What Took You So Long? The Short And Longer-Run Effects of Public Kindergarten on Maternal Labor Supply and Earnings

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This version: January 26, 2015

Preliminary and incomplete, a more recent version is available from
https://www.dropbox.com/s/ozqkqsyxnhby96s/jmp_Soldani.pdf?dl=0

Abstract

I compare the evolution of maternal labor supply and wages in the first six years after kindergarten, between women whose child started kindergarten at five and those who did not. Identification exploits geographical and inter temporal variation in the cutoffs which determine eligibility to enroll. Labor market participation (LFP) is up to 7 percentage points lower among mothers of non-enrolled children and it takes them up to five years to close the LFP gap. Furthermore, they are 10% more likely to have additional children within the first two years. The average wages for mothers of enrolled children are lower in the short-run, but higher in the medium run. Within a search model, this can be explained by a combination of self selection and positive returns to experience. Among teen mothers, kindergarten has a positive impact on school enrollment (+16%).

1 Introduction

The gap between male and female labor force participation (LFP) in the US is mainly driven by the low participation rate of women who have children. At age thirty, for example, the difference in LFP between women with children and those without children accounts for about 80% of the 9

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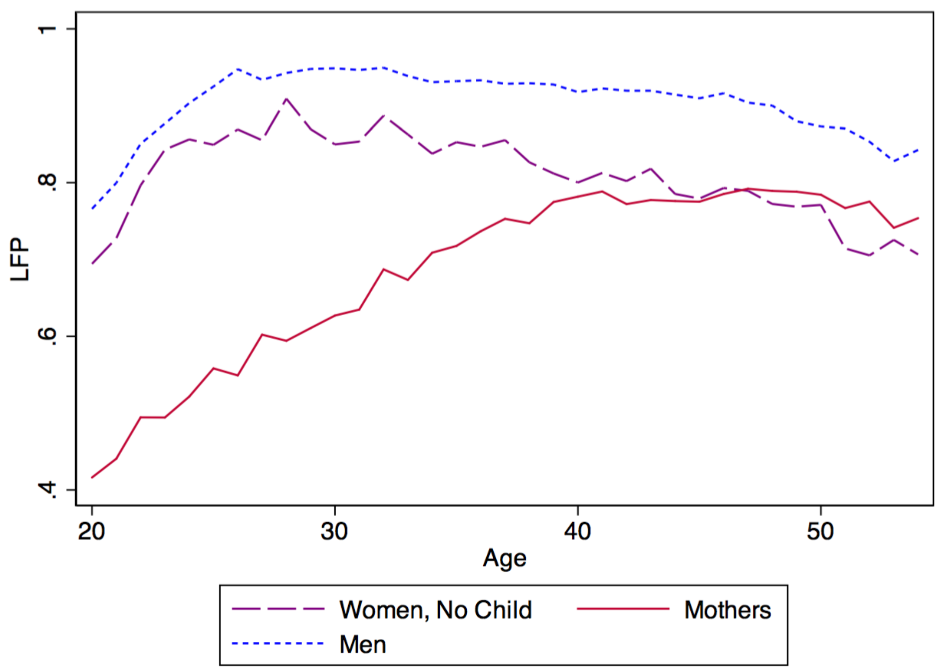
percentage points gap between male and female.[1] Figure (1) shows that this fact is true of other age ranges as well.

Figure 1: Female Labor Force Participation, By Presence of Child



Data: 2012 American Community Survey.

Survey evidence suggests that some important portion of the gap is driven by the fact that mothers face high opportunity costs of working (or engaging in other activities) because they need to find alternative arrangements for their children while they are at work. How much of the gap between mothers and non-mothers LFP could be bridged by providing such arrangements at little or no price? Would this have longer term repercussions? How may other important outcomes, e.g. fertility and human capital, be affected by such policies?

This paper attempts to answer these important questions by comparing mothers who differ in their children's eligibility to enroll in public kindergarten at age five. I show that the differences induced by access to public kindergarten are substantial and long lasting. The LFP of mothers whose child is eligible to enroll is some 0.7 percentage points higher. Furthermore, it takes at least five years for the gap in LFP to close. This delay has an effect on (gross) earnings: eligible mothers who

[1] Author's calculations based on 2012 American Community Survey.

enroll their children earn almost $10,000 more in the first year and a total of $39,000 extra within the first six years. I show that 5% of the difference in earnings is due to higher wages, whereas 95% is due to higher LFP. I also show that enrollment in kindergarten has a negative impact on subsequent childbearing and that it increases the school enrollment rate of teen mothers. An eligible teen mother is 16% more likely to be enrolled in school than a non-eligible teen mother.

My findings suggest that traditional short-run evaluations significantly underestimate the impact of public kindergarten. Using my estimates for total average wage earnings as a measure of the increase in production, the long-run impact is about four times higher than the short-run estimate.[2] The figure might potentially be higher if the benefit of higher wages (Dahl and Lochner (2012), Duncan et al. (2011)) and increased schooling (Blau (1999)) in terms of children's own cognitive and non-cognitive development were taken in account. However, some possible negative impact on children's development and well-being could derive from decreased time spent with parents (Baker et al. (2005)).

The main methodological challenges of estimating the impact of kindergarten are the endogeneity of enrollment and the identification of the counterfactual. Endogeneity issues arise because maternal labour supply and child's enrollment are likely to be jointly determined. My identification strategy overcomes this challenge through the combined use of a child's season of birth and of state-specific cut-off dates as exogenous determinants of eligibility to enroll. In most states, eligibility to enroll is based on a cut-off date. Children who turn five before the cutoff can enroll in public kindergarten, but children who turn five after the cutoff need to wait one more school year. I exploit this exogenous marginal variation in eligibility to estimate the intention-to-treat effect on later outcomes. A similar use of eligibility cut-offs has been suggested also in Gelbach (2002) and Elder and Lubotsky (2009), but the crucial identifying assumption that eligibility is exogenously determined has not been discussed. This paper suggests an indirect test of exogeneity of the child's eligibility to enroll, which in part exploits geographical variation in the cut-offs.

The identification of the counterfactual relies on estimating how many mothers would use private programs if they could not access public kindergarten. I show that 45% of the eligible children who enroll in public kindergarten would otherwise be in private programs (crowding out), while the remaining 55% would stay home or use unofficial childcare arrangements. Under the assumption that the labor supply of mothers who would otherwise use private programs is not affected, I propose a back-of-the-envelope calculation for the costs and the benefits of public kindergarten.

Section 2 highlights in more detail the contribution of my paper with respect to previous litera-

[2]The figure abstracts from the potential reduction in home production, as well as from potential general equilibrium adjustments in the labor market.

ture, Section 3 describes the offer of public kindergarten in the United States, Section 4 suggests a simple theoretical framework to think about how subsidizing childcare might affect the labor supply and the earnings of mothers in both the short and longer run, Sections 5 and 6 describe the datasets used and the empirical methodology, Section 7 presents the main results, Section 8 discusses the importance of crowding out from private childcare services and the robustness of my empirical findings and suggests a back-of-the-envelop calculation of the costs and benefits of public kindergarten, Section 9 concludes.

2 Related Literature

This paper contributes to the literature on female labor supply by considering the effects of public kindergarten, which reduces the opportunity cost of working for mothers. A large literature exists on the determinants of female LFP, which has underlined the importance of the availability of time-saving technologies for home production (Greenwood et al. (2005)), of relative demand of workers in sectors where women hold a comparative advantage (Rendall (2010), Jensen (2012)) and of the allocation of powers within the household (Heath and Tan (2014)). More specifically, the LFP of women who have children has been shown to react to cultural norms (Fernández et al. (2004), Fernández and Fogli (2006)) and to the availability and costs of childcare (Brilli et al. (2011), Del Boca et al. (2009), Del Boca and Vuri (2007), Haan and Wrohlich (2011)).

A few other papers have discussed the impact of public kindergarten on maternal outcomes. My estimates of the short-run effects of public kindergarten are similar in magnitude to the findings of including Gelbach (2002), Cascio (2009) and Berlinski and Galiani (2007). However, this paper extends the previous literature by looking at longer-term dynamics, by looking at heterogeneous effects and by exploiting geographical variation to test the validity of the identifying assumptions.

The growing literature on working status dependence (Blank (1989), Eckstein and Wolpin (1989), Del Boca and Sauer (2009), Heckman and Willis (1977), Nakamura and Nakamura (1985), Card and Hyslop (2005), Francesconi (2002) and Shapiro and Mott (1994)) is definitely relevant for my longer-term analysis, although my findings refer to the very specific subpopulation of mothers of young children.

In the literature, the only other attempt to estimate the persistence of the effects of public school or childcare on maternal labor supply is Nollenberger and Rodriguez Planas, which however focuses on a different research question. They exploit a reform in the Spanish pre-K system to compare the labor supply of treated mothers in the first years after kindergarten with the supply of mothers of children which are about to start kindergarten. Their goal is to measure how long it takes for treated mothers to revert to the levels of mothers of pre-kindergarten children. Because the reform was introduced in all regions and extended to all children of age three, they lack a comparison group to measure differences in outcomes between treated and non-treated mothers in the long-run.

The fact that kindergarten has any positive impact on labor supply might seem at odd with previous papers which have found a null effect of pre-K (Fitzpatrick (2010) and Schlosser (2005)) and childcare subsidizes (Baker et al. (2005) and Havnes and Mogstad (2011)). The difference is actually not surprising, because kindergarten is very different from pre-K and day-care along two important dimensions. First, public kindergartens usually offer full-day programs, whereas full-day pre-K is less common.[3] This is important, because the availability of full-day care is likely crucial for working mothers who face inflexible work schedules. Second, kindergarten was introduced in order to promote child development and readiness for school, rather than to provide subsidized childcare (Cascio (2009)). As a result, take up is extremely high even among children of non-working mothers and eligible mothers are unlikely to perceive kindergarten as inferior with respect to alternative arrangements.[4] Take up of pre-K and nursery programs among children of non-working mothers, instead, is below 50%.[5] It is worth noting that some debate still exists about the benefits and disadvantages of non-maternal care both for children in kindergarten age and below (Baker et al. (2005), Cascio (2008), Elder and Lubotsky (2009), Cascio and Drange et al. (2012)).

