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An Empirical Validation of
Quasi-induced Exposure

presented by

Dale Reed Lighthizer

has been accepted towards fulfillment
of the requirements for

Ph.D. degree in Civil Engineering

Major professor

Date November 10, 1989

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AN EMPIRICAL VALIDATION OF QUASI-INDUCED EXPOSURE

BY

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A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

Department of Civil and Environmental Engineering

1989

ABSTRACT

AN EMPIRICAL VALIDATION OF QUASI-INDUCED EXPOSURE

BY

Dale Reed Lighthizer

Traffic and safety engineers continue to be plagued by problems in to estimating exposure to traffic accidents. Such estimates are required in order to calculate accident rates which are useful in, for example, identifying the relative safety of different driver-vehicle groups.

The de facto standard for exposure has become vehicle miles of travel (VMT), although it is has been found to be wanting in a number of respects. In the mid 1960s, new methods were suggested, which utilized accident data as the basis of the exposure estimate. One of these, which required the specification of guilt or innocence of drivers involved in accidents, was known as quasi-induced exposure.

The quasi-induced technique has been employed by a number of researchers and has been shown by some to generally produce results consistent with other work. However, there have been few attempts to validate this method. The current research takes an empirical approach to validation. The focus is the assumption that the innocent

victims (non-responsible driver-vehicle combination) involved in two-car collisions, constitute a random sample of the driver-vehicle combinations on the road.

The validation makes use of two techniques: direct observation of the values of key variables in the field and comparison with those from the quasi-induced method; and a technique referred to here as complementary sets analysis. The latter involves an analysis of the internal consistency of the accident data. That is, if at-fault drivers are partitioned into two complementary sets, each set should encounter the same proportions, "types", of "innocent" drivers in two-vehicle accidents.

Accident data for this study were extracted from the Michigan Department of Transportation's (MDOT) 1982-1988 accident files. Field data were collected for Interstate 94 in southwest Michigan.

Generally, the comparison of field and quasi-induced exposure estimates indicated agreement between the two. While these results were not always statistically significant, they were in reasonably good qualitative agreement. The complementary sets analysis consistently yielded results supportive of the hypothesis that the non-responsible driver is a random sample of driver-vehicle combinations on the road.

ACKNOWLEDGEMENTS

I want to thank the members of my committee: Dr. William C. Taylor, Dr. John Kreer, Dr. M. Rajendra, and my able co-chairmen Dr. Thomas L. Maleck, and Dr. Richard W. Lyles for their assistance.

I also wish to thank the staff of the Michigan Department of Transportation for their cooperation. In particular, I want to express my thanks to Mr. Jack Benac who was instrumental in obtaining the required accident data, and other information.

I want to express my thanks to my father Arden Dale Lighthizer for his help in collecting and counting field data, and to Robert Zidell who helped with typing.

I want to express my gratitude to my friend Lera-Jo Cavanaugh for her support and help throughout this entire endeavor. Again, I want to thank Dr. Richard W. Lyles for his encouragement and support. Without these two individuals, I could not have realized my dream.

I want to thank my mother Katherine Lighthizer for encouraging me through the years to pursue education. I also want to thank my brothers James and Richard, my friend Paula Singer for her words of encouragement, and my buddy "Hobie", who helped me study for several years.

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1.0 INTRODUCTION

The inability to accurately and consistently measure the safety of drivers on the highway, the vehicles used, and the highway system itself has plagued traffic safety professionals for some time. While the analysis of the frequency of accidents can provide valuable insight into some highway traffic safety problems and/or the effectiveness of different solutions, it is generally conceded that investigation of simple accident frequencies is often insufficient. There is a need to assess the relative risk of certain types of drivers, vehicles, or road/environmental conditions being associated with accidents. In order to accomplish this, it is necessary to first calculate (or evaluate) how often different driver/vehicle combinations are "exposed" to certain situations.

Traditional measures of exposure include calculations of vehicle miles of travel (VMT), vehicle registrations, and number of licensed drivers along with more complex and costly derivations based on survey data. These measures are typically used in the development of overall accident rates (e.g., accidents/VMT) or disaggregated rates such as the number of males involved in accidents per VMT.

Unfortunately, virtually all measures of exposure have been found wanting due to problems with collecting appropriate unbiased data, accuracy, and/or cost.

The research effort described here deals with the investigation of a method utilizing accident data not only in the numerator of the accident rate formulation, but also as the basis for exposure in the denominator--specifically, this work is concerned with the validation of a quasi-induced measure of exposure.

One of the issues that must be dealt with initially is the question of what exposure really means--the notion of exposure turns out to be quite complicated. At a symposium at the University of California in the 1950s, over one hundred variables were identified as being measures of, or related to, exposure. Table 1 indicates some of the factors which tend to confuse the measurement of exposure.

Haight (1971) indicates that the idea of exposure to accidents evolved from the epidemiologic concept of exposure to disease. He provides a number of important definitions related to this area of research, and which are used throughout the literature.

Of importance here is the distinction that Haight makes between direct and indirect exposure. The former is expressed in terms of direct measurement of traffic parameters. The latter often involve the use of quantities other than traffic parameters, e.g. surrogates such as income, assuming that income could be associated with the

Table 1 - Factors Tending to Confuse The Measure of Exposure

Factor	Typical Problems
Sex of Driver	- simple counts of males/females may be misleading if, for example, males are more prone to accidents independent of whether they are exposed more
Age of Driver	- some age groups may be more prone to accidents regardless of level of exposure
Time of Day	- may involve factors such as light conditions, driver alertness, trip purpose, and intensity of traffic
Trip Purpose	- may affect driver behavior, aggressiveness, alertness, quality of exposure.
Vehicle Type	- some vehicle types, for some reason, may be more susceptible to involvement in accidents or certain types of accidents (e.g., certain 4-wheel drive vehicles in roll-over accidents)
Vehicle Speed	- vehicles traveling at high speeds may face a greater variety of hazards and be "exposed" more
Traffic Volumes	- more traffic-more exposure, but is it the same as less traffic, higher speeds?
Roadway Type	- some road designs or locations may be susceptible to more/less accidents regardless of exposure
Roadway Conditions	- difficult to isolate factors; some vehicles drivers may be more prone to accidents under certain conditions regardless of level of exposure
Roadside Development	- may affect the quality of exposure
Environmental Conditions	- may affect exposure for some drivers who are not systematically affected by adverse driving conditions, for example by age, sex, or experience group

risk of a highway accident.

A further refinement of indirect measurement is induced exposure which is based on accident experience. Induced exposure requires no indication of the responsible party, while quasi-induced exposure is derived from accident information including an indication of which driver-vehicle combination is identified as the responsible party. The issues related to responsibility are critical, and the distinction between the two is important.

Generally, "induced exposure" is used broadly to include quasi-induced exposure, and it should not be. Exposure will be defined here in Haight's terms, quasi-induced exposure deals specifically with a technique which uses accident data including an indication of which party is responsible.

It is important to understand how indirect measures and especially quasi-induced exposure, fit into a context of concern for appropriate exposure measures. While more detail is provided later, some key points are presented here. For example, Carroll (1971: 1) states that "driving exposure is the frequency of traffic events which create a risk of accident." This is a flexible definition of the exposure concept: it allows for a wide variety of measures; it is not exclusive in time or space; and it can be applied to any element of the system.

Carroll suggests that while this basic definition typically implies some measure of driving, it can also admit consideration of the nature of driving. He notes that the most common measure of exposure is vehicle miles of travel. When using this measure of exposure, it is assumed that all driving is equally susceptible to risk.

In related work, Chapman (1973: 95) reviews much of the literature to date and develops a concept of exposure to road accidents. He provides a summary of efforts by highway researchers and concludes that "the concept of exposure is in fact a general one; it is a concept by which the researcher tries to take account of the amount of opportunity for accidents which the driver or traffic system experiences." Chapman's work is an exhaustive review of exposure development, and he makes the important observation that the meaning of exposure has often been developed to fit the purpose of a given analysis or the available data.

For example, the meaning of exposure when applied to large groups, areas, or times is taken to be some gross measure of vehicle miles traveled, or system-level exposure. By contrast, studies concerned with exposure measures for specific locations, persons, or times often use some other definition, e.g., the exposure to accidents at a particular site as drivers pass by it.

In general, it is relatively easy to establish the value of the of the numerator of an accident rate. Problems occur with establishing a value for the denominator, the measure of exposure for a given driver-vehicle combination. While VMT has become a de facto standard measure of exposure, there are a number of problems and limitations associated with it. These fall generally in the following areas:

1) Accurate determination of VMT can be difficult to obtain as it is necessary to base the calculation on a series of assumptions and estimated values.

2) It is extremely difficult to stratify the VMT estimates into specific driver-vehicle categories (e.g., age, sex, road type).

3) Methods involving survey techniques to collect exposure data, while capable of providing quality categorical data, are extremely time consuming and expensive.

Recognition of problems associated with the collection and stratification of data into sub-groups has led to exposure being expressed as some measure of relative involvement. When exposure is viewed in this fashion, it is often obtained by induced methods, and is particularly useful in studies of a comparative nature. Various methods have been used to measure accident involvement, with considerable variation in complexity. The simplest approach is to compare the frequencies of accident involvement at locations, or for various driver-vehicle combinations. This comparison can be useful when no other information is available on exposure. However, the use of frequency information alone does not provide an opportunity for any normalized comparison between sites or groups. Further, it is clear that use of the raw number of accidents alone does not necessarily tell very much about how serious the safety problem at a location may be.

Shortcomings with use of accident frequencies and difficulties with calculating stratified estimates of VMT led to the investigation of techniques to quantify exposure with easier methods. This need stimulated the development of induced exposure methods which utilize readily available accident data. As stated, the present research deals with quasi-induced exposure where the measure of exposure is derived from the accident data. More specifically, the exposure measure of interest here is based on the distribution of "non-responsible" drivers in two-vehicle accidents. Based on such distributions, relative involvement rates can be developed for driver-vehicle combinations of interest. The fundamental premise is that under a specified set of conditions the non-responsible drivers, in two-vehicle accidents, constitute a random sample of all drivers on the road under those conditions. The relative involvement is then calculated by comparing the distribution of responsible and non-responsible drivers.

For example, suppose the relative involvement of men and women drivers in daytime accidents is of interest. The percentages of males and females involved in accidents during daylight hours can easily be determined from accident records. While the simple frequencies may be interesting, they do not address the differences between male and female drivers due to the amount of time on the road, the miles traveled, or any factors which may affect the possibility of an accident. On the surface, the initial comparison for men and women might seem to indicate that males are over-involved in daylight accidents.

However, the ratio of the number of males involved as the "responsible drivers" to the number of "non-responsible" males can be calculated a relative involvement of males. It is argued that by using the non-responsible males as a relative base for comparison, the difference in the amount of driving (exposure) by men and women is included. This same approach can be used to examine other characteristics of the driver, vehicle, and roadway environment.

This method has been demonstrated by others (discussed later), and it seems to hold great promise. Criticisms of the approach include a limited theoretical basis and few successful attempts to validate the concept empirically. The focus of the current work is to provide additional empirical validation of this technique. It should be noted that complete validation is sequential in nature.

Two general hypotheses are examined here:

Hypothesis 1: The distribution of "non-responsible" drivers in two-vehicle collisions in the accident data is the same as actually found on the highway system and;

Hypothesis 2: The distribution of "non-responsible" drivers involved in accidents caused by a specified driver-vehicle combination is the same as that of "non-responsible" drivers involved in accidents caused by the complement driver-vehicle combination.

The latter hypothesis can be best demonstrated with an example. If sex of driver is the characteristic of interest, male and female responsible drivers should involve male and female non-responsible drivers in the same proportions. (If responsible males have 60% of their accidents with non-responsible males, so should responsible females.)

Evidence supporting the latter hypothesis also supports the contention that the distribution of non-responsible driver-vehicle combinations involved in accidents is a random sample of driver-vehicle combinations found on the highway system. In this context, the goal of this research is to contribute to the empirical proof of the fundamental premise of the quasi-induced exposure technique.

This technique is of interest for a number of practical reasons: it is relatively easy to use, incorporating data that are generally available; it can be used to accurately combine system-wide and spot statistics on risk; and, a sound method of estimating exposure is important to measuring success in addressing highway safety problems. An improved method of measuring exposure would aid in the detection and quantification of problem driver-vehicle categories such as young males at night, or older drivers at night.

The next section is a literature review which is addressed to the development and discussion of various exposure measures and measurement techniques. Of specific concern is the promise that quasi-induced methods seem to hold--if they can be validated.

2.0 HISTORICAL VIEW OF EXPOSURE

While there are instances when a consideration of simple accident frequencies provides adequate information for making decisions regarding safety problems, accident frequency must generally be modified by some consideration of "exposure" to accident risk. The arguments for the need for exposure measures and for different types of exposure measures are presented in detail in this section.

In 1976, Waller reviewed and demonstrated the need for exposure information, for safety analyses, where exposure is defined as the denominator in a ratio designed to express an accident rate. This denominator describes the population at risk of involvement in an accident. Exposure may be expressed by the numbers of people or vehicles using a facility, but, more often, additional information is needed on the amount and type of use.

Chapman (1973) also discusses a number of reasons for gathering exposure data. Exposure data are necessary, combined with accident data, to act as measures of effectiveness to evaluate the safety performance of the highway system. Such measures are used to identify problems not only in the system, but also with drivers and vehicles and to evaluate the effectiveness of accident countermeasures.

Simple accident frequencies have been widely used for years, but they are generally not adequate to identify problems or measure changes. Carroll (1971a) points out that while rough exposure data (vehicle miles) have been available for some time, there is a need to have these data classified into more meaningful categories. He also notes the growth in the use of accident rates with exposure data as the denominator. He states that an advantage of using accident rates as opposed to accident frequencies, as measures of effectiveness for assessing highway safety trends, is that they are not sensitive to changes in the population at risk (normalizes accident frequencies which can be misleading). In a paper reporting on a symposium examining driving exposure, Carroll (1973: 22) reports the following recommendations and conclusions regarding exposure:

1) Exposure data are needed in highway safety research, along with accident data, to permit identification of problem areas and evaluation of countermeasures.

2) In regard to exposure data, the most pressing need is for a comprehensive data bank at the national level, though consideration must still be given to exposure data needed for special studies.

3) A comprehensive national program of collecting exposure data should begin immediately.

4) The primary use of exposure data is as the denominator in calculations of accident rates within meaningful classes of corresponding accident and exposure data.

5) For general purposes, VMT should be used as a measure of driving exposure, but further research is needed.

6) Meaningful classifications of exposure data are extremely important. The independent variables driver age, sex, vehicle type, make and model year, road type, and day/night should be used for basic classification.

These conclusions from the symposium became the driving force for research for some time.

Efforts continued to examine the question of how to define exposure, and how the use of this information might improve the investigation of highway safety. For example Stewart et al. (1976) indicated that some exposure measure is necessary in order to formulate, implement, and evaluate highway safety programs. It is of particular importance when certain groups within the driving population are the objects of comparison. Detailed exposure information for various subgroups is necessary to identify differences in accident rates for the groups, and to understand the causes for the variation. While the most common measure of exposure is VMT, other measures include: traffic volume, driving time, number of trips, number of drivers, number of licensed vehicles, passenger miles, occurrence of traffic conflicts, and fuel consumption.

Waller (1976: 2) indicates that "unless there are good measures of exposure, it is impossible to accurately determine relative frequency, causation, or countermeasure effectiveness even if numerator information [e.g.,accident frequency] is excellent." Here, the author uses the term relative frequency to refer to an accident rate. Waller also demonstrated how the choice of a denominator can significantly effect the interpretation of the effectiveness of a given countermeasure.

2.1 Accident Rates and Exposure

As indicated, exposure measures are used as the denominator in ratios defined as accident rates. This denominator represents the population at risk, a control population. A number of exposure measures have been proposed and used with VMT emerging as a popular standard. This measure is a value that directly reflects a measure of travel, and is easy to obtain and apply, although data collection requires considerable equipment and/or other resources. VMT is best suited for use at the system level of analysis, but can also be estimated for a road segment as the product of the length of the segment and the average daily traffic (ADT). Unfortunately, use of VMT implies that all miles driven have the same level of risk of having an accident. Further, it is very difficult to stratify total VMT by population subgroup.

As early as 1945, Lauer et al. performed a study to examine the relative importance of factors believed to relate to the occurrence of traffic fatalities. Of interest was how these factors can be used to establish insurance rates. The study consisted of a correlation analysis relating various factors to the number of fatalities that occurred in one year in Iowa.

They concluded that the population density of a given area was the most important factor related to traffic fatalities, followed by VMT and the number of accidents. Car registrations were not found to be an important factor as they tended to overlap with other influences. The paper was offered to suggest a procedure for insurance companies, although the results should not be accepted for all locations. This early study provides some insight into the relative importance of potential exposure measures.

A paper by Smeed (1954) reports on work by others that involves several definitions of an accident rate. These include an equivalent measure of VMT and different expressions of density of vehicles per length of road. Under conditions where the number of accidents was proportional to traffic flow, an accident rate can be a useful criterion in determining the safety of a given type of road. Most of the paper presents findings on how the accident rates behave for different times of day, lighting conditions, road types, and road geometry.

In a related study, Mathewson and Brenner (1956: 38) report on industrial accidents and provide the following caveat on the use of any accident rates:

> The safety engineering profession is constantly confronted by the problem of having to decide whether or not there has been some change in accident likelihood. The usual statistical basis for this decision, until recent years, has been the accident rate. However, the profession should accept as fact that the accident rate is a statistic that will fluctuate even without any change in the underlying accident likelihood.

In another paper, Mathewson and Brenner (1957) express doubt as to the utility of vehicle mileage as a measure of exposure for engineering or enforcement purposes. They view this method as being seriously limited because the mileage figures are estimated by gross techniques, and not measured directly. For example, VMT is usually estimated from gasoline consumption figures, vehicle registrations, and average vehicle fuel consumption figures. This type of data may be adequate for system level analysis, but extremely difficult to disaggregate for use for specific classes of roads and/or locations.

As the distance over which these measurements are made is reduced (ultimately to a point), the utility of the VMT is reduced. Another problem is that the unit of risk (vehicle-mileage) is seldom identified with the site of the risk. Mathewson and Brenner demonstrate that, for a given road segment, mileage-based measures can be reduced to a volume-based measures divided by a distance constant. They indicate that this distance constant may, in fact, tend to obscure any safety problems with the road.

Mathewson and Brenner suggest a method for defining the unit of risk as a volume-based index. Their definition of vehicle accident rates relates the number of accidents at a "point" on the road (for a time period) to the volume passing that "point" in that time period (i.e., number of accidents/traffic encounters with the specific risk). A point on the road was defined as any length where the volume is essentially constant and in length is operationally meaningful.

In a paper by Breuning and Bone (1960) interchange accident exposure and accident rates are examined. Their findings indicate that the use of vehicle mileage exposure measures can lead to confusing results. For example, for interchanges, exposure to an accident does not seem to be truly related to the miles traveled. They develop and demonstrate a method defining interchange exposure as proportional to the product of the number of cars in the two merging or diverging streams. When defined in this manner, exposure can be reduced to a simple formula using traffic counts. Tests using this method yielded satisfactory results and the authors declare that it offers a good basis for quantitative comparative analysis of interchange accidents.

Surti (1965) presented a similar method in an application to at-grade intersections. An accident exposure index is determined as a function of collision point analysis. Manipulation of the volumes entering an intersection, by movement, leads to an index directly related to volume. This index measures the relative level of intersection safety and allows for comparison of intersections with different traffic characteristics.

In 1969 Surti reported on tests of this method for data from Washington, D.C. for different types of intersection geometry. He found a good correlation between his exposure index for intersections and the number of accidents recorded. It should be noted that this method does not consider single vehicle accidents, as it is based on two traffic streams merging.

In a series of papers, Folvary (1967a, 1967b, 1967c, 1968) develops a new method to deal with exposure and reports the results of extensive testing using Australian data. The method uses the movements of vehicles prior to collision and the driver error made to define various groups. Exposure for these groups is then developed as a measure of how frequently these groups meet each other on the road. In practice, the denominator in accident rates is eliminated, thus allowing the direct comparison of accident statistics as if they were accident rates.

Thorpe (1968) reports on the application of accident rates to data for signalized and unsignalized intersections in Australia. His accident rate is calculated as a function of the volumes of traffic entering the intersection of interest. Thorpe notes that his definition is based on work by Tanner, who defined the number of potential conflicts in an intersection. An interesting conclusion was that the accident rates for signalized and unsignalized intersections were nearly the same.

In a paper by Chapman (1969), several studies using accident rates are examined. This work is of interest because it showed the potential for variation in accident rates due to several factors:

1) On roads where flows, weather, light conditions, and length are equal, road accidents were not

distributed by chance--accidents accumulated at locations associated with some type of hazard.

2) The average number of accidents per vehicle during darkness was twice that during daylight. As volumes decreased, the accidents per vehicle decreased for both day and night conditions.

3) As flow increases the proportion of single accidents decreases.

4) Speeds were lower than average on sections where the number of accidents per vehicle mile was higher than average.

5) The rate of accidents per vehicle hour was found to have less variation than the rate of accidents per vehicle mile.

In related work, Homburger (1969) states that "to make raw accident statistics at all meaningful and comparable, they must be converted to some form of accident rate." He describes three types of accident rates according to different measures of exposure: occurrence rates based on relating the number of accidents to vehicle travel, vehicle registrations, or population; involvement rates, which relate the number of accident victims (injured parties) to total population or number of drivers in accidents to the population of drivers; and severity rates, which relate classes of injuries to some measure of exposure.

Homburger discusses several problems associated with these expressions of rates and/or the exposure measures, and finds no adequate method to compare injury severity. Homburg's method then introduces a new based on an accident severity rate where accidents result in either a fatal or an injury. The fatal severity rate is defined as the total fatalities divided by the total number of fatal and injury accidents. The injury severity rate is defined similarly.

The utility of this method is demonstrated by examining different fatality rates associated with collisions between motor vehicles and other types of vehicles or pedestrians. The author suggests that it is likely that superior severity rates might be developed that will prove even more useful in examining how severity varies by accident type, time of day, location, or other factors.

A significant issue that is left unaddressed is the idea that whether an accident results in a fatal or severe injury has a large random component (and/or is functionally related to "other" factors such as speed)--i.e., an accident may be "caused" by one set of factors and result in a fatality or serious injury by virtue of another set of factors.

In work addressing accident rates for different countries, Pfundt (1969) observes three major reasons why it is difficult to use accident rates as a means of comparing the safety of roads. The points he makes also have relevance here: there is a lack of clarity about the data used (type and severity of accidents), accident rates may vary with traffic volumes, and accident occurrences are not homogeneous so simple accident rates do not provide a sufficient description of safety.

In 1970 Cameron presented theoretical work addressing the problem of detecting real differences in accident rates. He presents a model for accident processes and examines the empirical support for it. The model for accident processes is a stationary Poisson process and accident risk is expressed in terms of an accident rate based on VMT.

While, the Poisson model may provide a mechanism to better understand accident rates given the intuitive appeal of examining the accident occurrence as a (0,1) process, this method also suffers from the inherent weaknesses associated with the need for accurate mileage data for various driver-vehicle categories. The proposed stochastic process does, however, provide variances for estimates of accident rates, and not just estimates of the means from a deterministic model. This approach potentially provides for more powerful methods of statistical inference to be used.

In a paper by Chirachavala and O'Day (1983), a model using an exposure measure of VMT, along with accident information, is used to predict accidents based on the mix of traffic on a roadway. This model incorporates proportions of VMT to measure exposure for categories of vehicles. The proportion of accidents for a given category, involving different vehicle mixes (e.g., single car, car-car, car-truck) and environmental or road conditions, is predicted by the model. While this model shows some promise, for developing driver-vehicle categories to control for various factors of interest, it suffers from the same problem as others by requiring difficult stratifications of VMT information.

In summary, there are still problems with use of accident rates, and with some of the popular methods of estimating exposure. Accident rates are often not capable of expressing differences that are of interest. Estimates of exposure based on VMT do not reflect the quality of exposure (e.g., time or speed) on a roadway segment.

2.2 Collection Methods for Exposure Data

In their widely used accident research manual, Council et al. (1980) note some of the problems with exposure data and its collection. The researcher needs data for the same variables for the population at risk and the accident population. For example, if the number of accidents occurring during snowy or slippery conditions is of interest for a specified section of road, then the researcher needs some measure of the opportunities for accidents under these circumstances. This is quite restrictive for the more traditional measures of exposure such as VMT. Other problems are related to the bias that may be introduced by the way exposure data are collected. Often, data are not collected on a random year-round basis, but by some "convenient" sampling process. It is important for the researcher to be aware of how the data are collected and how bias might be introduced.

In related work, Foldvary (1968) noted methods that have been used to gather VMT data for exposure. These included estimates of fuel consumption and driver surveys. He developed a method where a random sample of drivers is selected from vehicle registration records is surveyed by mail. Drivers received a trip log for recording detailed mileage and other trip information for a specific future day's activities. This method yielded satisfactory results for an application in Australia. In order to insure an adequate sample in the US, the method would doubtlessly prove to be quite expensive.

Work by Carroll et al. (1971, 1972) for the National Highway Traffic Safety Administration (NHTSA) also examined the design of exposure surveys. A random sample of some eight thousand drivers from eighteen representative states was analyzed. Six variables were identified as the best predictors of exposure based on VMT: driver sex, driver age, vehicle type, model year, day/night, and road type. These variables were identified using the automatic interaction detection (AID) computer program. Twenty-six unique combinations of these variables were defined for further analysis.

Additional surveys were conducted to determine the most effective methods of gathering data. Indirect methods of collecting exposure data, such as gasoline sales, were judged to not be cost-effective. This conclusion was reached as a result of analysis of the effort required to obtain the desired stratified data. Other methods included various interview techniques: office, home, phone, and mail questionnaires. The mail-out method was selected as the most accurate and cost effective. The survey instrument involved the completion of a one-day trip log. Based on this effort recommendations were made for a national exposure survey field test, annual operational surveys, and the study methodology.

In a separate part of the NHTSA study, Scott and Carroll (1971) report on the inaccuracies in existing sources of highway accident data. They conclude that there is a major bias due to under-reporting of accidents--primarily due to lenient and inconsistent policies for the reporting of accidents. The accuracy of the reporting police officer regarding the severity of an injury was also examined. It was determined that, in reality, a very low percentage of accidents coded as having severe injuries actually did. While this issue is not an insignificant one, the question that impacts the current work is the simpler one of whether accidents are reported or not--as long as unreported accidents are not attributable to specific age groups , sex of driver, or the like, there is no impact on the validity of the quasi-induced approach.

Scott and Carroll also indicate that corrections in accident frequency totals might be accomplished by extrapolating reported totals. Reported figures can be adjusted by using the ratios of non-reporting derived from sample comparisons of accident records and driver surveys.

Carroll (1975) also reports on a field test of the trip-log method in Michigan in 1973 and 1974. The 1974 survey confirmed the previous conclusion that a one day trip-log mail survey on an annual, statewide basis is feasible and cost effective, compared to other survey methods.

Waller (1976) discusses three methods commonly used to estimate exposure (VMT): gasoline consumption figures, household interviews, and roadside surveys. The first has the advantage of being relative inexpensive, but the information can only be used for large-scale analysis and not disaggregated by road type, age group, or any other variable of interest. Unfortunately such disaggregation for is of interest.

The second method, household surveys, has the advantage of capturing driver-vehicle information and relating mileage to it. There are, however, several disadvantages: people have trouble (accurately) estimating their annual vehicle miles traveled; it is difficult to proportion the miles traveled by hours, days, road types, or purpose; and the surveys do not capture any information on non-residents or commercial activity in a study area.

The third method, roadside surveys, usually only provides information on relative, not total exposure. It provides information for various classes of roads and for those vehicles intercepted during the study period. The disadvantages are: it is a expensive method; and it is difficult to sample at enough sites and during enough time frames to get a representative sample.

Stewart et al. (1976) report on a more cost-effective method for estimating disaggregate VMT which is based on odometer readings recorded during state vehicle inspections, numbers of registered vehicles, and supplemental exposure information obtained by mail survey of vehicle owners. This procedure was demonstrated successfully in North Carolina where differential exposure estimates were generated by age, sex, vehicle make and model, day/night, and urban/rural.

In conjunction with the design of a Canadian study of exposure, Rochon et al. (1978) provide a complete review of efforts in the areas of exposure measurement and data collection. This study was to be a national level collection and analysis of exposure data, and personal interview format was used with a trip-log for a one-week period. The intent in making these selections was to overcome the problems of an inadequate number of responses and the need to use a representative day for the trip-log. While this methodology should yield very detailed and accurate information on exposure, the cost is typically prohibitive.

Lawson (1982) reported on the results of the 1978-1979 Canadian exposure study and indicated that the trip-log method was not successful in obtaining exposure data by road class or road surface conditions due to respondent inability to partition VMT. On a more positive note, follow-up contacts with non-responding drivers indicated that non-respondent appeared to be quite similar to respondents.

In a statistical analysis of commercial vehicle accident factors, Philipson et al. (1978) propose the use of an extrapolation technique to directly estimate VMT. The method uses average annual daily truck traffic (AADTT), by number of axles, to estimate VMT for rather specialized types of vehicles. These data are categorized according to: truck type, number of axles, and weight to allow for analysis of VMT by categories. In this work the results of extrapolating direct exposure estimates of VMT by category were compared with estimates of VMT obtained using a quasi-induced approach. Total VMT was proportioned across categories of interest using a ratio of the number of non-responsible commercial vehicle accidents for a given category to the total number of accidents involving non-responsible commercial vehicles. The induced exposure results differed considerably from the direct results, but were generally smoother. It was not possible to determine which method of estimating is preferable.

In 1978 Wolfe presented an extensive bibliography of previous work related to the measurement of highway travel exposure and included various methods for estimating VMT. Included were studies which make use of public record data, roadside counting and observation, roadside interviews, home and driver license renewal interviews, and various types of questionnaires.

This effort by Wolfe represented the first stage of developing a national exposure data system (NEDS) to complement NHTSA'S national accident sampling system (NASS).

The goal of the system was to provide exposure data for determining reliable national accident rates for various classes of vehicles, drivers, roads, and environmental conditions.

Squires et al. (1980) address some of the data needs and data collection requirements for the NASS as it relates to exposure. They also report on a pilot study and field test of a prototype system for statistical analysis of pre-crash factors and accidents. The exposure data for the NASS included: data collected from the traffic stream at sample of sites stratified according to interest; data collected from a sample of drivers about their driving habits; and data collected through follow-up interviews of drivers observed in the traffic stream to link the first two sources.

The authors also discuss the potential of obtaining exposure information from accident data. The areas thought to be most promising were: those involving the relative risk of different driving actions; and those associated with waiting time to the next accident. The authors recommend further exploration of the use of accident data to estimate exposure as it is viewed as being a relatively fast, inexpensive means of collecting first estimates of comparative exposure for different groups of driver-vehicle combinations.

Several papers by various authors provide overviews of exposure data collection. For example Toomath and White

(1982) report on the use of personal interviews on a sample of drivers in New Zealand. Wolfe (1982) discusses various commonly used methods of collecting exposure data, and more importantly, the problem that little research has been directed to determining the validity, reliability, and cost of different methods.

Lee (1982) presents the results of Michigan's effort to establish a disaggregate data base on driving exposure. The approach was based on interviews conducted by license bureau personnel. While the effort was implemented at low cost, survey management was identified as the most critical factor regarding success.

On a somewhat different tack, Fernie (1982) reports on plans in South Africa to collect exposure data using continuous automatic measurements, intermittent automatic measurements, and occasional non-automatic sampling. The first two methods involve mechanical counters collecting volume and speed data while the third incorporates time-lapse photography, or a similar method to capture several traffic characteristics. This overall approach was expected to make collection of the required data possible at an acceptable cost. Results of this project were not reported, as actual data collection was apparently ongoing.

Cambois and Fontaine (1982) discuss the use of various types of surveys to collect exposure data and the combination of these results with other data. Surveys include measures of distances traveled and observations of vehicle types, speeds, and weather conditions. Additional data on drivers and vehicles were collected in surveys conducted in unique interview points. Motorists were interviewed when stopping at service stations. These data are combined with accident data to determine the groups of greatest risk.

In summary, several methods have been utilized to gather exposure data. These have largely been attempts to improve the disaggregation of VMT information. These efforts have involved methods that have generally proved to be costly and time consuming. There is still a need to collect exposure data differentiated by road and driver characteristics in an efficient manner.

2.3 Criticism of VMT

Although widely used, VMT is not without significant problems when used as the basis for accident rate determination. While some of these problems have already been mentioned, they are addressed in a more comprehensive fashion here.

Battey (1959) notes that the use of accident rates obtained by using population, number of vehicles, and vehicle miles as exposure measures are not adequate. These methods fail to reflect the underlying relationships between deaths and variation in the amount of exposure to a hazard.

Although not commenting on the problems of singling out fatalities, Battey proposes a method which analyzes four classes of accidents: collisions with pedestrians, collisions between motor vehicles, non-collision accidents, and others. A weighted index is used to combine the effects of changes in population, number of vehicles, and vehicle mileage. An index, calculated for each class separately, reflects the different types of exposure affecting the classes.

In another paper critical of VMT, Stewart (1960: 9) discusses the three assumptions underlying use of VMT:

(a) All driving involves some exposure to accident hazards.

(b) Exposure to accident hazards is always proportional to miles driven.

(c) The degree to which exposure is associated with miles driven is the same for all drivers.

Stewart challenges each of these assumptions. The first is observed to be almost impossible to prove or disprove (although it seems obvious). He does note that "the concept of exposure has a more narrow meaning, one which takes account of probable facts in one's present, and/or immediate past environment...an individual has been exposed to a disease after he has direct contact with some carriers or has had opportunity for contact...." That is, is "exposure" occurring continuously during driving, or just when some type of hazardous condition exists. In the second case, the question is whether exposure is actually proportional to total miles driven or those while exposed to some hazard. In this instance, Stewart suggests that various other traffic and personal behavior characteristics might better reflect different levels of potential hazards.

The assumption that exposure is the same for all drivers can be questioned even under equal driving conditions. Stewart feels that exposure to driving hazards may be a dynamic measure. It is constantly changing, a function of experience, behavior, and changing road conditions. That is, different drivers simply respond differently to the same set of road conditions--what is hazardous for some, may not be for others.

Jovanis and Delleur (1983: 1) offer a somewhat similar criticism based on a study of Indiana Tollway data. They use categorical data on vehicles, environmental conditions, and VMT to examine automobile and truck accidents. They note that "VMT alone does not capture the potentially important effects that conditions of travel may have on the relative risk or danger of an accident." Instead of using an accident rate, the authors used an accident involvement rate. This measure was calculated by classifying vehicles into categories and dividing by the VMT for that category. While the VMT is still being used, it is stratified so that it provides more information.

In summary, there are problems with the use of VMT as an estimate of exposure at other than the systems level. There are also problems with the lack of sensitivity of the VMT approach to the quality of the exposure. There is still a need for an alternative approach for estimating exposure.

2.4 Induced Exposure

Due to the problems with using VMT, vehicle registrations, number of drivers, and other measures to estimate exposure, as well as the costs associated with collecting the data, researchers turned their attention to other methods for normalizing accident frequencies. Since accident data are already being collected, it stands to reason that a method that depends solely on those data would be very cost-effective.

In this context, the idea of induced exposure was first introduced by Thorpe (1964: 26). He proposed a method which developed a measure of relative exposure to the risk of an accident from analysis of accident records. The method is based on five assumptions:

(a) Single-vehicle accidents are caused entirely by attributes of the driver-vehicle combination concerned.

(b) Collision accidents are caused by the first two vehicles to hit.

(c) In each collision accident there will be a responsible and a not-responsible driver-vehicle combination. (This is a simplification since in some collisions responsibility should be shared.)

(d) The relative likelihood of a driver-vehicle combination being the responsible combination in a collision accident will be the same as the likelihood of that combination being involved in a single-vehicle accident.

(e) The likelihood of any particular driver-vehicle combination being innocently involved in a collision accident (i.e., the not-responsible combination) will be the likelihood of meeting that combination on the road.

Using these assumptions Thorpe formulated a "Relative Accident Likelihood" (RAL). The RAL for single vehicle accidents was defined as:

$$RAL(S) = \frac{S(i)}{2T(i) - S(i)}$$

where:

S(i) = proportion of the ith driver-vehicle combination of all accident combinations found in single-vehicle accidents.

T(i) = proportion the ith driver-vehicle combination of all of all combinations found in collision accidents.

and for collision accidents as:

$$RAL(T) = \frac{T(i)}{2T(i) - S(i)}$$

(see table 2 for sample RAL calculations)

The RAL(S) value is for the ith driver-vehicle combination involved in single-vehicle accidents, RAL(T) for two-vehicle collisions, and a similar ratio for all accidents. The study used Australian accident data to develop RAL figures for drivers classified by age and driving experience.

The calculated RAL values were compared to those determined for groups of drivers using the proportions of licensed drivers as a substitute for exposure. The ratio developed for comparison was defined as the proportion of single vehicle accidents involving drivers in an age group

Table 2. Sample RAL Calculations

		Percent
Male - Single-vehicle accidents	5137	(73.0)
Female - Single-vehicle accidents	1904	(27.0)
	7041	
Male - Two-vehicle accidents	4029	(76.0)
Female - Two-vehicle accidents	1271	(24.0)
	7041	

$$\text{MALE} \quad RAL(S) = \frac{73.0}{2(76.0) - 73.0} = 0.92$$

$$\text{FEMALE} \quad RAL(S) = \frac{27.0}{2(24.0) - 27.0} = 1.30$$

$$\text{MALE} \quad RAL(T) = \frac{76}{79} = 0.96$$

$$\text{FEMALE} \quad RAL(T) = \frac{24}{21} = 1.14$$

to the proportion of licensed drivers in that age group. While Thorpe found that the RAL agreed well with the "rate" using licensed drivers as exposure, the results are questionable, as the number of licensed drivers is an inherently weak measure of exposure. No comparison was made between the RAL values and any other measures of exposure, such as VMT. Further, Thorpe's paper offers little theoretical development of the proposed method.

However, the important element of Thorpe's work is the introduction of a method to determine the relative exposure various driver-vehicle combinations to traffic accidents using proportions of single-car and two-car accidents. As stated by Thorpe, the method allows the determination of a measure of exposure for very specific driver-vehicle combinations. This method does not require any determination of which driver is responsible for an accident, but does make the assumption that single-vehicle and two-vehicle driver-vehicle characteristics are the same.

As a result of Thorpe's original work, several similar methods were proposed and demonstrated by various researchers. These efforts moved in several directions. In a series of papers, Haight (1970, 1971, 1973) discusses induced exposure and proposes a modified version of Thorpe's model, based on changing one of Thorpe's assumptions. He asserts that, for example, if sex of driver is the relevant variable, the percent of males who are responsible in multi-vehicle accidents will necessarily be the same as the percent of males involved in single-vehicle accidents (who are, by definition, responsible for such accidents). In the Thorpe model a situation can arise where the ratio of a category of single-vehicle accidents could be larger than a category of multi-vehicle accidents leading to a negative result. Haight's formulation is superior to Thorpe's as it will always lead to positive results. In addition Haight provides a hypothetical example to demonstrate his method.

Haight's model unfortunately suffered from a flaw related to "null" categories. Null categories are those that are not meaningful in terms of accidents. Examples include: drivers classified by height, or vehicles by last digit of registration number. For the proposed model, the null case should yield results where the exposure proportions should exactly be equal to the proportions of singles. However, this only occurs when the number of elements in each category are equal. It can be shown that the Thorpe model will always yield the predicted proportions of singles.

In subsequent papers, Haight proposes another model which corrects the problem with null categories. The modified model involved a change in how the proportions are calculated to insure that the exposure proportions will equal those of the single proportions for the null categories. Haight does not demonstrate his approach with any application to actual data. However, in a later investigation of induced exposure models, Wass (1977) applied Haight's formulation to Danish data and found that it was not satisfactory. This was due to the model's almost total lack of sensitivity to changes in the number of single -vehicle accidents, a quantity which should significantly affect the magnitude of exposure. The lack of sensitivity was demonstrated analytically, with example calculations modifying the levels of single vehicle accidents. This method does not require that for single-vehicle accidents the driver characteristics be the same as those of the non-responsible driver in a collision, or even be the same as those for the responsible driver, for that matter.

Other quasi-induced exposure models were introduced around the same time period. Several efforts dealt with the problem concerning the specification of a responsible party in any accident. Indeed, the issue of accurately determining the responsible party in an accident became a serious issue in applying these methods.

