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

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Abstract

The United States' incarceration rate has more than quintupled since 1970, reaching a peak of 1% of adults in 2007. Using an intensity-of-treatment research design, I examine the effect of a policy change that increased sentencing severity on women's fertility and family formation. The results show that after this policy, unmarried and young Black women experienced a decrease in fertility, and the composition of births shifted toward higher socioeconomic status mothers. Among those who gave birth, the quality of partner matches declined. Additionally, white women were less likely to be married, while there was no effect on the marriage rates of Black women.

Keywords: Fertility, family formation, incarceration
JEL Codes: J13, J12, K14

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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 them in childhood, with far-reaching consequences for their 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 this ratio decreases, fewer men are available to form relationships. This increased scarcity may also affect relationships that do form by encouraging women to accept lower-quality partners.

Many communities in the United States have seen large changes in the ratio of men to women due to mass incarceration. Since 1970, the country's incarceration rate has more than quintupled, peaking at 1% of adults in 2007 (Kaeble and Glaze, 2016). As incarceration disproportionately affects men, this shift has important compositional effects on community dynamics and family formation. Over 90% of prisoners in state and federal facilities are men, with Black men being 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 men—effectively removing them from the community—during this "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 and fertility.

In this paper, I explore the impact of a policy change that increased sentencing severity on women's fertility and family formation outcomes. Enacted in 1994, the North Carolina

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

Structured Sentencing Act (NCSSA) raised the severity of criminal sentences, leading to a quick growth in the state's incarceration rate.[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 (DPS), the National Center for Health Statistics, and the 1990 and 2000 censuses.

I find that the NCSSA reduced the birth rates of affected women, driven primarily by Black women under age 25 and unmarried women. However, I find no evidence of a decrease in total completed fertility at later ages, implying that the observed reduction in fertility among younger women was a *delay*. For women who continued to give birth, they did so with older, less educated, or less committed partners, suggesting that the effects of this policy extended beyond the mechanical effects of fewer men living in these communities. Additionally, I find that the NCSSA reduced the probability of marriage for white women.

My work speaks to a broad literature in economics on the effect of sex ratios on family formation and marriage outcomes. 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 that men must cross before women will consider them marriageable.[4] If incarcerated men are so negatively selected,

[2]This policy is typical of those passed by states in the 1980s and 1990s that were designed to make criminal sentences more punitive (by increasing offenders' time served) and to reduce sentencing disparities (through the application of clear guidelines) (Travis et al., 2014).

[3]Author's calculations using National Prisoner Statistics and Surveillance, Epidemiology, and End Results (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.

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 that increased male incarceration decreases a woman's probability of being married for Black and white women. Focusing only on Black women, Mechoulan (2011) finds no evidence of an effect on marriage when using variation in incarceration rates across states over time. Also only considering Black women and children, Liu (2021) finds a reduction in marriage probability and an increase in the probability of a child living with an unmarried mother, using a synthetic instrumental variable approach. None of these studies directly examine the fertility of adult women or cohabitation, though 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 incarceration growth 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 remained 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. Extensive qualitative (Edin and Kefalas, 2011) and quantitative (Lundberg et al., 2016) evidence shows that women are increasingly making their decisions around fertility and marriage at different times in their

lives, and thus focusing solely on marriage ignores an important margin of adjustment.

Third, I show that incarceration changed 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. Additionally, 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. These findings are relevant beyond North Carolina as the NCSSA is typical of "tough-on-crime" policies,[6] suggesting that the effects I estimate may have occurred more broadly.

The rest of the paper proceeds as follows. Section 2 discusses the policies and institutional factors that led to the sharp increase in incarceration rates, and Section 3 details the data used in the analysis. Section 4 discusses the empirical strategies used to estimate the effects of this change, Section 5 presents the results, and Section 7 concludes.

2 The NCSSA

This paper leverages variation from the 1994 NCSSA to explore how increased sentencing severity affects family formation patterns. State sentencing reforms like the NCSSA were a key part of the policy landscape that contributed to the historic rise in incarceration rates in the United States. In this section, I describe the NCSSA and its impact on incarceration in North Carolina.

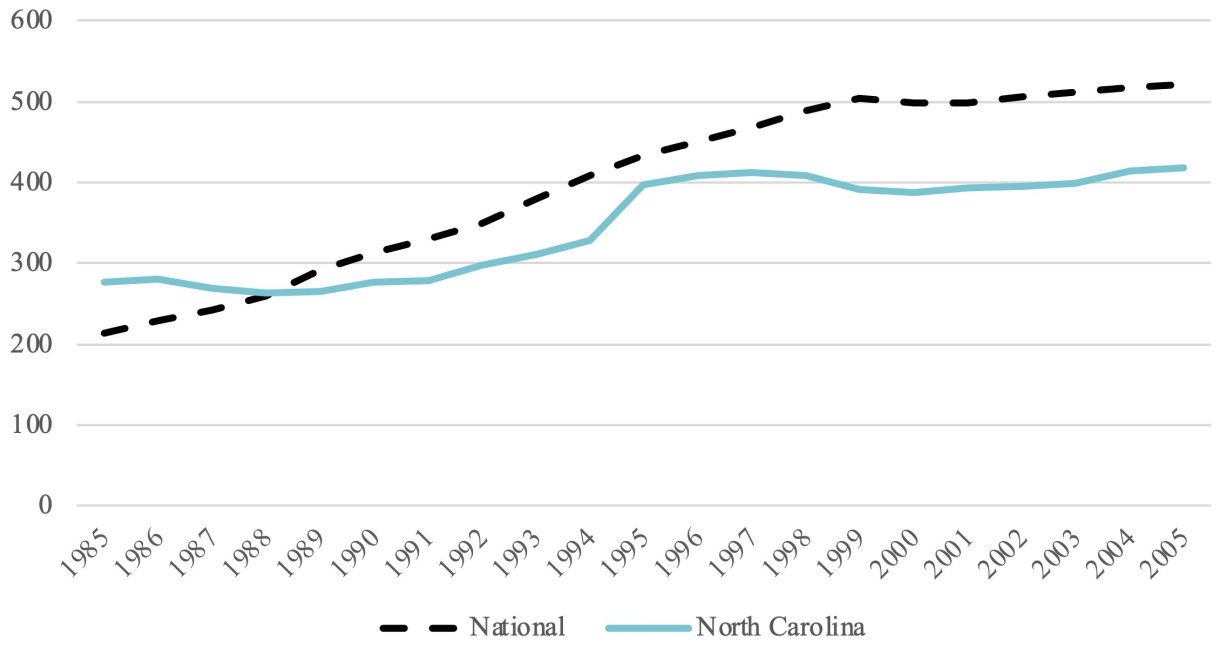
In the 1970s, sentencing reforms focused on increasing the consistency of sentences imposed by judges. Critics of the previous system, known as "indeterminate sentencing," claimed that the lack of strict guidelines opened the door for racially disparate or otherwise arbitrary sentences. Beginning in the late 1980s, reforms focused on increasing the severity

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

[6]From 1970 to 2000, over 20 states ended or significantly curtailed discretionary parole programs, and 48 states and the federal government enacted laws designed to increase time served per criminal defense (Neal and Rick, 2016). As discussed in Section 2, these are two main features of the NCSSA.

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 the reforms pursued by states in this later period. However, due to institutional and political factors unique to North Carolina, the NCSSA led to a sharp change in the state incarceration rate, as shown in Figure 1, whereas other states experienced smoother increases throughout the 1990s.

Figure 1: Prisoners per 100,000 population



Source: Bureau of Justice Statistics, National Prisoner Statistics.

The North Carolina Sentencing and Policy Advisory Commission was created by the state legislature in 1990 with a mandate to create a system for criminal sentences that improved consistency in sentencing and would keep the incarcerated population within the capacity of state prisons (Wright and Ellis, 1993).[7] The commission presented its final recommendations to the legislature in January 1993; they were adopted with minimal changes on July 24, 1993,

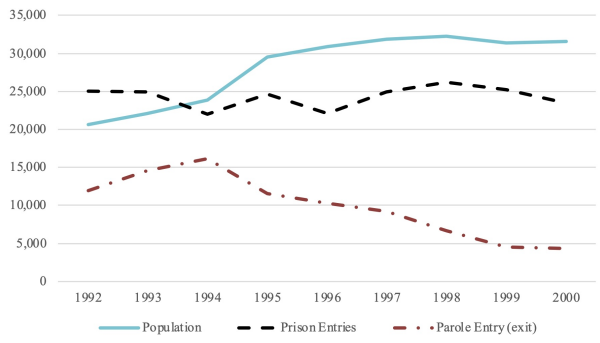
[7]Twenty-five states created a sentencing commission as part of the sentencing reform process during this time period (Neal and Rick, 2016).

with an enactment date of October 1, 1994 (Wright, 2002).[8]

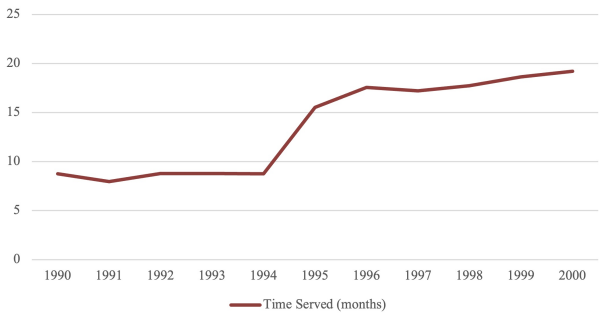
The NCSSA created a detailed sentencing grid based on offense type, severity, and previous criminal record. Judges could not give sentences outside of these narrower bands, which specified a specific minimum sentence for an inmate before which they could not be released.[9] The NCSSA also replaced the previous good time system with an "earned time" system, which was less generous to inmates, and abolished discretionary parole (Collins and Spencer, 1999). As shown in Figure 1, these changes dramatically affected North Carolina. After years of relative stability, the 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: Incarceration in North Carolina, before and after the NCSSA

(a) Prison entry, exit, and population



(b) Average time served by year of entry



Source: Bureau of Justice Statistics, National Prisoner Statistics, North Carolina Department of Public Safety. The calculation of mean time served excludes prisoners serving life sentences.

