test results for the selected states in Table 1 also are supported by Fatal Accident Reporting System (4) data on driver fatalities in 15 states that routinely perform BAC tests. It is estimated that 80 to 90 percent of all fatally injured drivers in these states are tested, and the results show that in 1982, 48 percent had a blood alcohol concentration of 0.10 percent or more.

The Texas Department of Public Safety (DPS) recognizes that driving-while-intoxicated fatalities may indeed be underreported; the department published the following statement in its 1982 annual report, Motor Vehicle Traffic Accidents (5):

> Accidents in which DWI was reported as a factor do not tell the whole story. As Texas has no law requiring chemical tests on drivers in fatal accidents, it is possible that many injured or deceased drivers who were driving while intoxicated were not reported as DWI.

TABLE 1 Results of BAC Tests Nationwide and in Selected States for 1982 (*4-8*)

	United States	Percent	Virginia	Percent	New Jersey	Percent	Maine	Percent	Rhode Island	Percent
Total number of driver fatalities	24,690		500		524		109		63	
Total number of drivers tested	16,050	65	425	85	453	86	83	76	61	97
Test results										
Positive BAC (≥0.10)	6,907	43	195	46	270	60	43	52	30	49
Negative BAC (<0.10)	6,489	40	222	52	183	40	40	48	31	51
Unknown	2,654	17	8	2	0	0	0	0	0	0

Source: New Jersey State Police Accident Unit.

Although Texas does not perform BAC tests by statute, 10 counties (Bexar, Dallas, El Paso, Galveston, Harris, Johnson, Nueces, Tarrant, Travis, and Wichita) in the state do employ full-time medical examiners who routinely investigate all fatalities, including traffic fatalities. Although these 10 counties constitute a small percentage of the total area of the state of Texas (3.5 percent), they represent more than one-half of the state's population (51.8 percent) and contain all of the major cities in the state. They also represent 37 percent of the state's driver fatalities for 1983.

A comparison of BAC tests results and reporting practices in other states, especially those states that require BAC tests, would provide extremely useful data; however, such studies have been virtually nonexistent. Thus the main purpose of this study is to compare medical examiners' findings with the investigating officers' conclusion of alcohol as a contributing factor according to the accident reports for 1,260 fatally injured drivers. Results of these comparisons are used to estimate the underreporting of accidents in which alcohol is a contributing factor. However, this study is descriptive rather than inferential in nature. No attempt is made to generalize the results based on this sample of fatally injured drivers to total fatal accidents or alcohol-involved accidents in general.

METHODOLOGY

Information from two major data sources was merged to provide the necessary information for matching BAC with the accident report data. BAC is measured in percentage terms by volume; that is, a BAC of 0.10 means one tenth of one percent alcohol in the blood by volume. A driver whose BAC is greater than or equal to 0.10 is considered legally intoxicated or is said to be driving while intoxicated.

BAC data were obtained from 9 of the 10 medical examiners in the counties that routinely perform postmortem BAC tests. These data contained minimal information concerning the accident itself and identified fatally injured victims by case number, name, age, and sex. The only accident information generally available was the date, and sometimes the time, of the accident.

Extensive accident information regarding accident, occupant, and vehicle characteristics was available from the Texas accident data file originally supplied by the Texas Department of Public Safety. Records in this file do not, however, provide the name of the fatally injured person. After matching the age, sex, and date of accidents from the medical examiners' records to the same information on the accident records, the BAC for each case could be compared to the entry in the corresponding accident report that cited whether or not, in the opinion of the investigating officer, alcohol was believed to have been a contributing factor in the accident.

The BAC data were obtained manually from the medical examiners' files. In general, the data collection involved a two-stage procedure. First, driver fatalities were identified from hand-recorded log books maintained at the respective county morgues. All information on the data collection form, except BAC, was recorded. The case number obtained from the log book was then matched with the files maintained in the medical examiner's toxicology lab, and the BAC test results were recorded.

In the nine counties for which BAC test results were available, data were collected on all driver fatalities occurring between January 1, 1983 and December 31, 1984. (Note: Some counties also submitted data for previous years, namely: Johnson and Travis for 1981, and Johnson, Travis, Tarrant, and Wichita for 1982. The 42 matched records for these years are included in this study, as well. Because of the descriptive nature of the analysis, the inclusion of these additional records is not believed to bias the sample.)

COMPARISON OF BAC TEST RESULTS AND ACCIDENT REPORTING PRACTICES

Table 2 gives the distribution of the 1,260 fatally injured drivers for which BAC test results were matched with the reporting officer's assessment of whether or not alcohol use (on the part of the deceased) contributed to the accident. Because a BAC test result of 0.10 percent or more is considered legal intoxication, BAC results were grouped as less than 0.10 percent (within the legal limit) and greater than or equal to 0.10 percent (legally intoxicated, or DWI) by volume.

Two categories of potential disagreement exist between alcohol involvement as reported on the accident report and the BAC test result. These are represented by the off-diagonal cells in Table 2. One category of disagreement contains those cases in which the driver's BAC did not exceed the legal limit, yet alcohol was cited as a contributing factor in the accident. The second category contains those cases in which the driver was DWI, yet alcohol was not cited as a contributing factor in the accident. This second category represents the larger percentage of disagreement between these sources. In Table 2, only 7 percent of the 618 drivers whose BAC did not exceed the legal limit had alcohol cited as a contributing factor in their accidents. The accident reports indicate that only 249 (20 percent) of the drivers were alcohol impaired, whereas in actuality 642 (51 percent) of the fatally injured drivers were DWI.

A statistical test of the hypothesis that the proportion of alcohol-involved driver fatalities from accident reports is equal to the proportion of DWI drivers based on BAC test results is rejected ($p < .0001$). The statistical test used was the test of equality of two proportions using the normal approximation to the binomial (Z-test).

TABLE 2 BAC Test Results by Alcohol as a Contributing Factor

	Alcohol Contributing Factor		
BAC Test	No	Yes	Total
BAC <0.10	573 (0.93)	45 (0.07)	618 (0.49)
BAC ≥0.10	438 (0.68)	204 (0.32)	642 (0.51)
Total	1,011 (0.80)	249 (0.20)	1,260

Based on these results, it can be expected that of every 100 fatally injured DWI drivers, only 32 of their accident reports will show alcohol as a contributing factor. Of every 100 accidents in which the driver was fatally injured, only 20 of those accident reports will show alcohol involvement when, in actuality, 51 of the drivers in these fatal accidents were DWI.

DESCRIPTIVE PROFILE OF DRIVERS AND ACCIDENTS ACCORDING TO BAC

An analysis of the 1,260 driver fatalities also revealed that the average BAC value was 0.114 percent. In other words, the average level of blood alcohol content of the total sample of fatally injured drivers exceeded the legal limit of 0.10 percent. The average BAC for those driver fatalities in which the driver had a BAC in excess of 0.10 percent was 0.211, twice the legal intoxication limit. Eighty percent (1,011) of the 1,260 accident reports did not cite alcohol as a contributing factor; however, the average BAC value for those 1,011 drivers was 0.096 percent, which is extremely close to the legal intoxication value.

Driver Age

Inspection of the BAC value by age group (Table 3) revealed that the category with the highest average BAC value (0.135) was the 26 to 30 year old age group. Only three of the age groups had average BAC values below the legal limit: those drivers aged 65 and over (0.021), those 18 and under (0.063), and those in the 41 to 64 year age group (0.095).

TABLE 3 Age Distribution of Driver Fatalities

Age	Driver Fatalities	Mean BAC	Proportion of BAC ≥0.10	Mean of BAC ≥0.10
≤15	57	0.063	0.28	0.194
19-20	125	0.109	0.52	0.191
21-25	309	0.124	0.61	0.194
26-30	242	0.135	0.58	0.222
31-40	254	0.129	0.55	0.225
41-64	207	0.095	0.40	0.224
≥65	59	0.021	0.10	0.199
Unknown	7	—	—	—

The 21 to 25 year olds had the highest proportion of fatalities involving legally intoxicated drivers (61 percent), followed by the 26 to 30 year olds (58 percent). Although the 65 and over age group had the lowest average BAC value and the smallest proportion of fatalities involving legally intoxicated drivers, they still had an average BAC of 0.199 for those persons who were DWI--almost twice the legal limit.

Sex of Driver

A significantly higher proportion of male driver fatalities than female had BAC values above the 0.10 percent limit. For males, 55 percent of their driver fatalities were the result of DWI, compared with 32 percent of all female driver fatalities (Table 4). The average BAC values were 12 percent among males and 7 percent among females.

TABLE 4 BAC Test Results by Sex of Driver Fatality

BAC Test	Male	Female	Total
BAC <0.10	455 (0.45)	160 (0.68)	615 (0.49)
BAC ≥0.10	563 (0.55)	77 (0.32)	640 (0.51)
Total	1,018 (0.81)	237 (0.19)	1,244[a]

[a]Sex of driver was unrecorded on 16 accident reports for which BAC test results were available.

Time of Day

Legally intoxicated drivers account for a significant proportion of total driver fatalities between the hours of 11:00 p.m. and 3:00 a.m. Figures 1 and 2 show the hourly fatal accident frequencies for drivers with BACs less than 0.10 percent and greater than or equal to 0.10 percent, respectively. Note that the hourly distributions are fairly uniform for BAC values of less than 0.10 percent, indicating that the probability of a fatal accident occurring at any given hour is the same among driver fatalities in which the driver was not legally intoxicated. However, the proportion of fatalities among DWI drivers is substantially higher between the hours of 11:00 p.m. and 3:00 a.m. when 70 percent of the drivers in these accidents had BAC values in excess of 0.10 percent. These results are also consistent with the national trend reported in the 1982 Fatal Accident Reporting System overview (4).

Day of Week

More than 50 percent of the fatally injured drivers involved in accidents on Sunday, Saturday, and Thursday were DWI (see Table 5). Although the highest average BAC value occurred on Sunday (0.13), it is interesting that the average values were above the legal limit on all days except Monday (0.09).

UNDERREPORTING OF ALCOHOL INVOLVEMENT BY SELECTED VARIABLES

Driver Age

Table 6 gives a summary of the findings of the comparison of BAC values with reported alcohol involvement by age. The column labeled "Percent Alcohol Involvement" is the percentage of driver fatalities in that age group whose accident reports list alcohol involvement. These percentages are noteworthy because they are the figures most often used in policy decision making and reported in accident statistics. However, the column labeled "Percent BAC ≥ 0.10" is the actual percentage of DWI driver fatalities in that age group, based on BAC test results. These percentages represent the true extent of alcohol involvement in this data set.

The discrepancies between these two sets of figures are quite large (Table 6). For example, only 23

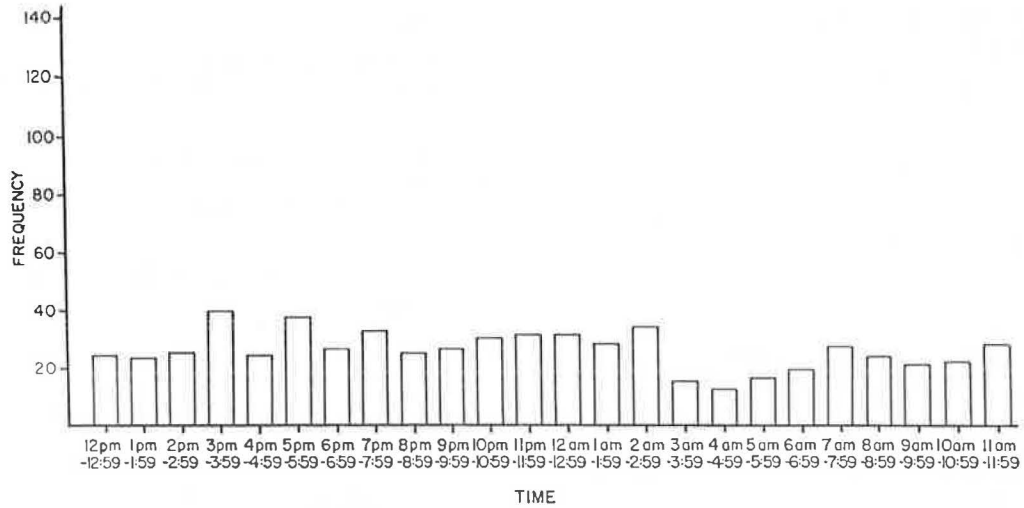

FIGURE 1 Non-DWI driver fatalities by time of day.

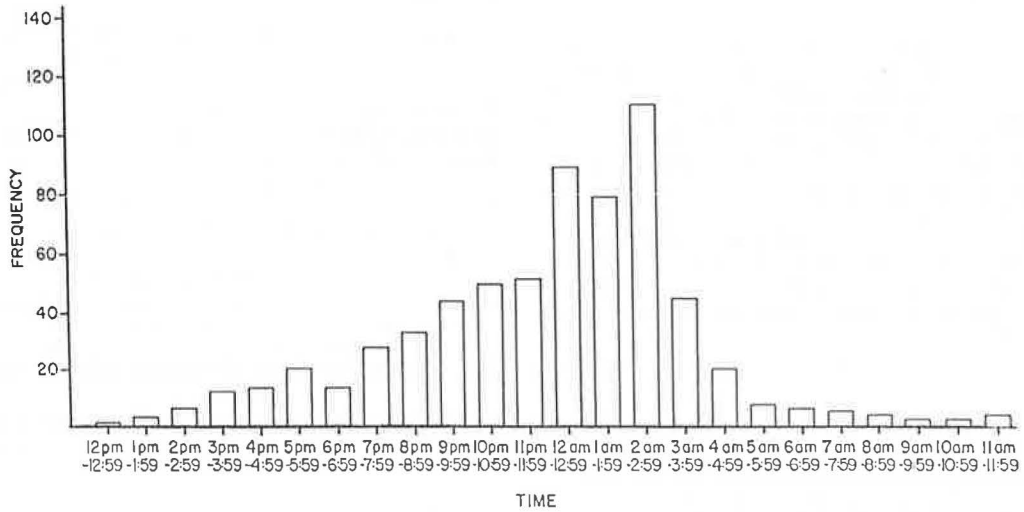

FIGURE 2 DWI driver fatalities by time of day.

TABLE 5 Driver Fatalities by Day of Week

Day	Total	Percent BAC ≥0.10	Average BAC
Sunday	177	59	0.13
Monday	161	42	0.09
Tuesday	163	45	0.10
Wednesday	145	49	0.11
Thursday	141	53	0.12
Friday	211	49	0.11
Saturday	256	56	0.12
Unknown	6	–	–

TABLE 6 BAC Test Results and Reported Alcohol Involvement by Age of Driver

Age	Percent Reported Alcohol Involvement	Percent BAC ≥0.10
≤18	14.0	28.1
19-20	23.2	52.0
21-25	22.0	60.5
26-30	24.4	57.9
31-40	18.9	55.1
41-64	15.9	40.1
≥65	3.4	10.2

percent of the 125 driver fatalities in the 19 to 20 year age group had alcohol cited as a contributing factor on the accident reports, whereas in actuality, 52 percent of these drivers were DWI. In the 21 to 25 year age group, only 68 of the 309 fatally injured drivers (22 percent) had reports in which alcohol was cited as a contributing factor, but 61 percent (187 drivers) were found to be legally intoxicated.

Underreporting rates did not vary significantly by either the age or the sex of the fatally injured drivers.

Investigating Officer

As can be seen in Table 7, a large difference in the underreporting rate existed between local police officers and the Texas Department of Public Safety officers. DPS officers failed to cite alcohol as a contributing factor for 30 percent of the DWI drivers, whereas local police failed to note alcohol involvement on the accident reports of 76 percent of the DWI drivers.

Conversely, DPS had a higher overreporting rate than did the local police officers. DPS officers cited alcohol as a contributing factor for 15 of the

TABLE 7 BAC Test Results by Alcohol as a Contributing Factor for Accidents Investigated by DPS Officers Versus Local Police Officers

	Alcohol Contributing Factor		
BAC Test	No	Yes	Total
DPS Officers			
BAC <0.10	75 (0.83)	15 (0.17)	90 (0.46)
BAC ≥0.10	32 (0.30)	75 (0.70)	107 (0.54)
Total	107 (0.54)	90 (0.46)	197
Local Police Officers			
BAC <0.10	498 (0.94)	30 (0.06)	528 (0.50)
BAC ≥0.10	406 (0.76)	129 (0.24)	535 (0.50)
Total	904 (0.85)	159 (0.15)	1,063

90 (17 percent) fatally injured drivers whose BACs did not exceed the legal intoxication limit. Local police officers, on the other hand, noted alcohol involvement for non-DWI drivers only 30 times out of 528 (6 percent). This finding suggests that DPS officers may be more willing than their local police counterparts to report alcohol as a contributing factor in a driver fatality. This conclusion is also supported by the statistic that whereas DPS officers cited alcohol as a contributing factor in 46 percent of the driver fatality accidents they investigated, local police officers noted alcohol as a contributing factor in only 15 percent of their corresponding accident reports.

ESTIMATION OF THE DEGREE OF ALCOHOL INVOLVEMENT IN DRIVER FATALITIES

Estimation of the degree of alcohol involvement in driver fatalities is a difficult problem. In order to obtain this estimate, it is necessary to obtain alcohol content information on all drivers involved in an accident resulting in a fatality, not just driver fatalities. Unfortunately, this information is not generally available. The nonfatal driver is seldom tested for alcohol content, and even if tested, the results are not easily attainable.

Attempting to estimate the degree of alcohol involvement in driver fatalities by using only information based on the blood alcohol content of driver fatalities is difficult because of the amount of missing data and the few counties in which BAC tests are performed. Although an exact estimate of the proportion of driver fatalities in which the driver is legally intoxicated is not available because of these problems, upper and lower bound estimates are available based on the reported data. Of the 781 driver fatalities reported in the 10 counties in 1983, at least 260 had BACs equal to or in excess of the legal limit of 0.10, that is, at least 33 percent of the drivers were legally intoxicated. If all of the driver fatalities for which BAC results are unknown were legally intoxicated, this proportion could be as high as 68 percent. This may be an unrealistic upper bound; however, because the true proportion of driver fatalities that involved alcohol lies within this wide range the need for mandatory BAC test requirements is emphasized to enable a more precise estimation of the extent of the alcohol problem. Based on the 516 BAC test results for 1983, 50.4 percent of the driver fatalities tested for BAC were legally intoxicated. Whether this estimate of the true proportion is biased upward or downward cannot be determined from the data in this study, but it is known that the true proportion cannot exceed 68 percent or be less than 33 percent of all driver fatalities in the 10 counties studied.

CONCLUSIONS

The extent of alcohol involvement in driver fatalities is not adequately represented by the reporting of alcohol as a contributing factor on accident reports. The actual percentage of DWI drivers among the 1,260 driver fatalities examined in this study was considerably higher than the percentage of accident reports that cited alcohol as a contributing factor in those same accidents (51 percent versus 20 percent, respectively). Among the DWI driver fatalities, 68 percent of the accident reports did not cite alcohol as a contributing factor in the accident.

BAC test results appear to be the only data source for accurately estimating the proportion of DWI driver fatalities. This information exists for only a small proportion of driver fatalities in Texas (37 percent). An accurate statewide estimate of the number of DWI driver fatalities will not be possible until more data on BAC test results are available throughout the state.

It is hoped that the results of this study emphasize the need for more complete information on alcohol involvement than is currently available. Such information is essential for effective countermeasure evaluation of highway safety programs. For example, the fact that legally intoxicated drivers account for a significant proportion of total driver fatalities between the nighttime hours of 11:00 p.m. and 3:00 a.m. suggests that the nighttime driver population may consist of a larger proportion of DWI drivers than the daytime driving population. If this is true, the evaluation of any countermeasure based on nighttime accident frequencies risks being confounded with the DWI problem. Reliable and complete BAC test results could provide information that would be useful in evaluating such countermeasures independent of the effect of alcohol as a contributing factor.

The only way to measure the full extent of drunk driving and the effect of various countermeasures designed to reduce alcohol-related accidents is to record a BAC for every driver involved in a traffic accident or killed in one. Even further insights into the economic and social costs of drinking and driving would be gained if BAC data for fatally injured passengers and pedestrians could be obtained. The availability of such data would greatly facilitate the development of reliable and valid estimates of the extent of alcohol involvement at the state and national levels.

ACKNOWLEDGMENTS

The research for this paper was sponsored by the Texas State Department of Highways and Public Safety.

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Publication of this paper sponsored by Committee on Alcohol, Other Drugs and Transportation.

Problems of Combination Trucks on Wet Pavements: An Accident Analysis

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ABSTRACT

A study of wet-pavement truck accidents was carried out for over-the-road trucks authorized by the Interstate Commerce Commission (ICC). The study was based on accident data from the Bureau of Motor Carriers Safety (BMCS) for 1979 through 1981. The analysis was limited to truck accident involvements on four-or-more-lane highways in Texas. Discrete-multivariate methods were used for the analysis. The analysis indicates that empty trucks show up to three times higher propensity for single-truck accident involvements (run-off-road, jackknife, overturn, and separation of units) on wet pavements than do loaded trucks. The ratios of wet-pavement to dry-pavement accident involvements were found to be influenced by the following factors: empty/loaded, truck type, and accident type, but not by day/night. The ratio of single-truck accident involvements on wet pavements to those on dry pavements was found to be much higher for empty trucks than for loaded trucks, after adjusting for truck type. Heavy-truck involvements in multivehicle collisions were used as a comparison group. These findings appear to strongly support the prediction by Horne and the laboratory study conducted by Ivey, that truck tires can hydroplane at highway speeds when the trucks are empty or lightly loaded.

The purpose of this paper is to identify possible causes of combination-truck accidents that result from loss of control. In particular, an in-depth analysis of past accident experience of empty combination trucks in wet conditions will be carried out. The data source for this investigation is the Bureau of Motor Carriers Safety (BMCS) file for the Interstate Commerce Commission [(ICC) authorized)] carriers.

INTRODUCTION

Combination truck accidents that result from loss of control are complex phenomena. They are usually the result of failures in the system comprising vehicle, roadway, driver, visibility, and environmental characteristics, as well as chance. Although theoretical work on vehicle dynamics, laboratory simulation, and vehicle testing have greatly enhanced the knowledge about the factors that lead to lack of stability of trucks in wet conditions, past accident records of these heavy trucks have not been thoroughly analyzed to provide evidence in support of these theories.

Ivey et al. (1) reported that the following elements, independently or interactively, had been identified in past studies as possible causes of combination trucks losing control in wet conditions:

1. Low tire pavement friction,
2. Brake system characteristics,
3. Speed,
4. Reduced visibility, and
5. Hydroplaning.

Loss of control of combination trucks may result in reported accidents such as jackknife, overturn, run-off-road, and separation of units. These four

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types of accidents are collectively referred to in this paper as single-truck accidents.

It was not until recently that dynamic hydroplaning was believed to contribute to loss of control of lightly loaded combination trucks (2). The accident analysis presented here will systematically identify factors that affect the probability of single-truck accidents in general first. Then an in-depth analysis of truck accident records will be performed to determine and to compare single-truck accident propensity on wet pavements for empty trucks and for loaded trucks. In this way, past accident experience of these trucks may be used to provide supporting evidence (or otherwise) for the hydroplaning hypothesis of Horne (2).

LITERATURE REVIEW

Horne (2) was the first to predict that, contrary to conventional wisdom, truck tires were subject to dynamic hydroplaning at highway speeds when empty or lightly loaded. A verification of Horne's prediction was carried out by Ivey (1), who used a test trailer in simulated highway environments and recorded the speeds at which the tires began to spin down. In reporting these results in support of Horne's prediction, Ivey also explained the following:

> In the early 1960s, Horne and his fellow engineers in NASA discovered and studied the phenomenon of hydroplaning as it related to aircraft tires. Because of the way aircraft tires are constructed, the shape of the contact patch (that portion of the tire actually in contact with the ground) remains much the same for a fairly wide variation of tire load. The NASA group found that one could predict hydroplaning speed as a simple function of tire pressure. This relationship predicted hydroplaning speeds of tires with 60 to 100 psi inflation pressure well above what could be achieved by highway vehicles. Since truck tires normally required pressures in this range, it was felt that they would not be subjected to speeds high enough to hydroplane. Further work in the late 1960s on automobile tires confirmed that hydroplaning speeds would be extremely high at high levels of tire pressure. These studies of automobile tires, including testing by A.J. Stocker, B.M. Gallaway, and D.L. Ivey at TTI, pointed to tire loads as being an unimportant variable. The following was not appreciated. While an automobile tire for a 4,000 lb vehicle may have a normal range of loads from 800 to 1,200 lb, a truck tire may be operated with loads varying from 600 to 6,000 lbs. With this extremely wide load variation, the aspect ratio of a truck tire surface contact zones varies spectacularly, leading to hydroplaning conditions for a lightly loaded, albeit normally inflated, truck tire at speeds common to highway vehicles. The aspect ratio is the ratio of the surface contact zone width to length.

A recent study by Chira-Chavala (3), based on analyses of accident data for combination trucks, revealed that for empty trucks on rural highways the proportion of total truck accident involvements that were single-truck (as opposed to collisions with at least another vehicle) substantially increased in wet conditions (up to three times of that on dry pavements). However, the single-truck accident proportion for loaded van, flatbed, and tanker semitrailers in wet conditions was only 1.5 times or less of that in dry conditions.

