The Effects of Increased Sentencing Severity on Fertility and Family Formation*

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Abstract

The United States' incarceration rate has quintupled since 1970. By 2007, one percent of the adult population was incarcerated, and over 90% of those incarcerated were men. Since incarcerated men are physically separated from their communities, their absence may affect those who are left behind. Using the change in incarceration caused by a sentencing reform in North Carolina, with an intensity of treatment research design, I show that incarceration policies have spillover effects on family formation patterns. In the wake of the policy change, unmarried and young Black women reduce their fertility, and the composition of births shifts towards women of higher socioeconomic status. At the same time, I find that among those who gave birth, the quality of partner matches declines. White women are less likely to be married, but there is no effect on marriage rates of Black women.

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1 Introduction

Family formation patterns have important consequences for social inequality and intergenerational mobility. If and when a woman has children affects her labor force participation, hours worked, and earnings (Angrist and Evans, 1998; Jacobsen et al., 1999; Lundborg et al., 2017). The family structure and circumstances around a child's birth are also important determinants of the resources available to him in childhood with far-reaching consequences for his life (Lundberg et al., 2016). Economic theory, starting with Becker's (1981) seminal marriage model, shows that a determining factor of these patterns is the ratio of men to women in a community. When the male-female ratio decreases, there are fewer men available to form relationships. This increased scarcity can also affect relationships that still form by encouraging women to accept lower quality partners.

Many American communities have seen large changes in the ratio of men to women due to mass incarceration. Since 1970, the United States' incarceration rate has more than quintupled, reaching a peak of one percent of adults in 2007 (Kaeble and Glaze, 2016). As incarceration is most common for men, this has important compositional effects. Over 90% of prisoners in state and federal facilities are men, and Black men are four to five times more likely to be incarcerated than their white counterparts. Incarceration is also concentrated among young men (Travis et al., 2014). Because of this age gradient, incarceration incapacitates (removes from the community) men during the "demographically dense" period of their lives, when people are most likely to partner and have children (Rindfuss, 1991). Male incarceration may also disrupt these processes for women in their partner market,[1] with important consequences for family formation. Weaker family structure is associated with lower levels of upward mobility – not only for the children of single parents, but for all children in the community (Chetty et al., 2014). I exploit changes in incarceration driven by a state sentencing reform to understand how these changes to the sex ratio affect communities.

[1] I use the term partner market, as opposed to the more traditional term marriage market, because I will also include nonmarital relationships in the analysis.

Many factors influence incarceration, including crime and enforcement practices, which complicates the identification of the effect of increased male incarceration on women's family formation patterns. In this paper, I leverage changes in incarceration levels driven by the North Carolina Structured Sentencing Act (NCSSA). Enacted in October 1994, this policy increased the severity of criminal sentences, and the state's incarceration rate quickly grew in response.[2] Over the next year, the number of men incarcerated per prime-age Black (white) woman increased by 60% (40%).[3] I employ an empirical design that leverages exogenous variation in a woman's level of exposure to this policy change across partner markets. Using administrative data from the North Carolina Department of Public Safety and the State Center for Health Statistics with the 1990 and 2000 Censuses, I find the NCSSA reduced the birth rates of affected women. This effect is driven by Black women under age 25 and unmarried women. However, I find no observable effect on total competed fertility at later ages, implying that the observed reduction in fertility for young women is a *delay*. For women who continue to give birth, they are doing so with older and less educated or less committed partners. The NCSSA also reduced the probability of being married for white women.

My work speaks to a broad literature in economics on the effect of sex ratios. Previous studies have found evidence in support of Becker's theoretical predictions using war-time mobilization and mortality (Abramitzky et al., 2011; Bitler and Schmidt, 2011; Bethmann and Kvasnicka, 2012; Brainerd, 2017) or immigration inflows (Angrist, 2002; Lafortune, 2013) as exogenous shocks to sex ratios. However, the selection into and stigma associated with incarceration are different than that of military service or immigration. These effects are important in light of Wilson's (1987) "marriageable men" hypothesis, which suggests there is a quality threshold men must achieve before women will consider them marriageable.[4] If

[2] This policy is typical of policies passed by states in the 1980s and 1990s that were designed to make criminal sentences more punitive by increasing the length of time offenders spent in prison (Travis et al., 2014).

[3] Author's calculations using National Prisoner Statistics and Surveillance, Epidemiology, and End Results Program (SEER) population data.

[4] Empirical work has found partial support for this hypothesis, particularly for the most economically disadvantaged (Ellwood and Crane, 1990; Oppenheimer et al., 1997; Ruggles, 2015). See Autor et al. (2018) and Kearney and Wilson (2017) for examples of recent work motivated by this hypothesis.

incarcerated men are so negatively selected, their incapacitation may not affect the family formation patterns of women in their partner market. Alternatively, previously incarcerated men experience stigma across social settings (Braman, 2004; Pager, 2008). If a history of incarceration makes a man unmarriageable, increases in the incarceration rate may affect family formation beyond their contemporary effect on the sex ratio. These confounding effects may explain why previous studies of the impact of incarceration on family formation have not found consistent results across settings. Using variation in drug enforcement, Charles and Luoh (2010) find increased male incarceration decreases a woman's probability of being married for Black and white women. Focusing only on Black women, Mechoulan (2011) does not find evidence of an effect on marriage when using variation in incarceration rates across states over time. Neither study looks directly at the fertility of adult women or cohabitation, although Mechoulan finds some evidence of a reduction in Black teen births.

I contribute to this literature in several ways. First, I expand our understanding of the broader consequences of mass incarceration by focusing on a different and unexplored type of policy variation: a state sentencing reform. State sentencing reforms were important drivers of the growth in incarceration that occurred between 1990 and 2000 (Travis et al., 2014). I show that the NCSSA quickly increased the incarceration rate solely by lengthening the time offenders served in prison, while other potentially confounding factors were unchanged. This setting provides a natural experiment to isolate the incapacitation effects of incarceration apart from the selection and stigma effects discussed above. Second, I extend previous analyses beyond marriage and provide a comprehensive investigation of how changes in partner markets affect women's fertility patterns. There is extensive qualitative (Edin and Kefalas, 2011) and quantitative (Lundberg et al., 2016) evidence that women are increasingly making their decisions around fertility and marriage at different times in their lives. Focusing solely on marriage ignores an important margin of adjustment. Third, I show that incarceration changes the distribution of fathers. Since these fathers were not incarcerated at the time of conception,[5] this documents spillover effects beyond those directly affected by incarceration. Also, paternal quality is an important contributor to children's outcomes (Aizer et al., 2018). Finally, I observe differences in marriage outcomes by race: evidence consistent with Wilson's marriageable men hypothesis for Black women and evidence more consistent with Becker's bargaining effect for white women.

The rest of the paper proceeds as follows: section 2 discusses the policies and institutional factors that led to this sharp increase in incarceration rates. In section 3 I detail the data used in the analysis; section 4 discusses the empirical strategies I use to estimate the effects of this change. Finally, I present results in section 5 and conclude in section 6.

2 Policy Background

This paper leverages variation from a 1994 sentencing reform in North Carolina, the Structured Sentencing Act (NCSSA), to understand how increased incarceration affects family formation patterns. In this section, I describe North Carolina's previous sentencing framework, the political and institutional factors that led to the passage and quick implementation of the NCSSA, and the effects of the NCSSA on incarceration in North Carolina.

State sentencing reforms were an essential component of the policy landscape that contributed to the historic increase in incarceration rates in the United States. In the 1970s, initial reforms focused on increasing the consistency of sentences imposed by judges. Critics of the previous system, called "indeterminate sentencing", claimed that the lack of strict guidelines opened the door for racially disparate or otherwise arbitrary sentences. Beginning in the late 1980s, reforms focused on increasing the severity of criminal sentences, usually through sentencing laws that increased the time served per offense. A reaction to historically high crime rates, these reforms were a driving force behind increasing incarceration rates in the 1990s (Travis et al., 2014). The NCSSA was typical of reforms pursued by states in this later period. However, due to institutional and political factors unique to North Car-

[5] The North Carolina prison system does not allow conjugal visits (Division of Prisons, 2010).

olina, the NCSSA led to a sharp change in the state incarceration rate, whereas other states experienced smoother increases throughout the 1990s.

2.1 The Fair Sentencing Act (the Pre-NCSSA Policy)

North Carolina's first attempt at sentencing reform, the Fair Sentencing Act (FSA) of 1979, was designed to move the state towards "determinant sentencing" by constraining the discretion of judges. The FSA attempted to achieve this by including an exact recommended sentence length for each class of felonies, called a "presumptive" sentence, intended to function as a default sentence. However, the law made it easy for judges to deviate from this default by listing either mitigating[6] or aggravating[7] factors in the written decision. Further, the guidelines included beyond the presumptive sentence were very broad.[8] In practice, judges still had wide discretion in sentencing (Markham, 2014).

Adding more uncertainty to sentence lengths, most prisoners could receive one day of "Good Time" credit against their sentence for every day without a major disciplinary infraction (Markham, 2014). Additionally, North Carolina still had a system of discretionary parole, which most offenders qualified for after serving one quarter of their sentence. The legislature had hoped to introduce consistency through the FSA, but the actual time served and the legal presumptive sentence were often unrelated (Wright, 2002).

As can be seen in figure 1, the incarceration rate stayed relatively stable after the implementation of the FSA. However, this was a time of population growth in North Carolina, so the rate masks increases in the total prison population. Soon, the number of inmates in North Carolina prisons grew beyond their capacity, the number of inmates a prison can safely hold. In 1985, attorneys for state prisoners filed a class action lawsuit alleging that the extent of overcrowding in North Carolina prisons constituted cruel and unusual punish-

[6]Mitigating factors include limited mental capacity or aiding in the apprehension of another felon.

[7]Aggravating factors include previous offenses or gang membership. For a full list of aggravating and mitigating factors in North Carolina see North Carolina General Statutes §15A-1340.16.

[8]For example, the presumptive sentence for a Class D felony (which includes crimes like first-degree burglary and arson) was 12 years. However, a judge could sentence the offender to a maximum of 40 years if she included a description of any aggravating factors in her written decision (Markham, 2014).

ment. This case was not settled until 1988, but in 1985 the legislature passed the Emergency Powers Act which gave parole boards additional power to release prisoners before the end of their sentences.(Wright, 2002).

2.2 The Structured Sentencing Act

Realizing the FSA had not brought determinacy to the legal system, the legislature created the North Carolina Sentencing and Policy Advisory Commission. The commission had a mandate to create a system for criminal sentences that improved consistency in sentencing and take into account the corrections resources available, particularly prison capacity, hoping to avoid repeating the circumstances that led to the state's overcrowding legal troubles (Wright and Ellis, 1993).[9]

The commission presented its final recommendations to the legislature in January 1993; they were adopted with minimal changes on July 24, 1993, with an enactment date of October 1, 1994 (Wright, 2002).[10] The NCSSA created a detailed sentencing grid based on offense type, severity, and previous criminal record. Unlike the FSA, judges could not give sentences outside of these narrower bands.[11] The grid specified a specific minimum sentence for an inmate before which they could not be released. The NCSSA also replaced the previous Good Time system with an "Earned Time" system, which was less generous to inmates. Additionally, the NCSSA abolished discretionary parole (Collins and Spencer, 1999).

As seen in figure 1, the NCSSA had a dramatic effect on the North Carolina prison system. After years of relative stability, the North Carolina incarceration rate increased by close to one third between 1994 and 1995, corresponding to the October 1994 enactment of the NCSAA, before stabilizing again. Figure 2 shows the entry and exit dynamics that caused this quick increase and return to stability. While there was clearly year-to-year variation in

[9]Twenty-five states crated a sentencing commission as part of the sentencing reform process during this time period (Neal and Rick, 2016).

[10]The NCSSA only applies to sentences for crimes committed on or after Oct 1, 1994. Inmates already in prison were not affected.

[11]A 1996 study by the Commission found that all sentences judges gave in 1995 for felonies committed after the enactment of the NCSSA were in accordance with the sentencing grid (Collins and Spencer, 1999).

entries into prison over this period, there was no trend break around the implementation of the NCSSA. Over this period, other characteristics about entrants (racial composition, age, felonies vs. misdemeanors, percent drug offenses, percent male) also remained stable (North Carolina Department of Public Safety, 1992-2000).

The increase in the prison population was caused by a decrease in exits. Time served by prisoners in North Carolina sharply increased after 1994, doubling from an average of 8.7 months for prisoners who entered in 1994 to 17.5 months for prisoners who entered in 1996 (figure 3). As the prison system adjusted to the new sentence lengths, exits temporarily diverged from entrances. By the end of the 1990s, entrances and exits converged again and the North Carolina prison system entered a new steady state level of incarceration. Although the incarceration rate increased for all groups after the enactment of the NCSSA, the increases for Black and white men occurred on different absolute scales. The incarceration rate for white men is consistently and significantly lower than that of Black men before and after the enactment of the NCSSA (figure A2).

