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STUDY DESIGN FOR THE EVALUATION OF THE EFFECTIVENESS OF MITERS-TYPE PROJECTS - TECHNICAL REPORT

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Abstract

This Technical Report describes the design phase of a study to evaluate the safety effectiveness of certain minor traffic engineering and road safety projects undertaken in Australia.

A review was made of sources of data available on accidents, traffic flows and minor traffic engineering improvements for road safety implemented in the States of Australia. It was concluded that the major data constraint relates to accident data and that South Australia and Western Australia are presently the only States with data bases in an immediately suitable form for the proposed evaluation.

A methodology was developed and recommended for the evaluation phase, based on the principles of the before-and-after study. Care was given to resolution of problems arising from changes in site exposure and changes in secular trends of accidents.

A trial analysis was conducted for some intersection signalisation projects undertaken in Adelaide in 1975/76. The feasibility of the proposed methodology was demonstrated and some preliminary results established concerning the safety effectiveness of these projects.

NOTE:

This report is to facilitate discussion on the methodology proposed to evaluate MITERS-type projects and its distribution has been limited to the people/organizations directly involved. The views expressed are those of the author(s) and do not necessarily represent those of the Commonwealth Government.

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C Boughton

- TECHNICAL REPORT -

STUDY DESIGN

FOR

EVALUATION OF THE EFFECTIVENESS OF MITERS-TYPE PROJECTS

PREPARED FOR THE

OFFICE OF ROAD SAFETY

DEPARTMENT OF TRANSPORT

by

Graeme Teale
Scott MacLean
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Peter Gipps

of

[illegible]

PREFACE

This Technical report describes the study design for the evaluation of the road safety effectiveness of minor traffic engineering and road safety projects undertaken in Australia. These projects are of the type which would be eligible for Commonwealth funding under the terms of the MITERS program; the program being a discrete part of the *Roads Grants Act, 1974* and the *State Grants (Roads) Act 1977* which together have provided funds to the States for expenditure on road projects for the six year period 1974/75 to 1979/80 inclusive.

In this report, a review is firstly made of sources of data available on accidents, traffic flows and projects implemented in the States of Australia. It is concluded that the major data constraint relates to accident data, and that South Australia and Western Australia are presently the only States with accident data bases of sufficient quality to enable comprehensive statistical evaluation of the effectiveness of projects.

Statistical methods are then developed and recommended for the evaluation phase, based on the principles of the before-and-after study. Care is given to resolution of problems arising from :

- changes in site exposure
- changes in secular trends of accidents
- seasonal factors.

Lastly, a trial analysis is conducted for some South Australia signalisation projects undertaken in Adelaide in 1975/76. The feasibility of the proposed methods is thereby demonstrated and some preliminary results are established concerning the safety effectiveness of the projects.

CONTENTS

CHAPTER 1

INTRODUCTION

1.1 BACKGROUND

This report describes the study design phase of a project to evaluate the effectiveness of MITERS-type projects in Australia[1]. MITERS-type projects are those road safety projects which would qualify for Commonwealth funding under the terms of the MITERS program.[2] The MITERS program is a discrete part of the *Roads Grants Act 1974* and the *State Grants (Roads) Act 1977* which together have provided funds to the States for expenditure on road projects for the six year period 1974/75 to 1979/80 inclusive.[3]

Typical of MITERS-type projects are :

(a) traffic signals;

(b) road signs and pavement marking;

(c) speed control systems;

(d) elimination of intersections on arterial roads, modification of multiple-street intersections, provision of median strips or new or modified traffic islands and roundabouts;

(e) pedestrian crossings (including flood lighting) and the provision of pedestrian safety zones;

(f) localised improvements to street lighting, or new lighting at isolated locations;

(g) bus stopping bays for safety reasons;

(h) turning lanes, channelization and lane marking;

(i) use of slip-base and frangible street lighting poles and sign supports at locations with high accident records;

(j) relocation or protection of roadside objects at hazardous locations;

(k) protection devices at railway crossings;

(l) the adjustment to super-elevation on curves, and improvement of visibility on crests or curves; and

(m) provision of guardrails on embankments, curves and bridge approaches.

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[1] The acronym MITERS stands for minor improvements for traffic engineering and road safety.

[2] Note that this definition does not restrict attention to only those projects which have been so funded. The distinction, rather, is between projects which would qualify, were funding sought, and projects which would not qualify.

[3] For an historical summary of the development of the MITERS program see Nicholas Clark and Associates, *[illegible] and [illegible] Administrative Arrangements for the [illegible] and Road Safety (MITERS) Program*, submitted to Roads Division, Common-[illegible]

1.2 SCOPE OF THE STUDY

The purposes of the study design phase of the study reported here were :

- to assess the feasibility of evaluating the safety effectiveness of MITERS-type projects;
- if the feasibility could be established, to develop a method for an evaluation study;
- to draw up a work program for such an evaluation study.

This report contains a description of the assessment of feasibility, together with details of the recommended procedures for an evaluation study, and a demonstration of the application of the recommended method.

The report is structured in four main chapters which are briefly described below :

Chapter

2 <u>Data requirements and availability</u> - A review and discussion of the sources of data on accidents, traffic flows and projects implemented in the States of Australia.

3 <u>Proposed method for evaluating effectiveness of MITERS-type projects</u> - A brief review of alternative procedures for assessing the effectiveness of road safety projects, followed by the development and presentation of a statistical methodology proposed for the evaluation phase of the study. The chapter is divided into two parts; a verbal discussion followed by an annex with details of statistical and mathematical principles.

4 <u>Trial Analysis</u> - A demonstration, using South Australia data, of the statistical methods proposed in the preceding Chapter. The feasibility of the methods is demonstrated, and some preliminary results are shown concerning the safety effectiveness of some traffic-signal installations in Adelaide.

CHAPTER 2

DATA REQUIREMENTS AND AVAILABILITY

2.1 INTRODUCTION

There are three main classes of data required for the evaluation of road safety projects :

- accident data
- exposure (traffic flow) data
- project description data

In this chapter we review, for each State in turn, the useability of each class of data.

2.2 ACCIDENT DATA

2.2.1 <u>Introduction and Summary</u>

To evaluate properly the effectiveness of MITERS-type projects, access is needed to reports of all, or some consistent subset, of accidents in the region under study. There are several fundamental requirements of these reports :

(i) The reports must be consistent; that is the information collected and stored for each accident should be determined by the same rules and procedures.

(ii) The reports must be free from duplication; that is, information for each accident should be collected and stored once only.

(iii) Information on the location of accidents must be adequate for matching with project locations.

(iv) The reports must be readily accessible, and the information contained in them able to be extracted efficiently and rapidly.

These requirements, particularly the last, indicate that the most appropriate source of accident reports will be the computerised accident data bases maintained in the various States.

In this section these accident data bases are reviewed in the light of the basic requirements stipulated above. The useability of the data bases is examined in terms of such things as the accuracy and details of reports, internal consistency of reports, verification that the same accident is not reported twice, and the ease of extraction of required subsets of the data from the main data base.

The major conclusion is that the accident data bases of only three States are likely to be suitable for use in the evaluation study. And of these three, only two (those of S.A. and W.A.) are immediately suitable. An important qualification is that <u>direct</u> comparison of MITERS-type project effectiveness is not possible between States, because of differences in accident reporting and recording criteria, except on a casualty accident only basis.

2.2.2 Reporting and Recording of Accidents

Table 2.1 shows current legal requirements for reporting of road traffic accidents to police in Australian States. The table demonstrates the variation which exists between States in such requirements. Most States try to exclude the least serious accidents from their records and all have regulations to the effect that property-damage-only accidents resulting in damage less than a minimum amount need not be reported. This minimum amount varies from State to State. For example, in Tasmania no accident which is of a property-damage-only (P.D.O.) nature is required to be reported, whereas in N.S.W. all accidents which result in either personal injury or property damage exceeding $300 must be reported.

In every State, the ability of drivers involved in an accident to estimate the cost of damage must be suspect, as indeed must be their compliance with reporting requirements should their estimate be above the minimum value. Further, the rapidly escalating costs of repairs means that this cut-off point is continually changing in terms of real damage. The net result is that not only do States differ in their reporting requirements, but the proportion of P.D.O. accidents reported within individual States is probably decreasing year by year.[1]

Within each State, too, the distinction between a casualty or non-casualty accident is subject to some uncertainty. In every State of Australia, a road accident fatality is defined to occur when any person is killed outright or dies within 30 days as the result of an accident. However, the police assessment of the existence of injuries forms the basis of the classification of serious, minor, or no injuries. While police reporting of road accident deaths is probably fairly reliable, there may still be a large subjective element inherent in the determination of injury type.

The reporting of accidents by accident type (e.g. right angle, rear end, etc.) is more accurate. There can, however, be considerable delay between the actual accident and the filling out of the relevant form and this may result in the recording of incorrect information. In addition police may rely heavily on the often conflicting reports of those involved in the accident, and a proportion of accidents may not be attended by police at all. Bagley[2] instances the case of South Australia, where in 1971, 43% of total reported casualty accidents were not attended by police.

Table 2.2 shows the practice of each State in recording into its data base details of an accident which has been reported. Again, the variation between States is apparent.

It is clear that in respect of both reporting and recording of road accidents, the States differ sufficiently in the extent that a complete comparative (i.e. between States) evaluation of the effectiveness of MITERS-type projects (i.e. an evaluation using both P.D.O. and casualty accident data) is not possible. However, provided that the data bases are otherwise suitable, there seems no impediment to conducting interstate comparisons using casualty accident data.

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[1] except where regular upwards revisions of this minimum value are made.

[2] F. D. Bagley, *The Role of the Australian Bureau of Statistics*, Road Accident Information Seminar, Canberra 1974.

TABLE 2.1: Legal Requirements for Road Traffic Reporting in Australian States

State	Road Traffic Accidents Required to be Reported to Police
New South Wales	(a) All accidents involving personal injury
	(b) All accidents where aggregate property damage exceeds $300. ($50 prior to July 1977)
Victoria	(a) All accidents involving personal injury
	(b) All accidents where there is property damage or an animal is injured and the owner or owner's representative is not present (since 1970)
Queensland	(a) All accidents involving personal injury
	(b) All accidents where aggregate property damage exceeds $1,000. ($300 prior to Oct 78: $100 prior to Jan 1976)
South Australia	(a) All accidents involving personal injury and/or injury to an animal
	(b) All accidents where aggregate property damage exceeds $100 ($50 prior to 1975)
Western Australia	(a) All accidents involving personal injury
	(b) All accidents where aggregate property damage exceeds $100, or where property damage level is in dispute, or where all interested parties are not present (since 1969; increase in level under consideration)
Tasmania	(a) All accidents involving personal injury

Source : State road and traffic safety authorities

TABLE 2.2: Practice for Road Traffic Accident Recording in Australian States

State	Road Traffic Accidents Recorded
New South Wales	Reported accidents involving injury or where at least one vehicle required towing away (all reported accidents prior to July 1975)
Victoria	All reported accidents involving injury and some reported P.D.O. accidents (a) where PDO exceeds $100 or an injury is sustained
Queensland	All reported accidents
South Australia	All reported accidents for which location is positively identified and PDO exceeds $100 or an injury is sustained
Western Australia	All reported accidents where PDO exceeds $100 or an injury is sustained
Tasmania	All reported accidents

(a) Note: mainly those P.D.O. accidents where police think litigation is likely.

Source : State road and traffic safety authorities.

Within individual States reporting requirements and recording practice do not in themselves suggest that an evaluation of MITERS-type project effectiveness will not be possible. To confirm this conclusion, it is necessary to examine more closely the accident data base maintained by each State.

2.2.3 N.S.W. Data Base

The N.S.W. accident data system changed substantially on July 1, 1975. Since that date more detailed data has been held on recorded accidents, although the criteria for recording accidents have been made more stringent. The only crashes now required to be recorded in the computer data bank are those where at least one vehicle requires towing away or there is a reported casualty. Written reports are, however, still held on other reported accidents, and a manual count of these would allow an estimate to be made of the ratio of reported to recorded accidents if this were desired.

Much effort is expended by TARU[1] to ensure that double recording does not occur and that reporting is consistent. The steps in both of these directions are considered as thorough as practically possible, and that virtually the only errors in the data held are due to errors in the original police report which do not cause inconsistencies. From a practical point of view, these errors must be accepted. Fortunately, they may be regarded as random errors and may be allowed for statistically. Further comments on this point are made in assessment of the S.A. data base.

Within the data base, the data are held in the form of sequential files (on tape) covering three month periods. The sequencing is effectively by time of arrival of the report. TARU does have programs to re-sort the data in various ways and regularly produces printed reports. Unfortunately, these are not permanently held on tape, although the programs may be re-run to reproduce these reports on tape for further research on the data. The two reports that would be of most use in evaluation of MITERS-type projects are :

1. Listing of accidents into each of :
   - (a) Local Government Area (numeric)
   - (b) Town or Suburb (alphabetic)
   - (c) Street Name "
   - (d) Type of Street "
   - (e) Identifying Object "

2. Listing of accidents on classified urban roads sorted by :
   - (a) Route Number
   - (b) Section Number
   - (c) Date of Accident
   - (d) Time of day of accident

This latter listing would provide a convenient data base for evaluation of projects on classified urban roads. However, the Sections are not firm subdivisions and further manual coding of position within Sections, using other information in the records, would be necessary. Nevertheless the ability to initially sort into Sections does reduce the amount of manual coding necessary.

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[1] TARU = Traffic Accident Research Unit, responsible for maintaining the N.S.W. road accident data base.

For non-classified or country roads, however, the problem is not as simple. The identification of a location (street names for example) as reported by the attending policeman, may not be simple. For example, in a special survey[1] of accidents on the Pacific Highway it was reported that 20% of the sites listed in records could be identified.

To summarise, it is believed that the data available in N.S.W. would require manual sorting after initial computer sorting for use in the proposed evaluation. Data manipulation for sites on main urban roads should be straightforward. Manipulation of data for other sites may be expensive in terms of professional time and computer resources.

2.2.4 South Australian Data Base

The South Australian accident data base has recently been re-organised. Within the metropolitan area each intersection or link has a unique ten digit identification code. This code is so constructed that whole subsets of locations which may be of interest (such as several adjoining sections of a main route) may be easily accessed as a group.

In rural areas all roads are divided into sections each of which has a unique code. This subdivision into sections is currently being refined to create smaller sections to allow finer classification of locations. All accident data for the last ten years is currently being sorted into one large sequential file which will be permanently held on disc. This file will be sequenced firstly on location code and secondly on date. This file could easily be converted into a random access file which would allow very rapid access to the complete accident record at any given location or group of locations.

The South Australian system requires the positive identification of the site of each reported accident before recording, and this is worthy of comment, especially in view of the experiences of some users of data from other States were the reported location is simply recorded. The S.A. Highways Department[2] has been coding sites regularly for nearly ten years, and during that period through liaison with the police has built up much knowledge of local naming conventions, etc. It is now claimed that little difficulty is encountered in coding of sites though initially it had posed many problems.

The records within the file are quite compact. A library of functions is maintained which allows easy access to the information within the file. Theoretically this information is available on line, though in practice due to the size of the accident file, retrieval is usually only carried out in batch.

Considerable care is taken in the coding and identification of accidents to ensure that the coding of accidents is accurate and consistent and that accidents are not doubly recorded after double reporting.

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[1] Peter Casey and Associates, *A Study of the Relationship between Accidents and Road Conditions in Rural Areas*, DMR (1979)

[2] Responsible for maintenance of the accident data base.

The procedures cannot of course detect inaccuracies in the original police reports that form the source of documents for the system. A recent report[1] has concluded that these errors are significant for PDO accident reports. There seems no reason why these inaccuracies cannot be adequately treated statistically as random errors. It is believed that property-damage-only accident records form useful statistical data despite the inaccuracies present in the corresponding reports.

In summary, the organisation of the S.A. data base will allow the extraction of the data necessary for an evaluation of MITERS-type projects. Discussions with Highway Department officers confirm that adequate data are available from the data base from January 1972 onwards.

2.2.5 West Australian Data Base

The W.A. accident data base has recently been upgraded. All accident reports from January 1976 onwards have been placed in a random access disc file which is part of a larger data base. This larger data base contains not only accident records, but also, amongst other things, information on traffic flows, road conditions, road layout and control devices (including installation and maintenance dates). The files containing these different classes of data are all keyed in a similar way allowing easy cross reference. This allows on-line access to a full set of information for a selected site. Information on a whole series of sites is available with little programming effort and with a minimum usage of computer time.

In the W.A. data base, each location is referred to by an unique code. The coding system is very similar to that employed in S.A. It is based on the assignment of an unique number to each road, rather than by the use of map co-ordinates as in the Victorian system. The chief difference between the W.A. and S.A. system is in the coding of between-intersection locations. In W.A. these are located by listing the distance from the starting point of the road, rather than by division of the roads into sections (as in country S.A.) or by listing the two end-points of the link (as is done in Adelaide).

Problems of identification of reported accident sites are largely overcome by the experience built up in regular operations. Similarly, through regular operations and the use of the editing routines, the coded reports are made consistent. These routines cover all the checks that are practical.

In summary, an integrated data base is available in W.A. which might well be taken as a benchmark for data organisation. The system allows ready access to any information required for an evaluation of MITERS-type projects.

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[1] P.G.Pak-Poy and Associates, Appraisal of Existing Accident Reporting and Recording Procedures in S.A. Report to O.R.S. (to be published)

2.2.6 Victorian Data Base

Little formal documentation is available on the accident data base maintained by the Road Safety and Traffic Authority. The following comments are based on personal discussions with members of ROSTA and articles written by ROSTA staff members (see Sach[1]).

One problem with the use of Victorian accident data for the proposed evaluation arises from practice in the reporting of accidents. This is beyond the control of ROSTA and cannot be tailored to meet their needs alone. Of course, problems stemming from the reporting system as required by State regulations are not unique to Victoria. However, for the type of data required for the current study, the weaknesses in the data arising from the reporting procedures in Victoria are more severe than in some of the other States.

For every reported accident, a 512 Accident Report Form is completed by police. This preliminary advice form establishes that an accident has been reported and that further information may follow. A copy of relevant information on the 512 form is contained on the 512A form and this is forwarded to ROSTA. ROSTA consider, however, that the 512A form does not contain sufficient information to locate accident sites accurately and so do not code up this information into their computer files.

Reported accidents involving injury are recorded on a 513 form by police and a 513A form (which duplicates relevant information) is sent to ROSTA. Information contained on the 513A form is coded into the computer files. Property-damage-only accidents need only be reported if the owners or owners' representatives were not present, and in this case, the police are required to complete at least a 512 form. In other cases, either at the instigation of the involved parties or of the police, the accident may be reported and could be recorded on a 513 form as well.

The net effect is that only a small proportion of property-damage-only accidents are coded. In 1975 in Victoria 55% of recorded accidents involved injuries. By comparison, for the same year in N.S.W. 38% of recorded accidents involved injury, while only 13% of reported accidents involved injury. The majority of this difference must be attributed to the differences in reporting practice or recording procedures. For the proposed evaluation study, it is important that data on minor accidents be available if at all possible.

No check is made of whether two separate police reports of the one accident have been coded, but this is not considered a problem by ROSTA. Checks are run of the internal consistency of the coded information. Items causing inconsistencies are checked with the reporting policeman.

In Victoria, the accident locations are recorded on a grid system during the recording exercise. The grid is based on the MELWAY street directory in the metropolitan area and on an assortment of maps in country areas.

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[1] [illegible] Sach, *The* [illegible] *Accident Location System*, ARRB Proc. Vol. 6, 197[illegible].

As maps are redrawn or new maps become available the grid references of some locations may change. While this does pose problems in obtaining a site history over a long period, these are not insuperable as pointers have been placed in the file. However, the use of a rectangular grid means that to obtain accident data for a particular location (e.g. an intersection), some manual searching is required. Further, easy access is not available to the history of a whole road or section of road, as with the S.A. or W.A. system.

In summary, the Victorian data base is not considered suitable for use in the proposed evaluation.

2.2.7 Tasmanian Data Base

The Tasmanian Transport Commission currently processes road accident data collected by the Tasmania Police and recorded on quite comprehensive accident report forms. Some 4,000 accident reports are received and processed annually.

The accident data are numerically coded and key-punched on to computer cards, but only a limited amount of analysis (this being manual) is carried out. To prepare accident histories for specific sites or areas is understood to be tedious and time consuming.

Whilst a study[1] has recently been completed to design a computer system for analysis and reporting of accident occurrence, implementation of the system is not completed. A further problem, in terms of the requirements of the present study, is that no complete coded road network is available for Tasmania. Thus, if there are (say) three road names at an intersection, there are three different pairs of road names which could be used on an accident report form. Alternatively, the use of 'local' street names rather than 'official' names could cause confusion, particularly in rural areas. The problem is similar to that existing in New South Wales.

The Tasmanian data base is not considered suitable for use in the proposed evaluation.

2.2.8 Queensland Data Base

Responsibility for reporting and recording accident data in Queensland is divided between three organisations :

1. Police Department - Police officers record details of accidents on a report form which is not suitable for subsequent research analysis.

