The Impact of Youth Medicaid Eligibility
on Adult Incarceration[*]

Samuel Arenberg[†]   Seth Neller[‡]   Sam Stripling[§]

November 9, 2020

This paper identifies an important spillover associated with public health insurance: reduced incarceration. In 1990, Congress passed legislation that increased Medicaid eligibility for individuals born after September 30, 1983. We show that Black children born just after the cutoff are 5 percent less likely to be incarcerated by age 28, driven primarily by a decrease in incarcerations connected to financially motivated offenses. Children of other races, who experienced almost no gain in Medicaid coverage as a result of the policy, demonstrate no such decline. We find that reduced incarceration in adulthood substantially offsets the initial costs of expanding eligibility.

[†]The University of Texas at Austin. Email: samuel.arenberg@gmail.com
[‡]The University of Texas at Austin. Email: seth.neller@gmail.com
[§]The University of Texas at Austin. Email: samstripling@utexas.edu

1   Introduction

As municipalities across the United States consider models of public safety that place more emphasis on social programs, several elected officials have announced plans to increase spending on public health (e.g., Los Angeles Times, 2020; New York Times, 2020; Washington Post, 2020). In a June 8, 2020 television interview, United States Senator Kamala Harris summarized these efforts, "We need to re-imagine how we, as a society, are going to achieve public safety... We should be putting resources into our public health systems. We should be looking at our budgets and asking, 'Are we getting the best return on investment as taxpayers?'" This paper provides evidence that an investment in public health systems can indeed improve public safety and can do so in a cost-effective manner.

The investment in public health we examine is an expansion of Medicaid, the largest means-tested program in the United States. In the late 1980s and early 1990s, Congress sought to increase health insurance coverage among disadvantaged children. We study the coverage expansion included in the Omnibus Budget Reconciliation Act of 1990 (henceforth, "OBRA90" or "the Expansion"), which greatly expanded Medicaid eligibility nationwide among children in families below the Federal Poverty Level. The public safety outcome we examine is incarceration, which represents a substantial burden on families, neighborhoods, and governments. Given the high social cost of incarceration, investments yielding even modest effects are likely to be cost-effective (Freeman, 1996). The question we ask is whether receiving Medicaid eligibility as a child reduces the probability of going to prison as an adult.

The OBRA90 Medicaid expansion is well suited to address this question for two reasons. First, it applied only to individuals born after September 30, 1983. This cutoff creates a jump in youth Medicaid eligibility with respect to date of birth, which we leverage within a regression discontinuity framework to estimate the impact of OBRA90 on incarceration.[1] Second, while Medicaid *eligibility* increased for children of all races as a result of the Expansion, Medicaid *coverage* (i.e., enrollment) increased only for Black children (Wherry and Meyer, 2016; Wherry et al., 2018). In our setting, Black children in the relevant age range experienced a large coverage increase—7.1 or 10.5 percentage points, depending on specification—while children of other races showed no signs of a coverage increase.[2] Because only Black children gained Medicaid coverage in response to OBRA90, we focus on the outcomes of Black individuals and designate the outcomes of other races as useful comparisons. Focusing on Black people is important because incarceration disproportionately affects Black communities. Despite comprising 13 percent of the total population, Black Americans comprise 40 percent of the prison population, the largest share of any race (Sawyer and Wagner,

---

[1]   This variation has been utilized previously by Card and Shore-Sheppard (2004), Wherry and Meyer (2016), and Wherry et al. (2018) to estimate the short and long-run health impacts of childhood insurance.

[2]   The increase in coverage is calculated using National Health Interview Survey respondents from the Southern Census region, the closest available proxy for our setting of Florida. The difference in coverage rates exists because Black children were more likely to be in the treated income range and more likely to take up. The fact that Black children exhibit higher take-up—which we define as coverage gain divided by eligibility gain—is true of many Medicaid expansions, not just OBRA90 (Sommers et al., 2012).

2020).

The setting in which we explore incarceration is the state of Florida, the third largest state in the US in terms of both total population and prison population. The publicly available prison records from the Florida Department of Corrections are ideally suited to examine the long-run effects of the Expansion, as they include the key variables needed for our analysis: race, prison admission dates, prison release dates, and exact dates of birth. Furthermore, because data collection began in 1997, we observe the entire adult lives of birth cohorts born near the eligibility cutoff.

In a series of regression discontinuity figures, we show that Black cohorts born just after September 30, 1983 have 5.1 percent fewer individuals incarcerated by age 28 compared to those born just before the cutoff. We detect no change in incarceration for other races, consistent with the fact that the policy did not significantly affect their enrollment in Medicaid. These results imply that the Expansion also reduced the incarceration gap in Florida. Among those born just prior to the cutoff, about 10.8 percent of Black individuals and 2.8 percent of individuals of other races had been incarcerated as of age 28, a difference of 8.0 percentage points. Among those born just after the cutoff, we estimate that the difference falls to 7.5 percentage points, a 6.3 percent decrease in the incarceration gap.

The effects that we find are in response to a large treatment: an estimated 24 percent of Black children born just after the cutoff gained an average of 6.10 years of Medicaid eligibility. This means that, across all Black children born just after the cutoff, OBRA90 increased youth Medicaid eligibility by an average of 1.46 years per child (6.10 years × 24 percent). Therefore, a simple interpretation of our treatment effect is that an additional *population-level* eligibility-year of Medicaid among Black children leads to 3.5 percent fewer of those children going to prison as adults (5.1 percent / 1.46 years).

Our estimates are stable across a variety of robustness tests, including alternative bandwidths and specifications. We find no evidence that children on either side of the cutoff differ systematically in observable characteristics other than in youth Medicaid eligibility. We also show that the treatment effect is strongest in areas of Florida where eligibility gains are likely the largest (i.e., in zip codes with a high concentration of households living below the poverty line). We show finally that our results are not unique to Florida. When we replicate our analysis using a secondary data source that collects prison records from multiple states, we obtain similar results.

To the extent possible with the available data, we also shed some light on the changes in behavior that lead to decreased imprisonment in adulthood. First, we show that the observed decline in incarceration is driven primarily by a decrease in incarcerations connected to financially motivated crimes. While drug trafficking, selling, manufacturing, and distribution fall sharply at the cutoff, drug possession is unchanged. Although we see an effect on violent crime, the impact is driven solely by robbery. These reductions are consistent with a large literature showing that financially motivated criminal activity is sensitive to changes in economic conditions (e.g., Carr and Packham, 2019; Foley, 2011; Tuttle, 2019; Wright et al., 2017). Second, we show that the OBRA90 expansion dramatically increased the detection of attention deficit and hyperactivity disorder in children, a

disorder that has been connected to a host of problems, including increased rates of incarceration (Mohr-Jensen and Steinhausen, 2016).

We conclude our analysis by demonstrating the cost-effectiveness of the OBRA90 expansion. We calculate under conservative assumptions that each dollar spent on the provision of Medicaid returned 40 cents in savings on direct incarceration costs alone (i.e., expenses associated with confining an inmate). This estimate rises to 66 cents on the dollar if the economic losses (e.g., decreased earnings) caused by imprisonment are incorporated into the calculation (Mueller-Smith, 2015). These benefits do not take into account the underlying decrease in crimes that lead to incarceration, which would imply a substantially greater return. Overall, we find that the OBRA90 expansion is highly cost-effective, even without considering any direct socioeconomic and health benefits provided by youth Medicaid eligibility.

Our result that additional years of youth Medicaid eligibility reduces adult incarceration builds on three strands of economic research. First, we contribute to the literature investigating the impact of social safety net programs on incarceration. Much of this work has focused on program eligibility changes that affect the financial incentives of ex-offenders to recidivate (e.g., Agan and Makowsky, 2018; Tuttle, 2019; Yang, 2017a,b). We find a large impact of Medicaid on *first-time* incarcerations. The current research on the safety net and first-time incarcerations is sparse. Recent research shows that children with greater exposure to Food Stamps were significantly less likely to be convicted and imprisoned as adults (Barr and Smith, 2019; Bailey et al., 2020). Although not safety net policies by typical definitions, there is also evidence that certain educational opportunities in childhood can reduce incarceration (e.g., Barr and Gibbs, 2019; Deming, 2011; Heckman et al., 2010; Johnson and Jackson, 2019). To the best of our knowledge, this is the first paper to causally explore the relationship between childhood health insurance and adulthood incarceration.

Second, we build on the literature connecting health insurance to criminal activity (see Doleac (2018) for a review). To date, this research has focused on contemporaneous impacts. Multiple papers have shown, for example, that Medicaid expansions reduce crime rates soon after passage (e.g., He and Barkowski, 2020; Vogler, 2020; Wen et al., 2017). There is also evidence that the Affordable Care Act's dependent coverage mandate resulted in fewer arrests within a few quarters (Fone et al., 2020) and that the Affordable Care Act Medicaid expansions lowered recidivism within two years of release among repeat offenders (Aslim et al., 2019). In concurrent work, Jácome (2020) finds that individuals who automatically disenroll from CHIP at age 19 are more likely to be incarcerated by age 21 than a matched control group of low-income individuals. We extend this literature by illustrating a long-run, rather than short-run, relationship between health insurance and criminal activity. While these short-run papers find effects on violent crime, our paper finds effects exclusively on financially motivated offenses, of which only robbery is violent. This difference suggests that the mechanisms driving contemporaneous changes in criminal behavior are distinct from the mechanisms underlying the long-term impact that we estimate.

Third and finally, this paper adds to the large and growing literature documenting the long-

term impacts of access to Medicaid in childhood (e.g., Brown et al., 2019; East et al., 2017; Goodman-Bacon, 2016; Miller and Wherry, 2019; Wherry et al., 2018). Specifically, our finding—that Medicaid reduces long-run incarceration—builds on previous research demonstrating the impact of childhood health insurance on health, earnings, and use of government assistance. Furthermore, our result underscores the fact that "critical periods" of exposure depend on the treatment and outcome. In particular, while much of the early-life literature focuses on treatments occurring at or below age 5, we find a considerable effect for a treatment that targeted individuals in later childhood and early adolescence. This effect is large enough and incarceration is costly enough to imply that current cost-benefit analyses of Medicaid, which already show high returns (e.g., Hendren and Sprung-Keyser, 2020), should be even more favorable.

2   Background

Prior to the 1980s, Medicaid eligibility was linked to the receipt of cash welfare payments under the Aid to Families with Dependent Children ("AFDC") program. This linkage generally limited Medicaid eligibility to single-parent households with incomes well below the Federal Poverty Level ("FPL"). The income cutoff for AFDC eligibility varied greatly across states but averaged around 60 percent of the FPL (Cutler and Gruber, 1996). As noted by Shore-Sheppard (2000), insurance coverage in 1980 therefore followed a U-shaped pattern across the income distribution. Specifically, children from the lowest decile of income had higher rates of insurance coverage than children from the second and third-lowest deciles. In order to narrow this disparity, Congress enacted a series of laws during the 1980s and early 1990s that severed the link between Medicaid and AFDC receipt and increased health insurance coverage for children not eligible for AFDC. Subsequently, Medicaid eligibility rates nationwide for children age 18 or younger rose from 16.7% in 1988 to 28.6% in 1994 (Card and Shore-Sheppard, 2004).[3]

The two reforms driving most of this increase in youth Medicaid eligibility were the Omnibus Budget Reconciliation Act of 1989 ("OBRA89") and the Omnibus Budget Reconciliation Act of 1990 ("OBRA90"). OBRA89 required that states give Medicaid eligibility to most children under the age of 6 from families with household income less than 133% of the FPL. The policy that provides the identifying variation for this study, OBRA90, required states to extend eligibility through age 18 for children from families below 100% of the FPL. Congress, however, stipulated that this expansion (henceforth, "OBRA90" or "the Expansion") apply only to children born after September 30, 1983. As a result, children (in the relevant income range) born on October 1, 1983 experienced more years of Medicaid eligibility than those born on September 30, 1983. The Expansion became effective in 1991 and was superseded by the introduction of the Children's Health Insurance Program ("CHIP") in 1997, which granted eligibility through age 18 to children born on either side of the cutoff. Therefore, the discontinuity in eligibility existed for about 6 years. Summarily, the

---

[3]   Card and Shore-Sheppard (2004), Currie and Gruber (1996), Cutler and Gruber (1996), and Shore-Sheppard (2000) provide thorough descriptions of the various Medicaid expansions from the 1980s and 1990s.

Table 1 – Impact of the OBRA90 Expansion on Medicaid Eligibility

	Percent gaining eligibility		Average years gained for all children		Average years gained for newly eligible children	
	Black	Non-Black	Black	Non-Black	Black	Non-Black
National	17.3%	8.7%	0.869	0.405	4.911	4.408
Southern region	22.2%	11.9%	1.233	0.660	5.443	5.399
Florida	23.6%	11.2%	1.463	0.643	6.098	5.595

Notes: The purpose of this table is to display gains in eligibility stemming from the OBRA90 expansion. The columns detail the percentage of children gaining eligibility from the Expansion, the population-average gains in eligibility (across all children), and the average increase in eligibility among children who gained some eligibility, respectively. Each set of columns is split by race (Black or Non-Black). Each row represents a different set of states used to calculate the eligibility gains in the table.

Source: Author calculations using the Wherry et al. (2019) replication file and 1981-88 Annual Social and Economic Supplements of the CPS.

OBRA90 expansion effectively increased Medicaid eligibility from ages 8 to 14 for post-cutoff birth cohorts with household incomes between AFDC and FPL thresholds.[4]

In order to understand the effect of Medicaid access on incarceration, it is important to quantify the impact of the OBRA90 expansion on Medicaid eligibility and Medicaid coverage. For this "first-stage" analysis, we follow the approach of Wherry et al. (2018). To determine eligibility gains, they apply the eligibility rules described above to cohorts in the Current Population Survey ("CPS").[5] We replicate their procedure to populate Table 1.[6] Because our main analysis of incarceration uses data from Florida (as discussed in the next section) we show eligibility gains for the Southern Census Region, for Florida, as well as nationwide. In addition to splitting by geography, we show figures separately for Black and "Non-Black" outcomes where our Non-Black categorization includes all other races, consistent with Wherry et al. (2018).[7]

A striking feature of the OBRA90 Medicaid expansion is the large relative increase in eligibility for Black children: 17.3% of Black children gained eligibility versus only 8.7% of Non-Black children

---

[4] Because Medicaid and other welfare programs underwent significant changes in the 1990s (Blank, 2006), our regression discontinuity design is arguably preferable to methods that exploit state-by-year variation (Wherry et al., 2018; Lee and Lemieux, 2010). In Section 4, we discuss this identification strategy and provide evidence that no other policies coincided with this treatment.

[5] Specifically, they first pool all children (ages 0 to 17) in the Annual Social and Economic Supplement of the CPS from 1981 to 1988, which has detailed demographic information but not children's date or month of birth. The authors then simulate age-specific eligibility once as if each child had been born in September of 1983 and once as if each child had been born in October of 1983. The determination of eligibility at each age involves the child's family structure, household income, and parental employment (which are assumed fixed) per state and federal rules. A simple summation gives the child's total years of eligibility (once for September and once for October). Children for whom October-1983 eligibility-years exceed September-1983 eligibility-years are counted as gaining eligibility as a result of the Expansion.

[6] We thank Wherry et al. (2019) for making their data and code available for public use.

[7] We will carry this categorization forward in all analyses. The inclusion (or removal) of Hispanics from the Non-Black category does not qualitatively change the results in this paper. We choose to group Hispanics into the Non-Black category because in Florida Hispanics and Whites have similar household incomes and incarceration rates (Sawyer and Wagner, 2020).

nationwide. This disparity is due to lower family incomes in Black households, not because of any race-specific provisions of the program. Similarly, average years of eligibility gained by Black children nationwide are also more than twice those for Non-Black children. Because Medicaid eligibility prior to OBRA90 was less generous in Florida, the eligibility gains there were much larger: nearly 24% of all Black children gained eligibility, with an increase of roughly 6.10 eligibility-years among the newly eligible. This translates into an average gain of 1.46 eligibility-years across *all* Black children born just after the cutoff (6.10 years × 24 percent) or nearly 71,000 total eligibility-years for Black children born in the year after the September 30, 1983 cutoff (48,450 individuals according to the 2010 Census × 1.46 years of average eligibility).

To determine coverage gains, Wherry et al. (2018) apply a regression discontinuity design to data from the National Health Interview Survey ("NHIS"), which collects year-month of birth and health insurance variables (but lacks the state identifiers and granular income measures needed to determine eligibility). We run a similar analysis in Section 5.1, but we briefly preview the qualitative result here. As with eligibility gains, there are large differences in coverage gains between Black and Non-Black children. While Black children experienced substantial gains in coverage (7.1 or 10.5 percentage points, depending on specification), there is no evidence that insurance coverage for Non-Black children increased due to the OBRA90 expansion.[8] We will leverage the fact that the policy-induced increase in Medicaid enrollment was driven almost entirely by Black families. Specifically, our analysis focuses on the impact of the Expansion on Black children, while Non-Black children are used as a comparison group (i.e., we expect little or no change in Non-Black incarceration).

3 Data

The setting for our analysis is Florida, which in 2020 comprised 6.5 percent of the US population and 7.9 percent of the US prison population (Sawyer and Wagner, 2020). The source of incarceration data is the Florida Department of Corrections ("Florida DOC"), which makes its Offender Based Information System ("OBIS") available to the public.[9] These data contain active and released offenders who were convicted of a crime in a state court and sentenced to a stay in state prison.[10] The data include all stays associated with a particular offender. They also include all the offenses committed, the county in which the offense was committed, and the sentence tied to each

---

[8] Critically, Wherry et al. (2018)—and earlier papers, such as Card and Shore-Sheppard (2004)—find no evidence of significant crowd-out from private health insurance. We also find that the Expansion did not crowd-out private health insurance coverage (see Appendix Figure A5).

[9] Using administrative data to measure incarceration outcomes is vital. According to Pettit (2012), nearly all widely utilized surveys, including the American Community Survey, fail to accurately count institutionalized persons.

[10] We do not have information on Federal prisoners in Florida. This omission is not a serious concern because the ratio of state to Federal prisoners nationwide is almost 6-to-1 (Sawyer and Wagner, 2020). Additionally, we do not observe people passing in or out of local jails. Most of these individuals, however, have not been convicted (i.e., will either post bail or are awaiting trial). Those who have been convicted are, in the vast majority of instances, serving significantly less than a year for a misdemeanor.

offense.[11] Offender demographics include race, exact date of birth, and, for stays linked to parole or probation, the zip code to which offenders were released. Collectively, we know how many individuals have been incarcerated in a certain time frame or age range. We also know how many years each offender has served—or has been sentenced to serve—in a given window.[12]

The coverage of the OBIS data aligns well with the timing of the treatment. For our final sample, we restrict the data to offenders and ex-offenders born within three years of the cutoff for the Expansion (i.e., born between October 1, 1980 and September 30, 1986). The OBIS records begin with incarceration spells that started or finished on or after October 1, 1997. Therefore, the oldest members of our final sample are 17 years old when data first become available. Offenders will typically not be incarcerated in a state prison until age 18.[13] Our main outcomes will be measured at age 28 (inclusive of age 28). According to the most recent report produced by the Bureau of Justice of Statistics, almost 70 percent of people who will ever go to prison will go to prison by age 28 (Bonczar, 2003). Furthermore, this age matches some key papers on early-life exposures and long-term outcomes, such as Brown et al. (2019). Most importantly, we show that our results are not sensitive to this choice of age.

For our primary analysis, we bin the incarceration records by date of birth, such that each observation in the final dataset is a date of birth. A column in the final dataset is, for example, the total number of Black individuals born on a particular day who went to prison by age 28. A point to emphasize is that our primary analysis examines incarcerated and formerly incarcerated individuals. That is, we do not observe individuals who never go to prison.

We present summary statistics of the incarceration data in Table 2. The summary statistics, like the main results to follow, are split by race, Black and Non-Black. The "Pre" column represents individuals born on or before September 30, 1983; the "Post" column, after this date. The "% Δ" column is the percentage change from Pre cohorts to Post cohorts. These percentage changes reveal a naive treatment effect. In the first row of Table 2, we see that the number of Black adults incarcerated in Florida by age 28 decreases for Post cohorts (Post cohorts received additional years of youth Medicaid eligibility). The number of Non-Black adults incarcerated by age 28, on the other hand, increases slightly (consistent with their lack of coverage gains at the cutoff). As shown in the second row, this pattern is not driven trivially by differences in the sizes of Pre and Post cohorts. In the final three rows, we see the same result for years served, years sentenced, and offenses committed: decreases for Black adults and increases for Non-Black adults. We examine these data formally in a regression discontinuity design that is discussed in the next section.

---

[11] Throughout this paper, the term "offenses committed" refers to offenses that lead to a prison sentence. We do not observe offenses that do not result in such convictions.

[12] To handle life sentences and other judgments that exceed any reasonable life expectancy, we adjust sentences as follows:

$$Sentence_i^{adj} = \min\{Sentence_i, LifeExpectancy_i\},$$

where $Sentence_i$ is the original assigned sentence (in years) and $LifeExpectancy_i$ is an inmates' expected longevity based on their race, sex, and age at the time of sentencing.

[13] Offenders under age 18 usually go to juvenile facilities, for which we do not have data.

Table 2 – Summary Statistics

	Black			Non-Black		
	Pre	Post	% Δ	Pre	Post	% Δ
People incarcerated by age 28	15,271	14,305	-0.06	15,670	16,202	0.03
Cohort population (in Florida)	140,880	149,030	0.06	564,240	568,360	0.01
Years served by age 28	46,028	43,023	-0.07	36,158	37,307	0.03
Years sentenced by age 28	269,264	256,178	-0.05	211,209	222,505	0.05
Offenses committed by age 28	60,759	55,052	-0.09	56,337	58,246	0.03

Notes: The purpose of this table is to display summary statistics of the Florida DOC Incarceration data for the birth cohorts we examine. Tabulations in the "Pre" columns are derived using birth cohorts from October 1, 1980 through September 30, 1983, while tabulations in the "Post" columns are calculated using birth cohorts from October 1, 1983 through September 30, 1986. The percentage difference between the "Pre" and "Post" columns are given in the % Δ column.

Source: Author calculations using Florida DOC Incarceration Data and 2010 Census 10% Sample (Ruggles et al., 2020).

4   Methodology

To evaluate the long-run effects of youth Medicaid eligibility, we leverage the discontinuous increase in Medicaid coverage induced by the OBRA90 expansion within a regression discontinuity design. Specifically, we estimate the following equation:

$$Y_c = \alpha + \delta \cdot Post_c + f(DOB_c) + \lambda_{m(c)} + \varepsilon_c \, , \tag{1}$$

where c indexes a date-of-birth cohort. In our primary analysis, the dependent variable, Y_c, is the log count of inmates for a given date of birth.[14] We utilize log counts rather than rates, because the data necessary to construct the adult population measure in Florida for each DOB-cohort is not available.[15] Treatment is defined by the binary variable $Post$, which is equal to one if a given cohort c was born after the September 30, 1983 cutoff and zero otherwise. The associated coefficient, δ, is the parameter of interest. When Y_c is constructed as log inmates, δ represents the percent change in the number of incarcerees per day as a result of increased childhood Medicaid access. Because Medicaid coverage and eligibility are determined by many factors—and birth date is only one such factor—Equation 1 is a "fuzzy" regression discontinuity. If we had data on inmates' childhood coverage (eligibility), we could use the discontinuity as an instrumental variable

---

[14] All date-of-birth cells have non-zero incarceration counts. We also consider level counts as an outcome and achieve qualitatively similar results.

[15] In Appendix A, we construct a proxy denominator by combining Census and natality data, and we show that the results to follow are robust to using the corresponding rates (rather than using log counts or counts). Because creating these rates utilizes an approximation of the "true" denominator—thereby introducing measurement error—and involves a degree of researcher discretion, we also show for transparency that the adult population in Florida is smooth through the cutoff using (only) the 2010 Census 10% count file, which includes quarter of birth (Ruggles et al., 2020). The continuity in population allows us to interpret a discontinuity in log inmate counts as a change in the likelihood of incarceration. This result is shown in Appendix Figure A2 and comes from estimating a modified version of Equation 1 when Y_c is the number of people born in quarter c.

for coverage (eligibility) and estimate the local average treatment effect of public insurance on incarceration. However, because we do not have data on childhood circumstances, we instead estimate a reduced-form equation, such that δ represents the effect of an average increase of 1.46 years of youth Medicaid eligibility among the Black population (Table 1). In Section 5, we scale this reduced-form estimate using separate estimates of eligibility and coverage.

The function $f(\cdot)$ represents a polynomial in normalized day of birth (DOB_c - Cutoff) that is fit separately for each side of the cutoff (i.e., separately for treated and untreated cohorts). In order to control for seasonality in birth outcomes (Buckles and Hungerman, 2013), a calendar-month-of-birth fixed effect, $\lambda_{m(c)}$, is also included. In our preferred specification, we estimate this equation using a linear polynomial, rectangular kernel, and a bandwidth of three years on either side of the cutoff. Our bandwidth choice is nearly identical to the data-driven bandwidth chosen by the Calonico et al. (2014) bandwidth selection procedure (which chooses a bandwidth of 2.98 years for our main analysis). As further discussed in Section 5, we find that our estimates are robust to varying the bandwidth and specification choices discussed here.

As stated in Lee and Lemieux (2010), the key assumption of regression discontinuity designs is that unobservable factors are continuous with respect to the cutoff. While this is inherently untestable, we consider whether characteristics in childhood (but before the age of increased Medicaid eligibility) change discontinuously around the September 30, 1983 eligibility threshold. Specifically, we estimate Equation 1 using the fraction of children in poverty and in single-parent households as outcomes, both of which are strongly associated with later-life incarceration (Chetty et al., 2018). The results of these analyses are presented in Appendix Figure A1. We find no statistical or visual evidence of a discontinuity in either outcome.[16] Additionally, we note that the timing of the policy precludes an endogenous fertility response: the Expansion was enacted in 1991, when children born just after the cutoff date were nearly 8 years old. Another potential issue is that other policies unrelated to the OBRA90 expansion also applied differentially to cohorts born after the cutoff. Although the prior literature examining the Expansion (e.g., Wherry and Meyer, 2016; Wherry et al., 2018) have not identified any such policies, we conducted a LexisNexis search of legislation, news sources, law journals, and legal cases for indications of confounding policies. Our search returned nothing that would suggest other policies differentially affected the birth cohorts we study in this paper. Lastly, we note the September 30 cutoff was likely chosen because it coincides with the end of the government fiscal year. We show in Subsection 5.3 that other September-October cutoffs (excluding 1983) demonstrate no discontinuities in incarceration.

---

[16] We also consider whether enrollment in Medicaid caused children and families to contemporaneously increase take-up of other social services, such as food stamps or cash assistance. This could occur, for example, if enrollment in Medicaid reduced frictions that previously prevented enrollment in other social safety net programs. However, as displayed in Appendix Figure A3, we do not find any meaningful increase in use of non-Medicaid programs.

