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

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ABSTRACT

While Pre-K enrollment has expanded rapidly over the last decade, there is little evidence to date regarding the long-run effects of statewide universal preschool programs, only studies of programs targeted at more at-risk populations (e.g. Head Start and Perry Preschool) that are often more resource-intensive. I estimate the impact of Oklahoma's universal prekindergarten program (UPK) on later criminal activity, an outcome that accounted for 40-65% of the large estimated long-run benefits of Perry Preschool. I assemble data on criminal charges in the state of Oklahoma and identify the effect of UPK availability using a regression discontinuity design that leverages the birthdate cutoff for UPK in the program's first year of implementation. I find significant negative impacts of UPK availability on the likelihood that black children are later charged with a crime at age 18 or 19 of 7 percentage points for misdemeanors and 5 percentage points for felonies. I find no impact on the likelihood of later charges for white children. The results suggest that universal Pre-K can, like more targeted programs, have dramatic effects on later criminal outcomes, but these effects are concentrated among more at-risk populations.

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1. INTRODUCTION

In the last decade, campaigns to provide high quality prekindergarten (Pre-K) to four year olds have achieved remarkable success. The fraction of four year olds attending Pre-K reached 28% in 2013, double what it was in 2002 (Barnett et al, 2013). The Wall Street Journal called this increase "one of the most significant expansions in public education in the 90 years since World War I" (Solomon, 2007). Supporters of increasing funding for early childhood education make the case that major economic and social problems, such as crime and teen pregnancy, can be traced to low cognitive and socio-emotional skill levels. Differences in these skill levels in advantaged and disadvantaged children appear early in childhood, but these differences can be alleviated by intervening early, leading to substantial reductions in negative later-life outcomes and therefore very high benefit-cost ratios and rates of return (Heckman et al, 2010a). These arguments have won the support of many policymakers at the state and federal level, including President Obama. In his 2013 State of the Union address, he announced the Preschool for All Initiative to expand high-quality preschool access to every child in America by allocating federal funds to finance states' provision of Pre-K. The plan's goal is to reduce the achievement gap by helping to "level the playing field" for children from low income families.[1]

Seven in ten Americans support expanding preschool programs using federal funds, according to an August 2014 Gallup poll.[2] Despite this widespread public support, important disagreements arise around the scope of early childhood educational interventions: Should interventions be targeted only at the most at-risk children or should they be universal? The commonly cited evidence showing high long-run returns to early childhood education programs

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[1] "Fact Sheet President Obama's Plan for Early Education for All Americans," https://www.whitehouse.gov/the-press-office/2013/02/13/fact-sheet-president-obama-s-plan-early-education-all-americans (February, 12, 2013).
[2] Jones, Jefferey. "In U.S., 70% Favor Federal Funds to Expand Pre-K Education," (September 8, 2014).

is largely limited to preschool programs targeted at especially at-risk children, and often to programs that are highly resource-intensive. This evidence includes quasi-experimental evaluations of Head Start (Deming, 2009; Ludwig and Miller, 2007; Garces et al, 2002) and experimental evaluations of HighScope Perry preschool (Heckman et al, 2010a) and the Abcedarian program (Campbell et al, 2012). Universal Pre-K differs dramatically from these programs because it is available to all children and typically involves lower levels of funding per child, especially when compared to Abcedarian and Perry preschool.

Evidence regarding the effectiveness of less resource-intensive but universally available preschool programs has thus far been necessarily restricted to short-run impacts. This is largely because the first of these programs were only implemented relatively recently, in the mid to late 1990s.[3] These evaluations look at outcomes in middle school or earlier and tend to find mixed results. Some find evidence of substantial "fade out" in early test score effects, though this does not rule out large long-run effects. Similar fade out in cognitive effects have also been observed in a number of other early childhood interventions that nonetheless found large long-run effects (e.g. Heckman et al, 2013; Deming, 2009). The lack of evidence on the long-run effectiveness of universal Pre-K programs is remarkable given growing political momentum behind these programs. The result is a critical blind spot for state policymakers deciding how to best allocate early childhood education funding.

I estimate the impact of Oklahoma's universal Pre-K program (UPK), introduced in 1998, on an important later-life outcome: teenage criminal activity. I assemble data on criminal charges in the state of Oklahoma and use a regression discontinuity design which leverages the

[3] See section 2 for a review of this literature.

birthdate cutoff for UPK eligibility in the program's first year of implementation. This approach yields estimates of the effect of UPK availability (or the intent-to-treat effect of state Pre-K) compared to the prior mix of preschool services. I compare the effect of UPK availability differentially by race, as black children in Oklahoma are four times more likely to be charged at age 18 or 19 than are white children (31% vs. 7%). I find a significant negative impact of UPK availability on the likelihood that a black child is later charged with a misdemeanor or felony at age 18 or 19 (7 and 5 percentage points, respectively), but no impact on the likelihood of later charges for white children. This suggests that, like more targeted preschool programs, UPK has a large and important impact on a measure frequently associated with socio-emotional skills, but the impact on this measure is concentrated within a higher-risk population.

The rest of this paper is organized as follows. In Section 2, I review the existing evidence on the long-run effects of preschool. In Section 3, I discuss the details of the universal Pre-K program in Oklahoma. In Section 4, I describe my data sources. In Section 5, I present my empirical strategy. In Section 6, I discuss my empirical results. In Section 7, I use my results to approximate an alternative estimand. In Section 8, I conclude and discuss future work.

2. EXISTING EVIDENCE ON LONG-RUN PRESCHOOL EFFECTS

In this section I review the existing literature on the long-run impacts of early childhood education interventions. This literature 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 primarily from studies of the targeted programs. Studies of universal Pre-K programs involve shorter timeframes and obtain more mixed results.

2.1 Targeted Preschool Pilot Programs

The most frequently cited evidence of the long-run impacts of early education interventions comes from the HighScope Perry Preschool Program. This 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). Only children judged to be disadvantaged by family socioeconomic status and IQ scores were eligible to participate, and the eligible 123 children were randomly assigned to the program or the control group.[4] Heckman et al (2010a) uses data that includes periodic follow-up interviews to age 40 to estimate 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 et al, 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, 2010a). 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

[4] Heckman et al (2010) adjusts their effect estimates for the potentially problematic reassignment of treatment and controls after the initial random assignment.

extent to which the Perry sample was selected on disadvantage. For example, 37 of 39 (95%) of men assigned to the control group in the Perry study were arrested by age 40, and averaged 12.4 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, but it differed somewhat in the intensity of its treatment. 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).[5] 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. 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 (likely a noisier measure).

2.2 Large-scale Targeted Preschool Programs

Evidence of the effectiveness of small-scale, high-intensity single-site interventions like Perry Preschool and Abcedarian naturally raises concerns about the replicability of these programs and their results. The federal Head Start and the Chicago Child-Parent Centers (CPC) programs are also targeted to disadvantaged children, but at a much larger scale. This larger scale alleviates some of the external validity and scalability concerns with the smaller pilot

[5] Ramey et al state that the cost of the preschool component of Abcedarian was $6,000 per child per year. Children entered the preschool component between 1972 and 1983.

programs. However, there is not yet experimental evidence on the impact of Head Start or CPC on adult outcomes, making it much more difficult to determine their effects.[6]

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 et al (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 the siblings do not differ systematically. Garces et al use the 1964-1977 birth cohorts of the Panel Survey of Income Dynamics. The authors find that Head Start increases educational attainment, but only for white children. They also find dramatic reductions in the likelihood of facing criminal charges, but only for black children (12 percentage points). Deming looks at a later cohort of children, born in the early 1980s, using data from the National Longitudinal Mother-Child Supplement. He 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. He finds no impact on later crime, but measures crime differently than Garces et al., as an indicator equal to one if an individual is currently incarcerated or reports having been convicted, sentenced, or on probation.

[6] There is a national experimental evaluation of Head Start, but thus far it only observes outcomes to third grade (Puma et al, 2012).

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.

The CPC program began in 1967 and was designed to provide educational and family support to children in high-poverty neighborhoods in Chicago that did not have access to Head Start. The program is operated by Chicago Public school system and provides a variety of services to children ages 3 to 9, including a preschool program. Preschool teachers are required to have a college degree and child-to-staff ratios are relatively low, 17:2 (Reynolds and Ou, 2011). In 1985, the average cost per child of the preschool component was $9,636 in 2015 dollars (Reynolds et al, 2014).

Reynolds et al (2007) identifies the impact of CPC on adult outcomes by comparing CPC participants with a non-experimental comparison group of children in Chicago and controlling for covariates. At age 24, they find that CPC preschool participants were 4.6 percentage points (22%) less likely to have been arrested for a felony and 5.0 percentage points (20%) less likely to have been incarcerated. They also find that CPC participants were more likely to be employed, more likely to have a high school degree, and less likely to have depressive symptoms.

2.3 State-run Universal Pre-K

To date, there is no evidence on the impacts of state-run universal Pre-K programs on later adult outcomes. There have, however, been short-run impact studies in a number of states, including Georgia (Fitzpatrick, 2008), Tennessee (Lipsey et al, 2013), and Oklahoma (Gormley and Gayer, 2005; Gormley et al, 2011). Fitzpatrick uses a difference-in-difference approach with data from the National Assessment of Educational Progress and finds that the availability of Georgia Pre-K improved fourth grade test scores and on-grade percentage, but only for disadvantaged students in small towns and rural areas. 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.

Gormley and Gayer (2005) investigate the achievement impact of Oklahoma's universal Pre-K program using data from Tulsa Public Schools in 2001. They use a birthdate regression discontinuity approach which leverages a strict cutoff in whether students were eligible to attend Pre-K in 2000 or 2001. However, since students on either side of this cutoff will both eventually receive the same access to Pre-K, they can only look at outcomes immediately after Pre-K, when a treatment contrast still exists. In essence, Gormley and Gayer compare 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.

Looking again at universal Pre-K in Tulsa Public Schools, Gormley et al (2011) investigate the socio-emotional effects of the program using teacher ratings of kindergarteners in 2006. Using a teacher fixed effects approach with propensity score matching, they find that Pre-K participation was associated with higher attentiveness and lower timidity. The effects on these socio-emotional measures predict lower rates of future delinquency in adolescence (Moffitt et al, 1990).

3. OKLAHOMA UNIVERSAL PRE-K

Universal Pre-K (UPK) in Oklahoma differs from other early state universal programs (e.g. Georgia, New York) in both its scale and its quality. Unlike other states that operate Pre-K similar to a voucher system, Oklahoma treats Pre-K largely as another grade in the public education system, though an optional one. This distinction 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 other grades. Pre-K teachers are required to be certified in early childhood education and to be paid at the same rate as other teachers. The adult-to-child ratio in classrooms cannot exceed 1:10 (Rose, 2011). These features of pre-K in Oklahoma make the quality of provision much more consistent than in other states that contracted their pre-K classrooms to outside providers. However, this consistent quality comes with a higher per student price tag than other universal pre-K programs, $7,700 per student in 2013, with roughly half coming from the state and the remainder coming from federal and local sources (Barnett et al, 2013).[7] This per student cost is similar to Head Start, but less than half that of Perry Preschool or Abcedarian.

In 1980, Oklahoma initiated a small-scale Pre-K pilot program targeted toward children from low-income families, but its funding was limited and it reached few children for the first two decades of its existence. By the 1997-98 school year, only 4% of four year olds who would

[7] Inflation adjusted to 2015 dollars.

eventually enter Oklahoma public schools were enrolled in state Pre-K.[8,9] The rest of the preschool market was divided as follows: roughly one quarter of four year olds attended private preschool, 20% attended head start, and half did not attend any preschool.[10] Starting in 1998-99, Oklahoma expanded the program to make all four year olds (as of September 1) eligible while simultaneously increasing the per-pupil funding for four year olds in the state funding formula. The result was a large jump in Pre-K access and enrollment, which I exploit to identify the impact of the program.

I measure the extent and distribution of the 1998-99 increase in Pre-K access and enrollment using data from the Common Core of Data's (CCD) Public Elementary/Secondary School Universe Survey, administered by the National Center for Education Statistics (NCES). Figure 1A shows that the total Pre-K enrollment in Oklahoma increased more than eight-fold (2,000 to 17,000) from 1997-98 to 1998-99. Figures 1B and 1C show the percent of future Oklahoma public school first graders who enrolled in Pre-K and who went to a school that provided Pre-K, respectively, by race.[11] They show that from 1997-98 to 1998-99 Pre-K enrollment (access) increased from 4% to 32% (10% to 45%) for white children and 6% to 41% (20% to 60%) for black children.[12] Consequently, the contrast in Pre-K enrollment (access) in

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[8] Pre-K enrollment rate is calculated as the number of students enrolled in Pre-K divided by the number of students in first grade two years later (Source: National Center for Education Statistics, Common Core of Data).

[9] Given that 46% of students in the state were eligible for free or reduced price lunch (FRL) in 1998-99, only a very small fraction of children from low-income families were served by state Pre-K prior to 1998-99 (National Center for Education Statistics, Common Core of Data).

[10] Private preschool and no preschool attendance rates are calculated from the October Current Population Survey. Head Start attendance rate is calculated as the total head start enrollment divided by the number of students in first grade two years later.

[11] Percent Pre-K enrollment for a given race is the calculated as the aggregate state Pre-K enrollment of that race in year t divided by the aggregate state Grade 1 enrollment of that race 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 that race (it is then aggregated to the state-level).

[12] Figure A5 shows similar increases in public preschool enrollment (Head Start and Pre-K) for the full sample of Oklahoma four year olds using data from the October Current Population Survey.

the first year of UPK is 6 percentage points (5 percentage points) larger for black children than white children.

The increase in Pre-K enrollment and access differed substantially by socio-economic status. Figures 2A and 3A show the relationship between the percent of free and reduced lunch-eligible (FRL) students in a given zip code and the percent Pre-K enrollment and access in that zip code, respectively.[13] The top panel depicts a locally weighted regression of percent Pre-K enrollment (or access) on percent FRL separately for 1997-98 and 1998-99, where the zip code is the unit of observation.[14] The bottom panel shows the grade 1 enrollment distribution by zip code percent FRL. The top panel of both Figures 2A and 3A show a dramatically larger 1998-99 increase in Pre-K enrollment and access in higher percent FRL (lower socio-economic status) zip codes. In other words, children from low-income families, those at higher risk of later criminal charges, were more likely than children from higher-income families to have access to and enroll in Pre-K in the first year of UPK (relative to the prior year). Figures 2B and 3B show the same story using a zip code's family median income in 1999 as its measure socio-economic status, rather than percent FRL.[15] Cascio and Schanzenbach (2013) present a similar picture of preschool enrollment responses to universal Pre-K implementation in Georgia and Oklahoma with data from the October Current Population Survey. Using a difference-in-differences strategy, they estimate a much larger increase in preschool enrollment among children with less-educated mothers.

In addition to the risk levels of children that enrolled, the effect of Pre-K will depend on the preschool services that those enrolled children would have experienced if they did not attend

[13] A student is considered to be part of a zip code if they attend a school located in that zip code.
[14] Zip codes with grade 1 enrollment less than 20 are excluded.
[15] Zip code median family income obtained from Table P077 of the 2000 Census.

Pre-K. Figure 4 shows the mix of preschool services in Oklahoma in the two years before and after UPK implementation. The fractions in the figure are imprecise, as they are calculated using only the 115 observations of Oklahoma four year olds in the October Current Population Survey (1996-1999). Despite this limitation, the figure suggests that the UPK increase in Pre-K enrollment drew four year olds who would not have attended preschool or who would have attended private preschool. There is no evidence that UPK drew enrollment away from Head Start.

4. OKLAHOMA CRIME AND BIRTH DATA

I use two sources of data to measure the long-run impact of universal Pre-K availability on criminal outcomes in Oklahoma. First, I obtain data on criminal charges filed against 18 and 19-year-olds in Oklahoma for the cohort of individuals that turned five in the years surrounding UPK implementation. Second, I obtain data on births in the state of Oklahoma for the same cohort of individuals. I use these two data sources to construct a measure of the likelihood of criminal charges at age 18-19 for individual birthdate cohorts.

4.1 Criminal Court Data

Two organizations provide public access to criminal court data in Oklahoma: The Oklahoma State Courts Network (OSCN) and On Demand Court Records (ODCR). Together, they cover 71 of the 77 counties in Oklahoma. These counties account for 96% of the arrests in Oklahoma.[16] Each organization maintains a website that provides public access to detailed

[16] Source: Oklahoma State Bureau of Investigation, "State of Oklahoma Uniform Crime Report, Annual Report January-December 2013."

charge and defendant information, presented separately on individual webpages by court case.[17] I systematically scrape relevant information from the html code on these webpages and compile it into a dataset with observations at the defendant by case level, which contains the names, birthdates, demographics, and charges of defendants.

The compiled data includes criminal charges filed against 18 and 19-year-olds in Oklahoma from January 1, 2010 to May 1, 2014. For those born between January 1, 1992 and May 1, 1994 (the cohorts surrounding the first exposed to UPK), the data cover the timeframe between their 18th and 20th birthdays (the full years they were age 18 and 19). For a larger range of birthdates, from January 1, 1992 to May 1, 1995, the data covers the timeframe between 18th and 19th birthdays (the full year they were age 18). The number of unique criminal court cases observed in my sample for 18-19 year olds (5,346 in 2013) is 54% of the number of arrests of 18-19 year olds reported in Oklahoma (9,892 in 2013).[18,19] This relationship mirrors the 53% of arrests that result in charges for southern 18-19 year olds in the National Longitudinal Survey of Youth 1997 (NLSY97), suggesting that my sample of court records is broadly consistent with officially reported law enforcement data.[20]

I collapse the criminal court data to the birthdate-level to obtain the number of unique individuals with a given birthdate who were charged with a crime at age 18 or 19. I repeat this process for various subsamples by race (i.e. black only and white only), age (i.e. 18 only and 19

[17] Figures A1 and A2 show example screenshots of these webpages.
[18] Unique criminal cases are defined as unique combinations of court ID, defendant first and last name, defendant birthdate, and case filing date. The number of arrests in Oklahoma is obtained from the Oklahoma State Bureau of Investigation, "State of Oklahoma Uniform Crime Report, Annual Report January-December 2013." Arrest statistics exclude traffic offenses while the court records do not.
[19] In 2012, the number of unique criminal cases (5,717) is 51% of the total number of arrests (11,112).
[20] This statistic reflects the ratio of the total arrests and total charges reported "since last interview" by 18-20 year olds in the south region (which includes Oklahoma) in the NLSY97. 20 year olds are including because arrests and charges that they report may have occurred while they were age 19.

only), and charge severity (i.e. felony only or misdemeanor only). Table 1 shows the number of these individuals in the year surrounding the birthdate eligibility cutoff for UPK availability in 1998-99. In all, 2,517 unique 18-19 year olds were charged with a misdemeanor and 2,005 were charged with a felony.

ODCR and OSCN are missing race information for 55% of the criminal defendants in my sample, complicating my subsample analyses.[21] For these defendants I impute race using a race prediction index based on the likelihood that a person is of a particular race given their first and last name. Specifically, the index is the predicted probability from a probit regression using observations in my sample that observe race. The dependent variable of this regression is an indicator for a given race and the independent variables consist of cubics of the fraction of criminal defendants with the same first or last name that are of a given race. Appendix Figures A3 and A4 show the distribution of the race prediction index by observed race for the white name and black name index. These figures show that the prediction index fits the observed race of defendants quite well. For my main analysis, I select a relatively tight threshold for imputing a defendant's missing race in order to minimize type I error. Specifically, if a defendant's prediction index for a given race is greater than or equal to 0.9, then I include them in subsamples limited to that race. In supplementary analyses, I exclude individuals with missing race and also use looser imputation thresholds (i.e. 0.1, 0.25, 0.5, and 0.75). This has no qualitative effect on my results, though it does affect the magnitudes of the coefficient estimates as would be expected.[22] After imputing race based on first and last names, I have race

[21] OSCN provides the defendant's race, but this information is missing or listed as "Unknown" for 15% of defendants. ODCR does not provide information on defendant's race.

[22] Reducing the number of individuals whose charges are included in the analysis lowers the numerator of the outcome variable (count of unique individuals with a criminal charge) but not the denominator (count of births), therefore it lowers the baseline mean of the outcome variable. The expected result is a reduction in the magnitude of the coefficient estimate.

information for 97% of the defendants in my sample. Table 1 shows the number of unique individuals charged with a crime by race both including (column 2 and 3) and excluding charges with imputed race (column 4 and 5). Excluding charges with imputed race leads to subsample charge counts that are between one quarter and one half the size of the size of the imputed subsample charge counts.

4.2 Births Data

I obtain data on all births in the state of Oklahoma for the years 1992-1995 from the public-use Natality File maintained by the National Center for Health Statistics (NCHS) at the Centers for Disease Control and Prevention. This data is recorded at the individual-birth level and contains the year, month, and day of the week of the birth (i.e. Sunday, Monday, etc.), as well as the sex of the child and the state of residence and race of the mother. I restrict the sample to mothers residing in Oklahoma and collapse the data to obtain the number of births by year-month-day of the week. Table 1 shows the number of births in Oklahoma in the year surrounding the birthdate eligibility cutoff for UPK in 1998-99. For the full sample and separately by mother's race, 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.[23]

4.3 Criminal Outcomes for Individual Birthdate Cohorts

I combine the birthdate-level criminal charge and birth data to construct my primary outcome measure: the likelihood of criminal charges at age 18-19 by date of birth. This is the count of unique individuals with a given birthdate that were charged with a crime in Oklahoma at

[23] I find no change in my results when I account for the reduced number of 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).

age 18-19, divided by the count of individuals born in Oklahoma on that date. I repeat this construction for various subsamples by race (i.e. black only and white only), age when charged (i.e. 18 only and 18-19), and charge severity (i.e. felony only or misdemeanor only). For the outcome measure constructed separately by race, I use mother's race to determine the denominator (number of births) and the individual's reported or imputed race to determine the numerator (number of individuals charged).