3 Public Kindergarten In The United States

Compulsory public education in the United States typically starts at age six. However most states offer universal, non-compulsory, free-of-charge public kindergarten for five-year-old children. Originally, kindergartens were introduced outside of the public school system and they were tuition-based. Local governments at the county and city level started funding kindergarten during the 60s, especially in urban areas, but the main increase in kindergarten offer happened during the 60s and 70s, when all states but Mississippi and North Dakota introduced grants to school districts operating kindergarten (Cascio (2009)). This led to a fast and substantial increase in kindergarten

[3] In 2012, over 77% of five years enrolled in public kindergarten were in full-day programs, while the percentage among 3-4 years-olds enrolled in nurseries/preschools was slightly below 50%. While these numbers result from a combination of supply and demand, the difference is striking. Source: Current Population Survey, Nursery and Primary School Enrollment of People 3 to 6 Years Old, by Control of School, Attendance Status, Age, Race, Hispanic Origin, Mother's Labor Force Status and Education, and Family Income, October 2012).

[4] In the 2012 wave of the Current Population Survey, 99.87% of women who are not in the labor force nonetheless enroll their five-year-old children in public or private kindergarten.

[5] In the 2012 CPS survey, take up of nursery and pre-K programs for four-years-old whose mothers are out-of-the-labor-force is 48%, while overall take up of free pre-K in Georgia and Oklahoma is 50-60%, Fitzpatrick (2010)).

Figure 2: Eligibility Deadlines

[Gr 1]: States with eligibility deadline around September

- AL, AK, AZ, DL, ID, IL, IA, KS, KY, LA, MA, MI, MN, MS, MO, NE, NV, NM, OR, SC, SD, TN, TX, UT, VA, WA, WV, WI, WY

[Gr 2]: States with January 1^{st}/December 31^{st} deadline

- CT, VT

[Gr 3]: States where the deadline has been changed over time

- CA, DC, MD, RI

enrollment rates, on average around 30 percentage points within two years from the introduction of state-funding).

Eligibility regulations are determined at the state or, in same cases, school-district level. The general rule is that only children who turn five within a state-specific deadline are eligible for enrollment in each given school year, while the other children must wait one other year before they are eligible. In most state the deadline has been fixed around the second half of August and the end of September. Because the end of September also happens to be the end of the third quarter of the calendar year, a child's quarter of birth *de facto* determines her eligibility to enroll in public kindergarten in the year she turns five and can therefore be used as an instrumental variable for enrollment in kindergarten (Gelbach (2002) and Elder and Lubotsky (2009)). However, some states adopt a different strategy, choosing deadlines in other months. At the extremes, the deadline is January 1^{st} in Vermont and December 31^{st} in Connecticut. Over the period of interest, several states have moved from a December deadline to a September deadline. The adoption of different deadlines across states and over time creates variation in the strength of the link between quarter of birth and eligibility. Intuitively, one would expect the link to be weaker the further the distance between the cutoff date and the end of the third quarter. Figure 2 lists the states where the deadline is around the end of the third quarter, those where it is around the end of the year and those where the deadline has been modified over time.

The identification strategy proposed in this paper exploits data from states in all three groups to identify the impact of eligibility for public kindergarten.[6]

4 A Simple Theoretical Framework

I propose an infinite-period search model of labor supply, which is an extension of McCall standard model (McCall (1970)). The model provides a simple and clear theoretical framework to think about the impact of subsidized childcare on maternal labor supply and earnings. The focus is on mothers' choices rather than market frictions, and the model abstracts from involuntary unemployment and treats labor supply and employment as equivalent choices.

Time is discrete and each period represents one year. Agents are infinitely lived, they are born in $t = 0$ and all have one child. Each period while working, mothers incur an exogenous opportunity cost of working, which is meant to capture the price of childcare. To represent the US context, the economy is populated by two types of women, which represent the mothers of children who are eligible for public kindergarten in the year they turn five (type e women) and the mothers of children who are eligible one year later (type n women). Type e mothers have access to subsidized childcare since $t = 0$ and their opportunity cost of working is normalized to zero, $k_t^E = 0, \forall t$. Type u mothers only gain access to subsidized childcare in $t = 1$. In $t = 0$ the opportunity cost of working for a type n mother is κ

$$
\begin{aligned}
k_t^E &= 0, \forall t \\
k_t^N &= \begin{cases} \kappa \text{ if } t = 0, \\ 0 \ \forall t > 0 \end{cases}
\end{aligned}
$$

Each period while unemployed, a mother must decide whether to accept or reject a take-it-or-leave-it wage offer ω, which is randomly and i.i.d. drawn from a distribution $F(\omega)$ over the support $[0, B]$. Recall and on the job search are ruled out and employment is an absorbing state: if a woman accepts a wage offer ω in period τ, she will be employed at any subsequent point in time. To allow for returns to experience, the wage in each subsequent period is $\omega_t = \omega\gamma^{t-\tau}$ where $\gamma > 1, \gamma\beta < 1$ (wages increase with tenure). Entry-level wages are stationary, as all draws in all periods are i.i.d. drawn from the same distribution $F(\omega)$. The lifetime value of accepting an offer ω in τ is $\sum_{t=\tau}^{\infty} \beta^{t-\tau}\gamma^{t-\tau}\omega = \sum_{\tilde{t}=0}^{\infty} \beta^{\tilde{t}}\gamma^{\tilde{t}}\omega = \dfrac{\omega}{1-\beta\gamma}$, where $\tilde{t} := t - \tau$ corresponds to job-tenure or experience

[6] The distribution of the state-specific deadlines is shown in the bubble graph in Figure (10), where the red vertical line corresponds to the end of September. The graph (and the sample from now on) exclude Alaska, Hawaii and the states where the cutoff is district or institute-specific (Massachusetts and Colorado) and those where universal Pre-K exists (Florida, Georgia and Oklahoma).

a. If the offer is rejected, the unemployment benefit b is collected, which can also be interpreted as the utility from leisure in dollar terms.

The maximization problem for a type i mother with offer ω in hand can be represented through the value function $v^i(\omega)$, which represents the optimal discounted stream of future income

$$v^i(\omega) = \max_{A,R} \left\{ \frac{\omega}{1-\beta\gamma} - k_t^i, b + \beta \int_0^B v(\omega')\,\mathrm{d}F(\omega') \right\}$$

For type E mothers, $k_t^i = 0, \forall t$ and the optimal solution is to accept any initial wage offer above the reservation wage $\overline{\omega}$ and reject all lower offers

$$v(\omega) = \begin{cases} b + \beta \int_0^B v(\omega')\,\mathrm{d}F(\omega') \text{ if } \omega \leq \overline{\omega} \\ \dfrac{\omega}{1-\beta\gamma} \text{ if } \omega \geq \overline{\omega} \end{cases} \tag{1}$$

At the reservation wage,

$$\frac{\overline{\omega}}{1-\beta\gamma} = b + \beta \int_0^B v(\omega')\,\mathrm{d}F(\omega'), \tag{2}$$

or

$$\frac{\overline{\omega}}{1-\beta\gamma} = b + \beta \int_0^{\overline{\omega}} \frac{\overline{\omega}}{1-\gamma\beta}\,\mathrm{d}F(\omega') + \beta \int_{\overline{\omega}}^B \frac{\omega'}{1-\gamma\beta}\,\mathrm{d}F(\omega'), \tag{3}$$

A few rounds of algebra lead to

$$\frac{1-\beta}{1-\beta\gamma}\overline{\omega} - b = \frac{\beta}{1-\beta\gamma} \int_{\overline{\omega}}^B (\omega' - \overline{\omega})\,\mathrm{d}F(\omega') \tag{4}$$

where the left hand side represents the cost of searching one more time when an offer $\overline{\omega}$ is in hand and the right side is the expected return from rejecting the offer and searching one more period, in terms of the expected present value for a new draw $\omega' > \overline{\omega}$. Notice that this can be rewritten as

$$\overline{\omega}(1-\beta) + (1-\beta\gamma)b = g(\omega),$$

where

$$g(\omega) = \beta \int_{\overline{\omega}}^B (\omega' - \overline{\omega})\,\mathrm{d}F(\omega')$$

To see that this expression admits one and only one solution, notice that the left hand side is increasing in the reservation wage $\overline{\omega}$ and takes value $b(1-\gamma\beta) < 0$ at $\overline{\omega} = 0$, and that the right hand side is decreasing in $\overline{\omega}$, convex and takes value zero when $\overline{\omega} = B$ and value $\beta\mathbb{E}[\omega] > 0$ at $\overline{\omega} = 0$.[7] Therefore, $\overline{\omega}$ exists and is unique.

[7] The first derivative of the right hand side can be computed using Leibniz' rule, which gives $g'(\omega) = -\beta[F(B) - F(\omega)] = -\beta[1 - F(\omega)] < 0$. The second derivative is $g''(\omega) = -\beta F'(\omega) > 0$. Therefore, $g(\omega)$ is decreasing and convex.

Type N mothers face the same maximization problem, except for period $t = 0$, when they face the extra cost of childcare κ. Their Bellman Equation in t_0 is

$$v_0^N(\omega) \quad = \quad \max\left\{\frac{\omega}{1-\beta\gamma} - \kappa, b + \beta\int_0^B v(\omega')\,\mathrm{d}F(\omega')\right\},$$

but for all $t > 0$ they face the exact same problem as eligible mothers. Because the problem is time-varying, the optimal policy also is. In particular, in any period after the first one, non eligible mothers will follow the exact same reservation wage policy as eligible mothers. But in the first period $t = 0$ rejecting an offer ω at hand has a lower cost for non-eligible others, as it saves them the cost of childcare. The reservation wage $\hat{\omega}$ for $t = 0$ therefore needs to satisfy

$$\frac{\hat{\omega}}{1-\beta\gamma} - \kappa = b + \beta\int_0^B v(\omega')dF(\omega'),$$

where again the left hand side is clearly increasing in the reservation wage $\hat{\omega}$ and the left hand side is

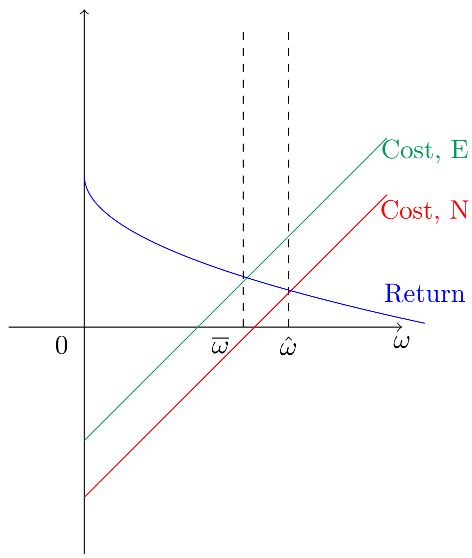
Comparing this to Equation (2), the only difference is the extra $-\kappa$ in the left hand side. Therefore, $\hat{\omega} - \overline{\omega} = (1-\beta\gamma)\kappa > 0$ and $\hat{\omega} > \overline{\omega}$. In the short run, mothers of non-eligible children have a higher reservation wage, because accepting a job offer implies a higher opportunity cost. But in each of the next year, corresponding to the medium and longer run, they have the same reservation wage as eligible mothers. Figure (3) graphically shows that in the short-run non-eligible mothers have a higher reservation wage, as a result of the lower opportunity cost of accepting an offer, which in turn is a result of the higher opportunity cost of working.