Figure 1 – First Stage: Impact of the OBRA90 Expansion on Medicaid Coverage (NHIS)
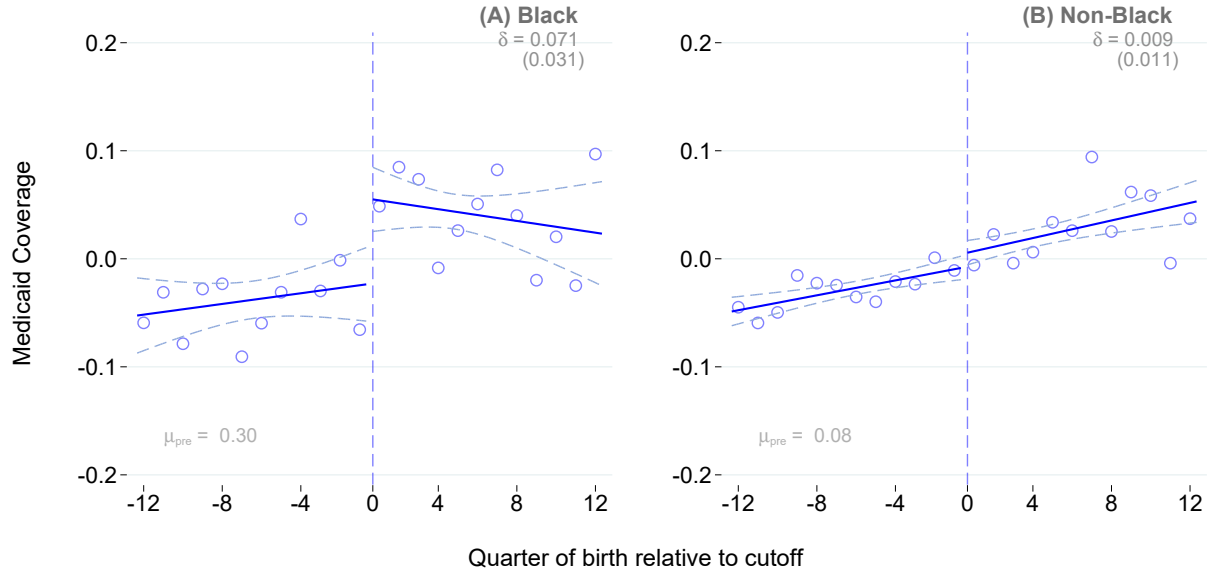
Notes: The purpose of this figure is to display the increases in Medicaid coverage as a result of the OBRA90 expansion. Each dot represents the average of the outcome variable in 3-month bins, after partialling-out calendar month effects. The lines presented are generated from linear regressions with associated 90 percent confidence intervals (displayed using dashes). The estimated coefficients, δ, and associated standard errors generated from Equation 1 are presented in the upper right of each panel, while the pre-cutoff means of coverage are presented bottom left. Standard errors are clustered on the year-month of birth. Figures utilize 3,208 and 9,852 observations for Black and Non-Black children, respectively, from the Southern Census Region of the United States.

Source: Author calculations using the 1992-96 National Health Interview Surveys.

5 Results

5.1 First Stage Results

We begin by estimating the impact of the OBRA90 expansion on childhood Medicaid coverage. To do so, we apply the regression discontinuity design from Equation 1 to the 1992-96 NHIS, which contain information on Medicaid coverage as well as month and year of birth.[17] To most closely match our setting of Florida, we restrict our sample to respondents in the Southern Census Region.[18]

The estimates from this analysis are displayed graphically in Figure 1, which shows the discontinuity in Medicaid coverage induced by the OBRA90 expansion. To produce the figure, we adjust the outcome variable by residualizing out calendar month effects, $\lambda_{m(c)}$, in order to account for seasonal variation in birth timing. Additionally, values of Y_c are de-meaned across the entire

---

[17] The period 1992-96 is chosen because 1992 is the year after the policy went into effect and 1996 is the year before CHIP expanded Medicaid to all income-eligible children under the age of 18, regardless of date of birth.

[18] The NHIS does not include identifiers for individual states, so we use the Southern Region as a proxy for Florida. Although the restricted-use version of the NHIS contains state-level identifiers, it is unlikely that a Florida-specific analysis would have sufficient power to provide a statistically meaningful result, as there are likely only 300 to 400 Black respondents in Florida within our bandwidth in the entire pooled sample.

bandwidth. Each point represents the mean of the adjusted Y_c values by birth quarter relative to September 30, 1983.[19] Each regression line is associated with its 90% confidence interval. Estimated δ coefficients from Equation 1, along with standard errors clustered at the level of the running variable, are displayed in the upper right-hand corner. Pre-cutoff means of the unadjusted outcome variable are displayed in the bottom left.

We find that, consistent with the greater eligibility gains discussed in Section 2, coverage increased by 7.1 percentage points ("p.p.") for Black children in the Southern Region. We are unable to detect any change in coverage for Non-Black children. When we use a quadratic fit (rather than a linear fit) on either side of the cutoff, we estimate a 10.5 p.p. increase for Black children, while still finding no coverage increase for Non-Black children (Appendix Figures A4C and A4D).[20] When we perform a similar analysis at the national level (Appendix Figures A4E through A4H), we find that the OBRA90 expansion increased coverage by 4.7 p.p. (linear fit) or 7.0 p.p. (quadratic fit), which is consistent with findings from Wherry et al. (2018).

These coverage increases imply that roughly 32.0% of eligible Black children in Southern States enrolled in Medicaid (7.1 p.p. coverage gain / 22.2 p.p. eligibility gain). This "take-up rate" is 47.3% when using our quadratic specification. Further, these rates are similar to the nationwide take-up rate of 27.2% (4.7 p.p. coverage gain / 17.3 p.p. eligibility gain) or 40.5% when using quadratic fits. Because the estimated coverage gain for Non-Black children is small, their take-up rate is also low. (N.B.: Low take-up among other races is a pervasive feature of Medicaid expansions (Sommers et al., 2012; Wherry et al., 2018). Currie (2004) describes some of the key factors that determine take-up of social programs broadly, but why take-up rates vary across race within program remains an open question.) As discussed in Section 2, we incorporate the racial difference in coverage increases into our analysis. We focus on the effects of the OBRA90 expansion on Black people, while using Non-Black people as a comparison group for which we expect to see no economically significant changes.
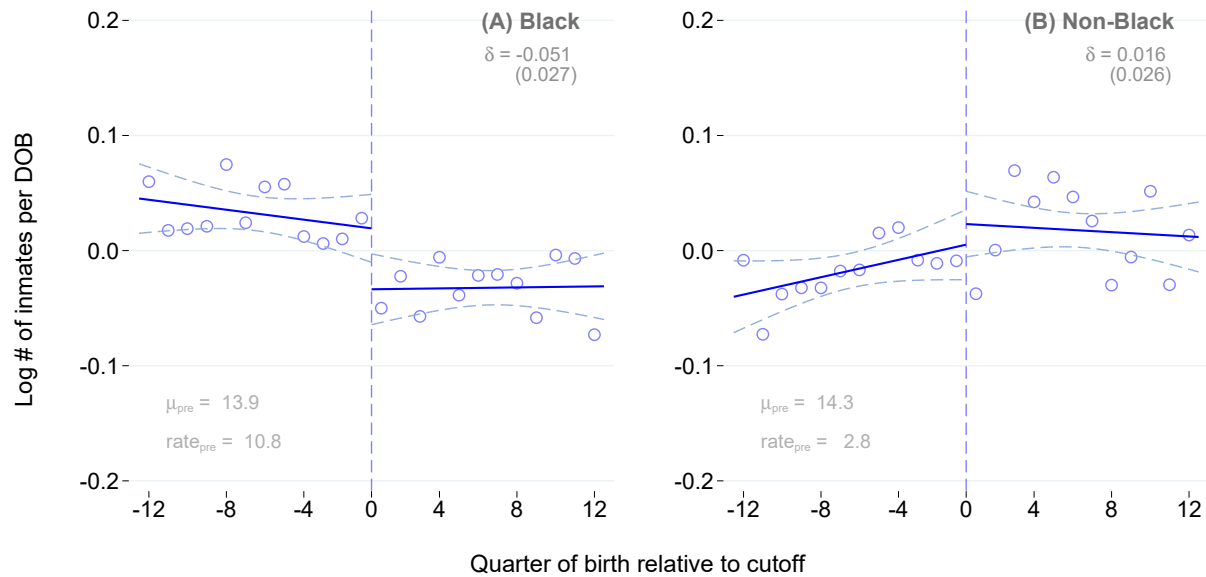
5.2 Main Results

The main result of this paper is shown in Figure 2. We find a clear discontinuity in the number of incarcerations for Black adults born after September 30, 1983, a decrease of approximately 5.1% (90% CI: -9.6% to -0.7%).[21] This decline translates into 259 fewer incarcerations of Black individuals

---

[19] All of the regression discontinuity results in this paper are given in figures that follow the same structure. Quarterly averages are presented in graphs for aesthetic purposes, but the underlying fitted lines are estimated using daily cohort data.

[20] We use a linear fit on either side as it minimizes the Akaike information criterion ("AIC"), a method for polynomial choice suggested by Lee and Lemieux (2010). The AIC is minimized when using linear polynomials for all of the main results presented in this paper. Nonetheless, we include quadratic fits in our first-stage discussion because the difference between the linear and quadratic point estimates is economically meaningful.

[21] Recall that our main outcome is the logged number of individuals ever incarcerated by age 28 (inclusive of age 28). In Figure 2, as in Figure 1, the estimated δ coefficients from Equation 1, along with standard errors clustered at the date-of-birth level, are displayed in the upper right-hand corner. Pre-cutoff means of the outcome variable (*in level counts*) are displayed in the bottom left. For instance, the level count of 13.9 displayed in Figure 2A indicates that there are an average of 13.9 Black Floridians incarcerated per date-of-birth cohort prior to the cutoff. For analyses that display the effects on the Expansion on our main outcome (individuals ever incarcerated), we also display the

Figure 2 – Main Results: Impact of the OBRA90 Expansion on Adult Incarceration

Notes: The purpose of this figure is to display the main results of our analysis. Each dot represents the average of the outcome variable in 3-month bins, after partialling-out calendar month effects. The lines presented are generated from linear regressions with associated 90 percent confidence intervals (displayed using dashes). The estimated coefficients, δ, and associated standard errors generated from Equation 1 are presented in the upper right of each panel, while the pre-cutoff means of the *level* count of incarcerations (μ_{pre}) and pre-cutoff incarceration rates per 100 individuals ($rate_{pre}$) are presented bottom left. Standard errors are clustered on the date of birth.

Source: Author calculations using Florida DOC Incarceration Data.

born in the year after the cutoff (13.9 incarcerated individuals per exact-date-of-birth cohort $\times$ 365 days $\times$ -5.1%). Importantly, this effect seems to manifest by age 22 and stabilize by age 27, as shown in Appendix Figures A9 and A10. Non-Black adults show no evidence of a discontinuity, as expected. Accordingly, for the remainder of this paper, we focus our attention on results for Black individuals, while continuing to show (null) Non-Black results in Appendix Figures.

We frame our main result first with respect to increases in eligibility and coverage and then in terms of population-level incarceration rates. As shown in Table 1, OBRA90 increased Medicaid eligibility across all Black children in Florida on average by 1.46 years. Therefore, our estimate of -5.1% can be interpreted as a 3.5% reduction in incarceration per year of *population-level* eligibility (5.1% / 1.46).[22] This reduction is equivalent to a decline of 3.6 incarcerated individuals per 1,000

---

incarceration rates (per 100 individuals) prior to the cutoff.

[22] To scale our reduced-form estimates based on increases in coverage (rather than eligibility), we divide them by the calculated take-up rate of Medicaid—32.0% or 47.3% depending on the usage of a linear fit or quadratic fit (respectively) in the estimation of coverage—discussed in Section 5.1. This division gives a 10.9% (or 7.4%) reduction in incarcerations per additional year of population-level coverage (3.5% divided by 32.0% or 47.3%). Estimates scaled by coverage, however, should be treated with caution for two primary reasons. First, as noted in Section 5.1, our take-up estimates are based on the Southern Census region and thus may not be representative of Florida. Second, as noted by the U.S. Census Bureau (2008), the NHIS substantially underestimates increases in Medicaid coverage: among NHIS respondents who were included in administrative Medicaid Statistical Information System records, 34.6% incorrectly responded that they were not covered by Medicaid (false-negatives). This is opposed to a false-

additional eligibility-years (259 fewer incarcerees / 71,000 eligibility-years from Section 2). For coverage, scaling by the take-up rates in Section 5.1 implies 11.4 (or 7.7 if quadratic fits are used) fewer incarcerated individuals per 1,000 additional years of coverage (3.6 divided by 32.0% or 47.3%, respectively). Finally, to express our estimated effect as a change in the population-level incarceration rate, we multiply the pre-cutoff rate of 10.8 per 100 (from Figure 2A) by our estimate of -5.1%, yielding a decrease in the Black incarceration rate by 0.55 per 100 individuals.[23] As before, we scale down this treatment effect by the total eligibility impact of the OBRA90 expansion and find that an additional year of eligibility results in 0.38 fewer incarcerated individuals per 100 Black people (0.55 / 1.46 years).

Our main result is consistent with recent research that finds policies targeting economically disadvantaged youth lead to large reductions in later-life imprisonment (discussed further in Section 5.5). When considering the magnitude of our result, it is important to note that incarceration is heavily concentrated in the low-income Black population, which is also the population affected by the OBRA90 expansion. Using data made available by Chetty et al. (2018), we calculate that Black children from the households in the lowest income quintile make up over half of the Black prison population in 2010.[24] Hence, policies that reduce the incarceration risk of these low-income individuals could result in large changes in total incarcerations. Furthermore, Black children from the lowest-quintile households comprise 25% of *all* inmates, despite comprising roughly 5% of the population.

Given the large racial disparities in incarceration and the asymmetric nature of our treatment (i.e., that only Black people were meaningfully affected), a natural interpretation of our results is in terms of the incarceration gap. Among Black Floridians born in the year before the Expansion, roughly 108 per 1,000 had been incarcerated as of age 28, while the rate for Non-Black Floridians was 28 per 1,000, a difference of 8.0 percentage points in the cohort-level incarceration rate. If these cohorts had instead been born one year after the cutoff date, we estimate that the incarceration gap would have fallen to 7.5 percentage points, a decrease of 6.3 percent.

In addition to our ever-incarcerated outcome, we consider the effect of increased Medicaid eligibility on several other incarceration measures. First, Panels A and B of Appendix Figure A6 show that our results are similar when using level counts instead of log counts as the outcome. Panels C through F illustrate the treatment effect when the outcome is the number (or log number) of offenses committed by a given daily cohort. The patterns using offense-level outcomes mirror our main analysis, though the effects are slightly larger (a 7.1% reduction in total offenses committed). We also analyze measures of time served in prison and time sentenced to prison. These outcomes, which show declines of a similar magnitude to our main result, are discussed further in Section 7.

---

positive rate of 1.1% to 1.6%. Accordingly, it is possible that use of take-up rates generated from the NHIS leads to overstatement of estimates that are scaled by coverage. For these reasons, we frame most of our results in terms of effects from increased eligibility, which can be measured more reliably.

[23] These results are very similar to those described in Appendix A and Appendix Figure A8. The analyses displayed in that figure estimate Equation 1 using approximate incarceration rates as an outcome and find reductions of 0.42 or 0.73 fewer incarcerated individuals per 100 Black adults, depending on specification.

[24] In 1991, 22.4 percent of children under age 15 were below 100% of FPL.

Figure 3 – Robustness: Treatment Effects by Bandwidth Choice (Black)
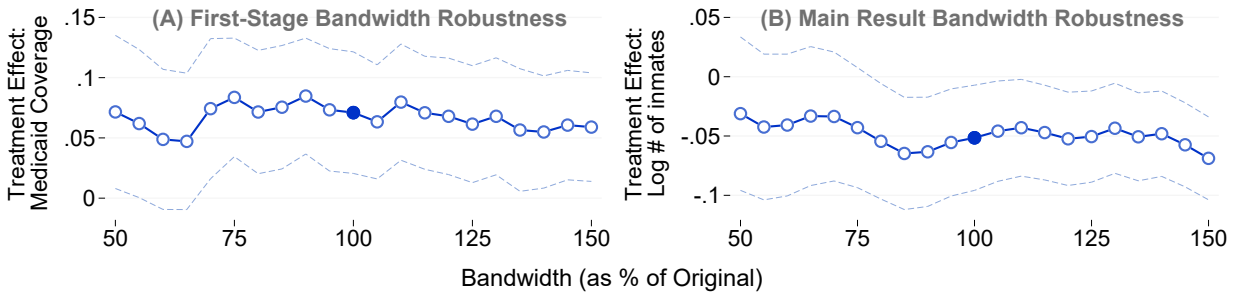
Notes: The purpose of this figure is to display how the results of our first-stage (Panel A) and main results (Panel B) vary by choice of bandwidth. Within the figure, each dot represents the estimated coefficient δ from a separate regression (our primary estimate is shaded dark blue). Dashed lines indicate 90 percent confidence intervals.

Source: Author calculations using National Health Interview Surveys and Florida DOC Incarceration Data.

We also evaluate the impact of the policy on several other measures—mean years sentenced, mean years incarcerated, the mean number of offenses committed, and the recidivism rate—all of which are conditional on incarceration. These estimates, which are displayed in separate rows of Appendix Figure A7, are difficult to interpret because they include both the effects due to the changing composition of offenders as well as the impacts due to intensive-margin responses of inmates (e.g., changes in the severity of crimes, conditional on committing a certain type of crime). We find no evidence of a discontinuity at the cutoff in any of these measures. We performed further analyses—available upon request—that include offender-type fixed effects (to account for compositional changes) which yield qualitatively similar measures. While such results should be interpreted with caution, they suggest that the OBRA90 Expansion did not affect the severity of crimes among individuals who were incarcerated regardless of childhood health coverage.

5.3 Robustness and Sensitivity

We conduct several robustness checks to ensure that our findings do not change qualitatively when we alter the estimation strategy. First, we consider how the specification choices discussed in Section 4 affect our estimates. Of these specifications, the most important is likely our choice of a three-year bandwidth. Accordingly, we re-estimate our first-stage and main results using separate regressions for a range of bandwidth selections, spanning 18 months (50% of the original choice) to 54 months (150%). The results of these regressions are displayed in Figure 3, where each point is a regression estimate and the dashed lines represent 90% confidence intervals (for reference, our primary estimate is shaded dark blue). The estimates for both our first-stage and main analyses are consistent over this range, though they do become less precise at narrower bandwidths, consistent with the bias-variance trade-off. Similar Non-Black bandwidth tests are included in Appendix Figures A11 and A12. The impact of other specification choices—such as functional form (i.e. Poisson regression vs. log counts), kernel weighting, and polynomial order—are collated graphically in

Figure 4 – Heterogeneity by Poverty of Release Zip Code (Black)
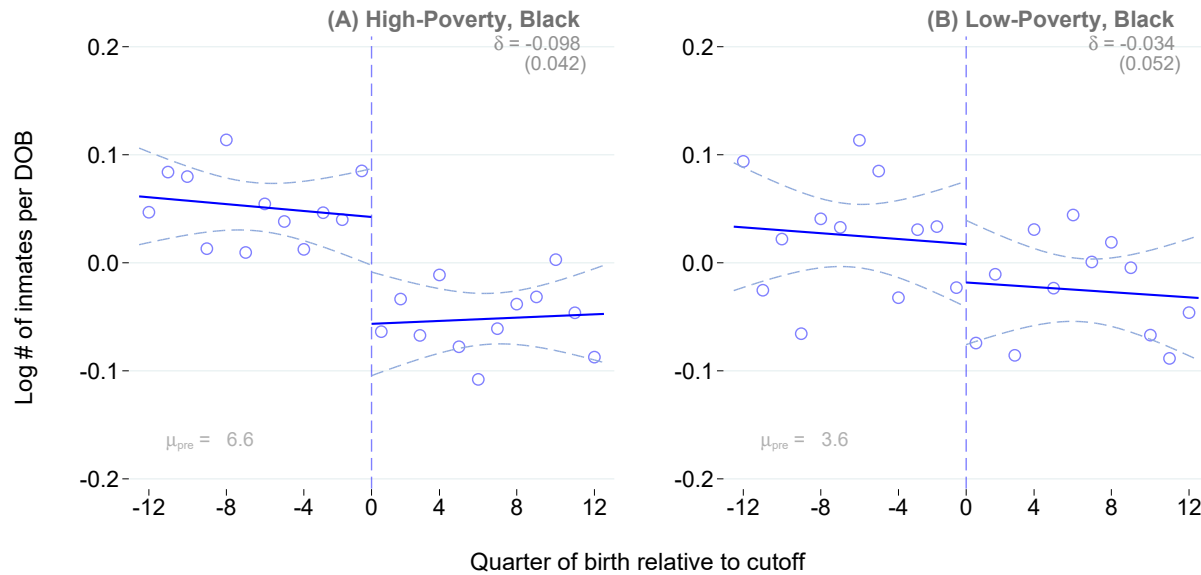
Notes: The purpose of this figure is to display the results of our heterogeneity analysis by poverty rates of the zip codes to which inmates were released. Each panel represents log counts of individuals in each daily birth cohort that have ever been incarcerated for a different sub-sample. Panels A and B focus on Black inmates who were released into relatively high and low-poverty zip codes, respectively. See Section 5 for additional detail on what constitutes high and low-poverty zip codes. Note that means displayed in the bottom-left corners of each panel do not sum up to those in Figure 2 because this analysis includes a sub-sample of offenders who have been released from prison.

Source: Author calculations using Florida DOC Incarceration and 2007-2011 American Community Survey Data (Manson et al., 2019).

Appendix Figure A14. The point estimates detailed in that figure closely match our main estimate.

Next, we run a simple check to ensure that groups that experienced the largest gains in Medicaid exhibit the strongest treatment effects. The Expansion targeted families between the AFDC threshold and the FPL. Therefore, it is useful to verify that the treatment effect is largest among those from high-poverty areas, in what amounts to a crude "dose-response" exercise. Because we do not have data on childhood circumstances, we proxy for childhood economic status using the zip code of an inmate's residence after release.[25] Because many inmates return to neighborhoods of family and close friends upon release (Simes, 2019), we consider this a reasonable proxy for childhood neighborhood. (N.B.: Neighborhood quality could be a function of our treatment; however, insofar as inmates return to childhood neighborhoods, concerns that we are conditioning on an outcome are alleviated.) We first divide previously released inmates into groups associated with high- and low-poverty zip codes. This high-versus-low classification is determined by dividing the entire prison population (regardless of race) into roughly equal halves based on the poverty rate of their associated zip code.[26] Those in the high-poverty group were associated with zip codes with

---

[25] These data are available only for inmates who have been released from prison. Consequently, this is a selected sample, but the within-sample differences remain informative. Reassuringly, we have a zip code for 71% of inmates.

[26] Ideally, we would isolate neighborhoods with a large fraction of households between the threshold for AFDC eligi-

an average poverty rate of 26.1% (roughly the 89th percentile of zip codes nationwide), whereas the low-poverty group's zip codes had a mean rate of 11.7% (approximately the 47th percentile). We then performed separate versions of our main analysis on each sub-sample, the results of which are displayed in Figure 4. Among Black individuals, the effect size is nearly 3 times as large in the higher-poverty zip codes (versus the lower-poverty zip codes). The Non-Black effects (displayed in Appendix Figure A18) are also substantially larger in high-poverty zip-codes, though they are imprecise.

We also address concerns that the observed effect reflects other policies that activate for October-1st birthdays, regardless of year. Although most potential confounding patterns would also manifest as specious decreases for Non-Black cohorts (which we do not see), certain patterns may be particular to Black individuals. To investigate this possibility, we run a separate analysis to determine the effect of the September 30th cutoff for all years in our bandwidth *except* 1983 (the treated year). Specifically, we pool all observations from 1980-82 and 1984-86 and estimate the following equation:

$$Y_c = \alpha + \delta \cdot Post_c + f(DOB_c) + \varepsilon_c$$

where the outcome Y_c is the same as our main specification (the log number of inmates per cohort ever incarcerated). However, in this analysis, c denotes a *day* of birth cohort (e.g., all children born on September 30th, regardless of year) and $Post_c$ is defined as being born in the 60 days following September 30th of a given year. (The bandwidth of 60 days was chosen as that is in-line with other calendar-date cutoff literature, such as the school entry date analysis performed in Cook and Kang, 2016.) The results of this exercise, displayed in Appendix Figure A13, show no evidence of a discontinuity related to non-treated September 30th cutoffs..

Finally, we consider the possibility that endogenous mobility may affect our estimates. As demonstrated in Appendix Figure A2, there does not appear to be a change in the sizes of cohorts born just after the cutoff, suggesting that meaningful (net) migration did not occur in response to the OBRA90 expansion. Nonetheless, it is still possible that migration-induced changes occurred that are undetected by these analyses. Accordingly, we present two additional pieces of evidence that our results are not affected by endogenous mobility. First, we note that, as part of their examination of the long-run health impacts of the OBRA90 expansion, Wherry et al. (2018) test for endogenous mobility using the Restricted NHIS (which has state of residence and state of birth) and do not detect differential migration. Second, we exclude border counties from our analysis—based on the assumption that they are most likely to be sensitive to migratory patterns—and find a result nearly identical to our main estimate.

5.4   External Validity

A potential limitation of our findings is that they were generated from a single state and thus may lack external validity. To understand whether the Expansion had effects in other states, we obtained

---

bility and the FPL, but the American Community Survey does not release sufficiently detailed income data by zip code (only a poverty indicator and median income).

Figure 5 – External Validity: Impact of the OBRA90 Expansion on Adult Incarceration (NCRP)
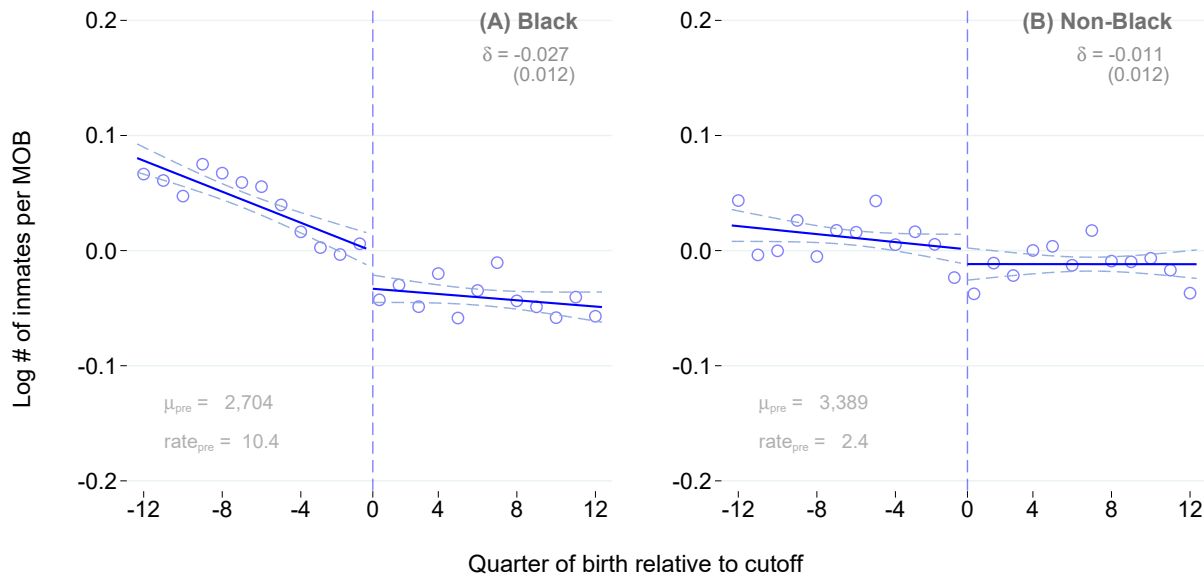
Notes: The purpose of this figure is to display the results of our main analysis using the National Corrections Reporting Program Data from 2000-2016. Panels A and B detail the log number of individuals ever incarcerated as of age 28. All figures are derived using year-month of birth ("MOB") cohorts, as opposed to the DOB cohorts used in Figure 2. The coefficients of interest, δ, are generated from a modified version of Equation 1, with the year-month of birth as the running variable. These coefficients and associated standard errors (clustered at the year-month level) are displayed in the upper-right corner. Pre-cutoff means of the *level* count of incarcerations (μ_{pre}) and pre-cutoff incarceration rates per 100 individuals ($rate_{pre}$) are in the presented bottom left. See more detail on the structure of the regression discontinuity plots in Figure 2.

