ORIGINAL ARTICLE

Public kindergarten, maternal labor supply, and earnings in the longer run: Too little too late?

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Abstract

By facilitating early re-entry to the labor market after childbirth, public kindergarten might positively affect maternal human capital and labor market outcomes: Are such effects long-lasting? Can we rely on between-individuals differences in quarter of birth to identify them? I isolate the effects of interest from spurious associations through difference-in-difference, exploiting across-states and over-time variation in public kindergarten eligibility regulations in the United States. The estimates suggest a very limited impact in the first year, and no longer-run impacts. Even in states where it does not affect kindergarten eligibility, quarter of birth is strongly and significantly correlated with maternal outcomes.

KEYWORDS
childcare, longer run, maternal labor supply, quarter of birth

JEL CLASSIFICATION
J13; J22

The gap between male and female labor force participation (LFP) in the United States is largely driven by the low participation rate of women who have children. For example, based on American Community Survey data, at age thirty the difference in LFP between women with and without children is about 80% of the 9% points gap between male and female.[1] Figure 1 shows that this difference exists in other age ranges as well.

Survey evidence attributes the low LFP of mothers to the high opportunity costs of working, and the need to find alternative arrangements for their children while they are at work.[2] What portion of the
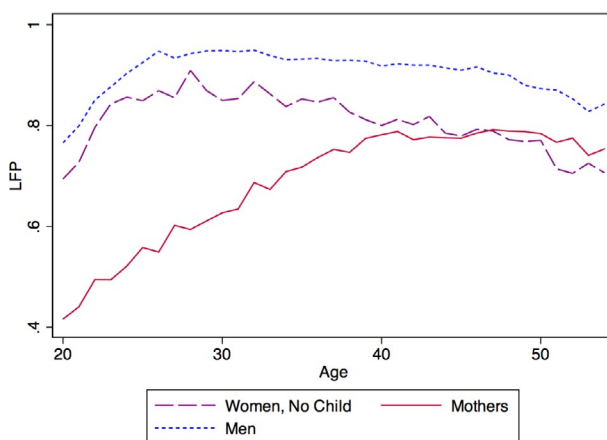
FIGURE 1   Female labor force participation, by presence of children. Data: 2012 American Community Survey [Colour figure can be viewed at wileyonlinelibrary.com]

gap between the LFP of mothers and non-mothers could be bridged by providing such arrangements at little or no price? What portion of such estimated impact can be causally attributed to kindergarten, as opposed to spurious factors? Are there any longer-term repercussions on employment and earnings? I answer these important questions by comparing mothers who differ in their children's eligibility to enroll in public kindergarten in the United States at age five. The results suggest that public kindergarten has negligible impacts on maternal labor supply and earnings, both in the shorter and in the longer run.[3]

The contribution to the existing literature is twofold. First, disentangling spurious and causal effects allows me to reconcile the contributions to the literature documenting sizeable impacts of kindergarten on maternal labor outcomes (Gelbach, 2002) with those suggesting little or no impact (Cascio, 2009c; Fitzpatrick, 2010). Second, I provide novel evidence on longer-run impacts, which should be central to policy discussion.

Providing empirical evidence of the effects of public kindergartens is complicated by the fact that parents' choice to enroll children in public kindergarten is likely endogenous to their labor supply: The two choices are often taken together and influenced by a common set of observable as well as unobservable characteristics. To improve identification, I exploit the fact that eligibility for public kindergarten in the fifth year after birth depends on age at a given date, and that such date differs across states. Many states chose a date around the end of the third quarter of the year; others chose December 31 or January 1. In the former states, quarter of birth (e.g., the quarter of the year when childbirth occurred) affects kindergarten eligibility. In the latter states, it does not. In a difference-in-difference approach, I compute the differences in maternal outcomes across quarters of birth in the first group of states and then subtract from this the corresponding difference in the second group of states. This design allows me to separate the effect of public kindergarten eligibility from other confounding mechanisms. The underlying assumption is that the other mechanisms operate similarly across states. The maternal outcomes I consider are labor force participation, employment, hours and weeks of work, hourly wages, and total labor earnings.

The resulting estimates suggest that, in the shorter run, public kindergarten eligibility has a small positive impact on maternal hours of work and hourly wages and, as long as no younger children are present in the household, on maternal labor force participation. In general, the outcomes vary across quarters of childbirth, providing no convincing evidence of larger differences in states where the quarter of birth affects kindergarten eligibility. Longer-run estimates, based on mothers of children in the age range 6–10, also suggest no impact. As I argue below, the effects of public kindergarten are

probably limited by the fact that it starts too many years after childbirth, in a context with virtually no parental leave, and offers limited hours.

My method is closely related to two existing approaches in the literature and helps us reconcile their apparently contradicting results. The first approach is to use quarter of birth as an instrumental variable for public kindergarten enrollment. Such method yields large positive estimates for the impact of kindergarten on employment, earnings, and hours and weeks of work (as documented by Gelbach (2002) using data for all states in the 1980 US Census).[4] Its validity relies on the exclusion restriction assumption that quarter of birth only impacts labor market outcomes via its effect on access to public kindergartens. In other words, the assumption is that children on either side of the quarter cutoff, and their families, are similar. This assumption has been challenged by the timing of birth literature, which documents strong correlations between season of birth and personal and parental attributes, including maternal education, age, and marital status at the time of conception (Buckles and Hungerman, 2013).[5] Consistently with this literature, I document significant correlations between quarters of birth and maternal outcomes (i) in states where the quarter of birth has no impact on public kindergarten eligibility and (ii) among children born in the first three quarters of the year, who are all equally eligible for public kindergarten.

The second approach adopted in the literature is regression discontinuity, which relies on a weaker version of the exclusion restriction assumption but requires data on exact dates of birth. Using this approach on restricted-access data from the 2000 Census, Fitzpatrick (2012) found negligible impacts of kindergarten on maternal labor market outcomes.[6]

In comparison with these two approaches, difference-in-difference requires neither the strong exclusion restriction assumptions of the former nor exact date of birth as the latter. This is possible by leveraging differences in regulation across states.

The remainder of the paper is structured as follows: Section 1 introduces the relevant context and discusses relevant theoretical predictions for the effects of public kindergarten on maternal labor market outcomes. Section 2 highlights the contribution of the paper with respect to previous literature, Sections 3 and 4 describe the data and empirical method, Section 5 presents the main results, and Section 6 discusses their policy implications and the possible threats to identification posed from migration and timing of birth. Section 7 concludes.

1 | CONTEXT

In the United States, children typically enter elementary public schools at age six. In addition, 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. While local governments at the county and city level started funding kindergarten during the 60 s, especially in urban areas, the main increase in kindergarten offer happened during the 60 s and 70 s, when all states but Mississippi and North Dakota introduced grants to school districts operating kindergarten. In most cases, public funding of kindergarten was motivated by the will to reduce school failure rates (Cascio, 2009b) and improve children's cognitive and social skills (Cascio, 2009a, 2010), which could result in lower state expenditures on subsequent social programs. Within two years from the introduction of state funding, the enrollment rates in public kindergarten rose, on average, by around 30% points. The period considered for this analysis is the school year 1979/1980. In this period, the use of public kindergarten was already well spread (statistics reported in Section 3), with the exception of the state of Mississippi, where public kindergarten was introduced in 1982.

However, not all five-year-old children are admitted to public kindergartens: Depending on the date of birth and state of residence, some children gain eligibility in the fifth year after birth, and the

others in the sixth. This is due to the fact that eligibility for public kindergarten typically depends on the child's age at a certain, state-specific, cutoff date. For example, in states where the chosen cutoff is December 31 (or, equivalently, January 1 of the following calendar year), all children born in 1974 are eligible for public kindergarten in the school year 1979/1980 (the state-specific cutoffs are shown in Table B1, and the list of states in each category is given in footnote 21 below). In states where the chosen cutoff is earlier, instead, not all children born in 1974 are eligible in the school year 1979/1980, but only those who turn five before the state-specific cutoff. As discussed in Section 4, in many states the cutoff falls around the end of the third quarter of the year, so that a child's quarter of birth *de facto* determines her eligibility for public kindergarten.[7]

In the sixth calendar year after birth, all children are eligible for public education, either by entering public kindergarten for the first time or (for those who already attended kindergarten) by enrolling in first grade. In the empirical analysis, I will refer to the fifth year after childbirth as the *short run* and to the next five years as the *longer run*.

Childcare options for children below age five include nurseries, nursery schools, daycare centers, private pre-kindergarten, and informal sources of care, such as grandparents and other family members. In 1979, day nurseries, child care centers, and nursery schools were relatively common (Kamerman and Gatenio-Gabel, 2007). In 1979, 35% of all children in the age range 3–4 were enrolled in nursery schools/pre-kindergartens (generally for a fee) or Head Start (Kamerman, 1983).[8]

For children of age five, an alternative to public kindergartens is offered by (paid) private kindergartens. As the requirements for enrollment in private kindergartens are not regulated by federal or state authorities, they vary across individual facilities.[9]

1.1 | Kindergarten and maternal labor market outcomes: theoretical framework

In the short run, mothers whose children can enroll in public kindergartens face lower opportunity costs of working, while mothers of not-eligible children must find alternative forms of childcare.[10] In the longer run, all mothers should face the same opportunity cost of working.

Standard static individual labor supply models help predict how this would affect maternal labor supply in the short run. Because public kindergarten has a negative impact on mothers' opportunity costs of working, under standard assumptions it will have a positive impact on the extensive margin of labor supply and an ambiguous effect on the intensive margin, as substitution and income effects operate in opposite directions.[11] The predicted impact on wages is negative, as lower opportunity costs translate into lower reservation wages. The standard model also suggests that the impact of kindergarten on labor supply will depend on how many hours of care it offers, relative to the length of a workday: If too few hours are offered, the resulting budget constraint will exhibit a kink, inducing more mothers to reduce their hours of work.

To gain predictions on longer-run effects, a dynamic model is required. Employment may be positively affected in the longer run if the probability of getting a job offer is decreasing in the time spent out of the labor markets. This would be the case if, for example, human capital depreciates over time. Wages and earnings might also be affected and this effect is best understood in job search models. For example, in the Appendix, I derive the predictions on labor supply and earnings from the augmented McCall (1970) model of job search with discrete-time, infinite horizon, and returns to experience (Appendix A).[12] Intuitively, there are two contrasting forces at play when we compare the longer-run wages of mothers whose children gained public kindergarten eligibility in the fifth year after birth and mothers whose children had to wait one year longer. On the one hand, the first group of mothers may

have higher wages, because on average they enter employment one year earlier and hence have higher experience (or tenure). On the other hand, the wages of the mothers who re-entered the labor market in the fifth year even though their children could not access public kindergartens will be even higher and could drive up the average wages in the second group. This is because these mothers started with higher wages (as their wages must have exceeded their higher reservation wages to compensate them for childcare costs) and also have higher experience.

To summarize, theory predicts that in the short-run public kindergartens should have a positive effect on the extensive margin of labor supply and a negative effect on wages, while it offers no clear predictions for the longer run and the intensive margin.

2 | LITERATURE

Previous literature has highlighted the importance of time-saving technologies for home production (Greenwood et al., 2005), of relative demand of workers in sectors where women hold a comparative advantage (Jensen, 2012; Rendall, 2010), and of allocation of powers within the household (Heath and Tan, 2014) in determining women's participation to the labor force (LFP in short). More specifically, the LFP of women with children has been shown to react to cultural norms (Fernández et al., 2004; Fernández and Fogli, 2006) and to earned income taxation (Azmat and González, 2010; Blundell et al., 1998; Eissa and Liebman, 1996).

As discussed in Section 1, standard models of labor supply predict that childcare subsidies should have a positive impact on the labor force participation of parents, but do not offer clear predictions on their impact on hours of work (intensive margin). The same applies to public kindergartens and pre-kindergartens, to the extent that they provide free-of-charge childcare. Empirical evidence on this matter is mixed and limited to short-term effects (Anderson and Levine (1999); Blau (2003); Blau and Currie (2006) offer an excellent review). In particular, the effects on maternal labor supply seem to depend on the context, the age of the youngest child, and the marital status of the mother. For the United States, Cascio (2009c) and Fitzpatrick (2012) find no impact of public kindergartens on the general population of mothers of five-year-old children, and a positive impact for single mothers without younger children, while Gelbach (2002) points to significant increases in various measures of labor supply for most mothers (except for single mothers with additional younger children). My analysis speaks to possible reasons for this apparent disagreement.

Larger effects could be expected for childcare policies that target younger children, given the evidence of strong path dependence in maternal labor supply (Del Boca and Sauer, 2009). The empirical evidence indeed seems to support this hypothesis: Positive effects on maternal labor supply have been documented more often in countries and states which target children below age 4, rather than in places where children of age 4 or 5 are targeted. Specifically, positive impacts have been documented for ages 0–4 in Quebec (where Baker et al. (2008) find evidence of strong impact on labor supply but also of adverse effects on children well-being and the quality of parental relationships), age 3–4 (Bauernschuster and Schlotter, 2015) and age 0–3 (Müller and Wrohlich, 2020) in Germany, age 3–4 in Israel (Schlosser, 2005), age 3–5 in Poland (Akgündüz et al., 2020), age 3 in Italy (Del Boca, 2002; Del Boca and Vuri, 2007; Brilli et al., 2011; Carta and Rizzica, 2018) and Spain (Nollenberger and Rodríguez-planas, 2015) and average age below 4 in Kentucky (Berger and Black, 1992). On the other hand, negligible or null effects have been found for mothers of children of age 3–6 in Norway (Havnes and Mogstad, 2011), age 1–9 in Sweden (Lundin et al., 2008), and 0–4 the UK (Viitanen, 2005) and in Oklahoma and Georgia (Fitzpatrick, 2010).[13]

Beyond average impacts, two additional findings in the literature are of particular relevance for my analysis. The first one is that subsidized childcare may induce sizable *crowding-out* from unsubsidized or private facilities for children below kindergarten age, and this might limit its impact on maternal labor supply (Baker et al., 2008; Havnes and Mogstad, 2011), especially for highly educated mothers (Cascio et al., 2013). My estimates suggest that this also happens for kindergarten-aged children, as nearly 20% of the increase in public kindergarten enrollment induced by eligibility is compensated by a decrease in the enrollment in private facilities.

The second finding is that the labor supply effects of childcare and public pre-kindergartens might be limited to women who are not married or cohabiting (Berger and Black, 1992; Fitzpatrick, 2012; Goux and Maurin, 2010) or to those who have no additional younger children (Berlinski et al., 2011; Fitzpatrick, 2012). This fact motivates my particular focus on these subpopulations of mothers for the estimation of both shorter- and longer-run effects.

The literature additionally documents that in certain contexts childcare facilities are severely rationed, with excess demand resulting in queuing or lack of access to the service for some eligible children (Hermes et al., 2020; Brilli et al., 2011). As discussed in Section 6, the resulting selection of eligible children into kindergarten may pose additional threats to identification in such contexts and it would also mediate the effectiveness of childcare in stimulating maternal labor supply. In the context of this study, there is, however, no evidence of an under-provision of kindergarten seats (Cascio, 2010).

Until today, very little is known about the longer-run impacts. For women in general, empirical evidence suggests that the effect of interventions aimed to incentivize labor supply is temporary (Card and Hyslop, 2005; Zabel et al., 2010) and ceases once the incentive is repealed. There is hope that interventions aimed at women with young children, as opposed to women in general, could have longer-lasting effects. The reason is that the literature has found path dependence in working status to be particularly strong among women who have recently experienced childbirth (Blank, 1989; Del Boca and Sauer, 2009; Eckstein and Wolpin, 1989; Francesconi, 2002; Heckman and Willis, 1977; Nakamura and Nakamura, 1985; Shapiro and Mott, 1994). On the other hand, it is possible that kindergarten mostly affects the labor supply of women with low education (Gelbach, 2002), who in general experience the lowest returns to experience (Blundell et al., 1999, 2013; Dustmann and Meghir, 2005). Therefore, even if an effect is found in the short run, it is unclear whether that would last long.

The literature has also considered the potential impacts of public kindergarten and childcare subsidies on children, with mixed results which originated a lively debate in the public opinion and specialized literature (Baker et al., 2008; Brilli et al., 2011; Cascio, 2008, 2009b; Drange et al., 2012; Elder and Lubotsky, 2009; Fitzpatrick, 2008; McEwan and Shapiro, 2008).

As outlined above, this paper makes two main contributions to the existing literature: (i) it proposes a way to disentangle the spurious and causal links between access to public kindergarten and maternal labor market outcomes, and (ii) it shows that the small positive impacts on maternal labor supply do not persist in the longer run. In considering longer-run impacts, my work is complementary to Nollenberger and Rodriguez Planas (2011), who exploit the introduction of pre-Kindergarten for three-year-old children in Spain and show that, as children grow older, the labor supply of their mothers remains higher than that of mothers of two-year-old children.[14]

3 | DATA

My analysis is based on census data from 1980, accessed via the Integrated Public Use Microdata Series (IPUMS-USA, 5% sample). The census is a national survey designed by the U.S. Census Bureau and collecting demographic, economic, social, and housing data. The reference date for the census

questionnaire is April 1 and the current school year at the time of interview is therefore 1979/1980. While the census survey was conducted every ten years, the quarter of birth of the respondent and her children were not reported in surveys conducted after 1980, making the resulting data not compatible with my empirical needs.[15]

The analysis is composed of a short-run and a longer-run portion. The sample used for the short-run analysis includes all children born in 1974 (that is, children who turned 5 in 1979). The sample used for the longer-run analysis of the impact t years after exposure includes all children born in the year 1974-t: These children were in their fifth year of life t years before data collection. All children whose mothers were (i) above age fifty, (ii) below age 16, (iii) affected by disabilities preventing working activities, (iv) not citizens, or (v) whose hourly wage is below the 1st percentile or above the 99th are excluded from the analysis.

