MRI

EVALUATION OF THE TRAFFIC CONFLICTS TECHNIQUE

FINAL REPORT
December 1975

Contract No. DOT-FH-11-8121

MRI Project No. 3825-E

For

U.S. Department of Transportation
Federal Highway Administration
Office of Research and Development
Washington, D.C. 20590

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DISCLAIMER

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by

J. C. Glennon
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I. INTRODUCTION

Traffic accidents are the ideal measure of safety for a highway location. But attempts to estimate the relative safety of a highway location are usually fraught with the problems associated with the unreliability of accident records and the time required to wait for adequate sample sizes. For these reasons, the Traffic Conflicts Technique (TCT) was developed as a surrogate measure in an attempt to objectively measure the accident potential of a highway location without having to wait for an accident history to evolve.

The TCT was originally developed by the General Motors Research Laboratories (GMR) in 1967.[1/] It was conceived as a systematic method of observing and measuring accident potential. Conflicts were defined as the occurrence of evasive vehicular actions and characterized by braking and/or weaving maneuvers.

Based on the positive results of a large investigative study by the Federal Highway Administration (FHWA), reported by Baker[2/] in 1971, the TCT has gained popularity as an evaluative tool. The Washington State Department of Highways is using the TCT as a diagnostic tool to determine appropriate countermeasures at high-accident locations. Others have suggested the technique as a priority ranking criterion for programming the order for the implementation of spot improvements. And, the FHWA has incorporated the TCT as a research tool into several recent problem statements in its contract research program.

This report critically evaluates the state-of-the-art of the TCT and the results[3/] of recent attempts to develop a rigorous experimental design using traffic conflicts as the basic response variable to measure the effectiveness of access control techniques at commercial driveways.

II. DESCRIPTION OF THE TRAFFIC CONFLICTS TECHNIQUE

The GMR procedures defined a traffic conflict as either an evasive action of a driver or a traffic violation. Evasive actions are evidenced by brake-light indications or weaving maneuvers (lane changes) forced on a driver by an impending accident situation or a traffic violation. Traffic violations are recorded as conflicts, regardless of the presence of other vehicles.

GMR defined the basic types of accidents at intersections as left-turn, weave, cross-traffic, red-light violation, and rear-end incidents. For these five basic categories, over 20 objective criteria were defined for specific conflict situations at intersections. A summary of the GMR traffic conflicts criteria is given in Figure 1.[1/] Following is a brief description of each of the five basic conflict types. Details may be found in GM Research Publication GMR-895, "GMR Traffic Conflicts Technique Procedures Manual,"[4/] as prepared by Stuart R. Perkins.

1. Left-Turn Conflict: This conflict is defined by a situation where a left-turn vehicle crosses directly in front of an opposing through vehicle, causing the through vehicle to brake or weave.

2. Weave Conflict: This conflict occurs when a vehicle changes lanes into the path of another vehicle, causing the offended vehicle to brake or weave to avoid an impending collision.

3. Cross-Traffic Conflict: This conflict is defined by a situation where a vehicle crosses or turns into the path of a through, right-of-way vehicle, causing the through vehicle to brake or weave.

4. Red-Light Violation Conflict: This conflict occurs when a vehicle enters the intersection and crosses the curb line on a red signal.

5. Rear-End Conflict: Generally, this conflict is defined by a situation where two vehicles are traveling as a pair and the first vehicle stops or slows unexpectedly as viewed by the following driver. The second vehicle is forced to take evasive action by braking or changing lanes. Rear-end conflicts can be initiated by previous traffic conflicts. In these cases, both the initiating conflict and the rear-end conflict are recorded.

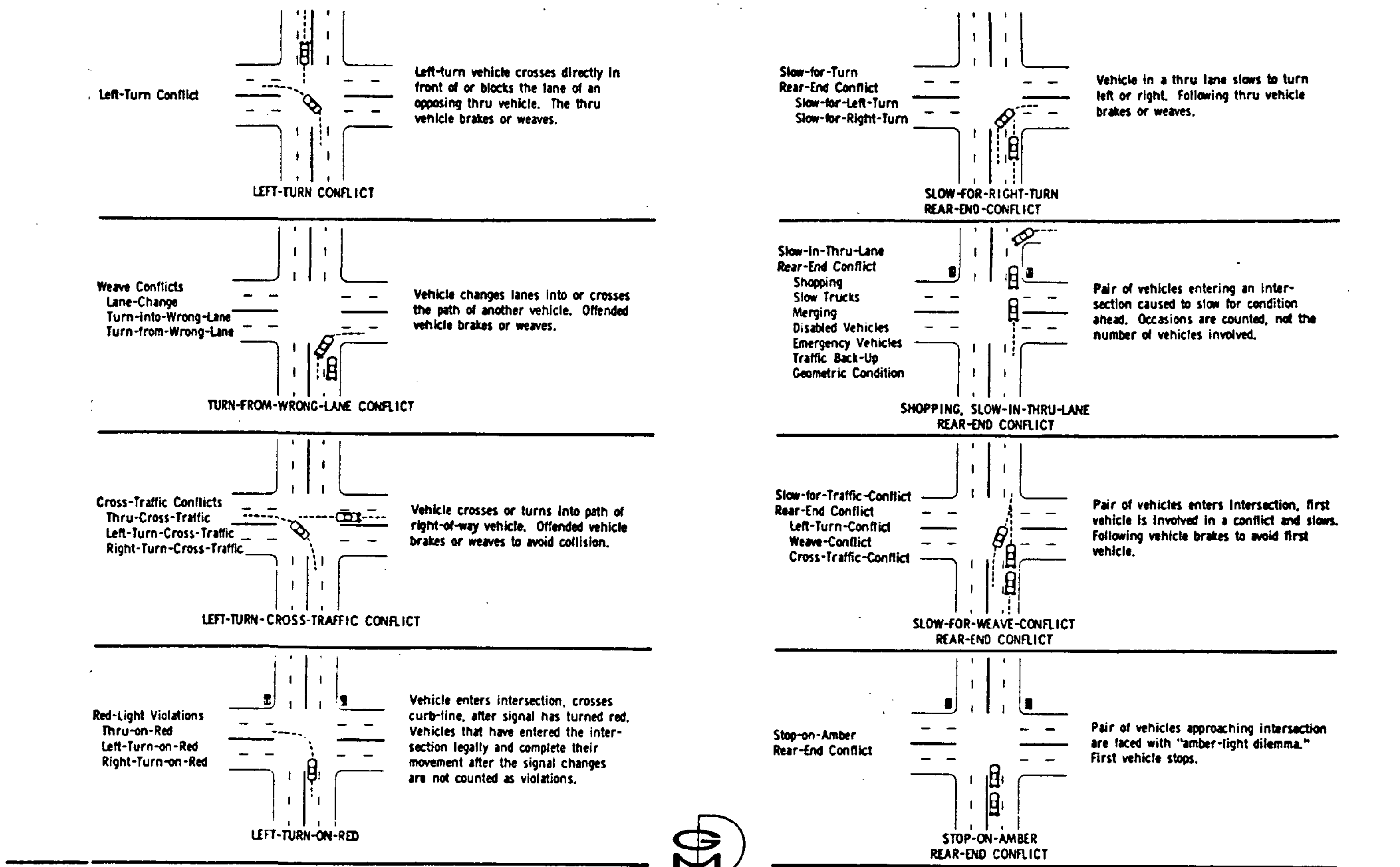

Figure 1 - Traffic Conflict Criteria

The major procedures and definitions used in the TCT are given below.

A traffic conflicts survey is a systematic 1-day surveillance of two opposite intersection approach legs. Observations are recorded in a 10-hr counting day (0730-1200 and 1245-1815) by a team of two persons in a single vehicle. The team members observe one approach leg at a time, with one observer counting conflicts and the other observer recording volume data for all movements. Observations are taken with the vehicle parked on the roadside about 100-300 ft from the intersection. Fifteen-minute data samples are taken alternately on each intersection approach leg, allowing 15 min after each sample count to record the data and move to the opposite approach. Continuing through the counting day, the team alternately surveys the two approach legs. This procedure records 2-1/2 hr of data each day for each of the approach legs.

Six different data sheets are used in the TCT. These sheets include: (1) counter's inventory of existing highway features; (2) analyst's inventory of improvements; (3) conflict counts; (4) volume counts; (5) accident data; and (6) photographs. Example data sheets and descriptions of each are included in the "GMR Traffic Conflicts Technique Procedures Manual."[4]

III. CRITICAL REVIEW OF PREVIOUS TRAFFIC CONFLICT STUDIES

Many studies have been conducted in an attempt to establish the potential utility of the TCT. Study objectives have ranged from developing equations that predict accidents from conflict counts to using the TCT as a diagnostic tool to identify specific safety deficiencies of intersections.