Carr (1969: 344) used a quasi-induced exposure technique to develop a risk function he calls the Relative Risk (RR), a further modification of the Thorpe model. He applies this method to 101,000 accident records from the Province of Ontario for 1966 and 1967. Carr's model is based on four assumptions with the first three being the same as Thorpe's. The fourth is fundamental to Carr's method, and to the quasi-induced method of estimating exposure, as it is explicitly addressed to a definition of exposure:

> The frequency of involvement of any driver-vehicle combination as the non-responsible party combination in collision accidents is a measure of the exposure of that combination to (collision) accident risk.

This assumption provides the investigator with a control group, inferred from accident data, with which to compare the responsible group. Relative involvement in accidents for specified groups can then be assessed: for collision accidents, the control group is matched with the responsible group, while controlling for all environmental factors.

Carr used two criteria to determine if a driver-vehicle combination was responsible or not. For collisions involving two moving vehicles, the vehicle most responsible was determined by the investigating police officer. For the case where one vehicle was parked or stopped, it was not at fault. If no determination of responsibility could be made, the accident was dropped from consideration. This occurred less than five per cent of the time.

The RR statistic is defined as: the ratio of the frequency of occurrence of the ith category in the responsible population to the frequency of occurrence of the ith category in the non-responsible population. Carr chose to use the non-responsible population in two-car collisions as the measure of exposure for single-vehicle accidents although no effort was made to demonstrate whether this was correct.

Carr's applied work on driver age showed the calculated RR values were consistent with intuition and the results obtained by other researchers: young and old drivers were both over-represented in collisions. An important result, based on analysis of the RR statistic as a function of driver age, was that analysis of the Ontario data did not support Thorpe's fourth assumption which states:

> The relative likelihood of a driver-vehicle combination being the responsible combination in a collision accident will be the same as the likelihood of that combination being involved in a single-vehicle accident.

This assumption was critical to the development of Thorpe's method.

Carr also points out some of the criticisms of using large data sets to estimate exposure. There are problems with not all accidents being reported, and whether conclusions, based on only those that are reported, are valid. However, this is not critical unless there is bias in those accidents that go unreported. There is also the issue of whether the investigating police officer makes an unbiased determination of the responsible party. Carr cites evidence that a police officer may bias the data because of his law enforcement viewpoint.

In 1969, Hall applied a similar quasi-induced exposure technique to examine age of the vehicle operator and the age of the vehicle as accident factors. His methodology differs from Carr's in that Hall assigned responsibility to the driver-vehicle combination that received a summons. Cases involving a two-car collision where no summons was issued were discarded. Further, Hall did not include those cases where one vehicle was stopped (defined as non-responsible by Carr).

Hall's results indicated that younger and older drivers were over-represented in the accident-responsible population. The results on the effect of vehicle age were not conclusive.

Hall also noted the possible bias in this method due to determination of accident responsibility. For example, findings related to operator age may reflect a bias against old and young drivers by law enforcement personnel. This potential bias, or any introduced by the method of assigning responsibility for the accident, has been addressed, at least in part, by others (see Taylor and DeLong 1986).

Hall agreed with Carr on the overinvolvement of younger and older drivers, and, in general, on the utility of the quasi-induced exposure technique in producing results consistent with previous research. Hall also notes that the issue of any bias associated with the assessment of responsibility for the accident must be resolved before this method can be applied meaningfully.

With regard to the bias issue, Taylor and DeLong (1986) showed that, in two-car collisions, the responsible driver for the accident is seldom incorrectly assigned. Furthermore, work by McKelvey and Maleck (1987) indicated that in the few cases where the role of responsible driver was reversed, it had no effect on results. This work is discussed in more detail later.

In related work, Carlson (1970) used Hall's data and the same logic to examine whether older vehicles are overinvolved as either the responsible or non-responsible vehicle. He also examined the issue of whether younger or older drivers were more involved as either the responsible or the non-responsible party. Population data on vehicle registrations, and on drivers were used to "normalize" values. This produced estimates of responsible and non-responsible crashes per driver or vehicle by age group.

Carlson found that: older vehicles are overinvolved in responsible crashes as compared with their involvement in non-responsible crashes; and older and younger drivers are overinvolved in responsible crashes compared to their involvement in non-responsible crashes. He also concluded that the concept of induced exposure combined with population data provides a useful tool to identify overinvolved crash driver-vehicle groups. However, it is clear that population data can only provide at, best, a crude measure of exposure.

In a study of car and truck involvements, van der Zwaag (1971) applied the quasi-induced exposure method to a sample of Oakland County (Michigan) data. This study involved passenger cars and trucks (no pick-ups). Responsibility for an accident was assigned by determining if a driver had committed a violation.

The conclusion was that trucks were overinvolved in reportable accidents in this sample of Michigan data. His findings also tended to support Carlson's regarding overinvolvement of older drivers in crashes. The results were qualified with the observations that these results may not apply to other data, and that using another exposure measure may alter the results. These qualifications were necessary due to the relatively small sample size (approximately 27,000 cases), and that there was no standard to which the results of the new method could be compared.

Cerrelli (1972a) also used quasi-induced exposure methods in combination with vehicle registration data to develop exposure measures from large sets of accident data. The following indices were developed: a "liability index" which is defined as the ratio of percent responsible drivers in Class (i) to the percent licensed drivers in Class (i); an "exposure index" defined as the ratio of percent non-responsible drivers in Class (i) to the percent licensed drivers in Class (i); and a "hazard index" defined as the ratio of the two previously described indices. This index is essentially the same as a "relative involvement ratio" to be examined later in this paper. The data reviewed indicated that the "hazard index" did not vary significantly for a class of driver by location, day of week, lighting conditions, and hour of the day. This implies that relative driving performance of a class of drivers is a function of the class and is independent of environmental factors.

The values for the liability index were compared to the insurance rate index, which is based on insurance premium rates, in an attempt to validate the results. Both the liability index and the insurance rate are viewed as being similar in that they attempt to assign to a class of drivers their proportion of responsibility.

Cerrelli offers the observation that insurance premium rates might be used to replace the assignment of responsibility in quasi-induced exposure models. He indicated that finely classified insurance premium rates for driver classes might prove to be helpful, using this model, to provide reliable measures of exposure for various classes. Clearly, caution must be exercised in this approach. The present methods of determining insurance premiums do not provide a reasonable basis to determine exposure. It is easy to show that older drivers tend to be over-involved in accidents, yet their share of insurance burden does not fall on them, as their rates are usually lower. Detailed results of this study can be found in Cerrelli (1972b).

In a related paper Craw and Ku (1973) performed a mathematical analysis examining the sensitivity of indices associated with two vehicle accidents. These included the indices developed by Cerrelli. The authors examined the probability of being assigned to a class of passively involved drivers (non-responsible). This included an analysis of those who are randomly assigned responsibility for an accident, incorrectly assigned, and the effect of using a composite assignment rule.

Craw and Ku report only the theoretical aspects of the problem of assignment and do not provide any application or any indication of the impacts of incorrect assignment. Their work is noted here to indicate that the issue of assignment to the appropriate class of involvement has been addressed, at least theoretically.

Joksch (1973) reported on a field study which could be considered a pilot for the present research. The purpose of his study was to test the feasibility of gathering data by observation of traffic and to investigate the reliability of the data collected.

Joksch determined the proportions of male and female drivers through direct observation in the field at locations on selected streets. These observed data were then compared with induced exposure estimates based on accidents from these locations. An important distinction must be made here as to how the exposure estimate was made from the accident data. That is, Joksch used males and females (only) for single vehicle accidents, for comparison with the field data. Although this study involved locations that had only a few accidents the percentage of females predicted by the accident data was shown to be the same as that determined by the field counts. Joksch did conclude that the direct observation technique was a feasible method to collect data, producing results within the range of previous research.

His work can hardly be considered conclusive considering the small sample involved in the study. The study was limited to only ten locations (including some segments) with about one hundred five minute observations taking place for each. Only accidents for a one week period, for the same time period of field observation, were considered. This amounted to only 19 cases of single-vehicle accidents and 67 two-vehicle accidents.

Joksch also presents a theoretical analysis of two key points: whether the observed ratio of male/female drivers is constant; and whether the variation is random (Poisson-distributed) chance. His data suggested a rejection of the second hypothesis. However, Joksch notes that this hypothesis might be used as a first approximation until more data and a more sophisticated analysis are available.

A method of determining relative accident probabilities based on different assumptions of risk for drivers in two-car collisions is also presented. Three models are examined: equal accident risk for men and women, different risks for men and women with both parties contributing to a two-vehicle accident, and two different accident risks with one party causing a two-vehicle accident. None of the models fit the field data very well. (It is noted that the third model is essentially the same as Thorpe's.) The author notes an assumption important to the current research project: it is necessary to make the assumption that their are no changes in the ratio of males to females over time, if the method is applied over time.

In a paper reporting on a similar field experiment, Polus et al. (1988) report the results of comparative evaluations of male and female automobile drivers. Part of this study involved the direct observation of passenger car drivers on rural and urban highways in Israel, with the sex of driver being recorded. The percentages of female drivers for each class of roadway were compared to estimates of the relative numbers of females involved in accidents. Again, an important difference in how exposure was defined must be pointed out, that is, the authors use females driving passenger cars involved in all accidents. All female passenger car drivers involved in injury or fatal accidents were considered in the sample. There was no distinction made between drivers who were responsible or not responsible for an accident. The quasi-induced exposure technique makes use of only the non-responsible driver-vehicle combination as an estimate of relative exposure, which implies that the non-responsible driver-vehicles represent a random sample from the traffic stream.

The authors examined male and female accident rates for different road types, accident types, accident severity, and day of week. They concluded that there were no significant differences between the relative accident involvement rate of males and females in Israel. This finding was consistent with previous research on the subject.

A more theoretical development was presented by Koornstra (1973). An induced exposure model was introduced and the results of its application presented. This formulation is a multivariate statistical method which uses available accident data. This particular model, and several related variations, have received considerable attention. The Koornstra model purports to simultaneously determine exposure and proneness. Proneness here is meant to be that effect in accident causation that driver behavior plays for a given class of drivers under equal circumstances. The assertion is that some persons are simply more prone to be involved in accidents, both as the responsible and non-responsible driver. Wass (1977) examines the Koornstra approach at great length.

Wass applied the Koornstra approach to Danish data and obtained results which compared favorably with the results of other models. He developed a computer program known as ELXIA, which is an application of the Koornstra model that simultaneously considers exposure and liability. Extensive comparisons were made with results from applying both Thorpe's and Haight's models. This work contains a full review of induced and quasi-induced models, including a complete theoretical development of the induced exposure concepts.

Wass concluded that the Thorpe and original Haight models produced similar and acceptable results. However, Haight's corrected model did not produce acceptable results. The "corrected" Haight model produced results which diverged considerably from the exposure-liability model (Koornstra's model). Wass also concluded that the exposure liability model is superior to the Thorpe and Haight models because it produced results for exposure and liability simultaneously. The exposure-liability model is also considered by Waas to be more advanced theoretically. However, it has not been demonstrated in any body of work that there is any need for the simultaneous determination of exposure and liability, and in fact, the role of proneness has not been clearly identified. Frequently, the requirements made of the available accident data by the Koornstra model are such that calculations cannot always be carried out.

Perry and Callaghan (1980) conducted a study comparing methods for calculating exposure and accident involvement rates as a function of driver sex and driver age. These methods included use of an induced exposure technique (an application of EXLIA, the computerized version of Koonstra's model) and vehicle mileage surveys conducted in Queensland. Generally, the two methods were found to give comparable results, and the conclusion was that this induced exposure model could be useful in determining exposure and accident involvement on a yearly basis.

Mengert (1982: 11, 1985) also examines both the development and application of the Koonstra model. He states that "Koonstra's model provides a general framework in which to view induced exposure," but he makes some specific criticisms of model development and proposes a number of modifications.

When Mengert applied the Koonstra methods to data from

New York and North Carolina, he concluded, contrary to the Perry and Callaghan study, that they were not of any practical use for producing exposure estimates. This conclusion was based on the comparison of exposure estimates determined by the Koornstra methods and exposure values estimated by the Thorpe model, the Haight model, and data (VMT categorized by age, sex, and other variables) collected by direct observation in the field. Exposure estimates using Thorpe's method were also developed and compared with field estimates of exposure and found to be unacceptable.

Waller et al. (1973: 14) have also examined quasi-induced exposure models, comparing their model with a sample of reported driver exposure. Responsibility was assigned for an accident based on analysis of violations where the driver with a violation was determined to be responsible. Driver exposure was obtained from a survey of a sample of drivers drawn during license renewal.

Their conclusions form the basis for an interesting view of the induced or quasi-induced exposure concept. One result supports the finding by Carr that single vehicle accident drivers should not be used to determine the distribution of guilty drivers in multiple vehicle collisions. More importantly, in collision accidents innocent drivers more closely resembled the exposure distribution than the guilty drivers in collision accidents.

They point out obvious possible problems with mileage exposure estimates, determination of driver responsibility, and possible sampling problems in both areas. One reason offered for a lack of good comparison is that vehicle mileage may not be a good measure of driver-vehicle exposure in that it does not consider any sort of time factor. The authors make the statement that:

> It is not widely recognized unfortunately that the concept of exposure used by Thorpe in deriving induced exposure methods, which refers to the probability of a vehicular encounter, is considerably different from the concept of exposure as measured by mileage. (It is interesting to note that Thorpe matter of factly equates total exposure with total vehicle miles.)

The "probability of encounter" implies that exposure has a time component and vehicular mileage does not.

The authors also point out the need for a number of assumptions in order to compare vehicle miles as an exposure measure with those based on "road encounters." These include: speeds are essentially the same for all groups of drivers, and other road condition factors are homogeneous. Differences in road factors can cause different vehicle miles to be unequal in terms of accident frequency. The lack of validity for these assumptions can lead to poor comparisons between the results of induced exposure measures and vehicle mileage.

The authors also state that they are less comfortable with the idea of induced exposure. This is in part due to the difficulty of specifying exactly what the induced exposure measures actually describe, relative to what can be empirically validated. If induced exposure methods are to enjoy widespread use, methods must be developed to verify the results. Indeed this comment provides some of the impetus, for the work undertaken.

In another application, Kuroda (1984) used the quasi-induced exposure measure to examine vehicle design and roadway geometry as factors in highway accidents. His work demonstrated the utility of this exposure method in considering a variety of driver-vehicle combinations and road classifications. Kuroda's method is based on two assumptions:

1) The likelihood of being the object (the second vehicle) of an accident is proportional to the exposure of that vehicle.

2) The likelihood of being an object of an accident is common to any vehicle design if the exposure is the same.

The first assumption is, in part, the same as assumptions made by Thorpe, Haight, and Carr. The second reflects the application of the concept to a specific subgroup (vehicle designs). He concluded that this methodology was a valid and useful tool for accident analysis although his work did not validate the approach per se--the objective was primarily to apply this method to a specified problem.

Kuroda also reported the results of comparisons between the quasi-induced exposure results and exposure estimates made using vehicle registrations. While there was general agreement, he offers several explanations for the differences. For example, there is information from other studies, Stewart and Carroll (1980), that registration information is biased with respect to other exposure measures such as vehicle miles. It is also possible there is some error due to changes in vehicle fleet sizes over the analysis period.

One of the major criticisms of, and one of the major practical problems with, applications of the quasi-induced exposure method, is the quality of the accident data. Can, for example, fault really be accurately assigned and are there any systematic or random errors in reporting and coding such data that would be serious enough to call the assignment of responsibility into question. A number of researchers have examined the Michigan Department of Transportation (MDOT) data base which is the source of accident information for the current study. Taylor and DeLong (1986) investigated two aspects of the (MDOT) accident records: the distribution of errors in the coding of the Vehicle Identification Number (VIN); and the ability of the recording officer to correctly identify the responsible driver-vehicle combination were studied.

Taylor and Delong indicated that there appeared to be no systematic errors in this information that would lead to bias in the quasi-induced exposure technique. It was noted that while the errors associated with incorrect identification of the responsible driver-vehicle combination did not bias this exposure method, it did increase the standard error of the estimated exposure. This is of particular importance when the stratification of data leads to small cell sizes for any vehicle-driver combination.

In work related to the problem of determining the responsibility in crashes, Wasielewski and Evans (1985) developed a statistical approach to estimate driver responsibility in two-car crashes. Their model was based on relating driver age distributions in one-car and two-car crashes to the probability that both of the drivers in a two-car crash are similarly responsible for the crash as are drivers in a single-car crash. The model was applied to data from the U.S. fatal accident reporting system (FARS) for 1975-1980 and from North Carolina for 1979.

Wasielewski and Evans concluded that for two-car crashes involving a driver fatality, both drivers contribute to the responsibility for an accident in about forty percent of the cases. In less severe crashes, only one of the two drivers is generally responsible.

Studies examining the relationship between driver age and highway accidents by McKelvey and Maleck (1987) used the quasi-induced method and contained several conclusions as well as support for the approach. Results of the age study were consistent with previous studies e.g., older drivers were overinvolved in causing accidents. The results also provided implicit support for the quasi-induced exposure method as a useful tool for this type of investigation.

McKelvey and Maleck further concluded that the MDOT master accident file was a reliable source of data for research on the relationship between driver age and highway accidents. As used by McKelvey and Maleck, the definition of the responsible driver-vehicle combination is that combination which committed a hazardous action, a somewhat more liberal interpretation than used previously. They developed and tested a method by which those vehicle-driver combinations mis-coded as the non-responsible party (that combination coded as committing a hazardous action) could be corrected. There was no bias discovered in these mis-coded records. That is, there was no indication of any pattern in the mis-coded records by sex of driver, age of driver, region, or police agency. The recoding technique allows for the preservation of the data set. This is important when confronted with small size samples.

These finding are of interest here, as the ultimate source of the accident information for this study is the MDOT master accident file. The studies by Taylor and DeLong and by McKelvey and Maleck also provided an approach to the problem of the assignment of fault for an accident.

2.5 Summary

In this section, a summary of the development of methods to estimate exposure has been presented. The importance of accurate methods for estimating exposure was demonstrated in terms of better identification of driver-vehicle combinations which are over-represented in accidents, and more effective evaluations of accident countermeasures.

The shortcomings of other methods of estimating exposure such as gasoline sales, surveys, and the traditional VMT fall generally into two areas: cost considerations, and the need for data which can be disaggregated into fine strata. These difficulties led safety researchers into efforts to find an improved method of estimating exposure of driver-vehicle combinations to traffic accidents. Induced exposure and quasi-induced exposure techniques take advantage of traffic accident data already available in many cases. These methods also allow the data to be examined for fine stratifications of driver-vehicle combinations.

Basically, the criticisms and alleged shortcomings of the quasi-induced method come down to these central questions:

1) How are one-vehicle accidents handled?

2) How is the validity of assigning fault in an assessed.

3) Are non-responsible drivers a sample of all drivers?

4) Is overall validation of the quasi-induced approach possible?

While the problem of single vehicle accidents has been argued for some time, is research does not attempt to deal with it. For this research only two-vehicle accidents are considered in order to avoid confusion that might be introduced by the inclusion of single-vehicle accidents.

A key element of the quasi-induced exposure method is the need for one driver to be assigned the fault for the accident. This feature has drawn considerable criticism, including concerns regarding bias from assignment of fault and under reporting of accidents. Examination of Michigan accident data suggests that there is no bias in the assignment of fault. Further, it is argued that if there is any bias in the reporting of accidents, this would affect the responsible driver-vehicle combinations and not the non-responsible driver-vehicle combinations (the latter are chosen at random). While the bias issue cannot be totally dismissed, the most common sources of bias affect virtually all measures of exposure and/or the frequency of accidents. This is not to say that such bias can be ignored, but that it should not be a principal argument in rejecting the quasi-induced approach. For the purposes of this study, the issue concerning the validity of the assignment of relative responsibility for an accident is addressed by eliminating those where fault is not clearly defined. This is accomplished, as in previous studies, by analyzing violations and hazardous actions of the drivers involved.

The central focus of this study is the issue of whether or not the non-responsible driver-vehicle combinations are indeed a random sample of all driver-vehicle combinations--what does a sample of non-responsible driver-vehicle combinations really measure; and can it be proven that the non-responsible driver-vehicle combinations are a random sample of all traffic?

The overall validation of the quasi-induced exposure approach is incremental in nature and depends on the uncompromised defense of all the issues already discussed. This effort represents the first steps in such an overall validation. It must be noted that a major problem is encountered when validation of any exposure measure is reduced to comparing the results obtained using this approach versus those obtained by using others--it is not at all clear that there is unqualified "truth" to compare to in terms of exposure of differentiated driver, vehicle, and environmental characteristics. For example, comparisons of non-responsible driver-vehicle combinations in two-vehicle accidents with VMT may not be relevant at anything but the grossest level.

Induced exposure methods have been employed by a number of researchers, in a number of different areas, with encouraging results. However, this method is not as widely used as it might be due to a lack of theoretical development, or to the lack of a concerted effort to validate it.

It is the intent of this research to present two methods designed to, in part, validate this approach to estimating exposure. One method makes a direct comparison of results predicted by quasi-induced exposure with measurements made in the field. The other involves an empirical investigation, internal to the accident data, also designed to test the validity of the quasi-induced exposure method.

Table 3 provides a summary of the developments in the area of induced exposure.

Table 3. Induced Exposure Review

Date	Author(s)	Exposure Type	Comments
1964	Thorpe	Induced	Introduction of Technique
1969	Carr	Quasi-induced	Application
1969	Hall	Quasi-induced	Application
1970,71,73	Haight	Modified Thorpe	No assignment of fault
1970	Carlson	Quasi-induced	Application
1973	Joksch	Induced	Pilot validation
1973	Cerrelli	Quasi-induced	Application
1973	Koornstra	Multivariate	Combine exposure+proneness
1973	Waller	Quasi-induced	Test & validation
1977	Wass	All models	Test & theoretical develop.
1982,85	Mengert	Koornstra	Test & criticism
1984	Kuroda	Quasi-induced	Application, some validation
1986	Taylor,DeLong	Quasi-induced	Examined assignment fault MDOT records.
1987	Maleck,McKelvey	Quasi-induced	Application & examined quality of MDOT accident file

3.0 PROBLEM STATEMENT

Quasi-induced exposure is an attractive method of providing a relative estimate of exposure, having several advantages over more traditional methods:

1) The method is based on already available accident data.

2) Because it is based on accident data, relative exposure measures for any driver-vehicle class can be developed for any road-environmental condition. This allows for much finer exposure measurements.

However, the problem of trying to validate the quasi-induced exposure model is a difficult one. This is due to the complexity of exposure and the lack of an agreed-upon "correct" answer to how much exposure any driver-vehicle combination really has to highway accidents. The problem must be approached in a stepwise fashion, and the work here represents an early effort in this regard.

Previous work by safety researchers has demonstrated the utility of the quasi-induced methods of establishing exposure and relative accident involvement rates. This approach is simple and intuitively appealing--why has it not been used more extensively? There appear to be four reasons: how to handle single-vehicle accidents; the issue of assigning fault--there is a large body of existing accident data where the driver at fault is not identified or questionable; little theoretical or empirical proof of the method has been advanced (e.g., testing of any of the required fundamental assumptions); and there may be an accident "proneness effect," the non-responsible driver may actually be prone to having accidents.

This work is designed to address, in part, that area of concern involving the presentation of empirical proof. Two hypotheses, which are fundamental to whether the quasi-induced exposure method accurately reflects actual levels of exposure for various driver-vehicle categories, are addressed in the work presented here:

Hypothesis 1: The actual distribution of driver-vehicle characteristics of chosen variables which, in part, define the traffic stream will be reflected in like distributions of non-responsible driver-vehicle accident data (accident victims).

Hypothesis 2: The distribution of non-responsible drivers involved in accidents caused by a specified driver-vehicle category are the same as those of non-responsible drivers involved in accidents caused by the complement set of driver-vehicle combinations.

Fundamental to the quasi-induced method is the assertion that the distributions of driver-vehicle characteristics for the non-responsible driver-vehicle combinations are random samples and consequently are a measure of exposure for that driver-vehicle combination. The gist of the work here is the performance of certain empirical tests to determine whether this assertion is true.

Driver-vehicle characteristics of concern here are sex of driver and fleet mix. For the purposes of this work, fleet mix will be defined in terms of the percentages of passenger cars, pickup trucks, and "other" vehicles. "Other" includes utility trucks, semi-trucks, and all other trucks. This last category is an aggregation of several vehicle types because the numbers of accidents for these types, separately, were not adequate for analysis (see table 4).

The data to be used for various analyses are either from the State of Michigan's (MDOT's) accident files or collected in the field.

3.1 Approach for First Hypothesis

The general approach to testing the first hypothesis will be to compare estimates of exposure from MDOT's accident records with exposure data collected in the field. The latter include counts of vehicles by vehicle type and counts of drivers by sex (stratified by vehicle type). Data for the two exposure measures will be examined in terms of comparable trends and actual values for the variables noted above.

More formally, for each of the stratifying variables:

The proportion of males driving automobiles, for a given place and period of observation in the field, will be equal to the proportion of males driving automobiles who were the non-responsible parties in two-vehicle collisions.

The proportion of males driving pickup trucks, for a given place and period of observation in the field, will be equal to the proportion of males driving pickup trucks who were the non-responsible parties in two

Table 4. Study Variable Definitions

Sex of Driver

$$\%MAUTO = \frac{\text{males driving autos + station wagons}}{\text{All autos + station wagons}}$$

$$\%MPICKUP = \frac{\text{males driving pickups + vans}}{\text{pickups + vans}}$$

Fleet Mix

$$\%\ AUTO(1) = \frac{\text{autos + station wagons}}{\text{autos + pickups + others}}$$

$$\%PICKUP(1) = \frac{\text{pickups + vans}}{\text{autos + pickups + other}}$$

$$\%OTHER(1) = \frac{\text{semi-trucks + trucks + utility vehicles}}{\text{autos + pickups + other}}$$

$$\%\ AUTO(2) = \frac{\text{autos + station wagons}}{\text{autos + pickups + vans}}$$

(1) -- fleet mix one

(2) -- fleet mix two

vehicle collisions.

The proportion of automobiles (from automobiles plus pickup trucks plus others) involved in accidents, for a given place and period of observation in the field, will be equal to the proportion of automobiles from (automobiles plus pickup trucks plus others) which were not the responsible vehicle in two-vehicle collisions.

The proportion of pickup trucks (from automobiles plus pickup trucks plus others) involved in accidents, for a given place and period of observation in the field, will be equal to the proportion of pickup trucks from (automobiles plus pickup trucks plus others) which were not the responsible vehicle in two-vehicle collisions.

The proportion of others (from automobiles plus pickup trucks plus others) involved in accidents, for a given place and period of observation in the field, will be equal to the proportion of others (from automobiles plus pickup trucks plus others) which were not the responsible vehicle in two-vehicle collisions.

The proportion of automobiles (from automobiles plus pickup trucks) involved in accidents, for a given place and period of observation in the field, will be equal to the proportion of automobiles (from automobiles plus pickup trucks) which were not the responsible vehicle in two vehicle collisions.

The proportion of pickup trucks (from automobiles plus pickup trucks) involved in accidents, for a given place and period of observation in the field, will be equal to the proportion of pickup trucks (from automobiles plus pickup trucks) which were not the responsible vehicle in two-vehicle collisions.

3.2 Experimental Approach for Second Hypothesis

For the second hypothesis, an analysis of the available accident record data will be carried out to examine the distributions of responsible and non-responsible drivers--this is referred to here as complementary set analysis. Of interest is the distribution of different sub-groups of non-responsible drivers--that is, the distribution of those driver-vehicle combinations that are "hit" in accidents. Using crosstabulations of the variables of interest the distribution of non-responsible versus responsible driver-vehicle combinations will be examined. The purpose is to discover the proportions in which responsible driver-vehicle combinations strike non-responsible driver-vehicle combinations (i.e., by sex of driver). Table 5 demonstrates the general principles. For example, the cell value p(m1-m2) is the proportion of accidents where a "responsible male driver-one collides with a "non-responsible" male driver-two. This proportion will be compared with the proportion of non-responsible male driver-twos, struck by responsible female driver-ones: support for the quasi-induced method occurs when, for example, p(m1-m2)=p(f1-m2)=p(d2-m). The marginal proportions are used to calculate an involvement ratio (IR) that will be discussed in detail later. In addition to the analysis of the actual accident data, a random number model of the process will be examined to establish some practical sample size limitations.

3.3 Summary of Problem and Potential Contributions

This work can potentially make a significant contribution to the credibility of the quasi-induced exposure method. The focus is on empirical verification of the fundamental assertion that the characteristics of non-responsible driver-vehicle combinations is indeed a random sample of all driver-vehicle combinations on the highway

Table 5. Schematic Distributions of D1s and D2s by Sex

		driver-2 male	female	d1-total
	male	p(m1-m2)	p(m1-f2)	p(d1-m)
driver-1	female	p(f1-m2)	p(f1-f2)	p(d1-f)
	d2-total	p(d2-m)	p(d2-f)	N

Where: driver-1 is "at-fault,"

driver-2 is an "non-responsible,"

p(m1-m2) is a cell proportion, the proportion of accidents where driver-1 is male and driver-2 is male (the percentage of total accidents in the row) (typical);

p(d2-m) is a marginal proportion, the proportion of all driver-2s who are male (typical); and

N is the total number of accidents being considered.

And, the involvement ratio for males and females can be calculated from the marginal proportions. That is:

IR(male) = p(d1-m)/p(d2-m) and

IR(female) = p(d1-f)/p(d2-f).

system--that is, the non-responsible combination is a measure of exposure. The quasi-induced exposure method offers a number of improvements over the traditional exposure measures. It may prove significant in improving methods for accident counter-measure evaluations, and in identifying specific problem areas or drivers to be targeted for special programs.

4.0 GENERAL METHODOLOGY AND DEFINITIONS

The discussion begins here with a description of quasi-induced exposure methodology, along with some examples. The discussion continues in (chapter 5) by describing the development of procedures to test the first hypothesis using a comparison of accident data estimates of exposure with estimates of exposure determined in the field. The methodology involves a review of the experiment design, accident data collection, field data collection, and the associated data analysis. These procedures were developed to provide the basis for a field validation of the quasi-induced exposure method. The results of this approach are then presented and discussed.

The last portion of the discussion (in chapter 6) is concerned with the "complementary sets" analysis, formulated to test the second hypothesis. This discussion proceeds with a description of the logic, the accident data base, the analysis, and the random number simulation experiments. This methodology was developed to provide an internal empirical means to test the randomness of the non-responsible driver-vehicle combination. This discussion ends with a section on the results.

4.1 The Quasi-induced Exposure Approach

A highway crash is a function of the characteristics of the driver, the vehicle, the road, and the environmental conditions. The relative contributions of the various characteristics to accidents will vary--indeed it is this the researcher often wants to capture.

There are only four possible combinations of "responsible" or "non-responsible" driver-vehicles in any accident involving two vehicles: driver-vehicle #1 (hereinafter, D1) is the principal party at fault and driver-2 (D2) is non-responsible (an innocent victim); D1 and D2 substantially share in causing the accident; D2 is the party responsible for the accident and D1 is innocent; and D1 and D2 are both the victims of circumstance and neither are responsible for the accident.

Michigan provides an ideal location to validate quasi-induced exposure because all accidents are reported on common accident reporting forms (the UD-10) by all police agencies. Information is reported as to which driver (either or both), was cited for a violation and/or hazardous action. Officers investigating accidents are instructed to code D1 data first since D1 is defined on the UD-10 as "the motor vehicle and driver, which was most responsible for causing the accident" (Mi: 1984). D2 is considered to be non-responsible or "innocent". Information concerning whether a driver was cited for a violation or hazardous action is coded for each driver separately. There may be some errors in assigning responsibility or it may be impossible as both drivers are cited for some hazardous action and contributed to the accident, or neither did (weather related), or the most responsible driver could not be determined.

The data are verified in the sense that the driver designated as D1 is checked against whether the driver is cited with an actual citation or with a hazardous action. If only the category where D1 is clearly responsible (D1 received a citation and/or was cited for a hazardous action) and D2 is clearly not-responsible (D2 did not receive any citation, nor noted as having made a hazardous action) is selected for analysis (the other three discarded), it is suggested that the D2s constitute a random sample of driver-vehicle combinations found on the roadway--D2s are measures of exposure. For example, the system-wide proportions of male/female drivers showing up as D2 will be the proportion of males and female drivers encountered on the road (system-wide). The non-responsible driver (innocent victim) in a traffic accident is "selected at random," from all the driver-vehicle combinations present, by the driver that caused the accident. If, for example, it is known that there are more males driving on interstates than there are females, there will be more male D2s if two-vehicle interstate accidents are examined.

The ratio of D1 characteristics to D2 characteristics provides a relative measure of over- or under-involvement in accidents. For example, if 60% of all D2s are males (non-responsible involvement or exposure) and 75% of all D1s are male (responsible involvement), then males are over-represented according to the ratio of the two proportions, 75/60 or 1.25--that is, they cause disproportionately more accidents than the other group (females). The important point here is that this ratio is adjusted for the number of male drivers on the highways under some specified set of conditions. For female drivers, the ratio would be .625; they are under-represented. A value of 1.0 for males would mean, in this case, males cause accidents proportionately to their presence on the road. Simply looking at the D1 involvements would indicate that males cause (relative to females) accidents at a 3:1 rate, a serious over-estimate. Indeed, the "involvement ratio" approach indicates that, relative to females, males are over-represented in causing accidents at a 2:1 (1.25/.625) rate (although this ratio of ratios should be used with great care).

5.0 COMPARISON OF FIELD OBSERVATIONS AND ACCIDENT DATA

One of the most direct methods to validate the quasi-induced exposure techniques is to subject its fundamental principles to testing. This section describes an experiment designed to compare measures of exposure estimated by the quasi-induced technique (D2 proportions) with measures of exposure collected in the field.

From the accident data, variables of interest here are the sex of the non-responsible driver, and the fleet mix percentages for the non-responsible vehicle. The values of interest are the relative percentages of males and females, and various vehicle types, on a given road type. The object here is to test the following hypothesis (as stated earlier):

Hypothesis 1: The actual distribution of driver-vehicle characteristics of chosen variables which, in part, define the traffic stream will be reflected in like distributions of non-responsible driver-vehicle accident data (accident victims).

The essential point is the assertion that the distribution of non-responsible drivers in accidents is the same as a random sample from the traffic stream. It follows, that if this is true, that a traffic stream could be sampled, and the values observed from a field determination would be similar to those estimated by analysis of accident data. That is, these two data sources should yield similar results if the hypothesis is correct.

Discussed in this section are some observations on the characteristics of the field data collected, and how this field data compares with that observed in previous work by others. Results of comparing the field data with the quasi-induced exposure data are also reported and analyzed.

5.1 Experiment Methodology

This section describes the methodology used to test the first hypothesis. Four parts of the experimental plan are discussed: site selection for data collection; field-data collection; accident data collection; and data analysis.

5.1.1 Field Study Location

The area chosen for this study is a segment of interstate highway 94 (I-94) passing through six southern Michigan counties (see figures 1 and 2). These counties can generally be characterized as rural, with some urban influence, particularly in the east. This site was selected because it allowed for manageable, accurate collection of the data of interest. Preliminary examination of accident data for the entire I-94 corridor across Michigan indicated some potential data reliability problems in the Detroit metropolitan area. Hence, the metropolitan counties were not included in the study. The limitations of both time and

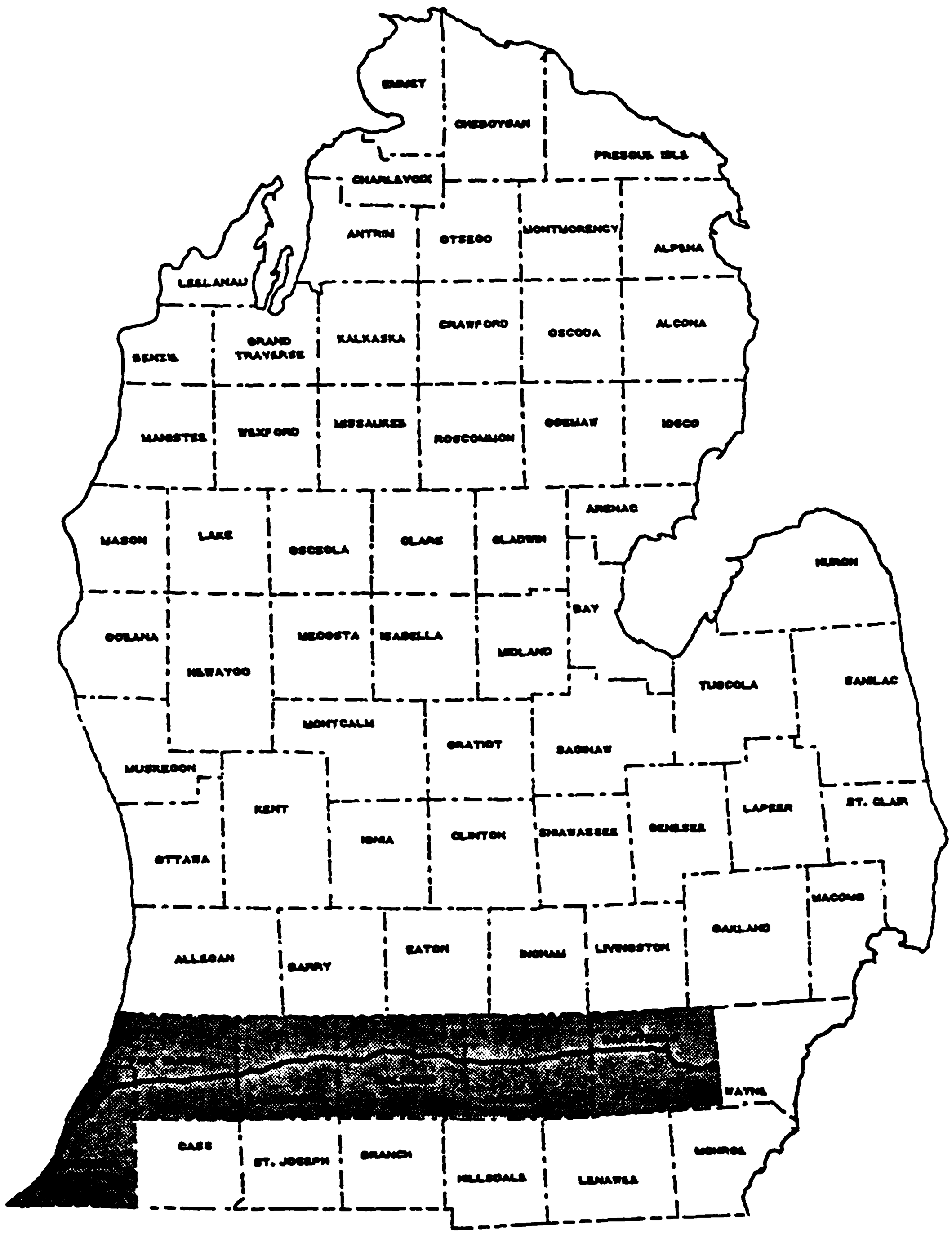

Figure 1. Statewide Location Map

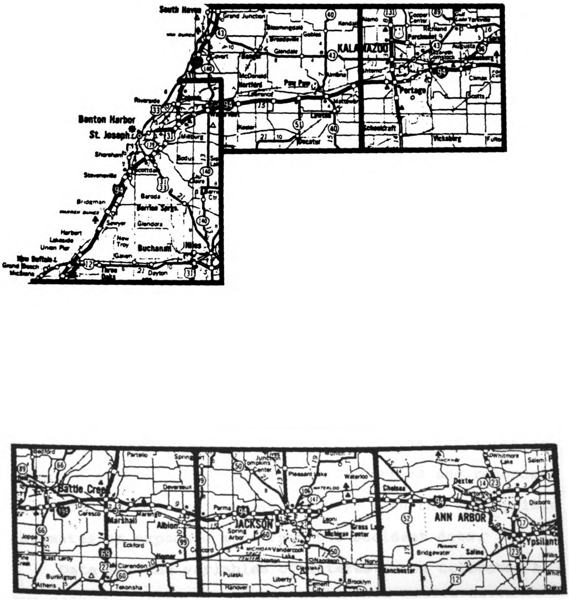

Figure 2. County Location Map

manpower also served to limit the amount of field data that could be gathered.

It was decided that one type of roadway would be isolated to control for any effect of roadway type. This provides the opportunity to investigate a well-defined roadway class, preferable to one less well-defined which may reflect an aggregation of several classes of roadway data. This also served to keep the data collection portion of this research at a reasonable level.

5.1.2 Field Data Collection

Field data for this study were generally collected in rest areas along the I-94 corridor (see figure 3), except in Van Buren County, where data were collected at a dead-end road adjacent to the freeway right of way. These locations proved to be safe and satisfactory observation positions.