For the difference-in-difference analysis, for reasons explained in Section 4 below, only data from 21 states are included. The summary statistics for the whole shorter-run sample and the difference-in-difference sample are shown in Table 1.

The sample only includes children born in 1974 and their mothers. As shown in Table 1, 70% of the children in the full sample (and 68% of those in the difference-in-difference sample) were enrolled in public kindergartens in the school year 1979/1980 and 17% in private kindergarten or pre-schools. The labor force participation rates of their mothers were 58% (61% in the difference-in-difference sample); the average total family income was around 20,000 USD (19,000 USD), a small portion of which (around 30 to 40 USD on average in the two samples, zeros included) from social security transfers. Mothers were on average around 30 years old, 82% were married or cohabiting, about 80% were of "white" race, <50% completed high school, about 30% attended college and 47% (46% in the smaller sample) had additional younger children. Among women in the difference-in-difference sample, 34% lived in states where all children born in 1974 were eligible to enroll in public kindergarten in 1979. The remainder (66%) lived in states where only children born in the first three quarters of the year 1974 were eligible in 1979.[16]

The main limitation of this data source is the lack of panel observations: mothers' working status, earnings, work experience, and job tenure in the years before the survey would be very informative, but cannot be observed. The main advantage of the data, besides its representativity and large sample size, is the fact that it includes children's year and quarter of birth, their enrollment status in public and private schools and kindergarten, and their mothers' labor market status.

4 | EMPIRICAL STRATEGY

The goal of this paper is to measure the effects of public kindergarten on maternal labor market outcomes, at different points in time, using census data from 1980. The definition of short and longer-run is necessary *ad hoc* with the context. The first four years after childbirth are not considered in my analysis, as during such period mothers had no access to universal public childcare or kindergartens.[17] The fifth calendar year after childbirth is considered *short-run*, as this is the time when children differ in their eligibility for public kindergarten. The next five years are considered the *longer run*: At this point, all children are eligible to attend public kindergartens and schools.

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. Empirical evidence of this is presented in the result section, showing substantial differences in several observable characteristics between mothers of children who are enrolled in public kindergarten at age five and mothers of children who are not. Given the differences in observable outcomes, it is likely that the two groups of

TABLE 1 Summary statistics, short-run sample

	Mean	S.D.	Min	Max
Full sample (147,621 observations)				
Child enrolled in public kindergarten	0.70	0.46	0.00	1.00
Child enrolled in private kindergarten	0.17	0.37	0.00	1.00
In the labor force	0.58	0.49	0.00	1.00
Mother's non-labor income (10^3 USD)	16.97	12.93	0.00	77.65
Total family income (10^3 USD)	20.12	13.33	0.00	77.66
Social security income (10^3 USD)	0.03	0.37	0.00	7.75
Age	30.51	5.33	16.00	50.00
Married/Cohabiting	0.82	0.38	0.00	1.00
High school	0.46	0.50	0.00	1.00
College or higher	0.30	0.46	0.00	1.00
Has no younger child	0.53	0.50	0.00	1.00
Race: white	0.83	0.37	0.00	1.00
Quarter of birth	2.53	1.11	1.00	4.00
State with Quarter cutoff	0.21	0.41	0.00	1.00
Number of own children in HH	2.58	1.22	1.00	9.00
Difference-in-difference sample (47,482 Observations)				
Child enrolled in public kindergarten	0.68	0.47	0.00	1.00
Child enrolled in private kindergarten	0.17	0.38	0.00	1.00
In the labor force	0.61	0.49	0.00	1.00
Mother's non-labor income (10^3 USD)	15.86	12.36	0.00	75.00
Total family income (10^3 USD)	19.09	12.74	0.00	75.00
Social security income (10^3 USD)	0.04	0.39	0.00	7.75
Age	30.26	5.40	16.00	50.00
Married/Cohabiting	0.82	0.39	0.00	1.00
High School	0.47	0.50	0.00	1.00
College or higher	0.28	0.45	0.00	1.00
Has no younger child	0.54	0.50	0.00	1.00
Race: white	0.80	0.40	0.00	1.00
Quarter of birth	2.53	1.11	1.00	4.00
State with Quarter cutoff	0.66	0.47	0.00	1.00
Number of own children in HH	2.54	1.21	1.00	9.00

Full Sample: IPUMS 5% 1980 US Census, mothers of children born in 1974. Difference-in-Difference Sample: mothers of children born in 1974, selected states. Mother's non-labor income is computed as total family income minus the labor earnings of the mother.

mothers also differ i additional, unobserved, characteristics. These unobserved differences cannot be controlled for, and hence pose a real threat to identification.

One solution to the endogeneity problem is to leverage cross-sectional variation in enrollment to kindergarten, due to variation in childbirth quarter (Elder and Lubotsky, 2009; Gelbach, 2002). The simple differences by childbirth quarter along an array of outcomes Y are captured by coefficients β_q in the following equation

$$Y = \sum_{q=1}^{3} \left(\beta_q \cdot Q_q \right) + \text{FE} + \gamma' X + \varepsilon \tag{1}$$

The outcomes of interest Y include children enrollment in public kindergarten, maternal labor supply, maternal employment, hours and weeks of work, hourly wages, and yearly labor earnings. The equation, estimated via ordinary least squares, includes state fixed effects and observable household and individual characteristics (all covariates and a constant are included in the matrix X). State fixed effects absorb all characteristics which vary across states and not across quarter of birth (such as local labor market characteristics, local regulations concerning parental leave, or the availability of private child care).[18] The binary variables Q_q take value one for children born in the qth quarter of the year and zero otherwise. The fourth quarter is used as the baseline, so that the coefficients β_q capture the *ceteris paribus* simple difference in outcomes between children born in each of the first three quarters and those born in the fourth quarter (or, depending on the outcome, the differences between their mothers).

As mentioned in Section 1 and discussed more in detail below, childbirth quarters determine eligibility for public kindergartens in most (but not all) states. Assume that this is the only mechanism through which childbirth quarters are correlated with the outcomes of interest. If this assumption holds, the coefficients β_q in Equation (1) give us the intention-to-treat effect of public kindergartens on the corresponding outcomes. In this case, because children born in the first three quarters of the year do not differ in their eligibility, we should not expect significant differences between the coefficients for the first, second, and third quarters ($\beta_1, \beta_2, \beta_3$ in Equation (1)). The assumption would be violated if children born in different quarters (and their mothers) differed in unobserved characteristics and these characteristics were correlated with the outcomes of interest. In this case, spurious effects would arise and the interpretation of β_q in Equation (1) as the intention-to-treat effect of public kindergarten on outcome Y would be problematic, because of omitted variable bias.[19]

If the spurious effects are constant across states, the difference-in-difference approach offers a solution, which leverages the across-states variation in the date chosen as cutoff for public kindergarten eligibility.[20] It is possible to identify three groups of states: (i) those with eligibility cutoffs around the end of the year (December 31st, 1974, or January 1st, 1975), (ii) those with a cutoff around the end of the third quarter of the year, and (iii) the remainder, where the eligibility cutoffs vary by school district or are on an intermediate date. In the first group of states, which can be used as the benchmark, quarter of birth does not affect public kindergarten eligibility. In such states, any potential difference in outcomes by quarter of birth should be interpreted as spurious correlations, not due to public kindergarten eligibility. In the second group of states, which I refer to as "*Quarter Cutoff*" states, quarter of birth determines eligibility: There, the difference in outcomes by quarter of birth would reflect both the potential impact of kindergarten and the potential spurious correlations. In the empirical analysis, all states with a cutoff in the range September 15th–October 15th are assigned to this second group. In the third group of states, the impact of quarter of birth on public kindergarten eligibility is unclear: This group is excluded from the estimation sample.[21] The difference-in-difference method estimates the (intention-to-treat) effect of public kindergarten as the difference between the simple-difference estimates obtained via Equation (1) in the two groups of states.[22] This can be achieved in one step by estimating via ordinary least squares the equation

$$Y = \sum_{q=1}^{3} \left(\alpha_q \cdot Q_q + \beta_q \cdot Q_q \times \{ Cutoff \} \right) + \text{FE} + \gamma' X + \varepsilon \tag{2}$$

The state fixed effects (FE) absorb the systematic differences in outcomes across states for children born in the fourth quarter of 1974 (and their mothers). The binary variable *Cutoff* takes value one for residents in quarter cutoff states and zero for the residents of states where the quarter of birth has no impact on eligibility.[23] In states where quarter of birth does not affect public kindergarten eligibility, the *ceteris paribus* differences in outcomes between children born in quarter q and those born in the fourth quarter are captured by the coefficients α_q in Equation (2). As these children are all equally eligible, such differences cannot be ascribed to differences in eligibility. In quarter cutoff states, the *ceteris paribus* differences in outcomes between each year quarter q and the fourth are given by the sum of $\beta_q + \alpha_q$ from Equation (2). In such states, for each quarter q, the corresponding coefficient β_q in Equation (2) captures the (intention-to-treat, or ITT) effect of public kindergarten. As previously discussed, previous literature suggests that the impact of childcare (and hence kindergarten) might be stronger for unmarried mothers and/or for those without additional younger children. To further investigate this possibility, after documenting the impact on the general population of mothers, I restrict the sample to those who do not have younger children and I estimate an extended model. The extended model (Equation (3)) is obtained from Equation (2) by adding appropriate interaction terms with the indicator variable *Single*, which takes value 0 for married and cohabiting mothers and 1 for single, divorced, widowed and separated ones:

$$Y = \sum_{q=1}^{3} \left(\alpha_q \cdot Q_q + \beta_q \cdot Q_q \times \{\text{Cutoff}\} + \gamma_q \cdot Q_q \times \{Single\} + \delta_q \cdot Q_q \times \{Single\} \times \{\text{Cutoff}\} \right) + \text{FE} + \gamma'X + \varepsilon \quad (3)$$

In the extended model (3), the ceteris paribus impact of quarter of birth in states where it does not determine kindergarten eligibility is captured, depending on the mother's marital status, by the coefficients α_q (for married and cohabiting mothers) or $\alpha + \gamma_q$ (for single, widowed, separated, and divorced ones). Similarly, the impact of kindergarten eligibility is captured by coefficients β_q and $\beta_q + \delta_q$ (depending once again on the mother's marital status).

The age difference is largest between children born in the first and fourth quarters (from 184 to 264 days) and smallest (from 1 to 183 days) between children born in the third and fourth quarters. For this reason, differences in outcomes between the first and fourth quarters are more likely to capture differences in unobservable characteristics or the fact that parents of older children are more likely to participate in the labor market and to use public kindergartens. This is also the reason why regression discontinuity estimates focus on comparing children born in a small range of dates around the eligibility cutoff (Fitzpatrick, 2010). In this spirit, after discussing the evidence for all quarters of birth in the short-run analysis, I focus the longer-run analysis on the comparison in outcomes between the third and fourth quarters (coefficient α_3). The oldest children I consider for longer-run estimates were born in 1969. Depending on their quarter of birth and state of residence, they became eligible for public kindergarten five or four school years before the survey. Pooling data on mothers who experienced childbirth between 1969 and 1974, I estimate via ordinary least squares the equation

$$Y_t = \sum_{t=0}^{5} \alpha_t \cdot Q3 \times YoB_t + \sum_{t=0}^{5} \beta_t \cdot Q3 \times \{\text{Cutoff}\} \times YoB_t + \text{FE} + \gamma'X_t + \varepsilon \quad (4)$$

where YoB_t is a set of five binary indicators for the year of childbirth and Y_t indicates maternal labor market outcomes in the $(5+t)^{\text{th}}$ calendar year after childbirth.[24] The estimation of Equation (4) only uses data on childbirths occurring in the last two quarters of each year, unlike the short-run estimates, but estimates are not qualitatively affected by including data on all four quarters. The indicator variable $Q3$ takes value one for women who experienced childbirths in the third quarter of each year and zero for the fourth

quarter. The coefficients α_t capture *ceteris paribus* differences in maternal outcomes by quarter, in states where the quarter of birth has no impact on public kindergarten eligibility, $5 + t$ years after childbirth. The analogous difference in states with a Quarter cutoff is captured by the sum $\beta_t + \alpha_t$. The coefficients α_t capture spurious correlations between quarters of birth and maternal outcomes, and β_t the longer intention-to-treat effect of kindergarten eligibility, $t + 5$ years after childbirth.

As a benchmark, I also estimate the longer-run effects via simple difference:

$$Y_t = \sum_{t=0}^{5} \tilde{\beta}_t \cdot Q3 \times YoB_t + FE + \gamma' X_t + \varepsilon \tag{5}$$

Assume that childbirth quarters are correlated to maternal labor market outcomes only through eligibility for public kindergartens. Under this assumption, we should expect null estimates for α_t in Equation (4). We should also expect the simple-difference estimates for $\tilde{\beta}_t$ in Equation (5) to be smaller than the estimates for β_t from Equation (4). Conversely, if the quarter of birth is correlated with maternal outcomes through other channels, the estimates for $\tilde{\beta}_t$ from Equation (5) would be biased by the presence of spurious correlations and should not be interpreted as the intention-to-treat effect of public kindergarten. As largely discussed below, the comparison of the estimates for $\tilde{\beta}_t$ from Equation (5) and for β_t from Equation (4) suggests that spurious effects are sizable.

Finally, note that the responsiveness of mothers' behavior is likely to vary with income and education level and the presence of additional younger children in the household. The wage benefits due to the higher participation in the market induced by kindergarten may also differ by education level, because of complementarity between experience and education (Blundell et al., 1999, 2013; Dustmann and Meghir, 2005). To identify possibly heterogeneous effects on labor supply and earnings, I report separate estimates for the relevant subpopulations in the longer-run analysis.

All regressions include state fixed effects and demographic characteristics (age, years of education and educational attainment, number of children by age), and the standard errors are clustered at the state level.[25]

5 | RESULTS

Causal identification of the impact of kindergartens on maternal labor supply is complicated by the fact that the use of public kindergartens correlates with individual and household characteristics: As some of these cannot be observed, the *ceteris paribus* condition is hard to meet. Table 2 shows substantial differences in observable characteristics between mothers of children who are enrolled in public kindergarten at age five and mothers of children who are not, in the sample used for the main analysis.[26] The third column reports the values for the *t*-test of equal means, with unequal variances.

Based on the table, women whose children are enrolled in public kindergarten are slightly younger and less likely to be married or cohabiting, be white or having completed college, and have lower total family income (differences statistically significant at the 5% level).

To the extent that the quarter of birth has an impact on enrollment in public kindergartens, its cross-sectional variation may be leveraged to causally identify the impact on public kindergartens on maternal labor market outcomes. Table 3 shows that the quarter of birth indeed has a statistically significant impact on enrollment rates. The first column reports estimates based on the traditional simple-difference approach (Equation (1)), while the second and third ones report estimates from the difference-in-difference Equation (2). Covariates for maternal (age in linear and quadratic form, education level dummies, race dummies, marital status, and a dummy which equals one if the woman

TABLE 2 Average characteristics, by child's enrollment in public kindergarten

	(1)	(2)	(3)
	Enrolled	**Not enrolled**	**Difference (*t*-test)**
In the labor force	0.57	0.58	1.62
Mother's non-labor income (10^3 USD)	16.48	18.11	21.42
Total family income (10^3 USD)	19.50	21.56	26.18
Social security income (10^3 USD)	0.04	0.03	−2.87
Age	30.44	30.68	7.90
Married/Cohabiting	0.81	0.85	18.78
High School	0.47	0.43	−12.27
College or higher	0.28	0.35	27.48
Has no younger child	0.53	0.52	−1.78
Race: white	0.82	0.86	21.53
Quarter of birth	2.32	3.02	114.21
Number of own children in HH	2.60	2.52	−12.01
Observations	103,384	44,237	147,621

has additional younger children) and household characteristics (total and social security income) are included.

The first two columns suggest that, controlling for the full set of characteristics listed above, the enrollment rate in public kindergartens is between 30% and 39% points higher among children born in any of the first three quarters of the year, with respect to those born in the fourth quarter. Importantly, the point estimates obtained on the entire sample (column 1) are similar to those obtained on the double-difference sample (column 2).

The third column further reveals that statistically significant differences of around 7 to 10% points in enrollment rates by quarter of birth exist in states where quarter of birth has no impact on kindergarten eligibility (coefficients on $Q1$, $Q2$, $Q3$). Such differences may be due to spurious correlations, including the fact that children born in the first three quarters are simply older than those born in the fourth. In states where the quarter of birth determines eligibility, these differences are 40–45% points larger (coefficients on $Q1 \times$ Quarter Cutoff, $Q2 \times$ Quarter Cutoff, $Q3 \times$ Quarter Cutoff): This can be interpreted as the effect of kindergarten eligibility.

To limit the differences in children's age, Column 4 only considers children born in the 3rd and 4th quarters of 1974. The difference-in-difference estimator on this sub-sample ($Q3$ in column 4, which obviously is equivalent to the one estimated in Column 3, with lower precision due to the drop in sample size) confirms that (i) enrollment is higher for children born in the third quarter, and (ii) significantly more so in states where the quarter of birth affects eligibility. The 40% points increase in enrollment identified in Column 4 corresponds to a 57% increase with respect to the overall enrollment rate in the difference-in-difference sample (68%, Table 1) and to a 100% increase with respect to children born in the fourth quarter (whose enrollment rate is 40%).

