

Stigma in the Labor Market: Evidence from Juveniles Transferred to Adult Court and Occupations with Mandated Criminal Background Checks

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Abstract

At the end of 2011, almost seven million US residents were under the supervision of the correction system. An unknown but significantly larger number of people has at some point been under the supervision of the correction system. As ex-offenders are released, they face the challenge of reentering the labor market. This paper examines a broad set of policies that influence whether an individual's criminal history record is observed by a potential employer. Using the first fourteen waves of the NLSY97, there is evidence that labor market outcomes are worse for ex-offenders when their criminal histories are easily accessible to employers. Non-offenders from highly offending groups have significantly better labor market outcomes in the presence of open records. The net effect of open information supports the notion that employers statistically discriminate in the absence of criminal history data.

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1 Introduction

At the end of 2011, almost seven million US residents were under the supervision of the correction system (Glaze and Parks, 2012). An unknown but significantly larger number of people has at some point been under the supervision of the correction system. As ex-offenders are released, they face the challenge of reentering the labor market. Legitimate employment is a critical predictor of criminal desistance (Sampson and Laub, 1993; Uggen, 2000; Lageson and Uggen, 2013), so re-entry is an important consideration for measuring the social costs of crime. Ex-offenders face stigma in their search for work, and information about their criminal histories can now be expected to be long lasting in the age of the Internet. This paper examines a broad set of policies that influence whether an individual's criminal history record is observed by a potential employer.

We can categorize these policies into those that affect the legal access to criminal history data and those that affect the diligence of employer searches. The primary access point to criminal history data is through online searches. Since the late 1990s, 26 states have started sharing their criminal history databases through public websites (Finlay, 2009; DeBacco and Greenspan, 2011; Lee, 2012). This has rapidly reduced the cost of obtaining criminal history data. While these databases are primarily composed of adult records, databases in six states also include juvenile offenses. Moreover, an increasing number of juvenile offenders are transferred to adult court, and the records for these juveniles will also enter the adult databases.

Employers will likely be more diligent in their criminal background checks when they are mandated to do so by law, when criminality is negatively correlated with productivity in the particular job, and when potential liability for negligent hiring is great.

Given their low employment after release, it follows that ex-offenders have difficulty finding work. Increased access to criminal history records is expected to worsen their labor market opportunities. But economic theory predicts effects for non-offenders as well. Employers have imperfect information about the criminal records of applicants, so rational employers may use observable correlates of criminality as proxies for criminality and statistically discriminate against groups with high rates of criminal activity or incarceration. In the absence of open records, non-offenders from groups with high incarceration rates would be adversely affected. When accurate criminal history records become easier to obtain, the labor market outcomes of these non-offenders should

improve as employers can determine with greater certainty whether applicants have criminal records.

This paper tests these hypotheses using detailed criminal and labor market histories from the 1997 cohort of the National Longitudinal Survey of Youth (NLSY97). The criminal history variables in this survey allow me to distinguish ex-offenders from non-offenders. I also use the criminal histories to model employer perceptions of criminality assuming that they are based on rational expectations of the probability of prior conviction. Using the first fourteen waves of the NLSY97, there is evidence that labor market outcomes are worse for ex-offenders when their criminal histories are easily accessible to employers. Non-offenders from highly offending groups have significantly better labor market outcomes in the presence of open records. The net effect of open information supports the notion that employers statistically discriminate in the absence of criminal history data.

The impacts are largest for employment and smallest for wages. Economists entertain the idea that employers should be able to discount in wages any risk created by asymmetric information. My results suggest that employment is a real barrier, since open information has little effect on wages for ex-offenders or non-offenders who look like ex-offenders. One possible explanation is that the risk created by negligent hiring lawsuits is too large to be offset by wage reduction, especially near a binding minimum wage.

This study makes two important contributions to the empirical literature on the labor market effects of employer use of pre-employment screening technologies: it exploits an exogenous change in the employer's information set to identify the effect of that information and it uses observed criminal history data to distinguish effects for less desirable applicants (offenders) from more desirable applicants (non-offenders). The research design makes use of technological changes in the amount of criminal history data available to employers. This strategy contrasts with research that uses variation in employer decisions to conduct criminal background checks, since these decisions are likely endogenous to the composition of applicant pools (Holzer et al., 2007). The research design used in this paper is similar to one used by Autor and Scarborough (2008) to study the diffusion of pre-employment personality tests at a national retail chain. They find that the relative hiring of blacks did not fall after the introduction of the tests, despite the fact that blacks in general perform worse on the tests, and they suggest that managers were effectively

statistically discriminating before the tests. In a very different research design, Pager (2003) conducted an audit study of the effect of criminal records. The callback rate for ex-offenders was less than half of the callback rate for non-offenders.

In addition to providing an empirical test of statistical discrimination, the results of this paper are important for understanding the transition of ex-offenders back into the legitimate labor force. As the flow of released prisoners increases over the next ten years, the issue of re-entry into the legitimate labor market will force policy makers to consider the unintended consequences of open criminal history records. Legitimate employment is a strong predictor of criminal desistance (Sampson and Laub, 1993; Needels, 1996; Uggen, 2000; Lageson and Uggen, 2013), so expanded use of criminal background checks has the potential to increase recidivism and the long-term fiscal costs of criminal punishment. But there may also be some beneficiaries from open records. All else equal, individuals who do not have criminal records but come from highly offending groups stand to benefit from a more transparent criminal records system.

2 Stigma and statistical discrimination

Economic theory predicts nuanced effects for non-offenders and ex-offenders that may result from statistical discrimination by employers. If employers are averse to hiring ex-offenders, then they have an incentive to use observable correlates of criminality or incarceration as proxies for those qualities. Using these proxies, employers can classify individuals as coming from groups with low rates of incarceration (or low perceived criminality) or high rates of incarceration (high perceived criminality). In the absence of open records, one would observe an averaging of the labor market outcomes for individuals within either group. For example, black men who are high school dropouts have very high incarceration rates. If employers statistically discriminate, then the outcomes for black non-offenders that have not completed high school will be relatively worse than they would have been without statistical discrimination, but ex-offenders from that group will have relatively better outcomes. Similarly, white ex-offenders should benefit from statistical discrimination because they come from a group with relatively low rates of incarceration.

Now suppose that criminal history records become publicly available. If employers can directly observe criminal history records, they no longer need to rely on statistical discrimination.

This will cause a separation in the labor market outcomes of ex-offenders and non-offenders within highly offending groups. Specifically, ex-offenders should do worse and non-offenders should have improved labor market outcomes. Figure 1 illustrates the main features of the model. Each panel shows a plot of labor market outcomes (hiring odds or wages) against an index of perceived criminality. Perceived criminality is an index created by the employer using observable proxy variables as a substitute for observed criminality. Panel A shows the hiring policy when criminal records are not available to employers. The dotted line shows that labor market outcomes are decreasing in perceived criminality. Panel B shows the hiring policy when criminal records are available to employers. In this case, there would be a bifurcation from the hiring policy under closed records. Now that employers can distinguish offenders from non-offenders, the labor market outcomes of non-offenders with high perceived criminality improve. Also note that offenders with low perceived criminality suffer a greater decline in labor market outcomes relative to other offenders.

A simple empirical model can capture the main characteristics of the model shown in Figure 1. For a relevant labor market outcome Y , one could estimate the following regression:

$$Y = \beta_0 + \beta_1 PC + \beta_2 Access \cdot Inc + \beta_3 PC \cdot Access + \beta_4 PC \cdot Access \cdot Inc + \beta_5 PC \cdot Inc + \beta_6 Access + \beta_7 Inc + \varepsilon, \quad (1)$$

where *Access* indicates if employers have access to criminal history records, *Inc* is a dummy for an individual's own criminal record, *PC* is the employer's perception of the individual's criminality, and ε is an error term. Note that this model could apply generally to any situation in which some employers have technical access to the criminal history data of their applicants and some do not (and this accessibility is not a function of an employer's decision). For the moment, I will abstract away from a more complete model that includes the individual controls and fixed effects required to identify the effects of open records in my institutional context.

This model can be used to test the two primary hypotheses generated by the model of statistical discrimination. First, the model predicts that the main effect of true criminality should become more negative when employers can access criminal history records. This effect is captured by the coefficient on the interaction of *Inc* and *Access*, so it can be tested with the alternative hypothesis

$\beta_2 < 0$ and null hypothesis $\beta_2 \geq 0$. Second, the model predicts that non-offenders with high perceived criminality should have improved labor market outcomes if potential employers can verify that they are non-offenders (i.e., when criminal history records are publicly available). This hypothesis mirrors one in which ex-offenders with high perceived criminality have relatively worse outcomes from similar non-offenders once records are open. This can be tested with the alternative hypothesis $\beta_3 < 0$ and null hypothesis $\beta_3 \geq 0$. Note that both of these hypotheses test relative and not absolute effects of employer access to criminal history data.

3 Criminal majority, occupation-specific background checks, and public availability of records

The US Equal Employment Opportunity Commission (1987a; 1987b; 2012) has declared that employers may violate Title VII of the Civil Rights Act of 1964 if they broadly deny employment to applicants with criminal records, but that employers can ban applicants who have committed particular offenses if employers demonstrate these offenses are directly related to job functions. Some states have more severe restrictions, but there is little evidence of active enforcement. In this section, we highlight the policies that affect criminal history record access and effort than employers expend to find these records.

Criminal majority and juvenile transfer provisions

The proportion of juveniles taken into police custody who were transferred to criminal or adult court doubled from 1990–2010. Figure 2 shows the disposition of juveniles taken into police custody annually (from the FBI’s Uniform Crime Reports). Although the total proportion of juveniles referred to adult court is low, it is the fastest growing disposition category. There is some growth in referrals to juvenile court, and these two trends are offset by a corresponding decrease in juveniles whose status is handled within the police department and who are subsequently released. This figure likely understates the role of adult courts in processing juvenile felonies, since cases handled within departments tend to be minor offenses and cases sent originally to juvenile court may eventually find their way to adult court.

