An Analysis of The Effects of Installing Pedestrian Countdown Timers on the Incidence of Pedestrian Crashes in the City of Detroit Michigan

Brad Huitema, Ron Van Houten, and Hana Manal

Western Michigan University

Author Note

This study was supported from a Grant from the Michigan Department of Transportation. The authors would like to thank Deirdre Thompson and the other members of the review panel for their helpful suggestions. Correspondence concerning this article should be addressed to Brad Huitema or Ron Van Houten, Department of Psychology, Western Michigan University, Wood Hall, Room 3700, Kalamazoo, MI 49008. E-Mail: ron.vanhouten@wmich.edu

Keywords: Pedestrian Countdown Timers, crash analysis, pedestrian safety

Abstract

In large cities, pedestrians account for 40% to 50% of traffic fatalities. Previous studies based on relatively small samples have concluded that Pedestrian Countdown Timers can reduce pedestrian crashes at signalized intersections. The purpose of the present study was to examine the effect of Pedestrian Countdown Timer (PCT) on crashes in a large city with a long baseline and relatively patterned introduction of PCTs. No general drop-off in crash frequency was found throughout a baseline interval of over five-years; only when the PCT were introduced in large numbers was a consistent crash reduction observed. Because the magnitude of the crash reduction was shown to be a function of the extent to which the timers were introduced, the evidence for an intervention effect is strong. The results of the PCT statistical analysis provided unequivocal evidence that the pedestrian countdown timers reduced pedestrian crashes. The size of the effect in the Detroit sample was quite large (70% crash reduction). Like all treatments that target pedestrians, the installation of PCTs should be expected to produce the largest benefits in locations with very poor pedestrian safety compliance before installation.

Pedestrian countdown timers (PCT) display the available crossing time in seconds to complement the conventional flashing DON'T WALK phase of a traffic signal cycle. The Manual on Uniform Traffic Control Devices provides guidance on the pedestrian countdown timer and presents it as the standard pedestrian signal configuration (MUTCD, 2009).

Pedestrian countdown signals were shown to be more intuitive for users in communicating the amount of available crossing time at intersections, which also may result in better levels of service for pedestrians at signalized intersections. The Florida Department of Transportation (FDOT), for example, conducted a study to determine pedestrians' understanding of the traditional flashing DON'T WALK sign versus the pedestrian countdown timer. The study showed that the pedestrian countdown timer was more intuitive than the traditional flashing DON'T WALK display, which contributed to pedestrians making better decisions about when to begin crossing and when to wait for the next WALK signal. The study showed that, under the traditional flashing DON'T WALK signal, pedestrians were more likely to start crossing during the flashing DON'T WALK phase, run out of time while crossing, return to the starting side of the crossing, or even stop in the roadway when the light changed (Huang & Zegeer, 2000).

Other studies have shown that a pedestrian countdown timer reduces crashes when compared to a traditional flashing DON'T WALK signal (Eccles, Tao, & Mangum, 2007; Markowitz, Sciotino, Fleck, & Yee, 2006). The Minnesota Department of Transportation (MnDOT) measured the change in pedestrian understanding by measuring the number of pedestrians who successfully crossed an intersection before the flashing

DON'T WALK phase ended. Their research showed an average 12% increase in successful pedestrian crossings with the implementation of pedestrian countdown timers (Institute of Transportation Engineers (ITE), 2007).

Additionally, the use of pedestrian countdown timers showed that pedestrians were less likely to cross near the end of a pedestrian WALK phase, if it appeared that there was insufficient time, and that pedestrians that were crossing during the flashing DON'T WALK phase increased their walking speed in an attempt to finish the crossing within the amount of time shown on the countdown signal (ITE, 2007).

A summary report of various crash reduction methods and their effectiveness was prepared by the FHWA (2007) and included pedestrian countdown timers. When countdown timers are added to existing pedestrian signals, crashes have been shown to decrease by 25% (FHWA, 2007).

In 2006 the City of Detroit began a program to replace regular pedestrian signals with count down timers. The purpose of this study was to evaluate the crash reductions using a much larger sample and a phased introduction plan that allowed for a careful statistical analysis of the crash results.

Statistical Background

The method for analyzing the safety of roadways and intersections that is most commonly recommended by safety researchers is some version of Bayesian modeling (either full or empirical). The essential purpose of these models is to provide a sound estimate of the expected frequency of crashes (or some other outcome) for some site or sites that will be exposed to some intervention. Once this estimate is available, it can be used as a baseline against which the actual accident frequency is compared in a Before-

After Intervention design. Usually the number of sites that receive the intervention of interest is relatively small, and the number of untreated comparison sites is substantial.

Estimating expected frequency frequently involves two types of information. First, a model of the expected frequency of crashes is computed for each untreated site (i.e., using the number of days in the Before-Intervention period as the predictor variable in a negative binominal regression). This estimate and the associated dispersion measure are acknowledged in an evaluation of the adequacy of the estimate of the number of crashes to be expected at the end of the Before-Intervention period for each intervention site. The second type of information used to estimate the expected number of crashes for each to-be-treated site is the actual number of crashes in the pre-intervention period for the site.

The two types of information are then combined in such a way that the reliability of each source of information is acknowledged in an optimal estimate. This optimal estimate is then subtracted from the actual frequency of crashes. This difference is computed for each treated site. Then, an overall effect estimate is computed by integrating the information from the individual sites; statistical inference is applied to this measure.

Although the Bayesian approaches are now acknowledged as the preferred approach for typical limited data structures in safety research, the approach used in the current study is different. The reason for this departure is that this study has an unusually rich database that includes ten years of monthly crash information at 449 sites in Detroit. Before and after intervention data were collected on 362 of these sites; the remaining 87 were used as control sites. The PCT intervention was initiated on different dates;

information regarding these dates and sites was available and was utilized in a new comprehensive intervention model.