This review traces the evolution of the conceptual elements of the TCT and examines the various ways it was expounded. Several of the major studies are discussed. Included in each discussion are the general objectives, basic procedures, analysis techniques, conclusions of the study, and a critical evaluation presented by the authors of this report.

A. Federal Highway Administration Evaluation

The TCT was evaluated by the Federal Highway Administration (FHWA) in cooperation with the State Highway Departments of Washington, Ohio, and Virginia. In addition to field testing the technique, an attempt was made to find a statistical relationship between traffic accidents and traffic conflicts. The technique was also used to determine if conflicts data provided information advantageous in determining the need for safety improvements.

In all three states, the data collection technique used was the same as that specified by GMR. A total of 886 intersection approach legs were counted before engineering improvements were made, while 420 approaches were counted after construction of the improvements. Counts were made at intersections already scheduled for improvement as a result of analyses based on accident experience. At least 1 month was allowed after completion of construction before the "after" counts were taken.

After all the data were collected, the FHWA, Office of Traffic Operations, performed statistical regression analyses to determine the relationships between conflicts and accidents. Correlation coefficients were calculated for rear-end, weave, left-turn, head-on, and cross-traffic conflict categories.

Also reported were the number of conflicts per 1,000 opportunities and the number of expected accidents per 100,000 conflicts. These ratios were specified for the weave, left-turn head-on, cross-traffic, and rear-end situations with the data stratified by signalized and nonsignalized intersections.

Conflicts at signalized and nonsignalized intersections were separated when checking the null hypothesis of no correlation between number of accidents and conflicts. Both the signalized and nonsignalized intersections were further classified as three-leg or four-leg.

Results of the FHWA analysis were reported by Mr. William T. Baker[2/] as follows:

1. The data compiled in the study tended to support the hypothesis that conflicts and accidents are associated. Table I lists correlation coefficients for the distinct conflict-accident situations, and Table II gives the conflict/opportunity and accident/conflict ratios for the different conflict-accident situations.

2. On the basis of the experience of the three states, it appears that safety deficiencies at intersections can be pinpointed more quickly and reliably using the TCT than using conventional methods.

3. The TCT may be particularly valuable at low-volume rural intersections where the accident reporting level is low.

4. The TCT, because of its usefulness in pinpointing intersection problems more precisely, should lead to lower-cost remedial actions.

5. The TCT can be applied with minor modification to locations other than intersections.

6. The effect of intersection improvements may be demonstrated from conflicts counts taken shortly after completion of a "spot improvement" type change.

7. The general surveillance information obtained during the conduct of conflict counts may be valuable in improving the overall operations of intersections.

TABLE I

FHWA CORRELATION COEFFICIENTS (r) FOR T AND 4-LEGGED RIGHT-ANGLE INTERSECTIONS

	Conflict-Accident Situation						
Intersection	Weave	Left-Turn Head-On	Cross-Traffic	Rear-End	All Maneuvers	Critical r	Sample Size
Signalized							
T	-0.207	-0.128	-0.170	0.075	-0.172	± 0.532	14
4-legged right-angle	0.360[a]	0.661[a]	0.209[a]	-0.018	0.410[a]	± 0.179	122
All[b]	0.402[a]	0.615[a]	0.136	-0.017	0.326[a]	± 0.160	157
Nonsignalized							
T	0.294[a]	0.432[a]	0.830[a]	0.410[a]	0.837[a]	± 0.205	94
4-legged right-angle	0.159	0.459[a]	0.602[a]	0.213[a]	0.653[a]	± 0.192	106
All[b]	0.276[a]	0.453[a]	0.655[a]	0.295[a]	0.671[a]	± 0.130	235
All combined[c]	0.356[a]	0.546[a]	0.429[a]	0.154[a]	0.458[a]	± 0.100	392

a Indicates statistical significance at the 5 percent level.

b Includes other intersection types such as skewed and multileg as well as T and 4-legged right-angle.

c Composed of all signalized and nonsignalized intersections.

TABLE II

FHWA CONFLICT/OPPORTUNITY AND ACCIDENT/CONFLICT RATIOS

Intersection	Conflict-Accident Situation			
	Weave	Left-Turn Head-On	Cross-Traffic	Rear-End
Conflicts per 1,000 opportunities				
Signalized 4-legged right-angle	51	28	15	29
Nonsignalized 4-legged right-angle	64	28	26	26
All combined[a]	65	28	25	25
Accidents per 100,000 conflicts				
Signalized 4-legged right-angle	7	20	56	3
Nonsignalized 4-legged right-angle	7	9	16	1
All combined[a]	6	15	20	3

a Includes other intersection types such as skewed, multileg, and T as well as signalized and nonsignalized 4-legged right-angle.

Evaluation: The FHWA report is very brief, basically covering only the gross correlation between conflicts and accidents. No attempt was made to account for site parameters other than intersection type and, therefore, even the significant correlations may not truly reflect a causal relationship. For example, it is reasonable to suppose that both accidents and conflicts are positively correlated to traffic volume. Further analysis of the FHWA data regarding the correlation of accidents and conflicts partialled on ADT is given in Section IV-A.

Most of the conclusions in the FHWA report are not supported by the data. Only the first conclusion that accidents and conflicts are associated is supported by the fact that most of the correlations are significant. Although these correlations are significant, they only explain a small portion of the total accident variability. Therefore, the conclusion that the TCT can pinpoint safety deficiencies and the other conclusions that follow from that are not statistically supported.

B. Ohio Conflicts Analysis Program

At the time that the FHWA research program ended, the Ohio Department of Transportation decided to pursue their own evaluation of the TCT. After several initial tests, results indicated that the predictions developed by FHWA were not suited for the Ohio data. Even though the Ohio data were utilized by FHWA in predicting accidents, R. D. Paddock and D. E. Spence[5/] state that the nature of the Washington and Virginia data biased the equations towards urban trends and ignored the Ohio data, which were basically rural.

Major emphasis in the Ohio program was directed to the possible relationship between intersection accidents and conflicts. Other applications of the technique, such as determination of operational problems and freeway analysis, were also considered. In addition, the utilization of additional parameters such as cross-street volume, percentage of commercial vehicles, etc., was expected to improve the prediction ability of the TCT.

During 1972 and the first half of 1973, the Ohio data base was enlarged to 611 approach legs. The additional data were obtained using a slight modification of the technique outlined by General Motors. In place of the usual two-man team, the survey crew consisted of only one observer, who recorded data on a count board. All other procedures were identical to those of GMR.

After enlarging the data base, a series of regression models was generated to determine the optimum means of projecting accidents within Ohio. As in the FHWA study, signalized and nonsignalized approaches were distinguished. The Ohio Department of Transportation concluded that an accurate accident prediction value per 2 years (AP2Y) was a function of one or more of the following variables.[5/]

1. ADT - Average Daily Traffic in 10,000's, as calculated from the conflict counts.

2. SPLIT - Ratio of the sum of the cross volume to the counted approach volume.

3. OPOPP - Opposing Conflict Opportunities (opportunity is a term used to denote the maximum number of potential conflicts--traffic volume).

4. RROPP - Rear-End Conflict Opportunities.

5. TTOPP - Total Conflict Opportunities.

6. OPCON - Opposing Conflicts.

7. CPT - Total of Conflicts/10 Opportunities.

8. OCPO2 - Square of Opposing Conflicts/10 Opportunities.

9. RATE - Accidents/2 years/10,000 ADT.

The resulting prediction equation for signalized approaches

$$\begin{aligned} AP2Y = {} & 1.16153 + 11.6345\ (ADT) - 0.0503\ (CPT) - 0.0321\ (RROPP) \\ & + 0.0387\ (OCPO2) + 0.0285\ (TTOPP) - 0.02255\ (OPOPP) \end{aligned}$$

The following prediction equation for unsignalized approaches

$$\begin{aligned} AP2Y &= 0.36 + RATE\ (ADT) \\ &= 0.36 + ADT\ [22.3568 + 17.773\ (SPLIT) - 36.7045\ (ADT^{1/2}) \\ &\quad - 1.6785\ (SPLIT^2) + 18.2544\ (ADT) - 0.0264\ (OPOPP) \\ &\quad + 0.8385\ (OPCON)] \end{aligned}$$

The prediction of accidents using the developed regression equations resulted in the following prediction errors:

Data Class	Number of Points	Prediction Error (Acc/Year) 50%	75%	95%
All Data	611	± 1.2	± 2.0	± 4.2
Signalized	220	± 1.5	± 2.4	± 4.6
Unsignalized	391	± 1.1	± 1.8	± 3.8

In addition to using the TCT in standard intersection analysis, Ohio was hopeful that the basic technique could be applied to freeway-gore areas, weave sections, and other areas of traffic conflict to evaluate flow problems and predict accidents. Thus, they felt the technique appeared to be well based in both theory and practice and deserved additional study and development.

