Do Traffic Signals Affect Safety? Some Methodological Issues

BHAGWANT N. PERSAUD

This paper is based on a review of often-cited studies on the safety impact of traffic signal installation. Most of these studies are found wanting with respect to methods of analysis or inferences from the results. Two common pitfalls—regression to the mean and incorrect inferences from cross-section studies—are illustrated. The first might lead to erroneous conclusions about the circumstances under which signal installation is likely to improve or degrade safety. The second might lead to underestimation of the safety effectiveness of installing signals at relatively unsafe intersections. Most of the studies reviewed were conducted before these pitfalls came to light. Nevertheless, the upshot of these revelations is that there is very little substantial knowledge about the safety impact of traffic signal installation. The question of how to improve this somewhat embarrassing state of affairs using the latest methods of analysis is addressed, and recommendations are made on how to incorporate useful knowledge into the allocation of resources for signal installation.

The net impact of traffic signal installation is usually assessed by evaluating the effects on vehicle operating costs, motorist delay, vehicle emissions, and safety. In evaluating the likely safety impact, a traffic engineer would ideally like to draw on the knowledge accumulated in numerous past evaluations. However, as many previous reviewers have noted [see paper by Box and Alroth (*1*), for example], these studies have failed to produce a consensus on the safety impact of signals. Yet these studies are apparently the principal source for many prevailing beliefs about the conditions under which signals are likely to reduce or increase accidents. Even where there is a semblance of consensus, as on the influence of preinstallation accident history, one cannot, as will be contended in this paper, be certain that what is taken as common knowledge is based on a strong foundation.

One important but often overlooked explanation for the shaky foundations of knowledge on this subject is that there might have been practical limitations or methodological deficiencies in several of the studies on which current knowledge is based. Before the results of a study are accepted as useful knowledge, it seems prudent to examine the study to ensure that the assumptions and methods of analysis are proper, that the conclusions drawn are supported by the data, and that the findings are applicable. This is the guiding principle of the review on which this paper is based.

2 Elmbrook Crescent, Etobicoke, Ontario M9C 5B4, Canada.

The paper begins with a brief description of the apparent status of knowledge on a number of issues. The main focus of this paper is to point out how errors of analysis and interpretation committed in many evaluations might cast a shadow on much of the empirical evidence on the safety impact of signal installation. Therefore, a special section is devoted to illustrating two of the pitfalls that plague safety studies. Against this background, the conventional wisdom on the safety impact of signal installation is then scrutinized. Finally, there are some suggestions on how the status of knowledge might be improved.

To provide some background for the discussion to follow, relevant features and results for a selected number of before-and-after studies are summarized in Table 1. (Note that in Table 1 and throughout this paper, percent reductions are specified, so negative values indicate increases in accidents.)

APPARENT STATUS OF KNOWLEDGE

Overall Safety Impact

The data in the fourth through eighth columns of Table 1 indicate an apparent lack of consensus on the overall safety impact of signal installation. This inconsistency is not surprising to most reviewers, who generally recognize that the safety effect of signal installation is likely to depend on a complex web of factors—approach volumes, intersection geometry, signal design, accident history and characteristics, approach speeds, and so on. Yet it is not uncommon to find accident reduction factors for signal installation specified in such authoritative sources as the Institute of Transportation Engineers (ITE) handbook (*15*) (18 percent for total accidents, 32 percent for injuries, and 49 percent for fatalities). On the basis of Table 1, the source or applicability of such factors is unclear.

It should be pointed out that the studies cited in Table 1 are of the before-and-after type. Another type of study is the cross-section type, in which the accident experience of signalized intersections is compared with that of unsignalized intersections. The results of these studies almost always show that signalized intersections have a higher accident rate. As will be discussed later in this paper, it is perhaps incorrect to infer that this higher rate is due to signal installation, because the higher accident rate may have already existed before installation and may actually have been part of the reason for installing the signals.

Effect on Specific Types of Collision

Most traffic engineers are cautious in using factors such as those in the ITE handbook, believing that because signal installation is likely to increase rear-end accidents and reduce right-angle accidents, the overall safety impact should depend on the relative number of each of these two types of accidents. This belief makes good sense intuitively; however, as the data in Table 1 indicate, the support is weak—the impact of signal installation on each of these two accident types is reasonably, but not totally, consistent.