The North Carolina incarceration rate drastically increased between 1994 and 1995 as a direct result of inmates serving longer sentences in accordance with the sentencing guidelines provided by the NCSSA. In section 4.1, I will discuss other potential confounders and show they are not driving the increase in incarceration rates. I use this policy change as natural experiment to understand the effects of increased incarceration on fertility and family formation.

3 Data

I combine data from a variety of sources to estimate the effect of an increase in sentencing severity on fertility and family formation. In this section, I will describe the outcomes I examine and their sources. Then, I will detail the incarceration measures I use to capture women's exposure to the NCSSA. Finally, I will discuss additional data used as covariates in the primary analyses.

Fertility and birth related outcomes are from the North Carolina Detailed Birth Database. These files contain information derived from birth certificates for the universe of births occurring in North Carolina between 1989 and 2014. I limit my sample to women ages 15 to 40 who reside in North Carolina. I only include Black and white mothers in my sample, who make up 96.8% percent of mothers in North Carolina from 1990 to 2000. These data include extensive information on the birth and on the mother, including her age, race, and county of residence. Most records include information on the completed weeks of gestation; in those cases, I estimate the time of conception by taking the date of birth minus the number of weeks reported. When that information is missing, I impute the time of conception as occurring nine months before the birth and use the respective year. I also use this sample to look at reported father characteristics and the composition of mothers. Data on fathers include his age, race, and education. To compare total fertility across cohorts, I supplement this data with the National Center for Health Statistics Natality Detail Data. These data are functionally identical to the North Carolina Detailed Birth Data but are available beginning in 1968.

Marriage data are from the 1990 and 2000 public use microdata samples (IPUMS-USA). By using these data, I can include cohabitation as an outcome of interest, an increasingly important and common option for couples looking to form households (Lundberg et al., 2016). I limit my sample to Black and white women in North Carolina ages 20 to 40.

The data used to measure a woman's partner market's exposure to the NCSSA are from the North Carolina Department of Public Safety (DPS). To create a measure of incarceration by age, race, and county, I use public offender information from the universe of convictions in North Carolina. These data are available beginning in 1972. This database includes information on the type of sentence (prison or probation) and time served, as well as offender characteristics, such as date of birth, race, sex, and the county of conviction.

I restrict my sample to white and Black men ages 15 to 44.[12] I observe when the offender began his sentence and can count the number of men within an age and race group who were in prison from each county over the course of a calendar year. I follow the procedure for calculating the prison population used by DPS. An individual is counted in prison in the first (second) half of the year if his entry date is before June 30 (December 31) and his exit date is after. I then use the offender characteristics to calculate the population by age, race, and geographic groups.

To create a measure of women's exposure to male incarceration, I divide the prison population by the same age and race female population, using population data from the Surveillance, Epidemiology, and End Results Program (SEER). I use the female population for two reasons. First, since an important mechanism by which incarceration affects partner markets is by skewing the sex ratio, the number of men incarcerated per woman more directly captures this than the traditional incarceration rate. Second, this reduces potential measurement error arising from the fact that the incarcerated are considered part of the population in the county in which they are incarcerated, not the county they lived in before incarceration. This will artificially inflate the incarceration rate in areas where many incarcerated men come from and artificially reduce it in areas with prisons. North Carolina has a relatively decentralized prison system, with over 80 facilities in the early 1990s. However, women are incarcerated at a much lower rate, so any measurement error from misassignment is smaller.[13]

I also use information on crime rates, police efficacy, and unemployment in my primary analyses to capture additional time varying factors that could influence partner markets, discussed further in section 4.1. Data on the crime rate, defined as the number of offenses known to police divided by the population, and the clearance rate, the number of clearances divided by the total number of offenses known to police, are from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) program. The FBI considers a crime

[12] I observe too few men of other races to create consistent series for those groups.

[13] Additionally, I exclude the two CZs with a women's correctional facility as a robustness check.

"cleared" if at least one person has been arrested, charged and turned over to the court or if the offender has been identified but exceptional circumstances, such as their death, prevents the agency from arresting and charging the individual. Data on unemployment are from the Bureau of Labor Statistics' (BLS) Local Area Unemployment Statistics.

The above series are defined at the county level. For analysis, I aggregate them to the commuting zone (CZ) level following the USDA 1990 CZ definitions. To estimate the effect of the NCSSA by marital status, I use data from the 1980 through 2000 IPUMS-USA Census samples to create a population series by sex, age, race, and marital status. I allocate the Census Public Use Microdata Area (PUMA) to CZs using the procedure in Dorn (2009). After obtaining the census year estimates of the number of married and unmarried women, I use linear interpolation to create a series over the entire period. However, my results are not dependent on this interpolation. I find similar results when restricting my sample to the 1990 and 2000 census years, discussed in section 5.4.

Summary statistics for the sample used in this analysis are available in tables A1 and A2.

4 Specification

Because this policy is applied to the entire state at once, I cannot invoke a traditional differences-in-differences estimation strategy. Instead, I leverage the fact that the NCSSA had a much larger affect on partner markets with higher pre-period incarceration rates. This empirical approach is often called an "intensity of treatment" research design. Other examples include Acemoglu and Johnson (2007), Bleakley (2007), and Lucas (2010). The intuition behind this approach is that the introduction of a policy that lengthens prison sentences will have a stronger impact in communities where a high portion of men receive prison sentences. As a pseudo first-stage, I show pre-period incarceration rates are a strong predictor of incarceration later in the period (table A3).

Importantly, the NCSSA did not change the rate at which people entered prison. Instead, the resulting increase in the incarceration rate was caused by prisoners serving longer sentences. In areas where few men are incarcerated, the increase in time served affected a small portion of men. Consequentially one would not expect to see a large effect of the policy change in partner markets where few men were ever incarcerated. Moreover, within areas with higher incarceration rates, the strong age gradient in incarceration patterns suggests that the effects of the policy should be much more pronounced among younger women.

Consistent with previous work, I define a woman's partner market to be men of her same race, in the same geographic area, who are her age or slightly older. Charles and Luoh (2010) confirm that marriages conform to this pattern in recent census years. Using North Carolina natality data, I can confirm this matching pattern for births as well. Only one to two percent of births report the father's race to be different than the mother's race (figure A3). Like previously documented patterns for marriage, women are most likely to have children with men who are slightly older, but still close in age. In the sample used for analysis, fathers are slightly older (by 2.3 to 2.4 years) than mothers (figure A4). I am able to define partner markets at a sub-state level, improving upon prior work which assigned partner markets at the state level (Charles and Luoh, 2010; Mechoulan, 2011).

My analyses are based on commuting zones (CZ). CZs are units of analysis designed to reflect observed patterns of economic and social activity. Market (or other) relationships are not bound by the nearest county line, but often form across these boundaries. By using the CZ as the geographic level in my analyses, I can better account for spillovers between neighboring counties.[14] For a map of North Carolina counties and CZs, see figure A5. Additionally, aggregating from counties to CZs causes the effective number of observations within my clusters will be more similar across clusters, improving the quality of inference (MacKinnon and Webb, 2017). Performing the analysis at the CZ level also allows me to estimate subgroup populations by marital status, as discussed in the previous section; the data needed to produce those estimates is not available at the county level.

[14]See Lindo (2015) for a discussion of the importance of accounting for these spillovers.

I collapse my data into cells based on the woman's race, age, CZ of residence, and the time period of conception. I model births using the following equation:

$$E[Y_{\rho t}] = exp(\beta Post_t * \ln(\overline{IR}^{9093})_\rho + \theta \mathbb{X}_{\rho t} + \alpha ln(pop)_{\rho t} + \lambda_\rho + \gamma_t) \quad (1)$$

where ρ is the woman's partner market and t is the time period of conception. I divide each year into two periods (January through June and July through December). I exclude the six-month period around the introduction of the law (July through December 1994) because I cannot precisely assign births that were conceived in that period to before or after the policy change.

$Y_{\rho t}$ is the number of births to women in partner market ρ, conceived in time period t. Because I am looking at precisely defined groups of women, I sometimes observe zero births in a period, especially when looking at subgroups. Since the natural log is not defined at zero, a log-linear specification, common in papers examining fertility at the state level, is not appropriate here. Instead I estimate the number of births using a Poisson model which has the additional advantage that its estimate of the conditional mean is robust to model misspecification (Wooldridge, 1999). The main results are similar when using the natural log of the birth rate as the dependent variable and a linear functional form, as will be discussed in section 5.4.

$\overline{IR}_\rho^{9093}$ is the average proportion of men incarcerated per woman in a partner market, as defined above, from 1990-1993 (the years leading up to the passage of the NCSSA). β can be interpreted as an elasticity, similar to how one would interpret a linear regression on the natural log of the birth rate. Specifically it is the expected percent effect for a partner market with a pre-period incarceration exposure of 1%. The average pre-period male incarceration rate is just below that at 0.8%. I group women into five age groups: 15 to 19, 20 to 24, 25 to 29, 30 to 34, and 35 to 39. Consistent with prior work, I match women with men their age and slightly older (of their race who live in their CZ). For example, for women ages 20 to 24, $\overline{IR}^{9093}$ would be the pre-NCSSA incarceration rate of same race men ages 20 to 29 in their CZ of residence. This measure is then interacted with $Post_t$, which is an indicator variable equal to one when the period is after the enactment of the NCSSA.

λ_ρ is a fixed effect to capture time unvarying characteristics of the partner market. γ_t is a year fixed effect. All analyses allow standard errors to be be correlated within CZs overtime. Clustering the standard errors breaks the traditional link between mean and variance in Poisson specifications, so the data do not need to be equi-dispersed to satisfy the assumptions for consistency (Cameron and Trivedi, 2010). $\mathbb{X}_{\rho t}$ is a vector of time-varying controls for the crime rate, a measure of police efficacy, and the unemployment rate. I also include the natural log of the total female population for whom the outcome is measured as they are the population "at risk" of giving birth.

I also analyze how the NCSSA changed the composition of women entering in to motherhood. I focus on mothers' age, marital status, and educational attainment. Women who are older, who are married, and/or who have higher levels of education tend to have more resources available to invest in their children (McLanahan, 2004). In turn, these positive investments lead to improved child outcomes across a range of social and economic dimensions, such as health status, labor force participation, and criminal activity (Lundberg et al., 2016). Since average age is an outcome of interest here, I cannot define the cells at the age group level. I estimate:

$$Y_{\rho' t} = \beta Post * \ln(\overline{IR}^{9093}_{\rho' t}) + \theta \mathbb{X}_{\rho' t} + \lambda_{\rho'} + \gamma^R_t + \varepsilon_{\rho' t} \tag{2}$$

where ρ' is a race-CZ group and t is the time of conception. $\ln(\overline{IR}^{9093}_{\rho' t})$ is the race-CZ measure of pre-period incarceration, which is interacted with a variarable $Post$ to indicate when the NCSSA comes into effect. $\mathbb{X}_{\rho' t}$ is the same vector of time varying CZ characteristics discussed above. $\lambda_{\rho'}$ is a fixed effect to capture time unvarying characteristics of the partner market and γ_t is a year fixed effect. $\varepsilon_{\rho' t}$ is a random error term, clustered at the commuting zone level.

To understand the long-run effects of the NCSSA, I need to distinguish between a permanent reduction in the number of children to which women ever give birth (total fertility) versus a temporary reduction for younger women that they make up for at older ages. To estimate the effect of the NCSSA on total fertility. In the equation below, treatment is assigned to women based on their age in 1995, with κ corresponding to cohorts based on the age of women in 1995.

$$Y_{\kappa rc} = \beta \ln(\overline{IR}_{\kappa rc}^{9093}) + \omega_r + \omega_\kappa^r + \omega_c^r + \nu_{\kappa rc}, \tag{3}$$

where κ denotes the age cohort, r denotes race, and c denotes CZ. $Y_{\kappa rc}$ is either a measure of the portion of women who are childless or the total number of children born to women in a partner market. I follow the procedures used by Ananat et al. (2007) and Currie and Schwandt (2014) to create both measures. To capture the number of childless women, I add up all first births observed to women in a race and age cohort group in a CZ. This gives an estimate of the number of women who have ever had any children. Comparing this measure to the number of women in the cohort creates a measure of the fraction of women who are childless, the extensive margin of fertility. For the second measure, I add up all births observed to women from a cohort and divide that number by the number of women in the cohort to create an estimate of the number of children born per woman, the intensive margin. I calculate both of these measures when the women in the cohort are age 35 and again at age 40. While it would be preferable to measure completed fertility at a later age, these are the oldest ages for which I can observe for women who were teenagers and young adults in 1995.

$\overline{IR}_{\kappa rc}^{9093}$ is the same measure of exposure used in previous specifications, but now the treatment is assigned based on the woman's age in 1995. For example, a woman who was 16 in 1995 would be matched to her partner market's level of exposure to the NCSSA at age 16. ω_r is a race fixed effect, ω_κ^r is a race-specific age cohort fixed effect, and ω_c^r is a race-specific CZ fixed effect. $\nu_{\kappa rc}$ is a random error term, clustered at the CZ level.

