

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

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Abstract

This paper focuses on 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. We also provide an alternative scenario in assessing the value of a statistical life, estimating the value of a life to be \$1.16 million.

Keywords: Enforcement, Traffic Safety, Police and Crime

JEL Classification: R41, K14, K42

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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, estimates put the damages up to \$230 billion per year (Blincoe et al. (2000)).¹ We investigate one of the most common (but less studied) policies intended to increase roadway safety: police enforcement. Likewise, speeding is one of the most common violations of the law² and one of the most frequent causes of fatalities³.

In this paper, we study the deterrence effects of highway patrol officers on roadway safety. Because of simultaneity problems in estimating the effect of police on crime, we identify the effect of a change in enforcement by studying a mass layoff (35 %) 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. The results suggest that enforcement can play a substantial role in driver behavior, consistent with a Becker (1968) model of crime where speeders respond to the probability of apprehension.

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. Empirical work on the impact of deterrence on crime has been provided by Levitt (1997) and

¹Although 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.

³See <http://www-nrd.nhtsa.dot.gov/Pubs/809915.PDF>

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 hired more police officers, presumably in an effort to reduce crime, and hence much work has been done to overcome this type of reverse causality. Although both papers establish some evidence of a negative relationship between enforcement and crime, for the most part the final estimates are imprecise.⁵ Replying to the comment on his 1997 work, Levitt (2002) suggests the budget of firefighters as another potential instrument to uncover the causal relationship between police and crime. Similar to this notion, we focus on the Oregon State Police, and the layoff of state troopers which resulted from a large and immediate budget cut.

For our estimation, we link records of traffic accidents on highways provided by the Oregon Department of Transportation, with detailed records of trooper employment and all issued citations—as maintained by the Oregon State Police. Section 2 provides a background of the political climate and discussion of the exogeneity of a massive legislatively mandated budget cut—due to *House Bill 5100* and the failure to pass *Measure 28* — that decreased the number of Oregon State Police by approximately 35 percent in 2003. Section 3 provides a model of police production and summarizes its predictions. Section 4 provides an empirical examination of the effects of enforcement levels on several measures of roadway safety. Section 5 discusses policy implications and estimates the voters’ implicit value of a statistical

⁴See also Ehrlich (1973).

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

life, while Section 6 concludes.

2 Background of the Budget Cut and Police Layoff

Oregon's state budget has been in turmoil ever since 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. Since 1997, funds for state agencies have continued to tighten until the beginning of 2002. At that time, 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, 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 Oregon State Police would "put staffing levels back to roughly the levels of the 1960s".⁶ 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 turnout⁷, *Measure 28* failed with 575,846 votes in favor and 676,312 voting against.

⁶"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.

⁷"Oregonians make a painful choice." Larry Leonard, *Oregon Magazine*, Jan. 31, 2003.

Table 1
Time-Line of Events

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

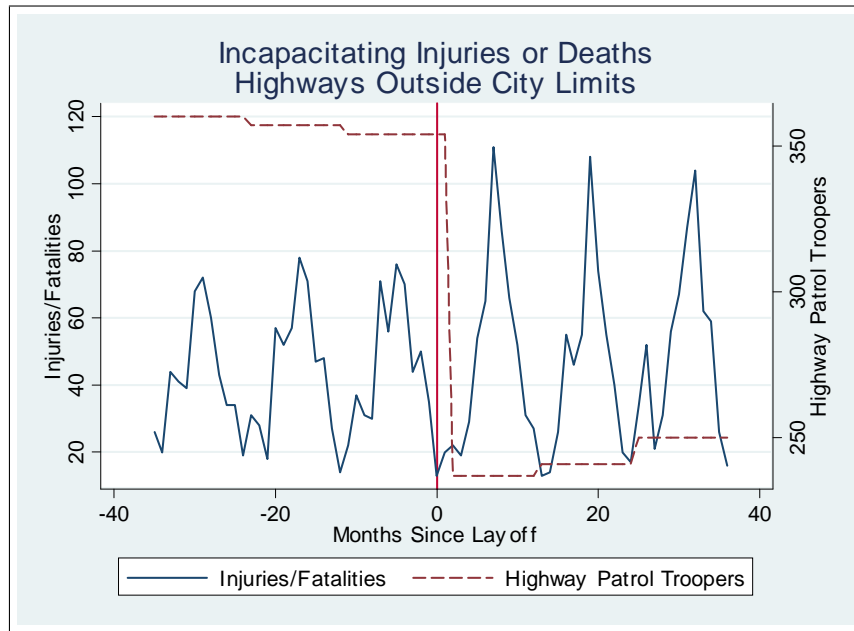
On February 1, 2003 the budget cuts implied by *House Bill 5100* went into effect and the Oregon State Police complied by laying off 117 out of 354 full-time roadway troopers.⁸ Layoffs were decided solely by seniority, with trooper specific performance playing no role. Several months after the reduction 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 enforcement on roadway safety.⁹ *Measure 30*, which was essentially a carbon copy of *Measure 28*, was introduced in 2004 and faced the same fate as its predecessor. *Figure 1A* contains trends for both the number of police employed and the number of incapacitating injuries or deaths (on highways outside of city limits and under fair weather conditions,

⁸Some other personnel who worked in the state crime lab were also let go. 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”.

