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ABSTRACT

We study the long-run effects of a large public expansion of pre-primary education in Argentina. Between 1993 and 1999, the federal government financed the construction of new preschool classrooms targeted to departments with low baseline enrollment and high poverty, creating roughly 186,000 additional places. We link administrative records on classroom construction to four population censuses and estimate difference-in-differences models that compare treated and untreated cohorts across high-and low-construction departments. An additional preschool seat per child increases post-kindergarten schooling by about 0.5 years, raising the probability of completing secondary school by 11.9 percentage points and of attaining any post-secondary education by 7.1 percentage points. For women, access to the program also reduces fertility: an additional seat lowers the number of live births per woman by 0.20. Across several checks using cohort sizes, household composition, province-of-birth restrictions, and direct migration measures, we find little evidence consistent with systematic selective migration, suggesting that migration is unlikely to be a major source of bias in our estimates. Our results show little impact on labor-market outcomes at the census date, consistent with beneficiaries still being in school or in the early stages of their careers. A benefit-cost analysis based on the estimated schooling gains, standard Mincer returns, and observed construction and operating costs yields a benefit-cost ratio of about 11 and an internal rate of return of 13%. Our findings show that universal at-scale pre-primary expansions in middle-income countries can generate sizable improvements in human capital and demographic outcomes at relatively low fiscal cost.

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1 Introduction

Expanding pre-primary education is considered a good investment in a country's future as it is expected to improve children's cognitive and social skills, increase long-term educational outcomes, and create a more productive labor force (Berlinski & Schady, 2015; Currie & Almond, 2011; OECD, 2011).[1] There has been a large global expansion in pre-primary enrollment worldwide, with the global gross enrollment ratio increasing from 29% in 1990 to 61% in 2019 (UNESCO Institute for Statistics, 2024). Although there is substantial evidence of short-term benefits from these investments, an important question remains: do they pay off in the long term? This study examines the long-term effects of increasing universal pre-primary education in Argentina, where the pre-primary enrollment rate increased from 49% to 78% between 1990 and 2000.

In the 1990s, Argentina implemented a large-scale public school construction program to increase pre-primary education attendance. This initiative, carried out from 1993 to 1999, constructed new classrooms to accommodate approximately 186,200 additional children in pre-primary education. The government strategically targeted construction in more economically disadvantaged areas with low pre-primary enrollment rates. All of the new pre-primary places created by the construction program were quickly filled; Berlinski and Galiani (2007) estimate that the program itself raised the national pre-primary enrollment rate by about 7.5 percentage points. Students who gained access to preschool through this expansion performed better in third grade on standardized test scores and behavioral measures, including attention, effort, class participation, and discipline (Berlinski *et al.*, 2009).

We estimate the causal effect of the pre-primary school expansion on educational attainment, fertility, and labor-market outcomes for individuals aged 18 to 26 at the time of follow-up. Fertility outcomes are observed only for women. We implement a difference-in-differences identification strategy that compares high construction areas to low construction areas using data from several decennial population censuses and the location and intensity of the construction program in the universe of departments (over 500—roughly equivalent to a US county). Our results indicate that an additional pre-primary place per eligible child led to an increase of 0.5 years of post-kindergarten

[1] Pre-primary education refers to normal educational programs designed for children typically aged 3 to 6 years old.

education resulting in an 11.9 percentage point gain in the probability of finishing secondary school and a 7.1 percentage point gain in the probability of attaining any post-secondary education. Furthermore, an additional place led to a decline of 0.20 live births per woman. Our results are robust to several strategies to deal with differential pre-trends and alternative implementations of difference-in-differences models.

We perform a benefit-cost analysis (BCA) of constructing one new pre-primary classroom and estimate a benefit-cost ratio (BCR) of approximately 10.97 and an internal rate of return (IRR) of 13.44%. The high BCR reflects three features of this policy: relatively low construction costs (about $213 per seat-year), material impacts on completed schooling (0.5 years per additional seat), and a persistent life-cycle earnings gradient with schooling. Under a simple MVPF-style calibration based on the same Mincer earnings gains, the benefit-cost ratio implies that the program would be fiscally self-financing at plausible effective tax rates, placing it in the "win–win" region of the Marginal Value of Public Funds criterion. In other words, if the schooling gains translate into earnings at standard Mincer returns, projected tax revenue exceeds program costs.

These results are especially relevant for policymakers in developing and middle-income countries considering further investments in early childhood education. By demonstrating the potential for universal pre-primary education to improve long-term educational outcomes and influence demographic trends, our study offers valuable insights that can inform evidence-based policy decisions and contribute to broader discussions on human capital development and economic growth.

This paper makes four main contributions to the literature on early childhood education. First, we extend the evaluation of Argentina's preschool expansion from short-run test scores measured in third grade to long-run outcomes—educational attainment and fertility—observed 10–20 years after exposure. Second, by studying a large-scale, universal public expansion in a middle-income country, we show that at-scale pre-primary investments can generate long-run schooling gains that are comparable in magnitude to, albeit somewhat smaller than, those found for some targeted programs in high-income settings. Third, we provide novel evidence that such expansions can meaningfully reduce fertility among beneficiary women, an outcome that has received relatively little attention in the pre-primary expansion literature. Fourth, we implement a benefit-cost analysis based on these long-run outcomes and derive a policy-relevant internal rate of return for a universal preschool expansion in a developing-country context. Relative to the earlier evaluations of this same program in Berlinski and Galiani (2007) and Berlinski *et al.* (2009), our design exploits additional cohorts and a broader age window, uses a simple canonical two-group difference-in-differences framework with modern robustness checks, and shifts the focus from short-run test-score gains to long-run schooling and fertility effects.

Our research contributes to the relatively small literature on the long-term impacts of at-scale expansions of public pre-primary education. Behrman *et al.* (2024) analyze Mexico's universal preschool mandate and document sustained gains in educational attainment nearly two decades after reform. In high-income settings, DeMalach and Schlosser (forthcoming) study Israel's preschool law and find improvements throughout the school cycle with substantially higher post-secondary enrollment and reductions in male juvenile delinquency and early marriage among females. Havnes and Mogstad (2011) show that Norway's nationwide expansion of subsidized child-care raised educational attainment and improved adult outcomes. Gray-Lobe *et al.* (2022) exploit Boston's admission lotteries to show higher high-school graduation and college attendance. Cascio (2009) finds that the introduction of state-funded public kindergartens reduced high-school dropout and institutionalization among white children, without any corresponding gains in employment or wages. Baker *et al.* (2019) find that children exposed to Quebec's universal childcare program showed persistently worse outcomes as young adults, including lower health and life satisfaction, higher criminality, and greater welfare dependence.

This study is most similar to Behrman *et al.* (2024), who also study a pre-primary school expansion at the national level in a middle-income country. They exploit the 2002 Mexican reform that mandated three years of preschool before entering primary school and employ a regression-discontinuity approach to investigate the impacts of the mandate on educational outcomes. Relative to Behrman *et al.* (2024), we analyze a construction-driven expansion in a difference-in-differences design and examine demographic outcomes alongside schooling.

Other at-scale expansions of pre-primary school have been evaluated on short- or medium-term outcomes rather than the long-term outcomes emphasized here. For instance, Uruguay's expansion shows gains by about age 16 (Berlinski *et al.*, 2008). Spain's expansion for three-year-olds yields effects measured at age 15 (Felfe *et al.*, 2015). Germany's expansion shows heterogeneous effects with small averages (Cornelissen *et al.*, 2018). Universal pre-K programs in the US at the state level generate sizable immediate test-score gains, especially for low-income children (Cascio & Schanzenbach, 2013).

There is also related literature on the effects of improving preschool quality. In Colombia, government-led (but not universal) quality improvements indicate that adding teacher training to teaching assistants improves child development, while hiring assistants alone does not (Andrew *et al.*, 2024). In India, early stimulation for toddlers combined with an enhanced preschool program for three-plus-year-olds raises IQ and school readiness in the short run (Meghir *et al.*, 2023).

Our study of universal pre-primary education is also related to the literature on the long-term effects of small-scale or targeted preschool programs. Canonical examples of such interventions include the Perry Preschool Program and the Abecedarian Project (Campbell *et al.*, 2002; Heckman *et al.*, 2013). In addition, our analysis relates to the long-run evidence on the impacts of Head Start documented by Garces *et al.* (2002) and Bailey *et al.* (2021).

Our fertility analysis is grounded in the well-documented causal link from education to delayed and reduced fertility. Quasi-experimental studies show that increases in schooling shift births to later ages and reduce early childbearing (Black *et al.*, 2008; Breierova & Duflo, 2004; Monstad *et al.*, 2008; Osili & Long, 2008). Against this backdrop, we ask whether Argentina's pre-primary expansion, by raising secondary completion and post-secondary attainment, reduced fertility among beneficiary women at census ages.

The remainder of the paper is organized as follows. Section 2 describes the census data, the pre-primary classroom construction program, and the identification strategy. Section 3 presents the estimated effects on educational attainment, fertility, and labor-market outcomes. Section 4 reports the benefit-cost analysis and the MVPF-style fiscal calculation. Section 5 presents the robustness exercises, including alternative difference-in-differences estimators, checks for selective migration, the birth-date cutoff design, and the administrative-data analysis of fertility. Section 6 concludes.

2 The classroom construction program

2.1 Census data

This study relies on data from the Argentine Census (*Censo de Población, Hogares y Viviendas*), carried out in 1980, 1991, 2001, and 2010. It contains information on educational, demographic, and labor-market individual-level variables. We focus on the 2010 wave, when beneficiaries of the construction program were aged 18 to 26. Fertility variables are available only for women. Appendix Table A2 provides definitions and descriptive statistics for the main variables we use from the censuses. We use the Census's extended questionnaire sample, which collected more information than the basic questionnaire sample. This extended questionnaire was applied to about 16.7 million people (42% of the population of Argentina), with samples from medium and large cities as well as the full population in rural areas and small cities and towns. The estimates presented below are based on weights that make the results representative of the country's population. Appendix D compares the weighted sample with the total population and establishes their equivalence.

2.2 Argentine pre-primary education and the construction program

Pre-primary education in Argentina, called "initial level" or "initial education", is intended for children aged 3–5 and is structured in three school years (designated years 3, 4, and 5). Its main objectives are to complement education at home and to foster the cognitive and non-cognitive skills needed for primary school. It typically operates in two shifts (morning and afternoon), each lasting about three and a half hours, from Monday to Friday for nine months a year. In 1993, the Federal Education Law (LFE) was enacted to make the last year of initial education compulsory and to universalize access to the first two years.[2]

Within the framework of Argentina's federal system, the 1993 regulatory changes were not immediately implemented by all provinces, since in many of them the supply

[2] The structure and objectives of early education detailed here are those established by Federal Education Law No. 24,195, enacted in 1993. Although this law has been repealed, this section refers to it because it was in force during the implementation of the policy analyzed, and its replacements did not establish significant changes at the early education level.

of education services was insufficient to meet the new mandates. Therefore, the national government undertook a massive infrastructure construction program for the pre-primary level, which involved financing a total of 3,724 classrooms between 1993 and 1999. Considering that each classroom has an average capacity of 25 children and would operate in two shifts, this meant some 186,200 new places at the pre-primary level. Total pre-primary enrollment, reflecting all sources of expansion and not only this construction program, rose by at least 10 percentage points in every province between 1991 and 2001 (Berlinski *et al.*, 2009).

The location of the new classrooms constructed under this policy was not arbitrary but was based on an Unsatisfied Basic Needs (UBN) index constructed with data from the 1991 Census, with the objective that the policy would mainly benefit the most disadvantaged areas with low enrollment rates. Indeed, there is a clear negative correlation between the total number of classrooms built and the pre-primary gross enrollment rates of children in 1980, as can be seen in Figure 1a, which presents the levels of enrollment by local area (called "departments") in the pre-primary level in 1980 and the number of student places created per child at the pre-primary level. The same correlation is present when we instead consider measures of poverty and human development at the department level, such as the Census's UBN index (a basic multi-dimensional poverty measure).[3]

Figure 2 maps the cumulative rollout of these new pre-primary places across departments and years. The figure shows substantial heterogeneity in exposure: some departments received no new places, while others experienced sizable increases in capacity relative to the local population of 3- to 5-year-old children in 1991. This geographic variation, including within-province differences, is the basis for our treatment definition and helps illustrate the targeted nature of the construction program.

Additionally, the Census data allows us to document a clear increase in pre-primary enrollment pre- and post-construction program. Figure 1b presents the distribution of gross pre-primary enrollment rates at the department level for 1991 (pre-treatment) and 2001 (post-treatment). The distribution shows a substantial shift to the right, indicating an increase in pre-primary enrollment. Averaging across departments rather than taking the provincial minimum, the mean increase in departmental pre-primary

[3] Additional results available upon request.

enrollment over the same 1991–2001 window was about 15 percentage points (pp.).

2.3 Identification strategy

Our identification strategy exploits cross-department variation in construction intensity interacted with cohort exposure. Placement was not random: as documented in Section 2.2, construction was deliberately targeted toward departments with lower baseline pre-primary enrollment and higher unsatisfied basic needs. These characteristics are observed at baseline. Department fixed effects absorb time-invariant differences across departments, cohort-by-province fixed effects absorb province-specific cohort shocks, and cohort-specific interactions with baseline pre-primary enrollment allow differential cohort patterns along the enrollment margin that guided placement. Identification therefore requires parallel trends conditional on these controls rather than exogenous placement, an assumption for which we provide extensive supporting evidence in Sections 3 and 5. Our results are based on measures of exposure to the construction program for individuals in the 2010 Census by department, which we construct from information provided by the Secretariat of Infrastructure of the Argentine National Ministry of Education. Due to the characteristics of the school calendar (which runs from March to December in Argentina), the new rooms built in a given calendar year only became available in the following year. Thus, the total number of new pre-primary places available in year t is the number built from the beginning of the program in 1993 until year $t-1$. This is the strategy used to measure the intensity of the construction program by department in the previous literature on this program (Berlinski & Galiani, 2007; Berlinski *et al.*, 2009).

Program exposure has both temporal and geographic dimensions. The temporal dimension we consider in our analysis is the school cohort, which we define in terms of "school age", a child's age as of June 30 of a given year, which in Argentina establishes whether a child can enter a given educational level in a given year. For example, a child born between July 1, 1988, and June 30, 1989, had a school age of 5 years in 1994, so in that year they were eligible to enter a year 5 classroom. Thus, cohort c is defined as the cohort born between July 1 of $c-1$ and June 30 of c. The child in the above example belongs to the 1989 cohort. The cohorts that benefit from the program are those from 1989 onward. The geographic dimension of our program exposure measure is based on each individual's department of residence in 2010. Appendix Figure A2 summarizes the timing of the reform, classroom construction, cohort exposure, and follow-up measurement.

Our main treatment measure is a dummy variable indicating that the total number of new student places per child aged 3 to 5 in 1991 in the department exceeds the median of the distribution by department (approximately 0.098).[4] Treated departments are therefore above-median construction-intensity departments, while control departments are below-median departments, including those with no new places. Our estimates therefore reflect the average impact of the policy for cohorts exposed in above-median construction-intensity departments, as in related papers in the literature (Havnes & Mogstad, 2011).

With this measure, we first estimate the following two-way fixed effects (TWFE) equation:

$$\begin{aligned} Y_{icpj} = \mu_j + \mu_c + \beta_{post}\left[I(\text{cohort} \geq 1989) \times D_j\right] + \beta_{pre}\left[I(\text{cohort} \leq 1987) \times D_j\right] \\ + \gamma_{cp} + Enroll80_{cj} + \varepsilon_{icpj} \end{aligned} \tag{1}$$

where Y_{icpj} is an outcome of interest for individual i, of cohort c, resident in province p and department j. μ_j are departmental fixed effects, which control for time-constant departmental characteristics. μ_c are fixed effects by cohort, which control constant characteristics of the cohort across departments. γ_{cp} is a set of interactions between cohort and province of residence, which control unobservable differences between cohorts by province. $Enroll80_{cj}$ represents a set of interactions between cohort and departmental pre-primary gross enrollment in 1980, which controls for differential trends associated with pre-treatment enrollment levels. D_j is the dummy variable indicating that department j received a high intensity of treatment. $I(\text{cohort} \geq 1989)$ indicates that the individual belongs to the 1989 cohort or younger, and $I(\text{cohort} \leq 1987)$ is analogous for 1987 or older. Finally, ε_{icpj} is an error term. We cluster standard errors at the level of department of residence.

Given our design, the TWFE regression coincides with the canonical two-group, pre/post difference-in-differences estimand. Departments carry a time-invariant high-

[4] Figure A1 and Table A1 in Appendix A provide descriptive statistics and an illustration of the distribution of the number of new student places by department.

exposure indicator D_j, and eligibility turns on once and nationally with the 1989 school-age cohort. Treated units switch exactly once, while controls never switch. Under (i) strict exogeneity of ε_{icpj} conditional on the fixed effects and controls in equation (1), (ii) conditional parallel trends in untreated potential outcomes given those controls, and (iii) no anticipation for cohorts $c \leq 1988$, OLS with department and cohort fixed effects consistently estimates the average post-policy effect for cohorts $c \geq 1989$ in high-exposure departments (our intention-to-treat [ITT]) (Wooldridge, 2025). Because adoption is single-onset and D_j is fixed across cohorts, the Goodman-Bacon decomposition collapses to a single 2×2 comparison: the TWFE coefficient equals the standard difference in differences and avoids the negative-weight pathologies that arise with staggered timing (Goodman-Bacon, 2021). Pooling all pre- and post-cohorts yields greater precision than estimating separate pairwise contrasts (holding fixed the same controls and fixed effects). We report heteroskedasticity- and cluster-robust standard errors at the department level and probe pre-trends and alternative estimators in Section 5.

The parameter β_{post} measures the average causal effect of the construction program on children from cohorts of 1989 or later who reside in departments with high intensity of treatment. Since this parameter averages the effects of the reform on all children in the department, regardless of preschool attendance, we interpret it as an ITT effect. We also calculate an average treatment effect on the treated (ATT). The ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups.[5] The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. The re-scaled estimate can thus be interpreted as the effect of an additional pre-primary place per eligible child. This is the policy parameter of interest: the number of places built and operated was the government's choice variable, and the benefit-cost analysis in Section 4 is expressed per place constructed.

We also present the estimate of β_{pre}, which captures whether the policy had effects on untreated cohorts (i.e., those before 1989). This provides a direct way to test for the presence of differential pre-trends in the outcomes of interest.[6] Since equation (1)

[5] Strictly speaking, this rescaling delivers a per-place effect under a linear (constant effect per place) dose-response interpretation. We refer to it as the ATT throughout the paper.

[6] We also assess the parallel trend assumption with the approach of Rambachan and Roth (2023). These additional results, which reinforce our findings from the TWFE model, are presented in Appendix F.

allows us to summarize pre- and post-effects of the new pre-primary facilities, we refer to it as the "compact specification".

Other important changes in the educational system took place in Argentina during the 1990s. One of them is the transfer of the management of secondary schools from the federal to provincial level between 1992 and 1994 (Galiani *et al.*, 2008). Another relevant reform was the 1993 Federal Education Law, which expanded compulsory education to the last year of pre-primary and the first two years of secondary school (Alzúa & Velázquez, 2017; Alzúa, Gasparini, & Haimovich, 2015; Crosta, 2009; Lopez, 2012; Ministerio de Educación Argentina, 2001). Finally, as part of the implementation of the Federal Education Law, funds were allocated to other initiatives that possibly affected the educational performance of individuals (see e.g. Nicolini *et al.*, 2000).

These additional changes in the education system could threaten the consistency of the estimates in model (1) if they were associated with the pre-primary construction program. However, interventions that similarly affect individuals in a given cohort and province are controlled for cohort-province fixed effects. Therefore, only interventions that differentially affected individuals from the same cohort within the same province could bias the estimate of β_{post} in equation (1).[7] To rule out this possibility, we further test whether the expansion of classrooms correlates with the outcomes of the cohorts which were not exposed to the program by estimating a more flexible specification:

$$Y_{icpj} = \mu_j + \mu_c + \sum_{c \neq 1988} \beta_c \left[d_c \times D_j \right] + \gamma_{cp} + Enroll80_{cj} + \varepsilon_{icpj} \tag{2}$$

where d_c is a dummy variable that indicates whether the individual belongs to cohort c. The remaining terms are defined as before.

This specification allows us to estimate a coefficient β_c for each cohort c, which offers at least two advantages. First, we can observe whether the policy had effects on untreated cohorts, which would constitute evidence against the validity of our identification strategy. Second, it allows us to observe whether there were heterogeneous

[7] The ideal direct test would use the department-level allocation of funds for the other Federal Education Law initiatives as an outcome in our specifications. To our knowledge, no comprehensive department-level records of these allocations exist, so we cannot implement this test, and we do not claim that contemporaneous policies are fully ruled out. The cohort-by-cohort estimates and the birth-date cut-off design below, which compares children born within weeks of the same eligibility threshold in the same departments, provide indirect checks: a confounding policy would have to reproduce both the timing and the discontinuity.

effects among the treated cohorts: those from 1989 onward. We refer to equation (2) as our "dynamic specification".

3 The effects of the school construction program

3.1 Effects on educational attainment

We begin by assessing the effects of pre-primary education expansion on post-kindergarten attainment. First, we estimate equation (1) using S_{icpjm} as the dependent variable, where S_{icpjm} is a dummy indicating whether individual i completed m years or more of education. This is equivalent to estimating the impact on the cumulative distribution function (CDF) of post-kindergarten years of schooling. We define years of post-kindergarten education as the approved years completed in primary and secondary school. We observe post-kindergarten years because the Census records only the highest grade attained. In our sample, a child could enroll in primary school with three, two, one, or no years of kindergarten.

As a summary and illustration of our main results, Figure 3 plots the estimates of β_{post} (the intent-to-treat effect, ITT) for $m = 0, 1, \ldots, 12$, covering the primary and secondary cycles.[8] We find no effect for the first six post-kindergarten years—consistent with historically high primary completion in Argentina[9]—and positive, statistically significant effects from seven years onward, corresponding mainly to secondary schooling. Thus, the gains in attainment are concentrated in secondary education.

