Life and Death in the Fast Lane: Police Enforcement and Traffic Fatalities

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October 6, 2011

Abstract

This paper estimates the causal effect of police on traffic fatalities and injuries. Due to simultaneity, estimating the causal effect of police enforcement 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, which dramatically reduced citations. The subsequent decrease in enforcement corresponds with a 10 to 20 percent increase in injuries and fatalities on highways, with the strongest effects under fair weather conditions and outside of city-limits. Utilizing various counterfactual groups, we find similar effects. Based on our estimates, we find a highway fatality can be prevented with $320,000 in expenditures on state police.

Keywords: Enforcement, Police and Crime, Deterrence, Traffic Fatalities, Roadway Safety
JEL Classification: K1, K4, H4, R4

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†We thank Peter Kuhn, Peter Rupert, Olivier Deschênes, Phil Cook, Jason Lindo, Dan Rees, Gary Charness, Doug Almond, Richard Arnott, Kelly Bedard, Javier Birchenall, Chris Costello, and Doug Steigerwald for helpful comments and advice. We also thank particpants at the NBER summer institute and the 2008 WEAI, CEA, APPAM, and 2009 AEA/ASSA annual meetings. We also thank seminar participants at the University of Oregon, University of Colorado at Boulder, Brigham Young University, University of Texas at Arlington, and Rensselaer Polytechnic Institute for insightful comments.

1 Introduction

It is estimated that between 750,000 and 1,180,000 lives are lost worldwide annually due to motor vehicle accidents (Peden et al., 2004). Translating the costs of accidents into dollars, some estimates have put the damages from accidents in the range of $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 decrease traffic fatalities and injuries is police enforcement. Police officers frequently issue tickets for speeding as speeding is one of the most common violations of the law and also is one of the most frequent causes of roadway fatalities.[2] While speeding may seem like a minor crime, traffic fatalities due to speeding in the U.S. have approached the number of murders reported in recent years.[3] In this paper, we estimate the causal effect of highway patrol officers on traffic fatalities and serious injuries, exploiting a recent mass layoff of state police in Oregon that consequently reduced the likelihood that speeders were apprehended by police.

Increasing fines and apprehension probabilities have long been considered as options for the government to reduce criminal activities. This has been supported by economic models of crime from Becker (1968), Polinsky and Shavell (1979), Imrohoroglu et al. (2004), and Lee and McCrary (2009) that find that increased expected penalties can reduce engagement in criminal behavior. However, estimating the degree to which fines and apprehension probabilities deter crime has posed a difficult problem empirically due to simultaneity. Regions with high crime rates tend to have higher levels of enforcement, presumably in an effort to reduce crime, creating omitted variable bias in estimates which rely solely on cross-

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

[3] See http://www-nrd.nhtsa.dot.gov/Pubs/809915.pdf and http://www.disastercenter.com/crime/uscrime.htm.

sectional variation in enforcement (Levitt and Miles, 2006). To overcome this type of reverse causality, recent research that concerns the apprehension probabilities and crime has relied on quasi (or natural) experiments. Notable examples of research utilizing sources of quasi-experimental variation in enforcement probabilities include the addition of a third referee in professional basketball (McCormick and Tollison, 1984), the hiring of police due to electoral cycles (Levitt, 1997), the provision of federal grants allowing local agencies to employ additional police beyond what local resources would allow (Evans and Owens, 2007), and recently, local variation in police employment after instrumenting to adjust for measurement error (Chalfin and McCrary, 2011).[4]

Although the studies mentioned above take advantage of plausibly exogenous variation in enforcement, ours is the first to utilize a large layoff of police.[5] Key for our identification, the budget shortfall causing the layoff resulted from limitations in property taxes enacted in the previous decade due to a tax-revolt. This offers a unique quasi-experiment for studying the effects of policing on traffic fatalities and injuries, as the study of recent police layoffs in the current recession would likely be confounded with the severity of the current economic downturn.[6] Likewise, this highlights the relevance of our findings for many states that are

[4] 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 slightly 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.

[5] 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.