Field data were collected for a study section of road by direct observation and video recording. The variables of interest in the traffic stream were observed and recorded. These can be compared in several ways with the appropriate estimates of exposure made from the accident data.`

Data for the variables of interest were collected by two methods. Data on sex of driver were mechanically recorded, based on direct observation, and tallied for autos and pickup trucks. Sex of driver information for the "other" category was not recorded, since, during preliminary study, few female drivers were observed for this group.

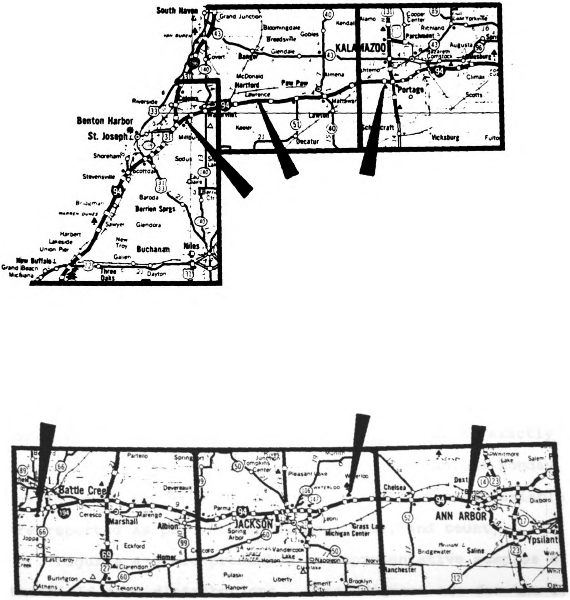

Figure 3. Count Location Map

The required data for fleet mix were collected, in the majority of cases, using video recording equipment. Data were read from the tapes in six vehicle categories, and later collapsed into the desired groups. In a few cases vehicle type data were monitored by direct observation and tallied in the field.

Field data were collected for one "week" in each county. Each "week" of data consisted of:

five days -- Monday, Wednesday, Friday, Saturday, Sunday.

six hours daily --
8:00 A.M. - 9:00 A.M.
9:00 A.M. - 10:00 A.M.
10:00 A.M. - 11:00 A.M.
2:00 P.M. - 3:00 P.M.
3:00 P.M. - 4:00 P.M.
4:00 P.M. - 5:00 P.M.

The purpose of the data collection scheme was to detect any variation by county, day of the week, and hour of the day. Unfortunately, limited resources precluded the possibility of making repeated measurements during exactly the same period of observation (i.e., more than one Monday 8-9 AM count for a given county). Values for each variable are reported as percentages by hour, day, and county.

Figures 4-9 are presented as representative samples of the field data distributions. These figures are for the sex of driver variable--percent males driving autos, for each of the six counties. The remaining data are compared in Appendix A. The following general observations can be made about the field data: there does not seem to be any strong

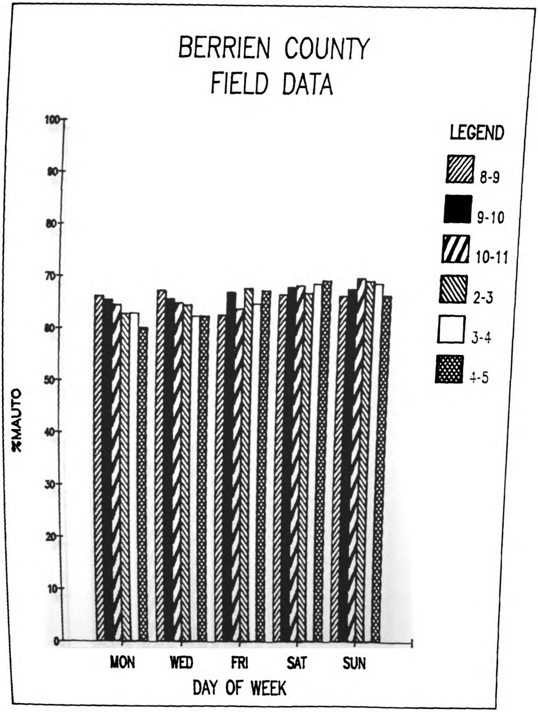

Figure 4. Field Data for Berrien County - %MAUTO

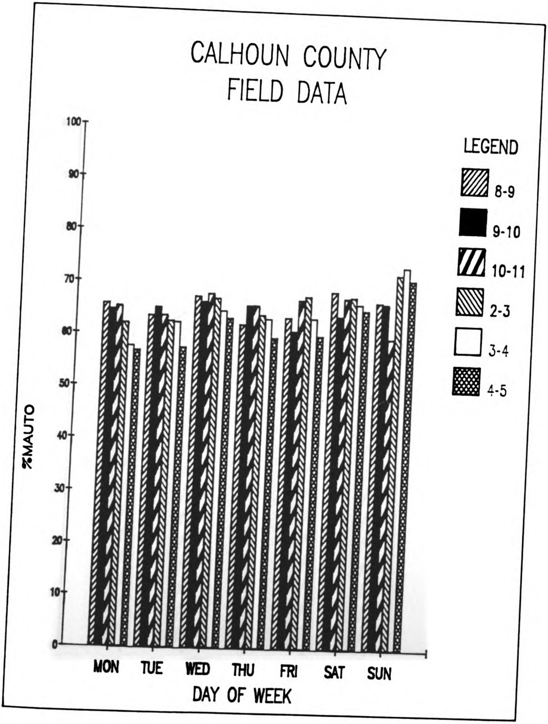

Figure 5. Field Data for Calhoun County - %MAUTO

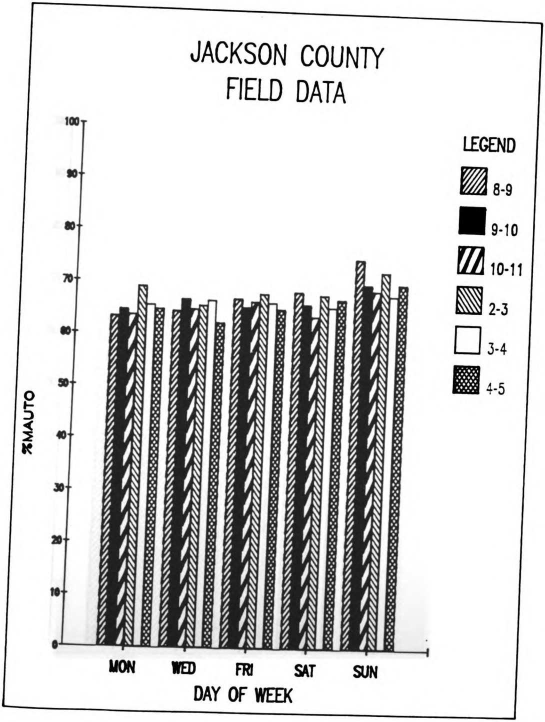

Figure 6. Field Data for Jackson County - %MAUTO

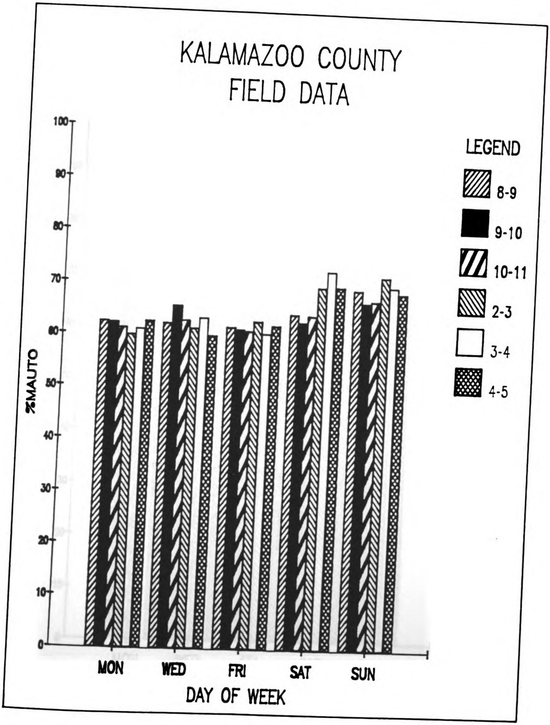

Figure 7. Field Data for Kalamazoo County - %MAUTO

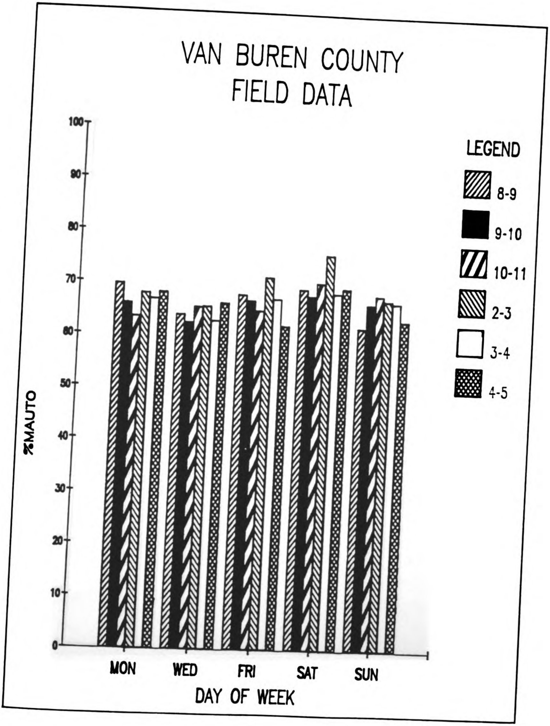

Figure 8. Field Data for Van Buren County - %MAUTO

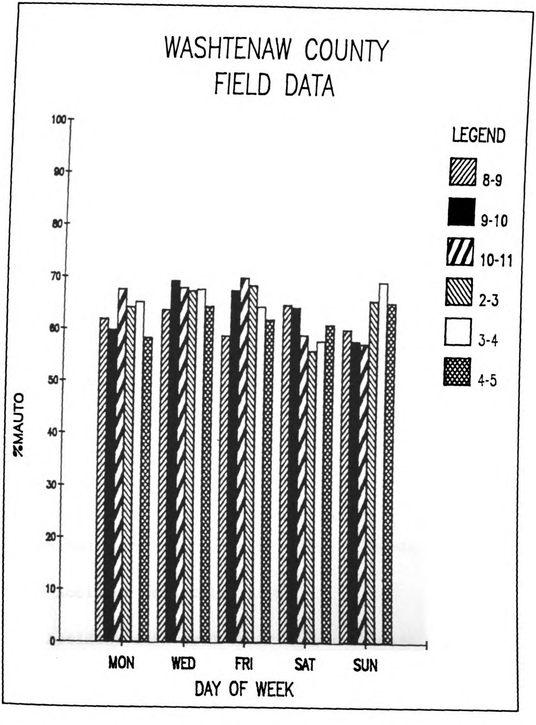

Figure 9. Field Data for Washtenaw County - %MAUTO

trends regarding the behavior of the variables with respect to either time of day, or day of week. There were slight trends towards more males driving on Saturdays and Sundays (figures 4, 5, 6, and 7) and towards less "other" vehicles (in particular semi-trucks) on weekends (figures 19, 24, 29, 34, 39, 44) in Appendix A.

Figure 10 shows the total percentage of males driving autos by county in which they were counted. There is a slight trend toward more males in the western counties (Berrien and Van Buren), which is a more rural area. (Similar figures for the remaining variables are in Appendix A.)

As data were gathered in the field it became clear that it is necessary to state a clear definition for each vehicle type, when classifying vehicles for fleet mix. Both in the field, and during reduction of the video-taped data, it was hard to classify some types of trucks and vans. There may also be some ambiguity in how some vehicles are classified by the police on the accident reports. This should not pose a major problem here, for after some investigation it was determined that there were not many cases involved.

5.1.3 Accident Data Collection

Accident data used to develop estimates of exposure for the variables of interest were extracted from MDOT's accident master file. Information was collected for the same section of I-94 as specified for the field data. The

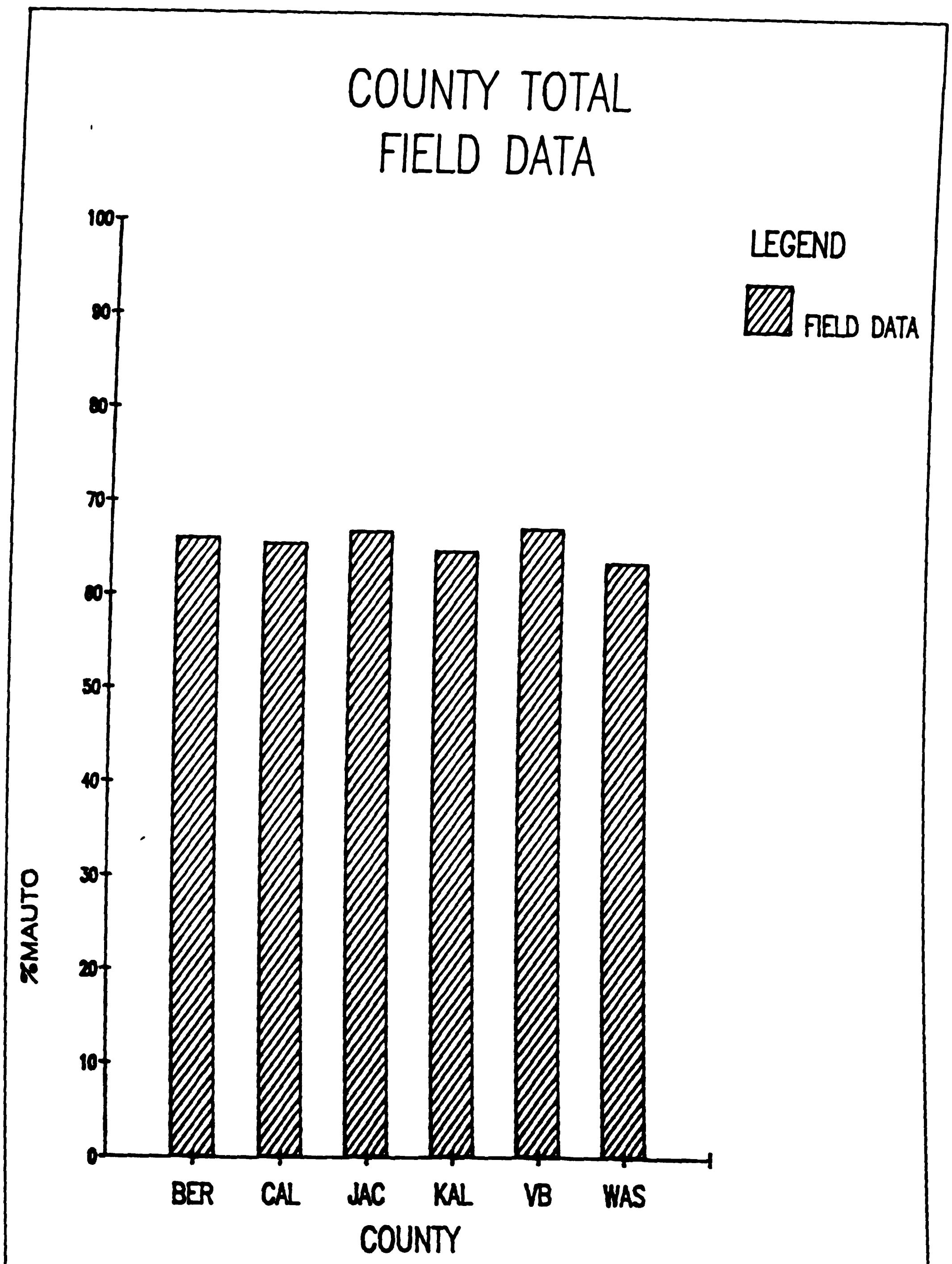

Figure 10. Field Data for %MAUTO by County

base record is the MDOT's so-called "252 format." This record was reduced to a more manageable size by including only those variables that are essential to performing this study (see table 6). In theory, the values for the variables of interest obtained from the field can be compared with those generated by the quasi-induced method applied to this accident data.

Six years of accident data, 1982-1987, were considered for the accident data base for this study. The preliminary analysis of the accident data turned out to be more difficult than expected--primarily due to small sample size. It was necessary to examine various aggregations of the data to see if they were "legitimate." The purpose was to identify distributions of accident characteristics that were reliable enough to be compared with the collected field data. For example, it became clear at a relatively early point that there was not enough accident data in any given year to compare with a specific hour of field data for a given county.

The issue of whether these data can be aggregated across seasons must be also addressed. That is, are there any seasonal differences in these data that would prevent the aggregation of data across all seasons into representative years. This issue must be considered here because field data were only collected during summer months. The goal is to develop an accident data set that is as large as possible prior to comparing it with the field data.

Table 6. Principal Variables of Interest

Accident Data Variables

Physical Road Section Number
Year
Weekday
Hour
Month
County
Weather Condition
Light Condition
Accident Type
Number of Vehicles Involved
Vehicle Type 1,2
Vehicle Make 1,2
Age of Driver 1,2
Sex of Driver 1,2
Violation of Driver 1,2
Hazardous action 1,2
Weight of Vehicle 1,2
Urban/Rural Flag

Notes:

1 - indicates data pertaining to driver-1 "responsible" driver-vehicle combination

2 - indicates data pertaining to driver-2 "non-responsible" driver-vehicle combination

An analysis of variance (ANOVA) procedure was used to compare the means of different sets or, combinations of hours. This was done to detect any significant differences in the data between each of the years, and each of the seasons. The tests were conducted for each of the variables defined previously.

An observation for each variable is the percent by hour of the characteristic of interest, as determined from the accident data. For example, the data might indicate that for %MAUTO, 60% of those involved in an accident during the hours of 3-4 PM were males. For each variable, hourly distributions of six hours, twelve hours, and twenty four hours were compared. The six-hour period represented a direct comparison with the hours that field data was collected. The twelve-hour period selected included the six-hour period and was extended to include the daylight hours. The analysis of these distributions, obtained from a crosstabulation of hour of the day by the variable of interest (e.g., sex of driver), did not include any cells which had either zero or one hundred percent these tended to dramatically influence the overall means in a misleading way.

Ideally, the comparisons of each variable, for six, twelve, and twenty-four hours, should have been made by year and by county, but unfortunately at this level of stratification the accident data set was inadequate. Thus, the comparison made was for all accidents stratified by year (all four seasons). The means for each of the three hourly periods, for each year were compared. For example, the mean value for the six-hour period of a given year was compared to the mean value for the six-hour for each of the other years. This procedure was followed for each variable.

In a separate comparison, these data were stratified by season, and the same type of comparison made. Table 7 indicates the results of this testing. The only condition found where there were no significant differences in the means for time periods, for all of the variables, was the twelve-hour period. Based on these findings, data for all variables were aggregated over twelve hours. Thus, the comparisons to field data will be based on an aggregation of twelve hours of accident data across all seasons and all years, stratified by county and day of week. These will be compared with field data by county and day of week.

In order to utilize the ANOVA procedures, certain assumptions must be made about the distributions being compared. In order to test for any difference in the means of the various distributions, the following assumptions are made: the measurements constitute a random sample from normal populations; the samples are mutually independent; and the sample variances are equal. The null hypothesis in these tests is that the means are equal (or, operationally in this case, that there are no differences between years or seasons) for the distributions of the variables of interest. The hypothesis is rejected for large values of the F-

Table 7. Summary of ANOVA Results

	HR	VAR 1	VAR 2 *2	VAR 3	VAR 4	VAR 5	VAR 6
		Variables of interest *1					
YEAR	24	YES	YES	YES	NO	YES	NO
	12	YES	YES	YES	YES	YES	YES
	6	YES	YES	YES	NO	YES	NO
SEASON	24	NO	YES	YES	YES	YES	YES
	12	YES	YES	YES	YES	YES	YES
	6	YES	YES	YES	YES	YES	YES

Key to ANOVA Summary:

YES: No statistical difference in means detected by either YEAR or SEASON--hence, these <u>could be</u> aggregated.

NO: Difference in means detected--<u>could not be</u> aggregated. For example: for variable 6, testing for aggregation by YEAR, for the twenty-four hour period, the means for one or more years were different and thus could not be aggregated

HR - hour combination considered, either 6, 12, or 24

YEAR - breakdown by year

SEASON - Data were disaggregated into four groups of three months representing the four seasons

*1 VAR1: %MAUTO
VAR2: %MPICKUP
VAR3: %AUTO(1)
VAR4: %PICKUP(1)
VAR5: %OTHER(1)
VAR6: %AUTO(2)

* 2 VAR2 included cells with 0's and 100's (resulting from very few accidents)

statistic.

As part of the ANOVA procedure, the Scheffe test was uniformly applied to examine any differences that might be detected in the means of these distributions. This is a multiple comparison test designed to protect against calling many differences significant. If the hypothesis is rejected, no further information or test results are needed (from the ANOVA procedure).

5.1.4 Data Analysis and Problems

As stated, the original source of accident information is the MDOT 252 record although the records were shortened. Physical road section numbers (in the data field) were used to confirm that accidents selected actually occurred on I-94 within the area of interest.

The accident data selected to compare with the field data consisted of all two-vehicle accidents occurring on I-94 in the six counties between 1982 and 1987 where responsibility was clear.

One of the issues of concern is the number of accident records available for analysis. Even with the aggregation of six years of twenty-four hour data the number of valid cases is only about 2700.

The final accident record file was then compared to the field data. For example, the variable %MAUTO by hour of the day was examined for three levels of analysis: an overall comparison; a county comparison; and a daily comparison.

The principal output value is the percentage of males and females driving autos for each of twelve hours. The average of these twelve-hour percentages were compared with the average of the six hours of field data. Typical output for a crosstabulation analysis is shown in table 8.

The "overall comparison" represents a comparison of estimates of exposure made by each method based on an aggregation of all data for a given variable. For example, table 8 shows all accidents used for the sex of driver analysis. The overall percentage of males driving autos is 63.1 as estimated by the quasi-induced exposure method (bottom line in table 8). This value was compared with the overall observed field value determined by dividing the total number of males driving autos by the total number of drivers. For the county comparison, the totals are disaggregated into proportions for each county, and for the daily comparison, the county numbers are further disaggregated into proportions for each of five days.

Field data for the sex of driver comparison is the aggregation of all the data for males driving autos--that is, the ratio is made up of all males counted over the total number of drivers counted. For the county level of comparison, the ratio is the number of males counted in that county divided by the total number of drivers counted in that county, and likewise for the daily comparison.

These three comparisons (overall, by county, and by day) were made for each of the variables of interest:

Table 8. Quasi-induced Estimates for %MAUTO (typical)

HR (*1)	Count / ROW PCT	MALE 1	FEMALE 2	(*3) ROW TOTAL
	8	(*2) 33 55.9	26 44.1	59 6.1
	9	47 61.0	30 39.0	77 8.0
	10	29 63.0	17 37.0	46 4.8
	11	34 68.0	16 32.0	50 5.2
11AM TO NOON	12	54 62.1	33 37.9	87 9.0
	13	37 57.8	27 42.2	64 6.6
	14	46 68.7	21 31.3	67 6.9
	15	56 68.3	26 31.7	82 8.5
	16	67 66.3	34 33.7	101 10.4
	17	72 60.0	48 40.0	120 12.4
5PM TO 6PM	18	82 65.1	44 34.9	126 13.0
	19	53 60.2	35 39.8	88 9.1
Column	Total	610 63.1	357 36.9	967 100.0

63.1 is the estimated exposure value for %MAUTO overall comparison

Notes:
- *1 HR: Indicates hour of the twelve hours aggregated
- *2 Cell entries: frequencies
  row percentage
- *3 Cell entries: row total
  row percentage

%MAUTO - proportion of males driving autos

%MPICKUP - proportion of males driving pickups

%AUTO(1) - proportion autos of (autos+pickups+others)

%PICKUP(1) - proportion pickups of (autos+pickups+others)

%OTHER(1) - proportion others of (autos+pickups+others)

%AUTO(2) - proportion autos of (autos+pickups)

In each instance the null hypothesis to be tested was: there is no difference between the field data and quasi-induced exposure proportions. Statistical testing for the overall, county, and daily comparisons were made for each variable using the chi-square test at a .05 level of significance. A large value for the chi-square statistic would indicate that this hypothesis should be rejected--there was evidence that the proportions are not the same.

In order to utilize the chi-square test, certain assumptions are required. The samples being compared are assumed to be random samples from multinomial distributions.

An approximation method was also investigated which allowed for the direct comparison of the percentages for the binomially distributed variables, such as %MAUTO, %MPICKUP and %AUTO(2). Since this method yielded results consistent with the chi-square test, and it could not be applied in the case of the fleet mix variable which involved three ratios, the binomial approximation method was not used in favor of the chi-square test. This method also required a large sample size ($n > 30$), which frequently was not available for the stratified accident data used by the quasi-induced exposure technique.

5.2 Experiment Results

In this section the results of the comparison of field estimates with the quasi-induced exposure estimates are made.

Throughout the comparison process a major problem surfaced: the lack of sufficient accident data. As the data were disaggregated for the county and daily comparisons, cells often had less than five cases and sometimes none. For the chi-square test to be valid, in general, a minimum of five cases are required. Frequently, it was not possible to perform the daily comparison for a number of variables.

It should also be noted that the chi-square test is very sensitive to the size of the samples (frequencies) involved. This can lead to somewhat misleading results when comparing proportions. Throughout the testing of proportions, large differences are accepted statistically (as being the same), and smaller ones are rejected. In short, the chi-square tests are not the only criterion for interpreting the comparison of accident and field data. Unfortunately, there is no statistical test available which is applicable, and does not have some limitations. Therefore, some parts of the discussion necessarily become qualitative in nature.

For the 2 X 2 matrix (e.g., sex of driver), at the .05 level of significance, the chi-square table value for comparison was 3.84. For the 3 X 2 matrix (fleet mix), at the .05 level of significance, the chi-square table value is 5.99.

5.2.1 Comparisons for Variable 1 - %MAUTO

Tables 9-11 show the comparison between field and quasi-induced estimates of %MAUTO (percent of auto drivers who are male) for the overall case (table 9), for the county level (table 10), and the daily level (table 11). In table 9, the null hypothesis has not been rejected for the overall comparison: there <u>is no</u> significant difference between the measures of exposure estimated by direct observation in the field and estimated by the quasi-induced exposure technique for the sex of driver variable. Indeed, the values of %MAUTO for the field data (65.5) compare very favorably to the quasi-induced estimate (63.1).

In table 10 the results of the county comparison chi-square test indicating that there <u>is</u> a difference between the proportions of interest in Jackson and Washtenaw Counties; but there <u>is not</u> a difference for the other countries. Figure 11 shows these results graphically. Operationally, the two estimates are quite close for some counties (1.3 for Van Buren) but for others (11.3 for Jackson).

In table 11 the results of the daily comparison are

Table 9. Overall Comparison for %MAUTO

	Field Estimates		Quasi-induced Estimates			
	n	%	n	%	X^2	Acc/Rej
Male	122,069	65.5	610	63.1	2.56	A
Female	64,192	34.5	357	36.9		
Total	186,261	100.0	967	100.0		

Key:

For field estimates

n - number of sex indicated observed in field

% - percentage

For quasi-induced estimates

n - number of accidents of sex indicated

% - percentage

Statistic

X^2 - computed chi-square value

Acc/Rej - accept or reject the null hypothesis

A - fail to reject (proportions are the same)

R - reject

Table 10. County Comparison for %MAUTO

	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/Rej
Berrien						
M	18,933	66.2	103	68.7	0.41	A
F	9,678	33.8	47	31.3		
Calhoun						
M	22,822	65.6	64	72.7	1.97	A
F	11,964	34.4	24	27.3		
Jackson						
M	20,559	66.9	70	55.6	7.27	R
F	10,176	33.1	56	44.4		
Kalamazoo						
M	18,313	64.8	149	68.7	1.43	A
F	9,961	35.2	68	31.3		
Van Buren						
M	14,640	67.3	35	66.0	0.04	A
F	7,129	32.7	18	34.0		
Washtenaw						
M	26,804	63.7	189	56.8	6.85	R
F	15,284	36.3	144	43.2		

* See explanatory notes in Table 9.

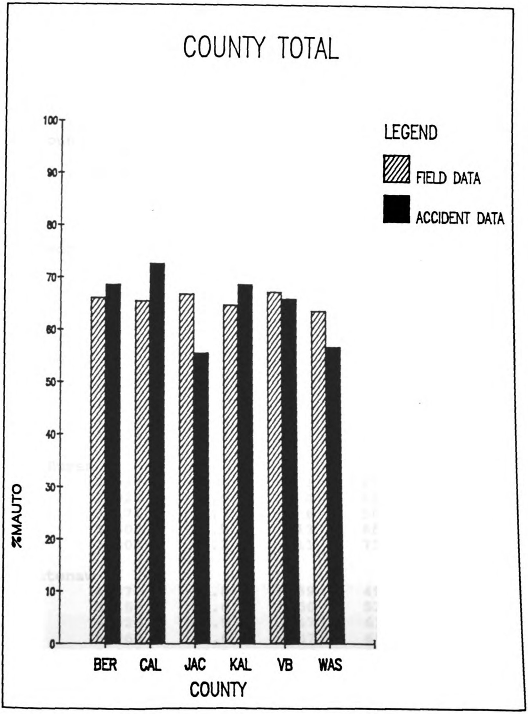

Figure 11. County Comparison for %MAUTO

Table 11. Daily Comparison for %MAUTO

	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/Rej
Berrien						
M	3,389	63.6	25	65.8	0.10	A
W	3.367	64.3	10	52.6	1.10	A
F	4,062	66.0	29	76.3	0.75	A
S	3,864	68.1	20	66.7	2.80	A
S	4,521	69.1	19	82.6	2.10	A
Calhoun						
M	3,767	61.8	14	66.7	0.20	A
W	4,324	66.0	12	66.7	3.20	A
F	5,569	63.8	15	68.2	0.20	A
S	5,208	66.7	9	90.0	2.40	A
S	3,954	70.5	14	82.4	1.20	A
Jackson						
M	3,607	65.3	14	58.3	0.50	A
W	3,266	64.3	17	53.1	2.00	A
F	4,266	66.0	19	51.4	3.70	A
S	4,389	68.1	5	41.7	3.30	A
S	5,029	69.1	15	71.4	0.00	A
Kalamazoo						
M	2,820	61.6	34	73.9	2.91	A
W	3,115	62.5	34	59.6	.19	A
F	3,839	61.6	35	68.6	1.06	A
S	4,035	67.6	19	76.0	0.81	A
S	4,504	69.3	27	71.1	0.06	A
Van Buren						
M	2,537	67.0	1	20.0	4.90	R
W	2,023	64.6	4	66.7	0.01	A
F	3,171	66.7	6	50.0	1.51	A
S	3,606	70.5	13	86.7	1.80	A
S	3,303	66.2	11	73.3	0.34	A
Washtenaw						
M	4,872	62.8	39	49.4	6.00	R
W	5,458	66.6	50	52.1	8.99	R
F	5,926	64.9	47	61.8	0.32	A
S	5,263	60.6	37	68.5	1.42	A
S	5,285	63.5	16	57.1	0.49	A

* See explanatory notes in Table 9.

presented. Caution must be used here in the application of the chi-square test, as the sample size is sometimes less than five, although the results are shown here for the sake of consistency. The table indicates that except for Monday in Van Buren, and Monday-Wednesday in Washtenaw counties, the null hypothesis is not rejected (i.e., while there is no significant difference in the exposure estimates). While considerable day-to-day variation is noted for the field data (up to 9.0 for Calhoun County), even greater variation is noted in the quasi-induced estimates which are based on very small numbers of accidents.

Overall, the results of the statistical tests, for the sex of driver variable, show general agreement at each level of comparison of the two exposure methods. This implies that, at least for this variable, the first hypothesis is supported: the non-responsible driver-vehicle combination in a multi-vehicle accident is a random sample of the traffic stream. Operationally, however, it is clear that while there is good overall agreement between the quasi-induced and field estimates, the agreement degrades as the data are disaggregated to the county and daily levels. The degradation would appear to be due to the decreasing sample size that form the basis for the quasi-induced estimates.

Examination of the table percentages shows a problem with the use of the chi-square test when there is great variation in the sample sizes. In some cases, large differences in the proportions are not rejected as being different, yet in other cases much smaller differences are.

5.2.2 Comparison for Variable 2 - %MPICKUP

In table 12 the overall comparison for the proportion of males driving pickup trucks (%MPICKUP) is shown. The results indicate there is no significant difference in the proportions observed in the field (84.4% male drivers) and those estimated by the quasi-induced exposure technique (87.7% male driver).

Table 12. Overall Comparisons for %MPICKUP

	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/Rej
M	42,507	84.4	100	87.7	0.93	A
F	7,833	15.6	14	12.3		

* See explanatory notes on table 9.

Table 13 shows the results of attempting a county-level comparison of the proportion of males driving pickup trucks. Again, in many cases, the appropriateness of the chi-square test due to small cell sizes. Even qualitatively considerable variation is noted. No tests were performed for the daily level of comparison since there was clearly insufficient accident data to have any meaning.

This variable did not produce the hoped for results--a severe problem with the number of accidents prevented any meaningful comparisons to be made below the county level.

Table 13. County Comparison - %MPICKUP

	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/Rej
Berrien						
M	6,761	83.7	14	93.3	1.01	A
F	1,314	16.3	1	6.7		
Calhoun						
M	7,446	83.3	10	50.0	15.90	R
F	1,488	16.3	10	50.0		
Jackson						
M	6,609	82.7	17	89.5	0.61	A
F	1,381	17.3	2	10.5		
Kalamazoo						
M	6,054	86.2	22	88.0	0.07	A
F	972	13.8	3	12.0		
Van Buren						
M	4,881	83.0	4	80.0	3.30	A
F	997	17.0	1	20.0		
Washtenaw						
M	10,756	86.5	33	82.5	0.54	A
F	1,681	13.5	7	17.5		

* See explanatory notes in Table 9.

However, the overall comparison (84.7 v.s. 87.7) did suggest that the first hypothesis is supported (D2s are a random sample of the traffic stream).

5.2.3 Comparisons of Variables 3, 4, 5 for %AUTO(1), %PICKUP, %OTHER(1)

The results of the overall comparison for the fleet mix variables are presented in table 14. Here, fleet mix is defined in terms of autos, pickups, and others. The results indicate significant differences in the proportions of each type of vehicle as estimated by field observation and the quasi-induced exposure technique. While the "auto" category estimates are arguably relatively close, the estimates for the "pickup" and "other" categories are quite dis-similar.

Table 15 shows the results of the county-level comparison of fleet mix. The results indicate that the proportions of interest are different for all of the counties.

Table 14. Overall Comparisons - %AUTO(1),%PICKUP(1), %OTHER(1)

	Field Estimate		Quasi-induced Estimate			
	n	%	#	%	X^2	Acc/Rej
A	219,549	64.2	963	69.4		
PU	62,395	18.2	114	8.2	99.0	R
O	60,092	17.6	309	22.3		

* See explanatory notes in Table 9.

Table 15. County Comparisons - %AUTO(1), %PICKUP(1), %OTHER(1)

	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/Rej
Berrien						
A	32,336	64.6	149	68.3		
PU	9.393	18.8	15	6.9	25.5	R
O	8,327	16.6	54	24.8		
Calhoun						
A	41,231	65.2	87	65.9		
PU	11,498	18.2	10	7.6	15.9	R
O	10,505	16.6	35	26.5		
Jackson						
A	33,125	65.6	127	61.7		
PU	8,624	17.1	19	9.2	24.2	R
O	8,773	17.3	60	29.1		
Kalamazoo						
A	33,142	62.6	215	69.8		
PU	10,071	19.0	25	8.1	81.8	R
O	9,767	18.4	68	22.1		
Van Buren						
A	24,900	61.4	53	70.7		
PU	7,044	17.4	5	6.6	6.1	R
O	8,594	21.2	17	22.7		
Washtenaw						
A	54,815	64.7	332	74.3		
PU	15,765	18.6	40	8.9	28.6	R
O	14,126	16.7	75	16.8		

*See explanatory notes in Table 9.

In table 16, the results of the daily comparisons of fleet mix are presented. This table includes the results for each of the two types of fleet variables defined. It can be seen that the results are inconsistent. Figures 12, 13, and 14 show graphically the county comparison of the exposure estimates for the %AUTO(1), %PICKUP(1), %OTHER(1) variables.

In should be noted that at the county level the vehicle-type percentages are reasonably consistent on a county-by-county basis. However, at the daily level, there appears to be considerable variation on a day-to-day basis for any given county.

Interestingly enough, while the quasi-induced estimates are fairly volatile there are still some discernible trends, especially when compared to the field data. In general, the quasi-induced approach consistently underestimates the pickup category relative to the field estimate. In many instances the auto estimate is also higher for quasi-induced.

Unfortunately, there is no convenient explanation for these problems. However, it should be noted that the field observations of vehicle type occurred in 1988, a single point in time when pickup sales are at an all-time high. The accident data, on the other hand, are representative of several earlier years. If the percentage of pickup trucks has been increasing steadily then the estimate using the quasi-induced approach would be much lower than the data for

Table 16. Daily Comparison - %AUTO(1), %PICKUP(1), %OTHER(1) (continued)

		Field Estimate		Quasi-induced Estimate			
		n	%	n	%	X^2	Acc/Rej
Berrien Day							
M	A	6188	61.3	37	71.2		
	PU	1816	18.0	3	5.8	5.3	A
	O	2084	20.7	12	23.1		
W	A	6721	63.1	19	55.9		
	PU	1875	17.6	2	5.9	9.2	R
	O	2054	19.3	13	38.2		
F	A	6896	62.7	40	64.5		
	PU	2094	19.0	6	9.7	4.8	A
	O	2008	18.3	16	25.8		
S	A	7267	70.5	30	73.2		
	PU	1802	17.5	3	7.3	4.3	A
	O	1242	12.0	8	19.5		
S	A	5264	65.7	23	79.4		
	PU	1806	22.5	1	3.4	6.3	R
	O	939	11.8	5	17.2		
Calhoun Day							
M	A	6371	59.4	21	70.0		
	PU	1672	15.6	2	6.7	2.1	A
	O	2674	25.0	7	23.3		
W	A	8628	62.8	18	62.1		
	PU	2248	16.3	3	10.3	1.3	A
	O	2872	20.9	8	27.6		
F	A	8586	60.1	22	61.1		
	PU	2944	20.6	1	2.8	10.9	R
	O	2746	19.3	13	36.1		
S	A	8783	70.6	10	58.8		
	PU	2437	19.6	2	11.8	7.4	R
	O	1228	9.8	5	29.4		
S	A	8863	73.6	16	80.0		
	PU	2197	18.2	2	10.0	0.9	A
	O	985	8.2	2	10.0		

* See explanatory notes in Table 9.

Table 16. Continued

		Field Estimate		Quasi-induced Estimate			
		n	%	n	%	X^2	Acc/Rej
Jackson							
Day							
M	A	5848	62.6	24	47.1		
	PU	1404	15.0	8	15.7	7.0	R
	O	2083	22.4	19	37.2		
W	A	5261	59.5	32	68.1		
	PU	1416	16.0	2	4.3	4.8	A
	O	2172	24.5	13	27.6		
F	A	5832	59.4	37	59.6		
	PU	1704	17.3	5	8.1	5.2	A
	O	2284	23.3	20	32.3		
S	A	7284	72.5	12	63.2		
	PU	1636	16.3	2	10.5	4.6	A
	O	1124	11.2	5	26.3		
S	A	8900	72.3	22	81.5		
	PU	2304	18.7	2	7.4	2.3	A
	O	1110	9.0	3	11.1		
Kalamazoo							
Day							
M	A	5913	58.4	46	67.6		
	PU	1981	19.6	6	8.8	5.1	A
	O	2231	22.0	16	23.5		
W	A	5821	56.7	57	72.1		
	PU	1829	17.8	6	7.6	8.8	R
	O	2623	25.5	16	20.3		
F	A	6888	58.6	51	66.2		
	PU	2099	17.9	6	7.8	5.3	A
	O	2769	23.5	20	26.0		
S	A	7260	67.9	25	62.5		
	PU	2252	21.1	2	5.0	21.5	R
	O	1184	11.0	13	32.5		
S	A	7260	71.7	36	81.8		
	PU	1910	18.8	5	11.4	2.3	A
	O	960	6.8	3	6.8		

* See explanatory notes in Table 9.

