

The Effect of Mass Incarceration on Black Women

Elisa Jacome *

Princeton University

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Abstract

This paper studies the relationship between rising incarceration rates and the outcomes of black women. Using state-level annual data documenting changes in sentencing policies between 1970 and 2002, I select two policies that likely satisfy as valid instruments for the incarceration rate: determinate sentencing and mandatory sentencing laws for offenses involving a weapon. I then use these instruments to estimate the effect of incarceration on black women's outcomes using two-stage least squares. Estimates that use the universe of black births from Vital Statistics data find no evidence that higher incarceration rates lead to lower birth rates, a higher share of out-of-wedlock births, or a higher share of low birthweight births. The results for labor market outcomes are less precisely estimated, but I do find some evidence that higher incarceration rates lead to higher employment and labor force participation rates for black women.

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1 Introduction

During the last two decades of the 20th century, the United States witnessed dramatic growth in its incarcerated population. The incarceration rate—defined as the number of inmates per every 100,000 U.S. residents—grew most rapidly during the 1980s and 1990s, but it did not hit its peak until 2008. Figure 1 shows the evolution of the incarceration rate for state prisons between 1978 and 2012.¹ Even though the incarceration rate rose in every state during this time period, the growth in the incarceration rate was not uniform so that by the 2000s, some states had significantly larger prison populations than others. As an example, figure 2 illustrates the difference in incarceration rates across states in 2003. (Appendix A also includes figures that plot each state’s incarceration rate; these figures highlight the very different trajectories in growth across states.)

This paper uses the time and state-level variation between 1977 and 2003—the time period in which the incarceration rate grew the most rapidly and for which policy data are available—to explore whether higher incarceration rates had an effect on the outcomes of women. Unlike previous studies, this paper focuses specifically on the outcomes of black women. This choice is motivated by the fact that changes in incarceration policy did not affect the general population uniformly; they had a disproportionate effect on black communities. To put this disparity into perspective, in 2004 slightly less than half of both state and federal prisoners were black even though black individuals constituted only 11 percent of the adult population at the time (Raphael and Stoll 2013b).²

Although most prisoners are men, it is natural to think that mass incarceration also impacted the outcomes of women. The mass incarceration of black men likely affected family formation and marriage market dynamics for black women, either through changes in the female-male ratio in their communities or more indirectly by altering the “marriageability” of men. The primary

¹ The first year of available data using the National Prisoner Statistics is 1978. It is worth noting that most of the growth in the incarceration rate occurred after this year so that the figure is not missing any important pre-trends (Kearney et al. 2014).

² This racial disparity can in part be explained by differences in arrest rates. For decades, arrest rates for blacks have been at least four times greater than arrest rates for whites, so more punitive sentencing policies disproportionately resulted in higher incarceration rates for black men (Neal and Rick 2016).

outcomes of interest in this paper are family formation outcomes: namely, birth rates, the share of births that are out of wedlock, the share of births that are low birthweight, and the share of single mothers. I also consider the marriage rate and the share of women who report no longer being married (i.e., divorced, separated, or widowed).

The secondary outcomes of interest in this paper are labor market outcomes. Higher incarceration rates might have resulted in fewer women getting married or more women being left in economically vulnerable positions if their partners or spouses were incarcerated. Mass incarceration might have therefore incentivized women to seek employment as a way to increase their financial independence or security. This paper focuses on three labor market outcomes: whether a woman is employed, in the labor force, and living in poverty.³

To estimate the causal effect of incarceration on these outcomes, this paper exploits the state-year variation in the adoption of sentencing laws and uses two policies as instruments for the incarceration rate. In particular, the paper begins by considering fourteen policy changes for which data are available for every state. These policies range from laws that attempted to add more structure to the criminal justice system to laws intended to punish violent and drug offenders. I explore the relationship between these sentencing laws and the incarceration rate, selecting the policies with the most predictive power. I then use event studies and other exercises to explore the validity of this subset of policies as instruments, finding that two policies—determinate sentencing and a mandatory sentencing law for offenses involving a weapon (from here on referred to as a “weapons mandatory”)—are likely valid instruments. The underlying assumption of this empirical strategy is that even though sentencing reform is closely linked to the incarceration rate, the exact timing of the adoption of these laws is exogenous once state-specific trends are accounted for.

Using this empirical approach, I find no indication that higher incarceration rates decreased birth rates, increased the share of out-of-wedlock births, or increased the share of low birthweight births for black women. I use the universe of black births from Vital Statistics data for these outcomes, resulting in coefficients that are precisely estimated. Specifically, I can reject with 95 percent

³ Poverty status is not strictly a labor market outcome, but given its close connection to employment and labor force participation, I include this variable as a labor market outcome for the remainder of the paper.

confidence a decline of 0.64 percent in the birth rate from a one percent increase in the incarceration rate. I can also rule out, with 95 percent confidence, an increase of 0.11 and 0.05 percentage points in the share of births that are out of wedlock and that are low birthweight, respectively, from a one percent increase in the incarceration rate.

The findings for the labor market outcomes are less precisely estimated, but estimates do provide some evidence that the incarceration rate increased employment and labor force participation rates for black women. In particular, a one percent increase in the incarceration rate results in a 0.30 percentage point increase (on a base of 61 percent) and a 0.28 percentage point increase (on a base of 72 percent) in the employment and labor force participation rate, respectively, of black women. The results are driven by women who classified as heads of their household. These findings are in line with the results of previous studies and consistent with the notion that higher incarceration rates incentivize women to increase their financial independence.

This paper contributes to the incarceration literature in a few notable ways. First, the main advantage of this paper is the richness of the state-level data that documents changes in sentencing policies for every year between 1970 and 2002. Unlike previous studies, I consider the relevance and validity of these policies in depth, ultimately showing that two of the policies can likely serve as instrumental variables for the incarceration rate. Second, this paper explores the causal effect of higher incarceration rates on the outcomes of black women, which is the demographic group for whom I would expect to see the largest effects. Importantly, since the rise in the incarceration rate coincided in timing with other social and economic trends (e.g., the rise in out-of-wedlock births), it is important to understand if and how these phenomena are related. This paper estimates the causal nature of these relationships in order to better understand how changes in the criminal justice system affected the lives of women. More broadly, given the various outcomes of interest, this paper also contributes to growing literatures in economics that focus on single motherhood, female labor supply, as well as the forces behind growing economic inequality.

The remainder of the paper is organized as follows. Section 2 presents background on sentencing reform, related literature, and theoretical predictions. Section 3 outlines the data utilized. Section 4 introduces the methodological framework and considers the relevance and validity of the policies as instruments. Section 5 presents summary statistics and results, and section 6 conducts robustness

checks and discusses the sensitivity of estimates. Section 7 concludes.

2 Background

2.1 Sentencing Reform

Starting in 1975, all states as well as the federal government started reforming their sentencing laws. The criminal justice system began adopting and reforming laws after it came under heavy scrutiny from both liberals and conservatives in the early 1970s. Conservatives criticized the fact that judges had ample room for leniency in determining sentences, while liberals criticized the disparate treatment or excessively harsh punishments that minorities and the poor received in this relatively unstructured system (Tonry 1996). Moreover, in response to high crime rates and a desire to appear tough on crime, many state legislatures made sentencing laws more punitive in the 1980s and 1990s.

The laws and policies adopted during this time period varied widely in intention, in the segment of the criminal justice system that they aimed to reform, and in the types of offenders they targeted. For example, some of these reforms—such as determinate sentencing—were intended to create more structure by reducing the power of judges and parole boards in determining the length of a criminal sentence. Other reforms—such as sentencing enhancements and mandatory minimums—were intended to more heavily punish violent, drug, or repeat offenders. Nevertheless, a common theme among the sentencing reforms of this time period was that they typically increased the likelihood that an arrested offender would be sent to prison as well as the time prisoners could expect to serve. Evidence suggests that the growth in the incarceration rate can be attributed to changes in these sentencing laws and policies (e.g., Neal and Rick 2016, Raphael and Stoll 2013).

2.2 Related Literature

Given this massive increase in the incarceration rate starting in the 1970s, several studies have tried to understand its effects. There is a substantial body of literature that has used state-level panel data to estimate how changes in the incarceration rate have affected crime rates (e.g., Johnson and Raphael 2012; Levitt 1996; Marvell and Moody 1994).

Another part of the literature has focused on the effect of mass incarceration on formerly incarcerated individuals. Specifically, a number of studies have focused on the reduced employment prospects of ex-offenders, many of whom are limited by a deficit or depreciation of human capital or by employers' reluctance to hire them (see e.g., Holzer et al. 2006, Kling 2006, Mueller-Smith forthcoming). Mass incarceration has also had a spillover effect on the employment prospects of black men more generally since some employers statistically discriminate against men from this demographic group in an attempt to avoid hiring individuals with criminal history records (e.g., Doleac and Hansen 2016, Pager 2003).

A number of studies in sociology have focused on the effects of incarceration on communities more broadly. Since mass incarceration coincided in timing with a number of other trends in lower-income communities, there has been a push to understand if and how these trends are related. For example, DeFina and Hannon (2013) find that growing incarceration has increased poverty and that the poverty rate would have been lower had it not been for mass incarceration. Pettit and Western (2010) argue that a substantial part of growing social inequality is associated with higher use of prisons and jails. Furthermore, as men were incarcerated in previously unseen proportions, women were often left behind, raising children without fathers. In 2010, more than half of inmates were parents with minor children, which translates into more than one million fathers (The Pew Charitable Trusts 2010). There is therefore also a sizable sociology literature considering the outcomes of children with incarcerated parents as well as changes in infant mortality (see e.g., Hagan and Foster 2012; Lee, Fang, and Luo 2013; Wildeman 2010a; and Wildeman 2010b).

This paper will add to the growing body of existing research on the effects of mass incarceration, but will focus on the outcomes of women. To my knowledge, there are very few studies, especially in the field of economics, that consider the effect of higher incarceration rates on women's outcomes. One exception is Charles and Luoh (2010). This study uses decennial Census data for 1970–2000 and a variety of estimation strategies (including an instrumental variable approach that uses a measure of drug caseloads in 1990 as an instrument) to estimate the effect of incarceration on women's marriage and labor market outcomes. The authors find that higher male imprisonment appears to have lowered the likelihood that women marry and reduced the quality of their spouses when they do marry. The authors also argue that women in affected marriage markets seem to have

increased their schooling and labor supply in response to incarceration.

This paper builds on Charles and Luoh (2010), employing annual data for roughly the same time period. Specifically, I am able to use annual state-level policy variation throughout this time period—as opposed to from only one year—as instrumental variables. This paper also extends the analysis to consider not only marriage and labor market outcomes, but also family formation outcomes such as the birth rate and the share of births that are out of wedlock. Finally, unlike previous studies, this paper focuses on estimating the effect of higher incarceration rates on the outcomes of black women ages 18–40, which is precisely the demographic group of women for whom I would expect to see the biggest effects.

Overall, the main advantage of this paper is the richness of the state-level data documenting changes in sentencing laws for more than twenty-five years. This paper carefully considers the relevance and validity of the fourteen sentencing laws, and selects two policies that have predictive power and whose timing of adoption seems relatively random so that they likely satisfy instruments' exclusion restriction. Using this data and methodology, this paper puts forth additional evidence regarding the causal relationship between higher incarceration rates and women's outcomes.

Theoretical Predictions

Becker (1973)'s model of marriage markets provides a good foundation for thinking about the relationship between incarceration and family formation outcomes. Mass incarceration reduces the relative number of men in communities (or alternatively, in a marriage market). This scarcity of men should in theory result in a lower marriage rate overall for women. Nevertheless, mass incarceration does not only alter the number of men in a community, but it also potentially changes the “quality” of these men and the return to marriage. For example, if ex-offenders face scarring or if black men generally face stigma in the labor market as a result of mass incarceration, then women might be hesitant to marry these men.⁴ If so, then I might expect higher incarceration rates to reduce the marriage rate of women in the longer term beyond the simple “scarcity” story.

⁴ It is important to note that many men in this time period cycled in and out of prison, so that many men in these communities likely had criminal history records. As an example, prisoners released in 1996 served on average 30 months in prison (Ditton and Wilson 1999).

Aside from the marriage rate, I might expect to see other trends in the marriage market. Given the number of men incarcerated during this time period, I would predict that more women that have ever been married will report that they are no longer living with their spouse, or that they are now separated, divorced, or widowed. However, it is also possible that I might see this share decrease in the longer term; if fewer women marry at all, then the ones who do get married might be more selective and have more stable marriages (Charles and Luoh 2010). With this ambiguous prediction in mind, this paper explores whether and how higher incarceration rates affect the share of women who report having been “married before.”

The scarcity of men as well as changes in their “quality” as a result of mass incarceration might have had an impact on other family formation patterns. On the one hand, I might expect an increase in the share of mothers who are single and a rise in the share of births that are out of wedlock.⁵ On the other hand, certain women might decide that the costs to having a child are now greater or more uncertain, so I might expect to see the birth rate of black women decrease. Moreover, with regard to the health of babies, higher incarceration rates might leave women in more precarious positions (e.g., economic, emotional), which might cause them stress and therefore increase the share of low birthweight births.

Finally, the reduction in the number of marriageable men as result of mass incarceration likely had other implications for women beyond family formation. For example, if fewer women married or if the mass incarceration of men left women in more precarious economic positions, then they might have sought employment as a way to achieve financial independence or security. This paper therefore considers employment and labor force participation as outcomes of interest. However, barriers to employment—such as household responsibilities or inadequate preparation—might have prevented women from seeking employment and simply kept them in these more precarious economic positions. This paper therefore considers a woman’s poverty status as an outcome.

Bearing these predictions in mind, I now turn to the instruments and outcome data before estimating the causal effect of incarceration on women’s outcomes.

⁵ One could tell a number of stories that would explain these increases, such as non-incarcerated men having more bargaining power because there are fewer of them or women still being interested in having children regardless of whether a father can be present.

3 Data

3.1 Sentencing Reform Data

All fifty states adopted or reformed their sentencing laws between 1970 and 2002. Popular reforms included adopting habitual offender laws, mandatory minimums, and sentence enhancements, among others. The wide array of laws and the disparate way in which they were implemented in each state make it difficult to cleanly classify policy changes over time. However, it is possible to pinpoint certain policies that were adopted in a large number of states. This paper considers fourteen policies that were popular in a large number of states. (The fourteen policies are described in detail in Appendix A.)

Stemen (2007) places policies under different categories based on which factors determined whether and when a state adopted a particular policy. For example, he argues that the adoption of “developmental” policies (like sentencing commissions and determinate sentencing) likely required substantial resources from the state, such as financial and staffing resources as well as political cooperation. These policies therefore took a longer time frame to construct and implement, and few states adopted a particular policy at any given time. In contrast, Stemen argues that “expressive” policies (like sentencing enhancements and mandatory minimums) had rapid diffusion patterns, with many states adopting a given policy in the same year. Although these policies seem like a tool for appearing tough on crime or expressing moral outrage, Stemen (2007) does not find any consistent predictors for their adoption. Finally, Stemen discusses “responsive” policies—such as parole and probation supervision fees—which were often implemented as a response to fiscal strains from increasing corrections expenditures or declining state revenue.

Data on the year that these laws were adopted or reformed comes from several sources: data on structured and determinate sentencing come from Stemen and Rengifo (2012); truth-in-sentencing laws come from GAO (1998); data on sentencing commissions, community corrections acts (CCAs), mandatory sentencing laws for offenses involving a weapon, sentence enhancements for offenses committed against a law enforcement official, sentence enhancements for sale or possession of drugs near a school, Megan’s Law, parole fees, and probation fees all come from Stemen (2007);

and the three habitual offender laws come from Stemen et al. (2006).⁶ The final dataset I use documents the policy changes for every state and every year between 1970 and 2002.⁷

Table 1 shows the share of states that had each of the fourteen policies in 1977 and in 2002.⁸ For a more gradual perspective, Appendix A includes figures that show the number of states that had each policy in each year between 1970 and 2002. Looking at this table and the figures, it is clear that some policies, such as determinate sentencing and sentencing commissions, were gradually adopted over time. The adoption of other policies, such as the weapons mandatory, was condensed into shorter time frames or specific years.

3.2 Incarceration and Outcome Data

Incarceration data come from the National Prisoner Statistics (United States Department of Justice 2014). This data is assembled by the Census Bureau and provides state-level counts of individuals in state prisons on December 31st of each year.⁹ Population counts come from the Survey of Epidemiology and End Results (National Bureau of Economic Research [NBER] 2007). State prison and population counts are used to construct a panel of state-level incarceration rates from 1978–2003.¹⁰

Data on family formation outcomes come from two sources: Vital Statistics data as well as the Current Population Survey (CPS). The advantage of the Vital Statistics data relative to the CPS is that it contains the full universe of births to black women; as such, the estimates that use this data are much more precisely estimated than those estimated using the CPS. The advantage of the CPS

⁶ I am very grateful to Liam Clegg for pointing me to many of these data sources. I am also grateful to Don Stemen for providing me with access to the original data files, which I used to supplement and verify the timing of law changes. Stemen et al. (2006) indicated the three-year window in which the habitual offender laws were passed, so I used the original data collection files to find the exact timing of the law change.

⁷ The exceptions are the habitual offender laws; data on these three policy changes only begin in 1975.

⁸ I choose 1977 since the data on incarceration rates begins in 1978 and since the policies are lagged one year in all of the paper's regressions.

⁹ Specifically, I use the count of inmates under the jurisdiction or legal authority of state correctional officials. This measure counts inmates beyond those in the custody of state officials in order to account for inmates housed in local jails, privately operated facilities, confinement facilities outside of that jurisdiction, etc.

¹⁰ Since the policy data collection stopped in 2002 and the policies are always lagged one year, 2003 is the final year in this paper's sample.

is that it includes information on all black women, not just mothers, and that it contains various other outcomes of interest, such as detailed marital and poverty status.

To be more precise, data on the number of births to black women as well as the share of births that are out of wedlock and that are low birthweight come from the Vital Statistics data (NBER 2017).¹¹ The count of births as well as the population counts mentioned above are used to construct the birth rate of black women (i.e., the number of births to black women per 1,000 black individuals) for each state and year. The remaining family formation outcomes come from the March Supplement of the CPS (Flood et al. 2015). I use this data to calculate the share of black women in that state and year who are single mothers (conditional on being a mother), who are married, and who have been married before (conditional on having ever been married). All of the labor market outcomes also come from the CPS data. I calculate the share of black women who are employed, who are in the labor force, and who are living in poverty.¹²

Since mass incarceration disproportionately affected black communities, this paper focuses on women identified as black in the CPS or in the Vital Statistics data. The CPS sample is also restricted to women ages 18–40. Since a vast majority of criminal offenders are under the age of 40 (Kearney et al. 2014), I expect mass incarceration to have the biggest effect on the outcomes of women in this age range.

Finally, when exploring the validity of instruments, this paper uses state-level panel data from University of Kentucky Center for Poverty Research (2016). Violent and property crime rates come from the FBI’s Uniform Crime Reporting Statistics (U.S. Department of Justice 2017).

¹¹ Before 1985, the Vital Statistics data was not the full universe of births in some states. Many states reported 100 percent of the births, but others only processed 50 percent of births for the data. I weight observations accordingly. Moreover, data on out-of-wedlock births is not reliable for every state before 1980; I treat states that do not systematically record this variable as missing for years before 1980.

¹² A woman is “married” if she is married at the time of the survey and her spouse is present. A woman is classified as “married before” if either she is married but the spouse is absent or if she is divorced, separated, or widowed. When calculating the share of women who have been married before, the denominator is the number of women who have ever been married. A woman is classified as a single mother if she is a mother and she has either never been married or has been married before. When calculating the share of single mothers, the denominator is the number of women who are mothers. A woman is employed if she is at work or has a job but did not work last week. A woman is in the labor force if she is classified as being in the labor force. A woman is in poverty if her total family income falls below the federal poverty level.

4 Methodological Framework

I follow the same approach as previous papers that have estimated the effect of incarceration using state-level panel data regressions (see e.g., Johnson and Raphael 2012, Levitt 1996, Marvell and Moody 1994). Instead of considering crime as an outcome, however, I consider primarily family formation outcomes, and then labor market outcomes.

Estimating the effect of incarceration using ordinary least squares (OLS) estimates is not an appropriate approach since the estimation would likely suffer from endogeneity bias. A simple example to illustrate this point is that there is likely a simultaneous relationship between the incarceration rate and the employment rate. If a state is undergoing economic hardship, more individuals might turn to crime and thereby contribute to the incarceration rate. This paper therefore uses state-level policy changes as instruments for the incarceration rate. The underlying assumption of this empirical strategy is that sentencing reform is closely linked to the incarceration rate, but that the timing of the adoption of these laws is exogenous to other state-level interventions or dynamics that might affect individual outcomes.

Using a subset of policies as instrumental variables for the incarceration rate, the principal estimates for the effect of higher incarceration rates on women's outcomes come from a two-stage least squares (2SLS) estimation using the following equations:

$$\ln(IncRate_{s,t}) = \alpha + \pi Policies_{s,t-1} + \kappa_s + \lambda_t + \mu_{st} + \eta_{s,t} \quad (1)$$

$$Y_{s,t} = \alpha + \beta \ln(IncRate_{s,t}) + \delta_s + \gamma_t + \omega_{st} + \varepsilon_{s,t} \quad (2)$$

where s and t index states and years, respectively. $Y_{s,t}$ is the family formation or labor market outcome of interest and $\ln(IncRate_{s,t})$ refers to the logged incarceration rate in that state and year. The model uses a one-period lag in the first-stage regression to allow time for the incarceration rates to respond to these policy changes. Both models include state and year fixed effects to account for other state-level or annual factors that influence the outcome of interest. The specifications also include state-specific linear time trends to account for factors that change smoothly over time within a state. Importantly, since the incarceration rate increased in every state during the sample

period, including state-specific linear time trends takes these long-run trends into account and coefficients are estimated using deviations from the trends. All standard errors are clustered at the state level.