Table 1 reports estimates from equation (1). In column (1), the average treatment effect on the treated (ATT) of an additional pre-primary place is an increase of 0.48 years of basic (post-kindergarten) schooling, consistent with Figure 3. This corresponds to roughly 5% of the pre-treatment mean in highly treated departments. Columns (2) and (3) show that an additional place raises the probability of completing lower secondary and secondary by 7.02 and 11.89 percentage points, respectively. Column (4) shows a 7.12 percentage point increase in enrollment in (or completion of) post-secondary education. All effects in Table 1 are significant at the 1% level.[10]

[8] We also show the estimates of β_{pre} in Figure C9 (Appendix C). Reassuringly, these indicate no systematic pre-trends in our outcomes of interest.

[9] UNESCO Institute for Statistics (2024) reports that Argentina has had 95% enrollment levels since 1990.

[10] We can compare our results for secondary completion and post-secondary education with those in the

Table 1 also reports the pre-treatment coefficients β_{pre} from equation (1). For post-kindergarten years of basic education, completion of lower secondary, completion of secondary, and any post-secondary education, the estimates are 0.008 years, 0.262 pp, 0.371 pp, and −0.018 pp, respectively—none statistically significant—supporting the absence of pre-trends.

We assess heterogeneity by gender in education and labor-market outcomes in Tables C1 and C2 (Appendix C) by estimating equation (1) separately for women and men. While point estimates differ slightly, we cannot reject equality of effects across genders. This aligns with Berlinski *et al.* (2009), who find no gender heterogeneity in primary-school test-score impacts.

Finally, using the dynamic specification in equation (2), Figure 4a shows no effects for pre-treatment cohorts (1985–1987) and positive, significant effects for post-treatment cohorts (1989–1992). Effects are relatively stable across treated cohorts, with a tendency to increase for younger cohorts who were more exposed to construction. Appendix Figures C1–C3 show similar patterns for related outcomes.

3.2 Effects on fertility

Given the documented education impacts, we also study the effects of the program on fertility.[11] We estimate equation (1) for women's fertility outcomes (the Census does not collect fertility for men). Women report whether they have ever had a live-born child,

existing literature. For secondary completion, estimated effects range from 8.7 pp in Behrman *et al.* (2024), 4.7 pp in DeMalach and Schlosser (forthcoming), 6.0 pp in Gray-Lobe *et al.* (2022), and 5.8 pp in Havnes and Mogstad (2011), while our estimate is approximately 11.9 pp. For any post-secondary education, the corresponding estimates are 11.1 pp, 6.7 pp, 5.4 pp, and 6.9 pp, respectively, compared to an estimate of about 7.1 pp in our case. These comparisons should be interpreted with caution, since baseline completion rates and institutional contexts differ substantially across studies. More broadly, the literature on compulsory schooling reforms suggests that increases in mandatory schooling often raise educational attainment, while labor-market effects are more mixed. Oreopoulos (2008) finds that raising the UK school-leaving age from 14 to 15 increased educational attainment by about 0.4 years and generated large earnings gains. By contrast, subsequent studies report more modest or null labor-market effects: Devereux and Hart (2010) find returns of about 3 percent on average, Clark (2023) finds close to zero effects on several labor-market outcomes, Stephens and Yang (2014) find that US estimates become small and statistically insignificant under more flexible specifications, and Fischer *et al.* (2020) find that Sweden's extension from six to seven compulsory years raised completed schooling by 0.83 years but increased earnings by only around 2–2.4 percentage points.

[11] On multiple testing: we follow a hierarchical ("gatekeeping") strategy (Calónico & Galiani, 2025). Education was prespecified as the primary family; conditional on rejecting the null there, fertility is tested as a downstream family, so its p-values are interpreted conditional on passing the education gate. Labor-market outcomes were ex ante exploratory with weak priors, and we do not view them as the locus for multiplicity adjustments.

the total number of live births, and the date of the last live birth. We define "Being a mother" as a dummy equal to one if the woman has had at least one live-born child. We define "Teenage mother" as one if the woman had at least one live-born child at age 19 or younger. For the latter, we infer earlier births using the last birth date and total live births. Because women are observed at ages 18 to 26, these outcomes capture fertility up to the 2010 Census rather than completed lifetime fertility. We therefore interpret the estimates as effects on early fertility and cumulative births by young adulthood.

Table 2 summarizes the main results. Column (1) shows that an additional pre-primary place reduces the probability of being a mother by 11.11 percentage points (about 17% of the pre-treatment mean). Column (2) shows a reduction of 0.20 live births per woman (about 18% of the pre-treatment mean). Column (3) shows no statistically significant effect on the probability of teenage motherhood.

Pre-treatment coefficients β_{pre} for being a mother, total live births, and teenage motherhood are -1.191 pp, -0.015 live births, and 0.460 pp, respectively, none statistically significant, supporting the absence of pre-trends. The dynamic specification (equation 2) for total live births in Figure 4b shows no effects for pre-treatment cohorts (1985–1987) and negative, significant effects for post-treatment cohorts (1989–1992), growing in magnitude for younger cohorts. Appendix Figures C5 and C6 display similar patterns for related outcomes.

3.3 Effects on labor-market outcomes

Table 3 reports estimates of equation (1) for labor-market outcomes. Column (1) shows that an additional pre-primary place increases the probability of being employed by 0.73 percentage points (about 1% of the pre-treatment mean), not statistically significant. Column (2) shows a 1.35 percentage point increase in the probability of formal employment (about 5% of the pre-treatment mean), also not statistically significant. Overall, we do not detect significant employment effects at the time of observation.

Pre-treatment coefficients β_{pre} in Table 3 are small: -0.985 percentage points for employment (significant at 10% but quantitatively minor) and -0.185 percentage points for formal employment (not significant). They are unlikely to account for our main results. Gender-specific estimates (Table C2, Appendix C) are similar for women and men, and we cannot reject equality of effects. The dynamic estimates for employment and formal employment (Figures C7 and C8) show no significant impacts for either pre-treatment (1985–1987) or post-treatment cohorts (1989–1992). Consistent with continued schooling among the youngest treated cohorts, Appendix Figure C4 shows positive effects on current post-secondary attendance, concentrated among the 1991 and 1992 cohorts.

Our interpretation of these employment results is as follows. In a one-sector benchmark with homogeneous labor—where firms hire efficiency units and workers are close substitutes—additional schooling primarily raises productivity and thus wages, with no necessary change in the probability of employment. By contrast, in a segmented market with imperfect substitutability between unskilled and skilled labor, extensive margin effects would arise only if the intervention pushes a nontrivial mass across a salient skill threshold. At the census ages we study, the program substantially increases secondary completion and post-secondary attainment, yet many beneficiaries either remain in the same broad segment or are still in school; accordingly, the extensive margin need not adjust and our employment coefficients are statistically indistinguishable from zero. A natural interpretation is that adjustment operates through earnings, hours, or job quality rather than employment status. Although the Census lacks wage measures to test this mechanism directly, our benefit-cost calculations map the schooling gains into expected wage premia, implying sizable returns even in the absence of employment effects.

4 Benefit-cost analysis

We conduct a social-planner benefit-cost analysis (BCA) for constructing one new pre-primary classroom that operates in two shifts (capacity $25 \times 2 = 50$ seats for a single cohort of five-year-olds). Costs are valued as real resource costs in constant USD (transfers excluded); benefits are the present value of earnings gains induced by the estimated increase in schooling. We discount at 3% throughout and use our estimated ATT of 0.48 additional years of completed post-kindergarten schooling per additional seat created. We map schooling to earnings with a standard Mincer return; in the baseline, we apply a 4.32% wage premium per treated child implied by $\theta = 0.48 \times r_s$ with $r_s \simeq 9\%$.[12]

[12]Using $r_s = 9\%$ implies $0.48 \times 0.09 = 4.32\%$.

Direct costs comprise capital and recurrent components. The construction is costed at \$15,000. For accounting and per-seat intuition, we annualize this capital outlay using the monthly amortization formula used in the calibration. Let $r_m = 0.10/12$ and $n = 25 \times 12$. The monthly capital payment is

$$C_{\text{cap},m} = \frac{r_m \cdot 15{,}000(1 + r_m)^n}{(1 + r_m)^n - 1} \simeq \$136.31.$$

Adding recurrent operations of \$750 per month (teacher plus materials), the direct preschool cost allocated to one cohort year is

$$C_D = 12(136.31 + 750) \simeq \$10{,}636,$$

or about \$213 per seat-year when spread across 50 seats. Indirect costs include (i) additional public outlays from higher retention in primary and lower secondary, valued using the annual direct preschool cost as a proxy for the public resource cost of an additional school year for those levels, and (ii) forgone youth earnings from ages 15–17 due to longer time in school, using a 6% employment rate and an average youth wage of \$200/month from the EPH (2007–2015). Taxes and transfers are treated as neutral, and we exclude non-pecuniary benefits; these choices render the BCA conservative.

Benefits are the lifetime earnings gains generated for each treated child. Let w_a denote average (gross, real) earnings at age a, e_a the employment probability, and δ the discount factor. For each treated child, the proportional earnings effect is θ. The present value of benefits per treated child is $PV_B = \sum_{a=a_0}^{A} \theta\, w_a\, e_a\, \delta^{(a-5)}$ with labor-market entry at $a_0 = 18$ and terminal working age $A = 65$. We recover $\{w_a, e_a\}$ from EPH microdata to trace age-earnings and age-employment profiles used in the simulations. Aggregating across 50 seats (the classroom's capacity for that cohort year) yields the classroom-level benefit stream. Net present costs include C_D, the direct preschool resource cost allocated to the entering cohort, together with the present value, discounted at 3%, of the future indirect costs described above. Because the direct costs associated with serving the cohort are incurred during its preschool year, they are treated as occurring at the beginning of the evaluation horizon.

Under these assumptions, the baseline calibration yields a benefit-cost ratio (BCR) of approximately 10.97 and an internal rate of return (IRR) of 13.44%. The high BCR reflects three features of this policy: relatively low per seat-year direct costs (about \$213), material impacts on completed schooling (0.48 years per additional seat), and a persistent life-cycle earnings gradient with schooling in the EPH data.

Several sensitivity checks confirm robustness. Varying the schooling return between 8% and 10% scales the wage premium nearly proportionally; the implied BCR shifts by roughly ±11% around baseline and remains well above one. Raising the social discount rate from 3% to 5% compresses distant benefits and lowers their PV, yet the BCR stays comfortably greater than unity because costs are modest and front-loaded. Inflating recurrent costs by 20% or adding a 20% shadow markup for the marginal cost of public funds also leaves the BCR high given the margin between benefits and costs. Appendix Table B1 reports the implied BCRs and IRRs for these alternative assumptions. Moreover, the baseline omits potential non-labor gains—such as reductions in early fertility, crime, or later remedial education—and parental labor-supply responses; incorporating even a fraction of such spillovers would raise returns.

Two remarks clarify interpretation. First, the unit of analysis is a single cohort occupying 50 seats in one academic year; benefits and the one-year flow of costs attributed to that cohort scale approximately linearly with capacity as long as per-seat costs move proportionally. Second, our use of a 10% capital amortization is purely an annualization device to report per-year costs; discounting for the BCA is performed at the social rate of 3%. Overall, the BCA indicates that the pre-primary expansion is highly cost-effective under reasonable assumptions, with margins that persist under conservative parameterizations.

The previous discussion does not account for the financing of this program. The marginal value of public funds (MVPF) provides the perspective of a budget-constrained government that raises revenue through distortionary taxation. We connect the BCA to this fiscal perspective with a simple MVPF-style calculation following Hendren and Sprung-Keyser (2020). Let $B = 50\,PV_B$ denote the classroom-level present value of adult gross earnings gains generated by the 50 seats, C_D the direct preschool resource cost allocated to the entering cohort, C_S the present value of the additional public schooling outlays induced by higher retention, and E_Y the present value of forgone gross youth earnings, so that total present-value costs in the BCA are $C = C_D + C_S + E_Y$. In our baseline, $B \simeq \$241{,}755$, $C_D \simeq \$10{,}636$, $C_S \simeq \$9{,}845$, and $E_Y \simeq \$1{,}554$, so $C \simeq \$22{,}035$. With a proportional tax rate τ, beneficiaries' willingness to pay is

$$\text{WTP} = (1-\tau)B - (1-\tau)E_Y,$$

since forgone youth earnings reduce beneficiaries' after-tax earnings gains, and the corresponding net fiscal cost is

$$\Delta G = C_D + C_S + \tau E_Y - \tau B,$$

where direct preschool spending and additional public schooling outlays are fiscal costs, τE_Y is lost tax revenue from lower youth earnings, and τB is additional tax revenue from higher adult earnings. The program is fiscally self-financing whenever $\Delta G < 0$, or equivalently whenever $\tau(B - E_Y) > C_D + C_S$. In our baseline, this requires only $\tau > (C_D + C_S)/(B - E_Y) \simeq 0.085$, so any effective fiscal rate on incremental adult earnings above approximately 8.5% places the policy in the self-financing region. For illustration, with $\tau = 0.25$,[13] this implies WTP $\simeq \$180{,}151 > 0$ and

$$\Delta G = 10{,}636 + 9{,}845 + 0.25(1{,}554) - 0.25(241{,}755) \simeq -\$39{,}570.$$

Thus, even after including the indirect fiscal components and treating forgone youth earnings as a reduction in willingness to pay, the net fiscal cost is negative. Since WTP > 0 and $\Delta G < 0$, the policy lies in the self-financing, or "win–win," region of the MVPF framework; under the standard MVPF reporting convention, such policies are assigned an infinite MVPF.

5 Robustness

In this section, we present a series of robustness checks aimed at assessing the sensitivity of the main results to alternative assumptions and potential limitations of the empirical strategy. These exercises are motivated both by recent methodological developments

[13] We use $\tau = 0.25$ as an illustrative effective fiscal rate. This value is broadly consistent with Argentina's tax structure, where the standard VAT rate is 21% and formal labor earnings are also subject to social-security contributions. The self-financing conclusion does not depend on this calibration: the baseline break-even tax rate is approximately 8.5%.

in the difference-in-differences literature and by specific concerns related to the context of the policy under study, such as selective migration and the definition of treatment exposure. The goal is to provide additional evidence that the estimated effects are not driven by particular modeling choices or data constraints.

We start by considering recent critiques of traditional pre-trend tests and two-way fixed effects (TWFE) estimators, and we apply alternative estimation strategies proposed in that literature. Then, we replicate and extend the approach used in a closely related paper to examine the consistency of the findings under a different treatment definition. We also explore the possibility that selective migration might bias the estimates and provide several tests to assess the extent of this issue. In addition, we implement a regression-discontinuity-type design using the enrollment cut-off rule to further test the results in a narrow bandwidth around the threshold. Finally, we replicate part of the fertility results using administrative records. Overall, the evidence from these robustness checks supports the main conclusions of the paper.

5.1 Alternative difference-in-differences specifications

The validity of our strategy requires that counterfactual outcomes in low-treated departments would have followed the same trend as in high-treated departments. A traditional diagnostic is to estimate effects in pre-treatment periods (when there should be none). We implement this using equations (1) and (2) and find no meaningful pre-trends, as discussed above. However, recent work highlights limitations of such tests (Rambachan & Roth, 2023; Roth, 2022; Roth *et al.*, 2023): pre-trend tests are typically low-powered and, by themselves, do not deliver identification when small violations of parallel trends are present. We therefore implement the honest difference-in-differences approach of Rambachan and Roth (2023), which constructs confidence intervals allowing bounded departures from parallel trends calibrated from the pre-period; our main effects remain within these intervals (Appendix F).

Concerns about two-way fixed effects (TWFE) with staggered adoption and heterogeneous effects—negative weights and contaminated leads/lags—are well documented (Goodman-Bacon, 2021; Sun & Abraham, 2021). These issues are far less salient in our setting. Exposure status D_j is fixed across cohorts, eligibility turns on once and nationally by school-age cohort, and there is no staggered timing. Consequently, already-treated units never serve as controls for later-treated units, the Goodman-Bacon decomposition collapses to a single 2×2 comparison, and in this single-onset design the TWFE coefficient coincides with the canonical difference-in-differences estimand (Goodman-Bacon, 2021; Wooldridge, 2025). Conditional on our fixed effects and controls, pooling yields more precise inference than pairwise contrasts does while preserving the same causal content.

Other recent contributions propose estimators tailored to heterogeneity and staggered adoption. To address these concerns, we present in Appendix E the estimation on our main results using the approach of Dube *et al.* (2025), Wooldridge (2023, 2025), Sun and Abraham (2021) and Callaway and Sant'Anna (2021). Sun and Abraham (2021) show that lead-lag TWFE is biased under staggered timing and propose an interaction-weighted estimator that compares each treated cohort only to not-yet-treated units, recovering dynamic effects under group-specific parallel trends. Callaway and Sant'Anna (2021) identify group-time treatment effects, $\text{ATT}(g, t)$, under (possibly conditional) parallel trends and provide aggregation/inference robust to heterogeneous effects. The approach of Wooldridge (2023, 2025)—implemented via the ETWFE estimator—is a flexible extension of standard fixed effects models that estimates average treatment effects in difference-in-differences settings while addressing issues like bad controls and negative weights. Finally, Dube *et al.* (2025) proposes a local-projections difference-in-differences with a "clean controls" condition that avoids using already-treated units as controls and nests several recent solutions. The results show that the estimated treatment effects and their patterns are similar to those presented in Appendix C for our main dynamic specification, suggesting that potential flaws in the TWFE specification are not a major concern in our setting.

5.2 Comparison with Berlinski *et al.* (2009)

To validate the robustness of our findings and ensure comparability with previous literature, we replicate and extend the methodology of Berlinski *et al.* (2009). First, in Appendix H we reproduce their original difference-in-differences design using standardized test scores in Mathematics and Spanish for third-grade students. This replication confirms a positive effect of pre-primary expansion on academic performance consistent with the skill development narrative suggested in the early literature on early childhood interventions (Cunha & Heckman, 2007).

Second, in Appendix I we adopt the treatment exposure measure proposed by Berlinski *et al.* (2009), which calculates a cohort-specific "department-cohort" measure based on the number of pre-primary places available when children were ages 3 to 5. This continuous treatment variable allows us to estimate the average treatment effect on the treated (ATT) of one additional year of pre-primary education. We then re-estimate our main outcomes using this alternative specification. The results are highly consistent across methods, suggesting that our main findings are not sensitive to the choice of treatment definition. Moreover, we demonstrate that it is possible to construct an equivalent ATT using our original design by adjusting the rescaling procedure to match the logic behind the Berlinski *et al.* (2009) approach. The comparability of both estimates reinforces the internal validity of our empirical strategy.

5.3 Selective migration

A further concern for our identification strategy is selective migration. We assigned our treatment exposure measure based on the department of residence in 2010, since the Census does not collect information about where individuals lived at pre-primary age. However, these locations may not necessarily match, and this could be an issue if the migration patterns are correlated with the intensity of the policy. We address this potential issue with several robustness checks.

Parents could have moved to departments where pre-primary was expanding so they could benefit from the new pre-primary places. The characteristics of these parents could also be systematically correlated with their children's long-term outcomes.

First, we analyze the impact of the new pre-primary facilities on departmental cohort sizes. For each department and cohort in our analysis window (1985–1992), we compute the cohort size observed in the 1991, 2001, and 2010 censuses. We then use as outcomes the ratios of cohort size in 2010 to cohort size in 1991 and cohort size in 2001 to cohort size in 1991, reported in the first and second columns of Table G1 of Appendix G, respectively. We do not find significant results. In the third column, we use the log cohort size in 2010 as the outcome, and again we do not find significant results.

Second, selective migration might have altered the composition of households or their characteristics even if cohort sizes remain unchanged. To address this concern, we identify households in the 1991 and 2001 censuses in which children from the relevant cohorts reside and assess whether changes in their parents' and households' characteristics are correlated with the new pre-primary facilities. These results are presented in Appendix G in Table G2. In the first column, we present the effect of the construction program on the ratio of the 2001 departmental average years of schooling of identified parents to the same average in 1991; parents are identified as the household head or the head's spouse when the child is listed as a child of the household head. We do not find significant effects on this ratio. We also investigate whether the policy is correlated with changes in the socioeconomic situation of households where children in our sample lived. In the second column of Table G2, we present the estimate of the effect of new pre-primary facilities on the ratio of the 2001 departmental proportion of households with some unsatisfied basic need (UBN) to the same proportion in 1991.[14] In the third column, we present the results of the same estimation but with the total UBN per household as the dependent variable. We do not find any significant effects of the pre-primary construction program on these variables either.

Third, as a further check of the potential bias introduced in our results by selective migration, we re-estimate equation (1) using only individuals who resided in 2010 in the same province where they were born (the Census collects information on province of birth, not department). We present these additional results in Appendix G for educational attainment, fertility, and labor-market outcomes in Tables G3, G4, and G5, respectively. The education estimates are close to the baseline, while the fertility estimates are somewhat smaller (a reduction of 0.15 rather than 0.20 live births per woman) and significant at the 10 percent level. Since conditioning on non-migration can itself change the estimand if the program affected schooling and schooling in turn affected mobility, we read this exercise as a robustness check rather than as a preferred specification.

Finally, we examine whether program exposure is associated directly with individual migration outcomes in the 2001 and 2010 censuses. We consider three indicators: residing in a province different from the province of birth, residing in a different lo-

[14] Census data allows us to determine five types of UBN: 1) Overcrowding, meaning that the house has more than three people per room; 2) Housing, meaning that people live in precarious or non-conventional housing; 3) Sanitary, meaning that the house does not have a toilet; 4) School attendance, meaning that some children of school age are not enrolled in school; 5) Subsistence, meaning that the household has four or more people per employed member.

cality than five years earlier but within the same province, and residing in a different province than five years earlier. We estimate the compact specification separately in each census, using the department of residence observed in that census. The results, reported in Tables G6 and G7 at the end of Appendix G, show no statistically significant post-treatment effects for any of the three migration measures. The pre-treatment coefficient for interprovincial migration in 2010 is positive and statistically significant, so that particular estimate should be interpreted cautiously; importantly, however, we find no corresponding post-treatment effect.

