ESSAYS ON THE ECONOMICS OF JUVENILE CRIME AND EDUCATION

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ABSTRACT

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This dissertation consists of three independent chapters. The first chapter focuses on the effects of expungement of records of juvenile delinquency. Despite differing terminology, all fifty states and the District of Columbia have statutory remedies allowing records of juvenile delinquency to be treated as if they do not exist, eliminating the possibility that a future college or employer may learn of the record. Whereas most states require an application for such "expungement" of a juvenile record, in fourteen states the expungement is automatic. To study the effect of expungement on youths, I develop a conceptual model to consider the dynamic incentives created by automatic expungement that predicts an increase in the incentives to initially commit crime but a reduction in the incentives to commit additional crime as an adult. Using unique data I obtain from three application states, I show that expungement is rarely used when an application is required. Based on these statistics and predictions in the conceptual framework, I use survey data to estimate the effects of expungement on juvenile arrest, recidivism as an adult, educational attainment, and future labor market outcomes. I find no response to the incentive for first time offenders in automatic states, but I do find a negative effect on long-term recidivism. I also find modest positive effects of expungement on pursuit of higher education and future earnings. These findings suggest that expungement is socially beneficial with limited social costs.

The second chapter continues to focus on juvenile crime by studying the effects of Graduated Driver Licensing (GDL) laws on teenage crime. Although GDL laws were adopted to reduce the risk associated with novice driving, I investigate a different potential effect of these

laws: might the benefits of GDL extend beyond driver safety and also reduce juvenile crime? GDL laws effectively impose a statutory driving curfew and a limitation on the number of passengers in motor vehicles. Both the timing of motor vehicle access and a limitation on the peer influences available in a motor vehicle could significantly affect the set of potential offenders and the marginal costs for certain crimes. Using a differencing strategy based on the implementation of GDL, I find evidence that these laws reduce violent and property crime among 16 year olds. I then show that nighttime restrictions are the component of GDL most responsible for the reduction in crime. These results suggest that there is another benefit to states for adopting GDL laws and provide insight into the production of teenage crime.

The third chapter, co-authored with Leslie Papke, studies the response of young teachers to changes in their retirement compensation. Several states have recently enacted reforms in an effort to reduce their future pension obligations, but the vast majority of public school teachers continue to be covered by defined benefit plans. While these defined benefit plans' strong retirement incentives have been the focus of much research, we focus instead on the early years of a teacher's career. We illustrate state differences in the actuarial present value of a teacher's pension wealth upon vesting. Then, we show that pension characteristics relevant to the early years of a teacher's career are negatively related to the fraction of younger teachers in a state. Finally, we use data from the National Longitudinal Survey of Youth to study the first exit from teaching for new teachers. We find that pension parameters, such as vesting requirements and availability of defined contribution alternatives, are significantly related to first exit from teaching. Our preferred estimates indicate that young teachers are 11 percentage points more likely to exit teaching in a state that increases its vesting rule from five to 10 years.

For my family.

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CHAPTER 1

HAVE YOU EVER BEEN CONVICTED OF A CRIME? THE EFFECTS OF JUVENILE EXPUNGEMENT ON CRIME, EDUCATIONAL, AND LABOR MARKET OUTCOMES

1.1 Introduction

Since the first juvenile court began hearing cases in 1899, the overarching philosophy of the juvenile court system has been to focus on the offender as opposed to the offense. Consequently, the juvenile court tends to provide more rehabilitative sanctions than punitive ones and most states maintain confidentiality of juvenile court proceedings and records, presumably to limit the stigma associated with appearance in juvenile court (Bilchik 1999). However, state statutes determine whether these records can be obtained by anyone from employers in sensitive industries, such as nursing homes or school districts, to the general public.[1]

A unique feature of the juvenile court system is the process of expungement. The ability to expunge or seal one's juvenile record is a legal remedy available to juvenile offenders in every state.[2] Despite cross-state differences in terminology, juvenile expungement statutes contain a number of similar clauses.[3] Conditional on certain requirements, these statutes allow those with records of juvenile delinquency to have their records either closed from all inspection or

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[1] Anecdotally, many citizens first learn that their juvenile record followed them when they apply for public assistance or certain jobs (Whigham 2012; Quevedo 2013).

[2] While expungement is also available to adults in many states, I focus on juveniles for two reasons. First, expungement is typically made more difficult for adults through more stringent eligibility requirements. More importantly, crimes committed by adults are not covered by the same confidentiality provisions available to many juvenile offenders, particularly with respect to the publication of names. Therefore, the growth of the internet has called the effectiveness of adult expungement into question (Calvert and Bruno 2010).

[3] The terminology and specifics of these statutes differ by state. Other names include setting aside, destruction, expunction, erasure, and closing (see Table G.1). Despite the differences in terminology, these statutes all describe a process that results in the delinquent activity being legally treated as if it never occurred. I use the term "expunge" or "expungement" throughout the remainder of the paper as an umbrella for all the different terms.

physically destroyed. However, the manner by which expungement is initiated is quite different: whereas most states require a petition of the court to expunge a juvenile record, fourteen states automatically expunge the record. Once the record has been expunged the event can be treated as if it did not occur on college and employment applications, and a criminal background check will not return any juvenile history.[4]

The existence of a juvenile record is important because previous studies have found that a criminal history can be a barrier in many important economic markets. For example, prior literature documents that individuals with criminal histories can face struggles in the labor market (Grogger 1995; Pager 2003; Bushway 2004; Holzer et al. 2006; Holzer et al. 2007; Stoll and Bushway 2008; Finlay 2009). Criminal records can also affect the ability to gain higher education through eligibility for federal loans (Lovenheim and Owens 2014). The American Bar Association (2013) recently argued that the collateral consequences associated with having a record of juvenile delinquency can be more severe than the actual punishment for the crime, and further argued that expungement reduces these consequences. In response to these potential collateral consequences, a recent federal bill titled the Record Expungement Designed to Enhance Employment (REDEEM) Act (2014) attempts to make the process of expungement automatic for all nonviolent juvenile offenses.

This paper focuses on three primary research questions. First, what are the incentives that are created by automatic expungement? Second, do expungement rates differ for states with automatic expungement and states that require a petition? Third, is there an empirical effect of automatic expungement on crime, educational, and labor market outcomes? Answering these

[4] There are some exceptions to this claim. State law enforcement officials may have access to expunged records. For example, in many states if the offender later commits a felony the expunged record can be used for sentencing purposes.

questions, which have never appeared in the literature, makes this the first empirical paper to focus on juvenile expungement.

To answer these questions, I proceed in three steps. First, I develop a conceptual framework that captures the incentives created by a policy of automatic expungement in the market for crime. Specifically, my model predicts an increase in the propensity to commit juvenile crime and a decrease in recidivism as an adult in automatic expungement states. Next, I present unique data collected from state agencies on the number of annual expungements in three application states. These data indicate a large difference in the expungement rate between automatic and application states, presumably due to lack of knowledge, myopia, or the different costs associated with the application process. This allows me to infer that the policy effect of automatic expungement can be interpreted as the overall effect of expungement. I use this variation in my empirical work to identify the effects of expungement on crime, educational, and labor market outcomes.

As a preview of the empirical results, I find that automatic expungement does not affect the propensity to commit juvenile crime, but that it does lead to lower rates of recidivism, higher rates of college attendance and graduation, and higher average earnings for those with records of juvenile delinquency. One plausible explanation consistent with these results is that juveniles are unaware of expungement policies and their potential benefits. Overall, my findings suggest that there are large benefits to expungement with limited costs to society.

1.2 Institutional Details

A record of juvenile delinquency typically begins with an interaction with law enforcement.[5] Once the juvenile offender is in custody, prosecutors and the police determine whether to file a delinquency petition.[6] As defined by the federal Office of Juvenile Justice and Delinquency Prevention (OJJDP), "a delinquency petition states the allegations and requests the juvenile court to adjudicate (or judge) the youth a delinquent, making the juvenile a ward of the court. This language differs from that used in the criminal court system (where an offender is convicted and sentenced)" (2013b).

Each state also has provisions allowing a juvenile to be tried in the criminal court instead of the juvenile court, but use of these waivers is fairly uncommon. For example, in 2009 fewer than five percent of all drug, person, property, or public order cases involving juveniles were waived to criminal court (Puzzanchera et al. 2012).

A court appearance by a juvenile results in the production of an official court record. There are separate provisions regarding the treatment of juvenile records that depend on whether the juvenile is adjudicated delinquent. I focus on individuals that are adjudicated delinquent because these records have potential to be a significant barrier to future educational endeavors or employment.[7]

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[5] According to the Office of Juvenile Justice and Delinquency Prevention law enforcement referrals accounted for 83 percent of all delinquency cases referred to juvenile court in 2009. The remaining referrals were made by others such as parents, victims, schools, and probation officers (2013a).

[6] A petition is filed for all cases that appear in juvenile court. Cases that are not petitioned are diverted out of the official juvenile court system, either to a formal diversion program or by the juvenile simply being released to a parent or guardian.

[7] For example, the American Bar Association details the availability of juvenile records in different states to anyone from sensitive employers, to law enforcement officials, to the general public (2013). Regarding higher education, the Common Application asks college applicants to report if they have been adjudicated delinquent, but informs them that they are not required to answer "yes" if the adjudication has been expunged (Common Application 2014).

There are two mechanisms by which expungement can affect the application process for education or employment for those with a record of juvenile delinquency. First, in the majority of states expungement allows the underlying criminal activity to be treated as if it never occurred, meaning an applicant can legally respond to the question "Have you ever been convicted of a crime?" with "No."[8] Second, an expunged record will not be returned in a criminal background check if conducted by any employer or institution. Therefore, a nineteen year old who committed assault at age fourteen will have a record of juvenile delinquency if he lives in a state where it is not expunged, while a similar nineteen year old will have no record in a state where it is expunged.

While all states offer juveniles the option to expunge a criminal history, there is an important difference in the process that I use in my empirical work to identify the effect of expungement. Conditional on eligibility, fourteen states are automatic expungement states, meaning the criminal record is expunged at some point in the future with no action required by the juvenile.[9] The remaining states are application states, meaning a record of juvenile adjudication will not be expunged without a formal petition of the court.[10] This petition may require various costs, including knowledge of institutional details, hiring of legal counsel, and payment of administrative fees. Understanding the effects of automatic expungement is particularly important in the current atmosphere of juvenile justice reform. The REDEEM Act

[8] Some states statutorily define adjudication separately from conviction, meaning those with a record of juvenile delinquency can still respond "No" to this question.
[9] Eligibility for expungement varies by state. Some examples of eligibility requirements are age thresholds, remaining arrest free for a certain period of time, and providing evidence of rehabilitation. Statutory rules regarding expungement can also differ by crime within state. For example, in many states certain crimes are ineligible for expungement. These crimes are typically either violent in nature or require registration on an offender registry, as is the case with many sexual assaults.
[10] Some states require either application or the court's own motion. I label these states as application states.

(2014), currently a bill in the Senate, would make expungement automatic for all nonviolent juvenile offenses.

In Table G.1 I briefly summarize the pertinent expungement statutes in each of the 50 states and the District of Columbia. I present further descriptive comparisons between the language of the state statutes in Table G.2, including the number of states that specify the event can be treated as if it never occurred and the number of states where an expunged record can be used against an offender if he or she recidivates.

1.3 Literature Review

While the effects of expungement have not been empirically studied, several conceptual analyses exist. These papers typically argue the advantages and disadvantages of confidentiality of records and expungement for society (Gough 1966; Volenick 1975; Snow 1992; Funk 1995; Funk and Polsby 1998; Henning 2004; Ruddell and Winfree, Jr. 2006; Raphael 2007; Calvert and Bruno 2010; Pyne 2010; Weissman et al. 2010). The majority of these papers conclude that the benefits outweigh the costs and that society should make expungement easier for former offenders. However, Funk (1995) and Funk and Polsby (1998) warn that expungement could have large costs for first time offenders and for society more broadly if former offenders recidivate after the record has been expunged.

Other literatures provide insight into various effects that can be used to further understand the impact of expungement. For example, one pertinent literature investigates the causes of crime and recidivism. Expungement statutes alter the incentives for potential offenders by lowering the marginal cost of being caught committing a first offense as a juvenile. Prior economic literature shows that juvenile criminals may respond rationally to incentives (Levitt

1998; Jacob and Lefgren 2003; Conlin et al. 2005; Mocan and Rees 2005; Carpenter 2007; Lochner 2010). For example, Levitt (1998) shows that juvenile criminals are responsive to the severity of criminal punishment in their state of residence. However, other literature provides evidence that juvenile criminals appear to be myopic (Lee and McCrary 2005).

Another pertinent literature discusses the effect of interaction with the justice system on recidivism. Generally, this literature finds that formal labeling and incarceration can lead to increased rates of recidivism (Becker 1963; Bernburg and Krohn 2003; Bernburg et al. 2006; Kurleycheck et al. 2006; Lanctôt et al. 2007; Bayer et al. 2009; Wilson and Hoge 2012; Aizer and Doyle, Jr. 2013). This literature is particularly applicable because expungement directly removes the formal label associated with a record of juvenile delinquency.

Focusing on the long-term outcomes, much literature in criminology, sociology, and economics studies the effect of delinquent behavior and official court involvement on educational attainment (Tanner et al. 1999; Sweeten 2006; Hjalmarsson 2008; Merlo and Wolpin 2008; Burdick et al. 2011; Gowen et al. 2011; Aizer and Doyle, Jr. 2013; Kirk and Sampson 2013). Generally, these papers conclude that delinquent behavior, court appearance, and incarceration have negative effects on high school completion and college enrollment, depending on the severity of the involvement. For example, Hjalmarsson (2008) finds that individuals with convictions before age 16 are 16 percentage points less likely to graduate from high school. Coincidentally, using completely different data, Kirk and Sampson (2013) estimate that individuals who have been arrested are 16 percentage points less likely to enroll in college than otherwise identical individuals who have not been arrested. Tanner et al. (1999) find significant negative effects of contact with the criminal justice system on college graduation.

There are also studies, although fewer, on delinquency and labor market outcomes. Generally, adult workers who apply for employment with a criminal record can face significant scrutiny compared to their peers without a criminal history (Grogger 1995; Pager 2003; Bushway 2004; Holzer et al. 2006; Holzer et al. 2007; Stoll and Bushway 2008; Finlay 2009). Literature specific to juvenile offenders also confirms this result (Snow 1992; Tanner et al. 1999; Bernburg and Krohn 2003; Lanctôt et al. 2007; Gowen et al. 2011). These papers show that former juvenile offenders are more likely to be unemployed and have shorter job tenures, even ten or more years after the offense (Tanner et al. 1999).

1.4 Conceptual Framework

To consider how expungement affects the incentives to commit crimes, I construct a simple two-period model that captures the dynamic incentives created by expungement statutes for the criminal behavior of individuals, ignoring any potential reactions of the juvenile justice system, police, or the labor market. I briefly lay out the structure and implications of the model here; see Appendix G.2 for its complete development.

Suppose each individual has ability a, where a is distributed over (0,1). In the first period everyone is simultaneously enrolled in school and participating in the low wage labor market, earning salary S_1a. In the second period those individuals who have no criminal record move to the high wage market and earn S_2a, where $S_2 > S_1$. Therefore, this model assumes that having a criminal record results in a future labor market penalty. In thinking about this framework, one can equate the first period of the model with being a juvenile and the second period with being an adult.

In each period the individual can choose whether to commit a crime or not. I describe the crime decision in period t using the binary variable C_t, where $C_t = 0$ denotes choosing no crime and $C_t = 1$ denotes choosing to commit a crime. Assume that the individual earns his salary in each period whether or not he commits a crime and all individuals are caught committing a crime with probability q. If he succeeds in committing the crime without being caught the individual earns an additional payoff b. However, if he is caught committing a crime he has to give up a fraction of his salary f in that period.

I use this framework to assess criminal behavior under two different policy regimes: automatic expungement and no expungement. First, consider the regime with no expungement. Given the simple specification of this framework, there exists a unique cutoff value in a that separates the individuals into two distinct types: those who commit a crime in both periods and those who commit crimes in neither period.[11] Those who commit crimes never choose $(C_1, C_2) = (1, 0)$. The intuition for this result is apparent in the marginal benefits and costs. The marginal benefit from committing a crime is the same in both periods. However, the wage penalty associated with a criminal record implies that the marginal cost of committing a crime in the first period is larger than the marginal cost of committing a crime in the second period conditional on committing a crime in the first period. Therefore, one would never choose $(C_1, C_2) = (1, 0)$. Lastly, the human capital development aspect of the model, where second period earnings are greater than first period earnings if the individual is not captured committing a crime, implies that no one will choose $(C_1, C_2) = (0, 1)$.

[11] This finding, which is clearly unrealistic, is a result of the simplicity of the model. The model could easily be extended to allow for the other outcomes; for example, adding a period-specific idiosyncratic marginal benefit of crime would cause the other outcomes to be chosen as well. However, since my goal is only to understand the incentives, I keep the model simple.

Next consider the regime with automatic expungement. In this regime no one incurs the labor market penalty in the second period because no one has a criminal record. There also exist unique cutoff values in a with automatic expungement separating the individuals into three types: low a, medium a, and high a. As in the regime with no expungement, the individuals with low a choose to commit a crime in both periods. However, the removal of the labor market penalty changes behavior in two ways: it reduces the marginal cost of committing a crime in the first period and it increases the marginal cost of committing a crime in the second period conditional on committing a crime in the first period. This implies that individuals with medium a will choose $(C_1, C_2) = (1, 0)$. Lastly, as in the regime with no expungement, individuals with high a will choose not to commit a crime in either period.

Figure D.1 summarizes how criminal behavior varies with a across these two policy regimes. In the regime with no expungement, the individual's decision is entirely based on his ability relative to a_2. In the automatic expungement regime, the behavior changes as described above, but only between a_1 and a_3. Automatic expungement takes the individuals with $a_1 < a < a_2$, who commit a crime in both periods in the regime with no expungement, and creates an incentive for these individuals to choose $C_2 = 0$. Automatic expungement also takes the individuals with $a_2 < a < a_3$, who commit a crime in neither period without expungement, and creates an incentive for these individuals to choose $C_1 = 1$.

The model predicts that automatic expungement states will have higher rates of first time juvenile offense and lower rates of recidivism, where recidivism is defined as committing a crime as both a juvenile and an adult.[12] These predictions, which assume a rational, forward-

[12] As in footnote 11, the simplifying assumptions of this model imply the unrealistic finding that those with low a have perfect recidivism. However, the takeaway from the model is the reduction in recidivism due to automatic expungement, not the magnitude of this reduction.

looking juvenile criminal, remain largely unchanged if I instead assume juveniles know nothing about the possibility of expungement as long as the penalty imposed by a record of juvenile delinquency is sufficiently large. The only difference is that I would not expect to find a higher propensity to offend in automatic expungement states because the juveniles are not aware that expungement has reduced the marginal cost of offending. However, because of the hurdle created by a record of juvenile delinquency, I would expect the effects on earnings and recidivism to remain unchanged.

I proceed by empirically estimating the effect of automatic expungement on criminal behavior, the pursuit of education, and labor market outcomes. I can also test some of the assumptions of the model by analyzing the effect of automatic expungement on pursuit of higher education and future income. A comparison of these outcomes can provide the first evidence of the impact of expungement policies.

1.5 Data

The very nature of the expungement process presents a challenge for empirical work. No survey asks former offenders if they have had a record expunged, and some states do not keep administrative records regarding individual expungements. To obtain evidence on the usage of expungement, I contacted officials in the State Administrative Office of the Courts as well as the State Police or Criminal Justice Information System in all 50 states and the District of Columbia. In response to this inquiry, three application states (Colorado, Michigan, and Washington) were able to provide comprehensive aggregate statistics and one other application state (Maine) responded with anecdotal evidence.[13] I use these data to understand how often juveniles use expungement by application.

The primary data sources for the empirical work are the pertinent state statutes detailed in Table G.1 and the National Longitudinal Survey of Youth of 1997 (NLSY97). The NLSY97 is an annual longitudinal survey of 8,984 individuals who were between age 12 and 16 on December 31, 1996. The survey is unique in its collection of data related to crime. Each wave collects self-reported information about arrests, charges, convictions, and incarcerations, along with a rich set of demographic and economic information about the respondent and his or her family.[14] While self-reported data may suffer from underreporting bias, this bias will not affect my identification as long as it is uncorrelated with state expungement status.[15] Many previous studies use this dataset to analyze juvenile arrest and criminal behavior despite the data being self-reported (Levitt and Lochner 2001; Sweeten 2006; Lochner 2007; Hjalmarsson 2008; Merlo and Wolpin 2008; Hjalmarsson 2009; Finlay 2009; Brame et al. 2014; Lovenheim and Owens 2014).

Throughout my analysis of NLSY97 data I assume individuals have a record of delinquency if they report that they were convicted or adjudicated in juvenile court and their age at the time of survey is less than the age of criminal majority in their state of residence. During the years of analysis in this paper, the age of criminal majority is 16 in three states, 17 in ten states, and 18 in the remaining states.[16] This could raise concerns if the age of criminal majority

[13] Table G.3 presents all of the data I collected from various states. Note that this table includes some data from automatic states that reported statistics for expungements by application. Expungement by application is available in these states for those interested in expungement before the automatic process occurs.
[14] Thornberry and Krohn (2000) argue that self-report data on delinquency are valid for research purposes.
[15] There is no reason to suspect that reporting is correlated with expungement status because respondents are interviewed annually and eligibility for expungement typically takes longer than one year.
[16] There have been two recent changes: Connecticut raised its age from 16 to 18 (beginning to take effect in 2010) and Massachusetts raised its age from 17 to 18 in 2013 (Mendel 2013; OJJDP 2013b).

is related to the state's expungement status or if my results are being driven by variation in the age of the sample. As a result, I test the robustness of my primary results to changes in this assumption at the end of Section 1.7.

For the purpose of my analysis I assign the state of residence for the individual in 1997. State assignment is critical as it determines whether the individual lives in an automatic or application state. While this method of assignment ensures that the state of residence is known for all respondents, it could introduce bias if juvenile offenders are mobile across states, particularly if they commit a crime in a state other than their assigned state. My results are robust to a number of different assignment strategies, such as the state of residence in other years. The preferred assignment strategy results in 20 percent of the sample residing in automatic expungement states, consistent with the average fraction of the juvenile population that lived in automatic expungement states between 2006 and 2010.

I use data from a number of other sources to provide important covariates throughout my analysis. See Appendix G.4 for a discussion of these data sources.

1.6 Empirical Strategy

The preceding discussion highlights the importance of understanding the effect of expungement. However, the nature of the statues and available data limit the options for empirically estimating this effect. For example, only one state, Vermont, has changed from application to automatic status in the past thirty years, and data are not available for analysis around the timing of the change in 1995. Because of these concerns with identification, the typical empirical tools used to estimate clear causal effects are not suitable.[17] Instead, I use

[17] For example, an instrumental variables framework is not feasible as there does not appear to be a valid instrument -- something that affects expungement policy but not the other outcomes.

several simpler, but distinct, strategies that exploit cross-state variation to provide a collage of evidence on the effects of the policy.

1.6.1 Empirical Concerns

To focus the discussion regarding empirical concerns that exist with exploiting cross-state variation, consider the regression model:

$$y_{is} = \boldsymbol{\beta}\mathbf{X}_{is} + \gamma \text{Auto}_s + \rho \text{Justice}_s + \varepsilon_{is} \tag{1}$$

The outcome variable y_{is} contains measures of crime, educational, and labor market outcomes for individual *i* who lives in state *s*. The vector $\mathbf{X}_{is}$ contains race, ethnicity, gender, parental characteristics, and household composition, among other important predictors for the outcomes of interest. The coefficient of interest, γ, measures the effect of automatic expungement on the given outcome conditional on all of the other covariates. The other state level covariates labeled Justice_s reflect the unobserved juvenile justice environment. Some examples of the covariates that comprise this vector include the intensity of police scrutiny of teenagers, the severity of the punishments imposed by the juvenile justice system, and the likelihood a state forgives an individual who interacts with law enforcement.

One advantage of using the NLSY97 is that I have an extremely rich set of individual level covariates available to include in $\mathbf{X}_{is}$. Importantly for my identification strategy, these data allow me to control for underlying propensities to commit crime or succeed in the education and labor markets.

There are two major concerns that threaten estimation of γ, the first of which is reverse causality. More specifically, it may be the case that states with lower arrest rates choose to have more lenient expungement policies. This argument does not appear to be a major concern because many of the expungement statutes date back to the early twentieth century when juvenile crime rates were much lower. Despite fluctuations in the crime and arrest rates over time, virtually none of the statutes have been changed.

The second concern with this model is omitted variable bias. More specifically, $Justice_s$ in equation (1) is unobserved and likely to be positively correlated with $Auto_s$.[18] Quite simply, it is likely that states choosing to automatically expunge records of juvenile delinquency also focus their juvenile justice environment on maximizing the chance of rehabilitation. In such a case, if $Justice_s$ was not appropriately controlled for, the estimated effect of the automatic expungement policy would reflect the direct effect of the expungement policy as well as this other unmeasured juvenile justice environment.[19]

To provide more empirical evidence regarding these potential concerns, I compare observable covariates across automatic and application states in Table A.1. In the crime-specific covariates at the top of Table A.1 there are no significant differences in arrest or incarceration rates. Furthermore, while the arrest rate or incarceration rate is slightly higher in application states, in other categories, such as the violent crime rate or state expenditures on the justice system, the means are larger for automatic states.

Similarly, the bottom panel of Table A.1 shows that there do not appear to be significant differences in demographics and economic indicators between the states.[20] The only means that are statistically different from each other in Table A.1 are the fraction of the population that is

[18] The implication of this argument is that $Justice_s$ is the only unobserved covariate biasing estimation of γ. That is, conditional on the detailed covariates in $\mathbf{X}_{is}$, what remains in ε_{is} is not likely to be correlated with $Auto_s$.
[19] While one could tell a story where states with an automatic expungement policy tend to adopt a stricter juvenile justice system to offset this lenience, therefore suggesting that the correlation is negative, such an argument would seem to be more applicable when the policy environment was simpler, with perhaps just two or three policies counteracting each other.
[20] Figure D.2 shows that there does not appear to be any systematic geographical difference between the states. Additionally, although not listed in Table A.1, there do not appear to be any discernible political differences, as measured by the political party of the governor, senators, and other state officials, between the states.

black, which is larger in application states, and the fraction of the population that is Hispanic, which is larger in automatic states. The concerns created by these differences are diminished given the rich set of individual level covariates I include in my analysis. Therefore, the findings in both panels support the notion that automatic and application states do not appear to be systematically different.

1.6.2 Empirical Techniques

I use two different techniques to mitigate the concern of omitted variable bias. In the first technique I add a vector of covariates to my weighted least squares regression that are likely to be correlated with $Justice_s$. I use four distinct proxy variables. The first divides the number of juveniles in residential placement by the total level of reported crime to measure the severity of the state juvenile justice system as in Levitt (1998). I also include a measure of sentencing severity within state prisons and the state level imprisonment rate for adults.[21] The fourth variable, which I define as the forgiveness ratio, measures the propensity of a state to parole prisoners. I divide the number of released prisoners by the population in custody for each state. This measure focuses on the level of forgiveness within the state as opposed to the severity of punishment. My second technique identifies within-state treatment and control groups, allowing me to include state fixed effects in a difference-in-differences framework.

To implement the first technique I estimate a cross-sectional regression by weighted least squares and include the detailed covariates available in the NLSY97. Using these covariates as well as the results of Table A.1, where there do not appear to be systematic differences between the states, the remaining major concern is failing to capture the underlying juvenile justice

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[21] I measure sentencing severity as the fraction of state prisoners under jurisdiction with a maximum sentence of greater than one year.

environment. I compare the results of the regression without the proxy variables to those that include the proxies, thereby partially controlling for Justice$_s$. This technique also provides some insight into the degree of omitted variable bias, assuming the proxy variables are valid.

My second technique uses difference-in-differences to effectively remove all fixed attributes from equation (1), including Justice$_s$, by focusing on within-state variation. I include state fixed effects to compare individuals who have been convicted of juvenile crimes to their peers who have not been convicted within the same state.[22] This alleviates the concern of unobserved cross-state differences biasing the estimated effect of automatic expungement.

An example of this difference-in-differences strategy can be expressed as follows:

$$y_{is} = \boldsymbol{\delta_1 X_{is}} + \delta_2 \text{JuvConvict}_{is} + \theta[\text{Auto}_s \text{xJuvConvict}_{is}] + \tau_s + \omega_{is} \quad (2)$$

The outcomes I consider in this analysis are long-term recidivism, college attendance and graduation, and average future income.[23] I define $\mathbf{X}_{is}$ in equation (2) as in equation (1). In this framework the coefficient of interest is θ, the coefficient on the interaction between living in an automatic expungement state and being convicted in a juvenile court.