As a result of the study, the investigators felt that the TCT was a viable accident prediction tool. They also concluded that accurate prediction equations could be generated with some additional development.

Evaluation: This is the only reported study where multiple regression of the variables has been attempted. But the conclusions must be interpreted in light of the analysis. In fact, the multiple regression equations are more sensitive to the various traffic volume counts (ADT, SPLIT, OPOPP, RROPP, and TTOPP) than the conflict counts (OPCON and CPT). Therefore, these equations do not establish the TCT as a viable accident prediction tool, but simply substantiate many earlier studies that document a high positive association between accidents and traffic volume measures.

C. Washington Conflicts Analysis Program

After completion of the FHWA study, the Washington State Department of Highways[6/] continued in the application and refinement of the TCT. Major effort and concern was centered in the following areas: (1) relating conflicts to accidents; (2) determining the reliability of the accident data; (3) determining the interrelationships between different types of conflicts and their contribution to the total number of conflicts; and (4) establishing priorities for corrective action on the basis of the number of conflicts at intersections.

The TCT was applied to 272 additional intersections. Prior to the data collection, several revisions were made in the general GMR survey procedures, as listed below:

1. One man is stationed in one vehicle to count conflicts and one man in another vehicle to make conventional manual turning movement counts (this eliminates Data Sheet D, Volume Counts).

2. Conflict counts are conducted on the day of the week that accident problems are most acute. Generally, the necessary information is gathered within a 4- to 6-hr period.

3. Conflicts are recorded by severity as follows: 1 is a routine conflict; 2 is a moderate hazard; and 3 is a near miss. Any operational problems or unusual events are also noted.

Of the 272 hazardous intersections where the conflicts technique was applied, only 240 were analyzed since accident data were not available at 32 intersections. The 240 intersections were grouped in four categories consisting of signalized and channelized, signalized only, channelized only, and nonchannelized and nonsignalized intersections. Within each category, five priority groups were established. Priority Group 1 was the top 15% of the hazardous intersections; Priority Group 2, the next 15%; Priority Group 3, 20%; and Priority Groups 4 and 5, 25% each. For example, if the number of conflicts per hour falls into Priority Group 1 for a particular intersection, corrective action should be taken as soon as possible.

The major tasks in the analysis phase included:

1. Calculating correlation coefficients between accidents and conflicts; conflicts were categorized by type and stratified by intersection type.

2. Computing the 3-year weighted total for mean-accidents and their standard deviation and also the mean and the standard deviation of conflicts per hour; these results were also stratified by intersection type.

3. Calculating the intercorrelations between the different types of conflicts and their relative contribution to the total number of conflicts.

4. Indicating the relationships between the unweighted number of accidents for the years 1970, 1971, and 1972 and also showing the relationship between each year and the 3-year total.

5. Relating conflicts per hour to an average number of unweighted accidents (3-year total) and classifying the results by priority group.

6. Developing a regression equation to predict accidents from the observed conflicts.

Based on the findings of this study, the Washington Department of Highways concluded that the TCT appeared to be a valuable tool for assessing accident potential. Although the accident-conflict correlations were rather low, as evidenced in Table III, Washington felt that the conflict data was beneficial in establishing priority groups which assured that accidents would be reduced when conflicts are lessened. The priority groupings are given in Table IV.

Another conclusion was that the lack of year to year consistency in accidents sets some practical limits to the probability of predicting accidents from conflicts. The year to year reliability of accident data in Washington ranged from 0.60 to 0.65. Finally, the researchers felt that more research on rating conflicts by severity would be beneficial.

Evaluation: As with the FHWA study, the Washington results only show a positive association between conflicts and accidents. The results do not indicate that conflicts will explain very much of the accident variability. Therefore, the conclusion that conflicts are a valuable tool for assessing accident potential is unsupported.

The results of Table IV seem to be encouraging, but are of little practical value because: (1) they probably also show that both conflicts and accidents have a high positive association with traffic volume; and (2) more important, they ignore the site parameters that could causally explain why the number of conflicts for some intersections in a group would vary significantly from the group average.

TABLE III

OHIO CORRELATION MATRIX--CONFLICTS VERSUS ACCIDENTS

Conflict Type	Intersection Type: Signalized, Channelized or Signalized and Channelized N = 108	Intersection Type: Nonsignalized and Nonchannelized N = 132	Intersection Type: Total N = 240
Opposing Left-Turn	0.06	0.25	0.13
Weave	0.54	0.36	0.53
Rear-End	0.38	0.43	0.39
Cross-Traffic	0.16	0.39	0.23
All Conflicts	0.38	0.49	0.42

TABLE IV

OHIO PRIORITY RANKING OF TOTAL ACCIDENTS BY TOTAL CONFLICT GROUPS

Priority Group	Conflicts per Hour	Number of Intersections	Average Number of Unweighted Accidents, 3-Year Total
1	40 and over	39	23.4
2	27 to 39	38	15.8
3	19 to 26	34	14.0
4	10 to 18	55	9.2
5	9 and under	74	6.3
		240	

The other conclusion regarding the reliability of accident records has some interesting implications. This lack of reliability does not necessarily mean that conflicts are a poor measure of safety, but that if they are reliable measures it is difficult to determine because of the necessary reliance on accidents records to show the correlation with conflicts.

D. Transport and Road Research Laboratory Evaluation

The results of three studies[7-9] of the Transport and Road Research Laboratory examined the TCT as a method for assessing the safety of intersections. But, B. R. Spicer, author of the reports, criticized the GMR Traffic Conflicts Technique on the basis that recording all conflicts, without grading them by the severity of the event, gives results more highly correlated to a flow count of all intersection maneuvers than to accidents. Therefore, these three studies were undertaken in an effort to extend the conflicts method, classifying conflicts by severity and correlating them with actual accidents.

The techniques used to measure conflicts differed considerably from those outlined by GMR. Each site was visited by a team of observers with one person stationed at each leg of an intersection. The average observation day was 10 hr (0800 to 1800). Conflicts were recorded when and where they occurred, the type and number of vehicles involved, the action taken by the drivers such as weaving, sounding horn, flashing headlights, etc., and any other relevant information.

In all studies, a 16-mm camera, running at a speed of 2 frames/sec, was mounted on a tower. The film provided a continuous record of the events taking place at the intersections, and provided additional data for the observers' report. The film allowed a before, during, and after study to be made of each conflict. The involvement of other vehicles, a flow count in all directions, and measurements of maneuver and delay time could also be determined from the film. Speed measurements were made of vehicles on the roads approaching the junction using radar speed meters.

After reviewing the written reports and the film, all conflicts were identified and graded by the severity of the event, according to the following criteria:

1. Precautionary braking or lane changing; collision very unlikely.

2. Controlled braking or lane changing to avoid collision, but with ample maneuver time.

3. Rapid deceleration, lane change, or stopping to avoid collision, resulting in a near miss situation. No time for steady controlled maneuver.

4. Emergency braking or violent swerve to avoid collision resulting in a very near miss situation or minor collision.

5. Emergy action followed by collision.

Only conflicts resulting from actions 3, 4, and 5 were classified as "Serious Conflicts."

For the first study,[7] data were collected at only one intersection. Serious conflicts were separated from the less severe conflicts to form two classifications--serious conflicts and all conflicts. These were further broken down by maneuver type (e.g., rear-end) and place of occurrence.

The number of accidents (from accident record files) and serious conflicts were then tabulated by time and also by location on the highway. Similar data were compiled for accidents and all conflicts. Accidents were limited to those involving personal injury.

Rank correlation coefficients were then calculated for the two sets, and significance was tested. Rank correlation coefficients were also calculated for traffic flow versus serious conflicts, all conflicts, and accidents, and these correlations were tested for significance. This was done to check the hypothesis that flow levels are not related to accidents or conflicts. The involvement of more than two vehicles was also assessed for both serious and nonserious conflicts. In addition, the approach speeds of vehicles were checked to find if any particular speed group of vehicles was being involved more often than others.