The sign and size of the other coefficients are mostly consistent with previous findings in the literature. In particular, the use of public kindergarten is less common among mothers with a higher education level or income and among married mothers and white mothers (Gelbach, 2002; Herman, 2007).[27]

TABLE 3 Impact of quarters of birth on enrollment

	Simple difference, Eq. (1)		Difference-in-difference, Eq. (2)	
	Full sample	**Double-difference sample**	**All quarters**	**Quarters 3, 4**
	(1)	**(2)**	**(3)**	**(4)**
$Q1 \times$ Cutoff			0.452***	
			(0.034)	
$Q2 \times$ Cutoff			0.435***	
			(0.033)	
$Q3 \times$ Cutoff			0.405***	0.405***
			(0.034)	(0.035)
$Q1$	0.375***	0.391***	0.094***	
	(0.038)	(0.065)	(0.009)	
$Q2$	0.375***	0.382***	0.096***	
	(0.038)	(0.062)	(0.011)	
$Q3$	0.309***	0.334***	0.067***	0.067***
	(0.029)	(0.060)	(0.007)	(0.008)
Age	0.003**	0.001	0.002	−0.001
	(0.002)	(0.003)	(0.003)	(0.004)
Age, squared	−0.007***	−0.004	−0.004	−0.001
	(0.002)	(0.005)	(0.005)	(0.007)
Total family income (10^3 USD)	−0.002***	−0.002***	−0.002***	−0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Social Security income (10^3 USD)	0.003	0.004	0.006	0.010
	(0.003)	(0.004)	(0.004)	(0.008)
Married/Cohabiting	−0.007	−0.008	−0.008	−0.021*
	(0.004)	(0.009)	(0.009)	(0.011)
High School	−0.013**	−0.004	−0.004	−0.005
	(0.006)	(0.007)	(0.007)	(0.009)
College or higher	−0.066***	−0.059***	−0.059***	−0.055***
	(0.007)	(0.011)	(0.011)	(0.011)
Has no younger child	−0.066***	−0.059***	−0.059***	−0.055***
	(0.007)	(0.011)	(0.011)	(0.011)
Race: white	−0.082***	−0.129***	−0.129***	−0.153***
	(0.015)	(0.022)	(0.023)	(0.024)
Constant	−0.533***	−0.560***	−0.554***	−0.630***
	(0.037)	(0.078)	(0.055)	(0.076)
No. Observations	147,734	47,528	47,528	24,625
R^2	0.168	0.195	0.231	0.245

Note: Linear probability model. Column 1: all states, children born in any quarter of 1974. Column 2: only states listed in Table B1, children born in any quarter of the year. Column 3: only states listed in Table B1, children born in the 3rd or 4th quarters of 1974. Standard errors, in parentheses, are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Exploiting the availability of data on enrollment in public and private schools and kindergartens, I show that the enrollment in private kindergarten is also affected by the quarter of birth: Around 17% of children in the full and difference-in-difference samples were enrolled in private kindergartens, pre-schools, or schools in the school year 1979/1980. A simple-difference comparison, controlling for all covariates in Table 3, suggests that in the full sample the enrollment is about 9–11% points lower for children born in the first three quarters than those born in the fourth (estimates in Column 1 of Table C1 in the Appendix). Difference-in-difference reveals that these gaps are only about 3–4% points in states where the quarter of birth does not impact eligibility and about 7.5–9% points wider in states where it does (estimates in Column 2 of Table C1 in the Appendix). In other words, considering children born in the third quarter in the difference-in-difference sample, the 40% points increase in enrollment in public kindergartens/schools is partially (about 19%) compensated by the 7.5 to 9% points decrease in enrollment in private ones: This effect is commonly referred to as *crowding-out*.

The simple-difference estimates for the short-run impact of public kindergarten eligibility on several measures of maternal labor supply and labor market outcomes, obtained from Equation (1), are shown in Table 4. The controls variables included are age (in linear and quadratic form), education level (through binary variables for high school and college), race (with a binary variable for "white" race), marital status (a binary variable for currently married/cohabiting as opposed to never married, divorced, separated, or widowed), and state, year, and state-year fixed effects.

The estimates in Table 4 identify statistically significant differences across quarters of childbirth in most outcomes, except for hourly wages (this is captured by the single coefficients $Q1$ and $Q2$ and by the F test of joint significance for $Q1, Q2, Q3$ shown in the bottom of the table). These differences are mostly found between the fourth and the first two quarters, while the differences in labor market outcomes between the fourth and third quarters are smaller in size and mostly not statistically significant. In contrast, the differences in public kindergarten enrollment reported in Table 3 do not greatly differ for the first three birth quarters. It is hard to reconcile this fact with the notion that birth quarter captures the impact of kindergarten eligibility on labor market outcomes. In particular, it is hard to tell to what extent such differences may be due to access to public kindergarten, rather than to children age since, at any point in time, children born in the first two quarters are 4–11 months older than those born in the fourth quarter of the same calendar year.

As discussed in Section 4, the use of double differences allows me to leverage differences in legislation across states to improve identification. However, this method also imposes a drastic reduction in sample size, as only some states can be included, due to their kindergarten eligibility regulations. This sample reduction does not result in a distortion: The bottom panel of Table 4 shows that the simple-difference estimates for this smaller sample are largely in line with those based on the entire sample. If anything, the differences across quarters of birth are somewhat larger in the smaller sample.

More insights on the mechanisms which could explain the correlation between quarters of birth and maternal outcomes come from the difference-in-difference estimates in Table 5. The table contains estimates for all mothers of children born in any quarter of 1974 in the selected states, based on Equation (2), where the baseline group is that of mothers of children born in the fourth quarter and living in states where the quarter of birth has no impact on kindergarten eligibility.

The estimates in Table 5 largely confirm what is observed in Table 4. The simple-difference coefficients ($Q1, Q2, Q3$) capture statistically significant differences in labor market outcomes in states where birth quarter has no impact on public kindergarten eligibility. These differences are particularly marked for mothers of children born in the second quarter of the year. This indicates that ceteris paribus these women have a higher labor supply (on the extensive as well as intensive margin) and receive lower average wages than mothers of children born in the fourth quarter and living in the same states (i.e., the baseline group). While small in size, these differences are statistically significant and lead to

TABLE 4 Maternal labor market outcomes, simple differences

Eq. (1): $Y_i = \sum_{q=1}^{3} \left(\beta_q \times \{\text{Born in Quarter}_q\} \right) + \text{FE} + \gamma' X + \varepsilon$

	(1) LFP	(2) Employment	(3) Hours	(4) Weeks	(5) Hourly Wage	(6) Labor Earnings
Full sample						
Q1	0.014***	0.012***	0.453***	0.695***	0.101	106.040***
	(0.004)	(0.003)	(0.111)	(0.126)	(0.247)	(29.442)
Q2	0.013***	0.013***	0.396**	0.581***	0.135	90.595***
	(0.004)	(0.005)	(0.148)	(0.188)	(0.179)	(32.983)
Q3	0.006	0.007**	0.170	0.246*	0.088	70.048**
	(0.004)	(0.003)	(0.160)	(0.145)	(0.133)	(34.529)
Age	−0.021***	0.009***	−0.868***	0.061	0.181	128.105***
	(0.002)	(0.002)	(0.079)	(0.099)	(0.193)	(24.479)
Age, squared	0.014***	−0.022***	0.675***	−0.569***	−0.174	−233.178***
	(0.004)	(0.004)	(0.125)	(0.161)	(0.287)	(38.054)
Married/Cohabiting	−0.096***	−0.083***	−4.574***	−4.743***	−0.136	−1082.243***
	(0.011)	(0.011)	(0.425)	(0.512)	(0.132)	(98.372)
High School	0.114***	0.136***	4.029***	6.534***	0.269*	1228.597***
	(0.006)	(0.005)	(0.284)	(0.308)	(0.154)	(52.608)
College or higher	0.188***	0.217***	5.616***	9.342***	1.646***	2429.239***
	(0.012)	(0.009)	(0.408)	(0.402)	(0.153)	(85.651)
Has no younger child	0.182***	0.183***	7.056***	8.948***	−0.281*	1596.407***
	(0.003)	(0.004)	(0.130)	(0.180)	(0.147)	(33.156)
Race: white	−0.093***	−0.074***	−3.459***	−3.766***	−0.851**	−1084.952***
	(0.010)	(0.009)	(0.338)	(0.458)	(0.343)	(128.421)
Constant	1.014***	0.261***	37.054***	19.234***	1.938	1065.046**
	(0.042)	(0.041)	(1.351)	(1.672)	(3.016)	(408.415)
R^2	0.082	0.077	0.090	0.089	0.004	0.079
F: Q1 = Q2 = Q3 = 0	6.677	4.162	7.084	10.154	0.284	5.100
p-value	0.001	0.010	0.000	0.000	0.837	0.004
No. Observations	148,112	148,112	148,112	148,112	79,568	148,112
Double-difference sample						
Q1	0.016**	0.014***	0.527**	0.554**	0.669	169.478***
	(0.007)	(0.005)	(0.215)	(0.214)	(0.620)	(36.292)
Q2	0.012***	0.017**	0.474**	0.783***	0.090	151.001***
	(0.004)	(0.006)	(0.180)	(0.232)	(0.170)	(48.169)
Q3	0.008	0.007	0.282	0.470*	0.408*	137.665**
	(0.005)	(0.004)	(0.225)	(0.256)	(0.220)	(53.964)
Age	−0.020***	0.007	−0.830***	0.129	0.455**	168.505***
	(0.003)	(0.005)	(0.146)	(0.175)	(0.189)	(28.513)
Age, squared	0.012**	−0.020**	0.579**	−0.716**	−0.596*	−305.528***
	(0.005)	(0.008)	(0.230)	(0.286)	(0.294)	(44.178)

(Continues)

TABLE 4   (Continued)

Eq. (1): $Y_i = \sum_{q=1}^{3} \left(\beta_q \times \{Born\ in\ Quarter_q\} \right) + FE + \gamma'X + \varepsilon$

	(1)	(2)	(3)	(4)	(5)	(6)
	LFP	Employment	Hours	Weeks	Hourly Wage	Labor Earnings
Married/Cohabiting	−0.106***	−0.088***	−5.012***	−5.456***	0.190	−1130.595***
	(0.011)	(0.010)	(0.547)	(0.552)	(0.252)	(145.182)
High School	0.121***	0.147***	4.428***	6.960***	0.485*	1283.984***
	(0.011)	(0.007)	(0.406)	(0.342)	(0.249)	(73.156)
College or higher	0.205***	0.240***	6.438***	10.114***	1.759***	2558.029***
	(0.018)	(0.014)	(0.706)	(0.744)	(0.214)	(163.429)
Has no younger child	0.188***	0.196***	7.473***	9.641***	−0.336	1647.566***
	(0.005)	(0.007)	(0.247)	(0.367)	(0.341)	(57.599)
Race: white	−0.111***	−0.086***	−3.557***	−4.213***	−1.174	−898.420***
	(0.011)	(0.013)	(0.473)	(0.585)	(0.886)	(193.380)
Constant	1.049***	0.319***	37.605***	19.749***	−2.893	340.588
	(0.051)	(0.078)	(2.313)	(2.802)	(2.898)	(483.691)
R^2	0.087	0.083	0.090	0.096	0.004	0.085
F: $Q1 = Q2 = Q3 = 0$	3.283	4.442	5.928	5.283	2.368	8.406
p-value	0.039	0.013	0.004	0.006	0.097	0.001
No. Observations	47,652	47,652	47,652	47,652	26,934	47,652

higher yearly labor earnings (approximately 160$ more per year, unconditionally on employment). As discussed above, such differences cannot be attributed to public kindergarten eligibility, as all children in these states are eligible, irrespective of their quarter of birth. Rather, they might be due to differences in unobserved maternal characteristics which are correlated with labor supply and earnings, or to the fact that children born in the second quarter are older than those born two quarters later. It is important to notice that these same unobserved characteristics may also be correlated with kindergarten enrollment: Indeed, as previously observed, a significant statistical association exists between quarters of birth and kindergarten enrollment, controlling for covariates, also in these same states where eligibility does not depend on the quarter of birth (coefficients $Q1$, $Q2$, $Q3$ in Table 3, column 2).

The impact of kindergarten eligibility is captured in Table 5 by the interaction terms $Q1 \times Cutoff, Q2 \times Cutoff$ and, $Q3 \times Cutoff$. The null hypothesis that all three coefficients on $Q1 \times Cutoff, Q2 \times Cutoff$ and $Q3 \times Cutoff$ are equal to zero is rejected at the 1 or 10% level for all outcomes except unconditional labor earnings (F and p-values shown at the bottom of Table 5). This result would confirm Gelbach's (2002) finding that kindergarten positively affects maternal labor supply and earnings.[28]

However, considering each coefficient one by one suggests a different story. Singularly taken, most of these coefficients are not statistically different from zero. Statistically significant impacts are only found for mothers of children born in the first quarter of the year (higher wages, labor earnings, and hours) and the second one (higher hourly wages). The corresponding children are some 94–364 days older than those born in the fourth quarter.[29]

The fact that statistically significant effects are only found for the first two quarters cannot be explained by differences in kindergarten take-up: As observed above, children born in the first three

TABLE 5   Maternal labor market outcomes, difference-in-difference

Eq. (2): $Y = \sum_{q=1}^{3} (\alpha_q \cdot Q_q + \beta_q \cdot Q_q \times \{Cutoff\}) + FE + \gamma'X + \varepsilon$

	LFP	Employment	Hours	Weeks	Hourly wage	Labor earnings
	(1)	(2)	(3)	(4)	(5)	(6)
$Q1 \times$ Cutoff	0.027	0.013	0.952**	0.655	1.569*	125.169*
	(0.016)	(0.012)	(0.403)	(0.486)	(0.841)	(61.955)
$Q2 \times$ Cutoff	−0.006	−0.018	−0.462	−0.587	0.574**	−16.972
	(0.009)	(0.011)	(0.362)	(0.381)	(0.214)	(86.363)
$Q3 \times$ Cutoff	0.018	0.006	0.461	0.187	0.212	72.931
	(0.011)	(0.009)	(0.472)	(0.625)	(0.494)	(127.043)
$Q1$	−0.002	0.006	−0.096	0.127	−0.367	87.544**
	(0.014)	(0.010)	(0.317)	(0.406)	(0.272)	(32.079)
$Q2$	0.016**	0.029***	0.779**	1.170***	−0.285**	162.267***
	(0.006)	(0.008)	(0.286)	(0.250)	(0.116)	(48.231)
$Q3$	−0.005	0.003	−0.021	0.347	0.271	89.681
	(0.010)	(0.007)	(0.412)	(0.577)	(0.425)	(114.085)
Age	−0.020***	0.007	−0.829***	0.130	0.452**	168.674***
	(0.003)	(0.005)	(0.146)	(0.175)	(0.188)	(28.447)
Age, squared	0.012**	−0.020**	0.576**	−0.718**	−0.591*	−305.782***
	(0.005)	(0.008)	(0.230)	(0.286)	(0.292)	(44.071)
Married/ Cohabiting	−0.106***	−0.088***	−5.013***	−5.457***	0.187	−1130.722***
	(0.011)	(0.010)	(0.549)	(0.553)	(0.248)	(145.409)
High School	0.120***	0.147***	4.426***	6.958***	0.488*	1283.675***
	(0.011)	(0.007)	(0.406)	(0.342)	(0.250)	(73.233)
College or higher	0.205***	0.240***	6.437***	10.113***	1.763***	2557.998***
	(0.018)	(0.014)	(0.707)	(0.743)	(0.214)	(163.609)
Has no younger child	0.188***	0.196***	7.474***	9.642***	−0.337	1647.759***
	(0.005)	(0.007)	(0.248)	(0.367)	(0.340)	(57.682)
Race: white	−0.111***	−0.086***	−3.557***	−4.213***	−1.178	−898.400***
	(0.011)	(0.013)	(0.473)	(0.585)	(0.887)	(193.445)
No. Observations	47,652	47,652	47,652	47,652	26,934	47,652
R^2	0.087	0.083	0.090	0.096	0.004	0.085
Joint F test	4.047	5.176	2.696	3.807	2.516	2.015
p-value	0.019	0.007	0.070	0.024	0.083	0.140

Note: All estimates were obtained with OLS. State, year, and state-year fixed effects included. The standard errors, in parentheses, are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The null hypothesis for the joint F test is that the three coefficients $Q1 \times$ Cutoff, $Q2 \times$ Cutoff, and $Q3 \times$ Cutoff are jointly zero.

quarters have, *ceteris paribus*, similar enrollment rates (captured by the coefficients $Q1 \times$ Quarter Cutoff, $Q2 \times$ Quarter Cutoff and $Q3 \times$ Quarter Cutoff in Table 3, which are all statistically significant).

As discussed in Section 2, several studies suggest that larger labor supply effects of public kindergarten might be found on single mothers and those without additional younger children (Fitzpatrick, 2012; Gelbach, 2002). To investigate this possibility, in Table 6 I restrict the attention only to mothers without younger children and estimate Equation (3), where a triple interaction is introduced between each quarter of birth (captured by the three binary variables $Q1$, $Q2$, and $Q3$), the relevance of quarter for eligibility (captured by the binary variable *Cutoff*) and an indicator for not-married mothers (binary variable *Single*). The interactions allow the identification of the extra impact of kindergarten eligibility on single mothers (total impact of kindergarten eligibility captured by $Q1 \times Cutoff + Q1 \times Cutoff \times Single$), compared with non-single ones (impact of kindergarten eligibility captured by $Q1 \times$ Cutoff).

