# Long-run effects of short-term grants in early childhood education

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**Abstract**

The expansion of early childhood education in developing countries has been supported by project interventions, which provide subsidized programs for a fixed period of time, followed by the introduction of small user fees in order to sustain the programs. How do short-term subsidies affect preschool enrollment and fees in the long run? This paper uses data from Indonesia, where a three-year subsidy expanded access to early childhood education. Using a difference-in-differences design, I show that the short-term subsidy had long-term effects on raising preschool enrollment rates but no significant impacts on preschool fees. I conduct a survey experiment to test different demand-side mechanisms. The results are most consistent with learning effects: parents updated their beliefs about the benefits of preschool from increased access to them as a result of the short-term subsidy.

***Keywords***— education; preschools; international development; subsidies; survey experiment
***JEL Codes***— I22, I28, O12

# 1 Introduction

Children living in poverty face considerable adversities from being exposed to multiple stressors over time. In low- and middle-income countries, an estimated 250 million children under age 5 are at risk of not reaching basic developmental milestones (Black et al., 2017). A growing body of evidence shows that high-quality early childhood education for disadvantaged children have long-term benefits on their developmental and labor market outcomes (Currie & Almond, 2011; Duncan & Magnuson, 2013; Gertler et al., 2014; Heckman, 2006). An important policy question is how to expand access to quality early childhood education in developing countries at scale.

In theory, there is strong economic rationale for governments to provide free or highly subsidized early childhood education in developing countries. In practice, however, developing countries invest little in expanding and scaling up affordable early childhood education programs. The average expenditure on early childhood education is less than 0.1 percent of the GDP in developing countries (UNESCO, 2015).[1] A common approach is to use project interventions, which provide free or highly subsidized programs for a fixed period of time, followed by the introduction of small user fees to sustain programs in the long run.[2] A key unanswered question in the existing literature is how these short-term subsidies affect long-run price and quantity of early childhood education. This question deserves attention given that it has direct implications on how to design public policy to promote the expansion of early childhood education in developing countries.

I examine the impact of short-term early childhood subsidies on price and quantity of early childhood education in a developing country. I use data from the Indonesia Early Childhood Education and Development (ECED) Project, which provided block grants to poor villages in rural Indonesia for three years. These block grants expanded access to early childhood education by offering free or heavily subsidized programs to families with pre-primary school aged children (World Bank, 2014). The Indonesia ECED Project offers an ideal scenario to test my research question for two reasons. First, there is empirical evidence that the program was beneficial to improving literacy and socio-emotional skills for a cohort of children (Brinkman, Hasan, Jung, Kinnell, & Pradhan, 2017; Jung & Hasan, 2016). Second, a follow-up survey was conducted four

[1] To put this into context, economic projections by UNESCO (2015) suggest that at least 1 percent of GDP is required to provide a minimum of one year of quality early childhood education to all young children.

[2] Introduction of user fees and cost-sharing are well-documented in the development economics literature in public health (Dupas, 2014) and education (Muralidharan, Singh, & Ganimian, 2016)

years after the subsidies ended. Thus, I am able to examine the long-run effects of a short-term early childhood education subsidy that was successfully implemented in a developing country.

My identification strategy leverages the cross-village variation in early childhood education subsidies from the Indonesia ECED Project, comparing the change in fee and enrollment before and after subsidies began in villages that received the block grants (treatment villages) relative to the change in fee and enrollment before and after subsidies began in villages that never received the block grants (comparison villages). A causal interpretation of my results requires the assumption that the trends in price and quantity would have been the same in both treatment and comparison villages in the absence of block grants. I rule out key threats to internal validity by showing that changes in preschool price and quantity are not driven by differences between treatment and comparison villages in their investments toward other levels of education or in their receipt of other governmental programs during the period of study.

I find that the impact of the short-term subsidy on preschool fees and enrollment varies considerably over time. One year after the start of the treatment, the short-term subsidies increased preschool enrollment rates among 3-5 year-old children by 66 percentage points and reduced monthly preschool fees per student by 92 percent. Seven years after the start of the treatment, the short-term subsidies had persistent effects on raising preschool enrollment rates among 3-5 year-old children by 33 percentage points but no significant impacts on preschool fees.

Next, I examine the channels driving these long-term effects. Data on preschool expenditures reveal that the short-term subsidies shifted the supply curve of preschools in treatment villages; the block grants were used to make investments in infrastructure and materials, as well as to improve preschool quality by hiring more educated teachers. I conduct a survey experiment to analyze how families responded to the supply shift caused by the short-term subsidy. On the demand-side, I examine parental investments in preschool as a function of learning effects (i.e., parents learning about the benefits of preschool) and anchoring effects (i.e., parents anchoring around the subsidized price and unwilling to pay for preschool later). Results from the survey experiment are most consistent with large positive learning effects from the short-term subsidy, net of any anchoring effects. Taken together, the evidence suggests that the short-term subsidies raised both supply and demand of preschools in treatment villages, resulting in long run effects on preschool enrollment.

My paper aims to advance our understanding of how to use financial instruments to expand access to early childhood education in developing countries. First, I contribute to a growing literature on the design of public subsidies and grants in the education sector. Research to date has focused on subsidies allocated in primary and secondary education, showing that the introduction of school grants improves enrollment (Grogan, 2008) and reduces dropout and repetition rates (Gertler, Patrinos, & Rubio-Codina, 2012). Recent studies have also examined the impact of increasing financial access in private schools to expand access to education (Andrabi, Das, Khwaja, Ozyurt, & Singh, 2018; Barrera-Osorio et al., 2017). These papers highlight how public subsidies affect both the demand and supply of education, resulting in changes in market equilibrium.

To my knowledge, my paper is the first to show the long-term general equilibrium effects of a short-term education subsidy. The topic has been understudied since subsidies provided in the education sector have historically been implemented as policies that do not have time limits. However, the recent expansion of non-compulsory education programs expanded by non-state actors in developing countries poses new questions about subsidies that are offered for a fixed period of time. In the case of early childhood education in developing countries, non-state actors (i.e., private sector, NGOs and external donors) play a major role in providing care.[3]

Second, this paper contributes to a large body of literature in psychology and economics examining how prices may affect demand through channels other than budget constraints. Both theoretical and empirical work on reference-dependent preferences suggest that price histories serve as anchors that can directly influence demand for a product (Kőszegi & Rabin, 2006; Tversky & Kahneman, 1991). Recent work in development economics have examined whether reducing short-term prices affects demand for various goods, ranging from antimalarial bed-nets (Cohen & Dupas, 2010; Dupas, 2014), home water purification (Ashraf, Berry, & Shapiro, 2010), food (Heffetz & Shayo, 2009), to medication (Fischer, Karlan, McConnell, & Raffler, 2018). The evidence is mixed with some studies showing that prior free distribution of a product reduces subsequent demand while others showing the opposite effect. Fischer et al. (2018) suggests a possible explanation for these divergent findings: the impact of free or highly subsidized distribution of goods on later demand depends

[3]In addition to early childhood education, other areas of education where short-term grants have been documented are after-school programs. Like early childhood education, these after-school programs are outside the scope of compulsory education. For example, a technology-aided after-school instruction program was heavily subsidized by philanthropic funding but ended soon after the evaluation of the program (see footnote 30 in Muralidharan et al. (2016)).

critically on whether consumers have a positive or negative experience with the subsidized good. As a result, my study not only estimates the impact of short-term subsidies on subsequent equilibrium outcomes in early childhood education, but also unpacks potential mechanisms by directly asking parents their views on early childhood education services in their villages.

Third, my results contribute to the broader literature on persistence of treatment effects after interventions are discontinued. Research has shown that brief interventions can have persistent effects years later in the domain of energy consumption (Allcott & Rogers, 2014), physical exercise (Charness & Gneezy, 2009), and community belonging at college campuses (Walton & Cohen, 2011). Yet the vast majority of research in other domains such as smoking cessation and personal financial savings do not find evidence of treatment-effect persistence (Frey & Rogers, 2014). My results highlight the importance of changing beliefs of parents (i.e., individuals with decision-making power) in understanding why treatment effects may persist years later.

An important implication of my work for policymakers is that personal experience shapes policy preferences. In the United States, individuals who are direct beneficiaries of Medicaid, public housing, Head Start and public assistance are 20 percent more likely to view these programs as effective compared to the general public (Mettler & Stonecash, 2008). Recent work suggests that policy preferences driven by personal experiences are quite robust and unlikely to be influenced by messaging by the media or political elites (Lerman & McCabe, 2017). Given the importance of public opinion on the success and failure of public policies, my results highlight the potential for short-term subsidies to jumpstart long-term investments in early childhood education in developing countries.

The rest of the paper is organized as follows. Section 2 introduces the study context and evaluation design. Section 3 describes the data and empirical strategy. Section 4 presents the main results and section 5 discusses possible channels. Section 6 concludes.

## 2 Study Context & Evaluation Design

Between 2009 and 2013, the Government of Indonesia launched the Indonesia Early Childhood Education and Development (ECED) Project to expand access to quality early childhood programs in poor, rural villages. The project provided villages with block grants that were equivalent to USD

18,000 and disbursed over three years. The block grants came with the requirement that no more than 20 percent could be spent on infrastructure. This limit meant that the vast majority of the funds were spent on educational materials and salary for teachers. In practice, 97 percent of villages chose to use the block grants to support the funding of preschools, which are known locally as playgroups (*kelompok bermain*) (Pradhan et al., 2013). Preschools in Indonesia typically cater to children between ages three and five. They largely focus on play-based learning in which children are exposed to various activities that encourage them to engage with materials and people in their environment (Hasan, Hyson, & Chang, 2013).

Along with the block grants, treatment villages received a community facilitator to assist village leaders in submitting a budget proposal for how the block grants would be used. The village leaders were instructed to make preschools accessible to the poorest families in the villages, although each village independently decided the fee amounts. The treatment villages were also eligible to send two teachers per preschool to a teacher training program. As a result, I cannot separate out the effect of these two additional components from the effect of receiving the block grants. However, implementation data of the intervention suggests that the two additional components likely played a secondary role. First, the community facilitator's role is fairly limited in the overall intervention. Second, only 38 percent of eligible teachers actually received the training. Among those who received training, 43 percent had attended less than half of the training. Given this evidence, I argue that the Indonesia ECED project was primarily a short-term subsidy to the treatment villages through the block grant.

