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Preschool Availability and Female Labor Force Participation

Evidence from Indonesia

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Abstract

At 50.9 percent, female labor force participation in Indonesia is far below the regional average of 60.8 percent. Is it being hindered by a lack of affordable childcare services in the country? This paper exploits the joint variations in preschool age eligibility and access to preschool across regions and over years in a difference-in-difference-in-differences framework. With a longitudinal survey that tracks individuals for an average of 22 years, a panel of mothers was constructed to estimate the elasticity of maternal employment to preschool access. The analysis finds that an additional public preschool per 1,000 children increases the work participation of mothers of preschool age eligible children by 11–16 percent from the baseline mean. Private preschools do not increase work participation at the extensive margin, but they increase the likelihood of holding a second job. The availability of preschools induces mothers to informal sector occupations that do not require full-time commitments.

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Preschool Availability and Female Labor Force Participation: Evidence from Indonesia*

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1. Introduction

The provision of early childhood education services has become a central policy debate in many countries. Massive long-term socioeconomic benefits from early childhood interventions coupled with the potential to encourage maternal employment suggest that expanding access to preschools may kill two birds with one stone: increasing intergenerational mobility and closing gender inequality (Blau and Currie 2006). While the benefits for children enrolled in an early childhood program are well-documented (see Garcia et al. 2016 for a review of this literature), the causal evidence of preschool expansion on maternal employment is more of a mixed bag.

The effect varies across contexts, particularly over two dimensions: (1) current rate of preschool utilization and (2) current rate of maternal employment. When both rates are high, preschool expansion may no longer be effective at the extensive margin of inducing women to enter the workforce and enrolling their children in preschools (Lundin et al. 2008, Bauernschuster and Schlotter 2015). It may not induce greater preschool enrollment if the public expansion of preschools simply crowds-out private provisions (Baker et al. 2008). Furthermore, it may not induce maternal employment if it re-allocates children from informal non-parental childcare to formal childcare services (Havnes and Mogstad 2011b, Havnes and Mogstad 2011a). In the United States, past evidence (Gelbach 2002, Cascio 2009) may no longer apply today, because female labor supply is not as elastic as it once was (Fitzpatrick 2010).

We contribute to this literature by studying preschool expansion and maternal employment in a context that might yet be relevant in policy discussions. We study an emerging economy where preschool attendance and maternal employment are low, there is a large informal labor market, and individuals have a large network of kin who can, presumably, provide informal childcare services in the absence of formal providers. Indonesia is the fourth most populous country and is one of the fastest-growing economies in the world. However, in 2016, female labor force participation (FLFP) in Indonesia stood at 50.9 percent, lagging other countries in East Asia and the Pacific where the average FLFP is 60.8 percent, and stagnating from 50.2 percent participation in 1990. Prior to the recognition of pre-primary education in the national education system in 2003, only a quarter of children were enrolled in preschools, lagging behind the world's average of 32.1 percent, the regional average of 43.3 percent, and the OECD average of 73.3 percent (World Development Indicators). We complement the growing body of research on low- and middle-income countries (see Mateo Diaz and Rodriguez-Chamussy 2016 for a survey of studies in Latin

America, Du and Dong 2013 in China, Jain 2016 in India, and Clark et al. 2017 in Kenya) by exploring the relationship between preschool expansion and FLFP in a region with little evidence to date.

After the passing of The National Education System Act (NSEA) in 2003, the Government of Indonesia accelerated efforts to expand preschool education with a medium-term plan to increase enrollment between 2004 and 2009. Block-grants were set up to incentivize private provision, while district-level governments were responsible for the public provision of preschools. In 2004, less than a quarter of children aged 3 to 6 ever attended preschools. By 2016, the gross pre-primary enrollment rate had increased to 60.3 percent (World Development Indicators). We exploit the spatial and temporal variations in preschool access and the exogenous overlap with the time when mothers have a preschool-aged eligible child to infer the causal effect of preschool access on maternal employment in a difference-in-difference-in-differences (DDD) framework. Our constructed panel tracks the same mothers on average for 22 years and allows us to include an individual fixed effect that accounts for unobserved individual career and family preferences, biological dispositions, and cultural traits.

We find that mothers of preschool-aged eligible children increase their work participation by 7.4 percentage points, or 13.8 percent from the mean, if they are exposed to an additional public preschool per 1,000 children. We further find that private preschools do not have a statistically significant effect on work participation, but eligible mothers are more likely to hold a second job. The results further indicate that public preschools increase the likelihood of mothers being unpaid family workers and agricultural workers, while private preschools induce mothers to become artisanal production workers. We do not find any effects on earnings or hours worked. An increase in informal sector employment that does not require a full-time commitment is compatible with the fact that preschools typically only operate 3 hours per day during typical working days. We note that public preschools are likely substitutes to private preschools, although the extent of substitution is likely limited because they cater to different sub-populations (Chang et al. 2006). Contrary to other studies that use the eligibility of the youngest child,[1] we offer some evidence that preschool access may be more meaningful for the first two children. Our results are robust to

[1] See Gelbach (2002), Cascio (2009), Contreras and Sepulveda (2017), Berlinski et al. (2011), and Baker et al. (2008) for some examples.

defining eligibility pertaining to the oldest child and restrict the comparison group to mothers whose oldest child is aged 0-2 and not yet eligible for preschool.

Our estimated effect of a 7.4 percentage point increase in women's work participation is in the ballpark of other studies of childcare provision on maternal employment. Berlinski and Galianski (2007) study the effect of preschool expansion in Argentina and find an increase of 7 to 14 percentage points in maternal employment. In the follow-up study, Berlinski et al. (2011) find that women are 19 percentage points more likely to work full-time. Meanwhile, Schlosser (2011) examines the introduction of free compulsory preschools in Israel and finds a 7-percentage point employment increase. The introduction of subsidized childcare raises the mother's labor force participation by 8 percentage points in Quebec (Baker et al. 2008, Lefebvre and Merrigan 2008) and by 6 percentage points in Italy (Carta and Rizzica 2018). In India, government-sponsored childcare raises mothers' employment by 15 percent (Jain 2016). Martinez and Perticara (2017), in a randomized-controlled trial in Chile, suggest that afterschool care leads to a 4.3 percentage point increase in mothers' work participation. In the United States, using quarter-of-birth as an instrument for kindergarten enrollment, Gelbach (2002) finds a 6-24 percent increase in mothers' labor supply. Meanwhile, Herbst (2017) estimates the effect of universal childcare under the U.S. Lanham Act of 1940 and finds that maternal employment increased by 4.4 percentage points.

While the magnitude of impacts is aligned with other studies, our findings are surprising in three ways. First, public preschools do, in fact, have a substantial positive effect on FLFP despite the presumed reliance on communal childcare. This suggests that childcare responsibilities constrain some women in Indonesia from fully participating in the workforce and that informal childcare services are not sufficient alternatives to parental or formal childcare services. Second, public preschools could affect employment at the extensive margin despite operating for only 3 hours daily. However, this finding is consistent with the case in Argentina where public preschools also operate for half-days daily (Berlinski and Galiani 2007, Berlinski et al. 2011) and in India where daycare services are provided to children aged 3-6 for 3 hours daily (Jain 2015).

Third, preschool access facilitates women's access to the informal sector work that supposedly still allows women to self-provide childcare. A recent cross-country and cross-year comparison by Aaronson et al. (2018) finds that the negative gradient between fertility and mother's work participation holds in developed but not in developing economies. They argue that agricultural and self-employment, common in developing economies, are more compatible for

childcare. Negative fertility effects start to kick in as women transition to wage work and the substitution effect dominates (Goldin 1995). Their finding, however, does not directly contradict our finding. In their estimation, they use twins and the sex of the first two children as instruments. Hence, their conclusion extends only to the second oldest and subsequent children. In other words, they find no evidence of a fertility effect on women's work participation at the intensive margin of having another child, but they do not provide any evidence of a fertility effect at the extensive margin of having the first child. Indeed, we show that the childcare burden and positive public preschool effect are most pronounced for the first child, which decline for subsequent children.

In the next section, we elaborate on the context of preschools in Indonesia in more detail. In Sections 3 and 4, we discuss our data and empirical strategy, respectively. We discuss our results in Section 5, while Section 6 contains discussions on the potential complementarity and substitutability of public and private provision of preschools, heterogeneity, and the robustness of the findings. Section 7 concludes our paper.

2. Preschools in Indonesia

There are various forms of early childhood education and development (ECED) services in Indonesia, mainly distinguished by formal and non-formal streams and the age ranges to which they cater. Preschools (TK/RA[2]) are non-mandatory, formal ECED intended for children between the ages of 4 and 6. They offer academic preparation for primary education. Preschools typically operate daily (5-6 times per week) for 3 hours per day. Government regulation stipulates that a preschool should have at most 20 students per teacher. In reality, however, the age-groupings are often ignored, and quality varies across regions and facilities (Brinkman et al. 2017).

The majority of preschools are privately owned and managed, with only around 2 percent of preschools being publicly provided (Chang et al. 2006). As such, preschools are mostly located in urban areas targeting better-off families. In 2004, less than a quarter of children aged 3 to 6 ever attended preschools, and Indonesia's gross preschool enrollment rate lagged behind the average rate in low-income countries. In addition, the availability of preschools varied across regions in Indonesia.

[2] TK stands for *Taman Kanak-kanak* and RA for *Raudhatul Afthal*. Both facilities are pre-primary education with the distinction that RA's curriculum puts more emphasis on Islamic teachings, moral education, and memorization of the Koran.

Costs of attendance play a role in hindering uptake. Private preschools are funded by charging fees. As an alternative, parents enroll their children earlier than the recommended age of 7 in the free-of-charge public primary schools (Jung and Hasan 2014). Appendix Table 10 tabulates the average annual cost of attending private and public preschools in 2000. Means and standard deviations are significantly higher in private compared to public preschools, which might signify both higher use and higher variability of resources among private preschools.

Recognizing the importance of ECED, the Government of Indonesia adopted it into the national education system in 2003. The medium-term plan between 2004 and 2009 envisioned an increase in ECED enrollment. The Ministry of Education and Culture offer a block-grant subsidy to encourage the private provision of ECED services: funds are directly channeled to the providers from the central government. Public expansions are harder to achieve. Following decentralization reform in 2001, districts are responsible for financing, implementing, and supervising ECED services, while the central government is responsible for developing quality assurance mechanisms (Chang et al. 2006).

Other ECED services, such as daycares and nurseries (PAUD[3]), may also affect mothers' work participation. We focus on preschools due to limited data availability. Figures 1 and 2 illustrate the geographical variation of public and private preschool access across the country. Figure 3 shows the upward trend in both public and private preschool access over the years, notably since the passing of the National Education System Act (NSEA) in 2003. In the next section, we elaborate how we exploit spatial and temporal variations in public and private preschool access to estimate the elasticity of maternal employment to preschool access.

3. Data

The Indonesia Family Life Survey (IFLS) is a longitudinal household survey, first conducted in 1993, with subsequent tracking of the original and split households in 1997, 2000, 2007, and 2014. It was first fielded in 13 (of 27[4]) provinces back in 1993, which represented 83 percent of the national population (Frankenberg et al. 1995). It has notably high re-contact rates, with 87.8 percent of households surveyed in 1993 being successfully tracked or confirmed dead in 2014 (Strauss, Witoelar and Sikoki 2016).

[3] PAUD stands for *Pendidikan Anak Usia Dini*. It is a non-formal ECED service catering to children aged 2 to 6.
[4] At the time, Timor-Leste, now an independent country, was one of the Indonesian provinces.

In the first round, more than 14,000 individual respondents were selected to provide detailed accounts of their employment, current and historical—going back five years to 1988. Target respondents expanded further in subsequent rounds.[5] By the fifth round in 2014, more than 34,000 individuals were interviewed in detail. Combining the current and recall employment survey modules enables us to construct an annual employment history from 1988 to 2015 for individuals who were successfully tracked in all five waves.

IFLS also includes a module for ever married women ages 15-49, which includes detailed questions about all their pregnancy incidences—still in womb, resulting in livebirth, stillbirth, and miscarriage. In the first round, close to 5,000 women were interviewed. Tracking the same women over time allowed us to add in subsequent pregnancies that occurred after the first wave. For each live birth, respondents were interviewed at about the year of (or age at) childbirth. We can, therefore, complement our annual employment data with information on children's age at each year, and thus their preschool eligibility. To implement the fixed effect model discussed in Section 4.3, we further restricted our sample to women who were found to be between ages 19 and 45 in at least two waves.[6] In the remaining text, we refer to this as our "constructed panel."

We define preschool access as the number of preschools per 1,000 preschool-aged children, aged 3-6, in each district (henceforth, referred to as preschool density). The number of preschools is obtained from pooling Village Census (PODES) cross-sections from 1990 to 2014. PODES is fielded, roughly, once every three years.[7] The population of preschool-aged children comes from the annual[8] National Socioeconomic Survey (SUSENAS) cross-sections of respective PODES years.

Following decentralization reform in 1999, regional governments have been entrusted with more policy-making decisions. Province, district, and village-level governments have legislative power[9] to make policies and to allocate public goods, in this case, pertaining to preschool access. We aggregate preschool access at the district-level because, apart from PODES, the other two

[5] For more details, see: Frankenberg and Thomas (2000), Strauss et al. (2004), and Strauss et al. (2009).
[6] For each individual mother, we need at least two observations. Some of our analyses, such as on main activity, are not available in the historical employment module. Hence, we have to ensure that individuals are observed in at least two rounds.
[7] Specifically, between 1990 and 2014 Podes was administered in 1990, 1993, 1996, 2000, 2003, 2005, 2008, 2011, and 2014.
[8] SUSENAS are sometimes fielded more than once per year. In such cases, we use the round with the biggest sample.
[9] Sub-district is an administrative level between district and village. It serves to demarcate geographic boundaries, but it does not have any legislative power.

datasets do not include village-level identifiers and, thus, district is the smallest policy-relevant geographical boundary possible. Since decentralization, districts have also often split over time. In 1993, there were 290 districts; by 2014, there were 511 districts. To ensure equal comparisons across time, we harmonize district boundaries as they existed in 1993. We then match our constructed panel with preschool access at the consistent district boundaries circa 1993.

PODES is not available annually. We employ three strategies to account for the missing years. First, we simply restrict our constructed panel to PODES years. Second, we infer preschool data from in-between PODES years using the closest upper year available. For instance, year 1992 is sandwiched between PODES 1990 and 1993, so we infer preschool data from the 1993 round.[10] Third, we predict preschool density for the missing years using linear projection with the closest two data points available. For year 1992, we fit a linear projection using preschool density data in PODES 1990 and 1993. In the subsequent section on empirical strategy, we discuss the results from each strategy.

Table 1 provides summary statistics from our constructed panel. We have 227,579 female-year observations, with an average age of 31.5 and a 30 percent probability of having a preschool-aged child in any year. They have a 52 percent work participation rate and 51 percent live in urban areas. They are subjected to an average of 0.15-0.16 public and 4.45-4.60 private preschool density in their district of residence. There are 10,340 distinct females, who are surveyed on average in 3.54 rounds representing an average of 22 years of observations and 7.5 years coinciding with PODES years.[11] The average age of first marriage and first birth are 20.23 and 22.13, respectively. On average, each mother has 2.74 children and 7.75 years of education, or the equivalent of halfway through lower secondary education. There are multiple observations of preschool access per district, ideally equal to the multiple of 9 PODES rounds between 1990-2014; however, some districts in West Sumatra province are not included in the 2011 PODES and SUSENAS because probability sampling does not always cover all the districts. Ultimately, there are 2,559 district-year observations of preschool access with an average of 0.24 public and 4.18 private preschool density.

4. Empirical Strategy

[10] In other instances, years 1988-1990 of a constructed panel are matched to 1990 PODES, years 1991-1993 to 1993 PODES, and years 2012-2015 to 2014 PODES.

[11] IFLS surveys span over 21 years. However, the employment history which extends as far back as 1988 allows a maximum of 27 years of observations between 1988 and 2015.

In this section, we outline three empirical strategies to identify the causal impact of preschools on FLFP: difference-in-differences (DD), difference-in-difference-in-differences (DDD), and DDD with individual fixed effect (DDD-FE). Each identification strategy relies on different identifying assumptions. Our results are robust to both DDD and DDD-FE. In Section 4.1, we discuss why the identifying assumption may not hold in DD.

4.1 Difference-in-differences (DD)

The adoption of preschools into the National System Education Act (NSEA) in 2003 and the subsequent increases in public and private preschool access provide a quasi-natural experiment to study its effect on the work participation of mothers with preschool-aged children. We exploit the spatial and temporal variations in preschool access in a difference-in-differences (DD) setup and estimate:

$$y_{ijt} = \alpha + \gamma\, TK_{jt} + \mu_j + \phi_t + \psi\, \boldsymbol{X_{ijt}} + \varepsilon_{ijt} \quad (1)$$

where y_{ijt} is the employment outcome of preschooler-mother *i* in district *j* in year *t* and TK_{jt} is the number of preschools per 1,000 children in district *j* in year *t*. Pre-existing regional disparities—such as natural resources, local labor market, and regional dispositions toward working women—and nationwide year-specific characteristics such as the business cycle and changes in attitudes toward working women over time—and that affect female work participation are accounted for by μ_j and ϕ_t: district and year fixed effects. $\boldsymbol{X_{ijt}}$ is a vector of time-variant individual characteristics, such as urban residence and female *i*'s age. We cluster our standard errors at the district-level to account for correlations within districts.

The strategy, however, requires a fairly strong assumption that absent variations in preschool access, be it across time or regions, preschooler-mothers would observe similar employment trends. This may not be the case if preschool expansion is correlated with FLFP. Privately-provided preschools driven by profits are likely positively correlated with the demand for preschools, which in turn is likely positively correlated with FLFP. Publicly-provided preschools may similarly be positively correlated with the demand for preschools if government is concerned with facilities' utilizations to maximize the cost-effectiveness of the investment, as was the case with family planning clinics (Pitt, Rosenzweig and Gibbons 1993). Alternatively, the allocation of preschools may be negatively correlated with FLFP. The government may invest in

public preschools, specifically, to drive up maternal employment. As such, they would target areas with low FLFP to begin with, similar to the INPRES primary schools targeting rule (Duflo 2001).

We test the plausibility of the common trend assumption by plotting the average employment of mothers with a preschool-aged child over time across districts that experience high and low growth in preschool density. We define high growth districts as districts that more than doubled[12] their preschool density between 2003 (when the NSEA was passed) and 2014. Panel A in Figure 4 shows that average employment of preschooler-mothers in high public preschool growth areas tracks well to that in low-growth areas up until 2003 when average employment in high-growth areas starts to grow larger than that in low-growth areas; this gap persists until 2014.[13] Panel B shows the parallel for private preschools. Employment trends before 2003 in high and low-growth areas do not coincide as well as those in Panel A; notably, average work participation in high-growth areas in 1996 is lower than that in low-growth areas by about 5 percentage points. We also note that average work participation in high-growth areas is not higher than that in low-growth areas after 2003, which suggests that private preschools may not have the same positive effect on maternal employment.

While Figure 4 shows plausible common pre-trends with regards to public preschools, we still have to assume that the similarity in employment trends continues after the preschool expansion in 2003. In other words, the post-trends would mimic the commonality of the pre-trends had it not been due to the preschool expansion and any differences in the post-trends would be solely attributed to differences in preschool density. This may not be true if preschool availability is correlated with FLFP, as discussed earlier. The observed positive correlation between childcare services availability and FLFP in Canada (Lefebvre and Merrigan 2008) suggests the inclusion of region-specific trends in the DD specification. As we show in Section 5.1, district trends prove to be driving our DD estimates, which suggests that the parallel post-trends assumption may not hold well in our context. We proceed to the next empirical strategy to address this identification challenge.

[12] The cutoff of 100 percent growth to indicate high growth districts is reasonable given that median growth in public and private preschools density are 85 percent and 92 percent, respectively. The trends look similar if "high" preschool investment districts are instead defined as districts with higher than average or median growth.

[13] Work participation rates among eligible mothers increased between 1990 and 1993 and, apart from the spike in 2000, stayed flat until 2003. The spike in work participation rates in 2003 may be caused by the East Asian Crisis in 1998/99. Thomas, Beegle, and Frankenberg (2000) indeed show that women were induced to work to mitigate the financial impact of the crisis.

4.2 Difference-in-difference-in-differences (DDD)

Our data allow us to know if a mother has any preschool-age child at any given year. We could also exploit the exogenous overlap in the time when mothers have an eligible child with the spatial and temporal variations in preschool access to estimate the preschool effect on maternal employment in a DDD setup. This affords us additional variation that we can difference within-year and within-district. Specifically, we estimate:

$$y_{ijt} = \alpha + \beta\, TK_{jt} \cdot Eligible_{it} + \gamma\, TK_{jt} + \delta\, Eligible_{it} + \mu_j + \phi_t + \psi\, \boldsymbol{X_{ijt}} + \varepsilon_{ijt} \qquad (2)$$

where y_{ijt}, TK_{jt}, μ_j, ϕ_t, and $\boldsymbol{X_{ijt}}$ are as described above. $Eligible_{it}$ is the dummy indicating if mother *i* in year *t* has any preschool-aged eligible children (age 3-6). The coefficient interacting preschool density and eligibility, β, is our DDD estimate.

Some studies studying the effect of childcare on maternal employment have used the age of the youngest child to define treatment category[14]; we opt to define a more general treatment category pertaining to any child in the preschool age because, in Indonesia, the incidence of first birth has a more substantial negative effect on maternal employment than subsequent births (Halim, Johnson, and Perova 2017). Moreover, with detailed fertility history, we can infer the age of all children born to the mother at any given year and assign a treatment status to all mothers who might have benefitted from preschool access.[15] We can take a more naïve stance and we do not have to assume that mothers do not have the incentive to return to the workforce until the youngest child enters preschool.