The key aspect to the validity of this strategy is selection of the control group. A potential concern with this method is that the effects of the juvenile justice system may differ for these groups and therefore not be captured by this technique. The strength of the assumption of constant effects of Justice$_s$ across treatment and control group varies by the outcome I use. For example, consider the market for higher education. It seems plausible that the effects of the

[22] I use the term "convicted" when working with the NLSY97 because it is the term used in the survey. In the text of the question itself the survey is specific in asking if the respondent was either convicted or adjudicated.

[23] Because the treatment and control groups are defined by arrest and conviction, I am unable to use this technique to analyze the probability of initial arrest. I measure long-term recidivism using an indicator for ever being arrested after age 20. I choose age 20 because this will allow sufficient time for individuals who are incarcerated as juveniles to be released. According to the Census of Juveniles in Residential Placement, in 2010 the median range of days since committed individuals had been admitted was 91 to 180 days (Sickmund et al. 2013).

juvenile justice system are similar for those who are convicted and those who are arrested but not convicted. However, in the market for long-term recidivism this assumption is much stronger.

I test the robustness of my results to different treatment and control groups to alleviate this concern. For example, consider a change of the treatment group to individuals arrested but not convicted. These individuals likely interact with a similar juvenile justice environment as those who are arrested and convicted, but expungement should not have an impact on their outcomes. Therefore, if I use those arrested but not convicted as the treatment group and those never arrested as the control group, I change the expected results of the analysis holding constant the unobserved juvenile justice environment. If the results of this analysis, where I expect no effect, are similar to the analysis using juvenile convicts as the treatment group, this would be evidence that the effect I am capturing is due to the unobserved juvenile justice environment and not to expungement. However, finding a large effect for those convicted but zero for those arrested and not convicted would be compelling evidence that I am capturing the effect of expungement.

For all empirical analyses my preferred calculations of standard errors are clustered at the state level to correct for the within-state correlation that exists in my data (Donald and Lang 2007). However, in some cases this causes the standard errors to shrink. Therefore, I also present non-clustered standard errors in Appendix G.6 for all key results.

The nature of the sampling framework used by the NLSY97, where black and Hispanic respondents are oversampled, implies that the sampling is endogenous because race is a significant predictor of arrest. As a result, I present weighted estimates in all analyses to ensure consistency (Solon et al. 2013).[24] I report unweighted analogs of the primary findings in Appendix G.6.

1.6.3 Do Juveniles Apply For Expungement?

In Table A.2 I provide the years of data that are available and average annual expungements I collected from the application states. To interpret the data more easily, I include the average number of cases handled formally in each of the states over the years 1997 to 2010. This is a measure of the amount of court activity that leads to the production of records of juvenile delinquency. I calculate the expected adjudication rate by multiplying the average number of formally handled cases by 60 percent, the approximate rate for petitioned delinquency hearings to result in adjudication since 1985 (Puzzanchera et al. 2012). Dividing expungements by expected adjudications gives a rough estimate of a rate of expungement of records for each state.

Table A.2 shows that rates of expungement are extremely low in states that do not allow for automatic expungement, both in raw levels and as a percentage of expected adjudications. I estimate that the average expungement rate among these three application states is between 0.2 percent and 10.7 percent. Additionally, although unable to provide statistics, a representative from the Maine Juvenile Justice Advisory Group informed me that leading juvenile prosecutors in Maine recalled handling fewer than 50 motions to expunge juvenile records during the past 20 years (K. McGloin, personal communication, August 26, 2013).

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[24] In particular, the NLSY samples 100 primary sampling units (PSUs) in the cross-sectional sample and 100 PSUs in the oversample, with only 147 of the PSUs not overlapping between the two. The nonrandom nature of the oversampling requires weighting for consistency (National Longitudinal Surveys 2014).

There are multiple explanations for this finding. One possibility is that the monetary and non-monetary costs associated with application for expungement are too high, deterring individuals from applying.[25] Another possibility, consistent with findings in the literature specific to youth, is that juveniles are extremely myopic (Lee and McCrary 2005; Oreopoulos 2007). Thus, they choose not to apply for expungement because the benefits of such application will not be realized until much later in their lifetimes.[26] A third explanation is that juveniles are unaware of the expungement laws in their state, particularly in application states. This explanation is most consistent with the low rate of expungement in application states. If former offenders knew about expungement statutes, I would expect to find more juveniles applying for expungement upon learning that their record of juvenile delinquency prevented them from gaining employment or education.

In the remainder of the paper I directly examine the overall policy effect of a state adopting automatic expungement. Importantly, the empirical results in Table A.2 imply that the rate of expungement in application states is near zero, suggesting that this policy effect is approximately equivalent to studying the effects of expungement itself.

[25] For example, some states require affidavits from the applicant reflecting his or her behavior as well as affidavits from others regarding the character of the applicant. There are also direct monetary costs, such as remittance of court fees or hiring of legal assistance.
[26] Myopia does not explain why former offenders do not apply for expungement when they are older. One explanation for this result is that the negative effect of a juvenile record slowly diminishes over time. Another is that former offenders never revisit their decision not to pursue expungement.

1.7 Results

The NLSY97 data I use contain 1,267 individuals who were arrested as a juvenile, 779 juveniles who were charged, 403 juveniles who were convicted, and 181 juveniles who were incarcerated. Table G.4 provides additional descriptive statistics for the overall sample.[27]

In Table A.3 I focus on the differences in the means of important covariates across automatic and application states for those never arrested, those arrested but not convicted, and those convicted as a juvenile. First, I compare the probability that a respondent reports being arrested as a juvenile in automatic and application states. I also report the probability that the respondent is convicted in juvenile court conditional on having been arrested. While the rate of arrest appears to be slightly higher in automatic states, there is no statistically significant difference in the reported conviction rate. This finding helps to alleviate the concern that states may be endogenously determining their juvenile conviction rates in response to their expungement policy.

Among those individuals who were convicted of a crime, the descriptive statistics suggest that outcomes in automatic expungement states are consistent with the conceptual framework; average rates of recidivism are smaller in automatic states, while rates of college attendance, college graduation, and average future income are all larger in automatic states than in application states. Furthermore, the means for these variables in the automatic states are very similar to the means for those arrested but not convicted. These findings are consistent if automatic expungement serves to increase the means of these variables and the policy itself is exogenous to these outcomes.

[27] I drop 1,515 observations from the original sample. These individuals missed at least one of the first five waves of the survey, and I am therefore unable to determine if these individuals had an arrest as a juvenile. These statistics are weighted by the NLSY97 sampling weights for 1997 that use the cumulative cases method. This method provides a weight for everyone in the sample and adjusts for the oversampling of blacks and Hispanics.

In Table A.4 I compare the baseline difference in the probability of juvenile arrest among the respondents in the NLSY97 from the different states. Each of the columns of this table report results from a linear probability model estimated using weighted least squares where the outcome is a binary indicator of ever being arrested as a juvenile. In column (1) I present results from a regression using all of the detailed NLSY97 covariates but excluding the vector of proxy variables. In column (2) I add the proxy variables to the regression to determine the level of concern raised by the unobserved juvenile justice environment. In columns (3) and (4) I repeat this exercise, but I also include a standardized measure of ability, the Armed Services Verbal Aptitude Battery (ASVAB), which contains the Armed Forces Qualifying Test (AFQT).[28] This examination is used in previous literature as an underlying measure of respondent's ability (Neal and Johnson 1996).

Across all columns I find no statistically significant effect of automatic expungement on juvenile arrest in this sample.[29] Although the direction of the estimated coefficient is positive, the magnitude of this effect is very small. Assuming that I use feasible proxy variables for $Justice_s$, the lack of a significant change from column (1) to column (2) and column (3) to column (4) suggests that the unobserved juvenile justice environment is not a big concern in this

[28] The ASVAB was administered voluntarily in the first wave of the NLSY97. As a result, ASVAB scores are missing for many of the individuals who are arrested as juveniles. When I include ASVAB in the analysis I also include an indicator for ASVAB being missing. However, this fundamental difference in the sample with ASVAB scores affects both the magnitude and the interpretation of the ASVAB estimate. Another measure of ability that is available is self-reported eighth grade achievement. Respondents in the NLSY97 are asked to report their grades in eighth grade as "mostly As," "about half As and Bs," "mostly Bs," and so on. If I define good grades as receiving mostly Bs or better and include this indicator in the analysis instead of ASVAB, the results are generally similar.

[29] Tables G.6 and G.7 show similar analysis using arrest rates from the Uniform Crime Reports. The estimated coefficients are from a regression of average arrest rates for the specified population for specific crimes over the years 2006 to 2010 on a number of pertinent state level covariates. The crimes in the eight columns are ordered in terms of likelihood to be expunged. Therefore, this analysis determines if juveniles are committing less serious crimes at differential rates between the states, possibly as a result of the incentives created by expungement statutes. This also tests the predicted unconditional reduction in second period crime from the conceptual framework by using adult arrest rates. The estimated coefficient on the automatic identifier in these tables is never statistically significant at conventional levels, indicating that the arrest rates for all of the crimes are not different across automatic and application states.

analysis once I have conditioned on the rich set of covariates. This result continues to support the finding that there are not large differences between automatic and application states other than their expungement policy. This finding also conflicts with the predictions of the conceptual framework, where I predicted that there would be higher levels of juvenile crime in automatic expungement states. However, this result is plausible if juveniles are unaware of expungement policies in their state of residence.

The signs and significance of the other covariates in Table A.4 are generally consistent with expectations and previous studies. The small, insignificant effect of black is surprising given the national trend in arrests showing black juveniles arrested at much higher rates than whites (OJJDP 2013a). However, this is not the first paper to find that the difference in arrest rates across races appears to be much smaller in the NLSY97 than in national statistics (Brame et al. 2014).[30] Additionally, other studies have found that conditioning on important covariates, such as family socioeconomic status, makes the effect of race insignificant in determining risk of juvenile arrest (Fite et al. 2009).

After analyzing the effect of expungement on arrest propensities, I shift the focus of the analysis to long-term outcomes for former offenders. First, to provide a baseline estimate for the negative effects of juvenile arrest and conviction on these outcomes, I estimate these effects for the entire NLSY97 sample in Table A.5. Each column presents the estimates from a regression of the outcome of interest on the same set of covariates as column (2) of Table A.4. However, instead of including an indicator for automatic expungement, I include an indicator for juvenile arrest in the top panel and an indicator for juvenile arrest and juvenile conviction in the bottom panel. The results confirm the negative effects of juvenile arrest and conviction across the

[30] A comparison of means test shows that black juveniles are arrested at a significantly higher rate than white juveniles in the NLSY97.

different outcomes and provide magnitudes that can be used for comparison with the results for expungement. For example, in the top panel I estimate that individuals who are arrested as juveniles are 18.2 percentage points more likely to be rearrested after age 20. The bottom panel shows that this rate is 15.0 percentage points for those who are arrested but not convicted, while the rate is 25.1 percentage points for those who are arrested and convicted.

Once I establish the negative effects of juvenile arrest and conviction, I focus on estimating the effect of expungement. In Table A.6 I present the results of weighted least squares regressions using dependent variables that reflect the long-term costs and benefits associated with expungement in my conceptual framework.[31] Each panel of Table A.6 contains the results from estimation of equation (1) for a different subset of the population, including those who are convicted as a juvenile, those who are arrested but not convicted, and those who are never arrested.[32] Columns (1) through (4) measure reduced recidivism, pursuit of higher education, and legal employment, where these outcomes are defined such that positive results would be considered social benefits. As in columns (2) and (4) of Table A.4, these estimates include the proxy variables to control for unobserved differences in the juvenile justice system, and the coefficients of interest are those on the indicator for automatic expungement.

The coefficient in the top panel of column (1) shows that individuals convicted as a juvenile who live in an automatic expungement state are 14.3 percentage points more likely to remain arrest-free after age 20, with this coefficient statistically significant at the five percent level.[33] This result is consistent with the prediction in my conceptual framework that automatic

[31] The unweighted analogs to Tables A.6 and A.7 appear in Tables G.8 and G.9.
[32] I cluster standard errors at the state level in Tables A.6 and A.7. Tables G.10 and G.11 show the standard errors for Table A.6 and Table A.7 without clustering.
[33] One concern with this analysis is that individuals who are incarcerated for long periods may be incapacitated, resulting in no future arrests. However, including an indicator in the regression for ever being incarcerated does not change the results.

expungement causes a reduction in crime in the second period. It is reassuring that I do not find this reduction among those who are arrested and not convicted or those who are never arrested, as the incentive created by expungement should not affect these populations.

The next two long-term outcome variables are educational outcomes. College attendance, the outcome variable in column (2), is defined by one's response to his or her highest grade completed as "first year of college" or more.[34] The outcome variable in column (3) is an indicator for college graduation defined as receiving a Bachelor's Degree or higher. These outcomes are important for two reasons: first, a record of delinquency may need to be disclosed in the college application process, affecting the probability of admission for former delinquents, and second, having a record of juvenile delinquency can affect the incentives to invest in human capital development. The estimated coefficients imply that living in an automatic expungement state increases the probability of college attendance for juvenile convicts by 7.7 percentage points and college graduation by 5.1 percentage points, although neither estimate is statistically significant at conventional levels. Again, the findings for the other two panels are close to zero and not statistically significant.[35]

In column (4) I further extend the analysis of long-term outcomes to the labor market. To understand the effects of a record of delinquency, I focus on the natural logarithm of average income between 2008 and 2010, when the average age among the respondents is 25.8 to 27.8.[36]

[34] In unreported results I do not find an effect of expungement on high school graduation. While one can imagine a story where a teenager who is convicted in an application state responds by dropping out of high school, this story is not apparent in the data.
[35] The marginal significance in the bottom row of column (3) is puzzling. However, the negative coefficient implies that those who are never arrested may be less likely to graduate college in automatic expungement states, strengthening the interpretation of the positive, albeit insignificant, coefficient in the top row.
[36] The timing of this analysis, when many of the respondents have not yet reached age 30, implies that this measure of current income may not be a good proxy for permanent income (Haider and Solon 2006). I use the income measure over multiple years to draw conclusions about labor market implications, not to make statements about permanent income.

The results of this analysis suggest a positive effect of automatic expungement on average income for those convicted as a juvenile. The reported coefficient on income implies that, among those with a record of juvenile delinquency, individuals who lived in automatic expungement states earned 25.3 percent higher income, on average, between 2008 and 2010 than those who lived in application states. Some or all of this difference may be driven by the difference in college attendance, as it is a well-documented fact that the earnings profile of individuals with a college education, even early in one's career, is much higher than those who never attend college (Chenevert and Litwok 2013). While the coefficient on average income in the top panel is not statistically significant at conventional levels, the magnitude of this coefficient is much larger than the estimate in the other two panels, where I do not expect to find an effect.

Generally, the results in Table A.6 show strong, compelling results for a reduction in recidivism, as I predicted in the conceptual framework. Furthermore, if I include the ASVAB measure from Table A.4 the magnitude for recidivism does not change and remains statistically significant. The effects on income and education remain positive and not statistically significant when I include the ASVAB measure, although the education effects are slightly smaller in magnitude.

In Table A.7 I turn to the difference-in-differences identification strategy. In each panel of Table A.7 I specify a different treatment and control group and report estimates of equation (2). In the first two panels, where juvenile convicts are the treatment group, I would expect to find an effect of expungement. The different control groups provide robustness for the assumption that the unobserved juvenile justice environment affects the treatment and control group equally. The bottom panel of Table A.7 acts as a falsification exercise for this analysis while holding the juvenile justice environment fixed. The coefficients of interest in this table are the coefficients on the interaction between either juvenile conviction or juvenile arrest and living in an automatic expungement state.

In column (1) the effect of expungement on future arrest remains statistically significant at conventional levels, implying either a 15.3 or a 12.0 percentage point increase in the probability of remaining arrest-free after age 20, depending on the control group. Thus, consistent with my conceptual framework, there remains supportive evidence that expungement of a record has an effect on future criminal behavior.

The positive estimate on educational outcomes in Table A.7 is fairly similar to Table A.6. Taking all of these results together, I find that automatic expungement raises the rate of college attendance among former juvenile offenders by approximately five to eight percentage points, although this is not statistically significant at standard levels. Similarly, although the direction of the coefficient is still positive, there is not a statistically significant effect on college graduation across the two tables.

Moving to labor market outcomes, there remains a large difference in average income of either 27.6 or 22.5 percent, depending on the control group. These estimates are similar in magnitude to the estimate in Table A.6. However, unlike the estimates in Table A.6, the coefficients in Table A.7 are both statistically significant.

Comparing the estimated effect for juvenile convicts to the control group in the first two panels of Table A.7 provides a simple plausibility check. Despite the consistent finding of positive effects across the columns, the magnitude of the estimates on the interaction terms is almost always smaller than the primary effect of juvenile conviction. For example, while juvenile convicts in automatic expungement states may be 8.6 percentage points more likely to attend college than their peers who were never arrested, the primary effect of being a juvenile convict suggests they will remain significantly less likely to attend college.

As was the case with Table A.6, the results in Table A.7 show positive outcomes for former offenders as a result of automatic expungement.[37] Similarly, adding ASVAB to Table A.7 does not affect the estimates for recidivism or income. Lastly, as with the falsification exercises in Table A.6, the magnitudes of the effects of automatic expungement are very small and statistically no different from zero in all regressions in the bottom panel. Despite the differing sources of variation that are identifying the effect of expungement with each method, the estimates in Table A.6 and Table A.7 are remarkably similar. This lends further credence to the claim that the estimation in the top panels is capturing the effect of expungement.

A comparison of the results from Table A.6 and Table A.7 to the estimates from Table A.5 gives some insight into the magnitude of the effect of expungement. In theory, expungement eliminates the effect of juvenile conviction. This would imply that, all else equal, I would expect the effect of expungement to be the same magnitude but opposite sign of the effect of being arrested and convicted. My results suggest that expungement removes a large percentage of the negative effects of arrest and conviction, but does not entirely undo them. This is a plausible result if, for example, there are scarring effects of appearing in juvenile court and being adjudicated delinquent.

[37] One way to generalize the results of Tables A.6 and A.7 is to statistically test the direction of the coefficient of interest across all of the estimated equations. I estimate the system of equations in each panel of Table A.6 and Table A.7 as seemingly unrelated regressions, allowing for some correlation to exist between the underlying error terms in each of the regressions, and test the coefficients on automatic expungement across the entire system. I run this test for each panel separately. In all cases where I expect to estimate the effect of expungement (excluding falsification exercises), I can reject the null hypothesis that the effect of automatic expungement is zero. These findings suggest that there is an overall effect of juvenile expungement, despite the weaker results for each of the outcomes individually.

I perform a number of robustness exercises for the primary results in Table A.6 and Table A.7. First, I try numerous strategies for assigning the state of residence to each respondent. For example, one strategy assigns the state of residence where the juvenile offender commits his or her first crime while using the 1997 state of residence for those who never commit crimes. This check does not have any significant impact on the results. Another robustness check focuses on the age of criminal majority. Instead of using the age of criminal majority specific to each state, I change the analysis to assume the definition of juvenile is age 16 or younger. The estimated results are no different as a result of this adjustment. The results are similarly robust to inclusion of the ASVAB measures. All linear probability models are robust to functional form assumptions; estimating the equations via probit and logit does not affect the results.

I also use a falsification exercise to understand if there are systematic differences between respondents in automatic and application states. For this exercise I use the preferred specifications but define the outcome variable as the measure of good grades in eighth grade. I do not find any statistically significant differences between automatic and application states. Generally, the results of these robustness checks and falsification exercises continue to support the finding that the effects I estimate are due to expungement and not influenced by a number of the assumptions I make in my preferred specifications.

1.8 Discussion and Conclusion

This paper is the first to empirically evaluate cross-state variation in the usage and effectiveness of expungement. I identify the existence of automatic and application states and present a conceptual framework that captures the dynamic incentives created by a policy of automatic expungement. I also provide evidence from unique data that the rate of expungement in automatic states is near one while the rate in application states is near zero.

My empirical analysis uses two very different estimation strategies, and both of these analyses support the implications of the conceptual framework. I do not find any evidence that the nature of expungement statutes affects the incidence of juvenile crime, the primary avenue through which there could be social costs from expungement. I then investigate the impact of expungement on future crime, education, and labor market outcomes. Using data from the NLSY97 I show that there are benefits to former delinquents as a result of automatic expungement. Specifically, my results suggest that former offenders living in an automatic expungement state are less likely to recidivate after age 20, more likely to attend college, and earn a higher average salary in their late twenties.

The incentives I discuss in the conceptual framework along with the empirical evidence on response to these incentives suggest that juvenile criminals are unaware of the expungement process. This conclusion supports all of the empirical findings in the paper: low rates of expungement in application states, no effect of automatic expungement on arrest propensities, and large effects of automatic expungement on long-term outcomes.

The results of this paper also address a new mechanism behind the findings in the crime literature: the effect of an observable record of juvenile delinquency. The coefficient estimates suggest that colleges and employers are considering individuals' criminal histories in the application process, and this is creating a significant barrier for many ex-offenders. The removal of these barriers to education and legal employment in automatic expungement states is another plausible explanation for my findings in these markets for adults. My analysis shows that the barrier created by the record of juvenile delinquency is separate from the effects of important covariates, and unobserved differences in state justice systems do not play a big role in explaining these results. Even when I add an ability measure to the model, the effects on future recidivism and employment remain significant.

One of the challenges with this work is finding a strong source of identification. Unfortunately, states have not changed their expungement process significantly over time, and many of the statutes date back to the first half of the twentieth century. Despite my efforts to reduce the bias caused by differences across state justice systems, my current identification strategy fails to capture any other unobserved differences, such as community programs that may have an impact on the outcomes of former offenders. While I am unable to control for some of this unobserved heterogeneity, my results provide compelling evidence that there are not large, systematic differences between the two types of states. Therefore, I conclude that I am identifying the underlying relationship between juvenile records and important economic outcomes.

While I do not perform a complete cost-benefit analysis for the policy, I use my results to think about the notable costs and benefits of expungement. Clearly, the results imply that automatic expungement has significant benefits for former offenders. One can imagine numerous other potential social benefits, such as the tax revenue if these individuals contribute to society via legal employment as opposed to a socially costly life of crime or the reduction in administrative costs caused by appearances in court. Turning to social costs, while the effect of automatic expungement on crime is not distinguishable from zero, I also cannot reject small, positive effects. To appropriately account for these costs, one would have to weigh the cost of these specific crimes, and it seems likely that the value of the costs is small.[38] Therefore, my

[38] In estimating the value of this social cost one should only consider crimes that are eligible for expungement. The costliest of crimes from a social perspective, such as murder, should not be included in this calculation. As an

sketch of a cost-benefit analysis concludes that the social benefits of expungement outweigh the social costs.

Generally, the process of expungement is one that deserves more attention in the literature. The ability to expunge one's juvenile record appears to be a very powerful legal remedy, and one that is not used due to lack of knowledge about the policy. There remains room in the literature for a precise estimate of the impact of expungement on a number of important outcomes. However, given the estimated return to former offenders of expungement of records of juvenile delinquency, the social benefits of such expungement, and the lack of evidence indicating social costs, policymakers should focus on raising awareness of expungement policies.

example of the magnitude of these costs, a realistic value is an estimate of assault victim cost at $13,000 instead of the statistical value of a life at $4.1 million (Heckman et al. 2010).

CHAPTER 2

DID GRADUATED DRIVER LICENSING LAWS DRIVE A REDUCTION IN CRIME?

2.1 Introduction

Over the past three decades each of the *Healthy People* publications, which outline the primary public health agenda for the United States, contains objectives regarding implementation of Graduated Driver Licensing (GDL) laws (United States Department of Health and Human Services 2000; National Center for Health Statistics 2001; United States Department of Health and Human Services 2013). GDL is a three tiered program designed to reduce the risk associated with novice driving by requiring the driver to complete two stages (learner's permit and provisional license) before receiving an unrestricted license.[39] The National Committee on Uniform Traffic Laws and Ordinances developed the original model GDL law, and state specific laws have been targeted at young drivers in the United States since 1996.

Numerous studies conclude that adoption of a GDL law causes a significant reduction in fatalities among young drivers (Dee et al. 2005; Karaca-Mandic and Ridgeway 2010; McCartt et al. 2010; Masten et al. 2011; Williams et al. 2012). Empirical evidence shows that strong passenger and nighttime driving restrictions in the intermediate stage are the components that are most important in explaining the reduction in teenage fatalities (Karaca-Mandic and Ridgeway 2010; McCartt et al. 2010; Masten et al. 2011).

While accidents and fatalities may be the most direct measures of risk reduction, GDL restrictions have the potential to affect other youth behaviors. One can view these policies as the introduction of both a statutory curfew and a limitation on the number of passengers that can be

[39] The learner's permit stage of GDL has been around since the first half of the twentieth century (Mayhew 2003). Therefore, one can view the primary innovation of GDL laws as the introduction of the provisional license.

in the vehicle. The response of teenage drivers to these restrictions can subsequently affect other outcomes that involve motor vehicle use among the target population.

One important such behavior is teenage crime. Specifically, restricting access to motor vehicles may prevent the opportunities to commit violent and property crimes.[40] For instance, GDL restricts driving at night, a time of day when some criminal activities, such as robberies, are more likely (Doleac and Sanders 2013). In addition, the interaction of motor vehicle access with risky behaviors, such as alcohol use, can lead to the production of violent and property crimes (Carpenter 2007). Unrestricted access to motor vehicles can also create social situations where multiple teenagers are in the vehicle at once, a significant input in the production of criminal activity (Zimring 1998).

This study provides the first estimates of the causal effect of GDL implementation on criminal behavior among teenagers. As a preview of the results, I find that GDL restrictions cause a decline in violent and property crime, particularly among 16 year olds. These results are generally consistent with previous literature regarding the effect on crime of similar policies, such as zero tolerance laws and curfews. I then show that the nighttime restrictions associated with GDL implementation are the primary mechanism causing the reduction in crime.

The remainder of the paper is organized as follows. First, I describe the GDL policy further and provide a review of the literature. Next, I describe the data sources and methodology. Then, I present my primary results along with a discussion of threats to validity and robustness checks, followed by an examination of heterogeneity in the results, the dynamics

[40] Criminology literature generally focuses on violent and property crime when analyzing criminal behavior. Violent crimes include murder, non-negligent manslaughter, forcible rape, robbery, and aggravated assault. Property crimes include burglary, larceny-theft, and motor vehicle theft.

of the policy, and the potential mechanisms causing my results. Finally, I draw conclusions based on the analyses.

2.2 Policy Background and Literature Review

2.2.1 GDL and Motor Vehicle Fatality

While the specifics of GDL policies vary by state, all GDL laws generally have a similar structure. A young driver enters the learner stage at a minimum entry age, remains in the stage for a mandatory holding period, and completes a minimum amount of supervised driving.[41] When the learner stage is complete the driver "graduates" to the intermediate stage, where the teen can drive without supervision. However, there are restrictions on the number of teenage passengers that can be in the vehicle as well as a timeframe at night when unsupervised driving is prohibited. Once the driver holds this provisional license for a sufficient amount of time and reaches another age milestone these restrictions are lifted, leaving the driver with an unrestricted license.[42] Today all 50 states and the District of Columbia have some version of a GDL law in place, though the severity of the restrictions varies across states.

The staggered implementation of a wide variety of GDL laws allows for analysis of the effect of the GDL law and its components on teenage fatalities. The literature concludes that adoption of a GDL law causes a reduction in driving-related fatalities for the target age group (Dee et al. 2005; Karaca-Mandic and Ridgeway 2010; McCartt et al. 2010; Masten et al. 2011). Furthermore, empirical evidence shows that more stringent nighttime and passenger restrictions

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[41] Note that GDL restrictions apply only to young drivers who apply for a learner's permit. A driver who applies for a driver's license at an older age, particularly over 18, is not be required to go through GDL.

[42] States vary on the specifics of their driver education program, which is different from their GDL law. In most states teenagers must complete driver education (or pass a written exam) in order to get their learner's permit. In some states, like Michigan, driver education requirements are intertwined with GDL restrictions. This paper focuses specifically on the GDL law and not on the driver education requirements.

cause a larger reduction in fatalities (Karaca-Mandic and Ridgeway 2010; McCartt et al. 2010; Masten et al. 2011). For a general review of the most recent literature pertaining to GDL laws and traffic safety, refer to Williams et al. (2012).

The literature on traffic fatalities also tries to identify the causal mechanism for the reduction in teenage fatalities. Understanding whether this result is caused by a decline in the prevalence of teenage drivers on the road, as opposed to an improvement in the habits of teenage drivers, can have important policy implications. However, reliable statistics regarding the incidence of teenage driving do not exist. The National Household Travel Survey is only administered every few years and gathers data among drivers of all ages. As a result, the sample of individuals between 16 and 17 years old is very small. Similarly, the American Time Use Survey has a very small sample of respondents between 16 and 17 years old and collects little data about driving activities.[43] Because of this lack of reliable data directly measuring the outcome of interest, Karaca-Mandic and Ridgeway (2010) use a structural model to infer the effect of GDL restrictions on teenage driving behavior. Their results support the claim that the reduction in fatalities is caused by a reduction of teenagers on the road.