CONCEPTUAL BASIS FOR ACCIDENT ANALYSIS

The analysis of accident data consists of two parts: (a) a preliminary analysis of factors influencing the types of truck accident involvements (i.e., single-truck or multivehicle accidents) in general and (b) an in-depth analysis of single-truck accident propensity on wet pavements for empty trucks and for loaded trucks. The preliminary analysis is required for the following reasons:

1. It provides a quick screening to determine whether the subsequent in-depth analysis is warranted. To be warranted, the preliminary analysis should indicate that pavement condition (wet or dry) and empty/loaded, were among the significant variables influencing the probability of single-truck accidents.
2. The propensity for single-truck accidents on wet pavements may be influenced by a number of other factors. The preliminary analysis will serve as a variable selection step to determine which significant variables, out of a large number of potential variables, are to be included in the in-depth analysis. In this way, a multivariate analysis can be effectively conducted without serious sample size problems, which may have arisen otherwise.

PRELIMINARY DATA ANALYSIS OF ACCIDENT TYPES

Truck accident involvements can be categorized as one of the following accident types:

1. Noncollision,
2. Collision with fixed object,
3. Collision with passenger vehicle, and
4. Collision with large commercial vehicle.

According to the BMCS, about 25 percent of the truck accident involvements reported annually were noncollisions, 10 percent were collisions with fixed objects, 45 percent were collisions with passenger vehicles, 15 percent were collisions with large commercial vehicles, and 5 percent were other accident types. For the noncollisions, about 90 percent were reported as run-off-road, jackknife, overturn, or separation of units.

Given that a combination truck is involved in an accident, the probability that it will be a noncollision accident, a fixed-object collision, or a multivehicle collision is likely to be influenced by factors such as vehicle, operational, driver, roadway, and environmental characteristics. Such a probability can be expressed as

$$P\,[\text{A specific accident type} \mid \text{An involvement}] = f\,(\text{vehicle, operation, driver, road, environment})$$

To identify those significant variables that influence this probability, and to discard those nonsignificant variables, the 1981 BMCS data for all ICC-authorized truck accident involvements were analyzed. Sixteen potential variables were initially examined. These variables and their levels are given in Table 1.

The procedure to determine the significant variables of accident types was based on the tests developed by Landis et al. (4) using two measures of association for contingency-table analyses: Q_{CMH} and Q_T. This procedure had been applied in a re-

TABLE 1 Potential Variables for Analysis of Accident Types

Variable	Level
Vehicle configuration	Single-unit, single, double
Trailer style	Van, flatbed, tanker
Number of axles of power unit	Two- or three-(tandem) axle
Load status	Empty, loaded
Gross vehicle weight	
Trip length	Over-the-road, local
Cargo type	General cargo, other
Road class	Undivided rural, divided rural, urban roads
Road surface condition	Dry, wet
Ramps	Yes, no
Day/night	Day, night
Weather	Clear, rain or snow
Driver experience	≤1 year, 2-4 years, 4+ years
Driver age	18-30, 31-45, 45+
Hours on duty	<2 hours, 2-5 hours, 5+ hours
Region of the country	Northeast, north, south

cent study concerning accident severity of combination-truck accidents (5). Only the result of the variable-selection analysis is reported here.

Of the 16 variables considered, those found to be significant were

1. Trip length
2. Road class
3. Dry/wet pavements
4. Ramps
5. Empty/loaded
6. Day/night
7. Driver experience
8. Driver age
9. Vehicle configuration
10. Trailer body style

As expected, wet/dry pavements and empty/loaded were among the significant variables identified by the variable-selection analysis. The subsequent in-depth analyses will determine single-truck accident propensity on wet pavements and the factors that affect this propensity.

ANALYSIS OF SINGLE-TRUCK ACCIDENT PROPENSITY ON WET PAVEMENTS

This analysis is aimed at determining single-truck accident propensity on wet pavements, particularly that which may be attributable to dynamic hydroplaning of truck tires. Specifically, single-truck accident propensity on wet pavements for empty trucks and for loaded trucks will be determined and compared. To this end, the BMCS-reported accidents involving at least one combination truck on four-or-more-lane highways in Texas were analyzed. The analysis was also restricted to the reported accidents involving ICC-authorized trucks in over-the-road service. This restriction was a result of the relatively high undercoverage of the BMCS-reported accidents involving private carriers.

For the accident data to be supportive of the hydroplaning theory by Horne (2), one would expect to see a significantly higher ratio of single-truck accidents (i.e., run-off-road, jackknife, overturn, and separation of units) on wet pavements to those on dry pavements for empty trucks than for loaded trucks. To ensure that this higher ratio was not an artifact of the truck exposure (e.g., empty trucks happened to travel more in wet weather than did loaded trucks, or empty trucks tended to travel faster than did loaded trucks), heavy-truck involvements in multivehicle collisions were used as a comparison group.

All of the significant variables that were identified in the preliminary data analysis were closely examined here. Trip length, road class, and ramps were incorporated into the analysis by considering only the accident involvements of over-the-road carriers and on four-or-more-lane highways. Driver age and experience were not included because their effect on the proportion of truck accident involvements that were single-truck was relatively small (3). Furthermore, within the same truck type, their effect on single-truck accident probability was found to be similar between wet and dry pavements, as well as between empty and loaded trucks (3).

Data Source

The BMCS file contains information on accidents involving interstate motor carriers that are subject to the U.S. Department of Transportation Act of 1966 (49 U.S.C. 1655). With few exceptions, these carriers are required to report to the BMCS any accident involving their vehicles that resulted in death, injury, or property damage exceeding $2,000. Exempted are occurrences that involve any boardings and alightings from stationary vehicles, loading and unloading of cargo, or farm-to-market agricultural transportation. The accident information is reported to the BMCS by the carriers themselves on standard forms.

There are a total of 74 variables that describe the place and time of accident, events leading to the accident, accident consequences, driver and occupant characteristics, vehicle characteristics, road, and environment. More than 30,000 accident involvements are reported to the BMCS each year. Of this total, about 80 percent involve ICC-authorized carriers and the remaining 20 percent involve private or other non-ICC-authorized carriers.

Data Input

Table 2 is a contingency table of the BMCS-reported truck accident involvements for Texas between 1979 and 1981, cross-classified by wet or dry pavements (V1), empty or loaded trucks (V2), truck type (V3), day/night (V4), and accident type (V5). Five truck types were defined: (a) single-unit trucks (also included tractor-only), (b) combination trucks pulling van trailers, (c) combination trucks pulling flatbed trailers, (d) combination trucks pulling tankers, and (e) combination trucks pulling other types of trailers. The day/night variable was defined so that night included dawn, dusk, dark, and artificial light conditions. Accident type was a dichotomous variable: single-truck accidents (run-off-road, jackknife, overturn, separation of units) or multivehicle collisions involving at least one heavy truck.

Table 2 also gives two useful descriptive statistics: the cross-product ratios (τ) between wet/dry and empty/loaded and the standardized cross-product ratios (Z).

A cross-product ratio expresses the odds of wet-pavement accident involvements for empty trucks to the odds of wet-pavement accident involvements for loaded trucks, or

$$\tau = X_{11}X_{22}/X_{12}X_{21}$$

where

X_{11} = the number of wet-pavement accident involvements for empty trucks,

X_{12} = the number of dry-pavement accident involvements for empty trucks,

X_{21} = the number of wet-pavement accident involvements for loaded trucks, and
X_{22} = the number of dry-pavement accident involvements for loaded trucks.

A cross-product ratio of 1 therefore indicates that the wet-pavement-accident propensity is the same for empty trucks and for loaded trucks. A ratio higher than 1 indicates a higher likelihood of wet-pavement accident involvements for empty trucks than for loaded trucks, and vice versa.

The values of cross-product ratios alone are not usually reliable measures for comparison because of their difference in standard errors. These standard errors, in turn, are influenced by the sample size (i.e., $X_{11} + X_{12} + X_{21} + X_{22}$). Standardized cross-product ratios, which take into account the magnitude of standard errors, are usually more useful as descriptive statistics.

A standardized cross-product ratio is defined by Griffin (6) as

$$Z = \ln \tau / [(1/X_{11}) + (1/X_{12}) + (1/X_{21}) + (1/X_{22})]^{1/2}$$

A τ value of 1 corresponds to a Z value of zero. A τ value less than 1 corresponds to a negative Z value, and a τ value greater than 1 results in a positive Z value.

To obtain the significant effect of the independent variables on the single-truck accident propensity on wet pavements, the following modeling method is used.

Analysis Method

In order to analyze and compare the ratios of single-truck accidents on wet pavements with those on dry pavements for empty and for loaded trucks, a discrete-multivariate model with a control group was used. The purpose of the modeling was to account for the significant effect of truck type, day/night, and chance variation so that the true effect of empty/loaded on the ratios of wet-to-dry single-truck accident involvements could be obtained. The control group of multivehicle collisions involving at least one heavy truck was also employed in the analysis to further enhance the credibility of the results. In this way, the effect due to confounding variables would be minimized and the estimates of wet-to-dry accident ratios might then be stable.

The model can be expressed as follows:

$$\ln [p/(1 - p)] = w + w_2 + w_3 + w_4 + w_5 + w_{23} + w_{24} + \ldots$$

TABLE 2 ICC-Authorized Truck Accident Involvements on Four-or-More-Lane Highways in Texas, 1979-1981

Accident-Type (V5)	Light (V4)	Truck Type (V3)	Empty/ Loaded (V2)	Pavement Condition (V1) Wet	Pavement Condition (V1) Dry	Cross-Product Ratio (τ)	Standardized CPR (Z)
Single-truck	Day	Single unit	E	2	4	0.25	-0.92
			L	2	1		
		Van	E	42	7	3.95	3.07
			L	76	50		
		Flatbed	E	8	2	7.33	2.29
			L	12	22		
		Tanker	E	16	5	10.67	3.42
			L	6	20		
		Other	E	10	4	3.75	1.51
			L	4	6		
	Night	Single unit	E	3	1	2.00	0.47
			L	3	2		
		Van	E	33	10	3.34	3.06
			L	80	81		
		Flatbed	E	1	3	1.61	0.39
			L	6	29		
		Tanker	E	9	3	7.50	2.26
			L	4	10		
		Other	E	2	1	5.00	1.09
			L	2	5		
Multivehicle collisions	Day	Single unit	E	5	43	0.52	-0.71
			L	2	9		
		Van	E	44	99	1.32	1.30
			L	102	303		
		Flatbed	E	27	86	1.35	1.00
			L	31	133		
		Tanker	E	15	49	1.53	0.96
			L	11	55		
		Other	E	13	22	2.73	1.92
			L	8	37		
	Night	Single unit	E	13	24	1.35	0.34
			L	2	5		
		Van	E	27	59	1.52	1.60
			L	90	299		
		Flatbed	E	9	52	1.23	0.48
			L	21	149		
		Tanker	E	13	23	2.31	1.73
			L	11	45		
		Other	E	8	13	2.98	1.79
			L	7	34		

Source: BMCS 1979, 1980, 1981.

where

p = the proportion of accident involvements that occurred on wet pavements. Therefore, $(1 - p)$ is the proportion of accident involvements occurring on dry pavements;
w = the overall mean;
w_2 = the main effect of empty/loaded;
w_3 = the main effect of truck type;
w_4 = the main effect of day/night;
w_5 = the main effect of accident type;
w_{23} = the interaction between empty/loaded and truck type, and so on.

Analysis Result

The model estimation was carried out using the FUNCAT program (7). The "best" model was found to be

$$\text{Ln}\ [p/(1 - p)] = w + w_2 + w_3 + w_5 + w_{25}$$

The chi-square goodness-of-fit statistic for this model was 17.28 for 12 degrees of freedom (p-value = 0.1394), which indicates a good fit.

The estimated model indicates that the ratios of wet-pavement to dry-pavement accident involvements, $p/1 - p$, were significantly influenced by load status (empty/loaded), truck type, accident type (single-truck/multivehicle), and the interaction between load status and accident type. However, the ratios of wet-pavement to dry-pavement accident involvements were not significantly influenced by day/night. Tables 3 and 4 give the summary of the modeling results. Table 5 gives the estimated ratios of wet-pavement to dry-pavement accident involvements by truck type and empty/loaded separately for single-truck accidents and multivehicle collisions.

TABLE 4 Parameter Estimates and Standard Errors

Term	Estimate	Standard Error
W	−0.4815	0.0755
W_2	0.4445	0.0617
W_3	−0.1785	0.1819
	0.3883	0.0851
	−0.3072	0.1066
	−0.0545	0.1267
W_5	0.7905	0.0599
W_{25}	0.2169	0.0598

TABLE 3 Summary of Modeling Results

Variable	Chi-Square	Degree of Freedom	P-Values
Load status	55.16	1	0
Truck type	38.03	4	0.0001
Accident type	178.65	1	0
Load status x accident type	14.22	1	0.0002

TABLE 5 Estimated Ratios of Wet-to-Dry Accident Involvements

		Wet/Dry Ratio	
Truck Type	Load Status	Single-Truck	Collisions
Single-unit	Empty	2.21	0.29
	Loaded	0.59	0.19
Van	Empty	3.89	0.52
	Loaded	1.04	0.33
Flatbed	Empty	1.94	0.26
	Loaded	0.52	0.16
Tanker	Empty	2.50	0.33
	Loaded	0.67	0.21
Other	Empty	3.07	0.41
	Loaded	0.82	0.26

Interpretation of Modeling Results

Figures 1 (a) and (b) show the plots of the estimated ratios of wet-to-dry accident involvements for single-truck accidents and for multivehicle collisions. It can be seen that the ratios of wet-to-dry accident involvements were consistently higher for empty than for loaded trucks regardless of the accident type or the truck type. However, this difference between empty and loaded trucks was far more pronounced for single-truck accidents than for multivehicle collisions. This differential finding was the result of the interaction between load status and accident type.

To illustrate this interaction graphically, Figure 2 shows a plot of the means of the ratios of wet-to-dry accident involvements for single-truck accidents and for multivehicle collisions, weighted by appropriate accident involvement frequencies. If the effect of wet pavements was not particularly pronounced for empty trucks in single-truck accidents, the two lines representing single-truck acci-

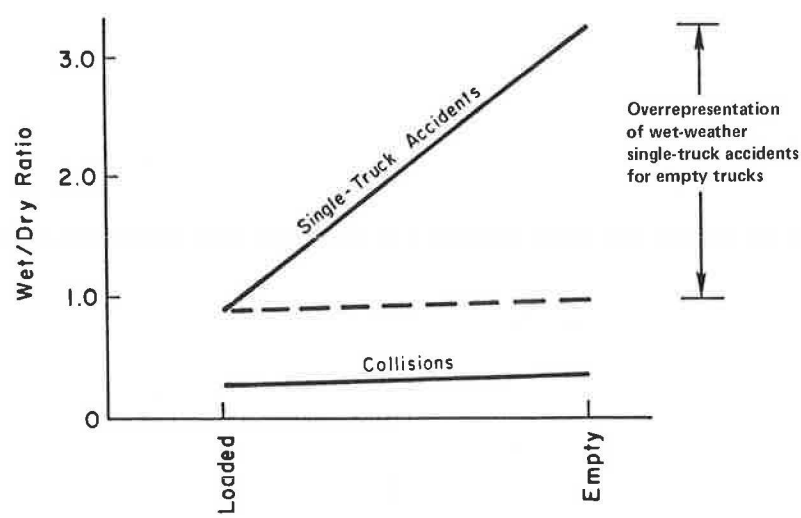

FIGURE 1 Estimated ratios of wet-to-dry accident involvements.

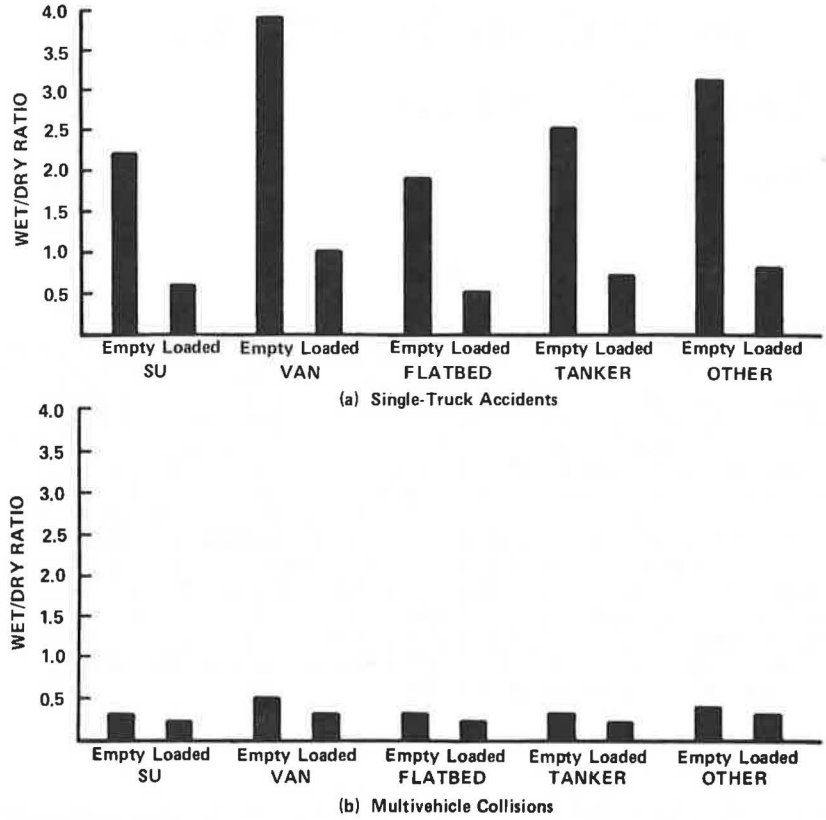

FIGURE 2 Weighted means of wet-to-dry truck accident involvement ratios.

dents and multivehicle collisions would be parallel as indicated by the dotted line. The data in Figure 2 indicate that the ratios of wet-to-dry accident involvements for empty trucks on four-or-more-lane highways in Texas were, on the average, about three times higher than expected when heavy-truck involvements in multivehicle collisions were used as a comparison group. This immediately suggests a very strong influence of wet pavements on single-truck accident involvements for empty trucks that was not observed for loaded trucks.

CONCLUSIONS

The foregoing analysis results clearly indicate that in wet conditions, empty trucks had a considerably higher estimated propensity for single-truck accident involvements than did loaded trucks. This higher propensity was indicated for all five truck types considered: single-unit trucks, combination trucks with van trailers, with flatbed trailers, with tankers, and with other trailer styles. Day/night had no significant influence on such propensity.

Whether the higher single-truck accident propensity of empty trucks in wet conditions was attributable to dynamic hydroplaning problems or whether some other factors were the primary causes warrants further research and investigation. Nevertheless, the accident analysis thus far appears to strongly support the prediction by Horne (2) and the recent laboratory findings by Ivey concerning the dynamic hydroplaning of truck tires at highway speeds.

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Publication of this paper sponsored by Committee on Methodology for Evaluating Highway Improvements.

Population Estimates From the National Truck Trip Information Survey

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ABSTRACT

The National Truck Trip Information Survey (NTTIS) is part of a research program at the University of Michigan Transportation Research Institute (UMTRI) to study the safety of large trucks (trucks with gross vehicle weight ratings greater than 10,000 lb) on the highway. The objective of the NTTIS is to provide descriptive information on the national population of large trucks and their use. This information will be combined with data from a companion survey of the fatal accident experience of all large trucks in the United States [called Trucks Involved in Fatal Accidents (TIFA)] to estimate involvement rates (fatal accident involvements per hundred million vehicle miles) for a broad range of truck configurations and use. Presented in this paper is a brief discussion of the overall methodology of the research program as background. The sampling frame, sample design, and survey methods are described, and preliminary estimates of the national population of large trucks are presented. The survey design and preliminary results are compared to the 1982 Truck Inventory and Use Survey conducted by the Bureau of the Census.

The National Truck Trip Information Survey (NTTIS) is part of the Truck Safety Research Program at the University of Michigan Transportation Research Institute. The objective of this survey is to provide population estimates and descriptive statistics on the national population of large trucks and their use. The overall objective of the Truck Safety Research Program is to identify from survey data on the truck population and its accident experience factors (characteristics of the driver, the vehicle, or its use) that are associated with accident involvement. Information on the fatal accident experience of all large trucks in the United States is being collected in a companion survey called Trucks Involved in Fatal Accidents (TIFA). The basic approach for this research program is to develop a data base with comparable scope and detail in both the accident and the exposure information. Vehicle mileage is used as the basic measure of exposure. With such a data base, multivariate statistical techniques can be used to identify factors associated with accident involvement. Incorporation of detailed information on the use of the vehicles is a major aspect of the overall program.

An overview of the Truck Safety Research Program is provided as background for the description of the National Truck Trip Information Survey. The description of the NTTIS includes discussion of the sampling frame, sample design, and survey method. Preliminary population estimates are presented, followed by a discussion of these results.

THE UMTRI TRUCK SAFETY RESEARCH PROGRAM

The basic analytic model for the accident process is the log-linear model for Poisson rates as described by Haberman (1). In fitting the accident frequencies, adjustments must be made for the exposure differences of the individual cells. In general, multivariate contingency table methods require fewer assumptions than other analytic approaches (2). Application of this method requires information on both the accident experience and the use of the vehicles with comparable coverage and detail. The scope of this research program includes all trucks with gross vehicle weight ratings greater than 10,000 lb. All pickup trucks and passenger vehicles are excluded. The current focus is on the relationship of vehicle configuration, size, weight, and use to the accident experience. Knowledge of the physical mechanisms involved and the relation of vehicle handling and stability to the configuration of the vehicle provide the basis for developing specific models and hypotheses to be tested with the survey data.

The handling and stability of various large truck configurations has been studied by conducting instrumented tests and through computer simulation. A summary of findings from this area that are pertinent to this analysis work is presented here. Most of this material is covered in three publications by Ervin et al. (3-5). Cab style and trailer length are relevant to the analysis in that shorter wheelbase units generally have poorer lateral stability than longer wheelbase units. This means that shorter wheelbase tractors (cab-over) are more likely to jackknife, for example. The number of axles also influences handling and stability. In general, tandem axles provide better lateral stability than single axles. Trailer body style, cargo, and weight are related to roll stability as follows. Roll stability is primarily determined by the height of the center of mass and the wheelbase. Combinations of cargo type and weight with trailer body style can serve as a surrogate for the height of the center of mass. Also, jackknife accidents are more likely to occur with empty vehicles because the drive axle is more likely to lock up during severe braking resulting in a loss of lateral stability.

The last of the three citations by Ervin (5) focuses on the amplification of lateral accelerations due to steering inputs in the last trailer of

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the combination. Significant variation in the rearward amplification ratio is observed for the various truck configurations currently in use. For example, the common five-axle tractor and semitrailer actually attenuates lateral accelerations with a rearward amplification ratio of less than one (0.8), whereas the lighter three-axle tractor and semitrailer is appreciably less stable with a rearward amplification ratio of about 1.4. By comparison, the double trailer combination with single-axle, 27-ft trailers has a rearward amplification ratio of 2.5.

The scope of the accident data collection program is all large trucks involved in fatal accidents in the contiguous 48 states and the District of Columbia. The objective of this program is to produce a single data file containing the data elements of both the National Highway Traffic Safety Administration (NHTSA) Fatal Accident Reporting System (FARS) file and the Bureau of Motor Carrier Safety (BMCS) accident file. The FARS file already contains a census of all fatal accidents in the United States, whereas the BMCS file provides a more detailed description of the involved truck. However, only trucks engaged in interstate commerce are required to file an accident report with the BMCS.

The truck accident program begins with the acquisition of the FARS and BMCS data tapes. These files are built in the appropriate formats for the necessary processing and analysis programs. A list of accidents involving medium and heavy trucks is sent to each of the states, and a copy of the police accident report is requested. Vehicles in the FARS file are then matched with the corresponding record in the BMCS file. About one-third of the trucks in the FARS file are matched with the BMCS report for the same vehicle and accident. For those trucks listed in the FARS file that are not matched with a corresponding BMCS report, the owner, as listed on the police report, is contacted by telephone or mail to obtain the BMCS data elements. For each truck hard copy files are assembled containing a summary listing of the FARS data elements, a copy of the police accident report, and either a summary listing from the matching report in the BMCS file, or the data form from the owner interview. The interview data are edited, keypunched, and added to the computerized files. In this way a national data file is produced with a record for every medium or heavy truck involved in a fatal accident and with the data elements of both the FARS and the BMCS files.

In order to carry out the planned analysis, information on the number of trucks in the United States and their use is required with the same level of detail as in the accident data. The Truck Inventory and Use (TIU) survey conducted by the Bureau of the Census every 5 years is the most detailed existing national exposure data for trucks. The 1982 survey results became available in fall 1985. The TIU survey data provide most of the necessary data elements that pertain to the description of the owner and the truck. However, necessary information on the day-to-day use of the truck such as road class, time of day, number of trailers, cargo weight, and length is lacking. The NTTIS is designed to provide these additional data elements.

SAMPLE DESIGN

The sample of trucks is a stratified simple random sample. Each state is a separate stratum, and within each state, straight trucks are sampled separately from tractors. Sample sizes were specified for each state roughly proportional to size, and an interval selection procedure was followed in each stratum. Survey dates were randomly assigned to each vehicle using a procedure to reduce intercluster correlations. The survey dates were organized into a sequence so that adjacent trucks are not surveyed on days close to each other and so that successive surveys of the same truck fall on different days of the week. A random start was selected, and the survey dates were then assigned in the specified sequence to the selected trucks (which were in selection order). The trip calls are being conducted over a 12-month period. Each truck will be surveyed on 1 randomly assigned day every 3 months, for a total of 4 survey days for each truck.