Figure 2a presents the entry and exit dynamics that caused this quick increase and return to stability. The figure shows no trend break in prison entries around the NCSSA's implementation, and other entrant characteristics (e.g., racial composition, age, felonies versus misdemeanors, percentage of drug offenses, percentage of males) also remain stable

[8]The NCSSA only applies to sentences for crimes committed on or after October 1, 1994. Individuals who were already in prison were not affected.

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

(North Carolina Department of Public Safety, 1992-2000). Instead, the increase in the prison population is driven by a decrease in exits.

Figure 2b shows that time served by prisoners sharply increased after 1994, doubling from an average of 8.7 months for prisoners who entered in 1994 to 17.5 months for those entering in 1996. 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.

Figure 3: Incarceration rates for high and low incarceration CZs

Source: North Carolina Department of Public Safety.

Important for my identification strategy, the NCSSA affected communities differently across the state, as the policy changes were on the intensive margin of incarceration: while the number of people entering prison remained stable, the length they served increased. As a result, in areas with relatively lower incarceration rates, the NCSSA had less impact compared to areas with higher rates of prison entry. In the spirit of a differences-in-differences analysis, Figure 3 plots the incarceration rate for males aged 15–45 separately for commuting zones (CZs) with incarceration rates above and below the median incarceration rate in 1990.

The figure shows that while the NCSSA's implementation increased incarceration rates in all areas, the effect was much stronger in high incarceration communities. The difference in incarceration rates across the two groups doubled from approximately 200–400 per 100,000 prime-aged men after the enactment. I expect the policy's effect to be strongest in communities where more men were incarcerated in the pre-period. More formally, Table 1 uses pre-NCSSA (1990–1993) incarceration rates to predict policy incarceration rates. This pseudo-first stage shows that pre-period incarceration rates are a strong predictor of rates after the NCSSA.

Table 1: Using pre-period incarceration rates to predict post-policy incarceration rates, 1995–2000

	Unweighted		Weighted	
	(1) No FX	(2) With FX	(3) No FX	(4) With FX
A: 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
B: 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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

North Carolina's 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 6.1, I discuss potential confounders and show visually and empirically that they are not driving this increase.

3 Data

Fertility- and birth-related outcomes from the North Carolina Detailed Birth Database from 1989–2014. These data include extensive information on both the birth and mother, including her age, race, and county of residence. I limit my sample to women aged 15–40 who reside in North Carolina, and only include Black and white mothers, who make up 96.8% of mothers in North Carolina from 1990 to 2000. I also use this sample to examine reported father characteristics and the composition of mothers. To compare total fertility across cohorts, I supplement these data with the National Center for Health Statistics' (NCHS) Natality Detail Data. These data are functionally identical to the North Carolina Detailed Birth Data but are available beginning in 1968. Data on marriage and cohabitation are from the IPUMS 1990 and 2000 decennial censuses (Ruggles et al., 2015).

The data used to measure a woman's partner market's level of exposure to the NCSSA are from the North Carolina DPS. I use public offender information from the universe of convictions in the state with SEER population data to create a measure of incarceration by age, race, and county. I also use data 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 6.1. These data are from the Federal Bureau of Investigation's Uniform Crime Reporting program and the Bureau of Labor Statistics, respectively. Additional information on these data and how they were prepared for analysis is in Appendix A3. Summary statistics for the full samples are in Table 2 and by race in Appendix table A6.

4 Specification

Because this policy was 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 effect on partner markets with higher pre-period incarceration rates, as

Table 2: 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.

seen in Figure 3. This empirical approach is often called an intensity-of-treatment research design.[10] 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.

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 were 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

[10]Other examples include Acemoglu and Johnson (2007), Bleakley (2007), and Lucas (2010).

of the policy should be much more pronounced among younger women.

Consistent with previous work, I define a woman's partner market to consist of men who are of her same race, in the same geographic area, and 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, as only 1%–2% of births report the father's race to be different than the mother's (Appendix 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 my analysis, fathers are slightly older (by 2.3–2.4 years) than mothers (Appendix Figure A4). Additionally, I can define partner markets at a sub-state level, improving upon prior work that assigned partner markets at the state level (Charles and Luoh, 2010; Mechoulan, 2011).

The geographic unit of analysis is the CZ, which is a unit of analysis designed to reflect observed patterns of economic and social activity. Market (and 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.[11,12] Additionally, aggregating from counties to CZs causes the effective number of observations within my clusters to 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 are not available at the county level.

I collapse the data into cells based on the woman's race, age, CZ of residence, and the time period of conception. Births and other outcomes are modeled as a function of pre-NCSSA incarceration rates using the following equation:

$$Y_{\rho t} = f(\beta Post_t * \ln(\overline{IR}^{9093})_\rho, \theta X_{\rho t}, \lambda_\rho, \gamma_t^A) + \varepsilon_{\rho t}, \quad (1)$$

[11]See Lindo (2015) for a discussion of the importance of accounting for these spillovers.
[12]For a map of North Carolina counties and CZs, see Appendix Figure A5.

where ρ is the woman's partner market and t is the time period of conception, with each year divided into two periods (January to June and July to December). I exclude the six-month period around the introduction of the law (July to December 1994) as I cannot precisely assign births that were conceived in that period to before or after the policy change. I use six-month time intervals so that after excluding births around the time of the law's passage, each cell is based on an equal time interval.[13]

For my primary outcome, this equation is estimated using a Poisson model, and $Y_{\rho t}$ is the number of births to women in partner market ρ, conceived in time period t. Studies examining the effect of market conditions on fertility frequently use the natural log of the birth rate as the dependent variable as births are rarely normally distributed (e.g., Dettling and Kearney, 2014; Schaller, 2016). Because I am examining 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 or log-log specification is not appropriate here. The Poisson model has the additional advantage that its estimate of the conditional mean is robust to model misspecification (Wooldridge, 1999). The main results are not dependent on this choice of functional form (see Section 6.3).

$\overline{IR}_{\rho}^{9093}$ is the average proportion of men incarcerated per woman in a partner market, as defined above, from 1990 to 1993 (the years leading up to the NCSSA's passage). β can be interpreted as an elasticity, similar to how one would interpret a linear regression on the natural log of the birth rate. I group women into five age groups: 15–19, 20–24, 25–29, 30–34, and 35–39. 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 aged 20–24, $\overline{IR}^{9093}$ would be the pre-NCSSA incarceration rate of same-race men aged 20–29 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 NCSSA's enactment.

λ_{ρ} is a fixed effect to capture time-unvarying characteristics of the partner market, andγ_t

[13]In Section 6.3 I show that the results are unchanged if the cells are collapsed by year. The results are also not sensitive to the inclusion of births conceived around the time of the law's enactment.

is a year fixed effect and is age specific in the pooled analyses. All analyses allow standard errors to be correlated within partner markets overtime.[14] Clustering the standard errors breaks the traditional link between mean and variance in Poisson specifications, and so the data do not need to be equi-dispersed to satisfy the assumptions for consistency (Cameron and Trivedi, 2010). $X_{\rho t}$ is a vector of time-varying controls for the crime rate, a measure of police efficacy, and the unemployment rate.[15] For the birth regressions, 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 run all regressions separately by race.This ensures that the estimates are never identified off cross-racial variation. Like the United States as a whole, incarceration rates for Black men are reliably higher than white men in North Carolina (see Appendix Figure A2 or Table A6). This discrepancy exists before and after the NCSSA, so it cannot be used to understand the relationship between sentencing severity and family formation. By estimating the effects of the NCSSA separately by race, I avoid this potential source of misidentification while also allowing for the possibility that Black and white women will respond to the policy in different ways.

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 the policy, not by 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 in Section 6.1.

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

[14]The standard errors are very similar when correlated across partner markets within a CZ-race group, as shown in Section 6.3.

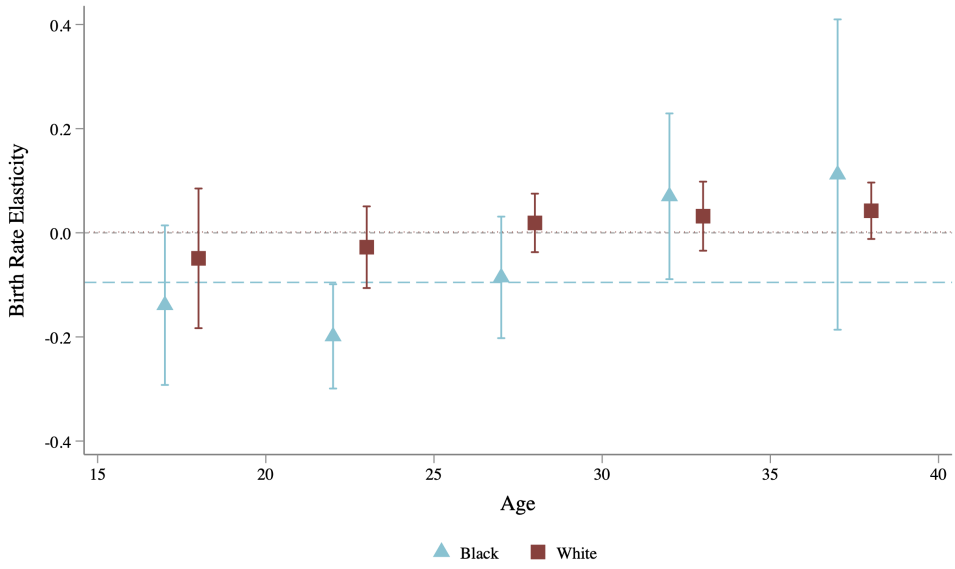
[15]In Section A4.2 I show that the results are not sensitive to the inclusion of these controls.

family formation has largely ignored this 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). Therefore, 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 4 presents the NCSSA's effect on births overall and by maternal age. The dashed and dotted lines represent the pooled effect for all Black and white women, respectively, and each point represents the result of estimating equation 1 by age-race group. The figure shows that the NCSSA decreased fertility among women in partner markets with high levels of incarceration before the law was enacted, particularly for Black women in their early 20s.

