Safety Evaluation of Continuous Shoulder Rumble Strips Installed on Freeways

Michael S. Griffith

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Abstract

Single vehicle run-off-the-road crashes result in approximately one-third of all highway fatalities and one-half million people injured annually, with a societal cost of $80 billion each year. Continuous shoulder rumble strips (CSRS) are one countermeasure used to address this significant safety problem. This study extracted data for two States (California and Illinois) from the Highway Safety Information System (HSIS) to estimate the safety effects of CSRS on freeways. Before-after evaluations of CSRS projects with the use of different comparison groups were conducted. The results from the evaluations estimate that CSRS reduce single-vehicle run-off-the-road crashes on average by 18.3 percent on all freeways (no regard to urban/rural classification) and 21.1 percent on rural freeways. Two types of potential adverse effects related to safety with CSRS were analyzed. The first type pertains to the crash risk that CSRS may present due to driver startle/panic responses. The second potential adverse effect of CSRS is crash migration. The research findings show that these potential adverse effects are insignificant.

Keywords

Continuous shoulder rumble strips, freeways, before-after evaluations, accident analysis, crash migration

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Single-vehicle run-off-the-road crashes represent a significant safety problem. A single-vehicle run-off-the-road crash is one involving one vehicle in which the first harmful event takes place off the roadway. The American Association of State Highway Transportation Officials (AASHTO) defines the roadway as being the portion of a highway, including shoulders, for highway users. The 1996 statistics from the Fatality Analysis Reporting System (FARS) show that 37,351 fatal crashes occurred, with 12,158 of these crashes being coded as single-vehicle run-off-the-road.[1]

Continuous shoulder rumble strips (CSRS) are a countermeasure used by highway agencies to prevent single-vehicle run-off-the-road crashes. CSRS operate as a countermeasure to a class of crashes related to driver inattention. Driver inattention comes in many forms, including distraction, daydreaming/competing thoughts, fatigue/drowsiness, and alcohol/drug impairment. CSRS have been experimented with since the 1950's as a simple device to alert inattentive drivers. CSRS are bands of raised material or indentations formed or grooved in the shoulders placed continuously to alert drivers starting to drift off the road. They alert drivers by transmitting sound and vibration through the vehicle. The warnings provided by CSRS give notice to drivers to take corrective action before they run off the roadway.

There have been several studies that have evaluated the safety effectiveness of CSRS. The estimates of effectiveness have been derived from a wide variety of study conditions. Given the methodologies used, the author questions the results from most of the past studies.

The National Cooperative Highway Research Program (NCHRP) synthesis report 191 *Use of Rumble Strips to Enhance Safety*(2) describes the state of the practice in 1993 with respect to placement, operational and safety effects, design, installation, cost, and service life of rumble strips. One of the most commonly referenced studies on CSRS that appears in the synthesis is a 1985 study conducted by Chaudoin and Nelson(3) that analyzed the safety effects of CSRS placed on interstate routes in California. Seven CSRS projects representing approximately 217 km (135 miles) of rural freeway were evaluated. An evaluation of 1 year of accident data before and after installation of the CSRS found that the run-off-the-road accident rate was reduced by 49 percent.

Benefit-cost analyses of CSRS are presented in a report produced for the Maine Department of Transportation (DOT) by Garder and Alexander.(4)

In 1997, the National Sleep Foundation (NSF) published a consensus report on CSRS.(5) A panel of individuals, including the author, was responsible for addressing several questions related to CSRS, which are outlined in the NSF report. The report also presents the results of the re-analysis of data from previous studies.

The most recent study cited in the literature is a study completed by Hickey(6) in which he found a 60- percent reduction in the run-off-the-road accident rate over 53 segments (560 km) on the Pennsylvania Turnpike. Hickey's study suffers from the lack of comparison group data.

This paper examines the safety effects of CSRS installed on rural and urban freeways. Before-after evaluations were conducted on projects that involved CSRS being rolled into the hot mix as part of a resurfacing project. The rumble strips placed on the asphalt shoulders are done by a roller that leaves grooves during the compaction of the asphalt on the shoulder.

Data were extracted from the Highway Safety Information System (HSIS) for California and Illinois. HSIS is a database managed by the Federal Highway Administration (FHWA) that provides quality data on numerous crash, roadway, driver, vehicle, and traffic variables from eight participating States (California, Illinois, Maine, Michigan, Minnesota, North Carolina, Utah, and Washington). The data are acquired annually from these States, processed into a common computer format, documented, and prepared for analysis. The Illinois data for this study include 63 CSRS projects (457.4 km) that were completed between 1990 and 1993. The California data include 28 CSRS projects (197.1 km) that were completed between 1988 and 1993.

Study Approach

There are two major approaches a researcher can use to study a highway safety treatment: before-after and cross-section. The latter arises when one is comparing the safety of one group of entities having some common feature (e.g., freeway sections with CSRS) with the safety of a different group of entities not having that feature (e.g., freeway sections without CSRS). This study employed a before-after approach to assess the safety effects of CSRS. The author believes the threats to the validity of conclusions drawn from before-after studies are many, but they seem to be better known and easier to avoid than threats to the validity of conclusions drawn from cross-section comparisons.

