THE EFFECT OF A LARGE EXPANSION OF PRE-PRIMARY SCHOOL FACILITIES ON PRESCHOOL ATTENDANCE AND MATERNAL EMPLOYMENT

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Abstract: We provide evidence on the impact of a large construction of pre-primary school facilities in Argentina. We estimate the causal impact of the program on pre-primary school attendance and maternal labor supply. Identification relies on a differences-in-differences strategy where we combine differences across regions in the number of facilities built with differences in exposure across cohorts induced by the timing of the program. We find a sizeable impact of the program on pre-primary school participation among children aged between 3 and 5. In fact, we cannot reject the null hypothesis of a full take-up of newly constructed places. In addition, we find that the childcare subsidy induced by the program increases maternal employment and that this effect is in line with the one previously found for the US.

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1. Introduction

Preschool education is politically and socially at the forefront right now. In the US, universal preschool for children between 3 and 5 years of age is at the vanguard of the educational policy agenda. This is motivated by the existing evidence about the long-term benefits of early childhood development and early education programs (see, among others, Currie, 2001; Heckman and Carneiro, 2003; Shonkoff and Phillips, 2000 and Shore, 1997). At the same time, public interest in childcare subsidies is quite high. For women with young children, maternal labor force participation and childcare are jointly determined. Childcare may represent a substantial cost of employment and it is seen as an obstacle towards labor force participation (see, for example, Blau and Currie, 2003 and Jaumotte, 2003). Public (or subsidized) pre-primary school (school based early childhood education) is seen as a potential solution to this problem.

Preschool attendance in the 3-5 age group is still far from universal even in developed countries. Recently, promoted by heavy public investment, the provision of pre-primary education has increased rapidly in many OECD countries. For example, in Portugal, a significant expansion of the public preschool network during the nineties correlates with a large and rapid increase in coverage for children over 3 (OECD, 2002). Nevertheless, this evidence is not causal, and the question of whether investment in infrastructure can cause large increases in pre-primary school attendance remains unanswered. This is specially so for developing countries, where lack of demand, especially among the poor, might be the reason behind low preschool enrollment.

In this paper, we rely on a dramatic policy experiment to provide new evidence on the impact of a large construction program of pre-primary school facilities on enrollment and maternal labor market behavior in a middle-income and predominantly urban developing country. In 1993, the Federal Ministry of Education of Argentina started a large infrastructure program aimed at expanding school attendance for children aged 3-5. Between 1994 and 2000 the construction program created approximately 175,000 preschool places. This represented an 18 percent increase over baseline pre-primary school enrollment in Argentina.

The construction program attempted to compensate geographically existent differences in enrollment rates by differentially expanding pre-primary school facilities. Conditioning on region and cohort fixed effects this political experiment generates plausible exogenous variability in the supply of school facilities (see Rosenzweig and Wolpin, 1988). Similarly to Card and Krueger (1992) and Duflo (2001), among others, we exploit the variation in treatment intensity across regions and cohorts to identify the effect of expanding pre-primary school facilities on school attendance and maternal labor supply.

Does investment in infrastructure increase human capital in developing countries? The answer to this question is central to policy decision-making. In developing countries, there is evidence that the availability of schooling infrastructure correlates positively with school enrollment. However, it might well be that, at the margin, school attendance is more constrained by demand factors than by supply ones. This might especially be the case among poor families (see, among others, Myers, 1995). Indeed, most scholarship programs, particularly in Latin America, are based on this presumption (see, for example, Schultz, 2001). However, recently, Duflo (2001) provides causal evidence on the positive impact on schooling of a large primary school construction program in Indonesia. In this paper, we find a large impact of expanding infrastructure on preschool enrollment. In fact, we cannot reject the null hypothesis of a full take-up of newly constructed places.

Our estimates suggest that pre-primary school construction induces a large increase in enrollment for the 3-5 age group. Therefore, it is natural to wonder about its impact on maternal labor market behavior. Thus, we also investigate the effect of the program on maternal labor supply. The parameters we study, however, differ from those of standard research in the childcare and female labor supply literature. In these studies, the response of childcare use and female labor market participation to childcare costs is estimated by measuring the latter by either observed household expenditures or area-level averages of prices or expenditure (see, for example, Blau and Robins, 1988; Connelly, 1992; and Kimmel, 1998). In the absence of credible instruments to recover these structural parameters, however, identification has proved difficult because of measurement error and simultaneity (see Browning, 1992).

Our approach is closer to the one in Gelbach (2002). He evaluates the labor supply effects of the implicit childcare subsidy generated by free kindergarten for five-year-old children in public schools in the US. This study exploits variation in quarter of birth and the fact that all states in the US impose a date-of-birth requirement for entry to kindergarten to identify the parameter of interest. The instrumental variable estimates reported by Gelbach (2002) indicate that access to free public school increases the employment probability of mothers whose youngest child is aged five and that this effect appears to be large.

In this paper, we also study the labor supply effects of free public school subsidies by exploiting the change over time in the supply of free pre-primary schools induced by the construction program. Our identification strategy relies on the fact that the changes in the stock of school facilities in a given province and time are likely to be uncorrelated with the unobserved characteristics that jointly determine pre-primary school attendance and female labor market outcomes. We find that the program has a positive and statistically significant effect on employment.

