Intertemporal Substitution in Maternal Labor Supply: Evidence Using State School Entrance Age Laws*

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Abstract

I propose a new framework to study the intertemporal labor supply hypothesis. I use an exogenous source of variation in maternal net earning opportunities, generated through school entrance age of children, to study intertemporal labor supply behavior. Employing data from the 1980 US Census and the NLSY, I estimate the effect of a one year delay in school attendance on long run maternal labor supply. IV estimates imply that having a 5 year old enrolled in school increases labor supply measures for married women, with no younger children, by between 7 to 34 percent. Further, using a sample of 7 to 10 year olds from the NLSY, I investigate persistence in employment outcomes for a married mother whose child delayed school entry. Results point towards long run intertemporal substitution in labor supply. Rough calculations yield an uncompensated wage elasticity of 0.76 and an intertemporal elasticity of substitution equal to 1.1.

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1 Introduction

The intertemporal labor supply hypothesis states that leisure is easily substitutable across periods. Hence, small and temporary movements in the perceived real wage induce individuals to allocate their time in a way such that periods of high labor supply coincide with periods of high transitory wages. The standard measure of this effect is the intertemporal elasticity of substitution which is predicted to be positive so that individuals work more during periods of high wages holding the marginal utility of wealth constant. A key concern, however, in estimating labor supply elasticities is that it is hard to find temporary and exogenous movements in real wages that can identify movements in labor supply. In the absence of good instruments for wage changes, most studies of intertemporal labor supply treat wages as exogenous.

In this paper, I use an exogenous source of variation in maternal net earning opportunities, generated through school entrance age of children, to study intertemporal labor supply behavior. Changes in maternal labour supply, over time, are determined, to a large extent, by the process of substitution between market and household work associated with bringing up children. In the absence of informal sources of child care, most parents have to incur child care costs in order to become employed. One of the biggest sources of child care subsidies for parents is the availability of schools. A child care subsidy, in the form of free or subsidized kindergarten, increases the likelihood of employment by increasing a mother's net wage at the employment margin. This implies that delaying entry to school may impose an additional year of child care for the mother and consequently a year less in the labor market. The main aim of this paper is to measure the extent to which mothers respond to this additional year of child care costs by substituting current work for future work. The identification strategy relies on comparing labor supply responses, over time, for two groups of women; those whose 5 year olds were enrolled in kindergarten and those who delayed enrollment of their child to age 6.

Uncovering the causal relation between age at enrollment and maternal labor supply is problematic because unobserved characteristics of parents and children are correlated with school entrance age. To deal with the endogeneity of school entrance age and, therefore, to identify the causal effect of delayed school entry, I exploit the exogenous variation in child month of birth and state kindergarten entrance age laws.

This paper adds to the growing body of literature that examine how public preschool availability affects maternal labor supply (Baker, Gruber and Milligan 2008; Berlinski and Galiani 2007; Cascio 2009; Fitzpatrick 2010; Gelbach 2002; Schlosser 2006). The contribution of this paper to this literature is twofold. First, I look at the effect of delayed school entry on long run maternal labor market outcomes as opposed to focusing only on a single period estimate. To my knowledge, this is the first study that explores the dynamic aspect of the school entry age and maternal labor supply relation. Second, I allow for the fact that some mothers may benefit from delaying school enrollment of their children while others may be hurt by it. The estimation strategy provides consistent estimates of the Local Average Treatment Effect (LATE) of entrance age on outcomes even if there is heterogeneity in the entrance age effect (Barua and Lang, 2010).

This paper also contributes to the literature that uses natural experiments and differences-in-differences methods to study intertemporal labor supply. Unlike those studies, I abstract from wage considerations and employ an alternative to directly inferring the intertemporal substitution effect from the relation between wages and labor supply. By examining the labor supply response to a variation in net earning opportunities that is credibly exogenous, I am able to estimate the extent of intertemporal substitution in maternal labor supply. In addition, the empirical strategy gives me a simple method to isolate substitution effects from wealth effects.

Using data from the US Census 1980 PUMS, I find that married women whose youngest child is 5 years old increase their labor supply by 7-34 percentage points if their five year old is enrolled in school. In contrast to the results for married mothers, there is no statistically significant effect on labor market outcomes for single mothers. Further, using a sample of

7 to 10 year old children from the NLSY, I investigate persistence in employment outcomes for a married mother whose child delayed school entry. Results obtained from analyzing the two data sets points towards intertemporal substitution in labor supply. In particular, when evaluated between age 7 and 10, the labor supply of a mother whose child delayed school entry increases by 12 percentage points relative to that of the mother whose child went to school at age 5. This increase can be attributed to a pure wealth effect. Rough calculations yield an uncompensated labor supply elasticity of 0.76, an intertemporal elasticity of substitution of 1.1 and a wealth elasticity of -0.37.

The remainder of the paper is organized as follows. Section II discusses the theoretical background that explores the relation between school entrance age and intertemporal maternal labor supply. In Section III, I address identification issues as well as outline the empirical model used in the baseline regressions. Section IV discusses data and sample selection issues and presents some summary statistics. Section V presents the main findings and results obtained from the baseline models. Finally, I conclude the discussion in section VI with particular emphasis on implications for education policy.

2 School Entrance Age and Intertemporal Maternal Labor Supply: Theoretical Background

The main issue in the empirical estimation of the intertemporal labor supply elasticities is the endogeneity of intertemporal wage changes since labor supply today depends on past and expected future wages. In the absence of a good instrument, most studies treat wages as exogenous or use age and education related variables as instruments for life cycle wage changes (Altonji, 1986).

Recently several researchers have used natural experiments and differences-in-differences approach to estimate intertemporal labor supply using cross-sectional variation in wages. The main motive behind these empirical strategies is to exploit certain life cycle events or policy changes that generate exogenous and anticipated wage changes that can be used to estimate intertemporal elasticities. Mulligan (1999) uses the termination of Aid to Families with Dependent Children (AFDC) as a life cycle event that causes an anticipated wage shock to study changes in labor supply over time. Several studies have used differences-in-differences estimates to measure the effect of tax reforms on labor supply (Eissa, 1995; Eissa and Liebman, 1995; Blundell, Duncan and Meghir, 1998). However, there are serious concerns of selection bias and the possible endogenous nature of wage changes in these studies. In addition, the choice of control groups is questionable in most of these studies.[1]

In this paper, I propose a new framework to study the intertemporal labor supply hypothesis. A child care subsidy, in the form of free or subsidized kindergarten, increases a mother's effective wage at the employment margin. However, there is substantial variation in the age at which a child may begin school. Thus, school entrance ages provide an exogenous source of variation in the net earning opportunities for a mother. I exploit this variation to study intertemporal labor supply behavior among mothers of school age children. Before explaining the empirical strategy, it would be worthwhile to understand how school entrance age affects maternal labor supply within a multiperiod context.

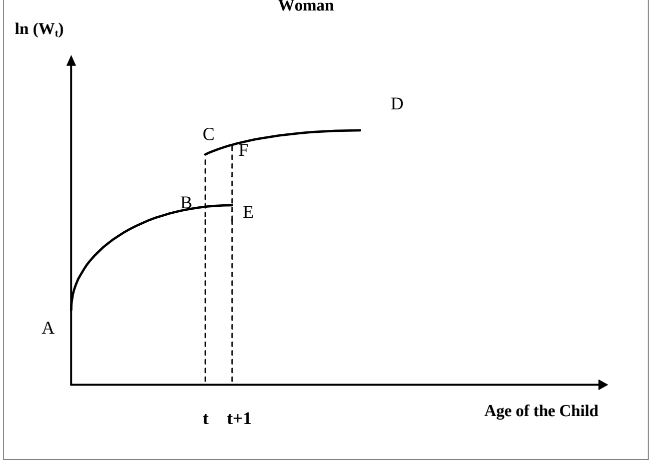
Gelbach (1999) shows that for mothers who would otherwise have not worked, free public school enrollment provides a 100% price subsidy for child care at the margin. This would increase her effective wage at the employment margin increasing the price of leisure relative to the price of consumption. This, in turn, would make her substitute towards work and away from leisure. For women who would choose to work more hours than the length of the school day, public school enrollment would be like an income transfer equal to the number of hours spent in school times the market hourly price of child care. Therefore, the budget set has a kink at the point that represents the length of the school day. Women located at the kink receive both a price subsidy and an income subsidy. Economic theory thus predicts that those women who work less than the number of hours of school day are the ones who are most

[1] See Blundell and MaCurdy (1999) in Handbook of Labor Economics, vol. 3A for a review of these studies.

likely to increase their labor supply in response to this subsidy. This has implications for different subgroups of women since single women, on an average, work more hours compared to married women.