There are several approaches one can take to conduct before-after evaluations. The two approaches used in this study are the before-after evaluation with yoked comparisons and the before-after evaluation with a comparison group. A third approach (before-after evaluation with an Empirical Bayes analysis) was considered but not implemented.

It's important to remember that the primary objective of conducting before-after evaluations is to estimate the safety effect of an improvement. The estimation procedure involves the prediction of what would have been the expected number of accidents in the after period at the treated sites if no improvement had been implemented. The expected number of accidents in the after period is then compared to the observed number of accidents to estimate the safety effect of an improvement. The evaluation approaches differ in the way they predict what would have been the expected number of accidents in the after period at the treated sites if no improvement had been implemented.

The first analysis approach (before-after evaluation with yoked comparisons) is a traditional approach to the evaluation of highway safety countermeasures. It involves one-to-one matching between treatment sites (e.g., sites treated with CSRS) and comparison sites (e.g., similar sites not treated with CSRS). The comparison site must have undergone no geometric design or traffic control improvements (beyond routine maintenance) during the periods for which data are available before and after improvements were made to the corresponding treated site. Accident data are obtained for specified periods before and after the improvement was made at each treated site and for the same time periods at the matched comparison site. This approach assumes that the change in the number of accidents from the before period to after the improvement was made at each treatment site, had it been left unimproved, would have been in the same proportion as at the matching comparison site. Under this assumption, the accident frequency at each treatment site in the before period would be multiplied by the ratio of after-to-before accidents at the comparison site to predict what would have been the expected number of accidents in the after period at the treated site without the improvement.

The second evaluation approach (before-after evaluation with a comparison group) is a variation of the approach just described. A suitable comparison group of sites is selected to match the group of treated sites. The matching process is not one-to-one matching as with yoked comparisons. There can be more or fewer comparison sites than treatment sites. Preferably, the comparison group should have more sites than the treatment group. The purpose of the comparison group is still to estimate the change in accidents that would have occurred at the treated sites if the treatment had not been made. Close agreement between the treatment and comparison groups in the monthly or yearly time series of accident frequencies during the period before improvement of the treated sites is important. It is assumed that the change in the number of accidents from the before period to after the improvement was made in the treatment group, had the treatment sites been left unimproved, would have been in the same proportion as in the comparison group. Under this assumption, the accident frequency for the treatment group in the before period would be multiplied by the ratio of "after-to-before" accidents at the comparison sites to predict what would have been the expected number of accidents in the after period for the treated sites without the improvement. There's an analytical method (test for comparability) to assess the appropriateness of a comparison group.

The before-after evaluation with an Empirical Bayes (EB) analysis has gained attention in recent years.(7) The EB method was developed to identify and adjust for the bias of how sites are traditionally selected for safety improvement. Highway segments and intersections are generally selected for safety treatment because the accident history for recent years is unusually high. If the treatment sites were selected because of high short-term accident rates, a selection bias exists that must be addressed in the analysis. If an improvement is made at a location whose accident experience is high during the before study period solely due to random variation, then a lower accident experience would be expected in the after study period, whether or not an improvement had been made. This phenomenon, known as regression-to-the-mean, is a potential threat to the validity of a study. This study did not use the before-after evaluation with an EB analysis since the sites selected to be improved with CSRS were not selected because of high short-term accident rates.

Results

Illinois

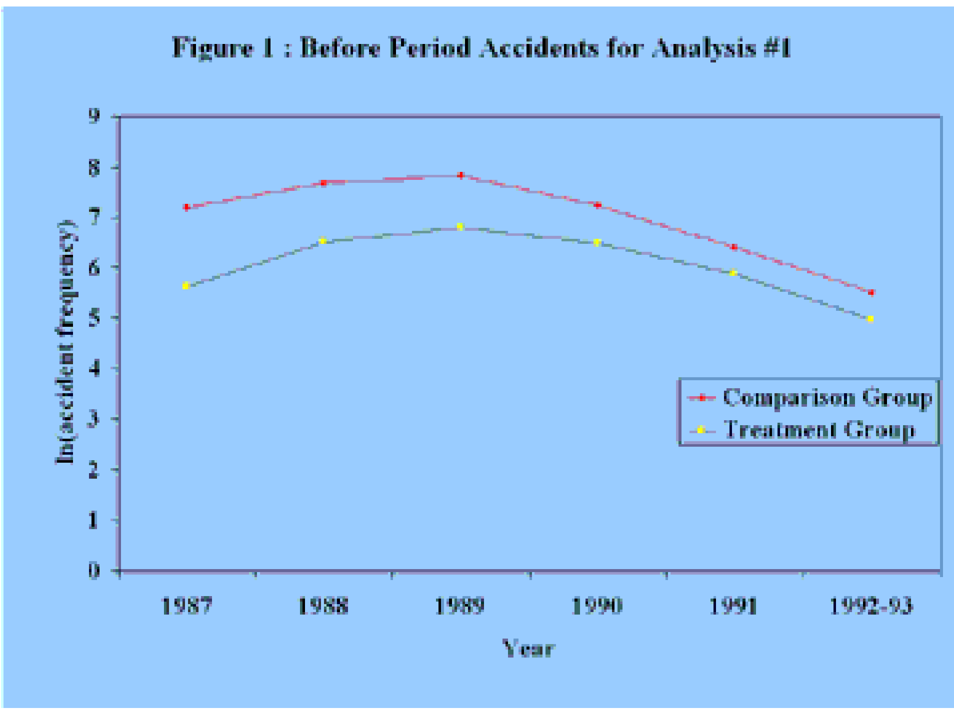
As in most States, Illinois has a practice of installing CSRS as part of maintenance projects. In Illinois, the CSRS are rolled into the hot mix as part of resurfacing and shoulder rehabilitation projects. The standard depth of Illinois' CSRS are 1.9 cm (0.75 in.) with a width of 0.9 m (3 ft.) and a spacing of 20.27 cm (8 in.). The outside boundary of the CSRS is 30.41 cm (12 in.) from the edge of pavement.