ESTIMATED SAMPLING ERRORS

The procedure used to determine the necessary sample sizes is described in this section. Information on the variance of truck mileage from previous surveys of truck use was used to estimate the sampling errors for the NTTIS. Tables 1 and 2 give the mean, sample size, standard deviation, and coefficient of variation for several categories of trucks. The figures given in Table 1 are taken from the FMVSS 121 safety impact evaluation (6) and are average daily mileages from a similar trip survey of 1977 model year trucks (conducted in 1978). Table 2 gives average annual mileages from the 1977 TIU survey (7). Examination of these tables illustrates that the standard deviations tend to vary in proportion to the mean, with categories having larger means also having larger standard deviations. The coefficient of variation is the ratio of the standard deviation to the mean, and it is somewhat more consistent than the standard deviations. Relatively homogeneous cat-

TABLE 1 Typical Means and Standard Deviations, Average Daily Mileages (6)

Category	N	Mean	Standard Deviation	Coefficient of Variation
Straight truck	638	76.5	99.8	1.30
Tractors	1,980	273.7	249.2	0.91
Straight truck—private	578	75.8	101.0	1.33
Straight truck—authorized	43	75.2	87.5	1.16
Straight truck—local use	459	62.5	47.1	0.75
Straight truck—short haul	138	102.3	115.7	1.13
Straight truck—long haul	37	148.8	158.2	1.06
Tractors—conventional cab	989	221.7	210.7	0.95
Tractors—cab-over	970	342.2	274.1	0.80
Tractors—private	941	261.9	239.3	0.91
Tractors—authorized	956	280.4	255.1	0.91
Tractors—exempt	61	380.1	301.5	0.79
Tractors—local use	289	105.1	143.7	1.37
Tractors—short haul	367	217.3	201.2	1.08
Tractors—long haul	1,307	328.5	260.3	0.79

TABLE 2 Typical Means and Standard Deviations, Annual Mileage—Tractors (7)

Category	N	Mean	Standard Deviation	Coefficient of Variation
Cab-over	4,519	65,861	48,199	0.73
Short conventional	2,191	35,632	36,744	1.03
Cab forward	432	29,070	33,598	1.16
Sleeper cab	3,620	70,262	45,636	0.65
One power axle	4,907	34,672	39,683	1.14
Three or more	189	44,145	43,127	0.98
Single trailer	11,480	49,046	51,912	1.06
Double trailer	318	66,175	50,591	0.76
Local	4,143	21,609	24,910	1.15
Long haul	3,379	85,853	45,079	0.53
Private	5,854	39,433	55,088	1.40
Common	2,804	64,836	49,908	0.77
Contract	1,221	66,594	40,743	0.61

egories with high means tend to have somewhat lower coefficients of variation. A coefficient of variation of 1.0 has been selected as typical from these tables, and it will be used to estimate the sampling errors given in the tables that follow.

Statistics will be computed at both the "truck" and the "day" levels. The effect of weighting on the variance will be ignored for these estimates because the weights will not vary greatly (straight trucks will have greatly different weights from tractors, but these groups will not be combined for analysis). Estimated sampling errors are presented here for proportions at the truck level and subclass means at both the truck and the day levels. Other statistics that will be computed include subclass population totals, ratios of means, and ratios of population totals at both the truck and day levels.

The variance of a proportion for a simple random sample is given by

$$\mathrm{Var}(p) = p(1 - p)/(n - 1) \qquad (1)$$

The approximate 95 percent confidence interval is given by plus and minus two times the square root of the variance. Table 3 gives the 95 percent confidence intervals for various proportions and sample sizes. The data in this table illustrate the expected accuracy for percentages at the truck level (percent cab-over, or percent operated by authorized carriers).

The variance of a subclass mean, $\bar{y}_m$, is given by

$$\mathrm{Var}(\bar{y}_m) = \mathrm{Sum}(y_m - \bar{y}_m)^2/m(m - 1) \qquad (2)$$

where the summation is over the subclass, m.

The data in Tables 4 and 5 illustrate the expected accuracy of subclass means at the truck and day levels, respectively. As for the proportions, the approximate 95 percent confidence interval is given by plus and minus two times the square root of the variance. In these tables, the figure shown is one-half the confidence interval (or twice the standard deviation) divided by the subclass mean, $\bar{y}_m$, and multiplied by 100. This may be considered as a percent error in the mean. The same information is presented in Table 5 for sample sizes and subset sizes appropriate for subclass means at the day level. The sample of days is a cluster sample of equal size for each truck. The influence of this clustering has been neglected in these estimates because the effect is not expected to be large. Statistics will not be computed for a single cluster (truck), but for subclasses made up of many trucks.

On the basis of data in Tables 3 through 5, target sample sizes of 4,000 tractors and 2,000 straight trucks were selected. Tractors operating with two trailers are expected to comprise about 5 percent of the tractor combinations. Accuracy for a subclass of this size would be about 14 percent at the truck level and 6 percent at the day level. Assuming a 20 percent nonresponse for the straight trucks and a 27 percent nonresponse rate for tractors, the required sample sizes increase to 2,500 straight trucks and 5,500 tractors. A higher nonresponse rate was assumed for the tractors because of some concern about the accuracy of the frame processing described in the next section.

TABLE 3 95 Percent Confidence Intervals on Proportions Versus Sample Size

	N			
P	2,000	3,000	4,000	5,000
0.001	±0.001	±0.001	±0.001	±0.001
0.01	±0.004	±0.004	±0.003	±0.003
0.10	±0.013	±0.010	±0.009	±0.009
0.20	±0.018	±0.015	±0.013	±0.011
0.30	±0.020	±0.017	±0.014	±0.013
0.40	±0.022	±0.018	±0.015	±0.014
0.50	±0.022	±0.018	±0.016	±0.014

TABLE 4 Percent Error in Average Annual Mileage Versus Subset Proportion and Sample Size

	Total Sample Size							
	2,000		3,000		4,000		5,000	
Category Proportion	N	$2S_{\bar{x}}/\bar{x}$ (%)	N	$2S_{\bar{x}}/\bar{x}$ (%)	N	$2S_{\bar{x}}/\bar{x}$ (%)	N	$2S_{\bar{x}}/\bar{x}$ (%)
0.25	500	8.9	750	7.3	1,000	6.3	1,250	5.7
0.10	200	14.1	300	11.5	400	10.0	500	8.9
0.05	100	20.0	150	16.3	200	14.1	250	12.6
0.01	20	44.7	30	36.5	40	31.6	50	28.3
0.005	10	63.2	15	51.6	20	44.7	25	40.0
0.001	3	115.5	4	100.0	5	89.4	2	141.4

TABLE 5 Percent Error in Average Daily Mileage Versus Subset Size and Sample Size

	Total Sample Size							
	8,000		12,000		16,000		20,000	
Category Proportion	N	$2S_{\bar{x}}/\bar{x}$ (%)	N	$2S_{\bar{x}}/\bar{x}$ (%)	N	$2S_{\bar{x}}/\bar{x}$ (%)	N	$2S_{\bar{x}}/\bar{x}$ (%)
0.25	2,000	4.5	3,000	3.7	4,000	3.2	5,000	2.8
0.10	800	7.1	1,200	5.8	1,600	5.0	2,000	4.5
0.05	600	8.2	800	7.1	1,000	6.3	400	10.0
0.01	80	22.4	120	18.3	160	15.8	200	14.1
0.005	40	31.6	60	25.8	80	22.4	100	20.0
0.001	8	70.7	12	57.7	16	50.0	20	44.7

SAMPLING FRAME

The sample of trucks was obtained from R.L. Polk, the same source as used by the Bureau of the Census for the Truck Inventory and Use survey. R.L. Polk maintains files of registered vehicles for every state except Oklahoma. The versions of these files reflecting registrations as of July 1, 1983 were used. In addition, Kansas, Maryland, Nevada, Oregon, Virginia, and Washington restrict the use of the information provided to R.L. Polk. Permission was obtained from each of these states to use the R.L. Polk data. Finally, the R.L. Polk data for California does not include trucks with model years before 1973. Hence, the NTTIS sampling frame includes the contiguous 48 states plus the District of Columbia, except for Oklahoma and pre-1973 model-year trucks in California.

Trucks included in the survey are straight trucks with gross vehicle weight ratings (GVWR) greater than 10,000 lb and all road tractors. Excluded are all pickup trucks (regardless of GVWR); all passenger vehicles (such as passenger vans, recreational vehicles, ambulances, and buses of any type); farm tractors; and government-owned trucks. An important feature of the selection procedure was the elimination of duplicate registrations from state to state. These duplicates could not be eliminated for the TIU survey because the frame is too large (about 34 million trucks as compared to an estimated 4 million trucks greater than 10,000 lb GVWR). R.L. Polk carried out extensive processing of the registration data in preparation for the sampling procedure. The objective of this processing was to identify the desired sampling strata: straight trucks with gross vehicle weight ratings greater than 10,000 lb and all tractors in each state. The algorithm used included extensive vehicle identification number (VIN)-decoding procedures supplied by UMTRI. It was hoped that this processing would produce accurate strata counts. In particular, the final sample sizes were based on an assumption that at least 90 percent of the trucks in the tractor strata would be tractors, and that negligible numbers of tractors would be in the straight truck stratum. The results of the implementation phase presented later in this paper show some of these assumptions to have been too optimistic.

The sampling frame totals obtained from R.L. Polk after processing the registration information and final sample sizes are given in Table 6. The unknown stratum is for trucks determined to have gross vehi-

TABLE 6 Frame Totals and Sample Sizes—1983 NTTIS

	Straight Trucks		Tractors		Unknown	
State	Frame	Sample	Frame	Sample	Frame	Sample
Alabama	42,481	56	29,140	91	1	0
Arizona	12,144	30	9,679	60	1	0
Arkansas	27,699	37	23,409	73	–	–
California	38,318	51	79,238	495	–	–
Colorado	30,980	41	18,211	60	–	–
Connecticut	14,625	30	11,793	60	96	2
Delaware	6,146	30	6,926	60	–	–
District of Columbia	600	30	487	60	–	–
Florida	59,137	78	63,306	198	2	0
Georgia	50,787	67	33,023	103	6,263	125
Idaho	11,289	30	11,512	60	46	1
Illinois	82,648	109	88,942	278	2	0
Indiana	61,777	82	61,554	192	2	0
Iowa	43,429	58	40,125	125	94	2
Kansas	82,622	109	29,544	92	–	–
Kentucky	56,651	75	22,168	69	–	–
Louisiana	32,699	43	29,211	91	3	0
Maine	12,501	30	7,715	60	1	0
Maryland	29,120	38	19,701	61	20	0
Massachusetts	28,974	38	27,073	85	13	0
Michigan	34,886	46	40,135	314	–	–
Minnesota	63,353	84	41,399	129	11	1
Mississippi	21,592	30	21,042	66	968	18
Missouri	56,462	75	33,946	106	–	–
Montana	25,214	33	11,482	60	8	0
Nebraska	43,255	57	24,590	77	18	1
Nevada	5,443	30	4,070	60	–	–
New Hampshire	5,992	30	6,607	60	1	0
New Jersey	30,148	40	45,161	141	1	0
New Mexico	13,626	30	11,719	60	–	–
New York	60,296	81	55,720	174	–	–
North Carolina	64,948	86	47,610	149	–	–
North Dakota	51,749	69	13,899	60	–	–
Ohio	68,867	91	57,247	235	3	0
Oklahoma	–	–	–	–	–	–
Oregon	18,848	30	22,567	70	–	–
Pennsylvania	71,012	94	66,994	209	–	–
Rhode Island	4,133	30	4,199	60	1	0
South Carolina	20,639	30	15,857	60	–	–
South Dakota	21,630	30	10,264	60	1	0
Tennessee	36,651	48	30,231	94	1	0
Texas	90,870	120	115,555	361	3	0
Utah	13,455	30	13,496	60	–	–
Vermont	5,269	30	3,732	60	–	–
Virginia	45,272	60	29,983	93	–	–
Washington	26,786	35	22,615	71	2	0
West Virginia	13,173	30	9,359	60	–	–
Wisconsin	42,529	56	36,917	115	10	0
Wyoming	9,297	30	10,741	60	21	0
Total	1,691,022	2,497	1,437,894	5,497	7,593	150

cle weight ratings greater than 10,000 lb that could not be assigned to either of the first two stratum with the algorithm used. Sample sizes were taken in proportion to the frame totals except that a minimum sample of 30 straight trucks and 60 tractors was imposed. After selection, the final sample sizes were 2,497 from the straight truck stratum, 5,497 from the tractor stratum, and 150 from the unknown stratum, for a total sample of 8,144 trucks.

PROTOCOL

Survey interviewing was conducted by telephone whenever possible. Mail versions of the interviews were used only when the interview could not be completed by telephone. The survey work was divided into five phases. The first, or implementation, phase is the initial contact with the owner. On the initial contact, owner cooperation must be secured, vehicle identification confirmed, descriptive information on the company and truck obtained, and arrangements made for acquisition of the detailed mileage information on the survey date. The remaining four phases correspond to the four survey dates for the detailed mileage information, one every 3 months for each truck. Sample survey data forms are shown in Figures 1 and 2.

RESULTS

The implementation phase was initiated the first week of January 1985 and was not completed until the middle of May. The overall response rate was 75.1 percent, including partial completions. About 6 percent of the trucks selected were found to be nonsample vehicles. Of these, two-thirds had been destroyed, and 12 percent were no longer registered. Another 8.2 percent of the nonsample vehicles were trucks with gross vehicle weight ratings of 10,000 lb or less, while 6.2 percent were not trucks. Excluding nonsample vehicles, the response rate was 80 percent. As expected, inability to locate the owner was the major problem, accounting for 84 percent of the nonresponse. For many of these vehicles, the registration information obtained from R.L. Polk appeared to be out of date. The listed owner would indicate that he had sold the truck; however, sometimes a follow-up check with the state department of motor vehicles would show him to still be the registered owner. Refusals were encountered on only 3 percent of the selected vehicles, making up the remaining 14 percent of the nonresponse.

Preliminary analysis of the information collected in the implementation interviews reveals that about 40 percent of the trucks selected from the tractor stratum were found to be straight trucks. Table 7 gives the R.L. Polk frame totals versus the survey responses. The column totals are the sampling frame stratum totals, whereas the row totals show the results of the survey responses. Vehicles shown in the "tractor" column were selected from the tractor stratum in the sampling frame. The row entries show the survey responses for these vehicles. Nonresponse on the question of power unit type is shown as the "unknown" row on this table, and is only 12 percent of the total. The straight truck stratum was relatively clean, containing only about 4 percent tractors. As mentioned earlier, only about 6 percent of the selected vehicles were found to be nonsample. Overall, the frame processing was quite accurate except for the straight trucks in the tractor stratum.

Finding that 40 percent of the trucks selected as tractors are actually straight trucks has a direct influence on the resulting population estimates. This has also reduced the number of tractors in the sample from the target sample size of 4,000 to about 2,500. The data in Table 8 compare the NTTIS population estimates with figures derived from the 1982 Truck Inventory and Use survey public use tape (8) that was recently received from the Bureau of the Census. For this table, the survey nonresponse has been distributed to the straight truck and tractor categories. This was done by first dividing the nonresponse into 24 categories based on sampling strata, manufacturer, model year, and the R.L. Polk body style derived from the original registration information. Survey responses were used to determine the proportion of straight trucks and tractors in each of the 24 categories, and the nonresponse was distributed according to these proportions. Although the sampling frame totals indicated a national population of 1,437,894 tractors, the survey responses indicate a tractor population of only 873,732.

These figures are not comparable to FHWA (9) counts because the FHWA figures include some pickup trucks, some utility (passenger) vehicles, and other trucks with GVWR of 10,000 lb or less. For the NTTIS, large trucks are defined as trucks that have a gross vehicle weight rating greater than 10,000 lb. For purposes of comparison, trucks registered in Alaska, Hawaii, and Oklahoma are excluded from Table 8 as well as pre-1973 model-year trucks in California.

In general, the agreement between the 1982 TIU survey and the 1983 NTTIS is good. The frame processing for the NTTIS included elimination of duplicate registrations from state to state. This was not done for the TIU survey sample. For the NTTIS, the GVWR was determined from the vehicle identification number and then confirmed when the owner was contacted in the implementation phase. Only 0.5 percent of the selected trucks were found to have GVWR of 10,000 lb or less. In comparison, the gross vehicle weight code in the 1982 TIU survey data is based on the owner's estimate of the average weight of the vehicle when carrying a typical payload during the past year. The use of VIN information followed by confirmation by the owner in the NTTIS would appear to provide a more accurate identification of trucks that have a manufacturers' gross vehicle weight rat-

TABLE 7 Estimated U.S. Large Truck Population, R. L. Polk Frame Totals Versus Survey Responses

	Polk Frame Totals			
Survey Data	Straight Truck	Tractors	Unknown	Survey Total
Straight truck	1,349,256	459,973	3,397	1,812,626
Tractor	49,321	693,820	50	743,191
Unknown	179,383	199,234	1,521	380,138
Nonsample	113,062	84,867	2,625	200,554
Polk totals	1,691,022	1,437,894	7,593	3,136,509

TABLE 8 Estimates of the U.S. Large Truck Population[a]

	Source	
Truck Type	1982 TIU	NTTIS
Straight truck	2,393,173	2,062,223
Tractors	863,385	873,732
Total	3,256,558	2,935,956

[a] Excluding Alaska, Hawaii, Oklahoma, and trucks with model years before 1973 in California.

COMPANY DESCRIPTION

OPERATING AUTHORITY:

Is this a daily rental truck? YES[ ]7
Is this truck govt. owned? YES[ ]6 (8)
(city/county/state/federal)
→ *SKIP to Power Unit Description below.*

Do any of your trucks ever carry goods interstate (across state lines)?

[ ]1 YES → Are you
- PRIVATE (Carry own goods) [ ]1 → [ ]1
- FOR HIRE (Carry other people's goods) [ ]2 →
  - ICC Authorized (common/contract) [ ]2
  - Exempt [ ]3
  - → Is the owner also the driver? YES [ ]1 NO [ ]2

[ ]2 NO → Are you
- PRIVATE (Carry own goods) [ ]1 → [ ]4
- FOR HIRE (Carry other people's goods) [ ]2 → [ ]5 (11) → Is the owner also the driver? YES [ ]1 NO [ ]2

[ ]9 UNKNOWN (9) →
- PRIVATE [ ]1
- FOR HIRE [ ]2 (10) → Is the owner also the driver? YES [ ]1 NO [ ]2 (12)

POWER UNIT DESCRIPTION

Verify the make, model year, and VIN, and ask for the model name and company unit number.

1. Make _____ Year: 19__ __ VIN _____
2. Model Name _____ Company Unit Number _____
3. *EDITOR:* Code the base state of operation __ __ (13 14)
4. POWER UNIT TYPE
   - Tractor [ ]8
   - Straight Truck [ ]1 (15)
   - *STRAIGHT TRUCK BODY STYLE:*
     - Van [ ]1
     - Flatbed [ ]2
     - Tanker [ ]3
     - Refrig. [ ]5
     - Dump [ ]6
     - Refuse [ ]7
     - Other [ ]8 (16)
     - _____ *(Specify)*
5. NUMBER OF AXLES
   - Two [ ]2
   - Three [ ]3
   - Four + [ ]4 (17)
6. CAB STYLE
   - Cab Forward [ ]1
   - Cab Over [ ]2
   - Short Conventional [ ]3
   - Med. Conventional [ ]4
   - Long Conventional [ ]5 (18)
7. FUEL
   - Gas [ ]1
   - Diesel [ ]2
   - Other _____ [ ]3 (19)
     (Specify)
8. Power Unit EMPTY WEIGHT: __ __ __ __ __ __ (20 21 22 23 24 25)
9. Power Unit LENGTH: __ __ __ (26 27 28)
10. Estimated Annual Mileage *for this power unit:* __ __ __ __ __ __ (29 30 31 32 33 34)
11. Percent of annual mileage for each trip type *for this power unit:*
    - Local *(Pickup and delivery, with 50 mile radius)* __ __ __% (35 36 37)
    - Short Haul *(Intercity, one-way, distance 50-200 miles)* __ __ __% (38 39 40)
    - Long Haul *(Intercity, one-way, distance 200+ miles)* __ __ __% (41 42 43)

    (Total=100%)
12. Does this power unit ever pull twin trailers?
    [ ] Yes Percent of annual mileage with twin trailers: __ __ __% (44 45 46)
    [ ] No *(Enter 000.)*
13. Odometer Reading __ __ __ __ __ __ (47 48 49 50 51 52) Date of Reading __ __/__ __ __/__ __ (53 54 55 56 57 58)

FIGURE 1 NTTIS company and power unit description.

WAS THIS VEHICLE IN USE ON THE SURVEY DAY? No[ ]00

TRIP 01

VEHICLE, CARGO, AND DRIVER

1. OPERATING AUTHORITY (Private Carriers only)
   Were you operating for-hire (e.g., on backhaul)?
   [ ]1 No
   [ ]2 Yes Was it as? ICC (common/contract) [ ]2
   Exempt (interstate hauling only) [ ]3
   Intrastate for-hire [ ]5

2. DRIVER AGE: ____ Yrs. 3. DRIVER YEARS WITH COMPANY: ____ Yrs.

4. CONFIGURATION: Any trailers? No [ ]1
   Yes [ ]2

	Power Unit	1st Trailer	2nd Trailer	3rd Trailer
Type:		Semi [ ]1		
		Full [ ]2	Full [ ]2	Full [ ]2
		Utility [ ]3	Utility [ ]3	Utility [ ]3
		Other [ ]4	Other [ ]4	Other [ ]4
		None [ ]5	None [ ]5	None [ ]5
Body:		Van [ ]1	Van [ ]1	Van [ ]1
		Flatbed [ ]2	Flatbed [ ]2	Flatbed [ ]2
		Tank [ ]3	Tank [ ]3	Tank [ ]3
		Auto C. [ ]4	Auto C. [ ]4	Auto C. [ ]4
		Dump [ ]6	Dump [ ]6	Dump [ ]6
		Other [ ]8	Other [ ]8	Other [ ]8
		(Specify)	(Specify)	(Specify)
No. Axles Used:				
Lengths (Ft):				
Empty Wts (Lbs):				
5. CARGO:	[ ]	[ ]	[ ]	[ ]
Cargo Wt (Lbs):				
Hazardous Cargo	Yes [ ]1 No [ ]2	Yes [ ]1 No [ ]2	Yes [ ]1 No [ ]2	Yes [ ]1 No [ ]2

6. GROSS COMBINATION WEIGHT for the trip (Lbs): ____ [1]

MILEAGE

1. Starting Point ____ (City) ____ (State) Time: ____ AM[ ] PM[ ]
2. End Point ____ (City) ____ (State) Time: ____ AM[ ] PM[ ]
3. Via ____
   (Describe route/give road nos., etc.)
4. Total Miles for Trip: ____
5. Breakdown of Mileage:

	LIMITED ACCESS		US/STATE/MAJOR ARTERY		OTHER	
	Day (6am-9pm)	Night (9pm-6am)	Day (6am-9pm)	Night (9pm-6am)	Day (6am-9pm)	Night (9pm-6am)
Rural:						
Sm Urban: (Pink & Orange)						
Lg Urban: (Yellow)						

6. Specific Large Urban Area: ____ ____ [2]

FIGURE 2 NTTIS survey day trips.

