Reinfurt, D.W. and Symons, M.J. (1974). Statistical Techniques for Evaluating the Effectiveness of State Motor Vehicle Inspection Programs in Reducing Highway Accidents. Chapel Hill NC: University of North Carolina Highway Safety Research Center.

Statistical Techniques for Evaluating the Effectiveness of State Motor Vehicle Inspection Programs in Reducing Highway Accidents

Donald W. Reinfurt

Michael J. Symons

University of North Carolina
Highway Safety Research Center
Chapel Hill, North Carolina

March 1974

The UNC Highway Safety Research Center was created by an act of the 1965 North Carolina General Assembly. A three-point mandate issued by the Governor authorized HSRC to 1) evaluate the state's highway safety programs, 2) conduct research, and 3) instruct and train other working professionals in highway safety.

ATTENTION

The enclosed report is a reprint of the original technical report which has recently gone out of print. Its content does not differ in any way from the original report. The format differs slightly due to time restrictions in the reprinting process.

We hope that this report will fulfill your interests. We appreciate your continued concern in highway safety.

ABSTRACT

Using accident and inspection data from two states, the effect of periodic motor vehicle inspection on highway crashes is investigated. In both states, accident data from the initial year of the statewide program are examined. Unfortunately, it was not possible to restrict the analysis to mechanically-caused accidents. In addition, there were serious difficulties with the phasing-in schedules and the necessary data file linkages, viz., non-compliance in North Carolina and linkage of inspection, license plate distribution, and corresponding accident information in Florida. As a result, the major contributions of this investigation would be in the statistical methodologies employed.

These data from North Carolina and Florida do not provide evidence of the effectiveness of periodic motor vehicle inspection in reducing highway accidents. However, with the limitations in these data and the probable small effect, if any, of vehicle inspection, it is not unexpected that these studies would fail to detect such an effect.

TABLE OF CONTENTS

ACKNOWLEDGMENTS

The data used in this study were made available by the North Carolina Department of Motor Vehicles and by the Florida Department of Public Safety.

In addition, appreciation is expressed to Dr. B. J. Campbell and Mr. William S. Rouse for their aid in the initial stages of this study; to Dr. Gary G. Koch and Mr. William D. Johnson for helping with the analysis; to Mr. Donald N. Levine and Dr. John A. Allen for their constructive criticisms of the original manuscript; and to Mr. William C. Fischer for coordinating the necessary computer programming.

I. INTRODUCTION AND BACKGROUND

Periodic motor vehicle inspection (PMVI) programs have been in existence in one form or another for over five decades. The basic premise upon which these programs have been based is that a mechanically-sound vehicle is safer than one with defects and therefore will be involved in fewer accidents. The purpose of this report is to examine this hypothesis using inspection and corresponding accident information from two states, North Carolina and Florida. For both, data was collected from their respective initial years of PMVI.

It should be noted at the outset that, for the purpose of this report, "mechanically-caused" accidents will refer to those accidents in which various items subject to inspection played a major role in the accident sequence. For North Carolina and for Florida, these components included the following: headlights, parking lights, license plate light, tail lights, stop lights, directional signals, foot brake, emergency brake, steering mechanism, windshield wiper and horn. In addition, Florida required tires to be inspected.

Historically, Finland in 1922 became the first country to require annual vehicle inspection for cars. Since then, PMVI programs varying in frequency and intensity have been initiated in Western European countries (e.g., Germany, Great Britain, Norway, Sweden, and Austria) and even in less industrialized countries such as New Zealand, Malawi and Zambia.

In the United States, PMVI originated in 1927 with the voluntary "Save-A-Life" campaigns in Massachusetts, Maryland and New York, in which car owners brought their cars to special garages for inspection. In 1928 a voluntary pilot inspection program in Pennsylvania revealed that only 42% of some 750,000 vehicles inspected were considered to be in safe driving condition. Startled by this, the Legislature enacted a law requiring annual inspection of all motor vehicles in Pennsylvania, the first such program in the United States. Later, in 1930, all six New England states began programs of vehicle inspection. Gradually, a number of other states added some form of PMVI while certain states discontinued their programs for a variety of reasons.

During the past decade PMVI has been employed by an increasing number of states in response to the motor vehicle inspection standard issued by the Department of Transportation under the Highway Safety Act of 1966. At present, 31 states and the District of Columbia have standard PMVI programs. Seven states (California, Michigan, Minnesota, Ohio, Oregon, Washington, and Wisconsin) employ systems of spot-check inspections.

Programs vary in terms of frequency of inspection (i.e., at random, semi-annually, and annually), vehicle-type coverage (from cars, motorcycles, and buses only, to all motor vehicles), and range of items inspected (from the basic programs which inspect lights, brakes, steering, windshield wipers and horn to the more elaborate systems that also inspect tires, suspension, exhaust and fuel systems, all mirrors, and even door locks).

The extent to which current PMVI programs are effective in preventing crashes due to mechanical failure is not known. However, several studies have examined the question of the influence of PMVI on the mechanical condition of vehicles (McCutcheon, 1968; Reinfurt and Pascarella, 1969; Reinfurt, House and Levine, 1971). Although these studies did establish that PMVI does improve mechanical condition, they did not investigate whether or not these mechanically-improved vehicles were subsequently involved in fewer accidents than their uninspected counterparts.

Most efforts directed at examining the basic premise underlying PMVI have been limited to simply showing a statistical correlation of PMVI data and death rates. For example, Mayer and Hoult (1963) categorized states by type of motor vehicle inspection: no inspection, some inspection, private garage inspection, or state-owned inspection system, assuming an ordinal scale. Death rates between 1948 and 1960 were compared for each of these four groups of states. Their major conclusions were:

1. There appears to be a positive relationship between motor vehicle inspection and low highway death rates in terms of fatalities per million vehicle miles.

2. The relationship between low highway death rates and inspection systems with the greatest potential for rigor and uniformity appears to be positive. The state-owned and operated inspection system is considered to be most rigorous of all.

However, there is no claim about causality, only an association. Other factors besides PMVI, such as urbanization or better road building and maintenance programs in these states, may well account for lower highway death rates.

Buxbaum and Colton (1966) compared the 1960 mortality rates among white and non-white males 45-54 years old in states with and without PMVI. They concluded that:

1. Motor vehicle accident mortality rates of both white and non-white males 45 to 54 were lower in states with motor vehicle inspection. Grouping inspection and non-inspection states on the basis of geographic area, population density, degree of urbanization, gasoline consumption, and registered motor vehicles per population yielded similar results favorable to motor vehicle inspection.

2. Overall motor vehicle accident mortality rates among non-whites exceeded that of whites.

3. Mortality rates were somewhat lower in states with two inspections per year as opposed to only one.

However, again there was no claim or evidence of causality, only an association. Other factors could well account for the findings.

The relationship between motor vehicle accident mortality and compulsory motor vehicle inspection was also examined by Fuchs and Levenson (1967) using data from 1959-61. Using regression techniques, they examined the relationship between age-standardized highway death rates and inspection status, motor fuel consumption per capita, population density, percentage of population aged 18-24 years, other accident mortality, percent non-white, alcohol consumption, percent of vehicles more than 9 years old, whether or not a vision inspection is required at license renewal, education, and income. It was concluded that:

1. Their results were consistent with Buxbaum and Colton in relating a reduction of mortality to motor vehicle inspection after controlling for a number of variables.

2. However, the effect of inspection upon the reduction in motor vehicle mortality was estimated to range from 5 to 10 percent, which was considerably less than estimated by Buxbaum and Colton.

It should be noted that this study, unlike the others, tried to statistically control for a variety of potentially-related variables. However, the results are weakened by strong interactions between inspection effects and both level of income and degree of education. Again there is no evidence of causality since many related variables, such as condition of roads, degree of enforcement, etc., are still omitted.

Garrett and Tharp (1969) examined the hypothesis that the probability of accident involvement increases as the number of months since inspection increases. Since they confined their study to Virginia inspection and accident data, they eliminated the problems of inter-state heterogeneity. In addition, they studied all accidents rather than fatals only. Based on an analysis of the ratio of accident to non-accident vehicles as a function of time elapsed since inspection, they concluded that:

1. Inspection did not appear to be effective in preventing accidents.

2. Because inspections should be of value only in preventing accidents precipitated by mechanical failure, the inclusion of all accidents in the analysis could have concealed any existing trend. If this were the sole reason for the inconclusive results, it followed that inspection had a very limited effect upon the overall accident picture.

As all vehicles in this study had been inspected at some point, the investigation does not directly examine the effect of inspection versus no inspection but rather whether there is a deterioration effect of inspection over time.

O'Sullivan (1971) has presented a careful discussion of the arguments for and against PMVI in Ireland. He discussed cost/benefit studies (based on U.S. experience), methods of measuring effectiveness, and the characteristics of an efficient PMVI program. However, only the concepts are presented; no data and analyses are available.

The remainder of this report will deal with statistical techniques for evaluating the effect of PMVI on accident involvement with application to data from North Carolina and from Florida. Study designs aimed at eliminating problems encountered in previous studies as well as problems stemming from massive data collection procedures will be discussed. Finally, a theoretical discussion of sample size requirements for determining an effect of a given magnitude will be given. These sample size requirements are derived for an ideal study somewhat different from those carried out in North Carolina and in Florida and are based on various assumptions of average time until an accident, length of study period, and extent of the problem of mechanically-caused accidents. This investigation suggests that it is not surprising that there was no effect detected.