Method

New Analysis

Since the research team had the luxury of (a) very extensive crash data for both those sites that were exposed (n_T = 362) and those not exposed (n_C = 87) to the PCT intervention and (b) the interventions were characterized by staggered intervention dates with both slow and rapid periods of intervention introduction, the research team was able to develop an analytic procedure that provides two different types of control: within-site and between-site. Both within- and between-site aspects of the design were acknowledged in the outcome analysis. The focus of the within-site aspect is on evaluating the intervention effect that occurs across time using an approach that relies on what the team calls an intervention penetration variable (described subsequently). The between site analysis allows the team to compare the within-site change in the intervention sites with the within-site change in control sites that were not exposed to the countdown timer intervention.

In addition, the large number of sites resulted in an outcome measure (i.e., the number of crashes per month in the whole sampling unit) having a disturbance distribution that was exceeding close to normal. This distribution is shown below in the lower left panel of Figure 1. Exact normality, which never happens in practice, is present when all of the dots in the normal probability plot (shown in the upper left panel) are on the straight line.

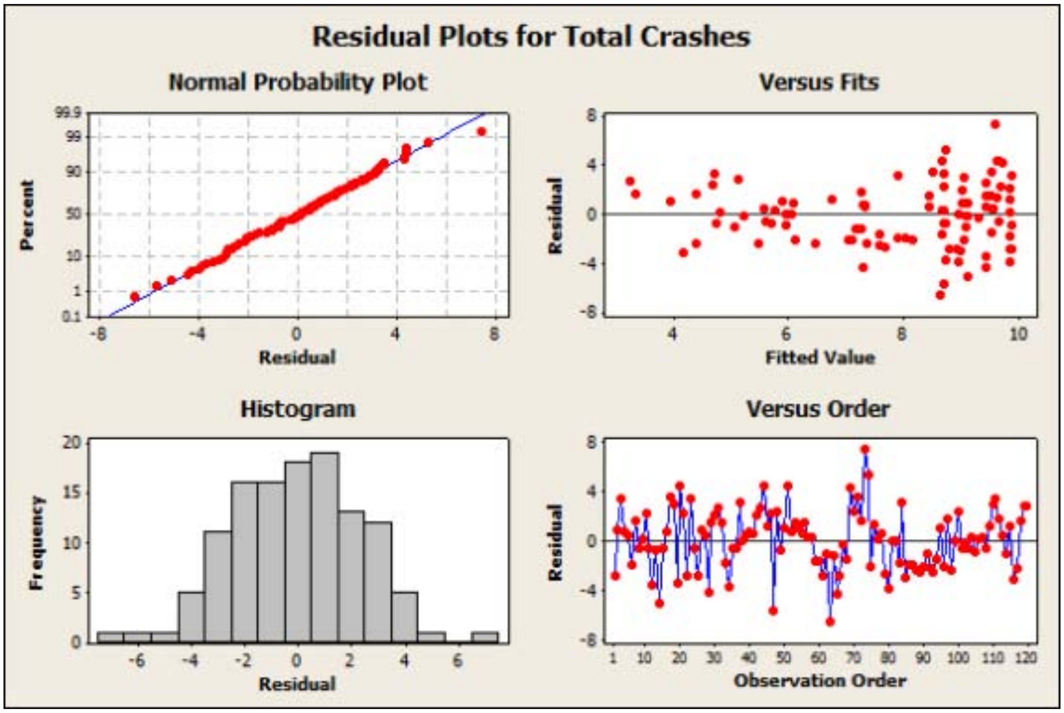

Figure 1: Diagnostic Plots for Evaluating the Adequacy of the Adopted Model.

A more detailed look at the normality issue is shown below in Figure 2; this figure includes both the confidence interval on the normal probability estimates and the results of the Anderson-Darling test for normality.

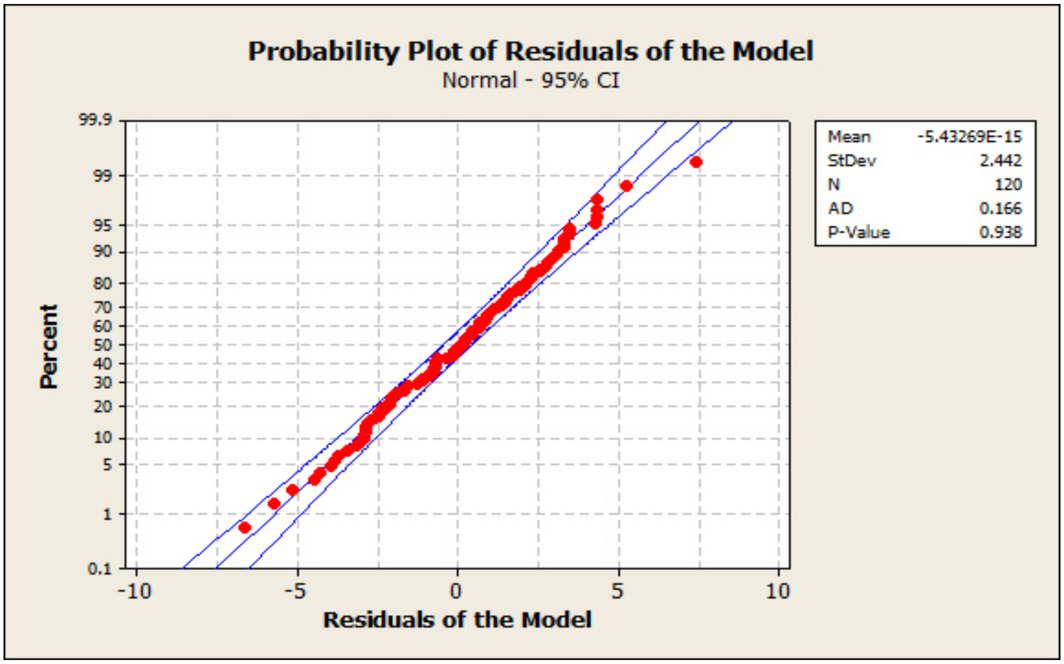

Figure 2: Probability Plot and AD Test that Confirm the Assumed Normality.