To sum up, there does not seem to be a significant correlation between the pre-primary construction program and different proxies of outcomes which we would have expected to change if selective migration interfered with our identification strategy.

5.4 Alternative approach: estimates near the cut-off

To further assess the robustness of our findings, we implement an alternative estimation strategy in Appendix J focusing on individuals born close to the eligibility cut-off for pre-primary enrollment. Specifically, we exploit the discontinuity generated by the birth-date rule that determines access to the program, using July 1, 1988, as the threshold. This allows us to compare individuals born just before and just after the cut-off, following a strategy similar to the "diff-in-disc" design of Grembi *et al.* (2016).

We estimate a difference-in-differences specification using narrow bandwidths (75, 100, and 125 days) around the cut-off. The results, presented in Tables J1, J2, and J3, reveal positive and statistically significant effects on educational outcomes such as years of education, completion of lower secondary education, and completion of upper secondary education. The reduction in total live births is also present and significant across bandwidths, while the remaining fertility and labor-market estimates are imprecise; Appendix J discusses these patterns. The education effects are consistent across bandwidths, reinforcing the robustness of our main findings.

5.5 Replication of fertility results with administrative data

Our fertility results can be partially replicated using administrative records of live births from the Argentine Ministry of Health. Specifically, we obtained data on the total live births per department and calendar year, and by department and birth cohort year of the mother, yearly for the period 2004–2022. We constructed panels of departments and year-of-birth cohorts, calculating live births of treated and untreated women in different windows. We estimate the effects of the new pre-primary facilities on this departmental measure of live births. Reassuringly, the results presented and discussed in Appendix K are consistent with our main estimates based on individual data from the Census.[15]

6 Conclusions

Our study provides robust evidence of the long-term benefits of expanding universal pre-primary education in a middle-income country context, specifically Argentina. The findings reveal that an additional pre-primary education place significantly increases educational attainment, with substantial gains in both secondary school completion and post-secondary attainment. Additionally, the expansion reduced fertility up to the census date among women who were induced to attend pre-primary by the construction program, consistent with a human-capital channel in which higher educational attainment raises the opportunity cost of early childbearing.

The long-run effects we document are consistent with the literature on early childhood interventions. The program likely operated through multiple channels: by promoting cognitive and non-cognitive skill development in treated children (Berlinski *et al.*, 2009; Cunha & Heckman, 2007), and by subsidizing childcare, potentially freeing up time for caregivers—particularly mothers—to reallocate toward the labor market (Berlinski & Galiani, 2007; Humphries *et al.*, 2024). However, the evidence suggests that maternal employment effects were small (Berlinski & Galiani, 2007), making skill formation the more plausible primary mechanism.[16]

These results underscore the importance of early childhood education as an in-

[15] We must clarify that this approach is only an imperfect proxy of our main results for 2010. In our benchmark estimations we rely on data on the total live births for each woman, while the results in Appendix K use aggregated data at the department-cohort panel, which is why we only claim a partial replication with this alternative data source.

[16] We complemented the results of Berlinski and Galiani (2007) by exploring the effects of new pre-primary facilities on maternal employment within our timeframe using 2001 Census data (which provide a much larger sample), based on specification (1). We find that an additional pre-primary place per child does not significantly increase the probability of maternal employment. The results of this additional exercise are available upon request.

vestment in human capital, particularly in developing and middle-income countries. Our research contributes to the broader literature on early childhood development by offering empirical support for the positive long-term effects of universal pre-primary education, filling a critical gap in understanding its impacts beyond high-income countries.

However, the mixed results observed in similar studies across different contexts suggest that the success of such programs may depend on a variety of factors, including the quality of implementation, the socioeconomic environment, and complementary policies. Policymakers should therefore consider these elements when designing and scaling early childhood education initiatives.

Overall, our study supports further expansion toward universal pre-primary education as a strategic policy to foster educational advancement and manage demographic trends, thereby promoting long-term economic growth and social development.

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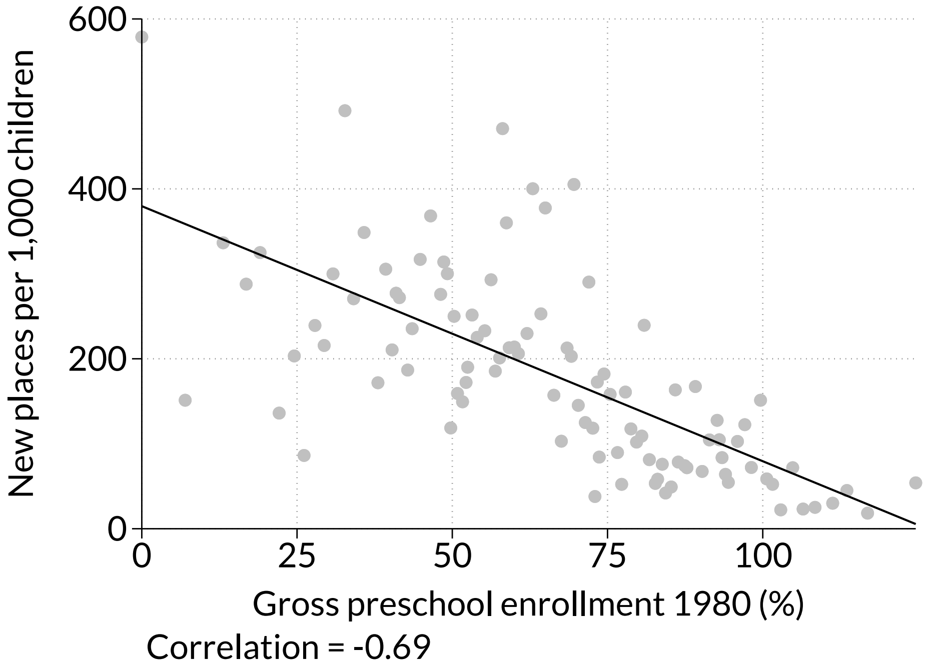
Figure 1: Program targeting and pre-primary enrollment growth

(a) Newly constructed pre-primary places and pre-reform enrollment rates



(b) Gross pre-primary enrollment by department in 1991 and 2001

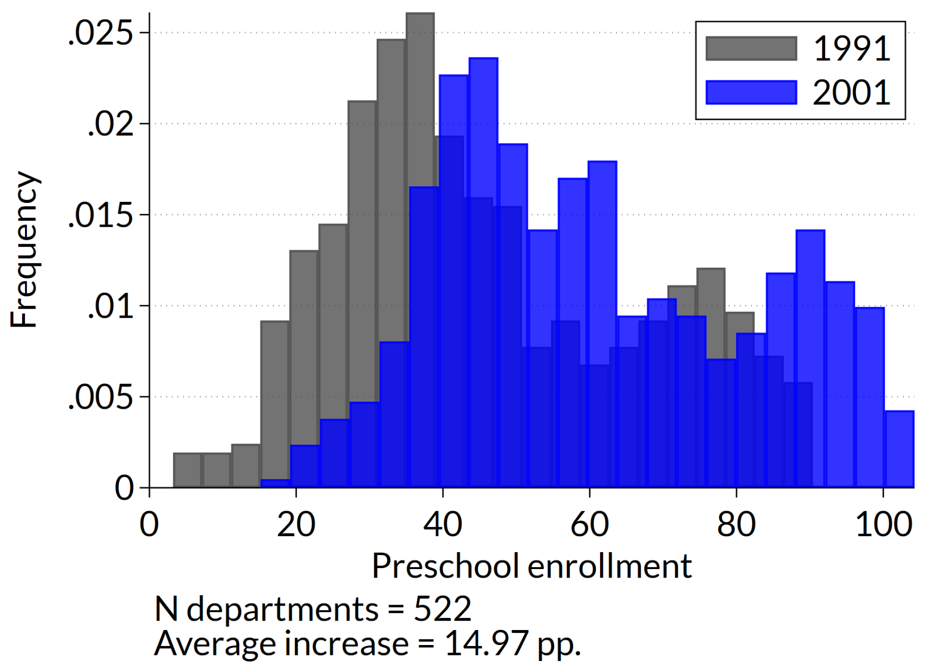
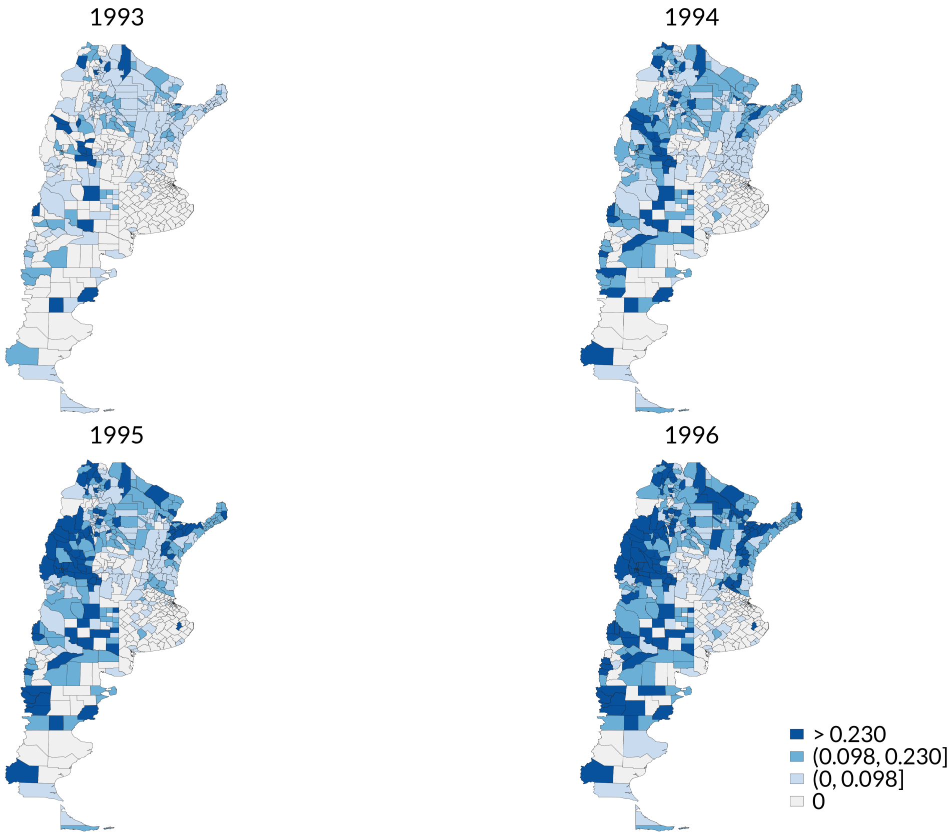
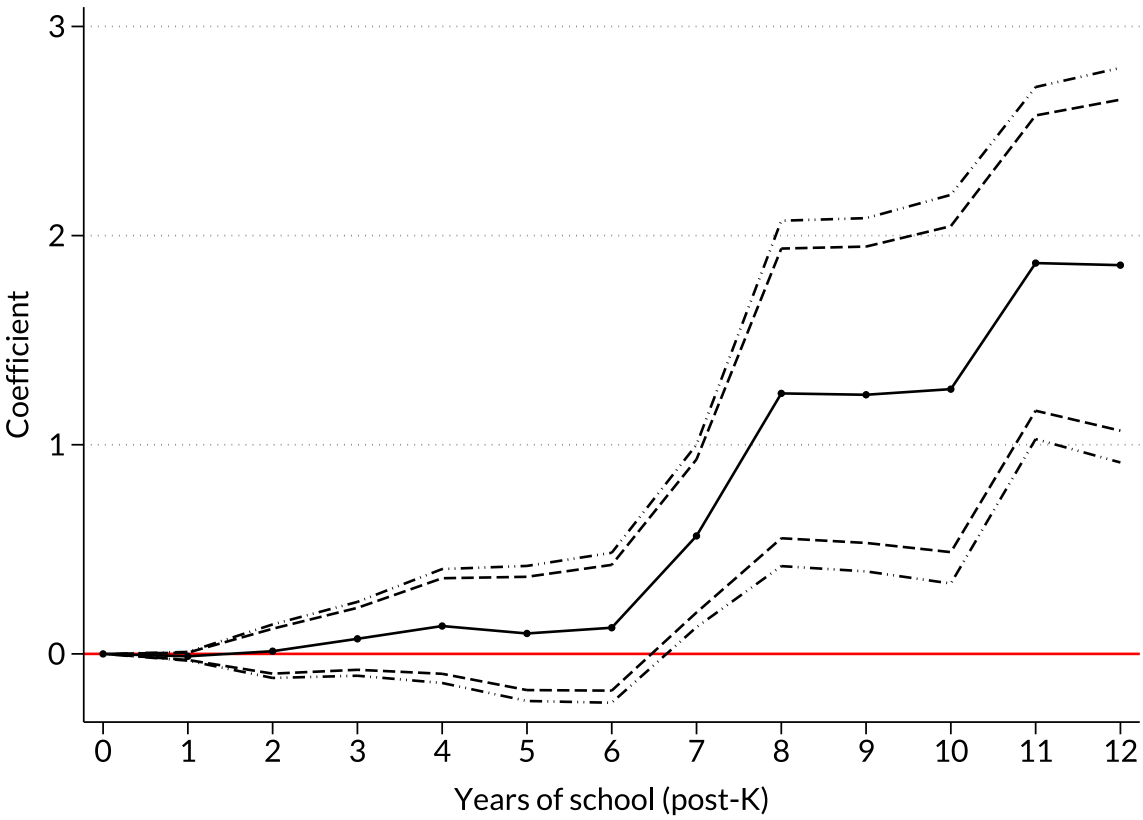


Figure 2: Cumulative new pre-primary places by department and year



Note: The figure reports cumulative new pre-primary places constructed through each year, by department, expressed as places per 3- to 5-year-old child in 1991. Each classroom is converted into 50 student places. The color scale is common across years. Categories correspond to departments with no new places, positive intensity up to the median of the department distribution (0.098), intensity above the median and up to the 75th percentile (0.230), and intensity above the 75th percentile. The first two categories correspond to the comparison departments, and the latter two to the treated departments, as depicted in Figure A1 and Table A1. Darker colors indicate higher construction intensity.

Figure 3: Difference-in-differences in the cumulative distribution function of post-kindergarten years of school. Post-treatment coefficients



Note: Each point of the figure represents the estimation of β_{post} of the equation (1) where the outcome is a dummy that indicates that the individual reached m post-kindergarten years of basic school or more, with $m = 0, 1, ..., 12$. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Table 1: Program effects on educational attainment

	Years basic school (pot-K.)	Lower secondary completion (pp.)	Secondary completion (pp.)	Any post secondary (pp.)
ATT	0.483***	7.024***	11.889***	7.128***
ITT	0.092***	1.337***	2.264***	1.357***
	(0.024)	(0.489)	(0.548)	(0.453)
	[0.000]	[0.006]	[0.000]	[0.003]
Pre coef.	0.008	0.262	0.371	-0.018
	(0.025)	(0.543)	(0.557)	(0.443)
	[0.754]	[0.630]	[0.506]	[0.967]
Mean pre-treat	9.448	54.950	42.245	19.774
N	1,999,633	1,999,633	1,999,633	1,999,633
N clusters	522	522	522	522
Controls and FE				
Department FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes	Yes

Note: The table presents results from estimating (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups. The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. Pre coef. is the estimate of β_{pre}. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, and province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Standard errors (in parentheses) are clustered at the department-of-residence level. p-values are in brackets. ${}^{*}p < 0.10$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$.

Table 2: Program effects on fertility outcomes

	Being a mother (pp.)	Total live births	Teen mother (pp.)
ATT	-11.108**	-0.197**	2.236
ITT	-2.115**	-0.037**	0.426
	(0.955)	(0.016)	(0.568)
	[0.027]	[0.022]	[0.454]
Pre coef.	-1.191	-0.015	0.460
	(0.790)	(0.013)	(0.553)
	[0.132]	[0.249]	[0.406]
Mean pre-treat	64.052	1.123	17.374
N	1,000,485	1,000,485	1,000,485
N clusters	522	522	522
Controls and FE			
Department FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes

Note: The table presents results from estimating (1) for different outcomes. We define "Being a mother" as a dummy variable indicating that the woman has had at least one live-born child. We define "Teenage mother" as a dummy variable indicating that the woman had at least one live-born child when she was 19 years old or younger. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups. The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. Pre coef. is the estimate of β_{pre}. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, and province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Standard errors (in parentheses) are clustered at the department-of-residence level. p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

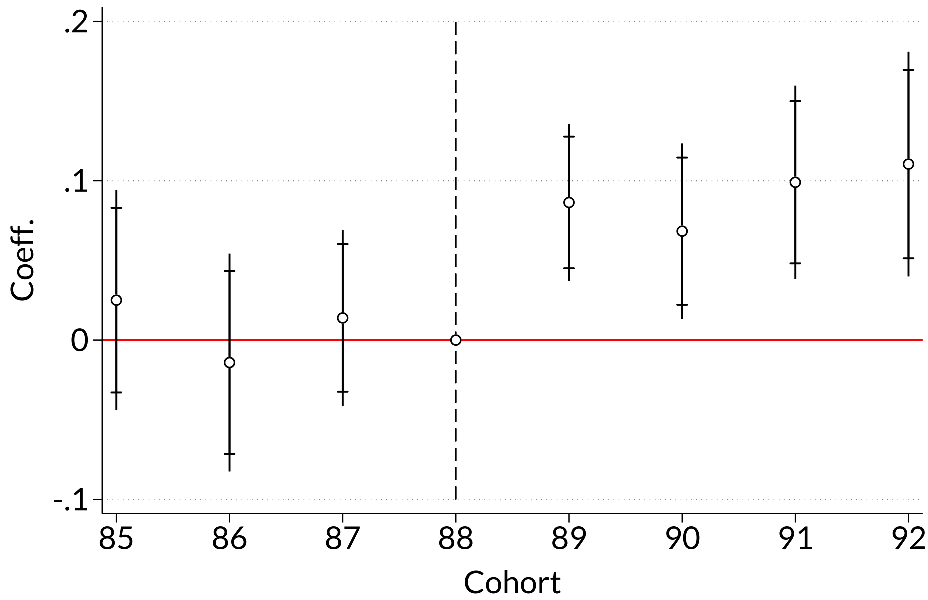
Table 3: Program effects on labor-market outcomes

	Employment (pp.)	Formal employment (pp.)
ATT	0.730	1.350
ITT	0.139	0.257
	(0.604)	(0.520)
	[0.818]	[0.621]
Pre coef.	-0.985*	-0.185
	(0.541)	(0.635)
	[0.069]	[0.770]
Mean pre-treat	60.088	27.552
N	1,999,633	1,999,633
N clusters	522	522
Controls and FE		
Department FE	Yes	Yes
Cohort FE	Yes	Yes
Coh. × Province	Yes	Yes
Coh. × Enr. 80	Yes	Yes

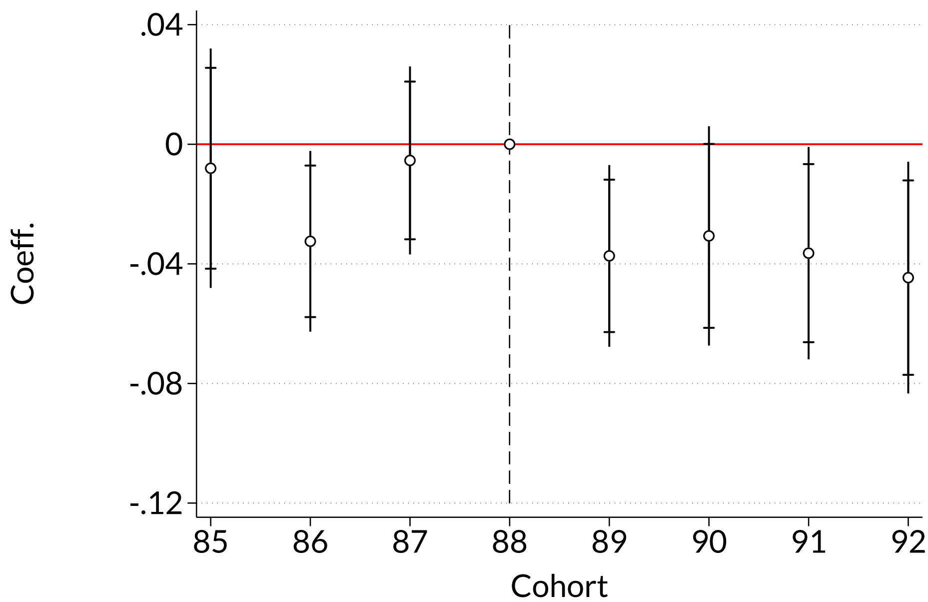
Note: The table presents results from estimating (1) for different outcomes. We define "Employment" as a dummy variable indicating that the individual was employed at the time of the interview. We define "Formal employment" as a dummy variable indicating that the individual was employed and was contributing to social security at the time of the interview. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups. The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. Pre coef. is the estimate of β_{pre}. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, and province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Standard errors (in parentheses) are clustered at the department-of-residence level. p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 4: Dynamic specification

(a) Years of basic school



(b) Total live births



Note: The figures present the results from estimating (2) for different outcomes. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, and province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Appendices

A Description of construction program and variables

In this section, we present further details on the distribution of treatment intensity by department and the main variables used in the paper. The treatment measure is a dummy variable equal to one if the department's total number of new places per child aged 3 to 5 in 1991 exceeds the department median, approximately 0.098. Treated departments are therefore above-median construction-intensity departments; control departments are below-median departments, including those with no new places. Our estimates therefore reflect the average impact of the policy for cohorts exposed in above-median construction-intensity departments. The complete distribution is presented in Figure A1. Figure A2 summarizes the timing of the reforms, classroom construction, cohort exposure, and follow-up measurement.