Table 1 shows how these rules have become more severe over the last twenty years. Appendix A goes into more detail on how these data were collected.

Criminal majority statutes, and to a lesser extent juvenile transfer provisions, have been used to study the effect of punishment incentives on the criminal propensities of juveniles. Levitt (1998) finds that ascending to the age of majority is associated with a significant drop in crime rates, once changes in the relative punitiveness of adult and youth courts are taken into account. Lee and McCrary (2009) focus on Florida from 1995 to 2002 and find very little effect around the age of majority. Similar research designs on a variety of offender datasets have been used to find little deterrent effect of criminal majority statutes (McCrary and Sanga, 2011) or juvenile transfer statutes (Finlay, 2012). One explanation for these results is that young people do not fully update their expectation of the change in severity of punishment at the age of majority (Hjalmarsson, 2009). For the purpose of this paper, these findings are helpful, since they support the assumption that application of juvenile transfer to adult court is not endogenous to whether a juvenile committed a crime.

Public availability of criminal history records

Figure 3 shows the states in which the public have online access to criminal history data and the first year this was available (Finlay, 2009; DeBacco and Greenspan, 2011; Lee, 2012). Appendix B discusses why the timing of availability can be considered exogenous. Juvenile records are only automatically added to adult criminal records databases in Idaho, Maine, Michigan, New Jersey, New Mexico, Pennsylvania, and Washington (Lee, 2012).

Occupation-specific background checks

(Not complete.)

4 NLSY97

This paper uses the criminal and labor market histories from the 1997 cohort of the National Longitudinal Survey of Youth (NLSY97). The NLSY97 includes a nationally representative sample of all youths aged 12–16 years by the end of 1996, and an over-sample of black and Hispanic youths

meeting the same age restriction. Currently, the NLSY97 has released 14 rounds of data, covering interviews from 1997 through 2010. The NLSY97 is an excellent sample for this project because it has information about both the criminal activity of respondents and their labor market outcomes. This is a rare quality for a nationally representative survey, and the NLSY97 is especially useful because the sample period coincides with the introduction of Internet sites for accessing criminal history records. The criminal records policies discussed above are matched with individual respondents using the state geocodes available in the private-release version of the NLSY97. There are a few drawbacks of using the NLSY97 for this research. This is a very young sample when the first states make their records available online. While young people are the most likely to be incarcerated, many of the survey respondents are still completing their schooling at or near the end of the sample period. This limits the extent to which the labor market outcomes of NLSY97 respondents can reasonably be affected by changes in criminal records openness.

The sample I use in regression analysis consists of men and women aged at least 18 years, covering survey years 1997–2010. I use both the representative sample and the minority oversample with sampling weights.¹

I use three labor market outcomes as dependent variables: employment status, the natural log of the hourly wage, and the inverse hyperbolic sine of annual earnings.² Employment status is equal to one if the respondent was employed at the date of the interview. Hourly wage is the NLSY-created hourly wage for the primary job since the last interview. The earnings variable is the total income from wages and salary in the calendar year before the interview.³

The NLSY97 also has extensive information on interactions with the criminal justice system. An iterative round of questions addressed any arrests and whether they led to conviction or incarceration. I created indicator variables for whether the respondent had been convicted or incarcerated at the time of the interview or since the date of the last interview. Individual crimes are matched to criminal majority and juvenile transfer provisions based on the age-crime profile and the state and year in which the crime occurred. Crimes committed above the age of criminal

¹Custom sampling weights for NLSY97 respondents in any survey year come from http://www.nlsinfo.org/web-investigator/custom_weights.php.

²The inverse hyperbolic sine, $\sinh^{-1}(x) = \ln(x + \sqrt{x^2 + 1})$, is an alternative to the natural logarithm when one wants to include zeroes in the analysis (Burbidge et al., 1988; MacKinnon and Magee, 1990). Away from zero, the derivatives of the two functions are approximately equal.

³Wages and earnings are inflated to 2005 dollars using the All-Urban series of the Consumer Price Index.

majority are considered to be visible to employers when records are open. Crimes eligible for mandatory transfer to adult court are also coded as visible to employers under open records.

I also include a number of other variables as controls. To control for labor market experience, I use the hours of accumulated labor market experience from age 13. Education controls include highest grade completed and a set of dummies for highest degree received as of June 30 of the survey year (namely, whether the individual has a general equivalency diploma (GED), a high school diploma, an associates degree, or a bachelors or postgraduate degree). I also use calculate the body mass index and include in the vector of observables considered by employers. In regressions without individual fixed effects, the Armed Services Vocational Aptitude Battery (ASVAB) test score and race, ethnicity, and gender indicators serve as controls.

The sample is restricted to respondents aged 18–31 years, although juvenile labor market and criminal justice experience is expressed in the cumulative variables. Table 2 shows means and standard deviations of variables for the analytic sample by age.

5 Empirical model

Define the vector of policies that affect employer access to criminal history data as \mathbf{A} . Elements of \mathbf{A} are defined in such a way that higher values are associated with lower costs for accessing criminal history data. For the moment, suppose \mathbf{A} is scalar and binary, so it is equal to one if criminal history records are available to employers online, and zero otherwise.

Now suppose that employers use observable characteristics to make forecasts of applicant criminality. They base this prediction on a vector of individual characteristics \mathbf{Z} , whose elements are easily observable and are known to be correlated with criminality or prior incarceration. Suppose employers create a regression-weighted index of variables in \mathbf{Z} , and use this as a proxy for criminality in their hiring decisions. Since prior incarceration is a low-probability event, I use a probit model of the following form:

$$P[W_{it} = 1] = P[\alpha_0 + \mathbf{Z}_{it}\alpha_1 + \eta_{it} > 0] = \Phi[\alpha_0 + \mathbf{Z}_{it}\alpha_1 + \eta_{it}], \quad (2)$$

where W_{it} is a measure of criminal history for individual i in year t , η_{it} is an error component, and

$\Phi[\cdot]$ is the cumulative distribution function of the standard normal distribution. After estimating this regression, employers can predict a measure of perceived criminality: $C_{it} \equiv P[\widehat{W}_{it} = 1] = \Phi[\hat{\alpha}_0 + \mathbf{Z}_{it}\hat{\alpha}_1]$.

Now consider labor market outcomes as a function of employer access to criminal history data, employer background check intensity, and perceived criminality:

$$Y_{it} = \mathbf{X}_{it}\beta_0 + \beta_1\mathbf{1}[W_{it} = 0]\mathbf{1}[A_{it} = 1] + \beta_2\mathbf{1}[W_{it} = 1]\mathbf{1}[A_{it} = 0] \quad (3)$$

$$+ \beta_3\mathbf{1}[W_{it} = 1]\mathbf{1}[A_{it} = 1] + \beta_4\mathbf{1}[W_{it} = 0]\mathbf{1}[A_{it} = 1]C_{it} \quad (4)$$

$$+ \beta_5\mathbf{1}[W_{it} = 1]\mathbf{1}[A_{it} = 0]C_{it} + \beta_6\mathbf{1}[W_{it} = 1]\mathbf{1}[A_{it} = 1]C_{it} \quad (5)$$

$$+ \beta_7C_{it} + \varepsilon_{it}, \quad (6)$$

where Y is a labor market outcome, \mathbf{X} is a vector of regressors, $\mathbf{1}[\cdot]$ is the indicator function, and ε_{it} is an error term.

Three variables measure the intersection of actual criminal history and record openness:

		Actual criminal history	
		non-offender	ex-offender
Observability	closed	Has no record, closed records (omitted)	Has record, closed records (β_2)
	open	Has no record, open records (β_1)	Has record, open records (β_3)

These three dummies are then interacted with the perceived criminality estimate (β_4 , β_5 , and β_6). Those six parameters are sufficient to examine the effects of eliminating asymmetric information. In particular, the effect of information on the outcomes for ex-offenders is equal to $\beta_1 + C\beta_4$. The effect of information on the outcomes of non-offenders is $\beta_3 + C\beta_6 - \beta_2 - C\beta_5$. Then, the net effect of information on the relative outcomes of non-offenders is $\beta_1 + C\beta_4 - \beta_3 - C\beta_6 + \beta_2 + C\beta_5$.

6 Results

I first show estimates used to construct measures of perceived criminality. Figure 4 shows kernel density estimates of predicted probability of a respondent having ever been convicted of a serious

crime. Probit regressions were estimated separately by gender, racial, and ethnic groups.⁴ The top panel shows the distributions for black, Hispanic, and all other men. The bottom panel shows the analogous distributions for women. Men in general are more likely to be convicted of a crime. There is some racial and ethnic differences in the distributions—black men and non-black, non-Hispanic women are more likely to have convictions. The racial differential in conviction rates is quite small compared to the racial differential in prior incarceration rates (both in the population and in this sample). The predicted probabilities of prior conviction in Figure 4 will serve as a measure of employers' perception of criminality given an assumption of rational expectations.

With an estimate of perceived criminality, I estimate regressions of labor market outcomes as a function of the typical regressors, actual criminal history, perceived criminal history, and records openness. Table 3 focuses on the parameters necessary to test hypotheses on asymmetric information and statistical discrimination.⁵ The 90th percentile of the perceived criminality distribution is 0.35, and I use these in the linear combination of estimated parameters.

In Table 3, odd columns are estimated as pooled cross-sectional samples but include time-invariant regressors. The even columns exploit the panel structure with individual fixed effects. For individuals with a criminal record, all but one estimate of the information effect is negative, although only the earnings estimate in the pooled model is significantly different from zero. These estimates are consistent with somewhat worse outcomes for ex-offenders once employers have easier access to criminal history data. For non-offenders, all but one estimate is positive and only the pooled employment model has a statistically significant result. The net effect is also of interest because it measures the bifurcation in the outcomes for non-offenders relative to ex-offenders. These estimated effects are consistently positive and the employment and earnings estimates are both statistically significant in the pooled models. The fixed effects estimates are similar magnitudes for employment and earnings, while the wage estimates seem indistinguishable from zero in both specifications. In economic context, open records improve the relative likelihood of employment of non-offenders by 2.8 percentage points and the relative annual earnings of non-offenders by 10.0 percent. Given the poor labor market outcomes of individuals in these high-risk groups, these are substantial improvements.