I also examine how the change in incarceration policy changed the distribution of fathers.

Becker predicts that a change in sex ratio will affect both the number of unions that form and the quality of those matches. Unfortunately, birth certificates do not contain extensive information on fathers, but I am able to look at their age and education level. I also examine the probability of the father being missing from the birth record. For these regressions, I use a linear functional form:

$$Y_{\rho t} = \beta Post * \ln(\overline{IR}_{\rho t}^{9093}) + \theta \mathbb{X}_{\rho t} + \lambda_\rho + \gamma_t + \varepsilon_{\rho t}, \tag{4}$$

where ρ is the partner market and t is the time of conception. $Y_{\rho t}$ represents the cell level average of the reported age difference between mothers and fathers, the fraction of birth records in the partner-market cell where the father is reported to have fewer years of education than the mother, or the fraction of birth records in the partner-market cell where all information on the father is missing. $\mathbb{X}_{\rho t}$ is the vector of time varying CZ characteristics, λ_ρ is a fixed effect to capture time unvarying characteristics of the partner market, and γ_t is a year fixed effect. Regressions are weighted by the number of births in the cell.

Because the data on marital status are structured differently than the birth data previously discussed, I operationalize my empirical strategy differently to examine the effect of increased sentencing severity on the marital status of women in North Carolina. The intuition of the specification is the same. I estimate a modified version of the preferred specification in Charles and Luoh (2010):

$$\Delta Y_\rho = \beta \ln(\overline{IR}^{9093})_\rho + \theta \Delta \mathbb{X}_\rho + \mu_\rho. \tag{5}$$

ρ represents the partner market and $\ln(\overline{IR}^{9093})_\rho$ is the same measure of an age-race-CZ group's exposure to increased incarceration as a result of the NCSSA. I continue to use the same control variables for crime, police efficacy, and unemployment mentioned above, but now those time varying characteristics are included as the change between the census periods. ΔY_ρ is the change in the portion of women in a partner market who report being married, divorced, never married, or cohabiting at the time of the census. μ_ρ is the error term, clustered at the CZ level. ρ continues to denote the partner market, defined at the age, race, and CZ level.

My ability to identify the effect of an increase in the incarceration rate on family formation outcomes relies on the assumption that the changes in the incarceration rate are being driven by policy, not changes in the community that could lead to both more severe sentences and different family formation patterns. I discuss these potential confounders in more detail below.

4.1 Threats to identification

In the spirit of a differences-in-differences analysis, figure 4 plots the birth rate separately for commuting zones with pre-period incarceration rates above and below the median incarceration rate for 1990 through 2000. The blue vertical line between 1994 and 1995 indicates the enactment of the NCSSA in October 1994. While birth rates are reliably higher in commuting zones with above the median level of pre-period incarceration, the two sets of communities have visually similar trends in the 1990s.

One may worry that an outside factor was driving the increase in incarceration which could also affect family formation and fertility. In this section I discuss several potential factors, including crime, enforcement, and labor markets. The first potential threat to identification is a change in crime. Similar to the previous figure, figure 5 plots the North Carolina crime rate for two groups of CZs over my period of analysis with a vertical line to indicate the implementation of the NCSSA. To ensure the state-level rate does not mask relevant heterogeneity, I divide CZs into high and low incarceration areas depending on if they are above or below the median pre-period incarceration rate. Like the rest of the United States, North Carolina saw crime rates plateau in the mid-1990s, followed by a decrease (Lofstrom and Raphael, 2016). Importantly for this identification strategy, there is no spike around the implementation of the NCSSA, and the trends in crime are similar for both high and low incarceration areas.

Alternatively, one might worry that even if the level of crime stayed the same, the composition of crimes or defendants changed in a way the led to increased incarceration. The incarceration data I use is conviction based. This is only a subset of crimes adjudicated by the court system and will not include any information on cases where the defendant is not convicted. A contemporary report commissioned to understand the effect of the NCSSA on state courts compared a sample of cases from January to June 1994 (before the law was enacted) to a sample of cases from January to June 1996 (after the law was enacted). The report found no differences in the demographic composition of defendants or the fraction of cases that were felonies versus misdemeanors. The authors also report no effect on prosecutor charging behavior or in the percentage of cases going to trial (Collins and Spencer, 1999).

Given that previous work has focused on variation caused by changes in enforcement, I also examine this pathway. Figure 6 displays offense-specific arrest rates with a vertical line to indicate passage of the NCSSA. Arrest rates for violent offenses and drug-related offenses are remarkably stable over the period, while the arrest rate for property crimes smoothly decreases. Assuming arrests reflect the composition of crimes committed, we can also turn to the arrest data for assurance that the observed increase in incarceration is not driven by a change in the composition of crimes. The composition of arrests within major offense categories is also relatively consistent over time, and there are no sharp changes around the enactment of the NCSSA (figure A6). The empirical strategy used by Charles and Luoh (2010) cannot be meaningfully applied in this study.

Another possibility is that my results reflect changes in labor market conditions. Theories of criminal activity predict a tight labor market with low unemployment will lead to a decrease in crime and incarceration, suggesting North Carolina's falling unemployment rate is not the cause of the increase in incarceration seen here. However, empirical work has not established a clear relationship between labor markets and crime (Mustard, 2010). Figure 7 shows that, like much of the country, North Carolina experienced decreasing unemployment throughout most of the 1990s. Again dividing CZs into high and low pre-period incarceration groups, I see the trends in unemployment are very similar across areas with relatively high or low incarceration rates.

All of the potential confounders discussed above are measured with error and none are observed at a level as precise as the partner market. If included on the right hand side, a poorly measured confounder may not affect the regression coefficients because measurement error attenuates the confounder's effect, not because there is no effect. To account for this, I implement the left hand side balancing tests suggested in Pei et al. (2018). This takes advantage of the econometric fact that a regression is more robust to measurement error in the dependent variable than the independent variable. To apply this test in my setting, I estimate equation 4, using crime, clearance, and unemployment rates as dependent variables. Both individually and jointly, the introduction of the NCSSA is not a significant predictor of changes in these potential confounders. The results of these balancing tests are shown in table A4. The results are robust to the inclusion and exclusion of a vector of controls, as discussed in section 5.4.

Finally, if women are migrating in response to the change in the composition of their partner market, this will bias the results. To ensure that migration patterns are not correlated with pre-period partner market incarceration rates, I use the information on 5-year migration status in the 2000 census to see whether groups of women differentially exposed to the NCSSA through their partner market are differentially likely to migrate. I classify a woman as a migrant if she reports moving across public use microdata areas between 1995 and 2000. I regress this indicator on her partner market's pre-NCSSA incarceration rate. There is no observable relationship between women's migration patterns and partner-market incarceration rates (table A5).

I argue that I am able to separate the effects of the stigma of being an ex-convict from the incapacitation effect of physically removing men from their partner markets. However, one may worry that serving a longer sentence will provide a negative signal to potential partners, creating its own length-related stigma effect. While economists have not studied romantic effects specifically, previous research has studied the effect of incarceration on labor market outcomes, which may be informative here. Audit studies which compare call-back rates regularly find job candidates with otherwise identical resumes and qualifications are less likely to receive a call back with a criminal record (Pager, 2008). More recently, studies use the random assignment of cases to judges with different propensities to incarcerate defendants as a way to understand how incarceration affects later outcomes for individuals convicted of a crime. These studies find evidence of worse labor market outcomes on the extensive margin, results for the intensive margin are mixed. Kling (2006) finds that longer sentences are actually associated with increased earnings after release. Using a stronger instrument, Mueller-Smith (2015) finds an additional year in prison results in decreases earnings.

However, both of these identification strategies provide an estimate for the marginal potential inmate. In my setting, none of men whose prison sentences are extended by the NCSSA can be thought of as on the margin as they would receive prison sentences under both legal regimes. In one of the only studies to focus specifically on sentence length, Jung (2011) finds improved labor market outcomes for men who served a longer sentence. Ultimately, even if there is a secondary stigma effect of a longer prison sentence in partner markets, it is likely smaller than the effect of the extensive margin.

5 Results

This section presents the effects of increased sentencing severity on fertility, partner choice, and marriage. I begin with the discussion of fertility as previous work on incarceration and family formation has largely ignored this important potential mechanism of adjustment to a change in the sex ratio. Circumstances in utero and around the time of a child's birth have strong effects on later life outcomes (Almond and Currie, 2011). Understanding if and how the NCSSA changed fertility patterns is an important step to understanding the long-term consequences of increased sentencing severity.

5.1 Fertility

Figure 8 presents the effects of the NCSSA on births overall and by maternal age. The first point in the figure (the black circle) is the pooled effect. Each point to the left of the pooled effect represents the result of estimating equation 1 by age-race group. 95% confidence intervals are marked on the figure. The black dotted line also represents the pooled effect, for comparison. I find the NCSSA decreased fertility among women in partner markets with high levels of incarceration before the law was enacted. Specifically, the pooled coefficient on $Post_t * \ln(\overline{IR}_\rho^{9093})$ implies that a partner market with a pre-period incarceration rate of 1% would see the birthrate decrease 0.08%. Relative to a pre-period average birthrate of 7%, this is an absolute decline in the birthrate of 0.06 percentage points. Using the state-wide pre-period incarceration rate, this implies approximately 650 fewer babies were born in the year after enactment. This is small relative to the total number of births in North Carolina, but reasonable relative to the absolute increase in the male prison population of around 10,000 prime-aged males. This effect is much smaller than estimates of economic conditions on fertility (Ananat and Hungerman, 2012; Schaller, 2016). This effect is largest in magnitude for Black women under age 25, consistent with the demographic composition of incarcerated men. Since young, Black men have the highest rates of incarceration, one would expect any fertility spillover effects to be strongest among young, Black women.

Figure 9 shows the same analysis with the sample restricted to unmarried mothers. This is an important subgroup for two reasons. First, most incarcerated men are unmarried (Western, 2006), so it is unmarried women who should be the most affected by the policy. Second, women who give birth while unmarried, even if they are cohabiting at the time of the birth, are more likely to raise their children alone or with a subsequent partner. Both the absence of a second parent (McLanahan et al., 2013) and the upheaval from a series of partners (Cherlin, 2009) are associated with negative long-term effects for children. However, Finlay and Neumark (2010) find that for women whose marriage decisions were affected by the same shift in incarceration policies leveraged by Charles and Luoh (2010), never-married motherhood does not lead to poorer outcomes. As incarcerated men are less than half as likely to be married than non-incarcerated men, unmarried women and their potential children are still particularly important for understanding the intergenerational effects of incarceration policies.

The decline in fertility resulting from the NCSSA is stronger and more consistently negative among unmarried women. The overall result suggests that a partner market with an average incarceration rate of 1% before the policy will observe a 0.24% decrease in the birth rate of unmarried women, relative to a mean birth rate of 3.6%. The effects are not exclusive to Black women. Although incarceration affects a smaller percentage of white men, there is still a fertility spillover to white women. Table A2 shows that births to unmarried women comprise a small fraction of births to white mothers. This explains why the negative effect for unmarried white women was not observable in the overall effect on births. Full regression results for these figures as well as results for married women are available in tables A6 through A8.

Table 1 shows how the demographic composition of mothers changes in response to the NCSSA. Women who give birth after the enactment of the NCSSA are on average older and particularly less likely to be teens. In a CZ with the average level of pre-period incarceration for Black men, the point estimates imply that Black mothers are .025 years older and 1.4 percentage points less likely to be teens. Additionally, there is a shift away from mothers without a high school diploma, consistent with the educational profile of incarcerated men. This is analysis is repeated for the composition of women having their first birth with similar results (table A9).

It is clear the fertility of young and unmarried women declined after the enactment of the NCSSA. The long-term ramifications of this change depend on whether the decline in fertility was temporary or permanent. Specifically, did affected women postpone their childbearing until more partners were available or did this policy generate a permanent decline in fertility? To answer this, I estimate equation 3 to understand the relationship between a cohort's exposure to increased sentencing severity and total fertility outcomes by ages 35 and 40.

There is no statistically significant relationship between a cohort's partner market's exposure to the NCSSA and their later fertility outcomes, either on the extensive or intensive margins. Using the confidence intervals to bound the possible effect, I can reject an increase greater than .002 percentage points in the percent of women with no children at age 40 with 95% certainty. Looking at the intensive margin, I can reject a decrease in the number of children born per woman at age 40 larger than .005 fewer children per woman with 95% certainty (table A10). An important conclusion is that the observed decline in fertility generated by the NCSSA was likely temporary. The women whose partner markets were most affected simply delayed their childbearing into the future.