Similar to Brodeur and Connolly (2013) and Herbst (2017), our non-eligible "comparison" group is the group of women without any preschool-aged children, which includes non-mothers and mothers with all children younger than or older than preschool ages. While employment trends of non-mothers may not be similar to those of preschooler-mothers, the identifying assumption in DDD is less stringent than that in DD. It requires that absent differences in preschool access (be it across regions or over time), the employment gap between mothers with preschool-aged children and mothers without preschool-aged children observe similar trends. Therefore, the common

[14] See for example: Gelbach (2002), Baker et al (2008), Cascio (2009), Berlinski et al. (2011), Brodeur and Connolly (2013), Nollenberger and Rodriguez-Planas (2015), Herbst (2017), and Contreras and Sepulveda (2017).

[15] In most cross-sectional data without detailed fertility history, e.g. Labor Force Survey or Census, we have to define treatment pertaining to the youngest child because the first child might have moved out of the household and, thus, is no longer observed. The youngest child, on the other hand, has a very small likelihood of living apart from her mother. With detailed fertility history, we know all children eligible for preschools and we define our treatment status accordingly.

trends assumption would be satisfied as long as the difference in employment trends of eligible mothers and non-eligible mothers is not systematically different across high and low-growth areas.

Figure 5 shows the employment trends for the four groups of comparison: (1) eligible mothers in high-growth districts, (2) eligible mothers in low-growth districts, (3) non-eligible mothers in high-growth districts, and (4) non-eligible mothers in low-growth districts. Panel A shows that pre-trends in high and low public preschool growth districts among eligible and non-eligible mothers coincide with one another well, respectively. Common trends for non-eligible mothers in high and low-growth areas persist after 2003, while the average work participation for preschooler-mothers in high-growth districts is consistently larger than that in low-growth districts since 2003. Panel B shows the parallel for private preschools. Eligible mothers in high and low private preschool growth districts seem to show similar pre- and post-trends. Average employment of non-eligible mothers in high-growth districts coincides with those in low-growth districts from 1990 to 1993. However, non-eligible mothers in low-growth districts seem to work more than non-eligible mothers in 1996 and the gap persists until 2014. The gap in employment trends between eligible and non-eligible mothers does not seem to be as systematically different across high- and low-growth districts. The common trends assumption in the DDD setting holds reasonably better than the one in the DD setting. As a precaution, we also show results with and without district-specific trends.

Nevertheless, it may be argued that non-mothers and mothers with children of non-preschool ages are a poor comparison group to mothers of young children eligible for preschools. Non-mothers may observe lower work participation rates because the median age of first birth is 22, which might imply that many young women do not have the opportunity to work after school and before starting a family.[16] On the other hand, non-mothers may have higher work participation rates because they do not face the childcare burden. In our sample, we note that the first effect dominates; work participation rates for non-mothers is lower than that of mothers at 32.9 and 57.5 percent, respectively. Mothers of primary school-aged children (age 7-12) may also have higher work participation rates because public primary schools are free and more abundantly available, thus, they may serve as a better childcare alternative. Meanwhile, mothers of older children may no longer need childcare services because older children can take care of themselves.

[16] A quarter of women in our sample first gave birth when they were 19. Meanwhile, high school is ideally completed at age 18.

Mothers of children aged 0-2 may be the ideal comparison group to mothers of preschool-aged children. However, if we define an eligible group as mothers with *any* child aged 3-6 and a non-eligible group as mothers with *any* child aged 0-2, then the two groups overlap over mothers with two children: a 3-6 year old child and a 0-2 year old child.[17] Therefore, we need to explore other uncontaminated ways to define 'eligibility.' As noted earlier, many previous studies have defined eligibility with regards to the youngest child, but the first childbirth may in fact introduce the largest negative shock to maternal employment, especially in the Indonesian context. Moreover, if we compare across mothers whose youngest child is aged 0-2 and aged 3-6, then we would still expect a contaminated comparison group because mothers whose youngest child is aged 0-2 may have an older child aged 3-6, who could benefit from preschools. As such, the estimated treatment effect may be biased downwards. The cleanest identification may be to compare mothers whose oldest child is not yet of preschool-aged and mothers whose oldest child is of preschool age. Section 5.2 discusses these results.

Figure 6 shows a similar comparison across eligible and non-eligible mothers in high and low preschool growth areas. However, eligibility is instead defined for the oldest child being of preschool age and the comparison group is defined for the oldest child being aged 0-2. Panel A similarly shows that mothers whose oldest child is not yet of preschool age have similar employment trends across high and low preschool growth areas, before and after the passing of the NSEA in 2003. Eligible mothers show similar pre-trends but mothers in high preschool areas have diverged on a higher trajectory since 2003; the gap persists until 2014. Panel B shows that eligible and non-eligible mothers in high and low preschool areas coincide with each other before 2003. However, non-eligible mothers have grown to have higher work participation rates than eligible mothers since 2003. Employment trends are nonetheless similar across high and low preschool areas among non-eligible and eligible mothers, respectively.

4.3 Difference-in-difference-in-differences with an individual fixed effect (DDD-FE)

It is difficult, however, to account for women's unobserved preferences for work and leisure, abilities, family circumstances, fertilities, and fecundities that certainly affect their decision to enter the workforce. A myriad of covariates can be introduced to approximately control these unobservables. For instance, we may consider pre-birth work hours as a proxy for career

[17] This is a very likely scenario. Appendix Figure 9 shows the distribution of gap years between childbirths. The mode is a 2 year gap between childbirths for all birth orders.

preference, educational attainment for abilities, household income for family circumstances, and number of children for fertility. The proximity of these measures may vary across contexts and, even so, it may be difficult to accurately measure these proxies. We argue that using an individual fixed effect in longitudinal data that follow the same individual over time is a better approach, since we are not necessarily interested in estimating the effect of, say, fecundity on work participation; instead, we are interested in accounting for unobserved individual dispositions toward work. However, it is important to note that we can only control for time-invariant individual characteristics and preferences. We need to assume, for example, that fertility, career, and family preferences do not change over time.

We can estimate:

$$y_{ijt} = \alpha + \beta\, TK_{jt} \cdot Eligible_{it} + \gamma\, TK_{jt} + \delta\, Eligible_{it} + \mu_j + \phi_t + \theta_i + \psi\, \boldsymbol{X_{ijt}} + \varepsilon_{ijt} \qquad (3)$$

where y_{ijt}, TK_{jt}, $Eligible_{it}$, μ_j, ϕ_t, and $\boldsymbol{X_{ijt}}$ are as described earlier. The inclusion of an individual fixed effect, θ_i, helps account for important omitted variables such as career and family preferences, which thereby minimizes the estimates' biases. It may also potentially relax the identifying assumption in the DDD setting. In Section 4.2, we have to assume that the employment gaps of *different* mothers with and without any preschool-aged child in high- and low-growth districts are not systematically different from one another. In DDD-FE, we compare within-mothers and we assume that in the absence of differences in preschool access, the employment gap of the same mother with and without any preschool-aged child would have been the same. In other words, as the child ages from 2 to 3 and becomes eligible for preschools, if the mother is exposed to the same preschool density, then the difference in her likelihood to work (when the child was of age 2 and when the child was of age 3) would not be systematically different. Note that since it is a special case of the DDD setup, the internal validity of the DDD strategy naturally lends itself to the DDD-FE strategy. Figure 5 and 6 similarly serve as justifications to the common trends assumption in DDD-FE.

Individual fixed effects, however, force comparisons within mothers. To leverage variations in preschool availability, we need frequent observations of the same mother across time. Identification, first, comes from variation in age eligibility—we compare a mother's work participation when her child is aged 2 and not eligible for preschool and the next year when her child is aged 3 and starts to be eligible for preschool. Second, identification comes from variations

in preschool availability during eligible ages—suppose that there few preschools when the child is aged 3 and the government builds more preschools when the child is aged 4.

Our annual panel affords us sufficiently frequent observations of mothers; however, our preschool data are only available roughly once in three years. Restricting our analysis to PODES years only could be problematic because we can only leverage variations as mothers transition in and out of eligibility with an older child growing out of, and a younger child reaching, preschool ages. Suppose that in year 2000, the mother only has one young child and in 2003, she has two young children. Her labor force participation calculus likely changes with the additional child. We must assume that individual preferences for work do not change over time and that variations in work participation are attributable only to variations in preschool availability. We must also assume that preschool availability does not affect individual preferences for children. Suppose that no preschools were available when a mother has a preschool-aged child and she decides not to have another child, so she can work. Since we only observe her three years later, we can only identify changes in preschool eligibility and not variations in preschool availability during eligible ages. We do not have to make these additional assumptions in DDD because we can compare across eligible mothers subjected to different preschool availabilities. We explore the validity and present the results of each strategy in the next section.

5. Results

This section describes the effect of public and private preschool availability on female employment outcomes in Indonesia. We first begin by examining the extensive margin of employment: work participation. Do preschools serve as an adequate alternative to childcare, enabling women to partake in employment? We describe the results across different empirical strategies and different treatment of missing years, without preschool data in PODES. Subsequently, we explore the intensive margins, such as the types of occupations and sectors that women go into. Given that preschool only provides 3 hours of childcare per day on average, can it enable women to work in the formal sector with rigid hours?

5.1 Work participation

Table 2 reports the regression results of our three empirical strategies with and without district-specific trends in the most conservative PODES-restricted years. Columns 1-2 estimate the work participation of mothers with preschool-aged children on public (Panel A) and private (Panel B)

preschool densities. Column 1, which does not include district-specific trends, suggests a negative effect of preschool density on preschooler-mothers' employment. The effect on public preschool is not statistically significant, while the effect on private preschool suggests a 1.0 percentage point reduction in maternal employment. District trends, however, substantially affect our estimates of public preschool; the DD estimate with district-trend on public preschool is positive but still not statistically significant. This suggests that there are important district-specific employment trends that are correlated with public preschool availability and that are not properly accounted for in the DD setup. The estimate for private preschool is also slightly affected, becoming less negative at 0.8 percentage point.

In columns 3-4, we implement a DDD model, adding the exogenous variation in the timing when mothers have a preschool-aged child to better account for differential employment trends across districts over time. The interaction term of preschool density and a dummy for having a preschool-eligible child is our estimate of interest. In Panel A, we show that having an additional public preschool per 1,000 preschool-aged children increases the likelihood of eligible mothers working by 4.8 to 5.0 percentage points. The coefficients are statistically significant at the 5 percent level. Public preschool density itself has no statistically significant effect on non-eligible mothers. Expectedly, having a young preschool-aged child has a negative effect on women's work participation. The negative effect of having a preschooler is almost compensated by having another public preschool per 1,000 children. In Panel B, we find that private preschools do not increase the likelihood of eligible mothers working. The estimated effects are precisely 0 with 0.002 standard errors. Better private preschool access has a small negative and marginally statistically significant effect on non-eligible mothers, in particular when the DDD regression also controls for district trends. Similarly, we find a negative effect of having a preschooler.

Columns 5 and 6 build on the DDD setup and add an individual fixed effect to allow comparison within-mother and better control for omitted variable bias, such as individual preferences for career and family. The results suggest that unobserved individual preferences and biological dispositions may meaningfully affect the estimated effects. An additional public preschool per 1,000 children increases the likelihood of preschooler-mothers to work by 7.9-8.0 percentage points. Similar to the DDD setup, district trends no longer affect the estimates. Both estimates are statistically significant at the 1 percent level. Private preschools still yield a precisely 0 effect on maternal employment. Public preschools do not have any effect on non-eligible

mothers, but private preschools have a small and statistically significant effect if we control for district trends. The presence of any preschool-aged child still negatively affects maternal employment, albeit with smaller magnitudes: -0.034 compared to -0.051 for public preschools in DDD and -0.025 to -0.042 for private preschools. This suggests that some of the reduction in the likelihood of working when taking care of a young child is attributable to individual career and familial preferences. It seems puzzling that eligible mothers with access to an additional public preschool per 1,000 children would have worked more than non-eligible mothers. This may suggest that, controlling for individual preferences, mothers with a young child have greater financial needs and would have been more motivated to work had they been afforded alternative childcare services.

5.2 Alternative specifications

Our preschool data were obtained from PODES, which is only available in 9 years of 28 years in our constructed panel. The most conservative approach is to restrict the analysis to PODES years only, as in Table 2. Our second approach is to infer preschool data in-between PODES years using the closest upper year available. Lastly, we can fit a linear projection between non-missing PODES years. Table 3 reports the estimates of the three empirical strategies with the three treatments of missing PODES years. All regressions include district, year, mother's age fixed effects and an urban residence dummy; and do not include district-specific trends. Columns 1-3 repeat the estimates from Table 2 without district trends. Columns 4-6 report estimates when we infer in-between PODES years and columns 7-9 report estimates when we fit a linear projection between non-missing PODES years. Across the board, we note that the general story and overall statistical significance do not change whichever preschool data we use. In the restrictive approach of using only PODES years, we introduce identification issues in the DDD-FE strategy, as we can only leverage variations as mothers transition in and out of eligibility over time, potentially giving up the statistical power to detect small effect sizes in subsequent analyses.[18] In fitting a linear projection, we can either use preschool densities directly or we can separately fit linear projections for preschool counts and children populations. We opt to project preschool densities directly.

[18] In results not shown, we find that women are 2.2 percentage points more likely to become agricultural workers if exposed to better public preschool access. The effect is statistically significant at the 10 percent level, but the statistical significance no longer holds after adjusting for multiple hypothesis testing. Inferring preschool data in-between PODES years substantially increases the sample size needed to detect the effect. We note that women are 2.9 percentage points more likely to become agricultural workers. The effect is statistically significant at the 5 percent level and stays significant after multiple hypothesis testing adjustment.

However, this assumes divisibility in the number of preschools and children. In subsequent analyses, we infer in-between PODES years because it requires fewer assumptions. Appendix Table 2 reports the parallel of Table 2 with inferred in-between preschool data.

Table 4 reports the effect of preschool availability on maternal employment when we change the definition of preschool eligibility with regards to any child, the oldest child, and the youngest child. Columns 1 and 2 are repeated from Columns 5 and 6 in Table 3; when the eligible group is defined as mothers with any child of preschool age. Columns 3 and 4 re-define eligibility with regards to the first child using DDD and DDD-FE strategies, respectively. In Panel A, the interaction coefficient for public preschools has a slightly smaller effect size, 0.041 compared to 0.057, and becomes less statistically significant with the DDD strategy, but stays the same in size and statistical significance with the DDD-FE strategy. The care burden of having an eligible child becomes less pronounced in DDD and more pronounced in DDD-FE. An additional public preschool per 1,000 children does not have a statistically significant effect on mothers whose youngest child is of preschool age in DDD strategy (but the effect is still positive) and has a slightly smaller effect in the DDD-FE strategy. This is likely explained by the fact that public primary schools are tuition free and are more widely available. Hence, the youngest child enrolled in preschool would still pose more childcare burden than the second youngest child enrolled in primary school. Mothers do not face reduced work participation if their youngest child is of preschool age. Panel B repeats the same analysis for private preschools. The interaction coefficients stay non-statistically significant across all three definitions of eligibility. The overall story for both public and private preschools stays relatively similar across definitions, although the effects are expectedly less pronounced for the youngest child.

Table 5 explores different choices of comparison ages. Columns 1-4 define preschool eligibility with regards to the oldest child and columns 5-8 with regards to the youngest child. Column 1 compares mothers whose oldest child is of preschool age (3-6) to mothers whose oldest child is aged 0-2, while Column 2 changes the comparison group with mothers whose youngest child is of primary school age (7-12), column 3 in secondary school ages (13-18), and column 4 includes mothers whose youngest child is of the wrong age and non-mothers. Columns 5-8 repeat the same pattern. Panel A shows interaction coefficients of eligibility and public preschool density with DDD and DDD-FE strategies, while Panel B shows that of private preschool density.

Column 1 suggests that an additional public preschool per 1,000 children increases the work participation of eligible mothers by 6.3 percentage points with the DDD strategy and 7.5 percentage points with the DDD-FE strategy, relative to mothers whose youngest child is aged 0-2. Private preschools lead to a 0.4 percentage point increase in work participation with both DDD and DDD-FE strategies, which are statistically significant at 5 and 1 percent, respectively. Column 2 shows that preschools do not have a statistically significant effect on the work participation of eligible mothers relative to mothers with a youngest child of primary school age. This is likely the case because public primary schools are free of charge and are more abundantly available than preschools; thus, they arguably provide a better childcare alternative than preschools. Column 3 combines mothers whose youngest child is aged 0-2, 7-12, older than 13, and non-mothers in the comparison group. Column 3 repeats columns 3 and 4 of Table 4.

Column 4 suggests that public preschools do not help to increase the work participation of mothers whose youngest child is of preschool age relative to mothers with a younger youngest child, but private preschools increase work participation by 0.6 and 0.4 percentages points, which are statistically significant at 10 and 5 percent using DDD and DDD-FE, respectively. Compared to mothers with a youngest child in primary school, public preschools do not have a statistically significant effect on work participation in both DDD and DDD-FE, while private preschools have a non-statistically significant effect on DDD-FE but a negative and statistically significant effect on work participation in DDD. Column 6 repeats Table 4 columns 5 and 6.

Taken together, the results suggest that the overall story is robust to different empirical strategies, choice of ways to define eligible ages, and choice of comparison group, despite requiring different identifying assumptions. Additional public preschool density leads to an increase in maternal employment in virtually all sensible specifications. Additional private preschool density may also have a positive, small, and statistically significant effect on work participation, in particular when we compare preschool ages to younger than preschool ages.

5.3 Sectoral changes

The finding that public preschools boost women's work participation may be surprising given that preschools only liberate an average of 3 hours of mothers' time per day. It is unlikely that mothers could take up a formal sector job with rigid hours. Table 6 examines the effect of preschool availability on women's work status. Our DD estimates suggest that eligible mothers exposed to higher public preschool density are not more likely to hold a second job or to be self-employed,

but they are slightly more likely to be a government worker at 0.6 increased percentage points—the estimate is statistically significant at the 10 percent level. Eligible mothers are not more likely to be a private worker but are significantly more likely to be an unpaid family worker. An additional public preschool per 1,000 children increases the likelihood of being an unpaid family worker by 4.1 percentage points, which is statistically significant at the 1 percent level. On the other hand, we find that an additional private preschool per 1,000 children increases both the probability of holding a second job and being self-employed by 0.1 and 0.2 percentage points, respectively.

To account for multiple hypotheses testing, we employ the Benjamini-Hochberg step-up method to control for the false discovery rate (FDR) (Benjamini and Hochberg 1995). A regular p-value of 0.05 suggests that 5 percent of all tests result in false positives. An FDR adjusted q-value of 0.05 instead suggests that 5 percent of *significant* tests result in false positives. We note that the government worker outcome is not robust to the simultaneous inference correction, but the unpaid family worker outcome is—it is still significant at the 1 percent level. None of the statistically significant effects of private preschool are still statistically significant with the FDR adjusted q-value.

We turn to Table 7 for occupational changes. We find that higher public preschool access leads to a 2.3 percentage point increase in the likelihood of being employed as an agricultural worker.[19] Being the only statistically significant outcome in seven tests, we find that it is almost robust to the Benjamini-Hochberg correction, with an adjusted q-value of 0.108. Meanwhile, higher private preschool access suggestively leads to the higher probability of being employed as a manager and a production worker. The first, however, does not stand the test of false discovery, while the latter does.

Table 8 lists the top 3 occupations per occupation category.[20] Agricultural and animal husbandry workers, planters and farmers, and forestry workers make up a 97.74 percent share of agricultural workers. Tailors, food and beverage processors, and spinners make up an 84.7 percent share of production workers. These occupations represent traditional occupations in the informal sector that do not require a full-time commitment from preschool-aged eligible mothers. Better

[19] Agricultural workers do not have to be paid. In fact, 49.39 percent of females working as agricultural workers are unpaid family workers.

[20] IFLS collects open-ended description of occupation and categorizes them in 2-digit occupation codes. We aggregate them to the 1-digit occupation headings to reduce the number of tests.

preschool access helps alleviate mothers' childcare burden, albeit for only a few hours a day. Figure 7 summarizes the occupational changes resulting from better public and private access. It plots the DDD-FE estimates in standardized z-scores to allow for a visual comparison of different occupations with different baseline employment shares.

Similarly, we investigate whether preschool availability affects the sectors that women enter. Figure 8 summarizes the sectoral changes resulting from better preschool access. Visually, we observe that higher public preschool density leads to a lower likelihood of working in the utility sector and a higher likelihood of working in the social services sector. Higher private preschool density does not have any effect on women's choice of industry. If anything, private preschool may have a marginally positive effect on women's likelihood to work in the manufacturing sector. Table 9 suggests, however, that none of these sectoral changes hold under the false discovery rate correction.

5.4 Other margins

Other employment margins, such as salary and work hours, are certainly of interest. Unfortunately, they are not collected in the historical employment module in all IFLS rounds,[21] resulting in a largely unbalanced panel. We provide suggestive evidence with the available data. We do not find any effect of higher public and private preschool density on monthly salary, net profit, and income, or on work hours per week (Appendix Table 5). This suggestive evidence seems reasonable given the fact that preschools only operate 3 hours a day and likely would not substantially contribute to these intensive margins of employment.

Appendix Table 6 suggests that higher access to public and private preschool does not increase the likelihood of mothers reporting working and job searching as their main activity in lieu of housekeeping. If anything, we find that higher private preschool density leads to a reduction in the likelihood of eligible mothers attending school. This puzzling finding does not seem to be a false discovery.

6. Discussion

6.1 Complementarity and substitutability of public and private preschools

[21] Earnings and work hours are asked pertaining to current and historical employment in rounds 1-3 but are limited to current employment in rounds 4-5. Main activities are limited to current employment in all rounds.

Public and private preschools appear to help women's employment outcomes. However, they seem to cater to different subsets of the population and separately induce women into different types of occupations. To make more informed policy recommendations, we would like to understand whether public and private preschools serve as complements or substitutes to one another. Can we reject equality between the public and private preschool effects?

Table 11 summarizes our exploration. Columns 1 and 2 present the same estimates as in Table 3 Column 6, separately for public and private preschools. Column 3 includes both public and private preschool densities and the interaction with the child's preschool age eligibility and tests the equality of public and private preschools DDD-FE coefficients. An additional public preschool per 1,000 children leads to a 7.1 percentage point increase in women's work participation, but a private preschool has virtually a zero effect on women's work participation. We formally test the equality of the two DDD-FE coefficients, and we can reject equality at the 1 percent significance level.