2. Transport Department - file accident reports.

3. Australian Bureau of Statistics - extract data from accident reports and prepare data tapes.

The location recorded on the data tapes is simply that recorded on the original report, although some effort is made to standardise spellings and ordering of street names. For intersection accidents most reports could be recovered by a mixture of computer and manual sorting. However,

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[1] P.G. Pak-Poy and Associates Pty. Ltd., A Computerised Analysis System for Tasmanian Road Accident Data, prepared for Tasmanian Transport Commission ([illegible])

there would be some doubt that all reports were recovered. Further, for non-intersection accidents only the street name and LGA area are recorded making the location of the actual accident site impossible. The procedures for checking double reporting and consistency of reports are limited.

It is recognised in Queensland that improvements could be made to the system a report is currently being prepared of shortcomings and appropriate changes.

After consultation with authorities in Queensland, it is considered that it would not be possible to utilise the Queensland data base in analysing the effectiveness of MITERS-type projects.

2.2.9 Conclusion

For an evaluation of MITERS-type projects, both the South Australian and Western Australian accident data bases are currently suitable for immediate use. The Western Australian base in particular is of a very high standard. The N.S.W. base could be usefully employed after substantial re-working where the difficulty lies primarily in the method of recording the location of accidents.

In Summary, it is expected that for the evaluation phase of the study, the data bases of certainly two and possibly three States will be suitable for detailed analysis of the safety impact of MITERS-type projects.

For South Australia, the data base is suitable for the evaluation for accidents occurring from January 1972 onwards. For Western Australia, the data base is suitable for accidents occurring from January 1976 onwards.

2.3 EXPOSURE (TRAFFIC FLOW) DATA

2.3.1 Introduction

The concept of exposure is a basic ingredient of almost all analysis of accident frequencies. The definition and interpretation of exposure varies between analyses depending on the background of the research worker and the purpose of the study. However the concept is interpreted, there is basic agreement on the desirability of taking account of the opportunity for accidents which the traffic system experiences, if at all possible.

There are two basic ways of describing the accident situation at a location or within a system. Information may be given in the form of an accident frequency, or as an accident rate. The essential difference is that an accident frequency refers to numbers of accidents per time period, whilst an accident rate refers to numbers of accidents per unit of exposure.

In order to make comparisons of accident situations, statistics describing each situation must be converted to a common basis. For example, to compare the accident situation at a particular location over two disjoint time intervals, it may be best to quote the accident frequency relevant to each interval of time, for example, as accidents per month or year. On the other hand, the [illegible] of traffic at each location may be a factor which [illegible] amount of traffic (or exposure) changes, [illegible] numbers of accidents would be affected. In this case, it is best to quote numbers of accidents relative to some measure of the amount of traffic, that is, to quote an accident rate. Equivalently, the observed numbers of accidents may be modified in accordance with the observed exposure, and these modified numbers used in the analysis.

The best way to measure the amount of traffic has long been a matter of debate. In most cases, however, exposure is calculated as some function of vehicle flows. For example, at an intersection, exposure would be calculated as

$$E = a(V_1 V_2)^b$$

where a, b are constants

V_1, V_2 are flows on intersecting legs of the intersection.

In proposing a study method for the evaluation phase on the study, (Chapter 3), it is recommended that the analysis be undertaken in terms of both accident numbers (or frequencies) and the same numbers modified in accordance with changes in exposure. It is necessary, therefore, to establish the availability of traffic flow data for each State for which an appropriate accident data base has been shown to be available. This is the purpose of this Section.

Before this is done, some general comments are made on the errors likely to be present, in Average Annual Daily Traffic (AADT) data, since this is the format in which traffic flows are summarised and presented in all States.

Recent work in Sydney[1] has cast considerable doubt on many of the current techniques for scaling short term counts up to AADT's. It appears that the errors involved in these techniques are considerably larger than those previously estimated. However, that study has involved an estimate of the true daily random fluctuation, as well as examination of the stability of that pattern. For major arterial sites, it was found that site patterns are extremely stable, and that random fluctuations around that pattern are of the order of 2½% for a single day, or 2% for a three or four day total. (There is substantial correlation between successive days, and so the variability of a three or four day count is not much lower than for a single day). Seasonal variations in the pattern of daily and weekly traffic flow during a year, however, mean that traditional scaling methods include different systematic errors depending on the time of year of the short term count.

When all these factors are allowed for, it is estimated that the traditional methods of calculating AADT from short term counts give estimates with errors of up to 10-15%. However, when comparing ratios of AADT's, the error is not necessarily as high. Should the two short term counts be taken at the same time of the year, then the statistical error of the ratio would only be of the order of 3-4%. If, however, the timing of the short term counts is unknown, then the error associated with the ratio may be of the order of 15%. If the timing of the short term counts is known, an estimate may be made depending on the times of year, and the accuracy of the estimate will be between these two values (i.e.4-15%).

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[1] G.L.Teale and A.S.MacLean, *A proposed traffic counting program for the County of Cumberland*, prepared for N.S.W. Department of Main Roads by Nicholas Clark and Associates, [illegible]

These statistical errors involved in the traffic flow data must be considered when adjusting accident rates for changes in exposure. Further discussion of this effect is given in Chapter 3.

2.3.2 South Australian Exposure Data

An extensive traffic counting program is conducted in Adelaide and in country areas. The Adelaide program involves short term counts at approximately 5,000 sites and long term counts at 25 sites in Adelaide. The country program involves short term counts at 7,500 sites, and long term counts at 41 sites. The short term counts are over 24 hours, and the dates of these counts are available. This allows some adjustment of the scaling procedures and reduces the statistical error associated with the ratio of the AADT's. In Adelaide short term sites are covered every two years while in the country short term sites are covered every four years. The geographic spread of the sites in Adelaide is such that there is a count available from a site usually within the block leading to an intersection, and never separated from the point in question by more than a couple of intersections. Similarly in country areas there is a counting site close to most locations, at least in the sense of possible entry/exit points between an accident and count location.

Currently this data must be extracted manually from the traffic flow files. The number of sites for which flow data will have to be extracted is not considered excessive.

2.3.3 Western Australian Exposure Data

The coverage of locations in the counting program in the Perth urban area is currently being altered substantially. However, at approximately 500 sites some estimate of traffic trends over the proposed period of this study can be made from automatic seven day counts.

To supplement these counts, manual counts of either four hour or 12 hour duration are taken at approximately 200 sites each year.

The non urban classified roads are covered every five years, while non classified roads are covered every five or seven years, though other counts are taken from time to time within this period for special purposes.

The geographic coverage in Perth therefore, is not as dense as in Adelaide. However, all main routes are covered by the counting program. Hence, while the specific exposure at a specific site may not be available, a very good indication of exposure for a locality will be available from a major road in the area.

Further, given the short period of time for which accident data is available, it is really only necessary to check that a site has not undergone a sudden change in volume. Normal changes should be well covered for this period by the general secular trend.

In rural areas changes in exposure data cannot reasonably be calculated. However, in general the chance of substantial changes above or below the secular trend, especially over the relatively short period under consideration, is considered to be very low.

2.3.4 Conclusion

The traffic counting programs in S.A. are sufficiently extensive that count data will be available from count sites sufficiently close to the accident sites to ensure that counts will be of the same traffic streams as those at the accident sites. That is an exposure index can be calculated for specific sites.

In W.A. the coverage is not as complete. However, an exposure index for a locality can be calculated to isolate sites likely to have undergone major changes.

2.4 PROJECT DESCRIPTION DATA

2.4.1 Introduction and Summary

To evaluate the safety effectiveness of MITERS-type projects, descriptions are needed of the projects themselves. The information required for each project includes :

- type and description
- special features
- costs
- location
- date of commencement
- date of completion

The proposed method of analysis (see Chapter 3) may also require similar information for road engineering or traffic management work carried out near the location of particular MITERS-type projects.

This section contains a brief discussion for Western Australia and South Australia, of the availability of project description data. Submissions for funding of MITERS projects under the terms of the *States Grants (Roads) Act 1977* and the earlier *Roads Grants Act 1974* are made to the Commonwealth each year by the States. The general format of the submissions is shown in *Figure 2.1.*

Inspection of past submissions made by South Australia and Western Australia confirms that these will be generally suitable for collection of most of the information specified above. In certain cases, further enquiry of road authorities will be necessary to establish, for example, the precise location at which projects were implemented, and the costs of operation and maintenance for implemented projects.

In the following sub-sections, brief comments for each of South Australia and Western Australia are made.

2.4.2 South Australia Project Description Data

Table 2.3 shows a summary of the South Australia MITERS project submissions to the Commonwealth for the first four years of the operation of the MITERS program. Inspection of the summary shows that :

FIGURE 2.1: STATES GRANTS (ROADS) ACT 1977: FORMAT FOR

SUBMISSION OF MITERS PROGRAMS BY STATES

Construction Authority	Project Type (a)	Municipality or Local Authority	Location	Estimated Total Cost of Project $	Accidents per annum				Types of Accidents inc. in Columns 7, 8, 9 & 10	Accident Record Period (Years)	Comments regarding Need for Project
					At location		Susceptible to correction				
					Casualty (7)	Total (8)	Casualty (9)	Total (10)			

Source: "Notes on Administration" States Grants (Roads) Act 1977 D.O.T. Canberra

Note: (a) Type A projects are those which are generally similar within categories, and/or of very low cost, and for which average or typical conditions can be assumed in evaluating the effectiveness of the various categories of projects.

Type B projects can vary considerably in complexity and cost within categories. These projects must be individually evaluated.

Type C projects are those for which special submissions are required. Projects in this class are

TABLE 2.3 SUMMARY OF SOUTH AUSTRALIAN SUBMISSIONS UNDER THE MITERS PROGRAM (1974/5 to 1977/8)

Project Type	No. of Projects	Accidents per year (a) Casualty	Total
STOP signs	1	2	5
Conversion of intersection to 'T'	70	52	161
Median closure	4	6	42
Median barrier	13	20	123
New or upgraded street lighting	43	141	797
Safety bars	91	62	238
Modify intersection signals	40	99	758
Modify intersection channelization	5	5	18
New intersection signals	22	76	480
New intersection channelization	40	84	381
New intersection signals with channelization	(5 (32	8 n.a.	40 n.a.
Modify intersection signals and channelization	(2 (16	6 n.a.	43 n.a.
Additional lane at intersection	(2 (4	1 n.a.	12 n.a.
Eliminate intersection	4	6	16
Roundabout	52	58	147
New pedestrian signals	25	20	62
New school pedestrian crossing	14	1	5
Rail crossing flashing signals	18	12	12
Re-alignment	2	2	4
Re-alignment and adjust superelevation	2	2	5
Guardrails . at bridge . on curve	 3 1	 1 0	 4 1
Visibility improvement not at intersection	1	1	1
Co-ordinated signal system	14	n.a.	n.a.
Area traffic management	(1 (1	n.a. 15	n.a. 45
One-way street conversion	2	n.a.	n.a.
Priority route schemes	102	19[illegible]	10,196

(a) [illegible]

(i) Funds were requested for 636 projects in total over the four year period.

(ii) Nearly half of the projects involved treatment of a single intersection.

(iii) For 16 out of [illegible] project types, over 15 accidents per year were designated by the State as susceptible for correction. This fact assumes importance in Chapter 3, where it is suggested that, as a rule of thumb, projects will be able to be evaluated satisfactorily if at least 100 accidents in total occur in before and after periods.

2.4.3 Western Australian Project Description Data

Table 2.4 shows a summary of MITERS project submissions for the Commonwealth by Western Australia for 1976/7 and 1977/8. The summary has been restricted to those years because suitable accident data will be available from January 1976 only.

Inspection of the summary shows that :

(i) Funds were requested for 419 projects in total over the two year period covered by the table.

(ii) Some 66% of the projects involved treatment of a single intersection.

(iii) For only 7 out of 22 categories of projects, over 35 accidents per year were designated by the State as susceptible to correction. Because the period for which accident data are available is so short, (at most three years for before and after periods combined) it appears that only a small proportion of total MITERS projects in Western Australia will be able to be evaluated satisfactorily. This matter is discussed in more detail in Chapter 3.

2.5 SUMMARY

For each State, the quality and useability of accident data has been reviewed. It was concluded that only the South Australian and Western Australian accident data bases are currently suitable for use in the evaluation study. For these two States, sources of data on traffic flows and projects were also assessed.

In South Australia, exposure indices will be able to be calculated for most sites. In Western Australia, exposure indices may only be calculated for localities to assist in isolating sites likely to have undergone changes in traffic flow.

TABLE 2.4 SUMMARY OF WESTERN AUSTRALIA SUBMISSIONS UNDER THE MITERS PROGRAM (1976/7 to 1977/8)

Project Type	No. of Projects	Accidents per year (a)	
		Casualty	Total
Rail crossing treatment	20	n.a.	n.a.
New intersection signals	25	32	153
New intersection channelization	55	54	237
New intersection signals and channelization	6	4	32
Modify intersection signals	-	-	-
Modify intersection channelization	2	0	9
Modify intersection signals and channelization	3	7	44
Additional lane at intersection	3	5	27
Pedestrian refuge/median island	32	12	92
New pedestrian signals	2	n.a.	n.a.
Upgrade street lighting	1	n.a.	n.a.
Guard rail provision	1	n.a.	n.a.
Realignment	1	0	3
Convert intersection to 'T'	2	4	8
Remove intersection	1	0	0
Median closure	1	1	2
GIVE WAY signs	27	12	81
STOP sign	155	67	465
Advisory speed signs	56	n.a.	n.a.
Clearways	3	n.a.	n.a.
Guide post reflectors	3	n.a.	n.a.
Various	20	n.a.	n.a.

Designated by State as "susceptible to correction"

In both States, data on MITERS projects may be obtained, initially, from State submissions for the Commonwealth made under the terms of the *States Grants (Roads) Act 1977*, and the *Roads Grants Act 1974*. Further project information will have to be obtained from road authorities to establish :

- dates of commencement and completion of projects;
- operation and maintenance costs;
- construction and design costs;
- precise locations for projects (especially with Category A projects).

CHAPTER 3

PROPOSED METHODOLOGY FOR EVALUATING THE EFFECTIVENESS OF MITERS-TYPE PROJECTS

3.1 INTRODUCTION

This chapter contains a description of the proposed methodological procedure for conducting an evaluation study of the effectiveness of MITERS-type projects. The methodology is based on the before-and-after approach in which the accident situation at selected sites is observed before implementation of a project, and compared with the accident situation after implementation.

Once the before-and-after approach is accepted decisions must be made concerning the appropriate statistical technique for determining whether any observed change in the accident situation represents a change that can be explained by random behaviour or is due instead to the project that has been implemented.

To appreciate properly the proposed method, the reader should be familiar with the concepts of statistical testing of hypotheses and with the mathematical and statistical theory involved. This Chapter has been written in two parts. The first contains a verbal summary of the method and the second constitutes an annex of statistical and mathematical details.

The structure of the Chapter is as follows :

Section 3.2 A brief description and assessment of alternative procedures for evaluating effectiveness of road safety improvements.

Section 3.3 A specific test for determining whether accident numbers have decreased after implementation of a particular project type. Selection of an appropriate sample size for the test procedure. Proposed methods to account for :

- seasonal effects
- secular trends in accident occurrence
- changing exposure
- bias in before and after period selection
- effects of non-reporting
- system <u>vs</u> site effects.

Section 3.4 Development of stratification criteria. Development of tests to detect variations within and between project strata.

Section 3.5 Development of a test to detect relative changes in the severity level or type of accidents.

Section 3.6 Development of a test to allow comparison of cost-effectiveness within and between project types.

Section 3.7 Summary

Annex Statistical and mathematical details for Sections 3.2 to 3.6.

3.2 ALTERNATIVE PROCEDURES FOR ASSESSING SAFETY EFFECTIVENESS

3.2.1 Introduction

In an earlier report[1], a review was made of the four major types of experimental design used in evaluations of road safety effectiveness. These were :

1. Multivariate studies
2. Single group studies
3. Matched group studies
4. Before-and-after studies

Prior to the development of a specific statistical methodology to be used in the evaluation phase of the present study, each of these approaches has been re-examined to determine its potential applicability to MITERS-type projects. The conclusion was that the before-and-after study technique is the most useful to apply to the problem of assessing the safety-effectiveness of specific MITERS-type projects. (This is consistent with the observation that the technique is by far the most widely used approach to evaluations of road safety effectiveness). Consequently, it was decided to adopt the before-and-after approach as a basis for the evaluation and testing work of the proposed study.

The following comments on all four approaches are made by way of summary of the earlier report[2]. They identify the main reasons why the first three approaches are inappropriate for the evaluation of MITERS-type projects.

3.2.2 Multivariate Studies

A multivariate technique of analysis involves the selection of a range of actual situations, in each of which all the factors to be studied are present in varying degrees. The extent of the contribution of each factor is determined by analysing each situation, and the result is usually given by a mathematical equation representing the number of accidents as some function of the factors considered.

A simple model illustrates the general concept that accidents are a function of vehicle factors V, road factors R, driver factors D, and environmental factors S. Thus

$$A = f\ (V,R,D,S)$$

where A is a measure of the accident phenomenon.

The technique allows simultaneous study of many factors and their interactions, even when there is no previous knowledge of the effects of some of the factors. But, inclusion of a large number of factors means that a large number of individual situations must be studied. The number of such situations depends also on the number of levels or values which each

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[1] Nicholas Clark and Associates, *Evaluation of the Effectiveness of Traffic Accident Countermeasures* prepared for the Commonwealth Department of Transport (1974)

[2] Ibid.

factor may attain. Apart from these difficulties, there are the further difficulties of recognising all possible influential factors, and of assigning a quantifiable or measurable value to the attribute(s) of a factor.

In the case of evaluating MITERS-type projects, assessing the interaction between a large number of factors is not the issue. The concern is to assess the influence of one particular factor. The multivariate approach is therefore considered unsuitable.

3.2.3 Single Group Studies

For a particular project type, single group studies involve the selection of a single group of sites, identical in all respects, with the exception that the factor under study appears at a different level in each case. Consequently, the technique is directed to the study of factors which can be quantified and for which the concept of at least three different levels is meaningful[1] (e.g. traffic volume).

The technique is useful for assessing the change in performance of a particular measure as the measure is implemented with differing intensities. In the case of evaluation of MITERS-type projects, however, the principal concern is with factors which cannot be treated in this way (an example would be 'STOP sign' vs 'no STOP sign'. That is, for MITERS-type projects there is generally not a range of possible levels of implementation and therefore this type of approach is inappropriate.

3.2.4 Matched Group Studies

For matched group studies the aim is to investigate the effects of a single factor, for example, the erection of STOP signs. Two groups of situations may be used, which are dissimilar only in that a particular factor is present in all situations in one group, and absent in all situations in the other. Alternatively, two groups could be selected, each covering a wide range of conditions, but dichotomised such that the factor under study is present in one group and not present in the other. The difference in the average effect is then a measure of the effect of the selected factor. A matched group study is really a special case of a multivariate study, and is clearly more efficient and less complicated at the price of being able to study the effect of only one factor. For the proposed study, this failing is unimportant.

In practice, however, it will be difficult either to select two groups of situations similar in all respects except that of the factor under study, or to select two groups of situations which are truly representative of all possible factor combinations, as required by the alternative. In fact, the ideal matched group would be a set of situations matched with themselves provided that other factors related to the passage of time either did not influence the occurrence of accidents, or could have the effect of their influence removed. That is the situations before a particular factor (a MITERS-type project) is introduced would be compared with the same situations after the factor is introduced. The approach then becomes the before-and-after study, discussed in the next section.

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[1]For two levels, the matched group technique, discussed in Section 3.2.4, is appropriate.

Finally, the technique also suffers from the difficulty that it may not be easy to say exactly what other factors are present, or not present.

In summary, an approach using the matched group technique is considered unsuitable for the evaluation phase unless it is reformulated in terms of a before-and-after study.

3.2.5 Before-and-After Studies

The before-and-after study is by far the most common technique used in the evaluation of road safety measure effectiveness. In the substantial body of road safety literature reviewed for the earlier study[1], alternative techniques were only very seldom found to have been used.

In essence, the technique constitutes a matched group study, except that each situation is matched with itself, first without and then with the factor present. The major difficulty is that there may be unwanted and perhaps undetected changes in factors other than the one under examination. This may be compensated for by the use of statistical controls. An example of these would be the total number of accidents occurring in the State in which a project was implemented.

Of the four techniques so far discussed, this technique is conceptually the simplest. It involves comparison of the accident occurrence respectively before and after the implementation of a project. Further, it is more relevant than the multivariate technique (that is, only one factor is under study), it is more relevant than the single group technique (that is, it is better suited to the analysis of projects which are either completely or not implemented at all), and it represents the matched group technique in its ideal form.

3.2.6 Summary

For the reasons identified above, it was decided to develop a methodoloby for the evaluation phase of the study based on the before-and-after technique. The technique is superior to alternative approaches in important respects. It is also conceptually simple and requires a minimum of data input.

