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

By Gregory DeAngelo and Benjamin Hansen*

Simultaneity complicates the estimation of the causal effect of police on crime. We overcome this obstacle by focusing on a mass layoff of Oregon State Police in February of 2003. Due solely to budget cuts, 35 percent of the roadway troopers were laid off, which dramatically reduced citations. The subsequent decrease in enforcement is associated with a significant increase in injuries and fatalities. The effects are similar using control groups chosen either geographically or through data-driven methods. Our estimates suggest that a highway fatality can be prevented with $309,000 of expenditures on state police. (*JEL* H76, K42, R41)

Recent estimates suggest that between 750,000 and 1,118,000 lives are lost worldwide annually due to motor vehicle accidents (Peden et al. 2004). Translating the costs of accidents into dollars, damages from accidents have been placed at $230 billion per year in the United States alone (Blincoe et al. 2002).[1] One of the most common (but less studied) policies intended to increase roadway safety and reduce deaths is police enforcement of maximum speed limits and other traffic laws. Furthermore, while speeding might seem like a minor crime, traffic fatalities due to speeding in the United States have approached the number of murders in recent years. Indeed, surveys suggest that 68 percent of drivers report fear and grave concern regarding the aggressive driving and speeding of other motorists, while only 38 percent of individuals report being afraid to walk in their neighborhood due to concerns about crime.[2] The current study estimates the causal effect of highway patrol officers on traffic fatalities and serious injuries by exploiting a mass layoff of state police in Oregon that reduced the likelihood that speeders were apprehended by police.

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[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 Karaca-Mandic 2006).

[2] National Survey of Speeding and Other Unsafe Driving Actions 2002 and Gallup Poll Social Series: Crime October 2011.

Punishments and enforcement have long been used by the government as options to reduce crime. This has been supported by various economic models of crime including Becker (1968), Polinsky and Shavell (1979), İmrohoroğlu, Merlo, and Rupert (2004), and Lee and McCrary (2009) that predict that increases in expected costs reduce engagement in criminal activity. However, estimating the degree to which punishments and apprehension probabilities deter crime poses a difficult problem due to simultaneity. Regions with higher crime rates tend to have more enforcement, creating omitted variable bias in estimates that rely solely on cross-sectional variation (Levitt and Miles 2006). To overcome this type of reverse causality, recent research has relied on quasi (or natural) experiments.[3] Notable examples include the addition of a third referee in professional basketball (McCormick and Tollison 1984), the hiring of police due to electoral cycles (Levitt 1997), and the provision of federal grants allowing local agencies to employ additional police beyond what local resources would allow (Evans and Owens 2007).[4]

Our study contributes to the aforementioned quasi-experimental studies by utilizing a large layoff of police.[5,6] Importantly, the effect of large-scale decreases in police may differ from hiring due to the experience of officers being laid off and media coverage of the layoff.[7] Moreover, potential speeders could respond differently to police than the subsample of the population at risk of committing violent or property crimes. Lastly, in other settings the effect of police on crime can be driven both by deterrence and by incapacitation (as criminals are incapacitated when arrested by police). Any effects we identify are likely driven nearly entirely by deterrence, as speeders are almost universally returned to the road immediately following a citation. In this sense, we more directly test the deterrent effect of police predicted by Becker's economic model of crime than many previous studies in this area.[8]

Key for our identification, the budget shortfall causing the layoff resulted from a property tax regime change that took place in the previous decade. This offers a unique quasi-experiment for studying the effects of policing on traffic fatalities and injuries, as recent police layoffs due to recessions would likely be confounded with the severity of the economic downturn. Likewise, the recent budget shortfalls in many

[3] Chalfin and McCrary (2012) also highlight the role that measurement error in the number police can play in estimating the causal effect of police on crime.

[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 smaller and less precise after the corrections. The estimates of McCormick and Tollinson (1984) remained significant at the 10 percent level after the necessary corrections.

[5] Our results complement recent research by Makowsky and Stratman (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 (2011) have a follow-up study that takes advantage of the endogenous reponse of local police to offset the exogenous decrease in local resources.

[6] Research from Clark (1969), Makinen and Takala (1980), and Pfuhl (1983) regarding police strikes found mixed evidence relating police to crime, while Mas (2006) found reductions in police enforcement in contract negotiations, commonly referred to as the "blue flu," led to increases in crime.

[7] DeAngelo and Owens (2012) provide evidence that less experienced police are less productive, in terms of citations issued.

[8] Even the few drivers receiving a suspended license are not truly incapacitated. Rather, they are subject to higher punishments if caught speeding or driving again.

states highlights the relevance of our findings for policy makers that are considering similar reductions to their highway patrols. We find that the reduction in state police employment is associated with significant increases in injuries and fatalities, respectively ranging from a 12 to 29 percent depending on the type of injury and the weather conditions. Furthermore, we find similar results utilizing counterfactual groups either chosen geographically or based on the synthetic control approach of Abadie, Diamond and Hainmueller (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 enforcement reductions in Oregon dating back to the 1980s, we find that Oregon would have experienced 2,302 fewer fatalities from 1979–2005 if the number of state police had been maintained at their 1979 levels.

The remainder of our paper is organized as follows. Section I provides a background of the political climate and discussion of the exogeneity of the legislatively mandated budget cut in Oregon that decreased the number of OSP troopers by approximately 35 percent in 2003. Section II reviews the data sources and econometric specifications, while Section III provides an empirical examination of the effects of the layoff on enforcement levels, traffic fatalities, and nonfatal injuries as well as discussing counterfactual simulations based on our estimates. Section IV concludes.

I. 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.[9] The public-sponsored initiative limited property tax rates and their growth in a manner similar to Proposition 13 of California. In consequence, funds for state agencies tightened during the 1997–2002 period. In early 2002, it became clear to the Oregon state government that unless taxes were raised, budget cuts would become necessary. Measure 28, which allowed for an increase in the state income tax to cover budget deficits, was put to a vote of the people on January 28, 2003.

In the weeks prior to the vote, media attention brought the impending budget crisis to the public spotlight. Coverage from *The Seattle Times* specifically highlighted that the budget cuts for the OSP would "put staffing levels back to roughly the levels of the 1960s."[10] 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 (the list of approved cuts is listed in Table A1). After the votes

[9] Cabral and Hoxby (2012) provide an excellent discussion of mechanisms often underlying property tax revolts like the 1997 revolt in Oregon.

[10] Hal Bernton, "A cutting edge Oregon wishes it wasn't on," *The Seattle Times,* December 29, 2002, accessed November 12, 2008, http://community.seattletimes.nwsource.com/archive/?date=20021229&slug=oregon29m. There was also publicity put out by the Oregon State Police: Rebecca Nolan, "State police already preparing for big cuts," *The Register-Guard*, December 29, 2002, A1. Diane Dietz, "Troopers look for jobs elsewhere," *The Register-Guard,* January 17, 2003, A1.

Timeline 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, maximum fines increase (15 percent)
January 1, 2006	Increase of fine, suspension, >100 mph
January 20, 2006	Hiring of 18 FTE troopers
June 18, 2007	Senate Bill 5533, 100 troopers hired

were counted in a record turnout, Measure 28 failed with 575,846 votes in favor and 676,312 voting against.[11]

On February 1, 2003 the budget cuts implied by House Bill 5100 went into effect and the OSP complied by laying off 117 out of 354 full-time roadway troopers.[12] 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 traffic fatalities.[13] 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 that may be most influenced by changes in state police enforcement) for 2000–2005. The three years before and three years after the layoff are a period when other policies such as graduated teenage licensing and drunk driving laws are constant. Likewise, the troopers were largely not yet re-hired (which began in 2006 and 2007), isolating more clearly the potential impact of the police layoff on injury rates.[14] Also over the 2000–2005 time period, the national fatality rate per VMT traveled was relatively flat, falling 3.7 percent.[15]

Two sources provide information regarding the association between the timing of the layoff and speeds traveled. From administrative records on speeding citations given out by the OSP, we calculate the difference between the posted and recorded speed, both measuring speed at the daily level. The Oregon Department of Transportation maintains automated speed recorders for several locations on

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

[12] Some 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 trooper." 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."

