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Preschool Availability and Women's Employment:

Evidence from Indonesia*

Daniel Halim,#† Hillary C. Johnson#, Elizaveta Perova#

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Abstract

While a large body of literature has documented positive impacts of institutional childcare on maternal labor supply, thinner evidence is available on whether childcare can also nudge women into better jobs in developing countries. We evaluate the impact of public preschool expansion in Indonesia on women's labor supply and characteristics linked to the quality of their employment, including employment types, earnings, and hours. We rely on a triple difference approach exploiting variations in preschool availability over time and across districts, as well as preschool-age-eligibility cutoffs, in a panel dataset spanning over 20 years. We find strong positive impacts on employment—an additional public preschool per 1,000 children in the district increases women's work participation by 9.1 percent. However, it is primarily driven by an increase in unpaid family work, typically in household farms or businesses. We do not find impacts on earnings or hours of work. These findings are likely explained by the modality of preschools in Indonesia: operating for only 3 hours per day, they are unlikely to enable women to secure a paid job outside the home with longer time commitments.

Keywords: female labor force participation, preschool, Indonesia, maternal employment.

JEL codes: J13, J16, J22, O15, I24.

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† Corresponding author. # World Bank. Contact information: World Bank Group. East Asia and Pacific Gender Innovation Lab, 1818 H Street, NW, Washington, DC 20433. Email: dhalim@worldbank.org; hjohnson1@worldbank.org; eperova@worldbank.org.

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1. Introduction

Women's economic empowerment requires improvement in access to more and better jobs. Numerous studies show that the provision of institutional childcare has the potential to advance the first aspect – increase access to jobs. To date, causal evidence of positive impacts of childcare provision on maternal labor force participation and employment is available for several developed and developing countries.[1] Only a few exceptions do not find statistically significant impacts of expansion of institutional childcare services.[2] Relatively high female labor force participation prior to institutional childcare expansion may explain zero impacts in these countries. Indeed, Akgunduz and Plantenga (2017), aggregating data from 11 countries, demonstrate an inverse U-shaped relationship between the elasticity of labor force participation and childcare prices, with childcare subsidies less effective in countries with very low and/or very high female labor force participation.

However, empirical evidence is thinner on potential of institutional childcare to bolster women's access to better jobs (Menon and Rodgers, 2018). Job quality is multidimensional and can include earnings, labor market security, and quality of the working environment (Cazes, Hijzen, Saint-Martin, 2015). Several studies from developed countries examine the impacts of institutional childcare on women's earnings and find positive impacts (García et al., 2020 and Herbst, 2017 in the USA; Andresen and Havnes, 2019 in Norway; and Lefebvre & Merrigan, 2008 in Canada).

[1] These countries include: Argentina (Berlinski and Galiani, 2007; Berlinski et. al., 2011), Brazil (Paes de Barros et al., 2011), Canada (Baker et al., 2008; Lefebvre and Merrigan, 2008; Lefebvre et al., 2009; Brodeur and Connolly, 2013), Chile (Berthelon et. al., 2015; Contreras and Sepulveda, 2017; Martínez and Perticará, 2017), France (Goux and Maurin, 2010), Germany (Bauernschuster and Schlotter, 2015; Müller and Wrohlich, 2020), India (Jain, 2016), Israel (Schlosser, 2011), Italy (Carta and Rizzica, 2018), Kenya (Clark et al., 2017), Mexico (Ángeles et al., 2011; Calderon, 2014, Padilla-Romo and Cabrera-Hernández, 2018), the Netherlands (Bettendorf et al., 2015), Norway (Andresen and Havnes, 2019), Spain (Nollenberger & Rodriguez, 2015), Switzerland (Felfe et. al. 2016), and USA (Gelbach, 2002; Cascio, 2009; Barua, 2014; Herbst, 2017; Garcia et al., 2020).

[2] Fitzpatrick (2010) in the case of universal enrollment in kindergarten in the USA, and Havnes and Mogstad (2011) in case of expansion of subsidized childcare in Norway. Dang et al. (2019) do not find impacts of childcare on women's labor force participation in Vietnam but do find impacts on the type of work women do. Lundin et al. (2008) estimate the effects of reduction in childcare prices to be close to zero in Sweden. Haeck et al. (2015) find expansion of preschool to be ineffective in increasing FLFP in Canada, unless combined with subsidized childcare.

However, the labor market structures in developed and developing countries are substantially different. In developed countries where informal and unpaid family work are rarer, higher female labor force participation and employment are likely closely associated with an increase in other welfare characteristics, such as earnings and income. In contexts with higher levels of job informality and unpaid family work, increased labor force participation may or may not translate into improved earnings.

Indeed, the few available studies from developing countries show mixed results of institutional childcare on indicators related to job quality. Clark et al. (2017) and Martínez and Perticará (2017) find that access to institutional childcare services did not trigger an increase in incomes in Kenya and Chile, respectively. Two studies in Mexico and in Vietnam, on the contrary, suggest improvements in the quality of labor force participation. Calderon (2014) in Mexico shows that access to childcare increased the likelihood of working in the formal sector and reduced the likelihood of earning zero income. Dang et al. (2019) in Vietnam find positive impacts of the availability of childcare on working in a wage job, formality of employment, annual wages, and household income.

In this paper, we evaluate the impact of preschool availability in Indonesia on women's labor force participation and the characteristics of their employment: type of work, work hours and earnings. As the placement of preschools is likely correlated with FLFP, we use a difference-in-difference-in-differences (or "triple differences") strategy to establish the causal impact of preschools on maternal employment —exploiting variations in preschool availability across regions and over time and the exogenous overlap with the timing when individual mothers have a preschool-aged child. We find that an additional preschool per 1,000 children in the district increases the likelihood of maternal employment by 4.8 percentage points or 9.1 percent. However,

we find that this increase in employment is driven by women entering unpaid family work.[3] Aligned with this finding, we do not observe any impacts of preschool access on women's earnings or hours worked.

