Life and Death in the Fast Lane: Police Enforcement and Roadway Safety

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Abstract

This paper considers the effect of police enforcement on roadway safety. Because of simultaneity, estimating the causal effect of police on crime is often difficult. We overcome this obstacle by focusing on a mass layoff of the Oregon State Police in February of 2003, stemming from *Measure 28*. Due solely to budget cuts, 35 percent of the roadway troopers were laid off. The decrease in enforcement, defined by either troopers employed or citations given, is strongly correlated with a substantial increase in injuries and fatalities on highways. Our estimates link the mass layoff of police to a 10–20 percent increase in injuries and fatalities, with the strongest effects under fair weather conditions outside of city-limits where state police employment levels are most relevant. To further corroborate our findings, we also estimate the relationship between trooper employment levels and fatalities over the period 1979-2005 for Oregon, Idaho, and Washington, finding a 10 percent increase in trooper employment per vehicle mile traveled reduces statwide fatalities per vehicle mile traveled by 1.8 percent and on highways outside of city of limits under fair weather conditions by 4.9 percent.

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1 Introduction

Automobile accidents are the leading cause of death for Americans between the ages of 4 and 34, accounting for some 19,036 fatalities in 2003. Translating the costs of accidents into dollars, some estimates have put the damages of accidents in updwards $230 billion per year in the United States alone (Blincoe et al., 2000).[1] One of the most common (but less studied) policies intended to increase roadway safety is police enforcement. Police officers frequently issue tickets for speeding as speeding is one of the most common violations of the law[2] and also is one of the most frequent causes of roadway fatalities.[3]

In this paper, we estimate the causal effect of highway patrol officers on roadway safety. Because of long-standing simultaneity problems in estimating the abiity of police to deter criminal behavior, we identify the effect of a change in enforcement by studying a mass (35 %) layoff of state police in Oregon due solely to budget cuts. We find that the reduction in police employment is associated with significant increases in injuries and fatalities on highways and freeways, respectively measuring 11 and 17 percent. Additional analysis of variation in state police employment in Oregon, Idaho, and Washington from 1979 to 2005 yield similar results, implying a 10 percent increase in state police per vehicle miles traveled (VMT) leads to a 2 percent reduction in all fatalities and 4.9 percent reduction in fatalities on highways outside of city-limits under dry weather conditions. These findings suggest that enforcement can play a substantial role in driver behavior on freeways and highways, consistent with a Becker (1968) model of crime where speeders respond to the probability of

[1] Although drivers may internalize some of these costs, many externalities remain. These include – but are not limited to – other vehicles not at fault in the accident, passengers, traffic delays (see Dickerson et al., 2000), and higher insurance premiums even for those not in the accident (see Edlin and Mandic, 2006).

[2] "Effectiveness of Double Fines as a Speed Control Measure In Safety Corridors." SPR 304-191, Oregon Department of Transportation Research Group.

[3] See http://www-nrd.nhtsa.dot.gov/Pubs/809915.PDF

apprehension and fines.

Fines and apprehension probabilities have long been considered as options to reduce criminal activities – in theory. For instance, Becker (1968), Polinsky and Shavell (1979), and Imrohoroglu et al. (2004) examine theoretical models of deterrence and crime. Some empirical evidence on the impact of deterrence on crime has been provided by Levitt (1997) and McCormick and Tollison (1984).[4] As noted in these studies, estimating the degree to which fines and apprehension probabilities deter crime has posed a difficult problem due to simultaneity. Regions with higher crime rates tend to have more enforcement, presumably in an effort to reduce crime, and hence much work has been done to overcome this type of reverse causality (Levitt and Miles, 2006). Although both papers establish some evidence of a negative relationship between enforcement and crime, for the most part their final estimates are imprecise.[5] We focus on the Oregon State Police (OSP), and a layoff of state troopers which resulted from a large and immediate budget cut.[6] This offers a unique quasi-experiment for studying the effects of policing on roadway safety, as the large decrease in state troopers occurred due to exogenous factors.[7]

For our estimation, we link records of traffic accidents on highways provided by the

[4]See also Ehrlich (1973).

[5]The original papers of McCormick and Tollison (1984) and Levitt (1997) found significant elasticities. Recent revisits to their analyses by Hutchinson and Yates (2007) and McCrary (2002) uncovered some minor coding mistakes and unintentional misclassifications, which both decreased the point estimates and increased the standard errors. Several of the pooled estimated elasticities between police and violent crime in Levitt (2002) were smaller and less precise after the corrections. The estimates of McCormick and Tollinson (1984) remained significant at the 10 percent level after the necessary adjustments.

[6]This approach is similar to Levitt (2002) where the budget of firefighters was utilized as a potential instrument to uncover the causal relationship between police and crime. In our analysis we study a specific budget cut that led to a mass-layoff of police, for which the historical events can also be analyzed to confirm the exogeneity of the layoffs.

[7]Note that our results also complement recent research by Makowsky and Stratmann (2009), which have found that poor local economic conditions can lead to increases in enforcement for local police jurisdictions (which are able to keep a large share of the revenue from their citations), while state police ticketing behavior is unresponsive to *local* budget shocks. Building off of their first paper, Makowsky and Stratmann (forthcoming) have a follow up study which takes advantage of the endogenous reponse of local police to offset the exogenous decrease in local resources. In large part, they find significant results for property damage accidents with elasticities that are very similar to our estimates for minor injuries.

Oregon Department of Transportation (ODOT) with detailed records of trooper employment and all issued citations – as maintained by OSP. We also utilize annual police employment records from Oregon, Washington, and Idaho in predicting changes in fatalities, which are collected from the Fatality Analysis and Reporting System (FARS) from 1979-2005. Section 2 provides a background of the political climate and discussion of the exogeneity of a massive legislatively mandated budget cut in Oregon – due to *House Bill 5100* and the failure to pass *Measure 28* – that decreased the number of OSP by approximately 35 percent in 2003. Section 3 reviews the data sources while Section 4 provides an empirical examination of the effects of enforcement levels on several measures of roadway safety. Section 5 discusses some policy implications of our findings while Section 6 concludes.

2 Background of the Budget Cut and Police Layoff

Oregon's state budget has been in turmoil since the onset of the "tax revolt", which began in 1997 with the passage of *Measure 50.* The public-sponsored initiative limited property tax rates and their growth in a manner similar to *Proposition 13* of California. In consequence, funds for state agencies tightened during the 1997-2002 period. In early 2002, it became clear to the Oregon State Government that unless taxes were raised, budget cuts would become necessary. *Measure 28*, which allowed for an increase in the state income tax to cover budget deficits, was put to a vote of the people on January 28, 2003.

In the weeks prior to the vote, media attention brought the impending budget crisis to the public spotlight. Coverage from *The Seattle Times* specifically highlighted that the budget cuts for the OSP would "put staffing levels back to roughly the levels of the 1960s".[8] Knowing

[8] "A cutting edge Oregon wishes it wasn't on". Hal Benton, *The Seattle Times,* December 29, 2002. There was also publicity put out by the Oregon State Police. "State police already preparing for big cuts."

that the public was weary of tax increases, *House Bill 5100* was approved on January 18, 2003 by Governor Kulongoski. *House Bill 5100* contained provisions that specified budget cuts that would be enforced on February 1, 2003 if *Measure 28* was not approved, making the threat of the budget cuts all the more credible. After the votes were counted in a record turnout[9], *Measure 28* failed with 575,846 votes in favor and 676,312 voting against.

Time-Line of Events

May 20, 1997	*Measure 50*, Passed
January 28, 2003	*Measure 28* Fails
February 1, 2003	*House Bill 5100*, Implemented. Layoff of 117/354 Troopers
September 1, 2003	*House Bill 2759C* Fines Increase (15 %)
February 4, 2004	*Measure 30* Fails
January 1, 2006	Increase of Fine>100 MPH
January 20, 2006	Hiring of 18 FTE Troopers
June 18, 2007	*Senate Bill 5533*, 100 Troopers Hired

On February 1, 2003 the budget cuts laid out in *House Bill 5100* went into effect and the OSP complied by laying off 117 out of 354 full-time roadway troopers.[10] Layoffs were decided solely by seniority, with trooper specific performance playing no role. Several months after the reduction in trooper employment, a 15 percent increase in the maximum allowable fine was enacted in September 2003. Because the police do not maintain the fine amounts in their ticket database, it is difficult to ascertain to what level *actual* fines increased. This other policy change – which we will set aside in our analysis purely because of data limitations and collinearity – suggests our estimates could actually be lower bounds of the effect of

Rebecca Nolan, *The Register-Guard*, Dec 29, 2002. "Troopers look for jobs elsewhere." Diane Dietz, *The Register-Guard,* Jan 17, 2003.

[9] "Oregonians make a painful choice." Larry Leonard, *Oregon Magazine,* Jan. 31, 2003.

[10] Some other personnel who worked in the state crime lab were also laid off. In our analysis, troopers are state police whose position is defined as a "roadway officer". Sergeants and lieutenants also are state police, however their role is largely managerial. Over 70 percent of the layoffs were state police whose position was designated as a "roadway trooper".

enforcement on roadway safety.[11] *Measure 30*, which was essentially a carbon copy of *Measure 28*, was introduced in 2004 and faced the same fate as its predecessor. The timeline of events leads to a unique national experiment in which the probability of apprehension of speeders fell substantially and remained lower for several years while other major policies affecting highway safety were unchanged. Also, the substanital publicity regarding the budget crisis increases the likelihood that the average driver might be immediately aware of the decrease in police.