The higher reservation wage in turn implies that their hazard of getting a job is also lower in the first period, and their waiting time is higher. The hazard can indeed be written as the probability to get an acceptable offer, that is $P\{\omega > \overline{\omega}\} = 1 - \int_0^{\overline{\omega}} F(w)\,\mathrm{d}\omega$ for type E mothers, and $P\{\omega > \hat{\omega}\} = 1 - \int_0^{\hat{\omega}} F(w)\,\mathrm{d}\omega$ for type N's. and therefore trivially $\hat{\omega} > \overline{\omega}$ implies $P\{\omega > \hat{\omega}\} \geq P\{\omega > \overline{\omega}\}$ [8]

In the medium run, $\forall t > 0$, however, all mothers adopt the same reservation wage strategy and, as a result, one should expect no persistence in the participation and employment, unless eligibility

[8]The model also predicts that non-eligible mothers will be unemployed longer. Let λ be the probability that an offer is rejected and let W be the number of unemployment time periods it takes before the first acceptable offer is drawn. For type E mothers, $\lambda = \int_0^{\overline{\omega}} dF(\omega')$ and the probability to accept an offer in $t = 0$ is $P\{W = 1\} = 1 - \lambda$, while the probability that the first offer is rejected and the second one is accepted is $P\{W = 1\} = \lambda(1 - \lambda)$. More generally, the probability to remain unemployed for the first $n-1$ periods and accept the n^{th} offer for type E mothers is $P\{W = n\} = \lambda^{(n-1)}(1 - \lambda)$, waiting time follows a (shifted) geometric distribution. The expected waiting time is then $\sum_{n=1}^{\infty}(n \cdot P\{W = n\}) = \sum_{n=1}^{\infty} n \cdot (1-\lambda) \cdot \lambda^{(n-1)} = (1-\lambda)^{-1} = \left(1 - \int_0^{\overline{\omega}} dF(\omega')\right)^{-1} = (1 - F(\overline{\omega})^{-1}$. For type N mothers the probability to reject the offer in the first period, in the fifth year of life of the child, is $P\{W \geq 1\} = \int_0^{\hat{\omega}} dF(\omega') = F(\hat{\omega})$ and the probability to reject exactly n offers, for $n > 1$ is $P\{W = n\} = F(\hat{\omega}) \cdot (F(\overline{\omega})^{(n-2)}(1 - F(\overline{\omega}))$. The expected waiting time for type N mothers is then $\sum_{n=1}^{\infty} n \cdot P\{W = n\} = F(\hat{\omega}) + \sum_{n=2}^{\infty} n \cdot (1-\lambda) \cdot \lambda^{(n-2)}$ Because $F(\hat{\omega}) \geq F(\overline{\omega})$, the expected waiting time for mothers of non-eligible children is higher.

Figure 3: Reservation Wages



affects other decisions, such as fertility behavior, which may in turn affect the longer run labor supply. If mothers incur subsequent childbirths, they will face the extra cost of childcare for the first four or five periods of the newborn's life, depending on his/her eligibility for kindergarten in the fifth year.

On the other hand, because of returns to experience, eligibility can have long-lasting effects on wage earnings even absent persistence in labor supply choices. This mechanism is discussed in details in the next section.

Average Earnings

The model offers one possible explanation for the impact of access to subsidized kindergarten on earnings of mothers. Consider the average wage of employed type E mothers. Because any wage offer below $\overline{\omega}$ is rejected, the expected wage of an employed mother in $t = 0$ is

$$\mathbf{E}[\omega|\omega \geq \overline{\omega}, i = E] = \int_{\overline{\omega}}^{B} \omega \, \mathrm{d}F(\omega)$$

while the expected wage of an employed type N mother is

$$\mathbf{E}[\omega|\omega \geq \hat{\omega}, i = N] = \int_{\hat{\omega}}^{B} \omega \, \mathrm{d}F(\omega)$$

Assume for example that the distribution of the wage offers, $F(\omega)$ is uniform and let y_t be the wage in time t and, only for unemployed mothers, let ω_t be the wage offer received in t. In $t = 0$ all employed mothers simply collect their initial wage offer $y_0 = \omega_0$ and therefore

$$\mathbf{E}[y_0^E] = |\mathbf{E}[y_0|\omega_0 \geq \overline{\omega}, i = E] = \frac{B - \overline{\omega}}{2}$$
$$\mathbf{E}[y_0^N] = \mathbf{E}[y_0|\omega_0 \geq \hat{\omega}, i = N] = \frac{B - \hat{\omega}}{2}$$

and the share of employed mothers are $1 - \int_0^{\overline{\omega}} \mathrm{d}F(\omega)$ for type E and $1 - \int_0^{\hat{\omega}} \mathrm{d}F(\omega)$ for type N. Because of returns to experience, in $t = 1$ the wage of mothers who accepted an initial-wage offer ω in $t = 0$ is $\omega\gamma$ and

$$\mathbf{E}[y_1|\omega_0 \geq \overline{\omega}, i = E] = \frac{B - \overline{\omega}}{2}\gamma$$
$$\mathbf{E}[y_1|\omega_0 \geq \hat{\omega}, i = N] = \frac{B - \hat{\omega}}{2}\gamma$$

In addition, in $t = 1$ all unemployed mothers draw a new offer $\omega' \sim F(\omega)$ and everyone adopts the reservation wage policy $\overline{\omega}$, because everyone has access to subsidized childcare ($k_1^i = 0, \forall i$). The expected wage for type E and type N mothers who start working in $t = 1$ is $\frac{B - \hat{\omega}}{2}$. Conditional on non-employment in $t = 0$, the probability of entering employment in $t = 1$ is the same for the two types of mothers, but the share of women who enter employment in $t = 1$ is higher for type N, because more of them start the period unemployed. This is reflected in the average wages

$$\begin{aligned}
\mathbf{E}[y_1^E] &= \frac{B - \overline{\omega}}{2}\gamma \cdot P\{\omega_0 \geq \overline{\omega}\} + \frac{B - \overline{\omega}}{2} \cdot P\{\omega_0 < \overline{\omega}, \omega_1 \geq \overline{\omega}\} \\
&= \frac{B - \overline{\omega}}{2}\gamma \cdot \frac{B - \overline{\omega}}{B} + \frac{B - \overline{\omega}}{2} \cdot \frac{\overline{\omega}}{B} \cdot \frac{B - \overline{\omega}}{B} \\
\mathbf{E}[y_1^N] &= \frac{B - \hat{\omega}}{2}\gamma \cdot P\{\omega_0 \geq \hat{\omega}\} + \frac{B - \overline{\omega}}{2} \cdot P\{\omega_0 < \hat{\omega}, \omega_1 \geq \overline{\omega}\} \\
&= \frac{B - \hat{\omega}}{2}\gamma \cdot \frac{B - \hat{\omega}}{B} + \frac{B - \overline{\omega}}{2} \cdot \frac{\hat{\omega}}{B} \cdot \frac{B - \overline{\omega}}{B}
\end{aligned}$$

Given $\hat{\omega}$ and $\overline{\omega}$ such that $\hat{\omega} > \overline{\omega}$ it is possible to find a γ^* such that $\mathbf{E}[y_1^E] > \mathbf{E}[y_1^N], \forall\gamma \geq \gamma^*$ and $\mathbf{E}[y_1^E] < \mathbf{E}[y_1^N], \forall\gamma < \gamma^*$. If the returns to experience are high enough, the average wage in the medium-run ($t \geq 1$) will be higher among type E mothers, although it is lower in the short-run ($t = 0$). Even more so, if we allow mothers to differ in their ability level and the wage draws to be higher for high ability mothers.

5 Data

My analysis is based on a pooled cross sectional sample of women from the Integrated Public Use Microdata Series of the American Community Survey (IPUMS-ACS).[9] The ACS is a national survey

[9] Only data collected after 2005 is used because of differences in the survey questionnaires, especially with regard to the child's quarter of birth variable.

Table 1: Average Characteristics, Entire Sample

	Mean	Std Deviation
Mothers In The Labor Force	0.70	0.46
Mother is Currently Married	0.76	0.43
Mother Was Married At Birth	0.59	0.49
Mother's Age at Birth	28.71	6.02
Age (mom)	35.88	6.28
Mother is White	0.79	0.41
N. of Children	2.60	1.18
N. of Children age 0-5	0.48	0.70
Mother's Years of Education	13.79	1.83
N. Obs.	861,760	

Sample: all women of age 15-50 with a child of age 4-12 interviewed between 2000 and 2012.

designed by the U.S. Census Bureau, collecting demographic, economic, social and housing data which was previously included in the long format of the decennial census. Each monthly sample includes around 250,000 housing units, but data is only released at the annual basis. Given the mandatory nature of the survey, non-response is very low.

The sample used for the short-run analysis includes all mothers of five-year-old children. The sample used for the longer-term analysis include all mothers of children between age six and twelve. I exclude all women who are older than fifty or younger than sixteen, those affected by disabilities which prevent working activities, those whose child's date of birth data is missing, all non-citizens and those whose hourly wage, if positive, is below the 1^{st} percentile or above the 99^{th}. The observations lost to these selections correspond to 19.57% of the initial sample, bringing the sample size to 861,760 observations. The summary statistics for the entire sample are shown in Table 5.

The LFP in the sample is around 70%, which is pretty high with respect to the average LFP for mothers in the US. The average age at birth is 28.7 years, most of the sample (79%) is of white ethnicity and 76% of mothers are married, but only 59% were married at birth. The average number of children is 2.6 and the average number of years of education is 13.8 years, which, in the absence of class retention, would correspond to completed high school.