Villages were selected in the following steps. First, the government identified 111 districts with high poverty rates and low educational attainment based on data from the Agency for National Development Planning Board (BAPPENAS). Second, 60 out of the 111 districts were selected based on capacity to monitor and report the program. 50 districts submitted their formal letter of commitment to participate. Third, nine out of the 50 districts were selected for the impact evaluation on the basis of their willingness to cooperate with an evaluation of the project and to represent geographic variation of the country. Fourth, in each of the nine districts, villages were ranked by population size since the government wanted to maximize the number of beneficiaries of the project. Within each district, 24 of the most populated villages were selected to treatment; half of the villages were assigned by lottery to receive the project first and the other half of villages

were assigned by lottery to receive the project later. Finally, district administrators recommended 10 comparison villages within each district that would never receive the project. These comparison villages were selected on the basis of having similar poverty rates and educational attainment as the treatment villages but were less densely populated (Hasan et al., 2013). Based on this selection process, the evaluation sample consists of 105 villages that received the project first (early treatment), 113 villages that receive the project later (late treatment), and 92 comparison villages never received the project.[4]

Previous papers evaluating the Indonesia ECED Project have focused on a cohort of children who were age four when the short-term subsidies began in treatment villages. In Jung and Hasan (2016), the authors compare children in treatment villages against those in comparison villages to estimate the one-year impact of the Indonesia ECED Project. They find positive effects on children's cognitive and non-cognitive skills, with the impacts concentrated among the poorest children in their sample. A three-year impact of the Indonesia ECED Project used the same cohort of children and found positive effects on social competence as well as language and cognitive development, again with impacts sustained among children from disadvantaged backgrounds (Brinkman et al., 2017).

This paper departs from these existing papers in several ways. First, I focus on the general equilibrium effects of the Indonesia ECED Project instead of the effects for a specific cohort of children who were (based on age) directly targeted by the program. I use the same treatment and comparison villages as the previous papers but use village administrative data in order to estimate impacts on overall enrollment and fees in preschools. Accounting for these general equilibrium effects are important as it helps evaluate the external validity of programs (Acemoglu, 2010). Second, I evaluate the longer-term impacts of the intervention by using a new wave of survey data collected in 2016. Third, I conduct a survey experiment as part of the data collection in 2016 to unpack the demand-side response to the short-term subsidy.

[4]The final village sample deviates slightly from 9 districts $\times \frac{24 \text{ treatment villages}}{\text{district}}$ and 9 districts $\times \frac{10 \text{ comparison villages}}{\text{district}}$ due to implementation challenges.

# 3 Data & Empirical Strategy

## 3.1 Data

I use four waves of data from the Indonesia ECED Project. As shown in Figure 1, baseline data were collected in 2009, followed by surveys in 2010, 2013 and 2016. The last block grant was disbursed in early 2013, which ended the provision of early childhood education subsidies to the treatment villages.[5] Due to delays in the survey, the baseline survey was fielded after early treatment villages received their first block grant. As a result, my analysis defines the late treatment villages as the treatment group and comparison villages as the comparison group.[6] The treatment in my study is defined as the receipt of a three-year subsidy followed by the subsequent loss of that subsidy.

I construct a village panel dataset, which includes key demographic information and preschool characteristics. Table 1 presents the means and standard deviations of key variables before and after the introduction of the short-term subsidy, separately for comparison and treatment villages.[7] The top panel of Table 1 describes general village characteristics. On average, the population size of treatment villages is larger than comparison villages, which reflects the design of the project to provide treatment in more populated areas. Poverty rates as measured by the proportion of households with no electricity and proportion of households identified as poor (defined by government classification) are similar across the two groups. In addition, the number of primary schools and kindergartens for every 1000 people in the village are also comparable across treatment and comparison villages. The last column, which shows the difference between the two groups over time, shows no statistically significant changes in the differences in demographic and education characteristics between villages that received short-term subsidies and those that never received short-term subsidies.

The bottom panel of Table 1 describes preschool characteristics measured at the village level. In both treatment and comparison villages, there were very few preschools in 2009 with an average of 0.3 preschools per village. After the short-term subsidies were introduced, comparison villages had an average of 1.3 preschools and treatment villages had an average of 2.8 preschools. Qualitative

[5] The block grants were disbursed between 2009 and 2013, with slight variation in when each treatment village received the first, second, and third disbursements.

[6] Although the early treatment villages are not part of my main analysis, I use the randomization into early and late treatment as a robustness check in Section 4.2.

[7] See Appendix Table A.1-3 for descriptives separately for 2010, 2013 and 2016 as the "After subsidy" period.

data from interviews with preschool teachers suggest that the barrier to entry of establishing a preschool in these rural villages was very low; preschools were usually set up in a teacher's home or in a room inside the village mosque. This explains the increase in the number of preschools over time in the comparison villages as well. Table 1 also shows that preschools operated similarly across the two groups over time, offering classes four to five days per week and catering to children with a mean age of four. Before the introduction of short-term subsidies, less than 10 children were enrolled in preschool per village and the average monthly fee of preschools was between 0.77 and 0.91 U.S. dollars (using an approximate conversion of 1 U.S. dollar for 10,000 Indonesian rupiah). To put this fee in context, 1 USD is approximately 1% of the minimum monthly wage for the districts in this study.

### 3.2 Empirical Strategy

I estimate the impact of short-term early childhood education subsidy on two outcomes: (ii) price, as measured by the average monthly fee charged per student in preschools and (ii) quantity, as measured by the number of children enrolled in preschools divided by the number of 3-5 year-old children. Specifically, I compare changes in price and quantity in villages that received short-term subsidies with changes in price and quantity in villages that never received short-term subsidies. By comparing changes between treatment and comparison villages, I control for observed and unobserved time-invariant characteristics as well as time-varying characteristics common to both groups that may be correlated with both short-term subsidies and equilibrium outcomes. I begin by estimating a differences-in-difference model:

$$Y_j = \alpha + \delta T_j + \lambda P_j + \beta T_j P_j + \varepsilon_j \tag{1}$$

where $Y_j$ is either the average monthly fee of preschools or enrollment rate in preschools for village $j$. $T_j$ is an indicator variable that takes the value of 1 for treatment villages and 0 for comparison villages. $P_j$ denotes year with 2009 set as the reference category. I pool together 2010, 2013 and 2017 as $P_j = 1$ to estimate the overall impact of the short-term subsidy. I also estimate 1 year, 4 year, and 7 year impacts of the short-term subsidy separately by respectively assigning 2010, 2013 and 2016 as $P_j = 1$ . The variable of interest is $\beta$ , which captures the effect of the short-

term subsidy. I extend this basic model in two ways. First, I include village-level covariates for total population and the proportion of households with no electricity (a proxy measure for poverty) in order to improve the precision of my estimates. Second, I interact the covariates with the year indicator to absorb bias caused by heterogeneity over time in the effect of the covariates. Covariates are centered at the mean so that $\beta$ is interpreted as the treatment effect with all covarates at their mean levels. Robust standard errors are used throughout.

The key identification assumption in my differences-in-difference model is that the change in mean outcomes for villages that never received the short-term subsidy are a valid counterfactual for change in mean outcomes in treatment villages if short-term subsidies had never been introduced. Ideally, one would compare the pre-treatment trend of the outcome variables between treatment and comparison villages. In this case, data on outcome variables do not exist before the baseline survey because very few preschool existed in the country at the time. The mean number of preschools per village was 0.3 in 2009, meaning that most villages had no preschool (as shown in Table 1). Instead, I draw on data from the 2003, 2005 and 2008 Survey of Village Potential (PODES) to examine pre-treatment trends of treatment and comparison villages for key village characteristics. Figure 2 presents the trend over time for four variables, which I selected because they were collected consistently across the various rounds of PODES. Figure 2 (a) and (b) show that the treatment and comparison villages track very similarly over time both in population and poverty rate (as measured by the proportion of households with no electricity in the village). Figure 2 (c) and (d) show that the number of kindergartens and primary schools for every 1000 people also trend similarly in treatment and comparison villages. These education variables are particularly informative since they indicate that the level of investment towards education for young children have historically been comparable across the treatment and comparison villages.

# 4 Effect of short-term subsidies on long run equilibrium

## 4.1 Main results

Table 2 presents the main results of the effect of short-term subsidies on long-run equilibrium outcomes. Each cell displays a separate treatment effect ($\beta$) and corresponding standard error. Column 1 reports results for the basic regression specification from equation (1), which also corresponds to

the last column in Table 1. The coefficient of 0.447 in column 1 indicates that after short-term subsidies were introduced, the proportion of children enrolled in preschool increased by 44.7 percentage points in the long-run. The coefficient of -0.546 in column 1 means that the introduction of short-term subsidies had a lasting effect on monthly fees by reducing preschool costs by 0.546 dollars per student. Both the enrollment rate and fee impacts are estimated precisely and remain significant with the inclusion of village covariates (column 2) and interaction of village covariates with the year indicator (column 3). My preferred specification is column 3 as this model relaxes the assumption of constant effects of covariates over time. The impacts of the short-term subsidies are robust to these alternative specifications; on average, the short-term subsidies increased enrollment by 49.1 percentage points and decreased monthly fees by 0.517 dollars per student.

To put these estimated impacts in context, increasing enrollment by 49.1 percentage points is equivalent to a fourfold raise in preschool enrollment over a seven year period. This large, positive effect should be viewed as an upper-bound estimate given that the denominator used to calculate enrollment rate is the number of children age 3-5 (i.e., the intended age of preschool), whereas many children who are younger or older often enroll in preschool as well. The reduction in preschool monthly fees by 0.52 dollars per student is equivalent to a 60 percent decline in student fees.

Next, I unpack the effect of short-term subsidies into discrete 1 year, 4 year, and 7 year impacts. Panel B column 3 indicates that the short-term subsidy had immediate impacts in the first year of receiving funding by increasing enrollment by 75.5 percentage points and decreasing student fees by 0.767 dollars per student. Panel C demonstrates that these initial impacts persisted over three years. The results presented in Panel C use enrollment and fees measured in 2013, which is the first year when treatment village stopped receiving the block grants. Column 3 shows that the short-term subsidy increased preschool enrollment by 36.1 percentage points and decreased fees by 0.518 dollars. Finally, Panel D shows the 7-year impacts of the short-term subsidy. Here, the regression is estimated using only 2016 as the post-treatment year. Results presented in column 3 show that enrollment rates increased by 33.5 percentage points while monthly fees decreased by 0.206 dollars. However, the effect on fees is imprecisely estimated across all model specifications when only using 2016 data in the Panel D specification.

