The Long-Run Effects of Universal Pre-K on Criminal Activity

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While public prekindergarten access has expanded rapidly over the last two decades, there is little evidence on the long-run effects of large-scale universal prekindergarten (UPK). I estimate the impact of Oklahoma's UPK policy on the likelihood of criminal conviction in early adulthood using a regression discontinuity design that leverages the birthdate cutoff for UPK in the program's first year of implementation. Using administrative criminal records from Oklahoma, I find that UPK reduces the likelihood of conviction in early adulthood by 1.3 percentage points (35%), with larger effects for black children.

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I. Introduction

In the last two decades, campaigns to expand access to prekindergarten (Pre-K) have achieved remarkable success. The fraction of four year olds attending state-based Pre-K has more than doubled since 2002 (Friedman-Krauss et al., 2018) representing "one of the most significant expansions in public education in

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the 90 years since World War I".[1] This dramatic expansion followed the adoption by many state and local policymakers of the view that critical economic and social problems in adulthood can be traced to cognitive and socio-emotional skill deficits that begin during early childhood and compound as a child ages. For these policymakers, Pre-K became an attractive policy tool to alleviate these early deficits, producing potentially large improvements by adulthood at relatively low cost.

Increasingly, many states and cities have elected to make their Pre-K programs universal rather than targeted toward children from low-income families. While there is a large research literature on the long-run adult impacts of early education interventions, it is limited to preschool programs targeted toward children at high-risk of poor adult outcomes. This body of evidence includes quasi-experimental evaluations of the large-scale federal Head Start program (Deming, 2009; Ludwig and Miller, 2007; Garces, Thomas and Currie, 2002; Barr and Gibbs, 2017) and the smaller Chicago Child-Parent Centers program (Reynolds and Ou, 2011), as well as experimental evaluations of small-scale but resource-intensive pilot programs like HighScope Perry preschool (Heckman et al., 2010; Heckman, Pinto and Savelyev, 2013) and the Abcedarian Project (Campbell et al., 2012). However, results from these programs may not generalize to universal Pre-K (UPK) for a number of reasons. First, UPK likely includes children with fewer early childhood disadvantages, which may make them less sensitive to an intervention. Second, counterfactual childcare options are likely to be substantially better for UPK children compared to children in earlier studies of targeted programs. This could arise from differences in childcare options by socio-economic status as well as overall improvements over time (many of the earlier studies use variation from the 1960s). An improved counterfactual may reduce the impact of more recent universal interventions, relative to earlier targeted interventions.

[1] Deborah Solomon. "As States Tackle Poverty, Preschool Gets High Marks". The Wall Street Journal(Aug. 2007).url:https://www.wsj.com/articles/SB118660878464892191.

Third, given its large scale, the quality of UPK may differ from resource-intensive pilot programs like Perry and Abcedarian. Evidence regarding the effectiveness of universally available programs has thus far been restricted to intermediate (i.e. elementary and middle school) outcomes, finding mixed results (Fitzpatrick, 2008; Gormley et al., 2005*b*; Gormley, Phillips and Anderson, 2018; Lipsey et al., 2013; Bartik and Hershbein, 2018).

In this paper, I provide the first evidence of the long-run effects of universal pre-K. I concentrate on a negative indicator of adult success: criminal conviction in early adulthood (age 18-22).[2] In addition to being correlated with other measures of an individual's success in adulthood (e.g. income and employment), criminal conviction is an individual outcome tied to a large social cost. In fact, crime reduction and its associated social cost played a central role in generating Heckman et al.'s widely publicized estimates of large social returns from a targeted early childhood education intervention (roughly 40 to 65% of the social benefits). Despite their importance to the broader debate, there are no estimates of UPK's effects on criminal outcomes, and even among evaluations of targeted programs the results have not been consistent. Previous studies find null effects for the Abcedarian Project (Campbell et al., 2012), mixed results by cohort for Head Start (Garces, Thomas and Currie, 2002; Deming, 2009), and large reductions for Perry Preschool (Heckman et al., 2010) and Chicago Child-Parent Centers (Reynolds and Ou, 2011).

I focus my analysis on the implementation of universal Pre-K in Oklahoma, an early high-profile example widely seen as a model for high-quality UPK due to its rigorous standards and relatively high teacher pay. To identify the impact of Oklahoma's UPK on later criminal convictions, I use a regression discontinuity design which leverages the birthdate cutoff in access to UPK produced by the

[2]Due to the recent nature of UPK expansion and data availability constraints, I am only able to observe relevant cohorts through age 22.

Kindergarten age cutoff in the program's first year of implementation. This approach compares the outcomes of children born just before the cutoff (exposed to UPK policy) with those born just after the cutoff (never exposed to UPK policy). It yields estimates of the effect of Oklahoma's UPK policy (or the intent-to-treat effect of state-run Pre-K) compared to the prior mix of preschool services.

Using administrative criminal records from Oklahoma along with birth records, I find that access to UPK reduces the likelihood that an individual will be convicted of a crime in Oklahoma between age 18 and 22 by 1.3 pp or 35% of the overall mean (statistically significant at the 5 percent level).[3] This estimate implies a treatment-on-treated effect size for Pre-K in Oklahoma that is roughly one third of Perry Preschool (Heckman et al., 2010).[4] Investigating the heterogeneity of UPK's impacts, I find larger effects on black children, a population in Oklahoma that is at much higher risk of criminal conviction in early adulthood.

I provide a number of additional analyses to support the internal validity of these main results. First, I check for robustness to alternative specifications and bandwidth selections. Second, I find no evidence of discontinuities in criminal convictions around the Kindergarten birthdate cutoff in years without a UPK policy contrast. This suggests that it is the UPK policy and not other differences across Kindergarten birthdate cutoffs (e.g. age relative to classmates) that is producing large reductions in conviction rates. Third, I run my primary specification for more than one thousand placebo cutoff dates (each date in three years) to determine that my true estimate is larger in magnitude than all but 2% of the placebo estimates. This strategy provides an alternative approach to inference and confirms that my main results are highly unlikely to be produced by chance.

[3]This estimate appears large compared to the baseline conviction rate, but this comparison is somewhat misleading. The actual increase in Pre-K enrollment was heavily skewed toward poor neighborhoods where children likely had higher baseline rates (Figure 4).

[4]This may overstate the treatment-on-treated effect of Pre-K in Oklahoma if there are substantial spillovers onto children that don't attend Pre-K, such as through peer effects on criminal behavior.

II. Existing Evidence on Long-run Preschool Effects on Crime

Viewing crime within a human capital framework implies that increases in human capital lead to increases in the opportunity cost of crime and therefore reductions criminal activity (Lochner and Moretti, 2004). In this framework, if early childhood education programs succeed in increasing human capital accumulation by adulthood they should yield reductions in criminal activity. The existing evidence on these long-run criminal effects is surprisingly inconsistent, even across studies of similar programs. The literature on the long-run impacts of early childhood education can be broadly categorized as evaluating three types of preschool programs: small pilot programs targeted to the most disadvantaged children, larger scale targeted programs, and state-run universal Pre-K programs. Evidence of potentially large long-run effects of preschool programs comes from studies of the targeted programs, while studies of universal Pre-K programs obtain mixed results for shorter run outcomes.

A. Targeted Preschool Pilot Programs

Two small-scale, high-intensity single-site early education interventions provide much of the frequently cited evidence of the long-run impacts of early education: the HighScope Perry Preschool Program and the Abcedarian Project. Perry Preschool was a program for three and four year olds conducted in Ypsilanti, Michigan during the 1960s, where children received 2.5 hours of preschool each school day and weekly home visits from teachers at a cost of $20,854 per student in inflation-adjusted 2015 dollars (Barnett, 1996). 123 children judged to be disadvantaged by family socioeconomic status and IQ scores were randomly assigned to the program or the control group. Heckman et al. (2010) uses data that includes periodic follow-up interviews to age 40 to find that Perry Preschool produced an annual social rate of return of 7-10%. This large estimated return

was a product of the program's beneficial effects on criminal, welfare, and earnings outcomes, the bulk of which were mediated by persistent changes in personality skills (i.e. reduced externalizing behavior) rather than changes in cognitive skills or academic motivation (Heckman, Pinto and Savelyev, 2013).

Perry Preschool's impact on crime played a central role in generating its large social returns, accounting for roughly 40-65% of the benefits of the program Heckman et al. (2010). Among men, the program caused an average reduction in the number of arrests by age 40 of 4.2, and a 13 percentage point reduction in the likelihood of arrest by age 40. However, it is uncertain whether similarly large impacts of the program could be expected in other contexts, given the extent to which the Perry sample was selected on disadvantage. For example, 37 of 39 (95%) men assigned to the control group in the Perry study were arrested by age 40 (12.4 average arrests).

The Abcedarian Project was similar to Perry Preschool in its scale (111 children), its focus on disadvantaged children, and its use of random assignment. Children assigned to the Abcedarian treatment group attended an educational child care program from infancy to the start of Kindergarten (mean entry age was 4.4 months). The preschool component of the program cost roughly $22,000 per child per year (2015 dollars). Campbell et al. (2012) find a substantial effect on educational attainment at age 30 from Abcedarian, but no effect on the likelihood of criminal conviction by age 30.[5]

B. Large-scale Targeted Preschool Programs

The federal Head Start and the Chicago Child-Parent Centers (CPC) programs also targeted disadvantaged children, but at a much larger scale. This

[5]The differences in crime effects between Perry Preschool and Abcedarian are not directly comparable, since studies of the former observe arrests while studies of the latter observe convictions.

larger scale alleviates some of the external validity and scalability concerns with the smaller pilot programs. The best long-run evidence on these programs comes from quasi-experimental studies, again yielding inconsistent results on their crime effects.

