Revising Our Thinking About the Relationship Between Maternal Labor Supply and Preschool

Maria Donovan Fitzpatrick

ABSTRACT

Many argue that childcare costs limit the labor supply of mothers, though existing evidence has been mixed. Using a child's eligibility for public kindergarten in a regression discontinuity instrumental variables framework, I estimate how use of a particular subsidy, public school, affects maternal labor supply. I find public school enrollment increases only the employment of single mothers without additional young children. I compare this result to previous work, focusing on striking increases in a similar setting but earlier period (Gelabch 2002). Differences in the population of mothers, labor supply, and patterns of lifecycle events likely drive the discrepancy in results.

I. Introduction

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 predictions from basic economic models about the effects of childcare subsidization are ambiguous. In the traditional two-good model used to describe mothers' childcare and labor supply choices the mother can choose between working (and purchasing care) or leisure

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(which implicitly includes taking care of the child herself). In this two-good framework, public school, which provides a set amount of care for young children "free of charge" to families, provides a full price subsidy for childcare on the margin for any woman working less than the length of the school day (and year). 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 public school, the program provides an income subsidy. This income subsidy exerts downward pressure on the amount of time in the workplace.

Within the last 15 years, investigators have used both demonstration programs (such as the New Chance program) and widespread targeted subsidies to examine the relationship between childcare subsidization and maternal labor supply. Researchers consistently find evidence that subsidization of childcare increases maternal labor supply (Bos et al. 1999; Granger and Cryton 1999; Blau and Tekin 2007).[1] In a slightly different approach, Cascio (2009) uses the timing of large increases in public funding of kindergartens (which largely occurred in the 1960s and 1970s) in the estimation of the effects of childcare availability on maternal labor supply. Cascio shows evidence of an increase in the labor supply of single mothers without other young children due to the increased funding of kindergarten, but no effects for other groups of women. 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 and finds preschool availability increases employment by about seven percentage points.

In work closely related to this study, Gelbach (2002) uses quarter-of-birth as an instrument for enrollment in kindergarten in 1980. The motivation underlying the use of quarter-of-birth dummies as instruments for public kindergarten enrollment stems from the same state administrative rules determining eligibility for kindergarten I use in this paper. The rules imply children born in April are generally eligible for public kindergarten when they are five while children born in December generally have to wait until the following year. Gelbach finds evidence of increases in labor supply measures of 6 to 24 percent, depending on the measure, and decreases in welfare assistance receipt of single women.

However, analyses of the elasticity of female labor supply in more recent periods show women are no longer as responsive to wage changes (Blau and Kahn 2007, Heim 2007). This decreased responsiveness to wages might mean childcare 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 and longer working hours 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. Indeed, when examining the

1. The analyses of the widespread subsidies are biased if the measures used to control for selection (for example, 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 socioeconomically 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 childcare subsidy. This makes it difficult to disentangle the effects of the subsidy alone.

effects of the introduction of a universal preschool subsidy for four-year-olds in Georgia and Oklahoma in the 1990s, Fitzpatrick (2010) finds no increases in maternal labor supply. Perhaps childcare subsidization no longer has the ability to increase the labor supply of mothers at the margin.

Given the differences in the results across these studies, I set out to determine whether the use of the free childcare subsidy provided through public school affects maternal labor supply. There are important distinctions between this study and the previous work. While many studies focus on the intent-to-treat effects of childcare subsidies, here the question is how actual use of the subsidy affects labor supply behavior. Second, the subsidy considered here is a universal subsidy—public school—rather than a subsidy available only to certain families, for example, those that are low-income. At the end of the paper, I also examine the use of targeted, means-tested programs like public prekindergarten, but the focus of this study is largely on the almost universally available kindergarten as a childcare subsidy. Third, the years the other authors study (except Fitzpatrick 2010) precede the new evidence that women in the United States are not responsive to wages. Finally, as with Fitzpatrick (2010), 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 comparison group who very closely match the treatment group. This is potentially an improvement over methodologies relying on quarter-of-birth and including children born throughout the year, who may be more likely to differ from one another in ways not captured by observable characteristics.

Using a child's eligibility for public kindergarten in a regression discontinuity instrumental variables framework, I estimate how public school enrollment affects maternal labor supply. I find that enrollment increases only the employment of single mothers without additional young children. The estimated effect on other labor supply measures for these women is also positive, but cannot be statistically distinguished from zero. For all other groups of mothers, the results are noisy and change signs frequently depending on the specification used.

These results are strikingly different from those reported earlier in previous work. I investigate possible reasons for these differences, focusing on the similar setting of public schooling but the earlier period of the 1980s (Gelabch 2002). I find no evidence that the differences in methodology can be held responsible the different results, though an exact comparison is difficult to make because of data limitations in the 1980 Decennial Census. If there are heterogeneous treatment effects of school enrollment on maternal labor supply, instrumental variables strategies like those used here produce estimates of the local average treatment effect. Descriptive evidence on the patterns of labor supply and other characteristics of mothers of young children in the two time periods suggest changes over the period from 1980 to 2000 that are consistent with an increase in the proportion of mothers who are inframarginal to the subsidy or who face different budget constraints because of increases in educational attainment or changes in household composition.

I start in Section II by briefly describing the data I use in the study. In Section III, I detail the results using a regression discontinuity instrumental variables framework to estimate how public school enrollment affects maternal labor supply in 2000. In Section IV, I place the results in the context, both methodologically and historically, with those in previous studies. Section V concludes.

II. A Brief Overview of the Decennial Census Data

For the analysis, I use data from the 2000 Restricted Access Decennial Census Long Form survey, which contains information about timing of birth, school enrollment, labor supply and family characteristics. This restricted-access data includes the entire Decennial Census Long Form sample and is generally a one-in-six sample of the U.S. population.[2]

A. Sample

In order to make this study analogous to previous work, I restrict the samples along several dimensions. In the analysis of the behavior of mothers of five-year-olds, I include only those mothers whose own singleton children were five years old at the time of the survey.[3] Additionally, I have dropped mothers younger than 16 and older than 50 years of age, as well as mothers with more than nine children.

The price of childcare likely varies with the number of young children in a home. On average, the resources available to pay for childcare will depend on whether or not a mother is married. While married families might have access to two incomes, state and federal welfare programs usually provide more assistance to single mothers. For these reasons, when I report estimates below, the samples are divided based on a mother's marital status and on whether she has other children younger than the five-year-old in her home. I define a mother to be married if she is not single, separated, divorced, or widowed.[4] Having additional children younger than five means the mother reports that another of her own children lives in the home and is less than five years of age.

Table 1 presents information about the average characteristics of mothers of five-year-olds in the sample (Columns 2 and 4). The table reports these descriptive statistics separately by marital status and presence of younger children. In general, the demographic characteristics are as expected for the sample of young women with children. In addition, single women have less educational attainment and are more likely than their married counterparts to be minorities. Household composition of single mothers is also slightly different than that of married mothers. For example, as might be expected, married mothers have more additional adults living in the home than their single counterparts.

2. The Census adjusts its sampling rates based on the density of geographic areas, but reports the overall sampling rate to be 1 in 6 persons.
3. The reference point for the Census is April 1 of the year in which it is given. This means that the quarters of birth include Q2 of the year six years before the Census through Q1 of the year five years before the Census. In other words, the children in the sample were born between April 1, 1994 and March 31, 1995. Measurement error in either the child's date-of-birth or enrollment status could bias the estimates, so I also remove any mothers whose children had allocated responses to these questions.
4. Note that this classification of mothers as either single or married is slightly different than that used by Gelbach (2002), who used the variable "household type" variable to define any mothers who were "heads of households without husbands present" as single. The slight difference in definition means that I do not exactly replicate Gelbach's samples or results, but the estimates and resulting conclusions are quite similar using either definition.