⁴The full results are in Table A-1.

⁵Full results can be found in Table A-2.

Figure 5 explores the results as a function of the full range of perceived criminality. Recall that most of the sample has a predicted probability of conviction below 0.4 for men and 0.2 for women. In these ranges, the net information effects for employment and earnings are clearly positive and statistically significantly different from zero. One might expect these lines to slope upwards—meaning non-offenders from highly offending groups benefit the most from more open information. They don't follow this pattern in point estimates, but in changes relative to the base outcomes there could be relative greater changes for those with higher perceived criminality. For wages, the point estimates stay quite close to zero across the range of perceived criminality.

Robustness checks

7 Conclusion

In a first-best world, the social costs of crime can be minimized with a small number of prisoners. But in our current context, millions of individuals have criminal records and rational employers use statistical discrimination to avoid hiring them. And open records can help non-offenders who otherwise look like offenders.

I find evidence that more open criminal history data is associated with improvements in employment and earnings of non-offenders relative to ex-offenders. This supports the hypothesis that employers statistically discriminate to avoid hiring ex-offenders. The evidence also refutes the hypothesis that employers are willing to lower wages to offset the risk of hiring ex-offenders. The initial hiring decision appears to be the primary difficulty for ex-offenders or non-offenders from highly offending groups.

Appendix A Data appendix: juvenile transfer statutes

Table A-3 shows the statutory references for each criminal majority and juveniles transfer laws (Finlay, 2012).

Our primary source for juvenile transfer data was the state statutes themselves. We accessed these statutes primarily using Westlaw, moving to microfiche and other physical records when Westlaw had gaps in its historical records. Also invaluable were the previous surveys of transfer law done by the National Center for Juvenile Justice and Office of Juvenile Justice and Delinquency Prevention. Although these surveys were not as comprehensive as what we sought to put together, they were useful as a series of crosschecks on our own data. As we built up our table with information directly from state statutes, we looked to see if it concurred with these organizations' summaries. This gave us a guard against missing important statutes. However, in the rare occasions where our interpretation of the statutes went against that of the NCJJ or OCJDP, we trusted our own judgment.

Having found our data source, we then needed to decide how to organize it. Clearly in data collection of this sort, there are tradeoffs regarding comprehensiveness and resource consumption as well as in simplicity versus complexity. In each of these questions we have done our best to strike a balance.

One choice was over which crimes to include in our sample. To have included every conceivable crime would have made our task unmanageably ambitious. We settled on our selection as a good and almost comprehensive spectrum of crimes that exist in every state and as a close match for the crimes noted by the National Longitudinal Survey of Youth 1997 (NLSY97) and Uniform Crime Reports (UCR). To be clear, MINP refers to misdemeanor property offenses, such as petty vandalism, and MAJP refers to felony property offenses, such as arson. Possession and Trafficking are recorded in our chart as each individual state defines them. So, for example, if a state has a transfer law with drug trafficking as a criteria, and that state additionally has laws stating that those possessing more than a certain amount of drugs as prima facie trafficking, the transfer criteria would be recorded on our chart in the trafficking category. We also sometimes stretched a category to take into account a state's peculiar semantics. If a state referred to Murder and Theft as Homicide and Larceny (great band name by the way), we had no qualms fitting these

into the aforementioned categories.

Another important early decision was regarding what types of transfer to record. Many previous studies meticulously discriminated between direct file, statutory exclusion, blended sentencing, and other legal mechanisms for trying or punishing youth as adults. Discriminating between these different factors in high resolution may be important for a lawyer, but for us who are merely focused on where the legislature has decided to make transfer optional and where it has decided to not make it optional this is too much detail. Rather, we decided to combine direct file and discretionary waiver as 'discretionary transfer', and mandatory waiver and statutory exclusion as 'mandatory transfer'.

For presumptive waivers we decided were in between and we mixed between the two depending on their flavor. We also made the decision early on to ignore blended sentencing statutes. Although these laws are no doubt important, our concern is primarily with which court a juvenile will be tried in, not how he or she is sentenced.

Two important rules that we did record, but ultimately did not factor into our analysis were Reverse Transfer and Once an Adult, Always an Adult policies. The first allows some transferred or statutorily exclusion individuals to transfer back to juvenile court. The latter makes it a rule that previously transferred individuals must be triad as adult for all or almost all subsequent offenses.

We quickly realized that in addition to age/offense criteria, there were several other factors that commonly influenced minimum age of transfer; our conditionals. In selecting conditionals, we attempted to choose those that came up most commonly. Unfortunately these were too varied to be documented completely. To account for this fact we included the 'if other' conditional, which gives the minimum age for transfer in the case that these, sometimes incredibly specific, sometimes more common conditions were satisfied. For example, an 'if other' conditional for assault might be assaulting a police officer, or committing aggravated assault. For assault and theft, which in many states are not automatically felonies, the modal IOT condition was if felony.

Decisions on other conditionals also bear explaining. ICO at many times refers to repeat offenders of that offense, but it sometimes applies to certain types of repeat offenders in general. However, it never overlaps with OAAA policies. Had we done otherwise, every state with OAAA would have ICO checked off for nearly every crime. Also important to note is that IWF does not apply to pure gun crimes. Straight up gun crimes, like trafficking and illegal possession are in

property offenses and almost exclusively MAJP.

Finally, although this was very rare, if it was ever ambiguous whether a certain circumstance granted eligibility for transfer, we erred on the side of interpreting the law as being more punitive. We reasoned that ‘dt’ is already about the largest possible set the state could legally transfer, so it makes sense to err on the side of inclusiveness.

Appendix B Data appendix: public access to criminal history records

A criminal history record positively identifies an individual and describes that person’s arrests and subsequent dispositions relating to a criminal event. Until recently, they been used primarily for law enforcement purposes. Criminal history records have been legally available to the public since the 1976 case *Paul v. Davis*, in which the Supreme Court ruled that the publication of official acts, including arrest, conviction, and incarceration records, were not protected by privacy rights.⁶ Widespread use of criminal background checks as a pre-employment screen is a relatively recent phenomenon that stems from expanded legal availability and technical improvements that have made records more accessible.

Employer demand for criminal background checks is driven by their aversion to hiring applicants with criminal records. Criminal offenders may have fewer skills or be more likely to commit crime at the workplace, which can expose employers to negligent hiring lawsuits.⁷ In a 2001 survey of employers, more than 60% would “probably not” or “definitely not” hire an ex-offender (Holzer et al., 2007). The US Equal Employment Opportunity Commission (1987a; 1987b) has declared that employers may violate Title VII of the Civil Rights Act of 1964 if they broadly deny employment to applicants with criminal records, but that employers can ban applicants who have committed particular offenses if employers demonstrate these offenses are directly related to job functions. Some states have more severe restrictions, but there is little evidence of active enforcement.

⁶*Paul v. Davis*, 424 US 693 (1976).

⁷Negligent hiring can occur when an employee causes injury to a customer or co-worker, and the employer failed to take reasonable action in hiring that could have prevented the injury. A 2004 survey of human resource managers found that 3% of their firms had been accused of negligent hiring in the three years before the survey (Burke, 2005). Although the incidence of negligent hiring suits can be small, the potential monetary costs can be substantial. Wider availability of criminal background checks may be an important cause of increased attention to negligent hiring, since it lowers the cost of “reasonable” due diligence. See Odewahn and Webb (1989), Johnson and Indvik (1994), and Connerley et al. (2001) for a background on negligent hiring.

Given the risks of hiring ex-offenders and the relatively low cost of conducting criminal background checks, human resource practitioners now recommend conducting checks on all hires (Ander and Herbst, 2003; Rosen, 2006). Evidence from employer surveys shows a large increase in the last two decades in the use of criminal background checks during the hiring process. Holzer et al. (2007) report responses from surveys of Los Angeles employers in 1992–1994 and 2001. In the 1992–1994 sample, 32% of employers reported that they always conducted criminal background checks. In the 2001 sample, 46% said they always conducted criminal background checks. In 2004, the Society for Human Resource Management surveyed its members about pre-employment screening practices. In this national sample, 68% responded that they always conduct criminal background checks. These samples are not directly comparable, but they suggest that employer use of criminal background checks has increased substantially over time.

Employers who conduct criminal background checks must decide who to have conduct the search and over what jurisdictions to search.⁸ Private providers of background checks are plentiful, but the accuracy of their searches is not guaranteed to be any better than if an employer conducts the check on his own (Bushway et al., 2007). In reality, employers have no access to a *national* criminal background check. The Federal Bureau of Investigation maintains the only national repository of criminal records, known as the National Crime Information Center (NCIC), but it is not accessible by the general public. In lieu of a national search, most employers settle for a localized search of criminal records, which have historically been conducted by couriers at local courthouses. Employers seeking a wider search of criminal history data can use state databases that aggregate local and state arrest, conviction, and incarceration records.

Until the mid-1990s, there were few state-level resources for criminal background checks, but state-level databases are increasingly the most comprehensive sources of criminal history data.⁹ Automation of records by the states was facilitated by the National Criminal History Improvement Program, which was mandated by the Brady Handgun Violence Prevention Act of 1993.¹⁰ The Act imposed a five-day waiting period for firearm purchases and required that prospective gun owners clear background checks during that waiting period. The Act also stipulated that within

⁸See Rosen (2006) and Hinton (2004) for thorough discussions of criminal background check sources and reliability.