Head Start began as a federal summer program for low income children in 1966 and expanded to a full-year program by the early 1970s. The program currently costs between $8,000 and $10,000 per child (2015 dollars), much less than Perry Preschool or Abcedarian, and enrolls 900,000 children (Deming, 2009). While quality standards for Head Start are set at the federal level, the program is administered locally, leading to substantial heterogeneity in implementation quality across localities and over time.

Two studies, Garces, Thomas and Currie (2002) and Deming (2009) identify the long-run effect of Head Start by comparing siblings who attended the program with those who did not, and assuming siblings do not differ systematically.[6] Garces, Thomas and Currie use the 1964-1977 birth cohorts of the Panel Survey of Income Dynamics. They find dramatic reductions in the likelihood of facing criminal charges, but only for black children (12 percentage points).[7] Deming looks at a later cohort of children born in the early 1980s using data from the National Longitudinal Mother-Child Supplement and finds no impact on later crime.[8,9]

The CPC program began in 1967 and was designed to provide educational and family support (including preschool) to children in high-poverty neighbor-

[6]Ludwig and Miller (2007) employ an alternative approach to examine the effects of Head Start on its early cohorts. Using a regression discontinuity design which compares counties around an eligibility threshold for grant writing assistance in 1965, they find that Head Start reduces childhood mortality rates and possibly increases educational attainment. They do not observe any criminal outcome measures.

[7]Garces, Thomas and Currie (2002) also find that Head Start increases educational attainment, but only for white children.

[8]Deming (2009) measures crime differently than Garces, Thomas and Currie (2002), as an indicator equal to one if an individual is currently incarcerated or reports having been convicted, sentenced, or on probation.

[9]Deming (2009) finds that Head Start participation increases a summary index of later adult outcomes by 0.23 standard deviations, despite the fadeout of short-run cognitive effects.

hoods in Chicago that did not have access to Head Start.[10,11] Comparing CPC participants with a non-experimental comparison group of children in Chicago (and controlling for covariates), Temple and Reynolds (2007) find that CPC preschool participants were 4.6 percentage points (22%) less likely to have been arrested for a felony by age 24.[12]

C. State-run Universal Pre-K

To date, there is no evidence on the impacts of state-run universal Pre-K programs on later adult criminal outcomes. There have, however, been shorter-run impact studies nationally (Bartik and Hershbein, 2018) and in a number of individual states, including Georgia (Fitzpatrick, 2008), Tennessee (Lipsey et al., 2013), and Oklahoma (Gormley et al., 2005*b*, 2011; Gormley, Phillips and Anderson, 2018). Bartik and Hershbein (2018) and Fitzpatrick (2008) use panel data from the National Assessment of Educational Progress to find no overall effect on test scores nationally from enrollment in an average-quality Pre-K program or in Georgia from access to Georgia Pre-K. However, both find substantial heterogeneity in the impact of Pre-K. For example, Bartik and Hershbein (2018) find positive math effects from high-quality Pre-K and large math and reading effects in majority-black districts. Lipsey et al. use random assignment of students applying to over-enrolled Pre-K programs in Tennessee to find that Pre-K improved achievement by the end of the Pre-K year. However, these gains were lost in the following year.

A number of studies have investigated the effect of Pre-K in Oklahoma on student outcomes. Gormley et al. (2005*b*) use a birthdate regression discontinu-

[10] Preschool teachers are required to have a college degree and child-to-staff ratios are relatively low, 17:2 (Reynolds and Ou, 2011).

[11] In 1985, the average cost per child of the preschool component was $9,636 in 2015 dollars (Reynolds and Ou, 2011).

[12] They also find that CPC participants were less likely to have been incarcerated, more likely to be employed, more likely to have a high school degree, and less likely to have depressive symptoms.

ity approach which leverages a strict cutoff in whether students were eligible to attend Tulsa's Pre-K program in 2000 or 2001. Since students on either side of this cutoff both eventually receive the same access to Pre-K, Gormley et al. are limited to comparing the outcomes of Kindergarten students who have just finished Pre-K with students who are just starting Pre-K. They find a 0.39 standard deviation increase in cognitive test scores for those who have attended Pre-K. The impact is concentrated among blacks and Hispanics, with little effect on whites. Gormley et al. (2011) and Gormley, Phillips and Anderson (2018) use propensity score matching to examine Pre-K impacts on later student outcomes in Tulsa and surrounding districts. They find that Pre-K participation was associated with higher attentiveness and lower timidity ratings in Kindergarten as well as improvements in middle school test scores and grade retention. The effects on socio-emotional measures, in particular, suggest potentially lower rates of future delinquency in adolescence (Moffitt, 1990).

III. Oklahoma Universal Pre-K

Universal Pre-K (UPK) in Oklahoma differs from other early state pre-K programs in its centralization, scale, and quality. In Oklahoma, Pre-K functions as an optional additional grade within the state's public education system rather than as a decentralized system of public and private centers. This is evident in three important features of Oklahoma's Pre-K provision. First, the majority of students attend school-based classrooms rather than independent centers. Second, four year olds are included in the formula for allocating state funds to districts rather than centers applying for funds directly from the state. Third, quality standards with regard to teachers and class sizes are enforced similarly for Pre-K and K-12. Pre-K teachers are required to be certified in early childhood education and paid at the same rate as other teachers, while classroom adult-to-child ratios may not exceed 1:10. These quality standards come with a higher

per student price tag than preschool programs in many other states, $8,024 per student in 2018 (Friedman-Krauss et al., 2018).[13] This per student cost is similar to Head Start and CPC, but less than half that of Perry Preschool or Abcedarian.

Oklahoma's UPK program began in the 1998-99 school year, expanding a small pilot program to all four year olds in the state, while also increasing the per-pupil funding for four year olds in the state funding formula. The resulting large increase in statewide Pre-K enrollment of 28% for white children and 35% for black children is depicted in Figure 2.[14] Figure 3 shows the share of four year olds in Oklahoma enrolled in Pre-K, Head Start, private preschool, or no preschool before and after the implementation of UPK.[15] Pre-K enrollment did not simply replace Head Start, rather it appears that roughly two-thirds of the additional Pre-K enrollment was drawn from children who would not have attended any preschool, while one-third crowded out private preschool. Unsurprisingly, this increase in Pre-K enrollment was much more dramatic for students in poorer neighborhoods (and therefore at higher risk of future encounters with the criminal justice system). Figure 4 shows this negative relationship between neighborhood median family income and the increase in Pre-K enrollment rate from 1997-98 to 1998-99.[16,17]

[13]In Georgia, the other state with a major universal pre-K program in the 1990s, pre-K was provided through a mix of public schools and private subcontractors. There was no comprehensive curriculum standards or college degree requirement for teachers. The state spending per student in 2018 was $4,411 (Friedman-Krauss et al., 2018).

[14]This figure uses school-level data from the Common Core of Data's (CCD) Public Elementary/Secondary School Universe Survey, administered by the National Center for Education Statistics (NCES). The Pre-K enrollment rate in year t is defined as the aggregate state Pre-K enrollment in year t divided by the aggregate state Grade 1 enrollment in year t+2.

[15]This figure is constructed using data from the October CPS for Oklahoma in the two years prior to UPK implementation and two years afterward. The four year olds observed attending "public preschool" in the CPS are allocated to Head Start and Pre-K based on each program's share of the statewide total enrollment in both programs in the relevant years (Head Start enrollment is obtained from Kids Count Data Center and Pre-K enrollment is obtained from the Common Core of Data).

[16]The top panel depicts a locally weighted regression of percent Pre-K enrollment on zip code median income (2000 Census) for 1997-98 and 1998-99, where the school zip code is the unit of observation. The bottom panel shows the grade 1 enrollment distribution by school zip code median income.

[17]Similarly, Cascio and Schanzenbach (2014) find a much larger increase in preschool enrollment among children with less-educated mothers in response to universal Pre-K implementation in Georgia and Oklahoma. They use a difference-in-difference approach with data from the October Current Population Survey.

IV. Criminal Conviction Data

To measure the long-run impact of universal Pre-K availability on criminal outcomes in Oklahoma I construct a measure of the number of unique individuals born on a given date that are convicted of a crime between ages 18 and 22 ("Conviction Count"). I define convictions as those that are serious enough to necessitate supervision by the Oklahoma Department of Corrections (ODOC), typically in the form of incarceration or probation. I construct this measure using ODOC administrative data with exact birthdate, race, and crime type for all offenders with convictions that meet this criteria.[18] I divide this conviction count by an estimate of the number of individuals born in Oklahoma on a given date to obtain a measure of the likelihood of conviction at age 18 to 22 for a given birthdate cohort. The estimate of daily births to mothers residing in Oklahoma is derived from National Center for Health Statistics public-use natality data.[19] I repeat the construction of both the conviction count and conviction rate by birthdate separately for crime category (Violent or Property Crime) and by race (using mother's race from birth records).[20] Table 1 shows the summary statistics for my primary data sample (120 days centered around September 1 in 1991-1994).

V. Empirical Strategy

I estimate the impact of Oklahoma's universal pre-K program (UPK) on later criminal behavior using a regression discontinuity (RD) design. This strategy yields estimates of the effect of exposure to the UPK policy (i.e. access to

[18]This database was made publicly available via a FOIA request by the Center for Investigative Journalism.

