Appendix for Online Publication

Arenberg, Neller, and Stripling (2023)

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 (National Vital Statistics System, 2019). 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 $\log(Y_c)$). We then estimate a version of Equation 1 that includes fixed effects that account for the day-of-year, and day-of-week of birth, as well as whether the birthday was on a holiday fixed effects. This is in lieu of the calendar-month-of-birth fixed effects in our main specification and are included to increase precision.[2] The results of this analysis, which are displayed in Figure A9, are highly consistent with our main results. There is a discontinuity of 0.397 fewer incarcerated Black adults per 100 in the base specification (Panel A), which increases to a reduction of 0.564 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 4.4 percent (linear) or 6.2 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

[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.

[2] Estimates when using calendar-month-of-birth fixed effects are highly similar, but are less precisely estimated. This is expected because we are introducing measurement error into our outcome variable through the use of mismeasured denominators. The increased granularity of the fixed effects offsets this by absorbing further variation.

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.

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."[3] 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.[4] 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 A27) 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.

[3] We do not use the files pertaining to post-confinement community supervision (e.g., parole).

[4] Each inmate has an identifier that is consistent with state but not across states (in the event of incarcerations in multiple 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 take-up rate (40.7%), since not all eligible children actually participated in the program.[5] This yields an estimate of 76,740 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 $143.6 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 generally 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.

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.[6] 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 ranging from one to eight. While a tuning parameter of eight is suggested by Kolesár and Rothe (2018) and a parameter of four is chosen by Goldsmith-Pinkham et al. (2020), it is possible that the appropriate parameter for our analysis is smaller due to the relative granularity of our running variable (which is

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

[6] 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.

monthly, as opposed to yearly data referenced in both papers).

The results of our estimation using these techniques are presented in Figure A28, 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 confidence intervals change as the tuning parameter changes. The NCRP-related analysis only achieves traditional levels statistical significance when the smallest parameter ($K = 1$) presented, with the most precise estimate yielding a 95% confidence interval ranging from -0.064 to -0.001. In contrast, the NHIS analysis on ADHD achieves statistical significance at a 95% level or greater for all K-parameters selected.

D Additional Results and Supporting Evidence

D.1 Additional Results: Hispanic

Our main analysis compares insurance and incarceration outcomes between Black and Non-Black individuals. The latter category includes two sizable groups in Florida: Non-Hispanic whites and white Hispanics. In this appendix section, we show outcomes specifically for white Hispanics and conclude that they are similar to those for Non-Hispanic whites, justifying our choice to collapse them into a single category.

There is, however, a challenge to conducting this analysis with the data at hand. The Florida incarceration data has a single race field, which predominantly has values of *either* "White," "Black," or "Hispanic" (i.e., there is not a separate field for ethnicity). In our sample, only about 4% of observations are identified as Hispanic, a curiously low fraction given that approximately 16% of Floridian children born during our bandwidth were Hispanic (per the Current Population Survey). To avoid relying too heavily on this variable, we incorporated data from the Census Bureau on the racial and ethnic makeup of surnames and first names to identify *likely* Hispanics. Specifically, we flagged an individual as "likely Hispanic" if either their first or surname was at least 50% Hispanic or they explicitly reported Hispanic status in the FL DOC data. This method resulted in 13.5% of observations being identified as likely Hispanic.

Before moving to our analysis, it is worth noting that, due to the high fraction of Floridian Hispanics who are of Cuban (and other Caribbean) heritage, Hispanics in Florida are socioeconomically quite dissimilar to Hispanics in other Southern states and are therefore *less* likely to experience large eligibility gains from the Expansion. This point is shown by Panel A of Appendix Figure A8, which demonstrates that, while Hispanic whites in the Southern Region and Nationally obtained eligibility gains that were similar to Black children, Floridian Hispanics had eligibility gains closer to the state's Non-Hispanic white population. Further, we find that, even conditional on income, Floridian white Hispanics have similar Medicaid coverage rates as White Non-Hispanics, both of which are much lower than Black children (Panel B). This suggests that the additional Medicaid eligibility made available by the Expansion was taken up at low rates for Hispanic whites, similar to Non-Hispanic whites.[7]

Because eligibility gains and coverage decisions of Floridian Hispanics mirror those of Floridian Non-Hispanics, we expect the impact of the OBRA90 Expansion on later-life incarceration for this group to be small. This expectation is confirmed by Panel C of Appendix Figure A8: we find a reduction of 1.5% in the number of incarcerated Hispanics. This estimate is, however, imprecise ($p = 0.757$). In fact, the 95% confidence intervals are wide to the extent that they subsume the confidence intervals for both the Black and Non-Black results (Figure 2).

[7] Note that, because Floridian Hispanics differ so sharply from Hispanics in other Southern States, a regression discontinuity analysis in the spirit of Figure 1 while restricting to Southern geography would not be informative about this sub-population.

D.2 Additional Supporting Evidence: Construction of Indices for Mental/Behavioral Health, Health Behaviors, and Physical Health

In order to evaluate the effects of the OBRA90 Medicaid Expansion on the detection and improvement of long-run health outcomes, we gathered a large number of variables from the National Health Interview Survey ("NHIS") from 1997 through 2014.[8] In order to obtain a holistic view of the Expansion's impact on self-reported health, we group these variables into three categories: those related to (i) mental and behavioral health, (ii) risky health behaviors, and (iii) physical health. We then combine these variables into indices via the following method:

1. We collapse variables to the race-cohort level;
2. Create within-race z-scores for each variable;
3. Code variables so that "detection" of ailments (e.g., "have you ever been diagnosed with ADHD") resulted in positive values, while *current* conditions (e.g., "have you had a cold in the last two weeks") are coded as negative values;[9]
4. We then combine these variables into an index via a simple mean of the z-scored variables.

The regression discontinuity plots for these indices (restricted to Black individuals) are presented in Appendix Figure A21. In addition to presenting the "full" indices (in Panels A, C, and D), we also present an index for mental/behavioral health that *excludes* both attention deficit and hyperactivity disorder ("ADHD") diagnoses (discussed at length in the main text), as well as whether an individual was homeless (because, based on the text of the underlying survey question, that variable potentially also includes incarceration, the main outcome of this paper). We find that OBRA90 had meaningful effects on both Mental/Behavioral Index (35.3% of a standard deviation) and Risky Behaviors Index (37.6%-SD). We do not, however, detect any improvements in our index of physical outcomes—our point estimate of -1.9%-SD is not statistically different from zero, and the upper end of the 95% confidence interval yields a value that is substantially lower (14.4%-SD) than the effects on the other two indices.

Appendix Figure A22 provides further insight into the estimates of these index values. Within the Mental/Behavior Health Index, we find a strong increase in the detection of ADHD, as discussed in the main text. We further find imprecisely estimated increases in the detection of developmental delay and receipt of special education services, alongside improvements in cognitive function and mental distress. Because these individual outcomes are imprecisely estimated, we caution against placing substantial weight on any individual outcome; however, the positive correlation across outcomes—and the fact that all nine index components move in the "expected" direction—does provide suggestive evidence that the Expansion may be improving mental/behavioral health in areas beyond ADHD. Additionally, the components of the Risky Behaviors Index indicate lower likelihood of smoking and performing activities with risk of HIV infection—which includes, but is not limited to, intravenous drug use. This suggests that increased healthcare resources

[8] We choose 1997 to begin the period as it is the first year *after* differential coverage for our treated cohorts ended due to the enacting of CHIP; additionally, it is the first year after a major re-design of the NHIS. The last year of 2014 was chosen as it is the most recent public survey with year and month available.

[9] The rationale for this is that health insurance benefits patients by increasing detection of underlying conditions (hence detection of ailments is assigned a positive value), but should in theory reduce current conditions through treatment (hence conditions are assigned negative values). Risky health behaviors were coded so that the absence of such behaviors are reflected as positive values. Thus, a positive change in the index at the cutoff reflects an increase in "good" behaviors.

during late childhood and early teenage years can lead to a reduction in risky behavior.[10] In contrast, because we do not detect meaningful or compelling effects of improved physical health, this suggests that a primary channel for the results that we find is through improved mental/behavioral health and reduced risk-taking.

D.3 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.[11] [12] 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.[13] 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).[14] 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

[10] Again, a potential channel for these results is diagnosis and treatment of ADHD, which has been associated with higher rates of smoking and drug use.

[11] 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. (2019a), which we inflate to 2019 dollars. Thus, the calculation is: \$1,800 per year $\times$ 6 years of eligibility among the eligible $\times$ \$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.

[12] 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.

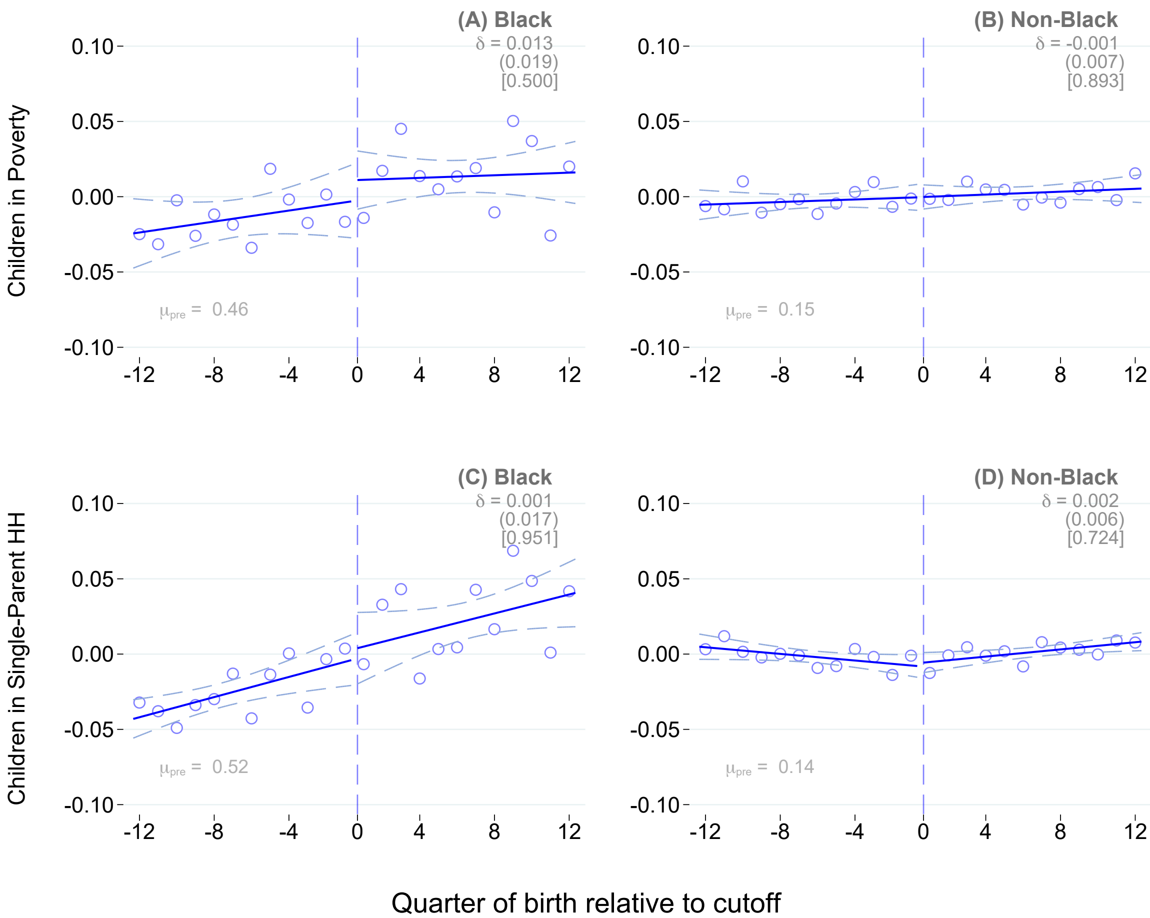
[13] Specifically, Chetty et al. (2018) provide county-level data on adult incarceration—defined as residence in a correctional facility in the 2010 Census—for children living in households at select percentiles of the income distribution. Because this data contains non-causal associations of household 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.

[14] 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.

that is the best proxy we have for county of childhood).

The results of this analysis are displayed in Appendix Figure A20. 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 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.

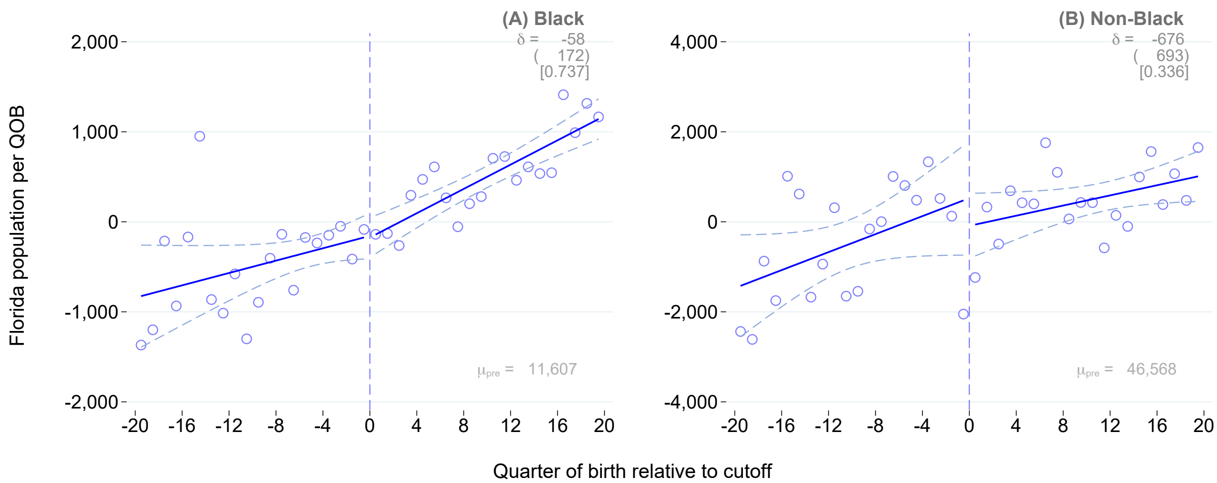
Figure A1 – Smoothness of Cohort Characteristics:
Household Variables for Children Age 0-7 (NHIS)



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 95 percent confidence intervals (displayed using dashes). The estimated coefficients, δ, and associated standard errors (in parentheses) and p-values (in brackets) 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 1982-91 National Health Interview Surveys.