Table 16. Continued

		Field Estimate n	%	Quasi-induced Estimate n	%	X^2	Acc/Rej
Van Buren Day							
M	A	4570	68.2	5	62.5		
	PU	1412	21.1	1	12.5	1.8	A
	O	720	10.7	2	25.0		
W	A	3578	50.9	6	60.0		
	PU	1268	18.0	-	0	2.2	A
	O	2188	31.1	4	40.0		
F	A	5176	60.0	12	63.2		
	PU	1200	13.9	1	5.2	1.3	A
	O	2256	26.1	6	31.6		
S	A	5956	61.0	15	71.4		
	PU	1528	15.6	2	9.6	5.3	R
	O	2280	23.4	4	19.0		
S	A	5620	72.5	15	88.2		
	PU	1412	18.2	1	5.9	2.2	A
	O	720	9.3	1	5.9		
Washtenaw Day							
M	A	9450	58.9	79	68.7		
	PU	3016	18.8	16	13.9	4.6	A
	O	3581	22.3	20	17.4		
W	A	10356	59.5	94	76.4		
	PU	3076	17.7	9	7.3	15.6	R
	O	3964	22.8	20	16.3		
F	A	12649	63.1	77	67.5		
	PU	3711	18.5	9	7.9	9.4	R
	O	3733	18.5	28	24.6		
S	A	11279	70.2	54	87.0		
	PU	3076	19.2	4	6.5	8.7	R
	O	1698	10.6	4	6.5		
S	A	11081	73.3	28	84.8		
	PU	2886	19.1	2	6.1	3.6	A
	O	1150	7.6	3	9.1		

* See explanatory notes in Table 9.

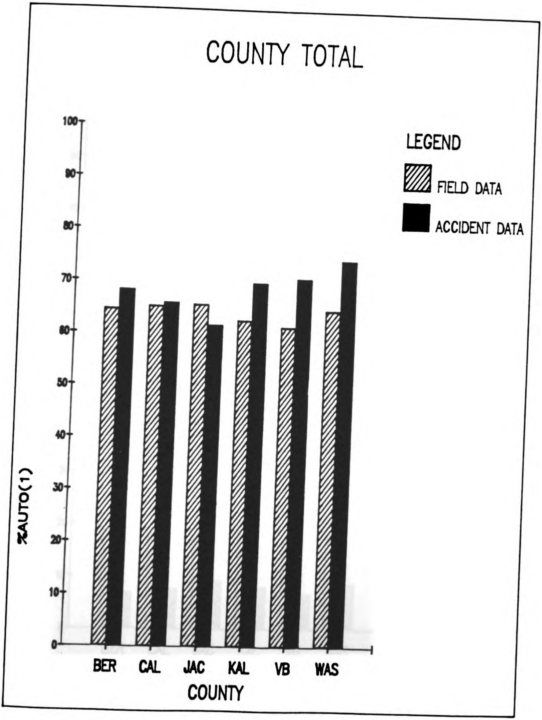

Figure 12. County Comparison for AUTO(1)

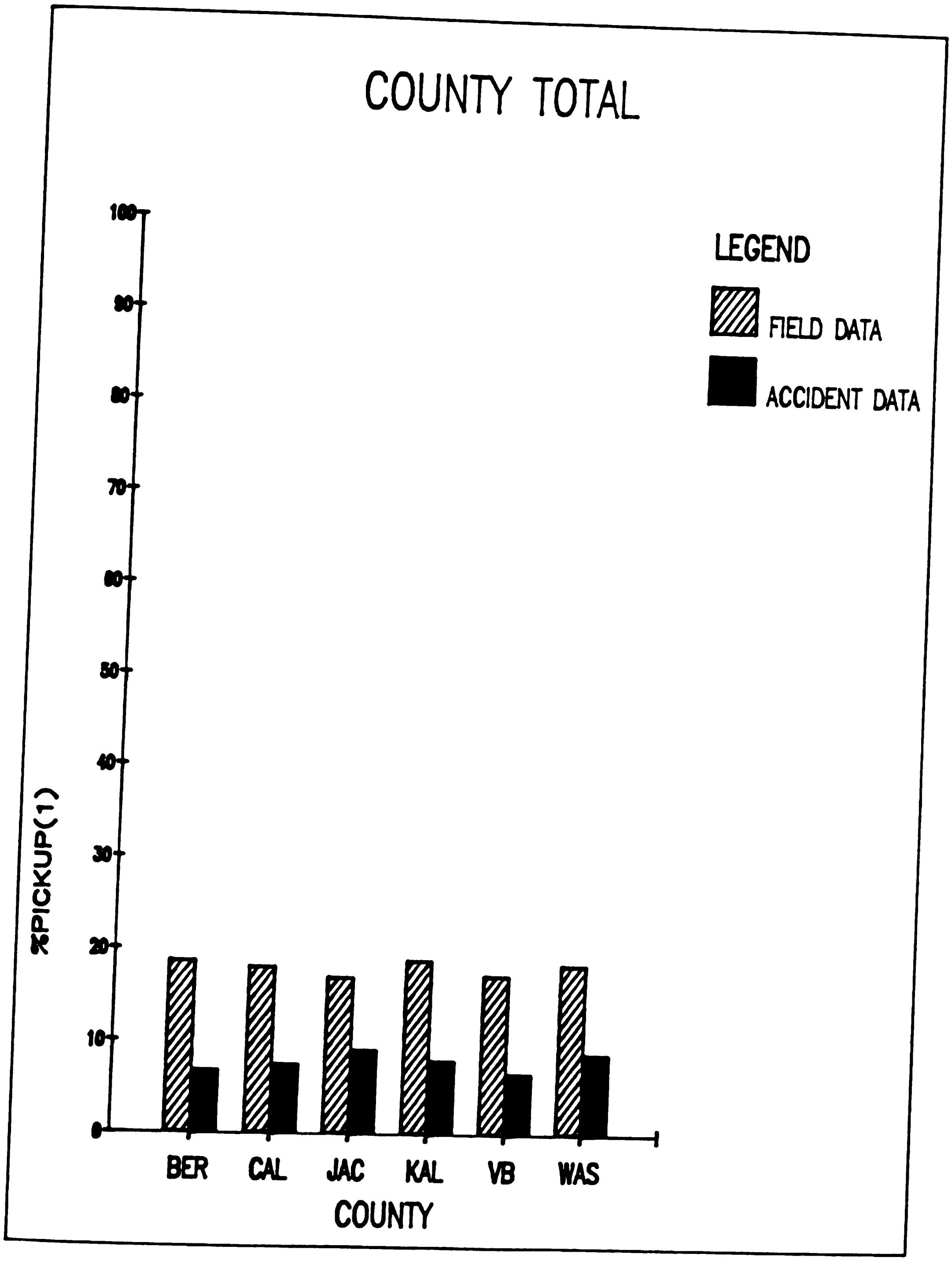

Figure 13. County Comparison for %PICKUP(1)

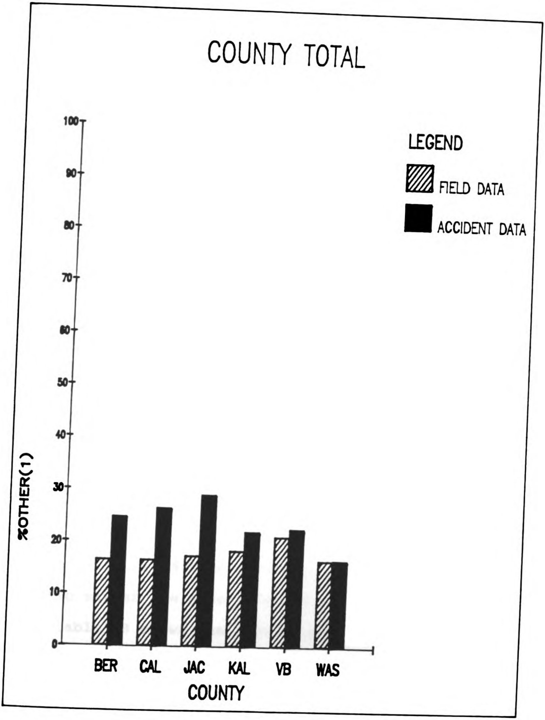

Figure 14. County Comparison for %OTHER(1)

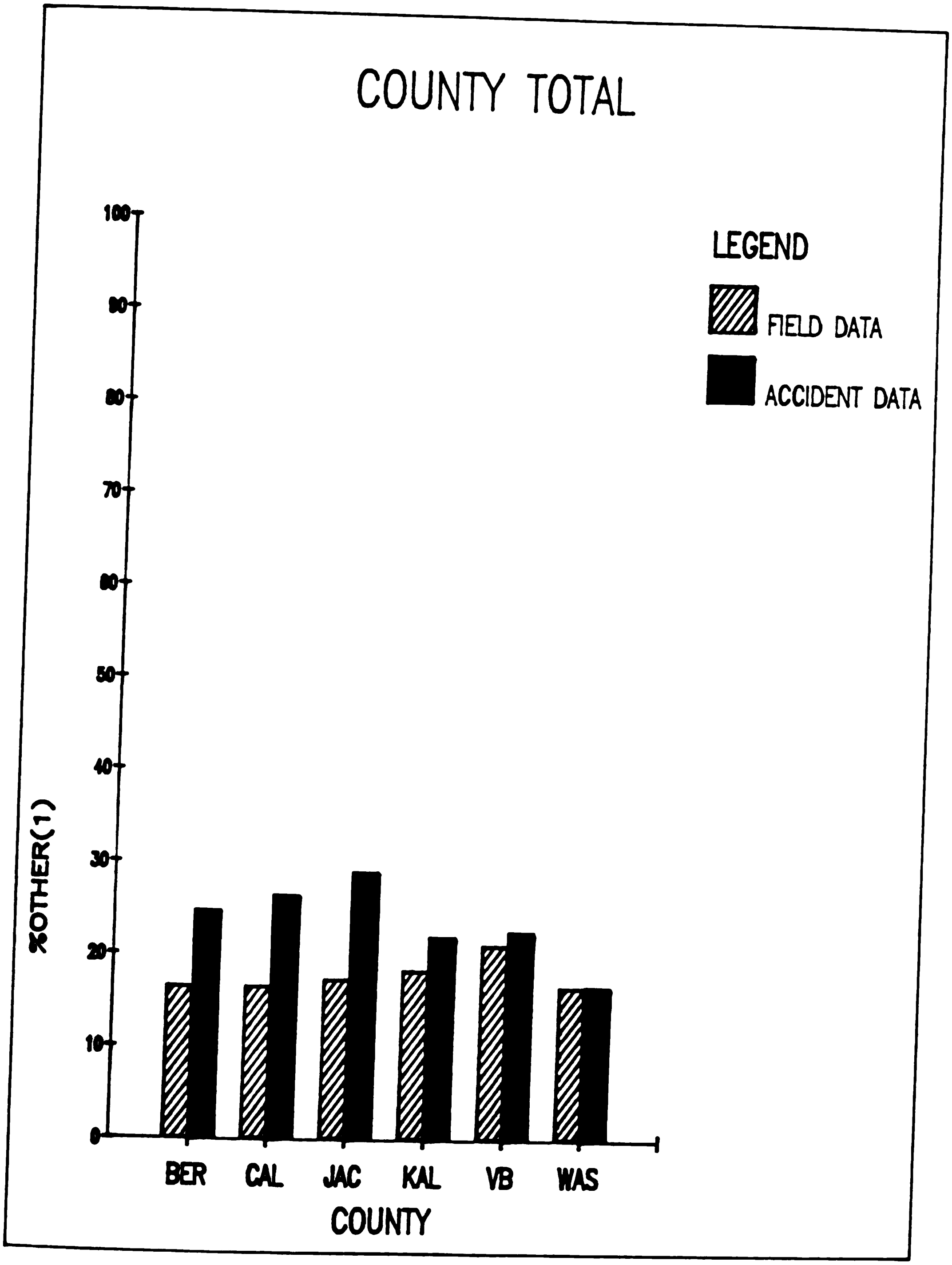

Figure 14. County Comparison for %OTHER(1)

a more recent year. Indeed, at all levels of stratification shown, the pickup percentage is lower than would be expected given the field data.

5.2.4 Comparison for Variable 6 - %AUTO(2)

An alternative definition of fleet mix variables were also considered--based only on consisting of only autos and pickups, due the interest in pickup trucks. This included the examination of the number of female pickup truck drivers. This choice was also made as a result of a perceived problem with some of the large truck data. Since large trucks make up a large proportion of the group defined as "others" it was decided to consider a fleet definition which eliminated them. This, in effect, isolates pickup trucks.

The results of the overall comparison for autos and pickups are presented in table 17. Examination of the proportions shows them to be 12 to 13 percent different, a statistically and operationally significant difference. However, the number of pickup accidents may not be large enough to provide an accurate estimate. The comments from section 5.2.3 regarding a trend toward more pickups in the traffic stream are also relevant here.

Table 18 shows the results of the county comparison for autos and pickups. The results indicate that the proportions of each vehicle type, as estimated in the field versus those estimated by the quasi-induced exposure

Table 17. Overall Comparison for %AUTO(2)

	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/rej
A	219,549	77.9	963	89.4	83.1	R
PU	62,395	22.1	114	10.6		

Table 18. County Comparison for %AUTO(2)

	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/rej
Berrien						
A	32,336	77.5	149	90.9		
PU	9.393	22.5	15	9.1	16.8	R
Calhoun						
A	41,231	78.2	87	89.7		
PU	11,498	21.8	10	10.3	7.5	R
Jackson						
A	33,125	79.3	127	87.0		
PU	8,624	20.7	19	13.0	5.2	R
Kalamazoo						
A	33,142	76.7	215	89.6		
PU	10,071	23.3	25	10.4	22.2	R
Van Buren						
A	24,900	77.9	53	91.4		
PU	7,044	22.1	5	9.6	6.1	R
Washtenaw						
A	54,815	77.7	332	89.2		
PU	15,765	22.3	40	10.8	28.7	R

technique are significantly different in all six counties.

Figure 15 show graphically the results of the county level of comparison for the percentages of autos and pickups.

No daily comparison was made for autos and pickups, as the sample sizes are small, and, in some cases, there are no data.

5.3 Related Comparisons

In spite of the lack of agreement between the two estimates of exposure, each set of estimates seems to be reasonably stable--i.e., the county-to-county variation in field observations is relatively small. The pattern of variation suggests that the overestimation by the quasi-induced method, relative to the field estimates, is very consistent. This suggests there may be an explanation such as the evolving vehicle mix. Data from other researchers for provide additional opportunities to examine the performance of the quasi-induced exposure technique. The general approach here is to look for any additional evidence that would help validate (or invalidate) the quasi-induced exposure concept.

5.3.1 Comparison of Data Trends

In the next several tables, two types of exposure estimates are presented. The data in columns labeled 1973 and 1974 are from studies conducted in Michigan by Carroll

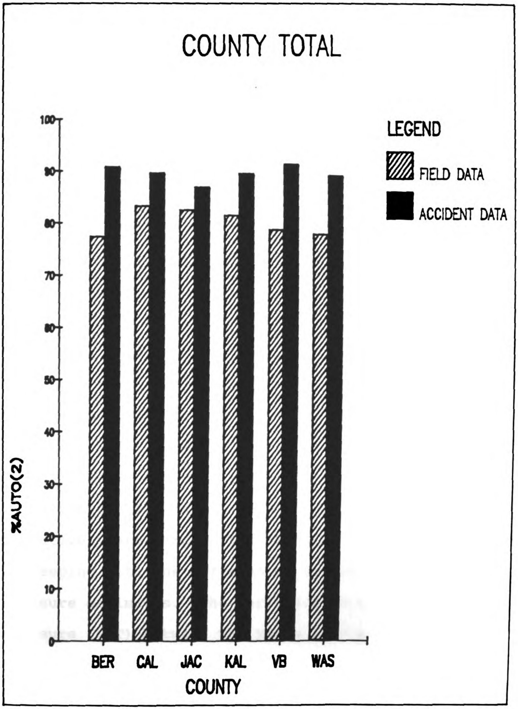

Figure 15. County Comparison for %AUTO(2)

(1975) where he tested the trip-log method of gathering data for estimating VMT. These estimates of VMT are for the entire road system. His results are shown for comparison with more recent data.

The data shown in columns labeled 1985, 1986, 1987 are estimates of exposure made using the quasi-induced exposure technique. The accident data are from the years specified, and are for the entire roadway system.

In table 19 exposure estimates for the sex of driver are shown. The Carroll estimates are for VMT, by sex of driver, for VMT. A fairly large year-to-year variation is indicated--more than would be intuitively expected for the entire system. The trend, however, is for decreasing VMT for males--although extrapolation would show VMT by males decreasing to zero within 15 years (by 1989). By comparison, the quasi-induced estimates are far more stable on an annual basis with a much more reasonable trend over time (relative male exposure). This is supported by the data shown in table 20 which shows the statewide driver registration, for males, in Michigan is slowly decreasing (relatively speaking). The magnitude of change is much more in keeping with the differences in the quasi-induced exposure estimates. The variation in Carroll's estimates of exposure (VMT) between the years 1973 and 1974 may imply that trip-log VMT is not a reliable method of estimating exposure as it is unlikely that the proportion of males on the road varies so radically in a single year.

Table 19. Exposure-Sex of Driver

	Carroll-VMT		Quasi-induced		
	1973	1974	1985	1986	1987
	(%)	(%)	(%)	(%)	(%)
Male	65.6	61.8	58.5	57.3	56.2
Female	34.4	38.2	41.5	42.7	43.8

Notes:

Exposure for years 1973 and 1974 based on trip-log method of VMT

Exposure for years 1985, 1986, and 1987 based on quasi-induced exposure method

Source: VMT data - Carroll (1975)

Table 20. Percentage Licensed Drivers -- Males

	Year					
	1982	1983	1984	1985	1986	1987
Michigan	52.2	52.2	51.0	50.8	50.6	50.4

Source: Highway Accident Statistics - US DOT - (1982-1987)

In table 21, a comparison between the same two types of exposure estimates is made for types of vehicles. Again, the quasi-induced estimates show variation which appear intuitively more reasonable when compared with the trip-log VMT estimates. Further, the quasi-induced exposure estimates agree with the existing trend toward increasing numbers of pickup trucks, and heavy trucks (others), in the fleet.

It is also interesting to note that the system-wide quasi-induced estimate for pickups (13.6%) is higher than that reported earlier (table 14--8.2%) although the latter is only for I-94. The 1987 figure and the 1985-1987 trend supports the earlier argument that the average figures used in table 14 may be downwardly "bias" the pickup percentage --i.e., the I-94 field observations of 18.2% and the systemwide 13.6% are considerably closer than the quasi-induced estimate for I-94.

The trip-log data also shows significant variation between the two years of data for fleet mix. This may be have occurred due to actual changes in the fleet; random chance in terms of the selection of drivers; the relative frequency of mileage being recorded in pickups or "other" vehicles (proportions may be sensitive); or the trip-log method may not be a reliable method to gather fleet mix data. The level of error using the trip-log method to estimate VMT, may be great enough to mask the real changes in these characteristics.

Table 21. Exposure-Vehicle Type

	1973 (%)	1974 (%)	1985 (%)	1986 (%)	1987 (%)
<u>Vehicle Type</u>					
Passenger Car	88.2	80.6	85.8	84.1	81.9
Pickup	10.9	17.8	10.7	12.0	13.6
Other	0.8	1.7	3.5	4.0	4.4

Notes:

Exposure for years 1973 and 1974 based on VMT

Exposure for years 1985, 1986, and 1987 based on quasi-induced exposure method

Source: VMT data Carroll (1975)

5.3.2 Comparisons with Other Direct Observation Data

In a series of direct observation studies by Wagenaar et al. (1985, 1986, 1987) on seat belt use in Michigan, other driver-vehicle information was also recorded. A random sample of driver-vehicles were observed at a carefully selected probability sample of 240 locations throughout the state of Michigan. These locations were predominantly signalized locations, in order to provide observers time to record data. (See Wagenaar, et al. 1984, for details on study methodology.)

The Wagenaar study provides information on driver-vehicle characteristics for some of the same variables considered earlier. Wagenaar recorded information of the sex of the driver, vehicle type, and restraint use by the driver and other vehicle occupants.

The source of accident data for the quasi-induced estimates was the Michigan 252 accident record. Data were analyzed by years for 1985, 1986, and 1987, for signalized intersections, for the entire state of Michigan. Only two-vehicle accidents, where the fault was clear, occurring during the hours of 7:00 AM to 7:00 PM, were selected for use. The time period was selected to match the hours of field observation. This approach provides a very large accident data base for using in the quasi-induced technique.

Table 22 shows the comparison between Wagenaar data and the quasi-induced estimates for sex of driver. Included in the table is a value for the chi-square test statistic as presented earlier. The high chi-square values force rejection of the null hypotheses which are of the form: there is no difference between the proportion estimated by direct observation and the proportion estimated by the quasi-induced exposure method for any variable.

In spite of the chi-square values, a number of favorable observations can be made regarding this comparison: the proportions estimated by field observations and the quasi-induced exposure method are within four percent of each other for all years; both indicate a slight trend towards a decrease in the exposure of males on Michigan roads; and the differences between the estimates are almost constant.

Table 22. Additional Comparison of Sex of Driver Estimates

1985	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/Rej
M	22,801	62.2	17,580	58.5	96.1	R
F	13,851	38.8	12,481	41.5		

1986	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/Rej
M	22,566	61.5	18,889	57.3	127.8	R
F	14,114	38.5	14,070	42.7		

1987	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/Rej
M	24,815	61.1	19,806	57.1	179.1	R
F	15,822	38.9	14,861	42.9		

* See explanatory notes in Table 9.

Source: Wagenaar (1985-1987)

The quasi-induced exposure estimates are, however, consistently higher than Wagenaar's estimates. This may well be due to the fact that the intersections included in the samples are different--Wagenaar's intersections are a subset of the ones used for the quasi-induced estimates.

Table 23 presents a comparison of fleet mix variables collected by direct observation during the Wagenaar study. Again, the chi-square values are high, but inspection of the table shows an overall agreement between the proportions with no more than 5 percent difference for any type of vehicle. Again, the differences between the estimates are consistent year-to-year (consistent magnitude and direction).

In table 24, the results of a comparison between values for the proportion of drivers wearing seat-belt restraints as estimated by the two exposure methods are shown. The poor agreement between the two estimates was somewhat unexpected, as the other variables far performed better. However, upon reflection the problem probably lies in how restraint use data is coded and processed by the Michigan State Police. That is, most of the seat belt use reported in the accident data is self-reported (the investigating officer had no way to check seat belt use), and would be expected to be high. Thus, in the end, the differences are in the direction expected.

Table 23. Additional Comparisons of Fleet Mix Estimates

	Field Estimate		Quasi-induced Estimate			
	n	%	n	%	X^2	Acc/Rej
1985						
A	19,860	81.7	25,801	85.8		
PU	3,760	15.5	3,206	10.7	286	R
O	697	2.8	1,054	3.5		
1986						
A	29,597	80.8	27,708	84.1		
PU	5,988	16.3	3,940	12.0	322	R
O	1,046	2.9	1,311	4.0		
1987						
A	30,665	79.0	28,450	83.8		
PU	6,909	17.8	4,209	12.4	415	R
O	1,246	3.2	1,288	3.8		

See explanatory notes in Table 9.

Table 24. Comparison of Drivers Not Using Restraints

<u>YEAR</u>	Direct Observation	Quasi-Induced
1985	56.2	69.4
1986	53.7	68.0
1987	51.9	68.7

Source: Wagenaar (1985-1987)

5.4 Summary

In general, the results of the field study produced moderately positive results. For the sex of driver variables, there was overwhelming good agreement at each of the three levels of comparison (overall, county-by-county, and day-by-day). This result indicates support for the first hypothesis, which asserts that vehicle-driver 2 (D2) is a random sample from the traffic stream. Unfortunately, the results for the fleet mix variables were not as positive. Frequently, the proportions of autos estimated by the two methods were in reasonable agreement although the quasi-induced was generally higher. The estimates for "pickups" and "others" were not very encouraging. This may be due in part to the small numbers of pickups involved and to problems resulting from fleet mix changes over the years being considered.

General comparisons between quasi-induced estimates of exposure and estimates of VMT based on trip-log information also indicated good agreement. These comparisons were made by examining the trends over a number of years (direct yearly comparisons were not possible). The quasi-induced exposure estimates seemed more stable than trip-log based estimates which showed a large year-to-year variation. For example, quasi-induced exposure results reflected increases in the number of females driving, and the increase in the number of pickup trucks on the road.

The results of comparing quasi-induced estimates with other studies yielded some positive results for several of the variables of interest. Comparison of quasi-induced exposure estimates at a statewide level to other direct observation studies also yielded good results. While these results were not statistically significant, the operational seemed within reason and may be explainable.

While none all results from the field study and of the comparison with other studies were satisfying, there were several very promising finding--the overall comparison of the estimates from the field data and the quasi-induced method for some variables and the statewide comparison from other studies. In general, the comparisons were better at more aggregated levels. As soon as the accident data became sparse, the quasi-induced approach began to break down. Overall then, the fundamental problem seems to be one of sample size--for the I-94 study there were simply not enough accident data.

6.0 ACCIDENT DATA COMPLEMENTARY SETS ANALYSIS

In this section, another empirical approach to the validation of the assumption that the non-responsible driver-vehicle combination is a random sample of the motorists and vehicles on the road is discussed. Here, an internal (to the accident data) check is used to try to establish validity. The methodology presented here is designed to test the second hypothesis:

Hypothesis 2: The distribution of non-responsible drivers involved in accidents caused by a specified driver-vehicle category is the same as that of non-responsible drivers involved in accidents caused by the complement set of driver-vehicle combinations.

That is, the distribution of non-responsible driver-vehicle combinations involved in accidents caused by driver-vehicle combinations with certain characteristics are compared with the distribution of non-responsible driver-vehicle combinations of accidents caused by the complement set of driver-vehicle combinations, they should be same. This has to be true if the D2s represent a random sample from the traffic stream.

For example, if sex of driver is the relevant characteristic, it has been argued here that the proportion of each sex of D2 is a measure of exposure, male D1s should have accidents with the same proportions of male and female D2s as female D1s--that is, in a simple 2x2 matrix describing the sex of the drivers in two-vehicle accidents, the row proportions should be identical. This is illustrated symbolically in table 25 where the proportion of male D1s who hit male D2s, p(m1-m2), should be equal to the proportion of female D1s who hit male D2s, p(f1-m2). Further, p(m1-f2) should equal to p(f1-f2). For a system where accident causation is dependent on the population of drivers on the road, the marginal row distribution (i.e., p(d2-m), p(d2-f)) would also equal the individual row distributions--that is, for example, p(m1-m2) would equal p(f1-m2) and p(d2-m).

6.1 Examination of the Sex of Driver Variable 1 - %MAUTO

Real data, arranged in the same format as table 25, are shown in table 26. Table 26 shows the male-female matrix for all two-vehicle accidents on state-numbered routes in Michigan during 1986. Table 27 shows a similar matrix for all two-vehicle accidents on the Michigan interstate system. According to the argument above, the proportional distributions across the rows would be expected to be the same in both cases. Subjectively, this appears to be the case in table 26 allowing for a reasonable margin of error (the proportions agree within about two percent).

As an aside, there is also reasonable agreement between the all-interstates observations here and the field data

Table 25. Schematic Distributions of D1s and D2s by Sex

		driver-2 male	driver-2 female	d2-total
driver-1	male	m1-m2 p(m1-m2)	m1-f2 p(m1-f2)	d1-m p(d1-m)
	female	f1-m2 p(f1-m2)	f1-f2 p(f1-f2)	d1-f p(d1-f)
	d1-total	d2-m p(d2-m)	d2-f p(d2-f)	N

Where:

driver-1 is "at-fault;"

driver-2 is "not-responsible;"

m1-m2 is a cell entry, the number of accidents where driver-1 is male and driver-2 is male (typical);

d2-m is a marginal total, the total number of accidents where driver-2 is male (typical);

p(m1-m2) is a cell proportion, the proportion of accidents where driver-1 is male and driver-2 is male (the percentage of total accidents in the <u>row</u>) (typical); and

p(d2-m) is a marginal proportion, the proportion of all driver-2s who are male (typical), and N is the total number of accidents being considered.

Hence, the involvement ratio for males and females can be calculated from the marginal proportions. That is:

IR(male) = p(d1-m)/p(d2-m) and

IR(female) = p(d1-f)/p(d2-f).

Table 26. Actual Distributions of D1s and D2s by Sex for Accidents on State-numbered Routes in 1986

		D2 male	D2 female	D1-total
D1	male	16731 (61.7)	10385 (38.3)	27116 (66.1)
	female	8302 (59.7)	5604 (40.3)	13906 (33.9)
	D2-total	25033 (61.0)	15989 (39.0)	41022

Hence, Involvement Ratio (IR)-male = 66.1/61.0 = 1.084

Involvement Ratio (IR)-female = 33.9/39.0 = .869

Table 27. Actual Distributions of D1s and D2s by Sex for Accidents on Interstate Routes

		D2 male	D2 female	D1-total
D1	male	4959 (68.0)	2334 (32.0)	7293 (75.6)
	female	1565 (66.5)	789 (33.5)	2354 (24.4)
	D2-total	6524 (67.6)	3123 (32.4)	9647

Hence, Involvement Ratio (IR)-male = 75.6/67.6 = 1.118

Involvement Ratio (IR)-female = 24.4/32.4 = .753

displayed in table 9. In fact, if the percentage of males is decreasing, the expected decrease between 1986 (table 26) and 1988 (table 9) would bring the two estimates even closer.

While male drivers classified as D1 outnumber female drivers classified as D1 by about a 2:1 margin (actually 1.95:1), their frequency as D2 suggests that they are present on state-numbered routes by somewhat less than that, about 1.57:1. If the D2 proportion is, in fact, an accurate measure of their use of the system, it would be expected that, all else equal, they would be involved in accidents at about the same ratio. Hence, while males greatly outnumber females in the D1 category, their proportional over-representation is not as striking. The male IR is only 1.084, an over-involvement, compared with the female IR of 0.869, an under-involvement (as would be expected with only two groups).

In table 27, it can be seen that males are most responsible for almost 76% of the accidents on interstate highways. This is an increase of about 10% from what was seen in table 26. More importantly, at least in regard to the central issue here, the distribution of D2s also changes. The marginal total (bottom row) shows that about 68% of the D2s are males on the interstate system, an increase of 7% from the state-numbered portion of the system. This implies that the average driver on interstate routes is more likely to see males driving than is the average driver on state-numbered routes. The point here is that male/female highway use patterns (D2 proportions) would be expected to vary according to what portion of the total highway system was being considered while the exposure of the D2s to different D1s (for a given portion of the highway system) should be the same. The method appears to be sensitive to both of these phenomena.

Comparison of the results in tables 26 and 27 suggests that male drivers are somewhat more "dangerous" on interstates than on state-numbered routes. Moreover, the comparison of these two tables also suggests that there is also a change in D2 characteristics corresponding to highway type--e.g., there appears to be a higher proportion of females on state-numbered roads than on interstates.

These distributions suggest that male drivers are more over-involved in accidents on interstate routes (IR=1.12) than on state-numbered routes (IR=1.08). Further, while males are clearly more likely to be the most responsible party for accidents on the interstate system, it is not nearly as disproportionate as suggested by simple comparison of the involvement of male and female drivers as the most responsible party in accidents.

It should be noted that if there are accident-related characteristics (in their own right) that are associated with the sex of driver, their effects in accident responsibility are not separated from the primary characteristics of sex of driver, at least at this level of analysis. For example, if young drivers are more likely to be responsible for accidents and male drivers are more likely to be younger, these effects are confounded at this level of analysis. Likewise, the effect of type of vehicle driven (e.g., pickups or "performance" cars may be more associated with males) is also confounded. Such issues can, however, be straightforwardly addressed through appropriate stratifications of the data.

Matrices similar to tables 26 and 27 were also constructed using 1987 data. Table 28 shows the comparison of the row percentages from tables 26 and 27 (1986 data) with the same figures using 1987 data. In summary, table 28 shows:

1. Male D1s are involved with the same proportions of male D2s (and female D2s) as female D1s for either of the two roadway types.

2. There is variation in the overall percentages of males and females involved as D1 and D2 by roadway type--the percentages of male D1s and D2s change by roadway type, but the result noted in point 1 is consistent.

3. While there is a slight year-to-year variation for a given roadway type, the results noted in points 1 and 2 are generally consistent for the two years considered.

Further, the minor variation between the two years shows that more females are both on the road (percent of D2s who are female) and more likely to be involved in accidents as D1s. This trend is the same for both route classes examined. As an aside, if any change in the percentage of females on the highway system was to be expected, it would seem logical that the trend would be one of increase given stronger role in other activities. As noted earlier,

Table 28. Year-to-year Trends for State-Numbered and Interstate Routes

State-numbered Routes

	D-2 male	D-2 male	marginal totals 1986	1987
D-1 male (1986)	61.7	38.3	66.1	64.9
D-1 male (1987)	60.5	39.5		
D-1 female (1986)	59.7	40.3	33.9	35.1
D-1 female (1987)	59.9	40.1		
marginal total (1986)	61.0	39.0		
marginal total (1987)	60.3	39.7		

Interstate Routes

	D-2 male	D-2 male	marginal totals 1986	1987
D-1 male (1986)	68.0	32.0	75.6	75.2
D-1 male (1987)	66.6	33.4		
D-1 female (1986)	66.5	33.5	24.4	24.8
D-1 female (1987)	66.9	33.1		
marginal total (1986)	67.6	32.4		
marginal total (1987)	66.7	33.3		

comparison of the all-interstate data to the I-94 field data also shows an increasingly close "fit".

Similar statistics were also examined for other parts of the highway system. Considering approximately 14,000 accidents in 1986 on US-numbered (not including interstates) routes, 67.6% of the D1s and 64.0% of the D2s were male for an over-involvement of 1.056. More importantly, 64.0% male D2s were involved with male D1s and 64.1% male D2s were involved with female D1s. However, the agreement was not as good between the two years since for 1987, 63.3% male D2s were involved with male D1s and only 61.3% male D2s were involved with female D1s. The year-to-year variation was greater than noted in table 28 although the increase, again, was for female drivers as both the responsible driver and the non-responsible driver.

On the local road system (county roads and city streets), examination of almost 119,000 1986 accidents showed that 59.8% male D2s were involved with male D1s and 57.4% male D2s were involved with female D1s. Male over-involvement in general is given by the overall ratio of male D1s (64.9%) and male D2s (58.9%) or 1.102. These percentages changed by less than 1% between 1986 and 1987. Once again, percentage gains were exhibited by females both as D1s and D2s.

The discussion of the data to here suggests that the exposure of males and females, as measured by the distribution of D2s by sex, varies according to the portion of the roadway system that is being considered. Moreover, for each roadway type the percentage of males and females involved as D2s were the same regardless of the sex of D1--strong empirical evidence that D2 characteristics are measures of exposure. Still further, although the percentages of males and females involved as D1 and D2 varied by roadway type, the year-to-year variation of these percentages for any given roadway type was relatively slight--although in all instances female involvement increased in terms of exposure and accident causation.

Variation in both D1 and D2 distributions might also be expected by time of day with the roadway type held constant. For example, it seems logical to expect that there would be higher proportions of male drivers both as D1 and D2. This assertion is based on a general review of the literature and the fact that males are more likely to be driving (even when both sexes are in the vehicle) at night.

Table 29 shows the day-night statistics for the interstate system for 1986 and 1987 accidents. The first thing that shows up is the difference between the night and day distributions of D1 and D2 by sex. The data suggest that there are significantly more males driving on the system at night than during the day, and, further, that males are even more over-represented in terms of causing accidents. The year-to-year variation is somewhat more pronounced, especially for the night accidents. The increasing trend toward more females as D1s and D2s is not

Table 29. Year-to-year Trends for day and night accidents on Interstates

Day	D-2 male	D-2 female	marginals
D-1 male (1986)	66.7	33.3	73.4
D-1 male (1987)	65.4	34.6	73.3
D-1 female (1986)	65.5	34.5	26.6
D-1 female (1987)	65.9	34.1	26.7
marginals	66.4	33.6	
	65.6	34.1	

Night	D-2 male	D-2 female	marginals
D-1 male (1986)	72.4	27.6	82.4
D-1 male (1987)	70.5	29.5	81.4
D-1 female (1986)	72.5	27.5	17.6
D-1 female (1987)	68.9	31.1	18.6
marginals	72.4	27.6	
	70.2	29.8	

apparent here.

The day-night comparisons were also made for other roadway types (e.g., US-numbered routes, local streets and roads) with similar results as reported for interstates. In each instance, the percentage of males increased at night--both as D1s and D2s. Generally speaking, the increase in the D1 male percentage was somewhat higher than the increase of the D2 male percentage. For example, on interstates the D1 male percentage was 73.4% during the day and 81.4% at night (an eight percentage point increase), while the driver D2 male percentage was 66.4% during the day and 72.4% at night (a six percentage point increase). For the local streets and roads, the increase for D1 males was 12% versus just less than 10% for D2.

The night accident statistics for local streets and roads also showed the most variation in the row distributions: for night accidents in 1987, for example, the percentages of males who were involved as D2s was 66.1% when D1 was male and 63.6% when D1 was female; a difference in the row distribution of 2.5%. Interestingly, a very similar variation was also seen for 1986 data.

It seems reasonable to expect that the sex of driver proportions might vary by time of day while holding the portion of the system being considered constant. For example, it seems logical to expect that there would be higher proportions of male drivers both as D1 and D2 at night. This is explored by defining three time periods:

non-rush hour daytime (9:00 AM-4:00 pm), rush-hour periods (7:00-9:00 AM and 4:00-6:00 PM), and non-rush nighttime (6:00 PM-6:00 AM). The differences in D1 and D2 proportions by sex are shown for these time periods for Michigan interstates in table 30.

Note the relative similarity in the D2 distributions across the rows for any time period. The rush-hour periods shows the greatest disagreement (about three percentage points), while the distributions for the non-rush periods are very similar. The data also suggest that, as expected, there are significantly more males driving at night than during the day, and further, that males are even more overrepresented in terms of causing accidents.

The application of the technique is clearly not limited to merely the sex of the driver--other researchers at Michigan State University have applied it to a consideration of different car types Kuroda (1984) and driver age McKelvey, et al. (1987). These investigations did not, however, demonstrate the validity or possible limitations of the technique. Several other authors have also used the involvement ratio (relative risk) without comment as to its validity or sensitivity.

Extension of the quasi-induced concept to age suggests that age distributions of the D2s should be the same regardless of which sub-population of D1s is considered. That is, male D1s should be involved with D2s with the same age distributions as female D1s are involved. The

Table 30. Variation in D1-D2 Matrix by Time of Day for Interstate routes

		D2 male	D2 female	D1 total	
a.	D1 male	1810 (65.8)	941 (34.2)	2751 (73.0)	IR male = 1.11
	D1 female	678 (66.7)	339 (33.3)	1017 (27.0)	
	D2 total	2488 (66.0)	1280 (34.0)		IR female = 0.80
b.	D1 male	1541 (63.4)	939 (36.6)	2480 (69.5)	IR male = 1.11
	D1 female	656 (60.2)	434 (39.8	1090 (30.5)	
	D2 total	2197 (61.5)	1373 (38.5)		IR female = 0.80
c.	D1 male	2232 (71.4)	894 (28.6)	3126 (78.4)	IR male = 1.11
	D1 female	605 (70.3)	256 (29.7)	861 (21.6)	
	D2 total	2837 (71.2)	1150 (28.8)		IR female = 0.80

a. - non-rush daytime

b. - rush-hour periods

c. - non-rush nightime

comparison that is suggested by this is shown in table 31. In the upper part of table 31, the age distribution of all D2s who were involved with male D1s is compared with the age distribution of all D2s who were involved with female D1s. There is a remarkable similarity between the two age distributions.

Operating under the assumption that D2 is a random sample of drivers on the road, the age distribution of male drivers might be expected to be somewhat different than female drivers--thus, the age distribution D2s by sex was also examined. The lower part of table 31 shows that the age distribution of the male D2s when D1 was a male is basically the same as the distribution of male D2s when D1 was a female. Likewise, the two age distributions of the female D2s are similar to each other but somewhat different than from the males. This suggests that the age distribution of drivers on the road is different by sex--males tend to be somewhat older.

As additional stratifications are considered for a set of accident statistics, the cell populations in the D1/D2 matrix become smaller and instability in the row distributions is noted. In addition, it is not clear if this instability is due to other phenomena or simply to sample size. The "other" phenomena simply could be that the technique does not work or that there is a "proneness" effect that is not being adequately considered. In the male-female example, males might just be more accident-

Table 31. D2 Age Distributions

D2 Age Distributions (not differentiated by sex)

age groups		D1 male	D1 female
	16-25	37361 (29.6)	20306 (29.8)
	26-45	55509 (44.0)	29833 (43.8)
	46-65	22390 (17.0)	11640 (17.1)
	>65	11019 (8.7)	6320 (9.3)

D2 Age Distributions differentiated by sex

		D2 male		D2 female	
		D1 male	D1 female	D1 male	D1 female
age groups	16-25	21856 (28.9)	11483 (29.1)	15505 (30.7)	8823 (30.8)
	26-45	32312 (42.6)	16588 (42.1)	23197 (45.9)	13245 (46.2)
	46-65	14054 (18.5)	7071 (17.9)	8336 (16.5)	4569 (15.9)
	>65	7507 (10.0)	4293 (10.9)	3512 (6.9)	2027 (7.1)
		75729	39435	50550	28664

prone, and thus more likely to show up as D1 or as D2. For these reasons the practical limits of the utility of the approach are explored using random numbers.