The *p*-value for this test is 0.94, which demonstrates that no evidence is present for a departure of the disturbance distribution from normality (i.e., strong evidence for non-normality is demonstrated when the *p*-value is ≤ 0.05). This is important because it supports the argument that the data conform to the assumptions underlying the general interrupted time-series model used for the within sites analysis (McKnight, McKean, and Huitema, 2000).

A Major Distinction Between the Time-Series Intervention Model and the Empirical Bayes Approach: Dynamic Change Pattern vs. Simple Pre-Post Difference

The adopted time-series regression model assumes autoregressive errors and normal disturbances. Because the within sites analysis examines the dynamics of change over a 120-month period rather than a single before-after change estimate (i.e., the typical estimator using the more traditional Empirical Bayes approach), a detailed examination of the nature of intervention effects throughout the duration of the experiment was possible.

Intervention Penetration Variable

The PCT intervention was introduced according to a schedule that began with a six-month baseline period during which the PCT were not introduced to any sites. Next, the PCT were gradually introduced to a small number of sites across several years. Then, in the last two years of the study there were two different periods where several PCT were introduced to many sites simultaneously. This complex and varied intervention introduction schedule can be captured by using what is called the intervention penetration variable. It is a measure that indicates the extent to which the intervention penetrates the sampling unit (i.e., the set of 362 treatment sites that eventually received the intervention) across the 120 months of the study.

The penetration variable ranges from zero through one. Zero indicates months during which no interventions are applied to any of the treatment sites; one indicates the month at which all treatment sites received the intervention. The penetration function for the installation of PCT is shown Figure 3.

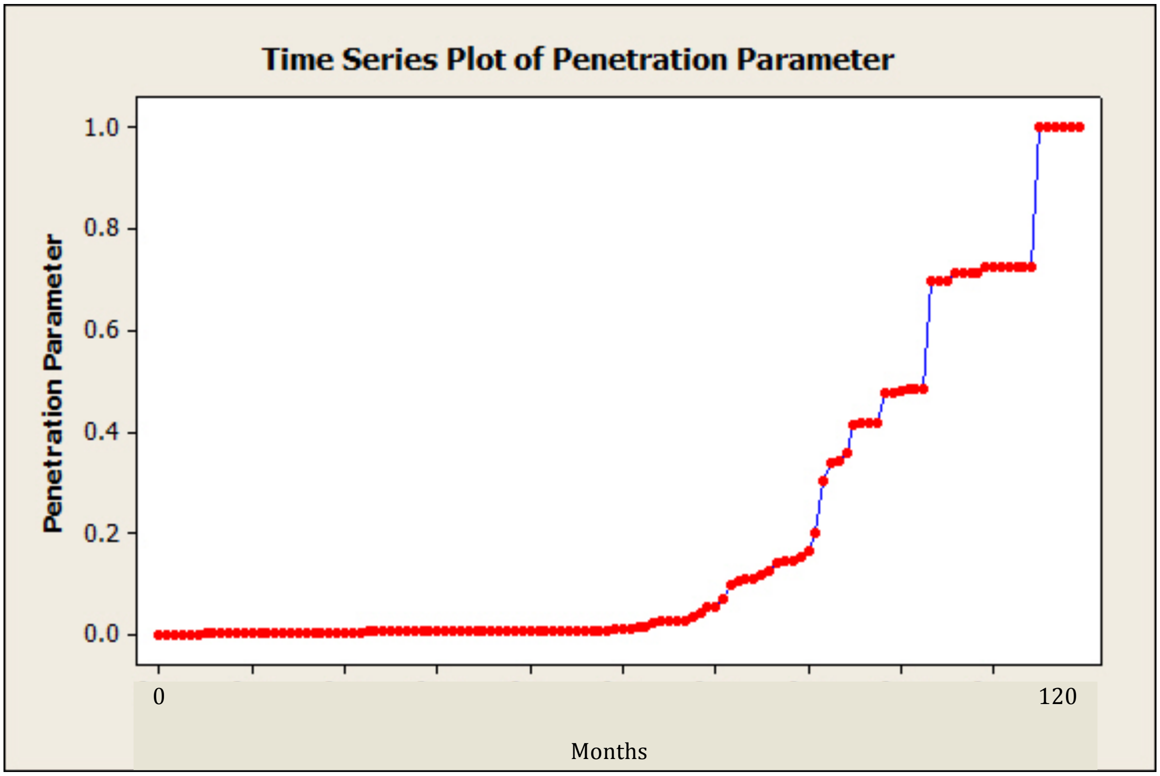

Figure 3: Degree of Intervention Penetration as a Function of Month Index.

During the first half of the 120 months of the study (labeled “Index” on the horizontal axis), the intervention did not penetrate the sampling unit. The penetration function is essentially flat and near zero until approximately month 72. Then, the penetration increased gradually until about month 87 when the slope of increase became much steeper. Two months were present during which massive increases in the extent to which the intervention penetrated the sampling unit. The whole treated unit (362 sites) was penetrated for the last six months of the study. The information contained in this penetration model was used to construct the intervention penetration variable in a time-series regression model. If the intervention is effective, the outcome measure (i.e., number of crashes) should reflect the extent to which the intervention penetrates (i.e., is applied to) the sites in the sampling unit (i.e., the 362 treated sites).