Data were obtained for 63 CSRS projects (457.4 km). At all of the 63 treatment sites, CSRS were installed in both directions of the highway on the inside and outside shoulders. All of the treatment sites and comparison sites used in the analysis of the Illinois data are located on rural and urban freeways.

Three primary comparison groups were considered for the analysis of the Illinois data. The first analysis examined multi-vehicle accidents at the treatment sites (all 63 projects) as the comparison group (comparison group #1). The second analysis involved yoked comparison sites (comparison group #2). These sites were selected to be adjacent and upstream to the treatment sites. They were selected to be upstream since it has been hypothesized that CSRS may cause crash migration, i.e., move the site of a crash to another site downstream without rumble strips. This potential adverse effect was examined specifically with crashes involving alcohol/drug-impaired drivers and the findings of this analysis are presented later in the report. Fifty-five of the treatment sites were used in this analysis and were matched with 55 comparison sites. The third analysis examined single-vehicle run-off-the-road accidents at selected treatment sites (29 projects) and a large comparison group of single-vehicle run-off-the-road accidents (comparison group #3).

Most of the analysis of the Illinois data focused on the 55 treatment sites and the 55 yoked comparison sites. The rural freeway and urban freeway mileage for the 55 treatment sites and 55 comparison sites is as follows: rural freeways (treatment group) - 316.1 km, urban freeways (treatment group) - 107.8 km, rural freeways (comparison group) - 309.0 km, and urban freeways (comparison group) - 80.9 km. The percent increase in total million vehicle-miles traveled (MVMT) in the comparison and treatment groups from the before to after periods is 4.4 percent and 4.2 percent, respectively. In the before and after periods, the treatment group has a higher representation of MVMT on urban freeways than the comparison group. In the before period, urban freeways account for 61 percent of the total MVMT within the treatment group and 55 percent of the total MVMT within the comparison group.

The first analysis used multi-vehicle accidents at the treatment sites (all 63 projects) as the comparison group (comparison group #1) to estimate the treatment effect of CSRS. The target accidents in the treatment group are single-vehicle run-off-the-road accidents.



The idea of using a subset (i.e., multi-vehicle accidents) of the total accidents at the treatment sites as the comparison group is intuitively appealing given that the target accidents (single-vehicle run-off-the-road accidents) and comparison accidents (multi-vehicle accidents) are probably exposed to the same conditions in the before-to-after periods. Figure 1 displays the before-period accidents for the treatment group (63 sites) and comparison group # 1 with the ordinate expressed as the natural logarithmic (ln) of accident frequencies. It can be seen from Figure 1 that accidents are changing differently across certain before period years in the comparison and treatment groups. It is noted that the line segments for the treatment and comparison groups are not parallel in the time periods from 1987 to 1988 and 1989 to 1990. The observance of non-parallel line segments is an indication that crashes in the comparison group are not following the pattern of crashes in the treatment group, and the comparability of the comparison group is brought into question.

A statistical analysis was conducted to check for comparability of comparison group #1 in the before period. The likelihood ratio chi-square test was used to test for comparability. A large chi-square value of 187.6 was obtained, which indicates that the comparison group lacks comparability during the before period, i.e., the comparison crashes do not reliably follow the pattern of the treatment crashes. Further analysis of multi-vehicle accidents as the comparison group was not considered.

The second analysis employed the before-after method with yoked comparisons for the data involving the 55 treatment sites and 55 comparison sites. Table 1 shows the total single-vehicle run-off-the-road accidents in the before period for the 55 treatment sites and 55 yoked comparison sites (comparison group #2).