However, not all women receive this subsidy at the same time. This is because children enter school at different ages depending upon parental preferences and/or state laws governing kindergarten entrance ages. In the United States, state laws require a child to turn five by the state cut-off date to be eligible to attend kindergarten in the beginning of the academic year, usually, in September. As a consequence, children born just before the state cut-off are almost a year younger, when they enter kindergarten, relative to children born after the cut-off. For example in California, where the cut-off is December $2nd$, the youngest child in a class (born on December $1st$) would be allowed to enter kindergarten in September when he is just four years and nine months old compared to the oldest child (born after the cut-off) who would be a year older. In Indiana where the cut-off is July 1st, the youngest child in a class would be five years and two months old when he enters school in September. This difference in chronological age between the youngest and the oldest kindergartner also generates variation in the time period at which a mother receives the implicit child care subsidy. This is shown below in the figure. Figure 1 illustrates the wage profiles of two mothers who are identical in all observable characteristics but differ in the age at which they send their child to school.

Figure 1: Wage Profile of a Constrained and an Unconstrained Woman



The 'constrained' woman refers to the mother whose child begins kindergarten at age 6 while the 'unconstrained' refers to a mother who sends her child to school at age 5. The life cycle wage profiles of the two women are identical except at the time period t that corresponds to the year the child turns 5. At time t, the unconstrained mother receives a child care subsidy in the form of free or subsidized schooling that increases her effective wage causing her wage profile to shift upwards (given by the curve ABCD). The constrained mother receives this subsidy in period $t+1$, corresponding to the year the child turns 6, and her wage profile shifts up by the same amount (the curve AEFD). An implicit assumption is that there is no loss in human capital accumulation associated with joining the workforce a year later. Given the wage profiles of the two women, how would their labor supply differ over the life cycle assuming an environment of perfect certainty?

Theoretically (MaCurdy,1981), labor supplied by the two women would differ over their life-cycle because of two reasons. First, in response to the higher net wages at period t the unconstrained mother would increase her labor supply relative to the labor supplied by the constrained mother. She adjusts her labor supply in response to intertemporal wage changes along her life cycle wage profile keeping lifetime wealth constant. This labor supply response to intertemporal wage changes along an individuals life cycle wage profile is measured by the intertemporal elasticity of substitution σ. The total increase in labor supply at period t due to this effect is given by the absolute value of difference between the net wages times the intertemporal elasticity σ. There is, however, an additional effect on labor supply. The unconstrained mother has higher lifetime wealth relative to the constrained mother given by the area BEFC in figure 1 above. This implies that at all time periods her labor supply will be lower than the labor supplied by the constrained mother. This represents a pure wealth effect associated with higher lifetime income. The sign on the effect of the subsidy on labor supply at period t is ambiguous and depends upon the strength of the substitution and the wealth effects.

3 Empirical Issues and Identification Strategy

Empirically identifying the causal effect of school enrollment age on maternal labor market outcomes is challenging due to the endogeneity of entrance age. Ideally, one can estimate the effect of kindergarten attendance on maternal labor supply using the following equation:

$$Y_i = \alpha S_i + X_i'\beta + \epsilon_i$$

Where Y_i measures maternal labor supply outcomes, S_i is an indicator for whether the child is attending kindergarten at age 5 and X_i is a vector of controls. The causal interpretation depends on the assumption that $E[S_i\epsilon_i|X_i] = 0$. However, there are two potential sources of bias in the OLS estimates of the effect of entrance age on maternal labor supply. First, entrance age is correlated with parental and child unobservable characteristics that may themselves be directly related to maternal labor market outcomes. For instance, parents are more likely to delay school entry for a child with learning disabilities. At the same time, mothers of such children are also less likely to work. Therefore, if we do not control for the unobserved ability of a child, we would overestimate the negative effect of school entrance age on the mother's labor supply. A second source of bias in the estimated coefficients may also be due to the simultaneity of school entrance age and parental labor supply. The OLS estimate of school entrance age on maternal labor supply may be contaminated by the fact that career driven women may send their children to school early so that their labor supply is not adversely affected by an additional year of child care. Thus, depending on the importance of these two factors and the variables for which we control, the sign of the overall bias could go in either direction.

In the existing literature, there are at least two strategies that researchers have used to infer causality from the school attendance and maternal labor supply relation. One identification strategy exploits variation in the availability of public schools or day care across states and over time. Cascio (2009) used variation in preschool availability from the introduction of kindergarten in the United States to study the effect of child care on maternal labor supply. Using a Differences-in-Differences (DD) approach, she finds that single women with kindergarten eligible children, but no younger children, were more likely to be employed with the availability of kindergartens. Schlosser (2006) exploits a Israeli policy change that introduced free public preschool for children aged 3 and 4 to study the effect on labor supply of Arab mothers. She finds an increase in labor supply as a consequence of the availability of free public schools among more educated mothers. Berlinski and Galiani (2007) provide evidence of increased maternal employment in Argentina as a result of large construction of pre-primary school facilities. Baker, Gruber and Milligan (2008) look at the effect of increased public financing for child care under Canada's '\$5 per day child care' program on a range of outcomes including maternal labor supply. Lefebvre, Merrigan and Verstraete (2009) show that Québec's universal childcare policy, which gave mothers of young children a temporary incentive to join the labor market, led to substantial life-cycle labour supply effects. Each of these studies finds some evidence that increase in the availability of preschools raises maternal employment, at least, for single mothers of preschool age children with no younger children. The only exception is Fitzpatrick (2010) who does not find a robust impact of universal pre-K availability on maternal labor supply.

An alternative identification strategy is instrumental variable estimation of the effect of age at enrollment on maternal labor supply. One widely used instrument is quarter of birth or, more generally, month of birth (Angrist and Krueger, 1991; Gelbach, 2002; Mayer and Knutson, 1999). If students are allowed to enter school in the year they turn five, children born in the later part of the year will be less likely to be enrolled in school. Assuming that month of birth is not correlated with unobservable characteristics, we can use month of birth as a valid instrument for age at enrollment. Using quarter of birth as an instrument for public school enrollment of five year old children, Gelbach (2002) finds that women with kindergarten eligible children worked more hours.

If quarter of birth is to be a valid instrument, it must be related to maternal labor market outcomes only because it affects the age of enrollment of the child. However, there is some evidence that suggests that maternal employment may be directly related to month of birth of a child. In a recent paper, Buckles and Hungerman (2010) find that babies born in the winter are more likely to have mothers who are unmarried, who are teenagers or who lack a high school diploma. Bobak & Gjonca (2001) find that the magnitude of seasonal variation in births was particularly strongly associated with maternal socio-demographic characteristics. Further, in occupations that are characterized by seasonalities in labor demand, it has been found that more babies are born in seasons of less work load (Nurge,1970). I control for quarter of birth in my regressions to address any such concerns.

Consider the following model of the relation between age at enrollment and maternal labor market outcomes:

$$Y_{it} = \alpha_t S_i + X_i^{'} \beta_t + \gamma_t M_i + \delta_t R_i + \epsilon_{it}, \qquad t = 5, 7, .., 10 \tag{1}$$

Where, Y_{it} is maternal labor supply measures for mother i when the child is t years old. S_i is a dummy variable indicating whether the child was enrolled in school in the year he turned five ($S_i = 0$) or had delayed entry to age 6 ($S_i = 1$). M_i is a set of dummy variables indicating the quarter of birth or the month of birth of the child. X_i is a vector of observable individual characteristics and R_i represents a set of state controls. All models are estimated separately by the age of the child. As discussed earlier, the age at which a child starts school is endogenous causing the OLS estimates of α to be biased. To control for this endogeneity, I propose an instrumental variable estimation strategy.

I use 2SLS estimates to identify α in equation (1) above where S_i is instrumented using a dummy variable Z_i that takes on a value of one if the law constrained the child to delay entry into kindergarten. In other words if the child's month of birth lies later than the state kindergarten entrance age cut-off date, Z_i equals one and zero otherwise. More formally, I estimate the parameters of equation (1) using 2SLS based on the following first-stage equation for observed enrollment:

$$S_i = \pi_t Z_i + X_i^{'} \lambda_t + \varphi_t M_i + \theta_t R_i + \upsilon_{it}, \quad t = 5, 7, .., 10 \tag{2}$$

All specifications include controls for the month of birth of a child to take into account the season of birth effects. In addition, I control for state of residence to take into account any state differences in maternal employment opportunities.

This is not the first study that uses variation in state kindergarten entrance age laws and month of birth to instrument for actual entrance age. Recently several researchers have exploited the cross state variation in school entrance age laws and variation in date of birth to instrument for actual entry age (For example, Bedard and Dhuey, 2006; Cascio and Lewis, 2005; Datar, 2005; Elder and Lubotsky, 2009). However, these studies have looked at the effect of school entry age on child outcomes. The only exception is Fitzpatrick (2010) who uses the discontinuity generated by birthday-based eligibility cutoffs to study the effect of universal pre-K availability on maternal labor supply. Unlike previous research, this is the first study that exploits the state laws and month of birth variation to look at long run maternal labor market outcomes. In addition, as discussed extensively in Barua and Lang (2010), if there is heterogeneity in treatment effects, the instrument used here identifies (under some reasonable assumptions) the Local Average Treatment Effect (LATE) i.e. the labor supply effect on those women who decide to delay school entry only because the law constrains them to do so.

I implement the above empirical strategy in the following way. First, I estimate the effect of a five year olds school enrollment on maternal labor supply. Next, I explore long run outcomes by estimating equation (1) using 2SLS for a sample of 7 to 10 year old children.