5. EMPIRICAL STRATEGY

I estimate the impact of Oklahoma's universal pre-K program (UPK) on later criminal behavior using a regression discontinuity (RD) design and difference-in-regression-discontinuity (DRD) design. These strategies yield estimates of the effect of UPK availability, or equivalently the intent-to-treat effect of state Pre-K, compared to the prior mix of preschool services in Oklahoma. This prior mix consisted mainly of head start, private preschool, and no preschool (approximately 20%, 25%, and 50%). The identification of the effect of UPK eligibility in both approaches leverages the treatment contrast between children just below and just above the Kindergarten birthdate cutoff in the first year of the UPK implementation. In this section, I first discuss this treatment contrast in further detail, then I present the standard RD framework, and finally I present the DRD framework.

5.1 Treatment Contrasts

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.

Table 2 shows how the schooling experiences of these children differed in the years surrounding the implementation of universal Pre-K (UPK). In the table, PK denotes that a child is *eligible* to attend state Pre-K as part of UPK. Therefore, the contrast between PK and no PK reflects the intent-to-treat (ITT) effect of the state's Pre-K program relative to the pre-existing mix of preschool services, rather than the treatment effect of actually attending state Pre-K. While not capturing the effectiveness of local Pre-K implementation because 60% of students did not attend state Pre-K, the ITT effect is the parameter of primary interest for state policymakers contemplating a similar policy of voluntary universal Pre-K.

In the 1998-99 school year, the first year of UPK implementation, Child C in Table 2 (born September 1, 1993) just meets the birthdate cutoff and therefore attends Kindergarten in 1998-99. She never experiences UPK eligibility because it had not been available the prior year. On the other hand, Child D in Table 2 (born September 2, 1993) misses the birthdate cutoff and is therefore eligible for UPK in 1998-99. This means that Child C and Child D differ in their access to UPK. But this is not the only difference between them. They also differ in relative age to their classmates: Child C is the youngest in her cohort and Child D is the oldest in her cohort. This difference is problematic for my identification strategy to the extent that it affects later criminal outcomes. Dobkin and Ferreira (2010) find that students who are young relative to their peers attain slightly higher levels of education, but do not perform as well academically. They find that these differences do not impact later job market outcomes but they do not observe criminal outcomes.

Except for the first year of UPK implementation, there is no policy contrast at the Kindergarten birthdate cutoff. In 1997-98, prior to UPK, Child A in Table 2 (born September 1, 1992) meets the September 1 cutoff and attends Kindergarten in 1997-98 as the youngest in her grade. Child B in Table 2 (born September 2, 1992) just misses the birthdate cutoff and so she does not attend Kindergarten in 1997-98, but attends in 1998-99 as the oldest in her grade. Critically, Child A and B differ in their relative ages, but *neither* are eligible for UPK because it is not implemented until they are too old. Similarly, Child E and F in Table 2 (born September 1 and 2, 1994) differ in their relative ages, but *both* are eligible for UPK. I will leverage the lack of policy contrast in these other years to test whether discontinuities in the likelihood of criminal charges at the Kindergarten birthdate cutoff in the first year of UPK are a result of UPK or relative age differences.

5.2 Regression Discontinuity Design

My basic regression discontinuity (RD) design looks at a one year window of birthdates around the Kindergarten birthdate cutoff in the first year of UPK implementation (March 2, 1993 to March 1, 1994) and estimates the discontinuity in the likelihood of later criminal charges that occurs at this eligibility threshold (September 1, 1993). Intuitively, this strategy compares children born just before and just after the eligibility threshold (Child C and Child D in Table 2). The regression specification is as follows,

$$Y_b = \alpha + \beta(z_b \geq 0) + f(z_b) + (z_b \geq 0) \cdot f(z_b) + \epsilon_b, \qquad (1)$$

where Y_b is the proportion of children born on date b who face criminal charges at ages 18 or 19 (or age 18 only) and z_b is the difference between date of birth b and the eligibility threshold for kindergarten in the 1998-99 school year (September 1, 1993). In most specifications, $f(z_b)$ is a linear function of z_b. By interacting $f(z_b)$ and $(z_b \geq 0)$, the specification allows the function to vary on either side of the eligibility threshold. The coefficient of interest, β, can be interpreted as the ITT effect of state Pre-K on children who just missed the Kindergarten cutoff (i.e. the oldest students relative to their grade).

The key identifying assumption of this RD approach is that variables related to the outcome must vary smoothly, and not discontinuously, through the cutoff. In other words, except for their UPK eligibility, Child C and Child D (from Table 2) must be the same in ways that might affect the outcome variable. A potential problem is that there is a substantial difference between Child C and Child D: they differ in relative age to their classmates (C is the oldest in her cohort and D is the youngest). This difference could impact their likelihood of criminal charges later in life, and therefore bias my RD estimates of the impact of universal pre-K. In one approach to investigating this concern, I estimate RD specification (Equation 1) for one year windows around the Kindergarten birthdate cutoff in the prior (or subsequent) year, when children on either side of the cutoff are either both UPK ineligible (i.e. Child A and B) or both UPK eligible (i.e. Child E and F).

Another potential internal validity problem would be differential exit from the state (by age 18-19) at the UPK eligibility threshold. I only observe criminal charges for those that stay in the state, so differential exit would affect the numerator of my outcome variable at the cutoff. Figure A6 shows the percent of Oklahoma-born 18-19 year olds who stay in Oklahoma, by quarter of birth, using data from the American Community Survey 2005-2013.[24] Though the measure of birth timing is coarse, the figure does not show any evidence of differential exit around the UPK eligibility threshold (1993Q3 vs. 1993Q4).

5.3 Difference-in-Regression-Discontinuity Design

[24] Individual birthdates are not available in the public-use American Community Survey.

In another approach to account for the potentially confounding factor of relative differences at the UPK eligibility threshold, I use a difference-in-regression-discontinuity (DRD) design. This design explicitly incorporates any outcome discontinuity at the eligibility threshold in the prior (or subsequent) year, when there was no UPK policy contrast, by measuring the outcome discontinuity in the first UPK implementation year relative to the discontinuity in the baseline year. Therefore, this approach accounts for differences between the oldest and youngest students (relative to their grade) that are not related to UPK eligibility.

Under additional assumptions, the DRD design can be used to make inferences regarding the impact of UPK on children that are not the relative oldest in their grade, thereby extending the external validity of the RD strategy discussed in Section 5.2. The intuition behind this extension is that Child A and C in Table 2 are the equivalent in terms their UPK eligibility and relative ages, and Child D and F are similarly equivalent. Therefore, I will assume that:

(A1) $Y^A = Y^C$, and

(A2) $Y^D = Y^F$,

where Y^x is the outcome for child *x* in Table 2. Given these assumptions, I will infer the effect of UPK on the relatively youngest children ($Y^E - Y^C$) by taking the difference of the outcome discontinuities at the Kindergarten birthdate cutoff in the first year of UPK ($Y^D - Y^C$) and the second year of UPK ($Y^F - Y^E$). This yields the identity $Y^E - Y^C = (Y^D - Y^C) - (Y^F - Y^E)$. Similarly, I will infer the effect of UPK on the relatively oldest children from the identity $Y^D - Y^B = (Y^D - Y^C) - (Y^B - Y^A)$.

For the DRD design, I employ following empirical specification using various birthdate ranges:

$$
\begin{aligned}
Y_{yb} = \alpha + \beta_1\left(UPK\ YEAR1_y\right) \times (z_b \geq 0) + \beta_2(z_b \geq 0) + \beta_3\left(UPK\ YEAR1_y\right) \\
+ f(z_b) + (z_b \geq 0) \times f(z_b) + \left(UPK\ YEAR1_y\right) \times (z_b \geq 0) \times f(z_b) + \epsilon_{yb}, \qquad (2)
\end{aligned}
$$

where the variables are defined similarly to Equation 1, but *y* indexes the school year of the relevant Kindergarten eligibility threshold around which the one year birthdate window is constructed. $UPK\ YEAR1_y$ is an indicator equal to one for the 1998-99 school year Kindergarten eligibility threshold, UPK's first implementation year. This specification estimates Equation 1 separately for the 1998-99 Kindergarten eligibility threshold and for the threshold in the baseline year within one regression (the polynomial in z_b is allowed to vary by school year *y* and on either side of the eligibility cutoff). β_1 is the difference between the discontinuity at the eligibility threshold in 1998-99 and in the baseline year.

If assumptions A1 and A2 hold, and the analysis sample includes windows around the 1998-99 and 1999-00 birthdate cutoffs (UPK years 1 and 2), β_1 can be interpreted as the effect of UPK availability on the youngest children relative to their cohort (i.e. $Y^E - Y^C$). This is because the only group *not eligible* for UPK in this timeframe was the relatively young children at the 1998-99 birthdate cutoff (Child C in Table 2). When the analysis sample includes windows around the 1998-99 and 1997-98 birthdate cutoffs (UPK Year 1 and prior year), β_1 can be interpreted as the effect of UPK availability on the oldest children relative to their grade (i.e. $Y^D - Y^B$). This is because the only group *eligible* for UPK in this timeframe was the relatively old children at the 1998-99 birthdate cutoff (Child D in Table 2).

6. RESULTS

Table 3 shows least squares estimates of β in Equation 1, weighted by the number of births on a given date.[25] The dependent variable is the likelihood of criminal charges at age 18 or 19. Estimates of β are presented by race and charge type (i.e. felony or misdemeanor) for one year windows (plus or minus six months) around Kindergarten birthdate eligibility thresholds in two years: the year prior to UPK implementation (1997-98) and the first year of UPK implementation (1998-99).[26] These are the latest cohorts for whom I observe criminal charges at age 18 and 19. In the first year of UPK implementation, I find large negative effects on later charge likelihood for black children who just missed the Kindergarten age cutoff (and were therefore eligible for UPK), but no effect for white children. The estimates can be interpreted as UPK eligibility causing a 4.5 percentage point (26% of the mean rate) reduction in the likelihood of a felony charge (Column 3) and a 6.8 percentage point (38% of the baseline rate) reduction in the likelihood of a misdemeanor charge (Column 4) at ages 18-19. I find no significant effects of missing Kindergarten birthdate cutoff in the year prior to UPK implementation, suggesting that these large estimated effects are not driven by differences on either side of the Kindergarten birthdate cutoff in students' ages relative to their classmates. I will test this explicitly using the DRD design.

Figures 5 and 6 depict the results in Table 3 graphically, showing the likelihood of a misdemeanor (Panel A) or felony (Panel B) charge at age 18-19 by birthdate (each dot shows the mean for a two week bin). Figure 6, Panel A shows no discontinuity in the likelihood of misdemeanors for blacks in the year prior to UPK implementation, but a large discontinuity in

[25] The weights account for the lower variance of the outcome variable on dates when more births occur.
[26] In some analyses, I also use a window around the birthdate cutoff in the second year of UPK.

the first year.   Figure 6, Panel B shows a small (and insignificant according to Table 3) discontinuity in the prior year and a larger discontinuity in the first year of implementation, suggesting that the felony effect in 1998-99 may be capturing both the effect of UPK and differences in the later felony rates of children that are relatively old and relatively you compared to their classmates.

Table 4 shows the same estimates of β in Equation 1 for the first year of UPK as Table 3, but for various bandwidths (plus or minus 1 to 6 months) around the Kindergarten birthdate cutoff. The insignificant estimates for white felonies and misdemeanors are consistent across bandwidths. The black misdemeanor estimates remain similar and statistically significant (at the six percent level or less) for all bandwidths, though the magnitudes are larger for smaller bandwidths. The black felony estimates also remain similar for smaller bandwidths, though the three month bandwidth estimate drops slightly below the ten percent significance level and the one month bandwidth is not significant. Table A1 and A2 show that these results are robust to various imputation thresholds for the race name prediction index, as well as to excluding charges with missing race. Table A3 and A4 also show similar results when using the count of individuals charged as the dependent variable (without dividing by the number of births).

Table 5 shows results from local linear (kernel regression) rather than a linear least squares estimation of β in Equation 1. Estimates are presented for various kernel bandwidth choices, including the optimal bandwidth following Imbens and Kalyanaraman (2011). The results are somewhat unstable at small bandwidths, but converge at higher bandwidths. At these higher bandwidths the results are similar to Table 3, though felony estimates are not significant and misdemeanor estimates are somewhat larger in magnitude (and significant or nearly significant). Figures A7 and A8 depict these results graphically, showing the local linear estimates and 95% confidence intervals by bandwidth choice.

Table A5 shows the same results as Table 3, but the dependent variable is the likelihood of criminal charges at age 18 only. Unlike the age 18-19 charge likelihood, this outcome can be observed for the window around the Kindergarten birthdate cutoff in the second year of UPK implementation (1999-00). Therefore, Table A5 presents results for two years where there should be no contrast in UPK availability, one year prior to UPK implementation (Child A and B in Table 2) and one year after (Child E and F in Table 2). Again, I find no significant effects in the years without treatment contrast and no significant effects on white children. I find a large negative impact of 3.5 percentage points on the likelihood of later misdemeanor charges for black children (40% of the mean rate), but no impact on the likelihood of later felony charges. Figure A9 and A10 present these results graphically (as in Figure 5 and 6). Table A6 and A7 show the same results for various bandwidths and race imputation thresholds. The black age 18 misdemeanor effect is less robust than for age 18-19, losing statistical significance at 1, 3, and 4 month bandwidths.

Table 6 shows least squares estimates for β_1 in Equation 2, the DRD design, for various comparison years. Panel A and C show the differential effect of missing the Kindergarten birthdate cutoff in the first year of UPK compared to the prior year (Child B vs. Child D in Table 2). The sample includes birthdates six months before the 1997-98 Kindergarten cutoff to six months after the 1998-99 Kindergarten cutoff. Panel B shows the differential effect of missing the Kindergarten cutoff in the first year of UPK compared to the prior year *and* the following year. The sample includes birthdates six months before the 1997-98 Kindergarten cutoff to six months after the 1999-00 Kindergarten cutoff. The Panel D shows the differential effect of missing the Kindergarten cutoff in the first year of UPK compared to the following year (Child C vs. Child E in Table 2). The sample includes birthdates six months before the 1997-1998 Kindergarten birthdate cutoff to six months after the 1999-2000 Kindergarten birthdate cutoff.

As in the basic RD results, there appears to be a negative impact on black misdemeanors, though it is a similar magnitude and statistically significant for misdemeanors at age 18-19. The estimates show a first year differential effect of UPK on the likelihood of misdemeanor for blacks of 5.7 percentage points at age 18-19. There is some evidence of a smaller impact on black felonies at age 18-19 of 2.8 percentage points, but it is not significant. Taken together, Table 6 lends further support to the hypothesis that UPK availability rather than relative age differences generating the change in criminal outcomes for blacks at the K cutoff.

While not significant, the similarity in the effect estimates of Panels C and D of Table 6 suggest that the effect of UPK availability may be similar on the youngest and oldest children (relative to their peers). That is, $Y^E - Y^C \approx Y^D - Y^B$. This is suggestive evidence that the RD estimates may be generalizable beyond the oldest students (relative to their peers) on which these estimates are identified.

7. APPROXIMATE TREATMENT-ON-TREATED EFFECT

While the ITT effect estimates in Section 6 are likely to be the relevant parameters for state policymakers considering similar Pre-K policies, local administrators may be more interested in the impact of state Pre-K attendance rather than UPK eligibility. In Appendix 1, I calculate a back-of-the-envelope estimate of the treatment-on-treated (TOT) effect on Oklahoma residents implied by the ITT effect estimates in Section 6. The TOT effect is the impact of UPK on UPK *compliers*, those who attend Pre-K as a result of the new policy. I calculate the TOT effect on 18-19 black misdemeanors by dividing the ITT effect estimate in Table 6 by the change in the Pre-K enrollment rate of black four year olds in the first year of UPK. The resulting implied TOT effect is a roughly 19 percentage point reduction in the likelihood of a charge. However, this approximation will over-estimate the true TOT effect if other preschool services (e.g. Head Start) responded to UPK by improving their own quality. There is some anecdotal evidence that this may have been the case. For example, the Community Action Project of Tulsa County established partnerships between Head Start providers and local school districts, where Head Start centers adopted some of the quality components of UPK.[27]

The interpretation of the magnitude of the TOT effect depends critically on the underlying baseline rate, which is likely to be higher than the general population if UPK compliers are typically from lower income families than the general population. In Appendix 1, I calculate the income distribution for UPK compliers based on the median family income of their school's zip code, and reweight the baseline criminal charge rate using this distribution. Figure 8 shows the zip code median family income distribution of UPK compliers vs. the overall grade 1 enrolled population. Figure 9 shows the TOT effect on Oklahoma residents as a percent of the calculated baseline criminal charge rate for various assumptions regarding UPK compliers' family income relative to their zip code median income (η) and criminal charge rates of new migrants to Oklahoma relative to those who have stayed in the state (α). For example, for $\eta = 0.45$ and $\alpha = 0.6$, the TOT effect on the likelihood of black misdemeanors at age 18-19

[27] "Head Start Pre-K Local Partnerships That Work: Tulsa, Oklahoma." http://eclkc.ohs.acf.hhs.gov/hslc/states/collaboration/OKTulsaHeadSta.htm (November 13, 2014)

is a roughly 50% decrease relative to the baseline rate for black UPK compliers who reside in Oklahoma.

8. CONCLUSION

In this chapter, I leverage a contrast in the availability of Oklahoma universal Pre-K (UPK) in the first year of its implementation to estimate the effect of UPK availability on later criminal outcomes at age 18 and 19. I find a significant negative impact of UPK availability on the likelihood that black children are later charged with a crime at age 18 or 19, but no impact on the likelihood of later charges for white children. As the first estimates of the long-run impacts of universal pre-K availability, 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 universal Pre-K can, like more targeted programs, have dramatic effects on later criminal outcomes, but these effects are concentrated among more at-risk populations. As with Perry Preschool, these large crime reductions are likely to determine a major part of the benefits of universal Pre-K in future benefit-cost analyses. The program’s benefits may outweigh its costs, but if effects on other outcomes (e.g. earnings) are also concentrated among at-risk populations, then it is liable to be a less efficient use of public funding than a more targeted program of equal quality.

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APPENDIX 1: Back-of-the-Envelope Calculation of Implied TOT Effect

In this section, I calculate an implied back-of-the-envelope estimate of the treatment-on-treated (TOT) effect of UPK. This is the effect on UPK compliers: those that attended state Pre-K as a result of the new policy. Because I only observe criminal charges of 18-19 year olds who remain in Oklahoma, I will focus on the impact on the UPK compliers who remain in Oklahoma.

Recall that the observed outcome measure is given by $Y_b = \frac{C_b^R}{N_b^B}$, where $C_b^R$is the number of Oklahoma residents born on date b who were charged with a crime at a given age. N_b^B is the number of births in Oklahoma on date b. We can think of β in Equation 1, the ITT effect of UPK on this measure, as representing $E[Y_{b^*} \mid eUPK = 1] - E[Y_{b^*} \mid eUPK = 0]$, where b^* is the birthdate eligibility cutoff for UPK and $eUPK$ is an indicator for UPK eligibility. In order to adjust β to reflect the ITT effect of UPK on the likelihood of criminal charges for current Oklahoma residents I multiply it by the ratio of residents born on b^* ($N_{b^*}^R$) to Oklahoma births ($N_{b^*}^B$).

$$\beta^R = \frac{E[C_{b^*}^R \mid eUPK=1] - E[C_{b^*}^R \mid eUPK=0]}{N_{b^*}^R} = \frac{N_{b^*}^B}{N_{b^*}^R} \cdot \beta. \tag{3}$$

I approximate $\frac{N_{b^*}^B}{N_{b^*}^R}$ as the ratio of the number of children born in Oklahoma to the number of 19 year olds residing in Oklahoma 19 years later, using population data from the Surveillance, Epidemiology, and End Results Program (SEER). For the black subsample, this yields the approximation $\frac{N_{b^*}^B}{N_{b^*}^R} \approx 0.76$.

Under the assumption that UPK has no impact on those that did not receive the treatment, we can write the TOT effect of UPK on those who remained in Oklahoma, τ^R, as follows:

$$\beta^R = p_{b^*} \cdot r_{b^*} \cdot \tau^R \tag{4}$$

where p_{b^*} is the fraction of Oklahoma four-year-olds born on b^* that were UPK compliers in the first year of implementation. r_{b^*} is the fraction of Oklahoma residents born on b^* who resided in Oklahoma at age four. I cannot observe p_{b^*} or r_{b^*} and so I approximate them from the Common

Core of Data and the 2000 Census, respectively.[28,29] For the black subsample, $p_{b^*} \approx 0.35$ and $r_{b^*} \approx 0.64$. Using the approximations of $\frac{N_{b^*}^B}{N_{b^*}^R}$, p_{b^*}, and r_{b^*}, discussed above, and the ITT effect estimate on black misdemeanors in Table 6 (-0.057), Equations 3 and 4 yield $\tau^R = -0.194$.

Interpreting the magnitude of this TOT effect depends critically on the baseline outcome variable (i.e. expected charge likelihood of the population of UPK compliers who remain in Oklahoma). Therefore, I reweight the sample mean of the relevant outcome variable, $\overline{Y^R} = \frac{c^R}{N^R}$, to reflect the expected value for the UPK compliers who remain in Oklahoma, $\tilde{Y}_B^R$.