Figure 4: The effect of increased sentencing severity on total births



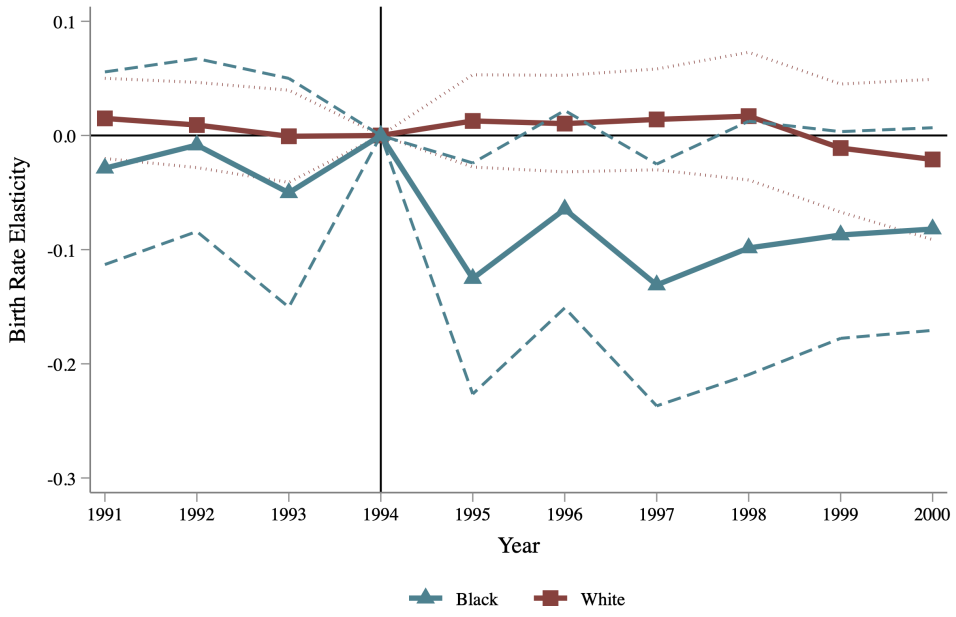
Notes: This figure represents the coefficients and 95% confidence intervals from a Poisson estimation of equation 1. See Section 4 for more details about the estimation procedure.

Specifically, the pooled coefficient on $Post_t * \ln(\overline{IR}_{\rho}^{9093})$ for Black women implies that a partner market with a 10% higher pre-period incarceration rate would have a 1% lower birth

rate. For example, the birth rate in partner markets at the 75th percentile incarceration rate (2.6%) are expected to see their birth rate fall 6% relative to those at the 25th percentile (1.6% incarceration rate). This is small relative to the total number of births in North Carolina, but reasonably relative to the absolute increase in the Black male prison population of around 6,400. The estimated impact is smaller than those 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 5 presents event-study coefficients that capture the dynamic effects before and after the NCSSA's enactment. Important for this empirical strategy, there is no statistical relationship between pre-period incarceration rates and births before the policy. However, following the policy, there is a visible decline in the number of births to Black women. Similar to the static analysis in Figure 4, there is no effect for white women.

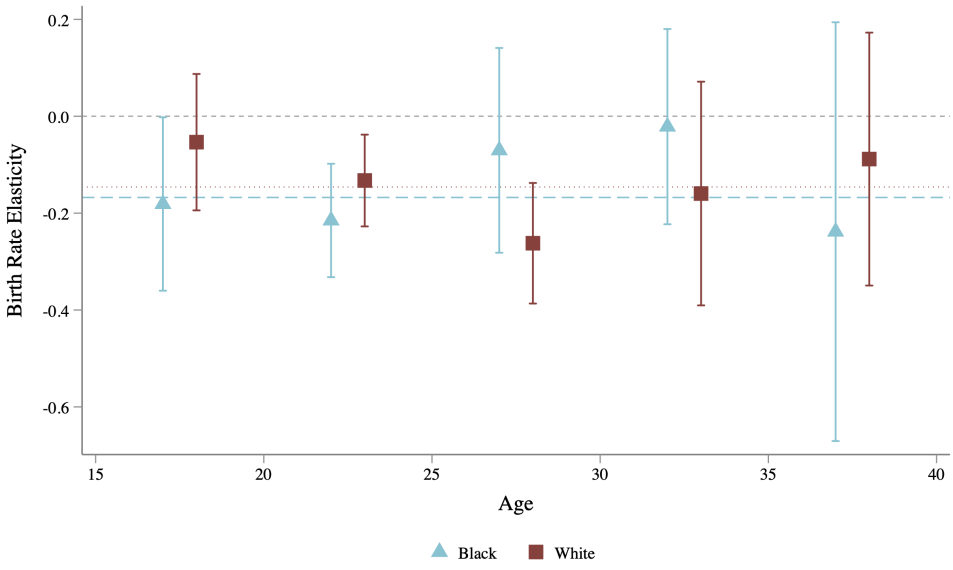
Figure 5: The dynamic effect of increased sentencing severity on total births



Notes: This figure represents the coefficients and 95% confidence intervals from equation 1 for all births in the sample. For more details, see Section 4.

Figures 6 and 7 repeat this analysis with the sample restricted to unmarried mothers, which is an important subgroup for two reasons. First, because most incarcerated men are unmarried (Western, 2006), unmarried women should be the most effected 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 particularly important for understanding the intergenerational effects of incarceration policies.

Figure 6: The effect of increased sentencing severity on births to unmarried women

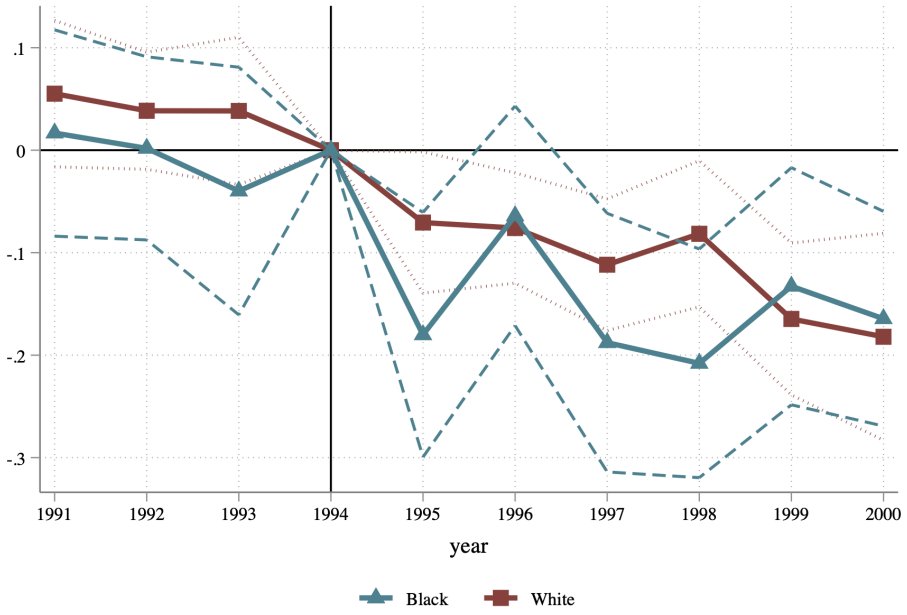


Notes: This figure represents the coefficients and 95% confidence intervals from equation 1 for unmarried women in the sample, as well as age and race subgroups. For more details, see Section 4.

Visible in both the static and dynamic versions, the decline in fertility resulting from

the NCSSA is stronger and more consistently negative for unmarried women. Although incarceration affected a smaller percentage of white men, there was still a fertility spillover to white women. Examining by age group, births to women through age 30 significantly decreased. In the dynamic specification, there is no statistical relationship between pre-period incarceration rates and births before the NCSSA, followed by a clear decrease after its enactment. In the case of births to unmarried women, this decrease is statistically significant for both white and Black women. Full regression results for these figures are available in Appendix Tables A3 and A4.

Figure 7: The dynamic effect of increased sentencing severity on births to unmarried women



Notes: This figure represents coefficients and 95% confidence intervals from equation 1 for unmarried women in the sample, as well as age and race subgroups. For more details, see Section 4.

Table 3 shows how the demographic composition of mothers changes in response to the NCSSA. Women who give birth after its enactment 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 in Panel A imply that Black mothers are 0.025 years older and 1.4 percentage points less likely to be teens. For white women, there is a shift away from mothers

without a high school diploma, consistent with the educational profile of incarcerated men. I repeat this analysis for the composition of women having their first birth and find similar results (Appendix Table A5).