⁹It 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.

regions and driving conditions likely to be most influenced by changes in police enforcement) for 2000-2005. The three years before and three years after the layoff are a 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.¹⁰

Figure 1A



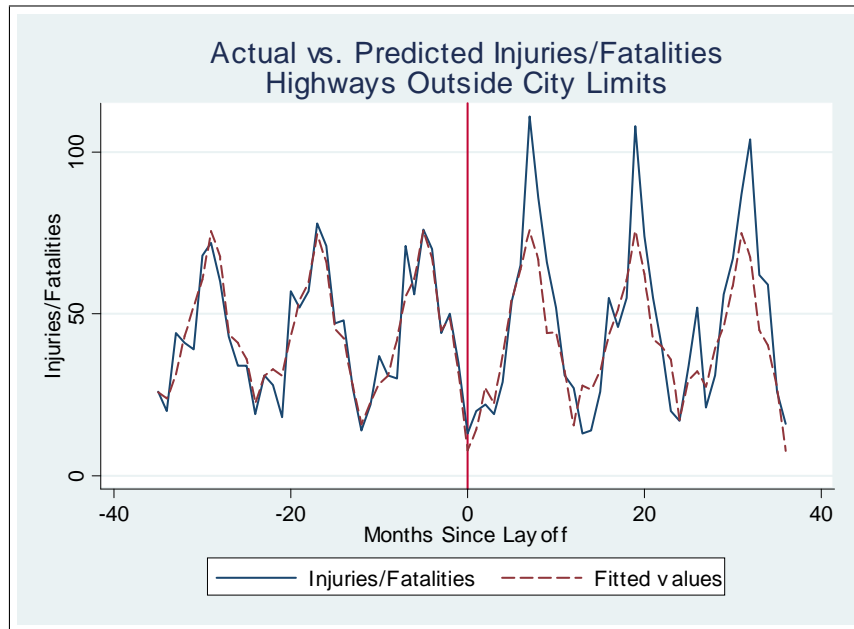
In the months after the layoff, the number of severe injuries and deaths is higher, most notably in the summer months.¹¹ This is not too surprising, as traffic in the summer months on highways and freeways is nearly double that of the rest of the year and also traffic flows increase by a few miles per hour in the summer. The impact on the summer months is shown particularly well in *Figure 1B*, which plots the actual number of injuries against the

¹⁰ 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.

¹¹ Days with severe weather where police have little effect on driver behavior do not experience the noticeable increase.

number of injuries predicted using weather and seasonality from the pre-layoff period.¹² In the summer months following the layoff, there has been an additional 15-30 incapacitating injuries or fatalities per month which is shown by the distance between the solid and dashed lines. We proceed to investigate a simple model of driver behavior and police production.

Figure 1B



3 Model

3.1 Driver Behavior

In the spirit of Becker (1968) and Ashenfelter and Greenstone (2004), an individual’s preferences towards speeding are affected by the net benefits of speeding and the likelihood of death or serious injury due to their choice of speed, as seen below:

¹²To 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.

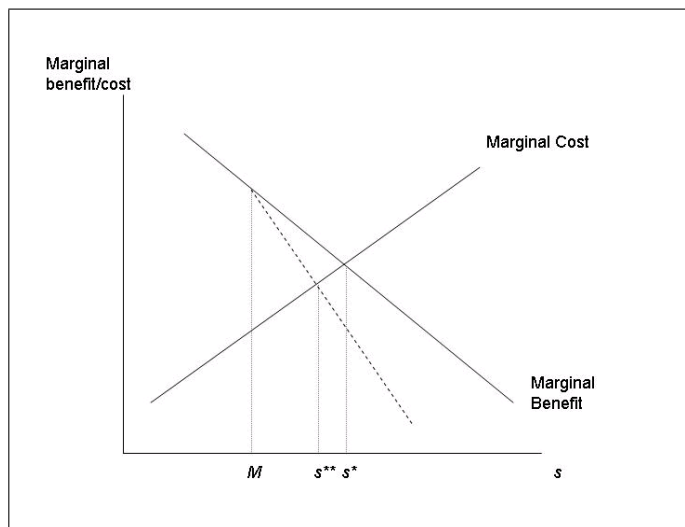
$$U(b(s) - e(s), f(s)), \tag{1}$$

where $b(s)$ is the benefit obtained from traveling at speed s , $e(s)$ is the level of enforcement at speed s and $f(s)$ is the likelihood of death or serious injury from traveling at speed s .¹³ Maximizing their utility, an individual would equate the marginal benefit to the marginal costs of increasing the speed traveled.

$$U_1[b'(s) - e'(s)] = -U_2f'(s) \tag{2}$$

This speed is depicted by s' in *Figure 2* below. This result is a slight modification to the model in Ashenfelter and Greenstone (2004).

Figure 2



If, however, the probability of being apprehended at any speed above a threshold increased, then we would observe a shift downward in the marginal benefit curve (denoted by

¹³Note that we have included the costs imposed on an individual by enforcement as a decrease in the benefit (i.e. in first term of the utility function). We could just as easily include this term as an addition to the cost (second) term of the utility function and achieved the same impact for the effect of enforcement.

the dotted line). The threshold M could be considered as either the posted speed limit or the understood limit (such as ten miles over the speed limit).¹⁴ As seen in *Figure 2*, increasing enforcement, or the expected monetary cost of speeding, decreases the individual’s optimal choice of travel speed, from s^* to s^{**} . A large decrease in the number of troopers employed, such as the police layoff in Oregon, would reduce the probability of a speeder’s apprehension. Due to the reduction in police employment, both the distance between officers and the number of shifts available would respectively increase and decrease.