For the second study,[8] data were again collected at only one intersection. Generally, the same analysis procedure was adhered to as in the first study. In addition, the crossing behavior of a sample of drivers, categorized by age of driver, was analyzed.

In the third study,[9] data were obtained at six intersections. The field methods and data analyses were similar to the methods employed in the first study.

The following conclusions were reported by B. R. Spicer and pertain to the first study:

1. A simple definition of a conflict as a situation involving one or more vehicles in evasive action does not provide a measure of accident potential correlating closely with accident data.

2. A serious conflict is defined to occur when, to avoid a collision with another vehicle in its proximate path, a vehicle is caused to decelerate rapidly, swerve violently, or stop, leaving no time for normal controlled avoidance behavior. Using this definition of the serious conflict, results were obtained that correlate well with reported accidents in details of location and time.

3. Study of the circumstances before these serious conflicts revealed that in 75% of them, vehicles other than the two immediately concerned with the conflict were present.

4. The importance of relative speed in generating accident situations was studied, with no evidence found that the speeds of main road vehicles in conflict situations are significantly different from the speeds of nonconflict vehicles. (This does not imply that the average speed level of vehicles on the main road is not a factor in provoking conflicts and accidents.)

5. This study was successful in showing that a definition of serious conflict can be used to provide information on intersection safety quickly. It was also shown at this site that a 10-hr observation period provided relevant information that would have required 5 years of reported accident records. The value of this technique in indicating important situations leading to accidents has been shown.

In addition to conclusions 1 and 2 above, Spicer reported that the following points were supported by the second study.

1. The relative frequencies of various types of conflict, e.g., blocking the median, overtaking vehicles, follow-out vehicles, etc., mostly substantiate those found in an earlier study at another intersection of similar design.

2. The study of vehicle maneuvers before and during serious conflicts revealed that vehicles other than the two immediately concerned in conflict were present in over 60% of conflicts studied.

3. The conflict and accident rate at this intersection increase with increasing vehicle flow.

4. The younger drivers on the major road and older drivers coming from the minor road are more likely to be involved in accidents at the intersection than other age groups.

5. The study of vehicle conflicts at this intersection has led to an assessment of the effect of vehicle speed, time of crossing, and the crossing path taken, as factors in accident causation.

In the third study, conclusions 1 and 2 of the first study were supported. The following results were also reported by Spicer.

1. For the six intersections studied, the number of serious conflicts observed was proportional to the number of injury accidents recorded. This relationship is shown in Figure 2.

2. At three sites, including the one used in the earlier study, the location of the conflicts identified the location, in order of importance, of the reported injury accidents. At the other three sites, the locations tended to be related, but numbers were too small to firmly establish the relation.

3. At each of the sites studied, at least 54% of serious conflicts involved the presence of more than the two vehicles actually in conflict, and these additional vehicles were judged to have affected the possible maneuvers of the conflicting vehicles.

4. No clear relation was shown between traffic flow and the serious conflict or injury accident rates. The effect of vehicle flow and speed patterns on the conflict and accident rate appears to be complex.

5. The study was successful in showing that data on serious conflicts can provide information enabling the ranking of intersections in order of safety. Ten-hours observation at each site has provided complementary relevant data to the 3-years reported accidents, to indicate the important situations leading to conflicts and accidents.

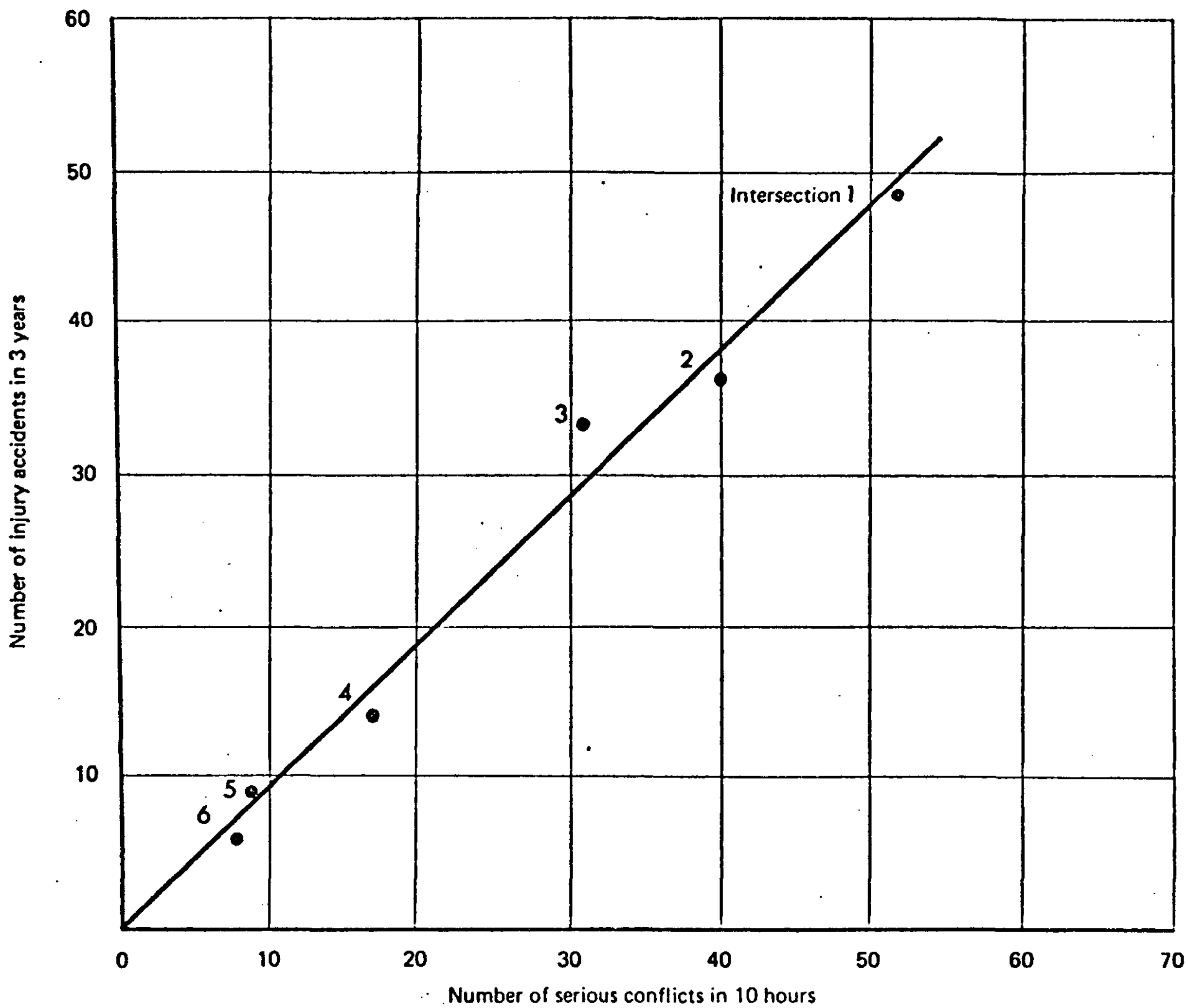

Figure 2 - Number of Injury Accidents and Serious Conflicts at Six Intersections in Great Britian

Evaluation: This paper appears to indicate the most promising results to date in relating conflicts to accidents. Although the sample size of six intersections seems inadequate, the reliability of the regression fit cannot be disputed at face value. But based on the characteristics of the study and the seemingly tenuous results of other studies, the almost perfectly correlated 45-degree regression between 3-year accident records and 10-hr conflict counts seems unbelievable. This doubt stems from what appears to be a highly subjective determination of conflict occurrence, the widely different and unusually complex intersections used, and the illogical conceptual connection between serious conflicts and severe accidents. What Spicer terms a serious conflict would logically seem to be related to accident occurrence, but not necessarily to severe accidents.

This study draws essentially the same conclusion as the Washington study in that conflicts can be used to rank intersections in the order of safety. Again this capability has no practical advantage because: (1) intersections can be ranked as well using traffic volume measures, and (2) ranking intersections this way does not necessarily identify hazardous intersections (those that have a significantly higher number of accidents than the average for similar intersections).

Spicer also drew a conclusion that is analogous to one in the FHWA study, which said that conflict measures can pinpoint specific safety deficiencies. The derivation of this conclusion ("... the location of conflicts identified the location, in order of importance, of the reported injury accidents.") was not documented in the TRRL reports.