[6] 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 that takes advantage of the endogenous reponse of local police to offset the exogenous decrease in local resources. While they find increased citations reduce property damage accidents, their estimates may be biased if the decrease in local resources are correlated with local economic

considering similar reductions to their highway patrols due to current budget shortfalls. We find that the reduction in state police employment is associated with significant increases in injuries and fatalities, respectively measuring 11 and 17 percent under fair weather conditions. Likewise, we find similar results utilizing various counterfactual groups either chosen geographically or based on the synthetic control approach of Abadie et. al (2010). Our results also complement the findings of Ashenfelter and Greenstone (2004), as a change in the speed limit can be viewed as a visible change in penalties. Lastly, given the 2003 layoff was the most recent of a series of layoffs in Oregon dating back to the 1980's, we find that Oregon would have experienced 1,658 fewer fatalities from 1979-2005 if the number of state police had been maintained at their original 1979 levels.

The remainder of our paper is organized as follows. 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 and econometric specifications, while section 4 provides an empirical examination of the effects of enforcement levels on fatalities and injuries as well as discussing counterfactual simulations based on our estimates. Section 5 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,

conditions.

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".[7] Knowing 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[8], *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.[9] Layoffs were decided

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

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

[9] 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,

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 enforcement on roadway safety.[10] *Measure 30*, which was essentially a carbon copy of *Measure 28*, was introduced in 2004 and faced the same fate as its predecessor. The time-line of events leads to a unique natural 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 substantial 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 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 the potential impact of the police layoff on injury rates.[11] Moreover,

however their role is largely managerial. Over 70 percent of the layoffs were state police whose position was designated as a "roadway trooper".

[10] 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.

[11] In 2003, Senate Bill 504 would have increased the Oregon speed limit on freeways from 65 to 70 MPH,

during the 2000-2005 time window the fatality rate per vehicle miles traveled (VMT) fell by 3.7 percent for the rest of the United States.[12]

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, average speeds increase by a few miles per hour during the summer months. The impact of increased VMT 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.[13] 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 the Oregon Department of Transportation (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. 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 fatalities (within 30 days of the accident), incapacitating

but it was vetoed by the governor. Measures to increase the fine structure further in 2005 never were passed by the legislature.

[12] Author's calculations.

[13] 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.

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 the minimum property damage necessary for a property-damage-only accident to be recorded in the database increased by 33 percent in 2004.[14] OSP provided information on trooper 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 in Table 1.

Even in the simple summary statistics in Table 1, an increase in deaths, incapacitating injuries, and visible injuries is evident and statistically significant[15] when adjusting for seasonality.[16] 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

[14] Estimated property damages are not recorded in the database, otherwise we would have constructed a consistent series for property damage accidents. Using the data on property damage accidents available through 2004 prior to the change in recording, we estimate a 4 percent increase in property-damage only accidents following the layoff.

[15] 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.

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

controlling for weather.[17] 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 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 affecting roadway safety.[18]

3.1 Specifications

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.[19] 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 sometimes 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

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

[18] 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, 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.

[19] 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.

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.[20] 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 coefficients can be interpreted as semi-elasticities. This type of count model produces similar conditional means to estimating a linear regression model in which $E(\ln y|x) = X'B$,[21] 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.[22] 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.[23]

[20] While we adjust for heteroskedasticity or over-dispersion, the reported results do not adjust for serial correlation. When we corrected for serial correlation through methods such as those in Newey and West (1987), the standard errors were more precise with greater statistical significance. As Andrews (1991) found that methods such as Newey and West (1987) often over-reject the null hypothesis in finite samples, we utilize the robust standards which are more conservative in our case.

[21] 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.

[22] 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.

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

4 Results

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.[24] If the OSP layoff is indeed responsible for the increase in injuries and fatalities, one would expect the increase in fatalities to be largest in the jurisdiction of the state police. It was infeasible to obtain the universe of 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, and so we might expect the areas outside of city-limits to be the most affected by the layoffs.[25] Moreover, injuries tend to be more severe in the areas where state police enforce, 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.

Three measures of the variation in police enforcement are utilized in separate regressions. The first is a before-after indicator for the layoff (which equals 1 after the layoff). The second measure of enforcement is the number of state troopers. The last measure is enforcement is

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

[25] 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.

the number of citations given out by the state police. The employment and citation measures can be utilized to generate enforcement elasticities.[26]

4.1 Oregon Results, 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 (via dummy variables for the month of the 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.[27] The initial OLS regressions can be seen in equation 1, while the Poisson regression is represented in equation 2.[28] 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.

$$\frac{f_{my}}{VMT_y} = \beta * \frac{enforce_{my}}{VMT_y} + m_m + weather'\alpha + \alpha_2 * unemp_{my} + u_{my} \tag{1}$$

[26]Importantly, when aggregate to the state level nearly all of the variation in employment and citations can be explained by the layoff. For instance, when regressing employment or citations on an indicator for the layoff, the layoff offers a partial R-squared of .97 and .74, respectively. At the state-level, utlizing the reduced form regressions with citations or employment generates estimates which are essentially identical to those generated when instrumenting employment or citations by the layoff indicator.