The rest of the paper is organized as follows. In Section 2, we introduce the basic features of the construction program, background facts about the educational system and the labor market in Argentina as well as the data used in the empirical analysis. In Section 3, we discuss the empirical methodology. In Section 4, we present the results, and finally, in Section 5, we present our conclusions.

2. Background, program information and data

2.1. Background and program information

Argentina is a middle-income and predominantly urban developing country. In 1994, its GDP per capita was approximately \$ 6.000 and the United Nations Human Development Index ranked it in the 34th place. The literacy rate is 97 percent. The country has an area of 2,780,000 km^2 and a population of 36,123,000 people. About 90 percent of the population lives in urban areas. In 1998, public expenditure in education represented 4.1 percent of GDP.

Until 1994 Argentina can be described as a relatively low unemployment country with the unemployment rate never exceeding the 10 percent barrier. However, unemployment increased substantially after a macroeconomic shock in 1995 with an average rate of 14.5 for the rest of the nineties. Annual hours worked are high and female participation is at Southern European level. In 1998, the female employment rate for the group aged 18 to 49 was 48 percent (see Galiani and Hopenhayn, 2003).

Overall, the Argentine labor market is not very rigid. Tax rates in Argentina are comparable to those in a typical non-European OECD country. Unions are an important feature of economic life with around half the workers having their wages bargained collectively and 45 percent of employees being union members. However, National minimum wages are set at a relatively low level and probably do not have much impact on employment. Finally, employment protection is at about the average OECD level (see Galiani and Nickell, 1999).

The country is federally organized in 24 autonomous political jurisdictions (23 provinces and the Autonomous City of Buenos Aires). Responsibility for pre-primary and primary education has been decentralized at the provincial level since 1978. Both free public schools and private institutions that charge fees to students supply education. In general, public schools operate in two shifts (morning and afternoon) with preschoolers attending school during a 180 days school term, for three and a half hours a day, five days a week. Pre-primary education is divided into three levels: level 1 (age 3), level 2 (age 4), and level 3 (age 5). The two main factors that determine the allocation of preschool vacancies across applicants is the distance to the school and whether any siblings attend the school.

Primary school starts at age 6 and has been compulsory since 1885. The Federal Education Law of 1993 made compulsory both attendance to level 3 of pre-primary education and the first two years of secondary school. The Federal Education Pact signed later on in 1993 stated that implementation should occur gradually between 1995 and 1999. However, the new compulsory rule has not been enforced. First, there is no penalty in place for non-compliers. Second, primary school enrollment is not impeded by lack of pre-primary schooling. Finally, there are still large dropout rates at ages 13 and older.

Argentina has a long tradition of public education with an effective process of primary school enrollment consolidated after the second half of the last century. Table 1 presents enrollment data for pre-primary and primary school by province from the 1991 and 2001 population censuses. In 1991, the gross enrollment rate for the three levels of pre-primary education was 49 percent. This rate exhibited a lot of variability, with participation as high as 80 percent in the Autonomous City of Buenos Aires and as low as 27 percent in Chaco. The growth in enrollment by 2001 is noticeable. The average enrollment rate increased to 64 percent and the number of children attending pre-primary school by 330,845. Comparing 1991 to 2001, all provinces increased gross enrollment in pre-primary education by at least 10 percentage points. In contrast, primary school enrollment is universal during this period increasing only from 97 percent in 1991 to 98 percent in 2001.

A large public school construction program supported the increase in enrollment of the nineties. As a consequence of the commitments established by the Federal Education Law and the Federal Education Pact, the National Ministry of Education financed the construction of rooms for pre-primary education across the country. From 1993 to 1999, the Federal Government financed the construction of 3,531 rooms. On average, each room has 45 square meters and an estimated cost of \$ 15,000 pesos.[1] Most of the rooms constructed are in preschool annexes of public primary education institutions. If we consider an average class size of 25 students for each room and the fact that most public preschools operate in 2 shifts, the construction program created 176,550 potential places during that period.

In Table 2, we present the total number of places per child in preschool age constructed over the 1993-1999 period in each province and the share of each province on total construction. The correlation between these figures and the pre-primary school enrollment rate at ages 3-5 in 1991 is -0.68 and -0.53 respectively, which shows that the program was compensatory in nature. This is concordant with information we gathered in interviews with government officials regarding the allocation rule used by the Ministry of Education. According to them, the government used an allocation rule based on an index of unsatisfied basic needs constructed with data from the 1991 Population Census. We must also point out that the share of rooms received by each province during the first four years of the program, where more than 85 per cent of the construction was done, was fairly stable.

2.2. Data

We use data from the Argentine household survey *Encuesta Permanente de Hogares* (EPH) that is representative of 70 percent of the urban population of Argentina. The survey is conducted since 1974 in the main urban agglomerates of each province of the country (with the exception of Rio Negro[2]) and the Autonomous City of Buenos Aires. We pool repeated cross-sections of individual level data from the May waves of the survey covering the 1992-2000 period. However, before 1994, the information on enrollment for children in pre-primary school age is incomplete and unreliable for many agglomerates. This is due to the fact that the data collection is decentralized at the province level and the National Statistical Agency did not publish information on school enrollment prior to this year. Thus, the information on this variable is unavailable for most provinces before this year.[3]

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[1] At the time of construction the exchange rate was pegged one to one with the US dollar.