First, I assume that Oklahoma residents who were in Oklahoma at age 4 differ in their criminal charge likelihood from those who were not by a factor α. Therefore,

$$\overline{Y^R} = \overline{Y_B^R}\big(r + \alpha(1 - r)\big) \tag{5}$$

where $\overline{Y_B^R}$ is the mean charge likelihood of residents who were residing in Oklahoma at age 4 and r is the fraction of residents who were in Oklahoma at age 4. This distinction is motivated by substantial observed differences in lifelong Oklahoma residents and new residents. Among black Oklahoma residents age 18-20, those that were born in Oklahoma are 8 percentage points (76%) more likely to be unemployed, 9 percentage points (38%) more likely to not have a high school diploma, and 13 percentage points (28%) less likely to have attended college.[30]

Next, I assign each Pre-K aged child in 1998-99 the median family income of their first grade school's zip code and make the conservative assumption that children did not leave Oklahoma differentially by income.[31] This means that I can rewrite the $\overline{Y_B^R}$ and $\tilde{Y}_B^R$ as

$$\overline{Y_B^R} = \frac{\sum_j E_j \cdot c(x_j)}{\sum_j E_j} \text{ and } \tilde{Y}_B^R = \frac{\sum_j E_j \cdot p_j \cdot c(\eta x_j)}{\sum_j E_j},$$

[28] I approximate q as the difference in OK Pre-K participation rates in the 1997-98 and 1998-99 school years, where participation rates are constructed as in Figure 1B.
[29] I approximate r_{b^*} from the 2000 Census using information on state of residence five years prior. I construct the approximation as follows, $r_{b^*} \approx p(OK4 \mid OK9) \cdot (OK9 \mid OK14) \cdot p(OK14 \mid OK19)$, where OK*x* is an indicator for whether an individual resided in Oklahoma at age *x*.
[30] Means are calculated from the American Community Survey 2005-2013 using population weights.
[31] Zip code median income for 1999 is obtained from the 2000 Census.

Where E_j is the 1998-99 grade 1 enrollment in zip code j, p_j is the increase in Pre-K enrollment in 1998-99 in zip code j, x_j is the median family income of zip code j, and η reflects the fraction of median family income of UPK compliers. $c(x)$ is the probability an individual will be charged with a crime at age 18-19 given their family income, *x*. It is approximated as,

$$c(x) = c_0\left(1 + \epsilon\left(\frac{x - x_0}{x_0}\right)\right),$$

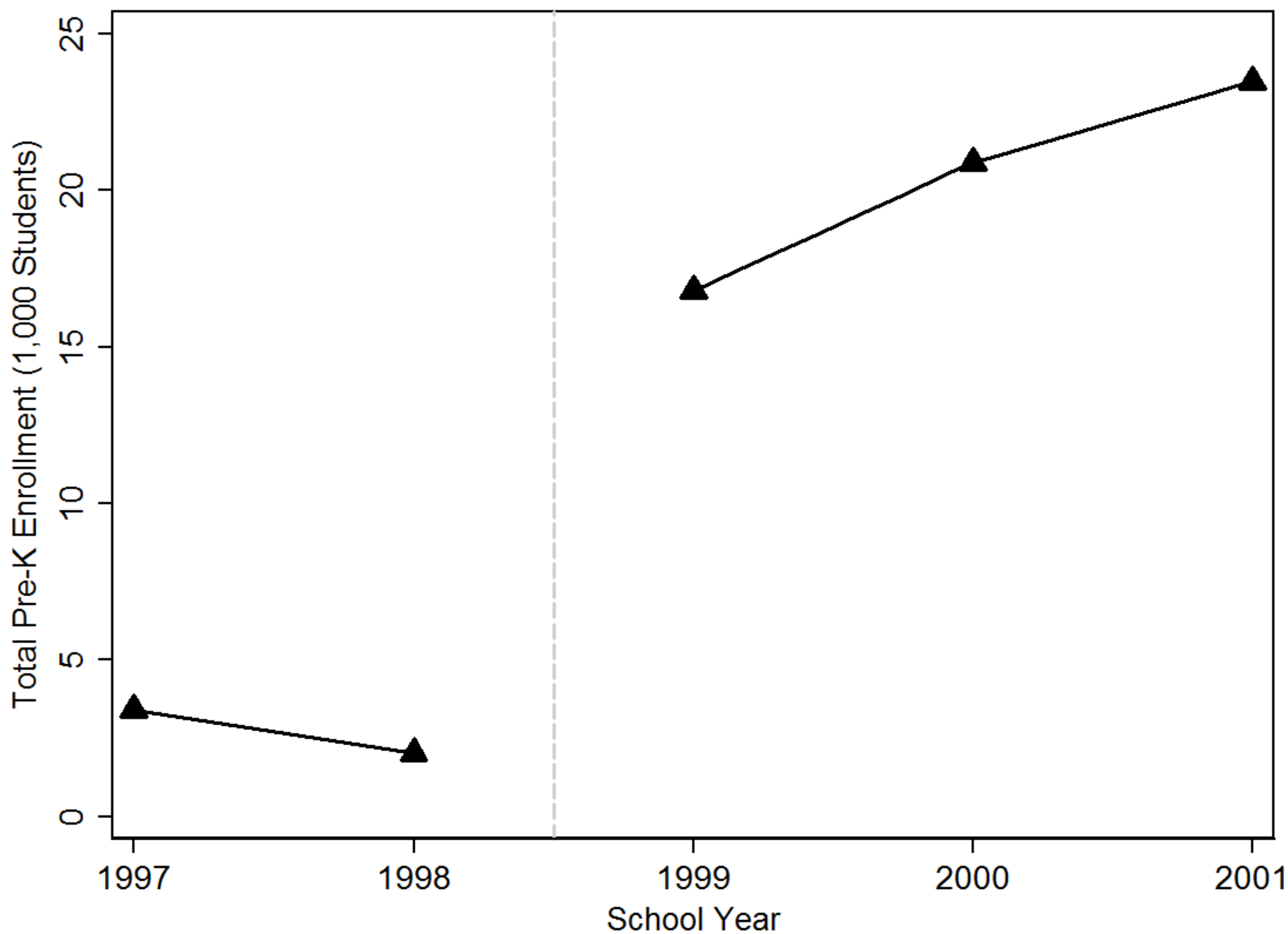
Where ϵ is the elasticity of family income with respect to crime and x_0 is the lowest zip code median family income. I estimate ϵ using a probit regression of an indicator for any criminal charge at age 18-19 on family income (at or before age 17) using black southerners in the National Longitudinal Survey of Youth 1997. This yields $\hat{\epsilon} = -0.41$.

I calculate $\theta(\eta) = \frac{\tilde{Y}_B^R(\eta)}{\overline{Y_B^R}}$ using the Common Core of Data and the 2000 Census. I use this ratio, an approximation of *r* from the 2000 Census and an approximation of $\overline{Y^R} = \frac{N^B}{N^R} * \bar{Y}$ to rewrite Equation 5 as,

$$\tilde{Y}_B^R(\eta,\alpha) = \theta(\eta) \cdot \overline{Y_B^R}(\alpha) = \theta(\eta) \cdot \frac{N^B}{N^R} \cdot \frac{\bar{Y}}{(r+\alpha(1-r))}, \qquad (6)$$

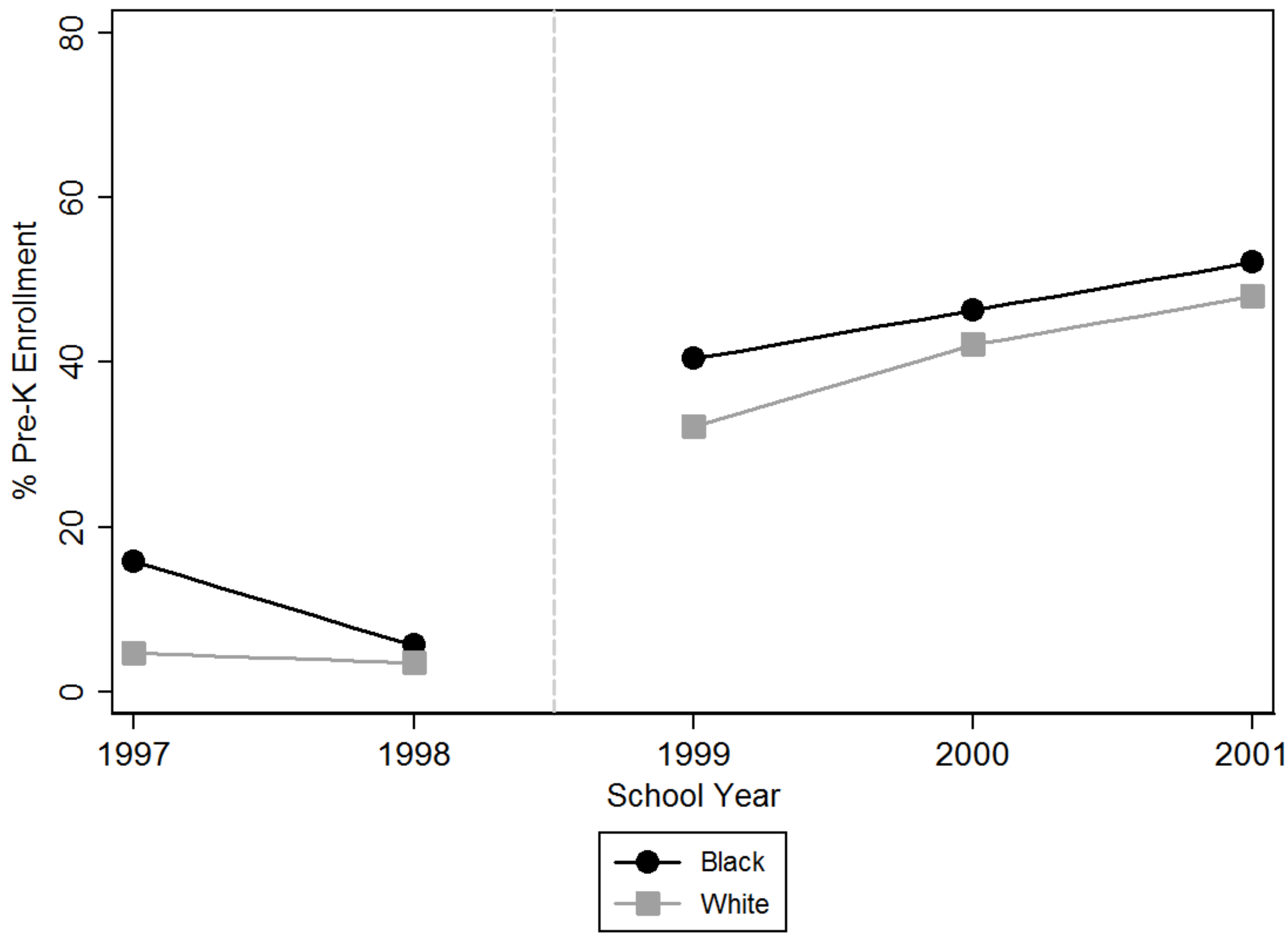
Where $\bar{Y}$, $\frac{N^B}{N^R}$, and *r* are observed or approximated. Figure 9 shows the TOT effect as percentage of $\tilde{Y}_B^R(\eta,\alpha)$ for various choices of η and α.

Figure 1A: Total Pre-K Enrollment in Oklahoma Public Schools



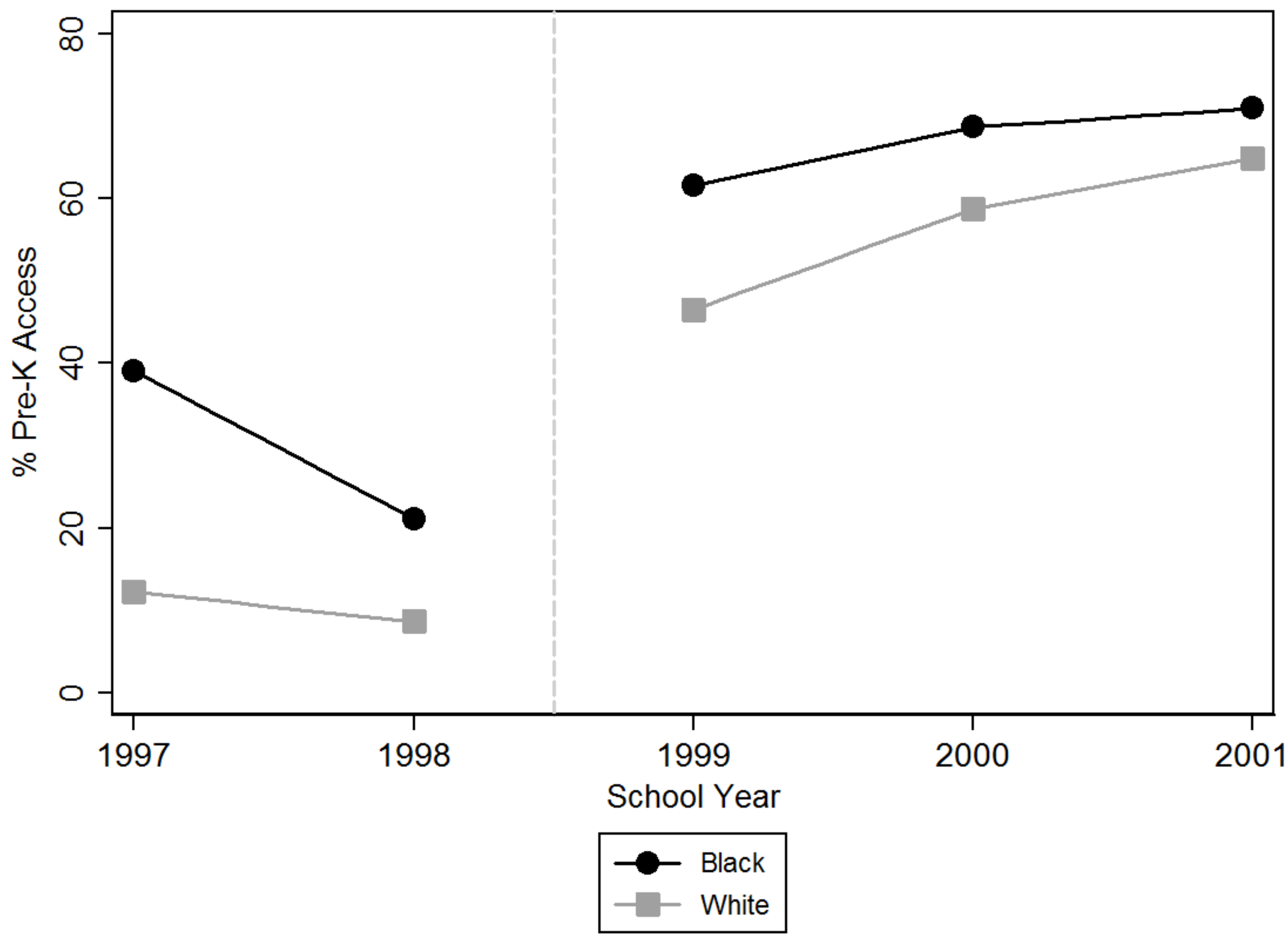
Notes: Oklahoma statewide Pre-K enrollment obtained by aggregating data from the School Universe Survey of the Common Core of Data produced by the National Center for Education Statistics.

Figure 1B: Percent Pre-K Enrollment in Oklahoma Public Schools, by Race



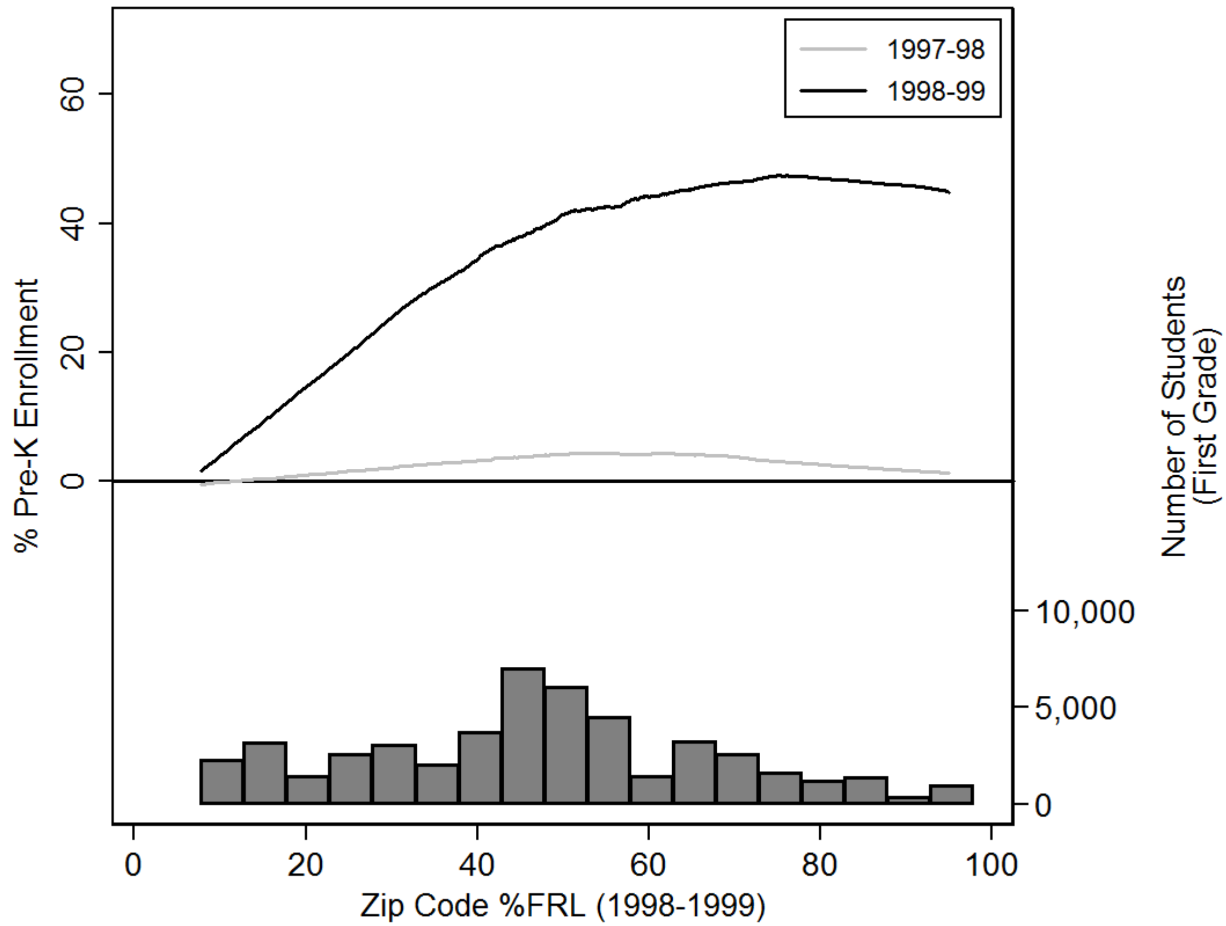
Notes: School-level Pre-K enrollment for Oklahoma is obtained from the School Universe Survey of the Common Core of Data produced by the National Center for Education Statistics. % Pre-K enrollment is defined as the aggregate state Pre-K enrollment in year t divided by the aggregate state Grade 1 enrollment 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 1C: Percent Pre-K Access in Oklahoma Public Schools, by Race



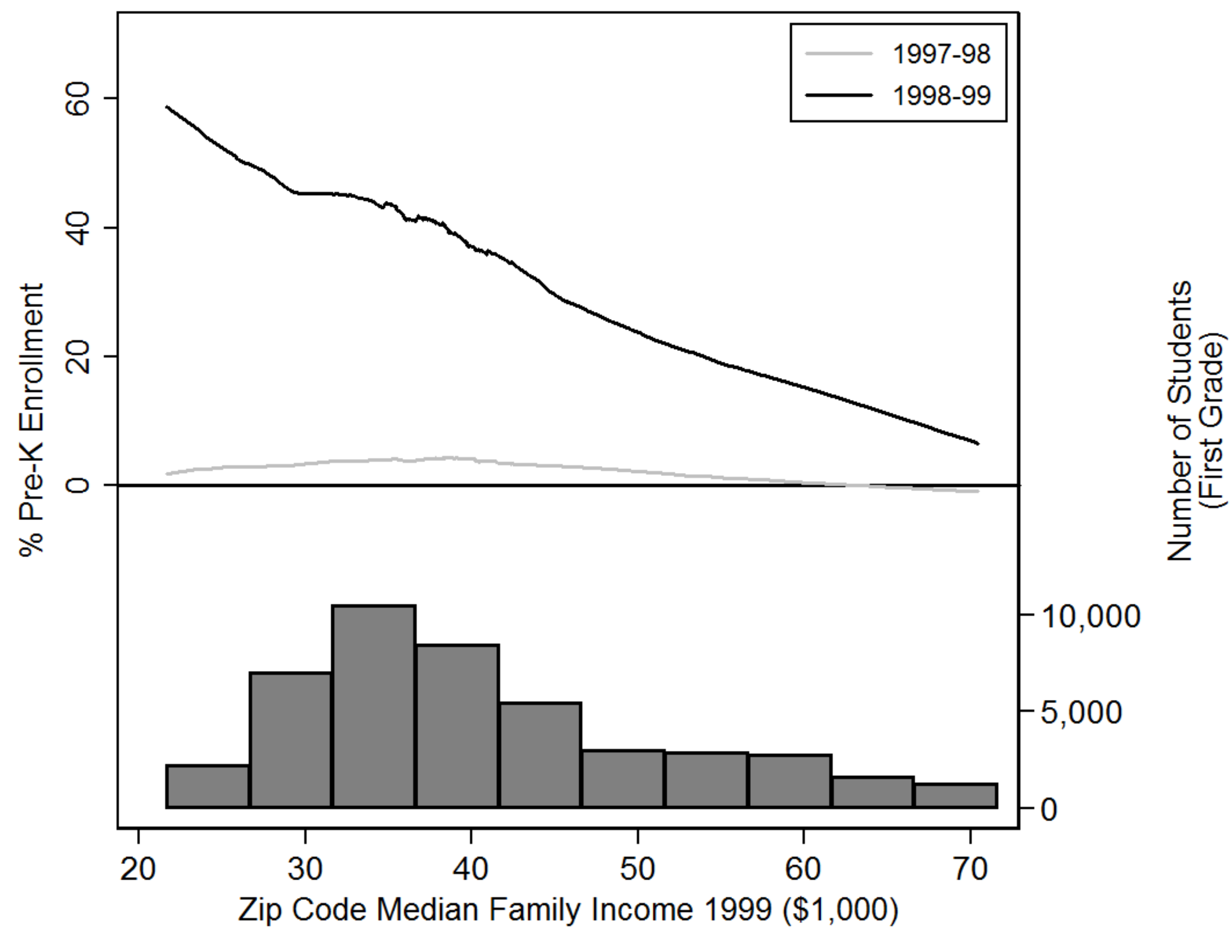
Notes: School-level Pre-K enrollment for Oklahoma is obtained from the School Universe Survey of the Common Core of Data produced by the National Center for Education Statistics. % Pre-K access is defined as the aggregate state Pre-K enrollment in schools with Pre-K enrollment greater than zero in year t divided by the aggregate state Grade 1 enrollment in t+2.