4 Data and Descriptive Statistics

4.1 US 1980 Census

The data for 5 year old children is drawn from 1980 US Census 5% Public Use Microdata (PUMS). Since the Census day in 1980 was April 1^{st}, I restrict the sample to 5 year olds who were born in quarters two through four and 6 year old children born in the first quarter (i.e. those who turned 5 in 1979). A children file was created for each household with corresponding information on the child's characteristics and mother's information.

For the analysis with five year old children, the main explanatory variable is the school attendance dummy variable. I use the census school attendance variable where I code attendance as 1 if the child is attending a public, private or church related private school. The dummy is coded as zero if the child is not enrolled in any school. Though school entry age laws do not directly affect attendance for children who go to private schools, I choose to keep them in the sample because the attendance pattern of these children are likely to be influenced by state laws. Some parents may send their children to private schools to get around the strict cutoffs imposed by public schools. In addition, the decision to send a child to a private school would also directly depend upon the availability of public schools.

While the Census provides accurate information on attendance for those children who turned 5 in 1979, it cannot be used to study the effect of prior school attendance on older children. This is because the Census does not report the school entrance age of respondents. School entrance age can still be computed for each child using grade information if I assume that no child repeats or skips a grade. However, entrance age computed using this method will be distorted because of the prevalence of grade repeaters. To circumvent this data limitation, I use the National Longitudinal Survey of Youth (NLSY) to estimate the effect of delayed school entry on long run maternal labor supply measures. The NLSY sample is discussed in detail in the next subsection.

The ideal instrument for this analysis would exploit variation in exact date of birth and state laws. But the census does not have month of birth information in the public use files. Instead I use information on quarter of birth and state laws to determine whether the law required a child to delay entry into kindergarten from the year he turned five to the year he turned six. I deleted observations for whom I could not determine whether the child was born before or after the state cut-off. For example, I dropped children born in the third quarter who went to kindergarten in states with a mid-third quarter cut-off. Similarly the sample does not include children who are born in the fourth quarter in states with a mid-fourth quarter cut off.

I estimate the equations separately for married mothers and single mothers between the age of 21 and 50. The census has extensive information on maternal characteristics, family background and schooling, allowing me to include a rich set of controls in the baseline regressions. All specifications control for quarter of birth of the child and state of residence. I also control for a quadratic function of the mother's age, her race, education, SMSA dummy, log of spouse' income, number of children, number of adult family members and whether she is the head of the household.

Table 1 presents descriptive statistics for the main variables used in the census regressions. Means and standard deviations are reported for the three samples on which the census analysis is based, namely, (i) married mothers whose youngest child is age 5, (ii) single mothers whose youngest child is age 5 and (iii) married mothers of 5 year old children who also have additional younger children. As would be expected, single mothers of five year old children work more than married mothers whose youngest child is five, are more likely to be household heads, less likely to be white, have fewer adult members in the household and are younger on average.

Table 1: Means and Standard Deviation (in parenthesis) of Variables for the sample of 5 Year olds (Census)

Variable	Married Mothers (youngest is 5 years old)	Single Mothers (youngest is 5 years old)	Married Mothers (with younger than 5-yr old Child)
Usual Hours Worked	19.85	26.60	14.37
	(18.98)	(18.99)	(18.21)
Employment in 1979	0.59	0.72	0.45
	(0.49)	(0.45)	(0.50)
Wks Worked in 1979	22.52	29.27	14.85
	(22.59)	(22.63)	(20.25)
White	0.87	0.64	0.86
	(0.33)	(0.48)	(0.34)
Age	32.57	29.97	29.21
	(5.39)	(5.85)	(4.47)
Education	12.24	11.74	12.33
	(2.54)	(2.42)	(2.80)
Log(Spouse's) Income	8.63		8.57
	(3.01)		(2.98)
Number of Children	2.35	2.03	2.92
	(1.17)	(1.25)	(1.18)
# of Adult Members	1.15	0.65	1.11
	(0.49)	(1.04)	(0.44)
SMSA	0.75	0.81	0.74
	(0.44)	(0.39)	(0.45)
Head of Household	0.03	0.80	0.02
	(0.16)	(0.40)	(0.15)
Total Sample Size	42,500	11,690	41,795

4.2 NLSY79

As discussed earlier, the census does not have school entrance age information. To study the long run effects of delayed school entry on maternal labor supply of married mothers, I use data from NLSY79. The National Longitudinal Survey of Youth 1979 cohort (NLSY79) is a panel survey of 12,686 nationally representative men and women between the ages of 14 and 21 as of December 31, 1978. The NLSY79 contains extensive information on the labor market experience, education, family, demographics and habits of the respondents. Since 1986, the children of the original 6,283 NLSY79 women have been assessed every two years. In addition to the public use files of the NLSY, I obtained information about exact date of births and state of residence from the NLSY confidential Geocode files.

The school entrance age variable was computed using data on last grade attended, interview dates and grades repeated or skipped for children who were enrolled in school. The survey contains several questions pertaining to grade attended and grades completed. I used this information, combined with information on grades skipped or repeated, to compute the age at which the child entered kindergarten. The NLSY asks the respondents questions about the ‘last grade attended or currently attending’. One problem with the way the question is framed is that a respondent who answered the question in January, for instance, would be referring to the grade that he entered in the previous calendar year. To address this data limitation, I used interview dates to verify the exact age of entrance. To be consistent, any respondent who was asked about his last grade attended in or before July was assumed to have started that grade in the previous calendar year. On the other hand any respondent who was interviewed in August or later was assumed to be referring to the grade he entered in the current calendar year. Observations that did not have sufficient information to compute the entrance ages were deleted from the sample leaving me with a sample size of 7448 children and young adults for whom entrance age information could be computed.

Unfortunately it is not possible to replicate the census analysis for five year old children using the NLSY sample. I ran into several data problems while trying to create the school attendance variable for five and six year old children and variables related to the mother’s labor supply. Unlike the census which has a unique census day (1^{st}April 1980), the NLSY is a rolling survey. For most years a majority of interviews were scheduled over the summer. This made it difficult to interpret the school enrollment variable for five and six year olds. For these two age groups, I could not ascertain whether they would be enrolled in kindergarten in the academic year beginning in Fall of the year of the interview. In addition, the mother’s work variables also corresponded to a period when the child was not enrolled in school. I could have used a sample of mothers of five and six year olds who were interviewed in September or later but, when I tried to construct this sample, I was left with a very small number of observations and the estimates obtained were very unstable. Since all children were enrolled in school by the age of 7, none of these concerns would bias my estimates and therefore I decided to restrict the sample to children aged 7 and above.

I construct a pooled cross section of children between the age of 7 to 10 years in the period 1980 to 2000. The mother’s labor supply measures as well as all the right hand side variables are created by the age of the child. For example, in the regressions for 7 year old children, I include the values of the right hand side variables and the dependent variable corresponding to the year that the child turned 7. All regressions include controls for month of birth of child, year of birth dummies and dummies for the state in which the child went to kindergarten. In addition, the regressions also include a set of controls for mother's characteristics including race, standardized AFQT, log of husbands income, age, family size, number of children, presence of an elderly relative, state of current residence dummies and a dummy for the presence of a child younger than age 5.[2]

I study the effect of delayed school entry on three different measures of maternal labor supply; employment status during the survey week, weeks worked since last interview and usual hours worked per week in current/most recent job. The weeks worked variable is defined as the proportion of weeks worked since last interview (weeks worked since last interview divided by weeks since last interview). The NLSY has another accompanying variable that tells the user the percentage of weeks unaccounted for while computing weeks worked. Those respondents who show some percentage of weeks unaccounted for have missing or inconsistent work information that does not allow an employment status for that week. In my analysis I only keep observations for whom all weeks have been taken into account. Finally, all hourly wages are converted to real terms ($2000) using the personal consumption expenditure price index.

4.3 State Kindergarten Entrance Age Policies

The identification strategy required knowledge of exact kindergarten entry cut-off dates for every state in the US. Data on state kindergarten entrance ages laws were gathered from various sources to get accurate information. I gathered information on school cut-off dates for several years from the Education Commission of the States. These laws were verified by looking at the US historical state statutes. If the history of the statute indicated a

[2]Presence of an elderly relative in the household is defined as a dummy that takes the value of 1 if the mother reports having parents, grandparents, in-laws and grandparents-in-laws living in the household during the time of the survey.

change in the state law at any given year, I examined the relevant state session law to determine the exact form of the change. As a result, I was able to gather information on kindergarten entrance age cut-off dates for all US states for the period 1979 to 2000. Table 2 lists the kindergarten entrance age cut-off dates in 1979 for all states included in the sample. Eight states (Colorado, Indiana, Louisiana, Massachusetts, New Jersey, New Hampshire, Pennsylvania and Vermont) that had given Local Education Authorities (LEA) the power to set the entrance age law were deleted from the sample.