[19]Only information on birth year, month, and day of the week is publicly available in this dataset. I impute the number of births on a given date by dividing the number of births in a year-month-day of the week bin by the number of days in that bin. I find no change in my results when I account for reduced births on holidays by deflating the births on these days by the average difference between births on Sundays and weekdays (and adjusting the other days in the same bin).

[20]Violent and Property Crime categories are based on the FBI's Type I property and violent crime definitions.

UPK), or equivalently the intent-to-treat effect of state Pre-K (compared to the prior mix of preschool services in Oklahoma).[21] To identify this effect, I leverage the contrast in UPK exposure between children just below and just above the Kindergarten birthdate cutoff in the first year of UPK implementation.

Students in Oklahoma must be 5 years old on September 1 to attend Kindergarten in the public school system. This creates a birthdate cutoff where children born on or before September 1 in a given year are assigned to a different school cohort than children born after September 1. Figure 1 shows how early schooling experiences of children differed across this birthdate cutoff in the years surrounding the implementation of universal Pre-K (UPK). In the 1998-99 school year, the first year of UPK implementation, a child born prior to September 1, 1993 (Child C) meets the Kindergarten birthdate cutoff and therefore attends Kindergarten in 1998-99. Therfore, she is never exposed to UPK because it was not available in the prior year. On the other hand, a child born after September 1, 1993 (Child D) is not old enough for Kindergarten and is therefore exposed to UPK in 1998-99. I leverage this contrast in UPK exposure between Child C and Child D to identify the impact of UPK using a regression discontinuity design.

My primary specification uses a 60 day bandwidth of birthdates on either side of the Kindergarten birthdate cutoff in the first year of UPK implementation and estimates the discontinuity in the likelihood of later criminal charges at the cutoff (September 1, 1993). In essence, this compares the criminal outcomes of children born just before and just after the birthdate cutoff (Child C and Child D in Figure 1). The regression specification is as follows,

$$Y_b = \alpha + \beta 1(z_b > 0) + f(z_b) + 1(z_b > 0) \times f(z_b) + \epsilon_b \tag{1}$$

where Y_b is the conviction rate (or count) for children born on date b who face

[21] See Figure 3 for the prior mix of preschool services.

criminal charges at ages 18 to 22 and z_b is the difference between birthdate b and the cutoff date for Kindergarten in the 1998-99 school year (September 1, 1993). $1(z_b > 0)$ is an indicator equal to one for children born after the kindergarten birthdate cutoff and therefore exposed to UPK. $f(z_b)$ is a function of z_b (linear in my preferred specification). The specification allows the function to vary on either side of the eligibility threshold by interacting $f(z_b)$ and $1(z_b > 0)$. The coefficient of interest, β, can be interpreted as the effect on children who just missed the Kindergarten cutoff and were therefore exposed to the UPK policy.

A. Threats to Internal Validity

Fundamental to this identification strategy is that assignment to UPK access is as-good-as-random at the birthdate cutoff. Parents manipulating the timing of their child's birth in order to access UPK would violate this assumption. This sort of manipulation would not have been feasible, however, as any decisions related to birth timing would have occurred well before the announcement of UPK in Oklahoma (more general age cutoff birth timing is addressed below). While I have found no evidence that parents or schools were able to circumvent the state age rules on UPK access, if this sort of manipulation took place it would bias my estimates of the impact of UPK access toward zero.

A related internal validity concern is that children on either side of the UPK access cutoff differ in their relative age to their classmates as well as differing in their access to UPK. Specifically, Child C has access to UPK and is the youngest in her cohort while Child D is the oldest in her cohort and does not have access to UPK. This difference is potentially problematic for my identification strategy if age relative to one's classmates affects later criminal outcomes. Prior evidence in the United States finds that children born after enrollment cutoffs attain lower levels of schooling (Angrist and Krueger, 1991; Cascio and Lewis, 2006; McCrary and Royer, 2011). Given the connection between education and crime (Lochner

and Moretti, 2004), this would suggest a possible positive bias in my estimate of the effect of UPK eligibility on later criminal activity. In other words, the difference in relative ages across the UPK eligibility cutoff may, if anything, work against finding that UPK access reduces criminal activity.

I use placebo regression discontinuities in years where there is no policy contrast at the Kindergarten birthdate cutoff to directly test whether relative age differences, or other differences across these cutoffs (e.g. skill differences due to birth timing by higher-income families), could be driving my results. For example, in 1997-98 (prior to UPK implementation) Child A in Figure 1 (born prior to September 1, 1992) attends Kindergarten in 1997-98 as the youngest in her grade, while Child B (born after September 1, 1992) attends Kindergarten in 1998-99 as the oldest in her grade. This means that Child A and B differ in their ages relative to their respective classmates, but neither are exposed to the UPK policy. Finding a substantial "effect" at this cutoff would suggest that my main results may be driven by relative age (or other differences around the cutoff) rather than the UPK policy.

Differential exit from the state at the UPK access cutoff could also be problematic for the internal validity of my emprical approach. Since I only observe criminal charges for those that stay in the state, differential exit would affect the number of individuals whose convictions could potentially be observed. Figure A2 shows the percent of individuals born in Oklahoma who reside in Oklahoma by adulthood using data from the American Community Survey. While the measure of birth timing (quarter of birth) is coarse, the figure does not show any evidence of a reduction in the likelihood of staying in Oklahoma for those who had access to UPK (born after September 1, 1993).

VI. Results

Table 2 shows least squares regression discontinuity estimates of the impact of UPK access on adult criminal activity. Estimates of β in Equation 1 are presented by school year for badwidths of 60 days on either side of the Kindergarten birthdate cutoff in the given school year.[22] The first school year of implementation for UPK was 1998-99, therefore this is the *only* year where there is a major policy contrast at the birthdate threshold (See Figure 1). The first panel shows results for the conviction rate, the likelihood of any conviction from age 18 to 22, and the second panel shows results for the conviction count, the number of individuals with any conviction from age 18 to 22.[23] I find a statistically significant drop of 1.3pp (35%) in the conviction rate and 2.0 (41%) in the conviction count at the Kindergarten birthdate cutoff in the first year of UPK (Column 3).[24] I find no effect on conviction rates or counts at the Kindergarten birthdate cutoff in other (placebo) years. Figure 5 (and Figure A3) presents these results graphically, showing the conviction rate (or count) by 10-day bin for 60 days on either side of the Kindergarten cutoff in each school year. The absence of a drop in conviction rate (or count) in years where there was not a substantial policy contrast suggests other differences across the kindergarten birthdate threshold (e.g. relative age) are not driving the effects in the first year of UPK.

Table 3 and Figure 6 show the main regression discontinuity estimates for the first year of UPK under various bandwidth choices (days on either side of the kindergarten eligibility threshold).[25]

[22] Estimates are weighted by the imputed number of births on a given date.

[23] Given the timeframe available in the conviction data, I am only able to observe convictions through age 21 for children born around the 1999-00 Kindergarten birthdate threshold. Therefore the conviction rate and count for column 4 is defined for age 18 to 21.

[24] Using a local linear regression with a triangular kernel and data-driven bandwidth selection (Calonico, Cattaneo and Titiunik, 2014) yields similar results with slightly larger standard errors (Conviction rate: -0.013 with 0.008 SE; Conviction count:-2.00 with 1.15 SE). This is also the case for using a quadratic rather than a linear function for $f(z_b)$ (Conviction rate: -0.012 with 0.009 SE; Conviction count:-1.97 with 1.30 SE).

[25] MSE-optimal bandwidth selections are 55.3 for conviction rate and 57.8 conviction count (Calonico,

In Figure 7, I use randomization as an alternative method of inference for the conviction rate results.[26] I assign each date from March 1991 to March 1994 as a placebo threshold and estimate Equation 1 (with a 60 day bandwidth) separately for each placebo threshold. Figure 7 depicts the distribution of these placebo regression discontinuity estimates. The estimate for the true UPK access threshold is shown in red (and the kindergarten cutoff in earlier years is shown in gray). Only 2 percent of the placebo estimates are larger in magnitude than the true estimate of access to UPK (Table 2 Column 3), implying a p-value 0.02. This means that the drop in criminal activity observed at the UPK access cutoff using Equation 1 is highly unlikely to be observed by chance. Furthermore, prior school year Kindergarten eligibility cutoffs have p-values of 0.48 and 0.93, suggesting that the drops in criminal activity observed at these thresholds are in line with what would be expected by chance.[27].

A. Effect Size and Heterogeneity

The estimate in Table 2 (Column 3) represents the effect of the UPK policy, or equivalently, the intent-to-treat effect of state-run Pre-K. A back-of-the-envelope calculation using the increase in Pre-K enrollment rates suggests a 4pp treatment-on-treated (TOT) effect of attending state-run Pre-K in Oklahoma. This effect size appears large compared to the baseline conviction rate, but this comparison is somewhat misleading. The actual increase in Pre-K enrollment was heavily skewed toward poor neighborhoods where children likely had higher baseline rates (Figure 4). Furthermore, the estimate is similar to the effect size from the CPC program (4.6pp reduction in felony arrest by age 24) and one-third of the effect size from Perry Preschool (12 percentage points on arrest by age 27).

Cattaneo and Titiunik, 2014).

[26] Figure A4 shows the same randomization inference analysis for the conviction count results. It yields a p-value of 0.01.