Figure 2A: Percent Pre-K Enrollment in Oklahoma Public Schools, by Zip Code %FRL



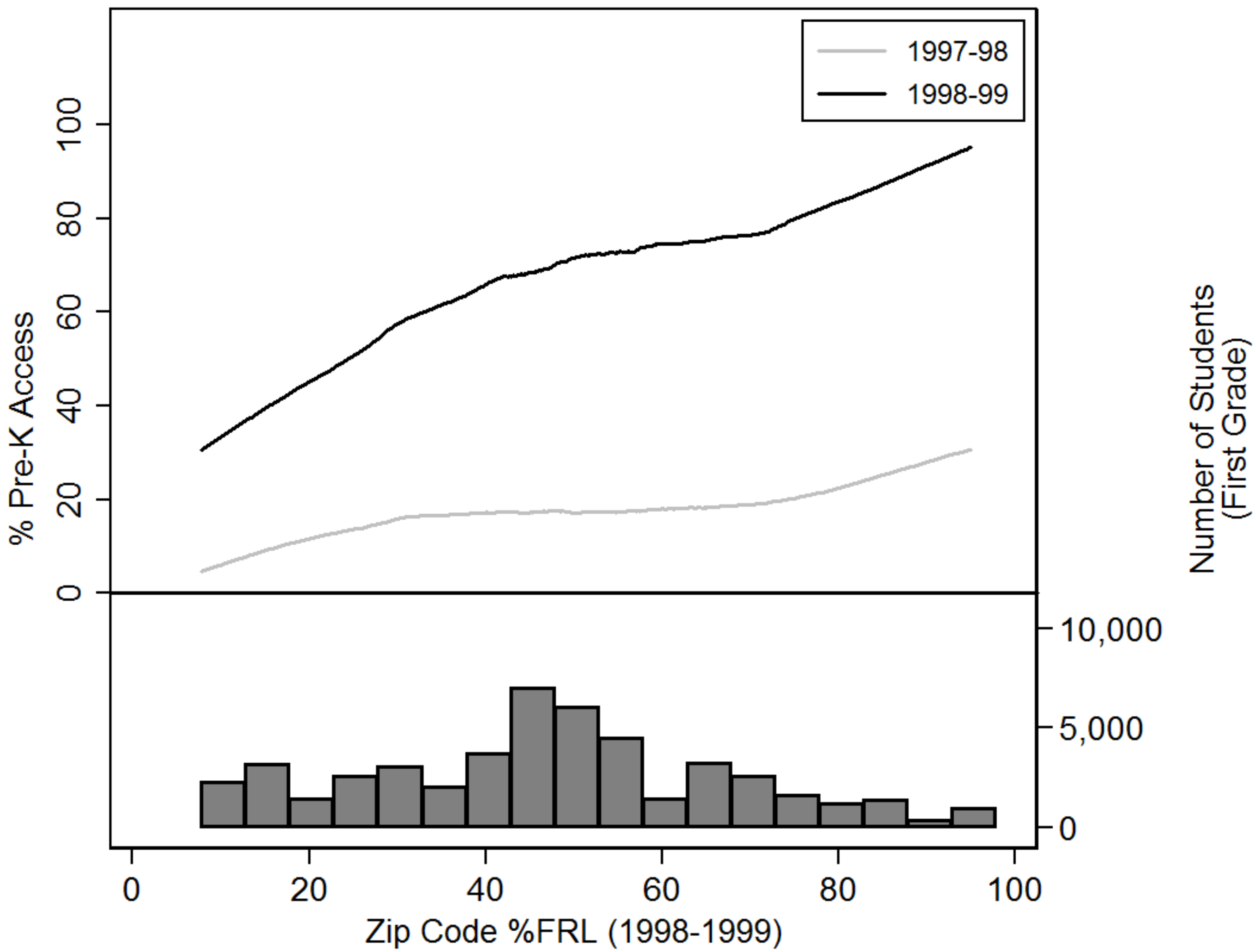
Notes: The top panel depicts a locally weighted regression of %Pre-K enrollment on %FRL (seperately for 1997-98 and 1998-99), where the school building zip code is the unit of observation. The bottom panel shows the distribution zip code %FRL. School-level Pre-K enrollment for Oklahoma is obtained from 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 aggregate zip code Pre-K enrollment in year t divided by the aggregate zip code Grade 1 enrollment in t+2. Zip code %FRL is defined as the total FRL students divided by the total enrollment in zip code in 1998-99.

Figure 2B: Percent Pre-K Enrollment in Oklahoma Public Schools, by Zip Code Median Family Income



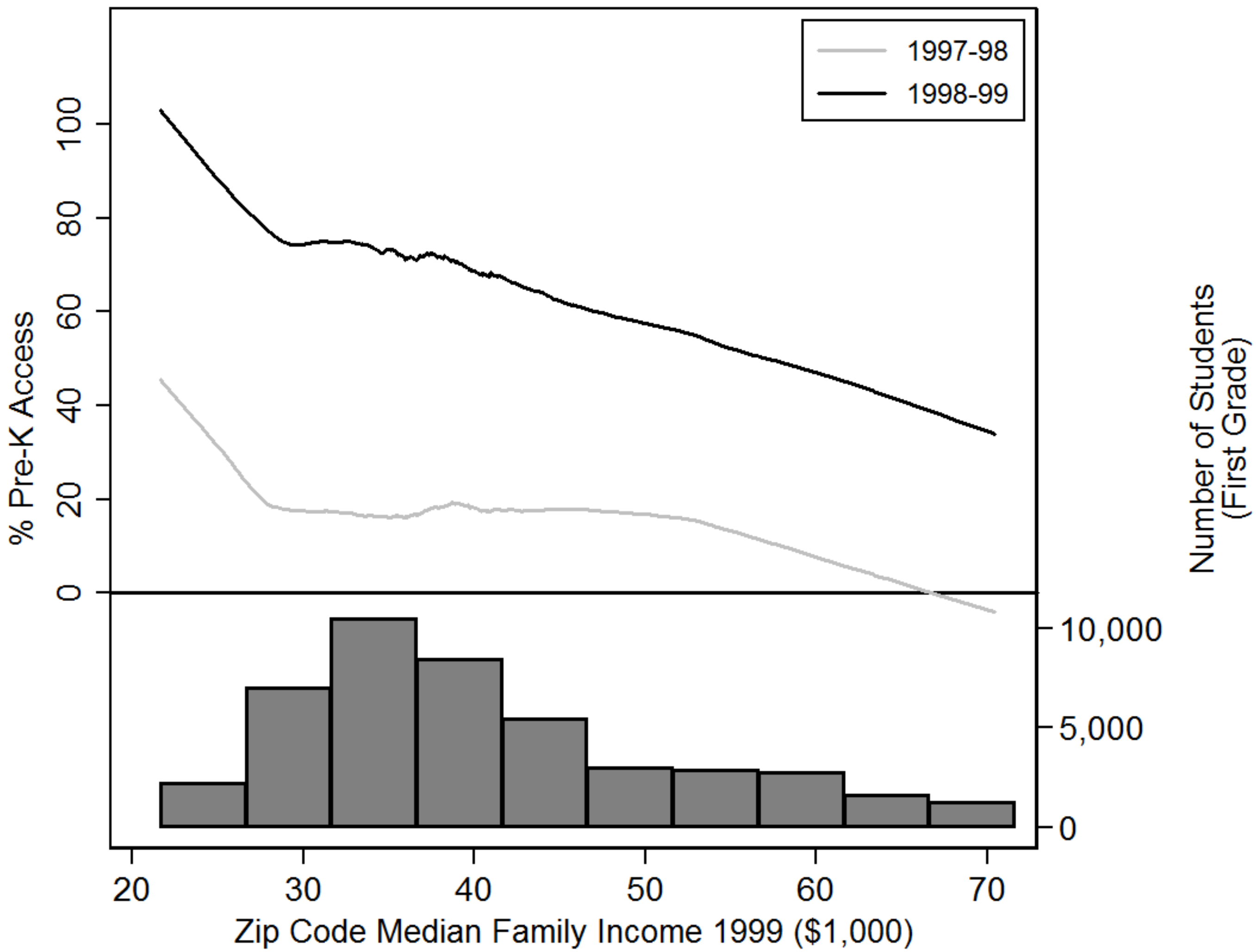
Notes: The top panel depicts a locally weighted regression of %Pre-K enrollment on zip code median family income (seperately for 1997-98 and 1998-99), where the school building zip code is the unit of observation. The bottom panel shows the distribution zip code %FRL. School-level Pre-K enrollment for Oklahoma is obtained from the School Universe Survey of the Common Core of Data produced by the National Center for Education Statistics. Enrollment information, aggregated to the zip code-level is merged to zip code median family income in 1999 from the 2000 Census. % Pre-K enrollment is defined as the aggregate zip code Pre-K enrollment in year t divided by the aggregate zip code Grade 1 enrollment in t+2.

Figure 3A: Percent Pre-K Access in Oklahoma Public Schools, by Zip Code %FRL



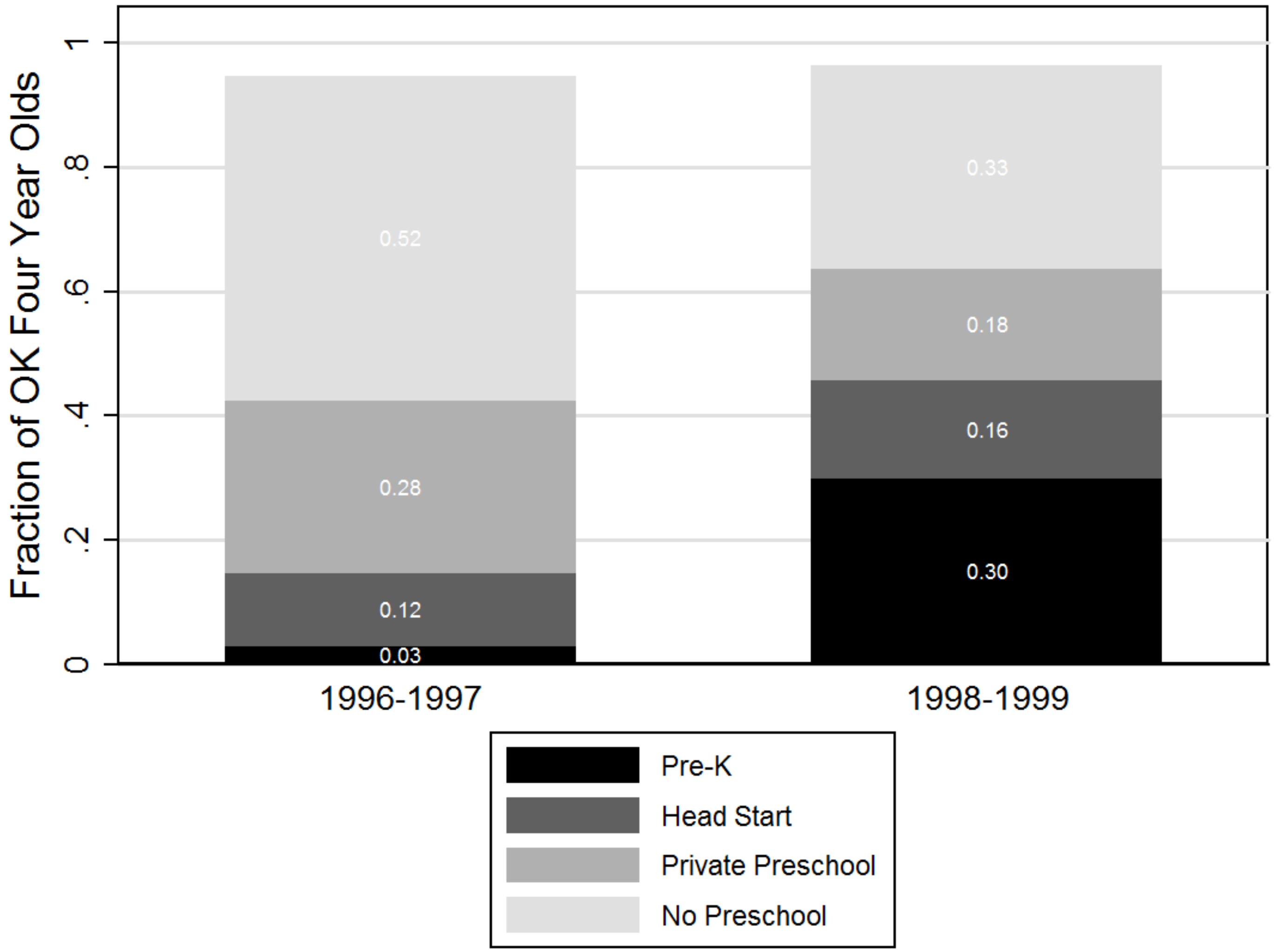
Notes: The top panel depicts a locally weighted regression of an indicator of Pre-K access on %FRL (seperately for 1997-98 and 1998-99), where the school building zip code is the unit of observation. The bottom panel shows the distribution zip code %FRL. School-level Pre-K enrollment for Oklahoma is obtained from the School Universe Survey of the Common Core of Data produced by the National Center for Education Statistics. Zip code Pre-K access is defined as one if the Pre-K enrollment in the zip code is greater than zero. Zip code %FRL is defined as the total FRL students divided by the total enrollment in zip code in 1998-99.

Figure 3B: Percent Pre-K Access in Oklahoma Public Schools, by Zip Code Median Family Income



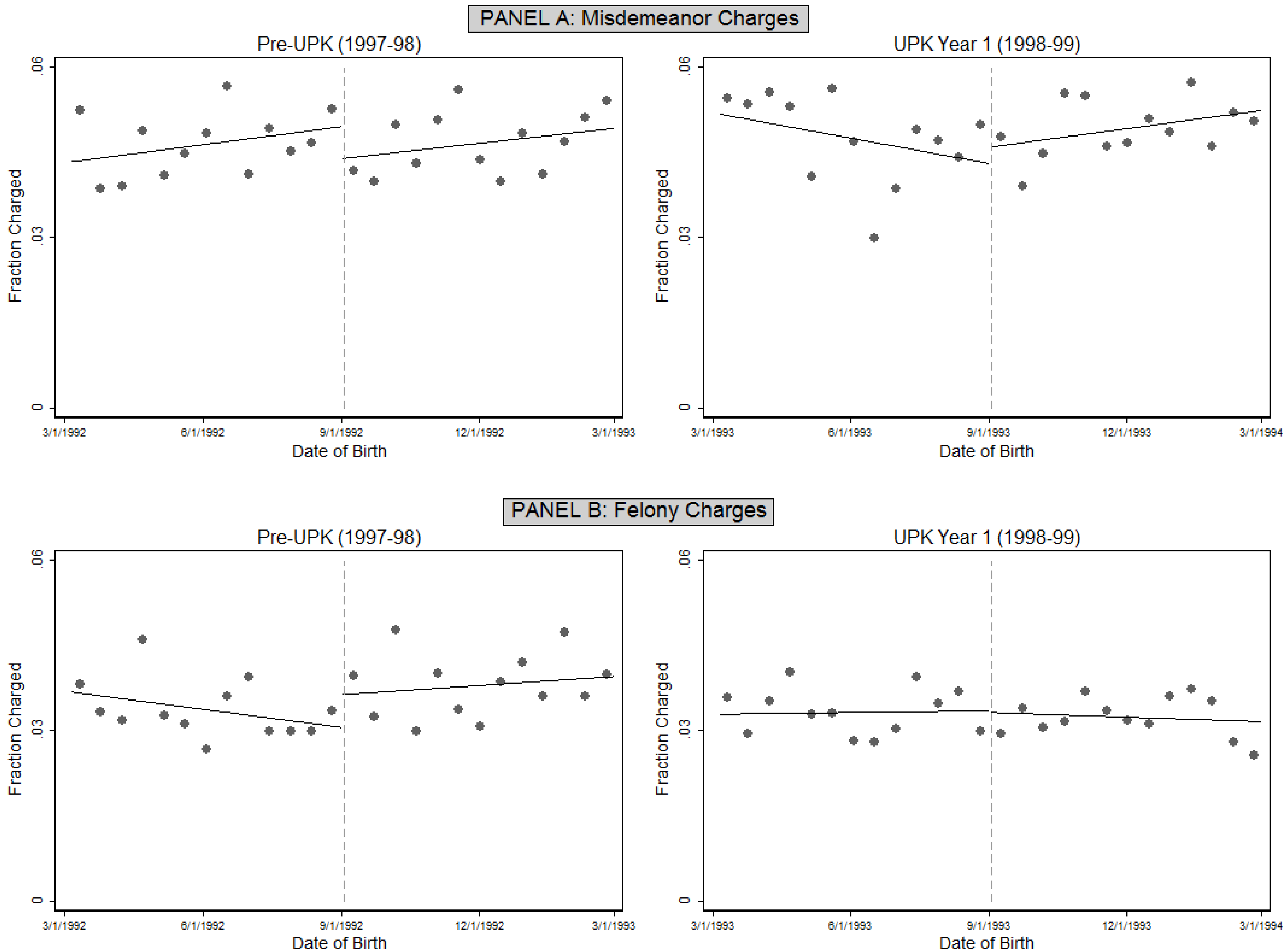
Notes: The top panel depicts a locally weighted regression of an indicator of Pre-K access on %FRL (seperately for 1997-98 and 1998-99), where the school building zip code is the unit of observation. The bottom panel shows the distribution zip code %FRL. School-level Pre-K enrollment for Oklahoma is obtained from the School Universe Survey of the Common Core of Data produced by the National Center for Education Statistics. Enrollment information, aggregated to the zip code-level is merged to zip code median family income in 1999 from the 2000 Census. Zip code Pre-K access is defined as one if the Pre-K enrollment in the zip code is greater than zero.

Figure 4: Fraction of Oklahoma Four Year Olds by Preschool Type (October CPS)



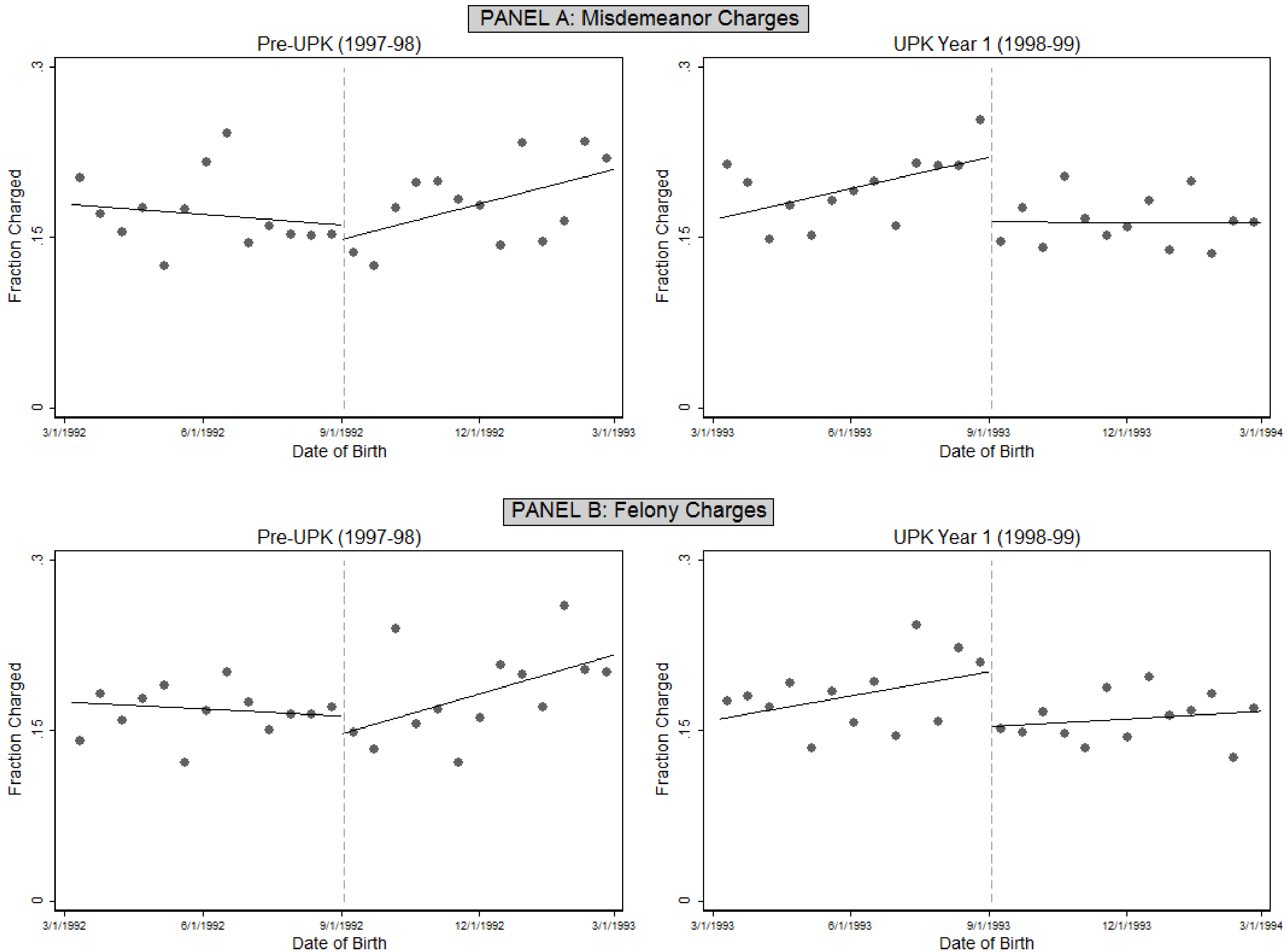
Notes: Data from October Current Population Survey. The survey is collected in October of each year, so 1998 falls in the 1998-99 school year (UPK Year 1). The fraction of four year olds attending public preschool is allocated to head start and Pre-K based on each program's share of the statewide total enrollment both programs in the relevant years.