Source: Author calculations using the 2000-2016 Restricted-Use National Corrections Reporting Program Data (Bureau of Justice Statistics, 2019).

restricted-access data from the National Corrections Reporting Program ("NCRP"). These data include voluntarily submitted incarceration records from multiple states for the years 2000 to 2016, and they include year and month of birth (although not exact date). In order to ensure that we observe continuous incarceration histories within the data, we created a sample from the states that submitted records consistently during the entire time period, leaving 19 states in total (Arizona, Colorado, Florida, Georgia, Illinois, Kentucky, Michigan, Minnesota, Missouri, Nebraska, New York, North Carolina, Oklahoma, Pennsylvania, South Carolina, Tennessee, Utah, Washington, and Wisconsin; see Appendix Section B for more details on the construction of the NCRP data). These states account for roughly half of the US population. Because the NCRP does not contain exact date of birth, we estimate a modified version of Equation 1, where the running variable is the year-month of birth and *Post* is defined as cohorts born on or after the month of October 1983.

The results of this analysis, displayed in Figure 5, exhibit a clear discontinuity at the cutoff for Black adults but not for Non-Black adults, consistent with our Florida-specific results. Among Black individuals, we find that incarceration at the cutoff decreases by approximately 2.7% (90% Standard CI: -4.7% to -0.7%). Below we discuss how the magnitude of this effect compares to our Florida-specific estimate, but first we discuss how our standard errors are affected by the structure

of the NCRP data. The NCRP only provides the month-year (rather than exact date) of birth and thus the running variable in our regression discontinuity design is coarse. Because clustering on a coarse running variable is generally insufficient for proper inference, we apply methods developed Kolesár and Rothe (2018), to obtain "honest" confidence intervals. The results of this procedure yield confidence intervals ranging from -6.2% to +0.6% (in the most precise case) to -16.1% to +9.5% (in the most imprecise case). The details of this method are further discussed in Appendix C.

Our estimate of 2.7% is half the size of our estimate for Florida, but we expect this estimate to be smaller because the eligibility increases in these states are roughly half the size of Florida's increase (a weighted-average of 0.60 years for NCRP states versus 1.46 for Florida). Indeed, if we scale the NCRP estimate up to represent an additional year of population-level eligibility, we arrive at an effect size of -4.5% per eligibility-year (-2.7% / 0.60 years), which is slightly larger than our finding for Florida (-3.5% per eligibility-year). We use the NCRP as a robustness check, rather than as a main result because the data contain only month of birth and because the data begin three years later and end three years earlier than the data from Florida, hampering our ability to observe full adult incarceration histories for all cohorts in our bandwidth and to explore age heterogeneity. Despite these limitations, we view the similarity between scaled estimates as evidence that the discontinuity in incarceration we see in Florida extends to other states.

5.5 Contextualizing Results

While this paper is the first to document the long-term incarceration effect of Medicaid access, it is not the first to find that policies and programs targeting economically disadvantaged youth lead to large reductions in later-life imprisonment. In order to provide additional context for our main result, we briefly review other interventions shown to reduce criminal behavior and incarceration.

As previously noted, the literature that examines long-run incarceration effects of social safety net programs is small. One of the few papers in this literature is Bailey et al. (2020), which finds that the roll-out of food stamps reduced incarceration rates by 0.5 percentage points. This treatment-on-the-treated estimate is striking because it is a measure of individuals incarcerated at a certain age rather than a cumulative measure, like the one we analyze in this paper.[27] The literature on the long-run effects of educational interventions on incarceration is more expansive and has generally shown large effects. Deming (2011) shows that winning a school-choice lottery reduces days incarcerated by 42% among the group that is *ex-ante* at highest risk for incarceration. This group is roughly 90% Black and economically disadvantaged, very similar to the group affected by the OBRA90 expansion. Furthermore, the effect is even larger for middle-school students, who are close in age to those affected by the increase in eligibility that we study. Johnson and Jackson (2019) show that increased educational resources also have large effects on incarceration. They

---

[27] In a similar vein, Barr and Smith (2019) examine the effects of the Food Stamp Program in North Carolina and find that full exposure to the program (from in-utero through age 5) reduced felony *convictions* by 0.7 percentage points relative to cohorts that were not exposed.

find that access to Head Start decreased later-life incarceration rates for poor children by 2.5 p.p., while a 10% increase in school funding decreased rates by 8.0 percentage points. Related, Gelber et al. (2015) consider the long-run impacts of youth employment programs on incarceration and find a reduction of 9.9% overall, 12.4% for Black children, and 16.3% for those 16 and under. Notably, these reductions occurred despite modest earnings increases ($535 cumulatively over 5 years) as a result of the program, suggesting that policies with relatively small financial impacts can still strongly influence long-run incarceration outcomes. Finally, research on early-life health interventions (e.g., Olds et al., 1998) and on lead exposure (e.g., Aizer and Currie, 2019; Billings and Schnepel, 2018) indicates that childhood health is a strong determinant of later-life criminal justice outcomes. Collectively, these papers underscore that childhood investments, particularly those targeting disadvantaged groups, have frequently generated large returns in terms of reduced incarceration.[28]

We conclude this section by reiterating that childhood Medicaid eligibility has also demonstrated substantial positive impacts on recipients' long-term human capital and health status (e.g., Boudreaux et al., 2016; Brown et al., 2019; Cohodes et al., 2016; Goodman-Bacon, 2016; Wherry et al., 2018). These findings motivate the next section, in which we illustrate, to the extent possible, that the results of this paper are consistent with existing economic research.

6   Supporting Evidence

The purpose of this paper is to establish that youth Medicaid eligibility reduces later-life incarceration, but an important question is *why* youth Medicaid eligibility reduces later-life incarceration. Identifying precise mechanisms would require detailed and comprehensive data that tracks individuals from childhood to adulthood and includes exact dates of birth as well as incarceration outcomes. To the best of our knowledge, such data do not currently exist. With the data we have, however, we shed some light on the changes in behavior that lead to reductions in adult incarceration. First, we show that the decrease in incarceration is driven by reductions in financially motivated crimes, consistent with a large literature showing that financially motivated criminal activity is sensitive to changes in economic conditions. Second, we provide evidence that increased Medicaid eligibility improved the detection of attention deficit and hyperactivity disorder ("ADHD"), a condition that is associated with adverse later-life criminal justice outcomes (Mohr-Jensen and Steinhausen, 2016). In the subsections below, we motivate and detail our findings.

---

[28] It is also important to note that the propensity to commit crime appears to be very sensitive to *contemporaneous* economic and health shocks as well. For example, Foley (2011) finds temporal patterns in financially motivated crime specifically in jurisdictions that do not stagger the disbursement of welfare payments (see also Wright et al. 2017 and Carr and Packham 2019). There are also several papers showing that the likelihood of recidivism responds strongly to the availability of welfare payments to ex-offenders and to local economic conditions upon release (e.g., Agan and Makowsky 2018; Tuttle 2019; Yang 2017a,b). There is even a result that days with unusually high pollen counts have lower crime rates (Chalfin et al., 2019).

Figure 6 – Impact of the OBRA90 Expansion on Adult Incarceration
by Offender Type (Black)
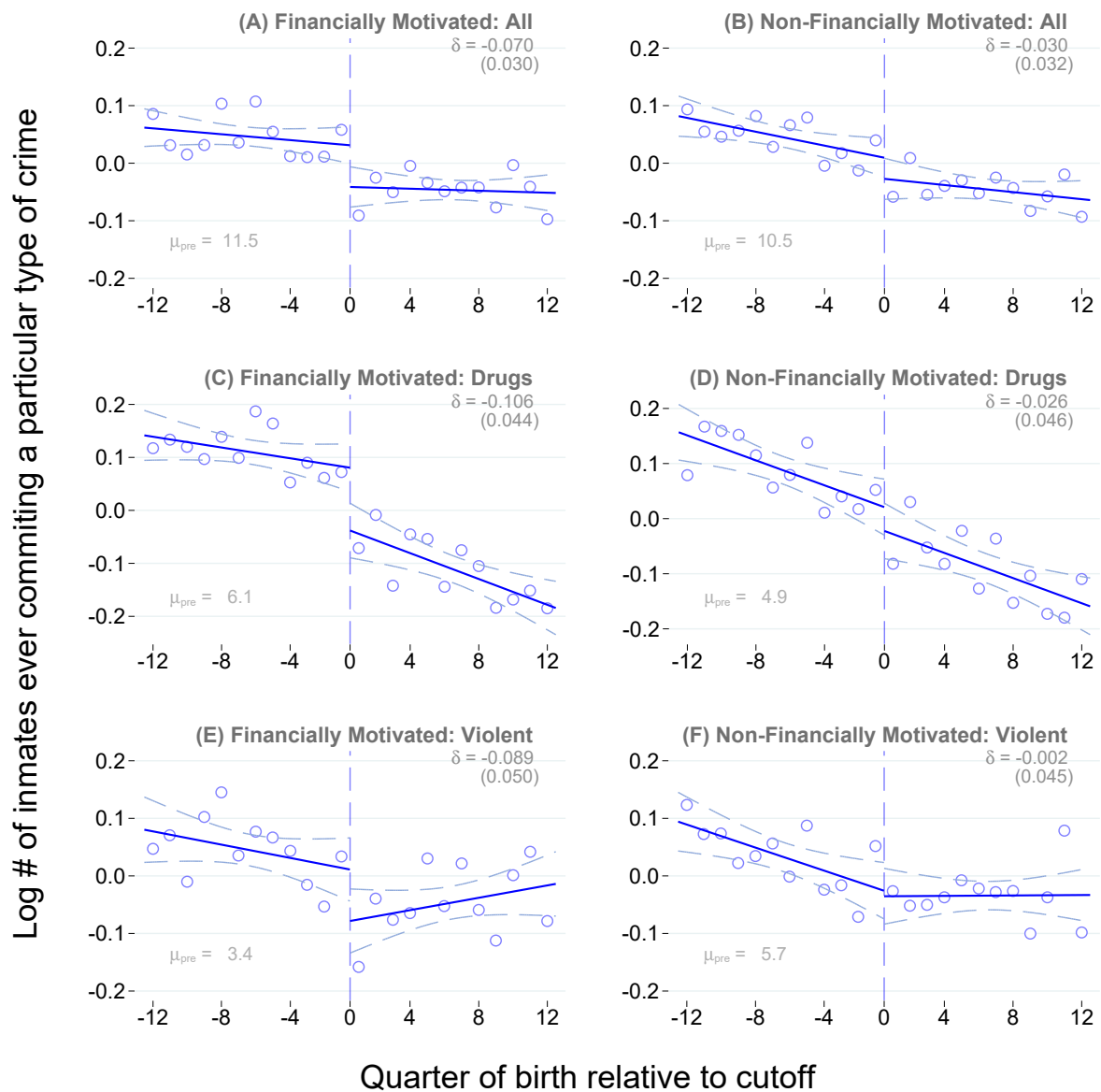
Notes: The purpose of this figure is to display the results of our heterogeneity analysis by type of crime. Each panel represents log counts of individuals in each daily birth cohort that have ever been incarcerated for committing that particular type of crime. Note that, since inmates often commit multiple crime types, certain inmates will be represented in multiple graphs. Financially motivated crimes are those done in service of achieving financial gain (e.g., drug trafficking/selling/manufacturing/distributing, robbery, forgery). See Figure 2 for a general description of the regression discontinuity plots.

Source: Author calculations using Florida DOC Incarceration Data.

6.1   Reductions in Financially Motivated Crimes

Childhood Medicaid access has been causally linked to improvements in human capital accumulation and labor market outcomes. For instance, Brown et al. (2019) study several Medicaid expansions from the early 1980s and find that an additional year of simulated childhood Medicaid eligibility increased total taxes paid (a summary measure of increased income and reduced EITC use) by 2.6% as of age 28. Cohodes et al. (2016) utilize a similar identification strategy and find increases in high school and college completion of 0.22 and 0.30 percentage points, respectively, per year of eligibility.[29] As first formalized by Becker (1968), an increase in the opportunity cost of committing a crime—which the effects in Brown et al. (2019) or Cohodes et al. (2016) would generate—is predicted to reduce criminal behavior as marginal (potential) offenders substitute away from illegal methods of income generation. This theorized effect has been shown to exist in a wide range of empirical settings as both crime and recidivism exhibit sensitivity to contemporaneous policies affecting financial circumstances (e.g., Agan and Makowsky, 2018; Carr and Packham, 2019; Foley, 2011; Tuttle, 2019; Wright et al., 2017; Yang, 2017a). Furthermore, this literature shows that financially motivated offenses are particularly sensitive to changes in economic conditions. Accordingly, we expect the long-run increases in economic opportunity from Medicaid to lead to reductions primarily in financially motivated crimes. To test this hypothesis, we estimate regressions separately for criminals incarcerated for financially motivated offenses and for criminals incarcerated for non-financially motivated offenses.[30]

The results, displayed in Panels A and B of Figure 6, align with our expectations: financially motivated incarcerations show a large reduction (-7.0%), while non-financially motivated incarcerations show a smaller decrease (a statistically insignificant -3.0%).[31] We then divide the sample further and investigate drug and violent crimes separately. As shown in Panels C and D, incarcerations for financially motivated drug crimes (i.e., selling, manufacturing, and distributing) show a large decrease of 10.6 percent, whereas incarcerations for other drug offenses (i.e., possession) appear largely unaffected. The same pattern exists for violent crimes, where there are meaningful decreases in robberies (the majority of violent crimes with financial motivation), but not in other types of violent crime.

We consider other types of offenses in Appendix Figure A15. They generally follow the same pattern: financial crimes (e.g., fraud and forgery) show large decreases, while sex crimes and weapons charges do not. Furthermore, we conduct *offense-level* analyses analogous to those described above (i.e., Y_c in Equation 1 is the number of offenses attached to offenders who were born

---

[29] These findings are echoed by Goodman-Bacon (2016), who studies the long-run effects of the roll-out of Medicaid to children and finds substantial increases in labor force participation and human capital achievement.

[30] An offense is defined as financially motivated if there is clear revenue-seeking behavior, such as grand theft. The distinction is subject to discretion, but our results are robust to using more strict or less strict definitions. Under our preferred classifications, we find that 61 percent of offenses are financially motivated. Criminals are associated with a given type of offense if they have ever been incarcerated for that particular type of crime. Therefore, criminals can be associated both with financially motivated and non-financially motivated offenses.

[31] We cannot reject the null hypothesis that these two values are equal at traditional significance levels, but we view the relative magnitudes and visual differences as noteworthy.

on day c).[32] The results, which are displayed in Appendix Figures A16 and A17, are qualitatively very similar to our offender-level results, although generally larger in magnitude.

Finally, it is worth contrasting the results in this section with the impacts of Medicaid on *contemporaneous* criminal justice outcomes. Papers that have studied these short-run impacts have found that reductions in crime (e.g., Vogler, 2020; Wen et al., 2017) and recidivism (Aslim et al., 2019) stem from reductions in violent crimes and aggravated assaults. Additionally, these studies often point to changes in substance abuse treatment as a likely mechanism behind the reductions in criminal activity. This provides a sharp contrast with our findings in two key ways. First, the only reductions in violent crimes we detect are financially motivated (robberies) as opposed to violent crimes related to impulsivity. Second, drug possession crimes, which are likely to be associated with substance abuse, do not appear to respond to childhood Medicaid coverage. These differences suggest that the mechanisms driving contemporaneous changes in criminal behavior are distinct from the mechanisms underlying the long-run impact that we estimate. Our results are consistent, however, with the hypothesis that individuals with improved economic prospects due to Medicaid are less likely to engage in financially motivated criminal activity.[33]

6.2 Improved Detection of Mental Health Conditions

Childhood Medicaid exposure has been causally linked to long-term improvements in health, including reductions in hospitalizations and in mortality (e.g., Boudreaux et al., 2016; Brown et al., 2019; Goodman-Bacon, 2016; Wherry et al., 2018). We ask whether other health effects exist. Specifically, because they have strong ties to behaviors that often lead to incarceration (Hall et al., 2019; Mohr-Jensen and Steinhausen, 2016), we investigate self-reported mental health and ADHD diagnoses.[34] Using data from the NHIS, we estimate a modified version of Equation 1 with the year-month as the running variable (the NHIS does not have exact date of birth).

Although we find no persuasive evidence of improvements in self-reported mental health during adulthood (Appendix Figure A20), this is consistent with our earlier findings that childhood Medicaid does not appear to have impacts on crimes associated with mental distress, such as non-financially motivated violent crime. We do, however, find a large (3.1 p.p.) increase in Black children who report that they have ever been diagnosed with ADHD.[35] These findings are displayed

---

[32] This exercise has the advantage of examining mutually exclusive categories, which differs from the inmate-level analyses that assign offenders to categories if they have *ever* committed a particular crime type and thus may include the same offender in multiple categories.

[33] A possible intermediate input to long-run improvements in human capital and labor market outcomes is the financial benefit that Medicaid coverage provides during childhood (Gross and Notowidigdo, 2011; Gruber and Yelowitz, 1999; Finkelstein et al., 2012). We investigate this possibility in Appendix Section D and consider other ways in which improved childhood financial circumstances may lead to fewer incarcerations.

[34] Improved health also leads to increased human capital accumulation (Case et al., 2002) and reduces financial strain from management of chronic conditions. Consequently, it is possible that healthier individuals have improved economic circumstances which operate in a similar manner to those discussed in Section 6.1. Likewise, enhanced economic opportunity may increase later-life health. Nonetheless, improvements in mental health, which is the focus of this subsection, may lead to reduced incarceration along margins other than those discussed in the previous subsection.

[35] As discussed in Appendix C, our ADHD findings are robust to inference techniques for coarse running variables

Figure 7 – Impact of the OBRA90 Expansion on ADHD Diagnosis (NHIS)
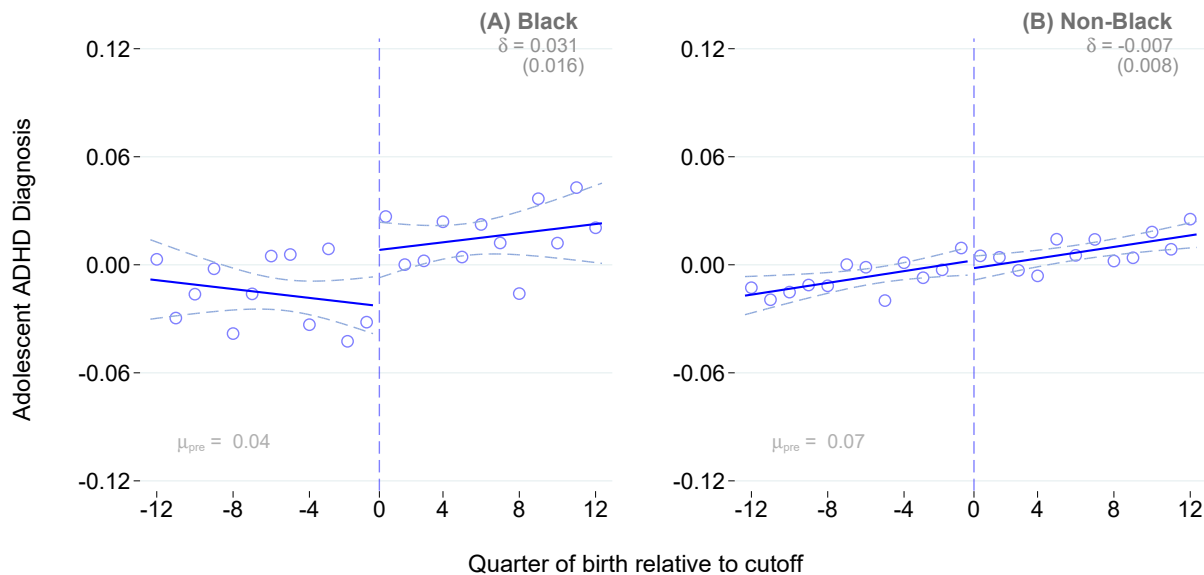
Notes: The purpose of this figure is to display the increase in ADHD diagnoses for children born just after the cutoff. The outcome measure asks whether a child has *ever* been diagnosed with ADHD and is measured from ages 11-17, which is the age range of our cohorts of interest when this variable was available. The coefficients of interest, δ, are generated from a modified version of Equation 1, with the year-month of birth as the running variable. These coefficients and associated standard errors (clustered at the year-month level) are displayed in the upper-right corner. As discussed in Appendix C, these coefficients remain significant at least at the 90% confidence level after applying robust inference methods developed by Kolesár and Rothe (2018). See also Appendix Figure A21 for analyses using quadratic fits. More detail on the structure of the regression discontinuity plots is detailed in the notes to Figure 2. Figures utilize National data (to increase statistical power in the detection of a relatively rare outcome), including 3,237 and 16,964 observations for Black and Non-Black children, respectively.

Source: Author calculations using the 1997-2004 National Health Interview Surveys (Blewett et al., 2019).

in Figure 7. They are particularly striking because the Expansion appears to have closed a wide gap between Black and Non-Black children in ADHD diagnosis (4% versus 7%, respectively, prior to the cutoff).[36] Most ADHD diagnoses occur during the ages of 8 to 12 (Danielson et al., 2018), which coincides closely with the ages covered by the OBRA90 expansion. Further, as noted by Currie et al. (2010), ADHD has substantial predictive power for long-run educational and social assistance outcomes, even after conditioning on health at birth and future realizations of physical health. When taken together, these findings suggest that the Expansion potentially provided substantial long-term benefits even among those who were covered by insurance at earlier ages.

Our findings on ADHD refer to diagnosis rather than treatment (treatment is not collected in the NHIS). However, increased diagnosis may be a precursor to a variety of interventions that could reduce incarceration risk among children and adolescents. As outlined in Mohr-Jensen and

---

(Kolesár and Rothe, 2018). For our adult mental health analyses, severe and moderate mental distress were defined using cutoff values of the Kessler K6 scale (Prochaska et al., 2012), which we constructed from the standard mental health questions asked by the NHIS.

[36] Because the increase in ADHD diagnoses at the cutoff is so large, we believe this finding warrants replication with administrative data, though doing so is beyond the scope of this paper.

Steinhausen (2016) and Prinz et al. (2018), there are many ways in which ADHD can lead to un-desirable outcomes. One possible path operates through schools. Children with undiagnosed ADHD may be at greater risk for disciplinary action in school; whereas children with *diagnosed* ADHD may be more likely to receive additional resources, such as medication, counseling, or special education (Currie and Stabile, 2006). Because disciplinary action in school greatly increases the likelihood of adult incarceration (Bacher-Hicks et al., 2019), increased ADHD diagnoses and subsequent treatment may have substantially reduced the number of Black children who are ultimately imprisoned.[37] Without additional data, this "school-to-prison pipeline" avenue remains a conjecture, but our findings establish a potentially important intermediate link between youth Medicaid eligibility and later-life incarceration.

7   Cost-Benefit Analysis

We conclude by estimating how much of the cost of the OBRA90 expansion was recouped through savings associated with reduced incarceration. Table 3 summarizes these costs and benefits. This exercise has several components which we briefly discuss here and elucidate further in the sub-sections below. The discussion that follows focuses on our cost-benefit calculations for Florida (Columns 1 and 2 of Table 3), while the details regarding the analogous cost-benefit for the NCRP States (Columns 3 and 4) are included in Appendix Section B.

We begin by estimating the years of incarceration that were avoided because of the policy. This outcome is distinct from our main result, the number of individuals ever incarcerated, as it incorporates time spent in prison for each inmate. Next, we calculate the cost of providing additional coverage to the Black cohorts born after September 30, 1983. We then move on to quantifying benefits. Using our estimated reduction in years incarcerated, we calculate the "direct" benefit of the prison costs avoided (reduction in incarceration years × annual cost of incarceration). We then add the estimated avoided "economic" costs of incarceration (i.e., earnings losses and increased government expenditure, which we take from Mueller-Smith (2015)). These components are then combined to obtain a final cost-benefit estimate, followed by a discussion of (potentially large) benefits that are not captured in the calculation.

7.1   Calculating the Reduction in Years Incarcerated

Because more years in prison require more expenditure on incarceration, a critical input for the benefit calculation is the number of incarceration years that were avoided as a result of the policy. So, we re-estimate Equation 1 when the dependent variable is the log of years spent in prison as of age 28 for a particular date of birth.[38] These results are displayed in Figure 8. The percentage change in

---

[37] Specifically, Bacher-Hicks et al. (2019) find that a 1-standard-deviation increase in suspensions at the school level increases the probability of ever being incarcerated as of age 21 by 2.5 percentage points. This effect increases to 4.4 percentage points for Black and Hispanic males.

[38] We use the log of incarceration years as our preferred outcome because our main result is in logs and because the distribution of incarceration years is right-skewed. We also show the effect on incarceration years in levels in Appendix

Figure 8 – Additional Result: Impact of the OBRA90 Expansion on Years Incarcerated
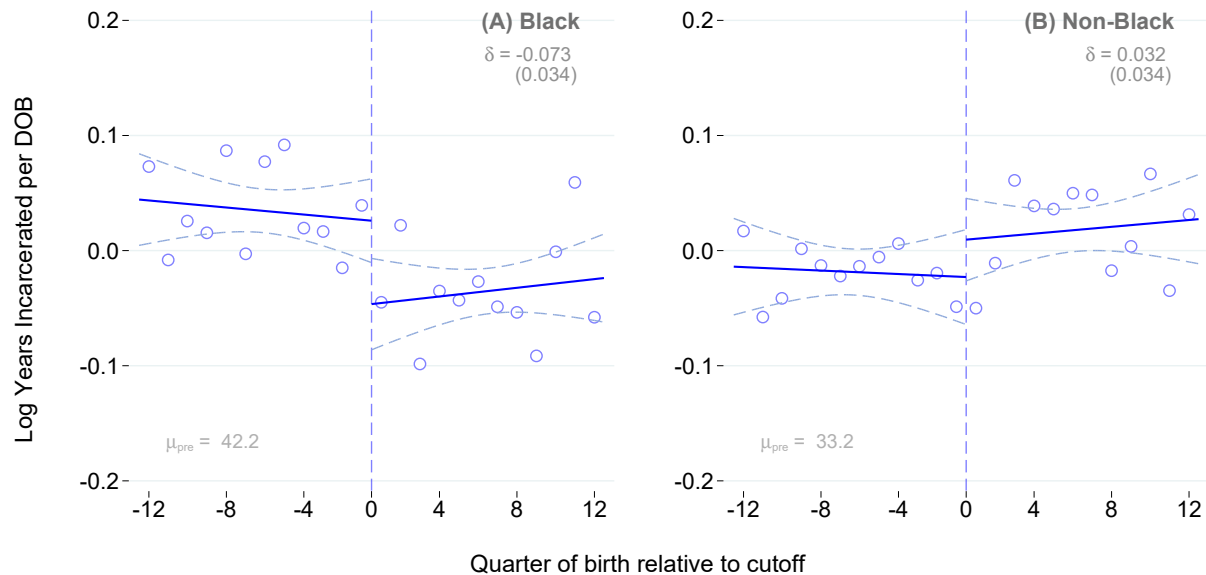
Notes: The purpose of this figure is to display the results of our regression discontinuity design with log (cumulative) years incarcerated as the outcome. See Figure 2 for a general description of the regression discontinuity plots.