Figure 1 contains trends for both the number of state police employed and the number of incapacitating injuries or deaths (on highways outside of city limits and under fair weather conditions, regions and driving conditions likely to be most influenced by changes in state police enforcement) for 2000-2005. The three years before and three years after the layoff are a period comprise a time period when other policies, such as graduated teenage licensing and drunk driving laws are constant and troopers were largely not yet rehired (which began in 2006 and 2007), isolating more clearly the potential impact of the police layoff on injury rates.[12] Morever, during the 2000-2005 time window the fatality rate per VMT traveled fell by 3.7 percent for the rest of the United States.[13]

In the months after the layoff, the number of severe injuries and deaths is higher, most notably in the summer months. This is not too surprising, as traffic in the summer months on highways and freeways is nearly double that of the rest of the year. Moreover, traffic

[11] It may also take much longer for drivers to learn about when fines increase relative to enforcement changes. Drivers learn about fine increases when they or someone they know receives a ticket. They can learn about enforcement changes by noticing the lack or presence of police on the road or via the news media.

[12] In 2003, Senate Bill 504 would have increased the Oregon speed limit on freeways from 65 to 70 MPH, but it was vetoed by the governor. Measures to increase the fine structure further in 2005 never were passed by the legislature.

[13] Author's calculations.

flows increase by a few miles per hour during the summer months. The impact of increased VMTs and driving speed during the summer months is displayed in Figure 2, which plots the actual number of injuries against the number of injuries predicted using weather and seasonality from the pre-layoff period.[14] In the summer months following the layoff, there was an additional 15-30 incapacitating injuries or fatalities per month, which is shown by the distance between the solid and dashed lines.

3 Data Sources

Data for accidents and injuries are obtained from the State-Wide Crash Analysis and Reporting System collected and published by ODOT. For the first part of our analysis, we restrict ourselves to the 2000-2005 time period, providing three years before and after the layoff.[15] For an initial analysis we aggregate the data into a monthly time series of accidents for the entire state on highways or freeways. The dependent variables analyzed are deaths (within 30 days of the accident), incapacitating injuries (those where a victim required immediate transportation to a hospital), and visible injuries (requiring treatment at the crash scene). Although property accident counts are available, we omit them from the analysis because in 2004 the minimum property damage necessary for a property-damage-only accident to be recorded in the database increased by 33 percent.[16] OSP provided information on trooper

[14] To predict the number of injuries/fatalities, a linear regression model was estimated using injuries as the dependent variable with precipitation, snow, and a vector of indicator variables for each month as regressors. Even using this somewhat limited range of controls yielded an R^2 of 0.88. Results from the regression are available upon request.

[15] Although we have data on accidents going back to 1987, Oregon implemented a graduated driver license program in 2000. Examining 2000-2005 yields a period where the only major policy change was the loss of state troopers.

[16] Estimated property damages are not recorded in the database, else we would have constructed a consistent series for property damage accidents.

employment and a complete record of all citations issued since January 1, 2000. Weather data were collected from the National Climatic Data Center Daily Cooperative files, while monthly employment data are from the US Census Bureau. Summary statistics for the aggregated monthly time series are provided below.[17]

Even in the simple summary statistics (see Table 1), an increase in deaths, incapacitating injuries, and visible injuries is evident and statistically significant[18] when adjusting for seasonality.[19] In addition, changes in VMT and driver characteristics are minimal, and the proportion of young drivers trend in a direction that would decrease injuries. Similarly the increase in precipitation would have lead to decreases in fatalities and injuries under dry weather conditions. We provide the summary statistics for fatalities and injuries across other conditions in Appendix Table 3.

Figure 3 shows the percentage increase in the number of injuries separately by each season, as well as the confidence intervals. The percentage increase is estimated using linear regression models (scaled by the mean in pre-layoff period to yield a percentage effect), also controlling for precipitation.[20] For the most part, the increase in the number of injuries is both the largest and most precisely estimated for injuries or fatalities in the summer months. This is further evidence consistent with increased speeding being a channel for the increase in injuries, because summer months are a time when there is more speeding on the freeways and thus enforcement can play a larger role in determining roadway safety.[21]

[17] These are the summary statistics for the time series of injuries from accidents with dry surface conditions.

[18] Although these simple t-tests do not adjust for serial correlation, adjusting for auto-correlation had almost no effect on the significance, actually reducing the p-value.

[19] Seasonal adjustment are made using a within mean transformation for each month.

[20] The regression results which produced Figure 3 are in Appendix Table 3.

[21] We also analyzed traffic stations collecting speed data, finding speeds increase by 0.4 miles per hour following the layoff. In addition, we analyzed traffic data for the limited traffic stations recording speeds,

4 Results

Deaths and injuries follow an implicit count process, as they are bounded below by zero and occur only in integer values. However, fatalities and injuries could increase due to fluctuations in the amount individuals choose to drive. Scaling injuries by VMT results in non-integer valued coefficients.[22] Thus we implement two types of models in our analysis: OLS regression where both the enforcement and the injury measure are scaled by VMT and Poisson regressions, a natural econometric model for count data. Although Negative-Binomial models are often used because they relax the assumption of equality between the conditional mean and variance, the Poisson maximum likelihood estimator has been shown to have consistency properties when the true data generating process is mis-specified – a feature not generally true of negative binomial models (Wooldridge, 1997). In order to correct for likely over-dispersion in the Poisson models, we use sandwich standard errors, which relax the assumption of equality between the conditional mean and variance. One important identifying assumption for the Poisson model is

$$E(Y|X) = \exp(X'\beta).$$

Because of this assumption about the nature of the conditional mean of Y, the estimated

finding speeds in the summer increase on average by over 0.7 miles per hour versus other times of the year. Previous research, such as Ashenfelter and Greenstone (2004), link a 1 mile per hour increase in speeds to a 20 percent increase in fatalities. Given the stations with speed recorders are on a select sample of high volume roads relatively close to urban regions, the 0.7 mile per hour increase represents the change in average speed for selected segments of roadway and excludes rural regions and two-lane highways.

[22] Scaling variables so they are non-integer valued does not affect the estimates of the Poisson regressions, however it can have implications for inference. The level of precision can depend on the units of the normalization. For instance, although the coefficients will not change, scaling injuries by billions of VMT will result in more precise standard errors relative to scaling by millions of VMT. Inference with OLS is invariant to such normalizations.

coefficients can be interpreted as semi-elasticities. This type of count model produces similar conditional means to estimating a linear regression model in which $E(\ln y|x) = X'B$,[23] but they allow for cases where the dependent variable takes on values of zero, which occurs in our sample when we disaggregate to the county level. Thus the coefficients should be interpreted as the percentage change in the dependent variable given a unit change in the regressor. If the regressor is the log of a variable, the coefficients can be viewed as elasticities.[24] In order to make the comparison of the two models easier, we scale the estimated coefficients from the linear regression models to represent elasticities or semi-elasticities.[25]

We consider injuries and fatalities that result under four different scenarios on freeways and highways: (a) outside of city-limits under dry weather conditions, (b) outside of city-limits for all weather conditions, (c) inside or outside of city-limits under dry weather conditions, and (d) inside or outside of city-limits for all weather conditions. Dry weather conditions are defined by weather conditions reported as clear and surface conditions reported as dry at the time of the accident.[26] If the Oregon State Police layoff is indeed responsible for the increase in injuries and fatalities, one would expect the increase in fatalities to be largest where the decrease in enforcement was the largest. It was infeasible to obtain the universe of

[23] We have also estimated OLS regressions with $\ln(injury_t) = \ln(enforcement_t) + X_t'\beta + u_t$ as the specification, obtaining nearly identical estimates. We also do not account for serial correlation in the presented results as adjusting for autocorrelation in linear regression models reduces the standard errors slightly.

[24] For the Poisson regressions, the injury measures are not normalized by VMT. While this normalization has been used elsewhere in the literature (e.g. Ashenfelter and Greenstone (2004)), if injuries were normalized by VMT in a given month or county, it would also be natural to normalize the level of enforcement by VMT. As noted above in a Poisson or negative-binomial regression $E(Y|X) = \exp(X'\beta)$. Hence $\frac{injury_t}{vmt_t} = \exp(\alpha \ln(\frac{enforcement_t}{VMT_t}) + X_t'\beta)$, therefore $\ln \frac{injury_t}{VMT_t} = a \ln(\frac{enforcement_t}{VMT_t}) + X_t'\beta$. Rewriting that expression, $\ln injury_t - \ln VMT_t = a \ln enforcement_t - a \ln VMT_t + X_t'\beta$, which can be represented by a model where $\ln VMT_t$ is included as a regressor. This is done in the county regression specifications, but not in the state level models because VMT is only reported at the annual level.

[25] This is accomplished by scaling the regression coefficients by a ratio of the mean of regressor and the mean of dependent variable.

[26] We have experimented with other classifications of dry weather conditions and find similar results using the climatic data from the National Climatic Data Center.

citations from all local municipalities in Oregon. However, the type of police officer (state, county or local) is recorded when a police officer responds to an accident. Figure 4 illustrates that OSP Troopers attend to the majority of accidents outside of city-limits (77 percent) and a minority of accidents (14 percent) inside of city-limits. This is suggestive of the patterns that likely exist for enforcement, with areas outside of city-limits likely being affected the most by the layoffs.[27] Moreover, injuries tend to be more severe outside of city-limits as the odds of a visible injury nearly double, the odds of an incapacitating injury triple, and the odds of a fatality increase eight-fold, all conditional on being in an accident. However, we note that when performing the analysis on dry highways outside of city-limits, we are considering locations and conditions that normally account for 1/3 of fatalities and account for less than 1/10 of the total number of visible injuries in an average year for Oregon. As such, estimates will be most representative of driver behavior on highways and freeways and not necessarily to all drivers or driving conditions.