⁹From 1993 to 2001, the number of individuals in state criminal record databases increased from 47 million to 64 million (SEARCH, 1994; Brien, 2005). Over the same period, the proportion of all criminal history records that were automated increased from 79% to 89% (SEARCH, 1994; Brien, 2005).

¹⁰Public Law 103-159, Title I, 30 November 1993, 107 Statute 1536.

five years of its effective date such checks should be performed instantaneously through a national criminal background check system maintained by the Department of Justice (which became the FBI's NCIC system), and allocated funds for states to automate their records. Since 1995, states have received approximately \$400 million to improve data quality and speed the time between a criminal history event and when it is entered into a state-level database (Brien, 2005). As a result of the Brady Act, states began to have the technical capability to make criminal history records more accessible. In the late 1990s, some states began to make these records available over the Internet. Internet-based criminal background checks are significantly more convenient than any other method and state-level aggregation increases the geographic scope of background checks, so provision of criminal history records over the Internet is one of the most significant changes in the accessibility of records since the Supreme Court declared them public records in 1976. For these reasons, I use the provision of records over the Internet as the policy variation to identify the effect of record openness on labor market outcomes of ex-offenders and non-offenders.

A state is classified as having open records if, in a given year, it provides online access to the criminal histories of individuals released from its prisons. I collected this panel of policy data directly from state departments of correction or state police agencies, starting with a cross section of policies reported by the Legal Action Center (2004). Officials were asked when their state first provided criminal history records of released prisoners over the Internet. These websites allow any member of the public to search for ex-offenders who served their time in that state's prison system. In general, this will not be all prisoners, but rather prisoners who were sentenced to a year or more of prison time in local or state but not in federal courts. Although this is a subset of all prisoners, it is the majority of the incarcerated population. The sites provide personal information—such as name and aliases, birthdate, physical characteristics, and race—that allow a searcher to positively identify an ex-offender. The searches also detail the offenses for which time was served, lengths of the sentences, and release dates for each offense. Some systems only identify current offenders, but this information is not useful to employers, so these states are not coded as having open records.

Figure 3 is a map of the US showing the states that provide access to criminal records over the Internet, and the first year that information was available online. Between 1997 and 2004, 16

states began to make their criminal records available over the Internet.¹¹ The map shows that the expansion of access to criminal history records at the state level has been geographically and temporarily disperse—an important feature of my identification strategy. To account for time-invariant unobservable differences across states that adopt open records versus states that do not adopt open records, all empirical models include state fixed effects. All models include year fixed effects to account for the overall relative employment trends of ex-offenders. Then, the effects of opening criminal records are identified if there are no contemporaneous trends in labor market outcomes for ex-offenders relative to non-offenders in states that open records versus states that do not. If these conditions hold, this research design will yield estimates of the causal effects of greater information for employers about the criminal histories of their applicants.

One threat to identification of a causal effect is if states choose to make records available online based on legislative decisions related to employer preferences toward hiring ex-offenders. Fortunately, state adoption of Internet-based records searches was primarily a function of administrative decisions and the removal of technological hurdles. Of the sixteen states distributing criminal history records to the public in my sample, eight states responded to a survey by SEARCH (2006) of their governments' criminal records practices.¹² Of these, seven states were providing records over the Internet because of an administrative decision that relied on some pre-existing statute. Only one state (Oklahoma) was opening records by a direct order of legislation. This evidence suggests that the timing of record openness was not primarily a result of specific legislation. In another survey by SEARCH (2001), state officials responsible for criminal history records emphasized the importance of technical issues in determining when records went online. The statements of these officials support the argument that the limiting factor in the public provision of criminal history data over the Internet was technology rather than political preferences.¹³ Therefore, the

¹¹Florida was the first state to open records in this way in 1997. It was followed by New York and Washington state in 1998; Michigan and South Carolina in 1999; Georgia, Indiana, New Mexico, and Wisconsin in 2000; Kansas, Nebraska, and North Carolina in 2001; Montana and Oklahoma in 2002; and Vermont in 2003.

¹²SEARCH, the National Consortium for Justice Information and Statistics, has conducted surveys of criminal records systems on behalf of the Bureau of Justice Statistics.

¹³For example, Dave Sim of the Kansas Bureau of Investigation alludes to a learning process in administrative agencies with respect to providing criminal history data over the Internet:

Kansas maintains a prototype system that provides select non-criminal justice entities with Internet access to criminal history record information. The State will expand access to all users when it migrates from the prototype to its final design later in 2001. The system was designed primarily for criminal justice agencies but Kansas provided limited non-criminal justice access as it gained experience with Internet operations. (SEARCH, 2001)

temporal variation in introduction of open records, combined with state and year fixed effects, should allow for the identification of the causal effect of expanded access to criminal records on the labor market outcomes of ex-offenders and non-offenders.

Appendix C Data appendix: criminal history search intensity by occupation and industry

(Not complete yet.)

In the same survey, Ruth Lunn of the Maine State Police reported that Maine had not even begun the automation process necessary to provide records over the Internet (SEARCH, 2001). Her responses do not mention statute as the limiting factor in providing records, but rather the technological issues. David Dishong of the Nebraska State Patrol also suggested that the timing of public access to records over the Internet was a function of “programming and procedural issues” (SEARCH, 2001).

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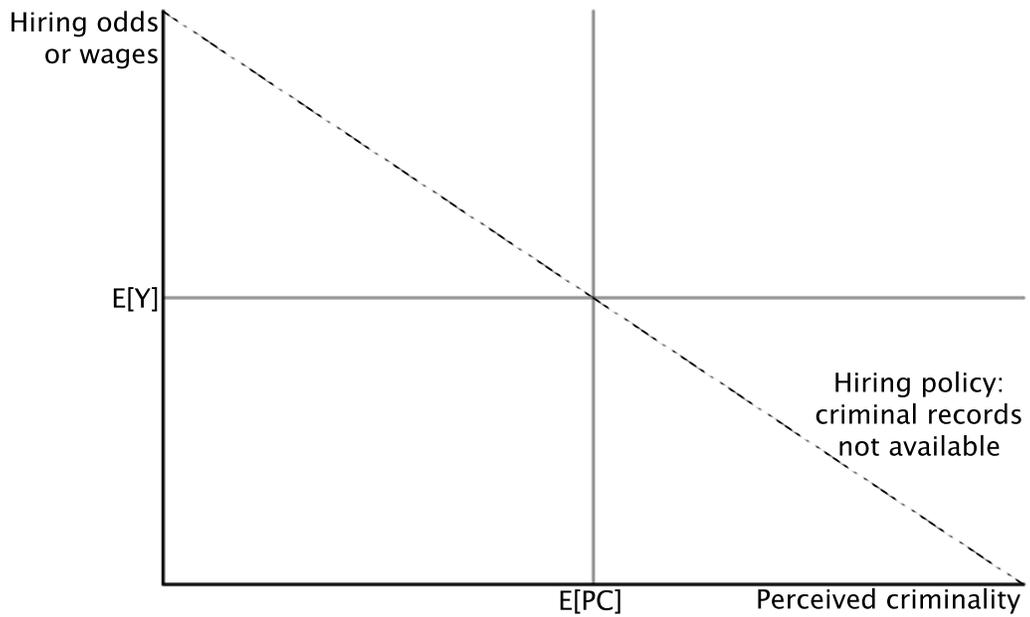
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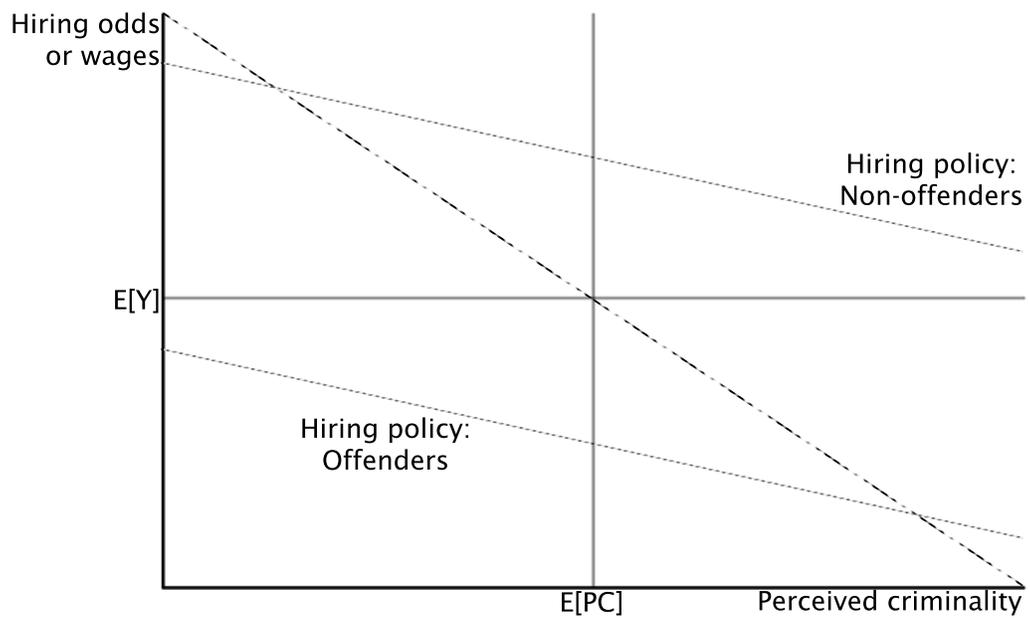
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Figure 1: Model of statistical discrimination in employment when there is asymmetric information about criminal history records