Results

Figure 4 shows the actual number of crashes (denoted as TCMNI) in the treatment sampling unit for each month of the study. As shown in Figure 4, noise is present from month to month, but a clear downward trend exists after the first half of the study.

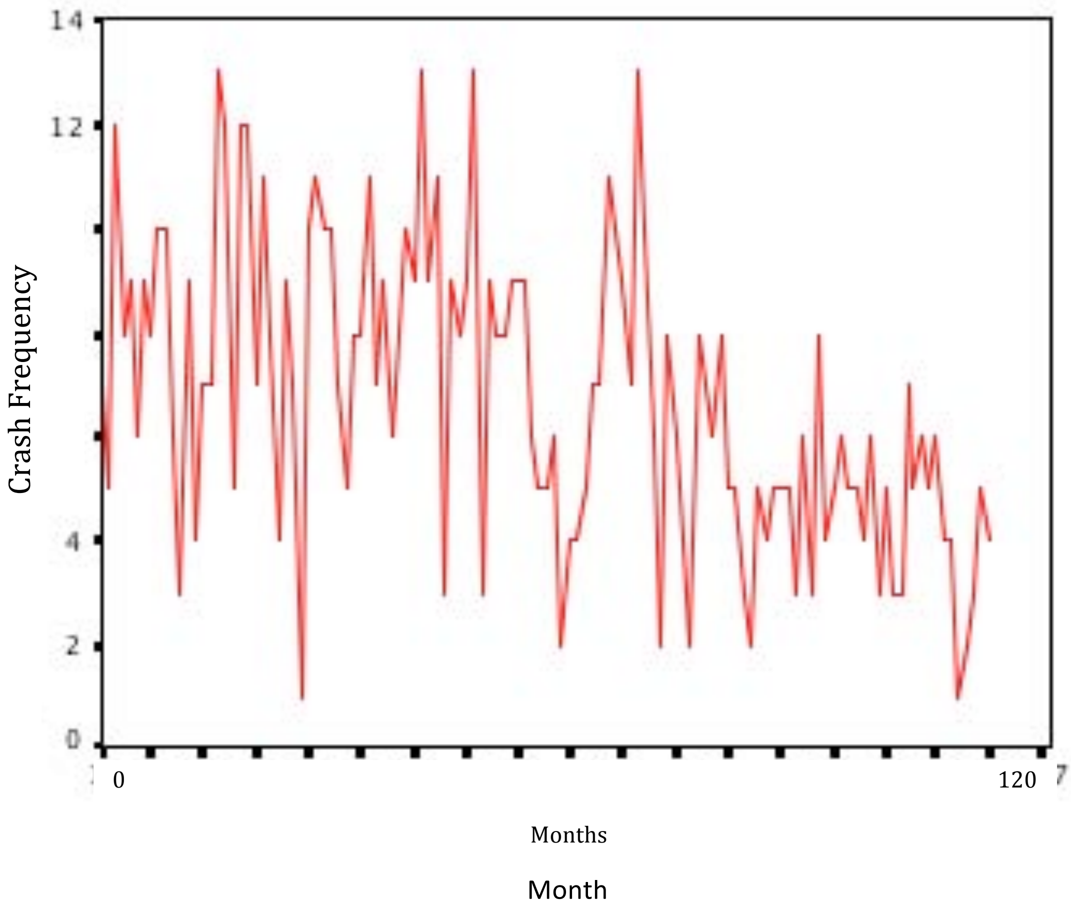

Figure 4: Detroit Crash Frequency (Intervention Sites) as a Function of the Month of the Study.

Figure 5 illustrates the fitted outcome of the autoregressive time-series intervention model that includes the penetration variable as a predictor. This figure does not show the actual data points; rather, it shows the values that the model predicts for each month, independent of the noise. During the first half of the study (i.e., when the intervention penetration was either zero or close to zero), the average number of monthly crashes for the whole sampling unit was approximately seven; by the end of the study when the penetration index was one (i.e., all treatment sites exposed to the PCT), the average crash level was less than three.

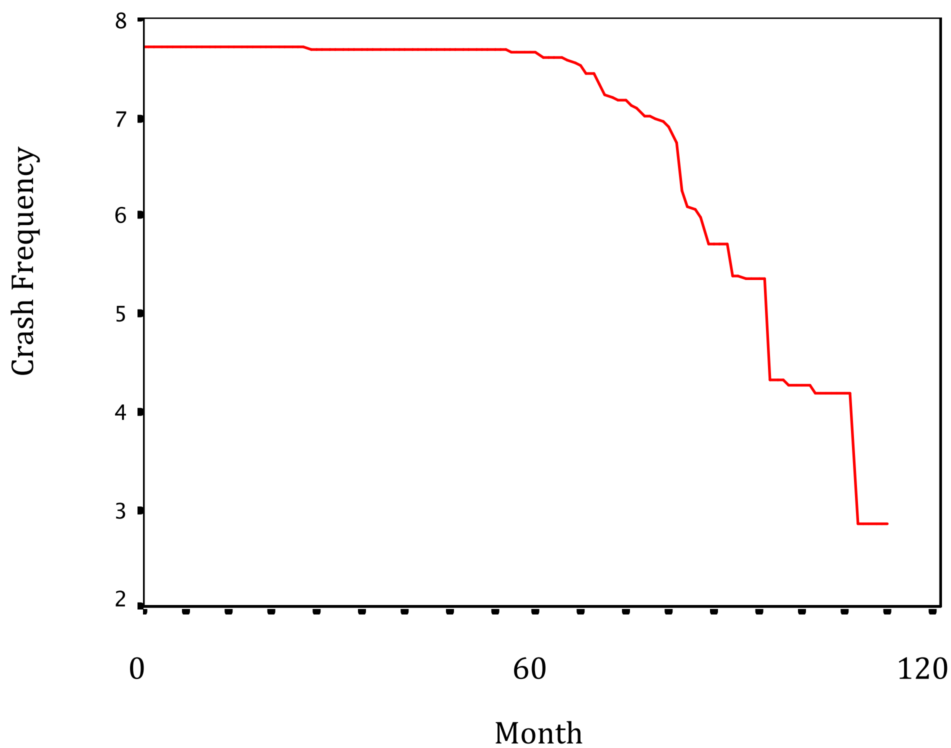

Figure 5: Average Crash Frequency Predicted from the Penetration Model for Detroit.