[27] Conviction data through age 22 is not available for the birth cohorts around the 1999-00 Kindergarten cutoff, therefore this cutoff is not included in the graph

This calculation may over-estimate the TOT effect of Pre-K if Oklahoma's UPK policy produced indirect spillover effects on those who did not attend Pre-K. There are a number of possible mechanisms for this to have occurred. For example, there is strong evidence of peer effects in juvenile criminal behavior (Bayer, Hjalmarsson and Pozen, 2009). This suggests that individuals who did not attend Pre-K may have reduced their criminal behavior as a result of having "less criminal" peers due to peer Pre-K attendance.

I investigate the heterogeneity of the main regression discontinuity conviction rate results by race and crime type in Tables 4 and 5. In Table 5, I find no notable difference in effects by type of crime. In Table 4, I find that the reduction in conviction rate is dramatically larger for black children than white children (even when accounting for the difference in the baseline rate). Figure A5 shows these results graphically, while Figure A6 shows robustness to bandwidth selection. These differential effects by race are consistent with other findings that black students saw larger socio-emotional (Gormley et al., 2005*b*) and academic (Bartik and Hershbein, 2018) improvements from Pre-K and larger crime reductions from Head Start (Garces, Thomas and Currie, 2002).

VII. Conclusion

I leverage a birthdate cutoff in eligibility for Oklahoma's UPK program in the first year of its implementation to estimate the effect of its UPK policy on later criminal convictions in early adulthood. I find a substantial reduction in the likelihood criminal conviction at age 18-22 (1.3pp) that is driven by a large effect on black children (6.1pp). As the first estimates of the long-run impacts of UPK, these results are an important first step towards bridging the disconnect between the policy debate around universal Pre-K and the evidence of its potential long-run effectiveness. The results suggest that UPK can, like small-scale targeted and resource-intensive programs, have dramatic effects on later criminal outcomes,

but these effects appear to be concentrated among a population at greater risk of interaction with the criminal justice system. As with Perry Preschool, these large crime reductions are likely to yield a major share of the social benefits of UPK in future long-run benefit-cost analyses.

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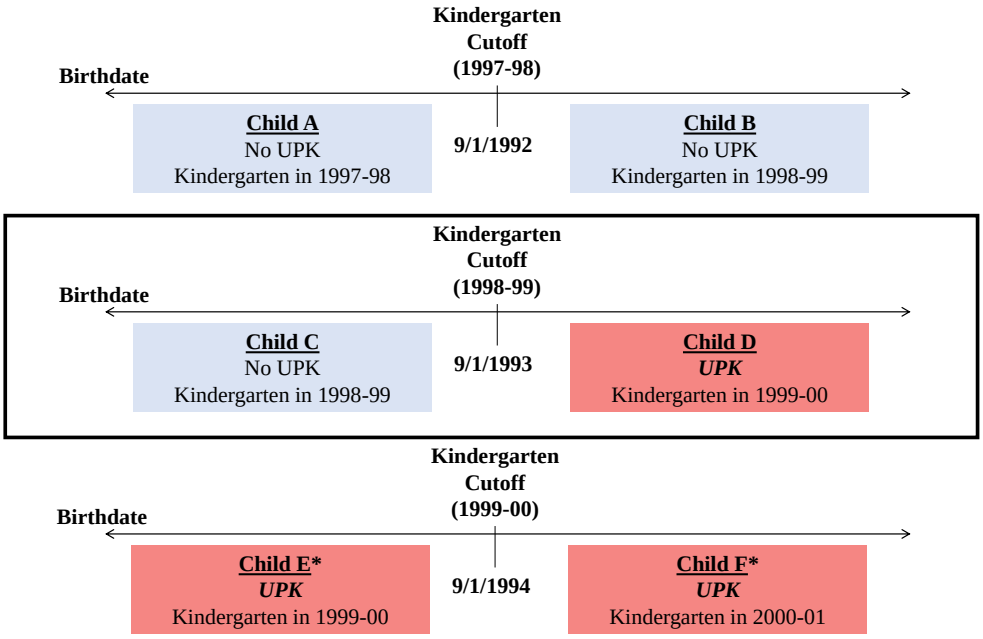
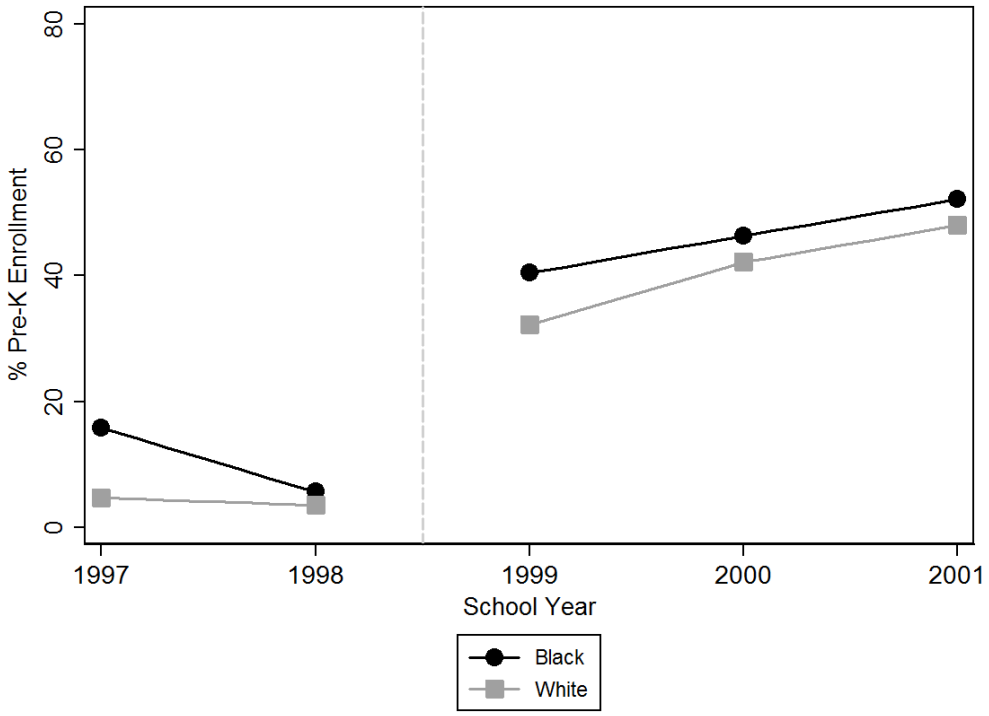
Figure 1. Variation in Exposure to Oklahoma's UPK



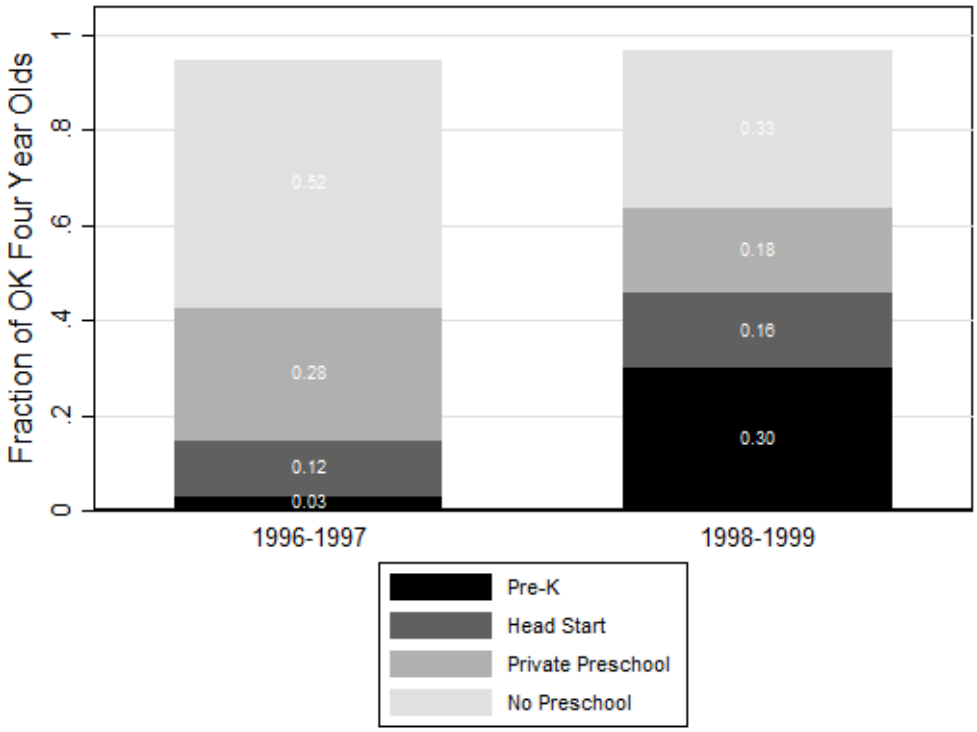
Note: Criminal conviction outcomes are only available through age 21 (not 22) for children born around the the 1999-00 Kindergarten cutoff (Child E and F).