The resulting estimates are reported in Table 6: The reference group is that of cohabiting/married mothers of children born in the fourth quarter of the year and living in states where the quarter of birth does not determine kindergarten eligibility. The relevant joint tests of significance for the coefficients related to quarters one to three, by marital status, are shown at the bottom of the table. The estimates once again confirm the existence of significant differences by quarter of birth in states where the quarter does not determine differences in kindergarten eligibility (for married mothers these are captured by coefficients $Q1, Q2, Q3$, while for single mothers they are captured by $Q1 + Q1 \times Single, Q2 + Q2 \times Single, Q3 + Q3 \times Single$), in line with what observed in Tables 4 and 5. In addition, consistently with previous studies, the estimates also suggest a statistically significant and positive impact of kindergarten eligibility on the labor force participation and the hours of work of married (or cohabiting) mothers who do not have younger children (coefficients $Q1 \times Cutoff$ and $Q3 \times Cutoff$, joint test of significance for all three quarters shown at the bottom of the table). Ceteris paribus, these mothers have a higher labor force participation and higher average hours of work than the reference group (these differences are captured by the sums $Q1 + Q1 \times Cutoff$ and $Q3 + Q3 \times Cutoff$).[30] For mothers of children born in the second quarter, several of the estimated coefficients for $Q2 \times Cutoff$ are similar in size and opposite in sign those for $Q2$. The negative $Q2 \times Cutoff$ coefficients imply that, within the group of states where quarter of birth determines eligibility, mothers of children born in the second quarter are slightly less likely to be employed and have lower hours of work. The fact that these coefficients are opposite in sign and similar in size to the $Q2$ coefficients imply, however, that the outcomes of these mothers are not statistically different from those of the reference group, i.e., mothers of children born in the fourth quarter and living in states where kindergarten eligibility is not affected by the quarter of birth (this difference is captured by $Q2 + Q2 \times Cutoff$).

In line with previous studies, the positive impact of kindergarten eligibility on each considered outcome is stronger, in a statistically significant way, for single mothers without younger children (for each quarter q the additional impact of kindergarten eligibility on these mothers, with respect to married ones, is captured by the coefficients $Qq \times Cutoff \times Single$). For this group of mothers, the association between kindergarten eligibility and most maternal outcomes (all except hourly wages) is positive, significant, and robust across quarters.

All in all, the difference-in-difference estimates in Tables 5 and 6 do not offer robust evidence of a positive impact of eligibility for public kindergarten on any maternal labor market outcome, except for the labor force participation of women without younger children. The fact that the impact of public kindergarten differs by maternal marital status and by the presence of younger siblings is consistent with previous findings in the literature (Fitzpatrick, 2010; Gelbach, 2002). In particular, the presence of younger children may discourage labor market participation and hence attenuate the estimated

TABLE 6 Mothers with no younger children: labor outcomes, by marital status

Eq. (3): $Y = \sum_{q=1}^{3} \left(\alpha_q \cdot Q_q + \beta_q \cdot Q_q \times \{Cutoff\} + \gamma_q \cdot Q_q \times \{Single\} + \delta_q \cdot Q_q \times \{Single\} \times \{Cutoff\} \right) + FE + \gamma'X + \varepsilon$

	LFP	Employment	Hours	Weeks	Hourly wage	Labor earnings
	(1)	(2)	(3)	(4)	(5)	(6)
$Q1 \times$ Cutoff	0.043*	0.030	1.321*	0.988	1.735	−53.882
	(0.024)	(0.021)	(0.677)	(0.878)	(1.533)	(135.162)
$Q2 \times$ Cutoff	−0.007	−0.031**	−0.552	−1.387***	−0.137	−232.717
	(0.012)	(0.015)	(0.547)	(0.478)	(0.352)	(171.647)
$Q3 \times$ Cutoff	0.029**	0.019	0.618	0.478	−0.586	58.886
	(0.012)	(0.012)	(0.595)	(0.758)	(0.497)	(154.777)
$Q1 \times$ Cutoff $\times$ Single	0.128***	0.129***	6.491***	7.339***	−2.165	1732.841***
	(0.016)	(0.017)	(0.817)	(1.155)	(1.606)	(351.154)
$Q2 \times$ Cutoff $\times$ Single	0.164***	0.156***	7.521***	9.427***	0.984**	2083.645***
	(0.017)	(0.026)	(0.842)	(0.951)	(0.385)	(240.193)
$Q3 \times$ Cutoff $\times$ Single	0.088***	0.055*	5.514***	4.949***	0.013	1061.474***
	(0.026)	(0.031)	(1.097)	(1.503)	(0.414)	(276.639)
Single	0.120***	0.108***	4.665***	6.604***	0.940	1500.150***
	(0.011)	(0.017)	(0.616)	(0.854)	(0.611)	(179.922)
Cutoff $\times$ Single	0.128***	0.107***	5.723***	6.110***	−0.153	1272.672**
	(0.027)	(0.027)	(1.609)	(1.536)	(0.288)	(508.757)
$Q1 \times$ Single	0.125***	0.163***	5.054***	7.458***	−0.619*	1209.574***
	(0.018)	(0.016)	(0.957)	(1.046)	(0.349)	(289.831)
$Q2 \times$ Single	0.067***	0.096***	4.448***	5.251***	0.436	1218.939***
	(0.020)	(0.023)	(0.540)	(1.596)	(1.183)	(292.483)
$Q3 \times$ Single	0.124***	0.114***	5.410***	6.322***	−0.278	1142.137***
	(0.017)	(0.025)	(0.604)	(0.967)	(0.562)	(116.249)
$Q1$	−0.019	−0.013	−0.338	−0.381	0.297**	200.679***
	(0.020)	(0.016)	(0.487)	(0.683)	(0.112)	(70.402)
$Q2$	0.013*	0.034***	0.876**	1.610***	0.206	306.212**
	(0.007)	(0.011)	(0.402)	(0.352)	(0.314)	(143.739)
$Q3$	−0.002	0.004	0.020	0.530	0.668	136.827
	(0.008)	(0.005)	(0.393)	(0.453)	(0.438)	(105.511)
Joint F test, Single mothers	43.501	33.820	34.390	48.143	2.526	29.648
p-value	0.000	0.000	0.000	0.000	0.083	0.000
Joint F test	4.531	6.096	3.443	4.825	1.007	1.727
p-value	0.012	0.003	0.033	0.009	0.408	0.189
R^2	0.077	0.061	0.080	0.070	0.004	0.071
No. Observations	25,881	25,881	25,881	25,881	16,509	25,881

Note: Estimates based on the sample as Table 5, adding interaction terms for marital status and presence of young children. All estimates were obtained with OLS. State, year, and state-year fixed effects included. The standard errors, in parentheses, are clustered at the state level. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. The null hypothesis for the (bi-lateral) joint test for single mothers is that the coefficients $Q1 \times$ Cutoff $\times$ Single, $Q2 \times$ Cutoff $\times$ Single and $Q3 \times$ Cutoff $\times$ Single are jointly zero. Similarly, the "Joint F test" refers to the null hypothesis that the coefficients $Q1 \times$ Cutoff, $Q2 \times$ Cutoff, and $Q3 \times$ Cutoff are jointly zero.

impact of public kindergarten eligibility on labor outcomes in the full sample. For the general population of mothers, the evidence is far from robust: The estimates are inconsistent across quarters and generally skewed toward the second quarter, suggesting that they may capture additional factors beyond kindergarten eligibility.[31] In particular, the data do not offer any strong support to the theoretical predictions of a positive impact on employment and a negative impact on hourly wages.[32]

In addition, the difference-in-difference estimates suggest that a statistically significant correlation exists between quarters of birth and maternal labor market outcomes, in states where the quarter of birth has no impact on kindergarten eligibility. While quarter of birth may be a valid instrumental variable in many applications, the results in Table 5 speak against its validity in this context. In this context, the exclusion restriction requires the instruments to have no impact on the outcomes of interest (maternal labor outcomes) other than through their effect on the eligibility for public kindergarten. Tables 3 and 5 show instead that in states where they do not affect eligibility for public kindergarten, the quarter of birth indicators are correlated with both public kindergarten enrollment and maternal labor market outcomes. For this reason, I do no report instrumental variable estimations.

The next section provides evidence on the evolution of the impacts in the longer run.

5.1 | *Longer-run Impacts*

The short-run estimates above are derived from the sample of mothers who gave birth in 1974. At the time of the survey, these women's children differ in their eligibility to enroll in public kindergartens, based on the state of residence and quarter of birth. These in turn induce small differences in mothers' labor market outcomes in the same year. I consider this a shorter-run impact. In contrast, I refer to the differences found among mothers of older children as longer-run impacts.

Even in the absence of short-run impacts, mothers of kindergarten-eligible children might exhibit better labor market outcomes in the longer run. One channel through which this may happen is offered by investments in human capital. A simple-difference comparison of maternal enrollment in formal schooling, for example, shows that in the fifth year after childbirth mothers are more likely to be enrolled in school if their children were born in the first two quarters of the year.[33] In the longer run, this may lead them to better employment opportunities, or higher earnings. For this reason, I present longer-run estimates for each outcome, irrespective of whether a significant impact is found in the shorter run.

To estimate the longer-run labor market impacts, I apply the difference-in-difference methodology to the sample of mothers of children born in the third or fourth quarter, in the years between 1970 and 1974, a described in Equation (4). Their children would have qualified (if born in the first three quarters) or not qualified (if born in the fourth quarter) to enter public kindergarten one to five calendar years before the census. If significant differences were found among these mothers, I would interpret them as evidence that the small gaps identified in the shorter run persist in the longer run and expand.[34]

The estimated longer-run effects on each outcome of interest, from Equation (4), are reported in the Appendix (Table C4 and C5) and graphically summarized in Figures 2–5. The figures show the ordinary least-squares estimates and 95% confidence intervals for the double-difference (α_t) in Equation (4). The difference-in-difference estimates equal the difference in *ceteris paribus* average outcomes between mothers of children born in the third and fourth quarters of the given cohort year in states where the quarter of birth affects kindergarten eligibility, net of the same difference in states where the quarter of birth does not affect kindergarten eligibility.[35] Each regression also includes the full set of covariates used for the short-run (age, marital status, educational attainment, race, and presence of
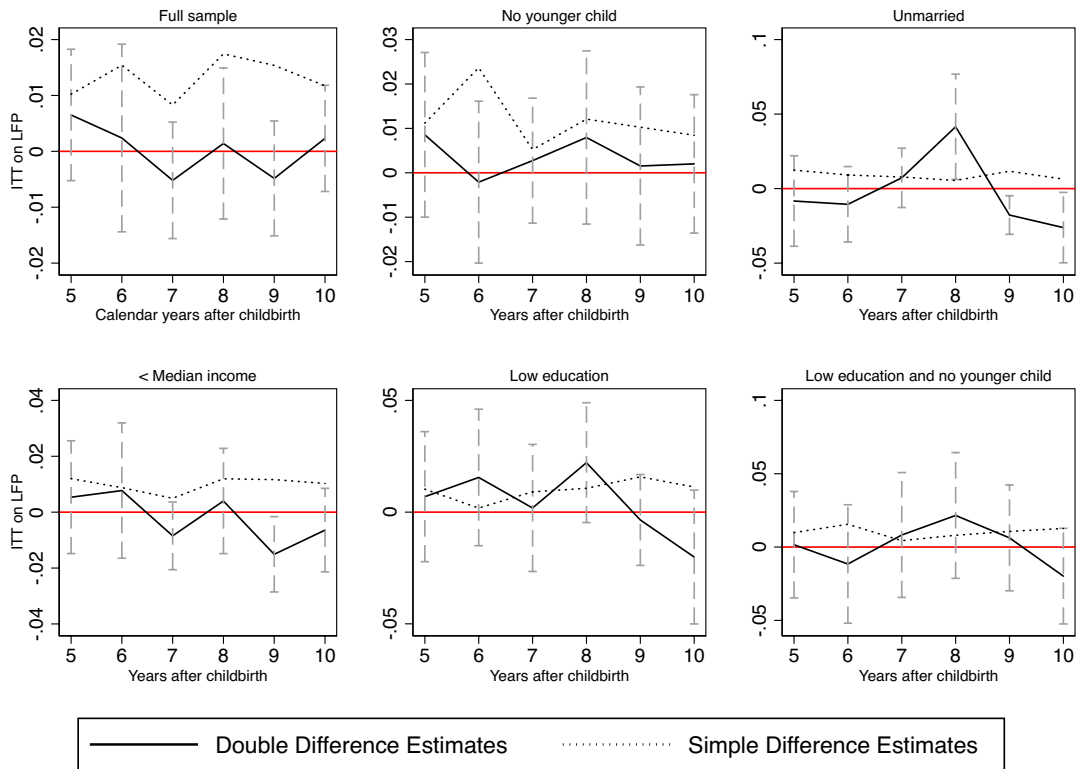
FIGURE 2 Longer-run effects, labor force participation [Colour figure can be viewed at wileyonlinelibrary.com]

younger children), plus a set of indicator variables for the child's cohort of birth, state, and state-and-cohort interactions. The figures also show the simple-difference estimates for β_t from Equation (5), which include all states, irrespective of the eligibility cutoff in use.

The six plots in Figure 2 show the estimated longer-run impact on LFP for the entire sample of mothers, for those who do not have additional younger children, those who are not married (this category includes single, widow, divorced, and separated mothers), those with income below the median and/or those who have not attended college or an equivalent educational institution. In each plot, the horizontal axis shows the numbers of years after childbirth: The estimated effects five years after childbirth correspond to the short-run estimates presented in the previous section. All sub-samples exhibit a similar pattern: the double-difference coefficients (α_t in C4) are not statistically significant and they oscillate in a relatively narrow range around zero. The fact that simple-difference estimates based on Equation (1) (coefficients β_t in Table C4) are systematically larger and mostly statistically significant confirms the presence of spurious correlations between quarters of birth and maternal labor force participation, also among children older than five. Based on the double-difference estimates, there is no evidence of public kindergarten inducing a long-lasting gap in maternal labor force participation.

Figures 3 and 4 suggest similar conclusions also for employment and hours of work: The difference-in-difference estimates suggest that kindergarten eligibility has no impact on these outcomes, for any of the considered subpopulations of mothers, while simple-difference estimates are in general larger and often statistically significant.

As previously mentioned, even in the absence of an effect on the intensive and extensive margins of labor supply, having access to public kindergarten might allow mothers to select better occupations,
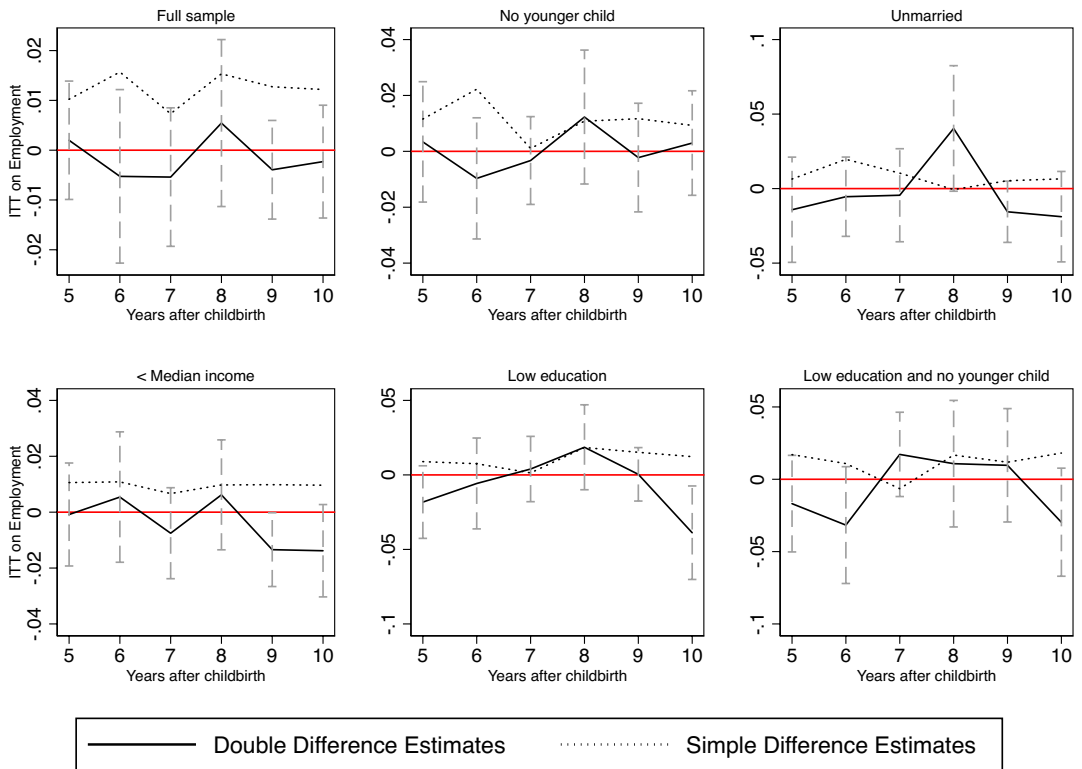
FIGURE 3 Longer-run effects, employment [Colour figure can be viewed at wileyonlinelibrary.com]

by making it easier to combine private and family life and careers. Such benefits may be captured by labor earnings, but Figure 5 offers no evidence of such effects: In the longer run, the null hypothesis of no effect cannot be rejected for any of the sub-samples of mothers considered. An exception is found for mothers with low education and no younger children: In this subpopulation, public kindergarten eligibility is associated with lower earnings in the sixth year after childbirth. It is hard to interpret this as evidence of an effect of public kindergarten because no effect is found for mothers of younger or older children.[36]

6 | DISCUSSION

In this section, I first discuss some possible reasons for the lack of evidence of robust short- or longer-run impacts of public kindergarten eligibility on maternal labor market outcomes. Then, I present additional empirical evidence about the possibility that migration and strategic timing of birth might bias my main results.

