PRESCHOOLERS ENROLLED AND MOTHERS AT WORK? THE EFFECTS OF UNIVERSAL PRE-KINDERGARTEN

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Abstract

Three states recently introduced Universal Pre-Kindergarten programs offering free preschool to all age-eligible children; policy makers in many other states are promoting similar programs. Using restricted-access data from the Census, together with year and birthday based eligibility cutoffs, I employ a regression discontinuity framework to estimate the effects of Universal Pre-K availability on overall preschool enrollment and maternal labor supply. Universal Pre-K availability increases statewide preschool enrollment by at least 14 percent but has little effect on the labor supply of most women. The exception is women residing in rural areas, whose probability of being employed increases by 20 percent.

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I. Introduction

Publicly subsidized Pre-Kindergarten (Pre-K) programs have received considerable attention in recent years as an avenue for providing child care and promoting school readiness. In Virginia, Governor Timothy Kaine campaigned on the promise of free preschool for all four years olds, while in New York, former Governor Eliot Spitzer called for the "universal" program already in place to be made available statewide (Hakim 2007; Glod 2005).[1] As part of her recent presidential campaign, Senator Hillary Clinton unveiled a plan to provide up to $10 billion of federal funds per year to states that offer Universal Pre-K.[2] While economists have studied the effects of Pre-K interventions on the cognitive and non-cognitive development of participating children (Heckman and Masterov 2004; Gormley and Gayer 2005; Fitzpatrick 2008), the extent to which availability of Universal Pre-K increases preschool enrollment or affects the labor supply behavior of mothers is unknown.

Using birthday and age information coupled with eligibility cutoffs in a regression discontinuity framework, I examine how the availability of Universal Pre-K affects the enrollment in preschool of four year olds and the labor supply of their mothers. The two Universal Pre-K programs I study, in Georgia and Oklahoma, provide direct subsidies to child-care centers selected by parents for age-eligible children.[3] To enroll in either state's program, a child must turn four by September 1st of the school year in which they wish to enroll. The identification of intention-to-treat effects of Universal Pre-K comes from the exogenous difference in the eligibility of otherwise identical children born just before and after the cutoff.

[1] The state of New York introduced a plan for a Universal Pre-K program in 1997. When the program began, the plan was to roll out Pre-K over several years, starting in the poorest districts of the state. However, the program was never fully funded and therefore has never become available to all children in New York. Governor Spitzer has discussed providing the funding to make the program available statewide.
[2] http://www.hillaryclinton.com/news/release/view/?id=1743
[3] Florida introduced its Universal Pre-K program in 2005, after the collection of the data I use.

Restricted access Decennial Census 2000 data containing date of birth information make this identification strategy possible.

Universal Pre-K increases statewide preschool enrollment by 14 to 17 percent; these results are statistically significant and robust. Women with less than a Bachelor's Degree are even more likely than the general population to enroll their children in preschool in response to Universal Pre-K availability. For example, the program's availability increases the preschool enrollment of children whose mothers have completed some college (but are not college graduates) by about 18 percentage points, or 25 percent. These findings are important, as research shows that disadvantaged children gain the most from early exposure to high-quality preschool programs (Fitzpatrick 2008; Gormley and Gayer 2005). However, Universal Pre-K availability has no discernable effect on the preschool enrollment of children whose mothers have at least a Bachelor's Degree, perhaps because the preschool enrollment rate of the children of these mothers is over 75 percent without Universal Pre-K, which is anywhere from 10 to 30 percentage points higher than that of their less educated counterparts.

The results indicate no robust impact of Universal Pre-K availability on maternal labor supply. However, minor changes in labor supply are observed for certain groups of mothers. For example, mothers living in rural areas are 10 percentage points or 20 percent more likely to be employed because of Universal Pre-K availability. There are also indications that the labor supply of single mothers without other young children in the household may increase because of Universal Pre-K availability, but the results are not precisely estimated. This general lack of effect is consistent with recent findings that female labor supply elasticities are smaller now than they once were (Blau and Kahn 2007; Heim 2004).

Section II reviews the institutional details of Universal Pre-K and summarizes existing evidence about the relationship between child care and maternal labor supply. Section III provides a description of the empirical methods and the unique data used in the analysis. Section IV presents the results and several specification checks. The conclusion follows in Section V.

II. Universal Pre-K Programs & Previous Evidence

II.a. Institutional Details

In 1993, Georgia instituted a lottery to fund the HOPE scholarship and a pre-kindergarten program for four year olds.[4] Both were initially available only to low- and middle-income households, but the programs were expanded two years later to include all age-eligible state residents. In the 2004-2005 school year, approximately 55 percent of four year olds were enrolled in Georgia Pre-Kindergarten (GPK) at a total state cost of $276 million. In 1980, Oklahoma began a rationed pilot Pre-K program providing care to a small number of four year olds at no cost to parents. In 1990, the program expanded to include all Head Start eligible four year olds in the state. In 1998, Oklahoma further expanded its Early Childhood Program for Four Year Olds (ECPFYO) to include all age-eligible children regardless of income. By the 2004-2005 school year, enrollment in the program reached 68% of four year olds and cost $80 million.[5] Figure I details how enrollment in these Universal Pre-K programs grew. Both programs were well-implemented at the time of data collection, though Oklahoma's program continued to grow after 2000.[6]

[4] The HOPE scholarship has received much more attention from politicians and economists than its sister program, Georgia Pre-K. For examples, see Dynarski (forthcoming), Dynarski (2000) and Long (2004).
[5] http://nieer.org/yearbook/pdf/yearbook.pdf (March 13, 2007)
[6] Perhaps this is a reason the estimated effects of Universal Pre-K for Oklahoma are not as large as those for Georgia.

Both Georgia's GPK and Oklahoma's ECPFYO are voluntary, free, and available to all children who turn four by September 1, regardless of family income. In both states, a wide range of approved facilities, including public schools, Head Start centers, private child care centers, faith-based centers and other non-profit centers can provide Pre-K. Programs in both states run five days a week for the length of the school year, but Georgia mandates a 6.5 hour day while Oklahoma offers both half- (2.5 hours) and full- (6 hours) day options.[7] Teachers and classroom assistants must meet educational requirements higher than those for non-Universal Pre-K centers in both states. Both programs impose a minimum staff to child ratio of 1:10 and a maximum of 20 students per classroom. In Georgia, providers may choose to follow one of several approved curricula, while in Oklahoma there are curriculum "guidelines."[8]

The statutory incidence of both subsidies is on the firm side. The state of Georgia transfers lottery funds directly to centers. In 2004-2005, expenditures per child averaged $3,889, though actual reimbursement rates varied slightly based on location and teacher education. In Oklahoma, public school districts receive money from the general revenue allotted for the program on a first-come first-served basis. The districts may then provide the service themselves or contract out to child care centers that meet the state requirements. In the 2004-2005 school year, expenditures averaged $2,517 per child, though actual reimbursement rates depended on whether children attended full- or part- day sessions.[9] To place the size of the subsidies in context, average child care costs for full-day center care for four year olds in the U.S. ranges

[7] This difference in program lengths may be another reason the estimated effects of Universal Pre-K in Oklahoma are not as large as those in Georgia. Both options in Oklahoma are free to parents but reimbursement rates to providers depend on the length of care provided. Both states encourage centers to offer additional care (after set program hours and during the summer). However, neither pays the cost of this "supplemental" care.

[8] All of these regulations are stricter than those for non-Universal-Pre-K child care centers. For example, centers in Georgia not receiving state money for Universal Pre-K must have a staff to child ratio of at least 1:18, a maximum group size of 36, and there is no minimum educational requirement for teachers or assistants.

[9] http://nieer.org/yearbook/pdf/yearbook.pdf (March 13, 2007). Centers cannot receive more than a small registration fee from parents in either state.

from $3,900 to $10,200.[10] Although Universal Pre-K does not provide full workday care, by caring for children for at least part of the day it provides a substantial subsidy for child care to families with four year olds.

While Georgia and Oklahoma were the only states providing Universal Pre-K in 2000, many other states had targeted Pre-K programs, and the federal government provided funds for Head Start, another means-tested preschool subsidy for four year olds. Forty-three states funded targeted Pre-K programs in the 2001-2002 school-year.[11] Including children in Georgia and Oklahoma, almost 15 percent of four year olds nationwide were enrolled in state-funded Pre-K programs in 2001-2002. The vast majority of these programs resemble Universal Pre-K in that they offer care for 6 or fewer hours of care, five days a week, for the length of the school year. Head Start enrolled another 12 percent of four year olds.[12] It, too, provides care to participating children for 6 or fewer hours per day.[13] The counterfactual of this study includes these means-tested Pre-K programs in existence in 2000. Thus, the question answered here is: "What effects does Universal Pre-K have on preschool enrollment and maternal labor supply compared to existing early childhood education subsidy programs?"

II.b. Related Evidence: Subsidization and Preschool Enrollment

Reports from Georgia and Oklahoma indicate enrollment in their Universal Pre-K programs between 50 and 60 percent of all four year olds (Figure I) - program take-up is high. But increases in Universal Pre-K enrollment do not necessarily represent increases in preschool

[10] http://www.naccrra.org/randd/ (October 25, 2007)
[11] I report 2001-2002 school-year data because it is the year closest to 2000 for which data are available. http://nieer.org/yearbook2003/pdf/yearbook.pdf#page=6 (October 25, 2007)
[12] http://www.acf.hhs.gov/programs/hsb/about/fy2003.html (October 25, 2007)
[13] In fact, the recommended number of hours is four. http://eclkc.ohs.acf.hhs.gov/hslc/Program%20Design%20and%20Management/Head%20Start%20Requirements/Head%20Start%20Requirements/1306/1306.32%20%20Center-based%20program%20op-tion..htm

enrollment. It may be that the children attending the Universal Pre-K programs would have attended other preschool programs in the absence of Universal Pre-K, raising classic questions of crowd-out from publicly provided services (seen most frequently in discussions of health insurance, e.g. Cutler and Gruber [1996]). However, very little evidence exists about whether crowd-out is an issue in the child care industry. None exists about the crowd-out effects of Universal Pre-K in particular. Additionally, the universality of the program makes it unlikely that eligible families will respond as do those eligible for the widely studied similar but targeted programs, such as Head Start or Perry Preschool. Higher income families are more likely to be using preschool in the absence of a subsidy making it less probable that they will change their behavior in response to preschool subsidization.

Whether Universal Pre-K improves children's academic outcomes more than alternative modes of child care probably influences whether families enroll their children. Although there has been considerable amount of research showing that targeted preschool programs have the potential to improve child outcomes (Belfield, et al. 2006, Campbell, et al. 2002, Magnuson, Ruhm and Waldfogel 2007, Figlio and Roth forthcoming), relatively little evidence exists about the effects of Universal Pre-K. Gormley and Gayer (2005) and Gormley, Gayer, Phillips and Dawson (2005) analyze the effect of participation in the Oklahoma Pre-K program on test scores of students in Tulsa. In order to attempt to control for potential selection bias, they compare test scores of children just finishing Pre-K to those just entering it. The underlying assumption is that both groups are comparable on unobservable characteristics since they have all chosen to participate in the Universal Pre-K program. The researchers find that Oklahoma Pre-K participation increases test scores by 0.24 to 0.39 standard deviations, depending on the test subject. In a differences-in-differences analysis of the intention-to-treat effects of Universal Pre-

K on achievement scores of fourth graders, Fitzpatrick (2008) finds that the availability of Georgia Pre-K increases the math and reading scores of school lunch eligible children (0.07 and 0.03 percent of a standard deviation, respectively).

II.c. Related Evidence: Child Care Subsidies and Maternal Labor Supply

Female labor force participation has changed dramatically in recent decades, fostering interest in the role of children in female decisions about work. As shown in previous work (Gelbach 2002), theoretical predications from basic economic models about the effects of child care subsidization are ambiguous. In the traditional two good model used to describe mother's child care and labor supply choices (Gelbach 2002, Cascio forthcoming) the mother can choose between working (and purchasing care) or leisure (which implicitly includes taking care of the child herself). In this two good framework, a Universal Pre-K subsidy provides a full price subsidy for child care on the margin for any woman working less than the length of the Universal Pre-K program. This price subsidy provides incentive for these mothers to enter work or increase the number of hours they work. For women who work more than the length of the program day in the absence of Universal Pre-K, the program provides an income subsidy. This income subsidy exerts downward pressure on the amount of time in the workplace for these women.