Panel A: criminal records not publicly available

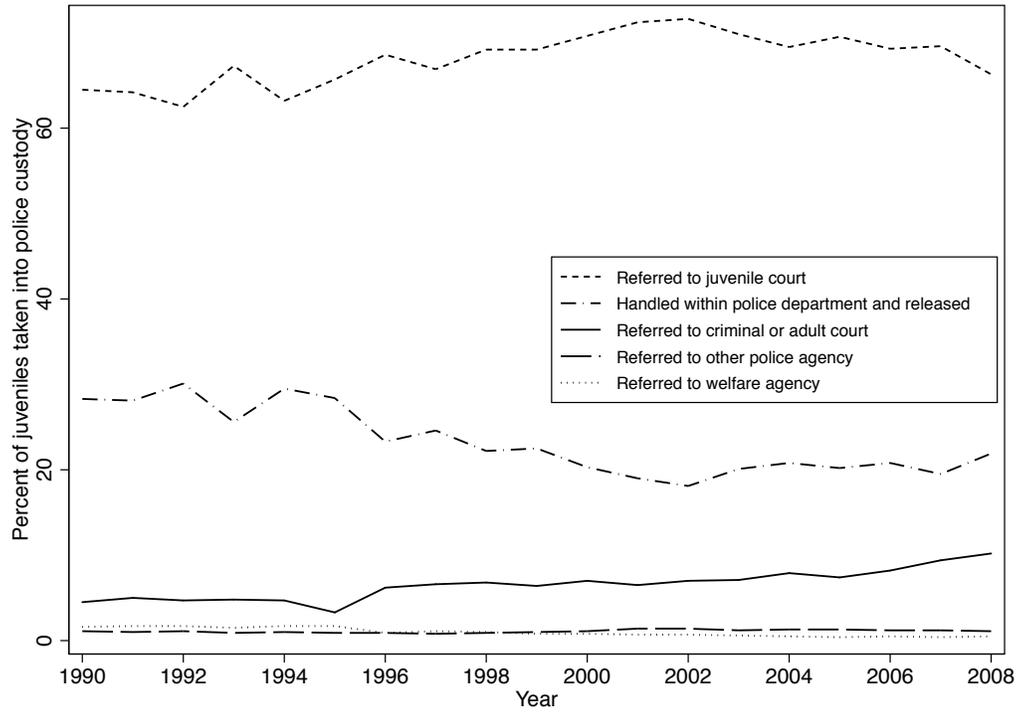


Panel B: criminal records available



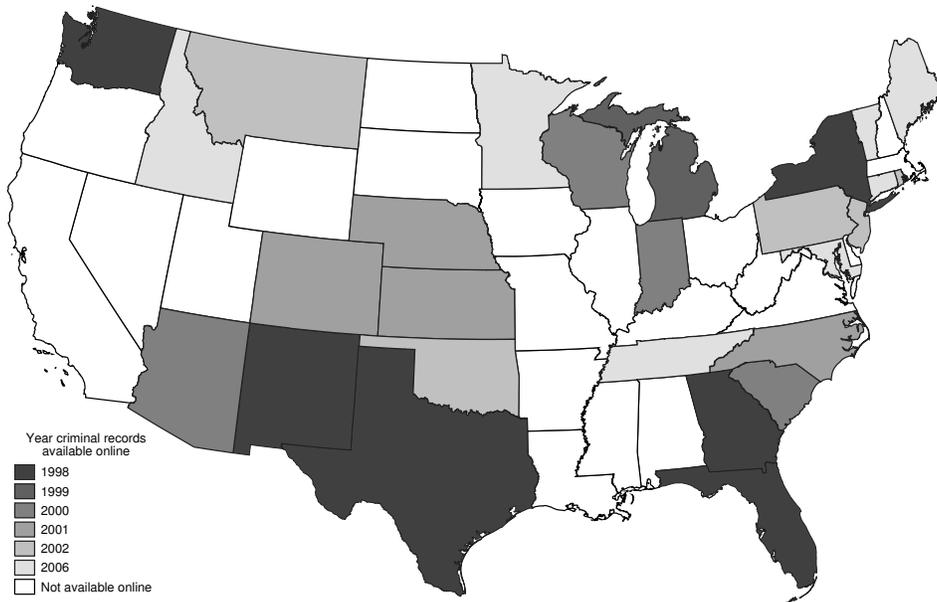
Notes:

Figure 2: Percent distribution of juveniles taken into police custody by method of disposition, US, 1990–2010



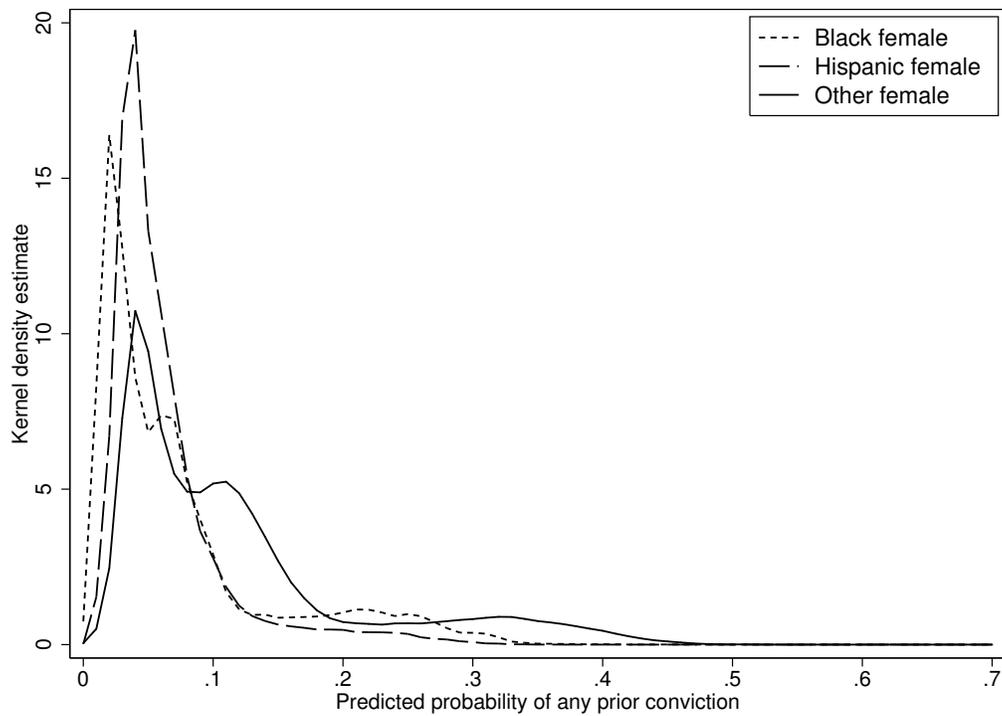
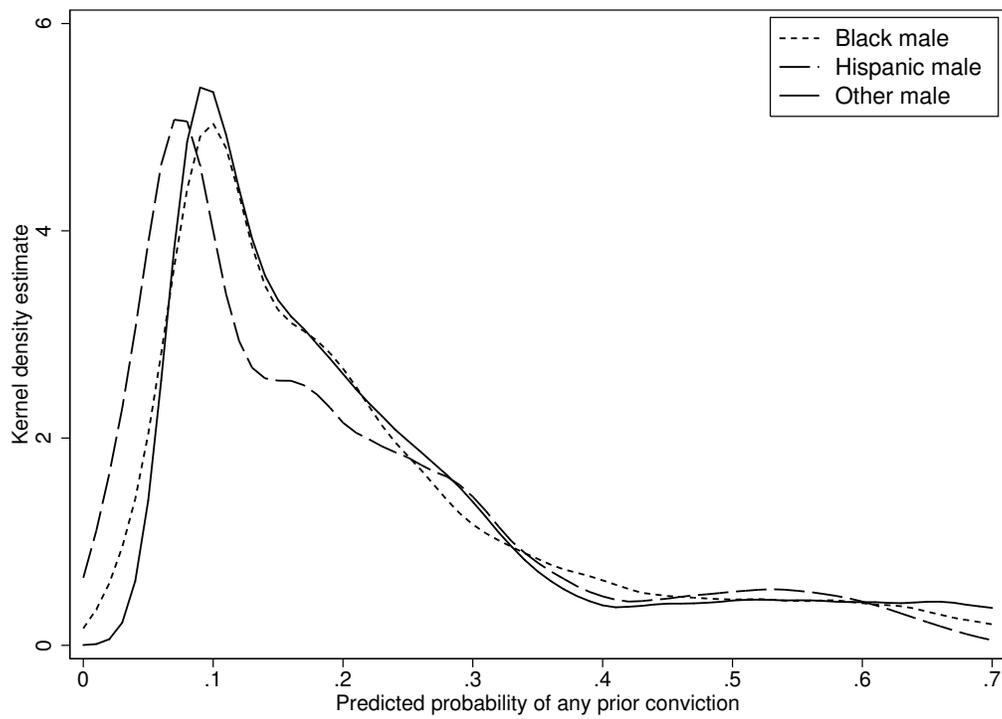
Notes: Source data is from Maguire (2011). Data accessed online at <http://www.albany.edu/sourcebook/pdf/t4262009.pdf> on October 29, 2011.

Figure 3: Public, online accessibility to criminal history records, first year by state, 1997–2010



Notes: Records are not available online in Alaska or Hawaii.