2.2.2 Crime

The effect of GDL implementation and, more broadly, the effect of motor vehicle access on crime has not been previously studied in the literature. However, there is a rich literature in juvenile crime that provides guidance as to how GDL may affect crime among the target population (McDowall et al. 2000; Levitt and Lochner 2001; Jacob and Lefgren 2003; Mocan and Rees 2005; Bayer et al. 2009; Kline 2012; Aizer and Doyle, Jr. 2013; Eriksson et al. 2013;

[43] For example, between 2003 and 2013 there are only 2,078 unique respondents across all 50 states between 16 and 17 years old who report any driving activity.

Anderson 2014). While it is outside the scope of this paper to completely characterize the model underlying juvenile crime, I briefly describe the potential effects of GDL restrictions on juvenile crime in the context of criminal opportunities.

Consider a model describing criminal behavior, such as the model described in Becker (1968) or Cook (1986). Before choosing his or her action a potential criminal weighs the expected payoff to successfully committing the crime against the expected punishment if he or she is caught. Such a model of criminal behavior suggests that GDL restrictions could affect crime either by altering the set of potential criminals or by affecting the marginal costs and benefits of crimes. While the direction of this effect is ultimately an empirical question, I provide a number of predictions for how GDL restrictions may affect crime.

The GDL restrictions may change the set of potential criminals in a number of ways. First, access to motor vehicles can affect criminal opportunities, where the potential criminal has a chance to compare the marginal costs and benefits of committing a crime. For example, the nighttime restrictions associated with GDL can act as a curfew, restricting criminal opportunities at a time when crime is common among the juvenile population. Prior literature shows that curfews among the juvenile population can lead to significantly lower rates of crime and arrest, particularly among certain violent and property crimes (McDowall et al. 2000; Kline 2012). GDL restrictions may also affect potential offenders through passenger restrictions. Passenger restrictions influence the peer pressures in a motor vehicle, and it is well documented that delinquent behavior occurs more commonly in groups (Zimring 1998). Similarly, a large literature argues that teens associating with delinquent peers, particularly during times of unstructured socializing, are significantly more likely to participate in delinquent behavior

(Agnew 1991; Osgood et al. 1996; Gaviria and Raphael 2001; Osgood and Anderson 2004; Bayer et al. 2009; Monahan et al. 2009; Mennis and Harris 2011).

There is also the theoretical effect of the increased costs associated with crime in the GDL system. Becker (1968) and Cook (1986) show that changes in the costs associated with crime can affect one's involvement in criminal behavior. In the context of GDL there may be changes in the probability of being caught and convicted as well as the punishment for the crime, giving two direct predictions. First, enforcement of the restrictions raises the probability that a young driver with numerous passengers will be stopped by a police officer, particularly in states with primary enforcement. Second, the threat of punishment under GDL can deter adolescents from criminal behavior. Teens who commit minor crimes while in the GDL system typically have their restrictions extended or their license suspended, imposing an additional cost for committing the crime (National Conference of State Legislatures 2011).

Overall, the intuition of this paper is comparable to the analysis of the effect of zero tolerance laws on criminal behaviors in Carpenter (2007). Zero tolerance laws prohibit the operation of motor vehicles by a driver under age 21 with any trace of alcohol in their system. Carpenter (2007) shows that zero tolerance laws cause a 3.4 percent reduction in property crimes among 18 to 20 year olds with no decline in violent crimes. GDL laws are very similar to zero tolerance laws: they were implemented by states toward the end of the twentieth century, they pertain to teenage driving behavior, and they interact with other teenage risky behaviors.[44] However, the effect of GDL laws may be more widespread among teenage drivers, as zero tolerance laws only refer to consumption of alcohol and driving. This difference can cause the effects of the policies to vary significantly, warranting further investigation.

[44] Because my panel runs from 1997 to 2010, there is no variation in zero tolerance laws for my primary analysis.

2.3 Data

2.3.1 GDL Policy Data

The Insurance Institute for Highway Safety (IIHS) collects data on the stringency of GDL policies by state as well as effective dates of implementation when GDL policies change. I use this resource as well as prior literature and state statutes to characterize the policy in effect in each year. For my primary specification I follow prior literature by coding the GDL policy variable as a binary variable that equals one when the state implements a three tiered driving system. In the year of implementation I use a fractional value that reflects the portion of the year where GDL restrictions are effective. Table H.1 summarizes the different dates of implementation for each state.

2.3.2 Outcome Data

For my analysis of crime I draw counts of arrest from the "Arrests by Age, Sex, and Race, Summarized Yearly" datasets maintained by the National Archive of Criminal Justice Data. These counts come from the Uniform Crime Reporting system operated by the Federal Bureau of Investigation. This system collects crime statistics from law enforcement agencies that voluntarily agree to participate and covers over 18,000 reporting agencies representing 95 percent of the United States population (Federal Bureau of Investigation 2013).

In addition, I use data from the "National Prisoner Statistics" series of the Bureau of Justice Statistics and the Census of Juveniles in Residential Placement to proxy for the severity of the criminal justice system in each state.[45] Data for these sources only go back to 1997, which

[45] Counts of juvenile offenders are not available for every year. As a result, I linearly interpolate the years that are not available between 1997 and 2010.

is where I begin my crime panel. I analyze data until 2010, resulting in a panel that has 51 states over 14 years (N=714).[46]

The measurement of crime is very difficult in empirical research because data are typically only available for either arrests or reported crime, two subsets of the outcome of interest. As a result, researchers typically make assumptions about the relationship between crime and arrest and use available data to draw conclusions about crime more broadly. My preferred measure of crime is the percentage of total arrests attributed to the target population. As a specific example, for 16 year olds this would be 100 times the number of arrests of 16 year olds divided by total arrests across all ages.[47] I refer to this measure as the arrest ratio throughout the paper.

It is important to note the specific assumptions I make by using this measure of crime. Using the arrest ratio as a proxy for crime implicitly assumes that the proportional relationship between arrests for a specific age group and the rest of the population is equivalent for crime among that age group. The measure also implicitly controls for state level policing behavior as long as police activity is uncorrelated with the age of the offender. Additionally, the coverage issues that exist in some states with the Uniform Crime Reports are not a concern with this outcome as long as consistency in reporting is not correlated with age.[48]

A second crime measure that I use to test for robustness, discussed in Levitt (1998), equates the proportion of arrests for a particular age group to the proportion of reported crime committed by that population. An advantage of this measure is that it produces interpretable and

[46] In the analytical sample I drop data from Florida and the District of Columbia due to poor data coverage. Additionally, I treat 5 other observations as missing due to no arrests being reported in the state and year.

[47] Crime literature typically does not use arrest levels as an outcome measure because arrests depend on both criminal activity and police activity. For example, an estimated decline in arrests could be the result of a reduced police presence or a reduction in criminal behavior.

[48] The Federal Bureau of Investigation reports data coverage indicators by state that reflect the overall quality of the arrest data collected for each state (Puzzanchera and Kang 2013).

comparable estimates of the effect of GDL implementation on the crime rate. However, because reported crime is collected at the state level, the proportion of arrests should be reflective of the entire state. To reduce any error associated with underreporting of arrests, I treat as missing any state with a reported coverage rate for arrests below 85 percent.[49]

Table B.1 contains summary statistics for the key variables that I use in the analysis. The top panel summarizes the outcome variables that I use throughout the paper. First, I present the pre-policy arrest ratios and crime levels for violent and property crime separately by gender to underscore the differences in baseline criminal activity between boys and girls.[50] These statistics also allow for comparison of the different measures of crime: the pre-GDL arrest ratio for violent crime among 16 year old boys implies that 3.55 percent of all arrests for violent crimes prior to GDL implementation were of 16 year old boys, while the crime measure in the top panel of Table B.1 implies that prior to GDL implementation 16 year old boys committed 10.46 violent crimes per 1,000 population on average. It is not surprising to find that young men commit much more crime than young women and the rate of property crime is much higher than the rate of violent crime among this age group. I focus on the different effects of GDL implementation on criminal activity among boys and girls when I examine heterogeneity in my results.

The bottom panel of Table B.1 reports descriptive statistics for the covariates in the regression analysis. First, I include a number of driving and alcohol related policies, such as highway speed limits, seat belt enforcement, legal blood alcohol concentration limits, zero tolerance laws, and administrative license revocation laws. Some of these laws, such as zero tolerance and administrative license revocation laws, have little independent variation because

[49] All results using this measure are robust to removing this sample adjustment.

[50] To be clear, only the numerator of the arrest ratio is sex-specific. These ratios are the number of arrests of 16 to 17 year old males or females divided by the total number of arrests regardless of gender. The level of reported crime and population measure I use in the crime calculations are not gender specific.

most states had implemented them by 1997. Others, like speed limits, seat belt enforcement, and blood alcohol concentration limits, reflect a state's attitude toward motor vehicle use and safety. Conditional on GDL implementation, 22 percent of states have secondary enforcement of the restrictions, meaning an officer may issue a citation for violation of the restrictions if and only if the officer has stopped the vehicle for some other reason. The panel also contains a number of demographic and economic indicators intended to partially explain state level differences in the level of crime. Lastly, I include a number of indicators of the justice system in each state. Among these covariates I include the number of police officers employed by the state per capita, total state expenditures on the justice system per capita, and a measure of prisoners in custody as a proxy for the severity of the state justice system. Following Levitt (1998), my measures of custody rates are the stock of juveniles in state facilities divided by the population age 15 to 17 (consistent with criminal deterrence) and the stock of juvenile in state facilities divided by the total number of reported crimes (consistent with criminal incapacitation).[51]

Figure E.1 shows a graphical correlation that motivates one aspect of my empirical work. The solid line in the figure shows the number of arrests per population for a specific age group in 2010. In the other two lines I show the number of states that allow for a person in this age group to have a learner's permit or a provisional license. The correlation in this figure implies that the ages when teenagers gain access to motor vehicles are also the ages when there is a large increase in teenage arrest rates, consistent with the theoretical relationship between motor vehicle access and criminal behavior. However, this correlation is also consistent with many other explanations, so I use the implementation of GDL restrictions to isolate a causal effect.

[51] Levitt (1998) shows that criminal activity among both adults and juveniles is sensitive to a measure of criminal justice severity.

2.4 Methodology

Like nearly all of the previous studies focusing on GDL implementation, I use a difference-in-differences estimation strategy, exploiting the variation in GDL adoption across states.[52] Many previous traffic fatality studies use models that incorporate count data, such as Poisson and negative binomial models. However, because the primary crime measure I investigate is a continuous variable, I rely on ordinary least squares in my preferred specification.

Formally, I use the following econometric model to understand how GDL affects crime:

$$y_{st} = \mathbf{X}_{st}\boldsymbol{\gamma} + \mathrm{GDL}_{st}\delta + \alpha_s + \mu_t + \varepsilon_{st} \tag{1}$$

The outcome variable y_{st} contains the arrest ratio for the crime of interest. In this setup $\mathbf{X}_{st}$ contains demographic, economic, and other policy related control variables for each state year cell. GDL_{st} is the binary indicator of GDL implementation. Equation (1) also includes state fixed effects (α_s) and year fixed effects (μ_t) to control for any time invariant or year specific unobserved heterogeneity. The error term ε_{st} captures any other idiosyncratic shocks. I cluster standard errors at the state level to mitigate concerns of within-state serial correlation that could affect inference in a difference-in-differences framework (Bertrand et al. 2004; Dee et al. 2005).

2.5 Results

2.5.1 Effects on Crime

Table B.2 reports the effects of GDL restrictions on the arrest ratio for violent and property crimes by specific years of age.[53] Each column of Table B.2 contains the coefficient on

[52] Given I revisit Dee et al. (2005) with additional years of data and additional states that have implemented GDL, I confirm the original results and test the estimates for robustness to additional years of data and changes in the definition of the outcome measure. The descriptive statistics and regression results appear in Tables H.2 and H.3.

[53] I exclude 15 year olds from Table B.2 because this age group is on the margin of being affected by the policy. For example, 15 year olds who interact with 16 year olds may be affected by the policy, while younger 15 year olds

GDL for the listed crime and age group from the preferred specification, which is a linear regression of the arrest ratio for the given age group and crime type on GDL implementation, indicators for driving policies, demographic and economic covariates, measures of the justice system, state fixed effects, and year fixed effects.[54] The custody measure, which proxies for severity, is lagged because of the concern that contemporaneous severity could have an effect on criminal behavior.[55]

The results in Table B.2 present the total effect for boys and girls together to focus the discussion on the differences between age groups. The estimates indicate a statistically significant reduction in the arrest ratios for violent and property crimes for 16 year olds with no significant effects for any other age group. These coefficients can be interpreted as the effect on the percent of arrests pertaining to each age group. Therefore, the results for 16 year olds imply a 0.322 and a 0.503 percentage point reduction in the percentage of arrests of 16 year olds relative to the rest of the population for violent crime and property crime, respectively.[56] These are reductions of approximately seven percent in the mean of the arrest ratios for violent and property crime prior to GDL implementation. There are generally reductions in crime for the other age groups, indicating spillover from the restriction of 16 year olds, but these estimates are not statistically significant. Also, the general decline in the magnitude of the coefficients as I move further away from the target population makes intuitive sense.

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may not be affected at all. I choose to exclude this age group because this heterogeneity complicates interpretation of the results.

[54] Employed police officers, state expenditures on the justice system, and juveniles in custody all have the potential to be endogenous. However, exclusion of these covariates does not affect the estimated coefficients; the estimated reduction for 16 year olds is 0.344 for violent crime (standard error is 0.156) and 0.488 for property crime (standard error is 0.187).

[55] In Table B.2 I use the incapacitation measure of severity, but results do not change if I use the deterrence measure. Results are similarly robust to excluding this measure altogether.

[56] My preferred specification is not weighted (Solon et al. 2013). However, weighting by population size does not influence the estimates. For example, the estimates for 16 year olds with population weights are -0.397 (standard error = 0.134) for violent crime and -0.430 (standard error = 0.183) for property crime.

The last column in Table B.2 also serves as a falsification exercise for the finding, as I should not expect to find a contemporaneous effect of the policy on older age groups. Motor vehicle access for this population is unrestricted both before and after implementation of the policy, so GDL implementation should not affect crime among these individuals. I report coefficients from regressions for violent and property crime by gender for age 18 to 20, finding point estimates that are statistically no different from zero.[57] This falsification exercise lends credibility to the finding that GDL implementation reduces violent and property crime among 16 year olds.

2.5.2 Threats to Validity

The difference-in-differences identification strategy relies on the common trends assumption to consistently estimate the effect of interest. In the framework of this paper this assumption implies that the observed trend in crime for states where GDL has not been implemented is identical to the trend that would have existed after GDL implementation. Comparing pretreatment trends across states provides an indication of the appropriateness of this assumption.

The event study analyses that correspond with the preferred estimates for 16 year olds appear in Figure E.2 and Figure E.3. The samples in these analyses are balanced such that I only include states with three years of data before and after GDL implementation. To operationalize these analyses, I include seven dummy variables instead of the binary GDL indicator in equation (1). These dummy variables include three years of leads, an indicator for the year of implementation, and three years of lags (where the last lag indicates three or more years after

[57] Although not reported in Table B.2, the same falsification exercise for older ages shows similar results. When applying the same specification to older age groups I adjust the measure of custody to reflect adult prisons.

GDL is implemented). Therefore, the reference group for interpretation of these estimates is the state mean four or more years prior to implementation of GDL.

In Figure E.2 there does not appear to be a systematic trend in violent crime before implementation of GDL, implying that states did not implement GDL restrictions in response to high violent crime rates.[58] There is a distinct decline in the arrest ratio in the first year after GDL implementation that remains unchanged two and three years after GDL. Similarly, the event study analysis for property crime in Figure E.3 shows no evidence of trends in the arrest ratio prior to the implementation of GDL restrictions, with a similar decline that begins after the implementation of the restrictions. While these models lack the statistical power to estimate precise effects, the results mitigate concern of policy endogeneity and provide support for the feasibility of the common trends assumption.

Another threat to the validity of the identification strategy is an unobserved shock that is correlated with the timing of GDL implementation. For example, if police decide to increase the targeting of young drivers upon GDL implementation, any observed effect on crime may be mistakenly attributed to the policy. In addition to the pertinent observables in equation (1), the falsification tests in Table B.2 alleviate concerns that any estimated reduction is not being caused by the policy itself. In the context of targeted enforcement, it seems unlikely that a targeted increase would affect 16 year olds but not 17 and 18 year olds.

There are a number of other approaches to understand the effect of the policy on the behavior of police. One way to test this concern is to use indicators of police enforcement, such as the number of employed police officers per capita, as the dependent variable in the preferred

[58] Generally, the pattern of estimates in prior literature that focuses on traffic fatalities also shows no evidence of policy endogeneity; there is no distinct trend in motor vehicle fatalities prior to GDL implementation and a noticeable decline in fatalities within states after GDL restrictions take effect (Dee et al. 2005; Karaca-Mandic and Ridgeway 2010).

specification. Such a regression does not yield statistically significant estimates of GDL implementation (coefficient: -0.006; standard error: 0.008). Alternatively, one could directly test this theory by analyzing the frequency of traffic citations or contact with the police among the target population. However, sources of such data, like the Police Public Contact Survey, are not suitable for analysis at the state level. Instead, I present more suggestive evidence in Table B.3 by comparing the effects of GDL restrictions in states with primary enforcement of nighttime and passenger restrictions to those with secondary enforcement. The intuition behind this analysis is that increased enforcement targeting young drivers upon GDL implementation would be more evident in states with primary enforcement of the restrictions.

In Table B.3 I present estimates from the primary specification but also include an interaction between GDL implementation and secondary enforcement of nighttime and passenger restrictions. Therefore, the coefficient on GDL reflects the effect in states with primary enforcement, while the sum of the two coefficients is the effect in states with secondary enforcement. Generally, the estimates in Table B.3 are consistent with the primary results in Table B.2. There do not appear to be statistically significant differences between states with primary and secondary enforcement of restrictions, providing additional evidence that an increased level of police enforcement is not a major threat to the validity of the preferred estimates.

2.5.3 Robustness

In Table B.4 I test for robustness of my primary findings to different measures of crime. In each of the columns I report estimates for the effect on violent and property crimes using the arrest ratio, an adjustment to the arrest ratio where I remove 16 year olds from the denominator, the natural logarithm of arrests, the natural logarithm of the arrest ratio, and the crime rate described in Levitt (1998), respectively. These estimates continue to reflect the total effect across both genders. I include column (1) of Table B.4, the preferred estimate for 16 year olds from Table B.2, to simplify comparison.

The adjustment to the arrest ratio corrects for the fact that total arrests of the target population appear in both the numerator and the denominator of the arrest ratio. Therefore, instead of including all arrests in the denominator, the adjusted arrest ratio only includes arrests for age 25 and older in the denominator. As a result, the coefficient can no longer be interpreted as the effect on the percentage of crimes committed by 16 year olds. However, it is reassuring to find that there is still a negative, marginally significant effect of the policy on the adjusted arrest ratio.

Using the natural logarithm of total arrests of 16 year olds is another way to eliminate the division bias in the arrest ratio. However, using only the numerator of the arrest ratio sacrifices the information on total arrests that was included in the denominator. As a result, in this specification I also include the natural logarithm of arrests age 25 and older as a covariate. This allows me to continue to control for aspects of the justice system that are constant across age, such as the intensity of police enforcement. I continue to find negative and statistically significant effects of GDL restrictions using the natural logarithm of total arrests.

Using the natural logarithm of the arrest ratio I estimate an 8.4 percent reduction in the arrest ratio for violent crime and a 7.7 percent reduction in the arrest ratio for property crime, with both statistically significant at conventional levels. Finally, in the last column I use the measure of crime from Levitt (1998), which has the added benefit of estimates that can be interpreted as effects on the crime rate. I estimate a 13.1 percent reduction in violent crime and an 8.2 percent reduction in property crime. The estimates across all of the columns show the robustness of the results to different measures of crime and arrest that have been used in the literature.

An added benefit of the results in Table B.4 is that they allow for comparison with other papers in the literature. For example, Carpenter (2007) finds that zero tolerance laws cause a reduction of 0.005 in the arrest ratio for property crime among 18 year olds as a result of the policy.[59] However, while Carpenter (2007) finds no effect on violent crime, I find marginal reductions in violent crime as a result of GDL. Kline (2012) finds a reduction of around 10 percent in arrests for both violent and property crimes in the target population as a result of curfew laws. This is generally consistent with the reduction I find in total arrests, the arrest ratio, and the crime rate for violent and property crime. The consistency of my findings with the previous literature on zero tolerance laws and juvenile curfews confirms that the magnitudes of my estimates are feasible.

2.5.4 Heterogeneity and Dynamics

Given the significant effects of the policy on 16 year olds, I focus on the heterogeneity in the results for this population. In Table B.5 I show the effects by gender. Given the pre-policy arrest ratios by gender and the theoretical effect of GDL restrictions on crime, I expect to find effects that are larger for males than for females and larger for property crime than violent crime. The coefficient estimates in Table B.5 support this hypothesis, with the largest magnitudes and most significant estimates coming from males. However, the 0.145 percentage point reduction in the arrest ratio for property crime among females suggests that the policy has an impact for

[59] Recall the estimate from Carpenter (2007) should be multiplied by 100 for comparison with my estimates.

females as well. This result makes intuitive sense because there is no reason to suspect that this policy should affect males but not, to a lesser degree, females.

Disaggregating the results by type of crime is also informative because the link between GDL restrictions and crime should be more apparent for certain crimes. For example, murder is unlikely to be affected by GDL restrictions because restricting motor vehicle access is unlikely to affect potential murderers and the change in the marginal cost of murder created by penalties under GDL is extremely small. However, less severe crimes, such as burglary and larceny, may be significantly affected by GDL restrictions because the restrictions are likely to influence a larger set of potential offenders and the change in the marginal cost of crimes is much larger.

The disaggregated results by specific crime appear in Table B.6.[60] The first set of columns indicate that changes in aggravated assault drive the results for violent crime in males. The estimates of violent crime for females show no significant effects for any particular crime, but the largest reduction is in aggravated assault. This is not surprising as there was no overall effect for females in Table B.5. Focusing on property crime, the reductions in burglary and larceny for males and larceny for females drive the effect on the arrest ratios for property crime. Generally, the lack of statistical significance for more serious crimes, such as murder, rape, and robbery, is reassuring in the context of this policy and the theoretical framework for its effect on criminal behavior.

Given the contemporaneous reduction in crime for 16 year olds, it is logical to ask if these results are permanent for 16 year olds who are exposed to GDL restrictions. I answer this question by comparing the arrest ratios for older age groups who were all exposed to GDL restrictions to the arrest ratios for that age group before GDL implementation. Table B.7

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[60] As with heterogeneity by gender, I only adjust the numerator of the arrest ratio by specific type of crime so that the sum of the disaggregated estimates in Table B.6 equals the estimate from Table B.5.

presents the results of this analysis for ages 18, 19, and 20. For the 18 year old column I compare the arrest ratios among 18 year olds for violent and property crime after GDL implementation to the same arrest ratios before GDL implementation, but I exclude the year of GDL implementation and the next two years. This ensures that everyone in the treatment group was exposed to GDL restrictions. For the analysis of 19 year olds I drop the first three years after GDL implementation, and for the analysis of 20 year olds I drop the first four years. None of the estimates in Table B.7 are statistically significant at standard levels, indicating that the reduction in crime among 16 year olds disappears by age 18.

2.5.5 Mechanisms

While the robust reduction in violent and property crimes due to GDL restrictions is informative, a complete understanding of the mechanisms causing this reduction is useful for policymakers. I first examine the effects of the underlying components of the policy, specifically nighttime and passenger restrictions. I focus on these components because the restrictions have direct implications for criminal behavior.[61] Additionally, empirical evidence shows that nighttime and passenger restrictions are the primary mechanism by which GDL laws affect traffic fatalities (Karaca-Mandic and Ridgeway 2010; McCartt et al. 2010; Masten et al. 2011).

In Table B.8 I replace the binary GDL indicator in the preferred specification with indicators for the implementation of nighttime and passenger restrictions. This allows me to separately identify the effects of these restrictions with the caveat that the identification is coming from states where nighttime or passenger restrictions were not implemented

[61] The other components of GDL that I ignore are less applicable to crime, such as mandatory holding periods and minimum amounts of supervised driving that must be completed during the learner stage.

simultaneously with the three tiered adoption.[62] The estimates for violent and property crime suggest that the nighttime restrictions are the primary mechanism causing the overall reduction in crime with marginally significant reductions of 0.407 and 0.480 percentage points, respectively (standard errors are 0.213 and 0.259, respectively). The positive coefficients on passenger restrictions, although not statistically significant, are puzzling. Passenger restrictions reduce peer influences in the vehicle, which should theoretically reduce the propensity to commit crime. However, the large standard errors on the estimated coefficients do not rule out the possibility of large effects of passenger restrictions in either direction.

Because nighttime restrictions appear to be the primary mechanism causing the reduction in crime, I further analyze the effect of hourly restrictions on crime and arrest by time of day. This introduces a new source of variation to my analysis: the hours of the day restricted by GDL. To estimate the effect on crime by time of day I use data on arrestees from the National Incident Based Reporting System (NIBRS). These data provide the hour of the day when the offense occurred as well as the age of the arrestee.[63] The NIBRS data I use in this analysis come from reporting agencies in 37 states over 14 years (1997 to 2010), although many of the states do not have 14 years of data available. To overcome potential data coverage issues, I continue to measure the criminal activity of 16 year olds relative to the rest of the population in the agencies that report in the state. While there could be potential selection issues with the reporting agencies, the NIBRS is the only available data source with information on the hour of offenses, which is vital for using the hourly variation in nighttime restrictions.

[62] The implementation of nighttime and passenger restrictions was not simultaneous to the implementation of a three tiered system in 23 out of the 51 states (includes the District of Columbia).
[63] I drop all cases where the relevant age or incident hour is missing.

In Figure E.4 I show graphical evidence of the effect of hourly restrictions on the arrest ratio for violent and property crime for 16 year olds. I plot the average arrest ratio for 16 year olds for each hour of the day separately by states where the hour is restricted. There are no restrictions between 6:00 A.M. and 8:00 P.M., so there is only one line during these hours. However, between 8:00 P.M. and 6:00 A.M. I show significantly lower arrest ratios in the states where the hour is restricted.[64] While the average arrest ratio is lower during all restricted hours, the reduction is most striking between 8:00 P.M. and midnight. In addition, while this figure shows the overall reduction in violent and property crime, unreported analyses by individual crime shows the largest reductions in larceny and no reduction in murder, rape, robbery, or motor vehicle theft, consistent with Table B.6. This general reduction in the arrest ratio during the restricted hours is consistent with the findings in Table B.8.

2.6 Discussion and Conclusion

This paper provides a discussion of the effects of GDL implementation on juvenile criminal behavior. I show evidence that GDL laws cause a reduction of 0.322 and 0.503 percentage points in the relative arrests of 16 year olds for violent and property crime, and I confirm this result using a number of different measures of crime. There is no evidence that the estimates are being caused by other policies that may be correlated with GDL implementation, such as targeted enforcement policies. The magnitudes of my estimates are consistent with prior literature on similar policies, such as zero tolerance laws and juvenile curfews. I also show this reduction among both boys and girls, that the reduction is being driven by a decline in

[64] Although not included, regression analysis using the hourly data support this finding. For example, if I use a panel of hourly arrest ratios in the primary specification and also add hour of day fixed effects, I find a negative and statistically significant effect of the hour being restricted.

aggravated assault and larceny, and no permanent effects of the policy. Once I establish the reduction in crime, I focus on understanding the causal mechanisms for these results. I use multiple analyses to show that nighttime restrictions are the primary mechanism causing the decline in crime.

Data limitations prohibit me from exploring important questions, and future work should focus on sharpening the answers to these questions. For example, I am unable to test for a demographic shift in the population of drivers in response to GDL restrictions. It would be important to know if individuals are choosing not to pursue a driver's license in response to GDL restrictions, particularly if this is correlated with criminal behaviors. In addition, data sources describing the peer influences in motor vehicles could help determine if young drivers are responsive to passenger restrictions and if peer effects in the vehicle play any role in the production of crime.

Overall, this study presents an example of an unintended effect of a policy that impacts teenage behavior. The reduction in crime caused by GDL is an added benefit of the implementation of GDL policies that is not discussed in prior literature. The conclusive evidence I show regarding the effect of GDL policies, specifically nighttime restrictions, on crime is important for policymakers as states implement GDL laws to be in compliance with the *Healthy People* publications.