Within the last fifteen years, investigators have used both demonstration programs (such as the New Chance program) and widespread targeted subsidies to examine the relationship between child care subsidization and maternal labor supply. Researchers consistently find evidence that subsidization of child care increases maternal labor supply (Bos, Huston, Granger,

Duncan, Brock and McCloyd 1999; Granger and Cryton 1999; Blau and Tekin 2003).[14] However, more recent analyses of the elasticity of female labor supply show women are no longer very responsive to wage changes (Blau and Kahn 2007; Heim 2004). This decreased responsiveness to wages might mean child care subsidies will have less impact on maternal labor supply today than in the past. What is more, the higher rates of female labor force participation may mean the subset of mothers for whom the subsidy has the potential of both price and income effects is now smaller than it once was.

The presence of variants on "universal" preschool subsidies in other countries and for children of different ages provides further opportunity for measurement of the effects of child care subsidization on labor supply. Baker, Gruber and Milligan. (2005) study a program providing child care for an out-of-pocket price of $5 per day (of any length) to all children under five years old in Québec, Canada. The researchers use a differences-in-differences approach, comparing the labor supply of mothers across Canadian provinces before and after the program began. The authors find a statistically significant and sizeable increase in employment of married mothers of 7.7 percentage points. Schlosser (2005) studies the introduction of free compulsory public preschool in Israel for children ages three and four. She uses variation in the timing of program introduction across localities to identify the effects of the program on maternal labor supply. She also finds effects of about 7 percentage points. My research differs from these studies in both the type of subsidy considered and the population served. Moreover, the identification strategies could be problematic if shocks specific to the areas during the

[14] The analyses of the widespread subsidies are biased if the measures used to control for selection (e.g. waiting lists, instruments) are invalid. Some of these experiments involve random assignment and therefore typical selection bias problems do not contaminate estimates of treatment effects. However, because these studies are geographically and socio-economically concentrated and have small sample sizes, the results may not generalize to larger and/or more diverse groups. In addition, many of these programs included packages of services and treatments along with the child care subsidy. This makes it difficult to disentangle the effects of the subsidy alone.

periods the researchers study are related to the policy change. Here, the use of age cutoffs as an additional source of identification alleviates this potential problem because any shock in Georgia or Oklahoma to mothers of four year olds likely has similar effects on those born both before and after September 1st.

In work closely related to this study, Gelbach (2002) uses quarter of birth as an instrument for enrollment in kindergarten in 1980. He then estimates the impact of the use of this large implicit child care subsidy on the labor supply of mothers in the U.S. He finds evidence of a 6-24 percent increase in labor supply measures.[15] In a slightly different approach, Cascio (forthcoming) uses the timing of kindergarten introduction (which largely occurred in the 1960s and 1970s) as an instrument for kindergarten participation in the estimation of the effects of kindergarten enrollment on maternal labor supply.[16] She shows evidence of an increase in maternal labor supply of single mothers without other young children due to the increased funding of kindergarten, but no effects for other groups of women.

Fitzpatrick (2008b) examines how the estimates of Gelbach (2002) are updated when more recent and detailed data are available. She uses restricted access Decennial Census data on families of five, four and three year olds. Using the same specification as Gelbach (2002) on the samples of single and married mothers with and without other young children in a replication exercise, it appears kindergarten enrollment in 2000 does not increase the labor supply of single mothers but does increase the labor supply of married mothers. The availability of exact date of

[15] If it is the case that Universal Pre-K affects maternal labor supply, it might also decrease the dependency of some women on government assistance. Gelbach (2002) also examines the effects of kindergarten enrollment on the receipt of public assistance of mothers in 1980. Although he finds that kindergarten reduced public assistance receipt by 10 percent, the increased participation of mothers in the workforce in recent decades, coupled with more stringent welfare laws enacted in the 1996 Personal Responsibility and Work Opportunity Reconciliation Act, might make the mothers receiving public assistance income in 1999 quite different from those doing so in 1979.

[16] In another related paper, Cascio (2005) uses variation in the funding of kindergarten programs as an instrument to control for selection bias in a study of the effects of increased kindergarten access on children's longer-term academic outcomes. She finds that the programs decreased grade retention between 20 to 40 percent but had little effect on high school graduation.

birth information in the restricted-access data allows for incorporation of more precise controls for subsidy eligibility and the creation of more comparable 'treatment' and 'control' groups of women whose children were born in a narrow window around the cutoff. These improvements in methodology change the estimated impact of kindergarten enrollment on maternal labor supply. The results are also quite sensitive to the assumption that a child's age is unrelated to his/her mother's labor supply. The regression discontinuity estimates show only the labor supply of single mothers without younger children in the home increases due to kindergarten enrollment.

Given the differences in the results across these studies, whether a Universal Pre-K program, such as the ones studied here, affects maternal labor supply remains to be seen. There are very important distinctions between this study and the previous work. First, this study focuses on the intent-to-treat effects of Universal Pre-K programs, rather than the effects of child care use more broadly.[17] Second, the years the other authors study (except Fitzpatrick 2008b) precede the new evidence that women in the U.S. are not responsive to wages. Also, societal convention may make it more likely that a mother enroll her children in kindergarten than in preschool.[18] Finally, as with Fitzpatrick (2008b), the precise birthday information in the unique data set I use here allows for comparison of groups relatively close to the eligibility cutoff in an effort to create a counterfactual control group who very closely mach the treatment group, a distinct improvement over methodologies relying on quarter of birth and therefore including children born throughout the year.

[17] Alternatively, one could think about estimating the local average treatment effect of Universal Pre-K on maternal labor supply. Unfortunately, because there are only two states, the treated groups are too small to avoid a weak instruments problem.

[18] To be clear, Gelbach (2002) examines the effect of preschool enrollment on maternal labor supply, but because of differences in the preschool program studied and the counterfactual early childhood education landscape, as well as dramatic changes in maternal labor supply patterns, the results of this study have the potential to be quite different.

III. Methods

III.a. The Regression Discontinuity Framework

$Y_i(1)$ and $Y_i(0)$ represent the outcome if an individual, *i*, is eligible or ineligible for Universal Pre-K, respectively. In cross-sectional data, it is impossible to observe individuals at points in time when they both did and did not receive treatment, so researchers typically examine differences between average outcomes for groups of individuals who did and did not receive treatment. Defining the treatment individuals receive as W_i, this difference is $E[Y_i \mid W_i = 1] - E[Y_i \mid W_i = 0]$. If the assignment to treatment is random, this difference represents the causal effect of treatment on the outcome. In regression discontinuity analyses, the value of some observable characteristic (D_i) determines treatment status, such that $W_i = 1\{D_i \geq d\}$.

Focusing on within-state variation in Universal Pre-K eligibility, children in Georgia and Oklahoma born on or before September 1, 1995 differ from those born on or after September 2, 1995 in that only the former are eligible for Universal Pre-K in the 1999-2000 school-year. Hahn, Todd and van der Klaauw (2001) term this a *sharp* regression discontinuity design: a child's date of birth completely determines the treatment, *eligibility* for Universal Pre-K. The effect of Universal Pre-K eligibility on an outcome, such as enrollment in preschool, can therefore be estimated by

$$\delta = \lim_{D_i \downarrow d} E[Y_i \mid D_i = d] - \lim_{D_i \uparrow d} E[Y_i \mid D_i = d].$$

Y_i is the dependent variable of interest for mother *i*, D_i represents the day on which mother *i*'s child turns four and *d* represents September 1st. The underlying assumption is that the relationship between birthdate and preschool enrollment would be smooth through the cutoff

(September 1st) were it not for the Universal Pre-K program. This identification strategy assumes that mothers are not able to manipulate their children's eligibility, much as in a randomized controlled trial in which participants are not able to change their assignment to treatment group. Also similar to a randomized controlled trial, there should be no differences between the demographic characteristics of mothers whose children are in either "treatment" group, i.e. between those who are eligible and those who are not.

The model for the estimation of the treatment effect is

$$Y_i = f(Days_i) + \theta cutoff_i + \upsilon_i .$$

Here, *Days*, equals the distance in days between a child's date of birth and the cutoff date for kindergarten in his/her state of residence. Eligibility is given by the discrete variable *cutoff*. The regression discontinuity literature uses two types of estimators to estimate Δ: the local polynomial and the flexible parametric model. In what follows I choose to use the flexible parametric model. One reason is general familiarity with this type of estimation. Another is the discrete support of birthdays which arguably makes it difficult to get arbitrarily close to the cutoff date (without time of birth).[19] I use a cubic and interact it with the cutoff term. Continuing to focus on within-state variation in eligibility, the equation becomes $Y_i = \alpha + \beta X_i + \sum_{j=0}^{3} \pi_j Days_i^j + \theta cutoff_i + \sum_{j=1}^{3} \lambda_j Days_i^j cutoff_i + \varepsilon_i$. The inclusion of demographic characteristics in X_i is not necessary given the identification strategy, but allows for variance reduction. It is possible that the use of this parametric functional form introduces specification error resulting a common variance component to the error term for all children with the same age

[19] Because this point is debatable, I also have done the analysis using local polynomial methods. The results are qualitatively the same.

relative to the cutoff date. I employ the methods outlined in Lee and Card (2008) to adjust the standard errors for inference.

Although only two states have Universal Pre-K, every state in the U.S. mandates that some type of kindergarten be made available for their residents; almost all states set restrictions on the age of children enrolled.[20] This eligibility restriction for kindergarten in other states further helps identify the effects of Universal Pre-K. Because the eligibility dates in Georgia and Oklahoma for kindergarten and Universal Pre-K are the same, without the use of other states it would not be possible to distinguish between enrollment effects of Universal Pre-K and enrollment effects resulting from a child's eligibility for kindergarten in the following year. Consider the extreme case where, regardless of Universal Pre-K availability, a family would have decided to send their child to preschool in the year before he or she will be age-eligible for kindergarten. Without controlling for the kindergarten eligibility that also comes with the September 1st cutoff, I would inappropriately attribute this child's preschool enrollment to Universal Pre-K. Incorporating states without Universal Pre-K, the estimation equation is the following:

$$Y_i = \alpha + \beta X_i + \gamma State_i + \sum_{j=0}^{4} \pi_j Days_i^j + \theta cutoff_i + \sum_{j=1}^{3} \lambda_j Days_i^j cutoff + \delta_1 GA_i \times cutoff_i + \delta_2 OK_i \times cutoff_i + \varepsilon_i \quad (1)$$

$State_i$ represents state fixed effects that control for fixed differences across states in outcomes. The variable $cutoff_i$ has a value of one if the mother's child was of age before the cutoff date in his or her state (in time for Universal Pre-K in the fall of 1999 or kindergarten in the fall of

[20] It should be noted that although state legislatures have mandated that school districts offer kindergarten, most states do not require kindergarten attendance. A handful of states – Colorado, Illinois, Massachusetts, New Hampshire, New Jersey, Pennsylvania, and Washington – allowed local municipalities to set the minimum entrance age for their school districts at the time of the 2000 Census. These states are not included in this analysis.

2000). The interaction term $GA \times cutoff_i$ ($OK \times cutoff_i$) will have a value of one for mothers whose children in Georgia (Oklahoma) are old enough to participate in Universal Pre-K. The effect of Universal Pre-K on the dependent variable is then measured by δ_1 and δ_2.[21] Identification of program effects comes from variation in behavior within states, for different ages of children and by different cutoff dates, specifically among those who are induced to change their behavior because of their treatment status. This *sharp* regression discontinuity framework therefore identifies the local average intention-to-treat effect of Universal Pre-K.[22]

III.b. Restricted Access 2000 Decennial Census Data

To conduct this analysis, I use the Census Decennial Long Form Restricted Access Data. The data generally comprise a one-in-six sample of the population of the United States surveyed in April 2000 and include demographic, labor force participation and educational enrollment information about survey respondents and other members of their households. The information provides a set of variables, *X*, about the mothers to use as controls in the analysis. The set consists of age and education, gender of the child, race, a set of dummy variables for whether the family lives in an urban area, rural area or urban fringe, state of residence, and the number and ages of other members of the household.