A somewhat more complex version of the model described above was used in the formal inferential analysis of the intervention effect. This model is a generalization of the intervention time-series regression model with autoregressive errors that is described in McKnight, McKean, and Huitema (2000); it includes both the intervention penetration variable and a set of indicator variables (to control for seasonal effects) as predictors. The residuals of this analysis were well modeled using a first-order autoregressive structure. The remaining disturbances (illustrated in Figure) are essentially white noise. The results of this intervention analysis are presented in Table 1.

Table 1*: Full Intervention Effect Estimates for PCT in Detroit.*

Descriptive Outcome Measure	**Estimate**
Initial Level of Crashes	6.96
Change in Level Associated with Intervention (i.e., the level change coef.)	-4.88 (*t* = -5.45; *p* = .0000003)
Level at End of Study	2.08
Percentage reduction in crashes	70%
Standardized Within Site Effect Size	2.03 (Large)
Amount of Total Variation Explained by the Intervention (that is not explained by seasonality and autocorrelation)	17% (Large)

The initial (baseline) level is estimated to be about seven crashes per month. The intervention effect coefficient associated with the ultimate level change is -4.88. This means that the crash level declined by almost five points by the end of the study. The product of this level change coefficient multiplied by a penetration variable score can be used to provide an estimate of the number of crashes associated with any specified degree of intervention penetration. When the intervention penetrates all sites in the sampling unit of 362 sites, the value of the penetration index is 1.0 and the regression coefficient (i.e., -4.88) is equal to the ultimate level-change statistic; it is interpreted as the average change in crashes that has occurred between the baseline level and the ultimate level at the end of the study. Correspondingly, the decrease predicted when only half of the sites have a PCT is 0.50 x -4.88 = -2.44.

Three alternative outcome metrics (in addition to the level-change statistic) also are shown in Table 1. All three yield values (i.e., percentage reduction from baseline, standardized within sites effect size, and variation explained by treatment) that are considered “large” using conventional statistical rules of thumb (e.g., Cohen, 1988).

An empirical display of the relationship between the actual crash frequency and the penetration variable is presented in Figure 6. As shown in the figure, intermediate degrees of the intervention (e.g., penetration scores of, say, 0.5 or 0.7) are associated with smaller reductions in crashes than when the full intervention (i.e., a penetration score of 1.0) is applied. This valuable information is not provided in a conventional before-after design and analysis.

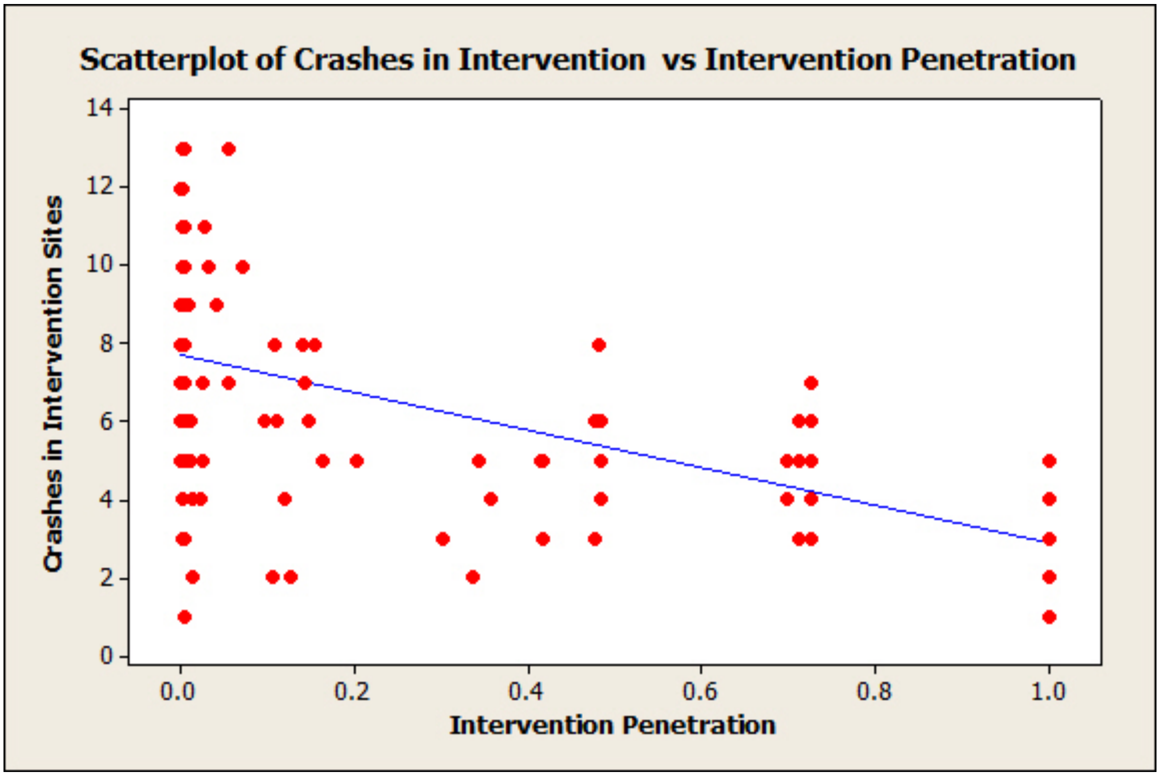

Figure 6: Scatterplot of Crashes on Penetration of the PCT Intervention.