In the next Section, the development of a test procedure appropriate for use with the before-and-after approach to be applied in the evaluation phase of the study is described.

3.3 A TEST TO ASSESS ACCIDENT REDUCTION

3.3.1 Introduction

The test described in this Section has been developed for use with a before-and-after study evaluation technique. The purpose of the statistical test is to provide a basis for determining whether any change in accident behaviour observed after the introduction of a particular project can confidently be attributed to that project's implementation.

[1] Nicholas Clark and Associates op. cit.

Because, in most cases, the incidence of accidents at a particular site is not high, any procedure designed to test for a change in the accident experience of a particular site will not in general be very powerful. That is, there will be only small probability that the test will *indicate* a change to have occurred when, in fact, a change has occurred. For this reason it will be necessary to combine results from several sites where the same type of MITERS improvement has been carried out.

The test proposed is therefore based on comparing the number of accidents which fall into each of the before-and-after periods at a group of sites at which a particular type of MITERS improvement has been carried out. In the initial description of the test no account is taken of such factors as :

- changes in site exposure
- changes in the secular trend of accidents
- seasonal factors

Modifications to be made to take account of these factors are described later.

3.2.2 A Test for Accident Reduction

Suppose we have a group of sites numbered from 1 to n, at each of which a particular type of MITERS project has been implemented. For site i, let

p_i be the ratio of accidents expected in the after period to the total number of accidents observed in the before and after period if no improvement in accident incidence occurs

N_i be the total number of accidents observed in the before and after periods

$N_i p_i$ be the expected number of accidents in the after period if no improvement

n_i be the observed number of accidents in the after period.

The test is designed to answer the following question :

> Does the number of accidents observed in the after period, when compared with the number in the before period, indicate that the project has improved the safety of the group of sites?

Using the notation given above, we want to know if

$\sum_{i=1}^{m} n_i$ is significantly[1] smaller[2] than $\sum_{i=1}^{m} N_i p_i$

where m is the number of sites.

---

[1] An explanation of the concept of statistical significance may be found in any standard statistical text.

[2] Strictly speaking the first quantity may be larger than the second and still indicate an improvement in safety. We assume, however, for the purposes of exposition, that the before and after periods are of equal length and that changes in exposure and in secular trends are negligible. [illegible]

In the Annex to this Chapter, we show how to determine a critical value $\delta(\alpha)$, such that if

$$\sum_{i=1}^{m} n_i \text{ is smaller than } \sum_{i=1}^{m} K_i P_i - \delta(\alpha)$$

we can conclude that the project has improved the safety of the group of sites. That is $\delta(\alpha)$ is the minimum number by which the total accidents observed in the after period at sites 1 to m must be less in order to indicate that the project has improved safety.

The quantity α represents the probability that the conclusion drawn is wrong[1]. That is, if we conclude that the project <u>has</u> improved safety, there is a 100α per cent probability that the indicated improvement was due to chance alone and that the project has <u>not</u>, in fact, led to any improvement in safety.

The value assigned depends on the level chosen for α. This will typically be of the order of .05 or .10 indicating respectively a 5% and 10% probability that an indicated improvement could be explained by chance.

3.3.3 Power of the Test

In the above a test was described for which the conclusion that safety has been improved by the project is subject to a small 100α% chance of being wrong. Conversely, we should like to be reasonably sure that if safety <u>has</u> been improved, then the test will indicate that it has. In statistical terms, we denote $1-\beta$ as the probability of the test indicating an improvement in safety, given that one has, in fact, occurred[2]. The quantity $1-\beta$ is known as the *power* of the test.

In this section, we investigate this attribute. If, for test situations[3] likely to arise, the power can be shown to be acceptably high, then the test procedure can on this basis, be taken as satisfactory. Alternatively, it is useful (indeed, essential) to determine those situations which yield a test of unacceptably low power.

The power of the test is defined in terms of, and is therefore dependent on, the actual improvement in safety produced by the implementation of the project under test. We can quantify this dependency as follows. Let

- λ_b be the true mean number of accidents per year over all sites in the before period, and
- λ_a be the true mean number of accidents per year over all sites in the after period
- K be the ratio λ_a/λ_b

---

[1] This is known as the probability of a type I error.

[2] The quantity β is known as the probability of a type II error.

[3] By situation, we mean the state of affairs described by such things as : the total number of accidents observed, the relative lengths of before-and-after periods, the actual effect of the project on safety and the value chosen for α.

We therefore wish to determine the behaviour of $1-\beta$ when κ is actually less than unity, that is, when there is a lower accident frequency in the after period.

Some simplifying assumptions are useful. Suppose :

> t is the sum of the durations of all after periods (in years)
> T is the sum of the durations of all periods (in years)

Suppose further that sites are homogeneous with respect to time within each period. That is, the expected number of accidents for a given time period is directly proportional to the length of that time period. Then values of $1-\beta$ can be derived as a function of α, κ, ΣN, and t/T (see Annex). The results are shown in Table 3.1 and Figure[1] 3.1. It can be seen that in the majority of cases, if the total number of accidents observed is sufficiently large, the power of the test will exceed 0.90, particularly if α is set to equal 0.10 rather than 0.05.

That is, we now know that for the test proposed, provided the number of accidents observed is sufficiently large :

- there is low probability α (say 5% or 10%) of concluding that an increase in safety has occurred when an increase has not, in fact, occurred;
- there is high probability $1-\beta$ (in most cases 90% or better) of concluding that an increase in safety has occurred when it, in fact, has occurred.

3.3.4 Sample Size

In general, the selection of an appropriate sample size for a sample survey depends upon :

(i) the precision required of the results; that is, the accuracy with which a sample statistic should estimate an estimated parameter;

(ii) the cost of sampling.

In case of hypothesis testing, with which we are concerned, the first criterion may be replaced by

(ia) the desired power of the test: that is, the degree of confidence with which the central hypothesis will be rejected if it is in fact untrue.[1]

In the proposed evaluation study the cost of sampling will be a less important factor, initially, than the revised criterion stated above. This is because much of the work will be performed by computer. The selection of an appropriate sample size will therefore involve specification of the desired power of the test and, through reference to tables such as Table 3.1, determination of the corresponding sample size (in terms of numbers of accidents observed over both before and after periods).

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[1] In our case, this hypothesis is that no improvement in safety has occurred.

TABLE 3.1: Power of Proposed Test (a) (b)

Total no. of accidents observed ΣN_i	Ratio of accident frequency after to before κ	Power of 1-β: Ratio of after periods duration to total periods duration t/T 0.75 α=0.05	0.75 α=0.10	0.50 α=0.05	0.50 α=0.10	0.25 α=0.05	0.25 α=0.10
50	.9	.100	.177	.101	.181	.086	.160
	.8	.190	.297	.194	.309	.144	.249
	.7	.335	.464	.342	.486	.236	.375
	.6	.538	.664	.550	.692	.371	.536
	.5	.762	.849	.775	.873	.552	.719
100	.9	.125	.214	.131	.225	.109	.195
	.8	.276	.401	.296	.432	.218	.347
	.7	.511	.642	.548	.638	.395	.554
	.6	.773	.859	.811	.896	.627	.773
	.5	*.940*	*.973*	*.903*	*.985*	.848	*.930*
200	.9	.169	.273	.184	.295	.148	.251
	.8	.423	.560	.431	.614	.353	.503
	.7	.749	.843	.806	.891	.647	.780
	.6	*.953*	*.977*	*.975*	*.980*	.895	*.953*
	.5	[illegible]	*.999*	*1.000*	[illegible]	*.998*	*.997*
500	.9	[illegible]	.409	[illegible]	.458	.251	.383
	.8	.726	.928	.801	[illegible]	.662	.789
	.7	*.974*	*.989*	*.990*	[illegible]	*.951*	*.980*
	.6	*1.000*	*1.000*	*1.000*	*1.000*	*.999*	*1.000*
	.5	[illegible]	*1.000*	*1.000*	[illegible]	*1.000*	*1.000*
1000	.9	.435	.576	.508	.649	.402	.550
	.8	[illegible]	[illegible]	*.970*	*.959*	[illegible]	[illegible]
	.7	*1.000*	*1.000*	*1.000*	*1.000*	*.999*	*1.000*
	.6	*1.000*	*1.000*	*1.000*	*1.000*	*1.000*	*1.000*
	.5	*1.000*	*1.000*	*1.000*	*1.000*	*1.000*	*1.000*

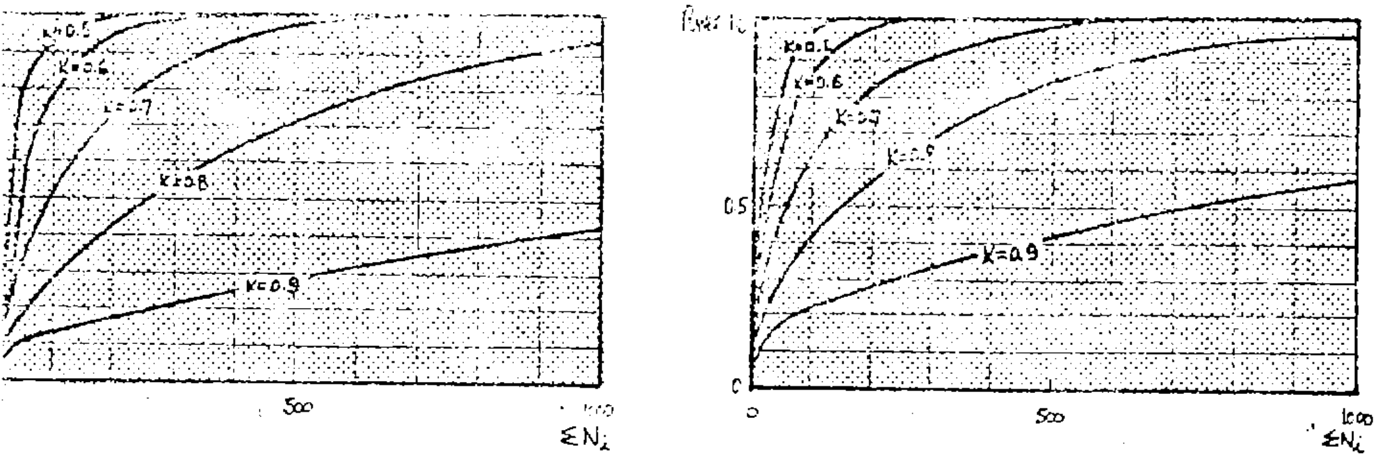
(a) For example, for a sample size resulting in 200 accidents being observed, for equal length before and after periods, for α chosen to equal 0.10, and for a true reduction in mean accidents per year in the after period of 60% of the mean frequency in the before period, there is a 99% chance that the test will detect a reduction in accident occurrence.

(b) [illegible] values [illegible] shown in italics.

FIGURE 3.1 : Power of Proposed Test

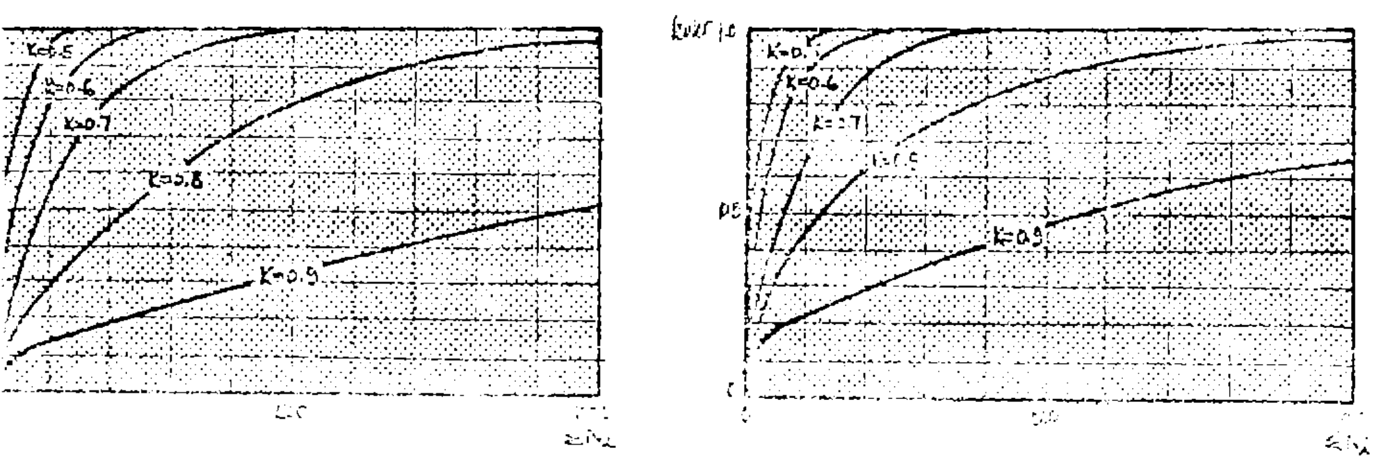
t/T = 0.75

$\alpha = 0.05$ $\alpha = 0.10$



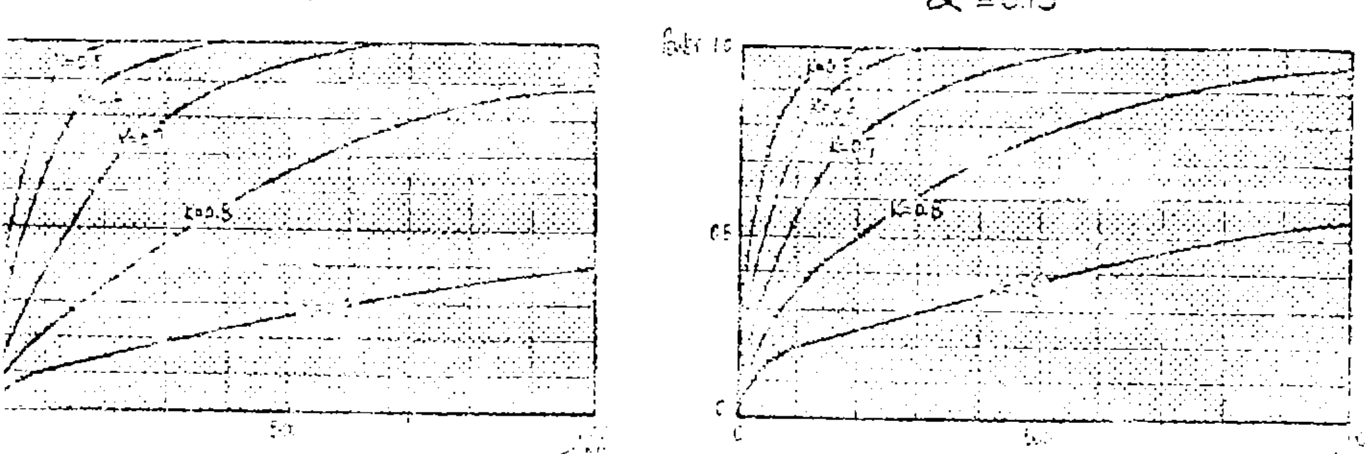
[illegible]

$\alpha = 0.05$ $\alpha = 0.10$



t/T = 0.25

$\alpha = 0.05$ $\alpha = 0.10$



A major constraint on the attainment of the selected sample size will be that particular types of projects will not have been implemented in any great number. Other types of projects will not be associated with large numbers of accidents. In either case, it will not be possible to attain the selected sample size, simply because the aggregation of individual projects of the one type will not result in a sufficient number of accidents.

This does not mean that the analysis should not proceed. Rather, should the test indicate that no reduction in accident numbers has occurred, then the power of the test should be examined to see if this is likely to have been adequate.

The question of sample size becomes more significant when it is desired to test the effect of projects on different types or severity of accident. In this case, the numbers of accidents of different types or severity may be effectively reduced to a small fraction of the total. To test for a reduction in numbers of accidents will in many cases be pointless, and it will be only the major types of accident which may be analysed in this way.

Finally, if it may be assumed that a sample size governed by $\Sigma N_i = 100$ will be appropriate in most cases (i.e. projects selected for inclusion in the analysis must yield a total of accidents in the before and after periods equal to at least 100). Tables 2.3 and 2.4 of Chapter 2 provide an initial assessment of project types for which this sample size will be attained.

In the case of South Australia, suitable accident data are available from January 1972 onwards. Thus to yield a total of at least 100 accidents in before and after periods combined, the number of accidents per year for each project type should be about 15 or more. Using Table 2.3, the project types for which this is the case (for either casualty or total accidents) are as shown in Table 3.2.

For Western Australia, the analogous compilation is shown in Table 3.3. In this case, suitable accident data are available only from January 1976 onwards. To yield a total of at least 100 accidents in before and after periods combined, the number of accidents per year for each project type should therefore be about 35 or more.

3.3.5 Suitability of Proposed Test for Project Evaluation

It is appropriate, at this stage, to examine the aptness of the proposed test procedure for the evaluation of MITERS-type projects. There are many different classes of projects which may be placed in the 'MITERS-type' category. Some of these, listed in the first Chapter, are repeated here for convenience.

a. traffic signals

b. road signs and pavement marking

c. speed control systems

d. elimation of intersections on arterial roads, modification of multi-street intersections, provision of median strips or new or modified traffic islands and roundabouts;

TABLE 3.2 PROJECT TYPES IN SOUTH AUSTRALIAN [illegible] (1974/5 to 1977/8) FOR WHICH TOTAL OF ACCIDENTS IN BEFORE AND AFTER PERIODS COMBINED IS LIKELY TO EXCEED 100

Project Type	No. of Projects	Accidents per year (a) Casualty	Total
Conversion of intersection to 'T'	70	52	161
Median barrier	13	20	123
Median Closure	4	6	42
New or upgraded street lighting	43	141	797
Safety bars	91	62	238
Modify intersection signals	40	99	758
Modify intersection channel'n		5	18
New intersection signals	22	76	480
New intersection channelization	40	84	381
Modify intersection signals and channelization	2	6	43
New intersection signals and channelization	5	8	16
Eliminate intersection	4	6	16
Roundabout	52	58	147
New pedestrian signals	25	20	62
Area traffic management	1	15	43
Priority route scheme	102	1456	10196

(a) Designated by State as "susceptible to correction"

Source: Table 2.3

TABLE 3.3 PROJECT TYPES IN WESTERN AUSTRALIAN MITERS PROGRAM (1974/5 to 1977/78) FOR WHICH TOTAL OF ACCIDENTS IN BEFORE AND AFTER PERIODS COMBINED IS LIKELY TO EXCEED 100.

Project Type	No. of Projects	Accidents per year (a) Casualty	Total
New intersection signals	25	32	153
New intersection channel'n	55	54	237
New intersection signals and channel'n	6	4	32
Modify intersection signals and channel'n	3	7	44
Pedestrian refuge/median island	32	12	92
GIVE WAY sign	27	12	81
STOP sign	155	67	465

(a) designated by State as "susceptible to correction"

Source: Table 2.4

e. pedestrian crossings (including fixed lighting) and the provision of pedestrian safety zones;

f. localised improvements to street lighting, or new lighting at isolated locations;

g. bus stopping bays for safety reasons;

h. turning lanes, channelisation and lane marking;

i. use of slip-base and frangible street lighting poles and sign supports at locations with high accident records;

j. relocation or protection of roadside objects at hazardous locations;

k. protection devices at railway crossings;

l. the adjustment to super-elevation on curves, and improvement of visibility on crests or curves; and

m. provision of guardrails on embankments, curves and bridge approaches.

At first sight, it might seem that a single statistical method could not be appropriate for the evaluation of each of a number of widely differing types of project. In all cases, however, projects are implemented to reduce accidents; and in all cases we wish to test for a reduction in accident occurrence. This is what the proposed test is designed to do. There is [illegible] that the proposed statistical method may not be applicable in every case.

A major difference in approach to the evaluation of each type of project is, however, required in deciding which accidents should be included in the analysis. Other necessary differences in approach relate to the treatment of exposure, statistical control groups and system effects. These matters are all discussed in the following sections.

3.3.6 Modifications to the Proposed Tests

In this Section, procedures are described for treating a number of factors which could otherwise unduly influence the outcome of the application of the proposed statistical method.

Seasonal effects : In developing and describing the proposed statistical method, accident occurrence has been assumed homogeneous with respect to time. In fact, there will be seasonal effects due to largely consistent changes in a number of factors throughout the year. These factors include :

(i) weather patterns
(ii) hours of daylight available per 24 h period
(iii) traffic flows

The effect is that some months are inherently more likely to contain a higher number of accidents than are others.

Unless the before and after periods for each project included in the sample for a particular project type start and finish at the same time of the year, the calculated accident frequency will be biased. It will generally be necessary to truncate one or other of the before and after periods to ensure that periods are made up of multiples of a whole year, in order to eliminate annual seasonal effects.