I now explore in more detail which of the fourteen sentencing laws are closely related to changes in the incarceration rate (i.e., the existence of a strong first stage) and will argue—through event studies and a number of exercises—that a subset of these policies are likely exogenous to the outcomes of interest and therefore can serve as valid instruments.

4.1 Instrument Relevance

Although I have data on fourteen sentencing policies, it is not obvious that all of them will have a strong relationship to the incarceration rate. To explore this first-stage relationship, I use event studies where the event is the adoption of a law. Specifically, I estimate the following equation:

$$\ln(IncRate_{st}) = \sum_{k=-5}^5 \theta_k D_{st}^k + \kappa_s + \lambda_t + \mu_{st} + \eta_{st} \quad (3)$$

where D_{st}^k is a dummy indicating that the law was adopted k periods ago.¹³ The estimate of θ_k can be plotted over time and provide estimates of mean incarceration rates in “event time” after taking into account state and year fixed effects as well as state-specific linear time trends.

Figure 3 shows the event study estimates for the four policies that seem to have the strongest relationship with the incarceration rate, both in terms of the first-stage F-statistic as well as the rise or fall in the incarceration rate following the adoption of the law.¹⁴ (Appendix B includes the event studies for all fourteen policies.) Since eleven states abolished their sentencing commissions, I also estimate equation (3) where the event is the abolition of the sentencing commission. These event studies illustrate that these four policies have a strong relationship with the incarceration rate and would thus be relevant instruments to use.

¹³ The endpoints are binned at five years before and after the adoption of the law.

¹⁴ I confirm the choice of these four policies using a Lasso procedure for selecting optimal instruments (Belloni et al. 2012). The procedure selects these four policies as being the best predictors of the incarceration rate.

The positive associations between the incarceration rate and the weapons mandatory as well as the habitual offender law (HOL) for violent offenses are consistent with the punitive nature of these policies. The weapons mandatory requires a term of incarceration for some offense (or group of offenses) if it is committed while armed with or by use of a deadly weapon. It is thus not surprising to see a rise in the incarceration rate after the adoption of this policy. Similarly, habitual offender laws are those that increase penalties or alter release decisions or time served requirements for offenders with prior felony convictions. This specific habitual offender law is intended to more heavily punish repeat violent offenders. The rise in the incarceration rate after the adoption of this law is therefore also not surprising.¹⁵

Unlike these two reforms, determinate sentencing and sentencing commissions seem to be *negatively* associated with the incarceration rate; alternatively, failing to adopt these policies seems to raise the incarceration rate. Determinate sentencing eliminates the discretion of parole boards in release decisions and guarantees that the time served by offenders is primarily determined by the sentence imposed. Although the negative relationship between this policy and incarceration rates might seem initially puzzling, this association has been well documented in the literature. Regardless of the motive for adopting determinate sentencing, this policy places constraints on judges and parole boards by reducing discretion. It therefore seems to have stabilized sentences and time served, ultimately keeping the growth in incarceration rates in states with the policy below that of states without the policy.¹⁶ As for sentencing commissions, these independent agencies are in charge of evaluating existing laws and proposing reforms. During this time period, they were also often tasked with reducing prison populations as a way to curb corrections expenditures (Stemen 2007). I therefore expect the incarceration rate to be negatively associated with the adoption of sentencing commissions. What is perhaps more surprising is the strength of the positive relationship between the repeal of a sentencing commission and the incarceration rate; the incarceration rate seems to grow quickly following the abolition of a commission.¹⁷

¹⁵ It is interesting to note that the two policies that have a strong positive association with the incarceration rate are those related to violent, not drug, offenders, which is in line with Pfaff (2017)'s recent conclusions.

¹⁶ I refer the reader to Stemen and Rengifo (2011) for more detail.

¹⁷ The underlying mechanism behind this relationship is not clear from these event studies. However, the results seem to suggest that the mere presence of a sentencing commission is important for maintaining stable incarceration rates.

4.2 Instrument Validity

Now that the relevant instruments have been chosen, I explore their validity through additional event studies and other exercises. I want the instruments to have a strong relationship with the incarceration rate, but do not want them to be related to any other state-level policies or dynamics that might affect the outcomes of interest. In essence, I want the timing of the adoption of these laws to be relatively random.

One important threat to the validity of these instruments would be that states adopted these policies following a sudden upsurge in crime. To explore the trends in crime before the adoption (or repeal) of these policies, I run the same event studies as in equation (3), but with logged violent crime rates as the dependent variable. Figure 4 plots the resulting coefficients. (Appendix C includes analogous event studies using property crime rates.) There do not seem to be any sudden increases in crime rates in the years prior to the adoption of these policies that might have motivated their passage. The only perhaps problematic policy is the HOL for violent offenses, which seems to exhibit an increasing pre-trend in violent crime prior to its adoption. This pre-trend in violent crime is particularly concerning since the policy is intended to more heavily punish violent offenders; the timing of adoption therefore does not seem relatively random.

To further probe at this idea, I now consider whether any lagged state-level variables or outcomes can predict the adoption of these policies. If the timing of their adoption (or repeal) was indeed somewhat random, then these variables should not be able to predict their passage. Table 2 displays the results from linear probability regressions of the policy variable on a number of lagged state-level variables that I might be concerned about when considering endogeneity, state and year fixed effects, as well as state-specific linear time trends. On the one hand, it is clear from the coefficients in columns (1) and (4) in this table—as well as the F-statistic and p-value from the corresponding joint test on the regressors—that none of the lagged variables are related to the adoption of determinate sentencing or the weapons mandatory. In contrast, column (2) has a statistically significant coefficient for lagged violent crime rates. Even though the p-value from

In other words, adopting a sentencing commission for a few years does not seem to be sufficient for controlling future growth in incarceration rates.

the joint test on the regressors is relatively large, the statistically significant coefficient for violent crime in conjunction with the pre-trend in violent crime from the event study above makes us think that this policy might have been related to crime levels and thus might not be an ideal instrument. As for sentencing commissions, column (3) shows that the one-period lagged unemployment rate as well as gross state product per capita seem to be strongly related to the passage of this policy. Importantly, the p-value from the joint test on the regressors is quite low, at 0.07.¹⁸ Based on these results, I conclude that sentencing commissions are perhaps also related to other state-level forces that could threaten its validity as an instrument. To be safe, the remainder of the paper only considers the weapons mandatory and determinate sentencing as valid instruments.

Appendix C includes more event studies and exercises to further probe at the validity of these instruments. The bottom line remains unchanged: the weapons mandatory and determinate sentencing do not seem to be strongly or consistently related to state- or individual-level factors that might influence the outcomes of interest. These policies thus likely satisfy the exclusion restriction for instrumental variables.

5 Descriptive Statistics and Results

So far, this paper has discussed the relevance and validity of two instruments: determinate sentencing and the weapons mandatory law. I will now consider these policies slightly more in-depth as well as the trends in outcomes for black women ages 18–40. I will then use equation (2) to estimate the effect of incarceration on the outcomes of interest.

5.1 Descriptive Statistics

To better understand the adoption patterns of determinate sentencing and the weapons mandatory, the maps in figures 5 and 6 show which states adopted each of the two policies. The color scheme in the maps also illustrates the differences in the timing of adoption. The first map shows that

¹⁸ Intuitively, these associations are somewhat confusing since lower unemployment rates and lower economic growth (variables that typically move in the opposite direction) both seem to predict the passage of this law. However, the low p-value from the joint test suggests that I can reject the null hypothesis that the regressors are all zero at the 10 percent significance level.

the weapons mandatory was the earlier and more pervasive of the two policies, with more than 40 states adopting this law between 1970 and 1990. In contrast, as noted in the discussion above, adopting determinate sentencing likely required substantial resources and a longer time frame to construct and implement. The corresponding map highlights the more gradual adoption pattern, with a few states adopting determinate sentencing even in the late 1990s and early 2000s.¹⁹

Table 4 shows the mean and standard deviation for the incarceration rate as well as for all of the women's outcomes that this paper is considering. The table compares the mean and standard deviation of these outcomes in 1978 (the first year for which incarceration data are available) with the same summary statistics in 2003 (the last year in this paper's sample). As shown in figure 1, the incarceration rate rose dramatically during this time period.

With regard to the primary outcomes of interest—family formation outcomes—the table shows that the birth rate of black women declined between 1978 and 2003. Moreover, the share of births that are out of wedlock rose from 47 to 62 percent during this time period and the share of births that are classified as low birthweight did not change. Interestingly, these statistics suggest that despite the lower birth rate, the children who were born were not necessarily any healthier in terms of birth weight or more likely to be born into two-parent homes. This table also illustrates a trend that has been well documented in the literature: the share of black mothers who are single increased during this time period from roughly half of mothers to 62 percent. As for marriage market outcomes, this table shows that a lower share of black women in this age group were married in 2003 relative to 1978.²⁰ Of women who have ever been married, I see the share who report having been married *before* decrease, although the difference in means is not statistically different from zero.

Finally, in terms of labor market trends, I see an increasing share of black women working and

¹⁹ A quick look at these maps might suggest that neighboring states often adopted these policies in conjunction. However, regressions of the policy variable on a one-period lagged dummy variable equal to one when any of the neighboring states had the same policy (as well as state and year fixed effects and state-specific linear time trends) yields statistically insignificant coefficients on the neighbor dummy for both policies.

²⁰ The share of women who have ever been married declines from 0.60 to 0.44 during this time period. I thus can conclude that more black women in this age group are simply never getting married. A decline in the age of marriage likely cannot explain these trends since most women still get married before age 40. However, as a simple check, I consider black women ages 30–40; I see the share fall from 0.50 to 0.39, suggesting that the decline is likely explained by decisions on the extensive margin regarding marriage and not by changes in age of marriage.

joining the labor force between 1978 and 2003. Correspondingly, the table shows a slightly lower poverty rate for black women in 2003 than in 1978.

Appendix D includes a similar table but for white women as a point of comparison.²¹ Now that I have noted the trends in outcomes for black women across time, I turn to the main analysis and use equation (2) to estimate the effect of mass incarceration on these outcomes. I employ the two sentencing laws—the weapons mandatory and determinate sentencing—as instruments in a two-stage least squares approach.

5.2 Results

5.2.1 Family Formation

First, I consider the family formation outcomes and present the results in table 5.²² I notice right away that none of the coefficients in this table are statistically significant. However, the first three columns use the full universe of births to black women from the Vital Statistics and are thus much more precisely estimated. Based on these results, I conclude that there is no indication that higher incarceration rates altered birth rates, the share of out-of-wedlock births, or the share of low birthweight births among black women. Specifically, I can rule out, with 95 percent confidence, a decline of 0.64 percent in the birth rate from a one percent increase in the incarceration rate. I can also reject with 95 percent confidence an increase of 0.11 percentage points (on a base of 58 percent) and an increase of 0.05 percentage points (on a base of 12 percent) in the share of black births that are out of wedlock and that are low birthweight, respectively, from a one percent increase in the incarceration rate. Event studies of the reduced form relationship between these three outcomes and the policies, shown in figures 7–9, highlight and confirm these precisely estimated null results.

²¹ The overall patterns for white women are similar, albeit typically with very different baseline magnitudes. The only notable differences in trends are that white women are more likely to be married before and to be living in poverty in 2003 than in 1978.

²² For completeness, the results from the standard OLS regressions are shown in Appendix D. I notice that many of the OLS coefficients are different in sign and magnitude from the corresponding 2SLS ones, which further justifies the instrumental variables approach.

The coefficients estimated with the CPS, shown in columns (4) through (6), are also statistically insignificant, but since they are imprecisely estimated—with much larger standard errors—I do not draw concrete conclusions from these results. The lack of findings for the marriage outcomes does not necessarily counter the findings of previous studies, especially since samples and specifications differ across papers and since the marriage coefficients of this paper are relatively imprecise. In particular, if I use a more similar specification to Charles and Luoh (2010) and re-estimate columns (5) and (6), I notice that all of their estimates fall within the estimated 95 percent confidence intervals.²³

It is also worth noting that even though none of the coefficients are statistically significant in this table, the signs of the coefficients are consistent with the theoretical predictions: I see that higher incarceration rates seem to have a negative relationship with the marriage and birth rates as well as a positive relationship with the share of single mothers, the share of out-of-wedlock births, and the share of low birthweight babies. The signs of these coefficients are thus all in line with what I would predict for women’s family formation outcomes when there are fewer men in their communities or when the returns to marriage decline.

To probe the above results, I see whether any of these results are driven by women who are households heads. I acknowledge that there is a selection question when restricting the sample in this manner since I am using a repeated cross section; namely, the household heads that I observe in one year are not necessarily the same ones I observe in other years, and higher incarceration rates might alter who becomes a household head. Indeed, during this time period, the share of black women in this age group who were classified as household heads rose from 35 percent to 52

²³ Instead of the logged incarceration rate, Charles and Luoh (2010) assumes a linear model and uses the proportion of men that are incarcerated in a marriage market as the independent variable. To compare effect sizes, I re-estimate columns (5) and (6) using a similar specification to them: I use the proportion of men who are incarcerated in that state and year as the right-hand side variable and I weight the regression by the state’s black population in that year. The coefficients I find are bigger in magnitude (perhaps suggesting bigger effects for black women) but imprecisely estimated, so that the coefficients from their paper fall within the estimated 95 percent confidence intervals. A couple of notable differences are that their paper considers the share of women who are divorced, as opposed to previously married, and that their divorce coefficients are negative, while the coefficients in this paper for “married before” are typically positive. Finally, if I do the same exercise for the share of out-of-wedlock births, I find that my estimate is still a null result, albeit similar in magnitude to their estimate for the share of women who have a child out of wedlock.

percent.²⁴ With this caveat in mind, I now re-estimate equation (2) but only for black women ages 18–40 who are classified as a household head (roughly 45 percent of the original sample).

Table 6 presents the results for the three family formation outcomes that come from the CPS.²⁵ Despite bigger magnitudes, the estimates in the first two columns remain statistically insignificant and imprecisely estimated. Yet, I would not necessarily expect to see an effect in these columns; women who are identified as household heads are not typically married to begin with, so I would not expect to see big changes in marital status among this group as a result of higher incarceration rates.²⁶ I do see, however, a marginally significant effect when looking at the share of female household heads who report having been married before. Specifically, a one percent increase in the incarceration rate results in a 0.52 percentage point increase (on a base of 73 percent) in the share of female household heads who report that they are no longer married. This result is consistent with the notion that higher incarceration rates disrupt not only the matching process in the marriage market, but also marriages themselves, so that a higher share of female household heads report being separated or divorced.

5.2.2 Labor Market

Table 7 considers the labor market outcomes of black women. The signs and statistical significance of the coefficients are consistent with the idea that higher incarceration rates incentivize women to seek employment and join the labor force as a way to achieve economic security or independence. The results in columns (1) and (2) suggest that a one percent increase in the incarceration rate results in a 0.30 percentage point increase from a base employment rate of 61 percent and in a 0.28 percentage point increase from a base labor force participation rate of 72 percent.²⁷ Even though the coefficient on the poverty variable is not statistically significant, the negative sign is consistent

²⁴ A 2SLS regression of the share of black women who were household heads on the incarceration rate yields a statistically insignificant coefficient.

²⁵ I cannot do this type of analysis using the Vital Statistics data since it does not specify household head status.

²⁶ To be precise, roughly 87 percent of the female household heads in this sample are not married.

²⁷ Similarly to above, I re-estimate columns (1) and (2) using a similar specification to Charles and Luoh (2010). In both cases, I find estimates that are larger in magnitude than the coefficients from their paper and I see that their relatively small estimates fall within the estimated 95 percent confidence intervals.

with the idea that joining the labor force and working might have lowered the share of women living in poverty.

To put these results into perspective, the mean and standard deviation of the incarceration rate across states during the sample period are 270.9 and 158.4, respectively. A one standard deviation increase in the incarceration rate from the mean would imply a 58 percent increase in the incarceration rate, which would then imply a 17.4 and 16.2 percentage point increase in the employment rate and labor force participation rate, respectively. Nevertheless, since the specification is not linear and uses logged incarceration rates, these estimates are certainly upper bounds on the effects.

Finally, the results for the labor market outcomes of female household heads are shown in the last three columns of this same table. The magnitude and statistical significance of the estimates suggest the previous results were driven by household heads. With the same caveat in mind about selection, I conclude that the results in this table suggest that higher incarceration rates increased the share of black female household heads who were employed and in the labor force.

6 Robustness Checks

6.1 Permutation Tests

I now consider the first stage specification and ask whether I would find the same results if instead of using the actual timing of sentencing reform, I randomize the timing of the law changes in all states. Specifically, I run a simulation in which, for any given iteration, I randomly assign the year in which the policy changes for each state (i.e., I randomize the year in which the policy dummy turns from 0 to 1). Since the weapons mandatory policy changes occurred early in the sample period, I restrict the years I assign to the time frame in which this law was passed: 1977 through 1986. Similarly, I restrict the possible years for determinate sentencing to 1977–2000. I then estimate the first-stage regression in equation (1) using the simulated policy and store the π coefficient.

Figure 24 in Appendix D presents a histogram of the simulated π coefficient for 1000 draws. The dashed red line indicates the coefficient from the first-stage regression using the actual timing of

sentencing reform. For both policies, the true coefficient is larger than any of the coefficients produced by the 1000 iterations. These permutation tests show that the actual first stage result is extremely unlikely under random assignment of reform dates and thus provides support for the first-stage specification and results.

6.2 Robustness to Instruments and Specification

In order to check the sensitivity of the estimates, I estimate the main results using the two other policies that I initially chose as being predictive of the incarceration rate, but that I did not end up using because of potential violations to the exclusion restriction: the HOL for violent offenses and sentencing commissions. Including these two policies as instruments increases the predictive power in the first stage, but at the expense of potentially introducing bias into the estimation.

I first incorporate the HOL for violent offenses into the analysis, since this seemed like the less problematic instrument of the two based on the results from table 2. The results are shown in table 13 of Appendix D. The estimates are generally similar in magnitude and in statistical significance; the only notable change is that the coefficient on the birth rate changed signs. Table 14 then introduces sentencing commissions as a fourth instrument. Even though they remain statistically insignificant, the signs on many of the family formation estimates flip once I include this fourth instrument. In contrast, the coefficients on the labor market outcomes are relatively stable.

Overall, the results from these two tables highlight the relative stability of the null results that use the Vital Statistics data as well as of the employment and labor force coefficients. However, they also illustrate the potential bias that can enter the analysis if certain policies are included as instruments.

I perform two additional robustness checks to test the sensitivity of estimates to the specification chosen; the results are shown in tables 15 and 16 of Appendix D. First, I consider the logged *male* incarceration rate as the independent variable. I do not expect the results to change significantly since the total incarceration rate is driven by the incarceration of men, and indeed the coefficients hardly change. Finally, I consider a linear specification in which the independent variable is the untransformed (i.e., not logged) state incarceration rate; despite the lower F-statistics in these models, the main takeaways regarding the null results from the Vital Statistics data as well as the

employment and labor force estimates are unchanged.²⁸

6.3 Robustness to Weights

Up until this point, all of the regressions have been unweighted, so that each state is treated as an equally important entity for understanding the effects of mass incarceration. The question of whether this analysis should use weights does not have a straightforward answer. Looking at related literature, a number of previous papers that have used state-level panels to estimate the effect of incarceration on outcomes like crime rates—like Marvell and Moody (1994) and Levitt (1996)—do not weight their regressions. Other more recent papers—like Johnson and Raphael (2012) and Liedka et al. (2006)—use state population counts as weights. In some of the papers in the literature, weighting regressions is the logical approach given how the authors measured the right-hand side variable. For example, Charles and Luoh (2010) as well as Raphael and Stoll (2013a), use Census data to construct incarceration rates. It is therefore reasonable that these papers assign higher weight to the incarceration rates that are calculated with higher precision.²⁹ However, for papers like this one where the right-hand side variable is constructed using administrative data, the decision of whether to weight is not as obvious.

In terms of interpretation, state-level panel regressions without weights give equal weight to each observation, so that each state is in a way being treated as an equally valid “experiment.” In other words, preferring the unweighted estimates implies that I care about the effect of incarceration in a state like Delaware, which has fewer black individuals, as much as the effect of incarceration in a state like Louisiana, which has a large black population. On the other hand, as Johnson and Raphael (2012) note, weighting states by their population better reflects the typical effect experienced by the average black woman.

Since both approaches are common in the literature and reasonably interesting interpretations, I run the same regressions as in the main analysis, but this time weighting by the state’s black

²⁸ Appendix E contains an additional robustness check of the labor market results using white women as a potential comparison group.

²⁹ In both of the papers referenced, the regressions are weighted by the number of observations used to compute the incarceration rate.

population in that year. The results, shown in table 17 in Appendix D, illustrate that the results, both in terms of magnitude and statistical significance, are sensitive to weighting. For the family formation outcomes, the results are still statistically insignificant, but many of them are different in sign than the corresponding unweighted estimates. For the labor market outcomes, all of the coefficients carry the same sign as the corresponding unweighted ones, but are much smaller in magnitude. However, the first stage F-statistics from these models are significantly smaller than in the unweighted ones and usually below the desired threshold of 10, so I worry that these estimates are more likely to be biased.

Estimating these models with weights also highlights the fact that the standard errors from the unweighted models that use CPS data are consistently larger than the standard errors from the corresponding weighted ones. Given the greater imprecision of the unweighted models, I would have expected to find fewer statistically significant results in those regressions. However, the unweighted models typically had coefficients of bigger magnitudes, so that all of the statistically significant results in this paper (i.e., the employment and labor force participation rate coefficients) came from the unweighted specifications.