VEHICLE, CARGO, AND DRIVER

1. OPERATING AUTHORITY (Private Carriers only)
   Were you operating for-hire (e.g., on backhaul)?
   [ ]1 No
   [ ]2 Yes (10) Was it as? ICC (common/contract) [ ]2
   Exempt (interstate hauling only) [ ]3
   Intrastate for-hire [ ]5 (11)

2. DRIVER AGE: _____ Yrs. (12-13) 3. DRIVER YEARS WITH COMPANY: _____ Yrs. (14-15)

4. CONFIGURATION: Any trailers? No [ ]1
   Yes [ ]2 (16)

	Power Unit	1st Trailer	2nd Trailer	3rd Trailer
Type:		Semi [ ]1		
		Full [ ]2	Full [ ]2	Full [ ]2
		Utility [ ]3	Utility [ ]3	Utility [ ]3
		Other [ ]4	Other [ ]4	Other [ ]4
		None [ ]5 (17)	None [ ]5 (18)	None [ ]5 (19)
Body:		Van [ ]1	Van [ ]1	Van [ ]1
		Flatbed [ ]2	Flatbed [ ]2	Flatbed [ ]2
		Tank [ ]3	Tank [ ]3	Tank [ ]3
		Auto C. [ ]4	Auto C. [ ]4	Auto C. [ ]4
		Dump [ ]6	Dump [ ]6	Dump [ ]6
		Other [ ]8 (20)	Other [ ]8 (21)	Other [ ]8 (22)
		(Specify)	(Specify)	(Specify)
No. Axles Used:	(23)	(24)	(25)	(26)
Lengths (Ft):	(27-29)	(30-32)	(33-35)	(36-38)
Empty Wts (Lbs):		(39-44)	(45-50)	(51-56)
5. CARGO:	[ ] (57-58)	[ ] (59-60)	[ ] (61-62)	[ ] (63-64)
Cargo Wt (Lbs):	(65-70)	(71-76)	(77-82)	(83-88)
Hazardous Cargo	Yes [ ]1 No [ ]2 (89)	Yes [ ]1 No [ ]2 (90)	Yes [ ]1 No [ ]2 (91)	Yes [ ]1 No [ ]2 (92)

6. GROSS COMBINATION WEIGHT for the trip (Lbs): _____ (93-98) [1] (99) DUP 1-9

MILEAGE

1. Starting Point _____ (City) _____ (State) Time: _____ AM[ ] PM[ ]
2. End Point _____ (City) _____ (State) Time: _____ AM[ ] PM[ ]
3. Via _____
   (Describe route/give road nos., etc.)
4. Total Miles for Trip: _____ (10-13)
5. Breakdown of Mileage:

	LIMITED ACCESS		US/STATE/MAJOR ARTERY		OTHER	
	Day (6am-9pm)	Night (9pm-6am)	Day (6am-9pm)	Night (9pm-6am)	Day (6am-9pm)	Night (9pm-6am)
Rural:	(14-17)	(18-21)	(22-25)	(26-29)	(30-33)	(34-37)
Sm Urban: (Pink & Orange)	(38-41)	(42-45)	(46-49)	(50-53)	(54-57)	(58-61)
Lg Urban: (Yellow)	(62-65)	(66-69)	(70-73)	(74-77)	(78-81)	(82-85)

6. Specific Large Urban Area: ____ (86) ____ (87) [2] (99)

FIGURE 2 (continued)

ing greater than 10,000 lb. Despite these differences, the agreement between the population estimates from the NTTIS and the 1982 TIU survey is reassuring. This is the first time that an independent national survey has been conducted to corroborate the TIU survey results. The combination of these two surveys substantially reduces the range of estimates of the U.S. large truck population.

ACKNOWLEDGMENT

The work described in this paper is part of a long-term truck safety research program at the University of Michigan Transportation Research Institute sponsored by the Motor Vehicle Manufacturers Association, the American Trucking Associations, Inc., the Western Highway Institute, the Truck Safety Equipment Institute, Freightliner Corporation, and the National Highway Traffic Safety Administration. The author is grateful for the opportunity to contribute to the knowledge of truck safety that this kind of continuing support makes possible.

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Publication of this paper sponsored by Committee on Motor Vehicle Technology.

A Note on Accident Risk

D. MAHALEL

ABSTRACT

The use of accident rates as risk estimators, though widespread, presents a potential error. This may occur when the relationship between exposure and accidents is not linear (i.e., a decreasing derivative); then, an increase in exposure might be misinterpreted as leading to a decrease in accident risk. To obviate such error, a definition of risk as a triplet of exposure, accidents, and probability is presented. Accordingly, the risk level of a system can only be expressed in relation to a specific exposure level. The definition of exposure resulting from this definition of risk is simply any traffic situation from which the number of accidents can be estimated.

A common method of defining the safety level of a transport system is by means of risk and exposure. Risk estimates are used to describe the safety level of transportation systems in a manner that is invariable to their exposure level. This approach gains impetus in a "before and after" safety-improvement comparison or in a comparison of two structurally different systems (e.g., two different road sections or two intersections), where differences in exposure level are known to exist.

The most widely used means of describing transportation-system risk is the accident rate. According to Wolfe (1), ". . . comparison of accident rates can assist road safety researchers in developing safety countermeasures in ways that comparisons of absolute frequencies of accident cannot." Thus Frantzeskasis (2) compared highway risk in different countries on the basis of accident rates.

Accident rates are usually defined as the number of accidents (whether total number of accidents, certain types of accidents, or severity of accidents) divided by exposure measures. Exposure is generally defined as the number of opportunities for accidents--for example, total mileage or the number of pedestrians crossing, or as a certain function of traffic volumes at intersections.

A methodological problem, however, is inherent in the use of accident rates: the need to assume that the number of accidents increases by a constant amount with a certain increase in exposure. This assumption is equivalent to assuming the existence of a linear relationship between road accidents and exposure, a situation described in Figure 1. As can be observed, the linear relationship between exposure and accidents creates a constant risk (slope of the curve) for each exposure. Thus the risk in System 1 is always greater than the risk in System 2; for any given exposure level, there are always more accidents in System 1 than in System 2.

The situation changes when the derivatives of the curves decrease with an increase in exposure. This occurs, as shown in Figure 2, when increased exposure worsens the safety situation by decreasing units. Here the exposure levels in Systems 1 and 2 are F_1 and F_2, respectively; the risk (or the accident rate) in System 1 at point A is lower than that in System 2 at point B. Without prior knowledge of the type of curves, a wrong conclusion could be drawn, that is, that System 1 is less risky than System 2.

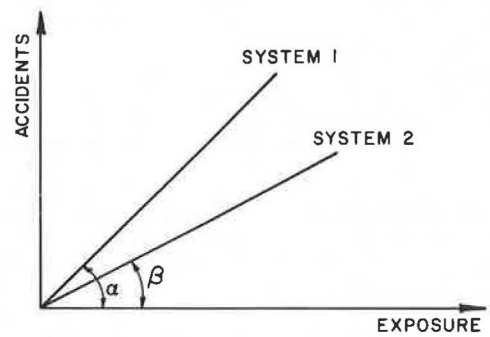

FIGURE 1 Hypothetical linear relationship between exposure and accidents.

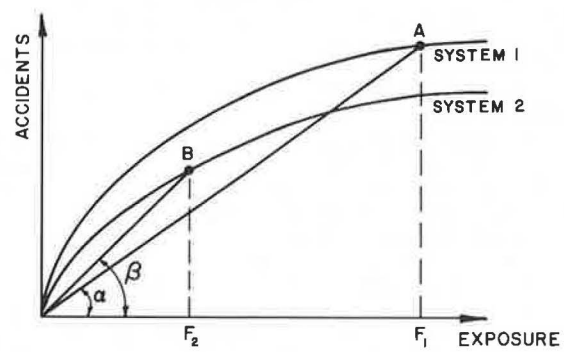

FIGURE 2 Hypothetical nonlinear relationship between exposure and accidents.

DEFINING THE PROBLEM

The popular definition of risk as a ratio between the number of road accidents and the amount of exposure [see, for example, Chipman (3); Cameron (4); Chapman (5); Wolfe (1); Hauer (6)] necessitating the existence of a linear relationship (with a zero intercept) between accidents and exposure appears log-

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ically appealing: it implies that risk is the probability of an accident's resulting from one exposure unit. Consequently, the number of accidents is seen as a binomial process whose expectation is the product of probability (risk) and the number of trials (amount of exposure).

This definition, however, creates a problem in that it negates situations in which certain systems function effectively (low risk) at a certain level of exposure and less effectively (high risk) at a different level of exposure. Traffic signals may be viewed as an example of a situation of this type: they are effective in reducing the number of accidents for high traffic volumes, but can cause an increase in the number of accidents for low traffic volumes.

Under certain simple assumptions (an increase in the number of accidents with an increase in density, and a linear relationship between speed and density), the relationship between accidents and an exposure estimator (traffic volumes) is not constant (see section on Variation of Risk in Accordance with Traffic Volumes). In other words, risk may vary with the amount of exposure, a phenomenon not permitting the use of risk as a constant scalar factor for characterizing a system.

To summarize, as a result of formal definitions, there appears to be a vicious circle in which, on the one hand, the accepted definition of risk necessitates a linear relationship between accidents and exposure; on the other hand, it is difficult (or even impossible) to find exposure estimators that fulfill this limitation. Therefore a lack of analytical or empirical tools exists whenever the need arises to evaluate the safety aspects of a specific facility. The question now is whether it is desirable to change the definition of risk to allow the use of an extensive set of exposure measures.

VARIATION OF RISK IN ACCORDANCE WITH TRAFFIC VOLUMES

In this section it is demonstrated how risk varies in accordance with the traffic-flow conditions under which the system exists during exposure measurements. According to the widespread approach, risk is defined as the ratio between the number of accidents (A) and the amount of exposure (E).

As a starting point, examine a road section in which the length is known (ℓ) for a time interval of t hours. The expected number of accidents in the section is assumed to be dependent on both the number of vehicles on the road section and their travel speed. The density (D) of these vehicles determines the relative proximity in space between them, and with travel speed, also their relative proximity in time. The use of density is appealing because it is possible to obtain the same level of traffic volumes for two different levels of density and speed. Roess et al. (7) also chose density and speed as recommended criteria in their proposal for revising procedures for level of services.

Earlier, Haight (8) proposed that the expected number of accidents in a road section be a quadratic function of density. From this simple model, two characteristics relating density to accidents can be defined:

1. The marginal increase in density to a road section increases the number of accidents $[(dA/dD) > 0]$; and
2. For a constant increase in density, the marginal increase in accidents increases $[(d^2A/dD^2) > 0]$ as D increases.

This second assumption does not necessarily exist in high densities. Following a decrease in travel speeds, it is possible that for a certain range of low speeds $d^2A/dD^2 < 0$.

The effect of traffic volumes on accidents may be obtained from the relationship

$$V = D \cdot S$$

where V is traffic volume (vehicles per time unit) and S is space mean speed (km/h).

Using Greenshields' (9) suggestion for the linear relationship between density and speed

$$S = a - bD \qquad (b > 0)$$

where a is mean free speed, $b = a/D_j$, and D_j is jam density, it follows that

$$V = D(a - bD) = dD - bD^2$$

The first derivative with respect to accidents will be

$$dV/dA = a(dD/dA) - 2\,bD(dD/dA) = dD/dA\ (a - 2\,bD)$$

using the relationship

$$dA/dV = (1/DV)/dA$$

gives the following

$$dA/dV = 1/(1/dA/dD)(a - 2bD)$$

Thus, the range of traffic volumes in which an increase in density follows an increase in volume $(D < a/2b)$, it follows that $dA/dV > 0$; for the range of traffic volumes in which volumes decrease with an increase in density,

$$dA/dV < 0$$

One conclusion from the foregoing is that the ratio A/V, which is widely used for risk, is not constant, being determined by traffic-flow conditions. In the range where $D > a/2b$, the risk (A/V) decreases with an increase in V; however, in the range where $D < a/2b$, the risk increases or decreases in accordance with the behavior of d^2A/dV^2.

In order to investigate the behavior of d^2A/dV^2, the second derivative of the inverse function should be evaluated

$$d^2V/dA^2 = d^2D/dA^2\ (a - 2bD) - 2b(dD/dA)^2$$

The second derivative of the function $A = f(v)$ is

$$\begin{aligned} d^2A/dV^2 &= [-(d^2V/dA^2)/(dV/dA)^2] \cdot dA/dV \\ &= \{-[(d^2D/dA^2)(a - 2bD) - 2b(dD/dA)^2] \\ &\quad \div [dD/dA(a - 2bD)]^3\} \end{aligned}$$

From the foregoing expression, it is possible to determine a series of conditions that determine the changes in A/V with an increase in V. For example, if traffic volumes increase with density $(D < a/2b)$ and if $d^2A/dD^2 > 0$ (i.e., a quadratic function between density and accident), it follows that $d^2A/dV^2 > 0$. In other words, an increase in V also increases A/V.

It should be emphasized that a convex function similar to that shown in Figure 2 can also be obtained when the volume is an increasing function of D. For example, when $D < a/2b$, $d^2D/dA^2 > 0$, and $d^2D/dA^2\ (a - abD) > 2b\ (dD/dA)^2$. Evidence for the existence of varying risk levels for different traffic volumes is described by Ceder and Livneh (10).

ALTERNATIVE DEFINITION OF RISK

Following ROWE (11)--"Risk is the potential for realization of unwanted negative consequences of an event"--the risk function of a road system must express the probability of a certain number of accidents for each possible traffic situation in that system. In other words, the risk function is aimed at estimating the number of expected accidents (or the probability of a certain number of accidents) at each exposure level in the system. Following Kaplan (12), at a given exposure level the risk (R_o) can be described by the triplet

$$R_o = < E_o, A_o, P_o >$$

where

R_o = risk at the E_o exposure level;
A_o = expected number of accidents, or a vector describing the severity of accidents; and
P_o = probability of A_o accidents (possibly a vector).

The definition of risk for any exposure level is the set of all triplets:

$$R = \{ < E, A, P > \quad E > 0 \}$$

The exposure level itself can be a vector of different exposure measures; for example, the number of pedestrians and traffic volumes.

The fundamental characteristic of this alternative definition of risk is its ability to express the expected number of accidents in a system or the probability of a certain number of accidents at any exposure level. Accordingly, the risk level of a system can be expressed only in relation to a specific exposure level.

The task of the researcher involved in risk analysis may be seen as the search for a black box in which input is exposure and in which output is accidents and probabilities. The image of the black box fits the situation in which the researcher seeks, not the physical law relating exposure to accidents, but a mathematical model relating the input variables to the output variables of a system.

Risk function can be described graphically with various cross-sections. Figure 3 shows such an example, describing the risk level of a number of systems at specific exposure levels. As can be seen, the probability of a certain number of accidents in System B is higher than in System A; thus at exposure level E_0, System B is more dangerous than System A. Figure 4 shows the probability for A_0 or more accidents in Systems A and B at each exposure level. Although System B is more dangerous than System A up to the exposure level of E_1, the reverse holds true for exposure levels greater than E_1.

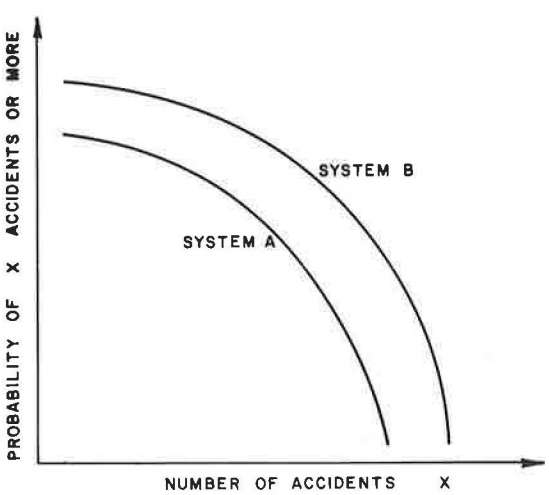

FIGURE 3 Risks at E_o exposure level for two systems.

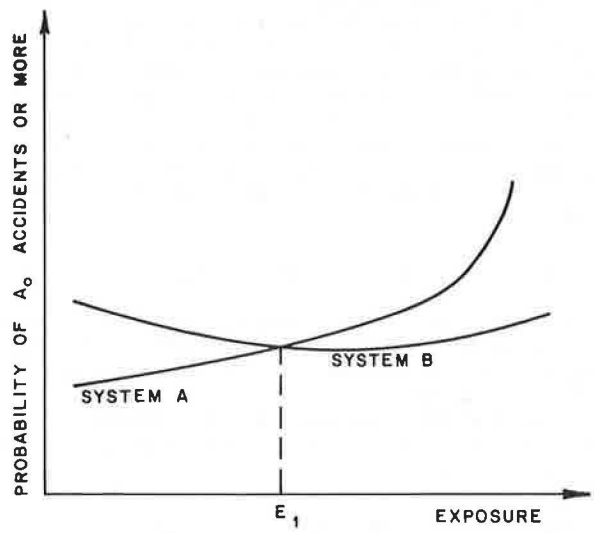

FIGURE 4 Probability of A_o accidents or more as a function of exposure.

EXPOSURE ESTIMATORS

The present alternative definition of risk does not impose limitations on the choice of exposure estimators (such as the linear relationship to accidents); therefore, exposure may be defined as any traffic situation from which the number of accidents can be estimated. Nevertheless for a safety evaluation, the preference of one exposure estimator over others must be based on the following two criteria:

1. Data collection ability--the empirical ability to collect exposure data.
2. Validity--the analytical ability to estimate the vector of accidents and probabilities from exposure.

The criterion of empirical ability to collect exposure data gives priority to measures based on available data or easily collectable data. Such data as vehicle kilometers and total number of vehicles entering an intersection, will therefore receive preference over such exposure measurements as number of lane changes and number of stops. The second criterion determines the validity level of the exposure measures. The methodology for this determination involves standard statistical procedures in model-building, such as minimum least squares, correlation coefficients, and so forth. These two criteria assure that the justification for using a certain exposure measurement is primarily practical and empirical, and not methodological and theoretical.

The process of validity evaluation involves the building of a mathematical model that is used to calculate the vector of accidents and probabilities for each input level of exposure. For each system having a different exposure measure, a different model must be built; this process means a model for intersections, road sections, and so forth. Further-

more, the need for estimating a model sometimes arises for similar exposure measures, for example, straight road sections and horizontal curves. Separate models (or black boxes) also are often used for single-vehicle accidents and multivehicle accidents. In all the preceding cases, the degree of validity achieved is the criterion for building the separate black boxes. Successful examples for exposure models are described by Cleveland et al. (13), Cleveland and Kitamura (14), and Zegeer and Mays (15).

When it is empirically apparent or when it may be theoretically assumed that two systems have the same black box, a risk analysis can be carried out that is based solely on exposure data, without the need for accident data. In these situations, if an increasingly monotonous relationship exists between exposure and accidents, the number of accidents can be assumed to be greater when the amount of exposure is higher.

ADVANTAGES OF PRESENT APPROACH ILLUSTRATED

The example that follows emphasizes the advantages of the approach presented in this paper for evaluating risk as opposed to the conventional approach, which uses a constant scalar.

Assume that two alternatives were evaluated for a transport investment. To obtain risk estimators, the data given in Table 1 were collected, obtained from the doublets $\langle N_i, E_i \rangle$. N_i is the mean number of accidents at Alternative 1 and E_i is traffic volumes or exposure at that site. A calculation of the mean number of accidents per one million exposure units shows that 520 accidents per one million units would apparently occur if Alternative 1 was used, whereas 598 accidents per one million exposure units would occur if Alternative 2 was used. The conclusion is that Alternative 2 is more dangerous and, therefore, inferior to Alternative 1.

A closer look at the existing relationship between exposure and accidents leads to the conclusion that it is possible to adapt a different model for each alternative.

Model for Alternative 1: $A = 3.10^{-3} * E^{0.8}$
Model for Alternative 2: $A = 0.1 * E^{0.4}$

where A is the expected number of accidents and E is the amount of exposure.

Figure 5 shows the risk curve for exposures of 5,000 and 10,000 vehicles. A Poisson model is used to calculate the probabilities. Alternative 1 can be seen to offer an advantage with a lower exposure level, whereas Alternative 2 is more attractive with higher exposure levels. The break point between the two alternatives is at traffic volumes of 6,457 vehicles per day, which, it should be remembered, completely disappears in traditional risk analysis.

TABLE 1 Accident Data and Traffic Volumes for Two Alternatives

	Alternative 1		Alternative 2	
Site	Mean No. of Accidents	Exposure (vehicles per day)	Mean No. of Accidents	Exposure (vehicles per day)
1	4.6	9,500	2.9	4,500
2	3.0	5,640	3.2	6,000
3	2.3	4,100	2.7	3,800
4	3.3	6,400	3.4	6,950
5	2.5	4,500	2.6	3,500
6	15.7	30,140	14.8	24,750

DISCUSSION OF APPROACH

In this paper a conceptual framework is presented with which, in the opinion of this author, risk analysis can be carried out more effectively.

The advantage of the present approach lies in its capacity to use a variety of mathematical models for describing risk in a system. Definition here does not limit or dictate linear or other assumptions during the empirical estimation of risk. Instead, the broad definition of risk that is presented allows for different traffic situations as exposure estimators in accordance with two criteria: one for

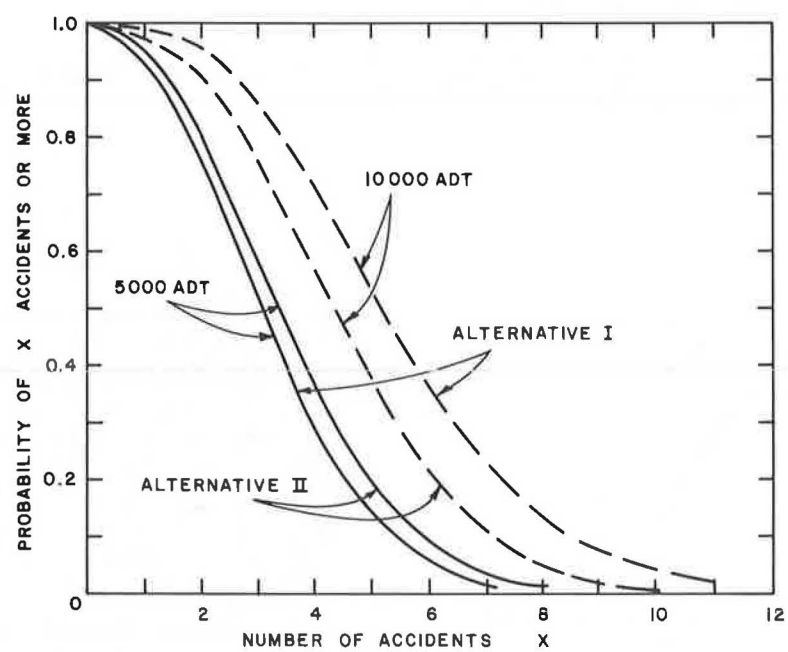

FIGURE 5 Risks at different exposure levels for the two alternatives.

facilitating data collection and another for validity. An empirical base supplies the justification for the preference of one exposure measurement over another. Risk, in accordance with the definition given in this paper, is the set of triplets that expresses the probabilities for the number of accidents at any given exposure.

Forfeiting the use of a constant scalar to describe risk is the main disadvantage of the present definition. This forfeiture, however, is not arbitrary, but results from the empirical fact that the relationship between exposure measures and accidents might not be linear. For those cases in which a certain exposure measure shows a linear relationship with the number of accidents, accident rates can then be used as a risk measure.

It should be emphasized that in situations in which a safety evaluation is required, the need for estimating risk function does not always arise. For example, in a comparison of two existing systems with similar exposure levels, the safety level can be evaluated directly by means of accidents thereby eliminating the need for the risk function.

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Publication of this paper sponsored by Committee on Methodology for Evaluating Highway Improvements.

Highway Safety: Twenty Years Later

JULIE ANNA CIRILLO and FORREST M. COUNCIL

ABSTRACT

In 1970 a review was conducted of research and evaluation studies that analyzed some 57 highway-related safety countermeasures. As a result of that review only eight countermeasures were identified for which good to excellent estimates of effectiveness had been made. Although for most safety countermeasures it is known whether the countermeasure is better than nothing, it is often not known under what condition a countermeasure is most effective or how effective. Reviewed in this paper is what is known about the effectiveness of various safety countermeasures, as well as what is not known. Countermeasures discussed include various roadside hardware devices as well as geometric features. The reasons for the lack of knowledge are also discussed. This discussion focuses on the quality of safety evaluation studies and methods for improving the quality of these studies are recommended.

In 1970 after years of research related to roadway safety, Solomon, Starr, and Weingarten reviewed research and evaluation studies that analyzed 57 highway-related safety countermeasures (1). The authors believed that they had found "good to excellent" estimates of effectiveness for only 8 of the 57 countermeasures; for the remaining 49, effectiveness estimates were ". . . based either on engineering judgment, involved only fair or poor data, or were little more than guesses." In the past 14 years, the situation has improved somewhat as is evidenced by a reduction in fatality rate from 5.2 to 2.7 per 100 million vehicle miles of travel. Much of this improvement has been the result of increased safety funding, both in terms of increased roadway research funding from the Federal Highway Administration (FHWA) and increased funding for trial programs or countermeasures related to the driver and the vehicle from the National Highway Traffic Safety Administration.

Indeed, in discussing a given countermeasure, if the question is asked: "Is this countermeasure better than the 'do nothing' alternative?" the answer can usually be given with some certainty. For most countermeasures, a study or series of studies have been conducted that, when combined, give a fairly clear indication of whether the treatment has any degree of effectiveness. Consider the example of providing a 30-ft clear roadside. From past studies, little doubt exists that providing a 30-ft clear roadside will reduce both the frequency and the severity of run-off-road type collisions.

On the other hand, if the question concerning countermeasure effectiveness is more specific and concerns "How much better is the countermeasure than the 'do nothing' alternative for a specific type of roadway or accident situation?" or, "How much better is one countermeasure than a similar countermeasure?" then the answer cannot be given with much certainty. For example, although it is logical that clear roadsides would be more beneficial on curves than on tangent highway sections because of the increased probability of a vehicle leaving the pavement, the difference between the effectiveness on curves and on tangent sections cannot be specified. In like fashion, the increase in benefit gained from clearing roadsides to a 30-ft width versus the benefit gained from clearing roadsides to only 20 ft (a much less expensive treatment) cannot be specified.

This latter type of information, incremental effectiveness for specific locations and vehicles, is now needed in the decision-making process. Policy makers face continually increasing needs coupled with increased treatment costs. This results in heavy reliance on economic analyses for all highway programs, including safety. These economic analyses usually involve some method for comparing the predicted benefits of a given treatment to its cost and then comparing this benefit-to-cost index for one treatment to other alternatives that could be funded. Such an approach is obviously necessary and justified.

During the last 5 years, significant emphasis has been placed on improving economic methodologies used in carrying out such analyses, but in almost every case, the accuracy of the economic methodology far exceeds the accuracy of the critical input variable--predicted level of effectiveness of the countermeasure. Without accurate inputs of predicted benefits, the outputs are often worthless.

