Deterability by Age

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Abstract: The most effective use of law enforcement resources for reducing crime has generated significant attention across law enforcement agencies, federal, state, and local decision-making committees as well as many academic disciplines. One of the more spirited discussions revolves around law enforcement agents targeting criminal activity based on a suspect's race and age. While racial profiling has received considerable attention, discussions about age-based patrolling and age-graded penalties have received much less attention. In the current analysis, we test the response, by age, of speeding on roadways (a crime that is often considered to be linked to age) to decreases in the probability of being apprehended. We find that all drivers appear to quasi-uniformly increase their speed in response to the reduced chance of being apprehended. Additionally, more egregious and seasoned offenders tend to be more responsive to fluctuations in law enforcement presence.

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Introduction

There is a growing cost-benefit conversation about the best policy strategies to prevent crime (e.g. Donohue and Siegelman 1998; Durlauf and Nagin 2011; Cook, Ludwig, and McCrary 2012). Part of this conversation focuses on identifying the best strategies for preventing crime by particular population sub-groups. This conversation mirrors developments in criminology, where the discussion has changed from a "what works" mentality to a "what works for whom" mentality (Cullen 2005). One of the most meaningful subgroups for this discussion is adolescents and young adults, who are responsible for a large percentage of overall crime. For example, people in the 15 to 24 age group account for 14% of the population but 40% of all arrests reported to the Uniform Crime Reporting system in 2009 (Crime in the United States, 2009, Table 38).

The desire to identify policies which might have a strong effect on youth and young adults is supported by lifecourse criminology's emphasis on age-graded theories of crime (Abbott 2001; Elder 1998; Sampson and Laub 2005). In an age graded theory, factors like work and romantic relationships have different meanings and different consequences at different times in life. Research in psychology and neuroscience also support the idea of "age-gradedness" with research that shows that adolescent and young adult brains are not fully formed. From this perspective, adolescents and young adults are literally processing information and making decisions in different ways (Steinberg 2010). Some specific examples include the claims that youth may be more likely to discount the future, allow emotions to play a larger role in decisions or be more susceptible to peer pressure. (Cauffman et al. 2010; Monahan et al. 2009; Steinberg et al. 2009).

These types of differences in decision making have very clear implications for deterrence. Individuals who discount the future heavily, or worse yet don't even consider the future, are less likely to be deterred (Paternoster and Pogarsky 2009). Although some researchers have argued against severe penalties against youth on the grounds that their developmental immaturity makes them less responsible for their actions (Steinberg and Scott 2003), the same developmental immaturity might also make punishment-heavy strategies ineffective or at least less effective for youth.

However, there is surprisingly little research on the differential impact of deterrent threats by age. A 1998 review of the general deterrence literature by Daniel Nagin (1998) identified only one deterrence study that disaggregated the deterrent effect by age (Sampson and Cohen 1988). Despite Nagin's call that such efforts become standard in deterrence studies (Nagin 1998: 32), we are aware of few additional deterrence studies that have disaggregated the deterrent effect of a policy change by age.[1]

This void does not exist because of indifference to the idea. The study of deterrence has unique challenges due to the simultaneity between enforcement levels and crime at both the individual and aggregate level (Nagin 1998, Cook 1980). This character of deterrence research limits the credible causal study of the deterrent threats to situations where the researcher can claim plausible exogeneity in the threat of enforcement. These rigorous studies are relatively uncommon (Durlauf and Nagin 2011). Within this small subset of plausible studies, some of the policies looked at by researchers are age-specific, like the change from the juvenile to the adult criminal justice system (Levitt 1998, Hjalmarsson 2009, Lee and McCrary 2009) or the change in the drinking age (Carpenter 2008).[2] As a result, comparisons of effectiveness across age groups are not possible. Even when the policy affects all age groups, like policing levels, the data may not support breakdowns by age. For example, most aggregate studies that look at the impact of changes in policing use crimes reported to the police as the outcome variable (Shi 2009, Klick and Tabarrok 2005, DiTella and Schargrodsky 2004). Offenses are used instead of arrests because of legitimate concerns about capturing changes in the behavior of the police, rather than changes in the behavior of the individuals. In addition, arrest data is considered more problematic than offense data. Yet, while age is available in arrest data, it is not available in offense data.

In this paper, we explicitly study the differential impact of the speed of a citation by age due to an exogenous change in the threat of citations for speeding in Oregon. DeAngelo and Hansen (2010) demonstrate how the budget crisis in Oregon led to a 35% reduction in the size of

[1] For an exception, see Drago et al. (2009). They found that the threat of longer prison sentences deterred all age groups of ex-offenders in a similar way. However, the youngest age group in this study are those 32 and under.

[2] Some of the policies are not age specific, but only affect older offender by construction. For example, the three strike law in CA (Helland and Tabarrok 2007) or the prison release in Italy (Drago et al. 2009) and France (Maurin and Ouss, 2009).

the state police, and a corresponding increase in speed and accidents on Oregon roadways.[3] In this paper, we will focus on data from individual citations from the state police in Oregon, in which the age of the driver is recorded. And, rather than focus on the age distribution of the citations solely on the existence of a citation, we focus on the speed of the cited driver, as a function of the posted speed limit. Not surprisingly, given the earlier results from DeAngelo and Hansen (2010), we find that the average speed of a cited driver increases after the layoffs. We also find that younger drivers who are cited tend to be driving faster than older cited drivers. When we initially examine the driving response to changes in law enforcement presence, we find almost no age-specific response to the layoff. However, when we examine the data more closely, we find that certain subgroups respond to the reduction in law enforcement. Most notably, more experienced, older speeders respond by increasing their speeds in the aftermath of the layoff.

We acknowledge that using citation data, rather than an outside source of speed data (which is not available by age), creates concerns about changes in police behavior that might be misinterpreted as changes in individual behavior. However, we believe that speed citation data has advantages relative to raw arrest data. Most notably, traffic crimes are by far more frequent than more "serious" crime. In addition, the enforcement of traffic related crimes promotes a large positive externality of considerably reducing traffic related fatalities, which account for 750,000 – 1,180,000 fatalities each year (Peden et al., 2004), whereas intentional homicides account for 468,000 fatalities per year (UNODC, 2010). The examination of more serious crimes (e.g. homicide, robbery, drugs, etc.) also convolutes the examination of the effect of age on deterrence, since most serious crimes result in incarceration. Thus, we cannot disentangle the impact of simply being older versus changes in the expected cost of punishment on the propensity to commit a proscribed activity. Lastly, the examination of speed related citations is a measure of egregiousness that is easily measured and verified. We proceed in the next section by providing background on the employment situation in Oregon.

Background

The state budget in Oregon has faced considerable pressure over the last decade. Oregonians passed *Measure 50* in 1997 that effectively limited the state legislature's ability to

[3] The use of changes in the size of the police force (due to grants, budget cuts, and the like) to examine the impact on crime has become commonplace (see Evans and Owens 2006, Levitt 1997).

change the tax rate without requiring a vote of approval from the citizens of Oregon. The inability of the state government to increase the income tax rate resulted in a sizeable budget deficit of $385.8 million, which was approximately 20.4-27.2 percent of the state budget in 2003.[4] In order to reduce the deficit and because the state already had almost $5 billion dollars in obligation bonds issued, the state had two choices: increase the state income tax or cut spending on public services.

As discussed in DeAngelo and Hansen (2010), *House Bill* 5100 was approved by Governor Kulongoski, which specified budget cuts to several government agencies should *Measure 28* not be approved. Table 1 is replicated from DeAngelo and Hansen (2010) and details the budget cuts that would result from the implementation of *House Bill 5100*. In short, *Measure 28* would have approved an income tax rate increase that would have offset the budget cuts that would be experienced by state agencies. In a record turnout to the polls, *Measure 28* failed to be passed on January 28, 2003. Four days later, the budget cuts set forward in *House Bill 5100* (and displayed in Table 1) were carried out, resulting in the firing of 117 of the 354 troopers that were employed at the time.[5] It is this reduction in the presence of law enforcement that we will exploit when examining the behavioral responses, by age, of drivers in the next sections of the paper.