Column 4 adds in the triple interaction of public and private preschool densities and the preschool age eligibility dummy. The negative and statistically significant triple interaction coefficient suggests that public and private preschools are likely substitutes to one another, although they might be poor substitutes given the small magnitude of -0.003 and marginal statistical significance at the 10 percent level. Building more public preschools may not crowd-out private preschools enrollment at the current level of preschool availability and the current level of preschool enrollment.

6.2 Heterogeneous effects by child's age

We are also interested to know whether preschools could have spillover effects on other non-eligible mothers. Mothers could anticipate the rollout of preschools and increase their work participation even before their children are of preschool age. On the other hand, preschools may allow mothers to work when their children are age-eligible, accumulate longer work experience, and command higher wage rates when their children get older and graduate from preschools (Lefebvre et al. 2009). Longer work tenure and higher wage rates presumably push FLFP upward later in the life cycle. While Tables 2 and 3 suggest that preschools do not benefit non-eligible mothers in general, the effects may be washed out among the large group of non-eligible mothers—those without children and those with all younger or older children.

In this section, we aim to estimate the heterogeneous effect of preschool access on mothers' employment by their child's age and relative to the omitted group of non-mothers and mothers of children older than 18. Specifically, we estimate:

$$y_{ijt} = \alpha + \sum_{a=0}^{18} \beta_a \, TK_{jt} \cdot 1(age_{it} = a) + \gamma \, TK_{jt} + \sum_{a=0}^{18} \delta_a \, 1(age_{it} = a) + \mu_j + \phi_t + \theta_i + \psi \, \boldsymbol{X_{ijt}} + \varepsilon_{ijt} \quad (4)$$

where y_{ijt}, TK_{jt}, $Eligible_{it}$, μ_j, ϕ_t, $\boldsymbol{X_{ijt}}$, θ_i are as described earlier. $1(age_{it} = a)$ is an indicator if mother *i* has a child of age *a* in year *t*. Hence, β_a and δ_a are estimated for each child's age. Standard errors are clustered at the district level.

Figure 9 plots the estimates for public preschools. First, we note that mothers of preschool-aged children have positive and statistically significant effects relative to the omitted group. We also note that there is no positive anticipation effect with a child age 0-2. There are some spurious positive effects following preschool ages, specifically with a child age 7-9. However, the spillover on older ages may be caused by the way we define our eligibility dummies. For instance, a dummy for age 8 is equal to 1 for mothers who have at least 1 child of age 8. However, the same mother may also have a younger child of age 6 who might benefit from preschool. Since mothers in our sample have on average 2.74 children, mothers of a preschool-aged child are likely to have another child slightly older and/or younger than the preschool age range.

Figure 10 plots the parallel for private preschools. We confirm that having better preschool access does not benefit mothers of preschool-aged children. The null effects persist to age 18. It is interesting to note, however, that private preschools have negative and statistically significant anticipatory effects on maternal employment. Notably, mothers of children aged 0-2 are negatively affected. This may suggest that mothers reduce their work participation in anticipation of being able to utilize preschools when their children are of preschool age.

Alternatively, we may also be interested in the mother's transition in and out of the workforce relative to the year when their child first becomes eligible for preschool. We can conduct an "event study" type of analysis surrounding a mother's firstborn's eligibility to enter preschool. We can estimate:

$$y_{ijt} = \alpha + \sum_{a=-6}^{1} \beta_a \, TK_{jt} \cdot 1(age_{it} = a) + \sum_{a=3}^{18} \beta_a \, TK_{jt} \cdot 1(age_{it} = a) + \sum_{a=-6}^{1} \delta_a \, 1(age_{it} = a)$$
$$+ \sum_{a=3}^{18} \delta_a \, 1(age_{it} = a) + \gamma \, TK_{jt} + \mu_j + \phi_t + \theta_i + \psi \, \boldsymbol{X_{ijt}} + \varepsilon_{ijt} \quad (5)$$

which is very similar to equation 4, with the following exceptions. First, treatment is defined relative to the firstborn only. Second, we trim observations at the tails—aggregating average work participation 6 and more years before and 18 and more years after the first child is born. Therefore, we interpret the regression coefficients relative to the omitted group of mothers whose first child was of age 2, one year before the child is eligible for preschool.

Figure 11 shows the results for public preschools. We first note that relative to the year before preschool eligibility, mothers' work participation only benefits from better public preschool access starting from age 4. This is consistent with the official age of entry into preschools (age 4) and when the majority of children first enter preschools (Appendix Figure 6). The effects increase up to age 7 and decline after that. The effects are no longer statistically significant from age 10 onward. This either suggests some evidence for the dynamic labor supply effect (Lefebvre et al. 2009) or that mothers whose firstborn is of age 7 are also likely to have a younger child who is eligible for preschool. The effects prior to childbirth are more spurious, with some negative statistically significant effects 2 and 5 years before the first childbirth.

Figure 11 shows the parallel for private preschools. We do not find that mothers of preschool-aged children have a better likelihood of entering the workforce once their firstborn is of preschool-age. There is a slightly positive and statistically significant effect for mothers whose firstborn is age 3. However, the effects at subsequent ages are virtually never statistically significant, with some spurious findings at age 7. There are no statistically significant effects at any of the younger ages. Appendix Figures 4 and 5 show similar event study analyses surrounding the first child's birth event. The overall stories are largely the same.

6.3 Heterogeneous effects by child's birth order

We are also interested in knowing if a similar effect can be expected of subsequent children. If there is an economy of scale in childrearing, women's work participation is particularly hurt with one child, but the marginal time-cost of an additional child might be minimal. As such, mothers with many children may not benefit as much from better public preschool access.

Table 12 re-estimates equation 3 but narrows down the way we define eligibility. Eligibility is no longer defined for any child being of preschool age, but it is defined for each child's birth order. Therefore, column 1 defines eligibility relative to the first child, column 2 to the second child, and so on. If a mother only has 2 children, preschool eligibility status will only be defined for the first two columns pertaining to her first two children. Hence, the number of observations will fall as we go across the columns because the number of mothers with three children is fewer than the number of mothers with just one child.

Additional public preschool density has positive and statistically significant effects on the work participation of mothers with preschool-aged eligible first, second, and third children. The statistical significance of the third child is still robust to simultaneous inference with an FDR q-value of 0.095. The effects on the fourth child are positive but not statistically significant; on the fifth and sixth children they are negative and not statistically significant. Having better access to a private preschool does not seem to matter for work participation. We find a spurious positive and statistically significant effect for the second child; but the indicated positive effect does not stand the false discovery test.

6.4 Heterogeneous effects by preschool enrollment level

Previous studies suggest that the effect of childcare services on maternal employment may depend on utilization rates; if the rate is already high then additional childcare services may no longer have an important effect on employment. It is important to note, however, that null findings in other contexts have 90 percent (Bauernschuster and Schlotter 2015) and 80 percent childcare utilization rates (Lundin et al. 2008). Meanwhile, average preschool enrollment rates among children aged 3-6 per district between 2003 and 2014 in Indonesia is 15.6 percent; the 99th percentile is 49.8 percent.[22] As such, we may not expect preschools in Indonesia to have reached the saturation point yet.

Table 13 provides suggestive evidence on the heterogeneous effects of preschool availability on maternal employment by preschool enrollment rates using DDD and DDD-FE strategies. The sample is restricted to PODES years with preschool enrollment data,[23] which leads to a total of 37,547 individual-year observations (less than half of the sample in Table 2). Each strategy divides the sample into three groups: those subjected to less than 20 percent (N = 23,076),

[22] Data obtained from National Socioeconomic Surveys (SUSENAS) in 2003, 2005, 2011, and 2014.
[23] Preschool enrollment data coincide with PODES years in 2003, 2005, 2011, and 2014.

between 20-40 percent (N = 11,982), and beyond 40 percent (N = 2,489) preschool enrollment rate in the district. First, we note that public preschools are most effective in districts with low preschool enrollment rates, at 0.117-0.125 effect sizes compared to 0.048-0.080 on average. This magnitude reduces with higher utilization rates; turning less statistically significant in column 2 and negative but not statistically significant in column 3. On the other hand, private preschools have the opposite trend: not statistically significant effects for low utilization rates up to 40 percent, which become more positive beyond 40 percent preschool enrollment rates. These may suggest that affordable public preschools may help mothers catch up and join the workforce in low preschool penetration areas. On the other hand, the quality of private preschools may be better in high preschool penetration areas, which results in more mothers opting to enroll their children in preschools and joining the workforce.

6.5 Heterogeneous effects by mother's education level

Our results seem to suggest a segmented market between public and private preschools, whereby public preschools cater to lower-income groups of the population and private preschools are utilized by mothers who can afford their costs. We proxy income generating ability with mothers' highest educational attainment. Table 14 summarizes the effects of preschool availability on maternal employment by their level of education. Public preschools have positive and statistically significant effects for mothers with primary and less than primary education and for mothers with at least upper secondary education. Private preschools have no statistically significant effects on the lower educated mothers and a positive and statistically significant effect for mothers with at least upper secondary education. These results suggest that public preschools may help those who may have greater financial needs and that preschools in general benefit higher educated mothers who better understand the benefits of preschools.

6.6 Robustness checks

The previous analysis focuses on an indicator if mothers have at least one eligible child. We may think that there is linearity to having multiple eligible children. Appendix Table 3 presents a parallel to our Table 2, but instead interacts the continuous preschool density with the continuous number of eligible children. We still find a strongly positive effect on work participation with public preschools, but not private preschools. Column 5 suggests that an additional public preschool per 1,000 children increases women's work participation by 6.5 percentage points per preschool-aged eligible child.

It may also be of interest in comparing our point estimates to other studies where the preschool ages are between age 3 and 5. Appendix Table 4 is an analogue to Table 2 but changes the age eligibility to be between 3 and 5. Our findings are consistent with this incremental change. In Column 5, suggesting that an additional public preschool per 1,000 children increases women's work participation by 6.6 percentage points per preschool-aged eligible child.

Appendix Table 7 explores whether our findings on sectoral changes hold, conditional on employment. We do not find evidence that women switch occupations as a result of having better access to public or private preschools, conditional on being employed. Reconciling this with Table 7 suggests that women with eligible children are more likely to work and, if they work, they work in occupations that do not require full-time commitment. However, once they work, they are not more likely to switch to any other occupation. Appendix Table 8 does the same thing for industry choice.

The estimated preschool availability effects on maternal employment are also robust to dropping large population districts. Population is defined based on the population in 1996 to minimize possible sorting to better preschool access districts.[24] Appendix Table 11 summarizes our findings with DDD (column 1-3) and DDD-FE (column 4-6) strategies. Columns 1 and 4 are the most restrictive, examining districts with less than 0.5 million population in 1996. Columns 2 and 5 include districts with a population of up to 1 million in 1996, while columns 3 and 6 include a population of up to 1.5 million. Coefficients should be compared to columns 5 and 6 in Table 3. While the positive effect of public preschools and null effect of private preschools generally remains, it is worth noting that effect sizes do get smaller in smaller districts. The interaction coefficient of public preschools is no longer statistically significant in districts with a population of less than 0.5 million with the DDD strategy and only statistically significant at the 10 percent level with the DDD-FE strategy. This potentially suggests that preschools may be more beneficial to mothers living in highly populated districts with better economic opportunities.

6.7 Sorting to better preschool access

To check if mothers with preschool-aged children sort into districts with high preschool access, we aggregate our constructed panel of individuals over the years to a panel of district-year.[25] We

[24] 1996 is the earliest round of SUSENAS, with complete coverage of the 290 districts.

[25] By construction, one PODES year is matched to several years in the panel of mothers. For instance, PODES 1993 is assigned to year 1991-1993 in the panel of mothers. If the time unit is defined annually, we obtain, mechanically,

investigate whether districts with higher preschool access induce a net migration of preschool-aged eligible mothers. Specifically, we regress the change in net migration of eligible mothers on either the change, or the lagged change, of private/public preschool density in the district. Appendix Table 9 reports our findings. We do not find any evidence of sorting for better preschool access.

7. Conclusion

Prior studies have documented the mixed evidence of preschool access on maternal employment. We find that an additional public preschool per 1,000 young children in Indonesia increases eligible mothers' work participation by 5.7-8.2 percentage points, or by 11-16 percent from the baseline mean. Mothers take up unpaid family work and jobs as agricultural workers. Improved private preschool density does not lend the same effect at the extensive margin, but it does increase the likelihood of working as an artisanal production worker. The tendency to work in informal sectors is in conjunction with the fact that preschools operate daily for less than half a day. We also document some evidence that improved preschools may particularly help mitigate the negative fertility effect of the first two children on maternal employment. The Indonesian experience with preschools may lend some guidance to other countries with low preschool enrollment and female work participation rates.

The benefits of expanding public preschool access are not limited to short-run gains in increased female employment. The World Bank (2016) argues that increasing FLFP is a more effective policy than attracting in-migrants or increasing elderly work participation to offset the shrinking workforce due to the rapidly aging economy. In this paper, we also elude to the well-documented long-term benefits of early childhood education that tend to favor children born in poor households (Berlinski et al. 2009, Havnes and Mogstad 2015, Garcia et al. 2016; Brinkman et. al. 2017). A study in Indonesia suggests that the benefits from early childhood education intervention on children's outcomes alone exceed the costs by an average of 6.01: the benefit-cost ratio is even higher for the poorest quintile (Chang et al. 2006). When conducting an economic analysis to assess the cost and benefits of preschool expansion, policy makers should keep all these various benefits in mind to ensure the right policy decisions are made.

zero change in (public/private) preschool density in district *j* between 1993 and 1992. For the purpose of this test, we restrict our analysis to PODES years and collapse our individual-level panel of mothers to the district-level.

Access alone, however, may not be sufficient. Preschools' hours of operation seemingly affect the type of employment in which women can partake. Increasing the hours of preschool operation may also be instrumental in women's work participation (Berthelon et al. 2015, Contreras and Sepulveda 2017, Padilla-Romo and Cabrera-Hernandez 2018). Similarly, mothers may also benefit from childcare for children in other age categories (Goux and Maurin 2012, Barua 2014, Bettendorf et al. 2015, Felfe et al. 2016). We also document that preschools are largely utilized by parents within walking-distance. Attanasio and Vera-Hernandez (2004), in fact, use distance to childcare centers as an instrument for participation in the childcare program. Location choice then becomes another important variable for policy makers. Chang et al. (2006) show that publicly-provided preschools in Indonesia are mostly located in urban areas, but long-term benefits are highest for poor households in rural areas. While it is likely difficult to incentivize private provisions in rural areas with sparse populations, an expansion of public preschools may be warranted to afford parents equitable access to ECED and to improve intergenerational mobility.

Finally, our results show that childcare responsibilities are constraining FLFP, at least for some women. In addition to considering preschool expansion, hours of operation, or location of preschools, other policies designed to alleviate the childcare constraints could be beneficial in boosting FLFP.

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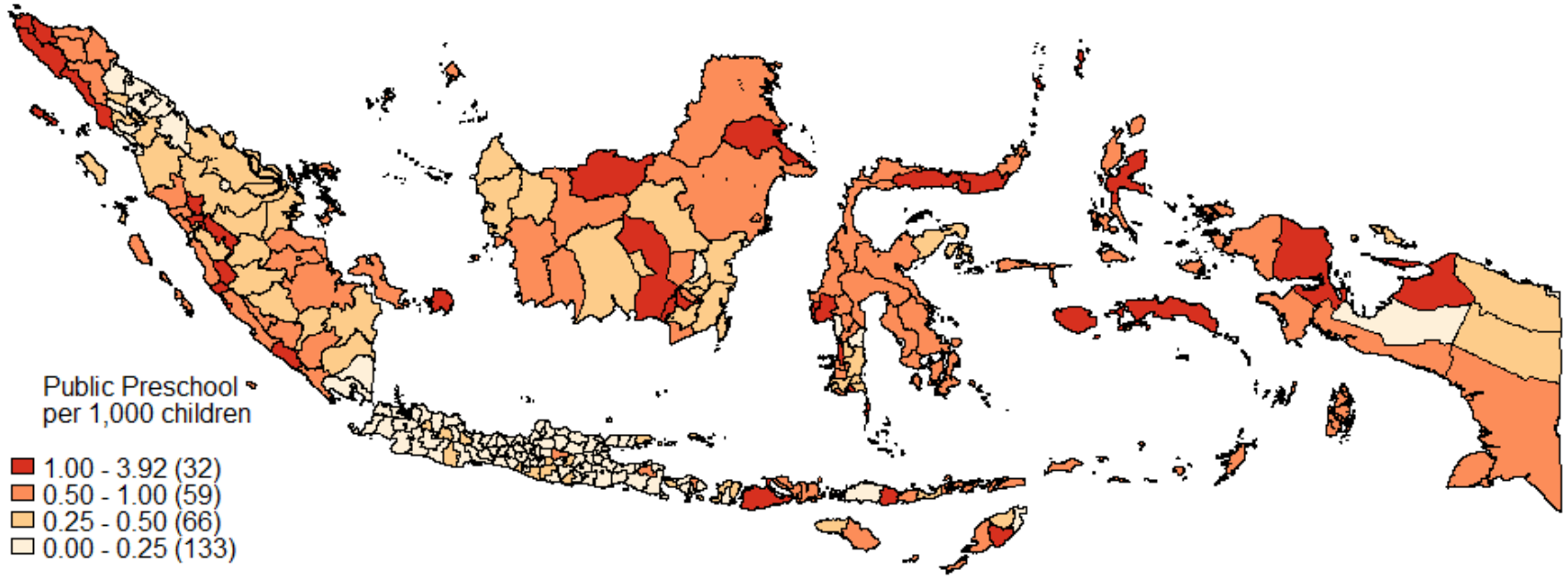

Figure 1. Spatial distribution of public preschools per 1,000 children in 2014

Notes: Number of public preschools is obtained from Podes 2014 and the population of children aged 3-6 is obtained from Susenas 2014. The legend indicates the range and distribution of public preschool densities across the Indonesian archipelago. The numbers in parentheses refer to the number of districts that fall in that range. The total number of districts, 290, reflects their existence in 1993. Districts often split over time; by March 2016, there were 511 districts. In our analyses, we maintain the 1993 district boundaries to allow comparisons over time.

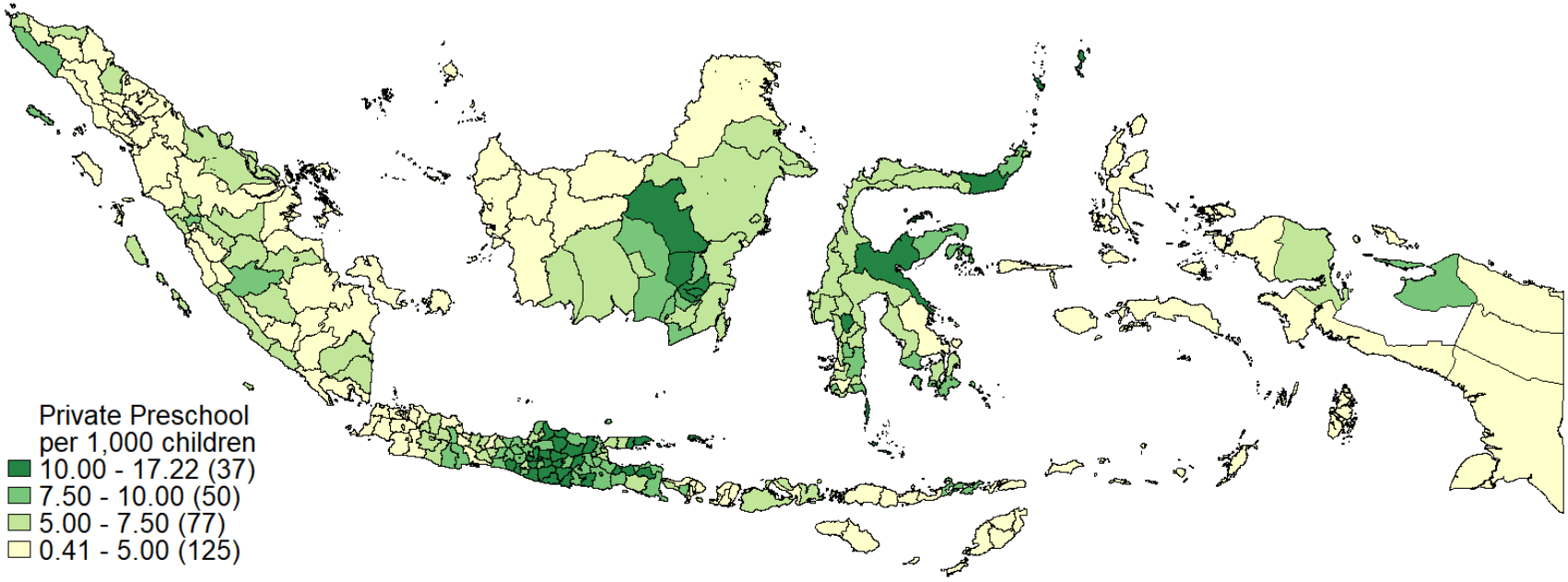

Figure 2. Spatial distribution of private preschools per 1,000 children in 2014

Notes: Number of private preschools is obtained from Podes 2014 and the population of children aged 3-6 is obtained from Susenas 2014. The legend indicates the range and distribution of private preschool densities across the Indonesian archipelago. The numbers in parentheses refer to the number of districts that fall in that range. The total number of districts, 290, reflects their existence in 1993. Districts often split over time; by 2014, there were 511 districts. In our analyses, we maintain the 1993 district boundaries to allow comparisons over time.