Effect on Accident Severity

Table 1 also challenges some other beliefs. For example, there is a reasonable belief that right-angle accidents are more severe than rear-end accidents; thus by reducing

TABLE 1 PERCENT REDUCTION IN ACCIDENTS AFTER SIGNAL INSTALLATION

Author/ Reference/ Study Period *	Location	No. of sig-nals	% reduction by accident type: Total	Rear-End	Right Angle	Injury	Left Turn	Other Issues **
Solomon (2)	Michigan Rural	39	-23	-200	+51	+20	n/a	B, D
King (3) 1-2 yr.	Virginia ?	30	-24	-181	+34	+18	-16	C, D
King (3)	Michigan ?	33	-8	-84	+45	n/a	-236	
N.Y.DOT (4) 3-4 yr.	New York Rural ?	39	+7	+21	+13	-11	-13	D
Hammer (5)	California Rural ?	170	+21	-90	+76	+32	+14	
Clyde (6)	Michigan Urban	52	-34	-98	+45	-11	-66	
Short (7) 3 yr.	Milwaukee Urban	31	+2	-37	+34	-6	n/a	A, B, C, D
Vey (8)	22 cities Mixed	599	+20	-37	+56	n/a	n/a	C
Cribbins (9) 1 yr.	N.Carolina Rural	19	-7	-147	+73	-21	-21	D
Malo (10)	Detroit Urban	20	+47	+24	+75	n/a	n/a	
San Francisco (11) 1 yr.	Urban	48	+53	+72	+80	+50		A, B, C, D
Leckie (12) 1 yr.	Ontario Rural ?	13	+8			-27		
Schoene (13) 2 yr.	Illinois Rural	30	-16	-221	+48	-26		A, B, C
Smith (14) 1 yr.	California Mixed	32	+39					

* Typical lengths for before and after periods, where given.

** A - Safety of warranted vs unwarranted installations
B - Influence of volumes
C - Influence of pre-installation accident record
D - Influence of geometrics, layout, and/or approach speed.

right-angle accidents, signal installation will reduce injury accidents. From Table 1, it is not at all clear that this is so; there is no consensus.

Several researchers have attempted to assign weightings to accidents of different severity, but findings with respect to these weighted values vary from study to study or are inclusive. Abramson (*16*) reported an increase in total accidents after signal installation with no significant change in accidents weighted by severity. Because few details of the study were provided, the methodology could not be scrutinized; thus caution should be exercised in accepting these results. Chung-Cha (*17*) used Abramson's weightings and reported that signalized intersections do not necessarily result in smaller figures of merit than STOP-sign-controlled intersections. As will be discussed in depth later, this type of result could be misleading, because it is based on a cross-section type of analysis. A before-and-after study by Cribbins and Walton (*9*) of 19 low-volume, high-speed (45 mph) rural intersections is particularly relevant in the context of this paper. Although there was a 7 percent increase in total accidents (which was insignificant), the authors found a decrease (insignificant) in accidents "normalized" to equivalent property damage only (EPDO) accidents. At undivided highway intersections, there was a significant 41 percent decrease in EPDO accidents. Though these results are interesting, one cannot readily accept them because there are hints that high-accident locations tended to be selected; as discussed later in this paper, such simple before-and-after comparisons may overestimate safety benefits.

Effect on Pedestrian Accidents

Requests by the public for signal installation are quite common, and these are often inspired by a concern for pedestrian safety. It seems important, therefore, to know how signal installation affects pedestrian accidents. However, in the studies reviewed, pedestrian accidents either were not reported or were so few that the results were inconclusive. In a study by Short et al. (*7*), pedestrian accidents decreased (insignificantly) from 19 in 3 years to 12 in a similar period after 31 signal installations. In a study by the New York State Department of Transportation (NYSDOT) (*4*), these accidents increased from 0.1 per year to 0.2 per year after signal installation. Findings in a study by Yagar (*18*) suggest that installation of signals at 25 metropolitan Toronto intersections that had pedestrian crossovers decreased the pedestrian accident rate by about 40 percent; on the other hand, at the 43 installations that had no prior pedestrian protection, the pedestrian accident rate increased by about 20 percent. These statistics, however, were based on an average of fewer than 30 pedestrian accidents per year, and Yagar points out that the 20 percent increase is insignificant. In addition, it is not clear whether there were dramatic changes in pedestrian counts after signal installation; in particular, a reduction in pedestrian volumes might suggest a "migration" of pedestrian traffic, and possibly pedestrian accidents, to other locations.

Warranted Versus Unwarranted Signals

Most jurisdictions have installation warrants for traffic signals, many of which are based on the U.S. Manual on Uniform Traffic Control Devices (MUTCD) (*19*). Traffic engineers' faith in these warrants is founded in part on a belief that installation of warranted signals is likely to reduce accidents, whereas unwarranted signals can increase accidents (*20*). This belief is apparently a reflection of evidence on the effect of traffic volume or preinstallation accident record, or both; this evidence is summarized next.