[illegible] in choosing [illegible]

type. If care is not taken to exclude those accidents unconnected with the project of interest, an incorrect assessment of the project's effectiveness may result. In other words, the inclusion of additional and irrelevant information reduces the power of the test to determine whether the project has been effective in reducing the types of accidents to which it has been directed.[1]

Further, the evaluation must allow us to identify cases where the occurrence of a certain type of accident has been increased by the implementation of a project. For example, while intersection signalisation projects are primarily directed at the reduction of accidents involving vehicles from conflicting traffic streams, it is now known that this type of project will generally cause an increase in front-to-rear collisions for vehicles in the same traffic stream.[2] Therefore, whilst an initial assessment might suggest that only the former type of accidents be included in the analysis, a true evaluation of the projects' effectiveness will only result if seemingly unrelated accidents are included as well.

On the other hand, the inclusion of too widely a defined set of accidents may cause the results to be 'contaminated' by the influence of other projects or factors. For example, consider a section of road several km in length. Suppose a central median has been installed, with openings at some intersections. Then an appropriate set of accidents would be all those occurring on the length of road, <u>including</u> those occurring at intersections. Suppose now that GIVE WAY signs are installed on the minor approaches to those intersections. It is no longer possible to assess the true effect of the median alone, since the GIVE WAY signs will themselves cause a change in accident occurrence. What could be done is to include in the evaluation of the median only single vehicle accidents and head-on accidents involving vehicles on the section of road. It may be argued that it is to these accidents that the median is <u>primarily</u> directed and that they will therefore provide a useful indication of the effectiveness of the median. Again, it may be argued that medians are installed to reduce the number of 4-way intersections on a road and the consequent conflicts and accidents. In this case, accidents at the remaining intersections should be included in the analysis because they reflect the change in conflict patterns caused by the median.

In summary, it is difficult to specify in advance all the types of accidents which will be affected by a particular type of project. Equally, it is difficult to specify those accidents which should be excluded because they fall under the influence of other projects or factors. It is therefore suggested that a procedure be followed which allows the evaluation to be made for an initially broadly defined set of accidents, and subsequent sets defined more narrowly. The analysis then becomes a 'sensitivity' analysis, in that an attempt is made to determine the sensitivity of the results to alternative definitions of the relevant accident data base. For example, in the case of a central median with GIVE WAY signs installed on minor approaches to remaining 4-leg intersections, the analysis could be undertaken using :

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[1] This may be shown using Table 3.1.

[2] See Chapter 4 for a demonstration of this effect.

(i) all accidents occurring on the length of road and at all intersections on that road;

(ii) as above, but excluding accidents at 4-leg intersections involving vehicles proceeding from the minor approaches;

(iii) as above, but excluding accidents involving vehicles proceeding from minor approaches of all remaining intersections.

A useful adjunct to this procedure would be provided if a test were available to distinguish the effect of a particular type of project on different classes or types of accidents. In Section 3.5, below, such a test is proposed. This will, for example, allow us to test formally whether the effect of intersection signalisation projects is the same for accidents involving vehicles in conflicting streams as for accidents involving vehicles in the same stream. If the effects are shown to be different, further tests on each class of accident would be made to determine the direction (increase or decrease in accident number) and size of individual effects.

<u>Bias in 'before' data</u> : Sites that are nominated for improvement by implementation of a MITERS-type project may have been initially chosen because of their 'bad' accident record. This may have serious implications for carrying out a valid evaluation. The following example[1] shows the nature of the problem.

Suppose a region has 1,000 potential accident sites of a particular type. Of these, suppose

- 900 are 'safe' (i.e. at a particular site the distribution of accidents per annum is Poisson with mean 0.5)

- 100 are 'dangerous' (i.e. at a particular site the distribution of accidents per annum is Poisson with mean 1.5).

Thus, in a given year the following situation is expected to exist :

Number of Accidents	Expected number of sites with given number of accidents in year		
	Safe	Dangerous	Overall
0	545.9	[illegible]	[illegible]
1	[illegible]	33.5	[illegible]
2	[illegible]	25.1	93.3
3	11.4	12.6	24.0
4	1.4	4.7	6.1
5	.1	1.4	1.5
6	–	.4	.4
7	–	.1	.1
All	900	100	1,000

[1] Statistical and mathematical working is shown in the Annex.

Of the approximately 32 sites with 2 or more accidents, 13 were safe sites and 19 were dangerous and yielded a total of 107 accidents. If there were now a delay of one year before the safety devices could be installed at these 32 sites, the total number of accidents at these sites would be expected to drop substantially to approximately 35 (13 x ½ + 19 x 3/2) before the devices were installed. *Thus a major drop in accident occurrence could occur before any projects were implemented.*

The example shows that :

(i) A decision concerning appropriate treatment for the accident sites will probably be biased if it is based on the data for this one year, or maybe even on several years' data including the bad year.[1] Even some 'dangerous' sites are seen to have an unusually bad accident record, solely through the operation of the underlying stochastic process.

(ii) To obtain a more reliable indication of the long-run accident situation at a site selected for improvement, it would be essential to monitor the accident records in years following the year or years used for selection, until the improvements were made.

Extending accident records for several years back beyond the date of the decision to undertake an improvement would not necessarily eliminate the bias. It can be shown that the effect of one bad year can overwhelm the influence of other years.[2]

Because we do not know whether the postulated source of bias exists, we propose that the evaluation be undertaken using two before periods, where possible. These will be :

(i) a before period using data available for all accidents occurring before implementation of a project;

(ii) a before period using only data for accidents which occur after the decision to implement a project was made.

In the case of the MITERS program (see Chapter 1) the commencement of the latter before period could, for example, be taken as the date of submission by a State of the program into which a particular project falls.

Statistical control : There are two major influences on the accident experience of a site :

- changes in the distance travelled by vehicles using the site;

- secular trends in the occurrence of certain types of accidents.

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[1] See Annex.

[2] See [illegible]

37.

In each case, the true effect of the project at the site is ... by these influences ... Consequently, the ... should be modified in order to yield the accident ... which would have occurred in the absence of these influences.

There would be severe practical problems in matching sites. To match sites it would be necessary to carry out site inspections, an inherently ... process ... of ... Instead, the following is proposed.

... To account for the effect of ... appropriate control could comprise ... sites large enough that, if the test site(s) were included in this group, their effect on the control group could be ignored. For the evaluation phase of the study, ... appropriate control ... of the State. Using accident data ... group, a correction factor may then be calculated and applied to ... after-period data. Whilst the ... counted, they may be treated ... for the purpose of applying the statistical methods developed in previous sections.

The use of ... control for ... illustrated in Chapter 4 and is not further ... here.

... Changes ... of the test sites should also be allowed for. It is generally accepted that the number of ... exposure ... accepted ... function of ...

... exposure ... through two influences :

a) the influence of the project or treatment itself on ... flow ...

... for ... of a nearby intersection.

... patterns ... would behave ... that the distribution ... (i.e. a ... thought ... implementation of the project, but indeed double it. Hence one of the ...

The change in local exposure due to the project's influence and the consequent impact on accident occurrence is properly seen as an effect of the project and should not therefore be used to adjust the observed accident data. The change in local exposure due to those general influences reflected in the overall control accident data will be taken into account in making the overall control correction.[1] The change in local exposure due to those external factors not accounted for by using an overall control is the only change which should form the basis of further adjustment of observed accident data.

It is not possible to determine the proportions of the total exposure change which fall into each of the three categories. Consequently, adjusting for changes in local exposure will be an approximate procedure. The approach proposed is to adjust at the extremes, namely :

(i) assume that all the exposure change should be used to adjust observed accident data;

(ii) assume that no exposure change should be used to adjust observed accident data.

The correct adjustment will lie somewhere between these two extremes, so that the approach advocated above will, in effect yield a 100% confidence interval for the proper value of the adjusted accident figures. Both extreme values should then be used for the evaluation of project effectiveness.

To adjust for exposure changes for locations at intersections, we propose using indices of the form :

$$E = T\ (V_m)^x\ (V_c)^y$$

where V_m refers to AADT on the main route
V_c refers to AADT on the cross route
T equals length of time period.

One study[2] suggests that the appropriate values for x, y are $x=y=\frac{1}{2}$.

The use of this index to adjust observed accident numbers for changes in local exposure is illustrated in Chapter 4.

For mid-block locations we propose using the following indices[3] :

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[1] In effect the control group is so large that the variance of accident numbers within the group due to the stochastic process is relatively very small. The correction of the raw data for trends will then not materially affect the variance in the reduction of the number of accidents at a site; thus by comparison with the matched pair approach where the variance is doubled, the proposed approach is to be preferred due to its improved power.

[2] G.R.Hodge and [illegible] Proc. 9th ARRB Conference.

[3] R.A.Chapman [illegible] p.184, (1967).

(i) single vehicle accidents (one vehicle, any direction)

$$E = TV$$

where V is the AADT for one-way flow,

T is length of time period

(ii) head-on collisions (two vehicles, opposite directions)

$$E = V_1 V_2 LT \left(\frac{1}{v_1} + \frac{1}{v_2}\right)$$

where V_1, V_2 are AADT value for opposing flows

L is length of road section
T is length of time period
v_1, v_2 are assumed mean speeds of flows 1 and 2 respectively.

Impact on test results : The trial analysis described in Chapter 4 suggests that in fact the adjustment of observed accident numbers for secular trends and for changes in local exposure may not significantly affect the conclusions of tests made for accident reduction. Certainly for the case studied in Chapter 4, no change in conclusions regarding safety effectiveness was required.

In any case, it must be remembered that the procedures proposed are necessarily approximate. The adjustment for secular trends assumes that secular influences are felt uniformly at every site studied and, further, that these influences are the same for different types of accident.

In adjusting for exposure by using vehicle flow data, there are several potential sources of error.[1] The two most important of these are :

(i) The exposure which is relevant is that which occurs at the time of each accident. The use of AADT to calculate exposure indices constitutes an assumption that exposure is the same constant value for each moment of the year.[2] Because of the format in which vehicle flow data is stored [illegible], there is no way of improving the method of calculating exposure indices.

(ii) AADT values themselves are known to be inaccurate. Recent work[3] suggests that the error can be 20% or more (that is,

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[1] See G.A. [illegible], *A study of intersection accident [illegible]*, Masters Thesis, [illegible] University, [illegible].

[2] When data is to be used for whole year periods and the exposure indices are only used to calculate relative exposures, then AADT's may be used if it is assumed that flow patterns at the two sites being compared are of the same shape. Recent work (see footnote) shows that this is a reasonable assumption for a large number of classes of site. However, for a fixed AADT an increase in the proportion of traffic at a site in the peak period of, say, 1% would increase average exposure at that site by approximately [illegible] for typical flow patterns, the exact increase depending on the flow pattern.

[3] G.L. [illegible] and [illegible], [illegible] for the [illegible], prepared for the Department of Main Roads by [illegible].

the difference between estimated AADT and actual AADT can be 20% or more of the actual value).

To show the effect of inaccuracies in AADT data, consider the following. At intersections the proposed exposure index for a particular year is $E = (V_m V_c)^{\frac{1}{2}}$. The error in E, given a 15% error in each of V_m and V_c in the same direction is therefore

$$(1.15V_m)^{\frac{1}{2}} \ (1.15V_c)^{\frac{1}{2}} - (V_m V_c)^{\frac{1}{2}}$$

$$= \quad 1.15\ (V_m V_c)^{\frac{1}{2}}$$

or 15% of E. Similar results hold for the other exposure indices proposed.

In summary, the results obtained from the evaluation of a project after adjustment for secular trends and exposure will have to be interpreted with caution.

<u>System effects</u> : The assessment of system effects due to particular groups of projects is an exceedingly complex task. For all but particularly straightforward cases, the effect of a project on the traffic system will be confounded by other countermeasures undertaken simultaneously. Thus, direct measurement of system effects will be possible only when either :

- individual project locations are substantially isolated from the influence of other projects, or
- the project constitutes an area-wide or system-wide application of a particular countermeasure.

In the former case, an appropriate procedure will be to make an assessment, either on-site or using maps, of which nearby sections of the road system may be affected through changes in traffic flow induced by each project. It is difficult to be precise about this approach to the problem, even for specific types of projects. Both the nature of the immediately surrounding road system and the extent of <u>changes</u> to the road system co-incident with or subsequent to implementation of the project under evaluation can be expected to vary substantially from site to site.

In the latter case, there would generally seem to be little possibility of a MITERS-type project being implemented on an area-wide or system-wide basis. If such a situation were found, the evaluation of the system effect could follow the procedure used by, for example, Ungers.[1]

In summary, system effects of MITERS-type projects though real, will generally be impossible to assess. Such an assessment will be possible only in very specialized circumstances, and the nature of the assessment will be governed closely by the nature of these circumstances.

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[1] R.Ungers, [illegible] Traffic Safety Conference, Ottawa, May [illegible].

3.4 STRATIFICATION TESTS

3.4.1 Introduction

The test procedure described in Section 3.3 is designed to answer the following question :

> Suppose a particular type of project is implemented at each of a group of sites. For the group as a whole, is the change in the accident situation consistent with the hypothesis that implementation of the project has improved safety?

Application of the test will therefore yield information concerning the general safety effectiveness of particular types of projects.

The test as described will not, however, answer a number of other important questions. For example, for a particular type of project, there will often be differences in engineering design or implementation and it is possible that these differences may result in different impacts on safety. To illustrate, a traffic signal installation at an intersection may be characterised by the presence or absence of :

- channelization
- special phases for turning vehicles
- vehicle activation of signals
- turning lanes

In each case, the effect on safety may be quite different. It is important to be able to determine both the existence and size of these differences.

An equally important question relates to determining the relative effectiveness, not of different versions of the same type of project, but of different project types. For example, it is important to know whether a high-cost type of project is proportionately as effective as a low-cost type of project. To illustrate, pedestrian operated signals and pedestrian (zebra) crossings perform the same function[1] at mid-block locations, yet the former are more costly than the latter. It would be useful to know which were the more effective in reducing accident occurrence.

The effectiveness of identical projects may also differ according to the type of sites at which they are implemented. Again, pedestrian (zebra) crossings provide a good illustration. It may be the case that substantial differences in effectiveness result in situations different in respect of traffic flows, street widths, traffic speeds, etc..

As a last example, it may be the case that the effect of projects implemented in more recent years will be the less, simply because the worst 'black spots' have already been eliminated. The verification of this conjecture would have significant implications for future MITERS-type project implementation.

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[1] that is, allow pedestrians to cross a road safely

st to Detect Differences between Different Groups of ects

in this case is essentially the same as that discussed in s section, except that the numbers of accidents involved to be larger. This is because we are now comparing groups [1] rather than projects at individual sites. Because of the ers of accidents, it is possible to propose a different ure which is simpler to apply yet yields at least as good the test described in the previous section.[2]

d test involves construction of a contingency table and the of a Chi-square statistic to test for differences between rojects. Details are given in the Annex. When the before of equal length, the contingency table is set out as follows :

be the number[3] of accidents observed in the before and after periods for project group i.

be the number[3] of accidents observed in the after period for project group i.

ency table is then

Before	After	Total both periods
$N_1 - n_1$	n_1	N_1
$N_2 - n_2$	n_2	N_2
..	..	..
..	..	..
..	..	..
..	..	..
..	..	..
$N_m - n_m$	n_m	N_m
$\sum_1^m (N_i - n_i)$	$\sum_1^m n_i$	$\sum_1^m N_i$

1 ups may be defined in any way at all (subject to the requirement accident numbers for each group are sufficiently large). For the grouping may be by project type, by type of site by year of ation or by project cost.

shown that the tests described in this and the previous section the same thing when accident numbers are sufficiently large.

ustment for exposure, etc.

c is compared with the value it should take
at there is no difference between groups. If
clude that group differences do exist.

tratification Tests

stratification tests should be undertaken as

e required comparisons to be made, for
her between :

ent versions of a particular project type

ent types of projects

ent sites for a particular type of project

ts of a particular type implemented in
ent years

pply the methods of Sections 3.4.2 or 3.4.3
te.

nce is indicated between projects or groups
(as the case may be), undertake pairwise
of individual projects or groups of projects
ablished location parameter test (e.g.
test).

procedure is given in Chapter 4.

the stratification tests which we would propose
ojects listed in Tables 3.2 and 3.3 is shown in

S IN ACCIDENT SEVERITY AND TYPE

LITERS-type projects on the occurrence of accidents
verity, we propose a contingency table approach
Section 3.4.3. In effect, the groups are replaced
severity or type categories.

r of accidents observed in the ith class
re and after periods

er of accidents observed in the ith class
r period.

are equal and the after periods are equal[1], the
on :

res when this is not the case are given in the

Total both periods
N_1
N_2
..
..
..
..
..
$\sum_{1}^{m} N_k$

H with the value
accident classes.

h class were too
In this case, the
ent type or severity.

verity and type is

rent project types,
that both safety
units. It is recom-
terms of the average
n projects.

t types according
We do not consider

periods

the

the after

cluding

bers of
jects

To determine the factor C_i, the total current money value of costs for projects of type i, we need to know :

- initial construction costs
- annual operating costs
- the interest rate[1] applicable to each of the years since implementation of the projects

Both capital and operating costs will have to be determined manually using State records.

Mathematical details of the proposed test procedure for comparing the cost effectiveness of different project types are contained in the Annex. The test procedure may, however, be visualised as follows.

Suppose the reduction in accident frequency for each class of MITERS project is plotted against the cost in current dollars of implementation of each class. The cost of effectiveness of the class is then given as the slope of the line joining this point to the origin. To test whether all classes of projects are equally cost-effective we may examine the scatter about a regression line through the origin fitted to the plotted points. If we conclude that the different classes of projects are not equally cost-effective, pair-wise comparisons may be carried out in order to determine the ranking of cost-effectiveness.

The test is equally validly applied whatever the type or severity of accident included in the calculation of accident frequency. It may, however, lead to different conclusions depending on the severity of accident used in determining the effectiveness. The most appropriate treatment to overcome this difficulty is probably to combine severity types according to a number of alternative assumptions concerning relative importance.

For example, the relative importance of each severity class could be assessed according to :

- published accident cost data[2]
- the relative frequency of occurrence of each severity class
- the preference of the analyst

In the first case, the importance of a severity class would be <u>directly</u> proportional to the cost of accidents in that class. In the second case, the importance of a severity class would be <u>inversely</u> proportional to the relative frequency of occurrence of accidents in that class. In the third and last case, the intuitive determination of the Analyst may be used to weight each severity class.

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[1]For a detailed discussion of the calculation of present worth of costs and of the definition and use of interest rates, see E.J.Mishan, *Cost-Benefit Analysis*, George Allen and Unwin Ltd., 1972.

[2]See, for example, P.N.Troy and N.G.Butlin, *The Cost of Collisions*, Cheshire, 1971.

Illustration of the proposed procedures for assessing and comparing the cost-effectiveness of different classes of projects has not been made. The time available did not allow the extraction of the necessary cost information.

3.7 SUMMARY

This chapter, and the statistical annex which follows, presents details of the procedures which are proposed for the evaluation of the effectiveness of MITERS-type projects. The procedures are based on the before-and-after study technique. The basic test answers the question :

> Does the number of accidents observed in the after period, when compared with the number in the before period, indicate that the project has improved the safety of a group of sites?

Various extensions and modifications of the basic test were proposed to account for the influence of :

- seasonal effects
- secular trends in accident occurrence
- changing exposure
- bias in before and after period selection
- effects of non-reporting
- system vs site effects.

Further tests, based primarily on contingency tables, were proposed for the investigation of :

- differences within and between project strata
- changes in the severity level or type of accidents.

Finally, a test was proposed for assessing and comparing the cost-effectiveness of projects. In Chapter 4, the feasibility of these statistical procedures is demonstrated.

ANNEX

STATISTICAL AND MATHEMATICAL DETAILS

In this Annex we set out statistical and mathematical arguments supporting the discussion given in the main body of the Chapter.

<u>Section 3.3.2</u>

We have, for site i:

p_i	the ratio of expected accidents in the after period (assuming no change in accident incidence) to the total number of accidents observed in the before-and-after periods.
N_i	the total number of accidents observed in the before-and-after periods.
$N_i p_i$	the expected number of accidents in the after period if no improvement.
n_i	the observed number of accidents in the after period.

Let Π be the theoretically expected proportion over all sites, (i.e. ratio of accidents, occurring in the after period over the total number of accidents in both before-and-after periods).

Let Π_o be the theoretically expected proportion over all sites if no improvement has occurred; and let $\Pi_1 < \Pi_o$ be the theoretically expected proportion if some improvement did occur.