6.2 Random Number Analysis

If two sets of random numbers are generated (one set for D1, one set for D2) and paired, a matrix which is analogous to the D1/D2 matrix can be constructed. In tables 32 and 33, the results of table 26 and 27 are "recreated" using quasi-random numbers and the observed marginal distributions. The latter defined the "outcomes" (whether D1 and D2 were male or not) for observations in table 32. Random numbers were assigned to both D1s and D2s in the same ratio of males to females found in the real data. Observation totals of N=18016 and N=10644 were used for tables 32 and 33, respectively. The latter is quite similar to the actual number of observations in table 27.

In table 32 minor variations in the row and marginal proportions, relative to real data in table 26, can be seen. Likewise, there is variation in the real IR and the simulated IR. The row and marginal proportions vary a maximum of 1.1 percentage points (between the tables) while the IR varies by 0.01.

The results shown in table 33 are similar in that they are consistent with table 27. Again, it is seen that there are some variations in both the cell and marginal proportions. The largest difference is seen for females

Table 32. Random Number (simulated) Distributions of D1s and D2s by Sex for Michigan Accidents on State-numbered Routes

		D2 male	D2 female	D1-total
D1	male	7376 (61.9)	4534 (38.1)	11910 (66.3)
	female	3713 (60.4)	2393 (39.6)	6106 (33.7)
	D2-total	11089 (61.4)	6927 (38.6)	18016

Hence, Involvement Ratio (IR)-male = 66.1/61.6 = 1.073

Involvement Ratio (IR)-female = 33.9/38.4 = .883

(Compare this table with table 26)

Table 33. Random Number (simulated) Distributions of D1s and D2s by Sex for Michigan Accidents on Interstate Routes

		D2 male	D2 female	D1-total
D1	male	5386 (67.3)	2617 (32.7)	8003 (75.2)
	female	1819 (68.9)	822 (31.1)	2641 (24.8)
	D2-total	7205 (67.7)	3439 (32.3)	10644

Hence, Involvement Ratio (IR)-male = 75.2/67.7 = 1.111

Involvement Ratio (IR)-female = 24.8/32.3 = .768

(Compare this table with table 27)

where the distribution changes from 66.2 and 33.8 to 68.9 and 31.1. The IRs change from 1.118 to 1.111 for males and 0.753 to 0.768 for females. Given that the cell sizes and overall N are fairly large, it seems rather clear that there are practical limitations in using the IRs to detect small differences--i.e., on the order of 0.02 at best for a simple 2x2 matrix. Random errors can obviously account for at least that much variation. Indeed, another simulation, similar to table 33 (with a sample size of 10649, slightly higher than shown here), yielded identical row proportions but slightly different overall D1 proportions by sex which, in turn, resulted in variations in the calculated IRs--i.e., for males, 1.108 versus 1.111, and for females, 0.774 versus 0.768.

Examination of the results using a standard contingency table approach resulted in chi-square values with a significance of between 10 and 15%--that is, there is a 10-15% chance that the observed differences in the data could be simply due to sampling error.

The issue of sample size was also investigated for a different number of row categories (five) to simulate groupings of driver-vehicle combinations that would occur with other variables--e.g., five driver age groups. This was done for values of N, the total number of observations in the matrix, ranging from 500 to 25,000. In each instance, one of five possible values was randomly assigned to each "D1" and "D2" so that a five-by-five matrix resulted. With a total of 500 observations each cell should end up with the same number of observations (20), each row and column should total 100, and the row proportions should be 20%. Considerable variation among the cell proportions, was noted, although the known distributions should have been 20%, observed cell proportions ranged from 9 to 28%. The IRs calculated from such row and column totals also vary considerably.

Hence, the IRs are relatively unstable with modest total sample sizes for a five-by-five matrix. The question becomes at what value of N (and for the column totals) does the IR stabilize. This was investigated using larger sample sizes. With a total sample size of 500, there are relatively small samples in the cells of a five-by-five matrix and they may vary by a considerable amount (9 to 28 observations, 9.3 to 28.9%). Only when the overall N equals 3,000, table 34, do the marginal proportions all fall within plus or minus one percentage point--that is, all of the row and column percentages are within the range 19.0 < [marginal proportions] < 21.0. Even then, the variation in the calculated values of the IRs, table 35, is fairly large--plus or minus about 0.05 (all IRs should =1.0).

The above result implies that with a five-by-five matrix, simple random variation limits the accuracy of the interpretation of the IR to plus-or-minus 0.05 given N=3,000 and even that is dependent on the "true" proportions being approximately equal. In order to be able to discern

Table 34. The Effects of Overall Sample Size on Marginal Totals

marginal totals (for D2 simulated data)

1	2	3	4	5	N
102 20.4	119 23.8	89 17.8	96 19.2	94 18.8	500
273 18.2	297 19.8	299 19.9	319 21.3	312 20.8	1,500
582 19.4	625 20.8	602 20.1	588 19.6	603 20.1	3,000
929 20.6	905 20.1	857 19.0	891 19.8	918 20.4	4,500
1786 19.8	1773 19.7	1879 20.9	1779 19.8	1783 19.8	9,000
2521 20.2	2465 19.7	2569 20.6	2473 19.8	2472 19.8	12,500
2935 19.6	3092 20.6	3003 20.0	3027 20.2	2943 19.6	15,000
4981 19.9	4987 19.9	5071 20.3	4914 19.7	5047 20.2	25,000

typical cell entries numbers and proportions are of marginal totals for D2

Table 35. Calculated Involvement Ratios (simulated data)

1	2	3	4	5	N	sdev
1.1176	0.8151	1.0899	0.8958	1.1277	500	.14391
1.0330	1.0202	0.9799	0.9718	0.9952	1,500	.02607
1.0412	0.9567	0.9522	1.0153	1.0299	3,000	.04178
0.9612	1.0348	1.0579	0.9848	0.9755	4,500	.04141
1.0051	1.0152	0.9809	1.0202	0.9798	9,000	.01895
1.0198	1.0254	0.9612	1.0051	0.9848	12,500	.02644
1.0153	0.9563	1.0150	0.9851	1.0357	15,000	.03104
1.0050	1.0151	0.9754	1.0102	0.9901	25,000	.01623

Typical entry is (d-1 proportion)/(d-2 proportion) (e.g., from table 4 with N=500, the value for IR in the upper left corner here is 22.8/20.4 = 1.118).

differences when one or more of the proportions is (are) small, an even larger overall sample (N) is required.

Table 36 shows the effect of sample size on the stability of the calculated IR, for a two-by-two matrix. In this case, the true proportions were set for D1 at 76.2 and 23.8 percent, and for D2 at 68.3 and 31.7 percent. This is to designed to simulate the real data shown in table 27. With an overall sample size of about 3000 the marginal proportions fall within plus-or-minus one percentage point. It can be seen that with a sample size of 3000 the interpretation of the IR is limited to plus-or-minus 0.01.

6.3 Examination of the Fleet Mix Variable

Real data are again shown in table 37--actual distributions of D1s and D2s where the variable of interest is the type of vehicle driven (on largely rural sections of I-94 in Michigan). In this instance, only two-vehicle accidents involving only pickups and/or standard automobiles are considered (i.e., accidents involving buses, trucks, and "other" vehicles are not admitted, nor are one-vehicle accidents). There are four possible accident combinations: D1 can be driving either a pickup or a standard auto, and D2 has the same options. The principal purpose again is to examine the distributions of D2--have the drivers of pickups and auto's "caused" accidents with the same proportions of those types of vehicles. In spite of the small sample sizes, the row distributions (as well as the marginal total)

Table 36. Simulated Data 2X2 Matrix

	D1 M	D1 F	D2 M	D2 F		N
	.766	.234	.702	.298		500
	.741	.259	.671	.329		1,500
	.763	.237	.682	.318		3,000
	.762	.238	.664	.336		4,500
	.758	.242	.676	.324		9,000
	.759	.241	.674	.326		12,500
	.755	.245	.676	.324		15,000
	.756	.244	.676	.324		25,000

IDEAL	.756	.244	.676	.324		9,647

Marginal totals for 2X2 matrix, simulating table 27

IDEAL - indicates what the marginal percentages

For N=3000

IR - Males - .763/.682 = 1.119

IR - Females - .237/.318 = 0.745

Table 37 Actual Distributions of D1s and D2s for Pickups and Standard Autos on I-94

		D2 auto	D2 pu	D1-total
D1	auto	893 (92.0)	78 (8.0)	971 (89.3)
	pu	105 (90.5)	11 (9.5)	116 (10.7)
	D2-total	998 (91.8)	89 (8.2)	1087

Hence, Involvement Ratio (IR)-auto = 89.3/91.8 = 0.973

Involvement Ratio (IR)-pickup = 10.7/8.2 = 1.305

appear to be similar--within 1.5 percentage points. The IR-auto is 0.973 which indicates that automobile drivers tend to cause disproportionately fewer accidents than pickup drivers. A simple use of the incidence of automobiles as D1 would indicate that they are nine times as causally-involved as pickups--a gross overstatement relative to their proportionate responsibility and exposure and, indeed, actually misleading (although automobiles do account for the large majority of accidents for the stated conditions).

However, it would appear that pickups are disproportionately represented--potentially an important point with increasing sales of pickups and replacement of the auto as the vehicle of choice with certain age groups. Again, it should be noted that, there may be considerable interaction between the vehicle type and the sex and age of the drivers.

It should also be pointed out here that relatively modest changes in the number of pickups changes the IR-pickup significantly. For example, the addition of two pickups as D2 and two less as autos (the cell entries in the top row and the bottom margin change) cause the IR-pickup to vary by 0.032 while the IR-auto changes by 0.002. Obviously, the magnitudes of the IR must be used and interpreted judiciously when sample sizes are small or the relative proportions of the variable groups are small.

The distributions of D2 vehicle types involved with D1s of different sex were examined. The quasi-induced exposure approach suggests that these distributions should be similar (i.e., D2s constitute a random sample of vehicle types on the road system). Some of the results of this investigation are shown in table 38. Starting with all accidents on the Michigan road system in 1988, it seems clear that both males and females who were at-fault collided with similar distributions of vehicles.

When only interstate accidents are considered, several things are of note: there are relatively minor differences in the two vehicle-type distributions; the percentages of pickups are about the same on interstates as on the total system; the percentage of truck-involved accidents increases significantly for both male and female D1s; and there is a reduction in the proportion of auto-involved accidents.

Table 38 Driver-vehicle Type

all acc		auto	pickup	truck	other
D1	male	109522 (83.5)	12979 (9.9)	3602 (2.7)	5000 (3.8)
	female	58690 (82.9)	7319 (10.3)	1883 (2.7)	2901 (4.1)

interstate only		auto	pickup	truck	other
D1	male	6687 (79.9)	667 (8.0)	689 (8.2)	323 (3.9)
	female	2263 (76.2)	279 (9.4)	280 (9.4)	147 (5.0)

US-numbered only		auto	pickup	truck	other
D1	male	8410 (80.4)	1270 (12.1)	429 (4.1)	345 (3.3)
	female	4114 (78.9)	657 (12.6)	215 (4.1)	228 (4.4)

US-numbered only		auto	pickup	truck	other
D1	male	66287 (84.6)	7303 (9.3)	1622 (2.1)	3164 (4.0)
	female	37429 (84.3)	4254 (9.6)	906 (2.0)	1834 (4.1)

Considering the distributions for the other two roadway types shown in table 38 shows that the distributions "within" a roadway type are in better agreement than they were for interstates and there are some differences evident in the vehicle-type distributions by roadway type. Summarizing some of the results in table 38:

1) there is good agreement between the D2 vehicle-type distributions within a roadway type;

2) there are differences among the D2 vehicle-type distributions by roadway type; and

3) the differences among the distributions by roadway type correspond to those that would be expected a priori--that is, for example, accidents with large trucks are more likely to occur on interstates (where it is known that truck percentages are likely to be higher) and less likely on local roads (where they are less prevalent).

6.4 Summary

The complementary sets analysis provided results which support the second hypothesis. For relatively simple circumstances (e.g., sex of driver), it has been demonstrated that the characteristics of D2 appear to be reasonably good measures of exposure. This conclusion is based on comparisons of the distributions of D2 characteristics involved with different types of D1s. For example, on state-numbered routes in Michigan 61.7% of the accidents caused by male D1s were with male D2s. Likewise, 59.7% of the accidents caused by female D1s were with male D2s. The similarity of the distributions of D2s provides implicit support that D2s constitute a random sample of all those driver-vehicle combinations on the road under the specified conditions.

Comparisons of selected "real" data with simulated observations from known distributions were used to arrive at some practical limits for the interpretation of the IRs. It was seen that for 2x2 matrices (e.g., M-F) that the D2 distributions appeared to give a general indication of over- or under-involvement with a sample size of about 3000 with one of the proportions about 0.4. On the other hand, interpretation of the actual values of the IR was severely limited. For larger matrices (e.g., 5x5 with a known even distribution, an expected value of .2, across the row) the row proportions and the IR did not stabilize until the overall sample size reached about 3000.

It has also been shown that the method is sensitive to changes in the characteristics of the category, or driver-vehicle combination. For example, where the type of roadway is the category of interest, the percentage of male and female drivers varied according to roadway type. However, the hypothesis of similar D2 characteristic distributions within a category remains valid. The method was also demonstrated for other variables--the types of vehicle being driven by D1s and D2s, and D2 age.

7.0 LIMITATIONS OF THE QUASI-INDUCED EXPOSURE METHOD

The quasi-induced exposure potentially method overcomes a number of problems associated with estimating exposure to the risk of traffic accidents. It makes use of traffic accident data, which is generally available, avoiding a number of the problems associated by gathering exposure data by other means. Prudent selection of those cases where the driver-vehicle combination is clearly not "at fault", overcomes some of the concerns expressed about the innocence or guilt of drivers. This still represents a limitation, to some, of this technique as there is some debate as to whether the so-called innocent drivers are completely innocent.

This method appears to be appropriate at various levels of analysis. Where large data sets are available, data can be stratified on exposure estimates at fine levels can be made, but care must be taken to insure adequate cell sample sizes. As was shown in the complementary sets analysis, for two-by-two matrices, a sample size of 3000 is required, and for a five-by-five matrix a sample size of 3000 is required for the IR value to stabilize. This reflects the problem with the overall sample size, but the individual cell sample sizes are also critical with respect to statistical considerations. The results of the I-94 analysis tend to support the sample sizes required as indicated by the random number analysis. For the variables studied in the field study, there were not adequate sample sizes for the cells in the accident data matrices. This made statistical testing virtually impossible. Certainly the accuracy of the estimates made by the quasi-induced method, based on the inadequate accident data, are suspect.

Since this method is based on accident data, the quality of the exposure estimates are dependent on the quality of the accident data. Other researchers have suggested that there are problems with most accident data. These range from problems associated with the assignment of responsibility for an accident, to those associated with potential bias in the reporting of accidents. Studies of data collected in Michigan suggest that these occurrences are random in nature and the data quality is adequate.

If a particular driver-vehicle combination was somehow incorrectly represented in the accident statistics (e.g., systematically under-reported), this would impact the relative proportion reflecting their exposure. However, it seems likely that those driver-vehicle combinations that are involved as the non-responsible combination in an accident, are randomly unreported. If there is a bias in driver-vehicle combinations responsible for accidents not reporting accidents, it will influence the numerator in IR calculations. It does not affect the measure of exposure, which is the issue here.

The quasi-induced exposure method is capable of only producing relative measures of exposure. The method can only be used to make comparisons between several classes to each other but with no "absolute" measure of exposure.

This method, like most estimating techniques, suffers some limitations with respect to sensitivity. The complementary sets analysis was used to explore the range to which the quasi-induced exposure method is capable of measuring. This analysis suggests that for a five-by-five matrix, simple random variation limits the accuracy of the interpretation of the IR to plus-or-minus 0.05, given an overall sample size of 3000.

8.0 CONCLUSION AND SUGGESTIONS FOR FURTHER RESEARCH

If it is assumed that in many accidents there is a driver who is "responsible" (D1) and one who is "non-responsible" D2 it has been argued that characteristics of the latter (e.g., sex, age, type of vehicle driven) constitute a random sample of the characteristics of driver-vehicle combinations on the road at the time of the accident. That is, the characteristics of D2s are implicit measures of "exposure." Subsequently, it was argued that the ratio of the proportions of D1s with stated characteristics to D2s with those same characteristics is an involvement ratio, (IR), a measure of the "relative risk" associated with that characteristic. If IR is greater than one, then that characteristic is disproportionately over-represented in accident causation. For example, if the proportion of males who are D1 is higher than the proportion of males who are D2, then males are disproportionately more likely to cause accidents--the magnitude of IR indicates, within reason, how disproportionate the effect is.

In order to study the effects of various driver-vehicle combinations, and the effects of different types of roadways and roadway conditions, the accident data must be stratified. If a certain vehicle-driver-roadway combination is desired, that combination can be selected by stratification based on the values available in each general category. These might include: type of vehicle; make of vehicle; vehicle age; sex of driver; age of driver; interstate, divided or undivided, local streets; and dry, wet, slippery, or snow. By controlling these, specific effects of each can be isolated.

While the concept of using the IRs as a measure of those driver, vehicle, and roadway characteristics which are most related to accident risk is not particularly new, validation of the technique and the quasi-induced exposure approach has been lacking. The empirical study described here was designed as part of the first step in trying to validate this procedure.

This paper has presented two empirical methods to test hypotheses fundamental to the quasi-induced exposure method of estimating exposure. The essential point to be examined was that the non-responsible driver-vehicle combination represents a random sample from the traffic stream. This research should be viewed as the first step of a multi-step process designed to validate this exposure technique. The benefits and limitations of this approach have been presented, as well as the results of the two empirical studies.

Unfortunately, the field portion of this research was less successful than hoped. For the percent males/females driving autos, the results indicated agreement between the field observations and quasi-induced exposure estimates. The results for the rest of the variables were mixed. The major problem was the lack of adequate accident data for the road segment of interest.

The complementary sets study produced very encouraging results. For several variables that were examined, this method yielded results consistent with the stated hypothesis, offering evidence to support the random sample concept. This analysis also provided some insight into the sensitivity and level of error associated with the quasi-induced exposure method.

It has also been shown that the method is sensitive to changes in the characteristics of the category, or driver-vehicle combination. For example, where the type of roadway is the category of interest, the percentage of male and female drivers varied by roadway type. However, the assumption that D2 characteristic distributions within a category are the same remains valid. The method was also demonstrated for other variables--the types of vehicle being driven by D1s and D2s, and D2 age.

Overall, these results indicate that this empirical approach to validating the quasi-induced method of estimating exposure has great potential. However, further work in this area is required for the "proof" to be complete. The following are suggested:

- Repeat this basic research in an area where there are larger numbers of accidents occurring. This might be accomplished in urban areas where automatic data recording is available for the traffic data.
- Expand the number of variables to be examined by both methods of testing presented here--both the field comparison and the complementary sets analysis.

- Conduct similar field observations on turnpike segments where there is a sufficient accident history. It may be possible to simplify some of the data collection activities.
- Identify other studies which may provide useful estimates of exposure. For example, direct observation studies conducted for purposes other than collecting exposure information such as the seat belt study.
- Make more comprehensive data collection efforts on a series of roads, using automated vehicle counters.

As a closing note, the use of the quasi-induced method to estimate exposure is an attempt to circumvent the need to collect some other data. Quasi-induced exposure estimates could be used much more effectively than VMT when stratifications finer than the system level are required. Finally, quasi-induced exposure estimates are just that--estimates. VMT should not be considered more than one estimate of exposure either. In this context, quasi-induced estimates do not have to be "better" than VMT to be more efficient to use.

APPENDIX A

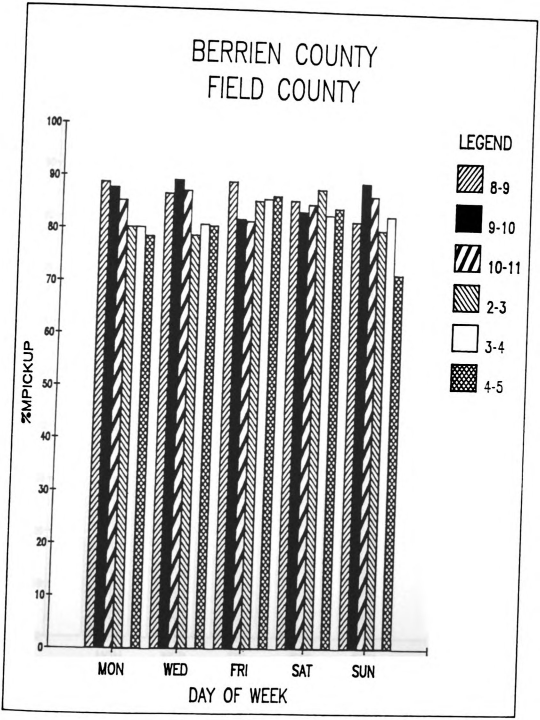

Figure 16. Field Data for Berrien County - %MPICKUP

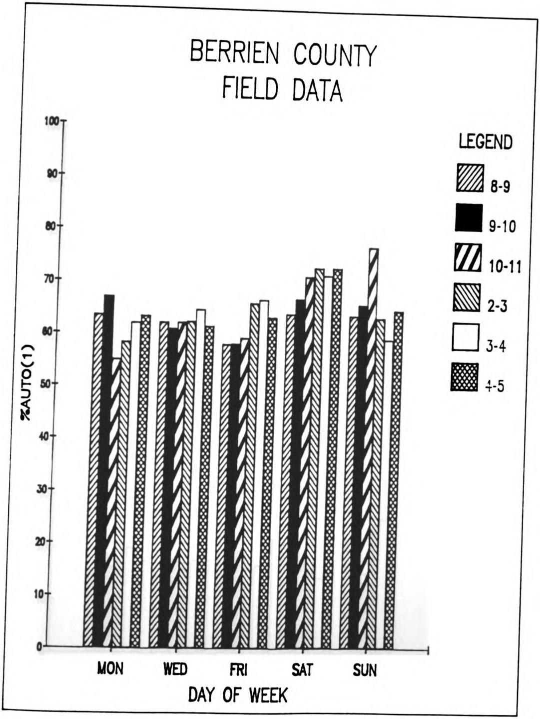

Figure 17. Field Data for Berrien County - %AUTO(1)

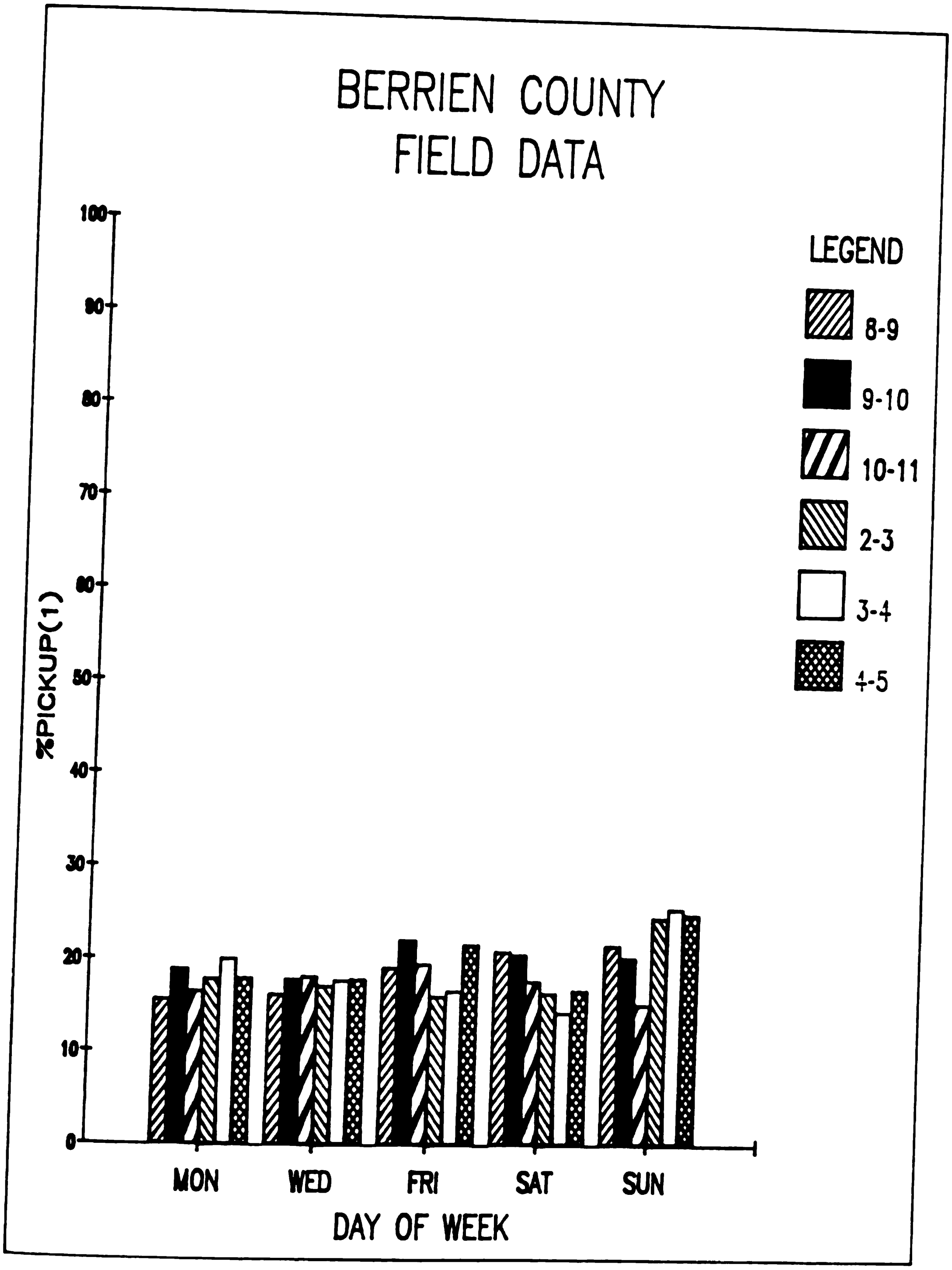

Figure 18. Field Data for Berrien County - %PICKUP(1)

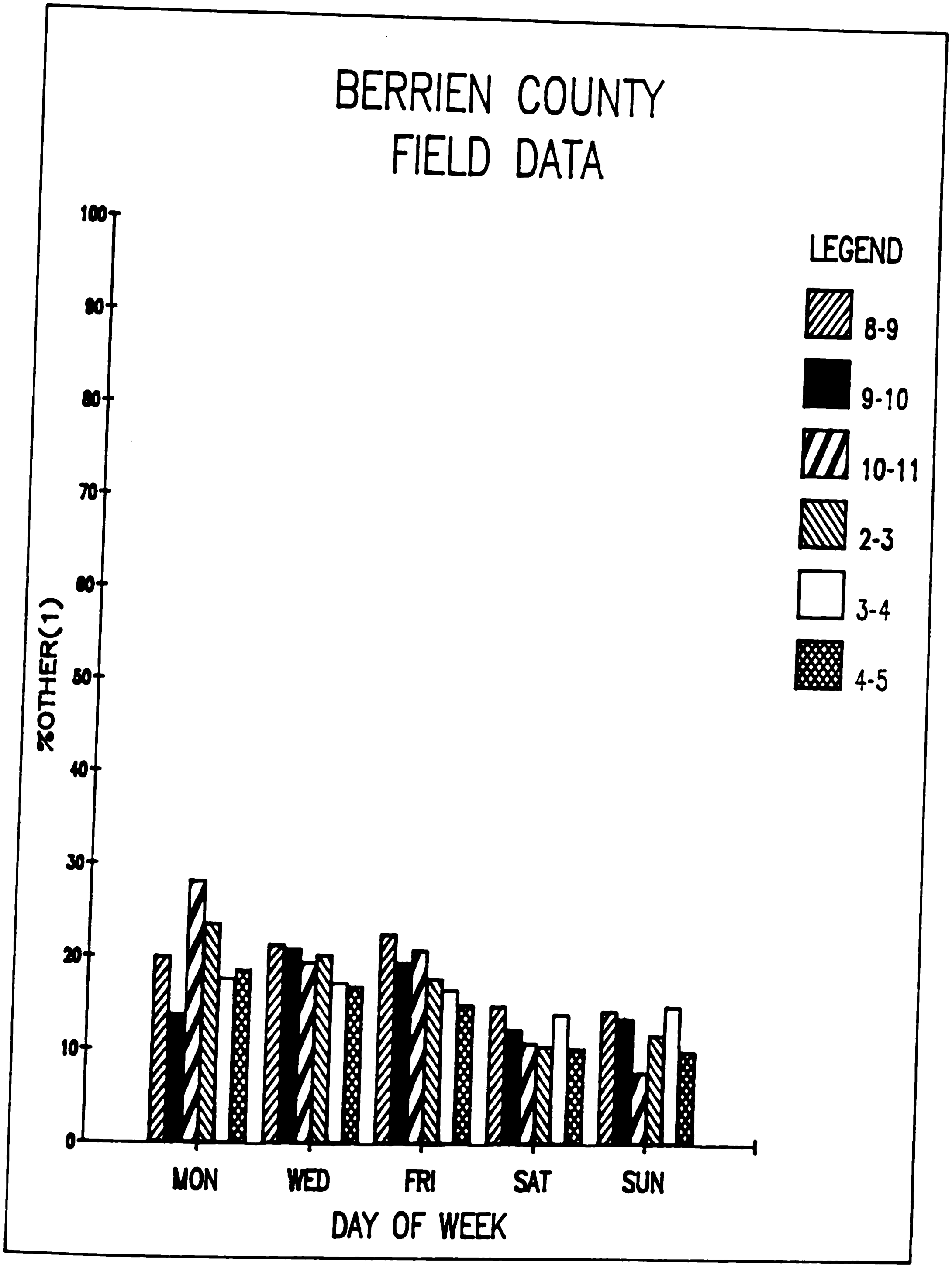

Figure 19. Field Data for Berrien County - %OTHER(1)

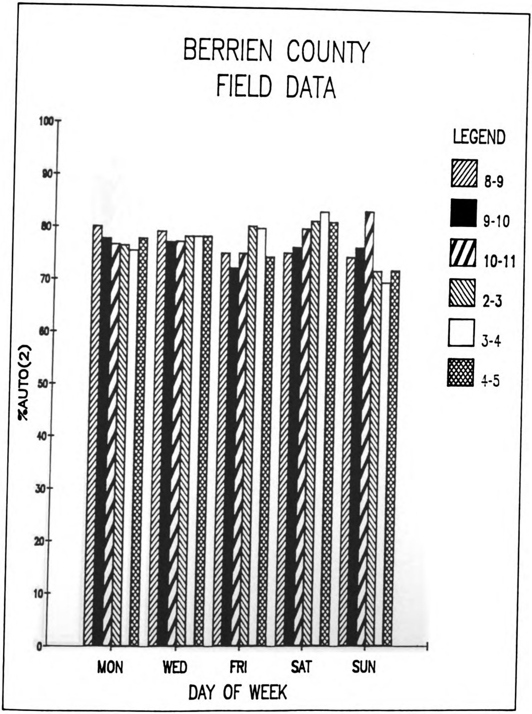

Figure 20. Field Data for Berrien County - %AUTO(2)

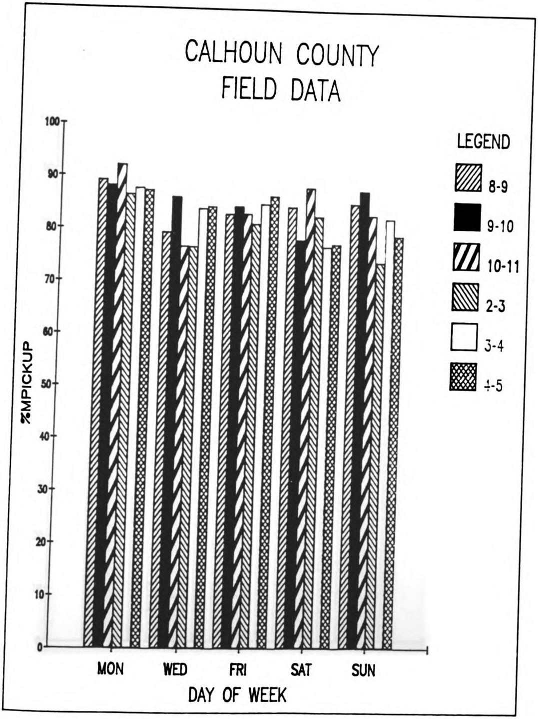

Figure 21. Field Data for Calhoun County - %MPICKUP

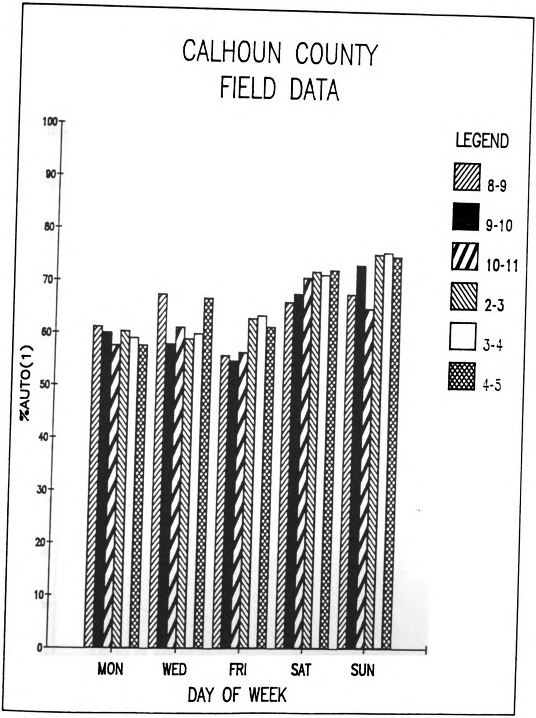

Figure 22. Field Data for Calhoun County - %AUTO(1)

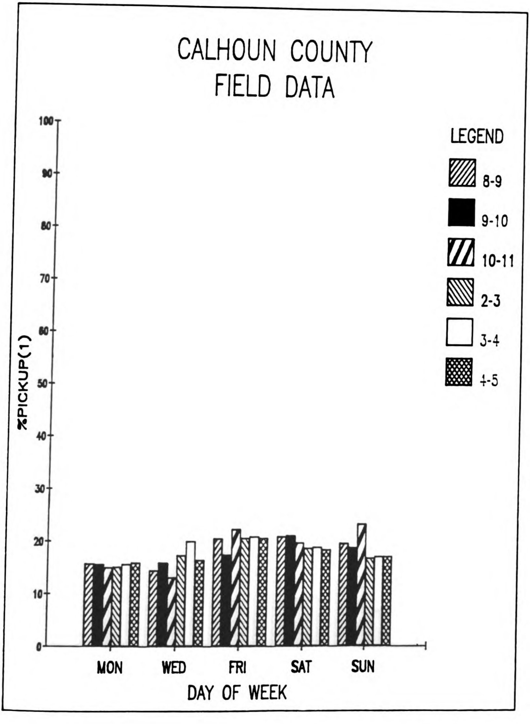

Figure 23. Field Data for Calhoun County - %PICKUP(1)

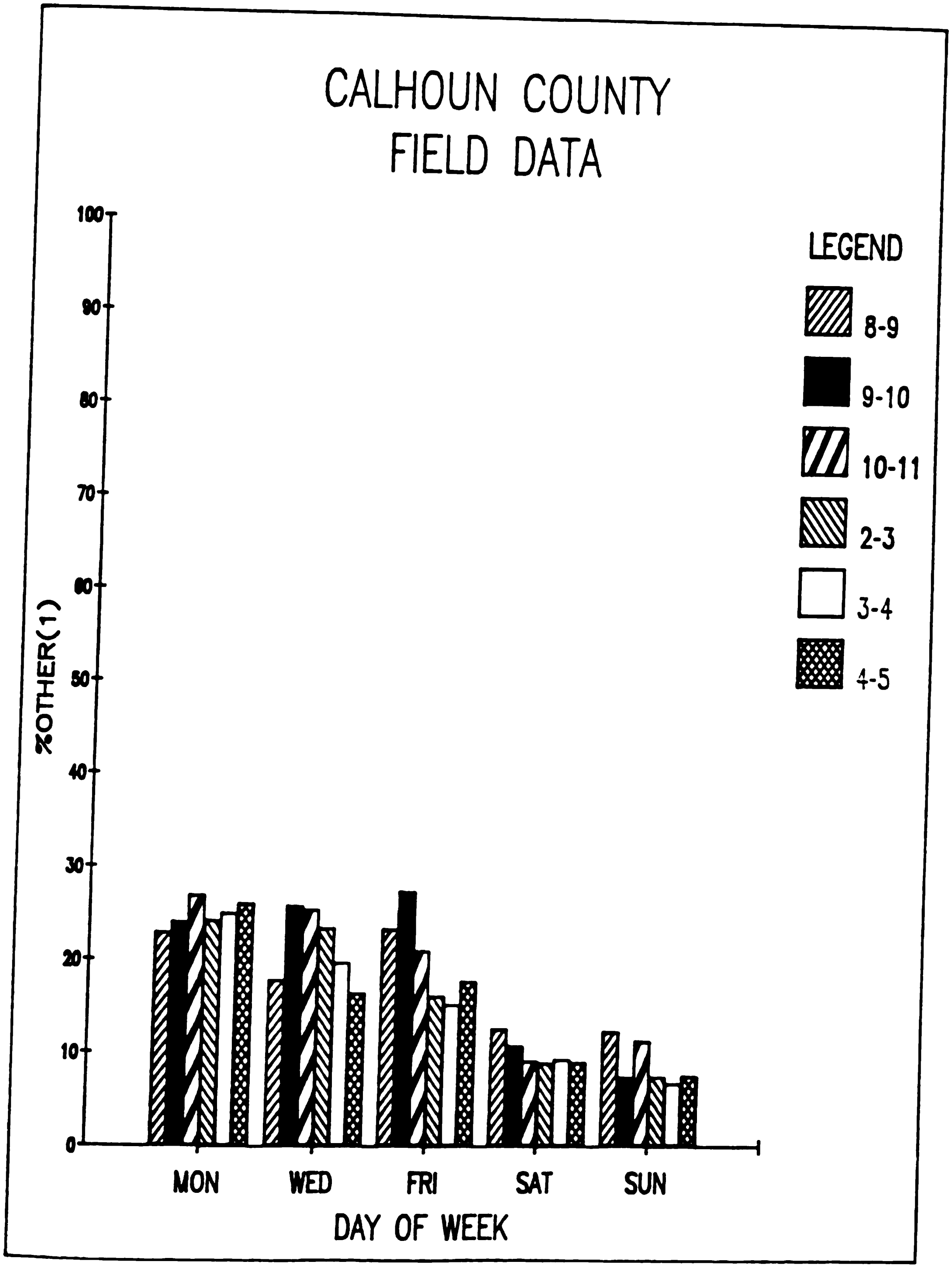

Figure 24. Field Data for Calhoun County - %OTHER(1)

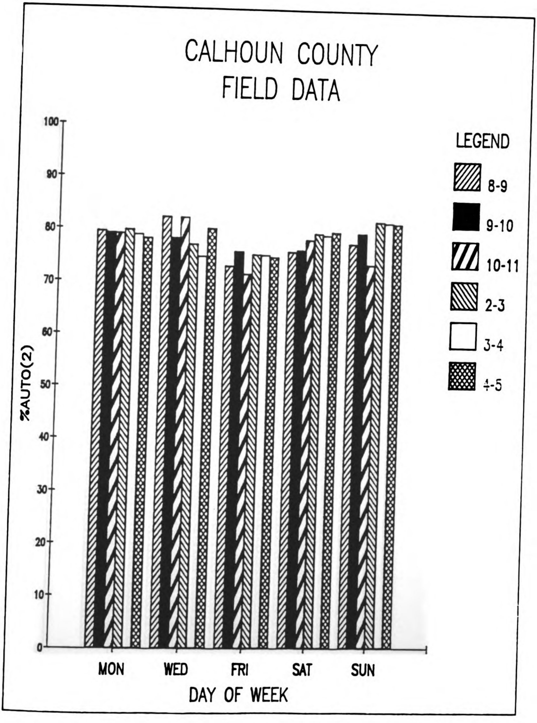

Figure 25. Field Data for Calhoun County - %AUTO(2)