Figure 2. Oklahoma Pre-K Enrollment Rate over Time, by Race



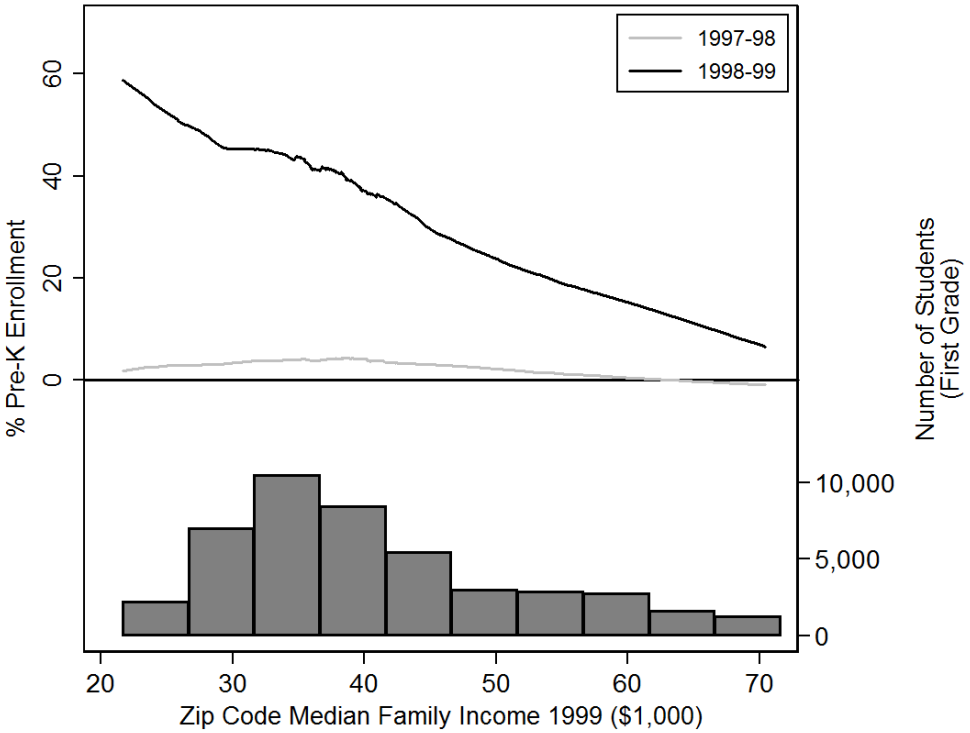
Note: School-level Pre-K enrollment for Oklahoma is obtained from the School Universe Survey of the Common Core of Data produced by the National Center for Education Statistics. % Pre-K enrollment is defined as the Pre-K enrollment in year t divided by the Grade 1 enrollment in t+2. Pre-K enrollment by grade and race is not available prior to 1998-99, therefore it is imputed for all years by multiplying Pre-K enrollment in each school by the fraction of school enrollment of a given race. These enrollment counts are then aggregated to the state level.

Figure 3. Educational Experiences of Oklahoma Four-Year-Olds



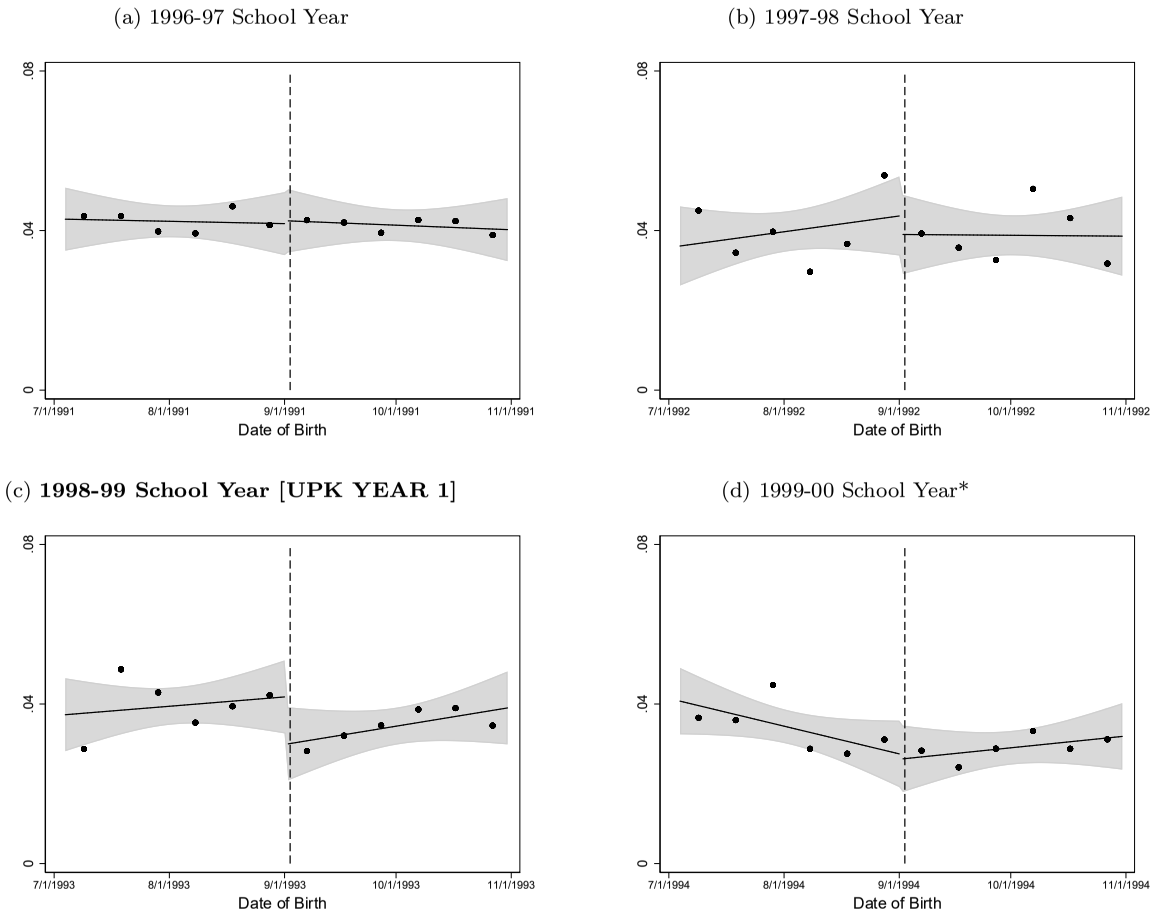
Note: Data is obtained from the October Current Population Survey (CPS) for Oklahoma in the two years prior to UPK implementation and two years post (October 1998 falls in the 1998-99 school year). The four year olds observed attending "public preschool" are allocated to Head Start and Pre-K based on each program's share of the statewide total enrollment in both programs in the relevant years (Head Start enrollment is obtained from Kids Count Data Center and Pre-K enrollment is obtained from the Common Core of Data).

Figure 4. Oklahoma Pre-K Enrollment by Zip Code Median Family Income



Note: The top panel depicts a locally weighted regression of %Pre-K enrollment on zip code median family income (seperately for 1997-98 and 1998-99), where the elementary school building zip code is the unit of observation. The bottom panel shows the distribution of first grade students by zip code median family income. School-level Pre-K enrollment for Oklahoma is obtained from the School Universe Survey of the Common Core of Data produced by the National Center for Education Statistics. % Pre-K enrollment is defined as the Pre-K enrollment in year t divided by the Grade 1 enrollment in t+2.

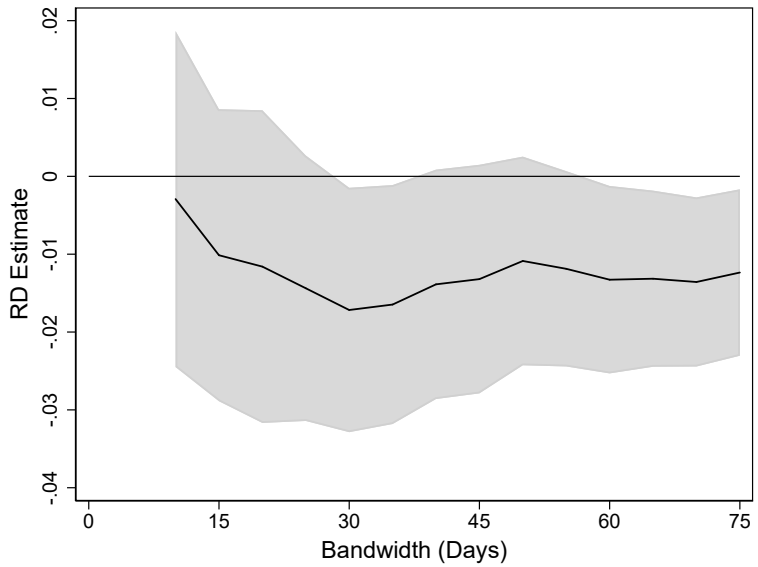
Figure 5. Birthdate Cohort Conviction Rate by School Year



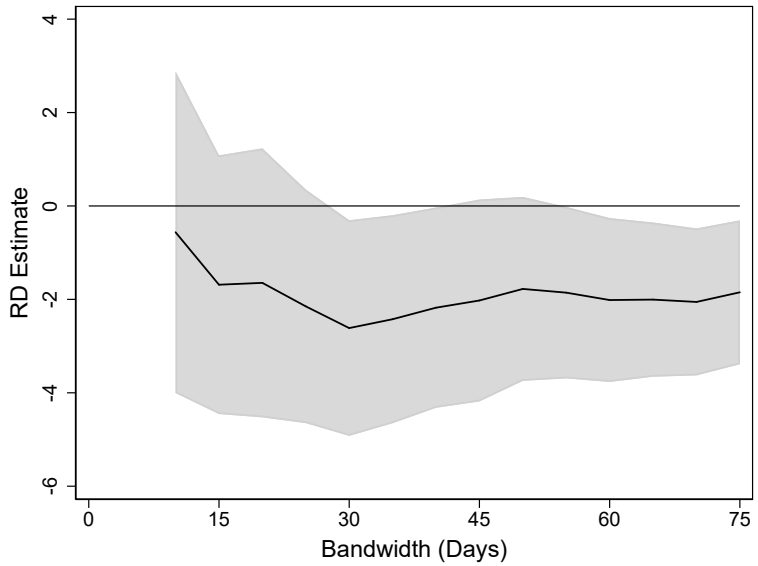
Note: Dots represent means across 10 day bins. Conviction rate for a given birthdate cohort is constructed as the number of unique individuals with that birthdate who were convicted of a crime in Oklahoma between age 18 and age 22 (Conviction Count) divided by the number of births on that date (imputed using Oklahoma birth records). Observations are at the birthdate cohort-level. In Oklahoma, the birthdate cutoff for Kindergarten is September 1. Children born after September 1, 1993 had access to UPK, and so the only year with a major contrast in UPK access at the Kindergarten cutoff was 1998-99 [UPK Year 1]. For children born near the 1999-00 Kindergarten cutoff, conviction data is only available for age 18 to 21.