Figure A1: Distribution of new pre-primary places per child

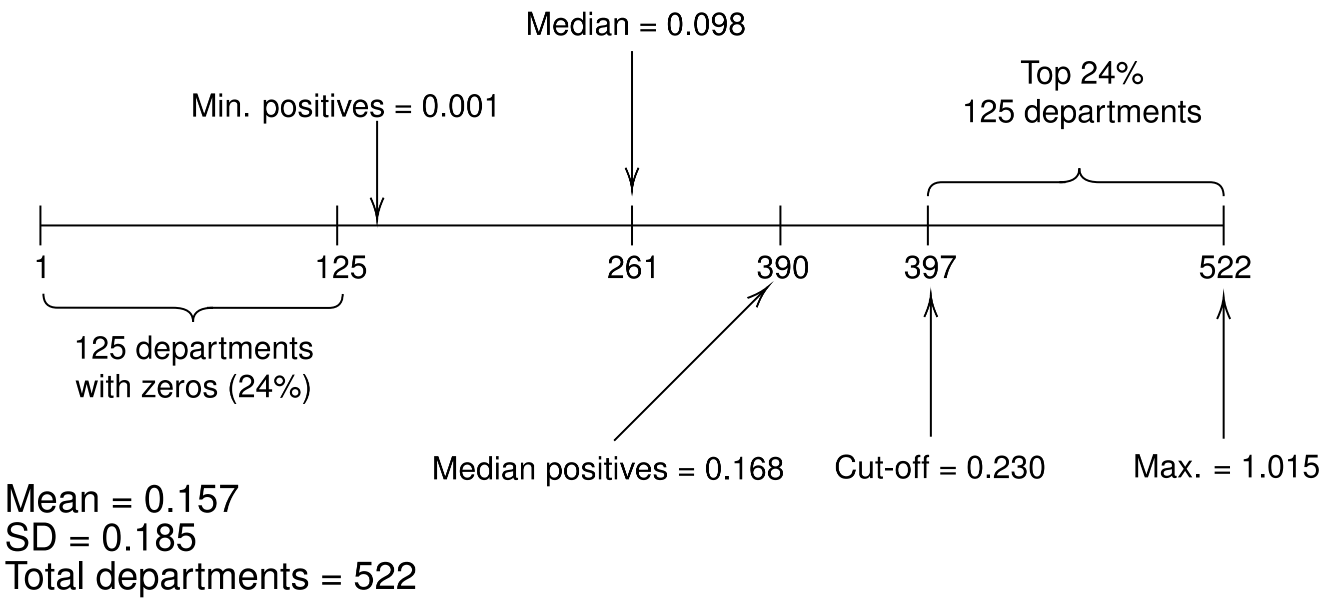


Table A1: Description of treatment measure

Treatment measure: dummy D_j	
Treated (D_j = 1) 261 departments	Departments that received places per child above median (0.098).
Control (D_j = 0) 261 departments	Departments that received places per child below median (0.098), including zeros
Total number of departments: 522	

Figure A2: Timeline of reforms, construction program, cohorts, and follow-up

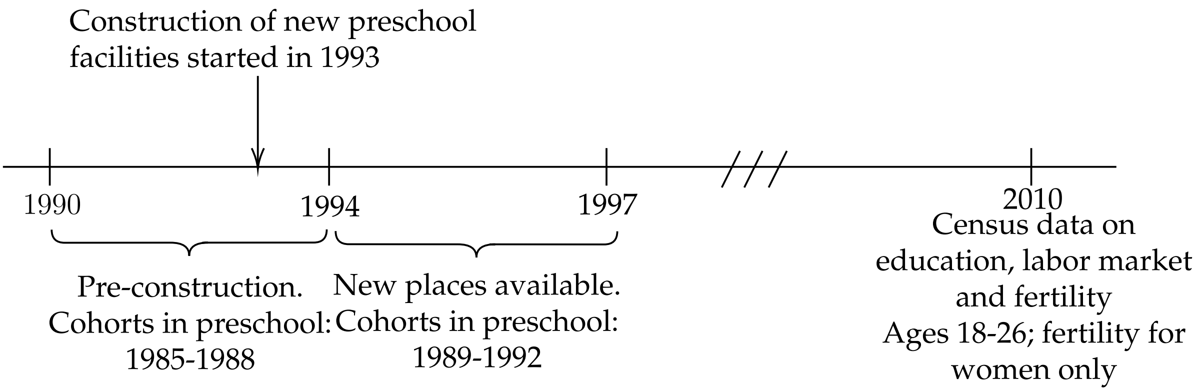


Table A2: Definition and summary statistics of variables

Variable	Definition	Source	Mean	Min	Max
Lower secondary	Equal to 100 if the individual completed the first three years of secondary education, 0 otherwise.	2010 Census	67.96	0	100
Secondary completed	Equal to 100 if the individual completed secondary education, 0 otherwise.	2010 Census	50.93	0	100
Any post-secondary education	Equal to 100 if the individual attained any post-secondary education, 0 otherwise.	2010 Census	29.56	0	100
Total live births	Number of live births. Women only, because the Census does not collect fertility information for men. Fertility outcomes are missing for women reporting more than six live-born children, treated as outliers.	2010 Census	0.59	0	6
Being a mother	Equal to 100 if the woman had at least one live birth, 0 otherwise. Women only, because the Census does not collect fertility information for men.	2010 Census	38.61	0	100
Teen mother	Equal to 100 if the woman had at least one live birth as a teenager, 0 otherwise. Women only, because the Census does not collect fertility information for men.	2010 Census	15.41	0	100
Employment	Equal to 100 if the individual was employed, 0 otherwise.	2010 Census	58.47	0	100
Formal employment	Equal to 100 if the individual was employed and made social security contributions, 0 otherwise.	2010 Census	27.68	0	100
Post-kindergarten years of basic school	Completed primary and secondary years of schooling.	2010 Census	10.20	0	12
Cohort	School cohort from date of birth: cohort t if born between July 1 of year $t-1$ and June 30 of year t.	2010 Census	1988.63	1985	1992
Age	Age in years in the 2010 census.	2010 Census	21.69	18	26
Pre-primary gross enrollment in 1980	Children aged 5 or older enrolled in pre-primary education divided by children aged 5, times 100. The numerator includes enrolled children older than the official age group, so the gross rate can exceed 100.	1980 Census	66	0	128
Pre-primary gross enrollment in 1991	Children aged 3 to 5 attending pre-primary education divided by children aged 3 to 5, times 100.	1991 Census	46	3	90
Pre-primary gross enrollment in 2001	Children aged 3 to 5 attending pre-primary education divided by children aged 3 to 5, times 100.	2001 Census	61	15	104
Province	Province of residence in the 2010 census. The sample covers Argentina's 24 provinces.	2010 Census	–	–	–
Department	Department of residence in the 2010 census. The sample covers 522 departments.	2010 Census	–	–	–

B Benefit-cost sensitivity analysis

Table B1 reports the benefit-cost ratios and internal rates of return implied by the sensitivity checks discussed in Section 4. Each row varies one assumption at a time relative to the baseline calibration.

Table B1: Benefit-cost sensitivity analysis

Scenario	BCR	IRR (%)
Baseline	10.97	13.44
Schooling return = 8%	9.75	12.80
Schooling return = 10%	12.19	14.03
Discount rate = 5%	6.26	13.44
Recurrent costs +20%	9.48	12.62
Cost shadow markup +20%	9.14	12.45

Note: The table reports the benefit-cost ratio (BCR) and internal rate of return (IRR) under alternative assumptions. The baseline calibration uses a 9% return to schooling, a 3% social discount rate, the baseline recurrent cost, and no shadow markup for the marginal cost of public funds. Sensitivity checks vary one assumption at a time. Since the IRR is computed from the underlying cost and benefit streams, changing only the social discount rate affects the BCR but not the IRR.

C Dynamic and other alternative specifications

During the 1990s, other important changes in the educational system took place in Argentina in addition to the pre-primary construction program. One of them is the transfer of the management of secondary schools from the federal to provincial level between 1992 and 1994 (Galiani *et al.*, 2008). Another relevant reform was the Federal Education Law of 1993, which expanded compulsory education to the last year of pre-primary and the first two years of secondary school (Alzúa & Velázquez, 2017; Alzúa, Gasparini, & Haimovich, 2015; Crosta, 2009; Lopez, 2012; Ministerio de Educación Argentina, 2001). Finally, as part of the implementation of the Federal Education Law, funds were allocated to other initiatives that possibly affected the educational performance of individuals (see e.g., Nicolini, Sanguinetti, and Sanguinetti (2000)).

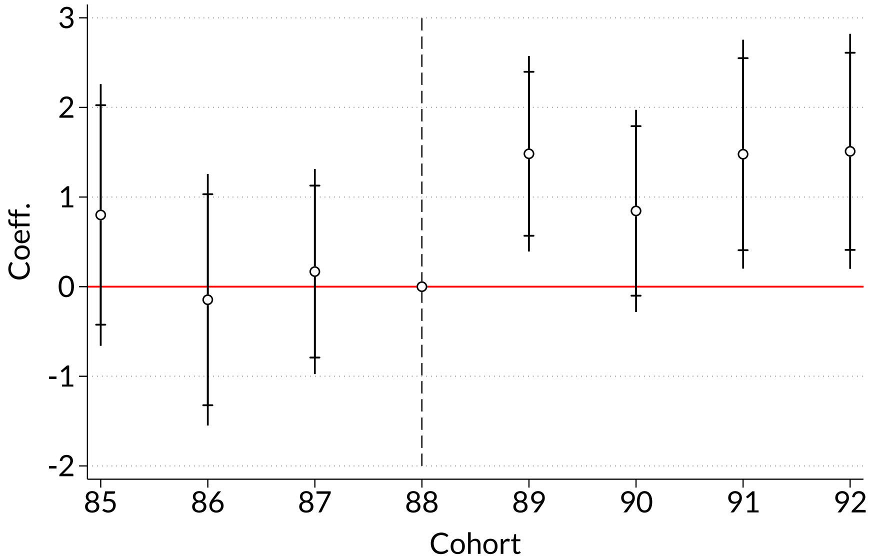
If the aforementioned changes were associated with the pre-primary construction program, the consistency of the estimates in model (1) would be threatened. However, interventions that similarly affect individuals in a given cohort and province are controlled for cohort-province fixed effects. Therefore, only the interventions that differentially affected individuals from the same cohort within the same province could bias the estimate of β_{post} in equation (1). To rule out this possibility, we test further whether the expansion of classrooms correlates with the outcomes of the cohorts not under the program. To do this, we extend the specification (1) in the following way:

$$Y_{icpj} = \mu_j + \mu_c + \sum_{c \neq 1988} \beta_c \left[d_c \times D_j \right] + \gamma_{cp} + Enroll80_{cj} + \varepsilon_{icpj} \tag{C1}$$

where d_c is a dummy variable that indicates whether the individual belongs to cohort c. The remaining terms are defined as before.

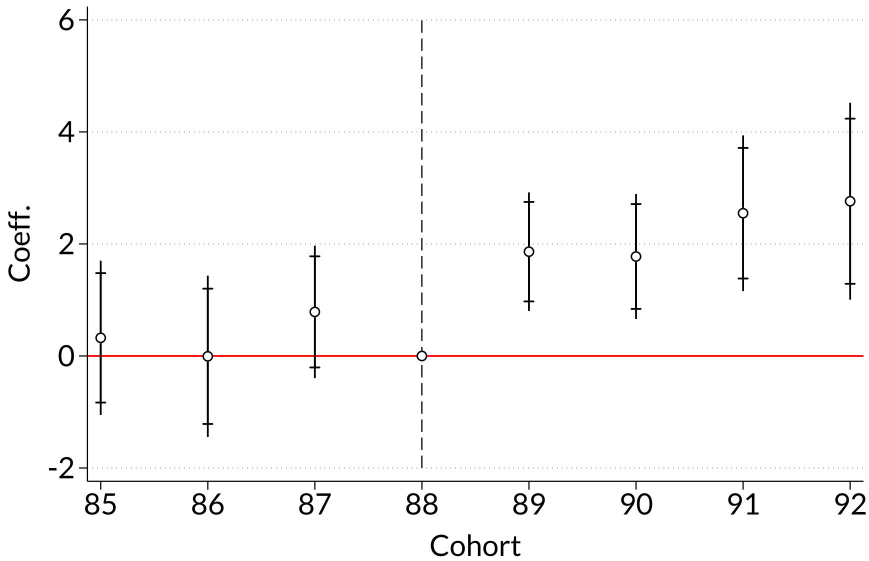
With this specification, it is possible to estimate a coefficient β_c for each cohort c, which offers at least two advantages. First, it also allows us to observe whether the policy had effects on untreated cohorts. Second, it allows us to observe whether there were heterogeneous effects among the treated cohorts, i.e., those from 1989 onwards. We present the results in the following figures.

Figure C1: Impact of new pre-primary facilities on the probability of completing lower secondary. Dynamic specification



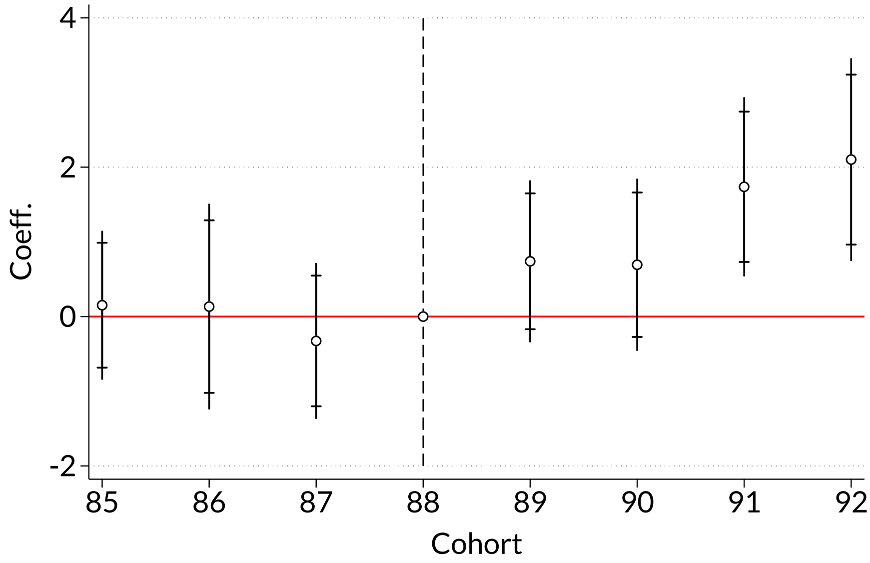
Note: The figure presents results from estimating (2) for different outcomes. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure C2: Impact of new pre-primary facilities on the probability of completing secondary. Dynamic specification



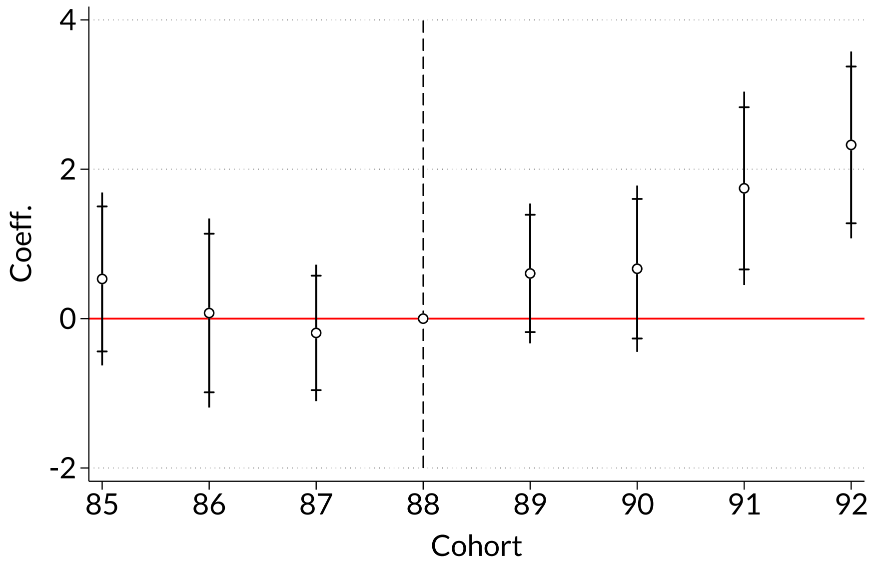
Note: The figures present the results from estimating (2) for different outcomes. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure C3: Impact of new pre-primary facilities on the probability of attaining any post-secondary education. Dynamic specification



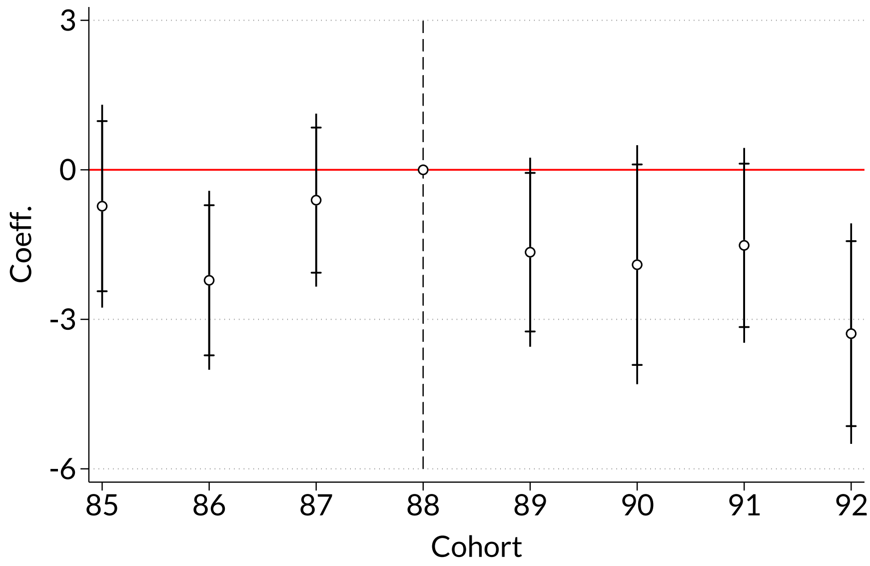
Note: The figures present the results from estimating (2) for different outcomes. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure C4: Impact of new pre-primary facilities on current tertiary or university attendance. Dynamic specification



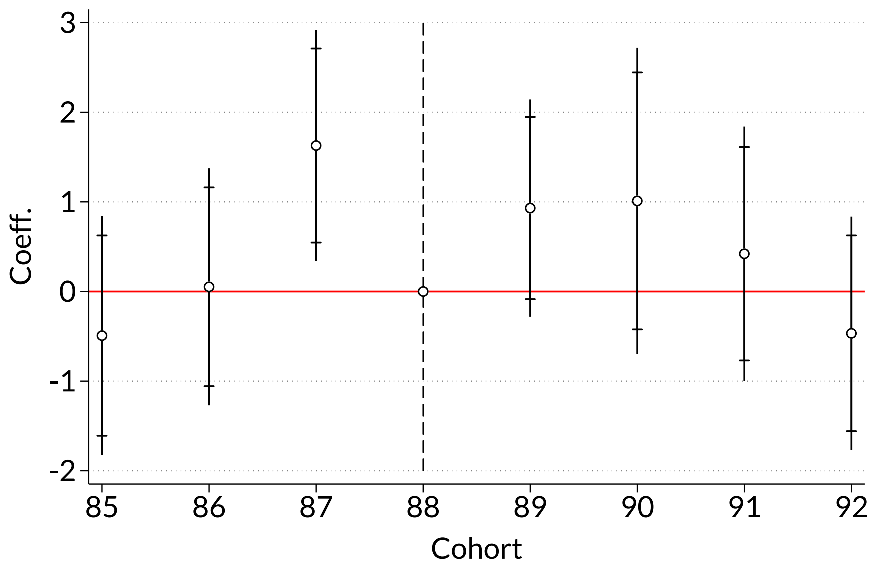
Note: The figures present the results from estimating (2) for different outcomes. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure C5: Impact of new pre-primary facilities on the probability of being a mother (only women). Dynamic specification



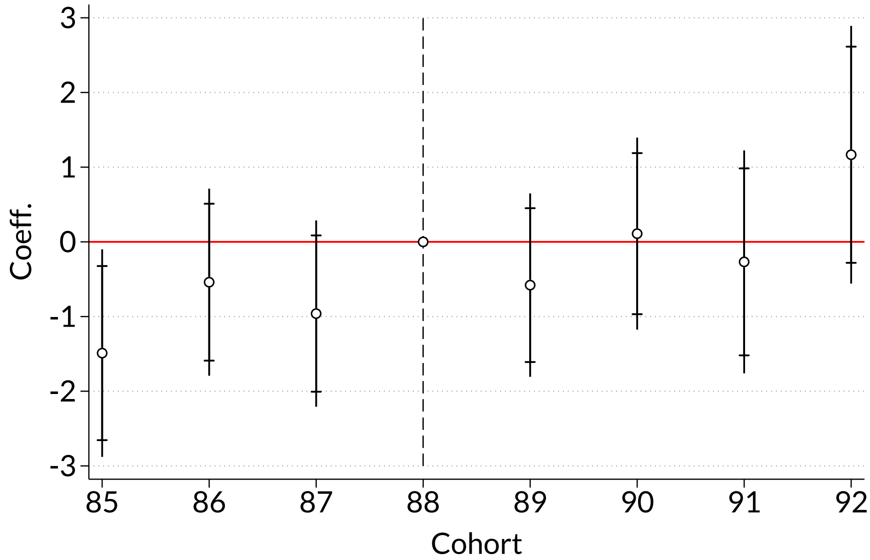
Note: The figures present the results from estimating (2) for different outcomes. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure C6: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). Dynamic specification