The above micro model of driver behavior can be summarized by a speed supply function $S(E)$ where $S'(E) < 0$, where E refers to the total number of hours police enforce on roadways. A small scale field study of trooper deployment patterns by Sisiopiku and Patel (1999) finds that increased enforcement decreases speeds traveled, offering evidence in support of this assumption. In addition we assume that higher speeds increase accidents via the accident production function $\tilde{A}(S(E))$. This assumption is supported by Ashenfelter and Greenstone (2004), who find that higher speeds are associated with increases in fatal accidents. Combining this with the speed supply function implies we can summarize driver outcomes as

$$A(E), \tag{3}$$

where

$$A'(E) < 0. \tag{4}$$

¹⁴There is almost always a “cushion” above the speed limit where an individual is technically speeding but the enforcement authority does not issue a citation.

3.2 Police Production

With this model of driver behavior, we proceed to theoretically analyze the impact of changes in patrol services on accidents. The model for police production is simple: we assume that highway patrol officers allocate their time either to accident management or speed limit enforcement. In Oregon the revenue for tickets goes to a general fund for the state legislature. Therefore it is likely that the police are motivated more by safety concerns than raising revenue by ticketing. Each enforcement agency has a precinct level time constraint,

$$TH = \tau_a A + E \tag{5}$$

where T denotes the total patrol officers at the precinct, H the total hours worked per officer, τ_a is the time cost of attending an accident, A the total accidents attended, and E is the total time spent enforcing speed limits (either writing tickets or waiting to do so). If we solve 5 for E , we obtain

$$E = TH - \tau_a A, \tag{6}$$

with an additional constraint that enforcement must be non-negative. Even if citations have no influence on driver behavior, there would be increasing returns to scale in hiring more troopers (for non-negative citation ranges). Increasing returns exist because we have modeled law enforcement as a production process with a set up cost, where attending to all accidents is the set up cost that must be fulfilled prior to giving out citations.

Given equations 4 and 6, the effect of a unit change in trooper employment on enforcement

is

$$\frac{dE}{dT} = \frac{H}{1 + \tau_a A'} \quad (7)$$

This equation shows that if citations issued by troopers have no effect on driver behavior, then hiring an additional trooper yields an H increase in citations if all accidents have been cared for. If, on the other hand, trooper enforcement influences drivers, then we will observe spillovers via the number of accidents on the road. Combining equations 7 with 4, the effect of a change in trooper employment on accidents is

$$\frac{dA}{dT} = A' \frac{H}{1 + \tau_a A'} \quad (8)$$

With this result we can determine a prediction for equation 8. To wit, $\frac{dA}{dT} \leq 0$.¹⁵ We now proceed to estimate magnitudes for the effect of the layoff with this theoretical prediction in mind.

4 Results

4.1 Data

Data for accidents and injuries are obtained from the State-Wide Crash Data System collected and published by the Oregon Department of Transportation. For our present analysis, we restrict ourselves to the 2000-2005 time period, providing three years before the layoff

¹⁵This is provided $A(E)$ satisfies a lipschitz condition where $A'(E) > \frac{-1}{\tau_a}$.

and three after the layoffs.¹⁶ 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. 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.¹⁷ The Oregon State Police provided information on trooper employment and a complete record of all citations issued since January 1, 2000. Weather data was collected from the National Climatic Data Center Daily Cooperative files. Summary statistics for the aggregated monthly time series are provided below.¹⁸

¹⁶Though 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.

¹⁷Estimated property damages are not recorded in the database, else we would have constructed a consistent series for property damage accidents.

¹⁸These are the summary statistics for the time series of injuries from accidents with dry surface conditions.

Table 2
Summary Statistics

		Before Layoff.	After Layoff	t-test	t-test seasonally adjusted
<i>Outcomes</i>	Deaths	11.9	14.2	1.41	2.03**
	Incapacitating Injuries	42.8	48.6	1.04	1.84*
	Visible Injuries	164.2	183.7	0.98	1.80*
<i>Enforcement</i>	Citations	7,369.0	5,450.0	5.64	7.30
	Troopers	356.9	242.8	114.06	114.09
<i>Road Characteristics</i>	Yearly VMT (in Billions)	20.45	20.60	N/A	N/A
	Precipitation (inches)	2.88	3.11	0.40	1.06
	Snowfall (inches)	1.66	1.51	0.26	0.25
<i>Driver Characteristics</i>	Tot. Pop w/ License	2,807,435	2,900,125	N/A	N/A
	Pop<25 w/ License	432,992	426,377	N/A	N/A
Observations		37	35		

All Injuries, Citations, Prcp., and Snow are monthly rates, while the rest are annual averages

Even in the simple summary statistics, an increase in deaths, incapacitating injuries, and visible injuries is evident (and statistically significant¹⁹ when adjusting for seasonality). Along those lines, changes in VMT and driver characteristics are minimal, and the proportion of young drivers trend in a direction that would decrease injuries rather than increase them.