Table 3: The effect of increased severity on maternal composition

	(1) Mean Age	(2) % Teens	(3) % Married	(4) % First Birth	(5) Yrs Edu	(6) % Less Than HS	(7) % HS/SC	(8) % Coll
A: Black mothers								
Post*9093 IR	0.961*	-0.052***	0.064+	-0.018	0.209	-0.027	0.002	0.026
	(0.354)	(0.016)	(0.032)	(0.019)	(0.133)	(0.019)	(0.019)	(0.024)
R-Squared	0.853	0.781	0.818	0.484	0.911	0.736	0.741	0.905
Cells	477	477	477	477	477	477	477	477
B: White mothers								
Post*9093 IR	0.208	-0.018***	-0.002	-0.010+	0.206	-0.039*	0.035***	0.004
	(0.122)	(0.006)	(0.005)	(0.006)	(0.134)	(0.019)	(0.012)	(0.011)
R-Squared	0.972	0.907	0.930	0.528	0.939	0.870	0.947	0.983
Cells	504	504	504	504	504	504	504	504

Notes: Observations are collapsed into race-CZ half-year cells, covering the years 1990–2000. The dependent variable in column (1) is the average age of women in the cell, and the dependent variable in column (4) 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 are weighted by the number of births in the cell. Standard errors, in parentheses, are clustered by race-CZ group. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The fertility of young and unmarried women decreased after the NCSSA's enactment, and the long-term ramifications of this change depend on whether the decline 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 the following equation:

$$Y_{\kappa rc} = \alpha + \beta \ln(\overline{IR}^{9093}_{\kappa rc}) + \omega_c + \nu_{\kappa rc}, \tag{2}$$

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 of Ananat et al. (2007) and Currie and Schwandt (2014) to create both measures. To capture the number of childless women, I sum all first births observed to women in a race and age cohort group in a CZ, which provides 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 sum 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 1,000 women, the intensive margin. I calculate both of these measures when the women in the cohort are age 35 and again at age 40.

Treatment is assigned to women according to their age in 1995. $\overline{IR}^{9093}_{\kappa rc}$ is the same measure of exposure used in previous specifications, but now 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_c is a CZ fixed effect, and $\nu_{\kappa rc}$ is a random error term, clustered at the partner market level.

The results, presented in Table 4, show that the effects of increased sentencing severity on long-term fertility further diverge for Black and white women. For white women, there is no statistically significant relationship between a cohort's partner market's exposure to

Table 4: 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: 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
B: 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 are collapsed into race-CZ-age group cells, including births from 1968 to 2014 to women born between 1955 and 1985. The dependent variable in columns (1) and (2) is the percentage of women in a cohort who are not observed having a first birth by the age denoted in the column header. The dependent variable in columns (3) and (4) is the total number of births per 1,000 women in a cohort by the age denoted in the column header. The fertility decrease p-value reports the p-value for a one-sided hypothesis test of fertility reduction (more childless women and fewer children born per women). Regressions are weighted by cohort size, and standard errors are clustered by partner market. $+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

the NCSSA and their later fertility outcomes, either on the extensive or intensive margins. Thus, the observed decline in fertility for unmarried white women was likely temporary, and those whose partner markets were most affected simply delayed their childbearing into the future. Delayed motherhood is associated with increased earnings (Miller, 2011), and by having children later in life, these women have had more time to accumulate human capital and other resources, which may contribute to their ability to make greater investments in their children (Bailey et al., 2014). However, the health effects of delaying childbearing 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.

For Black women, I find no evidence that the short-term declines in births to unmarried women translate into long-term decreases on the extensive or intensive margin of fertility. Instead, there is weak evidence of an increase in both the portion of women who ever have children and the total number of children they have. The effects are marginally significant and very small: when evaluated at the mean pre-NCSSA incarceration rate, this amounts to a 0.006–0.007 percentage point decrease in the number of women without children. On the intensive margin, these coefficients represent an increase of approximately 0.01 children born per 1,000 women. It may seem counterintuitive that a policy with an overall negative effect on births could increase the number of women with children. However, skewed sex ratios are associated with increases in risky sexual behavior, such as having multiple concurrent partners and a higher risk of sexual transmitted infections (Adimora et al., 2013; Green et al., 2012). If a subset of women respond to the change in the sex ratio with riskier sexual behavior, this could lead to more unintended pregnancies even as the overall number of births decreases.

5.2 Partner choice

Becker predicts that a change in the sex ratio will affect both the number of unions that form and the quality of those matches. 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 5 presents evidence that the NCSSA disrupted matching patterns for Black women. Panel A, 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 0.007 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, as prior work has shown that women are most likely to search for partners close to their own age (Hitsch et al., 2010), and women with older partners not only start out less satisfied with their relationships but their relationship satisfaction also declines more quickly (Lee and McKinnish, 2018).

Table 5: The effect of increased sentencing severity on father characteristics

	(1) Age Diff	(2) Has Less Education	(3) Missing
A: Black women			
Post*9093 IR	0.322***	0.026***	0.016
	(0.112)	(0.009)	(0.015)
R-Squared	0.591	0.722	0.911
Cells	2,001	2,001	2,074
B: White women			
Post*9093 IR	0.023	-0.001	0.015***
	(0.040)	(0.003)	(0.003)
R-Squared	0.913	0.731	0.934
Cells	2,513	2,513	2,514

Notes: Observations are collapsed into race-CZ-age group-half-year cells, including the years 1990–2000. The dependent variable in column (1) is the cell average of the father's reported age minus the mother's reported age. The 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 that of the mother. The 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) is missing from the birth record. Regressions are weighted by cell-level number of births, and standard errors are clustered by partner market. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Research shows that women generally prefer to partner with men of a similar education level (Hitsch et al., 2010). However, column (2) shows that the NCSSA also increased Black mothers' probability of partnering with a man who had a relatively lower level of education, an effect size of 0.2% relative to the pre-period mean. Therefore, in communities most affected by the NCSSA, Black women who continued to have children were more likely to match with lower-quality partners. Additionally, the NCSSA did not affect the probability

that the father was missing from the birth records of Black mothers.

The effects on partner choice are less clear for white women. Panel B of Table 5 shows that while there is no observable change in partner composition, the probability of the father missing from the birth record increased. Evaluated at the pre-period mean rate of white male incarceration, this implies a 0.1% relative increase in probability. 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 continued to have children after the enactment were having them with less committed partners and that those children would 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. Because the data on marital status are structured differently than the birth data used above, I estimate a modified version of the preferred specification in Charles and Luoh (2010). The intuition of the specification is the same.

$$\Delta Y_\rho = \beta \ln(\overline{IR}^{9093})_\rho + \theta \Delta X_\rho + \mu_\rho, \tag{3}$$

where ρ continues to denote the partner market, defined at the age, race, and CZ level, 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 use the same control variables for crime, police efficacy, and unemployment described 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-race level.

Table 6 shows that white women in partner markets most affected by the NCSSA were less likely to be married after the enactment. This decline in marriage is not due to an increase in divorce but 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 by the 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 the stigma effect is driving part of the effect observed by previous work: 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.

Table 6: The effect of increased sentencing severity on marriage, divorce, and cohabitation

	(1) Married	(2) Divorced	(3) Never Married	(4) Cohabiting
A: 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
B: 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 are collapsed into race-CZ-age group cells. The dependent variable is the change in the percentage of women in a cell who report the relationship status in the column header from 1990 to 2000. Cells are weighted by the 1990 female population. Standard errors, in parentheses, are clustered by race-CZ group. $+$ $p < 0.1$, $*$ $p < 0.05$, $**$ $p < 0.01$, $***$ $p < 0.001$.

Column (4) shows that most of these women were still forming unions, shifting away from marriage into cohabitation. Like the previous result on "missing" fathers, this provides additional evidence that the NCSSA changed commitment levels in white women's relation-

ships. 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). Previous work has not examined the effects on cohabitation, so I cannot compare the effect sizes for this outcome. However, this work shows that this is an important margin of adjustment.

I find 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 of primed-aged Black men was about six times larger than that of white men in North Carolina in this period. While the incarceration rate for both groups roughly doubled after the NCSAA, the magnitude of the changes differed. Throughout the decade, the incarceration rate for white men remained 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 that draws from both types of variation and also finds no effect on marriage for Black women.

The increase in sentencing severity that followed the NCSSA's enactment had a defined effect on women's fertility and family formation patterns, as it led to decreased fertility, particularly among young and unmarried women. There is no evidence that this policy

reduced completed fertility, implying that the decline was temporary for these women. By delaying their fertility after their teens and early 20s, they 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 were less likely to be married and more likely to be cohabiting at the end of the decade. I find no effects on marriage outcomes for Black women, potentially consistent with the marriageable men hypothesis.

6 Robustness

6.1 Threats to identification

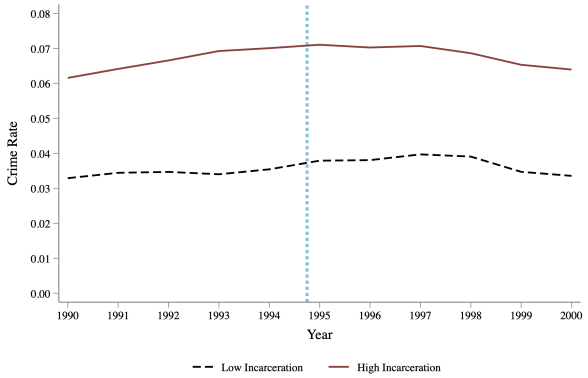
One may worry that a factor other than the policy was driving the increase in incarceration rates, which could also affect family formation and fertility. In this section I discuss several potential factors, including crime, enforcement, and labor markets. An increase in crime may change women's preferences around partnership and childbearing, regardless of any change to sentencing policy. Alternatively, the change in fertility estimated above may have its own effects on the crime rate: pregnancy and birth lead to a decrease in criminal arrests for both women and men (Massenkoff and Rose, 2024).