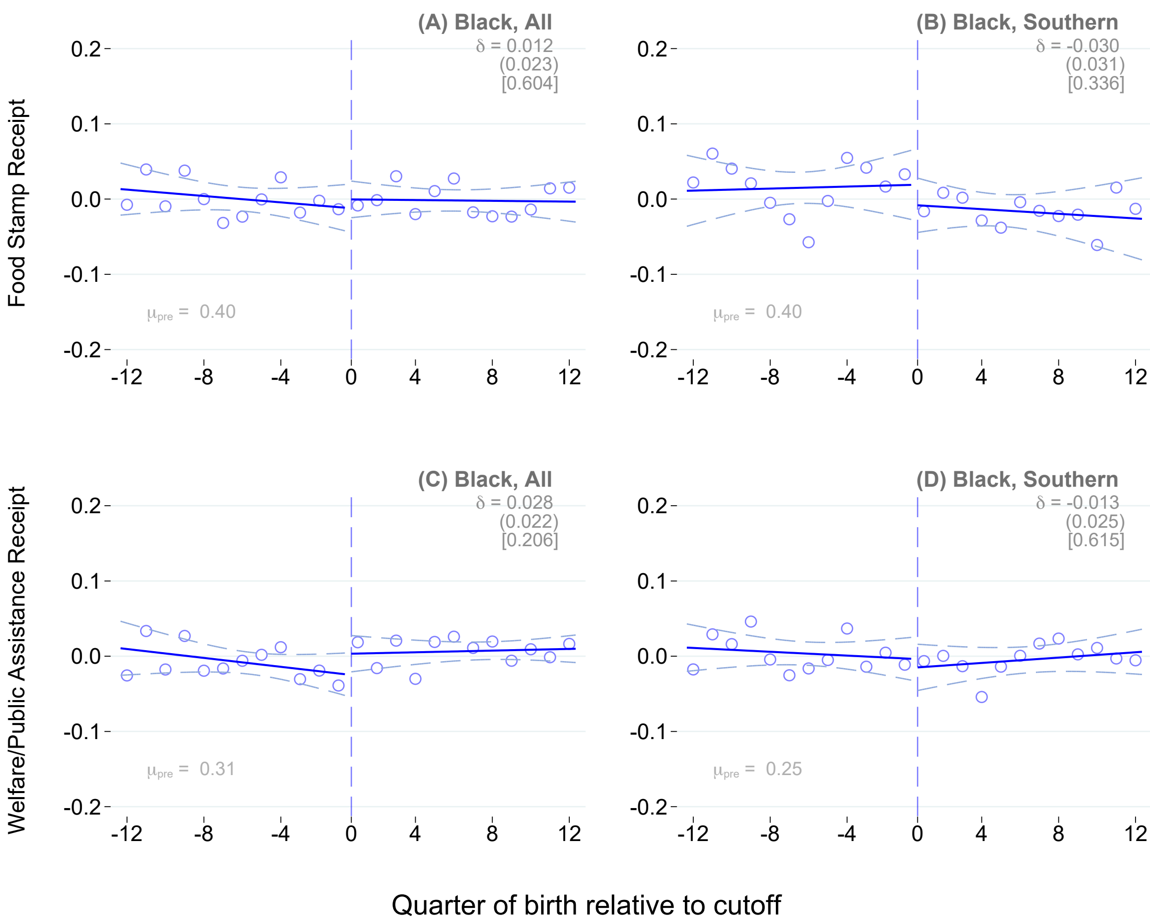
Figure A2 – Smoothness of Cohort Characteristics:
Florida Population (2010) by Quarter of Birth



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 (in parentheses, clustered at the year-quarter level) and p-values (in brackets) 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)

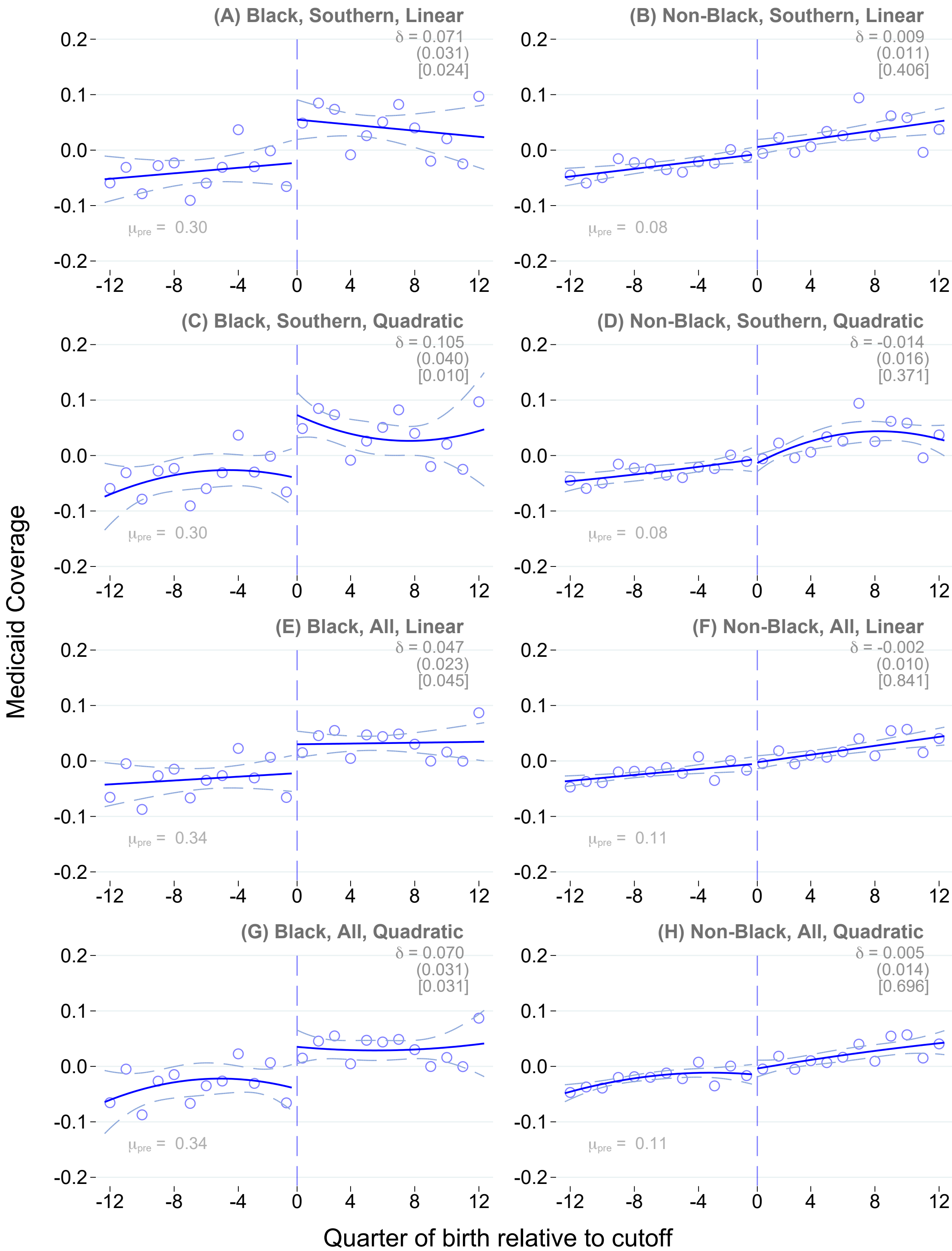
Figure A3 – Impact of the OBRA90 Expansion on Other Social Program Take-up (Black, NHIS)



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.

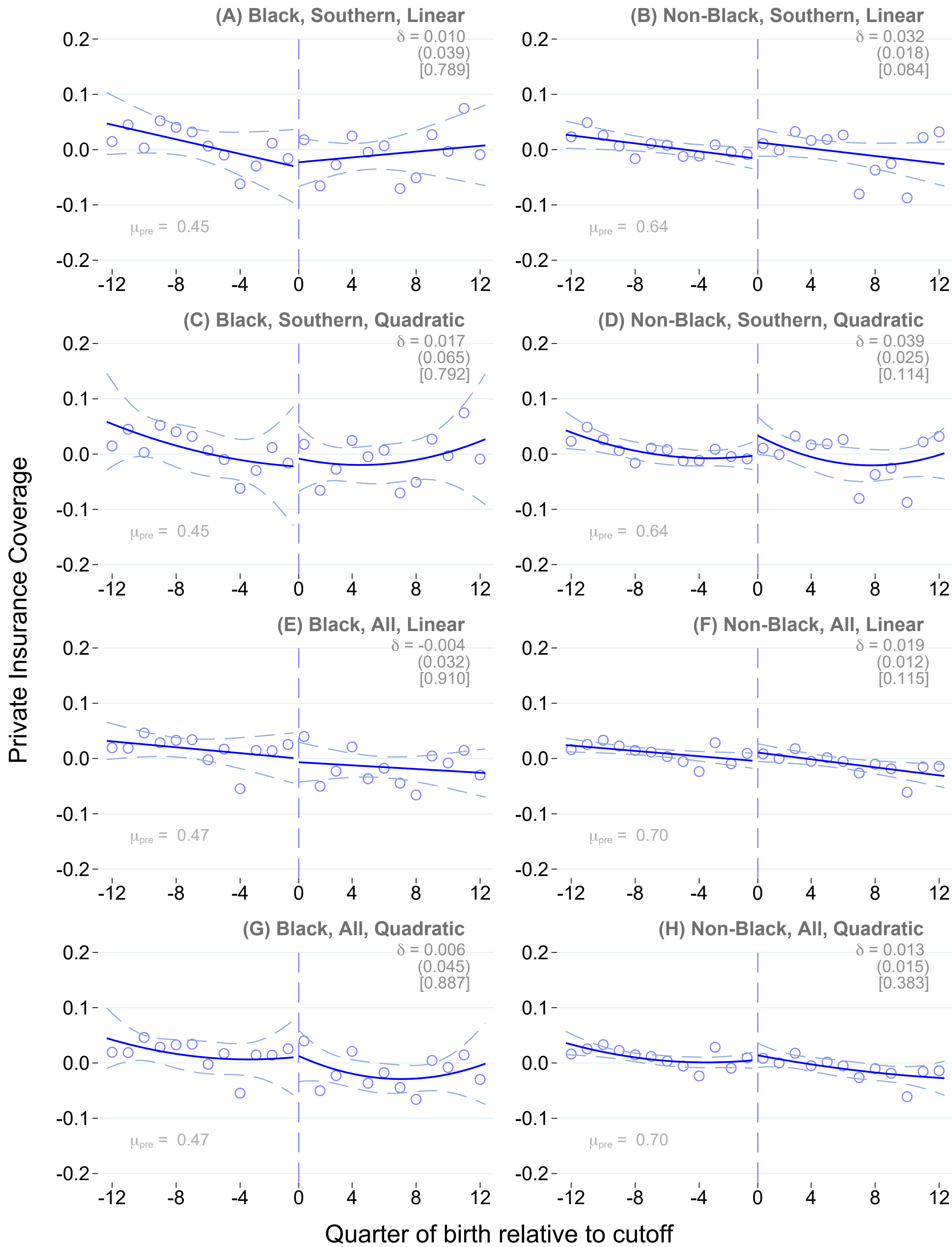
Figure A4 – First Stage: Impact of the OBRA90 Expansion on Medicaid Coverage (Alternate Samples and Specifications, NHIS)



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.