CHAPTER 3

INTERSTATE DIFFERENCES IN PENSION VESTING RULES, K-12 TEACHER EXPERIENCE, AND TEACHER EXIT

3.1 Introduction

Recent education policies in the United States adjust teacher compensation in a number of different ways. While some policies tie teacher compensation to tangible results, such as value-added to student test scores, other policies focus on restructuring retirement compensation. For instance, responding to the stress of rampant underfunding, recent reforms to teacher pension systems attempt to alleviate fiscal stress by increasing vesting requirements and raising teacher and employer contributions.[65] These changes to retirement compensation for public school teachers have implications both for state budgets and for the composition of the teacher workforce if pensions influence labor market entry and exit decisions, employer changes, and mobility across state lines.

While the private sector has moved to defined contribution plans, and pension reform has made some inroads in public plans for general government workers, the vast majority of public school K-12 teachers continue to be covered by mandatory defined benefit plans with influential pension accrual patterns and limits to portability.[66] While the deferred compensation inherent in these defined benefit plans is designed to encourage teachers to remain on the job, these accrual patterns can affect a district's ability to attract younger, more mobile teachers or individuals from

[65] Actuarial valuations from state financial reports estimate a total of approximately $325 billion in unfunded liabilities, which the literature considers to be an underestimate due to unrealistic actuarial assumptions (Doherty et al. 2012; Novy-Marx and Rauh 2011). See the National Conference on State Legislatures (2013) for recent reforms.
[66] States vary in the availability of supplemental defined contribution plans as well as teacher contributions to Social Security. For example, in 2010 there were 15 states (includes the District of Columbia) where teachers were not covered by Social Security.

the private sector changing jobs mid-career. Changes to pension parameters designed to reduce state pension obligations may have important unintended effects on teacher experience. Depending on how teachers respond to such reforms, the changes to a state's K-12 teacher workforce age and experience may have implications for the educational quality of the state's future workforce.

In this paper we focus on vesting rules, portability through service credits, and pension wealth differences across states for public school teachers in the early years of their career.[67] Pension vesting rules have received little attention in the literature on teacher experience, yet they may have unintended effects on new teacher retention or teacher preferences for shorter-term employment, particularly for teachers who are forward-looking. For example, in 2012 the New York State Teachers' Retirement System changed the vesting requirement from five years to 10 years for new teachers. Additionally, they now require the teacher to contribute to his or her pension for the length of active membership, as opposed to only the first ten years of employment. There were similar reforms to pension parameters in 21 other states in 2012 alone (Doherty et al. 2012). All else equal, these changes may reduce the incentive for new teachers to stay in teaching for several years. Yet these early years of teaching are critical for teacher effectiveness. Several recent studies find that new teachers are less effective than those with some experience.[68]

Restrictions across state borders on purchasing credits may also reduce young teacher mobility. The formulaic nature of the defined benefit pension calculation implies that teachers

[67] Our results throughout the paper are applicable to public school teachers. These results are applicable to charter school teachers if the charter school opts to participate in the state retirement system. Olberg and Podgursky (2011) discuss the different retirement compensation programs for charter school teachers across a number of different states.

[68] For an overview, see Rice (2010), and for individual studies see Kane, Rockoff, and Staiger (2006), Ladd (2008), and Sass (2007).

who change retirement systems in mid-career can pay a significant penalty in pension wealth if they do not receive credit for prior time as a teacher. Some states allow teachers to purchase credits for prior service, limiting the severity of this penalty. However, other states limit the credits one can purchase or do not allow for purchases at all.

We illustrate the magnitude of differences in pension wealth, both across states and at different times in one's teaching career, using a simulation exercise across four states with different vesting rules. We construct the actuarial present value of pension liability (or, wealth from the teacher's perspective) that is commonly used in valuing pensions for legal matters, such as Qualified Domestic Relations Orders following divorce.[69] This termination liability, or accrued benefit obligation, is a measure of the pension liability owed at different points through a teacher's career should she separate from service.[70]

Next, we use cross-sectional aggregate data on the state level experience distribution of teachers to calculate the relationship between important pension parameters, such as vesting requirements, and the composition of teacher experience. Our results suggest a negative relationship between the years required to vest and the percentage of teachers with experience between zero and four years. This finding implies that the current system of teacher retirement compensation is not helping to retain young teachers.

Lastly, we use the variation in characteristics of the state pension system to predict first exit from teaching among a sample of new teachers in the National Longitudinal Survey of Youth of 1997 (NLSY97). We show that vesting requirements and availability of defined contribution alternatives significantly affect the hazard of first exit from teaching. These results

[69] Papke thanks Robert Raasche for providing detailed information about these arrangements.
[70] This is similar to the accrued benefit obligations emphasized in Rauh (2010).

imply that adjustments to teacher retirement compensation may significantly affect the composition of the teacher labor force.

The next section briefly reviews the literature on mobility and retirement effects of defined benefit plans with an emphasis on teacher pensions. In Section 3.3 we discuss our calculations of individual teacher pension wealth and compare wealth for teachers upon vesting across four states as an illustration. Section 3.4 provides evidence that these interstate differences in pension vesting rules may affect the distribution of teacher experience across states. In Section 3.5 we use data from individual teachers to calculate the effect of different pension parameters on the hazard of first exit from teaching. Section 3.6 concludes.

3.2 Related Literature

Previous studies of mobility and pension wealth focus on retirement incentives at the end of the career for public sector workers. Friedberg (2011) reviews retirement and mobility implications of defined benefit plans and the related literature for public employees and teachers in particular. She finds that defined benefit pension incentives play a significant role in the timing of one's retirement from the labor market. However, she notes that empirical evidence suggests that younger workers with defined benefit plans are less likely to switch jobs as pension wealth accrues, but the evidence is not definitive. Using the Survey of Consumer Finances (SCF) data from 1983, Friedberg and Owyang (2002) find that private sector workers with a defined benefit pension have total expected tenure that is 5-7 years longer on average than workers without any pensions, but that workers with defined contribution plans also have longer tenure than workers without pensions. Using the Current Population Survey and Public Plans Database, Munnell et al. (2012) find that the probability of remaining with a single plan until retirement eligibility is reduced if the employee also has a defined contribution plan and is covered by Social Security.

A similar literature focuses specifically on teacher retirement incentives associated with defined benefit plans (Furgeson, Strauss, and Vogt 2005; Costrell and Podgursky 2009; Friedberg and Turner 2010; Friedberg and Turner 2011). This literature describes the incentives created by the defined benefit programs and provides state-specific and national studies of teacher response to these incentives. For example, there was a large increase in teacher retirement in Pennsylvania from 1997-1998 to 1998-1999 in response to more generous retirement benefits (Furgeson, Strauss, and Vogt 2005).

Another related literature on the retirement incentives imbedded in Social Security benefits examines the effect of the peak value of benefits on retirement. The peak value concept subtracts current pension wealth from the peak of pension wealth that is available in the future.[71] Costrell and McGee (2009) use administrative data from Arkansas to describe pension wealth differences, particularly at the peak value, and their effects on retirement behavior. Friedberg and Turner (2011) use the Teacher Follow-Up Survey of the Schools and Staffing Survey in 2000 and 2004 (SASS). Using a peak value approach along with data on teacher satisfaction, they find that teachers who are dissatisfied with their jobs respond more strongly to pension retirement incentives. Teachers who express job satisfaction still respond to retirement incentives, but with a much smaller magnitude.

A subset of the teacher retirement literature focuses specifically on cross-state variation in teacher pension wealth and provides simulation evidence of peak wealth (Costrell and Podgursky 2009; Toutkoushian et al. 2011). These calculations, like ours in Section 3.3, use the

[71] Coile and Gruber (2007) use peak value to measure Social Security incentives and Friedberg and Webb (2005) use data from the Health and Retirement Survey for individuals aged 50 and over.

characteristics of state pension programs to calculate the present discounted value of a teacher's pension benefits under a number of different assumptions about teacher age, experience, and salary growth. Costrell and Podgursky (2009) focus on six states in their simulation, and show the cross-state variation in spikes in pension wealth. Toutkoushian et al. (2011) calculate a simulation for one identical career teacher in all 50 states, providing a ranking for the most generous pension plans.[72]

3.3 Vesting Rules and Teacher Pension Wealth

In this section we describe our simulation of pension wealth. Rather than focusing on the generosity of plans at the normal retirement age, our simulations provide insight into the present discounted value of pension wealth upon vesting for new teachers. In addition, we improve upon earlier assumptions by using actual state starting salaries and salary caps so that teacher salaries do not grow to unrealistic values.

The four states we include in this simulation show the variability in pension wealth upon vesting due to pension plan parameters. Specifically, the four states (California, Florida, Michigan, and Wisconsin) all have different vesting rules – some as a result of recent changes. Table C.1 describes the specific parameters that go into the pension calculation for each state. Michigan requires ten years of service before a teacher is vested, while Florida only requires six years. California requires five years for vesting, but teachers do not contribute to Social Security and are no longer allowed to retire prior to the traditional retirement age with full lifetime benefits (Doherty et al 2012). Prior to 2011 teachers were immediately vested in Wisconsin, but

[72] Because of the large scope of this simulation, they only present results for one type of teacher who spent their entire career in the teaching profession with no salary cap. In addition, they assume one starting salary across all states and a salary growth rate of 3 percent with no cap. These assumptions result in six figure final salaries for lifetime teachers.

today Wisconsin has a vesting rule of five years. The national average for vesting in similar plans is 5.78. The remainder of Table C.1 shows that these states also differ in the age for retirement with full benefits, teacher contribution rates, teacher salaries, and Social Security coverage.

We collect information on pension plan parameters from a number of sources. First, we use plan participation measures from 2001 to 2010 of the Public Fund Survey, which collects statistics on public retirement systems. We supplement these statistics with vesting rules, benefit formulas, and contribution rates from summary plan handbooks as well as portability measures defined by the National Council on Teacher Quality (Doherty et al. 2012).

Based on these pension plan parameter values and assumptions described below, we can calculate the present discounted value of pension wealth at any point in time for a hypothetical teacher. We calculate the annual pension benefit as follows:

$$\text{Benefit} = \text{Final Average Salary} \times \text{Factor} \times \text{Years of Service}$$

Final average salary and the multiplicative factor are plan-specific parameters that we obtain from each state's pension plan brochures. We multiply this value by the probability of survival at each age to get the actuarial value of annual benefits, assuming the individual will live until age 100. Lastly, we calculate the present discounted value of pension wealth using a discount rate r as follows:

$$PDV = \sum_{t=0}^{t=100-C} \frac{A}{(1+r)^t}$$

Here A is the actuarial value of annual benefits, t indicates the year at which the calculation is done, and C is the teacher's age at the time of the calculation.

This calculation requires that we make a number of assumptions. First, we set the teacher's starting salary equal to the state's average salary for the teacher's level of education from the 2008 SASS. This implies that there is no difference in the starting salaries for individuals who hypothetically begin working in different years. Salaries grow at three percent per year until they reach the top step reported by the SASS. Once the salary reaches the top step for the state, it remains constant. Our reported present value deducts contributions and assumes they are returned if the teacher leaves the system early, but we omit the possible interest payments.[73] We assume a three percent discount rate in our calculations and use the 2008 female combined-race life tables to estimate the probability of survival to the next year (Arias 2012). In footnote 74 we illustrate variations due to racial differences in mortality.

Defined benefit plans also typically include cost of living adjustments (COLAs). In some states, such as Michigan, COLAs are a constant predetermined percent of initial benefit. In other states, the legislature votes annually on the possibility of a COLA that year. In other plans, COLAs are linked to an inflation index with a cap. Because our focus is on the front end of a teacher's career, and because inserting a COLA would require arbitrary and different assumptions across states, we do not include COLAs in our empirical work. For illustration, in Table C.2 we compare pension wealth estimates for Michigan teachers with and without the COLA – three percent per year (not compounded) starting in October after one full year of retirement. Clearly, COLAs can have significant effects on present discounted values for retirees who work as teachers throughout their careers (depending on actual levels of inflation).

[73] While teachers typically begin contributing to the pension as soon as they begin working, if the teacher leaves before vesting these contributions are refunded (sometimes with interest or employer contributions as well). In California, Michigan, and Wisconsin employees who leave receive interest. However, teachers who leave early in Florida receive less than or equal to their own contributions (Doherty et al. 2012). We omit the interest payments to remain consistent across states.

However, they make a much smaller difference early in the teacher's career, our demographic of interest.

Figure F.1 shows our calculation of the present discounted value of pension wealth - net of teacher contributions - over a typical teacher's lifetime career for our illustrative states. We assume this teacher started working at age 25 with a Bachelor's degree and no prior experience. The x-axis displays the exit ages when the teacher stops teaching and accruing pension benefits. The y-axis displays the present discounted values of pension wealth assuming a three percent discount rate and taking into account the probability of survival and salary growth. The shape of Figure F.1 is commonly found in analyses of defined benefit pension plans. The accrual pattern of these plans creates significant jumps in pension wealth at particular ages and strong incentives to retire when wealth reaches a maximum at the plan's normal retirement age.

Our focus is on the front end of the career trajectory. In Figure F.2, we zoom in on the early years of the teacher's career. Note the jump in pension wealth when the teacher becomes vested – at different levels of experience in the four states. The magnitude of this windfall can be approximately an additional year of salary or more for the young teachers; for example, the windfall of $45,171 in Michigan is approximately 132 percent of a starting teacher's salary. While the pension formulas differ with respect to salary and pension multiplier, the cross-state variation in vesting requirements alone accounts for timing differences in any pension wealth. Recall, several states recently increased the years required before vesting. This policy may reduce the future pension obligation but may also make it harder to retain young teachers. Depending on the quality of the teachers that exit and the teachers who replace them, this could have significant implications for student achievement.

The significant differences between the states in Figure F.1 and Figure F.2 highlight the differences in pension wealth that are caused by plan parameters. Many of the parameters in the simulation contribute to these differences, including contribution rates, salary levels, and benefit formulas. For example, consider the calculation of the final average salary used in the defined benefit formula. In Michigan and Wisconsin this number is the average of the highest three years of compensation, in California it is the highest consecutive twelve months, and in Florida it is the average of the highest eight years. Wisconsin's significantly lower present value in Figure F.1 is a result of a relatively high contribution rate coupled with a significantly lower salary. At $30,700 the average starting salary for a new Wisconsin teacher with Bachelor's degree is substantially lower than it would be in the other states. Furthermore, the Wisconsin peak salary of $57,100 is the lowest among the four states. Wisconsin's pension plan also includes a relatively high teacher contribution rate of 6.65 percent of salary.

In Table C.3, we calculate pension wealth for various early stages in a teacher's career. For a teacher who started teaching at 25 years old with a Master's degree, we compare the present discounted value of her pension if she quits after two years, five years, and 10 years and also at typical retirement ages. The first column highlights interstate differences in the peak value of her pension along with the age at which the peak will occur. The remaining columns include the difference from the peak value in parentheses. This difference is one measure of the opportunity cost of quitting or moving across state (district) boundaries in terms of pension wealth.[74]

[74] We can incorporate race into our calculations by adjusting the life tables in our calculations. If the teacher is white, the peak value of her pension (from Table C.3) in California, Florida, Michigan, and Wisconsin, respectively, would be $693,921, $684,392, $666,664, and $372,561. If she is black these values would be $638,249, $634,157, $614,999, and $342,051.

The simulated values of pension wealth in Table C.3 indicate that young teachers with a defined benefit pension earn virtually nothing toward their pension wealth before they are vested. In contrast, in the bottom row of Table C.3 we also simulate the value of a young teacher's pension if she were contributing to a defined contribution plan, using the Michigan teacher defined contribution plan offered to new hires as of September 2012 as an example.[75] The defined contribution pension wealth steadily grows for this worker, even in the early years. For instance, if a teacher quits after two or five years she still earns $6,341 or $17,322, respectively, a sizable amount of pension wealth if she participates fully in this defined contribution plan. Further, this benefit is portable to other plans or can be rolled over into an IRA.

3.4 Pension Plan Characteristics and the Distribution of Teacher Experience

In this section, we analyze the relationship between two key pension plan characteristics and the distribution of teacher experience across 50 states. We add data from the 2008 and 2011 SASS on the age and experience distributions for teachers as well as starting salary to our data on pension information across states.[76] The SASS provides the percentage of teachers in each state in the following experience categories: fewer than four years of experience, between four and nine years, 10 to 14 years, and 15 plus. Table C.4 provides summary statistics that highlight the cross-state variation in the experience distribution of teachers, vesting requirements, the ability to purchase service credits, and starting salary. These statistics show that around 15 percent of teachers in the survey have less than four years of experience and around 27 percent

[75] We assume that a teacher contributes six percent of her salary to the account with a 50 percent match rate (up to three percent) by the employer. We further assume that this account grows at three percent per year.
[76] Data on starting salary are only available for 2008. Data on average age are not available in the District of Columbia, Florida, Hawaii, Maryland, and Rhode Island for 2011. We use the estimates from 2008 for 2011 for these states.

have between four and nine years. The average plan vests its members in more than five years because the vesting requirement in 16 states is 10 years.[77] The variation in the starting salary for young teachers is also striking, ranging from $24,800 to $42,700.

Table C.5 reports results from a regression of the state level experience categories on pension characteristics to understand the relationship between pension characteristics that affect the early career and the distribution of teacher experience. The caveat to the analysis in Table C.5 is that it uses cross-sectional variation from two snapshots in time; as a result, one should not try to draw causal inference from these estimates. We focus on years until vesting and the ability to purchase credits in a new district/state. We also include as controls the average age among teachers in the state, the natural logarithm of the starting salary for a teacher with no prior experience, and an estimate of pension wealth for a hypothetical teacher.[78] In the first panel – percentage of teachers with less than four years of full time teaching experience – the vesting coefficient of -.0361 (p-value .015) suggests that for each additional year of waiting time required until any pension wealth is owned, a state will have more than one third a percent fewer new teachers. A vesting period of 5 years is common – 31 of these 50 largest public plans require five years. Those states will have 1.8 percentage points fewer teachers in early career stages. Ten states require 10 years – they are predicted to have 3.6 percentage points fewer newer teachers – almost one standard deviation in the mean of this variable. Vesting rules do not have a statistically significant effect on the percentage of teachers with four to nine years of experience – many of these are already vested and the rest are close.[79] Years to vesting is

[77] Some states have made changes to their vesting rules. The 16 states we reference vest at 10 years at some point between 2002 and 2010, but not necessarily for the entire period. For example, seven states have raised the vesting rule from five to 10 years between 2008 and 2012 (Doherty et al. 2012).

[78] The estimate of pension wealth, which comes from Table 8 of Toutkoushian et al. (2011), is net of contributions. While we prefer our assumptions for the simulation exercise, their estimates of pension wealth are highly correlated with our results. We use their estimates for this analysis so we have estimates for all states.

[79] Papke (2004) finds that quit rates in public employment drop off steeply right before vesting.

positively related to the percentage of teachers with 10 to 14 years experience – since these percentages sum to 100 the vesting coefficients must be of opposite sign at some point, and vesting cannot have any influence at this point in their career. This cohort is vested near mid-career.

Credit purchasing has a negative relationship for the younger experience categories and positive for the higher experience categories. The positive relationship with the higher categories makes intuitive sense because the ability to purchase service credits may result in higher retention in the teaching field for older teachers, making them more experienced. Also note that higher starting salaries are generally positively correlated with the percentage of younger teachers. Lastly, the relationship between pension wealth and the distribution of experience reflects the incentives for remaining on the job; states with higher values of pension wealth have a larger fraction of teachers in the most experienced category.

3.5 Evidence from the National Longitudinal Survey of Youth of 1997

In this section we focus on the relationship between pension plan characteristics and first labor market exit for young teachers. Our data for this analysis comes from the NLSY97. This nationally representative survey began following a cohort of teenagers in 1997 and interviews them every year, covering topics including income, employment, family, fertility, and health. This survey is most appropriate for us because it allows us to focus on the labor market behavior of young teachers across different states. It also allows us to include covariates in our analysis that are not typically available when using administrative data, as is the case in much other retirement compensation literature. We identify all individuals who ever report teaching between 2002 and 2010 and follow their teaching career over time. We choose to begin our analysis in 2002 because this ensures all respondents were old enough to teach. For a complete description of the data in this analysis, see Appendix I.1.

Our methodological strategy for this analysis is to use a discrete time hazard model to isolate the effect of pension characteristics, specifically vesting requirements, measures of portability, and existence of defined contribution alternatives, on the period-specific hazard of the respondent's first exit from teaching:

$$\Pr[\mathrm{P}_{\mathrm{ist}} = 1] = \lambda(\mathbf{X}_{\mathbf{ist}}\boldsymbol{\alpha} + \mathbf{R}_{\mathbf{st}}\boldsymbol{\beta} + \delta_{\mathrm{t}}) \tag{1}$$

In equation (1) we model the hazard of first exit from the labor market (P_{ist}) as a function of year effects (t), state level covariates (s) including the pension parameters of interest, and individual level covariates (i). Therefore, the vector $\mathbf{X}_{\mathbf{ist}}$ contains the covariates that vary by individual, state, and year, such as marital status or number of children in the household. The vector $\mathbf{R}_{\mathbf{st}}$ contains our variables of interest, such as pension plan characteristics, as well as other covariates that only vary at the state and year level. Our preferred estimates of equation (1) are estimated with standard logit assumptions, although results are robust to estimating via probit or linear probability modeling. We choose to model the hazard of first exit from teaching because we only see one exit from teaching for the majority of this sample.[80]

Table C.6 provides an overview of the teacher panel we use from the NLSY97. In the top panel of the table we provide descriptive statistics pertaining to the individual teachers. It is not surprising to find that a majority of the population of our teachers, 66 percent, are female. It is also not surprising to find that a large majority of the teachers are white. According to the National Center for Education Statistics, in 2011-2012 76 percent of teachers were female and 81

[80] Of the 779 teachers in the sample, we see 425 exits from teaching. Only 79 individuals, around 10 percent, return to teaching during our sample, and 64 of the 79 move to a new employer.

percent identified as white, not Hispanic (Goldring et al. 2013). The discrepancy between our statistics and the national averages is likely because we focus only on a cohort of younger teachers, while the national averages include the entire distribution of teachers.

Given the relatively young ages of the sample population, it is also not surprising to find that respondents are single for the majority of the observations. We condition on marital status and children in the household in our analyses because changes to these covariates could significantly affect the probability of exit from employment.

The Armed Services Vocational Aptitude Battery is an ability exam that is given to all respondents in the first round of the NLSY97. Those teachers who opted to take this exam scored in approximately the 69th percentile, on average, among all NLSY97 respondents.[81] Prior literature shows that underlying ability is positively related to exit from teaching (Podgursky et al. 2004).

One concern in this analysis of teachers is that individuals select into teaching or accept a specific position because of the pension benefits. Selection of a specific teaching position is not a concern as long as teachers are not comparing options across state lines. Regarding career selection, one plausible way to compare the outside options for these teachers is to use information on the industries where these individuals work when they are not teaching. In unreported tabulations, the largest industries (other than educational services) are retail trade, accommodations and food services, and arts, entertainment, and recreation. It is hard to draw any conclusion regarding selection based on this analysis alone. However, positions in these industries are likely in the private sector where employees will be covered by defined contribution plans.

[81] A total of 110 individual teachers, or 14.1 percent of the sample, opted not to take the ASVAB.

The middle panel of Table C.6 focuses on the characteristics of the different employment relationships we see among the teacher sample. For example, the average among those teachers who report a starting salary is $17.50 per hour, or approximately $35,000 per year.[82] Among those who remain teachers for five years or more, the salary grows to $21.28 per hour, or approximately $42,500 per year. Next, we indicate the type of teacher we see in the sample: preschool/kindergarten, elementary/middle school, secondary, postsecondary, and other teacher.[83] Last, we show that the average job tenure among the teacher sample is 2.16 years. This average would fall well below the median vesting requirement of five years. However, because we focus on young teachers, a demographic with high turnover rates, this result is not surprising.

In the bottom panel of Table C.6 we focus on the pension parameters that pertain to our sample between 2002 and 2010.[84] We include the number of years required to vest in the state pension plan, indicators for supplemental defined contribution options, an indicator for a choice between defined benefit and defined contribution plans, the retirement factor used in benefit calculations, an indicator for coverage by Social Security, and the required employee pension contribution rate. The average time to vesting for respondents in our sample is similar to the national average in Table C.4. Note that while 33 percent of plans offer a defined contribution add-on option, only 11 percent of plans offer a choice between defined benefit and defined contribution plans, indicating that the defined contribution plan is usually supplemental to the mandatory defined benefit plan. There is little variability in the retirement factor (it typically lies

[82] The NLSY converts all reported earnings and units of time to an hourly wage. Wage data are available for 73 percent of all observations where a teacher is employed.
[83] These categories are not mutually exclusive. They indicate that the individual teacher ever fell into the category between 2002 and 2010. The “other” category includes special education teachers and other teachers and instructors who do not fall into one of the other categories.
[84] We assume that teachers work in the state where they reside. The NLSY geocode data allow us to determine state of residence but not state of employment or any specific information about the employer.

between one and two percent), although states where teachers are not covered by Social Security tend to have higher retirement factors. Lastly, the average employee contribution rate for individuals in contributory plans is six percent.

The tenure determination may also predict labor market exit early in a teacher's career. As a result, we control for statutory requirements for tenure in each state. Table C.6 shows that, on average, states will grant a teacher tenure after 3.15 years. Lastly, at the bottom of Table C.6 we summarize six indicator variables that describe the portability of the state pension plan. The first three indicate the return of contributions if a teacher withdraws from the plan before vesting: a refund of their contributions, a refund of their contributions with accumulated interest, or a refund of their contributions, accumulated interest, and some of the employer contributions. The last three indicate the ability to purchase credits in the system for prior service: unlimited purchasing of service credits, limited purchasing of service credits, or no purchasing of service credits.

Table C.7 reports the results of the discrete time hazard model described in equation (1) estimated via logit. The dependent variable in each column is a binary variable that equals zero when the individual is teaching and equals one when the individual first exits teaching. In column (1) we control for a number of individual covariates as well as year dummies. The estimate for the ASVAB percentile implies that a ten unit increase in the ASVAB percentile increases the hazard of exit by 0.02. This finding, consistent with prior work in the literature, suggests that teachers with higher ability scores are more likely to exit teaching (Podgursky et al. 2004). We also find that women are less likely to leave teaching, as are respondents who are married. Although children in the household are not statistically significant, the positive relationship between fertility and exit from teaching is intuitive.

In column (2) we add a number of parameters related to the teacher's pension wealth.[85] These variables include a person-specific variable that reflects the number of years until the individual is vested in their retirement system, an indicator for availability of a defined contribution plan, an indicator for choice between the defined benefit and defined contribution plans, an indicator for the teacher having tenure based on the state's tenure rules, the retirement factor, an indicator for Social Security coverage, and the teacher contribution rate.[86] Time to vesting and availability of a defined contribution plan are both positive and significantly related to the hazard of first exit. The coefficient on time to vesting implies that a young teacher is 2.3 percentage points more likely to exit teaching for each additional year he or she must work before vesting. This implies that a change in vesting from five to 10 years would increase the hazard of exit by 11.5 percentage points.

The effect of offering a defined contribution add-on is positive, statistically significant, and sizeable. The magnitude of the effect is more than five times as large as the effect of time to vesting in column (2). The positive relationship suggests that teachers with more portable pension wealth take advantage of the portability by exiting the teaching market.

In column (3) we repeat the estimation from column (2) but control for time to vesting more flexibly by including dummy variables for one through five or more years until vesting and an indicator after the individual is vested. The omitted category for interpretation is the year the teacher vests. The estimated coefficients imply that teachers are significantly more likely to exit when they remain multiple years from vesting. The hazard declines as teachers approach vesting

[85] In unreported results we include a lagged measure of hourly wage, when available, in columns (2) and (3) to control for the opportunity cost of leaving teaching. The estimates across these columns are qualitatively similar to the estimates in Table C.7. However, the effect of the lagged wage is not statistically different from zero. We drop this analysis because many teachers do not report a wage, significantly reducing our sample size.
[86] In column (2) the Time to Vesting variable remains at zero after the individual is vested.

and becomes negative and statistically significant after vesting, implying teachers are less likely to leave once they are vested in the state pension system. This makes intuitive sense because teachers should respond to the incentives created by the deferred compensation in their defined benefit plan upon vesting by remaining with their employer. Overall, these results are consistent with forward-looking behavior among these teachers.