Delayed motherhood is associated with increased earnings (Miller, 2011). By having children later in their lives, these women have had more time to accumulate human capital and other resources which may contribute to an ability to make greater investments in their children (Bailey et al., 2014). However, the health effects of delay are more complicated, as both early and late childbearing are associated with worse birth outcomes (Royer, 2004). In this case, the observed decrease in births to teens is likely to lead to improved health outcomes for mothers and infants, while the overall increase in maternal age may be small enough to avoid adverse effects. However, the weathering hypothesis suggests that negative health effects of advanced maternal age occur earlier for Black women as a result of the cumulative effects of socioeconomic disadvantages (Geronimus, 1992). Love et al. (2010) find evidence that the incidence of negative birth outcomes for Black women increases monotonically after the early twenties. Since I observe the largest decreases for black women ages 20–24, these delays may lead to worse health outcomes for Black mothers and their children.

5.2 Partner choice

As discussed previously, a change in the male-female ratio may not just reduce the probability of finding a partner, it may also affect the types and quality of matches that form. Although there is limited information about fathers on the birth records, I can examine three dimensions of partner choice: relative age, relative education level, and if the father is listed on the birth record. Table 2 presents evidence that the NCSSA disrupted matching patterns for Black women. In Panel B, column (1) shows that the policy caused Black mothers to partner with relatively older men. Evaluated at the average pre-period incarceration rate for Black men this implies that the average age difference between couples increased by .007 years, a 0.3% effect relative to the pre-period mean. The direction of this effect is unsurprising as younger men are more likely to enter prison and thus were more likely to be taken out of the partner market by the NCSSA. This may be interpreted as a negative effect of this policy, since prior work has shown that women are most likely to search for partners close to their own age (Hitsch et al., 2010) and that women with older partners not only start out less satisfied with their relationships but that their relationship satisfaction declines more quickly (Lee and McKinnish, 2018).

Women also prefer to partner with men of a similar education level (Hitsch et al., 2010). Column (2), however, shows that the NCSSA also increased Black mother's probability of partnering with a man who had a relatively lower level of education 0.2% relative to the pre-period mean. In communities most affected by the NCSSA, therefore, Black women who continue to have children are more likely to match with lower quality partners. The NCSSA did not have an effect on the probability that the father was missing from the birth record from Black mothers.

The effects on partner choice are less clear for white women. Panel C of table 2 shows that while there is no observable change in partner composition, there is an increase in the probability the father is missing from the birth record. Evaluated at the pre-period mean rate of white male incarceration, this implies a relative increase in the probability of the father being missing from the birth record of 0.1%. Regardless of their "quality", men who are not on the birth record are less likely to be involved in the child's life or provide financial support (Argys and Peters, 2001). This suggests that white women who continue to have children after enactment are having those children with less committed partners and that those children will have fewer resources.

5.3 Marriage

Finally, I examine how increased sentencing severity affects women's marriage market outcomes, specifically her propensity to be married, divorced, never married, or cohabiting. These analyses include all women, not only mothers.

Results for these outcomes are reported in table 3. White women in partner markets that were most effect by the NCSSA are less likely to be married after enactment. This decline in marriage is not a result of an increase in divorce, but instead it is due to an increase in the percentage of women who have never been married. The effect I find is about one-third of the size of the effect found by Charles and Luoh (2010), who use variation in the incarceration rate due to increases in the number of men entering prison for drug related offenses during the War on Drugs. The variation I am leveraging here is driven purely by an incapacitation effect. The number and type of men entering prison is not changing, but the amount of time they spend behind bars is increasing. This implies that part of the effect previous work has observed is driven by the stigma effect – having a larger portion of men in the marriage market "marked" as an ex-convict alters marriage market outcomes beyond the direct effect of removing these men from the community.

Interestingly, most of these women are still forming unions. As can be seen in column (4), these women have shifted away from marriage into cohabitation. Like the previous result on "missing" fathers, this is additional evidence that the NCSSA is changing commitment levels in white women's relationships. Previous work has not examined the effects on cohabitation, so I cannot compare the effect sizes for this outcome. My work shows this is an important potential margin of adjustment.

There are no statistically significant effects on marriage or cohabitation for Black women. To provide insight into why these effects differ by race, recall the disparity in incarceration rates for white and Black men. The incarceration rate for primed-aged Black men is about six times larger than the rate for white men in North Carolina. While the incarceration rate for both groups roughly doubles after the NCSAA, the magnitude of the changes are different. Throughout the decade, the incarceration rate for white men is well below that of Black men. It is possible that the sex ratio in young Black women's partner markets is already so skewed that there is little room for additional adjustment on this margin.

This null effect for Black women is consistent with Wilson's marriageable men hypothesis: that there is a basic quality threshold that men must meet before women will consider them marriageable. In a partner market where many men are unemployed or involved in criminal activity the market will function as if those men were not present. If a history of serving a prison sentence moves a man into the unmarriageable group, then a policy that increases the number of men ever serving time will have a larger effect on marriage outcomes than one that merely affects the time served. However, women may be willing to enter non-marital relationships with men they would not marry, leading to effects on fertility due to the increased incapacitation effect of a longer sentence. Bridging the gap between my results and those of Charles and Luoh, Mechoulan (2011) uses an empirical strategy which draws from both types of variation and also finds no effect on marriage for Black women.

In summary, the increase in sentencing severity that followed the enactment of the NCSSA had a defined effect on women's fertility and family formation patterns. This policy led to decreased fertility, particularly among young and unmarried women. The NCSSA had divergent effects for women who delayed and women who continued to have children. There is no evidence that this policy changed completed fertility, implying that this decline was temporary for these women. By delaying their fertility after their teens and early twenties, these women may have had more resources to invest in the children they ultimately had

(Miller, 2011). However, for women who continued to give birth, they either partnered with observably worse men or were more likely to have no father listed on the birth record. For white women, the policy effects extended to marriage markets as well. White women in more exposed marriage markets are less likely to be married and more likely to be cohabiting at the end of the decade. I see no effects on marriage outcomes for Black women, potentially consistent with a marriageable men hypothesis.

5.4 Sensitivity analysis

I perform a variety of checks to test the robustness of my chosen specification. These results are robust to several alternative specifications, including a log-linear specification, as well as the exclusion of control variables, the inclusion of linear time trends, and different years. The results are also not sensitive to alternative measures of women's partner market exposure to the NCSSA. Finally, I show that these results are robust to exclusion of select geographic areas. The full results of these analyses are discussed in appendix section A3.

6 Conclusion

Mass incarceration is a uniquely American experiment with potential spillover effects beyond those directly imprisoned. My work contributes to a growing literature on the unintended consequences of incarceration by presenting new evidence on the effect of male incarceration on women's fertility, choice of partners, and marriage. In this paper, I leverage previously unexplored variation, the North Carolina Structured Sentencing Act, to identify the effects of increased incarceration rates on women's family formation outcomes. Between 1994 and 1995, the incarceration rate in North Carolina increased by one-third. This significant increase in the prison population was caused by an increase in the length of prison sentences, creating a natural experiment that allows me to isolate the effects of incapacitation due to incarceration.

I find that increased sentencing severity due to the NCSSA decreases the fertility of women in affected partner markets. The results are strongest for Black women under age 25 and unmarried women of both races, consistent with the demographics of incarcerated men. Although the results on completed fertility are imprecise, I can reject substantial decreases to women's total fertility by ages 35 or 40, indicating these reductions are likely delays. Having children at older ages may allow women to increase their educational attainment and gain labor force experience, but this may also lead to increased health risks for women and their later children. I also find the composition of mothers shifts towards women of higher socioeconomic status.

The composition of fathers is negatively affected, consistent with the predictions of the Becker model. Black women who have children after the NCSSA do so with relatively older men who have relatively less education. There is no observable change in fathers for white women, but the father is more likely to be completely missing from the birth record after the enactment of the NCSSA, indicating less committed partnerships and fewer parental resources for children.

Like the results for fathers, the effects of the NCSSA on marital status also differ by race. For white women, I again find evidence of less committed relationships. After the NCSSA, white women are less likely to be married and more likely to be cohabiting. I find no effects on marriage or cohabitation for Black women, consistent with Wilson's marriageable men hypothesis. These results are only partially consistent with previous work on male incarceration and marriage. This discrepancy may be because the source of variation used here allows me to isolate the incapacitation effects of incarceration separate from selection or stigma effects.

This study provides new information on how communities respond to an increase in sentencing severity. While this project focuses on North Carolina, these effects are potentially more widespread, as all states and the federal government passed some form of "tough on crime" legislation during this period (Neal and Rick, 2016). More recently, state and federal governments have been focused on reducing the prison population (Bragg, 2018; Fandos, December 18, 2018). This work shows that decreasing the incarcerated population will not completely reverse the effects of mass incarceration. Reducing the imprisoned population can only affect the incapacitation effect; these policies cannot reverse the stigma effects of previous time served.

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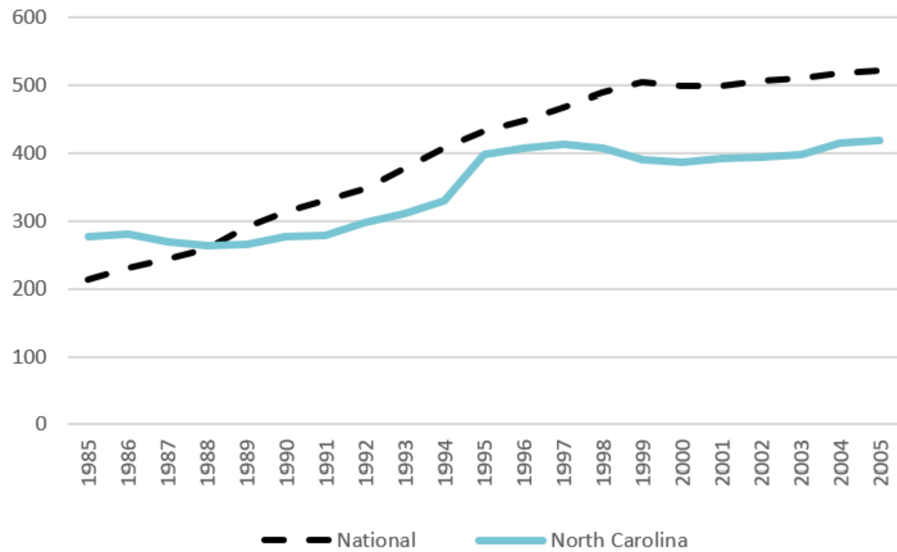
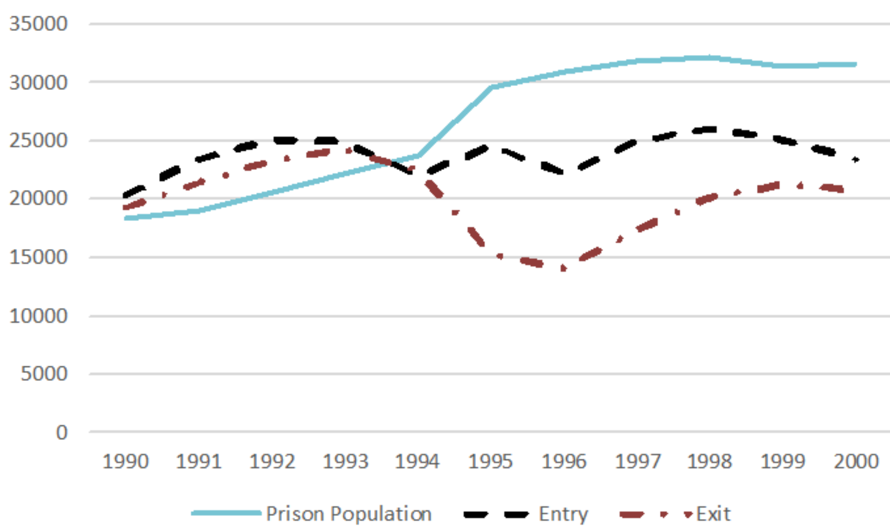
7 Figures

Figure 1: Prisoners per 100,000 population



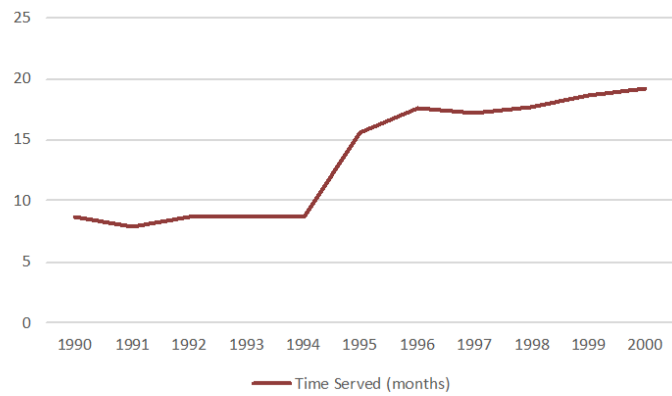
Source: Bureau of Justice Statistics, National Prisoner Statistics.