Our contribution to the literature is twofold. First, our paper is one of the first few examining the impacts of expanding access to institutional childcare on employment quality outcomes, such as earnings and type of employment, in a developing country context. Notably, existing studies analyze childcare services that are offered for a full (or almost full) workday. In Vietnam, Dang et al. (2019) analyze childcare services available 5 days a week from 7:30 am to 16:30 pm at least, which are sometimes open on Saturdays, and may work longer hours (Dang et al., 2019). In Mexico, *Estancias Infantiles*, analyzed in Calderon (2014) are open for 8 hours a day, 5 days a week. The Kenyan childcare centers studied in Clark et al. (2017) were open for approximately 7.5 hours per day. Martínez and Perticará (2017) study the impacts of expansion of full school day by a 3-hour after-primary-school program (from 4-7 pm) in Chile. Our study adds to this growing and inconclusive body of evidence by studying a different modality of childcare services that is much shorter in duration—3 hours per day as opposed to a full work day. We also explore the type of work that women enter, which can contextualize the impacts on their earnings. To the best of our knowledge, only one other study (Dang et al, 2019) explores the impacts of childcare access on the type of work (wage work, self-employment, etc.).

Second, while there is abundant causal evidence on the impacts of institutional childcare in Europe and the Americas, there are only two studies in low- and middle-income countries in Asia: Vietnam (Dang et al., 2019) and India (Jain, 2016). Thus, we expand evidence to a relatively understudied region, where context may introduce nuances to the relationship between childcare

[3] Unpaid family work is typically classified as employment in household surveys and generally implies working in family farms or businesses. It is different from unpaid care work.

services and women's labor market engagement. Indonesia is an emerging economy with robust and sustained economic growth, with an average of more than 5 percent GDP growth annually over the past 40 years, despite experiencing a tumultuous economic crisis in 1997/98. Over the same time, the total fertility rate almost halved, from 4.6 in 1979 to 2.3 in 2018. Despite this positive backdrop, female labor force participation in Indonesia has been persistently stagnant (Schaner and Das, 2016), remaining at 54 percent in 2019.[4] Moreover, preschool attendance is low, and large kin networks have traditionally provided informal childcare services.

The paper is organized as follows: in the next section, we elaborate on the context of preschools in Indonesia. In Sections 3 and 4, we discuss our data and empirical strategy, respectively. We discuss our results in Section 5, and Section 6 concludes the paper.

2. Preschools in Indonesia

There are various forms of early childhood education and development (ECED) services in Indonesia. This paper focuses on preschools (TK/RA) due to data availability.[5] Preschools are non-mandatory, formal ECED establishments 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; however, this restriction is frequently ignored, and quality varies across regions and facilities (Brinkman et al. 2017).

Recognizing the importance of ECED, the Government of Indonesia adopted it into the national education system in 2003.[6] This key legislation has allowed the continued expansion of

[4] *World Development Indicators*. Accessed on February 1, 2021.
[5] 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.
[6] The Ministry of National Development Planning of the Republic of Indonesia (BAPPENAS) regularly outlines 5-year development plans. The importance of ECED as a government is also reflected in the 5-year plan of 2004-2009: envisioning an increase in ECED enrollment.

preschools since 2003. To improve the standard of education in Indonesia, in 2005, the government issued a policy stipulating a minimum allocation of 20 percent of the annual national and regional budget to education expenses. The adoption of ECED into the national education system provided the legal foundation for ECED to be considered in the national and regional education budget allocation. In this paper, we focus on public preschools due to the endogenous placement of private preschools, and for simplicity, refer to them as just preschools.[7] Figure 1 Panel A shows geographical variation in preschool availably in 2014. Figure 1 Panel B shows upward trend in preschool access from 1990 to 2014, 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 preschool access to estimate its impact on maternal labor market decisions.

3. Data

We draw information on maternal employment and job characteristics from the Indonesia Family Life Survey (IFLS). 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) provinces back in 1993, which represented 83 percent of the national population (Frankenberg et al. 1995).[8] 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).

In the first round, more than 14,000 individual respondents were selected to provide detailed information on their LFP, including employment type, sector, occupation and income. Target respondents expanded further in subsequent rounds.[9] By the fifth round in 2014, more than 34,000

[7] Estimations which show endogenous placement of private preschools available upon request.
[8] At the time, Timor-Leste, now an independent country, was one of the Indonesian provinces.
[9] For more details, see: Frankenberg and Thomas (2000), Strauss et al. (2004), and Strauss et al. (2009).

individuals were interviewed in detail. In addition to current employment module, individuals were also asked to provide information on their employment status in survey gaps years (every year between previous and current round of IFLS survey). Combining current and historical data, we can construct an annual employment history from 1988 to 2014 for individuals who were successfully tracked in all five waves.

IFLS also includes a module for ever married women ages 15-49, which contains detailed questions about all pregnancies. 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 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. We limit the panel to women aged 15 to 45, and in order to implement the fixed effect model discussed in Section 6, we further restricted our sample to women who appear in the data in at least two waves.[10] 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). We use Village Census (PODES) cross-sections from 1990 to 2014 to obtain the number of preschools. PODES is fielded, roughly, once every three years.[11] The population of preschool-aged children comes from the annual National Socioeconomic Survey (SUSENAS) cross-sections of respective PODES years.[12]

[10] For each individual mother, we need at least two observations. Some of our outcomes, such as hours worked or earnings, are not available in the historical employment module. Hence, we need individuals who are observed in at least two rounds.
[11] Specifically, between 1990 and 2014 Podes was administered in 1990, 1993, 1996, 2000, 2003, 2005, 2008, 2011, and 2014.
[12] SUSENAS are sometimes fielded more than once per year. In such cases, we use the round with the biggest sample.

Following decentralization reform in 1999, regional governments have been entrusted with more policy-making decisions. Province, district, and village-level governments have legislative power to make policies and to allocate public goods, in this case, pertaining to preschool access.[13] We aggregate preschool access at the district-level because district is the smallest policy-relevant geographical boundary available in the SUSENAS data. Since decentralization, districts have also frequently 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.

PODES is not available annually. 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.[14] We carry out two alternative strategies—restricting our constructed panel to PODES years and predicting preschool density for the missing years using linear projection with the closest two data points available—as robustness checks.