[2] Urban Rio Negro was only incorporated to the survey in 2001. See, www.indec.gov.ar for detailed information on the Argentine Household Survey (EPH).

[3] For each urban agglomerate two types of databases could be collected and made available to researchers: Users databases (BU) and reduced databases (R2). BU contains all the information collected by a standard household survey while R2 only contains a reduced set of questions that include employment and hours worked but does not include school attendance information for individuals in pre-primary school ages –i.e., the information is always missing. Before 1994, a large

We construct a sample of households with mothers aged 18-49 and at least one child between 3 and 5 years of age. The unit of observation is the mother[4]. In Table 3, we define the variables used in the paper and their source. For the period 1994-2000, we have a sample of 29,817 mothers with information both on school enrollment for children in pre-primary school age and employment. In Table 4, we present descriptive statistics for this sample of mothers. We divide the sample by whether 50 percent or more of the children aged 3-5 in the household attend pre-primary school. We present means and test of differences in means for labor market outcomes and observed household and mother's characteristics.

We find that maternal employment and hours worked are higher for mothers in households where more than 50 percent of the children attend pre-primary school. Concurrently, mothers who are more likely to enroll their children in pre-primary school differ in observable characteristics. For example, they are older, more skilled and have fewer children. These findings show that households self-select into pre-primary education and employment based on observable characteristics and suggest that they might also do so based on unobserved ones. Thus, observed correlations between preschool enrollment and maternal employment should be treated with caution as they can be both caused by unobserved factors.

3. Empirical strategy

We seek to evaluate the causal effect of the construction program on pre-primary school enrollment and maternal labor supply. We measure exposure to treatment for child i aged 3-5 residing in province j in period t by the accumulated stock of preschool places constructed (stock of preschool rooms constructed x 50[5]) in province j between 1993 (i.e., when the

number of databases are R2 (more than 30 percent of urban agglomerates) and only since 1996, all datasets are BU. This severely constraints the information we have on pre-primary school attendance for the years 1992 and 1993.

[4] For each household, the survey collects information on the family relationship between household members and the head of household. Our analysis focuses only on households with children of the head of household because only in such households the mother of a child can be identified.

[5] This is because there is an average class size of 25 students and each room can be operated in two shifts per day.

construction program started) and year t-1.[6] We normalize this stock dividing it by the size of the respective age cohort in that province. Thus, for example, treatment exposure for child *i* in province *j* in year 1996 is given by the sum of preschool places constructed in 1993, 1994 and 1995 in that province divided by the number of children aged 3-5 in that year. We denote this variable $Stock_{jt}$.

Improved access to free public pre-primary education constitutes an implicit childcare subsidy. This implicit subsidy induces a kink in the household budget constraint and generates both price and income effects (see Burtless and Hausman, 1978 and Gelbach, 2002). Nevertheless, under normal circumstances, it can be deduced from simple utility maximizing behavior that a more intense exposure to the program should cause an increase in pre-primary school attendance. However, theoretically, the causal impact of the program on maternal labor outcomes (employment and hours worked) is generally ambiguous because of the offsetting forces of price and income effects.

We start by estimating the impact of the construction of pre-primary school facilities on pre-primary school enrollment. Identification of the parameter of interest relies on the compensatory differential intensity of program expansion across provinces and the differences in exposure across cohorts induced by the timing of the program. Thus, in estimating the causal effect of the program on enrollment we face the traditional problem of identifying the effects of a compensatory intervention (see, Rosenzweig and Wolpin, 1988). As shown in section 2.1, the allocation rule of the program was systematically related to pre-treatment preschool attendance, and hence, to the determinants of it. Thus, without controlling for these regional characteristics, our estimates are likely to be biased downwards. However, a standard way to circumvent this problem is to condition on region fixed effects. In addition, we also condition on year fixed effects to control for common trends in preschool enrollment.

More formally, we estimate the following model by Ordinary Least Squares (OLS):

[6] This is because the school year in Argentina goes from March to December. Thus, constructions in year t only are accessible in year t+1.

$$A_{ijt} = \alpha_{10} + \alpha_{11} X_{ijt} + \alpha_{12} z_{jt} + \beta_1 Stock_{jt} + \mu_{1j} + \lambda_{1t} + \varepsilon_{ijt} \qquad (1)$$

where A_{ijt} measures the proportion of children aged 3-5 attending pre-primary school in household *i,* province *j,* and period *t;* X_{ijt} is a vector of exogenous household characteristics, z_{jt} is a vector of time-varying province variables, $Stock_{jt}$ is the causing variable of interest, μ_{1j} is a region fixed-effect, λ_{1t} is a year effect common to all provinces in period *t,* and ε_{ijt} is a household specific error assumed to be distributed independently across provinces and independently of all μ_{1j} and λ_{1t}.

The parameter of interest is β_1, which captures the average effect of an extra place per child aged 3-5 on pre-primary school enrollment. In theory, if households are only constrained by the lack of public preschool places and there is perfect take-up, β_1 should be equal to one. It must be pointed out that we do not need to include z_{jt} and X_{ijt} in the model in order to identify the parameter of interest. They are included either to increase efficiency or to check the robustness of our results to the fixed effects assumptions. This is to say that, neither changes in the composition of the population over time or province idiosyncratic trends are systematically correlated with exposure to treatment.