Then an estimate of Π is:

$$\hat{\Pi} = \frac{\Sigma n_i}{\Sigma N_i}$$

and

$$\Pi_o = \frac{\Sigma N_i p_i}{\Sigma N_i}$$

The hypotheses to be tested are then:

$$H_o : \Pi = \Pi_o$$

$$H_1 : \Pi = \Pi_1$$

Applying the normal approximation to the binomial distribution[1] the number of accidents (Σn_i) is distributed approximately as

$$N\left(\Pi \sum_{i=1}^{m} N_i , \quad \Pi(1 - \Pi) \sum_{i=1}^{m} N_i\right)$$

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[1] See, for example, W. Feller *An Introduction to Probability Theory and Its Applications*: Volume 1 Third Edition, Wiley 1968. Pages 174-193.

and the null hypothesis (i.e. that the project category has had no effect) is rejected if

$$\sum_{i=1}^{m} n_i < \Pi_o \sum_{i=1}^{m} N_i - \phi^{-1}(\alpha) \sqrt{\Pi_o(1-\Pi_o) \sum_{i=1}^{m} N_i}$$

where $\phi^{-1}(\alpha)$ is the inverse function of the cumulative standard normal distribution; that is, where:

$$\alpha = \text{Probability} \left[\text{Standard Normal Variable} > \phi^{-1}(\alpha)\right]$$

Usually α is set at .05 or .10; $\phi^{-1}(.05) = 1.645$ and $\phi^{-1}(.10) = 1.282$

It is worth emphasising that the test described above is a one-tailed test and more sensitive than a two-tailed test such as the χ^2. That is, whilst a χ^2 test can be used only to determine whether a change has occurred, the test described determines whether a decrease only has occurred in accident frequency. Alternatively, a simple modification of the test would allow determination of whether an increase only, or change in either direction has occurred.

Section 3.3.3

We have:

λ_b the true mean number of accidents per year in the before period

λ_a the true mean number of accidents per year in the after period

κ the ratio λ_a/λ_b

t the sum of the durations of all the after periods

T the sum of the duration of all periods

If the sites are homogeneous with respect to time within each period

$$\Pi_o = t/T$$

$$\Pi_1 = \frac{\lambda_a t}{\lambda_a t + \lambda_b (T-t)}$$

$$= \frac{\kappa t}{\kappa t + T - t}$$

Under H_1, the number of accidents occurring in the after period is distributed approximately as[1]

$$N\left(\frac{\kappa t}{\kappa t + T - t} \sum_{i=1}^{m} N_i \;,\; \frac{\kappa t\,(T - t)}{(\kappa t + T - t)^2} \sum_{i=1}^{m} N_i\right)$$

[1] Using the normal approximation to the binomial distribution; see W. Feller *op cit*

and the power of the test of the hypothesis above is given by

$$1 - \beta = \Pr\left\{\Sigma\, n_i < \Pi_o\, \Sigma\, N_i - \phi^{-1}(\alpha)\ \sqrt{\Pi_o\,(1 - \Pi_o)\,\Sigma N_i}\right\}$$

$$= \Pr\left\{\Sigma\, n_i < \frac{t}{T}\ \Sigma\, N_i - \phi^{-1}(\alpha)\ \sqrt{\frac{t}{T}\ (1 - \frac{t}{T})\ \Sigma N_i}\right\}$$

where $\phi^{-1}(\alpha) = 1.645$ in this case.

Since $\sum_{i=1}^{m} N_i$ is normally distributed, we have

$$1-\beta = \phi\left\{(\frac{t}{T}\Sigma N_i - \phi(\alpha)\ \sqrt{\frac{t}{T}(1-\frac{t}{T}(1-\frac{t}{T})\Sigma N_i} - \frac{\kappa t}{\kappa t+(T-t)}\ \Sigma N_i) \Big/ \sqrt{\frac{\kappa t(T-t)}{\kappa t+T-t)}\Sigma N_i})\right\}$$

where $\phi(\alpha) = \Pr$ (std normal vble $\leq \alpha$)

Now, suppose $t/T = 3/4$. That is, we have one year of 'before' data and three years of 'after' data. Suppose also that the effect of the improvements on the accident frequencies was actually to reduce λ_b by 50%; then $\kappa = 0.5$.

Suppose $\Sigma N_i = 100$ and $\alpha = 0.05$. Then

$$1 - \beta = \phi\ \left\{(\frac{t}{T}\ \Sigma\ N_i - 1.645\sqrt{\frac{t}{T}\ (1 - \frac{t}{T})\ \Sigma\ N_i}\ \frac{\kappa t/T}{\kappa t/T+1-t/T}\quad \Sigma N_i) \Big/ \sqrt{\frac{\kappa t/T(1-t/T)\Sigma N_i}{(\kappa t/T+1-t/T)^2}}\right\}$$

$$= \phi\ \left\{(\frac{3}{4}\ .100 - 1.645\sqrt{\frac{3}{4}\ (1 - \frac{3}{4}\ )\ 100} - \frac{\frac{1}{2}\cdot\frac{3}{4}}{\frac{1}{2}.\frac{3}{4} + 1 - \frac{3}{4}}\ \ 100) \Big/ \sqrt{\frac{\frac{1}{2}\cdot\frac{3}{4}(1-\frac{3}{4})\ .100}{(\frac{1}{2}\cdot\frac{3}{4} +1 - \frac{3}{4})^2}}\right\}$$

$$= \phi\left\{(75 - 7.12 - 60)\ /\ 4.899\right\}$$

$$= 0.95$$

and the power of the proposed test is, in this case, very high.

Table 3.1 shows values of $1-\beta$, derived in the same manner, for various values of α, κ, ΣN_i and t/T. Figure 3.1 shows a graph of these relationships. It can be seen that careful selection of sample size will ensure a satisfactory power, particularly if α is set to equal .10.

Section 3.3.6

Bias in "before" data

(i) We have 900 'safe' sites at each of which the distribution of accidents per year is given by

$$\Pr(X = x) = \frac{e^{-\lambda}(\lambda)^{x}}{x!}; \ \lambda = 0.5$$

We have 100 'dangerous' sites at each of which the distribution of accidents per year is given by

$$\Pr(X = x) = \frac{e^{-\lambda}(\lambda)^{x}}{x!}; \ \lambda = 1.5$$

Multiplication of these factors by the actual total number of sites gives the expected number of sites with exactly x accidents per year. For example, for the 900 safe sites, the expected number with 2 accidents per year is 68.2.

(ii) To show that one year of 'bad' data may unduly influence several years of otherwise 'normal' data, consider the following hypothetical example.

At 'safe' sites, the expected number of accidents per year is 0.5. Over three years, say, the expected number of accidents would be 1.5. If a particular site were chosen for improvement because two (say) accidents were observed to occur in the first year[1], then after an additional three years, the expected total number of accidents would be 2.0 + 1.5 = 3.5. This should be compared with the expected number of accidents over four years (2.0) for a site not specially chosen.

The effect of the 'bad' year's results on the four year total for the specially chosen site is quite marked.

Section 3.4.2

We have:

λ_{b_i} the true accident frequency at the site i in the before period

λ_{a_i} the true accident frequency at the site i in the after period

κ_i the ratio $\lambda_{a_i}/\lambda_{b_i}$

---

[1] with probability of occurrence equal to 7.6%

The null hypothesis is that the actual ratio for each site i is independent of i, as follows:

$$H_o: \quad \kappa_i = \lambda_{a_i} \Big/ \lambda_{b_i} = \kappa = \lambda_a \Big/ \lambda_b$$

Under the null hypothesis, the observed ratio κ_i at each site is an estimate of the same parameter κ. These estimates will have different variances depending on the actual accident frequencies λ_a and λ_b. These variances can, however, be calculated as shown below.

The best estimate that can be given for κ will be a weighted mean of the observed κ_i after these have been corrected for bias. The variance of the corrected κ_i around the best estimate of κ can then be described in terms of a χ^2 distribution.

We first establish the bias of κ_i and estimate the variance of κ_i. As before for site i let:

> N_i be the number of accidents observed in the before-and-after periods.
>
> n_i be the number of accidents observed in the after period.

Further let:

> T_i be the total length of before-and-after periods.
>
> t_i be the length of the after period.

Then a good estimate of κ_i (dropping the subscript i for convenience) is:

$$k = \left(\frac{n}{N - n}\right) \cdot \left(\frac{T - t}{t}\right)$$

It can be shown[1] that a good and unbiased estimate of κ is:

$$k' = \left(\frac{n}{N - n}\right) \left(\frac{T - t}{t}\right) \Bigg/ \left(1 + \left\{\frac{1}{N - n}\right\}\right)$$

with variance estimated by:

$$\text{Var } (k') = \left(\frac{n}{N - n}\right)^2 \cdot \left(\frac{1}{n} + \frac{1}{N - n}\right) \cdot \left(\frac{T - t}{t}\right)^2$$

The best (i.e. minimum variance) estimate of κ, under the null hypothesis is thus.[2]

$$\kappa^* = \Sigma k'_i w_i$$

$$\text{where} \quad w_i = \frac{1/\widehat{\text{var}}\ (\kappa'_i)}{\Sigma\ 1/\widehat{\text{var}}\ (\kappa'_i)}$$

---

[1] See M. Tin *Comparison of Some Ratio Measures* J.A.S.A. 60, 1965, pp 294-307

[2] A standard result for stratified sampling

Finally, to test the null hypothesis, we use the test statistic:

$$\sum_{i=1}^{m} \frac{(k'_i - K^*)^2}{\mathrm{Var}\ (k'_i)}$$

which is distributed approximately as a χ^2 variable with $(m-1)$ degrees of freedom (one fewer than the number of sites).[1]

Section 3.4.3

Let:

N_i be the (adjusted) number of accidents observed in the before-and-after periods for group i.

n_i be the (adjusted)number of accidents observed in the after period for group i.

If the before periods for each group are of equal length and the after periods also of equal length, then the test for difference between groups may be stated as a standard contingency table:

	Before	After	Total both periods
Group 1	$N_1 - n_1$	n_1	N_1
Group 2	$N_2 - n_2$	n_2	N_2
..	..	..	..
..	..	..	..
..	..	..	..
..	..	..	..
..	..	..	..
Group m	$N_m - n_m$	n_m	N_m
Total all groups	$\sum_1^m (N_i - n_i)$	$\sum_1^m n_i$	$\sum_1^m N_i$

Under the null hypothesis that each group is equally effective, the two factors (group and period) are independent and test statistic

$$\sum_{k=1}^{m} \left\{ \left\{ \left[N_k \sum_1^m (N_i - n_i) / \sum_1^m N_i \right] - (N_k - n_k) \right\}^2 \Big/ \left\{ N_k \sum_1^m (N_i - n_i) / \sum_1^m N_i \right\} + \left\{ N_k \left[\sum_1^m n_i / \sum_1^m N_i \right] - n_k \right\}^2 \Big/ \left\{ N_k \sum_1^m n_i \Big/ \sum_1^m N_i \right\} \right\}$$

[1] The sum is the squares of m normalized approximately normal variables fitted to one estimated parameter K*.

is distributed as χ^2_{m-1}, provided that each of the observed quantities $N_i - n_i$ and n_i is greater than about ten.[1]

If the before periods or after periods are in fact of different lengths, the contingency table test described above must be modified. Adjustments must be made to make the factors of group and period independent, as follows.

The null hypothesis is that each group is equally effective with a reduction factor (unknown) of say κ. To estimate κ we assume that each group has no effect. Then the number of expected accidents in the after period f_i can be calculated using the techniques described in Section 3.3 of Chapter 3. The best estimate of κ then is:

$$k = \frac{\sum_{i=1}^{m} m_i \dfrac{N_i - f_i}{N_i - n_i}}{\sum_{i=1}^{m} f_i}$$

The expected frequencies in the contingency table may now be calculated using this value.

Let:

T_i be the total length of before-afer-period for group i.

t_i be the length of the after period for group i.

k_i be the reduction factor previously defined for group i.

Then the expected numbers of accidents before and after the group i are:

$$e_{ib} = N_i \frac{T_i - t_i}{T_i - t_i + k\,t_i}$$

$$e_{ia} = N_i \frac{k\,t_i}{T_i - t_i + k\,t_i}$$

Under the null hypothesis that each group is equally effective, $\kappa_i = \kappa$ for all i and test statistic:

$$\sum_{1}^{m} \frac{(N_i - n_i - e_{ib})^2}{e_{ib}} + \frac{(n_i - e_{ia})^2}{e_{ia}}$$

is distributed as χ^2_{m-1}

---

[1] See, for example, M.G. Kendall and A. Stuart *The Advanced Theory of Statistics*: Vol.2.

The following table gives an indication of the power of the test in the case where before periods and after periods are respectively equal in length:

Power of Test for Comparing Two Groups of Projects for $\alpha = 0.10$

κ_1	κ_2	N_1	N_2	Power $(1-\beta)$
1.00	0.50	50	50	0.51
0.80	0.40	50	50	0.51
1.00	0.50	100	100	0.77
0.75	0.50	100	100	0.40
0.80	0.50	100	100	0.50
0.80	0.40	100	100	0.76

Finally, when there are only small numbers of accidents, the underlying distribution is expected to be Poisson and so significantly skewed. Hence, in the terminology of the contingency table, the distribution of the observed number in each cell is Poisson and cannot reasonably be regarded as normal. Hence, the contingency table approach is not appropriate, and the approach of Section 3.4.2 must be used. However, when there are large numbers, the Poisson distribution approaches a Normal distribution and skewness disappears. The contingency table approach described above is then appropriate.

For sufficiently large accident numbers, the test statistics for Sections 3.4.2 and 3.4.3 can in fact be shown to be equal, when each group is regarded as comprising an individual site.

Section 3.5

We have, for projects of type i:

- T_i the length of before plus after time periods
- t_i the length of the after periods
- N_i the (adjusted) total number of accidents in before-and-after periods
- n_i the (adjusted) number of accidents in the after periods
- C_i the total present value (including capitalised maintenance costs) of the projects
- β_i the true reduction in (adjusted) accident frequency per dollar expenditure on projects (expressed in present value terms)

An estimator for β_i is:

$$B_i = \frac{N_i - n_i}{T_i - t_i} \qquad \frac{n_i}{t_i} \quad \frac{1}{C_i}$$

$$= \frac{N_i t_i - n_i T_i}{(T_i - t_i) \; t_i C_i}$$

We have, assuming N_i is constant:

$$E(B_i) = \beta_i \text{ and}$$

$$\text{Var }(B_i) = \frac{T_i^{\,2} \text{ Var }(n_i)}{\left\{(T_i - t_i) t_i C_i\right\}^2}$$

Again assuming N_i is constant, an estimate for Var (n_i) is:

$$\widehat{\text{Var}}\ (n_i) = \frac{n_i \ (N_i - n_i)}{N_i}$$

Therefore an estimate for Var (B_i) is:

$$\text{Var }(B_i) = \left\{\frac{T_i}{(T_i - t_i) t_i C_i}\right\}^2 \cdot \frac{n_i (N_i - n_i)}{N_i}$$

Suppose there are r different project types. We wish to test whether the various B_i come from populations with the same mean $\beta = \beta_i$. That is, we wish to test whether the various project types are equally cost effective. Pairwise comparisons will allow ranking of cost effectiveness.

The best (that is, minimum variance) estimate of β is:

$$\hat{\beta} = \frac{\sum_{i=1}^{r} \left\{ \frac{N_i t_i - n_i T_i}{(T_i - t_i) t_i C_i} \quad \frac{N_i}{n_i (N_i - n_i)} \quad \left[\frac{(T_i - t_i) t_i C_i}{T_i}\right]^2 \right\}}{\sum_{i=1}^{r} \left\{ \frac{N_i}{n_i (N_i - n_i)} \quad \left[\frac{(T_i - t_i) t_i C_i}{T_i}\right]^2 \right\}}$$

To test the null hypothesis that the β_i are equal (to β) the test statistic is:

$$S = \sum_{i=1}^{r} (B_i - \hat{\beta}) \frac{N_i}{n_i(N_i - n_i)} \left[\frac{(T_i - t_i)t_i C_i}{T_i} \right]^2$$

which, under the null hypothesis, is approximately[1] distributed as χ^2_{r-1}.

To carry out pairwise comparisons (if the null hypothesis is rejected) the test statistic is:

$$t = \frac{B_i - B_j}{\mathrm{Var}(B_i) + \mathrm{Var}(B_j)}$$

This statistic is approximately[1] distributed as a standard normal variable.

---

[1] M.G. Kendall and A. Stuart *The Advanced Theory of Statistics*, Vol. 2 Griffin

CHAPTER 4

TRIAL ANALYSIS: S.A. DATA BASE

4.1 INTRODUCTION

To illustrate the methods of Chapter 3, it was decided to conduct a trial analysis for one project type using the data available from one State. The South Australian accident data base was chosen.

For data related to the projects themselves, it was originally thought that the source easiest to use would be that incorporated in the traffic accident files, since this is already sequenced in the same way as the accident records. However, this source of data proved to be unreliable and had to be discarded. The main source of project data used was a copy of applications for MITERS funds submitted by the State of South Australia to the Commonwealth Department of Transport.

In this trial analysis, sites of traffic engineering improvements were location-coded as required. For the conduct of the main study, however, it is anticipated that it would be more economical to code up (assign location codes and enter into a data file) details for all minor traffic engineering improvements. In States such as New South Wales where there is a substantial Traffic Facilities program in addition to the MITERS program, improvements under both schemes could be coded.

The principal problem for the trial analysis was the determination of the implementation data of individual projects. This information can be obtained from State Road Authorities but a substantial manual check of records is involved. In the trial analysis a substantial gap was therefore left between the before-and-after periods, to allow for this uncertainty. Accurate information on implementation dates will allow the narrowing of this gap and hence improve the sensitivity of the tests.

The structure of this Chapter is as follows:

Section 4.2 - Project and accident data used in final analysis
Section 4.3 - Analysis of changes in raw accident numbers
Section 4.4 - The calculation of control factors for secular trends and changes in exposure
Section 4.5 - Analysis of changes in scaled accident numbers (that is, scaled to reflect changes in traffic flows and secular trends).
Section 4.6 - Concluding remarks

4.2 PROJECT AND ACCIDENT DATA USED IN TRIAL ANALYSIS

Projects examined in the trial analysis comprised traffic light installation at intersections in metropolitan Adelaide.

The raw accident data used are shown in Table 4.1. The projects were divided into two groups: Group A, where new traffic lights only were installed and Group B, where new traffic lights were installed together with modification of channelisation. All sites from the 1975/76 program within metropolitan Adelaide which fell into either of these two categories were included. These categories were not subdivided further by such criteria as the type of phasing used. One of the aims of the trial was to see if further subdivision was justified on statistical grounds.

The accident data of Table 4.1 are shown retabulated in aggregate in Table 4.2 (excluding the Daws Road-Marion Road intersection because of lack of data). Traffic flow data for all intersections were obtained from the South Australian Highways Department and are shown in Table 4.3.

A lack of time precluded identification of the precise installation dates for projects. This is certainly possible, but requires contact with individual District Officers of the South Australian Highways Department. We were, however, able to ascertain that all projects were undertaken and completed during the financial year 1975/76.

Before making a final choice of the years to be used for before-and-after periods, a brief examination was made of the reliability of the data base, in terms of the statistical characteristics of the accident data. This examination is described in the Annex to this Chapter. The main conclusion of the examination was that data for 1973 appeared to be unsatisfactory. Time did not allow resolution of the situation and, consequently, the years for before-and-after periods were chosen as 1974 and 1977 respectively.

4.3 ANALYSIS OF CHANGES IN RAW ACCIDENT NUMBERS

4.3.1 Introduction

In this Section, we discuss the results of applying the test procedures of Chapter 3 to analyse changes in raw accident numbers at sites where new traffic signals were installed. Tests conducted using scaled accident numbers are described later.

4.3.2 Overall Change: All Projects

Applying the test described in Section 3.3.2, we have:

N_i is the total number of accidents observed in before-and-after periods for site i.

n_i is the total number of accidents observed in the after period for site i.

So that, using Table 4.2

$$\Sigma N_i = 493$$

$$\Sigma n_i = 183$$

The null hypothesis is:

$$H_0 : \Pi = \Pi_0$$

where Π is the true proportion of accidents in the after period

Π_0 is the expected proportion of accidents in the after period if no improvement has occurred.

In this case, the before-and-after periods are of equal length, so that $\Pi_0 = \frac{1}{2}$.