Data

The exogenous change in the number of Oregon State Troopers provides a quasi-natural experiment to discuss the role that age might play in determining how individuals respond to enforcement tools. The reduction in troopers does not differentially affect specific age groups, resulting in a policy change that can examine how individuals of different ages respond to identical changes in law enforcement tools. We exploit this uniform variation in law enforcement presence in our analysis to investigate whether individuals respond differentially or not.

To conduct this analysis, data for traffic citations are obtained from the Oregon State Police, which include the citation speed, posted speed limit, date of birth, sex, and state of

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[4] See http://www.oregon.gov/DAS/BAM/docs/Capital_Investment/BondingPresentation2007Legislature.pdf?ga=t

[5] For a detailed discussion of the changes in Oregon's state budget as well as the nature of the layoff with regards which officers were laid off, see DeAngelo and Hansen (2010). For the purposes of this analysis it is most important to note that layoffs were based exclusively on seniority.

driver's license for the cited individual. We restrict our attention to the period 2000-2005, providing three years of data before and after the layoff discussed above.[6] We examine two different driver response measures to the laying off of the troopers: the number of citations and the difference between the citation speed and the posted speed limit (hereafter *speed difference*). Table 2 examines the number of citations per trooper and speed difference for the entire time period in addition to the pre/post-layoff periods.

The summary statistics in Table 2 display an interesting phenomenon that resulted from the layoff of the troopers. As expected, the average number of citations *decreased* after the layoff, but the average citations per officer increased. The composition of citations also remains relatively constant, with females receiving 30.4% of citations in the pre-layoff period and 31.9% of citations in the post-layoff period. Lastly, the average speed difference increased by approximately 2/3 miles per hour from before to after the layoff. The increase in the speed difference is of similar magnitude across males and females.

In what follows, we examine the role that age and sex play on driver behavioral responses to the layoff of troopers in Oregon. Before diving into the investigation of the role of the reduction in police presence on driver behavior, it is worth discussing the effect of law enforcement choices on the observed changes in driver behavior. Of notable concern is the potential confounding influence that changes in the behavior of law enforcement officials who remained employed might have on the individuals that received citations. For example, one might expect the officers that remain employed to face severe time constraints due to the reduction in fellow officers that result in less citations being issued. As a result, we might be concerned that the citations that they issue are only for the most egregious offenders on the road. This could be the case because, as noted in Table 2, both the number of citations and citations per officer decreased in the aftermath of the layoff. Moreover, we would expect speed difference to get larger (or at least remain constant given that fewer citations are actually being written). This is confirmed, as the speed difference increased by approximately 0.66 miles per hour. Thus, we cannot rule out the fact that officers might be citing the more egregious offenders with higher likelihood in the post-layoff period. To examine the effect that changes in law enforcement

[6] As discussed in Carpenter (2008) and Jackson and Owens (2010), alcohol related crime could convolute our analysis. As a result, we focus only on non-alcohol related crimes.

behavior have on the distribution of cited driving speeds, we include a placebo analysis at the end of the results section that attempts to mimic observed citation speeds for changes in law enforcement behavior, but without an actual layoff.

Figure 1 shows the "age-crime" curve by sex. For males aged 15-25, it appears that there are significant differences in the speed difference. However, these differences disappear from roughly age 35 onward. In addition, individuals aged 15-30 get apprehended for traveling 1.0-1.5 miles per hour faster than individuals over the age of 30, although the speed difference begins climbing again around age 70.

When we focus on the effect of the layoff on the speed difference (Figure 2), we observe a significant difference between the citation speed and speed limit. In fact, after the layoff has occurred, the speed difference is approximately 0.66 miles per hour larger at *every* age, with smaller difference at initial driving age and at the end of one's driving career.

In an attempt to determine whether male or female vehicle operators are the driving forces behind the observed speed difference, we examine the speed difference pre- and post-layoff for males and females separately, as seen in Figures 3a and 3b.

Figures 3a and 3b assist in disentangling the driving force behind the increase in the speed difference that was observed in Figure 2. Namely, the wedge in the speed difference curves is driven more by an increase in the speed difference for males when compared to females in the post-layoff period. However, for older drivers, female drivers tend to have a higher speed difference when compared to their male counterparts.

Tables 3 and 4 provide further empirical evidence for the shift in driver behavior. To start, Table 3 displays total citations by age. Drivers in their twenties and thirties tend to acquire the most citations, accounting for roughly 55 percent of all citations issued over the period 2000-2005. The majority of the citations that are given to individuals in their twenties and thirties can be further decomposed by gender. In fact, male citations account for 68 and 72 percent of all citations by individuals in their twenties and thirties, respectively. However, when comparing the number of citations by age group in the pre-layoff period to the post-layoff period, individuals in their teens – forties experienced the largest percentage decreases in citations, with 40, 37, 37, and 35 percent decreases, respectively. Finally, the composition of citations given to local drivers in the pre-layoff period versus the post-layoff period are essentially identical, indicating that the increase in citations is not due to increased traffic/speeding by drivers from neighboring states.

Table 4 proceeds to break down the difference between the citation speed and the speed limit. When we examine the speed difference across all drivers, we observe that teen and twenty year old drivers are cited at approximately 1-2 miles per hour higher speeds than their forty and fifty year old counterparts. Although there is little difference in the number of citations given to male and female drivers who are in their teens, the average citation speed is almost one mile per hour faster for male drivers. For thirty year old drivers, however, significantly more citations are given to male drivers despite very similar citation speeds. Interestingly, for drivers in their forties and fifties, males receive over double the number of citations that females receive, despite lower average citation speeds.

Table 4 also provides empirical support for changes in driver behavior that result from the layoff of the state troopers. Namely, we examine the average speed difference by age group in the pre- and post-layoff periods. The difference between the cited speed and posted speed when differencing the pre- and post-layoff periods is, on average, 0.6-0.7 miles per hour, regardless of the age of the cited driver. When we examine in-state drivers exclusively, we reinforce the general finding that the speed difference is 0.6-0.7 miles per hour for all age groups. Recall that this increase in citation speed is almost identical to the average increase in roadway speeds that were recorded by speed collecting devices throughout the state.

Results

Our econometric analysis aims to examine how the 35 percent reduction in Oregon State Troopers affected the driving behavior of Oregonians. We will be examining one dependent variable: the difference between the speed that an individual is cited for speeding and the posted speed limit. We will implement an OLS regression that examines the effect of higher law enforcement (pre-layoff) vs. lower enforcement (post-layoff) presence on the severity of crime (speed difference). Of main interest, however, we would like to explore whether different age groups respond differentially to the absence of law enforcement on roadways. We will estimate the following regression equation:

$$D_{it} = \alpha_0 + \beta_t layoff_t + \boldsymbol{\gamma_i I_t} + \boldsymbol{\delta_t x_t} + \boldsymbol{\varepsilon_{it}}.$$

In this equation D_{it} represents the speed difference for group *i* at time *t*, β_t represents the speed differential in time *t*, $\boldsymbol{I_t}$ is a vector of speed difference premiums that correspond to an interaction of being in the post-layoff period and age group *i*, and $\boldsymbol{\delta_t}$ represents a vector of returns to the characteristics in $\boldsymbol{x_{it}}$. To be clear, $\boldsymbol{I_t}$ includes $\{layoff_t * teen_t, layoff_t * twenty_t, \ldots, layoff_t * seventy_t\}$and $\boldsymbol{x_t}$ includes covariates such as trooper fixed effects, state level vehicle miles travelled, proportion of individuals wearing safety belts, number of licensed drivers over the age of 65 and under the age of 25, number of fatalities, incapacitating injuries non-incapacitating injuries, and accident related injuries in that county, state unemployment rates, county per capita income, and a dummy variable of whether the driver was an in-state driver or not. Other controls include county and month fixed effects as well as daily weather controls.[7]

Table 5 provides semi-elasticities for several specifications of OLS models that examine the effect of the layoff of police on the severity of speeding infractions. Column I runs a very simple regression that examines the effect of the trooper layoff on speed difference and finds that speed difference is approximately 2 percent larger in the post-layoff period. Additionally, being male does not necessarily lead to higher speed differences.

Column II takes a first step toward the main aim of this paper, which is to examine the potential differential effect of changes in law enforcement tools on different age populations. In this second specification, we include indicator variables for teens, twenty, thirty, fifty, sixty, and seventy year old drivers (forty year old drivers are the omitted group). We still find that speed difference is approximately 2 percent larger in the post-layoff period as compared to the pre-layoff period. Additionally, we note that the speed difference is approximately 10, 6 and 2 percent higher, respectively, for teenage, twenty, and thirty year old drivers relative to forty year old cited drivers.

