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The Economic Effects of Road Safety Improvements: An Insurance Claims Analysis

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Abstract

This article demonstrates the feasibility of exploiting insurance claims data to estimate the marginal benefits to society of highway infrastructure improvements. We construct a unique database linking claims expenditures for a major auto insurer in Michigan to infrastructure investments at 62 intersections in the City of Detroit, and conclude that the addition of a left-turn lane, or left-turn phase in the signal, decreases the insurer's average monthly claims costs at a representative intersection by $944 or $1,062, respectively. The evidence also indicates that these cost savings are a result of reductions in accident severity, rather than being a consequence of fewer accidents.

Introduction

It is well known in traffic engineering circles that intersection improvements may increase driver safety by reducing the incidence of automobile accidents.[1] What is less well understood is the extent to which improvements in intersection infrastructure impact on the economic costs of such accidents to drivers.[2] This omission is particularly striking since the application of cost-benefit analysis to the disposition of scarce highway improvement funds requires a monetary estimate of the likely benefits that can be compared to the attendant costs. The contribution of this article is to provide such estimates.

The traditional emphasis on accident rates rather than economic costs has been a consequence of the historical collaboration between traffic engineers and governmental

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[1] See, for example, Wattleworth, Atherley, and Hsu (1988); Ermer, Fricker, and Sinha (1992); and South Dakota Department of Transportation (1998).

[2] The only exception of which we are aware is our recent article reporting the results of the AAA Michigan Road Improvement Project (Feber, Feldmeier, and Crocker, 2000).

entities in the construction, maintenance, and improvement of highway infrastructure. Information on automobile accidents has generally been collected as a natural by-product of the government's policing activities, and these data are easily accessible to engineers and utilized for highway planning purposes. The economic costs associated with accidents, however, have fallen on the drivers and their insurance companies who have used the information gathered on accident claims for proprietary underwriting purposes. As a result, the data necessary to evaluate the economic benefits of highway infrastructure improvements have been heretofore inaccessible.

This article uses a unique data set that combines Michigan State Police accident records with intersection improvement data from the City of Detroit and the claims experienced by a major automobile insurer in the State of Michigan. By utilizing information from these three disparate sources, we obtain monthly data from 1994 to 1997 on 62 intersections in Detroit that we use to estimate the effect of specific intersection improvements, such as the addition of left-turn lanes or upgraded signals, on the actual insurance costs arising from automobile accidents.

THE DATA

The Detroit Police Department and the insurance companies that write policies in the city are the two primary sources for automobile crash information in Detroit. Since each maintains a unique and independent database, the first step in this study is to develop an integrated intersection database containing detailed collision and cost information on an intersection-by-intersection basis for the City of Detroit. The outline of this process is depicted in Figure 1.

Creating the Intersection Database

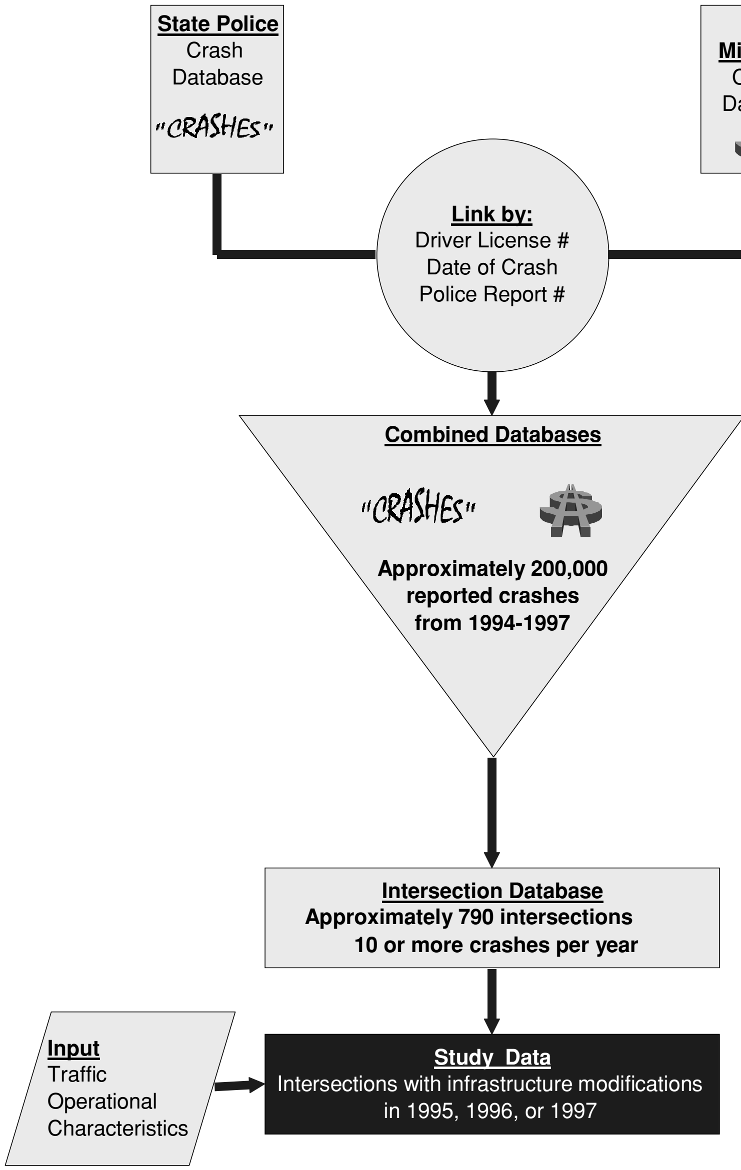
There are approximately 50,000 reported vehicle collisions in Detroit each year. When a crash occurs, a police crash report is completed by a local police officer detailing the specifics of the collision, including the location, the motorists involved, the type of crash, as well as dozens of other particulars. The local police forms are transferred to the Michigan State Police where the information is scanned into a computer database. The Michigan State Police then "sanitizes" this information by removing all personal information such as names, addresses, and drivers' license numbers, and redistributes this information to state, regional, and local road agencies for planning purposes.