Figure 2: North Carolina prison entry, exit, and population, 1990 – 2000



Source: North Carolina Department of Public Safety and author's calculations.

Figure 3: Average time served in months by year of entry for North Carolina prisoners, 1990 – 2000

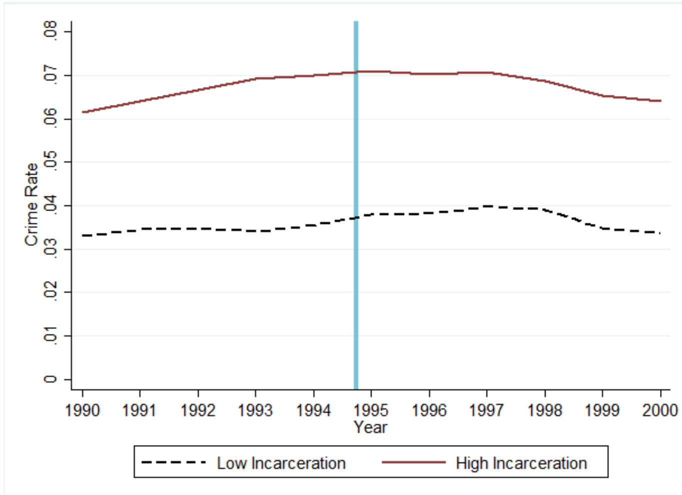


Source: North Carolina Department of Public Safety and author's calculations. Calculation of mean time served excludes prisoners serving life sentences.

Figure 4: Incarceration and birth rates, 1990 – 2000

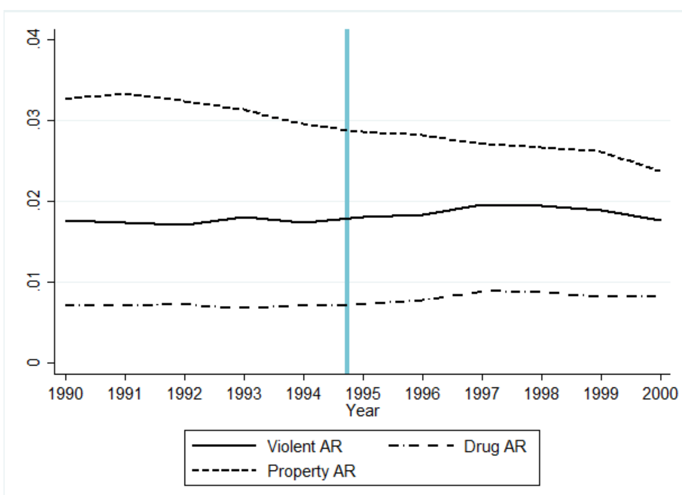
Source: North Carolina Department of Public Safety and North Carolina Detailed Birth Database. CZs were divided into high and low incarceration groups based on the median pre-period incarceration rate.

Figure 5: Incarceration and crime rates, 1990 – 2000



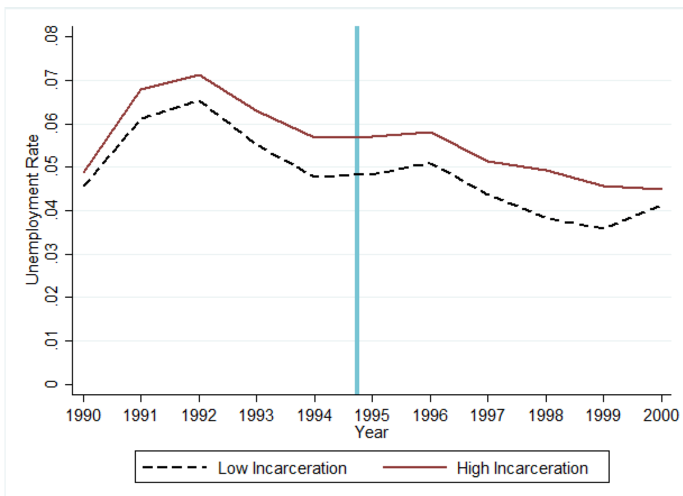
Source: North Carolina Department of Public Safety and the Federal Bureau of Investigation Unified Crime Reporting Program. CZs were divided into high and low incarceration groups based on the median pre-period incarceration rate.

Figure 6: Incarceration and offense-specific arrest rates, 1990 – 2000



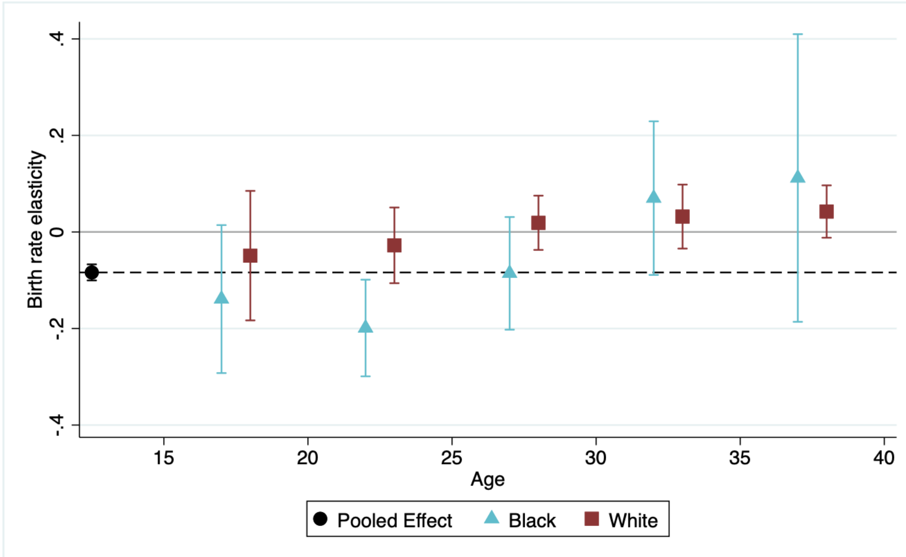
Source: North Carolina Department of Public Safety and the Federal Bureau of Investigation Unified Crime Reporting Program.

Figure 7: Incarceration and unemployment rates, 1990 – 2000



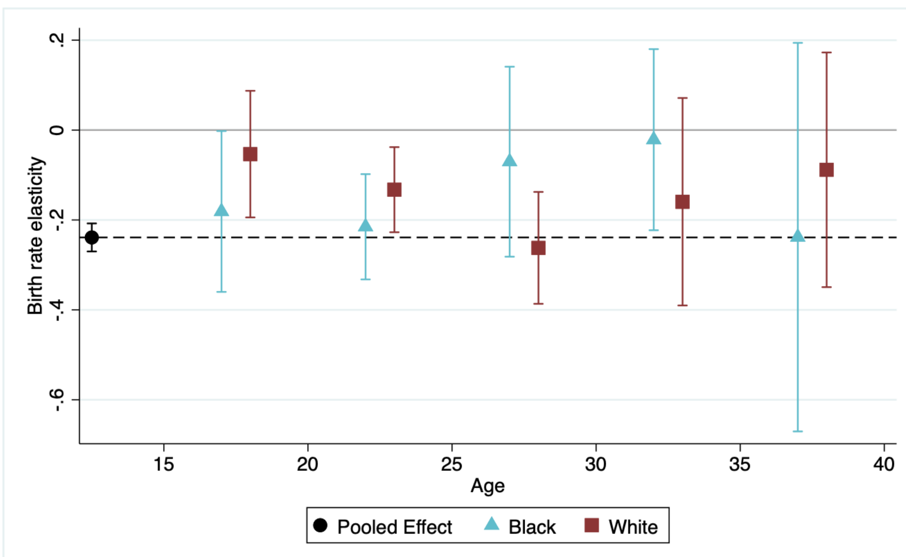
Source: North Carolina Department of Public Safety and Bureau of Labor Statistics. CZs were divided into high and low incarceration groups based on the median pre-period incarceration rate.

Figure 8: The effect of increased sentencing severity on total births, 1990 – 2000



Notes: This figure represents the results of equation 1 for all women in the sample, as well as age and race subgroups. For more details, see section 4.

Figure 9: The effect of increased sentencing severity on births to unmarried women, 1990 – 2000



Notes: This figure represents the results of equation 1 for unmarried women in the sample, as well as age and race subgroups. For more details, see section 4.

8 Tables

Table 1: The effect of increased severity on maternal composition, 1990 – 2000

	(1) Mean Age	(2) % Teens	(3) % Married	(4) Yrs Edu	(5) % Less than HS	(6) % HS/SC	(7) % Coll
A: All mothers							
Post*9093 IR	0.345***	-0.023***	0.010	0.228*	-0.040*	0.031*	0.009
	(0.079)	(0.004)	(0.007)	(0.101)	(0.015)	(0.012)	(0.007)
R-Squared	0.978	0.956	0.995	0.951	0.885	0.947	0.985
Cells	981	981	981	981	981	981	981
B: Black mothers							
Post*9093 IR	0.961*	-0.052***	0.064	0.209	-0.027	0.002	0.026
	(0.354)	(0.016)	(0.032)	(0.133)	(0.019)	(0.019)	(0.024)
R-Squared	0.853	0.781	0.818	0.911	0.736	0.741	0.905
Cells	477	477	477	477	477	477	477
C: White mothers							
Post*9093 IR	0.208	-0.018***	-0.002	0.206	-0.039*	0.035***	0.004
	(0.122)	(0.006)	(0.005)	(0.134)	(0.019)	(0.012)	(0.011)
R-Squared	0.972	0.907	0.930	0.939	0.870	0.947	0.983
Cells	504	504	504	504	504	504	504

Notes: Observations collapsed into race-CZ-half year cells. Includes years 1990 – 2000. Column (1) dependent variable is the average age of women in the cell. Column (4) dependent variable is the cell-level average reported years of education. All other dependent variables are the cell-level mean of an indicator variable equal to one when the maternal characteristic listed at the top of the column is true. Cells weighted by the number of births in the cell. Standard errors, in parenthesis, are clustered by CZ of residence. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 2: The effect of increased sentencing severity on father's characteristics, 1990 – 2000

	(1) Age diff	(2) Has less education	(3) Missing
A: All women			
Post*9093 IR	0.080*	0.003	0.012
	(0.031)	(0.004)	(0.006)
R-Squared	0.843	0.725	0.974
Cells	4514	4514	4588
B: Black women			
Post*9093 IR	0.322***	0.026*	0.016
	(0.103)	(0.011)	(0.014)
R-Squared	0.591	0.722	0.911
Cells	2001	2001	2074
C: White women			
Post*9093 IR	0.023	-0.001	0.015***
	(0.018)	(0.003)	(0.003)
R-Squared	0.913	0.731	0.934
Cells	2513	2513	2514

Notes: Observations collapsed into race-CZ-age group-half year cells. Includes years 1990 – 2000. Dependent variable in column (1) is the cell average of the father's reported age minus the mother's reported age. Dependent variable in column (2) is the cell-level mean of an indicator equal to one if the reported education level of the father is less than the reported maternal education level. Dependent variable in column (3) is the cell-level mean of an indicator equal to one if all possible information about the father (age, race, and educational attainment) are missing from the birth record. Regressions are weighted by cell-level number of births. Standard errors clustered by CZ of residence. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3: The effect of increased sentencing severity on marriage, divorce, and cohabitation, 1990 to 2000

	(1) Married	(2) Divorced	(3) Never Married	(4) Cohabiting
A: All women				
Post*9093 IR	0.001	-0.002	0.014***	0.003
	(0.003)	(0.002)	(0.003)	(0.003)
R-Squared	0.007	0.036	0.104	0.017
Cells	234	234	234	234
B: Black women				
Post*9093 IR	-0.001	0.006	-0.028	0.005
	(0.015)	(0.008)	(0.016)	(0.006)
R-Squared	0.020	0.027	0.094	0.050
Cells	114	114	114	114
C: White women				
Post*9093 IR	-0.012*	-0.004	0.025***	0.019***
	(0.005)	(0.004)	(0.005)	(0.004)
R-Squared	0.065	0.064	0.181	0.327
Cells	120	120	120	120

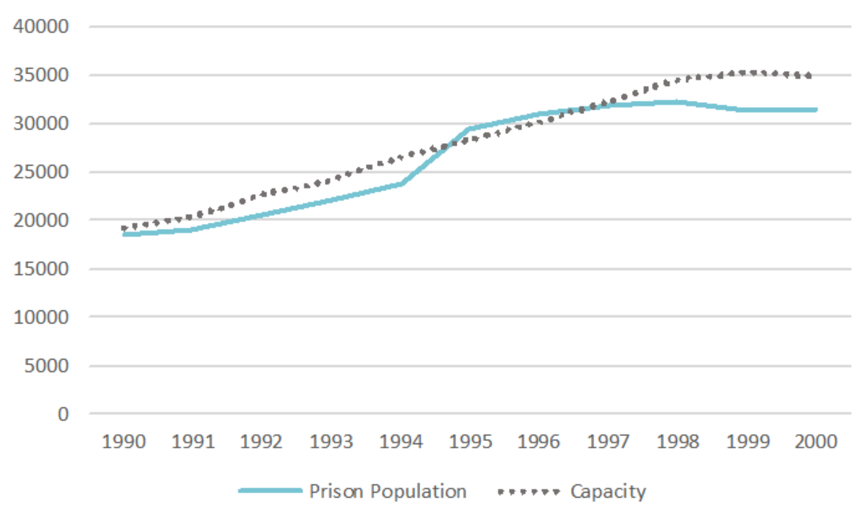
Notes: Observations collapsed into race-CZ-age group cells.. Dependent variable is the change in the percent of women in a cell that report the relationship status in the column header from 1990 and 2000. Cells are weighted by the 1990 female population. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix

This appendix contains additional evidence and analysis not included in the pain text.