[13] It may also take much longer for drivers to learn about when fines increase relative to enforcement changes. Drivers may learn about fine increases when they or someone they know receives a ticket and this type of learning is evident in recent research by Hansen (2012). However, drivers may learn about enforcement changes by noticing the lack or presence of police on the road.

[14] 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.

[15] Author's calculations.

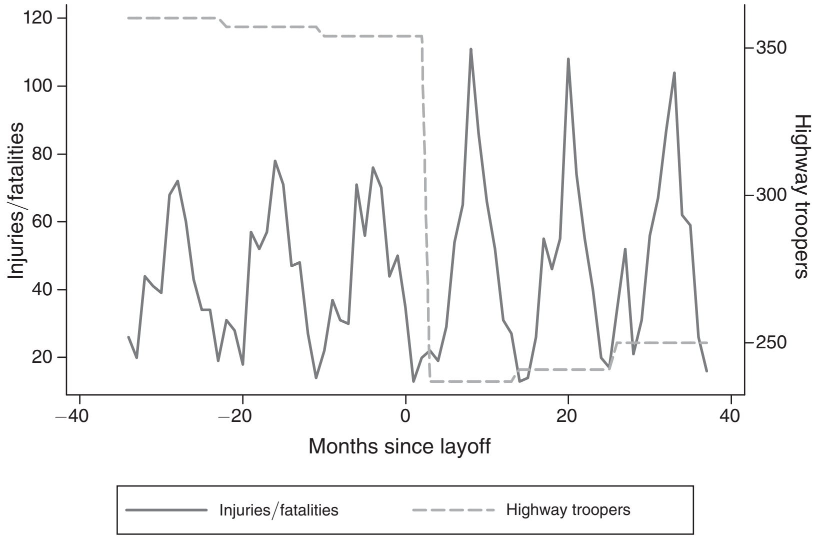

Figure 1. Incapacitating Injuries or Deaths Outside of City Limits

Oregon Highways.[16] As shown in Figure 2, both the citation speeds and automated recorders depict an increase in observed speeds that coincides with the timing of the OSP layoff. The automated speed recorders suggest average speeds increased by 0.5 miles per hour (mph), on average. One caveat is that the automated recorders represent a nonrandom subset of roadway segments focused on freeways which tend to be closer to urban regions, and thus might understate the true increase speeds on all Oregon highways. Regardless, this ancillary evidence is consistent with the hypothesis that increased speeding and reckless driving is a plausible channel for the increase in fatalities and injuries evident in Figure 1. It could be the case that unobserved factors that also coincided with the layoff could be driving both the shift in driver speeds traveled and injuries. With that in mind, the next section discusses our empirical strategy, which overcomes the potential effects of omitted variables by taking advantage of various counterfactual groups that were subject to similar underlying trends affecting traffic fatalities but did not experience a similar reduction in law enforcement on highways.

II. Data Sources and Empirical Models

In order to analyze the effect of the Oregon layoff, we combine together several different data sources. The Law Enforcement Officer Killed in Action (LEOKA) report of the Uniform Crime Reports, Census of Law Enforcement, and administrative records from Oregon, Idaho, and Washington provide information on state police enforcement levels. The OSP also provided administrative records for every citation given by the

[16] Unfortunately, the automated recorders only began recording data in 2002, and their number of automated recorders has increased steadily in every year since 2002. With those restrictions in mind, we utilized five automated speed counters that began recording data in 2002 that also cover the years 2003, 2004, and 2005 on a consistent basis.

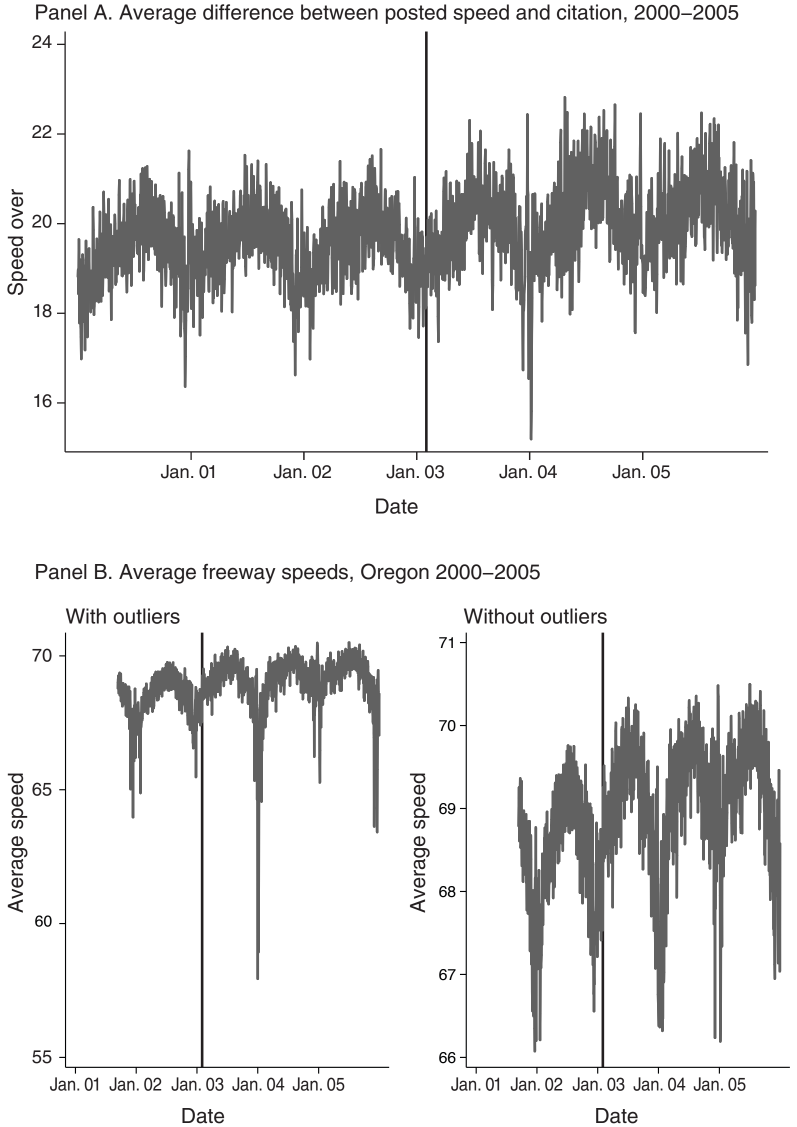

Figure 2

OSP from 2000–2005. The Fatal Analysis Reporting System (FARS) of the National Highway Traffic and Safety Administration (NHTSA) provide data on the timing and characteristics of the universe of fatal car accidents in United States, and NHTSA also provided data on vehicle miles traveled (VMT). In addition, the Departments of Transportation of Oregon, Idaho, and Washington provided information on nonfatal crashes. The Oregon Department of Transportation provided speeds traveled through automated recorders, and pavement quality based on annual administrative reports. We utilize the Daily Cooperative files of the National Climatic Data Center to construct weather variables, while monthly unemployment data are from the US Census Bureau. We also use American Community Survey (ACS) and Surveillance Epidemiology and

Table 1—Summary Statistics

	Mean (SD)	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*
Speed (automated)	68.85 (0.76)	68.4	69.1	15.44***	13.33***
Speed differences (citations)	19.74 (0.94)	19.5	20.0	14.77***	15.75***
Enforcement					
Citations	6,411.8 (1,726.2)	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 (4,774)	432,992	426,377	N/A	N/A
Observations		37	35		

Note: This table compares several outcomes and characteristics of Oregon roads before and after the layoff.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

End Results (SEER) to construct demographic variables, commuting characteristics, and public transportation utilization for each state.