The larger magnitudes in the unweighted models relative to the corresponding weighted ones seem to suggest that the effect of incarceration on labor market outcomes of black women is stronger in states that have smaller black populations.³⁰ The mechanism behind this result is not clear. However, one possible explanation could be that the effect of a shock—in this case, the incarceration of many black men—is magnified for black women or communities when they are small minority group.³¹ For example, black men in heavily white states might face even stronger stigmatization or discrimination in the labor market after the mass incarceration of their demographic group, so that black women have an even greater incentive to join the labor force and seek employment.

³⁰ This difference is not present if I compare weighted and unweighted estimates for white women. I therefore conclude that the larger magnitudes are not intrinsic to unweighted models, but reflect a heterogeneous effect of incarceration on the outcomes of black women across different states

³¹ Since the weights are a state's black population in that year, the concept of being a minority here is absolute, not relative. However, for many states that have fewer black individuals in absolute terms, the share of black people will also likely be smaller in relative terms.

7 Conclusion

This paper adds to the growing understanding of how mass incarceration affected the outcomes of women. Unlike previous studies, this paper focuses on estimating the effects of higher incarceration rates on black women since incarceration disproportionately affected black communities. The main advantage of this paper is the state-level annual data documenting changes in sentencing policies between 1970 and 2002. I first consider the strength of the first stage and look at event study plots of the incarceration rate relative to the adoption of the policies; I find that four policies can serve as relevant instruments. Through a series of additional event studies and exercises, I then conclude that two of the policies are likely valid instruments: determinate sentencing and mandatory sentencing laws for offenses involving a weapon (i.e., weapons mandatory). The rest of the paper then uses these two instruments in a two-stage least squares approach to estimate the effect of higher incarceration rates on various women's outcomes, the primary focus being family formation outcomes and the secondary being labor market outcomes.

Since the rise in incarceration rates coincided in timing with a number of other trends (e.g., single motherhood, inequality), it is often easy to conflate these phenomena or simply assume that they must be related. However, many other forces—whether institutional or economic—were at work during this time period, so it is important to understand if and how these trends are related. The instrumental variable approach of this paper allows us to account for endogeneity and thus isolate the causal effect of higher incarceration rates on various outcomes.

One key take away from this paper is that higher incarceration rates do not seem to have an effect on the birth outcomes of black women, namely birth rates, the share of births out of wedlock, and the share of low birthweight babies. The coefficients for these outcomes are estimated using the universe of black births from Vital Statistics data and are thus precisely estimated. I thus conclude that there is no indication that mass incarceration affected any of these outcomes.

The results for the labor market outcomes substantiate the findings of previous studies, providing some, albeit weaker, evidence that higher incarceration rates increase the employment and labor force participation rates of black women. The results are driven by women who are household heads and suggest that higher incarceration rates increase the share of black female household

heads who are employed and in the labor force. These results are consistent with the idea that the mass incarceration of black men incentivizes black women to join the labor force or seek employment as a way to increase their financial independence or security.

Beyond the results, this paper and its data sources also provide us with important lessons. First, many of the sentencing policies are not relevant instruments, having a relatively weak relationship to the incarceration rate. Of the policies that have stronger relationships, only a subset then seem likely to satisfy the exclusion restriction. Future research could utilize these instruments to continue exploring the effect of higher incarceration rates on other outcomes. Second, it is not surprising that the results from the Vital Statistics data are much more precisely estimated than the coefficients using the CPS. However, this difference illustrates that standard survey data like the CPS is not sufficient to estimate effects on minority groups like black women. Future research should thus seek out administrative data to further explore the effect of incarceration on the outcomes of minority individuals. Importantly, once we gain a better understanding of which outcomes are affected, future research should focus on pinning down the mechanisms behind these effects, so that policymakers can better help women who are left in precarious positions as a result of changes in the criminal justice system.

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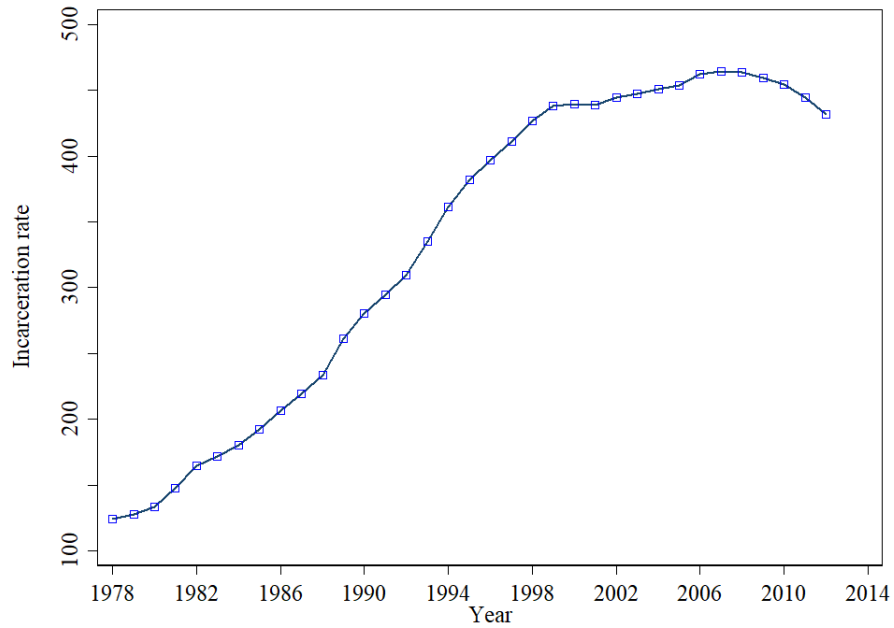
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9 Figures and Tables

Figure 1: State Incarceration Rate, 1978–2012



Note: This figure uses total prison counts from the National Prisoner Statistics and population counts for each state and year using NBER (2007).

Figure 2: Incarceration Rate by State, 2003

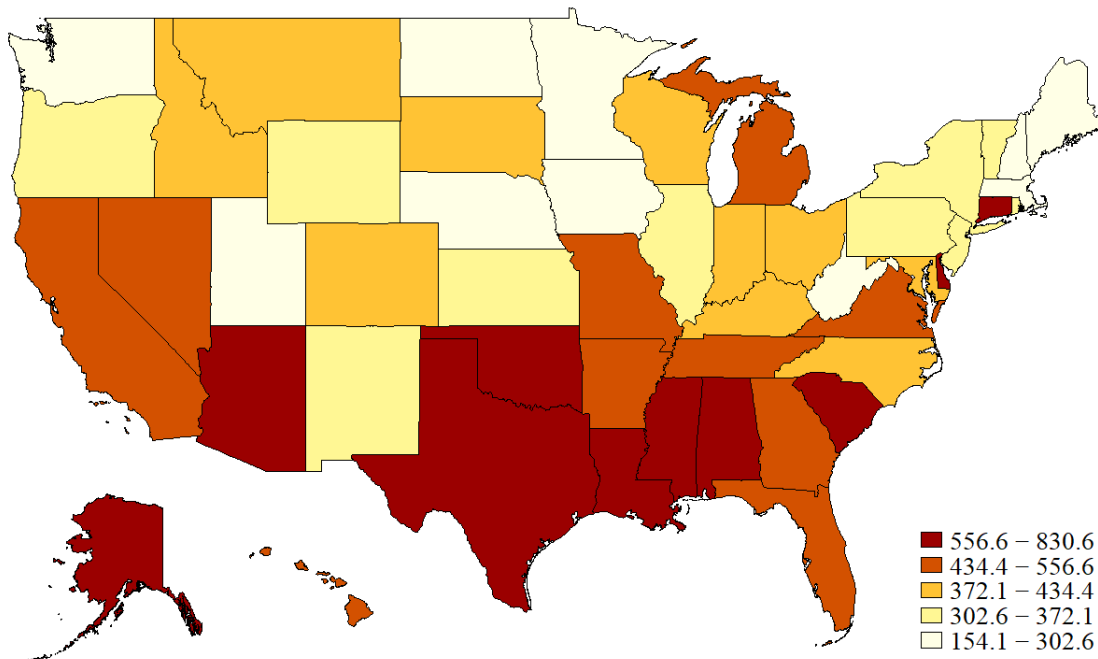
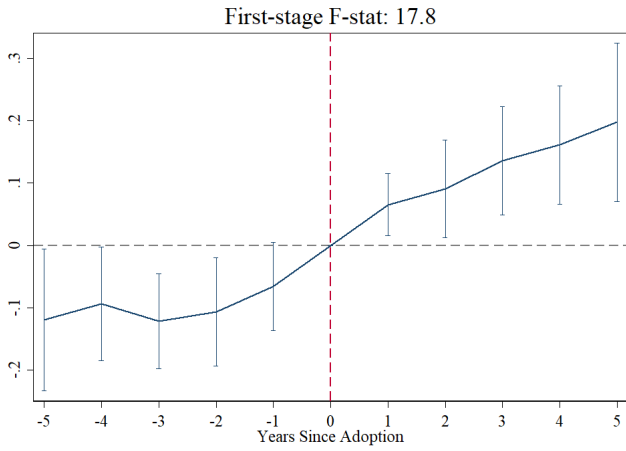
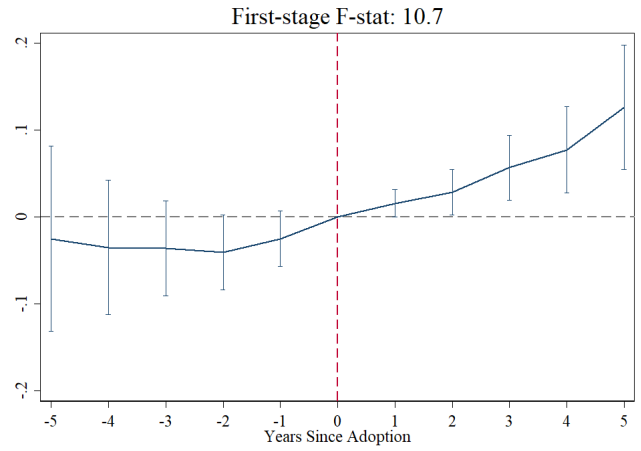


Figure 3: Incarceration Rate Event Study Plots

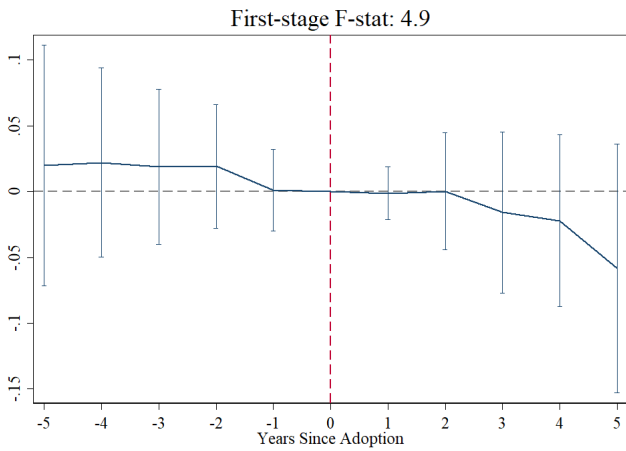
(a) Weapons Mandatory



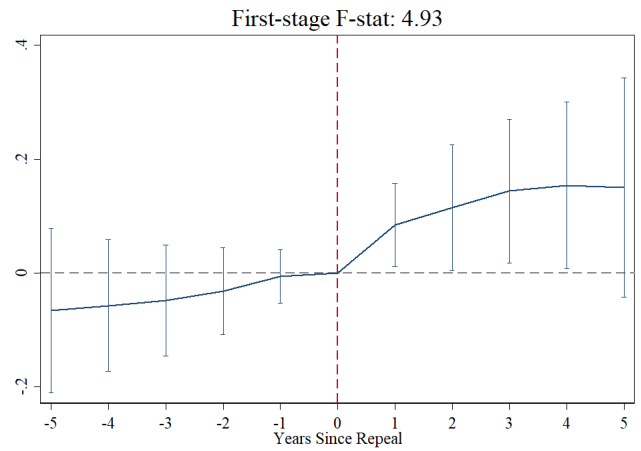
(b) Habitual Offender Law, Violent Offenses



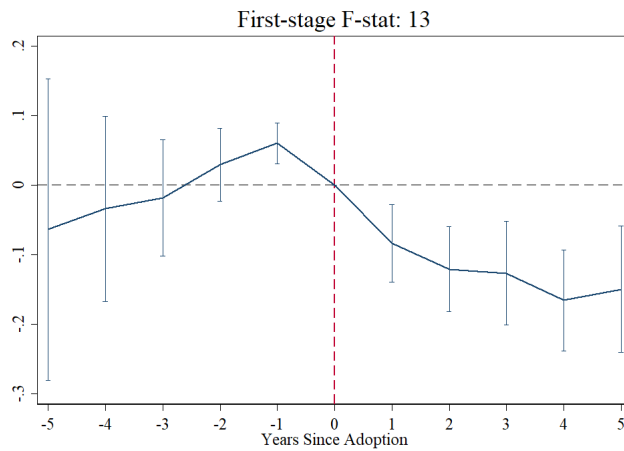
(c) Sentencing Commission, Adoption



(d) Sentencing Commission, Repeal



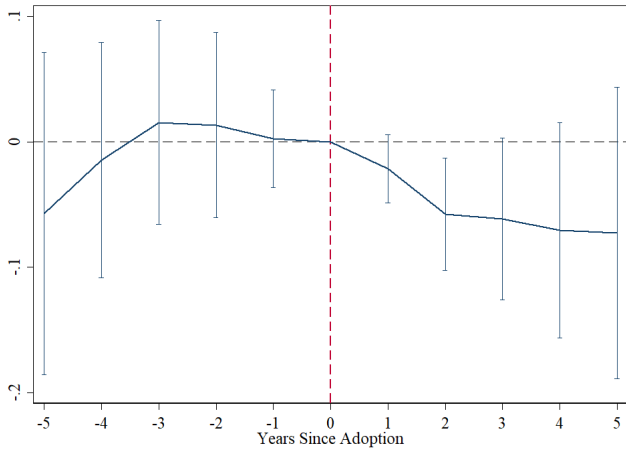
(e) Determinate Sentencing



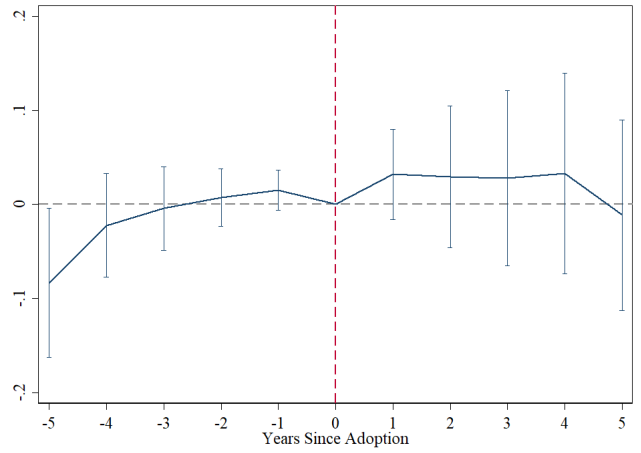
Note: Standard errors are clustered by state. All event studies include state and year fixed effects as well as state-specific linear time trends. The F-statistic is from a test on the policy using the estimate from a first-stage regression.

Figure 4: Violent Crime Rate Event Study Plots

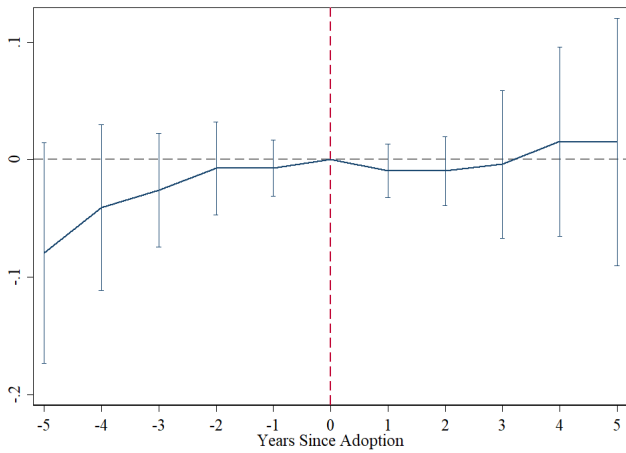
(a) Weapons Mandatory



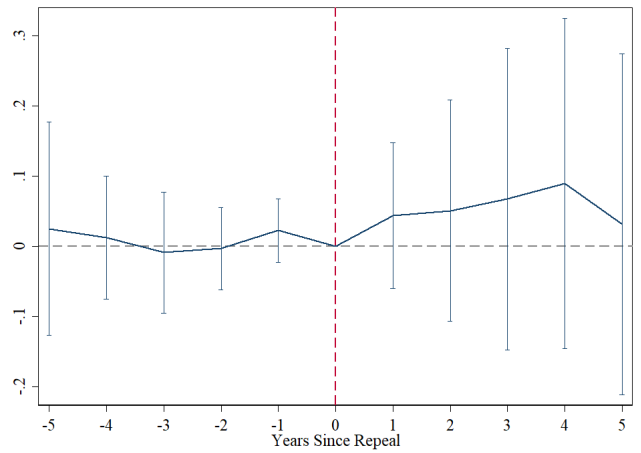
(b) Habitual Offender Law, Violent Offenses



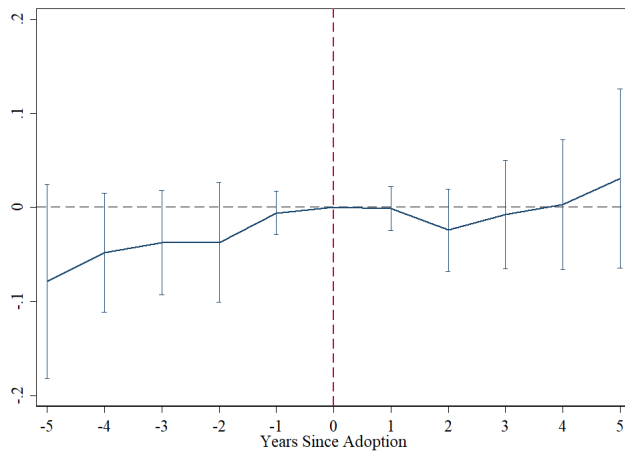
(c) Sentencing Commission, Adoption



(d) Sentencing Commission, Repeal



(e) Determinate Sentencing



Note: Standard errors are clustered by state. All event study regressions include state and year fixed effects as well as state-specific linear time trends.