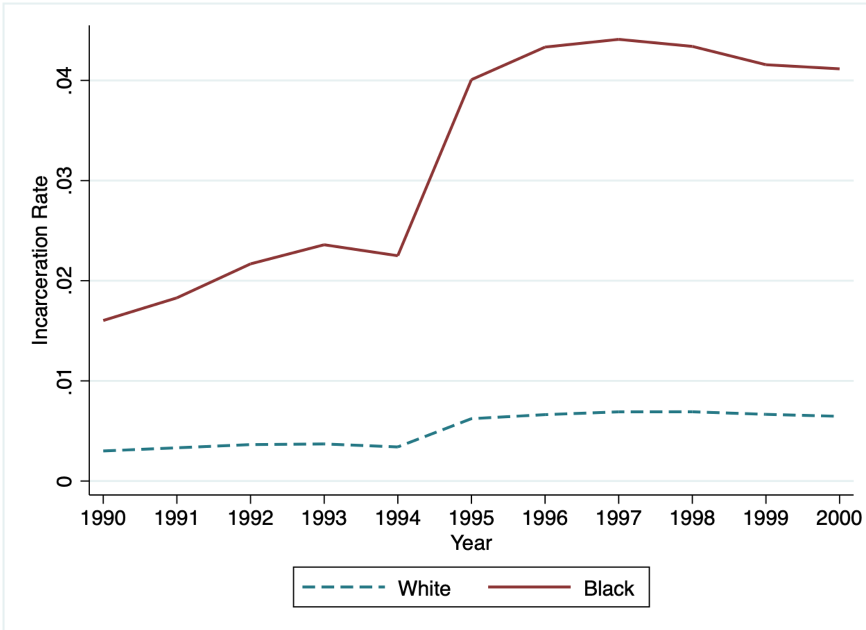
A1 Additional figures

Figure A1: North Carolina prison population and capacity, 1990 – 2000



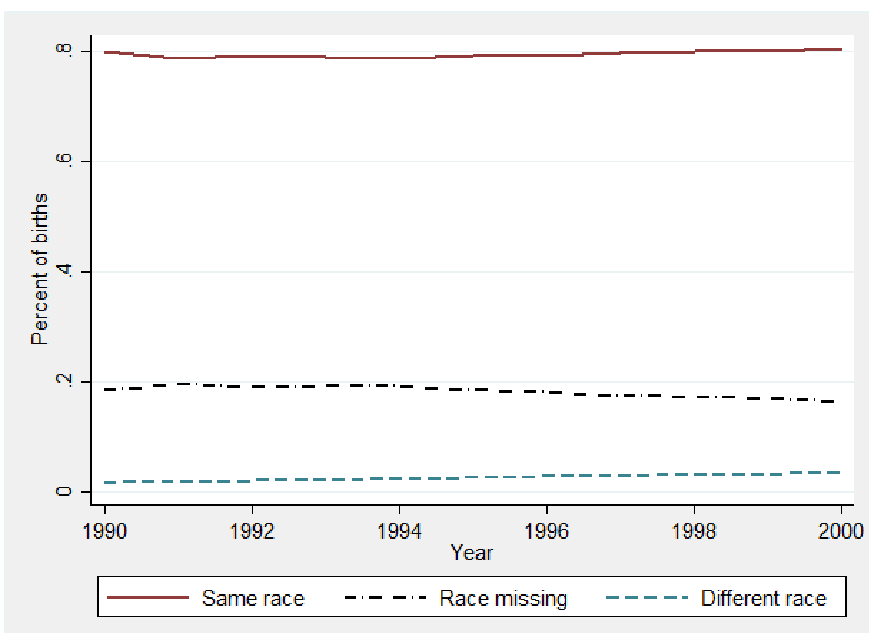
Source: North Carolina Department of Public Safety and the Census of State and Federal Adult Correctional Facilities.

Figure A2: North Carolina adult male incarceration by race, 1990 – 2000



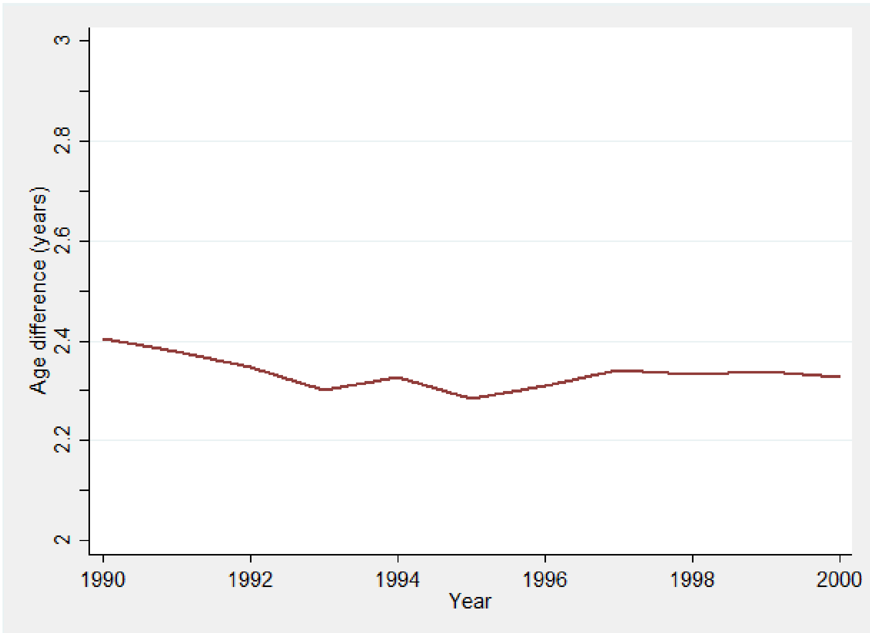
Includes men 15-44. Source: North Carolina Department of Public Safety and author's calculations.

Figure A3: Comparing mothers' and fathers' races as reported on the birth record, 1990 – 2000



Source: North Carolina Detailed Birth Database.

Figure A4: Comparing mothers' and fathers' ages as reported on the birth record, 1990 – 2000



Source: North Carolina Detailed Birth Database.

Figure A5: North Carolina Counties and Commuting Zones

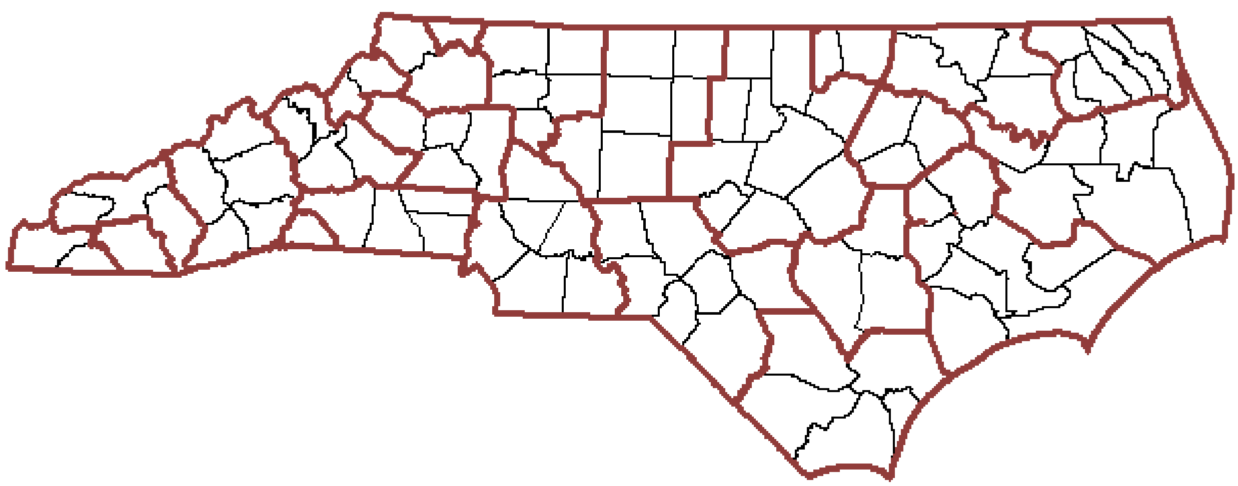
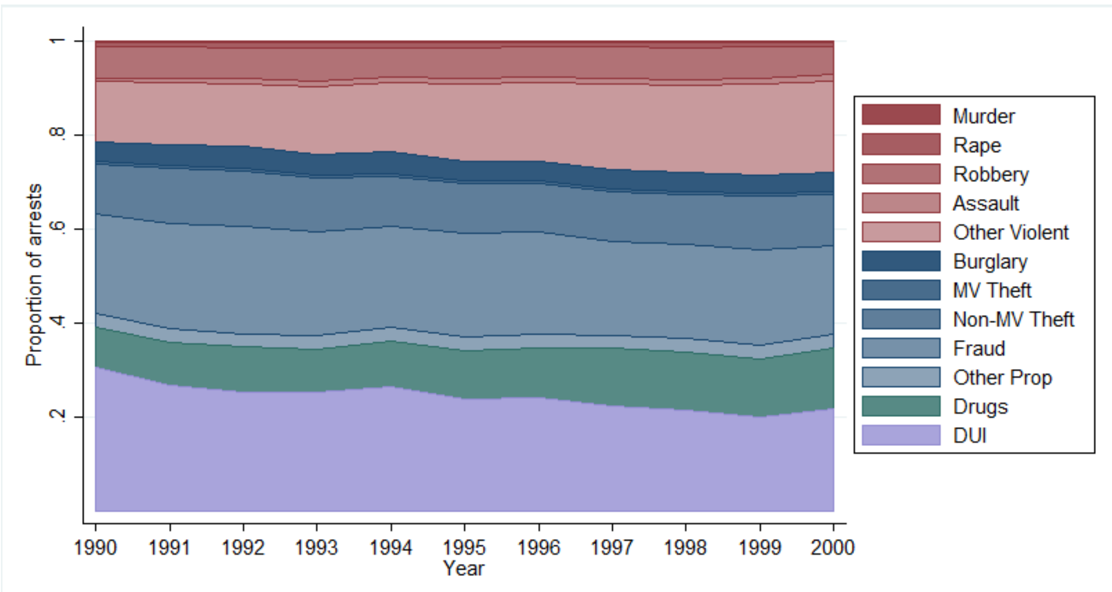


Figure A6: Offense composition of major arrests, 1990 – 2000



Source: North Carolina Department of Public Safety and the Federal Bureau of Investigation Unified Crime Reporting Program.

A2 Additional tables

Table A1: Summary statistics

Variable	Mean	(Std. Dev.)	N
A. Mother's characteristics			
Black	0.279	0.449	1119
Married	0.676	0.22	1119
Teen	0.149	0.063	1119
Mother's age	25.885	1.472	1119
Less than HS	0.225	0.059	1119
HS or some college	0.554	0.092	1119
College or more	0.221	0.112	1119
B. Fertility outcomes			
Year of conception	1994.582	3.451	5760
Births	598.258	526.833	5760
Female population	16457.384	11954.708	5760
Birthrate	0.074	0.037	5760
Married birth rate	0.137	0.096	5580
Unmarried birth rate	0.036	0.032	5736
C. Partner market and CZ characteristics			
Pre-period incarceration rate	0.008	0.009	5760
Crime rate	0.068	0.019	5760
Unemployment rate	0.043	0.016	5280

Notes: Observations in panel A collapsed into race-CZ-half year cells with means weighted by the number of births in the cell. Observations in panels B and C collapsed into race-CZ-age-half year cells with means weighted by the female population for the cell.