[27]We attempted to acquire a measure of income per capita or median household income, however 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.

[28]The subscipt m refers to month, subscript y refers to year, f_{my} is the outcome 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.

$$E(f_{my}|X_{my}) = \exp(\beta * enforce_{my} + m_m + weather'\alpha + \alpha_2 * unemp_{my}) \quad (2)$$

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. Panel A includes estimates for the roadways outside of city limits while Panel B presents estimates for highways both inside and outside of city limits. The first three columns provide estimates for 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 report) while columns 4-6 contain estimates for 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 and enforcement are notably larger than the elasticities estimated for non-fatal injuries and enforcement. This pattern supports speed increases as a likely mechanism as other research that has found increases in speed limits increase fatalities by a greater percentage than injuries (Rock, 1995). When the results are expanded to include roadways that are typically outside the geographic domain of the state police, the elasticities fall, particularly for fatalities. Other, more minor injuries continue to have a significant elasticity with police enforcement under more general weather conditions or jurisdictions.

We note that variation in 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. To the extent that this occurs, one can view the estimated elasticity between citations and injury rates as lower bounds for the true effect of additional citations on injuries. However, as noted earlier, over 75 percent of the variation in seasonally adjusted citations is explained by the layoff. Regardless of this potential bias for citations, for each of the injury types we find a negative elasticity between citations and injuries.[29]

Tables 4 and 5 present the OLS and Poisson regression results at the county level, respectively. Estimating regressions 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 average 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.

$$\frac{f_{cmy}}{VMTc_y} = \beta * \frac{enforce_{cmy}}{VMT_{cy}} + m_m + c_c + weather'\alpha + \alpha_2 * unemp_{cmy} + u_{my} \tag{3}$$

[29]State-level estimates that use the layoff as an instrument for citations are simlar, and are reported in Appendix Table 4.

$$E(f_{cmy}|X_{cmy}) = \exp(\beta * \ln enforce_{cmy} + m_m + c_c + weather'\alpha + \alpha_2 * unemp_{cmy} + \alpha_3 * \ln VMT_{cy}) \quad (4)$$

Once again, the majority of the OLS results are similar to those from the Poisson models. The county level estimates for the elasticity between citations and injuries are smaller than the state-level results. Heterogenous effects by counties and strategic layoffs on the part of the police might explain this finding. 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. Gibbons, Serrato and Urbancic (2010) discuss related issues in a more general context of average treatment effects (ATE) in fixed-effects estimation. 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. Thus if the police choose where to reduce enforcement (subject to mandated budget cuts) in order maximize the preservation of life, the citations estimates at the county level generated a weighted ATE based on variation, which is smaller than the state-level estimates.[30]

In summary, for each specification and aggregation we link decreases in enforcement

[30] Two ways to address the problem of recovering the ATE are to aggregate to the state level or to instrument county level citations with the state level number of police or budget shock. The instrumental variables approach isolates the variation in citations due to budget cuts rather than other factors, and generates a common amount of variation in citations across all counties. We have estimated these models and find that the number of police and the layoff are relevant instruments as shown in Appendix Table 6, and the instrumental variables estimates for citations are indeed larger as shown in appendix Tables 4 and 5. We note that in this case, the instrumental variables approach is only utililizing state-level variation in the level of enforcement, while adding additional noise. Since either approach only utilizes variation at the state level and the state level reduced form results are more precise, they are our preferred estimates.

to increases in injury rates. When the number of 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).

4.2 Difference-in-Difference Results, 2000-2005

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 shortcoming 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 traffic fatalities. We utilize the Fatal Analysis Reporting System (FARS) which maintains a database of the universe of accidents for the U.S. in which at least one fatality occurs.[31]

Control groups are often chosen geographically, as neighboring regions often experience similar, unobserved shocks other than the treatment of interest (see Card, 1990 and Card and Krueger, 1994, among many others). In Table 6 we compare the United States and

[31] Unfortunately, indicators for freeways and highways did not exist in the FARS data for the entire 1979-2005 window, as required in Section 4.3. 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. We obtain similar results when using an indicator for highway or freeway in Section 4.2.