The primary outcomes of interest are enrollment of the child in school as of February 2000, mother's employment in the week prior to the survey, and mother's employment, weeks of

[21] There are two separate interaction terms because, as detailed earlier in the paper, the programs in Georgia and Oklahoma differ on important parameters that might cause maternal responses to their introduction to differ. It is also important to note that because non-Universal Pre-K states have a range of kindergarten cutoff dates (from June 1st to January 1st, including non-program states with September 1st cutoff dates), the estimates of the effects of Universal Pre-K do not include information about the importance of any particular date.

[22] In general, a concern is that residents of the same state share unobservable characteristics that would cause the error terms in (1) to be correlated across states. One way to correct for this arbitrary correlation is to cluster the standard errors at the state level. The results when doing so had slightly smaller standard errors than when the error terms were not clustered. I therefore present the more conservative standard errors in what follows.

work, usual hours, wages, and family's public assistance receipt in 1999.[23] When the dependent variable is binary, e.g. preschool enrollment, I use probit estimation and report marginal effects.[24] In 2000, the Census Bureau asked for the date of birth of respondents and other household members; this is crucial information for identification of program effects.[25] Combined with data collected from the states on kindergarten cutoffs in 1999, as shown in Table 1, this information identifies program effects as described above.[26]

In order to create the sample for the analyses, I limit the entire sample to the subset of mothers who live with their own children who were born in the one year period between March 1, 1995 and February 28, 1996, so that there are six months worth of eligible children and six months worth of ineligible children. Of this sample, I dropped the mothers with multiple children born on the same day, more than four other adults in the same household and more than two of their own or step-children over 18 years of age because the parameters they face are likely very different than those of other mothers (though the results are not sensitive to these restrictions). Together these groups account for less than 3 percent of the sample of mothers of four year olds. In addition, I drop observations for which data were missing. Most of the results in the following sections use a sub-sample of these four year olds in a narrower range of the eligibility cutoff. Using a width of 30 days on either side of the cutoff creates groups eligible

[23] The enrollment question asks whether the child was enrolled in school as of February 2000. If a respondent answers yes, he/she is then asked what level of school the child is attending, where one of the options is preschool or nursery school and another option is kindergarten. I include children who are attending either, but the results hardly change if I limit the definition of the outcome to include only those enrolled in preschool. Also, the labor supply questions are intended to refer to all of 1999, while the program would have only affected the mothers of four year olds in the last four or five months of 1999. This means that estimates of program effects on the labor supply decisions may underestimate the impact of full participation in Pre-K.

[24] Reported here are the marginal effects at the mean for continuous variables and for a change of zero to one in dummy variables. Average marginal effects are qualitatively similar.

[25] Unfortunately in 1990, the Decennial Long Form Census did not ask respondents about date of birth, but just age, making a before-after comparison of the effects of the programs with this data impossible.

[26] Data on kindergarten cutoffs were collected from a survey conducted by the Indiana Department of Education, found at http://www.doe.state.in.us/legwatch/2000/a_kinder_issues.html (Accessed October 2, 2006). In general, the cutoffs reported there conform to those for the same period from other sources, such as the Education Commission of the States.

and ineligible for the program that are quite comparable.[27] The weighted number of observations in this 'two-month' sample is 430,681, which corresponds to about 65,000 observations.

Table 2 presents the average characteristics for these mothers by state of residence. These characteristics show support for the assumption underlying the regression discontinuity framework. Mothers of children born within 30 days of a state's cutoff are essentially the same on observable characteristics – they are, on average, the same age and race, are equally likely to be married, and have the same amount of education. This is true for states with and without Universal Pre-K. Similar tabulations of observable characteristics by eligibility status also show support for the exogeneity of treatment within the subgroups of mothers along which analyses are done separately later in the paper.

I also confirm the validity of the assumption underlying the identification strategy by examining the continuity of mothers' demographic characteristics on either side of the cutoff for enrollment. Figures 2.A.-2.E. show the percent of mothers who are white, the percent who are married, the average age, the percent who have additional children younger than the four year old and the percent who have any other children. In each panel there are 3 lines, representing Georgia, Oklahoma and states without Universal Pre-K. The horizontal axis measures the age (in days) of four year olds relative to the cutoff dates in their states of residence. Children to the left of zero are born after the age cutoff date in their state and those to the right are born before the age cutoff date. Due to the confidential nature of the data, the graphs are smoothed splines of the relationships between the child's age relative to the cutoff and the other variables.[28] The

[27] The 30 day width is on the order of the optimal bandwidth using the Fan and Gibels (1992) method detailed in Fitzpatrick (2008b).

[28] To preserve the nature of the original data, the splines are minimally smoothed. An examination of the actual data in bins by the author within the Research Data Center produced very similar results.

graphs confirm that there are no differences in the exogenous characteristics of the mothers of children born on either side of the cutoff.

If mothers manipulate the eligibility of their children by altering their actual or reported birthdays, we might see an increase (or decrease) in the number of children born just before the cutoff date (McCrary 2007). Figure 3 presents the number of mothers whose children are born on any given day (relative to the cutoff date in their state of residence). These pictures also show no differences on either side of the cutoff. Figures 2 and 3 provide additional evidence that there are no differences (other than eligibility for Universal Pre-K) between women with four year olds born in a narrow range on either side of the enrollment cutoff date in their state. Similar depictions of observable characteristics by a child's age relative to the eligibility cut point also show support for the exogeneity of treatment within the subgroups of mothers along which analyses are done separately later in the paper.

IV. Results

IV.a. A Visual Look at the Effects of Universal Pre-K

An illustration of the relationship between the age of the child and enrollment in preschool shows the basic identification strategy. Figure 4.A. plots a smoothed spline of the relationship between the age of children and their rates of enrollment in preschool for groups in three locations: Georgia, Oklahoma and states without Universal Pre-K. Again, the horizontal axis represents the distance in days between children's birthdays and the cutoff date in their states. Preschool enrollment rates for children born in time for Universal Pre-K in the fall of 1999 or kindergarten in the fall of 2000 are to the right of the cutoff day and for those born after the cutoff date are to the left.

The diagram shows that, on average, being born before the cutoff is positively related to enrollment in preschool. For states without Universal Pre-K, the increase in enrollment rates for those born before their cutoff is approximately 8 percentage points. This can be construed as an increase in parents' willingness to send children to preschool at the age of four because they will be old enough to attend kindergarten the following year.[29] For children born in Universal Pre-K states the increase is even larger, approximately 19 percentage points for Georgia and 15 percentage points for Oklahoma. This effect can be construed as the combination of being able to enroll your child in Universal Pre-K in the fall of 1999 and kindergarten the following year. The difference between the two, 11 percentage points in Georgia and 7 percentage points for Oklahoma, represents the difference-in-differences estimate of the effect of Universal Pre-K (for those who are affected by the age restriction).

A similar picture of the relationship between the age of a child (relative to the cutoff date in his or her state) and maternal employment in the week prior to the survey is in Figure IV.B. No discernable relationship exists between a child being born before or after the cutoff date and a mother's probability of being employed. Though not presented here, other measures of maternal labor supply similarly show no relationship with the age cutoff.

IV.b. Estimation Results for Preschool Enrollment

Regression analyses confirm the visual relationship between Universal Pre-K availability and preschool enrollment. I find an average effect when I look at enrollment for all four year olds in the state and within most sub-groups of women expected to be differentially affected by Universal Pre-K. The first and second rows of Table 3 present the estimates of the effect of

[29] This also captures any behaviors of preschool centers in other states showing preference to children able to attend kindergarten the following year.

Universal Pre-K in Georgia and Oklahoma, respectively, by area of residence. Universal Pre-K increases preschool enrollment of four year olds in Georgia by 10.4 percentage points and in Oklahoma by 7.1 percentage points. Given the baseline enrollment of non-eligible children in each state, the estimates translate to an increase in preschool enrollment of 17 and 14 percent, respectively.

Turning to the demographic characteristics, white mothers, married mothers and mothers with other household members and children are all less likely than their counterparts (non-white, single or those with no other household members or children) to enroll their children in preschool. Older mothers and those with more education are more likely than younger or less educated mothers to enroll their four year olds in preschool.

As a reminder, *cutoff* is a dummy variable that takes a value of one if the child was born before the cutoff date for kindergarten in the fall of 2000 (and Universal Pre-K in the fall of 1999). Being of age for kindergarten makes it 8 percentage points more likely that a four year old is enrolled in preschool in the spring of 2000, and the estimate is statistically significant at the one percent level (as are most of the other estimates). This 13 percent increase represents the additional willingness of mothers to enroll their children in preschool if the child will be eligible for kindergarten in the following year. Accounting for this increased willingness through the use of other states as a control group is vital for precisely estimating the effects of Universal Pre-K.

IV.b.i. A Discussion of Crowd Out

Of particular interest in this setting, as with government provision of any good, is whether the government program involves crowd-out. Crowd-out can be conceptualized in terms of the supply-side: are government businesses taking the place of or crowding-out private

enterprise? Crowd-out also can be defined in terms of the demand side: is the government subsidy merely providing a good or service that consumers would have purchased even in the absence of government intervention? Although it will not be possible to distinguish between the two types of crowd-out, with the Decennial Census data and information from the state administrative agencies I can perform two back-of-the-envelope calculations to shed some light on how much crowd-out is occurring with Universal Pre-K in Georgia and Oklahoma.

The first calculation involves looking at the effects of the program on public and private preschool enrollment separately. The 2000 Decennial Census questionnaire differentiates between enrollment in a public school versus a private school. Examining results using a dependent variable for each of these categories separately and comparing them to the effect on overall preschool enrollment gives a sense of how much crowd-out is accompanying the net increases in public preschool enrollment seen with Universal Pre-K. Columns 1 to 3 of Table 4 show the estimated statewide effects of Universal Pre-K when the dependent variable is any school enrollment, public school enrollment and private school enrollment, respectively.

The coefficients across the first row of the table suggest that Universal Pre-K in Georgia actually increased enrollment in public preschools by 16 percentage points. The difference between this increase in public preschool enrollment and the 10 percentage point increase in all preschool enrollment discussed earlier is a 6 percentage point decrease in private preschool enrollment (column 3). In Oklahoma, the 10 percentage point increase in public preschool enrollment is partially fueled by a 3 percentage point decrease in private preschool enrollment. Interestingly, the reported estimates of the coefficient on the cutoff variable (third row) imply this type of crowd-out does not occur with kindergarten eligibility. Both public and private

preschool enrollment increase when children become age-eligible for kindergarten in the following year.

The coefficient estimates reported in Table 3 suggest that Universal Pre-K programs induce some families to enroll their children in public rather than private preschools. There is an important caveat to this discussion, however. The results in Table 4 rely on families' reports of the type of preschool their children attend. Because of the complex nature of the early childhood education industry and the government's role in it, it is not obvious that parents are answering the question the way economists might hope or expect. For example, what type of preschool enrollment do parents report when their child is enrolled in preschool in a private center that is paid for by the state? Conversely, what type of enrollment do parents report when their child is enrolled in a public school Pre-K class for which the parents foot the bill? This issue is particularly salient in Georgia where the Universal Pre-K program is run largely through the network of private providers. However, the results presented in Table 4 are at least suggestive that some crowd-out occurs as families switch from private to public preschools.

The second calculation involves using administrative data on enrollment in the Universal Pre-K programs themselves. Reports put enrollment in GPK at 53 percent of the state's four year olds; in Oklahoma, 46 percent of four year olds were enrolled in the states' Universal Pre-K program (Figure 1). Using the estimated increases in net preschool enrollment in the two states as measures of *new* preschool enrollment due to Universal Pre-K, a back-of-the-envelope calculation tells that, in 2000, 81 percent and 85 percent of enrollment in Universal Pre-K in Georgia and Oklahoma, respectively, was enrollment in preschool that would have taken place in

the programs' absence.[30] The majority of spending on Universal Pre-K pays for preschool enrollment of children who would attend preschool regardless of the programs' existence.

IV.b.ii. Preschool Enrollment Differences by Geography

The cost of child care depends in part on one's location. For example, on average, the closest child care center in rural areas is farther from family homes than it is in urban areas, likely raising the fixed transportation costs involved with child care use in rural areas. The second through fourth columns of Table 3 present the estimation results when the sample is restricted to rural, urban cluster or urbanized areas, respectively.[31] These classifications are based on a block group's population density as well as its proximity to other block groups of high population density.[32] Functionally, urban cluster represents large separate towns rather than the suburbs of big cities.