Figure 5: Adoption of Weapons Mandatory, by State and Timing

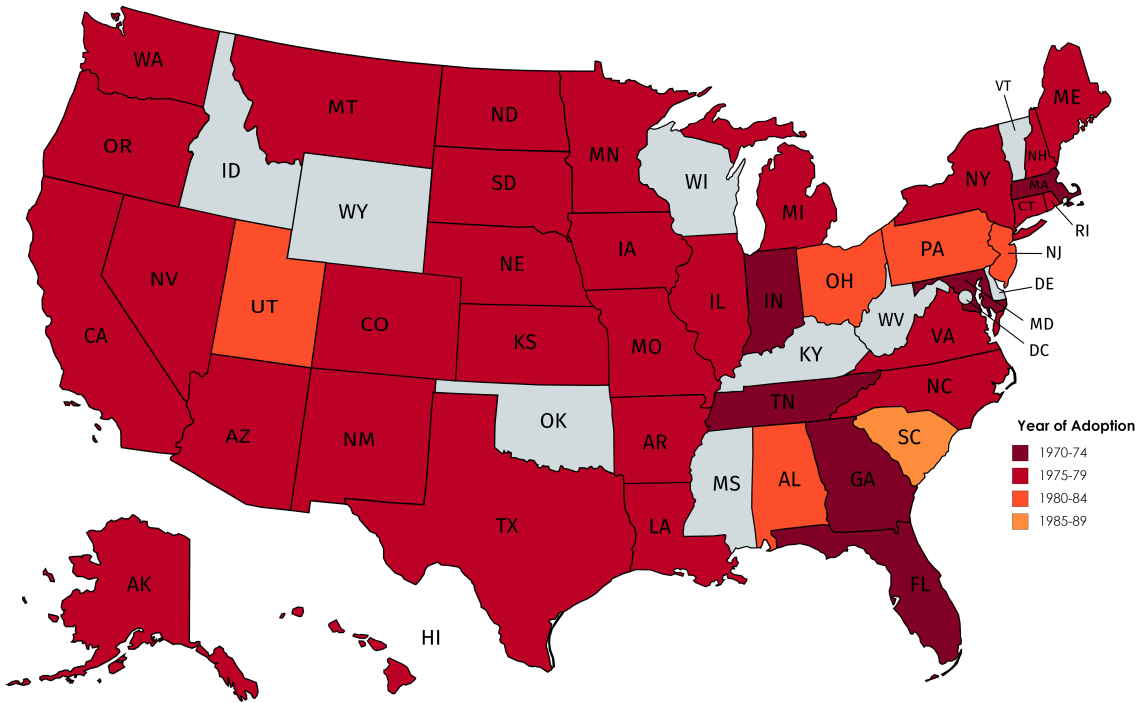


Figure 6: Adoption of Determinate Sentencing, by State and Timing

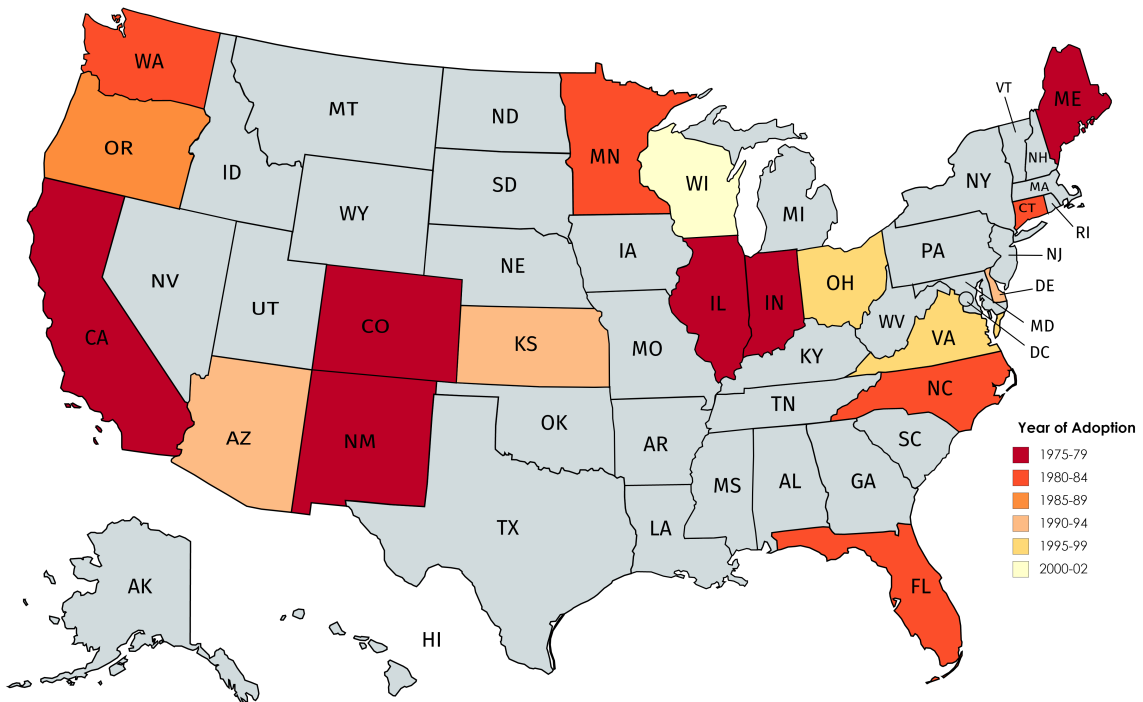
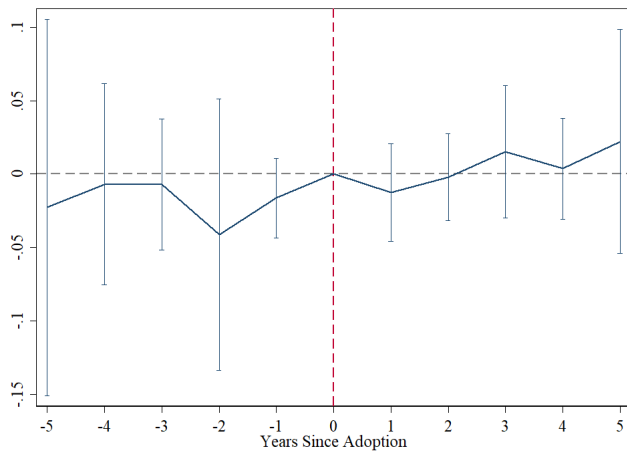
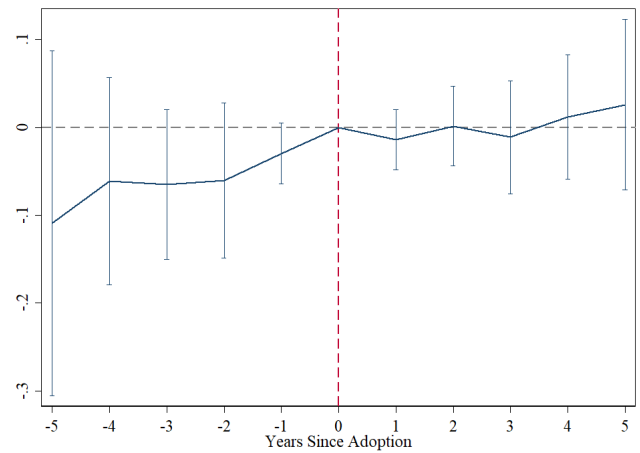


Figure 7: Event Study Plots of Logged Birth Rate Relative to Policy Adoption

(a) Determinate Sentencing



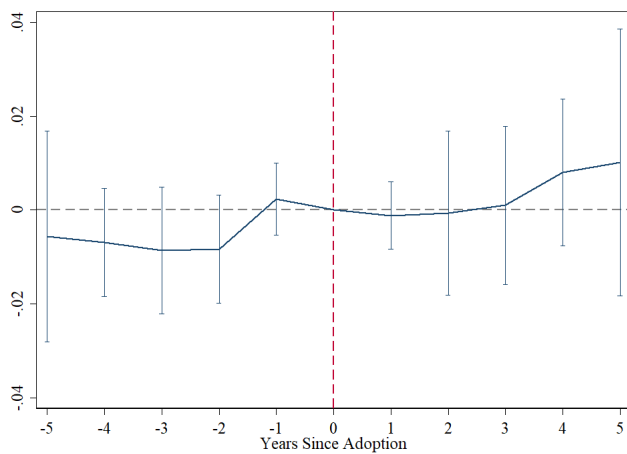
(b) Weapons Mandatory



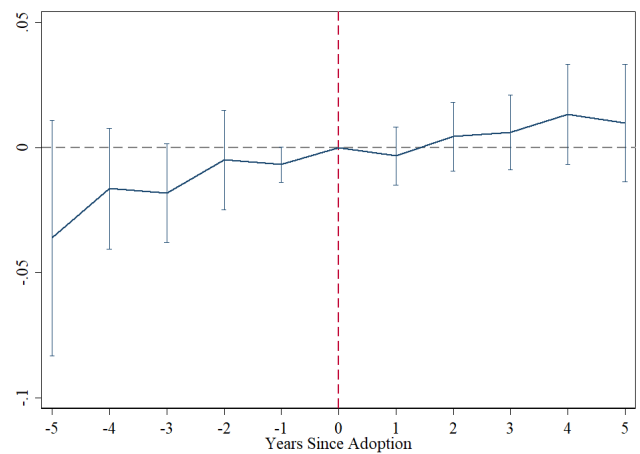
Note: Standard errors are clustered by state. All event study regressions include state and year fixed effects as well as state-specific linear time trends.

Figure 8: Event Study Plots of Share of Out-of-Wedlock Births Relative to Policy Adoption

(a) Determinate Sentencing

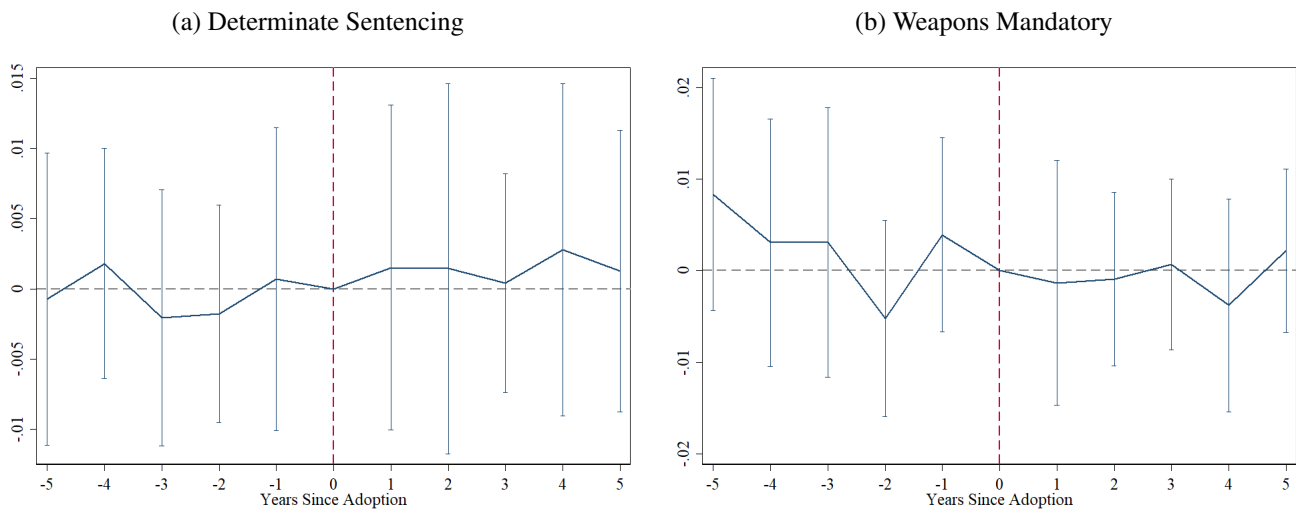


(b) Weapons Mandatory



Note: Standard errors are clustered by state. All event study regressions include state and year fixed effects as well as state-specific linear time trends.

Figure 9: Event Study Plots of Share of Low Birthweight Births Relative to Policy Adoption



Note: Standard errors are clustered by state. All event study regressions include state and year fixed effects as well as state-specific linear time trends.

Table 1: Share of States with Each Sentencing Law or Policy in 1977 and in 2002

	1977	2002
Structured Sentencing	0.04	0.52
Determinate Sentencing	0.06	0.32
Truth in Sentencing	0.02	0.54
LEO Enhancement	0.24	0.56
School Enhancement	0.02	0.98
Weapons Mandatory	0.54	0.82
Sentencing Commission	0.00	0.44
CCA	0.10	0.50
Megan's Law	0.00	1.00
HOL, second time	0.32	0.60
HOL, third time	0.60	0.84
HOL, violent offense	0.06	0.54
Parole Fee	0.10	0.62
Probation Fee	0.16	0.80

LEO refers to law enforcement official. CCA refers to community corrections act. HOL refers to habitual offender law that gets triggered by a specific time or offense. I refer the reader to Appendix A for more detailed descriptions of each policy.

Table 2: Regressions of Policy Dummies on Lagged State-Level Variables

	(1) Weapons Mandatory	(2) HOL, Violent Offense	(3) Determinate Sentencing
L.Violent crime rate	-0.017 [0.024]	0.040 [0.085]	0.005 [0.048]
L2.Violent crime rate	0.007 [0.025]	0.151** [0.074]	-0.001 [0.052]
L.Property crime rate	-0.061 [0.063]	-0.243* [0.140]	0.049 [0.107]
L2.Property crime rate	-0.017 [0.039]	-0.073 [0.164]	-0.139 [0.106]
L.Poverty rate	0.001 [0.001]	-0.001 [0.006]	-0.003 [0.004]
L.Marriage rate	0.049 [0.323]	-0.536 [0.367]	-0.149 [0.256]
L.Unemployment rate	0.000 [0.004]	0.004 [0.012]	-0.007 [0.011]
L.Share black	-0.105 [0.127]	-0.169 [0.697]	0.423 [0.448]
L.Share ages 15 to 17	0.075** [0.033]	0.005 [0.099]	0.021 [0.099]
L.Share ages 18 to 24	-0.022 [0.033]	-0.046 [0.037]	-0.007 [0.028]
L.Share ages 25 to 34	0.015 [0.009]	0.043 [0.041]	-0.022 [0.058]
L.Democrat governor	-0.007 [0.018]	-0.006 [0.030]	0.003 [0.020]
L.Log GSP per capita	0.104 [0.135]	0.078 [0.346]	-0.003 [0.164]
L.Welfare benefit	0.000 [0.001]	-0.001 [0.002]	0.003 [0.003]
R-squared	0.97	0.87	0.91
F-statistic	0.71	1.29	0.76
P-value	0.75	0.25	0.71
Observations	1100	1100	1150

Standard errors are clustered by state. Time trend refers to a state-specific linear time trend. HOL refers to habitual offender law. Welfare benefit refers to the maximum weekly benefit of AFDC/TANF for two-member families. The F-statistic and p-value presented are from a joint test on the ten regressors. All crime rates as well as the gross state product per capita are logged.

Table 3: Summary Statistics for 1978 and 2003

	<u>1978</u>		<u>2003</u>	
	Mean	Std. Dev.	Mean	Std. Dev.
State incarceration rate	117.5	54.4	424.2	164.3
<i>Family Formation</i>				
Birth rate	20.37	2.73	14.73	3.91
Out of wedlock births	0.47	0.17	0.62	0.14
Low birth weight	0.13	0.03	0.13	0.03
Single mother	0.50	0.23	0.62	0.20
Married	0.39	0.23	0.30	0.19
Married before	0.38	0.25	0.34	0.19
<i>Labor Market</i>				
Employed	0.54	0.22	0.65	0.16
In labor force	0.66	0.19	0.73	0.14
Living in poverty	0.27	0.19	0.24	0.15

The incarceration rate is an average of state incarceration rates. The sample for the outcome variables is black women ages 18–40 from the CPS, with the exception of the first three family formation outcomes, which use the universe of black women from the Vital Statistics. The mean of the outcome variables across states indicates the share of individuals who are classified under that specific outcome. The birth rate is the number of births to black mothers per 1,000 black individuals. The denominator for the share of women who are single mothers is all of the women who are mothers. The denominator for the share of women who have been married before is all of the women who have ever been married.

Table 4: 2SLS Regression of Family Formation Outcomes on Incarceration Rate for All Black Women

	Vital Statistics			CPS		
	(1) Birth Rate	(2) Out of Wedlock Births	(3) Low Birth Weight	(4) Single Mothers	(5) Married	(6) Married Before
Incarceration rate	0.017 [0.292]	0.033 [0.052]	0.018 [0.016]	0.032 [0.206]	0.017 [0.128]	0.040 [0.181]
F-statistic	18.99	17.31	18.99	18.43	20.01	18.50
Mean of Dep. Variable	2.91	0.58	0.12	0.58	0.33	0.37
Observations	1300	1275	1300	1159	1219	1172

Standard errors are clustered by state. The two instruments (determinate sentencing and the weapons mandatory) are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS or all black women in the Vital Statistics data. The incarceration rate is logged. The denominator for the share of women who have been married before is all of the women who have ever been married. The denominator for the share of women who are single mothers is all of the women who are mothers. The F-statistic is from a test on the first-stage instruments.

Table 5: 2SLS Regression of Family Formation Outcomes on Incarceration Rate for Women who are Household Heads

	(1) Single Mothers	(2) Married	(3) Married Before
Incarceration rate	0.178 [0.133]	-0.210* [0.124]	0.441** [0.224]
F-statistic	18.72	18.34	19.35
Mean of Dep. Variable	0.87	0.12	0.73
Observations	1074	1136	1053

Standard errors are clustered by state. The two instruments (determinate sentencing and the weapons mandatory) are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS. The incarceration rate and birth rate are both logged. The denominator for the share of women who have been married before is all of the women who have ever been married. The denominator for the share of women who are single mothers is all of the women who are mothers. The F-statistic is from a test on the first-stage instruments.

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Table 6: 2SLS Regression of Labor Market Outcomes on Incarceration Rate

	All Women			Household Heads		
	(1) Employed	(2) Labor Force	(3) In Poverty	(4) Employed	(5) Labor Force	(6) In Poverty
Incarceration rate	0.341** [0.171]	0.297** [0.132]	-0.203 [0.145]	0.518** [0.235]	0.480** [0.213]	-0.413 [0.257]
F-statistic	20.01	19.97	20.01	18.34	18.35	18.34
Mean of Dep. Variable	0.61	0.71	0.30	0.63	0.74	0.45
Observations	1219	1217	1219	1136	1135	1136

Standard errors are clustered by state. The two instruments (determinate sentencing and the weapons mandatory) are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS. The incarceration rate and birth rate are both logged. The F-statistic is from a test on the first-stage instruments.