Taken together, two key results emerge from the reduced form estimates. First, there is con-

siderable variation over time in the effect of the short-term subsidies on preschool enrollment and fee. Second, the short-term subsidies had long-run effects on raising preschool enrollment but no significant effects on reducing preschool fees.

### 4.2 Robustness checks

The key identification assumption in my differences-in-difference model is that trends in equilibrium outcomes in comparison villages are a valid counterfactual for trends in equilibrium outcomes in treatment villages if short-term subsidies had never been introduced. The internal validity of my model would be violated if there were unobserved variables influencing preschool enrollment and fees that varied between 2009 and 2016, and uniquely in either the treatment or comparison villages. A key concern is that patterns of increase in preschool enrollment and decline in fees may be driven by other governmental programs initiated to expand access to early childhood education during the same period. I provide two tests to explore this potential issue.

First, I run a placebo test by re-estimating my differences-in-difference model from Table 2 column 3 with kindergarten outcomes. The short-term subsidies were specifically aimed to expand access to preschools and not to kindergartens. As a result, there should be no difference in kindergarten enrollment or fees between treatment and comparison villages over time. The results are shown in Table 3 Panel A. Each cell reports the coefficient of the treatment and year interaction. As expected, none of the coefficients are distinguishable from zero, suggesting that unobserved factors affecting investments towards early education did not systematically vary across treatment and comparison villages between 2010 and 2016.

Second, I compare differences in the receipt of several government interventions between treatment and comparison villages in 2010, 2013 and 2016.[8] In particular, there are two major programs that have been implemented in the villages included in my analysis. One is the Family Hope Program, which is a conditional cash transfer program encouraged to improve education and health. Another is the National Program for Community Empowerment (PNPM), which is a large-scale poverty reduction program. In addition, there was a major legislative decision in 2015 that increased the size of fiscal transfer from the national government to village governments (Dana Desa). The concern with Dana Desa is that treatment and comparison villages may differ not only in how much

[8] Data on these other governmental programs were not collected in 2009.

money they receive from the national government, but also in how much of those funds were allocated towards preschool funding. In order to rule out the possibility that these other government interventions and decisions are upwardly biasing my causal estimates, I regress these variables on the treatment indicator. If there are no systematic differences between the two groups of villages on these other government interventions and decisions, the coefficient of the treatment dummy should be zero. Table 3 Panel B presents results of this regression, separately for each outcome and each year. As expected, the receipt of other government programs and the amount of funding allocated towards preschools is not systematically different between treatment and comparison villages in any of the observed years.

Finally, I conduct a test to see if the treatment effects estimated in Table 2 hold up for an alternative treatment group. Recall that the evaluation was initially designed using a randomized phase-in approach where villages were randomly assigned to receive the block grants early (early treatment) or late (late treatment). However, due to implementation schedules, early treatment received the block grant about six months before the 2009 baseline survey while late treatment received the block grant about five months after the baseline survey. If the estimated average treatment effect of the short-term subsidies is robust, I should observe the same levels of preschool enrollment and fees in the early treatment group. I regress preschool enrollment and fees on an indicator for whether villages were assigned to early (=1) or late (=0) treatment groups. These results are reported in Table 3 Panel C. In 2009, early treatment villages had more children enrolled in preschools (0.115) and were charging lower fees (-0.187) compared to late treatment villages. This positive effect makes sense given that the early treatment group had received the block grant for six months at this point while the late treatment group had not yet received the block grant. However, the magnitude of this 6-month effect is about half of what would have been expected if we halved the 1-year treatment effect from Table 2 column 3. This difference may be due to differences in implementation maturity; the late treatment villages received the block grants after the government had time to learn from issues and experience with dealing with the disbursement of the block grants from the early treatment villages. Alternatively, the effect of the subsidies may not be linear over the first 12 months. The first six months may have much smaller effects because families and preschools are just learning about the subsidy. In 2010, 2013 and 2016, there are no differences in the enrollment and fees between early and late treatment groups, suggesting that my

treatment effect estimates are robust for the long-run equilibrium outcomes.[9]

# 5 Potential Channels

Figure 3 summarizes the trends in preschool enrollment and fees for treatment and comparison villages. In the first year after treatment began, treatment villages experienced an increase in enrollment and a decrease in fees relative to comparison villages. This initial effect predominantly reflects a response to the shift in supply from the short-term subsidy. In Section 5.1 below, I describe how treatment villages used the block grant during the short-term subsidy period to understand the supply-side channel.

Figure 3 also highlights that preschool enrollment continued to remain high in treatment villages even after the short-term subsidy ended, However, preschool fees in treatment villages quickly caught up to levels similar to that of comparison villages. Will this pattern of long-run effects on enrollment continue to persist? Answering this question requires an understanding of the demand-side response to the initial shift in the supply of preschools. In Section 5.2 below, I explore potential demand-side channels driving the long-term estimates.

## 5.1 Supply-side Channels

To understand how the block grants were used by preschools in treatment villages, I use a panel data of preschools, which are available for the 2011-2012 and 2014-2015 academic years. Of the 591 preschools in the sample, 346 preschools never received block grant funding (in comparison villages) and 245 preschools received block grant funding for three years (in treatment villages).

A limitation of this data is the lack of pre-treatment measures at the preschool level. To draw causal inference from this preschool-level data, one has to make the assumption that in the absence of the block grants, preschools that received the subsidies would have had the same characteristics as preschools that never received subsidies. This is a strong assumption given that preschools that received block grant funds may not have existed without the subsidy. As a result, I caution against interpreting these supply side channels as causal mechanisms and prefer to interpret the

[9] Given concerns that diff-in-diff specifications can be sensitive to functional form (Meyer, 1995), I also show that my estimates are qualitatively similar when using log-transformations of the outcome variables. Results are shown in Appendix Table A4.

data descriptively.

Recall that in receiving the block grants, treatment villages were required to make preschools affordable to the poorest families. However, the amount of fees were decided independently in each village. Moreover, preschools in treatment villages that received the block grants had discretion over how to use the funds. As a result, I examine expenditure data from preschools to understand how the short-term subsidy was used by preschools.

I compare the expenditure of preschools that received block grant funding with that of preschools that never received block grant funding. Fixed costs and variable costs are examined over time. Fixed expenditures refer to annual investments made before the school year on infrastructure and educational materials, while variable costs refer to recurring expenses throughout the school year on teacher salaries. If the short-term subsidies are spent primarily on fixed costs, preschools in treatment villages may increase their capacity to serve more students in the long-run. In contrast, if the short-term subsidies are spent largely on variable costs, preschools may be unable to continue serving students when the short-term subsidies are no longer available.

Table 7 presents the summary statistics of the preschool-level data. In the 2011-2012 academic year, there are distinct spending patterns between preschools. Those that received block grants spent approximately 637 dollars more on fixed costs (e.g., infrastructure and educational materials) compared to preschools that never received block grants. This large fixed expenditure is reflected in the higher quality of physical infrastructure in the subsidized preschools. I construct an index of physical infrastructure (mean 0, standard deviation 1) using principal component analysis of variables measuring the condition of the building, books, toys, and furniture in each preschool.

In contrast to fixed expenditures, preschools that received block grants had similar levels of variable expenditure as preschools that never received block grants. However, these variable costs were allocated in different ways. Preschool that received block grants hired fewer teachers (diff = -0.50) but more educated teachers (diff = 0.80) relative to preschools that never received block grants. Together, the expenditure data suggests that preschools that received short-term subsidies used these funds to invest in increasing capacity through fixed costs and improving quality through variable costs while they were receiving the subsidy.

By the 2014-2015 academic year, the short-term subsidy was depleted in treatment villages. Table 7 shows that there are no systematic differences in both fixed and variable expenditures

between preschools that never received the subsidy and those that had received the short-term subsidy. One exception is the educational attainment of preschool teachers, which remains 0.67 years higher in preschools that used to receive the subsidy. This suggests that preschools that used to receive the subsidy may have succeeded in retaining higher quality teachers for the same amount of spending even after the subsidies ended.

The distinct spending patterns between preschools in treatment and comparison villages suggest that increased financial access from the short-term subsidy enabled preschools to increase their capacity through investing in infrastructure and materials, as well as to improve their quality by hiring higher quality teachers in the long run. How did parents respond to this shift in the supply curve? I explore the demand-side response in the next sub-section.

## 5.2 Demand-side Channels

### 5.2.1 Conceptual framework

I consider a decision-making framework that nests several channels through which short-term subsidies could affect whether parents invest in preschool education. Parents' decision to invest in preschool for their children is a function of costs and perceived benefits. Costs include time, money and effort associated with attending preschool. Benefits include improvements in cognitive and non-cognitive skills as a result of attending preschool. Benefits to education are often difficult to perceive since they may not be salient. As a result, parents may underinvest in early childhood education if costs outweigh perceived benefits.

Short-term subsidies for early childhood education introduce two additional terms to the simple cost-benefit decision-making framework. First is a *learning effect*. The short-term subsidy makes preschool affordable and expands access to early childhood education. In response, parents update their beliefs about the value of early childhood education – either from their own children's experience or from learning about how other families benefit from their children attending preschools. In the decision-making framework, this learning effect changes the magnitude of the perceived benefits of preschool. Assuming that preschools serve its intended purpose of providing quality education to children, the learning effect will be positive.[10] If preschools provide no meaningful learning

[10]In this study context, the assumption that the learning effect is positive is plausible given empirical evidence that the program was beneficial to improving children's literacy and socio-emotional skills (Brinkman et al., 2017;

opportunities to children, the learning effect can be zero or negative.

Second, short-term subsidies introduce an *anchoring effect* to the decision-making model. If parents take previously encountered prices as reference points or anchors, they may be unwilling to pay higher prices once the subsidy is no longer available. The anchoring effect contributes to costs in the parental decision-making model and counteracts the positive learning effect. Taken together, the relative magnitude of the positive learning effect and anchoring effect becomes key determinants of whether parents decide to invest in early childhood education after subsidies end. If learning effects are larger (smaller) than anchoring effects, the model predicts parents deciding to invest (not to invest) in preschool education. I show results from a survey experiment and an analysis of spillover effects on younger siblings to provide suggestive evidence that positive learning effects are larger than anchoring effects in this study context.