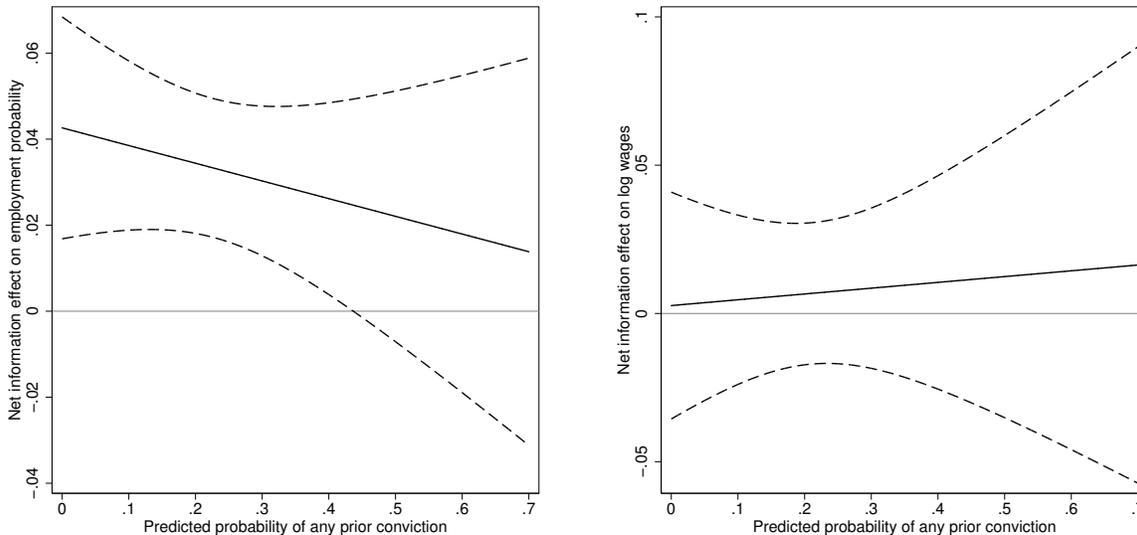
Figure 4: Kernel density estimates of the predicted probability of prior conviction, NLSY97, ages 18–31, 1997–2010



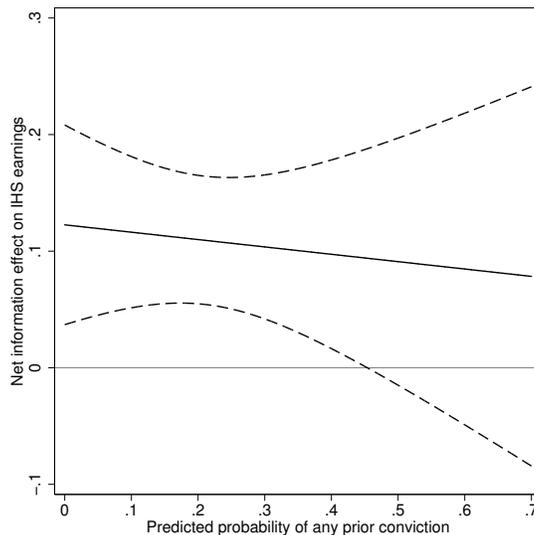
Notes: Predicted probabilities of prior conviction come from estimated probit models of prior conviction described in Equation 2. Full results can be found in Table A-1.

Figure 5: Estimated net effects of criminal history information on labor market outcomes as a function of perceived criminality (with 90% confidence intervals), NLSY97, ages 18–31, 1997–2010

Panel A: Net information effect on employment probability Panel B: Net information effect on log wages



Panel C: Net information effect on IHS earnings



Notes: Each point on the solid lines represents the linear combination of the estimated parameters in Table 3 (Columns 1, 3, and 5): $\beta_1 + C\beta_4 - \beta_3 - C\beta_6 + \beta_2 + C\beta_5$. The dotted lines are 90% confidence intervals.

Table 1: The means by which juveniles get to adult court

Category	Procedure	Number of states with rule in 1990	Number of states with rule in 2010	Median age for rule in 1990	Median age for rule in 2010	
Discretionary	Judicial waiver	}	31	32	14	14
	Direct file					
	<i>Murder</i>					
	<i>Robbery</i>					
	<i>Assault</i>					
	<i>Burglary</i>					
	<i>Theft</i>					
	<i>Major property</i>					
	<i>Drug possession</i>					
<i>Drug trafficking</i>						
Mandatory	Statutory exclusion	}	27	33	15	15
	Mandatory waiver					
	<i>Murder</i>					
	<i>Robbery</i>					
	<i>Assault</i>					
	<i>Burglary</i>					
	<i>Theft</i>					
	<i>Major property</i>					
	<i>Drug possession</i>					
	<i>Drug trafficking</i>					
Once an adult, always an adult						
Other rules	Presumptive waiver	—	—	—	—	
	Blended sentencing	—	—	—	—	
		States with age 16 in 1990	States with age 16 in 2010	States with age 17 in 1990	States with age 17 in 2010	
Criminal majority	Criminal majority	3	3	8	10	

Notes: Source is author's calculations from statutes shown in Table A-3.

Table 2: Selected descriptive statistics by age, NLSY97, ages 18–31, 1997–2010

	Age 18	Age 19	Age 20	Age 21	Age 22	Age 23	Age 24	Age 25	Age 26	Age 27	Age 28	Age 29	Age 30	Age 31
Ever incarcerated	0.02 (0.15)	0.03 (0.17)	0.04 (0.20)	0.05 (0.21)	0.06 (0.24)	0.07 (0.25)	0.08 (0.27)	0.08 (0.27)	0.09 (0.28)	0.09 (0.29)	0.10 (0.30)	0.11 (0.31)	0.11 (0.32)	0.22 (0.42)
Ever convicted	0.08 (0.28)	0.11 (0.31)	0.13 (0.33)	0.14 (0.34)	0.15 (0.36)	0.17 (0.37)	0.18 (0.38)	0.19 (0.39)	0.20 (0.40)	0.20 (0.40)	0.20 (0.40)	0.22 (0.41)	0.21 (0.41)	0.31 (0.47)
Employed	0.55 (0.50)	0.62 (0.49)	0.66 (0.47)	0.69 (0.46)	0.73 (0.44)	0.76 (0.43)	0.78 (0.42)	0.77 (0.42)	0.77 (0.42)	0.77 (0.42)	0.76 (0.43)	0.75 (0.43)	0.77 (0.42)	0.67 (0.48)
Hourly wage in primary job	7.65 (5.72)	8.50 (5.88)	9.27 (6.10)	10.16 (7.01)	11.32 (8.02)	12.74 (9.06)	13.98 (9.42)	14.91 (9.81)	15.66 (9.88)	16.59 (10.65)	17.27 (11.37)	17.69 (11.39)	19.03 (12.77)	20.44 (16.48)
Annual earnings (\$1000)	2.31 (3.72)	4.01 (5.46)	6.31 (7.51)	8.22 (9.19)	9.87 (10.84)	11.91 (12.90)	14.70 (15.88)	17.81 (18.16)	19.76 (20.45)	21.90 (22.36)	23.95 (24.24)	25.33 (26.30)	26.33 (28.65)	25.36 (31.92)
Experience since age 14 (1000s hours)	0.60 (1.27)	0.48 (1.37)	3.27 (2.74)	4.46 (3.40)	5.55 (4.11)	6.74 (4.92)	8.02 (5.85)	9.20 (6.75)	10.27 (7.68)	11.41 (8.63)	12.50 (9.69)	13.24 (10.35)	15.08 (11.15)	16.60 (10.83)
Tenure in primary job (weeks)	20.61 (39.72)	30.38 (46.52)	45.27 (56.42)	56.42 (67.10)	63.29 (74.98)	70.92 (83.33)	80.31 (91.74)	91.34 (101.94)	103.70 (113.50)	115.42 (125.73)	130.62 (138.45)	142.34 (150.46)	159.27 (167.23)	149.96 (163.29)
Earned GED	0.01 (0.12)	0.03 (0.17)	0.04 (0.20)	0.06 (0.23)	0.07 (0.25)	0.08 (0.28)	0.10 (0.30)	0.11 (0.31)	0.11 (0.32)	0.12 (0.32)	0.12 (0.33)	0.13 (0.34)	0.12 (0.33)	0.16 (0.37)
Earned HS diploma	0.44 (0.50)	0.71 (0.45)	0.75 (0.43)	0.75 (0.43)	0.68 (0.47)	0.59 (0.49)	0.53 (0.50)	0.50 (0.50)	0.47 (0.50)	0.46 (0.50)	0.45 (0.50)	0.45 (0.50)	0.47 (0.50)	0.36 (0.48)
Earned AA degree	0.00 (0.01)	0.00 (0.01)	0.01 (0.08)	0.02 (0.15)	0.04 (0.19)	0.05 (0.21)	0.05 (0.23)	0.06 (0.24)	0.06 (0.24)	0.06 (0.24)	0.07 (0.25)	0.07 (0.26)	0.07 (0.25)	0.11 (0.32)
Earned BA degree or more	0.00 (0.07)	0.01 (0.07)	0.01 (0.10)	0.02 (0.12)	0.07 (0.26)	0.15 (0.36)	0.20 (0.40)	0.22 (0.41)	0.24 (0.43)	0.24 (0.43)	0.26 (0.44)	0.25 (0.43)	0.25 (0.43)	0.22 (0.42)
Body mass index	24.04 (4.66)	24.49 (4.81)	24.88 (4.89)	25.33 (5.10)	25.75 (5.26)	26.14 (5.31)	26.51 (5.51)	26.71 (5.53)	27.03 (5.61)	27.25 (5.65)	27.63 (5.72)	27.61 (5.65)	27.95 (5.61)	27.56 (6.51)
BMI missing	0.04 (0.19)	0.05 (0.21)	0.06 (0.24)	0.07 (0.25)	0.07 (0.26)	0.07 (0.25)	0.05 (0.23)	0.06 (0.24)	0.06 (0.23)	0.06 (0.24)	0.07 (0.25)	0.06 (0.24)	0.06 (0.24)	0.09 (0.29)
Lives in central city	0.31 (0.46)	0.31 (0.46)	0.35 (0.48)	0.38 (0.48)	0.39 (0.49)	0.42 (0.49)	0.43 (0.50)	0.43 (0.49)	0.43 (0.49)	0.42 (0.49)	0.42 (0.49)	0.41 (0.49)	0.43 (0.50)	0.56 (0.50)
ASVAB	37.00 (31.85)	36.91 (31.86)	37.26 (32.12)	37.22 (31.96)	37.26 (32.05)	36.93 (31.92)	36.80 (31.94)	36.73 (31.89)	36.78 (31.92)	36.26 (31.84)	36.08 (31.76)	35.79 (31.60)	34.58 (31.58)	32.76 (30.64)
ASVAB missing	0.19 (0.39)	0.19 (0.40)	0.20 (0.40)	0.19 (0.39)	0.19 (0.39)	0.19 (0.39)	0.19 (0.40)	0.19 (0.39)	0.19 (0.39)	0.20 (0.40)	0.20 (0.40)	0.20 (0.40)	0.21 (0.41)	0.18 (0.39)
Female	0.50 (0.50)	0.49 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.51 (0.50)	0.49 (0.51)
Black	0.26 (0.44)	0.26 (0.44)	0.26 (0.44)	0.26 (0.44)	0.26 (0.44)	0.27 (0.44)	0.27 (0.45)	0.27 (0.44)	0.27 (0.45)	0.27 (0.45)	0.27 (0.45)	0.28 (0.45)	0.29 (0.45)	0.36 (0.48)
Hispanic	0.21 (0.41)	0.21 (0.41)	0.21 (0.41)	0.21 (0.41)	0.21 (0.41)	0.21 (0.40)	0.18 (0.39)							