Figure 6. RD Estimate of Effect of UPK Access, by Bandwidth

(a) Conviction Rate

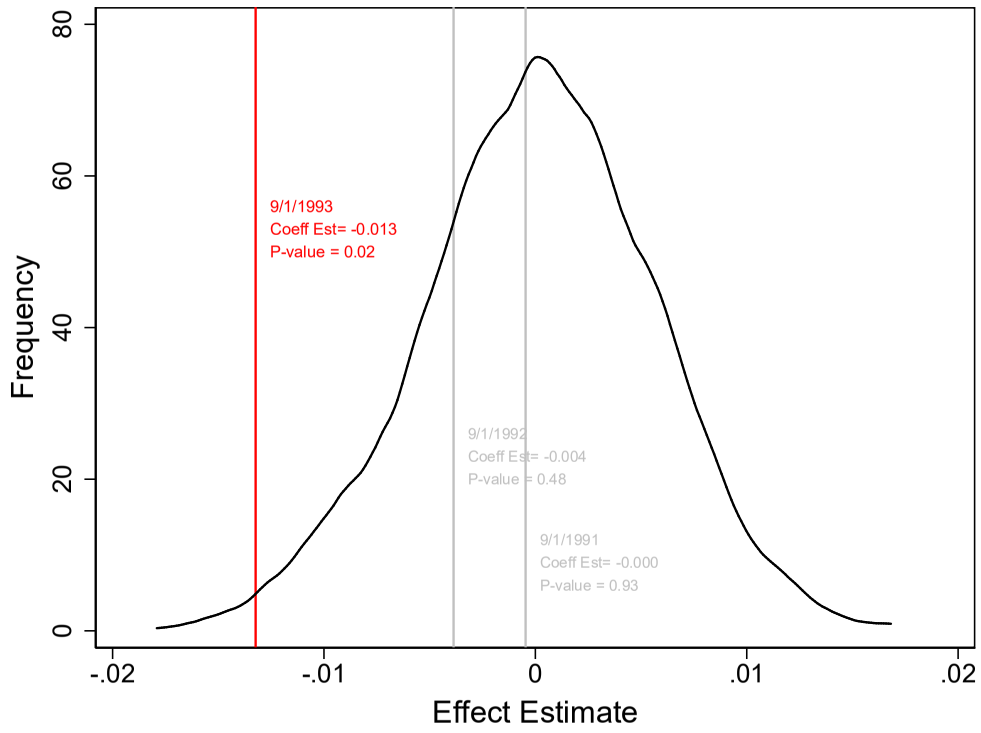


(b) Conviction Count



Note: Figure depicts RD estimates (Equation 1) for various bandwidths (days on either side of the September 1, 1993 UPK access birthdate cutoff). 95% confidence intervals are shaded in grey. The dependent variable in each regression is either criminal conviction rate or criminal conviction count for any crime.

Figure 7. Random Inference for RD Estimate of Effect of UPK Access on Conviction Rate



Note: Figure depicts the distribution of coeffient estimates based on placebo Kindergarten birthdate cutoffs. Equation 1 is estimated using each date between March 1991 and March 1994 as the placebo cutoff. The coefficient estimate for the true UPK access cutoff in 1998-99 (Year 1 of UPK) is shown in red. The coefficent estimates for the Kindergarten cutoff in prior years (with no contrast in access to Pre-K) are shown in grey. The dependent variable in each regression is the criminal conviction rate for any crime.

Table 1—Summary Statistics

	All (1)	White (2)	Black (3)
Birthdays	120	120	120
Individuals	15,556	12,163	1,622
Fraction w/ Any Conviction	0.037	0.021	0.105
Fraction w/ Violent Crime Conviction	0.011	0.004	0.041
Fraction w/ Property Crime Conviction	0.014	0.008	0.041

Note: Conviction rate for a given birthdate cohort is constructed as the number of unique individuals with that birthdate who were convicted of a crime in Oklahoma between age 18 and age 22 (Conviction Count) divided by the number of births on that date (imputed using Oklahoma birth records). Observations are at the birthdate cohort-level. The sample contains birthdates within 60 days of the kindergarten birthdate cutoff for the 1998-99 schoolyear. Crime categories are based on the FBI's Type I property and violent crime definitions. Therefore, some crimes are not classified as property or violent crimes.

Table 2—Regression Discontinuity by Year

			(UPK Year 1)	
	1996-97	1997-98	1998-99	1999-00*
	(1)	(2)	(3)	(3)
Conviction Rate	-0.000	-0.004	-0.013**	-0.001
	(0.006)	(0.007)	(0.006)	(0.005)
Mean	*0.042*	*0.039*	*0.037*	*0.032*
Obs	*120*	*120*	*120*	*120*
Conviction Count	0.391	-0.370	-2.014**	-0.249
	(0.777)	(0.860)	(0.895)	(0.675)
Mean	*5.700*	*5.192*	*4.833*	*4.000*
Obs	*120*	*120*	*120*	*120*

Note: Each cell presents estimates of Equation 1 from separate least squares regressions. The dependent variable in each regression is either criminal conviction rate or criminal conviction count for any crime. Conviction rate for a given birthdate cohort is constructed as the number of unique individuals with that birthdate who were convicted of a crime in Oklahoma between age 18 and age 22 (Conviction Count) divided by the number of births on that date (imputed using Oklahoma birth records). Observations are at the birthdate cohort-level. In Oklahoma, the birthdate cutoff for Kindergarten is September 1. Children born after September 1, 1993 had access to UPK, and so the only year with a major contrast in UPK access at the Kindergarten cutoff was 1998-99 [UPK Year 1]. For children born near the 1999-00 Kindergarten cutoff, conviction data is only available for age 18 to 21. The sample contains birthdates within 60 days of the kindergarten birthdate cutoff for the given school year. Observations are weighted by the number of births on the given date. Robust standard errors are shown in parentheses (* $p < 0.1$, ** $p < 0.05$).

Table 3—Regression Discontinuity by Measure and Bandwidth (UPK Year 1)

	Conviction Rate (1)	Conviction Count (2)
+/- 15 Days	-0.010	-1.686
	(0.010)	(1.413)
Obs	*30*	*30*
+/- 30 Days	-0.017**	-2.614**
	(0.008)	(1.178)
Obs	*60*	*60*
+/- 45 Days	-0.013*	-2.024*
	(0.007)	(1.103)
Obs	*90*	*90*
+/- 60 Days	-0.013**	-2.014**
	(0.006)	(0.895)
Obs	*120*	*120*
+/- 75 Days	-0.012**	-1.848**
	(0.005)	(0.786)
Obs	*150*	*150*

Note: Each cell presents estimates of Equation 1 from separate least squares regressions. The dependent variable in each regression is either criminal conviction rate or criminal conviction count for any crime. Conviction rate for a given birthdate cohort is constructed as the number of unique individuals with that birthdate who were convicted of a crime in Oklahoma between age 18 and age 22 (Conviction Count) divided by the number of births on that date (imputed using Oklahoma birth records). Observations are at the birthdate cohort-level. The sample contains birthdates within the given number of days of the kindergarten birthdate cutoff for the 1998-99 school year. Observations are weighted by the number of births on the given date. Robust standard errors are shown in parentheses (* $p < 0.1$, ** $p < 0.05$).

Table 4—Regression Discontinuity by Race (UPK Year 1)

	All (1)	Black (2)	White (3)
Conviction Rate	-0.013**	-0.061*	-0.006
	(0.006)	(0.031)	(0.005)
Mean	*0.037*	*0.103*	*0.021*
Obs	*120*	*120*	*120*
Conviction Count	-2.014**	-0.895*	-0.595
	(0.895)	(0.454)	(0.528)
Mean	*4.833*	*1.425*	*2.117*
Obs	*120*	*120*	*120*