The State Police crash database has very detailed information describing the event, including location and type, yet completely omits any information regarding the economic cost of accident damages. Insurer databases, on the other hand, have very specific information regarding claims costs, but do not contain very precise information on either location or type of crash. By linking the two databases, it is possible to determine the economic cost of accidents at a specific location for a given period of time.

The cost data utilized in this study is from AAA Michigan,[3] one of the major auto insurers in the State of Michigan. AAA Michigan collects information when one of

[3] AAA Michigan refers to a group of affiliated organizations that includes an automobile club with 1.7 million members in Michigan, "Automobile Club of Michigan" and a reciprocal insurance exchange that insures approximately 22 percent (as of 1997) of Michigan's drivers, "Auto Club Insurance Association Family of Insurance Companies."

FIGURE 1
Data Structure



its insured is involved in a crash and files a claim for payment, and so has detailed cost information for each collision reported. The cost data that we use in this study include both economic and noneconomic damages, although the magnitude of the latter is quite small since Michigan is an unlimited no-fault state. Fortunately, both the Michigan State Police and the AAA Michigan databases have the date and time of the crash, as well as the drivers' license numbers of the motorists involved. These three fields serve as the basis for linking the two databases.

Since the driver's license number field is traditionally removed from the sanitized version of the database released to the public, specific permission was granted from the Michigan State Police to obtain an unsanitized version of the Detroit crash database for 1994, 1995, 1996, and 1997. The database obtained identified approximately 400,000 motorists involved in roughly 200,000 crashes in Detroit from January 1, 1994 through December 31, 1997.

Once the database was obtained, the next task was to link all the collisions and motorists to intersections. The location information in the Detroit crash database is based on a system developed by the Michigan Department of Transportation called the Michigan Accident Location Index (MALI), which assigns every roadway a unique identification number. This primary road number (PR#) has a mile point indicator assigned to it which is broken down to the 100th of a mile, and which begins where a road originates and continues to increase for the length of the roadway. We assign a crash to an intersection if it occurred within a 150-foot radius of the center of the intersection, excluding corner parking lots. An intersection therefore consists of a pair of PR#s framed by a corresponding pair of mile point numbers representing approximately 150 feet on each approach entering the intersection.

Approximately 95 percent of the 200,000 reported crashes were assigned to an intersection using this method. In order to make the analysis tractable,[4] we include only those intersections that experienced at least 10 reported crashes per year. The result is a database of 790 intersections with crashes ranging from 10 to 97 per year.