The short- and longer-run analysis identifies the effects of having access to public kindergarten in the fifth year after childbirth, in comparison with having access one calendar year later. A few different reasons might explain why the estimated impacts are small and rarely statistically significant (besides the possible lack of statistical power). First of all, this is an intention-to-treat effect, and its size depends on compliance: How many of the children who are eligible for public kindergarten are actually enrolled, and how many non-eligible children obtain an exception and can enroll. Second, public kindergarten might not offer enough hours per day to significantly help mothers combine family and
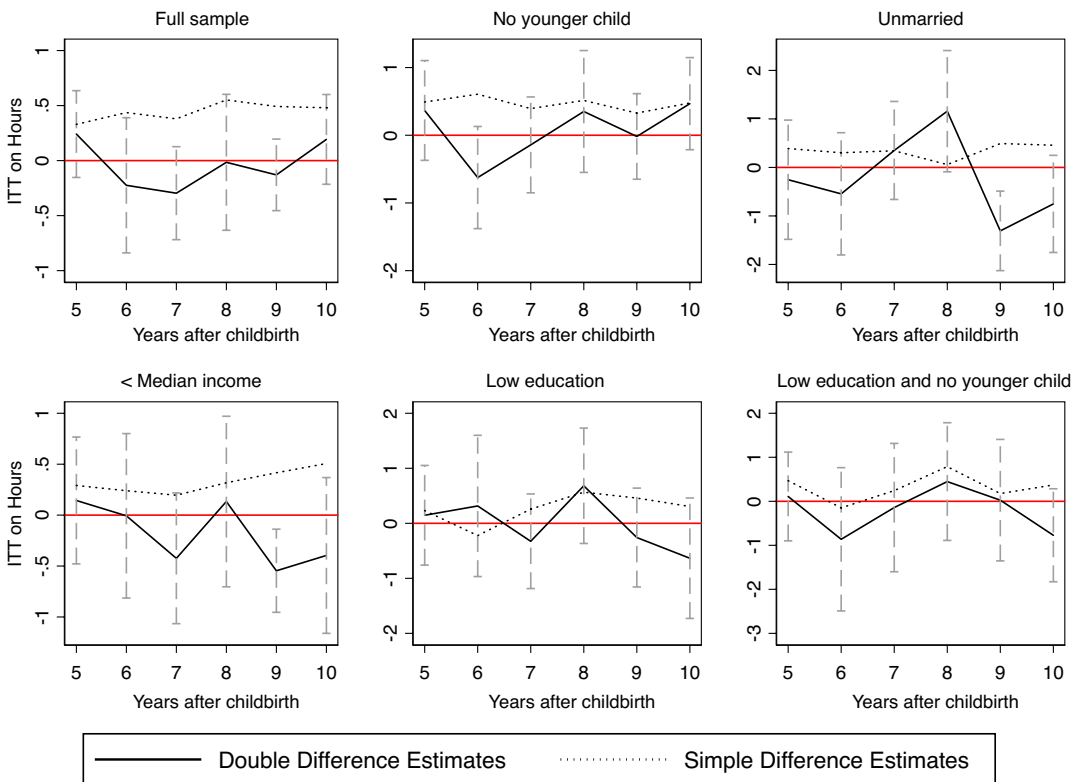
FIGURE 4 Longer-run effects, hours of work [Colour figure can be viewed at wileyonlinelibrary.com]

careers. Third, the impact of public kindergarten clearly depends on the availability of alternative arrangements, such as informal and private care. Last but not least, having access to such a service after five years might be too late: those previous five years are likely to have a lasting effect on mothers' chances and willingness to be employed, by affecting their human capital and the division of household chores within the household.

Regarding the first point, Table 3 shows that compliance is relatively high. In addition, it is possible to leverage children enrollment-in-kindergarten information and estimate the local average treatment effect (or average effect on compliers) in the short run: The validity of this approach relies on the assumption that quarters of birth have no impact on maternal outcomes, other than through public kindergarten (exclusions restriction assumption).[37] My estimates warn that such assumption is likely not satisfied. Ignoring this important warning and estimating the local average treatment effect (LATE) brings to estimated impacts that are in line with previous estimates in the literature. Related to this point, it should be noted that the average impact of kindergarten eligibility crucially depends not only on how many but also on which mothers decide to use the service: The literature documents important differences in take-up by socioeconomic status (Hermes et al., 2020).

The second point to consider is how many hours are offered in public kindergartens: While these typically vary across states and institutes, in general, public kindergartens often come short on the number of hours needed by a full-time working parent, especially in the period analyzed. According to a 2016 report by the Education Commission of States, things have not dramatically improved even in more recent years: Districts are required to offer full-day kindergarten only in 13 states and the district of Columbia (Parker et al., 2016).[38]
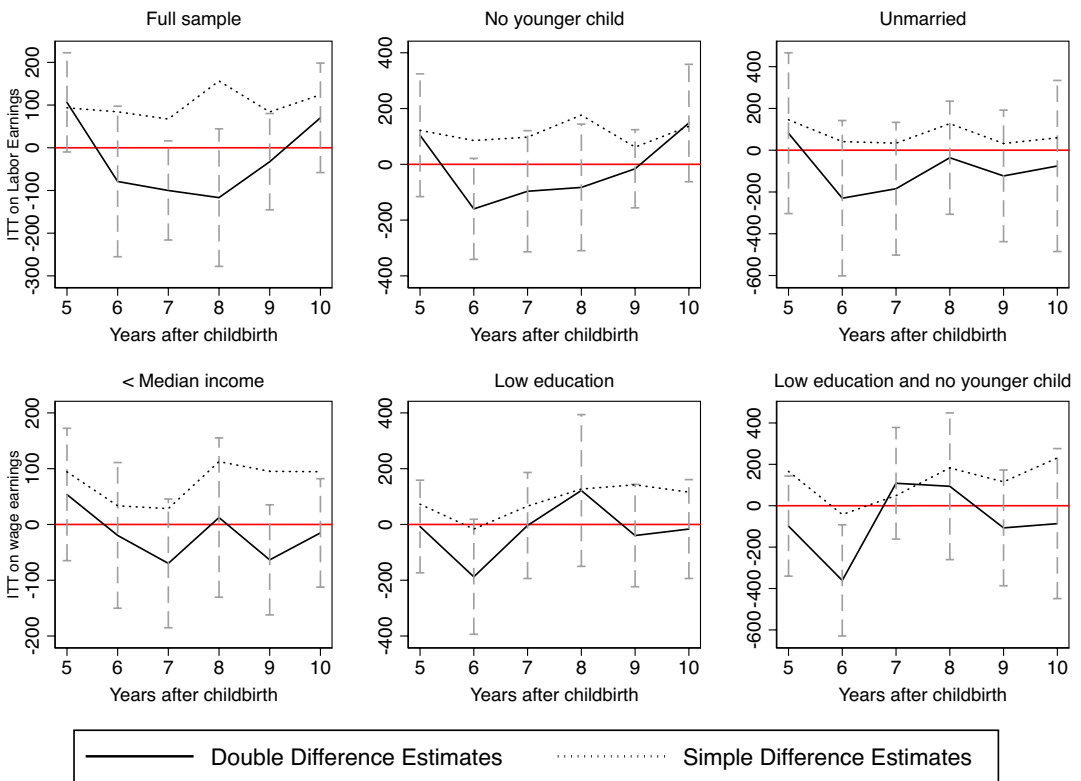
FIGURE 5 Longer-run effects, earnings [Colour figure can be viewed at wileyonlinelibrary.com]

The third and fourth points are closely related: The impact of public kindergarten clearly depends on the quality, availability, and costs of alternative arrangements, and families are particularly likely to know and use such arrangements in contexts where public kindergarten starts many years after childbirth and paid parental leave is absent. As outlined in Section 1, alternative arrangements were relatively non-rationed in the United States in the period considered and were largely informal. At the same time, no federal provision existed for paid maternal or parental leave and very few firms offered any (unpaid) leave.[39] Therefore, working mothers had to rely for several years on mostly informal private arrangements: In such context, public kindergarten would reduce the opportunity cost of working, but not significantly affect the time constraints of working parents. The reduction in the opportunity cost of working of course introduces both an income and a substitution effect: One possible explanation why maternal labor supply is not strongly affected is that these two forces balance each other (Gelbach, 2002).

Considering all these factors, the *timing* of access to public kindergarten may simply affect the *timing* of (re-)entry into the labor market and have no impact on mothers who already (re-)entered before the fifth year and on those who simply will not (re-)enter. In this sense, my findings that public kindergarten eligibility has a strong crowding-out effect on enrollment in private kindergartens and that it does not induce major shifts in labor supply are consistent with the "childcare-parental leave interaction" hypothesis (Akgündüz et al., 2020).

By relying on differences in legislation across states, my identification strategy allows me to credibly control for differences in unobservable maternal characteristics which correlate with the date of childbirth. What it does not control for, is reverse causality: The possibility that mothers who wish to (re-)enter the labor market early might move to states which grant them early access to public

kindergarten, or that they could even strategically choose when to become pregnant or when to give birth (if giving birth through C-sections, for example), in response to the state legislation. For example, Dickert-Conlin and Elder (2010) discuss the possibility that date-of-birth criteria may induce a response in the timing of planned parenthood.[40] Selective migration and timing of birth could both result in an upward bias in the estimated impact of public kindergarten eligibility. Empirically, I can perform a few robustness checks, to evaluate the relevance of both concerns in the context and sample used for my study.

To address the concern about migration, I restrict the sample to mothers who have not migrated from out of state in the last five years (this *de facto* excludes mothers who migrated after giving birth, corresponding to about 6% of my short-run sample). The estimates for the non-migrant sample confirm the finding on the full sample: There is no evidence of statistically significant effects of kindergarten eligibility. The estimated short-run intention-to-treat (ITT) impacts on each labor market outcome are reported in Table 7.

Migration status in the last five years is only observed for about 50% of my sample: Further dropping from the sample the observations without migration information yields the estimates reported in the bottom panel of Table 7. Overall, restricting the sample to non-migrants leads to smaller estimates, but all estimated impacts are statistically null: While I cannot reject the hypothesis that inverse causality might affect the estimates, this sanity check simply confirms the lack of any robust impact of public kindergarten eligibility on maternal outcomes.

To address the issue of timing of birth, I compare the distributions of births (by quarter) across states which adopt an eligibility cutoff around the end of the third quarter and those which do not, in the years 1969 till 1974. The two distributions, depicted in Figure 6, appear visually very similar.

An auxiliary regression confirms that a binary variable which equals one if the state adopts an eligibility cutoff around the end of the third quarter does not explain any of the variation in the distribution of quarters of birth for children younger than 5 years old.[41] To test the null hypothesis of no association between the state adoption of a quarter cutoff and the quarter of birth for children born between 1969 and 1974, I compute Pearson's ξ^2. As the ξ^2 equals 0.7639, with a *p*-value of 0.382, the null hypothesis of no association cannot be rejected: In other words, graphical inspection, Pearson's test, and the ancillary logistic regression show no evidence of timing of births responding to the state-specific cutoffs.

According to Bedard and Dhuey (2012), the states of Wisconsin and Kentucky changed their cutoffs in 1979: Kentucky switched from December 31st to October 1st, while Wisconsin changed from December 1st to September 1st. If parents time childbirths in response to existing legislations concerning entrance to public kindergartens and schools, the parents of children born in Kentucky and Wisconsin in 1974 would have not been able to do so, because their children were already born when the cutoffs were altered. If the estimated impact of quarter of birth was higher in other states than in Kentucky and Wisconsin, we could interpret this as indirect evidence of reverse causality, in the form of timing of births. This is because timing of birth effects would only be possible in the other states and not in Wisconsin and Kentucky. Table 8 reports the simple-difference intention-to-treat-effects for states adopting a quarter cutoff in 1979 (Equation (1)). The coefficients on $Q1$, $Q2$, $Q3$ correspond to the *ceteris paribus* differences in maternal outcomes between each quarter of childbirth and the fourth. The top panel reports such average differences in Kentucky and Wisconsin, and the bottom panel for all other states using a quarter cutoff.

Some of the estimated differences are larger in the bottom panel and others are smaller. Interestingly, the differences between third and fourth quarters are only statistically significant (for some outcomes) in states where timing of birth cannot be excluded: The hypothesis that the correlations between childbirth quarters and maternal outcomes might be reinforced by parental response to existing legislations can, therefore, not be rejected.

TABLE 7 Short-run impacts, non-migrants

Eq. (2): $Y = \alpha_{ITT} \cdot \{\text{Born in Q3}\} * \{QuarterCutoff\} + \beta \cdot \{BorninQ3\} + FE_{State} + \gamma'X + \varepsilon$

	LFP	Employment	Hours	Weeks	Hourly Wage	Labor Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Sample: Non-migrants (Less Restrictive)						
Q1 × Quarter Cutoff	0.008	0.005	0.115	0.266	0.083	−15.698
	(0.005)	(0.004)	(0.231)	(0.247)	(0.184)	(61.229)
Q2 × Quarter Cutoff	0.009	0.002	0.146	0.166	0.083	1.377
	(0.006)	(0.005)	(0.198)	(0.243)	(0.256)	(55.198)
Q3 × Quarter Cutoff	0.008	0.008*	0.248	0.395**	−0.056	19.848
	(0.006)	(0.004)	(0.236)	(0.183)	(0.190)	(44.284)
Q1	0.008***	0.008**	0.406***	0.456***	−0.136	114.934**
	(0.003)	(0.003)	(0.111)	(0.148)	(0.150)	(48.863)
Q2	0.004	0.006	0.183	0.280	−0.088	60.009
	(0.005)	(0.004)	(0.157)	(0.202)	(0.152)	(48.560)
Q3	0.001	0.000	0.014	−0.142	0.054	14.459
	(0.004)	(0.002)	(0.160)	(0.118)	(0.100)	(34.763)
Age	0.005***	0.027***	0.065	1.049***	0.243**	256.089***
	(0.002)	(0.002)	(0.075)	(0.088)	(0.108)	(15.763)
Age, squared	−0.019***	−0.045***	−0.565***	−1.819***	−0.287*	−403.720***
	(0.002)	(0.003)	(0.105)	(0.131)	(0.158)	(23.422)
Married/Cohabiting	−0.114***	−0.092***	−5.331***	−5.706***	−0.094	−1296.316***
	(0.011)	(0.010)	(0.557)	(0.552)	(0.106)	(151.501)
High School	0.123***	0.153***	4.430***	7.123***	0.374***	1394.896***
	(0.008)	(0.006)	(0.330)	(0.277)	(0.094)	(68.690)
College or higher	0.191***	0.232***	6.237***	9.789***	1.684***	2838.109***
	(0.014)	(0.012)	(0.587)	(0.597)	(0.105)	(158.617)
Has no younger child	0.153***	0.164***	6.288***	8.300***	−0.226***	1500.834***
	(0.005)	(0.003)	(0.139)	(0.154)	(0.080)	(38.616)
Race: white	−0.094***	−0.078***	−3.160***	−4.176***	−0.887***	−936.616***
	(0.010)	(0.011)	(0.438)	(0.518)	(0.272)	(199.467)
No. Observations	289,765	289,765	289,765	289,765	176,384	289,765
R^2	0.073	0.074	0.074	0.084	0.003	0.082
Sample: Non-migrants (More Restrictive)						
Q1 × Quarter Cutoff	0.004	0.002	0.002	0.144	−0.055	−81.987
	(0.006)	(0.006)	(0.309)	(0.316)	(0.171)	(88.720)
Q2 × Quarter Cutoff	0.010	0.002	0.145	0.272	−0.100	17.069
	(0.008)	(0.008)	(0.343)	(0.339)	(0.174)	(91.706)
Q3 × Quarter Cutoff	0.010	0.009	0.287	0.554*	0.034	3.246
	(0.007)	(0.006)	(0.279)	(0.313)	(0.261)	(74.269)
Q1	0.007**	0.007	0.313**	0.422**	0.002	137.486*

(Continues)

TABLE 7 (Continued)

Eq. (2): $Y = \alpha_{\text{ITT}} \cdot \{\text{Born in Q3}\} * \{QuarterCutoff\} + \beta \cdot \{\text{BorninQ3}\} + \text{FE}_{\text{State}} + \gamma'X + \varepsilon$

	LFP	Employment	Hours	Weeks	Hourly Wage	Labor Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
	(0.003)	(0.004)	(0.143)	(0.153)	(0.095)	(66.670)
Q2	0.001	0.003	0.046	0.047	0.185	12.574
	(0.006)	(0.005)	(0.284)	(0.285)	(0.144)	(81.840)
Q3	−0.001	−0.002	−0.110	−0.302	0.126	23.106
	(0.004)	(0.004)	(0.143)	(0.260)	(0.202)	(62.480)
Age	0.004*	0.028***	0.001	1.091***	0.081	262.657***
	(0.002)	(0.003)	(0.089)	(0.115)	(0.179)	(23.191)
Age, squared	−0.018***	−0.045***	−0.465***	−1.869***	−0.056	−413.787***
	(0.003)	(0.004)	(0.132)	(0.169)	(0.262)	(33.249)
Married/Cohabiting	−0.114***	−0.091***	−5.201***	−5.791***	−0.092	−1303.064***
	(0.011)	(0.010)	(0.565)	(0.525)	(0.152)	(155.625)
High School	0.124***	0.156***	4.557***	7.094***	0.235**	1387.239***
	(0.009)	(0.007)	(0.329)	(0.287)	(0.101)	(70.644)
College or higher	0.198***	0.239***	6.540***	10.076***	1.860***	2973.798***
	(0.014)	(0.012)	(0.607)	(0.549)	(0.125)	(166.937)
Has no younger child	0.150***	0.160***	6.243***	8.258***	−0.235***	1517.856***
	(0.005)	(0.004)	(0.153)	(0.165)	(0.071)	(40.490)
Race: white	−0.096***	−0.080***	−3.338***	−4.159***	−0.544***	−884.132***
	(0.011)	(0.012)	(0.465)	(0.599)	(0.189)	(186.684)
No. Observations	134,571	134,571	134,571	134,571	82,364	134,571
R^2	0.075	0.076	0.077	0.087	0.008	0.086
Demographic controls	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

7 | CONCLUSIONS

In the United States, some children are eligible to enter public kindergarten in the fifth year after birth, while others must wait one year longer. Access to public schools for children, this young can be thought of as subsidized childcare: Existing literature has suggested positive effects on maternal employment from having early access to public kindergarten.