We also estimate the causal effect of the childcare subsidy induced by the program on maternal labor supply. This exercise addresses the question of whether subsidies in the form of limited, directly provided care influence maternal labor supply. In order to estimate this parameter, we fit regression functions of the following form (using similar notation):

$$Y_{ijt} = \alpha_0 + \alpha_1 X_{ijt} + \alpha_2 z_{jt} + \beta_2 Stock_{jt} + \mu_{2j} + \lambda_{2t} + \omega_{ijt} \qquad (2)$$

Where Y_{ijt} is one of the following measures of maternal labor supply: a dummy indicator for employment status or weekly hours of work.

The outcomes of interest in this case are limited dependent variables. However, as noted in Angrist (2001), the problem of causal inference for these variables is not fundamentally different from the problem of causal inference with continuous outcomes. If there are no covariates or the covariates are sparse and discrete, linear models are no less appropriate for limited dependent variables than for other types of dependent variables. Certainly, this is likely to be the case in policy experiments where control variables are mainly included to improve the efficiency of the estimates but their omission would not bias seriously the estimate of the parameter of interest.

The advantage of estimating regression function (2) by OLS is that we directly estimate the parameter of interest. Alternatively, model (2) can be interpreted as a linear approximation to the true conditional expectation function, and in the case of dichotomous outcomes, as a linear probability model. In any case, in order to check that the OLS estimates are robust to the specification of the conditional expectation function, we also report estimates of the average impact of a marginal change in *Stock* on the expectation of the observed outcome of interest after estimating both Probit and Tobit models respectively.[7]

The errors in equations (1) and (2) vary at the mother, province, and year level. As it is standard, we assume that they are independently distributed of μ_j and λ_t (see Chamberlain, 1984). These errors, however, might be correlated across time and space. Error correlation could be present in the cross-sectional dimension of the panel because factors affecting a household in one province could affect other households in the same province. Also, the persistence of regional traits could induce time-series correlation at the province level. We take two approaches to avoid potential biases in the estimation of the standard errors. First, we compute standard errors clustered at the province-year level. Second, we allow for an arbitrary covariance structure within provinces over time by computing our standard errors clustered at the province level. However, the latter is quite a stringent requirement to our sample given that there are only 23 jurisdictions in our dataset and the asymptotically validity of these cluster robust standard errors might not hold.

[7] These are the parameters of interest, and hence, the ones that are directly comparable to those recovered by OLS.

Finally, after estimating equations (1) and (2), we interact the treatment variable with marital status and presence of children less than 3 years old. These dimensions mediate the behavioral response of the variables under study to the expansion of the program because they are likely to affect maternal home and market productivity. There is evidence from the US that the type of subsidy implied by free public pre-primary school might vary according to marital status and presence of younger siblings.

4. Results

4.1. The impact of the construction program on pre-primary school attendance

In Table 5, we analyze the impact of the construction program on pre-primary school attendance in households with at least one child aged 3-5. The dependent variable is the proportion of children between 3 and 5 years of age in the household that attend pre-primary school. The intensity of the program is measured by the variable *Stock*. The first set of standard errors we report are clustered at the province and year level. The second set is clustered at the province level.

In the first column of Table 5, we only condition on year effects. As we expected, without conditioning on region fixed effects the program seems to have a large negative and statistically significant impact on preschool enrollment. This is a consequence of the compensatory nature of the program. In the second column of Table 5, we condition on agglomerate and year effects.[8] In Column (2), the point estimate of 0.824 indicates that one place constructed per child in preschool age increases the likelihood of pre-primary school attendance by 0.842 percentage points. Moreover, we cannot reject the null hypothesis that the effect is different than one. This is to say, we cannot reject the null hypothesis of full take-up of vacancies.

[8] We condition on agglomerate fixed effects instead of province fixed effects because we have an unbalanced panel of urban agglomerates and conditioning on province fixed effects may bias our estimates. In particular, there are 3 urban agglomerates (i.e., Mar del Plata in the Province of Buenos Aires; Concordia in the Province of Entre Rios and Rio Cuarto in the Province of Cordoba) that are incorporated to the survey in 1995. It is worth noting, that none of the results of this paper are changed if we exclude these 3 urban agglomerates from the sample and we condition on province fixed effects.

Given that the average number of places constructed per child over the period was 0.09, the average increase in the probability of pre-primary school attendance as a consequence of the program is approximately 7.5 percentage points. Hence, the program explains about half of the 15 percentage point's increase in gross enrollment experienced from 1991 to 2001. The remaining 7.5 percentage points are explained by cohort effects and time-varying idiosyncratic province factors. We have already dealt with the cohort effects by including the year dummies. In the rest of this section, we show that our results are extremely robust to the inclusion of variables that may capture province idiosyncratic trends and could be systematically related to the program.

In Column (3), we allow for idiosyncratic trends in province enrollment levels in pre-primary education. As in Duflo (2001), we do this by interacting the 1991 pre-primary enrollment rate for the 3-5 age groups in each province with year dummies. Given that different provinces start with different enrollment rates, we may suspect that they naturally grow at different rates and these trends can be systematically correlated with the construction program. For example, in provinces with low rates of enrollment before the program, attendance may naturally grow faster as they converge to the rates in provinces with higher enrollment. If these trends are systematically correlated with the pace of the construction program they will bias upwards our estimates. Reassuringly, the added interaction term is not statistically significant and the point estimate is not affected significantly. Indeed, a Hausman test rejects the null hypothesis that the estimate in Columns (3) is statistically different than the one in Column (2).