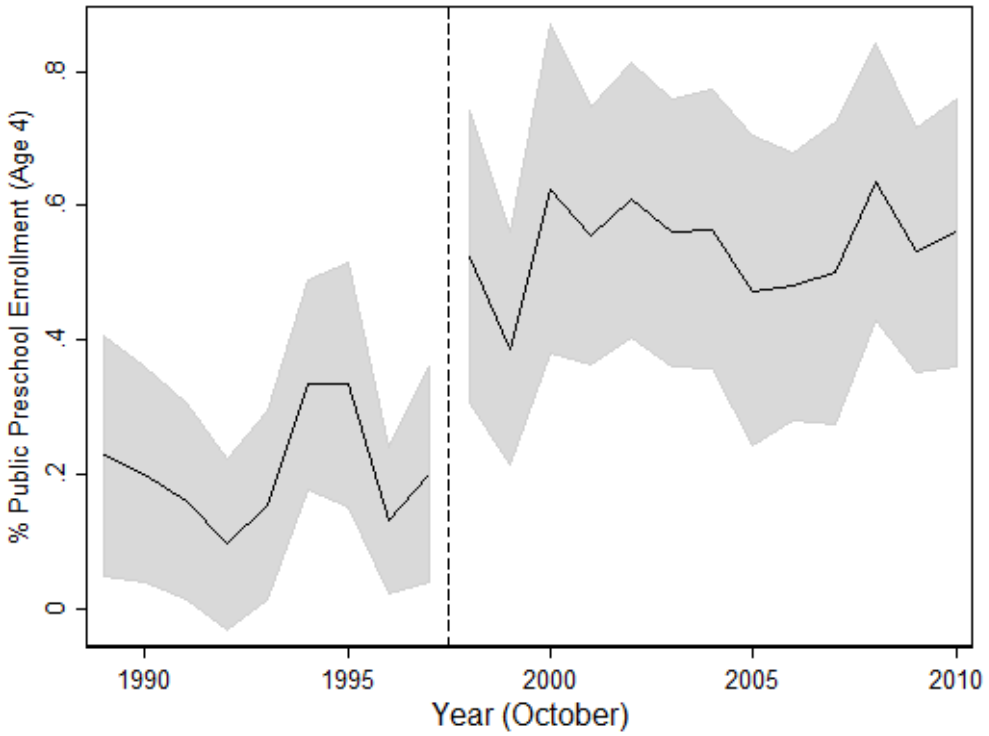
Note: Each cell presents estimates of Equation 1 from separate least squares regressions. The dependent variable in each regression is either criminal conviction rate or criminal conviction count for any crime. Conviction rate for a given birthdate cohort is constructed as the number of unique individuals with that birthdate who were convicted of a crime in Oklahoma between age 18 and age 22 (Conviction Count) divided by the number of births on that date (imputed using Oklahoma birth records). Observations are at the birthdate cohort-level. The sample contains birthdates within 60 days of the kindergarten birthdate cutoff for 1998-99 school year. Observations are weighted by the number of births on the given date. Robust standard errors are shown in parentheses (* $p < 0.1$, ** $p < 0.05$).

Table 5—Regression Discontinuity by Conviction Type (UPK Year 1)

	Any Crime (1)	Property Crime (2)	Violent Crime (3)
Conviction Rate	-0.013**	-0.007*	-0.006*
	(0.006)	(0.004)	(0.004)
Mean	*0.037*	*0.014*	*0.011*
Obs	*120*	*120*	*120*
Conviction Count	-2.014**	-0.964*	-0.964*
	(0.895)	(0.504)	(0.532)
Mean	*4.833*	*1.850*	*1.408*
Obs	*120*	*120*	*120*

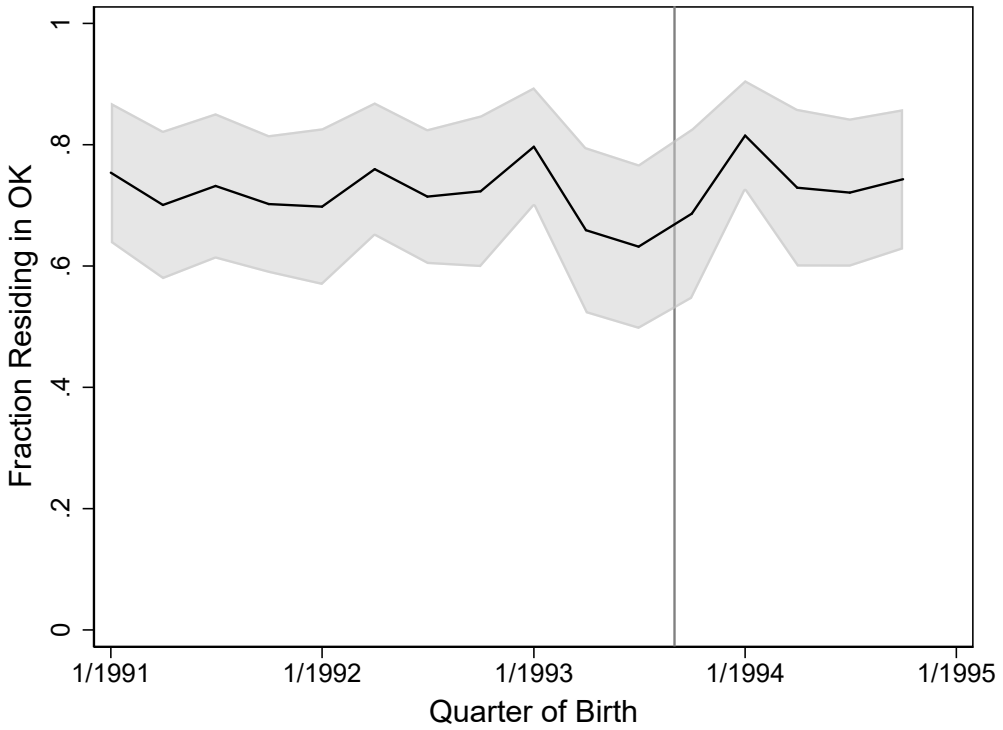
Note: Each cell presents estimates of Equation 1 from separate least squares regressions. The dependent variable in each regression is either criminal conviction rate or criminal conviction count for for the given crime category. Conviction rate for a given birthdate cohort is constructed as the number of unique individuals with that birthdate who were convicted of a crime in Oklahoma between age 18 and age 22 (Conviction Count) divided by the number of births on that date (imputed using Oklahoma birth records). Observations are at the birthdate cohort-level. The sample contains birthdates within 60 days of the kindergarten birthdate cutoff for 1998-99 school year. Observations are weighted by the number of births on the given date. Robust standard errors are shown in parentheses (* $p < 0.1$, ** $p < 0.05$).

Figure A1. OKlahoma Public Preschool Enrollment Rate by Year (October CPS)



Note: Data obtained from October Current Population Survey (1998 falls in the 1998-99 school year, Year 1 of UPK). The fraction of four year olds attending public preschool includes both Head Start and Pre-K. The shaded area depicts the 95% confidence interval. Population weights are used.

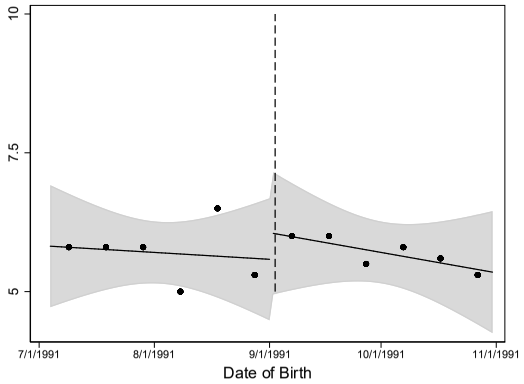
Figure A2. Oklahoma Residency Rate of Oklahoma-Born Young Adults by Birth Quarter



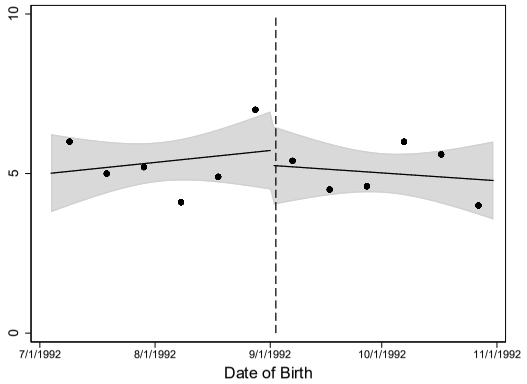
Note: Sample consists of Oklahoma-born 23-year-olds observed in the American Community Survey. The shaded area depicts the 95% confidence interval. Population weights are used.