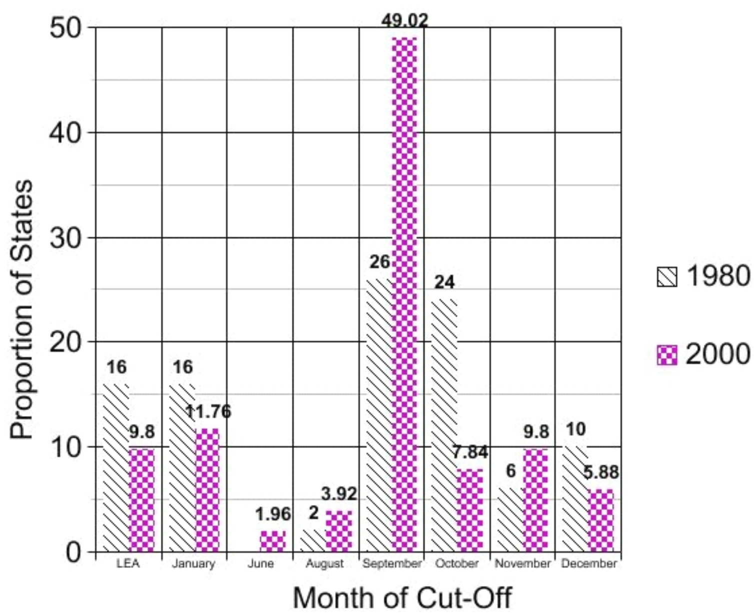
Table 2: Kindergarten Entrance Age Laws in 1979 for US States

State	State Cut Off Date	State Cut Off Month*
Alabama	1	October
Alaska	2	November
Arizona	1	January
Arkansas	1	October
California	1	December
Connecticut	1	January
Delaware	1	January
DC	31	December
Florida	1	January
Georgia	1	September
Hawaii	31	December
Idaho	16	October
Illinois	1	December
Iowa	15	September
Kansas	1	September
Kentucky	1	October
Maine	15	October
Maryland	31	December
Michigan	1	December
Minnesota	1	September
Mississippi	1	January
Missouri	1	October
Montana	10	September
Nebraska	15	October
Nevada	30	September
New Mexico	1	September
New York	1	December
North Carolina	16	October
North Dakota	1	October
Ohio	30	September
Oklahoma	1	November
Oregon	15	November
Rhode Island	31	December
South Carolina	1	November
South Dakota	1	September
Tennessee	31	October
Texas	1	September
Utah	1	September
Virginia	1	December
Washington	31	August
West Virginia	1	September
Wisconsin	1	September
Wyoming	15	September

Source: Education Commission of States, State Legal Statutes
*The States of Colorado, Indiana, Louisiana, Massachusetts, New Hampshire, New Jersey, Pennsylvania and Vermont were deleted from the sample because the eligibility age was set by the Local Education Authority (LEA) in these states.

Figure 2 compares the proportion of states by the cut-off month in 1979 with the corresponding proportions in 2000. Most states in 1979 had fourth quarter cutoffs, fourteen states had September cutoffs while five states had January first cutoffs. The prevalence of fourth quarter cutoffs also implies that most states allowed children to enter kindergarten before their fifth birthday. The figure shows that in recent years there has been a trend towards increasing the school entry age and most states are moving towards a September cut-off.

Figure 2: Proportion of States by Kindergarten Cut-Off Month, 1980-2000



Note:

- Compiled using data from various sources
- LEA refers to Local Education Authority
- End of month cut offs have been clubbed with the following month. For example, a 30th September Cut off is counted in the month of October

5 Results

5.1 OLS and 2SLS Estimates for 5 Year Old Children

Using data from the 1980 census, I estimate the effect of school enrollment on the labor supply of mothers[3]. The analysis is done separately for three samples of women, namely, married mother whose youngest child is five years old, single mothers whose youngest is age

[3] I have estimated similar models for fathers but I do not find any effect on their labor market outcomes.

5 and married mothers with children younger than five years old but whose five year old is the eldest.

Table 3: Summary of First Stage Regressions for 5 Year Olds from Census Data (Just Identified Model)

	Married Mothers (no younger children)	Single Mothers (no younger children)	Married Mothers (younger than 5 year old)
Coefficient on the Instrument	0.328***	0.289***	0.335***
	(0.021)	(0.023)	(0.034)
Quarter of Birth 2	-0.006	-0.012**	-0.005
	(0.005)	(0.005)	(0.004)
Quarter of Birth 3	-0.020***	-0.015**	-0.019***
	(0.006)	(0.005)	(0.005)
Quarter of Birth 4	-0.063***	-0.046**	-0.109***
	(0.015)	(0.020)	(0.032)
White	-0.013*	-0.002	-0.004
	(0.007)	(0.008)	(0.006)
Education (Mother)	0.010***	0.010***	0.009***
	(0.002)	(0.002)	(0.001)
# of Adult Family Members	-0.009**	-0.004	-0.003
	(0.003)	(0.004)	(0.004)
Log (Spouse's Income)	0.002***		0.003***
	(0.000)		(0.000)
# Own Children in the Household	-0.001	-0.002	-0.013***
	(0.004)	(0.003)	(0002)
SMSA	0.037***	0.020**	0.028***
	(0.007)	(0.009)	(0.006)
Household Head	0.000	0.012	-0.010
	(0.011)	(0.011)	(0.009)
Age (Mother)	0.022***	0.007	0.023***
	(0.003)	(0.004)	(0.004)
Age Square	-0.000***	-0.000	-0.000***
	(0.000)	(0.000)	(0.000)
F (test of excluded instrument)	253.62	159.44	96.92
P value of F-statistic for the Instrument	0.0000	0.0000	0.000
Centered R-Squared	0.225	0.145	0.248
Partial R-Squared	0.031	0.025	0.027
Sample Size	37246	10700	36941

Heteroskedasticity Robust standard errors in parenthesis
***Significant at 1%, **Significant at 5%, *Significant at 10%
Regressions also include State Fixed Effects

The first stage relation between the instrument (Dummy variable =1 if born before the cut off) and the endogenous variable (Dummy variable =1 if enrolled in school at age 5) provides some useful preliminary insight into the underlying relation between the variables of interest. Table 3 confirms that there is a very strong correlation between school eligibility and school enrollment. For mothers of five year olds, with no younger children, the first stage coefficient on the instrument is equal to 0.289 and 0.328 for the regressions on single and married women respectively. The coefficient on married mothers who have younger than five year old children is 0.335. These coefficients are very highly statistically significant. The first stage F-statistics are high, 159.4 for the regressions on single women and 253.6 for the married women regressions. These estimates also suggest that controlling for observable characteristics, children born in the fourth quarter are least likely to be enrolled in school as compared to children born in the first quarter. The likelihood of school attendance is declining with the quarter of birth of the child. The F-statistics suggest that there is a strong correlation between the school attendance variable and the law enforced school eligibility and so the weak instrument problem should not be a concern in the analysis.

Tables 4 and 5 report the results from the regression of different measures of maternal labor supply, for married mothers whose youngest child is age 5, on a 5 year olds school attendance. I consider four measures of labor supply, namely, employment in 1979, weeks worked in 1979, usual hours worked per week and labor force participation. The endogenous explanatory variable in these regressions is a dummy variable that takes the value of one if the 5 year old is enrolled in a school and zero otherwise.[4]

In table 4, employment in 1979 is regressed on the school enrollment dummy. I tried probit versions of all regressions but since the results do not change much with the probit specification, I decided to report estimates from the linear models only. Column (1) reports the OLS estimate from a linear regression with no controls. In column (2), a rich set of controls are added. In addition, regressions include controls for quarter of birth and state fixed effects. The reported heteroskedasticity robust standard errors are clustered by state times quarter of birth. I get comparable standard error estimates when clustered only at the state level.

The OLS estimates imply that, among married mothers, having a child enrolled in school increases employment by 5.2 percent. The IV estimates imply that OLS is downward biased, confirming the results obtained in previous studies (Gelbach, 2002). This is perhaps because high income parents are more likely to delay entry. But such parents are also more likely to continue working since they are able to afford child care costs. Controlling for quarter of birth, IV estimates in column (4) suggest that married mothers of 5 year old children, who are enrolled in school, are 11 percent more likely to work. This is a big effect and amounts to a

[4]Since school term begins in September in most states, this implies that my estimates for 5 year olds is measuring the effect of school enrollment only for the month of September and the last quarter. Therefore, it should be kept in mind that the effect of a child care subsidy for the entire year would be almost three times as large as the estimates reported in this paper.