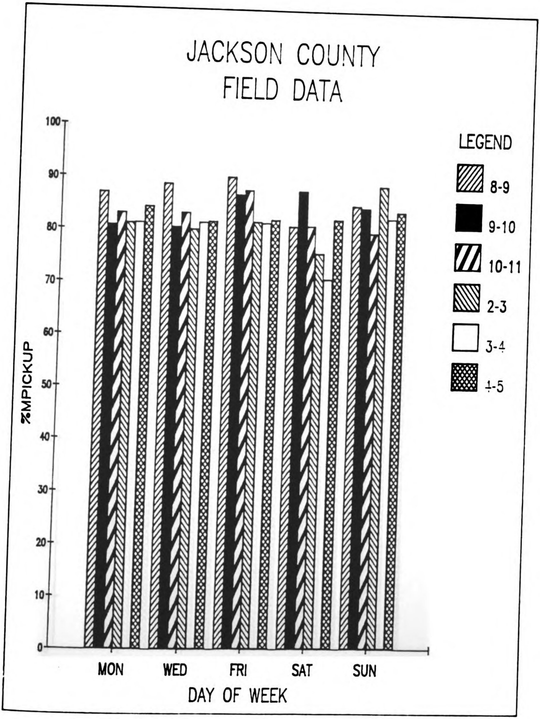

Figure 26. Field Data for Jackson County - %MPICKUP

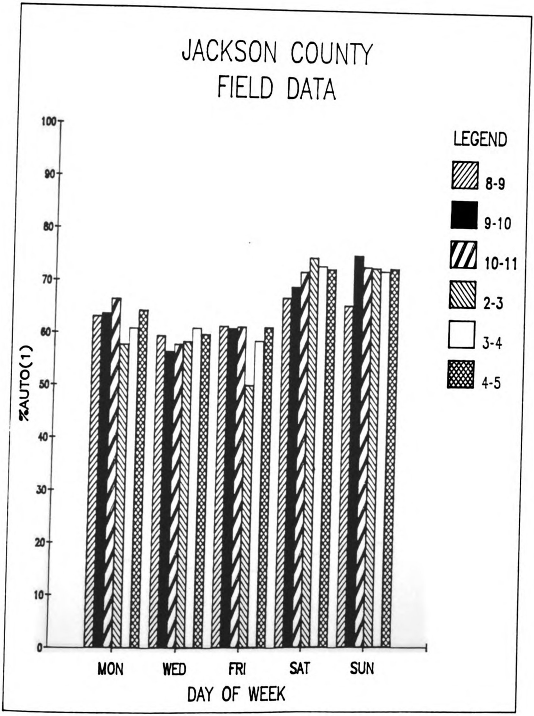

Figure 27. Field Data for Jackson County - %AUTO(1)

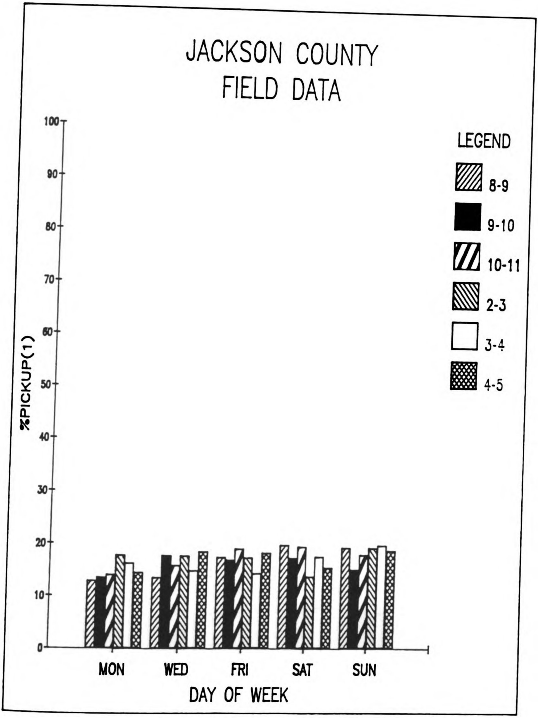

Figure 28. Field Data for Jackson County - %PICKUP(1)

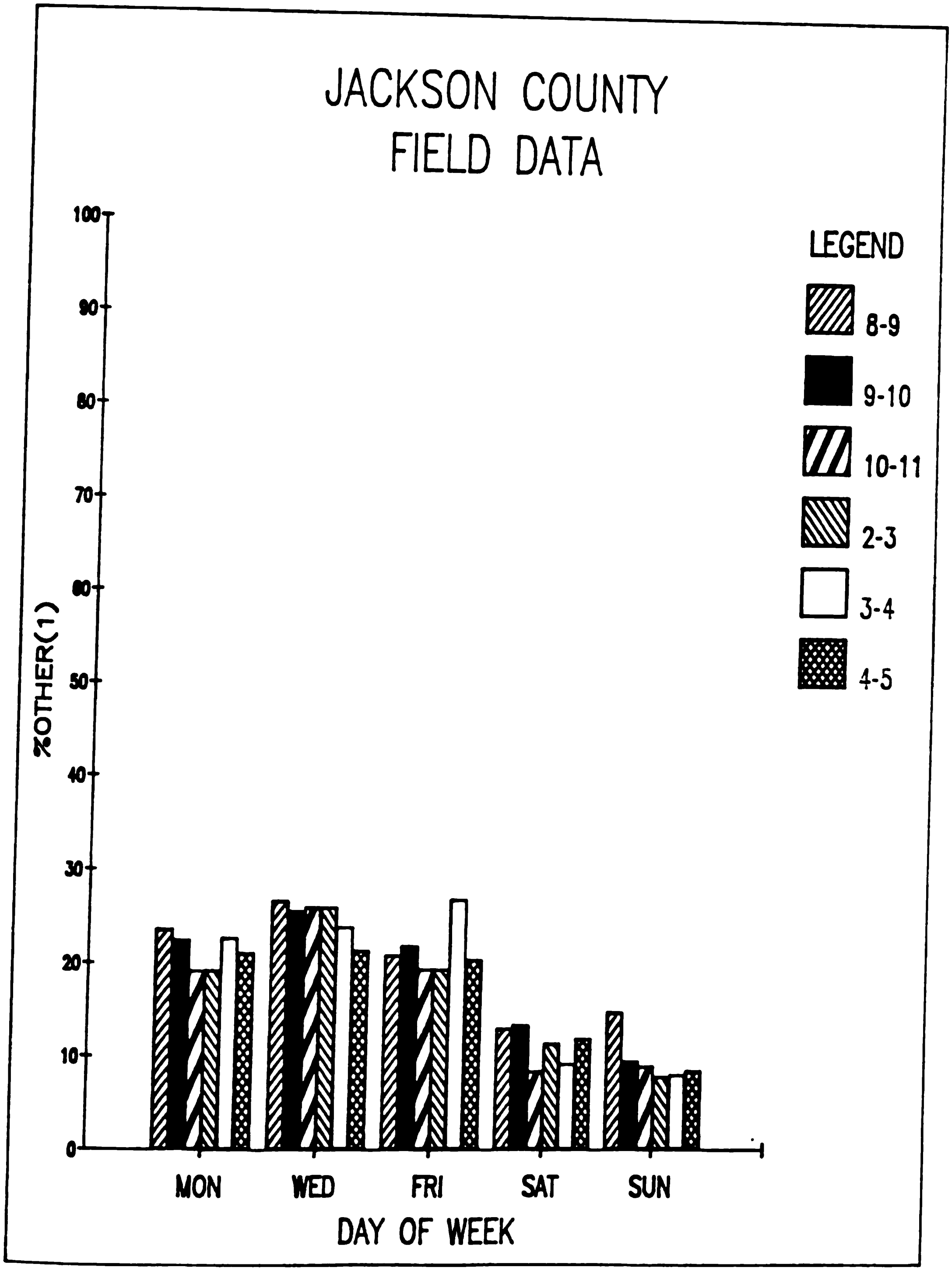

Figure 29. Field Data for Jackson County - %OTHER(1)

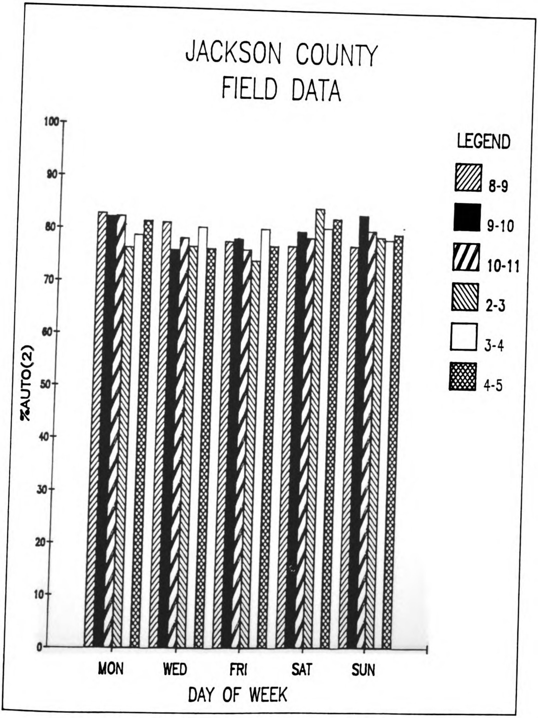

Figure 30. Field Data for Jackson County - %AUTO(2)

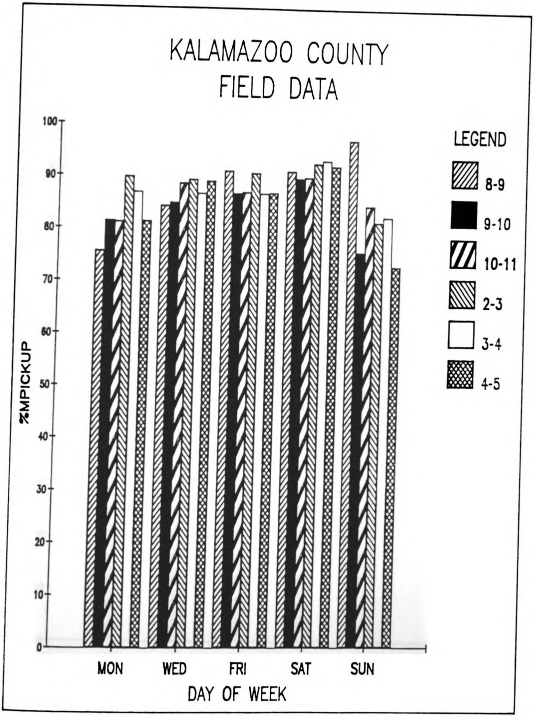

Figure 31. Field Data for Kalamazoo County - %MPICKUP

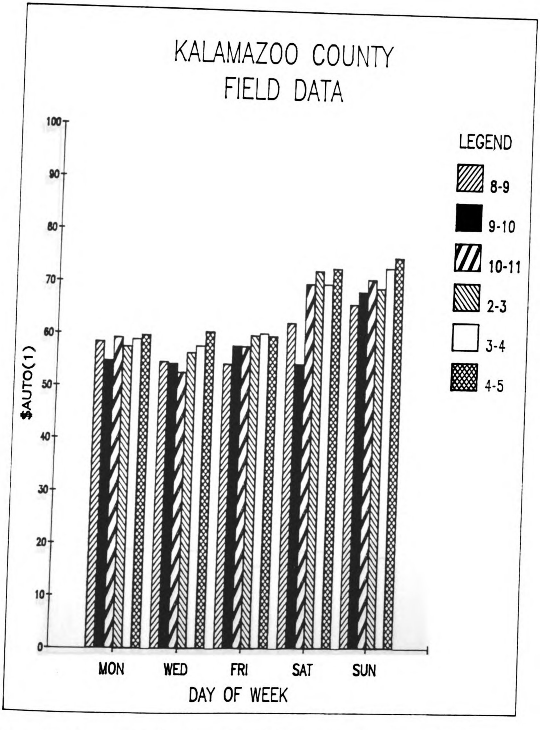

Figure 32. Field Data for Kalamazoo County - %AUTO(1)

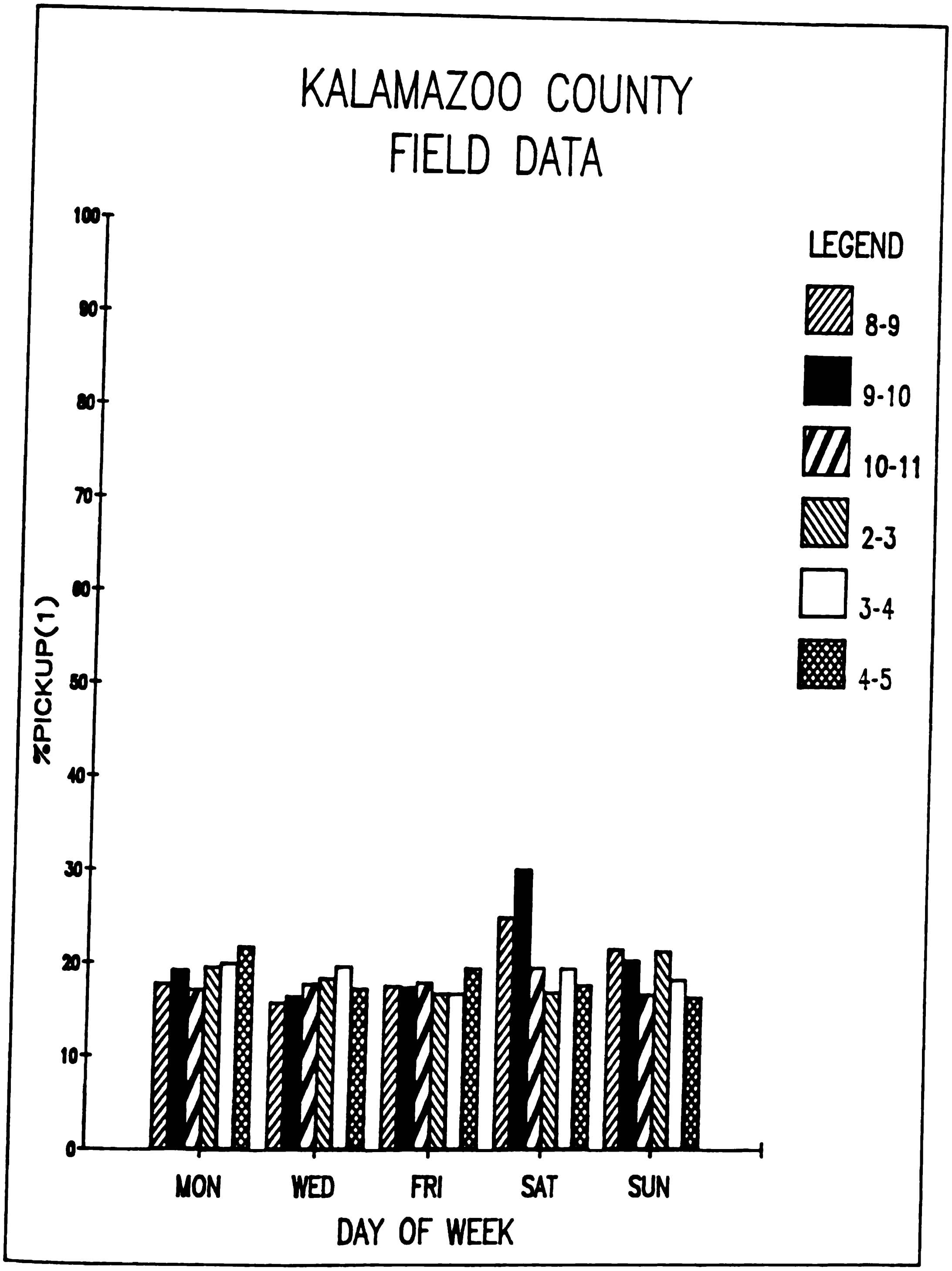

Figure 33. Field Data for Kalamazoo County - %PICKUP(1)

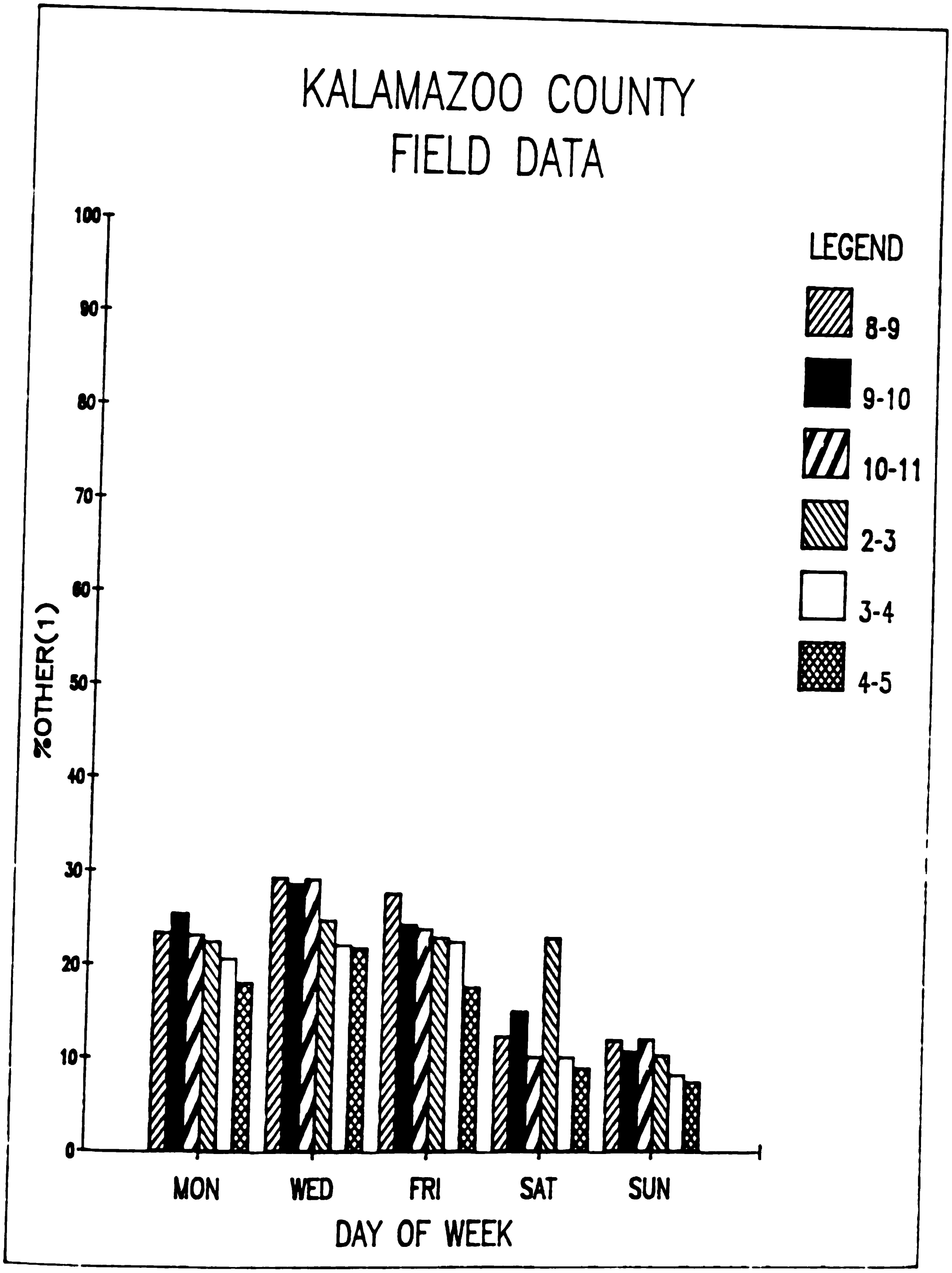

Figure 34. Field Data for Kalamazoo County - %OTHER(1)

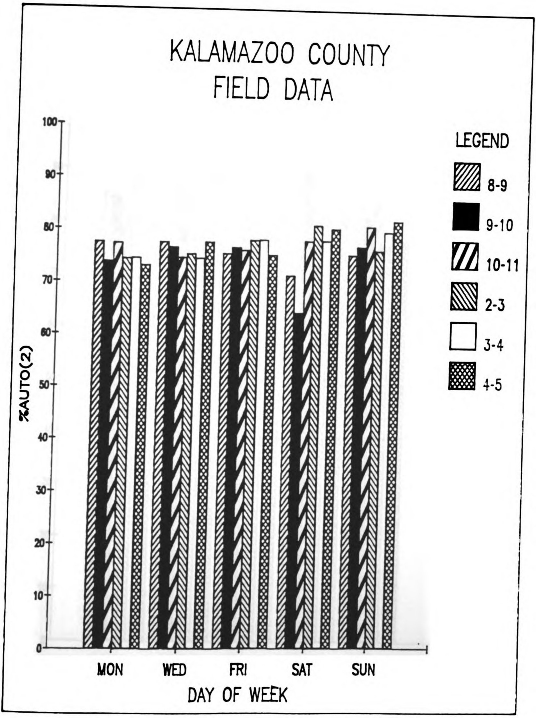

Figure 35. Field Data for Kalamazoo County - %AUTO(2)

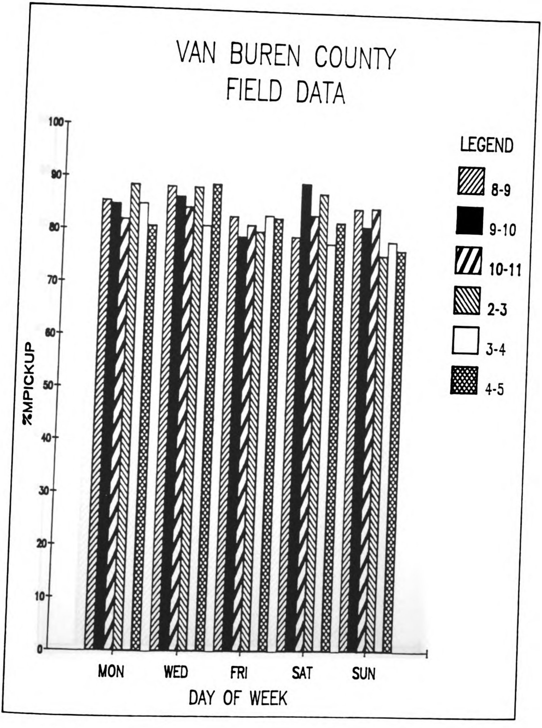

Figure 36. Field Data for Van Buren County - %MPICKUP

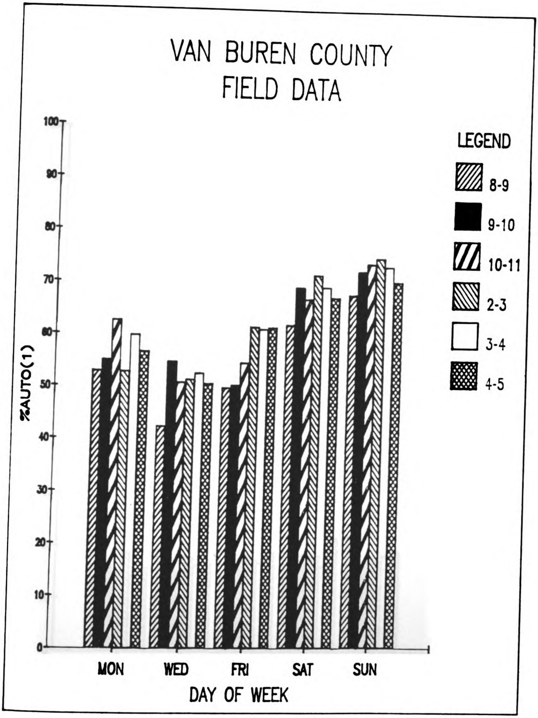

Figure 37. Field Data for Van Buren County - %AUTO(1)

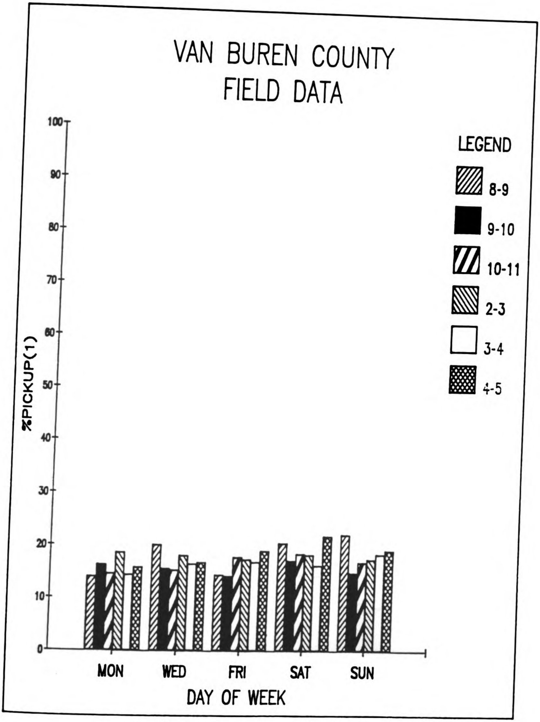

Figure 38. Field Data for Van Buren County - %PICKUP(1)

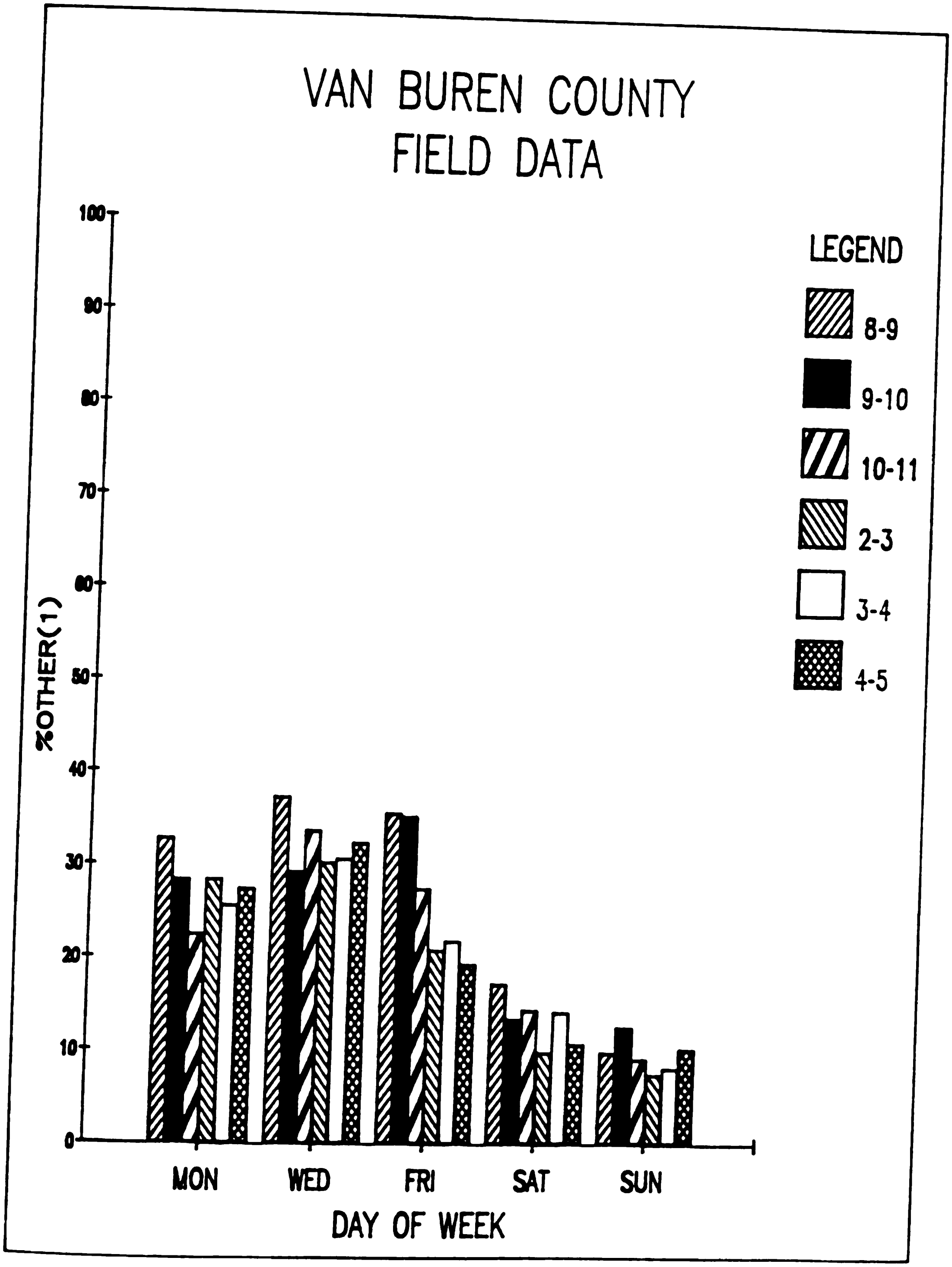

Figure 39. Field Data for Van Buren County - %OTHER(1)

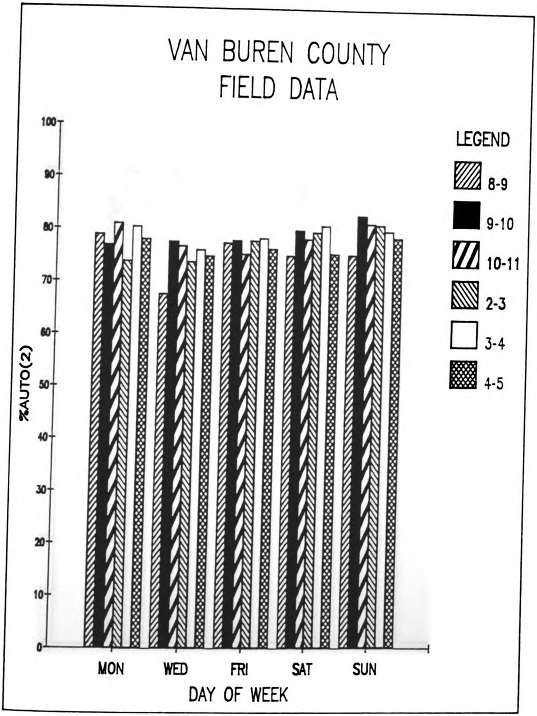

Figure 40. Field Data for Van Buren County - %AUTO(2)

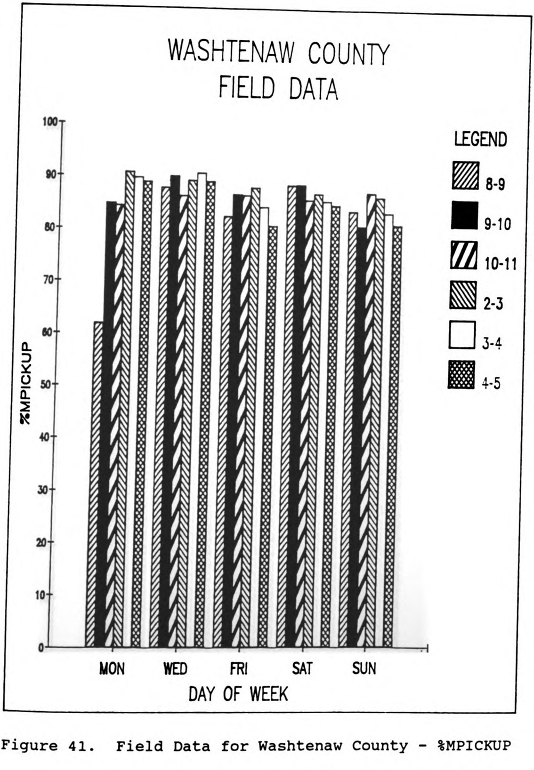

Figure 41. Field Data for Washtenaw County - %MPICKUP

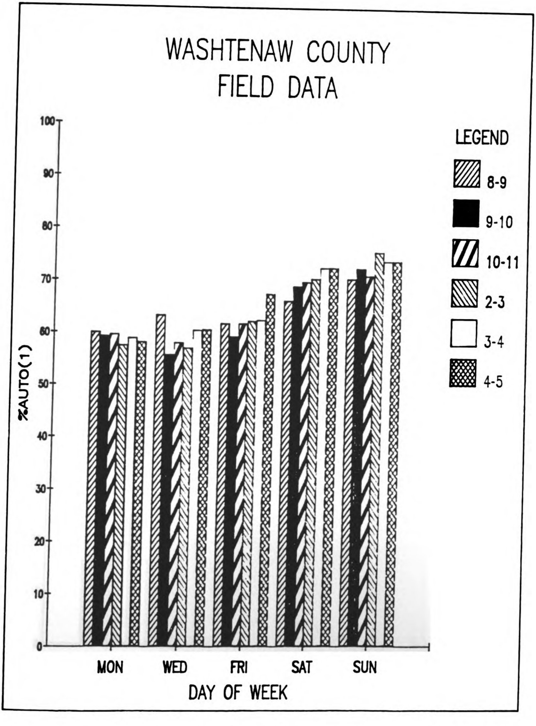

Figure 42. Field Data for Washtenaw County - %AUTO(1)

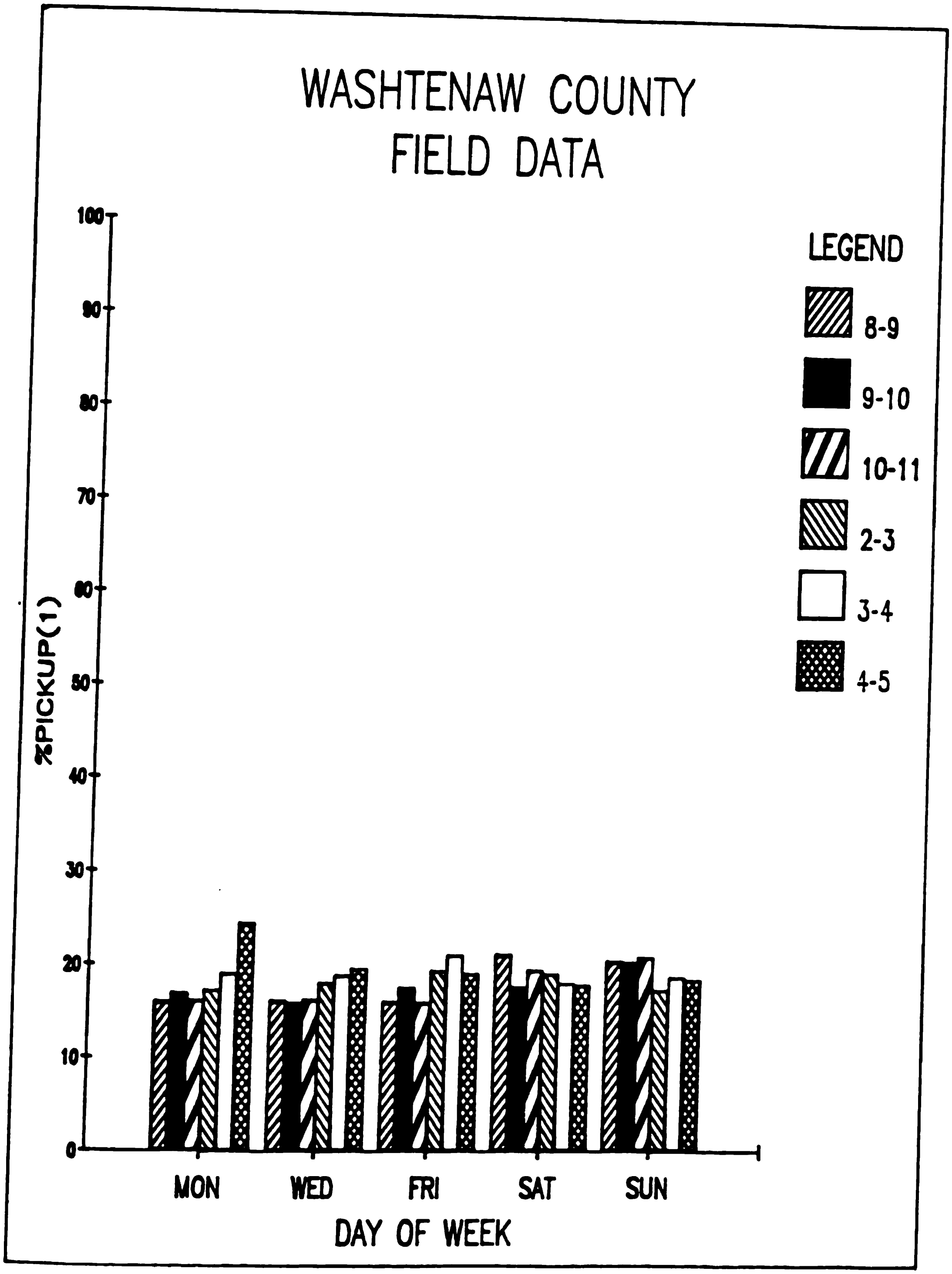

Figure 43. Field Data for Washtenaw County - %PICKUP(1)

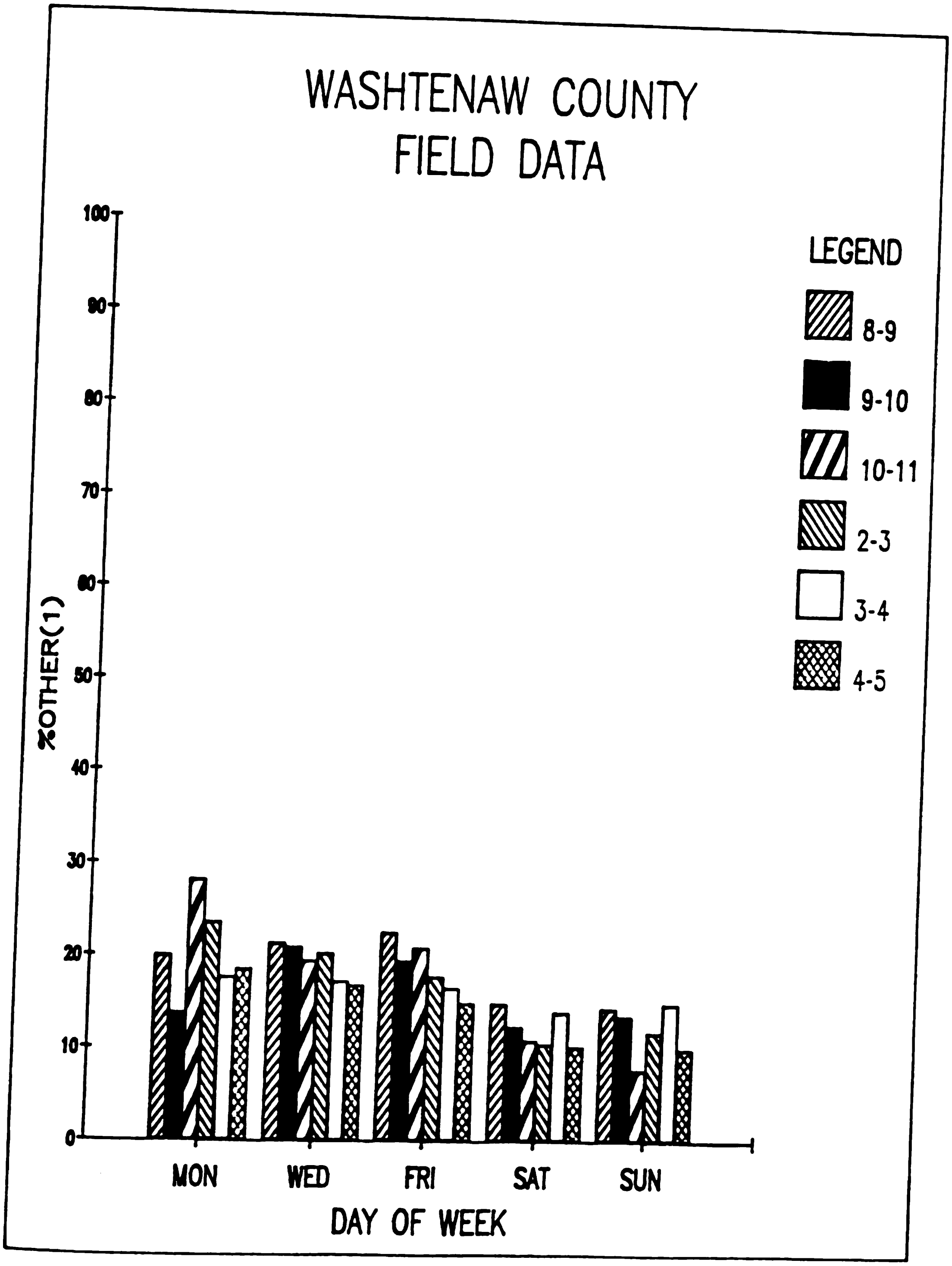

Figure 44. Field Data for Washtenaw County - %OTHER(1)

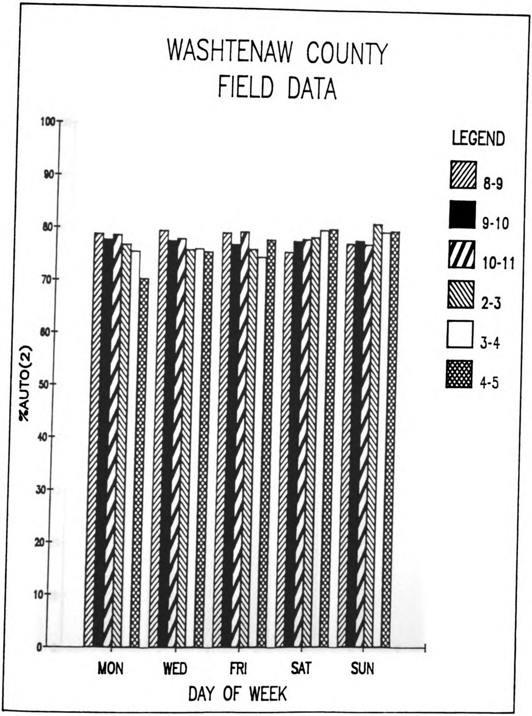

Figure 45. Field Data for Washtenaw County - %AUTO(2)