To disentangle such effects in the short as well as the longer run from any endogeneity bias, I exploit two distinct sources of variation in the time of entry to public kindergarten. The first source is across-subjects variation in birth dates for children who live in the same state; the second source is variation in regulations across states. While birthdate has been leveraged in previous studies, its exogeneity to factors affecting maternal labor supply and other outcomes has later been challenged. Combining the two sources of variation in a difference-in-difference approach improves identification.
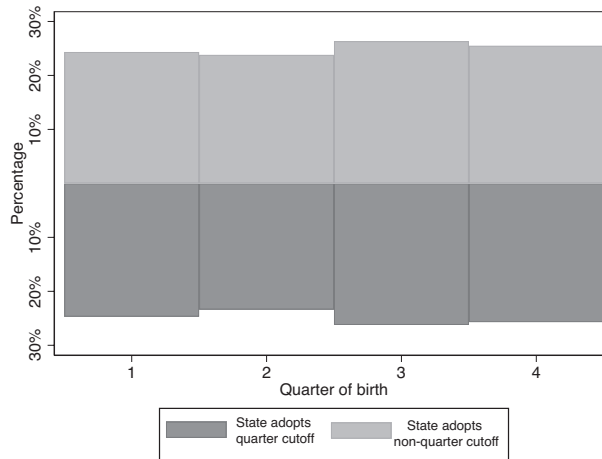
FIGURE 6 Distribution of quarter of birth, by state adoption of quarter cutoff

TABLE 8 Timing of births? Short-run impacts, simple difference

Eq. (1): $Y = \beta \cdot \{Q3\} + \gamma'X + \varepsilon$						
	LFP	**Employment**	**Hours**	**Weeks**	**Hourly wage**	**Labor earnings**
WI, KY						
Q1	0.018**	0.012*	0.537**	0.668**	0.128	100.935
	(0.007)	(0.007)	(0.272)	(0.320)	(0.210)	(69.316)
Q2	0.006	0.004	−0.051	0.093	0.005	52.827
	(0.007)	(0.007)	(0.274)	(0.322)	(0.212)	(69.843)
Q3	0.001	0.002	−0.022	0.207	0.385*	49.601
	(0.007)	(0.007)	(0.268)	(0.315)	(0.207)	(68.222)
No. Observations	37,028	37,028	37,028	37,028	21,479	37,028
R^2	0.076	0.083	0.060	0.079	0.008	0.073
Demographic controls	✓	✓	✓	✓	✓	✓
Other states with quarter cutoff						
Q1	0.016***	0.013***	0.518***	0.694***	−0.068	91.291***
	(0.003)	(0.003)	(0.122)	(0.143)	(0.152)	(32.154)
Q2	0.014***	0.010***	0.348***	0.519***	−0.009	60.729*
	(0.003)	(0.003)	(0.123)	(0.144)	(0.152)	(32.307)
Q3	0.010***	0.008***	0.292**	0.273*	−0.052	40.073
No. Observations	184,175	184,175	184,175	184,175	113,472	184,175
R^2	0.069	0.068	0.072	0.080	0.003	0.074
Demographic controls	✓	✓	✓	✓	✓	✓

Note: $* p < 0.1$, $** p < 0.05$, $*** p < 0.01$.

My estimates suggest that maternal outcomes are correlated with childbirth quarters, even in states where the latter have no impact on children's date of entry to public kindergartens. In addition, these correlations (i) are not systematically higher in states where they could capture the effect of public kindergarten

eligibility; (ii) are somewhat smaller in states where kindergarten reforms rule out the possibility that part of the correlations might be due to timing-of-birth effects. All in all, my estimates do not support the claim that eligibility to enroll one's child in public kindergartens has any sizable effect on maternal labor supply, hourly wages, or total labor earnings in the shorter or longer run. The positive association between quarters of birth and maternal labor market outcomes may be attributed to endogenous mechanisms.

ACKNOWLEDGMENTS

ENDNOTES

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

[2] In the 2014 wave of the Current Population Survey, for example, a significant share of non-employed women mention the need to provide care for young and elderly relatives as one of the top reasons not to work.

[3] In this setting, "shorter run" refers to the fifth year after childbirth and "longer run" to the next five years.

[4] An instrumental strategy similar to the one proposed by Gelbach (2002) has also been used to estimate the impact of age at school entry on a variety of outcomes. Among others, Angrist and Krueger (1991) used this method to estimate wage returns to schooling, Angrist and Krueger (1992) and Fertig and Kluve (2005) to estimate the impact of age at entry on school attainment, and Elder and Lubotsky (2009) and Strom (2004) on children achievements in test scores.

[5] The exclusion restriction has been validated for selected samples of children born in a narrow range of weeks around the cutoffs (Fitzpatrick, 2010; Dickert-Conlin and Elder, 2010). Extending the range to the entire quarters, however, seems to be problematic. Besides the correlation to maternal characteristics, the season of birth also exhibits correlations with race (Lam and Miron, 1991), family income (Kestenbaum, 1987; Bound and Jaeger, 2001), personality traits (Gortmaker et al., 1997), and an array of medical conditions (for example, see BMJ Editorial (1978) for a review of the studies on schizophrenia).

[6] The exact date of birth is contained in the 2000 Restricted Access Decennial Census Long Form data used by Fitzpatrick (2010), but not in census data for the previous decades. This same data and approach have also been used to show that, in Oklahoma and Georgia, public pre-kindergarten for children of age 4 has significant impacts on their learning outcomes (Fitzpatrick, 2008), but not on maternal labor supply (Fitzpatrick, 2010).

[7] In general, parents may petition the local school district for early admission, in any case after the fifth birthday (i.e. a child who turns five on November 5, 1979, in a State with a September cutoff may be exceptionally allowed to attend public kindergarten after November 5, 1979). Such exceptions are rare, and the districts decide on a case-by-case basis. The conditions for early admission vary by state, but typically parents may request (and be granted or rejected) early admission for children who are exceptionally gifted or talented (in such cases, children's readiness for kindergarten is formally tested) or have already attended public kindergarten in another state where they are eligible (Kerley et al., 2020).

[8] Private nurseries and childcare centers have existed in the United States since the 1830 s and significantly expanded during the World conflicts and in the 1960 s and 1970 s. Public childcare for children below age five was introduced much later (Cascio, 2009b). Since 1976, parents can apply for the Child Care Tax Credit, yet its non-refundable nature arguably makes it less valuable for households at the bottom of the income distribution (Anderson and Levine, 1999).

[9] In general, very little information is available at the national or state level on the availability and eligibility requirements for private childcare facilities, especially before 1987. In describing the supply of informal daycare, Hofferth (1979) declares: "Little is known about such [individual daycare] providers. They either work out of their own homes or go to the homes of their clients, are probably not licensed, and their clientele is located by word of mouth. We

don't know how many providers there are, what their motivations are, how profitable the business of child care is, nor what type of care is given". Based on data from the US Census Bureau (U.S. Bureau of the Census, 1987), in 1987 the number of facilities per thousand children ranged from 7 (Louisiana) to 33 (Vermont) for what I define below as "control states" and from 9 (Alabama) and 49 (North Dakota) in what I later define as "quarter cutoff states". Unfortunately, no data are available on the number of workers per child. As discussed below, in the empirical analysis such state-specific characteristics are captured by state "fixed effects" and robustness checks confirm that controlling for childcare availability has no impact on the main estimates of interest (robustness check described in footnote 18).

[10] The oldest data on childcare costs in the census are from 1985, when the average cost, among families with a child below age 15 and an employed mother, was $97 per week (in constant 2021 US dollars).

[11] See for example Boeri and Van Ours (2013) for an intuitive proof, under the standard assumptions that the costs of childcare increase with hours of work, and that leisure is a normal good.

[12] The objective of the theoretical framework is purely to provide some formal background for intuitions, and not to guide estimation or offer a detailed description of reality. For this reason, the model is extremely simple and abstract from further complications such as firms and workers heterogeneity, on the job search, and involuntary unemployment.

[13] However, it should be noticed that Lundin et al.'s (2008) heterogeneous effects analysis by age of the youngest child finds no significant impact also for age groups 1–3 and 3–6.

[14] The reform analyzed in Nollenberger and Rodriguez Planas (2011) did not create any variation in access to the service within children's cohort of birth and, therefore, does not allow the estimation of longer-run impacts within calendar years. Because the labor supply of mothers generally increases as children grow older, the fact that the labor supply of treated mothers remains higher than that of mothers of two-year-old children does not necessarily imply that childcare has a long-lasting impact: It is possible that in the absence of treatment the labor supply of treated mothers would grow over time as children grow older and reach the same levels as with treatment.

[15] In previous versions of this project, I had relied on ACS data but this only contains information on the quarter of birth, and not on the year of birth. In addition, the ACS data are collected throughout the year: Some respondents will answer the survey in January, others in February, and so on. As a result, using ACS data did now allow me to disentangle (i) to what school year the data on school attendance refers to and (ii) what is the legal age of the individual at the time of interview. These two data limitations in ACS make the use of census data more suitable for my goal.

[16] As detailed below, the exact cutoff date used to define eligibility varies by state. The list of states, and the reason why this distinction is useful, are given in Section 4.

[17] While no universal program existed in 1979 for children younger than 5, some means-tested programs such as Head Start could be available. In 1992, the first universal public pre-kindergarten program was introduced in Georgia, for children in the fourth calendar year after birth.

[18] The estimates for Equations (1) and (2) are robust to the inclusion, among the covariates, of the state-level number of childcare facilities, based on data from 1987. Estimates available from the author upon request.

[19] Some studies, like Gelbach (2002), estimate the local-average-treatment effect (or LATE in short) of enrollment using quarters of birth as instrumental variables, while others focus on the eligibility arguing that this, and not take up or actual enrollment, is the relevant policy lever (Currie and Gruber, 1996). I follow the latter interpretation and report only intention-to-treat effects.

[20] There are two notable cases in which spurious effects may in fact not be constant across states: the case in which mothers manipulate date of birth, and the case in which they migrate in response to existing eligibility cutoff. The first case has received attention in the so-called timing of birth literature, which suggests that parents and/or doctors could manipulate the date of childbirth to meet kindergarten eligibility cutoffs (Dickert-Conlin and Elder, 2010) or in response to financial incentives induced by tax regulations (Gans and Leigh, 2009). This would essentially happen via C-section, which was, however, a relatively rare procedure in 1974 (according to the American College of Obstetricians and Gynecologists, the incidence of elective C-sections is around 2.5% of all births in the United States in recent years, and was probably lower in the past (American College of Obstetricians and Gynecologists, 2019; Ecker, 2013)) and mostly occurred for medical reasons, rather than on maternal request (Taffel et al., 1987). While I temporarily ignore both potential issues for the core of the analysis, I come back to them in Section 6 and find no empirical evidence that they affect the main results of interest.

[21] The states in the first group are Connecticut, Delaware, Florida, Hawaii, Louisiana, Maryland, Mississippi, Rhode Island, and Vermont. Alabama, Arkansas, Idaho, Iowa, Kentucky, Maine, Missouri, Nebraska, Nevada, New Hampshire, North Carolina, Ohio, Tennessee, and Wyoming are in the second group. The third group includes the states where the choice of the cutoff was left to local school authorities (Colorado, Massachusetts, New Jersey, Virginia). It also includes the states which in the school year 1979/80 did not adopt a cutoff system, or adopted one which does not coincide with the end of a quarter of the year (such as December 1, September 1, August 31, or November 1 or 2). Table B1 in the Appendix lists the cutoff dates for each of the states included in the analysis. As shown in Table C3 in Appendix C, the main estimates of interest are robust to including in the sample all states with a cutoff in the third or fourth quarter of the year and defining a continuous measure of the impact of birth quarters on eligibility, based on the exact date of the state-specific cutoff.

[22] This approach is similar in spirit to the use of control or placebo groups in the development literature. Because in such groups the treatment is expected to have no effect, they can be used to test the robustness of the identification strategy (Heckman and Hotz, 1989; Rosenbaum, 1996; Imbens, 2004; Duflo, 2001; Hoynes et al., 2012; Heckman et al., 1997).

[23] It is possible to also include among the covariates a binary variable that takes value one in states with eligibility cutoff in the period September 15–October 15. However, as this is absorbed by state fixed effects, its inclusion along the other covariates has no effect on the resulting estimates.

[24] An alternative approach is to estimate the intention-to-treat effect for each cohort separately. The intention-to-treat effect after t school years is then captured by the coefficient α_t in the equation $Y_t = \alpha_t \cdot Q3 + \beta_t \cdot \{Q3\} \times \{QuarterCutoff\} + FE + \gamma'X_t + \varepsilon$, where setting $t = 0$ returns the short-run equation (2) above.

[25] Also notice that I will follow Angrist (2001) and Havnes and Mogstad (2011) and use linear probability models, which are appropriate as long as the covariates are not saturated. Probit and instrumental probit yield qualitatively similar estimates, which are available upon request.

[26] "Non-enrolled children" include children who are not at school as well as those who are enrolled in private institutes. In the sample, 2102 children are recorded as attending grade 1 in a public school in their fifth year of life, out of 24,691. I present results excluding these children, but including them in the sample has no noticeable impact on the estimates.

[27] The F statistics for columns 1, 2, 3, and 4 are, respectively, 147.73 (with degrees of freedom 12 and 50), 76.13 (with degrees 12 and 23), 252.83 (with degrees 15 and 23), 252.83 (15 and 23), and 245.88 (16 and 23).

[28] The estimates reported are to be interpreted as intention-to-treat effects: They refer to the impact of eligibility for public kindergarten. This will in general be smaller than the impact of enrollment in public kindergarten. Under additional assumptions, the latter can be estimated via instrumental variable methods (Gelbach, 2002). As enrollment cannot and should not be imposed, intention-to-treat effects are arguably the relevant ones for policy considerations. While at first glance the positive association between kindergarten eligibility and maternal wages seems at odds with the theoretical prediction that eligibility would negatively affect the reservation wages and (therefore) average accepted wages, this association could reflect differences in unobserved job characteristics.

[29] To limit the differences in children age, it is also possible to limit the sample to mothers of children born in the third and fourth quarter: While the reduction in sample size results in larger standard errors, the point estimates are comparable to those obtained for $Q3 \times Cutoff$ in Table 5: 0.017 (standard error 0.011) for participation, 0.004 (0.009) for employment, 0.391 (0.473) for hours, 0.099 (0.607) for weeks, 0.238 (0.489) for hourly wages, and 55.533 (124.240) for yearly earnings.

[30] An alternative to restricting the sample to mothers who do not have younger children would be to use the entire sample and extend Equation (2) to include an interaction between $Qq \times Cutoff$ and a binary indicator for the presence/absence of younger siblings in the household. The resulting estimates, available from the author upon request, confirm that kindergarten eligibility has a stronger impact on mothers who do not have additional younger children, resulting in higher labor force participation.

[31] In ancillary regressions, available on request, I estimate the differences in labor force participation rates between childbirth quarters, controlling for enrollment in public kindergarten, and the observable characteristics in Table 5. Holding enrollment in public kindergarten constant, childbirth quarters still significantly correlate with maternal labor force participation.

[32] As discussed above, the theoretical predictions for the impact on the intensive margin (hours and weeks of work, in my data) are ambiguous and depend on whether the substitution or income effect dominates. The substitution and income effect will pull hours in the opposite directions under standard assumptions that the costs of childcare increase with hours and weeks of work, and that leisure is a normal good (Boeri and Van Ours, 2013). Similarly, the impact on labor earnings depends on whether the increase in employment (and possibly hours) is strong enough to compensate for lower average wages (and possibly shorter hours of work).

[33] In contrast, double-difference estimation which leverages differences across states in kindergarten eligibility cutoffs, in the spirit of Equation (2) fails to identify any significant impact. Results are shown in Table C6.

[34] As previously mentioned, data limitations prevent me from observing the career paths, spells of unemployment, and other characteristics which affect longer-run impacts.

[35] Alternatively and equivalently, the coefficient can be seen as the across-states difference in average outcomes between mothers of children born in the third quarter of the given cohort, net of the same difference for mothers of children born in the fourth quarter, holding all other covariates fixed. Estimating the coefficient for each cohort in separate regressions yields qualitatively and quantitatively similar results. I have additionally conducted the analysis for up to ten years after kindergarten, obtaining similar results, available on request. Yet another alternative would be to estimate the longer-run impact on each subsample of interest (mothers without younger children, married mothers, and those with lower income and/or education) through an interaction, rather than by restricting the sample (for the longer run, such restrictions to the sample do not result in particularly small sample sizes). This approach yields qualitatively similar results.

[36] Recent evidence in the literature points to the existence of a "motherhood penalty" in the United States. Using panel data of earnings before and after childbearing, Budig and Hodges (2010) finds 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, one may wonder whether public kindergarten, by making the work-family balance easier, may buffer and weaken the penalty, either in the short or long run. As shown in the Tables and Figures, my estimates for labor earnings provide no evidence of such positive impacts.

[37] For the long-run analysis, estimating the LATE is not possible as no retrospective information is available on children's enrollment in kindergarten in the years before the survey.

[38] In addition, the definition of full-day and the funding largely vary across states (Parker et al., 2016).

[39] Even in 1993, after the introduction of an unpaid parental leave of 12 weeks, only about half of the labor force were actually eligible to request it, in case of childbirth Ruhm, 1998.

[40] In contrast, some scholars suggest that in recent decades US parents might be increasingly delaying the entry of their children to kindergartens and elementary schools, in an attempt to improve their learning outcomes, a phenomenon which is known as "red-shirting" (Dhuey, 2016).