II. NORTH CAROLINA STUDY

Background

PMVI was inaugurated in North Carolina during 1966. Since no such program existed previously, there should have been maximum contrast between the mechanical condition of inspected and uninspected vehicles. Due to the manner in which vehicles were scheduled for inspection, a built-in study design for evaluating the effect of PMVI on subsequent highway crashes existed. Specifically, North Carolina motorists were instructed that they should have their vehicles inspected according to the terminal digit of their license plate; i.e., those with terminal digit 3 were to be inspected during March, 1966; terminal digit 4 in April; 5 in May; etc.

This procedure provided several features important to the evaluation. First, since accident reports included both license plate number and month of accident, an inference could be drawn as to whether or not the accident-involved cars had been inspected. (As will be shown, non-compliance with the intended inspection schedule created a major road-block to making any valid inferences.) Secondly, since the terminal digit was randomly assigned, the design controlled for differences between cars and their drivers as well as for differences in vehicle usage.

Description of the Data

The primary data examined in this study consisted of 85,462 privately-owned North Carolina passenger cars involved in accidents in North Carolina during the first ten months of 1966. In addition, there was information on 9538 out-of-state vehicles as well as 12,814 North Carolina vehicles which were not privately-owned passenger cars. The analysis focused on privately-owned North Carolina passenger cars. (See Table 1 for the month of accident by license plate terminal digit distribution of the 85,462 cars.)

Using the standard North Carolina accident report form, month of accident by license plate terminal digit distributions were available for the levels of the following variables: age of driver, sex of driver,

Table 1. Month of accident by license plate terminal digit distribution -- overall.

Month of Accident	Terminal Digit 3	4	5	6	7	8	9	0	1	2	TOTAL
January	866	913	897	931	938	974	926	899	920	924	9188
February	755	799	764	736	851	745	805	759	735	748	7697
March	744	718	757	748	761	750	749	680	806	726	7439
April	859	833	838	888	821	854	827	853	813	800	8386
May	868	870	889	915	895	895	882	859	947	885	8905
June	752	765	771	828	772	756	792	779	745	778	7738
July	888	890	867	851	860	862	869	816	870	849	8622
August	900	830	865	881	942	926	924	892	874	885	8919
September	888	859	926	885	879	878	824	878	855	844	8716
October	996	982	1008	1009	965	978	961	972	988	993	9852
TOTAL	8516	8459	8582	8672	8684	8618	8559	8387	8553	8432	85462

driver injury level[1], model year of vehicle, accident type, time of accident, and weather. See Table 2 for the corresponding frequency distribution.[2]

In addition, month of accident by terminal digit distributions were also available for various combinations of the above mentioned variables. Specifically, driver age, driver sex, driver injury level, model year of vehicle, time of accident and weather distributions were available for the following categories: North Carolina privately-owned passenger cars (single car accidents, two car accidents - striking vehicle, struck vehicle); North Carolina vehicles not classified as privately-owned passenger cars; and out-of-state vehicles. Table 3 exhibits the single vehicle accident distribution where the effect of periodic motor vehicle inspection might be expected to be most evident.

Methodology and Results

Since terminal digits were essentially randomly assigned to license plates in North Carolina, it was expected that without inspection no relationship would exist between terminal digit and month of accident, and therefore, for any given month, the accidents would be approximately equally (or uniformly) distributed across terminal digits. Thus, if PMVI had a detectable effect, the row distributions in Table 4 would become progressively less uniform (or rectangular) starting with the month of March. In fact, the cells to the left of the "0" cell for a given row should have considerably fewer observations than the remaining cells in that row with the "0" cell intermediate since that cell represents the corresponding cars that were to be inspected that month.

To test for departure from a uniform distribution across terminal digits for a given month, the Pearson Chi-square statistic (d.f. = 9) was used. Most X^2 statistics were far from significant. (With 9 d.f. and $\alpha = .05$, the critical value is $X^2 = 16.92$). In fact, if anything, there were fewer significant X^2 values than would be expected on the basis of chance alone. See Table 5 for single car accidents and for all accidents involving privately-owned North Carolina passenger cars.

---

[1]See Manual on Classification of Motor Vehicle Traffic Accidents, National Safety Council, Chicago, 1962.

[2]"Not stated" are omitted from the frequency distributions.

Table 2. Accident frequency distribution for privately-owned North Carolina passenger cars.

Variable	Level	Frequency
Age of driver	0-24	33,754
	25-54	42,009
	55+	9,648
Sex of driver	Male	62,821
	Female	22,634
Driver injury	F (fatal)	973
	A or B	19,807
	C	6,606
	PD (property damage)	58,076
Vehicle model year	55 and earlier	8,968
	56-58	12,387
	59-61	19,484
	62-64	24,794
	65-67	19,740
Accident type	Single car	21,736*
	Two car	27,752
	Other (e.g.,car vs truck)	35,974
Time of accident	Day	55,900
	Night, dawn, or dusk	29,537
Weather	Clear	69,218
	Cloudy, rain, snow, sleet, hail, or fog	15,896

*Number of cars involved

Table 3. Month of accident by license plate terminal digit distribution -- single car accidents.

Month of Accident	Terminal Digit 3	4	5	6	7	8	9	0	1	2	TOTAL
January	197	211	223	221	217	271	204	229	236	231	2240
February	177	210	185	155	191	157	193	168	184	159	1779
March	166	173	180	174	174	174	182	146	204	163	1736
April	201	209	221	218	231	212	213	235	213	223	2176
May	234	230	243	255	222	230	231	233	248	229	2355
June	186	208	227	232	191	197	210	192	194	214	2051
July	229	251	239	244	225	260	236	231	223	220	2358
August	217	220	214	218	249	233	231	231	217	201	2231
September	239	232	251	251	224	228	189	222	249	229	2314
October	232	232	246	268	238	245	253	281	240	261	2496
TOTAL	2078	2176	2229	2236	2162	2207	2142	2168	2208	2130	21736

Table 4. Month of accident by license plate terminal digit distribution of elapsed time (in months) since inspection according to the study design.

Month of Accident	Terminal Digit 3	4	5	6	7	8	9	0	1	2
January	–	–	–	–	–	–	–	–	–	–
February	–	–	–	–	–	–	–	–	–	–
March	0	–	–	–	–	–	–	–	–	–
April	1	0	–	–	–	–	–	–	–	–
May	2	1	0	–	–	–	–	–	–	–
June	3	2	1	0	–	–	–	–	–	–
July	4	3	2	1	0	–	–	–	–	–
August	5	4	3	2	1	0	–	–	–	–
September	6	5	4	3	2	1	0	–	–	–
October	7	6	5	4	3	2	1	0	–	–

Key: – = uninspected
0 = inspected during given month
1 = inspected during preceding month
. .
. .
7 = inspected seven months prior to month of accident

Table 5. Chi-square test of uniformity of accident distributions across terminal digits within month

Month of Accident	Observed Chi-square statistic X^2 Single Car Accidents	All Accidents
January	16.89	7.98
February	16.50	16.25
March	11.34	12.74
April	4.35	7.06
May	3.85	6.58
June	10.92	6.49
July	6.33	4.65
August	7.06	11.00
September	13.37	8.04
October	9.40	2.54

Regression analyses using the general linear model provided an additional, more powerful examination of North Carolina's PMVI data. In the regression analyses, a square root transformation on the accident frequencies was used, assuming an underlying Poisson distribution of accidents. Again, single car accidents (see Table 3) were used in the analyses.

The analyses regressed the square root of the accident frequencies on month of accident, terminal digit of license plate, (presumed) elapsed time since inspection, and selected interactions (e.g., month of accident by terminal digit interaction). The null hypotheses tested included the following: no month of accident effect; no terminal digit effect; no effect of elapsed time since inspection.

Although the model provided a good fit to the data (multiple correlation coefficient R^2 exceding 0.8), the least ambiguous finding was that there was a significant month effect. However, various month by terminal digit interactions were also significant.

It was anticipated at the outset that there would be a problem due to non-compliance with the phasing-in of a statewide PMVI program, namely, both early and late inspections. Ideally, the investigating officer at the scene of an accident occurring during the study period could have recorded on the accident report form whether or not there was a valid inspection sticker on the accident vehicle and, if so, indicated the month of inspection. However, this was not practical due to the number and variety of officers filling out the standard form statewide. The only alternative was to rely on the inspection schedule by license plate terminal digit with a concurrent study of the magnitude and nature of non-compliance.

In order to study the problem of non-compliance, a random sample of approximately 1 percent of the February through October, 1966, inspection records were collected from inspection stations across the state. For any given inspection station selected by the sampling design, records of all inspections performed by that station during the specified month were examined. This procedure guaranteed that both high and low volume stations, i.e., presumably urban and rural stations, respectively, would be adequately represented and that the sample would be representative with respect to car makes and model years.

Several points should be noted. Although the phasing-in schedule by license plate terminal digit commenced March 1, 1966, PMVI actually started in mid-February. Although the official PMVI records for the period from mid-February through the end of March are lumped together (see Table 6), using the monthly compliance distributions, it is estimated that approximately 178,000 vehicles were inspected "early", i.e., during February. Therefore, if involved in an accident, a large proportion of these inspected vehicles would be incorrectly classified as non-inspected by virtue of their license plate terminal digit. The distribution of this sample of 11,218 privately-owned North Carolina passenger cars by month of inspection and compliance with the inspection schedule is given in Table 7.

Table 6. Distribution of monthly inspections performed during initial year of PMVI in North Carolina

Month	Number of Inspections
February March	399,468
April	233,475
May	229,585
June	211,433
July	231,076
August	214,939
September	183,967
October	191,568

Table 7. Percentage distribution of special sample of cars by month of inspection and compliance with the inspection schedule.