In Column (4), we add dummies for the age of the mother and her skill level to the basic fixed effects model. In Column (5), we also condition for the structure of the household: presence of children less than 3 years of age, presence of children aged 6-18, number of children less than 19 years old living in the household, presence of a spouse, and number of other adults in the household. On the one hand, if the fixed effects assumptions are correct, these variables will improve the efficiency of the estimates by reducing the standard error of the regression. On the other hand, they provide a robustness check to the assumptions that there are no systematic changes in household composition across provinces that are correlated with the program. In each column, the education and age variables and the household structure variables are jointly statistically significant determinants of pre-primary school attendance. However, a Hausman test rejects the null hypothesis that the estimates in Columns (4) or (5) are systematically different than the one in Column (2).

In Column (6), we include yearly measures of province unemployment and real GDP per capita. Although the allocation rule of the program is related to pre-treatment traits, as we explained in Section 2, it may have been the case that the pace at which the rooms were allocated was somehow related to macroeconomic conditions that may affect pre-primary school attendance and maternal labor supply. Also, it could have been the case that enrollment increased as a consequence of raising provincial income and this is correlated to the program. These new conditioning variables, however, do not have a jointly statistically significant effect on pre-primary school attendance. As a consequence, the point estimates for the impact of school construction on pre-primary school attendance are not affected by their inclusion. All in all, Columns (3) to (6) show that the benchmark fixed effects estimate is robust to the controls we include in the regression and that the relation between school construction and pre-primary enrollment can be safely interpreted as causal.

Given that the implicit subsidy of public preschool is not targeted to a population in particular, we may wonder whether the program has a differential effect in some demographic groups. In Table 6, we explore whether the presence of a spouse in the household (Column (1)) and the presence of any children under the age of 3 in the household (Column (2)) generate differences on the impact of the program. Taking as a benchmark the model in Column (5) of Table 5, where we condition on year effects, agglomerate effects, age and skill level of the mother and household composition, we cannot reject the null hypothesis that the treatment effect is homogenous across the groups defined above. In other words, the take-up rates induced by the expansion of the program for the groups in which we divided the population are proportional to the share of that group in the total population.[9]

[9] The interpretation of the coefficients in this Table is different from the one for those in Table 5. Note that each set of interaction dummy variables partitions the population of mothers in different subpopulations. In order to produce sets of coefficients that should theoretically add to one, and

4.2. The impact of the construction program on maternal labor market outcomes

For women with young children, maternal labor force participation and childcare are jointly determined. Our estimates suggest that the construction of pre-primary school facilities induces a large increase in preschool attendance for children aged 3-5. Thus, it is natural to ask, what impact does the program have on maternal labor supply? In Table 7, we study how differences in exposure to the program, measured by the variable *Stock*, affect maternal employment and weekly hours of work. In other words, we study the causal effect on maternal labor market outcomes of increasing public school facilities in their area of residence. We condition on time fixed effects, agglomerate fixed effects, mother's skill level and age, and household composition but our results are robust to excluding these variables from the model.

In Table 7 we report both OLS estimates and the marginal effect of Probit/Tobit estimates for employment/hours.[10] In Columns (1), we use data for the period 1994-2000 only.[11] The point estimates are positive and large although are not significant at conventional levels of statistical significance. A problem in interpreting these results is that the standard errors are fairly large (almost twice the point estimates). Nevertheless, it is worth noting that these point estimates suggest an effect of pre-primary school attendance similar to the one estimated by Gelbach (2002) for the US. His instrumental variables estimates imply that public school enrollment increase the likelihood of maternal employment in 5 percentage points while our fixed effects estimates suggest that if we increase the stock of rooms from 0

have the same interpretation of those in Table 5, it is necessary to divide the stock of new rooms by the cohort of children corresponding to each of the subpopulations in which the set mothers is divided instead of dividing it by the whole cohort of children. However, we are not interested in the distribution of take-ups as a result of the program among subpopulations but on whether or not the effect is homogenous across groups.

10 Marginal effects are computed for the expected value of the dependent variable. The standard errors for the Tobit models are bootstrap standard errors using 100 replications.

11 The number of observations is larger than in Tables 5 and 6 because we do include the agglomerates for which R2 databases are available –i.e. those databases without information on pre-primary school attendance (see footnote 3). The point estimates are lower if these observations are excluded from the sample even though a Hausman test does not reject that they are equal.

to 1, and there is a full take-up of the new places, the likelihood of maternal employment would increase in 7 percentage points.[12]

Clearly, we do not have enough power to conduct the exercise reported in Column (1) since even if the point estimates were twice as large as they are, we would not find them to be statistically significant at conventional levels. In order to overcome this nuisance, in Column (2) we add into the analysis the available pre-treatment observations for the years 1992 and 1993. These observations should add statistical power since they increase the sample by more than 30 percent. Now, the standard errors are substantially smaller and we do not reject the null of absence of effect of pre-primary public school attendance on maternal employment at the 8.6 percent. Although the estimate is larger now, we still do not reject that the effect of the program on maternal employment is similar to the one estimated for the US. Thus, our fixed effects estimates suggest that if we increase the stock of rooms from 0 to 1, and there is full take-up of the newly constructed places, the likelihood of maternal employment would increase between 7 and 14 percentage points

Columns (3) and (4) report the impact of the program on hours worked in a given week of reference. The point estimates are perfectly in line with the estimates in Columns (1) and (2) where we find that an increase in *stock* from 0 to 1 would raise the likelihood of maternal employment between 7 and 14 percentage points. Given that the average number of hours worked per week in our sample is 32, a back of the envelope calculation suggests that we should estimate an increase in hours worked in the range of 2.24 to 4.5 hours per week. Nevertheless, we should point out that our estimates are too imprecise as to focus too much in this outcome.