Column III includes an interaction between the age indicator variable and the trooper layoff indicator variable in order to more closely examine the effect of the trooper layoff on specific age groups. In this specification, we find that the age effect overwhelms the layoff effect

[7] County unemployment rates were acquired from the American Community Survey, vehicle miles traveled, seat belt use, and driver age controls were obtained from the Oregon Department of Transportation, fatality and injury information was obtained from the Oregon Crash Analysis Reporting System and daily weather conditions were obtained from the National Climatic Data Center.

for all drivers. Stated differently, it appears that being a teenager, not less police presence, drives the higher speeding rates amongst teenage drivers. Similar stories can be told for each age group. Thus, the reduction in police presence does not appear to have a differential effect on age groups, as it appears that all age groups increase their driving speed quasi-uniformly in response to the reduction in police presence.

Decile Regressions

While the OLS regressions above do not display a relationship between a driver's age, the layoff and the severity of driving speeds, it could be the case that the interaction effect is not an average treatment effect. As described in the introduction, some individuals are acutely aware of law enforcement presence while others are impulsive, and this is likely to vary by age and egregiousness of the offender. To better examine whether or not driver behavior varies by age/egregiousness and with the layoff, we run decile regressions using specification III from Table 5. The results are reported in Table 6.

When examining the data at the decile level we continue to observe the significant layoff and age effects for all deciles. Interestingly, though, the age-layoff interaction coefficients are significantly different from zero for a subset of age-decile combinations. In particular, more experienced (30 and 50 year old drivers) and egregious (40^{th}-90^{th} percentile) offenders respond differentially to the layoff. Specifically, in the post-layoff period, thirty and fifty year old drivers that are more egregious offenders have a speed difference that is 0.5-1.5 percent faster than forty year older drivers in the layoff period.

As discussed in the introduction, there is significant research in neurology, psychology, criminology and economics supporting the hypothesis that young offenders are not particularly deterred by law enforcement, as they tend to be myopic. Alternatively, older offenders are more forward thinking and acutely aware of changes in law enforcement presence. Our empirical results are consistent with this hypothesis, as younger drivers consistently offend and do not seem to be particularly impacted by the layoff, whereas older, more experienced offenders appear to be aware of the layoff and respond with more egregious driving behavior.

Dry Road Citations

Our final specifications look exclusively at roadways that do not suffer from adverse weather conditions. More specifically, we only examine daily observations for which there is no reported snow or precipitation. The inclusion of driving days for which adverse weather conditions exist could bias our results downward, as police might be more inclined to write a citation at a lower speed due to the conditions.

We find evidence to support the claim that the inclusion of adverse weather observations in Table 7 decreased our semi-elasticities. All three columns of Table 7 show larger semi-elasticities on the layoff indicator variable. However, when we compare the age indicators in columns II and III of Table 7 to the same variables in columns II and III of Table 5, we do not observe significant differences. Once again we also find that the age-layoff interaction variables in column IX are insignificant and very similar in size to the same semi-elasticities in columns III and VI.

Testing the Stability of Age Distributions

In the previous sections, we demonstrated that the average change in the egregiousness of offenses following the mass layoff is stable across the age distribution for OLS regressions. While this is consistent with the homogeneity of deterability across the age distribution, it is estimated on the selected sample of individuals who chose to speed and were apprehended. Alternatively, the decile regression analysis displays heterogeneity of deterability across the age distribution, with more experienced drivers being more sensitive to the reduction in law enforcement presence. Finally, the behavior of individuals who choose to speed might be independent of age. Thus, the fraction of individuals choosing to speed following the layoff might vary with the layoff. In this section, we test the stability of the age distribution amongst speeders.

Figure 4 provides kernel density plots of the age distribution of cited individuals before and after the layoff, showing very little difference between the age distributions. In fact, in Figure 5 we graphically display the results of comparing the age-specific effects of the layoff to the average effect, which is approximately a 0.60 mile per hour increase in speed. We can reject the null hypothesis that the age-specific coefficient is different from the average effect of the layoffs for four age groups, or 6.7 percent of the time. This is approximately what one would expect if the null hypothesis that the effect across the entire age distribution is the same as the average effect.

Table 8 contains summary statistics for the distribution of ages both before and after the layoff of the state police. Panel A presents raw unadjusted (for underlying trends) statistics for the mean, 10^{th} percentile, 50^{th} percentile, and 90^{th} percentile while Panel B contains adjusted (with a linear trend) for similar sample characteristics. The unadjusted statistics reveal a slight to moderate increase in average ages, depending on the part of the distribution. On average, speeders pulled over by the police are 0.52 years older following the layoff. Examining percentiles, most of the increase in average age is driven by the upper percentiles of the age distribution, while low percentiles are not changing, which is reflected in the decile regressions. The shifting of the age distribution to the right is muted if the overall age distribution is adjusted using a linear trend (estimated only using the period prior to the layoff). Importantly, an increase in average ages might be driven by changes in demographics. Due to the aging of the baby-boomer generation and declining birth rates, shifts in the age distribution are to be expected.

While the above table contains evidence of a slight shift of key features of the age distribution after the layoff, the Kolmogorov-Smirnov (K-S) distributional tests offer a nonparametric method of testing distributional equality between two groups. We test the distributional equality before and after the layoff pairwise comparisons of adjacent years (for both unadjusted and trend-adjusted trends) in Table 9. A revealing pattern emerges. The K-S test rejects distributional equality for the pre- and post-layoff age distributions both for unadjusted and trend-adjusted ages. Comparing similar statistics for adjacent years that are both in the pre-layoff period, or adjacent years that are also both in post-layoff period yields similar results. Indeed, the comparison of 2002/2003 (the years immediately before and immediately after the layoff) yield coefficients that are on average no bigger than the test statistics comparing adjacent years that in similar pre/post layoff periods. This suggests that the differences in the age distribution before and after the layoff can likely be attributed to other underlying demographic shifts (e.g. aging, changing birth rates, etc.) rather than differential responses to the layoff across the age distribution.

Placebo Tests

In the above section we tested the age distribution amongst speeders and found that the age distribution of speeders has been changing over time, independent of the layoff. In this section, we address the fact that law enforcement behavior could have also changed with the layoff. In particular, we conduct a placebo test that examines the expected impact of changes in law enforcement behavior on the distribution of cited drivers.

To conduct the placebo tests, we narrow our analysis to examine only observations in our data prior to the layoff (January 2000 – December 2003). We then divide this data in half and treat the period January 2000–July 2001 as the placebo pre-layoff period and August 2001–January 2003 as the placebo post-layoff period. To simulate the behavior of law enforcement in the aftermath of the layoff, we assume that law enforcement officials are attempting to keep roadways as safe as possible by issuing citations to offenders and that a steady supply of driving offenders remain on the highway. Given these assumptions, it would seem that law enforcement will issue citations with a higher average speed difference.[8] On the other hand, if law enforcement agents attempt to issue more citations in order to account for the layoff, then this would reduce the average speed difference, assuming that driving behavior remains unchanged.[9]

To allow for our data to simulate the selection behavior of law enforcement agents we drop a random selection of citations with a speed difference less than 15 miles per hour in the placebo post-layoff period.[10] With the change in law enforcement behavior simulated in the data, we re-examine the decile regressions presented in Table 6. We are specifically interested in the

[8] Suppose that the driving speed of drivers is normally distributed about the speed limit with mean 0 and standard deviation of 10 miles per hour. Prior to the layoff, imagine that police are able to intercept the fastest 5% among speeding drivers. We would expect anyone driving more than 16.4 miles per hour over the speed limit to be cited, and the average citation speed to be 20 mph. Suppose driver behavior remains unchanged, but the number of available officers has been reduced by 35%, so that officers cite only the fastest 3.25% of drivers. Only driving that is more than 18.1 miles above the speed limit is caught, and the average cited speed would be 22 mph. Thus, although driver behavior remained constant, the speed difference increased. We thank an anonymous referee for pointing this out to us.

[9] From Table 2 we observed that police, on average, issued more citations and that the speed difference increased.