In the spirit of a differences-in-differences analysis, Figure 8a plots the North Carolina crime rate for two groups of CZs over the study's period of analysis with a vertical line to indicate the NCSSA's implementation. To ensure that 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. As with the rest of the United States, North Carolina's crime rates plateaued in the mid-1990s, followed by a decrease (Lofstrom and Raphael, 2016). Importantly, for this identification strategy, there

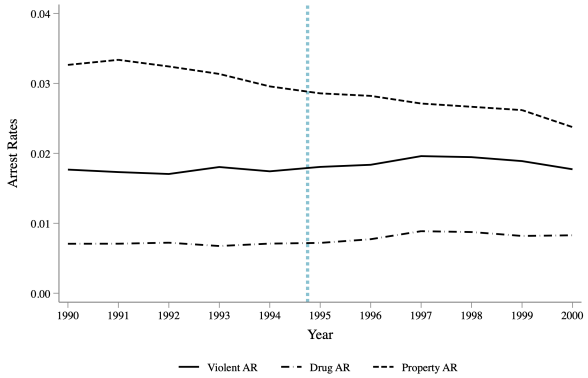
is no spike around the policy's implementation, and the trends in crime are similar for both high and low incarceration areas.

Figure 8: The NCSSA and potential confounders, 1990–2000

(a) Crime rates



(b) Offense-specific arrest rates



(c) Unemployment rates

Notes: CZs are divided into high and low incarceration groups based on the median pre-period incarceration rate. Source: North Carolina Department of Public Safety, FBI Unified Crime Reporting Program, Bureau of Labor Statistics.

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 are conviction based. These represent only a subset of crimes adjudicated by the court system and do not include any information on cases where the defendant is not convicted. A contemporary report commissioned to understand the NCSSA's effect 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 it was enacted), and it found no differences in the demographic composition of defendants or the fraction of cases that were felonies versus misdemeanors. The authors also reported 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 8b displays offense-specific arrest rates with a vertical line to indicate the NCSSA's passage. Arrest rates for violent 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 NCSSA's enactment (Appendix Figure A6). The empirical strategy used by Charles and Luoh (2010) could not be meaningfully applied in this setting.

Another possibility is that my results reflect changes in labor market conditions. Theories of criminal activity predict that a tight labor market with low unemployment will lead to a decrease in crime and incarceration, suggesting that 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 8c 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, 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 approach takes advantage of the econometric fact that a regression is more robust to measurement error in the dependent variable than in the independent variable. To apply this test in my setting, I estimate equation 1 using OLS, with 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, providing additional support to my identification strategy.[16] Because the NCSSA is not affecting these measures, they are appropriate to include in the analysis in order to be more consistent with prior work in this area (Charles and Luoh, 2010; Mechoulan, 2011) and increase the precision of the estimates. The results are also robust to the inclusion and exclusion of this vector of controls, as discussed in Section 6.3.

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 data on five-year migration status in the 2000 census to see whether groups of women differentially exposed to the NCSSA through their partner market are more or less likely to migrate. I classify a woman as a migrant if she reports moving across public use microdata areas between 1995 and 2000. I then 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 (Appendix Table A2).

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 that compare callback rates

[16]The results of these balancing tests are shown in Appendix Table A1.

regularly find that job candidates with otherwise identical resumes and qualifications are less likely to receive a callback if they have a criminal record (Pager, 2008). More recently, studies use the random assignment of cases to judges with different propensities to incarcerate defendants to understand how incarceration affects later outcomes for individuals convicted of a crime. While they 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 that an additional year in prison results in decreased earnings.

However, both of these identification strategies provide an estimate for the marginal potential inmate. In my setting, none of the 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. Using sentencing discontinuities created by the NCSSA, Garin et al. (2024) show there is no effect of past incarceration on earnings and employment. While partner markets may react differently to longer sentences than labor markets, this is suggestive evidence that longer sentences should not create additional stigma for previously incarcerated men.

6.2 Placebo tests

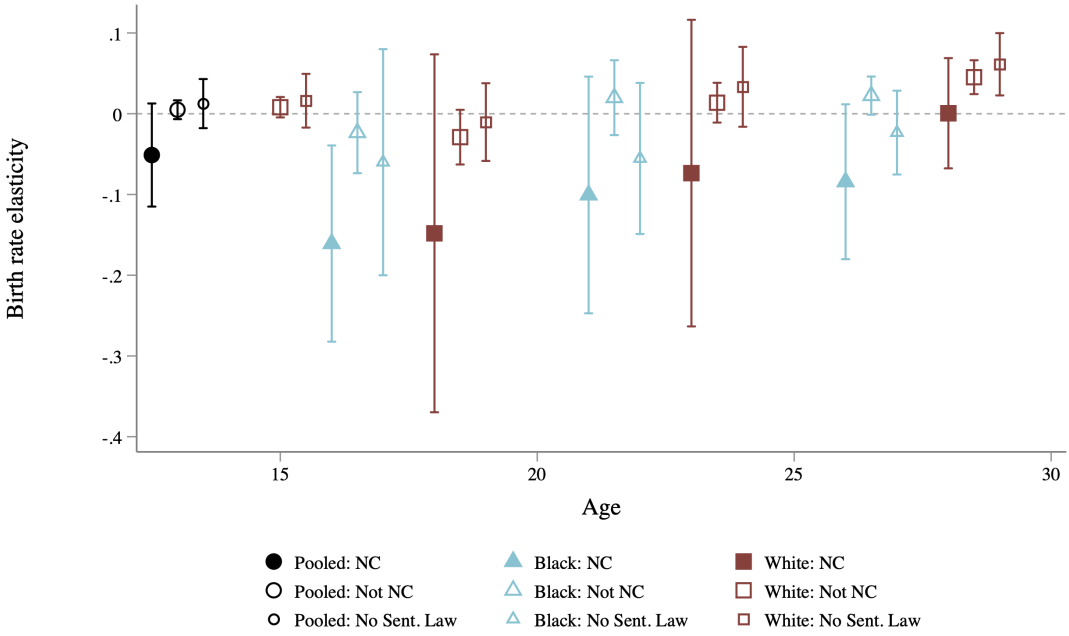
To be confident that my results capture a true relationship between sentencing severity and fertility, I run a series of placebo regressions using data from other states. Unfortunately, I cannot observe age- and race-specific incarceration rates at the sub-state level in other states. However, the Vera Institute of Justice has collected county-level prison population data by race as part of its Incarceration Trends project. During the time period of this study, data are available for 33 states, including North Carolina. I combine these data with public use data on births and estimate equation 1 for three groups. Full details about the data and specification are in Section A4.1.

Figure 9a presents the results of these regressions for young women. First, I reproduce

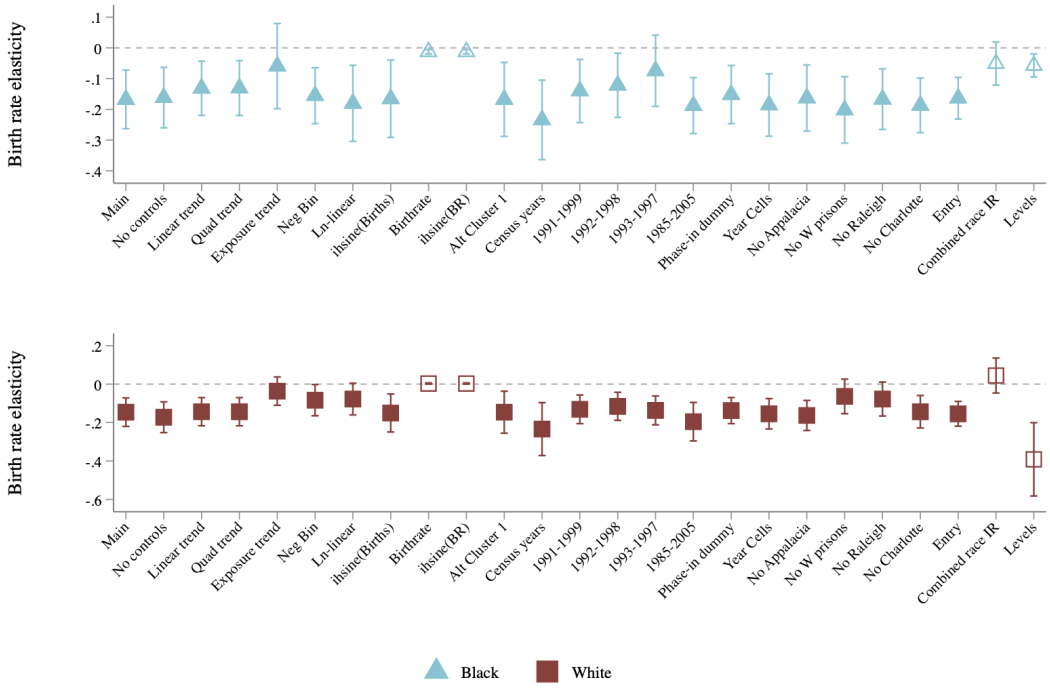
the results for North Carolina. Given the differences in data quality, it is not surprising that these estimates are less precise. However, the overall pattern is similar to my primary analysis, shown in Figure 4. Overall, births decrease for women exposed to the NCSAA through their partner markets. This decline is strongest for Black teenagers, though the point estimates for other young women are also negative (albeit imprecisely estimated). The effects I observe in North Carolina are not driven by broad national-level trends. To provide a potential placebo group, I next run the same regression for all non-North Carolina states with available data. However, during the "tough-on-crime" era, many other states passed sentencing reforms in the 1990s (Travis et al., 2014), and while these laws are not identical to the NCSSA, these states do not represent a true control group.