Influence of Traffic Volume

Warrants are generally based on minimum traffic volume or accident levels, or both. Because most installations tend to be warranted on the basis of traffic volume, the belief that signals improve safety only where they are warranted is apparently a reflection of another belief—that the safety benefit of signal installation increases with traffic volume. There seems to be a concurrence in many studies that traffic volumes play a role, but there is conflicting evidence on the nature of the influence. To illustrate, in Figure 1, which is based on several well-known studies, the influence of traffic volume is not clear. Because the unwarranted signals would tend to be in the lower volume range, it is unclear how this graph, taken as a whole, would support a belief that installation of unwarranted signals can increase accidents.

The inconsistency in Figure 1 might possibly be explained by evidence indicating that the safety effect of signal installation depends not only on the total entering volume but also on how this volume is divided among the approaches. In this respect, the before-and-after study by Schoene and Michael (*13*) of 30 rural intersections is the classic. They reported that accidents generally increased after signalization of intersections with major:minor volumes greater than 6:1; for warranted installations the cutoff ratio was even lower (4:1). Though these results might be somewhat useful, the small sample size precludes the establishment of a relationship among safety effect, total volume, and ratio of major-road volume to minor-road volume.

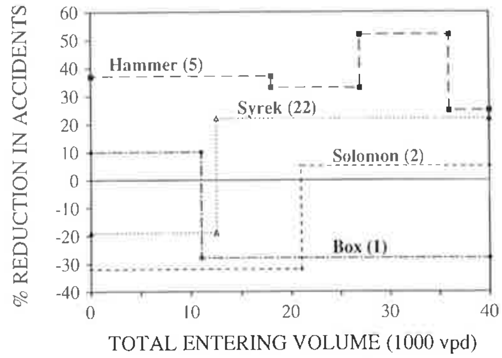

FIGURE 1 Safety impact of signal installation by total entering volume.

That the safety effect of signal installation depends on the details of the traffic volume is underscored in some recent work by Hauer et al., which is published elsewhere in this Record. They explored various models to estimate the safety of a signalized intersection on the basis of information about its traffic flow and accident history and concluded that "logically sound models require that frequency of collisions be related to the traffic flows to which the colliding vehicles belong and not to the sum of entering flows." Because this relation has never been attempted in the examination of the safety effect of signal installation, it would seem that there is a lot yet to be learned about the dependence of this effect on volume. How this might be accomplished is addressed later.

Influence of Preinstallation Accident History

Most jurisdictions permit signals to be warranted, at least in part, on the basis of accident history. For example, many jurisdictions use the MUTCD warrant (*19*), which specifies that if 80 percent of the volume warrant, and some other conditions, is met, a signal may be warranted if five or more "correctable" accidents have occurred in a 12-month period. The Ontario warrant (*23*) is similar, but emphasis is placed on averaging the accident totals over a 36-month period; this would indicate that 15 or more correctable accidents are required over 36 months. In a Canadian warrant (*24*), priority points are assigned that generally increase with, among other factors, the total number of preinstallation reportable accidents per year (averaged over 4 to 5 years). Signal installation is warranted at intersections that have at least 100 priority points. Accident priority points are determined from a chart similar to Figure 2, which actually suggests negative priority points for intersections with fewer than eight accidents per year before signalization. Taken together with the MUTCD and the Ontario accident warrants, the sloping line in Figure 2 must therefore be a reflection of the belief that not only does the safety impact of signal installation increase with the number of preinstallation accidents, but also that signals can increase accidents if this number is small enough.

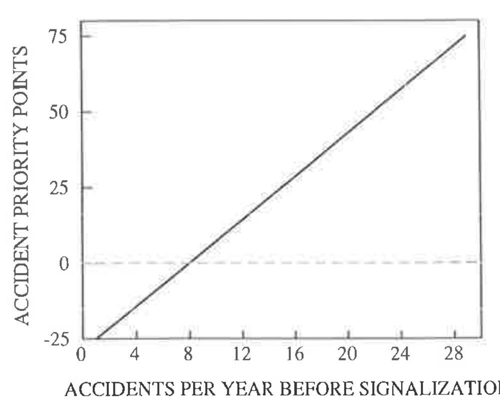

FIGURE 2 Accident priority points applied in Canadian signal warrant (*24*) for $200 minimum for reportable accidents.