[10] To accomplish this task, we assign every observation a random number between zero and one. We then define another variable that takes on the maximum value $\{0, \frac{15 - speed\ difference}{15}\}$. The threshold of 15 miles per hour above the speed limit is a product of the data, as 10 percent of speed difference observations are smaller than 15 miles per hour. Thus, these are citations that are unlikely to be issued in the post-layoff period given the change in law enforcement behavior. We drop the observation in the placebo post-layoff period if the random number is smaller than the variable described above.

age-interaction layoff coefficients, as the placebo data now includes the likely change in law enforcement behavior that would have resulted from a decrease in law enforcement presence. Most importantly, though, by restricting the use of our data to only pre-layoff observations, our data should be free of any driver behavioral changes that would have resulted from the layoff of law enforcement. Table 10 presents the results of the decile regressions.

As expected, we find significant increases in speed difference in the post-layoff period for less egregious offenders (10^{th}-40^{th} percentiles). We also find that speed difference varied by age, which is consistent with the previous decile regression results. Given that no reduction in law enforcement actually occurred in this placebo analysis, we should expect to see no significance associated with the placebo layoff/age interaction variables. We confirm this expectation in our results, finding very few instances where the placebo layoff-age interaction variables are significantly different from zero. Additionally, the layoff-interaction variables that were most significant in Table 6 (older, more egregious offenders) are not significant at all in this analysis.

Figures 6a and 6b graphically confirm the results from tables 6 and 10, displaying a positive trend across deciles for the age-interaction coefficients of 20, 30 and 50 year olds in Table 6a. On the other hand, the placebo results (Table 6b) display a positive trend for younger drivers. These results are not significant, and they are entirely driven by the simulated decrease in citations issued to individuals that are marginal offenders. Thus, the observed positive trend in Table 6b is entirely driven by decreases in speed difference for lower deciles, which make speeds at higher deciles appear more egregious. Table 6a, however, does experience an increase in speed difference at higher deciles, which provides the upward sloping trend that is observed in the actual results.

Conclusion

The examination of the effect of age on an individual's propensity to commit a crime has been a central question of criminology almost since the beginning of the field (for a brief history, see Gottfredson and Hirschi 1990). Legal scholars have also considered the role of age in the construction of the criminal justice system, most notably with respect to the split between the adult and juvenile justice system (e.g. Feld 1998). Concerns that the ability to deter might also vary by age have been raised at least since Sampson and Cohen (1988). While there are reasons to believe that the age of an individual might impact their willingness to participate in a proscribed activity, economists have argued that if all individuals face similar incentives/dis-incentives to participate in an activity, then we should observe little difference in the response of individuals by age, ceteris paribus. Empirical research on this topic has been somewhat limited, at least in part due to the absence of credible data with age information across a wide portion of the age distribution. The one existing study in criminology by Sampson and Cohen (1988) found that youth are less deterrable by the threat of police than adults. Drago et al. (2009) found that ex-prisoners under the age of 32 are equally deterred by the threat of prison as older adults. Lee and McCrary find little evidence that delinquent youths are deterred by the move to the adult system in Florida, and Hjalmarrson (2009) finds evidence of only small changes in the perceptions of threat at the age of adulthood for a more general population. In contrast, Carpenter (2008) finds evidence that young adults change their behavior in response to the drinking age and drunk driving enforcement. Despite a less than sufficient treatment of this subject, policy debates about the efficacy of enforcement efforts for different age groups continue to grow (Secret 2011).

The aim of the current research is to examine the role that age plays, if any, when changes in the legal environment are not age specific. We find that all age groups respond to reductions in their likelihood of being apprehended, but older and more egregious offenders are more responsive to the decrease in law enforcement presence. Moreover, we confirm that the population of those apprehended remains stable over time, further reinforcing our results. Our results support the claim that individuals respond differentially to a change in the probability of apprehension by age and egregiousness, which bolsters findings about age-graded responsiveness to changes in the punishment. Recent research (Anwar and Loughran, 2011; Hansen, 2011) about penalties suggests there might be learning and indeed the benefits of information or learning could vary by age, and hence future work should also investigate heterogeneity across age groups for punishments. Additionally, future research in this area could be greatly improved by including additional information about the behavior of law enforcement agents and their interaction, or lack thereof, with individuals who would have been otherwise apprehended.

Tables

Table 1 details the budget cuts that Oregon faced as a result of the failure to pass Measure 28. While voters were required to approve or reject an income tax increase and did not have the ability to line item veto portions of the budget cuts, we provide this table in order to show that the only budget cuts that appear to impact whether or not a driver is apprehended is the reduction in budget to the Oregon State Police.

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*.

Table 2 provides monthly summary statistics for the number of citations before and after the layoff by county as well as a measure of the egregiousness of the citation, as measured by the speed difference.

Table 2: Summary Statistics for Citations, Citations per Officer,

and Speed Difference by County, 2000-2005

	All	Pre-Layoff	Post-Layoff
Average monthly	341.98	363.83	326.76
citations	(197.33)	(204.83)	(190.46)
Average monthly	236.87	255.07	223.86
male citations	(136.71)	(142.50)	(130.87)
Average monthly	106.86	110.64	104.38
female citations	(63.07)	(63.82)	(62.45)
Average monthly	25.66	24.47	26.49
citations per officer	(14.96)	(12.74)	(16.28)
Average monthly	25.67	24.49	26.52
male citations per officer	(14.98)	(12.78)	(16.32)
Average monthly	25.63	24.43	26.42
female citations per officer	(14.92)	(12.64)	(16.19)
Average Speed	19.831	19.572	20.232
Difference	(7.246)	(60.131)	(80684)
Average Speed	19.824	19.570	20.229
Difference, Male	(7.345)	(60.295)	(8.745)
Average Speed	19.844	19.576	20.239
Difference, Female	(7.015)	(5.723)	(8.550)

Standard deviations are reported in parentheses

Table 3 provides a breakdown of citations by age groupings, which are further broken down by sex, pre vs. post layoff, and for local (in-state) drivers only.

Table 3: Summary Statistics for Citations, 2000-2005

	Teen	Twenty	Thirty	Forty	Fifty	Sixty	Seventy
All citations	45,870	166,914	108,794	87,432	57,646	22,990	7,944
Male Citations	28,250	113,761	78,186	61,767	41,275	17,479	6,234
Female Citations	17,620	53,153	30,608	25,665	16,371	5,511	1,710
Pre-Layoff Citations	28,635	102,267	66,781	53,114	33,901	13,244	4,772
Post-Layoff Citations	17,235	64,647	42,013	34,318	23,745	9,746	3,172
Local Driver Citations	34,751	105,109	50,706	50,706	33,446	12,698	4,625
Local Driver Citations: Pre-layoff	21,838	64,471	39,071	31,148	19,669	7,320	2,869
Local Driver Citations: Post-layoff	12,913	40,638	24,333	19,558	13,777	5,378	1,756

Table 4 is quite similar to the previous table, but focuses on the differences between the citation speed and the posted speed limit.

Table 4: Summary Statistics for Difference between Cited and Posted Speed, 2000-2005

	Teen	Twenty	Thirty	Forty	Fifty	Sixty	Seventy
Speed Difference	21.305	20.348	19.600	19.150	18.991	19.041	19.473
	(8.182)	(7.610)	(6.712)	(7.513)	(6.618)	(5.044)	(4.993)
Male Speed	21.605	20.485	19.563	19.039	18.885	18.935	19.483
Difference	(9.269)	(7.802)	(6.816)	(7.284)	(6.218)	(5.145)	(5.105)
Female Speed	20.825	20.055	19.695	19.415	19.258	19.376	19.435
Difference	(6.012)	(7.173)	(6.437)	(8.033)	(7.526)	(4.696)	(4.563)
Pre-Layoff Speed	21.044	20.089	19.349	18.862	18.701	18.736	19.172
Difference	(6.296)	(6.336)	(6.338)	(6.069)	(5.180)	(4.921)	(4.916)
Post-Layoff Speed	21.739	20.757	19.999	19.595	19.405	19.455	19.926
Difference	(10.583)	(9.260)	(7.249)	(9.300)	(8.230)	(5.180)	(5.073)
Local Driver Speed	21.224	20.290	19.575	19.171	18.950	18.965	19.177
Difference	(7.826)	(7.264)	(6.037)	(7.992)	(6.411)	(4.800)	(4.777)
Local Driver Speed	20.979	20.031	19.334	18.895	18.662	18.657	18.900
Difference: Pre-layoff	(6.252)	(5.684)	(5.370)	(6.465)	(4.940)	(4.679)	(4.713)
Local Driver Speed	21.637	20.700	19.962	19.610	19.362	19.384	19.629
Difference: Post-layoff	(9.923)	(9.217)	(6.958)	(9.345)	(8.040)	(4.929)	(4.847)

Table 5 provides three OLS specifications of the impact of the reduction in the probability of apprehension on the egregiousness of the offense, as defined by the difference between the citation speed and posted speed limit.