Table 1
Average Demographic Characteristics, Mothers of Five-year-olds

	1980	2000	1980	2000
	Single Mothers		Married Mothers	
	Youngest Child is Five Years Old			
Age	30.3	32.23	32.3	35.33
Central city	0.32	0.12	0.16	0.09
White	0.67	0.56	0.89	0.78
Number of children				
Age 0 to 5	1	1	1	1
Age 6 to 12	0.79	0.63	0.96	0.8
Age 13 to 17	0.27	0.27	0.30	0.27
Age 18 +	0.06	0.05	0.07	0.04
Number of household members				
Age 0 to 17	0.06	0.08	0.03	0.13
Age 18 +	0.20	0.31	1.06	1.11
HSD	0.43	0.31	0.48	0.25
Some college	0.18	0.38	0.18	0.34
BA degree	0.04	0.09	0.09	0.2
Grad degree	0.03	0.03	0.05	0.08
N	10,928	43,199	53,300	148,264
	Youngest Child is Younger Than Five			
Age	27.2	28.11	29.0	32.11
Central city	0.38	0.14	0.16	0.1
White	0.56	0.46	0.89	0.8
Number of children				
Age 0 to 5	2.27	2.24	2.24	2.2
Age 6 to 12	0.63	0.54	0.51	0.42
Age 13 to 17	0.13	0.13	0.07	0.08
Age 18 +	0.02	0.02	0.01	0.01
Number of household members				
Age 0 to 17	0.06	0.07	0.02	0.08
Age 18 +	0.18	0.35	1.06	1.11
HSD	0.39	0.33	0.44	0.22
Some college	0.12	0.29	0.19	0.32
BA degree	0.02	0.05	0.10	0.24
Grad degree	0.01	0.02	0.05	0.09
N	6,878	23,066	52,286	123,354

Notes: Based on the author's calculations using the 1980 Census PUMS and the 2000 Restricted Access Long Form Data. The presented statistics are mean values in the sample of mothers of five-year-olds (see text for sample construction). The education variables represent percents of the population with each amount of education, where the omitted category is those who did not finish high school.

B. Labor Supply Variables

The Census data contain several measures of maternal labor supply, including two measures of employment. Women are "currently employed" if they report working in the week before responding to the survey (approximately April 1, 2000). Women were "employed last year" if they worked at any point in the calendar year prior to the survey (1999). Note that the public school subsidy provides care from approximately September of 1999 to June of 2000 (depending on the exact timing of the school calendar). Therefore the subsidy may have had an effect on labor supply in either the previous year or at the time of the survey. I examine both outcomes because there may be a lag in the ability of women to find employment, which would lead to there being an impact on employment at the time of the survey, but not in the previous year.

The Census also measures employment on the intensive margin by collecting information about hours and weeks worked. In 2000, the Census asked respondents how many weeks they worked in 1999 and the number of hours they usually worked per week in 1999. Finally, from responses about individuals' income in the year before the survey I create two measures. First, wage and salary income is the reported income from "wages, salary, commissions, bonuses or tips from all jobs." Second, a dummy variable for public assistance receipt equals one if a mother reports "any public assistance or welfare payments from the state or local welfare office" and zero otherwise.

Information about the labor supply patterns of these mothers of five-year-olds is presented in Table 2. At least half of all mothers report working at the time of the survey and more than 75 percent report working at some point in 1999. On average, women without other young children in the home worked more hours, more weeks and earned higher wages than their counterparts who had additional children younger than five. Mothers with additional young children were twice as likely as those without additional young children to receive welfare income. Also, single women were at least nine times more likely than their married counterparts to have received welfare income in 1999.

C. Age Eligibility and School Enrollment

To the data from the 2000 Census, I add information about exact ages for kindergarten eligibility in each state over the period.[5] Using this information, I create a dummy variable, *cutoff*, equal to one if a child was born before the kindergarten cutoff date in his/her state of residence and zero otherwise. Observations for children in states who did not set the entrance age for kindergarten (either because they left it to the local education authority or because they did not have state-wide laws governing kindergarten) are dropped.

I define a child as enrolled if the child was reported to be enrolled in public school, no matter what the recorded grade level. Using this definition, more than 80

5. Specifically, I use kindergarten cutoffs reported by the Education Commission of the states for 1999 for 2000. These generally match the dates reported in Elder and Lubotsky (2009). See Appendix A Table 1.

Table 2
Average Outcome Variables, Mothers of Five-year-olds

	1980	2000	1980	2000
	Single Mothers		Married Mothers	
	Youngest Child Is Five Years Old			
Current employment	0.591	0.680	0.484	0.604
	(0.492)	(0.465)	(0.500)	(0.489)
Employment in previous year	0.701	0.862	0.578	0.722
	(0.458)	(0.345)	(0.494)	(0.448)
Weeks worked in previous year	28.41	38.15	21.72	31.49
	(22.74)	(19.60)	(22.49)	(22.82)
Usual hours per week in previous year	25.7	32.9	19.1	25.3
	(19.1)	(16.0)	(18.9)	(18.8)
Wage and salary income in previous year	11,902	17,808	8,085	16,296
	(13,458)	(26,727)	(12,093)	(25,822)
Welfare receipt in previous year	0.343	0.149	0.022	0.016
	(0.475)	(0.356)	(0.146)	(0.126)
Kindergarten enrollment	0.793	0.866	0.764	0.851
	(0.405)	(0.340)	(0.425)	(0.356)
	Youngest Child Is Younger Than Five Years Old			
Current employment	0.343	0.539	0.328	0.482
	(0.475)	(0.498)	(0.469)	(0.500)
Employment in previous year	0.489	0.762	0.438	0.607
	(0.500)	(0.426)	(0.496)	(0.488)
Weeks worked in previous year	15.99	29.84	14.25	24.89
	(20.63)	(21.81)	(19.91)	(23.36)
Usual hours per week in previous year	17.4	28.4	13.8	20.3
	(19.7)	(18.2)	(18.0)	(19.3)
Wage and salary income in previous year	5,793	11,721	4,950	12,695
	(10,283)	(22,680)	(9,970)	(24,307)
Welfare receipt in previous year	0.549	0.280	0.030	0.024
	(0.498)	(0.449)	(0.169)	(0.152)
Kindergarten enrollment	0.734	0.827	0.732	0.835
	(0.442)	(0.378)	(0.443)	(0.371)

Notes: Based on the author's calculations using the 1980 Census PUMS and the 2000 Restricted Access Long Form Data. Standard deviations are in parentheses. Population weights are used when available. 1980 dollars have been inflated to 2000 dollars.

percent of five-year-olds were enrolled in public school in 2000. This definition results in a larger fraction of five-year-olds being enrolled than if I were to have only included children whose parents responded that they were enrolled in kindergarten per se.

III. Children's Age, Age Cutoffs, and Kindergarten Enrollment in 2000

The willingness of parents to enroll their children in public kindergarten likely varies with the developmental readiness of their child and the family's resources to care for the child if not enrolled. It is the ability of parents to choose the age at which their children start kindergarten that poses problems when attempting to estimate the effects of enrollment on maternal labor supply. Ordinary least squares estimates of the effect of enrollment on labor supply will be biased if the parents who enroll their children in school are those parents who do so because of the mother's desire to participate in the labor force.

More precisely, consider estimating the following relationship, $Y_i = \phi + \beta D_i + \gamma X_i + \varepsilon_i$, where Y_i is a labor force outcome for woman i and D_i is a dummy variable corresponding to her child's enrollment in public kindergarten. X_i represents a vector of observable characteristics of the mother, including fixed effects for her state of residence, assumed to be uncorrelated with the error term. Correlation of D_i and ε_i (caused by selection) leads to biased estimates of β.