18 percentage point increase in baseline participation (the mean employment for this group is 59 percent). Note that the estimates in column (3), which do not control for quarter of birth, are in line with the results reported in Gelbach (2002). This could reflect the importance of controlling for seasonalities in birth. At the same time, it is important to acknowledge that if there is treatment effect heterogeneity, then it is not obvious whether the results from this paper are directly comparable to other studies since the population parameters identified would have different LATE interpretations. As noted earlier, my results should be interpreted as the labor supply response of women who decide to delay school enrollment of their 5 year old only because the law constrains them to do so

Table 4: Effect of 5-Year Olds School Attendance on 1979 Employment Status for Married Mothers (with no younger children)

	(1) OLS	(2) OLS	(3) IV	(4) IV
School attendance	0.062***	0.052***	0.045***	0.106**
	(0.014)	(0.010)	(0.016)	(0.052)
White		-0.133***	-0.133***	-0.132***
		(0.007)	(0.007)	(0.007)
Mothers Education		0.022***	0.022***	0.021***
		(0.002)	(0.002)	(0.002)
# of HH Adult Members		0.032***	0.032***	0.032***
		(0.005)	(0.005)	(0.005)
Ln(Spouse's Income)		0.006***	0.006***	0.005***
		(0.001)	(0.001)	(0.001)
# of own children		-0.028***	-0.028***	-0.027***
		(0.003)	(0.003)	(0.003)
SMSA		-0.020***	-0.019***	-0.022***
		(0.007)	(0.007)	(0.007)
Household Head		0.103***	0.103***	0.103***
		(0.016)	(0.016)	(0.016)
Age		-0.020***	-0.020***	-0.022***
		(0.004)	(0.004)	(0.005)
Age Squared		0.000**	0.000**	0.000**
		(0.000)	(0.000)	(0.000)
Observations	42500	37246	37246	37246
R-squared	0.00	0.06	0.06	0.06
State Fixed Effects	No	Yes	Yes	Yes
Quarter of Birth Dummies	No	Yes	No	Yes

In table 5, the first row looks at the effect of school attendance on usual hours worked per week. I estimate both a linear model and a tobit model to take into account the censored hours data. OLS estimates are downward biased and IV estimates imply an increase of 4.3 hours per week in the linear specification (column 2) and 6.8 hours per week in the tobit specification (column 4). The mean hours worked by this sample of women is about 20 hours per week implying a 21.5 to 34 percentage points increase in baseline hours worked. I also report estimates from two more labor supply measures. Having a child enrolled in school increases labor force participation by 7 percent, but this effect is not statistically significant at conventional levels of significance. Average weeks worked in 1979 increased by 1.64 weeks in the linear specification and 4.5 weeks in the tobit model. The IV estimates therefore imply that having a child enrolled in school increases baseline labor supply for married women by between 7.3 to 34 percentage points.

Table 5: Effect of 5-Year Olds School Attendance on Labor Supply Measures of Married Mothers (with no Younger Children)

	(1) OLS	(2) IV	(3) Tobit	(4) IV Tobit
Usual Hrs Worked/Week	1.421***	4.268**	2.766***	6.764**
	(0.453)	(1.782)	(0.740)	(2.948)
Labor Force Status	0.074***	0.071		
	(0.014)	(0.059)		
Weeks Worked in 1979	2.924***	1.625	8.082***	4.482
	(0.539)	(2.050)	(1.428)	(3.290)
Quarter of Birth Dummies	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes

Sample size is 37246

Note that the IV estimates, though large in magnitude, are imprecise relative to the OLS estimates. The standard Hausman test is not applicable in this analysis due to clustered standard errors. Instead, I use an asymptotically equivalent version of the Hausman test to test for endogeneity. I take the residual from the first stage of the regression and include it as an additional regressor in the original OLS equation. The t value on the residual is insignificant for all the labor supply measures. A Hausman test fails to reject the hypothesis that the OLS estimates are consistent, indicating that endogeneity may not be a substantial issue here.

Table 6 and 7 estimate the corresponding labor supply equations for single mothers. The OLS estimates for single mothers are larger than the OLS estimates for married mothers, but the effect disappears in the IV specification. In comparison to the results for married women, IV estimates for single women imply that having a child in school does not have any statistically significant effect on labor supply measures. Single women with 5 year olds, who are enrolled in schools, are 2.1 percent more likely to be employed in 1979, work about 2 hours more per week and 1.6 weeks more relative to mothers whose child is not enrolled in school. In terms of the magnitude of this effect with respect to the baseline means, this translates to very small and statistically insignificant effect on labor supply measures. Once again, Hausman test fails to reject exogeneity. If one is willing to accept failure to reject as an acceptance of the hypothesis, then OLS estimates are in line with Gelbach (2002).

Table 6: Effect of 5-Year Olds School Attendance on 1979 Employment Status for Single Mothers (with no younger children)

	(1) OLS	(2) OLS	(3) IV	(4) IV
School attendance	0.083***	0.079***	0.053*	0.021
	(0.016)	(0.015)	(0.031)	(0.056)
White		0.079***	0.079***	0.079***
		(0.013)	(0.013)	(0.013)
Mothers Education		0.043***	0.044***	0.044***
		(0.002)	(0.002)	(0.003)
# of Adult HH Members		0.014**	0.014**	0.014**
		(0.006)	(0.006)	(0.006)
# of own children		-0.045***	-0.045***	-0.045***
		(0.006)	(0.006)	(0.006)
SMSA		-0.042***	-0.041***	-0.041***
		(0.012)	(0.012)	(0.012)
Household Head		0.079***	0.079***	0.079***
		(0.015)	(0.015)	(0.015)
Age		0.024***	0.025***	0.025***
		(0.007)	(0.007)	(0.007)
Age Squared		-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)
Observations	11690	10700	10700	10700
R-squared	0.00	0.14	0.14	0.14
State Fixed Effects	No	Yes	Yes	Yes
Quarter of Birth Dummies	No	Yes	No	Yes

Table 7: Effect of 5-Year Olds School Attendance on Labor Supply Measures of Single Mothers (with no Younger Children)

	(1) OLS	(2) IV	(3) Tobit	(4) IV Tobit
Usual Hrs Worked/Week	2.506***	0.187	3.849***	0.247
	(0.654)	(2.919)	(0.949)	(3.721)
Labor Force Status	0.082***	- 0.028		
	(0.018)	(0.075)		
Weeks Worked in 1979	3.996***	1.656	10.287***	1.426
	(0.786)	(3.433)	(2.070)	(4.263)
Quarter of Birth Dummies	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes

Sample Size = 10700

The difference in the estimates for single and married women is not surprising for atleast three reasons. First, several researchers have found that the labor supply measures for single mothers are less responsive to child care prices than labor supply measures for married mothers (Connelly 1990; Kimmel 1998; Michalopoulas et. al. 1992). Single women are the sole bread earners for the family and their own income is typically low. As a result they are more likely to rely on relatives and less likely to rely on center-based arrangements or private market child care facilities (U.S. House of Representatives, 1998). Since free public schools are simply a substitute for the informal child care provided through relatives and friends, the labor supply measures do not respond to the availability of free schools. Second, single women, on an average, work more hours than married women. Theoretically, as explained in section II, women who work less than the hours of school day are the ones who are most likely to be induced to increase their labor supply. Finally, the effect of school enrollment on the labor supply of single women is not easily interpretable due to the complex behavioral relation between paid child care, public assistance (such as the AFDC) and labor supply. The labor supply response of single women to the school enrollment of her child may be inelastic due to the high cost associated with losing AFDC eligibility.

Next, I estimate the effect of a five year olds school attendance on the labor supply of married women who have additional younger children at home.[5] For women who have younger than school age children at home, the labor supply response to her 5 year olds school attendance is slightly more complicated. The hourly rate for childcare depends, among other things, on the number of children being cared for. For women who do not have additional younger children, the drop in child care costs when her 5 year old enrols in school can be substantial. On the other hand, for women with additional younger children, the drop in hourly child care costs from caring for three children to two children, for example, is only marginal. Thus, we should expect the labor supply response to be relatively smaller in magnitude. Table 8 shows the coefficients from regressions of school attendance on all four labor supply measures for this sample of women. Column (1) reports the OLS estimates from the linear regression of all four labor supply measures on public school attendance dummy.

[5] In regressions not shown here, I don't find any effect of school enrollment on labor supplied by single mothers who have additional younger children.

Table 8: Effect of 5-Year Olds School Attendance on Labor Supply Measures of Married Mothers with Younger than 5 Year Old Children

	(1) OLS	(2) IV	(3) Tobit	(4) IV Tobit
Worked in 1979	0.051***	0.052		
	(0.009)	(0.036)		
Usual Hrs Worked/Week	1.253***	1.096	3.431***	3.517
	(0.360)	(1.286)	(0.787)	(2.743)
Labor Force Status	0.052***	0.050		
	(0.010)	(0.034)		
Weeks Worked in 1979	1.942***	4.124***	6.452***	3.767
	(0.378)	(1.521)	(1.175)	(2.469)
Quarter of Birth Dummies	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes

Sample size = 36941

Column (2) reports the 2SLS estimates while columns (3) and (4) report estimates from the corresponding tobit model for weeks of work and hours of work. Interestingly for this group the OLS and IV have the same magnitude for three of the four labor supply variables. However, unlike the OLS estimates, IV coefficients are not significantly different from zero for three of the measures of labor supply. There is a large effect of school enrollment on weeks worked in 1979, an increase of 4 weeks or 27 percent of mean weeks, in the linear model, but the effect disappears when censoring is taken into account in column (4). Perhaps these women are choosing to work for longer (in terms of weeks of work) periods of time in exchange for no change in intensity of work.