At a glance there appears to be formidable support for this belief. Vey (*8*), in the earliest reported study on this subject, found that installing signals at intersections with three or fewer accidents per year increased accidents by 39 to 70 percent, whereas at other intersections accidents decreased by 19 to 49 percent. In another widely quoted study, Schoene et al. (*13*) indicated an increase in accidents at intersections that had fewer than 10 right-angle accidents for 2 years before signalization. King and Goldblatt (*3*) found that total accidents increased following signal installation at intersections not meeting the MUTCD accident warrant (those with fewer than five right-angle accidents per year). Hakkert and Mahalel (*25*) compared before-and-after accident records for 34 urban signal installations in Israel and found that at intersections with more than five accidents per year, there was generally a decrease in accidents after installation of a signal; at installations with fewer than two accidents per year, accidents generally increased. Presumably on this basis, they concluded that where warranted according to vehicle volumes, traffic signals "most probably have a beneficial effect on the number of accidents."

As will be seen later, much of the evidence on the influence of the preinstallation accident record on the safety impact of traffic signals is tainted by the fact that increases and decreases in number of accidents at intersections may have little to do with changes in safety and a lot to do with random fluctuation; increasing the length of the "before" period to 3 to 5 years reduces, but does not eliminate, this bias. This problem may render useless much of the evidence on this issue, as well as the warrants that seem to be based on this evidence.

Other Evidence on the Impact of Warrants

Some studies do not separate those signals warranted on the basis of traffic volumes and those meeting the safety warrant, and so leave unclear the influence of the individual factors. As will be seen, the validity of these studies is also subject to question. Hanna et al. (*26*) conducted a cross-section analysis of signals already installed in rural areas, found similar accident rates for warranted and unwarranted installations, and inferred that this contradicts common belief that unwarranted signals cause higher accident rates. This inference might be "unwarranted," as will be seen later when the dangers of cross-section studies are discussed. Another study that appeared to contradict the belief about the increase in unsafe conditions after unwarranted installations was the 3-year before-and-after comparison of 31 urban installations by Short et al. (*7*), who concluded that "whether or not a signal was warranted seemed to have little bearing on whether accidents would be reduced," and then claimed, without reporting the evidence, that "one of the best measures found to indicate accident reduction is the average number of correctable accidents per year over a several year period."

In a well-known study, Young (*27*) conducted a one-year before-and-after comparison of 32 urban installations and found that of seven intersections with reductions of more than five accidents, five met at least one MUTCD (*28*) warrant, and three met the accident warrant; conversely, of 10 intersections showing an increase in accidents after signalization, "only 2 met any semblance of a warrant." On this basis, Young concludes that his evaluation shows "a pattern of support for the present warrants with respect to the effect of signalization on safety." However, apparently in contradiction of this statement, Young goes on to state that the study gives very little support to the idea that unwarranted signals per se will increase accidents, because accidents actually decreased at nine unwarranted installations. In any case, as will be described in the next section, this evidence may be almost totally useless.

PITFALLS IN SAFETY EVALUATION

Regression to the Mean

Perhaps the most common pitfall in studies of the safety impact of signal installation is the failure to account for regression to the mean. This cumbersome term refers to a phenomenon whereby, even if signal installation has no safety impact, intersections that recorded many accidents in the period before signalization would, on average, record fewer accidents in the after period, whereas accidents would increase at intersections that had few accidents. By simply comparing before-and-after accident totals or rates, therefore, one could erroneously conclude that signal installation improved safety only where the safety warrant was satisfied. Furthermore, if, as is sometimes the case, intersections are selected for signal installation on the basis of an unusually high accident record, a simple before-and-after comparison is likely to overestimate the effect of the treatment. [For more details, see the paper by Hauer and Persaud (*29*).]

To illustrate, right-angle accidents recorded in two consecutive 2-year periods at unaltered, two-way stop-controlled intersections in Philadelphia are summarized in Table 2. [The data are taken from a thesis by Ebbecke (*30*).] The first row of numbers shows that 17 intersections had no right-angle accidents in either period, 11 that had none in the first period had 1 in the second period, and so on. The numbers connected by the diagonal line are for intersections whose right-angle accident total remained unchanged; the numbers to the right of this line are for intersections at which an increase in right-angle accidents was reported, whereas the number of intersections that recorded a decrease are to the left of the diagonal line.