Figure 9: Summary of robustness checks

(a) Alternate estimates of fertility effects: North Carolina versus other states



(b) Summary of sensitivity analysis



Notes: These figure present summaries of a placebo test and other sensitivity checks. In figure (b), the coefficients represented by an unfilled symbol represent robustness checks where either the left or right hand side of the regression is scaled differently. For complete details, see Sections 6.3 and A4.

For the final comparison, I repeat the analysis for the subset of states with incarceration data that did not pass similar sentencing laws during this time period. For both potential placebo groups, there is no evidence that the 1990–1993 incarceration rates predict declines in fertility. The overall effect is statistically zero, and the point estimates for all other groups are both smaller in magnitude than that of North Carolina and generally not statistically significant. If anything, the national-level trends point to an increase in fertility for white women aged 25–30, the opposite of what I find.

6.3 Specification sensitivity

I also perform several 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 to the exclusion of control variables, the inclusion of linear time trends, different years, and alternative standard error clusters. The results are also not sensitive to alternative measures of women's partner market exposure to the NCSSA. Finally, I find that these results are robust to the exclusion of select geographic areas. Figure 9b provides a summary of these results for births to unmarried women. The full results of these analyses are discussed in Section A4.2.

7 Conclusion

Mass incarceration is a uniquely American experiment with potential spillover effects beyond those directly imprisoned. In this paper, I leverage previously unexplored variation, from the North Carolina Structured Sentencing Act, to identify the effects of a policy change that increased sentencing severity on women's fertility and family formation. Between 1994 and 1995, the incarceration rate in North Carolina increased by one-third due to this policy, effectively increasing the length of prison sentences and creating a natural experiment that allows me to isolate the effects of incapacitation due to incarceration.

I find that the increased sentencing severity decreased 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. I find no evidence of decreases in total fertility by ages 35 or 40, indicating that 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 future children. I also find that the composition of mothers shifted toward women of higher socioeconomic status.

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

Like the results for fathers, the NCSSA's effect on marital status also differed by race. For white women, I again find evidence of less committed relationships. After the NCSSA, white women were 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 stem from the fact that 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 almost 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 underscores 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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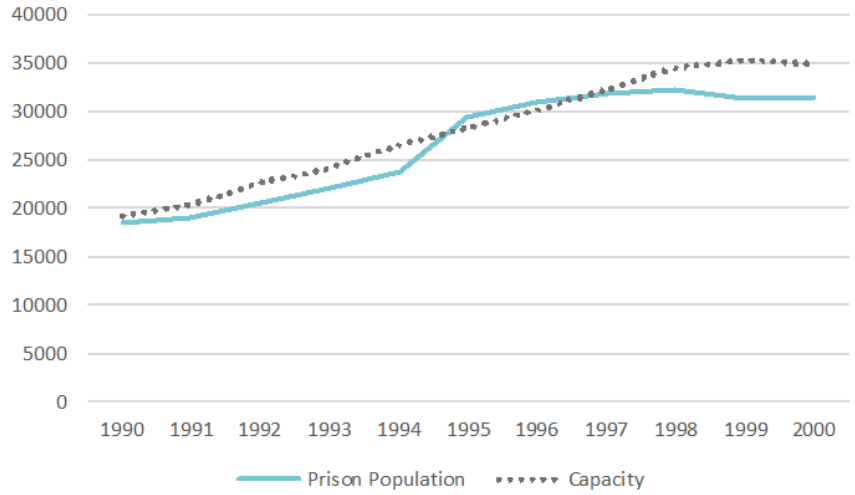
Appendix

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

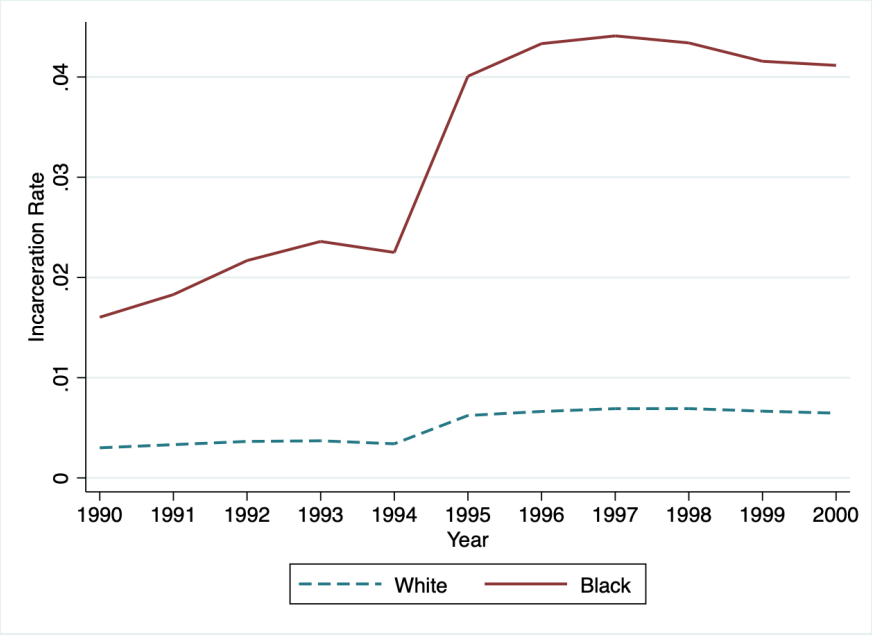
A1 Additional figures

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



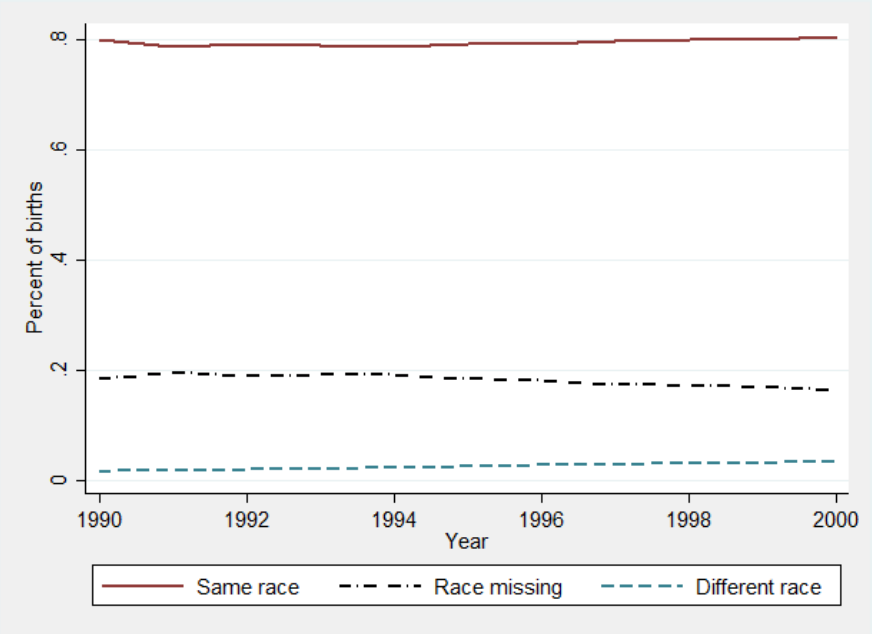
Source: Author's calculations using data from the North Carolina Department of Public Safety and the Census of State and Federal Adult Correctional Facilities. Information on the location of correctional institutions, as well as their capacity and staffing information, are from the Census of State and Federal Adult Correctional Facilities (CSFACF). Collected every five years by the Bureau of Justice Statistics, this series includes information on all correctional facilities in the state, including when they opened and how many prisoners they can accommodate, as well as their number of full time staff. To convert this information from a quinquennial series to a yearly series, I assume any major increase or decrease in staffing or capacity occurs at the time of a renovation or a move to a new building. For example, the Warren Correctional Center reports a maximum capacity of 56 in 1995 and 668 in 2000, with reported full-time staff increasing from 62 to 340. This institution moved to a new building in 1997, so I assign this increase to 1997. When necessary, information from the CSFACF is supplemented with information on opening, closing, and renovation dates from the North Carolina DPS website.