Appendix Table 1 provides summary statistics from our constructed panel. We have 186,877 female-year observations, with an average age of 29.91 and a 36 percent probability of having a preschool-aged child in any year. The women in our panel have a 53 percent work participation rate and 51 percent live in urban areas. They live in districts where only one public preschool is available for approximately 6.6 thousand children aged 3 to 6.

There are 10,340 distinct women, who are surveyed on average in 3.54 rounds. We have recall data for them for 18 years on average, approximately 6 of which are covered in PODES.[15]

[13] Sub-district is an administrative level between district and village. It serves to demarcate geographic boundaries, but it does not have any legislative power.
[14] 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.
[15] 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.

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 preschool density.

4. Empirical strategy

To identify the impact of preschool availability on maternal labor market decisions, we exploit the exogenous overlap in children's eligibility for preschools with the spatial and temporal variations in preschool access. Specifically, we use triple differences (DDD) set up and estimate:

$$y_{ijt} = \alpha + \beta\, TK_{jt} \cdot Eligible_{ijt} + \gamma\, TK_{jt} + \delta\, Eligible_{ijt} + \mu_j + \phi_t + \psi\, \boldsymbol{X_{ijt}} + \varepsilon_{ijt} \qquad (1)$$

where y_{ijt} is the employment outcome of female *i* in district *j* in year *t*; TK_{jt} is the number of preschools per 1,000 children in district *j* in year *t*, and $Eligible_{ijt}$ is the dummy indicating if female *i* in district *j* in year *t* has any preschool-aged eligible children (age 3-6).[16]

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—are accounted for by district and year fixed effects: μ_j and ϕ_t. $\boldsymbol{X_{ijt}}$ is a vector of time-variant individual characteristics, including urban residence, female *i*'s age fixed effect, and the number of children in three age groups: 0-2, 7-12, and 13-18. We cluster our standard errors at the district-level to account for

[16] While preschools are intended for the age of 4 to 6, the entry age is not strictly observed. In our data, we observe 6 percent preschoolers first entered preschools by the age of 3.

correlations within districts. The coefficient interacting preschool density and eligibility, β, is our DDD estimate.

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.[17] Employment trends of non-mothers may not be similar to those of preschooler-mothers. However, the common trend assumption in DDD only requires that absent differences in preschool access (be it across regions or over time), the gaps in labor market outcomes between mothers with preschool-aged children with higher or lower access to preschools and the gaps between women without preschool-aged children with higher or lower access to preschools are not systematically different.

We examine the plausibility of the common trend assumption by plotting the average employment of mothers with and without 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 their preschool density between 2003 (when the NSEA was passed) and 2014.[18] Figure 2 shows that pre-trends in high and low preschool growth districts among eligible and non-eligible mothers coincide with one another well. Common trends for non-eligible women in high and low-growth areas persist after 2003, while the average work participation for

[17] Some studies of the effect of childcare on maternal employment have used the age of the youngest child to define treatment category (e.g. Gelbach, 2002, Baker et al., 2008, Cascio, 2009). We define treatment based on 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, unlike in most cross-sectional data without detailed fertility history, e.g. Labor Force Survey or Census, where we have to define based on the youngest child because the first child might have moved out of the household and, thus, is no longer observed.

[18] 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.

preschooler-mothers in high-growth districts is consistently larger than that in low-growth districts since 2003.

We also formally test parallel trends assumption limiting our sample to 1988-2003 period and estimating:

$$y_{ijt} = \alpha + \eta_1 Growth_j Post_t^p Eligible_{ijt} + \eta_2 Growth_j Post_t^p + \eta_3 Growth_j Eligible_{ijt} + \eta_4 Post_t^p Eligible_{ijt} + \eta_5 Eligible_{ijt} + \mu_j + \phi_t + \psi \boldsymbol{X_{ijt}} + \varepsilon_{ijt} \quad (2)$$

where $Growth_j$ captures change in preschool density between 2003 (when the NSEA was passed) and 2014 and $Post_t^p$ is a dummy variable equal to 1 if year *t* is greater than year *p*, and zero otherwise. Other terms are defined as in equation (1). We run equation (2) thrice, for *p* equal to 1993, 1996, 1999. Table 1 confirms graphical representation in Figure 2: coefficients on the placebo, η_2, are not statistically distinguishable from zero, and very low in magnitude for all the three placebo cutoffs.

Another potential threat to our identification strategy may be due to a possibility that women's fertility decisions may respond to the availability of preschools. We confirm that the composition of eligible and non-eligible groups in our DDD framework is not affected by the expansion of preschool services by regressing a dummy for having a preschool aged child on preschool density.[19] Table 2 shows preschool availability is not correlated with the likelihood of having a preschool aged child.

Lastly, to confirm the validity of our identification strategy we check if mothers with preschool-aged children sort into districts with high preschool access. We aggregate our constructed panel of individuals to a panel of district-years.[20] We explore whether districts with higher

[19] It includes other terms—district and year fixed effects and time-variant individual characteristics—as defined in equation (1).

[20] 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, zero

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 preschool density in the district. Table 3 reports our findings. We do not find any evidence of sorting for better preschool access.

5. Results

5.1 Main results

We begin by examining the extensive margin of labor market engagement: employment. Do preschools serve as an adequate alternative to parental childcare, enabling women to join the labor market? Table 4 shows that an additional preschool per 1,000 children increases work participation of eligible mothers by 4.8 percentage points relative to non-eligible women, and this result is robust to adjustments for multiple hypothesis testing. The adjusted q-value using the Benjamini-Hochberg (1995) step-up method to control for the false discovery rate (FDR) is 0.028.[21] Preschool availability induces a sizeable increase in the labor force participation of women with preschool-aged children: the 4.8 percentage point change represents a 9.1 percent increase from the average of 53 percent of women in our sample participating in the workforce. Preschool density in 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. Notably, the negative effect of having a preschooler is almost compensated by having another public preschool per 1,000 children.

Having established the impact of preschool on the likelihood of maternal work, we now turn to exploring the quality of this work. We first examine the impact on work status. Table 4 also presents the results of regression (1) on having a side job, being self-employed, government

change in 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.