Summation of the appropriate numbers in Table 2 shows that, even though the intersections remained essentially unaltered, right-angle accidents increased at many intersections (80), decreased at still more (88), and remained unchanged at only 43 intersections. This type of conclusion accords precisely with the common belief that signals sometimes increase and sometimes decrease accidents, yet it was derived from data on intersections that remained unchanged and results from changes largely due to random fluctuation. The upshot of this paradox is that statements about the frequency of increases and decreases in accidents following signalization must be disregarded unless one can determine how much of the change is due to random fluctuation and how much is due to signal installation.

Although the most common method of measuring safety impact is from the changes in the number of accidents or in the accident rate (per unit volume), many studies report their findings and make conclusions on the basis of the number of intersections showing a decrease in accidents and the number showing an increase following signal installation. Among the most notable is the widely quoted study by Young (*27*), who reported that, on signalization, the 5-year accident total increased at 102 intersections, decreased at 23, and did not change "significantly" at 27. Then, noting that it was "much more common for accidents to increase," Young concluded that there was no obvious advantage of signalization as an accident prevention measure. In a later study reported in the same paper, a 1-year before-and-after comparison of 32 subsequent installations found accident reductions to be more prevalent, which led to the conclusion that "this study shows far more favorable results from signalization than did the earlier study." Clearly, the relative size of the increases and decreases should matter, so one should be cautious about accepting this statement. Furthermore, as shown earlier, statements about the number of intersections with increases and decreases are not very meaningful because such changes can be due mainly to random fluctuation.

This phenomenon also appears to be present in the several studies (*13, 14, 27*) that attempt to identify common factors (volume, accident history, warrant compliance) among the intersections with increases (or those with decreases) in accidents. Invariably, as mentioned earlier, these studies find that the intersections with increases in accidents following signalization tend to have had low volumes or few accidents, or both, before signalization. Because these are the intersections that tend not meet the common warrants (e.g., those of the MUTCD), it is often concluded that unwarranted installations can increase accidents. Perhaps the chicken came before the egg, and accident warrants, such as those of the MUTCD, are in part based on the finding that signals improve safety only where there were numerous accidents. This might also be the inspiration for the Canadian warrant (*24*) (Figure 2) that assigns negative priority points to intersections with fewer than eight (total) accidents per year.

An examination of the data in Table 2 shows how one can erroneously conclude that only warranted signals reduce accidents; one can arrive at this conclusion even if signal installation has no safety effect. For example, the 204 intersections with less than 10 right-angle accidents in the first period might not satisfy the MUTCD accident warrant (five or more "correctable" accidents in 12 months). Table 3 shows that these "unwarranted" inter-

TABLE 2 RIGHT-ANGLE ACCIDENTS AT TWO-WAY-STOPPED INTERSECTIONS

Accs. in Period 2 / Period 1	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
	Number of intersections with indicated numbers of accidents in each period														
0	17	11	7	4	1	2	1								
1	10	9	4	5	1	3	1								
2	6	7	5	5	6	3	2	1	1	2					
3	3	10	7	3	3	1		1	2			1			
4	3	2	4	3	5	1			2	1					
5		1	2	1	2	3	1	1		2		1			
6	2			2	1	3									
7		1	3	1	2	1		1							
8		1	1	1	1	1							1		
9														2	
10			1												
11								1	1						
12			1						1						
13															
14											1				
..															
20															1

TABLE 3 CHANGES IN RIGHT-ANGLE ACCIDENTS IN GROUPS OF UNALTERED INTERSECTIONS

GROUP	No. of intersections	Total accidents in period 1st. 2 years	2nd. 2 years	CHANGE
10 or more accidents	7	90	54	Decrease of 36
Less than 10 accs.	204	525	558	Increase of 33

sections had an increase of 33 accidents in the second period; on the other hand, the seven intersections that would satisfy the accident warrant (and be more likely to meet the volume warrant) showed a decrease of 36 accidents (40 percent). So, were the 211 intersections (or a representative sample) to be signalized, and signalization had no effect on safety, one could erroneously conclude that unwarranted, low-volume, low-accident installations increase accidents, whereas warranted, high-accident, high volume installations increase safety. Because this erroneous conclusion is derived from changes in accidents due to random fluctuation, studies that make such claims must be taken with a grain of salt unless, once again, changes due to random fluctuation are extracted.

The problem described in the preceding paragraph can arise whenever the selected group contains a mixture of high- and low-accident intersections. In cases where intersections are selected for signal installation because they have had many right-angle accidents, as might be the case for the seven intersections in Table 2 with 10 or more such accidents, which would meet the MUTCD accident warrant, a further complication arises. As indicated in Table 3, these intersections had a 40 percent decrease in right-angle accidents solely because of random fluctuation. So, were signals to be installed at these intersections, a simple comparison of the number of accidents before and after installation could cause the impact of signalization on right-angle accidents to be overestimated by 40 percent.