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.[32] 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 method also validates Washington and Idaho as a reasonable counterfactual group.

In Table 7, we present estimates of difference-in-difference regressions utilizing the various counterfactual groups identified above. The estimated coefficients are scaled by the average

[32] 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.

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-in-difference equations take the form seen in equation 5.[33]

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

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

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 a counterfactual group (which appears to be the worst control group), the estimates are, in general, smaller but remain statistically significant.[34] In summary, when 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

[33]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. For example, the demographic and transportation variables are slowly evolving over time and are nearly perfectly colinear with state fixed effects. Near perfect multi-colinearity in controls substantially reduces the precision of estimates (Farrar and Glauber, 1967).

[34]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 uncertainty, 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.

in Oregon, the difference-in-difference estimates from 2000 to 2005 imply a fatality-trooper elasticity ranging from -0.34 to -0.57, depending on the weather conditions and jurisdiction.

4.3 Tri-State Panel Results, 1979-2005

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.[35] 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. The previous layoffs in the state police occurred as the funding for the state police was moved to be part of that state's general fund, whereas previously the funding had been provided by a guaranteed portion of gasoline taxes.

Similar controls for the unemployment rate, weather, and VMT are included in this analysis. Driver's license 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 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

[35] 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.

maximum speed limit and the presence of mandatory seat belt laws.[36] 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 that are common across the region in a given year.[37] 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.

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

$$+a_3 * sp\_limit_{smy} + \alpha_4 * seat\_belt_{smy} + a_5 * pop\_16_{sy} + a_6 * pop\_65_{sy} + u_{smy} \quad (6)$$

Table 8 contains the regression results for these additional findings, which have been scaled to be elasticities for ease of comparison with previous results.[38] The estimated coefficients suggest a 10 percent increase in troopers per VMT would reduce fatalities per VMT on all 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

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

[37] 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.

[38] 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). 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.

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.

4.4 Robustness

Although we have estimated a significant negative relationship between injuries and enforcement, it is important to examine if other observable trends that could have affected the number of fatalities and injuries coincided with the mass-layoff of state police. 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 decline 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 returned to the baseline levels in 2004 and 2005. Indeed, the fact that mandatory seat belt laws affect seat belt usage (Cohen and Einav, 2003) suggests that drivers respond to expected financial penalties for not wearing seat belts. 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. Note that these other potential channels do not bias any of our reduced form elasticities between police and injuries.[39]

Another important factor which determines traffic accidents is 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).[40] On average, pavement quality actually increased slightly over the 2000-2005 period. This is illustrated in Figure 7, which plots 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 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 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

[39] It would create bias if we wanted to estimate the effect of average speeds on fatality rates and instrumented average speeds with enforcement.

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

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 9, troopers employed and citations given on days with inclement weather 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.

4.5 Counterfactual Simulations

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 results 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.[41] The results of these estimates are

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

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 10, 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 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.[42] In addition, our analysis of the recent mass layoff of OSP suggests that additional police presence would likely prevent other injuries that are not accounted for

[42]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.

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.[43]

5 Conclusion

Police play a key role in enforcing speed limits on highways. We offer evidence concerning the causal effect of police on traffic fatalities and injuries, motivated by a mass layoff of the Oregon State Police due solely to budget cuts. Our results indicate that a decrease in enforcement, defined by either troopers employed or citations given, is associated with an increase in injuries and deaths on Oregon's 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 injuries/deaths due to traffic accidents. We find similar estimates when adjusting for common unobserved trends with various counterfactual groups or when expanding the time window of analysis. Utilizing our estimates, we find that there would have been 1,658 fewer deaths over the 1979-2005 time span if the OSP 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 from 1979-2005 at an average cost of $320,000 per life saved.

While our estimates suggests that drivers may respond to increases in enforcement with

[43] 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 on 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 to be laid off or would have elected to keep the police and cut other budgets.

safer driving, we note that nonlinearities or decreasing returns to enforcement imply that our estimates are most relevant for other state police departments that are facing large changes in enforcement. Due to the current recession, many states are currently considering layoffs or furloughs that are similar in scope to the 2003 layoff of the OSP. Some examples include Illinois[44] (which had plans to layoff 460 state police), Virginia[45] (recently laid off 104 state police), Michigan[46] (recently laid off 100 police), Connecticut (will soon lay off 56 police)[47] and many other states where layoffs remain a possibility including California, New York, Arizona, Minnesota, and Louisiana. Our findings suggest that these cuts in state police 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.[48] While harsher punishments can result in fewer crimes, this may in large part be due to incapacitation effects (see Owens, 2009). Given many offenses for unsafe driving do not revoke driving privileges, future work could investigate more fully the effect of fine increases on driver behavior and their usefulness as another variable in deterrence.