This decision-making framework is not meant to provide an exhaustive list of mechanisms but rather focus on two dominant ideas explored in the literature on short-term subsidies (Dupas, 2014; Fischer et al., 2018). I recognize that in addition to learning and anchoring effects, other mechanisms may drive demand-side responses. For example, short-term subsidies may change the opportunity costs for mothers in treatment villages, encouraging them to seek employment outside of the home. Empirically, I do not find evidence of this potential channel in this study context of rural Indonesia where employment opportunities outside of the home are limited (see variable "Mother is working" in Table 4). Another plausible mechanism is through the creation of social capital among parents. Research from the United States show that mothers of young children expand their social networks when their children enroll in preschools (Sommer et al., 2017). I view this process of socialization to be part of the learning effect as it captures positive spillovers that lead parents to update their beliefs about the value of preschools.

### 5.2.2 Survey Experiment

First, I examine whether there are differences in parental perception of early childhood education between treatment and comparison villages. I construct a household panel dataset from households sampled in treatment and comparison villages. Approximately ten households were randomly selected in each village among households that had at least one child between the ages of one and

Jung & Hasan, 2016).

two in 2009. The age criteria ensured that these children would be eligible for preschool during the tail-end of when the short-term subsidies were being provided in treatment villages. I combine this panel household data with a cross-sectional measure of parents' perceptions about early childhood education in 2016. Table 4 presents the means and standard deviations of the households in 2009 and 2016. At baseline, there are no systematic differences in household and child characteristics between those sampled in treatment and comparison villages. In 2016, the only difference observed between the two groups of households is whether children were ever enrolled in preschool, which is expected based on the design of the intervention. On average, panel children in treatment villages were 20.6 percent more likely to have been ever enrolled in preschool than panel children in comparison villages.

In the 2016 survey, I randomly assigned parents in each village to one of three types of frames about early childhood education in the household questionnaire. The first group (reference category) framed early childhood education as a public investment and received the prompt: "A way to support more children to enroll in preschools is by increasing village government funding for preschools." A total of 354 parents in treatment villages and 284 parents in comparison villages received this public framing of the survey. The second group received a short vignette informing them about the benefits of preschool. The interviewer read out-loud the following information: "Children benefit from attending high-quality preschools. In particular, children from poor families who had the opportunity to enroll in preschools perform better in school compared to other poor children who never enrolled in preschools." 376 parents in treatment villages and 307 parents in comparison villages received this information. The third group framed early childhood education as a private investment and received the prompt: "A way to support more children to enroll in preschools is by charging tuition fees to families." This private framing of the survey was assigned to 406 parents in treatment villages and 323 parents in comparison villages. After one of the three assigned prompts was provided, all parents responded to the question "Do you support expanding access to preschools in your village?" In order to ensure that data collected in the 2016 survey were not affected by the survey experiment, the item was the last question in the questionnaire.

Interviews with local parents revealed that the public framing of the survey was understood to mean that preschools would be funded as aid from the government – not as an increase in village taxes on parents since local taxes usually do not exist in poor, rural villages. As a result, the

public framing of the survey is interpreted as parents' baseline support for preschools when there are no financial responsibilities on families. In contrast to the public framing, the private framing of the survey reflects parents' support for preschools when parents bear some financial burden. The information survey can be interpreted as parents' support for preschools conditional on having information about the value of early childhood education.

The results of the survey experiment are presented in Figure 3. The first key result is that across all survey types, parents in treatment villages are significantly more likely to support expanding access to preschools. In the public framing of the survey, the difference between treatment and comparison village households in their support for expanding preschools is 9.61 percentage points. Similar results are observed for the information survey with households in treatment villages expressing support by an additional 9.42 percentage points compared to households in comparison villages. The public frame yields a larger difference of 25.6 percentage points between treatment and comparison village households. The consistent results across all three types of the survey – that parents in treatment villages express significantly higher support for preschools relative to parents in comparison villages – is important given that responses to survey questions can depend on seemingly arbitrary details about how the question is asked (Chong & Druckman, 2007; Deaton, 2012).

The second key result from the survey experiment is that the contrast between treatment and comparison in the private framing of the survey (25.6 percentage points) is significantly larger than the contrast between treatment and comparison in the public framing of the survey (9.61 percentage points). The difference between the two contrasts provides suggestive evidence of the relative magnitude of positive learning effects and anchoring effects in contributing to parents' perception of preschools. If anchoring effects loom large for parents in treatment village, parents would not be supportive of expanding access to preschools through the use of tuition fees (private frame). As a result, we would expect a smaller treatment-comparison contrast in the private frame than in the public frame if the magnitude of anchoring effects is larger than the magnitude of positive learning effects. In contrast, we would expect a larger treatment-comparison contrast in the private frame than the in the public frame if the magnitude of positive learning effects is larger than the magnitude of anchoring effects. Thus, the additional 16-percentage point difference in the treatment-comparison contrast in the private frame relative to the contrast in the public frame is

consistent with a decision-making model whereby the short-term subsidies induced larger positive learning effects than anchoring effects.

Next, I examine which parents are driving the additional 16-percentage point difference in the treatment-comparison contrast in the private frame relative to the contrast in the public frame. I define village-level change in preschool enrollment as a measure of the potential opportunity for learning effects to take place. I also define village-level change in preschool fees as a measure of the potential opportunity for anchoring effects to take place. Based on these definitions, my framework predicts that parents in villages that experienced large increases in preschool enrollment (and therefore had substantial opportunity for positive learning effects) would be more supportive of expanding access to preschools using tuition fees. Similarly, parents in villages that experienced large increases in preschool fees (and therefore more prone to anchoring effects) would be less supportive of expanding access to preschools using tuition fees. To test these model predictions, I use the following regression model:

$$Y_{ij} = \alpha + \delta T_j + \lambda S_{ij} + \tau Q_j + \beta T_j S_{ij} + \gamma T_j Q_j + \mu S_{ij} Q_j + \pi T_j S_{ij} Q_j + \theta \mathbf{X_{ij}} + \varepsilon_{ij} \qquad (2)$$

where $Y_{ij}$ is whether parent $i$ in village $j$ supports expanding access to preschools. $T_j$ is an indicator variable that takes the value of 1 for treatment and 0 for comparison villages. $S_{ij}$ is a categorical variable for the survey condition with the public framing as the omitted or reference category. $Q_j$ is a categorical variable for the quartile of change, either in preschool enrollment or in preschool fees. For quartile changes in preschool enrollment, the bottom quartile (q1) means that village $j$ experienced little or no increase in enrollment during the short-term subsidy period (i.e., change between 2009 and 2010). For quartile changes in preschool fees, the bottom quartile (q1) means that village $j$ experienced little or no increase in fees between the short-term subsidy period and post-subsidy period (i.e., change between 2010 and 2016). The vector $\mathbf{X_{ij}}$ are household and child covariates, which include household wealth, mother's highest level of education, parenting score, child's gender, age, birth weight, and whether the child was ever enrolled in preschool. The coefficient of interest $\pi$ is the three-way interaction of treatment, survey condition, and quartile change.

The results of the regression model are presented in Table 5. Column 1 reports the coefficients

from a basic model without the quartile interactions and vector of covariates in equation (2). These results are the same as Figure 3. Column 2 reports the coefficients from a basic model without the quartile interaction in equation (2). Controlling for household and child covariates reduces the difference in the treatment-comparison contrast in the private frame relative to the contrast in the public frame from 16.0 to 14.3 percentage points. The last two columns present evidence that this difference in contrasts is not uniform across villages. Column 3 shows that villages that experienced small to no changes in preschool enrollment (bottom quartile) are only 2.7 percentage points more likely to support expanding access to preschools. In contrast, villages that experiences large changes in preschool enrollment (top quartile) are 22.2 percentage points more likely to support expanding access to preschools. Column 4 shows that villages that experienced large changes in preschool fees (third and top quartile) are either less supportive of or are indifferent about expanding access to preschools while villages that experienced the least amount of change in preschool fees (bottom quartile) are 18.0 percentage points more supportive. While the quartile interactions are imprecisely estimated and suffer from endogeneity, these patterns of heterogeneity are consistent with the learning effects and anchoring effects included in my model: parents with more opportunities for learning effects are more supportive of expanding preschools while parents for whom anchoring effects are more salient are less likely to support expanding preschools.

#### 5.2.3 Younger Siblings

The results from the survey experiment provide suggestive evidence that the short-term subsidy induced larger positive learning effects than anchoring effects. But do treatment village parents' higher levels of support for preschools translate into more investments in their children's preschool education? To see if there are differences in parents' actual investments in preschool between treatment and comparison villages, I examine a subset of the panel households that had a panel child who was age-eligible to be enrolled in preschools during the short-term subsidy and had a younger sibling who was age-eligible to be enrolled in preschools in 2016 when the short-term subsidy was no longer available in treatment villages. Of the 2020 panel households, 570 households met the selection criteria. The intuition behind analyzing this sub-sample is that these are the households for which both learning effects and anchoring effects are likely to be most pronounced. If learning effects are stronger (weaker) than anchoring effects, I would expect younger siblings in treatment

village households to be significantly more (less) likely to be enrolled in preschools than younger siblings in comparison village households.

I re-estimate the difference-in-differences model in equation (1) at the household-level, using whether the younger sibling was ever enrolled in preschool as the outcome. Results are presented in Table 6. First, column 1 and 2 present results for whether the panel child was ever enrolled in preschool as a check to make sure that there was a treatment effect for this sub-sample. As expected, panel children in treatment villages were 22.1 percent more likely to have ever enrolled in preschools than panel children in comparison villages. The treatment effect remains large at 23.4 percent even after controlling for household and child covariates. Second, columns 3 and 4 present results for the younger sibling. Despite the fact that parents in treatment villages are anchored by the free or highly-subsidized preschool that was available for their older child, they are still 7.2 percent more likely to enroll the younger sibling in preschool compared to similar households in comparison villages. This suggests that the short-term subsidy induced stronger learning effects than anchoring effects, even for households in which the gain and subsequent loss of the short-term subsidy were most salient.

# 6 Conclusion

A concern among development practitioners and policymakers is whether short-term subsidies used to expand access to preschools have long-run impacts on enrollment and fees. This question deserves attention given that it has direct implications on how to design public policy to promote the expansion of early childhood education in developing countries. In this paper, I answer this question by leveraging the cross-village variation in early childhood education subsidies across poor, rural villages in Indonesia. I find that the impact of short-term subsidies on preschool enrollment and fees varies considerably over time.