Summary statistics for Oregon before and after the layoff in Table 1 reveal an increase in deaths, incapacitating injuries, and visible injuries (which is statistically significant when adjusting for seasonality). Likewise, average speeds and cited speeds both increase in the period after the layoff.[17] In addition, there appear to

[17] Linking the automated speed data with average weather conditions provided evidence the speed increases were larger on days with no rain or snow. Using county averages of daily snow conditions we considered the following breakdown of weather conditions: no rain or snow, rain or snow > 0, rain or snow > 50 percentile, rain or snow > 75 percentile, and rain or snow > 90 percentile. We found respectively that average speeds (with t-statistics in parentheses) increased by 0.53 (17.45), 0.492 (7.09), 0.38 (3.10), 0.149 (0.84), and −0.12 (−0.49). This analysis would not be appropriate for the citation speed data as we found citations fell significantly with rain and snow, introducing selection effects on the measurement of speed.

be only minimal changes in VMT and driver characteristics, and the proportion of young drivers trend in a direction that would tend to decrease injuries. Similarly, the slight increase in precipitation would have lead to decreases in fatalities and injuries under dry weather conditions (due to an exposure decrease). Although the summary statistics are suggestive, the main results in Section IV will take advantage of various counterfactual groups to guard against potential omitted variables coinciding with the timing of the OSP mass layoff.

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, and so we scale injuries and deaths by VMT.[18] We implement two types of models in our analysis: OLS regressions and Poisson regressions. Although negative binomial models have been 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 of equality between the conditional mean and variance.

One important identifying assumption for the Poisson model is $E(Y|X) = \exp(X'\beta)$. Because of this assumption about the nature of the conditional mean of Y, the estimated coefficients can be interpreted as semi-elasticities. Thus the Poisson model is similar to estimating a linear regression model in which $E(\ln y|x) = X'B$, but allows for cases where the dependent variable takes on values of zero, which occurs in rare instances. In order to make the comparison of the two models easier, we scale the estimated coefficients from the linear regression models to represent semi-elasticities.[19] We utilize robust standard errors in the OLS regression models to allow for heteroskedasticity and to relax the conditional mean and variance equality for the Poisson models. We also estimated standard errors adjusting for clustering at the state level and found that this resulted in smaller standard errors. However, clustering with few regions can often over-reject the null hypothesis (Bertrand, Duflo, and Mullainathan 2004; Cameron, Gelbach, and Miller 2008). Although bootstrapping methods have been found to limit the over-rejection of the null hypothesis in cases with relatively few clusters, they computationally require the number of clusters to be greater than or equal to six. We utilize the robust standard errors as they resulted in the most conservative test-statistics, similar to the implications of hybrid methods to control size in hypothesis testing advanced by Andrew and Guggenberger (2009).

We consider injuries and fatalities that result under two scenarios on freeways and highways: (i) outside of city limits under dry weather conditions and (ii) inside or outside of city limits for all weather conditions. Dry weather conditions are defined by weather conditions reported as clear or cloudy and surface conditions reported as dry at the time of the accident. If the OSP layoff is indeed responsible for the

[18] Scaling variables so they are noninteger valued does not affect the estimates of the Poisson regression, provided the standard errors are adjusted to relax the mean-variance equality assumption.

[19] This is accomplished by scaling the regression coefficients by the mean of dependent variable.

increase in injuries and fatalities, one would expect the increase in fatalities to be largest where the decrease in enforcement was the most evident. 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. Based on crash reports from Oregon, OSP Troopers attend to the majority of accidents on highways outside of city limits (77 percent) and a minority of accidents (14 percent) inside of city limits. This is suggestive of the patterns that likely exist for enforcement, with areas outside of city limits being affected the most by the layoffs.[20] Moreover, injuries tend to be more severe outside of city limits as the odds of a visible injury nearly double, the odds of an incapacitating injury triple, and the odds of a fatality increase eightfold, all conditional on being in an accident. While speeds and traffic flows tend to be the highest under fair weather conditions, police officers may affect driver behavior under all surface and weather conditions. The degree to which drivers may differentially respond to the layoff under dry weather conditions will depend on the preferences of drivers regarding accident risk and the perceived odds of citation.

The empirical models in Section IV employ a difference-in-difference approach comparing Oregon to counterfactual groups. This approach will offer unbiased estimates of the effect of the layoff provided that Oregon and the counterfactual group share common trends absent the treatment, or in our case, the layoff. One potential counterfactual is the rest of the continental Unites States. However, many states in the United States have different underlying demographic shifts and economic trends. For this reason, many previous authors including Card (1990) Card and Krueger (1994) and Sabia, Burkhauser, and Hansen (2012) have utilized geographically chosen control groups. For this study, Idaho and Washington serve as states proximate to Oregon which have similar demographics, economic trends, and weather patterns. Figure 3 illustrates the trends in enforcement levels, scaled relative to the year 2000 as a base year to ease comparisons across states, both for the LEOKA sworn police time series and administrative records for Oregon, Washington, and Idaho. While the administrative data capture the timing of the layoff more accurately, both comparisons suggest that Oregon had a similar trend in sworn state police relative to the rest of the nation prior to 2003, after which Oregon experienced the nation's largest decrease in sworn state police. The validity of Idaho and Washington is demonstrated more clearly in panel B of Figure 3, as the three states share similar trends in state highway troopers prior to 2003, after which Oregon experienced a disproportionately large layoff while trooper employment in Idaho and Washington continued along their preexisting trends. Furthermore, as shown in Figure 4, Idaho and Washington have injuries and fatalities trends which were relatively flat prior to the layoff, similar to Oregon. After the layoffs the trends in Washington and Idaho remained relatively flat, while Oregon posted a noticeable increase in injuries.

[20] In addition, Oregon passed other laws in 2003 that confound using nonhighways inside of city limits as counterfactuals. These include the usage of automated red-light cameras and the distribution of automated speed camera sites.