Table 1: Before-Period Accidents for Treatment Group (55 sites) and Comparison Group #2

Period	Before						
Year(s)	1987	1988	1989	1990	1991	1992-1993	Total
Treatment	276	644	863	596	310	112	2801
Comparison	240	515	646	521	259	107	2288

A crude check was conducted on the data to assess the potential for regression-to-the-mean. This check is done by comparing the before-period accident rates for the treatment and comparison groups.[8] The before-period accident rate (single-vehicle run-off-the-road accidents per MVMT) for the comparison sites and treatment sites is 0.29 and 0.34, respectively. The accident rates of the two groups are very similar, which supports the fact that Illinois did not select the treatment sites because they had an unusually high accident rate.

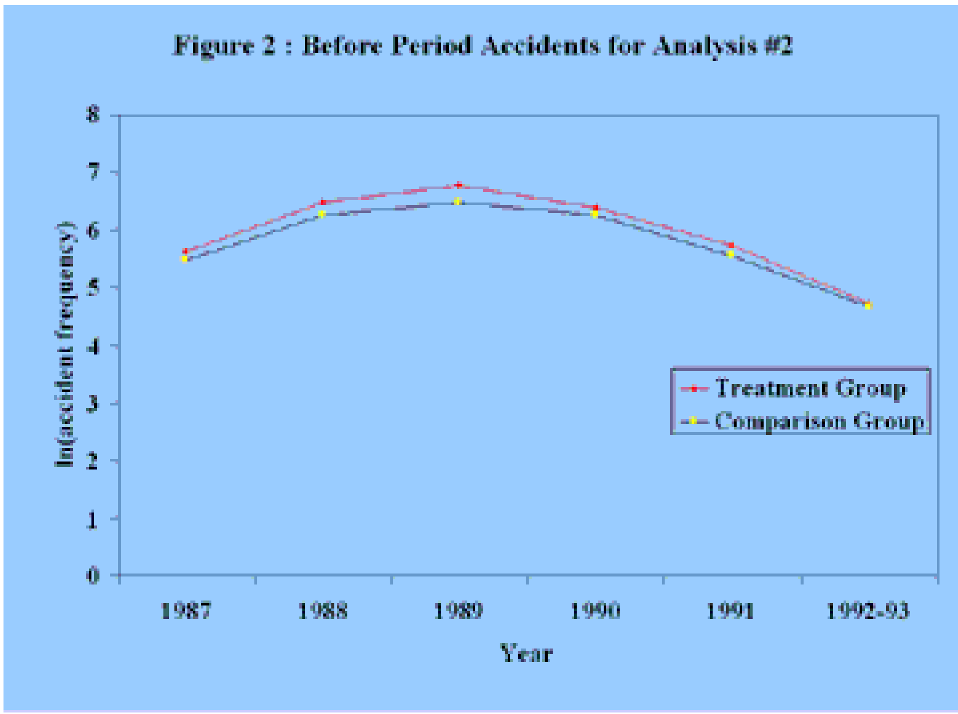


Figure 2 : Before Period Accidents for Analysis #2

Figure 2 displays the before-period accidents from table 1 with the ordinate expressed as the natural logarithmic (ln) of accident frequencies. The time series of accidents appear to be in close agreement. The line segments are fairly parallel so there is no indication that comparability of the comparison group is suspect. From the likelihood ratio chi-square test, the fairly small chi-square value of 6.1 was obtained, which provides further support for the appropriateness of the comparison group.

The odds ratio was calculated to determine the effectiveness of CSRS in addressing single-vehicle run-off-the-road accidents. The odds ratio for this data is:

$$(2801)(1833) / (1895)(2288) / (1+1/1895+1/2288) = 1.183 \qquad (1)$$

The average safety effect of CSRS is estimated to be a reduction of single-vehicle run-off-the-road accidents by 18.3 percent. The standard deviation of this estimate of average safety effect is ±6.8 percent.

Another approach to analyzing this same data is to obtain a weighted average effect of the 55 individual pairs of treatment and comparison sites. The standard odds ratio used in the previous analysis does not use an explicit weighting scheme to give more weight to sites that have more crashes. The standard odds ratio works with the sums of the before and after crashes to calculate the overall effectiveness of a treatment resulting in the averaging of the estimated effects at each of the treatment sites. A weighted average odds ratio was calculated by using a logit model. The CATMOD procedure from Statistical Analysis Software[9] was used to analyze the data in the form of a logit model.

The equation for the logit model used for the 55 treatment sites and 55 yoked comparison sites takes the following form:

$$L = \beta_1 + \beta_2 X_2 + \Sigma \beta_i X_i \qquad (2)$$

Where: L represents an estimated logit, either $\ln(A_i/B_i)$ or $\ln(a_i/b_i)$. A_i represents the number of after- period accidents at treatment site i, B_i represents the number of before period accidents at treatment site i, a_i represents the number of after-period accidents at comparison site i, and b_i represents the number of before-period accidents at comparison site i.