[21] 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.

employee, employee in a private company, or unpaid family worker. An additional preschool per 1,000 children increases the likelihood that preschooler mothers become unpaid family workers by 3.6 percentage points, which is significant and robust to the simultaneous inference correction (q-value of 0.028). Entry into unpaid family work accounts for 75 percent of the gains in women's labor force participation. Aligned with this finding, we do not find statistically significant impacts on women's earnings or hours worked (Table 5). Preschools in Indonesia are open for 3 hours per day on average. This time window is not sufficient to secure a wage job, or to successfully open a business. Unpaid family work appears to be the only feasible option given such short relief from childcare duties.

5.2 Robustness checks

We test the robustness of our results to two alternative specifications: DDD with individual fixed effects and an event study. Estimating the impact of preschool availability in a DDD-fixed effects framework allows us to account for women's unobserved preferences for work and leisure, abilities, fertilities, and fecundities which may simultaneously affect childrearing and labor market decisions. Given that inclusion of individual fixed-effects enables us to control for only time-invariant characteristics, this identification strategy requires that we assume that fertility, career, and family preferences do not change over time, which may be a strong assumption.

We estimate

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

where θ_i is an individual fixed effect, and the remainder of the notation remains the same as in equation (1). Identification relies on within-mothers comparisons, exploiting two sources of 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 eligible), and in preschool

availability during eligible ages (for example, preschool density may increase from the time when the child is aged 3 until the time when the child is aged 4). The results are very similar to the results from our main specification. We find that preschool availability increases the likelihood that a mother works by 6.6 percentage points, or 12 percent (Tables 6 and 7, upper panels).

We carry out an event study as our second robustness check. Specifically, we focus our analysis on mother's work in the years surrounding her firstborn's eligibility to enter preschool and estimate:

$$Y_{ijt} = \alpha + \sum_{a=-6}^{1} \beta_a \, TK_{jt} \cdot 1(age_{it} = a) + \sum_{a=3}^{18} \beta_a \, TK_{jt} \cdot 1(age_{it} = a) + \sum_{a=-6}^{1} \delta_a \, 1(age_{it} = a)$$

$$+ \sum_{a=3}^{18} \delta_a \, 1(age_{it} = a) + \gamma \, TK_{jt} + \mu_j + \phi_t + \theta_i + \psi \, \boldsymbol{X_{ijt}} + \varepsilon_{ijt} \qquad (4)$$

where $1(age_{it} = a)$ is an indicator variable equal to 1 if the firstborn of mother *i* is aged *a* in year *t*. The coefficients β_a and δ_a are estimated for each year of age, and capture the impact of preschool on work participation relative to the omitted group of mothers whose first child was of age 2, one year before becoming eligible for preschool. All other terms are defined as in equation (1).

Figure 3 shows the results, which again are largely consistent with our main specification. We first note that relative to the year before preschool eligibility, mothers' work participation only benefits from better public preschool access starting from age 5. This is one year after the official age of entry into preschools (age 4) and when the majority of children have already entered preschools.[22] 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

[22] Information on the first age of entry into preschools is available in IFLS 4 (2007/08) and IFLS 5 (2014/15). There is a stark jump in preschool entries at the age of 4: 35 percent and 40 percent of young children ever enrolled in preschools first entered preschool at the age of 4 in IFLS 4 and 5, respectively.

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.

As discussed in section 3, the preschool data obtained from the PODES is only available in 9 of 28 years in our constructed panel. Our main specifications infer the number of preschools by using the first PODES observation available after year *t*. We test the robustness of our results to two alternative approaches. First, we test a conservative approach that restricts the analysis to PODES years only. Second, we fit a linear projection between non-missing PODES years. Tables 6 and 7 shows that our main results are robust to these alternative definitions of preschool density.

5.3 Welfare analysis

Increasing women's labor market engagement is an important policy objective in Indonesia. Increased female labor force participation is likely to slow down rising inequality (Cancian and Reed, 1998), may help households better insure against risk (Blundell, Pistaferri, and Saporta-Eksten, 2016; Ellieroth, 2019), and has been shown to be more effective in countering the problem of an ageing population than increased migration or delayed retirement (World Bank, 2016a).[23]

How effective is provision of preschools in achieving this objective? Our estimates show that building 1 additional preschool per 1,000 children in a district is likely to bring into workforce 23 mothers from that district. Given that they are most likely to enter the labor market as unpaid family workers and we cannot observe their wages, we estimate the value of their work at a predicted market wage rate for individuals with comparable observable characteristics. Specifically, we regress log hourly earnings as a function of education, experience and ability, proxied using the score on Raven's Progressive Matrices, as well as their squared values, controlling for urban residency and including district fixed effects for 2014, the latest year in our data. We use the

[23] World Bank (2016b) registers increase in inequality in Indonesia over the last decade.

estimated coefficients to construct the hypothetical average hourly wage for unpaid family workers: IDR 1,781, which is approximately 18 percent of paid workers' average hourly wage of IDR 10,031.[24] To estimate the annual increase in household welfare due to the construction of one preschool, we assume that women who join the labor force only work during the hours of preschool operation. With preschools operating for 3 hours a day and 5 days a week, one mother would participate in productive work for 780 hours per year (15*52=780). Using the estimated shadow wage rate for unpaid family workers, one additional public preschool would generate IDR 31,951,140 per year in improved household welfare (23*1781*780=31,951,140).[25]

As unpaid family work is not taxable, we can think about the estimated increase in shadow earnings as a transfer to households. At approximately IDR 1,389,180 per woman per year, it falls in the ballpark of the benefits of the Government's flagship cash transfer program PKH, whose beneficiaries receive between IDR 600,000 (US$67) and IDR 2.2 million (US$247) annually depending on family composition (Alatas et al., 2016).

6. Discussion

Aligned with previous studies, our study shows that preschool availability—one type of institutional childcare—increases women's labor force participation in Indonesia. An additional public preschool per 1,000 children in the district increases the labor force participation of mothers of preschool-aged children by 4.8 percentage points, which represents a 9.1 percent increase over average labor force participation. However, likely due the fact that preschools are only open for 3 hours per day, preschool expansion does not systematically enable women to access better jobs. Three-fourths of preschooler mothers who enter the labor force go into unpaid family work, which may be more amenable to the limited provision of childcare. Not surprisingly, we do not find

[24] 0.44 and 2.48 in PPP adjusted 2014 US dollars.
[25] 7,895 in PPP adjusted 2014 US dollars.

impacts on women's earnings or work hours. In order to enable women to access better jobs, extended preschool hours or after-care services may be needed. For example, in Dang et al. (2019), full-day childcare services enabled women in Vietnam to switch from self-employment into wage employment, formal jobs, and also increase their earnings.