One way around this potential selection bias is to use an instrument correlated with enrollment, but not correlated with a mother's labor supply through any mechanism other than its relationship to enrollment. The age at which children are allowed to enter kindergarten in the United States varies from state to state and is arguably such an instrument. The states' use of these eligibility cutoffs for kindergarten creates an artificial difference between children born the day of the cutoff and those born just one day later. The former are eligible for public kindergarten enrollment in the year they turn five, while the latter must wait until the following year to enroll in public kindergarten.

The kindergarten eligibility cutoffs present an ideal setting to use a regression discontinuity instrumental variables framework (RDIV, also known as a fuzzy regression discontinuity design) to estimate the effects of public school enrollment on maternal labor supply. More formally, I estimate the following first and second stage equations

$$(1) \quad D_i = \alpha + \rho X_i + \sum_{j=1}^{n} \pi_j Days_i^j + \delta Cutoff_i + \sum_{j=1}^{n} \lambda_j Days_i^j cutoff_i + \upsilon_i$$

and

$$(2) \quad Y_i = \phi + \beta \hat{D}_i + \gamma X_i + \sum_{j=1}^{n} \kappa_j Days_i^j + \sum_{j=1}^{n} \varphi_j Days_i^j cutoff_i + \upsilon_i.$$

Both equations include a polynomial, here a linear term, in the age of the child relative to the cutoff date (*Days*) to capture differences in the enrollment and labor supply patterns of mothers with children of different ages.[6] The instrument excluded from the second stage is the dummy variable $Cutoff_i$, which takes on a value of one if the five-year-old of woman i was eligible for public kindergarten in the year he/she turned five and zero otherwise. This instrument is interacted with the relative age polynomial, as is traditional with RDIV. The underlying assumption is that there is nothing driving any discontinuous changes in maternal labor supply for mothers of children born just before and after the kindergarten eligibility cutoff date other than the differences in the children's kindergarten eligibility.

Tables 3 and 4 present the results for single and married mothers, respectively. In each table, the top panel contains the estimated effects of kindergarten enrollment for mothers without additional younger children and the bottom panel for those mothers with another child younger than five. Column 1 presents the coefficient estimates of β using the samples of mothers of five-year-olds whose children are born at any point during the year.

The only statistically significant coefficient in Column 1 of Tables 3 and 4 is for the estimated effect of public school enrollment on the probability of a single mother without younger children being employed at the time of the survey. The coefficient suggests that these mothers are 12.2 percentage points more likely to be employed in the spring of 2000 than mothers of children who are not enrolled in public school. More generally, most of the coefficient estimates in Column 1 are positive, suggesting that public school enrollment increases maternal labor supply. However, the estimates are not precise enough to rule out the conclusion that public school had no effect on maternal labor supply in 2000.[7]

The exclusion restriction underlying the use of the RDIV strategy is that the kindergarten eligibility associated with a child being born before the cutoff date is unassociated with maternal labor supply. But being born before the cutoff date is highly correlated with a child's age. The oldest children in the sample are those who are eligible for public kindergarten, while the youngest are those who are ineligible. This could be problematic as a child's age, either calendar or relative to the other members of her class, may influence a mother's decisions about labor supply. Also, research has shown that women with children born in different seasons of the year, say June and December, have different observable characteristics (Bound and Jaeger 2000; Lam and Miron 1991; Bobak and Gjonca 2001; Buckles and Hungerman 2010). If mothers of children born at different times of the year have different patterns of labor supply because of their underlying characteristics, it could confound my estimates of the relationship between public school enrollment and labor supply.

6. The optimal size of the polynomial depends on the shape of the data. In what follows, I present results using a first degree polynomial. The results are usually similar with other polynomial sizes. I make note of the occasions when they differ.

7. A concern may be that the linear polynomial in the child's age may not adequately fit the data, thereby leading to spurious inconclusive results. I have compared the estimates using higher order polynomials and they too find statistically insignificant relationships between public school enrollment and maternal labor supply.

Table 3
RDIV Estimates of the Effects of Public Enrollment on Maternal Labor Supply, Single Mothers of Five-year-olds in 2000, Various Sample Widths

	1	2	3	4	5
	With No Younger Children				
Current employment	**0.122**	**0.153**	**0.192**	0.158	−0.092
	(0.048)	(0.054)	(0.091)	(0.111)	(0.186)
Employment in prior year	0.055	0.092	0.017	0.070	0.110
	(0.035)	(0.040)	(0.056)	(0.060)	(0.074)
Weeks worked in prior year	2.65	2.37	−2.54	−5.66	−7.66
	(1.98)	(2.41)	(3.51)	(4.04)	(4.17)
Usual hours per week in prior year	3.01	3.43	0.89	1.23	9.14
	(1.64)	(2.01)	(3.03)	(3.67)	(5.29)
Wage and salary income, prior year	278	561	393	965	3520
	(2,807)	(3,160)	(4,449)	(5,547)	(8,697)
Welfare receipt in prior year	0.003	−0.029	0.015	0.031	0.047
	(0.036)	(0.047)	(0.076)	(0.090)	(0.147)
First stage estimate	**0.232**	**0.226**	**0.202**	**0.223**	**0.209**
	(0.007)	(0.012)	(0.016)	(0.022)	(0.021)
$F-$statistic	974	365	152	103	97
Number of observations	43,199	23,625	12,068	7,282	2,536
	With Younger Children				
Current employment	0.002	0.047	0.172	**0.359**	**0.513**
	(0.072)	(0.077)	(0.108)	(0.127)	(0.199)
Employment in prior year	−0.035	−0.007	−0.027	0.158	0.267
	(0.062)	(0.076)	(0.122)	(0.146)	(0.250)
Weeks worked in prior year	−1.10	0.49	−3.95	4.86	6.24
	(3.08)	(3.20)	(4.80)	(5.44)	(9.55)
Usual hours per week in prior year	−2.14	−0.21	3.55	**13.58**	12.51
	(2.65)	(3.21)	(5.08)	(6.18)	(9.13)
Wage and salary income, prior year	1,467	2,974	5,380	6,354	3,240
	(2,616)	(2,797)	(4,008)	(4,550)	(6,163)
Welfare receipt in prior year	−0.019	−0.027	0.002	0.058	0.186
	(0.065)	(0.077)	(0.122)	(0.126)	(0.175)
First stage estimate	**0.208**	**0.220**	**0.193**	**0.197**	**0.186**
	(0.010)	(0.016)	(0.023)	(0.030)	(0.052)
$F-$statistic	452	180	68	43	12
Number of observations	23,066	12,638	6,651	4,007	1,382
Width of Sample In Days	Full	100	50	30	10

Notes: Based on the author's calculations using the 2000 Restricted Access Long Form Data. Standard errors are in parentheses. Mothers in each sample are those born within the number of days indicated in the last row. Both stages include a linear polynomial in the child's age relative to the cutoff data interacted with the cutoff date. Regressions include state fixed effects and the set of controls described in the text. Numbers in bold are significant at the 5 percent level or lower. $F-$statistics test the null in which the eligibility for public kindergarten has no effect on public kindergarten enrollment.