4.1 Oregon Estimates for 2000-2005

Initially we estimate the relationship between enforcement levels for Oregon at the state level, under the various city-limit and weather combinations mentioned in the previous section. For each of the state level equations we include controls for seasonality (month of year), precipitation, snow, and the unemployment rate to account for economic conditions that could affect the decision to drive or choice of new vs. used vehicle.[28] The equation

[27] In addition, Oregon passed other laws in 2003 that confound examining injury-rates inside of city-limits including the usage of automated red-lights and the distribution of automated speed ticketing sites.

[28] We attempted to acquire a measure of income per capita or median household income, however for the state level results would have only been available at the annual level and the county level results are only available for counties with a population greater than 60,000. In any case, these measures are intended as proxies to adjust for local economic conditions.

representing the initial OLS regressions can be seen in equation 1, while the Poisson regression is represented in equation 2.[29] As indicated before, the nature of the Poisson regression allows the variables to be interpreted as elasticities and for ease of comparison between the models the OLS regression coefficients are scaled by the ratio of the mean of the dependent variable to the mean of the regressor in order to represent elasticities. Equations 1 and 2 specify the regression models utilized for estimates in Tables 2 and 3, respectively.

$$\frac{f_{my}}{VMT_y} = \beta * \frac{enforcememt_{my}}{VMT_y} + m_m + \alpha_1 * prcp_{my} + \alpha_2 * snow_{my} + \alpha_3 * unemp_{my} + u_{my} \quad (1)$$

$$E(f_{my}|X_{my}) = \exp(\beta * enforce_{my} + m_m + \alpha_1 * prcp_{my} + \alpha_2 * snow_{my} + \alpha_3 * unemp_{my}) \quad (2)$$

The state level OLS results are presented in Table 2 while the state level Poisson regression results are presented in Table 3. Within each table, the results of each cell represent separate estimations of the above equations. The first three rows are the injury rates for the roadways outside of city limits while the results in rows 4-6 include highways both inside and outside of city limits. The first three columns examine roads under dry weather condition (at the time of accident, both the weather is clear and the road is dry as reported in the crash

[29] The subscipt m refers to month, subscript y refers to year, f_{my} is the injury measure of interest, m_m is the month fixed effect, $enforcement_{my}$ is one of the three measures of enforcement, $prcp_{my}$ is precipitation, $snow_{my}$ is snow, $unemp_{my}$ is the unemployment rate, and u_{my} are the unobservables.

report) while columns 4-6 contain all weather conditions.

As shown in Tables 2 and 3, the layoff in police is associated with increases in fatalities and injuries. In addition, the elasticities for fatalities are largest for the roads under dry weather conditions outside of city limits, in which the OLS model estimates the elasticity between troopers employed and fatalities to be -0.38 while the Poisson model estimates the elasticity to be -0.43. Under dry driving conditions, the elasticities for fatalities are notably larger than the elasticities estimated for injuries, similar to other research that has found increases in speed limits outside of city limits increase fatalities by a greater percentage than injuries (Rock, 1995). When the results are expanded to include roadways that are, geographically, typically outside the domain of the state police enforcement, the elasticities fall, in particular for fatalities. Other more minor injuries continue to have a significant elasticity with police enforcement when considering more general weather conditions or jurisdictions.

We note that citations could be considered endogenous. In addition to responding to overall staffing levels, police could give out more citations in response to or in anticipation of increased accident rates. If this is the case, then one can view the estimated elasticity between citations and injury rates as lower bounds for the true effect of additional citations on injuries. Regardless of this potential bias for citations, for each of the injury types we find a negative elasticity between citations and injuries.

Tables 4 and 5 present the OLS and Poisson regression results at the county level, respectively. Estimating equations 1 and 2 at the county level allows us to include additional controls that vary by county and year, such as VMT in the Poisson regression and the number of drivers younger than 25 or older than 65 for both models, in addition to making the weather controls more precise (varying by the county, month and year, rather than the av-

erage weather in a month and year for the entire state). It should be noted that state police are not deployed at the county level, hence we focus on citations as a measure of enforcement which varies at the month and county level. The OLS and Poisson equations for the county level regression models are represented in equations 3 and 4.

$$\frac{f_{cmy}}{VMTc_y} = \beta * \frac{enforce_{cmy}}{VMT_{cy}} + m_m + c_c + \alpha_1 * prcp_{cmy} + \alpha_2 * snow_{cmy} + \alpha_3 * unemp_{cmy} + u_{my} \quad (3)$$

$$E(f_{cmy}|X_{cmy}) = \exp(\beta * \ln enforce_{cmy} + m_m + c_c + \alpha_1 * prcp_{cmy} + \alpha_2 * snow_{cmy} + \alpha_3 * unemp_{cmy} + \alpha_4 * \ln VMT_{cy}) \quad (4)$$

The majority of the OLS results are similar to those from the Poisson models. In addition, the county level estimates for citations are smaller than the state-level results. Endogenous behavior on the part of the police might explain this finding. While the other two enforcement measures are constant across counties but vary over time, the citations given vary both over time and across counties. If the police wanted to minimize the loss of life when facing reductions in employment, they would reduce enforcement levels and consequently citations more in the regions where they expect the smallest response to enforcement reductions. This tactic would lead to relatively smaller increases in deaths or injuries compared to regions that have higher response rates to enforcement and citations. Thus if the police endogenously choose where to reduce enforcement (subject to mandated budget cuts) in order maximize the preservation of life, the citations estimates would be biased towards zero relative to the

state-level estimates.[30]

In summary, for each specification and aggregation we link decreases in enforcement to increases in injury rates. When citations is used as a measurement of enforcement, the state-level estimates are slightly larger than the county level estimates, potentially because there is some redistribution of police across counties after the layoff to minimize the layoff's impact. However, the most noteworthy pattern is that for both the state and county level analysis the elasticities were generally largest for the regions where state police have the largest presence (outside of city-limits) and under conditions where speeding is more prevalent and enforceable (fair weather conditions).

4.1.1 Oregon, Washington, and Idaho for 1979-2005

In the preceding analysis, we estimated a significant increase in injuries and fatalities in the period following the mass-layoff of police in Oregon. The fact that the increases were largest outside of city limits and under fair weather conditions is consistent with the reduced police presence leading to increases in speeding and dangerous driving. However, one short-coming of the previous analysis could be the presence of omitted variables. In this section, we utilize various counterfactual groups to adjust for potential unobserved time trends when estimating the effect of police on roadway safety.

Control groups are often chosen geographically as neighboring geographic areas often experience similar unobserved shocks other than the treatment of interest (see Card 1990,

[30] One way to address this is to estimate instrumental variable models by instrumenting citations with the number of police or budget shock. This isolates the variation in citations due to budget cuts rather than other factors. We have estimated these models and find that the number of police and the layoff are relevant instruments, and the instrumental estimates for citations get larger, maintaining similar degrees of statistical significance. These results are available upon request.

Card and Krueger 1994, among many others). In Table 6 we compare the United States and various subsets of states to serve as a counterfactual for Oregon with data collected from the American Community Survey over the time period 2000-2005. Column 1 contains Oregon, Column 2 contains the continental U.S. sans Oregon, Column 3 includes Washington and Idaho, while Column 4 contains a weighted average of states selected using the synthetic control design approach introduced by Abadie et al. (2010) – the details of which are discussed below.

Comparing Oregon with the rest of the continental U.S. numerous differences are evident, from weather patterns and economic conditions to demographic characteristics and commuting patterns. The differences are much smaller when contrasting Oregon with Washington and Idaho. By construction, the synthetic control design approach generates a counterfactual group designed to have similar trends and levels of observable factors. We follow the suggestion of Abadie et al. (2010), and select the weights based upon the ability of the regressors to predict the evolution of the dependent variable of interest (the number of roadway fatalities outside of city limits under fair driving conditions) during the pre-treatment window from January, 2000 to January, 2003.[31] Five states receive positive weights using the Abadie et al. (2010) method: Idaho (34.3 percent), Washington (32.9 percent), Montana (19.1 percent), New York (7.0 percent), Kansas (6.7 percent). Given Idaho and Washington account for nearly 70 percent of the synthetic control design counterfactual group, that methods also validates Washington and Idaho as a reasonable counterfactual group.

Obtaining information on all accidents and injuries over the 1979-2005 time period is

[31] Nearly identical weights are generated using different pre-treatment windows or different measures of fatalities including total fatalities state-wide or total fatalities outside of city-limit under all weather conditions.

not possible due to data limitations, however fatalities for this time period are collected by the Fatality Analysis and Reporting System (FARS). Unfortunately, indicators for freeways and highways did not exist in the FARS data for the entire 1979-2005 window. To create a similar measure we define a road as a highway or freeway if the reported speed limit at the crash site is greater than 45 miles per hour.

In Table 7, we estimate difference-in-difference regressions utilizing the various counterfactual groups identified above. The estimated coefficients are scaled by the average of the number of deaths per VMT to represent a semi-elasticity, which in turn allows easy comparison with the Poisson results in Table 5 and 8. The difference equations take the form seen in equation 5.[32]

$$\frac{f_{smy}}{VMT_{sy}} = \beta * After\_Layoff_{smy} * Oregon_s + S_s + M_m + Y_y + weather'\alpha + \alpha_2 * unemp_{smy}$$

$$+ a_3 * sp\_limit_{smy} + u_{smy} \tag{5}$$

When utilizing Idaho and Washington or the synthetic group as counterfactuals, we estimate similar increases in fatalities as those previously reported (i.e. point estimates ranging from 14 to 16 percent). When using the rest of the continental U.S. as counterfactual group, the estimates are, in general, smaller but remain statistically significant.[33] When

[32] Some of the variables regarding demographics and transportation utilized in the synthetic control design weight construction are not utilized in the difference-in-difference models as the demographic and transportation variables are slowly evolving over time (and hence are nearly perfectly colinear with state fixed effects). Near perfect multi-colinearity in controls can generate bias in finite samples (CITATION?).

