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Barua, Rashmi

Boston University

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

Rashmi Barua[†]

Department of Economics
Boston University

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Abstract

In this paper, 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. To deal with the endogeneity of school attendance age, I exploit the variation in child month of birth and state kindergarten entrance age laws.

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. In contrast to the results for married mothers, I do not find any statistically significant effect on labor market outcomes for single mothers or mothers of 5 year olds with additional younger children. 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. The estimates suggest that delayed school enrollment has long run implications for maternal labor supply. Results point towards significant 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 2005; Berlinski and Galiani 2005; Cascio 2006; Gelbach 2002; Schlosser 2005). 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 (Angrist and Imbens, 1994) even if there is heterogeneity in the entrance age effect.

This paper also contributes to the literature that uses natural experiments and differences-in-differences methods to study intertemporal labor supply¹. 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 or mothers with younger than five year old children. 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 suggest significant intertemporal substitution in labor supply. In particular, when evaluated at age 7 to 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 a particular emphasis on implications for education policy .

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

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, 1995,1996; 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.

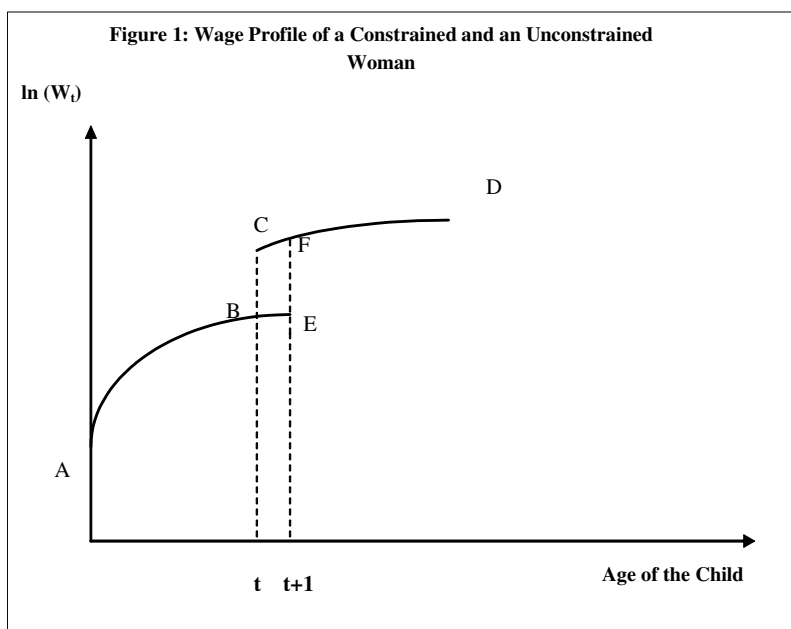
In this paper, I propose a new framework to study the intertemporal labor supply hypothesis. A child care subsidy, in the form of free or subsidized kindergarten, increases a mother's effective wage at the employment margin. At the same time, there exists a lot of variation in the age at which a child 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.

In a recent study, 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 leisure².

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

²For 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. Thus 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.

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 exactly 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 in the figure below that 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.



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). In period $t + 1$, corresponding to the year the child turns 6, the constrained mother also receives this subsidy and her wage profile shifts up by the same amount (the curve AEF D). Note that an implicit assumption in this

analysis is that there is no loss in human capital accumulation. 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 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 oriented women may be sending 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 across states and over time. Cascio (2006) 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 (2005) 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 amongst more educated mothers. Berlinski and Galiani (2005) provide evidence of increased maternal employment in Argentina as a result of large construction of pre-primary school facilities. Baker, Gruber and Milligan (2005) 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. 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.

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 that 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.