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



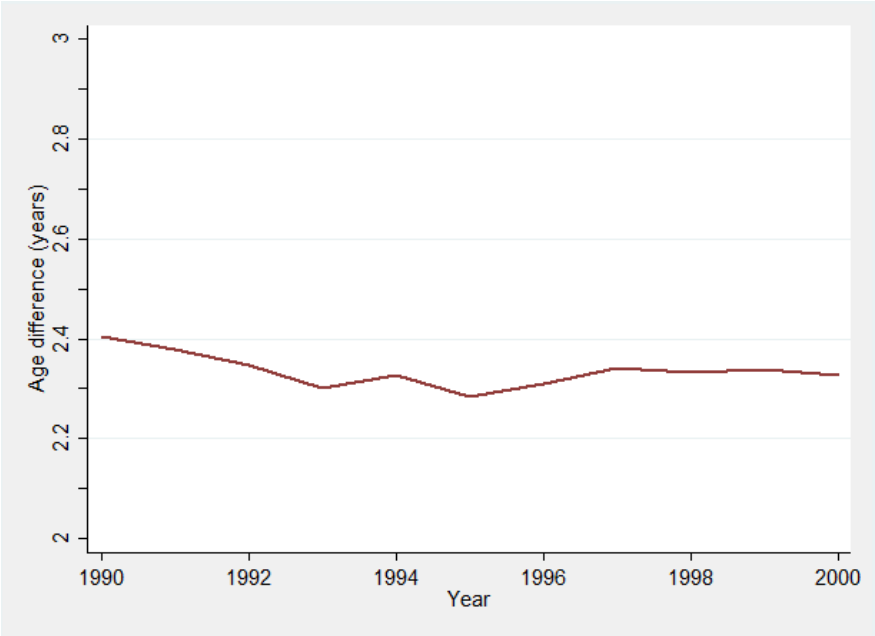
Includes men 15-44. Source: North Carolina Department of Public Safety and author's calculations.

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

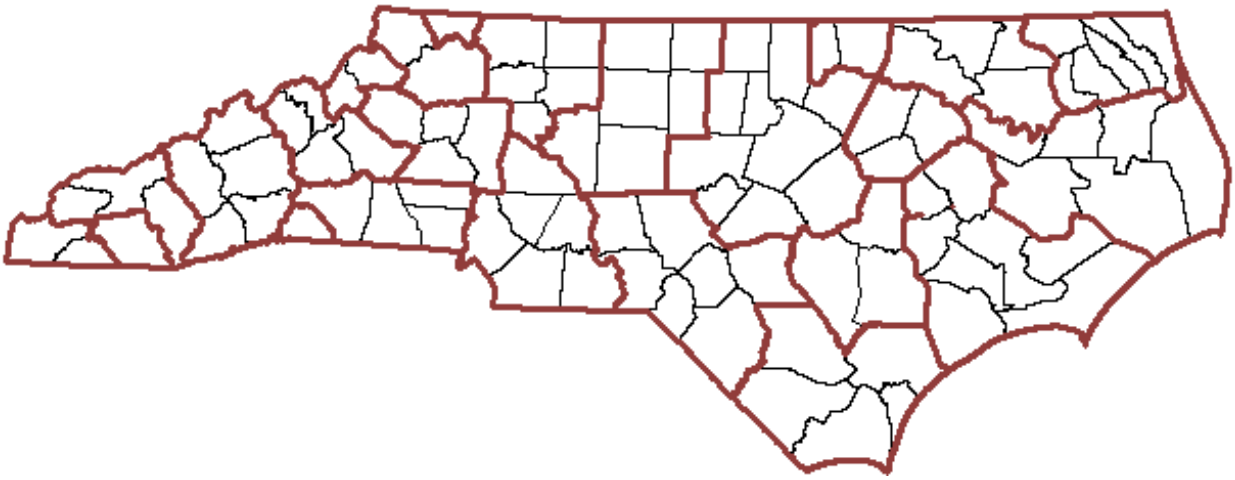
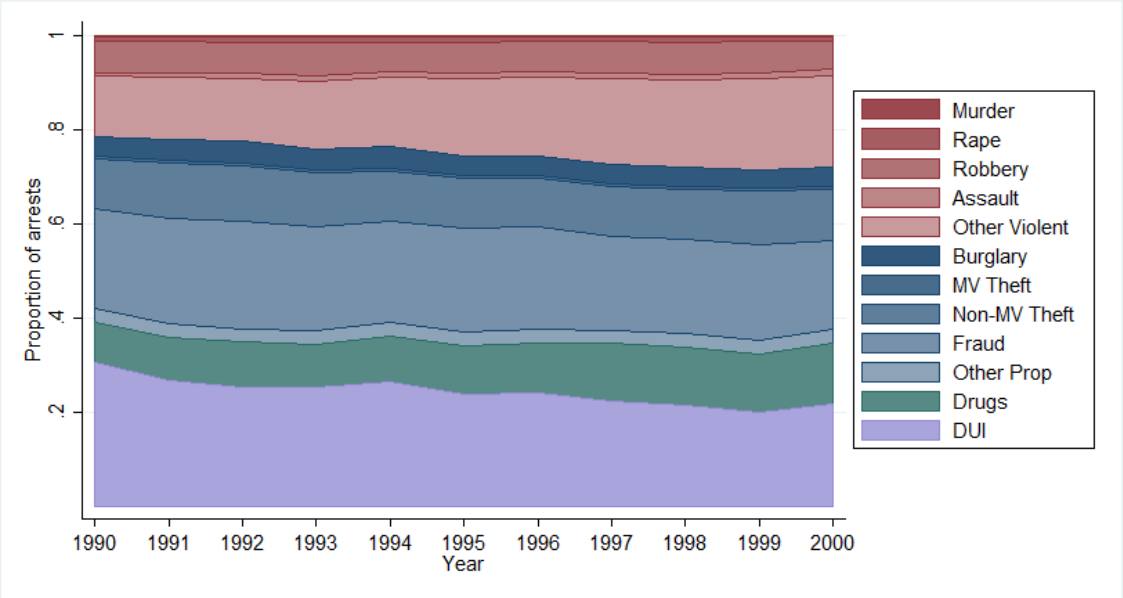


Figure A6: Offense composition of major arrests, 1990 – 2000



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

A2 Additional tables

Table A1: 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)	(0.006)	(0.001)
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
	(0.001)	(0.008)	(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 partner market. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A2: 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
	(1.618)	(1.546)	(8.914)
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 A3: 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: Black women						
Post*9093 IR	-0.095*	-0.139+	-0.199***	-0.086	0.070	0.112
	(0.039)	(0.078)	(0.051)	(0.060)	(0.081)	(0.152)
Crime Rate	0.526	0.464	2.108	-2.064*	-2.718*	-0.750
	(1.027)	(1.502)	(1.482)	(0.977)	(1.385)	(2.138)
Clearance Rate	0.115	-0.023	0.158	-0.012	0.059	0.547
	(0.088)	(0.127)	(0.143)	(0.136)	(0.168)	(0.422)
Unemp. Rate	-0.410	0.400	0.677	-2.365*	-2.100	-4.134*
	(0.580)	(1.148)	(0.806)	(1.002)	(1.303)	(1.852)
Cells	2415	483	483	483	483	483
B: White women						
Post*9093 IR	0.002	-0.049	-0.028	0.019	0.032	0.042
	(0.023)	(0.068)	(0.040)	(0.029)	(0.034)	(0.028)
Crime Rate	-1.511*	-3.017*	-2.485+	-0.554	-0.367	-2.346*
	(0.659)	(1.278)	(1.294)	(1.011)	(0.957)	(0.928)
Clearance Rate	-0.200+	-0.490***	-0.341	0.011	-0.095	-0.129
	(0.105)	(0.190)	(0.281)	(0.169)	(0.110)	(0.154)
Unemp. Rate	-0.181	0.563	0.126	-0.036	-1.179+	-0.976
	(0.531)	(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 partner market. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A4: 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: Black women						
Post*9093 IR	-0.168***	-0.181*	-0.215***	-0.070	-0.021	-0.238
	(0.049)	(0.091)	(0.060)	(0.108)	(0.103)	(0.221)
Crime Rate	-0.136	-1.317	2.511*	-3.611+	0.018	-8.415*
	(1.089)	(1.822)	(1.256)	(1.894)	(3.108)	(3.514)
Clearance Rate	-0.036	-0.195+	0.223	-0.454	-0.025	0.926+
	(0.130)	(0.100)	(0.163)	(0.334)	(0.300)	(0.474)
Unemp. Rate	-0.428	-0.101	0.888	-2.853+	-0.868	-7.855*
	(0.654)	(1.180)	(0.887)	(1.509)	(1.992)	(3.131)
Cells	2371	483	464	481	483	460
B: White women						
Post*9093 IR	-0.146***	-0.053	-0.133***	-0.262***	-0.159	-0.088
	(0.038)	(0.072)	(0.048)	(0.064)	(0.118)	(0.133)
Crime Rate	-3.759***	-3.724+	-6.110***	-2.927	0.439	1.359
	(1.202)	(1.981)	(1.843)	(2.927)	(3.152)	(2.440)
Clearance Rate	-0.383*	-0.353+	-0.488+	0.040	-0.448	-1.643*
	(0.150)	(0.207)	(0.256)	(0.306)	(0.724)	(0.775)
Unemp. Rate	0.662	-0.104	0.564	0.016	2.611	5.669
	(1.062)	(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 partner market. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A5: 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: 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
B: 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-race group. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

A3 Data Appendix

This section contains additional information on the data sets used in this project and how they were prepared for analysis.

Outcome data: I obtain fertility- and birth-related outcomes from the North Carolina Detailed Birth Database. These files contain information derived from birth certificates for the universe of births occurring in North Carolina since 1989. I limit my sample to women aged 15–40 who reside in North Carolina, and only include Black and white mothers, who make up 96.8% of mothers in North Carolina from 1990 to 2000. These data include extensive information on both the birth and mother, including her age, race, and county of residence. Most records include information on the completed weeks of gestation; for 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 data to examine reported father characteristics and the composition of mothers. Data on fathers include age, race, and education. To compare total fertility across cohorts, I supplement these data with the National Center for Health Statistics' (NCHS) Natality Detail Data. These data are functionally identical to the North Carolina Detailed Birth Data but are available beginning in 1968. Marriage and cohabitation data are from the IPUMS 1990 and 2000 decennial censuses. I limit my sample to Black and white women who live in North Carolina aged 20–40.