E. Ministry of Transport, Canada, Analysis and Evaluation of Predicting Intersection Accidents

This study[10/] was conducted during 1971 and 1972 by the Road and Motor Vehicle Traffic Safety Branch of the Ministry of Transport. The primary intent of the report was to summarize the state-of-the-art concerning the prediction and analysis of accidents at intersections. Consideration was also given to assessing the efficiency of various accident predictor models, especially the concept of traffic conflicts.

The Universities of Toronto, New Brunswick, and British Columbia were contracted by the Ministry of Transport to collect data. Analysis of the data was then performed by the Road and Motor Vehicle Safety Branch.

A pilot investigation of the TCT was conducted in 1971 on three intersections in the Ottawa area. The primary objective was to investigate the definitions and observational techniques of the TCT. Results of the pilot study included the development of an Intersection Studies Manual for training observers and the selection of a conceptual definition of a conflict which eliminated precautionary or anticipatory actions.

A continuation of the project was justified by the pilot study. It was determined to simultaneously study four Canadian cities. Some of the major changes in the GMR methods and procedures that were applied to this study included:

1. A study period of two, 14-hr days (counted in four 7-hr days) was selected for recording conflict data at an intersection. The 14-hr day (0700-2100) was used to insure full coverage of all traffic conditions. Only weekdays were used for data collection.

2. The study team consisted of four observers and a field supervisor. Each observer was assigned to a separate approach leg or intersection area.

3. Traffic volumes (separated by maneuver) were counted and recorded for one 14-hr period immediately following the conflict count. Violations were recorded in one 14-hr counting day and at the same time as conflicts.

4. Intersection exposure times for through vehicles and all types of maneuvers were sampled during morning peak, off-peak, and afternoon peak periods, either during the preliminary study at each intersection or during the period of volume counting.

5. The study was restricted to nonsignalized intersections in order to obtain a better sample of maneuver conflicts.

A total of 59 unsignalized intersections were used in the analysis including 13 "tee," 37 four-leg, two-way and 9 four-leg, one-way. Accidents and conflicts for each intersection were divided into five basic categories consisting of cross-traffic, left-turn, right-turn, weave and rear-end. Table V shows the conflict/opportunity,

TABLE V

ACCIDENT/OPPORTUNITY, CONFLICT/OPPORTUNITY AND CONFLICT/ACCIDENT RATIOS FROM CANADIAN CONFLICTS STUDY

Conflict or Accident Situation	Accidents/Million Opportunities	Conflicts/Million Opportunities	Conflicts/ Accident
Weave	3.50	8,737	2,496
Right-turn	0.43	3,400	7,907
Cross-traffic	6.32	15,700	2,484
Left-turn			
Main legs	1.47	9,560	6,503
Minor legs	0.04	825	20,625
All legs	0.82	5,450	6,646
Rear-end			
Main legs	0.20	1,210	6,050
Minor legs	1.30	4,160	3,200
All legs	0.25	1,340	5,360
Total	11.32	34,627	3,059

accident/opportunity, and conflict/accident ratios for each of the five categories.

The most significant part of the analysis consisted of testing the correlation between many of the variables selected for collection. Among the comparisons were: conflicts and accidents, accidents and time-volume exposure index, accidents and total volumes, conflicts and time-volume exposure index, accidents and conflicts by intersection, accidents and conflicts by time of day, accidents and all variables (multiple regression equation) and conflicts and violations. Results of these correlations are presented in Table VI.

Cooper concluded that the application of the concept of traffic conflicts was not very efficient or practical. But, by using the detailed observational techniques of conflict analysis, he felt that hazardous locations could be identified by noting their operational deficiencies through trained observance.

The following statements were also expressed by Cooper. In particular, conflicts were found to be very volume dependent and could not account for differences in accidents when corrected for volume exposure. Also, conflict definitions, depending on the degree of subjectivity (or objectivity), cause problems of either uneconomical and impractical collection procedures or poor results in predicting accidents. Cooper felt the need to obtain a measure (definition) that is both efficient and repeatable. Some measure of volume exposure, rather than conflicts, resulted in better correlation when ranking intersections by gross accidents. But, the application of the TCT in identifying high-accident areas within an intersection did appear beneficial. The use of time exposure in conjunction with cross-volume products was generally found to be superior to conflicts. No parameters or combinations were sufficiently reliable when identifying specific problem locations or accident causation at an individual intersection. Finally, no definite conclusions could be made concerning traffic violations.

Evaluation: Cooper appears to have correctly interpreted his results by saying: (1) traffic conflict measures were neither efficient nor practical; (2) conflicts were very volume-dependent; and (3) the degree of objectivity in the conflicts definition causes a tradeoff between the cost and practicality of collection procedures and the precision of accident prediction.

TABLE VI

LINEAR CORRELATION COEFFICIENTS BETWEEN STUDY VARIABLES FROM CANADIAN CONFLICTS STUDY

INTERSECTION GEOMETRICS

	4 leg (All 2-way)			4 leg (Two 1-way)			All 4-leg			Tee			All		
	Sample Size	Linear Correlation Coefficient	Level of Significance	Sample Size	Linear Correlation Coefficient	Level of Significance	Sample Size	Linear Correlation Coefficient	Level of Significance	Sample Size	Linear Correlation Coefficient	Level of Significance	Sample Size	Linear Correlation	Level of Significance
All Accidents vs All Conflicts	37	0.471	1%	9	-0.246	NS	46	0.407	1%	13	0.473	NS	59	0.453	1%
Manoeuver Accidents vs Manoeuver Conflicts	37	0.461	1%	9	-0.169	NS	46	0.376	5%	13	0.434	NS	59	0.402	1%
Weave Accidents vs Weave Conflicts	37	0.170	NS	9	-0.158	NS	46	0.157	NS	13	0.175	NS	59	0.220	NS
Left-Turn Accidents vs Left-Turn Conflicts	37	0.137	NS	9	0.099	NS	46	0.179	NS	13	0.480	NS	59	0.332	5%
Right-Turn Accidents vs Right-Turn Conflicts	37	0.353	5%	9	-0.183	NS	46	0.299	NS	13	0.766	1%	59	0.494	1%
Cross Traffic Accidents vs Cross Traffic Conflicts	37	0.340	5%	9	-0.143	NS	46	0.273	NS	13	0.414	NS	59	0.297	5%
Rear End Accidents vs Rear End Conflicts	37	0.133	NS	9	-0.467	NS	46	0.193	NS	13	0.434	NS	59	0.344	5%
Pedestrian Accidents vs Pedestrian Conflicts													13	0.705	1%
All Accidents vs Time-Volume Indices	37	0.682	1%	9	0.485	NS	46	0.651	1%	13	0.840	1%	59	0.620	1%
Manoeuver Accidents vs Time-Volume Indices	37	0.654	1%	9	0.445	NS	46	0.587	1%	13	0.798	1%	59	0.488	1%
All Conflicts vs Time-Volume Indices	37	0.587	1%	9	-0.062	NS	46	0.587	1%	13	0.474	NS	59	0.503	1%
Manoeuver Conflicts vs Time-Volume Indices	37	0.638	1%	9	-0.034	NS	46	0.629	1%	13	0.686	1%	59	0.582	1%
All Accidents vs Total Volumes	37	0.704	1%	9	0.380	NS	46	0.653	1%	13	0.671	5%	59	0.670	1%
Time-Volume Indices vs Total Volumes													59	0.685	1%
Volume Indices (without time factor) vs Accidents	37	0.657	1%	9	0.516	NS	46	0.617	1%	13	0.615	5%	59	0.569	1%
All Accidents vs Violations	37	0.137	NS	9	-0.067	NS	46	0.098	NS	13	-0.004	NS	59	0.098	NS
All Conflicts vs. Total Volumes													59	0.570	1%
All Accidents/Volume vs. All Conflicts/Volume													59	-0.078	NS
All Accidents/Volume vs. Total Volumes													59	-0.357	5%
All Accident/Volume vs. Time - Volume Indices													59	-0.175	NS
All Accidents (Weekdays, 7 am - 9 pm, April-Oct.) vs All Conflicts													59	0.436	

Cooper also drew essentially the same conclusion as the FHWA and TRRL studies by saying that the application of conflicts measures appears beneficial for identifying high-accident areas within an intersection. As with the other reports, this report does not quantitatively document the derivation of this conclusion.