Sampled Month	Months Early 10	9	8	7	6	5	4	3	2	1	0	Months Late 1	2	3	4	5	6	7
February	5.43	5.06	5.92	6.04	5.55	7.89	9.25	10.23	14.30	30.33	*							
March		3.19	3.19	3.13	4.29	3.52	4.01	5.22	6.81	12.36	54.28							
April			2.27	2.18	3.36	3.19	2.52	4.20	5.21	9.33	59.92	7.82						
May				1.99	2.51	2.86	2.95	3.47	5.37	11.18	56.76	9.27	3.64					
June					2.55	2.89	2.81	3.57	4.93	9.61	54.08	12.67	4.25	2.64				
July						2.78	3.09	3.63	5.18	12.22	50.89	12.22	4.95	2.71	2.32			
August							3.50	4.64	4.78	10.98	49.71	14.41	3.71	2.85	3.00	2.43		
September								4.68	6.12	11.93	50.60	12.84	4.68	2.64	2.27	2.42	1.81	
October									6.49	12.88	52.39	14.12	4.01	2.67	2.10	1.62	1.81	1.91
											(53.48)							

* A priori empty since <u>no</u> cars were scheduled for inspection during February.

As is obvious from Table 7, non-compliance with the phasing-in of the inspection schedule was a major problem. During this initial year, only slightly over half (53.48%) of the vehicles inspected between March 1 and October 31 were inspected during the month in which it was assumed that they were inspected. Unfortunately, there is no method available for re-distributing the "early" vehicles into the "inspected" cells of Table 3 and likewise the "late" vehicles into the "uninspected" cells since Table 3 deals with accident vehicles only which is but a small portion of the total fleet of vehicles. Thus, a given cell in Table 3 may, in fact, contain both inspected and uninspected cars whereas the design called for it to contain only inspected (or uninspected) cars. It is not surprising, therefore, that the relationship (if one exists) between PMVI and accident reduction could not be determined from this study very possibly because of this problem of non-compliance.

III. FLORIDA STUDY

Description of the Data

As in North Carolina, the phasing-in of PMVI was accomplished using the license plate terminal digit. Those vehicles with terminal digit 4 or 5 were scheduled for inspection during June, 6 or 7 during July, etc. Thus, the phasing-in period in Florida lasted from June through October, 1968.

The data analyzed in the Florida Motor Vehicle Inspection Study were taken from three collection files: the tag (or license plate), inspection, and accident files. The tag file contained information about the vehicle gathered at the time of purchase of a 1968 license plate. The license plate number could be decoded to reveal the type and weight class of vehicle and county of residence of the owner. The inspection file contained the following data: date the vehicle passed inspection, information particular to the inspection, and vehicle identification data including license plate number. Accidents during 1968 were located on the accident file. The date of the accident, description of the vehicle, accident type (e.g., single vehicle), severity of accident (e.g., property damage), and disposition of charges (e.g., driver at fault) were included for each accident. The study period was 163 days in length, running from June 21 to November 30, 1968.

The basic measurement on each vehicle was its "potential" or "exposure" time as a non-inspected and/or an inspected vehicle. This potential or exposure time was the length of time the vehicle was a member of the non-inspected (or inspected) fleet during the study period. Only those vehicles inspected during the study period contributed to the experience of the inspected fleet of vehicles. If the vehicle was involved in an accident, the "survival" time until the crash was noted. If the vehicle was not involved in an accident, its survival time equalled its potential time. The basic configurations of non-inspected and/or inspected exposure and survival intervals are illustrated in Figure 1. Accidents indicated on the diagrams show the most complicated configurations although the majority of vehicles survived the entire study period without an accident.

It should be noted that information on at most one accident was allowed for either the non-inspected or inspected interval due to the possibility of a vehicle being lost to follow-up study by being totally demolished in an accident. For a study period of only 163 days, this is clearly a minor restriction since motor vehicle accidents are relatively rare events in the first place.

A large number of vehicles were eliminated from the study for a variety of reasons. Incomplete or obviously erroneous data were major factors. In the accident file, a vehicle was deleted if the data of an accident was unknown. Basic editing of the license plate file resulted in discarding cases if the vehicle was not:

(1) In one of eight weight classes, or

(2) Registered in one of Florida's 67 counties, or

(3) Tagged just prior to or during the study period.

This last restriction was necessary in order that a common license plate number identified the same vehicle in all three files. Another major area of attrition resulted from the required computer linkages of these three large data files.

Methodology and Results

Descriptive statistics

For each of the vehicle classifications (e.g., large motorcycle), a vehicle's experience was recorded for both its exposure as a non-inspected and/or an inspected vehicle. A tabulation was made of the number of survival days, number of potential days, and number

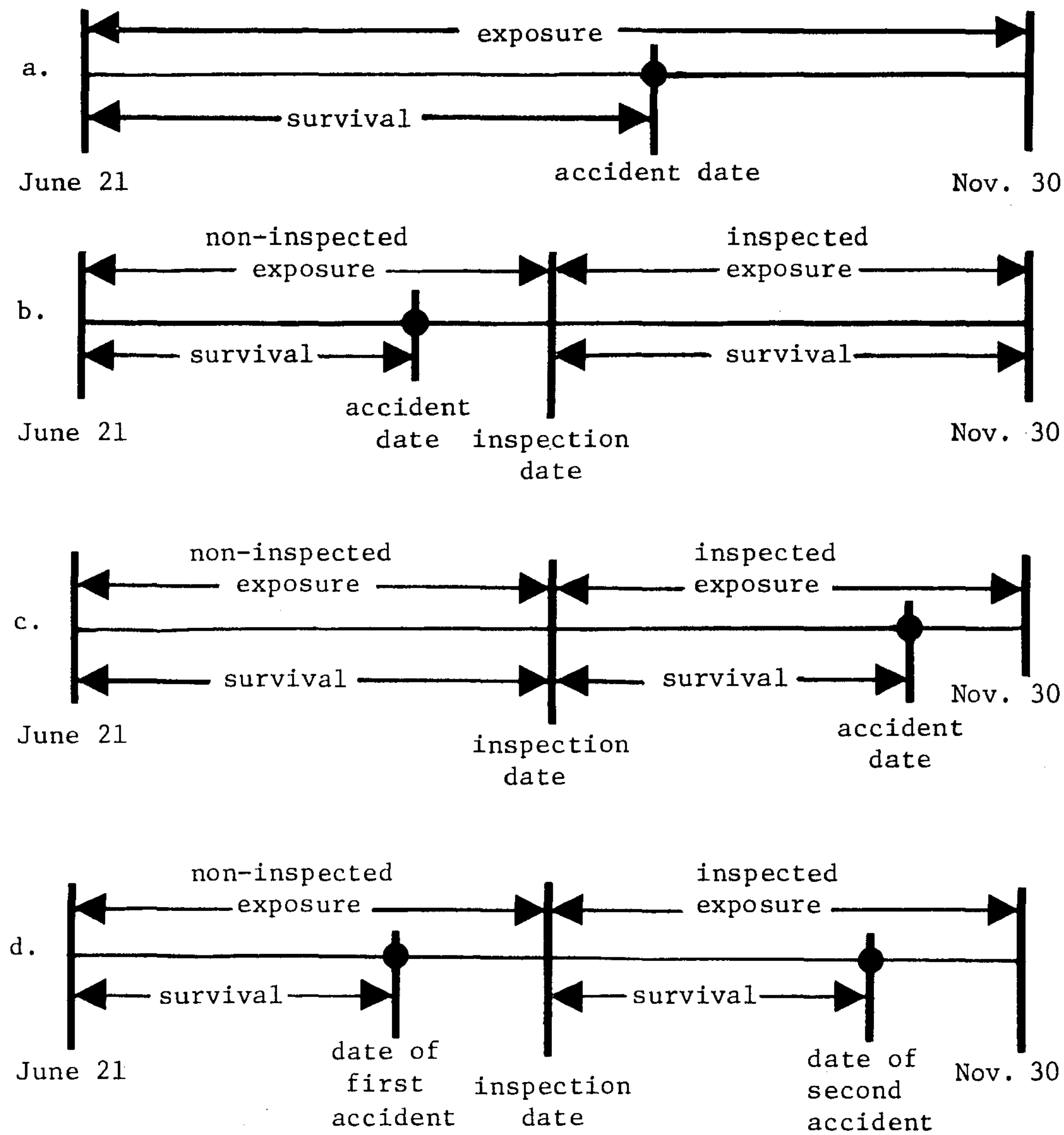

Figure 1. Typical configurations of exposure and survival intervals for inspected and non-inspected crash-involved vehicles.

a. Non-inspected or inspected prior to June 21

b. Accident in non-inspected exposure interval

c. Accident in inspected exposure interval

d. Accidents in both exposure intervals

of vehicles contributing to each. For those vehicles involved in accidents, several circumstances describing the accident were noted. These included:

1. Extent of damage

2. Accident type

3. Assignment of fault

Tables 8 - 14 illustrate the results for several vehicle types (large motorcycles, the smallest cars, standard cars, and the larger trucks) as well as for the three types of inspection systems (public, private, and Dade County[1]).

The columns of Tables 8 - 14 are as follows:

1. Number of vehicles.

2. Percentage distribution for column 1 (for non-inspected fleet and for inspected fleet).

3. Average survival days.

4. Standard deviation of survival days.

5. Average potential (exposure) days.

6. Standard deviation of potential (exposure) days.

7. Ratio of mean survival to mean potential. (Note that if the inspection is effective, this ratio for the inspected fleet should be closer to unity than the ratio for those vehicles not inspected).