Table 4
RDIV Estimates of the Effects of Public Enrollment on Maternal Labor Supply, Married Mothers of Five-year-olds in 2000, Various Sample Widths

	1	2	3	4	5
	With No Younger Children				
Current Employment	0.027	−0.002	0.061	0.053	0.026
	(0.023)	(0.085)	(0.049)	(0.037)	(0.026)
Employment in prior year	0.013	−0.108	0.055	0.061	0.025
	(0.022)	(0.068)	(0.039)	(0.032)	(0.023)
Weeks worked in prior year	0.66	−0.12	2.84	3.09	1.12
	(1.09)	(4.36)	(2.15)	(1.77)	(1.24)
Usual hours per week in prior year	0.02	−2.51	1.93	2.15	0.85
	(0.91)	(4.03)	(1.83)	(1.53)	(1.06)
Wage and salary income, prior year	−1,371	−2,041	868	1,209	−1,232
	(1,163)	(4,329)	(3,035)	(2,305)	(1,491)
Welfare receipt in prior year	0.002	−0.012	−0.003	0.014	0.010
	(0.006)	(0.020)	(0.013)	(0.010)	(0.007)
First Stage Estimate	**0.285**	**0.227**	**0.254**	**0.249**	**0.271**
	(0.004)	(0.039)	(0.016)	(0.012)	(0.008)
$F-$statistic	4,309	36	254	450	1,227
Number of observations	148,264	8,636	25,060	41,403	81,178
	With Younger Children				
Current employment	0.014	0.033	0.001	0.053	−0.002
	(0.026)	(0.102)	(0.067)	(0.049)	(0.032)
Employment in prior year	0.032	0.044	0.011	0.049	−0.008
	(0.026)	(0.092)	(0.069)	(0.051)	(0.032)
Weeks worked in prior year	1.16	−1.70	−2.95	0.53	−0.91
	(1.22)	(3.93)	(2.68)	(2.12)	(1.36)
Usual hours per week in prior year	2.22	2.94	1.19	1.95	0.50
	(1.03)	(3.87)	(2.60)	(1.94)	(1.22)
Wage and salary income, prior year	2,617	−5,030	−6,788	−1,237	−782
	(1,284)	(5,130)	(2,917)	(2,392)	(1,426)
Welfare receipt in prior year	0.016	0.025	0.005	0.019	0.011
	(0.009)	(0.035)	(0.022)	(0.018)	(0.012)
First stage estimate	**0.289**	**0.217**	**0.231**	**0.235**	**0.266**
	(0.004)	(0.019)	(0.014)	(0.011)	(0.009)
$F-$statistic	3,579	36	254	450	1,228
Number of observations	123,354	7,125	20.574	34,127	67,441
Width of sample in days	Full	10	30	50	100

Notes: Based on the author's calculations using the 2000 Restricted Access Long Form Data. Standard errors are in parentheses. Mothers in each sample are those born within the number of days indicated in the last row. Both stages include a linear polynomial in the child's age relative to the cutoff data interacted with the cutoff date. Regressions include state fixed effects and the set of controls described in the text. Numbers in bold are significant at the 5 percent level or lower. $F-$statistics test the null in which the eligibility for public kindergarten has no effect on public kindergarten enrollment.

To be sure these differences in child's age are not biasing my estimates of the relationship between public school enrollment and maternal labor supply, I now examine how the results change as I narrow the width of the sample around the cutoff date for kindergarten. Narrowing the width of the sample around the cutoff helps to create "treatment" and "comparison" groups that are similar to one another, other than the difference in eligibility.[8] It is much more difficult to argue that women whose children were born within just weeks of one another display the same differences that those born six months apart may.[9] The samples used thus far in this paper have included children born between April 1, 1994 and March 31, 1995. The use of an entire calendar year sample makes it likely that there are systematic differences between mothers of eligible and ineligible children confounding my estimates of the effects of kindergarten enrollment on maternal labor supply.

In order to see whether this is the case, I examine the effect of the sample window width on the size and statistical significance of the estimates. The remaining columns of Tables 3 and 4 present these coefficient estimates for single and married mothers, respectively. Going across the tables from Column 2 to Column 5, the sample of mothers used in the estimation becomes more and more limited. In Column 2, the sample includes mothers of children born within 100 days of the cutoff date; in Column 5, the sample includes only those mothers whose children were born within 10 days of the cutoff date.

In general, the results suggest there is not much of a relationship between public kindergarten enrollment and maternal labor supply. By and large, the results are statistically insignificant and quite frequently the estimates change signs and magnitude, jumping around as the width of the sample window changes. One exception is that public school enrollment continues to have a generally positive effect on the employment of single mothers. For single mothers without younger children, the estimated RDIV relationship between public school enrollment and being employed last week is on the order of a 15 to 20 percentage point increase for all but the most narrow sample widths, which may not have enough power to identify an effect. Additionally, many of the estimates are statistically significant. For these women, enrollment of their five-year-olds in public school also increases the probability they were employed in the year before the survey by about five to ten percentage points, depending on the width of the sample, but the estimates are not statistically significant. Also, the estimated relationship between a child's public school enrollment and current employment is positive for single mothers with younger children. However, the estimate is not consistent in either magnitude or statistical significance, making me cautious in placing too much weight on the result.[10]

8. In practice, there are two methods used with the RDIV framework to try to control for the effect of the running variable (in this case the child's age): narrowing the width of the sample around the cutoff date and including additional polynomial terms in the estimation.

9. In fact, comparison of mean characteristics of women whose children are born within 30 days of the cutoff shows no systematic differences. Results are available from the author upon request. Also see Fitzpatrick (2010) and McCrary and Royer (forthcoming) for examples where mothers and children born in a narrow window around the cutoff date for kindergarten have similar observable characteristics.

10. More precisely, the coefficient estimates are large and statistically significant when the sample is at its most limited, but statistically indistinguishable from zero when the sample includes a wider range of birth dates. This could be due to the fact that the linear control for children's age is not adequately capturing

IV. Placing These Results in Context

The finding of a limited relationship between public kindergarten enrollment and maternal labor supply when using the RDIV methods in 2000 is in contrast with previous findings. Most notably, using Decennial Census data from 1980, Gelbach (2002) found that public school enrollment of five-year-olds increased the labor supply of nearly all mothers (save those who were single and had additional younger children in the home). Additionally, the increases in labor supply occurred across a host of labor supply outcomes, not just employment. Though it is usually the case that the confidence intervals on the RDIV estimates in 2000 are wide enough to include the estimates from Gelbach (2002), the opposite is not true. This, plus the distinctly different patterns of estimates (that is, not all of the coefficients in this study are positive) across studies warrants closer examination.

Why might such differences exist between the findings of the two studies? In this section, I investigate two potential causes. First, I discuss the role of methodological differences. Second, I detail how the labor supply patterns of and the policy environment faced by mothers with young children changed between 1980 and 2000. Both potentially have an important role in explaining the differences between the findings of this study and those of Gelbach (2002).

A. Methodological Differences

The main methodological difference between this study and Gelbach (2002) is the choice of an instrument for identifying the effect of public kindergarten enrollment on maternal labor supply. As detailed above, I employ an RDIV framework, where the excluded instrument is a measure of a child's eligibility for public kindergarten enrollment in the year the child turns five. Gelbach (2002) uses a strategy that is similar in spirit, but slightly different in implementation. Specifically, the excluded instruments in his study are a set of dummy variables representing a child's quarter-of-birth. With this quarter-of-birth instrumental variables (QOBIV) strategy, the exclusion restriction requires that a child's age not be related to the labor supply of mothers other than through its relationship to the child's enrollment in school. The QOBIV therefore requires a more restrictive assumption than the RDIV because the latter allows there to be either a direct or an indirect relationship between the child's age and maternal labor supply.

Whether the more restrictive assumption matters in practice is an empirical question. I replicate the QOBIV strategy using the 2000 Decennial Census data in Tables 5 and 6. For ease of comparison, I also present the QOBIV estimates in 1980 in the tables and present the average labor supply outcomes for mothers who enroll their

the true relationship between age and maternal labor supply and renders the estimated effect of public preschool to be negligible at wide sample widths. However, the lack of a relationship between public preschool enrollment and employment of these mothers remains when the sample includes the children born throughout the year and I include higher order polynomials in the child's relative age. Because the data demands of RDIV are high, I choose not to put much weight on the statistically significant estimate of public school enrollment on the labor supply of these mothers seen at narrow widths.