Location Selection

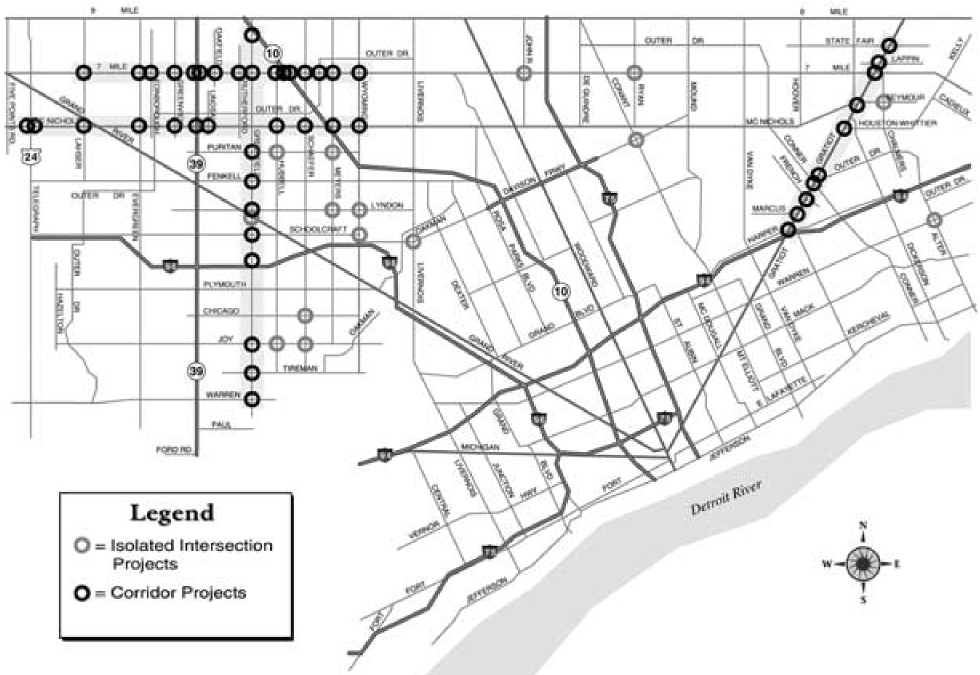
To assess the effects of infrastructure modifications on accident costs, we selected those that had experienced some form of infrastructure improvement in 1995, 1996, or 1997, and for which AAA Michigan had experienced at least one claim from this list of intersections. Sixty-two intersections satisfied both criteria.

The intersections fell into two groups, as depicted in Figure 2. One group (47 projects) consisted of intersection improvements along predetermined corridors, whereas the others (15 projects) were isolated intersection improvements. Interviews were conducted with the Michigan Department of Transportation, Wayne County Road Commission, and City of Detroit Department of Public Works to obtain the rationale for the project selection. The results of the interviews suggested that the projects at isolated locations tended to be safety projects at preidentified high-crash locations,[5] whereas the corridor projects tended to be maintenance projects that were selected because the

[4] Note that our analysis requires the labor-intensive identification of physical intersection characteristics and modifications (see below) gleaned from City, County, and State files, as well as in-person observation.

[5] This description, however, is not entirely supported by the data. For the corridor projects prior to improvement, the average monthly loss was $3,133 and the accident rate (using the variable *Accident* described in Table 1) was 0.38. The AAA road improvement projects demonstrated a slightly higher monthly loss ($3,320) and a substantially higher accident rate (0.46) prior to improvement. The projects initiated by the City of Detroit, however, had prior to improvement monthly losses ($2,960) and accident rates (0.36) that were *lower* than those experienced by the corridor improvement projects.

FIGURE 2
1995-1997 Detroit Projects: Corridor/Intersection



traffic signal equipment along the corridors was nearing the end of its useful service, and because government funding was available for upgrades. Of the 62 intersections in our data, 47 were corridor projects initiated by the State of Michigan or Wayne County, 12 were selected for improvement by the City of Detroit, and 3 were selected by AAA as part of the AAA Michigan demonstration road-improvement project.[6]

Location Description

All of the locations in the study are at-grade urban arterial roads controlled by a traffic signal.[7] The average daily traffic entering these intersections ranged from 22,000 to 91,000 vehicles per day. Extensive data collection efforts were employed to gather data on the physical characteristics and infrastructure improvements for each of the 62 locations contained within these projects. First, the actual as-built construction diagrams were obtained from the appropriate government agency to identify the specific

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[6] For a description of this program, see Feber, Feldmeier, and Crocker (2000).

[7] Note that "at-grade" is a purely functional road classification used by highway departments to describe the type of road. At-grade urban arterial roads are typically three- to five-lane roads that control intersecting points with traffic signals rather than bridges. All roads that fit this description would be classified as at-grade urban arterial roads regardless of their crash or injury rates.

changes that were implemented at each location. Field inspections confirmed that the blueprints reflected the current conditions in the field for all locations. Interviews were also conducted with the appropriate governmental agencies to insure that no other changes had been implemented at the intersections between 1994 and 1997. The construction diagrams indicated any traffic signal changes, additions of left-turn lanes (either through restriping or widening), and additions of left-turn phases to the traffic signals.