10 Appendix A: Background

Figure 10: Incarceration Rate, by State for 1978–2012

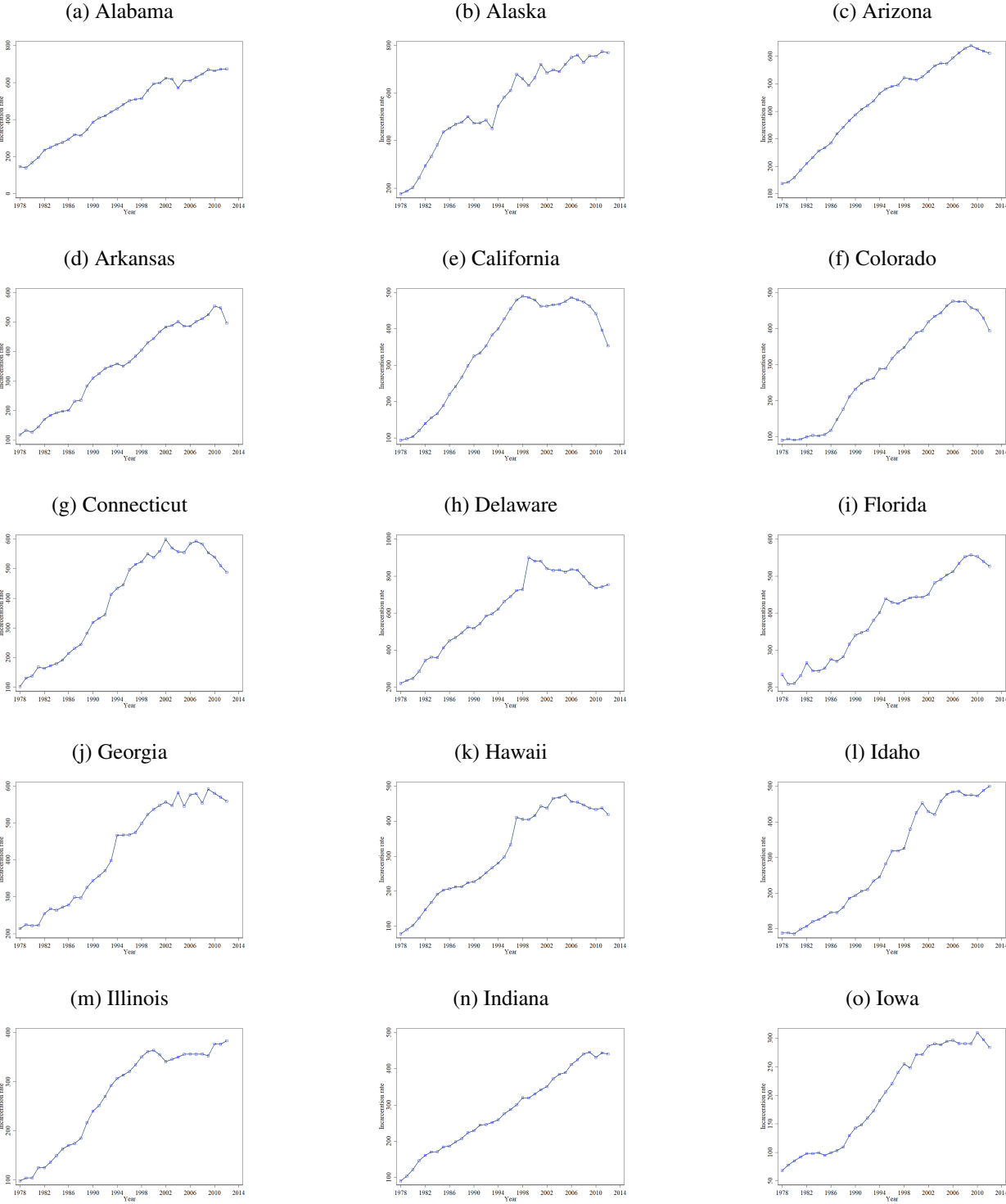


Figure 11: Incarceration Rate, by State for 1978–2012

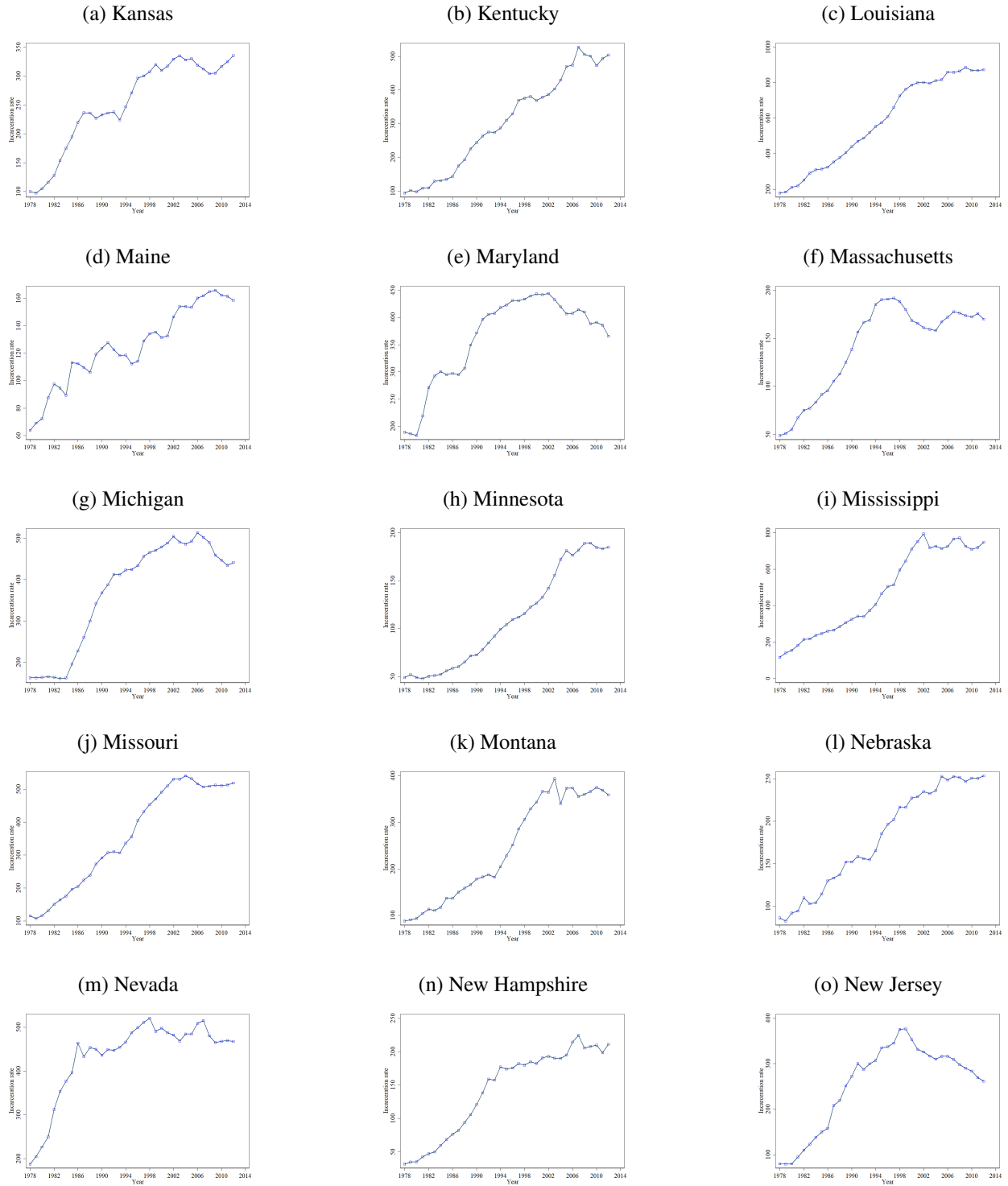


Figure 12: Incarceration Rate, by State for 1978–2012

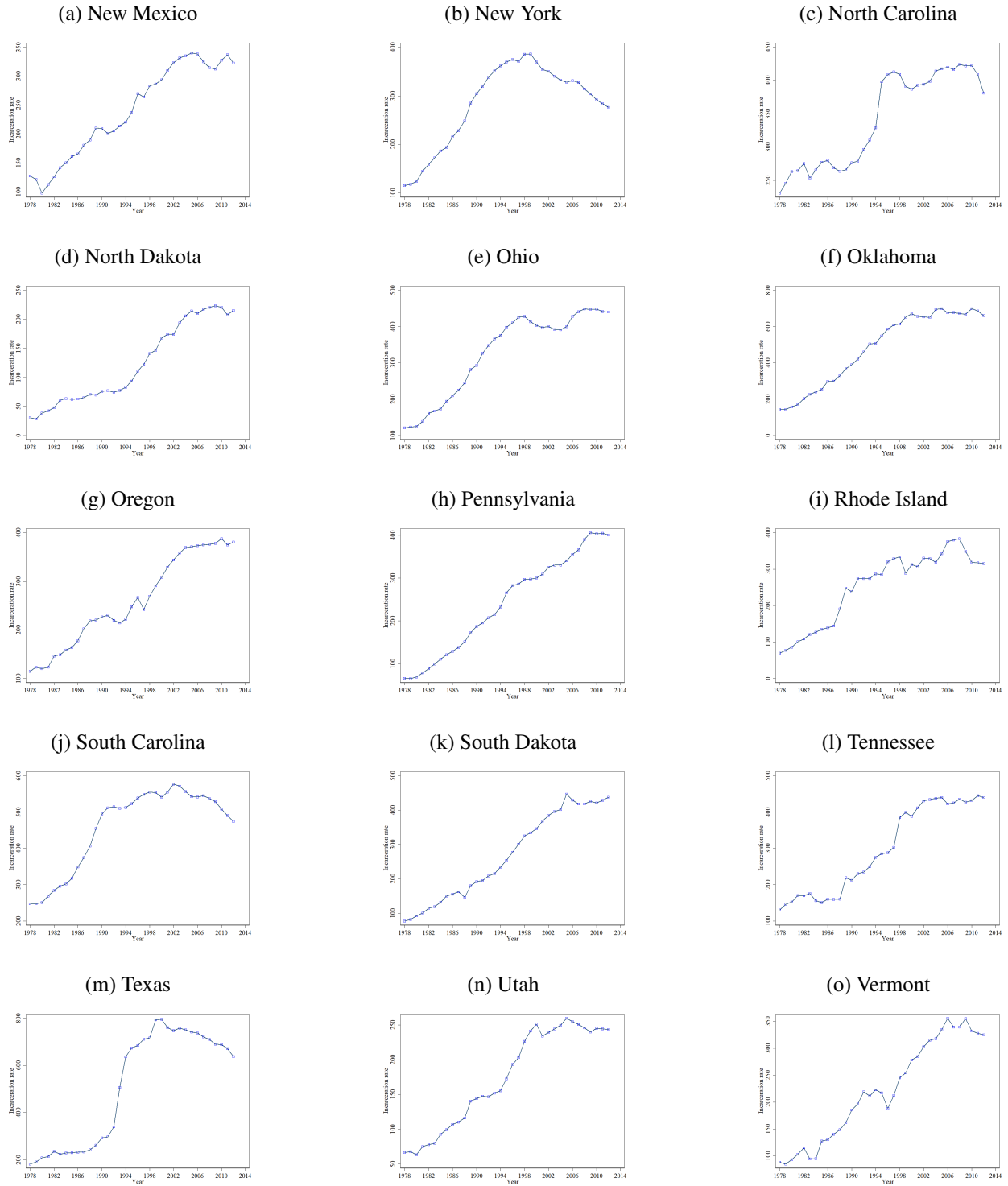
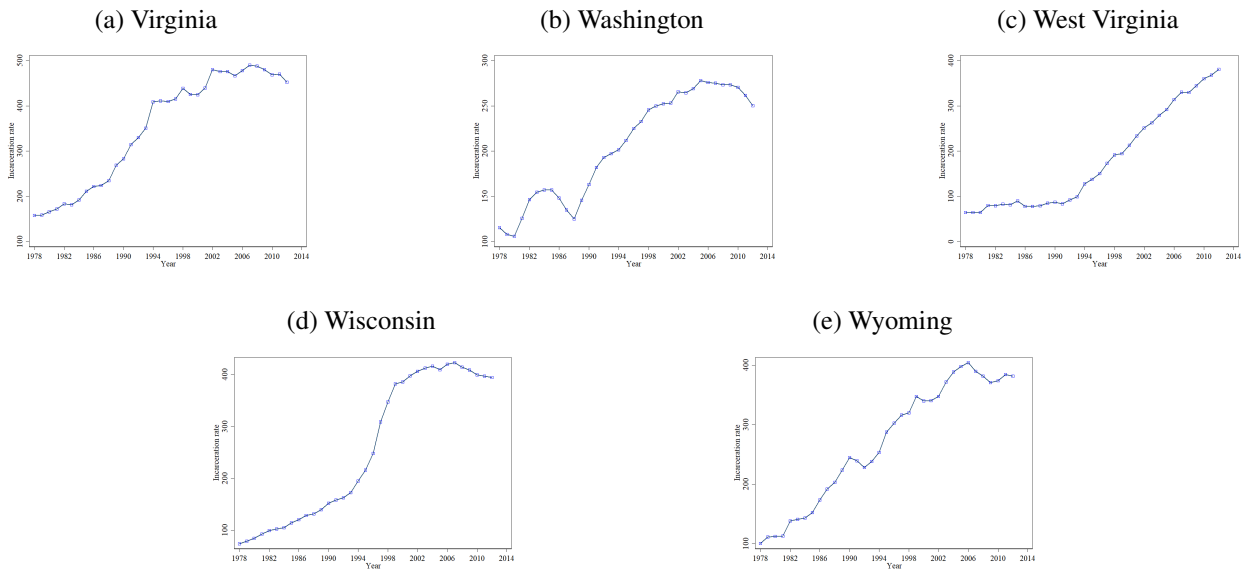


Figure 13: Incarceration Rate, by State for 1978–2012



This paper considers fourteen policy changes that occurred in a number of states between 1975–2002. I refer the reader to Stemen (2007), Stemen et al. (2006), and Stemen and Rengifo (2012) for more detailed discussions of each policy.

Two instruments:

- *Determinate sentencing*: Before the 1970s, sentencing policy in all fifty states consisted of an indeterminate sentencing model, in which judges exercised discretion over the disposition and duration of sentences, and parole boards maintained authority over the duration of sentences served through discretionary release. Starting in 1976, states began to adopt determinate sentencing models, in which statutory sentence ranges available to judges became constricted by statute. This change in sentencing also abolished discretionary release by a parole board and curtailed parole guidelines. The goal of this policy was to ensure that the time served by offenders was primarily determined by the length of the sentence imposed by the judge, rather than by the discretionary decision-making of the parole board. However, it is worth noting that many of the states that adopted determinate sentencing also revised and narrowed sentence ranges for offenses, so that a number of studies have found a negative association between the adoption of determinate sentencing and incarceration rates.
- *Mandatory sentencing law for offenses involving a weapon (i.e., weapons mandatory)*: The variation in mandatory sentencing laws across states is significant, but a common aspect is that many of these laws require judges to impose incarceration. Between 1975 and 2002, every state adopted some form of mandatory sentences. Due to wide heterogeneity and data restrictions, this paper only considers mandatory sentencing laws for offenses involving the use of a weapon (regardless of the underlying offense). These laws required a term of incarceration for the offense if the individual was armed with or used a deadly weapon.

Other relevant (but perhaps not valid) instruments:

- *Sentencing Commissions*: Along with the movement toward increasing structure in sentencing, many states created sentencing commissions to serve as independent agencies for evaluating existing sentencing laws and proposing future reforms. Many of these sentencing commissions were also tasked with reducing prison populations as a way to reduce corrections expenditures.
- *Habitual offender laws (HOL) for violent offenses*: Habitual offender laws target offenders with prior convictions for any felony offense. These laws impose longer sentences, mandatory sentences, or restrictions on release for offenders with previous convictions or terms of incarceration. Most states already had some form of habitual offender laws before the 1970s, but many of them increased the severity or scope of these laws between 1970–2000. This paper considers habitual offender laws targeted for violent crimes.

Other policies that were considered:

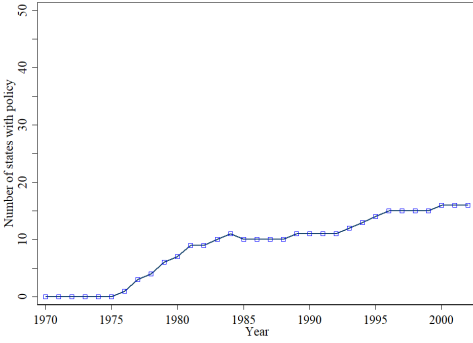
- *Structured sentencing*: While determinate sentencing was more about determining the release of prisoners, structured sentencing was about narrowing judicial discretion and controlling sentencing decisions. This policy sought to ensure that prison terms imposed for similar offenses or offenders were uniform and that the criteria for imposing sentences were consistent for all offenses and offenders. States could accomplish structured sentencing in two ways: presumptive sentencing systems or sentencing guidelines systems. For the purpose of this paper, a state will be considered to have adopted structured sentencing if it adopted either of these options.
- *Time served requirements (i.e., Truth in Sentencing)*: Like determinate sentencing, this policy sought to ensure that time served by offenders was primarily determined by the length of the sentence imposed by the court, rather than by a parole board. This change in policy was largely a result of 1994 federal government legislation, which mandated that states require violent offenders to serve 85 percent of the sentence imposed in order to receive funding from the federal government to expand jail and prison capacity.

- *Sentence enhancements for offenses committed against law enforcement officials (i.e., LEO Enhancement)*: In response to growing concern about the safety of public officials in the face of gun violence and high crime rates, many states increased the sentences for offenses committed against public officials. These policies included offenses committed against a wide range of officials—from law enforcement officials, to corrections officers, to school bus drivers, among others.
- *Sentencing enhancement for sale or possession of drugs near a school (i.e., Drug Enhancement)*: Aside from mandatory sentencing laws, many states also adopted sentencing enhancements for specific offenders. Broadly speaking, sentence enhancements allowed judges to increase the length of prison sentences if a particular “triggering event” occurred during the offense. Given data availability, this paper considers sentencing enhancement laws for the sale or possession of drugs near a school.
- *Community Corrections Acts (i.e., CCAs)*: Many states established community corrections acts as a way to provide funding to local governments and community agencies for developing and adopting alternatives to incarceration. The CCAs varied significantly across states. As an example, they differed on which types of offenders they focused on and which administrative agencies were involved.
- *Community Notification Laws for Sex Offenders (i.e., Megan’s Law)*: Sentences for sex offenders have changed drastically since the 1970s and by the early 1990s, sex offenders were 20 percent of the adult prison population in ten states. Although the specific laws vary by state, all states have increased penalties for sex offenses and many have also adopted sex offender registration laws, residency restrictions, civil commitment laws, lifetime supervision requirements, and community notification laws. Given data availability, I focus on sex offender notification laws, otherwise known as Megan’s Law. These laws make information about sex offenders available on request to individuals and organizations, or they require different departments or agencies (e.g., law enforcement agencies) to disseminate information about released sex offenders to their community.

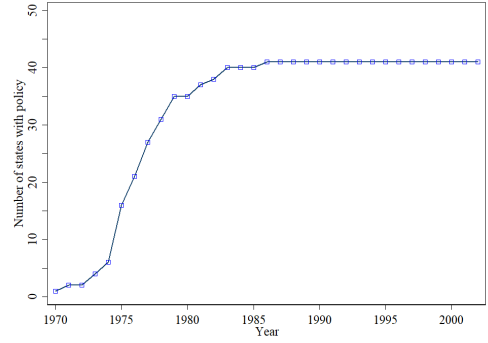
- *Habitual offender laws (HOL) for second offenses and third offenses:* These policies are similar to the habitual offender law described above. However, these policies are targeted toward second-time and third-time offenders.
- *Parole and probation supervision fees:* Supervision fees are payments that offenders must make while they are a part of the criminal justice system. Parole and probation fees are payments that offenders must make while they are on parole and probation, respectively. The payment schedule and fee amount varies widely by state.

Figure 14: Number of States with Policy, by Year for 1970–2002

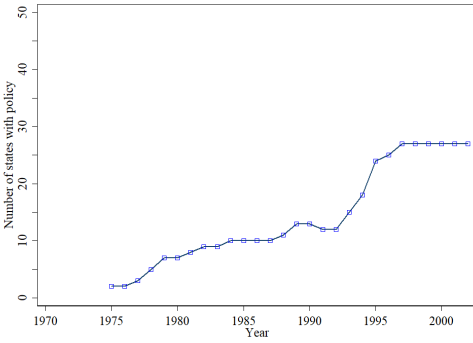
(a) Determinate Sentencing



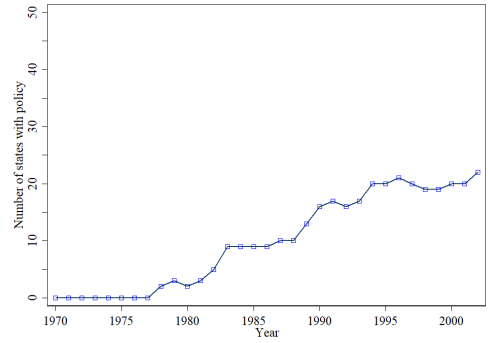
(b) Weapons Mandatory



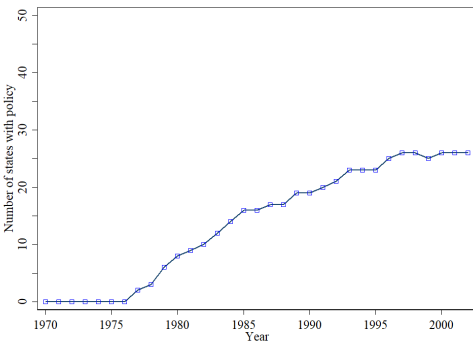
(c) Habitual Offender Law, Violent Offenses



(d) Sentencing Commissions



(e) Structured Sentencing



(f) Truth-in-Sentencing Requirements

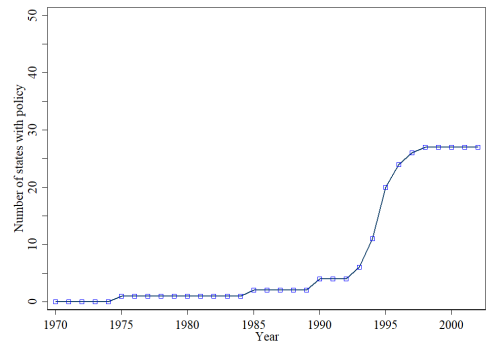
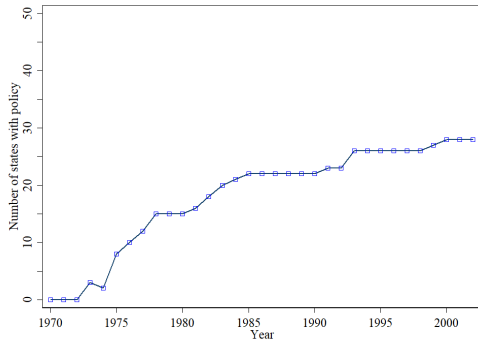
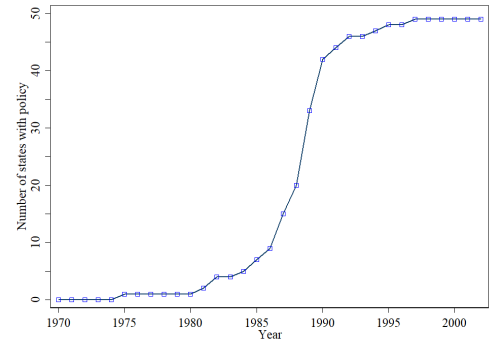


Figure 15: Number of States with Policy, by Year for 1970–2002 (continued)

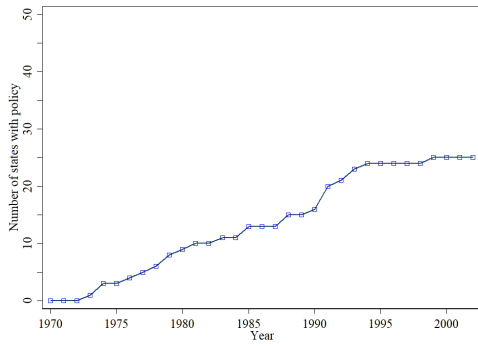
(a) Enhancement for Offenses Against Public Officials (i.e., LEO Enhancement)



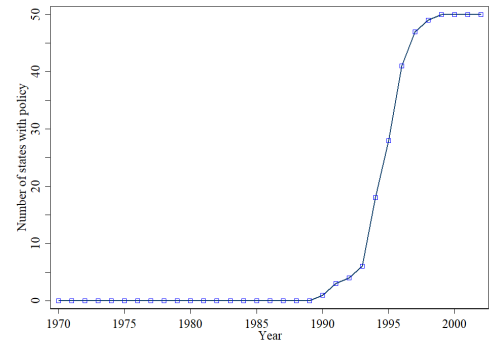
(b) Enhancement for Sale or Possession of Drugs Near School



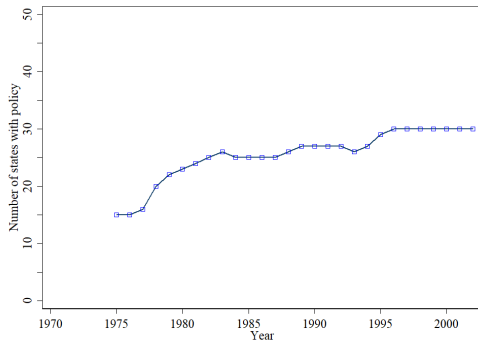
(c) Community Corrections Act



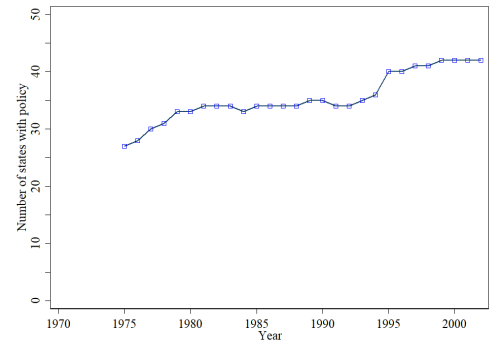
(d) Megan's Law



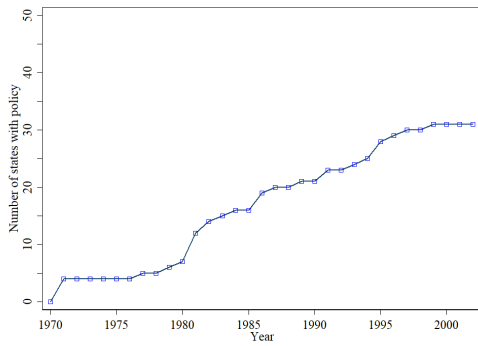
(e) Habitual Offender Law, Second Offense



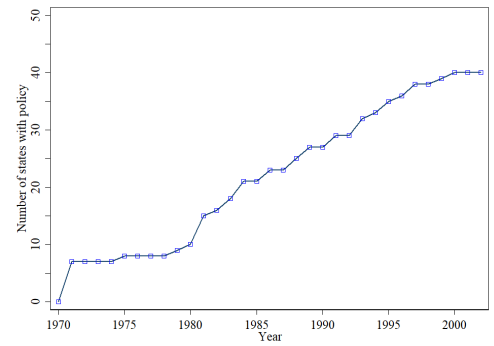
(f) Habitual Offender Law, Third Offense