TABLE 4.1:ACCIDENT DATA FOR NEW TRAFFIC SIGNALS IN METROPOLITAN ADELAIDE 1975/76 PROGRAM

GROUP A (NO NEW CHANNELISATION)

[illegible] & Code	Year	Total	Type: Head on	Rear end	Side swipe	Right angle	Others	Severity: Property Damage Only	Injury Accidents excluding fatals	Fatal accidents
[illegible]ghton Road	1977	17	0	12	1	3	1	15	2	0
[illegible]y Road	1976	36	1	20	0	12	3	28	8	0
[illegible]07210	1975	26	2	6	7	10	1	25	1	0
	1974	24	0	10	3	10	1	21	2	1
[illegible] Street	1977	18	0	12	1	3	2	17	1	0
[illegible]n Street	1976	38	1	20	0	17	0	34	4	0
[illegible]70000	1975	66	5	23	1	34	3	54	12	0
	1974	90	1	15	0	71	3	82	8	0
[illegible] Road	1977	37	1	22	2	10	2	30	7	0
[illegible]n Road	1976	36	1	27	1	6	1	33	3	0
[illegible]03000	1975	na								
	1974	na								
[illegible]s Hill Rd	1977	12	0	10	2	0	0	11	1	0
[illegible]) South Road	1976	6	0	5	0	1	0	6	0	0
[illegible]16020	1975	7	0	2	0	5	0	4	2	1
	1974	7	0	1	1	5	0	6	1	0
[illegible]) South Road	1977	25	0	18	1	5	1	25	0	0
[illegible]s Road	1976	24	0	19	2	3	0	20	4	0
[illegible]16000	1975	15	0	5	1	7	2	10	5	0
	1974	28	0	8	3	17	0	23	4	1
[illegible] Terrace	1977	51	5	26	6	11	3	46	5	0
[illegible]ens Road	1976	32	1	14	2	15	0	27	5	0
[illegible]818010	1975	42	1	11	2	28	0	34	8	0
	1974	51	2	8	2	39	0	42	9	0

TABLE 4.1: (cont'd) ACCIDENT DATA FOR NEW TRAFFIC SIGNALS IN METROPOLITAN ADELAIDE 1975/76 PROGRAM
GROUP B (NEW CHANNELISATION)

			Type					Severity		
Site & Code	Year	Total Accidents	Head on	Rear end	Side swipe	Right angle	Others	Property Damage Only	Injury Accidents excluding fatals	Fatal accidents
Goodwood Road	1977	26	0	7	2	15	2	22	4	0
Grange Road	1976	26	3	8	1	13	1	23	3	0
1091626000	1975	23	3	7	3	10	0	21	2	0
	1974	61	2	15	2	42	0	53	8	0
Commercial Road	1977	10	0	5	3	2	0	7	3	0
Dale Street	1976	16	0	2	3	11	0	16	0	0
1150514750	1975	10	1	0	1	8	0	9	1	0
	1974	19	0	4	6	8	1	17	2	0
Bridge Road	1977	24	2	15	2	5	0	22	2	0
Montague Road	1976	19	1	9	1	7	1	17	2	0
1420655000	1975	32	1	12	0	19	0	27	5	0
	1974	30	0	10	2	18	0	21	8	1

The test statistic is $\Sigma n_i = 183$, distributed as N (246.5, 11.102) under the null hypothesis and is to be compared with:

232.27 for $\alpha = 0.10$

228.24 for $\alpha = 0.05$

220.68 for $\alpha = 0.01$

Overall, therefore, there was a significant reduction in accident numbers at the 1% level of significance. That is, there is less than a 1% chance that the observed reduction could have occurred by chance, and that no real change occurred.

Category A Projects

For category A projects, the test statistic is $\Sigma n_i = 123$, distributed as N (161.5, 8.99) under the null hypothesis and is to be compared with:

149.98 for $\alpha = 0.10$

146.72 for $\alpha = 0.05$

140.60 for $\alpha = 0.01$

For category A projects, therefore, there was a significant reduction in accident numbers at the 1% level.

Category B Projects

For category B projects, the test statistic is $\Sigma n_i = 60$, distributed as N (85, 6.32) under the null hypothesis and is to be compared with:

76.64 for $\alpha = 0.10$

74.28 for $\alpha = 0.05$

70.42 for $\alpha = 0.01$

For category B projects, therefore, there was a significant reduction in accident numbers at the 1% level.

4.3.3 Comparison between Groups A and B

The test described in Section 3.4.3 was used to determine the possibility of a difference in the effect of groups A and B.

We have:

$m = 2$

$N_1 = 323$

$n_1 = 123$

$N_2 = 170$

$n_2 = 60$

TABLE 4.2: SUMMARY OF ACCIDENT DATA FOR NEW TRAFFIC SIGNALS IN METROPOLITAN ADELAIDE: 1975/76 PROGRAM

			Type					Severity		
Group	Year	Total Accidents (a)	Head on	Rear end	Side swipe	Right angle	Others	Property Damage Only	Injury Accidents excluding fatals	Fatal accidents
A Traffic Lights	1977	123	5	78	11	22	7	114	9	0
	1976	136	3	78	4	48	3	115	21	0
	1975	156	8	47	11	84	6	127	28	1
	1974	200	3	42	9	142	4	174	24	2
B Traffic Lights and new channelization	1977	60	2	27	7	22	2	51	9	0
	1976	61	4	19	5	31	2	56	5	0
	1975	65	5	19	4	37	0	57	8	0
	1974	110	2	29	10	68	1	91	18	1

The value of the test statistic is:

$$\frac{(323\ (200 + 100)/493 - 200)^2}{323(200 + 110)/493}$$

$$+ \frac{(170\ (200 + 110)/493 - 110)^2}{170\ (200 + 110)/493}$$

$$+ \frac{(323\ (183)/493 - 123)^2}{323(183)/493}$$

$$+ \frac{(170\ (183)/493 - 60)^2}{170(183)/493}$$

= 0.3705, distributed as χ^2, under the null hypothesis and is to be compared with:

2.71 for α = 0.10

3.84 for α = 0.05

6.63 for α = 0.01

The conclusion is therefore that there is no significant difference between the effectiveness of the two groups A and B of projects.

4.3.4 Differences within Group A

The test described in Section 3.4.2 was used to determine the possibility of different degrees of effectiveness for the various sites in Group A.

For each of the five sites in Group A for which accident data are available, unbiased estimates of the reduction factors k_i are:

$$k_i = \frac{n_i}{N_i - n_i} \Big/ \frac{T_i - t_i}{t_i} \left\{ 1 + \frac{1}{N_i - n_i} \right\}$$

$$= n_i \,/\, (N_i - n_i + 1) \quad \text{since } T_i = 2\, t_i$$

where N_i is the number of accidents observed in before-and-after periods at site i.

n_i is the number of accidents observed in after period at site i.

We have, using Table 4.1:

k'_1 = 0.680

k'_2 = 0.198

k'_3 = 1.500

k'_4 = 0.852

k'_5 = 0.961

The variance of the k_i' is estimated by:

$$\text{Var}\ (k_i') = \left\{\frac{n_i}{N_i - n_i}\right\}^2 \left\{\frac{1}{n_i} + \frac{1}{N_i - n_i}\right\}$$

$$= \frac{n_i\ N_i}{(N_i - n_i)^3}$$

Again, using Table 8, we have:

$$\widehat{\text{Var}}\ (k_1') = 0.0500$$

$$\widehat{\text{Var}}\ (k_2') = 0.0027$$

$$\widehat{\text{Var}}\ (k_3') = 0.665$$

$$\widehat{\text{Var}}\ (k_4' = 0.006$$

$$\widehat{\text{Var}}\ (k_5') = 0.392$$

Under the null hypothesis, the best estimate of $\kappa_i = \kappa$ is:

$$k^* = \Sigma k_i' w_i$$

Where $$w_i = \frac{1/\widehat{\text{Var}}(k_i')}{\Sigma\left\{1/\widehat{\text{Var}}(k_i')\right\}}$$

We have:

$$w_1 = 0.034$$
$$w_2 = 0.634$$
$$w_3 = 0.0026$$
$$w_4 = 0.285$$
$$w_5 = 0.0438$$

and $$k^* = 0.441$$

Finally, the test statistic $\sum_{i=1}^{5} \frac{(k_i - k^*)^2}{\widehat{\text{Var}}\ (k_i')}$, which is distributed as χ_4^2

under the null hypothesis, is 61.68
and is to be compared with:

7.78 for α = 0.10

9.49 for α = 0.05

13.28 for α = 0.01

The conclusion is therefore that there is a very significant variation in effectiveness at different sites in Group A.

4.3.5 Differences within Group B

As above, we have, using Table 4.1:

$$k_1' = 0.419$$

$$k_2' = 0.500$$

$$k_3' = 0.774$$

$$\hat{Var}\ (k_1') = 0.001$$

$$\hat{Var}\ (k_2') = 0.423$$

$$\hat{Var}\ (k_3') = 0.048$$

$$w_1 = 0.957$$

$$w_2 = 0.0226$$

$$w_3 = 0.0199$$

Therefore

$$k^* = 0.428$$

and the test statistic which is distributed as χ_2^2 under the null hypothesis is 2.70. This is to be compared with

4.605 for $\alpha = 0.10$

5.991 for $\alpha = 0.05$

9.210 for $\alpha = 0.01$

The conclusion is that there is no significant variation in the effectiveness of Group B projects.

4.3.6 Type of Accident

The only types of accidents which occurred with sufficient frequency to allow a reasonable analysis were rear-end and right-angle accidents. Using the test described in Sections 3.3.2, 3.4.3 and 3.5, the following results were obtained:

(i) group A; rear-end: test statistic = 78, distributed as N(60, 5.48) under null hypothesis;

(ii) group A; right-angle: test statistic = 22, distributed as N(82, 6.40) under null hypothesis;

(iii) group B; rear-end: test statistic = 27, distributed as N(28, 3.74) under null hypothesis;

(iv) group B; right-angle: test statistic = 22, distributed as N(45, 4.74) under null hypothesis;

(v) group A vs group B: rear-end: test statistic = 4.74, distributed as χ^2 under null hypothesis;

(vi) group A vs group B; right-angle: test statistic = 4.92, distributed as χ^2_1 under null hypothesis;

(vii) group A; right-angle vs rear-end: test statistic = 48.29, distributed as χ^2_1 under null hypothesis;

(viii) group B; right-angle vs rear-end: test statistic = 54.3, distributed as χ^2_1 under null hypothesis;

(ix) both groups; right-angle vs rear-end: test statistic = 147.9, distributed as χ^2_1 under null hypothesis.

Conclusions were therefore:

(a) Both group A and B signals caused a significant decrease (at 1%) in the number of right-angle accidents but not in the number of rear-end accidents.

(b) There was a significant difference in the effect of group A and B signals on both rear-end and right-angle accident numbers (at 5%).

(c) For groups A and B individually and together there was a significant change in the proportion of right-angle and rear-end accidents.

4.3.7 Accident Severity

Using the tests described in Sections 3.3.2, 3.4.3 and 3.5, the following results were obtained:

(i) group A; P.D.O.: test statistic = 114, distributed as N(144,849) under null hypothesis;

(ii) group A; injury plus fatal: test statistic = 9, distributed as N(17.5, 2.96) under null hypothesis;

(iii) group B; P.D.O.: test statistic = 51, distributed as N(71, 5.96) under null hypothesis;

(iv) group B; injury plus fatal: test statistic = 9, distributed as N(14, 2.65) under null hypothesis;

(v) group A vs group B; test statistic = 0.54, distributed as χ^2_1 under null hypothesis;

(vi) group A; injury plus fatal vs P.D.O.: test statistic = 0.326, distributed as χ^2_1 under null hypothesis;

(vii) group A; injury plus fatal vs P.D.O.: test statistic = 2.51, distributed as χ^2_1 under null hypothesis;

(viii) group B; injury plus fatal vs P.D.O.: test statistic = 0.15, distributed as χ^2_1 under null hypothesis;

(ix) group A and B; injury plus fatal vs P.D.O.: test statistic = 2.27, distributed as χ^2_1 under null hypothesis.

Conclusions were therefore that:

(a) Both group A and B projects caused significant (at 1%) decreases in both P.D.O. and injury plus fatal accident categories.

(b) There was no significant difference between these effects of group A and B projects.

(c) For groups A and B individually and together there was no significant change in the relative proportion of injury (including fatal) and P.D.O. accidents.

4.4 CONTROL

The control group used for secular trends was an overall one. Scaling factors were based on the slope of the lines through the origin relating the numbers of accidents in two separate years. Taking 1975 as the base year the factors for metropolitan Adelaide intersections were:

Year	Scaling Factor
1974	0.828
1975	1.000
1976	1.016
1977	1.010

The need for using a control group for secular trends may be gauged from the very noticeable change in reported accident numbers between 1974 and 1975. These factors were calculated from samples which comprised approximately 10,000 accidents, giving a standard error for the scaling factors of 0.015. The factors for 1976 and 1977 are, therefore, not significantly different from 1 but the factor for 1974 is very significantly different from one.

The reason for a difference such as this deserves further investigation in the main study but is not relevant to the trial analysis reported in this Chapter. In particular, as well as a genuine reduction in the number of accidents, this change may have been caused by a change in the proportion of accidents reported. This could occur as a result of changes in administrative procedures, penalties imposed etc. For example, introduction of compulsory breath tests could make drivers less inclined to report accidents in some cases. Another factor worth investigation would be the effects of seat belts.

To account for the effect of changes in site exposure, factors were calculated using the flow data of Table 4.3 for 1974 and 1977. The values V_1 and V_2 to be used in the exposure calculation were determined by taking the mean of the two-way AADT values applicable to opposite legs of the intersection.

For example, at intersection 1251507210 (ref. Tab 4.1) the values for V_1 and V_2 in 1974 were calculated as:

$$V_1 = (22500 + 23900)/2 = 23200$$

$$V_2 = 8700$$

and the site exposure was determined as:

$$E = k\ (23200 \times 8700)^{\frac{1}{2}}$$

$$= 14207k,\ k \text{ a constant}$$

Finally, Table 4.4 shows, for each site for which data were available, the total numbers of accidents, the scaling factors for secular trends, the scaling factors for changes in exposure (obtained by dividing after-exposure by before-exposure) and the numbers of accidents modified in accordance with both sets of scaling factors.

4.5 ANALYSIS OF CHANGES IN SCALED ACCIDENT NUMBERS

4.5.1 Overall Change
All Projects

Using the test described in Section 3.3.2, the possibility of an overall reduction in scaled accident numbers was determined, as follows:

(i) *Scaled for secular change only*

The test statistic is $\Sigma n_i = 141.8$, distributed as N (216.4, 7.36) under the null hypothesis and is to be compared with:

207.0 for $\alpha = 1.10$

199.3 for $\alpha = 0.01$

(ii) *Scaled for secular and exposure change*

The test statistic is $\Sigma n_i = 151.1$, distributed as N (221.1, 10.51) under the null hypothesis and is to be compared with:

207.6 for $\alpha = 0.10$

196.7 for $\alpha = 0.01$

In both cases, therefore, there is indicated a significant reduction in accident numbers at the 1% level of significance.

Category A Projects

(i) *Scaled for secular change only*

The test statistic is $\Sigma n_i = 100.8$, distributed as N (150.4, 8.67) under the null hypothesis and is to be compared with:

139.3 for $\alpha = 0.10$

130.3 for $\alpha = 0.01$

TABLE 4.3: TRAFFIC FLOW DATA (TWO-WAY AADT) FOR INTERSECTIONS SHOWN IN TABLE 4.1

Site and Code (c)	Year (a)				
	1974		1976		1977 (b)
Brighton Rd	22500(S)	23900(N)	25700(S)	25700(N)	22850
Jetty Rd	8700(W)		8550(W)		6180
1251507210					
Adam St	7000(W)	15600(E)	7200(W)	15800(E)	14500
Manton St	12500(S)	3050(N)	13200(S)	5000(N)	8000
114107000					
Chandlers Hill Rd	5400		7400		6000
Main South Rd	45500(S)	50000(N)	53200(S)	5500(N)	53000
1172016020					
Main South Rd	47000(S)	4700(N)	55000(S)	50000(N)	53550
Majors Rd	4700		8200		6800
1172016000					
David Terrace	10000(W)	7800(E1) 8700(E2)	10800(W)	9100(E1) 4700(E2)	7300
Torrens Rd	19900(S)	26700(N)	19800(S)	2400(N)	19700
1160818010					
Goodwood Rd	30200(S)	30700(N)	2800(S)	31000(N)	30800
Grange Rd	5000(E)	3500(W)	3800(E)	2850(W)	4500
1901626000					
Bridge Rd	11800(S)	17500(N)	13700(S)	20000(N)	14600
Montague Rd	12500(E)	5600(W)	3300(E)	6500(W)	9050
1420655000					

Notes: (a) Letter in parentheses indicates leg (compass point) of intersection for which flow value applies.

(b) For 1977, flow values are means of N plus S and E plus W values respectively.

(c) Flow data not available for intersections 1221608000 and 1150514750.

TABLE 4.4: Scaled accident data for new traffic signals in metropolitan Adelaide: 1975/76 Program

Project Code	Year	Total Accidents	Relative Exposure(a)	Relative Secular Change(b)	Total accidents adjusted for secular change	Total accidents adjusted for change in exposure	Total accidents adjusted for exposure and secular change
Group A							
1251507210	1977	17	0.700	1.010	13.9	24.3	19.9
	1974	24	1.000	0.828	24.0	24.0	24.0
114107000	1977	18	1.320	1.010	14.8	13.6	11.2
	1974	90	1.000	0.828	90.0	90.0	90.0
1172016020	1977	12	1.233	1.010	9.8	9.7	8.0
	1974	7	1.000	0.828	7.0	7.0	7.0
1172016000	1977	25	1.648	1.010	20.5	15.2	12.4
	1974	28	1.000	0.828	28.0	28.0	28.0
1160818010	1977	51	0.699	1.010	41.8	73.0	59.8
	1974	51	1.000	0.828	51.0	51.0	51.0
Sub-total	1977	123	-	-	100.8	135.8	111.3
Group A	1974	200	-	-	200.0	200.0	200.0
Group B							
109162600	1977	26	1.059	1.010	21.3	24.6	20.1
	1974	61	1.000	0.828	61.0	61.0	61.0
1420655000	1977	24	0.997	1.010	19.7	24.1	19.7
	1974	30	1.000	0.828	30.0	30.0	30.0
Sub-total	1977	50	-	-	41.0	48.7	39.8
Group B	1974	91	-	-	91.0	91.0	91.0
TOTAL ALL	1977	173	-	-	141.8	184.5	151.1
GROUPS	1974	291	-	-	291.0	291.0	291.0

Notes: (a) Normalised with respect to 1974 year

(b) See Section 6.2.3 for details

(ii) *Scaled for both secular and exposure change*

The test statistic is n_i = 111.3, distributed as N (155.7, 8.82) under the null hypothesis and is to be compared with:

144.4 for α = 0.10

136.0 for α = 0.01

In both cases, therefore, there is indicated a significant reduction in accident numbers at the 1% level of significance.

Category B Projects

(i) *Scaled for secular change only*

The test statistic is Σn_i = 41.0, distributed as N(66.0, 5.74) under the null hypothesis and is to be compared with:

58.6 for α = 0.10

52.7 for α = 0.01

(ii) *Scaled for both secular and exposure change*

The test statistic is Σn_i = 39.8, distributed as N (65.4, 5.72) under the null hypothesis and is to be compared with:

58.1 for α = 0.10

52.6 for α = 0.01

In both cases, therefore, there is indicated a significant reduction in accident numbers at the 1% level of significance.

Comparison Between Groups A and B

Using the test described in Section 3.4.3, the possibility of a difference in the effect of groups A and B was investigated, as follows:

(i) *Scaled for secular change only*

We have:

m = 2

N_1 = 300.8

n_1 = 100.8

N_2 = 132.0

n_2 = 41.0

The value of the test statistic is 0.24, distributed as χ^2_1 under the null hypothesis and is to be compared with:

2.71 for α = 0.10

6.63 for α = 0.10

(ii) *Scaled for both secular and exposure change*

We have:

$$m = 2$$
$$N_1 = 311.3$$
$$n_1 = 111.3$$
$$N_2 = 130.8$$
$$n_2 = 39.8$$

The value of the test statistic is 1.16, distributed as χ^2_1 under the null hypothesis and is to be compared with:

2.71 for $\alpha = 0.10$

6.63 for $\alpha = 0.01$

In both cases, therefore, there is indicated no significant difference between the effectiveness of the two groups A and B of projects.

4.5.3 Differences within Group A

The test described in Section 3.4.2 was used to determine the possibility of of different degrees of effectiveness for the various sites in Group A.

(i) *Scaled for secular changes only*

We have:

$$k_1' = .0.556$$
$$k_2' = 0.163$$
$$k_3' = 1.225$$
$$k_4' = 0.707$$
$$k_5' = 0.804$$
$$\hat{Var}\ (k_1') = 0.038$$
$$\hat{Var}\ (k_2') = 0.002$$
$$\hat{Var}\ (k_3') = 0.48$$
$$\hat{Var}\ (k_4') = 0.045$$
$$\hat{Var}\ (k_5') = 0.029$$
$$w_1 = 0.045$$
$$w_2 = 0.855$$
$$w_3 = 0.0036$$

$w_4 = 0.038$

$w_5 = 0.059$

Therefore:

$k^* = 0.243$

The value of the test statistic is 23.4, distributed as χ^2_4 under the null hypothesis, and is to be compared with:

7.78 for $\alpha = 0.10$

13.28 for $\alpha = 0.01$

(ii) *Scaled for both secular and exposure changes*

We have:

$k_1' = 0.796$

$k_2' = 0.123$

$k_3' = 0.000$

$k_4' = 0.428$

$k_5' = 1.150$

$\hat{\mathrm{Var}}(k_1') = 0.63$

$\hat{\mathrm{Var}}(k_2') = 0.0016$

$\hat{\mathrm{Var}}(k_3') = 0.350$

$\hat{\mathrm{Var}}(k_4') = 0.23$

$\hat{\mathrm{Var}}(k_5') = 0.50$

$w_1 = 0.022$

$w_2 = 0.884$

$w_3 = 0.004$

$w_4 = 0.061$

$w_5 = 0.028$

Therefore:

$k^* = 0.189$

The value of the test statistic is 31.4, distributed as χ^2_4 under the null hypothesis, and is to be compared with:

$$7.78 \text{ for } \alpha = 0.10$$

$$13.28 \text{ for } \alpha = 0.01$$

In both cases, therefore, there is a very significant variation in effectiveness at different sites in Group A.