Next, county and state files were reviewed to obtain the actual completion date of each intersection project in order to separate the data between the "before" and "after" periods. A 3-month "during construction" period was removed from the cost data at each intersection to accurately define the before and after periods. Construction at each location took approximately 1 to 2 months to complete. Therefore, the month in which a project was completed, plus the three prior months, were eliminated from the data for each location.[8] As a result, the data consist of 44 monthly observations for each of the 62 intersections, for a total of 2,728 observations.

Project Description

The baseline treatment to all 62 locations was traffic signal modernization. A traffic signal modernization project is essentially removing the old outdated traffic signal and replacing it with a new updated one. This typically includes the traffic signal displays, the poles and other mounting equipment (mast arms or span wires), the traffic signal controller, and any required electrical wiring. Traffic signal modernization projects tend to result in collision reductions at intersections due to better signal head placement and enhanced visibility. Many of the projects involved restriping the pavement markings to add a left-turn lane within the existing road width, and left-turn phases (left-turn green arrows) were also added in some cases. The locations of the intersections contained in the data, and whether they were corridor or isolated intersection improvements, are depicted in Figure 2.

THE EMPIRICAL MODEL

The empirical relationship between monthly insurance losses and intersection improvements may be written as

$$L_{i,t} = \alpha_i + \beta X_{i,t} + \sum_{j=1}^{K} \gamma_j D_{i,j,t} + \varepsilon_{i,t},$$

where $L_{i,t}$ denotes the accident costs incurred at intersection i in month t, α_i is an intersection-specific effect, the $X_{i,t}$ represent any time-varying intersection characteristics, and $D_{i,j,t}$ is a dummy variable indicating whether improvement j was in place at intersection i during month t. The $\varepsilon_{i,t}$ are assumed to be independent and identically distributed error terms with zero mean and constant variance σ^2, and we require that $\text{cov}(\varepsilon_{jt}, \varepsilon_{kt'}) = 0$ for every $t \neq t'$, $j \neq k$.

[8] We have estimated the model for various cut-offs (including eliminating only the month of construction), and the results are not sensitive to the alternative chosen.

TABLE 1
Variables and Summary Statistics

Variables	Definition	Mean	Minimum	Maximum
$Loss_{i,t}$	loss (in dollars) incurred by AAA Michigan at intersection i during date t	2884.0510 (10025.6600)	0	184,123
$NewSignal_{i,t}$	1 if a new signal is present at intersection i during time t; 0 otherwise	0.5308 (0.4991)	0	1
$LeftLane_{i,t}$	1 if a new left turn lane was present at intersection i during date t; 0 otherwise	0.0216 (0.1455)	0	1
$LeftPhase_{i,t}$	1 if a new left turn phase was present at intersection i during date t; 0 otherwise	0.0139 (0.1172)	0	1
$AAA_{i,t}$	1 if intersection i was improved as part of the AAA demonstration project; 0 otherwise	0.0484 (0.2146)	0	1
$City_i$	1 if intersection i was selected for upgrade by the City of Detroit	0.1935 (0.3952)	0	1
$Corridor_i$	identifies traffic corridor in which intersection i is located	5.5806 (3.5181)	1	14
$Intersection_i$	identifies intersection i	31.5000 (17.8981)	1	62
$Accident_{i,t}$	1 if at least one accident occurred at intersection i during date t; 0 otherwise	0.3515 (0.4775)	0	1

Note: Number of observations: 2,728 (standard deviations in parentheses).

Data for intersections without any improvements (so that $D_{i,j,t} = 0$ for every j and t) would only be useful for estimating the parameter β, since the summation term would equal zero for all such observations. Moreover, intersections that have experienced some improvement j during the 1994–1997 time period may be used to estimate the parameter γ_j even in the absence of data on any other time-varying intersection characteristics $X_{i,t}$.[9] Since the goal of this article is to identify the effect on accident costs of intersection improvements, or concern is with the estimation of the γ_j coefficients.