(g) Parole Supervision Fee



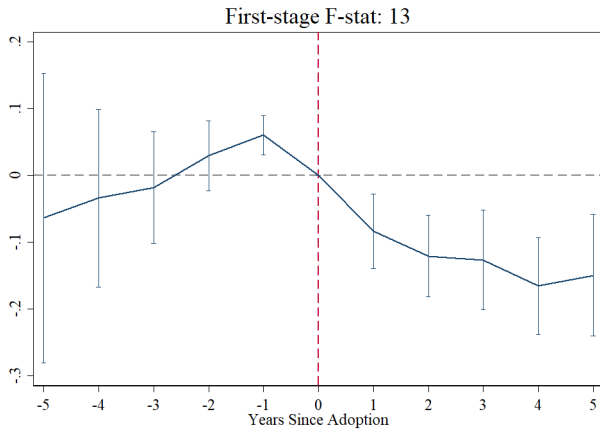
(h) Probation Supervision Fee



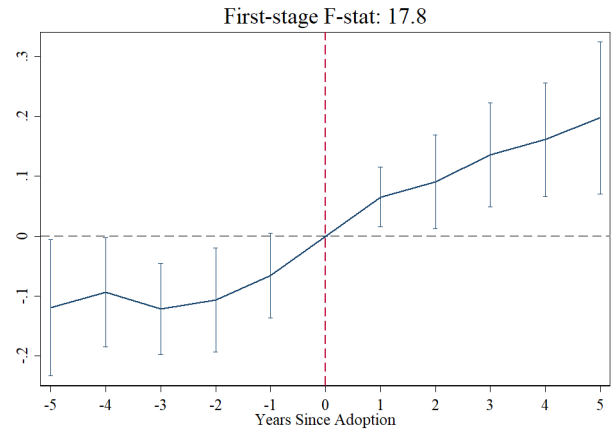
11 Appendix B: Instrument Relevance

Figure 16: Incarceration Rate Event Study Plots for All Policies

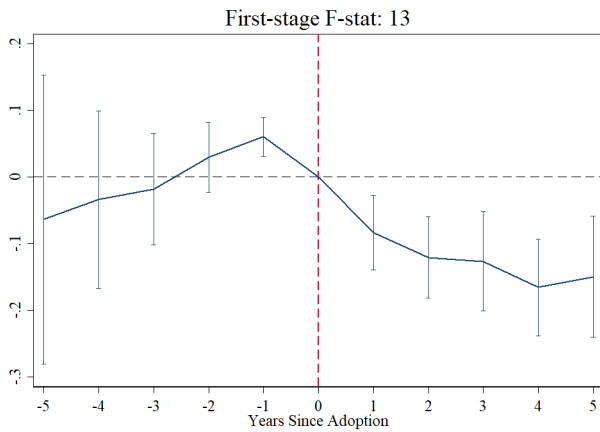
(a) Determinate Sentencing



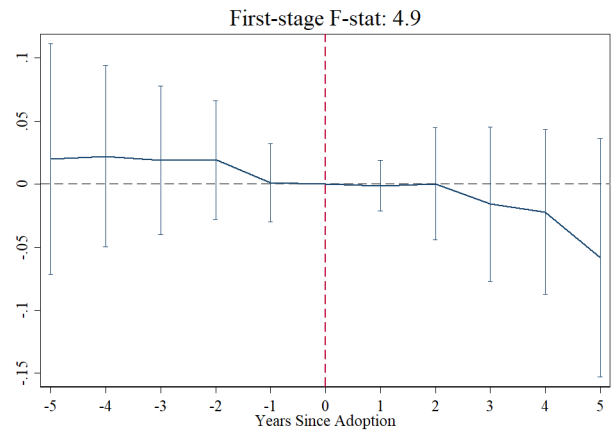
(b) Weapons Mandatory



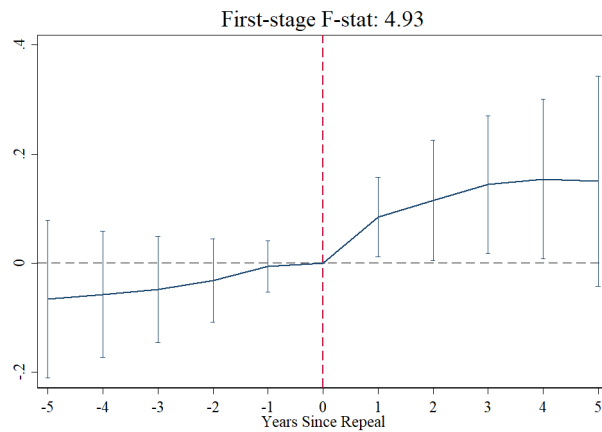
(c) Habitual Offender Law, Violent Offenses



(d) Sentencing Commission, Adoption



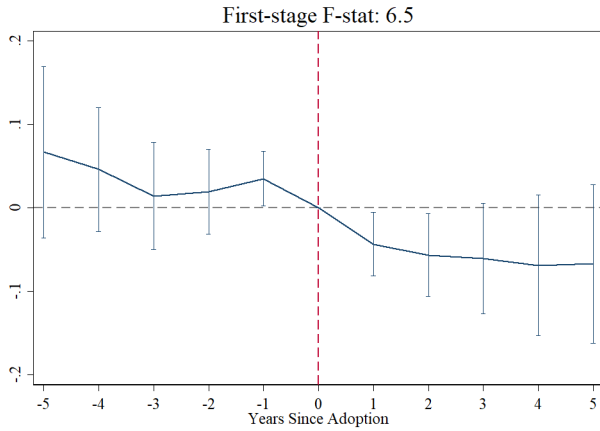
(e) Sentencing Commission, Repeal



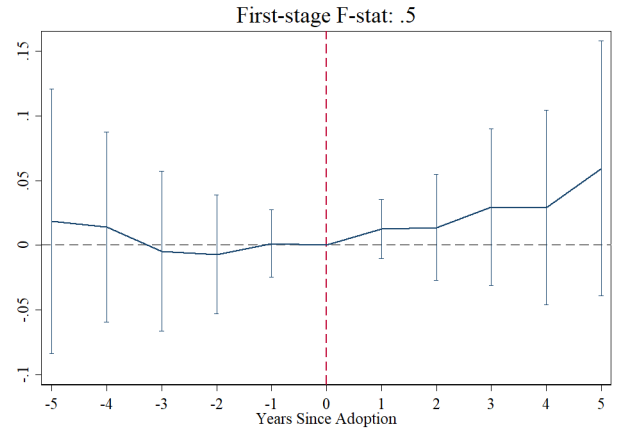
Note: Standard errors are clustered by state. All event study regressions include state and year fixed effects as well as state-specific linear time trends. The F-statistic presented is from a test on the policy using the estimate from a first-stage regression.

Figure 17: Incarceration Rate Event Study Plots for All Policies (continued)

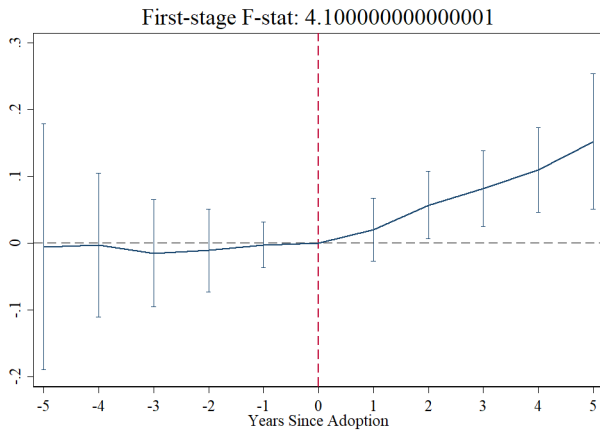
(a) Structured Sentencing



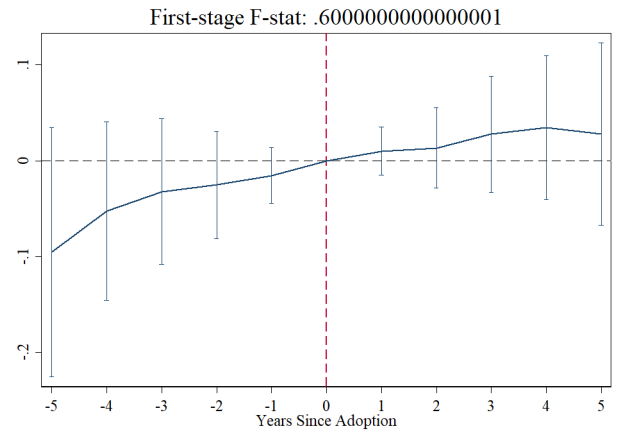
(b) Truth-in-Sentencing Requirements



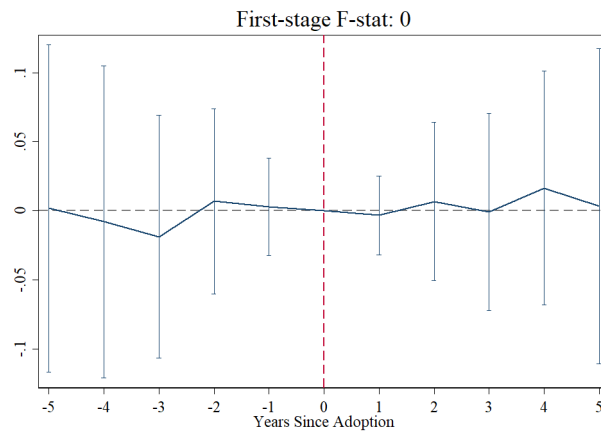
(c) Enhancement for Offenses Against Public Officials (i.e., LEO Enhancement)



(d) Enhancement for Sale or Possession of Drugs Near School



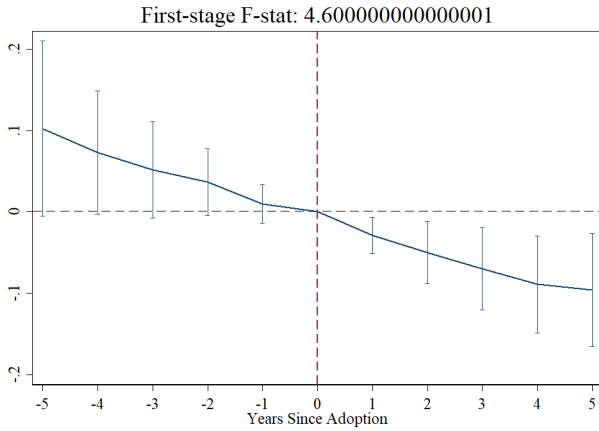
(e) Community Corrections Act



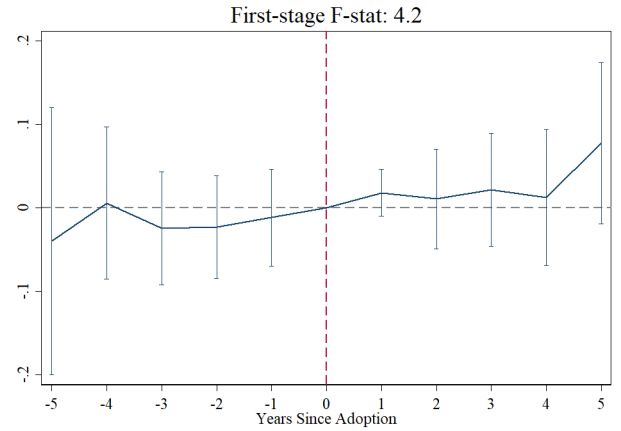
Note: Standard errors are clustered by state. All event study regressions include state and year fixed effects as well as state-specific linear time trends. The F-statistic presented is from a test on the policy using the estimate from a first-stage regression.

Figure 18: Incarceration Rate Event Study Plots for All Policies (continued)

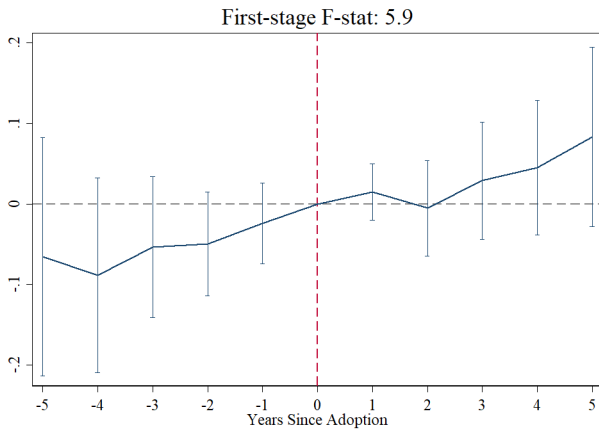
(a) Megan's Law



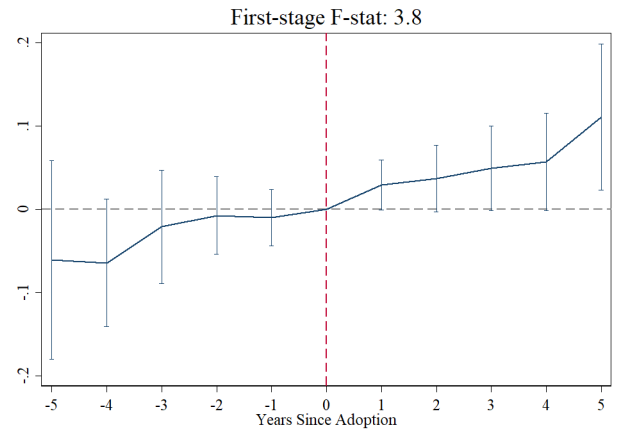
(b) Habitual Offender Law, Second Offense



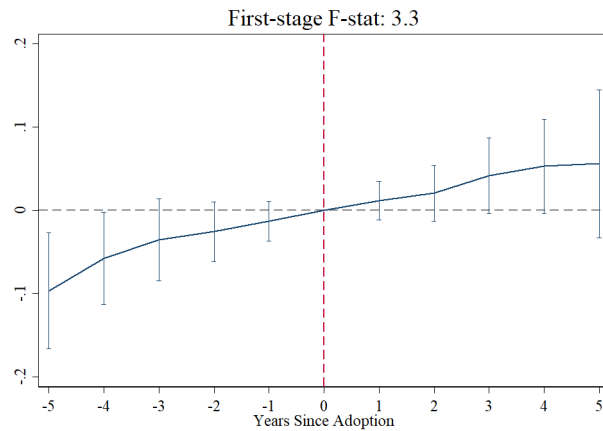
(c) Habitual Offender Law, Third Offense



(d) Parole Supervision Fee



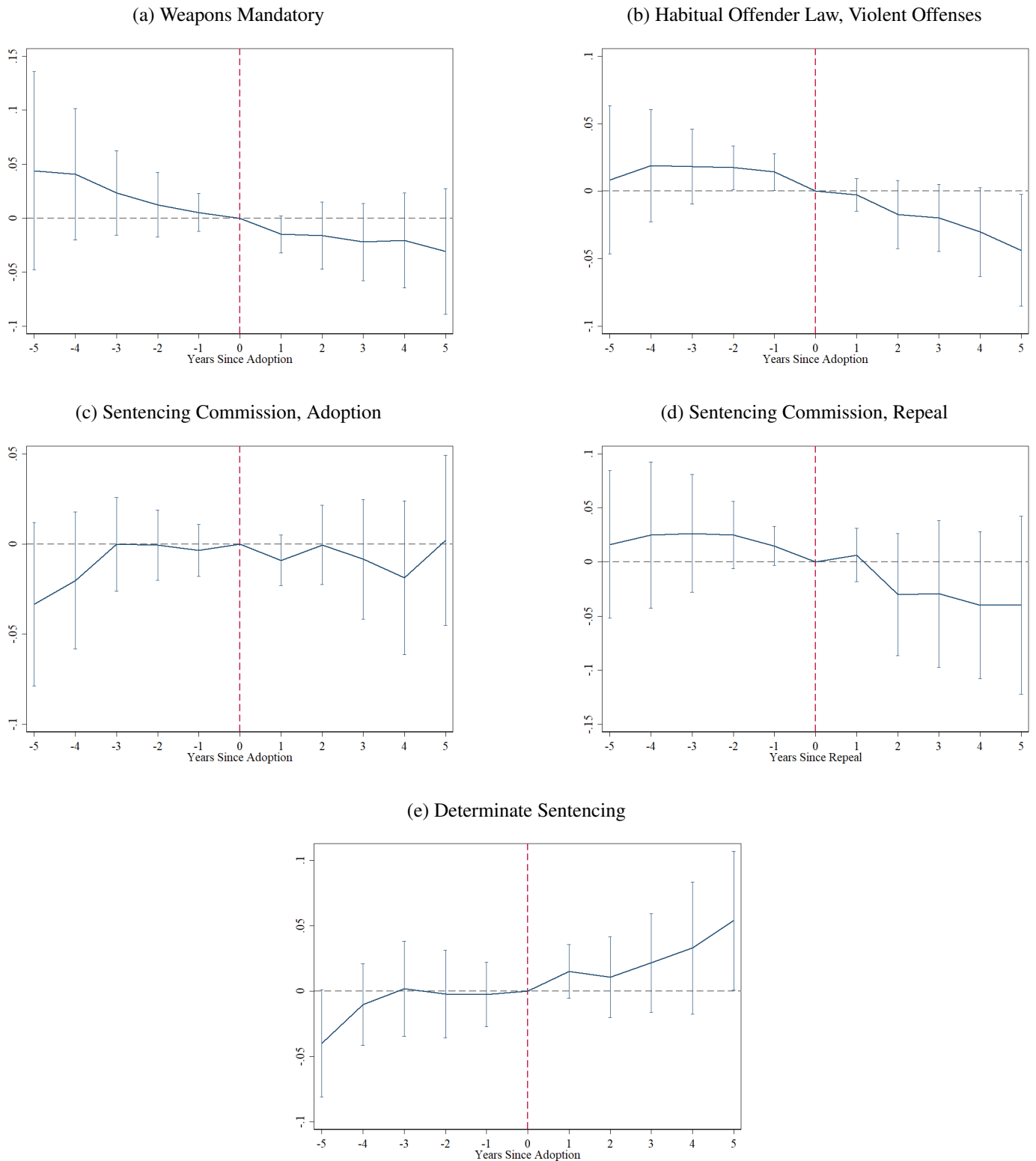
(e) Probation Supervision Fee



Note: Standard errors are clustered by state. All event study regressions include state and year fixed effects as well as state-specific linear time trends. The F-statistic presented is from a test on the policy using the estimate from a first-stage regression.

12 Appendix C: Instrument Validity

Figure 19: Property Crime Rate Event Study Plots



Note: Standard errors are clustered by state. All event study regressions include state and year fixed effects as well as state-specific linear time trends.

Actual versus Predicted Outcomes:

Another exercise I perform to explore the validity of these instruments considers the reduced-form relationship between the policies and the *predicted* outcomes of interest. I focus on the outcomes that come from the CPS since I can use more individual-level observations (not just mothers) and demographic information. Specifically, I use the full individual-level CPS sample (without any restrictions on race or age) and I regress the outcomes of interest on individual-level demographic information as well as state-level variables. I choose individual- and state-level regressors that might have an effect or are otherwise related to the outcome variables, but likely not through an incarceration channel.³² Using the regression results, I calculate predicted values for the outcomes of interest.³³ In doing so, I am attempting to embody as many of the potentially endogenous factors as possible in the predicted outcome. I can then compare the reduced-form relationship between the policies and the actual outcomes with the relationship between the policies and the predicted outcomes. Ideally, I want the policies to be much less, if at all, related to the predicted outcomes. This difference would suggest that the sentencing policies, through the incarceration rate, are indeed related to the outcomes of interest, but are less (if at all) related to the individual- or state-level endogenous factors that might pose a threat to the validity of the instruments.

The following event studies compare the relationships between a given policy and the actual outcomes against the relationships between the same policy and the predicted outcomes. The main takeaway from these event studies is that the policies have essentially a zero or negligible relationship to the predicted outcomes, as desired for instrument validity.

³² Specifically, from the CPS, I choose pre-determined characteristics: a quartic in age as well as indicator variables for whether an individual is black, Hispanic, and female. I use a number of variables from the University of Kentucky data: state minimum wage; state gross domestic product per capita; AFDC/TANF maximum monthly benefit for two-, three-, and four-person families; the maximum allotment food stamp (SNAP) benefit for one-, two-, three-, and four-person families; total maximum SSI benefits; state EITC rate as a percentage of federal credit; an indicator variable for whether the state has a Democratic governor; fraction of state lower house that is Democrat; and fraction of state upper house that is Democrat. I also include state and year fixed effects. Given that the state-level variables begin in 1980, I have two fewer years of data in these regressions than in the main state-level panel regressions.

³³ The R-squared for single mother, married, married before, employed, in labor force, and in poverty is 0.15, 0.37, 0.10, 0.28, 0.32, and 0.07, respectively.