8. Accidents per year per 100,000 vehicles. (Note that this rate attempts to make comparisons of the inspected and non-inspected vehicles on a more

---

[1] Dade County had had a county-operated PMVI program for nearly a decade prior to 1968. As a result, for Dade County the "non-inspected" period corresponds to the interval prior to a re-inspection during the study period. To a certain extent, Dade County serves as a control group for this study.

Table 8. Descriptive statistics for inspected and non-inspected large motorcycles (> 5 BHP) for accidents by extent of damage, accident type, and fault and for the non-accident fleet.

		Number of vehicles	Percentage dist.	Survival mean	Survival s.d.	Potential mean	Potential s.d.	(Surv. mean) / (Pot. mean)	Acc./Yr./ 100,000 veh.
NO ACCIDENT	N-I	2375	97.1	72.9	114.3	72.9	114.3	1.00	N.A.
	I	14673	96.7	119.8	100.2	119.8	100.2	1.00	N.A.
ACCIDENT									
Extent of damage									
PD	N-I	11	0.4	48.5	98.8	99.3	39.1	0.49	3390.1
	I	72	0.5	53.0	97.1	134.9	57.7	0.39	2642.6
I	N-I	58	2.4	39.8	67.4	101.9	69.3	0.39	17081.4
	I	414	2.7	56.7	107.7	132.5	52.8	0.43	15118.6
F	N-I	1	0.0	12.0	0.0	61.0	0.0	0.20	503.7
	I	10	0.1	42.7	32.0	129.4	14.4	0.33	384.2
Accident type									
SV	N-I	20	0.8	33.9	36.3	98.3	50.7	0.34	6201.5
	I	146	1.0	52.0	104.1	130.7	51.6	0.40	5502.8
MV	N-I	50	2.0	43.6	45.8	101.9	53.3	0.43	14768.0
	I	350	2.3	57.5	109.5	133.7	54.6	0.43	12725.1
Fault									
F	N-I	32	1.3	37.8	59.6	92.3	42.5	0.41	10520.4
	I	257	1.7	56.2	108.8	133.4	51.8	0.42	9417.0
NF	N-I	38	1.6	43.3	58.2	108.2	52.0	0.40	10629.0
	I	239	1.6	55.6	106.3	132.1	56.2	0.42	8858.6

Table 9. Descriptive statistics for inspected and non-inspected small cars (≤ 2500 lbs) for accidents by extent of damage, accident type, and fault and for the non-accident fleet.

		Number of vehicles	Percentage dist.	Survival mean	Survival s.d.	Potential mean	Potential s.d.	(Surv. mean) / (Pot. mean)	Acc./Yr./ 100,000 veh.
NO ACCIDENT	N-I	47945	97.4	77.0	118.6	77.0	118.6	1.00	N.A.
	I	115098	96.7	107.6	140.2	107.6	140.2	1.00	N.A.
ACCIDENT									
Extent of damage									
PD	N-I	830	1.7	45.9	100.5	95.0	101.1	0.48	13081.5
	I	2567	2.2	62.1	118.3	131.6	89.8	0.47	12100.4
I	N-I	434	0.9	47.7	105.3	95.6	101.1	0.50	6848.0
	I	1378	1.2	62.0	121.5	135.9	77.5	0.46	6353.4
F	N-I	15	0.0	56.1	118.7	105.1	100.4	0.53	217.3
	I	18	0.0	63.9	128.3	139.3	56.3	0.46	82.0
Accident type									
SV	N-I	223	0.5	52.4	99.8	97.6	94.4	0.54	3464.1
	I	807	0.7	61.3	118.5	136.4	75.3	0.45	3727.2
MV	N-I	1056	2.1	45.4	102.3	94.8	102.1	0.48	16589.5
	I	3156	2.7	62.3	119.6	132.3	87.9	0.47	14722.7
Fault									
F	N-I	615	1.2	46.7	102.2	94.9	100.6	0.49	9742.4
	I	2070	1.7	62.3	119.5	134.0	81.8	0.47	9626.3
NF	N-I	664	1.3	46.6	102.7	95.7	101.4	0.49	10421.5
	I	1893	1.6	61.9	119.3	132.3	90.1	0.47	8931.5

Table 10. Descriptive statistics for inspected and non-inspected <u>standard cars</u> (3501-4500 lbs) for accidents by extent of damage, accident type, and fault and for the non-accident fleet.

		Number of vehicles	Percentage dist.	Survival mean	Survival s.d.	Potential mean	Potential s.d.	(Surv. mean) / (Pot. mean)	Acc./Yr./ 100,000 veh.
NO ACCIDENT	N-I	137570	97.3	78.7	119.0	78.7	119.0	1.00	N.A.
	I	299106	96.4	104.3	144.9	104.3	144.9	1.00	N.A.
ACCIDENT									
Extent of damage									
PD	N-I	2524	1.8	46.9	101.8	99.3	100.5	0.47	13239.0
	I	7486	2.4	60.7	119.7	131.6	90.7	0.46	13539.6
I	N-I	1210	0.9	49.1	104.3	96.7	105.9	0.51	6578.7
	I	3645	1.2	61.2	119.7	134.9	79.6	0.45	6514.0
F	N-I	37	0.0	46.0	101.6	94.6	105.9	0.49	207.6
	I	83	0.0	58.0	112.7	139.6	73.5	0.42	145.0
Accident type									
SV	N-I	712	0.5	47.6	108.7	98.2	103.2	0.49	3828.7
	I	2341	0.8	60.0	118.7	133.9	88.9	0.45	4233.8
MV	N-I	3059	2.2	47.6	101.0	98.5	102.2	0.48	16115.9
	I	8873	2.9	61.1	119.7	132.5	86.9	0.46	15876.3
Fault									
F	N-I	1925	1.4	46.5	102.0	97.3	100.3	0.48	10353.5
	I	6005	1.9	60.9	119.1	133.2	85.6	0.46	10789.8
NF	N-I	1846	1.3	48.7	103.2	99.7	104.6	0.49	9697.1
	I	5209	1.7	60.9	120.3	132.3	89.6	0.46	9443.1

Table 11. Descriptive statistics for inspected and non-inspected large trucks (> 3500 lbs) for accidents by extent of damage, accident type, and fault and for the non-accident fleet.

		Number of vehicles	Percentage dist.	Survival mean	Survival s.d.	Potential mean	Potential s.d.	(Surv. mean) / (Pot. mean)	Acc./Yr./ 100,000 veh.
NO ACCIDENT	N-I	46360	98.2	74.1	119.3	74.1	119.3	1.00	N.A.
	I	105937	97.4	107.9	137.0	107.9	137.0	1.00	N.A.
ACCIDENT									
Extent of damage									
PD	N-I	556	1.2	47.4	100.4	96.6	103.3	0.49	8958.0
	I	1968	1.8	61.9	118.7	131.3	90.2	0.47	10140.8
I	N-I	259	0.5	45.2	99.5	92.4	111.0	0.49	4387.9
	I	809	0.7	64.4	123.8	135.4	84.1	0.48	4086.0
F	N-I	11	0.0	43.3	68.1	97.5	49.0	0.44	177.5
	I	30	0.0	81.6	99.2	139.3	70.6	0.59	148.3
Accident type									
SV	N-I	184	0.4	47.7	105.8	97.4	107.2	0.49	2961.9
	I	665	0.6	60.8	116.8	134.3	84.3	0.45	3392.0
MV	N-I	643	1.4	46.5	98.5	94.8	105.8	0.49	10538.6
	I	2142	2.0	63.4	120.8	132.0	89.9	0.48	10957.4
Fault									
F	N-I	455	1.0	47.0	104.7	94.8	107.6	0.50	7486.4
	I	1663	1.5	61.9	118.1	133.2	87.6	0.46	8469.4
NF	N-I	372	0.8	46.4	93.4	96.1	103.1	0.48	6048.7
	I	1144	1.1	64.2	123.0	131.6	89.8	0.49	5925.7

Table 12. Descriptive statistics for inspected and non-inspected standard cars in public inspection counties for accidents by extent of damage, accident type, and fault and for the non-accident fleet.

		Number of vehicles	Percentage dist.	Survival mean	Survival s.d.	Potential mean	Potential s.d.	(Surv. mean) / (Pot. mean)	Acc./Yr./ 100,000 veh.
NO ACCIDENT	N-I	89260	97.2	80.0	41.1	80.0	41.1	1.00	N.A.
	I	204456	96.0	107.1	50.8	107.1	50.8	1.00	N.A.
ACCIDENT									
Extent of damage									
PD	N-I	1720	1.9	47.4	34.4	100.9	33.6	0.47	13680.0
	I	5658	2.7	62.0	41.3	134.9	29.9	0.46	14573.2
I	N-I	864	0.9	49.6	34.3	98.9	34.3	0.50	7073.3
	I	2780	1.3	62.6	41.6	137.4	27.4	0.46	7124.9
F	N-I	24	0.0	45.1	35.8	96.4	39.0	0.47	203.6
	I	57	0.0	63.6	43.9	144.9	23.5	0.44	140.4
Accident type									
SV	N-I	485	0.5	46.9	33.8	99.2	32.9	0.47	3977.9
	I	1759	0.8	62.9	42.1	137.8	27.5	0.46	4519.8
MV	N-I	2123	2.3	48.4	34.6	100.4	34.1	0.48	16886.5
	I	6736	3.2	62.1	41.3	135.3	29.5	0.46	17211.7
Fault									
F	N-I	1321	1.4	46.5	33.7	98.6	33.8	0.47	10799.4
	I	4512	2.1	62.3	41.5	136.2	28.7	0.46	11574.3
NF	N-I	1287	1.4	49.8	35.1	101.9	33.9	0.49	10186.6
	I	3983	1.9	62.2	41.4	135.4	29.5	0.46	10306.0

Table 13. Descriptive statistics for inspected and non-inspected standard cars in private inspection counties for accidents by extent of damage, accident type, and fault and for the non-accident fleet.