The measure of monthly intersection accident costs used in this study, $Loss_{i,t}$, is the dollar amount of insurance claims paid by AAA Michigan during month t at intersection i. As indicated in Table 1, these losses average about $2,884 per

[9] Put differently, since our goal is to estimate the effect, γ, of intersection improvements on insured losses, we do not require a control group of unimproved intersections for the analysis, nor do we require data on other time-varying intersection characteristics $X_{i,t}$. In essence, the panel nature of our data permits each of the intersections to provide its own control group, namely, the observations from the time period prior to the implementation of the intersection improvements.

TABLE 2
Summary of Test Statistics

Dependent Variable	Specification	Variables	Statistic	χ^2 *df*
		Cook–Weisberg Heteroskedasticity Test		
Loss	OLS	Unspecified	12.21*	1
		Corridor	0.01	1
		Intersection	3.71**	1
		Breusch–Pagan Lagrangean Multiplier Test		
Loss	OLS	Corridor	0.03	1
		Intersection	6.04*	1
		Hausman Specification Test		
Loss	OLS	*Intersection*	6.53**	3

*Significance beyond the 5% level; **significance beyond the 10% level.

intersection in the data, and vary from zero (no claims paid at an intersection during a given month) to $184,123. All of the intersections in the data received the baseline improvement of a signal upgrade at some time during the 1994–1997 time period. This is reflected by the explanatory variable $NewSignal_{i,t}$, which is a dummy variable with the value of one in the postimprovement period, and zero otherwise. The other explanatory variables noted in Table 1 are the dummy variables $LeftLane_{i,t}$ and $LeftPhase_{i,t}$, which indicate whether the intersection improvement also involved the addition of a left-turn lane or a left-turn phase, respectively. Each of the intersection improvements are hypothesized to result in reduced accident costs, so the hypothesis is that each of the estimated coefficients γ_j should be negative.

As controls, we include the dummy variables $City_i$ and AAA_i, which indicate intersection improvements initiated by either the City of Detroit or as part of the AAA Michigan road improvement demonstration project, and the variables $corridor_i$ and $intersection_i$, which identify the traffic corridor and the specific intersection, respectively. Finally, $Accident_{i,t}$ is a dummy that takes on the value of one if intersection i experienced at least one accident in month t. This variable will be used in "Is It Frequency or Severity?" to investigate the extent to which intersection improvements affect the frequency of automobile accidents.

RESULTS

The estimation results are presented in Table 3 for several different specifications of the empirical model. We begin with simple OLS, and include as controls the indicator variables *AAA* and *City* to account for the possibility that the criteria used to select intersections for improvement may differ across these entities.[10] These coefficient estimates are presented in the first row of the Table. Results from the test suggested by Cook and Weisberg (1983), which are given in the first row of Table 2, indicate

[10] The omitted category is the set of intersections selected by the State or the County for improvements which, as noted above, are primarily corridor-improvement projects.

TABLE 3
Parameter Estimates. Dependent Variable: Loss

Specification	Constant	*AAA*	*City*	*NewSignal*	*LeftLane*	*LeftPhase*
(1) OLS	3277.33	96.30	−616.66	−458.53	−943.71	−1061.95
White	(10.52)*	(0.15)	(−1.49)	(−1.12)	(−2.00)*	(−1.93)**
White (corridor)	(24.21)*	(0.11)	(−1.67)**	(−1.51)	(−1.81)**	(−2.48)*
White (intersection)	(9.80)*	(0.12)	(−1.38)	(−1.09)	(−2.05)*	(−2.86)*
(2) Intersection Fixed-Effects	1115.37	–	–	−247.14	−3099.44	−1111.828
	(0.72)			(−0.54)	(−1.93)**	(−0.055)
White	(1.84)**			(−0.64)	(−1.98)*	(−1.10)
White (corridor)	(5.12)*			(−0.90)	(−2.62)*	(−2.03)*
White (intersection)	(3.72)**			(−0.66)	(−2.42)*	(−1.34)
(3) Intersection Fixed-Effects:	3077.88	–	–	−221.41	−3212.40	−1073.97
AR1 ($\hat{\rho} = 0.0013$)	(9.92)*			(−0.47)	(−1.99)*	(−0.53)
(4) Intersection and Month	1108.94	–	–	332.78	−2693.14	−749.31
Fixed-Effects	(0.56)			(0.41)	(−1.65)**	(−0.37)
White	(0.79)			(0.47)	(−1.71)**	(−0.72)
White (corridor)	(1.04)			(0.72)	(−1.98)*	(−1.06)
White (intersection)	(0.85)			(0.54)	(−1.95)**	(−0.81)
(5) Intersection Fixed-Effects	1259.04	–	–	499.94	−3099.44	−1111.83
and time trend	(0.81)			(0.66)	(−1.93)**	(−0.55)
White	(0.04)			(0.70)	(−1.98)*	(−1.10)
White (corridor)	(4.94)*			(1.26)	(−2.62)*	(−2.03)*
White (intersection)	(3.86)*			(0.79)	(−2.42)*	(−1.34)