One year after the start of the treatment, the short-term subsidies increased preschool enrollment rates among 3-5 year-olds by 66 percentage points and reduced monthly preschool fees per student by 92 percent. Data on preschool expenditures show that the subsidies were used to largely offset costs associated with infrastructure and educational materials. After the short-term subsidies ended, preschool fees in treatment villages quickly rose to levels similar to that of comparison vil-

lages. However, preschool enrollment in treatment villages continued to remain significantly higher than that of comparison villages. Seven years after the start of the treatment, short-term subsidies had a persistent effect on preschool enrollment rates among 3-5 year-old children by 33 percentage points.

I argue that the persistent effects of the short-term subsidy can be explained by large positive learning effects, which dominated any anchoring effects. Four years after the short-term subsidy ended, parents in treatment villages were 9.61 percentage points more likely to support expanding access to preschools in their village relative to parents in comparison villages. Moreover, parents in treatment villages were more supportive of charging fees to parents in order to expand access to preschools relative to parents in comparison villages. This suggests that the potential negative anchoring effects of the short-term subsidy were overwhelmed by positive learning effects for parents. Moving beyond stated preferences, I show that revealed preferences of parents are also consistent with positive learning effects overwhelming any potential negative anchoring effects. Younger siblings in treatment villages are 7.2 percent more likely to enroll in preschools relative to younger siblings in comparison villages even though these are the families for whom both anchoring and learning effects are likely to be most salient.

How generalizable are the findings of this study to other early childhood interventions in developing countries? In this paper, I focused on long-term general equlibrium effects. This alleviates one common critique about the external validity of program evaluations (Acemoglu, 2010) as I account for the spillovers of the program beyond the targeted beneficiaries. In addition, the short-term subsidy implemented in rural Indonesia is common in many low- and middle-income countries where the vast majority of investments in early childhood education are made through grants from donor agencies and NGOs (IEG, 2015).

At the same time, the results in this paper underscore the importance of parents' beliefs about the value of preschools in explaining the persistence of treatment effects after the short-term subsidies ended. Thus, it is likely that other preschool subsidies may not have similar persistent effects if parents perceive the costs to outweigh the benefits of preschool (and related positive spillover effects such as expanding parents' social capital). This type of negative learning effect has been documented in school-based deworming programs in Kenya, where the negative side effects of the deworming medication led to reduction in take-up among treated households relative to control

households after the one-time subsidy ended (Kremer & Miguel, 2007). The empirical evidence in this paper suggests that, at least for some types of quality, early childhood education interventions, a short-term price reduction can help jumpstart long-term investments in preschools in developing countries.

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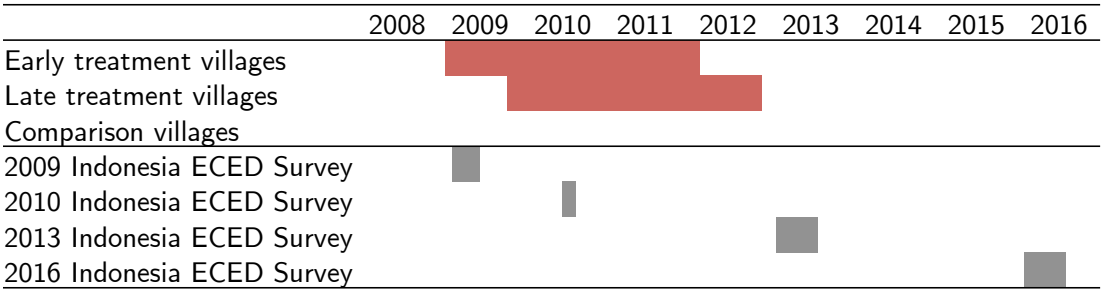


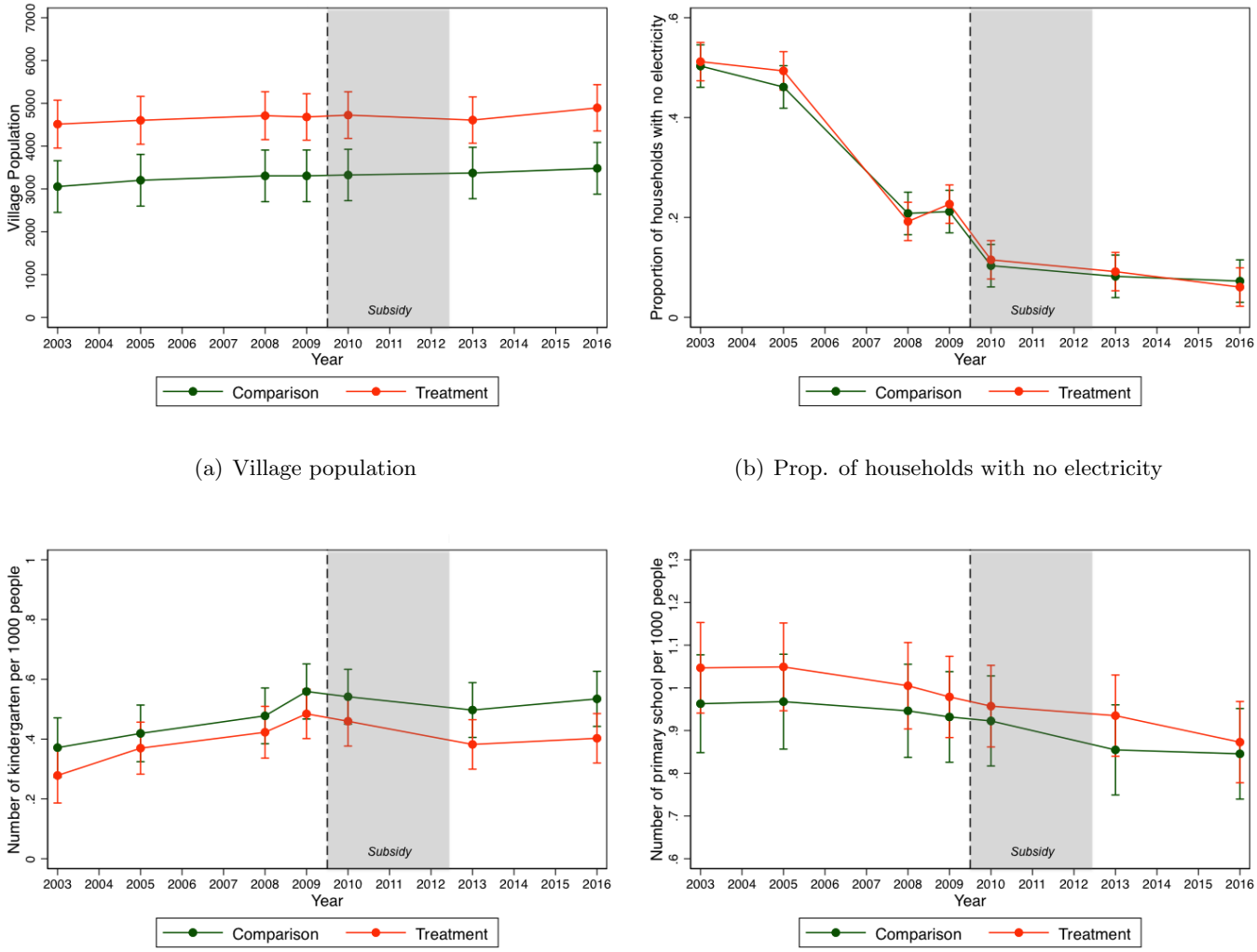

**Figure 1:** Timeline of block grants and data collection

Note: Red shaded area indicate period when block grants were disbursed to early treatment villages and late treatment villages. Comparison villages never received block grants. Grey shaded area indicates period of data collection for each round of the Indonesia ECED survey.

**Table 1:** Summary statistics of village panel data

| | Before subsidy Year = 2009 | | After subsidy Years = 2010, 2013, 2016 | | |
|---|---|---|---|---|---|
| | (1) Comparison | (2) Treatment | (3) Comparison | (4) Treatment | (5) Diff-diff |
| Village characteristics: | | | | | |
| Total population | 3315.7 | 4681.5 | 3397.1 | 4741.5 | -21.32 |
| | (2351.8) | (3531.5) | (2371.8) | (3499.0) | (122.2) |
| Population of children age 0-6 | 340.9 | 454.7 | 287.6 | 337.1 | -64.36 |
| | (280.4) | (439.5) | (287.6) | (305.4) | (50.91) |
| Prop. of households with no electricity | 0.210 | 0.220 | 0.106 | 0.0961 | -0.0190 |
| | (0.190) | (0.200) | (0.147) | (0.119) | (0.0242) |
| Number of primary schools per capita | 0.922 | 0.979 | 0.876 | 0.921 | -0.0119 |
| | (0.598) | (0.512) | (0.477) | (0.394) | (0.0598) |
| Number of kindergartens per capita | 0.553 | 0.484 | 0.523 | 0.414 | -0.0397 |
| | (0.646) | (0.511) | (0.481) | (0.362) | (0.0592) |
| Preschool characteristics: | | | | | |
| Number of preschools | 0.286 | 0.292 | 1.322 | 2.844 | 1.515*** |
| | (0.532) | (0.706) | (1.771) | (1.916) | (0.200) |
| Mean days per week of preschool | 4.284 | 3.981 | 4.796 | 4.646 | 0.153 |
| | (0.796) | (1.300) | (1.108) | (1.114) | (0.269) |
| Mean age enrolled in preschool | 3.746 | 3.969 | 3.667 | 3.926 | 0.0353 |
| | (0.819) | (0.507) | (0.703) | (0.595) | (0.190) |
| Number of children enrolled in preschool | 9.636 | 8.281 | 31.92 | 74.38 | 43.81*** |
| | (22.63) | (27.80) | (43.47) | (59.17) | (6.062) |
| Prop. of children age 3-5 enrolled | 0.108 | 0.0812 | 0.287 | 0.708 | 0.447*** |
| | (0.244) | (0.196) | (0.331) | (0.691) | (0.0597) |
| Mean monthly fee for preschool | 0.771 | 0.908 | 1.157 | 0.749 | -0.546** |
| | (1.044) | (0.723) | (0.900) | (0.871) | (0.239) |
| Observations | 92 | 113 | 276 | 339 | 820 |

Notes: Data from the Indonesia ECED Surveys. Means and standard deviations (in parentheses) presented in columns 1-4. Treatment effect and robust standard errors (in parentheses) in column 5. Mean monthly fee are in units of 10,000 Indonesian Rupiah (approx. 10,000 IDR = 1 USD). See Appendix Table 1-3 for post-treatment separately for 2010, 2013 and 2016. * p<0.1, ** p<0.05, *** p<0.01




(a) Village population

(b) Prop. of households with no electricity

(c) Number of kindergartens per capita

(d) Number of primary schools per capita

**Figure 2:** Trends of village characteristics from 2003 to 2016

Note: Data from PODES (2003, 2005, 2008) and Indonesia ECED Survey (2009, 2010, 2013, 2016). Mean with 95% confidence intervals shown in each figure. Dashed vertical line indicates when treatment villages began receiving subsidies. Grey shaded area indicates period when short-term subsidies were provided to treatment villages.