$ß_1$ is an estimate of the intercept parameter.
X_2 is a binary variable that distinguishes between treatment sites and comparison sites.
$ß_2$ is the estimated regression parameter for X_2.
X_i (X_3 through X_{56}) is a set of 54 dummy variables representing the 55 treatment-comparison pairings. $_i$ is the estimated regression parameter for the ith dummy variable, i.e., 54 regression parameter estimates will be generated ($ß_3$ through $ß_{56}$).

The logit model estimates that the average safety effect of CSRS is a 10.7 percent reduction in single-vehicle run-off-the-road crashes. The model defined here does not perfectly fit the data. There is some error or residual left over to reflect the degree on how well the model fits the data. The chi-square associated with the residual provides an estimate of the degree to which the 55 individual estimates of treatment effectiveness are equivalent. The large chi-square value (68.09) is an indication that there is not a homogenous estimate of effectiveness across treatment sites. The sites in a treatment group typically do not form a perfectly homogenous group so this finding is not surprising. While in an average application of CSRS one may expect a 10.7 percent reduction in single-vehicle run-off-the-road accidents, it should not be expected at all sites.

Total injury crashes for single-vehicle run-off-the-road involvements for the 55 treatment and comparison sites were also analyzed. The odds ratio for the injury data is:

$$(1135)\ (765)\ /\ (877)\ (874)\ /\ (1 + 1/877+1/874) = 1.130 \qquad (3)$$

The average safety effect of CSRS is estimated to be a reduction of single-vehicle run-off-the-road crashes resulting in an injury by 13 percent. The standard deviation of this estimate of average safety effect is ±11.7 percent.

There was interest in this study to exercise the before-after evaluation method with a large comparison group (comparison group #3). The challenge was to set up a proper evaluation to allow for the use of a large comparison group (as measured by the number of accidents). As one would expect, there is significant variation in the before and after time periods across the 63 CSRS projects identified. This significant variation is due to the fact that the CSRS projects were implemented in different years and had construction periods with various time durations. The projects cover four sets of before time period intervals: 1988-1991, 1987-1990, 1989-1992, and 1990-1993.

The variation in coverage from year to year in the before and after periods in the treatment group needs to match the variation in coverage from year to year in the before and after periods in the comparison group. One approach to handling this requirement is to select a set of treatment sites that have fairly common before and after periods and then select a group of comparison sites that have before and after periods that closely resemble the typical before and after periods of the selected treatment group. The years 1988, 1989, and 1990 have the greatest coverage in the before period for the 63 treatment sites. A total of 29 of the 63 treatment sites provide sufficient coverage for these years. These 29 treatment sites were selected to conduct an analysis using the before-after method with a comparison group. A total of 11 sites were selected for the comparison group. These 11 sites represent all of the non-treatment mileage (locations without CSRS) on 11 freeway routes in Illinois.

The rural freeway and urban freeway mileage for the treatment group (29 sites) and comparison group #3 is as follows: rural freeways (treatment group) - 219.3 km, urban freeways (treatment group) - 78.5 km, rural freeways (comparison group) - 1878.7 km, and urban freeways (comparison group) - 473.9 km. The percentage increase in total MVMT from the before to after period in comparison group #3 and its associated treatment group was 5.5 percent and 5 percent, respectively. Separate analyses were conducted for the urban and rural freeway data.

The test of comparability was conducted for the rural freeway data from comparison group #3 and this subset of data from comparison group #3 was found to be appropriate to use for analysis. The odds ratio using the rural freeway data from comparison group #3 and the treatment group (29 sites) is 1.211. The average safety effect of CSRS installed on rural freeways is estimated to be a reduction of single-vehicle run-off-road accidents by 21.1 percent. The standard deviation of this estimate of average safety effect is ±10.2 percent.

The analysis of the injury crash data estimates the average safety effect of CSRS installed on rural freeways to be a 7.3 percent reduction in single-vehicle run-off-road **injury** accidents. The standard deviation of this estimate of average safety effect is ±15.5 percent.

The subset of data from comparison group #3 representing urban freeways did not meet the check of comparability. Therefore, the analysis of urban freeway data was not pursued further.

California

For California, a total of 28 CSRS projects (197.1 km) were identified for the study. All of the analyses conducted with the California projects combined rural and urban freeway data. Only a limited sample (32.0 km) of urban freeways were treated with CSRS. The completion dates for these projects occurred between the years 1988 and 1993. At 19 of the treatment sites, CSRS were installed in both directions of the highway on the inside and outside shoulders. At the remaining nine treatment sites, CSRS were installed in only one direction of the highway on the inside and outside shoulders.

In California, shoulder rumble strips are 1.9 cm (0.75 inches) or less in height if raised or 2.5 cm (one inch) or less in depth if indented and extend along the highway shoulder.[(10)] The maximum width of a shoulder rumble strip shall not exceed 0.9 m (3 feet).

It should be noted that approximately 18 percent of the mileage in the treatment group allows for the use of bicycles. This study did not address issues specific to bicyclists. Highway agencies have reported concerns expressed by bicyclists about shoulder rumble strips. Shoulder rumble strips can be a nuisance to bicyclists, which may encourage them to ride in the traveled way in situations where the highway agency would prefer for them to use the shoulder.