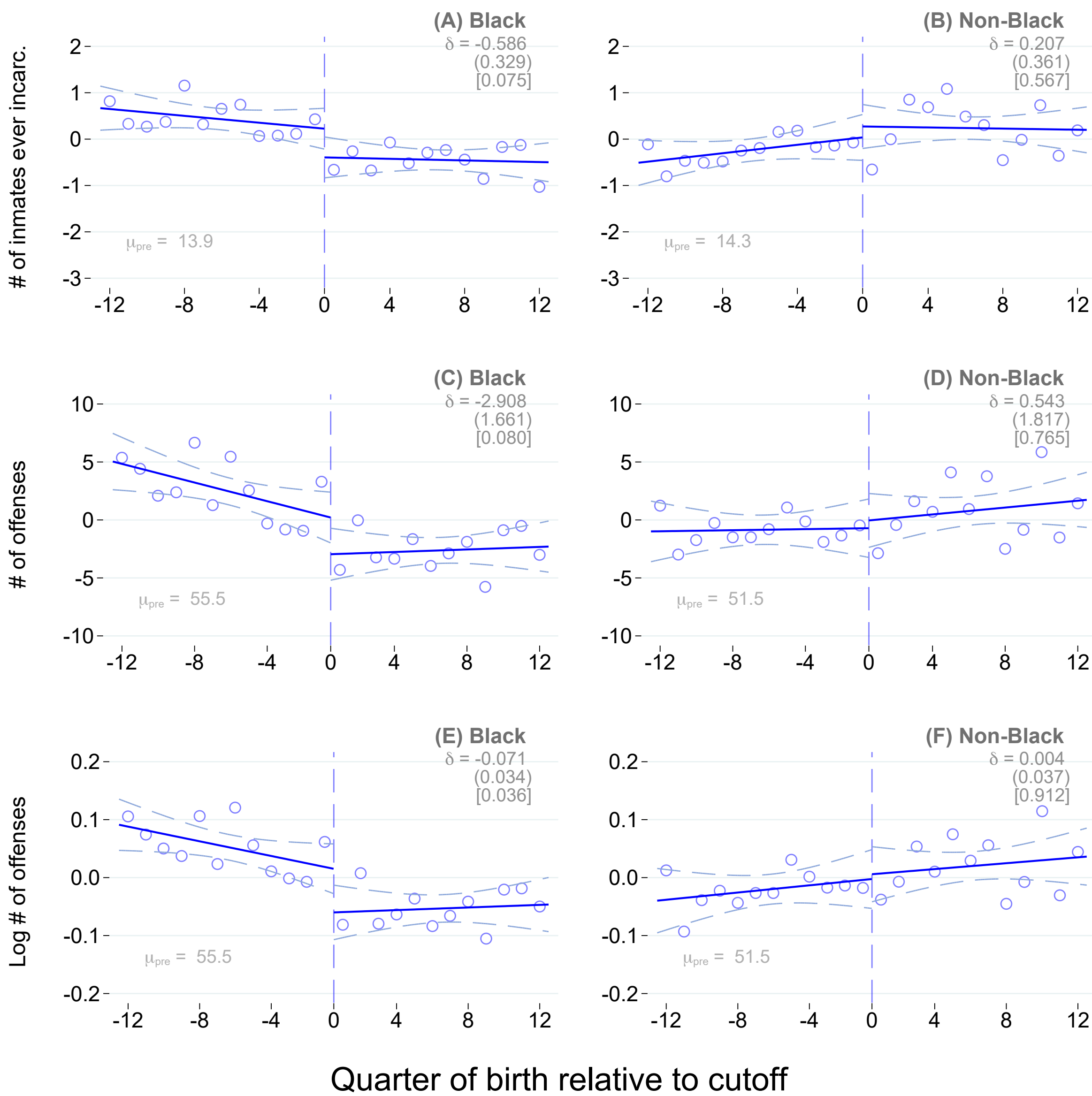
Figure A5 – Impact of the OBRA90 Expansion on Private Insurance Crowd-Out
(Alternate Samples and Specifications, NHIS)



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.

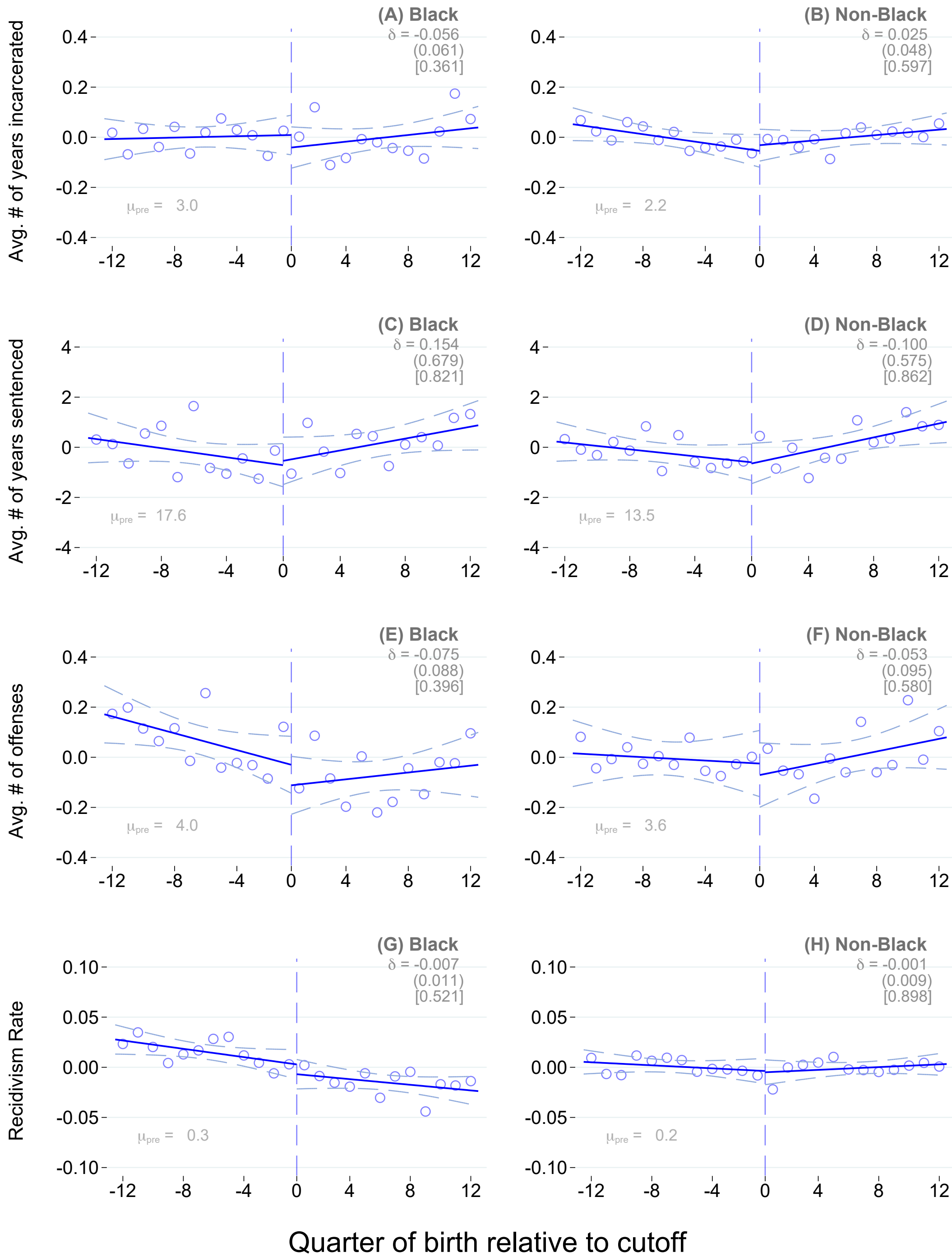
Figure A6 – Additional Results: Impact of the OBRA90 Expansion on Alternative Incarceration Measures



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.

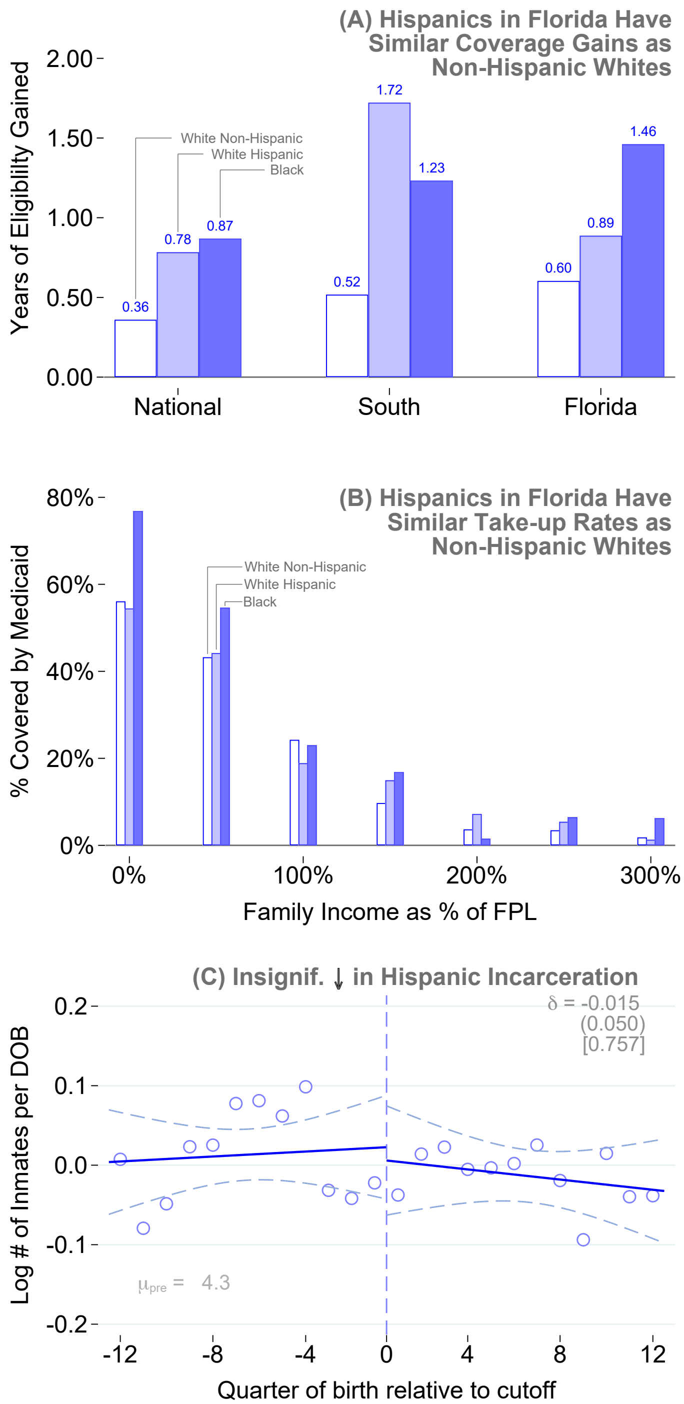
Figure A7 – Additional Results: Impact of the OBRA90 Expansion on Intensive-Margin Incarceration Measures



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 adjusted years sentenced per inmate; (3) average number of offenses per inmate; and (4) recidivism rate for each DOB cohort, respectively. Recall that adjusted years sentenced is constructed as $Sentence_i^{adj} = \min\{Sentence_i, LifeExpectancy_i\}$ in order to limit the sentenced term to the inmates' life expectancy. 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.

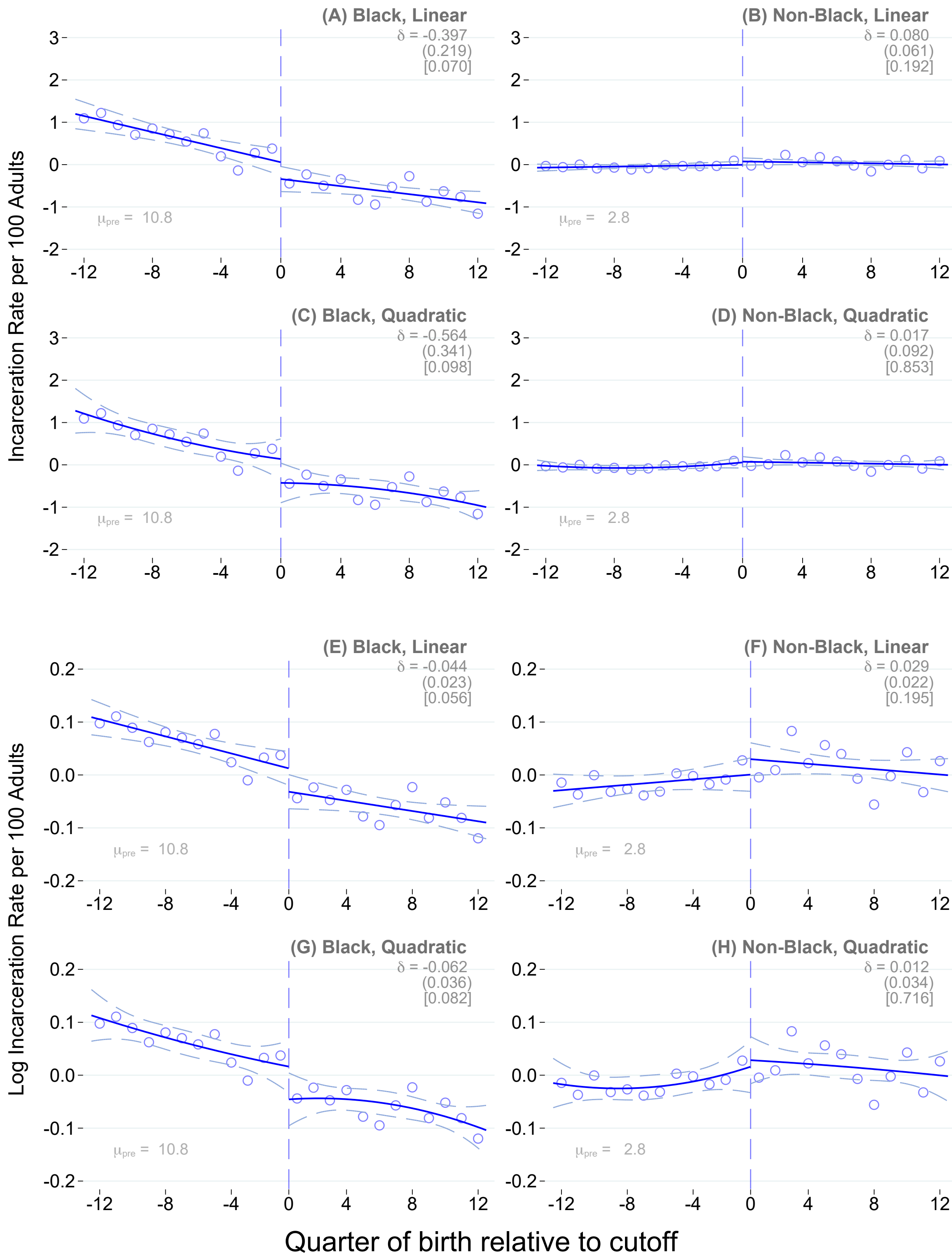
Figure A8 – Additional Results: Impact of the OBRA90 Expansion on Hispanic Eligibility, Coverage, and Incarceration Measures



Notes: The purpose of this figure is three-fold: (A) to display eligibility gains by race/ethnicity combinations and geography (and to demonstrate that eligibility gains for Hispanic whites were similar to Non-Hispanic whites in Florida and were similar to Black individuals in other geographies); (B) to display the fraction of individuals covered by Medicaid by race/ethnicity groups and family income (and to demonstrate that, conditional on income, Floridian Hispanics have similar coverage rates to Floridian Non-Hispanics); and (C) to demonstrate the effect of the OBRA90 Expansion on Hispanic Incarceration. See Appendix Section D.1 for further discussion of these results and Figure 2 for a general description of the regression discontinuity plots.