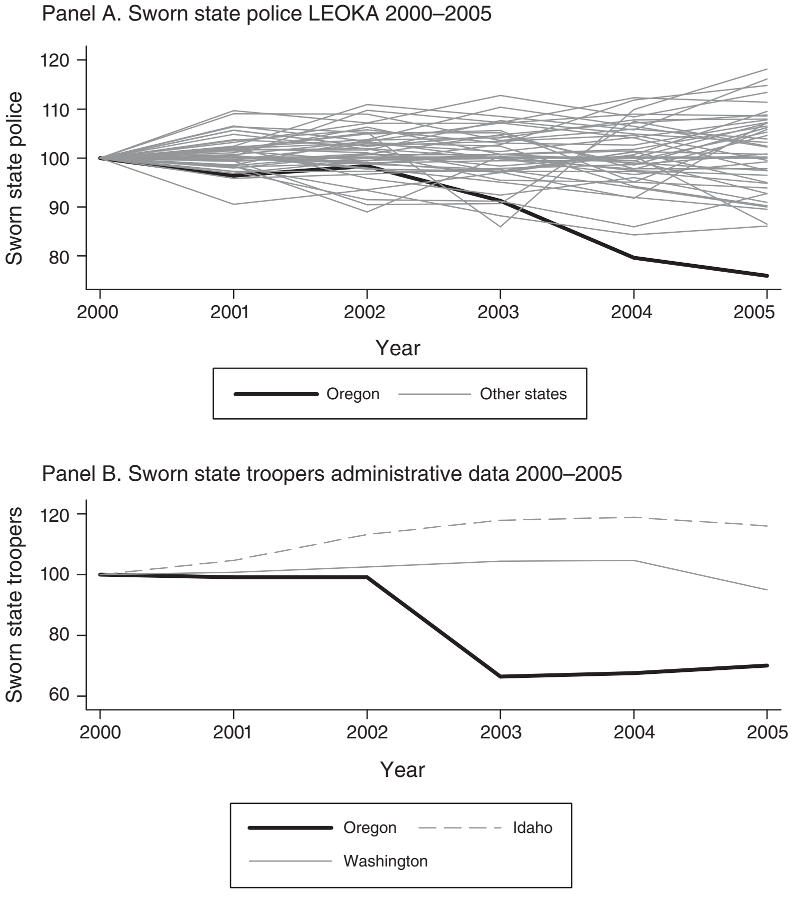

FIGURE 3

Potentially improving on geographically chosen control groups and also suggesting a way to confirm whether a geographically chosen group is appropriate, Abadie, Diamond, and Hainmueller (2010) offers a data-driven approach to create a synthetic control group. This method creates a weighted average of all the available control group units (states in our case) where the weights are based on the degree to which observable characteristics predict the outcome variables of interest. We utilize the set of characteristics in Table 2.[21] If the weighted combination of Idaho and Washington is an optimal comparison for Oregon, then the weights on Idaho and Washington will sum to one, with all other states receiving zero weight. Depending on the optimization method, the synthetic control method places weight on Idaho and Washington which ranges from 0.57 to 0.96. For our main results, we use an average of the weights implied by the fully nested convergence method for the synthetic control approach, resulting in Washington, Idaho, Nevada, and West

[21] We obtain similar results using a subset of the predictors, including only weather and economic conditions.

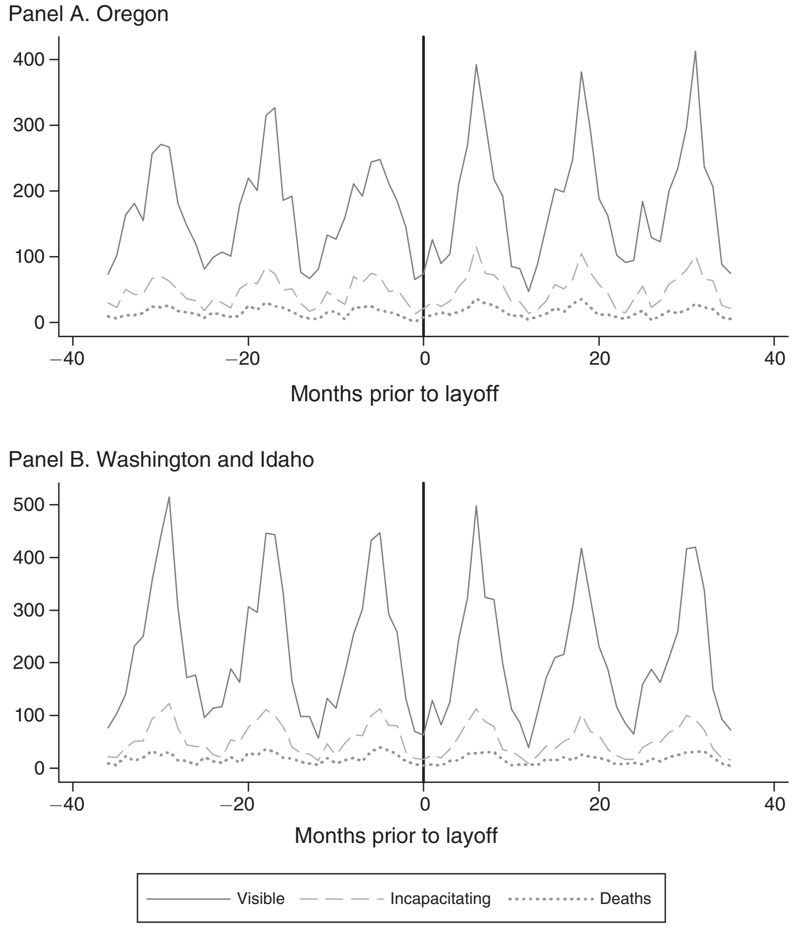

Figure 4. Injuries on Highways in Oregon, Washington, and Idaho Outside City Limits—Dry Weather Conditions

Virginia, respectively, receiving the weights of 0.781, 0.108, 0.089, and 0.022.[22] As shown in Table 2, the geographically chosen control group and the synthetically created control group most closely mirror Oregon's enforcement levels, driving patterns and demographics.[23]

III. Results

In this section, we estimate the effects of the OSP mass layoff. We first examine the effect of the layoff on enforcement, utilizing the LEOKA, Census of Law Enforcement, and administrative records from Washington and Idaho. Next, we

[22] The nested convergence method in general acheives more accurate results than the quadratic regression based method, while sacrificing some computing time. We utilize an average of the two weights to confirm that the difference in the estimated effects across dependent variables is driven by different effects rather than different control groups. However we obtain similar effects if we allow the control groups to vary as well.

[23] Differences in average injury rates appear due to differences in injury classification and reporting across states.

Table 2—Characteristics of Oregon and Potential Counterfactual Groups

	Oregon	United States (w/o Oregon)	Washington and Idaho	Synthetic
Fatalities/VMT	11.3	13.2	11.3	11.4
	(2.4)	(4.6)	(4.4)	(3.6)
Incapacitated/VMT	28.0	—	67.7	—
	(5.1)		(40.7)	
Visible/VMT	131.4	—	364.5	—
	(18.7)		(207.3)	
Precipitation	2.0	2.9	2.1	2.4
	(1.6)	(1.9)	(1.6)	(1.8)
Temperature	49.3	53.2	47.3	49.1
	(12.0)	(17.3)	(14.0)	(13.5)
Unemployment	6.8	4.5	5.5	5.8
	(1.2)	(1.2)	(1.3)	(1.2)
Sworn state police/VMT	198.5	225.5	219.8	206.2
	(23.0)	(108.4)	()	(39.3)
Commute via car	85.8	90.0	87.4	87.6
	(0.5)	(4.3)	(1.3)	(1.8)
Telecommute	5.5	3.7	5.2	4.3
	(0.4)	(1.1)	(0.6)	(0.6)
Public transportation	3.6	2.5	2.8	3.9
	(0.2)	(3.5)	(1.8)	(1.6)
Transportation time	19.2	21.0	20.7	22.0
	(0.4)	(3.4)	(2.4)	(2.3)
High school graduate	91.0	88.7	90.6	90.9
	(0.3)	(3.3)	(1.5)	(0.3)
College graduate	30.2	28.3	28.9	30.8
	(1.0)	(5.4)	(5.6)	(4.6)
White	92.2	85.6	90.8	88.1
	(0.6)	(8.5)	(3.9)	(3.3)
English speaking	84.4	83.6	83.4	82.6
	(0.4)	(7.4)	(1.6)	(3.2)

Notes: This table compares characteristics of Oregon and potential counterfactual groups during the pretreatment period. Standard errors are reported in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

estimate the effect of the layoff on traffic fatalities, fatal accidents, and fatalities per accidents utilizing records on fatal accidents from the FARS. Subsequently, we examine the impact of the layoff on nonfatal accidents and injuries. We then explore whether other confounders in Oregon may have changed simultaneously with the layoff, and test the validity of the control group by re-estimating the main models in a placebo period prior to the state police mass layoff.