[33] One complication in conducting appropriate hypothesis testing in the synthetic control design group is uncertainty over the actual weights in the construction of the synthetic group. To adjust for this uncertain,

utilizing other regions to adjust for potentially unobserved trends that could be driving the increase in fatalities in Oregon, the increase in traffic related deaths on highways (particularly those outside of city limits and on highways) remain. Given the percentage decrease in troopers (35 percent) observed in Oregon, the difference-in-difference results from 2000 to 2005 imply an elasticity for highway troopers ranging from -0.34 to -0.57, depending on the weather conditions and jurisdiction.

The layoff in Oregon in 2003 is the most recent of a series of reductions which have been ongoing since the early 1980's. This allows for a longer time period to be analyzed to confirm the relationship between police and roadway fatalities while controlling for common yearly regional trends present in Oregon, Idaho and Washington. We obtained records on police employment from Oregon and Idaho from 1979-2005, while Washington was only able to provide data from 1997-2005.[34] Figure 5 illustrates that there has been substantial variation in the state police departments in Oregon and Idaho since 1979. Indeed, in 1979 Oregon employed 641 police, more than double current trooper employment levels, but at a time when VMT were less than 60 percent of today's VMT.

Similar controls for the unemployment rate, weather, and VMT are included in this analysis. Driver's licence data were not available for this period. In the absence of this information, we used data from the U.S. Census Bureau to construct a measure of the

Abadie et al. (2010) suggest a placebo approach. We note that their suggested approach is complicated when geographic units have varying populations (and hence varying statistical noise). In addition, earlier potential placebo time periods (prior to 2000) had many laws changing across states including speed limits, mandatory seat belt usage, and stricter drinking laws. These complications prohibit the construction of a test statistic using the placebo groups. Therefore, our usage of the synthetic control design is best viewed as a robustness test of Idaho and Washington's validity as a counterfactual group.

[34] This results in an unbalanced panel data model. We have estimated models imputing likely values according to conversations with the Washington State Police or omitted Washington altogether and obtained similar results. According to several sources, the employment should be linked structurally to VMT, but we have been unable to verify this in official documents or statutes.

fraction of individuals between the ages of 16-25 and older than 65. Since laws pertaining to roadway safety have changed over the 1979-2005 time period, we also include controls for the maximum speed limit and the presence of mandatory seat belt laws.[35] In addition, we include state, year and month fixed effects to adjust for state-level time constant unobservables, seasonality, and common regional yearly trends. By including these counterfactual regions we can better control for changes in unobservables common across the region in a given year.[36] Equation 6 displays the econometric specification used in estimating the effect of troopers per VMT on fatalities per VMT for the 1979-2005 time frame.

$$\frac{f_{smy}}{VMT_{sy}} = \beta * \frac{enforce_{smy}}{VMT_{sy}} + S_s + M_m + Y_y + \alpha_1 * prcp_{smy} + \alpha_2 * snow_{smy} + \alpha_3 * unemp_{smy}$$

$$+a_4 * sp\_limit_{smy} + \alpha_5 * seat\_belt_{smy} + pop\_16_{sy} + pop\_65_{sy} + u_{smy} \tag{6}$$

Table 8 contains the regression results for these additional findings, which have been scaled to be elasticities for ease of comparison with previous results.[37] The coefficients suggest a 10 percent increase in troopers per VMT would reduce fatalities per VMT on all

[35] Seatbelt laws are taken from Cohen and Einav (2003).

[36] We have also estimated models using the national fatality rate per VMT for the rest of the nation as an additional control to adjust for unobservable changes in the national fatality rate per VMT, finding similar estimates and precision. Results are available upon request.

[37] While we use robust standard errors in these regressions, we also explored the use of other options to account for auto-correlation more generally as raised by Bertrand et al. (2004). Clustering at the state level actually reduced the the standard errors substantially, however the asymptotic approximation may be poor when there are only 3 cluster regions (states). In addition, while bootstrap methods have been shown to work well in Cameron et al. (2008), their performance drops dramatically when the number of clusters is less than 6. Indeed, their preferred method, the Wild bootstrap, has no power for any alternative hypotheses when the number of clusters is fewer than 6. See Sabia et al. (2010) for a more detailed explanation of the decline in power for those bootstrap methods.

roads by 2 percent and reduce fatalities on highways and freeways outside of city limits under dry weather conditions by 4.9 percent. One limitation to using the FARS data is that we are unable to corroborate our previous estimates for incapacitating and visible injuries over the entire 1979-2005 period. That said, the fatality estimates we obtain in this analysis are quite similar to the fatality elasticities estimated in our analysis of the recent mass layoff of police in Oregon.

5 Testing Other Potential Confounders

Although we have estimated a significant negative relationship between injuries and enforcement, it is import to examine if other observable trends in other determinants of fatalities and injuries could play a role in explaining the results. In Figure 6, trends for the number of teenage drivers, VMT across the state and the proportion of drivers wearing seat belts are compared to the timing of the layoff. All values are scaled using 2000 as a base year (so the base year takes on the value of 100), so we can interpret the levels as percentage changes from the 2000 level. Teenage drivers decline in number over the time span we study (they declined even more in proportion). Although VMT are slightly higher in the post-layoff years, they peaked in 2002, and in Figure 1 there was not a corresponding jump in deaths or injuries until the layoff in 2003. The proportion of people that reported wearing their seat belts in accidents fell only slightly in 2003 (by roughly 2 percent), and it was at the baseline levels in 2004 and 2005. In addition we also examined the incidence of drunk driving as a cause of accidents on freeways and highways, finding that they increased from one to two percentage points following the layoff. While this is large in relative terms, it is small in

absolute terms and could also have been caused by decreases in enforcement. Indeed, the fact that mandatory seat belt laws affect seat belt usage (Cohen and Einav, 2003) suggests the drivers are respond to financial penalties for not wearing seat-belts We also note that this other potential channel does not bias any of our reduced form elasticities between police and injuries (it would create bias if we wanted to estimate the effect of average speeds on fatality rates and instrumented average speeds with enforcement).

Another important factor which could be affected by budget cuts could be roadway construction and along with it, overall pavement quality. In our investigations of state budgets, we found no evidence linking the budget crisis with decreases in roadway funding, as illustrated in Appendix Table 1. In addition, the Oregon Department of Transportation conducts biannual reviews of the pavement quality of all highways (switching from odd to even years in 2004, resulting in two years where the reviews where conducted annually in 2003 and 2004).[38] On average, pavement quality actually increased slightly over the 2000-2005 period. This is demonstrated in Table 9, which contains the fraction of highways with pavement rated as good or better. Importantly, the roadway quality improved gradually every time an inventory was taken, and did not sharply increase around the time of layoff. In addition, in Figure 7, we plot the distribution of pavement quality (measured on 0-100 scale) for major sections of highway in the years 1999, 2001, 2003, 2004, and 2006. The evolution of the distributions over the 1999-2006 time period suggests that the fraction of roadways of a good or very good quality (rated 80 or better) increased in the years after the layoff, while the fraction of roadways rated as fair or poor (less than 80) fell. This confirms

[38] See http://www.oregon.gov/ODOT/HWY/CONSTRUCTION/pms_reports.shtml for the reports used to obtain these findings.

that changes in road quality were likely not a reasonable cause for the increase in injuries and fatalities that occurred after the police layoff.

Although observed factors do not explain the increase in injuries, unobservable driver behavior changes should be taken into account. In the previous section, the examination of the police layoff focused on injuries occurring on dry roads. Days with snow, rain, or ice could still be influenced by unobserved changes in driver behavior, but are unlikely to be affected by changes in enforcement. Under adverse weather conditions police officers are likely to be occupied with accidents, not having time to issue citations. And even if time allowed for enforcement, pulling drivers over in the rain or snow could also be dangerous to both the driver and the police officer. Estimating the relationship between troopers employed and injury rates under adverse weather conditions offers a simple test regarding whether drivers have become inherently more risk-loving coincidently with the layoff or if roadway quality has declined. As shown in Table 10, troopers employed and citations show seemingly no relationship (both in magnitude and statistical significance) with injuries occurring under hazardous weather conditions. Only more minor injuries show weak evidence of reductions following the layoff. Under conditions where the change in police enforcement is unlikely to influence driver behavior, the various measures associated with enforcement levels have no statistical relationship with injury rates.

6 Policy Implications

The 2003 police layoff in Oregon was not the only reduction in employment that OSP has experienced. In 1979, Oregon employed 641 police, which fell to 250 troopers by 2005. Simultaneously, VMT have increased by 80 percent. We consider two hypothetical scenarios: 1) the OSP remain at their 1979 levels throughout the entire time period and 2) the OSP levels increase at the same rate as VMT. Using our previous estimates we estimate the predicted number of fatalities in each month for Oregon from 1979 through 2005 by adding the number of fatalities per VMT to the difference in the number of hypothetical police per VMT and then multiplying by the relevant coefficient from Table 8.[39] The results of these estimates are depicted in Figure 8. The blue line represents the fatality rate at actual police levels and the red, dashed lines represents the predicted fatality rate if OSP had been allowed to increase their staff with VMT. Although fatalities per VMT fell during 1979-2005, potentially due to improvements in car safety features, roads, medical technology or other changes, the decrease would have been larger had trooper employment increased at the rate of change of VMT.