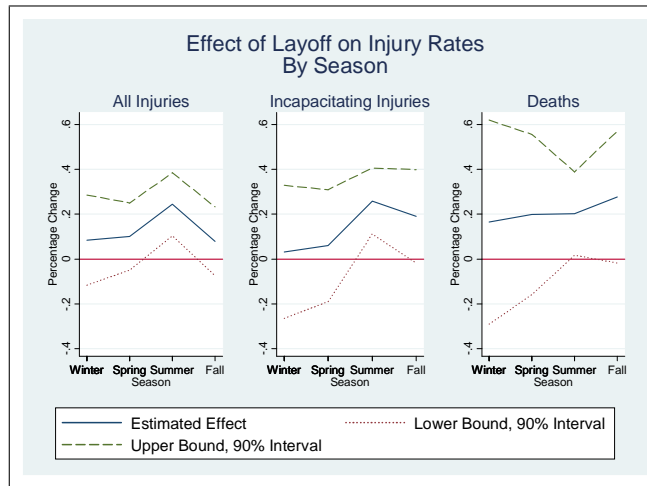
Figure 3 shows the percentage increase in the number of injuries separately by each season, as well as the confidence intervals. The percentage increase is estimated using linear regression models (scaled by the mean in pre-layoff period to yield a percentage effect), also controlling for precipitation.²⁰ The increase in the number of injuries is both the largest

¹⁹Although these simple t-tests do not adjust for serial correlation, adjusting for auto-correlation had no almost no effect on the significance, actually reducing the p-value.

²⁰The regression results which produced *Figure 3* are in Appendix Table 2.

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.

Figure 3



4.2 Econometric Models

Deaths and injuries follow an implicit count process, as they are bounded below by zero and occur only in integer values. A natural econometric model for estimation is the Poisson model. Although Negative-Binomial models are 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 misspecified – 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 that of equality between the conditional mean

and variance. We also include Negative-Binomial models for completeness.²¹ One important identifying assumption for the model is

$$E(Y|X) = \exp(X'\beta). \quad (9)$$

Because of this assumption about the nature of the conditional mean of Y , the estimated coefficients can be interpreted as semi-elasticities. Either count model is similar to assuming that $E(\ln y|x) = X'B^{22}$, but they allow for cases where the dependent variable takes on values of zero, which occurs in our sample when we disaggregate to county levels. 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.²³

In the analysis incapacitating injuries are defined as incapacitating or worse, hence deaths are included in this count. Similarly, deaths and incapacitating injuries are also considered visible injuries. This makes the results robust to spill-over effects in the severity of injuries.

The different injury rates refer to all injuries of a given type observed on highways or freeways

²¹In the county level panel analysis, dummy variables are included for counties rather than modifying the likelihood function, as suggested by both the theoretical discussion and monte carlo results of Allison and Waterman (2002). They find that incidental parameters are largely not a problem and that the fixed-effects Negative Binomial model suggested by Hausman et al. (1984) does not account for correlation between time constant unobservables and regressors.

²²We have also estimated OLS regressions with $\ln(injury_t) = \ln enforcement_t + X'_t\beta + u_t$, as the specification, obtaining nearly identical point estimates, and with slightly larger standard errors. We also ignore serial correlation in the presented results. However when correcting for autocorrelation in linear regression models the estimated standard errors are slightly smaller.

²³For all of our analysis, the injury measures are not normalized by the VMT, while this normalization has been used elsewhere in the literature (Ashenfelter and Greenstone (2004) for instance). Were one to normalize injuries for 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 $\ln \frac{injury_t}{vmt_t} = \alpha \ln(\frac{enforcement_t}{VMT_t}) + X'_t\beta$. Rewriting that expression, $\ln injury_t - \ln VMT_t = \alpha \ln enforcement_t - \alpha \ln VMT_t + X'_t\beta$, and which could be represented by a model where $\ln vmt_t$ is included as a regressor. This is done in the county regression specifications, but not the state level models because the VMT is only reported at the annual level.

outside of city-limits (where the presence of state troopers is almost the sole form of law enforcement) and under dry road conditions (where weather is not a confounding factor in accidents). Month fixed effects, precipitation and snow serve as controls for the all of the regressions estimated.²⁴ In the county-level panel regression, both the number of licensed drivers under 25 (in logs) and the number of drivers over 65 (in logs) and VMT (in logs) are also included, and multiplicative poisson fixed-effects adjustments are made for each county.²⁵

4.3 Estimates

The state level results suggest that the decrease in enforcement is associated with an increase in injuries. *Table 3* contains estimates of the elasticities between enforcement and injury rates estimated at the state level, with each cell representing a different regression with the same controls. We offer three measures of enforcement: an indicator variable for the post-layoff period, troopers employed and citations given. Death rates have the largest enforcement elasticities out of the injury measures. For a 10% decrease in troopers the fatality rate is estimated to rise by 4.7 percent, while incapacitating and visible injuries rise respectively by 3.3 and 2.7 percent. This is also consistent with previous studies which have found that speed and deaths have an elasticity nearly double that of speed and injuries (Rock 1995).