The validity of quarter of birth as an instrument has been criticized on grounds of potential failure of the exclusion restriction (Bound and Jaeger, 2000). 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, several studies that estimate the patterns in birth seasonality find that births are highly seasonal with great variation in the timing of seasonal patterns across populations (Lam and Miron, 1991). Bound and Jaeger (2000)

present evidence in favor of correlations between season of birth and family background, education, and earnings. Bobak & Gjonca (2001) find that the magnitude of seasonal variation in births was particularly strongly associated with maternal socio-demographic characteristics. This evidence suggests that maternal employment may be directly related to month of birth of a child. For instance, 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). Similarly, women in professional jobs may avoid births during the end of the fiscal year when the work load is very heavy.

In this paper I propose an identification strategy that is not subject to the above criticism. To identify the causal effect of school entrance age, I exploit the exogenous variation in child month of birth and state kindergarten entrance age laws.

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

$$Y_{it} = \alpha_t S_i + X_i' \beta_t + \gamma_t M_i + \delta_t R_i + \epsilon_{it}, \quad t = 5, 7, \dots, 10 \quad (1)$$

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 dummies 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 demographic 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:

$$S_i = \pi_t Z_i + X_i' \lambda_t + \varphi_t M_i + \theta_t R_i + v_{it}, \quad t = 5, 7, \dots, 10 \quad (2)$$

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 (Bedard and Dhuey, 2006; Cascio and Lewis, 2005; Datar, 2005; Elder and Lubotsky, 2006; McCrary and Royer, 2005). However, these studies have looked at the effect of school entry age on child outcomes. 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 (2007), in the presence of heterogeneity in treatment effects it is not clear which treatment parameter those studies are able to identify. 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 1st, 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 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 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 to 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.

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 to 21 as on 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.

Following my working paper (Barua and Lang, 2007), 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 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 (1st 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 5³.

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 while computing weeks worked. Those respondents who show some percentage of weeks unaccounted 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 downloaded from <http://www.bea.gov/>.

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 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.

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

³Presence 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.

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.

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

The first stage relation between the instrument and the endogenous variable 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 of the order of 0.335. These coefficients are very highly statistically significant. The first stage F-statistics is high, 159.4 for the regressions on single women and 253.6 for the married women regressions. In addition, the first stage R-squared are of the order of 0.145 and 0.225 for regressions on single and married women. The first stage regression estimates also suggest that controlling for everything else, 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 weak instrument problem should not be a concern in the analysis.

Tables 4 and 5 report the results from the regression of a 5 year olds school attendance on different measures of maternal labor supply for married mothers whose youngest child is age 5. I consider four measures of labor supply, namely, employment in 1979, weeks worked in 1979, usual hours worked per week and labor force participation⁴. 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.

In table 4, employment in 1979 is regressed on the school enrollment dummy. I tried

⁴Note that two of the dependent variables are maternal labor supply measures for the year 1979. 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.

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, I control for quarter of birth effects as well as state fixed effects. The reported heteroskedasticity robust standard errors are clustered by state times quarter of birth. 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 18 percentage point increase in baseline participation (the mean employment for this group is 59 percent). Comparing these results to column (3), where I do not control for birth quarters, shows that IV estimates that do not control for quarter of birth may be biased downwards.

In table 5, the first row looks at the effect of school attendance on usual hours worked per week by the mother. I estimate both a linear model and a tobit model to take into account the censored hours data. Again I find that OLS is 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.

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 residuals 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 which suggests that endogeneity may not be an 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 of a 5 year olds school enrollment on the labor supply measures for single women.

An obvious question that arises from these estimates is related to the difference in IV estimates for married versus single women. 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; Michalopoulos et. al. 1992). Kimmel (1998) compared the elasticity of labor supply to child care costs of married and unmarried women and found single women's employment elasticity to be lower than married women's. There are at least three potential reasons why one would expect the labor supply of single women to be relatively inelastic.

First, 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, 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. Cash benefits under AFDC are restricted to single-parent families and families where one parent is not biologically related to the children. Two-parent families can qualify for AFDC if both parents are unemployed. The AFDC welfare system causes the budget constraint of single mothers to become non linear at a threshold number of hours of work, H^* . When the mother is unemployed, her family collects welfare benefits such as AFDC, Medicaid, and food stamps. As she begins to work, however, her AFDC and other benefits are taxed away at a high rate. Beyond a threshold number of hours, H^* , she loses AFDC eligibility status. This could be another explanation in favor of the results produced in this paper. 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.