**Table 2:** Effect of short-term early childhood education subsidy on preschool enrollment and fees

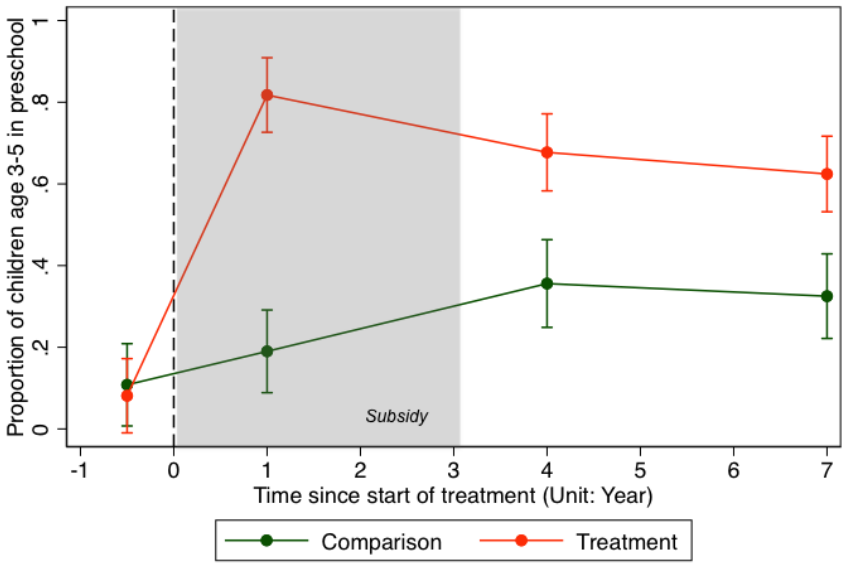
| | (1)<br>Base model | (2)<br>Add controls | (3)<br>Controls x Year |
|---|---|---|---|
| Panel A. Pooled years (N=820): | | | |
| Prop. of children age 3-5 enrolled | 0.447*** | 0.442*** | 0.491*** |
| | (0.060) | (0.061) | (0.065) |
| Mean monthly fee per child | -0.546** | -0.535** | -0.517** |
| | (0.239) | (0.239) | (0.226) |
| Panel B. 1 year impact (N=410): | | | |
| Prop. of children age 3-5 enrolled | 0.655*** | 0.658*** | 0.755*** |
| | (0.088) | (0.089) | (0.101) |
| Mean monthly fee per child | -0.779*** | -0.771*** | -0.767*** |
| | (0.243) | (0.245) | (0.230) |
| Panel C. 4 year impact (N=410): | | | |
| Prop. of children age 3-5 enrolled | 0.348*** | 0.341*** | 0.361*** |
| | (0.081) | (0.080) | (0.090) |
| Mean monthly fee per child | -0.532** | -0.529** | -0.518** |
| | (0.259) | (0.259) | (0.245) |
| Panel D. 7 year impact (N=410): | | | |
| Prop. of children age 3-5 enrolled | 0.326*** | 0.324*** | 0.335*** |
| | (0.067) | (0.067) | (0.071) |
| Mean monthly fee per child | -0.271 | -0.239 | -0.206 |
| | (0.256) | (0.257) | (0.248) |

Notes: Data from the Indonesia ECED Surveys. Each cell is the diff-in-diff estimate from a separate regression. Robust standard errors in parentheses. Mean monthly fee are in units of 10,000 Indonesian Rupiah (approximately 10,000 IDR = 1 USD). Column (1) is the basic diff-in-diff specification and corresponds to the last column in Table 1. Column (2) adds village-level controls: population size and proportion of households with no electricity. Column (3) interacts the village controls with the year indicator. * p<0.1, ** p<0.05, *** p<0.01

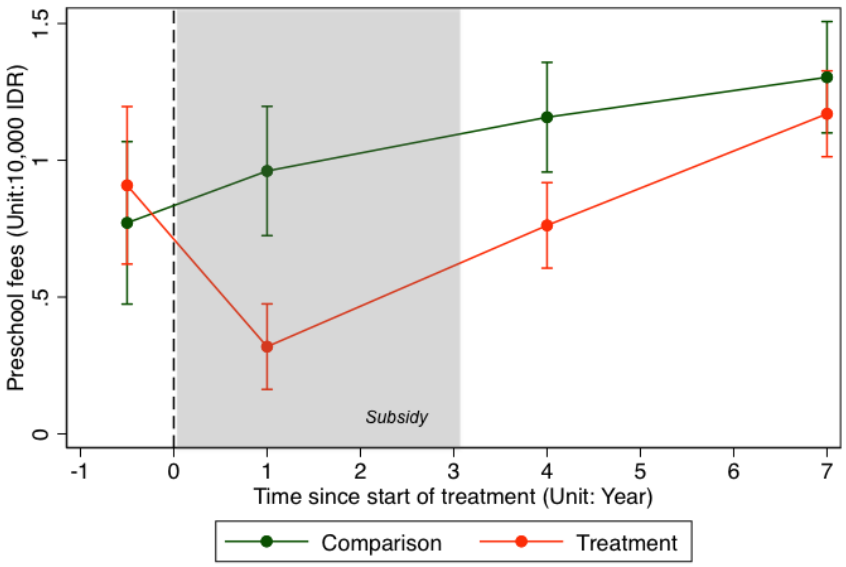
**Table 3:** Robustness checks for effect of short-term early childhood education subsidy

| | (1) Pooled | (2) 1 year | (3) 4 years | (4) 7 years |
|---|---|---|---|---|
| Panel A: Impacts on kindergarten | | | | |
| Number of children in kindergarten | -2.151 | 3.151 | -6.176 | -3.427 |
| | (14.623) | (14.840) | (14.873) | (15.838) |
| Mean monthly fee for kindergarten | 0.167 | 0.063 | 0.123 | 0.316 |
| | (0.288) | (0.281) | (0.272) | (0.374) |
| Observations | 820 | 410 | 410 | 410 |
| | 2009 | 2010 | 2013 | 2016 |
| Panel B: Other government programs | | | | |
| Diff. in receipt of cash transfer program | n.a. | -0.030 | 0.068 | 0.023 |
| (Family Hope Program) | n.a. | (0.050) | (0.070) | (0.057) |
| Diff. in receipt of poverty reduction program | n.a. | -0.021 | 0.051 | 0.019 |
| (PNPM) | n.a. | (0.051) | (0.063) | (0.041) |
| Diff. in amount of village fund | n.a. | n.a. | n.a. | 1920.9 |
| (Dana Desa) | n.a. | n.a. | n.a. | (1674.3) |
| Diff. in amount of village fund allocated | n.a. | n.a. | n.a. | 373.4 |
| for preschools | n.a. | n.a. | n.a. | (363.1) |
| Observations | 0 | 205 | 205 | 205 |
| | 2009 | 2010 | 2013 | 2016 |
| Panel C: Late vs. early treatment group | | | | |
| Diff. in number of children in preschool | 0.115** | -0.061 | 0.112 | 0.110 |
| | (0.050) | (0.098) | (0.092) | (0.074) |
| Diff. in mean monthly fee for preschool | -0.187 | -0.002 | 0.065 | -0.058 |
| | (0.290) | (0.063) | (0.118) | (0.153) |
| Observations | 218 | 218 | 218 | 218 |

Notes: Data from the Indonesia ECED Surveys. Each cell is the result of a separate regression. "n.a." for data not collected or not applicable. Robust standard errors in parentheses. Panel A estimates the diff-in-diff specification from Table 2 column 3 using enrollment and fees in kindergarten (which were not part of the intervention by design). Panel B regresses receipt of other programs by an indicator for treatment, separately for each year. Village fund (Dana Desa) was implemented for the first time in 2015. Panel C regresses preschool enrollment and fees by an indicator for whether treatment villages received intervention before baseline (=1) or after baseline (=0). Due to implementation issues, treatment villages were randomly assigned to receive the intervention before or after baseline. Mean monthly fee are in units of 10,000 Indonesian Rupiah (approx. 10,000 IDR = 1 USD). * p<0.1, ** p<0.05, *** p<0.01




(a) Preschool enrollment




(b) Preschool fees

**Figure 3:** Trends over time for preschool enrollment and fees

Note: Data from PODES (2003, 2005, 2008) and Indonesia ECED Survey (2009, 2010, 2013, 2016). Mean with 95% confidence intervals shown in each figure. Dashed vertical line indicates when treatment villages began receiving subsidies. Grey shaded area indicates period when short-term subsidies were provided to treatment villages.