Figure 5: Likelihood of Criminal Charge at Age 18-19, by Birthdate (Whites)



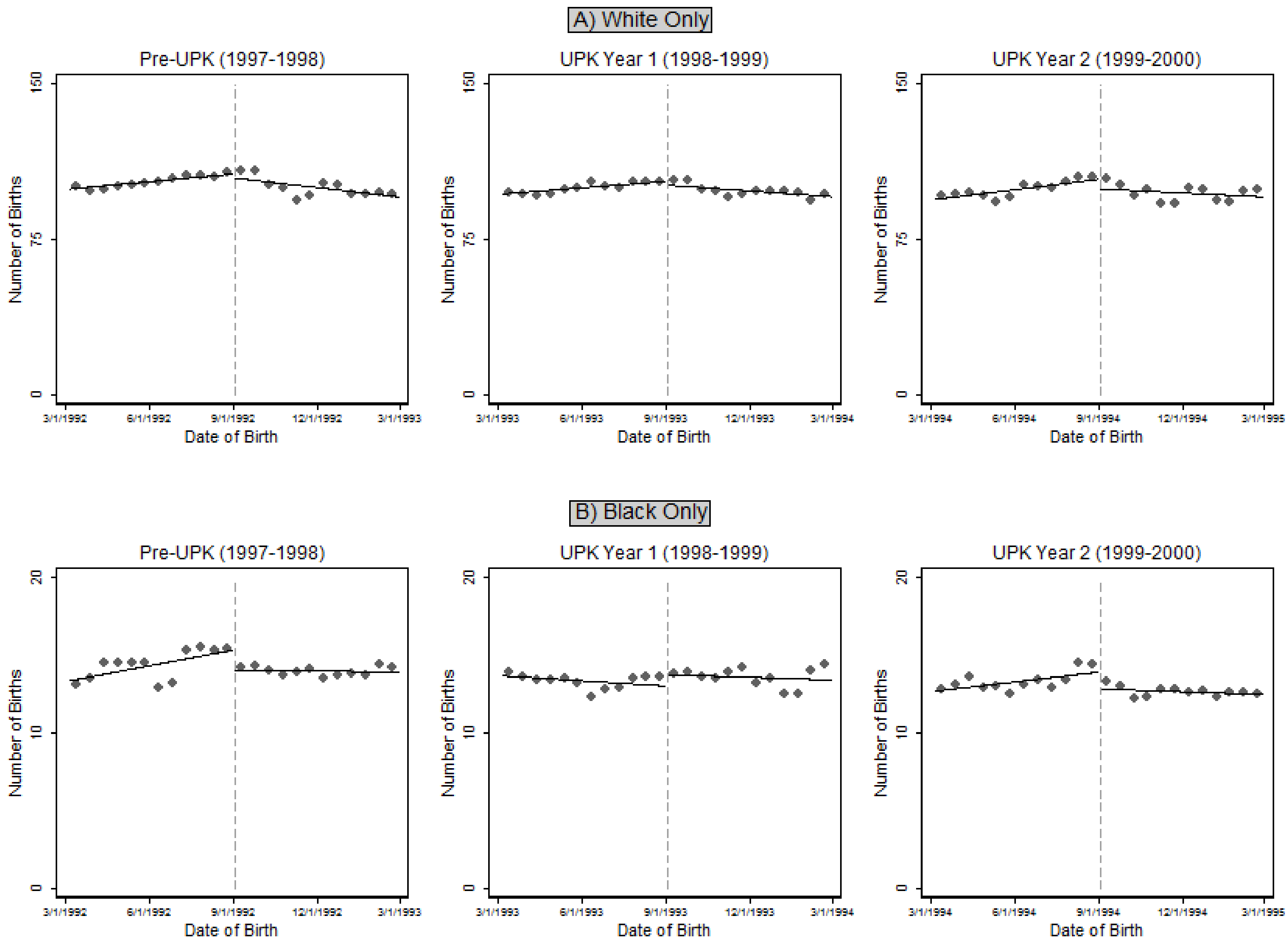
Notes: Data from Oklahoma criminal court records and birth records. Each dot corresponds to the mean for a bin of 15 days. Likelihood of Criminal Charge at Age 18-19 = Number of individuals charged with a crime at age 18-19 in OK with given birthday / Number of individuals born on that day in OK.

Figure 6: Likelihood of Criminal Charge at Age 18-19, by Birthdate (Blacks)



Notes: Data from Oklahoma criminal court records and birth records. Each dot corresponds to the mean for a bin of 15 days. Likelihood of Criminal Charge at Age 18-19 = Number of individuals charged with a crime at age 18-19 in OK with given birthday / Number of individuals born on that day in OK.

Figure 7: Number of Births, by Birthdate



Notes: Data from Oklahoma birth records. Each dot corresponds to the mean for a bin of 15 days.

Figure 8: Distribution of Black Grade 1 Population and Black UPK Compliers, by Zip Code Black Median Income

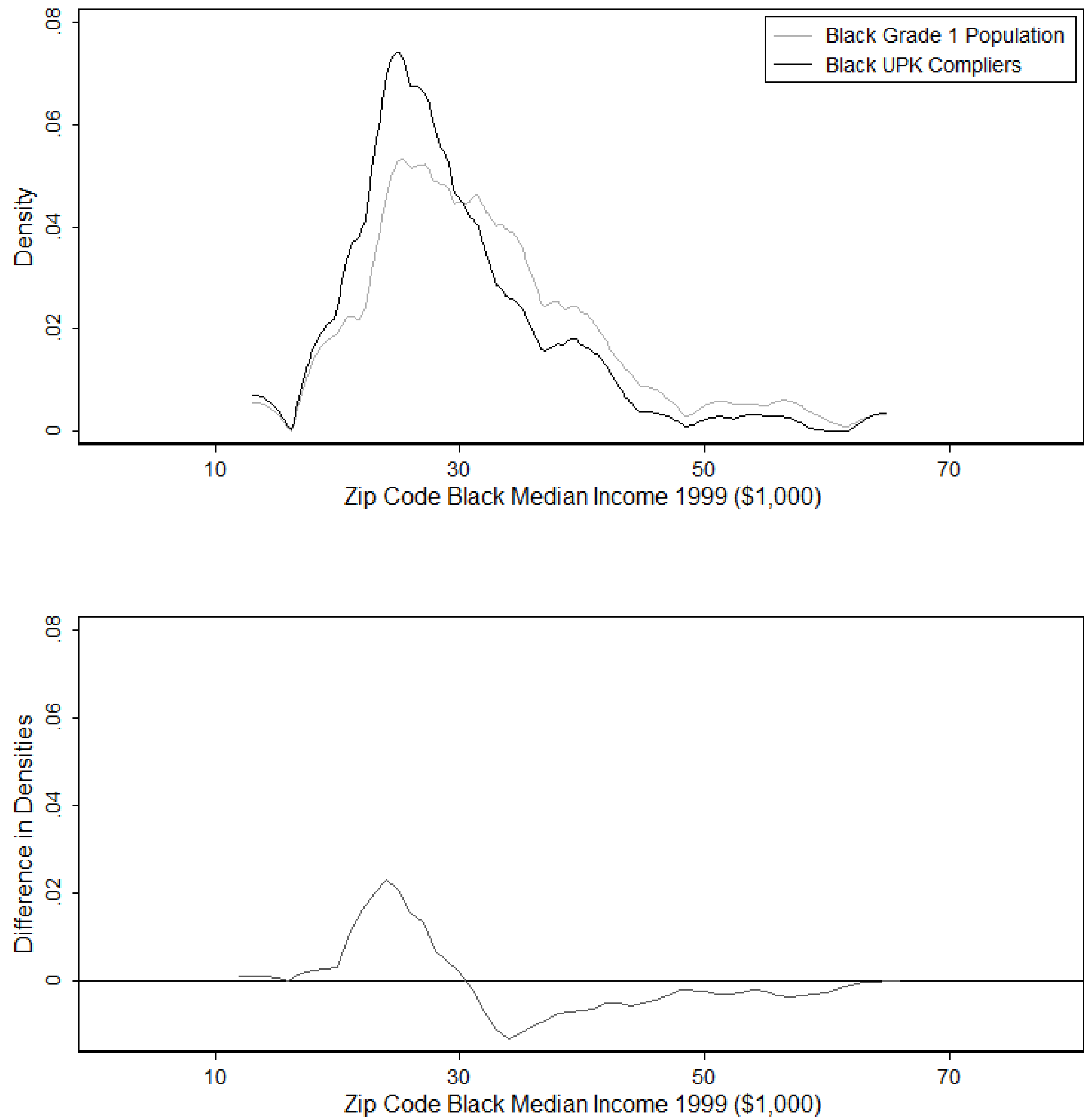


Figure 9: Back-of-the-Envelope TOT Effect Relative to Baseline Likelihood of Misdemeanor for Black Residents (Age 18-19)

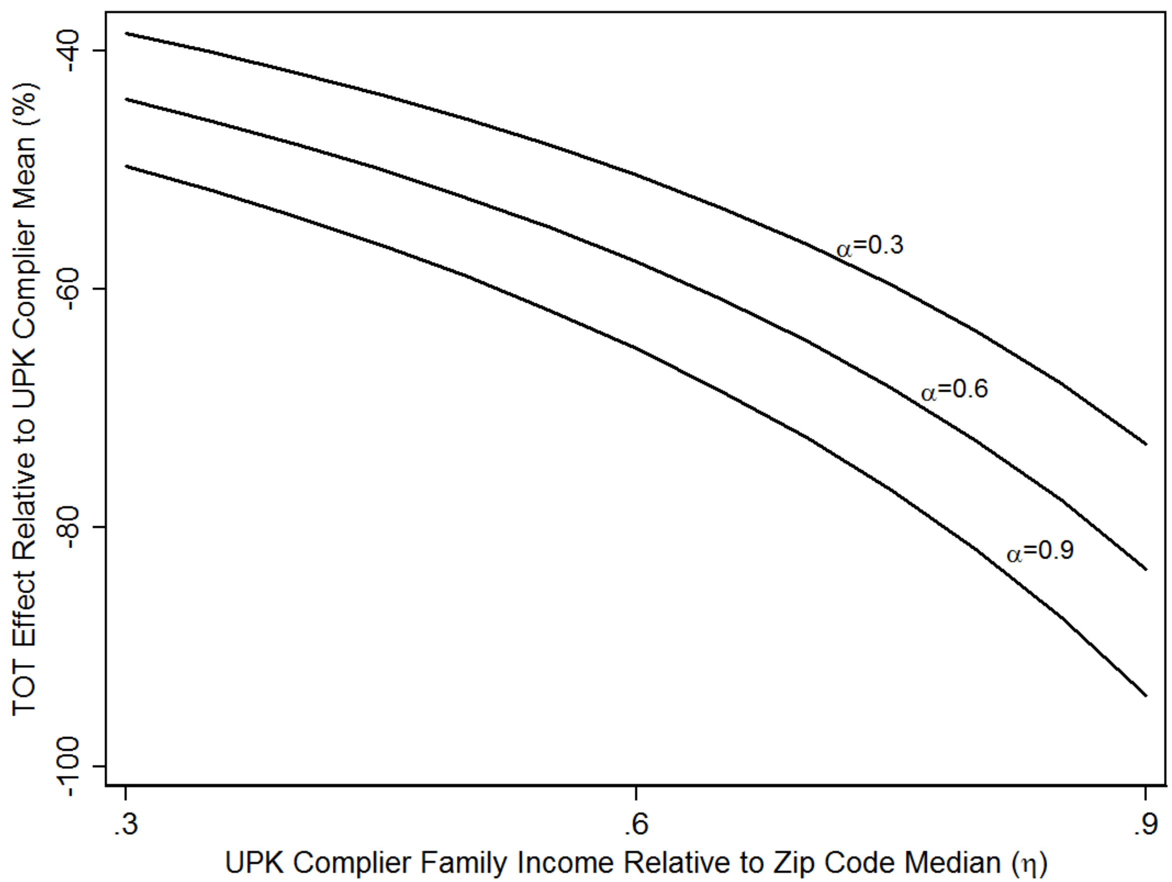


Figure A1: On Demand Court Records Screenshot

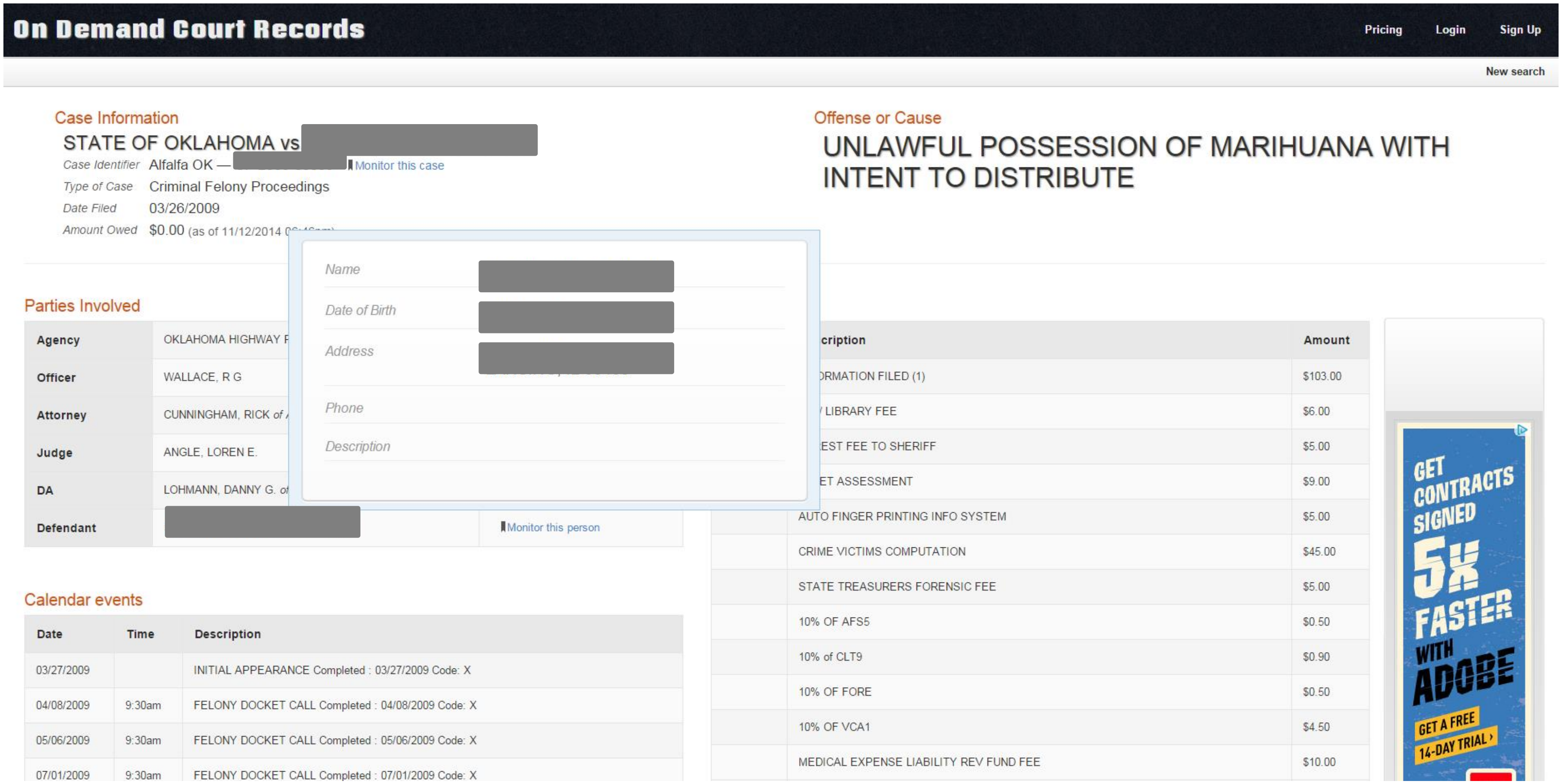
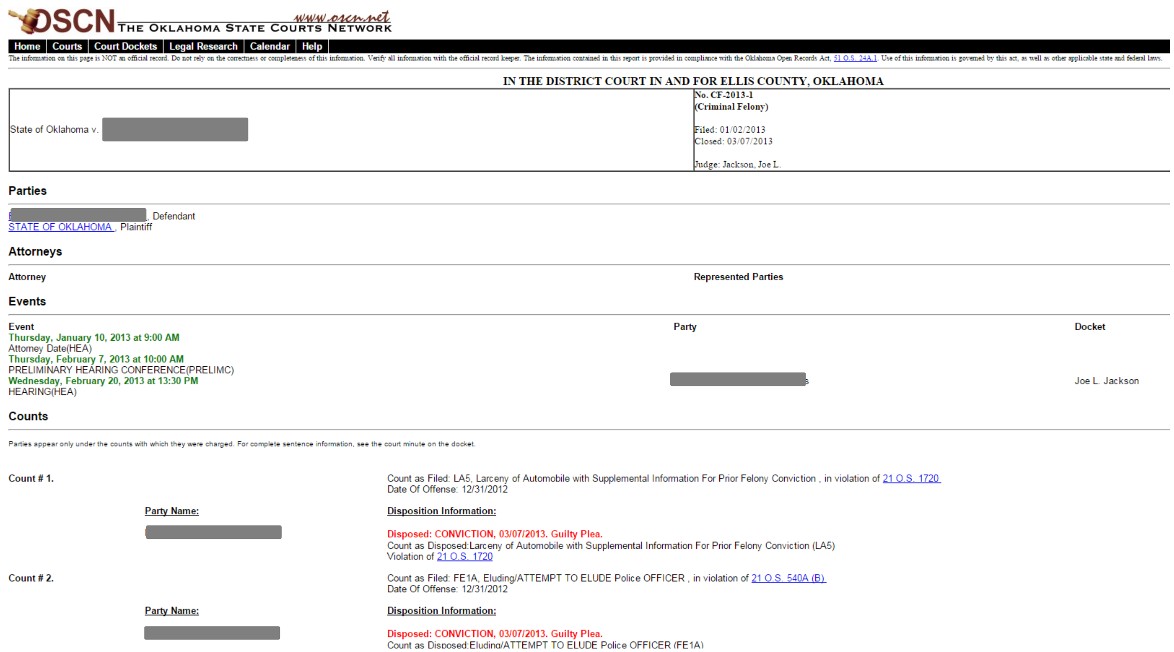


Figure A2: Oklahoma State Courts Network Screenshot



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IN THE DISTRICT COURT IN AND FOR ELLIS COUNTY, OKLAHOMA

State of Oklahoma v. [redacted]	**No. CF-2013-1** **(Criminal Felony)** Filed: 01/02/2013 Closed: 03/07/2013 Judge: Jackson, Joe L.

Parties

[redacted], Defendant
STATE OF OKLAHOMA, Plaintiff

Attorneys

Attorney	Represented Parties

Events

Event	Party	Docket
Thursday, January 10, 2013 at 9:00 AM Attorney Date(HEA) **Thursday, February 7, 2013 at 10:00 AM** PRELIMINARY HEARING CONFERENCE(PRELIMC) **Wednesday, February 20, 2013 at 13:30 PM** HEARING(HEA)	[redacted]	Joe L. Jackson

Counts

Parties appear only under the counts with which they were charged. For complete sentence information, see the court minute on the docket.