*Significance beyond the 5% level; **significance beyond the 10% level (*t*-ratios in parentheses); and number of observations is 2,728.

that heteroskedasicity is present in the model, both generally and with respect to the individual intersections.[11] Accordingly, the first set of *t*-ratios presented for the coefficients estimated by OLS in Table 3 are calculated using the robust standard errors that have been constructed according to the method suggested by White (1980).

The usual White-corrected standard errors, however, require independence of the observations, a property that may not hold in the current setting. It is unlikely that that the losses from accidents are truly independent within a given traffic corridor, or in a particular intersection over time. Without a doubt, an intersection or corridor that experiences above-average losses in one month is likely to experience higher losses in other months as well due to the persistence of traffic patterns and the like. We therefore present the *t*-ratios calculated from the robust standard errors in which the observations are only assumed to be independent within traffic corridors (*White*

[11] The test developed by Cook and Weisberg assumes $\mathrm{var}(\varepsilon_i) = \sigma^2 \exp(z\alpha)$, where z is a list of specified varibles (such as *corridor* or *intersection*, as noted in Table 2), or the predicted values of the dependent variable from the regression (the "unspecified" category of variables noted in Table 2). The null hypothesis of no-heteroskedasticity is $\alpha = 0$. The details of the test statistic are contained in *Stata Statistical Software: Release 7.0* (2001), volume 3, p. 114.

(corridor)) or within intersections (*White (intersection)*). These results indicate that the baseline intersection improvement of adding a new signal does not significantly reduce accident costs, but that the addition of a left-turn lane or a left-turn phase is predicted to reduce the monthly accident costs incurred by AAA Michigan at a representative intersection by \$944 or \$1,062, respectively. Given that the monthly accident costs for AAA Michigan average \$2,884, the implementation of both of these improvements would result in a 69 percent reduction in losses.[12]

Since our data consist of 48 monthly observations for each of 62 individual intersections, omitted-variables bias is likely to be an issue in the choice of estimation procedure. Specifically, if the loss experience at an intersection or in a particular traffic corridor is driven in part by unobserved attributes particular to each intersection or traffic corridor, and if those unobserved attributes were correlated with observable variables included in the regression, the error term would no longer be uncorrelated with the included explanatory variables. The result would be biased and inconsistent estimates of the true parameter values. Fortunately, we can test for this possibility, and correct for it if necessary by including intersection- or corridor-specific fixed effects in our estimation procedure.[13]

To ascertain the extent to which corridor- or intersection-specific effects are present, we use the Lagrangean multiplier test first suggested by Breusch and Pagan (1980), the results for which are presented in Table 2. While we cannot reject the null hypothesis that there are no corridor-specific effects, the null of no intersection-specific effects is decisively rejected with a statistic (6.04) that exhibits a degree of significance beyond the 5 percent level.[14] Consequently, rather than having a single intercept as in the classical regression model, it appears that a more appropriate specification would be to permit intersection-specific effects.

One possibility would be to use a random effects model, but such an approach requires that the intersection-specific effect be uncorrelated with the other regressors. In order

[12] Given that the 1997 market share of AAA Michigan was 22 percent statewide, and assuming that their loss experience in Detroit is representative of that experienced by other insurance carriers, one could extrapolate from the AAA Michigan loss experience to obtain an estimate of the cost reductions experienced by all insurers in Detroit. Under these assumptions, our results predict that the installation of a left turn phase would reduce *monthly* accident costs of the insurance industry by $(1062)/(0.22) = \$4,827$, and the effect of adding a left turn lane would be monthly reductions of $(944)/(0.22) = \$4,291$. The accuracy of this forecast depends, of course, on the extent to which the AAA Michigan loss experience is representative. In this regard, one referee inquired about the relationship between the proportion of AAA Michigan's book of nonstandard business as compared to the State as a whole. As it turns out, Michigan is a "take all comers" state for all drivers meeting the eligibility standards of the Michigan Essential Insurance Law. There is a joint underwriting association as the insurer of last resort for those outside of the eligibility requirements. As a consequence, Michigan does not have an active nonstandard market.