Although a different type of childcare service may be more effective in promoting women's access to more and better jobs, expanding preschools that operate for only 3 hours a day can also have some welfare improving impacts for Indonesian women and families. Even if preschooler mothers who enter unpaid family work do not directly receive income from their contributions to family farms or businesses, their labor contributions presumably increase the productivity of these enterprises, which can enhance household welfare. Using an estimated shadow wage for unpaid family workers, we find that a preschooler mother who enters unpaid family work only during the hours of preschool operation could generate approximately IDR 1,389,180. This increase in welfare is on par with the Indonesian Government's flagship cash transfer program, PKH, suggesting that preschool expansion can be an attractive policy option for boosting household welfare.

While our welfare analysis is limited in scope; an increase in female labor force participation has other benefits for the overall economy, including counteracting the negative impacts of aging and the shrinking workforce (World Bank, 2016a). Cameron et. al. (2019) noted that Indonesia could increase GDP by $123 billion by increasing its FLFP to the G20 goal of 58.5 percent by 2025. The success of institutional childcare provision in increasing maternal labor supply offers a glimmer of hope amid stalled improvement in Indonesia's FLFP during decades of high economic growth. Gradually changing attitudes toward women working, especially in urban settings of Indonesia (Cameron et. al. 2019), indicate an opportune timing for policymakers to address childcare constraints facing women in accessing more and better jobs.

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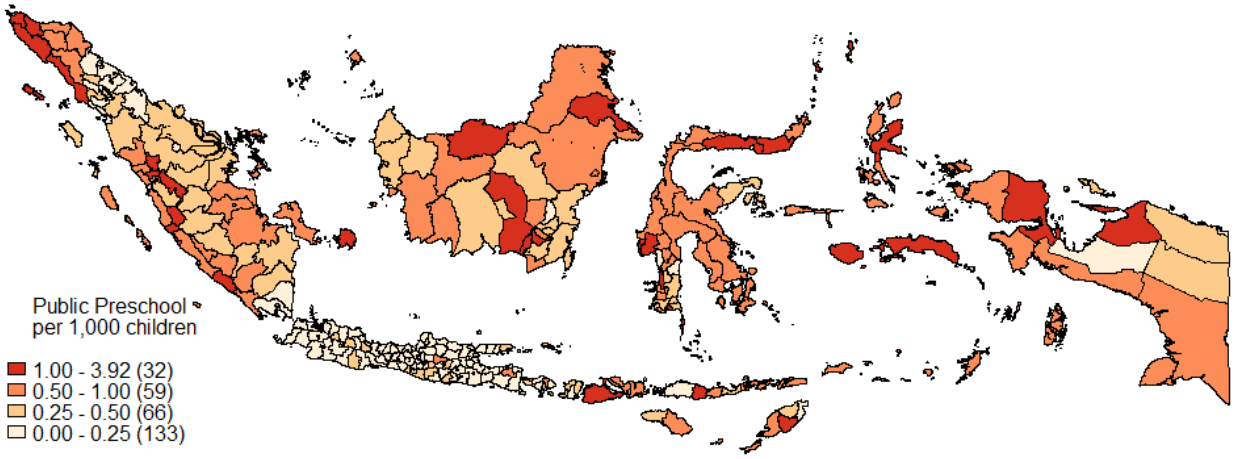
Panel A: Spatial distribution of public preschools per 1,000 children in 2014



Panel B: Density of public preschools across districts over time

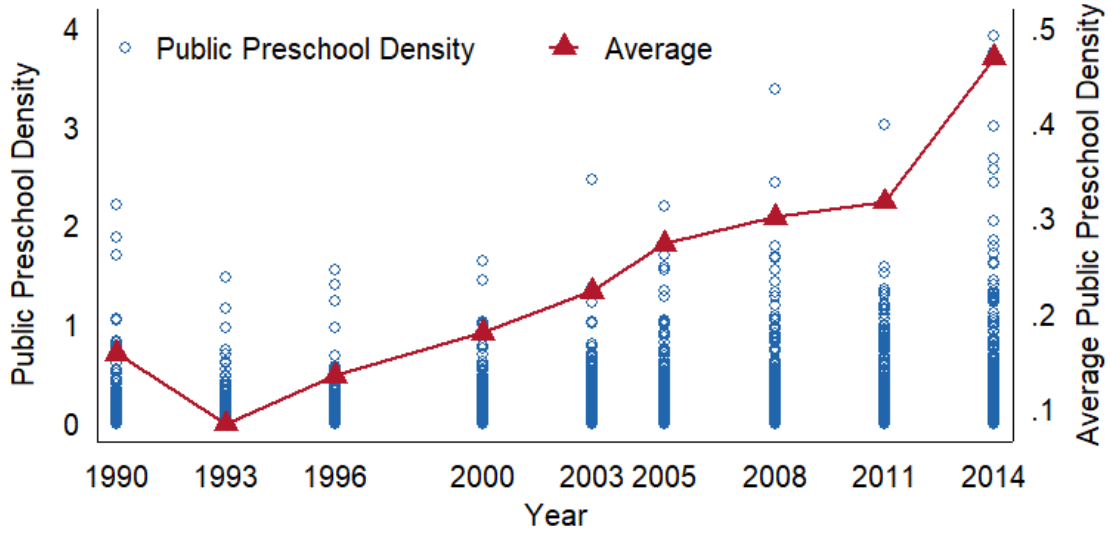


Figure 1. Spatial and temporal distributions of preschools in Indonesia

Notes: Number of public preschools is obtained from PODES and the population of children aged 3-6 is obtained from SUSENAS. Preschool density is defined as the number of preschools per 1,000 children aged 3-6. In Panel A, 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 falling in that range. In Panel B, public 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. 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.