Source: Author calculations using Florida DOC Incarceration Data.

our incarceration-years result will differ from our ever-incarcerated main result if the typical length of imprisonment changes due to intensive-margin effects or compositional changes. However, as shown in Appendix Figure A7, intensive-margin measures, including average years incarcerated per daily cohort, change little after the cutoff. Consequently, we anticipate a percentage change in incarceration years that is similar in magnitude to the extensive-margin effect displayed in Figure 2. Panel A of Figure 8 confirms this: we find a 7.3% reduction in years served by age 28, well within the confidence interval of our main effect.[39] Using the pre-cutoff mean of 42.2 years incarcerated per birth-date-cohort, we find that this effect translates to 1,124 avoided years of incarceration by age 28 for the cohort born in the year after the cutoff (42.2×365 days $\times$ -7.3%). Importantly, the impact on the total number of incarceration years appears to stabilize by age 28, as displayed in Figure A23, which is consistent with the effect-by-age results for our ever-incarcerated analysis. This stability suggests that a cost-benefit analysis conducted at a later age (i.e., with more data) would produce similar results.

Figure A22. It is similar to the log specification.

[39] We again see no clear effect for Non-Black incarceration years (Panel B of Figure 8). A similar outcome worth investigating is the total years sentenced to prison. The years-sentenced results are displayed in Appendix Figure A22. While the estimates are imprecise ($p \approx 0.17$), the coefficient is similar to the effect on years incarcerated (a 6.4% decrease).

7.2 Costs of Coverage

Our cost estimate is presented at the top of Table 3. For simplicity, we restrict our analysis—costs and benefits—to the "Initial Treated Cohort": Black Floridians born in the year after the cutoff. The size of this cohort as of the 2010 Census is given in the first row. We then multiply this count by the cohort-level eligibility increase in Table 1 to obtain the total number of eligibility-years for the Initial Treated Cohort (70,883). However, as noted in Section 2, not all eligible children actually participated in the program. Accordingly, we further multiply the number of eligibility years by a take-up rate of 47.3% to obtain an estimate of coverage-years (33,525).[40] Finally, to estimate the cost per coverage year, we use data from the Medicaid Statistical Information System ("MSIS").[41] According to MSIS records, the average yearly cost of providing Medicaid to non-disabled Floridian children between 1991 and 1996 was $1,789 (inflated to 2012 dollars using a 3% discount rate).[42] We use figures from 1991 to 1996 because in these years the Initial Treated Cohort is between the ages of 8 and 13, the approximate ages for which they gain eligibility as a result of the treatment.[43] We inflate to 2012 as this is the year in which the Initial Treated Cohort turned 28, the age at which we conduct our analysis. Multiplying this cost amount with the estimated coverage years yields a final cost estimate of $59.97 million dollars for the coverage of the Floridian Initial Treated Cohort.

7.3 Direct and Economic Benefits of Reduced Incarceration

We now ask how much of this cost was recouped through reduced incarceration. We begin by calculating the direct cost of state prison facilities (Component B of Table 3), often referred to as the bed cost.[44] The direct cost is obtained by multiplying the number of avoided incarceration years by the annual incarceration cost per inmate. As shown above, the number of avoided incarceration years for the Initial Treated Cohort is 1,124. The estimate for the per-inmate cost of imprisonment comes from the Vera Institute of Justice (2012). These expenses include labor costs, capital costs, inmate healthcare, pension payments, and administrative costs. For Florida, the annual cost per inmate is $21,581 in 2012 dollars. Taking the product, we find that the policy saved approximately $24.3 million in direct costs as a result of housing fewer inmates. While this is a substantial sum in

---

[40] Recall that the take-up rate is defined as the estimated increase in children covered (10.5 p.p.) divided by the increase in children eligible (22.2 p.p.). This calculation uses the more conservative of the take-up rates calculated in Section 5.1, which are based on increases in coverage and eligibility across the Southern Census Region (the sensitivity to this choice is discussed at the conclusion of Section 7.3). Recall also that the Southern region is used as a proxy for Florida because the NHIS does not have sufficient geographic identifiers or sample coverage to estimate a Florida-specific take-up rate.

[41] This data was generously aggregated and made available by Brown et al. (2019) as part of their Medicaid eligibility calculator.

[42] For the purposes of our cost-benefit exercise, inflating Medicaid costs using a 3% rate is the same as calculating a net present value by discounting costs and benefits back to a common year (i.e., 1991) using a 3% discount rate.

[43] Costs from 1997 (age 14) may be confounded by the introduction of CHIP. Unfortunately, we do not know the cost of providing Medicaid to children by age, only by age group. Specifically, we have figures for non-disabled children under the age of 21. This should lead to conservative cost estimates. Using the Medical Expenditure Panel Survey, we find that spending among Medicaid beneficiaries is 17% to 19% lower for ages 8 to 14 compared to the under-21 average.

[44] For clarity, although we use the word *cost* frequently in this subsection, we are referring to avoided costs and thus benefits.

Table 3 – Cost-Benefit Analysis

State(s) Covered Economic Costs Included	(1) Florida No	(2) Florida Yes	(3) NCRP No	(4) NCRP Yes
Costs:				
Increase in Medicaid Costs (A):				
Cohort size:	48,450	48,450	314,270	314,270
× Cohort-average eligibility increase (in years)	1.463	1.463	0.603	0.603
× Estimated take-up*	47.3%	47.3%	40.5%	40.5%
× Avg. yearly cost of coverage ('91-'96)	1,789	1,789	1,871	1,871
Total Costs (A)	**59,974,758**	**59,974,758**	**143,500,502**	**143,500,502**
Benefits (Avoided Costs):				
Direct Incarceration Costs (B):				
Cost of incarceration	21,581	21,581	32,901	32,901
× Reduction in years incarcerated	1,124	1,124	2,508	2,508
	24,257,044	24,257,044	82,507,812	82,507,812
Economic Incarceration Penalty (C):				
Lost earnings + gov't assistance		16,000		16,000
× Reduction in incarcerations		259		876
		4,140,000		14,017,536
Economic Duration Penalty (D):				
Lost earnings + gov't assistance (per year)		10,000		10,000
× Reduction in years incarcerated		1,124		2,508
		11,240,000		25,077,600
Total Benefits (B + C + D)	**24,257,044**	**39,637,044**	**82,507,812**	**121,602,948**
Benefits / Costs	0.40	0.66	0.57	0.85

Notes: The purpose of this table is to display calculations for our cost-benefit analysis. Each column represents a calculation with a different set of inputs. Specifically, we vary (1) the states analyzed in our cost-benefit calculation and (2) whether or not the economic costs of incarceration (derived from Mueller-Smith, 2015) are included in the analysis. The combination used for a given column is presented at the top of the table. All dollar figures are denominated in 2012 dollars.

*We are unable to calculate take-up rates for Florida and the NCRP states due to the absence of state identifiers in the NHIS. Accordingly, the estimated take-up rate used for Florida scales utilizes the Southern Census Region take-up rate of 47.3%. Likewise, the estimated take-up rate used for the NCRP states utilizes the national take-up rate of 40.5%. These take-up rates, which are based on conservative assumptions, are discussed in greater detail in Section 5.1.

Source: Author calculations using Florida DOC Incarceration Data, the 2010 Census 10% Sample (Ruggles et al., 2020), direct incarceration costs from The Vera Institute of Justice (2012), and estimates of the economic impact of incarceration from Mueller-Smith (2015).

comparison to the costs of the program, it omits several key costs related to incarceration, such as the economic disruption associated with imprisonment.

In order to capture the "indirect" costs of imprisonment, we incorporate results from Mueller-Smith (2015), who estimates the causal impact of incarceration on labor-force and government-assistance outcomes within five years of release using the random assignment of defendants to courtrooms.[45] We add these losses to our cost-benefit in Columns 2 and 4 of Table 3. Critically, Mueller-Smith (2015) argues that these costs of incarceration are non-linear with respect to years served. Specifically, he calculates the costs separately for stays of 6 months, 1 year, and 2 years. For tractability—and because our sample has average stays longer than 2 years—we impose linearity in duration and estimate (i) a one-time cost of ever going to prison and (ii) a per-year cost thereafter.[46] We refer to the one-time cost as the *Economic Incarceration Penalty* and term the per-year cost the *Economic Duration Penalty*.[47] Our estimate of the Economic Incarceration Penalty is approximately $16,000. In Component C of Table 3, we multiply this penalty by the reduction in the number of ever-incarcerated individuals to get $4.1 million in benefits for the Initial Treated Cohort. Our estimate of the Economic Duration Penalty is approximately $10,000. In Component D, we multiply this yearly penalty by the number of avoided incarceration years and find an additional $11.2 million in indirect benefits for Black cohorts born one year after the cutoff.

Altogether, our calculations suggest that each dollar spent on the Expansion returned 40 cents in reduced direct incarceration costs *alone*. Including estimates of the avoided economic damage from incarceration, this estimate increases to 66 cents for each dollar spent.[48] As in Section 5, we consider the external validity of these findings by applying the methodology outlined in the Subsections 7.1 through 7.3 to the NCRP states. We find that the policy recouped 57 cents (direct) or 85 cents (direct and economic) for each dollar spent, which suggests that the OBRA90 expansion was also cost-effective at a national level.[49] Thus, while our cost-benefit calculation only considers incarceration—and is therefore not a comprehensive cost-benefit calculation in the spirit of Hendren and Sprung-Keyser (2020)—we find that the OBRA90 expansion is highly cost-effective, even without considering any of the other benefits it has been shown to provide.

---

[45] Mueller-Smith uses a 5 percent discount for these post-release costs. Accordingly, it should be noted that the five-year post-release window will lead us to understate the indirect costs of incarceration if the effects persist longer than five years.

[46] Explicitly, we fit a line through the three cost-duration coordinates. The estimated y-intercept is the cost of entering prison for any amount of time. The estimated slope is the yearly cost of being incarcerated on post-release economic outcomes.

[47] Intuitively, the Economic Incarceration Penalty may be seen primarily as a negative signal common to all ex-offenders while the Economic Duration Penalty may represent the erosion of human capital from imprisonment.

[48] Again, these calculations utilize the more conservative of the take-up rates calculated in Section 5.1. If the take-up rate of 32.0% were used instead, our estimated returns would increase to between 60 cents on the dollar (direct incarceration costs only) and 98 cents on the dollar (when including economic losses from imprisonment).

[49] See Appendix Section B for a more comprehensive discussion of these calculations. These ratios are larger than those for Florida, primarily due to higher direct costs of incarceration in NCRP states, where imprisonment is nearly 50% more expensive.

7.4   Unincorporated Benefits of Reduced Incarceration

The savings we calculate are large despite the fact that they do not capture all of the public safety benefits related to reduced incarceration. Namely, they omit the underlying decrease in crimes that lead to prison.[50] We illustrate the magnitude of this omission in two different ways using the 11.0% reduction in robberies shown in Appendix Figure A16E. One way is to sum the victim costs and the associated criminal justice expenses (e.g., arrest and prosecution) with estimates from Cohen and Piquero (2009). We find that the reduction in robberies, with no consideration for incarceration costs, recouped 9 cents per dollar. If we instead use estimates of victims' willingness-to-pay to prevent the robberies (also taken from Cohen and Piquero, 2009), we calculate a savings of 48 cents per dollar. Another way to frame the reduction in criminal activity is in terms of a more standard public safety expenditure. Specifically, we relate our estimate to reductions in robberies caused by increased police presence. Evans and Owens (2007) show that a 1% increase in police officers leads to a 1.34% decrease in robberies. Accordingly, the 11% reduction in robberies that we find as a result of the youth Medicaid expansion had an effect comparable to an 8.2% increase in police force.[51]

8   Conclusion

Policymakers from all levels of government have recently charged that public safety can be achieved more effectively and more equitably by focusing on investments in social programs. A common theme that emerges from recent discussions is to invest more money into public health systems. This paper provides a clear demonstration that investments in public health can yield public safety benefits. We examine a large youth Medicaid expansion that increased access to healthcare for thousands of children from low-income households, particularly Black families. As a result of the policy, 17% of all Black children gained Medicaid eligibility and 7% gained Medicaid coverage. We find that 1,000 years of additional youth Medicaid eligibility in the Black population leads to 3.6 fewer Black individuals going to prison by age 28. Summarily, racial disparities in incarceration decreased, public safety improved, and taxpayers benefited from reduced prison expenses. Our findings highlight the need for future research to consider the long-run impact of other social safety net programs on public safety outcomes.

---

[50] We do not include the reductions in crime into our main cost-benefit analysis for three reasons. First, the Florida incarceration data do not capture the complete effects of the Expansion on criminal activity. Insofar as childhood Medicaid influenced criminal activity not leading to imprisonment (i.e., misdemeanors), that will not be captured in the data, making any calculation of benefits from reduced crime incomplete. Second, not all criminal offenses—such as drug trafficking, which was highly affected by the Expansion—have readily available cost valuations. Third, valuations of the cost of criminality incorporate a high degree of judgment and uncertainty in the cost quantification process, and thus we prefer to discuss them separately.

[51] These comparisons are not on exactly equal footing: Evans and Owens (2007) are estimating the single-year effect of police presence on many cohorts, while we are estimating the reductions from a single cohort over many years. Nonetheless, we believe the contrast to be instructive.

References

Agan, A. Y. and M. D. Makowsky (2018). The Minimum Wage, EITC, and Criminal Recidivism. Working Paper 25116, National Bureau of Economic Research.

Aizer, A. and J. Currie (2019). Lead and Juvenile Delinquency: New Evidence from Linked Birth, School, and Juvenile Detention Records. *Review of Economics and Statistics 101*(4), 575–587.

Aslim, E. G., M. C. Mungan, C. Navarro, and H. Yu (2019). The Effect of Public Health Insurance on Criminal Recidivism. *George Mason Law & Economics Research Paper* (19-19).

Bacher-Hicks, A., S. B. Billings, and D. J. Deming (2019). The School to Prison Pipeline: Long-Run Impacts of School Suspensions on Adult Crime. Working Paper 26257, National Bureau of Economic Research.

Bailey, M. J., H. W. Hoynes, M. Rossin-Slater, and R. Walker (2020). Is the Social Safety Net a Long-Term Investment? Large-scale Evidence from the Food Stamps Program. Working Paper 26942, National Bureau of Economic Research.

Barr, A. and C. Gibbs (2019). Breaking the Cycle? Intergenerational Effects of an Anti-Poverty Program in Early Childhood. Working Paper 19-141, EdWorkingPaper Series.

Barr, A. and A. Smith (2019). Fighting Crime in the Cradle: The Effects of Early Childhood Access to Nutritional Assistance. Working paper. http://people.tamu.edu/ abarr/AB_AS_FoodStamps_Crime_6_18_2019.pdf [Date Accessed: July 24, 2020].

Becker, G. S. (1968). Crime and Punishment: An Economic Approach. *Journal of Political Economy 76*(2), 169–217.

Billings, S. B. and K. T. Schnepel (2018, July). Life after Lead: Effects of Early Interventions for Children Exposed to Lead. *American Economic Journal: Applied Economics 10*(3), 315–44.

Blank, R. (2006). What Did the 1990s Welfare Reforms Accomplish? *Public Policy and the Income Distribution*, 33.

Blewett, L. A., J. A. Rivera Drew, M. L. King, and K. C. Williams (2019). IPUMS Health Surveys: National Health Interview Survey: Version 6.4 [dataset]. Minneapolis, MN. https://doi.org/10.18128/D070.V6.4.

Bonczar, T. P. (2003). Prevalence of Imprisonment in the U.S. Population, 1974-2001. Technical report, Bureau of Justice Statistics.

Boudreaux, M. H., E. Golberstein, and D. D. McAlpine (2016). The Long-Term Impacts of Medicaid Exposure in Early Childhood: Evidence from the Program's Origin. *Journal of Health Economics 45*, 161–175.

Brown, D. W., A. E. Kowalski, and I. Z. Lurie (2019). Long-Term Impacts of Childhood Medicaid Expansions on Outcomes in Adulthood. *The Review of Economic Studies 87*(2), 792–821.

Buckles, K. S. and D. M. Hungerman (2013). Season of Birth and Later Outcomes: Old Questions, New Answers. *Review of Economics and Statistics 95*(3), 711–724.

Bureau of Justice Statistics (2019). National Corrections Reporting Program, [United States], 2000-2016. Inter-university Consortium for Political and Social Research [distributor].

Calonico, S., M. D. Cattaneo, and R. Titiunik (2014). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica 82*(6), 2295–2326.

Card, D. and L. D. Shore-Sheppard (2004). Using Discontinuous Eligibility Rules to Identify the Effects of the Federal Medicaid Expansions on Low-Income Children. *Review of Economics and Statistics 86*(3), 752–766.

Carr, J. B. and A. Packham (2019). SNAP Benefits and Crime: Evidence from Changing Disbursement Schedules. *The Review of Economics and Statistics 101*(2), 310–325.

Case, A., D. Lubotsky, and C. Paxson (2002). Economic Status and Health in Childhood: The Origins of the Gradient. *American Economic Review 92*(5), 1308–1334.

Chalfin, A., S. Danagoulian, and M. Deza (2019). More Sneezing, Less Crime? Health Shocks and the Market for Offenses. *Journal of Health Economics 68*, 102230.

Chetty, R., J. N. Friedman, N. Hendren, M. R. Jones, and S. R. Porter (2018). The Opportunity Atlas: Mapping the Childhood Roots of Social Mobility. Working Paper 25147, National Bureau of Economic Research.

Cohen, M. A. and A. R. Piquero (2009). New Evidence on the Monetary Value of Saving a High Risk Youth. *Journal of Quantitative Criminology 25*(1).

Cohodes, S. R., D. S. Grossman, S. A. Kleiner, and M. F. Lovenheim (2016). The Effect of Child Health Insurance Access on Schooling: Evidence from Public Insurance Expansions. *Journal of Human Resources 51*(3), 727–759.

Conger, R. D., X. Ge, G. H. Elder, F. O. Lorenz, and R. L. Simons (1994). Economic Stress, Coercive Family Process, and Developmental Problems of Adolescents. *Child development 65*(2), 541–561.

Cook, P. J. and S. Kang (2016). Birthdays, Schooling, and Crime: Regression-Discontinuity Analysis of School Performance, Delinquency, Dropout, and Crime Initiation. *American Economic Journal: Applied Economics 8*(1), 33–57.

Currie, J. (2004). The Take Up of Social Benefits. Working Paper 10488, National Bureau of Economic Research.

Currie, J. and J. Gruber (1996). Health Insurance Eligibility, Utilization of Medical Care, and Child Health. *The Quarterly Journal of Economics 111*(2), 431–466.

Currie, J. and M. Stabile (2006). Child Mental Health and Human Capital Accumulation: The Case of ADHD. *Journal of Health Economics 25*(6), 1094 – 1118.

Currie, J., M. Stabile, P. Manivong, and L. L. Roos (2010). Child Health and Young Adult Outcomes. *The Journal of Human Resources 45*(3), 517–548.

Cutler, D. M. and J. Gruber (1996). Does Public Insurance Crowd out Private Insurance? *The Quarterly Journal of Economics 111*(2), 391–430.

Danielson, M. L., R. H. Bitsko, R. M. Ghandour, J. R. Holbrook, M. D. Kogan, and S. J. Blumberg (2018). Prevalence of Parent-Reported ADHD Diagnosis and Associated Treatment Among U.S. Children and Adolescents. *Journal of Clinical Child and Adolescent Psychology 47*(2), 199–212.

Deming, D. J. (2011). Better Schools, Less Crime? *The Quarterly Journal of Economics 126*(4), 2063–2115.

Doleac, J. (2018). New Evidence that Access to Health Care Reduces Crime. Technical report, The Brookings Institute.

East, C. N., S. Miller, M. Page, and L. R. Wherry (2017). Multi-Generational Impacts of Childhood Access to the Safety Net: Early life Exposure to Medicaid and the Next Generation's Health. Working Paper 23810, National Bureau of Economic Research.

Evans, W. N. and E. G. Owens (2007). COPS and Crime. *Journal of Public Economics 91*(1), 181 – 201.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. P. Newhouse, H. Allen, K. Baicker, and O. H. S. Group (2012). The Oregon Health Insurance Experiment: Evidence from the First Year. *The Quarterly Journal of Economics 127*(3), 1057–1106.

Foley, C. F. (2011). Welfare Payments and Crime. *The Review of Economics and Statistics 93*(1), 97–112.

Fone, Z., A. Friedson, B. J. Lipton, and J. Sabia (2020). The Dependent Coverage Mandate Took a Bite Out of Crime. Working Paper 12968, IZA Discussion Paper.

Freeman, R. B. (1996). Why Do So Many Young American Men Commit Crimes and What Might We Do About It? *Journal of Economic perspectives 10*(1), 25–42.

Gelber, A., A. Isen, and J. B. Kessler (2015). The Effects of Youth Employment: Evidence from New York City Lotteries . *The Quarterly Journal of Economics 131*(1), 423–460.

Goldsmith-Pinkham, P., M. Pinkovskiy, and J. Wallace (2020). Medicare and the Geography of Financial Health. Working paper. Retrieved from http://paulgp.github.io/papers/GPW_compressed.pdf [Date Accessed: June 27, 2020].

Goodman-Bacon, A. (2016). The Long-Run Effects of Childhood Insurance Coverage: Medicaid implementation, Adult Health, and Labor Market Outcomes. Working Paper w22899, National Bureau of Economic Research.

Gross, T. and M. Notowidigdo (2011). Health Insurance and the Consumer Bankruptcy Decision: Evidence from Expansions of Medicaid. *Journal of Public Economics 95*(7), 767–778.

Gruber, J. and A. Yelowitz (1999). Public Health Insurance and Private Savings. *Journal of Political Economy 107*(6), 1249–1274.

Hall, D., L.-W. Lee, M. W. Manseau, L. Pope, A. C. Watson, and M. Compton (2019). Major Mental Illness as a Risk Factor for Incarceration. *Psychiatric services 70*(12), 1088–1093.

Hawkins, D., K. Mettler, and P. Stein. 'Defund the Police' Gains Traction as Cities Seek to Respond to Demands for a Major Law Enforcement Shift. *The Washington Post*.

He, Q. and S. Barkowski (2020). The Effect of Health Insurance on Crime: Evidence from the Affordable Care Act Medicaid Expansion. *Health Economics*.

Heckman, J. J., S. H. Moon, R. Pinto, P. A. Savelyev, and A. Yavitz (2010). The Rate of Return to the HighScope Perry Preschool Program. *Journal of Public Economics 94*(1-2), 114–128.

Hendren, N. and B. Sprung-Keyser (2020). A Unified Welfare Analysis of Government Policies. *The Quarterly Journal of Economics 135*(3), 1209–1318.

Johnson, R. C. and C. K. Jackson (2019). Reducing Inequality through Dynamic Complementarity: Evidence from Head Start and Public School Spending. *American Economic Journal: Economic Policy 11*(4), 310–49.

Jácome, E. (2020). Mental Health and Criminal Involvement: Evidence from Losing Medicaid Eligibility. *Working paper*. https://elisajacome.github.io/Jacome/Jacome_JMP.pdf [Date Accessed: October 31, 2020].

Kolesár, M. and C. Rothe (2018). Inference in Regression Discontinuity Designs with a Discrete Running Variable. *American Economic Review 108*(8), 2277–2304.

Lee, D. S. and T. Lemieux (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature 48*(2), 281–355.

Manson, S., J. Schroeder, D. Van Riper, and S. Ruggles (2019). IPUMS National Historical Geographic Information System: Version 14.0 [dataset]. Minneapolis, MN. http://doi.org/10.18128/D050.V14.0.

Miller, S. and L. R. Wherry (2019). The Long-Term Effects of Early Life Medicaid Coverage. *Journal of Human Resources 54*(3), 785–824.

Mohr-Jensen, C. and H.-C. Steinhausen (2016). A Meta-Analysis and Systematic Review of the Risks Associated with Childhood Attention-Deficit Hyperactivity Disorder on Long-Term Outcome of Arrests, Convictions, and Incarcerations. *Clinical psychology review 48*, 32–42.

Mueller-Smith, M. (2015). The Criminal and Labor Market Impacts of Incarceration. Working paper. Retrieved from https://sites.lsa.umich.edu/mgms/wp-content/uploads/sites/283/2015/09/incar.pdf [Date Accessed: June 18, 2020].

Olds, D., C. R. Henderson, Jr, R. Cole, J. Eckenrode, H. Kitzman, D. Luckey, L. Pettitt, K. Sidora, P. Morris, and J. Powers (1998, 10). Long-term Effects of Nurse Home Visitation on Children's Criminal and Antisocial Behavior15-Year Follow-up of a Randomized Controlled Trial. *JAMA 280*(14), 1238–1244.

Pettit, B. (2012). *Invisible Men : Mass Incarceration and the Myth of Black progress*. New York: Russell Sage Foundation.

Prinz, D., M. Chernew, D. Cutler, and A. Frakt (2018). Health and Economic Activity Over the Lifecycle: Literature Review. Working Paper 24865, National Bureau of Economic Research.

Prochaska, J. J., H. Sung, W. Max, Y. Shi, and M. Ong (2012). Validity Study of the K6 scale as a Measure of Moderate Mental Distress Based on Mental Health Treatment Need and Utilization. *International Journal of Methods in Psychiatric Research 21*(2), 88–97.

Rainey, J., D. Smith, and C. Chang. Growing the LAPD was Gospel at City Hall. George Floyd Changed That. *The Los Angeles Times*.

Ruggles, S., S. Flood, R. Goeken, J. Grover, E. Meyer, J. Pacas, and M. Sobek (2020). IPUMS USA: Version 10.0 [dataset]. Minneapolis, MN. https://doi.org/10.18128/D010.V10.0.

Sawyer, W. and P. Wagner (2020). Mass incarceration: The whole pie 2020. Technical report, Prison Policy Initiative.

Shore-Sheppard, L. D. (2000). The Effect of Expanding Medicaid Eligibility on the Distribution of Children's Health Insurance Coverage. *ILR Review 54*(1), 59–77.

Simes, J. T. (2019). Place After Prison: Neighborhood Attainment and Attachment During Reentry. *Journal of Urban Affairs 41*(4), 443–463.

Sommers, B. D., M. R. Tomasi, K. Swartz, and A. M. Epstein (2012). Reasons For The Wide Variation In Medicaid Participation Rates Among States Hold Lessons For Coverage Expansion In 2014. *Health Affairs 31*(5), 909–919. PMID: 22566429.

Stockman, F. and J. Eligon. Cities Ask if It's Time to Defund Police and 'Reimagine' Public Safety. *The New York Times*.

Tuttle, C. (2019). Snapping Back: Food Stamp Bans and Criminal Recidivism. *American Economic Journal: Economic Policy 11*(2), 301–27.

U.S. Census Bureau (2008). Estimating the Medicaid Undercount in the National Health Interview Survey and Comparing False-Negative Medicaid Reporting in NHIS to the Current Population Survey. Technical report.

Vera Institute of Justice (2012). The Price of Prisons: What Incarceration Costs Taxpayers. Technical report. Technical report, Center on Sentencing and Corrections.