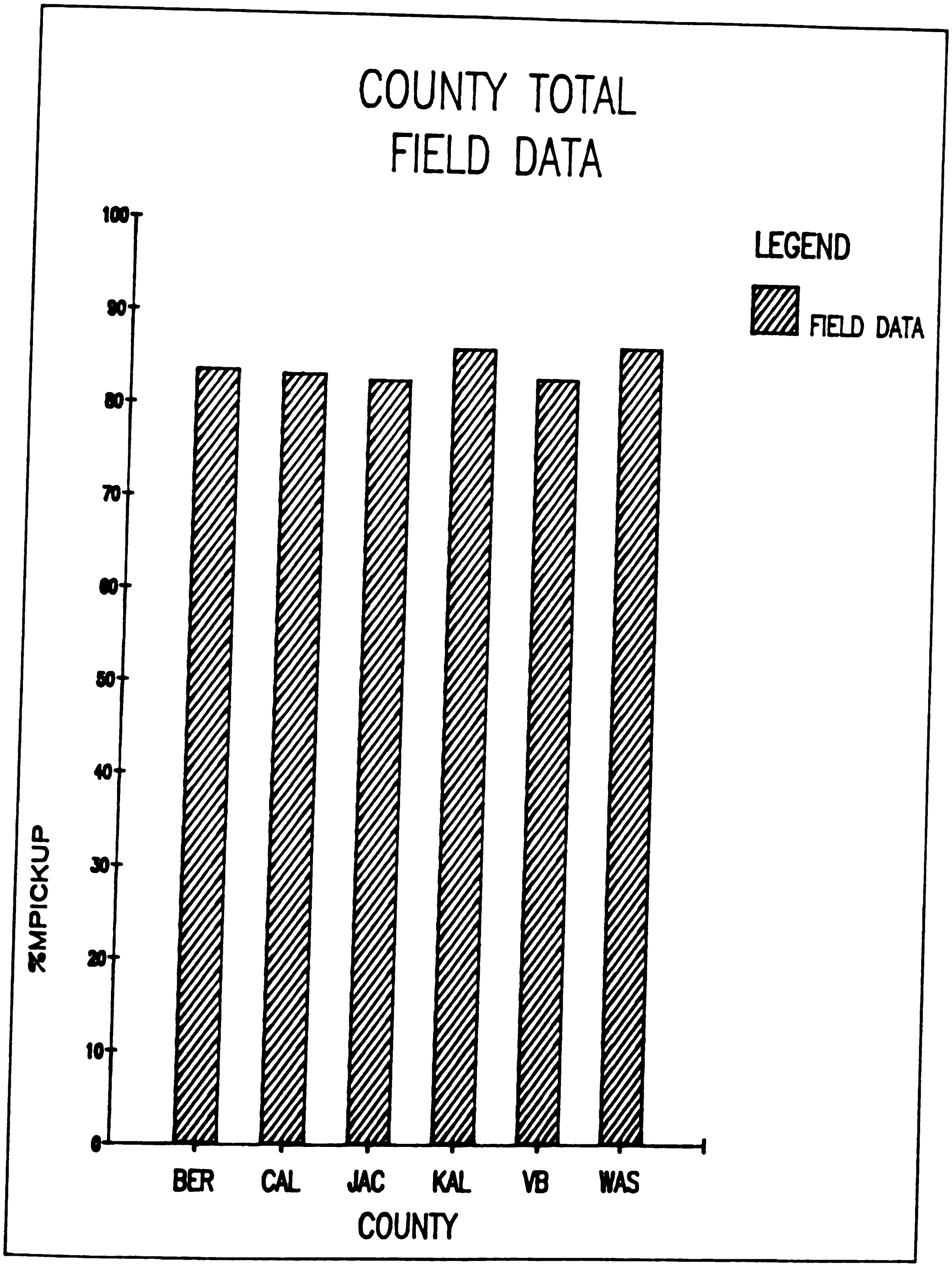

Figure 46. Field Data for %MPICKUP by County

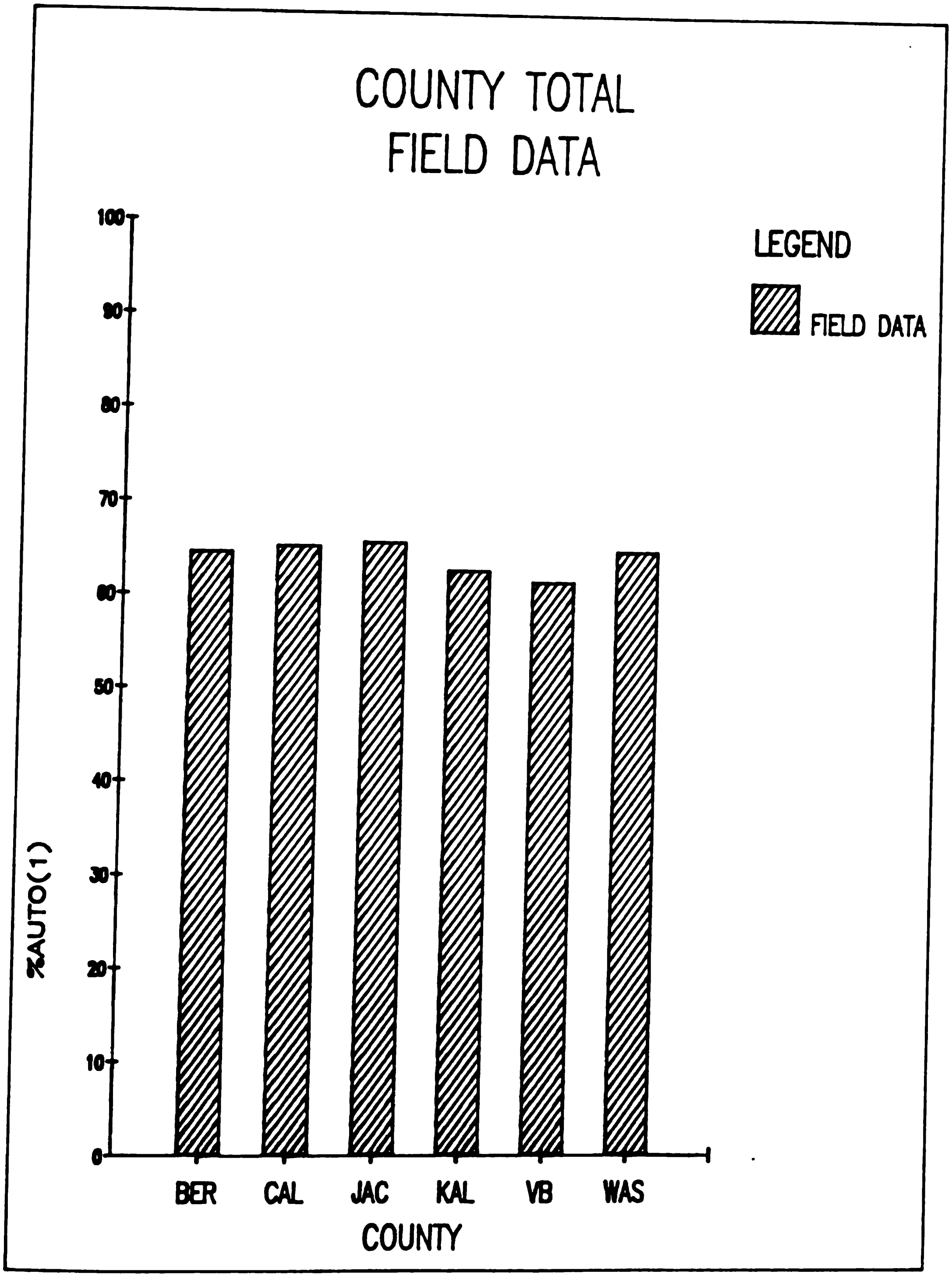

Figure 47. Field Data for %AUTO(1) by County

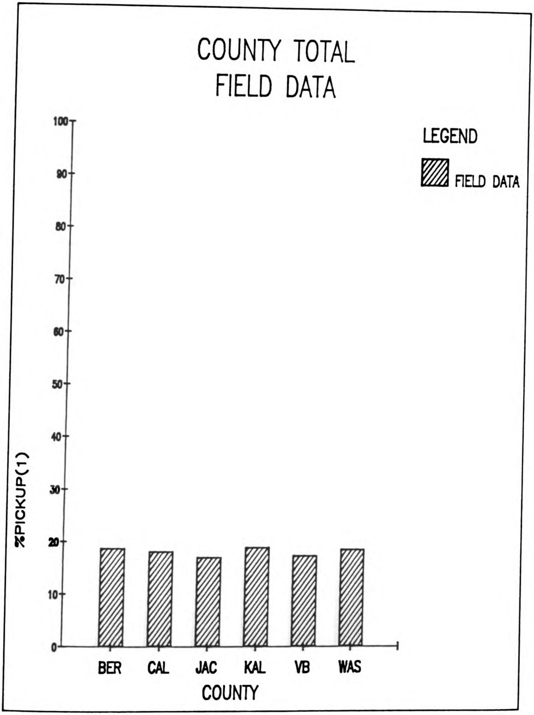

Figure 48. Field Data for %PICKUP(1) by County

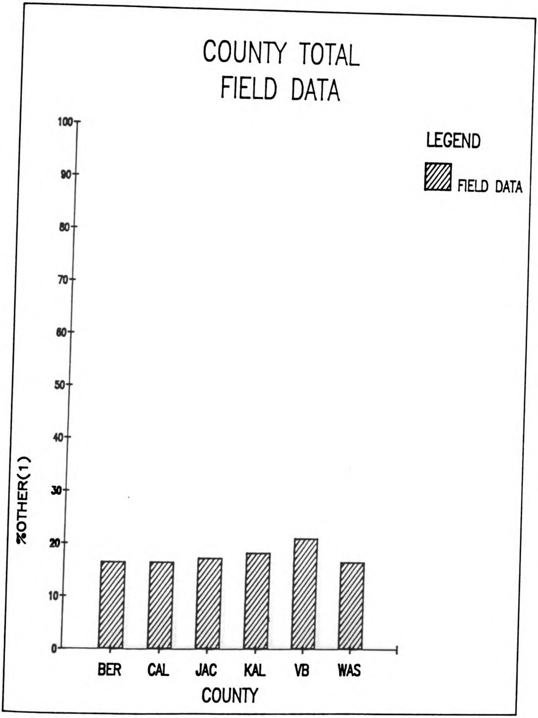

Figure 49. Field Data for %OTHER(1) by County

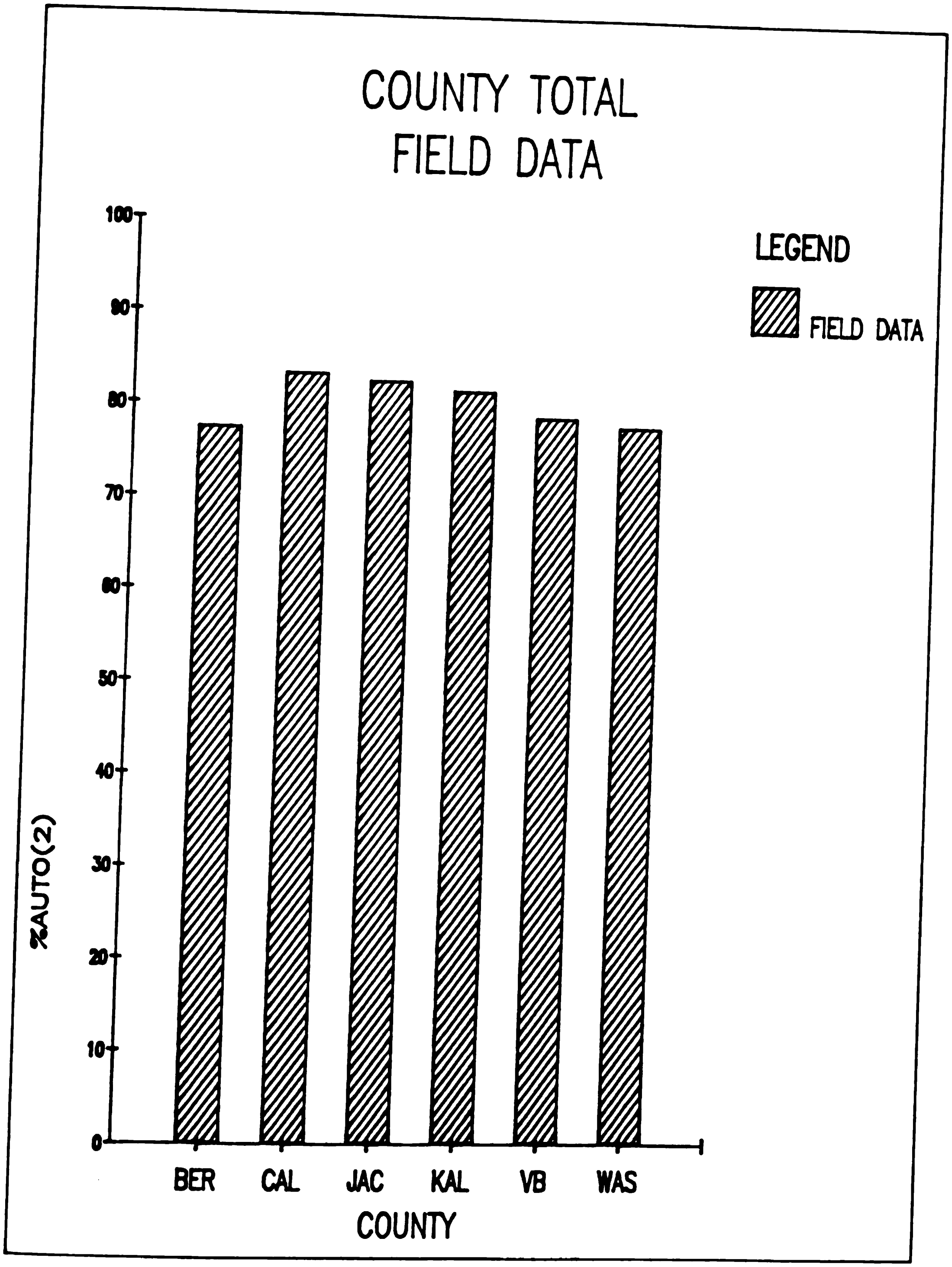

Figure 50. Field Data for %AUTO(2) by County

APPENDIX B

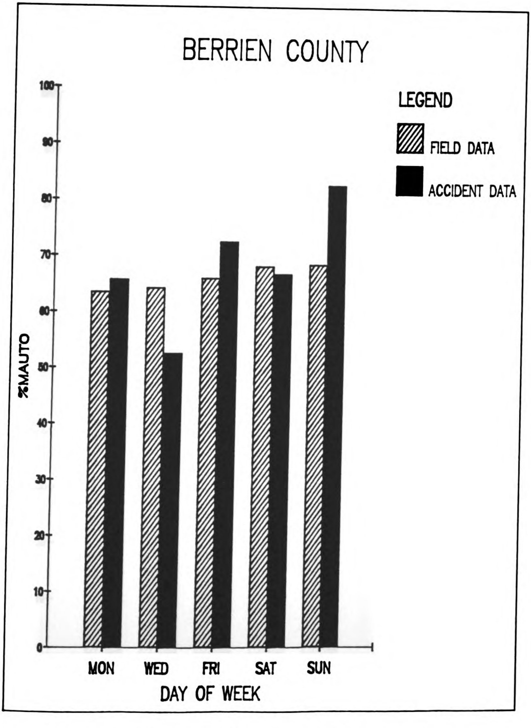

Figure 51. Daily Comparison for Berrien - %MAUTO

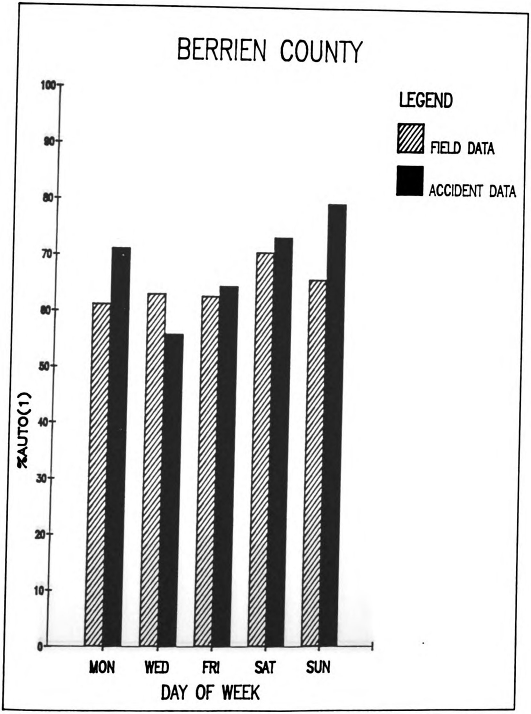

Figure 52. Daily Comparison for Berrien - %AUTO(1)

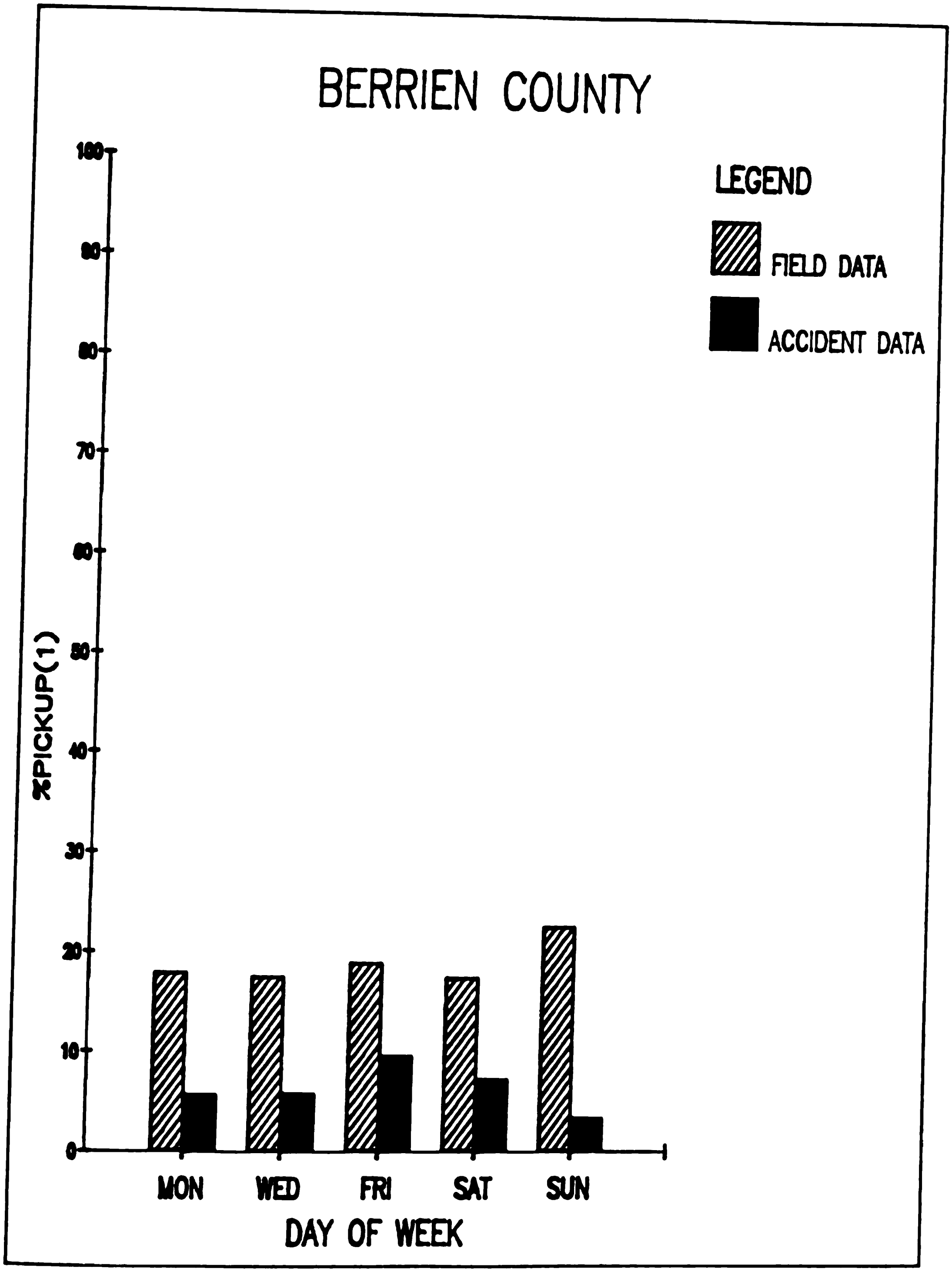

Figure 53. Daily Comparison for Berrien - %PICKUP(1)

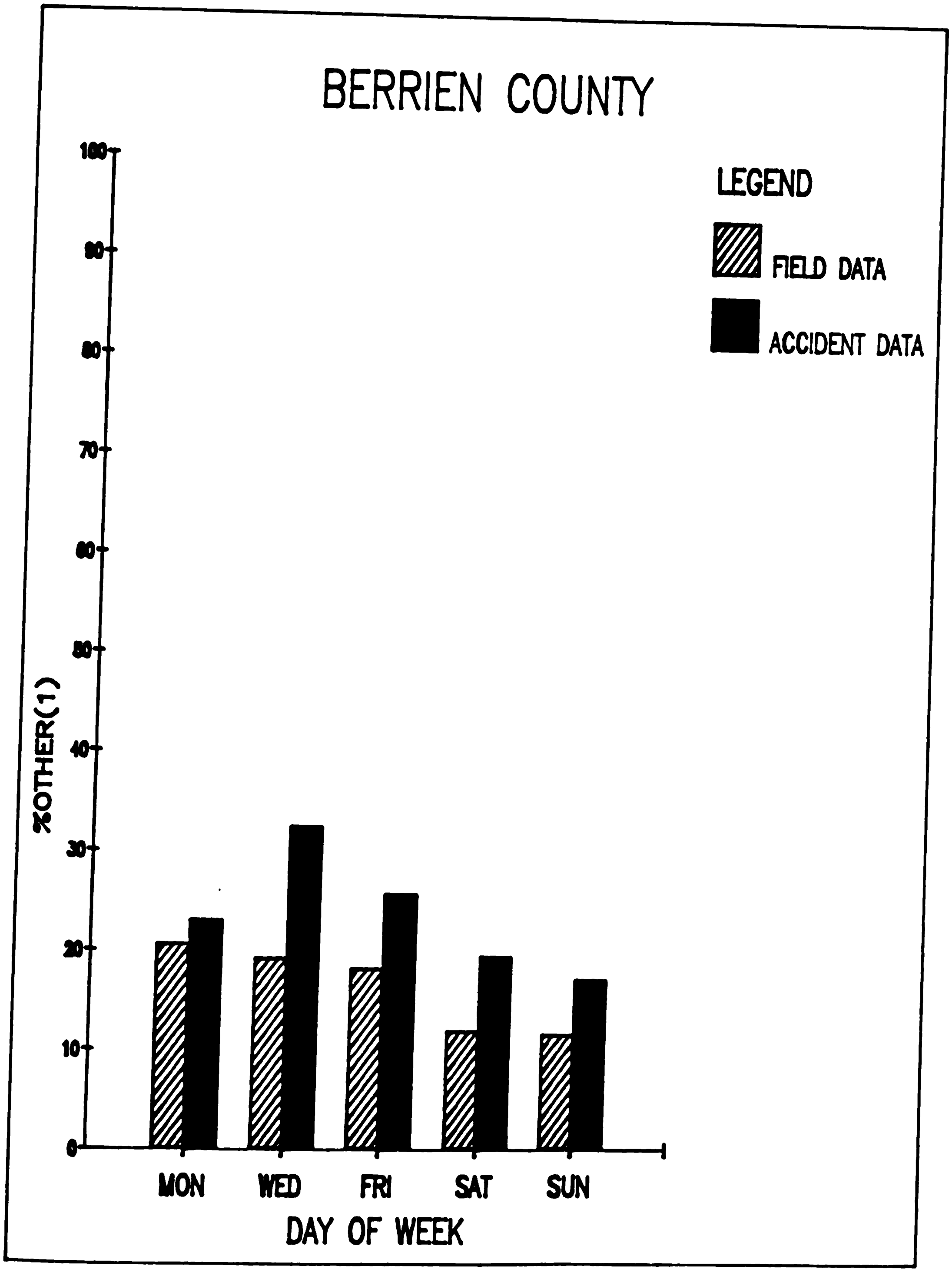

Figure 54. Daily Comparison for Berrien - %OTHER(1)

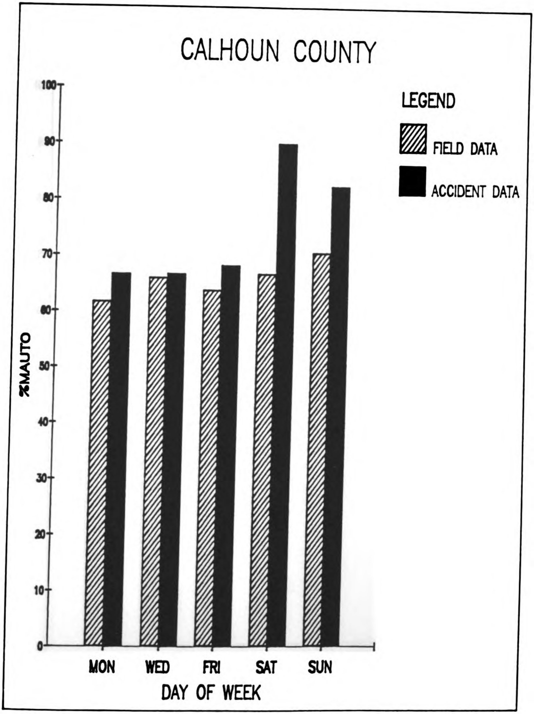

Figure 55. Daily Comparison for Calhoun - %MAUTO

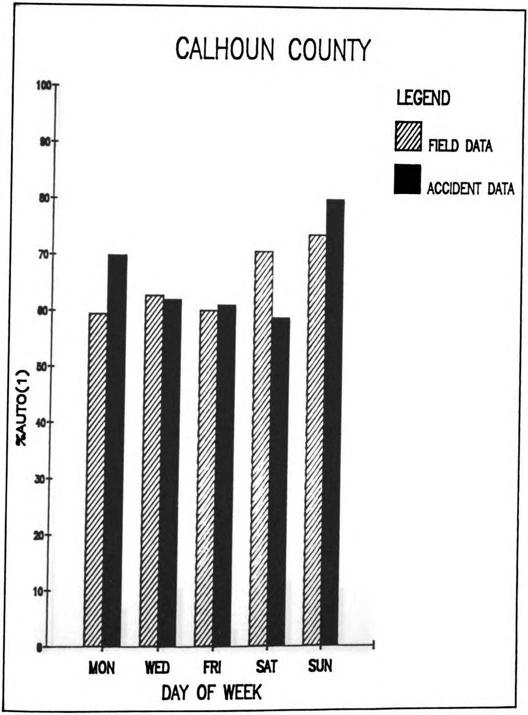

Figure 56. Daily Comparison for Calhoun - %AUTO(1)

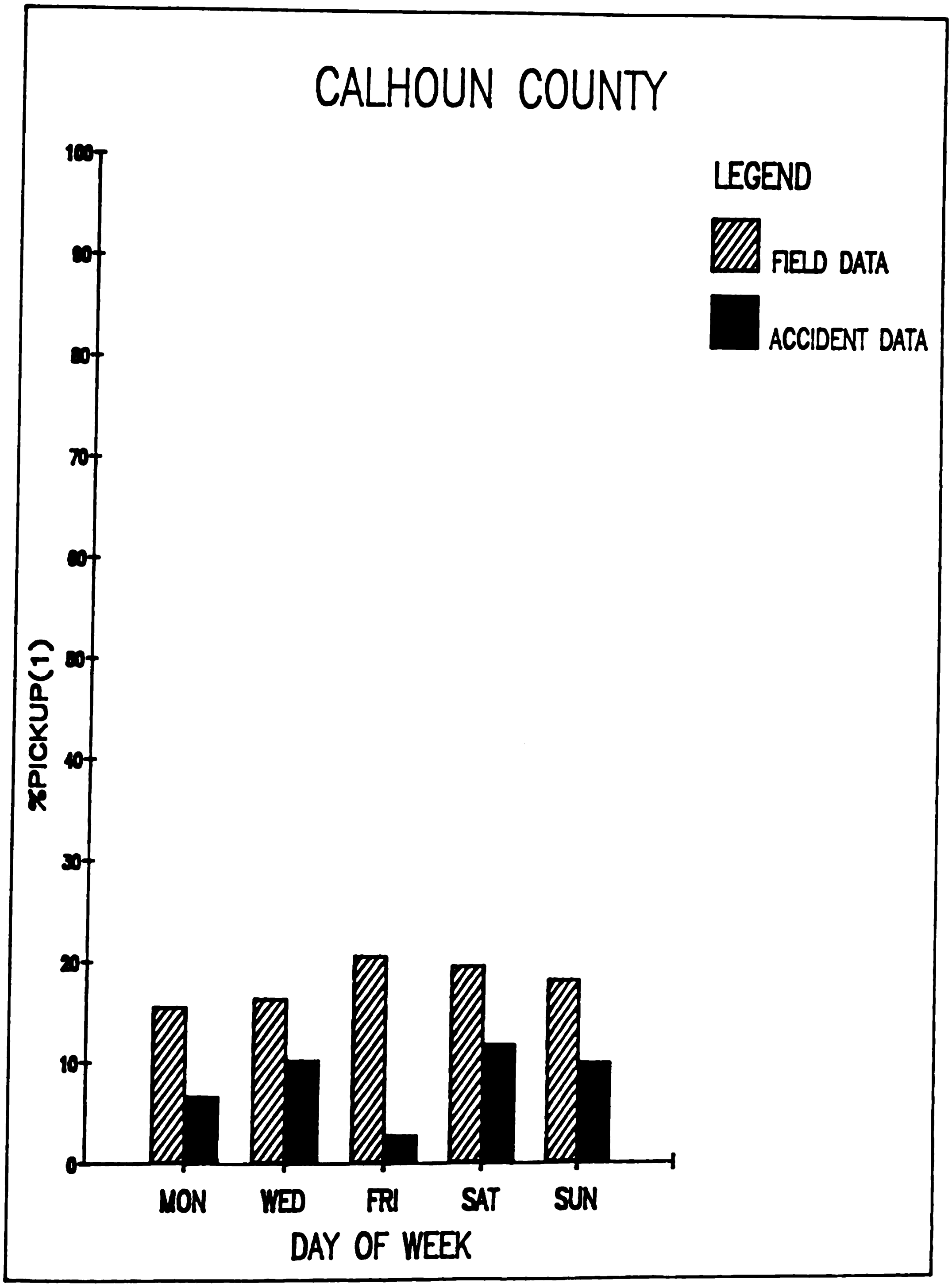

Figure 57. Daily Comparison for Calhoun - %PICKUP(1)

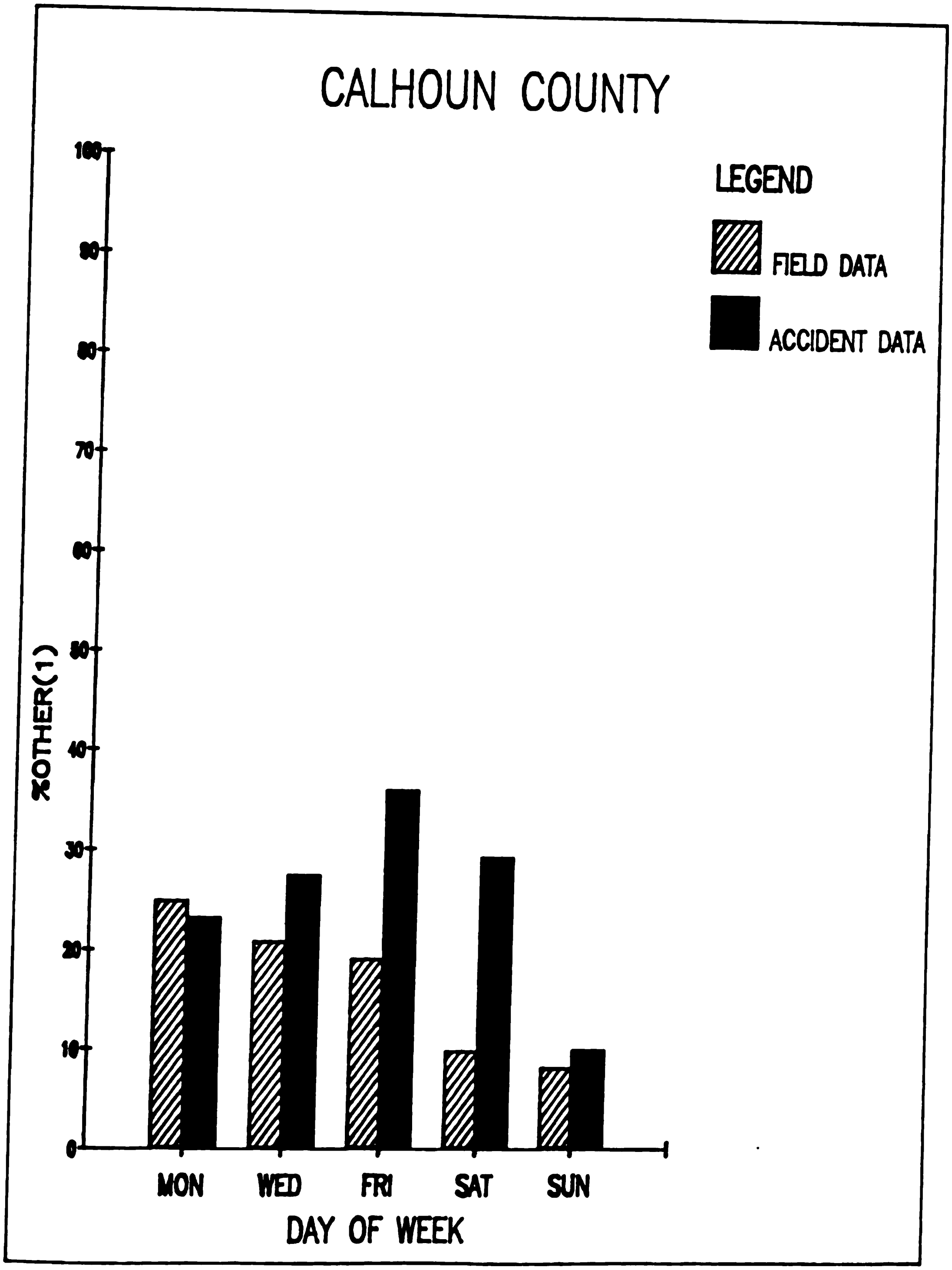

Figure 58. Daily Comparison for Calhoun - %OTHER(1)

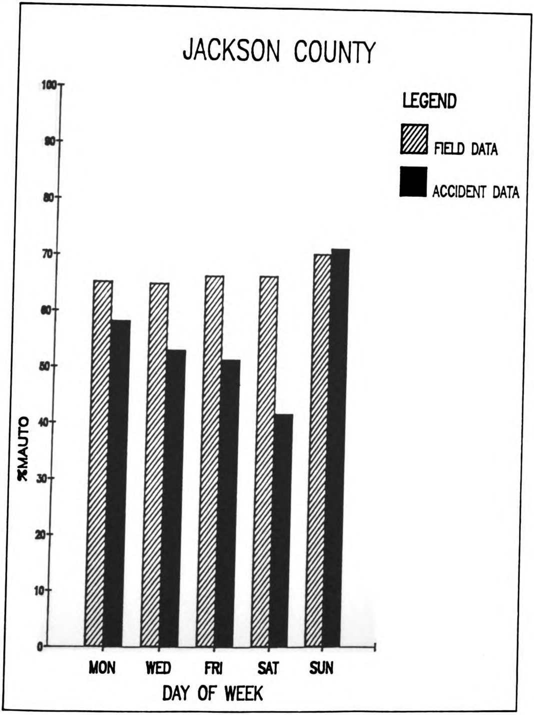

Figure 59. Daily Comparison for Jackson - %MAUTO

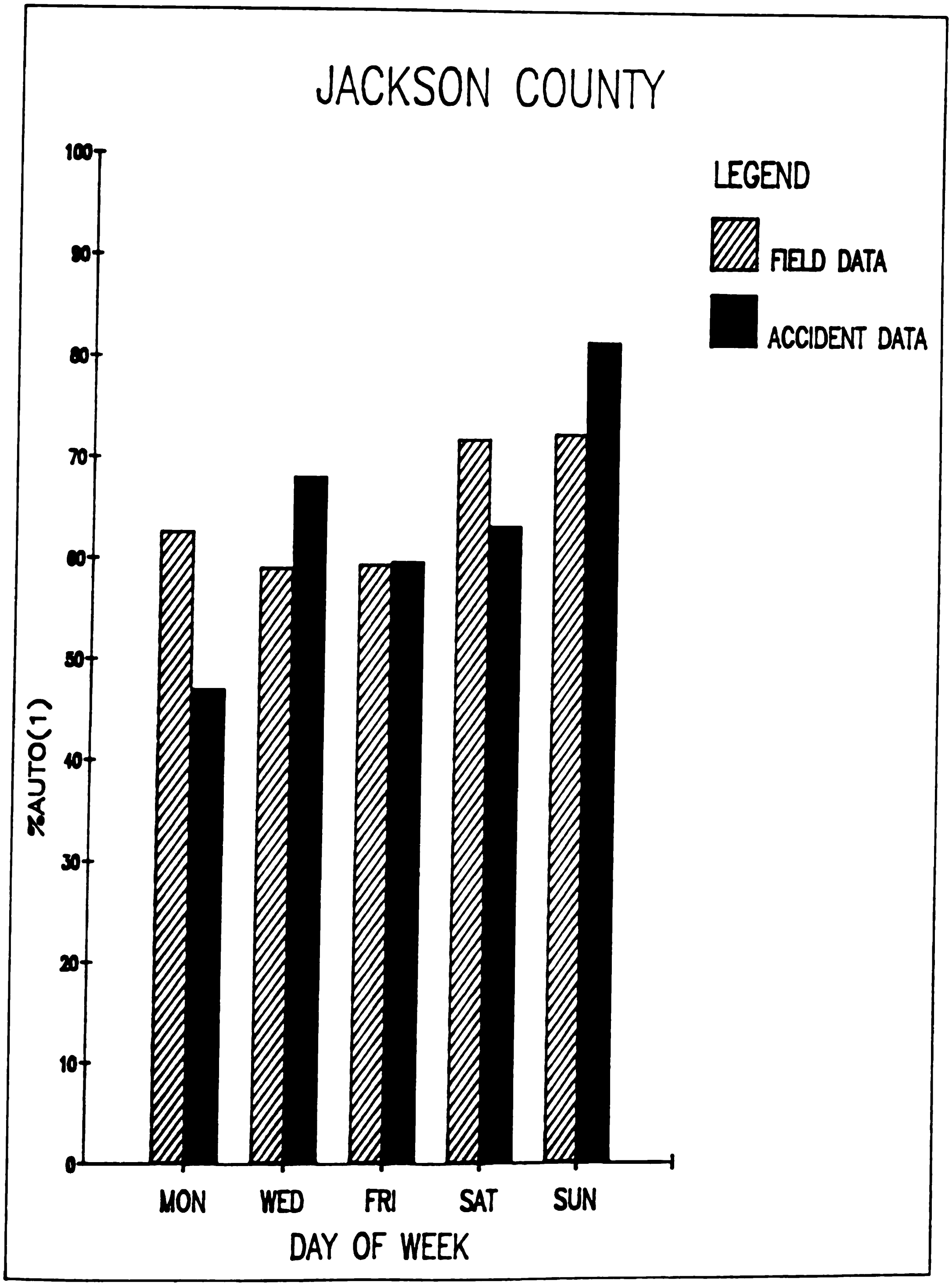

Figure 60. Daily Comparison for Jackson - %AUTO(1)