Table 5: OLS Models of Driver Response to Changes in Enforcement Presence (Semi-Elasticities)

	Dependent variable: Speed Difference		
	I	II	III
Trooper Layoff	0.021***	0.022***	0.019***
	(0.004)	(0.005)	(0.117)
Teen	-	0.098***	0.100***
		(0.006)	(0.101)
Twenty	-	0.060***	0.062***
		(0.004)	(0.004)
Thirty	-	0.022***	0.024***
		(0.002)	(0.002)
Forty	-	-	-
Fifty	-	-0.008***	-0.008***
		(0.0002)	(0.002)
Sixty	-	-0.007***	-0.005**
		(0.002)	(0.002)
Seventy	-	0.014***	0.016***
		(0.005)	(0.005)
Teen Interaction	-	-	0.004
			(0.007)
Twenty Interaction	-	-	0.002
			(0.005)
Thirty Interaction	-	-	0.002
			(0.006)
Forty Interaction	-	-	0.006
			(0.005)
Fifty Interaction	-	-	0.004

			(0.005)
Sixty Interaction	-	-	-0.001
			(0.005)
Seventy Interaction	-	-	-
Male	-0.004	-0.001	-0.001
	(0.004)	(0.003)	(0.004)
County FE	X	X	X
Month FE	X	X	X
Trooper FE	X	X	X
Weather Controls	X	X	X
Other Controls	X	X	X
Observations	493,051	493,051	493,051

Weather controls include daily precipitation and snow at each location. Other controls include the state unemployment rate, number of vehicle miles traveled, estimate of state level safety belt use, and the number of drivers under the age of 25 and over the age of 65.

Standard errors are clustered at the county level.

*,**,*** denote significance at the 10, 5, and 1 percent, respectively.

Table 6: Decile Regression Model of Driver Response to Changes in Enforcement Presence

	10	20	30	40	50	60	70	80	90
Layoff	0.017*** (0.002)	0.023*** (0.002)	0.023*** (0.002)	0.022*** (0.002)	0.022*** (0.002)	0.017*** (0.002)	0.017*** (0.002)	0.022*** (0.003)	0.018*** (0.004)
Teen	0.050*** (0.003)	0.059*** (0.002)	0.063*** (0.002)	0.067*** (0.002)	0.071*** (0.002)	0.078*** (0.002)	0.095*** (0.004)	0.119*** (0.003)	0.143*** (0.004)
Twenty	0.038*** (0.002)	0.040*** (0.001)	0.038*** (0.001)	0.040*** (0.001)	0.038*** (0.002)	0.044*** (0.001)	0.049*** (0.002)	0.065*** (0.002)	0.077*** (0.003)
Thirty	0.018*** (0.023)	0.016*** (0.019)	0.014*** (0.001)	0.011*** (0.002)	0.011*** (0.002)	0.011*** (0.001)	0.014*** (0.001)	0.019*** (0.001)	0.024*** (0.002)
Fifty	-0.006** (0.003)	-0.004* (0.002)	-0.007** (0.002)	-0.012*** (0.002)	-0.012*** (0.002)	-0.012*** (0.002)	-0.011*** (0.002)	-0.013*** (0.002)	-0.019*** (0.002)
Sixty	-0.028*** (0.006)	-0.029*** (0.006)	-0.027*** (0.005)	-0.027*** (0.005)	-0.029*** (0.004)	-0.024*** (0.004)	-0.019*** (0.005)	-0.018*** (0.005)	-0.012 (0.007)
Seventy	0.035*** (0.005)	0.030*** (0.005)	0.022*** (0.004)	0.013 (0.004)	0.012*** (0.004)	0.009*** (0.003)	0.005 (0.004)	-0.001 (0.004)	-0.018*** (0.007)
Teen_int	0.001*** (0.003)	0.001 (0.002)	0.003 (0.002)	-0.005* (0.003)	-0.003 (0.003)	0.002 (0.003)	0.008* (0.004)	0.002 (0.004)	-0.004 (0.005)
Twenty_int	-0.002 (0.001)	0.001 (0.002)	0.003* (0.001)	0.002 (0.002)	0.003* (0.001)	0.002 (0.002)	0.008*** (0.002)	0.007*** (0.002)	0.007*** (0.003)
Thirty_int	0.001 (0.003)	0.005*** (0.002)	0.005*** (0.002)	0.010*** (0.002)	0.006*** (0.002)	0.010*** (0.002)	0.007*** (0.002)	0.010*** (0.002)	0.011*** (0.003)
Fifty_int	0.003 (0.003)	-0.004 (0.003)	0.002 (0.003)	0.008*** (0.003)	0.010*** (0.003)	0.007*** (0.003)	0.004 (0.003)	0.002 (0.003)	0.010*** (0.005)
Sixty_int	-0.006 (0.007)	0.001 (0.007)	0.011 (0.007)	0.009 (0.007)	0.016*** (0.006)	0.014*** (0.005)	0.013** (0.007)	0.012 (0.008)	0.003 (0.012)
Seventy_int	0.000 (0.007)	-0.001 (0.010)	-0.002 (0.005)	0.008 (0.005)	0.001 (0.005)	-0.001 (0.003)	-0.006 (0.005)	-0.006 (0.008)	0.009 (0.009)

Additional regressors include number of troopers, citations, gender, county and month controls, weather controls, unemployment rate, number of vehicle miles traveled, estimate of state level safety belt use, and the number of drivers under the age of 25 and over the age of 65.

Standard errors are clustered at the county level.

*,**,*** denote significance at the 10, 5, and 1 percent, respectively.

Table 7 examines citations that were issued on dry surface conditions, as these are the most likely conditions for state troopers to issue citations.

Table 7: OLS Models of Driver Response to Changes in Enforcement Presence on Dry Surface Conditions (Semi-Elasticities)

	Dependent variable: Speed Difference		
	VII	VIII	IX
Trooper Layoff	0.023***	0.024***	0.023***
	(0.004)	(0.004)	(0.007)
Teen	-	0.100***	0.097***
		(0.005)	(0.006)
Twenty	-	0.057***	0.060***
		(0.004)	(0.004)
Thirty	-	0.018***	0.019***
		(0.002)	(0.002)
Forty	-	-	-
Fifty	-	-0.013***	-0.012***
		(0.002)	(0.003)
Sixty	-	-0.009***	-0.006***
		(0.002)	(0.003)
Seventy	-	0.010*	0.012**
		(0.006)	(0.006)
Teen Interaction	-	-	0.002
			(0.007)
Twenty Interaction	-	-	-0.001
			(0.006)
Thirty Interaction	-	-	0.002
			(0.006)
Forty Interaction	-	-	0.006
			(0.007)
Fifty Interaction	-	-	0.002
			(0.006)

Sixty Interaction	-	-	-0.002
			(0.007)
Seventy Interaction	-	-	-
Male	-0.03	-0.001	-0.001
	(0.003)	(0.003)	(0.003)
County FE	X	X	X
Month FE	X	X	X
Trooper FE	X	X	X
Weather Controls	X	X	X
Other Controls	X	X	X
Observations	274,324	274,324	274,324

Weather controls include daily precipitation and snow. Other controls include the unemployment rate, number of vehicle miles traveled, estimate of state level safety belt use, and the number of drivers under the age of 25 and over the age of 65.

Standard errors are clustered at the county level.

*,**,*** denote significance at the 10, 5, and 1 percent, respectively.