Count # 1. Count as Filed: LA5, Larceny of Automobile with Supplemental Information For Prior Felony Conviction , in violation of 21 O.S. 1720
Date Of Offense: 12/31/2012

Party Name: [redacted]

Disposition Information:

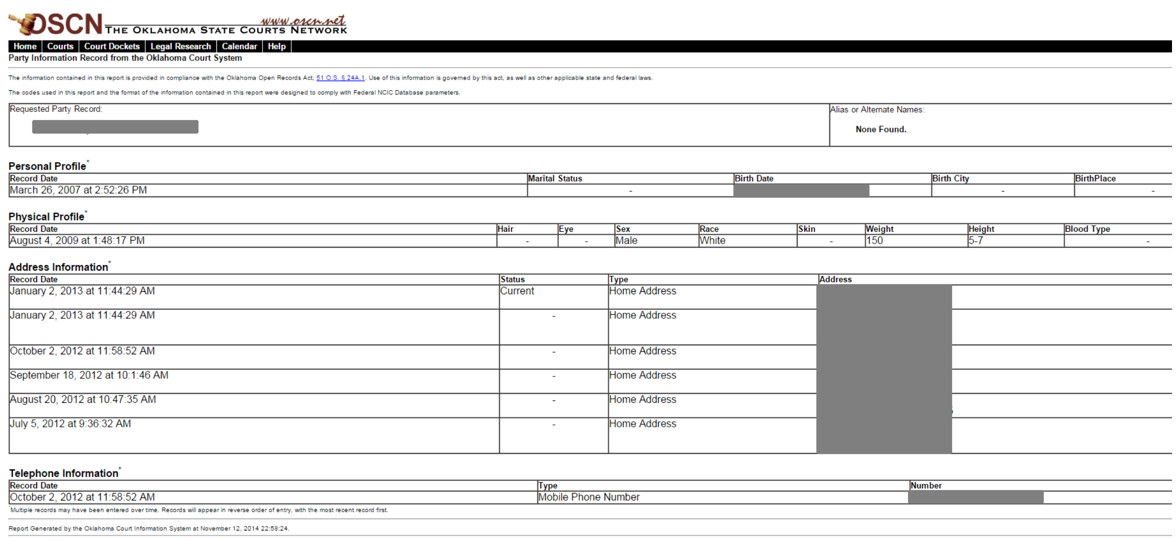
Disposed: CONVICTION, 03/07/2013. Guilty Plea.
Count as Disposed:Larceny of Automobile with Supplemental Information For Prior Felony Conviction (LA5)
Violation of 21 O.S. 1720

Count # 2. Count as Filed: FE1A, Eluding/ATTEMPT TO ELUDE Police OFFICER , in violation of 21 O.S. 540A (B)
Date Of Offense: 12/31/2012

Party Name: [redacted]

Disposition Information:

Disposed: CONVICTION, 03/07/2013. Guilty Plea.
Count as Disposed:Eluding/ATTEMPT TO ELUDE Police OFFICER (FE1A)



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Party Information Record from the Oklahoma Court System

The information contained in this report is provided in compliance with the Oklahoma Open Records Act, 51 O.S. § 24A.1. Use of this information is governed by this act, as well as other applicable state and federal laws.

The codes used in this report and the format of the information contained in this report were designed to comply with Federal NCIC Database parameters.

Requested Party Record:	Alias or Alternate Names:
[redacted]	**None Found.**

Personal Profile*

Record Date	Marital Status	Birth Date	Birth City	BirthPlace
March 26, 2007 at 2:52:26 PM	-	[redacted]	-	-

Physical Profile*

Record Date	Hair	Eye	Sex	Race	Skin	Weight	Height	Blood Type
August 4, 2009 at 1:48:17 PM	-	-	Male	White	-	150	5-7	-

Address Information*

Record Date	Status	Type	Address
January 2, 2013 at 11:44:29 AM	Current	Home Address	[redacted]
January 2, 2013 at 11:44:29 AM	-	Home Address	
October 2, 2012 at 11:58:52 AM	-	Home Address	
September 18, 2012 at 10:1:46 AM	-	Home Address	
August 20, 2012 at 10:47:35 AM	-	Home Address	
July 5, 2012 at 9:36:32 AM	-	Home Address	

Telephone Information*

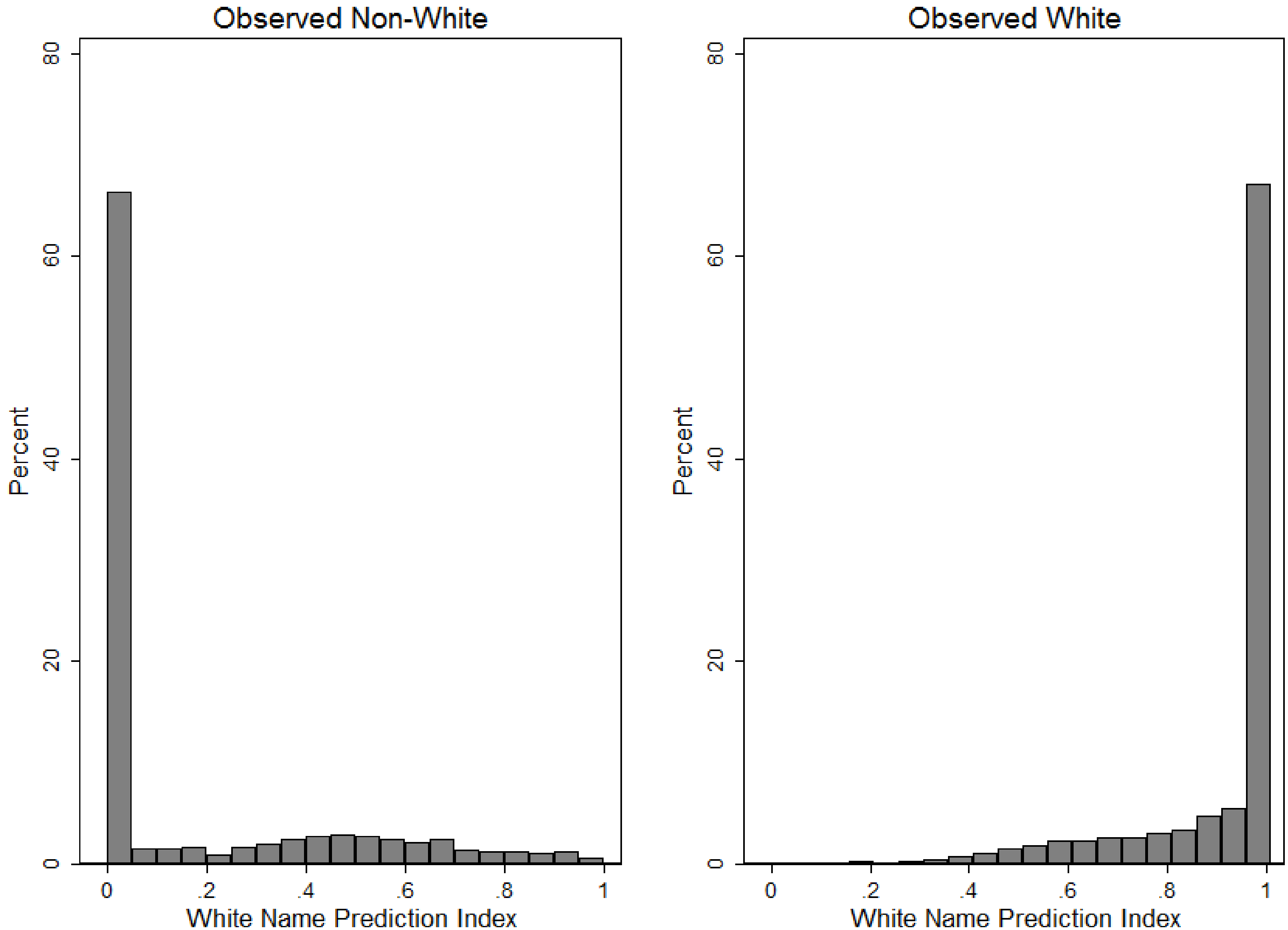
Record Date	Type	Number
October 2, 2012 at 11:58:52 AM	Mobile Phone Number	[redacted]

*Multiple records may have been entered over time. Records will appear in reverse order of entry, with the most recent record first.

Report Generated by the Oklahoma Court Information System at November 12, 2014 22:58:24.

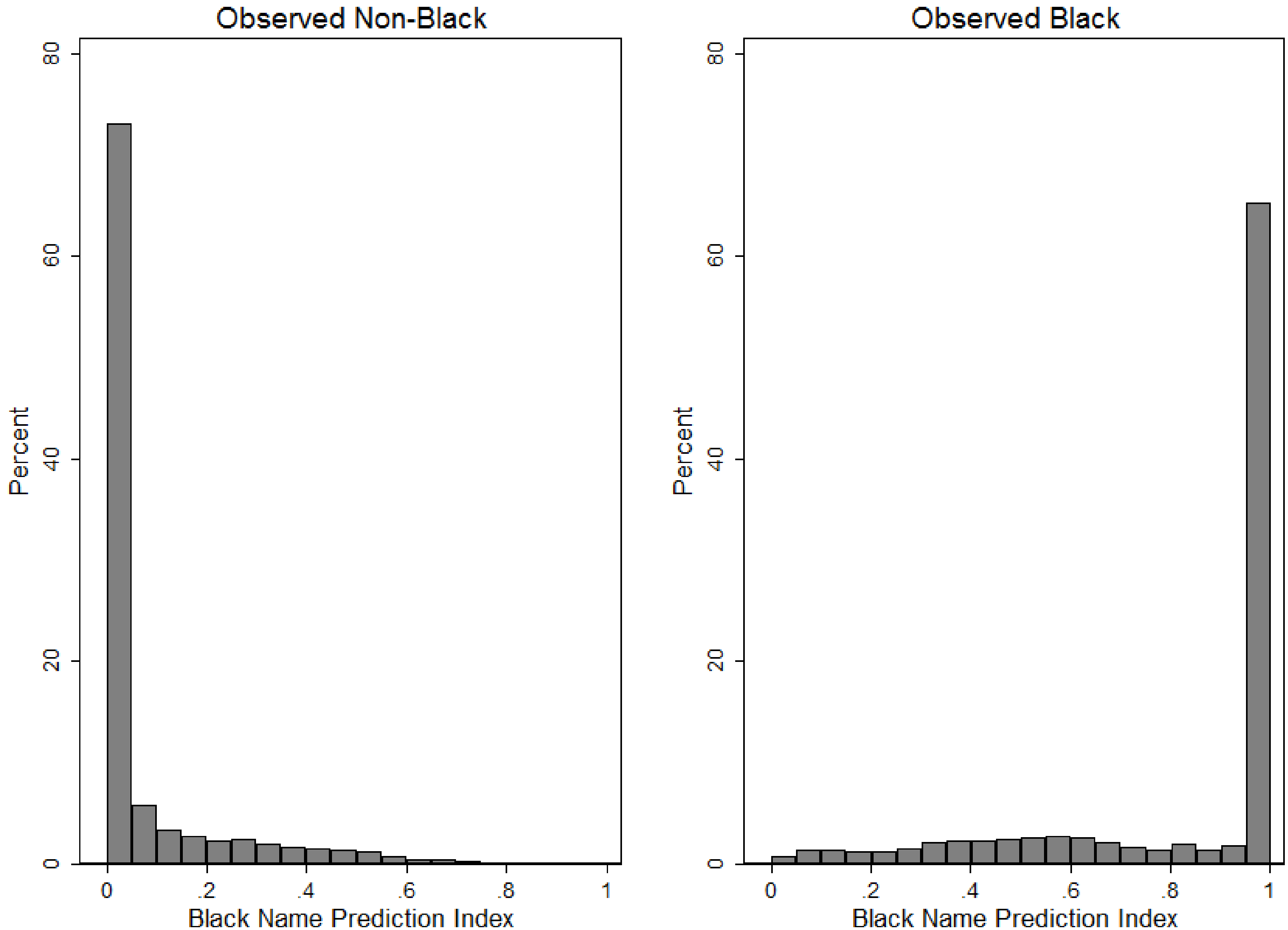
End of Transmission.

Figure A3: Distribution of White Name Prediction Index by Observed Race



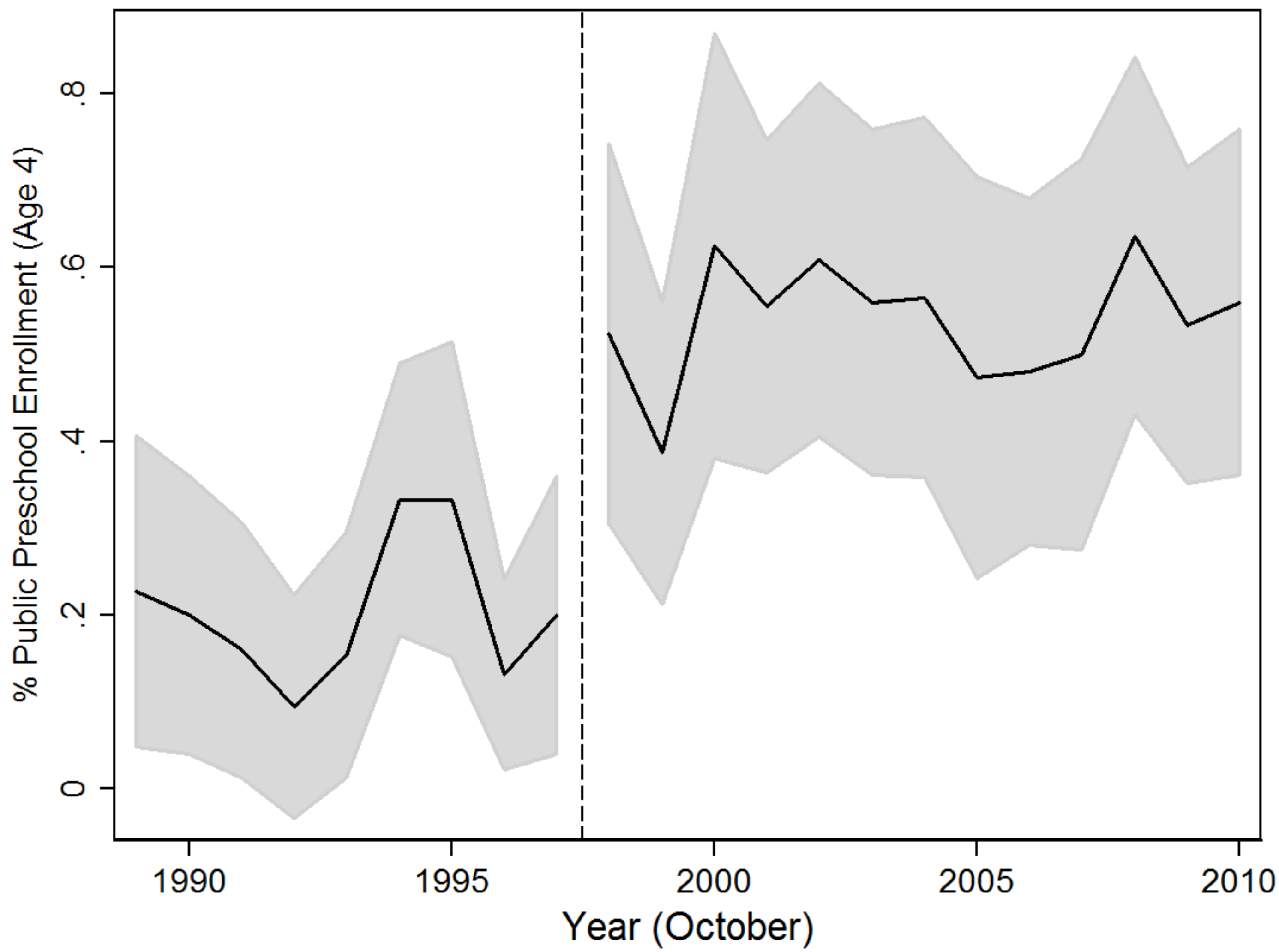
Note: White Name Prediction Index is the predicted probability from a probit regression with an indicator for white as the dependent variable and cubics of the fraction of each race observed with the same first name and cubics of the fraction of each race observed with the same last name.

Figure A4: Distribution of Black Name Prediction Index by Observed Race



Note: Black Name Prediction Index is the predicted probability from a probit regression with an indicator for black as the dependent variable and cubics of the fraction of each race observed with the same first name and cubics of the fraction of each race observed with the same last name.

Figure A5: Percent of Oklahoma Four-Year Olds Enrolled in Public Preschool (October CPS)



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

Figure A6: Percent of Oklahoma-born 18 and 19-Year Olds Residing in Oklahoma
American Community Survey 2005-2013

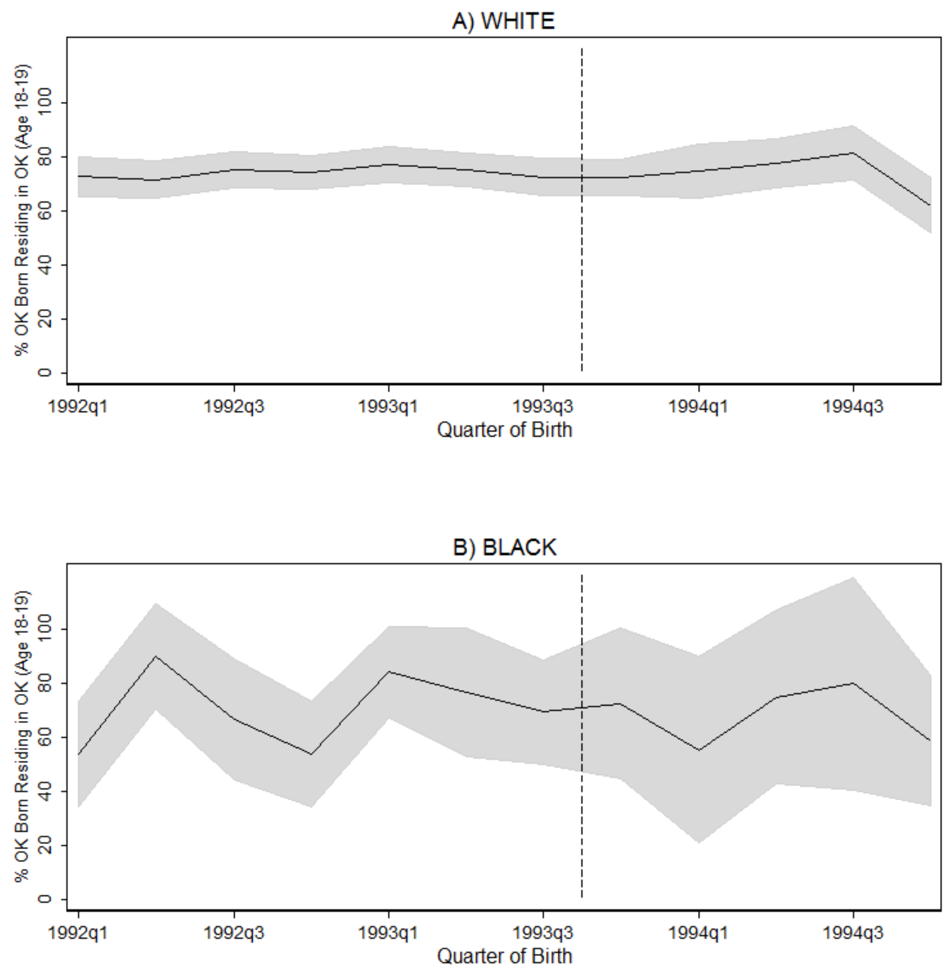


Figure A7: Local Linear Regression Discontinuity Estimates for White Felony and Misdemeanor Charges, by Kernel Bandwidth (in Days)

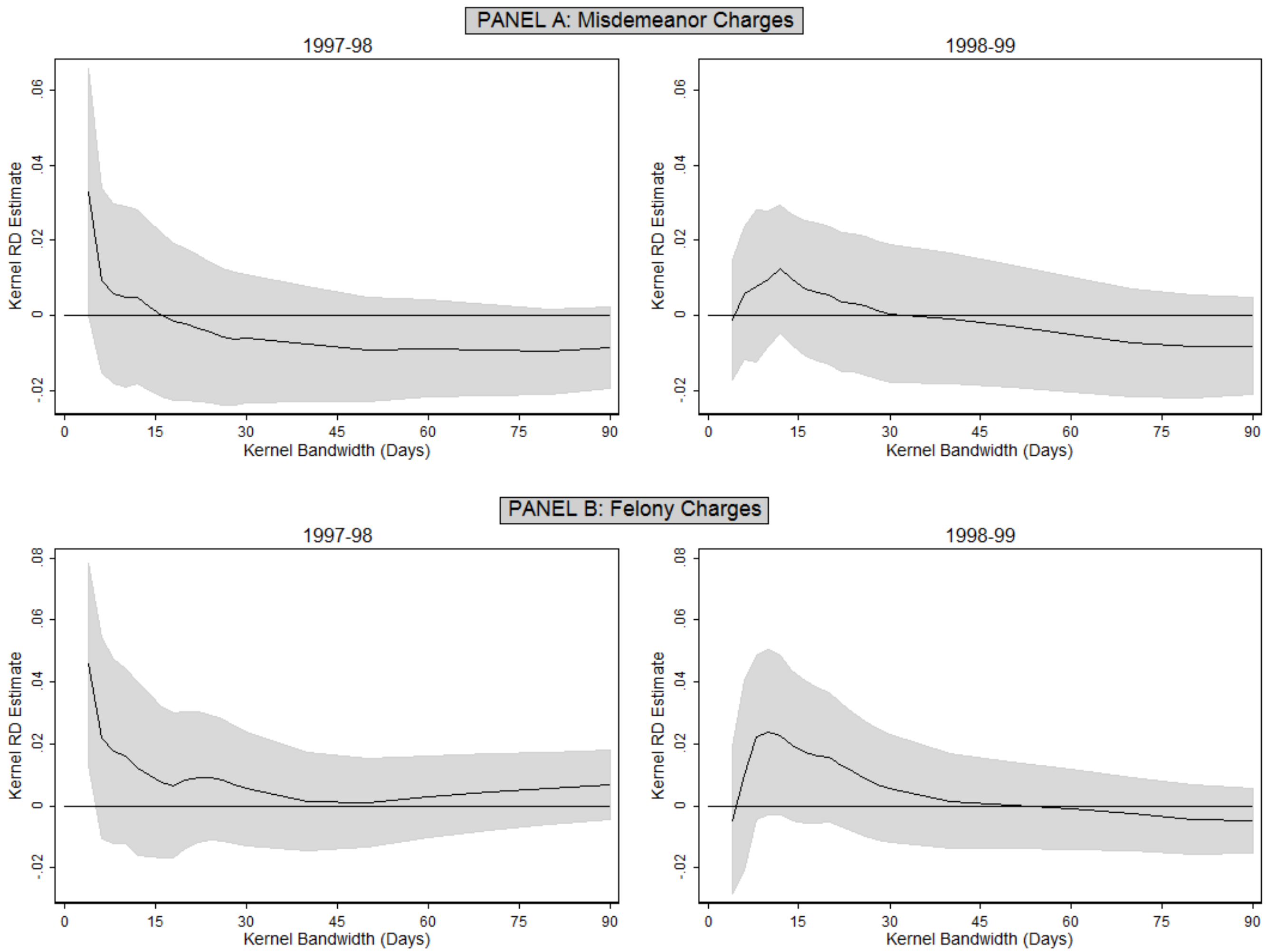


Figure A8: Local Linear Regression Discontinuity Estimates for Black Felony and Misdemeanor Charges, by Kernel Bandwidth (in Days)