This distinction is important because of the differences in the estimates across these location types. Universal Pre-K has the largest effect in the less densely populated areas. The program increases preschool enrollment in rural Georgia by 11.9 percentage points and in rural Oklahoma by 9.7 percentage points, or 22 and 25 percent, respectively. Both estimates are statistically significant at the one percent level. In urban clusters, the estimated effects are 14.4 and 10.5 percentage point increases in enrollment for Georgia and Oklahoma, respectively, though only the estimate for Georgia is shown to be statistically significant. It translates into a

[30] These numbers are calculated by subtracting the new enrollment in preschool from the total enrollment in the programs' themselves and dividing by the total enrollment in the programs. For example, in Georgia: (53-10)/53=0.81.

[31] An alternative way to separate the sample for the following analyses is to use the whole sample and include interaction terms between eligibility and residential area (or marital status or educational attainment or the presence of younger children). Doing so, the results are not qualitatively different. The results from the pooled sample with interaction terms were used in determining whether differences in estimates of the effects of Universal Pre-K for mothers with different characteristics were statistically significant from each other.

[32] For definitions, see http://www.census.gov/geo/www/ua/ua_2k.html.

24 percent increase in preschool enrollment for children living in urban clusters. In urban areas in Georgia, Universal Pre-K is estimated to have increased preschool enrollment by 9.2 percentage points or 13 percent. In urban areas in Oklahoma, the estimate is essentially zero. The increases in urban areas are statistically smaller than the program's effects in rural areas or urban clusters. Such differences across location types suggest a supply side response to the Universal Pre-K program. Existence of "thin markets" for preschool is one potential explanation. There may not have been enough demand by families with four year olds in less densely populated areas to induce supply. Once the government raises demand by subsidizing preschool, firms enter the market and more children enroll.[33]

IV.b.iii. Preschool Enrollment Differences by Marital Status & the Presence of Younger Siblings

Different mothers face different constraints when making decisions about the use of child care and the decision to work. In the context of the theory outlined earlier, married mothers likely have more "exogenous" income (if their husbands work) than single mothers. Also, it is more expensive to place two children into care than one, but work is only possible if both children are cared for by others. Because of these differences, I estimate the effects of Universal Pre-K separately for the samples of married and single mothers with and without additional household children under age four (Table 5). Universal Pre-K in Georgia increases the likelihood that single mothers enroll their four year olds in preschool by 11.3 percentage points, or 16 percent, compared to 9 percentage points, or 13 percent, for married mothers (these numbers are not in the table, but are available upon request). Both estimates are statistically

[33] In order to find support for this supply side theory, I estimated equation (1) with measures of population density (of four year olds) interacted with state fixed effects and the age-eligibility terms. The resulting estimate of a positive relationship between population density and the effects of Universal Pre-K supports the thin markets story. Results are available from the author upon request.

significant at the one percent level, but the difference between the two groups is not statistically significant. In Oklahoma, the effect of Universal Pre-K is positive for both single and married mothers, but the coefficient estimate of 6.6 percentage points is only statistically significant for married mothers. Here too though, I cannot rule out the possibility that the effects for both groups are the same. Turning to the results in the Table for the groups of single and married mothers further subdivided by whether they have other young children (under age four) in the home, we can see the estimates imply that Universal Pre-K availability increases the preschool enrollment of all children, regardless of whether there are other young children in the home. However, only the estimated effects on preschool enrollment of children without younger siblings in Georgia are statistically significant.

Another dimension along which mothers deciding about child care differ is in the wages they will face if they enter the labor force. Mothers with more education on average receive higher market wages than those with less education. To see whether these differences translate into different adjustments to Universal Pre-K, I repeat the estimation of equation (1) separately for married versus single mothers and by levels of educational attainment – less than a high school diploma, exactly a high school diploma, some college attendance or a completed Bachelor's Degree, graduate or professional degree (available upon request). The results show that Universal Pre-K availability induces new preschool enrollment of young children from most backgrounds, with the largest effect for children of women with lower levels of educational attainment. This pattern of seeing results for women with low levels of educational attainment but none for women with higher levels of educational attainment is consistent with the predictions of economic theory because more educated (higher wage) mothers are likely to be working and hence using a large amount of non-maternal child care even in the absence of

Universal Pre-K. Preschool enrollment rates of children ineligible for Universal Pre-K whose mothers have a BA or higher equal approximately 75 percent, which is 10 to 30 percentage points higher than the preschool enrollment rates of children whose mothers are less educated.

IV.c. Estimation Results for Maternal Labor Supply[34]

Given that Universal Pre-K availability induced new preschool enrollment, the question becomes whether the increased enrollment served to alter the labor supply of mothers. As Table 6 shows, the effects of Universal Pre-K availability on the average statewide labor supply of mothers vary in sign and are not statistically significant.[35] For example, Universal Pre-K in Georgia increased mothers' probability of employment at any point in the previous year by 0.7 percentage points but the estimate is not statistically different from zero. A similar lack of effect is observed when hours or weeks worked is the dependent variable. Universal Pre-K availability may have decreased the probability of public assistance receipt by about 1 percentage point, a 20 percent decrease for this group of mothers. However, I am cautious about placing too much emphasis on these results because of the small sample sizes and vague definition of "public assistance receipt."[36]

Variation among women in preferences, wages and the price and quality of available child care, may affect labor supply responses to the introduction of Universal Pre-K. To determine whether this is the case, I again estimate equation (1) on different sub-samples of women defined in terms of demographic characteristics. First, I separate the sample based on the

[34] Other outcomes were also measured including maternal enrollment in school, commuting time of the mother. However, none of these estimates were statistically significant, so they have been left out.
[35] The estimates of the coefficients on demographic variables are generally of magnitudes and directions as predicted by economic theory and previous research. They are available from the author upon request.
[36] The actual Census questionnaire asks respondents to report the annual amount of "any public assistance or welfare payments from the state or local welfare office." It is not clear what this means to respondents and the answers to the question do not tell us anything about which type of public assistance the respondent is reporting (e.g. child care subsidies or food stamps).

type of residential area (columns II through IV of Table 6). Many of the estimates of the effects of Universal Pre-K on maternal labor supply for women in different residential areas are statistically indistinguishable from zero. A notable exception is that Universal Pre-K increases employment of rural women in Georgia by 10 percentage points. This 20 percent increase in employment occurs whether I use employment in 1999 or in the week prior to the survey in 2000 as the dependent variable. Coupled with the dramatic increases in preschool enrollment in rural areas because of Universal Pre-K, this result suggests that the program had very different effects in these rural areas of the state than it did in more densely populated areas.

Next, I group women by marital status and education (available upon request) and by marital status and whether the four year old was the mother's youngest child (Table 7). Although some mothers, specifically single mothers, tend to increase their labor supply because of Universal Pre-K availability, most of the estimates are not precise enough to be distinguished statistically from zero. Single mothers with no children under the age of four in the home are estimated to have increased employment on the extensive margin by 2 to 11 percent, though the estimate is only statistically significant for mothers in Oklahoma when the dependent variable is employment at any point in 1999. Cascio (forthcoming) and Gelbach (2002) also estimated there to be positive effects of kindergarten on single mothers of five year olds without younger children in the home. Chi-square tests cannot reject their estimates at the 95 percent confidence level, which leads me to leave open the possibility there was an effect on the labor supply of these women.

IV.e. Specification Checks

One concern is that the choice of width – the 30 day window on either side of the cutoff that I use to select the sample – influences the results. There is a tradeoff between making the sample window wider (thereby increasing the sample size) or making it narrower (making the groups on either side of the cutoff as comparable as possible). In the extreme case, it would be difficult to argue that there are huge differences between otherwise observationally identical women whose children are born one day apart. But unobservable differences are much more likely when comparing observationally similar mothers whose children are born in June versus December.[37] Table 8 displays results of regressions run on samples with different widths. The estimates illustrate the tradeoff between precision and comparability. As the width of the sample narrows, the standard errors (reported in parentheses) generally grow while the estimated effect sizes also grow (in absolute value). However, the changes in the estimated effects of Universal Pre-K are not appreciably different across the columns.

Another way to check the exogeneity of the treatment is to compare the regression results when controls are included and excluded. This comparison can be performed by contrasting the results reported in the top panel (which includes controls) and the bottom panel (which does not include controls) of Table 8. There are very few statistically significant differences in the results with any of the dependent variables shown. The same is true for outcomes not reported in the table.[38]

Examining the results when using a "placebo" cutoff additionally supports the conclusion that the estimated effects are the effects of Universal Pre-K rather than artifacts of the specifications. For this reason, I tested several placebo cutoffs, including those at 25, 30, 50, 60,

[37] These differences across quarter of birth are highlighted in Bound, Jaeger, and Baker 1995; Bound and Jaeger 2000 and McCrary and Royer 2005.
[38] Results are available from the author upon request.

75, 90 and 100 days before and after the actual cutoff. The results, available from the author upon request, show no relationships between the outcomes of interest and these placebo cutoffs.

Another concern one might have with the empirical strategy employed in this paper is that the entire set of states without Universal Pre-K does not equal the best counterfactual for the response of families in Georgia and Oklahoma to the kindergarten eligibility attached to the September 1st cutoff date. The concern is that there are unobservable characteristics of Georgians and Oklahomans that are also related to their responsiveness to the eligibility cutoff for kindergarten. If this were the case, it would lead to biased estimates of the effects of Universal Pre-K. To test this concern I re-estimate the effects of Universal Pre-K using various samples selected because the included states are arguably closer to Georgia and Oklahoma on unobservable characteristics than the entire country.

The first column of Table 9 shows the results when the sample is limited to only states with September 1st cutoffs. (Table 1 shows which states these are.) The second and third columns employ a matching technique to create control groups of those mothers in states where the mean of ineligible women and families most closely resemble men outcomes of ineligible Georgians and Oklahomans, respectively. This technique is in the spirit of Abadie, Diamond and Hainmuller (2008), but rather than creating a weighted sample of observations from states without Universal Pre-K I have included the ten states that best 'match' Georgia or Oklahoma. In order to determine the best 'matches', for each state I calculated the sum of the squared deviations in mean outcomes per day (between each state and Georgia and Oklahoma, separately, for the period before the cutoff). The ten states for which this statistic were the lowest comprise the control groups in columns II and III.[39] Though the results vary slightly in terms of magnitude

[39] The states that make up the matched sample for Georgia include Florida, Louisiana, Maryland, North Carolina, Ohio, South Carolina, Virginia, Mississippi, Missouri and Vermont. The states that make up the matched sample for

and statistical significance, the general conclusions discussed of the analyses are the same – Universal Pre-K availability increases preschool enrollment but has indiscernible effects on maternal labor supply outcomes.

V. Conclusion

This paper has two key findings – first, that Universal Pre-K significantly and substantially raised preschool enrollment of four year olds in Georgia and Oklahoma, and second, that labor supply of mothers of four year olds in those states generally did not increase.

The availability of Universal Pre-K in Georgia and Oklahoma raised the preschool enrollment of four year olds by 14 to 17 percent statewide. Preschool enrollment rates of children residing in less densely populated residential areas are most affected by the availability of Universal Pre-K. The largest effects of the program are in rural areas, while smaller but still significant effects are found in larger and smaller urban areas. Also, the increases in enrollment are largest for children with mothers at the middle to lower-end of the educational spectrum and for those without younger siblings. These most affected groups increase preschool enrollment by about 25 percent because of Universal Pre-K availability. In contrast, women with at least a Bachelor's Degree are not estimated to have changed the preschool enrollment behavior of their children because of Universal Pre-K availability, likely because these women are heavier users of preschool even in the programs' absence.

Although Universal Pre-K increases preschool enrollment in both states, the estimated effects on the labor supply of most mothers are statistically indistinguishable from zero. The estimated confidence intervals bound the estimated effects of Universal Pre-K availability in

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Oklahoma include Kentucky, Michigan, Minnesota, Nebraska, Oregon, Tennessee, Texas, Utah, Wisconsin and Wyoming. The results are quite similar to those using the methods in Abadie, Diamond and Hainmueller (2008).