Vogler, J. (2020). Access to Healthcare and Criminal Behavior: Evidence from the ACA Medicaid Expansions. *Journal of Policy Analysis and Management*.

Wen, H., J. M. Hockenberry, and J. R. Cummings (2017). The Effect of Medicaid Expansion on Crime Reduction: Evidence from HIFA-Waiver Expansions. *Journal of Public Economics 154*, 67–94.

Wherry, L. and B. Meyer (2016). Saving Teens: Using a Policy Discontinuity to Estimate the Effects of Medicaid Eligibility. *Journal of Human Resources 51*(3), 556–588.

Wherry, L., S. Miller, R. Kaestner, and B. Meyer (2018). Childhood Medicaid Coverage and Later-Life Health Care Utilization. *Review of Economics and Statistics 100*(2), 287–302.

Wherry, L., S. Miller, R. Kaestner, and B. Meyer (2019). Replication Data for: "Childhood Medicaid Coverage and Later Life Health Care Utilization".

Wright, R., E. Tekin, V. Topalli, C. McClellan, T. Dickinson, and R. Rosenfeld (2017). Less Cash, Less Crime: Evidence from the Electronic Benefit Transfer Program. *The Journal of Law and Economics 60*(2), 361–383.

Yang, C. S. (2017a). Does Public Assistance Reduce Recidivism? *American Economic Review 107*(5), 551–55.

Yang, C. S. (2017b). Local Labor Markets and Criminal Recidivism. *Journal of Public Economics 147*, 16–29.

Appendix: for Online Publication

A   Analysis Using Incarceration Rates

As discussed in Section 4, the outcome in our main analysis is log counts of incarcerated individuals, rather than incarceration rates, because the data needed to construct the denominator in a rate—the adult population in Florida by exact date of birth—do not exist. However, there are two data sources that we can combine in order to generate a reasonable approximation of Florida's population by date of birth. The first data source is the 10% count files from the 2010 Decennial Census (Ruggles et al., 2020), which provides adult population counts by *quarter* of birth. We utilize these data in Appendix Figure A2 to demonstrate that the Florida population is smooth through the cutoff, but we cannot use them alone to define the denominator in an incarceration rate. Due to the highly-aggregated temporal frequency (quarter rather than date of birth), we can neither control for intra-quarter birth seasonality nor utilize the appropriate number of clusters to achieve proper inference (Kolesár and Rothe, 2018). The second data source that is useful but not sufficient for a denominator is natality data, which provide counts of births in a given state by exact date of birth. These files do not suffer from issues of temporal aggregation (as with the Census files), but, in many states, births are not representative of the (later) adult population due to migration. For instance, per the 2010 American Community Survey (Ruggles et al., 2020), only 42% of Florida adults were *born* in Florida. Below, we explain how these two sources are combined to create an incarceration rate, and we show that our main result is unchanged when the outcome is measured in rates instead of in levels.

We construct incarceration rates using an adjusted population denominator of the form:

$$\widetilde{b}^{adjusted}_{race,sex,dob} = \frac{b^{census}_{race,sex,qob}}{b^{vitalstats}_{race,sex,qob}} \times b_{race,sex,dob},$$

where $\widetilde{b}$ is a measure of births for a given race/sex/exact date-of-birth combination that has been weighted by the population counts from the Census (for each race/sex/quarter-of-birth combination) relative to the birth counts from the Florida birth files (again, for each race/sex/quarter-of-birth combination). This method allows us to (i) capture the "true" overall adult population counts in Florida while (ii) accounting for the intra-quarter seasonality of births.[1] Using this denominator, we construct our rate outcome as $Y_c = \frac{count_c}{\widetilde{b}^{adjusted}_c}$ (or as $\log(Y_c)$) and estimate Equation 1. The results of this analysis, which are displayed in Figure A8, are highly consistent with our main results. There is a discontinuity of 0.424 fewer incarcerated Black adults per 100 in the base specification (Panel A), which increases to a reduction of 0.729 incarcerated Black people per 100 when using a quadratic fit (Panel C). These results align with the estimated 0.55 fewer incarcerations per 100 that we find using our count-based analysis in Section 5.2. As before, we do not find any effects of Non-Black individuals (Panels B and D). When using log rates as our outcome measure (Panels E through H), we again find similar results: a 4.0 percent (linear) or 6.4 percent (quadratic) decrease in Black adults ever incarcerated. These are very similar to our main result, a 5.1 percent decrease in Black incarcerations, displayed in Figure 2A. Non-Black people again show no impact from the policy. Accordingly, we conclude that, even though rate-based measures are imperfect due to data limitations, our main finding is the same whether we use counts or rates as the outcome variable.

---

[1]   Note that, while Florida births were used to capture this seasonality, our results are robust to using births from other states, weighted by the fraction of those children that live in Florida as adults. These results are available upon request.

B   National Corrections Reporting Program Analysis

B.1   Data

To augment our findings from the state of Florida, we acquired the restricted-access 2000-2016 National Corrections Reporting Program (NCRP) data, which was the most recent data available as of July 2020. These data are housed by the National Archive of Criminal Justice Data (NACJD) and disseminated through the Inter-university Consortium for Political and Social Research (ICPSR). The data are constructed from files sent by state Departments of Corrections and Parole on a voluntarily basis to the Bureau of Justice Statistics (BJS), which contracts with Abt Associates to compile the multitude of state files into a single set of national files. In the period 2000-2016, many states reported these records to the BJS at least once, but only a few reported consistently through the period. Because our main outcome requires complete incarceration histories for the cohorts near the cutoff, we use only states that reported consistently across the entire period. This restriction leaves 19 states in our NCRP sample: Arizona, Colorado, Florida, Georgia, Illinois, Kentucky, Michigan, Minnesota, Missouri, Nebraska, New York, North Carolina, Oklahoma, Pennsylvania, South Carolina, Tennessee, Utah, Washington, and Wisconsin. Like the Florida data, these data capture only the state prison population (not Federal inmates).

The NCRP contains several different files, but our analysis uses only the "Prison Term File."[2] This file is constructed from prison admission and release records or from prison custody records (i.e., regular snapshots), depending on how a state reports. Each row in the Prison Term File is a stay in prison for a particular inmate.[3] For each stay, the most serious offense is listed (i.e., the offense carrying the longest sentence). The data also include standard offender demographics, including race and, in the restricted file, year and month of birth. For a thorough description of how these files were constructed, consult the National Corrections Reporting Program White Paper Series.

B.2   Additional Analysis: Years Incarcerated

Our use of the NCRP is intended to provide an external validity check for our main results (discussed in Section 5) and cost-benefit calculation, the latter of which is discussed here. The NCRP cost-benefit analysis closely follows the Florida-specific analysis in Section 7, which discusses the construction of our estimates in more detail. As discussed in Section 7, we estimate the multi-state impact of the Expansion on years incarcerated and find that the NCRP states experienced a 2.7% decrease (Appendix Figure A24) in incarceration years as a result of the policy. As in the case of Florida, this estimate is nearly identical to the reduction in individuals ever-incarcerated, again consistent with the fact that most of the impact of the policy loads onto the extensive margin. This represents a 4.5% decrease per year of additional eligibility (-2.7% / 0.60 additional years of eligibility), which is very similar to Florida's scaled estimate of 5.0% per eligibility-year (-7.1% decline / 1.46 additional years). Applying this change to the pre-cutoff mean yields an estimate of 2,508 saved incarceration years for the Initial Treated Cohorts living in the NCRP states.

Turning now to costs of coverage in NCRP states (Component A in Columns 3 and 4 of Table 3), we again focus on the Initial Treated Cohort, Black residents of NCRP states born one year after the cutoff. The size of this cohort as of the 2010 Census is given in the first row. We then multiply this count by the cohort-level eligibility increase of 0.60 (calculated in a similar manner to those in Table 1) to obtain the total number of eligibility-years for the Initial Treated Cohort (189,505). As noted in Section 7, we then multiply by the

---

[2]   We do not use the files pertaining to post-confinement community supervision (e.g., parole).
[3]   Each inmate has an identifier that is consistent with state but not across states (in the event of incarcerations in multiple states).

take-up rate (40.5%), since not all eligible children actually participated in the program.[4] This yields an estimate of 76,678 coverage-years. Finally, we multiply the number of coverage years by the cost per year of coverage from the MSIS. The product of these two components is an increase in Medicaid expenditure on the Initial Treated Cohort of $143.5 million.

Next, we consider the benefits of the policy. First, we quantify the direct benefits in Component B. The direct cost of an incarceration year is generally much higher outside of Florida—a weighted average of $32,901 in the NCRP states versus $22,581 in Florida (Vera Institute of Justice, 2012)—and, as calculated above, this cost was avoided for 2,508 years' worth of incarceration. Next, we show the indirect (economic) benefits in Components C and D (determined using estimates from Mueller-Smith, 2015, as discussed in detail in Section 7). Combining the direct costs of incarceration with the economic losses, we find that total benefits range from $82.5 to $121.6 million, where the range is determined by whether post-release economic losses are included in the calculation. Finally, taking the ratio of benefits to costs, we estimate that the policy recouped between $0.57 and $0.85 from avoided imprisonment on every dollar spent on new enrollees. These ratios are larger than those for Florida, due primarily to substantially larger incarceration costs for these states (nearly 50% larger than Florida), while the impact of the policy per eligibility-year are roughly the same. While these estimates are not robust to inference procedures developed by Kolesár and Rothe (2018)—and thus should be interpreted with a degree of caution—they are suggestive that the cost estimates generated from our Florida-specific analysis have generalizability to a national level.

---

[4] For NCRP states, the take-up rate was calculated using national increases in coverage and eligibility.

C   Robust Inference for Discrete Running Variables

For certain datasets used in our analysis—namely the NCRP data used to assess external validity and the NHIS data used to evaluate the OBRA90 expansion's policy on ADHD diagnoses—we are required to use year-month of birth (rather than exact date of birth) for our running variable due to data limitations. As noted by Kolesár and Rothe (2018), when the running variable is discrete, additional procedures are necessary to achieve robust inference, as clustering on the running variable of does not provide sufficient coverage. Accordingly, the authors suggest a procedure which involves specification of a tuning parameter, K, that bounds the second derivative of the conditional expectation function (in absolute value). Effectively, this places an upper bound on how quickly the polynomial in the running variable, $f(\cdot)$, can change over a single year-month birth cohort.[5] To determine this K-parameter, we follow rules of thumb suggested by Kolesár and Rothe (2018) as well as Goldsmith-Pinkham et al. (2020). Specifically, we fit a quadratic function to the observations to the three years left of the cutoff, recover the coefficient associated with the quadratic terms—i.e., the second derivative—and multiply it by a scalar. For purposes of our analysis, we choose scalars equal to four (following Goldsmith-Pinkham et al., 2020) and equal to eight (following rules of thumb suggested by Kolesár and Rothe, 2018). We refer to the confidence intervals that are generated from these K-parameters as our "Narrow-Bound CI" and "Wide-Bound CI," respectively.

The results of our estimation using these techniques are presented in Figure A25, with the impact on the log number of inmates ever incarcerated (using NCRP data) on the left and the impact on ADHD diagnoses among adolescents (using NHIS data) on the right. Each panel illustrates how the estimates and confidence intervals change as the bandwidth for evaluation narrows. The NCRP-related analysis never achieves traditional levels statistical significance over the bandwidths presented, using either the Narrow-Bound or Wide-Bound confidence intervals. The most precise estimate generated is using the 18-month bandwidth, which yields a 90% confidence interval ranging from -0.063 to +0.006. In contrast, the NHIS analysis on ADHD achieves statistical significance at a 90% level or greater for all bandwidths and both K-parameters selected.

---

[5]   Technically, the running variable used for our main analysis, exact date of birth, is also discrete and is therefore subject to this procedure. However, due to the granular nature of the variable, the logical choice of K approaches zero, which provides inference that is generally equivalent to clustering on the running variable.

D   Additional Supporting Evidence: Childhood Inputs

In addition to the supporting evidence discussed in Section 6, we also explore hypotheses relating to the increased financial resources that are made available to low-income households as a result of Medicaid coverage. As demonstrated by Gruber and Yelowitz (1999), expanded Medicaid eligibility leads to meaningful increases in consumption, which in turn may reduce household financial stress and increase investment in childhood.[6] [7] While improved childhood resources may operate through the channel discussed in Section 6.1 (increased economic opportunity), they may also reduce incarceration in other ways. For instance, Conger et al. (1994) note that increased economic stress is associated with adolescent behavioral issues (notably anti-social and aggressive behavior), which could in turn lead to criminal activity. Further, increased resources may allow families to move to better neighborhoods with lower levels of criminal activity and/or police presence.

If Medicaid's impact on childhood financial circumstances is a channel for reduced future imprisonment, we anticipate that our effects will be more pronounced in areas demonstrating stronger relationships between marginal financial improvements and decreased incarceration. To test this, we incorporate county-level data from Chetty et al. (2018) that describe adult incarceration rates with respect to the distribution of parental income in early life.[8] Using these data, we estimate the relationship between adult incarceration and childhood income rank for each county and recover a county-specific slope, the estimated "income-incarceration gradient." These estimates are then used to categorize counties into those with steep slopes (i.e., those where marginal increases in income are associated with above-median drops in adult incarceration) and shallow slopes (vice versa).[9] We then re-estimate Equation 1 for offenders from steep- and shallow-slope counties (offenders were assigned to counties based on the location of their first offense, since that is the best proxy we have for county of childhood).

The results of this analysis are displayed in Appendix Figure A19. First, in Panel A, we demonstrate the relationship between adult incarceration and parental income for above-median (steep) slope and below-median (shallow) slope counties. While the two groups have similar overall rates of imprisonment, the incarceration rate for Black men at the bottom of the income distribution is 7 percentage points higher in steep-slope counties. Further, as shown in Panel B, while these slopes are not causally estimated, they are

---

[6]   In particular, Gruber and Yelowitz (1999) estimate that an additional $1,000 in Medicaid eligibility results in a $100 increase in consumer spending among eligible beneficiaries. The Expansion increased eligibility by approximately 6 years among the eligible, and the annual cost of childhood Medicaid was nearly $1,800 in 2019 dollars. When combined with the Gruber and Yelowitz (1999) estimates, this translates to roughly $1,080 in increased spending as a result of this expansion. This calculation uses the average cost of childhood Medicaid coverage from 1991 to 1996, as determined using the Medicaid Statistical Information System data made available by Brown et al. (2019), which we inflate to 2019 dollars. Thus, the calculation is: $1,800 per year × 6 years of eligibility among the eligible × $100 in spending per $1,000 of eligibility. Given that the affected families were below the FPL ($25,250 in 2019 dollars), this consumption shock is a meaningful one.

[7]   In addition to increased household resources, Medicaid expansions have also been shown to reduce bankruptcy (Gross and Notowidigdo, 2011); therefore, Medicaid may also reduce financial risk and alleviate domestic stress. Similarly, Finkelstein et al. (2012) find that adult Medicaid coverage improves self-reported health, including mental health, within the first month of coverage, an effect they attribute to reduced financial strain.

[8]   Specifically, Chetty et al. (2018) provide county-level data on adult incarceration—defined as residence in a correctional facility in the 2010 Census—for children living in households at select percentiles of the income distribution. Because this data contains non-causal associations of household income and later life adulthood, it is an imperfect proxy for our ideal dataset, which would ideally detail causal relationships between household resources and adult incarceration.

[9]   Before this classification occurs, slopes are adjusted by partialling-out the effect of baseline incarceration rates. This generates two groups of counties that have similar overall rates of incarceration, but different rates at lower points in the income distribution.

uncorrelated with poverty, which is itself strongly associated with high incarceration rates. Finally, the bottom half of Figure 6 displays the log counts of individuals ever incarcerated, with separate analyses for inmates from above-median (steep) slopes in Panel C and below-median (shallow) slopes in Panel D. We find that the effects of the OBRA90 expansion are roughly twice as large in above-median counties (-7.5%) as below-median (-3.5%). In order to attribute these differences solely to improved childhood financial circumstances, then it would be necessary to first establish that these income-incarceration gradients are indicative of a causal relationship, which we cannot do. Nonetheless, this higher degree of responsiveness, while only suggestive, is consistent with the idea that the early-life financial benefits provided by Medicaid are a component of the long-term effects that we observe.

Figure A1 – Smoothness of Cohort Characteristics:
Household Variables for Children Age 0-7 (NHIS)

Quarter of birth relative to cutoff
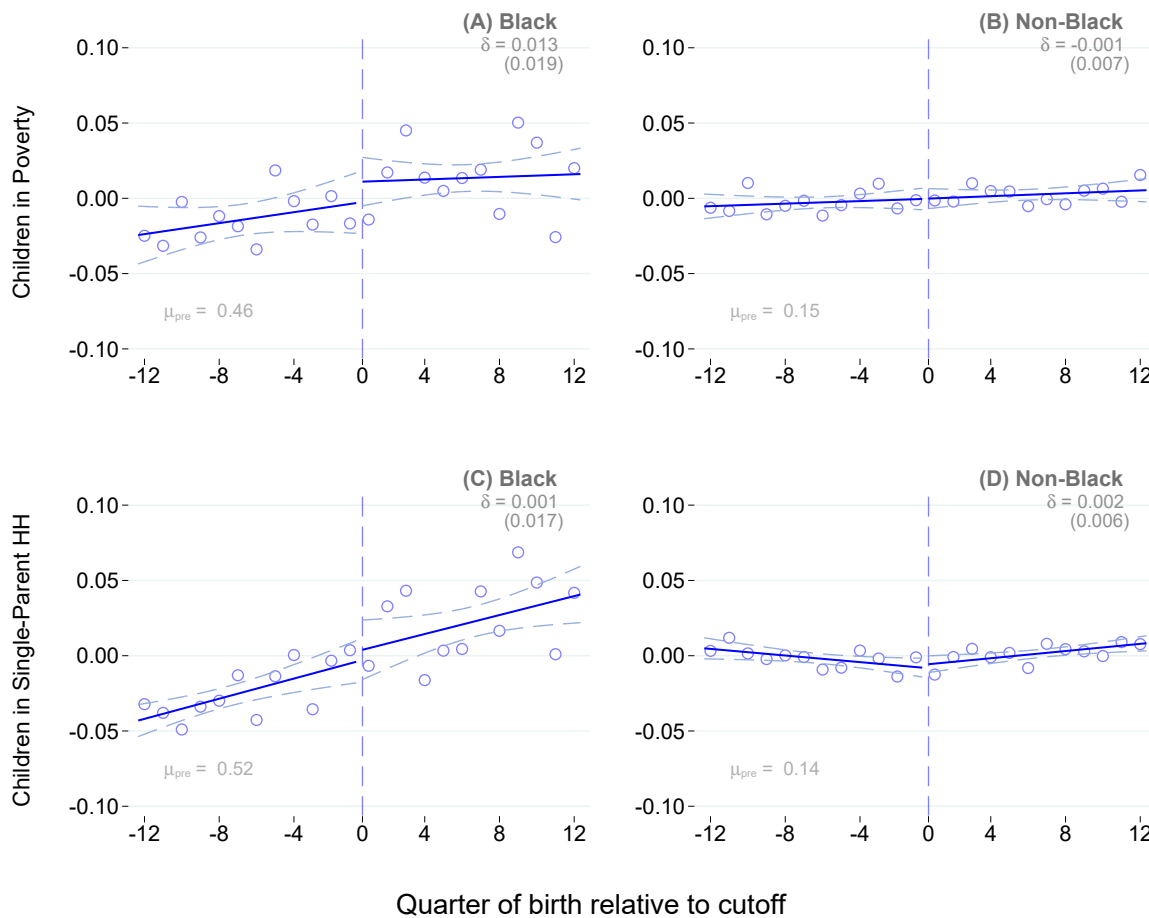
Notes: The purpose of this figure is to display the smoothness of cohort characteristics across the cutoff. The sample includes all children ages 0-7 born within 3 years of the cutoff (none of which had yet been treated by the OBRA90 expansion). Panels A and B detail the fraction of Black and Non-Black children in poverty, respectively, while Panels C and D detail the fraction of children in single-parent households. Each dot represents the average of the outcome variable in 3-month bins, after partialling-out calendar month effects. The lines presented are generated from linear regressions with associated 90 percent confidence intervals (displayed using dashes). The estimated coefficients, δ, and associated standard errors generated from Equation 1 are presented in the upper right of each panel, while the pre-cutoff means of coverage are presented bottom left. Standard errors are clustered on the year-month of birth. Figures utilize 12,920-14,679 and 64,599-69,095 observations for Black and Non-Black children, respectively.

*Source:* Author calculations using the 1982-1991 National Health Interview Surveys.

Figure A2 – Smoothness of Cohort Characteristics:
Florida Population (2010) by Quarter of Birth

Quarter of birth relative to cutoff
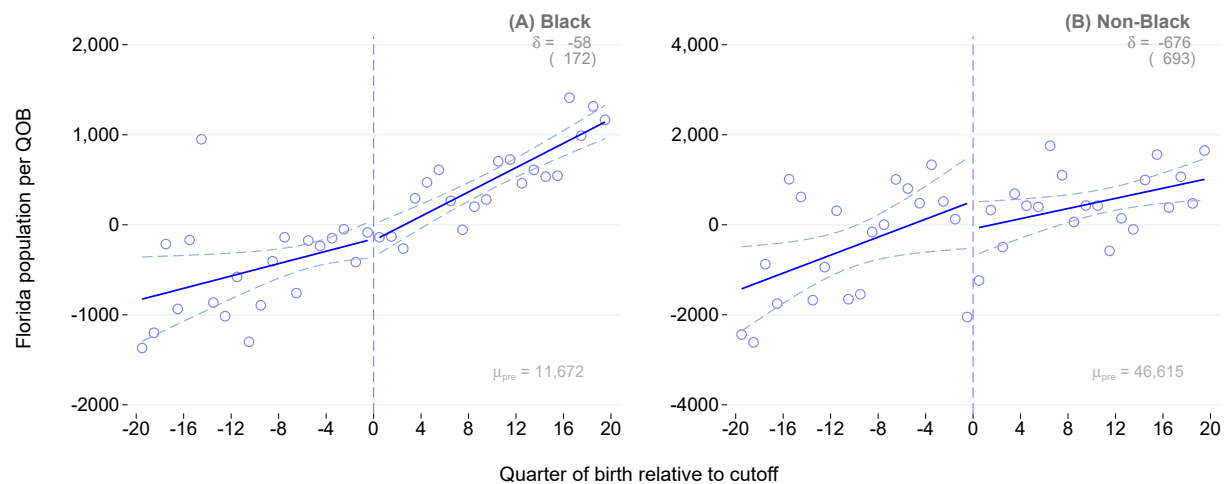
Notes: The purpose of this figure is to display the smoothness of cohort population across the cutoff. The sample includes 10% of respondents to the 2010 Census born with 5 years of the cutoff. Panels A and B detail the de-seasonalized population for Black and Non-Black Floridians, respectively. The coefficients of interest, δ, are generated from a modified version of Equation 1, with the year-quarter of birth as the running variable. These coefficients and associated standard errors (clustered at the year-quarter level) are displayed in the upper-right corner. Pre-cutoff means of population by birth quarter (μ_{pre}) are in the presented bottom right. Note that, unlike all other plots presented in this paper, this analysis uses a bandwidth of 5 years. This is to increase the number of clusters used to calculate standard errors (from 24 in a 3-year bandwidth to 40 in a 5-year bandwidth) and to increase precision. Results using a 3-year bandwidth, which are available upon request, are qualitatively similar. See more detail on the structure of the regression discontinuity plots in Figure A1.

Source: Author calculations using the 2010 Decennial Census 10% Sample (Ruggles et al., 2020)

Figure A3 – Impact of the OBRA90 Expansion on Other
Social Program Take-up (Black, NHIS)
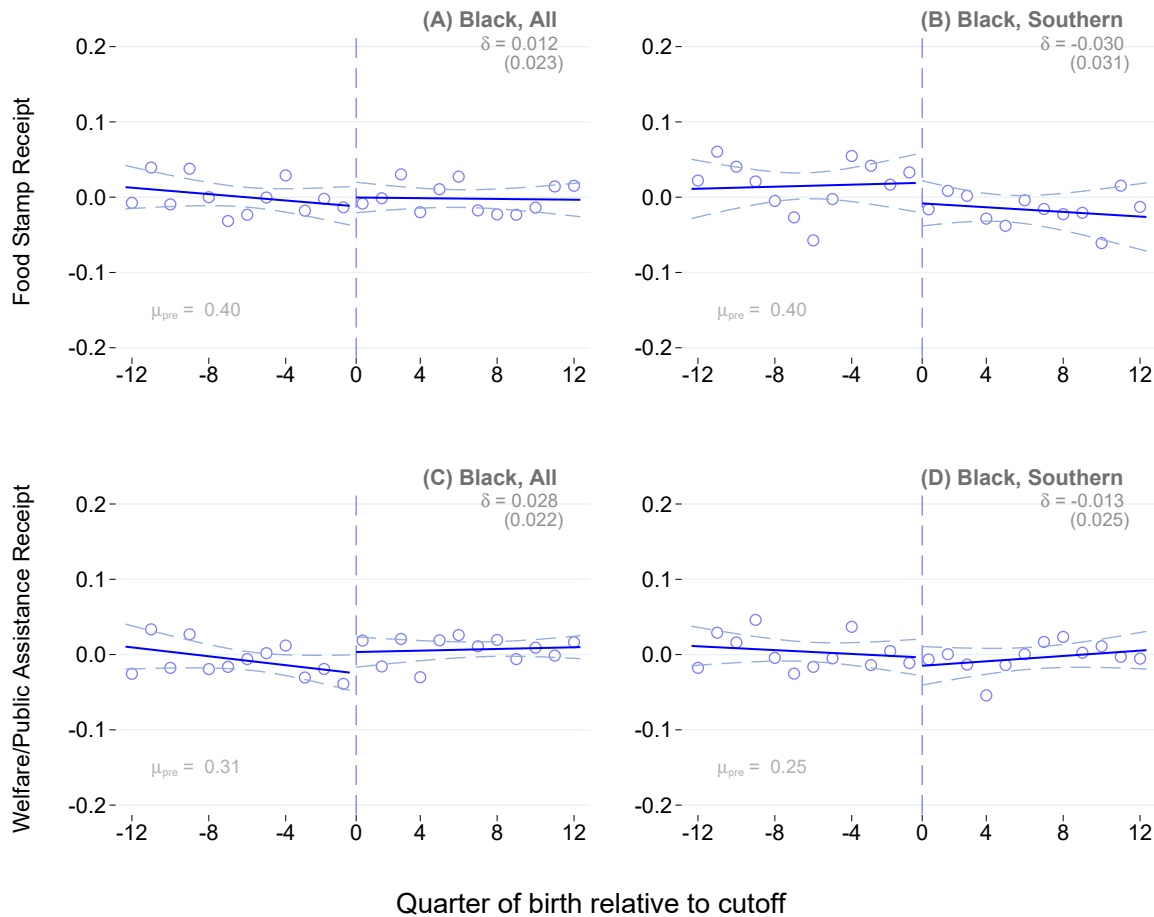
Notes: The purpose of this figure is to display the impact—or lack thereof—of the OBRA90 Expansion on take-up of other social insurance programs. Panels A and B detail the fraction of children in living in households receiving food stamps nationally and in the Southern Census Region, respectively, while Panels C and D detail the fraction of children in households receiving welfare or other public assistance. See Figure A1 for more detail on the structure of regression discontinuity plots. Figures utilize 6,651-7,268 and 3,274-3,636 observations for National and Southern samples, respectively.