Lastly, in Table C.8 we show the preferred specification from Table C.7 for subsets of teachers in elementary and middle school, and for teachers in secondary school. Generally, the estimates in Table C.8 are consistent with the prior analyses. Focusing on the pension parameters, the effect of time to vesting is consistent with the results from Table C.7 for both populations. However, the availability of a defined contribution option has a much stronger effect for secondary school teachers, nearly three times as large as the effect for elementary and middle school teachers. Among the other covariates, the effects of marriage and the number of children in the household are positive, sizeable, and statistically significant for secondary school teachers, but do not appear to affect the hazard of first exit for elementary and middle school teachers.

3.6 Conclusion

This paper adds to the literature on the incentives created by teacher pension benefits by focusing on the early career, specifically the incentives created by vesting requirements. Our simulations of pension wealth at various points throughout a teacher's career show the variation across four states in the initial jump in pension wealth that occurs upon vesting. We also provide cross-sectional evidence that vesting requirements are related to the experience distribution of the teaching labor force. Lastly, we show that pension parameters, such as time to vesting and availability of defined contribution options, have significant effects on the probability of exit from teaching.

These findings have important implications for policies affecting the accumulation of teacher pension wealth, particularly in the current climate of pension reform. Using our baseline estimate, which suggests that an additional year to vesting increases the hazard of first exit by 0.023, we predict that an increase in a state's vesting requirements from 5 to 10 years increases the hazard of first exit by 0.11, an estimate that negates roughly one third of the effect of being granted tenure.

Given the literature relating teacher experience to student achievement, our findings could have important implications for student achievement. Future work in this literature should focus on identifying the teachers that are exiting the labor force in response to changes in retirement compensation. Improving our understanding of the exiting teachers will allow us to determine if the changes to the teaching distribution are helping or hurting students. While our current results do not extend to students directly, our evidence implies that young teachers are responsive to changes in pension wealth, and these effects need to be considered by policymakers.

APPENDICES

APPENDIX A

Tables for "Have You Ever Been Convicted of a Crime? The Effects of Juvenile Expungement on Crime, Educational, and Labor Market Outcomes"

Table A.1: State Level Descriptive Statistics

	Application (N=37)		*Automatic* (N=14)	
	Mean	Std. Error	Mean	Std. Error
Crime Indicators				
Juvenile Arrest Rate (Violent and Property Crime, per 1,000 population)	6.306	0.396	5.607	0.529
Juveniles in Residential Placement (per 1,000 juvenile population)	0.230	0.016	0.213	0.022
Violent Crime Rate (per 1,000 population)	3.628	0.210	3.736	0.492
Property Crime Rate (per 1,000 population)	28.844	0.971	27.986	1.682
Adult Prison Population (per 1,000 adult population)	1.164	0.075	1.144	0.120
Fraction of Prisoners with Maximum Sentence more than One Year	0.960	0.015	0.900	0.043
Forgiveness Ratio	0.272	0.038	0.392	0.103
Employed Police Officers (per 1,000 population)	2.590	1.153	2.145	0.253
State Expenditures (per 1,000 population)	269.612	92.990	311.342	150.506
Background Indicators				
Fraction of Population < 15	0.200	0.003	0.193	0.005
Fraction of Population 15 to 65	0.608	0.003	0.609	0.004
Median Household Income (1,000s)	52.881	1.229	53.695	2.428
Fraction of Population 25+ with High School Diploma	86.313	0.549	86.020	1.094
Fraction of Population 25+ with Bachelor's Degree	27.468	0.937	26.514	1.332
Fraction Black	0.137	0.020	0.072	0.018
Fraction Hispanic	0.088	0.005	0.126	0.016
Fraction Urban	0.750	0.022	0.705	0.049
Fraction of Population Living in Poverty	0.134	0.002	0.130	0.004
Fraction of Population Blue Collar Workers	0.237	0.003	0.225	0.004
Unemployment Rate	0.064	0.002	0.058	0.004
Head Start Participants (per 1,000 population)	3.796	0.250	3.669	0.409

Source: Author's calculations.
Note: All variables are averaged over 2006 to 2010. Forgiveness ratio measures the fraction of released prisoners per population in custody. State expenditures are state expenditures on the justice system. Crime rates and state expenditures unavailable for the District of Columbia. State level education data unavailable in 2010, so these variables are averaged from 2006 to 2009. Blue Collar workers are defined as workers in production, transportation, construction, installation, and maintenance.

Table A.2: Aggregate Expungement Statistics in Application States

State	Colorado	Michigan	Washington
Average Formal Handlings (1997-2010)	16,112	47,351	18,711
Expected Adjudications (1997-2010)	9,667	28,411	11,263
Average Expungements	187.18	50	1,210.65
Average Expungements ÷ Expected Adjudications	0.019	0.002	0.107
Years of Data Available	2003-2013	2009-2013	1997-2013

Source:

Colorado: Expungement case numbers come from Table 19 of the Annual Reports of the Judicial Branch of the State of Colorado.

Michigan: The number of juvenile set asides come from the Criminal History Unit of the Criminal Justice Information Center of the Michigan State Police.

Washington: Expungement numbers come from the Washington Administrative Office of the Courts.

Note: Formal handlings (delinquency petitions) come from the National Juvenile Court Data Archive, available at www.ojjdp.gov/ojstatbb/ezaco. The unit of count is cases disposed in all states with the exception of Colorado, where the unit of count is petitioned case filings by fiscal year, which include both delinquency and status offense cases. Therefore, the number reported as Average Formal Handlings for Colorado is likely biased upward. Note, however, that in the United States in 2009 there were 4.7 status offense cases for every 1,000 juveniles, while there were 49.3 delinquency cases per 1,000 juveniles (Puzzanchera et al. (2012). This implies that the magnitude of the bias is not likely to be particularly large.

Table A.3: Descriptive Statistics by Regime

	Application		*Automatic*	
	Mean	Std. Error	Mean	Std. Error
Total Sample (N=7469)				
Arrested as a Juvenile	0.159	0.014	0.188	0.014
Convicted as a Juvenile	0.316	0.015	0.332	0.027
Juvenile Conviction (N=403)				
Not arrested after age 20	0.532	0.031	0.639	0.051
Ever Attended College	0.239	0.027	0.314	0.052
Graduated College	0.063	0.016	0.111	0.037
Average Income (1,000s, 2008-2010)	21.467	1.345	23.782	2.142
Arrested, Not Convicted (N=859)				
Not arrested after age 20	0.671	0.020	0.639	0.037
Ever Attended College	0.344	0.020	0.342	0.038
Graduated College	0.098	0.014	0.089	0.023
Average Income (1,000s, 2008-2010)	25.772	1.016	23.892	1.524
Never Arrested (N=6188)				
Not arrested after age 20	0.860	0.005	0.846	0.011
Ever Attended College	0.658	0.007	0.611	0.015
Graduated College	0.349	0.007	0.301	0.014
Average Income (1,000s, 2008-2010)	29.996	0.366	29.793	0.704

Source: Author’s calculations.
Note: These statistics reflect responses from 7,469 respondents in the NLSY97 weighted by 1997 sampling weights (cumulative cases method). I drop 1,515 observations of individuals who missed at least one of the first five waves. I am unable to identify if these individuals had an arrest as a juvenile. Convicted as a juvenile is conditional on being arrested as a juvenile. Graduated college is an indicator of highest degree being Bachelor’s or higher.

Table A.4: Baseline Differences in Arrests

	(1) Juvenile Arrest	(2) Juvenile Arrest	(3) Juvenile Arrest	(4) Juvenile Arrest
Automatic Expunge	0.006	0.009	0.007	0.008
	(0.022)	(0.021)	(0.019)	(0.019)
Parental Income (1997)	-0.003*	-0.003*	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Age (1997)	0.000	0.000	-0.000	-0.000
	(0.003)	(0.002)	(0.002)	(0.002)
Black	0.005	0.005	-0.025+	-0.025
	(0.015)	(0.015)	(0.014)	(0.015)
Hispanic	-0.014	-0.014	-0.034*	-0.034+
	(0.017)	(0.018)	(0.017)	(0.018)
Female	-0.097**	-0.097**	-0.091**	-0.092**
	(0.012)	(0.012)	(0.012)	(0.012)
Living with Biological Mom	0.091**	0.092**	0.079**	0.079**
	(0.012)	(0.012)	(0.012)	(0.012)
Other Household Composition	0.117**	0.117**	0.100**	0.100**
	(0.022)	(0.021)	(0.021)	(0.020)
Custody Measure		2.424		2.454
		(2.803)		(2.540)
Sentencing Measure		0.004		-0.021
		(0.136)		(0.125)
Imprisonment Rate		-0.002		-0.001
		(0.009)		(0.008)
Forgiveness Ratio		0.015		0.027
		(0.038)		(0.035)
ASVAB			-0.054**	-0.054**
			(0.006)	(0.006)
R^2	0.063	0.063	0.082	0.083

Source: Author's calculations.
Note: The dependent variable is a binary indicator of arrest as a juvenile. All regressions are weighted using 1997 sampling weights and also include log of number of employed police officers per capita, log of expenditures on the state justice system per capita, unemployment rate, father's education, mother's education, an indicator for living in an urban area, log of Head Start enrollment, number of household members under 6 years old in 1997, household size in 1997, number of household members under 18 in 1997, and indicators for parental income, mother's education, or father's education missing. Columns (3) and (4) also include standardized ASVAB score along with an indicator for ASVAB missing. The reference group for household composition is living with both biological parents. Custody measure is the average number of juveniles in residential placement divided by average reported crime over 2006 to 2010. Sentencing measure is the fraction of prisoners under jurisdiction with maximum sentence greater than one year. Forgiveness ratio measures the fraction of released prisoners per population in custody. Nineteen observations are lost because expenditures are unavailable for the District of Columbia. Standard errors are clustered at the state level. Sample size is 7450 in all regressions. + P<0.10, * P<0.05, ** P<0.01.

Table A.5: Effect of Arrest and Conviction on Long-Term Outcomes

	(1) Not Arrested After Age 20?	(2) Attended College	(3) Graduated College	(4) log(Average Income)
NLSY Sample (N=7450)				
Juvenile Arrest	-0.182**	-0.284**	-0.160**	-0.287**
	(0.016)	(0.018)	(0.012)	(0.036)
R^2	0.097	0.192	0.209	0.120
NLSY Sample (N=7450)				
Juvenile Arrest	-0.150**	-0.223**	-0.155**	-0.229**
	(0.017)	(0.023)	(0.013)	(0.039)
Juvenile Convict	-0.101**	-0.080**	-0.014	-0.184**
	(0.037)	(0.030)	(0.016)	(0.063)
R^2	0.099	0.193	0.209	0.121

Source: Author's calculations.

Note: Each panel presents the results of a regression of the outcome of interest on an indicator for juvenile arrest (top panel) or indicator for juvenile arrest and juvenile conviction (bottom panel). Additional covariates are the same as column (2) in Table A.4. All regressions are weighted using 1997 sampling weights (cumulative cases method). Standard errors are clustered at the state level. Average income is calculated over 2008 to 2010. Nineteen observations are lost in this analysis because expenditures are unavailable for the District of Columbia. An example of the full regression output appears in Table G.12. + P<0.10, * P<0.05, ** P<0.01.

Table A.6: Long-Term Effects of Automatic Expungement: Proxy Variable Analysis

	(1) Not Arrested After Age 20?	(2) Attended College	(3) Graduated College	(4) log(Average Income)
Juvenile Convict Sample (N=403)				
Automatic Expunge	0.143*	0.077	0.051	0.253
	(0.060)	(0.067)	(0.037)	(0.157)
R^2	0.094	0.153	0.238	0.184
Juvenile Arrest Sample (N=859)				
Automatic Expunge	-0.000	0.017	-0.003	0.030
	(0.036)	(0.050)	(0.032)	(0.081)
R^2	0.070	0.203	0.178	0.177
Never Arrested Sample (N=6188)				
Automatic Expunge	0.010	-0.021	-0.028^{+}	-0.001
	(0.012)	(0.017)	(0.014)	(0.043)
R^2	0.056	0.142	0.182	0.107

Source: Author's calculations.

Note: Each panel restricts the sample to one of three categories: those who are never arrested as a juvenile, those who are arrested but not convicted, and those who are convicted. All regressions are weighted using 1997 sampling weights (cumulative cases method). Standard errors are clustered at the state level. Average income is calculated over 2008 to 2010. Additional covariates are the same as column (2) in Table A.4. Nineteen observations are lost in this analysis because expenditures are unavailable for the District of Columbia. An example of the full regression output appears in Table G.12. + P<0.10, * P<0.05, ** P<0.01.

Table A.7: Long-Term Effects of Automatic Expungement: Difference-in-Differences Analysis

	(1) Not Arrested After Age 20?	(2) Attended College	(3) Graduated College	(4) log(Average Income)
Treatment: Convicted				
Control: Arrested, Not Convicted				
Juvenile Convict x Automatic Expunge	0.153*	0.053	0.045	0.276+
	(0.057)	(0.056)	(0.056)	(0.159)
Juvenile Convict	-0.133**	-0.108*	-0.027*	-0.320**
	(0.045)	(0.043)	(0.012)	(0.083)
R^2	0.087	0.198	0.205	0.178
Treatment: Convicted				
Control: Never Arrested				
Juvenile Convict x Automatic Expunge	0.120*	0.086	0.055	0.225*
	(0.047)	(0.054)	(0.054)	(0.104)
Juvenile Convict	-0.279**	-0.316**	-0.177**	-0.473**
	(0.038)	(0.024)	(0.012)	(0.077)
R^2	0.094	0.176	0.198	0.124
Treatment: Arrested, Not Convicted				
Control: Never Arrested				
Juvenile Arrest x Automatic Expunge	-0.031	0.026	0.012	0.003
	(0.038)	(0.052)	(0.031)	(0.090)
Juvenile Arrest	-0.145**	-0.226**	-0.155**	-0.236**
	(0.017)	(0.026)	(0.015)	(0.047)
R^2	0.086	0.182	0.205	0.126

Source: Author's calculations.

Note: Each panel specifies the assumed treatment and control group for this difference-in-differences analysis. All regressions are weighted using 1997 sampling weights (cumulative cases method). Standard errors are clustered at the state level. Average income is calculated over 2008 to 2010. Additional covariates are the same as column (2). Nineteen observations are lost in this analysis because expenditures are unavailable for the District of Columbia. An example of the full regression output appears in Table G.12. + P<0.10, * P<0.05, ** P<0.01.

APPENDIX B

Tables for "Did Graduated Driver Licensing Laws Drive a Reduction in Crime?"

Table B.1: State Level Summary Statistics

Variables	Mean	Std. Dev.
Outcome Variables		
Pre-GDL Violent Arrest Ratio: Boys Age 16	3.55	1.073
Pre-GDL Violent Arrest Ratio: Girls Age 16	0.64	0.281
Pre-GDL Property Arrest Ratio: Boys Age 16	5.03	1.233
Pre-GDL Property Arrest Ratio: Girls Age 16	2.22	0.655
Pre-GDL Violent Crime: Boys Age 16 (per 1,000 pop)	10.46	5.201
Pre-GDL Violent Crime: Girls Age 16 (per 1,000 pop)	1.95	1.152
Pre-GDL Property Crime: Boys Age 16 (per 1,000 pop)	130.97	32.571
Pre-GDL Property Crime: Girls Age 16 (per 1,000 pop)	57.51	17.137
Covariates		
Speed Limit - 65	0.37	0.480
Speed Limit - 70+	0.59	0.490
Seat Belt (Primary Enforcement)	0.39	0.485
Seat Belt (Secondary Enforcement)	0.60	0.487
Blood Alcohol Concentration 0.08	0.71	0.439
Blood Alcohol Concentration 0.10	0.27	0.432
Admin License Revocation	0.92	0.270
Zero Tolerance	0.99	0.087
Secondary Enforcement of GDL Restrictions	0.22	0.412
Percent Black	0.12	0.116
Percent Less Than 15 Years Old	0.21	0.018
Percent 15-19 Years Old	0.07	0.006
Percent 20-24 Years Old	0.07	0.007
Percent 25-44 Years Old	0.28	0.022
Percent 45-64 Years Old	0.19	0.016
Percent 65 or Older	0.18	0.022
Percent Urban	0.73	0.150
Unemployment Rate	0.05	0.019
Median Household Income (thousands)	53.19	8.003
Total Number of Police Officers (per 1,000 population)	2.42	0.964
Total Expenditure on Justice System (per 1,000 population)	239.92	100.70
Custody – Deterrence	0.01	0.003
Custody – Incapacitation	0.01	0.004

Source: Author’s calculations.
Note: The unit of observation is state*year between 1997 and 2010. Arrest ratio proxies for the percent of all crimes committed by the specific population. Median household income is in 2011 dollars. Secondary enforcement mean is conditional on having GDL restrictions implemented. Following Levitt (1998), my measures of custody rates are the stock of juveniles in state facilities divided by the population age 15 to 17 (consistent with criminal deterrence) and the stock of juvenile in state facilities divided by the total number of reported crimes (consistent with criminal incapacitation).

Table B.2: Effect on Arrest Ratio by Age

	Age 13 – 14	Age 16	Age 17	Age 18 - 20
Violent Crime				
GDL	-0.095	-0.322^{+}	0.007	-0.076
	(0.200)	(0.162)	(0.109)	(0.181)
Pre-GDL Mean	3.995	4.185	4.748	14.542
Property Crime				
GDL	-0.175	-0.503*	-0.219	-0.103
	(0.192)	(0.196)	(0.154)	(0.198)
Pre-GDL Mean	9.281	7.256	7.015	15.943

Source: Author's calculations.
Note: The dependent variable in each regression is the arrest ratio for the specified crime and age categories. Arrest ratio can be interpreted as the percent of all crimes committed by the specific population. The additional covariates included in the regression are: a lagged measure of custody to proxy for severity, the natural logarithm of the state population, the natural logarithm of the number of employed police officers per capita, the natural logarithm of state expenditures on the justice system per capita, the fraction of the state population less than 15 years old, 20 to 24 years old, 25 to 44 years old, 45 to 64 years old, and older than 65, the fraction of the state that is urban, median household income, state policies related to driving, year fixed effects, and state fixed effects. Standard errors (in parentheses) are adjusted for clustering at the state level. Sample size is 632 in all regressions. + P<0.10, * P<0.05, ** P<0.01.

Table B.3: Primary and Secondary Enforcement, Age 16

	Arrest Ratio
Violent Crime	
GDL	-0.347*
	(0.166)
GDL x Secondary	0.272
	(0.209)
Property Crime	
GDL	-0.500*
	(0.203)
GDL x Secondary	-0.035
	(0.257)

Source: Author's calculations.

Note: The dependent variable in each regression is the arrest ratio for the specified crime categories for age 16. Arrest ratio can be interpreted as the percent of all crimes committed by 16 year olds. The additional covariates included in the regression are: a lagged measure of custody to proxy for severity, the natural logarithm of the state population, the natural logarithm of the number of employed police officers per capita, the natural logarithm of state expenditures on the justice system per capita, the fraction of the state population less than 15 years old, 20 to 24 years old, 25 to 44 years old, 45 to 64 years old, and older than 65, the fraction of the state that is urban, median household income, state policies related to driving, year fixed effects, and state fixed effects. Standard errors (in parentheses) are adjusted for clustering at the state level. Sample size is 632 in all regressions. + P<0.10, * P<0.05, ** P<0.01.

Table B.4: Robustness of Dependent Variable, Age 16

	Arrest Ratio	Adjusted Arrest Ratio	Log(Arrests Age 16)	Log(Arrest Ratio)	Crime Rate
Violent Crime					
GDL	-0.322^{+}	-0.815^{+}	-0.084*	-0.084*	-0.131*
	(0.162)	(0.482)	(0.040)	(0.037)	(0.063)
Pre-GDL Mean	4.185	8.169	5.001	1.388	2.409
Property Crime					
GDL	-0.503*	-1.271^{+}	-0.063^{+}	-0.077*	-0.085**
	(0.196)	(0.661)	(0.035)	(0.030)	(0.030)
Pre-GDL Mean	7.256	20.331	6.917	1.955	5.208

Source: Author's calculations.

Note: The dependent variable in each regression is the listed measure for the specified crime for age 16. Arrest ratio can be interpreted as the percent of all crimes committed by 16 year olds. The additional covariates included in the regression are: a lagged measure of custody to proxy for severity, the natural logarithm of the state population, the natural logarithm of the number of employed police officers, the natural logarithm of state expenditures on the justice system, the fraction of the state population less than 15 years old, 20 to 24 years old, 25 to 44 years old, 45 to 64 years old, and older than 65, the fraction of the state that is urban, median household income, state policies related to driving, year fixed effects, and state fixed effects. When using Log(Arrests Age 16) as the dependent variable I also include the natural logarithm of total arrests age 25 and older as a covariate. Standard errors (in parentheses) are adjusted for clustering at the state level. Sample size is 632 in all columns other than Crime Rate. The sample size for Crime Rate is 529. This reflects data from 49 states for the years that have over 85 percent coverage according to the Federal Bureau of Investigation. + P<0.10, * P<0.05, ** P<0.01.

Table B.5: Effect on Arrest Ratio by Gender, Age 16

	Male	Female
Violent Crime		
GDL	-0.293*	-0.029
	(0.141)	(0.045)
Pre-GDL Mean	3.548	0.637
Property Crime		
GDL	-0.358*	-0.145+
	(0.144)	(0.076)
Pre-GDL Mean	5.032	2.224

Source: Author's calculations.
Note: The dependent variable in each regression is the arrest ratio for the specified crime and gender for age 16. Arrest ratio can be interpreted as the percent of all crimes committed by the specific population. The additional covariates included in the regression are: a lagged measure of custody to proxy for severity, the natural logarithm of the state population, the natural logarithm of the number of employed police officers per capita, the natural logarithm of state expenditures on the justice system per capita, the fraction of the state population less than 15 years old, 20 to 24 years old, 25 to 44 years old, 45 to 64 years old, and older than 65, the fraction of the state that is urban, median household income, state policies related to driving, year fixed effects, and state fixed effects. Standard errors (in parentheses) are adjusted for clustering at the state level. Sample size is 632 in all regressions. + P<0.10, * P<0.05, ** P<0.01.

Table B.6: Effect on Specific Offenses by Gender, Age 16

	Violent Crime				*Property Crime*		
	Murder	Rape	Robbery	Aggravated Assault	Burglary	Larceny	Motor Vehicle Theft
Male							
GDL	-0.003	0.017	-0.077	-0.231**	-0.081+	-0.240*	-0.037
	(0.008)	(0.020)	(0.084)	(0.084)	(0.042)	(0.099)	(0.036)
Female							
GDL	0.000	0.001	-0.005	-0.025	0.000	-0.129+	-0.016
	(0.002)	(0.001)	(0.009)	(0.042)	(0.007)	(0.075)	(0.010)

Source: Author's calculations.
Note: The dependent variable in each regression is the arrest ratio for the specified crime and gender for age 16. Arrest ratio can be interpreted as the percent of all crimes committed by the specific population. The additional covariates included in the regression are: a lagged measure of custody to proxy for severity, the natural logarithm of the state population, the natural logarithm of the number of employed police officers per capita, the natural logarithm of state expenditures on the justice system per capita, the fraction of the state population less than 15 years old, 20 to 24 years old, 25 to 44 years old, 45 to 64 years old, and older than 65, the fraction of the state that is urban, median household income, state policies related to driving, year fixed effects, and state fixed effects. Standard errors (in parentheses) are adjusted for clustering at the state level. Sample size is 632 in all regressions. + $P<0.10$, * $P<0.05$, ** $P<0.01$.

Table B.7: Permanent Effects for Older Age Groups

	Age 18	Age 19	Age 20
Violent Crime			
GDL	-0.225	-0.073	-0.123
	(0.144)	(0.216)	(0.198)
Pre-GDL Mean	5.233	4.953	4.356
Property Crime			
GDL	-0.143	-0.107	0.088
	(0.162)	(0.154)	(0.149)
Pre-GDL Mean	6.680	5.248	4.015
N	448	406	365

Source: Author's calculations.
Note: The dependent variable in each regression is the arrest ratio for the specified crime and age categories. For Age 18 I drop observations within two years of GDL implementation to ensure all 18 year olds have been exposed to the policy. For Age 19 I drop observations within three years of GDL implementation, and for Age 20 I drop observations within four years of GDL implementation. Therefore, these results can be interpreted as the difference in the arrest ratio for the specified age between those who were not exposed to GDL and those who were exposed to GDL. Arrest ratio can be interpreted as the percent of all crimes committed by the specific population. The additional covariates included in the regression are: a lagged measure of custody to proxy for severity, the natural logarithm of the state population, the natural logarithm of the number of employed police officers per capita, the natural logarithm of state expenditures on the justice system per capita, the fraction of the state population less than 15 years old, 20 to 24 years old, 25 to 44 years old, 45 to 64 years old, and older than 65, the fraction of the state that is urban, median household income, state policies related to driving, year fixed effects, and state fixed effects. Standard errors (in parentheses) are adjusted for clustering at the state level. + $P<0.10$, * $P<0.05$, ** $P<0.01$.

Table B.8: Effect of Nighttime and Passenger Restrictions, Age 16

	Arrest Ratio
Violent Crime	
Nighttime Restrictions	-0.407^{+}
	(0.213)
Passenger Restrictions	0.284
	(0.180)
Property Crime	
Nighttime Restrictions	-0.480^{+}
	(0.259)
Passenger Restrictions	0.161
	(0.195)

Source: Author's calculations.
Note: The dependent variable in each regression is the arrest ratio for the specified crime categories for age 16. Arrest ratio can be interpreted as the percent of all crimes committed by 16 year olds. The additional covariates included in the regression are: a lagged measure of custody to proxy for severity, the natural logarithm of the state population, the natural logarithm of the number of employed police officers per capita, the natural logarithm of state expenditures on the justice system per capita, the fraction of the state population less than 15 years old, 20 to 24 years old, 25 to 44 years old, 45 to 64 years old, and older than 65, the fraction of the state that is urban, median household income, state policies related to driving, year fixed effects, and state fixed effects. Standard errors (in parentheses) are adjusted for clustering at the state level. Sample size is 632 in all regressions. + P<0.10, * P<0.05, ** P<0.01.

APPENDIX C

Tables for “Interstate Differences in Pension Vesting Rules, K-12 Teacher Experience, and Teacher Exit”

Table C.1: State Teacher Pension Parameters

State	Retirement Rule	Factor	Salary (Bachelor's, Experience=0)	Salary (Top Step)	Contribution Rate	Covered by Social Security?
California	60/5 Vesting = 5 FAS = highest year salary	1.4% to 2.4%, depending on age at retirement	$40,100	$75,400	8%	No
Florida	62/6, A/30 Vesting = 6 FAS = average of highest 8 years	1.6%	$33,300	$60,800	3%	Yes
Michigan	60/10, 46/30 Vesting = 10 FAS = average of highest 3 years	1.5%	$34,200	$66,700	$510 + 6.4% of any income over $15,000	Yes
Wisconsin	65/5, 57/30 Vesting = 5 FAS = average of highest 3 years	1.6%	$30,700	$57,100	6.65%	Yes

Source: SASS and state-specific handbooks detailed below.
California: CALSTRS 2013 Member Handbook (available at http://www.calstrs.com/sites/main/files/file-attachments/memberhandbook2013_web_v4.pdf)
Florida: Florida Retirement System Pension Plan Summary Plan Description (available at https://www.rol.frs.state.fl.us/forms/spd-pp.pdf)
Michigan: Michigan Public School Employees Retirement System Member Handbook (available at http://www.michigan.gov/documents/MPSERS1_92795_7.pdf)
Wisconsin: Wisconsin Retirement System Benefit Handbook (available at http://etf.wi.gov/publications/et2119.pdf)
Note: Retirement rule provides the minimum age and minimum years of service required for full retirement benefits. This is written as a fraction: minimum age/minimum years of service. "A" implies full retirement benefits at any age (provided the teacher has the minimum years of service). FAS stands for Final Average Salary.

Table C.2: Cost of Living Adjustments

State	Peak Value	Quit After 10 Years	Retire at 55	Retire at 60	Retire at 65
Michigan No COLA	$457,101 Age 55	$48,963 (-408,138)	$457,101 (0)	$443,489 (-13,612)	$399,078 (-58,023)
Michigan With COLA	$663,788 Age 55	$65,909 (-597,879)	$663,788 (0)	$632,911 (-30,877)	$561,805 (-101,983)

Source: Authors' calculations.
Note: This table compares the present discounted value of pension wealth for a simulated teacher in Michigan both with and without Cost of Living Adjustments (COLA). The assumed formula for Cost of Living Adjustments is three percent of annual benefit each year (not compounded) starting the October after one full year of retirement.