The main limitation of the data is the lack of longitudinal data, in particular I cannot observe working status and earnings in the years before the survey, not work experience or job tenure. I

Table 2: Average Characteristics, By Enrollment In Public Kindergarten

	Mean	Std Deviation	Enrolled	Not Enrolled	t-test
Mothers In The Labor Force	0.68	0.47	0.70	0.64	-25.08***
Mother is Currently Married	0.77	0.42	0.73	0.83	50.15***
Mother Was Married At Birth	0.46	0.50	0.44	0.49	18.36***
Mother's Age at Birth	28.94	6.10	28.45	29.79	44.92***
Age (mom)	33.92	6.12	33.43	34.77	44.86***
Mother is White	0.79	0.41	0.76	0.84	41.10***
N. of Children	2.50	1.15	2.49	2.52	5.47***
N. of Children age 0-5	0.55	0.70	0.53	0.59	19.30***
Mother's Years of Education	13.77	1.83	13.60	14.07	51.79***
N. Obs.		178,392	112,744	65,648	-

Sample: all women of age 15-50 interviewed between 2000 and 2012, whose child turns five between six months before September 30^{th} of the year of the interview and six months later.

follow the literature and use the number of years of maximum theoretical experience (age-6-years of education) as a proxy for experience.

6 Empirical Strategy

The goal of the paper is to measure the effects of public kindergarten on maternal outcomes, at different points in time. Estimates based on observed enrollment would be biased because the decision to enroll one's child in public kindergarten might be a result of endogenous selection. Indeed, substantial differences exist between mothers of children enrolled in public kindergarten at age five and mothers of non-enrolled children. Table (6) shows the sample averages and t-test of equal means by enrollment status for five-year-old children. Notice that "non-enrolled children" include children who are not at school as well as those who are enrolled in private kindergarten or pre-Kindergarten institutes.

Mothers of enrolled and not enrolled children differ along several important dimensions. Women whose child is enrolled in public kindergarten are more likely to participate in the labor market, they are just above one year younger, but slightly less educated and less likely to be currently married. Also, a lower fraction of them is white and they are more likely to have additional children under age five. All differences are statistically significant, suggesting that the decision to enroll one's child in public kindergarten varies with observable and possibly unobservable characteristics of the parents. This is the nature of the endogeneity threat in estimating the effect of kindergarten enrollment on maternal outcomes, which my identification strategy aims to overcome.

Because eligibility to enroll a child in public kindergarten in the United States depends on whether the child turns five before a deadline, previous literature suggested the use children's date or quarter of birth as an exogenous source of variation in enrollment (Gelbach (2002) and Elder and Lubotsky (2009)). Since a child's exact date of birth is generally not observable in the data, the literature mostly defines eligibility as turning five in the third or fourth quarter of the year, as opposed to the fourth quarter. I adopt the same definition, although restricting the sample to children born in the third (eligible) or fourth (non-eligible) quarters only yields similar estimates, albeit larger standard errors.[10] The main flaw in this approach is the fact that a child's date of birth might be related to his parents' characteristics, which would be a violation of the exclusion restriction. In other words, the impact of a child's quarter of birth on her mother's labor supply might be the composition of both the impact of the child's eligibility to enroll in kindergarten and the direct impact of her season of birth and the impact of pre-existing characteristics of her mother which led her to give birth in that particular quarter in the first place. Indeed, a lively debate exists on whether parental characteristics might be linked to children's season of birth and whether mothers might use C-sections and other technologies to conveniently pick their children's date of birth (Buckles and Hungerman (2013), Bound and Jaeger (2001), Dickert-Conlin and Elder (2010)). Table (3) compares the characteristics of mothers of one-year-old children, by period of birth, to check whether significant differences exist before the exposure to eligibility which could affect labor supply. The third column reports the two tails t-tests of differences-in-mean.[11] Notice that significant, albeit small, differences exist in mother's ethnicity and completed years of education.

In order to cope with this challenge, I exploit the combination of cross-sectional variation in children's date of birth and the variation of the deadline both across states and over time within states. The effect of eligibility to enroll in public kindergarten can be separated by the direct effect of quarter of birth by comparing the effect of quarter of birth in states where the quarter determines eligibility (Group 1 states in Figure 2) to its effect in states where it does not (Group 2 states in

[10]Notice that this use of quarter of birth is closely linked to the work of Angrist and Krueger (1991), who used quarter as an instrumental variable for years of education among high-school educated youth to estimate the returns to education.

[11]Tests based on quartiles yield similar results.

Table 3: Background Characteristics By Season Of Birth

	Born in July-Sept		Born in Oct-Dec		Difference	p-value
	Mean	SD	Mean	SD		
Mother is in Labor Force	0.640	0.480	0.639	0.480	0.001	0.778
Mother's Years of Education	13.805	1.880	13.725	1.906	0.080	0.003
Mother's Age at Birth	29.490	5.962	29.491	6.085	-0.001	0.968
Mother Was Married at Birth	0.487	0.500	0.486	0.500	0.001	0.763
Proportion White	0.800	0.400	0.791	0.406	0.009	0.000
N. Children	2.083	1.161	2.089	1.181	-0.005	0.447
N. Obs.	83,712[a]		39,919[b]			

[a]Data on father only available for 63,336 cases

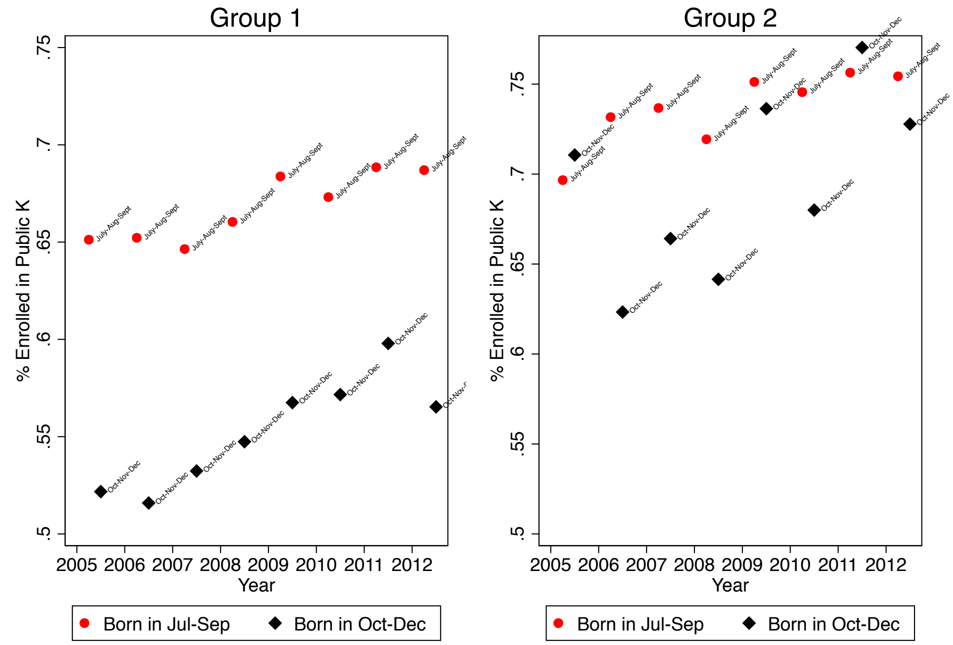
[b]Data on father only available for 29,617 cases.

Figure 2).[12] The intuition for this approach can be more conveyed graphically, through Figures (5) and (??)). Figure (5) reports the enrollment rate for public kindergarten, by survey year, child's quarter of birth and type of state. The red dots represent the enrollment rate of children born in the third quarter, for each survey year, while the black dots are children born in the fourth quarter. The left panel refers to Group 1 states, where the eligibility deadline is around the end of the third quarter, while the right panel shows states in Group 2, where the deadline is either the first or the last day of the year. Notice that in Groups 1 states, in each survey year, quarter of birth appears to have a strong and positive impact on enrollment, while in Groups 2 states the link is blurry. Therefore in Group 2 states any effect of a child's quarter of birth on her mother's labor outcomes is not likely to come from the effect of kindergarten eligibility. This fact can be used to disentangle the impact of eligibility form that of quarter of birth per se.

Figure **??** shows the impact of a child quarter of birth on her mothers repeat's labor supply, again by type of state and survey year. In order to control for other maternal characteristics, the figure has been constructed using data only for white mothers of age 20 to 50, with more than 10 but less than 16 years of education. Notice that the quarter of birth seems to have a positive impact on maternal labor supply in both types of states, but the difference is generally much larger in Group 1 states. Intuitively, the econometric methodology proposed in this paper computes the distance between each pair of blue and black dots in the left panel and then subtracts the distance between

[12]A similar use of control subsamples, where the treatment is expected to have no effect is sometimes used in the development and applied micro literature (Heckman and Hotz (1989), Rosenbaum (1996), Imbens (2004), Duflo (2001), Hoynes et al. (2012) and Heckman et al. (1997)).

Figure 4: Enrollment in Public Kindergarten By Quarter of Birth

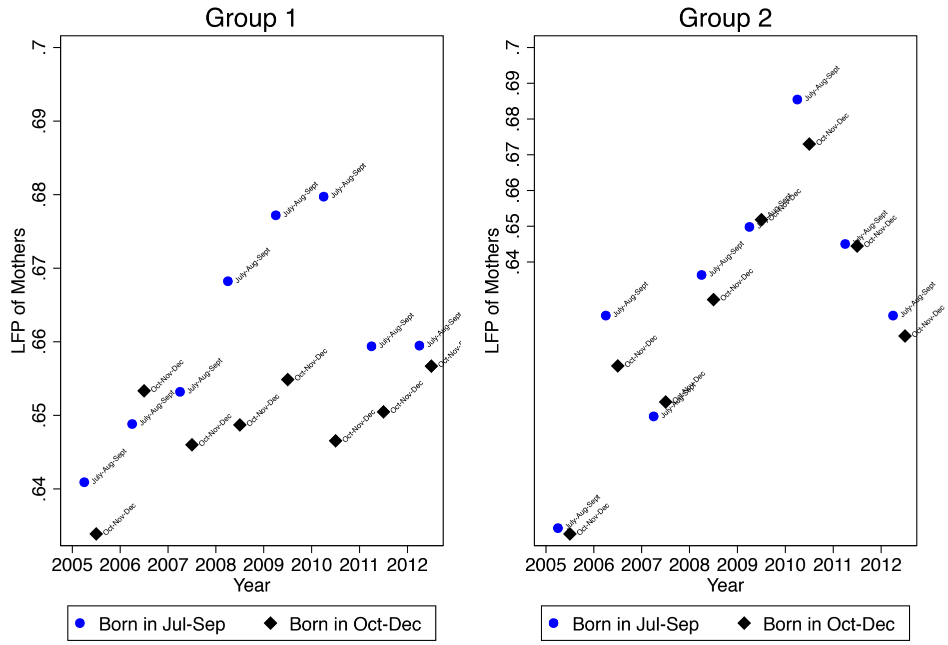


the corresponding pair from the right panel. That is, it estimates the difference in outcomes between mothers of children born before and after the end of the third quarter in Group 1 states net of the difference in outcomes between the same two groups of women in Group 2 states.[13]

Furthermore, the variation of the deadline across time in Group 3 states allows me to control for endogenous timing of birth. In these states, the deadline has been recently changed through reforms. In case mothers had planned the quarter of birth of their children in order to take advantage of the eligibility deadlines in their state, they would have likely done it with respect to the deadline which was in place at the time of conception, rather the new deadline which was introduced through subsequent reforms. Focusing on this subsample thus ensures that the estimated impact is safe from endogenous timing of birth.