Table 8: Summary Statistics for Age Distributions

	Before Layoff	After Layoff	Difference
Panel A: Unadjusted Distribution			
Average	35.3	35.8	.52***
			(0.04)
10 Percentile	19.9	20.0	0.024***
			(0.004)
50 Percentile	32.0	33.0	0.95***
			(0.007)
90 Percentile	55.0	56.0	1.01***
			(0.005)
Panel B : Trend-Adjusted Distribution			
Average	35.05	35.2	0.13**
			(0.004)
10 Percentile	19.6	19.3	-0.34***
			(0.007)
50 Percentile	31.9	32.3	0.46***
			(0.024)
90 Percentile	54.9	55.5	0.63***
			(0.03)

This table tests differences for key summary statistics of the age distribution before and after the layoff of state police. Panel A tests the raw, unadjusted age distribution while Panle B contains tests for a trend adjusted age distribution, using a linear trend.

*,**,*** denote significance at the 10, 5, and 1 percent, respectively.

Table 9: Kolmogorov-Smirnov Tests of Age Distribution Equalities

	K-S Statistic	P-Value
Panel A: Unadjusted Age Distribution		
Before/After Layoff	0.016	0.000***
2000/2001	0.005	0.168
2001/2002	0.011	0.000***
2002/2003	0.010	0.000***
2003/2004	0.006	0.147
2004/2005	0.016	0.000***
Panel B: Trend Adjusted Age Distribution		
Before/After Layoff	0.038	0.000***
2000/2001	0.041	0.000***
2001/2002	0.043	0.000***
2002/2003	0.038	0.000***
2003/2004	0.049	0.000***
2004/2005	0.040	0.000***

This table contains test statistics for Kolmogorov-Smirnov test for distributional equality before and after the layoff. Panel A contains test statistics and p-values for the raw, unadjusted age distribution while Panel B has results for a trend-adjusted age distribution, utilizing a linear trend.

*,**,*** denote significance at the 10, 5, and 1 percent, respectively.

Table 10: Decile Regression Model of Placebo Driver Response to Changes in Enforcement Presence

	10	20	30	40	50	60	70	80	90
Layoff	0.064***	0.034***	0.020***	0.009**	0.007*	0.002	-0.004	-0.001	-0.002
	(0.007)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)
Teen	0.060***	0.062***	0.069***	0.071***	0.077***	0.084***	0.096	0.122***	0.147***
	(0.005)	0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.007)
Twenty	0.040***	0.042***	0.041***	0.042***	0.040***	0.047***	0.046	0.066***	0.087**
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)
Thirty	0.020***	0.018***	0.017***	0.018***	0.016***	0.018***	0.016	0.022***	0.030***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Fifty	-0.009***	-0.002	0.001	0.000	-0.004	-0.003	-0.006***	-0.008***	-0.009***
	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)
Sixty	-0.031***	-0.030***	-0.021***	-0.022***	-0.022***	-0.023***	-0.009	-0.008	-0.014
	(0.011)	(0.010)	(0.007)	(0.008)	(0.007)	(0.009)	(0.007)	(0.010)	(0.013)
Seventy	0.036***	0.033***	0.021***	0.013	0.013	0.017	0.007	-0.001	-0.007
	(0.007)	(0.008)	(0.007)	(0.009)	(0.008)	(0.008)	(0.007)	(0.009)	(0.013)
Teen_int	-0.027***	-0.012***	-0.012**	-0.004	-0.002	-0.001	-0.000	0.002	0.009
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)	(0.005)	(0.005)	(0.009)
Twenty_int	-0.013***	-0.004	-0.002	0.003	0.004	0.005	0.007***	0.005	-0.002
	(0.005)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.006)
Thirty_int	-0.007	-0.005	-0.002	-0.003	-0.000	-0.002	0.003	0.002	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.005)	(0.005)
Fifty_int	0.006*	-0.002	-0.006***	-0.006***	-0.002	-0.002	-0.001	-0.001	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.005)	(0.006)
Sixty_int	0.010	0.001	-0.008	-0.008	-0.007	-0.006	-0.015	-0.013	0.008
	(0.014)	(0.015)	(0.011)	(0.010)	(0.009)	(0.010)	(0.010)	(0.014)	(0.015)
Seventy_int	-0.009	-0.003	0.005	0.012	0.008	-0.000	0.005	0.006	-0.012
	(0.010)	(0.013)	(0.008)	(0.011)	(0.010)	(0.011)	(0.009)	(0.012)	(0.015)

Additional regressors include number of troopers, citations, gender, county and month controls, weather controls, unemployment rate, number of vehicle miles traveled, estimate of state level safety belt use, and the number of drivers under the age of 25 and over the age of 65.

Standard errors are clustered at the county level.

*,**,*** denote significance at the 10, 5, and 1 percent, respectively.

Figures

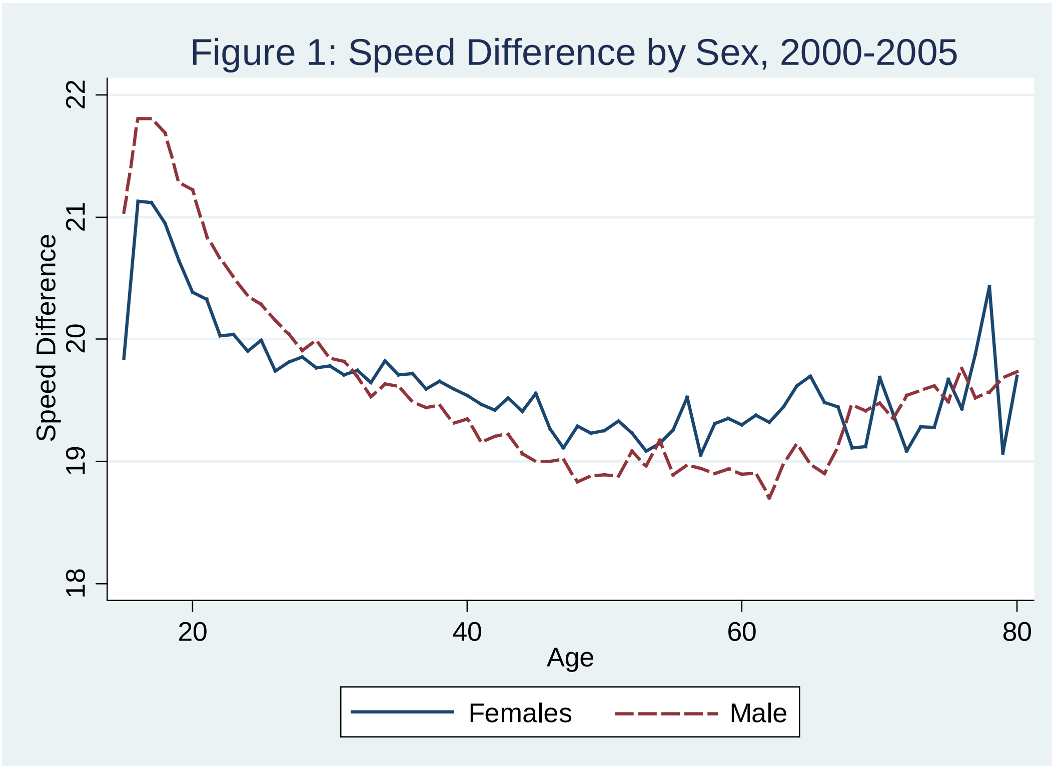


Figure 1: Speed Difference by Sex, 2000-2005
Speed Difference
18
19
20
21
22
20
40
60
80
Age
Females
Male