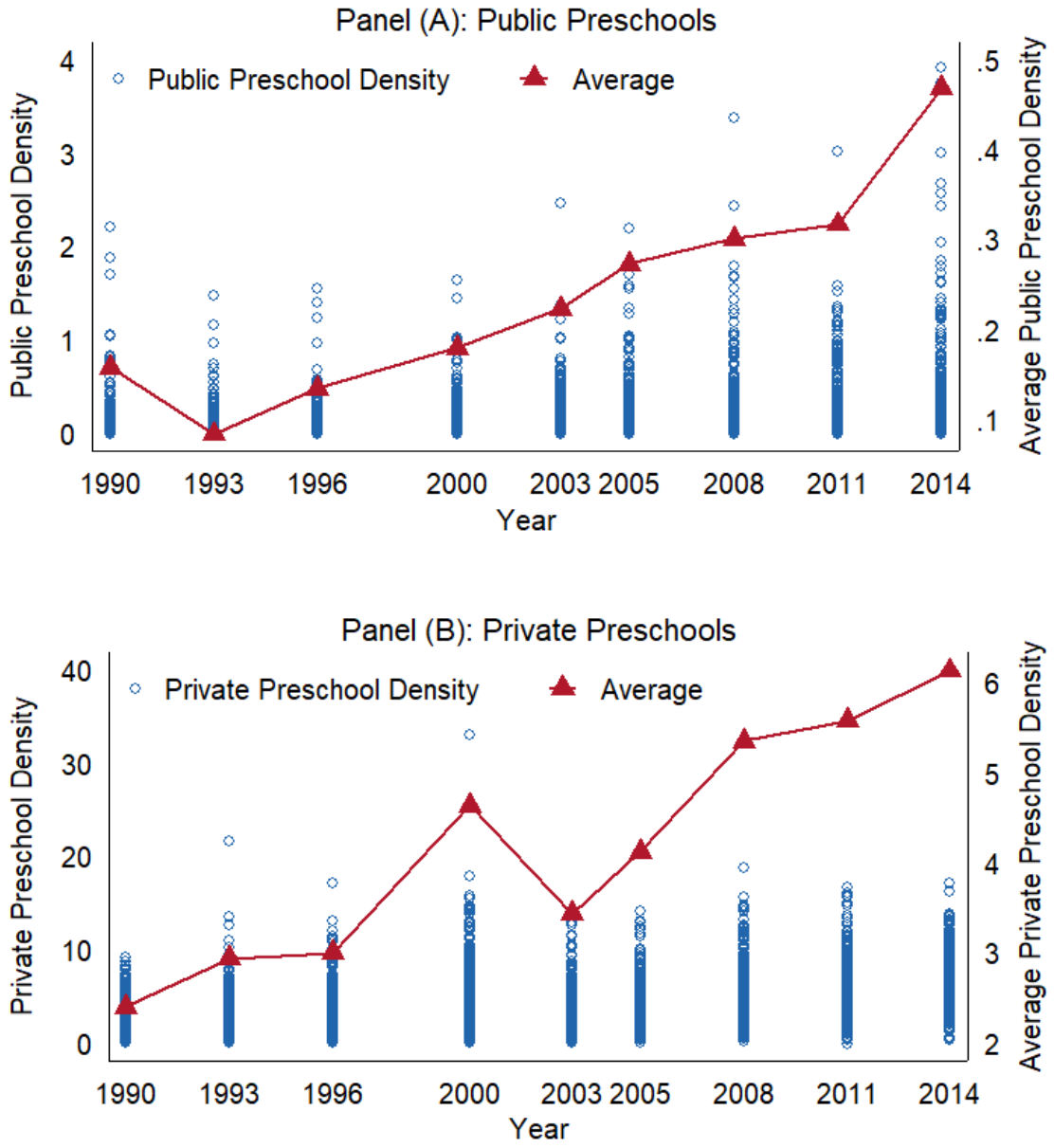

Figure 3. Density of public and private preschools across districts over time

Note: Density is defined as the number of preschools obtained from Podes divided by the population of children aged 3-6 obtained from Susenas, in respective years. Panel A and B indicate density of public and private preschools, respectively. Scatterplot of preschool densities across 290 districts, as they existed in 1993, over time are shown in blue hollow circles. Red triangles indicate the average density of preschools across 290 districts per year. Districts often split over time; by 2014, there were 511 districts. In our analyses, we maintain the 1993 district boundaries to allow comparisons over time.

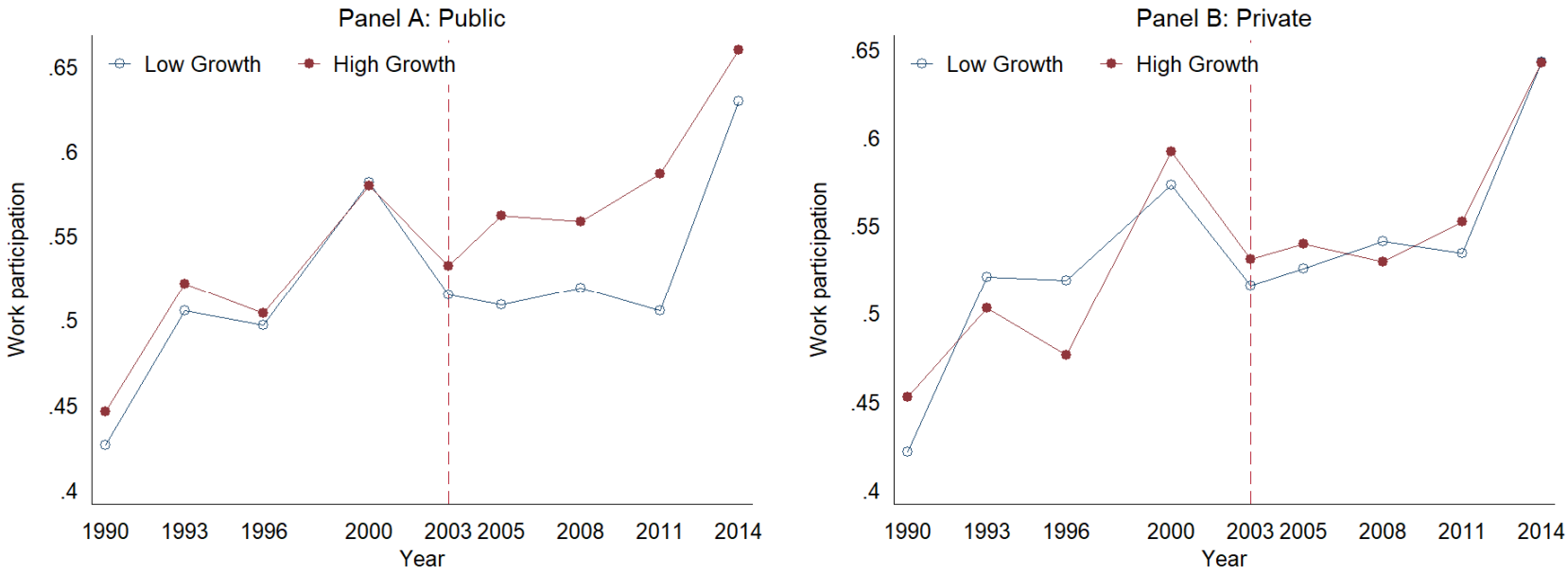

Figure 4. Average work participation of mothers with preschool-aged children in high and low preschool growth districts

Note: Sample is restricted to mothers with preschool-aged children (age 3-6) in PODES years. High preschool growth districts are defined as districts that at least double the density of preschools between 2003 and 2014. Median public and private preschool density growth between 2003 and 2004 are 85 and 92 percent, respectively. Panel A shows average employment across high and low public preschool density growth districts; Panel B shows the parallel for private preschools. Pre-trends and post-trends denote employment trends before and after National System Education Act was passed in 2003, respectively.

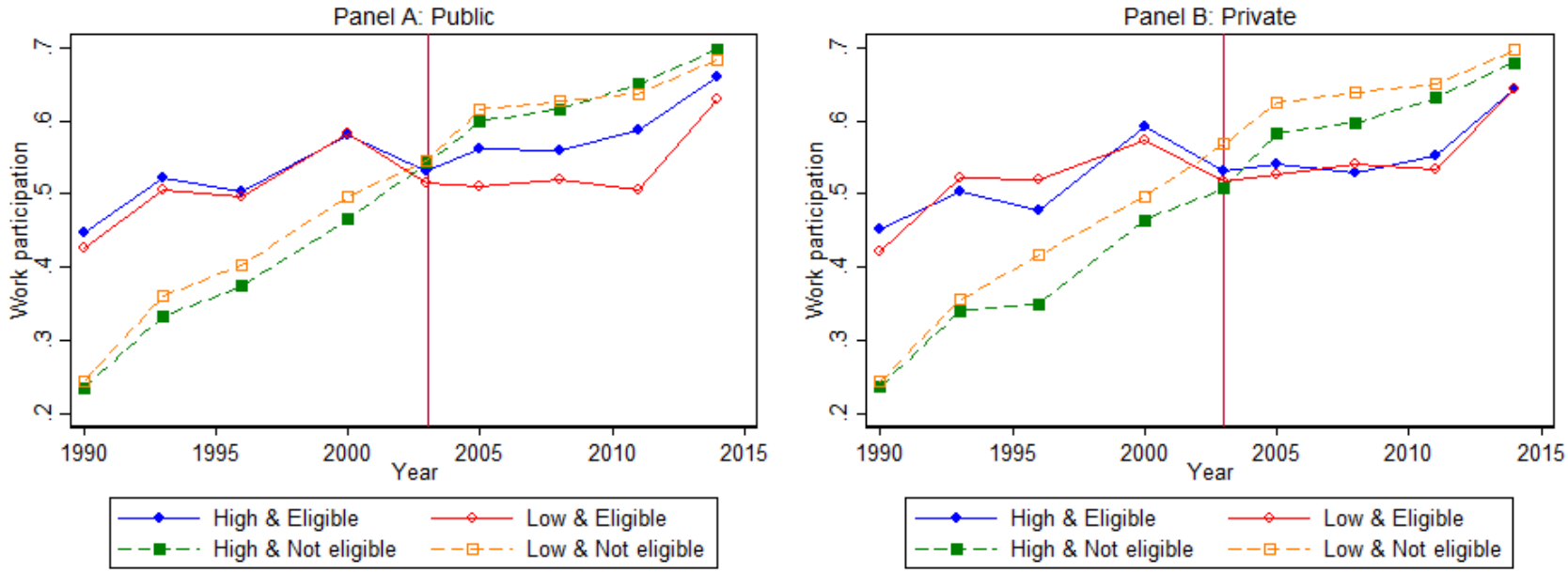

Figure 5. Average work participation of mothers with and without preschool-aged children in high and low preschool growth districts

Note: Sample includes eligible mothers with preschool-aged children (age 3-6) and non-eligible women without any preschool-aged children in PODES years. Non-eligible women include non-mothers and mothers with children outside of preschool ages. High preschool growth districts are defined as districts that at least double the density of preschools between 2003 and 2014. Median public and private preschool density growth between 2003 and 2004 are 85 and 92 percent, respectively. Panel A shows average employment across high and low public preschool density growth districts; Panel B shows the parallel for private preschools. Pre-trends and post-trends denote employment trends before and after National System Education Act was passed in 2003, respectively. Solid lines indicate eligible mothers and dashed lines indicate non-eligible mothers. Solid symbols indicate high-growth districts and hollow symbols indicate low-growth districts.

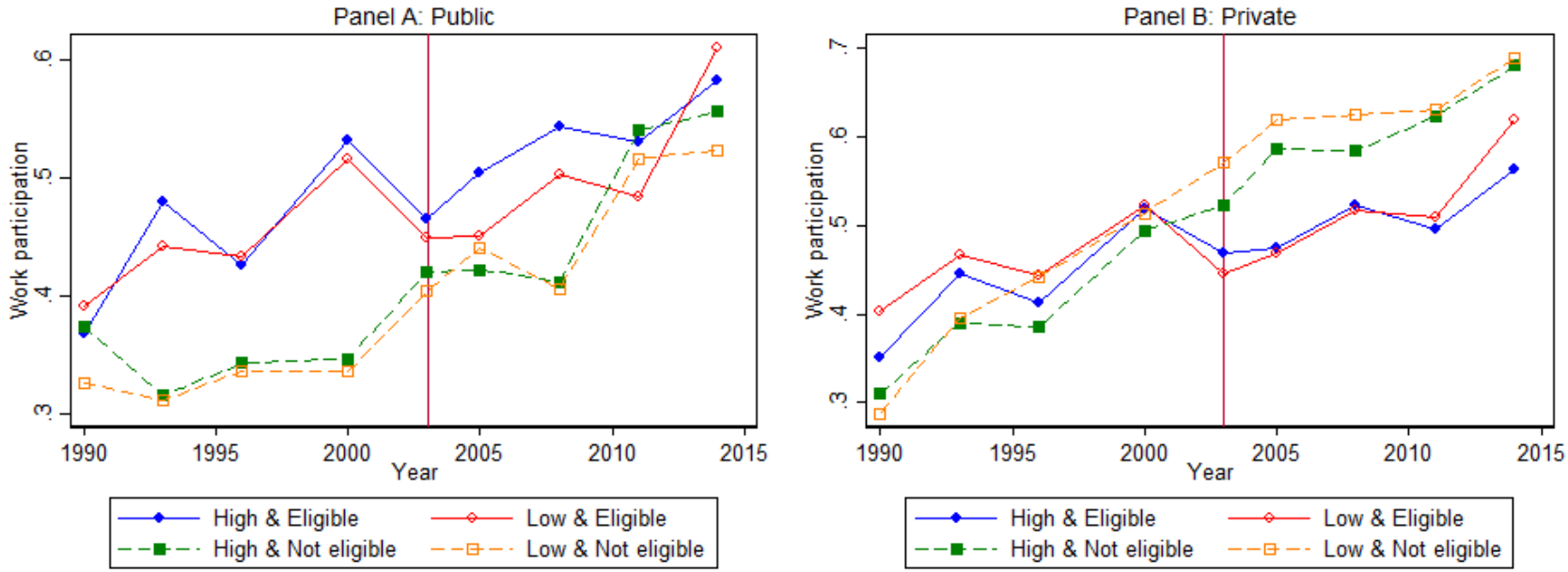

Figure 6. Average work participation of mothers whose first child is preschool-aged (ages 3-6) and mothers whose first child is too young for preschools (ages 0-2) in high and low preschool growth districts

Note: Sample includes eligible mothers whose first child is preschool-aged (age 3-6) and non-eligible mothers whose first child is too young for preschools (ages 0-2) in PODES years. High preschool growth districts are defined as districts that at least double the density of preschools between 2003 and 2014. Median public and private preschool density growth between 2003 and 2004 are 85 and 92 percent, respectively. Panel A shows average employment across high and low public preschool density growth districts; Panel B shows the parallel for private preschools. Pre-trends and post-trends denote employment trends before and after National System Education Act was passed in 2003, respectively. Solid lines indicate eligible mothers and dashed lines indicate non-eligible mothers. Solid symbols indicate high-growth districts and hollow symbols indicate low-growth districts.

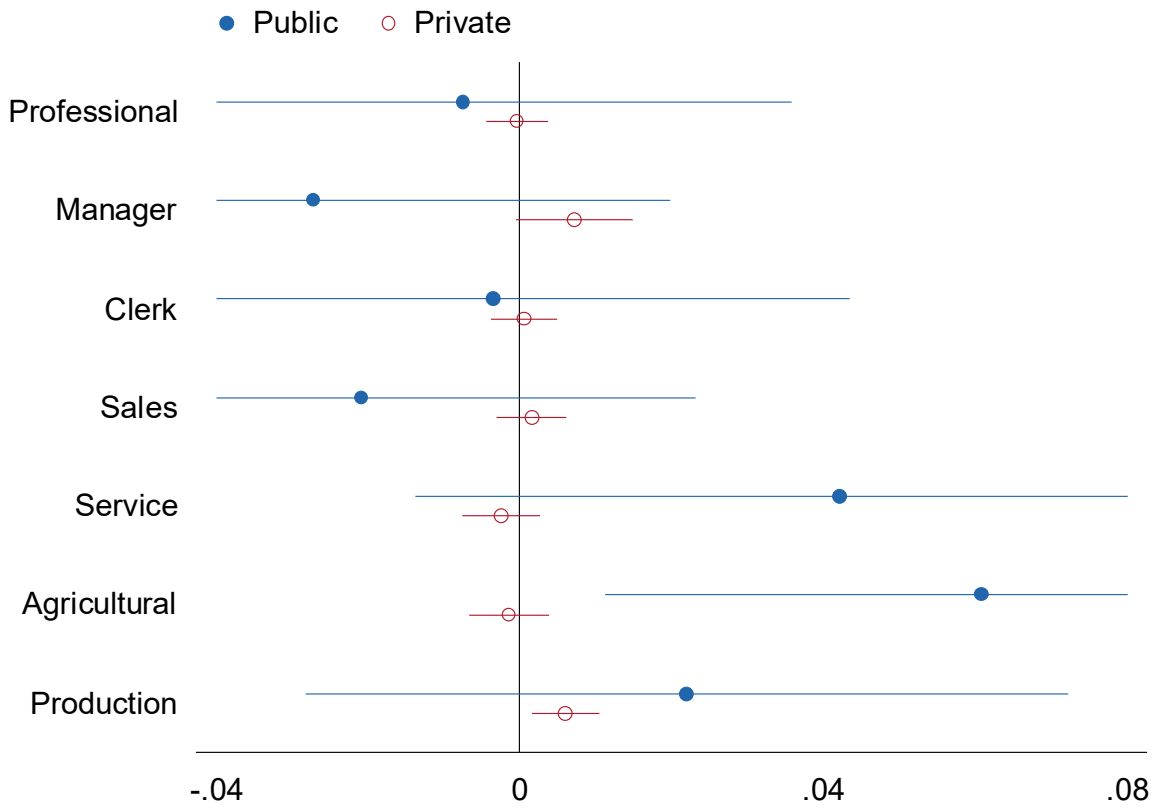

Figure 7. Effect of preschools on mothers' choice of occupation

Note: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each dot represents the interaction coefficient of public/private preschool density in one's district of residence and a dummy for having a preschool-eligible child on the probability of being employed in a certain occupation, standardized in z-scores to allow visual comparison across occupation categories. IFLS collects 2-digit occupation codes; we group occupations by their headings (see Table 8 for a list of top-3 occupations per heading). 95 percent confidence intervals are represented by horizontal lines. See Table 7 for non-standardized effect sizes and adjustments to multiple hypotheses testing.

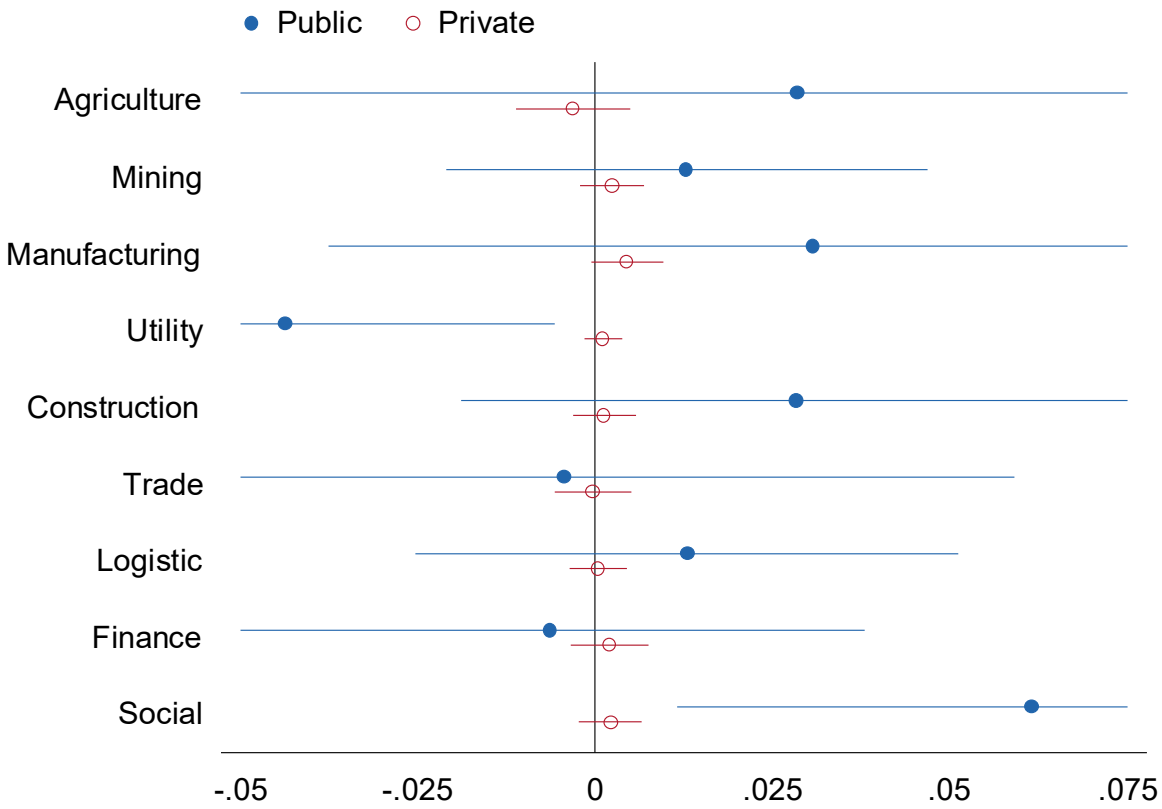

Figure 8. Effect of preschools on mothers' choice of industry

Note: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each dot represents the interaction coefficient of public/private preschool density in one's district of residence and a dummy for having a preschool-eligible child on the probability of being employed in a certain industry, standardized in z-scores to allow visual comparison across industry categories. IFLS collects 1-digit industry codes (see Table 10 for a list of top-3 occupations per industry code). 95 percent confidence intervals are represented by horizontal lines. See Table 9 for non-standardized effect sizes and adjustments to multiple hypotheses testing.