In Table 11, we compare the total number of fatalities occurring under each scenario against the total number of additional full time equivalents (FTE) of state police and the final employment levels in 2005. The total state police FTE needed for each scenario are calculated by adding the total number of police employed annually across all years, 1979-2005. The first row contains actual state police employment and fatalities, while rows 2 and 3 contain the counterfactual police and predicted number of fatalities. If police employment

[39] Algebraically, $\hat{f}_{ym}^{1979} = f_{ym} + (\frac{enf}{VMT}_{ym}^{1979} - \frac{enf}{VMT}_{ym}) * \hat{\beta}_{\frac{enf}{VMT}}$.

had stayed at 641 troopers, fatalities would have fallen by 1,654 while an additional 5,445 state police FTE would have been needed. Similarly, if the state trooper levels had increased to keep pace with VMT then there would have been 3,841 fewer fatalities from 1979-2005 while the state police FTE would have more than doubled to 24,505 over the same time period. The current cost of outfitting a trooper per year is approximately $100,000, implying scenario 1 would have cost an additional $545 million while scenario 2 would have cost $1.2 billion. These results imply it would have cost approximately $320,000 per life saved over the 1979-2005 window, which is far less than the general range of accepted estimates for the value of a statistical life.[40] In addition, our analysis of the recent Oregon mass layoff suggests that additional police presence would likely prevent other injuries that are not accounted for in the FARS data. However, to fully assess the net social benefits one must also consider other factors such as time saved, the value of time, and other injuries potentially reduced or property damage prevented by the increases in enforcement.[41]

7 Conclusion

Police have long been a tool for enforcing speed limits on highways. We offer evidence concerning the effect of police on roadway safety, motivated by the mass layoff of Oregon State Police due solely to budget cuts. Our results indicate that a decrease in enforcement, defined

[40] For instance, Ashenfelter and Greenstone (2004) find voters reveal their value of statistical life to be $1.4 million, while Viscusi and Aldi (2003) estimates the median value of statistical life among US workers to be close to $7 million.

[41] In an earlier version of the paper, we also utilized the layoff to estimate the value of statistical life, estimating it to be $1.2 million. However a number of complications arise when conducting this anaysis. The largest in our view, is that voters had to vote an entire menu of budget cuts when they voted in favor or against *Measure 28*. As such, we cannot assess whether Oregonians preferred the police be laid off or would have elected to keep the police and cut other budgets.

by either troopers employed or citations given, is associated with an increase in injuries and deaths on Oregon highways. Our preferred estimates for the elasticity between enforcement and injuries range between -0.2 and -0.5, suggesting a non-trivial association between enforcement and safety. In addition to studying the Oregon mass-police layoffs, we also study police employment in Oregon and two neighboring states, Idaho and Washington. Estimating the relationship between enforcement and fatalities from 1979-2005 while controlling for common regional trends shared by the states, we find that the effects of police on roadway safety (as measured by fatalities) are quite similar to those estimated using the budget cut in Oregon. An analysis of the reduction in state police in Oregon since 1979 suggests that there would have been 1,158 fewer deaths over the 1979-2005 time span if the state police had maintained their original staffing levels. Moreover, if the police force were allowed to grow at the same rate as the increases in VMT (which would amount to a 360 percent increase over actual staffing levels in 2005), then there would have been 3,840 fewer fatalities over 1979-2005.

It is worth noting that to the extent that nonlinearities or decreasing returns to enforcement exist, these estimates could be upper bounds. In addition, our estimates are the strongest for highways and freeways on dry weather conditions outside of city-limits, which are regions and conditions that account for roughly 1/3 of the total fatalities in Oregon, and a much smaller fraction of less severe injuries. As such, it is worth bearing in mind that our results have implications for driver behavior under those conditions, and driver responses to changes in local enforcement may differ.

Because of the current budget shortfalls many state legislatures are either currently considering or have recently implemented large layoffs or furloughs in their state police forces,

such as Illinois[42] (which had plans to layoff 460 state police), Virginia[43] (recently laid off 104 state police), and Michigan[44] (recently laid off 100 police). Recent research has suggested that poor local economic conditions actually lead to increases in citations among local jurisdictions (see Makowsky and Stramann,2009), while our findings suggest that these budget cuts in state police and the following reductions in enforcement would likely be followed with increased injuries and fatalities unless the states utilize other enforcement tools – such as increased fines – to offset the reduction in enforcement.[45] Future work could investigate more fully the effect of fine increases on driver behavior and their usefulness as another variable in deterrence.

[42] See http://qctimes.com/news/local/article_cc6bfc2e-37b1-11df-b2a2-001cc4c002e0.html

[43] See http://www2.timesdispatch.com/rtd/news/state_regional/state_regional_govtpolitics/article/JOBS17_20090916-222607/293459/

[44] See http://detnews.com/article/20090506/POLITICS02/905060364/Michigan-budget-cuts-hit-police-ranks

[45] See Graves et al. (1989) for a discussion of optimal fines and enforcement on roadways. Increases in fines are currently under debate in Illinois.