4.5.4 Differences within Group B

The test described in Section 3.4.2 was used to determine the possibility of different degrees of effectiveness for the various sites in Group B.

(i) *Scaled for secular changes only*

We have:

$$k_1' = 0.345$$

$$k_2' = 0.635$$

$$\hat{Var}(k_1') = 0.0077$$

$$\hat{Var}(k_2') = 0.036$$

$$w_1' = 0.824$$

$$w_2 = 0.176$$

and $$k^* = 0.396$$

The value of the test statistic is 1.92, distributed as χ^2_1 under the null hypothesis and is to be compared with:

$$2.706 \text{ for } \alpha = 0.10$$

$$6.635 \text{ for } \alpha = 0.01$$

(ii) *Scaled for both secular and exposure changes*

We have:

$$k_1' = 0.324$$

$$k_2' = 0.635$$

$$\hat{Var}(k_1') = 0.0072$$

$$\hat{Var}(k_2') = 0.036$$

$$w_1 = 0.833$$

$$w_2 = 0.167$$

and $$k^* = 0.376$$

The value of the test statistic is 2.24, distributed as χ^2_1 under the null hypothesis and is to be compared with:

2.706 for α = 0.10

6.635 for α = 0.01

In both cases, therefore, there is no significant difference in effectiveness at different sites in Group B.

4.5.5 Type of Accident and Accident Severity

Exposure data will generally not be able to be obtained to allow a corresponding analysis of accident type or severity in terms of scaled accident numbers. Factors reflecting secular change for different types and severity of accident were not able to be obtained in time for the analysis to be undertaken. These latter factors are expected to be available for the main study.

4.6 CONCLUSION

In this Chapter, we have demonstrated the application of techniques developed in Chapter 3, for the analysis of the effectiveness of MITERS-type projects. It has been shown that those techniques are both appropriate and workable and thereby suitable for use in the evaluation phase of the study.

The demonstration itself yielded some interesting results:

(i) The particular signalization projects analysed were, as a whole effective in reducing both overall accident numbers and accident rates.

(ii) Projects involving channelisation were equally as effective as those not involving channelisation in reducing both overall accident numbers and accident rates.

(iii) For projects not involving channelisation, the effectiveness in reducing both accident numbers and accident rate varied significantly according to the site of installation.

(iv) Both types of projects caused a significant decrease in the number of right-angle accidents but not in the number of rear end accidents.

(v) Projects not involving channelisation caused a significant increase in the number of rear end accidents.

(vi) Both types of projects were equally effective in reducing both P.D.O. and injury (including fatal) accidents.

ANNEX

RELIABILITY OF S.A. DATA BASE

INTRODUCTION

In this Annex, we describe a brief investigation conducted using accident data extracted from the South Australia data base. The investigation was undertaken to determine the reliability of the data base.

We first develop a theoretical argument to determine characteristics which should be shown by the data, given certain plausible assumptions, then check the behaviour of actual data against these theoretical expectations.

Expected Behaviour of Accident Data

It is widely accepted that accident numbers generally follow a Poisson distribution. Suppose we assume that the distribution of the number of accidents at site i in each of two successive periods is Poisson and independent, with an expectation of λ_1 in the first period and $\lambda_2 = c\lambda_1$ in the second period. We then plot the accident records on a scatter diagram as points (n_{1i}, n_{2i}), where n_{1i} and n_{2i} are the observed accident numbers at site 1 for the first and second periods respectively. It is easy to show that the various plotted points would be distributed about a line of slope c with standard error $\sqrt{c(1+c)\lambda_1}$.

If, however, the variables are transformed to $x_1 = \sqrt{n_1}$ and $x_2 = \sqrt{n_2}$ and the points (x_1, x_2) plotted, it can be shown that these points would be distributed around a line of slope c with a standard error of 0.707 (i.e. $\frac{\sqrt{2}}{2}$). This is in fact, an asymptotic result. To show that it is a good approximation for even small values of λ_1, Table 1 gives the exact result for integral values of λ_1 up to 30.

4.2.2 Comparison of S.A. Data with Expected Behaviour

In the above discussion, we have predicted the scatter which would occur when accident numbers in one period were plotted against accident numbers in another period. More precisely, we predicted that if the square roots of the numbers of accidents were plotted then the points representing sites should be scattered around a line with a standard error of $\frac{\sqrt{2}}{2}$.

To test this with actual data, the accident records for the period 1972-1977 for all intersections within metropolitan Adelaide where there were at least four accidents in any year were obtained from the South Australian Highways Department. These records contained information on accident types and severity. Full details of the records obtained are given in Figure 1.

Accident records for South Australia are currently held in a sequential file for each year sorted on the location code. All the files obtained except for the 1973 file were in this format. However, there were some sequencing errors in the 1973 file which may have created incomplete records for some sites and so 1973 data at these sites were not used. In fact, complete records for the period 1972-1977 were obtained at 484 sites within the metropolitan area and records for some years within this period were obtained for some other sites.

TABLE 1: Exact standard error for regression of x_2 on x_1 where x_1, x_2 are derived from Poisson variables with means λ_1 and $\lambda_2 = c\lambda_1$ respectively.

λ	E (x_1)	Std Err
1	0.773	0.897
2	1.269	0.883
3	1.631	0.825
4	1.922	0.782
5	2.171	0.756
6	2.393	0.742
7	2.594	0.734
8	2.781	0.729
9	2.956	0.725
10	2.121	0.723
11	3.277	0.721
12	3.427	0.720
13	3.510	0.719
14	3.707	0.718
15	3.840	0.717
16	3.968	0.716
17	4.092	0.716
18	4.212	0.715
19	4.330	0.715
20	4.446	0.714
21	4.555	0.714
22	4.663	0.714
23	4.769	0.713
24	4.873	0.713
25	4.975	0.713
26	5.074	0.712
27	5.172	0.712
28	5.267	0.712
29	5.362	0.712
30	5.454	0.712

Using yearly periods calculation of the standard error for a regression of the square roots of accident numbers in successive years was carried out for years from 1972 to 1977. The data are shown graphically in Figures 2 to 9. The details of these tests are given in Table 2. Summarising these results, however, the data for the period 1974 to 1977 appear to behave as expected. Indeed, the observed scatter was almost equal to that predicted.

However, the scatter of 1973 data when plotted against 1972 or 1974 data (see Figures 8 and 9) was far greater than for the other comparisons, thereby throwing considerable doubt on the reliability of 1973 data. These results together with sequence errors mentioned above which were detected when the 1973 file was read, suggest that there were some omissions in the original 1973 data file.

Conclusion

From the analysis of South Australian accident data described above, we may conclude that these data are generally sufficiently reliable except for that data relating to 1973.

Figure 1: Format of Tapes Supplied by S.A. Highways Department

---

A. One sequential file was generated for each year from 1972 to 1977 with the following information.

Record layout:

FIELDS

1. Location Code (sequence)
2. Intersection Type
3. Road Controls (Medians, signs painted on road, etc.)
4. Erected Controls (Signals, etc.)
5. Number of accidents
6. Number of Head-on (including side swipe - opposite directions)
7. Number of Rear-end
8. Number of side-swipe (same direction)
9. Number of Right angle
10. Number of Other
11. Number of Property damage only accidents
12. Number of Injury accidents (excluding fatal accidents)
13. Number of Fatal accidents

The total of Fields 6 - 10 is the same as Field 5.

The total of Fields 11 - 13 is the same as Field 5.

B. One sequential file containing records relating location codes and location fields.

1. Location code (sequence)
2. Location description

Further detailed information about coding of these fields may be obtained from S.A. Highways Department manual *RMLIB, In Accident Retrieval Library.*

Figure 2 Plot of Actual Numbers of Accidents at 729 South Australian Sites; 1977 vs 1976

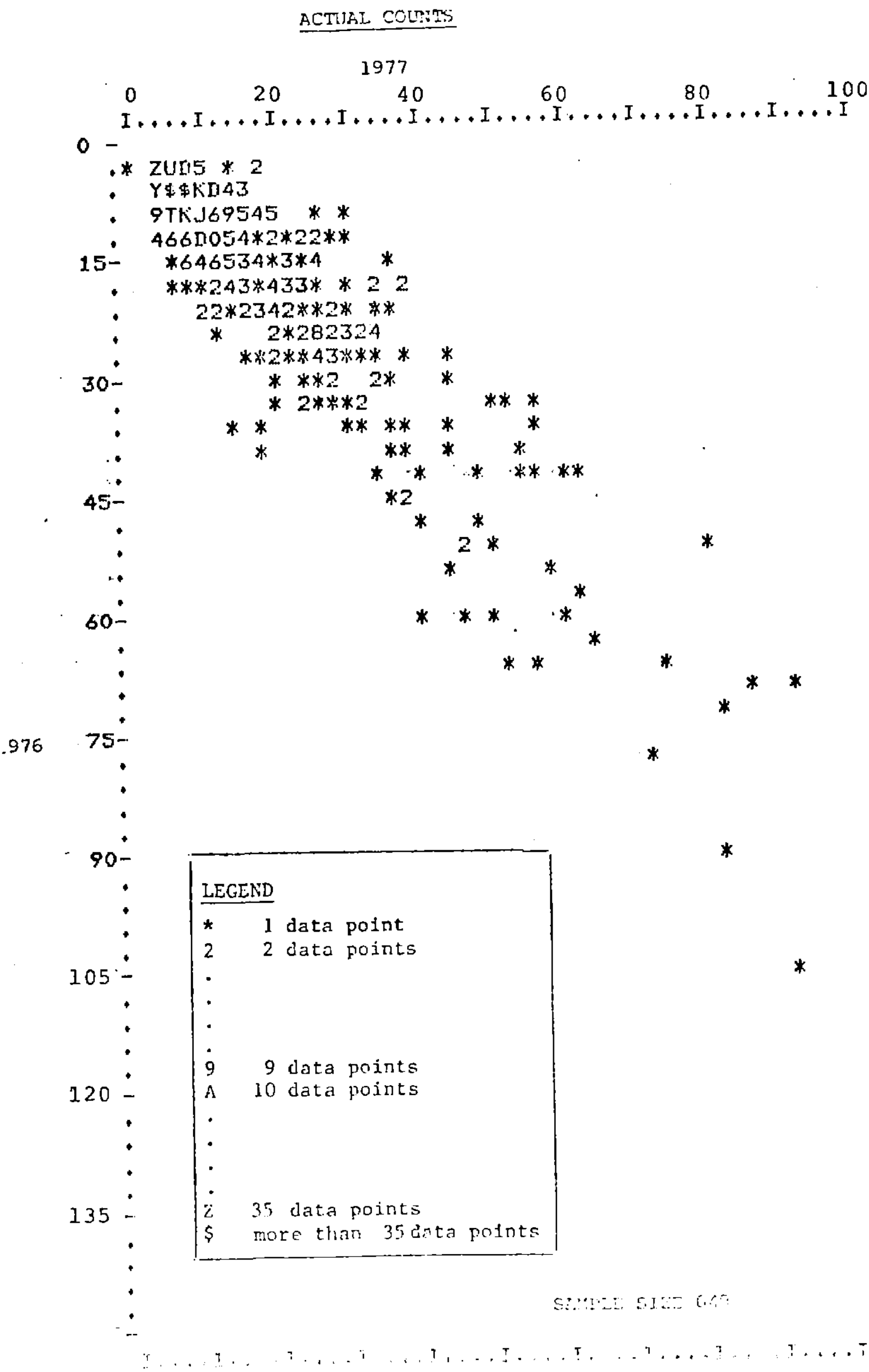


Figure 3 Plot of Actual Numbers of Accidents at 653 South Australian Sites 1976 vs 1975

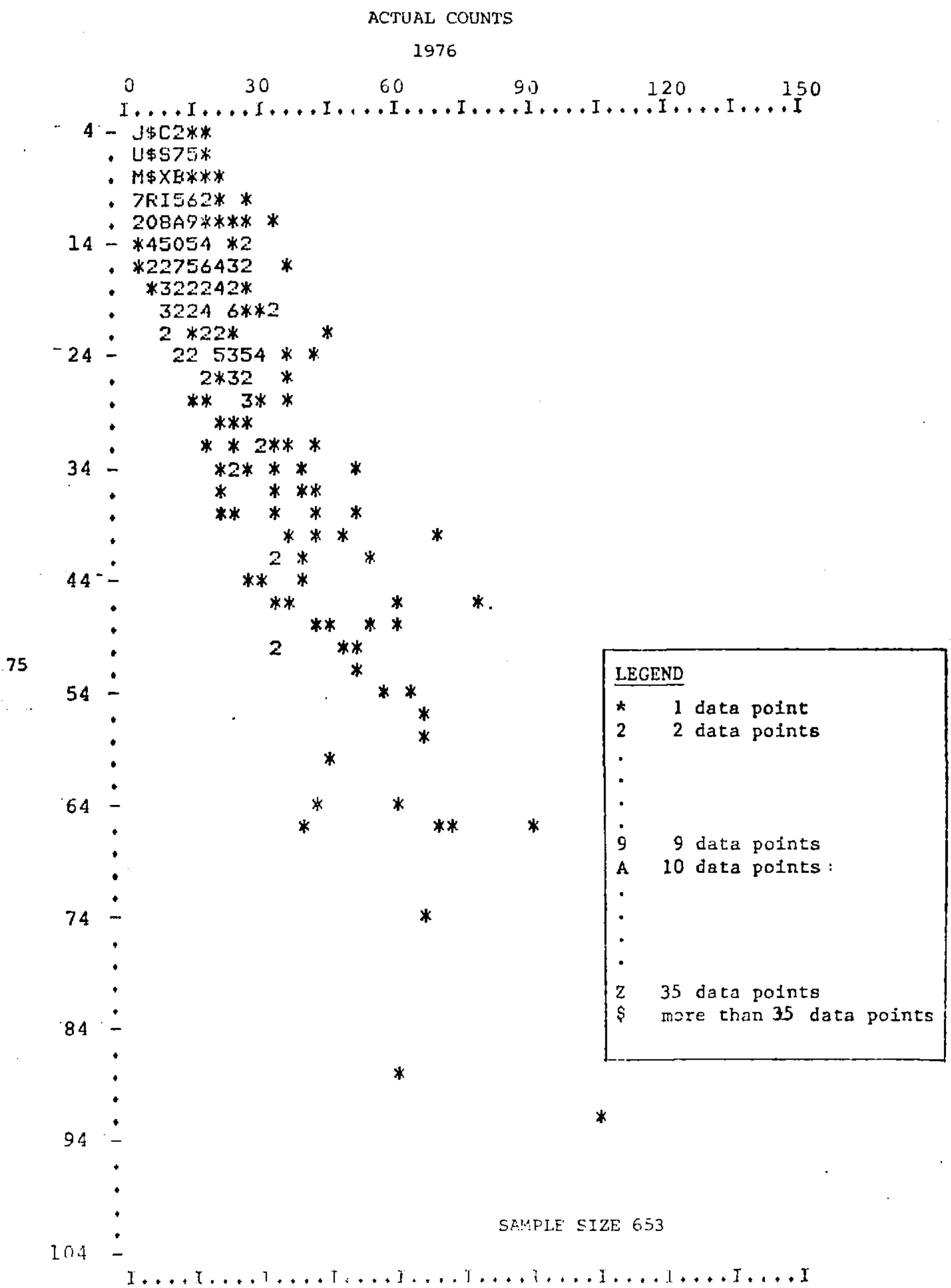


Figure 4 : Plot of Actual Numbers of Accidents at 649 South Australian Sites; 1977 vs 1976

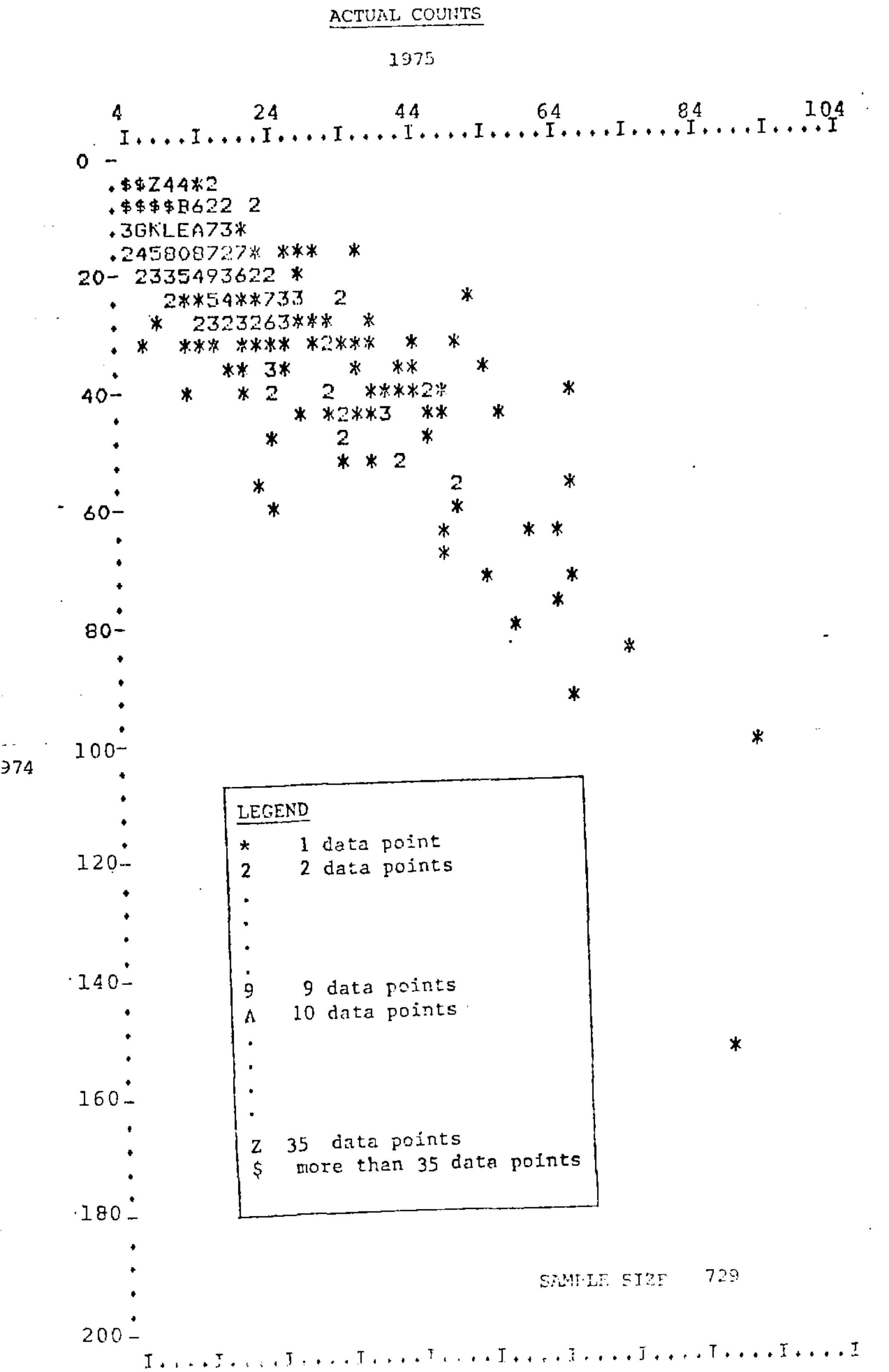


Figure 5 : Plot of Square Root of Accident Numbers at 649 South Australian Sites: 1977 vs 1976