[13] Note that this problem, which is often referred to as that of "unobserved heterogeneity," cannot be solved by the use of random effects estimation techniques. For an informative discussion on this issue, see Judge et al. (1985), p. 527.

[14] More precisely, the null hypothesis for the test of intersection-specific effects is that the variance of α is zero.

to determine whether this is the case in our data, we may apply the specification test developed by Hausman (1978), the results of which are presented in Table 2. Since the statistic (6.53) demonstrates significance at conventional levels, we must reject the null hypothesis, which is that the intersection effects are uncorrelated with the other explanatory variables. Given the results of the Breusch–Pagan and Hausman tests, the fixed effects model appears to be the best choice.

The coefficient estimates for the model with intersection-specific fixed effects are presented in the second row of Table 3.[15] Four sets of *t*-ratios are presented, the first of which is constructed in the usual fashion, and the other three use robust standard errors that are analogous to those derived in the OLS model.[16] From these coefficient estimates, we may conclude that the addition of a new signal does not reduce monthly accident costs significantly, and there is weak evidence that the addition of a left-turn phase may reduce accident costs. But, the most striking result is that the construction of a left-turn lane significantly reduces accident costs by approximately $3,099 for each intersection that is so improved.[17]

A final concern with these data is the potential for autocorrelation, given the time series nature of the data observed for each intersection.[18] The most common test for the presence of autocorrelation is the test developed by Durbin and Watson in a series of articles beginning in 1950. In the case of the OLS model, we calculate the Durbin–Watson statistic to be 2.036. Given that we have 44 time series observations[19] and five regressors (without the constant term), the critical values for the Durbin–Watson statistic are $d_{*l} = 1.29$ and $d_{*u} = 1.78$.[20] From these values, we may conclude that we cannot reject the null hypothesis of *no autocorrelation*[21] if the Durbin–Watson statistic falls between 1.78 and 2.22, as it does in this particular case.

Although we cannot reject the null hypothesis, we may nonetheless wish to account for any autocorrelation in our models. We accomplish this in three ways. First, we return to our model with intersection-specific fixed effects, but this time assuming that the error term is first-order autoregressive (AR1).[22] These results are presented in the third

[15] The fixed effects regression includes a dummy variable for 61 of the 62 intersections represented in the data. The estimated coefficients for these dummies are not included in the results presented in Table 3.

[16] Note that the *AAA* and *City* dummies have been omitted from the fixed effects model because of collinearity with the intersection-specific fixed effects.

[17] To put this number in context, the average monthly losses prior to improvement for the subset of intersections that eventually received left turn lanes is $4,774, resulting in a postimprovement reduction in monthly accident costs of 64.9 percent.

[18] For example, there could be city-wide factors that generate trends in losses during the sample-time period, such as mandatory seat belt laws, increased enforcement of drunk-driving laws, increased traffic enforcement, or the like. The time trend, time dummies, and first-order autoregressive models should correct for such eventualities.

[19] Although we have 48 monthly observations for each intersection, recall that the month in which the intersection improvement was completed, as well as the three previous (construction) months, were dropped from the data.

[20] These are the 5 percent critical values; see Savin and White (1977).

[21] More precisely, the null hypothesis is that $\rho = 0$.

[22] We use the "xtregar" procedure from the STATA statistical package to estimate these coefficients. See StataCorp (2001), volume 4, p. 456.

TABLE 4
Summary of Test Statistics

Dependent Variable	Specification	Variables	Statistic	χ^2 *df*
	Cook–Weisberg Heteroskedasticity Test			
Accident	OLS	Unspecified	2.84**	1
		Corridor	0.48	1
		Intersection	0.16	1
	Breusch–Pagan Lagrangean Multiplier Test			
Accident	OLS	*Corridor*	5.65**	1
		Intersection	135.53*	1
	Hausman Specification Test			
Accident	OLS	*Corridor*	8.06	5
		Intersection	3.34	3

*Significance beyond the 5% level; **significance beyond the 10% level.

row of Table 3. The second approach is to augment the model with intersection-specific fixed effects to include month-specific fixed effects as well, the results of which are reported in the fourth row. The third is to include a time trend as a regressor, and those results are contained in row 5 of the Table.[23] All of these results are consistent with those of the baseline intersection-specific fixed effects model reported in the second row of Table 3.

IS IT FREQUENCY OR SEVERITY?