Table C.3: Simulation Results

State	Peak Value	Quit after 2 years	Quit after 5 Years	Quit After 10 Years	Retire at 55	Retire at 60	Retire at 65
California	$691,288	$0	$27,069	$56,223	$487,271	$691,288	$612,237
	Age 60	(-691,288)	(-664,219)	(-635,065)	(-204,017)	(0)	(-79,051)
Florida	$681,595	0	0	49,971	681,595	667,366	616,536
	Age 55	(-681,595)	(-681,595)	(-631,624)	(0)	(-14,229)	(-65,059)
Michigan	$663,788	0	0	65,909	663,788	632,911	561,805
	Age 55	(-663,788)	(-663,788)	(-597,879)	(0)	(-30,877)	(-101,983)
Wisconsin	$370,893	0	11,279	19,237	322,055	354,977	304,814
	Age 57	(-370,893)	(-359,614)	(-351,656)	(-48,838)	(-15,916)	(-66,079)
Michigan (Defined Contribution Plan)	---------	6,341	17,322	40,161	664,480	802,186	927,981

Source: Authors' calculations.
Note: Calculations assume teacher began work at age 25 with a Master's Degree. Difference from peak value appears in parentheses below the present discounted value.

Table C.4: Summary Statistics

	Mean	Standard Deviation	Minimum	Maximum
Experience < 4 years	15.39	4.864	6.04	28.30
4 < experience <= 9	27.12	4.669	18.60	52.01
10 <experience <= 14	17.83	3.690	9.59	27.22
Experience > 15	39.67	6.218	20.65	55.36
Years until vested	5.78	2.431	0	10
Purchase credits	0.63	0.486	0	1
Starting salary	$33,172	4,181	24,800	42,700

Source: Authors' calculations.

Notes: Experience measures the fraction of teachers that fall into each experience bin in each state. Purchase credits refers to the ability to purchase credits for prior service as a teacher.

Table C.5: Regression Results for Teacher Experience

	Percent with fewer than 4 years exper		Percent with 4 to 9 years exper		Percent with 10 to 14 years exper		Percent with 15 or more years exper	
Years until vested	-0.361*	-0.377*	0.201	-0.054	0.376**	0.276*	-0.217	0.156
	(0.145)	(0.163)	(0.156)	(0.198)	(0.118)	(0.135)	(0.232)	(0.255)
Purchase credits		-0.812		-2.128*		0.476		2.457*
		(0.735)		(1.025)		(0.784)		(1.114)
Average age		-0.929**		-1.154**		-0.036		2.114**
		(0.178)		(0.281)		(0.233)		(5.59)
Log (starting salary)		0.611		15.837**		4.667		-21.156**
		(2.731)		(4.581)		(3.678)		(4.331)
Log (pension wealth)		0.268		-1.999		-2.859+		4.546
		(1.788)		(2.101)		(1.675)		(2.962)
Constant	14.227**	44.218	26.365**	-59.696	17.421**	8.750	41.991**	107.959
	(1.014)	(36.421)	(1.360)	(49.864)	(0.884)	(41.301)	(1.889)	(65.177)
Obs.	102	102	102	102	102	102	102	102
R^2	0.48	0.62	0.02	0.36	0.29	0.34	0.04	0.48

Source: Authors' calculations.
Notes: Pension data are from the Public Fund Survey (2001-2010). Dependent variables are distribution of teacher experience from Schools and Staffing Survey (2008 and 2011). Pension wealth estimates come from Table 8 of Toutkoushian et al. (2011). Regressions also contain year dummies. Robust standard errors are in parentheses. + P<0.10, * P<0.05, ** P<0.01.

Table C.6: NLSY Descriptive Statistics 2002-2010

Variables	Mean	Std. Dev.
Teacher Population (779 individuals)		
Age when Begin Teaching	23.78	2.314
Female	0.66	0.473
Black	0.17	0.372
Hispanic	0.15	0.353
Married	0.20	0.399
Single	0.76	0.426
Number of Kids in Household	0.23	0.624
Family Income (Thousands)	66.51	65.931
ASVAB Percentile	68.72	24.143
Employment Characteristics		
Starting Wage (Hourly)	17.50	18.984
Wage After 5 Years (Hourly)	21.28	9.886
Postsecondary Teacher	0.25	0.432
Preschool/Kindergarten Teacher	0.05	0.210
Elementary/Middle School Teacher	0.40	0.490
Secondary School Teacher	0.19	0.390
Other Teacher	0.41	0.492
Job Tenure	2.16	1.424
Pension Characteristics		
Years to Vesting	5.88	2.316
DC Plan Available?	0.33	0.469
Choice Between DB and DC?	0.11	0.313
Retirement Factor	0.02	0.004
Covered by Social Security?	0.60	0.490
Employee Contribution Rate	0.06	0.022
Years to Tenure	3.15	0.738
Withdraw: Less or Equal Own Contribution	0.11	0.316
Withdraw: Own and Interest	0.77	0.423
Withdraw: Own, Interest, and Employer	0.12	0.320
Purchase Credits: No	0.29	0.456
Purchase Credits: Limited	0.29	0.456
Purchase Credits: Unlimited	0.41	0.491

Source: Authors' calculations.
Notes: ASVAB is an ability exam administered in the first wave of the NLSY. Other Teacher includes special education teachers and other teachers who do not fall into the other teaching categories. Contribution rate is conditional on rate being nonzero. Withdraw variables describe refund of contributions if teacher leaves the system before vesting. Service credits (for prior work as a teacher) are available for purchase in some states.

Table C.7: NLSY Regression Results

	(1)	(2)	(3)
Time to Vesting		0.092**	
		(0.021)	
		[0.023]	
5+ Years from Vesting			0.567*
			(0.250)
			[0.141]
4 Years from Vesting			0.578*
			(0.257)
			[0.143]
3 Years from Vesting			0.439^{+}
			(0.254)
			[0.109]
2 Years from Vesting			0.548*
			(0.244)
			[0.136]
1 Year from Vesting			-0.120
			(0.274)
			[-0.030]
Vested			-1.048**
			(0.328)
			[-0.238]
DC Also		0.538**	0.466**
		(0.122)	(0.123)
		[0.134]	[0.116]
Choice between DB/DC		-0.219	-0.282
		(0.189)	(0.192)
		[-0.054]	[-0.070]
ASVAB Percentile	0.008**	0.008**	0.009**
	(0.002)	(0.002)	(0.002)
	[0.002]	[0.002]	[0.002]
Age	0.008	0.059^{+}	0.065^{+}
	(0.032)	(0.034)	(0.035)
	[0.002]	[0.015]	[0.016]
Female	-0.187*	-0.191^{+}	-0.216*
	(0.091)	(0.098)	(0.099)
	[-0.047]	[-0.048]	[-0.054]
Black	0.045	0.133	0.127
	(0.124)	(0.134)	(0.136)
	[0.011]	[0.033]	[0.032]
Hispanic	0.240^{+}	0.217	0.170
	(0.133)	(0.147)	(0.149)
	[0.060]	[0.054]	[0.042]
Married	-0.198^{+}	-0.186	-0.143
	(0.107)	(0.116)	(0.117)

Table C.7 (cont'd)

	[-0.049]	[-0.046]	[-0.036]
Number of Kids in HH	0.076	0.052	0.093
	(0.071)	(0.078)	(0.080)
	[0.019]	[0.013]	[0.023]
Has Tenure		-1.410**	-1.303**
		(0.113)	(0.134)
		[-0.330]	[-0.307]
Retirement Factor		46.968*	28.681
		(21.921)	(22.252)
		[5.471]	[7.155]
Covered by Social Security		0.138	0.165
		(0.152)	(0.153)
		[0.034]	[0.041]
Employee Contribution Rate		-3.204	-1.505
		(3.408)	(3.456)
		[-0.800]	[-0.375]
N	2479	2479	2479

Source: Authors' calculations.
Notes: Dependent variable is the hazard of first exit from teaching. Regressions in columns (2) and (3) also contain year dummies, reported family income, indicator for mixed race, indicator for marital status unknown, indicator for separated/divorced, and indicators for pension portability. Standard errors in parentheses. Marginal effects in brackets. + P<0.10, * P<0.05, ** P<0.01.

Table C.8: NLSY Regression Results by Teacher Type

	(1) Elementary & Middle	(2) Secondary
Time to Vesting	0.117**	0.139*
	(0.034)	(0.066)
	[0.024]	[0.028]
DC Also	0.250	0.710*
	(0.219)	(0.329)
	[0.053]	[0.151]
Choice between DB/DC	-0.341	0.703
	(0.364)	(0.612)
	[-0.067]	[0.157]
ASVAB Percentile	0.000	-0.002
	(0.004)	(0.006)
	[0.000]	[-0.000]
Age	0.121^{+}	0.499**
	(0.067)	(0.123)
	[0.025]	[0.100]
Female	-0.472*	-0.433^{+}
	(0.191)	(0.247)
	[-0.103]	[-0.088]
Black	0.418^{+}	-0.232
	(0.222)	(0.383)
	[0.092]	[-0.045]
Hispanic	1.187**	0.974**
	(0.251)	(0.364)
	[0.277]	[0.220]
Married	-0.010	-0.620*
	(0.204)	(0.291)
	[-0.002]	[-0.120]
Number of Kids in HH	0.073	0.394*
	(0.130)	(0.199)
	[0.015]	[0.079]
Has Tenure	-2.109**	-1.897**
	(0.218)	(0.305)
	[-0.381]	[-0.347]
Retirement Factor	-48.492	8.479
	(43.856)	(62.129)
	[-10.157]	[1.704]
Covered by Soc Security	0.664*	-0.133
	(0.275)	(0.434)
	[0.135]	[-0.027]
Employee Contrib Rate	5.951	3.764
	(6.903)	(10.047)
	[1.247]	[0.756]

Table C.8 (cont'd)

N	999	483

Source: Authors' calculations.
Notes: Dependent variable is the hazard of first exit from teaching for the specific population. Regressions also contain year dummies, reported family income, indicator for mixed race, indicator for marital status unknown, indicator for separated/divorced, and indicators for pension portability. Standard errors in parentheses. Marginal effects in brackets. + P<0.10, * P<0.05, ** P<0.01.

APPENDIX D

Figures for "Have You Ever Been Convicted of a Crime? The Effects of Juvenile Expungement on Crime, Educational, and Labor Market Outcomes"

Figure D.1: Crime Decision by Ability and Expungement Policy

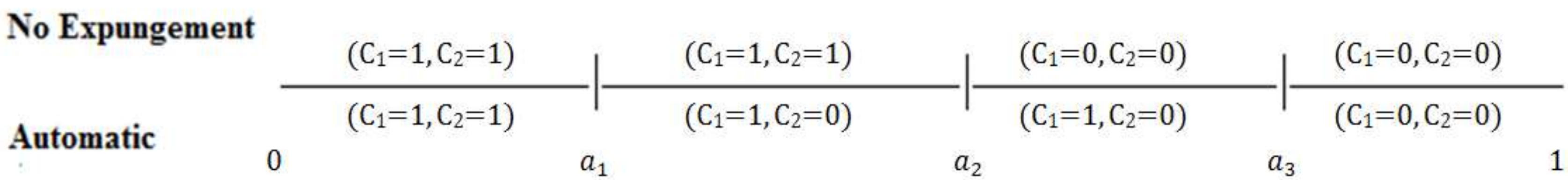
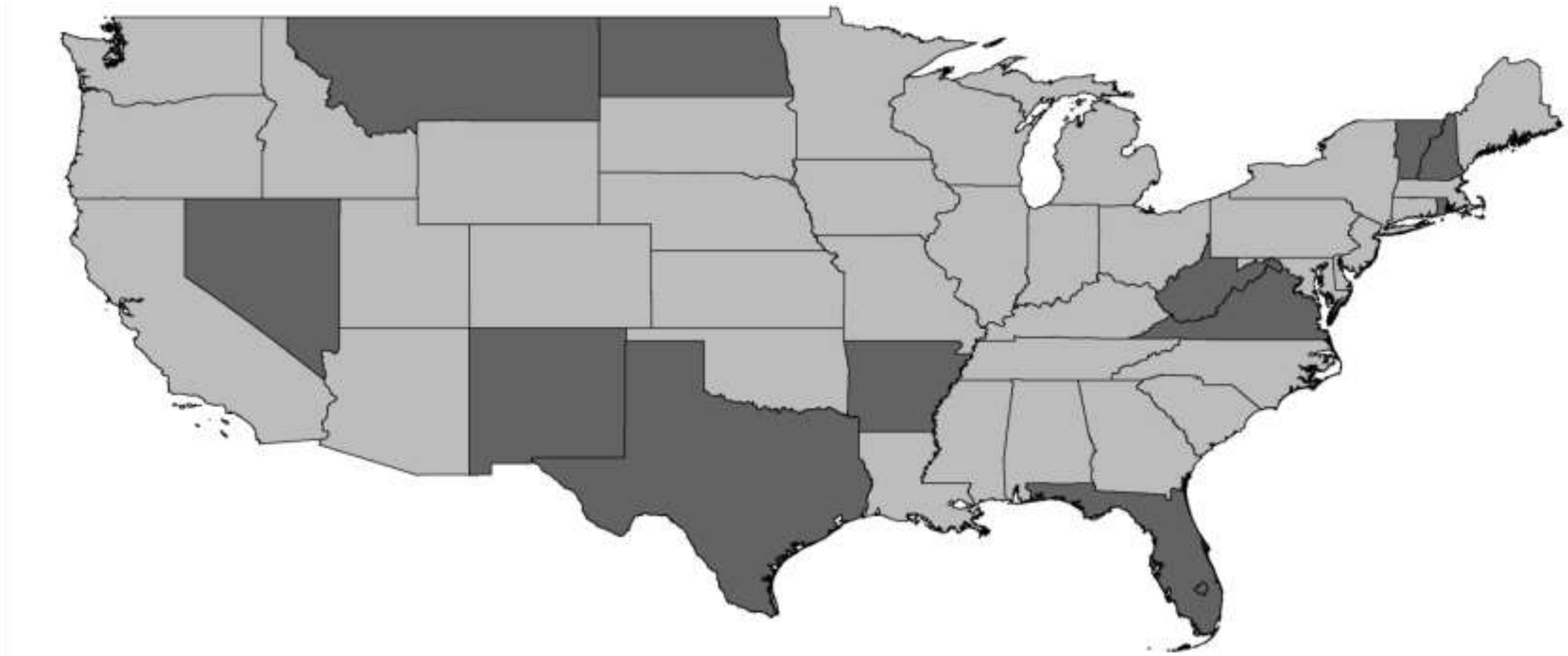


Figure D.2: Automatic Expungement States

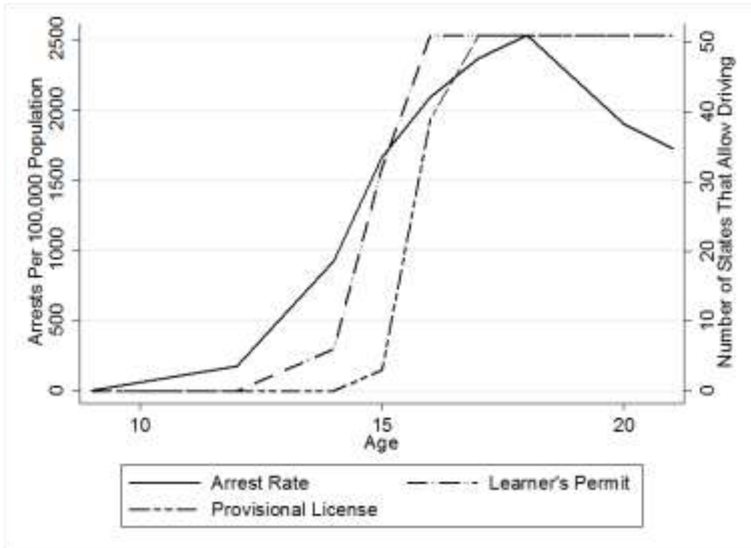


Source: Statutes detailed in Table G.1.
Note: States with automatic expungement statutes appear in dark gray. Alaska and Hawaii (excluded from this picture) both have automatic expungement statutes.

APPENDIX E

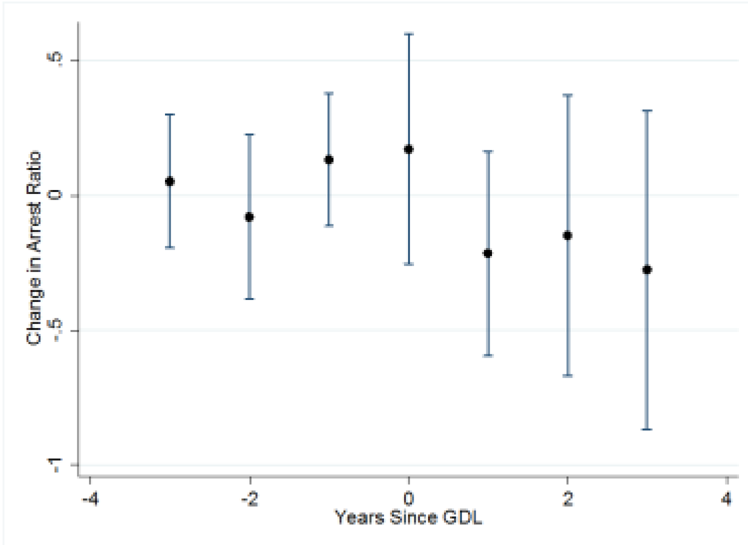
Figures for “Did Graduated Driver Licensing Laws Drive a Reduction in Crime?”

Figure E.1: Motor Vehicle Access and Crime



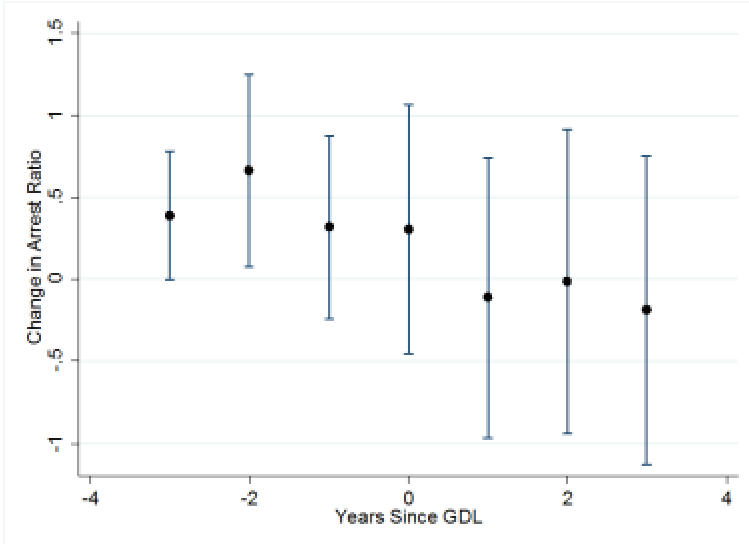
Source: Uniform Crime Reports (2010), Insurance Institute for Highway Safety (2013)
Note: All data in the figure are as of 2010. Arrest data come from Uniform Crime Reports and are national counts of arrests for violent and property crime divided by the population for the specified age group. The District of Columbia and Florida are excluded from the arrest data because arrests by age are unavailable.

Figure E.2: Event Study, Violent Crime



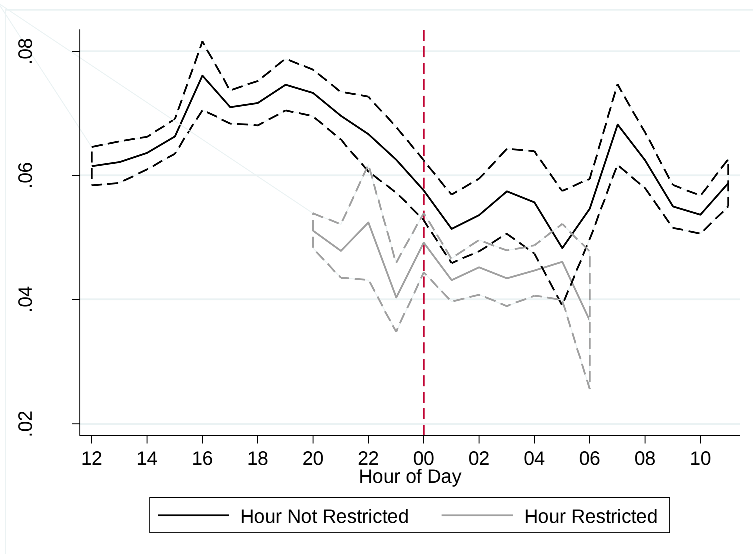
Source: Author's calculations.
Note: Each estimate comes from the preferred specification including dummy variables for leads and lags, pertinent covariates, state fixed effects, and year fixed effects. Arrest ratio can be interpreted as the percent of all crimes committed by 16 year olds. The reference group for the event study is four or more years prior to GDL implementation. The unit of observation is state*year. Standard errors are adjusted for clustering at the state level. Standard error bars are the 90 percent confidence interval for the estimates.

Figure E.3: Event Study, Property Crime



Source: Author's calculations.
Note: Each estimate comes from the preferred specification including dummy variables for leads and lags, pertinent covariates, state fixed effects, and year fixed effects. Arrest ratio can be interpreted as the percent of all crimes committed by 16 year olds. The reference group for the event study is four or more years prior to GDL implementation. The unit of observation is state*year. Standard errors are adjusted for clustering at the state level. Standard error bars are the 90 percent confidence interval for the estimates.

Figure E.4: Arrests by Time of Day

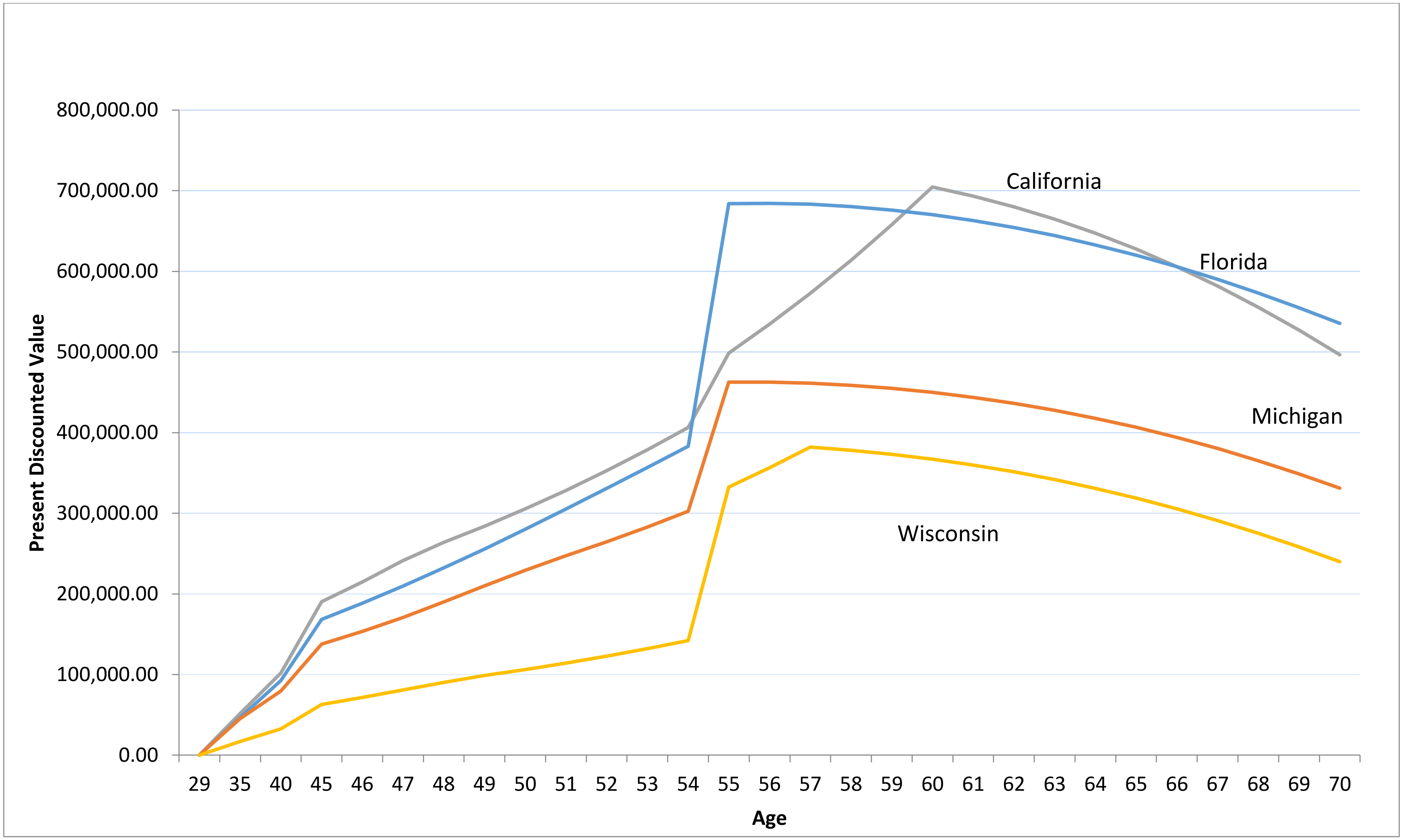


Source: Author's calculations, National Incident Based Reporting System.
Note: Each curve shows the fraction of total arrests that were 16 years old at each hour of the day. For each hour I plot the fraction in states where that hour is restricted due to a GDL restriction and the fraction in states where the hour is not restricted. No states have restrictions between 6:00 AM and 8:00 PM. Dashed lines are the 90% confidence interval for the estimates. The vertical dashed line represents midnight.

APPENDIX F

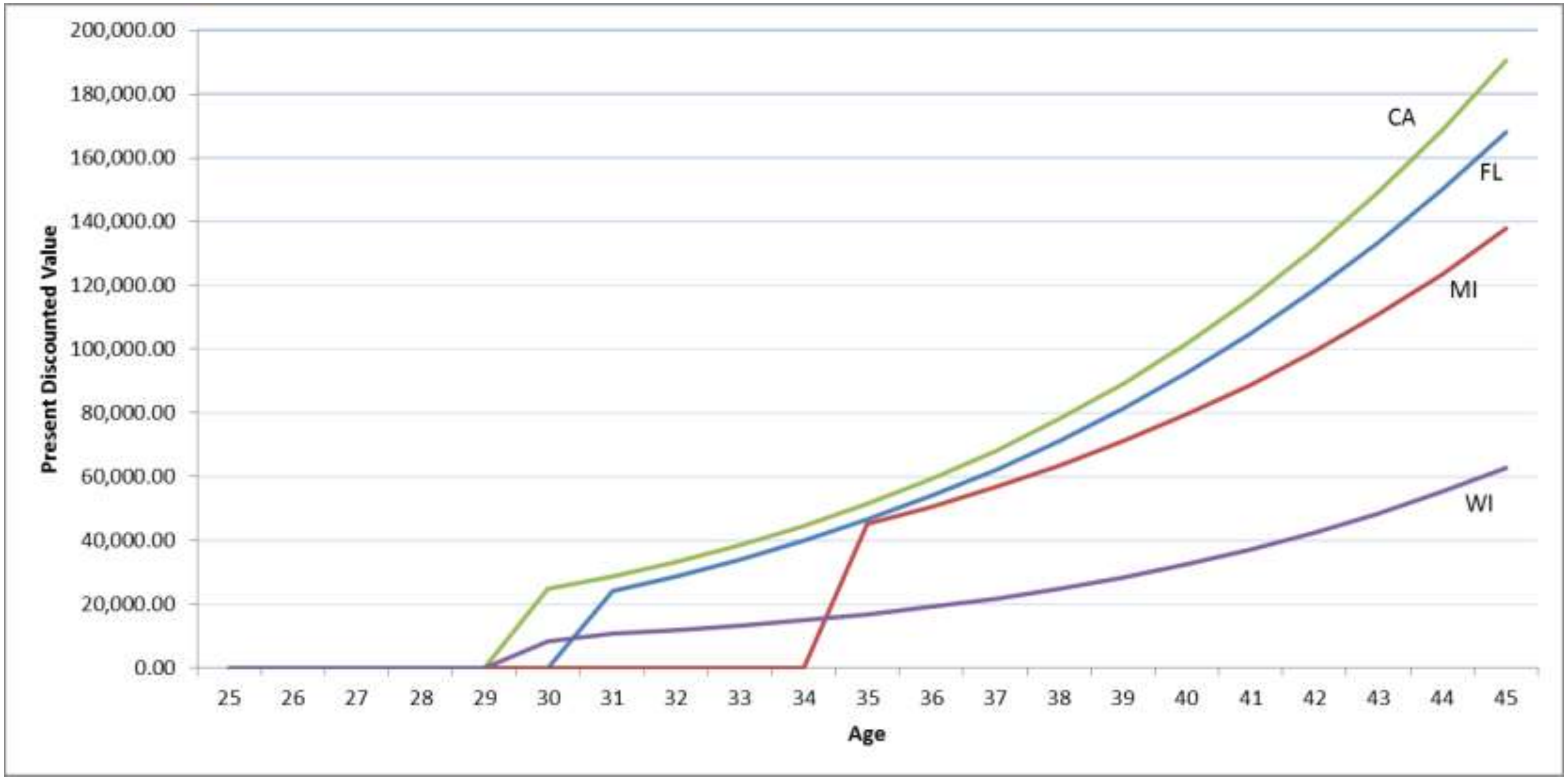
Figures for "Interstate Differences in Pension Vesting Rules, K-12 Teacher Experience, and Teacher Exit"

Figure F.1: Pension Wealth over the Teaching Career



Source: Authors' calculations.
Notes: Calculations assume teacher was hired at age 25 with a Bachelor's degree and no prior experience.