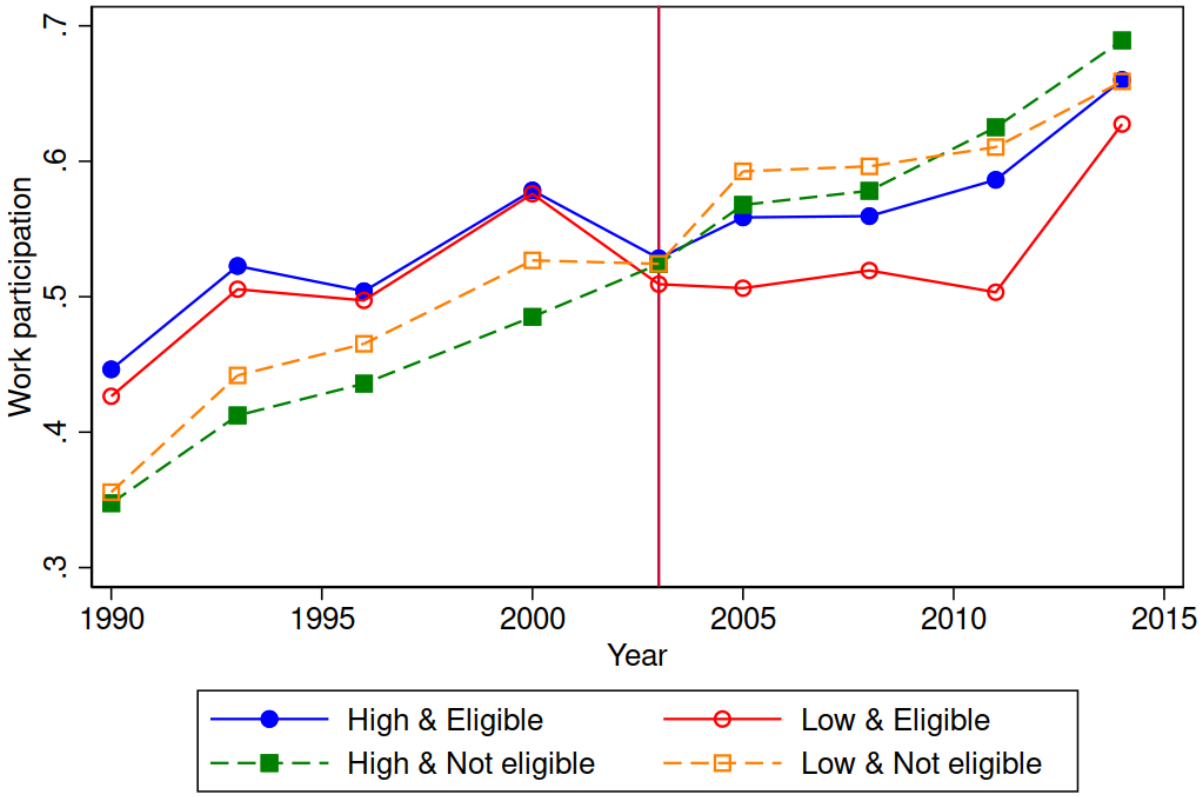


Figure 2. Average work participation of mothers with and without preschool-aged children in high and low preschool growth districts

Notes: Sample is restricted to females aged 15-45 years old of 19-45 years old female in at least two IFLS rounds. 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 preschool density growth between 2003 and 2004 is 85 percent. The vertical line marks the year 2003, when the National System Education Act was passed—incorporating early childhood education and development (ECED) into the national education system—which leads to continued expansion of preschools since 2003. Solid lines indicate eligible mothers and dashed lines indicate non-eligible women. Solid symbols indicate high-growth districts and hollow symbols indicate low-growth districts.

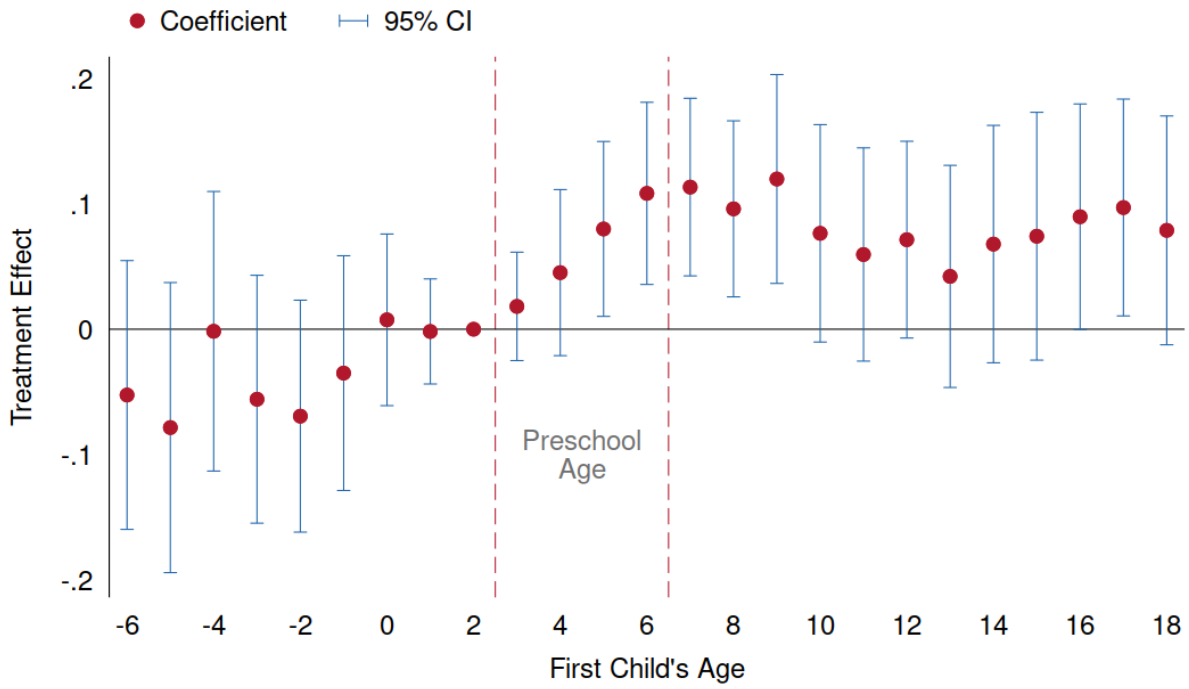


Figure 3. Event study on the effect of preschools on mothers' work participation by first child's age relative to pre-preschool-age level

Note: Sample is restricted to females aged 15-45 years old who appear in at least two IFLS rounds. We infer preschool data in-between PODES years. Each dot represents the interaction coefficient of 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 group of mothers whose first child was of age 2, one year before becoming eligible for preschool. Blue spikes represent 95 percent confidence intervals.