[13]When using the interaction of quarter of brith and states as an instrument for enrollment in kindergarten, the validity of the exclusion restriction is crucial. Although this assumption is, by definition, non-testable, an indirect test is sometimes suggested in the applied literature to check whether the instrument (quarter of birth) has any impact on the dependent variable of interest (maternal outcomes), once the endogenous regressor (enrollment in kindergarten) is controlled for. This can be done by estimating the reduced form equation $Y_{it} = \alpha Eligibility_i + \beta Enrollment_i + \varepsilon_i$. The estimates, both with and without additional controls for mother's education, age, race, state and marital status, are shown in Table 10 in the Appendix.The null hypothesis $\alpha = 0$ that the child's quarter of birth has no impact on maternal outcomes other than through kindergarten cannot be rejected.

Figure 5: Mother's LFP By Child's Quarter of Birth



My analysis focuses on the impact of eligibility rather than the impact of enrollment, because any policy intervention is likely to act on the eligibility lever rather than on actual enrollment, since actual makeup clearly is a choice (Currie and Gruber (1996)).[14] Borrowing terms from the quasi-experimental literature, the *treatment* of interest is enrollment in public kindergarten, whereas eligibility can be thought of as the *assignment to treatment*. Therefore, the impact of eligibility is the Intention to Treat (ITT) effect, whereas the impact of enrollment would be the Average Treatment Effect (ATE). It is important to notice that the effect of eligibility is not equivalent to the effect of enrollment (which might be measured through the Average Treatment Effect or ATE), because compliance of treatment with the assignment is not perfect, for two reasons. On the one hand, not all eligible children are enrolled. Some children, irrespective of their eligibility status, stay at home or choose private institutes (*never-takers*) or enroll later ("red-shirts"). On the other hand, it is possible, although not common, for families of non-eligible children to apply for an exception and, if accepted, enroll in public kindergarten (*always-takers*). The presence of defiers (children who would enroll if not eligible, but not enroll if eligible) is instead arguably unlikely. Under standard

[14] Also note that data limitations prevent me from formally estimating the longer run impact of enrollment, as enrollment in kindergarten is only observed for children whose mothers are surveyed in the year of kindergarten, no retrospective information on past enrollment is available.

assumptions, the impact of enrollment on compliers (Local Average Treatment Effect or LATE) can however be estimated by using assignment, that is the interaction of quarter of birth and state specific dummies, as an instrumental variable.[15]

$$LATE_t \quad := \mathbb{E}[Y_{it}(Enr_i = 1) - Y_{iy}(Enr_i = 0)|Enr_i(Elig_i = 1) - Enr_i(Elig_i = 0) = 1]$$

or

$$LATE = \frac{\mathbb{E}[(Y_{it}|, Enr(Elig_i = 1), Elig_i = 1) - (Y_{it}|Enr_i(Elig_i = 0), Elig_i = 0)]}{\mathbb{E}[Enr_i(Elig_i = 1) - Enr_i(Elig_i = 0)]}{}_{16}$$

where the denominator corresponds to the effect of eligibility on enrollment

$$ITT_{1i} = \mathbb{E}[(Enrollment_i|Eligible_i = 1) - (Enrollment_i|Eligible_i = 0)] \tag{5}$$

In reduced form, the LATE can be estimated through the two steps instrumental variable procedure

$$\begin{aligned} Y_{it} &= \alpha_1 \cdot Enrolled_i + \beta X + \varepsilon_i t \qquad (6) \\ Enrolled_i &= \gamma \cdot Eligibility_i + \delta Z_{it} + \mu_i, \end{aligned}$$

The responsiveness of mothers' behavior to subsidized childcare is likely to vary with their income, marital status and educational level and with the presence of adults and of additional younger children in the household. **?** and **?** suggest that returns to experience may differ by educational attainment of the worker. If this is the case, the wage benefits accruing to mothers because of kindergarten-driven increased participation, if any, might also vary with education. The resulting potential heterogeneity in outcomes is investigated by considering the impact for several subsamples of mothers and through quantile regressions, which allow to estimate the impact of kindergarten on the distribution of wages and hours.

7 Results

This section presents the empirical evidence of the short and longer-term effects of eligibility to enroll in public kindergarten on maternal LFP, employment, human capital investments and fertility. The focus is on the intention-to-treat (ITT) effects, but short-term LATE effects will also be briefly discussed.[17]

[15] The standard assumptions required to estimate the LATE are: exclusion restriction, SUTVA, monotonicity of the effect of eligibility on enrollment, exclusion restriction and non-zero average casual effect of eligibility on enrollment.

[17] The longer-term effects n years after exposure to kindergarten eligibility regulations can only be estimated for mothers who are surveyed in the year their children are $5 + n$. Because retrospective information on school and kindergarten enrollment is not available, the LATE effect for these mothers cannot be computed.

Table (4) shows the estimated impact of eligibility on public kindergarten enrollment. The first column reports the estimates for the sample of children who turn five between April of the current year and March of the following. The ITT is computed as the mean difference in enrollment between children who turn five in the second or third quarter (April to end of September) and those whose fifth birthday happens in the fourth quarter of the current year or in the first quarter of the following one (October to March). The difference is estimated controlling for age, race, marital status, number of children and education level of the mother and for state, years and region-year fixed effects. The enrollment rate of eligible children is 24 percentage points higher than among non-eligible children. This corresponds to a 52% increase with respect to the baseline enrollment rate (47%). The coefficient is significant at the 1% level and the F-statistic is 98.81: the null hypothesis of weak instruments can be rejected (Staiger and Stock (1997)). The effect is even stronger if the mother has no younger child. This will be the sample used for all the subsequent estimations.[18]

The sign and size of the other covariates is mostly consistent with previous literature (Herman (2005)). In particular, the use of public kindergarten is lower among women who completed some college education or higher and among those who have additional younger children. The age of the mother does not seem to have any statistically significant effect on the enrollment of the child. The F-statistic for the first stage, 250.82, confirms that the instrument has a strong impact on enrollment.

Maternal Labor Supply

The results in the previous section show that eligibility to enroll has a strong impact on enrollment: how does this affect mothers' LFP decisions? Table (5) answers this question by looking at the impact of both eligibility and enrollment on LFP. Column (A) reports the linear least square effect of enrollment on maternal labor force participation, ignoring the endogeneity of kindergarten for the moment. Under this specification, which is biased if enrollment is endogenous, the LFP of mothers of enrolled children is about 4.6 percentage points higher than the LFP of mothers of non-enrolled children.

The effect is estimated controlling for age (in linear and quadratic form), education level (though five binary variables for middle school or less, junior high or high school, college of less than four years, college of four or more years and graduate school), race (with a binary variable for white race), the number of own children under age 16 and under age 5 who live in the household, marital status (a binary variable for currently married as opposed to never married, divorced, separated or widowed) and age of the mother at childbirth. Additional controls include binary variables for each

[18] Focussing on the differences between children born in the third and fourth quarter only, that is dropping those born in the second, yields similar results, albeit larger standard errors.

Table 4: First Stage: The Effect of Eligibility on Kindergarten Enrollment

	$\beta/[t]$
Child Turned Five in Apr-Sept.[1]	0.239***
	[26.91]
Child Turned Five in Apr-Sept. * {No Younger Child}	0.009*
	[1.74]
Age [of mother]	0.007***
	[3.01]
Age Square [of mother]	-0.000***
	[-4.98]
High School Degree	-0.002
	[-0.13]
Associated Degree	-0.024
	[-1.24]
College Degree	-0.116***
	[-5.45]
Graduate Degree	-0.141***
	[-6.07]
Race=White	-0.078***
	[-7.11]
Number of children in the household	0.005
	[1.25]
Mother Is Currently Married	-0.069***
	[-17.81]
No Younger Children	0.030***
	[9.28]
N. Obs.	173,844

[1] The residual group is that of children who turn five between October of the current year and March of the next year.

[2] The residual group is that of children who turn five between October and December of the current year. In both specification, additional covariates include: mother's age and age square, number of other children in the household, marital status and binary variables for education level, race and presence of younger children.

Table 5: Effect of Child's Eligibility on Mother's LFP

	Effect of Enrollment (OLS)	Effect of Eligibility (OLS)		
	(A)	(B)	(C)	(D)$^{(*)}$
Child Enrolled in Public K	0.046***			
	[18.24]			
Child born in 2^{nd} Q or 3^{rd} Q		0.011***	0.008***	0.015***
		[5.54]	[3.55]	[7.62]
Child born in 2^{nd} Q or 3^{rd} Q in Group 2 states		-0.004	-0.009	0.016
		[-0.38]	[-1.08]	[0.66]
Age [of mother]	0.022***	0.022***		0.027***
	[11.52]	[11.55]		[13.11]
Age Sq. [of mother]	-0.000***	-0.000***		-0.000***
	[-12.80]	[-12.95]		[-14.68]
High School Degree	0.148***	0.148***		0.160***
	[20.53]	[19.51]		[14.82]
Associated Degree	0.238***	0.237***		0.247***
	[17.15]	[16.47]		[15.57]
College Degree	0.201***	0.196***		0.213***
	[15.02]	[14.36]		[13.79]
Graduate Degree	0.314***	0.307***		0.311***
	[24.31]	[22.86]		[24.74]
Race=White	-0.065***	-0.069***		-0.053***
	[-14.81]	[-14.65]		[-10.88]
Number of children	-0.053***	-0.053***		-0.043***
	[-27.31]	[-26.59]		[-19.84]
No Younger Children	0.079***	0.080***		.
	[22.70]	[23.05]		.
Mother Is Currently Married	-0.140***	-0.144***		-0.123***
	[-24.66]	[-25.40]		[-18.73]
N. Obs.	173,844	173,844	173,844	98,112

Linear Probability Model, Dependent variable is $\mathbb{1}\{\text{Mother in Labor Force}\}$. State, year, region and year*region fixed effects and a constant are included in all regressions. The T-scores shown in brackets are based on robust standard errors, clustered at the state level. $^{(*)}$ Sample for Column D: five-year-old children who do not have any younger siblings.

state, year of survey, region and interaction of year and region and cluster the standard error at the state level[19].