F. A Discussion Paper on the Fundamental Issues of the Traffic Conflicts Technique

A study[11/] conducted by the Department of Civil Engineering, University of Toronto was mainly interested in determining under what circumstances the TCT could generate more reliable accident rate estimates than those obtained from an accident history. Expressions of the variance for both methods were derived to aid in providing answers to this question. The variability of the "accident-to-conflict" ratio was also examined, based on several applications of the TCT by others. The analysis furnished answers to the aforementioned question, and guidelines were established concerning the conduct of traffic conflict studies.

The first major task involved estimating the variance of the expected annual accident rate, derived from both traffic conflicts and accident records. The estimation method that produced the greater variance could then be labeled inferior. Estimating the variance using accident records was based on theory and also the accident records for 1,800 intersections in Toronto during 1970-1974. An expression for the variance using traffic conflicts was based on the ratio of accidents to conflicts and its variability.

Results of several applications of the TCT were analyzed to obtain estimates of the accident-to-conflict ratio and its variability. An equation was developed for the variability by a least squares fit.

Graphs were produced to aid users in determining whether the TCT would be beneficial for their purposes. These graphs are based on several parameters including: accident-to-conflict ratio and its variability, years of available accident history, annual accident rate, conflict count duration, and expected variance.

Hauer concluded that the TCT is more accurate than accident records in predicting the expected annual accident rate at locations with fewer than three or four accidents per year and/or when the accident history is very short. Conflict count duration also seemed to have a limited effect on the accuracy of estimation. He stated that an economical and practical choice appeared to be one day. Finally, the study indicated that if a single operational definition of conflict is desired, the use of a restrictive definition would be more beneficial. Such a definition was thought to be applicable to locations of relatively high-accident rate while still working reasonably well for low-accident rate situations. But, Hauer thought a more desirable practice is to adopt two operational definitions for conflicts; one for low accident rate conditions and the other for locations characterized by a high number of annual accidents.

Evaluation: Due to the gravity of seemingly clearcut and practical conclusions of this paper, an in-depth analysis of its theoretical bases was warranted. In reviewing the paper in-depth, many theoretical contradictions and tenuous assumptions were apparent. The discussion below highlights some of these problems.

In developing the estimate for the variance of the expected annual accident rate derived from accident records, the basic model proposes to regard the observed annual accident frequencies, a_i , at a site as n random samples of size 1 from n independent Poisson densities characterized by the expected annual accident rates, λ_i . In this model, the mean, $\bar{a}$, of observed frequencies is assumed to be an unbiased estimate of the current expected accident rate, say λ_{n+1} . But this assumption contradicts a justification made earlier in the paper stating ". . .only a fraction of those [accidents] is reported."

In estimating the value of $E[(\bar{a} - \lambda)^2]$, λ is initially defined as the current expected accident rate, λ_{n+1} , and later defined as the mean, say μ_λ , of past expected accident rates, λ_i . Also the derived working equation uses λ to mean μ_λ in one term and $\bar{a}$ in another term, although a numerical example claims that the current expected accident rate, λ_{n+1} , is being described. All of this confusion appears to stem from the claim that $E[\Sigma\lambda_i/n] = \lambda_{n+1}$, when in fact, $E[\Sigma\lambda_i/n] = E[\lambda_{n+1}]$ instead.

The tenuous nature of the estimate of variance for expected annual accident rate derived from accident records is verified by Hauer's own numerical example that attempts to illustrate its practical use. This example calculates a variance of 9 for a density with a mean equal to 10. This value is unrealistically low since any reliable estimate of the variance should be greater than the value of 10 calculated when the same Poisson density exists every year.

In developing the estimate for the variance of the expected annual accident rate derived from traffic conflict counts, several additional inferences seem misguided. This estimate requires inputting values for the accident/conflict ratio, p , and its variance, both of which must be determined from empirical data. This assumes that conflicts (regardless of the attendent definition) are Poisson distributed, although empirical data does not necessarily verify this assumption. The application of the variance estimate also assumes, a priori, that there is a reliably predicted causal relationship between accidents and conflicts (again regardless of conflict definition) independent of site conditions. This assumption also is not necessarily borne out by empirical data.

Hauer's conclusion that the TCT is a better estimator than accident records when the expected annual accident rate is less than four (which, by the way, there is no clear way to estimate this value for a particular site) is based on the tenuous developments previously discussed and the use of a grand average for the variability in the accident/conflict ratio. This grand average is inferred from the widely different average values of several studies based on varying conflict definitions. As such, it appears to have little resemblence to reality.

Hauer's conclusion that using a more restrictive definition would make the TCT outperform the reliance on accident records in all practical cases, is the most contradictory part of his entire argument. This conclusion is not only based on the tenuous developments previously discussed, but also holds to the assumption that as the conflict definition is more restrictive (as p approaches 1), the prediction ability of the TCT improves. In other words, the ideal accident/conflict ratio equals one, where a conflict is synonomous with an accident. But this unsupported conclusion is logically equivalent to the contradictory conclusion that observing accidents is necessarily better than observing conflicts.

IV. EVALUATION OF TRAFFIC CONFLICTS AS AN EXPERIMENTAL RESPONSE VARIABLE

As a part of a larger study,[3] aimed at evaluating the cost-effectiveness of alternative techniques for controlling direct access on arterial highways, traffic conflicts were examined as a basic response variable. The objective was to develop detailed before-afger experi ental procedures, using conflicts measures, that would allow highway agencies to precisely quantify predictive accident reduction values for implementing various access control techniques under a variety of site conditions.

This report is a spin-off of the research effort described above. This section of the report describes the attempts to develop an experimental procedure for the purposes described above. Unfortunately, the lack of available data needed to estimate the population distribution characteristics of the conflict measure seriously limited the quality and utility of the developed experimental procedure.

A. Initial Development of Sample Size Requirements

The basic plan was to use conflicts recorded on the two highway approaches to the three-leg driveway intersection. By measuring conflicts before and after implementation of an access control technique for various site conditions, it was thought that the accident reduction effectiveness of the technique could be precisely quantified.

Data from the Ohio study[5] were selected to develop preliminary sample size requirements. Actually, this was the only raw data available to the research team at the time this task was started. The sample size requirement was the number of days of conflict data collection (both before and after an engineering change) that was necessary to detect a certain percentage reduction in predicted accidents with a desired confidence level. By predicting sample sizes, the practicality and economics of conducting field studies could be assessed.

The selected Ohio data included 38 unsignalized, t-intersections (which would be similar to driveway intersections). The data furnished a sample distribution of conflict observations (2-1/2 hr), where each observation represented a different intersection.

To relate conflicts to accidents, several linear regressions using (total) conflicts as a predictor of (total) accident rate were constructed from the data; specifically, accident rate = a + b (total conflicts) per approach, with approaches combined, and analogous functions with total conflicts normalized by approach ADT and approach ADT + crossroad ADT. In no case was the estimated prediction equation encouraging, i.e., the correlation coefficients (r) ranged from ~ 0 to a maximum of 0.27. Assuming the data to be reliable, there are only three interpretations of this result:

1. The "dependence" of accidents upon conflicts requires a more complex (nonlinear) model to be detected; and/or

2. Uncontrolled and unobserved variables in the data "wash out" the dependence of accidents upon conflicts because the dependence itself varies according to the values of these auxiliary variables; or

3. There is no worthwhile relationship between conflicts and accidents.

From these results, the decision was made to use the conflict measure itself as the basis for computing effectiveness measures. Although the lack of ability to predict accidents was highly undesirable, there seemed to be no alternative. Reinforcement of this decision was provided later when the raw data from the FHWA Conflicts Study[2] was obtained and analyzed. Although the FHWA study had reported a high correlation (84%) between conflicts and accidents at 94 unsignalized t-intersections, the subsequent analysis of the raw data performed in this study showed a much lower correlation coefficient (59%).

A simple correlation coefficient of 0.59 means that 35% (r^2) of the variability in accidents is "explained" by accounting for the effects of conflicts; i.e., the variance of the accident rates adjusted for conflicts is about two-thirds as large as the total variance of the accident rates.

This correlation coefficient of 0.59 is correct in the sense that it does describe how useful conflicts are in predicting accidents. But this coefficient may not be meaningful as a descriptor of the genuine (cause and effect) relationship between conflicts and accidents because both conflicts and accidents may themselves be correlated to other site variables rather than genuinely related.