Table 5
Effects of Public Kindergarten Enrollment on Maternal Labor Supply, Single Mothers of Five-year-olds, QOB Dummies as Instruments

Outcome	1980 Average Using School	1980 Estimate	1980 Percent Change	2000 Average Using School	2000 Estimate	2000 Percent Change
With No Younger Children						
Current employment	0.623	**0.057** (0.021)	10%	0.834	−0.005 (0.017)	−1%
Employment in prior year	0.724	**0.042** (0.020)	6%	0.674	−0.002 (0.013)	0%
Weeks worked in prior year	29.8	**3.64** (0.97)	14%	36.4	−0.24 (0.71)	−1%
Usual hours per week in prior year	26.6	**2.20** (0.82)	9%	31.67	−0.04 (0.59)	0%
Wage and salary Income, prior year	12,903	**2,223** (583)	21%	16,127	−1335 (991)	−9%
Welfare receipt in prior year	0.317	−**0.048** (0.020)	−13%	0.170	0.006 (0.013)	4%
Number of observations	10,928			51,382		

	With Younger Children					
Current employment	0.357	−0.019 (0.025)	−5%	0.522	−0.025 (0.024)	4%
Employment in prior year	0.506	0.022 (0.026)	5%	0.742	0.021 (0.020)	3%
Weeks worked in prior year	16.6	0.25 (1.07)	2%	28.4	0.49 (1.02)	2%
Usual hours per week in prior year	18.01	0.44 (1.03)	3%	27.42	0.63 (0.88)	2%
Wage and salary Income, prior year	6,106	−176 (531)	−3%	10,913	1,280 (844)	13%
Welfare receipt in prior year	0.543	0.009 (0.026)	2%	0.294	0.002 (0.022)	1%
Number of observations	6,878			27,178		

Notes: Based on the author's calculations using the 1980 Census PUMS and the 2000 Restricted Access Long Form Data. Estimates are from a 2SLS estimation of the outcome indicated on public kindergarten enrollment using quarter-of-birth dummy variables as the excluded instruments. Standard errors are in parentheses. Population weights are used when available. Regressions include state fixed effects and the set of controls described in the text. The "Average Using School" column presents the average of each outcome for the relevant sample of mothers whose children are enrolled in public school. Percent changes are estimated changes in the outcomes due to public school enrollment calculated as described in the text. Numbers in bold are significant at the 5 percent level or lower.

Table 6
Effects of Public Kindergarten Enrollment on Maternal Labor Supply, Married Mothers of Five-year-olds, QOB Dummies as Instruments

	1980			2000		
Outcome	Average Using School	Estimate	Percent Change	Average Using School	Estimate	Percent Change
			With No Younger Children			
Current employment	0.513	**0.047** (0.008)	10%	0.606	**0.050** (0.008)	9%
Employment in prior year	0.601	**0.044** (0.008)	8%	0.714	**0.052** (0.007)	8%
Weeks worked in prior year	22.83	**1.55** (0.37)	7%	30.73	**2.13** (0.36)	7%
Usual hours per week in prior year	19.9	**1.57** (0.31)	9%	25.2	**1.90** (0.30)	8%
Wage and salary Income, prior year	14,392	**402** (195)	3%	15,121	**1,249** (423)	9%
Welfare receipt in prior year	0.02	0.001 (0.002)	4%	0.020	−0.003 (0.002)	17%
Number of observations	53,300			181,718		

			With Younger Children			
Current employment	0.347	**0.039**	13%	0.477	**0.050**	11%
		(0.008)			(0.008)	
Employment in prior year	0.454	**0.031**	7%	0.604	**0.054**	10%
		(0.008)			(0.008)	
Weeks worked in prior year	14.96	**1.50**	11%	24.60	**2.94**	14%
		(0.32)			(0.38)	
Usual hours per week in prior year	14.19	**0.99**	8%	20.6	**2.49**	14%
		(0.29)			(0.32)	
Wage and salary Income, prior year	5,361	**429**	9%	11,666	**1,678**	17%
		(159)			(417)	
Welfare receipt in prior year	0.028	0.000	1%	0.029	−0.003	10%
		(0.003)			(0.003)	
Number of observations	52,286			151,299		

Notes: Based on the author's calculations using the 1980 Census PUMS and the 2000 Restricted Access Long Form Data. Estimates are from a 2SLS estimation of the outcome indicated on public kindergarten enrollment using quarter-of-birth dummy variables as the excluded instruments. Standard errors are in parentheses. Population weights are used when available. Regressions include state fixed effects and the set of controls described in the text. The "Average Using School" column presents the average of each outcome for the relevant sample of mothers whose children are enrolled in public school. Percent changes are estimated changes in the outcomes due to public school enrollment calculated as described in the text. Numbers in bold are significant at the 5 percent level or lower.

children in school.[11] There are two general conclusions to be made from the coefficient estimates presented in the tables.

First, the 2000 QOBIV framework produces quite different estimates of the relationship between public school enrollment and the labor supply of mothers of five-year-olds than the RDIV strategy. The QOBIV estimates would lead one to the conclusion that, in 2000, public school enrollment had little effect on the labor supply of single women and increased the labor supply of married women. However, the RDIV results of the last section led to the conclusion that public school enrollment increased the labor supply of single mothers without having a statistically discernable effect on the labor supply of married mothers. (Note that the results for single mothers and married mothers change in opposite directions when shifting from the RDIV to the QOBIV, an issue I will return to later.)

As discussed, the main difference between the RDIV and the QOBIV is the underlying assumption about the relationship between a child's age and maternal labor supply. Since the RDIV includes controls in the second stage of estimation for the child's age (relative to the cutoff date), it allows for there to be a relationship. The QOBIV, however, does not include a control for age in the second stage. The assumptions underlying the use of the QOBIV strategy, therefore, could be inviolate if there is a direct relationship between a child's age and maternal labor supply separate from the school enrollment effect. Alternatively, if mothers of children born in different quarters behave differently with respect to labor supply for reasons not directly related to the age of their children, this would also violate the assumptions underlying the QOBIV strategy.

I start by investigating the latter possibility—that mothers of children born at different points of the year are different in ways that may affect their labor supply behavior. To do so, I compare the observable characteristics of mothers of five-year-olds born in each quarter. Mean characteristics for single mothers without additional young children in the home are presented in the top panel of Table 7. In the last two columns, I present the F-statistics and the corresponding p-values for a test of equivalence of the characteristics across the samples of mothers born in different quarters. This test was also performed by Gelbach (2002), making it possible to compare the suitability of the QOBIV strategy in the two time periods.

Using the 2000 data, I am much more likely to reject the null hypothesis of equivalence across the quarters of birth than was Gelbach (2002) using the 1980 data. Although Gelbach reported that only the age of single mothers without younger children and the age composition of their children differed across the quarter-of-birth of the five-year-old, I also find differences in the household composition of these women. The women of children born earlier in the year are less likely to have another adult in the home, which might serve to make the QOBIV results more negative than the RDIV. (This would be true if mothers without additional help find it harder to move into the labor market because of their caretaking responsibilities.)

11. Note that these estimates are not exact reproductions of those in Gelbach (2002). Instead, as described earlier, there is a slight difference in the categorization of women as single or married that causes slight differences in the estimates. These differences are inconsequential to the interpretation of the results or the conclusions of the study.

The second panel of Table 7 contains the average characteristics of married mothers whose children are born in each quarter of the sample. As the last two columns show, these married mothers of children born at different times of the year are even more likely than their single counterparts to be different from one another. For example, married mothers of children born in the first two quarters of the sample have higher educational attainment levels than their counterparts whose children were born later in the sample period. If mothers with more educational attainment are more attached to the labor force, it could help to explain why the QOBIV results for these women are positive while the RDIV results are negative.