Incarceration measures: The data used to measure a woman's partner market's level of exposure to the NCSSA are from the North Carolina DPS. To create a measure of incarceration by age, race, and county, I use public offender information from the universe of convictions in the state, which 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 county of conviction.

I restrict my sample to white and Black men aged 15–44. I observe too few men from other races to create consistent series for those groups. 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. Next, I follow the procedure for calculating the prison population used by the DPS, where 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 SEER. I use the female population for two reasons. First, because skewing the sex ratio is an important mechanism by which incarceration affects partner markets, the number of men incarcerated per woman more directly captures this effect than the traditional incarceration rate. Second, using the female population 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. Women are incarcerated at a much lower rate, so any measurement error from misassignment is smaller. As a robustness check, I exclude areas with a women's correctional facility (Appendix Table A7c).

Additional data: I also use data on crime rates, police efficacy, and unemployment in my primary analyses to capture additional time-varying factors that could influence partner markets, discussed in Section 6.1. Data on crime rates (defined as the number of offenses known to the police divided by the population) and clearance rates (the number of clearances

divided by the total number of offenses known to the police) come from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) program. The FBI considers a crime "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 the analysis, I aggregate them to the CZ level following the USDA 1990 CZ definitions. To estimate the effect of the NCSSA by marital status, I use data from the IPUMS 1980–2000 census to create a population series by sex, age, race, and marital status (Ruggles et al., 2015). I allocate the Census Bureau's 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 6.3.

Table A6: 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.

A4 Additional Robustness

A4.1 Placebo regressions using other states

This analysis focuses specifically on one state. This decision has several advantages. Given the fractured nature of state policy making, focusing on one state avoids treating two states with different policies as equally treated while also avoiding including a partially treated state in the so-called control group. Additionally, the availability of the conviction data in North Carolina allows me to more accurately model in partner markets by taking age into account. Similar data is not available in other states for this time period. However, one may worry that the effects I find in North Carolina are spurious or driven by some broader national trend that happens to align with the date of the NCSAA. To investigate this, I re-estimate equation 1 for both North Carolina and other states. The main outcome of interest is the number of births, estimated using a Poisson process. This is modeled as a function of the 1990-1993 race-specific county incarceration rate interacted with a post variable. The additional variables included in the regression (female population, crime rate, clearance rate, and unemployment rate) are nationally available and are the same as detailed in section 3.

There are several differences between these regressions and the main results, all driven by differences in data availability. First, public-use natality data only identifies the county of residence for counties with at least 100,000 residents and so only large counties are included in the analysis. As a result, I define the partner market at the county level instead of the CZ level, since for most CZs I only observe the most populated county (if any) in the CZ. Additionally, the most detailed incarceration information I am able to observe across multiple states for the preperiod (1990–1993) is available by county and race. This is less precise than what I use in the main results, where the incarceration rate is also gender and age specific. Because of these differences, I re-estimate the effect for North Carolina as well as for other states. While the results for North Carolina are less precise than the main results using more detailed data, the pattern is the same: women in partner markets more exposed

to the NCSSA see declines in fertility. The results are strongest for Black teenagers.

Additionally, county-level prison population data are only available for 33 states during this period. Data are not available for Alaska, Arizona, Arkansas, Connecticut, Delaware, the District of Columbia, Hawaii, Idaho, Indiana, Kansas, Massachusetts, Montana, Nevada, New Mexico, Ohio, Rhode Island, and Vermont; these states are not included for this reason. The "Not NC" group is comprised of all other states. However, as noted in the main text, many of these states also passed laws related to sentencing severity during this time period, making them imperfect controls for North Carolina. There are a handful of states that did not pass similar sentencing laws during this time period (Ditton and Wilson, 1999), four of which also have county-level incarceration data: Alabama, Colorado, New Jersey, and South Dakota. These states are the "No Sent. Law" group. There is no evidence that women in partner markets with higher levels of increased incarceration in 1990–1993 decreased their fertility in this time. If anything, white women in these partner markets may have been increasing their number of births. If this is true and represents a true national-level trend, this implies that results I find for white women ages 20 to 30 using more precise data are potentially attenuated. Either way, the effects I observe in North Carolina are clearly not driven by broad national level trends.

A4.2 Sensitivity analysis

I perform a variety of checks to test the robustness of my preferred specification. The results of these analyses on births to unmarried women are presented in tables A7a through A7c and summarized in figure 9b. For reference, the first column of table A7a show the baseline estimates. In column (2), I show the results are robust to the exclusion of control variables. In columns (3) through (5), I include a CZ specific linear or quadratic time trend or an exposure-level specific linear time trend. The CZ-level time trends produce very similar results. The results for the exposure-level specific time trend are attenuated, but as this specification gives 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 results are not dependent on the choice of functional form. Column (6) of table A7a displays the estimate using a more common log-linear specification which produces similar estimates. Columns (7) through (10) use alternative functional forms, including the negative binomial, the inverse hyperbolic sine, and the raw birth rate. Results are generally robust to these choices. The effect using the birthrate is attenuated, although the result for Black women remains statistically significant and is qualitatively similar to my main result: a partner market with a 1% pre-period incarceration rate would expect a 0.012 percentage point decrease in the birth rate. Relative to mean birthrate this is a .15% decrease in births, which is remarkably close to the coefficient in column (1). Finally, in my preferred specification, I cluster standard errors at the partner market level. I choose this level in part because it gives a large number of clusters. However, it is possible that idiosyncratic shocks could reverberate across partner markets. With this in mind, I try an alternative cluster defined at the CZ-race level. This cluster definition produces 48 clusters. Using this definition does not change the interpretation of the results.

These results are also to robust the inclusion or exclusion of additional years, as seen in table A7b. 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. Next, I estimate the results around a shrinking time window (1991–1999, 1992–1998, 1993–1997) around the introduction of the NCSAA. Alternatively, I also include a wider time window by adding an additional 5 years to each side of the estimate. In all 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.

I also try alternative measures of a partner market's exposure to increased incarceration that resulted from the NCSSA. These results are presented in tables A7c. 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. The overall conclusion is unchanged. 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, there is still suggestive evidence of a negative relationship.

Finally, I exclude selected areas from the analysis to ensure that my results are not being driven by any one geographic area. 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. The results are robust to these exclusions.

Table A7a: The effect of increased sentencing severity on births, specification checks

	Specification										
	(1) Main	(2) No Controls	(3) + CZ Trend	(4) +Quad Trend	(5) + Exp. Trend	(6) Linear	(7) Neg. Bin	(8) IHS(Births)	(9) BR	(10) IHS(BR)	(11) Alt. Cluster
main											
A: Black women											
Post*9093 IR	-0.168***	-0.162**	-0.131**	-0.131**	-0.059	-0.180**	-0.155***	-0.165*	-0.012**	-0.013***	-0.168**
	(0.049)	(0.050)	(0.045)	(0.045)	(0.071)	(0.063)	(0.046)	(0.064)	(0.004)	(0.004)	(0.061)
Cells	2371	2371	2371	2371	2371	1984	2413	2436	2413	2413	2371
main											
B: White women											
Post*9093 IR	-0.146***	-0.172***	-0.143***	-0.144***	-0.036	-0.078+	-0.083*	-0.150**	0.003+	0.003*	-0.146**
	(0.038)	(0.041)	(0.037)	(0.037)	(0.038)	(0.042)	(0.041)	(0.051)	(0.002)	(0.002)	(0.056)
Cells	2520	2520	2520	2520	2520	2412	2520	2520	2520	2520	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 for unmarried women. Dependent variable in all other columns is the number of births to unmarried women. Non-linear regressions include the natural log of the applicable female population as a regressor. Linear regression is weighted by the applicable female population. Standard errors, in parenthesis, are clustered by CZ. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A7b: The effect of increased sentencing severity on births, temporal checks

	Time					
	(1) Census Yrs	(2) 1991-1999	(3) 1992-1998	(4) 1993-1997	(5) 1985-2005	(6) Phase-in 𝕀
A: Black women						
Post*9093 IR	-0.234***	-0.140**	-0.121*	-0.074	-0.188***	-0.152**
	(0.066)	(0.052)	(0.053)	(0.059)	(0.046)	(0.048)
Cells	430	1921	1456	1008	4715	2484
B: White women						
Post*9093 IR	-0.234***	-0.131***	-0.116**	-0.137***	-0.195***	-0.138***
	(0.070)	(0.038)	(0.037)	(0.038)	(0.051)	(0.035)
Cells	480	2040	1560	1080	4920	2640

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 applicable female population as a regressor. Linear regression is weighted by the applicable female population. Standard errors, in parenthesis, are clustered by CZ. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A7c: The effect of increased sentencing severity on births, alternative exposure measures and geographic checks

	Exposure Measure		Geography			
	(1) Entry	(2) Level	(3) No Appalachia	(4) No W Prison	(5) No Raleigh	(6) No Charlotte
A: Black women						
Post*9093 IR	-0.164***	-0.057**	-0.163**	-0.202***	-0.167***	-0.187***
	(0.035)	(0.019)	(0.055)	(0.055)	(0.050)	(0.045)
Cells	2413	2455	1470	2161	2266	2266
B: White women						
Post*9093 IR	-0.155***	-0.391***	-0.163***	-0.064	-0.078^{+}	-0.144***
	(0.033)	(0.097)	(0.040)	(0.046)	(0.045)	(0.043)
Cells	2520	2520	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 applicable female population as a regressor. Linear regression is weighted by the applicable female population. Standard errors, in parenthesis, are clustered by CZ. $+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.