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8 Figures and Tables

Figure 1

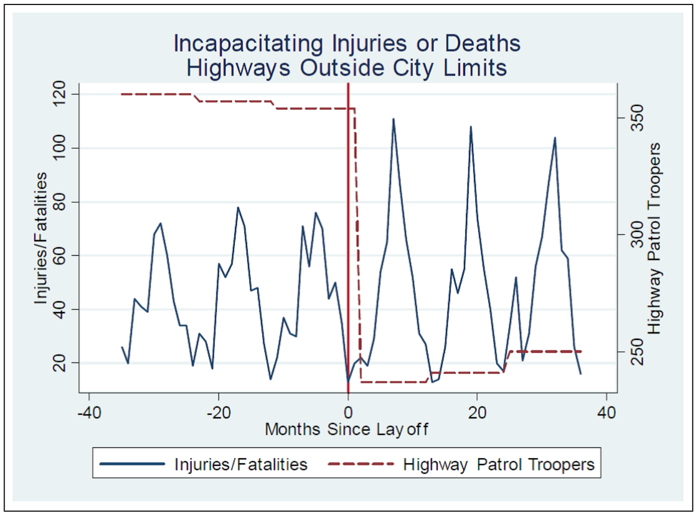


Figure 2

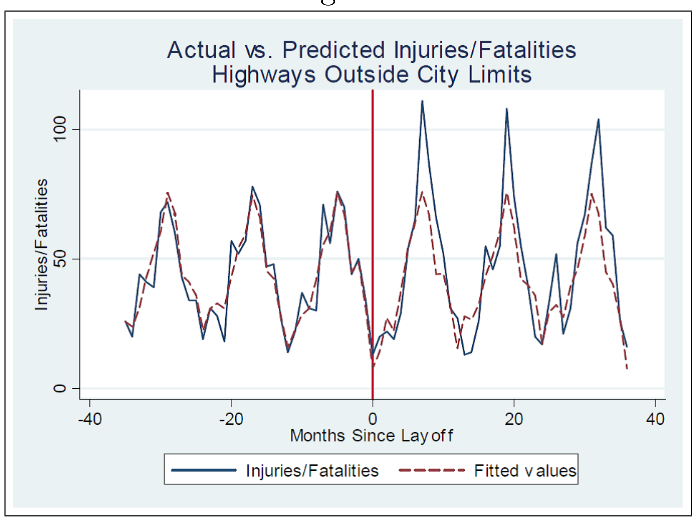


Figure 3

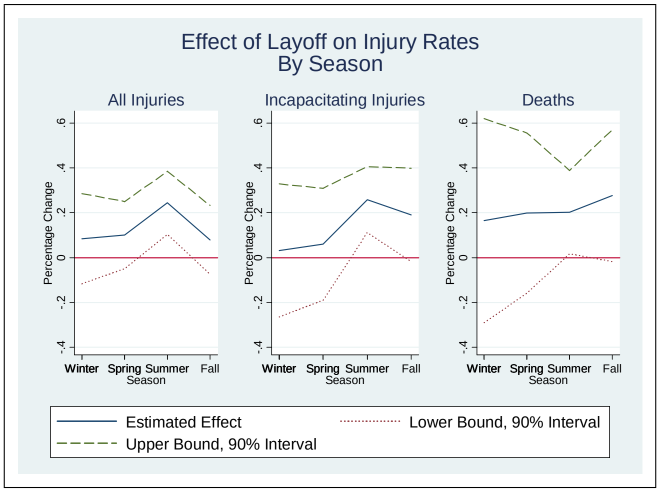


Figure 4

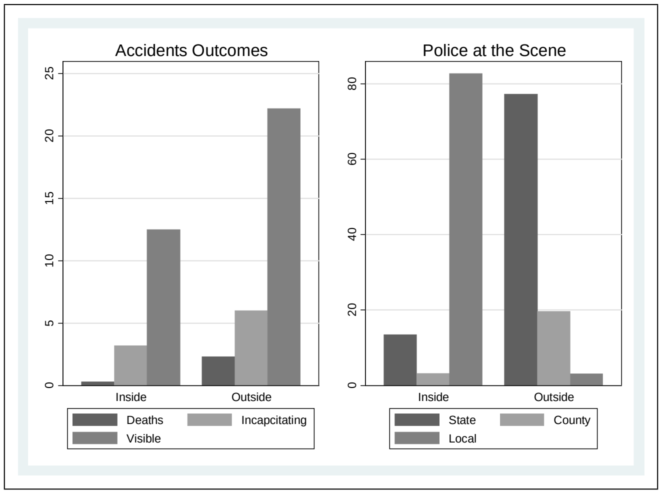


Figure 5

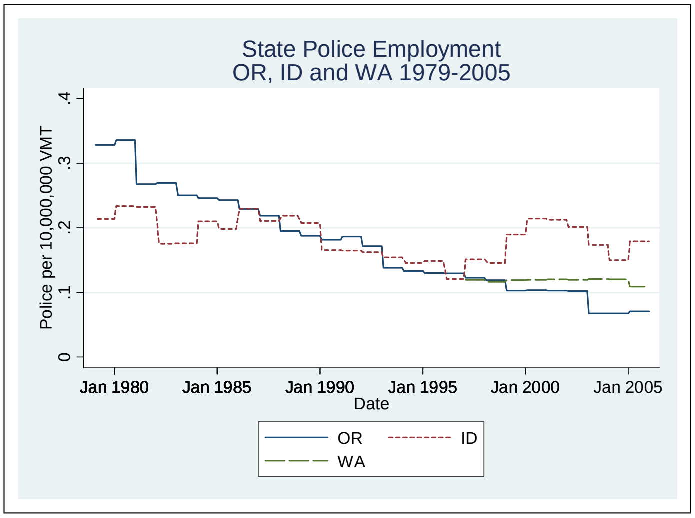


Figure 6

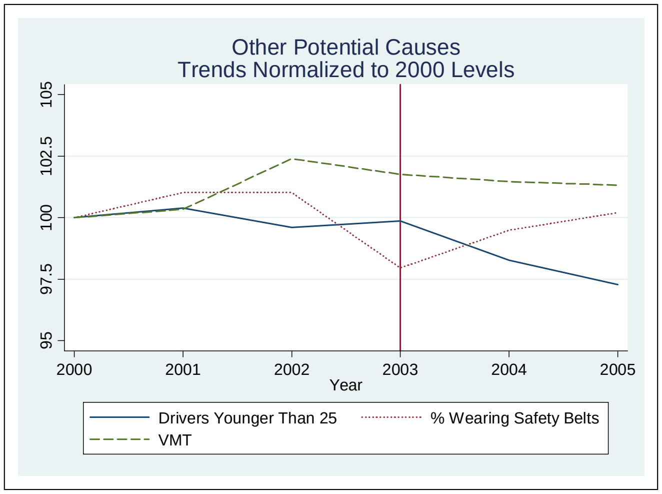


Figure 7

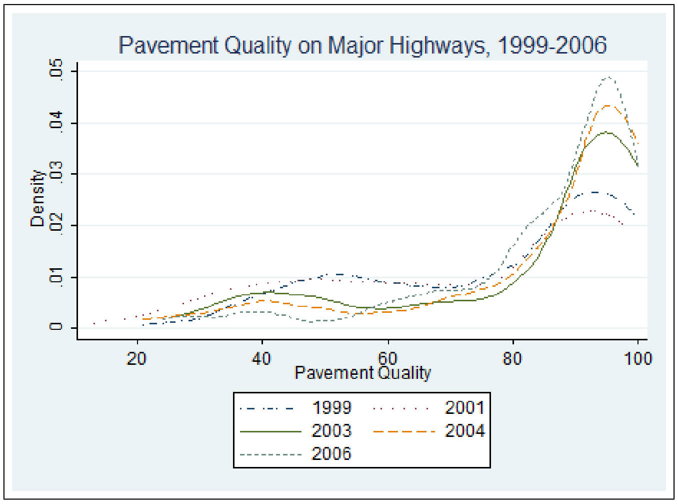


Figure 8: Oregon Fatality Counterfactuals

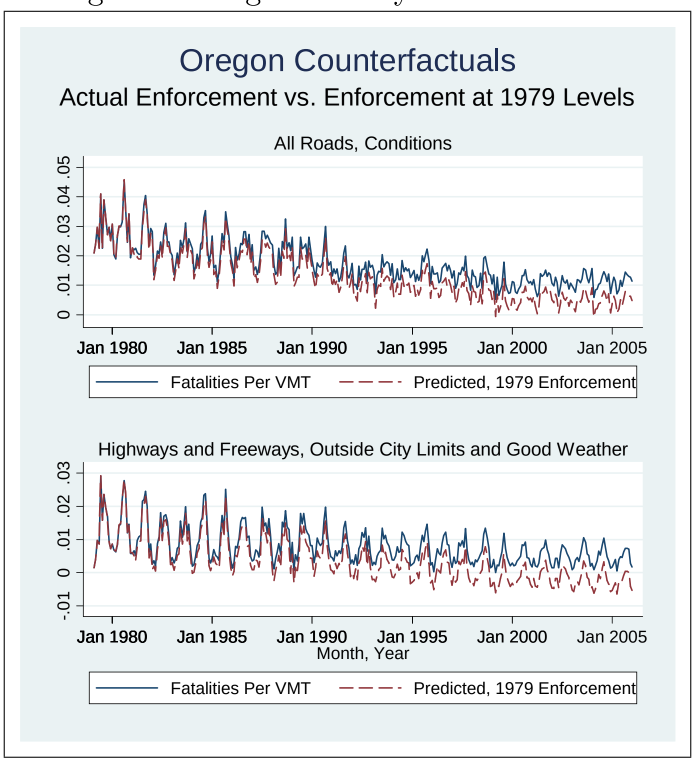


Table 1
Summary Statistics for Injuries Under Dry Weather Conditions

		Mean (s.d.)	Before Layoff	After Layoff	|t-test|	|t-test| seasonally adjusted
		State Level Summary Statistics				
Outcomes	Deaths	13.05 (6.9)	11.9	14.2	1.41	2.03**
	Incapacitating Injuries	45.6 (23.6)	42.8	48.6	1.04	1.84*
	Visible Injuries	173.7 (85.0)	164.2	183.7	0.98	1.80*
Enforcement	Citations	6411.8 (1726.2)	7,369.0	5,450.0	5.64***	7.30***
	Troopers	301.5 (57.6)	356.9	242.8	114.06***	114.09***
Road Characteristics	Yearly VMT (in Billions)	20.7 (0.17)	20.5	20.60	N/A	N/A
	Precipitation (inches)	2.99 (2.43)	2.9	3.1	0.40	1.06
	Snowfall (inches)	1.59 (2.50)	1.6	1.5	0.26	0.25
Driver Characteristics	Pop<25 w/ License	429,686 (4774)	432,992	426,377	N/A	N/A
Observations			37	35		
		County Level Summary Statistics				
Outcomes	Deaths	0.37 (0.80)	.35	.40	1.79*	1.54
	Incapacitating Injuries	1.76 (2.69)	1.7	1.9	1.72*	1.50
	Visible Injuries	7.12 (8.69)	6.8	7.4	1.80*	1.50
Enforcement	Citations	178.1 (162.5)	207.2	147.4	9.53***	10.07***
Road Characteristics	Yearly VMT (in Billions)[46]	5.7 (6.6)	5.7	5.8	.13	.13
	Precipitation (inches)	2.99 (3.44)	2.9	3.1	1.7*	3.4***
	Snowfall (inches)	1.59 (4.03)	1.6	1.5	0.69	0.69
Driver Characteristics	Pop<25 w/ License	11,936 (16,908)	12,207	11,839	0.28	0.28
Observations			1,332	1,260		

All injuries, citations, prcp., and snow are monthly measures, while the rest are annual averages.
*, **, ***, indicate significance at the 10, 5, and 1 percent levels, respectively

[46] Estimates of state level VMT were available for highways only, while county level VMT measures include all roads, both highways and non-highways. In as much as the proportion of VMT on highways remained stable after the layoff, our results will remain unaffected by this source of measurement error.

Table 2: Enforcement-Injury Elasticities
Oregon State-Level OLS Estimates

		Dry Weather Conditions			*All Weather Conditions*		
		Deaths Per VMT	*Incap. Per VMT*	*Visible Per VMT*	*Deaths Per VMT*	*Incap. Per VMT*	*Visible Per VMT*
Highways Outside City Limits	*After Layoff*	0.14^{*}	0.12^{**}	0.10^{**}	0.07	0.09^{*}	0.11^{***}
	Semi-Elasticity	(0.08)	(0.05)	(0.05)	(0.06)	(0.05)	(0.03)
	Troopers Per VMT	-0.38^{*}	-0.31^{**}	-0.27^{***}	-0.31^{*}	-0.24	-0.20^{**}
	Elasticity	(0.21)	(0.15)	(0.08)	(0.19)	(0.14)	(0.08)
	Citations Per VMT	-0.36^{*}	-0.27^{**}	-0.26^{**}	-0.30^{*}	-0.21^{**}	-0.23^{**}
	Elasticity	(0.17)	(0.14)	(0.12)	(0.16)	(0.10)	(0.11)
All Highways	*After Layoff*	0.08	0.11^{**}	0.13^{**}	0.06	0.08^{*}	0.12^{**}
	Semi-Elasticity	(0.07)	(0.05)	(0.04)	(0.06)	(0.043)	(0.04)
	Troopers Per VMT	-0.20	-0.28^{**}	-0.34^{**}	-0.17	-0.22^{*}	-0.32^{***}
	Elasticity	(0.16)	(0.12)	(0.12)	(0.17)	(0.12)	(0.10)
	Citations Per VMT	-0.16	-0.23^{**}	-0.32^{**}	-0.14	-0.16	-0.29^{***}
	Elasticity	(0.16)	(0.11)	(0.12)	(0.14)	(0.11)	(0.10)