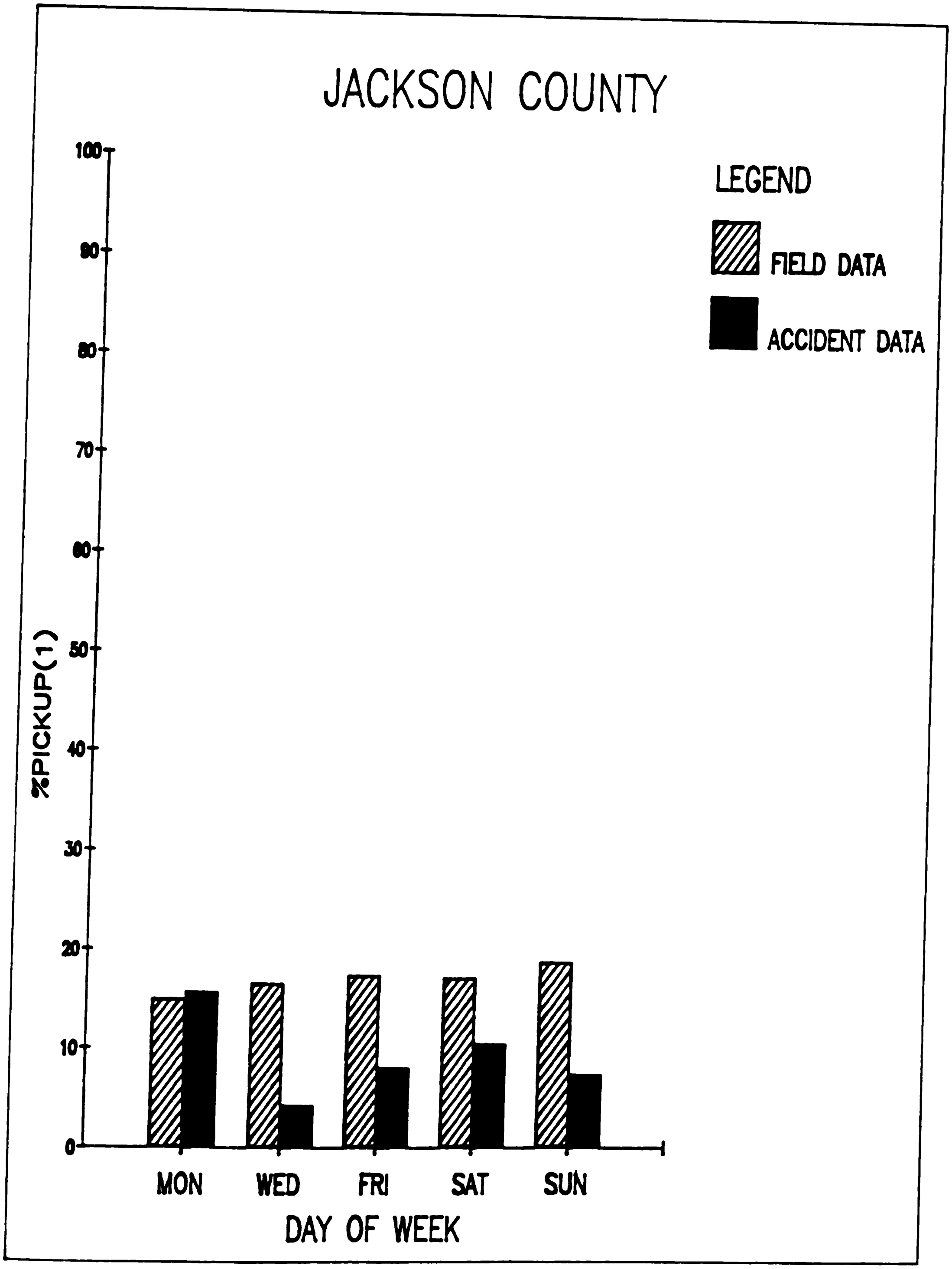

Figure 61. Daily Comparison for Jackson - %PICKUP(1)

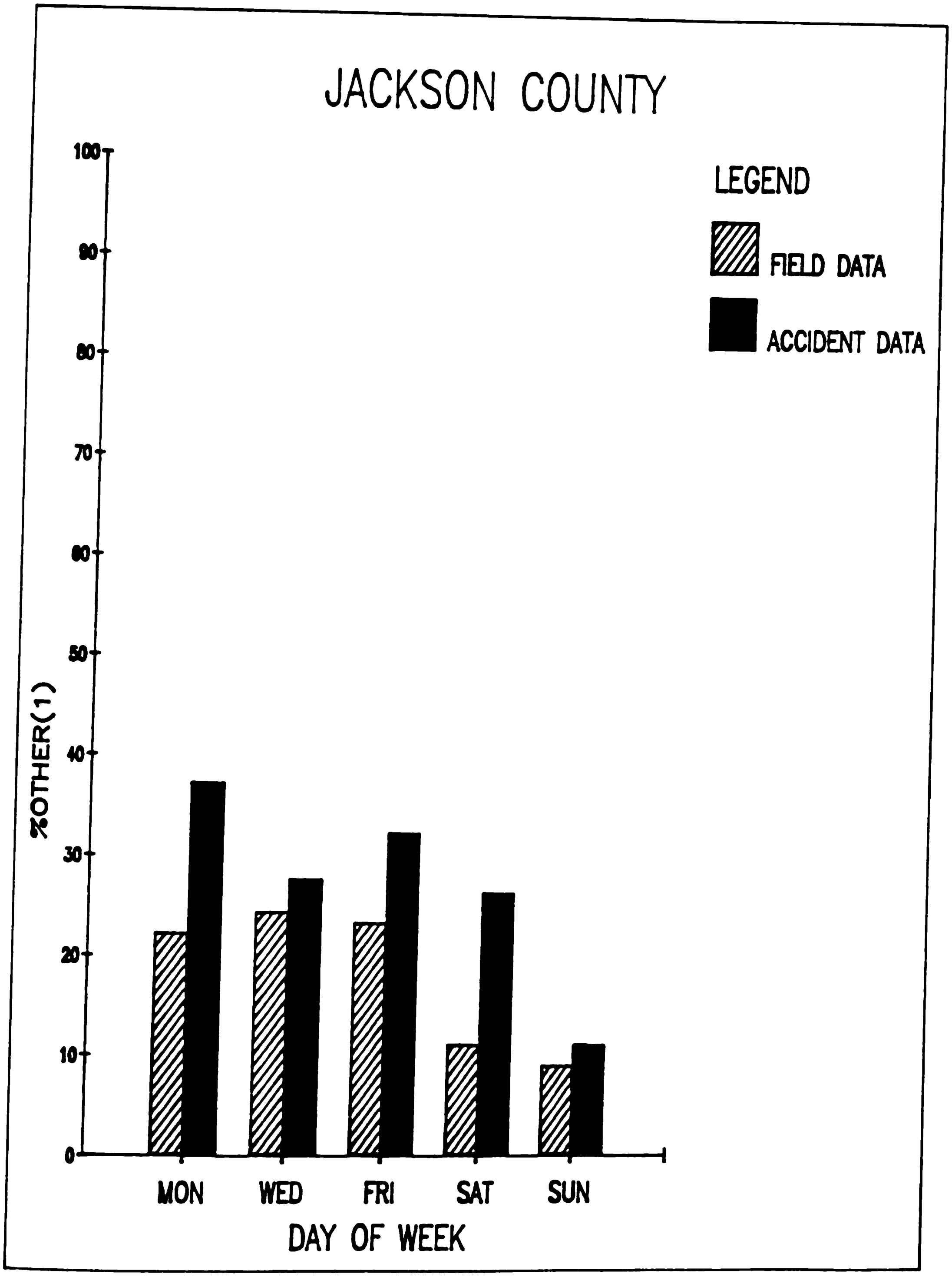

Figure 62. Daily Comparison for Jackson - %OTHER(1)

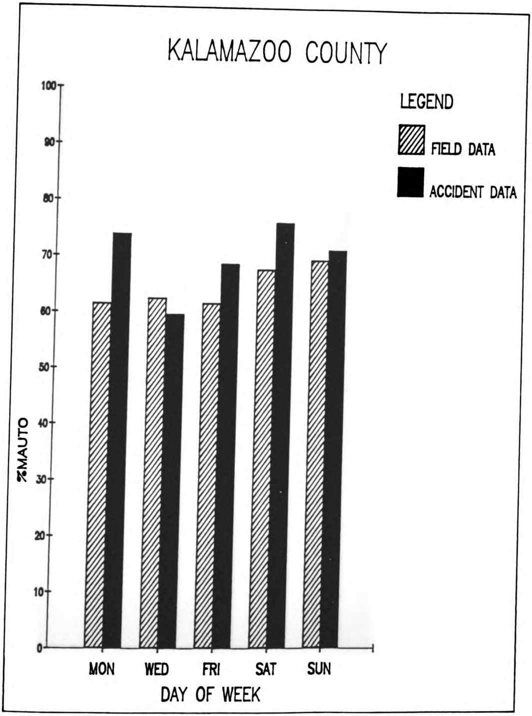

Figure 63. Daily Comparison for Kalamazoo - %MAUTO

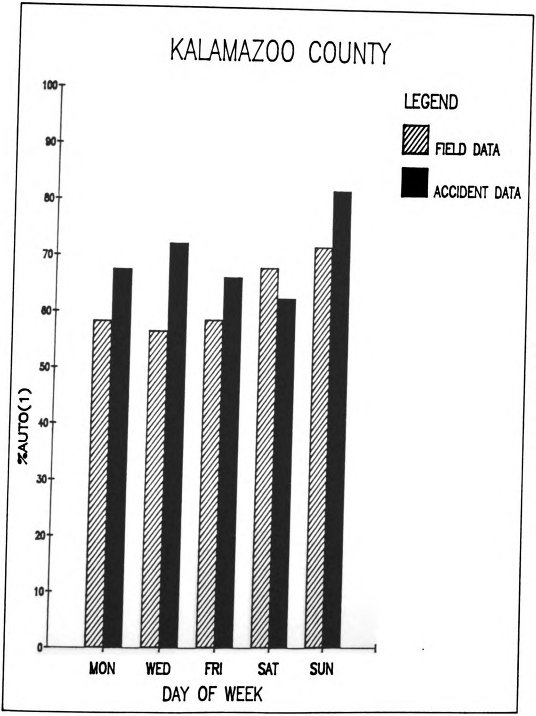

Figure 64. Daily Comparison for Kalamazoo - %AUTO(1)

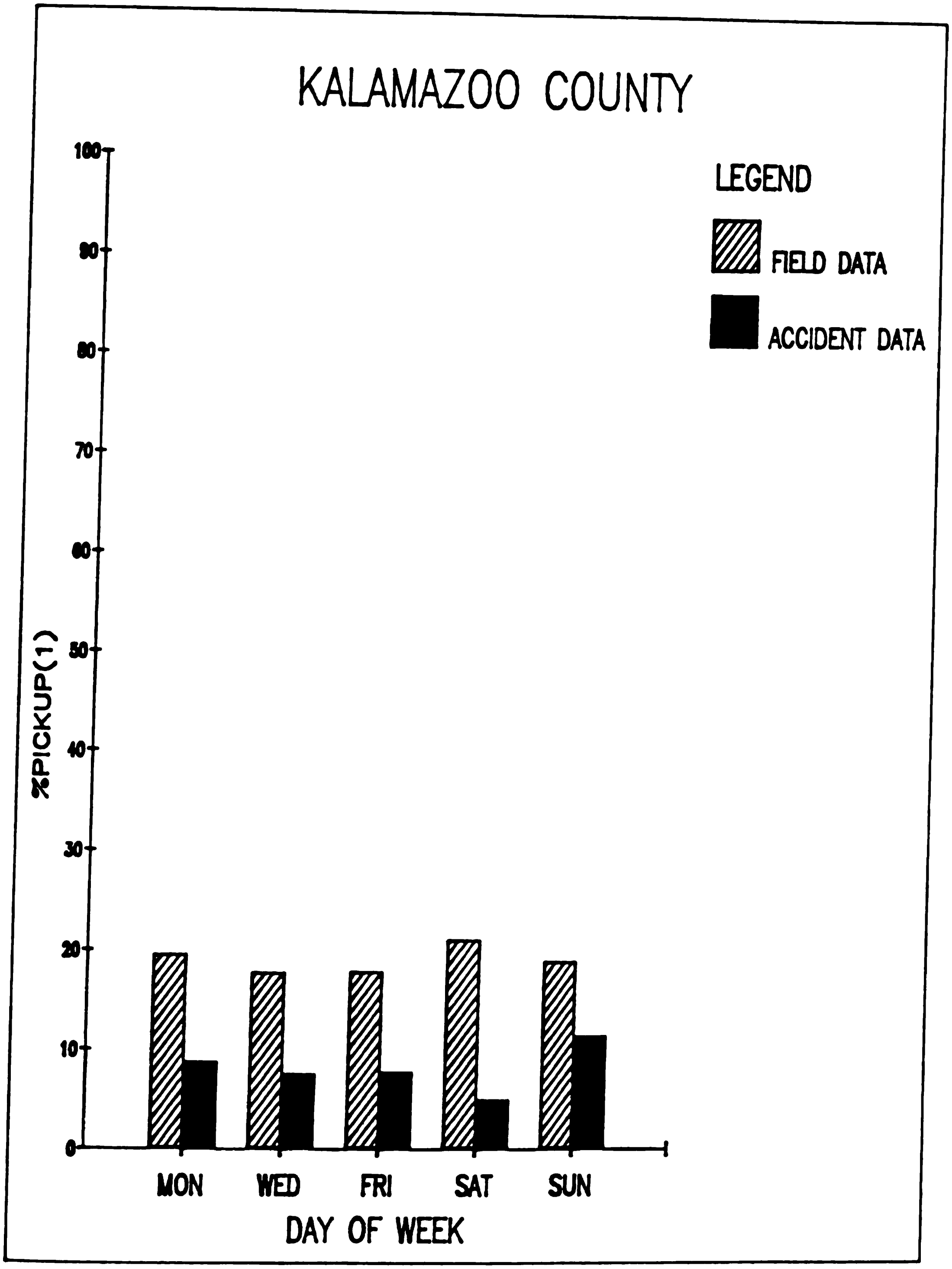

Figure 65. Daily Comparison for Kalamazoo - %PICKUP(1)

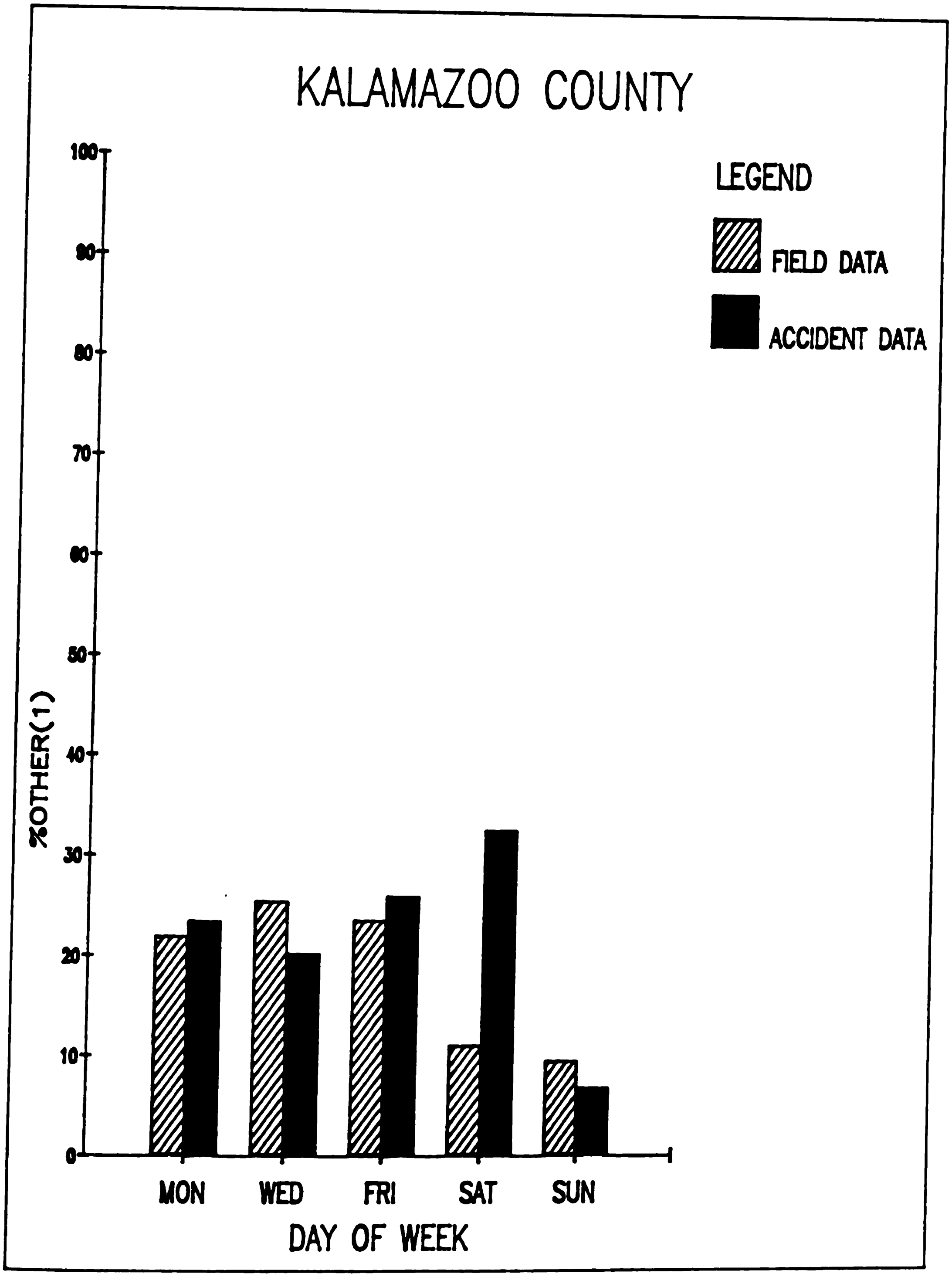

Figure 66. Daily Comparison for Kalamazoo - %OTHER(1)

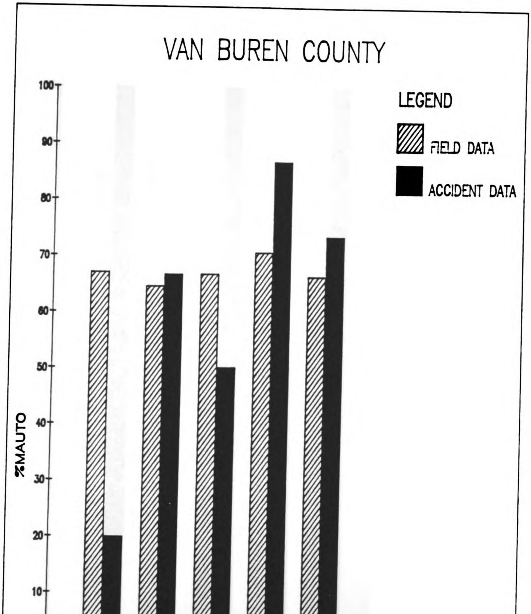

Figure 67. Daily Comparison for Van Buren - %MAUTO

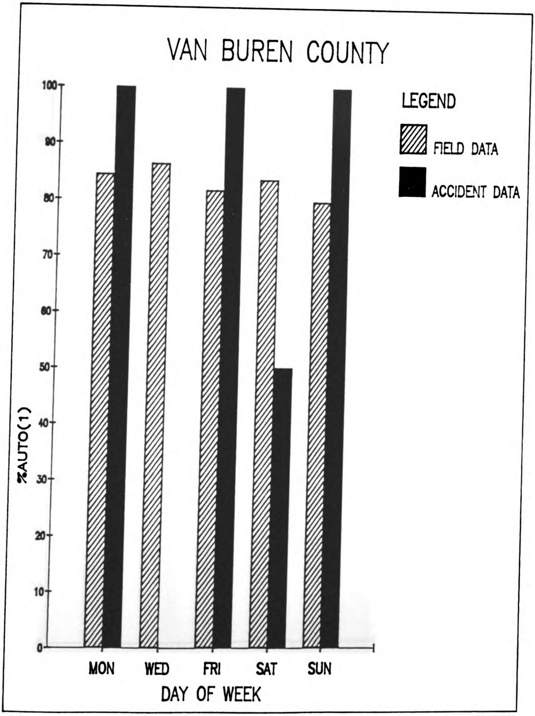

Figure 68. Daily Comparison for Van Buren - %AUTO(1)

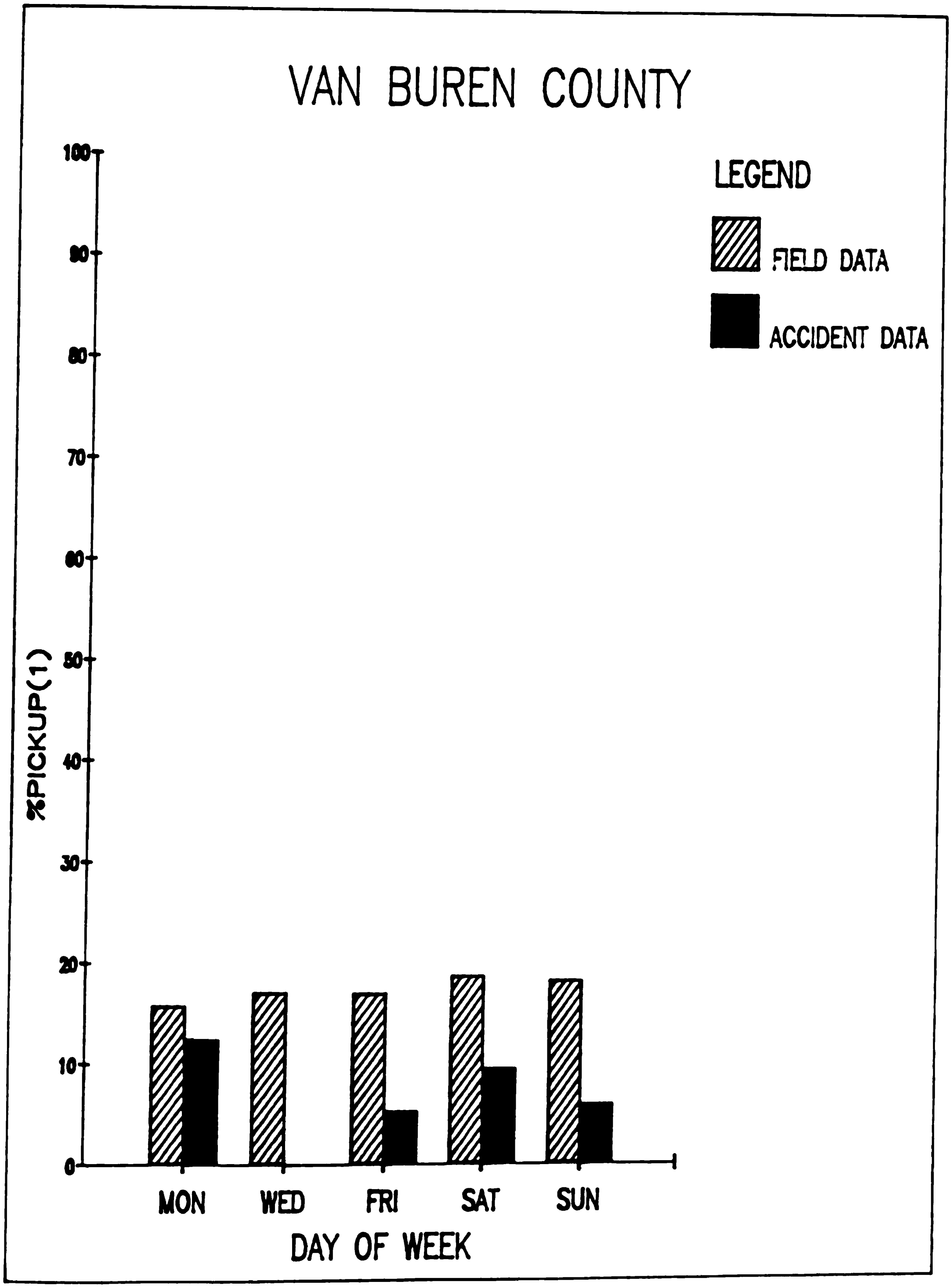

Figure 69. Daily Comparison for Van Buren - %PICKUP(1)

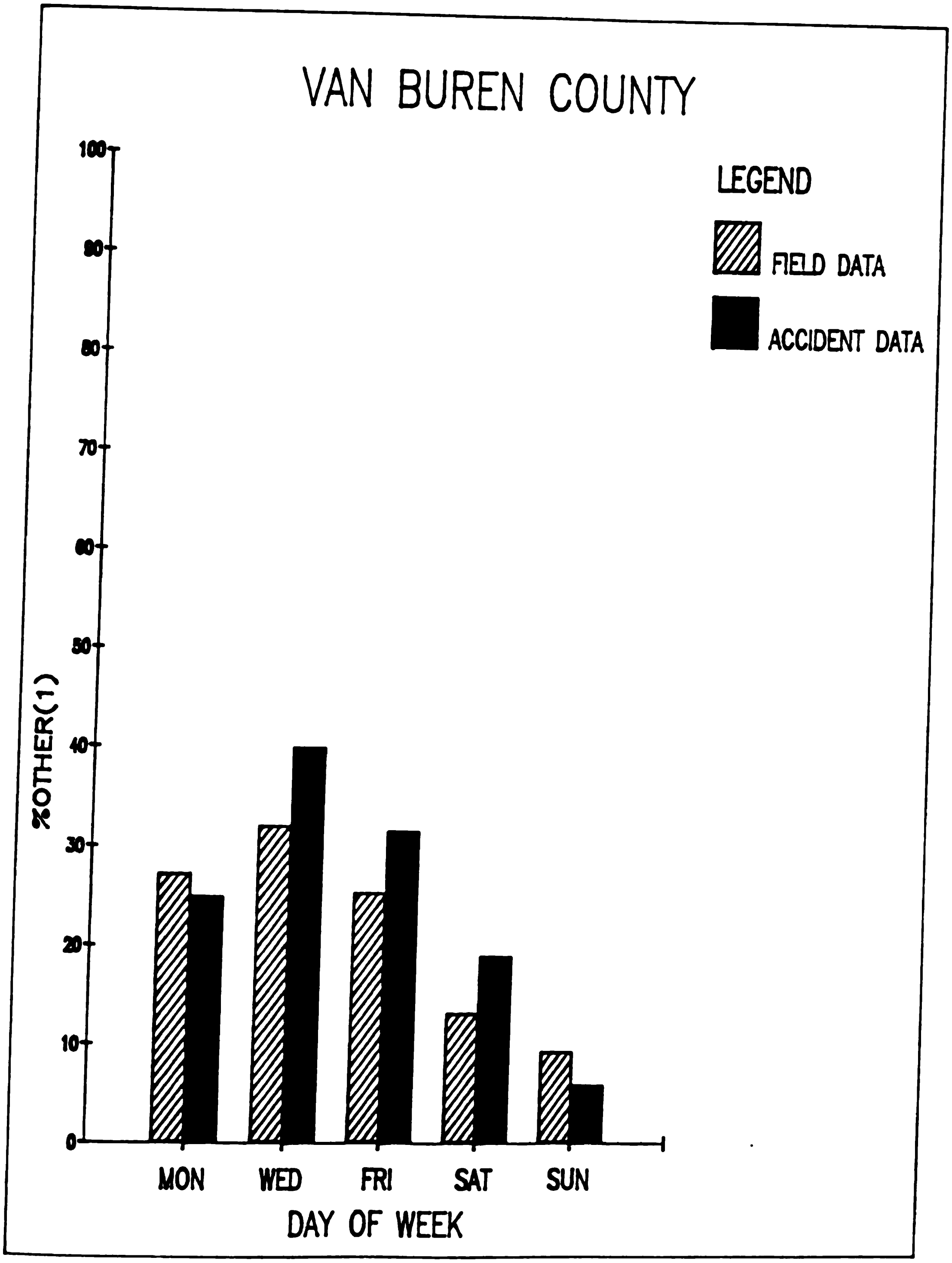

Figure 70. Daily Comparison for Van Buren - %OTHER(1)

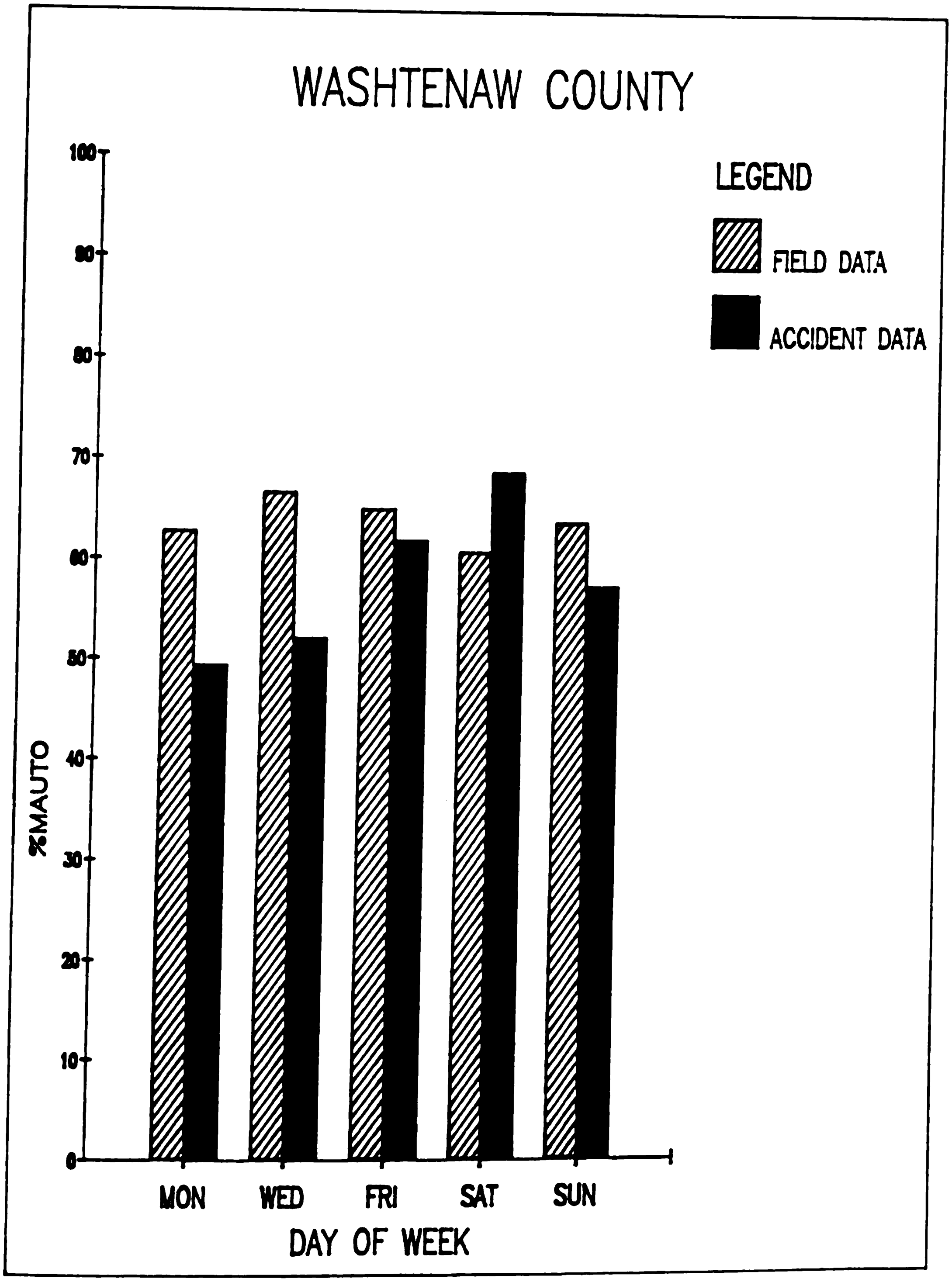

Figure 71. Daily Comparison for Washtenaw - %MAUTO

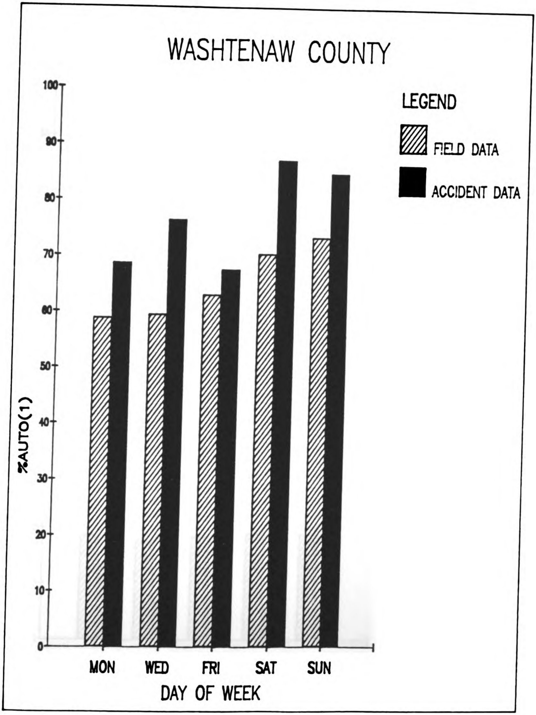

Figure 72. Daily Comparison for Washtenaw - %AUTO(1)

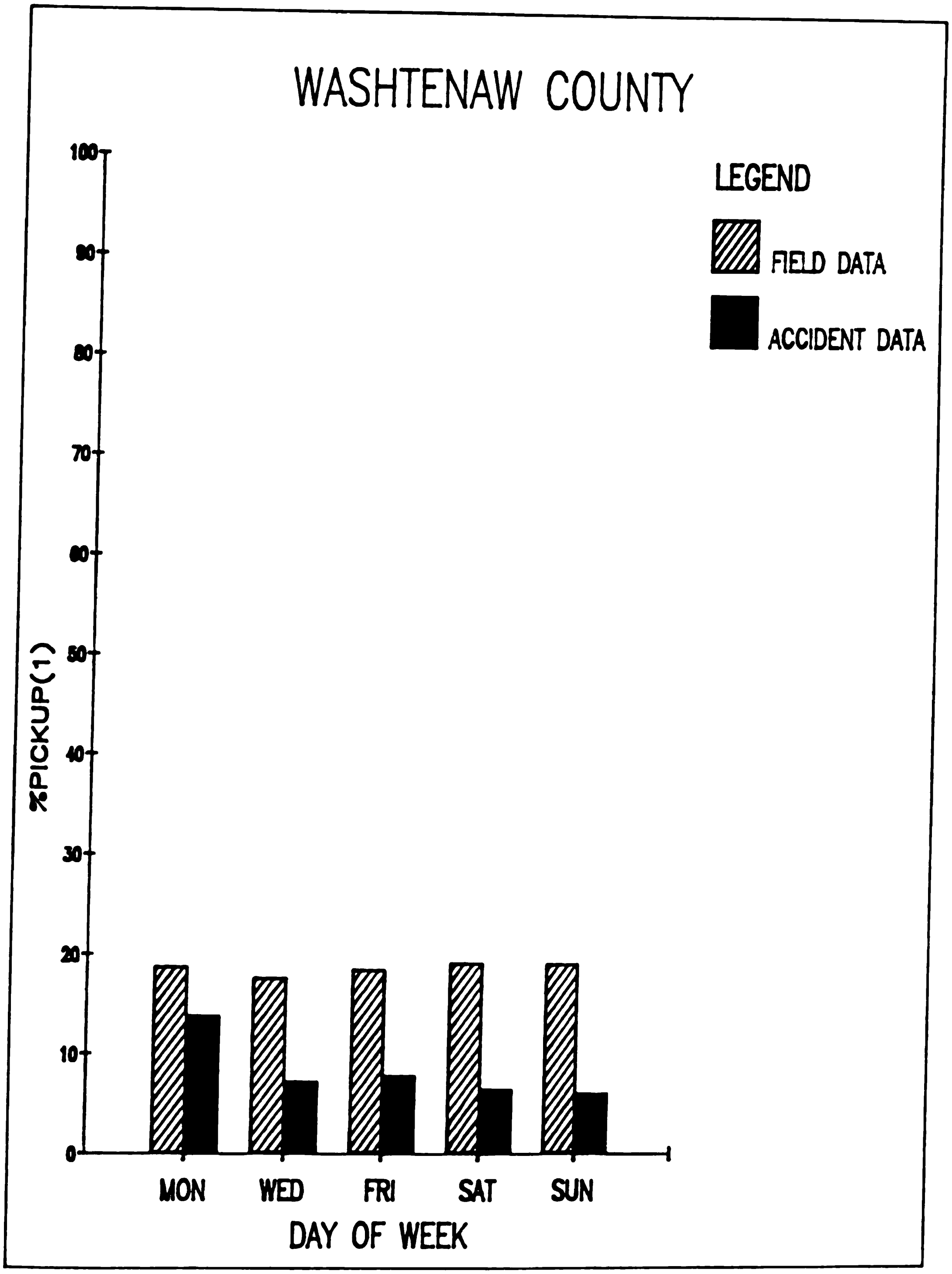

Figure 73. Daily Comparison for Washtenaw - %PICKUP(1)

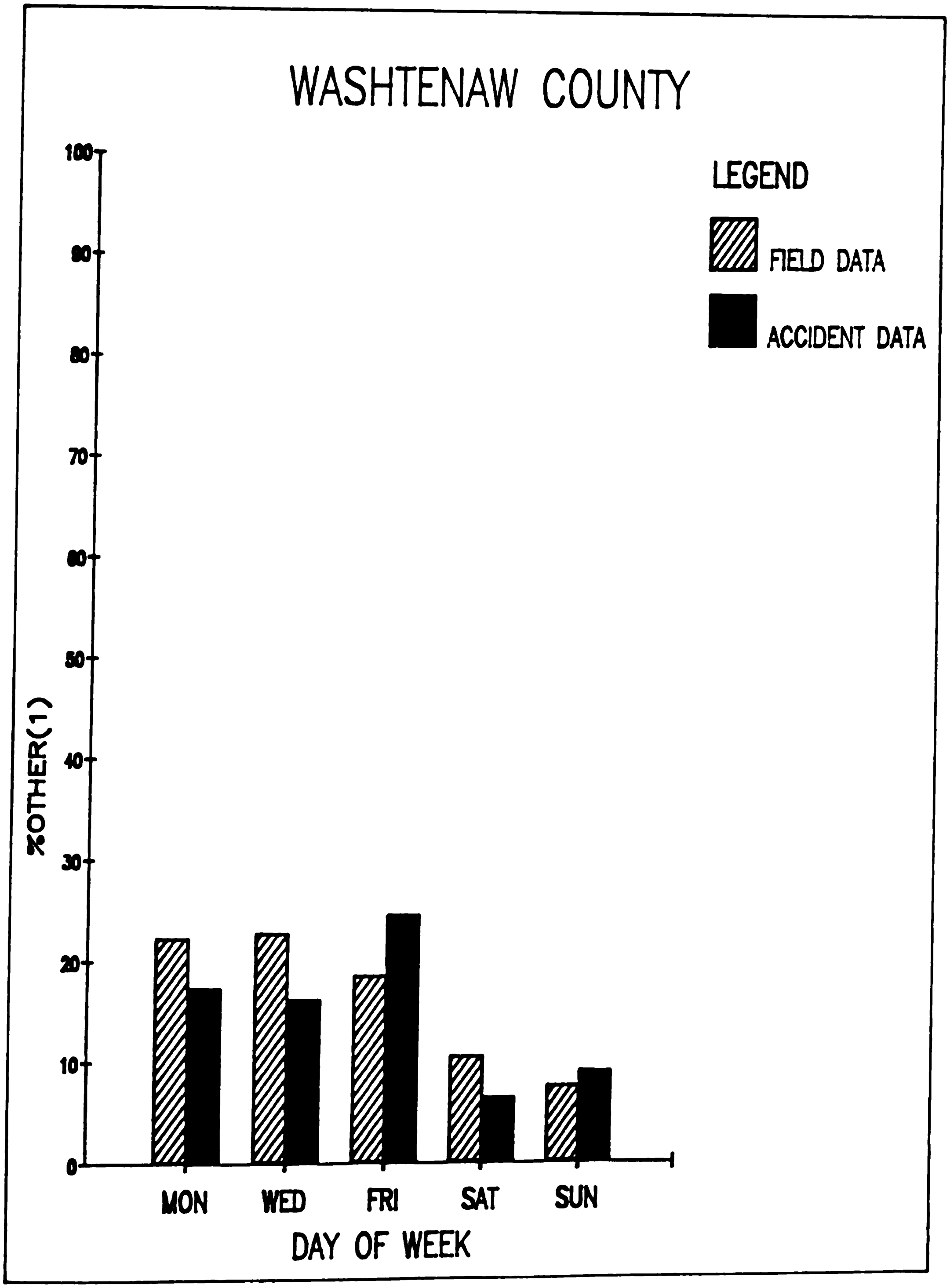

Figure 74. Daily Comparison for Washtenaw - %OTHER(1)

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