The results are very comparable across the Poisson and Negative Binomial specifications. The elasticities between enforcement are nearly identical for fatalities, not noticeable when rounding to two digits. The estimates for the negative binomial analysis are slightly smaller

²⁴These controls are included because in months where there is a lot of snow or rain, there would be less injuries and deaths on *dry* roads.

²⁵Poisson multiplicative fixed effects are identical to including dummy variables for each county, see Allison and Waterman (2002).

than those of the Poisson model for incapacitating and visible injuries, however one cannot reject the null that the coefficients from the two models are the same.

We note that citations could be considered endogenous. For instance, 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 measure of injuries we find a negative elasticity between enforcement and injuries.

Table 3: Enforcement-Injury Elasticities
State-Level Estimates

	<i>Poisson</i>			<i>Negative Binomial</i>		
	<i>Deaths</i>	<i>Incap.</i>	<i>Visible</i>	<i>Deaths</i>	<i>Incap.</i>	<i>Visible</i>
<i>After Layoff</i>	0.18*** (0.06)	0.13*** (0.04)	0.11*** (0.03)	0.17*** (0.06)	0.11** (0.05)	0.076** (0.03)
<i>Log Troopers</i>	-0.47*** (0.16)	-0.33*** (0.11)	-0.27*** (0.08)	-0.47*** (0.17)	-0.27*** (0.12)	-0.19** (0.09)
<i>Log Citations</i>	-0.25* (0.14)	-0.21** (0.10)	-0.18** (0.07)	-0.25 (0.16)	-0.19* (0.11)	-0.13* (0.07)

Notes: Each cell is a separate count regression for the number of injuries occurring under fair weather conditions. Controls include month fixed effects, precipitation, and snow. All Poisson regressions use a robust variance covariance matrix, relaxing the mean-variance-equality assumption.

*, **, ***, significant at 10, 5, and 1 percent levels, respectively

In the next set of results in *Table 4*, the relationship between injuries and enforcement is estimated at the county level. This allows us to include further controls that vary by county and year, such as vmt and the number of drivers younger than 25 or older than 65,

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 the trooper variable represents troopers employed at the state level and therefore is constant across counties.

The Poisson and Negative Binomial county-level results are similar in sign and magnitude, although some differences between the specifications arise. For both models the elasticities of enforcement with respect to fatalities increase relative to the state-level estimates, while the elasticities for incapacitating or visible injuries slightly decrease. The county level results are some whatever noiser than the state-level analysis, which could be partially due to including controls such as VMT and the number drivers less than 25 which vary only at the annual county level. The county level results are nearly identical to the state level results when the annually varying controls are ommitted.

The majority of the Negative Binomial results are slightly noisier than the Poisson model, although the point estimates are quite similar. The exception to this is the fatality rate, which has very similar estimates across both the Poisson and Negative Binomial models. The estimates for fatalities are significant for both the indicator for the layoff and also the state-wide level of troopers employed as measures of enforcement.

In addition, the county-level estimates for citations are smaller than the state-level. Endogeneous 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 they expect to least respond to enforcement reductions. This sort

of behavior would lead to regions with relatively larger changes in citations having relatively smaller changes in deaths or injuries. 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.

Table 4: Enforcement-Injury Elasticities
County Level Estimates

	<i>Poisson</i>			<i>Negative Binomial</i>		
	<i>Deaths</i>	<i>Incap.</i>	<i>Visible</i>	<i>Deaths</i>	<i>Incap.</i>	<i>Visible</i>
<i>After Layoff</i>	0.27** (0.13)	0.11 (0.07)	0.09* (0.05)	0.26** (0.12)	0.12 (0.084)	0.08 (0.05)
<i>Log Troopers</i>	-0.62* (0.33)	-0.24 (0.20)	-0.19 (0.12)	-0.62** (0.30)	-0.26 (0.21)	-0.15 (0.13)
<i>Log Citations</i>	-0.16* (0.09)	-0.16** (0.06)	-0.09** (0.03)	-0.13 (0.10)	-0.08 (0.06)	-0.05 (0.04)

Notes: Each cell is a separate count regression for the number of injuries occurring under fair weather conditions. Controls include month fixed effects, precipitation, and snow, VMT and the number of drivers over 65 or under 25. All Poisson regressions use a robust variance covariance matrix, relaxing the mean-variance-equality assumption.

*, **, ***, significant at 10, 5, and 1 percent levels, respectively

In summary, for both specifications and aggregations we link decreases in enforcement to increases in injury rates. The policy decisions were made at the state-level and hence those results will be used in the cost-benefit analysis in Section 5.

4.4 Robustness Checks

For robustness, exponential decay variables are also employed to estimate the effect of the layoff on injuries and deaths. 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 $\left(\exp(1) - \exp\left(\frac{1}{1-\lambda*mon_t}\right)\right) / (\exp(1) - 1)$, 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.²⁶ But given enough time for any value of $\lambda > 0$, eventually all drivers would become aware of the lack of troopers on the road. *Figure 4* shows how the rate of learning varies across values of λ .