Columns (B) and (C) report the ITT effect of season-of-birth-based eligibility, estimated with and without other covariates. The estimated difference in LFP between mothers of eligible and non-eligible children is small (0.7 percentage points) but statistically significant. A child is considered eligible if he turns five in the two quarters before the end of September, non-eligible if he turns five in the next six months. Notice that in *placebo states* there is no significant difference between mothers of "eligible" and "non-eligible" children.[20] This is the first test to confirm that the effect of the eligibility variable truly captures that of access to kindergarten, rather than any other pre-existing differences. A second indirect confirmation comes from Column (D), which replicates the same estimation on the subsample of mothers who do not have any younger children beside the five-year-old. One should expect the effect of eligibility to be stronger on this subsample for two reasons. First, as shown in Table (4), the effect of eligibility on enrollment is larger for this subsample. Second, even if the five-year-old child is eligible, mothers who have other younger children who decide to participate in the labor market will need to find alternative care arrangements for the younger children. The effect of kindergarten itself on labor participation will therefore be smaller for this subsample. The ITT should also be smaller, if it truly captures only the effect of kindergarten. The estimated ITT within the subsample is indeed smaller.

In each specification, the ITT is around 1 percentage point, which corresponds to a 1.4% increase in LFP with respect to the average in the group on non-eligible mothers. This is a small but statistically significant (at the 1% level) effect.

Table (6) reports the LATE effect of eligibility (in non-placebo states) on compliers, for the entire sample (Column (A)) and for women who do not have any younger children (B). The estimation is performed through the two stage least squares instrumental variable approach in Equation (7), where the first stage corresponds to Table (4).

The difference in LFP induced by (instrumented) enrollment is 5.1 percentage points. This corresponds to the treatment effect on the treated and it is around seven times higher than the intention-to-treat effect (in Column (B) of Table (5)). Notice that the LATE is also higher than the (biased) effect estimated in Column (A) of Table (5). This is likely due to the fact that employed

[19]The results are robust to the inclusion of binary variables for the level of non-labor income and similar estimates are obtained when restricting the sample alternatively to married and to non-married mothers.

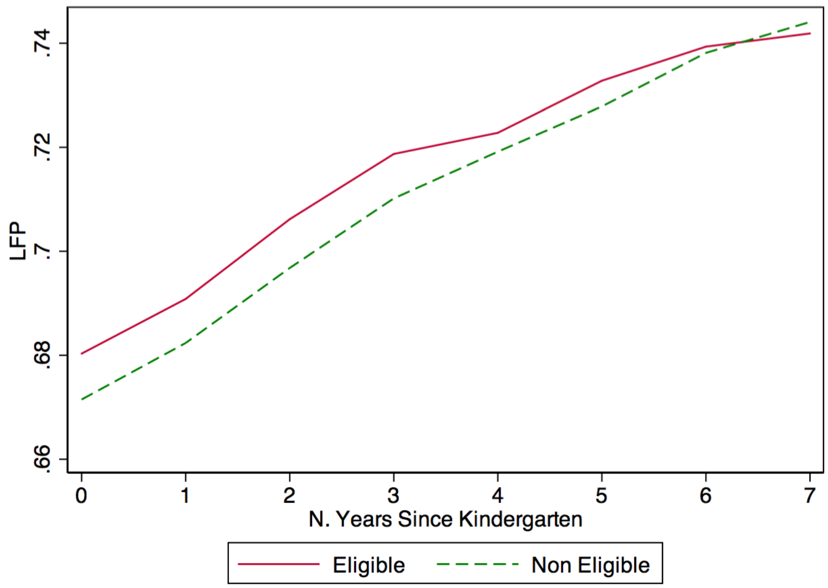
[20]The fact that season-of-birth only matters in those states where it determines eligibility to enroll is also confirmed when running two separate regressions for "treatment" and "placebo" states. The season of birth of the child does not have any statistically significant effect in placebo states, even when bootstrap methods are used to reduce the standard errors.

Table 6: The Effect of (Instrumented) Public Kindergarten on Maternal LFP

	Entire Sample (A)	No Younger Child (B)*
Child Enrolled in Public K (Instrumented)	0.047***	0.060***
	[5.37]	[7.43]
Age [of mother]	0.022***	0.025***
	[10.75]	[12.76]
Age Sq. [of mother]	-0.000***	-0.000***
	[-11.95]	[-14.16]
High School Degree	0.149***	0.165***
	[18.82]	[14.38]
Associated Degree	0.238***	0.255***
	[15.86]	[15.00]
College Degree	0.201***	0.225***
	[14.18]	[13.54]
Graduate Degree	0.318***	0.326***
	[22.29]	[25.37]
Race=White	-0.065***	-0.048***
	[-16.55]	[-10.76]
Number of own children in the household [of mother]	-0.054***	-0.043***
	[-26.54]	[-20.15]
Mother Is Currently Married	-0.141***	-0.119***
	[-22.84]	[-16.79]
No Younger Children	0.079***	0.000
	[20.76]	[.]
N. Obs.	154,356	87,006

Linear Probability Model, Dependent variable is $\mathbb{1}\{\text{Mother in Labor Force}\}$.
State, year, region and year*region fixed effects and a constant are included in all regressions. The T-scores shown in brackets are based on robust standard errors, clustered at the state level. * Sample: five-year-old children who do not have any younger siblings.

Figure 6: LFP of Mothers, By Eligibility of Children



women are more likely to use private kindergartens, which usually offer longer and/or more flexible hours. Column (B) confirms that the effect is larger for women who do not have younger children.

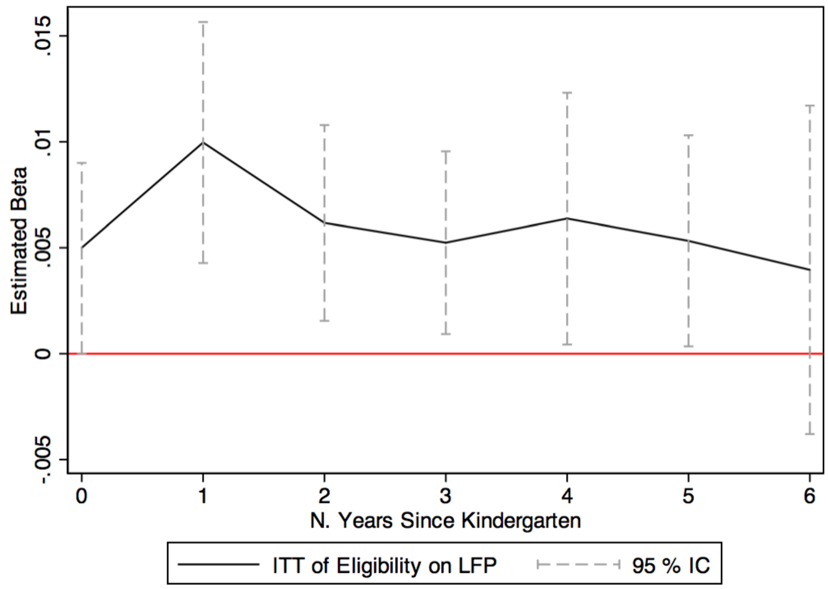
The estimates reported so far confirm previous findings that, in the short-run, providing access to public kindergarten for children of age 5 is effective in encouraging maternal labor supply and that the effect is particularly stronger for women who do not have any additional younger children. Over the next few years, however, all children in the sample turn six and qualify for public school: will the differences between their mothers gradually fade away? This question is obviously very relevant for policy evaluation.

In order to investigate the evolution of labor supply in the subsequent years, I extend the original pooled cross section sample to include mothers of children above age 5. By observing the labour supply of mothers of six-years-old, I can estimate the ITT effect one year after entry/non-entry to kindergarten. Similarly, the ITT after two years can be estimated by observing mothers of 7-year-old children. Figure (6) plots the raw labor force participation rates of eligible and not eligible mothers, from the pooled dataset. The LFP of both eligible and non-eligible mothers steadily increases in their child's age, but non-eligible mothers seem to lag behind for a few years.

The estimated ITT's for zero to seven years after kindergarten, when all individual characteristics are included, are shown in Figures (8) and (7). The graphs also show the corresponding 95% confidence intervals.

Figure (7) shows the estimates for the pooled cross-sectional sample of women interviewed in any

Figure 7: Persistence of The Impact Of Eligibility To Enroll On LFP, Pooled Cross Section



survey between 2005 and 2012. The estimated models include all the observable covariates and fixed effects used in the short-run regressions plus a new set of year-state-age-of-the-child fixed effects. The latter ensure that the ITT estimates are not capturing any time trends nor the effects of other events. As a further check, it is also possible to re-estimate the same models on a single cross-section. The estimates are shown in Figure (8). Notice that the precision is much lower (higher standard errors lead to wider confidence intervals), but the trend is similar to that estimated on the pooled cross section, which is shown as a dashed red line. It should also be noted that when using one cross-section, the estimates reflect the comparisons of the behavior of mothers of different cohorts of children.