The household production model predicts that women will participate in the labor market when market productivity (net of childcare costs) exceeds home productivity. Among the prime factors that affect market productivity and the cost of childcare is the presence of young children while home productivity is largely affected by marital status. In Table 8, we

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[12] In general, the estimates are not strictly comparable since, under the presence of heterogeneous response, Gelbach's estimate identifies a local average treatment effect (see Angrist et al., 1996) while our estimates identify treatment on the treated.

explore whether the presence of a spouse in the household (Column (1)) and the presence of any children under the age of 3 in the household (Column (2)) generate differences on the impact of the program. We cannot reject the null hypothesis that the treatment effect is homogenous across groups.

5. Conclusion

We rely on an unusual policy experiment to provide new causal evidence on the impact of a large construction of pre-primary school facilities on pre-primary school attendance and maternal labor market behavior in a middle-income predominantly urban developing country. We identify the impact of the program by using a differences-in-differences estimation strategy. We find that the construction program has a sizeable impact on pre-primary school enrollment among children aged 3-5. The results are similar for households with and without spouses present, and with and without children younger than 3. For women with young children, maternal labor force participation and childcare are jointly determined. In fact, we also find that the childcare subsidy induced by the program increases maternal employment and that this effect is in line with the one previously found for the US.

Our findings have important implications for the design of public policy. First, the large impact of the program on preschool participation suggests that supply constraints may act as bottlenecks when it comes to investing in children human capital. Second, the expansion of free pre-primary education causes an increase in maternal employment.

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Table 1: Pre-primary and Primary School Participation in Argentina

	Primary School Gross Enrollment Rate: Age 7		Pre-primary School Gross Enrollment Rate: Age 3- 5		Pre-primary School Enrollment Level: Age 3- 5	
Province	1991	2001	1991	2001	1991	2001
Ciudad Autónoma de Buenos Aires	0.98	0.99	0.80	0.93	89,353	85,728
Buenos Aires	0.98	0.99	0.60	0.76	442,757	558,623
Catamarca	0.96	0.99	0.36	0.48	7,286	11,493
Córdoba	0.98	0.99	0.49	0.67	78,538	110,322
Corrientes	0.95	0.97	0.33	0.48	20,314	31,584
Chaco	0.89	0.96	0.27	0.40	17,857	30,137
Chubut	0.98	0.99	0.43	0.60	11,339	15,534
Entre Ríos	0.97	0.99	0.43	0.59	28,913	41,301
Formosa	0.95	0.98	0.31	0.42	10,365	15,964
Jujuy	0.97	0.99	0.34	0.50	14,023	21,882
La Pampa	0.97	0.99	0.38	0.49	6,297	8,175
La Rioja	0.97	0.98	0.44	0.62	7,169	12,468
Mendoza	0.97	0.99	0.36	0.50	33,583	46,089
Misiones	0.93	0.95	0.23	0.40	15,437	29,789
Neuquén	0.98	0.99	0.43	0.62	13,165	18,527
Río Negro	0.97	0.99	0.42	0.63	15,736	21,421
Salta	0.96	0.98	0.33	0.46	23,442	36,849
San Juan	0.97	0.98	0.34	0.50	12,025	19,577
San Luis	0.96	0.98	0.46	0.60	8,763	14,503
Santa Cruz	0.99	1.00	0.64	0.73	7,603	9,406
Santa Fe	0.98	0.99	0.52	0.72	86,246	112,520
Santiago del Estero	0.94	0.97	0.36	0.50	18,775	30,018
Tucumán	0.97	0.98	0.35	0.49	27,849	43,655
Tierra del Fuego	0.99	1.00	0.59	0.83	3,477	5,590
Total	0.97	0.98	0.49	0.64	1,000,310	1,331,155

Source: Population Census, 1991 and 2001.

Table 2: Share of Rooms Constructed and Places Constructed per Child in Preschool age by Province: 1993-1999

Province	Share of Total Rooms Constructed	Places Constructed per Child
Ciudad Autónoma de Buenos Aires	0.03	0.05
Buenos Aires	0.03	0.01
Catamarca	0.02	0.19
Córdoba	0.02	0.02
Corrientes	0.08	0.22
Chaco	0.09	0.23
Chubut	0.03	0.20
Entre Ríos	0.06	0.15
Formosa	0.04	0.21
Jujuy	0.05	0.20
La Pampa	0.02	0.17
La Rioja	0.03	0.28
Mendoza	0.07	0.13
Misiones	0.07	0.19
Neuquén	0.02	0.09
Río Negro	0.03	0.12
Salta	0.05	0.13
San Juan	0.07	0.33
San Luis	0.02	0.18
Santa Cruz	0.01	0.03
Santa Fe	0.08	0.09
Santiago del Estero	0.04	0.15
Tucumán	0.07	0.15
Tierra del Fuego	0.01	0.09
Total	1.00	0.09

Source: Ministry of Education.