WHAT IS KNOWN

Insufficient knowledge exists to predict with absolute certainty the benefits of all safety features. However, there is ample knowledge to make rational decisions in selecting safety features. It is the latter definition that is used in this section.

Roadside Hardware

It is perhaps easiest to determine the effectiveness of roadside hardware because these devices are designed to reduce accident severity. However, in most cases the initial effectiveness assessment of these devices is not the result of evaluation of full-scale implementation, but rather an assessment of crash test results. Therefore, devices are accepted for implementation before true effectiveness measurements.

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Crash Cushions

By far, one of the most effective devices to date has been the crash cushion. Studies have shown that crash cushions reduce fatalities and serious injuries by 75 percent (2). This device, which comes in a variety of designs, has been shown to be extremely effective in reducing fatalities at locations where object removal has been impossible (e.g., elevated bridge gores). The following designs are commonly used throughout the United States:

Steel Drum:	with cable guides; with or without side panels
Hi-Dro System:	with or without cable guides; with or without side panels
Hi-Dri System:	with cable guides and side panels
G-R-E-A-T Inertial System:	with or without side panels; with or without cable guides

Sign and Luminaire Supports

The next most effective device is the breakaway sign and luminaire support. Developed in the late 1960s, these devices have recently been studied for their effectiveness relative to very small passenger cars:

1. Shoe mount (no yielding)
2. Cast aluminum transformer base
3. Slip base
4. Frangible couplings
5. Shearbase

Results indicate a 30 percent reduction in injuries when breakaway supports are used. The effectiveness of breakaway luminaires is more dependent on impact speed than on vehicle weight (i.e., the higher the impact speed, the more effective the device regardless of vehicle weight), thus breakaway luminaires will not be effective when operating speeds are low (30 to 35 mph) (3).

Longitudinal Barriers

Research on longitudinal barriers has met with somewhat less success. In the early 1970s, the concrete safety shape was developed, tested, and redesigned. Since then it has become one of the most effective and widely used barriers in the United States. It is almost 100 percent effective in reducing barrier penetration/vaulting head-on accidents. Thus the severity of accidents has been reduced at locations where this barrier replaced other barriers. However, as is the case with most barriers, the number of accidents will increase if the barrier is installed where no barrier previously existed. Moreover, recent accident information indicated that longitudinal barriers may be a problem for very small vehicles (1,800 lb), causing them to roll over. This phenomenon is currently under study.

Other longitudinal barriers that have been successfully tested include the modified thrie beam (see Figure 1), which eliminates snagging for small vehicles, and the self-restoring guardrail (SERB), which can contain the entire range of vehicles (1,800-lb passenger car to 80,000-lb tractor-trailer). The longitudinal barrier also has a restoring action to reduce maintenance costs and to keep the barrier functional (Figure 2). This particular design has a high initial cost but has been shown to be most effective at high-accident locations. At four locations where the barrier is currently being evaluated, serious accidents have been eliminated. At least 60 impacts with the barriers have been observed with only 4 reported accidents and practically no maintenance.

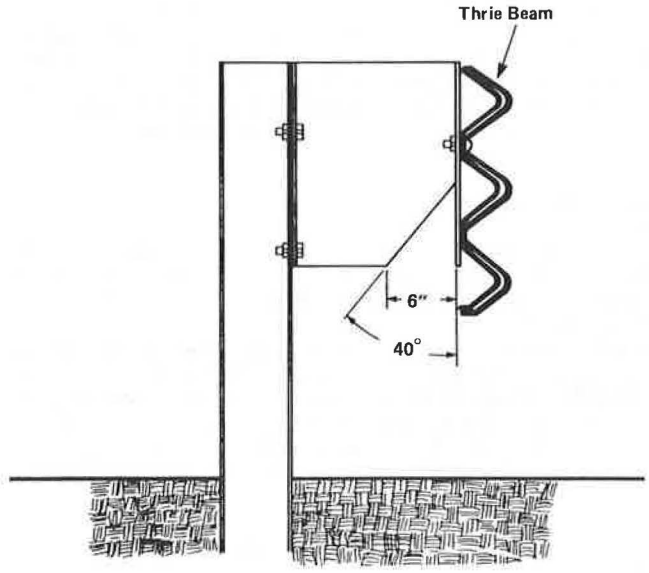

FIGURE 1 Modified thrie-beam guardrail.

The effectiveness of several devices has been discussed under the following assumptions: (a) the device has been installed where it has been needed, and (b) the device has been installed properly. As will be discussed later in this section, the biggest problem in the area of roadside accidents is the development of criteria to determine where and what type of device is warranted.

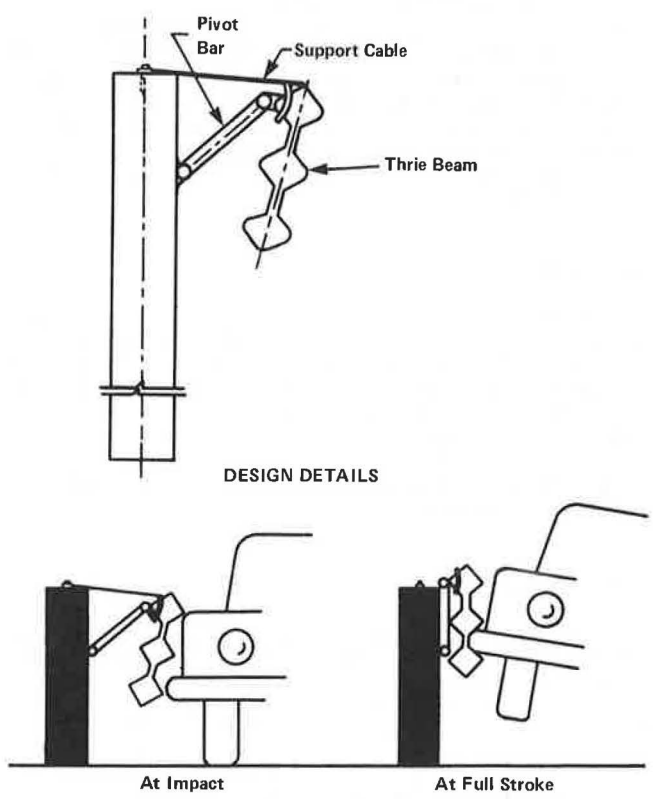

FIGURE 2 Self-restoring barrier (SERB) guardrail.

It appears to be much easier to determine the effectiveness of safety countermeasures whose objective is to ameliorate the effects of an accident rather than to prevent the accident from occurring. A discussion of the effectiveness of those items designed to reduce accident frequency follows.

Cross-Sectional Elements

During the past 10 years no group of items has received as much attention in the United States as cross-sectional elements (Figure 3). Considerable controversy has raged about the safety impacts of these items since the federal government agreed to participate in funding resurfacing, restoration, and rehabilitation (RRR) projects. The RRR program provided financial relief in the area of heavy maintenance. Before its institution, maintenance was strictly a state function with no federal funding.

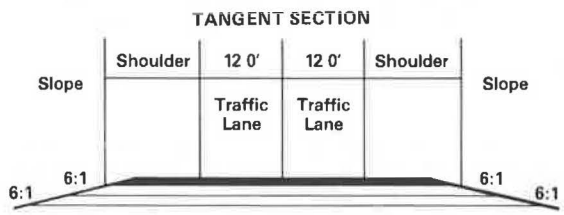

FIGURE 3 Cross-sectional elements.

With the addition of RRR work, certain groups contended that all geometric elements should be constructed to new construction standards (12-ft lanes, 8-ft shoulders, etc.), regardless of the traffic volume or the roadways' functional class. Others in the highway community (mainly state and local officials) contended that requirements to reconstruct all facilities would result in an unreasonable financial burden and would thwart the intention of the RRR program--that of maintaining the highway infrastructure. Currently, the National Academy of Sciences, under the direction of the Congress, is attempting to resolve this controversy.

The following sections are an assessment of geometric elements.

Lane Width

In general, 11-ft lanes provide the most appropriate balance between safety and traffic flow. This is true for all classes of highways where the percent of truck traffic does not exceed 8 percent. For facilities with truck traffic in excess of 8 percent and operating speeds in excess of 40 mph, 12-ft lanes should be used. Figure 4 shows the relationship between lane width and accident rates for two-lane rural highways (4).

Shoulder Type

When shoulders exist, particularly on high volume freeways, they should be paved. Other than access control, no geometric element has shown a more consistent relationship to safety (i.e., reduced accidents) than shoulder type. Estimates of accident reduction due to paved shoulders range from 1.3 accidents per year per 10,000 average daily traffic (ADT) for freeway noninterchange sections to 4 accidents per year for loop ramps at interchanges (5).

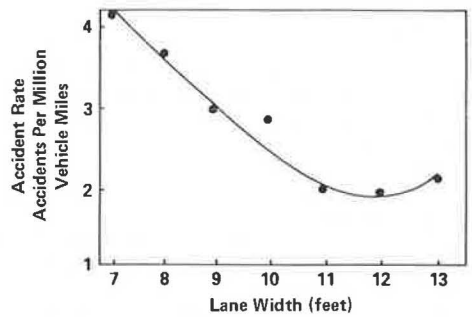

FIGURE 4 Relationship between lane width and accident rate on rural, two-lane roads.

Other Elements

Controversy still exists about shoulder width, sideslope, and horizontal and vertical curves. Most studies agree that shoulders up to 6 ft wide on facilities with greater than 1,000 ADT provide a safety benefit. The effect beyond 6 ft is not clear; existing studies conflict. Left shoulders on divided highways should not exceed 6 ft. Wider shoulders appear to encourage vehicle stopping on the left, which violates drivers' expectancy and causes safety problems.

Studies agree that slopes of 2:1 are dangerous and 10:1 are safe. Controversy still exists about slopes between 3:1 and 6:1. This area is particularly important when many miles of highways are being widened to improve safety. If insufficient information is available about slopes, the widening improvement may be causing safety problems because the existing slope will become steeper after the widening project.

The problems associated with horizontal and vertical curves are more complex. Studies agree that horizontal curves should be less than 3 degrees with vertical curves less than 6 percent. However, on low-volume, two-lane roadways, it is almost never cost-effective to redo highway alignment. The question then becomes "What is cost-effective to do?" It is the answer to this question that is being sought by the National Academy of Sciences.

TRAFFIC CONTROL DEVICES

By far, the least is known about traffic control devices and their effect on safety. For example, the traffic signal--installed to provide protected crossing maneuvers--invariably increases intersection accidents. However, it also eliminates the more serious angle and head-on accidents observed at uncontrolled intersections, and it smooths traffic flow--sometimes. Variations on traffic signal indications, timing, and phasing are more difficult to quantify because the changes are slight and the measure of effectiveness is sometimes too gross to detect change.

Traffic signs fall into the same class. Although some publications praise the cost-effectiveness of signs, there have been few evaluations of signs that have been properly documented. Thus, although it is intuitively believed that signs are effective, their specific effectiveness cannot always be demonstrated. Unfortunately, because misinformation on the effectiveness of signs has been widely distributed, and because signs are cheap and intuitively appealing, their use is widespread. In some cases signs are installed in lieu of other available, but perhaps more expensive, countermeasures.

The one type of traffic control device that has been adequately tested is pavement edge markings. In two studies reported in 1960 and 1961, both Ohio (6) and Kansas (7) demonstrated the effectiveness of pavement markings on two-lane rural highways. Both studies showed significant reductions in accidents (19 and 46 percent, respectively) at intersections when edge markings were used. Both studies were conducted with control sections.

Using comparison sections, additional studies have indicated significant benefit as a result of installation of centerline markings on low-volume, two-lane roadways (8).

WHAT IS UNKNOWN?

Illustrated in the previous section are some examples of what has been learned. Much remains to be learned and, in some cases, past knowledge has to be updated to reflect current trends, changes in technology, and improvements. For example, because of continuous changes in the motor vehicle population, engineers have had difficulty in specifying a design vehicle. Recently, the weight of new passenger cars has been decreasing with each model year, and today 25 percent of the vehicle population in the United States consists of vehicles less than 2,400 lb. Some of the other major gaps in knowledge include: roadside clear zones, guardrail location, guardrail end treatments, bridge rail design, luminaire/sign support design for small vehicles, breakaway utility poles, discontinuities at the edge of pavement (i.e., pavement edge-drop), and the general effectiveness of roadway signing, signalization, and illumination. As indicated earlier, geometric design countermeasures are currently undergoing review by the National Academy of Sciences. Until that review is completed, the authors are withholding judgment. Geometric issues of concern include horizontal curvature, sideslope design, bridge widths, and general intersection design.

Some explanation of these lists is certainly in order. At first glance, it is almost heresy for the authors to say that "we do not know the effects of roadway clear zones, guardrail location, or certain breakaway devices." The problem is that we know these countermeasures are effective, but we do not know how effective they are for specific situations. Roadway clear zones are unquestionably better than cluttered roadsides. As indicated earlier, for years roadside standards have cited the need for a 30-ft clear zone to allow errant vehicles to recover. There is little, if any, data on the differential effectiveness of the 30-ft clear zone over a 20- or 25-ft clear zone or for a 30-ft clear zone versus a partially clear roadside containing only small trees.

From the multitude of crash tests conducted, a tremendous amount of knowledge has been developed about the forces to the vehicles resulting from crashes into various guardrail designs, luminaire/sign support designs, bridge rail transition sections, guardrail terminals, and other hardware. Roadside hardware standards are continually enhanced and upgraded based on the results of such tests. Unfortunately, it is almost impossible to convert these g-force decreases to the vehicles to some meaningful measure of the decrease in predicted injury to the occupants. Little is known about what a decrease of five g's to the vehicle means in terms of the percent reduction in fatal injuries for a belted or unbelted occupant. Thus the determination of effectiveness still has to result from assessment following implementation.

Finally, where accident studies have been conducted, they are often conducted at high-accident locations. Bypassing the later discussed issue of the accuracy of effectiveness measured at such locations, there remains the troubling question of whether these results can be extrapolated or transferred to other situations. For example, reductions in the frequency or rates of fatalities or injuries derived from studies conducted in rural areas may or may not be transferable to urban areas where speeds are lower, driver behavior may be different, and accidents in general are less severe.

This issue of transferability of results is becoming more critical because of the shift in the vehicle fleet to much smaller passenger cars accompanied by a shift to larger and heavier cargo-carrying trucks. The overwhelming majority of crash tests and accident studies to date have been based on data sets from a fleet of larger (1,130 to 2,050 kg), more stable passenger cars and somewhat smaller and lighter trucks. Knowledge is just beginning to develop about which designs will not work with the smaller cars. For example, many of the guardrail terminal designs that appear to be quite adequate for larger passenger vehicles are now causing smaller passenger cars to ramp and roll over or to snag and be stopped violently.

It is thus apparent that (a) there are many areas in which adequate accident research has not been conducted to provide levels of effectiveness, and (b) there are certain countermeasures for which a way is yet to be found to convert information on changes in forces to the vehicle to meaningful measures of reducing the severity of injuries to occupants. In addition, even in areas where general effectiveness factors have been specified to some level of certainty, there remains the issue of transferability to other locations and to the smaller passenger vehicles.

WHY IT IS UNKNOWN

Gaps in knowledge concerning the effectiveness of countermeasures are the result of a number of different causes, most of which are under the control of the researcher and research administrator.

One of the basic causes of the lack of good effectiveness measures is the propensity on the part of roadway researchers to use less than adequate study design. Unfortunately, the study design that has been used most often in the past is also the design which, in many cases, provides either little or no sound information related to countermeasure effectiveness--the simple before-after design.

In this design, data are collected for a short period before the implementation of the countermeasure and are compared directly with data collected from a similar period following implementation of the countermeasure. This design is easy to implement, it requires little planning on the part of the researcher because it can be implemented at any time (even after the treatment has been implemented), it appears logical, and it has a long history of use in the field. Unfortunately, the design often produces results that have little relationship to reality. The problem is compounded when the design is used in evaluating a treatment that has been applied to a high-accident location.

The problems with the design have been discussed by many authors (9). Briefly, the major problems include the following:

1. Many other causes for a measured change often occur at the same time as the treatment, making it virtually impossible to ascertain the true cause of the change;
2. Underlying long-term trends in accident rates

can either disguise a true effect or produce a false effect; and

3. The ever-present threat of regression-to-the-mean in which high-accident locations, as defined by elevated accident rates during a short period, will usually improve, with or without treatment.

In contrast, little use has been made of stronger experimental designs that often require (a) planning before being implemented and (b) definition of control or comparison groups of sites where the treatment is withheld. A variety of reasons for not using these stronger designs are cited by researchers:

1. The need for short-term results (successes). A primary reason for using the simple before-after design is that it allows the researcher to use short periods of data collection to draw what appear to be accurate estimates of benefit. Because there is continual pressure on the research community to produce short-term results for policy makers, this design often appears to be the only answer. Realistically, what the policy maker is looking for are successes resulting from his or her decisions. And unfortunately, because of its nature, the before-after design often tends to produce a very optimistic picture of the benefits of a particular countermeasure, particularly if the countermeasure has been implemented at a high-accident location.

2. Quality of the data. Police accident reports are the usual source of accident data but often have inadequate information. Thus, many researchers argue that there is no need to use a strong design because the data are so poor. Unfortunately, the use of a weak design with poor data only compounds the problem.

3. Difficulties in establishing control groups. It is much more difficult to establish a nontreated control group ahead of time, or even to identify a good comparison group after the fact than it is to simply implement a treatment and look at the sites treated.

4. Legal/political reasons for the lack of control groups. Finally, the current climate in the United States toward increased litigation has affected roadway research by providing another reason for not using control groups. There is a cited fear among both state administrators and some researchers that if they were to withhold treatment from a control group and if the treatment ultimately proved to be a success, then those persons involved in accidents at control sites might well sue the implementing agency for its lack of implementation. Although no such case has yet entered the court system, this fear provides administrators a reason not to institute control groups for a treatment that they believe may have a beneficial effect.

The preceding impediments to the use of sound study designs are directly related to the nature of the designs themselves. Other factors also influence the design used.

The Congress requires a yearly report from the Secretary, U.S. Department of Transportation, on the progress of the states in implementing the hazard elimination and pavement marking program. The report must include ". . . number of projects . . means and methods used, and the previous and subsequent accident experience at improved locations" (10). To meet this requirement, each state is required to report the costs and safety benefits of their safety improvements. The states, because of limited available dollars to determine the effectiveness of improvements, have chosen to use the easiest possible design (before/after).

A movement is underway within FHWA to change this. During the past 2 years, some states have been allowed to trade off a large number of before-after studies for a limited number of in-depth studies. National coordination of this activity would make it possible to design evaluations with control or comparison sites. For example, each state could pick one countermeasure to evaluate for a specific time period, for example, 5 years. Studies would be carefully designed and countermeasure installation and data collection (specified in study design) would be done by states. Data from several states could be pooled and analyzed. States would no longer have to evaluate a large number of safety improvements, and the state of knowledge on countermeasure effectiveness would be significantly improved. For each 5-year period, approximately 10 countermeasures could be evaluated. Such a coordinated effort would significantly improve the state of knowledge in this area by permitting an accumulation of a listing of reduction factors and an updating of these reduction factors as new research is conducted.

Perhaps the major reason for poor safety research in the United States is the researchers themselves. For lack of a better definition, this could simply be characterized as a lack of peer pressure to conduct good research. For example, there have been numerous syntheses of research in certain areas. However, these syntheses tend only to repeat results presented by the original authors, with no judgment provided concerning the accuracy of the estimates. There have been few critical reviews of past studies, and even when critical reviews have been attempted, the findings have often conflicted. It appears that the problem is finding researchers who are knowledgeable about the area being researched, the proper use of research methods and statistics, and who have the status and the fortitude to criticize poor work done by their peers. Indeed, even the critical review procedures, established journals, and safety conferences are not very discriminating. Poor data, research designs, and interpretations are still presented at major national conferences and in engineering research journals.

It is encouraging to note that there are now attempts to rectify the situation. Approximately 20 individuals involved in highway research have petitioned the Transportation Research Board to remove the names of authors and organizations from papers submitted for review. It is believed that this change will provide a more objective review process. It is the authors' belief that the U.S. research community should encourage pointed and direct questioning and challenging of research studies. Pressure from peers can only tend to make a researcher more careful.

Finally, as noted earlier, at least one major cause exists for a lack of knowledge over which no group has control--the rapidly changing vehicle fleet. The effect of this change will continue to affect the benefits of highway countermeasures that were designed for larger passenger cars and smaller trucks. Here, the major gaps in knowledge are a result of an inability to keep pace with changes in the overall system so that the benefits of improvements can be accurately predicted.

CONCLUSIONS

In summary, through years of research efforts, the U.S. highway safety community has developed significant knowledge concerning certain countermeasures. Decisions concerning crash cushions, sign and luminaire supports, median barriers, lane widths, paved shoulders, pavement edge markings, and in some cases, intersections signalization can be made with

some certainty. Much less is known about other countermeasures, particularly where the information needed is a specific level of effectiveness for use in economic analyses. There are gaps in our knowledge about the effectiveness of horizontal and vertical curvature, side slope design, pavement edge treatments, certain roadside clear-zone configurations, guardrails, guardrail end treatments, bridge widths and bridge rail designs, breakaway utility poles, and others. In addition, the shift to smaller passenger cars and larger trucks is resulting in new unknowns.

The highway field needs to utilize improved methods of evaluating safety features, provide continual review and updating of effectiveness factors, and increase "peer pressure" in the research community to upgrade research efforts.

The safety community must continually strive to overcome these problems by using the existing methodological knowledge, by building on what has been learned in the past, and, most important, by making poor research unacceptable.

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Publication of this paper sponsored by Committee on Methodology for Evaluating Highway Improvements.

New Directions for Learning About the Safety Effect of Measures

EZRA HAUER and JANE LOVELL

ABSTRACT

Much of what is known about the safety effect of various measures must be extracted from implementations in real life rather than from experiments that are staged to meet the dicta of rigorous scientific experimental design. The tools for extracting usable knowledge from data must be tailored to suit this reality. Methods of estimation that appear well suited for this task are reported here. First it is shown that what is commonly done is incorrect; it is incorrect to compare the count of "before" accidents with the count of "after" accidents and from this to draw conclusions about the safety effect of a measure. A simple method is provided for the correct analysis of "before" and "after" data. Next the likelihood function is introduced; it serves a dual purpose: First, it allows the assessment of the accuracy with which the safety effect is known. Second, it is a coherent formal device by which results from diverse studies can be accumulated. The ability to accumulate empirical evidence from many small studies is the key to progress in research on safety. The test of the advocated methods is in application; in this case, the examination of the effect on intersection safety of a change from two-way to multiway stop control. Details are given in two companion papers appearing elsewhere in this Record.

What is known about the safety effect of some treatment or measure is based mostly on data derived from instances of implementation. The implementation of a real measure is usually fashioned by the circumstances of the real world and only seldom by the requirements of scientific experimental design. For measures that are in the orbit of highway or traffic engineering, most data come in the form of before and after accident counts, perhaps supplemented by the corresponding changes in exposure. This is why much of the traditional knowledge about the effect of such measures is based on before-and-after studies.

The before-and-after study is almost always too small to be statistically conclusive. It is also vulnerable to a variety of threats to the validity of the inferences that the data permit. This is why the purist will often refuse to consider evidence based on uncontrolled studies of this kind. However, it helps little to bemoan the fact that the before-and-after experimental design is subject to threats and that its results are not statistically significant. The challenge is to devise methods that minimize such threats and that allow this ubiquitous source of information to be used constructively.

In this paper the authors report on an approach that appears well suited for the task of extracting useful information from uncontrolled before-and-after studies and that facilitates the accumulation of empirical evidence from diverse studies, each of which, standing alone, may be inconclusive.

Even though the focus of this paper is methodological, the authors rely more on common sense and intuition than on mathematical formalism; in this way they hope to convince a wide readership that it is unsound and therefore unprofessional to draw conclusions about the safety effect of a measure from a simple comparison of accidents before to accidents after (even when changes in exposure and the secular time-trend are taken into account). This is discussed next. In the subsequent section the correct methods of analysis are discussed. Later the concept of the likelihood function is introduced and its attraction and use are explained. In the final section of the paper accomplishments are reviewed and some of the problems yet unresolved are discussed.

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WHY IT IS INCORRECT TO COMPARE THE COUNT OF "BEFORE" ACCIDENTS WITH THE COUNT OF "AFTER" ACCIDENTS

A typical before-and-after study follows a simple pattern: at some time a measure (treatment) that affects safety is implemented on a few entities. Entities may be intersections or drivers, cities, road sections, or vehicles. The count of accidents on these entities before treatment is compared with the record of accident occurrence after treatment. On the basis of such a comparison, inferences are made about the effect of the measure or treatment.

The mind is so accustomed to this kind of comparison that the logic behind it is seldom examined; a crucial assumption that turns out to be incorrect is overlooked. It is not incorrect because of some theoretical niceties but because it is contradicted by mountains of empirical evidence. To recognize the faulty assumption it must be spelled out.

To learn about the effect of the treatment, what would have happened during the after period had the treatment not been implemented is compared with what actually has happened during the after period with the treatment in place.

This simple logical construct is behind all experimental designs, no matter how sophisticated or how simple. It can never be known "what would have happened ...." To avoid this difficulty the tendency is to assume that

> What has happened during the period before treatment implementation is a good indication of what would have happened during the after period had the treatment not been implemented.