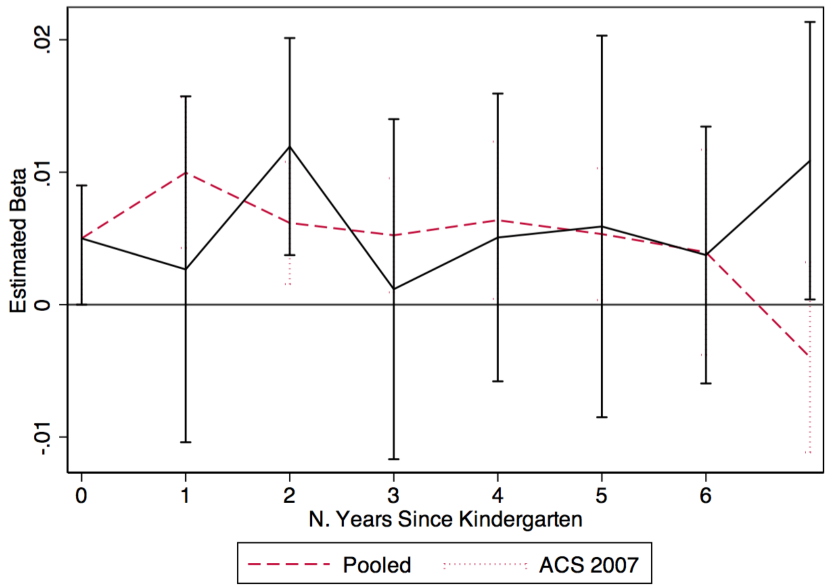
Notice that the mean-difference in LFP between eligible and non-eligible mothers in Figure (7) persists at least until the year the child turns 10.[21] This is an important result, as it states that previous (short-run) evaluations of the effect of kindergarten largely underestimate the impact on maternal labor choices, because they overlook the effects over the longer-term.

Hours of Work

Access to free-of-charge kindergarten may theoretically affect the intensive margin, too, pushing mothers to work longer hours. Columns (1) and (2) of Table (11) compare the average working

[21] Extending the horizon to up to ten years after kindergarten, the coefficients after six years bounce around the zero but are never statistically significant.

Figure 8: Persistence of The Impact Of Eligibility To Enroll On LFP, Single Cross Section



hours by enrollment status of the child, ignoring the threat of endogenous selection. On average, after controlling for other observable characteristics, mothers of children who are enrolled in public kindergarten work slightly longer hours, but the difference is very small (about half an hour per week) and it is also likely endogenous, as mothers who work long shifts may have a stronger incentive to use free kindergarten, possibly in combination with private childcare to cover the length of their workings day.

Table **??** shows the effect of eligibility on both the average number of hours (Columns 1, 2) and the share of women working at least 40 hours per week (Columns 3, 4). The interest in considering the share of mothers working full time stems from the fact that the distribution of hours of work, shown in Figure (**??**), is strongly clustered around 40 hours per week. In order the handle the clustering, I the effect of enrollment on hours of work in two alternative ways: in Columns (1,2) in Table (**??**) the dependent variable is the number of hours of work per week, while in Column (3) the dependent variable is an indicator variable which takes value equal to one if the woman works full-time (30+ hours/week).

Column (1) estimates the impact of enrollment ignoring the endogeneity issues and including controls for race, education, age marital status in the current year and at childbirth and number of other young children. I find that women whose 5-years-old is enrolled in public K on average work 0.45 more hours, a coefficient which is significant at the 1% level. This estimate is likely to be biased if enrollment and labor supply are chosen together. Once we adopt the 2SLS method to get consistent estimates, in Column (2), the estimated impact is smaller (0.23) and not statistically significant at the 10% level. I fail to find a significant impact even when restricting the sample only to currently married mothers, to non currently married or to those who have no additional younger children.

In Column (3), the probability that a mother works full time, conditional on employment is 1.9 percentage points higher among mothers of eligible children, but it is not significant at the 10% level. All in all, Table (11) shows that in the short-run, while mothers whose child is enrolled in public school tend to work longer hours, the effect of an exogenous variation in enrollment due to quarter of birth is not statistically relevant. The longer-term effect in the next six years is also largely insignificant.[22]

Conditional and Unconditional Earnings

The fact that women who benefit from public kindergarten are more likely to participate in the labor market both right away and over the next few years is interesting for at least two reasons. First, increased participation means higher aggregate production in the local economy. Second, working mothers earn additional income which adds up to the family earnings. These two effects can be investigated by estimating the impact on unconditional and conditional earnings.

Wages conditional on employment are also interesting for another reason. Recent evidence in the literature has pointed to the existence of a "motherhood penalty" in the US. Using panel data of earnings before and after childbearing, Budig and Hodges (2010) find that having a child increases a man's average wage by 6%, but has a negative impact on the wage of the mother (up to -6% per childbirth for low-income mothers), even after controlling for working hours and type of job. In light of this evidence, it is interesting to ask whether kindergarten, by making the work-family balance easier, can potentially act as a buffer and weaken the penalty, either in the short or long-run.

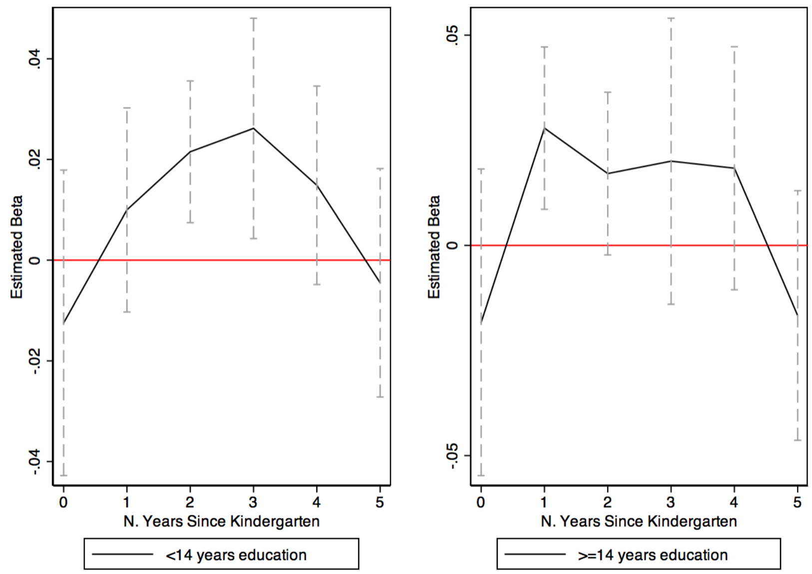
Table (7) estimates the short-run mean effects of eligibility and enrollment on hourly wages and total yearly earnings.[23] Column (A) shows that the average difference between the hourly wages of eligible and non-eligible mothers, controlling for observables and self-selection, is just about 1% (significant at the 1% level). The effect of eligibility on unconditional yearly earnings is around $300 (Column (C)), while the effect on the treated, that is the LATE effect of (instrumented) enrollment, is about $1,200 (Column (D)). Column (B) confirms that conditional earnings are not affected in *placebo states*.[24] Notice that the effect on unconditional earnings combines increased

[22] See Figure (**??**) in the Appendix for a plot of the estimated coefficients.

[23] Self-selection is accounted for through the standard Heckman two-steps procedure. The first stage consists in a LFP model, and the corresponding inverse Mills ratio is included as a regressor in the second stage.

[24] Although not shown here, the same is true for unconditional yearly earnings.

Figure 9: The Effect on (ln) Earnings, Conditional on Employment



employment and increased wages.[25] The effect of the other observable individual characteristics is as expected: wages are increasing in maximum potential experience (but decreasing in its square) and in education (women who have completed high school on average earn 16% more, while women who have completed at least one year of college earn 57% more than women who stopped after junior high school or earlier) and white women on average earn 7% more.[26]

Evidence presented so far shows that mothers whose child is enrolled in public kindergarten at age five on average get higher hourly wages and yearly earnings, which seems to suggest that access to public-K allows them to secure better paid jobs.

What is the longer-term effect? Figure (9) shows the evolution of the ITT effect of eligibility to enroll on gross yearly wage earnings up to seven years after the child's fifth year of life. The coefficients have been estimated using the basic OLS structure of Column (A) in Table (7), but allowing for the effect to vary by educational attainment of the mother. The resulting reduced form model is

$$\ln w_{ijl} = \gamma_{el} \cdot \mathbb{1}\{\text{Lag=l \& Eligible }\} + \alpha_l \cdot \mathbb{1}\{\text{Lag=l}\} + \alpha_e \cdot \mathbb{1}\{Eligible\} + \beta X, \qquad (7)$$

where $X\beta$ includes the usual control variables and state, age, year and region/year fixed effects.

The graph suggests that the initial negative impact on conditional wages turns positive within a couple of years and stays such for a few more years.

[25]The estimated impact of eligibility and enrollment on hours of work, on the other hand, is not significant.
[26]Potential experience is computed as age minus years of education minus 6.

Table 7: Effect On Hourly Wages And Gross Yearly Earnings

	Log Hourly Wages		Yearly Earnings	
	ITT (A)	ITT In Placebo States (B)	ITT (C)	LATE (D)
Eligibility (ITT)	0.010**	0.014	328.294***	.
	[2.65]	[2.03]	[2.83]	
Enrollment (LATE)	.	.	.	2613.253***
				[2.59]
Inverse Mills Ratio (λ)	-0.222**	.	.	.
	[-2.63]			
Mother's Potential Experience	0.059***	0.061***	954.288***	980.449***
	[20.34]	[16.67]	[13.52]	[13.82]
Mother's Potential Experience Sq.	-0.001***	-0.001***	-23.668***	-24.091***
	[-17.60]	[-11.94]	[-11.19]	[-11.71]
High School Education	0.163***	0.112**	5458.867***	5463.476***
	[8.02]	[5.90]	[28.12]	[26.42]
College Education Or More	0.577***	0.601***	14651.192***	14972.813***
	[24.38]	[11.35]	[34.80]	[28.66]
Race=White	0.072***	0.03	-2159.905***	-1895.497***
	[5.86]	[1.02]	[-5.42]	[-5.27]
_cons	1.742***	1.669***	-1490.358**	-5411.610***
	[49.65]	[178.84]	[-2.59]	[-5.06]
N. Obs.	61,810	7,730	112,642	112,642

The selection model includes years and state dummies, age and age squared of mother at the time of the survey and at the child's birth, dummy for college education and marital status, number of own children in household, by age groups, white race dummy. In each column, I exclude teen mothers and women whose theoretical experience is zero.