A. *Enforcement*

Based on the timing of the events described in Section II, it is a matter of historical fact that Oregon experienced a substantial decline in the number of state police on February 1, 2003. Likewise, while Figure 3 suggests Oregon's decrease deviated

Table 3—Effect of the Oregon Layoff on Enforcement

	LEOKA			Admin. data	
Counterfactual	United States (w/o Oregon)	Washington and Idaho	Synthetic	Washington and Idaho	Washington and Idaho
Oregon × after layoff	−0.14***	−0.23***	−0.19***	−0.37***	−0.35***
	(0.04)	(0.07)	(0.06)	(0.06)	(0.01)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Month FE	N/A	N/A	N/A	N/A	Yes
Controls	No	No	No	No	Yes
Observations	235	18	30	18	216

Notes: This table estimates the effect of the layoff on state police per VMT. All estimates are based on OLS regressions, with the estimate coefficient scaled by the mean of the dependent variable to reflect a semi-elasticity.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

from the trends of neighboring states or the rest of the United States, in this section we investigate the empirical magnitude and statistical significance of Oregon's decrease relative to potential counterfactual groups. While we have administrative records on citations and trooper employment from the OSP, obtaining comparable administrative data from the rest of the United States is infeasible. We take advantage of the LEOKA staffing data, which keeps track of annual employment of all law enforcement officers, including sworn state police. Importantly, while the LEOKA identifies sworn state police counts, it does not separate highway troopers (those on the road enforcing speed limits and giving citations) from other state police (such administrative officers, police in crime labs, those working at training academies, investigative police, special task forces, and others). Although prior to the layoff Oregon highway troopers accounted for fewer than half the OSP, the administrative records suggest that roughly 3/4 of the layoffs in the OSP affected highway troopers. Given the highway troopers were disproportionately represented in the layoff, we would expect the estimates using the LEOKA data to understate the effect of the layoff on percentage decrease in enforcement on highways.

We estimate the effect of the layoff using OLS regressions based on equation (1):

$$\frac{sworn_{sy}}{VMT_{sy}} = \beta \times OR \times after_{sy} + y_y + s_s + u_{sy}, \tag{1}$$

where s indexes states and y indexes years (2000–2005). The estimates are reported in Table 3. The key estimate we are interested in comes from the interaction between the indicator for Oregon and an indicator for after the layoff. The point estimate is scaled by the mean of the dependent variable (sworn state police per VMT), allowing the reported coefficients in Table 3 to be interpreted as a semi-elasticity, or the percentage decline in enforcement attributed to the layoff. As the LEOKA are only reported annually, we limit the control variables state and year fixed effects due to the limited number degrees of freedom.[24] As shown in Table 3, relative to the rest of the US, Oregon experienced a 14 percent decline in enforcement. When utilizing the geographic control group, the estimated percentage decrease of State Police in Oregon is 23 percent, decreasing slightly to 19 percent when utilizing the synthetic control group.[25] When utilizing the administrative data on state troopers we have from Oregon, Washington, and Idaho, the estimated decline increases to 37 percent. Likewise, utilizing monthly level administrative records on state troopers and including additional control variables has practically no effect on the estimate (a slight decline to 35 percent) and increases the precision substantially. The difference in the results when comparing the LEOKA data and administrative data is largely driven by the inability of the LEOKA data to identify highway troopers separately from state police employed in other capacities. In addition, measurement error might play a role in the LEOKA data (see Chalfin and McCrary 2011).[26]

The Census of Law Enforcement also asked about "sworn personnel responding to calls" in 2000. We use this adjustment to the LEOKA data so it can be more representative of the number of state troopers (the officers responsible for enforcing speed limits and issuing citations). To illustrate that the sworn personnel responding to calls captures the highway trooper levels, consider the state of Oregon. In 2000, based on administrative records, 360 highway troopers were employed. In the Census of Law Enforcement, 823 total state police were employed, while 377 were state police which responded to a call. Because the question was not asked in 2004, we perform an adjustment to the LEOKA data by subtracting off the difference between sworn state police and the state police responding to calls.[27] Estimates of the effect of the layoff on sworn police with that adjustment are reported in Table A2. The adjusted LEOKA suggests that enforcement in Oregon declined by 34 to 43 percent relative to either the synthetic control or geographic control group. These estimates more closely resembles the estimated percentage decrease evident when using the administrative trooper employment records. With three different data sources on state police, the LEOKA, the Census of Law Enforcement, and administrative trooper staffing records, we estimate the number of sworn state police declined by 20 percent relative to comparable states, while the number of highway troopers (sworn state police that patrol highways and enforce traffic laws) declined by 33 to 37 percent.

[24] We observe nearly identical point estimates are produced when including other controls, with slightly reduced precision. This is classic evidence of multi-colinearity (Farrar and Glauber 1960), likely due to the fact that few of the control variables change at the year level, and hence are nearly perfectly colinear with the state fixed effects.

[25] Although the Census of Law Enforcement is only conducted every four years, it produces estimated decreases nearly identical to the LEOKA time-series.

[26] While the LEOKA data might measure long-term trends and substantial cross-sectional differences correctly, Chalfin and McCrary (2011) suggested substantial measurement error was present in the LEOKA series. In our data, a substantial decrease in sworn state police is evident in the LEOKA series for Oregon. However, the LEOKA data suggests there was a modest decrease in enforcement in 2003, followed by a large decrease in 2004. Given the presence of measurement error in the LEOKA series, we would expect estimates based on that source to be attenuated.

[27] For context, in Oregon, this would imply subtracting the difference of 823 and 377 (446) from the LEOKA sworn police figures.

Table 4—Effect of the Oregon Layoff on Traffic Fatalities per VMT

	All highways			Outside city limits		
	All weather conditions			Dry weather conditions		
	United States (w/o Oregon)	Washington and Idaho	Synthetic	United States (w/o Oregon)	Washington and Idaho	Synthetic
OLS						
Oregon × after layoff	0.05***	0.14**	0.13***	0.07***	0.22**	0.26***
Semi-elasticity	(0.01)	(0.06)	(0.05)	(0.02)	(0.10)	(0.08)
Poisson						
Oregon × after layoff	0.06***	0.12**	0.13**	0.09***	0.19**	0.26***
Semi-elasticity	(0.01)	(0.06)	(0.05)	(0.02)	(0.09)	(0.07)
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
Observations	3,456	216	360	3,456	216	360

Notes: This table estimates the effect of the layoff on traffic fatalities per VMT. Each cell is from a separate OLS or Poisson regression with the estimated coefficient scaled by the mean of the dependent variable to reflect a semi-elasticity for the OLS models. Additional controls include temperature, precipitation, the maximum speed limit, and the unemployment rate.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

B. *Traffic Fatalities*

Having confirmed that the OSP layoff resulted in a substantial decrease in enforcement relative to comparison states, we estimate the effect of the layoff on traffic fatalities. As in subsection 4.1, the key estimate of interest is the coefficient on the interaction between the Oregon indicator and the indicator for the period after the layoff. In order to facilitate comparison across the OLS and Poisson models, we scale the estimated OLS coefficient by the mean the dependent variable, allowing the reported estimates in Table 4 to be viewed as semi-elasticities.[28] Specifically, we estimate equation (2) using OLS:

$$(2) \quad \frac{fatals_{smy}}{VMT_{sy}} = \beta \times OR \times after_{smy} + y_y + s_s + m_m + X'_{smy}a + u_{smy},$$

where m indexes months. Equation (3) is estimated as a Poisson regression:

$$(3) \quad E\left(\frac{fatals_{smy}}{VMT_{sy}}\right) = \exp(\beta \times OR \times after_{smy} + y_y + s_s + m_m + X'_{smy}a).$$

[28] No scaling is needed for the Poisson regression models, as binary regressors in Poisson regressions can be interpretated as semi-elascities due to the underlying assumption about the conditional mean.

Table 4 contains estimates of the effect of the layoff on traffic fatalities. Columns 1–3 focus on all highway fatalities under all weather conditions, while columns 4–6 consider highway fatalities outside of city limits and under dry weather conditions. The first row of estimates are based on OLS regressions, while the second row is based on Poisson models. When compared to the rest of the United States, Oregon highway fatalities are estimated to have increased by roughly 5–6 percent, and 7–9 percent under dry weather conditions outside of city limits. Utilizing Washington and Idaho as comparison groups, the layoff in Oregon is estimated to have increased highway fatalities by 12–14 percent, and highway fatalities under dry weather conditions by 19–26 percent. The synthetically chosen control group provides similar estimates to the geographically proximate control group, which is not surprising given the significant weight the synthetic control group assigns to Washington and Idaho.