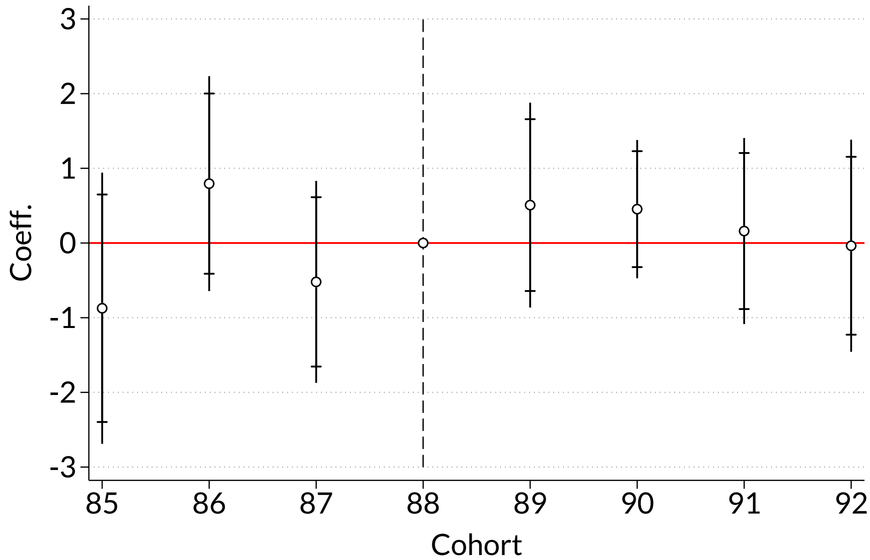
Note: The figures present the results from estimating (2) for different outcomes. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure C7: Impact of new pre-primary facilities on the probability of employment. Dynamic specification



Note: The figures present the results from estimating (2) for different outcomes. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure C8: Impact of new pre-primary facilities on the probability of formal employment. Dynamic specification

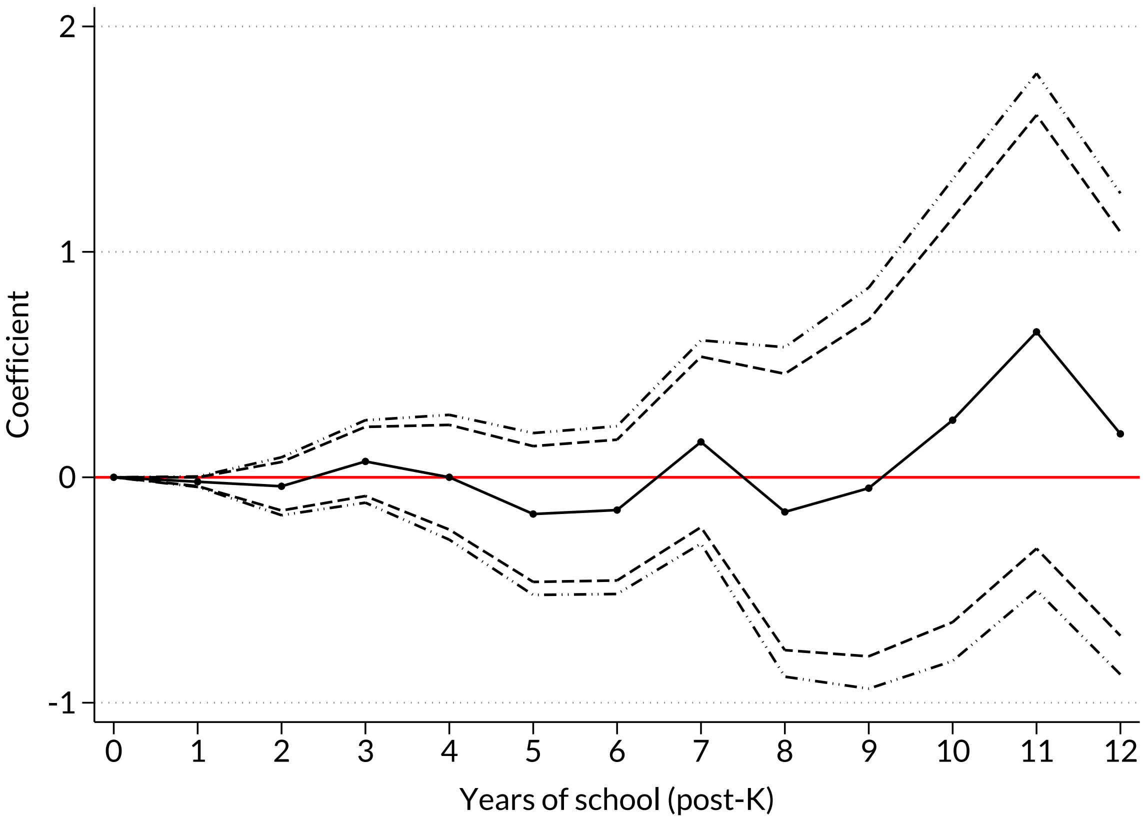


Note: The figures present the results from estimating (2) for different outcomes. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

We estimate equation (1) using S_{icpjm} as the dependent variable, which is a dummy variable indicating whether individual i completed m years of education or more. This is equivalent to estimating the impact on the cumulative distribution function for the post-kindergarten years of schooling. We present the estimates of β_{pre} for $m = 0, 1, \ldots, 12$ in Figure C9, which corresponds to basic school (primary and secondary).

We also assess heterogeneity by gender in Tables C1 and C2. Specifically, we estimate equation (1) for women and men in our sample. As we found point estimates very similar to those for the overall sample, we cannot reject the hypothesis that the effect of pre-primary education is homogeneous for males and females. This result is in line with Berlinski *et al.* (2009), who find that there is no heterogeneity by gender in the impact of pre-primary on test scores in primary.

Figure C9: Difference-in-differences in the cumulative distribution function of post-kindergarten years of school. Pre-coefficients



Note: Each point of the figure represents the estimation of β_{pre} from equation (1), where the outcome is a dummy that indicates that the individual reaches m post-kindergarten years of basic school or more, with $m = 0, 1, ..., 12$. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Table C1: Educational attainment by gender

	Years basic school		Lower secondary (pp.)		Secondary (pp.)		Post secondary (pp.)	
	Women	Men	Women	Men	Women	Men	Women	Men
ATT	0.436***	0.520***	3.975	10.034***	9.205**	14.453***	8.529**	5.443
ITT	0.083***	0.099***	0.757	1.910***	1.753**	2.752***	1.624**	1.036
	(0.031)	(0.038)	(0.659)	(0.689)	(0.790)	(0.700)	(0.709)	(0.695)
	[0.007]	[0.009]	[0.252]	[0.006]	[0.027]	[0.000]	[0.022]	[0.136]
Pre coef.	-0.009	0.023	-0.641	1.184	-0.364	1.104	0.438	-0.483
	(0.031)	(0.044)	(0.633)	(0.821)	(0.664)	(0.806)	(0.653)	(0.684)
	[0.784]	[0.605]	[0.312]	[0.150]	[0.584]	[0.171]	[0.502]	[0.480]
Mean pre-treat	9.730	9.156	59.731	50.005	47.628	36.677	24.808	14.567
N	1,001,581	998,052	1,001,581	998,052	1,001,581	998,052	1,001,581	998,052
N clusters	522	522	522	522	522	522	522	522
Diff. post p-val.	[0.742]		[0.200]		[0.322]		[0.585]	
Controls and FE								
Department FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table presents results from estimating (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups. The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. Pre coef. is the estimate of β_{pre}. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Standard errors (in parentheses) are clustered at the department-of-residence level. p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C2: Labor market outcomes by gender

	Employment (pp.)		Formal employment (pp.)	
	Women	Men	Women	Men
ATT	0.760	1.965	0.773	2.625
ITT	0.145	0.374	0.147	0.500
	(0.951)	(0.669)	(0.731)	(0.636)
	[0.879]	[0.576]	[0.840]	[0.432]
Pre coef.	-1.262	-0.541	-0.736	0.366
	(0.798)	(0.610)	(0.839)	(0.735)
	[0.114]	[0.375]	[0.381]	[0.619]
Mean pre-treat	42.454	78.329	18.816	36.588
N	1,001,581	998,052	1,001,581	998,052
N clusters	522	522	522	522
Diff. post p-val.	[0.841]			
Controls and FE				
Department FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Coh. $\times$ Province	Yes	Yes	Yes	Yes
Coh. $\times$ Enr. 80	Yes	Yes	Yes	Yes

Note: The table presents results from estimating (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups. The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. Pre coef. is the estimate of β_{pre}. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Standard errors (in parentheses) are clustered at the department-of-residence level. p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

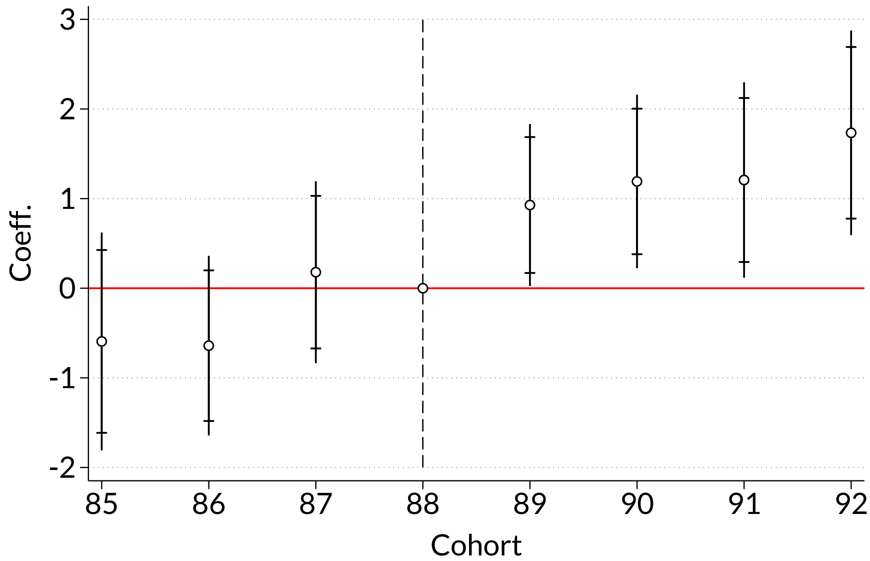
D Census data: Comparison of results with the expanded and basic questionnaire samples

In our main estimates, we used data corresponding to the extended questionnaire, which has more information than the basic questionnaire. It was applied to 16.7 million people (42% of the population of Argentina). Samples were taken in medium and large cities, and in rural areas and small agglomerates the total population was covered. In our estimates, we use a weighting that makes our results representative of the population.

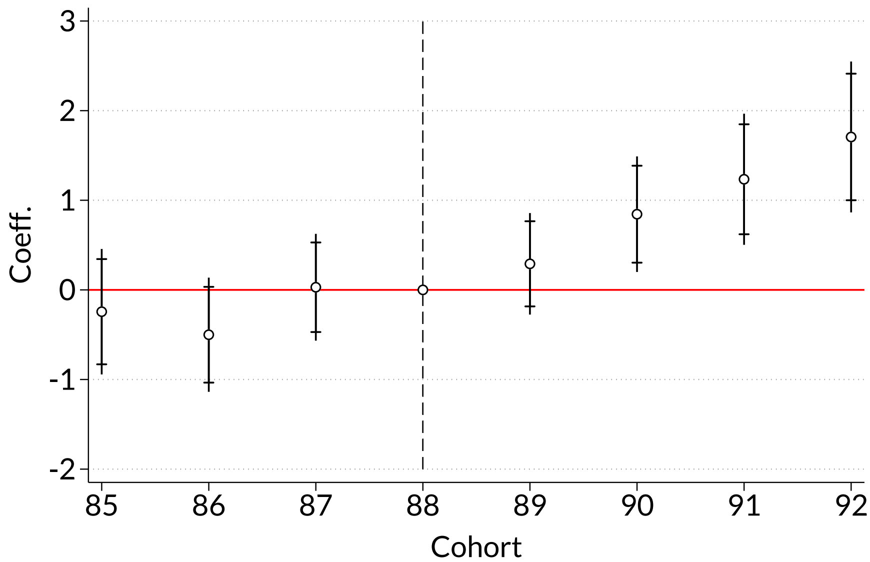
In this section, we assess the equivalence between the weighted sample and the total population. We use data from both the expanded and basic questionnaires to re-estimate our main results. To assign treatment, we also redefine cohorts, because birth date was not asked in the basic questionnaire; we therefore use declared age to define the cohort. We estimate the dynamic specification (2) for the educational outcomes and find similar results.

Figure D1: Lower secondary completed

(a) Expanded



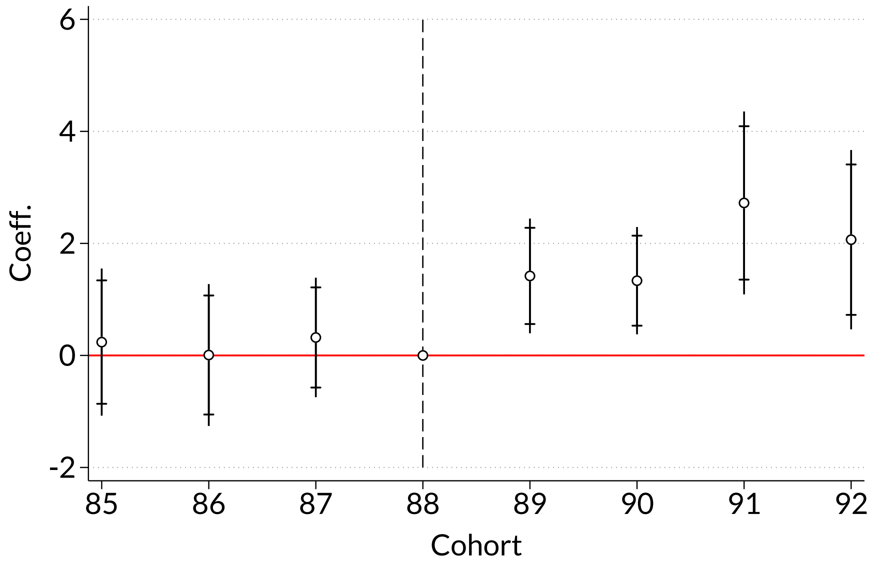
(b) Basic



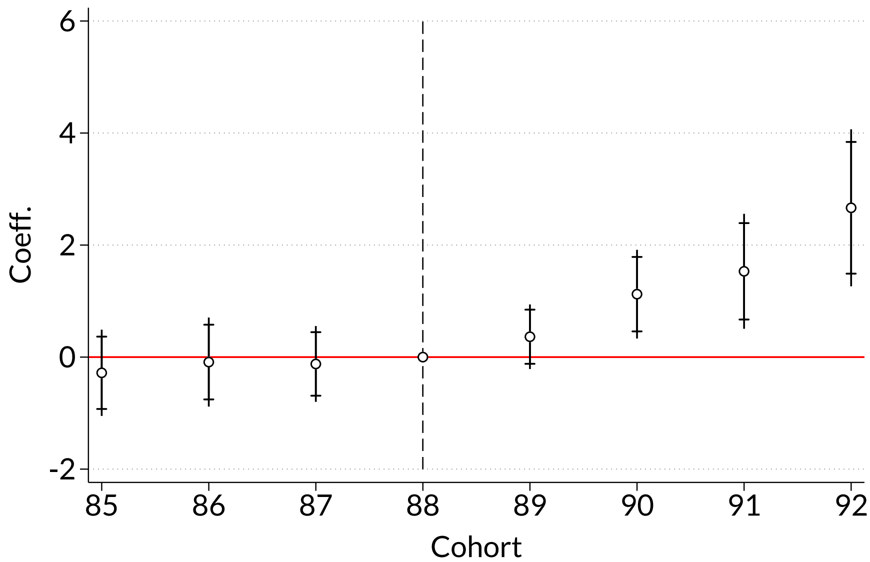
Note: The figures present the results from estimating (2) with data on basic and expanded surveys. Since date of birth is unavailable in the basic questionnaire, cohorts in both samples are defined as census year minus declared age. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure D2: Secondary completed

(a) Expanded



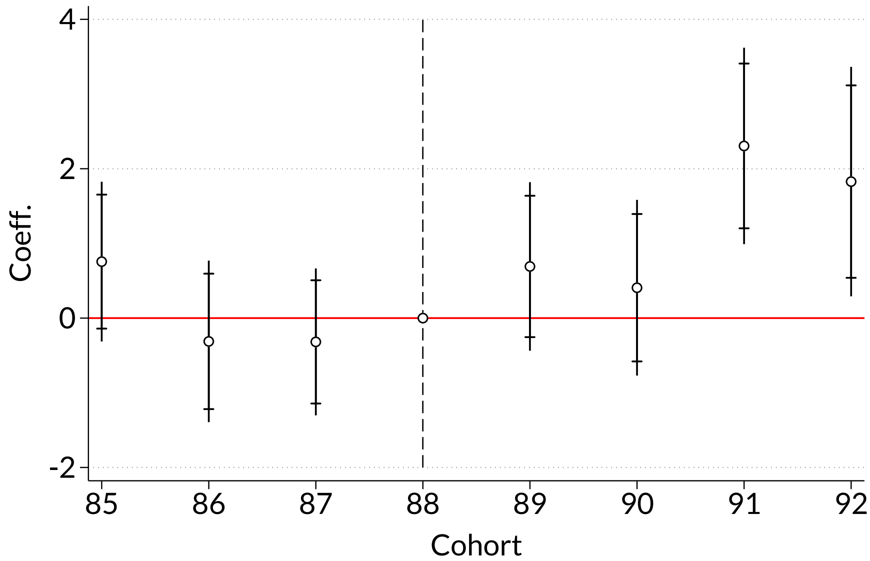
(b) Basic



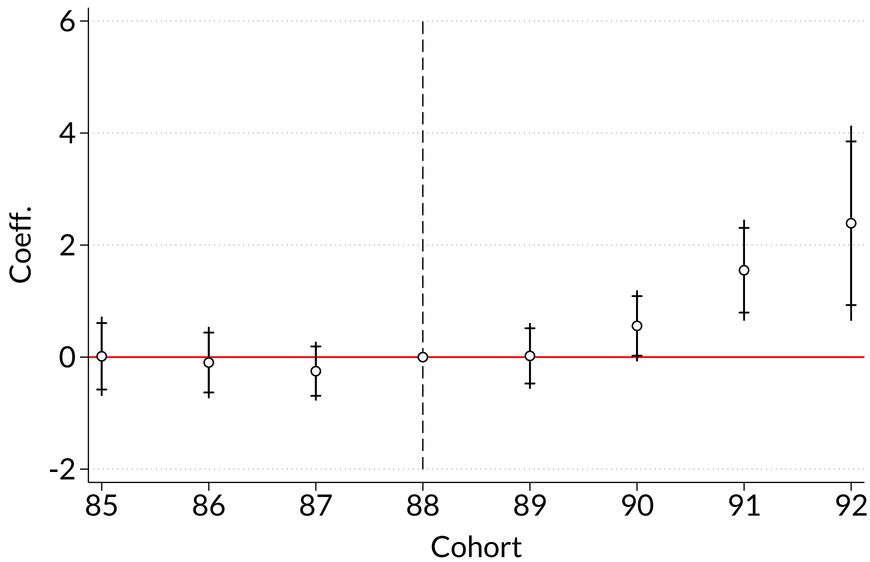
Note: The figures present the results from estimating (2) with data on basic and expanded surveys. Since date of birth is unavailable in the basic questionnaire, cohorts in both samples are defined as census year minus declared age. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure D3: Any post-secondary education

(a) Expanded



(b) Basic



Note: The figures present the results from estimating (2) with data on basic and expanded surveys. Since date of birth is unavailable in the basic questionnaire, cohorts in both samples are defined as census year minus declared age. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary *gross enrollment* in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

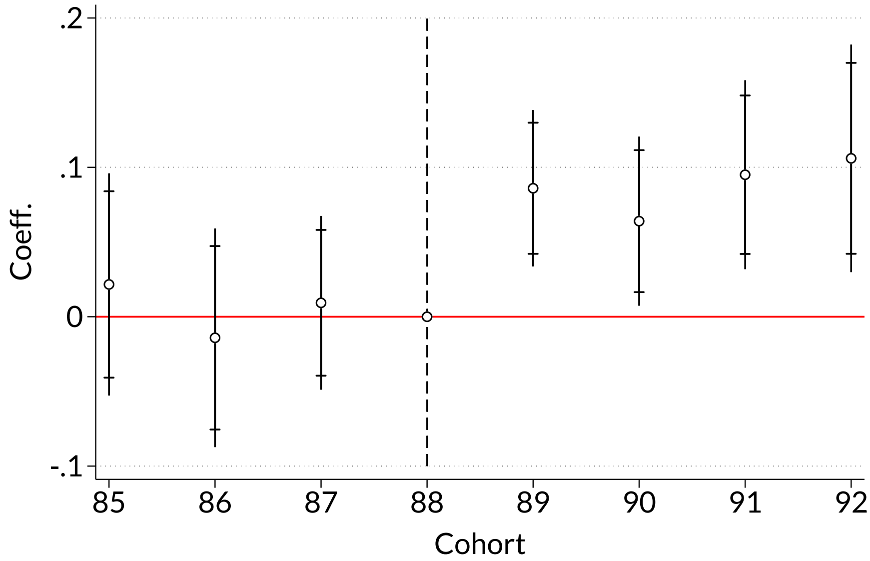
E Alternative difference-in-differences estimators

Recent literature on difference-in-differences has proposed alternative methods that are robust to various flaws in the traditional two-way fixed effects (TWFE) approach (Borusyak *et al.*, 2024; Callaway & Sant'Anna, 2021; Dube *et al.*, 2025; Goodman-Bacon, 2021; Sun & Abraham, 2021). Specifically, the TWFE estimator is a weighted average of all possible two-group and two-time (2×2) difference-in-differences comparisons, where heterogeneity in treatment effects across groups and time can lead to negative weights. As a result, the TWFE estimator may yield a treatment effect estimate with the opposite sign of the true effect. This issue arises when "forbidden comparisons" occur, where treated units are mistakenly included in the control group.

To address these concerns, in this section we present estimates for our main results using the approach of Dube *et al.* (2025), Wooldridge (2025), Sun and Abraham (2021) and Callaway and Sant'Anna (2021). The results of this analysis are displayed in the following figures. These figures show that the estimated treatment effects and their patterns are similar to those presented in Appendix C for our main dynamic specification, suggesting that potential flaws in the TWFE specification are not a major concern in our setting.