Additional Results: Fertility and Human Capital Investments

A negative trade-off has been documented between child bearing and labour supply (Willis 1973, Browning 1996, Angrist and Evans 1998, Bailey 2006). Because eligibility to use public kindergarten affects labor supply, investigating its effect on childbearing is of interest. On the one hand, because eligibility increases labour supply, we might expect that it might adversely affect fertility of kindergarten-treated mothers. On the other hand, because it potentially weakens the trade-off between work and childbearing, it might actually lead to higher fertility for eligible mothers. In order to assess which effects prevails, I estimate the ITT of eligibility on fertility over the first two years after entry (or non-entry) to kindergarten. I focus on mothers of seven-years-old and estimate the difference in the probability that they have had a new childbirth over the last two years.[27]. In the first column of Table (8) I ignore the possible link between childbearing and labor supply and simply estimate the marginal effect of eligibility on childbearing.

In the second column, following Fernández and Fogli (2005), I estimate the impact of quarter of birth on the joint decision. Under either model specification, eligible women are slightly (but significantly from a statistical point of view) less likely to have experienced subsequent childbirths. Notice that the null hypothesis of zero correlation between fertility and labor supply decisions can be rejected (the ξ^2 is 320.556 and significant at the 0.01 level). When we account for this, in the second column, the difference in mean of fertility rates between eligible and non-eligible mothers is about 4 percentage points.

By offering subsidized care for children during the school day, public kindergarten might potentially affect the schooling decisions of young mothers, which can be interpreted as formal human capital investments. Table (9) provides evidence that teen mothers whose child is eligible to enroll in public kindergarten are more likely to be in school. The effect is substantial (around 8 percentage points) and statistically significant (at the 5% level).

Column (A) shows that enrollment in school decisions are only affected for mothers shoe were fifteen or younger at childbirth. Column (B) on the other hand shows that in *placebo states* there is no effect.

8 Discussion

My empirical analysis provides evidence that access to public kindergarten has a positive impact on maternal employment and earnings, which is line with previous findings in Berlinski and Galiani

[27]The results shown here are those for the subsample of women who, when the seven-years-old was five did not have any other children, but including all women leads to analogous estimates.

Table 8: Effect of Eligibility on Fertility

	OLS	SUR
	b/(t)	b/(t)
QoB * (State with cutoff)	-0.026***	-0.039***
	(-5.51)	(-6.60)
QoB * (Control State)	-0.015	-0.022
	(-1.25)	(-1.38)
Mother's Age at Birth	0.005**	-0.011***
	(2.00)	(-4.13)
Age Sq. [of mother]	-0.000***	-0.000***
	(-9.40)	(-3.32)
College Educ [of mother]	0.030***	0.054***
	(7.41)	(10.11)
Race=White	0.014***	-0.018***
	(3.04)	(-2.85)
Age [of father]		-0.007***
		(-14.99)
_cons	0.541***	1.131***
	(16.32)	(28.38)
N	50,057	31,849

The dependent variable is P(A new child born in the first 2 years after Kindergarten). State, Year, Region and Year*Region fixed effects included in each stage. Correlation between residuals of Labor Supply and Fertility equations is -0.1169 (ξ^2=320.556***) in States where the cutoff is applied, -0.1220 (ξ^2=45.56***) in placebo States. The null of zero correlation is always strongly rejected.

Table 9: Effect On Maternal Enrollment

	ITT	ITT, Placebo States
(A)	(B)	
Eligible	-0.002	-0.001
	[-1.08]	[-0.32]
Eligible * Teen Mother ($\leq$ 20)	0.079**	-0.024
	[2.05]	[-1.48]
Teen Mother ($\leq$ 20)	0.218***	0.189
	[5.54]	[1.22]
Age [of mother]	-0.032***	-0.041***
	[-23.28]	[-12.34]
Age Sq. [of mother]	0.000***	0.000***
	[17.57]	[11.88]
Race=White	-0.041***	-0.051**
	[-6.59]	[-8.01]
Middle or Junior High School	0.009	0.016
	[0.98]	[0.61]
High School	0.049***	0.043**
	[5.36]	[4.60]
College, Less Than 4 Years	0.196***	0.160***
	[21.48]	[18.57]
College, More Than 4 Years	0.128***	0.114**
	[11.35]	[8.76]
Graduate School	0.139***	0.115***
	[12.99]	[13.08]
Mother Is Currently Married	-0.042***	-0.031 *
	[-11.54]	[-1.69]
No Younger Children	0.006*	-0.004
	[1.97]	[-0.60]
N. of Children	0.008***	0.005***
	[12.98]	[12.91]
N. of Children Age 0-5	-0.030***	-0.030**
	[-11.42]	[-9.86]
N	125,432	15,026

(2007) and Gelbach (2002). This might seem at odd with the evidence that other policies such as universal pre-kindergarten apparently do not affect labor supply (Fitzpatrick (2010)). This can actually be easily explained if mothers of young children value leisure time more, i.e. in an extension of my model where the utility of leisure b is decreasing in the age of the youngest child.[28]

9 Conclusions

In this paper I have analyzed the short (same academic year) and long-run (up to 5 academic years later) effects of access to public pre-kindergarten and kindergarten, which can be interpreted as an implicit subsidy to child care, on female participation to the labour force. A quasi-experimental approach based on quarter of birth of the child allowed me to show that enrollment to public school increases maternal labour supply and that the effect is persistent for up to 5 years.

I also show that, unconditionally on employment, treated women get higher yearly earnings but that conditional earning are actually initially lower (which I show is consistent with predictions from the theoretical framework). After the first year, however, they get higher earnings even conditioning on employment, as a result of increased experience and human capital.

My paper also underlines two surprising result, previously not investigated in the literature. First, kindergarten increases school enrollment among teen mothers, which is likely to lead to even higher welfare benefits. Second, fertility in the first two years is higher among non-eligible mothers.

My findings underscore the significant long-term benefits of public kindergarten to women's welfare, which significantly increases the social return to investing in these programs.

Given the evidence found in the data, one obvious extension, which is current work-in-progress, is the construction of a structural life cycle model of labor participation which matches the observed dynamics of LFP and earnings. The calibrated model will then be used to simulate the impact of a set of alternative policy interventions, including the introduction of universal pre-kindergarten.

[28]Note that such assumption would be reinforce the rationale for my empirical finding that the effect on labor supply is stronger for women who do not have other younger children.

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Appendix

Figure 10: Cumulative Distribution of States by Cutoff Date

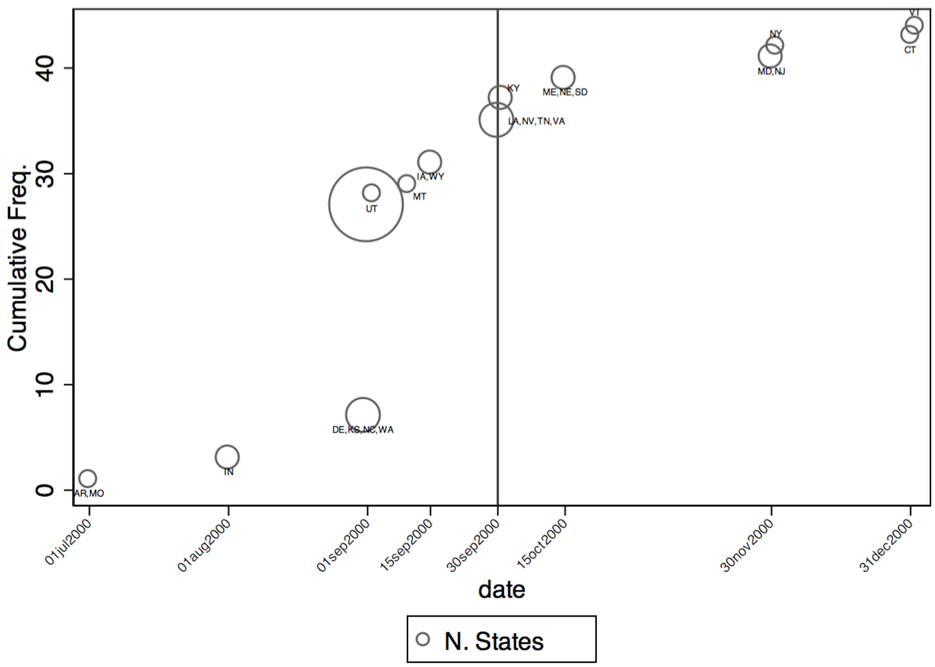

Table 10: Exclusion Restriction, Dependent Variable: Mother's LFP[1]

	No Controls	Full Controls[2]
	(a)	(b)
Child Born in April-September	0.000	-0.000
	[0.09]	[-0.10]
Child Enrolled in Public K	0.061***	0.049***
	[11.08]	[16.49]
N	133,195	133,063

[1] Sample: mothers of five-year-old children. State, year and region*year fixed effects and age of child fixed effects included. Standard Errors have been clustered at the state level.

[2] Controls include: mother's age and age square, number of other children in the household, marital status and binary variables for education level, race and presence of younger children.

Table 11: The Effect of Kindergarten on Hours of Work

	OLS	2SLS	
		N. of Hours/Week	𝟙{Full time}
Child Enrolled in Public K	0.455***	0.230	0.019
	[3.11]	[0.47]	[0.43]
Age [of mother]	-0.288**	-0.223	0.115***
	[-2.60]	[-1.22]	[3.35]
Age Sq. [of mother]	-0.009***	-0.009***	-0.001***
	[-13.27]	[-13.41]	[-15.62]
Middle or Junior High School	-0.216	-0.226	0.105
	[-0.18]	[-0.19]	[1.47]
High School	0.659	0.653	0.143**
	[0.60]	[0.60]	[2.17]
College, Less Than 4 Years	0.442	0.430	0.144**
	[0.41]	[0.40]	[2.18]
College, More Than 4 Years	0.436	0.406	0.182***
	[0.39]	[0.37]	[2.72]
Graduate School	2.618**	2.583**	0.245***
	[2.53]	[2.53]	[3.78]
Race=White	-2.053***	-2.068***	-0.091***
	[-14.58]	[-14.82]	[-11.22]
N. of Children	-1.036***	-1.030***	-0.045***
	[-10.10]	[-10.24]	[-12.74]
N. of own children of Age 0-5	-0.861***	-0.870***	0.077*
	[-7.14]	[-7.10]	[1.91]
Mother Is Currently Married	-1.680***	-1.692***	-0.039***
	[-10.73]	[-11.16]	[-6.22]
No Younger Children	-0.092	-0.092	.
	[-0.53]	[-0.53]	.
Mother's Age at Birth	0.937***	0.871***	-0.066*
	[8.95]	[4.89]	[-1.89]
N. Obs.	113,788	113,788	68,597

The dependent variable is 𝟙{Mother works part-time}