		Number of vehicles	Percentage dist.	Survival mean	Survival s.d.	Potential mean	Potential s.d.	(Surv. mean) / (Pot. mean)	Acc./Yr./ 100,000 veh.
NO ACCIDENT	N-I	29021	98.1	78.0	41.3	78.0	41.3	1.00	N.A.
	I	58837	97.4	105.2	49.5	105.2	49.5	1.00	N.A.
ACCIDENT									
Extent of damage									
PD	N-I	397	1.3	46.5	34.1	98.4	36.7	0.47	10012.9
	I	1063	1.8	59.6	42.1	128.6	31.8	0.46	10074.6
I	N-I	167	0.6	52.6	37.6	97.9	38.7	0.54	4264.3
	I	500	0.8	61.0	40.9	134.1	26.3	0.46	4587.5
F	N-I	7	0.0	52.9	35.2	92.3	42.2	0.57	190.8
	I	16	0.0	44.2	37.2	125.6	36.1	0.35	158.1
Accident type									
SV	N-I	119	0.4	51.6	38.5	99.1	39.2	0.52	3008.2
	I	353	0.6	53.2	40.6	129.2	32.5	0.41	3369.4
MV	N-I	452	1.5	47.5	34.3	97.9	36.8	0.49	11430.6
	I	1226	2.0	61.8	41.8	130.6	29.6	0.47	11408.4
Fault									
F	N-I	305	1.0	47.7	34.8	97.3	36.4	0.49	7801.2
	I	882	1.5	60.2	42.0	131.0	29.9	0.46	8228.9
NF	N-I	266	0.9	49.1	35.8	99.2	38.4	0.50	6685.5
	I	697	1.2	59.5	41.4	129.4	30.8	0.46	6605.5

Table 14. Descriptive statistics for inspected and non-inspected standard cars in Dade County for accidents by extent of damage, accident type, and fault and for the non-accident fleet.

		Number of vehicles	Percentage dist.	Survival mean	Survival s.d.	Potential mean	Potential s.d.	(Surv. mean)/(Pot. mean)	Acc./Yr./ 100,000 veh.
NO ACCIDENT	N-I	19289	97.0	73.6	36.6	73.6	36.6	1.00	N.A.
	I	35813	96.9	86.6	44.6	86.6	44.6	1.00	N.A.
ACCIDENT									
Extent of damage									
PD	N-I	407	2.1	45.1	33.3	93.8	30.2	0.48	16088.0
	I	765	2.1	52.8	36.3	111.9	29.0	0.47	13643.0
I	N-I	179	0.9	43.2	32.4	85.1	32.8	0.51	7891.9
	I	365	1.0	50.4	37.1	116.9	26.0	0.43	6301.8
F	N-I	6	0.0	41.7	30.6	90.0	24.7	0.46	252.2
	I	10	0.0	48.3	31.6	132.0	13.8	0.37	154.4
Accident type									
SV	N-I	108	0.5	46.1	36.4	92.6	31.1	0.50	4387.5
	I	229	0.6	48.2	36.0	111.4	29.0	0.43	4161.9
MV	N-I	484	2.4	44.1	32.2	90.7	31.2	0.49	19691.0
	I	911	2.5	53.0	36.6	114.2	27.8	0.46	15852.7
Fault									
F	N-I	299	1.5	45.2	33.6	91.6	30.1	0.49	12162.9
	I	611	1.7	51.4	35.6	113.9	27.0	0.45	10749.7
NF	N-I	293	1.5	43.7	32.4	90.6	32.3	0.48	12061.9
	I	529	1.4	52.7	37.6	113.4	29.3	0.47	9371.1

equitable basis since, in general, the exposure of non-inspected vehicles exceeds that of the inspected vehicles).

Examination of Tables 8 - 11 (particularly the last two columns) indicates that there is no consistent advantage enjoyed by the inspected vehicles over their non-inspected counterparts. In fact, these numerical summaries flip-flop back and forth between the inspected and non-inspected groups in a fairly random manner. This can only be interpreted as evidence favoring the null hypothesis of no effect of PMVI.

Tables 12 -14 compare the standard car across the three types of inspection systems: public, private, and Dade. Although Dade County has had a PMVI program for some time, this set of tables also appears to support the hypothesis of no effect of the inspection program in reducing motor vehicle accidents. Similar inconsistencies exist for the tables not presented in this report.

Exponential modeling

One method of examining the effectiveness of a PMVI program in reducing highway crashes would be to compare the average time until a crash for the inspected and non-inspected vehicles within groups of similar type vehicles. A successful program should increase the average time until a crash by requiring the vehicle owner to correct any mechanical defects discovered by the inspection. It seems clear that the effect of mechanical improvements should be to increase the average time until a crash by virtue of reducing the chance of a crash due to a mechanical failure.

It is convenient for the estimation of the average time until a crash to specify a model or probability law which approximately describes the pattern of these "waiting" times. The actual choice of the model is somewhat subjective, but, in a study of rare events phenomena like the occurrence of motor vehicle crashes, the exponential distribution is a first choice. The specific details involved are described in Appendix B.

Estimates of the average time (in days) until a crash, a single vehicle crash, and an at-fault crash are given in Tables 15 - 17, respectively. The column labeled "public" contains the estimates obtained from counties with state-operated inspection stations. The estimates from counties with privately-owned inspection stations, like gasoline stations, are in the "private" column. Dade County is separated from the others since inspections were previously required of vehicles registered there. Most of the vehicles in Dade County

Table 15. Estimates ($\hat{\theta}$) of average time (in days) until a crash, total sample size (n) and number (m) of vehicles with a crash by type of county, vehicle classification, and inspection status.

Vehicle Classification			Public $\hat{\theta}$	Public n	Public m	Private $\hat{\theta}$	Private n	Private m	Dade $\hat{\theta}$	Dade n	Dade m	Total $\hat{\theta}$	Total n	Total m
Motorcycles (by horse-power)	less than five horse power	N-I†	2756	667	19	*	88	0	6805	116	1	3288	871	20
		I	4305	5782	166	6804	1620	31	4724	850	18	4701	8252	215
	at least five horse power	N-I	2302	1808	59	2804	280	7	5145	357	4	2514	2445	70
		I	3282	10817	393	5413	2565	60	3968	1787	43	3599	15169	496
Passenger Cars (by vehicle weight)	less than 2501 lbs.	N-I	2617	29891	888	4228	10695	191	3083	8638	200	2931	49224	1279
		I	2890	76718	2893	4448	25259	611	3379	17084	459	3187	119061	3963
	2501 lbs. to 3500 lbs.	N-I	2748	88758	2558	3576	31675	692	2556	23342	682	2861	143775	3932
		I	2711	196910	7509	4072	65516	1682	2972	41370	1198	2962	303796	10389
	3501 lbs. to 4500 lbs.	N-I	2787	91868	2608	4014	29592	571	2443	19881	592	2918	141341	3771
		I	2641	212951	8495	3978	60416	1579	2772	36953	1140	2842	310320	11214
	4501 lbs. and over	N-I	2798	11289	313	3594	1858	37	2814	2584	64	2872	15731	414
		I	3051	26165	908	6448	6296	114	2802	5029	152	3348	37490	1174
Trucks (by gross wt.)	less than 3051 lbs.	N-I	3655	4493	93	6682	1142	12	3417	833	15	3928	6468	120
		I	4033	10669	294	6412	2947	51	2953	1939	56	4185	15555	401
	at least 3051 lbs.	N-I	3972	28329	539	6067	12341	145	3193	6517	142	4205	47187	826
		I	3784	68209	1976	5924	27510	501	3533	13025	330	4136	108744	2807

† I = inspected, N-I = non-inspected

* Since no vehicles were involved in crashes only an unbounded estimate of the average time until a crash is available for this cell.

Table 16. Estimates ($\hat{\theta}$) of average time (in days) until a <u>single vehicle crash</u>, total sample size (n), and number (m) of vehicles with a single vehicle crash by type of county, vehicle classification, and inspection status.

Vehicle Classification			Type County: Public $\hat{\theta}$	Public n	Public m	Private $\hat{\theta}$	Private n	Private m	Dade $\hat{\theta}$	Dade n	Dade m	Total $\hat{\theta}$	Total n	Total m
Motorcycles (by horse-power)	less than five horse power	N-I †	10675	667	5	*	88	0	6805	116	1	11132	871	6
		I	14759	5782	49	35526	1620	6	14287	850	6	16755	8252	61
	at least five horse power	N-I	8638	1808	16	20060	280	1	6882	357	3	8946	2445	20
		I	11303	10817	116	17255	2565	19	15697	1787	11	12409	15169	146
Passenger Cars (by vehicle weight)	less than 2501 lbs.	N-I	14404	29891	164	25447	10695	32	23133	8638	27	17046	49224	223
		I	14628	76718	583	19498	25259	141	18957	17084	83	15924	119061	807
	2501 lbs. to 3500 lbs.	N-I	16052	88758	445	16667	31675	150	17527	23342	101	16398	143775	696
		I	13763	196910	1510	18355	65516	378	14471	41370	250	14658	303796	2138
	3501 lbs. to 4500 lbs.	N-I	15212	91868	485	19450	29592	119	13599	19881	108	15676	141341	712
		I	13033	212951	1759	18035	60416	353	14044	36953	229	13886	310320	2341
	4501 lbs. and over	N-I	21204	11289	42	22417	1858	6	22813	2584	8	21564	15731	56
		I	17860	26165	158	26462	6296	28	14413	5029	30	18496	37490	216
Trucks (by gross wt.)	less than 3051 lbs.	N-I	18086	4493	19	26811	1142	3	25836	833	2	19822	6468	24
		I	24082	10669	50	3835	2947	10	16782	1939	10	24315	15555	70
	at least 3051 lbs.	N-I	18324	28329	118	25266	12341	35	14787	6517	31	19048	47187	184
		I	15800	68209	480	24937	27510	120	18166	13025	65	17680	108744	665

† I = inspected, N-I = non-inspected

* Since no vehicles were involved in a crash, only an unbounded estimate of the average time until a single vehicle crash is available for this cell.