Source: Author calculations using the 1992-96 National Health Interview Surveys.

Figure A4 – First Stage: Impact of the OBRA90 Expansion on Medicaid Coverage
(Alternate Samples and Specifications, NHIS)
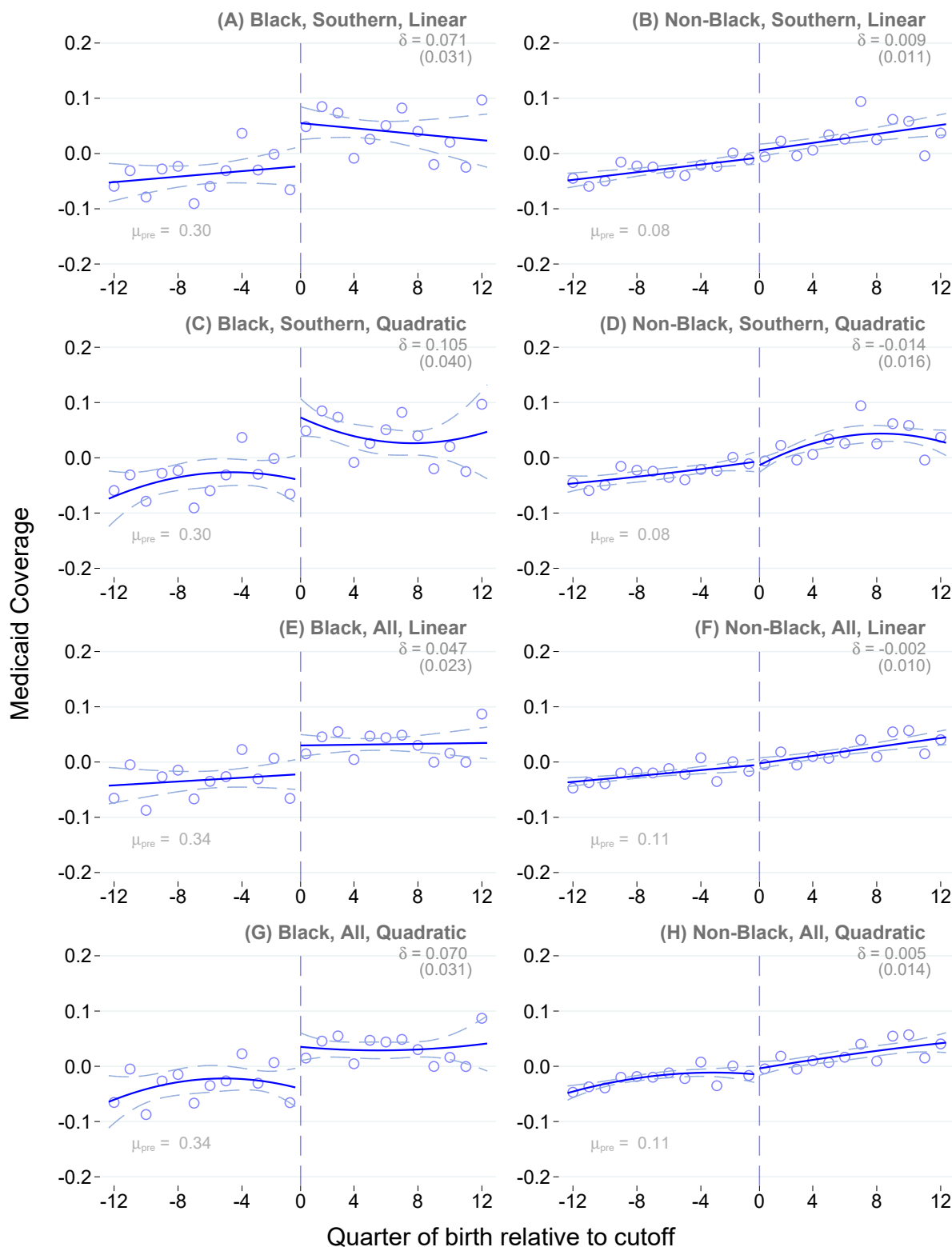
Quarter of birth relative to cutoff

Notes: The purpose of this figure is to display the increases in Medicaid coverage as a result of the OBRA90 expansion. See Figure 1 for more detail on the structure of regression discontinuity plots. Figures utilize 3,208-6,529 and 9,852-32,836 observations for Black and Non-Black samples, respectively.

Source: Author calculations using the 1992-96 National Health Interview Surveys.

Figure A5 – Impact of the OBRA90 Expansion on Private Insurance Crowd-Out
(Alternate Samples and Specifications, NHIS)
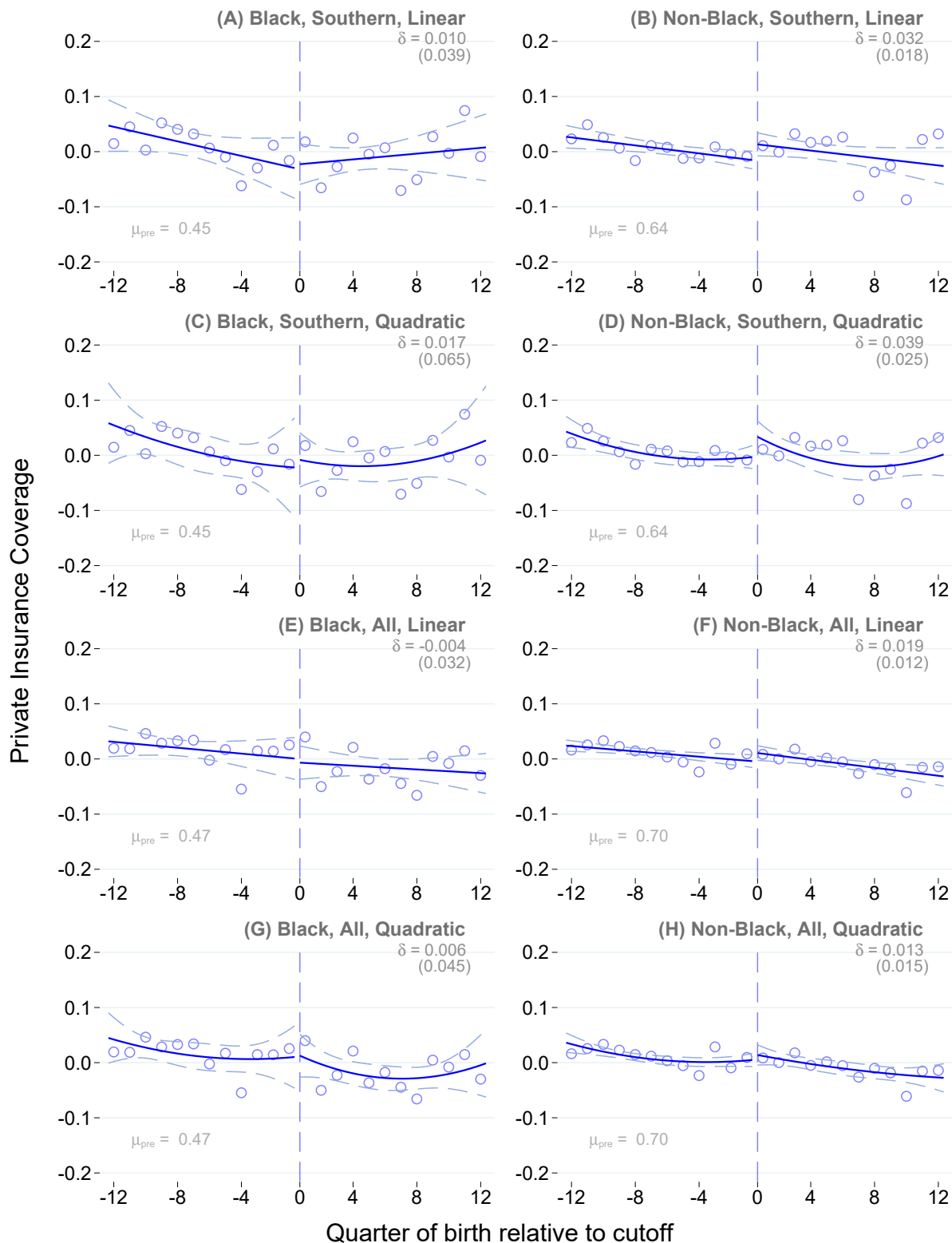
Notes: The purpose of this figure is to display the lack of private insurance crowd-out as a result of the OBRA90 expansion. See Figure 1 for more detail on the structure of regression discontinuity plots. Figures utilize 3,182-6,458 and 9,796-32,690 observations for Black and Non-Black samples, respectively.

Source: Author calculations using the 1992-96 National Health Interview Surveys.

Figure A6 – Additional Results: Impact of the OBRA90 Expansion
on Alternative Incarceration Measures
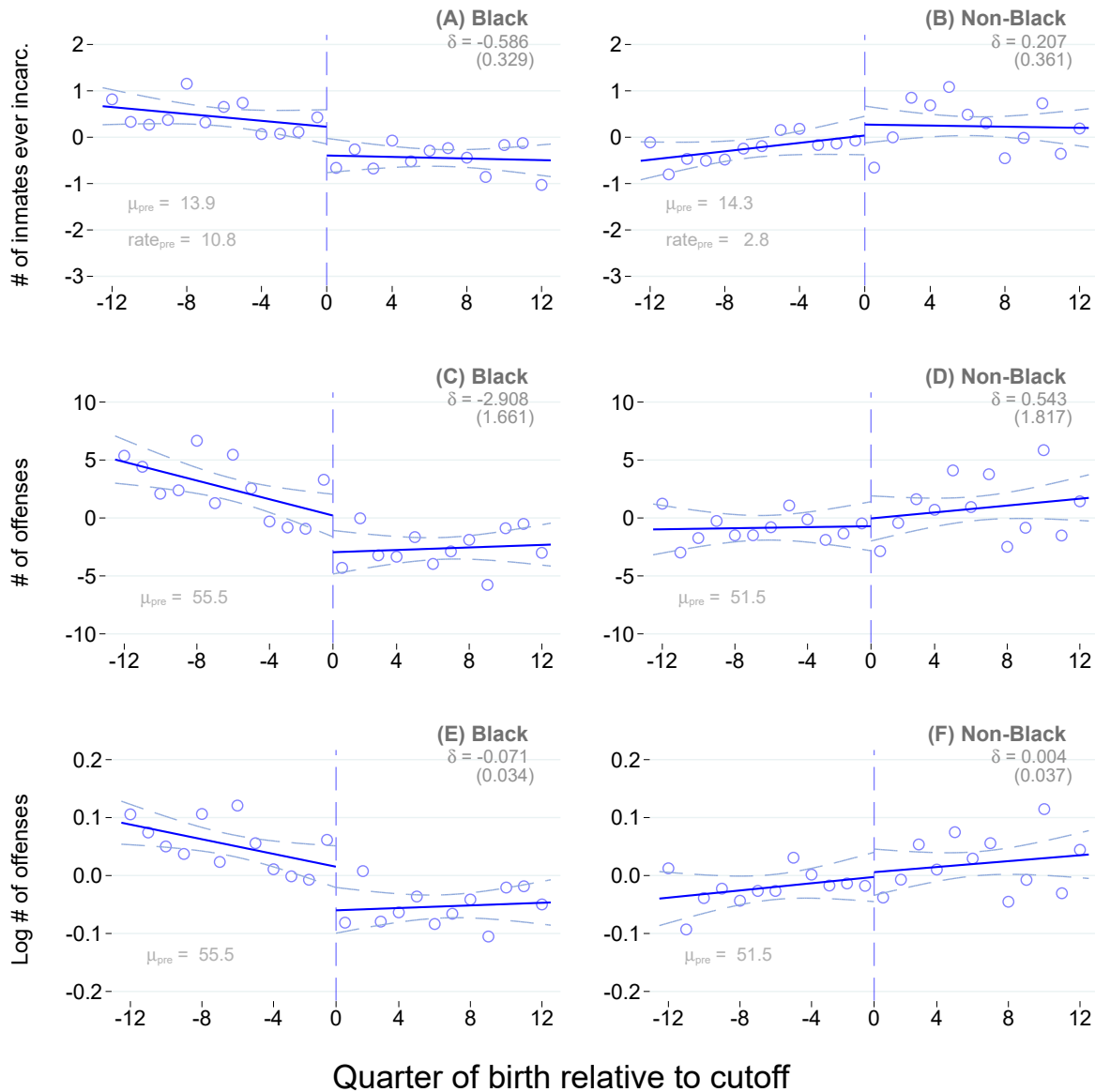
Quarter of birth relative to cutoff

Notes: The purpose of this figure is to display the results of our analysis when using alternate incarceration measures. All left-hand columns present results for Black inmates, while right-hand columns present results for Non-Black inmates. The first row (Panels A and B) details results using counts of ever-incarcerated individuals for each DOB cohort, rather than log counts as presented in Figure 2. The second row (Panels C and D) represent the count of offenses committed (rather than inmate counts) by each DOB cohort. Finally, the last row (Panels E and F) present the log versions of Panels C and D, respectively. As in the main text, all outcomes are measured as of age 28. See Figure 2 for a general description of the regression discontinuity plots.

*Source:* Author calculations using Florida DOC Incarceration Data.

Figure A7 – Additional Results: Impact of the OBRA90 Expansion
on Intensive-Margin Incarceration Measures
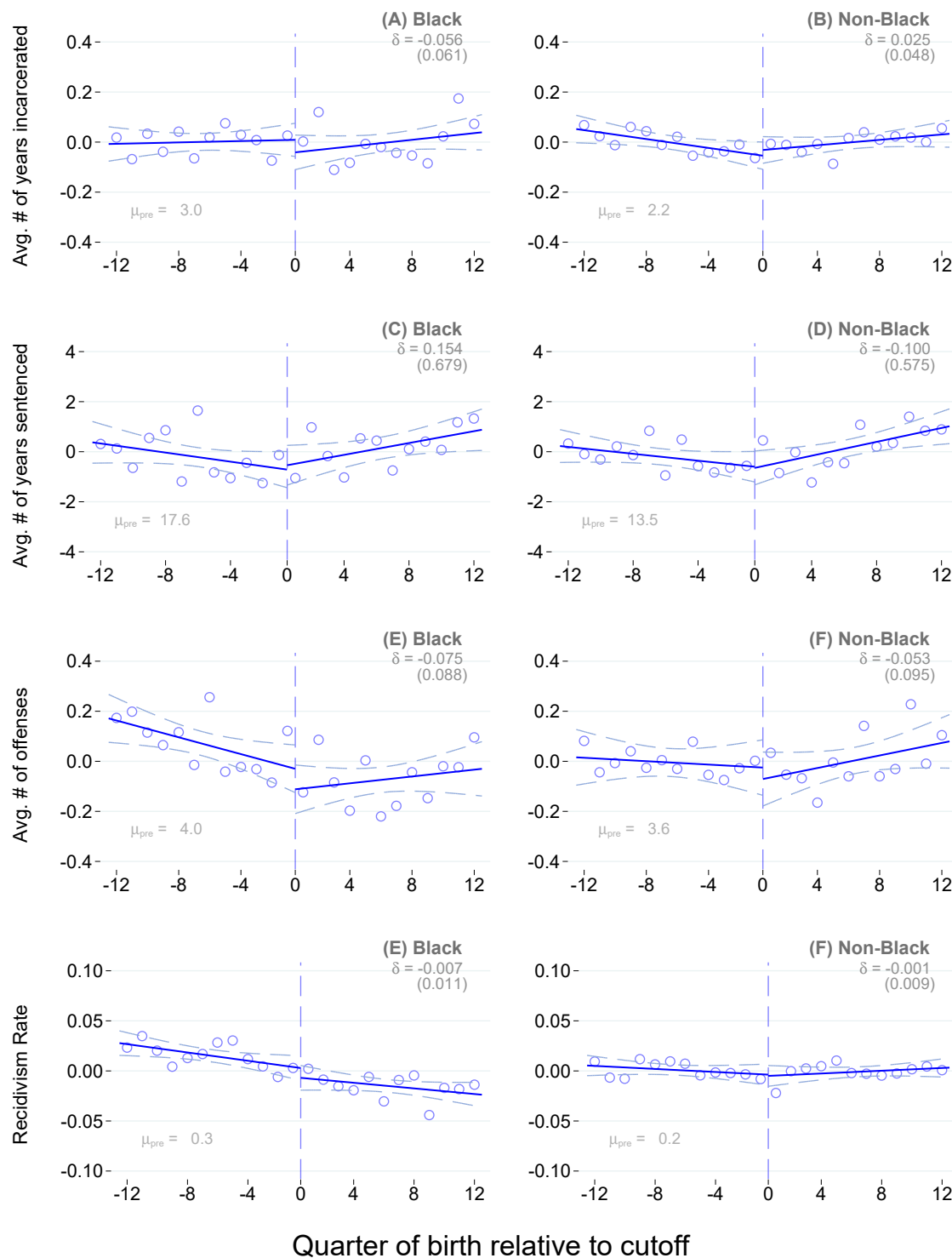
Quarter of birth relative to cutoff

Notes: The purpose of this figure is to display the results of our analysis when using alternate intensive-margin outcomes. All left-hand columns present results for Black inmates, while right-hand columns present results for Non-Black inmates. The rows represent: (1) average years incarcerated per inmate; (2) average years sentenced per inmate; (3) average number of offenses per inmate; and (4) recidivism rate for each DOB cohort, respectively. As in the main text, all outcomes are measured as of age 28. See Figure 2 for a general description of the regression discontinuity plots.

Source: Author calculations using Florida DOC Incarceration Data.

Figure A8 – Additional Results: Impact of the OBRA90 Expansion on
Adult Incarceration (in Rates)
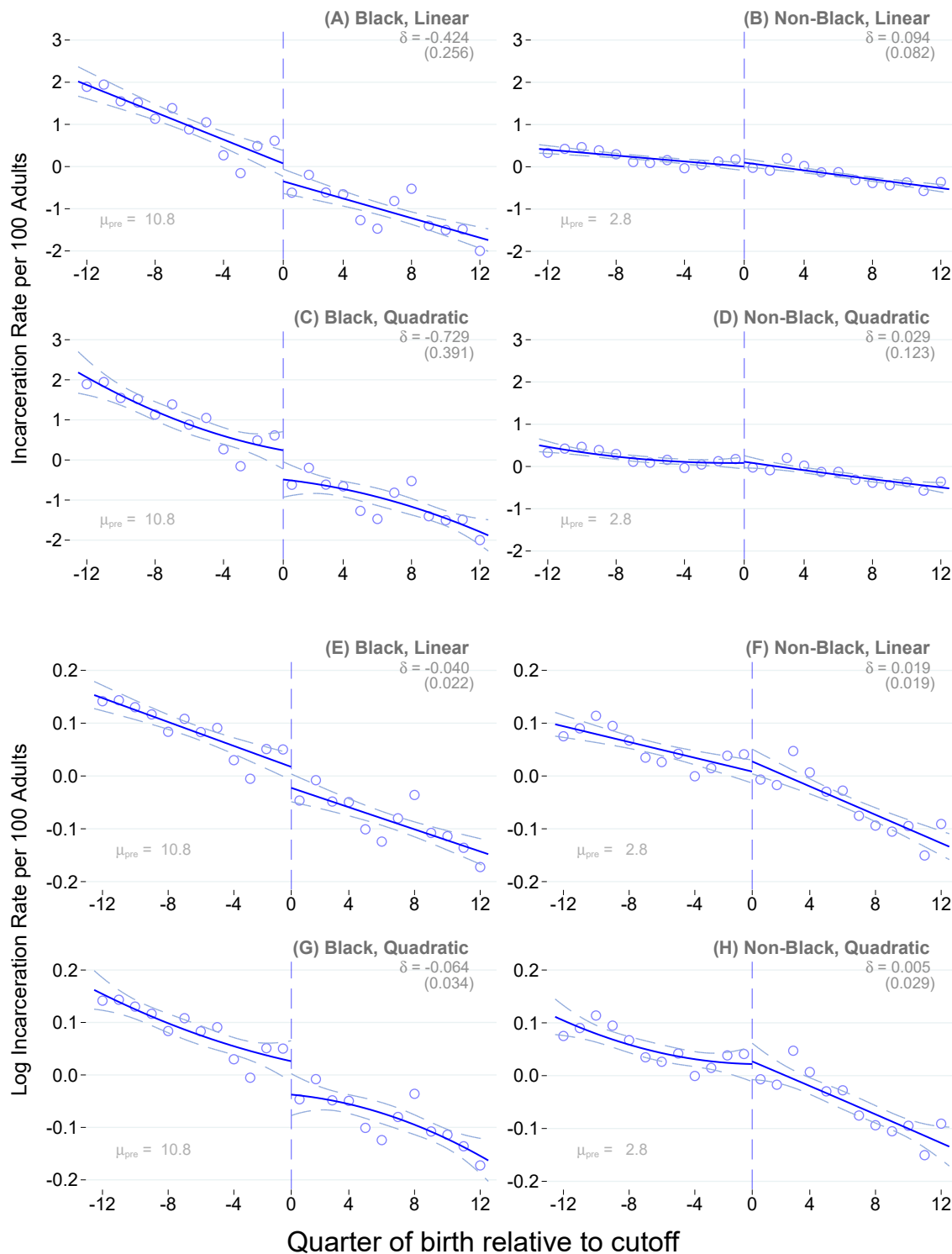
Notes: The purpose of this figure is to display the results of our analysis when using rate-based—rather than count-based—measures of incarceration. See Appendix A for further discussion of rate construction and justification for count-based measures as our preferred outcome. The first row (Panels A and B) details results using level rates of ever-incarcerated individuals for each DOB cohort, rather than log counts as presented in Figure 2. The second row (Panels C and D) performs the same analysis while using a quadratic specification. Finally, the last two rows (Panels E through G) present the log versions of Panels A through D, respectively. See Figure 2 for a general description of the regression discontinuity plots.

Source: Author calculations using Florida DOC Incarceration Data.

Figure A9 – Additional Results: Effects on Incarcerations by Age
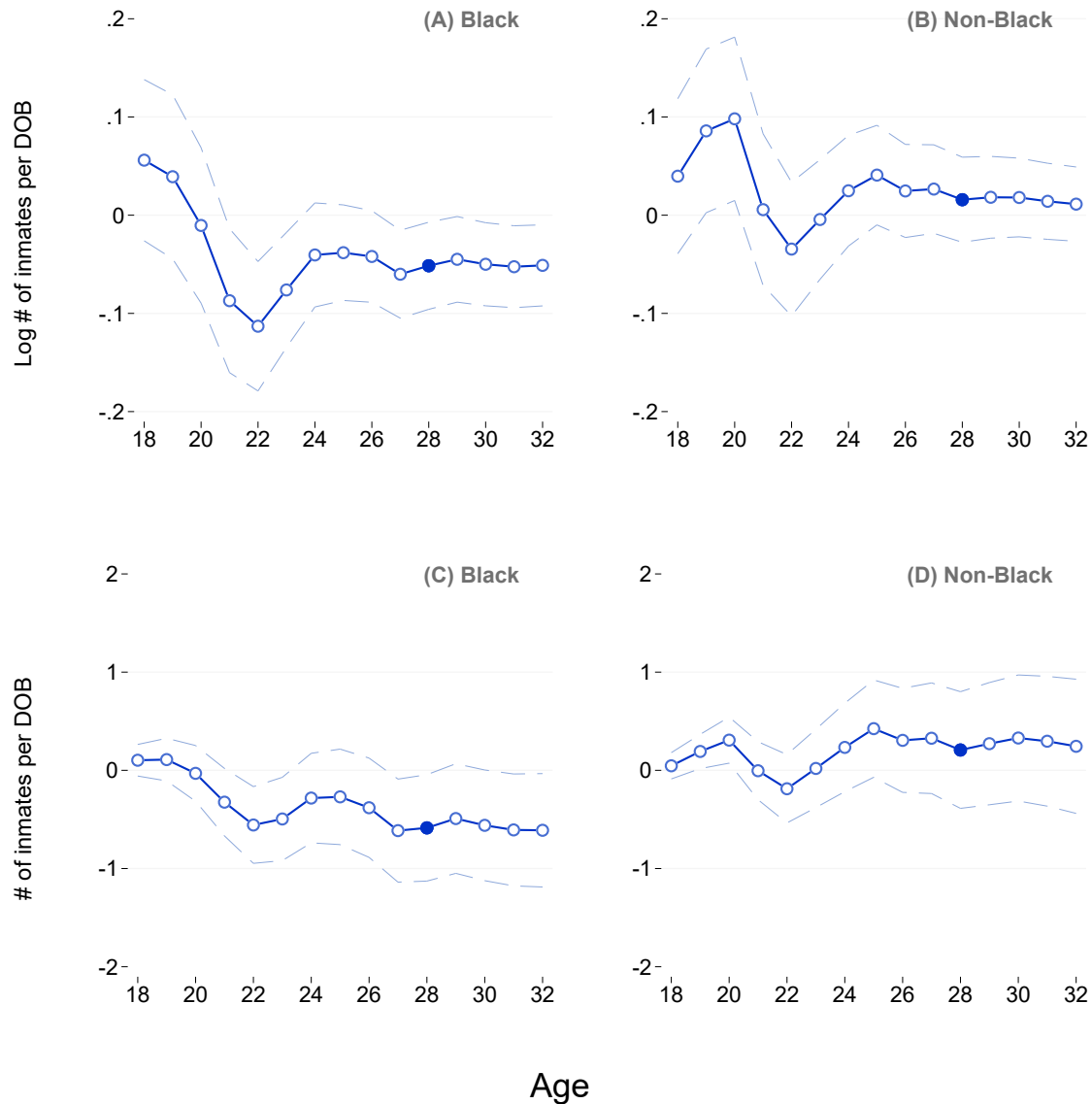
Notes: The purpose of this figure is to display the results of Equation 1 for outcomes at varying ages. Panels A and B displays estimates of the reduction in log incarcerations (our main outcome) at various ages, while Panels C and D detail estimates when using counts rather than logs. Each dot represents the estimated coefficient δ from a separate regression (our primary estimate is shaded dark blue). Dashed lines indicate 90 percent confidence intervals. See also Figure A10 for descriptive statistics on total individuals ever incarcerated by age.

Source: Author calculations using Florida DOC Incarceration Data.