This is the assumption the authors claim to be contrary to empirical fact. Only one piece of empirical evidence is given here; however, the authors have examined literally dozens of data sets and every one of those sets corroborates the conclusion that the aforementioned assumption is incorrect. The reader is invited to furnish his or her own evidence. All that is needed is at least 2 years of accident data about several hundred entities that remained largely unchanged. Such data are easy to find. When the data are examined, as in the following example, the conclusion is inescapable.

Consider the entries in Table 1. The table is based on the count of accidents occurring during the years 1974 and 1975 at 1,142 intersections in San Francisco. All intersections in this population had stop signs on the two approaches carrying the lesser flows and remained virtually unchanged in these 2 years. Column 1 gives the number of intersections [n(x)] on which the count of accidents in 1974 was x = 0, 1, 2... as shown in Column 2. Column 3 gives the average of the count of accidents [M(x)] for the same n(x) intersections during 1975.

TABLE 1 Accident Count at 1,142 Intersections, 1974-1975

1 Number of Intersections [n(x)]	2 Number of Accidents per Intersection in 1974 [x]	3 Average Number of Accidents per Intersection in 1975 [M(x)]
553	0	0.54
296	1	0.97
144	2	1.53
65	3	1.97
31	4	2.10
21	5	3.24
9	6	5.67
13	7	4.69
5	8	3.80
2	9	6.50

Note: Two intersections had 13 accidents, one had 16.

Were the assumption correct, it should be observed that if an intersection registered, for example, x = 3 accidents in 1974 and if it remained largely unchanged, it should record, on the average, three accidents in 1975. However, inspection of Table 1 reveals that intersections that registered three accidents in 1974, registered 1.97 accidents on the average in 1975. Similar discrepancies between the entries of Columns 2 and 3 exist for all values of x (except for x = 1, which will turn out to be the rule-confirming exception). These discrepancies cannot be reasonably attributed to chance; nor are they likely to reflect a sudden, large, and peculiarly systematic change between these 2 years. (The total number of accidents at these intersections was 1,253 in 1974 and 1,216 in 1975). It must be concluded therefore, that in this case the 1974 count of accidents is not a good indication of the average count in 1975 for any value of x (except for x = 1). Therefore, the accident count "before" is a systematically bad guess of what would have happened after.

Tables 2 and 3 give similar information for the same 1,142 intersections during the pairs of years 1975-1976 and 1976-1977. The preceding conclusion remains unchanged. It follows that there was nothing unique or peculiar about the years 1974-1975 (Table 1); what happened "before" did not prove to be a good indication of what happened after in 1975-1976 and 1976-1977 either. It is worth noting that there is a pronounced similarity between the corresponding entries of the third columns in the three tables. This regularity will be explored in the section: How to Analyze Before-and-After Data.

TABLE 2 Accident Counts at 1,142 Intersections, 1975-1976

1 Number of Intersections [n(x)]	2 Number of Accidents per Intersection in 1975 [x]	3 Average Number of Accidents per Intersection in 1976 [M(x)]
559	0	0.55
286	1	0.98
144	2	1.41
73	3	1.82
35	4	1.97
18	5	2.50
11	6	3.91
9	7	4.22
3	8	2.00
1	9	3.00
2	10	2.50
1	11	5.00

TABLE 3 Accident Counts at 1,142 Intersections, 1976-1977

1 Number of Intersections [n(x)]	2 Number of Accidents per Intersection in 1976 [x]	3 Average Number of Accidents per Intersection in 1977 [M(x)]
562	0	0.53
287	1	0.94
155	2	1.37
74	3	1.72
33	4	2.61
13	5	3.00
11	6	2.64
4	7	2.25
1	8	1.00
2	9	3.50

The results in Tables 1, 2, and 3 are not an exception or aberration. They are used here merely to illustrate a general phenomenon found in many other data sets. Based on diverse and ample empirical evidence it can be concluded that the assumption (what has happened during the period before treatment implementation is a good indication of what "would have happened during the after period had the treatment not been implemented") is contrary to empirical fact and is therefore wrong.

Because the assumption on which the simple before-and-after comparison is based is incorrect, so must be conclusions drawn from such comparisons. To illustrate, consider a site (similar to those in Table 1) that recorded, for example, three accidents before treatment and one accident during a corresponding period after treatment. The incorrect comparison is between three and one. It is clear from Tables 1, 2, and 3 that sites that record three accidents in the before period, when left untreated, record approximately two accidents in the after period. Therefore, the correct comparison is between two and one accidents. To be sure, no conclusions will be drawn from a few accidents recorded at one site. Accident counts from many sites will usually be added and the sums compared. However, if every term in the addition is incorrect, so will be the sum. The errors in the sum

of before accidents will cancel only if treatment is administered at random and is implemented at very many sites. In practice, the number of treated sites is limited and professionals do not usually treat sites at random.

It is concluded, therefore, that it is incorrect to compare the count of before accidents to the count of after accidents as is common practice in before-and-after studies. A valid comparison requires that there be a way to estimate "what would have happened had the treatment not been implemented," which is in accord with empirical fact. How to obtain such estimates is described next.

HOW TO ANALYZE BEFORE-AND-AFTER DATA

The task is to obtain a good estimator to replace that which is in common use but is shown to be faulty. One wishes to estimate the number of accidents expected to be recorded during the after period had the treatment not been implemented if, during the before period, the entity recorded x accidents. The symbol $\xi(x)$ will be used to denote an estimator. It turns out that there are several candidate estimators of which two $[\xi_1(x), \xi_2(x)]$ are recommended for use in practice.

One obvious option is to use $\xi(x) = M(x)$ (see Column 3 in Tables 1-3). The symbol M(x) stands for "average after-period count of accidents on those entities that recorded x accidents in the before period and were left without treatment." The use of $\xi(x) = M(x)$ amounts to stating: "It is expected that had the treated entity, which in the before period recorded x accidents, been left untreated, it would have recorded during the after period, on the average, what has in fact materialized on similar entities that were left untreated." In essence, the entities with x before accidents, which were left untreated, are regarded as a control group.

The trouble with $\xi(x) = M(x)$ is that, to have an accurate estimate, a sufficient number of "similar entities" have to be found that during the before period had the same number of accidents as the treated entity but that were left without treatment. This is often difficult to do. Ordinarily, it is the entities with many accidents that are treated, and there are not many such entities to begin with. Once some entities have been treated, only a few remain for the calculation of M(x). This difficulty is easy to see in Tables 1-3. In the lower reaches of these tables (where few intersections are used to calculate the average) the values of M(x) fluctuate widely. Furthermore, were some of these intersections treated, even fewer could be used for the calculation of the M(x), making it even less reliable. This renders the estimator M(x) of little use in practice.

Another estimator that can be justified on theoretical grounds (1) is $\xi(x) = (x+1) \cdot n(x+1)/n(x)$. The advantage of this estimator is that only data about accidents occurring during the before period are needed. However, because in the lower reaches of Tables 1-3, the n(x) are small, the ratio $n(x+1)/n(x)$ is subject to vagaries of chance that are similar to those that plague M(x). Thus, the problem is how to smooth out the random fluctuations that plague both estimators. Two sensible ways to obtain smooth estimates are discussed in the following paragraph.

First, a continuous function $\xi_1(x)$ can be fit to the points $(x+1) \cdot n(x+1)/n(x)$. Thus, using the data in Columns 1 and 2 of Table 1, values of $(x+1) \cdot n(x+1)/n(x)$ for x = 0, 1, ..., 8 were calculated. These are the ordinates of the points in Figure 1. The bars around each point designate one standard deviation. In this case, a straight line appears to be the

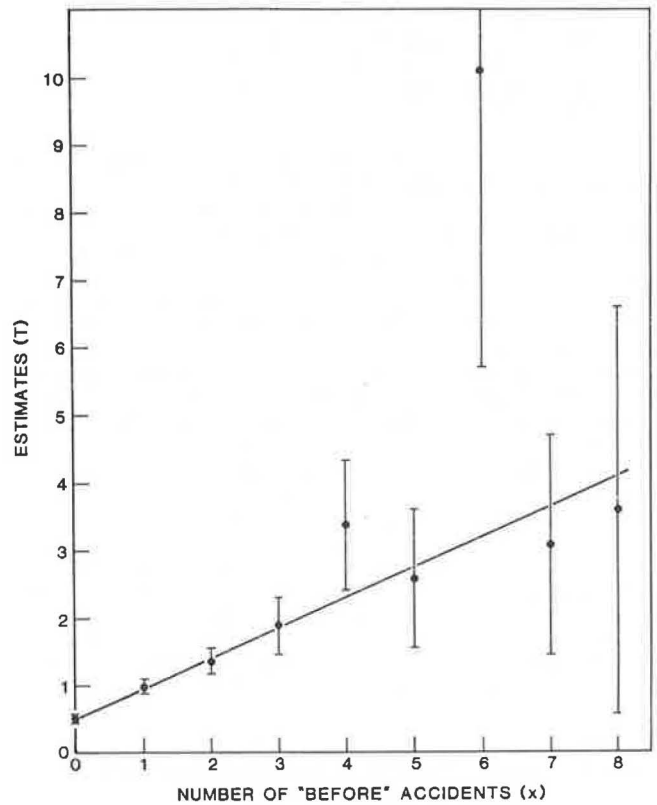

FIGURE 1 Least-squares fit of a linear function $\xi_1(x)$ to points $(x+1) \cdot n(x+1)/n(x)$ based on data in Table 1.

sensible choice of a function to fit the data points. The ordinate of the fitted function at x = 0, 1, 2... is the estimate $\xi_1(x)$. When fitting a smooth function to the data points two technical issues must be given attention. First, the data points have different standard deviations. When fitting a curve, each point is to be weighed in inverse proportion to its variance, which is estimated by $[(x+1) \cdot n(x+1)/n(x)]^2 \cdot [1/n(x+1) + 1/n(x)]$. Second, for reasons of logical consistency, one would like to ensure that $\Sigma[\xi_1(x) \cdot n(x)] = \Sigma[x \cdot n(x)]$ when the summation is over all values x.

Except for these guidelines to curve fitting, the approach is perfectly general and requires no assumptions. It consists of two basic steps: (a) values of data points $(x+1) \cdot n(x+1)/n(x)$ for x = 0, 1, 2... are calculated and plotted, and (b) a legitimate function $\xi_1(x)$ is selected and fitted to the data points.

If a linear fit to the data points appears sensible, the task of curve fitting may be replaced by a much simpler and more transparent estimator, $\xi_2(x)$. First, the sample mean and sample variance are calculated by using

$$\bar{x} = \Sigma[x \cdot n(x)]/\Sigma n(x) \quad (1)$$

$$s^2 = \Sigma[(\bar{x}-x)^2 \cdot n(x)]/\Sigma n(x) \quad (2)$$

Then, by using $\bar{x}$ and s^2

$$\xi_2(x) = x + (\bar{x}/s^2) \cdot (\bar{x}-x) \quad (3)$$

Equation 3 is not magic nor does it contain "fudge factors." It is a rigorous result obtained by deduc-

tion, and it holds under broad conditions described elsewhere (2). Its main appeal is simplicity in use and clarity in interpretation.

The two terms of the sum in Equation 3 have recognizable meaning. The first term is the count of before accidents; the second term is a correction for regression-to-the-mean. The larger the difference between the count of before accidents (x) and its mean in the population of similar entities ($\bar{x}$), the larger is the correction required. It is positive when $x < \bar{x}$ (see first line in Table 1), negligible when $x \cong \bar{x}$ (see second line in Table 1) and negative when $x > \bar{x}$ (see lines below line 2 in Table 1).

The role of the sample mean-to-variance ratio ($\bar{x}/s^2$) is also interesting to examine. If it was known that all entities in the population had the same expected number of accidents (and if accident occurrence obeys the Poisson probability law), the ratio would approach 1. Under such conditions Equation 3 instructs that the expected number of accidents for a specific entity be estimated by $\bar{x}$ (not by x !). On reflection, this is as should be. If, on the other hand, the entities in the population are very different in terms of their expected number of accidents, $s^2 >> \bar{x}$, the correction will be small. In this case, $\xi_2(x)$ is very close to x. This is also as should be.

Before summarizing, it is of interest to examine in Table 4 the performance of the candidate estimators on the basis of the data in Table 1.

TABLE 4 Comparison of Estimates

1 [n(x)]	2 [x]	3 M(x)	4 (x+1)n(x+1)/ n(x)	5 $\xi_1(x)$ Curve Fit	6 $\xi_2(x)$ Eqn. 3
553	0	0.54	0.54	0.53	0.44
296	1	0.97	0.97	0.98	1.04
144	2	1.53	1.35	1.43	1.64
65	3	1.97	1.91	1.88	2.24
31	4	2.10	3.39	2.32	2.84
21	5	3.24	2.57	2.77	3.44
9	6	5.67	10.11	3.22	4.04
13	7	4.69	3.08	3.67	4.64
5	8	3.80	3.60	4.11	5.25
2	9	6.50	n.a.	4.56	5.85

A few points deserve mention. First, having established earlier that it is incorrect to use the raw number of before accidents in before-and-after comparisons, it was necessary to show what should be used instead. Several candidate methods of estimation have been presented. No matter which estimator is used, all correct estimates differ from the raw number of before accidents, which have been shown to be systematically biased.

Second, Column 3 in Table 4 is M(x) and therefore indicates what actually happened during the after period for entities that were left untreated. Where M(x) is a reliable average it could be used as a yardstick against which to judge the performance of $\xi_1(x)$ and $\xi_2(x)$. These estimates are observed to approximate the entries in Column 3 with varying degrees of success. The agreement is good in the upper part of Table 4 where the entries of Column 3 are quite accurate. Not much can be made of the discrepancies in the lower part of the table because here the entries of Column 3 (being averages over only a few intersections) are unreliable.

Third, in the authors' view, either $\xi_1(x)$ or $\xi_2(x)$ should be used because in the domain of interest they smooth out some of the fluctuations due to randomness. When the plot of points indicates a nonlinear fit, use least-squares curve fitting to find $\xi_1(x)$; otherwise use Equation 3 to obtain $\xi_2(x)$.

It remains to be demonstrated how these results are to be used in the context of a before-and-after study. This is done by example. Assume that 49 intersections (similar to those used to construct Table 1) were converted from two-way to four-way stop control. (Data and estimates are summarized in Table 5.)

Row 2 in Table 5 gives the number of intersections, which, during the before period had the number of accidents listed in Row 1. Row 3 gives the number of accidents for each group of intersections. Thus, for example, the 8 intersections, which during the before period recorded 2 accidents each, had together 16 accidents. During the before period there were 172 accidents at the 49 sites. Row 4 gives the number of accidents during the corresponding after period, which totaled 50. The authors argued that it is wrong to compare 172 to 50. Row 5 gives an estimate (here $\xi_2(x)$ from Table 4) of the number of accidents that should be expected had the intersections not been converted to four-way stop control. In this illustration, the changes in exposure and the secular trend in accidents is disregarded. Complete details are given in the paper "The Safety Effect of Conversion to All-Way Stop Control" elsewhere in this Record. In Row 6 the number of accidents in each group of intersections that should be expected had they remained unconverted has been calculated. Thus the seven intersections, which during the before period recorded no accidents, should be expected to record 7 x 0.44 = 3.1 accidents during the after period had they remained with two-way stop control. The sum of these expected accidents is 124.8. The effectiveness of the conversion should be judged by comparing what "would have happened without treatment" (124.8) with what actually transpired (50 accidents). In this numerical example it is estimated that the effect of conversion to all-way stop control was to reduce the expected number of accidents by 60 percent [= 100 x (124.8-50)/124.8].

THE LIKELIHOOD FUNCTION AND ITS USE

Point estimates of the type mentioned at the end of the previous section (a 60 percent reduction in expected accidents) are often the main figure of merit when it comes to practical decisions. However, for sound decisions it is necessary to have, in addition to the point estimate, a good idea about the uncertainty surrounding it. Unfortunately, real-life studies are almost without fail small in the sense

TABLE 5 Before-and-After Comparison

1	No. of before accidents per intersection	0	1	2	3	4	5	6	7	8	9	10	Row sums
2	No. of such intersections in sample	7	6	8	7	4	6	4	3	0	2	2	49
3	No. of before accidents (1x2)	0	6	16	21	16	30	24	21	–	18	20	172
4	No. of after accidents	2	3	7	5	4	12	6	5	–	2	4	50
5	Estimate $\xi_2(x)$ from Table 4	0.44	1.04	1.64	2.24	2.84	3.44	4.04	4.64	–	5.85	6.45	
6	No. expected w/o treatment (5x2)	3.1	6.2	13.1	15.7	11.4	20.6	16.2	13.9	–	11.7	12.9	124.8

that the estimates of safety effect derived from them are inaccurate. Also, taken singly, such estimates are only a frail guide for sound decisions. Thus, to make progress it is necessary to combine the information contained in many small studies in order for reliable knowledge to gradually emerge. The likelihood function is proposed for both purposes: (a) to characterize the accuracy with which the safety effect of a measure is known, and (b) to accumulate information obtained from diverse studies. It is best to postpone giving the reasons for this choice until after the use of the likelihood function in this context is explained.

To illustrate the use and interpretation of the likelihood function, another numerical example is introduced. It is concerned with the safety effect on right-angle accidents of converting 10 rural intersections in Michigan to all-way stop control (4). The data for the likelihood function are given in Table 6.

TABLE 6 Data for the Likelihood Function for Michigan Right-Angle Accidents (4)

Site	α_i	β_i	$(\epsilon'/\epsilon)_i$	x_i Accidents Before	x_i' Accidents After	B_i Years Before	A_i Years After
1	1.5603	0.1434	1.2237	14	6	3	3
2	1.6187	0.1457	1.0657	16	3	3	3
3	1.5603	0.1434	1.0189	18	9	3	3
4	1.5603	0.1434	1.0549	28	7	3	3
5	1.5603	0.1434	1.1387	15	3	3	3
6	1.4733	0.1339	1.0643	28	1	3	3
7	1.7044	0.1592	0.9976	4	0	2	2
8	1.5603	0.1434	0.8642	1	3	3	3
9	1.7044	0.1597	0.9659	6	2	2	2
10	1.4733	0.1339	1.0069	6	2	3	3

In Table 6, $\hat{\alpha}_i$, $\hat{\beta}_i$ are estimates of parameters given by

$$\hat{\alpha}_i = \bar{x}_i/(s_i^2 - \bar{x}_i)$$

$$\hat{\beta}_i = \bar{x}_i^2/(s_i^2 - \bar{x}_i) \qquad (4)$$

where $\bar{x}_i$ and s_i^2 are the sample mean and variance of the number of before accidents in the population of entities of which the treated entity i is a part. $(\epsilon'/\epsilon)_i$ is the ratio of exposures of the after to the before period for entity i. x_i is the number of accidents (of a certain type) occurring on entity i (i = 1, 2, ..., n) during a before period that for this entity is B_i years long. x_i' is the number of accidents (of the same type) occurring on entity i during an after period which, for this entity, is A_i years long.

The likelihood function for this case can be written as

$$L(\theta) = \prod_{i=1}^{n} \theta^{x_i'} [B_i + \alpha_i + (\epsilon'/\epsilon)_i A_i \theta]^{-(x_i+\beta+x_i')} \qquad (5)$$

The variable θ serves here as the index of safety effect. If a measure reduces the expected number of accidents to, for example, 90 percent of its previous value, θ = 0.90. If it causes an increase of 5 percent, θ = 1.05. (The detailed derivation of Equation 5 may be found in study by Hauer et al. (4).

Using the entries in Table 6, the likelihood function (Equation 5) takes on the form

$$\begin{aligned} L(\theta) = {} & \theta^6 [4.5603 + (3 \times 1.2237\theta)]^{-20.14} \\ & \theta^3 [4.6187 + (3 \times 1.0657\theta)]^{-19.15} \\ & \theta^9 [4.5603 + (3 \times 1.0189\theta)]^{-27.14} \\ & \theta^7 [4.5603 + (3 \times 1.0549\theta)]^{-35.14} \\ & \theta^3 [4.5603 + (3 \times 1.1387\theta)]^{-18.14} \\ & \theta^1 [4.4733 + (3 \times 1.0643\theta)]^{-29.13} \\ & \theta^0 [3.7044 + (2 \times 0.9976\theta)]^{-4.16} \\ & \theta^3 [4.5603 + (3 \times 0.8642\theta)]^{-4.14} \\ & \theta^2 [3.7044 + (2 \times 0.9659\theta)]^{-8.16} \\ & \theta^2 [4.4733 + (3 \times 1.0069\theta)]^{-8.13} \end{aligned} \qquad (6)$$

Each line in Equation 6 corresponds to one of the 10 sites and thus to one row of Table 6.

With the stage set the meaning and use of the likelihood function can be discussed. The likelihood function has two important properties: (a) it preserves, in a condensed form, the entire information content of the data, and (b) it makes the merging of information contained in separate data sets simple. Thus, for example, the first line in Equation 6 captures all that can be learned (about the safety effect on right-angle accidents of conversion from two- to four-way stop control) from what has been observed at Site 1 alone. The corresponding likelihood function is shown by curve A in Figure 2.

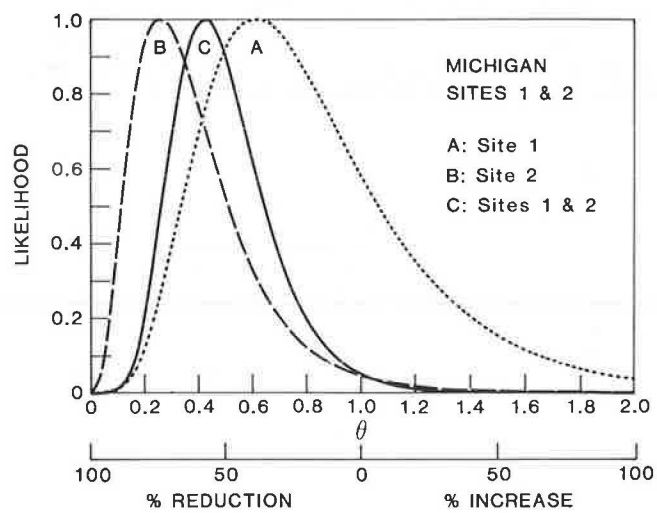

FIGURE 2 Likelihood functions and their combination.

The ordinate for a certain value of θ is proportional to the probability of recording 14 right-angle accidents during a 3-year before period and 6 right-angle accidents during a 3-year after period if that index of safety effect (θ) actually prevailed. The larger this probability, the more likely is the value of θ said to be. Values of θ for which the likelihood is small compared to its largest value (scaled to be equal to 1) are deemed unlikely.

The information contained in a reduction in number of accidents from 14 to 6 [when α, β, and (ϵ'/ϵ) are as in line 1 of Table 6] is meager. This is reflected in Figure 2 by the fact that curve A is quite flat near its peak, and a wide range of θs has likelihoods that are not much lower than 1. Although it is meager, whatever information the 14- to 6-accident reduction contains is now preserved. In a similar manner the second line in Equation 6 preserves all the information that can be extracted from the accident history of Site 2. The likelihood function for Site 2 is shown in Figure 2 by curve B.

How can the results from Sites 1 and 2 be combined? As indicated by Equation 5 (or 6), the ordi-

nate of the joint likelihood function is proportional to the product of the two component ordinates. For computational convenience, the sum of the logarithms is used. The joint likelihood function for Sites 1 and 2 is shown by curve C in Figure 2. It represents all that can be learned from the data of Sites 1 and 2 taken together.

To complete the illustration, imagine one study encompassing Sites 1 to 4 and a later study encompassing Sites 5 to 10. The likelihood function for the first study is shown in Figure 3 by curve A. When, at some later time, data from Sites 5 to 10 become available (curve B), the two data sets can be combined to yield the joint likelihood function for all 10 sites (curve C).

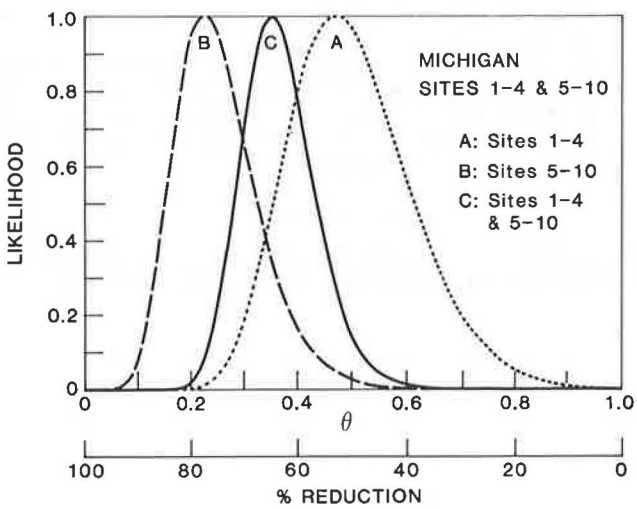

FIGURE 3 Likelihood functions for right-angle accidents at 10 Michigan intersections.

The reasons for choosing the likelihood function to represent, preserve, and accumulate information about the safety effect of a measure are now clear. The likelihood function (a) identifies the most likely value of θ and represents the uncertainty surrounding it in an intuitively clear fashion; (b) preserves in condensed form all that can be extracted from a data set; (c) represents a structured process for the accumulation of information and learning from experience. At any point in time it represents the current state of knowledge. When new data become available, the corresponding likelihood function is used to revise the existing data and to create a new (current) state of knowledge; and (d) facilitates the use of formal decision analysis and is an essential ingredient for making coherent decisions.