Source: Author calculations using Current Population Survey and Florida DOC Incarceration Data.

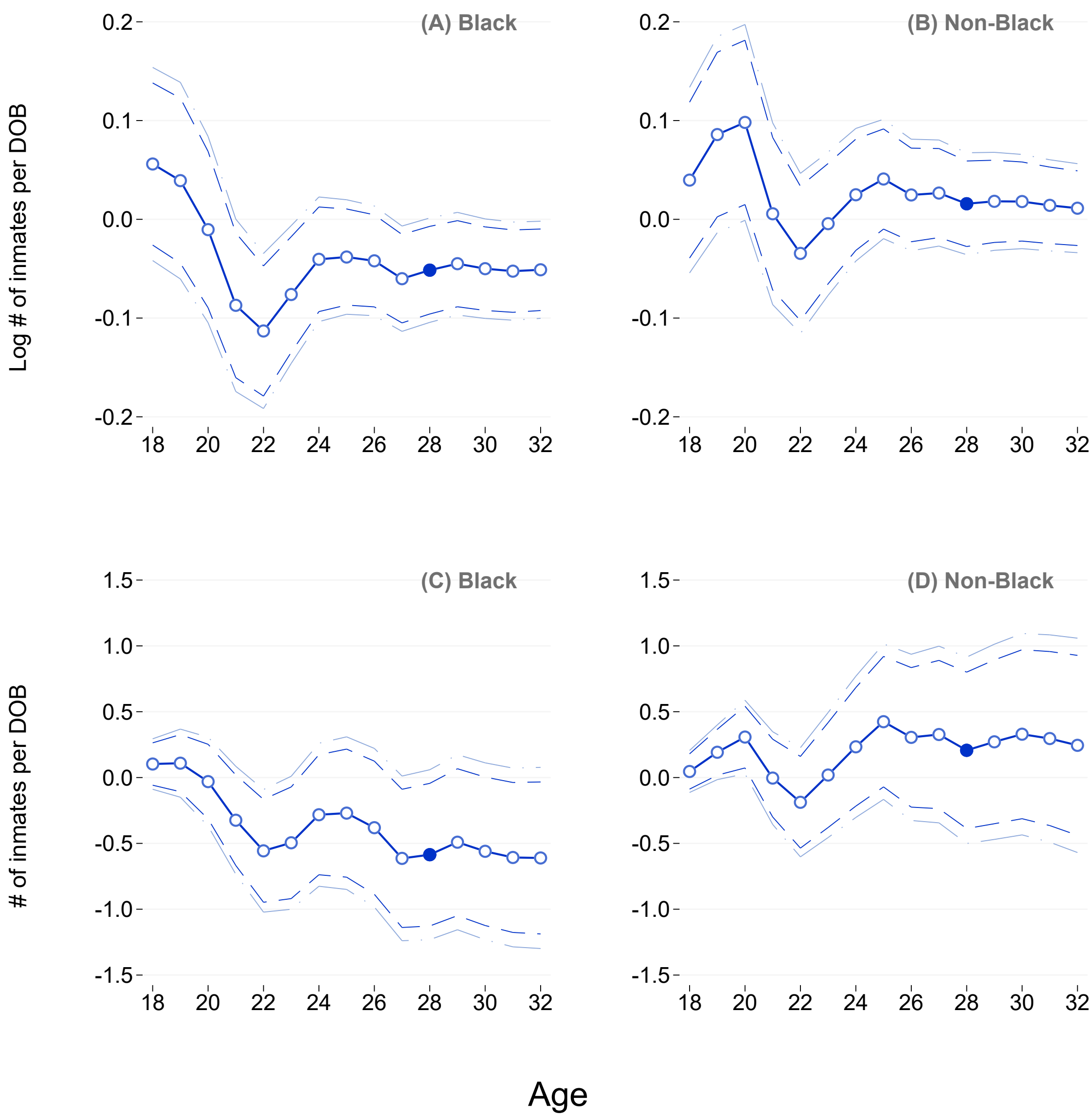
Figure A9 – Additional Results: Impact of the OBRA90 Expansion on Adult Incarceration (in Rates)



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.

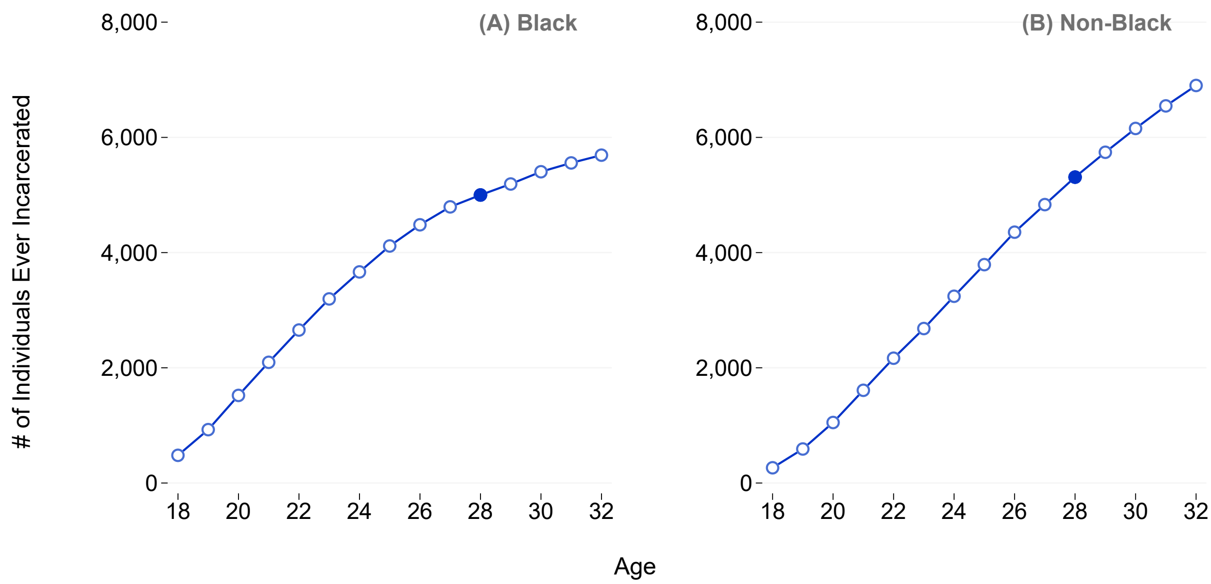
Figure A10 – Additional Results: Effects on Incarcerations by Age



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). Dark and light dashed lines indicate 90 and 95 percent confidence intervals, respectively. See also Figure A11 for descriptive statistics on total individuals ever incarcerated by age.

Source: Author calculations using Florida DOC Incarceration Data.

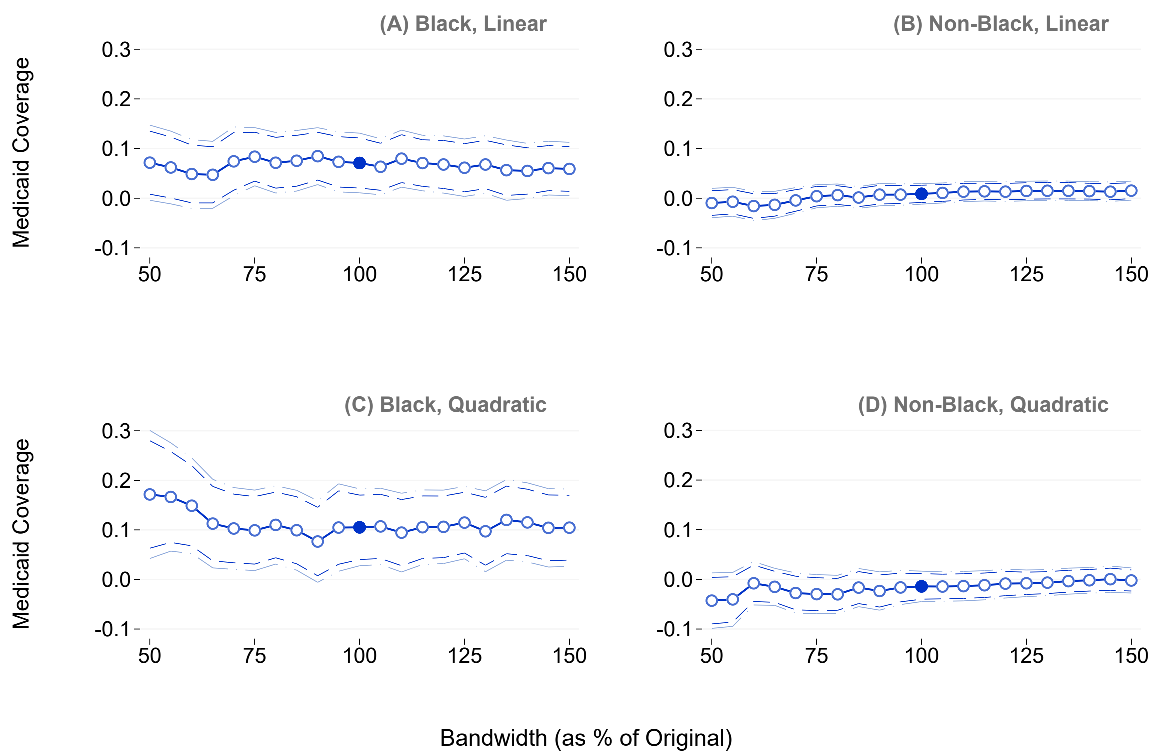
Figure A11 – Descriptive Results: Cumulative Individuals Ever Incarcerated by Age



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 A10.

Source: Author calculations using Florida DOC Incarceration Data.

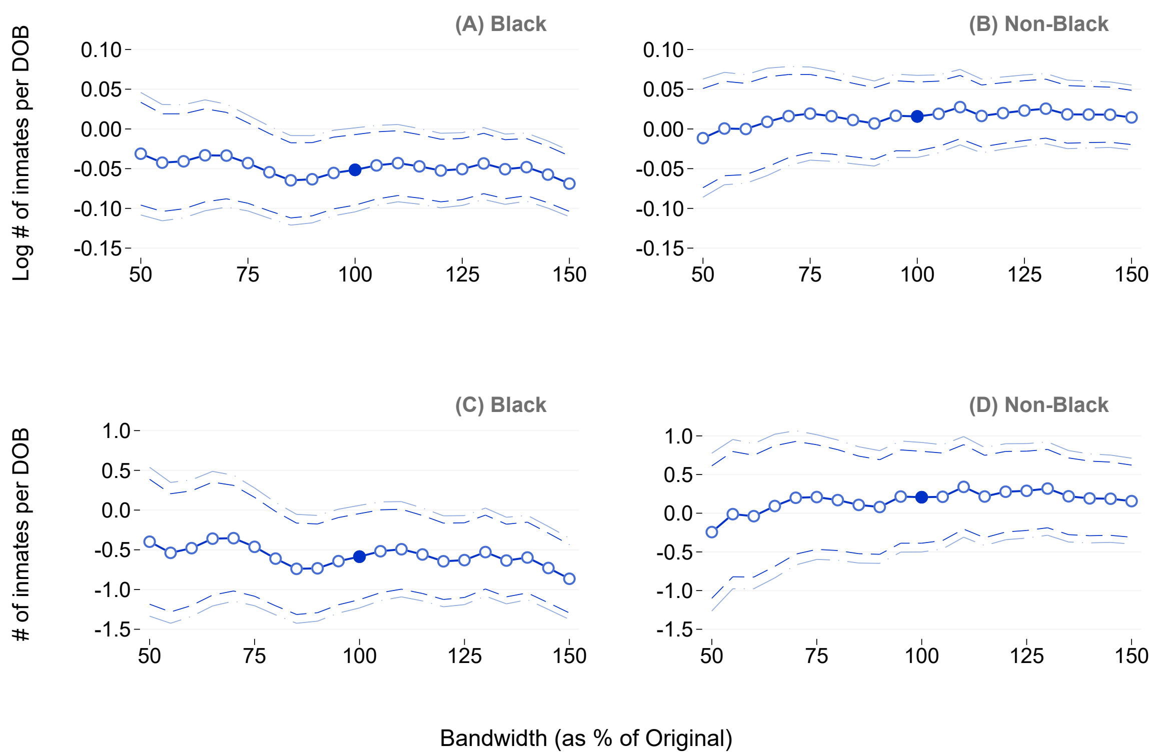
Figure A12 – Robustness: Treatment Effects by Bandwidth (First Stage)



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). Dark and light dashed lines indicate 90 and 95 percent confidence intervals, respectively.