In addition, a large majority of single mothers are low educated, lack previous work experience and have poor labor market opportunities. Having a child enrolled in school may

not necessarily imply that these mothers have better chances of getting absorbed in well paying jobs. Women who anticipate that they will have no more income by working, have little incentive to work.

A good robustness check for whether the instrument identifies the effect of school enrollment on labor supply measures is to study the behavior of mothers who have younger children at home. Women who have younger than school age children at home should not be as affected by the school attendance of her five year old. Next, I estimate the effect of a five year olds school attendance on the labor supply of married women with additional children who are younger than five years of age. Table 8 shows the coefficients from 2SLS 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. Column (2) reports the 2SLS estimates while columns (3) and (4) report estimates from the 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. 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).

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 or mothers with younger than five year old children.

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 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 children⁵. Column (1) reports

⁵I replicated the analysis described in this section for single women from the NLSY. I get extremely

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 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.

The effect of delayed school enrollment on usual hours worked per week in the main job is shown in Table 10⁶. Each row corresponds to the age of the child for whom the analysis is done. 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 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

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.

⁶The complete tables of results including the coefficients on demographic covariates are available on request

by the time the child is 10 years old.

This result points towards intertemporal substitution in labor supply and in particular towards significant 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. The large positive effects on maternal employment continue to persist for older children as well. A potential concern with the weeks worked variable is that it refers to weeks worked since last interview which may be subject to recall bias even after taking into consideration all the unaccounted weeks. Similarly, the employment status variable refers to the respondents employment during the interview week. There may be selection problems with both of these variables that may bias the estimates. To check the robustness of these results, I look at the effect of delayed school enrollment on labor supply of married mothers of 8 year olds from the Census.

I choose to look at 8 year olds for three reasons. First, by the age of 8 all children must be enrolled in school and so there should not be any selection bias. The second reason is to minimize measurement error while computing the entrance age. The census does not report school entrance age of children. I use the highest grade completed and the age of the child as of Census day, April 1st 1980, to infer the entrance age assuming that no child repeats a grade. However, entrance age computed using this method will be distorted because of prevalence of grade repeaters. Several studies have found that younger school entrants are more likely to have repeated grades (Elder and Lubotsky, 2007; Barua and Lang, 2006). If there is a large fraction of children who repeat grades then I will incorrectly specify them to have delayed entry and this would bias the estimates. To minimize the error due to this assumption, I look at the earliest age when all children must be in school. Finally, I look at 8 year olds because this is the age group for which I find a significant increase in hours worked in the NLSY results.

I study the effect of delayed school entry on maternal labor supply when the child is 8 years old. I instrument for the endogenous explanatory variable, delay, using the state law and quarter of birth of the child as specified in equation (2). The sample consists of only

those children for whom the state of birth and the state of residence at age 8 are identical. This ensures that there is no mobility across states and so I can assign the correct state cut-off to the child. In addition, I restrict the sample to married mothers whose 8 year old is her youngest child. Results from OLS and 2SLS estimates of a regression of labor supply on "delay" are reported in table 12.

OLS is downward biased and wrong signed for all four labor supply measures. IV estimates imply that there is a positive effect on labor supply confirming the NLSY results. However the estimates are very imprecise and, looking at the confidence intervals, I cannot reject the null. The statistical insignificance of the delay variable may simply reflect measurement error, rather than the absence of a true effect on maternal labor supply. In fact in the presence of grade repeaters, the IV would be biased downwards suggesting that the coefficients would be even larger in magnitude.

Overall, I find strong 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 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 the sample-selection bias. The proposed imputation technique is similar in spirit to Neal (2004)⁷. 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 as 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 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.

5.4 Elasticity Estimates

A recent 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 enrollment⁸. The

⁷Following Neal (2004), several authors have used panel data to carry out wage imputations to estimate median wage regressions. I use a variant of this technique to estimate OLS and IV regressions.