With all its merits, routine use of the likelihood function to combine information extracted from diverse data sets is not free of difficulties. The central question (presently unresolved) can be explained with reference to Figure 3. Is there some real difference between the group of Sites 1-4 (curve A) and the group of sites 5-10 (curve B), which is the reason why the same treatment (conversion to four-way stop control) may affect the safety of both groups differently?

If there is such a difference, the two likelihood functions should not be fused into curve C. Rather, an attempt should be made to describe the difference. Thus when conversion to four-way stop control for another intersection is contemplated, one will be in a position to assess whether curve A or curve B applies. If the treatment effect varies randomly from site to site, curves A and B should be fused into curve C. In this case, curve C properly represents the uncertainty surrounding the estimate of the average safety effect of the treatment. It is the role of further research to shed light on this important and difficult question.

SUMMARY AND DISCUSSION

The authors have attempted to devise a methodology that facilitates the extraction of useful information from real-world instances of treatment implementation. Such instances are the predominant source of information about the safety effect of highway and traffic engineering measures. Therefore, the methodology devised here appears particularly suited for the creation of substantial knowledge in this field.

A simple comparison of before-and-after accident counts is shown to be incorrect even if corrections for exposure and secular trend are applied. Inasmuch as most of the traffic and highway engineering traditional knowledge about the effect of safety measures is based on such simple (and incorrect) before-and-after comparisons, a wholesale revision of this body of knowledge is in order.

Two smoothed estimates, $[\xi_1(x)$ and $\xi_2(x)]$, are recommended for use. To calculate their values, some additional data are required. The needed additional data are the count of before accidents on all similar entities.

What constitutes a similar entity? The answer that the analyst gives to this question influences the estimate and therefore introduces into the analysis an element of the arbitrary. Procedures that allow the analyst some freedom of choice tend to be viewed with suspicion. Two arguments can be raised in defense.

In practice the determination of what constitutes a sensible choice of the population of similar entities does not appear unduly difficult. The choice is seen to be severely circumscribed by what data can be obtained and by the interpretation of what can be described as a homogeneous population of entities. However, that in practice the choice is narrow, is only a weak defense against the charge that scientific methods should be devoid of the arbitrary. A stronger defense is that all known methods for the statistical interpretation of data require a similar measure of the arbitrary. Thus, for example, were it at all possible to match a control group (of entities left untreated) to the treated entities, a judgment would have to be made as to what entities are to be considered similar for the purposes of matching. This is precisely the judgment required to delineate a population of similar entities. If one is accepted as scientifically defensible, so must be the other.

The recommended procedure for before-and-after comparisons is an improvement on two counts. First, it is asymptotically unbiased and automatically eliminates regression-to-the-mean effects. Second, the accuracy of estimation is enhanced. However, it suffers from an ugly asymmetry. Although variance-reducing methods are devised for the utilization of before data, the use of after data remains primitive. Future research in this direction might lead to further improvements in estimation accuracy.

An attempt has been made to erect a methodological basis for extracting information from data and for the accumulation of such data. The likelihood function appears to be well suited for this purpose. The formal logic is sound, the application is straightforward, and the interpretation is relatively free from obfuscation.

In the methodological domain an intriguing conceptual question remains to be explored: When should

likelihood functions derived from different data sets be combined? It has its practical translation: When are results obtained in city A applicable to city B? The commonly voiced contention: "but our conditions are different," which exerts a paralyzing effect on rational safety management, stems from the same source.

It has been shown that the chosen methodological framework worked well when the safety effect of conversion to all-way stop control was examined (Lovell and Hauer, and Persaud elsewhere in this Record). It is not surprising that a storehouse of empirical information on this issue exists. After all, there is more than half a century of application and use to rely on. It proved relatively simple to assemble the additional data that were required to do the proper analysis. The net result of this effort is a defensible current estimate of the safety effect of this measure.

Much of the traditional knowledge about the safety effect of highway and traffic engineering measures is based on simple before-and-after comparisons, and estimates based on these comparisons are now known to be incorrect. Furthermore, a method for analyzing before-and-after data has been presented in this paper, and experience indicates that past data can be used to set the record straight. It follows that a concerted effort to do so appears appropriate.

ACKNOWLEDGMENT

The work discussed in this paper was performed as part of a study sponsored jointly by Transport Canada and the U.S. Department of Transportation. The authors are grateful to both sponsors for their support and guidance.

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Publication of this paper sponsored by Committee on Methodology for Evaluating Highway Improvements.

The Safety Effect of Conversion To All-Way Stop Control

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ABSTRACT

Past studies documenting the safety effect of converting intersection traffic control to all-way stops have consistently shown impressive accident reductions. Because, ordinarily, it was high-accident locations that were converted, it was difficult to know how much of the reduction was real and how much was an artifact of regression-to-the mean. Data from three recent studies were reanalyzed and debiased. In addition, a new data set was assembled and examined. Analysis revealed that, although somewhat inflated, the reductions reported in the earlier studies were quite real and were confirmed by the new data. The empirical information contained in the data sets was captured in likelihood functions and the four functions were joined. Taken individually, the four data sets showed reductions in total accidents ranging from 37 to 62 percent. The joint likelihood function indicates a most likely accident reduction of 47 percent in total accidents.

A number of studies in which an attempt has been made to estimate the safety effect of all-way stops have been conducted in the past. Because the conversion of high-accident intersections was examined in virtually all of these studies, the reported estimates of effectiveness are biased (inflated) by an unknown amount. The source of this bias and methods for its removal are explained in a companion paper, "New Directions for Learning About the Safety Effect of Measures," elsewhere in this Record.

The safety effect of conversion from two-way to all-way stop control at both urban and rural intersections was examined. To obtain unbiased estimates of safety effectiveness, the reported estimates were first cleansed of bias. This was done by using the data from three recent studies. In addition, a new data set was assembled and analyzed to determine the safety effect of the conversion to all-way stop control at 79 intersections in Toronto. The data and results of this analysis are described in this paper. The circumstances and factors affecting the safety effect of all-way stops (such as traffic flow, flow balance, and number of past accidents) are examined separately in a second companion paper, "Safety Migration, Influence of Traffic Volumes, and Other Issues in Evaluating Safety Effectiveness--Some Findings on Conversion of Intersections to Multiway Stop Control," elsewhere in this Record.

It appears both desirable and feasible to assemble and join the information contained in several data sets in order to represent the total current state of knowledge. The likelihood function is used for this purpose. Its application to the four data sets is discussed.

REVIEW

A summary of a review of past studies is given in Table 1. In assessing the accuracy of the various estimates contained in the table it should be noted that studies covering a large number of intersections tend to provide more reliable results. For the same reason estimates for right-angle, injury, and total accident categories are generally more accurate than for the less frequent accident categories (rear-end, pedestrian, etc.). Estimates have not been cleansed of selection-bias and may, therefore, be inflated by some unknown amount. If this source of inaccuracy is disregarded, the following general conclusion can be drawn immediately: safety benefits of conversion to all-way stop sign control are consistent and reductions in accidents considerable (in the 50 to 90 percent range for both total and injury accidents).

The next four sections are devoted to a reanalysis of the three most recent data sets and to the analysis of a newly assembled set. The aim is to obtain estimates of safety effects that are free of bias.

SAN FRANCISCO

The earliest study for which the original data were available was documented in a report published in 1974 by the San Francisco Department of Public Works (2). The report was titled "Study of High Accident Intersections" and included the results of a 1-year before-and-after comparison of accidents occurring at 49 intersections converted from two-way to four-way stop control during the 5-year period running from 1969 to 1973. A 71 percent drop in total accidents was reported in the study. The reduction in right-angle and injury accidents appeared to be as high as 88 and 81 percent.

As the title of the report indicates, intersections were slated for conversion on the basis of a history of many accidents. This criterion for site selection invariably leads to inflated apparent effectiveness due to regression-to-the-mean.

The first task, therefore, was to determine to what extent the results were biased and to remove the bias that was found.

Debiasing involves comparison of the number of accidents occurring after conversion with the number of accidents expected to occur had no conversion taken place (rather than comparing the number of recorded "after" accidents with the number of accidents recorded during the "before" period).

An asymptotically unbiased estimator for the expected number of accidents [$\xi_2(x)$] was developed earlier and is described in the paper "New Direc-

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TABLE 1 Summary of Findings and Estimates of Percent Reduction in Accidents *(1)*

Reference	McEachern 1949	Syrek 1955	Wenger 1958	Leisch et al. 1967	Hammer 1968	Heany 1970	San Francisco 1974	Ebbecke 1976	Briglia 1981
No. of intersections	38	420	38	29	6	57	49	222	10
City	Four	Los Angeles	St. Paul	Chicago, San Francisco, New York, Toronto	California	Philadelphia	San Francisco	Philadelphia	Michigan (rural)
Right-angle			75%		84%		88%	83%	75%
Rear-end			-20%		-30%		-60%	33%	48%
Left-turn				(Gives regression equations that depend on several independent variables)			50%		39%
Head-on	(Number and severity decrease)	(Gives accident rates by major and minor road, ADT)	67%						
Turning-vehicle			33%						
Fixed-object			50%					-4%	
Pedestrian							67%	48%	
Injury			52%		81%	91%	81%	81%	77%
Total			56%		73%	87%	71%	54%	61%

tions for Learning About the Safety Effect of Measures" elsewhere in this Record. The appropriateness of the estimates was checked by comparing them with the average number of recorded after accidents [M(x)], as extracted from a larger population of unchanged two-way stop controlled intersections. Therefore, it was not only necessary to have the before-and-after accident data for the converted sites but also accident data from all similar sites that were not so converted.

In early 1983 San Francisco officials were approached and their cooperation solicited in supplying the data used in their 1974 study. Although the 1-year before-and-after accident data for the 49 intersections converted from two-way to four-way stop control were still on file, the accident data covering the study period for all nonconverted intersections in San Francisco were no longer available. The closest dates for which data were obtainable were the 4 years from 1974 through 1977. This was supplied in hard copy along with a list of all intersections controlled by two-way stop signs. From the hard copy information pertaining to the 4,681 accidents that occurred during the 1974 to 1977 period at 1,142 intersections with two-way stop control was extracted.

The estimator $\varepsilon_2(x)$ provided good estimates and a comparison was made between the expected number of accidents and those observed at the treated sites in the after period. This was done for six accident types (right-angle, rear-end, left-turn, pedestrian, injury, and total). The results are given in Table 2.

It should be noted that Column 4 is derived from a simple before-and-after comparison and therefore gives biased estimates of percent accident reduction. The correct results are given in Column 5. Thus, for example, in calculating the reduction in total accidents, 130 (Column 3), computed using $\varepsilon_2(x)$ and corrected for exposure, should be used rather than 172 (Column 1). (Both volume changes at the treated sites and changes in numbers of accidents across the larger population of similar but unchanged sites were taken into account.) In this case the biased estimate (71 percent) is close to the unbiased estimate (62 percent) because the reduction is large. When speaking in terms of percent, it matters little whether the reduction is from 172 to 50 or from 130 to 50.

Estimates for total, right-angle, and injury accidents are fairly reliable, whereas others are not. The likelihood function, discussed later in this paper, was used to describe estimate reliability.

PHILADELPHIA

The second data set was obtained from Philadelphia. During the mid-1960s, residents of several Philadelphia neighborhoods resorted to barricading streets in order to force City Hall to install traffic signals at particular intersections. Perhaps initially triggered by accidents at these sites, these actions gradually lost their safety-related motivation and acquired the air of general community unrest. As an alternative to costly signal installation, the decision was made in 1967 to use all-way stop control to placate the escalating public demand for traffic signals. During the next 8 years Philadelphia engaged in an extensive program to convert intersections to all-way stop control.

In his master's thesis, Ebbecke (4) reported the results of a 2-year before-and-after study on the safety effect of the conversion of 222 intersections of one-way streets to all-way stop control. The results showed a decrease of 55 percent in total accidents; right-angle and injury accidents decreased by 83 and 81 percent, respectively.

Because the original public pressure was related to perceived high-accident occurrence, there was

TABLE 2 Safety Effect by Accident Type in San Francisco (3)

Accident Type	(1) Number of Before Accidents	(2) Number of After Accidents	(3) Expected Number of After Accidents	(4) [(1) - (2)]/(1) Apparent Percent Reduction	(5) [(3) - (2)]/(3) Unbiased Percent Reduction
Right-angle	129	16	93	88	83
Rear-end	10	16	4	-60	-300
Left-turn	14	7	10	50	30
Pedestrian	6	2	6	67	67
Injury	48	9	35	81	74
Total	172	50	130	71	62

TABLE 3 Safety Effect by Accident Type in Philadelphia (*5*)

Accident Type	(1) Number of Before Accidents	(2) Number of After Accidents	(3) Expected Number of After Accidents	(4) [(1) - (2)]/(1) Apparent Percent Reduction	(5) [(3) - (2)]/(3) Unbiased Percent Reduction
Right-angle	726	126	558	83	77
Rear-end	151	101	123	33	18
Pedestrian	139	75	123	46	39
Fixed object	254	266	200	−4	−33
Injury	313	60	226	81	73
Total	1,329	616	1,072	54	43

again reason to suspect a bias in the results of a simple before-and-after comparison.

An appendix to Ebbecke's thesis included accident data for all intersections in the study area, as well as for the converted sites. It was relatively straightforward to recode the information from hard copy and to proceed in the same manner as for the San Francisco data. Recoding of the data yielded information about 8,934 accidents at 893 intersections from 1968 to 1975.

As in the case of the San Francisco data, the estimates of the expected number of after accidents $[\varepsilon_2(x)]$ corresponded well to the average number of after accidents recorded at the larger population of untreated sites. The estimates were then compared to the recorded number of after accidents at the treated sites. Table 3 gives the results for the major accident types.

MICHIGAN

The studies reviewed thus far have dealt with urban intersections. The third data set reexamined addressed the effect of four-way stop control at intersections of low-volume, high-speed rural roads in Michigan.

In 1981 the Michigan Department of Transportation published a report (6) documenting the change in accidents as a result of converting 10 rural intersections from two-way to four-way stop control over a 7-year period from 1971 to 1977. Total accidents were reported to have fallen by 61 percent while right-angle accidents were reduced by 75 percent.

The 10 converted intersections had been identified as having persistent right-angle accident patterns, and for most of these locations, several less restrictive measures, such as "stop ahead" signs and flashers had been tried without success.

Here again, with a history of many accidents as a reason for conversion, there is danger of regression-to-the-mean biasing results.

Accident data for the converted sites for 2- and 3-year before-and-after periods were appended to the original report. The Michigan Department of Transportation provided additional information in the form of a computer tape that contained accident data for all rural two-lane, two-way, nonsignalized intersections on Michigan's state trunkline system for the years 1974 through 1976. On the 8,578 intersections across rural Michigan, 12,569 accidents were recorded during those 3 years.

Estimates of expected number of accidents $[\xi_2(x)]$ were compared with the average recorded after accidents for the large body of untreated sites. Again, there was good correspondence between the two sets of values.

The data in Table 4 show the results of the comparison between the expected and recorded after accidents.

TORONTO

The last data set examined was from the city of Toronto. Computer tapes containing details of all intersection accidents that occurred in Toronto between 1973 and 1983 were examined. From the 408,040 records originally supplied, information about 16,059 accidents occurring at 1,279 nonsignalized intersections was extracted. For the effectiveness evaluation, 79 intersections were selected that had undergone conversion from two-way to four-way stop control between 1975 and 1982.

Reexamination of the proposal files indicated that 28 of the 79 intersections were converted because of a history of high numbers of accidents. An additional 15 intersections were converted to improve safety.

Following the procedure used in the three earlier analyses, the estimate of expected number of after accidents was compared with those recorded for the larger population of sites. Again the estimates proved good. Comparing the estimates to the recorded after accidents generated Table 5.

Finally, what remained to be done was to amalgamate the four separate sets of results into a coherent whole.

TABLE 4 Safety Effect by Accident Type in Michigan (*1*)

Accident Type	(1) Number of Before Accidents	(2) Number of After Accidents	(3) Expected Number of After Accidents	(4) [(1) - (2)]/(1) Apparent Percent Reduction	(5) [(3) - (2)]/(3) Unbiased Percent Reduction
Right-angle	146	36	102	75	65
Rear-end	44	23	28	48	18
Left-turn	18	11	9	39	−28
Injury	118	27	70	77	61
Total	277	108	230	61	53

TABLE 5 Safety Effect by Accident Type in Toronto (*8*)

Accident Type	(1) Number of Before Accidents	(2) Number of After Accidents	(3) Expected Number of After Accidents	(4) [(1) - (2)]/(1) Apparent Percent Reduction	(5) [(3) - (2)]/(3) Unbiased Percent Reduction
Right-angle	175	85	170	51	50
Rear-end	56	26	33	54	22
Left-turn	17	12	17	29	29
Pedestrian	1	2	3	−100	33
Injury	75	9	62	88	63
Total	331	172	286	48	40

LIKELIHOOD FUNCTIONS

Because automobiles, drivers, and intersections across North America have a good deal in common, it is not unreasonable to expect the safety effect of a treatment to be similar in Michigan, Philadelphia, San Francisco, and Toronto. However, there is much that is unique to each of the four regions, and it is those unique elements that might limit the degree of similarity.

To emphasize similarity, difference, and accuracy, the results of the four data sets will be juxtaposed in this section. Next, the four estimates will be combined into a single estimate of percent accident reduction by accident type.

The likelihood function was the chosen tool of analysis. For a more detailed discussion of the workings of the likelihood function, see "New Directions for Learning About the Safety Effects of Measures" elsewhere in this Record. The horizontal axis of the plots in Figure 1 gives the various possible values for percent reduction in total accidents. The ordinate gives the relative likelihood for the percent reduction. The most likely percent reduction is the point at which the likelihood function has a value of 1.

Thus, for example, in Figure 1a (total accidents, San Francisco) the most likely percent reduction is 62 percent; the relative likelihood of anything outside 40 to 80 percent is negligible. Note that the likelihood function for Philadelphia (Figure 1b) is much more compact, mainly reflecting the fact that it is based on more information (222 intersections as opposed to 49 in San Francisco).

The joy of using likelihood functions is their ability not only to store all empirical information and present it in a clear manner, but also to easily accumulate information as it becomes available. This

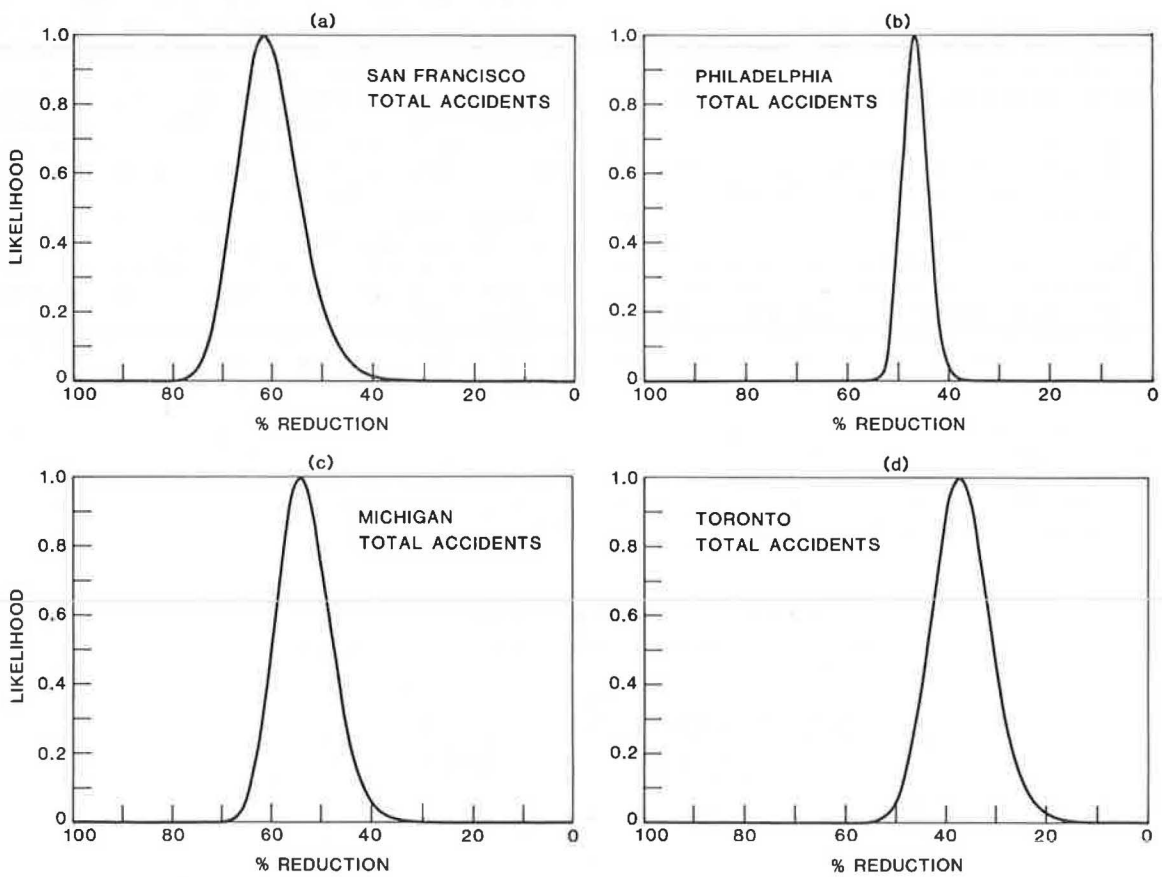

FIGURE 1 Likelihood functions for total accidents.

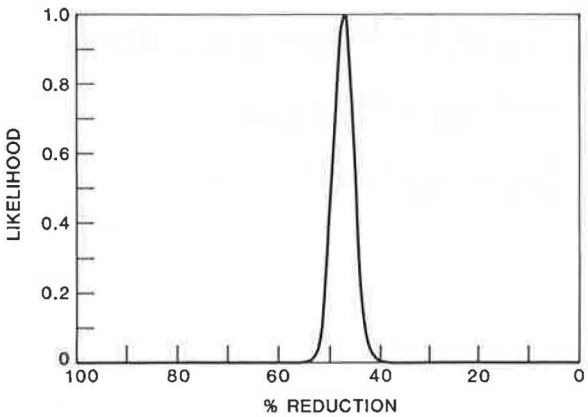

FIGURE 2 Combined likelihood function for total accidents.

can be done simply by multiplying the corresponding ordinates (or adding their logarithms). Because of this facility the functions for each of the data sets were joined to produce a joint estimate of countermeasure effectiveness. This joint likelihood function for total accidents appears in Figure 2. The data in Table 6 give the most likely values for percent accident reduction for all four data sets and for the combined set.

TABLE 6 Most Likely Percent Accident Reductions

Accident Type	San Francisco	Philadelphia	Michigan	Toronto	Combined
Right-angle	84	78	64	48	72
Rear-end	-305	20	19	22	13
Left-turn	33	–	-7	25	20
Pedestrian	66	40	–	42	39
Fixed object	–	-30	–	–	–
Injury	74	74	62	63	71
Total	62	47	59	37	47

SUMMARY AND CONCLUSIONS

The goal of this work was to estimate the safety effect of converting intersection control from two-way to all-way stop control. A review of available empirical evidence revealed fairly consistent and impressive safety effectiveness. However, because in many cases high-accident locations were treated, estimates were inflated to an uncertain extent.

Three recent data sets were reanalyzed to obtain unbiased effectiveness estimates. An additional set of data from Toronto was assembled and examined. Tables 2-5 give detailed unbiased estimates of effectiveness for each case.

The four data sets were represented by likelihood functions and were combined. The combined most likely estimates of effectiveness are given in Table 6. It appears that the conversion to all-way stop control may be expected to reduce total accidents by 47 percent with right-angle and injury accidents dropping by 72 and 71 percent, respectively.

ACKNOWLEDGMENT

The work reported in this paper was performed as part of a study sponsored jointly by Transport Canada and the FHWA, U.S. Department of Transportation. The authors are grateful for their support and help.

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Publication of this paper sponsored by Committee on Methodology for Evaluating Highway Improvements.

Safety Migration, the Influence of Traffic Volumes, and Other Issues in Evaluating Safety Effectiveness—Some Findings on Conversion of Intersections to Multiway Stop Control

BHAGWANT N. PERSAUD

ABSTRACT

Five issues of interest to safety management in general are addressed in the context of an examination of the safety effect of converting intersections from one-street-stopped to multiway stop control. On the first issue, the results support a long-held belief that the more accidents a site is expected to have, the more effective a safety measure is likely to be. This means that for affected measures, effectiveness (percent reduction in accidents) should not be specified as a single accident reduction factor as is currently the practice. Next, on the much debated question of whether improved safety at treated sites leads to a degradation in safety elsewhere, the findings suggest that this safety migration may indeed exist. Accordingly, safety benefits at treated sites should be weighed against any resulting degradation in safety elsewhere. On the other three issues, the findings are somewhat contrary to common belief. First, there is no evidence that conversion of intersections to multiway stop control is effective only for certain ranges of total entering volumes; neither is it apparent that effectiveness depends on how this volume is split among the approaches. Second, a learning period after conversion does not appear to be detrimental to safety. Finally, effectiveness does not decline as the use of this measure becomes widespread. Although all of these issues are addressed for a specific measure, some of the findings might be quite general.