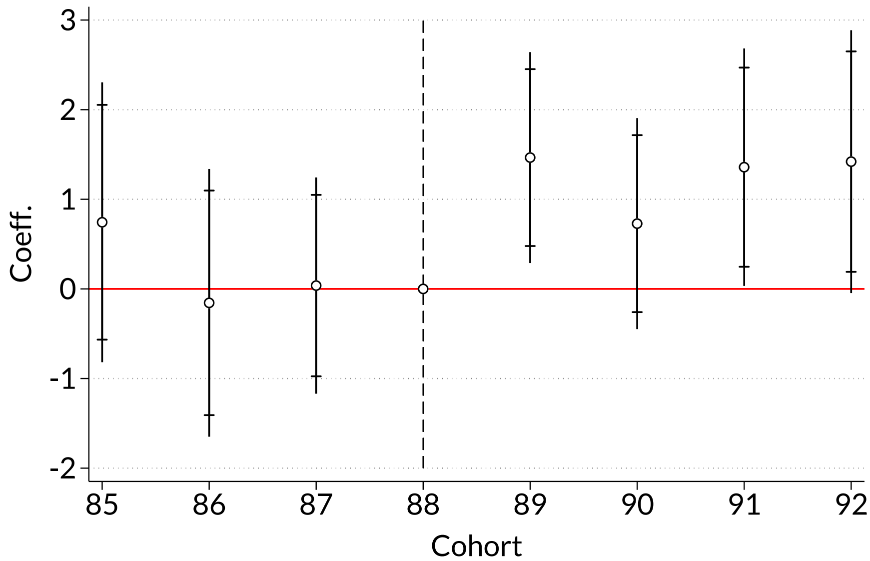
E.1 Results based on Dube *et al.* (2025) (LP-DID)

Figure E1: Impact of new pre-primary facilities on the years of basic school. LP-DID



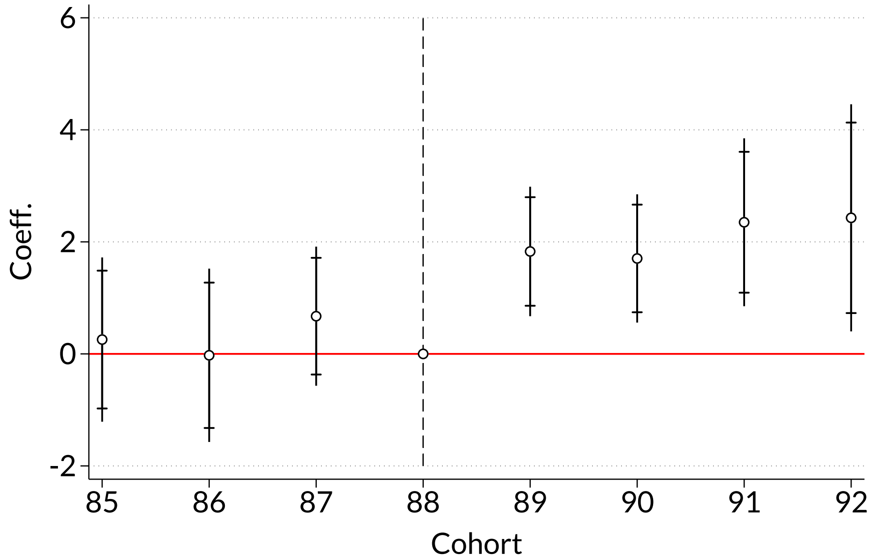
Note: The figure presents the results of the estimate of the effect on the years of basic school using Dube *et al.* (2025). The estimates use data collapsed to the department-by-cohort level. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. SE clustered by department of residence.

Figure E2: Impact of new pre-primary facilities on the probability of completing lower secondary. LP-DID



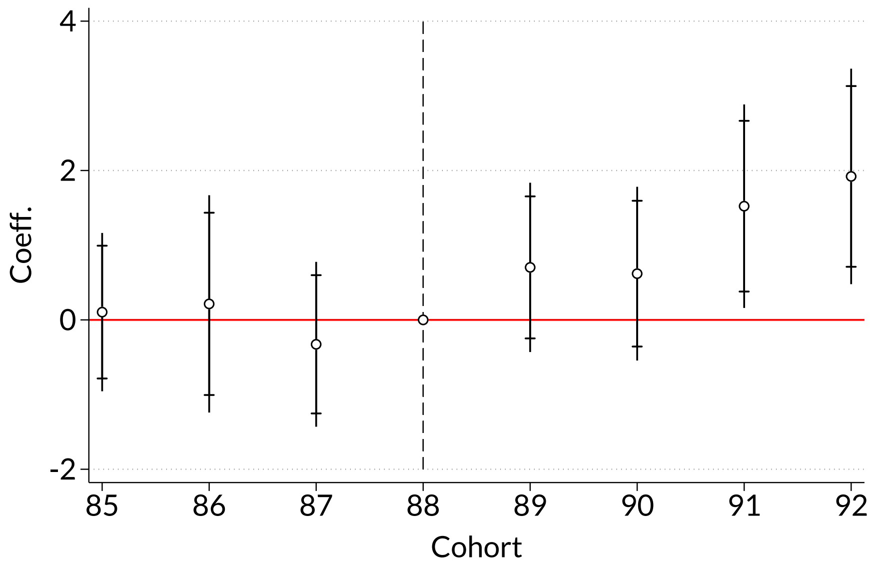
Note: The figure presents the results of the estimate of the effect on the probability of completing lower secondary using Dube *et al.* (2025). The estimates use data collapsed to the department-by-cohort level. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. SE clustered by department of residence.

Figure E3: Impact of new pre-primary facilities on the probability of completing secondary. LP-DID



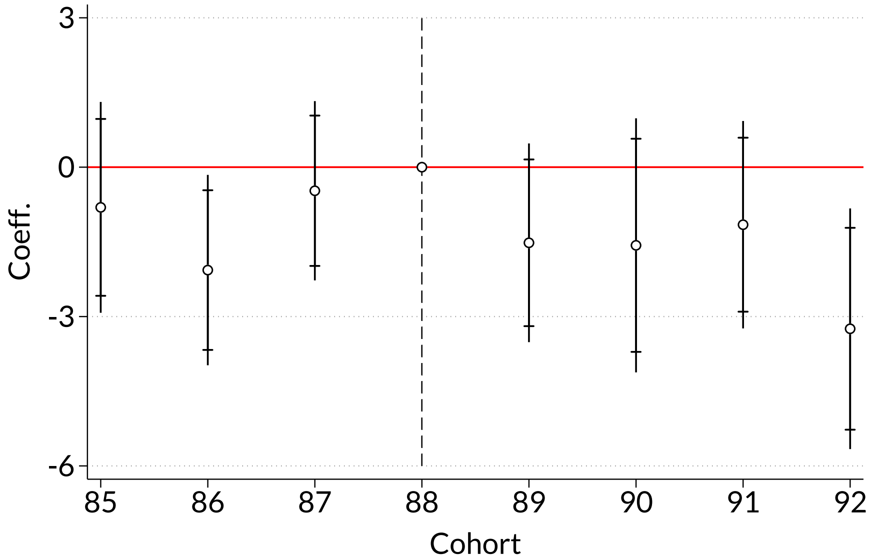
Note: The figure presents the results of the estimate of the effect on the probability of completing secondary using Dube *et al.* (2025). The estimates use data collapsed to the department-by-cohort level. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. SE clustered by department of residence.

Figure E4: Impact of new pre-primary facilities on the probability of attaining any post-secondary education. LP-DID



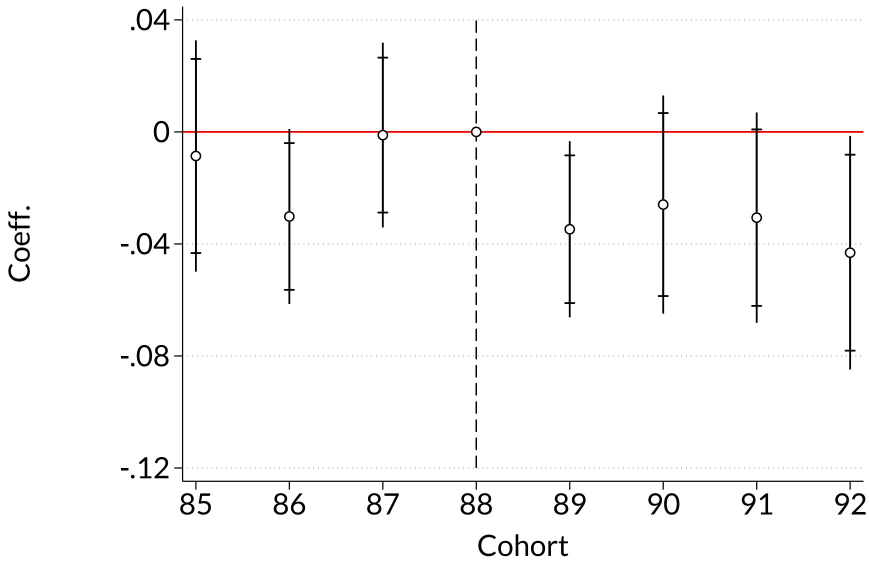
Note: The figure presents the results of the estimate of the effect on the probability of attaining any post-secondary education using Dube *et al.* (2025). The estimates use data collapsed to the department-by-cohort level. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. SE clustered by department of residence.

Figure E5: Impact of new pre-primary facilities on the probability of being a mother (only women). LP-DID



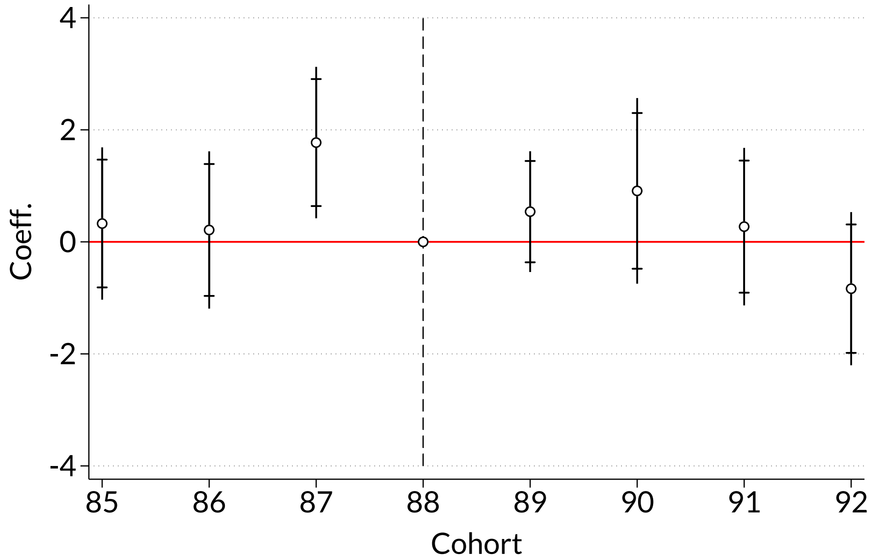
Note: The figure presents the results of the estimate of the effect on the probability of being a mother using Dube *et al.* (2025). The estimates use data collapsed to the department-by-cohort level. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. SE clustered by department of residence.

Figure E6: Impact of new pre-primary facilities on the total number of live births (only women). LP-DID



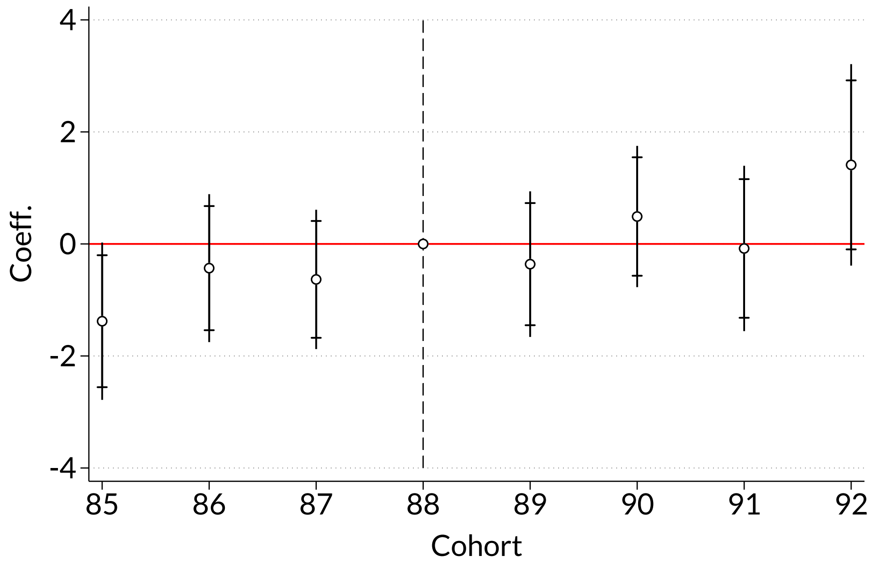
Note: The figure presents the results of the estimate of the effect on the total number of live births per woman using Dube *et al.* (2025). The estimates use data collapsed to the department-by-cohort level. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. SE clustered by department of residence.

Figure E7: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). LP-DID



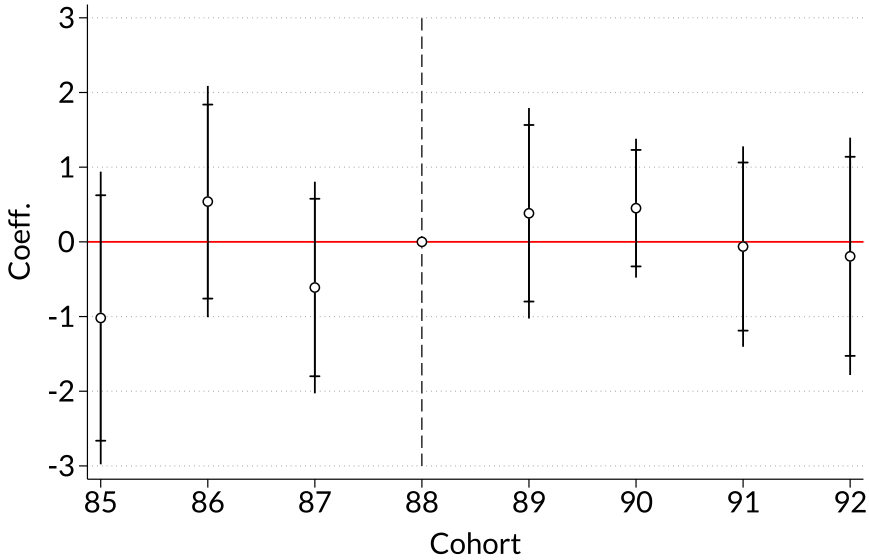
Note: The figure presents the results of the estimate of the effect on the probability of teenage motherhood using Dube *et al.* (2025). The estimates use data collapsed to the department-by-cohort level. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. SE clustered by department of residence.

Figure E8: Impact of new pre-primary facilities on the probability of employment. LP-DID



Note: The figure presents the results of the estimate of the effect on the probability of employment using Dube *et al.* (2025). The estimates use data collapsed to the department-by-cohort level. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

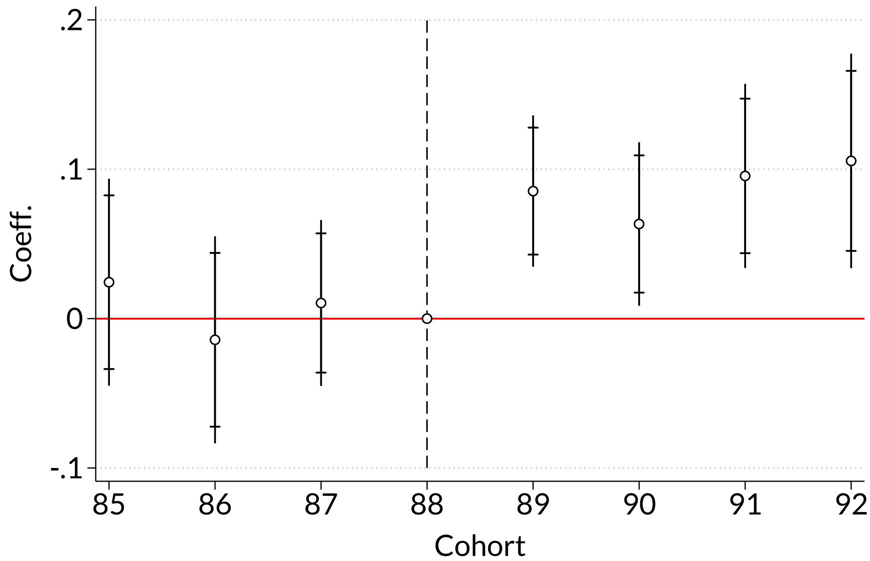
Figure E9: Impact of new pre-primary facilities on the probability of formal employment. LP-DID



Note: The figure presents the results of the estimate of the effect on the probability of formal employment using Dube *et al.* (2025). The estimates use data collapsed to the department-by-cohort level. Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

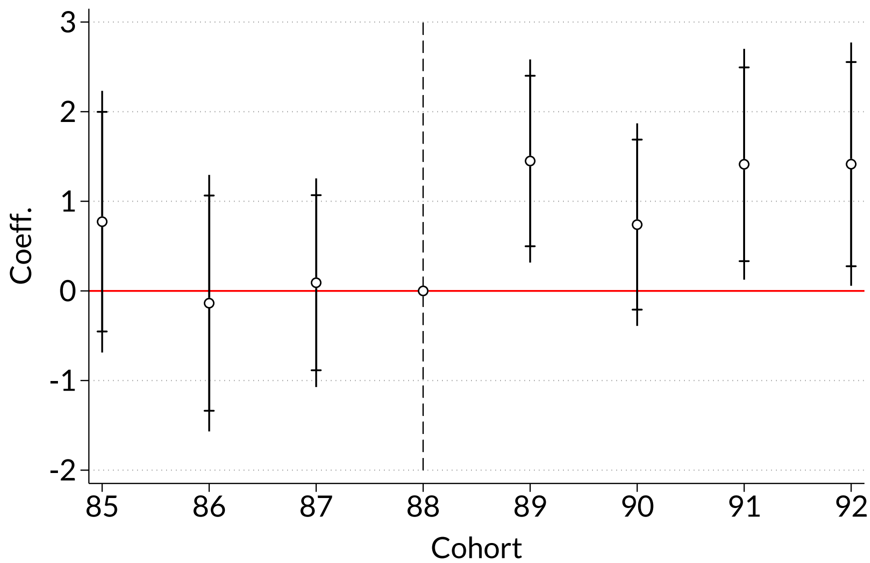
E.2 Estimation using the approach of Wooldridge (2023, 2025) (JW-DID)

Figure E10: Impact of new pre-primary facilities on the years of basic school. JW-DID



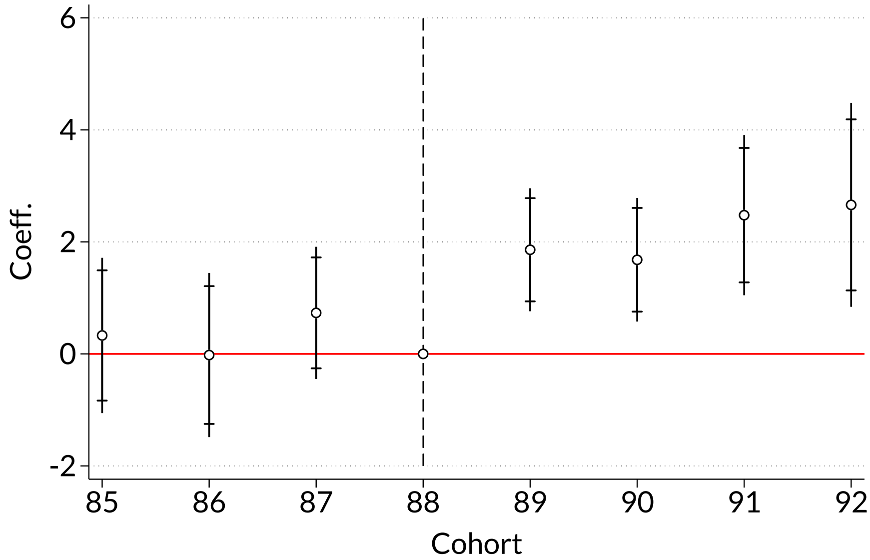
Note: The figure presents the results of the estimate of the effect on the years of basic school using Wooldridge (2023, 2025). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E11: Impact of new pre-primary facilities on the probability of completing lower secondary. JW-DID



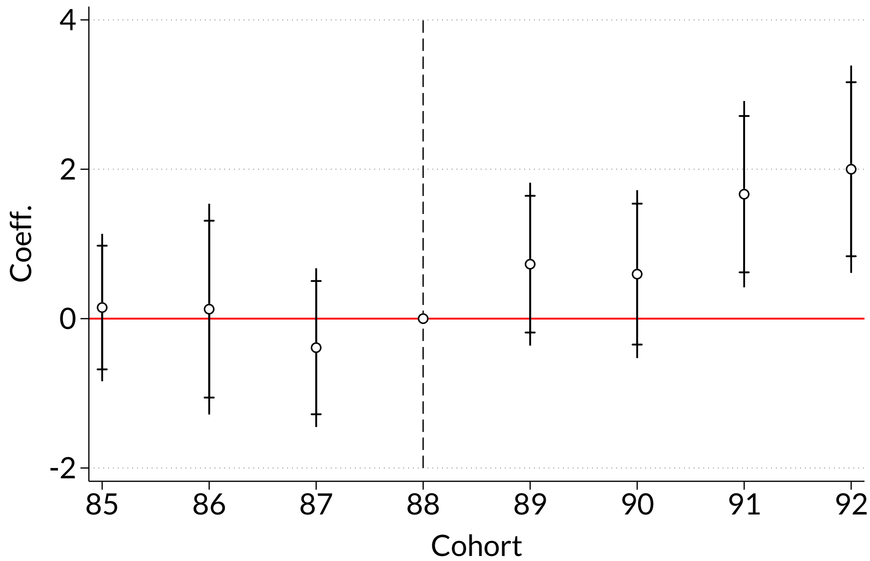
Note: The figure presents the results of the estimate of the effect on the probability of completing lower secondary using Wooldridge (2023, 2025). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E12: Impact of new pre-primary facilities on the probability of completing secondary. JW-DID