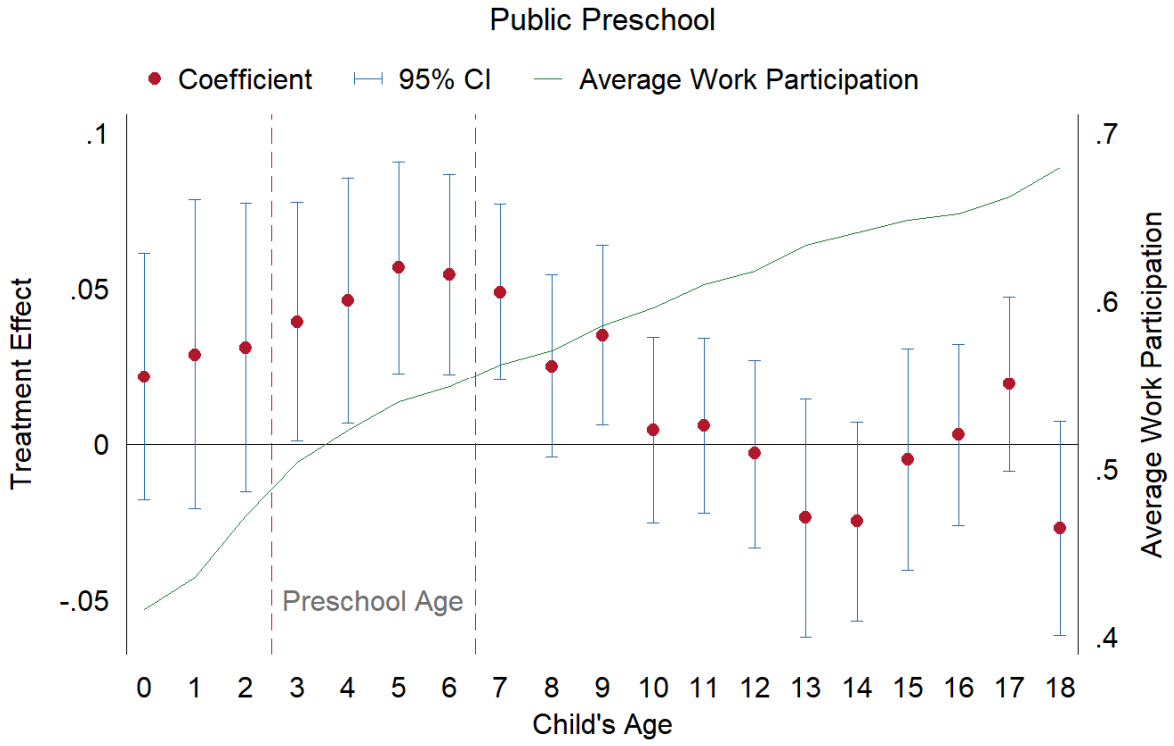

Figure 9. Effect of public preschools on mothers' work participation by child's age

Note: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each dot represents the interaction coefficient of public preschool density in one's district of residence and child's age. Children aged 18 and older are lumped in 18-year-old child group. Treatment effects are interpreted relative to the omitted group: non-mothers. Blue spikes represent 95 percent confidence intervals. Green line shows average work participation by child's age.

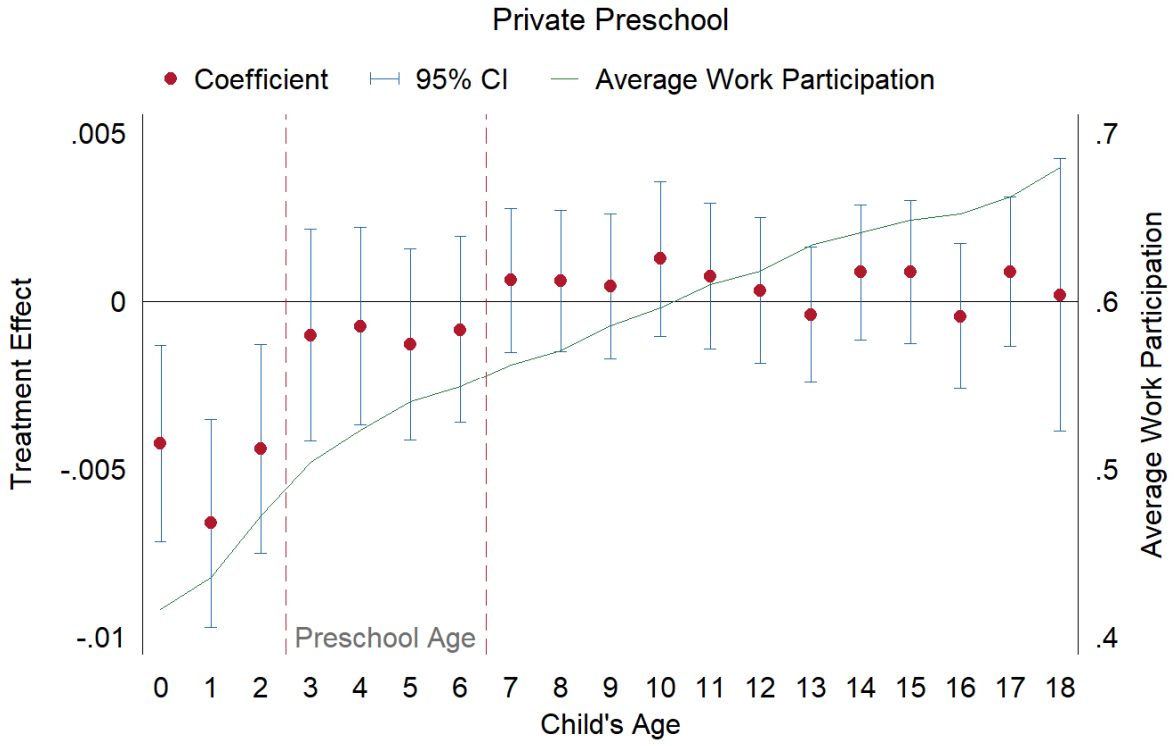

Figure 10. Effect of private preschools on mothers' work participation by child's age

Note: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each dot represents the interaction coefficient of private preschool density in one's district of residence and child's age. Children aged 18 and older are lumped in 18-year-old child group. Treatment effects are interpreted relative to the omitted group: non-mothers. Blue spikes represent 95 percent confidence intervals. Green line shows average work participation by child's age.

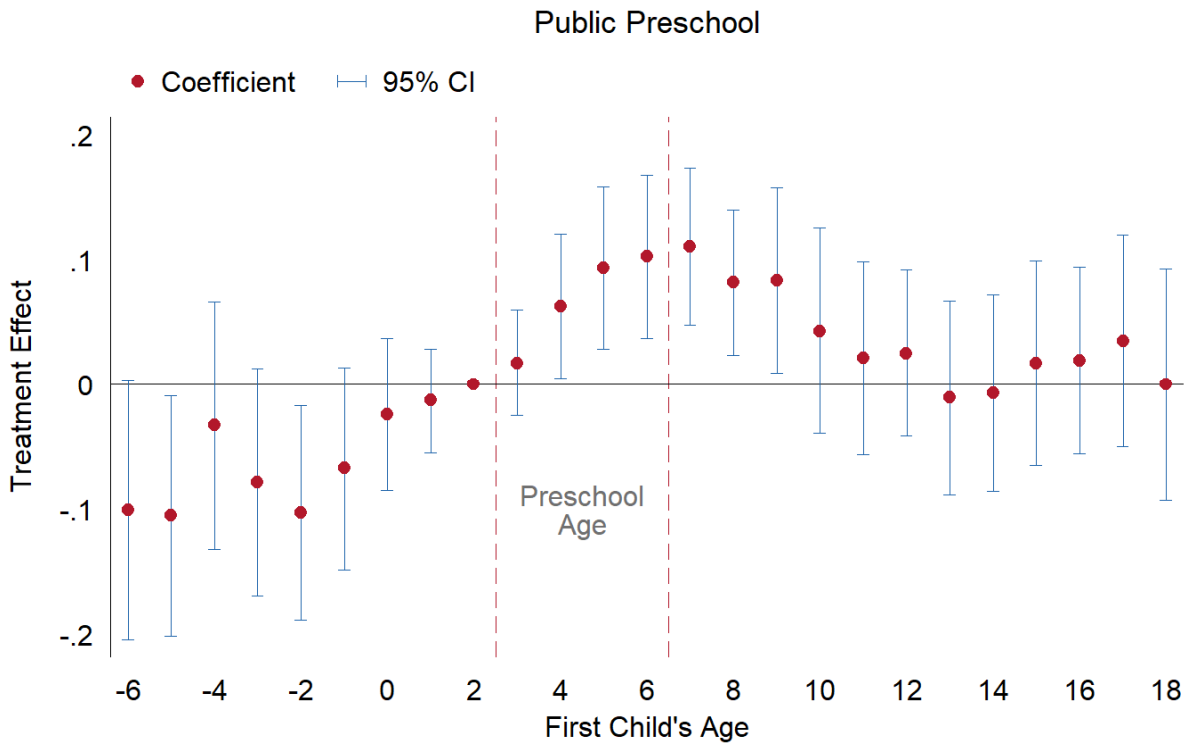

Figure 11. Event study on the effect of public preschools on mothers' work participation by first child's age relative to pre-preschool-age level

Note: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each dot represents the interaction coefficient of public preschool density in one's district of residence and first child's age. Mother's work participations are averaged at the tails; 6 and more years prior to the first childbirth and when the first child was 18 and older. Treatment effects are interpreted relative to the omitted year prior to the first child's age third birthday. Blue spikes represent 95 percent confidence intervals.

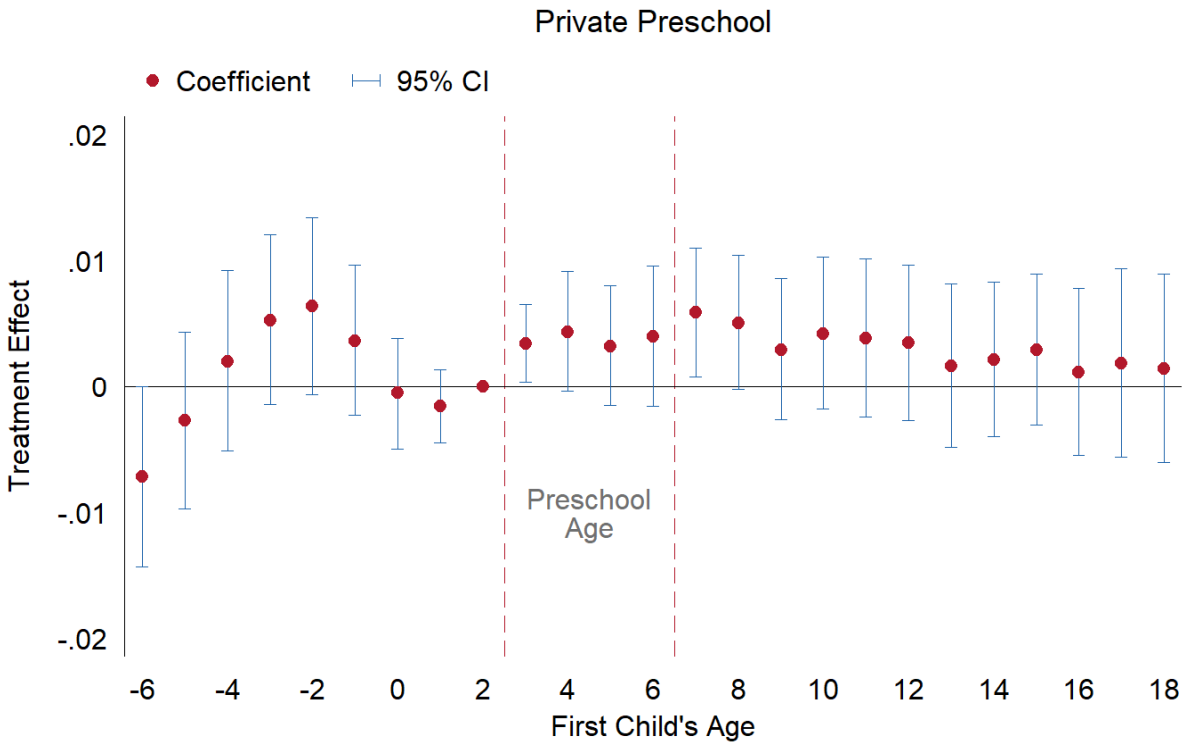

Figure 12. Event study on the effect of private preschools on mothers' work participation by first child's age relative to pre-preschool-age level

Note: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each dot represents the interaction coefficient of private preschool density in one's district of residence and first child's age. Mother's work participations are averaged at the tails; 6 and more years prior to the first childbirth and when the first child was 18 and older. Treatment effects are interpreted relative to the omitted year prior to the first child's age third birthday. Blue spikes represent 95 percent confidence intervals.

Table 1. Summary statistics

	Obs	Mean	SD
Panel A: Individual-year means			
Age	227,579	31.57	11.20
Have preschool-aged child	227,579	0.30	0.46
Work participation	227,559	0.52	0.50
Public preschool density (Inferred in-between)	226,420	0.16	0.22
Private preschool density (Inferred in-between)	226,420	4.60	3.25
Public preschool density (Linear projection)	227,579	0.15	0.21
Private preschool density (Linear projection)	227,579	4.45	3.16
Urban	227,579	0.51	0.50
Panel B: Individual-year means (PODES years only)			
Age	77,318	31.98	11.22
Have preschool-aged child	77,318	0.30	0.46
Work participation	77,312	0.53	0.50
Public preschool density	76,957	0.16	0.22
Private preschool density	76,957	4.52	3.22
Urban	77,318	0.51	0.50
Panel C: Individual means			
Number of surveys	10,340	3.54	1.13
Number of years	10,340	22.01	5.00
Number of PODES years	10,340	7.48	1.46
Age of first marriage	10,329	20.23	4.59
Age of first birth	10,337	22.13	4.52
Number of children	10,340	2.74	1.59
Years of education	10,140	7.75	4.39
Panel D: District-year means (PODES years only)			
Number of districts	290		
Public preschool density	2,559	0.24	0.35
Private preschool density	2,559	4.18	3.24
Public preschool count	2,592	10.27	14.11
Private preschool count	2,592	227.48	243.76
Child age 3-6 population	2,566	61,206	56,815

Notes: We constructed individual-year panel of mothers aged 19-45 in at least two IFLS rounds. Panel A describes the pooled observations of mothers across all the observed years. Panel B describes the pooled observations of mothers in PODES years only. Panel C describes unique observations of individual mothers. Panel A, B, and C are constructed from IFLS 1-5. Panel D is constructed from multiple rounds of PODES and Susenas from 1990-2014, as described in Section 3. Panel D describes pooled observations of districts across PODES and Susenas years. Preschool counts are obtained from PODES and child age 3-6 population from Susenas; densities are defined as preschool count divided by 1,000 children in the district. Districts often split over time; by 2014, there were 511 districts. In our analyses, we maintain the 1993 district boundaries to allow comparisons over time.

Table 2. Comparison of effects of preschool availability on maternal employment across econometric strategies

Econometric strategy:	Work participation					
	DD		DDD		DDD-FE	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Public						
Preschool * Eligible			0.048**	0.050**	0.080***	0.079***
			(0.021)	(0.021)	(0.022)	(0.022)
Preschool density	-0.014	0.013	-0.011	0.005	-0.017	-0.008
	(0.026)	(0.031)	(0.015)	(0.018)	(0.015)	(0.018)
Eligible child			-0.051***	-0.052***	-0.034***	-0.034***
			(0.008)	(0.007)	(0.007)	(0.007)
Panel B: Private						
Preschool * Eligible			-0.000	-0.000	0.001	0.001
			(0.002)	(0.002)	(0.002)	(0.002)
Preschool density	-0.010***	-0.008***	-0.003	-0.003*	-0.001	-0.004**
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Eligible child			-0.042***	-0.044***	-0.025**	-0.025**
			(0.012)	(0.011)	(0.010)	(0.010)
Observations	22,737	22,737	76,951	76,951	76,951	76,951
Mean	0.538	0.538	0.534	0.534	0.534	0.534
District Trend		X		X		X

Note: Sample in column 1-2 is restricted to mothers with preschool-aged children (age 3-6). Sample in column 3-6 includes mothers with and without preschool-aged children (age 3-6). Column 1-2 regress work participation on preschool density in a difference-in-differences (DD) strategy (Section 4.1). Column 3-4 regress work participation on preschool density, a dummy for having a preschool-aged child, and the interaction between the two in a difference-in-difference-in-differences (DDD) strategy (Section 4.2). Column 5-6 builds on the DDD strategy and adds individual fixed effect (DDD-FE) to allow comparison within-mothers (Section 4.3). Column 2, 4, 6 add district-specific trends. All regressions include district, year, mother's age fixed effects and an urban residence dummy. Panel A and B look at the effect of public and private preschool densities separately. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. Observations, work participation means, and inclusion of district trends are indicated in the last three rows.

Table 3. Effect of preschool availability on maternal employment with various preschool data

	Work participation								
Preschool data:	PODES years only			Infer in-between			Linear projection		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Public									
Preschool *		0.048**	0.080***		0.057***	0.074***		0.062***	0.082***
Eligible		(0.021)	(0.022)		(0.017)	(0.016)		(0.020)	(0.019)
Preschool	-0.014	-0.011	-0.017	0.002	-0.016	-0.015	-0.007	-0.021	-0.019
density	(0.026)	(0.015)	(0.015)	(0.023)	(0.013)	(0.012)	(0.027)	(0.016)	(0.014)
Eligible child		-0.051***	-0.034***		-0.051***	-0.034***		-0.052***	-0.035***
		(0.008)	(0.007)		(0.007)	(0.005)		(0.007)	(0.005)
Panel B: Private									
Preschool *		-0.000	0.001		0.001	0.002		0.001	0.003*
Eligible		(0.002)	(0.002)		(0.002)	(0.001)		(0.002)	(0.001)
Preschool	-0.010***	-0.003	-0.001	-0.007**	-0.003*	-0.001	-0.008**	-0.002	0.000
density	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)
Eligible child		-0.042***	-0.025**		-0.046***	-0.032***		-0.047***	-0.034***
		(0.012)	(0.010)		(0.011)	(0.008)		(0.011)	(0.008)
Method	DD	DDD	DDD-FE	DD	DDD	DDD-FE	DD	DDD	DDD-FE
Observations	22,737	76,951	76,951	67,431	226,400	226,400	67,788	227,559	227,559
Mean	0.538	0.534	0.534	0.529	0.520	0.520	0.530	0.521	0.521

Note: Column 1-3 are restricted to PODES years only. Column 4-6 infer preschool data in-between PODES years using the closest upper year available, e.g. year 1992 sandwiched between PODES 1990 and 1993 will use 1993 round. Column 7-9 predict preschool density using linear projection with the closest two data points available, e.g. year 1992 fits a linear projection using preschool density data in PODES 1990 and 1993. Column 1, 4, 7 regress work participation on preschool density in DD method. Column 2, 5, 8 regress work participation on the interaction of preschool density and preschool eligibility dummy in DDD method. Column 3, 6, 9 add individual fixed effect to the DDD method. All regressions include district, year, mother's age fixed effects and an urban residence dummy; and do not include district-specific trends. Panel A and B look at the effect of public and private preschool densities separately. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. Observations and dependent variable means are indicated in the last two rows.

Table 4. Effect of preschool availability on maternal employment depending on which child is preschool-aged eligible

	Work participation					
Eligible Child	Any		Oldest		Youngest	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Public						
Preschool *	0.057***	0.074***	0.041*	0.075***	0.035	0.055**
Eligible	(0.017)	(0.016)	(0.025)	(0.020)	(0.025)	(0.023)
Preschool	-0.016	-0.015	-0.004	-0.003	-0.003	-0.000
density	(0.013)	(0.012)	(0.013)	(0.012)	(0.014)	(0.012)
Eligible child	-0.051***	-0.034***	-0.042***	-0.046***	-0.001	-0.004
	(0.007)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
Panel B: Private						
Preschool *	0.001	0.002	-0.000	0.002	-0.001	0.001
Eligible	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
Preschool	-0.003*	-0.001	-0.003	-0.001	-0.002	-0.001
density	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Eligible child	-0.046***	-0.032***	-0.035***	-0.046***	0.008	-0.000
	(0.011)	(0.008)	(0.010)	(0.009)	(0.010)	(0.009)
Method	DDD	DDD-FE	DDD	DDD-FE	DDD	DDD-FE

Note: Sample includes all mothers with and without a preschool-aged eligible child (age 3-6). Comparison group includes non-mothers and mothers of children with the wrong ages. Definition of eligibility varies across columns. Column 1-2 define eligibility pertaining to any child, column 3-4 to the oldest child, and column 5-6 to the youngest child. All columns regress work participation on preschool density, preschool eligibility dummy, and their interaction. All regressions include district, year, and an urban residence dummy. Odd-numbered columns are estimated using DDD method. Even-numbered columns add individual fixed effects in DDD-FE method. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. There are 226,400 observations in all columns.

Table 5. Effect of preschool availability on maternal employment with different comparison groups

	Work participation					
Eligible Child	Oldest			Youngest		
Comparison Cohort	0-2	7-12	All	0-2	7-12	All
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Public						
DDD	0.063**	-0.022	0.041*	-0.020	0.001	0.035
	(0.028)	(0.027)	(0.025)	(0.029)	(0.022)	(0.025)
DDD-FE	0.075***	0.023	0.075***	-0.037	0.023	0.055**
	(0.026)	(0.026)	(0.020)	(0.027)	(0.018)	(0.023)
Panel B: Private						
DDD	0.004**	-0.002	-0.000	0.006***	-0.004***	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
DDD-FE	0.004*	-0.001	0.002	0.004**	0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	49,067	69,329	226,400	54,661	58,653	226,400
Mean	0.445	0.520	0.520	0.548	0.640	0.520

Note: Sample includes eligible mothers whose first/last child are aged 3-6 (preschool-aged) and comparison mothers whose first/last child are aged as indicated in the column heading. Ages 0-2 are too young for preschools and ages 7-12 are primary school ages. The 'All' columns include mothers of children aged 0-2, 7-12, 13+, and non-mothers in the comparison group. Column 1-3 define eligibility pertaining to the oldest child only and column 4-6 to the youngest child only. All columns regress work participation on preschool density, preschool eligibility dummy, and their interaction; only interacted coefficients are reported. All regressions include district and year fixed effects, and an urban residence dummy. Estimation strategy used is indicated at each row. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. Number of observations and means of work participation for each column are indicated in the last two rows.