Figure F.2: Pension Wealth Early in the Teaching Career



Source: Authors' calculations.
Notes: Calculations assume teacher was hired at age 25 with a Bachelor's degree and no prior experience.

APPENDIX G

Appendices for "Have You Ever Been Convicted of a Crime? The Effects of Juvenile Expungement on Crime, Educational, and Labor Market Outcomes"

G.1: Expungement Statutes

This appendix presents tables focusing on expungement statutes.

Table G.1: Overview of Current Expungement Statutes by State

State	Current Statute
Alabama	**Citation:** Ala.Code 1975 § 12-15-136 **Terminology:** Seal **Brief Summary:** Requires application: 2 years since entry of order or final discharge from supervision and no other convictions.
Alaska	**Citation:** AS § 47.10.090 **Terminology:** Seal **Brief Summary:** Automatic sealing within 30 days of the child's 18th birthday or the day on which jurisdiction is released (whichever is later).
Arizona	**Citation:** A.R.S. § 8-348, 349 **Terminology:** Set Aside, Destruction **Brief Summary:** Requires application: must be at least 18 years old, not convicted of a felony, and no pending criminal charges. Certain crimes require waiting until 25 years old. More specifics depend on the initial crime.
Arkansas	**Citation:** A.C.A. § 9-27-309, A.C.A. § 16-90-901 through 16-90-905 **Terminology:** Expunge, Seal **Brief Summary:** Court may expunge record at any time and shall expunge record on 21st birthday. No specific requirements given for when an individual can apply for sealing.
California	**Citation:** Cal.Welf. & Inst.Code § 781 **Terminology:** Seal **Brief Summary:** Requires application: must be either 18 years old or five years after end of jurisdiction/final discharge; certain offenses cannot be sealed
Colorado	**Citation:** C.R.S. § 19-1-306 **Terminology:** Expunge **Brief Summary:** Requires application: certain offenses cannot be expunged, no pending charges, proof of rehabilitation to the court; amount of time required to wait depends on final disposition of the case.

Table G.1 (cont'd)

Connecticut	**Citation:** C.G.S.A. § 46b-146 **Terminology:** Erasure **Brief Summary:** Requires application: amount of time required to wait depends on nature of offense; no pending charges, child has reached 18 years of age.
Delaware	**Citation:** Del. Code Ann. tit. 10, § 1015 **Terminology:** Expunge **Brief Summary:** Requires application: amount of time required to wait depends on nature of offense; no pending charges.
District of Columbia	**Citation:** DC ST § 16-2335 **Terminology:** Seal **Brief Summary:** Requires motion of petitioner or Division's own motion; two years after final discharge/entry of order not involving supervision, no subsequent convictions or adjudications.
Florida	**Citation:** F.S.A. § 943.059, F.S.A. § 943.0585, F.S.A. § 943.0515 **Terminology:** Seal, Expunge **Brief Summary:** Petition required for sealing or early expungement: petitioner must obtain Certificate of Eligibility, certain crimes are ineligible to be expunged; if not a "serious" or "habitual" offender record is automatically expunged at age 24.
Georgia	**Citation:** Ga. Code Ann., § 15-11-79.2 **Terminology:** Seal **Brief Summary:** Requires application or the court's own motion: two years since the final discharge of the person, no pending charges, person has been rehabilitated.
Hawaii	**Citation:** HRS § 571-84 **Terminology:** N/A **Brief Summary:** The statute states that all records are open to inspection only by the persons whose official duties are concerned with the juvenile court, except as otherwise ordered by the court. According to the Hawaii Office of the Public Defender, this statute implies that all criminal records are automatically "per se sealed" (American Bar Association 2013).
Idaho	**Citation:** I.C. § 20-525A **Terminology:** Expunge **Brief Summary:** Requires application: petitioner must be at least 18 years old, amount of time depends on the nature of the offense, certain crimes ineligible to be expunged.
Illinois	**Citation:** 705 ILCS 405/5-915 **Terminology:** Expunge **Brief Summary:** Requires application: can apply when person has reached 17 years of age or all court proceedings have been terminated (whichever is later), certain crimes ineligible to be expunged, for more serious offenses must wait longer amount of time to apply.
Indiana	**Citation:** IC 31-39-8 **Terminology:** Expunge **Brief Summary:** Requires application: any person may petition at any time; court will consider a number of factors in determining whether to grant the expungement.

Table G.1 (cont'd)

Iowa	**Citation:** I.C.A. § 232.150 **Terminology:** Seal **Brief Summary:** Requires application or the court's own motion: person must be 18 years or older and two years must have elapsed since last action in case, no subsequent adjudications or convictions and no pending charges, restitution paid.
Kansas	**Citation:** K.S.A. 38-2312 **Terminology:** Expunge **Brief Summary:** Requires application: person must be 23 years old or two years have elapsed since final discharge, certain crimes ineligible for expungement, no subsequent adjudications or convictions and no pending charges.
Kentucky	**Citation:** KRS § 610.330 **Terminology:** Expunge **Brief Summary:** Requires application: two years must have passed since court's jurisdiction over the person or since person's unconditional release, certain crimes ineligible for expungement, no subsequent adjudications or convictions and no pending charges.
Louisiana	**Citation:** LSA-Ch.C. Art. 917 - 920 **Terminology:** Expunge **Brief Summary:** Requires a motion: person must be 17 years of age or older, certain crimes ineligible for expungement, five or more years elapsed since most recent judgment, no criminal felony convictions and no criminal court convictions for misdemeanors involving a weapon, no outstanding indictment or charges.
Maine	**Citation:** 15 M.R.S.A. § 3308 **Terminology:** Seal **Brief Summary:** Requires a petition: three years must have passed since discharge from the disposition ordered for the crime, no subsequent adjudications or convictions, no pending charges.
Maryland	**Citation:** MD Code, Courts and Judicial Proceedings, § 3-8A-27 **Terminology:** Seal **Brief Summary:** Requires a petition or the court's own motion. Records will be sealed if petitioner is over age 21.
Massachusetts	**Citation:** M.G.L.A. 276 § 100B **Terminology:** Seal **Brief Summary:** Requires a petition: three years since court appearance or final disposition, no subsequent adjudications or convictions (excluding certain motor vehicle offenses).
Michigan	**Citation:** M.C.L.A. 712A.18e **Terminology:** Set Aside **Brief Summary:** Requires application: offenses determine how many and which adjudications are eligible to be set aside; must wait one year following imposition of the disposition, one year following completion of any term of detention, or age 18 (whichever occurs latest).
Minnesota	**Citation:** M.S.A. § 260B.198 Subd. 6 **Terminology:** Expunge **Brief Summary:** Requires application. The court may expunge the adjudication of delinquency at any time that it deems advisable.

Table G.1 (cont'd)

Mississippi	**Citation:** Miss. Code Ann. § 43-21-263 **Terminology:** Seal **Brief Summary:** Requires application or its own motion: child who was the subject of the cause has attained 20 years of age, if the youth court dismisses the cause or if the youth court sets aside an adjudication in the cause.
Missouri	**Citation:** V.A.M.S. 211.321 **Terminology:** Seal, Destroy **Brief Summary:** Requires application by the child or its own motion: child must have reached 17th birthday, must be in best interests of the child.
Montana	**Citation:** Mont. Code Ann. § 41-5-216 **Terminology:** Seal **Brief Summary:** Records are automatically sealed on youth's 18th birthday; if jurisdiction extends beyond 18th birthday records must be sealed upon termination of jurisdiction.
Nebraska	**Citation:** Neb.Rev.St. § 43-2,108.01 through Neb.Rev.St. § 43-2,108.05 **Terminology:** Seal **Brief Summary:** Requires a proceeding to seal the record: the court may order the record sealed if it finds the juvenile has been rehabilitated to a satisfactory degree; factors determining rehabilitation include age of the juvenile, nature of the offense, behavior of the juvenile after the disposition or sentence, and education and employment history of the juvenile.
Nevada	**Citation:** N.R.S. 62H.130 - 150 **Terminology:** Seal **Brief Summary:** IF UNDER 21: Requires a petition by the child or a probation officer on behalf of the child: must wait three years since last adjudicated or was last seen in court; during this three year period child must not have been convicted of a felony or misdemeanor involving moral turpitude, and child must have been rehabilitated to satisfaction of the court. WHEN CHILD REACHES 21: All records are automatically sealed (some crimes are excepted).
New Hampshire	**Citation:** N.H. Rev. Stat. § 169-B:35 **Terminology:** Closed **Brief Summary:** Once a delinquent reaches 21 years of age all records shall be closed and placed in an inactive file.
New Jersey	**Citation:** N.J.S.A. 2C:52-4.1 (expunge); N.J.S.A. 2A:4A-62 (seal) **Terminology:** Expunge, Seal **Brief Summary:** SEAL: Requires motion by the person who has been subject of a complaint or court's own motion: two years must have elapsed since final discharge or since last entry of the court not involving custody or supervision, no subsequent adjudications or convictions and no pending charges EXPUNGE: five years must have elapsed since final discharge or since last entry of the court not involving custody or supervision, no subsequent adjudications or convictions and no pending charges, certain offenses ineligible to be expunged, has never had previous offense expunged, did not complete any diversion program.

Table G.1 (cont'd)

New Mexico	**Citation:** N. M. S. A. § 32A-2-26 **Terminology:** Seal **Brief Summary:** Before age 18 requires motion by or on behalf of the person who has been the subject of the delinquency proceedings: two years must have elapsed since release of person from custody or since entry of judgment not involving legal custody or supervision, no subsequent felony or misdemeanor involving moral turpitude and no pending charges, must show good cause for sealing. Upon age 18 or at the expiration of disposition (whichever occurs later) records are sealed automatically.
New York	**Citation:** Family Court Act § 375.2 **Terminology:** Seal **Brief Summary:** Requires motion of the respondent: motion may be filed at any time subsequent to the entering of finding of delinquency; motion may not be filed until the respondent is 16 years of age.
North Carolina	**Citation:** N.C.G.S.A. § 7B-3200 **Terminology:** Expunction **Brief Summary:** Requires a petition of the court: person must have reached 18 years of age if undisciplined or 16 years of age if delinquent, certain offenses ineligible to be expunged, 18 months since person was released from juvenile court jurisdiction, no subsequent adjudications or convictions.
North Dakota	**Citation:** NDCC, 54-23.4-17 **Terminology:** Sealed **Brief Summary:** Juvenile or law enforcement records must be sealed at the conclusion of proceedings. Sealed records are eventually destroyed pursuant to rules and policies established by the Supreme Court.
Ohio	**Citation:** R.C. § 2151.356 **Terminology:** Seal (sealed records can later be expunged) **Brief Summary:** Requires application of the person or the court's own motion: certain crimes ineligible to be expunged, must wait six months from date of either termination of order of the court, unconditional discharge of the person, or court order that the child is no longer a juvenile offender registrant; the court will order the record sealed if the person has been sufficiently rehabilitated.
Oklahoma	**Citation:** 10A Okl.St.Ann. § 2-6-108 **Terminology:** Seal **Brief Summary:** The court may order the records sealed if one of a number of conditions occur: one year has elapsed since the later of dismissal/closure of the case by the court or notice to the court of final discharge of supervision, the person has no subsequent criminal offenses in either juvenile or adult proceedings, and no juvenile or criminal proceeding is pending; no adjudication occurred; completion of diversion program; completion of military mentor program.
Oregon	**Citation:** O.R.S. § 419A.262 **Terminology:** Expunction **Brief Summary:** Requires application of the person or on court's own motion: if the matter is contested the following must be true: five years must have elapsed since most recent termination, no subsequent convictions of any felony or Class A misdemeanor, no pending criminal proceedings or investigations.

Table G.1 (cont'd)

Pennsylvania	**Citation:** 18 Pa.C.S.A. § 9123 **Terminology:** Expunge **Brief Summary:** Requires motion of the child, parent, guardian, or the court's own motion: six months have elapsed since the individual completed informal adjustment, diversion program, all terms and conditions of the sentence imposed following a conviction for a summary offense, all terms and conditions of the sentence imposed following a conviction for a violation; five years have elapsed since the final discharge of the person from commitment, placement, probation or any other disposition and referral; no subsequent convictions or adjudications, no pending charges, individual is 18 years or older.
Rhode Island	**Citation:** Gen.Laws 1956, § 14-1-6.1 **Terminology:** Seal **Brief Summary:** All court records shall be sealed upon final disposition of the case in the event of a no information, dismissal or not guilty finding or upon the completion of any sentence, probation and/or parole imposed.
South Carolina	**Citation:** Code 1976 § 63-19-2050 **Terminology:** Destruction **Brief Summary:** Requires petition by the person who committed the offense: certain offenses ineligible to be expunged, person must be at least 18 years of age, successfully completed any dispositional sentence imposed, and no subsequent criminal charges.
South Dakota	**Citation:** SDCL § 26-7A-114 through SDCL § 26-7A-116 **Terminology:** Seal **Brief Summary:** Requires court's own motion or petition of any party to the action: must occur one year after the unconditional release of the child from the court's jurisdiction or the discharge of the child by the Department of Corrections (whichever is later), no subsequent adjudications, no pending proceedings involving felonies, sexual contact offenses, or misdemeanors involving moral turpitude, child must be rehabilitated to court's satisfaction.
Tennessee	**Citation:** T. C. A. § 37-1-153 **Terminology:** Expunction **Brief Summary:** Requires petition by someone who was tried and adjudicated: must be 18 years or older, at least one year removed from most recent delinquency adjudication, certain offenses ineligible to be expunged, maintained a consistent and exemplary pattern of responsible, productive and civic-minded conduct for one or more years immediately preceding the filing of the expunction petition or has made such an adjustment of circumstances that the court believes that expunction serves the best interest of the child and the community.
Texas	**Citation:** V.T.C.A., Family Code § 58.003; V.T.C.A., Family Code § 58.204 **Terminology:** Sealing **Brief Summary:** At age 21 record is automatically sealed under "Automatic Restriction to Access of Records." Once this occurs the record can only be viewed by criminal justice agencies. Prior to age 21 requires application by the juvenile: two years must have passed since the final discharge of the person, no subsequent adjudications or convictions, no pending charges; These rules are slightly different for certain classes of offenses.

Table G.1 (cont'd)

Utah	**Citation:** U.C.A. § 78A-6-1105 **Terminology:** Expunge **Brief Summary:** Requires petition by person who has been adjudicated: must be 18 years or older, one year must have passed from the termination of the jurisdiction of the juvenile court or since the person's unconditional release from custody, certain offenses ineligible to be expunged.
Vermont	**Citation:** 33 V.S.A. § 5119 **Terminology:** Seal **Brief Summary:** NOTE: Statute change in 1996 changed Vermont to automatic sealing: for adjudications occurring after July 1, 1996 records are automatically sealed two years after the final discharge of the person unless the state's attorney objects and the person has committed certain offenses or has not been sufficiently rehabilitated. For adjudications occurring before July 1, 1996: record will be sealed if on application of the child or on the court's own motion the court finds: not convicted of certain offenses which are ineligible to be expunged, no pending charges or adjudications, the person's rehabilitation satisfies the court.
Virginia	**Citation:** VA Code Ann. § 16.1-306 **Terminology:** Expunge **Brief Summary:** On January 2 of each year the clerk destroys all records connected with juvenile proceeding if the juvenile has attained age 19 and five years have elapsed since the date of the last hearing in any case of the juvenile which is subject to this section (ie: this occurs automatically). Records for certain offenses are ineligible to be destroyed.
Washington	**Citation:** West's RCWA 13.50.050 **Terminology:** Seal **Brief Summary:** Requires a motion by the person who is the subject of the complaint: Specifics of the process depend on the offense which is trying to be sealed ; those who have gone through diversion programs may request that the records be destroyed.
West Virginia	**Citation:** W. Va. Code, § 49-5-18 **Terminology:** Marked **Brief Summary:** One year after the juvenile's eighteenth birthday, or one year after personal or juvenile jurisdiction has terminated, whichever is later, the records of a juvenile proceeding are automatically marked and moved to a separate secure confidential place; Marking the juvenile records to show they are to remain confidential has the legal effect of extinguishing the offense as if it never occurred.
Wisconsin	**Citation:** W.S.A. 938.355 (4m) **Terminology:** Expunge **Brief Summary:** Requires petition of court: person must have reached 17 years of age, person must have satisfactorily complied with the conditions of dispositional order and that the juvenile will benefit from and society will be harmed by expungement.
Wyoming	**Citation:** W.S.1977 § 14-6-241 **Terminology:** Expunge **Brief Summary:** Requires petition of the court: juvenile must have reached the age of majority, certain offenses ineligible to be expunged, no subsequent convictions, adjudications, or pending proceedings, rehabilitation of petitioner must satisfy the court.

Note: Automatic statutes are shaded in gray. While I include the primary citation for the pertinent statute, additional citations and explanations are available from the American Bar Association (2013).

Table G.2: Summary of Expungement Statutes

	Yes	No	Specific States
Does the state have automatic expungement?	14	37	Automatic states: AK, AR, FL, HI, MT, ND, NH, NM, NV, RI, TX, VA, VT, WV
After expungement did the event ever occur?	15	36	States that do not specify event never occurred: AK, AL, AZ, DE, HI, MD, MN, MS, ND, NH, NJ, NY, OH, RI, SD
Can an expunged record be used if the offender recidivates?	37	14	States where statutes do not mention that record can be reopened conditional on recidivism: AZ, CA, CT, IA, ID, IN, KY, MD, OR, RI, SC, UT, VA, WY

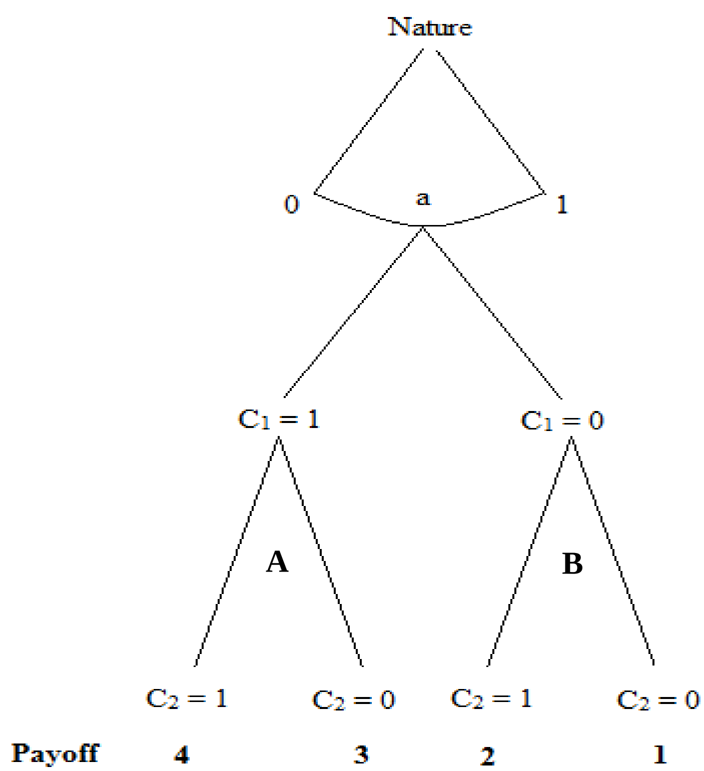
Source: State statutes detailed in Table G.1.

Note: This table is designed to show some of the variation in expungement statutes. The first row summarizes the states that have automatic expungement. The second row refers to states where after expungement the underlying criminal action is deemed to never have occurred by statute. The final row details if the record can be used against the juvenile if he or she commits a future crime.

G.2: Conceptual Framework

This appendix develops the conceptual framework completely.

Figure G.1: Game Tree and Payoffs



Payoffs:

No expungement

1. $C_1 = 0$, $C_2 = 0$: $S_1 a + S_2 a$
2. $C_1 = 0$, $C_2 = 1$: $S_1 a + S_2 a + (1-q)b - qfS_2 a$
3. $C_1 = 1$, $C_2 = 0$: $S_1 a + (1-q)b - qfS_1 a + qS_1 a + (1-q)S_2 a$
4. $C_1 = 1$, $C_2 = 1$: $S_1 a + (1-q)b - qfS_1 a + q[S_1 a + (1-q)b - qfS_1 a] + (1-q)[S_2 a + (1-q)b - qfS_2 a]$

Automatic expungement

1. $C_1 = 0$, $C_2 = 0$: $S_1 a + S_2 a$
2. $C_1 = 0$, $C_2 = 1$: $S_1 a + S_2 a + (1-q)b - qfS_2 a$
3. $C_1 = 1$, $C_2 = 0$: $S_1 a + (1-q)b - qfS_1 a + S_2 a$
4. $C_1 = 1$, $C_2 = 1$: $S_1 a + (1-q)b - qfS_1 a + S_2 a + (1-q)b - qfS_2 a$

I make the following assumptions in solving this game. The discount rate between the two periods is equal to one. Because q is a probability it must be between 0 and 1. For simplicity I assume q and b are uncorrelated with a, so all individuals are committing crimes with the same payoff and have the same probability of being caught. I also assume that q is large enough to impact behavior, so $q > \frac{1-f}{f}$. Therefore, I must assume f is between ½ and 1. The lower bound for f ensures that the assumption above remains within the bounds of q, and the upper bound ensures that one does not give up more than his entire salary if caught committing a crime. Lastly, S_2 must be sufficiently larger than S_1, so I assume $S_2 - S_1 > fS_1$.

I begin by solving the game in the world with no expungement. Using the payoffs above I can use backwards induction to determine how many people in the population will choose each action. Define the following three points (note that a_1 is the indifference point between payoffs 1 and 2 and a_2' is the indifference point between payoffs 3 and 4):

$$a_1 = \frac{(1-q)b}{qfS_2}$$

$$a_2 = \frac{2(1-q)b}{q[S_2 - S_1 + fS_2 + fS_1 - qfS_2 + qfS_1]}$$

$$a_2' = \frac{(1-q)b}{q^2 fS_1 + (1-q)qfS_2}$$

I can show that $a_1 < a_2 < a_2'$ and in this world individuals with $a < a_2$ choose $C_1 = 1$, $C_2 = 1$, while individuals with $a > a_2$ choose $C_1 = 0$, $C_2 = 0$.

Proof of $a_1 < a_2 < a_2'$:

$$q > \frac{1-f}{f}$$

$$fS_2 > S_2 - S_1 + fS_1 - qfS_2 + qfS_1$$

$$\frac{2(1-q)b}{q[S_2 - S_1 + fS_2 + fS_1 - qfS_2 + qfS_1]} > \frac{(1-q)b}{qfS_2}$$

$$a_2 > a_1$$

$$\frac{f-1}{f} < q$$

$$(S_1 - S_2)(f-1) > qf(S_1 - S_2)$$

$$S_2 - S_1 + fS_1 > qfS_1 + (1-q)fS_2$$

$$\frac{(1-q)b}{q^2 fS_1 + (1-q)qfS_2} > \frac{2(1-q)b}{q[S_2 - S_1 + fS_2 + fS_1 - qfS_2 + qfS_1]}$$

$$a_2' > a_2$$

To prove the result of the game I use a number of cases:

<u>Case 1</u>: Suppose $a < a_1$. Then the individual always chooses $C_1 = 1$, $C_2 = 1$.

Proof by contradiction: Because $a < a_1$, it is trivial to see the individual will always choose $C_2 = 1$. Suppose the individual chooses $C_1 = 0$. This implies:

$$S_1 a + S_2 a + (1-q)b - qfS_2 a$$
$$> S_1 a + (1-q)b - qfS_1 a + q[S_1 a + (1-q)b - qfS_1 a] + (1-q)[S_2 a + (1-q)b - qfS_2 a]$$

$$a > \frac{(1-q)b}{qfS_1 - qS_1 + qS_2 - q^2 fS_2 + q^2 fS_1}$$

But I can show:

$$q > \frac{1-f}{f}$$

$$q > \frac{(1-f)(S_2 - S_1)}{f(S_2 - S_1)}$$

$$q^2 f(S_2 - S_1) > q(1-f)(S_2 - S_1)$$

$$qfS_2 > qfS_1 - qS_1 + qS_2 - q^2 fS_2 + q^2 fS_1$$

$$\frac{(1-q)b}{qfS_1 - qS_1 + qS_2 - q^2 fS_2 + q^2 fS_1} > \frac{(1-q)b}{qfS_2}$$

$$\frac{(1-q)b}{qfS_1 - qS_1 + qS_2 - q^2 fS_2 + q^2 fS_1} > a_1$$

This implies $a > a_1$, a contradiction. So the individual always chooses $C_1 = 1$, $C_2 = 1$.

<u>Case 2</u>: Suppose $a_1 < a < a_2$. Then the individual chooses $C_1 = 1$, $C_2 = 1$.

Proof by contradiction. Because $a_1 < a < a_2'$ the individual will choose $C_2 = 1$ if at node **A** in the game tree, and the individual will choose $C_2 = 0$ if at node **B** in the game tree. Suppose the individual chooses $C_1 = 0$. This implies:

$$S_1 a + S_2 a > S_1 a + (1-q)b - qfS_1 a + q[S_1 a + (1-q)b - qfS_1 a] + (1-q)[S_2 a + (1-q)b - qfS_2 a]$$

$$a > \frac{2(1-q)b}{q[S_2 - S_1 + fS_2 + fS_1 - qfS_2 + qfS_1]}$$

$$a > a_2$$

a contradiction. So the individual always chooses $C_1 = 1$, $C_2 = 1$.

<u>Case 3</u>: Suppose $a_2 < a < a_2'$. Then the individual chooses $C_1 = 0$, $C_2 = 0$.

Proof by contradiction. Because $a_1 < a < a_2'$ the individual will choose $C_2 = 1$ if at node **A** in the game tree, and the individual will choose $C_2 = 0$ if at node **B** in the game tree. Suppose the individual chooses $C_1 = 1$. This implies:

$$S_1 a + S_2 a < S_1 a + (1-q)b - qfS_1 a + q[S_1 a + (1-q)b - qfS_1 a] + (1-q)[S_2 a + (1-q)b - qfS_2 a]$$

$$a < \frac{2(1-q)b}{q[S_2 - S_1 + fS_2 + fS_1 - qfS_2 + qfS_1]}$$

$a < a_2$

a contradiction. So the individual always chooses $C_1 = 0$, $C_2 = 0$.

<u>Case 4</u>: Suppose $a > a_2'$. Then the individual will always choose $C_1 = 0$, $C_2 = 0$.

Proof by contradiction: Because $a > a_2'$, it is trivial to see the individual will always choose $C_2 = 0$. Suppose the individual chooses $C_1 = 1$. This implies:

$$S_1 a + S_2 a < S_1 a + (1-q)b - qfS_1 a + qS_1 a + (1-q)S_2 a$$

$$a < \frac{(1-q)b}{qS_2 - qS_1 + qfS_1}$$

But I can show:

$$q > \frac{f-1}{f}$$

$$qf(S_2 - S_1) > fS_2 - fS_1 + S_1 - S_2$$

$$qS_2 - qS_1 + qfS_1 > q^2 fS_1 + (1-q)qfS_2$$

$$\frac{2(1-q)b}{q[S_2 - S_1 + fS_2 + fS_1 - qfS_2 + qfS_1]} > \frac{(1-q)b}{qS_2 - qS_1 + qfS_1}$$

$$a_2 > \frac{(1-q)b}{qS_2 - qS_1 + qfS_1}$$

This implies $a < a_2$, a contradiction. So the individual always chooses $C_1 = 0$, $C_2 = 0$.

Next I solve the game in the world with automatic expungement.

Using the payoffs above, I can use backwards induction to determine how many people in the population will choose each action. Define the following points (note that a_1, which is also defined above, is the indifference point between payoffs 1 and 2 and the indifference point between payoffs 3 and 4 in this world):

$$a_1 = \frac{(1-q)b}{qfS_2}$$

$$a_3 = \frac{(1-q)b}{qfS_1}$$

I can show that in this world individuals with $a < a_1$ choose $C_1 = 1$, $C_2 = 1$, individuals with $a_1 < a < a_3$ choose $C_1 = 1$, $C_2 = 0$, and individuals with $a > a_3$ choose $C_1 = 0$, $C_2 = 0$.

Proof of $a_1 < a_3$: It is trivial to see that $a_1 < a_3$ because $S_2 > S_1$.

I prove the result of the game using a number of cases:

<u>Case 1</u>: Suppose $a < a_1$. Then the individual always chooses $C_1 = 1$, $C_2 = 1$.