**Table 4:** Summary statistics of panel households

| | Year = 2009 | | | Year = 2016 | | |
|---|---|---|---|---|---|---|
| | (1) Comp | (2) Treat | (3) Diff | (4) Comp | (5) Treat | (6) Diff |
| Household characteristics: | | | | | | |
| Household wealth (z-score) | 0.055 | -0.040 | -0.095 | -0.049 | -0.004 | 0.045 |
| | (0.989) | (0.985) | (0.071) | (1.039) | (0.950) | (0.051) |
| Mother's education (years) | 8.039 | 8.128 | 0.089 | 8.044 | 8.126 | 0.082 |
| | (3.597) | (3.630) | (0.254) | (3.449) | (3.495) | (0.234) |
| Positive parenting practices | 83.41 | 82.78 | -0.627 | 79.91 | 79.69 | -0.219 |
| | (7.385) | (6.943) | (0.574) | (6.557) | (6.390) | (0.352) |
| Mother is working (1=Yes) | 0.241 | 0.287 | 0.046 | 0.446 | 0.481 | 0.035 |
| | (0.428) | (0.453) | (0.032) | (0.497) | (0.500) | (0.037) |
| Child characteristics: | | | | | | |
| Gender of child (1=Female) | 0.474 | 0.485 | 0.011 | 0.460 | 0.484 | 0.024 |
| | (0.500) | (0.500) | (0.023) | (0.499) | (0.500) | (0.024) |
| Age of child (years) | 1.508 | 1.500 | -0.008 | 8.309 | 8.336 | 0.027 |
| | (0.286) | (0.286) | (0.014) | (0.380) | (0.363) | (0.031) |
| Child's birth weight (kg) | 3.190 | 3.168 | -0.022 | 3.169 | 3.150 | -0.019 |
| | (0.535) | (0.578) | (0.030) | (0.508) | (0.515) | (0.025) |
| Ever enrolled in preschool (1=Yes) | 0.011 | 0.003 | -0.008 | 0.310 | 0.516 | 0.206*** |
| | (0.104) | (0.051) | (0.006) | (0.463) | (0.500) | (0.040) |
| Observations | 911 | 1146 | 2057 | 911 | 1146 | 2057 |

Notes: Data from the Indonesia ECED Surveys. Household wealth is constructed based on asset/ownership of objects in the household. Positive parenting practices were measured using 24 items describing parent-child relationships adapted from the Longitudinal Study of Australian Children (LSAC) (Zubrick et al. 2008). Higher scores indicate more positive caregiving patterns. * p<0.1, ** p<0.05, *** p<0.01

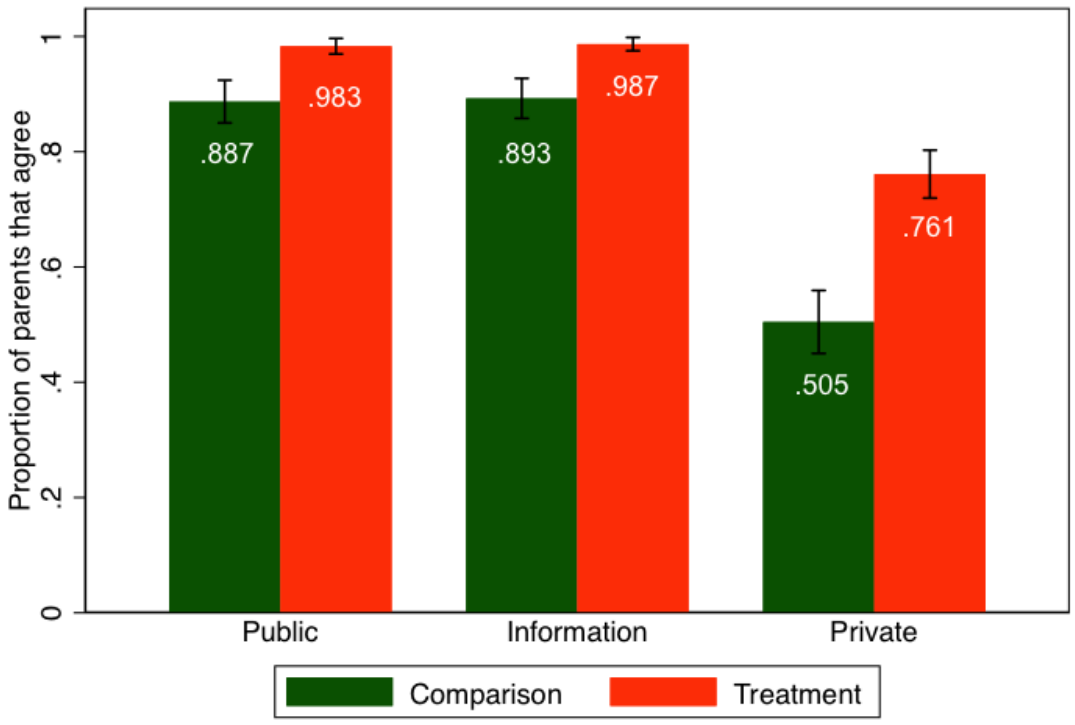



**Figure 4:** Parental support for expanding access to preschools by treatment status and survey type

Note: Data from Indonesia ECED Survey (2016). Mean with 95% confidence intervals shown in each figure. Sample size is 2020 parents.

**Table 5:** Heterogeneity of survey experiment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Framing (ref: Public) | | | | |
| Information | 0.006 | 0.018 | 0.127 | -0.032 |
| | (0.840) | (0.542) | (0.339) | (0.710) |
| Private | -0.382*** | -0.361*** | -0.277*** | -0.320*** |
| | (0.000) | (0.000) | (0.003) | (0.000) |
| Treatment (ref: Comparison) | | | | |
| Treatment | 0.096*** | 0.110*** | 0.218** | 0.071 |
| | (0.000) | (0.000) | (0.019) | (0.217) |
| Interactions | | | | |
| Information × Treatment | -0.002 | -0.019 | -0.115 | 0.060 |
| | (0.948) | (0.528) | (0.391) | (0.501) |
| Information × Treatment × 2nd quartile | | | 0.074 | -0.126 |
| | | | (0.606) | (0.299) |
| Information × Treatment × 3rd quartile | | | 0.108 | -0.121 |
| | | | (0.459) | (0.204) |
| Information × Treatment × Top quartile | | | 0.108 | -0.017 |
| | | | (0.457) | (0.885) |
| Private × Treatment | 0.160*** | 0.144*** | 0.027 | 0.180* |
| | (0.000) | (0.002) | (0.796) | (0.067) |
| Private × Treatment × 2nd quartile | | | 0.092 | 0.080 |
| | | | (0.494) | (0.599) |
| Private × Treatment × 3rd quartile | | | 0.098 | -0.145 |
| | | | (0.498) | (0.278) |
| Private × Treatment × Top quartile | | | 0.222* | 0.015 |
| | | | (0.088) | (0.914) |
| Observations | 2048 | 1878 | 1878 | 1878 |
| $R^2$ | 0.199 | 0.196 | 0.203 | 0.227 |
| Household and child covariates | No | Yes | Yes | Yes |
| Quartile interactions | No | No | $\Delta$ enrollment | $\Delta$ fees |

Notes: Data from the 2016 Indonesia ECED Survey. Outcome variable is whether parents support expanding access to preschools in their village (1=Yes). Robust standard errors clustered at the village-level. Household and child controls are household wealth, mother's education, parenting score, gender of child, age of child, child's birth weight, and whether the child was ever enrolled in preschool. * p<0.1, ** p<0.05, *** p<0.01

**Table 6:** Effect of short-term subsidy on preschool enrollment for panel households with younger siblings

| | Panel child was ever enrolled in preschool | | Younger sibling was ever enrolled in preschool | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 2016 (ref: 2009) | 0.282*** | 0.514* | 0.090*** | 0.090*** |
| | (0.000) | (0.054) | (0.000) | (0.000) |
| Treatment (ref: Comparison) | -0.025* | -0.027** | -0.000*** | -0.002 |
| | (0.060) | (0.045) | (0.000) | (0.466) |
| 2016 × Treatment | 0.221*** | 0.234*** | 0.072** | 0.072** |
| | (0.000) | (0.000) | (0.015) | (0.015) |
| Observations | 1141 | 1141 | 1141 | 1141 |
| $R^2$ | 0.262 | 0.311 | 0.077 | 0.101 |
| Household covariates | No | Yes | No | Yes |
| Panel child covariates | No | Yes | No | No |
| Younger sibling covariates | No | No | No | Yes |

Notes: Data from the Indonesia ECED Surveys. Robust standard error clustered at the village-level. Household covariates include household wealth, mother?s education and parenting score. Panel child covariates include gender of child, age of child, child?s birth weight. Younger sibling covariates include gender of younger sibling, age of younger sibling, and birth order of younger sibling. * p<0.1, ** p<0.05, *** p<0.01

**Table 7:** Preschool-level data on expenditure, teachers, and physical infrastructure

| | 2011-2012 Academic year | | | 2014-2015 Academic year | | |
|---|---|---|---|---|---|---|
| | (1) Never received | (2) Receiving | (3) Diff. | (4) Never received | (5) Used to receive | (6) Diff. |
| Expenditure: | | | | | | |
| Fixed costs | 0.371 | 1.008 | 0.637*** | 0.829 | 0.461 | -0.368 |
| | (0.596) | (1.897) | (0.106) | (1.919) | (1.075) | (0.187) |
| Variable costs | 0.612 | 0.589 | -0.023 | 0.659 | 0.663 | 0.004 |
| | (1.174) | (0.690) | (0.102) | (0.935) | (0.936) | (0.086) |
| Teacher characteristics: | | | | | | |
| Number of teachers | 3.526 | 3.025 | -0.500** | 3.693 | 3.381 | -0.312 |
| | (1.648) | (1.622) | (0.166) | (1.671) | (1.532) | (0.166) |
| Mean years of education of teachers | 7.744 | 8.541 | 0.797* | 6.923 | 7.586 | 0.663* |
| | (3.284) | (3.280) | (0.340) | (2.938) | (3.012) | (0.300) |
| Physical characteristics: | | | | | | |
| Quality of phys. infra. (z-score) | -0.237 | 0.086 | 0.323** | 0.013 | -0.005 | -0.018 |
| | (1.107) | (0.945) | (0.113) | (1.068) | (0.976) | (0.117) |
| Observations | 346 | 245 | 591 | 346 | 245 | 591 |

Notes: Robust standard errors clustered at village level. Data on 2011-2012 academic year from 2013 Indonesia ECED Survey and data on 2014-2015 academic year from the 2016 Indonesia ECED Survey. Expenditure data is in units of 10,000,000 Indonesian Rupiah (equivalent to units of 1,000 USD). The column "Never received" refers to preschools that never received block grant funding. The column "Receiving" and "Used to receive" are preschools that received block grant funding for three years. The difference column presents the mean difference between columns (1) and (2) or between columns (4) and (5). * p<0.1, ** p<0.05, *** p<0.01