It should be pointed out that in cases where there is no selection bias—where selection of intersections for signal installation is not based on accidents recorded at all—the danger of overestimating the overall safety impact would not be present. One can show empirically that a randomly selected group of intersections from Table 2 would show little or no change in the total number of accidents over the two periods. The extent to which intersections are selected for signal installation on the basis of a high number of accidents is not clear, although in the literature there are hints that this is the basis for some installations. For example, the Canadian warrants (*24*), by assigning accident priority points that increase with the total number of accidents, might tend to favor selection of intersections that recorded many accidents. (It should be stated in fairness that this warrant does suggest a 4- or 5-year "before" period; this reduces the overestimation, but it has been shown empirically (*30*) that the overestimation can still be substantial.)

It is also worth emphasizing that even when selection for signal installation is not based on accident record, the danger of regression to the mean still lingers if one were to try to examine the safety effect of subgroups that are identified on the basis of accident record. To illustrate, consider the study by Kay et al. (*31*), who compared accident records before and after signals were removed and found that, on signal removal, accidents increased at intersections that had few accidents and decreased at other intersections. On this basis, the authors suggested that intersections with at least two or more accidents per year are "good candidates for signal removal." For reasons given earlier, this conclusion cannot be made on the basis of a simple before-and-after comparison of intersections grouped according to accident record before the change. The irony in the conclusion by Kay et al. is that the potential danger of regression to the mean was recognized, but the assumption was made that, because they "were not selecting 'extreme' cases for treatment," there was a small chance that the findings would be affected. As Table 4 shows, this phenomenon might, in fact, have substantially affected their findings; right-angle accidents at unaltered signalized intersections in Philadelphia decreased at those with fewer than two accidents per year in one period, whereas there was a compensating decrease in these accidents at other intersections.

The main message from this rather detailed description of one of the common pitfalls in safety evaluation studies is that, in the comparison of accident frequency before and after signal installation, one must separate the changes that are due to random fluctuation from the changes that might be due to signal installation. It should be noted that most of the studies on which our knowledge about the

TABLE 4 CHANGES IN ACCIDENTS AT UNALTERED SIGNALIZED INTERSECTIONS

Accidents in Group	Number of Intersections	Two-year Accident Totals Period 1	Period 2	Change
4 or more	298	2221	2040	8% Decrease of 181 accs.
< 4	79	196	376	92% Increase of 180 accs.

safety impact of signal installation is based were undertaken before the traffic engineering profession came to recognize the dangers of regression to the mean. This partly redeems the studies, but cannot justify the continued acceptance of knowledge based on questionable findings.

Incorrect Inference from Cross-Section Studies

A second major pitfall in safety evaluation occurs when one tries to extend the results of cross-section ("with and without") studies to make inferences about the safety impact of adding a feature. As an illustration, consider the study by Syrek (*22*), who compared accident rates for intersections with different forms of control and found, for example, that signalized intersections with major- and minor-street annual average daily traffic (AADT) of 10,000 and 8,000, respectively, had higher accident rates than STOP-controlled intersections with similar volumes. This might be taken to indicate that installing signals at intersections with such volumes can cause accidents to increase, but what is overlooked in this type of inference is that the signalized intersections might have had a higher level of unsafe conditions for a variety of reasons that may have nothing to do with the installation of signals. Indeed, this higher level of unsafe conditions might have been partly the cause for the signal installation rather than its effect.

In a similar type of study, David and Norman (*32*) examined accidents during 3 years at intersections with various forms of control and found that STOP-controlled intersections had 30 to 60 percent fewer accidents than signalized intersections with similar volumes and the same number of approaches. They also found that two-phase intersections with left-turn lanes had a higher accident rate than those without, and inferred that the introduction of left-turn lanes increases accidents. The inference might be incorrect because the difference in safety might have existed before the introduction of left-turn lanes and might have been partly the cause for their introduction. Similarly, one might question the basis for the recommendation by David and Norman that the introduction of a third phase will reduce accidents at those signalized intersections already having left-turn lanes. Although the subject of this paper is not the safety impact of turn lanes or signal phasing, this digression was made to point out that, because statements about the safety impact of turn lancs and left-turn phasing were made on the basis of a cross-section study, it would not be inconsistent to make the absurd inference from the results of the same study that signal removal decreases accidents by 30 to 60 percent, which therefore makes it a worthwhile countermeasure.