To sum the results obtained from the census regressions; IV estimates imply that having a child enrolled in school increases labor supply measures for married women, with no younger children, by between 7 to 34 percent. In contrast to the results for married mothers, I do not find any statistically significant effect on labor market outcomes for single mothers. For married mothers with younger than five year old children, there is a non trivial increase in weeks worked but the other labor supply measures are not statistically significant though the estimated magnitudes are generally not small.

5.2 Long Run Effect of Delayed School Entry on Maternal Labor Supply

The census results show that having a 5 year old in school increased the labor supply of married women. This is not a surprising result given that public schooling in the United States is free and theoretically it is comparable to a 100% price subsidy on child care. A more interesting question is related to the labor supply of mothers whose child delayed school entry. How would the life cycle labor supply profile of these women differ from the labor supply profile of women who received the subsidy a year earlier? To answer this question, I use the NLSY to study labor supply outcomes for a pooled cross section of mothers of 7 to 10 year olds. As discussed earlier, due to data limitations, I do not estimate the effect of school enrollment of 5 or 6 year old children on maternal outcomes using data from the NLSY.

Table 9 presents descriptive statistics for the variables used in the NLSY regressions for a pooled cross section of married mothers of 7 year old children.[6] Column (1) reports means and standard deviations of variables for the entire sample of 7 year old children and their mothers. Column (2) and (3) report descriptive statistics for the sample of 7 year old children born before and after, respectively, the state kindergarten entrance age cut-off. The last column reports t-statistics and p-values for a test of differences in means from column (2) and (3). Two things are evident from this table. First, the means suggest that mothers of children born after the cut-off, and thus who delayed school entry, have higher average work measures not controlling for any other factors. This is verified by looking at the t-values and p-values for at least two of the three variables, namely, weeks worked and employment status. Second, comparing the average values of the variables with the census variables (table 2) it is clear that the NLSY sample is not different in terms of the observables. All the control variables have similar averages across the two sample. However, the average usual hours

[6] I replicated the analysis described in this section for single women from the NLSY. I get extremely imprecise and unstable estimates for these women, confirming the results obtained from the census. Though I have not reported these estimates, the full tables are available on request.

worked per week variable is very large in the NLSY relative to the census (34 hours and 20 hours respectively). This difference is due to the different ways that the variable is measured in the two samples. While the census reports the average hours worked per week in all jobs, the NLSY variable reports the average hours worked per week in the current/most recent job. Similarly, the difference in the weeks worked variable is due to the different ways it is measured in the two samples. In the NLSY it is measured as the proportion of weeks worked since last interview after taking into consideration any unaccounted weeks by respondents.

Table 9: Means and Standard Deviation of Variables for a sample Married Mothers of 7 Year old Children (NLSY)

		Sample of 7 Year Olds		
Variable	Entire Sample (1)	Born Before the cut off (2)	Born After the cut off (3)	t-test (Difference in means is zero)[1] (4)
Usual Hrs Worked	33.80	33.65	34.23	-0.96
	(12.67)	(12.53)	(13.08)	(0.3340)
Employment	0.61	0.60	0.64	-2.33
	(0.49)	(0.50)	(0.48)	(0.0199)
Weeks Worked	0.58	0.57	0.61	-2.37
	(0.43)	(0.44)	(0.42)	(0.0176)
Black	0.19	0.18	0.21	-2.36
	(0.39)	(0.38)	(0.41)	(0.0183)
Hispanics	0.22	0.23	0.20	1.93
	(0.42)	(0.42)	(0.40)	(0.0531)
Mothers AFQT	-0.05	-0.06	-0.02	-1.22
	(0.95)	(0.96)	(0.92)	(0.2206)
Mothers Grade	12.33	12.32	12.36	-0.40
	(2.42)	(2.46)	(2.27)	(0.6915)
Mothers Age	30.82	30.97	30.36	3.53
	(4.32)	(4.33)	(4.24)	(0.0004)
Ln(Spouse's) Income	8.76	8.80	8.62	1.27
	(3.39)	(3.38)	(3.46)	(0.2028)
Child less than 5	0.53	0.53	0.54	-0.99
	(0.50)	(0.50)	(0.50)	(0.3187)
Number of Children	2.61	2.61	2.60	0.39
	(1.07)	(1.09)	(1.03)	(0.6936)
Family size	4.68	4.68	4.67	0.17
	(1.23)	(1.24)	(1.17)	(0.8615)
Elderly in Household	0.03	0.03	0.03	-0.69
	(0.17)	(0.17)	(0.18)	(0.4884)
Observations	3803	2886 (75.89%)	917 (24.11%)	

The effect of delayed school enrollment on usual hours worked per week in the main job is shown in Table 10. Each row corresponds to the age of the child for whom the analysis is conducted. As with the census estimates, I report coefficients from linear and tobit models. The first column shows that the first stage coefficients are large and very highly statistically significant. OLS estimates for both linear (Column 3) and tobit (Column 5) model are downward biased, wrong-signed, and very small and statistically insignificant in magnitude up to age 9. The OLS and Tobit specifications do not yield significantly different results owing to the small numbers of zero's in the work variables in the pooled NLSY sample. The IV estimates suggest an interesting result in the pattern of hours worked. A mother of a 8 year old child, who delayed school entry, works about 4 hours more per week as compared to a mother whose child went to school at age 5. This is true for mothers of 9 year old children as well. By the time the child is 9 years old, these women are working 3 hours more per week relative to the mothers whose child did not delay. The estimate using the Tobit specification yield even larger coefficients, roughly 4 hours, and are almost three times the standard error. These numbers translate to approximately a 12 percentage point increase in baseline hours worked per week by mothers of 8 and 9 year olds who delayed school entry. The positive effect of delayed enrollment on hours worked becomes statistically insignificant by the time the child is 10 years old.

Table 10: Effect of Delayed School Attendance on Usual Hours Worked per Week by Married Mothers of 7 to 10 Year old Children (NLSY)

Age of Child (1)	First Stage (2)	OLS (3)	2SLS (4)	Tobit (5)	IV Tobit (6)	Observations (7)
Age 7	0.58***	-0.91	0.66	-0.78	0.87	2177
	(0.03)	(0.71)	(1.72)	(0.70)	(1.75)	
Age 8	0.53***	-0.73	4.13**	-0.75	4.31**	1994
	(0.03)	(0.81)	(1.97)	(0.79)	(1.85)	
Age 9	0.58***	-0.76	3.12**	-0.57	4.17***	1879
	(0.03)	(0.86)	(1.45)	(0.82)	(1.40)	
Age 10	0.55***	-2.04**	-1.89	-1.94**	-1.29	1659
	(0.03)	(0.85)	(2.11)	(0.82)	(1.99)	

This result points towards intertemporal substitution in labor supply and in particular towards wealth effects associated with lower lifetime wealth (figure 1). Though the hours variable in the NLSY is the cleanest variable for this analysis, I study two other measures of labor supply to further investigate this effect.

Table 11 reports estimates from two more labor supply measures; employment status during the interview week and proportion of weeks worked since last interview. Once again, the OLS estimates are very small in magnitude, often wrong signed and very imprecise. The

IV estimates suggest that delayed school enrollment has long run implications for maternal labor supply. In particular, I find that a one year delay in school entry raises the probability of employment of a married mother by 22 percent when the child is in school at age 7. This translates to a increase of 36 percentage points over the mean employment. There is a 26 percentage point increase in proportion of weeks worked since last interview. In the contemporaneous labor supply estimates using the census, I find that weeks of work are affected only for women with additional younger children. Due to the small sample size in the NLSY, I cannot separate married women by number of younger children in the household. This makes it difficult to determine who is driving the results in Table 11, i.e., whether it is being driven by married women with or without additional younger children in the household. Regardless of this sample size limitation, table 11 shows that the large positive effects on maternal employment continue to persist for older children as well.

Table 11: Effect of Delayed School Attendance on Employment and Weeks Worked by Married Mothers of 7 to 10 Year old Children (NLSY)

Age of Child (1)	First Stage (2)	OLS (3)	2SLS (4)	Tobit (5)	IV Tobit (6)	Observations (7)
			I. Employment			
Age 7	0.59***	0.04*	0.22***			2874
	(0.02)	(0.03)	(0.05)			
Age 8	0.56***	-0.02	0.02			2629
	(0.03)	(0.03)	(0.06)			
Age 9	0.57***	0.04	0.17***			2392
	(0.03)	(0.03)	(0.07)			
Age 10	0.54***	-0.00	0.14**			2054
	(0.03)	(0.03)	(0.07)			
			II. Weeks Worked			
Age 7	0.59***	0.01	0.15***	0.01	0.19***	2871
	(0.02)	(0.02)	(0.04)	(0.03)	(0.05)	
Age 8	0.56***	0.01	0.08*	0.01	0.12**	2623
	(0.02)	(0.02)	(0.05)	(0.03)	(0.06)	
Age 9	0.56***	0.01	0.16***	0.01	0.20***	2399
	(0.03)	(0.02)	(0.05)	(0.03)	(0.06)	
Age 10	0.53***	-0.02	0.12*	-0.01	0.17**	2056
	(0.03)	(0.03)	(0.07)	(0.03)	(0.08)	

Overall, I find evidence to support the intertemporal substitution hypothesis. Using the census results I find that a child care subsidy, in the form of free or subsidized kindergarten, increases a mother's net wage and thereby increases her labour supply relative to mothers who do not receive this subsidy until a year later. When I look at older age groups, I find that mothers of delayed enrollers have higher labor supply compared to mothers of early enrollers. This cross-sectional approach can be interpreted in a life cycle context as discussed in Section II. For the census results, the IV estimates identify a combination of wealth effects and intertemporal substitution effects corresponding to period t in figure 1. On the other hand, the higher labor supply estimates from the NLSY can be attributed to a pure wealth effect associated with lower lifetime wealth for mothers of delayed enrollers (i.e. period $t+1$ in figure 1).