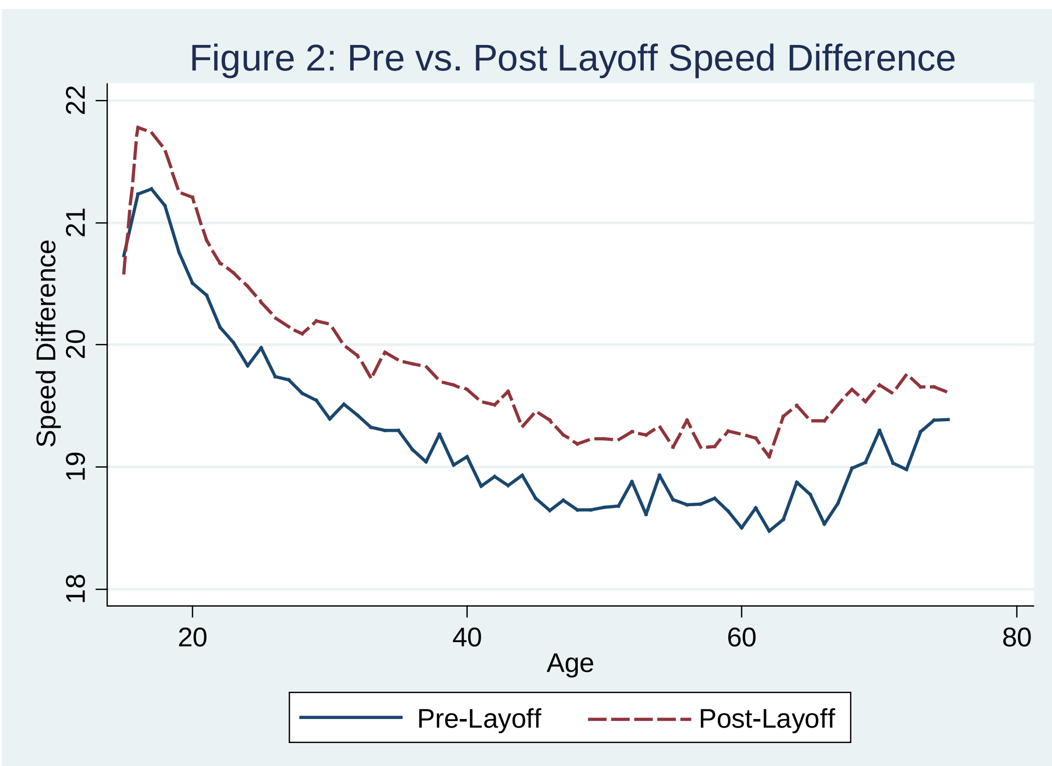


Figure 2: Pre vs. Post Layoff Speed Difference
Speed Difference
18
19
20
21
22
20
40
60
80
Age
Pre-Layoff
Post-Layoff

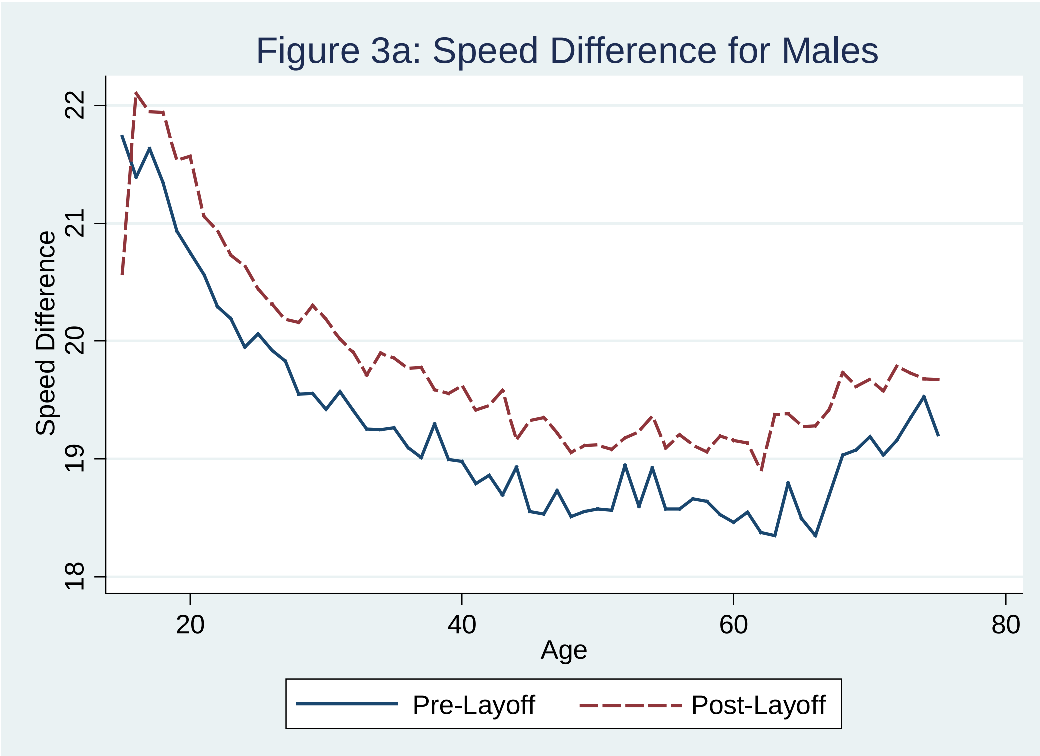


Figure 3a: Speed Difference for Males
Speed Difference
18
19
20
21
22
20
40
60
80
Age
Pre-Layoff
Post-Layoff

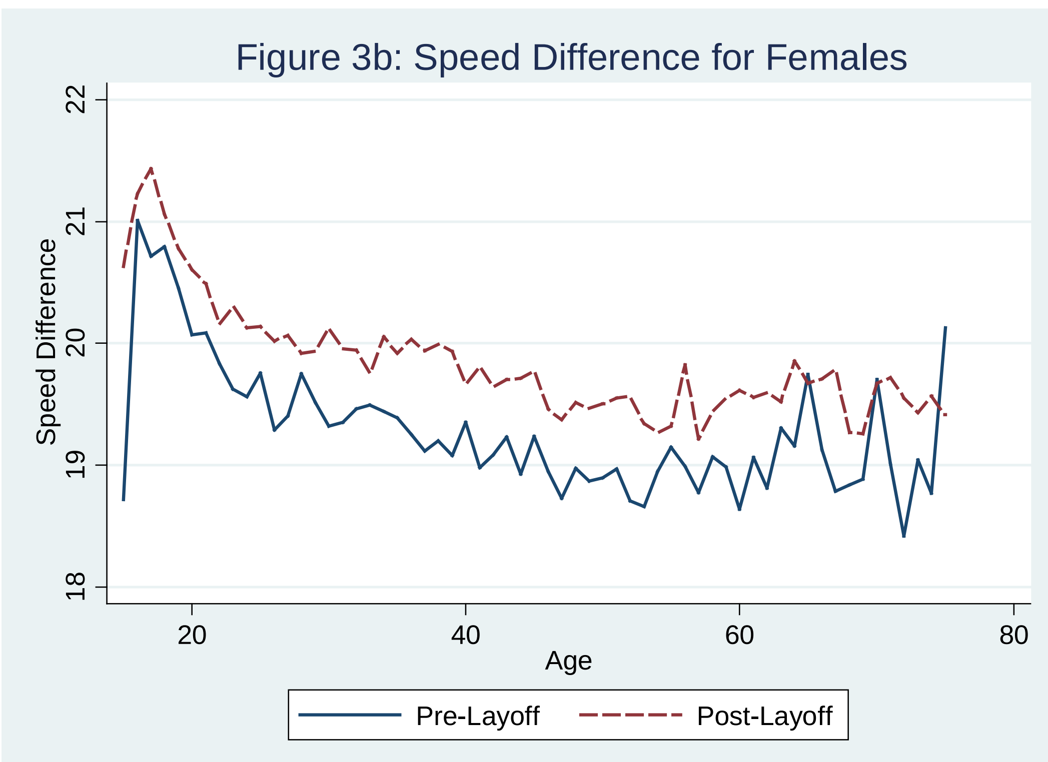


Figure 3b: Speed Difference for Females
Speed Difference
18
19
20
21
22
20
40
60
80
Age
Pre-Layoff
Post-Layoff