Seventeen comparison sites were identified, totaling 132.4 km. Eighty-four percent of the mileage in the treatment group is located on rural freeways and 76 percent of the mileage in the comparison group is located on rural freeways. A similar trend in traffic growth was recognized in the comparison and treatment groups. The percent increase in total MVMT in the comparison and treatment groups from the before to after periods was 5.5 percent and 5.3 percent, respectively. At the rural freeway locations, the percent increase in total MVMT in the comparison and treatment groups from the before to after periods is 5.1 percent and 4.7 percent, respectively.

As in Illinois, the selection of the CSRS projects in California was not motivated by safety concerns. Caltrans (California Department of Transportation) has a practice of installing CSRS as part of routine maintenance projects. The before period single-vehicle run-off-the-road accident rate (accidents per MVMT) for the treatment and comparison groups are 0.14 and 0.12, respectively. The accident rates of the two groups are very similar, which supports the fact that California did not select the treatment sites because they had an unusually high accident rate.

The only approach used to evaluate the California data was the before-after evaluation with a comparison group. Under this approach, two comparison groups were identified for use in analysis of the data. The first comparison group examined with the California data was multi-vehicle accidents that occurred at the treatment sites. This comparison group was found to be inappropriate for use in the analysis. It did not meet the test of comparability. The second comparison group used a set of sites adjacent and upstream to the treatment sites. As in Illinois, the ending milepost of a comparison site was selected to be at least 0.48 km away from the beginning milepost of a treatment site. A matching of comparison sites to treatment sites was attempted, but 11 of the treatment sites could not been be matched with a specific comparison site. The 17 comparison sites identified represent the comparison group as a whole for all 28 treatment sites.

The comparison group satisfied the check of comparability and was determined to be acceptable for use in the analysis. The odds ratio for the data using this comparison group is:

$$(579)(364) / (469)(417) / (1+1/469+1/417) = 1.073 \qquad (4)$$

The average safety effect of the CSRS is estimated to be a reduction of single-vehicle run-off-the-road accidents by 7.3 percent. The standard deviation of this estimate of average safety effect is ±13.4 percent.

Potential Adverse Effects

There is an ongoing debate on whether CSRS may have negative safety effects under certain circumstances. Previous research has alluded to the potential adverse effects of CSRS on safety but this research made no attempts to study these effects. Based on the available data (study data from Illinois was used), this research explored the potential for adverse safety effects to exist and estimated what the magnitude of these effects may be. There are two types of potential adverse effects related to safety with CSRS. The first type pertains to the possibility that CSRS may cause certain drivers to overreact or panic to their warning, resulting in loss of control of their vehicles. In an attempt to hold a State liable, some drivers may even claim that a crash occurred because the CSRS caused them to lose control of their vehicle . The crash risk that CSRS may present due to driver startle/panic responses was analyzed.

The second potential adverse effect of CSRS is crash migration. Crash migration occurs when a driver is temporarily saved from a crash at a treated site but crashes downstream of the treatment area or at a different point in the network.

Drivers that panic to the warning provided by CSRS and lose control of their vehicle may get into a single-vehicle or multi-vehicle accident. The creation of police-reported single-vehicle accidents due to driver startle/panic responses would be reflected in the total count of single-vehicle run-off-the-road crashes in the after period of the treatment group. The failure cases related to multi-vehicle accidents required further examination. Using the Illinois data, the before-to-after histories of multi-vehicle accidents at the 55 treatment and 55 comparison sites were compared. After the installation of CSRS, a 23 percent decrease in multi-vehicle accidents was found at the treatment sites and a 23 percent decrease in multi-vehicle accidents was also found at the comparison sites. Given this result, there's no evidence that CSRS are causing an increase in multi-vehicle accidents within the boundaries of the treatment area due to driver startle/panic responses. There was not a smaller percentage decline in multi-vehicle accidents found in the treatment group, which would imply a negative effect. The 23-percent decline in multi-vehicle accidents at the treatment sites should also not be interpreted as the CSRS having a positive effect on this class of accidents since the same percentage decline was detected at the comparison sites.

The study of crash migration issues was challenging with the available information. However, the Illinois data were explored to assess the potential migration problem. The primary issue examined is whether CSRS prevented "misbehaving" drivers from running off the road at the expense of these drivers creating multi-vehicle accidents. "Misbehaving" drivers are drivers that shouldn't be on the highway given the risks they present to other drivers and themselves. In this study, they included alcohol/drug-impaired drivers and fatigued/drowsy drivers. The average safety effect of CSRS involving alcohol/drug-impaired drivers was estimated to be 36.2 percent reduction of single-vehicle run-off-the-road accidents. Given this estimated "benefit", the potential for crash migration exists for alcohol/drug-impaired drivers. CSRS may alert an alcohol/drug-impaired driver, allowing him or her to maintain control of their vehicle long enough to get through the highway segment with rumble strips before losing control again and crashing downstream where no CSRS exist. The crash downstream may be a multi-vehicle accident caused by the alcohol/drug-impaired driver that involves harm to innocent victims. It was just reported that there's no evidence that CSRS increased the total number of multi-vehicle accidents within the boundaries of the treatment sites. However, it's still possible that alcohol/drug-impaired drivers kept on the highway by CSRS may be causing multi-vehicle crashes to occur downstream, outside the boundaries of a treatment area. It must also be stated that CSRS may have a positive impact on alcohol/drug-impaired drivers by causing them to pull over to sober up or letting a less impaired or unimpaired passenger to take over the driving.