The results of cross-section studies often differ from the results of before-and-after studies. Hanna et al. (*26*) found that warranted and unwarranted rural installations had similar accident rates and inferred that the safety impact of installation was similar for the two groups; on this basis they suggested that "the need to implement a policy of eliminating unwarranted signals is perhaps not so urgent as in urban areas." They also found that, for a given average daily traffic, signalized intersections had a 29 percent higher accident rate than intersections with STOP or YIELD control, and implied that this meant that signalization can increase accidents. Clearly, the inferences by Hanna et al. should be interpreted with caution, as were those of David et al., because differences in safety may have existed before the installation of signals and may have had little to do with the installation of the signals. Indeed, the 21 of 76 installations in the study by Hanna et al. that did not meet the volume warrants might have been justified by a concern for high accident rates—rates higher than those at intersections that remained STOP or YIELD controlled.

Perhaps the most telling example of the differences between cross-section and before-and-after studies is provided by studies by Thorpe (*33*) and Andreassend (*34*), which used essentially the same data base (accidents in Melbourne, Australia). Thorpe, in a cross-section study, found that signalized and unsignalized intersections had similar accident rates, whereas Andreassend, in a before-and-after study, found that signalization was followed by a "highly significant" 32 percent reduction in accidents. If in fact the signalized intersections had higher preinstallation accident rates than the other intersections, Andreassend's before-and-after comparison might have overestimated the safety benefit (because of regression to the mean); on the other hand, inferring from Thorpe's results that signals have no safety effect would underestimate the safety benefit, because such an inference would incorrectly assume that the preinstallation accident rate for signalized intersections was similar to that for other intersections.

To summarize the discussion of cross-section type studies, it seems clear that inferences from this type of analysis about the safety impact of installing signals must be interpreted with caution. Attributing differences in rates between intersections with and without signals to the installation of signals will almost certainly underestimate the safety effectiveness.

HOW TO IMPROVE THE STATUS OF KNOWLEDGE

In the previous sections of this paper, it has been suggested that very little is known about the safety impact of traffic signals and that the accident warrants for traffic signals are, despite appearances, not well supported. In suggesting how this state of affairs can be improved, it is perhaps best to first consider what tools should be available to jurisdictions responsible for decisions on signal installation. These might be described as follows:

1. A method for quantifying the likely safety impact of a contemplated installation,
2. Classification of circumstances under which signal installation is likely to be good or bad for safety, and

3. The incorporation of knowledge gained with the second tool into signal warrants or perhaps into a cost-benefit–resource allocation procedure for signals.

Suggestions on how these tools might be developed follow.

Safety Impact of Signal Installation (Tools 1 and 2)

To provide these tools, it is first necessary that some resource be undertaken to provide estimates of the safety impact of signals already installed and to relate these estimates to various installation circumstances—traffic volumes, the preinstallation accident experience and control type, geometric characteristics, approach speeds, signal design elements, and so on. The outlines of a suggested approach are described below.

To avoid the pitfalls of earlier studies, it is necessary to recognize the distinction between the accident count at an intersection for a short period of 1 to 5 years and its safety—defined as the number of accidents per year expected to occur in the long run at the intersection. We have already seen that comparing accident counts before and after signal installation can produce misleading results about the conditions under which signal installation is likely to be good or bad for safety. This bias would be present even if before-and-after periods of as long as 5 years are used, but it can be eliminated if one compares the long-run expected number of accidents per year (safety) before and after installation. The problem at hand therefore amounts to (a) estimating this quantity defined as safety for signalized and unsignalized intersections and (b) for actual installations, estimating the changes in safety and relating these estimates to various installation circumstances. How to provide these two types of estimates is addressed separately.

Estimating Intersection Safety

The theory for estimating safety is quite recent but is already in place and has been applied to rail–highway grade crossings (*35*) and to signalized intersections (Hauer et al., paper elsewhere in this Record). One would now like to be able to provide an estimate of the safety of an intersection before it has been signalized. This will be indicated by the expected (long-term average) number of accidents of various types for that intersection. These intersections might be categorized by number of approaches and approach lanes, by type of control (two-way or four-way stop controlled), and by location (urban or rural).