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

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

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

[47] http://www.nhregister.com/articles/2011/08/23/news/doc4e53e2c9cb0e5932514902.txt

[48] 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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6 Figures and Tables

Figure 1

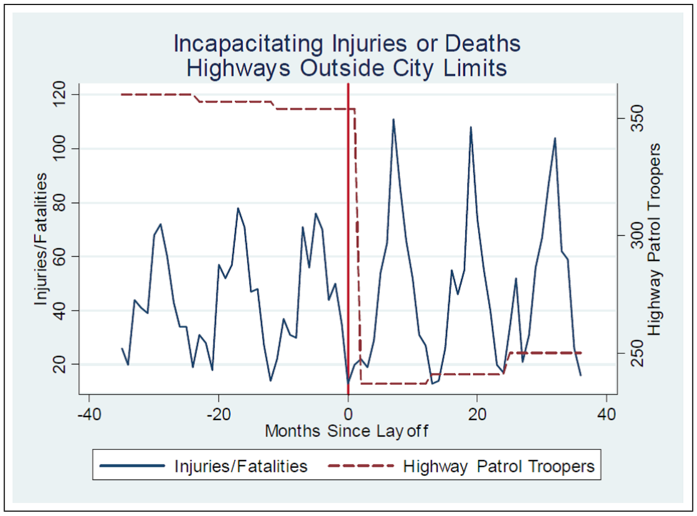

Figure 2

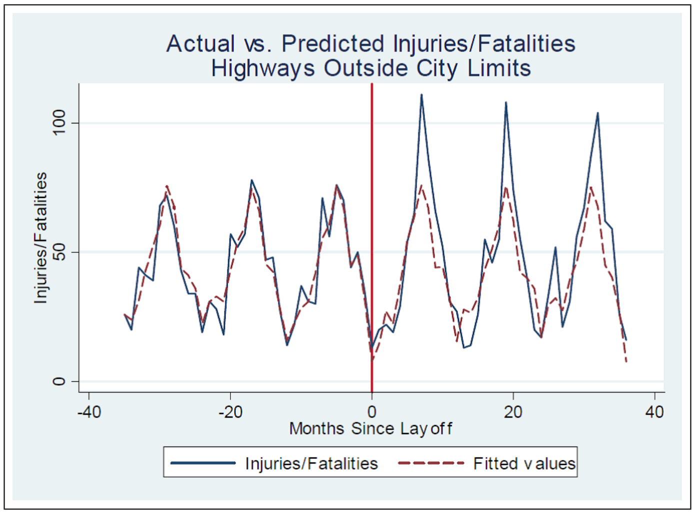

Figure 3

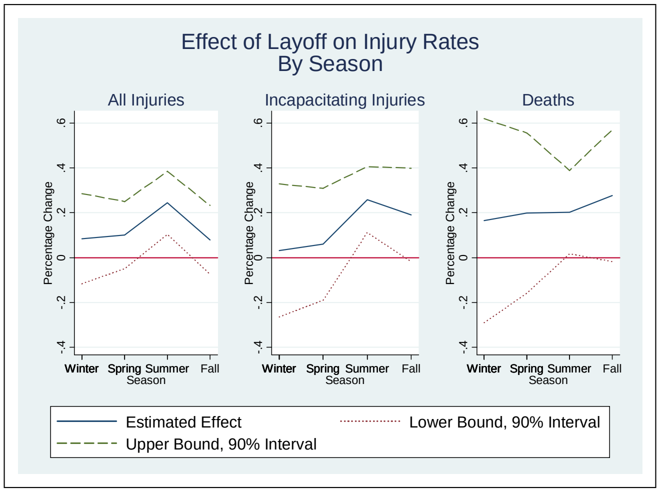

Figure 4

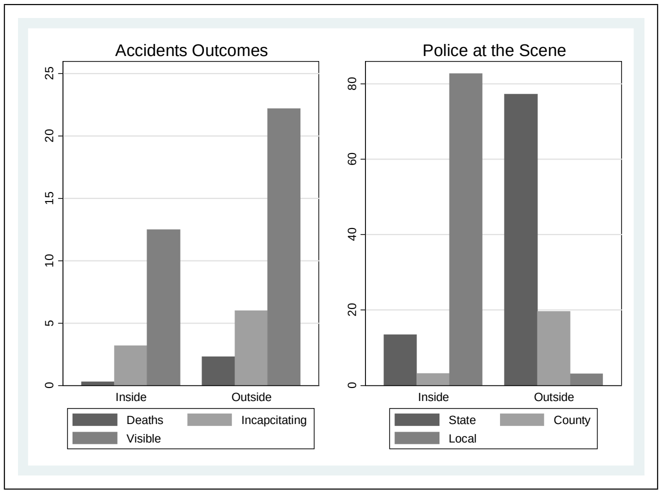

Figure 5

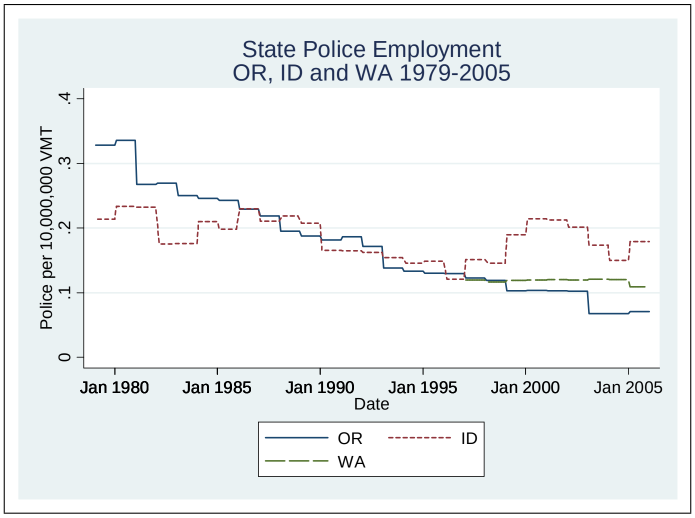

Figure 6

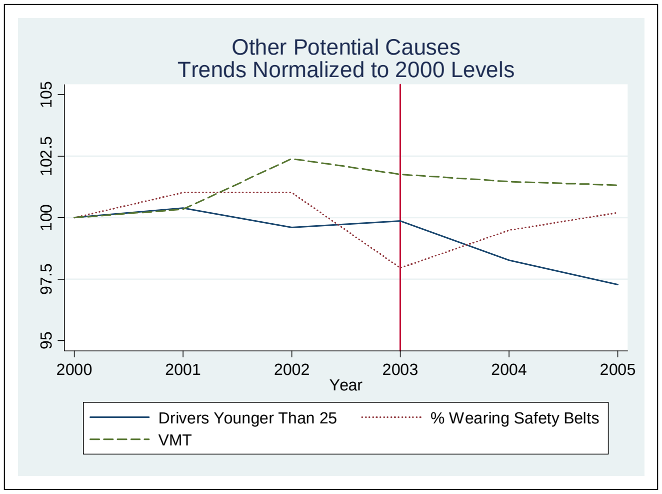

Figure 7

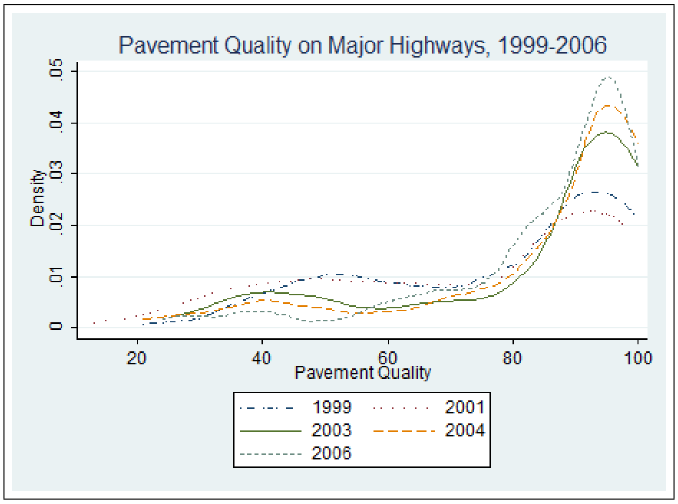

Figure 8: Oregon Fatality Counterfactuals

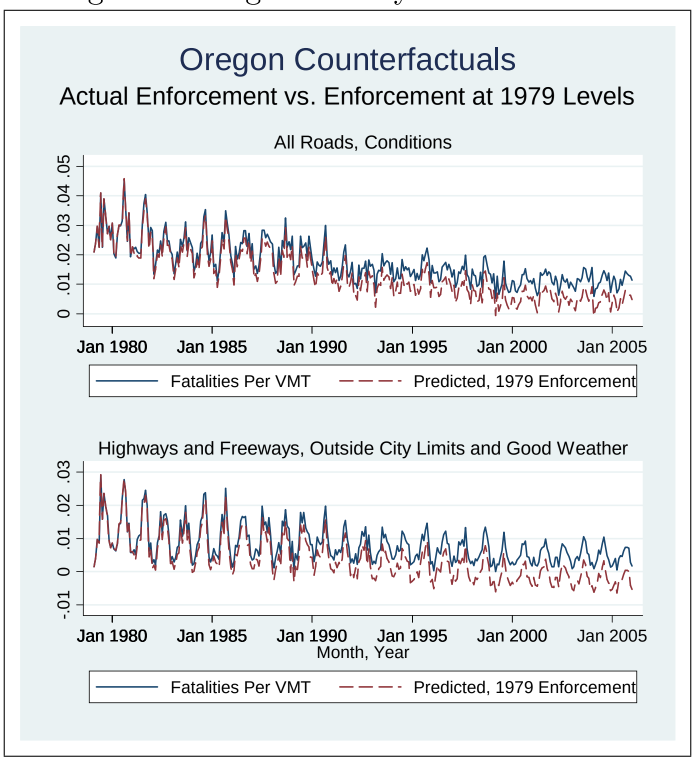

Table 1
Summary Statistics for Injuries Under Dry Weather Conditions

		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)[49]	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

[49] 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*		
Panel A: Highways Outside City Limits	*Deaths Per VMT*	*Incap. Per VMT*	*Visible Per VMT*	*Deaths Per VMT*	*Incap. Per VMT*	*Visible Per VMT*
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)
Panel B: 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*		
Panel A: Highways Outside City-Limits	*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)