Figure 20: Event Study Plots of Actual vs. Predicted Outcomes Relative to Adoption of Determinate Sentencing

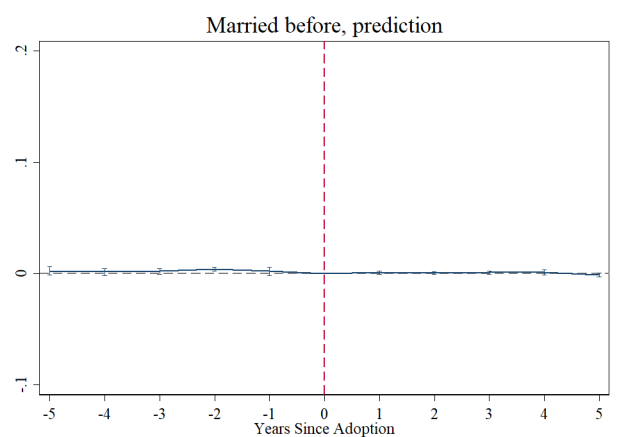
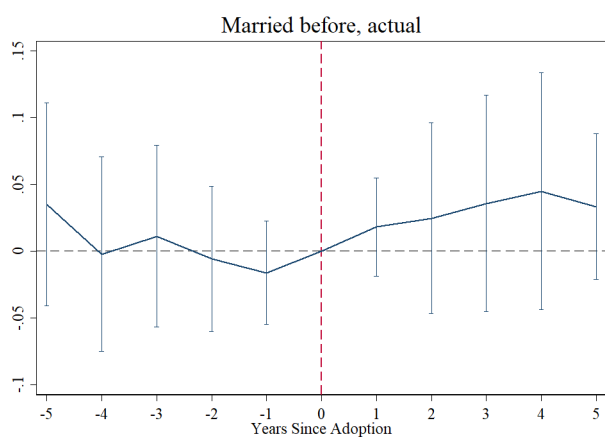
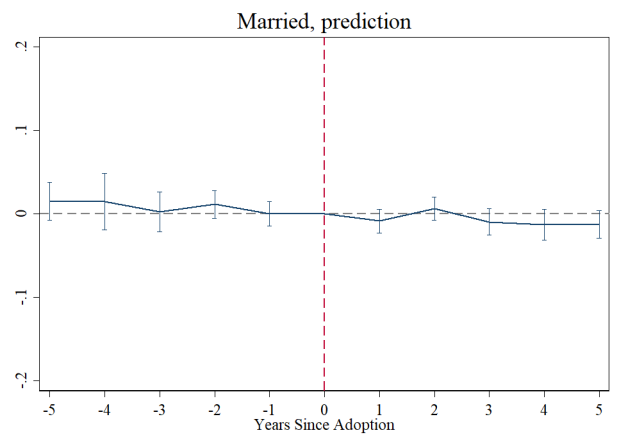
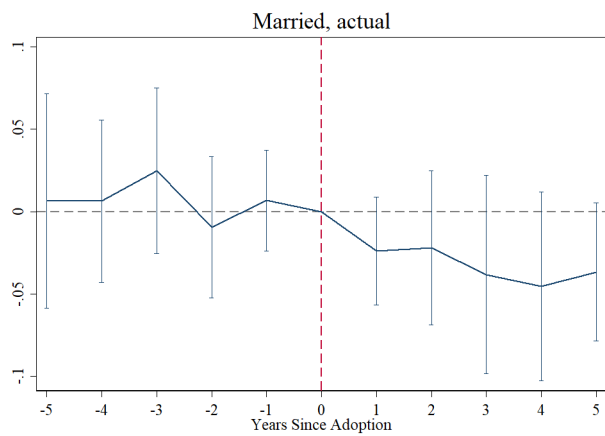
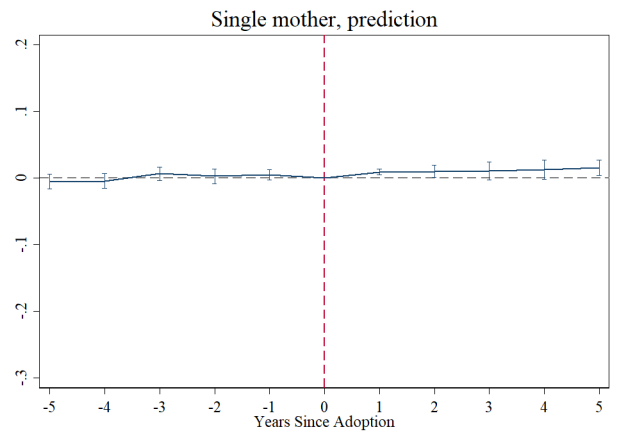
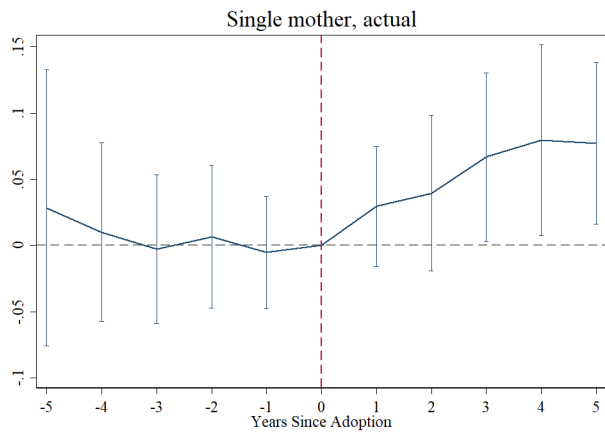


Figure 21: Event Study Plots of Actual vs. Predicted Outcomes Relative to Adoption of Determinate Sentencing (continued)

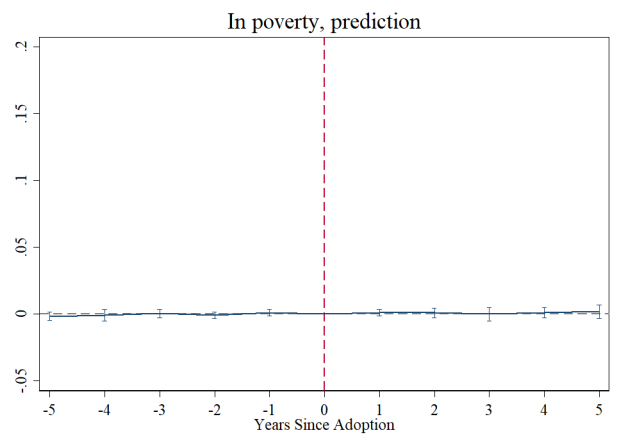
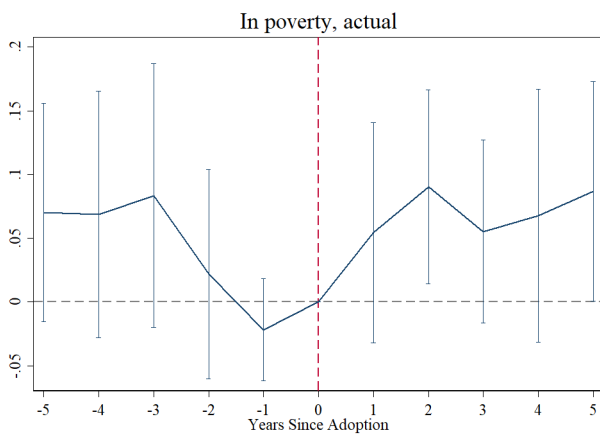
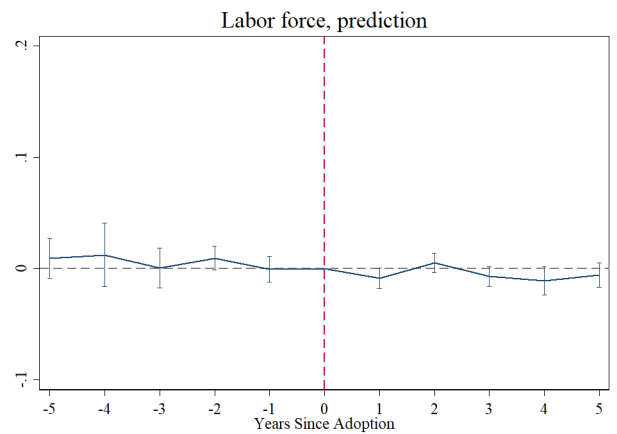
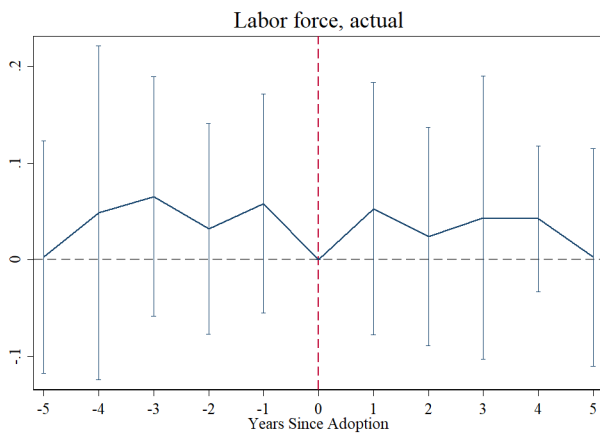
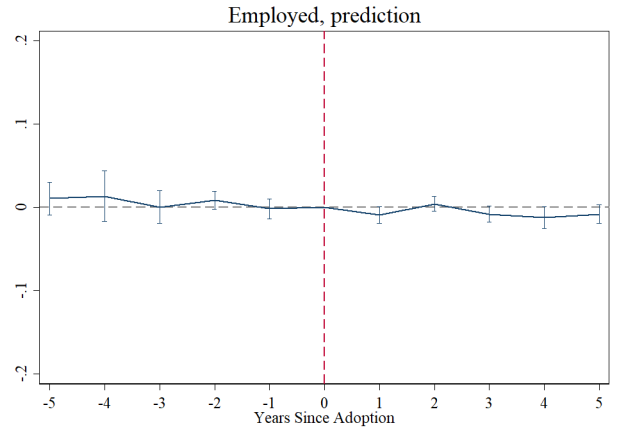
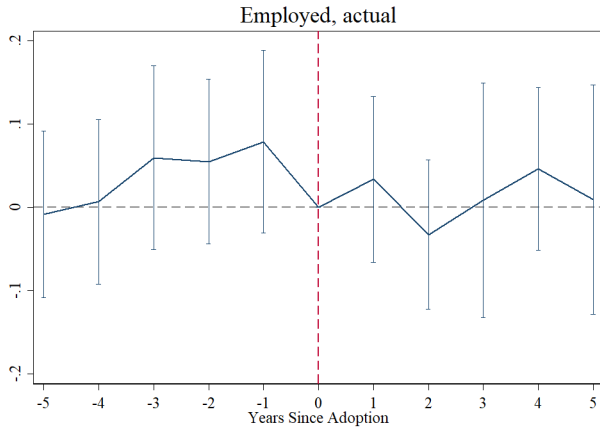


Figure 22: Event Study Plots of Actual vs. Predicted Outcomes Relative to Adoption of Weapons Mandatory

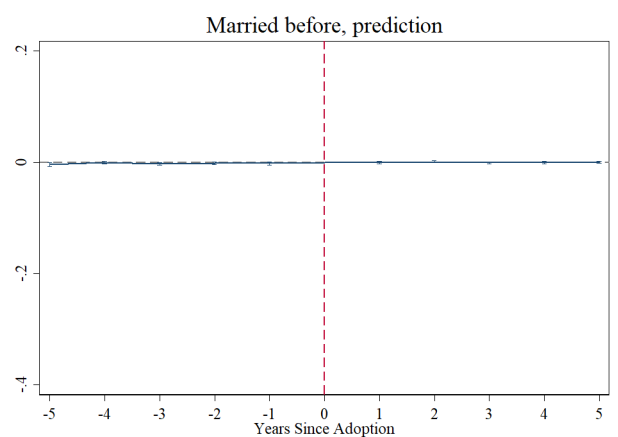
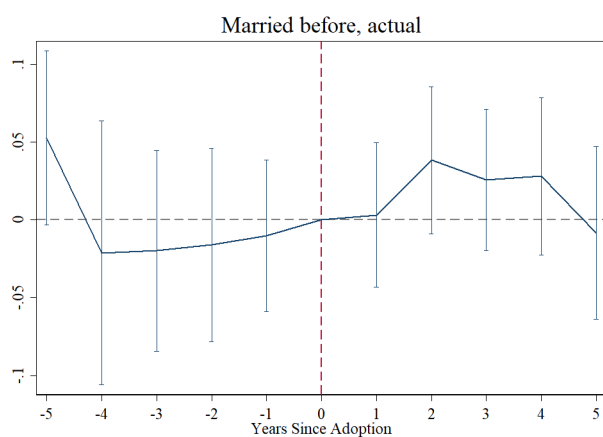
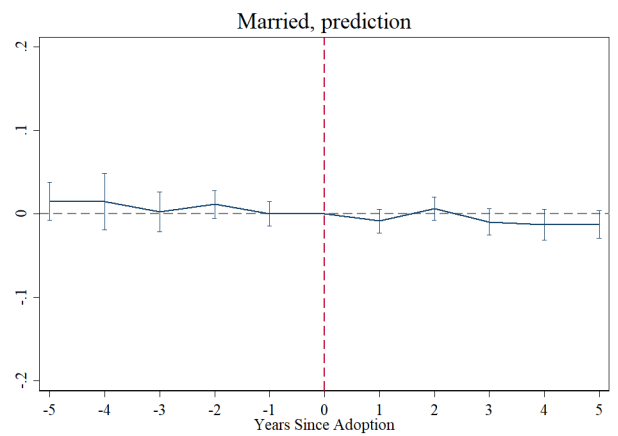
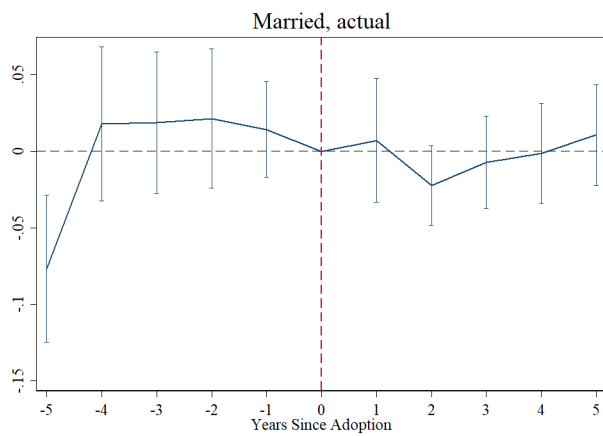
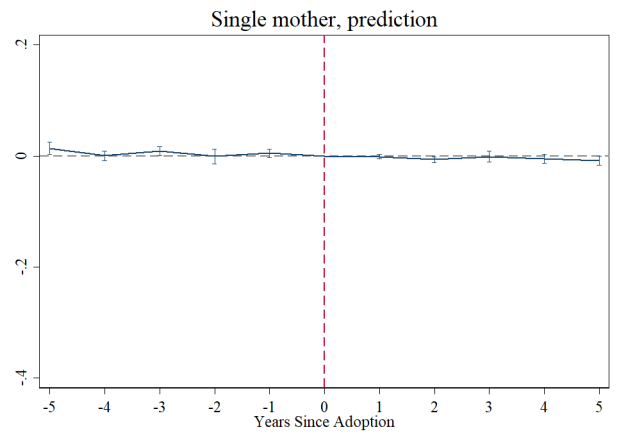
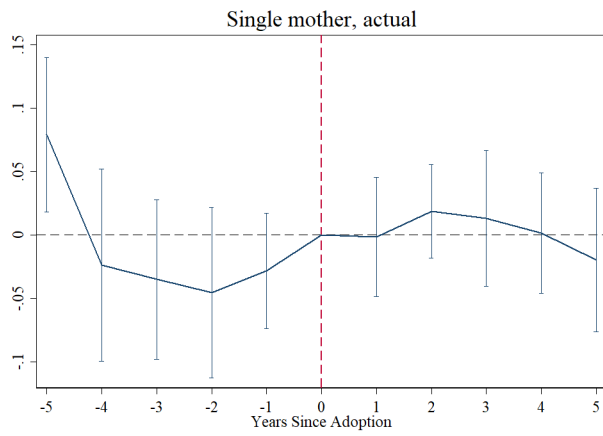
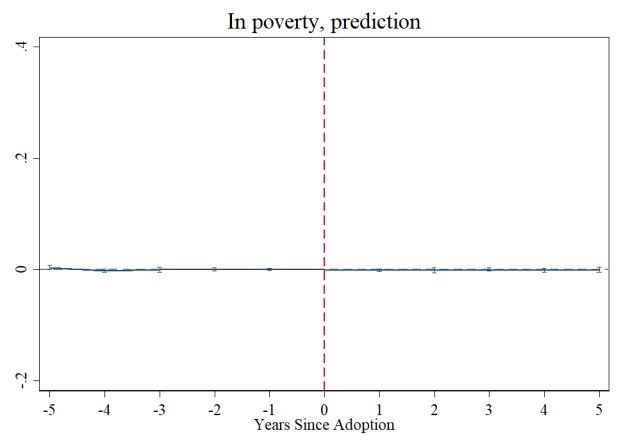
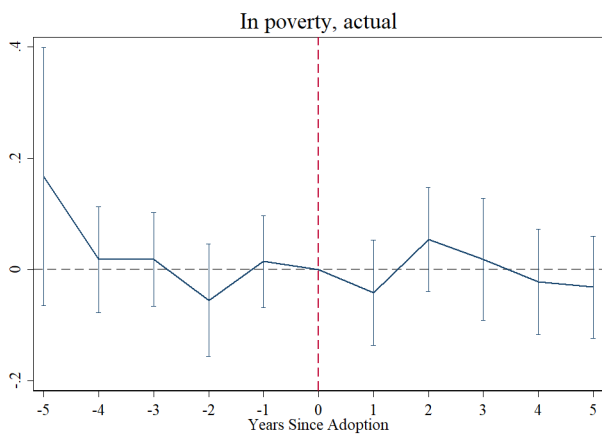
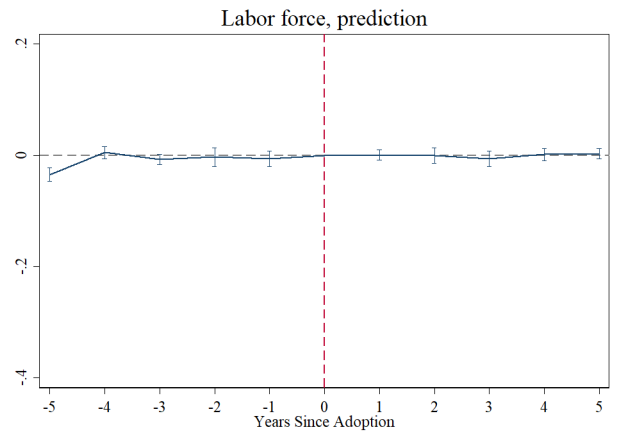
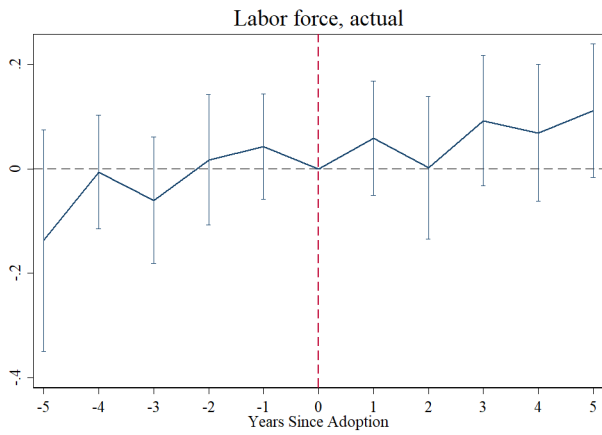
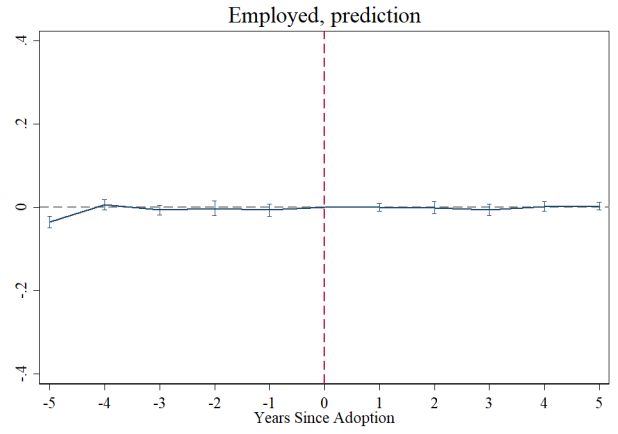
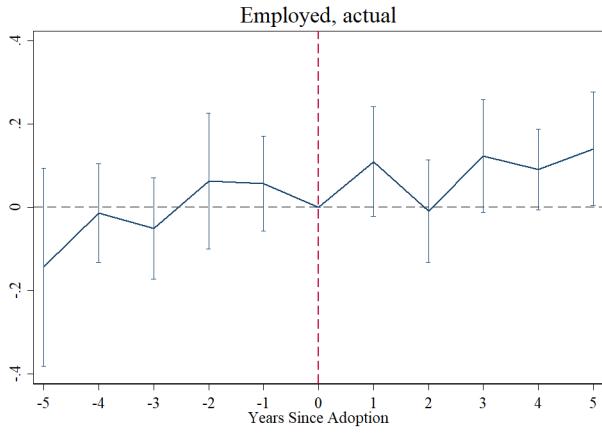


Figure 23: Event Study Plots of Actual vs. Predicted Outcomes Relative to Adoption of Weapons Mandatory (continued)



The previous event studies all considered the reduced-form relationship between the two instruments and the actual versus predicted outcomes. For completeness' sake, I now consider the two-stage least squares relationship using the instruments, once again using actual as well as predicted outcomes. Comparing the estimates in columns (1) and (2), I see that the coefficients from the regressions using actual outcomes are typically different in magnitude and sometimes even different in sign than the coefficients from the regressions using predicted outcomes. Overall, the goal of having the coefficients from the regressions using actual outcomes be different from the corresponding coefficients using predicted outcomes is satisfied, providing further evidence that these policies are not strongly related to endogenous factors and can thus serve as valid instruments.

Table 7: 2SLS Regression of Actual and Predicted Outcomes on Incarceration Rate

	Actual	Predicted
Single mother:	-0.001 [0.169]	-0.043** [0.019]
Married:	0.008 [0.093]	0.035 [0.026]
Married before:	0.074 [0.116]	0.005 [0.004]
Employed:	-0.001 [0.102]	0.028 [0.022]
In Labor Force:	0.164 [0.117]	0.023 [0.023]
In Poverty:	-0.006 [0.099]	-0.006 [0.007]

Standard errors are clustered by state. The sample used is black women ages 18–40 from 1980 to 2003. Regressions are weighted by the CPS weights. All regressions include state and year fixed effects as well as state-specific linear time trends. All regressions in this table use the two instruments (determinate sentencing and the weapons mandatory) lagged one period.

Relationship between Policies and State-Level Variables:

As a final way to test for instrument exogeneity, I substitute the incarceration rate in the first-stage regression with various state-level variables. Similar to what I saw before, the policies have a strong first-stage relationship with the incarceration rate, with the F-statistic being above the threshold level of 10 in column (1). In contrast, the F-statistic is generally very low (and corresponding p-value relatively high) for most of the remaining columns. Even though the third and sixth column show statistically significant coefficients, the results from table 2 already showed that these variables cannot predict the adoption of these laws.³⁴ Ultimately, the main takeaway from this table is that this set of instruments does not seem to be strongly or consistently associated with state benefit levels or other economic and political forces.