Figure A10 – Descriptive Results: Cumulative Individuals Ever Incarcerated by Age
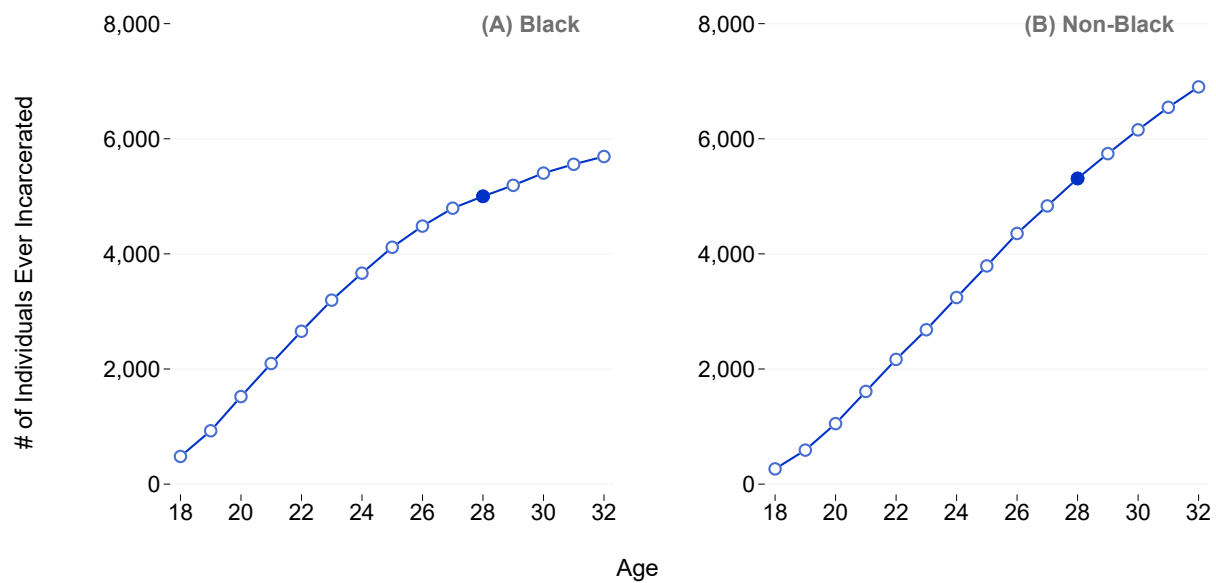
Notes: The purpose of this figure is to display the number of individuals who have ever been incarcerated as of a given age. The sample utilizes the birth cohorts in the year prior to the OBRA90 Expansion Cutoff. These statistics are useful for interpreting coefficients displayed in Figure A9.

Source: Author calculations using Florida DOC Incarceration Data.

Figure A11 – Robustness: Treatment Effects by Bandwidth (First Stage)

Bandwidth (as % of Original)

Notes: The purpose of this figure is to display the results of Equation 1 for varying bandwidths. Panels A and B display increases in coverage due to the Expansion at various bandwidths, while Panels C and D detail these estimates when a quadratic (rather than linear) fit. Each dot represents the estimated coefficient δ from a separate regression (our primary estimate is shaded dark blue). Dashed lines indicate 90 percent confidence intervals.

Source: Author calculations using NHIS Data.

Figure A12 – Robustness: Treatment Effects by Bandwidth (Main Analysis)
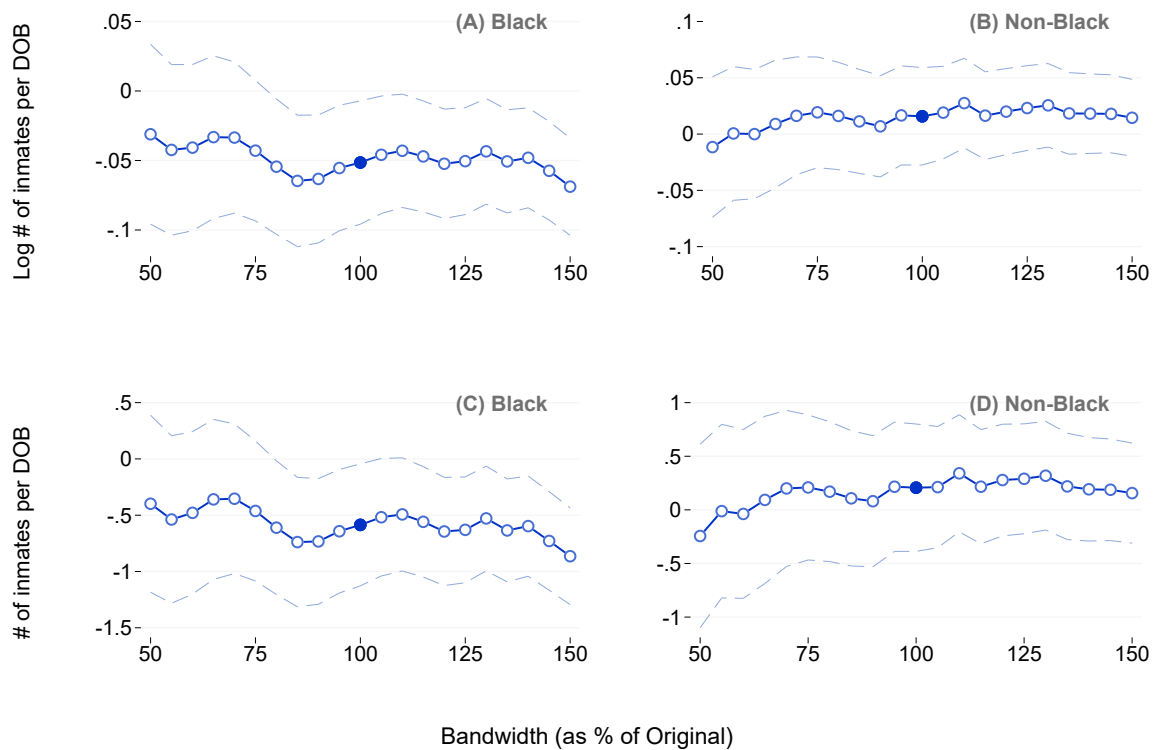

Bandwidth (as % of Original)

Notes: The purpose of this figure is to display the results of Equation 1 for varying bandwidths. Panels A and B display estimates of the reduction in log incarcerations (our main outcome) at various bandwidths, while Panels C and D detail estimates when using counts rather than logs. Each dot represents the estimated coefficient δ from a separate regression (our primary estimate is shaded dark blue). Dashed lines indicate 90 percent confidence intervals.

Source: Author calculations using Florida DOC Incarceration Data.

Figure A13 – Robustness: Treatment Effects for Other September 30th Cutoffs

Day of Birth Relative to September 30th
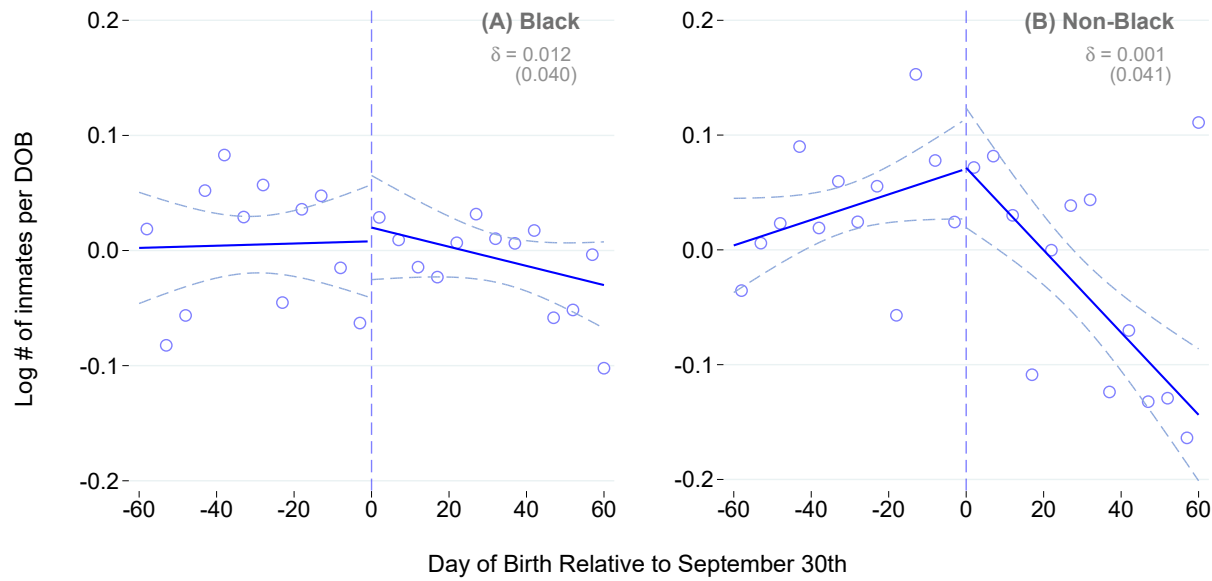
Notes: The purpose of this figure is to display the results a regression discontinuity analysis of all September 30th cutoffs during 1980-1982 and 1984-1986 (i.e., the years included in our bandwidth *excluding* the treated year of 1983). Each dot represents the average of the outcome variable (the log count of inmates ever incarcerated) in 5-day bins. The lines presented are generated from linear regressions with associated 90 percent confidence intervals (displayed using dashes). The estimated coefficients, δ, and associated standard errors are generated from the following equation:

$$Y_c = \alpha + \delta \cdot Post_c + f(DOB_c) + \varepsilon_c$$

where $Post_c$ is defined as being born in the 60 days *after* the September 30th cutoff. The bandwidth of 60 days was chosen as that is in-line with other calendar-date cutoff literature, such as the school entry date analysis performed in Cook and Kang (2016). Standard errors are clustered by day of birth.

Source: Author calculations using Florida DOC Incarceration Data.

Figure A14 – Robustness: Treatment Effects by Specification Choice
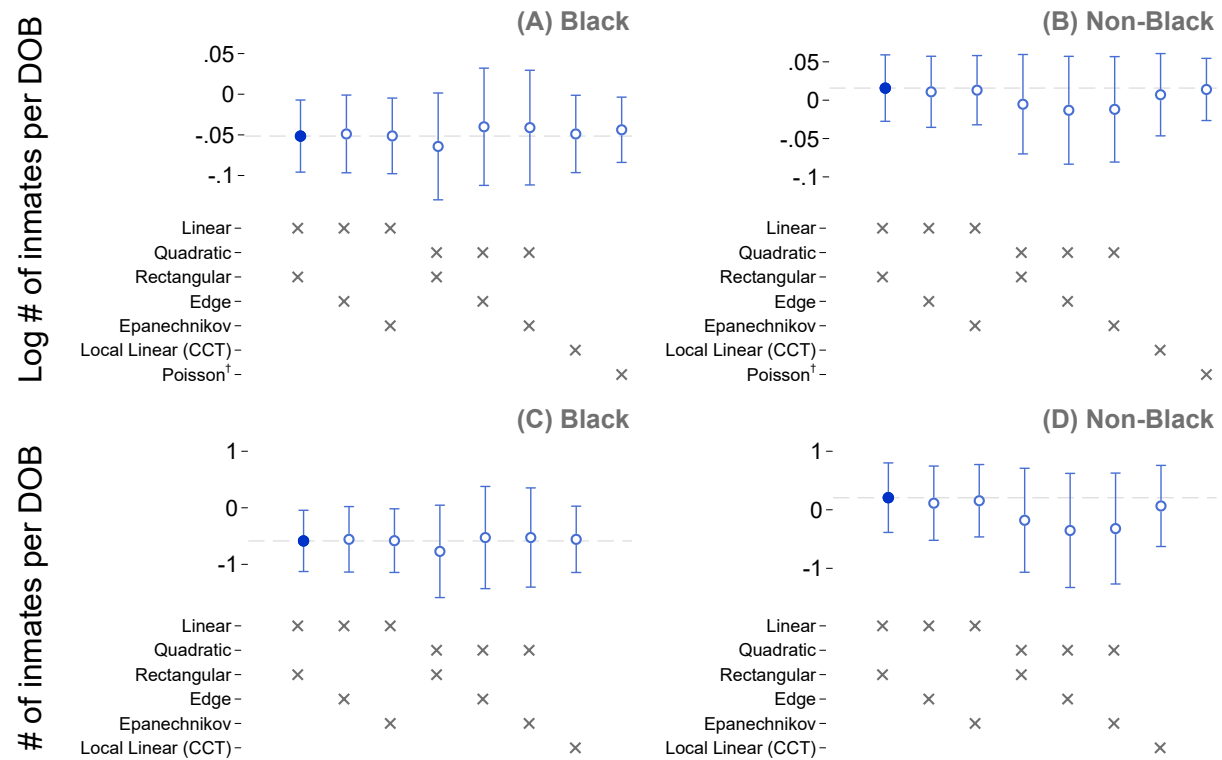
Notes: The purpose of this figure is to display the results of Equation 1 for varying specifications. Panels A and B display estimates of the reduction in log incarcerations (our main outcome), while Panels C and D detail estimates when using counts rather than logs. Each dot represents the estimated coefficient δ from a separate regression (our primary estimate is shaded dark blue), with bars indicating 90% confidence intervals. Dashed lines indicate the main estimate for reference. The bottom half of each panel indicates specification choices associated with each estimate. "Linear" or "Quadratic" indicates the polynomial choice, "Rectangular," "Edge," or "Epanechnikov" indicates the choice of kernel-weighting. "Local Linear (CCT)" indicate local-linear edge-weighted regressions using the Calonico et al. (2014) data-driven bandwidth selector, while "Poisson" indicates estimates from a Poisson regression specification.

†Note that while the Poisson specification includes counts as the outcome variable, it is presented alongside the estimates using logs, since the interpretation of Poisson coefficients is most comparable to log-specification estimates.

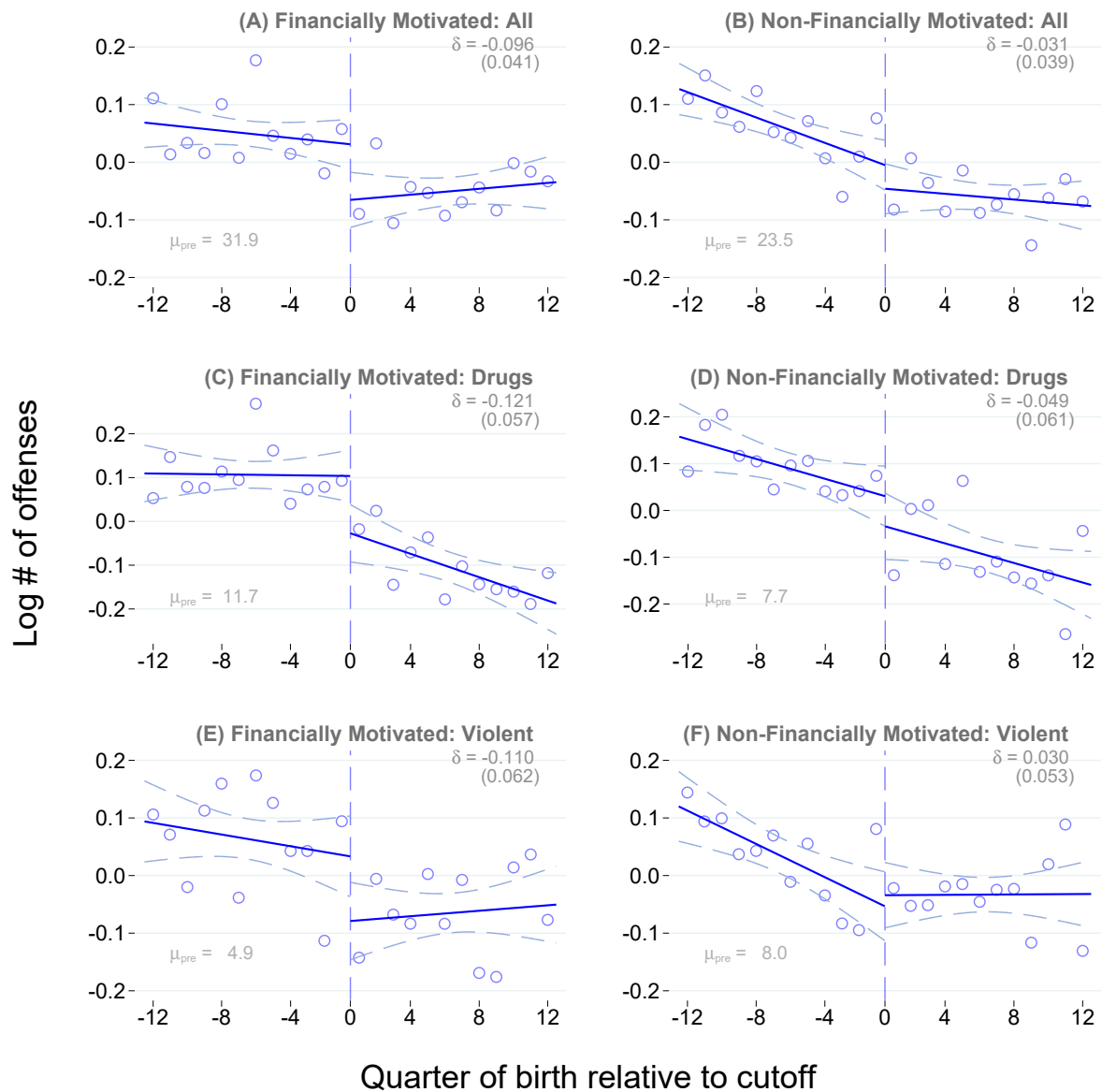*Source:* Author calculations using Florida DOC Incarceration Data.

Figure A15 – Impact of the OBRA90 Expansion on Adult Incarceration
by Other Offender Types (Black)
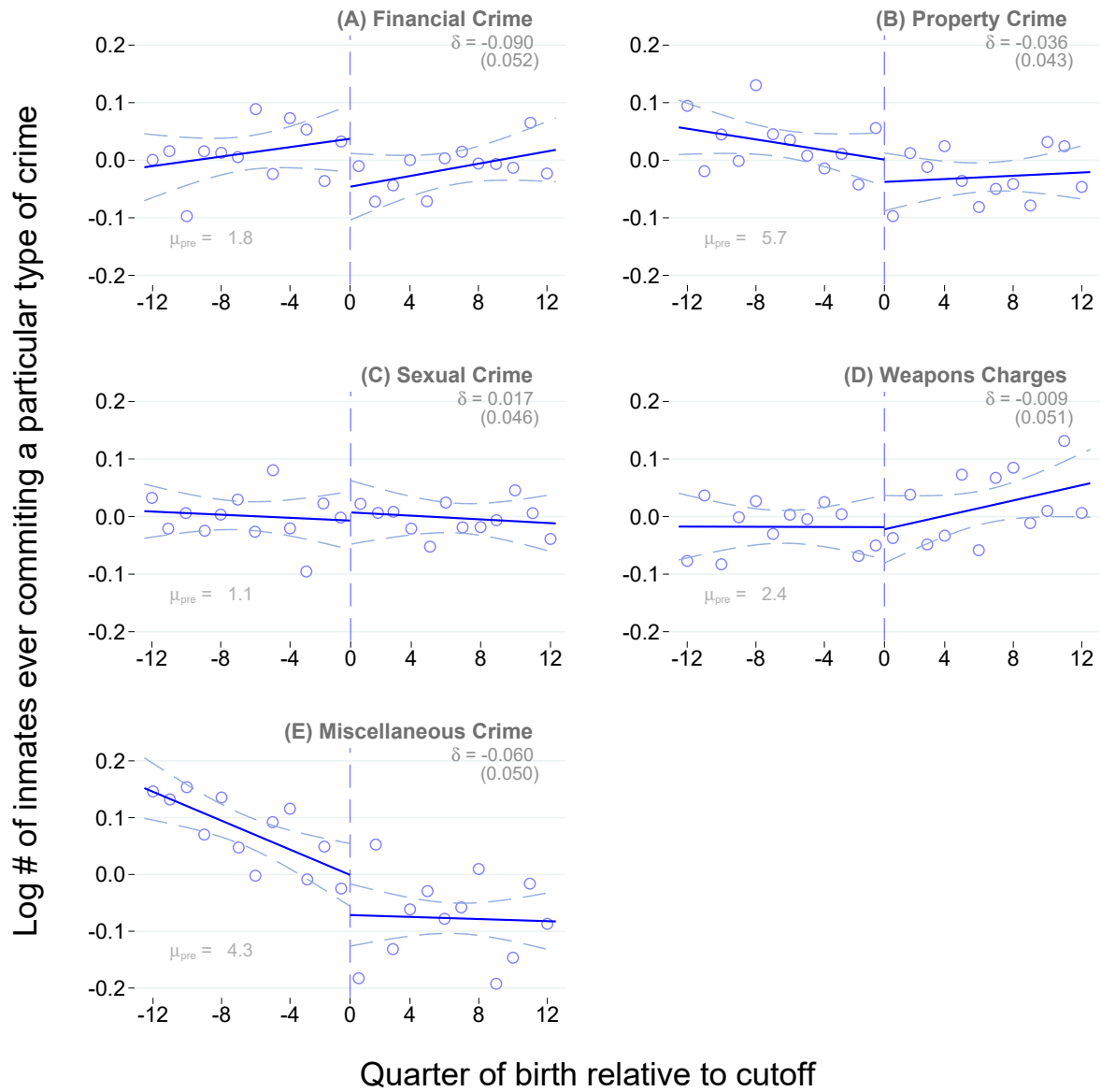
Notes: The purpose of this figure is to display the results of our heterogeneity analysis by type of crime. See the notes to Figure 6, which describes heterogeneity by financially and non-financially motivated offenses, for more detail.

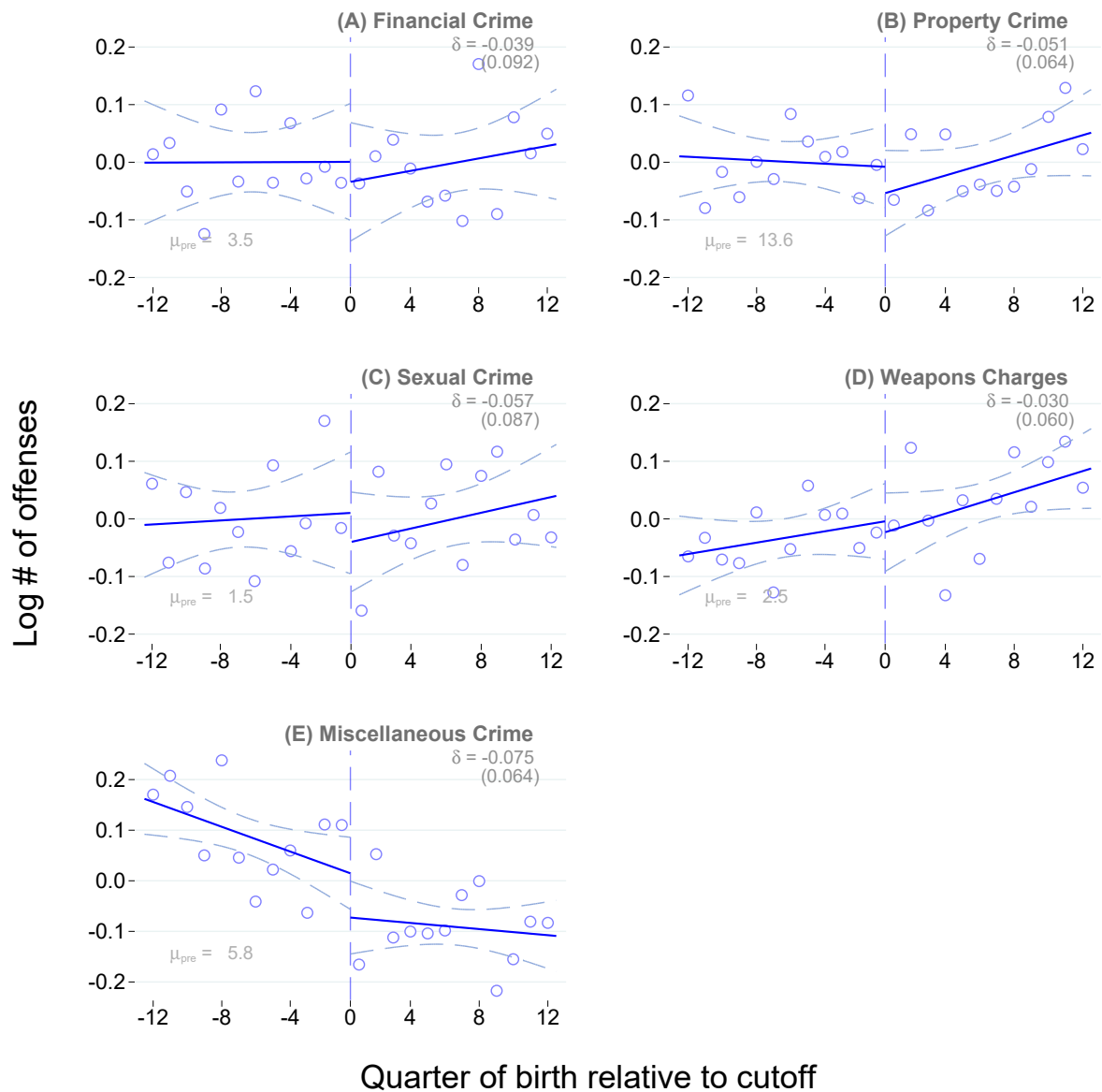*Source:* Author calculations using Florida DOC Incarceration Data.

Figure A16 – Impact of the OBRA90 Expansion on Offenses Committed
by Crime Type (Black, Offense-Level)

Notes: This figure replicates the analysis of Figure 6 on the offense level (rather than inmate level). Each panel represents log counts of offenses of a particular type committed by each daily birth cohort. See Figure 6 for more detail.

Source: Author calculations using Florida DOC Incarceration Data.

Figure A17 – Impact of the OBRA90 Expansion on Offenses Committed
by Other Crime Types (Black, Offense-Level)

Notes: This figure replicates the analysis of Appendix Figure A15 on the offense level (rather than inmate level). Each panel represents log counts of offenses of a particular type committed by each daily birth cohort. See Figure 6 and Appendix Figure A15 for more detail.

*Source:* Author calculations using Florida DOC Incarceration Data.

Figure A18 – Heterogeneity by Poverty of Release Zip Code (Non-Black)
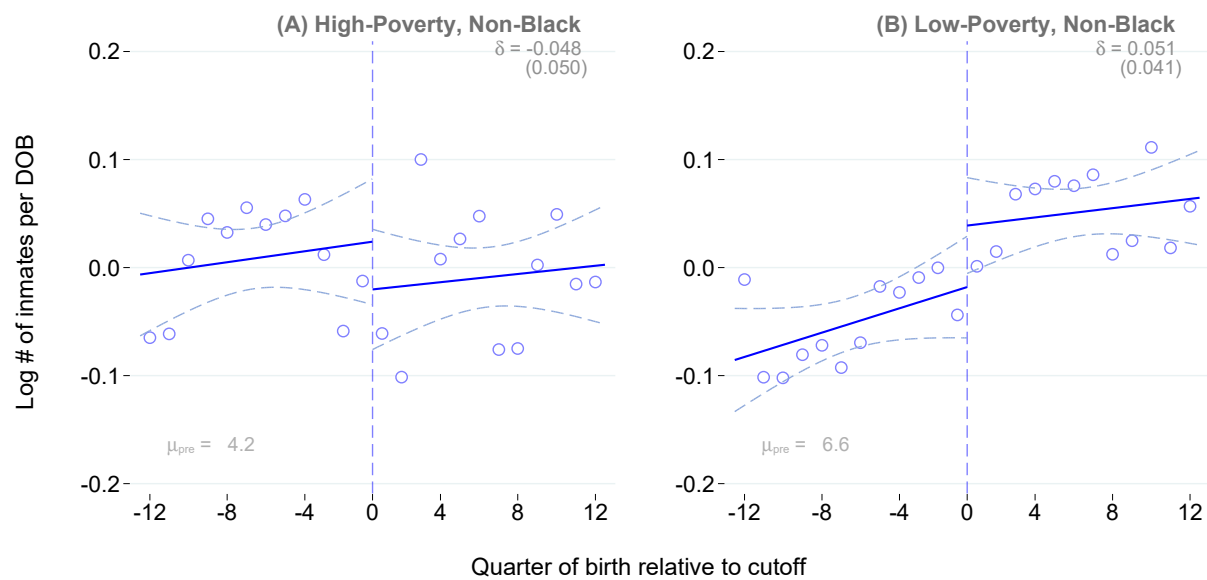
Notes: The purpose of this figure is to display the results of our heterogeneity analysis by poverty rates of the zip codes to which inmates were released. Each panel represents log counts of individuals in each daily birth cohort that have ever been incarcerated for a different sub-sample. Panels A and B focus on Non-Black inmates who were released into relatively high and low-poverty zip codes, respectively. See Section 5 for additional detail on what constitutes high and low-poverty zip codes. Note that means displayed in the bottom-left corners of each panel do not sum up to those in Figure 2 because this analysis includes a sub-sample of offenders who have been released from prison.