While the estimates in Table 4 suggest the layoff in Oregon was associated with a substantial increase in fatalities, this could have been driven by either an increase in fatal accidents (caused by more drivers driving dangerously) or an increase in fatalities per fatal accident (dangerous drivers driving more recklessly). We explore those possibilities further in Table 5. The first row of estimates in Table 5 focuses on fatal accidents per VMT based on results from regressions based on equation (4):

$$(4)\quad \frac{fatal\,accidents_{smy}}{VMT_{sy}} = \beta \times OR \times after_{smy} + y_y + s_s + m_m + X'_{smy}a + u_{smy}.$$

The second row analyzes fatalities per accident and is based on equation (5):

$$(5)\quad \frac{fatals_{smy}}{fatal\,accidents_{smy}} = \beta \times OR \times after_{smy} + y_y + s_s + m_m + X'_{smy}a + u_{smy}.$$

The estimated coefficients are once again scaled by the mean of the dependent variable to allow interpretation as semi-elasticities. All of the estimates are positive, displaying evidence of both mechanisms driving the results. Once again, the synthetic and geographic control groups provide comparable estimates.

Utilizing the estimates from both this subsection and Section IIIA, we use indirect least squares to provide estimates of the elasticity between highway troopers and traffic fatalities. Based on the administrative records, the layoff is estimated to decreased troopers per VMT by 36 percent when averaging the estimates. Combined with a 12–14 percent increase in traffic fatalities for highways under all weather conditions, this implies an elasticity of −0.33 to −0.38. Likewise, the estimated 19 to 26 percent increase in highway fatalities outside of city limits under dry weather condition would suggest an enforcement/fatality elasticity of –0.52 to −0.72. These elasticities suggest that either increasing or decreasing the number of police on highways can have a substantial impact on traffic fatalities.

Table 5— Effect of the Oregon Layoff on Fatal Accidents and Fatalities per Accident

	All highways			Outside city limits		
	All weather			Dry weather conditions		
	United States (w/o Oregon)	Washington and Idaho	Synthetic	United States (w/o Oregon)	Washington and Idaho	Synthetic
Fatal Accidents per VMT						
Oregon × after layoff	0.03***	0.09	0.08	0.04***	0.19**	0.17**
Semi-elasticity	(0.01)	(0.06)	(0.05)	(0.01)	(0.09)	(0.07)
Fatalities/Fatal Accident						
Oregon × after layoff	0.03***	0.06**	0.06***	0.02***	0.04	0.06
Semi-elasticity	(0.01)	(0.03)	(0.02)	(0.005)	(0.06)	(0.04)
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
Observations	3,456	216	360	3,456	216	360

Notes: This table contains estimates of the effect of the layoff on fatal accidents per VMT and fatalities per fatal accident. Each cell is from a different OLS regression, with the point estimates scaled by the mean of the dependent variable to reflect a semi-elasticity. Additional controls include temperature precipitation, the maximum speed limit, and the unemployment rate.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

C. *Nonfatal Injuries*

Although fatal accidents are a costly outcome of traffic accidents, they are certainly not the only costly consequences of dangerous driving. For every fatal accident, hundreds of nonfatal but often debilitating injuries occur. In this analysis, we estimate the effect of the OSP layoff on nonfatal injuries. We explore two injury types: incapacitating injuries (those requiring an ambulance and further treatment at a hospital) and visible injuries (those which required medical care at the scene but were not transferred to a hospital for immediate further treatment).[29] Unfortunately, a nationwide clearinghouse of nonfatal accidents similar to the FARS is not available. However we obtained nonfatal crash data from Oregon, Idaho, and Washington which enables a difference-in-difference analysis for the geographically chosen control group from the previous subsection. We estimate OLS and Poisson regression models similar to the previous regressions, only with injuries replacing fatalities as the dependent variable of interest, with the results reported in Table 6.[30]

[29] Slight differences in reporting exist across the states. Oregon classifies injuries as incapacitating or visible, while Washington uses serious and visible, and Idaho classifies injuries on an A, B, C scale. Although level difference exist due to the differences in reporting, the fixed effects adjust for underlying differences in reporting which are constant over time.

[30] In Table A4, we investigate the robustness of the estimates to the level of aggregation. We re-estimate the difference-in-difference models for fatalities and injuries for Oregon with Idaho and Washington as counterfactuals aggregating injuries and fatalities to the daily level. Importantly, when aggregating to this level, calculating the number of fatalities per fatal accident or injuries per injury accident becomes problematic as $\frac{Fatals}{Fatal\ Accident} = \frac{0}{0}$ on a large number of days, and hence those results are not replicated at a finer level of disaggregation. We find

Table 6— Effect of Layoff on Injuries

	All highways		Outside of city limits	
	All weather		Dry weather	
	Incapacitating	*Visible*	*Incapacitating*	*Visible*
OLS				
Oregon × after layoff	0.17***	0.13**	0.29***	0.24***
Semi-elasticity	(0.05)	(0.05)	(0.08)	(0.05)
Poisson				
Oregon × after layoff	0.15***	0.16***	0.19***	0.19***
Semi-elasticity	(0.05)	(0.04)	(0.07)	(0.05)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	216	216	216	216

Notes: This table contains estimates of the effect of the layoff on injuries per VMT. Each cell is from a different OLS regression, and the point estimate is scaled by the mean of the dependent variable to allow interpretation as a semi-elasticity. Additional controls include temperature, precipitation, the maximum speed limit and the unemployment rate.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

In all cases, the period after the layoff was associated with additional incapacitating and visible injuries. Similar to the results in Section IVB, the effects were strongest when limiting the jurisdiction where state police are the principal enforcing agency, and under dry weather conditions. In Table 7, we replicate the previous decomposition suggested by equations (4) and (5), analyzing the effect of the layoff on accidents with injuries and the number of injuries per accident. We also find some evidence that both more injury accidents occurred (more drivers drove dangerously) and that there were more injuries per accident (dangerous drivers drove even more recklessly).[31]

D. *Robustness Checks*

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 in proportion). Although VMT are slightly higher in the post-layoff years, they peaked in 2002 prior to the layoff.

comparable estimates when aggregating to the daily, with some estimates increasing in precision, while others decrease slightly. As such, the main findings hold up when using more disaggregated data.

[31] We also investigated whether or not the effects varied by season (defined by summer versus nonsummer months). The point estimates would suggest that more severe fatal accidents increased the most in summer months, as did nonfatal injuries outside of city limits and under dry weather conditions. However, we were unable to reject the null that the effects did not vary by season.

Table 7—Effect of Layoff on Accidents and Injuries per Accident

	All highways		Outside city limits	
	All weather		Dry weather	
	Incapacitating	*Visible*	*Incapacitating*	*Visible*
Injury accidents per VMT				
Oregon × after layoff	0.15***	0.13***	0.21***	0.21***
Semi-elasticity	(0.05)	(0.03)	(0.07)	(0.05)
Injuries/injury acc.				
Oregon × after layoff	0.01	−0.01	0.07**	0.02
Semi-elasticity	(0.03)	(0.01)	(0.03)	(0.02)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	216	216	216	216

Notes: This table contains estimates of the effect of the layoff on injury accidents per VMT and injuries per injury accident. Each cell is from a different OLS regression, with the point estimates scaled by the mean of the dependent variable to allow interpretation as semi-elasticities. Additional controls include temperature, precipitation, and the unemployment rate.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