[41] In addition, the p-value associated with the estimated coefficient on such binary variable is 0.452.

[42] As usual, b can also be given the alternative interpretation of utility from leisure in dollar terms.

[43] To get to this expression the reservation wage, first split the integral in Equation (A8) to get

$$\frac{\overline{\omega}}{1-\beta\gamma} = b + \beta \int_0^{\overline{\omega}} \frac{\overline{\omega}}{1-\gamma\beta} dF(\omega') + \beta \int_{\overline{\omega}}^B \frac{\omega'}{1-\gamma\beta} dF(\omega') \tag{A10}$$

and then re-arrange to get

$$\frac{1-\beta}{1-\beta\gamma}\overline{\omega} - b = \frac{\beta}{1-\beta\gamma} \int_{\overline{\omega}}^B (\omega' - \overline{\omega}) dF(\omega'), \tag{A11}$$

where the left-hand side represents the cost of rejecting an offer $\overline{\omega}$ and searching one more period and the right-hand side is the expected return from doing so, in terms of the expected present value for a new draw $\omega' > \overline{\omega}$.

[44] The first derivative of the right-hand side can be computed using Leibniz's 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.

[45] 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 *eligible* 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 nth offer for *eligible* mothers is $P\{W = n\} = \lambda^{(n-1)}(1 - \lambda)$, and 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 *non-eligible* mothers the probability to reject the offer in the fifth calendar year after childbirth ($t = 0$), is $P\{W \geq 1\} = \int_0^{\widehat{\omega}} dF(\omega') = F(\widehat{\omega})$ and the probability to reject exactly n offers, for $n > 1$ is $P\{W = n\} = F(\widehat{\omega}) \cdot (F(\overline{\omega})^{(n-2)}(1 - F(\overline{\omega}))$. The expected waiting time for type N mothers is then $\sum_{n=1}^{\infty} n \cdot P\{W = n\} = F(\widehat{\omega}) + \sum_{n=2}^{\infty} n \cdot (1 - \lambda) \cdot \lambda^{(n-2)}$. Because $F(\widehat{\omega}) \geq F(\overline{\omega})$, the expected waiting time for mothers of non-eligible children is higher.

[46] If mothers incur subsequent childbirths, they will again face the extra cost of childcare till the newborn has access to public kindergarten, too. In this case, one might expect the participation and employment rates to remain higher for *eligible* mothers (i.e. those whose child can enroll in public kindergarten already the fifth calendar year after birth) than for *not-eligible* ones.

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APPENDIX A

MODIFIED MCCALL MODEL

Date of birth and state-specific deadlines create a difference in the year of entry into the public education system: It is the fifth year after birth for the former group of children, the sixth year for the latter. To the extent that access to public kindergarten reduces the need to hire public childcare providers and use private kindergartens, it can be modeled as a reduction in mothers' opportunity cost of working, which translates into a reduction of their reservation wage. To see this and to understand the possible implications for mothers' labor supply and earnings in the short and longer

run, I specify a simple search model, based on McCall (1970). In the model, each period is a calendar year, time zero corresponds to the fifth calendar year after childbirth, wages increase with job tenure, and future is discounted at rate x. The agents in the model are adult mothers, who enter the economy at time $t = 0$ and are infinitely lived. Each period, each agent receives a take-it-or-leave wage offer ω, which is a random i.i.d. draw from a distribution $F(\omega)$ over the support $[0, B]$. For simplicity, on the job search and the recall of past offers are not possible in the model, 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.

Each period t while working, mothers incur an exogenous fixed opportunity cost of working k_t, which captures the price of childcare. To mirror the US context and the difference in time of access to public kindergarten determined by a child's date of birth and by state-specific cutoffs, the model is populated by two types of women, *eligible* (e) and *non-eligible* (n). Type e mothers can use public kindergarten in t_0 (the fifth year after childbirth), which brings their opportunity costs of working down to zero. Type n mothers gain access to public kindergarten in $t = 1$ (the sixth year after childbirth). After $t = 1$, all children have the option to enter or stay in the public education system and k_t is hence set to zero. Summing up, the opportunity cost of working for each type of mother is

$$k_t^E = 0, \forall t$$

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

Entry-level wages are stationary, as all draws in all periods are i.i.d. drawn from the same distribution $F(\omega)$. Each period after accepting a wage offer, the wage is given by $\omega_{(t+\tau)} = \gamma^\tau \omega$.

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

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

ELIGIBLE MOTHERS

Let us first consider the problem of a mother who can use public kindergarten and, without loss of generality, normalize her opportunity cost of working to zero. With a slight abuse of language, these mothers will be referred to as "*eligible*" and distinguished from "*non-eligible*" mothers, whose children can only enroll in public kindergartens in the sixth calendar year post-partum.

Assuming that the opportunity cost of working for an eligible mother is constant over time, it can be normalized to zero. Given the setup, the problem of an *eligible* mother, therefore, corresponds to the standard McCall (1970) model. The lifetime value of accepting an offer ω in period τ is then

$$\sum_{t=\tau}^\infty \beta^{t-\tau} \gamma^{t-\tau} \omega = \sum_{\tilde{t}=0}^\infty \beta^{\tilde{t}} \gamma^{\tilde{t}} \omega = \frac{\omega}{1 - \beta\gamma} \tag{A6}$$

where $\tilde{t} := t - \tau$ is a woman's job tenure and $\gamma > 1$ captures the returns to job tenure and is such that $\gamma\beta < 1$. If the offer ω is rejected, the unemployment benefit b is collected.[42]

For eligible 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:

$$v(\omega) = \begin{cases} b + \beta \int_0^B v(\omega') \, dF(\omega') & \text{if} \quad \omega \leq \overline{\omega} \\ \dfrac{\omega}{1 - \beta\gamma} & \text{if} \quad \omega \geq \overline{\omega} \end{cases} \tag{A7}$$

The mother's reservation wage $\overline{\omega}$ needs to satisfy

$$\frac{\overline{\omega}}{1 - \beta\gamma} = b + \beta \int_0^B v(\omega') \, dF(\omega'), \tag{A8}$$

where β is the discount factor, γ is the return to job tenure, the opportunity cost of working is normalized to zero, the wage offers are i.i.d draws with distribution $F(\omega)$ over the support $[0, B]$, and $v(\omega')$ is the continuation value of having offer ω' at hand. A few rounds of algebra lead to the condition

$$\overline{\omega}(1 - \beta) + (1 - \beta\gamma) b = g(\omega), \tag{A9}$$

where $g(\omega) = \beta \int_{\overline{\omega}}^B (\omega' - \overline{\omega}) \, dF(\omega')$.[43] 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 $\overline{\omega} = 0$.[44] Therefore, a solution $\overline{\omega}$ exists and is unique.

NON-ELIGIBLE MOTHERS

Non-eligible (*n*) mothers face a very similar maximization problem to eligible ones, except for period $t = 0$, when they face the extra cost of childcare κ. The fact that the extra cost of childcare is only met for one year makes this maximization problem time-varying and implies that the corresponding optimal policy also is. In any period after time zero, *non-eligible* mothers will follow the same reservation wage policy as *eligible* mothers because their maximization problem becomes identical to that of *eligible* mothers (Equation (A6)). In contrast, in period zero the Bellman Equation of *non-eligible* mothers is

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

Rejecting an offer ω at hand in t_0 has a lower cost for *non-eligible* mothers than for *eligible* ones: As a result, they will reject more often. The reason is that rejecting a job offer saves them the cost of childcare. Their reservation wage $\widehat{\omega}$ for $t = 0$, therefore, needs to satisfy

$$\frac{\widehat{\omega}}{1 - \beta\gamma} - \kappa = b + \beta \int_0^B v(\omega') \, dF(\omega'). \tag{A12}$$

Comparing this to Equation (A8), the only difference is the extra $-\kappa$ on the left-hand side. Therefore, $\widehat{\omega} - \overline{\omega} > 0$ and $\widehat{\omega} > \overline{\omega}$. In the short run, mothers of non-eligible children have a higher reservation wage, because accepting a job offer implies a higher opportunity cost. This is graphically shown in Figure (A1).
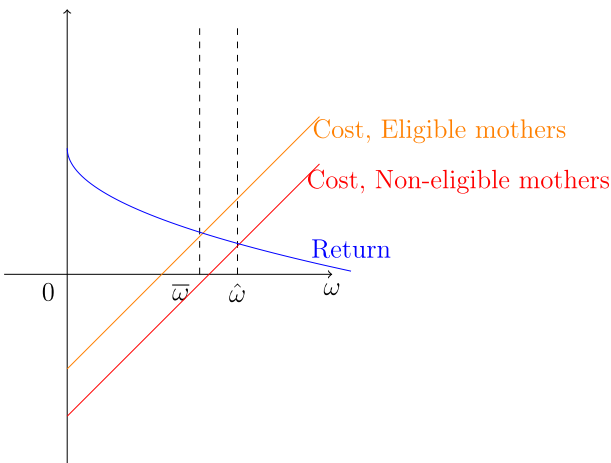
FIGURE A1 Reservation wages. [Colour figure can be viewed at wileyonlinelibrary.com]

Having a higher reservation wage obviously implies a lower probability of accepting an offer. While the model does not distinguish between labor force participation and employment, the empirical analysis considers these separately.

In the following periods, which correspond to the *longer-run*, non-eligible mothers face an identical problem to eligible mothers and therefore follow the same reservation wage policy (Equation (A9)).

PROBABILITY OF ACCEPTING AN OFFER

The higher reservation wage of *non-eligible* mothers implies that their hazard of getting a job is lower in the first period, and their waiting time till the first accepted offer is higher. The hazard can indeed be written as the probability to get an acceptable offer. This corresponds to $P\{\omega > \overline{\omega}\} = 1 - \int_0^{\overline{\omega}} F(w) \, d\omega$ for type e mothers, and $P\{\omega > \widehat{\omega}\} = 1 - \int_0^{\widehat{\omega}} F(w) \, d\omega$ for type n. Therefore, $\widehat{\omega} > \overline{\omega}$ implies $P\{\omega > \widehat{\omega}\} \geq P\{\omega > \overline{\omega}\}$.[45]

Because in the longer run all mothers adopt the same reservation wage $\overline{\omega}$, one should expect no persistence in participation and employment. Things might be different if women who did not work in period zero (the fifth year after childbirth) experience higher fertility or higher human capital depreciation: In this case, kindergarten might have a long-lasting impact on employment and participation, via these channels.[46]

WAGES

In the presence of returns to experience, kindergarten can have longer-lasting effects on wage earnings, even absent persistence in labor supply choices. To see this, consider the average wage of employed *eligible* mothers. Because they reject any wage offer below $\overline{\omega}$, their expected wage if employed in $t = 0$ is

$$\mathbf{E}[\omega \mid \omega \geq \overline{\omega}, i = E] = \int_{\overline{\omega}}^{B} \omega \, dF(\omega)$$

The expected wage of a *not-eligible* mother if employed in $t = 0$ is instead

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

Again, the difference is driven by the fact that *not-eligible* mothers have a higher reservation wage. Assume for example that the distribution of the wage offers, $F(\omega)$, is uniform over the interval $[0, B]$. For unemployed mothers, let ω_t be the wage offer received at time t. For employed mothers, let y_t be the wage received at time t, inclusive of returns to experience. In $t = 0$ job tenure is zero for everyone, so $y_0 = \omega_0$ for all employed mothers and the average wages for *eligible* (e) and *not-eligible* (n) mothers are

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

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

As shown above, the shares of employed mothers among *eligible* and *not-eligible* mothers are, respectively, $1 - \int_0^{\overline{\omega}} \mathrm{d}F(\omega)$ and $1 - \int_0^{\widehat{\omega}} \mathrm{d}F(\omega)$. Because $\widehat{\omega} \geq \overline{\omega}$, the latter are less likely to be employed but, if employed, they earn a bit more on average.

Now consider $t = 1$. Because of returns to experience, the wage of mothers who accepted an initial-wage offer ω_0 in $t = 0$ is $\omega_0 \gamma$. The resulting average wages for e (*eligible*) and n (*not-eligible*) mothers who started working in the previous period are

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

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

Women who did not start working in the previous period, on the other hand, draw a new offer $\omega' \sim F(\omega)$. Because at $t = 1$ everyone can use public kindergarten, all mothers accept/reject the offers based on the reservation wage policy $\overline{\omega}$. The expected wage for e and n mothers who start working in $t = 1$ is, therefore, $\frac{B - \widehat{\omega}}{2}$. Conditional on non-employment in $t = 0$, the probability of entering employment at $t = 1$ is the same for the two types of mothers. But the unconditional share of new employed is higher among *not-eligible* (type n) because more of them started the period unemployed. This is reflected in average wages

$$\mathbf{E}\left[y_1^E\right] = \frac{B - \overline{\omega}}{2} \gamma \cdot P\left\{\omega_0 \geq \overline{\omega}\right\} + \frac{B - \overline{\omega}}{2} \cdot P\{\omega_0 < \overline{\omega}, \omega_1 \geq \overline{\omega}\}$$
$$= \frac{B - \overline{\omega}}{2} \gamma \cdot \frac{B - \overline{\omega}}{B} + \frac{B - \overline{\omega}}{2} \cdot \frac{\overline{\omega}}{B} \cdot \frac{B - \overline{\omega}}{B}$$

$$\mathbf{E}\left[y_1^N\right] = \frac{B - \widehat{\omega}}{2} \gamma \cdot P\left\{\omega_0 \geq \widehat{\omega}\right\} + \frac{B - \overline{\omega}}{2} \cdot P\{\omega_0 < \widehat{\omega}, \omega_1 \geq \overline{\omega}\}$$
$$= \frac{B - \widehat{\omega}}{2} \gamma \cdot \frac{B - \widehat{\omega}}{B} + \frac{B - \overline{\omega}}{2} \cdot \frac{\widehat{\omega}}{B} \cdot \frac{B - \overline{\omega}}{B}$$

Given $\hat{\omega}$ and $\overline{\omega}$ such that $\hat{\omega} > \overline{\omega}$, it is possible to find a γ^* such that $\mathbf{E}\left[y_1^E\right] > \mathbf{E}\left[y_1^N\right]$, $\forall \gamma \geq \gamma^*$ and $\mathbf{E}\left[y_1^E\right] < \mathbf{E}\left[y_1^N\right]$, $\forall \gamma < \gamma^*$. In other words, if returns to experience are high enough and/or $\hat{\omega} - \overline{\omega}$ is small, the average wage in the longer-run ($t \geq 1$) will be higher for eligible mothers, although it is lower in the short-run ($t = 0$). Vice versa, with low returns to experience we will not expect to find this difference in wages.

In the model, every woman faces the same distribution of wage offers and the accumulation and depreciation of human capital are not modeled. In reality, these could have a similar effect as returns to job tenure: If eligible women lose less human capital because of their earlier re-entry in employment, they might receive higher wages or select better careers, which could lead to higher earnings in the longer run.

APPENDIX B

TABLE B1 Entry to public kindergarten: cutoffs by state, in the school year 1979/1980.

States with quarter cutoff		States with non-quarter cutoff	
Alabama	October 1st	Connecticut	January 1st
Arkansas	October 1st	Delaware	December 31st
Idaho	October 16th	Florida	January 31st
Iowa	September 15th	Hawaii	December 31st
Kentucky	October 1 st	Louisiana	December 31st
Maine	October 15th	Maryland	December 31st
Missouri	October 1 st	Mississippi	No public K till 1982
Nebraska	October 15th	Rhode Island	December 31st
Nevada	September 30th	Vermont	January 1st
New Hampshire	September 30th		
North Carolina	October 16th		
Ohio	September 30th		
Tennessee	September 30th		
Wyoming	September 15th		

Source: Table A1 in Bedard and Dhuey (2012), based on State Statutes and regulations.

CUTOFFS

Table B1 shows the public kindergarten eligibility cutoffs, for each state in the double-difference sample.

APPENDIX C

ADDITIONAL RESULTS

Table C1 presents the impact of public kindergarten eligibility on enrollment in private kindergartens, to quantify the so-called **crowding-out** effect.

To gain additional insight on the intensive margin effect of kindergarten eligibility, it is possible to consider the impact of quarter of birth on hours and weeks of work and on earnings **conditional on employment**. Given that eligibility has little if any impact on employment and labor force participation (5), in Table C2 I present simple OLS estimates for Equation (2), which do not apply a correction for the selection-into-employment bias. In line with the evidence on unconditional outcomes, the new set of estimates identifies significant differences by quarter of birth in states where these cannot be attributed to differences in public kindergarten eligibility.

In the preferred specification (Equation (2)), the legal relevance of quarter of birth for kindergarten eligibility is captured by the binary variable "*Cutoff*" which takes value 1 in all states with cutoffs in the time range September 15–October 15 and 0 in states with a December 31 or January 1 cutoff. As a robustness check, I now restrict the sample only to children born in the third and fourth quarters of the year and define the continuous measure "*Intensity*" as follows

$$
\text{Intensity} = \begin{cases} 1 - \dfrac{d\left(\text{Oct } 1^{\text{st}}, Cutoff\right)}{d\left(\text{Oct } 1^{\text{st}}, \text{Dec } 31^{\text{st}}\right)} \text{for cutoffs in the } 4^{\text{th}} \text{ quarter} \\ 1 - \dfrac{d\left(Cutoff, \text{Sept } 31^{\text{st}}\right)}{d\left(\text{Jul } 1^{\text{st}}, \text{Sept } 31^{\text{st}}\right)+1} \text{for cutoffs in the } 3^{\text{rd}} \text{ quarter} \end{cases},
$$

where $d\left(\cdot, \cdot\right)$ is the distance in days between any two dates. The variable *Intensity* captures the fact that the impact of quarter of birth on kindergarten enrollment is sharper in states with a cutoff closer to September 31 (where *Intensity* takes value 1), and weaker in those with a cutoff further apart, in either direction, from such date (*Intensity* takes value 0 when the cutoff is on December 31 or July 1). The impact of kindergarten eligibility on maternal labor supply Y_i is captured by the interaction *Intensity* $\times$ *Q*3 in the equation

$$
Y_i = \sum_{q=1}^{3} \left(\beta_q \times \left\{Quarter_q\right\} + \alpha_q \times \left\{Quarter_q\right\} \cdot \left\{Intensity\right\}\right) + FE + \gamma'X + \varepsilon \quad \text{(A13)}
$$

This approach, which allows me to exploit a larger sample, relies on the simplifying assumption that births are distributed uniformly over the days of the year. The resulting estimates, reported in Table C3, are widely consistent with the main findings in Table 5: They indicate a relatively small but statistically increase in labor force participation and hours and no effect on the other measures.