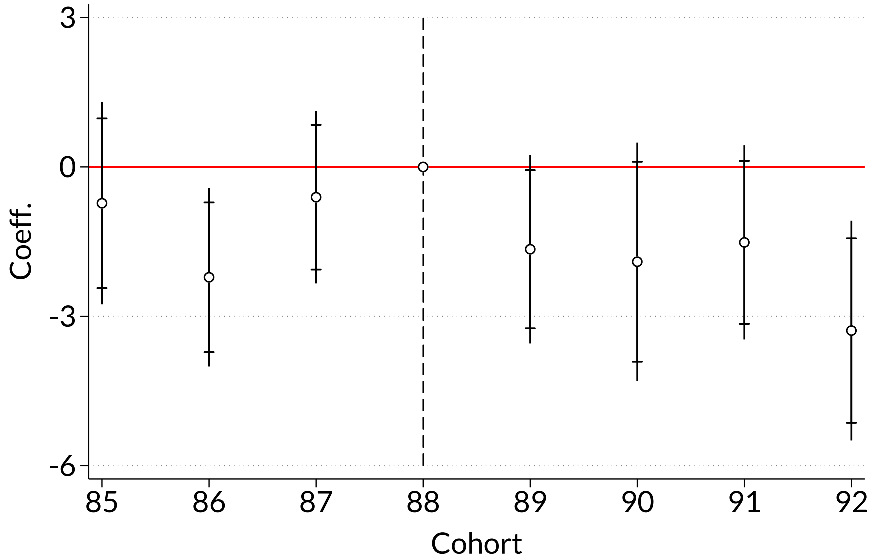
Note: The figure presents the results of the estimate of the effect on the probability of completing secondary using Wooldridge (2023, 2025). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E13: Impact of new pre-primary facilities on the probability of attaining any post-secondary education. JW-DID



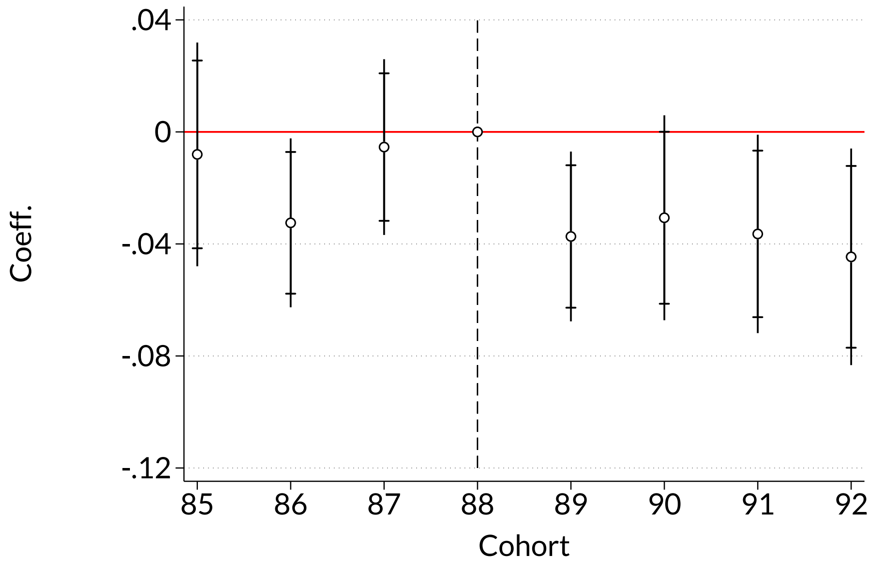
Note: The figure presents the results of the estimate of the effect on the probability of attaining any post-secondary education using Wooldridge (2023, 2025). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E14: Impact of new pre-primary facilities on the probability of being a mother (only women). JW-DID



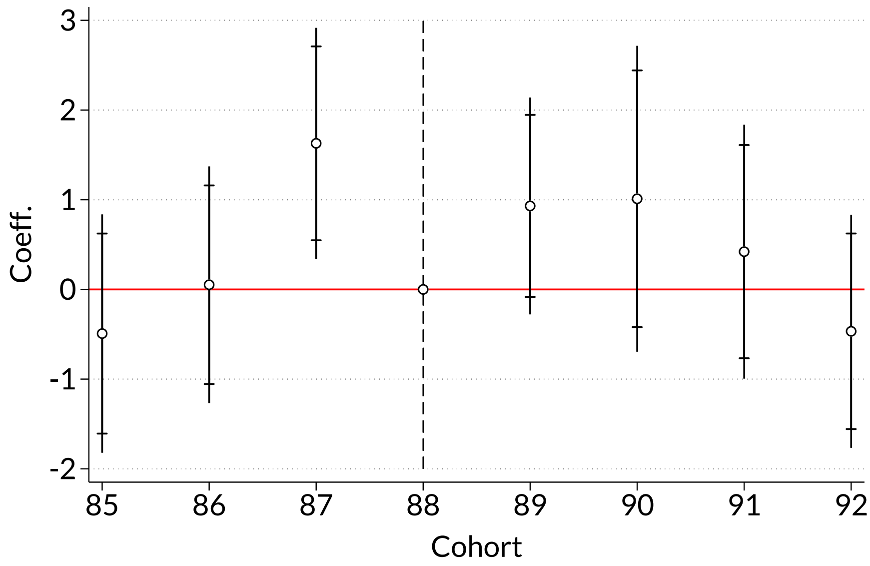
Note: The figure presents the results of the estimate of the effect on the probability of being a mother using Wooldridge (2023, 2025). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E15: Impact of new pre-primary facilities on the total number of live births (only women). JW-DID



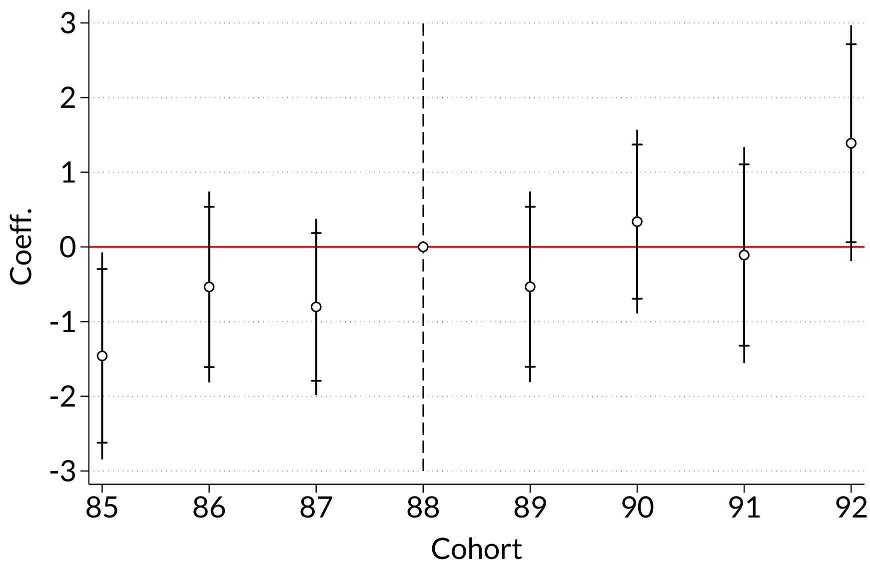
Note: The figure presents the results of the estimate of the effect on the total number of live births per woman using Wooldridge (2023, 2025). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E16: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). JW-DID



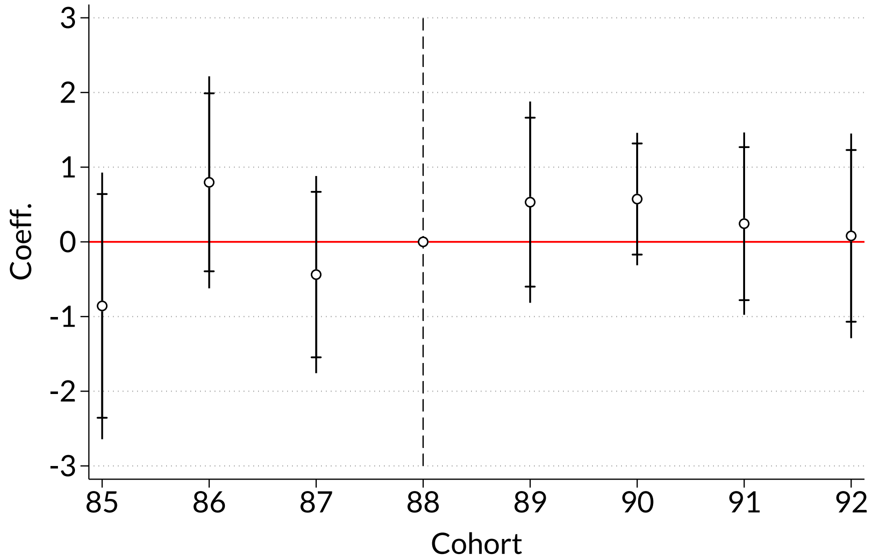
Note: The figure presents the results of the estimate of the effect on the probability of teenage motherhood using Wooldridge (2023, 2025). Each point reports an estimate of $\beta_c, c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E17: Impact of new pre-primary facilities on the probability of employment. JW-DID



Note: The figure presents the results of the estimate of the effect on the probability of employment using Wooldridge (2023, 2025). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

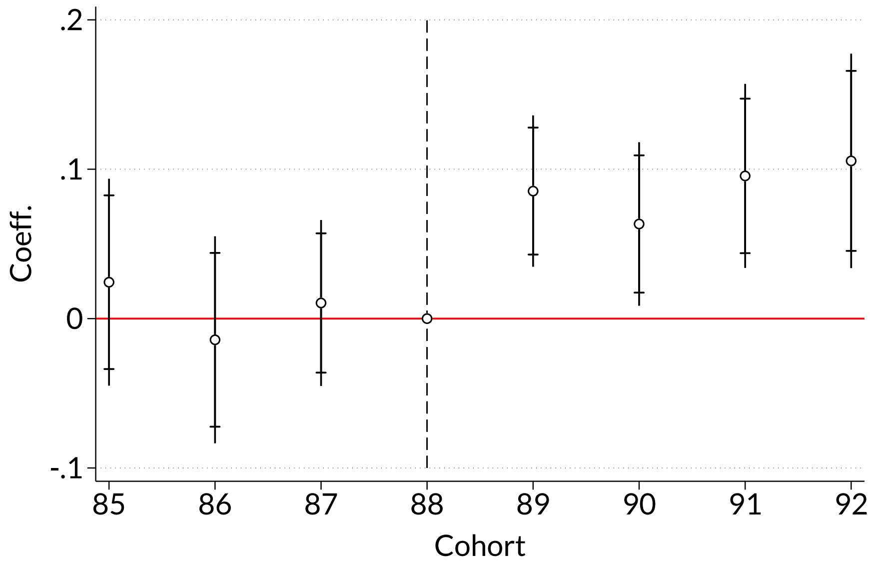
Figure E18: Impact of new pre-primary facilities on the probability of formal employment. JW-DID



Note: The figure presents the results of the estimate of the effect on the probability of formal employment using Wooldridge (2023, 2025). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

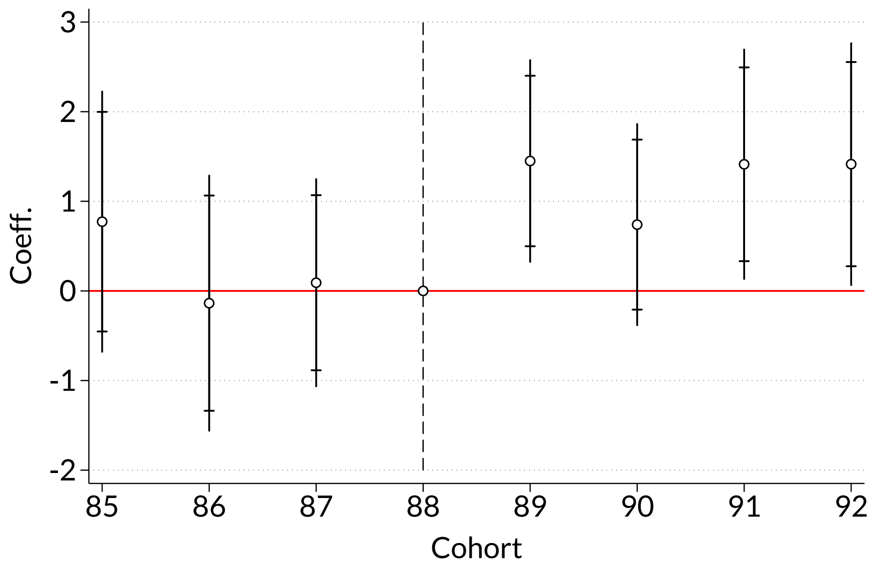
E.3 Estimation using the approach of Sun and Abraham (2021) (SA-DID)

Figure E19: Impact of new pre-primary facilities on the years of basic school. SA-DID



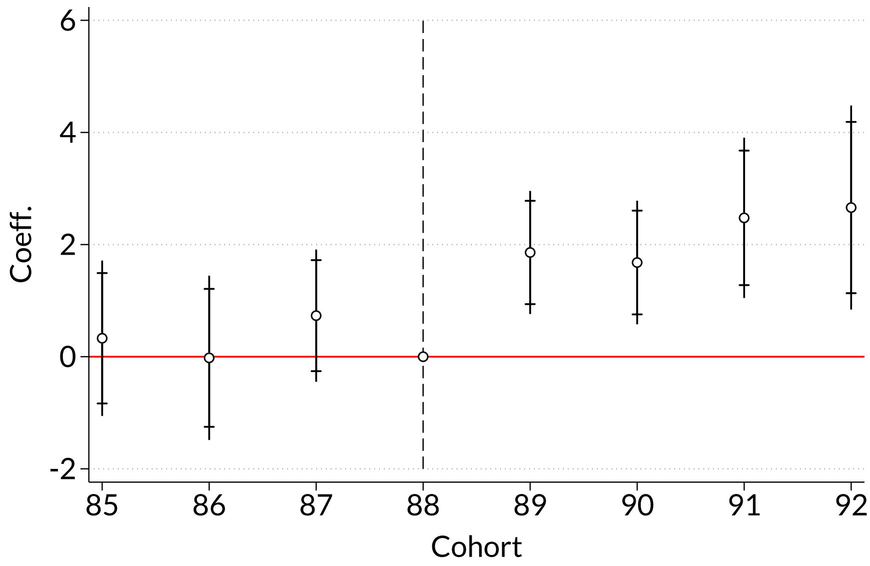
Note: The figure presents the results of the estimate of the effect on the years of basic school using Sun and Abraham (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E20: Impact of new pre-primary facilities on the probability of completing lower secondary. SA-DID



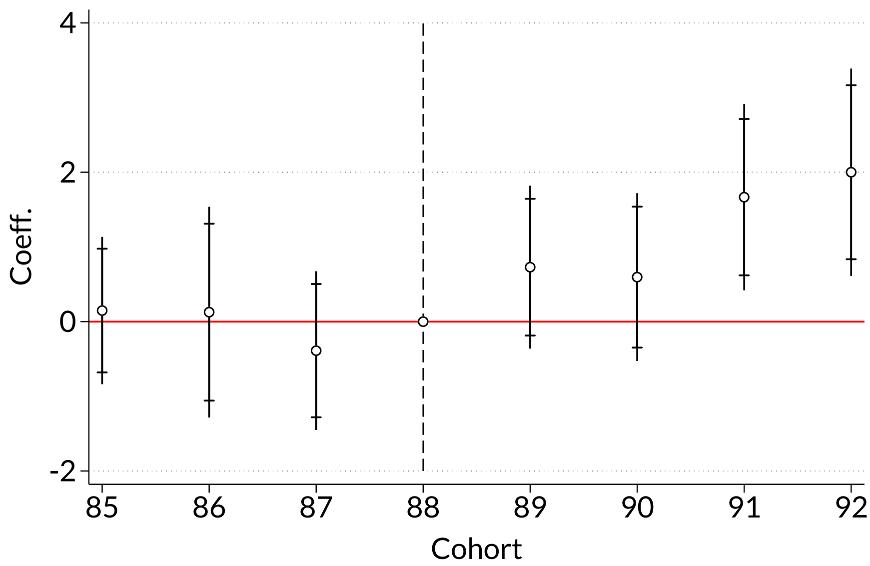
Note: The figure presents the results of the estimate of the effect on the probability of completing lower secondary using Sun and Abraham (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E21: Impact of new pre-primary facilities on the probability of completing secondary. SA-DID



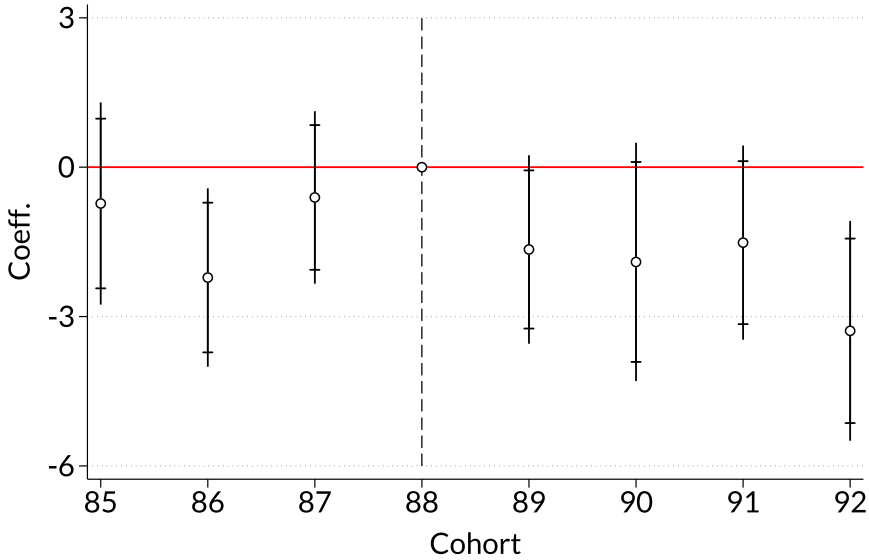
Note: The figure presents the results of the estimate of the effect on the probability of completing secondary using Sun and Abraham (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E22: Impact of new pre-primary facilities on the probability of attaining any post-secondary education. SA-DID



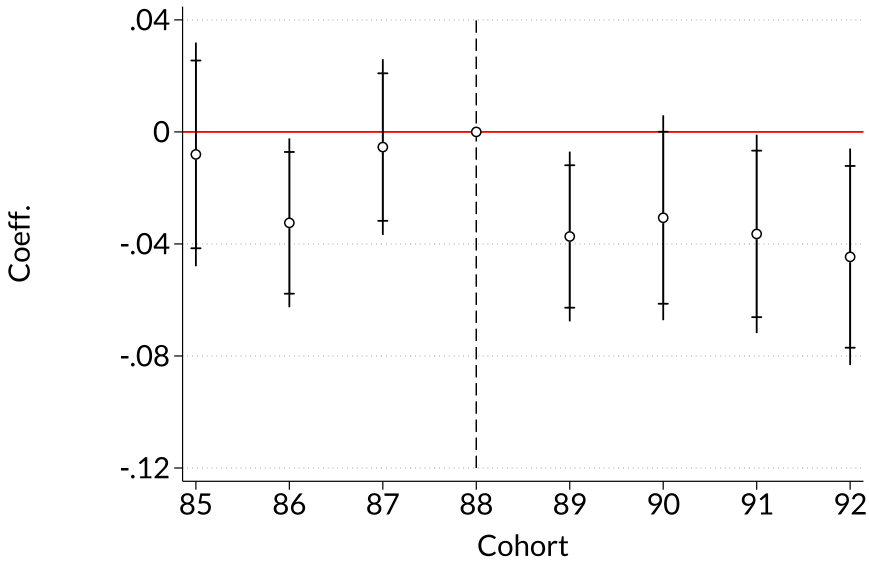
Note: The figure presents the results of the estimate of the effect on the probability of attaining any post-secondary education using Sun and Abraham (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E23: Impact of new pre-primary facilities on the probability of being a mother (only women). SA-DID



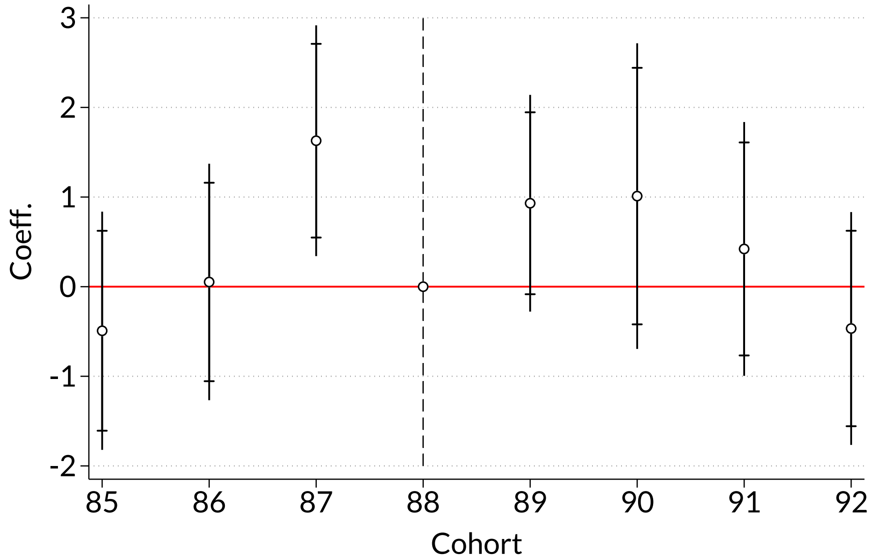
Note: The figure presents the results of the estimate of the effect on the probability of being a mother using Sun and Abraham (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E24: Impact of new pre-primary facilities on the total number of live births (only women). SA-DID