The findings for fatigued/drowsy drivers are unclear. The average safety effect of CSRS involving fatigued/drowsy drivers was estimated to be 23.6 percent increase of single-vehicle run-off-the-road accidents with the standard deviation of this estimate to be ±20.6 percent. The potential for migration was not established for fatigued/drowsy drivers since CSRS were not shown to benefit them. Therefore, the analysis of crash migration issues only involved alcohol/drug-impaired drivers.

The magnitude of the potential problem of alcohol/drug-impaired drivers being kept on the highway by CSRS was assessed. The Illinois variable used to analyze the alcohol/drug-impairment issue was the "contributing cause" variable. This variable captures the first contributing factor of the crash according to the subjective judgement of the investigating police officer. For the 55 treatment sites and 55 comparison sites, the number of crashes reported as alcohol/drug-impairment cases according to the contributing cause variable are: 160 crashes (treatment: before period), 83 crashes (treatment: after period), 113 crashes (comparison: before period), and 92 crashes (comparison: after period).

The comparison group predicts there would have been a 22.8 percent reduction in single-vehicle run-off-the-road crashes involving alcohol/drug-impaired drivers at the treatment sites if CSRS were not installed. Applying this predicted 22.8 percent reduction, one would have expected 130 single-vehicle run-off-the-road crashes in the treatment group's after period. The actual number of single-vehicle run-off-the-road crashes that occurred in the treatment group's after period is 83. Therefore, the estimated reduction in the number of single-vehicle run-off-the-road accidents involving alcohol/drug-impaired drivers attributable to CSRS is 47 (130 - 83 = 47). The estimated 47 alcohol/drug-impaired drivers saved by the CSRS are the group of unsafe drivers that may have cause some multi-vehicle crashes to occur downstream from a treated site.

It was reported earlier that a 18.3 percent reduction in single-vehicle run-off-the-road crashes is the estimated average safety effect of CSRS using comparison group #2. Applying this 18.3 percent reduction, it is estimated that 349 single-vehicle run-off-the-road crashes were prevented by CSRS. Clearly, CSRS appear to be beneficial even when one considers their possible negative effect of "creating" a maximum of 47 multi-vehicle accidents involving alcohol/drug-impaired drivers. The ratio of single-vehicle run-off-the-road crashes saved versus multi-vehicle crashes possibly caused by alcohol/drug-impaired drivers is 7.4 (349/47). One concern is whether the multi-vehicle accidents involving alcohol/drug impaired drivers tend to be more severe than single-vehicle run-off-the-road accidents involving alcohol/drug impaired drivers. It's of interest to examine the trade-off between single-vehicle run-off-the-road accidents involving alcohol/drug-impaired drivers and multi-vehicle accidents involving alcohol/drug-impaired drivers. The severity distributions of single-vehicle run-off-the-road crashes occurring on freeways involving an alcohol/drug-impaired driver and multi-vehicle crashes involving an alcohol/drug-impaired driver occurring on freeways only and on all other roadway types (non-freeways) are displayed in table 2. These distributions were generated with Illinois 1993-1995 data.

Table 2: Accident Severity Distributions Involving Alcohol/Drug-Impaired Drivers

Severity	Single-Vehicle Run-Off-the-Road (Freeway)	Multi-Vehicle (Freeway)	Multi-Vehicle (Non-Freeway)
No injury (Property Damage Only)	76 (33%)	122 (40%)	104 (28%)
Injury	147 (64%)	169 (56%)	241 (66%)
Fatal	7 (3%)	11 (4%)	22 (6%)
Total	230 (100%)	302 (100%)	367 (100%)

A higher percentage of single-vehicle run-off-the-road accidents on freeways involving an alcohol/drug-impaired driver resulted in a non-fatal injury than for multi-vehicle accidents involving an alcohol/drug impaired driver on freeways. In contrast, a slightly higher percentage of multi-vehicle accidents on non-freeways involving an alcohol/drug-impaired driver resulted in a non-fatal injury than for a single-vehicle run-off-the-road accidents on freeways involving an alcohol/drug impaired driver. The multi-vehicle accidents on freeways involving an alcohol/drug impaired driver resulted in a one percentage point higher of fatal injury cases than for single-vehicle run-off-the-road accidents on freeways. The situation is worse for multi-vehicle accidents on non-freeways in that 6 percent of these crashes resulted in a fatality compared to 3 percent for single-vehicle run-off-the-road accidents on freeways involving an alcohol/drug impaired driver. It appears that the trade-off of single-vehicle run-off-the-road crashes on a freeway with multi-vehicle crashes on the freeway is approximately equal from a severity stance. However, the transfer of single-vehicle run-off-the-road crashes on the freeway involving an alcohol/drug-impaired driver with multi-vehicle crashes on a non-freeway facility involving an alcohol/drug-impaired driver would likely result in a greater number of fatal crashes (an increase from 3 percent to 6 percent).