**Table A1:** Summary statistics of village panel data using 2010 post-treatment

| | Before subsidy Year = 2009 | | After subsidy Year = 2010 | | |
|---|---|---|---|---|---|
| | (1) Comparison | (2) Treatment | (3) Comparison | (4) Treatment | (5) Diff-diff |
| Village characteristics: | | | | | |
| Total population | 3315.7 | 4681.5 | 3326.0 | 4721.0 | 29.22 |
| | (2351.8) | (3531.5) | (2382.6) | (3671.6) | (106.9) |
| Population of children age 0-6 | 340.9 | 454.7 | 282 | 373.7 | -22.15 |
| | (280.4) | (439.5) | (236.2) | (334.4) | (50.73) |
| Prop. of households with no electricity | 0.210 | 0.220 | 0.103 | 0.115 | 0.00245 |
| | (0.190) | (0.200) | (0.179) | (0.183) | (0.0268) |
| Number of primary schools per capita | 0.922 | 0.979 | 0.931 | 0.955 | -0.0334 |
| | (0.598) | (0.512) | (0.535) | (0.411) | (0.0584) |
| Number of kindergartens per capita | 0.553 | 0.484 | 0.542 | 0.458 | -0.0153 |
| | (0.646) | (0.511) | (0.447) | (0.385) | (0.0633) |
| Preschool characteristics: | | | | | |
| Number of preschools | 0.286 | 0.292 | 0.924 | 2.336 | 1.406*** |
| | (0.532) | (0.706) | (1.225) | (0.922) | (0.142) |
| Mean days per week of preschool | 4.284 | 3.981 | 4.539 | 4.373 | 0.137 |
| | (0.796) | (1.300) | (1.175) | (1.123) | (0.286) |
| Mean age enrolled in preschool | 3.746 | 3.969 | 3.570 | 3.843 | 0.0489 |
| | (0.819) | (0.507) | (0.724) | (0.539) | (0.213) |
| Number of children enrolled in preschool | 9.636 | 8.281 | 20.05 | 76.79 | 58.09*** |
| | (22.63) | (27.80) | (28.23) | (46.57) | (5.528) |
| Prop. of children age 3-5 in preschool | 0.108 | 0.0812 | 0.190 | 0.818 | 0.655*** |
| | (0.244) | (0.196) | (0.260) | (0.856) | (0.0881) |
| Mean monthly fee for preschool | 0.771 | 0.908 | 0.961 | 0.319 | -0.779*** |
| | (1.044) | (0.723) | (0.589) | (0.404) | (0.243) |
| Observations | 92 | 113 | 92 | 113 | 410 |

Notes: Data from the Indonesia ECED Surveys. Means and standard deviations (in parentheses) presented in columns 1-4. Treatment effect and robust standard errors (in parentheses) in column 5. Mean monthly fee are in units of 10,000 Indonesian Rupiah (approx. 10,000 IDR = 1 USD). * p<0.1, ** p<0.05, *** p<0.01

**Table A2:** Summary statistics of village panel data using 2013 post-treatment

| | Before subsidy Year = 2009 | | After subsidy Year = 2013 | | |
|---|---|---|---|---|---|
| | (1) Comparison | (2) Treatment | (3) Comparison | (4) Treatment | (5) Diff-diff |
| Village characteristics: | | | | | |
| Total population | 3315.7 | 4681.5 | 3372.4 | 4608.9 | -129.3 |
| | (2351.8) | (3531.5) | (2340.2) | (3284.0) | (143.4) |
| Population of children age 0-6 | 340.9 | 454.7 | 281.6 | 319.4 | -76.04 |
| | (280.4) | (439.5) | (350.5) | (287.0) | (57.15) |
| Prop. of households with no electricity | 0.210 | 0.220 | 0.145 | 0.109 | -0.0455 |
| | (0.190) | (0.200) | (0.164) | (0.0606) | (0.0289) |
| Number of primary schools per capita | 0.922 | 0.979 | 0.855 | 0.935 | 0.0232 |
| | (0.598) | (0.512) | (0.463) | (0.394) | (0.0691) |
| Number of kindergartens per capita | 0.553 | 0.484 | 0.497 | 0.382 | -0.0464 |
| | (0.646) | (0.511) | (0.480) | (0.337) | (0.0655) |
| Preschool characteristics: | | | | | |
| Number of preschools | 0.286 | 0.292 | 1.337 | 3.071 | 1.728*** |
| | (0.532) | (0.706) | (1.859) | (2.120) | (0.256) |
| Mean days per week of preschool | 4.284 | 3.981 | 4.833 | 4.697 | 0.166 |
| | (0.796) | (1.300) | (1.114) | (1.196) | (0.285) |
| Mean age enrolled in preschool | 3.746 | 3.969 | 3.677 | 3.946 | 0.0453 |
| | (0.819) | (0.507) | (0.720) | (0.503) | (0.203) |
| Number of children enrolled in preschool | 9.636 | 8.281 | 36.51 | 74.25 | 39.09*** |
| | (22.63) | (27.80) | (48.51) | (64.85) | (7.680) |
| Prop. of children age 3-5 in preschool | 0.108 | 0.0812 | 0.356 | 0.677 | 0.348*** |
| | (0.244) | (0.196) | (0.383) | (0.655) | (0.0805) |
| Mean monthly fee for preschool | 0.771 | 0.908 | 1.157 | 0.762 | -0.532** |
| | (1.044) | (0.723) | (0.892) | (0.819) | (0.259) |
| Observations | 92 | 113 | 92 | 113 | 410 |

Notes: Data from the Indonesia ECED Surveys. Means and standard deviations (in parentheses) presented in columns 1-4. Treatment effect and robust standard errors (in parentheses) in column 5. Mean monthly fee are in units of 10,000 Indonesian Rupiah (approx. 10,000 IDR = 1 USD). * p<0.1, ** p<0.05, *** p<0.01

**Table A3:** Summary statistics of village panel data using 2016 post-treatment

| | Before subsidy Year = 2009 | | After subsidy Year = 2016 | | |
|---|---|---|---|---|---|
| | (1) Comparison | (2) Treatment | (3) Comparison | (4) Treatment | (5) Diff-diff |
| Village characteristics: | | | | | |
| Total population | 3315.7 | 4681.5 | 3492.7 | 4894.6 | 36.14 |
| | (2351.8) | (3531.5) | (2414.9) | (3555.4) | (183.6) |
| Population of children age 0-6 | 340.9 | 454.7 | 299.3 | 318.2 | -94.88* |
| | (280.4) | (439.5) | (266.7) | (292.2) | (53.42) |
| Prop. of households with no electricity | 0.210 | 0.220 | 0.0693 | 0.0645 | -0.0140 |
| | (0.190) | (0.200) | (0.0571) | (0.0621) | (0.0269) |
| Number of primary schools per capita | 0.922 | 0.979 | 0.841 | 0.873 | -0.0254 |
| | (0.598) | (0.512) | (0.426) | (0.375) | (0.0652) |
| Number of kindergartens per capita | 0.553 | 0.484 | 0.529 | 0.403 | -0.0573 |
| | (0.646) | (0.511) | (0.519) | (0.361) | (0.0619) |
| Preschool characteristics: | | | | | |
| Number of preschools | 0.286 | 0.292 | 1.707 | 3.124 | 1.411*** |
| | (0.532) | (0.706) | (2.052) | (2.311) | (0.288) |
| Mean days per week of preschool | 4.284 | 3.981 | 4.942 | 4.857 | 0.218 |
| | (0.796) | (1.300) | (1.037) | (0.966) | (0.282) |
| Mean age enrolled in preschool | 3.746 | 3.969 | 3.727 | 3.974 | 0.0233 |
| | (0.819) | (0.507) | (0.674) | (0.690) | (0.196) |
| Number of children enrolled in preschool | 9.636 | 8.281 | 39.21 | 72.12 | 34.26*** |
| | (22.63) | (27.80) | (48.40) | (64.65) | (8.334) |
| Prop. of children age 3-5 in preschool | 0.108 | 0.0812 | 0.325 | 0.624 | 0.326*** |
| | (0.244) | (0.196) | (0.326) | (0.499) | (0.0672) |
| Mean monthly fee for preschool | 0.771 | 0.908 | 1.304 | 1.170 | -0.271 |
| | (1.044) | (0.723) | (1.068) | (1.047) | (0.256) |
| Observations | 92 | 113 | 92 | 113 | 410 |

Notes: Data from the Indonesia ECED Surveys. Means and standard deviations (in parentheses) presented in columns 1-4. Treatment effect and robust standard errors (in parentheses) in column 5. Mean monthly fee are in units of 10,000 Indonesian Rupiah (approx. 10,000 IDR = 1 USD). * p<0.1, ** p<0.05, *** p<0.01

**Table A4:** Effect of short-term subsidy on log-transformed outcomest

| | (1) Base model | (2) Add controls | (3) Controls x Year |
|---|---|---|---|
| Panel A. Pooled years (N=820): | | | |
| Log(Prop. of chiildren age 3-5 enrolled) | 0.689*** | 0.721*** | 0.661*** |
| | (0.237) | (0.224) | (0.213) |
| Log(Mean monthly fee per child) | -0.377** | -0.379** | -0.362** |
| | (0.157) | (0.158) | (0.153) |
| Panel B. 1 year impact (N=410): | | | |
| Log(Prop. of chiildren age 3-5 enrolled) | 1.003*** | 1.056*** | 1.026*** |
| | (0.259) | (0.248) | (0.242) |
| Log(Mean monthly fee per child) | -0.845*** | -0.827*** | -0.832*** |
| | (0.172) | (0.175) | (0.172) |
| Panel C. 4 year impact (N=410) : | | | |
| Log(Prop. of chiildren age 3-5 enrolled) | 0.556** | 0.564** | 0.507** |
| | (0.243) | (0.232) | (0.224) |
| Log(Mean monthly fee per child) | -0.244 | -0.242 | -0.230 |
| | (0.170) | (0.171) | (0.167) |
| Panel D. 7 year impact (N=410): | | | |
| Log(Prop. of chiildren age 3-5 enrolled) | 0.544** | 0.562** | 0.463** |
| | (0.251) | (0.242) | (0.226) |
| Log(Mean monthly fee per child) | -0.164 | -0.134 | -0.050 |
| | (0.187) | (0.186) | (0.179) |

Notes: Data from the Indonesia ECED Surveys. Each cell is the diff-in-diff estimate of a separate regression. Robust standard errors in parentheses. Mean monthly fee are in units of 10,000 Indonesian Rupiah (approximately 10,000 IDR = 1 USD). Column (1) is the basic diff-in-diff specification. Column (2) includes the following village controls: population size and proportion of households with no electricity. Column (3) interacts the village controls with treatment dummy and with year dummy.* p<0.1, ** p<0.05, *** p<0.01