Table 3: Definition and Source of Variables

Variable	Definition	Source
Preschool Attendance	Proportion of children age 3-5 that attend pre-primary education.	Household Survey
Mother's Employment	Binary variable. Equals 1 if woman is employed when the survey is conducted, 0 if she doesn't work - whether or not she is looking for employment.	Household Survey
Mother's Hours Worked	Weekly hours worked during the week the survey is conducted. = 0 if the woman is not working. Observations with more than 84 hours of work a week are considered missing.	Household Survey
Stock	Stock of preschool places constructed per child in the 3 to 5 preschool cohort in each province. We allocate the flow of rooms constructed in 1993 to the 1994 preschool cohort, the sum of the flow of rooms constructed in 1993 and 1994 to the 1995 preschool cohort, and so on. We multiply by 50 each preschool room to get the number of places created and we normalize by cohort size.	Ministry of Education and Census 2001
Mother's Age	Age at the time the survey is conducted. We only sampled mothers that are 18 to 49 years old.	Household Survey
Mother's Skills	Highest skill level. Binary variables. Household level data.	Household Survey
Unskilled	At most incomplete secondary education.	
Semi-skilled	At most incomplete tertiary education.	
Skilled	Complete tertiary education.	
Spouse Present	Binary variable. = 1 when the spouse is residing in the household at the time the survey is conducted. If a husband is present we restric the sample to husbands between 18 and 59 years of age.	Household Survey
Number of children under 19	Number of children below 19 years of age who are living in the household at the time the survey is conducted.	Household Survey
Number of other Adult Household Members	Number of adults above 18 years of age, other than the women and her spouse, that reside in the household at the time the survey is conducted.	Household Survey
Presence of children aged 6-18	Binary variable. = 1 when there are children age 6-18 residing in the household at the time the survey is conducted. Household level data.	Household Survey
No presence of children less than 3	Binary variable. = 1 when there are no children less than 3 years of age residing in the household at the time the survey is conducted.	Household Survey
Unemployment rate (%)	Unemployment Rate. It varies by province and period.	Ministry of Labor
Real GDP per capita	Provincial GDP per capita, deflated using national GDP deflator (base year: 1993). In thousands. It varies by province and period.	Ministry of Labor

Table 4: Descriptive Characteristics of Households With at Least One Child between 3 and 5 Years of Age

Variables	Means: All	Proportion of Children 3-5 Attending Preschool: ≤ 0.5	>0.5	t-test
Mother's Employment	0.387	0.360	0.431	-12.071***
Mother's Hours Worked	12.494	11.530	14.064	-10.944***
	(19.098)	(18.658)	(19.693)	
Mother's Age	32.078	31.394	33.182	-24.168***
	(6.305)	(6.303)	(6.149)	
Mother's Skills				
Unskilled	0.625	0.663	0.563	17.127***
Semi-Skilled	0.251	0.231	0.284	-10.332***
Skilled	0.124	0.107	0.152	-11.144***
Spouse Present	0.912	0.915	0.908	2.066**
Number of Children less than 19 Years of Age	3.049	3.166	2.861	16.671***
	(1.591)	(1.668)	(1.441)	
Number of other Adult Household Members	0.257	0.249	0.271	-2.539**
	(0.730)	(0.730)	(0.731)	
No Children less than 3 Years of Age	0.605	0.582	0.643	-10.541***
Presence of Children Aged 6-18	0.666	0.650	0.692	-7.319***
Observations	29,817	18,406	11,411	

Source: *Encuesta Permanente de Hogares,* May 1994-2000.
Notes: Standard deviations in parentheses. The t-test is a test of differences in means with unequal variances.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: The Impact of the Stock of Preschool Places per Child on the Proportion of Children Aged 3-5 per Household Enrolled in Pre-primary Education

	Dependent Variable: Proportion of Children Aged 3-5 that Attend Pre-Primary School					
	(1)	(2)	(3)	(4)	(5)	(6)
Stock	-0.433***	0.824***	0.951***	0.819***	0.845***	0.887***
	(0.145)	(0.310)	(0.287)	(0.305)	(0.305)	(0.294)
	[0.301]	[0.488]	[0.450]	[0.477]	[0.473]	[0.483]
P-value of F-test for Added Controls:						
Pre-treatment Enrollment Rate x Year			(0.6982)			
			[0.0422]			
Skill Level and Age				(0.0001)	(0.0001)	(0.0001)
				[0.0001]	[0.0001]	[0.0001]
Household Composition					(0.0001)	(0.0001)
					[0.0001]	[0.0001]
Provincial Unemployment and Real GDP per Capita						(0.4799)
						[0.2211]
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
Agglomerate Effects	No	Yes	Yes	Yes	Yes	Yes
Observations	29,817	29,817	29,817	29,817	29,817	29,817

Source: *Encuesta Permanente de Hogares* , May 1994-2000.
Notes: OLS regressions. Robust standard errors clustered at the year and province level (155 clusters) in parentheses and at the province level (23 clusters) in brackets. There are 6 Year dummies and 28 agglomerate dummies. The Skill Level and Age variables include: 2 skill dummies and 6 age dummies. The Household Composition variables include: a spouse present dummy, number of children less than 19 years of age, a dummy for the presence of any children 6-18, a dummy for households without children less than 3 years of age, and number of other adults in the household. GDP per capita and unemployment controls vary at the province and year level.
P-values in parentheses correspond to province-year clustered standard errors and in brackets only to province clusters.
* significant at 10%; ** significant at 5%; *** significant at 1%. All correspond to the standard errors in parentheses.