Figure 4

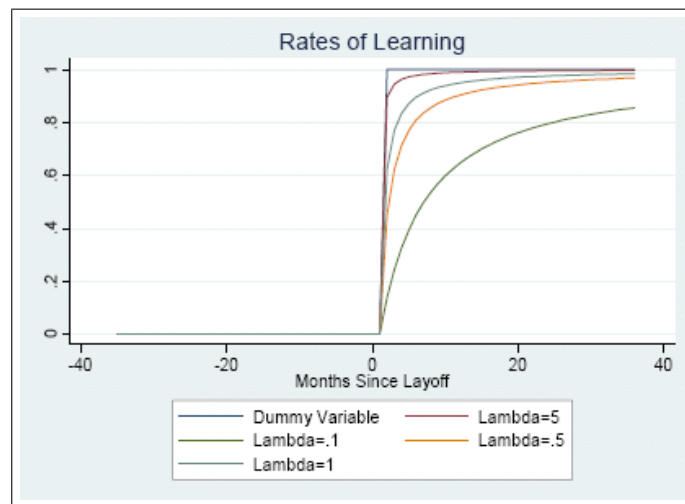


Table 5 contains the effect of the layoff measured by exponential decay variables with various rates of learning.²⁷ For the various rates of learning, the layoff is associated with a substantial increase in deaths or other injuries. As the rate of learning increases there is a minor decrease in the magnitude of the effect of the layoff on injury rates, possibly because

²⁶If $\lambda = 0$, then the variable would always be zero, as no one would ever learn about the layoff.

²⁷ If λ were estimated without restrictions, standard test statistics are no longer valid because of nuisance parameters not identified under the null. This problem was first identified by Davies (1977). 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.

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 17% increase in fatalities, similar to the effect of immediately learning estimated by the indicator variable in *Table 3*. Regardless of the value of λ specified²⁸, there is a positive association between the layoff and the number of injuries.

Table 5: Increase in Injuries, State-Level
Poisson Estimates

<i>Learning Rate</i>	<i>Deaths</i>	<i>Incapacitating</i>	<i>Visible</i>
$\lambda = .1$	0.19** (0.09)	0.20*** (0.06)	0.16*** (0.05)
$\lambda = .5$	0.18** (0.07)	0.15*** (0.05)	0.12*** (0.04)
$\lambda = 1$	0.18*** (0.07)	0.14*** (0.05)	0.12*** (0.04)
$\lambda = 5$	0.17*** (0.06)	0.13*** (0.04)	0.11*** (0.03)

Notes: Controls include month fixed effects, precipitation, and snow

All regressions use robust standard errors.

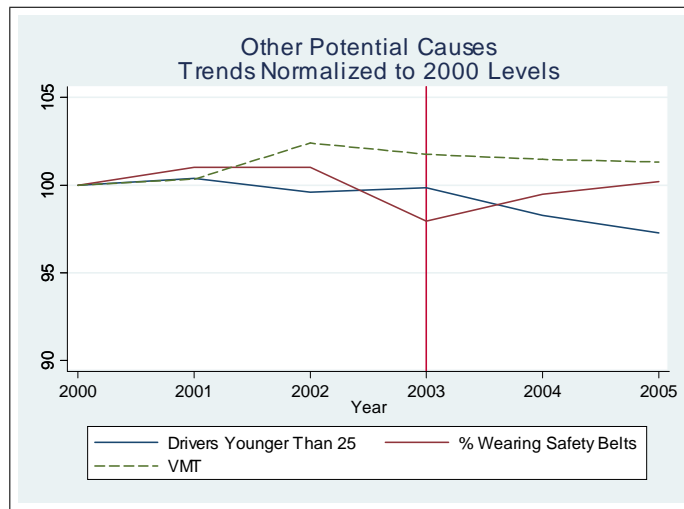
*, **, ***, significant at 10, 5, and 1 percent levels, respectively

Although we have estimated a significant negative relationship between injuries and enforcement, it is worth exploring how other factors could be playing a role. In *Figure 5*, 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 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

²⁸With the exception of 0, which would allow no learning.

in proportion). Although vehicle miles traveled (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 wearing seat belts did fall slightly in 2003, increasing back to its original levels in later years. However, this could be in part due to selection bias. People in accidents may lie about wearing seat belts, possibly for insurance purposes, however it becomes more difficult to lie when crashes are severe.

Figure 5



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. Thus 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 coincidentally with the layoff.²⁹ As shown in *Table 6*, troopers employed and citations show seemingly no relationship (both in magnitude and statistical significance) with injuries occurring under hazardous weather conditions. Similarly, exponential decay models that allow for either delayed (row 1) or immediate learning (row 2) are not associated with increases in injury rates. 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.

Table 6: Hazardous Roads, Increase in Injuries
State Level Estimates-Poisson

	<i>Deaths</i>	<i>Incapacitating</i>	<i>Visible</i>
$\lambda = 1$	0.01 (0.11)	-0.03 (0.08)	-0.005 (0.16)
<i>after - layoff</i> ($\lambda = \infty$)	-0.001 (0.26)	-0.02 (0.08)	0.004 (0.063)
<i>Log Troopers</i>	0.02 (0.26)	0.10 (0.20)	0.03 (0.16)
<i>Log Citations</i>	0.03 (0.25)	0.011 (0.16)	-0.17 (0.11)

Notes: Controls include month fixed effects, precipitation, and snow

All regressions use robust standard errors.