Table 6. Effect of preschool availability on female's work status

	(1) Has a second job	(2) Self-employed	(3) Government worker	(4) Private worker	(5) Unpaid family worker
Panel A: Public					
Public * Eligible	0.008	0.017	0.006*	0.011	0.041***
	(0.010)	(0.010)	(0.004)	(0.011)	(0.011)
Public Preschools	-0.015*	0.001	-0.004	-0.020*	0.006
	(0.009)	(0.007)	(0.005)	(0.010)	(0.010)
Eligible Child	0.001	-0.001	0.000	-0.040***	0.007**
	(0.003)	(0.003)	(0.001)	(0.004)	(0.003)
FDR q-value	0.435	0.299	0.299	0.435	0.002
Panel B: Private					
Private * Eligible	0.001*	0.002**	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Private Preschools	-0.000	-0.003**	-0.000	-0.000	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Eligible Child	-0.004	-0.007	0.002	-0.038***	0.012*
	(0.004)	(0.005)	(0.002)	(0.006)	(0.006)
FDR q-value	0.204	0.204	0.992	0.992	0.992
Observations	225,985	226,400	226,400	226,400	226,400
Mean	0.077	0.172	0.037	0.185	0.125

Notes: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each column regresses the dependent variable indicated in column heading on the density of public (Panel A) or private (Panel B) preschools, defined as the number of preschools divided by the population of children aged 3-6 in each district, a dummy for having a preschool-aged eligible child, and their interaction. Comparison group includes all mothers of children of the wrong ages and non-mothers. All regressions include mother's age fixed effect, urban dummy, district, year, and individual fixed effects. Robust standard errors clustered at district level. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR q-values for the interaction coefficient of preschool density and eligible child are computed over all 5 outcomes and are shown in the last row of each panel. FDR q-values indicate the probability of false positives among *significant* tests. Number of female-year observations and means of dependent variables are indicated in the last two rows.

Table 7. Effect of preschool availability on female's occupation

	(1) Professional	(2) Manager	(3) Clerk	(4) Sales	(5) Service	(6) Agricultural	(7) Production
Panel A: Public							
Public * Eligible	-0.002	-0.001	-0.001	-0.007	0.013	0.023**	0.006
	(0.005)	(0.001)	(0.004)	(0.008)	(0.009)	(0.010)	(0.007)
Public Preschools	-0.003	-0.000	-0.004	-0.007	-0.012	0.022*	0.002
	(0.004)	(0.001)	(0.004)	(0.007)	(0.007)	(0.013)	(0.007)
Eligible Child	-0.003*	0.000	-0.009***	-0.004	-0.011***	0.007**	-0.005**
	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)
FDR q-value	0.884	0.884	0.884	0.884	0.821	0.108	0.884
Panel B: Private							
Private * Eligible	-0.000	0.000*	0.000	0.001	-0.001	-0.001	0.002***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Private Preschools	0.000	-0.000	-0.000	0.000	-0.004***	0.003**	-0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)
Eligible Child	-0.002	-0.001**	-0.010***	-0.008	-0.006	0.013**	-0.011***
	(0.002)	(0.001)	(0.002)	(0.005)	(0.004)	(0.006)	(0.003)
FDR q-value	0.860	0.399	0.860	0.860	0.860	0.860	0.055
Observations	226,400	226,400	226,400	226,400	226,400	226,400	226,400
Mean	0.046	0.001	0.030	0.143	0.104	0.177	0.073

Notes: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each column regresses the dummy for being employed in a certain occupation category indicated in column heading on the density of public (Panel A) or private (Panel B) preschools, defined as the number of preschools divided by the population of children aged 3-6 in each district, a dummy for having a preschool-aged eligible child, and their interaction. Comparison group includes all mothers of children of the wrong ages and non-mothers. All regressions include mother's age fixed effect, urban dummy, district, year, and individual fixed effects. Robust standard errors clustered at district level. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR q-values for the interaction coefficient of preschool density and eligible child are computed over all 7 outcomes and are shown in the last row of each panel. FDR q-values indicate the probability of false positives among *significant* tests. Number of female-year observations and means of dependent variables are indicated in the last two rows. See Table 6 for a list of top-3 occupations per category.

Table 8. List of top-3 occupations within each occupation category

Occupation categories	N	percent (within category)
Professionals		
Teachers	7,073	67.43
Nurses, midwives, x-ray technicians, traditional medicine	1,500	14.30
Workers in religion	591	5.63
Managers		
Managers	236	69.62
Administrator unknown	44	12.98
Legislative officials and government administrators	31	9.14
Clerical workers		
Bookkeepers, cashiers, and related workers	2,965	43.53
Clerical and related workers not elsewhere classified	1,983	29.11
Government executive of officials	935	13.73
Sales workers		
Salesmen, shop assistants and related workers	26,727	82.50
Working proprietors (wholesale and retail trade)	4,298	13.27
Insurance, real estate, securities and business services salesman and auctioneers	507	1.57
Service workers		
Maids and related housekeeping service workers NEC	10,027	42.48
Working proprietors (catering and lodging services)	7,068	29.94
Cooks, waiters, bartenders and related workers	4,002	16.95
Agricultural workers		
Agricultural and animal husbandry workers	37,450	92.52
Planters and farmers	1,110	2.74
Forestry workers	1,004	2.48
Production workers		
Tailors, dressmakers, sewer, upholsterers and related workers	6,466	39.21
Food and beverage processors	5,299	32.14
Spinners, weavers, knitters, dyers, and related workers	2,201	13.35

Notes: Occupation categories are indicated in bold. Top-3 labeled 2-digit occupation codes within each category are listed. Number of female-year observations and percentage share within each category are indicated on the second and third columns.

Table 9. Effect of preschool availability on female's industry

	(1) Agriculture	(2) Mining	(3) Manufacturing	(4) Utility	(5) Construction	(6) Trade	(7) Logistic	(8) Finance	(9) Social
Panel A: Public									
Public * Eligible	0.013	0.001**	0.006	-0.001**	0.001	-0.004	0.000	-0.000	0.012**
	(0.015)	(0.000)	(0.007)	(0.000)	(0.001)	(0.009)	(0.001)	(0.001)	(0.006)
Public Preschools	0.031*	-0.001	-0.021**	0.000	0.001	-0.007	-0.001**	-0.002*	-0.013*
	(0.018)	(0.001)	(0.010)	(0.000)	(0.001)	(0.009)	(0.001)	(0.001)	(0.007)
Eligible Child	0.005	-0.001*	-0.008***	0.000	-0.001	-0.005**	-0.000	-0.000	-0.005**
	(0.004)	(0.000)	(0.002)	(0.000)	(0.001)	(0.003)	(0.000)	(0.001)	(0.002)
FDR q-value	0.754	0.323	0.754	0.149	0.754	0.754	0.754	0.754	0.323
Panel B: Private									
Private * Eligible	-0.001	0.000	0.001*	0.000	0.000	-0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Private Preschools	0.002	-0.000	-0.002*	-0.000	-0.000	0.001	-0.000**	-0.000	-0.001
	(0.002)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Eligible Child	0.010	-0.001*	-0.012***	-0.000	-0.001	-0.005	-0.000	-0.001	-0.005*
	(0.007)	(0.000)	(0.003)	(0.000)	(0.001)	(0.005)	(0.000)	(0.001)	(0.003)
FDR q-value	0.872	0.872	0.747	0.872	0.872	0.872	0.872	0.872	0.872
Observations	224,925	224,925	224,925	224,925	224,925	224,925	224,925	224,925	224,925
Mean	0.098	0.001	0.049	0.000	0.002	0.098	0.001	0.003	0.064

Notes: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each column regresses the dummy for being employed in a certain industry category indicated in column heading on the density of public (Panel A) or private (Panel B) preschools, defined as the number of preschools divided by the population of children aged 3-6 in each district, a dummy for having a preschool-aged eligible child, and their interaction. Comparison group includes all mothers of children of the wrong ages and non-mothers. All regressions include mother's age fixed effect, urban dummy, district, year, and individual fixed effects. Robust standard errors clustered at district level. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR q-values for the interaction coefficient of preschool density and eligible child are computed over all 9 outcomes and are shown in the last row of each panel. FDR q-values indicate the probability of false positives among *significant* tests. Number of female-year observations and means of dependent variables are indicated in the last two rows. See Table 8 for a list of top-3 occupations per category.

Table 10. List of top-3 occupations within each industry category

Industry categories	N	percent (within category)
Agriculture		
Agricultural and animal husbandry workers	19,882	87.16
Planters and farmers	525	2.30
Salesmen, shop assistants and related workers	380	1.67
Mining		
Agricultural and animal husbandry workers	207	30.94
Salesmen, shop assistants and related workers	101	15.10
Miners, quarrymen, well drillers and related workers	81	12.11
Manufacturing		
Tailors, dressmakers, sewer, upholsterers and related workers	2,354	20.19
Food and beverage processors	1,812	15.54
Spinners, weavers, knitters, dyers, and related workers	1,078	9.25
Utility		
Agricultural and animal husbandry workers	191	32.05
Salesmen, shop assistants and related workers	94	15.77
Bookkeepers, cashiers, and related workers	44	7.38
Construction		
Bricklayers, carpenters and other construction workers	241	25.56
Agricultural and animal husbandry workers	192	20.36
Salesmen, shop assistants and related workers	92	9.76
Trade		
Salesmen, shop assistants and related workers	12,766	56.33
Working proprietors (catering and lodging services)	3,168	13.98
Working proprietors (wholesale and retail trade)	2,080	9.18
Logistics		
Agricultural and animal husbandry workers	196	26.52
Salesmen, shop assistants and related workers	98	13.26
Bookkeepers, cashiers, and related workers	48	6.50
Finance		
Agricultural and animal husbandry workers	206	17.88
Insurance, real estate, securities and business services salesman and auctioneers	143	12.41
Salesmen, shop assistants and related workers	107	9.29
Social		
Teachers	4,201	27.80
Maids and related housekeeping service workers NEC	3,499	23.16
Nurses, midwives, x-ray technicians, traditional medicine	693	4.59

Notes: Industry categories are indicated in bold. Top-3 labeled 2-digit occupation codes within each category are listed. Number of female-year observations and percentage share within each category are indicated on the second and third columns.

Table 11. Complementarity, substitutability, and equality of public and private preschools

	Work Participation			
	(1)	(2)	(3)	(4)
Public * Private * Eligible				-0.003*
				(0.001)
Public * Private				0.000
				(0.001)
Public * Eligible	0.074***		0.071***	0.092***
	(0.016)		(0.017)	(0.022)
Public Preschools	-0.015		-0.013	-0.014
	(0.012)		(0.013)	(0.018)
Private * Eligible		0.002	0.001	0.002
		(0.001)	(0.001)	(0.002)
Private Preschools		-0.001	-0.001	-0.001
		(0.002)	(0.002)	(0.002)
Eligible Child	-0.034***	-0.032***	-0.040***	-0.043***
	(0.005)	(0.008)	(0.008)	(0.008)
Public = Private (p-value)			0.000	0.000

Notes: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each column regresses work participation on the density of public and private (Panel B) preschools, defined as the number of preschools divided by the population of children aged 3-6 in each district, a dummy for having a preschool-aged eligible child, and the respective double and triple interactions. Comparison group includes all mothers of children of the wrong ages and non-mothers. All regressions include mother's age fixed effect, urban dummy, district, year, and individual fixed effects. Robust standard errors clustered at district level. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. The last row tests the equality of 'Public * Eligible' and 'Private * Eligible' coefficients and reports the associated p-value. There are 226,400 female-year observations.

Table 12. Effect of preschool availability on female's work participation by birth order

	Work Participation					
	(1)	(2)	(3)	(4)	(5)	(6)
Child's birth order:	First	Second	Third	Fourth	Fifth	Sixth
Panel A: Public						
Public * Eligible	0.075***	0.090***	0.058**	0.026	-0.023	-0.052
	(0.020)	(0.024)	(0.026)	(0.036)	(0.043)	(0.071)
Public Preschools	-0.003	-0.010	-0.010	-0.029	-0.019	-0.054
	(0.012)	(0.014)	(0.014)	(0.022)	(0.032)	(0.044)
Eligible Child	-0.046***	-0.023***	-0.007	0.011	0.005	0.014
	(0.006)	(0.006)	(0.007)	(0.010)	(0.012)	(0.018)
FDR q-value	0.001	0.001	0.095	0.587	0.587	0.587
Panel B: Private						
Private * Eligible	0.002	0.004**	-0.002	-0.002	-0.005	-0.004
	(0.002)	(0.002)	(0.002)	(0.003)	(0.005)	(0.007)
Private Preschools	-0.001	-0.001	-0.001	-0.000	0.004	0.006
	(0.002)	(0.002)	(0.002)	(0.003)	(0.005)	(0.006)
Eligible Child	-0.046***	-0.026***	0.010	0.022	0.017	0.019
	(0.009)	(0.009)	(0.011)	(0.015)	(0.019)	(0.025)
FDR q-value	0.548	0.135	0.581	0.581	0.581	0.581
Observations	226,400	187,376	110,371	57,258	28,857	15,460
Mean	0.520	0.526	0.538	0.550	0.555	0.558

Notes: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each column regresses the work participation on a dummy that first child (or second, third, and so on) is preschool-aged eligible, the density of public (Panel A) or private (Panel B) preschools, defined as the number of preschools divided by the population of children aged 3-6 in each district, and their interaction. Comparison group includes all mothers of children of the wrong ages and non-mothers. All regressions include mother's age fixed effect, urban dummy, district, year, and individual fixed effects. Robust standard errors clustered at district level. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR q-values for the interaction coefficient of preschool density and eligible child are computed over all 5 outcomes and are shown in the last row of each panel. FDR q-values indicate the probability of false positives among *significant* tests. Number of female-year observations and means of dependent variables are indicated in the last two rows. Number of observations fall with higher child's birth order because sample is further restricted to mothers with at least as many children as the indicated birth order.

Table 13. Heterogeneous effects of preschool availability on female's work participation by average preschool enrollment in district of residence

	Work participation					
	DDD			DDD-FE		
Enrollment:	< 20 percent	20-40 percent	> 40 percent	< 20 percent	20-40 percent	> 40 percent
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Public						
Preschool *	0.117***	0.056*	-0.064	0.125***	0.015	-0.141
Eligible	(0.028)	(0.031)	(0.189)	(0.034)	(0.049)	(0.308)
Preschool	-0.031	0.033	0.026	-0.040	0.050	0.049
density	(0.028)	(0.043)	(0.089)	(0.036)	(0.053)	(0.120)
Eligible child	-0.061***	-0.080***	-0.105***	-0.038***	-0.042*	0.072
	(0.010)	(0.017)	(0.034)	(0.013)	(0.021)	(0.075)
Panel B: Private						
Preschool *	-0.001	0.004	0.029***	-0.000	0.004	0.038
Eligible	(0.003)	(0.004)	(0.008)	(0.006)	(0.007)	(0.023)
Preschool	-0.000	0.006	-0.005	-0.000	0.008	-0.010
density	(0.004)	(0.005)	(0.011)	(0.005)	(0.007)	(0.013)
Eligible child	-0.034**	-0.098***	-0.333***	-0.013	-0.067	-0.245
	(0.015)	(0.035)	(0.069)	(0.022)	(0.052)	(0.175)
Observations	23,076	11,982	2,489	23,076	11,982	2,489
Mean	0.583	0.639	0.583	0.583	0.639	0.583

Note: Sample includes all mothers with and without any preschool-aged eligible child (age 3-6). Comparison group includes non-mothers and mothers of children with the wrong ages. Sample is restricted to PODES years with preschool enrollment data (2003, 2005, 2011, 2014). Preschool enrollment is defined among children aged 3-6. Median preschool enrollment rate at the district-level is 15.6 percent and the 99th percentile at 49.8 percent. Median preschool enrollment rate at the individual-level is 17.2 percent. All columns regress work participation on preschool density, preschool eligibility dummy, and their interaction. All regressions include district, year, mother's age fixed effects and an urban residence dummy. DDD excludes while DDD-FE includes individual fixed effects. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. Number of observations and work participation means for each column are indicated at the last two rows.

Table 14. Heterogeneous effects of preschool availability on female's work participation by educational attainment

	Work participation							
	DDD				DDD-FE			
Education:	< Primary	Primary	Lower Secondary	> Upper Secondary	< Primary	Primary	Lower Secondary	> Upper Secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Public								
Num. kindergartens	0.059**	0.063**	0.018	0.054*	0.053**	0.083***	0.027	0.094***
* has kid 3-6	(0.029)	(0.025)	(0.034)	(0.029)	(0.027)	(0.028)	(0.035)	(0.024)
Public Preschools	-0.035*	-0.027	0.031	0.007	-0.036**	-0.016	0.030	0.002
	(0.018)	(0.021)	(0.032)	(0.018)	(0.017)	(0.019)	(0.034)	(0.016)
Has kid age 3-6	-0.030***	-0.044***	-0.029**	-0.069***	-0.002	-0.025***	-0.018*	-0.053***
	(0.009)	(0.010)	(0.013)	(0.009)	(0.007)	(0.008)	(0.010)	(0.008)
Panel B: Private								
Num. kindergartens	-0.006*	-0.003	0.000	0.007***	-0.002	-0.000	0.002	0.005***
* has kid 3-6	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Private Preschools	-0.002	-0.004	-0.002	0.000	-0.001	-0.002	-0.000	0.001
	(0.004)	(0.003)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)
Has kid age 3-6	-0.001	-0.018	-0.026	-0.095***	0.012	-0.010	-0.022	-0.066***
	(0.014)	(0.016)	(0.019)	(0.014)	(0.012)	(0.013)	(0.015)	(0.012)
Observations	58,976	61,541	35,362	67,034	58,976	61,541	35,362	67,034
Mean	0.613	0.514	0.441	0.494	0.613	0.514	0.441	0.494

Note: Sample includes all mothers with and without any preschool-aged eligible child (age 3-6). Comparison group includes non-mothers and mothers of children with the wrong ages. Each column is restricted to individuals with lifetime educational attainment as indicated in the column heading. All columns regress work participation on preschool density, preschool eligibility dummy, and their interaction. All regressions include district, year, mother's age fixed effects and an urban residence dummy. DDD excludes while DDD-FE includes individual fixed effects. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. Number of observations and work participation means for each column are indicated at the last two rows.

Online Appendix for

Preschool Availability and Female Labor Force Participation:

Evidence from Indonesia

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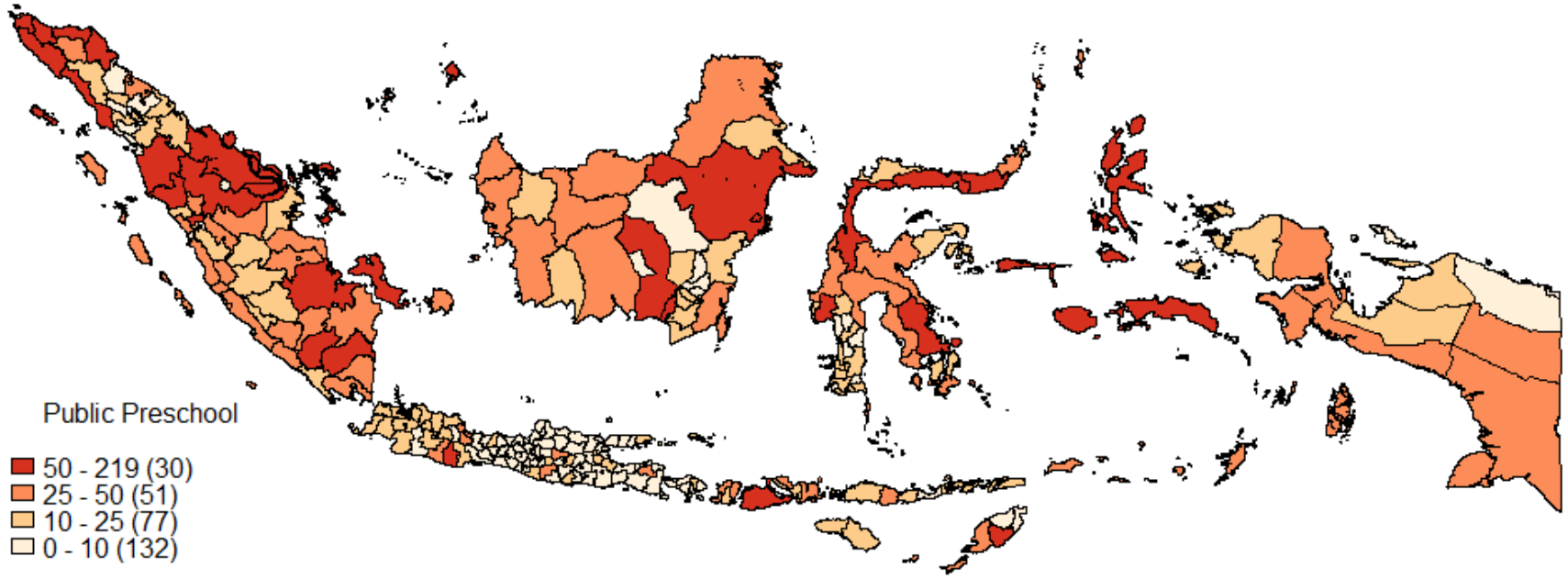
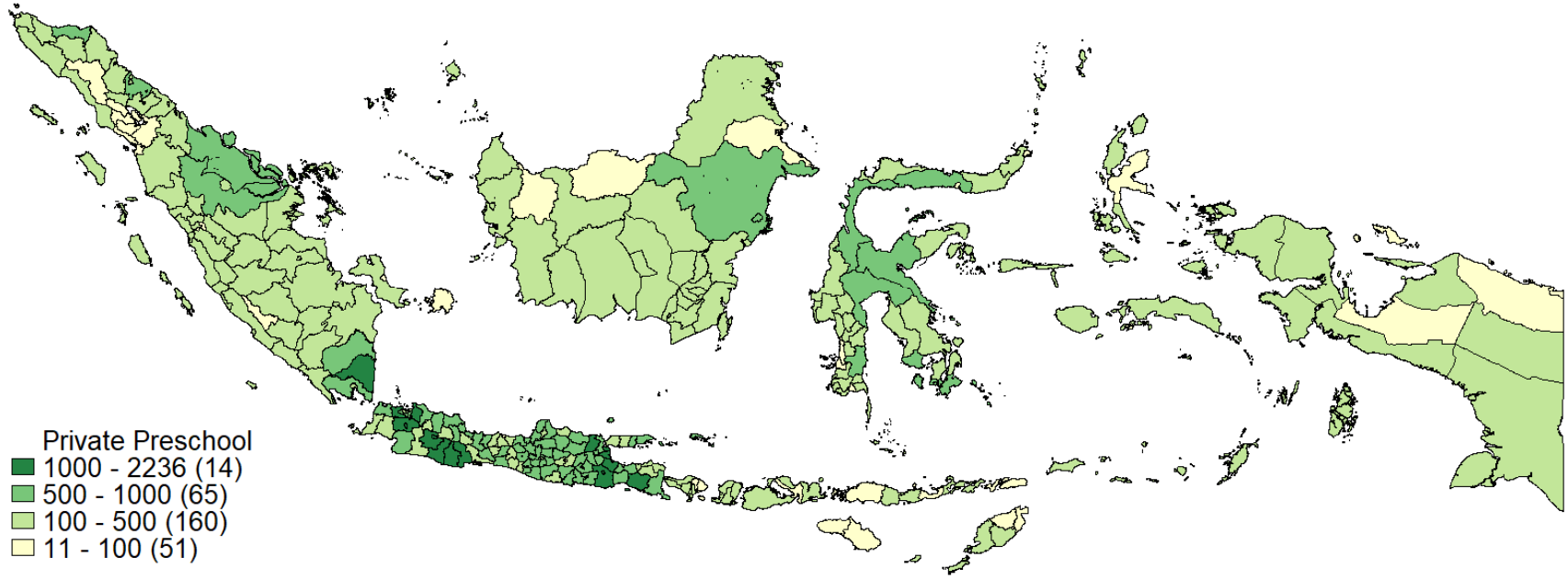
25 June 2019