Notes:

Table 3: Regressions of labor market outcomes on criminal justice variables, NLSY97, ages 18–31, 1997–2010

	Employment		Log wage		IHS earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Has no record, open records (β_1)	0.014*** (0.005)	0.003 (0.010)	-0.024*** (0.007)	0.014 (0.014)	0.034** (0.017)	-0.036 (0.032)
Has record, closed records (β_2)	0.010 (0.012)	-0.006 (0.022)	0.004 (0.017)	-0.031 (0.030)	0.128*** (0.038)	0.156** (0.074)
Has record, open records (β_3)	-0.019** (0.009)	-0.029 (0.027)	-0.024* (0.012)	-0.069 (0.042)	0.039 (0.030)	0.025 (0.089)
Perceived criminality	0.432*** (0.035)	0.189*** (0.073)	0.046 (0.053)	-0.175* (0.098)	0.836*** (0.122)	-0.000 (0.245)
Has no record, open records \times perceived criminality (β_4)	0.017 (0.025)	0.016 (0.046)	0.056 (0.038)	-0.016 (0.071)	0.008 (0.095)	0.195 (0.157)
Has record, closed records \times perceived criminality (β_5)	-0.232*** (0.040)	-0.126* (0.070)	-0.111* (0.064)	-0.106 (0.098)	-0.807*** (0.139)	-0.957*** (0.233)
Has record, open records \times perceived criminality (β_6)	-0.174*** (0.031)	-0.077 (0.065)	-0.075 (0.048)	0.013 (0.092)	-0.736*** (0.112)	-0.882*** (0.217)
Year fixed effects	\times	\times	\times	\times	\times	\times
Individual fixed effects		\times		\times		\times
Information effect (with record, $C = .35$)	-0.008 (0.009)	-0.005 (0.023)	-0.014 (0.015)	0.004 (0.031)	-0.064** (0.032)	-0.104 (0.076)
$H_0: \beta_3 + .35\beta_6 - \beta_2 - .35\beta_5 = 0$						
Information effect (no record, $C = .35$)	0.020*** (0.006)	0.009 (0.012)	-0.005 (0.010)	0.009 (0.019)	0.036 (0.024)	0.033 (0.042)
$H_0: \beta_1 + .35\beta_4 = 0$						
Net information effect ($C = .35$)	0.028** (0.012)	0.014 (0.026)	0.009 (0.019)	0.005 (0.037)	0.100** (0.043)	0.137 (0.088)
$H_0: \beta_1 + .35\beta_4 - \beta_3 - .35\beta_6 + \beta_2 + .35\beta_5 = 0$						
R^2	0.12	0.05	0.26	0.28	0.28	0.21
N	82,652	82,652	65,147	65,147	82,652	82,652
Mean of dep. var.	0.733	0.733	2.382	2.382	2.289	2.289
S.D. of dep. var.	0.442	0.442	0.606	0.606	1.672	1.672

Notes: Models in odd-numbered columns are estimated as pooled cross-sectional samples, while models in the even-numbered columns use the panel with individual fixed effects. All models include the following time-varying regressors: highest grade completed and its square; total work experience since age 13 and its square; age and its square; body-mass index; and dummies for completion of a GED, high school diploma, AA degree, or a bachelors degree or more; and dummies for central-city residence and missing body-mass index. Pooled cross-sectional models include the following time-invariant regressors: ASVAB score and dummies for missing ASVAB, female sex, black race, and Hispanic ethnicity. Wages models also include tenure in weeks in the primary job. Stars indicate statistical significance: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Full results can be found in Table A-2.

Table A-1: Probit regressions of prior conviction on variables observation to employers, NLSY97, ages 18–31, 1997–2010

	Men			Women		
	(1) Black	(2) Hispanic	(3) Other	(4) Black	(5) Hispanic	(6) Other
Highest grade completed	0.134** (0.068)	0.274*** (0.103)	-0.201*** (0.048)	0.343*** (0.102)	-0.002 (0.117)	0.025 (0.059)
HGC squared	-0.011*** (0.003)	-0.019*** (0.004)	0.003 (0.002)	-0.016*** (0.004)	-0.004 (0.005)	-0.005** (0.002)
Earned GED	0.298*** (0.051)	-0.027 (0.065)	0.233*** (0.043)	-0.124* (0.071)	0.520*** (0.091)	0.022 (0.053)
Earned HS diploma	-0.408*** (0.050)	-0.385*** (0.054)	-0.480*** (0.040)	-0.784*** (0.072)	0.064 (0.089)	-0.543*** (0.049)
Earned AA degree	-0.106 (0.114)	-0.428*** (0.143)	-0.612*** (0.074)	-0.675*** (0.147)	0.006 (0.185)	-0.495*** (0.084)
Earned BA degree or more	-0.322*** (0.116)	-0.145 (0.133)	-0.598*** (0.070)	-0.773*** (0.152)	0.013 (0.206)	-0.696*** (0.086)
Experience since age 14 (1000s hours)	-0.024*** (0.006)	-0.008 (0.007)	0.012*** (0.004)	-0.012 (0.009)	-0.007 (0.010)	0.025*** (0.006)
Experience squared	0.000 (0.000)	-0.000 (0.000)	-0.001*** (0.000)	0.001* (0.000)	-0.000 (0.000)	-0.001*** (0.000)
Age	0.658*** (0.076)	0.543*** (0.086)	0.550*** (0.052)	0.604*** (0.096)	0.264** (0.122)	0.348*** (0.065)
Age squared	-0.012*** (0.002)	-0.009*** (0.002)	-0.009*** (0.001)	-0.011*** (0.002)	-0.004* (0.003)	-0.006*** (0.001)
Body mass index	-0.023*** (0.003)	-0.007** (0.003)	-0.018*** (0.002)	-0.002 (0.003)	0.003 (0.005)	-0.010*** (0.002)
BMI missing	-0.808*** (0.101)	-0.256** (0.116)	-0.538*** (0.084)	-0.357*** (0.117)	-0.142 (0.151)	-0.273*** (0.085)
Lives in central city	0.063** (0.031)	-0.109*** (0.034)	-0.004 (0.022)	0.160*** (0.041)	0.090* (0.048)	0.114*** (0.027)
_cons	-8.639*** (1.008)	-8.387*** (1.249)	-5.601*** (0.701)	-10.647*** (1.354)	-4.755*** (1.657)	-5.125*** (0.883)
Pseudo R ²	0.14	0.12	0.13	0.12	0.05	0.10
N	10,650	8,802	22,085	11,415	8,720	20,980
Mean of dep. var.	0.271	0.223	0.212	0.075	0.064	0.098
S.D. of dep. var.	0.445	0.416	0.409	0.263	0.244	0.297

Notes: Models are estimated as pooled cross-sections. Stars indicate statistical significance: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-2: Regressions of labor market outcomes on criminal justice variables, NLSY97, ages 18–31, 1997–2010