Troopers	−0.43**	−0.36***	−0.32***	−0.32*	−0.23*	−0.26***
Elasticity	(0.20)	(0.13)	(0.11)	(0.19)	(0.13)	(0.09)
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)
Panel B: All Highways						
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)
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)
Citations	−0.20	−0.26***	−0.34***	−0.16	−0.14**	−0.26***
Elasticity	(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	*Incap.*	*Visible*	*Deaths*	*Incap.*	*Visible*
Panel A: Highways Outside City Limits	*Per VMT*	*Per VMT*	*Per VMT*	*Per VMT*	*Per VMT*	*Per VMT*
Citations Per VMT	$-.16^{*}$	$-.14^{**}$	$-.12^{***}$	$-.16^{**}$	$-.14^{***}$	$-.13^{***}$
Elasticity	(.09)	(.06)	(.04)	(.08)	(.05)	(.03)
Panel B: All Highways						
Citations Per VMT	$-.12$	$-.12^{**}$	$-.11^{***}$	$-.12$	$-.12^{**}$	$-.11^{***}$
Elasticity	(.09)	(.05)	(.03)	(.08)	(.05)	(.03)

This table reflects the elasticities between injury rates and enforcement. Each cell is a separate regression for the number of injuries. Controls include county fixed effects, month fixed effects, precipitation, snow, the unemployment rate, log of drivers over 65, and log of drivers under 25. Standard errors are clustered at the county level.
*, **, ***, indicate significance at 10, 5, and 1 percent levels, respectively. Standard errors are in parentheses.

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