Table 1. Test of parallel trends prior to preschool expansion (1988-2002)

	Work participation		
	(1)	(2)	(3)
Placebo cut-off: *YEAR* ≥ ...	1993	1996	1999
Preschool growth x After cut-off x Eligible child	-0.005	-0.002	-0.003
	(0.005)	(0.004)	(0.005)
Preschool growth x After cut-off	-0.003	-0.002	-0.001
	(0.004)	(0.003)	(0.003)
Preschool growth x Eligible child	0.007	0.005	0.005
	(0.005)	(0.004)	(0.004)
After cut-off x Eligible child	-0.033**	-0.030**	-0.001
	(0.014)	(0.013)	(0.012)
Eligible child	-0.018	-0.025**	-0.042***
	(0.013)	(0.011)	(0.011)
Observations	87,902	87,902	87,902
Mean	0.469	0.469	0.469

Notes: Sample is restricted to females aged 15-45 years old who appear in at least two IFLS rounds. We also restrict our sample to the years before preschool expansion (1988-2002). We test the common trends assumption by running a regression fully interacting three variables: (i) growth in preschool density within each district, (ii) a dummy for years after the arbitrary placebo cut-off year as indicated in the column heading, and (iii) a dummy for having a preschool-aged eligible child (aged 3-6). The common trends assumption holds if the null hypothesis holds for the coefficient of "*Preschool growth x After cut-off"*. All regressions include district and year fixed effects, and the following control variables: number of children aged 0-2, 7-12, and 13-18, mother's age dummies, and an urban dummy. Robust standard errors, clustered at district level, are shown in parentheses. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values.

Table 2. Preschool availability on the likelihood of having a preschool-aged child

	Has a kid age 3-6	
	(1)	(2)
Preschool density * Eligible child	-0.012	
	(0.011)	
(Lagged) Preschool density * Eligible child		-0.017
		(0.012)
Observations	185,906	174,482
Mean	0.358	0.367

Notes: Sample is restricted to females aged 15-45 years old who appear in at least two IFLS rounds. We test the validity of our triple differences specification by regressing the dummy for having a preschool-aged child (aged 3-6) on the contemporaneous or lagged preschool density. All regressions include district and year fixed effects, and the following control variables: number of children aged 0-2, 7-12, and 13-18, mother's age dummies, and an urban dummy. Robust standard errors, clustered at district level, are shown in parentheses. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values.

Table 3. Preschool availability on net migration of mothers with a preschool-aged child

	Net migration of mothers with a preschool-aged child	
	(1)	(2)
Net change in public preschool density	-0.383	
	(0.638)	
(Lagged) Net change in public preschool density		0.245
		(0.426)
Observations	1,706	1,705
Mean	0.002	-0.001
Mean of net change in preschool density	-0.002	0.000

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 mothers with a preschool-aged child between PODES survey years on the contemporaneous or lagged net change in preschool densities. All regressions include district and year fixed effects, and the following control variables: number of children aged 0-2, 7-12, and 13-18, mother's age dummies, and an urban dummy. Robust standard errors, clustered at district level, are shown in parentheses. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values.

Table 4. Effects of preschool availability on women's employment

			Employment types			
	(1)	(2)	(3)	(4)	(5)	(6)
	Work participation	Has a second job	Self-employed	Government worker	Private worker	Unpaid family worker
Preschool density * Eligible	0.048***	-0.005	0.000	-0.002	0.014	0.036***
	(0.017)	(0.010)	(0.012)	(0.009)	(0.012)	(0.013)
Preschool density	-0.015	-0.011	0.012	0.003	-0.031**	0.000
	(0.016)	(0.008)	(0.008)	(0.007)	(0.013)	(0.011)
Eligible child	-0.062***	0.001	0.004	-0.005**	-0.075***	0.013***
	(0.008)	(0.003)	(0.004)	(0.002)	(0.006)	(0.004)
Observations	185,906	185,536	185,906	185,906	185,906	185,906
Mean	0.525	0.073	0.160	0.037	0.201	0.126
FDR q-value of *Preschool density * Eligible*	0.028	0.987	0.987	0.987	0.987	0.028

Notes: Sample is restricted to females aged 15-45 years old who appear in at least two IFLS rounds. The table reports coefficients of the triple differences specification estimated in equation (1) on outcomes indicated in the column headings. All regressions include district and year fixed effects, and the following control variables: number of children aged 0-2, 7-12, and 13-18, mother's age dummies, and an urban dummy. Robust standard errors, clustered at district level, are shown in parentheses. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR adjusted q-values, computed over all 6 outcomes, are shown for the coefficient of interest interacting *Preschool density * Eligible*. FDR q-values indicate the probability of false positives among *significant* tests.

Table 5. Effects of preschool availability on women's earnings and work hours

	(1) Salary	(2) Net Profit	(3) Income	(4) Work hours
Preschool density * Eligible	0.040	0.117	0.051	-0.031
	(0.119)	(0.135)	(0.094)	(0.039)
Preschool density	-0.107	-0.297*	-0.194	-0.021
	(0.156)	(0.169)	(0.125)	(0.035)
Eligible child	-0.153***	-0.023	-0.095***	-0.044***
	(0.040)	(0.042)	(0.032)	(0.015)
Observations	18,722	15,699	33,947	43,643
Mean	399.79	413.97	410.03	38.18
FDR q-value of *Preschool density * Eligible*	0.736	0.736	0.736	0.736

Notes: Sample is restricted to females aged 15-45 years old who appear in at least two IFLS rounds. The table reports coefficients of the triple differences specification estimated in equation (1) on outcomes indicated in the column headings. Outcomes evaluated in this table are limited to IFLS survey years (without historical recalls). Salary, net profit, and 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). All regressions include district and year fixed effects, and the following control variables: number of children aged 0-2, 7-12, and 13-18, mother's age dummies, and an urban dummy. Robust standard errors, clustered at district level, are shown in parentheses. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR adjusted q-values, computed over all 4 outcomes, are shown for the coefficient of interest interacting *Preschool density * Eligible*. FDR q-values indicate the probability of false positives among *significant* tests. 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).