Between Group Comparisons

Although the major interest in the study is on the data from the PCT treated sites, another valuable interest was to evaluate change in similar sites that have not been exposed to the PCT intervention. Because time-series designs are susceptible to the effects of confounding events that occur concommitantly with the intervention, the availability of such controls can provide a basis of comparison that helps rule out alternative explanations for the apparent effect. This section provides results based on comparisons between the PCT treated sites and non-treated (control) sites.

Potential Confounding

The -4.88 point change from the baseline level of crashes is a meaningful intervention effect estimate as long as no confounding events are likely to have occurred during the same time interval that the PCT were introduced. Many events other than the intervention occurred during the study, but the concern is whether these events are correlated with the frequency of crashes. If they are correlated, they may be confounders. One potential confounding variable is traffic volume. This is a concern because if there is a decrease in volume that parallels the observed decrease in crashes in both the treated and control sites, the relationship may be causal. Hence an attempt was made to obtain local and general traffic volume measures.

Local data were quite sparse, but the research team was supplied with estimated Detroit traffic volume data for a 10-year period. Although complete annual data were not available for all years of the study, the team developed a model of likely annual volume from the incomplete traffic data provided by the Southeast Michigan Council of Governments (SEMCOG) (2012).

Estimated traffic volume in Detroit decreased over the ten-year period. The decrease from year to year was not linear. Rather, it was modeled as a quadratic function of time with a small curvature component. This function then was used to estimate total Detroit traffic volume for each year and month of the study. The annual estimates are shown in Table 2. Once these estimates were calculated, the team was able to correlate estimated traffic volume with observed crash frequency.

Table 2: Estimated Detroit Traffic Volume for Years 2001-2010.

Year	Estimated Traffic Volume (Millions)
2001	18.00

2002	17.89
2003	17.77
2004	17.69
2005	17.59
2006	17.50
2007	17.41
2008	17.32
2009	17.24
2010	17.16

The correlation of crash frequency with general Detroit traffic volume is essentially zero (-0.03, $p = 0.72$) for the control sites and 0.50 ($p < 0.001$) for the PCT intervention sites. A test on the difference between these two correlations yields a p-value < 0.001. An inspection of the two scatterplots associated with these correlations (see Figures 7 and 8) reveals an obvious difference that confirms the test results.

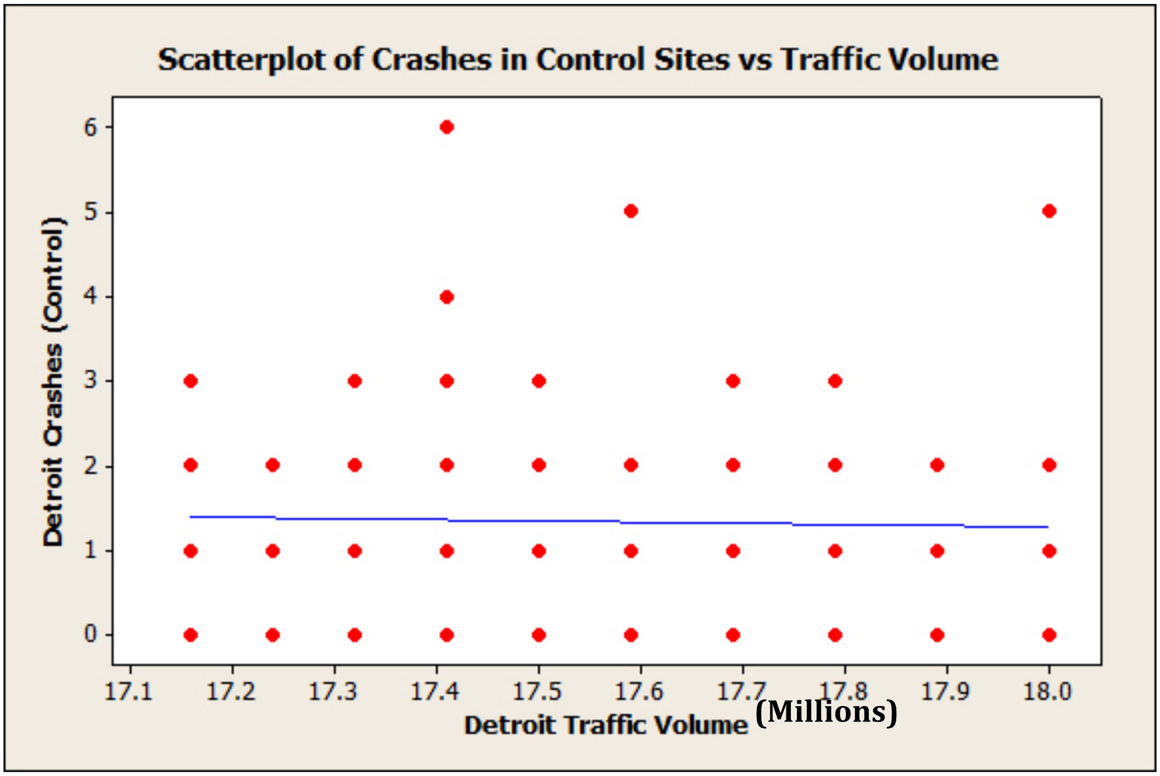

Figure 7: Scatterplot of the Relationship Between Traffic Volume and Crash Frequency for Control sites. (Sampling unit = month)