5.3 Younger Than Five Year Old Children

An alternative explanation for my results would rely on a static labor supply model with a mechanical savings rule.[7] More specifically, a woman whose child delays entry into school (or the 'constrained' woman) works less and has lower income and savings (assuming they save a constant fraction of income) than the 'counterfactual' woman who is not constrained by the law. Since leisure is a normal good, we would expect constrained women to work less in future due to lower wealth and non labor income. Though my results would be consistent with this interpretation of the static theory, I briefly discuss some evidence which strengthens the life cycle interpretation of my results.

If individuals make labor supply decisions according to the life cycle model, anticipated changes in economic and demographic factors should be factored into current labor supply decisions. In the context of the present study, comparing women whose youngest child has not yet reached school enrollment age would allow me to test this. Women who can enroll their children in school at an earlier age will have higher lifetime wealth and therefore, if leisure is a normal good, they must work less (relative to the constrained mothers) in years before their child is age eligible. On the other hand, if women make labor supply decisions according to the static model, we should not find any difference in labor supplied among the two groups of women in years before their child is eligible to start school. To do a formal instrumental variable analysis, I would need data on the actual entrance age of younger

[7] I thank Jonah Gelbach for pointing this out and suggesting a test of this alternative hypothesis.

children. Since I am looking at children who have not yet started school, I cannot do a formal test. However, I can use the census to compare summary statistics for the sample of mothers of 3 and 4 year old children born before and after the state kindergarten entrance age cut-off. Table 12 compares means and standard deviations (in parenthesis) for measures of labor supply between mothers whose youngest child is born before the cut off and those whose child is born after the state school entry age cut off date. Since I am looking at three and four year olds in the 1980 census, I merge the data with state laws from 1980 to 1982 i.e. the laws that would be in place in the year the child turns 5.

Table 12: Summary Statistics: Labor Supply Measures for Mothers of 3 and 4 Year Olds

	Hours/Week	Worked in 1979	Labor Force Status	Weeks in 1979	Sample Size
All					
Born before the cut off	17.63	0.53	0.46	19.04	137022
	(18.96)	(0.50)	(0.50)	(21.93)	
Born after the cut off	18.54	0.55	0.47	19.83	22783
	(19.25)	(0.50)	(0.50)	(22.06)	
t-test	-6.71	-5.58	-1.67	-5.01	
	(0.000)	(0.000)	(0.095)	(0.000)	
Married					
Born before the cut off	16.74	0.51	0.44	18.26	112614
	(18.67)	(0.50)	(0.50)	(21.67)	
Born after the cut off	17.32	0.52	0.44	18.70	19301
	(18.94)	(0.50)	(0.50)	(21.77)	
t-test	-4.02	-2.91	0.045	-2.59	
	(0.000)	(0.004)	(0.964)	(0.010)	
Single					
Born before the cut off	21.75	0.60	0.56	22.64	24408
	(19.72)	(0.49)	(0.50)	(22.73)	
Born after the cut off	25.30	0.68	0.62	26.10	3482
	(19.55)	(0.47)	(0.48)	(22.56)	
t-test	-10.0	-9.2	-6.7	-8.4	
	(0.000)	(0.000)	(0.000)	(0.000)	

I find that all mothers of three and four year old children who are born before the state cut off (i.e. who are eligible to enroll at age 5) have lower average labor supply. The t-statistic for the test of difference in means is large and the corresponding two-tailed p-value is less than 0.05 for all measures except labor force status. The same result holds for married mothers. However, interestingly, the biggest difference in means corresponds to single mothers for whom all measures of labor supply are significantly lower for mothers of children born before the state cut-off. I also verified these findings in reduced form regressions where I regressed labor supply measures on the instrument (a dummy variable Z_i that takes on a value of one if the law constrained the child to delay entry into kindergarten) and all the control variables used in the baseline regressions.[8] Reduced form regressions yield positive but statistically insignificant coefficients for married women. On the other hand, for single mothers, there is a positive and significant effect of being delayed by law on three of the four labor supply measures. They are 5% percent more likely to be employed in 1979, work about 2 hours more per week and 2.4 weeks more relative to mothers whose child is eligible to enroll in school at age 5. The reduced form regressions and the evidence presented in table 12 further give credibility to my baseline results. At the same time, they also suggest that single and married women are responding to the child care subsidy at different points of time. However, the data and identification strategy used in this paper does not allow me to investigate this further, I leave that to future research.

5.4 Effect of Delayed School Entry on Wages

Next, I estimate the effect of delayed school enrollment on maternal wages to study if the loss in experience translates into wage effects. One problem in empirically estimating a wage function is non random selection into work. Most studies treat non workers as earning zero wages or they drop them from the analysis. In the context of this paper, a potential problem with dropping women who were not working is that the decision to not work may be influenced by the age at which the child went to school. It is possible that the mother of a 'delayed' child decides not to work because she faces lower wages relative to mothers who have worked an additional year because they sent their child to school at age 5. In that case, dropping them from the analysis would bias the IV estimates. Given the sizeable increase in maternal labor supply, selection issues cannot be ignored. To correct for this selection bias, I conduct a wage imputation exercise and compare these estimates to the results obtained by treating non workers as having zero wages.

The longitudinal nature of the NLSY allows me to implement a method to correct for

[8] Not shown here, but available upon request.

the sample-selection bias. I exploit the panel nature of the NLSY to impute wages for those workers who were not working in at least one of the four years of analysis. First, I convert the hourly wage rates of the current or most recent job into real wages in 2000 dollars using the personal consumption expenditure price index. If a woman reports having never been employed, I drop her from the analysis. For women who worked exactly one year during the period when her child was between age 7 and age 10, I compute the percentile rank based on the wage distribution for that year. I assign an imputed wage to all the missing years that corresponds to this percentile ranking in the wage distribution at that point of time. For women who reported working in two or three years out of the four years of analysis, I construct a percentile ranking that is a weighted average of her percentile ranking in the wage distributions of the observed years. The weights correspond to the inverse of the square of the distance between the observed year and the year with the missing information. The implicit identifying assumption is that a person's percentile ranking in the wage distribution does not change when switching employment status.

Table 13: Effect of Delayed School Attendance on Log (Hourly Wage) Earned by Married Mothers of 7 to 10 Year old Children (NLSY)

Age	OLS (Imputed Wage)	IV (Imputed Wage)	Obs	OLS	IV	Observatio
Age 7	0.032 (0.036)	-0.024 (0.074)	2221	0.044 (0.040)	-0.046 (0.080)	2096
Age 8	0.087** (0.037)	-0.038 (0.089)	2149	0.095** (0.044)	-0.002 (0.091)	1931
Age 9	-0.042 (0.041)	-0.050 (0.090)	1952	-0.009 (0.042)	0.009 (0.084)	1816
Age 10	0.076* (0.041)	0.072 (0.093)	1811	0.100** (0.039)	0.045 (0.089)	1608

Table 13 reports estimates obtained using this method and compares them to results obtained from a wage regression without imputation. The OLS estimates suggest that mothers of children who delayed school entry earn higher wages relative to mothers whose child was enrolled in school at age 5. A mother of a 8 year old who delayed school entry earns 9% higher wages (Column 2) as compared to a mother whose child went to school at age 5. This effect disappears when I instrument for delayed enrollment suggesting that OLS is biased upwards. This is what one would expect given that rich parents are more likely to delay school entry and the OLS estimates do not control for this effect. On the other hand, the IV estimates suggest that there is no statistically significant relation between wages and delayed enrollment. The point estimates are very small in magnitude and statistically insignificant for all age groups. However, the sign on the wage coefficient for mothers of 7 and 8 year olds is what one would expect if there are any experience effects. Another interesting result is that the estimates are not highly sensitive to the wage imputation. Thus, selection bias does not seem to be a matter of concern in this analysis.

5.5 'Back-of-the-Envelope' Elasticity Estimates

A study by the U.S. Census Bureau provides some useful statistics that can be used to monetize the implicit child care subsidy to parents due to a year of school enrollment.[9] The study reports various aggregate child care statistics for the period 1984 to 2002 using data from the Survey of Income and Program Participation (SIPP). On average, working mothers, with children younger than age 5, spend $122 in weekly child care payments. Preschool age children, with working mothers, spent on an average 32.5 hours every week in paid child care arrangements (including day care centers and family based day cares). This amounts to an expenditure of $3.75 per hour in child care costs. In addition, data from the National Center for Education Statistics (NCES) suggest that the average length of school day in elementary school is about 6.7 hours in a 180 days school year (approximately 1200 hours in a year). Thus, for mothers who would otherwise not be working, free school for their child amounts to an average child care subsidy of $4522.5. For these women, the subsidy is a pure price effect that induces them to work. However, for mothers who choose to work more than 1200 annual hours, the subsidy has a pure wealth effect and therefore reduces hours of work.