Georgia on employment to be between a decrease of three and an increase of four percentage points, quite a narrow range. It does not appear that Universal Pre-K availability changes the labor supply of most mothers of four year olds (though it may have decreased their probability of receiving public assistance income). One exception is women living in rural areas in Georgia, where the estimated enrollment effects were also largest. Another possible exception is the group of mothers who are single without younger children. Although none of the coefficients were estimated to be statistically different than zero at conventional levels, the signs all move together, suggesting that Universal Pre-K availability might have increased the labor supply of these mothers.

The lack of a program effect on maternal labor supply is somewhat surprising given the previous literature, which generally finds some statistically significant effects of universal care subsidization on labor supply. The reason may be that the population of women working has changed (compared to earlier studies which mostly focused on data from twenty to forty years ago) and therefore so has the population of women at the margin.[40] The findings in the recent literature that female labor supply elasticities have declined over time (Blau and Kahn 2007; Heim 2004) may explain these results. Economic theory would predict the women most likely to be induced to increase their labor supply by this type of subsidy are those who work less than the number of hours of care provided by the programs. This amounts to just less than one-third of the population of ineligible mothers of four year olds in Georgia, the state with the longer program. Another thing to note is that the effect estimated here is the effect of Universal Pre-K availability on maternal labor supply as compared to the early childhood preschool and child care

[40] For example, the baseline rates of maternal employment in previous literature for women who had employment responses to universal subsidies were between 17 and 55 percent. The baseline employment rate for women in my analysis is about 77 percent. I note this difference as an important potential explanation for the differences between the results presented.

subsidy landscape existing in 2000. As such, many women in the "control group" who are likely to be induced to change their behavior by child care subsidization, such as those from low income backgrounds, may already be receiving subsidies (such as Head Start).

The combination of results – an increase in preschool enrollment coupled with little change in labor supply – signals that the return to the government's investment in Universal Pre-K should be measured by its effects on child outcomes. One potential explanation for the results would be a pattern of mothers of age-eligible children shifting from informal child care to formal child care. Because the Census asks about preschool, respondents likely do not answer affirmatively if children are enrolled at day care or are at a babysitter's or grandmother's house. If the results presented here are evidence of switching from informal to formal day care, the policy focus should be on the quality of Universal Pre-K relative to other existing modes of child care. Finally, the results for mothers living in rural and less densely populated areas suggest Universal Pre-K increases access of some families to preschool. Furthering our understanding of how Universal Pre-Kindergarten and other child care subsidies affect the supply side of the market for child care could provide valuable information about the choices women make about child care, including choices between informal and formal care, and labor supply.

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Table 1: Kindergarten Cutoff Dates by State, 1999

State	*Cutoff Date*	*State*	*Cutoff Date*
Alabama	September 1	Montana	September 10
Alaska	August 15	Nebraska	October 15
Arizona	September 1	Nevada	September 30
Arkansas	August 1	New Hampshire	LEA
California	December 2	New Jersey	LEA
Colorado	LEA	New Mexico	September 1
Connecticut	January 1	New York	December 1
Delaware	August 31	North Carolina	October 16
Florida	September 1	North Dakota	August 31
Georgia	September 1	Ohio	September 30
Hawaii	December 31	Oklahoma	September 1
Idaho	September 1	Oregon	September 1
Illinois	LEA	Pennsylvania	LEA
Indiana	June 1	Rhode Island	December 31
Iowa	September 15	South Carolina	September 1
Kansas	August 31	South Dakota	September 1
Kentucky	October 1	Tennessee	September 30
Louisiana	September 30	Texas	September 1
Maine	October 15	Utah	September 1
Maryland	December 31	Vermont	January 1
Massachusetts	LEA	Virginia	September 30
Michigan	December 1	Washington	LEA
Minnesota	September 1	West Virginia	August 31
Mississippi	September 1	Wisconsin	September 1
Missouri	August 1	Wyoming	September 15

Note: Data on kindergarten cutoffs are from a survey conducted by the Indiana Department of Education, http://www.doe.state.in.us/legwatch/2000/a_kinder_issues.html (Accessed August 21, 2007.) LEA states are those that leave the designation of kindergarten age eligibility cutoffs to the local education authorities.

Table 2: Means of Demographic Characteristics

	Georgia		Oklahoma		Other States	
	Before Cutoff	*After Cutoff*	*Before Cutoff*	*After Cutoff*	*Before Cutoff*	*After Cutoff*
Age	31.500	31.488	30.680	30.978	32.127	32.176
	(0.248)	(0.237)	(0.303)	(0.344)	(0.048)	(0.046)
Age Squared	1027.322	1025.926	970.457	999.628	1067.919	1070.471
	(15.916)	(15.500)	(19.327)	(22.375)	(3.118)	(3.037)
White	0.587	0.585	0.754	0.754	0.710	0.719
	(0.020)	(0.019)	(0.025)	(0.023)	(0.004)	(0.004)
Married	0.735	0.727	0.772	0.753	0.766	0.766
	(0.018)	(0.017)	(0.024)	(0.023)	(0.003)	(0.003)
Other Household	0.889	0.861	0.863	0.864	0.922	0.918
Members, 18+ Years	(0.026)	(0.024)	(0.024)	(0.027)	(0.005)	(0.005)
Other Household	0.130	0.099	0.171	0.122	0.111	0.102
Members, 0-17 Years	(0.027)	(0.015)	(0.033)	(0.025)	(0.003)	(0.003)
Own & Step Children,	0.016	0.024	0.033	0.019	0.027	0.026
18+ Years	(0.005)	(0.006)	(0.013)	(0.007)	(0.002)	(0.001)
Own & Step Children,	0.139	0.147	0.147	0.184	0.165	0.159
13-17	(0.019)	(0.015)	(0.025)	(0.025)	(0.004)	(0.004)
Own & Step Children,	0.530	0.552	0.604	0.570	0.615	0.609
5-12	(0.028)	(0.027)	(0.043)	(0.041)	(0.006)	(0.006)
Own & Step Children,	0.279	0.328	0.305	0.328	0.304	0.311
0-3	(0.020)	(0.020)	(0.030)	(0.027)	(0.004)	(0.004)
High School Degree	0.260	0.279	0.287	0.298	0.255	0.256
	(0.018)	(0.017)	(0.025)	(0.024)	(0.003)	(0.003)
Some College	0.343	0.298	0.358	0.362	0.358	0.356
	(0.020)	(0.018)	(0.027)	(0.026)	(0.004)	(0.004)
BA Degree	0.189	0.194	0.199	0.178	0.180	0.183
	(0.016)	(0.016)	(0.023)	(0.022)	(0.003)	(0.003)
Graduate/Professional	0.082	0.088	***	***	0.074	0.073
Degree	(0.011)	(0.012)			(0.002)	(0.002)
Days	-199.6	-168.8	-199.9	-169.2	-231.4	-200.6
	(0.357)	(0.342)	(0.465)	(0.490)	(0.395)	(0.388)
Weighted N	7,989	8,885	3,013	3,410	199,490	207,894
Approximate N	1,000	1,100	600	700	30,000	31,000

Note: Based on the author's calculations using the Restricted Access Decennial Census Long Form Data. The sample includes mothers whose own singleton children were born within 30 days of the kindergarten cutoff in their state of residence and for whom data on all variables was available. *Days* measures the distance in days from a child's date of birth and March 15, 1995. *** means that the number could not be released from the data center due to confidentiality restrictions. Sample weights were used and standard errors are in parentheses. Standard deviations and actual sample sizes are waiting to be cleared from the Data Center but will be available from the author upon request.

Table 3: Estimates of the Effect of Universal Pre-K on Preschool Enrollment by Type of Residential Area

Explanatory Variable	All Areas	Rural	Urban Cluster	Urban Area
	(I)	(II)	(III)	(IV)
GA Cutoff	**0.104**	**0.119**	**0.144**	**0.092**
	(0.027)	(0.043)	(0.063)	(0.028)
OK Cutoff	**0.071**	0.097	0.105	0.019
	(0.031)	(0.048)	(0.069)	(0.051)
Cutoff	**0.075**	**0.059**	**0.124**	**0.072**
	(0.012)	(0.024)	(0.036)	(0.015)
Age	**0.020**	**0.038**	0.008	**0.016**
	(0.003)	(0.007)	(0.010)	(0.004)
(Age x 100) Squared	-0.022	-0.001	0.000	0.000
	(0.504)	(0.000)	(0.000)	(0.000)
White	**-0.058**	**-0.153**	**-0.084**	**-0.033**
	(0.005)	(0.014)	(0.016)	(0.006)
Married	**-0.039**	**-0.052**	**-0.031**	-0.006
	(0.007)	(0.016)	(0.008)	(0.021)
Other Household Members, 18+ Years	**-0.032**	**-0.065**	-0.019	**-0.046**
	(0.005)	(0.011)	(0.012)	(0.005)
Other Household Members, 0 to 17 Years	**-0.030**	**-0.026**	**-0.036**	**-0.026**
	(0.006)	(0.011)	(0.016)	(0.007)
Own & Step Children, 18+ Years	**-0.056**	**-0.050**	-0.051	**-0.062**
	(0.013)	(0.010)	(0.044)	(0.016)
Own & Step Children, 13 to 17	**-0.055**	**-0.025**	**-0.039**	**-0.052**
	(0.005)	(0.006)	(0.016)	(0.006)
Own & Step Children, 5 to 12	**-0.031**	**-0.028**	**-0.026**	**-0.029**
	(0.003)	(0.006)	(0.009)	(0.004)
Own & Step Children, 0 to 3	**-0.037**	**-0.049**	**-0.031**	**-0.061**
	(0.004)	(0.009)	(0.005)	(0.013)
High School Degree	**0.062**	**0.073**	0.032	**0.071**
	(0.007)	(0.015)	(0.019)	(0.008)
Some College	**0.150**	**0.153**	**0.131**	**0.153**
	(0.006)	(0.015)	(0.019)	(0.008)
BA Degree	**0.227**	**0.203**	**0.186**	**0.227**
	(0.007)	(0.016)	(0.021)	(0.007)
Graduate/Professional Degree	**0.260**	**0.245**	**0.201**	**0.264**
	(0.007)	(0.019)	(0.029)	(0.007)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. Each column in the table represents results from a separate regression. The estimation is of equation (1) from the text, with a cubic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence who live in the residential area noted at the top of the column. Demographic characteristics include those listed in Table II. State fixed effects are used and sample weights are incorporated. The dependent variable is the child's enrollment in preschool. As such, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Table 4: Estimates of the Effects of Universal Pre-K on Various Types of Preschool Enrollment

	All Enrollment	Public Enrollment	Private Enrollment
	(I)	(II)	(III)
GA Cutoff	**0.104**	**0.161**	**-0.057**
	(0.027)	(0.022)	(0.020)
OK Cutoff	**0.071**	**0.104**	-0.033
	(0.031)	(0.032)	(0.026)
Cutoff	**0.075**	**0.060**	**0.015**
	(0.012)	(0.008)	(0.004)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. The estimation is of equation (1) from the text, with a cubic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence whose mothers have the characteristics noted. Demographic characteristics include those listed in Table II, where appropriate. State fixed effects are used and sample weights are incorporated. The dependent variable is the child's enrollment in preschool. As such, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Table 5: Estimates of the Effects of Universal Pre-K on Preschool Enrollment by Marital Status and Presence of Younger Children

		With Younger Children	With No Younger Children
		(I)	(II)
		Single Mothers	
Preschool Enrollment	GA Cutoff	0.132	**0.112**
		(0.088)	(0.050)
	OK Cutoff	0.106	0.028
		(0.128)	(0.082)
		Married Mothers	
Preschool Enrollment	GA Cutoff	0.052	**0.119**
		(0.046)	(0.029)
	OK Cutoff	0.065	0.070
		(0.062)	(0.041)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. The estimation is of equation (1) from the text, with a cubic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence whose mothers have the characteristics noted. Demographic characteristics include those listed in Table II, where appropriate. State fixed effects are used and sample weights are incorporated. The dependent variable is the child's enrollment in preschool. As such, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Table 6: Estimates of the Effect of Universal Pre-K on Maternal Labor Supply and Public Assistance Receipt by Type of Residential Area