This table reflects the elasticities between injury rates and enforcement. Each cell is a separate OLS regression for the number of injuries scaled by VMT. Controls include month fixed effects, unemployment, precipitation, and snow. All regressions use robust standard errors.
*, **, ***, significant at 10, 5, and 1 percent levels, respectively

Table 3: Enforcement-Injury Elasticities
Oregon State-Level Poisson Estimates

		Dry Weather Conditions			*All Weather Conditions*		
		Deaths	*Incap.*	*Visible*	*Deaths*	*Incap.*	*Visible*
	After Layoff	0.16**	0.13***	0.12***	0.12*	0.09*	0.10***
	Semi-Elasticity	(0.07)	(0.05)	(0.04)	(0.064)	(0.05)	(0.04)
Highways	*Troopers*	−0.43**	−0.36***	−0.32***	−0.32*	−0.23*	−0.26***
Outside	Elasticity	(0.20)	(0.13)	(0.11)	(0.19)	(0.13)	(0.09)
City Limits	*Citations*	−0.38**	−0.29***	−0.27***	−0.30**	−0.17	−0.20**
	Elasticity	(0.16)	(0.11)	(0.08)	(0.14)	(0.12)	(0.06)
	After Layoff	0.10	0.13***	0.16***	0.07	0.08*	0.13***
	Semi-Elasticity	(0.06)	(0.04)	(0.04)	(0.06)	(0.04)	(0.03)
All Highways	*Troopers*	−0.26***	−0.33***	−0.40***	−0.18	−0.22**	−0.32***
	Elasticity	(0.17)	(0.11)	(0.10)	(0.15)	(0.11)	(0.09)
	Elasticity *Citations*	−0.20	−0.26***	−0.34***	−0.16	−0.14**	−0.26***
		(0.14)	(0.09)	(0.08)	(0.13)	(0.09)	(0.07)

This table reflects the elasticities between injury rates and enforcement. Each cell is a separate count regression for the number of injuries. Controls include month fixed effects, precipitation, snow and the unemployment rate. All Poisson regressions use a robust variance covariance matrix, relaxing the mean-variance-equality assumption.

*, **, ***, indicate significance at 10, 5, and 1 percent levels, respectively

Table 4: Enforcement-Injury Elasticities
Oregon County-Level OLS Estimates

		Dry Weather Conditions			*All Weather Conditions*		
		Deaths Per VMT	*Incap. Per VMT*	*Visible Per VMT*	*Deaths Per VMT*	*Incap. Per VMT*	*Visible Per VMT*
Outside City-Limits	*Citations Per VMT Elasticity*	$-.16^{*}$	$-.14^{**}$	$-.12^{***}$	$-.16^{**}$	$-.14^{***}$	$-.13^{***}$
		(.09)	(.06)	(.04)	(.08)	(.05)	(.03)
All Highways	*Citations Per VMT Elasticity*	$-.12$	$-.12^{**}$	$-.11^{***}$	$-.12$	$-.12^{**}$	$-.11^{***}$
		(.09)	(.05)	(.03)	(.08)	(.05)	(.03)

This table reflects the elasticities between injury rates and enforcement. Each cell is a separate count regression for the number of injuries. Controls include month fixed effects, precipitation, snow, the unemployment rate log of drivers over 65, log of drivers under 25, and county fixed effects. The OLS regressions use a robust variance covariance matrix.
*, **, ***, indicate significance at 10, 5, and 1 percent levels, respectively

Table 5: Enforcement-Injury Elasticities
Oregon County-Level Poisson Estimates

		Dry Weather Conditions			*All Weather Conditions*		
		Deaths	*Incap.*	*Visible*	*Deaths*	*Incap.*	*Visible*
Highways Outside City-Limits	*Citations*	$-.18^{*}$	$-.16^{***}$	$-.12^{***}$	$-.21^{**}$	$-.17^{***}$	$-.14^{**}$
	Elasticity	(.09)	(.06)	(.04)	(.08)	(.05)	(.03)
All Highways	*Citations*	$-.14^{*}$	$-.12^{**}$	$-.09^{***}$	$-.15^{*}$	$-.12^{***}$	$-.09^{***}$
	Elasticity	(.08)	(.05)	(.03)	(.08)	(.04)	(.03)

Each cell is a separate count regression for the number of injuries. Controls include month fixed effects, precipitation, snow, county-level unemployment, log drivers under 25, log drivers over 65 and log of VMT. The Poisson regressions use robust standard errors, relaxing the mean-variance-equality assumption.
*, **, ***, represent significance at 10, 5, and 1 percent levels, respectively.

Table 6: Potential Counterfactual Groups for Oregon

	OR	US (w/o OR)	WA & ID	Synthetic
Precipitation	2.04	3.07	2.14	2.05
	(1.57)	(2.07)	(1.62)	(1.57)
Temperature	46.96	53.12	47.27	46.96
	(15.29)	(17.37)	(13.96)	(15.29)
Unemployment	6.79	4.84	5.51	5.22
	(1.16)	(1.15)	(1.26)	(1.22)
English Speaking	84.37	83.48	83.43	84.33
	(0.4)	(7.59)	(1.60)	(4.47)
High School Grad.	91.02	88.94	90.57	90.87
	(0.32)	(3.22)	(1.50)	(1.53)
College Grad.	30.16	28.67	28.88	29.12
	(0.96)	(5.60)	(5.59)	(0.42)
White	92.18	85.32	90.80	89.93
	(0.56)	(8.53)	(3.89)	(4.90)
Black	1.60	8.71	1.85	2.39
	(0.16)	(7.93)	(1.35)	(3.09)
Commute via Car	85.78	90.05	87.37	85.86
	(0.47)	(4.33)	(1.29)	(5.08)
Telecommute	5.51	3.95	5.16	5.43
	(0.41)	(1.12)	0.60	(1.17)
Public Transportation	3.59	2.45	2.79	3.46
	(0.17)	(3.54)	(1.8)	(5.24)
Transportation Time	19.77	21.18	20.74	19.84
	(0.44)	(3.33)	2.37	(3.71)

These data are calculated using the 2000-2005 American Community Surveys.

Table 7: Difference-in-Difference Estimates of the Change in Fatalities per VMT

	Outside City Limits			All Highways		
	Dry Weather Conditions			All Weather Conditions		
Counterfactual	WA & ID	US (w/o OR)	Synthetic	WA & ID	US (w/o OR)	Synthetic
Oregon*After Layoff	.20**	.10***	.17**	.14**	.05***	.12*
	(.09)	(.01)	(.08)	(.06)	(.01)	(.06)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

This table reflects the semi-elasticity of the police layoff and fatalities per VMT. In addition to the controls listed above, w
include the unemployment rate, maximum speed limit, and weather conditions as controls in the regressions.
All regressions are estimated using OLS and use robust standard errors.
*, **, ***, indicate significance at the 10, 5, and 1 percent levels, respectively

Table 8: Enforcement Elasticities and Fatalities per VMT

Washington, Oregon, and Idaho 1979-2005

Variables	All Roads	All Roads Dry Weather	Highways and Freeways Outside of City-Limits Dry Weather
Troopers Per VMT	-.23*	-.38**	-.49**
Elasticity	(.12)	(.18)	(.22)
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes

This table reflects the elasticity between troopers and fatalities per VMT. In addition to the controls listed above, we include the unemployment rate, maximum speed limit, presence of a mandatory seat belt law, proportion of population between 16 and 25 and the proportion of the population older than 65 as controls in the regression.
All regressions are estimated using OLS and use robust standard errors.
*, **, ***, indicate significance at the 10, 5, and 1 percent levels, respectively

Table 9: Average Pavement Quality 1999-2006

Year	All Highways	Interstate	Non-Interstate	State Highways
1999	78	88	83	69
2001	81	89	86	74
2003	84	92	88	77
2004	85	94	88	79
2006	87	98	87	82

This table contains the fraction of locations on highways reporting pavement conditions as good or very good from the Oregon Department of Transportation.

Table 10: Hazardous Roads, Increase in Injuries

		OLS Estimates			Poisson Estimates		
		$\frac{Deaths}{VMT}$	$\frac{Incapacitating}{VMT}$	$\frac{Visible}{VMT}$	$Deaths$	$Incapacitating$	$Visible$
	$After\ Layoff$ Semi-Elas.	.020	.002	.08	.032	.020	.08
		(.11)	(.10)	(.07)	(.11)	(.086)	(.06)
Highways and Freeways Outside of City-Limits	$\frac{Troopers}{VMT}$ $Elas.$	−.06	−.01	−.22	−.10	−.04	−.20
		(.38)	(.27)	(.18)	(.28)	(.23)	(.16)
	$\frac{Citations}{VMT}$ $Elas.$	−.07	.008	−.15	−.10	.04	−.09
		(.32)	(.24)	(.15)	(.23)	(.15)	(.12)
	$After\ Layoff$ Semi-Elas.	.02	−.005	.09	.02	.01	.09
		(.12)	(.010)	(.06)	(.10)	(.08)	(.05)
All Highways and Freeways	$Trooper\ Elas.$	−.04	.007	−.24	−.05	−.04	−.24
		(.32)	(.028)	(.16)	(.26)	(.22)	(.15)
	$Citations\ Elas.$	−.04	.06	−.19	−.06	−.06	−.13
		(.29)	(.23)	(.13)	(.22)	(.17)	(.11)

This table estimates the effect of the police layoff state-wide in Oregon, under conditions where police may not have a large influence on . driver behavior. Each cell represents a separate regression. Controls include month fixed effects, precipitation, snow, and the unemployment rate. All regressions use robust standard errors. *, **, ***, significant at 10, 5, and 1 percent levels, respectively.