Table A2: Summary statistics, by race

	White			**Black**		
Variable	Mean	(Std. Dev.)	N	Mean	(Std. Dev.)	N
A. Mother's characteristics						
Black	0	0	576	1	0	576
Married	0.810	0.046	576	0.331	0.053	576
Teen	0.117	0.033	576	0.233	0.039	576
Mother's age	26.547	1.116	576	24.179	0.711	576
Less than HS	0.212	0.054	576	0.261	0.039	576
HS or some college	0.525	0.088	576	0.633	0.043	576
College or more	0.262	0.098	576	0.106	0.048	576
B. Fertility outcomes						
Year of Conception	1994.578	3.454	2880	1994.596	3.443	2880
Births	703.063	559.66	2880	294.076	223.11	2880
Female population	19545	12184.818	2880	7490.245	4328.728	2880
Birthrate	0.072	0.034	2880	0.08	0.043	2880
Married birth rate	0.141	0.09	2880	0.126	0.113	2700
Unmarried birth rate	0.026	0.018	2880	0.067	0.043	2856
C. Partner market and CZ characteristics						
Pre-period IR	0.003	0.001	2880	0.021	0.009	2880
Crime rate	0.066	0.019	2880	0.076	0.019	2880
Unemployment rate	0.042	0.015	2640	0.046	0.017	2640

Notes: Observations in panel A collapsed into race-CZ-half year cells with means weighted by the number of births in the cell. Observations in panels B and C collapsed into race-CZ-age-half year cells with means weighted by the female population for the cell.

Table A3: Using pre-period incarceration rates to predict current incarceration rates, 1995-2000

	Unweighted		Weighted	
	(1) No FX	(2) With FX	(3) No FX	(4) With FX
A: All men				
IR 9093	1.794***	1.297***	1.997***	1.686***
	(0.258)	(0.344)	(0.066)	(0.097)
F	48.44	14.19	925.17	300.20
Cells	1728	1728	1728	1728
B: Black men				
IR 9093	1.444***	1.069**	1.709***	0.990***
	(0.337)	(0.322)	(0.079)	(0.127)
F	18.42	11.00	462.48	61.16
Cells	864	864	864	864
C: White men				
IR 9093	1.431***	0.644***	1.497***	1.031***
	(0.120)	(0.176)	(0.096)	(0.119)
F	142.14	13.41	241.74	74.57
Cells	864	864	864	864

Notes: The dependent variable for each column is the age-race-CZ male incarceration rate. Each observation represents a race-CZ-age group-year. Regressions are weighted by the applicable female population were indicated. Where indicated, regressions include partner market and year fixed effects. Incarceration data are from the North Carolina DPS and population data are from SEER.Standard errors are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A4: LHS Balance tests from Pei, Pischke, and Schwandt (2018)

	(1) Crime	(2) Clearance	(3) Unemployment
A: Unweighted			
Post*9093 IR	-0.000	0.003	0.002
	(0.001)	(0.009)	(0.002)
LHS Joint balancing test			
p-value	0.8668		
Cells	5192	5192	5192
B: Weighted by population			
Post*9093 IR	0.003	-0.002	-0.001
	(0.002)	(0.011)	(0.001)
LHS Joint balancing test			
p-value	0.3596		
Cells	5192	5192	5192

Notes: Observations collapsed into race-CZ-age group-half year cells. Includes years 1990 – 2000. Dependent variable in column (1) is the CZ crime rate, in column (2) is the CZ clearance rate, in column (3) is the CZ unemployment rate, in column (4) is CZ prison staffing as a percentage of the labor force, and in column (5) is the CZ prison capacity per capita. Regressions are weighted by cell female population in panel B. Standard errors clustered by CZ of residence. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A5: The effect of increased sentencing severity on propensity to move between 1995 and 2000

	(1) All	(2) Black	(3) White
9093 IR	0.525	1.089	-4.972
	(1.309)	(1.103)	(7.124)
R-Squared	0.091	0.010	0.004
Cells	287	143	144

Notes: This table reports estimates of the association between the pre-NCSSA partner market incarceration rate and women's propensity to move across geographic partner markets. Observations collapsed into race-CZ-age group-year cells. Dependent variable is the percent of women in a cell that report they migrated across public use microdata areas between 1995 and 2000. Cells are weighted by the 1990 female population. Pooled model includes a race fixed effect. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A6: The effect of increased sentencing severity on total births

	(1) All Ages	(2) 15-19	(3) 20-24	(4) 25-29	(5) 30-34	(6) 35-39
A: All women						
Post*9093 IR	-0.084***	-0.087	-0.062*	0.012	0.048	0.070*
	(0.008)	(0.050)	(0.026)	(0.022)	(0.030)	(0.032)
Crime Rate	-0.803	-1.342	-0.883	-0.775	-0.831	-2.413***
	(0.659)	(0.982)	(0.823)	(0.790)	(0.819)	(0.754)
Clearance Rate	-0.118	-0.313*	-0.197	-0.009	-0.062	-0.014
	(0.119)	(0.137)	(0.175)	(0.120)	(0.097)	(0.186)
Unemp. Rate	-0.536	0.816	0.483	-0.513	-1.345*	-1.719
	(0.563)	(0.835)	(0.731)	(0.655)	(0.612)	(1.071)
Cells	4935	987	987	987	987	987
B: Black women						
Post*9093 IR	-0.095*	-0.139	-0.199***	-0.086	0.070	0.112
	(0.046)	(0.078)	(0.051)	(0.060)	(0.081)	(0.152)
Crime Rate	0.526	0.464	2.108	-2.064*	-2.718*	-0.750
	(1.114)	(1.502)	(1.482)	(0.977)	(1.385)	(2.138)
Clearance Rate	0.115	-0.023	0.158	-0.012	0.059	0.547
	(0.085)	(0.127)	(0.143)	(0.136)	(0.168)	(0.422)
Unemp. Rate	-0.410	0.400	0.677	-2.365*	-2.100	-4.134*
	(0.527)	(1.148)	(0.806)	(1.002)	(1.303)	(1.852)
Cells	2415	483	483	483	483	483
C: White women						
Post*9093 IR	0.002	-0.049	-0.028	0.019	0.032	0.042
	(0.033)	(0.068)	(0.040)	(0.029)	(0.034)	(0.028)
Crime Rate	-1.511	-3.017*	-2.485	-0.554	-0.367	-2.346*
	(0.892)	(1.278)	(1.294)	(1.011)	(0.957)	(0.928)
Clearance Rate	-0.200	-0.490***	-0.341	0.011	-0.095	-0.129
	(0.161)	(0.190)	(0.281)	(0.169)	(0.110)	(0.154)
Unemp. Rate	-0.181	0.563	0.126	-0.036	-1.179	-0.976
	(0.771)	(1.268)	(1.323)	(0.786)	(0.602)	(0.984)
Cells	2520	504	504	504	504	504

Notes: This table reports estimates of the interaction of the pre-NCSSA partner market incarceration rate and a post-NCSSA dummy as in equation 1. Observations collapsed into race-CZ-age group-halfyear cells. Includes years 1990 – 2000. The dependent variable is the number of births. Regressions include the natural log of the female population as a regressor. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A7: The effect of increased sentencing severity on births to unmarried women

	(1) All Ages	(2) 15-19	(3) 20-24	(4) 25-29	(5) 30-34	(6) 35-39
A: All women						
Post*9093 IR	-0.239***	-0.111*	-0.185***	-0.164***	-0.066	-0.123
	(0.016)	(0.052)	(0.041)	(0.049)	(0.092)	(0.134)
Crime Rate	-1.340	-2.013	-1.248	-3.697*	-0.312	-4.181*
	(0.866)	(1.088)	(0.983)	(1.715)	(2.127)	(1.908)
Clearance Rate	-0.167	-0.254*	-0.180	-0.270*	-0.227	-0.299
	(0.132)	(0.119)	(0.143)	(0.117)	(0.372)	(0.690)
Unemp. Rate	-0.207	0.233	0.854	-1.978	0.304	-1.414
	(0.671)	(0.873)	(0.750)	(1.049)	(1.417)	(3.046)
Cells	4891	987	968	985	987	964
B: Black women						
Post*9093 IR	-0.168***	-0.181*	-0.215***	-0.070	-0.021	-0.238
	(0.062)	(0.091)	(0.060)	(0.108)	(0.103)	(0.221)
Crime Rate	-0.136	-1.317	2.511*	-3.611	0.018	-8.415*
	(1.378)	(1.822)	(1.256)	(1.894)	(3.108)	(3.514)
Clearance Rate	-0.036	-0.195	0.223	-0.454	-0.025	0.926
	(0.143)	(0.100)	(0.163)	(0.334)	(0.300)	(0.474)
Unemp. Rate	-0.428	-0.101	0.888	-2.853	-0.868	-7.855*
	(0.558)	(1.180)	(0.887)	(1.509)	(1.992)	(3.131)
Cells	2371	483	464	481	483	460
C: White women						
Post*9093 IR	-0.146***	-0.053	-0.133***	-0.262***	-0.159	-0.088
	(0.056)	(0.072)	(0.048)	(0.064)	(0.118)	(0.133)
Crime Rate	-3.759*	-3.724	-6.110***	-2.927	0.439	1.359
	(1.548)	(1.981)	(1.843)	(2.927)	(3.152)	(2.440)
Clearance Rate	-0.383	-0.353	-0.488	0.040	-0.448	-1.643*
	(0.259)	(0.207)	(0.256)	(0.306)	(0.724)	(0.775)
Unemp. Rate	0.662	-0.104	0.564	0.016	2.611	5.669
	(1.600)	(1.718)	(1.946)	(2.189)	(2.705)	(4.340)
Cells	2520	504	504	504	504	504

Notes: This table reports estimates of the interaction of the pre-NCSSA partner market incarceration rate and a post-NCSSA dummy as in equation 1. Observations collapsed into race-CZ-age group-halfyear cells. Includes years 1990 – 2000. The dependent variable is the number of births to unmarried women. Regressions include the natural log of the unmarried female population as a regressor. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A8: The effect of increased sentencing severity on births to married women

	(1) All Ages	(2) 20-24	(3) 25-29	(4) 30-34	(5) 35-39
A: All women					
Post*9093 IR	-0.055***	-0.063*	0.003	0.044	0.106***
	(0.012)	(0.029)	(0.025)	(0.042)	(0.041)
Crime Rate	-1.670	0.288	-1.597	-3.315***	-3.610***
	(0.956)	(0.938)	(1.164)	(1.259)	(1.011)
Clearance Rate	-0.049	-0.189	0.129	-0.039	-0.116
	(0.162)	(0.182)	(0.174)	(0.157)	(0.155)
Unemp. Rate	-1.704*	0.027	-0.856	-2.599***	-2.289*
	(0.691)	(0.867)	(0.862)	(0.908)	(1.048)
Cells	3859	939	987	971	962
B: Black women					
Post*9093 IR	0.064	-0.218*	-0.045	0.284*	0.398*
	(0.106)	(0.088)	(0.113)	(0.129)	(0.195)
Crime Rate	-6.128*	0.480	-4.973*	-12.251***	-5.007
	(2.435)	(2.710)	(2.334)	(3.169)	(3.751)
Clearance Rate	0.070	0.168	0.332	0.062	-0.208
	(0.250)	(0.248)	(0.344)	(0.409)	(0.397)
Unemp. Rate	-3.905*	-1.248	-3.706*	-5.600*	-3.417
	(1.559)	(1.441)	(1.546)	(2.312)	(3.206)
Cells	1843	435	483	467	458
C: White women					
Post*9093 IR	-0.008	-0.058	0.015	0.021	0.065
	(0.026)	(0.034)	(0.025)	(0.040)	(0.034)
Crime Rate	-1.040	0.137	-1.334	-1.518	-3.088***
	(0.900)	(0.835)	(1.102)	(0.988)	(1.013)
Clearance Rate	-0.075	-0.237	0.099	-0.071	-0.096
	(0.147)	(0.211)	(0.144)	(0.144)	(0.155)
Unemp. Rate	-0.916	0.049	-0.362	-1.720*	-1.675*
	(0.665)	(1.000)	(0.895)	(0.685)	(0.760)
Cells	2016	504	504	504	504

Notes: This table reports estimates of the interaction of the pre-NCSSA partner market incarceration rate and a post-NCSSA dummy as in equation 1. Observations collapsed into race-CZ-age group-halfyear cells. Includes years 1990 – 2000. The dependent variable is the number of births to married women. Regressions include the natural log of the married female population as a regressor. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A9: The effect of increased severity on maternal composition of women having their first birth, 1990 – 2000

	(1) Mean Age	(2) % Teens	(3) % Married	(4) Yrs Edu	(5) % Less than HS	(6) % HS/SC	(7) % Coll
A: All mothers							
Post*9093 IR	0.372***	-0.027***	0.003	0.236*	-0.035*	0.021	0.014*
	(0.086)	(0.006)	(0.007)	(0.084)	(0.014)	(0.013)	(0.006)
R-Squared	0.978	0.962	0.992	0.954	0.888	0.899	0.976
Cells	968	968	968	968	968	968	968
B: Black mothers							
Post*9093 IR	0.890***	-0.062***	0.059*	0.263	-0.029	0.001	0.029
	(0.259)	(0.019)	(0.027)	(0.154)	(0.025)	(0.027)	(0.027)
R-Squared	0.869	0.800	0.797	0.868	0.612	0.549	0.881
Cells	464	464	464	464	464	464	464
C: White mothers							
Post*9093 IR	0.266*	-0.020*	-0.006	0.211	-0.034	0.025	0.009
	(0.116)	(0.008)	(0.006)	(0.113)	(0.017)	(0.013)	(0.010)
R-Squared	0.966	0.905	0.906	0.946	0.855	0.934	0.976
Cells	504	504	504	504	504	504	504