Dependent Variable		All Areas	Rural	Urban Cluster	Urban Area
		(I)	(II)	(III)	(IV)
Worked Last Year	GA Cutoff	0.007	**0.106**	-0.037	-0.039
		(0.022)	(0.034)	(0.071)	(0.031)
	OK Cutoff	0.004	0.007	0.042	-0.030
		(0.032)	(0.047)	(0.061)	(0.053)
Worked Last Week	GA Cutoff	-0.012	**0.092**	0.055	-0.070
		(0.025)	(0.042)	(0.073)	(0.031)
	OK Cutoff	-0.002	0.006	0.006	-0.007
		(0.034)	(0.050)	(0.074)	(0.054)
Hours Worked per Week Last Year	GA Cutoff	-0.202	0.053	-2.106	-0.155
		(0.639)	(1.123)	(1.519)	(0.847)
	OK Cutoff	0.636	-1.643	3.780	0.795
		(0.955)	(1.285)	(2.027)	(1.627)
Weeks Worked Last Year	GA Cutoff	0.911	0.758	-0.380	0.896
		(0.822)	(1.432)	(2.719)	(1.065)
	OK Cutoff	0.163	2.590	-0.839	-1.162
		(1.213)	(1.909)	(2.806)	(1.877)
Wage & Salary Income Last Year	GA Cutoff	677.081	868.913	-5033.729	1534.123
		(1203.739)	(1868.616)	(3737.972)	(1592.680)
	OK Cutoff	1485.604	2230.142	3189.124	118.069
		(1312.144)	(1500.824)	(2984.145)	(2291.310)
Received Welfare Income Last Year	GA Cutoff	**-0.009**	-0.007	-0.025	-0.006
		(0.005)	(0.008)	(0.011)	(0.009)
	OK Cutoff	-0.003	0.020	-0.019	-0.006
		(0.008)	(0.022)	(0.014)	(0.013)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. Each column and row set in the table represents results from a separate regression. The estimation is of equation (1) from the text, with a cubic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence who live in the residential area noted at the top of the column. Demographic characteristics include those listed in Table II, where appropriate. State fixed effects are used and sample weights are incorporated. When the dependent variable is binary, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Table 7. Estimates of the Effect of Universal Pre-K on Employment and Public Assistance Receipt of Women b y Marital Status and Presence of Younger Children

		(I)	(II)	(III)	(IV)
		WITH YOUNGER CHILDREN	WITH NO YOUNGER CHILDREN	WITH YOUNGER CHILDREN	WITH NO YOUNGER CHILDREN
		Single Mothers		***Married Mothers***	
Worked Last Year	GA Cutoff	0.053	0.036	0.033	-0.019
		(0.072)	(0.037)	(0.046)	(0.033)
	OK Cutoff	0.289	**0.117**	0.068	-0.035
		(0.167)	(0.022)	(0.064)	(0.045)
Worked Last Week	GA Cutoff	0.035	0.024	-0.037	-0.012
		(0.098)	(0.056)	(0.048)	(0.035)
	OK Cutoff	0.005	0.022	0.035	-0.022
		(0.015)	(0.084)	(0.067)	(0.046)
Hours Per Week Last Year	GA Cutoff	0.709	0.179	0.892	-0.064
		(2.110)	(1.203)	(1.608)	(0.914)
	OK Cutoff	4.135	0.302	1.882	0.509
		(2.833)	(2.510)	(2.117)	(1.253)
Weeks Worked Last Year	GA Cutoff	**11.961**	1.197	1.937	1.014
		(3.302)	(1.712)	(1.908)	(1.132)
	OK Cutoff	**9.227**	1.713	1.785	0.637
		(4.740)	(2.627)	(2.513)	(1.624)
Wage & Salary Income Last Year	GA Cutoff	7866.67	3348.24	2361.83	-524.349
		(6494.00)	(2293.61)	(1789.70)	(1705.76)
	OK Cutoff	1380.50	-541.201	1485.19	1148.18
		(3190.33)	(2430.00)	(2356.39)	(1788.18)
Public Assistance Receipt	GA Cutoff	-0.079	-0.041	-0.005	-0.003
		(0.075)	(0.039)	(0.007)	(0.006)
	OK Cutoff	-0.077	-0.012	0.105	-0.003
		(0.125)	(0.060)	(0.081)	(0.006)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. The estimation is of equation (1) from the text, with a cubic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence whose mothers have the characteristics noted. Demographic characteristics include those listed in Table II, where appropriate. State fixed effects are used and sample weights are incorporated. When the dependent variable is binary, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses. A ** represents results unavailable because of small sample sizes.

Table 8. Estimates of the Effects of Universal Pre-K on Preschool Enrollment, Work in 1999 and Public Assistance Receipt in 1999 When Varying the Width of the Sample and the Inclusion of Controls

		(I)	(II)	(III)	(IV)	(V)	(VI)
		WHOLE SAMPLE	200 DAYS	100 DAYS	60 DAYS	30 DAYS	14 DAYS
Demographic Controls Included							
Preschool Enrollment	GA Cutoff	**0.074**	**0.075**	**0.076**	**0.077**	**0.094**	**0.116**
		(0.010)	(0.010)	(0.013)	(0.016)	(0.022)	(0.031)
	OK Cutoff	**0.069**	**0.069**	**0.063**	**0.073**	**0.074**	**0.087**
		(0.013)	(0.013)	(0.018)	(0.022)	(0.030)	(0.041)
Mother's Employment 1999	GA Cutoff	0.011	0.009	-0.003	-0.002	0.002	-0.008
		(0.010)	(0.010)	(0.013)	(0.016)	(0.023)	(0.033)
	OK Cutoff	0.008	0.009	0.008	-0.005	0.006	0.038
		(0.013)	(0.013)	(0.018)	(0.023)	(0.032)	(0.043)
Public assistance receipt	GA Cutoff	-0.006	-0.006	-0.004	-0.003	-0.012	**-0.032**
		(0.005)	(0.005)	(0.004)	(0.008)	(0.010)	(0.009)
	OK Cutoff	-0.002	-0.003	0.005	-0.009	-0.007	-0.002
		(0.007)	(0.007)	(0.006)	(0.010)	(0.015)	(0.021)
No Demographic Controls Included							
Preschool Enrollment	GA Cutoff	**0.079**	**0.079**	**..069**	**0.086**	**0.104**	**0.128**
		(0.010)	(0.010)	(0.013)	(0.016)	(0.022)	(0.030)
	OK Cutoff	**0.067**	**0.067**	**0.064**	**0.074**	**0.071**	**0.082**
		(0.013)	(0.014)	(0.017)	(0.022)	(0.031)	(0.041)
Mother's Employment 1999	GA Cutoff	0.012	0.010	-0.005	0.002	0.007	0.015
		(0.010)	(0.010)	(0.013)	(0.016)	(0.022)	(0.032)
	OK Cutoff	0.009	0.010	0.007	0.001	0.004	0.027
		(0.013)	(0.014)	(0.018)	(0.022)	(0.032)	(0.042)
Public assistance receipt	GA Cutoff	-0.005	-0.005	-0.002	-0.005	-0.009	**-0.013**
		(0.003)	(0.003)	(0.007)	(0.004)	(0.005)	(0.005)
	OK Cutoff	0.000	0.001	0.000	0.000	-0.003	0.003
		(0.004)	(0.004)	(0.009)	(0.006)	(0.008)	(0.013)

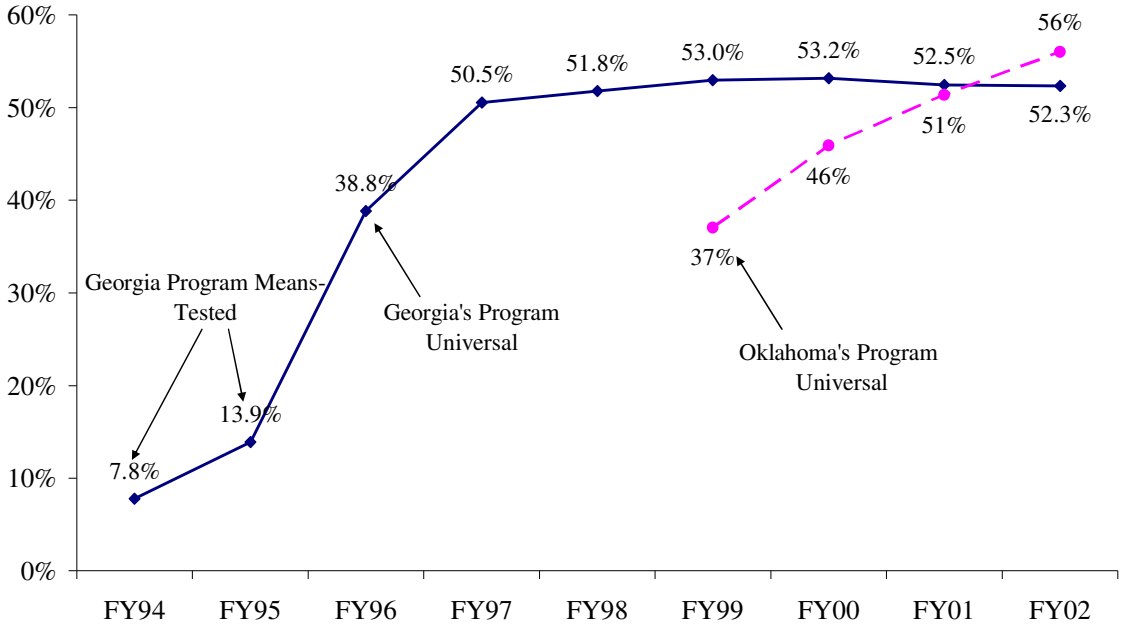
Notes: Based on the author's estimation of equation (1) using the restricted-access 2000 Decennial Long Form Data, with a quadratic in age of the child (in days). Each column and row set in the table represents results from a separate regression. Sample includes children born within the indicated number of days of the kindergarten cutoff in their state of residence. When included, demographic characteristics include those listed in Table II (upper panel). Because the dependent variables are binary, probit methods are used. Results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Table 9. Estimates of the Effect of Universal Pre-K on Preschool Enrollment and Maternal Labor Supply with Various Comparison Samples

Dependent Variable		(I) September 1 States	(II) Georgia Matched Sample	(III) Oklahoma Matched Sample
Preschool Enrollment	GA Cutoff	**0.078**	**0.108**	
		(0.023)	(0.021)	
	OK Cutoff	0.046		0.017
		(0.032)		(0.034)
Worked Last Year	GA Cutoff	0.012	0.016	
		(0.023)	(0.022)	
	OK Cutoff	0.013		0.005
		(0.031)		(0.032)
Worked Last Week	GA Cutoff	-0.018	-0.005	
		(0.026)	(0.025)	
	OK Cutoff	-0.006		-0.002
		(0.034)		(0.035)
Hours Worked per Week Last Year	GA Cutoff	-0.173	-0.382	
		(0.664)	(0.673)	
	OK Cutoff	0.679		1.039
		(0.971)		(0.983)
Weeks Worked Last Year	GA Cutoff	0.648	0.550	
		(0.852)	(0.863)	
	OK Cutoff	-0.067		-0.197
		(1.235)		(1.245)
Wage & Salary Income Last Year	GA Cutoff	796.610	-567.535	
		(1274.630)	(1280.600)	
	OK Cutoff	1584.731		1140.180
		(13826.289)		(1501.150)
Received Welfare Income Last Year	GA Cutoff	**-0.009**	**-0.008**	
		(0.004)	(0.004)	
	OK Cutoff	0.005		-0.004
		(0.007)		(0.007)

Notes: Based on the author's estimation of equation (1) using the restricted-access 2000 Decennial Long Form Data, with a quadratic in age of the child (in days). Each column and row set in the table represents results from a separate regression. Samples are defined as describe in the text and include children born within 30 days of the kindergarten cutoff in their state of residence. Demographic characteristics include those listed in Table II. When the dependent variables are binary, probit methods are used and results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Figure I. Percentage of Four Year Olds Enrolled in the Pre-K Programs



Notes: From Brackett, Henry and Weathersby (1999) and various web sources. A fiscal year runs from October of the previous year to September of the year in its name. For example, FY96 runs from October 1, 1995 to September 30, 1996. Percent of population of four year olds is calculated using the Census Bureau's Time Series of State Population Estimates by Age, which can be found at http://www.census.gov/.