Table 17. Estimates ($\hat{\theta}$) of average time (in days) until an <u>at fault crash</u>, total sample size (n), and number (m) of vehicles with an at fault crash by type of county, vehicle classification, and inspection status.

Vehicle Classification			Type County											
			Public			Private			Dade			Total		
			$\hat{\theta}$	n	m	$\hat{\theta}$	n	m	$\hat{\theta}$	n	m	$\hat{\theta}$	n	m
Motorcycles (by horse-power)	less than five horse power	N-I+	6629	667	8	*	88	0	6805	116	1	7382	871	9
		I	7123	5782	101	10596	1620	20	7110	850	12	7644	8252	133
	at least five horse power	N-I	5106	1808	27	20029	280	1	5145	357	4	5577	2445	32
		I	6491	10817	201	9901	2565	33	7473	1787	23	7017	15169	257
Passenger Cars (by vehicle weight)	less than 2501 lbs.	N-I	5830	29891	403	7440	10695	109	6032	8638	103	6149	49224	615
		I	5711	76718	1482	8394	25259	326	5964	17084	262	6166	119061	2070
	2501 lbs. to 3500 lbs.	N-I	5405	88758	1313	6915	31675	360	5099	23342	345	5622	143775	2018
		I	5167	196910	3988	7730	65516	893	5502	41370	653	5620	303796	5534
	3501 lbs. to 4501 lbs.	N-I	5552	91868	1321	7558	29592	305	4882	19881	299	5771	141341	1925
		I	5036	212951	4512	7178	60416	882	5225	26953	611	5370	310320	6005
	4501 lbs. and over	N-I	5783	11289	153	9576	1858	14	5859	2584	31	6063	15731	198
		I	5857	26165	478	11361	6296	65	5115	5029	84	6328	37490	627
Trucks (by gross wt.)	less than 3051 lbs.	N-I	6982	4493	49	13394	1142	6	6432	833	8	7523	6468	63
		I	7156	10669	167	12194	2947	27	5047	1939	33	7448	15555	227
	at least 3051 lbs.	N-I	7584	28329	284	9916	12341	89	5560	6517	82	7675	47187	455
		I	6450	68209	1168	10141	27510	294	5836	13025	201	7028	108744	1663

+ I = inspected, N-I = non-inspected

* Since no vehicles were involved in a crash, only an unbounded estimate of the average time until an At Fault crash is available for this cell.

were inspected the previous year so there should be a diminished effect (if any) of an inspection during the Study Period.

The 16 rows represent eight vehicle classes: two classes of motorcycles, four passenger car classes, and two truck classifications. Each vehicle classification has two rows in the table, one for the inspected and one for the non-inspected vehicles. Along with the estimate ($\hat{\theta}$) of the average time until a crash, the total number (n) of vehicles in that fleet and the number (m) of these vehicles with a crash are listed.

Inspection of the three tables of estimates of the average time until a specified type of crash reveals no consistent pattern in favor of a hypothesis that the average waiting time until a crash is longer for the inspected fleet of vehicles. Note that for each table the estimates are for eight classifications of vehicles and for three types of inspection programs. The estimates for the public and private counties exhibit the same pattern of changing from the non-inspected greater than inspected and vice versa as those for Dade County. Thus, there appears to be no consistency with a hypothesis that the inspection has lengthened the time until a crash by removing the risk of mechanical failures.

IV. SUMMARY AND DISCUSSION

At the outset, the study designs utilized in the North Carolina and Florida studies appeared promising in terms of being able to detect a relationship between PMVI and accident reduction if such a relationship existed. However, with all of the problems stemming from the initiation of such a large scale statewide program coupled with the probable minor effect on the overall accident picture of periodic inspection of certain convenient mechanical or other equipment components, it is not surprising that perhaps the major contribution of this work lies in the statistical analyses employed rather than in the final results.

A recent study by the Institute for Research in Public Safety at Indiana University (1973) concluded that vehicular factors were definite causes of only 6 percent of the accidents investigated. The most frequent vehicular factors implicated as causes were deficiencies in brake systems, tires, and wheels, followed by failures in the communication system (e.g., directional signals) and steering system. Less frequent problems involved body and doors, power train and exhaust, suspension system and driver seating and controls.

Since no state PMVI program inspects all of these components, even if it were 100 percent effective in correcting defects and maintaining the vehicle in satisfactory working condition between inspections, it could at most reduce the accident frequency by less than about 6 percent. And clearly it is unreasonable to expect a massive and inexpensive (on a per car basis) program such as PMVI to be 100 percent effective.

In an ideal study of the effectiveness of PMVI, the mechanically-caused accident experience of a large number of inspected and correspondingly uninspected vehicles would be compared over a long period of time since this type of vehicle crash is a most rare event in the first place. In addition to other problems arising, neither the North Carolina nor the Florida study design was able to focus on mechanically-caused accidents of a large fleet of vehicles over a long period of time. With these limitations, it can only be said that the data in these studies suggest that the effectiveness of PMVI in reducing mechanically-caused accidents is, at most, very minimal.

REFERENCES

Buxbaum, R.C., & Colton, T. Relationship of motor vehicle inspection to accident mortality. Journal of the American Medical Association, 1966, 197, 101-106.

Cohen, A.C. Maximum likelihood estimation in the Weibull distribution based on complete and on censored samples. Technometrics, 1965, 7 (4), 579-588.

Feller, W. An introduction to probability theory and its applications. Vol. 1, (2nd ed.) New York: John Wiley & Sons, Inc. 1957.

Fuchs, V.R., & Leveson, I. Motor accident mortality and compulsory inspection of vehicles. Journal of the American Medical Association, 1967, 201, 657-661.

Garrett, J.W., & Tharp, K.J. Development of improved methods for reduction of traffic accidents. (National Cooperative Highway Research Program Report 79) Washington, D.C.: Highway Research Board, 1969.

Indiana University Institute for Research in Public Safety. A study to determine the relationship between vehicle defects and crashes. (DOT-HS-034-2-263) Bloomington: Author, 1973.

Mayer, A.J., & Hoult, T.F. Motor vehicle inspection: A report on current information, measurement, and research. Detroit: Wayne State Institute for Regional and Urban Studies, 1963.

McCutcheon, R.W., & Sherman, H.W. The influence of periodic motor vehicle inspection on mechanical condition. Ann Arbor: University of Michigan Highway Safety Research Institute, 1968.

O'Sullivan, D. Periodic motor vehicle inspection in Ireland: An appraisal and work programme outline. Paper presented at the joint meeting of the Institution of Engineers of Ireland and the Institution of Mechanical Engineers, Dublin, Ireland, 1971.

The periodic technical inspection of motor vehicles: An international survey. International Road Safety and Traffic Review, 1965, 13 (3), 17-28.

Reinfurt, D.W., House, E.G., & Levine, D.N. Periodic motor vehicle inspection in North Carolina: A follow-up study. Chapel Hill: University of North Carolina Highway Safety Research Center, 1971.

Reinfurt, D.W., & Pascarella, E.A. Periodic motor vehicle inspection in North Carolina: A descriptive study. Chapel Hill: University of North Carolina Highway Safety Research Center, 1969.

APPENDIX A

Regression Analysis of North Carolina Accident Data

The analyses regressed the square root of the accident frequencies on month of accident, terminal digit of license plate, (presumed) elapsed time since inspection, and selected interactions. The square root transformation was used as it was assumed that the motor vehicle accidents had an underlying Poisson distribution.

More specifically, the model is given by

$$E\left(\sqrt{n_{ij}}\right) = \sum_{k=0}^{43} \beta_k x_k \qquad i,\ j=1,\ldots,10$$

where

n_{ij} = number of single vehicle accidents in month i involving cars with license plate terminal digit j

β_0 corresponds to overall mean

$\beta_1,\ldots,\beta_9$ correspond to differential month effects; e.g.,

$$x_1 = \begin{cases} 1 & \text{if February} \\ -1 & \text{if January} \\ 0 & \text{otherwise} \end{cases}$$

$\beta_{10},\ldots,\beta_{18}$ correspond to differential terminal digit effects; e.g.,

$$x_{10} = \begin{cases} 1 & \text{if terminal digit 3} \\ -1 & \text{if terminal digit 2} \\ 0 & \text{otherwise} \end{cases}$$

$\beta_{19},\ldots,\beta_{26}$ correspond to differential treatment effects (see Table 4); e.g.,

$$x_{19} = \begin{cases} 1 \text{ if cell "0"} \\ -1 \text{ if cell "-"} \\ 0 \text{ otherwise} \end{cases}$$

$\beta_{27},\ldots,\beta_{43}$ correspond to combinations of month, terminal digit, and treatment interactions where

$$x_{27} = x_1 x_{10}, \quad \ldots \quad , \quad x_{35} = x_1 x_{18}$$

$$x_{36} = x_2 x_{18}, \quad \ldots \quad , \quad x_{43} = x_9 x_{18}$$

NOTE: The number of observations (100) precludes the use of all possible interactions.