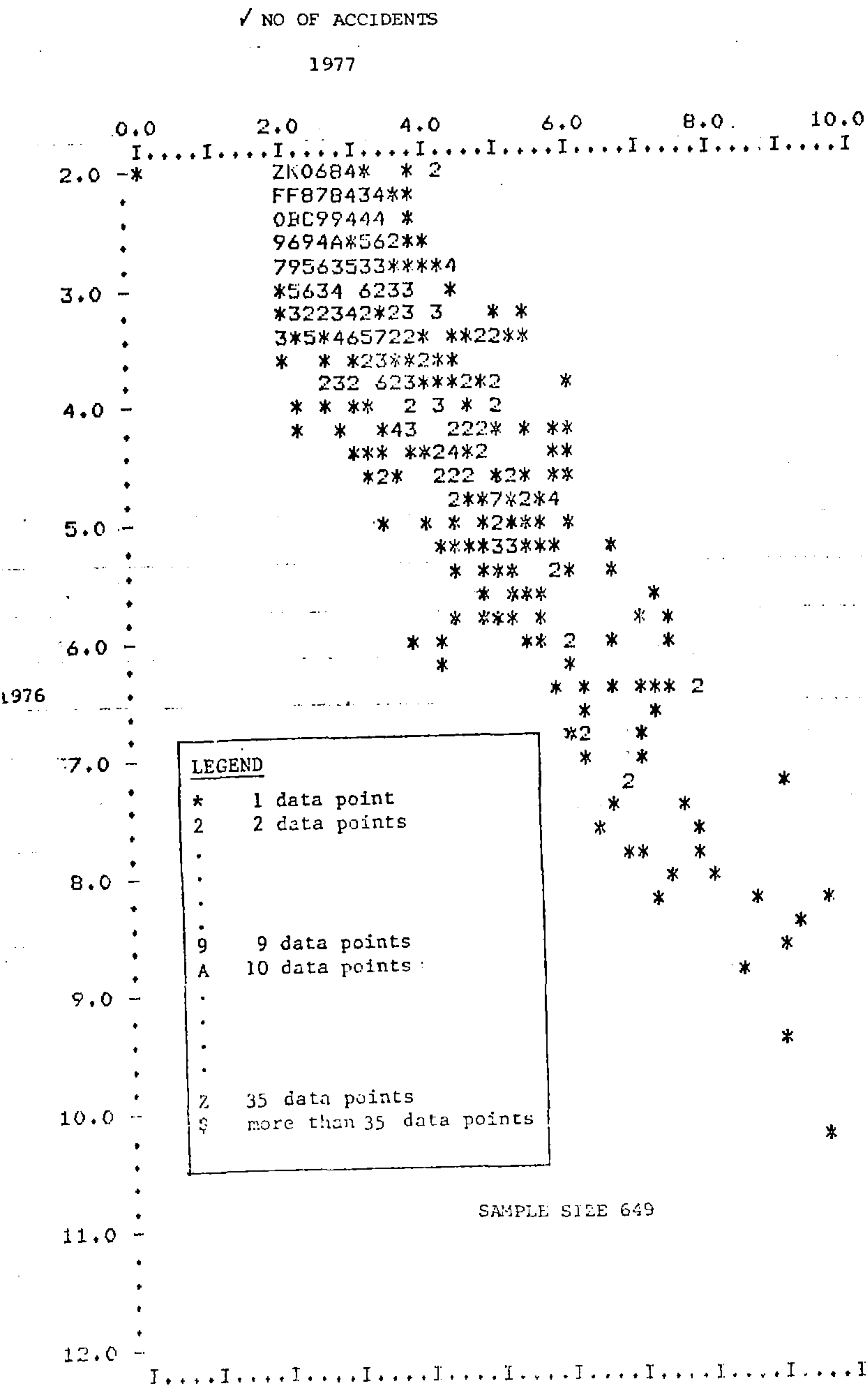


Figure 6 Plot of Square Root of Accident Numbers at 653 South Australian Sites: 1976 vs 1975

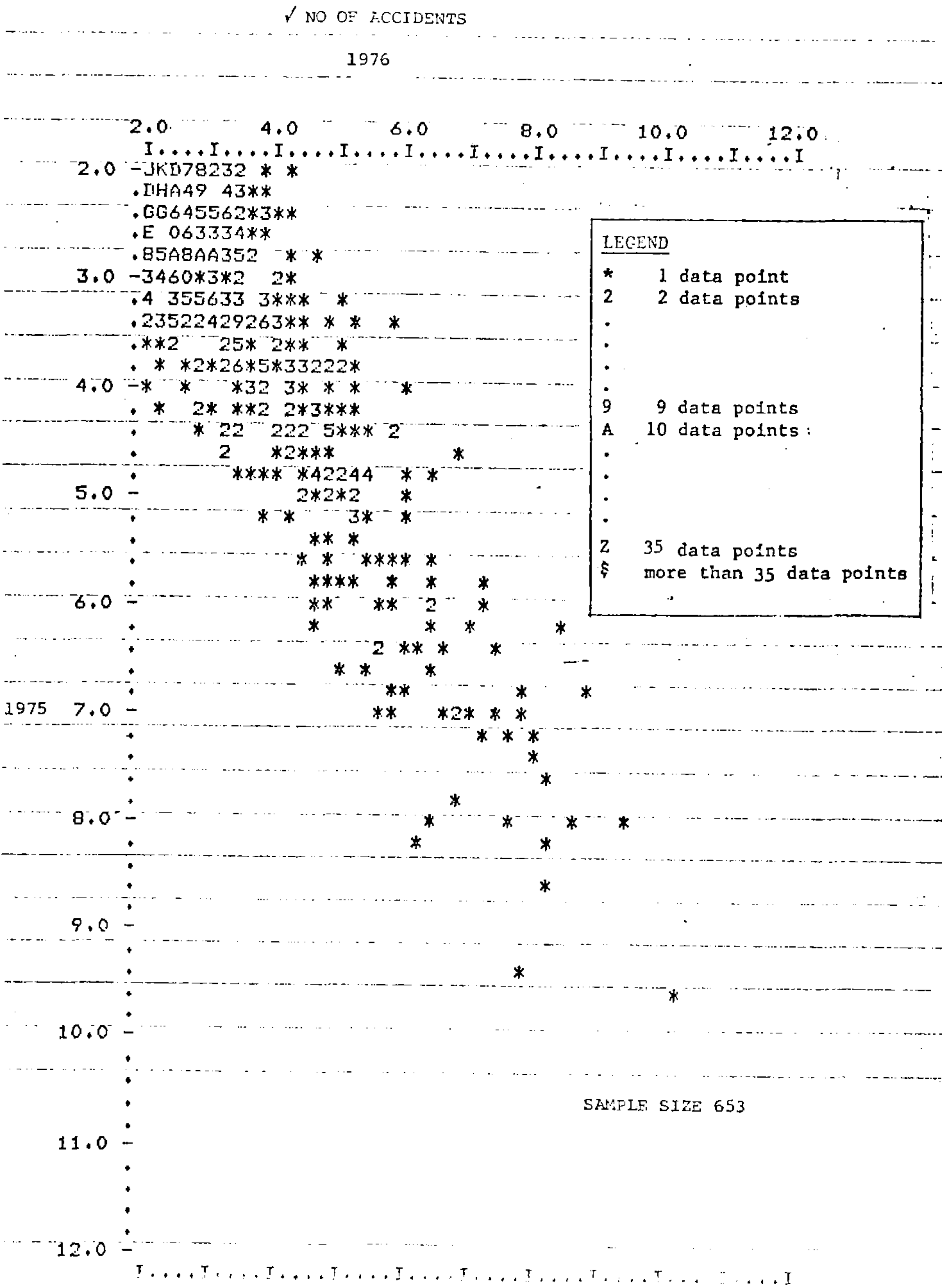


Figure 7.[illegible]: Plot of Square Root of Accident Numbers at 729 South Australian Sites: 1975 vs 1974

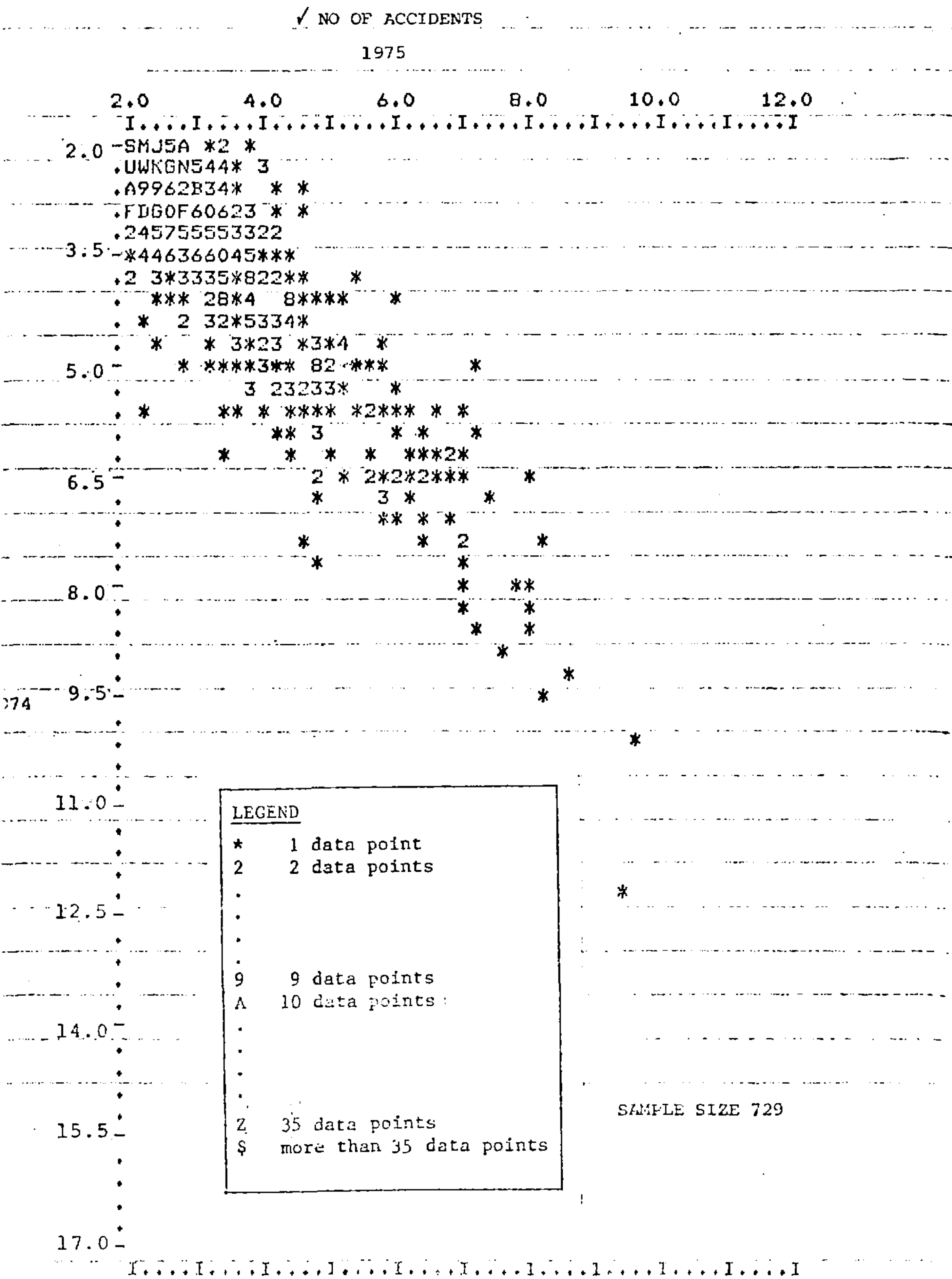


Figure 8 : Plot of Square Root of Accident Numbers at 261 South Australian Sites: 1974 vs 1973

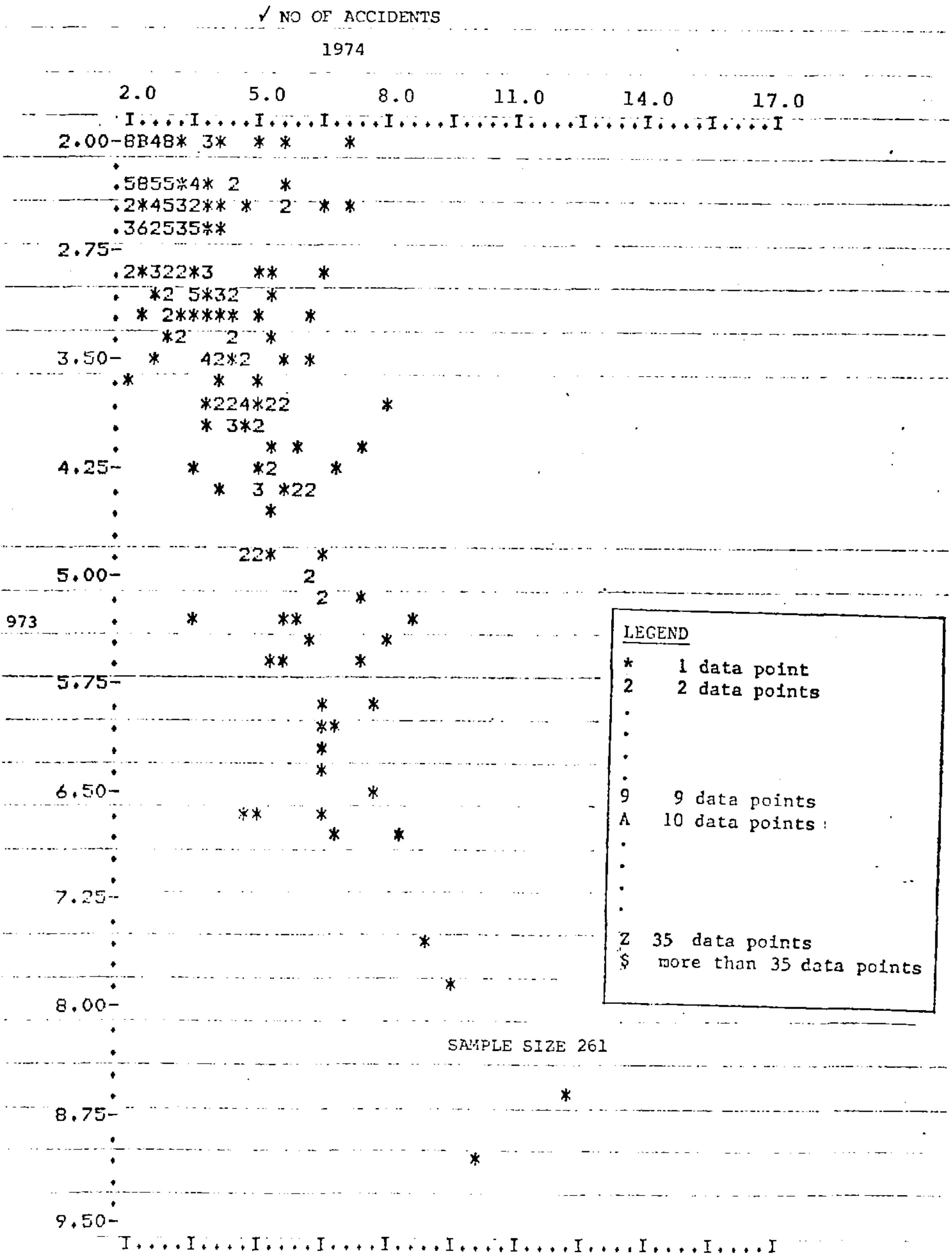
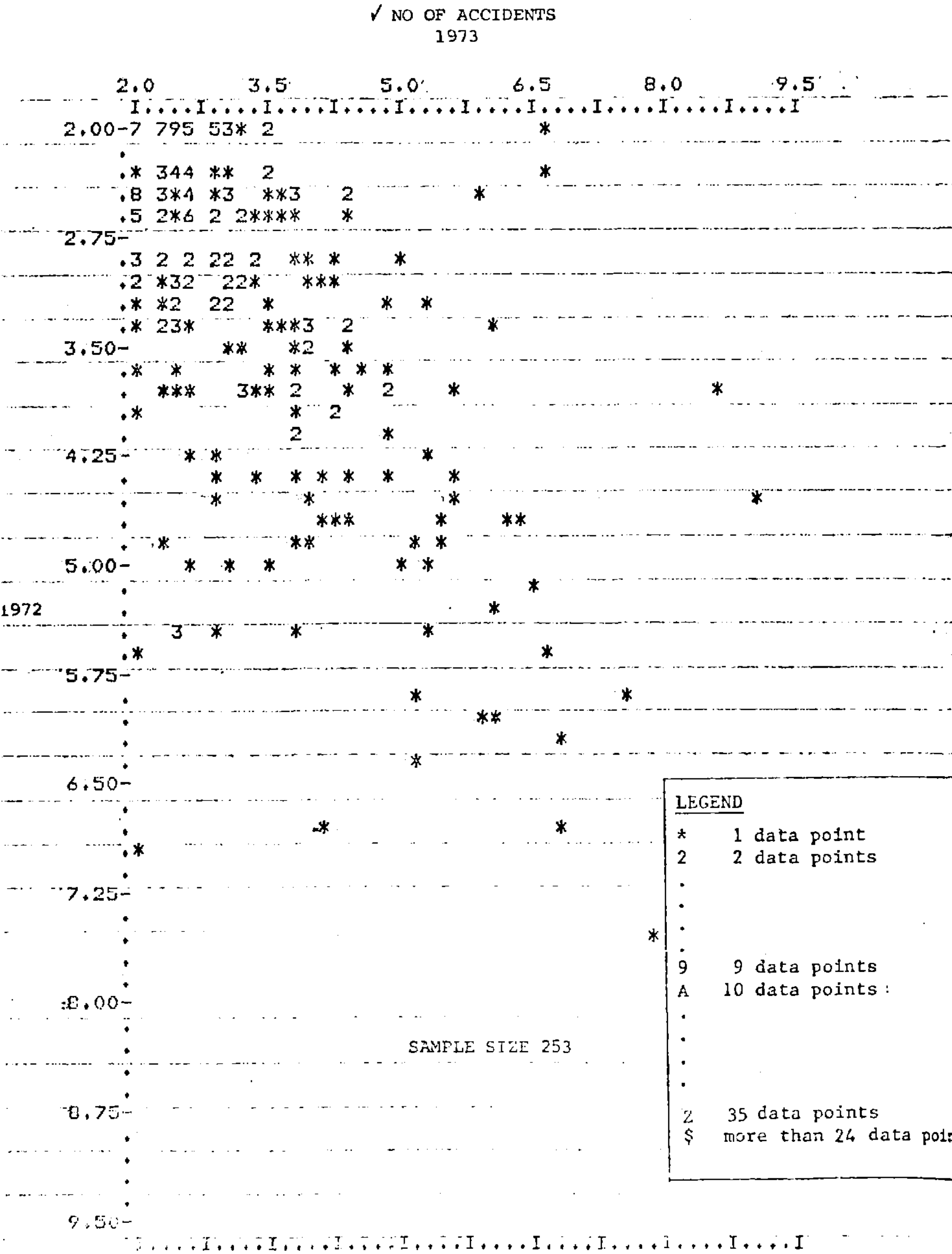


Figure 9 : Plot of Square Root of Accident Numbers at 253 South Australian Sites; 1973 vs 1972



BLE 2: Results of Tests of S.A. Data

			Dep 1977 vs	Indep 1976	Dep 1976 vs	Indep 1975	Dep 1975 vs	Indep 1974	Dep 1974 vs	Indep 1973	Dep 1973 vs	Indep 1972
TUAL [illegible] n_1, n_2)	General Line	Slope	0.958	(.017)	.943	(0.017)	0.758	(0.016)	1.273	(0.025)	0.688	(0.026)
		Intercept	1.29	(0.33)	1.04	(0.33)	1.98	(0.31)	3.47	(0.55)	4.81	(0.58)
		Standard error	5.785		5,837		5.424		10.407		9.953	
	Line Through Origin	Slope	1.006	(0.015)	0.984	(0.015)	0.828	(0.014)	1.421	(0.025)	0.942	(0.028)
		Standard error	5.850		5,875		5.598		10.655		10.362	
$\sqrt{n_1}, \sqrt{n_2}$)	General Line	Slope	0.930	(0.019)	0.912	(0.019)	0.798	(0.018)	1.035	(0.03)	0.635	(0.03)
		Intercept	0.31	(0.07)	0.32	(0.07)	0.54	(0.07)	0.66	(0.12)	1.291	(0.13)
		Standard error	0.683		0.679		0.641		1.040		1.105	
	Line Through Origin	Slope	1.008	(0.017)	0.993	(0.018)	0.930	(0.017)	1.209	(0.03)	0.988	(0.03)
		Standard error	0.693		0.698		0.676		1.066		1.185	
	SAMPLE SIZE		649		653		729		261		253	

Note: Standard errors of estimates shown in brackets.

CHAPTER 5

SUMMARY AND FINAL REMARKS

The aim of the study described in this report was to determine the feasibility of an evaluation of the effectiveness of MITERS-type projects in Australia.

A review was made (Chapter 2) of the data available in each State relating to:

- accidents
- MITERS-type projects
- exposure (traffic flows)

It was concluded that only in South Australia and Western Australia are the accident data bases in a suitable form for immediate evaluation. The New South Wales accident data base would also be suitable, if some largely manual work were carried out to improve the location coding of recorded accidents.

The number of different projects in South Australia and Western Australia able to be used in an evaluation is relatively limited. For many types of projects the number of accidents recorded is simply too low to allow meaningful statistical tests to be made. Nonetheless, projects which will yield to evaluation do account for most of the expenditure of these States on MITERS-type improvements.

Exposure data is available in South Australia and Western Australia in the form of AADT's. These traffic estimates are not as accurate, nor as relevant as would be desired, but they will allow the effect of changes in accident exposure to be incorporated in the evaluation.

The statistical tests proposed (Chapter 3) for use in the evaluation study follow fairly standard procedures. The proposed method involves a before-and-after type of approach, and the tests aim at detecting differences between accident occurrence before and after implementation of projects. Application of the tests was demonstrated in a trial analysis (Chapter 4) of some South Australia projects. These were intersection signal installations for the year 1975/76. Time available did not allow a complete evaluation to be undertaken (that is, using all the techniques described in Chapter 4) but it is clear that valuable information concerning project effectiveness will be able to be gained from a complete evaluation of suitable MITERS projects in South Australia and Western Australia.

The suggested tasks and timing of this evaluation study are given in the Summary report.