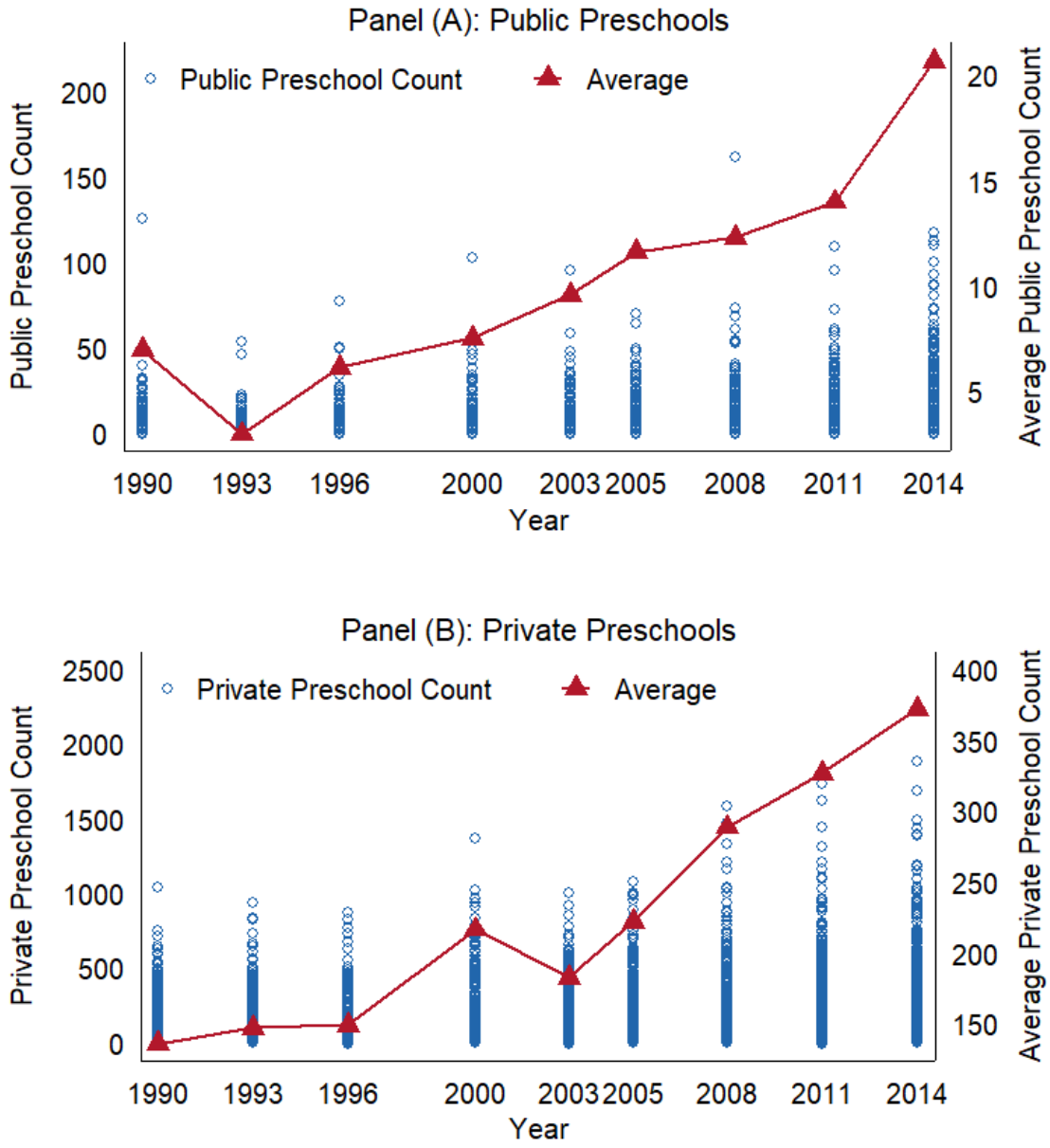
Appendix Figure 1. Spatial distribution of public preschool counts in 2014

Notes: Number of public preschools is obtained from Podes. The legend indicates the range and distribution of public preschool counts across the Indonesian archipelago. The numbers in parentheses refer to the number of districts that fall in that range. The total number of districts, 290, reflects their existence in 1993. Districts often split over time; by March 2016, there were 511 districts. In our analyses, we maintain the 1993 district boundaries to allow comparisons over time.



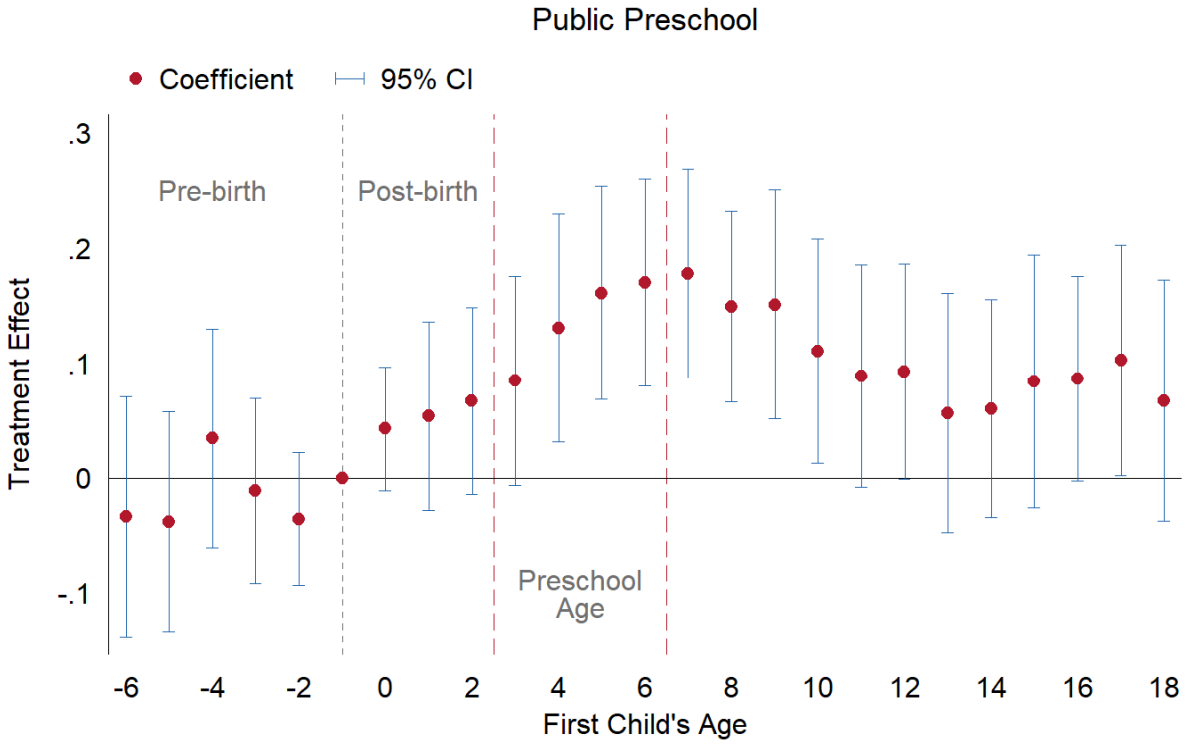
Appendix Figure 2. Spatial distribution of private preschool counts in 2014

Notes: Number of private preschools is obtained from Podes. The legend indicates the range and distribution of private preschool counts across the Indonesian archipelago. The numbers in parentheses refer to the number of districts that fall in that range. The total number of districts, 290, reflects their existence in 1993. Districts often split over time; by March 2016, there were 511 districts. In our analyses, we maintain the 1993 district boundaries to allow comparisons over time.



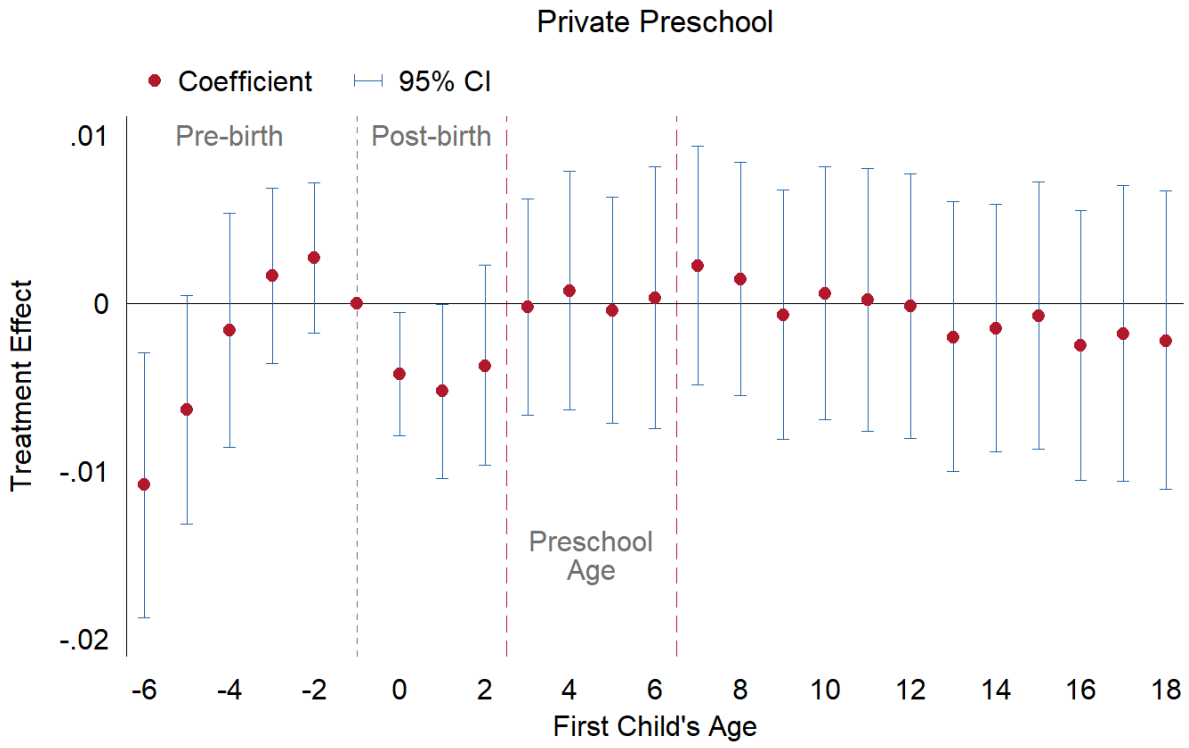
Appendix Figure 3. Count of public and private preschools across districts over time

Note: Number of preschools is obtained from Podes in respective years. Panel A and B indicate count of public and private preschools, respectively. Scatterplot of preschool densities across 290 districts, as they existed in 1993, over time are shown in blue hollow circles. Red triangles indicate the average density of preschools across 290 districts per year. Districts often split over time; by 2014, there were 511 districts. In our analyses, we maintain the 1993 district boundaries to allow comparisons over time.



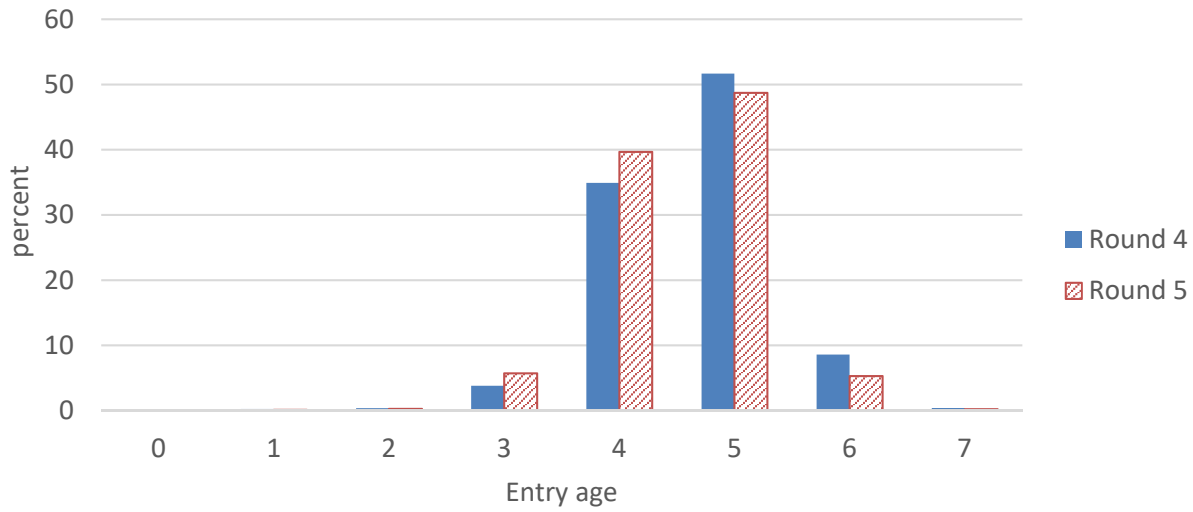
Appendix Figure 4. Event study on the effect of public preschools on mothers' work participation by first child's age relative to pre-birth level

Note: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each dot represents the interaction coefficient of public preschool density in one's district of residence and first child's age. Mother's work participations are averaged at the tails; 6 and more years prior to the first childbirth and when the first child was 18 and older. Treatment effects are interpreted relative to the omitted year prior to the mother's first childbirth. Blue spikes represent 95 percent confidence intervals.



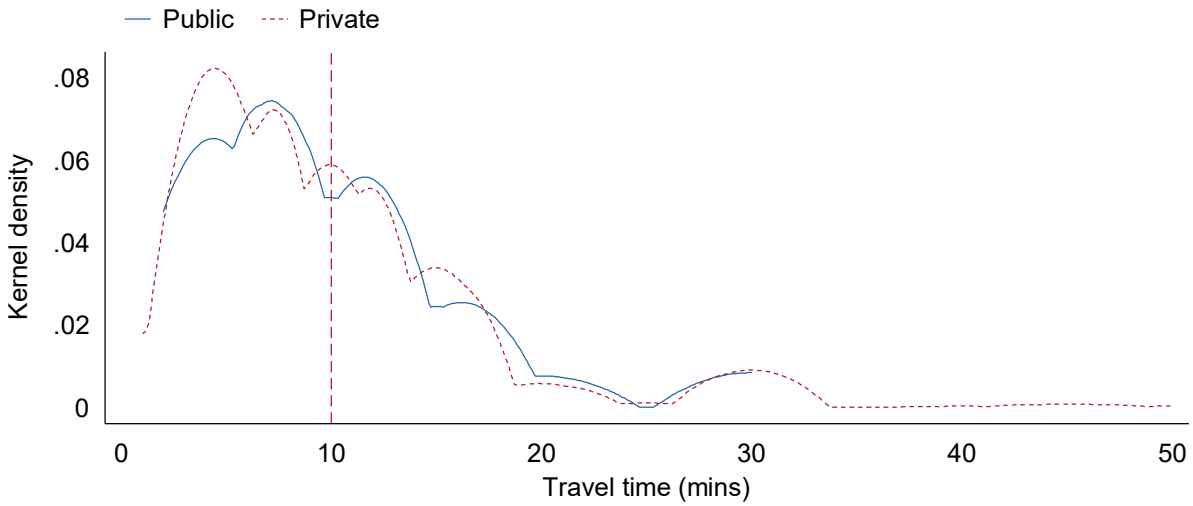
Appendix Figure 5. Event study on the effect of private preschools on mothers' work participation by first child's age relative to pre-birth level

Note: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each dot represents the interaction coefficient of private preschool density in one's district of residence and first child's age. Mother's work participations are averaged at the tails; 6 and more years prior to the first childbirth and when the first child was 18 and older. Treatment effects are interpreted relative to the omitted year prior to the mother's first childbirth. Blue spikes represent 95 percent confidence intervals.



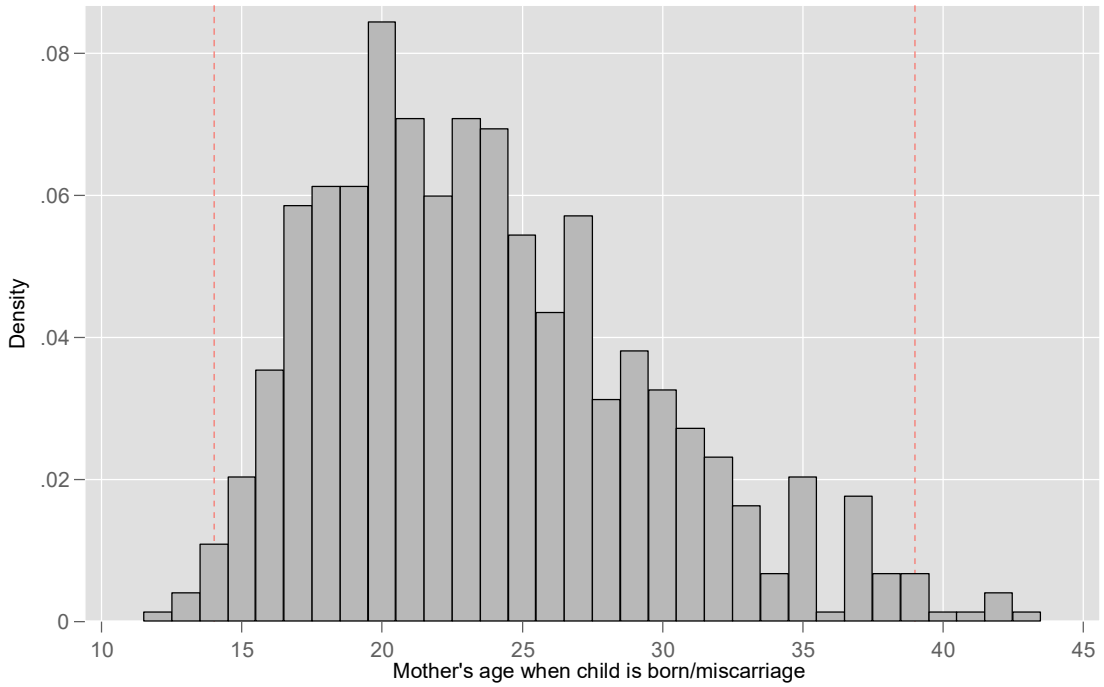
Appendix Figure 6. Distribution of age of entry into preschool

Notes: The graph plots the distribution of first age of entry into preschool obtained from child's education module in IFLS 4 (2007/08) and 5 (2014/15). The same information is not collected in previous IFLS rounds. Blue solid bar and patterned orange bar indicate the percent distribution for round 4 and 5, respectively.



Appendix Figure 7. Distribution of one-way travel time to preschool

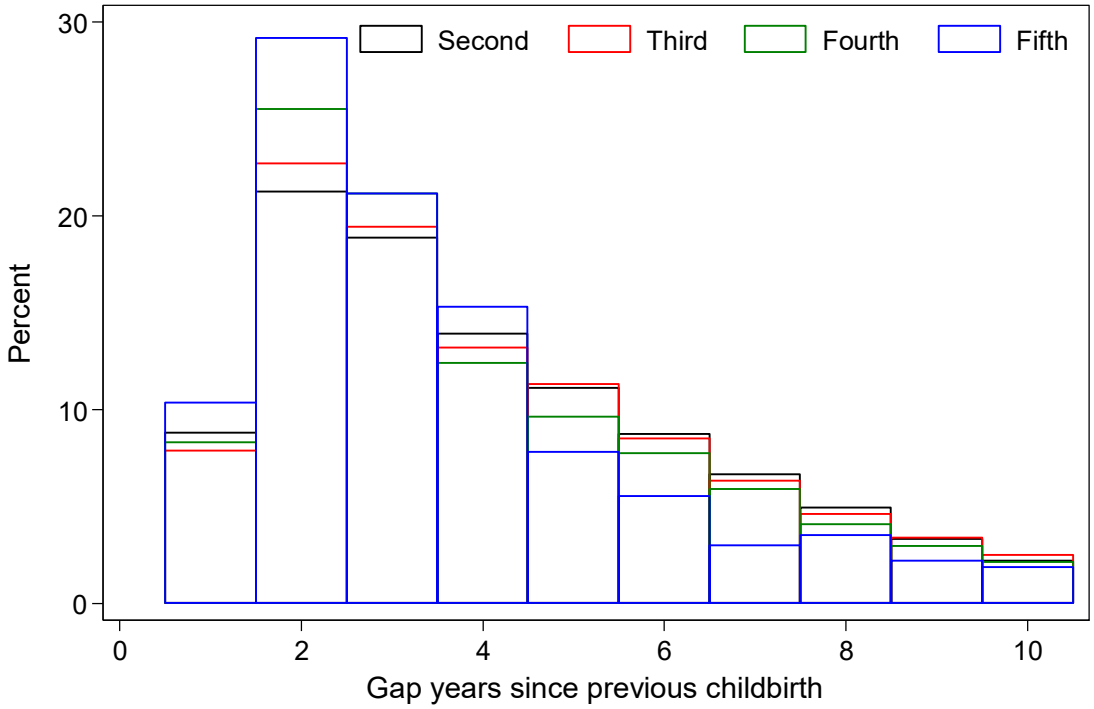
Notes: The graph plots kernel density of one-way travel time to public/private preschools conditional on preschool enrollment. Dashed vertical line indicates median travel time, 10 minutes, in both public and private preschools. Solid blue line and short-dashed red line indicates the distribution of travel time to public and private preschools, respectively.