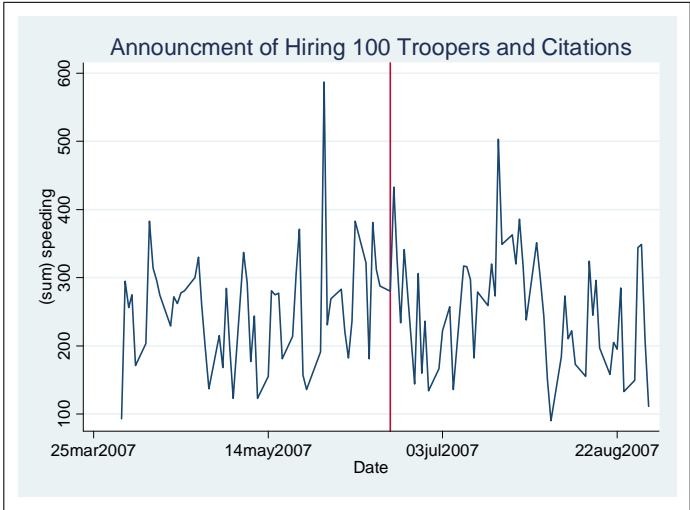
*, **, ***, significant at 10, 5, and 1 percent levels, respectively

One other factor that could play a role is police effort, more commonly known as “the blue flu”. In Oregon, public sector strikes are outlawed just as in many other states. As Mas (2006) shows, reference points can have strong effects on police effort. Although Mas (2006)

²⁹In some sense we could view this group as a “control group” were we to perform a difference-in-difference estimation. The estimates on hazardous road conditions are close enough to zero in sign and magnitude, such a strategy would not visibly affect the previous results.

addressed the effect of arbitration losses regarding wages on police effort in New Jersey, public sector unions often consider employment in their objective function (see Freeman (1986)). Similar to Lee and Rupp (2007) who find some evidence that effort declines with the announcement of wage reductions, we study the announcement of the hiring of 100 new troopers on police effort. In *Figure 6* we plot the time series of citations, which we take as a proxy for effort, with the news of the hiring of 100 new troopers – announced on June 18, 2007. There does not appear to be a systematic change in citations given. This relates to the effect of police hiring on morale, rather than the effect of a prolonged layoff. Given that consideration, even if “blue flu” effects are present, they would imply the results of this paper are specific to the type and size of layoff observed in Oregon. With that in mind, the elasticities between troopers and injuries estimated in this paper would perhaps be larger than those for a small change in police employment.

Figure 6



4.5 Cost-Benefit Analysis

Our estimates of the effect of police enforcement on roadway safety are based on the increases in fatalities, incapacitating injuries, and visible injuries that occurred as a result of the police layoffs in response to budget cuts when *Measure 28* failed. The layoff and budget cuts can have other effects on the lives of Oregonians. First, and in the spirit of Ashenfelter and Greenstone (2004), the value of a statistical life can be revealed from the analysis. Estimating the value of an Oregonian’s statistical life requires knowledge of the amount of time saved as a result of faster speeds due to less enforcement. Information on average speeds traveled is gathered from the ODOT’s speed collecting cameras, which are located on highways and interstates throughout the state.³⁰ The data from the automated recorders forms an unbalanced panel of daily observations for 2002-2005 (the stations were not in operation prior to 2002). One limitation is that the speed recorders are located relatively close to city-limits, and may underestimate the true speed increases in rural areas³¹ (thereby also underestimating the value of time saved). We include only days without precipitation or snow to minimize the effect of outliers due to poor weather.³² *Table 7* contains estimates of the increase in average speed that resulted from the layoffs, accounting for traffic flows and time-constant heterogeneity across speed collection locations via station fixed effects.

³⁰We were not able to use all speed monitoring devices in this analysis because several measuring devices were either recalibrated and/or used intermittently during the period of interest. We have restricted our attention to 8 devices that suffered very little from either of these issues.

³¹Given the increase in fatalities observed, Rock (1995) estimates would suggest average speeds should have increased by 0.8 to 1.0 miles an hour.

³²Several large snow storms occurred during the winter months of the years following the layoff. Focusing on days without precipitation negates the effect that these outliers would have on measured increases in speed after the layoff. In addition, we have focused on injury rates for days with good weather, hence it is natural to consider speeds under dry weather.

Table 7: Effect of Layoff on Average Speed

	All Months		Summer Months Only	
	I	II	III	IV
<i>After – Layoff</i>	0.40*** (0.029)	0.38*** (.027)	0.40*** (.036)	0.41*** (0.034)
<i>Log Total Cars</i>	.	1.42*** (0.064)	.	1.02*** (0.087)
<i>Mean (Before Layoff)</i>	69.24	69.24	70.25	70.25
<i>Station Fixed Effects</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>

All Regression use Huber-White Robust Standard Errors

*, **, ***, significant at 10, 5, and 1 percent levels, respectively

Estimating the increase in speeds – relatively stable at 0.4 miles per hour – is the first step in estimating the amount of travel time saved. The VMT for interstates and highways outside of city limits (approximately 12 billion vehicle miles³³) is multiplied by the increase in speed to determine the total number of hours saved. Finally, the average hourly wage (a proxy for the value of time) in Oregon is estimated to be \$15.03³⁴ from the 2005 *American Community Survey*. Multiplying the number of hours saved by the average hourly wage yields the "Value of Time Saved" in *Table 8*. The amount of money that drivers saved by paying less traffic citations is also reported in *Table 8*.³⁵ The monetary value of not passing *Measure 28* (lower taxes due to reduced police budget, fewer citations paid, and value of time saved) is approximately \$28.16 million. Dividing the total benefits (\$28.16 million) by the average annual increase in fatalities (≈ 24 lives as estimated by *Table 3*), the value of an Oregonian statistical life is approximately \$1.17 million. This value is based on the revealed preferences

³³Source: *Oregon Mileage Report and Transportation Volume, Table IV*.