In the 1980 data, only single mothers with additional young children in the home differed in educational attainment and race. These cross-QOB differences (coupled with results from overidentification tests and specification checks) led Gelbach to avoid making conclusions regarding the labor supply of these women using the QOBIV strategy. Were I to apply the same standards to the 2000 data, I would abandon use of the QOBIV strategy for *all* mothers. Of course, if the only differences in these mothers were in their observable characteristics, the problem would be resolved by including a set of controls for these characteristics. The concern, however, is that there may be other unobservable characteristics that also vary across mothers of children born at different times of the year but that I cannot adequately capture using available data.

Next, I investigate the question of whether there is a direct relationship between a child's age and her mother's labor supply by examining the coefficients on the controls for age used in the RDIV estimation. Table 8 contains coefficient estimates for the relationship between a mother's labor supply and her child's age when the outcome measured is the current employment of single mothers of five-year-olds without younger children in the home. To be clear, each reported coefficient is from the second stage of an IV estimation where the excluded instrument is the measure of eligibility defined earlier.

Columns 1 and 2 of Table 8 present estimates of Equation 2, where age is measured relative to the cutoff date for eligibility (as is standard in RD designs). The first column contains results when the polynomial is linear (the specification used previously) and the second when the polynomial is a cubic. The estimated coefficients on relative age using the linear polynomial are not individually or jointly significant. However, using a cubic polynomial, some of the relative age coefficients are individually statistically significant and a test of joint significance suggests relative age and maternal employment are connected. Results reported in other columns of Table 8 suggest that the calendar age, as measured by a linear (Column 3) or cubic (Column 4) polynomial or by a child's quarter-of-birth (Column 5), does not have a statistically significant relationship with maternal labor supply for these mothers.

More generally, there are six labor supply outcomes for each of four groups of mothers. Across these measures of labor supply, with both linear and cubic polynomials, the relative age coefficients show patterns of being jointly statistically significant for married mothers, both with and without younger children (results not shown, but available upon request). Given that the direction of the bias for these mothers with QOBIV relative to RDIV was positive, it should be no surprise that

Table 7
Average Characteristics by Child's Quarter-of-birth, Mothers of Five-year-olds Without Additional Young Children

	Single Mothers					
	Q294	Q394	Q494	Q195	$F-$Statistic	$P-$value
Age	32.606	32.446	32.063	31.604	44.63	0.00
Central city	0.134	0.135	0.131	0.132	0.32	0.80
White	0.620	0.614	0.610	0.609	0.88	0.45
Own children aged 6 to 12	0.618	0.619	0.627	0.637	1.89	0.13
Own children aged 13 to 17	0.272	0.275	0.256	0.241	6.30	0.00
Own children aged 18 +	0.048	0.052	0.048	0.045	1.46	0.22
Household members aged 0 to 17	0.069	0.068	0.070	0.071	0.89	0.44
Household members aged 18 +	0.289	0.290	0.299	0.305	2.69	0.04
HSD	0.319	0.316	0.321	0.312	0.41	0.75
Some college	0.381	0.383	0.385	0.387	0.49	0.69
BA degree	0.093	0.092	0.088	0.088	0.96	0.41
Grad degree	0.034	0.032	0.028	0.029	0.81	0.49
N	12,916	13,850	12,382	12,247		

	Married Mothers					
	Q294	Q394	Q494	Q195	$F-$Statistic	$P-$value
Age	35.582	35.264	35.065	34.753	145.6	0.00
Central city	0.095	0.099	0.096	0.099	2.32	0.00
White	0.823	0.815	0.805	0.812	8.85	0.00
Own children aged 6 to 12	0.792	0.796	0.802	0.817	10.09	0.00
Own children aged 13 to 17	0.282	0.286	0.270	0.240	55.69	0.00
Own children aged 18 +	0.044	0.042	0.044	0.038	3.34	0.02
Household members aged 0 to 17	0.127	0.129	0.132	0.129	0.48	0.40
Household Members aged 18 +	1.087	1.085	1.099	1.092	7.03	0.00
HSD	0.267	0.273	0.265	0.266	2.83	0.04
Some college	0.359	0.341	0.345	0.340	1.45	0.22
BA degree	0.197	0.189	0.186	0.183	6.60	0.00
Grad degree	0.079	0.073	0.074	0.077	5.37	0.00
N	38,209	39,937	36,692	36,460		

Notes: Based on the author's calculations using the 1980 Census PUMS and the 2000 Restricted Access Long Form Data. The presented statistics are mean values in the sample of mothers of five-year-olds (see text for sample construction). The education variables represent percents of the population with each amount of education, where the omitted category is those who did not finish high school.

Table 8

Estimates of the Relationship Between Child's Age and Maternal Employment, Single Mothers without Additional Young Children

	(1)	(2)		(3)	(4)		(5)
Relative age	0.000	**−0.00124****	Calendar age	−0.002	0.001	94_Q3 Dummy	−0.010
	(0.000)	(0.000)		(2.23)	(0.001)		(0.010)
Relative age squared		**−8.21E-06***	Calendar age squared		0.000	94_Q4 Dummy	−0.010
		(0.000)			(0.000)		(0.010)
Relative age cubed		−1.6E-08	Calendar age cubed		0.000	95_Q1 Dummy	0.000
		(0.000)			(0.000)		(0.010)
Interacted with cutoff			Interacted with cutoff				
Relative age	8.03E-05	**0.00101***	Calendar age	0.000	0.000		
	(0.000)	(0.000)		(0.650)	(1.940)		
Relative age squared		0.00000821	Calendar age squared		0.000		
		(0.000)			(0.330)		
Relative age cubed		1.96E-08	Calendar age cubed		0.000		
		(0.000)			(0.840)		

Notes: Based on the author's calculations using the 2000 Restricted Access Long Form Data. Standard errors are in parentheses. Population weights are used when available. Regressions include state fixed effects and the set of controls described in the text. Each column contains estimates from a separate regression where the dependent variable is a dummy variable measuring whether a mother of a five-year-old was employed at the time of the survey. Relative age measures the child's age in days relative to the cutoff date for public kindergarten in the child's state of residence. Calendar age measures the child's age in days relative to April 1, 1999. Column five contains estimated coefficients on dummy variables representing the child's quarter-of-birth, where the left out category is the group of children born in the second quarter of 1994. Numbers in bold are significant at the 5 percent level or lower.

the coefficient estimates suggest that mothers of older children have greater labor supply (and are less likely to receive welfare).

For single mothers without younger children, on the other hand, the relative age coefficients suggest that mothers of older children have lower levels of labor supply (as seen in Table 8). These coefficients are sometimes individually jointly or statistically different from zero depending on the measure of labor supply used as an outcome, but not as frequently so as the coefficients for married mothers. The estimated relationship between age and maternal labor supply for single mothers without younger children does not bear any similar pattern of direction or statistical significance.

Age of children is associated with maternal labor supply, albeit in varying patterns for single and married mothers. This estimated relationship between age and maternal labor supply may capture either a direct effect of age on maternal labor supply or differences in the characteristics of mothers whose children are born at different times of the year that have indirect effects on the mothers' labor supply. Data presented earlier suggested that mothers of children born at different points in the year differed demonstrably on observable characteristics. Because of this, and because it is likely mothers whose children are born at different times of the year also differ in unobservable characteristics, I hesitate to interpret the age coefficients as being indicative of a direct relationship between a child's age and maternal labor supply behavior.

The RDIV results in 2000 differ greatly from both the QOBIV results in 2000 and the QOBIV results in 1980. Ideally, I would use the RDIV techniques with the 1980 data for comparison. Unfortunately, the lack of exact date of birth information in the 1980 Decennial Long Form data makes a traditional RDIV strategy impossible.[12] Instead, several approximations to the RDIV strategy in 1980 are available using the limited data.[13] However, none are particularly satisfying given the noise introduced by the coarseness of the QOB variable. Given that the QOBIV strategy passed conventional tests in 1980 but did not in 2000, I turn a discussion of how mothers in 1980 and 2000 were different, how the policy environment faced by the women in both periods were different and what effects these differences might have on the relationship between public school enrollment and maternal labor supply.