Another possibility involving alcohol/drug-impaired drivers is that they stay alert enough after hitting CSRS to avoid running off the road on the freeway, but later on their same trip, while driving on a non-freeway facility (e.g., two-lane rural road) they lose control of their vehicle and drift off the road. What is the impact of trading off single-vehicle run-off-the-road crashes on freeways involving alcohol/drug-impaired drivers with single-vehicle run-off-the-road crashes on non-freeways involving alcohol/drug-impaired drivers? The single-vehicle run-off-the-road crashes on non-freeways involving alcohol/drug-impaired drivers have proportionally more (5 percent vs. 3 percent) fatal ones than the single-vehicle run-off-the-road crashes on freeways involving alcohol/drug-impaired drivers.

Philosophically, it can be argued that any highway safety device that keeps a percentage of alcohol/drug-impaired drivers on the road presents a significant risk to innocent drivers and is morally wrong. However, as stated earlier, for each multi-vehicle crash possibly created by CSRS involving an alcohol/drug impaired driver, one can expect a reduction of 7.4 single-vehicle run-off-the-road crashes. This finding is conservative since it is based on using the estimated maximum number of multi-vehicle crashes involving an alcohol/drug-impaired driver expected to occur. Clearly, the estimated safety benefits of CSRS outweigh their potential adverse effects. This statement holds true when one even considers the analysis that examined crash severity distributions. The maximum estimated increase in fatal crashes would be from 1.41 (3 percent x 47 crashes) to 2.82 (6 percent x 47 crashes) during the time period used in the analysis of the Illinois data with comparison group #2.

Conclusions

This study provides timely information regarding the safety effects of CSRS. Although there are recent studies in the literature related to CSRS, questions have been raised about the validity of past research. The use of a comprehensive data base (HSIS) and comparison group data in this study provided an advantage over prior studies. The main advantage of employing comparison data in a before-after analysis is the ability to account for those factors that might have produced some or all of the change in crash frequency at the treatment sites. This study evaluated CSRS applied on different occasions (time periods) and sites. It is advantageous to have a treatment implemented at different sites and points in time to reduce the possibility that a single extraneous factor (e.g., a change in accident reporting threshold, a rescinding of the National Maximum Speed Limit, etc.) could have impacted the results.(11)

The general findings indicate that CSRS provide a safety benefit to motorists on freeways. More weight is given to the Illinois findings than the California findings since larger samples were obtained for Illinois and the Illinois data are richer in detail. Based on the Illinois data, the best estimates of the average safety effectiveness of CSRS are:

- On all freeways, 18.3 percent reduction in **total** single-vehicle run-off-the-road accidents.
- On all freeways, 13 percent reduction in **injury** single-vehicle run-off-the-road accidents.
- On rural freeways, 21-percent reduction in **total** single-vehicle run-off-the-road accidents.

Sophisticated benefit-cost analyses were not conducted in this study. However, the cost to install CSRS is inexpensive. South Dakota DOT provided an estimate of approximately $217/km to roll-in CSRS for all shoulders on both sides of the highway.(12) It was estimated for one set of data that 349 single-vehicle run-off-the-road accidents were prevented in a short after period (average of 3 years per treatment site) over 423.9 km. CSRS prevented 0.82 accidents per kilometer per 3 years. In 1997 dollars, the average comprehensive cost of a single-vehicle run-off-the-road accident is $62,200.(13) Comprehensive costs incorporate not only economic losses, but a valuation for less tangible consequences such as "pain and suffering" and loss of life. Therefore, simple arithmetic shows that CSRS provide a substantial return to society if it is true that approximately one single-vehicle run-off-the-road accident (at the average cost of $62,200) is prevented every three years based on an investment of $217.

The potential adverse effects of CSRS were analyzed with the available data and were estimated to be insignificant. However, there's a need for video studies in the field to better understand what occurs when a driver impacts CSRS.

In terms of additional research, it is suggested that locations selectively improved with CSRS in order to reduce a significant run-off-the-road accident problem be evaluated. The estimated crash reduction effectiveness of CSRS from this study should be a reasonable value to expect from a broad use of CSRS. However, a different experience may emerge for high crash locations treated with CSRS. It's also recommended that the safety experience of milled-in CSRS be determined. There's an ongoing debate about the effectiveness of rolled-in versus milled-in CSRS. Finally, studies of CSRS installed on non-freeways (e.g., two-lane rural road) should be conducted.

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