Notes: Observations collapsed into race-CZ-half year cells. Includes years 1990 – 2000. Column (1) dependent variable is the average age of women in the cell. Column (4) dependent variable is the cell-level average reported years of education. All other dependent variables are the cell-level mean of an indicator variable equal to one when the maternal characteristic listed at the top of the column is true. Cells weighted by the number of births in the cell. Standard errors, in parenthesis, are clustered by CZ of residence. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A10: The effect of increased sentencing severity on long term fertility

	% No Children		Total Children	
	(1) At 35	(2) At 40	(3) At 35	(4) At 40
A: All women				
Post*9093 IR	-0.150	-0.118	0.368	0.253
	(0.161)	(0.180)	(0.349)	(0.395)
R-Squared	0.531	0.578	0.627	0.651
Cells	992	728	1123	889
B: Black women				
Post*9093 IR	-0.330	-0.262	0.732	0.569
	(0.163)	(0.147)	(0.360)	(0.350)
R-Squared	0.364	0.496	0.425	0.524
Cells	422	300	547	433
C: White women				
Post*9093 IR	-0.126	-0.104	0.313	0.218
	(0.178)	(0.193)	(0.390)	(0.426)
R-Squared	0.561	0.591	0.640	0.627
Cells	570	428	576	456

Notes: Observations collapsed into race-CZ-age group-cells. Includes births occurring 1968 to 2014 to women born in years 1955 to 1985. Dependent variables in columns (1) and (2) is the percentage of women in a cohort that are not observed having a first birth by the age denoted in the column header. Dependent variable in columns (3) and (4) is the total number of births observed to women in a cohort by the age denoted in the column header. Both dependent variables have been multiplied by 1000 to improve readability. Regressions are weighted by the size of the cohort. Standard errors clustered by CZ of residence. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

A3 Sensitivity analysis

I perform a variety of checks to test the robustness of my prefered specification. The results of these analyses are presented in tables A11a through A12c. For reference, the first column of tables A11a and A12a show the baseline estimates. Note, the pooled estimates are not equivalent to the weighted average of the race-specific coefficients because not all of the coefficients and fixed effects are race-specific. In column (2), I show the results are robust to the exclusion of control variables. In columns (3) and (4), I include a CZ specific or exposure-level specific linear time trend. The CZ-level time trend produces very similar results. The results for the exposure-level specific time trend are attenuated, but as this is almost like giving each partner market its own time trend, it's not surprising this attenuates the estimate.

As discussed in section 4, regressions examining fertility often use the natural log of the birth rate as the dependent variable. I choose a Poisson functional form in order to estimate the effect for smaller subgroups that include zeros and because the assumptions for consistency are weaker. The overall results are not dependent on the choice of functional form. Column (5) of tables A11a and A12a displays the estimate using a more common linear specification. The linear specifications produce similar estimates.

These results are also robust the inclusion or exclusion of additional years. First, I restrict the sample to only census years. Intercensal population estimates are based on estimates and may introduce measurement error into my exposure measure. Population estimates for census years (here, 1990 and 2000) will have less measurement error. Alternatively, I also include a wider time window by adding an additional 5 years to each side of the estimate. In both cases, I still find a negative relationship between increased sentencing severity and fertility. In the main specification I exclude births that would have been conceived around the time of the NCSSA's enactment since I cannot confidently assign the timing to before or after the NCSSA. In the column labeled "Phase-in $\mathbb{I}$" I keep births conceived during that time in the sample, but include a dummy variable to indicate the phase-in period. The results are not sensitive to the inclusion of these births.

Finally, in my preferred specification, I cluster standard errors at the CZ level. However, this only leaves 24 clusters, which may be too few clusters. Unfortunately, bootstrap-based methods to improve inference with a small number of clusters are only developed for linear models (Cameron et al., 2008). In the last column of tables A11a and A12a, I define the clusters by race and CZ. While I initially chose CZs to capture the interdependence of partner market shocks across age and within a geographic region, previous work has shown that partner markets remain mostly contained within racial groups (Charles and Luoh, 2010). The effects of idiosyncratic shocks are most likely to correlated across partner markets of the same race in the same area. The cluster definition produces 48 clusters. Using this definition marginally improves the precision of the pooled estimate and does not change the significance level.

I also try several alternative measures of a partner market's exposure to increased incarceration that resulted from the NCSSA. These results are presented in tables A11b and A12b. Instead of defining exposure using the average incarceration rate in the period just before the passage of the law, I define exposure as the average entry rate. The entry rate is the number of men who enter prison each year divided by the population. Although this reduces the magnitude of the point estimate, the overall conclusion is unchanged. I also narrow the age band of men I match women to, defining the partner market incarceration rate based on men who are in the same age group as the women in question. Additionally, I run my specification in levels, using the incarceration exposure rate, instead of the natural log. While the magnitude of the point estimate changes when I define the variable differently, the qualitative conclusions are unchanged.

Finally, I exclude selected areas from the analysis to ensure that my results are not being driven by any one geographic area in tables A11c and A12c. First, I exclude areas of the state considered to be part of Appalachia. This area is historically more rural and economically distressed that the rest of the state, which could contribute to different incarceration and family formation patterns. Then, I exclude the two CZs with a women's correctional facility, due to the concerns about measurement error discussed in section 3. Finally, I exclude the two most populous areas of the state one at a time: the Raleigh area and the Charlotte area. While I do lose precision as I exclude observations, particularly for white women, the effect is similar, particularly when focusing on unmarried mothers. The results for Black women are especially robust.

Table A11a: The effect of increased sentencing severity on births, specification checks

	Specification								
	(1) Main	(2) No Controls	(3) + CZ Trend	(4) + Exp. Trend	(5) Linear	(6) Census Yrs	(7) 1985-2005	(8) Phase-in I	(9) Alt. Cluster
A: All women									
Post*9093 IR	-0.084***	-0.084***	-0.078***	-0.027***	-0.074**	-0.128***	-0.102***	-0.077***	-0.084***
	(0.008)	(0.009)	(0.007)	(0.007)	(0.023)	(0.010)	(0.012)	(0.008)	(0.007)
Cells	4935	4935	4935	4935	4588	916	9635	5170	4935
B: Black women									
Post*9093 IR	-0.095*	-0.092	-0.071	-0.030	-0.129**	-0.158*	-0.079	-0.083	-0.095*
	(0.046)	(0.047)	(0.042)	(0.066)	(0.044)	(0.064)	(0.046)	(0.047)	(0.046)
Cells	2415	2415	2415	2415	2074	436	4715	2530	2415
C: White women									
Post*9093 IR	0.002	-0.006	-0.000	0.026	-0.062*	-0.049	-0.031	0.002	0.002
	(0.033)	(0.036)	(0.032)	(0.016)	(0.025)	(0.047)	(0.049)	(0.032)	(0.033)
Cells	2520	2520	2520	2520	2514	480	4920	2640	2520

Notes: Observations collapsed into race-CZ-age group-halfyear cells. Includes women aged 15-39 and years 1990 – 2000. Dependent variable in column (5) is the natural log of the birth rate. Dependent variable in all other columns is the number of births. Non-linear regressions include the natural log of the female population as a regressor. Linear regression is weighted by the applicable female population. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A11b: The effect of increased sentencing severity on births, alternative exposure measures

	Exposure Measure		
	(1) Entry	(2) Same Age	(3) Level
A: All women			
Post*9093 IR	-0.089***	-0.080***	-7.837***
	(0.009)	(0.009)	(0.794)
Cells	4977	4788	5019
B: Black women			
Post*9093 IR	-0.085**	-0.092*	-3.945*
	(0.032)	(0.045)	(1.696)
Cells	2457	2289	2499
C: White women			
Post*9093 IR	0.007	0.020	0.367
	(0.027)	(0.031)	(8.491)
Cells	2520	2499	2520

Notes: Observations collapsed into race-CZ-age group-halfyear cells. Includes women aged 15-39 and years 1990 – 2000. Dependent variable is the number of births. Non-linear regressions include the natural log of the female population as a regressor. Linear regression is weighted by the applicable female population. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A11c: The effect of increased sentencing severity on births, geographic checks

	Geography			
	(1) No Appalachia	(2) No W Prison	(3) No Raleigh	(4) No Charlotte
A: All women				
Post*9093 IR	-0.081***	-0.084***	-0.084***	-0.083***
	(0.008)	(0.012)	(0.012)	(0.011)
Cells	2940	4515	4725	4725
B: Black women				
Post*9093 IR	-0.083	-0.120*	-0.106*	-0.096*
	(0.054)	(0.053)	(0.046)	(0.049)
Cells	1470	2205	2310	2310
C: White women				
Post*9093 IR	0.001	0.045	0.023	0.002
	(0.036)	(0.033)	(0.034)	(0.037)
Cells	1470	2310	2415	2415

Notes: Observations collapsed into race-CZ-age group-halfyear cells. Includes women aged 15-39 and years 1990 – 2000. Dependent variable is the number of births. Non-linear regressions include the natural log of the female population as a regressor. Linear regression is weighted by the applicable female population. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A12a: The effect of increased sentencing severity on births to unmarried women, specification checks

	Specification							
	(1) Main	(2) No Controls	(3) + CZ Trend	(4) + Exp. Trend	(5) Linear	(6) Census Yrs	(7) Phase-in 𝕀	(8) Alt. Cluster
A: All women								
Post*9093 IR	-0.239***	-0.084***	-0.223***	-0.034**	-0.045	-0.393***	-0.226***	-0.239***
	(0.016)	(0.009)	(0.013)	(0.011)	(0.041)	(0.022)	(0.015)	(0.014)
Cells	4891	4935	4891	4914	4396	912	5124	4891
B: Black women								
Post*9093 IR	-0.168**	-0.162**	-0.131*	-0.061	-0.090*	-0.216**	-0.152*	-0.168**
	(0.062)	(0.060)	(0.052)	(0.082)	(0.043)	(0.076)	(0.062)	(0.062)
Cells	2371	2371	2371	2394	1984	432	2484	2371
C: White women								
Post*9093 IR	-0.146**	-0.172**	-0.143*	-0.033	-0.025	-0.160	-0.138**	-0.146**
	(0.056)	(0.058)	(0.056)	(0.036)	(0.059)	(0.111)	(0.051)	(0.056)
Cells	2520	2520	2520	2520	2412	480	2640	2520

Notes: Observations collapsed into race-CZ-age group-halfyear cells. Includes women aged 15-39 and years 1990 – 2000. Dependent variable in column (5) is the natural log of the birth rate. Dependent variable in all other columns is the number of births to unmarried women. Non-linear regressions include the natural log of the female population as a regressor. Linear regression is weighted by the applicable female population. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A12b: The effect of increased sentencing severity on births to unmarried women, alternative exposure measures

	Exposure Measure		
	(1) Entry	(2) Same Age	(3) Level
A: All women			
Post*9093 IR	-0.264***	-0.248***	-20.882***
	(0.020)	(0.017)	(2.056)
Cells	4933	4767	4998
B: Black women			
Post*9093 IR	-0.164***	-0.141*	-5.699*
	(0.038)	(0.063)	(2.659)
Cells	2413	2268	2478
C: White women			
Post*9093 IR	-0.155**	-0.065	-26.481
	(0.049)	(0.064)	(15.528)
Cells	2520	2499	2520

Notes: Observations collapsed into race-CZ-age group-halfyear cells. Includes women aged 15-39 and years 1990 – 2000. Dependent variable is the number of births to unmarried women. Non-linear regressions include the natural log of the female population as a regressor. Linear regression is weighted by the applicable female population. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A12c: The effect of increased sentencing severity on births to unmarried women, geographic checks

	Geography			
	(1) No Appalachia	(2) No W Prison	(3) No Raleigh	(4) No Charlotte
A: All women				
Post*9093 IR	-0.233***	-0.239***	-0.237***	-0.237***
	(0.014)	(0.021)	(0.020)	(0.021)
Cells	2940	4471	4681	4681
B: Black women				
Post*9093 IR	-0.163**	-0.202**	-0.167*	-0.187**
	(0.059)	(0.068)	(0.065)	(0.063)
Cells	1470	2161	2266	2266
C: White women				
Post*9093 IR	-0.163**	-0.064	-0.078	-0.144*
	(0.059)	(0.061)	(0.061)	(0.067)
Cells	1470	2310	2415	2415

Notes: Observations collapsed into race-CZ-age group-halfyear cells. Includes women aged 15-39 and years 1990 – 2000. Dependent variable is the number of births to unmarried women. Non-linear regressions include the natural log of the female population as a regressor. Linear regression is weighted by the applicable female population. Standard errors, in parenthesis, are clustered by CZ. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.