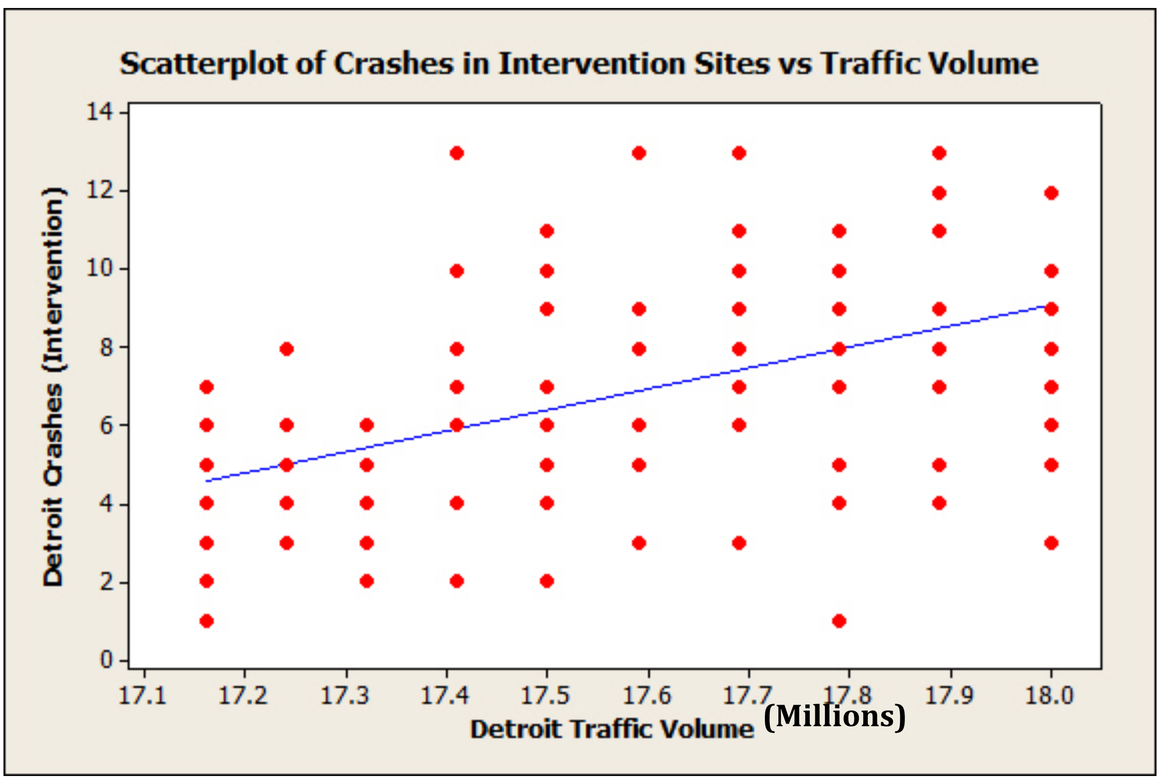

Figure 8: Scatterplot of the Relationship Between Traffic Volume and Crash Frequency for Treatment (PCT) Sites. (Sampling unit = month)

The lack of correlation for the untreated (i.e., control) sites is of great interest because the data suggests that the drop in total Detroit traffic does not cause a decrease in the crash measure used in this study. In contrast, the substantial correlation between traffic volume and crashes in the PCT treated sites does not mean that traffic volume causes crashes. Rather, the traffic volume measure used here is simply a marker for the introduction of the PCT.

Since the penetration of the PCT is an increasing function of time, and time is correlated with traffic volume, traffic volume is an indirect way to determine the extent to which the intervention has penetrated the sampling unit. This claim that the volume measure is a surrogate for the penetration measure is based on the almost perfect monotonic decreasing relationship ($r_{Spearman\ =}$ -0.98) between these two measures. This relationship is illustrated in Figure 9.