	Dry Weather Conditions			*All Weather Conditions*		
Panel A: Highways Outside City Limits	*Deaths*	*Incap.*	*Visible*	*Deaths*	*Incap.*	*Visible*
Citations	$-.18^{*}$	$-.16^{***}$	$-.12^{***}$	$-.21^{**}$	$-.17^{***}$	$-.14^{**}$
Elasticity	(.09)	(.06)	(.04)	(.08)	(.05)	(.03)
Panel B: All Highways						
Citations	$-.14^{*}$	$-.12^{**}$	$-.09^{***}$	$-.15^{*}$	$-.12^{***}$	$-.09^{***}$
Elasticity	(.08)	(.05)	(.03)	(.08)	(.04)	(.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 county fixed effects, month fixed effects, precipitation, snow, the unemployment rate, log of drivers over 65, and log of drivers under 25. Standard errors are clustered at the county level and relax the assumption of mean-variance equality.
*, **, ***, indicate 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, we include the unemployment rate, maximum speed limit, and weather conditions as controls.
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: Hazardous Roads, Increase in Injuries at State Level

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

This table estimates the effect of the police layoff 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 10: Counterfactuals 1979-2005

	Fatalities 1979-2005	Troopers, 2005	Troopers FTE 1979-2005
Panel A :			
Actual Levels	14,662	250	11,862
Panel B:			
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 that would have resulted under various counterfactual scenarios.