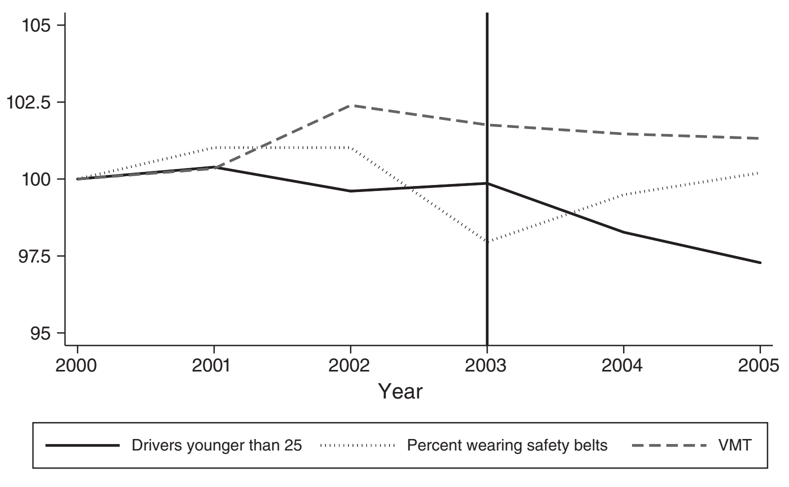

Figure 5. Other Potential Causes (*Trends Normalized to 2000 Levels*)

The proportion of people that reported wearing their seat belts in accidents fell only slightly in 2003 (by roughly 2 percent), and it was at the baseline levels again in 2004 and 2005.[32] 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

[32] Given that Cohen and Einav (2001) found evidence that manadatory laws affect seat belt usage, then decreases in enforcement could affect seat belt usage.

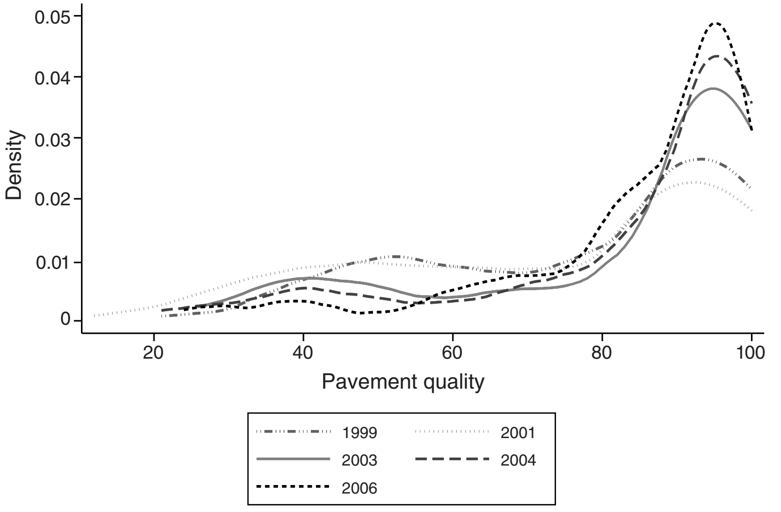

Figure 6. Pavement Quality on Major Highways, 1999–2006

in enforcement. These other potential channels do not bias any of our reduced form estimates of the layoff.[33]

Another important factor that could be affected by budget cuts is roadway construction and along with it, overall pavement quality. In our investigations of state budgets we found no evidence linking the budget crisis with decreases in roadway funding, as illustrated in Table A1. 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).[34] On average, pavement quality actually increased slightly at a smooth rate over the 2000–2005 period. This is illustrated in Figure 6, which plots the distribution of pavement quality (measured on a 0–100 scale) for major sections of highway in the years 1999, 2001, 2003, 2004, and 2006. The distributions suggest the fraction of roadways of a good or very good quality (rated 80 or better) increased smoothly both in the years before and after the layoff, while the fraction of roadways rated as fair or poor (less than 80) fell.

Although the observed factors discussed above do not explain the increase in fatalities and injuries, unobservable driver behavior changes or the nonvalidity of the control group could also introduce bias. One approach to test the validity of the control group in accounting for unobservables is to replicate the previous analyses for a placebo time period. In Table A3 we analyze a placebo period from 1997–2002, introducing the placebo law in February of 2000. As such the analysis is identical to that in Section IVB, only shifted three years earlier. If the common trends assumption of the difference-in-difference models were violated due to factors like unobserved shifts in driver behavior in Oregon, we would expect to see positive and significant estimates. Instead, during the placebo period from 1997–2002, the

[33] It would create bias if we instrumented average speeds with enforcement, as that would assume there was only one channel through which enforcement improved the behavior of drivers.

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

estimates are both statistically insignificant and economically small. This lends additional evidence supporting the credibility of the assumptions underlying the difference-in-difference models.[35]

E. *Policy Implications*

The 2003 police layoff in Oregon was not the only reduction in employment that the OSP has experienced. In 1979, Oregon employed 641 highway troopers which fell to 250 by 2005. Simultaneously, VMT have increased by 80 percent. With that in mind, we consider two hypothetical scenarios: (i) the OSP remain at their 1979 levels (641) throughout the entire time period and (ii) the OSP levels evolve at the same rate as VMT. The results of these estimates are depicted in Figure 7. The solid, dotted, and dashed lines respectively represent the fatality rate at actual highway police levels, the predicted rate fixed at 1979 levels, and the predicted rate with highway police per VMT fixed. 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 either been held constant or increased with VMT.

In Table 8, we aggregate the predicted differences to calculate 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 highway troopers had remained at 641 troopers, fatalities would have fallen by 2,167 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 5,031 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 $123,000, implying scenario 1 would have cost an additional $669 million while scenario 2 would have cost $1.5 billion. This implies a cost of approximately $309,000 per life saved, which is far less than the general range of accepted estimates for the value of a statistical life.[36] In addition, our estimates suggest that state troopers prevent other injuries. To assess the net social benefits one also needs to consider other factors such as time saved, the value of time, and other property damage prevented.[37]

[35] Another placebo approach would be to replicate the previous analyses for accidents that should be unaffected by the presence of police. We have tested the effect of the layoff under dangerous driving conditions where, in our view, the police would have little effect on driver behavior, finding little evidence of an increase in traffic fatalities and injuries under those conditions.

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

[37] In an earlier version of the paper, we utilized the layoff to estimate the value of statistical life. 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 be laid off or would have elected to keep the police and cut other budgets.

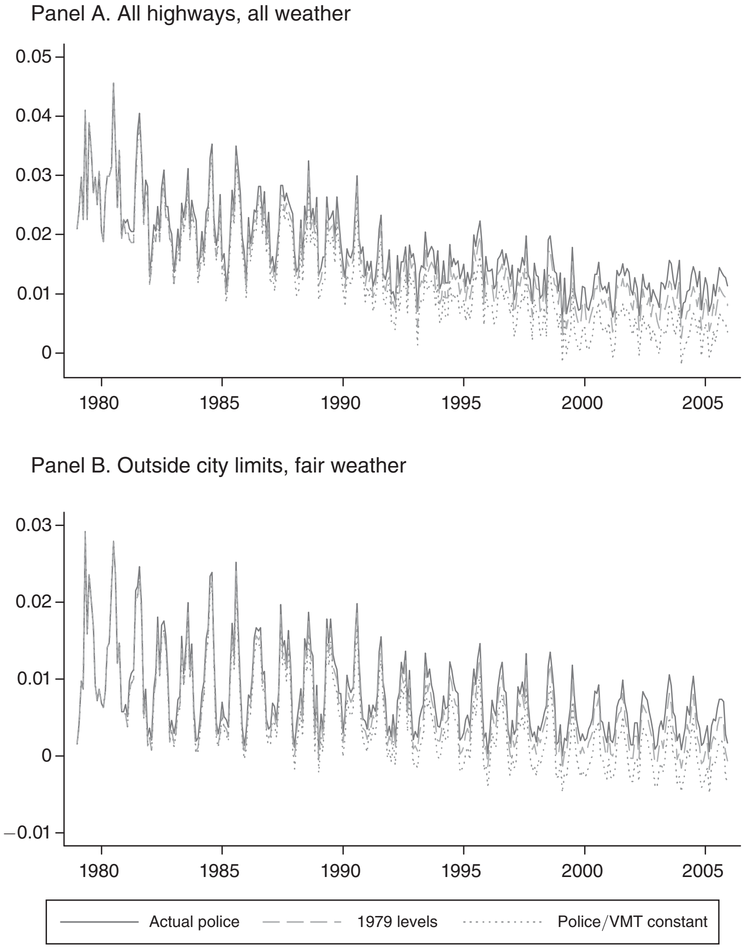

Figure 7. Oregon Fatality Counterfactuals

Table 8—Counterfactuals 1979–2005

	Fatalities 1979–2005	Troopers 2005	Troopers FTE 1979–2005
Actual levels	14,662	250	11,862
Counterfactuals			
Troopers = 641	12,496	641	17,307
$\frac{\text{Troopers}}{\text{VMT}} = \frac{\text{Troopers}}{\text{VMT}}$ 1979	9,631	1,159	24,505

Notes: This table contains comparison of fatality levels and trooper employment resulting under three scenarios: reality, holding trooper employment fixed, and holding trooper employment per VMT fixed from 1979–2005.