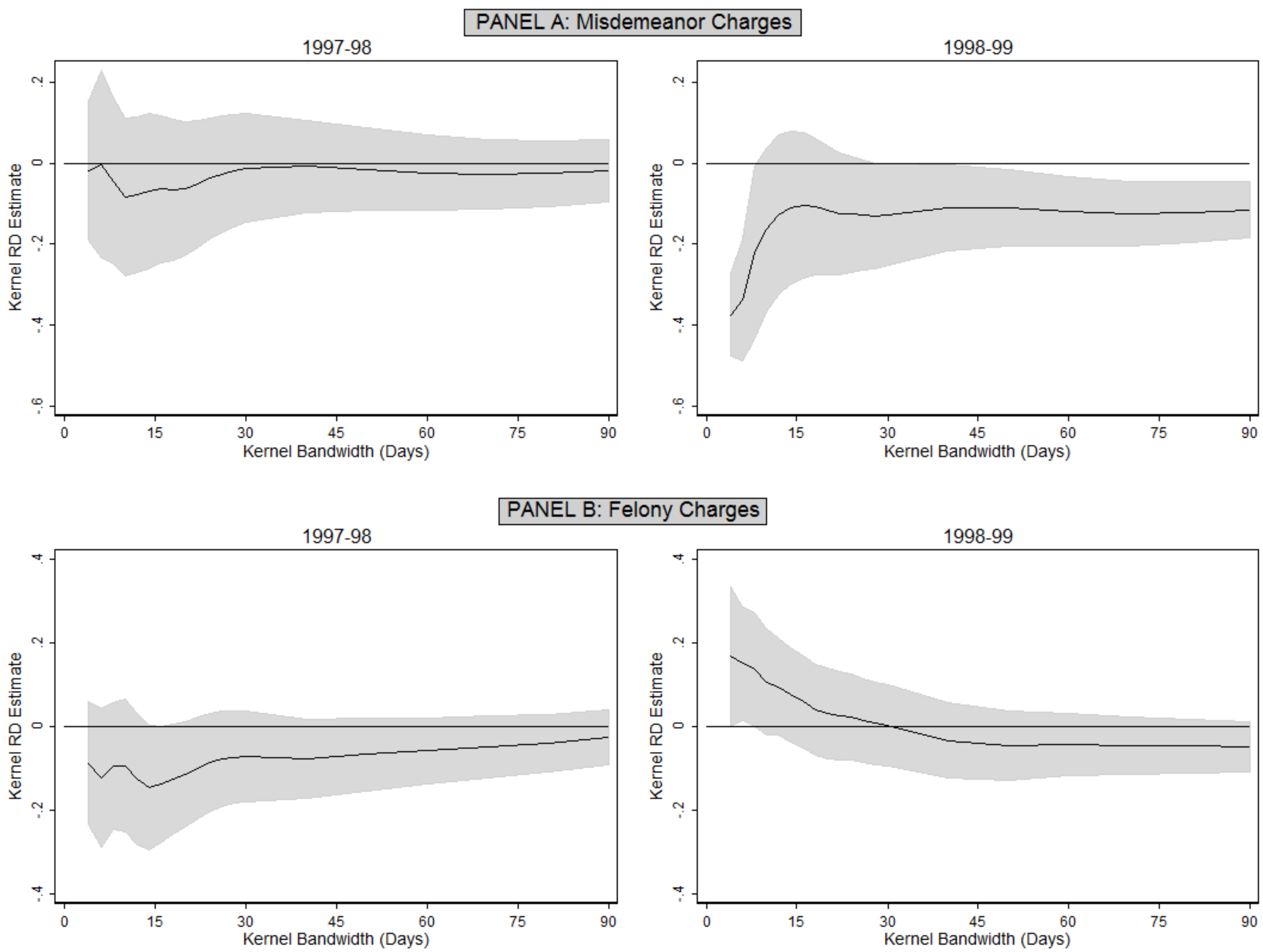
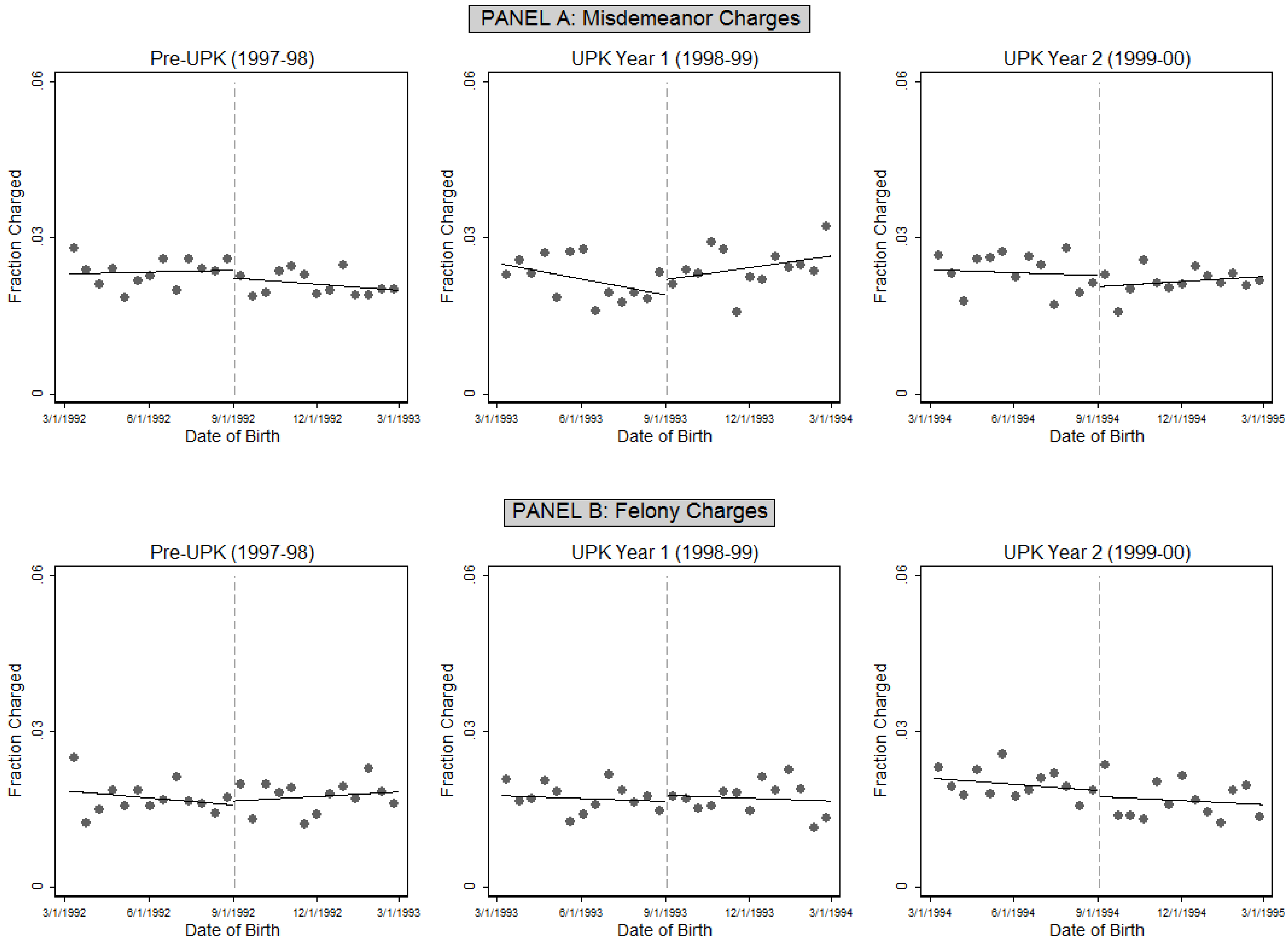
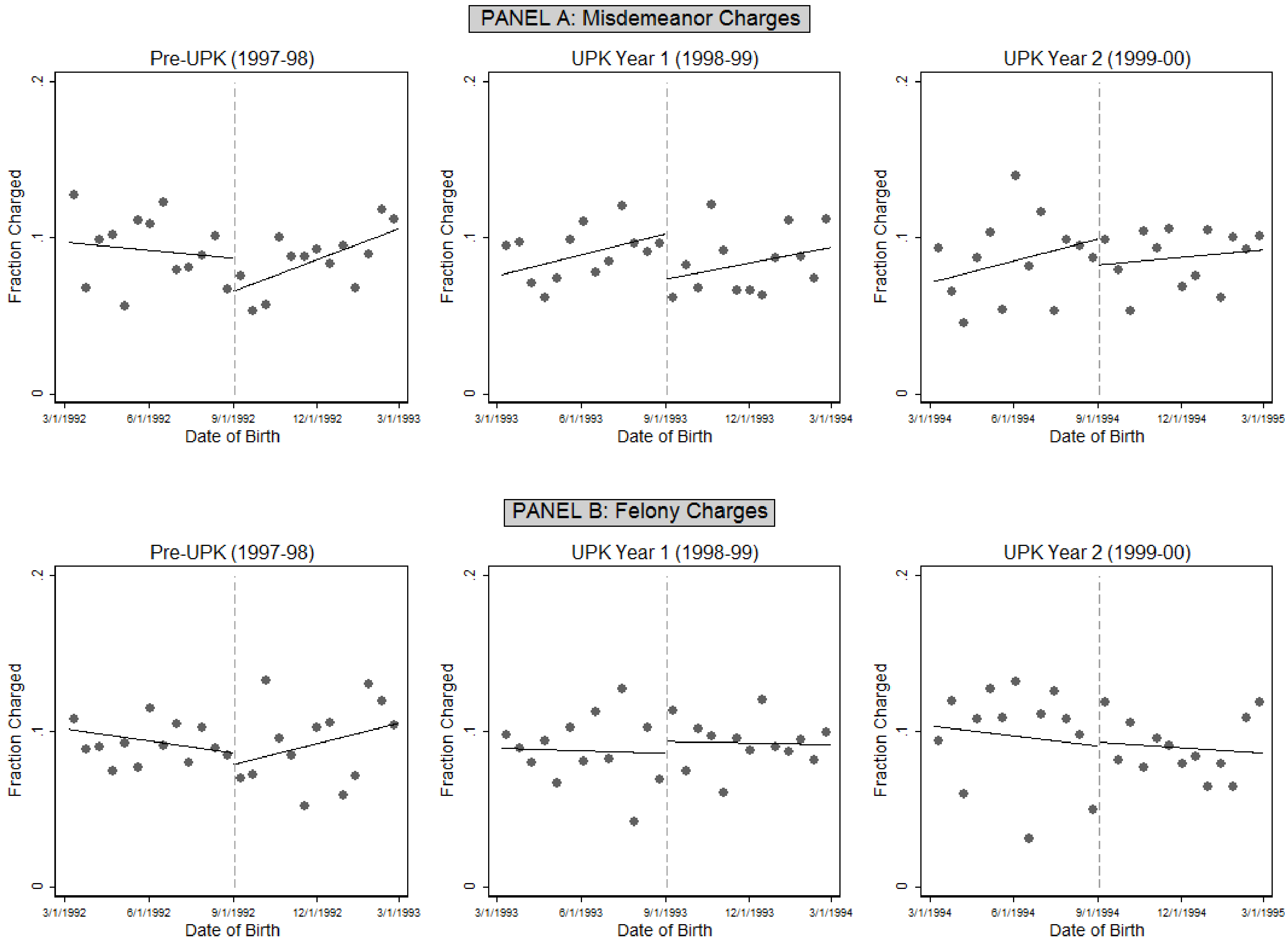


Figure A9: Likelihood of Criminal Charge at Age 18, by Birthdate (Whites)



Notes: Data from Oklahoma criminal court records and birth records. Each dot corresponds to the mean for a bin of 15 days. Likelihood of Criminal Charge at Age 18 = Number of individuals charged with a crime at age 18 in OK with given birthday / Number of individuals born on that day in OK.

Figure A10: Likelihood of Criminal Charge at Age 18, by Birthdate (Blacks)



Notes: Data from Oklahoma criminal court records and birth records. Each dot corresponds to the mean for a bin of 15 days. Likelihood of Criminal Charge at Age 18 = Number of individuals charged with a crime at age 18 in OK with given birthday / Number of individuals born on that day in OK.

Table 1 - Summary Statistics *(Birthdates 3/2/1993-3/1/1994)*

		Including Charges with Imputed Race		Excluding Charges with Imputed Race	
	Full Sample	White	Black	White	Black
	(1)	(2)	(3)	(4)	(5)
Total Births	46,217	36,134	4,904	36,134	4,904
Percent of Total Births		78.2%	10.6%	78.2%	10.6%
Charged at Age 18-19 (Unique Individuals):					
Misdemeanor Only					
Total	2,517	1,724	867	720	260
Percent of Births	5.4%	4.8%	17.7%	2.0%	5.3%
Felony Only					
Total	2,005	1,169	831	514	410
Percent of Births	4.3%	3.2%	16.9%	1.4%	8.4%
Misdemeanor or Felony					
Total	4,005	2,579	1,501	1,107	587
Percent of Births	8.7%	7.1%	30.6%	3.1%	12.0%
Charged at Age 18 (Unique Individuals)					
Misdemeanor Only					
Total	1,214	827	424	321	112
Percent of Births	2.6%	2.3%	8.6%	0.9%	2.3%
Felony Only					
Total	1,058	614	442	267	233
Percent of Births	2.3%	1.7%	9.0%	0.7%	4.8%
Misdemeanor or Felony					
Total	2,073	1,325	792	544	310
Percent of Births	4.5%	3.7%	16.2%	1.5%	6.3%

Mother's race is used to define race subsamples of births.
For charged individuals missing race, race is imputed using race name index thresholds of 0.9.

Table 2 - Treatment Contrasts at Kindergarten Birthdate Cutoffs in Various School Years

	1997-98		1998-99		1999-2000	
	Child A	**Child B**	**Child C**	**Child D**	**Child E**	**Child F**
Birthdate	*9/1/1992*	*9/2/1992*	*9/1/1993*	*9/2/1993*	*9/1/1994*	*9/2/1994*
Age Relative to Others in Grade	Youngest	Oldest	Youngest	Oldest	Youngest	Oldest
Grade at Age 4	-	-	-	-	PK	-
Grade at Age 5	K	-	K	PK	K	PK
Grade at Age 6	1	K	1	K	1	K
Criminal Charges Observed in Sample:						
Age 18	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Age 19	*yes*	*yes*	*yes*	*yes*	*no*	*no*
				→ ***OK Universal Pre-K***		

Table 3 - RD Estimates of Missing Kindergarten Birthdate Cutoff by School Year

OUTCOME: Likelihood of Criminal Charge at Age 18-19

	WHITE		BLACK	
	Felony	Misdemeanor	Felony	Misdemeanor
	(1)	(2)	(3)	(4)
1997-1998 School Year				
(Birthdates: 3/2/1991-3/1/1993)				
Missed Kindergarten	0.005	-0.006	-0.016	-0.010
	(0.00)	(0.00)	(0.02)	(0.02)
Obs	*365*	*365*	*365*	*365*
1998-1999 School Year *[FIRST YEAR OF UNIVERSAL PRE-K]*				
(Birthdates: 3/2/1993-3/1/1994)				
Missed Kindergarten	-0.001	0.003	-0.045 **	-0.068 ***
	(0.00)	(0.00)	(0.02)	(0.02)
Obs	*365*	*365*	*365*	*365*
Avg. Fraction Charged (by Birthdate)	*0.03*	*0.06*	*0.17*	*0.18*
Avg. Daily Births (Denominator)	*99.0*	*99.0*	*13.4*	*13.4*

Each entry represents the estimate of being born after the birthdate cutoff (Sept. 1) in the given year from a different RD regression.
Regression: y = a + **b***(z>=0) + c*z*(z>=0) +d*z*(z<0), where z is birthdate minus 9/1/YYYY and b is the coefficient of interest.
y = # of individuals charged with a crim at age 18 in OK with given birthday / # individuals born on that day in OK.
Robust standard errors in parentheses.

Table 4 - RD Estimates of Missing Kindergarten Birthdate Cutoff in UPK Year 1 (Various Bandwidths)
OUTCOME: Likelihood of Criminal Charge at Age 18-19

	WHITE		BLACK	
	Felony	Misdemeanor	Felony	Misdemeanor
	(1)	(2)	(3)	(4)
Bandwidth: +/- 6 Months				
(Birthdates: 3/2/1993-3/1/1994)				
Missed Kindergarten	-0.001	0.004	-0.045 **	-0.068 ***
	(0.00)	(0.00)	(0.02)	(0.02)
Obs	*365*	*365*	*365*	*365*
Bandwidth: +/- 5 Months				
(Birthdates: 4/1/1993-2/1/1994)				
Missed Kindergarten	-0.002	0.003	-0.054 **	-0.081 ***
	(0.00)	(0.00)	(0.02)	(0.03)
Obs	*307*	*307*	*307*	*307*
Bandwidth: +/- 4 Months				
(Birthdates: 5/1/1993-1/1/1994)				
Missed Kindergarten	-0.004	0.001	-0.067 ***	-0.081 ***
	(0.00)	(0.01)	(0.03)	(0.03)
Obs	*246*	*246*	*246*	*246*
Bandwidth: +/- 3 Months				
(Birthdates: 6/1/1993-12/1/1993)				
Missed Kindergarten	-0.005	-0.003	-0.047	-0.087 ***
	(0.00)	(0.01)	(0.03)	(0.03)
Obs	*184*	*184*	*184*	*184*
Bandwidth: +/- 2 Months				
(Birthdates: 7/1/1993-11/1/1993)				
Missed Kindergarten	-0.003	-0.011	-0.057 *	-0.156 ***
	(0.01)	(0.01)	(0.03)	(0.04)
Obs	*124*	*124*	*124*	*124*
Bandwidth: +/- 1 Months				
(Birthdates: 8/1/1993-10/1/1993)				
Missed Kindergarten	-0.002	0.001	-0.051	-0.109 *
	(0.01)	(0.01)	(0.05)	(0.06)
Obs	*62*	*62*	*62*	*62*

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year from a different RD regression.
Regression: y = a + **b***(z>=0) + c*z*(z>=0) +d*z*(z<0), where z is birthdate minus 9/1/1993 and b is the coefficient of interest.
y = # of individuals charged with a crime at age 18-19 in OK with given birthday / # individuals born on that day in OK.
Robust standard errors in parentheses.

Table 5 - Local Linear RD Estimates of Missing Kindergarten Birthdate Cutoff in UPK Year 1
OUTCOME: Likelihood of Criminal Charge at Age 18-19

	WHITE		BLACK	
	Felony	Misdemeanor	Felony	Misdemeanor
	(1)	(2)	(3)	(4)
Kernel Bandwidth (Days)				
4	-0.005	-0.001	0.167 **	-0.376 ***
	(0.01)	(0.01)	(0.08)	(0.05)
10	0.024 *	0.010	0.106	-0.164
	(0.01)	(0.01)	(0.06)	(0.10)
20	0.016	0.005	0.032	-0.117
	(0.01)	(0.01)	(0.05)	(0.08)
30	0.006	0.000	0.003	-0.128 **
	(0.01)	(0.01)	(0.05)	(0.06)
40	0.002	-0.001	-0.033	-0.110 **
	(0.01)	(0.01)	(0.05)	(0.05)
50	0.000	-0.003	-0.045	-0.109 **
	(0.01)	(0.01)	(0.04)	(0.05)
60	-0.001	-0.005	-0.043	-0.119 ***
	(0.01)	(0.01)	(0.04)	(0.04)
Optimal Bandwidth:	***4.19***	***4.38***	***5.24***	***5.47***
	-0.005	0.003	0.144 **	-0.354 ***
	(0.01)	(0.01)	(0.07)	(0.06)

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year from a different RD regression. Regression: y = a + **b***(z>=0) +f(z), where z is birthdate minus 9/1/1993 and b is the coefficient of interest.
Estimation uses a local linear (kernel regression) approach (Nichols, 2011).
y = # of individuals charged with a crime at age 18-19 in OK with given birthday / # individuals born on that day in OK.
Optimal bandwidth is calculated folowing Imbens and Kalyanaraman (2011). Standard errors in parentheses.