³⁴Before-taxes, hence likely an upper bound.

³⁵The amount of money saved by not paying traffic fines was determined by comparing pre-layoff to post-layoff citations levels and multiplying citations by their expected fine, which is described in Oregon Statute *HB 2759C*.

of the voters concerning the budget cuts, assuming that they were conscientious of the effects the layoffs would have on the fiscal budget, citations³⁶, and roadway safety.

Table 8: Cost Benefit Analysis

	Scenario I		Scenario II		Scenario III	
	Costs	Benefits	Costs	Benefits	Costs	Benefits
Cost of Visible Injuries	7.52	.	1.60	.	7.52	.
Cost of Incapacitating Injuries	10.94	.	2.67	.	10.94	.
Cost of Deaths	45.57	.	13.16	.	160.35	.
Police Budget	.	8.63	.	8.63	.	8.63
Citations Paid	.	4.25	.	4.25	.	4.25
Value of Time Saved	.	15.28	.	15.28	.	15.28
Total	56.51	28.16	15.83	28.16	171.29	28.16

All estimates have been adjusted for inflation and are reported in 2005 millions of dollars.

Using the budget cuts and corresponding layoffs to estimate the value of a statistical life assumes that voters knew all of the effects of the layoff and that they voted to reveal their preferences regarding the trade-off between speed and risk of life. However, *Measure 28* required voters to express their preferences regarding a set of budget cuts – rather than just the cut to the police force, complicating the decision and trade-offs the voters faced. A cost-benefit analysis estimates how voters would cast their ballots were they allowed to line-item veto portions of *Measure 28*. The benefits, discussed in the above paragraph, are estimated to be approximately \$28.16 million. Three scenarios of the cost of the layoffs are presented in *Table 8*. *Scenario I* uses the average cost of visible and incapacitating injuries, as determined by the Federal Highway Administration’s *Crash Cost Comparisons*, and the average VSL from Ashenfelter and Greenstone (2004) to compute the total costs of the

³⁶As 13 percent of the drivers are from other states, it may be reasonable to multiply the reduction in citations by 0.87, as voters from Oregon may have no benefits to reducing citations paid by drivers from other states.

layoffs. *Scenario II* uses the minimum cost of visible injuries, incapacitating injuries, and deaths in computing the total cost of the layoffs whereas *Scenario III* uses the maximum cost of visible and incapacitating injuries as well as the Oregon specific estimate of the VSL from Ashenfelter and Greenstone (2004).³⁷ Of the scenarios considered above, only in the scenario where the minimum values of the cost of injuries are considered, it is privately beneficial on the voters' end for the police to be laid off. In the other scenarios, the benefits of cutting the state police budget exceed the costs – from the perspective of the voters. Perhaps this isn't too surprising, as the legislature recently approved the re-hiring of 100 state troopers.

5 Conclusion

Police have long been a tool for enforcing speed limits on highways. We offer evidence of 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 by either troopers employed or citations given, is associated with an increase in injuries and deaths on Oregon highways. Our preferred estimates for the estimated elasticities between enforcement and injuries range between -0.1 and -0.4 for the most part, suggesting a non-trivial association between enforcement and safety. Other potential explanations such as changes in the age distribution or increased travel are not consistent with the observed trends. Comparing the decrease in travel time, reductions in tickets, and reduced budgets to the loss of life yields \$1.17 million as the implicit value of a statistical life.

Estimation of an implicit value of a human life assumes that voters were aware of all

³⁷We note that estimates of the VSL from Ashenfelter and Greenstone (2004) are upper bounds on VSL because they do not take the cost of injuries, property damage, accidents, etc. into account.

of the costs and benefits associated with the layoff. It is unknown to what extent voters were informed of the potential consequences of the enforcement reductions. In addition, the decision of the voters was complicated because *Measure 28* required voters to express their preferences for a menu of budget cuts, rather than just the police layoff. With this in mind, a cost benefit analysis provides insights into how voters might have cast their ballots if they are allowed to line item veto. Our cost benefit analysis reveals that for both moderate and high monetary amounts attached to injuries and deaths, the costs of the police budget cut exceeds the benefits, from the voters' perspective. We conclude by noting that perhaps this is not too surprising given the recent budget increases in 2007 to fund the hiring of an additional 100 troopers, scheduled throughout 2007, 2008 and 2009.

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6 Appendix

Although the layoff was exogenous to the state police and it coincides with an increase in injuries and deaths on roadways since the layoff, omitted variable bias could still play a role. The Oregon State Police was not the only agency to experience budget cuts. If for example (this is not the case), road maintenance was reduced due to budget cuts, we would be attributing the effect of roadway quality to enforcement. *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.

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

The other agencies that experienced budget reductions do not appear to be directly linked to roadway safety, suggesting that 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 2: Effect on 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. 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 controls. All models are estimated by OLS and use robust standard errors.