Table 11: Counterfactuals 1979-2005

	Fatalities 1979-2005	Troopers, 2005	Troopers FTE 1979-2005
Actual Levels	14,662	250	11,862
Counterfactuals			
Troopers=641	13,008	641	17,307
$\frac{\text{Troopers}}{\text{VMT}}=\frac{\text{Troopers}}{\text{VMT}}1979$	10,820	1,159	24,505

This table contains estimates for the number of fatalities which would have resulted under various counterfactual scenarios.

9 Appendix

Appendix Table 1 contains the budget cuts by agency, as mandated by *House Bill 5100*, to verify that the Oregon State Police is the only agency directly related to roadway safety that experienced budget cuts. The other agencies that experienced budget reductions do not appear to be directly linked to roadway safety, suggesting that there were not other large policy changes that would be collinear with the police layoff. Although prisons experienced budget cuts, the Oregon legislature never passed the necessary constitutional amendments to release prisoners from their sentences early (due to budget reasons rather than good behavior). This gives credence to the fact that estimating the effect of the layoff on injury rates will not be contaminated by other, omitted budget cuts.

Appendix Table 1

Schedule of Budget Cuts (in millions of dollars)

Agency	Biennium Budget Cut
K-12 Education	101.18
Community colleges	14.91
Higher education	24.50
Prisons	19.17
Oregon State Police	12.2
Oregon Youth Authority	8.52
Medical assistance programs	23.43
Programs for seniors and the disabled	23.43
Services for the developmentally disabled	12.78
Services for children and families	11.72

Sources: Oregon State Police budget information acquired from the 2003-2005 legislatively approved budget. Other budget information was obtained from House Bill 5100.

Appendix Table 2 contains additional summary statistics for deaths, incapacitating injuries and visible injuries in other jurisdiction boundaries and weather conditions, completing the summary statistics presented in Table 2 for the regression results in Tables 4 through 8.

Appendix Table 2
Summary Statistics

		Mean (s.d.)	Before Layoff	After Layoff	|t-test|	|t-test| seasonally adjusted
		State Level Summary Statistics				
Outside City Limits	Deaths	19.2 (5.7)	18.3	20.2	1.28	1.37
All Weather	Incapacitating Injuries	70.4 (18.0)	68.1	72.8	1.29	1.55
	Visible Injuries	278.6 (57.1)	270.4	287.2	1.26	1.98**
Inside and Outside	Deaths	16.5 (7.4)	15.8	17.3	0.86	1.02
Dry Weather	Incapacitating Injuries	67.7 (21.4)	67.2	72.4	1.03	1.28
	Visible Injuries	335.4 (111.3)	322.2	349.3	0.83	1.51
Inside and Outside	Deaths	22.7 (6.3)	22.2	23.3	.72	.74
All Weather	Incapacitating Injuries	96.8 (20.6)	94.2	99.6	1.11	1.56
	Visible Injuries	464.8 (84.2)	448.6	481.8	1.69	2.16**

*, **, ***, indicate significance at the 10, 5, and 1 percent levels, respectively

Appendix Table 3 contains the regression results used in creating Figure 3.

Appendix Table 3: Effect on Number of Injuries

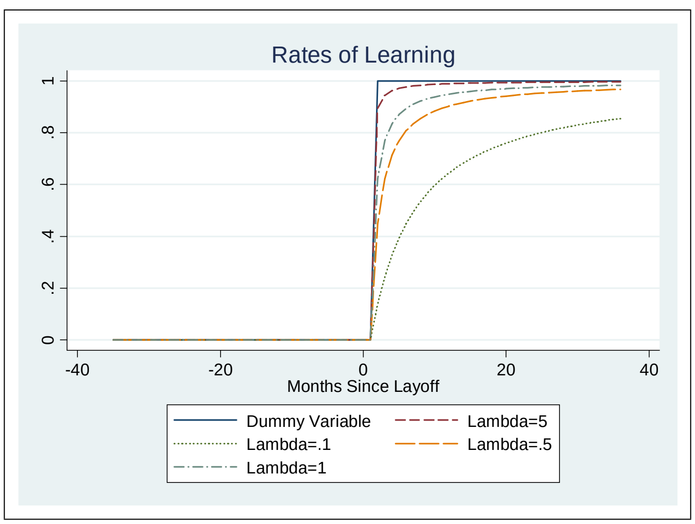
By Season

	Winter	Spring	Summer	Fall
Injuries	7.25	16.52	62.59**	12.62
	(10.47)	(14.92)	(21.8)	(14.82)
Incapacitating Injuries	0.73	2.53	17.48**	7.66
	(4.14)	(6.45)	(6.04)	(5.09)
Fatalities	1.17	2.20	3.92*	2.97
	(1.96)	(2.41)	(2.18)	(1.92)

Notes: This table contains estimates for the increase in the number of injuries, estimated separately for each season. The counts are determined for the number of injuries occurring on fair weather conditions on highways or freeways outside of city limits. Precipitation is included as a control. All models are estimated by OLS and use robust standard errors.

For robustness, exponential decay variables are also employed in Appendix Table 4 to estimate the effect of the layoff on injuries and deaths allowing for delayed learning. This gives a picture of the before/after effect of the layoff if there is a delay in drivers learning about the change in enforcement. We define the variables as $\left(\exp(1) - \exp(\frac{1}{1-\lambda * mon_t})\right) / (\exp(1) - 1)$, where λ represents a rate of learning and mon_t is zero before the layoff and then increases by 1 for each month after the layoff. Although this might seem a bit awkward at first, it has some intuitive appeal. If learning is immediate, then $\lambda = \infty$, in which case this variable is a standard indicator variable for the layoff – zero before and one after. As λ approaches zero, the rate of learning slows.[47] But given enough time for any value of $\lambda > 0$, eventually all drivers would become aware of the lack of troopers on the road. Appendix Figure 1 shows how the rate of learning varies across values of λ.

Append Figure 1



Appendix Table 4 contains the effect of the layoff measured by exponential decay variables with various rates of learning.[48] For the various rates of learning, the layoff is associated with

[47]If $\lambda = 0$, then the variable would always be zero, as no one would ever learn about the layoff.

[48] If λ was estimated without restrictions, standard test statistics would no longer be valid because of nuisance parameters not identified under the null. This problem was first identified by Davies (1977).

a substantial increase in deaths or other injuries. As the rate of learning increases there is a minor decrease in the magnitude of the effect of the layoff on injury rates, possibly because drivers had not yet fully learned of the decrease in enforcement in the months immediately following the layoff. However, even the smaller estimates suggest the layoff is associated with a 14% increase in fatalities, similar to the effect of immediately learning estimated by the indicator variable in Tables 2 and 3. Regardless of the value of λ specified[49], there is a positive association between the layoff and the number of injuries.

Because our choices for λ are ad hoc, standard test statistics remain valid, however there could be a decrease in power, as suggested by Hansen (1996). Thus, if we find significant results given an ad-hoc selection of the rate of learning, the level of statistical significance is likely conservative.

[49] With the exception of 0, which would allow no learning.

Appendix Table 4: Increase in Injuries, Oregon State-Level

	OLS Estimates			Poisson Estimates		
Learning Rate	$\frac{Deaths}{VMT}$	$\frac{Incapacitating}{VMT}$	$\frac{Visible}{VMT}$	*Deaths*	*Incap.*	*Visible*
$\lambda = .1$	0.16	0.17**	0.14**	0.18*	0.20***	0.17***
	(0.11)	(0.06)	(0.06)	(0.10)	(0.06)	(0.05)
$\lambda = .5$	0.14*	0.13**	0.11**	0.16**	0.16***	0.14***
	(0.09)	(0.06)	(0.05)	(0.08)	(0.05)	(0.04)
$\lambda = 1$	0.14*	0.12**	0.11**	0.16**	0.15***	0.13***
	(0.08)	(0.06)	(0.05)	(0.08)	(0.05)	(0.04)
$\lambda = 5$	0.14*	0.12**	0.10**	0.16**	0.14***	0.12***
	(0.08)	(0.06)	(0.05)	(0.08)	(0.04)	(0.04)

Notes: Controls include month fixed effects, precipitation, and snow, unemployment rate.

All OLS regressions use robust standard errors and Poisson regression use sandwich standard errors.

*, **, ***, indicated significance at 10, 5, and 1 percent levels, respectively.

Appendix Table 5 contains the summary statistics for the variables used in the tri-state regression analysis of section 4.2.

Appendix Table 5: Summary Statistics

	Oregon	Washington	Idaho
Years	1979-2005	1997-2005	1979-2005
Fatalities: All Roads, All	45.3	53.1	22.1
Weather Conditions	(12.1)	(10.1)	(8.5)
Fatalities: All Roads, Dry	31.4	37.7	17.3
Weather Conditions	(16.2)	(16.1)	(10.1)
Fatalities: Highways, Outside	20.6	19.4	12.8
City Limits, Dry Weather	(12.0)	(10.8)	(7.8)
VMT (millions)	27.6	53.7	10.7
	(5.7)	(1.5)	(2.8)
State Troopers	439	636	194
	(105)	(24)	(47)
Precipitation	3.2	3.6	1.6
	(2.5)	(2.6)	(0.9)
Snow	2.1	2.2	4.4
	(3.3)	(3.3)	(6.7)
Unemployment	7.1	5.6	6.1
	(1.8)	(1.0)	(1.4)
% with Age>=16 &<25	0.13	0.12	0.14
	(0.01)	(0.01)	(0.01)
% with Age >=65	0.13	0.10	0.11
	(0.01)	(0.002)	(0.01)
Observations	324	108	324