Table 8: Regression of Incarceration Rate and State-Level Variables on Two Instruments

	(1) Incarceration rate	(2) EITC state rate	(3) AFDC/TANF benefit	(4) Food stamp benefit	(5) State minimum wage	(6) GSP per capita	(7) Democratic governor
Determinate Sentencing	-0.129*** [0.035]	-0.537 [0.842]	20.392*** [7.577]	0.174 [0.404]	-0.086 [0.130]	131.725 [223.674]	0.069 [0.145]
Weapons Mandatory	0.142*** [0.037]	-0.466 [1.039]	-2.543 [6.640]	-0.348 [0.443]	-0.045 [0.140]	-90.450 [352.501]	-0.437 [0.367]
F-stat	18.99	0.30	4.97	0.32	0.22	0.23	0.81
P-value	0.00	0.75	0.01	0.73	0.80	0.80	0.45
Observations	1300	1200	1200	1200	1200	1200	1200

Standard errors are clustered by state. All policies are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. EITC state rate is measured as a percentage of the federal credit. AFDC/TANF benefit refers to the maximum monthly benefit for two-member families. Food stamp benefit refers to the maximum allotment food stamp benefit for two-member families. GSP per capita refers to the gross state product per capita of each state in that year. The F-statistic presented is from a test on the two instruments.

³⁴ Event study plots of the AFDC/TANF benefit and GSP per capita relative to the policies' adoption also do not indicate any clear trends.

13 Appendix D: Summary Statistics, Results, and Robustness Checks

Table 9: Summary Statistics for 1978 and 2003 for White Women

	<u>1978</u>		<u>2003</u>	
	Mean	Std. Dev.	Mean	Std. Dev.
<i>Family Formation</i>				
Birth Rate	15.04	3.03	12.88	3.21
Out of wedlock births	0.09	0.02	0.28	0.05
Low birth weight	0.06	0.01	0.07	0.01
Single mother	0.13	0.04	0.27	0.05
Married	0.65	0.05	0.52	0.05
Married before	0.14	0.03	0.19	0.03
<i>Labor Market</i>				
Employed	0.59	0.05	0.70	0.05
In labor force	0.63	0.05	0.74	0.05
Living in poverty	0.09	0.03	0.13	0.04

The sample for the outcome variables is white women ages 18–40 from the CPS, with the exception of the first three family formation outcomes, which use the universe of white women from the Vital Statistics. The mean of the outcome variables across states indicates the share of individuals who are classified under that specific outcome. The birth rate is the number of births to white mothers per 1,000 white individuals. The denominator for the share of women who are single mothers is all of the women who are mothers. The denominator for the share of women who have been married before is all of the women who have ever been married.

Table 10: OLS Regression of Family Formation Outcomes on Incarceration Rate

	Vital Statistics			CPS		
	(1) Birth Rate	(2) Out of Wedlock Births	(3) Low Birth Weight	(4) Single Mothers	(5) Married	(6) Married Before
Incarceration rate	-0.002 [0.125]	-0.016 [0.021]	0.007 [0.007]	0.072 [0.069]	0.032 [0.079]	0.090 [0.070]
Mean of Dep. Variable	2.88	0.58	0.12	0.58	0.33	0.37
Observations	1350	1325	1350	1256	1317	1268

Standard errors are clustered by state. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS or all black women in the Vital Statistics data. The incarceration and birth rate are both logged. The denominator for the share of women who have been married before is all of the women who have ever been married. The denominator for the share of women who are single mothers is all of the women who are mothers.

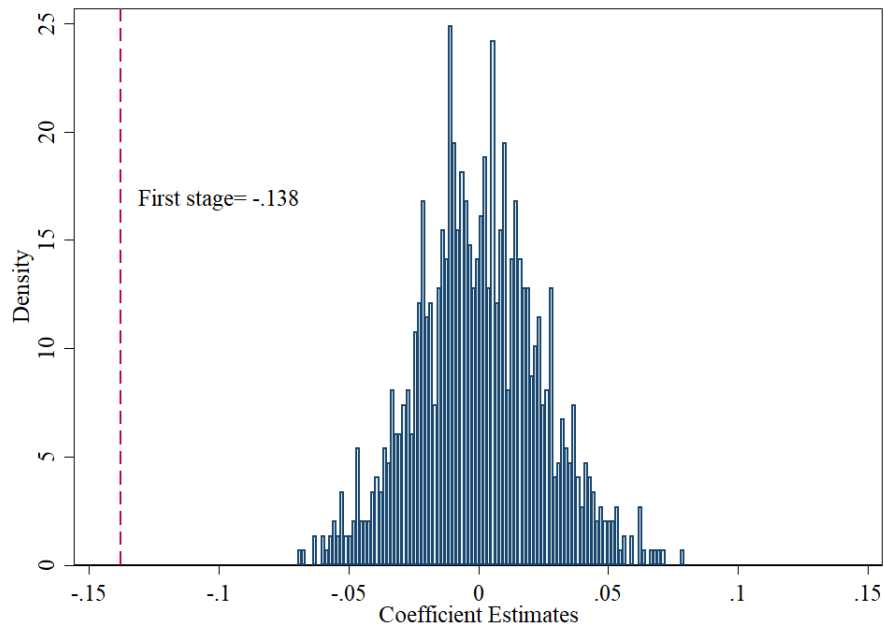
Table 11: OLS Regression of Labor Market Outcomes on Incarceration Rate

	(1) Employed	(2) Labor Force	(3) In Poverty
Incarceration rate	0.021 [0.071]	-0.031 [0.060]	0.084 [0.059]
Mean of Dep. Variable	0.61	0.72	0.30
Observations	1317	1315	1317

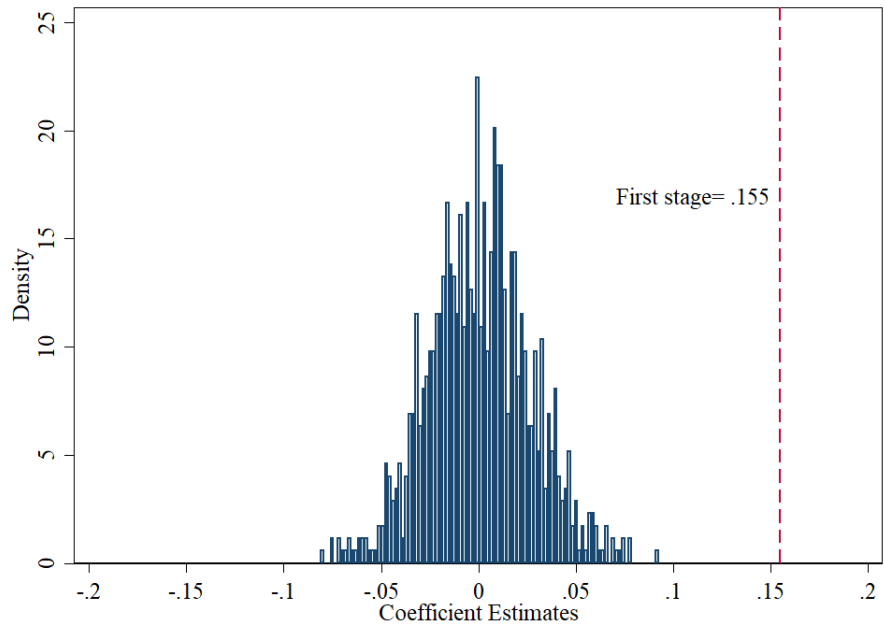
Standard errors are clustered by state. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS or all black women in the Vital Statistics data. The incarceration rate is logged.

Figure 24: Permutation Test Histograms of First Stage Coefficient Estimates from 1000 Draws of Randomly Assigned Policy Changes

(a) Determinate Sentencing



(b) Weapons Mandatory



Note: The figure shows the histogram that comes from a permutation test in which, for any given iteration, I randomly assign each state the year in which the policy changes. I restrict the possible years I assign to the time frame during which each policy was passed: 1977–1986 for the weapons mandatory and 1977–2000 for determinate sentencing. The figure shows the histogram of the first stage coefficient from 1000 iterations. The vertical red line shows the location of the true coefficient from the first stage regression.

Table 12: 2SLS Regression of Women's Outcomes on Incarceration Rate Using Three Instruments
(including HOL for violent offenses)

	Vital Statistics			CPS					
	(1) Birth Rate	(2) Out of Wedlock Births	(3) Low Birth Weight	(4) Single Mothers	(5) Married	(6) Married Before	(7) Employed	(8) Labor Force	(9) Poverty
Incarceration rate	0.137 [0.295]	0.028 [0.048]	0.019 [0.015]	0.052 [0.207]	-0.050 [0.120]	0.081 [0.174]	0.327* [0.172]	0.271** [0.135]	-0.180 [0.128]
F-statistic	20.12	19.00	20.12	16.78	18.64	16.99	18.64	18.75	18.64
Mean of Dep. Var.	2.91	0.58	0.12	0.58	0.33	0.37	0.61	0.71	0.30
Observations	1300	1275	1300	1159	1219	1172	1219	1217	1219

Note: Standard errors are clustered by state. The instruments are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS or all black women in the Vital Statistics data. The incarceration rate and birth rate are both logged. The F-statistic is from a test on the first-stage instruments.

Table 13: 2SLS Regression of Women's Outcomes on Incarceration Rate Using Four Instruments
(including HOL for Violent Offenses and Sentencing Commissions)

	Vital Statistics			CPS					
	(1) Birth Rate	(2) Out of Wedlock Births	(3) Low Birth Weight	(4) Single Mothers	(5) Married	(6) Married Before	(7) Employed	(8) Labor Force	(9) Poverty
Incarceration rate	0.095 [0.259]	0.017 [0.040]	0.013 [0.013]	-0.103 [0.173]	-0.014 [0.114]	-0.019 [0.186]	0.343** [0.170]	0.255** [0.126]	-0.199* [0.118]
F-statistic	19.08	17.98	19.08	16.43	17.64	16.69	17.64	17.80	17.64
Mean of Dep. Var.	2.91	0.58	0.12	0.58	0.33	0.37	0.61	0.71	0.30
Observations	1300	1275	1300	1159	1219	1172	1219	1217	1219

Note: Standard errors are clustered by state. The instruments are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS or all black women in the Vital Statistics data. The incarceration rate and birth rate are both logged. The F-statistic is from a test on the first-stage instruments.

Table 14: 2SLS Regression of Women's Outcomes on Male Incarceration Rate

	Vital Statistics			CPS					
	(1) Birth Rate	(2) Out of Wedlock Births	(3) Low Birth Weight	(4) Single Mothers	(5) Married	(6) Married Before	(7) Employed	(8) Labor Force	(9) Poverty
Incarceration Rate	0.011 [0.289]	0.030 [0.052]	0.018 [0.016]	0.024 [0.207]	0.018 [0.128]	0.038 [0.181]	0.340** [0.173]	0.292** [0.133]	-0.207 [0.144]
F-statistic	20.12	17.09	20.12	18.58	20.78	18.61	20.78	20.75	20.78
Mean of Dep. Var.	2.91	0.58	0.12	0.58	0.33	0.37	0.61	0.71	0.30
Observations	1300	1275	1300	1159	1219	1172	1219	1217	1219

Note: Standard errors are clustered by state. The male incarceration rate is the logged number of male inmates per every 100,000 male residents. The two instruments are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS or all black women in the Vital Statistics data. The incarceration rate and birth rate are both logged. The F-statistic is from a test on the first-stage instruments.

Table 15: 2SLS Regression of Women's Outcomes on Incarceration Rate With Linear Specification

	Vital Statistics			CPS					
	(1) Birth Rate	(2) Out of Wedlock Births	(3) Low Birth Weight	(4) Single Mothers	(5) Married	(6) Married Before	(7) Employed	(8) Labor Force	(9) Poverty
Incarceration Rate	0.040 [0.148]	0.028 [0.027]	0.010 [0.008]	0.053 [0.097]	0.000 [0.061]	0.032 [0.092]	0.154** [0.078]	0.153** [0.067]	-0.068 [0.064]
F-statistic	10.15	8.91	10.15	9.02	9.97	9.25	9.97	9.97	9.97
Mean of Dep. Var.	2.91	0.58	0.12	0.58	0.33	0.37	0.61	0.71	0.30
Observations	1300	1275	1300	1159	1219	1172	1219	1217	1219

Note: Standard errors are clustered by state. The incarceration rate is the number of inmates per every 1,000 residents. The two instruments are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS or all black women in the Vital Statistics data. Unlike previous tables, the birth rate is not logged. The F-statistic is from a test on the first-stage instruments.

Table 16: Weighted 2SLS Regression of Women’s Family Formation Outcomes on Incarceration Rate

	Vital Statistics			CPS					
	(1) Birth Rate	(2) Out of Wedlock Births	(3) Low Birth Weight	(4) Single Mothers	(5) Married	(6) Married Before	(7) Employed	(8) Labor Force	(9) Poverty
Incarceration rate	0.412 [0.502]	0.030 [0.052]	-0.012 [0.009]	-0.005 [0.130]	0.021 [0.069]	0.086 [0.105]	0.041 [0.078]	0.116 [0.078]	-0.051 [0.081]
F-statistic	19.21	17.90	19.21	19.35	19.30	18.93	19.30	19.33	19.30
Mean of Dep. Var.	2.94	0.65	0.13	0.62	0.29	0.38	0.60	0.70	0.31
Observations	1300	1275	1300	1159	1219	1172	1219	1217	1219

Standard errors are clustered by state. The two instruments (determinate sentencing and the weapons mandatory) are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is black women ages 18–40 from the CPS or all black women in the Vital Statistics data. The incarceration and birth rate are logged. The denominator for the share of women who have been married before is all of the women who have ever been married. The denominator for the share of women who are single mothers is all of the women who are mothers. The F-statistic is from a test on the first-stage instruments.

14 Appendix E: Comparison with White Women

Mass incarceration disproportionately affected black men and black communities, so in principle, I expect higher incarceration rates to have a bigger effect on black women than on white women. As such, I might think that a natural placebo check would be to run the same analysis, but considering the outcomes of *white* women.

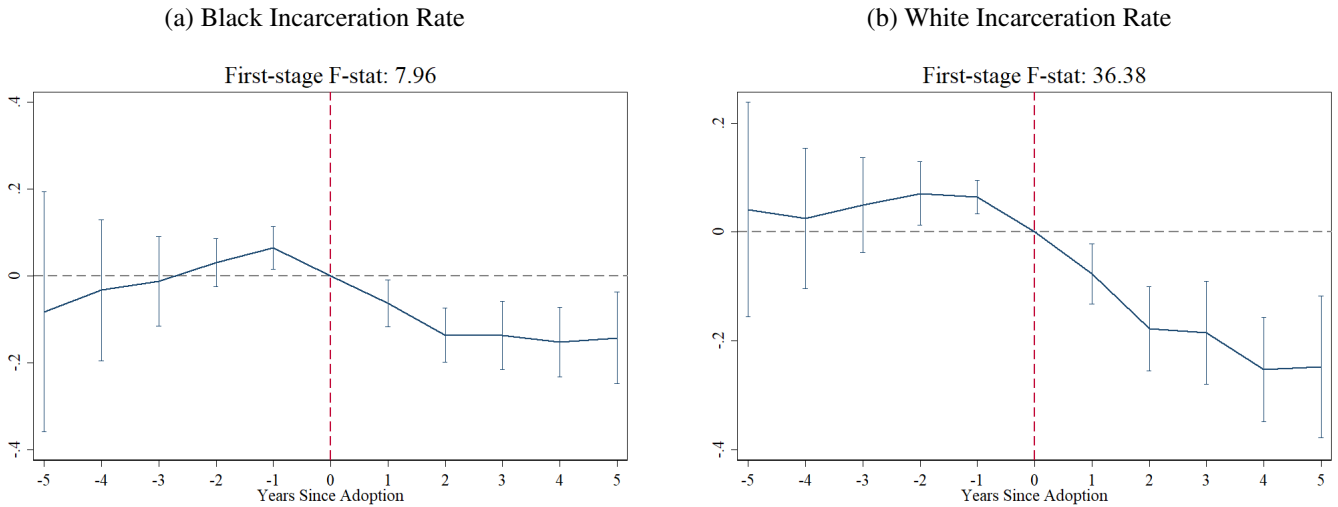
However, before doing so, I need to consider whether the policies that I use as instruments affect black communities more so than they affect white communities once I take into account state and year fixed effects as well as state-specific trends. In other words, if the policies also affect the growth in the white incarceration rate, then white women are not necessarily a suitable “placebo” group for whom I would expect a zero effect.

To test this idea, I consider the white incarceration rate (which includes Hispanic individuals) and the black incarceration rate separately.³⁵ I then estimate the first stage relationship between the each instrument and the black and white incarceration rates. Event study plots as well as the F-statistics from the first stage relationship are shown in figures 25 and 26.

These figures seem to suggest that the adoption of determinate sentencing decreased the white incarceration rate more than it decreased the black incarceration rate (although I cannot conclude that the effects are statistically different from each other). Similarly, I cannot conclude that the adoption of the weapons mandatory increased the black incarceration rate more than it increased the white incarceration rate. Given the relatively comparable effect of the policies on the black and white incarceration rates, I conclude that white women are not necessarily a reasonable placebo group.

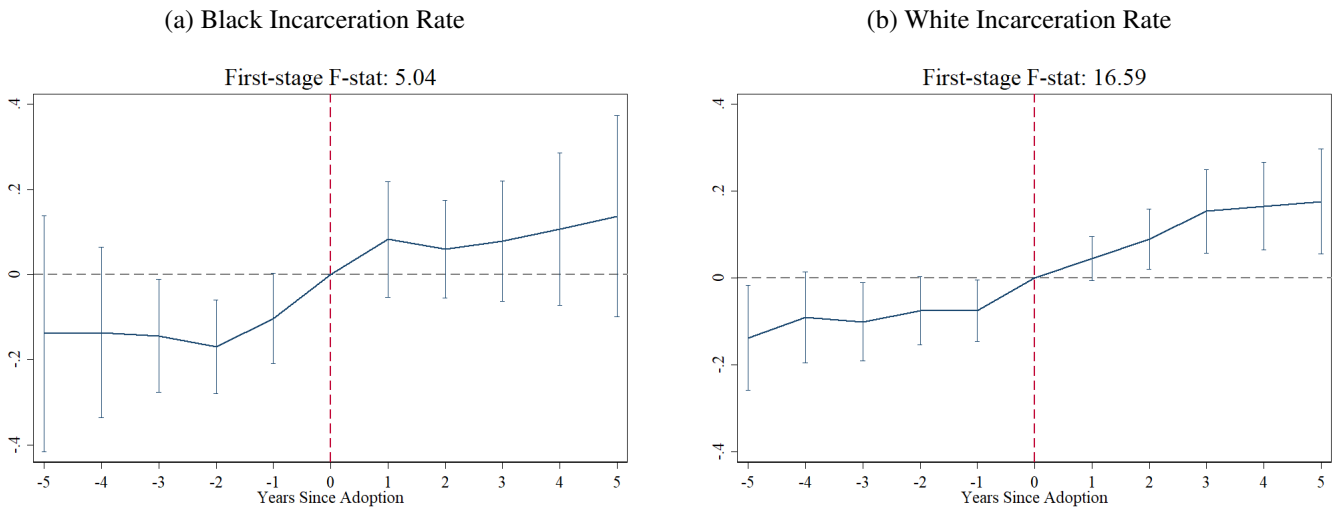
³⁵ Inmates can also be classified into a number of other categories, including Asian, American Indian or Alaska Native, and Native Hawaiian or other Pacific Islander, among others. I exclude Alaska and Hawaii from this part of the analysis since a large number of inmates are classified as “American Indian or Alaska Native” and “Native Hawaiian or other Pacific Islander,” respectively. It is also worth noting that since race classification and determination varies across states, these race-level incarceration rates are much more prone to suffering from measurement error. Specifically, many inmates are classified into categories that do not specify the inmate’s race (e.g., two or more races, unknown race, additional/other category). These inmates are excluded from this part of the analysis.

Figure 25: Event Study Plots of Incarceration Rate Relative to Adoption of Determinate Sentencing



Note: Standard errors are clustered by state. The endpoints are binned at five years before and after the adoption of the law. The two figures exclude Alaska and Hawaii. All specifications include state and year fixed effects as well as state-specific linear time trends.

Figure 26: Event Study Plots of Incarceration Rate Relative to Adoption of Weapons Mandatory



Note: Standard errors are clustered by state. The endpoints are binned at five years before and after the adoption of the law. The two figures exclude Alaska and Hawaii. All specifications include state and year fixed effects as well as state-specific linear time trends.

For completeness' sake, however, I still run consider the effect of higher incarceration rates on white women. Intuitively, even though these policies might have had a comparable effect on the white incarceration rate, there is always the possibility that white women responded differently than black women to higher incarceration rates.³⁶ To make this comparison, I focus on the two outcomes for which I found statistically significant results for black women.

The estimates are presented in table 18. I see that both coefficients are significantly smaller and even different in sign than the corresponding coefficients for black women. These results provide some evidence that the statistically significant results I found for black women are capturing some of their response to higher incarceration rates. However, it remains unclear why black and white women would react differently (in terms of labor market decisions) to higher incarceration rates and understanding this difference will need to be explored further in future research.

Table 17: 2SLS Regression of Outcomes White Women on State Incarceration Rate

	(1) Employed	(2) Labor Force
Incarceration rate	0.0059 [0.0286]	0.0016 [0.0309]
F-statistic	18.99	18.99
Mean of Dep. Variable	0.68	0.73
Observations	1300	1300

Standard errors are clustered by state. The two instruments (determinate sentencing and the weapons mandatory) are lagged one period. All regressions include state and year fixed effects as well as state-specific linear time trends. The sample used to construct the state-level panel is white women ages 18–40 from the CPS. The incarceration rate is logged. The F-statistic is from a test on the first-stage instruments.

³⁶ For example, if white women had more financial resources than black women, then they might have chosen to enroll in school, as opposed to finding a job, in order to increase their economic independence.