Table 6. Robustness checks on the effects of preschool availability on women's employment

			Employment types			
	(1) Work participation	(2) Has a second job	(3) Self-employed	(4) Government worker	(5) Private worker	(6) Unpaid family worker
Panel A: with individual fixed effects						
Preschool density * Eligible	0.066***	0.002	0.007	0.011***	0.018*	0.030***
	(0.017)	(0.010)	(0.011)	(0.004)	(0.011)	(0.011)
	[0.028]	[0.987]	[0.987]	[0.987]	[0.987]	[0.028]
Observations	185,906	185,536	185,906	185,906	185,906	185,906
Mean	0.525	0.073	0.160	0.037	0.201	0.126
Panel B: with linear projection of preschool density						
Preschool density * Eligible	0.051**	-0.001	-0.004	-0.003	0.016	0.043***
	(0.021)	(0.010)	(0.013)	(0.009)	(0.013)	(0.014)
	[0.065]	[0.894]	[0.894]	[0.894]	[0.877]	[0.014]
Observations	186,857	186,478	186,857	186,857	186,857	186,857
Mean	0.525	0.073	0.160	0.037	0.201	0.127
Panel C: restricted to PODES years only						
Preschool density * Eligible	0.040*	0.003	-0.018	0.001	0.017	0.041**
	(0.022)	(0.012)	(0.014)	(0.009)	(0.015)	(0.017)
	[0.324]	[0.909]	[0.774]	[0.909]	[0.796]	[0.086]
Observations	62,883	62,626	62,883	62,883	62,883	62,883
Mean	0.534	0.080	0.163	0.037	0.205	0.129

Notes: Sample is restricted to females aged 15-45 years old who appear in at least two IFLS rounds. The table reports coefficients of the triple differences specification estimated in equation (1) on outcomes indicated in the column headings. Each panel introduces a single deviation from the preferred specification. Panel A includes individual female's fixed effects. Panel B replaces the preferred method to impute preschool density with a linear projection. Panel C is restricted to PODES years only. All regressions include district and year fixed effects, and the following control variables: number of children aged 0-2, 7-12, and 13-18, mother's age dummies, and an urban dummy. Robust standard errors, clustered at district level, are shown in parentheses. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR adjusted q-values, computed over all 6 outcomes within each panel, are shown in squared brackets. FDR q-values indicate the probability of false positives among *significant* tests.

Table 7. Robustness checks on the effects of preschool availability on women's earnings and work hours

	(1) Salary	(2) Net Profit	(3) Income	(4) Work hours
Panel A: with individual fixed effects				
Preschool density * Eligible	0.040	0.117	0.051	-0.031
	(0.119)	(0.135)	(0.094)	(0.039)
	[0.627]	[0.627]	[0.627]	[0.627]
Observations	18,722	15,699	33,947	43,643
Mean	399.792	413.968	410.028	38.181
Panel B: with linear projection of preschool density				
Preschool * Eligible	0.034	0.164	0.109	-0.036
	(0.163)	(0.142)	(0.101)	(0.040)
	[0.833]	[0.740]	[0.740]	[0.740]
Observations	18,791	15,776	34,092	43,926
Mean	398.963	413.071	409.153	38.190
Panel C: restricted to PODES years only				
Preschool * Eligible	0.005	0.061	0.039	-0.072
	(0.243)	(0.326)	(0.193)	(0.046)
	[0.985]	[0.985]	[0.985]	[0.482]
Observations	7,573	6,411	13,779	17,897
Mean	392.516	445.806	421.003	37.735

Notes: Sample is restricted to females aged 15-45 years old who appear in at least two IFLS rounds. The table reports coefficients of the triple differences specification estimated in equation (1) on outcomes indicated in the column headings. Each panel introduces a single deviation from the preferred specification. Panel A includes individual female's fixed effects. Panel B replaces the preferred method to impute preschool density with a linear projection. Panel C is restricted to PODES years only. Outcomes evaluated in this table are limited to IFLS survey years (without historical recalls). Salary, net profit, and 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). All regressions include district and year fixed effects, and the following control variables: number of children aged 0-2, 7-12, and 13-18, mother's age dummies, and an urban dummy. Robust standard errors, clustered at district level, are shown in parentheses. Stars denote statistical significance at 1, 5, and 10 percent levels based on unadjusted p-values. FDR adjusted q-values, computed over all 4 outcomes within each panel, are shown in squared brackets. FDR q-values indicate the probability of false positives among *significant* tests. 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).

Online Appendix for

Preschool Availability and Women's Employment: Evidence from Indonesia

Daniel Halim
World Bank

Hillary C. Johnson
World Bank

Elizaveta Perova
World Bank

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Appendix Table 1. Summary statistics

	Obs	Mean	SD
Panel A: Individual-year means			
Age	186,877	29.91	8.00
Have preschool-aged child	186,877	0.36	0.48
Work participation	186,857	0.53	0.50
Public preschool density (per 1,000 children aged 3-6)	185,926	0.15	0.22
Urban	186,877	0.51	0.50
Panel B: Individual means			
Number of surveys	10,340	3.54	1.13
Number of years	10,340	18.07	4.72
Number of PODES years	10,340	6.11	1.60
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 C: District-year means (PODES years only)			
Number of districts	290		
Public preschool count	2,592	10.27	14.11
Public preschool density (per 1,000 children aged 3-6)	2,559	0.24	0.35
Child population	2,566	61,206	56,815

Notes: Sample is restricted to females aged 15-45 years old who appear in at least two IFLS rounds. Panel A describes the pooled observations of mothers across all the observed years. Panel B describes unique observations of individual mothers. Panel A and B are constructed from IFLS round 1 thru 5. Panel C is constructed from multiple rounds of PODES and Susenas from 1990-2014, as described in Section 3. Panel C 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.