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

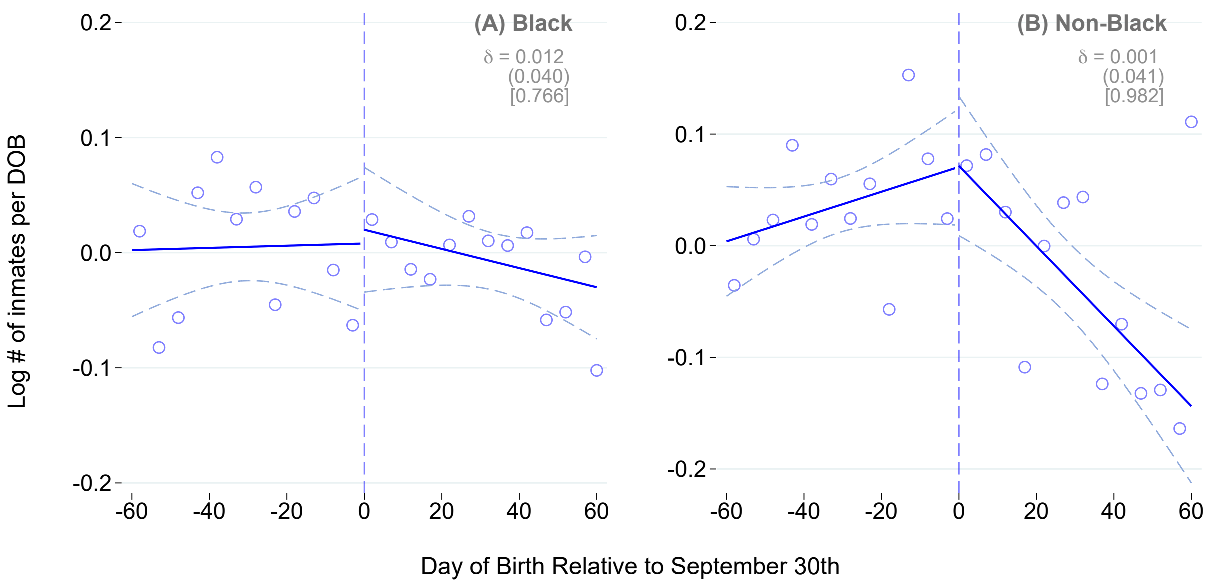
Figure A13 – Robustness: Treatment Effects by Bandwidth (Main Analysis)



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). Dark and light dashed lines indicate 90 and 95 percent confidence intervals, respectively.

Source: Author calculations using Florida DOC Incarceration Data.

Figure A14 – Robustness: Treatment Effects for Other September 30th Cutoffs



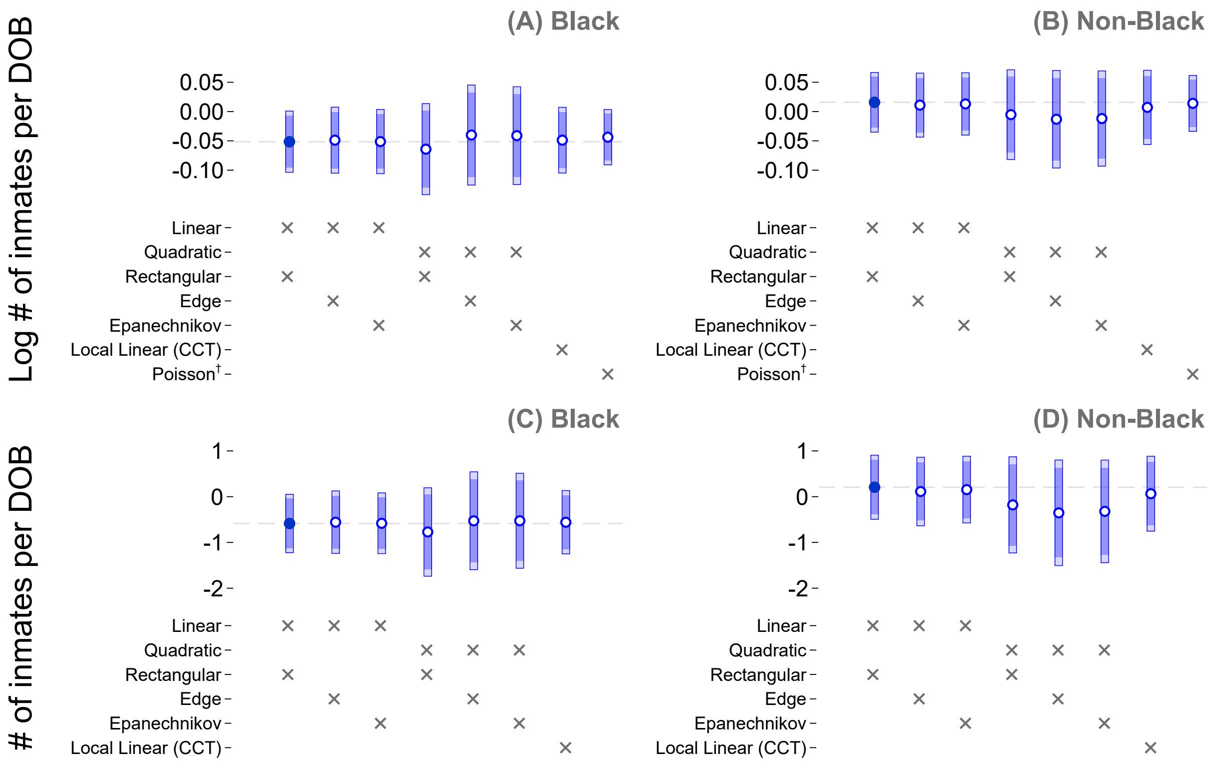
Notes: The purpose of this figure is to display the results a regression discontinuity analysis of all September 30th cutoffs during 1980-1982 and 1984-1986 (i.e., the years included in our bandwidth *excluding* the treated year of 1983). Each dot represents the average of the outcome variable (the log count of inmates ever incarcerated) in 5-day bins. The lines presented are generated from linear regressions with associated 95 percent confidence intervals (displayed using dashes). The estimated coefficients, δ, and associated standard errors (in parentheses) and p-values (in brackets) 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 A15 – Robustness: Treatment Effects by Specification Choice

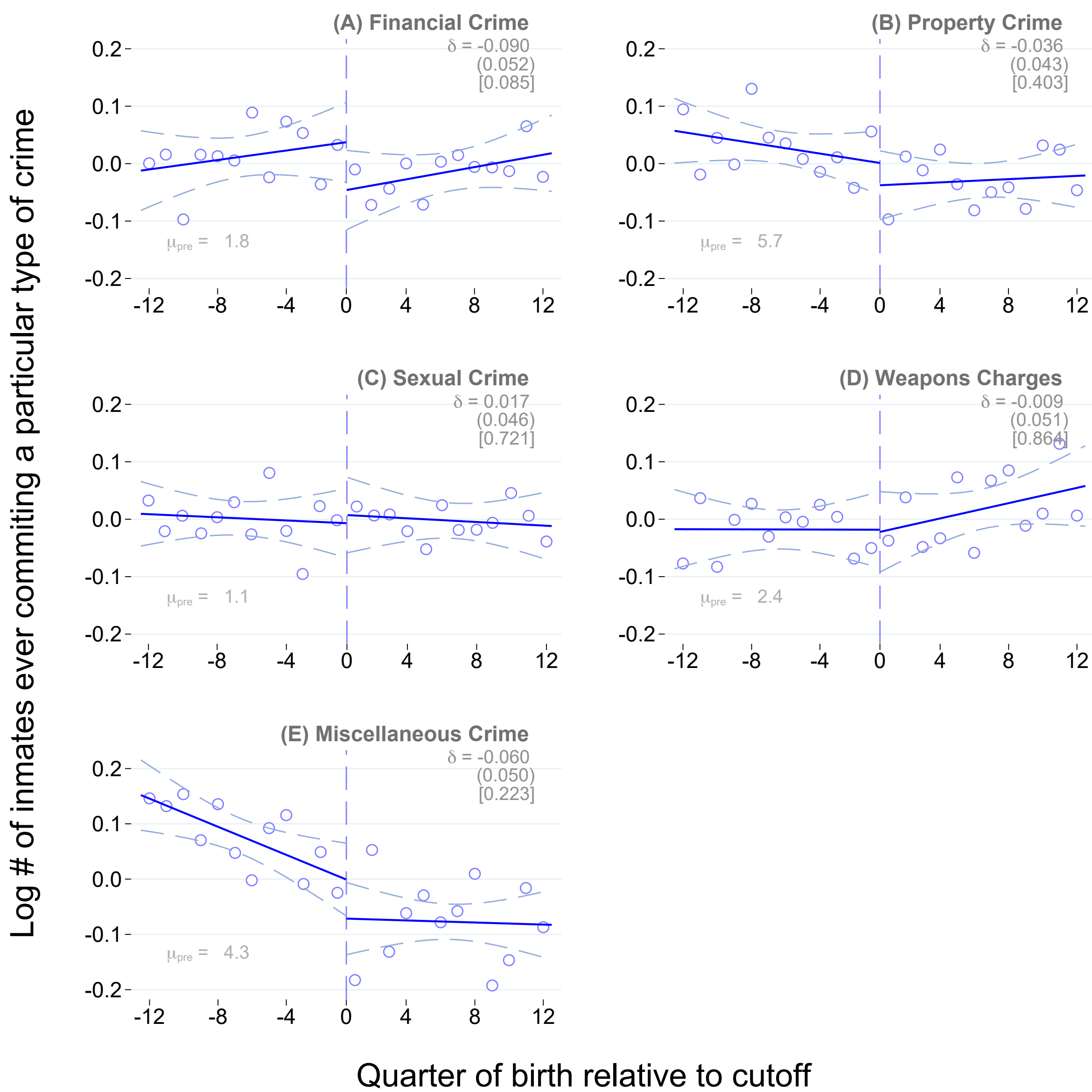


Notes: The purpose of this figure is to display the results of Equation 1 for varying specifications. Panels A and B display estimates of the reduction in log incarcerations (our main outcome), while Panels C and D detail estimates when using counts rather than logs. Each dot represents the estimated coefficient δ from a separate regression (our primary estimate is shaded dark blue), with dark bars indicating 90% and light/outlined bars indicating 95% 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.

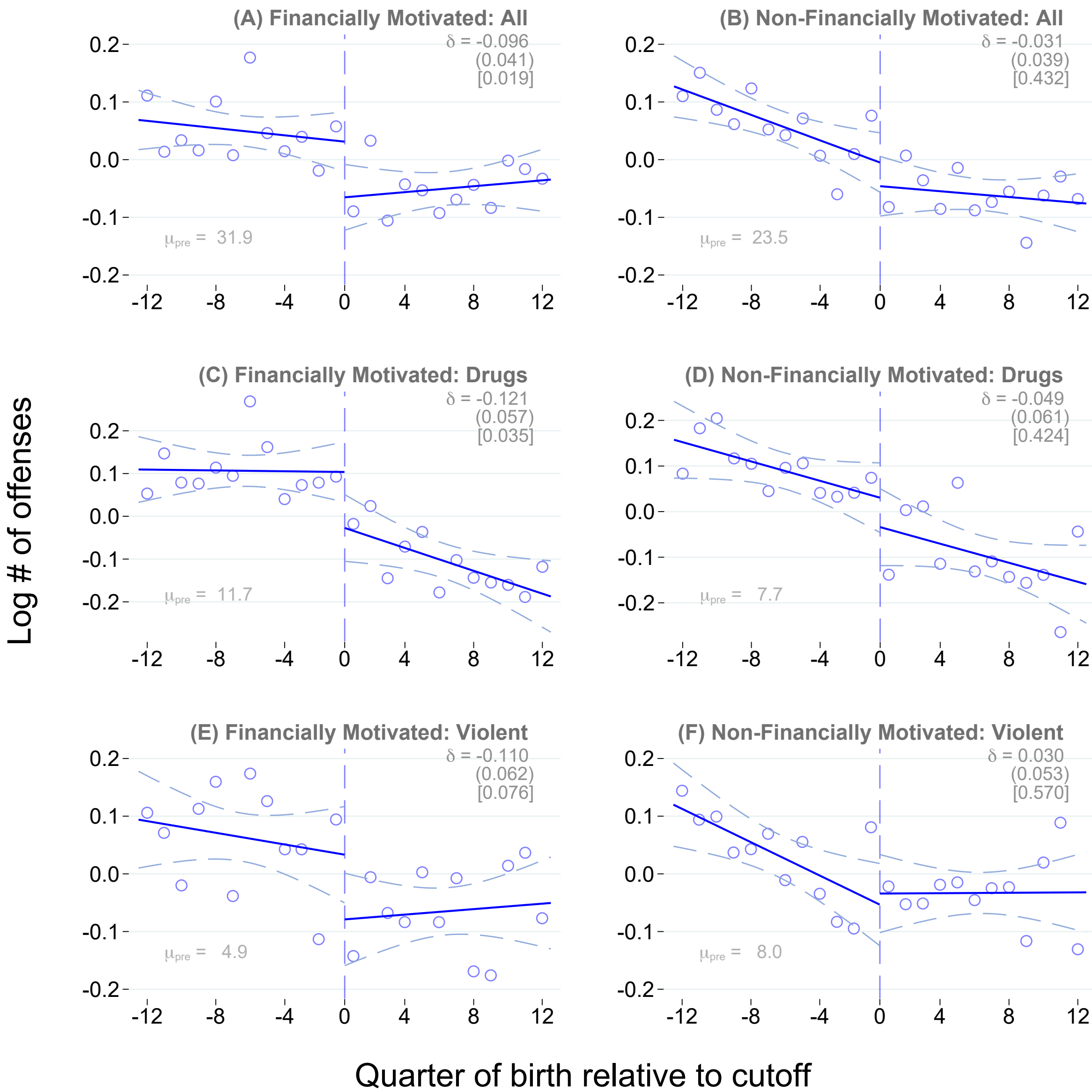
Figure A16 – Impact of the OBRA90 Expansion on Adult Incarceration by Other Offender Types (Black)



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.