IV. Conclusion

Police have long been an often-used tool for enforcing speed limits on highways. We offer evidence concerning the effect of police on traffic fatalities, identified by a mass layoff of Oregon State Police due solely to budget cuts. Our results indicate that a decrease in enforcement is associated with an increase in injuries and deaths on Oregon highways. Our preferred estimates suggests that enforcement-fatality elasticity ranges between -0.33 and -0.72. We also find evidence that nonfatal injuries also increased following the reduction in enforcement on Oregon highways. An analysis of the reduction in state police in Oregon since 1979 suggests that there would have been 2,167 fewer deaths over the 1979–2005 time span if the state police had maintained their original 1979 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 5,031 fewer traffic fatalities over 1979–2005.

It is worth noting that, to the extent that nonlinearities or decreasing returns to enforcement exist, these estimates could be upper bounds. Likewise, our estimates refer specifically to the effect of reducing policing, while the hiring of police may have a different effect if newly minted state police fresh from the academy are less effective. Importantly, while previous studies have estimated the elasticity between police and crime, the effect of police in those studies could be due to either deterrence or incapacitation.[38] Our estimates can be viewed as credibly identifying the deterrent effect of state police on highways, as nearly all drivers are allowed to return to driving immediately after receiving a citation. In other settings it is quite difficult to separate the deterrent effect of police from the incapacitation effect, as police also incapacitate criminals when arresting them.

Because of the budget shortfalls many state legislatures are either currently considering or have recently implemented large layoffs or furloughs in their state police forces, such as Illinois[39] (plans to layoff 460 state police), Virginia[40] (recently laid off 104 state police), and Michigan[41] (recently laid off 100 police). Recent research has suggested that poor local economic conditions actually lead to increases in citations among local jurisdictions (Makowsky and Stratmann 2009). Our findings suggest that budget cuts in state police and the subsequent reductions in enforcement would likely be followed by increased injuries and fatalities, unless the states utilize other enforcement tools such as increased punishments to offset the reduction in enforcement.[42] Future work could investigate more fully the effect of fine increases or other punishments on driver behavior and their usefulness as another factor in deterring dangerous driving.

[38] Furthermore, there is likely little geographic redistribution of crime (speeding) because of changes in the Oregon state police (people likely do move from Oregon to Washington or Idaho to speed).

[39] Kurt Erickson, "Illinois budget cuts hit state troopers," *Quad-City Times*, March 24, 2010, accessed May 8, 2010, http://qctimes.com/news/local/article_cc6bfc2e-37b1-11df-b2a2-001cc4c002e0.html.

[40] See Jim Nolan, "More state government workers facing layoffs," *Richmond Times-Dispatch*, September 17, 2009, accessed April 7, 2014, http://www.timesdispatch.com/news/more-state-government-workers-facing-layoffs/article_859d164c-951d-5659-bf71-e10a8f249a4c.html.

[41] Mark Hornbeck and Charlie Cain, "Michigan budget cuts hit police ranks," *The Detroit News*, May 6, 2009, accessed May 8, 2010, http://detroitnews.com/article/20090506/POLITICS02/905060364/Michigan-budget-cuts-hit-police-ranks.

[42] See Graves, Lee, and Sexton (1989) for a discussion of optimal fines and enforcement on roadways.

Appendix

Table A1— 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.

Table A2—Synthetic Method and Estimated Weights

	Nested		Quadratic	
	All highways	Outside	All highways	Outside
WA	0.802	0.76	0.366	0.255
ID	0.02	0.196	0.336	0.311
WV	—	0.044	—	—
NV	0.178	—	—	—
KS	—	—	0.034	0.138
MT	—	—	0.199	—
NY			0.064	0.089

Notes: This table contains the optimal derived weights based on synthetic control design models. The columns contain estimates based on the optimization method (quadratic or fully nested, and based on the dependent variable used traffic fatalities on all highways, or traffic fatalities under dry weather highways outside city limits.

Table A3—Effect of Layoff on Sworn Police per VMT Adjusting for Number Responding to Calls in 2000

	LEOKA		
	United States (w/o Oregon)	Washington and Idaho	Synthetic
Oregon × after layoff	−0.20***	−0.43***	−0.34***
	(0.06)	(0.12)	(0.11)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Note: This table contains estimates of the effect of the layoff on state police per VMT, with state police per VMT adjusted by the number of responded to calls based on the 2000 Census of Law Enforcement.

Table A4— Effect of Layoff on Injuries: Daily Level Poisson Regressions

	All highways			Outside city limits		
	All weather conditions			Dry weather		
Injuries per VMT	Fatal	Incap.	Nonincap.	Fatals	Incap.	Nonincap.
	0.16**	0.15***	0.12***	0.20*	0.14***	0.15***
	(0.07)	(0.04)	(0.03)	(0.11)	(0.01)	(0.01)
Injuries acc. per VMT	Fatal	Incap.	Nonincap.	Fatals	Incap.	Nonincap.
	0.13*	0.05***	0.05***	0.17	0.12***	0.12***
	(0.07)	(0.01)	(0.01)	(0.11)	(0.01)	(0.01)
Observations	6,576	6,576	6,576	6,576	6,576	6,576
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Day of week FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table contains estimates of the effect of layoff on injuries in Oregon with Idaho and Washington as counterfactual groups. The number of injuries are aggregated to the daily level, and all regressions are estimated as Poisson regressions. The regression models also include day of week fixed effects. Otherwise, the regression models are identical to the models estimated in Tables 4, 5, and 6.

Table A5—Placebo Period 2002

	All highways		Outside city limits	
	All weather conditions		Dry weather conditions	
	Washington and Idaho	Synthetic	Washington and Idaho	Synthetic
OLS				
Oregon × after layoff	−0.05	−0.04	−0.01	−0.05
Semi-elasticity	(0.06)	(0.05)	(0.09)	(0.08)
Poisson				
Oregon × after layoff	−0.06	−0.04	−0.01	−0.05
Semi-elasticity	(0.06)	(0.05)	(0.09)	(0.08)
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	216	360	216	360

Notes: This table contains estimates of a placebo layoff imputed three years prior to the actual layoff. The regression models are identical to those utilized for the estimates presented in Table 4.

Table A6—Placebo Period 1997–2002

	All highways		Outside city limits	
	All weather conditions		Dry weather conditions	
	Washington and Idaho	Synthetic	Washington and Idaho	Synthetic
Fatal acc./VMT				
Oregon × after layoff	−0.06	−0.04	0.003	−0.01
Semi-elasticity	(0.06)	(0.03)	(0.09)	(0.05)
Fatals/fatal acc.				
Oregon × after layoff	0.01	0.01	0.01	0.01
Semi-elasticity	(0.02)	(0.02)	(0.05)	(0.04)
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	216	360	216	360

Notes: This table contains estimates of a placebo layoff imputed three years prior to the actual layoff. The regression models are identical to those in utilized for the estimates presented in Table 5.

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