TABLE C1 Enrollment in private institutes

	Enrollment, Eq. (1)	Enrollment, Eq. (2)
Q1 × Quarter Cutoff		−0.093***
		(0.023)
Q2 × Quarter Cutoff		−0.082***
		(0.019)
Q3 × Quarter Cutoff		−0.075***
Q1	−0.106***	−0.035*
	(0.009)	(0.019)
Q2	−0.112***	−0.042***
	(0.010)	(0.013)
Q3	−0.087***	−0.029**
	(0.007)	(0.011)
Age	0.013***	0.016***
	(0.002)	(0.002)
Age, squared	−0.018***	−0.023***
	(0.002)	(0.003)
Total family income (10^3 USD)	0.003***	0.004***
	(0.000)	(0.000)
Social Security income (10^3 USD)	−0.005**	−0.009***
	(0.002)	(0.003)
Married/Cohabiting	−0.016***	−0.015**
	(0.005)	(0.007)
High School	0.058***	0.055***
	(0.005)	(0.007)
College or higher	0.144***	0.148***
	(0.008)	(0.012)
Has no younger child	−0.001	−0.004
	(0.003)	(0.004)
Race: white	0.067***	0.103***
	(0.013)	(0.017)
		(0.016)
No. Observations	147,734	47,528
R^2	0.099	0.124

Note: Linear probability model. Sample: IPUMS 5% 1980 US Census, children born in the third or fourth quarter of 1974. The dependent variable takes value 1 if the child is enrolled in a private kindergarten, a pre-school, or another similar private facility, and value 0 if the child is enrolled in a public facility or not enrolled at all. Standard errors, in parentheses, are clustered at the state level. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

TABLE C2   Short-run impacts conditional on employment, double-difference

	Hours	Weeks	Labor earnings
	(1)	(2)	(3)
Q1 × Quarter cutoff	0.054	−0.202	26.145
	(0.371)	(0.512)	(163.320)
Q2 × Quarter cutoff	−0.468	−0.113	214.859
	(0.498)	(0.478)	(140.962)
Q3 × Quarter cutoff	−0.511	−0.128	115.107
	(0.519)	(0.748)	(226.150)
Q1	0.200	0.529	222.733
	(0.230)	(0.372)	(137.100)
Q2	0.526	0.582**	−1.267
	(0.419)	(0.248)	(113.508)
Q3	0.696*	0.590	160.791
	(0.398)	(0.588)	(205.020)
Age	−0.276**	0.735***	399.912***
	(0.123)	(0.195)	(49.807)
Age, squared	0.231	−1.037***	−533.687***
	(0.197)	(0.309)	(72.598)
Married/Cohabiting	−2.829***	−2.664***	−897.944***
	(0.581)	(0.466)	(215.601)
High School	0.532	3.428***	1087.685***
	(0.398)	(0.446)	(127.356)
College or higher	−0.397	2.903***	2379.513***
	(0.663)	(0.571)	(202.680)
Has no younger child	2.940***	4.340***	980.880***
	(0.210)	(0.293)	(69.923)
Race: white	−1.264***	−1.353***	−640.780**
	(0.375)	(0.364)	(287.060)
Constant	39.774***	23.883***	−1757.020**
	(2.247)	(2.666)	(848.732)
No. Observations	21,804	21,804	21,804
R^2	0.044	0.033	0.065
F	0.450	0.073	1.159
p-value	0.720	0.974	0.347

[a]Full sample, double-difference estimates based on Eq. (2):
$Y_i = \sum_{q=1}^{3} \left(\beta_q \times \{Quarter_q\} + \alpha_q \times \{Quarter_q\} \cdot \{QuarterCutoff\} \right) + FE + \gamma'X + \varepsilon$. Standard errors, in parentheses, are clustered at the state level. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

TABLE C3   Short-run impacts, robustness check

Eq.: $Y_i = \sum_{q=1}^{3} \left(\beta_q \times \{Quarter_q\} + \alpha_q \times \{Quarter_q\} \cdot \{Intensity\} \right) + FE + \gamma' X + \varepsilon$

	LFP	Employment	Hours	Weeks	Hourly Wage	Labor Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Q3 × Intensity	0.020*	0.010	0.842*	0.416	0.396	91.692
	(0.010)	(0.009)	(0.420)	(0.494)	(0.476)	(99.260)
Q3	−0.005	−0.002	−0.328	0.030	−0.055	41.266
	(0.008)	(0.007)	(0.293)	(0.350)	(0.299)	(63.414)
Age	−0.024***	0.003	−1.175***	−0.154	0.460***	112.572***
	(0.003)	(0.004)	(0.125)	(0.152)	(0.117)	(37.954)
Age, squared	0.017***	−0.013**	1.097***	−0.268	−0.596***	−218.161***
	(0.005)	(0.006)	(0.197)	(0.246)	(0.173)	(61.935)
High School	0.098***	0.125***	3.438***	5.818***	0.147	1051.632***
	(0.009)	(0.008)	(0.389)	(0.383)	(0.147)	(65.012)
College or higher	0.183***	0.218***	5.336***	9.067***	1.820***	2377.223***
	(0.016)	(0.011)	(0.523)	(0.485)	(0.227)	(94.162)
Has no younger child	0.194***	0.192***	7.446***	9.466***	−0.527**	1677.317***
	(0.004)	(0.006)	(0.172)	(0.260)	(0.196)	(54.864)
Race: white	−0.112***	−0.086***	−4.299***	−4.645***	−0.866***	−1345.086***
	(0.014)	(0.012)	(0.429)	(0.578)	(0.250)	(144.278)
Constant	1.018***	0.309***	39.564***	19.888***	−2.509	771.508
	(0.054)	(0.062)	(2.053)	(2.468)	(1.898)	(620.048)
No. Observations	55,514	55,514	55,514	55,514	30,031	55,514
R^2	0.076	0.073	0.076	0.081	0.011	0.072

[a]Standard errors, in parentheses, are clustered at the state level. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

TABLE C4 Longer-run impacts: participation and employment

Eq. (4): $Y_t = \sum_{t=0}^{5} \alpha_t \cdot Q3 \times QuarterCutoff \times \{YoB_t\} + \beta_t \cdot Q3 \times YoB_t + FE_{State} + \gamma'X + \varepsilon$

	Full sample	Youngest child	Unmarried mothers	Low income	Low education	Low education, youngest child
	(1)	(2)	(3)	(4)	(5)	(6)
Labor force participation						
α_0	0.007	0.009	−0.008	0.005	0.007	0.002
	(0.006)	(0.009)	(0.015)	(0.010)	(0.014)	(0.018)
α_1	0.002	−0.002	−0.011	0.008	0.016	−0.012
	(0.008)	(0.009)	(0.013)	(0.012)	(0.015)	(0.020)
α_2	−0.005	0.003	0.007	−0.008	0.002	0.008
	(0.005)	(0.007)	(0.010)	(0.006)	(0.014)	(0.021)
α_3	0.001	0.008	0.041**	0.004	0.022	0.022
	(0.007)	(0.010)	(0.018)	(0.009)	(0.013)	(0.021)
α_4	−0.005	0.002	−0.018***	−0.015**	−0.004	0.006
	(0.005)	(0.009)	(0.006)	(0.007)	(0.010)	(0.018)
α_5	0.002	0.002	−0.026**	−0.006	−0.020	−0.020
	(0.005)	(0.008)	(0.012)	(0.007)	(0.015)	(0.016)
β_0	0.009**	0.009	0.014*	0.011**	0.009*	0.010
	(0.004)	(0.006)	(0.008)	(0.005)	(0.005)	(0.010)
β_1	0.015***	0.024***	0.011	0.007	−0.002	0.018**
	(0.004)	(0.005)	(0.007)	(0.005)	(0.007)	(0.008)
β_2	0.009***	0.005	0.006	0.007*	0.009	0.002
	(0.003)	(0.003)	(0.006)	(0.003)	(0.007)	(0.009)
β_3	0.017***	0.010**	−0.003	0.011***	0.006	0.003
	(0.003)	(0.004)	(0.005)	(0.004)	(0.006)	(0.006)
β_4	0.016***	0.010***	0.015***	0.015***	0.017***	0.009
	(0.003)	(0.004)	(0.004)	(0.004)	(0.006)	(0.007)
β_5	0.011***	0.008**	0.011*	0.012***	0.016***	0.017**
	(0.002)	(0.003)	(0.006)	(0.004)	(0.005)	(0.007)
R^2	0.083	0.072	0.167	0.108	0.057	0.052
Employment						
α_0	0.002	0.003	−0.014	−0.001	−0.018	−0.017
	(0.006)	(0.011)	(0.018)	(0.009)	(0.012)	(0.017)
α_1	−0.005	−0.010	−0.005	0.005	−0.006	−0.032
	(0.009)	(0.011)	(0.013)	(0.012)	(0.015)	(0.020)
α_2	−0.005	−0.003	−0.004	−0.008	0.004	0.017
	(0.007)	(0.008)	(0.016)	(0.008)	(0.011)	(0.015)
α_3	0.005	0.012	0.040*	0.006	0.019	0.011
	(0.008)	(0.012)	(0.021)	(0.010)	(0.014)	(0.022)

T A B L E   C 4 (Continued)

Eq. (4): $Y_t = \sum_{t=0}^{5} \alpha_t \cdot Q3 \times QuarterCutoff \times \{YoB_t\} + \beta_t \cdot Q3 \times YoB_t + FE_{State} + \gamma'X + \varepsilon$						
	Full sample	**Youngest child**	**Unmarried mothers**	**Low income**	**Low education**	**Low education, youngest child**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
α_4	−0.004	−0.002	−0.016	−0.013**	0.000	0.010
	(0.005)	(0.010)	(0.010)	(0.007)	(0.009)	(0.020)
α_5	−0.002	0.003	−0.019	−0.014*	−0.039**	−0.030
	(0.006)	(0.009)	(0.015)	(0.008)	(0.016)	(0.019)
β_0	0.010**	0.011*	0.009	0.011**	0.013*	0.021**
	(0.004)	(0.006)	(0.009)	(0.005)	(0.007)	(0.010)
β_1	0.017***	0.024***	0.021***	0.010**	0.009	0.018**
	(0.003)	(0.005)	(0.006)	(0.004)	(0.007)	(0.008)
β_2	0.008***	0.002	0.011*	0.008**	0.000	−0.010
	(0.003)	(0.003)	(0.006)	(0.004)	(0.006)	(0.008)
β_3	0.014***	0.008	−0.009	0.008**	0.014**	0.014
	(0.003)	(0.005)	(0.007)	(0.004)	(0.007)	(0.008)
β_4	0.014***	0.012***	0.008	0.013***	0.015**	0.010
	(0.003)	(0.004)	(0.006)	(0.005)	(0.006)	(0.008)
β_5	0.013***	0.009**	0.010	0.013***	0.021***	0.025**
	(0.004)	(0.004)	(0.007)	(0.004)	(0.006)	(0.011)
R^2	0.082	0.064	0.183	0.107	0.048	0.033
Demographic controls	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓
No. Observations	960,205	462,654	786,471	957,731	233,395	105,998

TABLE C5 Longer-run impacts: hours and earnings

Eq. (4): $Y_t = \sum_{t=0}^{5} \alpha_t \cdot Q3 \times QuarterCutoff \times \{YoB_t\} + \beta_t \cdot Q3 \times YoB_t + FE_{State} + \gamma'X + \varepsilon$

	Full sample	Youngest child	Unmarried Mothers	Low Income	Low Education	Low education, youngest child
	(1)	(2)	(3)	(4)	(5)	(6)
Hours of work						
α_0	0.241	0.366	−0.253	0.144	0.146	0.110
	(0.196)	(0.367)	(0.612)	(0.310)	(0.451)	(0.502)
α_1	−0.224	−0.625	−0.545	−0.007	0.315	−0.863
	(0.306)	(0.376)	(0.627)	(0.402)	(0.639)	(0.810)
α_1	−0.296	−0.142	0.350	−0.424	−0.326	−0.142
	(0.210)	(0.352)	(0.503)	(0.319)	(0.428)	(0.726)
α_3	−0.015	0.351	1.159*	0.134	0.682	0.448
	(0.308)	(0.448)	(0.622)	(0.417)	(0.522)	(0.666)
α_4	−0.130	−0.017	−1.308***	−0.546***	−0.259	0.026
	(0.162)	(0.315)	(0.408)	(0.203)	(0.447)	(0.687)
α_5	0.193	0.467	−0.752	−0.396	−0.635	−0.772
	(0.203)	(0.339)	(0.498)	(0.381)	(0.545)	(0.526)
β_0	0.278*	0.411*	0.440	0.258	0.199	0.443
	(0.141)	(0.211)	(0.278)	(0.177)	(0.250)	(0.388)
β_1	0.484***	0.738***	0.410	0.242*	−0.291	0.044
	(0.118)	(0.170)	(0.293)	(0.139)	(0.208)	(0.335)
β_2	0.442***	0.425***	0.275	0.291**	0.330	0.276
	(0.100)	(0.149)	(0.302)	(0.132)	(0.271)	(0.394)
β_3	0.555***	0.439**	−0.171	0.287*	0.411	0.682**
	(0.140)	(0.212)	(0.224)	(0.144)	(0.247)	(0.294)
β_4	0.519***	0.332**	0.740***	0.541***	0.518**	0.170
	(0.107)	(0.146)	(0.195)	(0.138)	(0.232)	(0.327)
β_5	0.441***	0.383**	0.599*	0.595***	0.448**	0.551*
	(0.082)	(0.143)	(0.309)	(0.180)	(0.187)	(0.324)
R^2	0.089	0.080	0.163	0.114	0.052	0.048
Labor earnings						
α_0	106.296*	104.320	81.571	53.714	−7.332	−98.256
	(57.820)	(109.609)	(191.628)	(59.090)	(82.711)	(120.544)
α_1	−78.892	−159.723*	−229.721	−19.637	−187.716*	−360.762***
	(87.798)	(90.199)	(185.190)	(65.031)	(102.687)	(133.752)
α_2	−99.974*	−96.525	−184.312	−69.817	−3.696	108.636
	(57.755)	(108.147)	(158.259)	(57.473)	(94.712)	(134.232)
α_3	−116.679	−82.688	−35.942	12.400	121.796	94.147
	(80.196)	(112.975)	(134.888)	(71.091)	(135.466)	(176.447)
α_4	−32.644	−15.855	−123.049	−63.454	−39.671	−106.949

(Continues)

TABLE C5 (Continued)

Eq. (4): $Y_t = \sum_{t=0}^{5} \alpha_t \cdot Q3 \times QuarterCutoff \times \{YoB_t\} + \beta_t \cdot Q3 \times YoB_t + FE_{State} + \gamma' X + \varepsilon$						
	Full sample	**Youngest child**	**Unmarried Mothers**	**Low Income**	**Low Education**	**Low education, youngest child**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	(56.132)	(69.748)	(156.762)	(49.153)	(91.428)	(139.259)
α_5	70.119	148.077	−75.576	−15.135	−16.456	−86.346
	(63.819)	(104.778)	(203.945)	(48.401)	(88.292)	(180.436)
β_0	71.015**	99.367**	129.655*	82.107***	74.305*	189.164**
	(31.757)	(47.747)	(72.138)	(28.541)	(39.781)	(78.654)
β_1	100.846***	119.099**	86.321	37.905	24.525	37.799
	(29.144)	(49.007)	(89.677)	(22.641)	(49.016)	(81.494)
β_2	88.448**	118.107**	69.300	44.185	66.634	23.910
	(40.022)	(48.318)	(87.915)	(31.003)	(66.981)	(101.856)
β_3	181.457***	194.405***	134.441*	109.918***	99.483	160.790*
	(41.543)	(61.180)	(75.564)	(36.370)	(62.564)	(88.052)
β_4	90.815***	65.165	55.517	109.716***	151.228***	140.594*
	(31.209)	(48.689)	(77.397)	(31.773)	(50.674)	(80.652)
β_5	109.784***	106.496***	73.531	97.964***	119.561**	251.159***
	(26.753)	(33.645)	(97.804)	(28.982)	(50.881)	(89.207)
R^2	0.088	0.080	0.164	0.177	0.036	0.025
Demographic controls	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓
No. Observations	960,205	462,654	786,471	957,731	233,395	105,998

TABLE C6 Maternal enrollment in formal schooling in the 5th year after childbirth

	Simple difference	Double difference
	(1)	(2)
Q1	0.013***	0.002
	(0.004)	(0.006)
Q1 × Quarter Cutoff		0.002
		(0.005)
Q2	0.014***	0.006
	(0.004)	(0.007)
Q2 × Quarter cutoff		−0.004
		(0.004)
Q3	0.005	0.008*
	(0.004)	(0.004)
Q3 × Quarter Cutoff		0.001
		(0.004)
Constant	0.906***	1.121***
	(0.042)	(0.025)
Demographic controls	✓	✓
State FE	✓	✓
R^2	0.108	0.036
No. Observations	147,621	47,482