Source: Author calculations using Florida DOC Incarceration and 2007-2011 American Community Survey Data (Manson et al., 2019).

Figure A19 – Heterogeneity by Counties with Steep and Shallow
Income-Incarceration Slopes (Black)
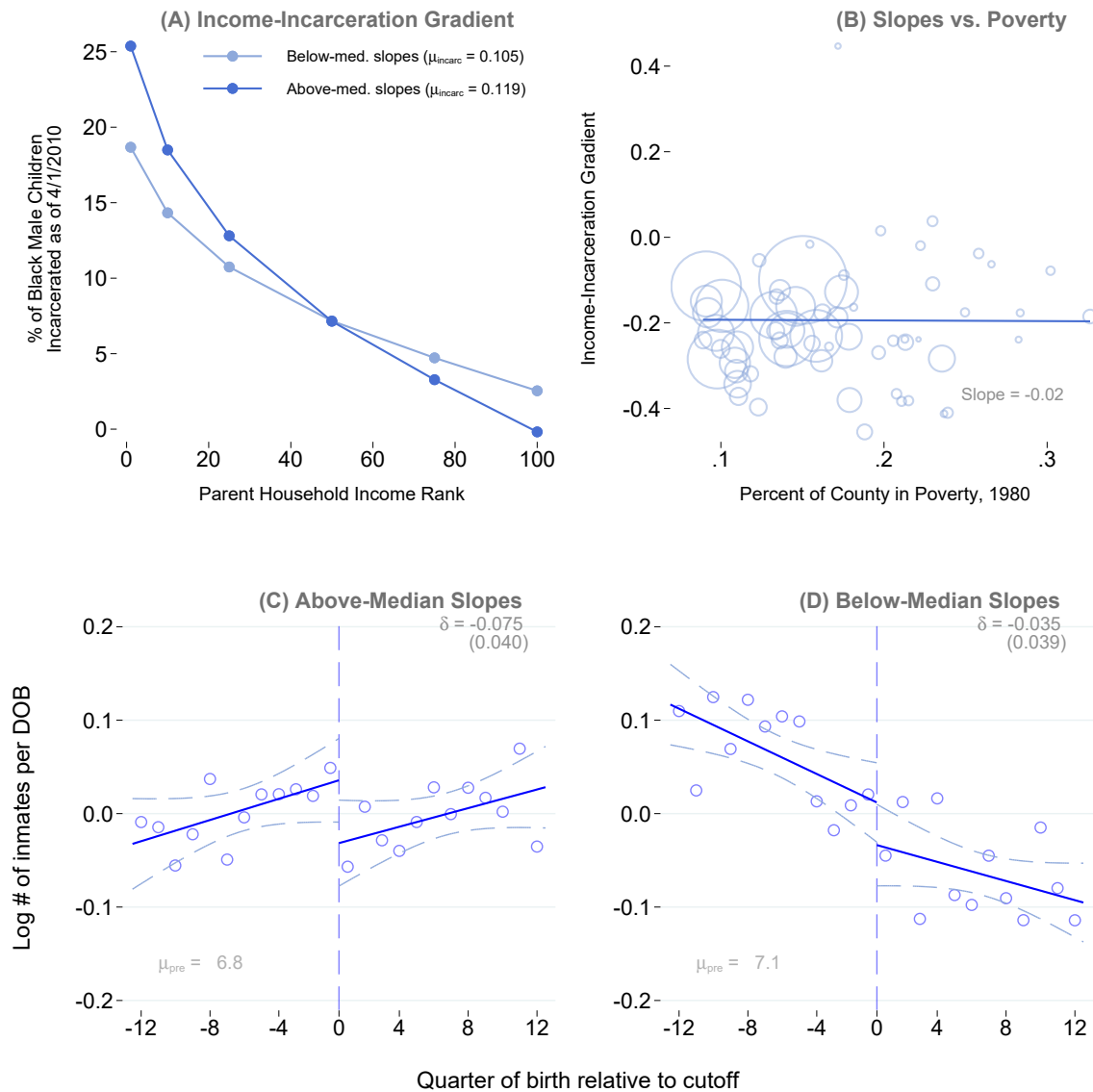
Notes: The purpose of this figure is to display the results of our heterogeneity analysis by counties with high and low income-incarceration gradients (See Appendix Section D for further discussion). Panel (A) illustrates the difference in slopes between above-median-slope and below-median-slope counties, along with the mean incarceration rate for Black male children in each group. Panel (B) illustrates that these slopes are not correlated with poverty rates in 1980 (the Census Year closest to the birth years of cohorts that we study). Panels (C) and (D) display regression discontinuity plots for inmates from above-median (steep-slope) and below-median (shallow-slope) counties, respectively. See Figure 2 for a general description of the regression discontinuity plots.

Source: Author calculations using Florida DOC Incarceration Data, Opportunity Atlas Data (Chetty et al., 2018), and Decennial Census Data (Manson et al., 2019).
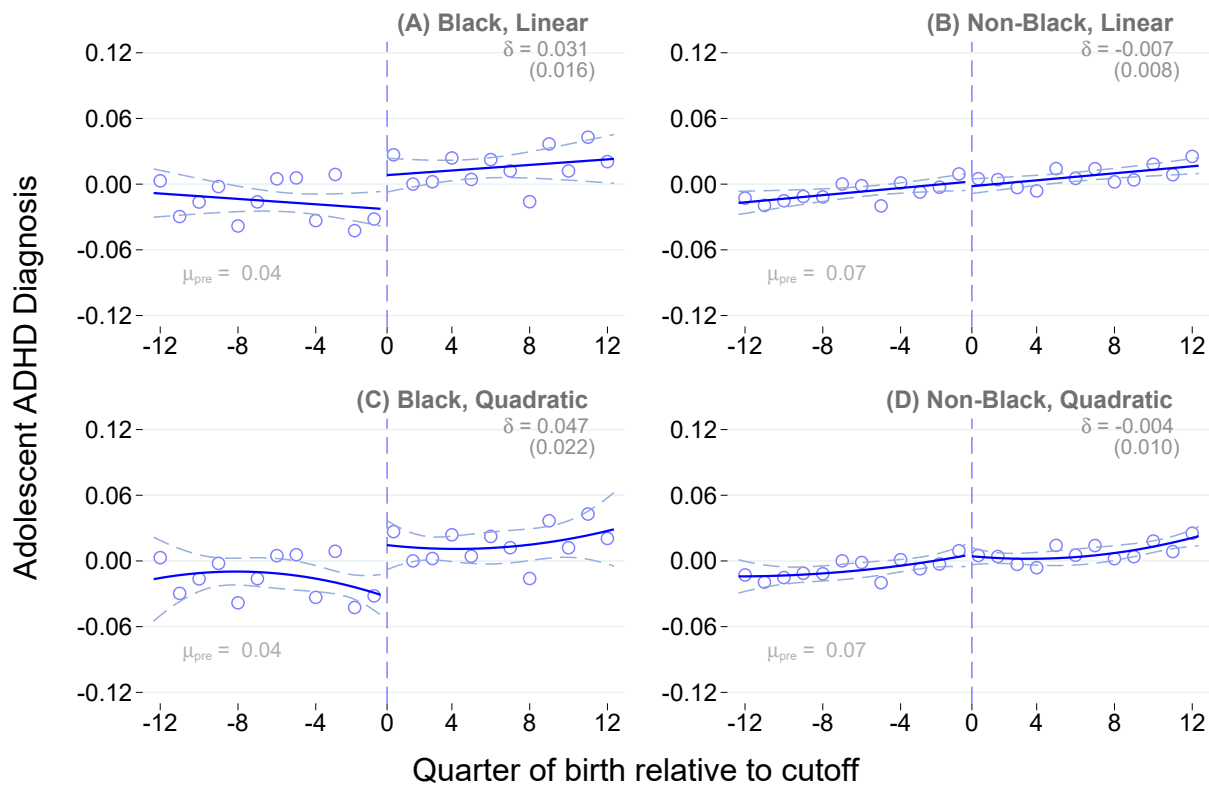
Figure A20 – Impact of the OBRA90 Expansion on Adult Mental Health (NHIS)

Quarter of birth relative to cutoff
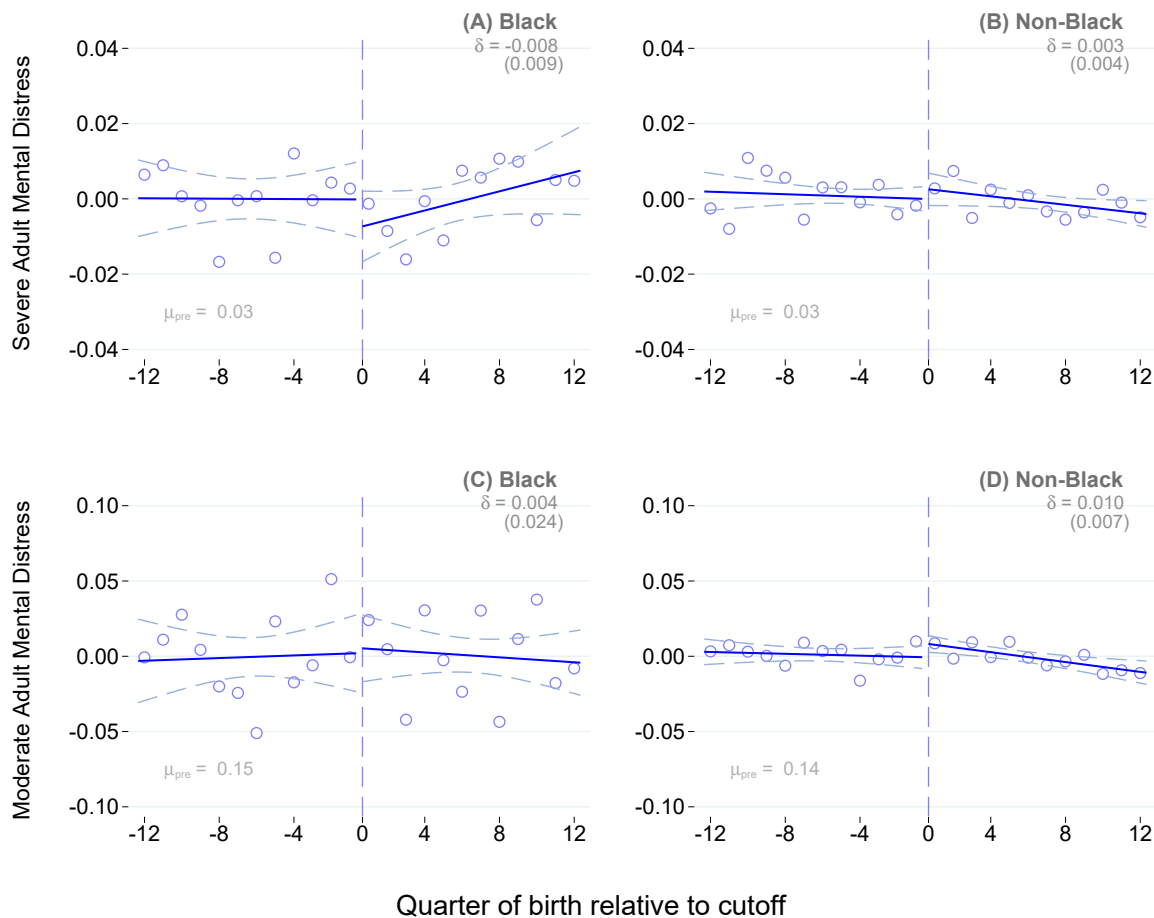
Notes: The purpose of this figure is to display the impact of the OBRA90 expansion on adult mental health. The outcome measures, severe mental distress (Panels A and B) and moderate mental distress (Panels C and D) were constructed using cutoff values of the Kessler K6 scale (Prochaska et al., 2012), which we constructed from the standard mental health questions asked by the NHIS. The co-efficients of interest, δ, are generated from a modified version of Equation 1, with the year-month of birth as the running variable. These coefficients and associated standard errors (clustered at the year-month level) are displayed in the upper-right corner. More detail on the structure of the regression discontinuity plots is detailed in the notes for Figure 2. Figures utilize 4,696 and 23,866 observations for Black and Non-Black samples, respectively.

Source: Author calculations using the National Health Interview Surveys (Blewett et al., 2019).

Figure A21 – Impact of the OBRA90 Expansion on ADHD Diagnoses (NHIS, Alt. Specifications)

Notes: The purpose of this figure is to display results of Figure 7 with differing specifications. Panels A and B recreate the analysis of Figure 7, while Panels C and D instead use a quadratic specification. See Figure 7 for more detail. Figures utilize 3,237 and 16,964 observations for Black and Non-Black samples, respectively.

Source: Author calculations using the National Health Interview Surveys (Blewett et al., 2019).

Figure A22 – Additional Results: Impact of the OBRA90 Expansion
on Years Incarcerated and Log Years Sentenced


Quarter of birth relative to cutoff
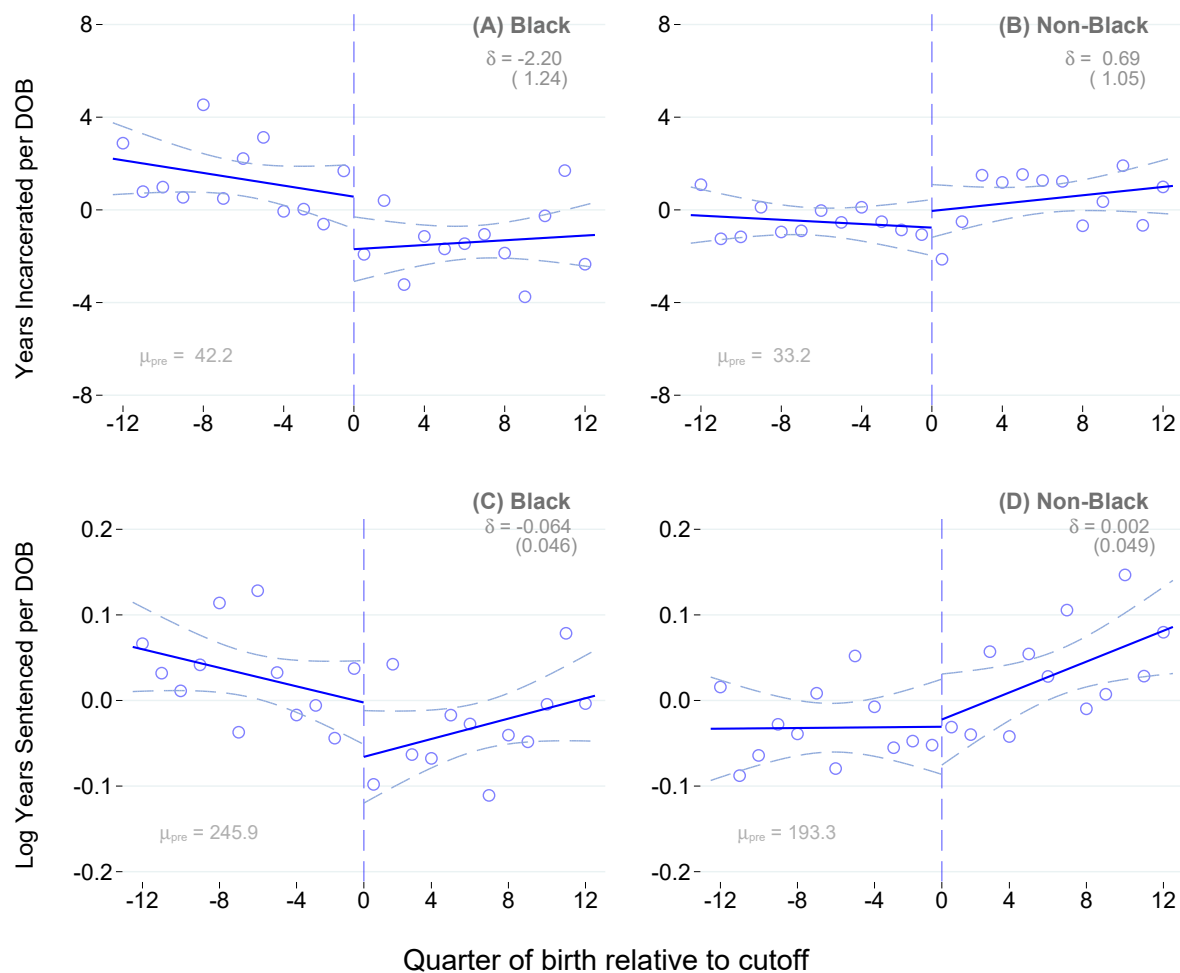
Notes: The purpose of this figure is to display the results of our analysis when using additional outcomes. All left-hand columns present results for Black inmates, while right-hand columns present results for Non-Black inmates. The first row (Panels A and B) details results using counts years incarcerated for each DOB cohort, rather than log counts as presented in Figure 8. The second row (Panels C and D) displays the policy's impact on log years sentenced, an alternate measure that captures both the extensive and intensive-margin responses. As in the main text, all outcomes are measured as of age 28. See Figure 2 for a general description of the regression discontinuity plots.

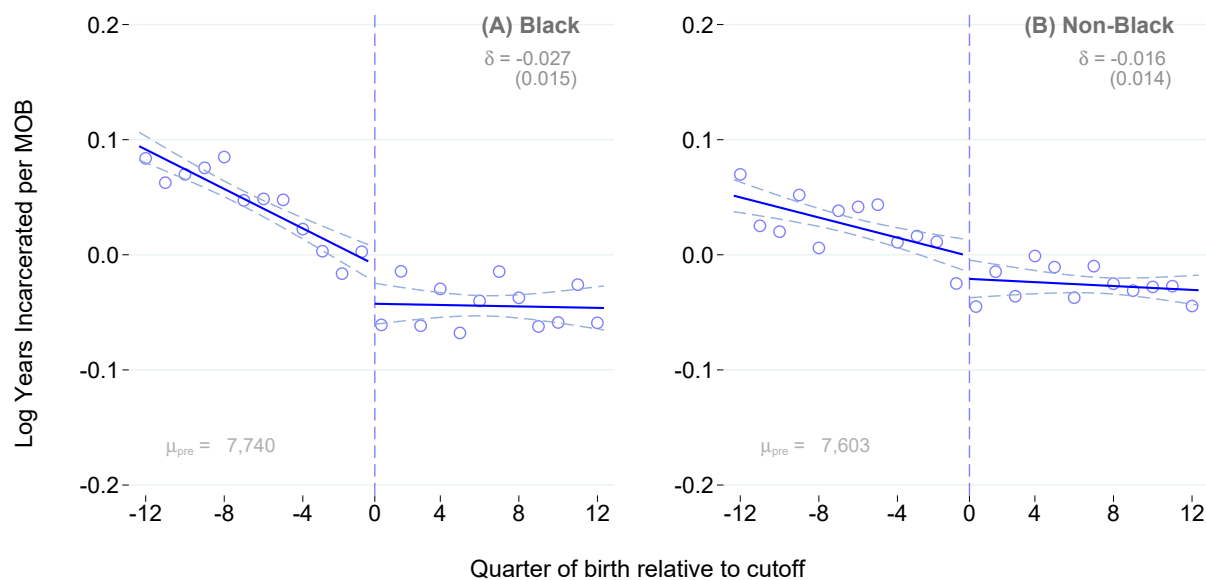*Source:* Author calculations using Florida DOC Incarceration Data.

Figure A23 – Additional Results: Effects on Years Incarcerated by Age
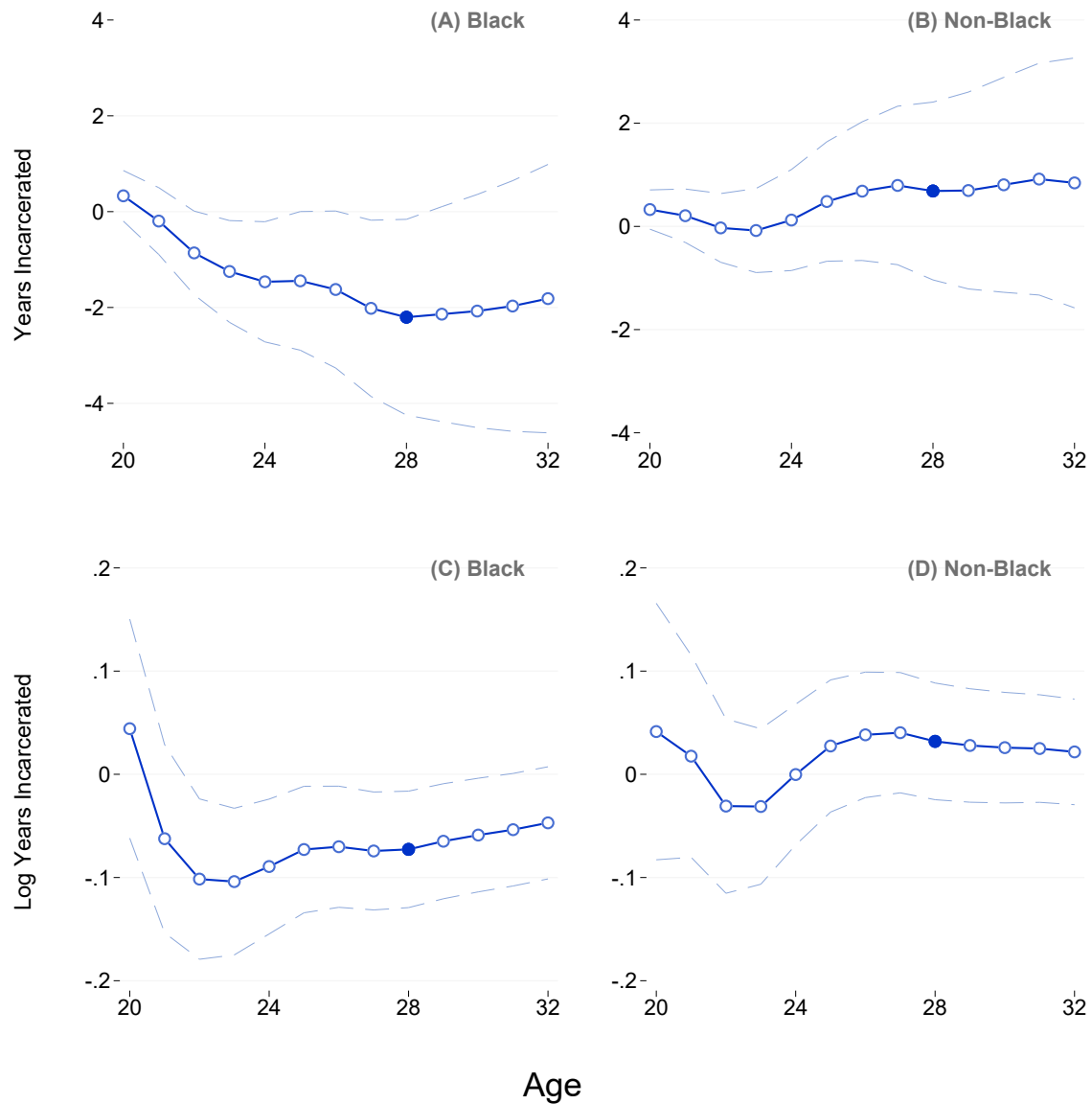
Notes: The purpose of this figure is to display the results of Equation 1 for outcomes measured at varying ages. Panels A and B display estimates for the level number of cumulative years incarcerated at various ages, while Panels C and D detail estimates when using logs rather than levels. Each dot represents the estimated coefficient δ from a separate regression (our primary estimate is shaded dark blue). Dashed lines indicate 90 percent confidence intervals.

*Source:* Author calculations using Florida DOC Incarceration Data.

Figure A24 – External Validity: Impact of the OBRA90 Expansion
on Years Incarcerated (NCRP Data)

Notes: The purpose of this figure is to display the results of our analysis on years incarcerated using the National Corrections Reporting Program Data from 2000-2016. The coefficients of interest, δ, are generated from a modified version of Equation 1, with the year-month of birth as the running variable. These coefficients and associated standard errors (clustered at the year-month level) are displayed in the upper-right corner. Pre-cutoff means of the *level* count of years incarcerated (μ_{pre}) are in the presented bottom left. See more detail on the structure of the regression discontinuity plots in Figure 2.

Source: Author calculations using the 2000-2016 Restricted-Use National Corrections Reporting Program Data (Bureau of Justice Statistics, 2019).

Figure A25 – Robustness: Inference When Using Coarse Running Variables
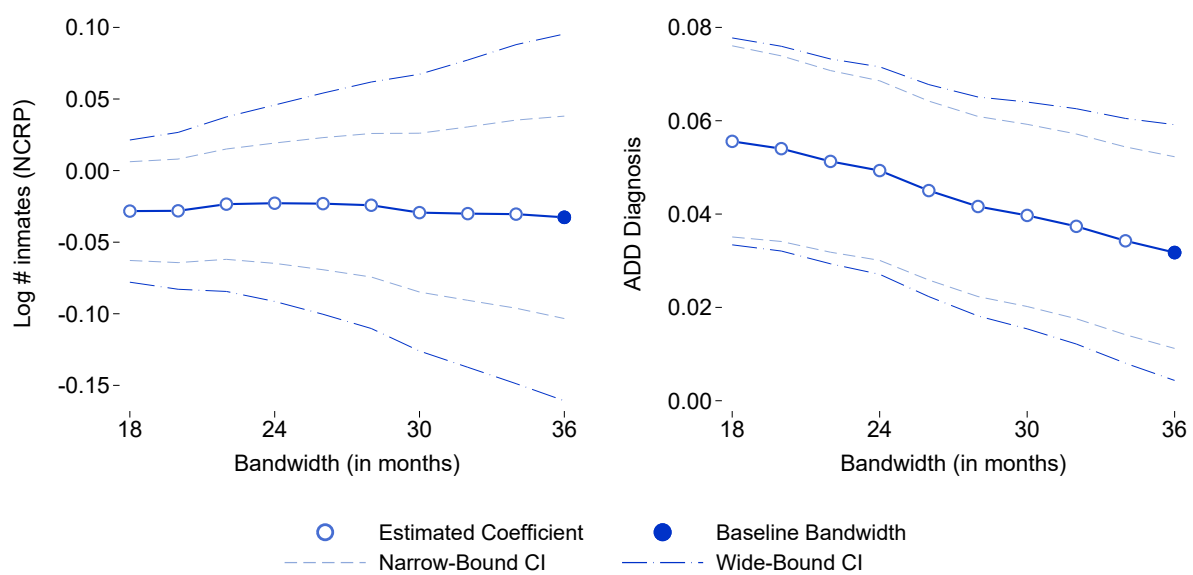
Notes: The purpose of this figure is to display estimates and confidence intervals calculated using methods developed by Kolesár and Rothe (2018) for regression discontinuity designs with discrete running variables. The *y*-axis represents coefficient estimates associated with the bandwidth choices on the *x*-axis. Short and long-dashed lines represent confidence intervals using the narrow and wide bounds discussed in further detail in Appendix Section C.

Source: Author calculations using the 2000-2016 Restricted-Use National Corrections Reporting Program Data (Bureau of Justice Statistics, 2019) and the National Health Interview Surveys (Blewett et al., 2019).