Figure II: Maternal Characteristics Around the Discontinuity

Figure II.A. Percent of Mothers Who are White

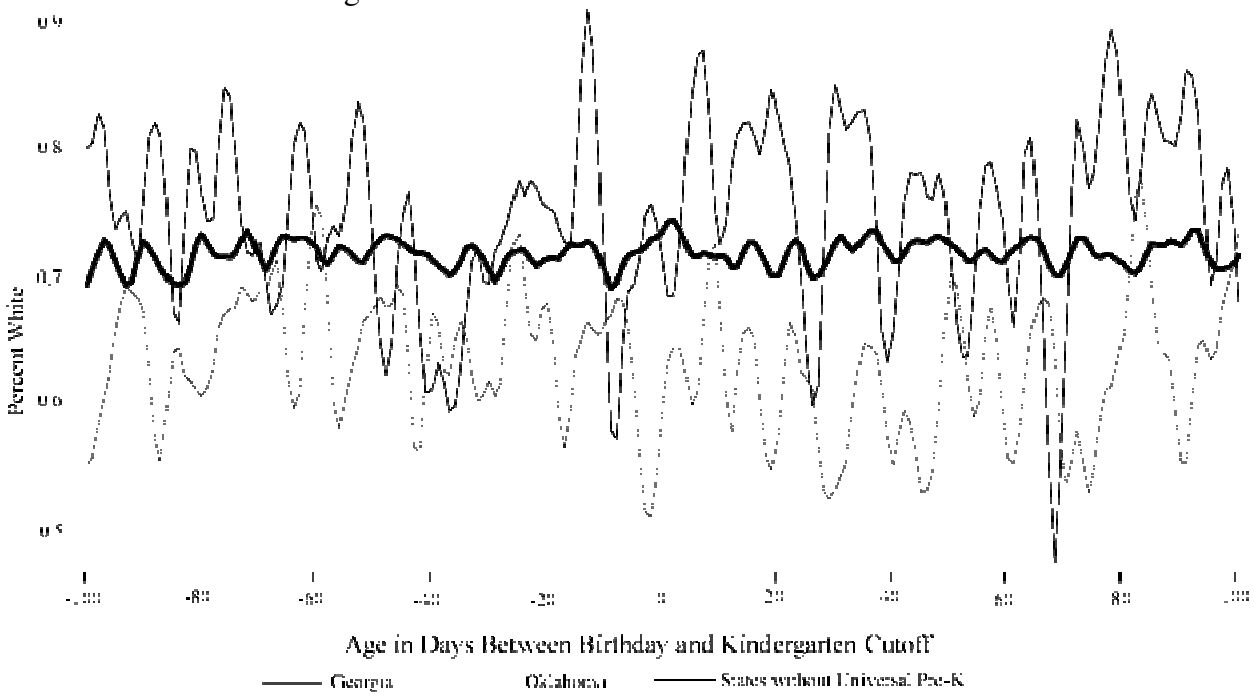


Figure II.B. Percent of Mothers Who are Married

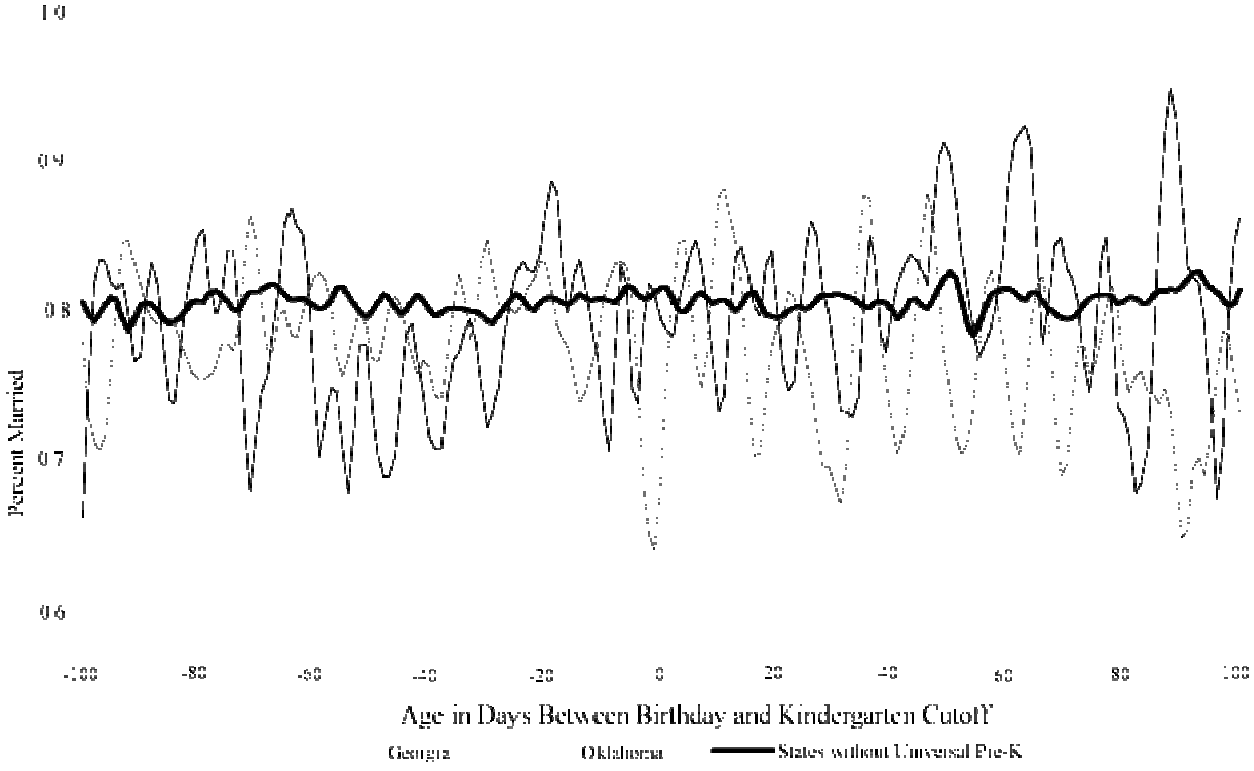


Figure II.C. Age of Mothers

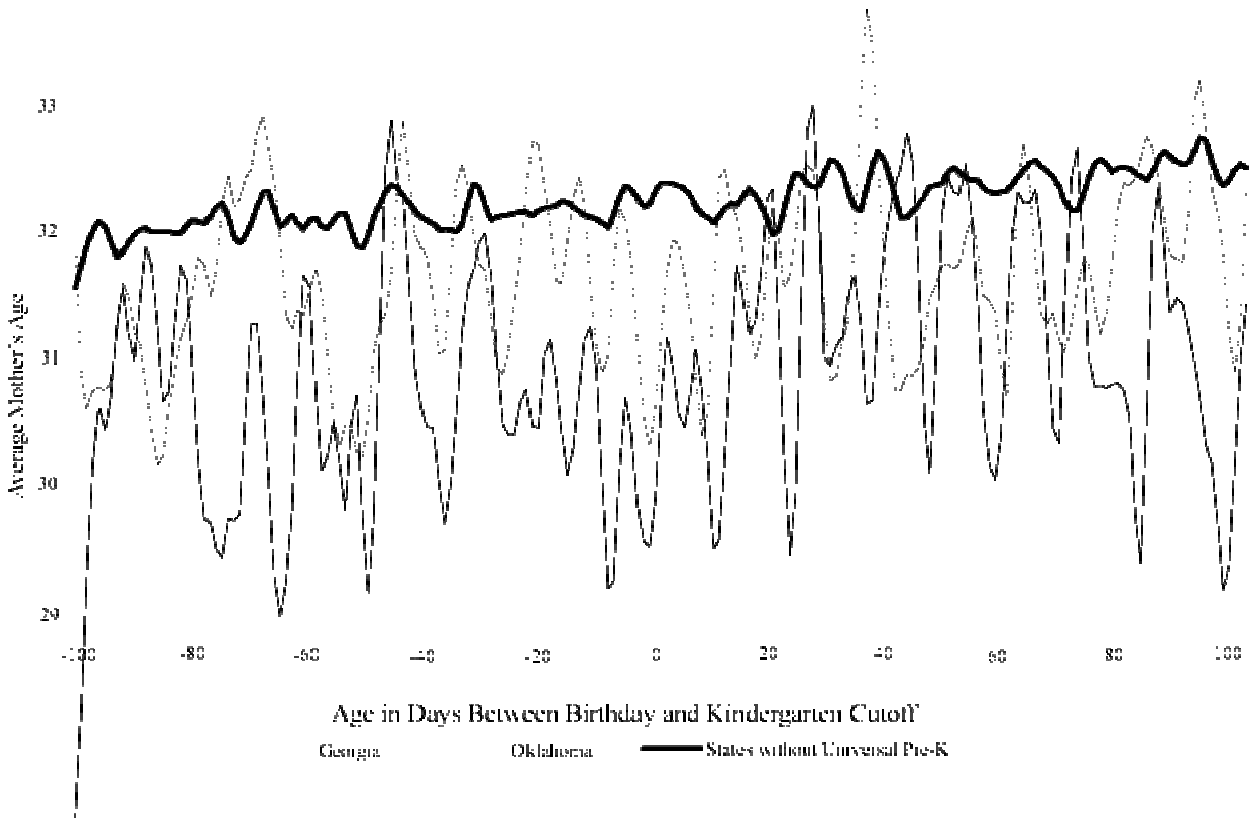


Figure II.D. Percent of Mothers with Children Aged Zero to Three

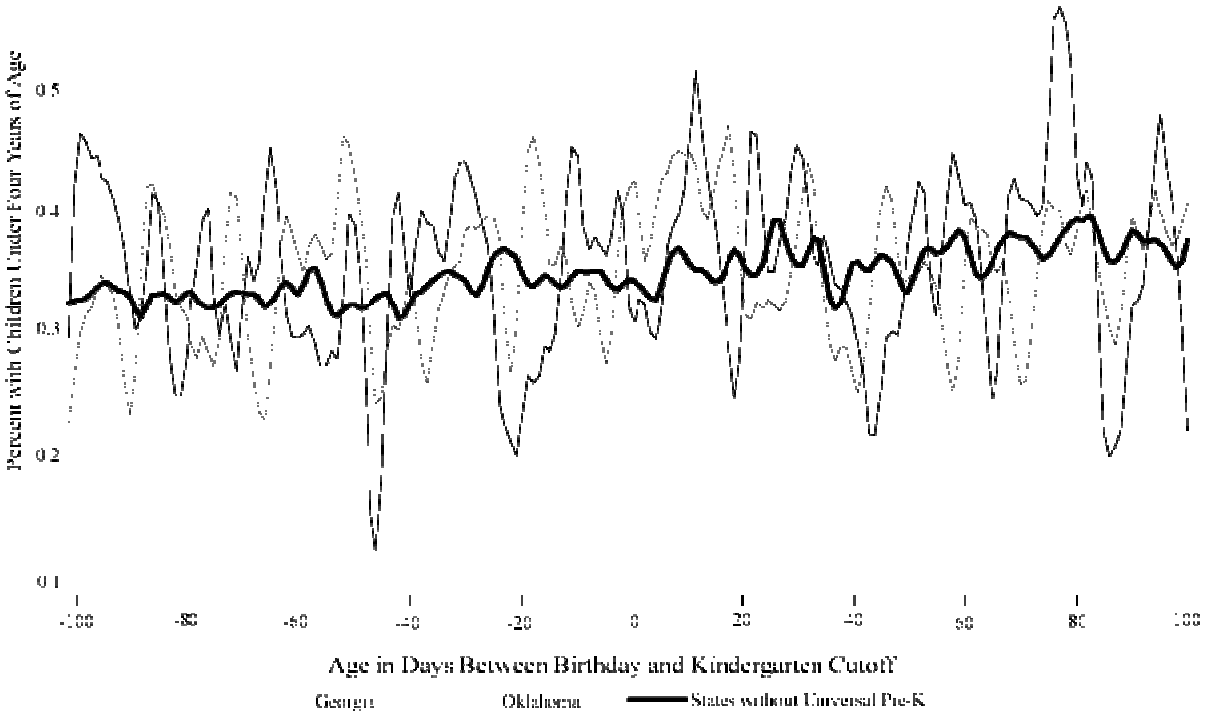
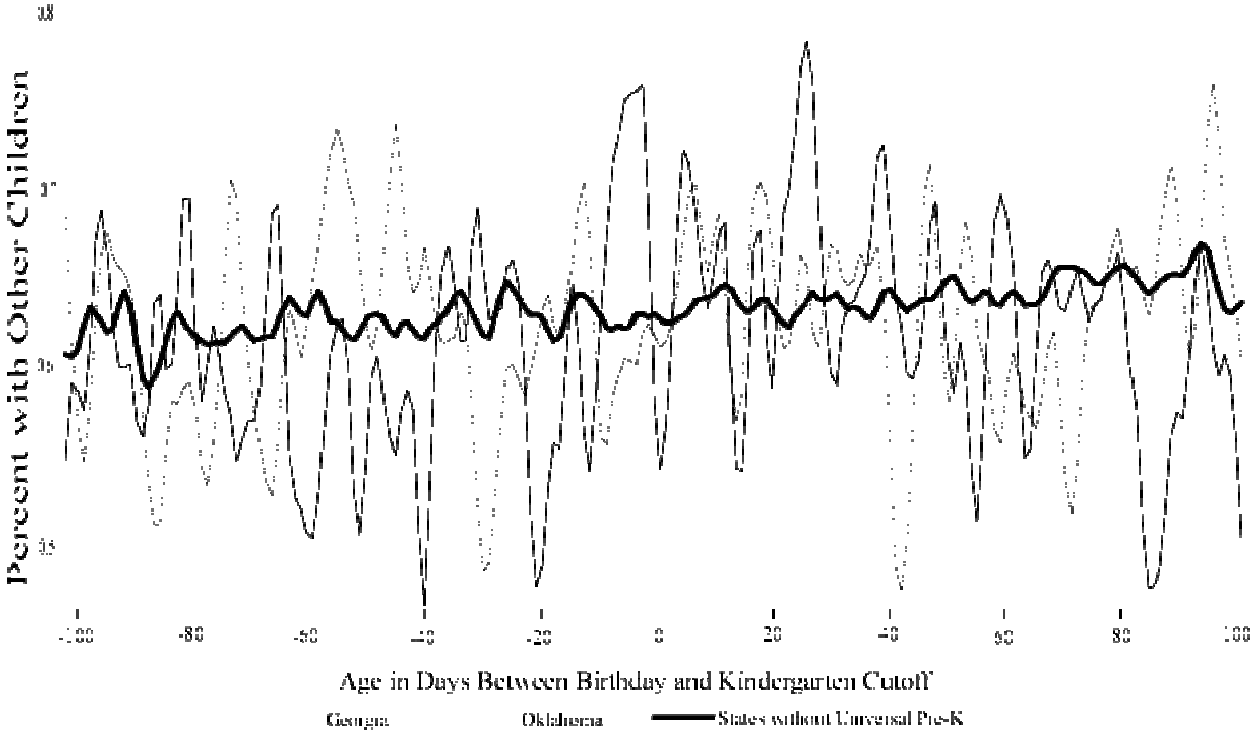
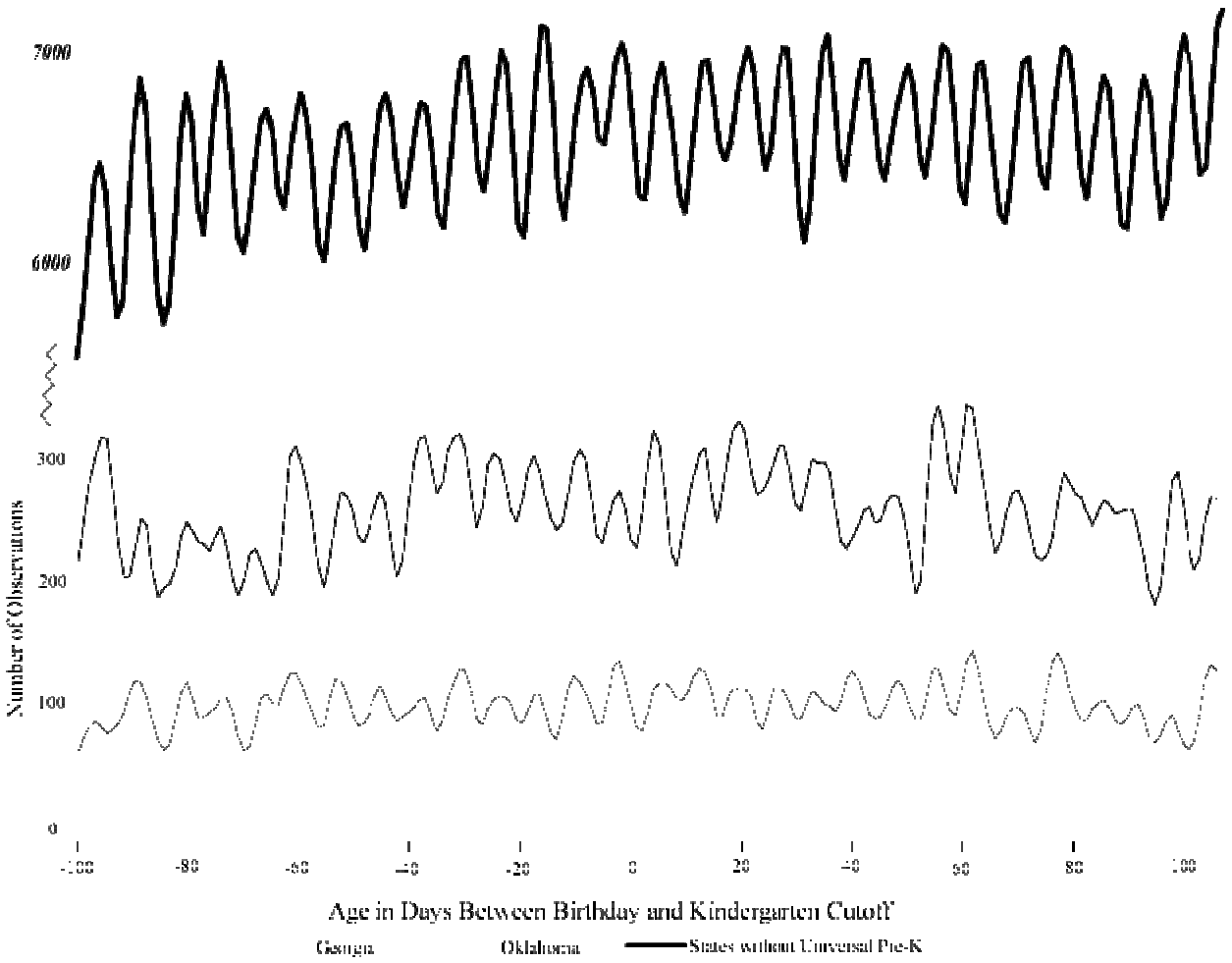


Figure II.E. Percent of Mothers with Children Under 18