B. Historical Differences

Recent decades have seen marked change in many life cycle events. This is particularly true of women, who have been increasing their educational attainment,

12. Both the 1980 and 2000 Decennial Census Long Form Surveys ask respondents about their date of birth (and the dates of birth of all other household members). However, exact date of birth information is unavailable in public use files of the data sets. Instead, the Census reports the quarter-of-birth in 1980 and only age in the 2000 public use files. And although use of the exact date of birth information is available in the restricted access version of the 2000 data, this restricted access data was unavailable to Gelbach as he completed his 2002 study. Furthermore, the Census does not make exact date of birth available on the 1980 restricted-use files.

13. For example, one could define eligibility precisely for those residing in states with cutoffs that perfectly align with dates differentiating quarters of the year and probabilistically for those residing in other states. The RDIV could then be implemented controlling for QOB in both stages of the estimation.

increasing their labor force participation, delaying their age at first marriage and reducing their number of children. If these characteristics are related to a change in which mothers are on the margin of having their labor supply affected by the public school childcare subsidy or if they represent changes in the budget constraints of mothers, it may help explain the different relationships between enrollment and maternal labor supply in 1980 and 2000. To illustrate the changing lifecycle patterns for the mothers of five-year-olds specifically, Table 1 contains the mean demographic characteristics of the mothers in my sample in 1980 and 2000. These characteristics are tabulated separately based on whether the mothers were married and whether they had additional young children in the home.

First, the age of mothers increased between 1980 and 2000. Also, mothers of five-year-olds in 2000 are much less likely than those in 1980 to be white and less likely to live in areas designated by the Census Bureau to be central cities. This follows patterns of changing ethnicity and residence location in the country. Because older people have more attachment to the labor force, it may be the case that the labor supply of mothers at the margin is less elastic in 2000 than it was in 1980.

Second, the other members of the households of mothers of five-year-olds in 2000 look much different than in 1980. The number of other children in the home has decreased while the number of other adults in the home has increased. For example, single mothers of five-year-olds who also had younger children in 1980 had on average 0.19 other adults in the home. By 2000, the average number of other adults in the home had nearly doubled to 0.35. Likely this reflects the increasing number of cohabiting couples. More adults in the home mean more potential informal child-care arrangements, which may allow the mother to work even in the absence of preschool subsidies. This may help explain why enrollment has less of an effect in 2000 than in 1980. The fact that there are fewer children in the home may also contribute to the differences between estimates over time.

Third, over the period from 1980 to 2000, the educational attainment of mothers of five-year-olds has increased almost across the board. For example, the percent of married mothers with graduate degrees rose from 5 in 1960 to 9 in 2000 while the percent of married mothers without even a high school diploma fell from 21 to 13 percent over the same period. As women and mothers obtain more education and spend more time in the labor market, we might expect them to have stronger labor force attachment even in the absence of school subsidies.

When pondering the results presented in the previous sections it is important to keep in mind that not only are there changes in the group of mothers of five-year-olds as a whole, but there are also some indications of changes over time within the groups of mothers used for estimation. For example, the change in average age for single mothers is not as large as the change in average age of married mothers over the period. If single women's labor supply is more elastic because they are still young, it could be an explanation for why the labor supply of these women continues to be affected by the public school enrollment of their children.[14]

14. Though these differences could merely reflect the changing patterns of demographics for the population as a whole, it may also be the case that there is some change in which mothers are classified as being in each group. For example, divorce is more common today than it was in 1980. I do control for observable characteristics, so to the extent that changes in the groups of women are correlated with these characteristics, this will not be a problem. However, caution should be taken in direct comparison of the estimates from 1980 and 2000.

These descriptive statistics suggest that the population of mothers of five-year-olds has changed over the past four decades. To the extent that these new demographic patterns represent changes in the preferences of mothers with young children, the effects of school subsidies might be different today than in previous decades. Even if preferences have not shifted, changes in other resources and factors (for example, the number of other adults at home) might mean mothers with young children respond to public school subsidization differently.

As described at the beginning of the manuscript, the public school subsidy can be expected to have either income or substitution effects, or both, depending on the levels of maternal labor supply in the absences of the subsidy. To get an understanding of how many women might be affected by these price and income effects, I present statistics describing maternal labor supply in the two periods (Table 2). The working habits of mothers of five-year-olds have changed greatly in the last several decades. Employment increased across the board for mothers of five-year-olds from 1980 to 2000. For example, in 1980, the employment rate for married mothers with a five-year-old and additional younger children was 33 percent. By 2000 it had increased to 48 percent. Similarly, for single mothers with additional young children, the employment rate at the time of the Census increased from 34 percent to 54 percent over the period. Increases in the probability of employment of mothers without younger children are smaller, but still present.

The group of mothers who are unemployed—the only group for whom the subsidy has pure price effects—has become smaller over time. Increases in labor supply because of public school enrollment, particularly on the extensive margin, are less likely to be seen now than in previous decades. Further, if mothers who are working have stronger labor force attachment in 2000 than in previous years (and those who are not working have stronger attachment to the home), it may dampen the effects of public preschool enrollment on maternal labor supply. This is in part because women with strong labor force attachment may already be in the workplace in advance of their child's eligibility for the subsidy of public kindergarten.

At the same time, the rate of public assistance receipt has decreased over the period from 1980 to 2000. This is particularly true of single mothers. For example, in 1980, 34 percent of single mothers whose youngest child was five report receiving public assistance income. In 2000, the share had dropped to 16 percent. This likely reflects changes to the welfare system over the period, most notably the passage of the Personal Responsibility and Welfare Opportunity Reconciliation Act of 1996, which made it more difficult for unemployed unwed mothers to receive welfare. The inability of mothers to perpetually depend on welfare for income may have forced them into the labor market before their children turned five, therefore limiting the ability of the childcare subsidy provided through public school to affect their labor supply.

C. Mothers of Younger Children

The effects of public school enrollment on the labor supply of mothers of five-year-olds in 2000 do not seem to be as large or as clear as the previous estimates in Gelbach (2002) would have suggested. Perhaps this is because children today are enrolled in school earlier than age five, moving the margin along which public school subsidization has the ability to affect maternal labor supply to mothers of younger children. In fact, preschool enrollment rates of young children in the United States have risen dramatically. In 1980, just 36 percent of four-year-olds were enrolled in some form of nursery or preschool; by 2000, the enrollment of four-year-olds had risen to 60 percent.

In order to investigate whether the labor supply of mothers with younger children is responsive to their children's enrollment in public preschool, I use the RDIV design on samples of mothers of four-year-olds in 2000.[15] With four-year-olds, the age eligibility cutoffs may have slightly different meaning than they did for five-year-olds. Many public programs for four-year-olds, like state prekindergarten programs, use the age eligibility cutoffs to determine eligibility, much like the kindergarten programs do. However, these programs are generally open only to some subset of the population, like low-income children. So, too, with Head Start, which may use age eligibility cutoffs as a way to ration the limited funding it receives to only serve children in the year before they will enter kindergarten. Similarly, if parents wish to enroll their children for just a year of socialization before they enter school, the age eligibility cutoffs will play a role in public school enrollment rates for four-year-olds.[16]

The RDIV results for mothers of four-year-olds are in Tables 9 and 10. Almost none of the second stage estimates of the effects of public preschool enrollment on the labor supply of mothers of four-year-olds are statistically significant. This is despite the fact that the estimates of the relationship between eligibility (as determined by the cutoff for kindergarten enrollment) and public preschool enrollment are statistically significant and F-statistics from the first stage suggest there is no weak instruments problem (except for when using the groups of single mothers with additional young children). Because the standard errors are quite large, I cannot reject the hypothesis that public preschool for four-year-olds has impacts on the order of those previously seen for kindergarten or for other universal preschool subsidies in other countries (Gelbach 2002; Baker, Gruber and Milligan 2008). Interestingly, the results are closest to those in Cascio (2009), which measure the intent-to-treat effects of the introduction of public kindergartens in the Southern United States. However, the instability of most of the estimates as the width of the sample changes slightly leads me to conclude that most of the results are driven largely by noise rather than by any underlying relationship between maternal labor supply and public preschool enrollment of four-year-olds.