⁸"Who's Minding the Kids? Child Care Arrangements: Winter 2002" *Household Economic Studies*, available at <http://www.census.gov/prod/2005pubs/p70-101.pdf>

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) suggests 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)⁹. 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 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. It should be kept in mind 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.

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:

$$\eta_t = \sigma + \sigma \frac{\partial \ln \lambda_t}{\partial \ln W_t} = \sigma + \gamma_t \quad (3)$$

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

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

⁹http://nces.ed.gov/surveys/pss/tables/table_15.asp

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:

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

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 $\frac{dh_5}{d(\text{subsidy})} = 6.8$ 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 $\frac{dh_8}{d(\text{subsidy})} = 4.3$ 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)¹⁰. 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 $1.13 > \zeta > 0.76$ ¹¹.

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

¹⁰In this setting, a standard time separable 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 $U''(\cdot)$ is small and W_t and W_{t+1} are close to each other.

¹¹Using the Slutsky equation, the own compensated elasticity is given by:
 $\zeta = \frac{W_t}{H_t} \frac{\partial H_t}{\partial W_t} |U = \frac{W_t}{H_t} \frac{\partial H_t}{\partial W_t} |A_0 - H_t W_t \frac{\partial \ln H_t}{\partial A_0} = \sigma + \gamma_t - H_t W_t \frac{\partial \ln H_t}{\partial A_0}$

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)¹². 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.

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 and married women with younger children 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 an uncompensated wage elasticity of 0.76, an intertemporal elasticity of substitution of 1.1 and a wealth elasticity of -0.37.

¹²See for example, Goldin (1990) table 5.2 and Blau and Kahn (2007).

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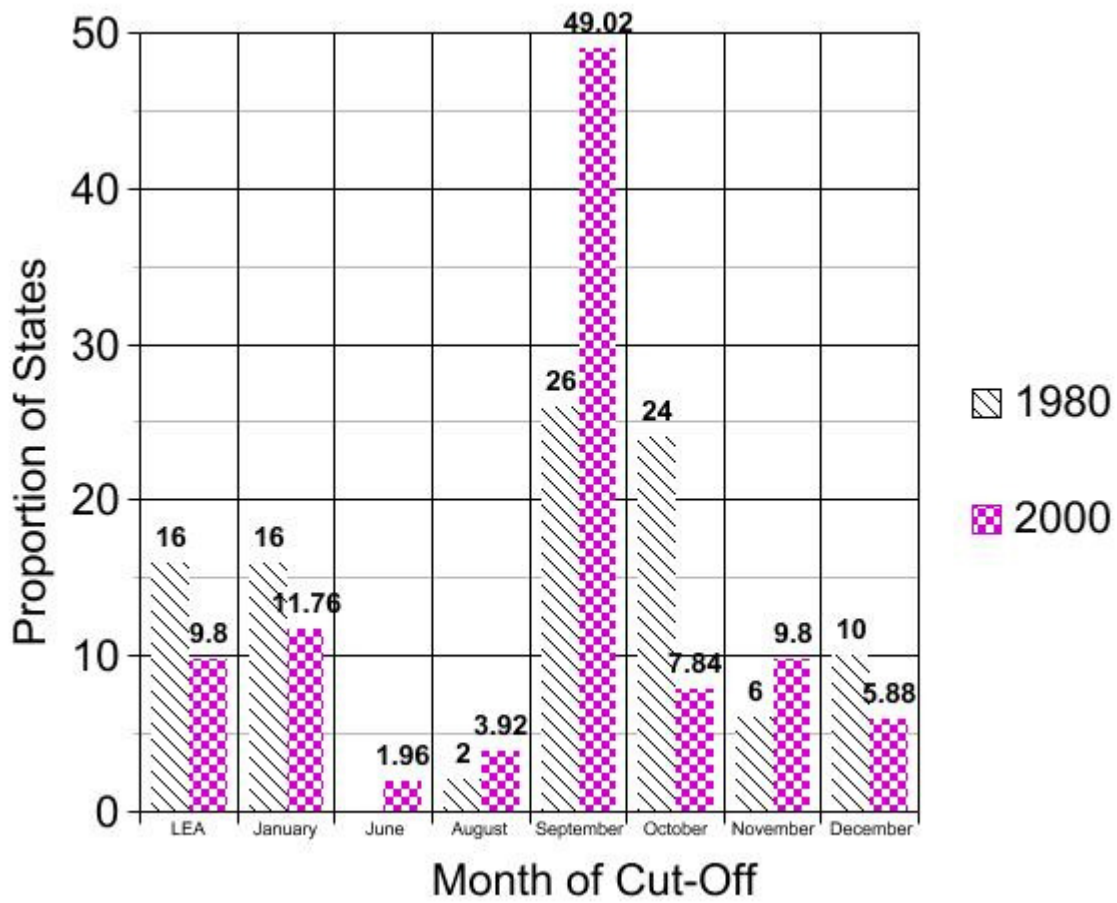
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Figure 2: Proportion of States by Kindergarten Cut-Off Month, 1980-2000