For example, in the FHWA data, total ADT was also observed at each site, and it is reasonable to suppose that both accidents and conflicts are positively correlated to traffic volume. Therefore, the partial correlation coefficient between accidents and conflicts, partialled on ADT ($r_{ac \cdot v}$), is a better estimator of the genuine association between accidents and conflicts. The partial correlation coefficient, in effect, is the correlation between accidents and conflicts that would exist if ADT were held constant. This coefficient in the FHWA data is $r_{ac \cdot v} = 0.35$, which is significantly smaller* than $r = 0.59$. In other words, conflicts and accidents are positively correlated, but they are also both positively correlated to ADT so that the computed association between conflicts and accidents appears stronger than the genuine relationship. Of course, other variables not observed in the FHWA data and associated with a site may also be important in defining the conflict-accident relationship.

For the purpose of determining sample size requirements based on predicting reductions in conflicts, the Ohio data furnished a sample distribution of (2-1/2 hr) conflict observations, where each observation represents a <u>different</u> site. The properties of this distribution can be manipulated in a conventional statistical way to estimate theoretical sample sizes for detecting reductions in conflicts. However, since the experimental design was intended to observe conflicts before and after treatment at <u>a</u> site, a site will be its own control.

Although the Ohio data base contained between-site variability, the before-after experiments would eliminate or minimize this component of variance. Therefore, the sample sizes predicted from the Ohio data are too large. For this reason an assumption was made about the within-site variability, and a second set of "theoretical" sample sizes was estimated.

The goal of determining a sample size is to "guarantee" that the desirable magnitude of reduction in average conflicts before and after treatment can be labeled statistically significant; i.e., to collect a sufficient number of readings so that the minimum worthwhile conflict reduction will be "detected." Statistically, to "guarantee" detection means that an observed change in two samples is very unlikely to occur if they are both drawn from the same population, i.e., the "confidence level" $(1 - \alpha)$ in claiming a reduction is sufficiently high. Basically, the relation

$$\frac{t\sigma}{\sqrt{N}} = \Delta \text{ , or } N = \frac{t^2\sigma^2}{\Delta^2} \text{ ,}$$

* By Fisher's Z, $\alpha = 0.05$.

where t = factor determined by desired confidence level

σ^2 = variance of response

Δ = desired detectable difference between before and after

determines a required sample size to establish a mean result within $\pm\ \Delta$ with $1 - \alpha$ confidence. As used here, Δ represents a difference between two samples, so that the value of N is larger by, roughly a factor $\sqrt{2}$.

The conflict observations alone (the "raw" observations) were used to determine sample sizes (Table VII). However, observations of approach ADT were also taken in the Ohio data, and to some extent ADT predicts conflicts. Therefore, the necessary sample sizes are smaller when the approach ADT is also measured than when conflicts alone are counted (Table VIII). Also, sample sizes were predicted by using a combination of approach ADT and crossroad ADT. The corresponding sample sizes are shown in Table IX. Note that the sample size entries refer to the number of observation pairs of before and after, e.g., it takes 203 observations before and after to detect a 10% reduction in conflicts with 90% confidence when both ADTs are measured (Table IX).

TABLE VII

SAMPLE SIZES BASED ON RAW CONFLICTS

	$1 - \alpha$		
Δ (%)	90	95	99
10	551	778	1,349
20	138	195	338
30	61	87	150

TABLE VIII

SAMPLE SIZES BASED ON RAW CONFLICTS AND ADJUSTED BY APPROACH ADT

	1 - α		
Δ (%)	90	95	99
10	402	568	985
20	101	142	247
30	45	64	110

TABLE IX

SAMPLE SIZES BASED ON RAW CONFLICTS AND ADJUSTED BY APPROACH ADT AND CROSSROAD ADT

	1 - α		
Δ (%)	90	95	99
10	203	286	496
20	51	72	125
30	23	32	55

This analysis indicates a discouragingly large number of observations to detect reductions in conflicts.

Of course, for the before-after experiment repeated observations would be taken at a site, so that between-site variability is eliminated and a lesser sample size is required. Unfortunately, no data was available to estimate the within-site repeatibility of conflicts. Therefore, a theoretical value for the within-site standard deviation was assumed by making the usual Poisson assumption about the distributional form of conflicts at a site, and the Ohio data used to estimate the parameter of such a Poisson.

Also, for the before-after experiment a 5-hr conflict count was chosen to be more practical, so an observation was redefined as a 5-hr total of conflicts (rather than 2-1/2 hr as in the Ohio data). The sample size results are illustrated in Table X. These entries represent, in effect, the necessary sample pairs at a site before and after treatment.

TABLE X

THEORETICAL SAMPLE SIZES

	$1 - \alpha$		
Δ (%)	90	95	99
10	20	28	49
20	5	7	12
30	2	3	5

At this point, no attempt was made to further the experimental design because validation studies had been planned to verify the experimental design. It was felt that these studies would provide better data on within-site variability. Although these studies were limited by the budget of the project, the sample size requirements were used in a general way to determine how many field experiments would be feasible.

B. Field Studies of Conflicts

The field studies were originally planned as a validation of the experimental design. Because of the difficulties encountered in the determination of sample size requirements, the field studies were conducted to: (1) test the ability of the conflict measure to show a significant change based on the implementation of an access control technique; and (2) estimate within-site variability of conflicts so that more reliable sample size estimates could be made.

The initial task in the field studies was to select study sites. The matched-pair study method was used because project time constraints precluded any before-after experiments. From the estimated sample size requirements, it was determined that three experiments could be conducted, based on 10 days per matched-pair experiment (5 days per site) and the available time and manpower.

Generally, each site of a matched-pair was selected to be identical to the other except one site would have uncontrolled access and the other would include one of the access control techniques. The major concern in the selection of matched-pairs was to keep between-site variability at a minimum. Three distinct access control techniques were finally selected for the field studies. The following two techniques were evaluated by using matched-pair experiments:

1. Install isolated median and deceleration lane to shadow and store left-turning vehicles (Experiment 1)

2. Increase the effective approach width of the driveway (Experiment 3)

The third access control technique was used in a simulated before-after comparison in which the same site was used as both the control location and test location. The technique used in the simulation experiment was:

3. Restrict parking on the roadways next to driveways to increase driveway turning speeds (Experiment 2)

A significant portion of the general survey procedures and also the conflict data collection used in the field studies was similar to methods and procedures outlined in the "GMR Traffic Conflicts Technique Procedures Manual."[4] However, some revisions were necessary to tailor the methods to these experiments.

An objective definition of a traffic conflict was formulated to eliminate some of the subjectivity present in the GMR conflict definition. A traffic conflict was defined to occur when a driver takes evasive action by either braking or weaving into a neighboring traffic lane in order to avoid a turning driveway vehicle. The evasive action must occur within two car lengths (approximately 50 ft) of the turning vehicle. The conflict data collected in the field was similar to the GMR data collection, except fewer conflict types needed to be recorded since commercial driveways were being observed.

As determined from the preliminary experimental design, 5 days of data with 5-hr counts on each day were collected at each field site. Surveys were conducted on weekdays from 10:00 a.m. to 6:00 p.m. Data collection during the day was divided into fifteen 20-min counting periods with 10 min allowed between periods for computations and rest.

The within-site standard deviations of conflicts per day are shown in Table XI. Also shown are the corresponding coefficients of variations (CV's).*

TABLE XI

WITHIN-SITE VARIANCES

		Conflicts	
	Site No.	σ_c (conflicts/day)	CV_c
Experiment 1	1T	27.6	24.0%
	1C	19.6	11.7%
Experiment 2	2T	28.5	17.8%
	2C	26.9	19.9%
Experiment 3	3T	17.4	8.7%
	3C	15.7	19.6%

* A coefficient of variation is the standard deviation divided by the mean, i.e., a relative dispersion.

The collection of all six conflict dispersions is homogenous,* i.e., the within-site variance of the conflicts distribution does not vary significantly from site to site. The pooled (average) conflict standard error is $\sigma_c = 23.2$ (CV = 16.2%) .

Call the number of conflicts "raw" responses, and label these values divided by traffic volume "normalized" responses. Now, if the raw response and traffic volume were independent, the variance of their ratio would be relatively larger than the variance of the raw response. If conflicts were entirely explained by traffic volume, then of course the normalized responses would have zero variance. Therefore, the CV's of conflicts (c) versus conflicts/volume (c/V) indirectly tell us something about the dependence of conflicts on volume.

In the field study data, the CV (c/V) was less than the CV (c). Therefore, some connection apparently exists between conflicts and volume, but it is not really strong. Unfortunately, the volume did not vary greatly during the validation studies, and the relationship between conflicts and volume remains to be determined.