Proof by contradiction: Because $a < a_1$, it is trivial to see the individual will always choose $C_2 = 1$. Suppose the individual chooses $C_1 = 0$. This implies:

$$S_1a + S_2a + (1-q)b - qfS_2a > S_1a + (1-q)b - qfS_1a + S_2a + (1-q)b - qfS_2a$$

$$qfS_1a > (1-q)b$$

$$a > \frac{(1-q)b}{qfS_1}$$

$$a > a_3$$

This implies $a > a_1$, a contradiction. So, the individual always chooses $C_1 = 1$, $C_2 = 1$.

<u>Case 2</u>: Suppose $a_1 < a < a_3$. Then the individual chooses $C_1 = 1$, $C_2 = 0$.

Proof by contradiction. Because $a > a_1$, it is trivial to see the individual will always choose $C_2 = 0$. Suppose the individual chooses $C_1 = 0$. This implies:

$$S_1a + S_2a > S_1a + (1-q)b - qfS_1a + S_2a$$

$$qfS_1a > (1-q)b$$

$$a > \frac{(1-q)b}{qfS_1}$$

$$a > a_3$$

a contradiction. So the individual always chooses $C_1 = 1$, $C_2 = 0$.

<u>Case 3</u>: Suppose $a > a_3$. Then the individual chooses $C_1 = 0$, $C_2 = 0$.

Proof by contradiction. Because $a > a_1$, it is trivial to see the individual will always choose $C_2 = 0$. Suppose the individual chooses $C_1 = 1$. This implies:

$$S_1 a + S_2 a < S_1 a + (1-q)b - qfS_1 a + S_2 a$$

$$qfS_1 a < (1-q)b$$

$$a < \frac{(1-q)b}{qfS_1}$$

$$a < a_3$$

a contradiction. So the individual always chooses $C_1 = 0$, $C_2 = 0$.

Finally, to be able to compare across these policy regimes, I prove that $a_3 > a_2'$:

$$S_1 < S_2$$

$$qfS_1 < q^2 fS_1 + qfS_2 - q^2 fS_2$$

$$\frac{(1-q)b}{q^2 fS_1 + qfS_2 - q^2 fS_2} < \frac{(1-q)b}{qfS_1}$$

$$a_2' < a_3$$

G.3: Data and Descriptive Statistics

This appendix focuses on descriptive statistics across each of my data sources.

Table G.3: All Expungement Data

	Application States			*Automatic States*		
Year	Michigan	Washington	Colorado	Texas	Florida	Virginia
1997	.	1289	.	.	.	27116
1998	.	1327	.	123	.	27553
1999	.	1277	.	309	.	27789
2000	.	1366	.	517	.	26037
2001	.	1268	.	516	.	24308
2002	.	1355	.	754	7961	21874
2003	.	1393	158	805	9736	19331
2004	.	1309	149	810	10607	15215
2005	.	1350	182	1114	10860	13638
2006	.	1331	185	890	11416	10889
2007	.	1561	202	1560	12053	8164
2008	.	1736	146	1446	13497	5421
2009	29	1679	183	1697	14491	2470
2010	34	1158	191	2045	16945	360
2011	40	713	174	1776	17796	40
2012	48	416	246	2041	18272	36
2013	99	53	243	.	12947	21

Source:

Michigan: Criminal Justice Information Center, Michigan State Police (juvenile set asides)
Washington: Washington Administrative Office of the Courts (expungement filing numbers)
Colorado: Annual Reports of the Judicial Branch of the State of Colorado, Table 19 (expungement case numbers)
Texas: Crime Records Service of the Department of Public Safety (expungements)
Florida: Florida Department of Law Enforcement (Certificates of Eligibility needed in the expungement process)
Virginia: Virginia Department of Juvenile Justice (expungements)

Note: The numbers reported for Texas and Florida reflect the number of expungements by application despite the fact that these states are automatic. Many automatic states allow for expungement by application before the automatic expungement occurs. The numbers reported for Virginia represent all automatic expungements in the state. These numbers decrease in recent years because the date associated with the statistic is the date of intake (or date of arrest), meaning that many recent cases are not yet eligible for automatic expungement. State officials from Florida and Virginia confirmed that the rate of expungement for those who are eligible is one.

Table G.4: NLSY Descriptive Statistics

Overall (N=7,469)	Mean	Std. Dev.
Female	0.490	0.500
Black	0.153	0.360
Hispanic	0.128	0.334
Urban (1997)	0.685	0.465
Age (1997)	14.271	1.489
Live with both biological parents (1997)	0.534	0.499
Live with only biological mother (1997)	0.238	0.426
Household size (1997)	4.459	1.426
Total under 18 in household (1997)	2.367	1.190
Automatic State (1997)	0.200	0.400
HS Grad	0.806	0.395
Ever Attended College	0.594	0.491
Graduated College	0.299	0.458
Juvenile Arrest	0.164	0.371
Juvenile Charge	0.101	0.302
Juvenile Conviction	0.054	0.226
Juvenile Incarceration	0.021	0.144
Age (2008)	25.830	1.452
Average Income (1,000s) (2008-2010)	29.000	21.252

Source: Author’s calculations.
Note: These statistics reflect responses from 7,469 respondents in the NLSY97 weighted by 1997 sampling weights (cumulative cases method). I drop 1,515 observations of individuals who missed at least one of the first five waves. I am unable to identify if these individuals had an arrest as a juvenile. Graduated college is an indicator of highest degree being Bachelor’s or higher.

Table G.5: Descriptive Statistics by Crime

	Application (N=36)		*Automatic States* (N=13)		
	Mean	Std. Error	Mean	Std. Error	p-value (difference)
Disorderly Conduct	199.338	27.411	169.367	42.171	0.569
Drug Crimes	184.935	18.101	163.759	15.856	0.508
Larceny	371.805	31.340	360.697	44.275	0.851
Burglary	76.388	5.628	65.804	7.648	0.316
Aggravated Assault	52.374	5.294	42.817	6.553	0.328
Robbery	27.197	4.434	17.620	4.590	0.232
Rape	3.933	0.393	3.982	0.581	0.948
Murder	0.966	0.106	0.659	0.123	0.116

Source: Author's calculations.
Note: This analysis reflects the juvenile arrest rate per 100,000 population, where "juvenile" is defined as being below the criminal age of majority. This analysis covers the years 2006 to 2010. Florida and Washington D.C. are excluded due to poor data quality.

G.4: State Data Sources

I use a number of different data sources to provide important covariates throughout my analysis. I describe those sources and the particular data elements I use in this appendix.

The primary source of data on crime and arrests at the state level are the Uniform Crime Reports (UCR) published by the Federal Bureau of Investigation. I use the total level of reported crime by state over 2006 to 2010 as the denominator of my proxy variable for the unobserved juvenile justice environment. I also use data from the UCR to calculate a number of the crime covariates included in Table A.1, and I use arrest rates in my state analysis in Table G.5, Table G.6, and Table G.7. Lastly, the UCR provides information on employed police officers and state expenditures on the justice system. I include these measures, scaled by population, as covariates in many analyses.

Another source of justice data I use is count data on the number of prisoners in custody. The Census of Juveniles in Residential Placement provides the number of juveniles in state custody over time. I use the average number of juveniles in residential placement between 2006 and 2010 as the numerator of my proxy variable for the unobserved juvenile justice environment.[87] The Bureau of Justice statistics collects similar data for counts of adult prisoners that I present in Table A.1.

The last source of justice data I use is the National Juvenile Court Data Archive (NJCDA). I use published data from the NJCDA on state and county juvenile court case counts to determine the number of petitioned delinquency case counts by state and year. I present these data as the denominator of the calculated expungement rate in Table A.2.

[87] Note that data are only collected in 2006, 2007, and 2010 for this time period.

Lastly, I use data from a number of different sources for the other background covariates I include in my analyses. For example, I use population by age measures from the Surveillance, Epidemiology, and End Results (SEER) Program to standardize many of the covariates. I use data from the U.S. Census Bureau to determine the demographic indicators I include in Table A.1. I also use data from the Bureau of Labor Statistics to calculate the unemployment rate and fraction of the population working in blue collar jobs. Data on the number of Head Start participants by state come from the Kids Count Data Center.

G.5: State Level Analysis

This appendix focuses on analyses of expungement using state level data sources.

Table G.6: State Level Juvenile Crime Regressions

	(1) Disorderly Conduct	(2) Drug Offenses	(3) Larceny	(4) Burglary	(5) Aggravated Assault	(6) Robbery	(7) Rape	(8) Murder
Automatic	-23.503	-50.963	-82.627	-13.353	-9.356	-1.646	-0.326	-0.265
	(61.311)	(32.235)	(54.022)	(11.385)	(9.049)	(6.963)	(0.813)	(0.185)
log(officers)	-68.189	-110.911	-409.032*	-66.195*	-20.939	-27.905	-2.236	-0.720
	(173.317)	(91.124)	(152.713)	(32.183)	(25.580)	(19.683)	(2.297)	(0.523)
log(expenditures)	-37.228	71.870+	75.376	29.235+	38.970**	21.765*	1.372	0.300
	(79.805)	(41.959)	(70.317)	(14.819)	(11.778)	(9.063)	(1.058)	(0.241)
Unemp Rate	-23.557	-23.488*	-52.746**	-0.397	-1.671	-0.805	-0.428	-0.055
	(20.921)	(10.999)	(18.434)	(3.885)	(3.088)	(2.376)	(0.277)	(0.063)
Fraction Black	1.442	0.951	-0.714	1.493*	0.952	1.233**	-0.001	0.042**
	(3.884)	(2.042)	(3.422)	(0.721)	(0.573)	(0.441)	(0.051)	(0.012)
Fraction Hispanic	0.010	0.008	-0.002	0.007	0.005	-0.015	-0.009	0.025+
	(0.012)	(0.017)	(0.010)	(0.009)	(0.010)	(0.012)	(0.011)	(0.015)
Fraction Urban	2.353	1.677	4.027+	0.247	0.708+	1.062**	0.040	0.003
	(2.429)	(1.277)	(2.140)	(0.451)	(0.358)	(0.276)	(0.032)	(0.007)
Observations	49	49	49	49	49	49	49	49

Source: Author's calculations.
Note: This analysis uses average juvenile arrest rates from 2006 to 2010 for the listed crime among 49 states. The District of Columbia and Florida are excluded due to poor data quality. Officers and expenditures are expressed in per capita terms. The dependent variable is the juvenile arrest rate per 100,000 population. Standard errors appear in parentheses. + P < .10; * P < .05; ** P < .01.

Table G.7: State Level Adult Crime Regressions

	(1) Disorderly Conduct	(2) Drug Offenses	(3) Larceny	(4) Burglary	(5) Aggravated Assault	(6) Robbery	(7) Rape	(8) Murder
Automatic	-14.697	-21.452+	-8.519	-2.011	-2.146	0.118	-0.035	-0.034
	(12.466)	(11.082)	(8.960)	(2.101)	(4.375)	(0.889)	(0.206)	(0.107)
log(officers)	30.838	-13.245	-32.175	-2.480	1.273	-2.328	-0.728	-0.274
	(35.240)	(31.328)	(25.328)	(5.938)	(12.368)	(2.514)	(0.581)	(0.303)
log(expenditures)	2.432	5.405	10.391	3.883	18.287**	1.851	0.599*	0.036
	(16.226)	(14.425)	(11.662)	(2.734)	(5.695)	(1.158)	(0.268)	(0.140)
Unemp Rate	0.376	-1.150	-0.700	1.296+	2.002	0.617*	-0.029	0.031
	(4.254)	(3.782)	(3.057)	(0.717)	(1.493)	(0.304)	(0.070)	(0.037)
Fraction Black	-0.460	2.172**	0.960+	0.365**	0.344	0.184**	0.015	0.034**
	(0.790)	(0.702)	(0.568)	(0.133)	(0.277)	(0.056)	(0.013)	(0.007)
Fraction Hispanic	-0.457	1.394*	0.281	0.195+	0.450+	0.015	-0.001	0.016**
	(0.667)	(0.593)	(0.480)	(0.112)	(0.234)	(0.048)	(0.011)	(0.006)
Fraction Urban	-0.075	0.030	0.065	-0.097	-0.133	0.089*	0.003	-0.005
	(0.494)	(0.439)	(0.355)	(0.083)	(0.173)	(0.035)	(0.008)	(0.004)
Observations	49	49	49	49	49	49	49	49

Source: Author's calculations.

Note: This analysis uses average adult arrest rates from 2006 to 2010 for the listed crime among 49 states, where adult is defined as being above the age of criminal majority. The District of Columbia and Florida are excluded due to poor data quality. Officers and expenditures are expressed in per capita terms. The dependent variable is the adult arrest rate per 100,000 population. Standard errors appear in parentheses. + P < .10; * P < .05; ** P < .01.

G.6: Robustness and Full Output

This appendix focuses on robustness of the primary results and presenting an example of full output.

Table G.8: Long-Term Effects of Automatic Expungement: Proxy Variable Analysis (Unweighted)

	(1) Not Arrested After Age 20?	(2) Attended College	(3) Graduated College	(4) log(Average Income)
Juvenile Convict Sample (N=403)				
Automatic Expunge	0.101+	0.068	0.041	0.317*
	(0.055)	(0.056)	(0.030)	(0.141)
R^2	0.106	0.144	0.206	0.177
Juvenile Arrest Sample (N=859)				
Automatic Expunge	-0.015	-0.005	0.016	-0.009
	(0.032)	(0.049)	(0.024)	(0.094)
R^2	0.072	0.186	0.152	0.181
Never Arrested Sample (N=6188)				
Automatic Expunge	0.009	0.003	-0.009	-0.029
	(0.013)	(0.017)	(0.017)	(0.040)
R^2	0.063	0.135	0.176	0.115

Source: Author's calculations.

Note: Each panel restricts the sample to one of three categories: those who are never arrested as a juvenile, those who are arrested but not convicted, and those who are convicted. Standard errors are clustered at the state level. Average income is calculated over 2008 to 2010. Additional covariates are the same as column (2) in Table A.4. Nineteen observations are lost in this analysis because expenditures are unavailable for the District of Columbia. An example of the full regression output appears in Table G.12. + P<0.10, * P<0.05, ** P<0.01

Table G.9: Long-Term Effects of Automatic Expungement: Difference-in-Differences Analysis (Unweighted)

	(1) Not Arrested After Age 20?	(2) Attended College	(3) Graduated College	(4) log(Average Income)
Treatment: Convicted				
Control: Arrested, Not Convicted				
Juvenile Convict x Automatic Expunge	0.113*	0.077	0.019	0.237
	(0.043)	(0.049)	(0.040)	(0.143)
Juvenile Convict	-0.130**	-0.122**	-0.023*	-0.314**
	(0.037)	(0.035)	(0.010)	(0.082)
R^2	0.096	0.182	0.173	0.182
Treatment: Convicted				
Control: Never Arrested				
Juvenile Convict x Automatic Expunge	0.056	0.074+	0.044	0.216*
	(0.046)	(0.039)	(0.041)	(0.103)
Juvenile Convict	-0.268**	-0.322**	-0.159**	-0.497**
	(0.037)	(0.018)	(0.011)	(0.079)
R^2	0.100	0.170	0.192	0.130
Treatment: Arrested, Not Convicted				
Control: Never Arrested				
Juvenile Arrest x Automatic Expunge	-0.048	-0.003	0.028	0.029
	(0.030)	(0.047)	(0.026)	(0.102)
Juvenile Arrest	-0.136**	-0.221**	-0.141**	-0.255**
	(0.015)	(0.028)	(0.011)	(0.050)
R^2	0.091	0.174	0.197	0.133

Source: Author's calculations.
Note: Each panel specifies the assumed treatment and control group for this difference-in-differences analysis. Standard errors are clustered at the state level. Average income is calculated over 2008 to 2010. Additional covariates are the same as column (2). Nineteen observations are lost in this analysis because expenditures are unavailable for the District of Columbia. An example of the full regression output appears in Table G.12. + P<0.10, * P<0.05, ** P<0.01.

Table G.10: Long-Term Effects of Automatic Expungement: Proxy Variable Analysis (Non-clustered)

	(1) Not Arrested After Age 20?	(2) Attended College	(3) Graduated College	(4) log(Average Income)
Juvenile Convict Sample (N=403)				
Automatic Expunge	0.143^{+}	0.077	0.051	0.253
	(0.076)	(0.070)	(0.043)	(0.176)
R^2	0.094	0.153	0.238	0.184
Juvenile Arrest Sample (N=859)				
Automatic Expunge	-0.000	0.017	-0.003	0.030
	(0.054)	(0.050)	(0.031)	(0.111)
R^2	0.070	0.203	0.178	0.177
Never Arrested Sample (N=6188)				
Automatic Expunge	0.010	-0.021	-0.028	-0.001
	(0.016)	(0.021)	(0.019)	(0.041)
R^2	0.056	0.142	0.182	0.107

Source: Author's calculations.

Note: Each panel restricts the sample to one of three categories: those who are never arrested as a juvenile, those who are arrested but not convicted, and those who are convicted. All regressions are weighted using 1997 sampling weights (cumulative cases method). Average income is calculated over 2008 to 2010. Additional covariates are the same as column (2) in Table A.4. Nineteen observations are lost in this analysis because expenditures are unavailable for the District of Columbia. An example of the full regression output appears in Table G.12. + P<0.10, * P<0.05, ** P<0.01.

Table G.11: Long-Term Effects of Automatic Expungement: Difference-in-Differences Analysis (Non-clustered)

	(1) Not Arrested After Age 20?	(2) Attended College	(3) Graduated College	(4) log(Average Income)
Treatment: Convicted				
Control: Arrested, Not Convicted				
Juvenile Convict x Automatic Expunge	0.153*	0.053	0.045	0.276+
	(0.073)	(0.067)	(0.047)	(0.152)
Juvenile Convict	-0.133**	-0.108**	-0.027	-0.320**
	(0.038)	(0.034)	(0.020)	(0.089)
R^2	0.087	0.198	0.205	0.178
Treatment: Convicted				
Control: Never Arrested				
Juvenile Convict x Automatic Expunge	0.120*	0.086	0.055	0.225+
	(0.060)	(0.057)	(0.044)	(0.126)
Juvenile Convict	-0.279**	-0.316**	-0.177**	-0.473**
	(0.032)	(0.028)	(0.018)	(0.075)
R^2	0.094	0.176	0.198	0.124
Treatment: Arrested, Not Convicted				
Control: Never Arrested				
Juvenile Arrest x Automatic Expunge	-0.031	0.026	0.012	0.003
	(0.043)	(0.043)	(0.030)	(0.090)
Juvenile Arrest	-0.145**	-0.226**	-0.155**	-0.236**
	(0.021)	(0.021)	(0.016)	(0.050)
R^2	0.086	0.182	0.205	0.126

Source: Author's calculations.

Note: Each panel specifies the assumed treatment and control group for this difference-in-differences analysis. All regressions are weighted using 1997 sampling weights (cumulative cases method). Average income is calculated over 2008 to 2010. Additional covariates are the same as column (2). Nineteen observations are lost in this analysis because expenditures are unavailable for the District of Columbia. An example of the full regression output appears in Table G.12. + P<0.10, * P<0.05, ** P<0.01.

Table G.12: Effect on College Attendance for Juvenile Convicts (Full Output)

	(1) Attended College
Father's Education	0.031*
	(0.012)
Mother's Education	0.010
	(0.007)
Parental Income (1997)	0.017
	(0.010)
Age (1997)	-0.000
	(0.017)
Urban	-0.082
	(0.056)
Black	-0.025
	(0.069)
Hispanic	-0.025
	(0.107)
Female	0.093^{+}
	(0.048)
Biological Mom	0.003
	(0.061)
Other Household Composition	-0.137^{+}
	(0.071)
Household Size (1997)	-0.005
	(0.036)
Household Under 18 (1997)	-0.034
	(0.036)
Automatic Expunge	0.077
	(0.060)
Unemployment Rate	3.392
	(3.381)
log(Officers)	0.010
	(0.081)
log(Expenditures)	-0.070
	(0.107)
log(Median Income)	0.333
	(0.241)
log(Head Start)	0.086
	(0.075)
Household Under 6 (1997)	0.063^{+}
	(0.036)
Parental Income Missing	-0.028
	(0.068)
Mother's Education Missing	0.094

Table G.12 (cont'd)

	(0.135)
Father's Education Missing	0.349*
	(0.132)
Custody Measure	11.342
	(9.622)
Sentencing Measure	-0.533^{+}
	(0.300)
Imprisonment Rate	0.012
	(0.016)
N	403
R^2	0.153

Source: Author's calculations.

Note: The dependent variable is an indicator for ever attending college. The regression is weighted using 1997 sampling weights (cumulative cases method). The reference group for household composition is living with both biological parents. Household under 18 reflects the number of household members under 18 at the time of interview in 1997. Standard errors are clustered at the state level. + P<0.10, * P<0.05, ** P<0.01.

APPENDIX H

Appendices for "Did Graduated Driver Licensing Laws Drive a Reduction in Crime?"

H.1: GDL Implementation

In this appendix I list the effective dates of GDL implementation I use in my analysis.

Table H.1: Effective Dates of GDL Implementation

State	Effective date of three tiered law
Alabama	October 1, 2002
Alaska	January 1, 2005
Arizona	June 30, 2008
Arkansas	July 1, 2002
California	July 1, 1998
Colorado	July 1, 1999
Connecticut	October 1, 2005
Delaware	July 1, 1999
District of Columbia	January 1, 2001
Florida	July 1, 1996
Georgia	July 1, 1997
Hawaii	January 9, 2006
Idaho	January 1, 2001
Illinois	January 1, 1998
Indiana	January 1, 1999
Iowa	January 1, 1999
Kansas	January 1, 2010
Kentucky	April 1, 2007
Louisiana	January 1, 1998
Maine	August 11, 2000
Maryland	July 1, 1999
Massachusetts	November 4, 1998
Michigan	April 1, 1997
Minnesota	August 1, 2008
Mississippi	July 1, 2000
Missouri	January 1, 2001
Montana	July 1, 2006
Nebraska	January 1, 1999
Nevada	July 1, 2001
New Hampshire	January 1, 1998
New Jersey	January 1, 2001
New Mexico	January 1, 2000
New York	September 1, 2003
North Carolina	December 1, 1997
North Dakota	January 1, 2012
Ohio	January 1, 1999
Oklahoma	November 1, 2005
Oregon	March 1, 2000
Pennsylvania	December 22, 1999
Rhode Island	January 1, 1999
South Carolina	July 1, 1998
South Dakota	January 1, 1999
Tennessee	July 1, 2001
Texas	January 1, 2002
Utah	July 1, 1999
Vermont	July 1, 2000
Virginia	July 1, 2001
Washington	July 1, 2001
West Virginia	January 1, 2001
Wisconsin	July 1, 2000
Wyoming	September 16, 2005

Source: Insurance Institute for Highway Safety (2013), Dee et al. (2005), Lexis-Nexis searches.

H.2: Traffic Fatalities

In this appendix I present analyses focusing on GDL restrictions and traffic fatalities.

Table H.2: Descriptive Statistics, Traffic Fatalities

Variables	Mean	Std. Dev.
Outcome Variables		
Pre-GDL: All Teenage Death	50.86	44.304
Pre-GDL: All Death with a Teenage Driver	62.09	54.560
Pre-GDL: All Teenage Driver Death	22.72	19.196
Covariates		
Speed Limit - 65	0.49	0.493
Speed Limit - 70+	0.47	0.493
Seat Belt (Primary Enforcement)	0.32	0.465
Seat Belt (Secondary Enforcement)	0.64	0.475
Blood Alcohol Concentration 0.08	0.57	0.485
Blood Alcohol Concentration 0.10	0.40	0.479
Admin License Revocation	0.86	0.349
Percent Black	0.12	0.117
Percent Less Than 15 Years Old	0.21	0.019
Percent 15-19 Years Old	0.07	0.006
Percent 20-24 Years Old	0.07	0.007
Percent 25-44 Years Old	0.29	0.026
Percent 45-64 Years Old	0.18	0.020
Percent 65 or Older	0.18	0.023
Percent Urban	0.72	0.150
Unemployment Rate	0.05	0.018
Med Household Income (thousands)	52.05	8.146
Zero Tolerance	0.86	0.339

Source: Author's calculations.
Note: The unit of observation is state*year between 1992 and 2010. Median household income is in 2011 dollars.

Table H.3: Extension of Previous Results on GDL and Fatalities

	(1)	(2)	(3)	(4)	(5)
Specification	Dee et al. (2005)	Replication	Additional Years of Data	All Death Teen Driver	Teenage Driver Death
Years	1992-2002	1992-2002	1992-2010	1992-2010	1992-2010
Dependent Variable	All Teen Fatalities	All Teen Fatalities	All Teen Fatalities	All Deaths with Teen Driver	Teen Driver Fatalities
GDL	-0.056*	-0.060*	-0.064**	-0.064**	-0.051+
	(0.026)	(0.026)	(0.023)	(0.024)	(0.030)
Observations	528	528	912	912	912

Source: Author's calculations.

Note: All regressions use conditional maximum likelihood of the Negative Binomial distribution and include binary indicators for the following driving policies: primary enforcement of seat belt laws, secondary enforcement of seat belt laws, speed limit 65 miles per hour, speed limit 70 or more miles per hour, legal blood alcohol concentration of 0.08, legal blood alcohol concentration of 0.10, administrative license revocation law, and zero tolerance law. Regressions also include state and year fixed effects. The unit of observation is state*year. Standard errors (in parentheses) are adjusted for clustering at the state level. Column (1) presents the preferred specification in Dee et al. (2005). + P<0.10, * P<0.05, ** P<0.01.

APPENDIX I

Appendices for "Interstate Differences in Pension Vesting Rules, K-12 Teacher Experience, and Teacher Exit"

I.1: NLSY Data

In this appendix we discuss the panel of teachers we use from the NLSY. To identify teachers we look at industry and occupation codes across all reported job lines in all years: we label someone a teacher if they are in the "Educational Services" industry and in the "Education, Training, and Library" occupations on any job line within any year. Next, we build a balanced panel for those who report being a teacher between 2002 and 2010 (age falls between 18 and 31) using the responses from these teachers. Using their recall in the interviews, where they provide their employment status by week, we fill in the panel with the time they report working as a teacher. We calculate the number of weeks worked as a teacher in the calendar year as well as the number of weeks worked in the fall. We also separately calculate the total number of weeks worked in each school year (school year is defined between week 32 of one year and week 32 of the next year—sometime around August 1). In the event of multiple employers within the same year, we choose the employer for the year with the most weeks worked. In the event that someone spent the same amount of time with two employers, we assume their employer for the year is the employer where they began working first.

We use state of residence to determine pension parameters for the teacher, implying that state of residence is the same as state of employment. It is not possible to determine state of employment or any specific information about the employer other than a numerical identifier in the NLSY Geocode Data. In addition, if state of residence is missing in any given year but available in years before and after the missing observation, we assume the state of residence has not changed if state of residence is constant for the adjacent non-missing observations. If there is any discrepancy between the non-missing observations we keep state of residence missing.

The hazard of first exit from teaching goes from missing to zero once a teacher begins working--we consider a teacher to have worked if they report working at least 12 weeks in the school year. Once they exit teaching for the first time the variable becomes one for the remainder of the panel. If they change employers but do not exit teaching the binary variable remains equal to zero. This happens in 262 individual by year observations, roughly 3.7 percent of our observations.

We merge a number of covariates with these data. The covariates include marital status, number of biological or unrelated children in the household, gender, age, race, ethnicity, ability score, census region, and family income. We generate indicators for female, black, Hispanic, and mixed race. We also generate indicators for married, single, and separated/divorced. To create these indicators we aggregated the number of months in each year that an individual reported being married, single, and separated/divorced. We consider them married for that year if they spent six or more months married. We consider them single or separated/divorced for the year if they spent more than six months single or separated/divorced.

Next, we add the specifics of the pension system using data from 2001 to 2011 in the Public Fund Survey. We supplement/update these data using NCTQ documents, National Education Association (2010), and our own research into plan handbooks. We merge the fiscal year of the plan onto the same year in the NLSY data. For example, we merge the 2005 fiscal year data with a respondent's survey results for 2005. The variables we include are: plan name, state information (name, numeric identifier), fiscal year, years to vest, indicator for DC plan available, indicator for choice between DB and DC plan, retirement factor, indicator for social security coverage, and employee contribution rate. We assume the vesting rule remains the same as the year when the teacher entered so policy changes do not affect participants retroactively.

We also create a separate variable that reflects the rule for teacher tenure. Note that we do not have any time variation in this variable.

Lastly, we create indicators for pension portability based on NCTQ tables. These variables indicate what a teacher receives if he or she withdraws funds (contributions, contributions plus interest, or contributions, interest, and some or all of the employer's contributions) as well as the ability to purchase service credits in the state.

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