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

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

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.

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*** (0.021)	0.289*** (0.023)	0.335*** (0.034)
Quarter of Birth 2	-0.006 (0.005)	-0.012** (0.005)	-0.005 (0.004)
Quarter of Birth 3	-0.020*** (0.006)	-0.015** (0.005)	-0.019*** (0.005)
Quarter of Birth 4	-0.063*** (0.015)	-0.046** (0.020)	-0.109*** (0.032)
White	-0.013* (0.007)	-0.002 (0.008)	-0.004 (0.006)
Education (Mother)	0.010*** (0.002)	0.010*** (0.002)	0.009*** (0.001)
# of Adult Family Members	-0.009** (0.003)	-0.004 (0.004)	-0.003 (0.004)
Log (Spouse's Income)	0.002*** (0.000)		0.003*** (0.000)
# Own Children in the Household	-0.001 (0.004)	-0.002 (0.003)	-0.013*** (0.002)
SMSA	0.037*** (0.007)	0.020** (0.009)	0.028*** (0.006)
Household Head	0.000 (0.011)	0.012 (0.011)	-0.010 (0.009)
Age (Mother)	0.022*** (0.003)	0.007 (0.004)	0.023*** (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

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

	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
School attendance	0.062*** (0.014)	0.052*** (0.010)	0.045*** (0.016)	0.106** (0.052)
White		-0.133*** (0.007)	-0.133*** (0.007)	-0.132*** (0.007)
Mothers Education		0.022*** (0.002)	0.022*** (0.002)	0.021*** (0.002)
# of HH Adult Members		0.032*** (0.005)	0.032*** (0.005)	0.032*** (0.005)
Ln(Spouse's Income)		0.006*** (0.001)	0.006*** (0.001)	0.005*** (0.001)
# of own children		-0.028*** (0.003)	-0.028*** (0.003)	-0.027*** (0.003)
SMSA		-0.020*** (0.007)	-0.019*** (0.007)	-0.022*** (0.007)
Household Head		0.103*** (0.016)	0.103*** (0.016)	0.103*** (0.016)
Age		-0.020*** (0.004)	-0.020*** (0.004)	-0.022*** (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

Table 5: Effect of 5-Year Olds Public 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*** (0.453)	4.268** (1.782)	2.766*** (0.740)	6.764** (2.948)
Labor Force Status	0.074*** (0.014)	0.071 (0.059)		
Weeks Worked in 1979	2.924*** (0.539)	1.625 (2.050)	8.082*** (1.428)	4.482 (3.290)
Quarter of Birth Dummies	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes

Sample size is 37246

Table 6: Effect of 5-Year Olds Public 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*** (0.016)	0.079*** (0.015)	0.053* (0.031)	0.021 (0.056)
White		0.079*** (0.013)	0.079*** (0.013)	0.079*** (0.013)
Mothers Education		0.043*** (0.002)	0.044*** (0.002)	0.044*** (0.003)
# of Adult HH Members		0.014** (0.006)	0.014** (0.006)	0.014** (0.006)
# of own children		-0.045*** (0.006)	-0.045*** (0.006)	-0.045*** (0.006)
SMSA		-0.042*** (0.012)	-0.041*** (0.012)	-0.041*** (0.012)
Household Head		0.079*** (0.015)	0.079*** (0.015)	0.079*** (0.015)
Age		0.024*** (0.007)	0.025*** (0.007)	0.025*** (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 Public 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*** (0.654)	0.187 (2.919)	3.849*** (0.949)	0.247 (3.721)
Labor Force Status	0.082*** (0.018)	- 0.028 (0.075)		
Weeks Worked in 1979	3.996*** (0.786)	1.656 (3.433)	10.287*** (2.070)	1.426 (4.263)
Quarter of Birth Dummies	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes

Sample Size = 10700

Table 8: Effect of 5-Year Olds Public 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*** (0.009)	0.052 (0.036)		
Usual Hrs Worked/Week	1.253*** (0.360)	1.096 (1.286)	3.431*** (0.787)	3.517 (2.743)
Labor Force Status	0.052*** (0.010)	0.050 (0.034)		
Weeks Worked in 1979	1.942*** (0.378)	4.124*** (1.521)	6.452*** (1.175)	3.767 (2.469)
Quarter of Birth Dummies	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes

Sample size = 36941

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

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

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*** (0.03)	-0.91 (0.71)	0.66 (1.72)	-0.78 (0.70)	0.87 (1.75)	2177
Age 8	0.53*** (0.03)	-0.73 (0.81)	4.13** (1.97)	-0.75 (0.79)	4.31** (1.85)	1994
Age 9	0.58*** (0.03)	-0.76 (0.86)	3.12** (1.45)	-0.57 (0.82)	4.17*** (1.40)	1879
Age 10	0.55*** (0.03)	-2.04** (0.85)	-1.89 (2.11)	-1.94** (0.82)	-1.29 (1.99)	1659

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)
<u>I. Employment</u>						
Age 7	0.59*** (0.02)	0.04* (0.03)	0.22*** (0.05)			2874
Age 8	0.56*** (0.03)	-0.02 (0.03)	0.02 (0.06)			2629
Age 9	0.57*** (0.03)	0.04 (0.03)	0.17*** (0.07)			2392
Age 10	0.54*** (0.03)	-0.00 (0.03)	0.14** (0.07)			2054
<u>II. Weeks Worked</u>						
Age 7	0.59*** (0.02)	0.01 (0.02)	0.15*** (0.04)	0.01 (0.03)	0.19*** (0.05)	2871
Age 8	0.56*** (0.02)	0.01 (0.02)	0.08* (0.05)	0.01 (0.03)	0.12** (0.06)	2623
Age 9	0.56*** (0.03)	0.01 (0.02)	0.16*** (0.05)	0.01 (0.03)	0.20*** (0.06)	2399
Age 10	0.53*** (0.03)	-0.02 (0.03)	0.12* (0.07)	-0.01 (0.03)	0.17** (0.08)	2056

Table 12: Effect of Delayed Enrollment on Labor Supply Measures of Married Mothers of 8 year olds (no Younger Children)

	(1) OLS	(2) IV	(3) Tobit	(4) IV Tobit
Worked in 1979	-0.009 (0.008)	0.009 (0.037)		
Usual Hrs Worked/Week	-0.249 (0.360)	1.414 (1.370)	-0.410 (0.787)	1.449 (2.129)
Labor Force Status	-0.007 (0.008)	-0.039 (0.034)		
Weeks Worked in 1979	-0.988*** (0.369)	1.002 (1.82)	-2.429** (0.998)	1.322 (2.340)
Quarter of Birth Dummies	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes

Sample size = 29053

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

Age of Child	OLS (Imputed Wage)	IV (Imputed Wage)	Observations	OLS	IV	Observations
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