Figure A3. Birthdate Cohort Conviction Count by School Year

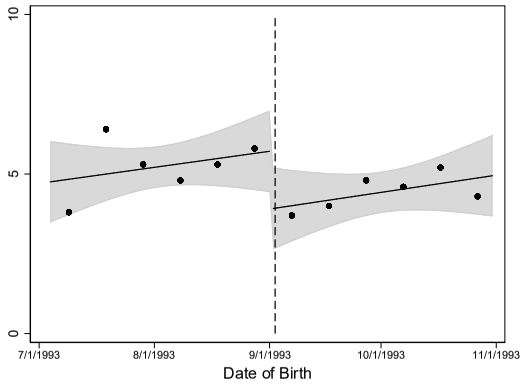
(a) 1996-97 School Year



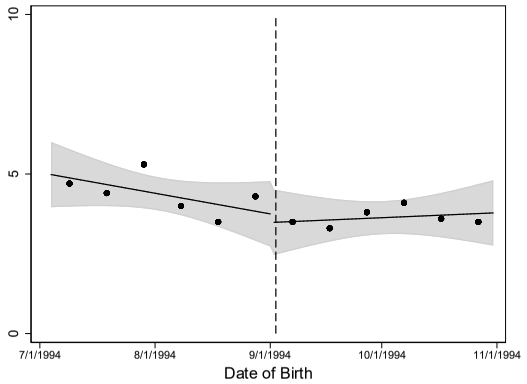
(b) 1997-98 School Year



(c) **1998-99 School Year [UPK YEAR 1]**

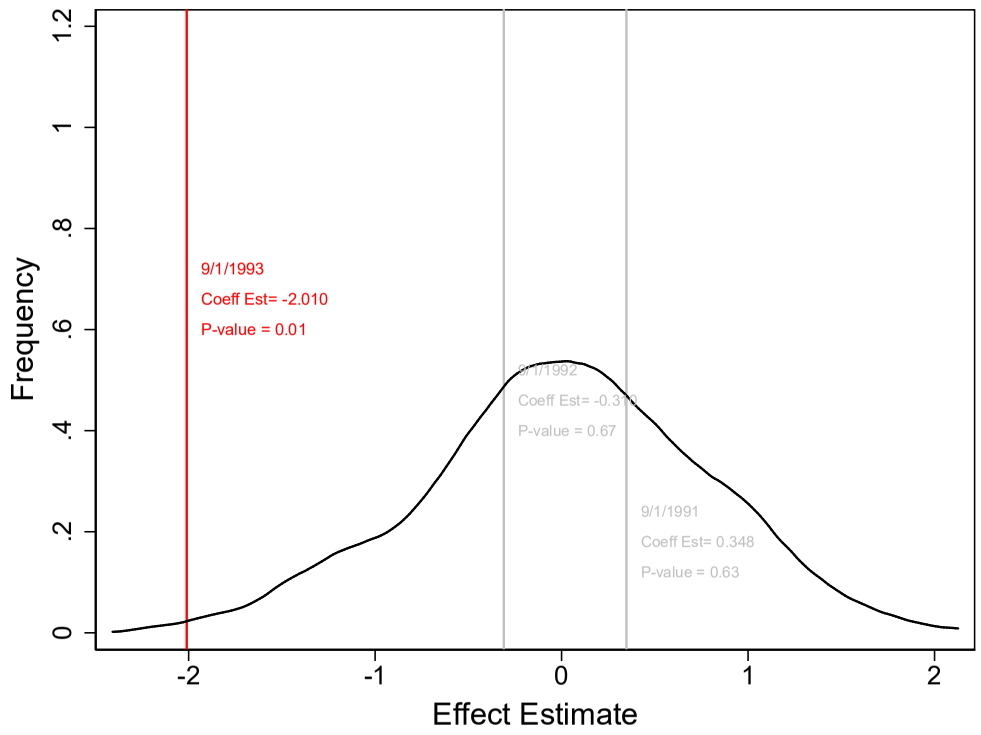


(d) 1999-00 School Year*



Note: Dots represent means across 10 day bins. Conviction count for a given birthdate cohort is constructed as the number of unique individuals with that birthdate who were convicted of a crime in Oklahoma between age 18 and age 22. Observations are at the birthdate cohort-level. For children born near the 1999-00 Kindergarten cutoff, conviction data is only available for age 18 to 21. In Oklahoma, the birthdate cutoff for Kindergarten is September 1. Children born after September 1, 1993 had access to UPK, and so the only year with a major contrast in UPK access at the Kindergarten cutoff was 1998-99 [UPK Year 1].

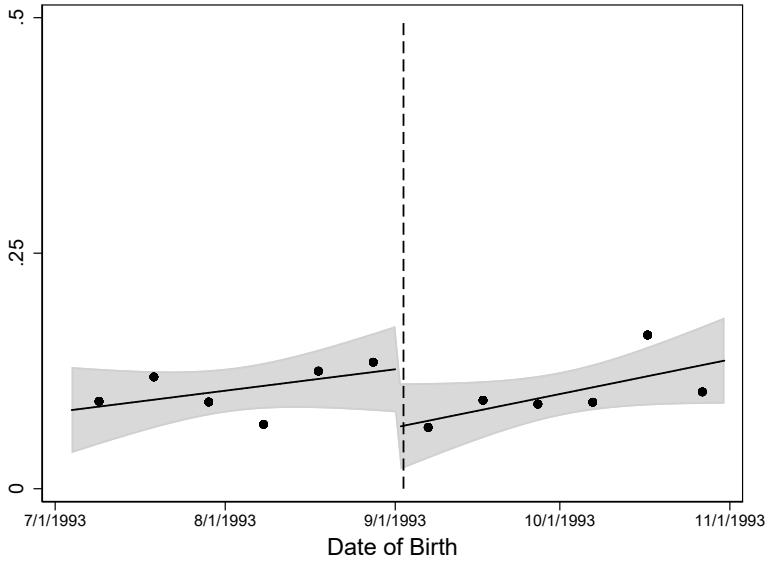
Figure A4. Random Inference for RD Estimate of Effect of UPK Access on Conviction Count



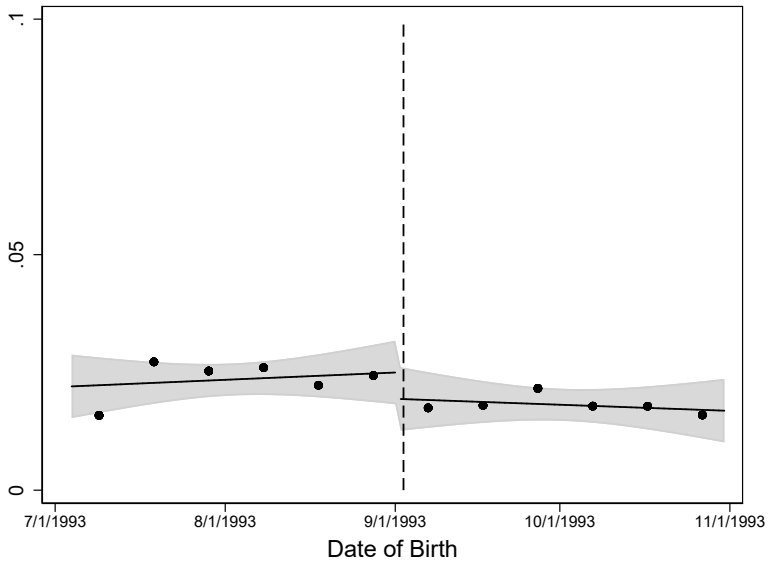
Note: Figure depicts the distribution of coeffient estimates based on placebo Kindergarten birthdate cutoffs. Equation 1 is estimated using each date between March 1991 and March 1994 as the placebo cutoff. The coefficient estimate for the true UPK access cutoff in 1998-99 (Year 1 of UPK) is shown in red. The coefficent estimates for the Kindergarten cutoff in prior years (with no contrast in access to Pre-K) are shown in grey. The dependent variable in each regression is the criminal conviction count for any crime.

Figure A5. Birthdate Cohort Conviction Rate by Race

(a) Black



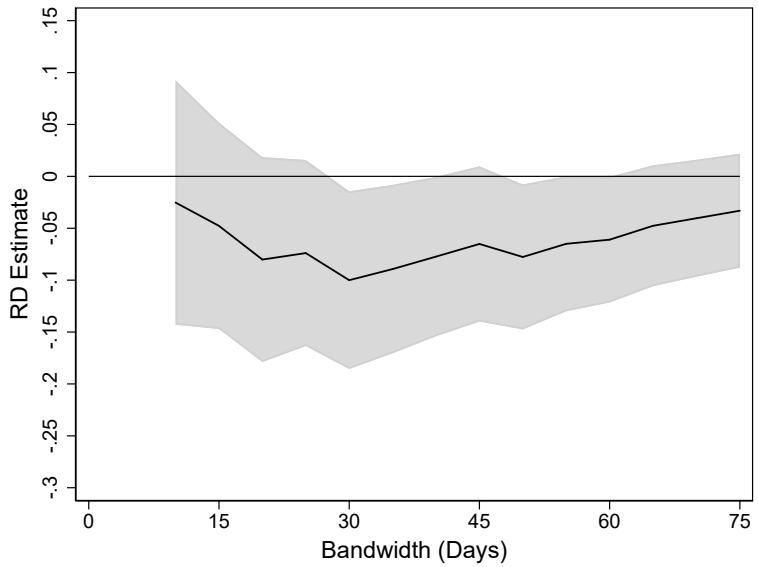
(b) White



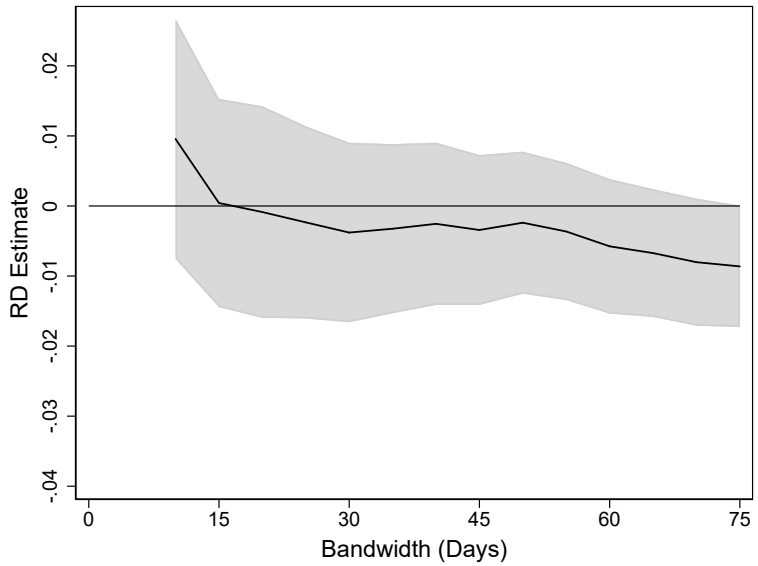
Note: Dots represent means across 10 day bins. Conviction rate for a given birthdate cohort is constructed as the number of unique individuals with that birthdate who were convicted of a crime in Oklahoma between age 18 and age 22 (Conviction Count) divided by the number of births on that date (imputed using Oklahoma birth records). Observations are at the birthdate cohort-level.

Figure A6. RD Estimate of Effect of UPK Access, by Bandwidth and Race

(a) Black



(b) White



Note: Figure depicts RD estimates (Equation 1) for various bandwidths (days on either side of the September 1, 1993 UPK access birthdate cutoff). 95% confidence intervals are shaded in grey. The dependent variable in each regression is the criminal conviction rate for any crime. .