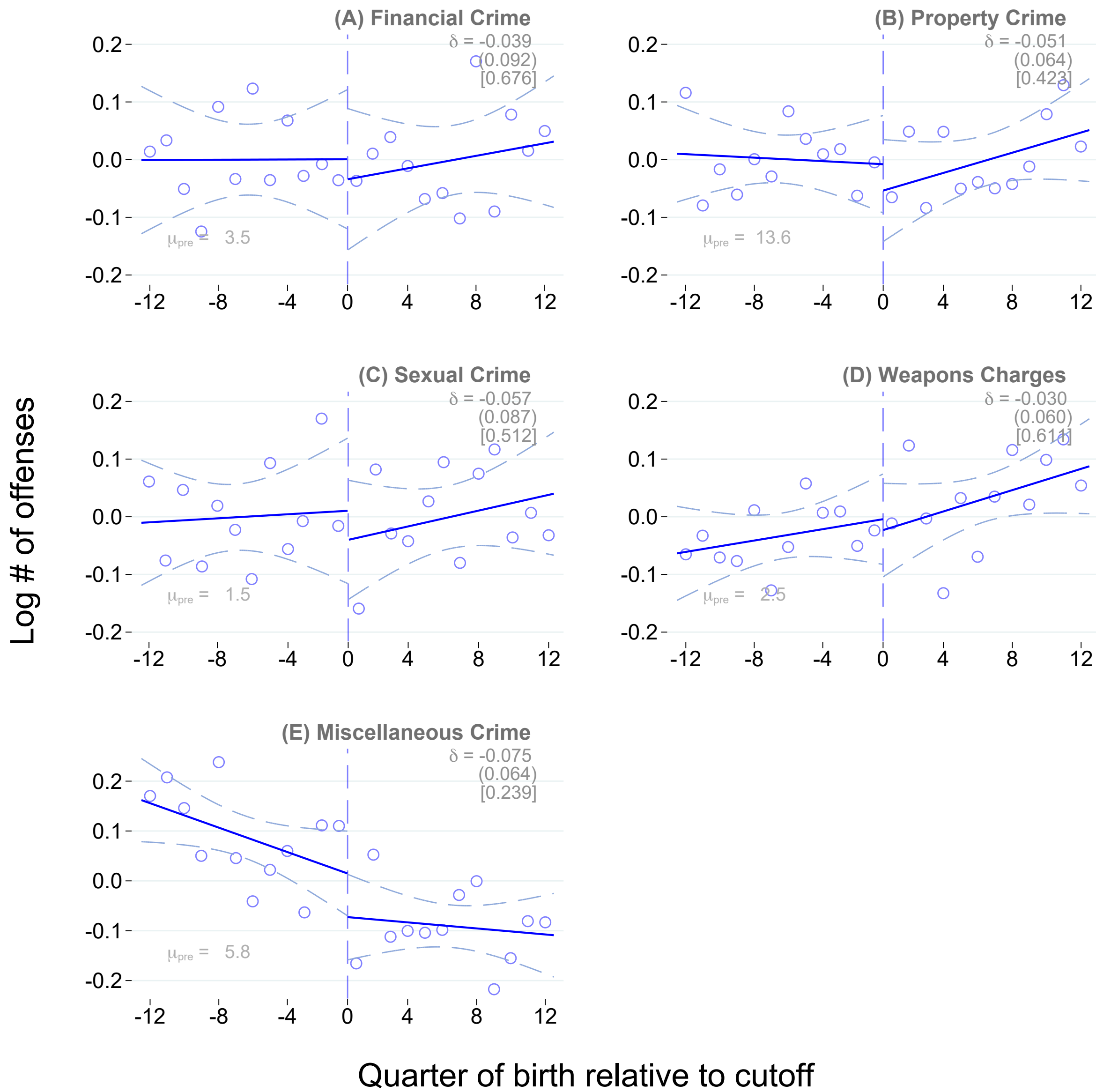
Figure A17 – Impact of the OBRA90 Expansion on Offenses Committed by Crime Type (Black, Offense-Level)



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.

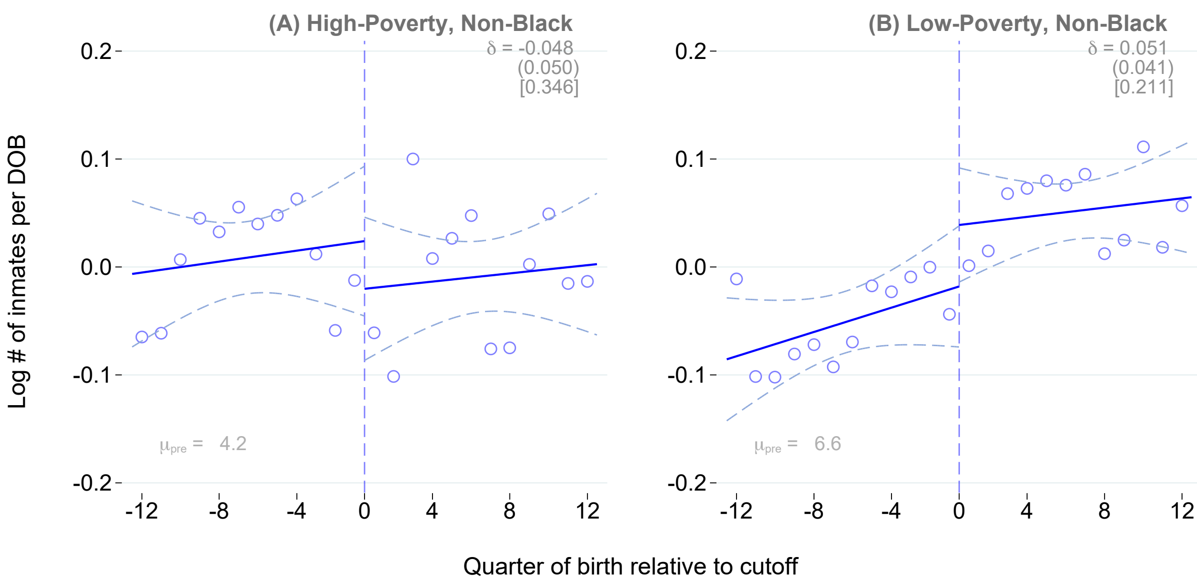
Figure A18 – Impact of the OBRA90 Expansion on Offenses Committed by Other Crime Types (Black, Offense-Level)



Notes: This figure replicates the analysis of Appendix Figure A16 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 A16 for more detail.

Source: Author calculations using Florida DOC Incarceration Data.

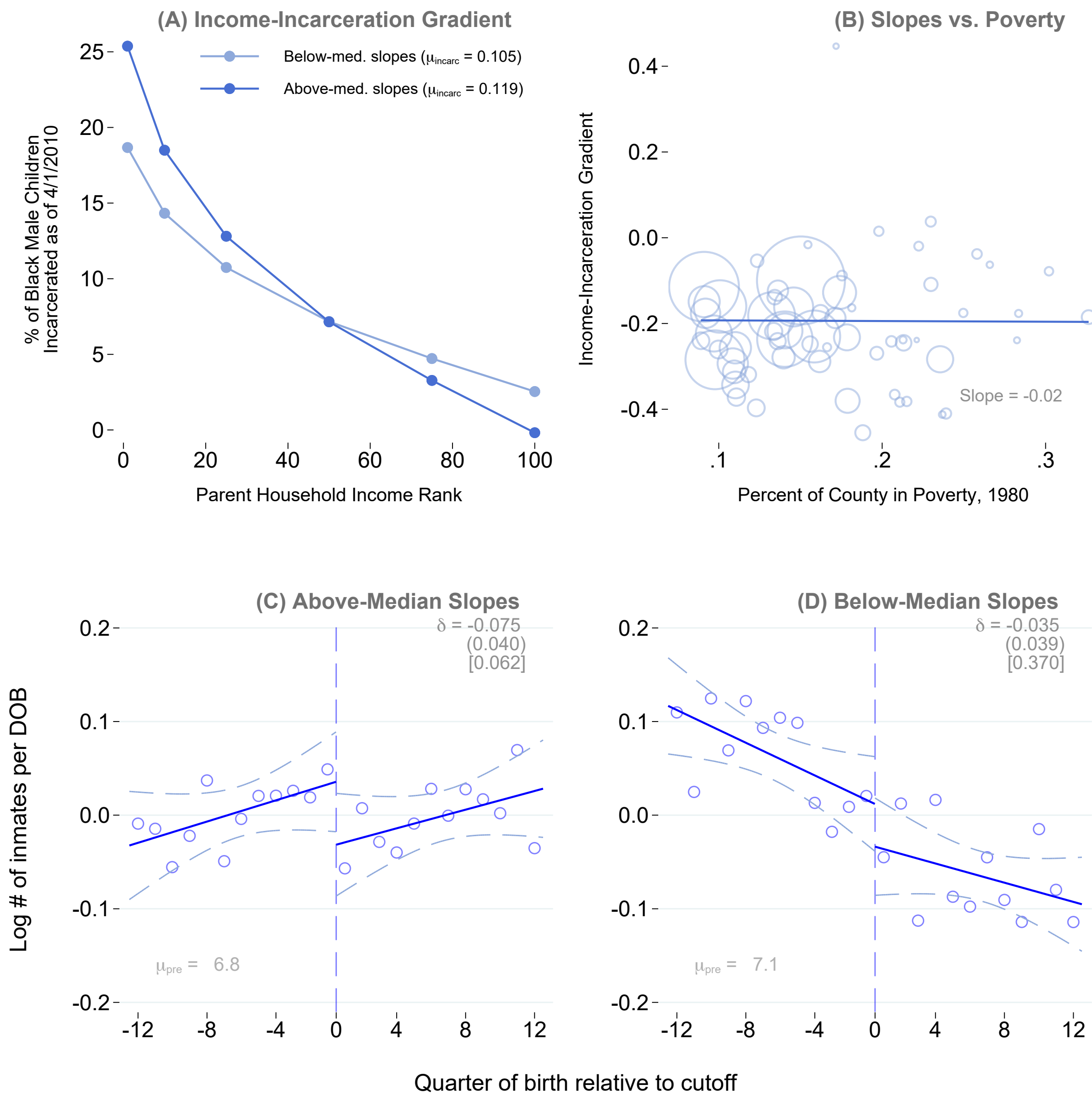
Figure A19 – Heterogeneity by Poverty of Release Zip Code (Non-Black)



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 2007-11 American Community Survey Data (Manson et al., 2019).

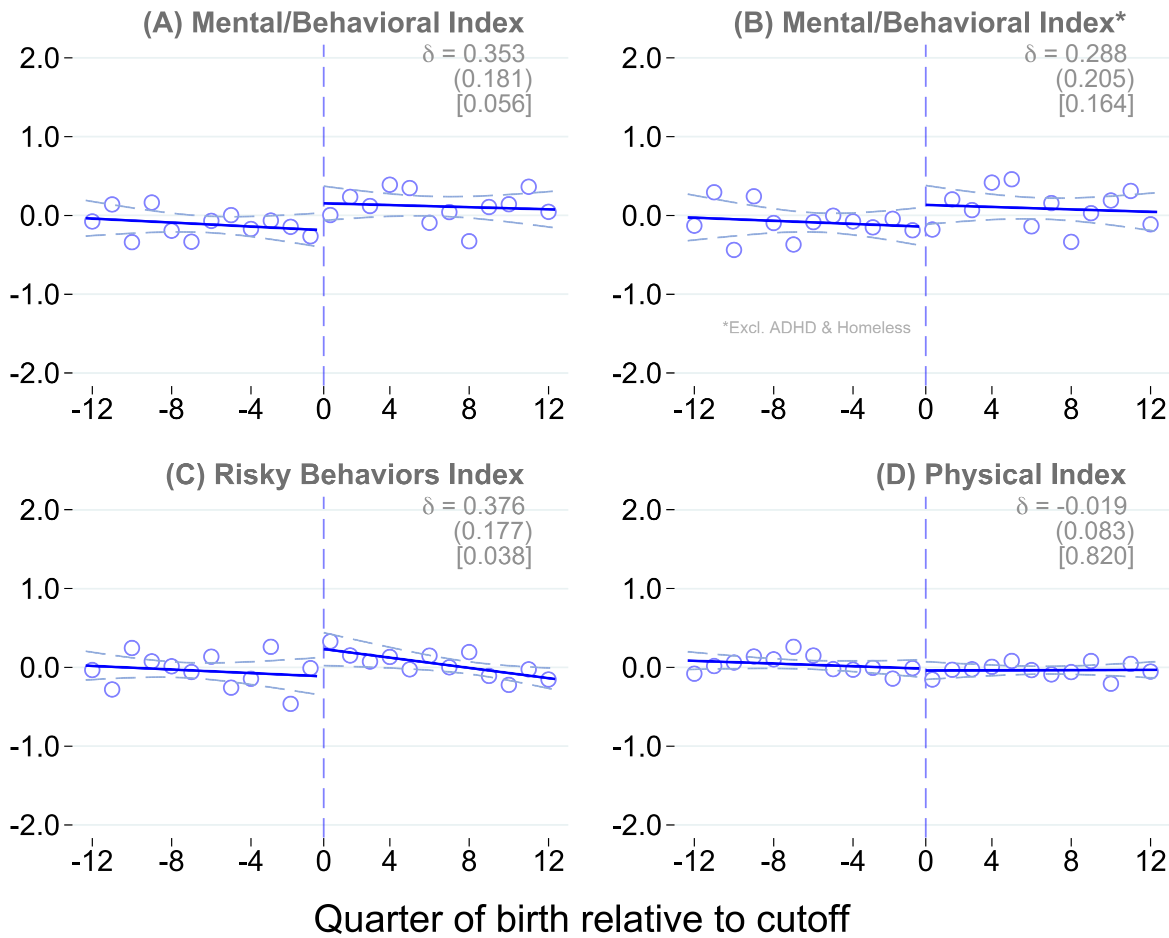
Figure A20 – Heterogeneity by Counties with Steep and Shallow Income-Incarceration Slopes (Black)



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.3 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).