Figure 4
Age Distribution Before and After Layoff

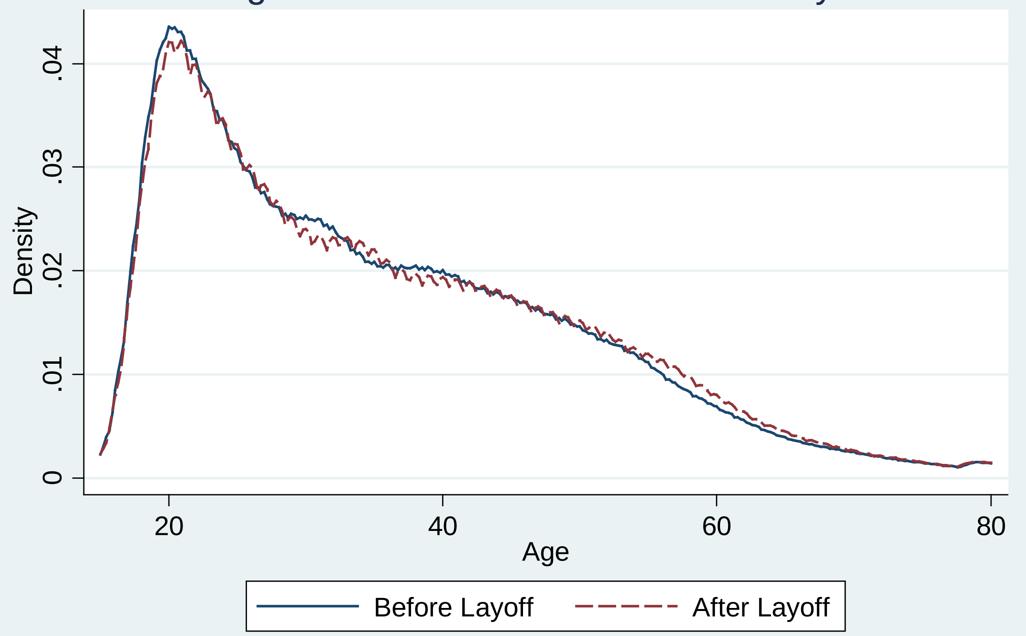
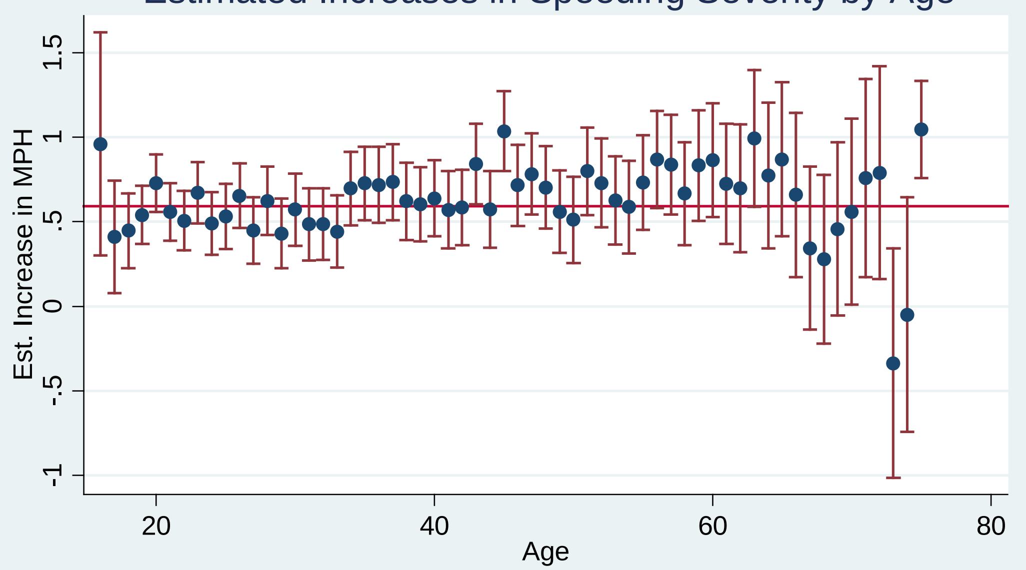


Figure 5
Estimated Increases in Speeding Severity by Age



This figure contains a plot of the estimated effect of the layoff on average speeds, along with 95 percent confidence intervals for each 1-year age bin from 16 to 75. The red-line represents the average effect over all ages.

Figure 6a: Age-Interaction Layoffs by Age

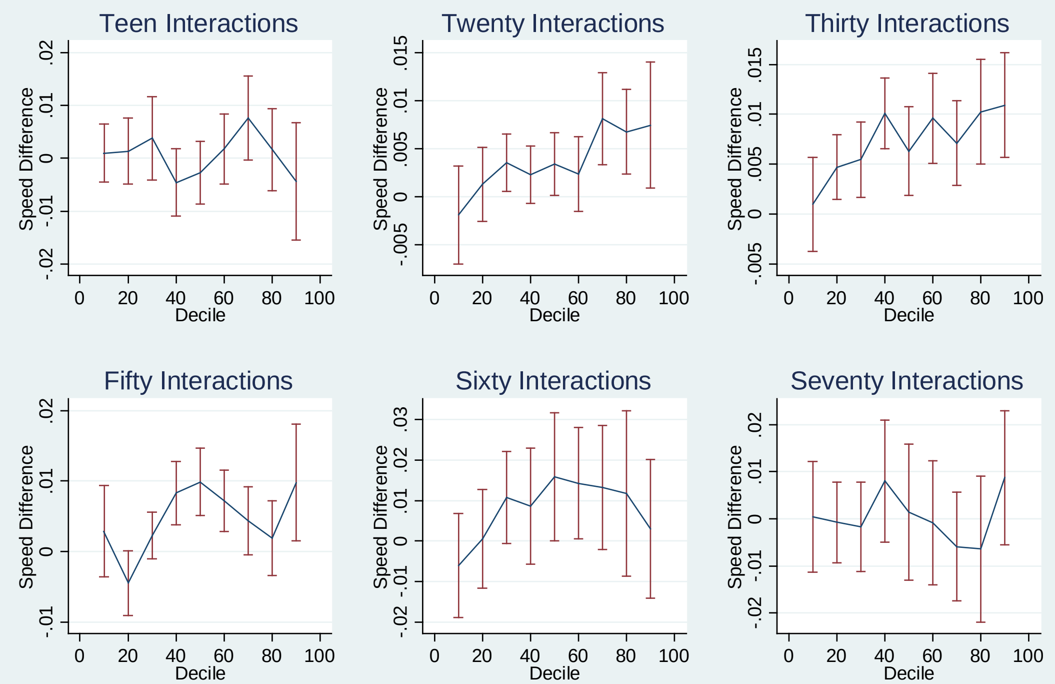
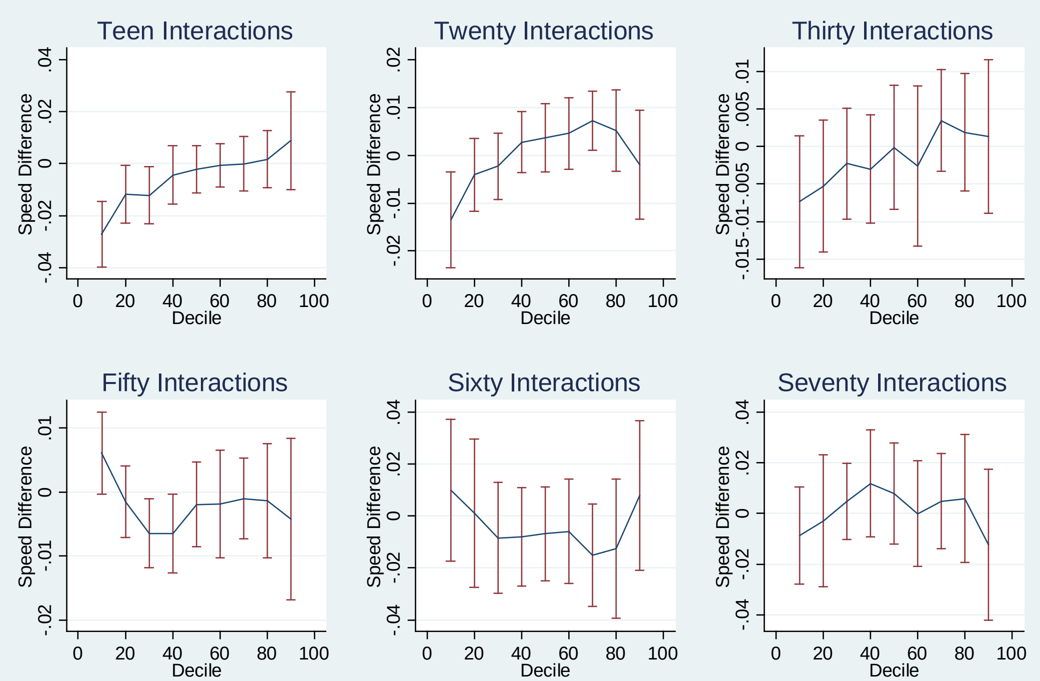


Figure 6b: Age-Interaction Layoffs by Age, Placebo Analysis



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