7 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
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**

This table contains additional summary statistics for deaths, incapacitating injuries and visible injuries in other geographic settings, completing the summary statistics presented in Table 2.
*, **, ***, indicate significance at the 10, 5, and 1 percent levels, respectively

Appendix Table 3: Estimated Number of Injuries By Season

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

Notes: This table contains estimates for the increase in the number of injuries, estimated separately for each season used in Figure 3. The counts are determined for the number of injuries occurring on fair weather conditions on highways or freeways outside of city limits. Weather and unemployment conditions are included as controls. All models are estimated by OLS and use robust standard errors.

Appendix Table 4: Enforcement-Injury Elasticities
Oregon State-Level IV Estimates

	Dry Weather Conditions			*All Weather Conditions*		
	Deaths	*Incap.*	*Visible*	*Deaths*	*Incap.*	*Visible*
Panel B: Highways Outside City Limits	*Per VMT*	*Per VMT*	*Per VMT*	*Per VMT*	*Per VMT*	*Per VMT*
Citations Per VMT	−.30	−.26*	−.25**	−.24	−.26**	−.28***
Elasticity	(.21)	(.15)	(.11)	(.20)	(.13)	(.09)
Panel B: All Highways						
Citations Per VMT	−.19	−.26**	−.32***	−.14	−.24**	−.33***
Elasticity	(.19)	(.12)	(.11)	(.19)	(.11)	(.09)

This table reports the elasticities between injury rates and enforcement. Each cell is a separate IV regression for the number of injuries per VMT. Controls include month fixed effects, precipitation, snow, the unemployment rate,
All models are estimated using Two Stage Least Squares and use robust standard errors.
*, **, ***, indicate significance at 10, 5, and 1 percent levels, respectively.

Appendix Table 5: Enforcement-Injury Elasticities
Oregon County-Level IV Estimates

	Dry Weather Conditions			*All Weather Conditions*		
	Deaths	*Incap.*	*Visible*	*Deaths*	*Incap.*	*Visible*
Panel B: Highways Outside City Limits	*Per VMT*	*Per VMT*	*Per VMT*	*Per VMT*	*Per VMT*	*Per VMT*
Citations Per VMT	−.67	−.64*	−.45*	−.27	−.33	−.30
Elasticity	(.84)	(.36)	(.26)	(.73)	(.27)	(.19)
Panel B: All Highways						
Citations Per VMT	−.59	−.61*	−.42*	−.22	−.35	−.34*
Elasticity	(.78)	(.34)	(.23)	(.67)	(.27)	(.19)

This table reflects the elasticities between injury rates and enforcement. Each cell is a separate regression for the number of injuries per VMT. Controls include county fixed effects, month fixed effects, precipitation, snow, the unemployment rate, log of drivers over 65, and log of drivers under 25. Standard errors are clustered at the county level.

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

Appendix Table 6: First Stage For Appendix Tables 4 and 5
Citations Per VMT and Enforcement Levels

	State Level	*County Level*
Panel A:		
Troopers Per VMT Elasticity	1.02^{***}	$.97^{***}$
	(.07)	(.04)
	[177.8]	[535.6]
Panel B:		
After Layoff Semi-Elasticity	$-.38^{***}$	$-.36^{***}$
	(.03)	(.02)
	[188.5]	[266.9]

This table reflects the elasticities between citations and police staffing, defined by either troopers employed per VMT or an indicator for the layoff. Each cell is a separate regression. Controls include month fixed effects, precipitation, snow, the unemployment rate. The county level regression also include the log of drivers over 65, and log of drivers under 25 and county fixed effects. Standard errors are clustered at the county level.. *, **, ***, indicate significance at 10, 5, and 1 percent levels, respectively. Parentheses indicate standard errors while brackets contain f-statistics from testing the coefficient is equal to zero.

Appendix Table 7: 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

This table contains the summary statistics for the variables used in the tri-state regression analysis of section 4.4.

Appendix Table 8: 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 reporting pavement conditions as good or very good from the Oregon Department of Transportation.