Note: Based on the author's calculations using Restricted Access 2000 Census Decennial Long Form Data.

Figure III. Density of Observations Near the Cutoff



Note: Based on the author's calculations using Restricted Access 2000 Census Decennial Long Form Data.

Figure IV: Maternal Outcomes around the Cutoff

Figure IV.A. Preschool Enrollment Rate

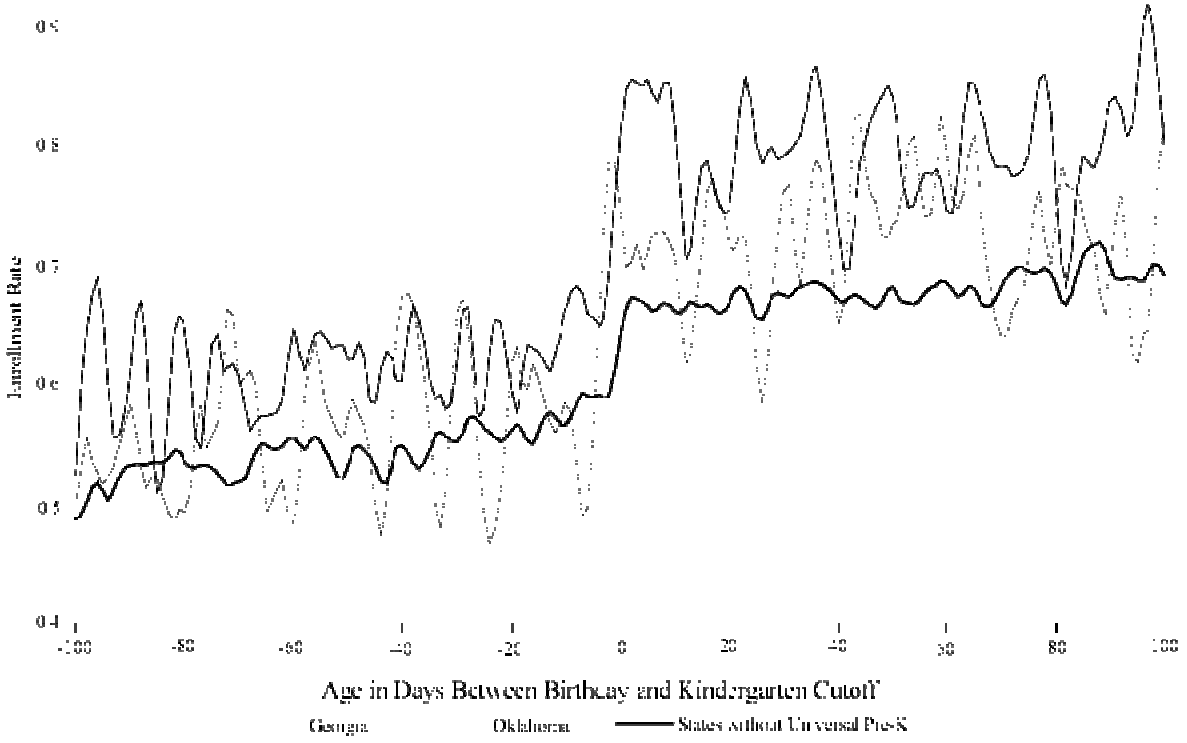
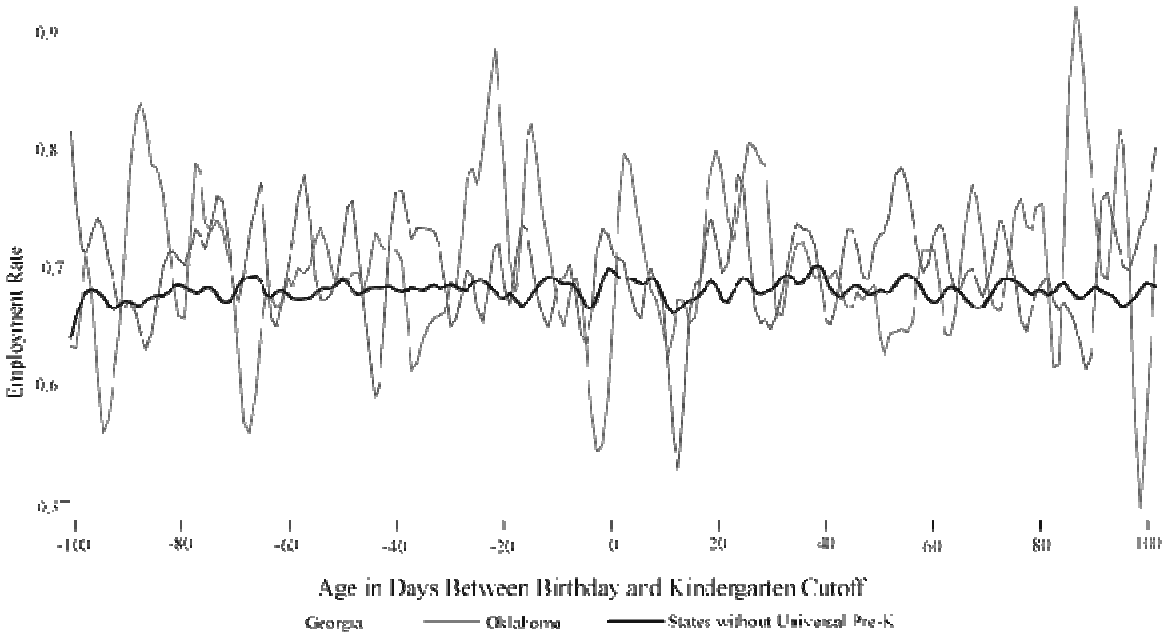


Figure IV.B. Employment Rate of Mothers in 1999



Note: Based on the author's calculations using Restricted Access 2000 Census Decennial Long Form Data.