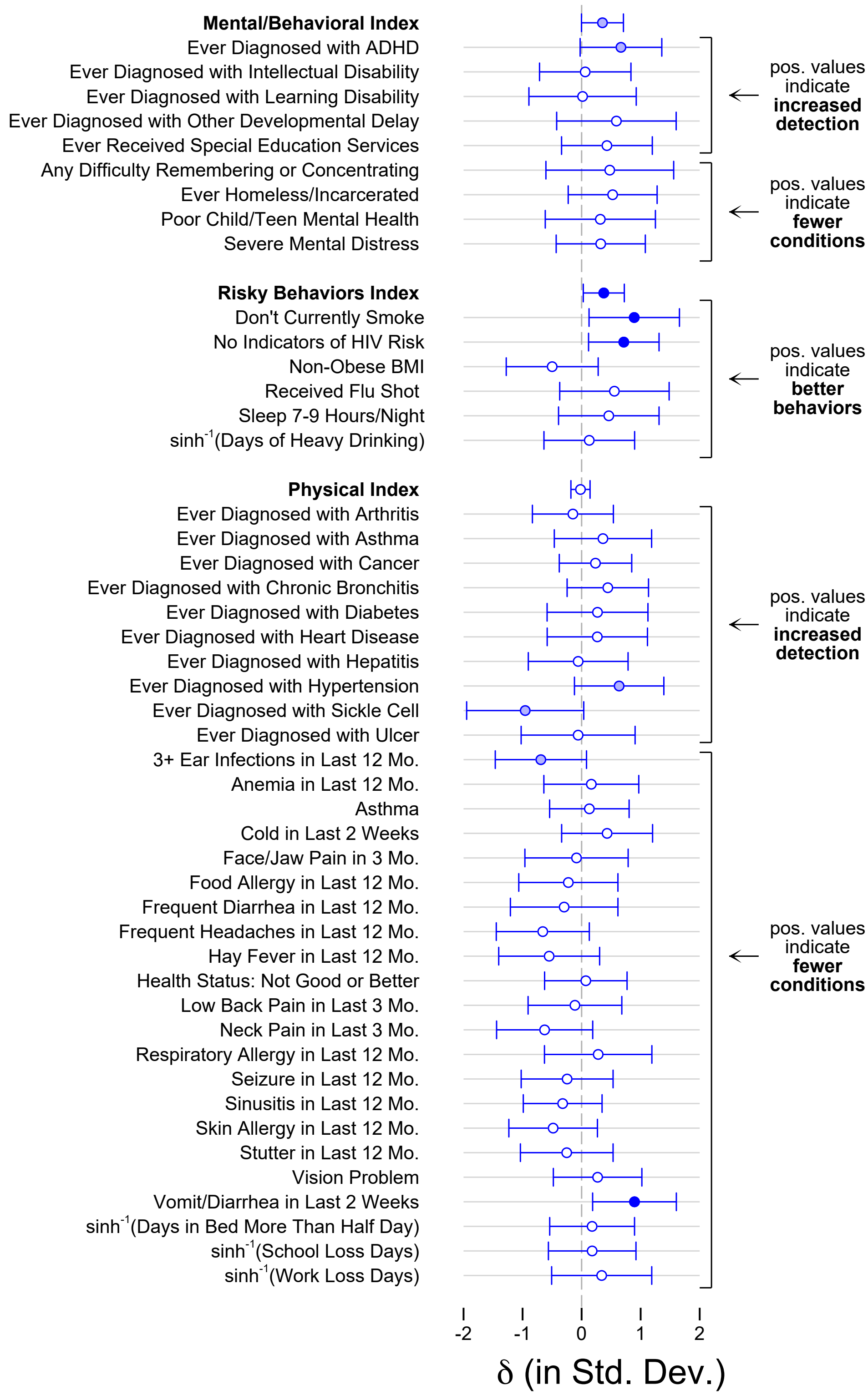
Figure A21 – Impact of the OBRA90 Expansion on Mental/Behavioral Health, Risky Behaviors, and Physical Health (Black, NHIS)



Notes: The purpose of this figure is to display the impact of the OBRA90 expansion on indices regarding: (1) the detection and current status of mental health issues (Panels A and B), (2) the status of current risky behaviors (Panel C), and (3) the detection and current status of physical health issues (Panel D). Within the figures, higher values indicate (a) improvements in the detection of illnesses, (b) better current health status, or (c) fewer risky behaviors, as applicable. The primary difference between Panels A and B are the exclusion of variables for attention deficit disorder (discussed at length in Section 6.2) and homelessness, which also may include incarceration. 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 (in parentheses, clustered at the year-month level) and p-values (in brackets) 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. See Appendix Section D.2 for more detail on construction of indices and Appendix Figure A22 for detail on individual index components.

Source: Author calculations using the 1997-2014 National Health Interview Surveys (Blewett et al., 2019).

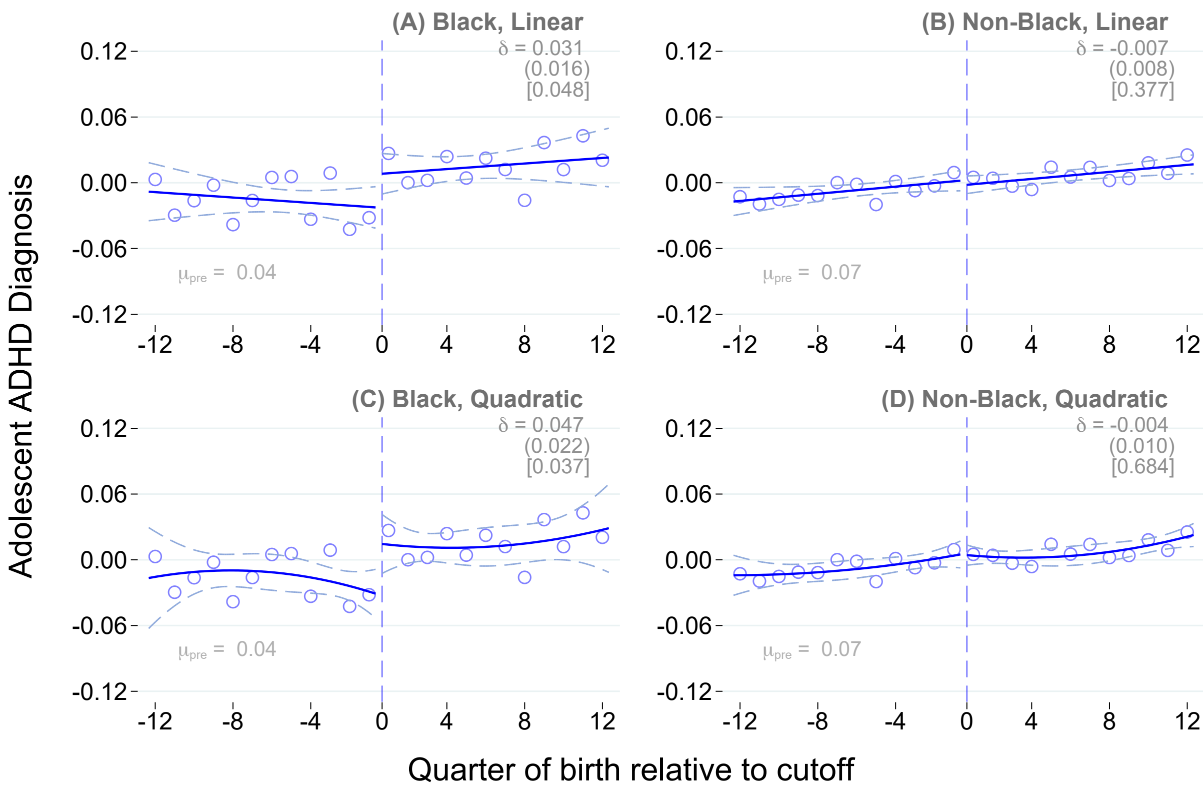
Figure A22 – Impact of the OBRA90 Expansion on Mental/Behavioral Health, Risky Behaviors, and Physical Health (Component Outcomes, Black, NHIS)



Notes: The purpose of this figure is to display estimates of the impact of the OBRA90 expansion on the *components* of the indices discussed in detail in Appendix Section D.2 and displayed in Appendix Figure A21. Each point represents the estimate (δ) along with associated 95% confidence intervals for the impact of the Expansion on the given index component.

Source: Author calculations using the 1997-2014 National Health Interview Surveys (Blewett et al., 2019).

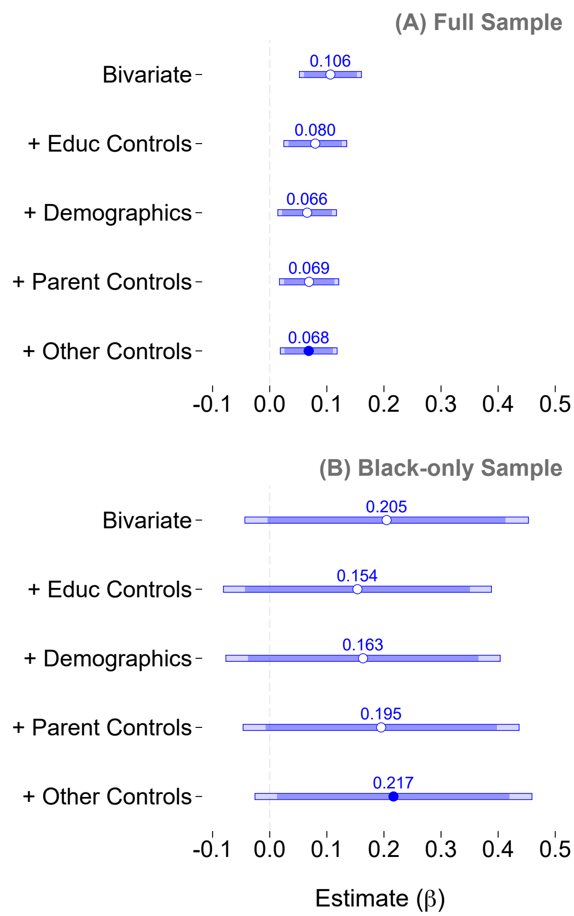
Figure A23 – Impact of the OBRA90 Expansion on ADHD Diagnoses (NHIS, Alt. Specifications)



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 1997-2004 National Health Interview Surveys (Blewett et al., 2019).

Figure A24 – ADHD is Associated with Later-Life Incarceration Outcomes, Even Conditional on Educational Attainment and Other Controls (ADD Health)



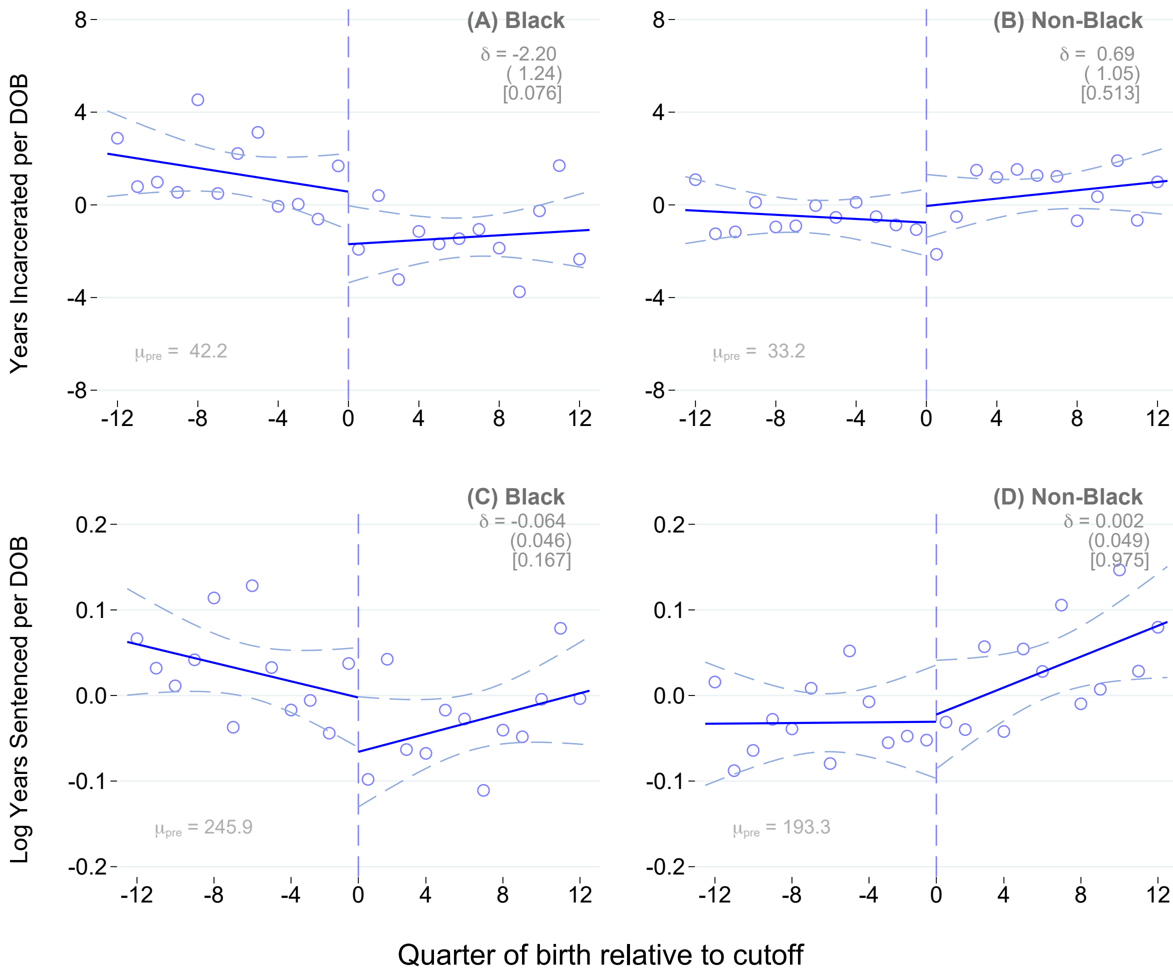
Notes: The purpose of this figure is to provide suggestive evidence that ADHD is predictive of later-life incarceration, even conditional on educational attainment and other controls. In other words, diagnoses—and subsequent treatment—of ADHD may have benefits in the reduction of incarceration outside of the benefits that it provides in terms of educational attainment. To obtain these estimates, we estimated regressions using microdata of the following form:

$$EverIncarcerated_i = \beta \cdot ADHD_i + f(X_i) + \xi_i,$$

where $EverIncarcerated_i$ and $ADHD_i$ are indicator variables for whether an individual was ever incarcerated and/or diagnosed with ADHD, respectively. The vector of controls is described by the y-axis of the figure, and further description of controls included in this figure is available upon request. Within the figure, each point represents an estimate from this equation (with the dark point representing the fully saturated association referenced in the text), along with 90% and 95% confidence intervals in dark and light bars, respectively.