Effective management of safety on a system requires sound knowledge of how the system reacts to the implementation of measures that affect safety--whether safety increases or decreases and by how much. In providing this information, several important issues need to be addressed--issues that have surfaced in evaluation studies because of a belief that, in some way, they are important considerations in safety evaluation. Five such issues are addressed in this paper in the context of an examination of the safety effect of converting one-street-stopped intersections to multiway (all-way) stop control. [See Persaud et al. (1) for more details.] These issues are presented in Figure 1 as questions of interest to safety management.

Issues 1 and 2 have been given the expanded coverage they deserve in other publications (2,3) and will not be addressed in any detail here. Issue 1 results from an apparent consensus among traffic engineers that a safety measure is more effective at locations where many accidents occur than at locations where few accidents occur--a belief that is often reflected in warrants. Issue 2 relates to the controversial question of whether improved safety where a measure is applied results in a degradation in safety elsewhere on a system. Issue 3 is based on a belief that the safety effect of certain measures depends on certain characteristics of traffic volume; for conversion to multiway stop control, the characteristics of interest are the total traffic volumes entering an intersection and how this volume is shared among the intersection approaches. Although in this paper the spotlight is shared by these three issues, the other two are no less important. A common belief that it takes time for drivers to get used to a change in traffic control forms the basis for Issue 4. The final issue to be addressed stems from a concern that traffic control tends to be disregarded if it is considered excessive or unwarranted; if this is so, then the safety effect of a measure will decline as its use becomes widespread.

The knowledge on these issues often comes from simple comparisons of accident records before and after the implementation of safety measures. The foundations of this knowledge have been shaken by researchers, such as Hauer (paper appears elsewhere in this Record), who have shown that such comparisons can lead to erroneous conclusions. Hauer presents details of an improved method for estimating safety effectiveness, and, therefore, for addressing these issues. In examining these issues as they relate to the conversion of intersections to multiway stop control, the intent was that, by removing any doubts as to the propriety of the methodology, some meaningful and forceful conclusions would emerge. In so doing, this work also serves as an illustration of the potential of the new method of analysis.

DATA

In undertaking a study such as this, a sufficiently elaborate data set is as important as the quality of

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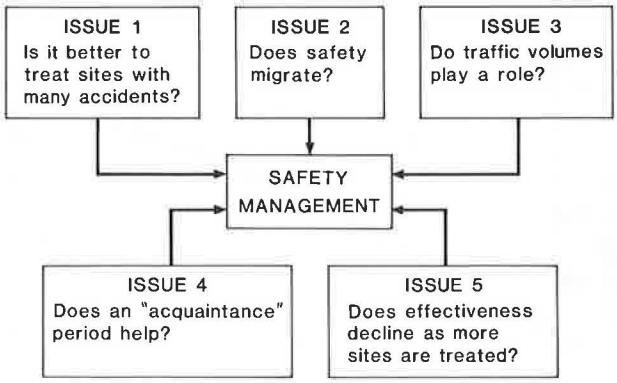

FIGURE 1 Illustrating the issues.

the methods of analysis. The main data set used, a rare find as it turned out, was provided by Ebbecke (4) in a thesis in which he examined the effect of converting 222 intersections of one-way streets from one-street-stopped control to multiway stop control. These conversions were implemented in Philadelphia, Pennsylvania, during the 4-year period from 1970 to 1973. The size of the conversion program can be observed in Table 1, which gives the number of intersections in the study area by type of control. The 222 conversions are reflected by the numbers in this table.

TABLE 1 Types of Intersection Control in Study Area

Control Type	Number of Intersections in Year Beginning 1970	1974
One-street-stopped	419	191
All-streets-stopped	99	321
Traffic signal	375	381
Total	893	893

METHODOLOGY AND OVERALL RESULTS

In examining all of the issues, the percentage reduction in accidents was estimated for intersections grouped in various ways according to the issue being addressed. To obtain these estimates, it was necessary to compare the number of accidents that would have been expected in the "after" period without the conversions with the number actually recorded. Hauer (paper appears elsewhere in this Record) demonstrates that it is usually incorrect to assume that the number of accidents recorded "before" is a reasonable estimate of the number expected to occur after. This common pitfall generally leads to overestimates of treatment effectiveness; that it can also lead to erroneous conclusions about the issues being addressed here provided the main motivation for this work.

To estimate T(x), the number of accidents expected to occur in the after period had the conversion not taken place at an intersection that recorded x accidents in the before period, the following expression, taken from Hauer was used:

$$T(x) = x + [(\bar{x}/s^2)(\bar{x} - x)] \quad (1)$$

where $\bar{x}$ is the sample mean of accidents of a given type in a population of similar one-street-stopped intersections during the before period, and s^2 is the sample variance.

For the results presented in this paper, $\bar{x}$ and s^2 were estimated by first calculating the sample mean and variance of accidents at one-street-stopped intersections grouped in total entering volume ranges of 0 to 999, 1,000 to 1,999, and so on. A weighted least-squares regression line fitted to these "data points" thus provided estimates of $\bar{x}$ and s^2 for any one-street-stopped intersection given its total entering volume. [In earlier work (1,2), traffic volume was accounted for in a different manner. Accordingly, the numerical results presented here are slightly different from those reported previously; the conclusions, however, remain the same.]

In order to provide a backdrop for the discussion of the issues, the estimates of effectiveness obtained in the preceding manner are reported in Table 2 (Column 1) for various accident categories. Also shown (Column 2) are the biased estimates obtained by merely comparing the before and after accident records.

TABLE 2 Safety Effect of Conversion to Multiway Stop Control

Accident Type	Percent Reduction: Unbiased 1	Biased 2
Total	45	54
Injury	73	81
Right-angle	79	83
Rear-end	17	33
Fixed-object	−31	−4
Pedestrian	39	46

Although these numbers are of interest in themselves, discussing them here will detract from the main issues. The reader interested in such discussion and further details is referred to the full report on this study (1).

ISSUE 1: ARE SAFETY MEASURES MORE EFFECTIVE WHERE MANY ACCIDENTS OCCUR?

As indicated earlier, this issue has been given generous coverage in a recent paper (2) and will be only briefly addressed here. Its importance is verified by the Manual on Uniform Traffic Control Devices (MUTCD) (5,p.24B), which specifies that one of the conditions that warrant a multiway stop sign is "An accident problem, as indicated by five or more reported accidents of a type susceptible of correction by a multiway stop installation in a 12-month period...." Part of the basis for such a warrant appears to be a widespread belief that the percentage reduction in accidents (effectiveness) or the accident reduction factor for such a measure is greater at locations where many accidents occurred than at those where few occurred.

A limited number of empirical studies of measures such as traffic signal and pedestrian crossing installation (6-10) appear to support this belief. However, a shadow of doubt may have been cast on this evidence by the many sources [see Hauer and Persaud (11), for example] that have shown that laws of chance alone can cause accidents to decrease at sites where unusually large numbers of accidents occur before treatment and increase at sites with

few or no accidents before. (This phenomenon has become known as regression-to-the-mean.) It is possible, therefore, to wrongly conclude on the basis of simple before-and-after comparisons that a measure is effective only for sites with numbers of accidents larger than some number. It is not clear whether the studies mentioned earlier had accounted for changes due to chance. Because the methods for doing so have become available and the Philadelphia data set was suitably substantial, it appeared natural to engage in a reexamination of this issue.

The 222 converted intersections were grouped according to the number of accidents recorded in the 2 years before conversion. For each intersection in a group, the number of accidents expected to occur without conversion was estimated by using the method described earlier. The sum of these estimates was compared with what was recorded 2 years after conversion to produce an aggregate effectiveness for that group. Effectiveness (percent reduction in accidents), by accident type for each group, was then plotted against the expected number of accidents (without conversion) for the average intersection in that group (Figure 2). Exponential type functions were fitted to these estimates. These plots clearly support the belief that the more accidents expected to occur at a site, the larger the safety effect of a measure is likely to be.

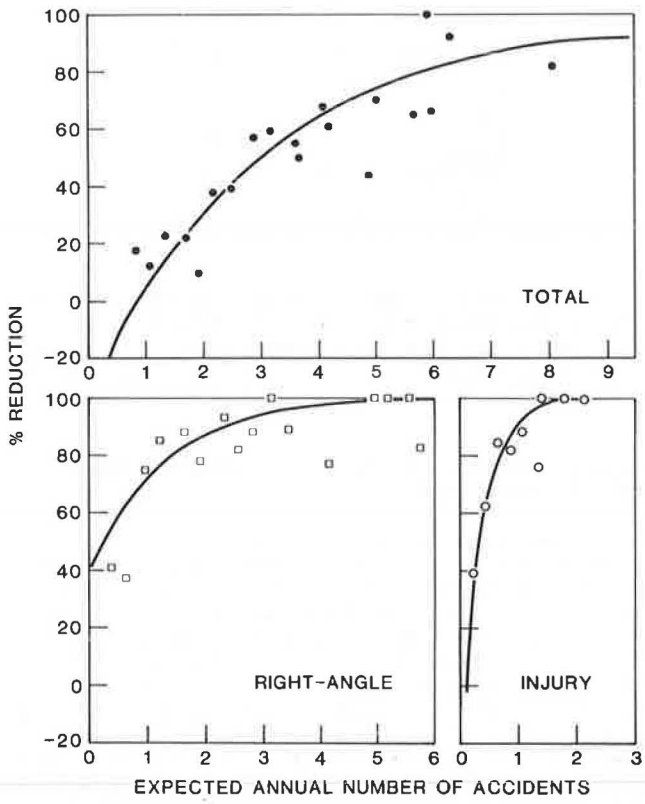

FIGURE 2 Effectiveness versus expected number of accidents—Philadelphia.

This conclusion is further supported by results obtained in a parallel study (12) of intersections converted from two-way to four-way stop control in San Francisco. In Figure 3, taken from this reference, the data points are more scattered because in this case only 49 intersections were converted. In spite of this noise, the message is quite clear;

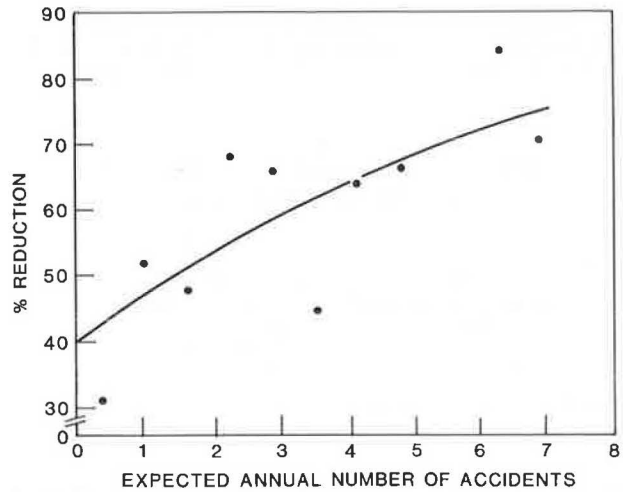

FIGURE 3 San Francisco data: effectiveness versus expected number of accidents—total accidents.

effectiveness of the conversions increases as the expected number of accidents at an intersection increases.

For affected measures, such as conversion to multiway stop control, there are two important implications of the finding on this issue. First, because different applications of this measure can lead to different accident reduction factors (depending on the expected number of accidents before treatment), effectiveness should be specified by its relationship to expected number of accidents rather than as a single accident reduction factor as is currently the practice. Second, the benefits (total reduction in accidents) of treating systems that are expected to have many accidents can be much larger than would be the case if constant effectiveness were assumed; for affected measures, this implication would favor more investment on high-accident systems than would have been the case with a constant effectiveness assumption.

The question remains: Why does effectiveness increase with expected number of accidents? Several explanations are possible [and are discussed (2)], but, despite the relative richness of the data set, there is insufficient evidence from this study to justify any of them. This void presents an interesting challenge for future research on this subject.

ISSUE 2: DOES SAFETY MIGRATE?

Like Issue 1, this issue has been given detailed coverage elsewhere (3) and will be only briefly addressed here. The issue arises from a belief by many safety professionals that an improvement in safety at a treated site leads to a degradation in safety elsewhere in the neighborhood of that site--a phenomenon that has become known as migration of safety. [The term "accident migration" has also been used and, more recently, the unusual term "(un)safety migration" has been suggested (3).] Obtaining insights is complicated because laws of chance alone can cause fewer accidents to occur at treated sites (usually those where many accidents occur) after than before; the converse will happen at untreated (low accident) sites. Taken together, these changes can be incorrectly construed as evidence that safety has migrated.

In one of the few papers on this subject, Boyle and Wright (13) found that a substantial portion of the accidents prevented at treated blackspots in

London, England, had apparently migrated to surrounding sites (generally within one block). This work has been subject to debate in the literature (14-22), and perhaps more to come. The last five exchanges (18-22) constitute a fascinating debate on whether laws of chance (regression-to-the-mean) could have caused an increase in accidents at the untreated surrounding sites, as Stein (18) and McGuigan (20,22) have claimed, or a reduction, as Boyle and Wright (19,21) have claimed. In the original paper (13), the authors apparently compromised by not accounting for regression-to-the-mean at all and attributed the accident increase at the surrounding sites to safety migration. The overall result of this debate is that there is still a thirst for knowledge on this issue.

Conversion of intersections from two-way to multiway stop control provides an almost ideal setting for studying this phenomenon. In setting the stage, Ebbecke (4,p.50) claimed that although multiway stop conversion in Philadelphia reduced accidents by about 50 percent where installed, "the total area accidents are not being reduced, they are just being rearranged." The problem with this conclusion is that Ebbecke apparently did not account for changes due to chance. Because his data set provided the means for doing so, it appeared in order to engage in a reexamination.

To address the issue of safety migration, the effect of the 61 conversions in 1969 was examined. Table 3 gives the changes in numbers of accidents that followed these conversions. Column 1 shows that 219 accidents were recorded at the 61 converted intersections in the 1-year period before conversion and 72 were recorded in the 1-year period after conversion--an apparent reduction of 147 accidents. Using Equation 1, it was estimated that 168 (not 219) accidents would have been recorded at these intersections in the after period had they not been converted. The unbiased change is a reduction of 96 accidents. Equivalent numbers for the 277 unconverted one-street-stopped intersections indicate an (unbiased) increase of 82 accidents. This means that most of the accidents prevented at the converted intersections had apparently migrated to the unconverted intersections.

TABLE 3 Accidents at Converted and Unconverted Intersections

	Converted Intersections 1	Unconverted Intersections 2
Number of intersections	61	277
Accidents recorded before	219	445
Accidents expected after	168	493
Accidents recorded after	72	575
Unbiased change	96	-82

Because there appears to be some support for the existence of safety migration, it might be useful to mention three potential explanations for these results. First, drivers may have been compensating for the reduced accident risk at the converted intersections by being less cautious elsewhere. Second, as Ebbecke suggested (4), it may be that the accident increases at unconverted intersections may be due to confused drivers who were uncertain as to whether those intersections were converted as well. Finally, the apparent migration of safety might have resulted from a redistribution of traffic as drivers sought to avoid the increased delay at the multiway stops. Although this redistribution was not evident in the traffic data provided by Ebbecke, it should be recognized that to explain a change as subtle as the increase of 82 accidents at 277 intersections, better detail is needed than is provided by the usual traffic surveys. These explanations and the implications of safety migration are explored in greater depth in the expanded paper (3).

ISSUE 3: DO TRAFFIC VOLUMES PLAY A ROLE?

Two related issues fall under the broad umbrella of the question of the role of traffic volumes. In a review of the literature on conversion to multiway stop control, Hauer (23) indicated that a belief exists that this measure is more effective when implemented on intersecting roads where the traffic volumes are nearly equal and the total of these volumes is between 6,000 and 12,000 vehicles per day. This belief is in part reflected by the Manual on Uniform Traffic Control Devices (MUTCD) (5,p.24B-3), which specifies that multiway stop control "...should ordinarily be used only where the volume of traffic on the intersecting roads is approximately equal" and that one of the conditions warranting a multiway STOP sign installation is "the total vehicular volume entering the intersection from all approaches must average at least 500 vehicles per hour for any 8 hours of an average day,..." (5,p.24B-4). An upper volume limit is indicated by Syrek (24), who found that four-way-stopped intersections with entering volumes larger than 12,000 vehicles per day had a higher accident rate than two-way-stopped intersections with similar entering volumes. As Hauer (23) points out, there are grounds for questioning the methods of analysis that may have been used in the studies on which these beliefs are based. It is therefore useful that the Philadelphia data provided an opportunity to remove these suspicions and gain some insights into the two traffic-related issues.

To examine the influence of total entering volumes, intersections were grouped in total entering volume ranges of 1,000. In Figure 4, effectiveness is shown by accident type for intersections in each of these volume groups. Although there is no clear

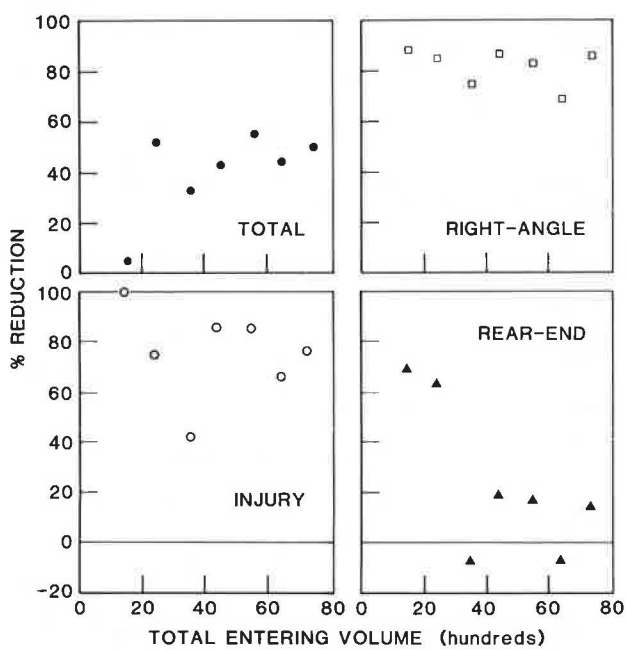

FIGURE 4 Effectiveness versus total entering volume.

trend for total accidents, it is quite clear that this measure can be just as effective for total entering volumes less than 6,000 per day as it is for larger volumes. The same can be said for right-angle accidents. For rear-end accidents, however, the picture is quite different; for this category, it appears that effectiveness decreases as total entering volume increases and can be negative at volumes larger than 6,000 vehicles per day. It is perhaps prudent to examine this trend in the light of findings on the effect of volume share.

To gain insights on the effect of traffic volume share, intersections were grouped in minor road volume share ranges of 5 percent. Figure 5 shows effectiveness values by accident category for intersections in each group. The plots for total and right-angle accidents show that, contrary to common belief, this measure is no more effective when the approach volumes are nearly equal than when they are unbalanced. Once again the rule confirming exception is rear-end accidents; for this category, it appears that effectiveness increases as minor road volume share increases but does not have a positive value until the minor road volume share exceeds 25 percent. Taken together, this finding and the earlier conclusion that effectiveness for rear-end accidents decreases with increasing traffic, produce an issue of considerable interest. It should be noted that the proportion of rear-end accidents is so small in this case that the dependence of effectiveness for rear-end accidents on these traffic characteristics is concealed when effectiveness for total accidents is examined.

Care must be taken in concluding on the overall issue of the role of traffic volumes. Two traffic characteristics have been examined and found to have little or no influence on the effectiveness of conversion of intersections to multiway stop control, except for the rear-end accident category. This does not necessarily mean that traffic volumes do not play a role. Certainly there could be other factors, other exposure measures for example, which could have an influence. Perhaps the changes in safety on a specific approach should be related to the traffic on that approach. Unfortunately, the Philadelphia data do not permit this type of analysis.

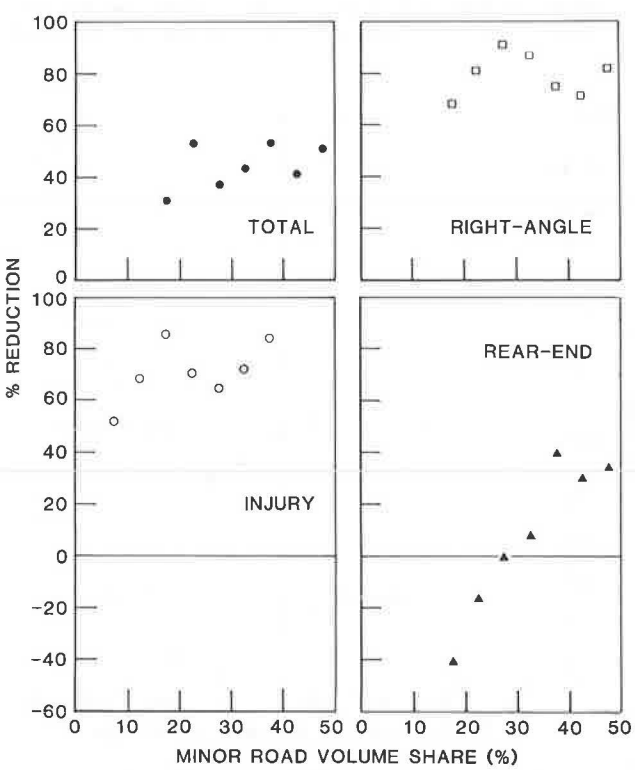

FIGURE 5 Effectiveness versus minor road volume share.

ISSUE 4: DOES AN ACQUAINTANCE PERIOD HELP?

It is often claimed that it takes time for drivers to become acquainted with a change in traffic control and therefore the initial period following conversion should be omitted from analysis of the safety effect of the change. If this claim were to apply to conversion to multiway stop control, then it could be expected that this measure would be less effective during some initial period than it would be later on. To examine this issue, effectiveness for each category of accidents was calculated based on an after period beginning 6 months after conversion. The results, given in Table 4, are compared with the effectiveness estimates based on an after period commencing immediately after conversion. From this comparison, it is clear that it makes little difference if a 6-month acquaintance period is allowed.

TABLE 4 Effectiveness with Acquaintance

Accident Category	Percentage Reduction for Acquaintance Period	
	0 Months	6 Months
Total	45	43
Injury	73	65
Right-angle	79	76
Rear-end	17	14
Fixed-object	-31	-40
Pedestrian	39	44

It is concluded, therefore, that even if it does take time for drivers to get used to multiway stop conversions, safety is not reduced during this learning period.

ISSUE 5: DOES EFFECTIVENESS DECLINE AS MORE SITES ARE CONVERTED?

This issue has an interesting background with respect to the multiway stop conversion program in Philadelphia. In a study of 57 intersections converted early in the program, Heaney (25) reported that total accidents were reduced by 87 percent. For the subsequent conversion program, 222 intersections were studied by Ebbecke (4) who reported a 55 percent reduction. On this basis, Ebbecke claimed that the safety effect decreased as more intersections were converted. However, the intersections studied by Ebbecke were selected in a somewhat haphazard fashion, whereas the intersections studied by Heaney were selected mainly on the basis of a poor accident record. It is therefore possible that the larger reductions reported by Heaney were a result of a regression-to-the-mean effect that is larger than that for Ebbecke's data. This concern has to remain as speculation because the data used in Heaney's study are not available. However it was possible to examine the same issue by using the data for the intersections studied by Ebbecke. Table 5 gives effectiveness values for these conversions by accident category according to the year of conversion.

TABLE 5 Effectiveness by Year of Conversion

Accident Category	Percentage Reduction for Conversions Done in 1970 74 Sites	1971 67 Sites	1972 38 Sites	1973 43 Sites
Total	45	43	50	50
Injury	74	67	79	73
Right-angle	76	82	82	80
Rear-end	23	30	−23	21
Fixed-object	−27	−43	−15	−33
Pedestrian	30	45	50	35

For each category, the effectiveness estimates vary from year to year but, however isolated, they do not support the claim that effectiveness decreases as multiway stop control proliferates in an area. Because it is fairly common practice to test a new measure at a few high-accident locations, there is an important lesson to be learned from the Philadelphia experience: without accounting for regression-to-the-mean, it is possible to wrongly conclude that effectiveness declines with subsequent implementation of the measure.

SUMMARY

Several issues of interest to safety management have been addressed in the context of an examination of the effect of conversion of one-street-stopped intersections to multiway stop control. For this measure, one belief--that effectiveness increases as the expected number of accidents at an intersection increases--was confirmed. On the controversial question of safety migration, the findings lend support to the belief that a measure that improves safety at one location can cause a degradation in safety elsewhere. For the other three questions, the findings are somewhat contrary to common belief. First, there is no evidence that this measure is only effective for certain ranges of total entering volumes; neither is it apparent that effectiveness depends on how this volume is split among the approaches. Second, safety is not reduced during a learning period after conversion. Finally, the novelty of this measure does not appear to wear off as its use becomes widespread.

All of the issues examined need to be addressed with respect to other safety measures as well, using improved methods of analysis such as those used in this study. The data set used in this study is more suited to this analysis than most that are available in practice; yet, many questions remain unanswered, the main reason being that this analysis was conducted so long after the conversion program. If there is a lesson to be learned, it is that when future safety measures are planned, a conscious effort should be made to gather the type of data required to more fully explore these and related issues.

ACKNOWLEDGMENTS

The work reported in this paper was performed as part of a study sponsored jointly by Transport Canada and the Federal Highway Administration, U.S. Department of Transportation. Their support and help is gratefully acknowledged.

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Publication of this paper sponsored by Committee on Methodology for Evaluating Highway Improvements.