Source: IFLS 1 (1993). Note: dashed lines indicate 1st and 99th percentiles.

Appendix Figure 8. Distribution of mother's age at childbirth or when miscarriage occurs

Note: Data is taken from IFLS 1 (1993). It shows the distribution of mother's ages at childbirth or when miscarriage occurs. Pregnancies pertain to any child order. Dashed lines indicate 1st and 99th percentiles at age 14 and 39, respectively. To avoid complications in teen pregnancies we limit our sample to mothers who were at least 19 in at least two rounds of IFLS. The upper boundary, age 45, was selected such that if the age-eligible kid is 6, then the woman has to be 39 or younger at childbirth.



Appendix Figure 9. Distribution of gap years between childbirths

Note: The graph plots histogram distributions of gap years between childbirths by birth orders. Second birth order, in black, indicates the gap years between the first and second childbirths. Subsequent birth orders are defined similarly. Y-axis shows the percent of mothers with the indicated gap years. Gap years are defined pertaining to the children's birth years.

Appendix Table 1. List of top-3 occupations within each work status

Work status	N	percent (within category)
Self-employed		
Salesmen, shop assistants and related workers	14,056	35.97
Agricultural and animal husbandry workers	6,674	17.08
Working proprietors (catering and lodging services)	3,640	9.32
Government workers		
Teachers	4,180	48.91
Nurses, midwives, x-ray technicians, traditional medicine	712	8.33
Government executive of officials	679	7.95
Private workers		
Agricultural and animal husbandry workers	8,162	19.40
Maids and related housekeeping service workers NEC	5,116	12.16
Salesmen, shop assistants and related workers	4,085	9.71
Unpaid family workers		
Agricultural and animal husbandry workers	19,247	67.28
Salesmen, shop assistants and related workers	3,690	12.90
Food and beverage processors	885	3.09

Notes: Work status are indicated in bold. Top-3 labeled 2-digit occupation codes within each category are listed. Number of female-year observations and percentage share within each category are indicated on the second and third columns.

Appendix Table 2. Effect of preschool availability on female's work participation with inferred in-between preschool data

Econometric strategy:	Work participation					
	DD		DDD		DDD-FE	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Public						
Preschool * Eligible			0.057***	0.057***	0.074***	0.071***
			(0.017)	(0.017)	(0.016)	(0.016)
Preschool density	0.002	0.030	-0.016	0.001	-0.015	-0.008
	(0.023)	(0.025)	(0.013)	(0.016)	(0.012)	(0.016)
Eligible child			-0.051***	-0.052***	-0.034***	-0.034***
			(0.007)	(0.007)	(0.005)	(0.005)
Panel B: Private						
Preschool * Eligible			0.001	0.001	0.002	0.002
			(0.002)	(0.002)	(0.001)	(0.001)
Preschool density	-0.007**	-0.003	-0.003*	-0.003*	-0.001	-0.004**
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Eligible child			-0.046***	-0.048***	-0.032***	-0.032***
			(0.011)	(0.010)	(0.008)	(0.008)
Observations	67,431	67,431	226,400	226,400	226,400	226,400
Mean	0.529	0.529	0.520	0.520	0.520	0.520
District Trend		X		X		X

Note: Sample in column 1-2 is restricted to mothers with preschool-aged children (age 3-6). Sample in column 3-6 includes mothers with and without preschool-aged children (age 3-6). We infer preschool data in-between PODES years. Column 1-2 regress work participation on preschool density in a difference-in-differences (DD) strategy (Section 4.1). Column 3-4 regress work participation on preschool density, a dummy for having a preschool-aged child, and the interaction between the two in a difference-in-difference-in-differences (DDD) strategy (Section 4.2). Column 5-6 builds on the DDD strategy and adds individual fixed effect (DDD-FE) to allow comparison within-mothers (Section 4.3). Column 2, 4, 6 add district-specific trends. All regressions include district, year, mother's age fixed effects and an urban residence dummy. Panel A and B look at the effect of public and private preschool densities separately. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. Observations, work participation means, and inclusion of district trends are indicated in the last three rows.

Appendix Table 3. Effect of preschool availability on female's work participation with continuous number of eligible children

Econometric strategy:	Work participation					
	DD		DDD		DDD-FE	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Public						
Preschool * Eligible			0.045***	0.047***	0.065***	0.064***
			(0.016)	(0.016)	(0.017)	(0.017)
Preschool density	-0.028	-0.014	-0.012	0.004	-0.015	-0.006
	(0.023)	(0.027)	(0.014)	(0.017)	(0.014)	(0.017)
Eligible child			-0.037***	-0.038***	-0.024***	-0.024***
			(0.006)	(0.006)	(0.005)	(0.005)
Panel B: Private						
Preschool * Eligible			-0.001	-0.001	-0.000	-0.000
			(0.002)	(0.002)	(0.001)	(0.001)
Preschool density	-0.011***	-0.009***	-0.002	-0.003	-0.001	-0.003**
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Eligible child			-0.026***	-0.027***	-0.014*	-0.013*
			(0.009)	(0.009)	(0.008)	(0.008)
Observations	22,737	22,737	76,951	76,951	76,951	76,951
Mean	0.538	0.538	0.534	0.534	0.534	0.534
District Trend		X		X		X

Note: Sample in column 1-2 is restricted to mothers with preschool-aged children (age 3-6). Sample in column 3-6 includes mothers with and without preschool-aged children (age 3-6). Sample is further restricted to PODES years. Column 1-2 regress work participation on preschool density in a difference-in-differences (DD) strategy (Section 4.1). Column 3-4 regress work participation on preschool density, a dummy for having a preschool-aged child, and the interaction between the two in a difference-in-difference-in-differences (DDD) strategy (Section 4.2). Column 5-6 builds on the DDD strategy and adds individual fixed effect (DDD-FE) to allow comparison within-mothers (Section 4.3). Column 2, 4, 6 add district-specific trends. All regressions include district, year, mother's age fixed effects and an urban residence dummy. Panel A and B look at the effect of public and private preschool densities separately. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. Observations, work participation means, and inclusion of district trends are indicated in the last three rows.

Appendix Table 4. Effect of preschool availability on female's work participation with preschool ages defined as 3-5

Econometric strategy:	Work participation					
	DD		DDD		DDD-FE	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Public						
Preschool * Eligible			0.047***	0.047***	0.066***	0.063***
			(0.017)	(0.017)	(0.016)	(0.016)
Preschool density	-0.008	0.020	-0.010	0.007	-0.009	-0.002
	(0.024)	(0.027)	(0.013)	(0.016)	(0.012)	(0.015)
Eligible child			-0.046***	-0.047***	-0.031***	-0.031***
			(0.006)	(0.006)	(0.005)	(0.005)
Panel B: Private						
Preschool * Eligible			0.001	0.001	0.002	0.002
			(0.002)	(0.002)	(0.001)	(0.001)
Preschool density	-0.007**	-0.004	-0.003*	-0.003*	-0.001	-0.004**
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Eligible child			-0.041***	-0.042***	-0.029***	-0.029***
			(0.010)	(0.010)	(0.008)	(0.008)
Observations	22,737	22,737	76,951	76,951	76,951	76,951
Mean	0.538	0.538	0.534	0.534	0.534	0.534
District Trend		X		X		X

Note: Sample in column 1-2 is restricted to mothers with preschool-aged children (age 3-6). Sample in column 3-6 includes mothers with and without preschool-aged children (age 3-6). Sample is further restricted to PODES years. Column 1-2 regress work participation on preschool density in a difference-in-differences (DD) strategy (Section 4.1). Column 3-4 regress work participation on preschool density, a dummy for having a preschool-aged child, and the interaction between the two in a difference-in-difference-in-differences (DDD) strategy (Section 4.2). Column 5-6 builds on the DDD strategy and adds individual fixed effect (DDD-FE) to allow comparison within-mothers (Section 4.3). Column 2, 4, 6 add district-specific trends. All regressions include district, year, mother's age fixed effects and an urban residence dummy. Panel A and B look at the effect of public and private preschool densities separately. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. Observations, work participation means, and inclusion of district trends are indicated in the last three rows.

Appendix Table 5. Effect of preschool availability on female's earnings and work hours

	(1) Salary	(2) Net profit	(3) Income	(4) Work hours
Panel A: Public				
Public * Eligible	0.001	0.199	0.040	0.001
	(0.120)	(0.232)	(0.126)	(0.034)
Public Preschools	0.046	-0.087	-0.017	0.007
	(0.079)	(0.169)	(0.078)	(0.038)
Eligible Child	0.029	-0.017	0.005	-0.029**
	(0.029)	(0.044)	(0.030)	(0.013)
FDR q-value	0.993	0.993	0.993	0.993
Panel B: Private				
Private * Eligible	-0.000	-0.002	-0.008	-0.005
	(0.008)	(0.011)	(0.008)	(0.003)
Private Preschools	-0.014	-0.018	-0.016*	-0.000
	(0.012)	(0.017)	(0.009)	(0.004)
Eligible Child	0.029	0.014	0.042	-0.008
	(0.043)	(0.062)	(0.040)	(0.018)
FDR q-value	0.987	0.987	0.987	0.434
Observations	20,193	17,804	37,523	48,537
Mean	442.201	467.009	457.837	37.890

Notes: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each column regresses the dependent variable indicated in column heading on the density of public (Panel A) or private (Panel B) preschools, defined as the number of preschools divided by the population of children aged 3-6 in each district, a dummy for having a preschool-aged eligible child, and their interaction. All regressions include mother's age fixed effect, urban dummy, district, year, and individual fixed effects. Salary, net profit, income are per month and adjusted for inflation using national consumer price index (CPI) with 2010 base year obtained from FRED. Income is defined as the sum of salary and net profit. Work hours are per week. We apply log transformation to all dependent variables so that estimates can be interpreted as percentage changes; zero values are imputed with log(0.1). Robust standard errors clustered at district level. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR q-values for the interaction coefficient of preschool density and eligible child are computed over all 4 outcomes and are shown in the last row of each panel. FDR q-values indicate the probability of false positives among *significant* tests. Number of female-year observations and means of dependent variables are indicated in the last two rows. Means are reported in nominal terms. Salary, net profit, and income are reported in IDR 10,000 increments and are adjusted for inflation using national CPI with 2010 base year (FRED). The exchange rate in 2010 was 1 USD for 9,090 IDR (FRED). Earnings and work hours are asked pertaining to survey years and historical employment in round 1-3 but are limited to survey years in round 4-5.

Appendix Table 6. Effect of preschool availability on female's main activity

	(1) Working	(2) Job searching	(3) Attending school	(4) Housekeeping
Panel A: Public				
Public * Eligible	0.017	0.002	-0.002	-0.003
	(0.027)	(0.003)	(0.008)	(0.028)
Public Preschools	0.015	0.002	0.014**	-0.034
	(0.030)	(0.003)	(0.006)	(0.031)
Eligible Child	-0.035***	-0.006***	-0.001	0.050***
	(0.009)	(0.001)	(0.002)	(0.009)
FDR q-value	0.912	0.912	0.912	0.912
Panel B: Private				
Private * Eligible	0.001	0.000	-0.002***	0.001
	(0.002)	(0.000)	(0.001)	(0.002)
Private Preschools	-0.002	-0.000	0.005***	-0.002
	(0.003)	(0.000)	(0.001)	(0.003)
Eligible Child	-0.037**	-0.007***	0.010***	0.045***
	(0.014)	(0.002)	(0.004)	(0.014)
FDR q-value	0.693	0.693	0.002	0.693
Observations	36,287	37,223	37,223	37,223
Mean	0.446	0.008	0.038	0.491

Notes: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each column regresses the dummy for having main activity as indicated in column heading on the density of public (Panel A) or private (Panel B) preschools, defined as the number of preschools divided by the population of children aged 3-6 in each district, a dummy for having a preschool-aged eligible child, and their interaction. All regressions include mother's age fixed effect, urban dummy, district, year, and individual fixed effects. Robust standard errors clustered at district level. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR q-values for the interaction coefficient of preschool density and eligible child are computed over all 4 outcomes and are shown in the last row of each panel. FDR q-values indicate the probability of false positives among *significant* tests. Number of female-year observations and means of dependent variables are indicated in the last two rows. Main activities are only asked pertaining to survey years and not to historical employment.

Appendix Table 7. Effect of preschool availability on female's occupation conditional on employment

	(1) Professional	(2) Manager	(3) Clerk	(4) Sales	(5) Service	(6) Agricultural	(7) Production
Panel A: Public							
Public * Eligible	-0.003	-0.002	0.004	-0.023**	0.009	-0.007	0.006
	(0.007)	(0.002)	(0.006)	(0.010)	(0.010)	(0.010)	(0.007)
Public Preschools	0.001	0.001	-0.005	0.002	-0.004	0.001	0.001
	(0.005)	(0.001)	(0.004)	(0.011)	(0.009)	(0.010)	(0.009)
Eligible Child	-0.001	0.001	-0.009***	0.006	-0.004	0.021***	-0.004
	(0.002)	(0.001)	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)
FDR q-value	0.600	0.600	0.600	0.141	0.600	0.600	0.600
Panel B: Private							
Private * Eligible	0.000	0.000	-0.000	-0.001	-0.001	-0.000	0.002**
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Private Preschools	0.002***	0.000	0.001	0.004***	-0.003**	-0.003***	-0.000
	(0.001)	(0.000)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Eligible Child	-0.003	-0.001	-0.007***	0.009	-0.000	0.020***	-0.011**
	(0.003)	(0.001)	(0.003)	(0.006)	(0.005)	(0.006)	(0.005)
FDR q-value	0.841	0.841	0.841	0.841	0.841	0.841	0.298
Observations	125,047	125,047	125,047	125,047	125,047	125,047	125,047
Mean	0.075	0.003	0.044	0.223	0.150	0.319	0.113

Notes: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each column regresses the dummy for being employed in a certain occupation category indicated in column heading, conditional on being employed, on the density of public (Panel A) or private (Panel B) preschools, defined as the number of preschools divided by the population of children aged 3-6 in each district, a dummy for having a preschool-aged eligible child, and their interaction. Comparison group includes all mothers of children of the wrong ages and non-mothers. All regressions include mother's age fixed effect, urban dummy, district, year, and individual fixed effects. Robust standard errors clustered at district level. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR q-values for the interaction coefficient of preschool density and eligible child are computed over all 7 outcomes and are shown in the last row of each panel. FDR q-values indicate the probability of false positives among *significant* tests. Number of female-year observations and means of dependent variables are indicated in the last two rows. See Table 8 for a list of top-3 occupations per category.

Appendix Table 8. Effect of preschool availability on female's industry conditional on employment

	(1) Agriculture	(2) Mining	(3) Manufacturing	(4) Utility	(5) Construction	(6) Trade	(7) Logistic	(8) Finance	(9) Social
Panel A: Public									
Public * Eligible	-0.023	0.000	0.008	-0.001**	0.001	-0.014	0.002	0.001	0.014
	(0.020)	(0.001)	(0.010)	(0.001)	(0.002)	(0.015)	(0.001)	(0.002)	(0.010)
Public Preschools	0.039	-0.002*	-0.030*	0.001	0.001	-0.001	-0.001*	-0.003	-0.008
	(0.026)	(0.001)	(0.016)	(0.001)	(0.002)	(0.015)	(0.001)	(0.002)	(0.013)
Eligible Child	0.013***	-0.001*	-0.006*	-0.000	-0.001	0.001	-0.001	-0.001	0.002
	(0.005)	(0.000)	(0.003)	(0.000)	(0.001)	(0.004)	(0.001)	(0.001)	(0.004)
FDR q-value	0.764	0.764	0.764	0.311	0.764	0.764	0.764	0.764	0.764
Panel B: Private									
Private * Eligible	-0.002	0.000	0.001	0.000	0.000	-0.000	0.000	0.000	0.001
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Private Preschools	-0.004*	-0.000	-0.001	-0.000	-0.000	0.004**	-0.000**	-0.000	0.001
	(0.003)	(0.000)	(0.002)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.002)
Eligible Child	0.016*	-0.001*	-0.007	-0.000	-0.001	0.001	-0.001	-0.001	-0.001
	(0.008)	(0.001)	(0.005)	(0.000)	(0.001)	(0.006)	(0.001)	(0.001)	(0.005)
FDR q-value	0.738	0.738	0.738	0.738	0.738	0.738	0.738	0.738	0.738
Observations	121,718	121,718	121,718	121,718	121,718	121,718	121,718	121,718	121,718
Mean	0.194	0.001	0.095	0.001	0.004	0.188	0.002	0.005	0.124

Notes: Sample is restricted to females aged 19-45 in at least two IFLS rounds. We infer preschool data in-between PODES years. Each column regresses the dummy for being employed in a certain industry category indicated in column heading, conditional on being employed, on the density of public (Panel A) or private (Panel B) preschools, defined as the number of preschools divided by the population of children aged 3-6 in each district, a dummy for having a preschool-aged eligible child, and their interaction. Comparison group includes all mothers of children of the wrong ages and non-mothers. All regressions include mother's age fixed effect, urban dummy, district, year, and individual fixed effects. Robust standard errors clustered at district level. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR q-values for the interaction coefficient of preschool density and eligible child are computed over all 9 outcomes and are shown in the last row of each panel. FDR q-values indicate the probability of false positives among *significant* tests. Number of female-year observations and means of dependent variables are indicated in the last two rows. See Table 10 for a list of top-3 occupations per category.

Appendix Table 9. Evidence for mothers sorting to high preschool access districts

	Net migration of mothers with preschool-aged kids			
	(1)	(2)	(3)	(4)
Net change in private preschool density	0.087			
	(0.055)			
Net change in public preschool density		-0.688		
		(0.476)		
(Lagged) Net change in private preschool density			-0.067	
			(0.055)	
(Lagged) Net change in public preschool density				0.102
				(0.474)
Observations	1,733	1,733	1,732	1,732

Notes: Sample is composed of a panel of districts over PODES survey years. We aggregate the number of preschool-aged eligible mothers in our constructed panel and regress the net migration of eligible mothers between PODES survey years on the net and/or lagged net change in private/public preschool densities. Number of observations indicate the number of district-year observations in the panel of districts.

Appendix Table 10. Annual cost of attending private and public preschools

	Private		Public		Private-Public
	Mean	SD	Mean	SD	
Registration fee	15.47	(38.58)	6.19	(13.53)	9.27***
Other scheduled fees	12.92	(33.79)	4.80	(8.86)	8.12***
Exam fees	0.23	(1.66)	0.01	(0.05)	0.22**
Books/writing supplies	5.57	(11.16)	2.61	(5.24)	2.95***
Uniform and sports supplies	5.54	(10.35)	5.16	(8.35)	0.38
Transportation costs	3.87	(18.50)	0.25	(1.57)	3.63***
Food/housing costs	13.96	(29.43)	9.40	(15.98)	4.57
Special courses	0.93	(13.70)	0.00	(0.00)	0.93
Other school expenses	1.22	(7.14)	0.00	(0.00)	1.22***
Observations	430		76		506

Notes: Sample is obtained from IFLS 3 (2000) conditional on enrollment in indicated public or private preschools. Means are reported in IDR 10,000 increments and are adjusted for inflation using national CPI with 2010 base year (FRED). The exchange rate in 2010 was 1 USD for 9,090 IDR (FRED).

Appendix Table 11. Heterogeneous effect of preschool availability on maternal employment by district's population

	Work participation					
	DDD			DDD-FE		
Population:	< 0.5 mil	< 1 mil	< 1.5 mil	< 0.5 mil	< 1 mil	< 1.5 mil
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Public						
Preschool *	0.026	0.038**	0.041**	0.031*	0.044***	0.054***
Eligible	(0.021)	(0.017)	(0.017)	(0.017)	(0.015)	(0.015)
Preschool	-0.020	-0.012	-0.010	-0.018	-0.013	-0.011
density	(0.016)	(0.014)	(0.013)	(0.013)	(0.013)	(0.012)
Eligible child	-0.040***	-0.038***	-0.044***	-0.025**	-0.024***	-0.030***
	(0.012)	(0.008)	(0.007)	(0.010)	(0.006)	(0.005)
Panel B: Private						
Preschool *	0.002	0.000	0.000	0.003	0.001	0.001
Eligible	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Preschool	-0.001	-0.003	-0.002	-0.000	-0.002	-0.001
density	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Eligible child	-0.041**	-0.032**	-0.038***	-0.028**	-0.022**	-0.026***
	(0.016)	(0.012)	(0.012)	(0.012)	(0.010)	(0.009)
Observations	61,811	134,751	168,739	61,811	134,751	168,739
Number of districts	149	233	263	149	233	263
Mean	0.552	0.548	0.539	0.552	0.548	0.539

Note: Sample includes all mothers with and without any preschool-aged eligible child (age 3-6). Comparison group includes non-mothers and mothers of children with the wrong ages. Sample is restricted to individuals living in districts with population as indicated in the column headings. Population count was based in 1996 because it was the earliest Susenas data covering all 290 districts. All columns regress work participation on preschool density, preschool eligibility dummy, and their interaction. All regressions include district, year, mother's age fixed effects and an urban residence dummy. DDD excludes while DDD-FE includes individual fixed effects. Standard errors clustered at the district-level is shown in parentheses; *, **, and *** indicate statistical significance at 10, 5, and 1 percent, respectively. Number of observations, number of districts, and work participation means for each column are indicated at the last three rows.