To apply the method, detailed data are required for at least 100 intersections in each category of traffic volumes (individual turning movements) and accidents (to the level of movements of vehicles before impact). [Recent work by Hauer et al. (paper elsewhere in this Record) has shown that the best predictor of collisions is the individual vehicle movements related to given types of collisions, and not measures such as total entering volumes.] For each intersection category and each combination of precollision movements, regression analyses could be performed to estimate the expected number of accidents as a function of the relevant volumes. This analysis provides equations for each category of intersection and collision type of the following form:

$$E(m) = \text{function (volume)}$$

where $E(m)$ is the expected number of accidents per year of a given type for a particular category of intersection. Now, in each category, some intersections will actually be safer than others with the same value. The reason for this is that, in grouping intersections, one could never obtain perfect homogeneity. It is therefore necessary to refine $E(m)$ for individual intersections in a category. This can be done by using the accident count (x) at an intersection in some before period. It has been shown (*35*) that, under reasonable assumptions,

$$\begin{aligned} E(m \mid x) &= \text{function}[E(m),x] \\ &= x + [E(m)/\text{Var}(x)]\ [E(m) - x] \end{aligned}$$

This is the estimate of safety that we would like to use in comparing the safety of intersections before and after signal installation. These equations have been developed for signalized intersections in an urban area (Hauer et al., paper elsewhere in this Record). It remains to develop them for other types of signalized intersections and for unsignalized intersections. One can then use them to estimate the safety impact of signal installation.

Estimating the Safety Impact of Signal Installation

At least 2 years of detailed traffic and accident data before and after as many installations as possible (a minimum of 100) is required. For each intersection, one could use the procedure developed in the previous section to estimate the expected number of accidents (by collision type) for the intersection before signalization. One could then use equations already developed by Hauer et al. to estimate the expected number of accidents (of given types) after signalization. (These equations are for urban signalized intersections; equations need to be developed for rural installations.) The percent change is calculated and can then be used as the dependent variable in a regression analysis that will use, as independent variables, relevant traffic volumes, the expected number of accidents before signalization, the type of signal, number of approaches and approach lanes, and so on.

Although the development of the procedures seems quite complicated, much of the groundwork has already been laid. By contrast, once the procedures have been translated into the form of interactive personal computer software, they will be quite easy to use.

Incorporating Knowledge on Signal Safety Impact into Warrants (Tool 3)

It has been suggested on the basis of the review detailed in this paper that safety warrants for signalized intersections are on a shaky foundation and that the belief that only warranted signals improve safety might be incorrect. The procedures suggested will not only offer a convenient method of estimating the likely safety impact of a proposed installation, but would also provide the means for rationally weighing the safety impact against other impacts, in effect, for combining the various warrants. In principle this would be similar to the Canadian procedure (*24*), except that with better knowledge about the safety impact, one can now confidently assign dollar values to the various effects instead of priority points. These procedures lend themselves well to implementation on a personal computer, which would eliminate the need for simplified charts such as Figure 2.

It should be emphasized that, even with current procedures for deciding whether a signal is warranted, it is still necessary to know about the likely safety impact of an installation and to decide how this knowledge will be used. One could, for example, have a policy that a signal is not warranted if it will increase accidents or that a signal is warranted if it satisfies 80 percent of the volume warrant and saves at least one accident per year.

SUMMARY AND CONCLUSIONS

The original intent of this paper was to present a balanced review of knowledge about the safety impact of signal installation. The potential defects in this knowledge turned out to be so universal and so serious that it was inevitable that what started out as an attempt to separate what is useful from what is not resulted in a paper with a distinctly negative tone.

Most will agree that a single set of accident reduction factors for signal installation is meaningless, because the safety impact is likely to depend on a complex web of factors. However, little or nothing is known, or there is no consensus, about the influence of important factors such as intersection geometry, volume, and the type of signal design.

Two potentially damaging pitfalls in safety evaluation studies were described. It was shown that these pitfalls might cast a shadow of doubt on much of the knowledge about the safety impact of signal installation. In particular, the foundation of the belief that, where unwarranted, signal installation is likely to increase accidents appears to be very shaky.

Much of our knowledge on this subject comes from studies that were conducted before these pitfalls and methods of avoiding them (Hauer et al., paper elsewhere in this Record; *29, 35, 36*) were known to researchers. This redeems the early researchers but cannot justify the continued adherence to beliefs that might be based on a shaky foundation. To restore respectability to the status of knowledge on this subject, it is necessary to take the drastic step of starting afresh and use recent data (assuming that the quality of data has improved over the years) and the much more advanced methods of analysis now available to come up with reasonably accurate answers to the following question: Given a set of circumstances for an intersection (approach volumes and speeds, geometry, accident history, signal design, and so on), what is the expected safety impact of installing a signal?

ACKNOWLEDGMENT

This paper is based on research performed under a contract with the Ontario Ministry of Transportation, whose support is gratefully acknowledged. The contents are the sole responsibility of the author.

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