Using conventional methods of weighted multiple regression with appropriate contrast matrices, a variety of hypotheses were tested which included the following:

$H_1{:}\beta_1 = \beta_2 = \ldots = \beta_9 = 0$ i.e., no month of accident effect

$H_2{:}\beta_{10} = \beta_{11} = \ldots = \beta_{18} = 0$ i.e., no terminal digit effect (or difference)

$H_3{:}\beta_{19} = \beta_{20} = \ldots = \beta_{26} = 0$ i.e., no treatment (i.e. elapsed time since inspection) effect

$H_4{:}\beta_{27} = \beta_{28} = \ldots = \beta_{35} = 0$ i.e., no January-February by terminal digit interaction

$H_5{:}\beta_{19} = 0$ i.e., non-significant initial impact of inspection

$H_6{:}\beta_{19} + 2\beta_{20} + 3\beta_{21} + 4\beta_{22} + 5\beta_{23} + 6\beta_{24} + 7\beta_{25} + 8\beta_{26} = 0$

i.e., non-significant linear decay effect of inspection

Obviously, the list extends.

Although there was a good fit of the model to the data ($R^2 > 0.8$), the least ambiguous finding was that there was a significant month effect. However, certain month by terminal digit interactions were also significant.

APPENDIX B

Exponential Modeling

<u>Exponential Waiting Times</u>

The exponential distribution of the waiting time t until an event with mean time of occurrence θ is expressible as

$$f(t;\theta) = \frac{1}{\theta} e^{-t/\theta} \qquad t > 0, \quad \theta > 0. \qquad (1)$$

The basic shape of this density function is given in Figure B-1.

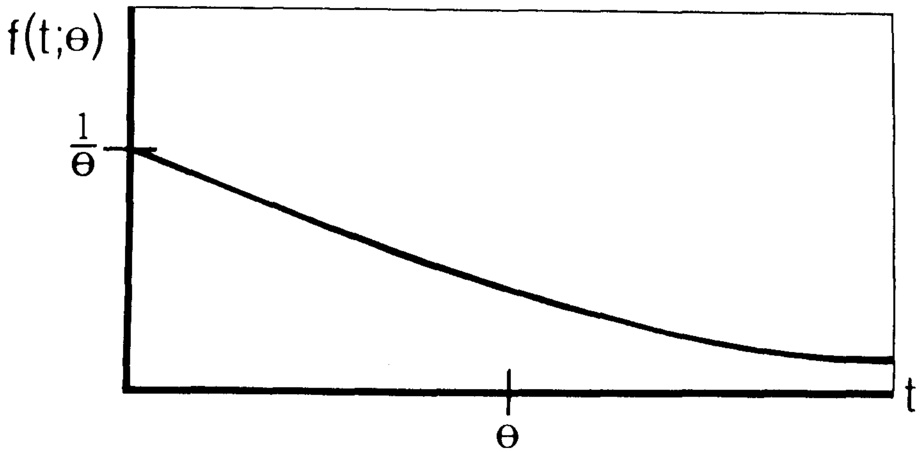

Figure B-1. The exponential distribution

The mean θ of the density can be visualized as the center of gravity, or balancing point, of the density given in (1) above.

The basic assumptions underlying this model can be described in two equivalent ways. First the distribution has the Markovian property; i.e., it has complete lack of memory. In the context of highway crashes, for a vehicle that has survived a period of time without a crash, the probability of a crash in the next t_1 units of time is the same as the probability of a crash in any previous t_1 units of time. Second, for a large fleet of vehicles, the distribution of the number of crashes in an interval of time follows a Poisson distribution. That is, the occurrence of crashes is at random and the probability of a crash in an interval of time is proportional to the length of the interval. These two descriptions are equivalent; for more details, see Feller (1957).

Truncated Observations

The fact that the mean time until the occurrence of a crash is considerably greater than the approximate six months represented by the study period presents some difficulties in the estimation of the average waiting time until a crash. The vast majority of the vehicles in the study will survive the entire period without a crash and will not be followed past the end of the study. Hence, for the non-inspected group, the observation on the i-th vehicle is either

i) the time (t_i) of survival; i.e., length of time from entry into the study until the vehicle is involved in a crash, or

ii) the time (u_i) of exposure; i.e., length of time from entry into the study until either the termination of the study or the vehicle is inspected.

Analogous definitions apply to a vehicle in the inspected category. It should be noted that any given vehicle may contribute as both a non-inspected and an inspected vehicle. Upon inspection, it changes status from the non-inspected to the inspected fleet. Since the entry into the study or the change in inspection status did not occur at the same time for all vehicles, the exposure time u_i differs from vehicle to vehicle, but is bounded by about six months, the approximate length of the study period. As the study period is less than half a year and the probability of a highway crash is relatively low, most vehicles in the study survive without a crash. The truncation (or censoring) of the data by either the end of the study or by inspection of the vehicle produces only an exposure time for most of the vehicles. As will be noted in the next section, the consequence of this is a decrease in the precision of the estimates of the average waiting time θ.

Estimation and Testing

The method of maximum likelihood is used to estimate the mean waiting time parameter θ for the non-inspected (inspected) vehicles of a specified type. The likelihood of the data is the product of contributions from each vehicle, assuming that they act independently of one another. The form of the contributing factor for the i-th vehicle depends upon whether or not the vehicle is involved in a crash. Specifically, the contribution to the likelihood is

i) $\frac{1}{\theta} e^{-t_i/\theta}$ (2)

if the vehicle is involved in a crash during the study with a survival time t_i. This is the exponential density function height at time t_i (see equation (1)) ; <u>or</u>

ii) $e^{-u_i/\theta}$ (3)

if the vehicle survives an exposure time u_i without a crash. This factor is the probability of surviving the exposure time u_i, assuming the exponential model (1). It can be visualized as the area under the density (1) and to the right of u_i (see Figure B-1).

For a group of n vehicles of a particular type of status (inspected or non-inspected), the likelihood is the product of n terms (either (2) or (3) for each vehicle). The maximum likelihood extimate of θ is the mode of the likelihood function, $L(\theta)$, i.e., the value of θ at which $L(\theta)$ is a maximum. If the number of vehicles with an accident during the study is denoted by m, then the likelihood can be written as

$$L(\theta) = \binom{n}{m} m!\ \theta^{-m} \quad \exp\left[\left(\sum_c t_i + \sum_{nc} u_i\right)/\theta\right] \tag{4}$$

where

$\sum_c t_i$ is the total survival time for those vehicles involved in crashes

$\sum_{nc} u_i$ is the total exposure for those vehicles without crashes.

For m at least one, the maximum likelihood estimate of θ is given by

$$\hat{\theta} = \frac{1}{m}\left(\sum_c t_i + \sum_{nc} u_i\right) = \bar{t} + \left(\frac{n-m}{m}\right)\bar{u} \tag{5}$$

where $\bar{t}$ is the average survival time of the crash-involved vehicles and $\bar{u}$ is the average exposure time of the n-m crash-free vehicles. Note that if all vehicles survive their exposure period without a crash,

$\hat{\theta}_2$ is unbounded. The variance of the estimate $\hat{\theta}$ can be approximated by $\hat{\theta}^2/m$. Although the crash-free vehicles contribute to the estimation of θ, the variance of the estimate decreases with m, the number of crash-involved vehicles. As would be expected, the precision of the estimate is reduced due to the truncation of some observations. One would either have to lengthen the study period or include more vehicles in order to improve the reliability of the estimates provided by this procedure. For further details, see Cohen (1965).

In a comparison of inspected with non-inspected vehicles of similar types, one test of interest is a test of the hypothesis that the mean survival time (θ') of an inspected fleet is not larger than that (θ) of a comparable non-inspected group with the alternative hypothesis that θ' exceeds θ; i.e.,

$$H_0: \theta' \leq \theta \qquad \text{vs.} \qquad H_a: \theta' > \theta$$

The appropriate large sample test statistic

$$z = \frac{\hat{\theta}' - \hat{\theta}}{\sqrt{\frac{\hat{\theta}'^2}{m'} + \frac{\hat{\theta}^2}{m}}} \tag{6}$$

is approximately distributed as a standard normal variate. An observed value of the statistic, z, would be significant at the α level if z exceeded the $(1-\alpha)\cdot 100$ percentile of the standard normal distribution. In such a situation, the data would favor the hypothesis that the average time until a crash among the inspected vehicles exceeds that for the non-inspected vehicles.

APPENDIX C

An Investigation of the Sample Size Requirements for Evaluating the Effectiveness of State Motor Vehicle Inspection Programs

Suppose, in an ideal study of the effectiveness of PMVI in reducing highway crashes, there is available a fleet of vehicles which can be divided randomly into two groups. One group of vehicles then participates in the inspection program. The accident experience of these two groups is examined over a specified period of time. How many vehicles should be included in this ideal study in order to be able to detect a given effect of inspection?

Specifically, let

N = total number of vehicles in fleet;

L = number of months in study period;

θ = expected survival time (months) for uninspected group; i.e., average time until an accident for an uninspected vehicle;

θ' = expected survival time (months) for inspected group;

$\Delta = \theta' - \theta$ = increase in average time until a crash due to the effect of vehicle inspection;

$\alpha = \Pr(\text{Type I error}) = \Pr(\text{claiming inspection has an effect} \mid \Delta = 0)$;

and

$\beta = \Pr(\text{Type II Error}) = \Pr(\text{claiming inspection has no effect} \mid \Delta > 0)$.

This appendix explores an approach to determine the sample size, N, required to detect a specified difference, Δ, in expected survival times, given θ, α, β, and L. Sample sizes are derived for a variety of combinations of Δ, θ, α, β, and L, assuming an underlying exponential distribution of waiting times, t, until an accident (see Appendix B).