Table 6: The Impact of the Stock of Preschool Places per Child on the Proportion of Children Aged 3-5 per Household Enrolled in Pre-primary Education. Differences by Household Composition

	Dependent Variable: Proportion of Children Aged 3-5 that Attend Pre-Primary School	
	(1)	(2)
Stock x Spouse Present	0.850***	
	(0.305)	
	[0.471]	
Stock x Spouse Not Present	0.803**	
	(0.322)	
	[0.501]	
Stock x No Children less than 3		0.826**
		(0.320)
		[0.498]
Stock x Some Children less than 3		0.875***
		(0.289)
		[0.436]
P-value of F-test for Equality of Treatment Effects	(0.6870)	(0.5684)
	[0.6571]	[0.5935]
Year Effects	Yes	Yes
Agglomerate Effects	Yes	Yes
Skill Level and Age	Yes	Yes
Household Composition	Yes	Yes
Observations	29,817	29,817

Source: *Encuesta Permanente de Hogares* , May 1994-2000.
Notes: OLS regressions. Robust standard errors clustered at the year and province level (155 clusters) in parentheses and at the province level (23 clusters) in brackets. The conditioning variables are similar to those described in Table 5.
P-values in parentheses correspond to province-year clustered standard errors and in brackets only to province clusters.
* significant at 10%; ** significant at 5%; *** significant at 1%. All correspond to the standard errors in parentheses.

Table 7: The Impact of the Stock of Preschool Places per Child on Maternal Employment and Weekly Hours of Work

	Dependent Variable: Employment				Dependent Variable: Weekly Hours			
	(1)		(2)		(3)		(4)	
	OLS	Probit	OLS	Probit	OLS	Tobit	OLS	Tobit
Stock	0.074	0.084	0.124*	0.145*	0.985	1.455	2.026	3.535
	(0.137)	(0.164)	(0.072)	(0.085)	(4.882)	(4.496)	(2.999)	(3.369)
	[0.183]	[0.218]	[0.106]	[0.125]	[7.463]	[4.311]	[4.839]	[2.432]
Year Effects	Yes		Yes		Yes		Yes	
Agglomerate Effects	Yes		Yes		Yes		Yes	
Skill Level and Age	Yes		Yes		Yes		Yes	
Household Composition	Yes		Yes		Yes		Yes	
Observations	31,049		40,697		30,724		39,841	

Source: *Encuesta Permanente de Hogares* , May 1992-2000.
Notes: The results are from OLS, Probit and Tobit models. We report marginal effects for Probit and Tobit. All Standard errors are clustered at the year and province level (155 clusters) in parentheses and at the province level (23 clusters) in brackets. Columns (1) and (3) cover the period 1994 to 2000. Columns (2) and (4) cover the period 1992 to 2000. The conditioning variables are similar to those described in Table 5.
P-values in parentheses correspond to province-year clustered standard errors and in brackets only to province clusters.
* significant at 10%; ** significant at 5%; *** significant at 1%. All correspond to the standard errors in parentheses.

Table 8: The Impact of the Stock of Preschool Places per Child on Maternal Employment.
Differences by Household Composition

	Dependent Variable: Employment			
	(1)		(2)	
	OLS	Probit	OLS	Probit
Stock x Spouse Present	0.125*	0.148*		
	(0.071)	(0.085)		
	[0.105]	[0.125]		
Stock x Spouse Not Present	0.116	0.114		
	(0.129)	(0.146)		
	[0.170]	[0.196]		
Stock x No Children less than 3			0.164**	0.194**
			(0.082)	(0.095)
			[0.108]	[0.128]
Stock x Some Children less than 3			0.068	0.065
			(0.073)	(0.089)
			[0.121]	[0.146]
P-value of F-test for Equality of Treatment Effects	(0.9340)	(0.7848)	(0.1265)	(0.0909)
	[0.9460]	[0.8528]	[0.2646]	[0.2165]
Year Effects	Yes	Yes	Yes	Yes
Agglomerate Effects	Yes	Yes	Yes	Yes
Skill Level and Age	Yes	Yes	Yes	Yes
Household Composition	Yes	Yes	Yes	Yes
Observations	40,967	40,967	40,967	40,967

Source: *Encuesta Permanente de Hogares*, May 1992-2000.
Notes: The results are from OLS and Probit. We report marginal effects for Probit. Robust standard errors clustered at the year and province level (155 clusters) in parentheses and at the province level (23 clusters) in brackets. The conditioning variables are similar to those described in Table 5. P-values in parentheses correspond to province-year clustered standard errors and in brackets only to province clusters.
* significant at 10%; ** significant at 5%; *** significant at 1%. All correspond to the standard errors in parentheses.