Source: Author calculations using the ADD Health Survey, Waves 1 and 4 (Public Version).

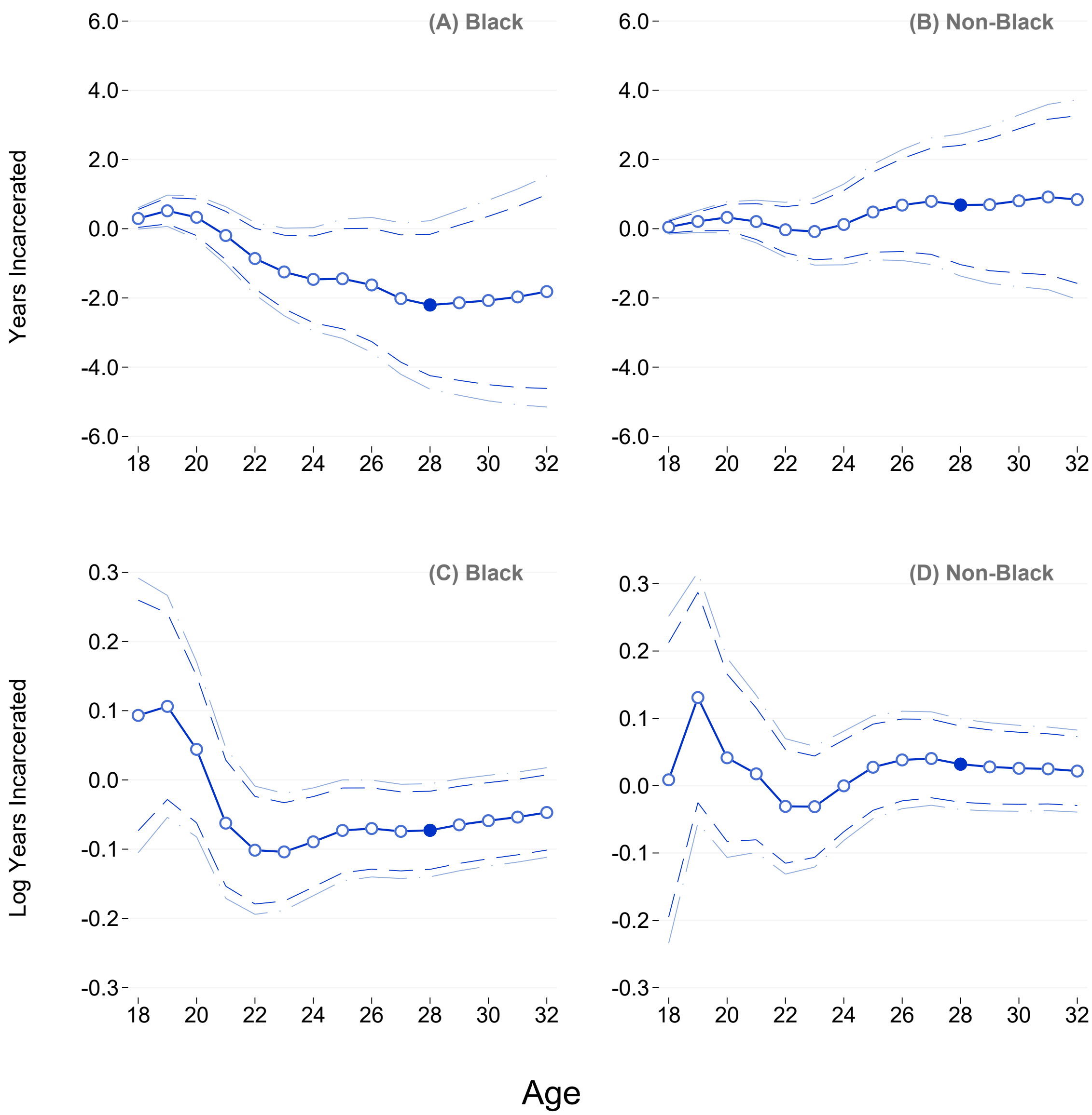
Figure A25 – Additional Results: Impact of the OBRA90 Expansion on Years Incarcerated and Log Years Sentenced



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.

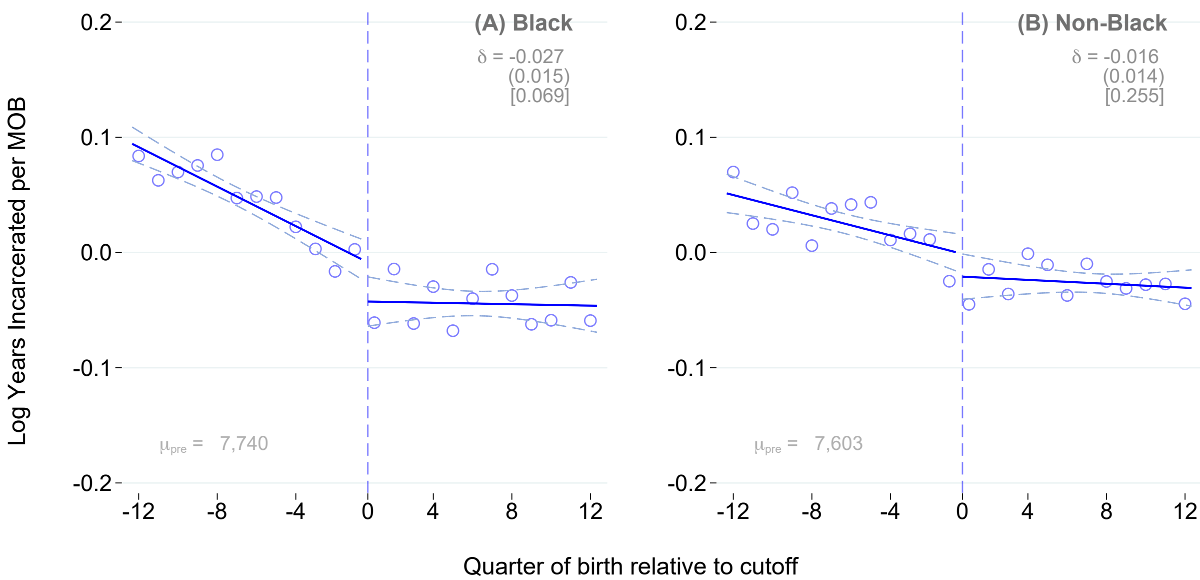
Figure A26 – Additional Results: Effects on Years Incarcerated by Age



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). Dark and light dashed lines indicate 90 and 95 percent confidence intervals, respectively.

Source: Author calculations using Florida DOC Incarceration Data.

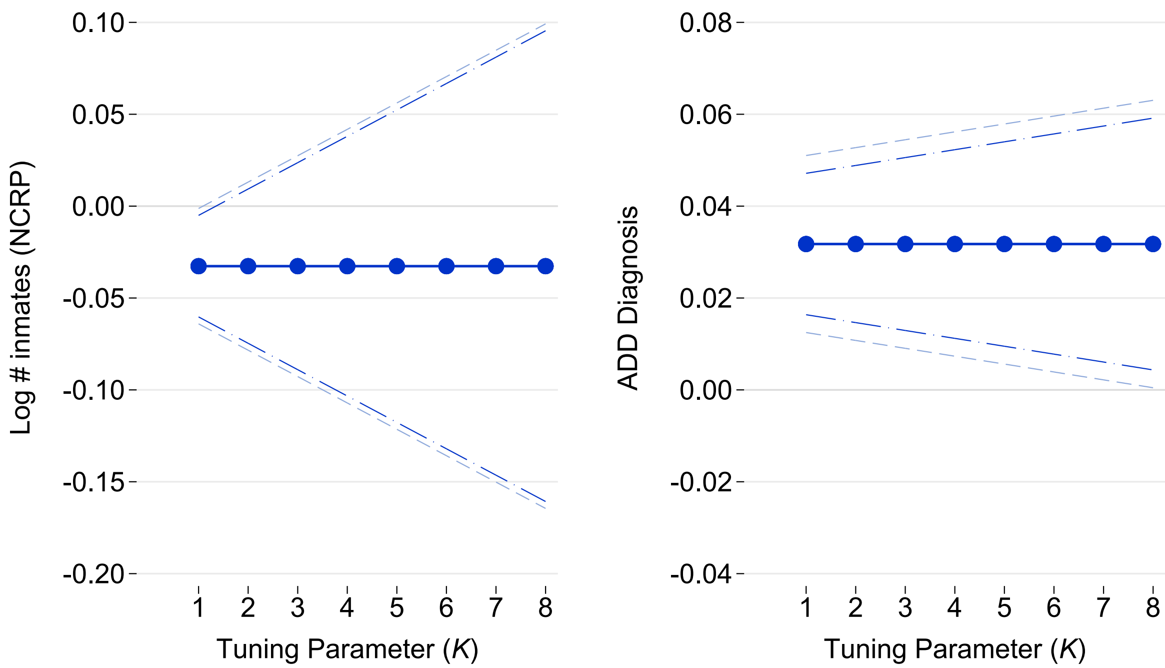
Figure A27 – External Validity: Impact of the OBRA90 Expansion on Years Incarcerated (NCRP Data)



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 (in parentheses, clustered at the year-month level) and p-values (in brackets) 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 2000-16 Restricted-Use National Corrections Reporting Program Data (Bureau of Justice Statistics, 2019).

Figure A28 – 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 and corresponding dark and light dashed lines representing 90% and 95% confidence intervals, respectively associated with selected tuning parameters on the x-axis. See Appendix Section C for a more detailed discussion of this inference method and nature of tuning parameters.

Source: Author calculations using the 2000-16 Restricted-Use National Corrections Reporting Program Data (Bureau of Justice Statistics, 2019) and the National Health Interview Surveys (Blewett et al., 2019).

Table A1 – Impact of the OBRA90 Expansion on Medicaid Eligibility: Further Context

	Average years gained for all children		Mean years of eligibility for all children (pre-Expansion)		Percent increase in eligibility years vs. pre-Expansion mean	
	Black	Non-Black	Black	Non-Black	Black	Non-Black
National	0.87	0.41	7.66	3.19	11%	13%
Southern region	1.23	0.66	6.73	2.24	18%	29%
Florida	1.46	0.64	6.51	2.21	22%	29%

Notes: The purpose of this table is to provide further context for the gains in eligibility from the OBRA90 Expansion. The first set of columns, which are the same as the middle set of columns of Table 1, detail the population-level average years of eligibility gained by race. The second set of columns detail the baseline *means* of population-level years of eligibility prior to the Expansion. Lastly, the third set of columns detail the percent gain in years of eligibility over the pre-Expansion mean.

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

Table A2 – Calculating the Reduction in Incarceration "Explained" by Improvements in Educational Attainment

	(1)	(2)
Johnson and Jackson (2019): Effect of a 1% Increase in Funding (in p.p.)		
Increase in High School Completion	1.10	1.42
Reduction in Individuals Ever Incarcerated	0.81	1.18
Ratio: (Δ Individuals Incarcerated, divided by Δ HS Completion) (A)	0.73	0.83
Cohodes et al. (2016): Effect of 1 Year of Medicaid Eligibility		
Increase in High School Completion (B)	0.22	0.26
Predicted Incarceration Reduction (in p.p.) (A $\times$ B)	**0.16**	**0.21**
OBRA90 Reduction Incarceration Rate per Year of Eligibility (Section 5.2)	**0.38**	**0.38**
% "Explained" by Education Results	**42%**	**56%**

Notes: The purpose of this table is to display how much of the reductions in incarceration could be mediated through the education channel. To do so, we first use estimates from Johnson and Jackson (2019) to recover causal effect of high school completion (induced by an increase in school funding) on later-life incarceration. We then use this education-incarceration relationship and multiply it by the increase in high school completion effected by an additional year of Medicaid eligibility (Cohodes et al., 2016). This allows us to obtain the *predicted* reduction in incarceration from the Medicaid-induced improvements in education, and compare it to the effects that we find in the main text of the paper. Within the table, each column represents different estimates from these papers, where the first column includes estimates that result in the smallest predicted reduction, and the second column includes estimates that result in the largest predicted reduction.

Source: Author calculations estimates from Johnson and Jackson (2019) and Cohodes et al. (2016).