How responsive is maternal labor supply to an increase in net wage due to an implicit

[9] "Who's Minding the Kids? Child Care Arrangements: Winter 2002" *Household Economic Studies*, available at http://www.census.gov/prod/2005pubs/p70-101.pdf

child care subsidy? The challenge in estimation of labor supply elasticities is to separate the part of the labor supply response attributable to intertemporal substitution effects from the part due to wealth effects. To estimate these elasticities, one would ideally estimate the parameters of a structural model in a life cycle setting. A formal analysis of this type is beyond the scope of this paper. However, to compare my results with the literature that directly estimates the elasticities, I do some 'back-of-the-envelope' calculations to get a sense of the labor supply response to intertemporal wage changes.

Note that, if there are some women who are reducing hours because their child is receiving public education, I would be underestimating the intertemporal and uncompensated elasticity. Also, kindergarten is often a mixture of full and half-day. The elasticity estimates, and particularly the wealth effects, will be overestimated if many women chose to send their children to half-day kindergartens. Data from the NCES suggests that in 1980, 30% of kindergartners between the ages of 4 to 6 attended full day kindergarten.[10] However, many half-day kindergartens had inexpensive extended day programs attached to them. So the estimated elasticities could be up to twice the true numbers but more plausibly is somewhere in between.

The change in labor supply in period t can be decomposed into a component due to change in wages holding the marginal utility of wealth constant and a component due to the wealth effect of a parametric permanent shift in the marginal utility of wealth. In terms of elasticities, this can be written as:

$$\eta_t = \sigma + \sigma \frac{\partial \ln \lambda_t}{\partial \ln W_t} = \sigma + \gamma_t \tag{3}$$

Where, η is the own uncompensated elasticity (holding constant initial wealth) of labor supply in period t, σ is the intertemporal elasticity of substitution and γ refers to the wealth effect of a permanent shift in marginal utility of wealth (λ). Since $\gamma_t < 0$, the intertemporal substitution elasticity exceeds the own uncompensated elasticity i.e. $\sigma > \{\sigma + \gamma_t\}$. In the

[10] http://www.childtrendsdatabank.org/pdf/102_PDF.pdf

context of this paper, the own uncompensated elasticity is given by:

$$\eta_t = \frac{d \ln h(t)}{d \ln w(t)} = \frac{d \ln h(t)}{d(subsidy)} * \frac{d(subsidy)}{d \ln w(t)} \tag{4}$$

Where h is the hours worked, w is the wage and '$subsidy$' is the implicit child care subsidy due to school enrollment at age 5. This may be re-written as:

$$\eta_5 = \frac{dh_5}{d(subsidy)} * \frac{1}{h_5} * \frac{d(subsidy)}{dw_5} * w_5 \tag{5}$$

I can estimate this elasticity with all values evaluated at the mean:

$$\eta_5 = 6.8 * \frac{1}{19.8} * \frac{1}{3.75} * 8.3 = 0.76$$

Where $\frac{dh_5}{d(subsidy)} = 6.8$ is taken from the coefficient on usual hours worked per week from Table 5 (column 4), h_5 is the average hours worked per week by married women with five year olds (Table 1) and w_5 is the average hourly wage for the same group of women.

In order to get an estimate of the wealth effect, in principal I would need measures of initial assets A(0), lifetime wage profile, interest rates, rate of time preference and unmeasured characteristics. However, as shown in figure 1, the effect of an increase in net wage on labor supply at any period following period t may be attributed to a pure wealth effect. Therefore, the wealth elasticity of labor supply using data for mothers of 8 year old children from the NLSY is given by (evaluated at mean):

$$\eta_8 = \gamma_8 = (-4.3) * \frac{1}{34} * \frac{1}{3.75} * 11 = -0.37$$

Where $\frac{dh_8}{d(subsidy)} = 4.3$ is the coefficient, from column 6 in Table 10, on hours worked per week by mothers of 8 year olds. The average hours worked per week for this sample is 34 hours and the average hourly wage is \$11. To get a rough estimate of the intertemporal elasticity, I assume that the wealth effects are the same in period t (corresponding to 5 year olds) and $t+1$ (corresponding to 8 year olds).[11] Thus, substituting the value of η and γ into equation (3), the intertemporal elasticity of substitution, σ, is equal to 1.13. Finally, given the value of η and σ, I can get bounds on the own compensated elasticity ζ (Macurdy, 1981). If leisure is a normal good, then, $\sigma > \zeta > \{\sigma + \gamma_t\}$ which gives bounds on the own compensated elasticity as $1.13 > \zeta > 0.76$.[12]

Blundell and MaCurdy (1999) report the estimates of own wage uncompensated elasticities from various recent studies. They find that the median elasticity among these studies was 0.78 for married women which is comparable to the uncompensated elasticity estimate of 0.76 obtained in this paper. The wealth elasticity estimate of -0.37 is in line with the estimates obtained by several authors for the elasticity of married women's labor supply with respect to nonlabor income (including assets, spouse's income and other nonlabor earnings).[13] Finally, there is a wide array of estimates in the literature for the intertemporal elasticity of substitution ranging from negative values to large positive values. In the seminal econometric research on life-cycle labor supply of married women, Heckman and Macurdy (1980) find that the intertemporal elasticity of substitution for women is equal to 1. However, as a simplifying assumption the Heckman and MaCurdy model assumes the wage profile to be exogenous. This paper corrects for the potential bias due to the wage exogeneity assumption that is commonly made in the intertemporal labor supply literature. My estimates of wage elasticities suggest that previous studies have not been unduly biased by this assumption.

[11] In this setting, a standard time seperable utility function yields the following conditions for the change in labor supply in period $t: dH_t = \lambda_t \frac{dW_t}{U''(H_t)} + \frac{W_t}{U''(H_t)} d\lambda_t$ and $t+1: dH_{t+1} = \frac{W_{t+1}}{U''(H_{t+1})} d\lambda_{t+1}$. Thus, wealth effects would be the same across the two periods if I assume that $U''(.)$ is small and W_t and W_{t+1} are close to each other.

[12] Using the Slutsky equation, the own compensated elasticity is given by:
$\zeta = \frac{W_t}{H_t} \frac{\partial H_t}{\partial W_t}|_U = \frac{W_t}{H_t} \frac{\partial H_t}{\partial W_t}|_{A_0} - H_t W_t \frac{\partial \ln H_t}{\partial A_0} = \sigma + \gamma_t - H_t W_t \frac{\partial \ln H_t}{\partial A_0}$

[13] See for example, Goldin (1990) table 5.2 and Blau and Kahn (2007).

6 Conclusion and Policy Implication

This is the first study that explores the dynamic aspect of the relation between school entrance age and maternal labor supply. I exploit the variation in school entrance ages to study maternal labor supply in an intertemporal framework. The identification strategy relies on comparing labor supply responses, over time, for two groups of women; those whose 5 year olds were enrolled in school and those whose children delayed enrollment. One of the advantages of this strategy is that it gives me a simple mechanism to separate wealth effects and substitution effects.

Using data from the US Census, I find that having a 5 year old enrolled in school increases labor supply measures for married women by between 7 to 34 percentage points. In comparison to the results for married women, single women do not have any statistically significant effect on labor supply. These results are consistent with theoretical models of labor supply where the provision of child care subsidies is expected to increase the labor supply of mothers.

Using a sample of older children from the NLSY, I investigate persistence in employment outcomes for married women whose children delayed school entry. I find evidence consistent with the intertemporal labor supply model. IV estimates imply a 12 percentage point increase in baseline hours worked per week by mothers of 8 and 9 year olds who delayed school entry relative to those mothers whose children were enrolled in school at age 5. This effect is attributed to the wealth effect associated with lower lifetime wealth for mothers of delayed enrollers relative to the other group. Rough calculations yield a uncompensated wage elasticity of 0.76, an intertemporal elasticity of substitution of 1.1 and a wealth elasticity of -0.37.

The choice of the right age at which to send a child to school has been a much debated issue among parents and policy makers. Most of this discussion has emerged in the light of the evidence, by various researchers, that older entrants perform better in test scores and are more equipped to handle the pressure of formal schooling. Though no consensus has yet been reached on this issue, an interesting new dimension to the debate that emerges from this paper is that school entrance laws may affect families in ways other than through child outcomes. In particular, the evidence from this paper shows that maternal labor supply is very responsive to school entrance ages. Moreover, an important result that comes up from my analysis relates to the large long run wealth effects associated with delaying school enrollment. These wealth effects may be especially large for low income families who are also credit constrained. Thus, education policy makers need to keep this aspect in mind while setting the entrance age. One potential area for future work would involve adequate modelling of intertemporal substitution effects in order to evaluate the impact of these policies on parental labor market outcomes.

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