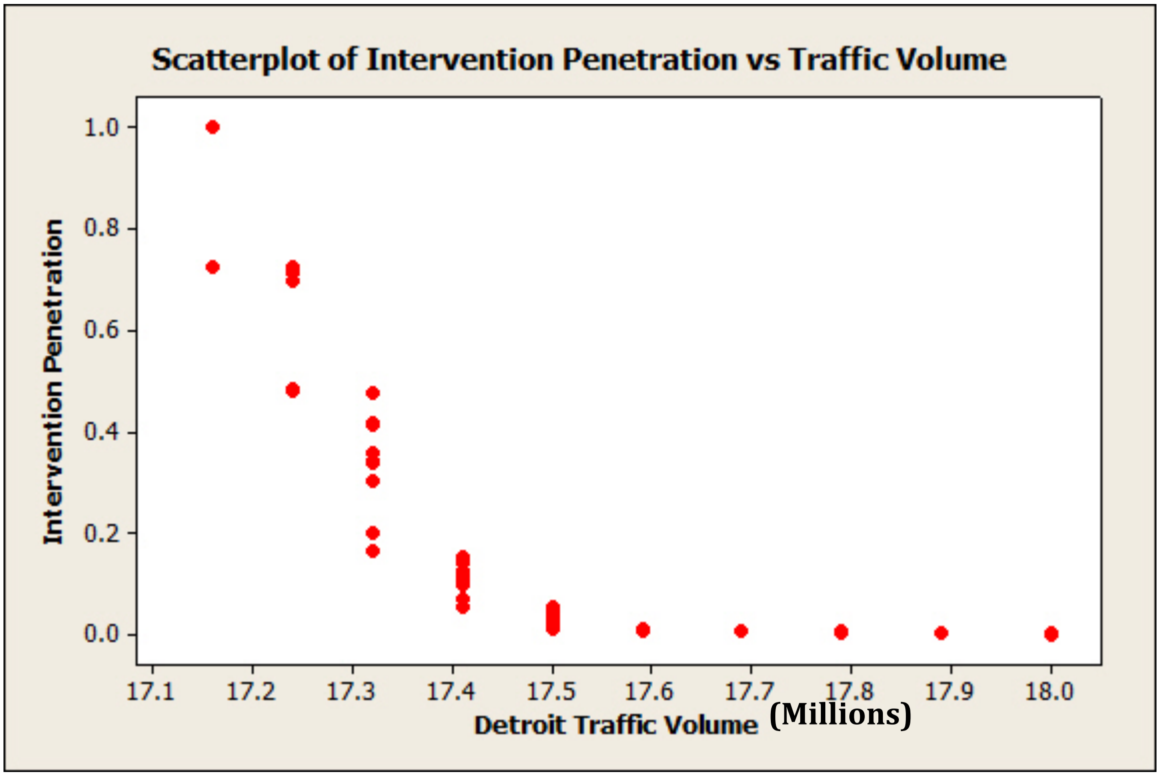

Figure 9: Illustration of Monotonic Decreasing Relationship between PCT Intervention Penetration and Detroit Traffic Volume.

Because the volume measure is an almost perfect proxy for intervention penetration in the treated sites, one interpretation is that the difference between the volume-crash correlations in the treated and untreated sites can be interpreted as evidence of an intervention effect. Consequently, the test result on this difference (reported above) is strong evidence of an intervention effect. An approach that requires less circuitious reasoning, however, is more desirable. A more direct approach is described herein.

Between Unit Comparison: Intervention Unit vs. Control Unit Outcomes

Although the outcome information already presented is strong evidence of an intervention effect, a more straightforward approach is to compare the intervention outcome results from the PCT Treated sites with the outcome results on the control sites that were measured over the same time interval and in the same general locations as were the treated sites.

The Control (i.e., no PCT) sampling unit consisted of 87 sites. The analysis of this unit (using the same intervention model as was applied to the intervention data) yields a nonsignificant ($p > 0.10$) level change estimate. However, a comparison of test conclusions from the intervention and control units (i.e., statistically significant vs. not significant) is not a valid approach for testing the difference in results found for these two units. What is called for is a method of testing the significance of the difference between the *p*-value from the intervention unit and the *p*-value from the control unit. Hence, a test developed for this purpose (Huitema, 2011) was applied. This test results in an obtained *z*-statistic that is associated with $p < 0.01$. The research team concluded that the evidence supporting a change in the intervention unit is significantly stronger than the evidence for change in the control unit. This test renders implausible the potential argument that the reduction in crashes in the PCT sites is explainable by the reduction in traffic volume that also affects the control sites.

Discussion and Conclusions

The intervention penetration model developed for this study indicates that a strong effect of the PCT intervention is present and that the size of this effect is a decreasing function of the extent to which the intervention has penetrated the sampling unit of 362 sites. As the number of PCT sites in this unit increases, the overall trend for crashes tends to decrease. When the intervention is fully introduced (i.e., the PCT penetration is 100 percent), the effect is a reduction in the average number of crashes from about seven per month to a little over two per month, resulting in a 70% reduction in all crashes. This change cannot plausibly be attributed to change in some unknown nonintervention variable that affects both PCT sites and control sites.

In addition, the common problem of regression effects that plague many versions of Before-After studies are not an issue in this long term time-series design because such effects are a decreasing function of time. That is, regression effects typically last for only a few time periods; they disappear in long time-series designs such as this one in which outcome measurements are obtained from the total number of sites at each of many time points. No general drop-off in crash frequency was found throughout a baseline interval of over five years; only when the PCT were introduced in large numbers was a consistent crash reduction observed. Because the magnitude of the crash reduction was shown to be a function of the extent to which the timers were introduced, the evidence for an intervention effect is strong. The results of the PCT statistical analysis provided unequivocal evidence that the pedestrian countdown timers reduced pedestrian crashes. The size of the effect in the Detroit sample was quite large (70% crash reduction).

References

Cohen, J. (1988). *Statistical Power Analysis for the Behavioral Sciences (2nd edition.).* Hillsdale, NJ: Lawrence Erlbaum.

Eccles, K.A., Tao, R., & Mangum, B.C. (2007). Evaluation of Pedestrian Signals in Montgomery County, Maryland. *Transportation Research Record*, *1878*, 36-41.

FHWA (2007). *Desktop Reference for Crash Reduction Factors*. Washington, DC.

Huang, H. and Zegeer, C. (2000). The Effects of Pedestrian Countdown Signals in Lake Buena Vista. Florida Department of Transportation. 2000.

Huitema, B. E. (2011). *The Analysis of Covariance and Alternatives: Statistical Methods for Experiments, Quasi-Experiments, and Single-Case Studies (2nd ed.)* Hoboken, NJ: Wiley.

Institute of Transportation Engineers (ITE). (2007). *Pedestrian Signal Safety for Older Persons.* Washington, DC.

Markowitz, F., Sciortino, S., Fleck, J.L. & Yee, B.M. (2006) Pedestrian countdown timers: experience with an extensive pilot installation. *ITE Journal*, *76*, 43-48.

McKnight, S., McKean, J. W., & Huitema, B. E. (2000). A double bootstrap method to analyze linear models with autoregressive error terms. *Psychological Methods*, *5*, 87-101.

Manual on Uniform Traffic Control Devices (2009). Federal Highway Administration, ATSSA/ITE/AASHTO. Publication Number MUTCD-10, 40000/Curtis1000/0310.