C. Development of Experimental Design

The field experiments furnished data on conflict distributions that were used in estimating sample sizes necessary for detecting reductions in average conflicts by implementing distinct access control measures. The sample size necessary for detecting a change in conflicts of size $\bar{X}_1 - \bar{X}_2$ with $1 - \alpha$ confidence is given by:

$$n = \frac{2Z^2_{\alpha/2}s^2}{(\bar{X}_1 - \bar{X}_2)^2} \quad , \text{ where}$$

$Z_{\alpha/2}$ is the standard normal variate associated with the desired $1 - \alpha$ confidence level. In the above equation, $s = 23$ (determined from the six conflict dispersions) and $\bar{X}_1 - \bar{X}_2$ was chosen at various points to span the region of interest. The conflict sample sizes are listed in Table XII and are based on a 95% confidence level.

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* Via Bartlett's test, $F(5, 1{,}000) < 1$.

TABLE XII

CONFLICT ANALYSIS SAMPLING REQUIREMENTS

μ_2	μ_1 50	100	150	200	250
$0.75\ \mu_1$	26	6	3	2	2
$0.80\ \mu_1$	41	10	4	2	2
$0.85\ \mu_1$	72	18	8	4	3
$0.90\ \mu_1$	162	41	18	10	6
$0.95\ \mu_1$	650	162	72	41	26

μ_1 = Before average total conflicts.
μ_2 = After average total conflicts.

For example, from the first entry in Table XII, we see that 26 (5-hr) observations before <u>and</u> after treatment are necessary to detect a 25% change in conflicts if the before level of conflicts is 50.

Since the FHWA data[2/] allowed the estimation of accidents/year (A) as a function of conflicts,* it is possible to estimate what reduction in accidents the sample sizes of Table XII would detect.

For example, the FHWA data regression line had a standard error of the estimate (S_e) of 3.37 and an average A value of 4.15. Thus, the prediction interval for an average A value is approximately given by $\pm \frac{Z_{\alpha/2}\ S_e}{\sqrt{n}\ \bar{A}} \times 100$ in percent. Note that S_e is almost as large as $\bar{A}$; i.e., the accident distribution has a very large relative dispersion even after conflicts have been used to help predict A . Also, in theory, this variability in A does not depend upon the level of C ; i.e., equal sample sizes are equally powerful for detecting accident reductions regardless of the conflict levels involved.

* The FHWA data conflict counts were 2-1/2 hr, this factor of 2 was accounted for in subsequent analysis.

These facts were used to construct a table of detectable accident reductions for various sample sizes (see Table XIII). As can be seen, large sample sizes are needed for reliability in predicting accident reductions.

TABLE XIII

SAMPLE SIZE REQUIREMENTS FOR DETECTING ACCIDENT REDUCTIONS

Sample Size	Detectable % Reduction in Accidents (95% Confidence)
5	± 71.2
10	± 50.3
20	± 35.6
30	± 29.1
40	± 25.2
50	± 22.5
75	± 18.4
100	± 15.9
150	± 13.0
200	± 11.3
300	± 9.2
500	± 7.1

The objective of the experimental design was, of course, to estimate the effect of the "treatment" upon the "response" (conflicts). In addition, the influences of highway ADT, crossroad ADT and location upon conflicts need to be observed. The effects and interrelationships of these factors will thus dictate when the treatment has a satisfactory effect, etc.

There will be four levels of highway ADT x four levels of crossroad ADT x two sites per category = 32 "cells" in the experimental model. Thus, if the number of observations/site = k , then the residual degrees of freedom are 32 (k - 1) and this value is the effective sample size that determines the precision of the conflict reduction.

Using the field data conflict standard deviation (s = 23), one can predict the appropriate sample size for detecting a given change in conflicts. Also, a regression curve from the Ohio data allows estimation of the expected number of conflicts as a function of highway and crossroad ADT (see Table XIV). Using s = 23 and the average number of conflicts (136) from Table XIV, a sample size necessary to detect a 10% reduction in average conflicts (95% confidence) is determined to be 22. Therefore, (k - 1) = 22/32 or k = 2 to the nearest whole number; i.e., in order to observe a 10% reduction in conflicts it is necessary to take two (5-hr) conflict observations per site.

TABLE XIV

THEORETICAL AVERAGE CONFLICTS (5-HR)

Crossroad ADT	Highway ADT ≤ 10,000	10,000-15,000	15,000-20,000	≥ 20,000
< 500	18	72	124	178
500-1,500	32	84	138	190
1,500-3,000	58	112	164	218
> 3,000	118	170	224	276

A schematic of the experimental design is shown in Table XV. Formally, the experimental design is a nested factorial analysis of variance model. An observation is the difference between before and after conflicts.* Therefore, the total number of 5-hr conflict counts per experiment is 128. In other words, to determine effectiveness measures for each kind of access control technique over the practical ranges of highway ADT and driveway ADT, 4 days of measurement at each of 32 sites is required.

* Alternatively, the ratio of before and after conflicts could be tried as a response, or the conditions "before" and "after" could be considered as levels of a design factor. In practice all these responses will be examined as candidates.

TABLE XV

SCHEMATIC OF EXPERIMENTAL DESIGN*

Crossroad ADT	Highway ADT							
	< 10,000		10,000-15,000		15,000-20,000		≥ 20,000	
	Site 1	Site 2	Site 3	Site 4	Site 5	Site 6	Site 7	Site 8
	X	X	X	X	X	X	X	X
< 500	X	X	X	X	X	X	X	X
	Site 9	Site 10	Site 11	Site 12	Site 13	Site 14	Site 15	Site 16
	X	X	X	X	X	X	X	X
500-1,500	X	X	X	X	X	X	X	X
	Site 17	Site 18	Site 19	Site 20	Site 21	Site 22	Site 23	Site 24
	X	X	X	X	X	X	X	X
1,500-3,000	X	X	X	X	X	X	X	X
	Site 25	Site 26	Site 27	Site 28	Site 29	Site 30	Site 31	Site 32
	X	X	X	X	X	X	X	X
> 3,000	X	X	X	X	X	X	X	X

* The number of observations at each site needs to be doubled for a before-after experiment.

V. ASSESSMENT OF THE TRAFFIC CONFLICTS TECHNIQUE

Based on the state-of-the-art review of Section III and the experimental design exercise explained in Section IV, the current reliability of the TCT for estimating accident potential is highly questionable. Although some studies have concluded that the TCT is a reliable tool, these conclusions are not well supported. Then too, for each study with a positive conclusion, there is at least one study that indicates the opposite. This is not to say that accident potential cannot be predicted by conflict counts, but that existing data does not define the population characteristics of conflicts well enough to estimate what sample size of conflicts is needed to reliably predict accident potential.

Basically, there are three practical applications that a reliable traffic conflict measure could be used for. These are: (1) to identify and rank locations for safety improvements; (2) to diagnose specific safety deficiencies at a location for the purpose of determining specific countermeasures; and (3) to measure the safety effectiveness of implemented countermeasures using the before-after study technique.

Although some users have suggested that conflicts be used to merely rank the accident potential of intersections, this is not a realistic use of conflict measurements. If the simple ranking of intersections is all that is desired, many previous studies indicate the ability to do this using the more straightforward measures of traffic volume. The more realistic use of conflict measures is to predict accident rates to identify those intersections with rates unusually higher than the expected average for similar kinds of intersections. This kind of evaluation would give the true indication of hazardous locations that could be made less hazardous through application of accident countermeasures.

For all three potential uses of conflict counts, existing relationships do not allow practical sample sizes. This is particularly true with suggested diagnostic study uses which attempt to pinpoint specific deficiencies at an intersection using component parts (e.g., left-turn conflicts) of the total conflict count. A basic problem with existing data and relationships is that they are ill-defined. In other words, data have not been adequately stratified and analyzed accordingly for significant conditional parameters such as highway ADT, crossroad ADT, number of approach legs, number of lanes, type of traffic control, etc. Then too, reliable estimates of the within-site variability of conflicts are not available.

Other very distinct problems in using existing data and relationships on conflicts, are that conflict definitions and sampling procedures vary significantly. With conflict definitions, an additional weakness is that none have a completely objective base. The field determination of a conflict occurrence depends on the observer's judgment of temporal variables such as the initial gap between lead and following vehicles or the magnitude of deceleration. Where the brake-light application is used as a criterion, additional sampling error stems from the proportion of vehicles with nonoperative brake lights.

This discussion is not intended to discourage the concept of conflict analysis, but rather to caution potential users and encourage a more rigorous development of an appropriate data base. For conflict analysis techniques to be useful, they must embody appropriate definitions and sampling procedures that allow a practical (cost-effective) method to reliably predict the expected annual average number of accidents for a particular site condition.

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