	Employment		Log wage		IHS earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Highest grade completed	0.072*** (0.007)	-0.010 (0.015)	-0.008 (0.012)	-0.104*** (0.022)	0.272*** (0.027)	0.024 (0.050)
HGC squared	-0.002*** (0.000)	0.001 (0.001)	0.001** (0.000)	0.004*** (0.001)	-0.009*** (0.001)	0.000 (0.002)
Earned GED	0.015* (0.008)	0.024 (0.015)	0.045*** (0.012)	0.040* (0.022)	0.105*** (0.028)	-0.061 (0.051)
Earned HS diploma	0.145*** (0.008)	0.057*** (0.012)	0.058*** (0.010)	0.008 (0.015)	0.389*** (0.023)	0.076** (0.034)
Earned AA degree	0.199*** (0.011)	0.075*** (0.018)	0.164*** (0.016)	0.092*** (0.024)	0.709*** (0.040)	0.231*** (0.055)
Earned BA degree or more	0.242*** (0.011)	0.189*** (0.017)	0.251*** (0.015)	0.218*** (0.021)	0.939*** (0.037)	0.696*** (0.049)
Experience since age 14 (1000s hours)	0.021*** (0.001)	0.009*** (0.001)	0.008*** (0.001)	0.006*** (0.002)	0.101*** (0.002)	0.081*** (0.004)
Experience squared	-0.000*** (0.000)	-0.000*** (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.001*** (0.000)	-0.002*** (0.000)
Age	-0.031*** (0.012)	0.054*** (0.017)	0.066*** (0.017)	0.138*** (0.023)	0.478*** (0.039)	0.638*** (0.058)
Age squared	0.000* (0.000)	-0.001** (0.000)	-0.001** (0.000)	-0.002*** (0.000)	-0.009*** (0.001)	-0.011*** (0.001)
Body mass index	0.001*** (0.000)	-0.002** (0.001)	-0.003*** (0.000)	0.001 (0.001)	0.004*** (0.001)	-0.000 (0.002)
BMI missing	-0.011 (0.012)	-0.063*** (0.022)	-0.127*** (0.016)	0.026 (0.030)	-0.189*** (0.039)	-0.132* (0.075)
Lives in central city	0.000 (0.003)	0.028*** (0.005)	0.005 (0.005)	0.017** (0.008)	-0.002 (0.012)	0.087*** (0.018)
ASVAB	0.000*** (0.000)		0.001*** (0.000)		0.003*** (0.000)	
ASVAB missing	0.002 (0.005)		0.013* (0.008)		0.054*** (0.018)	
Black	-0.063*** (0.004)		-0.071*** (0.006)		-0.341*** (0.014)	
Hispanic	0.024*** (0.004)		0.030*** (0.006)		-0.036** (0.016)	
Has no record, open records (β_1)	0.014*** (0.005)	0.003 (0.010)	-0.024*** (0.007)	0.014 (0.014)	0.034** (0.017)	-0.036 (0.032)
Has record, closed records (β_2)	0.010 (0.012)	-0.006 (0.022)	0.004 (0.017)	-0.031 (0.030)	0.128*** (0.038)	0.156** (0.074)
Has record, open records (β_3)	-0.019** (0.009)	-0.029 (0.027)	-0.024* (0.012)	-0.069 (0.042)	0.039 (0.030)	0.025 (0.089)
Perceived criminality	0.432*** (0.035)	0.189*** (0.073)	0.046 (0.053)	-0.175* (0.098)	0.836*** (0.122)	-0.000 (0.245)
Has no record, open records \times perceived criminality (β_4)	0.017 (0.025)	0.016 (0.046)	0.056 (0.038)	-0.016 (0.071)	0.008 (0.095)	0.195 (0.157)
Has record, closed records \times perceived criminality (β_5)	-0.232*** (0.040)	-0.126* (0.070)	-0.111* (0.064)	-0.106 (0.098)	-0.807*** (0.139)	-0.957*** (0.233)
Has record, open records \times perceived criminality (β_6)	-0.174*** (0.031)	-0.077 (0.065)	-0.075 (0.048)	0.013 (0.092)	-0.736*** (0.112)	-0.882*** (0.217)
Tenure in primary job (weeks)			0.000*** (0.000)	0.000*** (0.000)		
Year fixed effects	\times	\times	\times	\times	\times	\times
Individual fixed effects		\times		\times		\times
Information effect (with record, $C = .35$)	-0.008 (0.009)	-0.005 (0.023)	-0.014 (0.015)	0.004 (0.031)	-0.064** (0.032)	-0.104 (0.076)
$H_0: \beta_3 + .35\beta_6 - \beta_2 - .35\beta_5 = 0$						
Information effect (no record, $C = .35$)	0.020*** (0.006)	0.009 (0.012)	-0.005 (0.010)	0.009 (0.019)	0.036 (0.024)	0.033 (0.042)
$H_0: \beta_1 + .35\beta_4 = 0$						
Net information effect ($C = .35$)	0.028** (0.012)	0.014 (0.026)	0.009 (0.019)	0.005 (0.037)	0.100** (0.043)	0.137 (0.088)
$H_0: \beta_1 + .35\beta_4 - \beta_3 - .35\beta_6 + \beta_2 + .35\beta_5 = 0$						
R ²	0.12	0.05	0.26	0.28	0.28	0.21
N	82,652	82,652	65,147	65,147	82,652	82,652
Mean of dep. var.	0.733	0.733	2.382	2.382	2.289	2.289
S.D. of dep. var.	0.442	0.442	0.606	0.606	1.672	1.672

Notes: Models in odd-numbered columns are estimated as pooled cross-sectional samples, while models in the even-numbered columns use the panel with individual fixed effects. Stars indicate statistical significance: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-3: Statutory references for criminal majority and juvenile transfer laws

	Juvenile Defined	Direct File	Judicial Waiver or Light Presumptive	Mandatory Waiver or Tough Presumptive	Statutory Exclusion	Once an Adult Always an Adult
Alabama	§ 12-15-102	—	§ 12-15-203	—	§ 12-15-102; § 12-15-204	—
Alaska	47.12.020	—	47.12.100; Alaska Court Rules, Part VI, Rule 20	—	47.12.030	—
Arizona	8-201	13-501	8-327	—	8-202; 8-302; 8-341; 13-501	8-341
Arkansas	§ 9-27-303	§ 9-27-318	§ 9-27-318	—	—	—
California	Family Code-11-1-6500	Welfare and Institutions-2-1-2-17-707.1; Welfare and Institutions-2-1-2-17-707	Welfare and Institutions-2-1-2-17-707.1; Welfare and Institutions-2-1-2-17-707	—	Welfare and Institutions-2-1-2-17-707.1; Welfare and Institutions-2-1-2-17-707	Welfare and Institutions-2-1-2-17-707.1; Welfare and Institutions-2-1-2-17-707
Colorado	19-1-103	19-2-517	19-2-518	19-2-518	—	—
Connecticut	46b-120	46b-127	—	46b-127	—	—
Delaware	10-§ 901	—	10-§1010	10-§1010	10-§1010	10-§1010
District of Columbia	II-§ 16-2301	II-§ 16-2307	II-§ 16-2307	II-§ 16-2307	—	II-§ 16-2307
Florida	XLVII-985.03	XLVII-985.557	XLVII-985.556	—	XLVII-985.557, XLVII-985.56	XLVII-985.556; XLVII-985.557
Georgia	§ 15-11-2	§ 15-11-28	§ 15-11-30.2	§ 15-11-30.3	§ 15-11-28	—
Hawaii	3-31-§ 577-1	—	3-31-§571-22	—	—	3-31-§571-22
Idaho	20-5-32-101	—	20-5-20-508	—	20-5-20-509	20-5-20-509
Illinois	705-405/5-105	—	705-405/5-805; 705-405/5-810	705-405/5-805	705-405/5-130	705-405/5-130
Indiana	11-8-1-2	—	31-30-3-2; 31-30-3-3; 31-30-3-4; 31-30-3-5	31-30-3-6	14-15-10-3; 31-30-1-1; 31-30-1-2	31-30-1-2
Iowa	VI-232.2; XV-599.1	—	VI-232.45	—	VI-232.8	VI-232.45A
Kansas	38-101; 38-2302;	—	38-2347;	—	—	38-2302
Kentucky	LI-600.020	—	LI-640.01; LI-635.020	LI-635.02	—	—
Louisiana	VIII-1-804	III-4-305	VIII-11-862	VIII-11-857	III-4-305	—
Maine	15-501-§3003	—	15-503-§3101;	—	—	15-503-§3101;
Maryland	§4-202.1	—	§ 3-8A-06	—	§ 3-8A-03	§4-202
Massachusetts	XVII-119-52	—	—	—	XVII-119-74	—
Michigan	712A.2	712A.2	712A.2d	—	—	—
Minnesota	§ 260B.007	—	§ 260B.125	§ 260B.125	§ 260B.007	§ 260B.125

Mississippi	§ 43-21-151	—	§ 43-21-157	—	§ 43-21-151	§ 43-21-157
Missouri	XII-211.021	—	XII-211.071	—	—	XII-211.071
Montana	41-1-101	41-5-206	—	—	41-5-206	—
Nebraska	§ 43-245	§ 43-276	—	—	—	—
Nevada	62A.030	—	62B.390	62B.330	62B.330	—
New Hampshire	XII-169-B:2	—	XII-169-B:24	XII-169-B:24	—	XII-169-B:27
New Jersey	2A:4A-22	—	2A:4A-26	2A:4A-26	—	—
New Mexico	§32A-1-4; §32A-2-3	—	—	—	§32A-2-3	—
New York	11-A-U-§720.10	—	—	—	11-A-H-§180.75	—
North Carolina	§ 7B-1604	—	§ 7B-2200; § 7B-2203	§ 7B-2200	—	§ 7B-1604
North Dakota	14-10-01; 12.1-04-01	—	§ 27-20-34	§ 27-20-34	—	§ 27-20-34
Ohio	XXI-2152.02	—	XXI-2152.1	XXI-2152.1; XXI-2152.12	—	XXI-2152.02
Oklahoma	10A-§ 2-1-103	10A-§2-5-206	10A-§2-5-205	—	10A-§ 2-5-101	10A-§ 2-5-204
Oregon	34-§419C.005	—	34-§419C.349; 34-§419C.352	—	14-§ 137.707	Psuedo: 34-§419C.364
Pennsylvania	42-VI-§6302; Rules of Juvenile Court Behavior-Rule 120	—	42-VI-§6355	Rules of Juvenile Court Behavior-Rule 394	42-VI-§6302	42-VI-§6302
Rhode Island	§ 14-1-3	—	§ 14-1-7; § 14-1-7.1	§ 14-1-5	—	§ 14-1-7.3.
South Carolina	§ 63-19-20	—	§ 63-19-1210	§ 63-19-1210	§ 63-19-20	—
South Dakota	§26-7A-1; §26-8C-2	—	§26-11-4	—	§26-11-3.1	§26-11-4
Tennessee	37-11-102; 37-1-102	—	37-1-134	—	—	37-1-134
Texas	§ 51.02	—	§ 54.02	—	§ 51.03; § 8.07	§ 54.02
Utah	15-2-1	—	78A-6-703	78A-6-702	78A-6-701	78A-6-702
Vermont	33-51-§ 5102	33-52-§ 5203	33-52-§ 5204	—	33-51-§ 5102	—
Virginia	§ 16.1-228	§ 16.1-269.1	§ 16.1-269.1	§ 16.1-269.1	—	§ 16.1-271
Washington	26.28.010; 13.40.020	—	13.40.110	—	13.04.030	13.40.020
West Virginia	§49-1-2; §49-5-1	—	§49-5-10	§49-5-10	—	—
Wisconsin	938.02	—	938.18.	—	938.183	938.183
Wyoming	§ 14-6-203; §14-1-101	§ 14-6-203	§ 14-6-237	—	—	—