V. Conclusion

To summarize, I have shown that the labor supply of single mothers of five-year-olds without additional young children increases as a result of a child's

15. By the same logic, I also can replicate the results using the sample of mothers of three-year-olds. Using these samples, none of the estimates from the second stage are statistically significant. Further, the coefficients on the effects of eligibility on preschool enrollment of three-year-olds are often not statistically significant and the *F*-statistics are relatively small, suggesting that, among this sample, there is a weak instruments problem, which is likely exacerbated by the finite sample size.

16. The age eligibility cutoff dates for state provided preschool programs are generally identical to those used for kindergarten. This is likely because the education administrators want to allow children to pass from the public prekindergarten program directly to the public kindergarten classes.

Table 9
Fuzzy Regression Discontinuity Estimates of the Effects of Public Preschool Enrollment on Maternal Labor Supply, Single Mothers of Four-year-olds in the 2000 Census, Various Sample Widths

	1	2	3	4
	With No Younger Children			
Current employment	0.026	0.036	0.058	0.003
	(0.224)	(0.166)	(0.168)	(0.122)
Employment in prior year	0.361	0.231	0.184	0.069
	(0.190)	(0.124)	(0.119)	(0.085)
Weeks worked in prior year	−3.42	3.22	2.36	1.81
	(13.54)	(7.66)	(7.21)	(4.83)
Usual hours per week in prior year	7.63	5.42	7.28	2.05
	(9.46)	(6.17)	(6.01)	(4.26)
Wage and salary income, prior year	4,387	9,548	8,158	3,957
	(8,518)	(6,771)	(7,008)	(4,937)
Welfare receipt in prior year	−0.067	−0.064	−0.093	−0.009
	(0.226)	(0.153)	(0.127)	(0.084)
First stage estimate	**0.141**	**0.124**	**0.107**	**0.111**
	(0.041)	(0.022)	(0.017)	(0.012)
$F-$statistic	12	31	41	90
Number of observations	2,471	7,291	12,054	23,617
	With Younger Children			
Current employment	−0.955	−0.745	−0.341	−0.495
	(0.774)	(1.126)	(0.346)	(0.358)
Employment in prior year	−0.873	−1.810	−0.607	−0.425
	(0.782)	(1.982)	(0.340)	(0.298)
Weeks worked in prior year	−28.23	−68.53	−26.00	−19.64
	(36.09)	(78.70)	(16.55)	(15.28)
Usual hours per week in prior year	−45.25	−54.67	−19.91	−15.35
	(46.01)	(67.67)	(13.97)	(12.61)
Wage and salary income, prior year	4,299	10,850	−10,030	−8,952
	(24,807)	(41,254)	(15,500)	(13,926)
Welfare receipt in prior year	0.495	0.174	0.108	0.269
	(0.571)	(0.641)	(0.249)	(0.273)
First stage estimate	0.080	0.036	**0.085**	**0.067**
	(0.055)	(0.035)	(0.028)	(0.021)
F-statistic	2	1	9	10
Number of observations	1,052	3,199	5,226	10,138
Width of sample in days	10	30	50	100

Notes: Based on the author's calculations using the 2000 Restricted Access Long Form Data. Standard errors are in parentheses. Population weights are used when available. Regressions include state fixed effects and the set of controls described in the text. Numbers in bold are significant at the 5 percent level or lower. F Statistics are of the test of the null in which the eligibility for public kindergarten has no effect on public kindergarten enrollment.

Table 10

Fuzzy Regression Discontinuity Estimates of the Effects of Public Preschool Enrollment on Maternal Labor Supply, Married Mothers of Four-year-olds in the 2000 Census, Various Sample Widths

	1	2	3	4
	With No Younger Children			
Current employment	0.092	0.108	0.108	0.069
	(0.262)	(0.141)	(0.124)	(0.079)
Employment in prior year	0.212	0.145	0.085	0.012
	(0.191)	(0.108)	(0.101)	(0.068)
Weeks worked in prior year	11.92	5.30	4.15	0.38
	(13.35)	(7.06)	(6.22)	(3.77)
Usual hours per week in prior year	11.06	3.67	2.62	−0.61
	(6.27)	(4.29)	(4.07)	(2.77)
Wage and salary income, prior year	3,212	−2,346	565	2,082
	(11,118)	(6,535)	(5,835)	(3,852)
Welfare receipt in prior year	**−0.103**	−0.049	−0.042	0.005
	(0.043)	(0.031)	(0.028)	(0.018)
First stage estimate	**0.101**	**0.100**	**0.088**	**0.100**
	(0.022)	(0.013)	(0.010)	(0.007)
*F-s*tatistic	20	56	78	226
Number of observations	9,485	26,931	44,676	87,359
	With Younger Children			
Current employment	−0.230	−0.133	−0.202	−0.024
	(0.280)	(0.131)	(0.120)	(0.079)
Employment in prior year	0.125	0.056	0.100	0.120
	(0.269)	(0.153)	(0.133)	(0.092)
Weeks worked in prior year	−6.55	−4.49	−2.54	−0.16
	(13.01)	(6.79)	(5.81)	(4.08)
Usual hours per week in prior year	2.12	−2.20	−2.36	0.07
	(10.25)	(5.80)	(5.00)	(3.53)
Wage and salary income, prior year	5,311	−12,659	−14,862	−7,927
	(15,059)	(8,951)	(7,830)	(5,081)
Welfare receipt in prior year	−0.083	−0.011	−0.010	0.061
	(0.134)	(0.057)	(0.047)	(0.033)
First stage estimate	**0.080**	**0.100**	**0.094**	**0.097**
	(0.030)	(0.017)	(0.013)	(0.009)
F−statistic	7	36	55	108
Number of observation	6,272	18,182	29,893	58,881
Width of sample in days	10	30	50	100

Notes: Based on the author's calculations using the 2000 Restricted Access Long Form Data. Standard errors are in parentheses. Population weights are used when available. Regressions include state fixed effects and the set of controls described in the text. Numbers in bold are significant at the 5 percent level or lower. F Statistics are of the test of the null in which the eligibility for public kindergarten has no effect on public kindergarten enrollment.

enrollment in public school. The labor supply of other mothers of five-year-olds is likely unchanged by their children's public school enrollment. Though statistical tests do not allow me to rule out slight changes, there is no discernable pattern of labor supply shifts for these women as there is with the single mothers without other young children in the home. Similarly, there is no discernable pattern of a labor supply response to public preschool enrollment for mothers of four-year-olds.

The lack of a dramatic labor supply response to the public school enrollment of young children in 2000 is in contrast to results in earlier work, which has concluded that the labor supply of mothers of young children is responsive to childcare subsidization, particularly through the public school system. I rule out methodological differences as the driving force behind the different results presented in this study and the earlier work it matches most closely (Gelbach 2002). Changes in female labor supply behavior, lifecycle event patterns, and the childcare policy environment make the study of this setting potentially quite different from studies covering previous time periods. The differences between the results in this study and previous work highlight the importance of caution when extrapolating results about childcare across time periods or across different populations. More broadly, these results highlight the difficulty traditional childcare subsidy levers (like public school provision or Head Start) may have influencing maternal labor supply. More research is warranted to determine whether the effects of other childcare subsidies, like price subsidies, have also changed in recent decades.

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