Table 6 - RD Estimates of Missing Kindergarten Birthdate Cutoff in UPK Year 1 vs. Other Years
Outcome Measure: Likelihood of Criminal Charge at Age 18 or Age 18-19

	WHITE		BLACK	
	Felony	Misdemeanor	Felony	Misdemeanor
	(1)	(2)	(3)	(4)
A) UPK Year 1 and Prior Year [Charges at Age 18-19]				
(Birthdates: 3/2/1992-3/1/1994)				
Missed Kindergarten x UPK Year 1	-0.006	0.009	-0.028	-0.057 *
	(0.01)	(0.01)	(0.03)	(0.03)
Avg. Daily Fraction Charged	*0.03*	*0.05*	*0.17*	*0.18*
Obs	*730*	*730*	*730*	*730*
B) UPK Years 1-2 and Prior Year [Charge at Age 18]				
(Birthdates: 3/2/1992-3/1/1995)				
Missed Kindergarten x UPK Year 1	0.001	0.005	0.010	-0.019
	(0.00)	(0.00)	(0.02)	(0.02)
Avg. Daily Fraction Charged	*0.02*	*0.02*	*0.09*	*0.09*
Obs	*1,095*	*1,095*	*1,095*	*1,095*
C) UPK Year 1 and Prior Year [Charge at Age 18]				
(Birthdates: 3/2/1992-3/1/1994)				
Missed Kindergarten x UPK Year 1	-0.001	0.004	0.017	-0.017
	(0.00)	(0.00)	(0.02)	(0.02)
Avg. Daily Fraction Charged	*0.02*	*0.02*	*0.09*	*0.09*
Obs	*730*	*730*	*730*	*730*
D) UPK Years 1-2 [Charge at Age 18]				
(Birthdates: 3/2/1993-3/1/1995)				
Missed Kindergarten x UPK Year 1	0.002	0.006	0.003	-0.022
	(0.00)	(0.00)	(0.02)	(0.02)
Avg. Daily Fraction Charged	*0.02*	*0.02*	*0.09*	*0.09*
Obs	*730*	*730*	*730*	*730*

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year (relative to other years) from a different RD regression.
Regression: y = a + **b1***(YEAR 1)*(z>=0) +b2*(z>=0) + c*(YEAR1)*z*(z>=0) + d*z*(z>=0)… (other interaction terms) ,
where z is birthdate minus 9/1/YYYY and b is the coefficient of interest.
y = # of individuals charged with a crime at age 18 in OK with given birthday / # individuals born on that day in OK.
Robust standard errors in parentheses.

Table A1 - White Sample Robustness Checks for RD Estimates in UPK Year 1 (Likelihood of Criminal Charge at Age 18-19)

	FELONY						MISDEMEANOR					
	Not Imputed	WNI ≥ 0.1	WNI ≥ 0.25	WNI ≥ 0.5	WNI ≥ 0.75	WNI ≥ 0.9	Not Imputed	WNI ≥ 0.1	WNI ≥ 0.25	WNI ≥ 0.5	WNI ≥ 0.75	WNI ≥ 0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bandwidth: +/- 6 Months	-0.002	-0.002	-0.002	-0.002	-0.001	-0.001	0.003	0.003	0.003	0.005	0.006	0.004
(Birthdates: 3/2/1993-3/1/1994)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Bandwidth: +/- 5 Months	-0.002	-0.004	-0.004	-0.004	-0.003	-0.002	0.004	0.004	0.003	0.005	0.006	0.003
(Birthdates: 4/1/1993-2/1/1994)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Bandwidth: +/- 4 Months	-0.003	-0.006	-0.006	-0.007	-0.006	-0.004	0.002	0.003	0.002	0.004	0.003	0.001
(Birthdates: 5/1/1993-1/1/1994)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bandwidth: +/- 3 Months	-0.005	-0.007	-0.007	-0.008	-0.006	-0.005	0.003	-0.002	-0.002	-0.001	-0.001	-0.003
(Birthdates: 6/1/1993-12/1/1993)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bandwidth: +/- 2 Months	-0.003	-0.003	-0.002	-0.003	-0.003	-0.003	0.003	-0.008	-0.009	-0.008	-0.009	-0.011
(Birthdates: 7/1/1993-11/1/1993)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bandwidth: +/- 1 Months	-0.005	0.002	0.002	0.000	-0.001	-0.002	0.008	0.005	0.004	0.004	0.004	0.001
(Birthdates: 8/1/1993-10/1/1993)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year from a different RD regression.

See text for construction of White Name Index (WNI).

Regression: y = a + **b***(z>=0) + c*z*(z>=0) +d*z*(z<0), where z is birthdate minus 9/1/1993 and b is the coefficient of interest.

y = # of individuals charged with a crime at age 18-19 in OK with given birthday / # individuals born on that day in OK.

Robust standard errors in parentheses.

Table A2 - Black Sample Robustness Checks for RD Estimates in UPK Year 1 (Likelihood of Criminal Charge at Age 18-19)

	FELONY						MISDEMEANOR					
	Not Imputed	BNI ≥ 0.1	BNI ≥ 0.25	BNI ≥ 0.5	BNI ≥ 0.75	BNI ≥ 0.9	Not Imputed	BNI ≥ 0.1	BNI ≥ 0.25	BNI ≥ 0.5	BNI ≥ 0.75	BNI ≥ 0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bandwidth: +/- 6 Months	-0.035 **	-0.059 **	-0.045 *	-0.042 *	-0.041 *	-0.045 **	-0.035 ***	-0.076 ***	-0.094 ***	-0.081 ***	-0.067 ***	-0.068 ***
(Birthdates: 3/2/1993-3/1/1994)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
Bandwidth: +/- 5 Months	-0.043 **	-0.075 ***	-0.056 **	-0.049 **	-0.051 **	-0.054 **	-0.042 ***	-0.093 ***	-0.110 ***	-0.101 ***	-0.083 ***	-0.081 ***
(Birthdates: 4/1/1993-2/1/1994)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Bandwidth: +/- 4 Months	-0.054 ***	-0.088 ***	-0.067 **	-0.063 **	-0.066 **	-0.067 ***	-0.042 ***	-0.086 ***	-0.103 ***	-0.095 ***	-0.080 ***	-0.081 ***
(Birthdates: 5/1/1993-1/1/1994)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Bandwidth: +/- 3 Months	-0.037 *	-0.061 *	-0.045	-0.042	-0.046	-0.047	-0.035 *	-0.084 **	-0.105 ***	-0.096 ***	-0.085 **	-0.087 ***
(Birthdates: 6/1/1993-12/1/1993)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Bandwidth: +/- 2 Months	-0.053 **	-0.057	-0.051	-0.049	-0.063 *	-0.057 *	-0.063 ***	-0.147 ***	-0.162 ***	-0.160 ***	-0.159 ***	-0.156 ***
(Birthdates: 7/1/1993-11/1/1993)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Bandwidth: +/- 1 Months	-0.077 **	-0.037	-0.033	-0.027	-0.051	-0.051	-0.081 ***	-0.084	-0.103 *	-0.100 *	-0.105 *	-0.109 *
(Birthdates: 8/1/1993-10/1/1993)	(0.04)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.03)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year from a different RD regression.

See text for construction of Black Name Index (BNI).

Regression: $y = a + \mathbf{b}*(z>=0) + c*z*(z>=0) + d*z*(z<0)$, where z is birthdate minus 9/1/1993 and b is the coefficient of interest.

y = # of individuals charged with a crime at age 18-19 in OK with given birthday / # individuals born on that day in OK.

Robust standard errors in parentheses.

Table A3 - White Sample Robustness Checks for RD Estimates in UPK Year 1 (Number of Individuals Charged at Age 18-19)

	FELONY						MISDEMEANOR					
	Not Imputed	WNI ≥ 0.1	WNI ≥ 0.25	WNI ≥ 0.5	WNI ≥ 0.75	WNI ≥ 0.9	Not Imputed	WNI ≥ 0.1	WNI ≥ 0.25	WNI ≥ 0.5	WNI ≥ 0.75	WNI ≥ 0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bandwidth: +/- 6 Months	-0.299	-0.325	-0.356	-0.373	-0.280	-0.188	0.316	0.330	0.301	0.471	0.634	0.348
(Birthdates: 3/2/1993-3/1/1994)	(0.25)	(0.41)	(0.41)	(0.40)	(0.39)	(0.36)	(0.30)	(0.52)	(0.51)	(0.51)	(0.51)	(0.50)
Bandwidth: +/- 5 Months	-0.304	-0.605	-0.637	-0.628	-0.538	-0.358	0.421	0.323	0.291	0.507	0.598	0.279
(Birthdates: 4/1/1993-2/1/1994)	(0.28)	(0.44)	(0.44)	(0.43)	(0.42)	(0.39)	(0.32)	(0.57)	(0.56)	(0.55)	(0.55)	(0.55)
Bandwidth: +/- 4 Months	-0.406	-0.786	-0.781	-0.800	-0.727	-0.516	0.297	0.347	0.303	0.508	0.492	0.168
(Birthdates: 5/1/1993-1/1/1994)	(0.31)	(0.50)	(0.50)	(0.50)	(0.49)	(0.45)	(0.35)	(0.61)	(0.61)	(0.60)	(0.61)	(0.61)
Bandwidth: +/- 3 Months	-0.511	-0.778	-0.787	-0.816	-0.672	-0.542	0.479	0.108	0.057	0.213	0.173	-0.063
(Birthdates: 6/1/1993-12/1/1993)	(0.36)	(0.56)	(0.56)	(0.55)	(0.54)	(0.50)	(0.40)	(0.69)	(0.68)	(0.68)	(0.69)	(0.70)
Bandwidth: +/- 2 Months	-0.420	-0.461	-0.411	-0.489	-0.443	-0.436	0.497	-0.730	-0.791	-0.692	-0.829	-1.018
(Birthdates: 7/1/1993-11/1/1993)	(0.43)	(0.65)	(0.65)	(0.65)	(0.65)	(0.59)	(0.48)	(0.83)	(0.83)	(0.82)	(0.85)	(0.87)
Bandwidth: +/- 1 Months	-0.654	-0.229	-0.229	-0.350	-0.371	-0.541	0.912	0.426	0.281	0.403	0.372	0.043
(Birthdates: 8/1/1993-10/1/1993)	(0.60)	(0.89)	(0.89)	(0.88)	(0.90)	(0.81)	(0.64)	(1.13)	(1.12)	(1.11)	(1.14)	(1.18)

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year from a different RD regression.

See text for construction of White Name Index (WNI).

Regression: y = a + **b***(z>=0) + c*z*(z>=0) +d*z*(z<0), where z is birthdate minus 9/1/1993 and b is the coefficient of interest.

y = # of individuals charged with a crime at age 18-19 in OK with given birthday.

Robust standard errors in parentheses.

Table A4 - Black Sample Robustness Checks for RD Estimates in UPK Year 1 (Number of Individuals Charged at Age 18-19)

	FELONY						MISDEMEANOR					
	Not Imputed	BNI ≥ 0.1	BNI ≥ 0.25	BNI ≥ 0.5	BNI ≥ 0.75	BNI ≥ 0.9	Not Imputed	BNI ≥ 0.1	BNI ≥ 0.25	BNI ≥ 0.5	BNI ≥ 0.75	BNI ≥ 0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bandwidth: +/- 6 Months	-0.441 **	-0.651 *	-0.444	-0.441	-0.434	-0.484 *	-0.544 ***	-1.024 ***	-1.288 ***	-1.158 ***	-0.971 ***	-0.982 ***
(Birthdates: 3/2/1993-3/1/1994)	(0.22)	(0.34)	(0.32)	(0.30)	(0.29)	(0.29)	(0.18)	(0.38)	(0.36)	(0.35)	(0.35)	(0.35)
Bandwidth: +/- 5 Months	-0.537 **	-0.854 **	-0.572 *	-0.521	-0.543 *	-0.586 *	-0.633 ***	-1.209 ***	-1.468 ***	-1.387 ***	-1.145 ***	-1.128 ***
(Birthdates: 4/1/1993-2/1/1994)	(0.23)	(0.37)	(0.34)	(0.32)	(0.31)	(0.31)	(0.19)	(0.41)	(0.40)	(0.38)	(0.38)	(0.38)
Bandwidth: +/- 4 Months	-0.733 ***	-1.136 ***	-0.817 **	-0.792 **	-0.818 **	-0.834 **	-0.649 ***	-1.210 ***	-1.466 ***	-1.387 ***	-1.171 ***	-1.177 ***
(Birthdates: 5/1/1993-1/1/1994)	(0.26)	(0.42)	(0.39)	(0.37)	(0.36)	(0.36)	(0.21)	(0.45)	(0.43)	(0.42)	(0.42)	(0.43)
Bandwidth: +/- 3 Months	-0.560 *	-0.919 *	-0.663	-0.639	-0.672 *	-0.677 *	-0.597 **	-1.383 ***	-1.697 ***	-1.590 ***	-1.420 ***	-1.439 ***
(Birthdates: 6/1/1993-12/1/1993)	(0.30)	(0.48)	(0.46)	(0.42)	(0.40)	(0.39)	(0.25)	(0.50)	(0.49)	(0.47)	(0.48)	(0.48)
Bandwidth: +/- 2 Months	-0.742 **	-0.800	-0.666	-0.672	-0.829 *	-0.750	-0.975 ***	-2.223 ***	-2.444 ***	-2.448 ***	-2.408 ***	-2.372 ***
(Birthdates: 7/1/1993-11/1/1993)	(0.36)	(0.54)	(0.51)	(0.48)	(0.46)	(0.46)	(0.29)	(0.62)	(0.60)	(0.59)	(0.59)	(0.59)
Bandwidth: +/- 1 Months	-1.071 **	-0.543	-0.489	-0.383	-0.663	-0.663	-1.225 ***	-1.350	-1.657 *	-1.619 *	-1.655 *	-1.706 *
(Birthdates: 8/1/1993-10/1/1993)	(0.52)	(0.82)	(0.79)	(0.72)	(0.70)	(0.70)	(0.43)	(0.92)	(0.90)	(0.87)	(0.89)	(0.90)

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year from a different RD regression.
See text for construction of Black Name Index (BNI).
Regression: y = a + **b***(z>=0) + c*z*(z>=0) +d*z*(z<0), where z is birthdate minus 9/1/1993 and b is the coefficient of interest.
y = # of individuals charged with a crime at age 18-19 in OK with given birthday.
Robust standard errors in parentheses.

Table A5 - RD Estimates of Missing Kindergarten Birthdate Cutoff by School Year

OUTCOME: Likelihood of Criminal Charge at Age 18

	WHITE		BLACK	
	Felony	Misdemeanor	Felony	Misdemeanor
	(1)	(2)	(3)	(4)
1997-1998 School Year				
(Birthdates: 3/2/1991-3/1/1993)				
Missed Kindergarten	0.001	-0.001	-0.008	-0.018
	(0.00)	(0.00)	(0.02)	(0.02)
Obs	*365*	*365*	*365*	*365*
1998-1999 School Year *[FIRST YEAR OF UNIVERSAL PRE-K]*				
(Birthdates: 3/2/1993-3/1/1994)				
Missed Kindergarten	0.001	0.003	0.009	-0.035 **
	(0.00)	(0.00)	(0.02)	(0.02)
Obs	*365*	*365*	*365*	*365*
1999-2000 School Year				
(Birthdate Window: 3/2/1994-3/1/1995)				
Missed Kindergarten	-0.002	-0.003	0.006	-0.013
	(0.00)	(0.00)	(0.02)	(0.02)
Obs	*365*	*365*	*365*	*365*
Avg. Fraction Charged (by Birthdate)	*0.02*	*0.02*	*0.09*	*0.09*
Avg. Daily Births (Denominator)	*99.0*	*99.0*	*13.4*	*13.4*

Each entry represents the estimate of being born after the birthdate cutoff (Sept. 1) in the given year from a different RD regression.
Regression: y = a + **b***(z>=0) + c*z*(z>=0) +d*z*(z<0), where z is birthdate minus 9/1/YYYY and b is the coefficient of interest.
y = # of individuals charged with a crim at age 18 in OK with given birthday / # individuals born on that day in OK.
Robust standard errors in parentheses.

Table A6 - White Sample Robustness Checks for RD Estimates in UPK Year 1 (Likelihood of Criminal Charge at Age 18)

	FELONY						MISDEMEANOR					
	Not Imputed	WNI ≥ 0.1	WNI ≥ 0.25	WNI ≥ 0.5	WNI ≥ 0.75	WNI ≥ 0.9	Not Imputed	WNI ≥ 0.1	WNI ≥ 0.25	WNI ≥ 0.5	WNI ≥ 0.75	WNI ≥ 0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bandwidth: +/- 6 Months	-0.001	0.000	0.000	-0.001	0.000	0.001	0.004 **	0.003	0.002	0.003	0.004	0.003
(Birthdates: 3/2/1993-3/1/1994)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Bandwidth: +/- 5 Months	-0.002	-0.002	-0.002	-0.003	-0.002	-0.001	0.005 **	0.003	0.003	0.004	0.004	0.004
(Birthdates: 4/1/1993-2/1/1994)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Bandwidth: +/- 4 Months	-0.003	-0.004	-0.004	-0.004	-0.004	-0.002	0.003	0.005	0.005	0.005	0.005	0.005
(Birthdates: 5/1/1993-1/1/1994)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Bandwidth: +/- 3 Months	-0.002	-0.002	-0.002	-0.003	-0.001	0.000	0.005 *	0.004	0.004	0.005	0.005	0.005
(Birthdates: 6/1/1993-12/1/1993)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Bandwidth: +/- 2 Months	0.000	0.004	0.004	0.004	0.005	0.004	0.005	-0.004	-0.004	-0.004	-0.004	-0.004
(Birthdates: 7/1/1993-11/1/1993)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bandwidth: +/- 1 Months	-0.001	0.009	0.009	0.009	0.009	0.007	0.003	-0.002	-0.002	-0.001	-0.002	-0.004
(Birthdates: 8/1/1993-10/1/1993)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year from a different RD regression.
See text for construction of White Name Index (WNI).
Regression: y = a + **b***(z>=0) + c*z*(z>=0) +d*z*(z<0), where z is birthdate minus 9/1/1993 and b is the coefficient of interest.
y = # of individuals charged with a crime at age 18-19 in OK with given birthday / # individuals born on that day in OK.
Robust standard errors in parentheses.

Table A7 - Black Sample Robustness Checks for RD Estimates in UPK Year 1 (Likelihood of Criminal Charge at Age 18)

	FELONY						MISDEMEANOR					
	Not Imputed	BNI ≥ 0.1	BNI ≥ 0.25	BNI ≥ 0.5	BNI ≥ 0.75	BNI ≥ 0.9	Not Imputed	BNI ≥ 0.1	BNI ≥ 0.25	BNI ≥ 0.5	BNI ≥ 0.75	BNI ≥ 0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bandwidth: +/- 6 Months	-0.003	-0.002	0.010	0.010	0.011	0.009	-0.016 *	-0.042 **	-0.046 ***	-0.041 **	-0.033 **	-0.035 **
(Birthdates: 3/2/1993-3/1/1994)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Bandwidth: +/- 5 Months	-0.004	-0.003	0.010	0.011	0.013	0.012	-0.022 **	-0.045 **	-0.048 **	-0.044 **	-0.035 *	-0.037 **
(Birthdates: 4/1/1993-2/1/1994)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Bandwidth: +/- 4 Months	-0.009	-0.002	0.014	0.014	0.014	0.014	-0.021 *	-0.026	-0.027	-0.027	-0.020	-0.023
(Birthdates: 5/1/1993-1/1/1994)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Bandwidth: +/- 3 Months	0.010	0.023	0.033	0.036	0.038 *	0.037 *	-0.012	-0.026	-0.029	-0.025	-0.020	-0.022
(Birthdates: 6/1/1993-12/1/1993)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Bandwidth: +/- 2 Months	0.010	0.043	0.043	0.044	0.038	0.042 *	-0.019	-0.057 *	-0.060 **	-0.057 **	-0.061 **	-0.060 **
(Birthdates: 7/1/1993-11/1/1993)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Bandwidth: +/- 1 Months	-0.004	0.067	0.068 *	0.075 **	0.067 *	0.067 *	-0.026	-0.009	-0.015	-0.020	-0.022	-0.022
(Birthdates: 8/1/1993-10/1/1993)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year from a different RD regression.

See text for construction of Black Name Index (BNI).

Regression: y = a + **b***(z>=0) + c*z*(z>=0) +d*z*(z<0), where z is birthdate minus 9/1/1993 and b is the coefficient of interest.

y = # of individuals charged with a crime at age 18-19 in OK with given birthday / # individuals born on that day in OK.

Robust standard errors in parentheses.

Table A8 - RD Estimates of Effect of Missing Kindergarten Birthdate Cutoff (UPK Year 1) on Number of Births

	Mother's Race	
	White	Black
	(1)	(2)
Bandwidth: +/- 6 Months		
(Birthdates: 3/2/1993-3/1/1994)		
Missed Kindergarten	-1.904	0.734
	(3.89)	(0.47)
Daily Mean	*99.0*	*13.4*
Obs	*365*	*365*
Bandwidth: +/- 5 Months		
(Birthdates: 4/1/1993-2/1/1994)		
Missed Kindergarten	-3.180	0.870 *
	(4.22)	(0.51)
Daily Mean	*99.2*	*13.3*
Obs	*307*	*307*
Bandwidth: +/- 4 Months		
(Birthdates: 5/1/1993-1/1/1994)		
Missed Kindergarten	-1.603	0.396
	(4.77)	(0.60)
Daily Mean	*99.7*	*13.4*
Obs	*246*	*246*
Bandwidth: +/- 3 Months		
(Birthdates: 6/1/1993-12/1/1993)		
Missed Kindergarten	1.822	-0.211
	(5.46)	(0.69)
Daily Mean	*100.8*	*13.4*
Obs	*184*	*184*
Bandwidth: +/- 2 Months		
(Birthdates: 7/1/1993-11/1/1993)		
Missed Kindergarten	0.508	0.107
	(6.68)	(0.88)
Daily Mean	*101.4*	*13.5*
Obs	*124*	*124*
Bandwidth: +/- 1 Months		
(Birthdates: 8/1/1993-10/1/1993)		
Missed Kindergarten	-3.027	-0.046
	(9.41)	(1.33)
Daily Mean	*102.6*	*13.7*
Obs	*62*	*62*

Each entry represents the estimate of being born just after the birthdate cutoff (Sept. 1) in the first implementation year from a different RD regression.
Regression: y = a + **b***(z>=0) + c*z*(z>=0) +d*z*(z<0), where z is birthdate minus 9/1/1993 and b is the coefficient of interest.
y = # of individuals charged with a crime at age 18-19 in OK with given birthday / # individuals born on that day in OK.
Robust standard errors in parentheses.