Assume that the waiting time, t, until an accident for the uninspected fleet of vehicles with mean time of occurrence, θ, is expressible as

$$f(t;\theta) = \frac{1}{\theta} e^{-t/\theta} \qquad t > 0, \quad \theta > 0. \qquad (1)$$

Then,

m = expected number of <u>uninspected</u> vehicles involved in crashes during the study period

$$= (\text{no. of uninspected vehicles}) \Pr\begin{pmatrix}\text{crash during the study period} \\ \text{for an uninspected vehicle}\end{pmatrix}$$

$$= \frac{N}{2} \int_0^L \frac{1}{\theta} e^{-t/\theta} dt$$

$$= \frac{N}{2} (1-e^{-L/\theta}). \qquad (2)$$

Similarly,

m' = expected number of <u>inspected</u> vehicles involved in crashes during the study period

$$= \frac{N}{2} (1-e^{-L/\theta'})$$

$$= \frac{N}{2} (1-e^{-L/(\Delta+\theta)}). \qquad (3)$$

Let $\hat{\theta}$, $\hat{\theta}'$ represent the maximum likelihood estimates (MLE's) of θ, $\theta' = \Delta+\theta$, respectively. Then, by the asymptotic normality property of MLE's,

$$\hat{\Delta} = \hat{\theta}' - \hat{\theta} \mathrel{\dot\sim} \mathfrak{n}\left(\Delta, \frac{(\Delta+\theta)^2}{m'} + \frac{\theta^2}{m}\right) \qquad (4)$$

where $\dot{\sim}$ denotes approximately distributed, $\mathfrak{N}(\mu,\sigma^2)$ denotes normally distributed with mean μ and variance σ^2, and the variance assumes independence of inspected and uninspected groups. That is (using (2) and (3)),

$$\hat{\Delta} \;\dot{\sim}\; \mathfrak{N}\left(\Delta,\ \frac{(\Delta+\theta)^2}{\frac{N}{2}\left[1-e^{-L/(\Delta+\theta)}\right]} + \frac{\theta^2}{\frac{N}{2}\left[1-e^{-L/\theta}\right]}\right) \tag{5}$$

The null hypothesis of interest is that inspection does not increase the expected survival time until a crash with the alternative hypothesis that inspection increases the average survival time until a crash, i.e.,

$$H_0:\ \Delta \leqslant 0 \qquad \text{vs.} \qquad H_a:\ \Delta > 0$$

Under H_0,

$$\hat{\Delta} \;\dot{\sim}\; \mathfrak{N}\left(0,\ \frac{4\theta^2}{N\left[1-e^{-L/\theta}\right]}\right), \tag{6}$$

while under H_a,

$$\hat{\Delta} \;\dot{\sim}\; \mathfrak{N}\left(\Delta,\ \frac{2(\Delta+\theta)^2}{N\left[1-e^{-L/(\Delta+\theta)}\right]} + \frac{2\theta^2}{N\left[1-e^{-L/\theta}\right]}\right) \tag{7}$$

From Figure C-1, the decision rule is as follows: Reject H_0 if

$$\hat{\Delta} = \hat{\theta}' - \hat{\theta} > \Delta_c$$

and accept H_0 if $\hat{\Delta} \leqslant \Delta_c$.

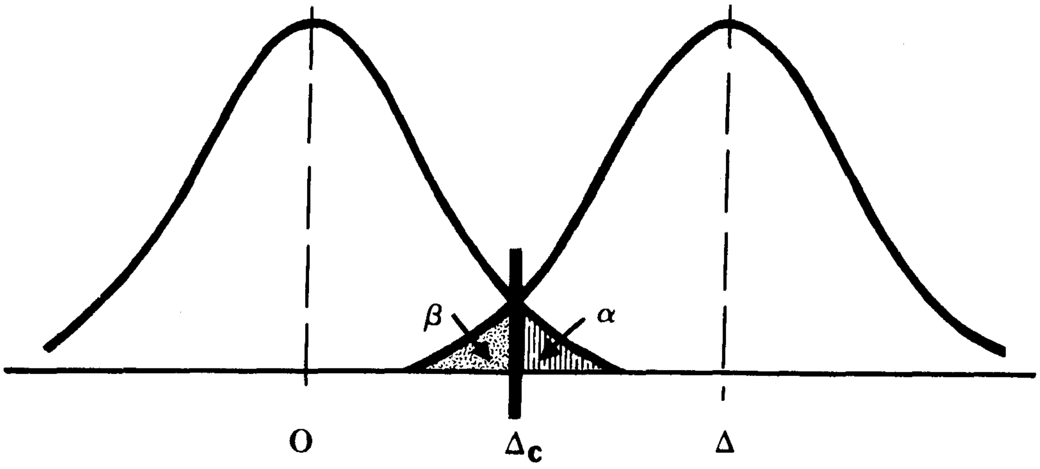

Figure C-1. Distribution of $\hat{\Delta}$ under H_0, H_a with corresponding critical value, Δ_c.

The critical value, Δ_c, is chosen to satisfy pre-specified α and β. For α = Pr (Type I error), the test statistic under H_0 relates the critical value Δ_c and the appropriate percentile of $\mathcal{N}(0,1)$. Specifically,

$$\frac{\Delta_c - 0}{\sqrt{\dfrac{4\theta^2}{N[1-e^{-L/\theta}]}}} = z_{1-\alpha} \tag{8}$$

where

> $z_{1-\alpha}$ is the $(1-\alpha)100$ percentile of the normal distribution with mean zero and variance unity.

Solving for Δ_c, we have

$$\Delta_c = z_{1-\alpha}\sqrt{\frac{4\theta^2}{N[1-e^{-L/\theta}]}} \tag{9}$$

Similarly, for β = Pr(Type II error), the test statistic under H_a relates the critical value Δ_c and the appropriate percentile of $\mathfrak{N}(0,1)$. Specifically

$$\frac{\Delta_c - \Delta}{\sqrt{\dfrac{2(\Delta+\theta)^2}{N\left[1-e^{-L/(\Delta+\theta)}\right]} + \dfrac{2\theta^2}{N\left[1-e^{-L/\theta}\right]}}} = -z_{1-\beta} \qquad (10)$$

where, due to the symmetry of the normal distribution, $z_\beta = -z_{1-\beta}$. Again solving for Δ_c, we have

$$\Delta_c = \Delta - z_{1-\beta}\sqrt{\frac{2(\Delta+\theta)^2}{N\left[1-e^{-L/(\Delta+\theta)}\right]} + \frac{2\theta^2}{N\left[1-e^{-L/\theta}\right]}} \qquad (11)$$

Equating (9) and (11) and solving for N yields

$$N = \frac{\left(z_{1-\alpha}\sqrt{\dfrac{4\theta^2}{1-e^{-L/\theta}}} + z_{1-\beta}\sqrt{\dfrac{2(\Delta+\theta)^2}{1-e^{-L/(\Delta+\theta)}} + \dfrac{2\theta^2}{1-e^{-L/\theta}}}\right)^2}{\Delta^2} \qquad (12)$$

Expression (12) was then evaluated for all combinations of the following conditions:

θ = 66, 72, 78, 84, 90, 96 (months), i.e., average time until a crash for an uninspected vehicle ranging from 5 1/2 to 8 years;

Δ = 1, 2, 3, 6, 9, 12 (months), i.e., inspection having effect of increasing average survival time from 1 to 12 months;

L = 6, 12, 18, 24 (months), i.e., study period ranging from 6 months to 2 years in length;

and

$\alpha = \beta = .01, .05, .10$, i.e., $z_{1-\alpha} = 2.326, 1.645, 1.282$, respectively.

See Table C-1 for various fleet size (N) requirements for selected combinations of θ, Δ, L, and $\alpha(=\beta)$.

Table C-1. Sample size, N, required to detect an effect of inspection, Δ, with a study period of length, L, given θ and $\alpha(=\beta)$.

		L			
		6		24	
θ	Δ	$\alpha=\beta=0.1$	$\alpha=\beta=0.01$	$\alpha=\beta=0.1$	$\alpha=\beta=0.01$
66	1	1,333,091	4,388,371	379,814	1,250,301
	3	151,560	498,918	43,138	142,006
	6	39,256	129,224	11,156	36,723
	12	10,566	34,783	2,993	9,851
78	1	2,181,576	7,181,478	609,627	2,006,818
	3	247,144	813,569	69,013	227,183
	6	63,656	209,546	17,755	58,448
	12	16,930	55,732	4,711	15,508
96	1	4,031,178	13,270,139	1,103,890	3,633,869
	3	455,013	1,497,845	124,540	409,971
	6	116,526	383,591	31,870	104,913
	12	30,618	100,790	8,361	27,523

Although the study design for the Florida investigation was not identical to that described in this appendix, it was sufficiently similar to examine whether it was realistic to expect a probable small effect of inspection to have been detectable. Thus, for example, from (12), for

L = 3 months (total of 163 days--overall about 3 months in uninspected category followed by 3 months in inspected category)

N = 451,661 standard-sized cars (the largest group)

θ = 78 months

$\alpha = \beta = .01$

The sample size was possibly large enough to detect an effect of Δ = 6 months (require $N \geq 411{,}352$) but not nearly large enough to have detected a more modest and reasonable Δ = 3 months (require $N \geq 1{,}596{,}742$).

Note that for any given θ, L, and $\alpha(=\beta)$, the sample size required for Δ = 1 month is approximately 9 times as large as for Δ = 3 months. And perhaps Δ = 1 month is a realistic expectation for the levels of inspection programs in existence. If so, it is not surprising that such an effect would be most difficult to detect in any real-world investigation.