While the results of the previous section clearly demonstrate that intersection improvements may result in substantial reductions in monthly accident costs, the source of these reductions has yet to be identified. In particular, a reduction in the accident costs could be a consequence of a reduction in the frequency of accidents (holding severity constant) or, alternatively, the lower cost could be a result of less severe accidents (holding frequency constant). Since we claim that there is value added to examining accident costs as opposed to simple frequencies, it would be useful to disentangle these two effects.

To that end, we create the variable $Accident_{i,t}$ which has a value of 1 if at least one accident occurred at intersection i during month t that resulted in a claim paid by AAA Michigan, and is 0 otherwise. The issue addressed in this section is the extent to which intersection improvements affect the occurrence of accidents. We begin by presenting in the first row of Table 5 the coefficient estimates obtained from a simple OLS model. Since an application of the Cook–Weisberg test (see in Table 4) indicates the presence of heteroskedasticity, the t-ratios reported are calculated using the robust standard errors in the fashion previously described. Given the dichotomous nature of the dependent variable, a probit model is appropriate, and those results are reported

[23] The time trend variable is significant only in the White (corridor) results, where it is negative (−28.73) with a t-ratio of −1.87.

TABLE 5
Parameter Estimates. Dependent Variable: Accident

Specification	Constant	*AAA*	*City*	*NewSignal*	*LeftLane*	*LeftPhase*
(1) OLS	0.3896	0.0613	−0.0381	−0.0640	−0.0047	0.0345
White	(23.58)*	(1.30)	(−1.56)	(−3.19)*	(−0.07)	(0.41)
White (corridor)	(21.64)*	(0.86)	(−1.12)	(−4.94)*	(−0.08)	(0.31)
White (intersection)	(15.92)*	(0.94)	(−1.18)	(−2.88)*	(−0.09)	(0.32)
(2) Probit	−0.2794	0.1562	−0.1040	−0.1735	−0.0121	0.0922
White	(−6.43)*	(1.30)	(−1.55)	(−3.20)*	(−0.07)	(0.44)
White (corridor)	(−5.90)*	(0.86)	(−1.12)	(−4.76)*	(−0.08)	(0.33)
White (intersection)	(−4.35)*	(0.95)	(−1.19)	(−2.88)*	(−0.09)	(0.35)
(3) Random Effects	−0.2794	0.1563	−0.1040	−0.1735	−0.0121	0.0972
Probit (corridor)	(−6.40)*	(1.31)	(−1.54)	(−3.18)*	(−0.07)	(0.45)
(4) Random Effects	−0.3029	0.1945	−0.0900	−0.1626	0.0027	−0.1481
Probit (intersection)	(−4.81)*	(0.90)	(−0.77)	(−2.78)*	(0.01)	(−0.59)

*Significance beyond the 5% level; **significance beyond the 10% level.
t-ratios in parentheses; number of observations is 2,728.

in the second row. In each case, we find that the baseline improvement of a new traffic signal significantly reduces the probability of having at least one accident at an intersection in a given month, and that probability does not appear to be effected by the other intersection improvements.

We report in Table 4 the Breusch–Pagan statistics, which indicate the presence of both corridor- and intersection-specific effects in the data. Moreover, the Hausman specification test statistics, which are also reported in Table 4, indicate that a random effects model may be appropriate. Accordingly, we report in Table 5 the results from random effects probit models that account for corridor and intersection effects, respectively.

The conclusion is that alternative intersection improvements have differential effects on accident frequency and severity. The baseline improvement of adding a new signal appears to reduce the *number* of accidents, but the payoff in terms of reduced accident *costs* seems to be in the reduced severity of accidents that result from the addition of left-turn lanes and phases. Put differently, an examination of the frequency relationship (Table 5) would lead traffic engineers to install new signals, whereas our results on monthly accident costs (Table 3) indicate that the real payoff results from the installation of left turn lanes or, perhaps, phases.

CONCLUSIONS

The traditional approach to highway infrastructure projects has focused on engineering studies of traffic patterns, and predictions of likely crash-rate reductions. Data on the economic benefits associated with such improvements have had surprisingly little role in these deliberations, even though the economically efficient allocation of limited highway improvement funds necessarily requires a comparison of the expected project benefits with known project costs.

This article demonstrates the feasibility of exploiting insurance claims data to estimate the marginal benefits to society of highway infrastructure improvements. Using the claims experiences of insurers in conjunction with accident and infrastructure data from governmental agencies, we estimate the monetary benefits, in terms of reduced insurance payments to injured drivers, from specific highway improvements. Our results indicate that the data collected by insurers in the course of settling claims can be an important input into the highway planning process.

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