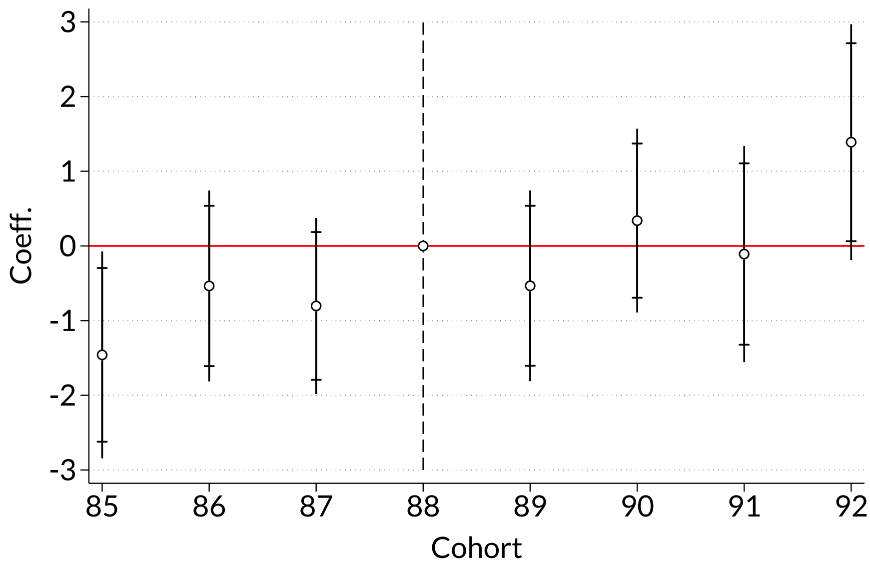
Note: The figure presents the results of the estimate of the effect on the total number of live births per woman using Sun and Abraham (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E25: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). SA-DID



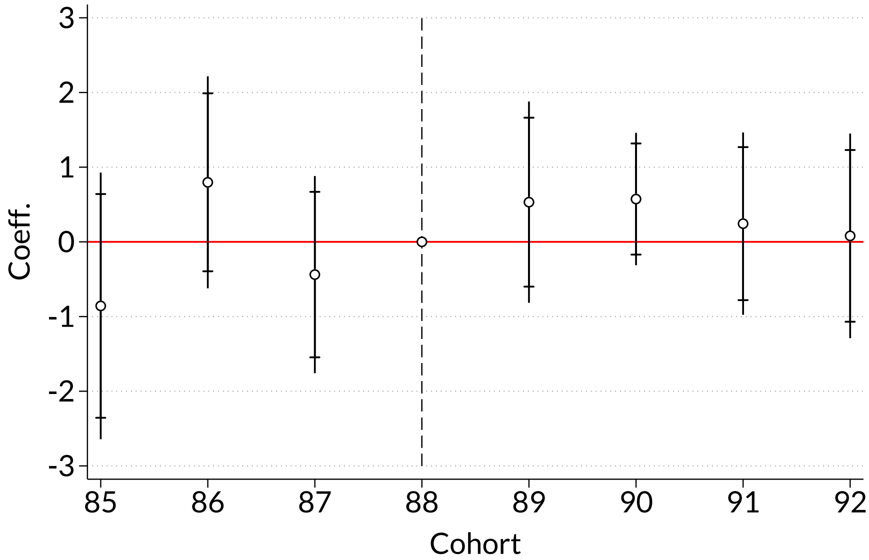
Note: The figure presents the results of the estimate of the effect on the probability of teenage motherhood using Sun and Abraham (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E26: Impact of new pre-primary facilities on the probability of employment. SA-DID



Note: The figure presents the results of the estimate of the effect on the probability of employment using Sun and Abraham (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

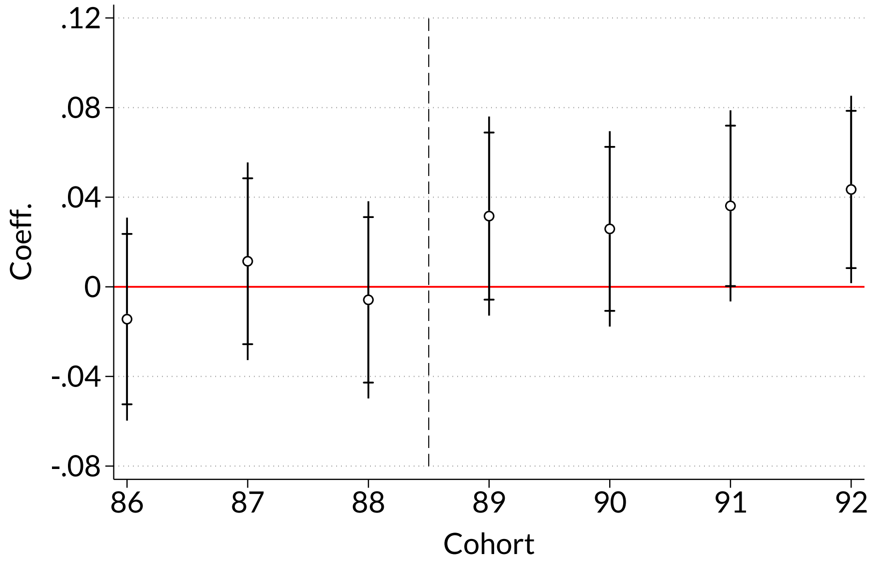
Figure E27: Impact of new pre-primary facilities on the probability of formal employment.SA-DID



Note: The figure presents the results of the estimate of the effect on the probability of formal employment using Sun and Abraham (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

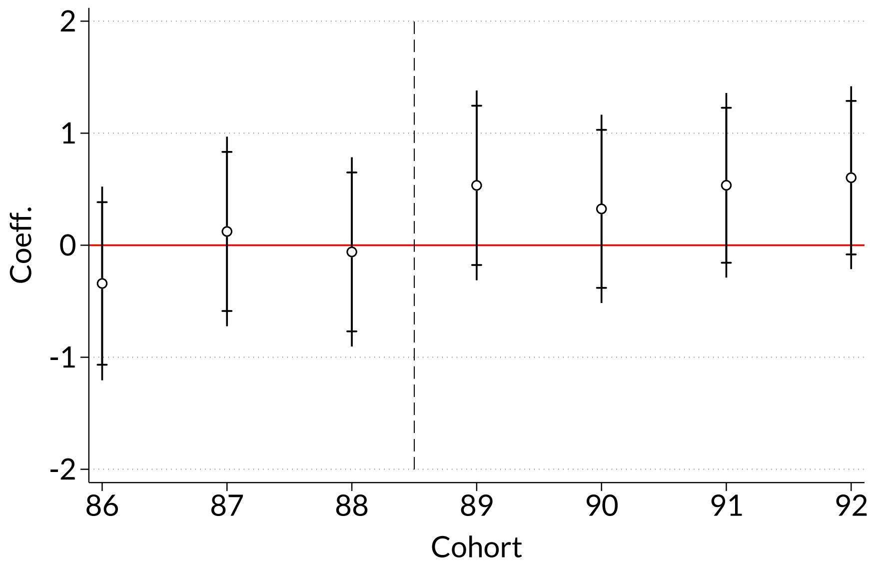
E.4 Estimation using the approach of Callaway and Sant'Anna (2021) (CS-DID)

Figure E28: Impact of new pre-primary facilities on the years of basic school. CS-DID



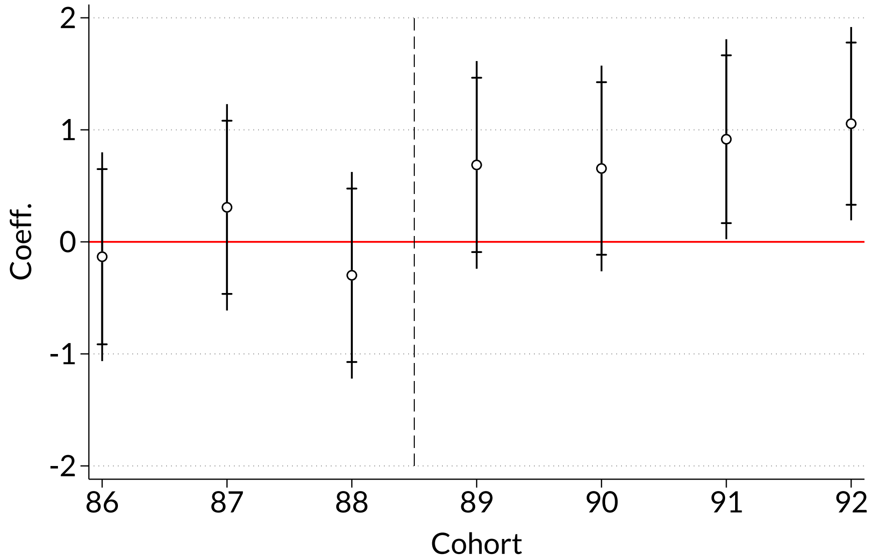
Note: The figure presents the results of the estimate of the effect on the years of basic school using Callaway and Sant'Anna (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. Before applying CS-DID, we residualize each outcome on cohort-by-province fixed effects, cohort-by-1980 pre-primary gross enrollment trends, and a female indicator when the sample includes both genders. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E29: Impact of new pre-primary facilities on the probability of completing lower secondary. CS-DID



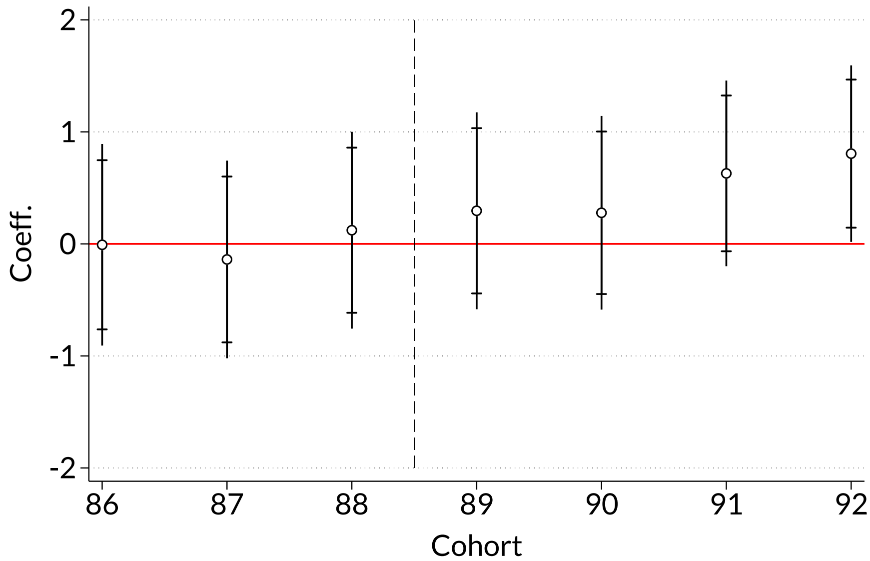
Note: The figure presents the results of the estimate of the effect on the probability of completing lower secondary using Callaway and Sant'Anna (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. Before applying CS-DID, we residualize each outcome on cohort-by-province fixed effects, cohort-by-1980 pre-primary gross enrollment trends, and a female indicator when the sample includes both genders. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E30: Impact of new pre-primary facilities on the probability of completing secondary. CS-DID



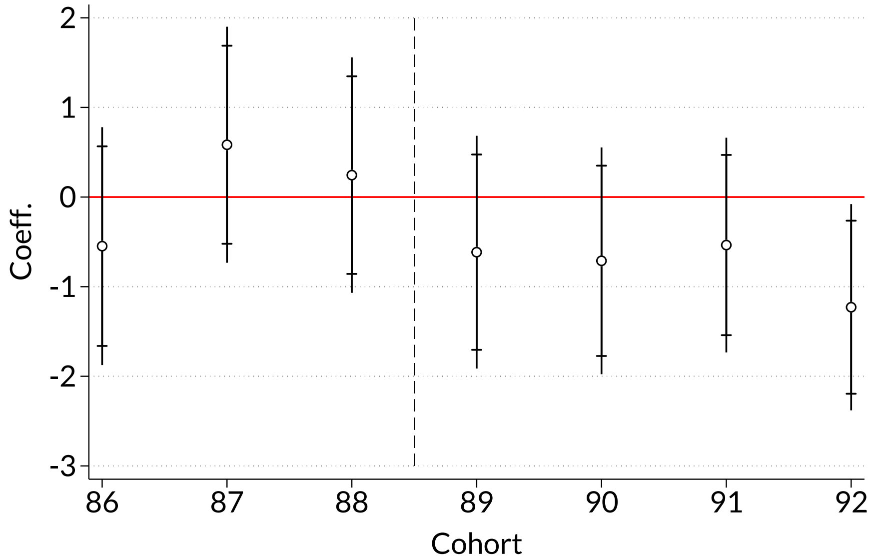
Note: The figure presents the results of the estimate of the effect on the probability of completing secondary using Callaway and Sant'Anna (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. Before applying CS-DID, we residualize each outcome on cohort-by-province fixed effects, cohort-by-1980 pre-primary gross enrollment trends, and a female indicator when the sample includes both genders. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E31: Impact of new pre-primary facilities on the probability of attaining any post-secondary education. CS-DID



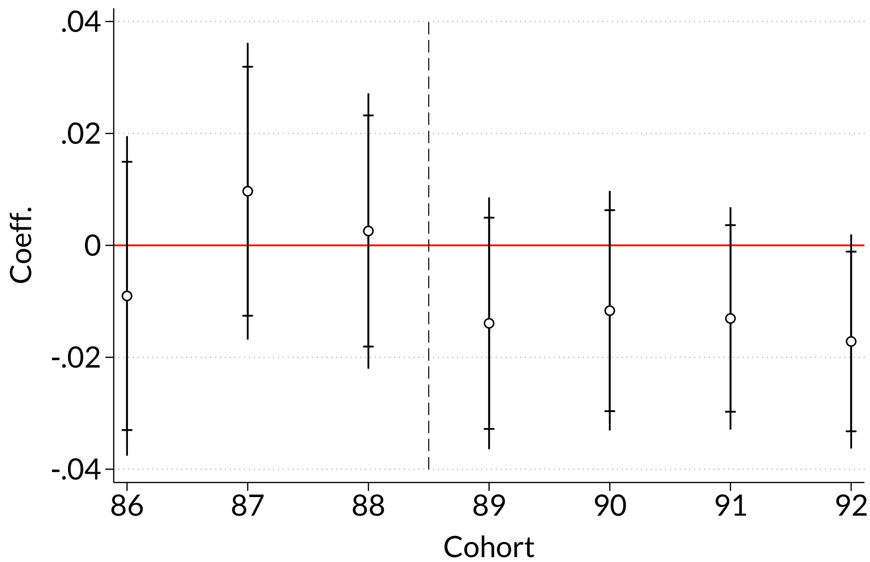
Note: The figure presents the results of the estimate of the effect on the probability of attaining any post-secondary education using Callaway and Sant'Anna (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. Before applying CS-DID, we residualize each outcome on cohort-by-province fixed effects, cohort-by-1980 pre-primary gross enrollment trends, and a female indicator when the sample includes both genders. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E32: Impact of new pre-primary facilities on the probability of being a mother (only women). CS-DID



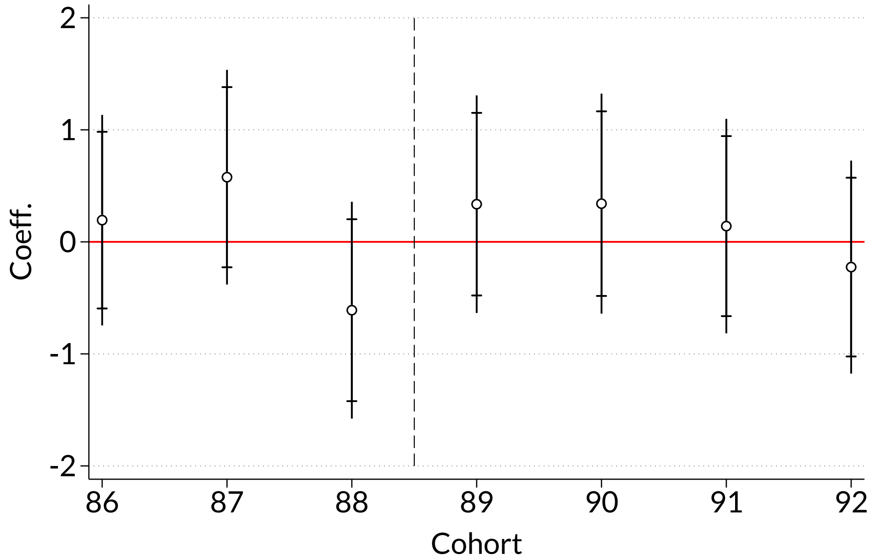
Note: The figure presents the results of the estimate of the effect on the probability of being a mother using Callaway and Sant'Anna (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. Before applying CS-DID, we residualize each outcome on cohort-by-province fixed effects, cohort-by-1980 pre-primary gross enrollment trends, and a female indicator when the sample includes both genders. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E33: Impact of new pre-primary facilities on the total number of live births (only women). CS-DID



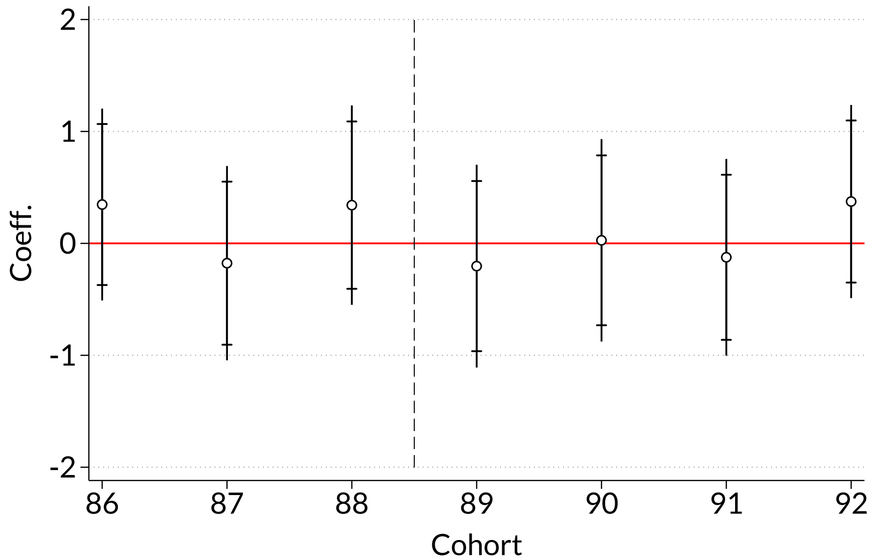
Note: The figure presents the results of the estimate of the effect on the total number of live births per woman using Callaway and Sant'Anna (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. Before applying CS-DID, we residualize each outcome on cohort-by-province fixed effects, cohort-by-1980 pre-primary gross enrollment trends, and a female indicator when the sample includes both genders. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E34: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). CS-DID



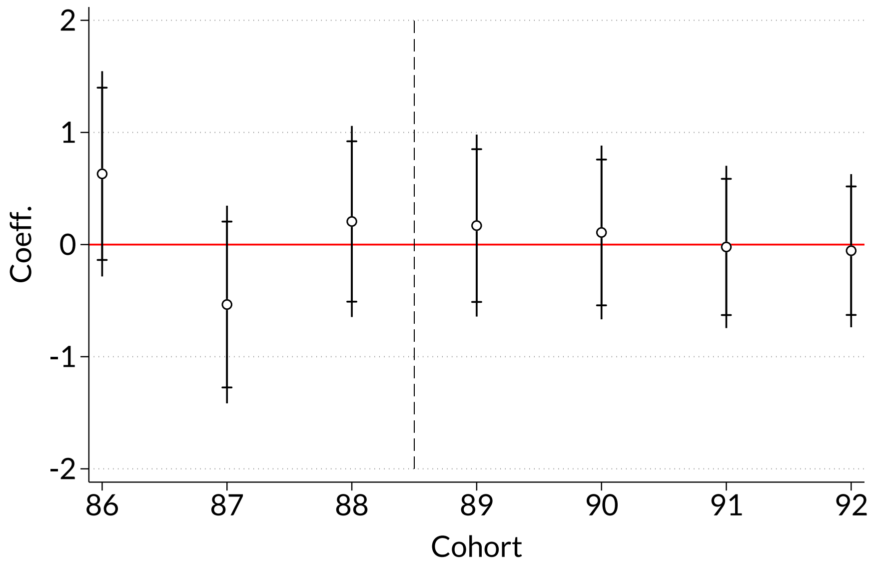
Note: The figure presents the results of the estimate of the effect on the probability of teenage motherhood using Callaway and Sant'Anna (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. Before applying CS-DID, we residualize each outcome on cohort-by-province fixed effects, cohort-by-1980 pre-primary gross enrollment trends, and a female indicator when the sample includes both genders. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E35: Impact of new pre-primary facilities on the probability of employment. CS-DID



Note: The figure presents the results of the estimate of the effect on the probability of employment using Callaway and Sant'Anna (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. Before applying CS-DID, we residualize each outcome on cohort-by-province fixed effects, cohort-by-1980 pre-primary gross enrollment trends, and a female indicator when the sample includes both genders. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

Figure E36: Impact of new pre-primary facilities on the probability of formal employment. CS-DID



Note: The figure presents the results of the estimate of the effect on the probability of formal employment using Callaway and Sant'Anna (2021). Each point reports an estimate of β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. Before applying CS-DID, we residualize each outcome on cohort-by-province fixed effects, cohort-by-1980 pre-primary gross enrollment trends, and a female indicator when the sample includes both genders. Confidence intervals at 90% and 95% are presented. Standard errors are clustered by department of residence.

F Robustness to deviations from parallel trends

In this section, we adopt the approach proposed by Rambachan and Roth (2023) to assess the robustness of our findings against alternative assumptions about different trends between treated and control departments. We assume the existence of a violation of parallel trends δ (i.e., a differential trend between treated and control groups) and assess whether a causal interpretation of our results is possible. We re-calculate the confidence intervals of our estimations by assuming that δ lies within a set of possible differences in trends Δ. In this section, we impose that the differential trends evolve smoothly over time by bounding the extent to which their slope may change across consecutive periods. Formally:

$$\Delta^{SD}(M) := \{\delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq M, \forall t\} \tag{F1}$$

where δ_t is the difference in trends between treated and control groups at time t.

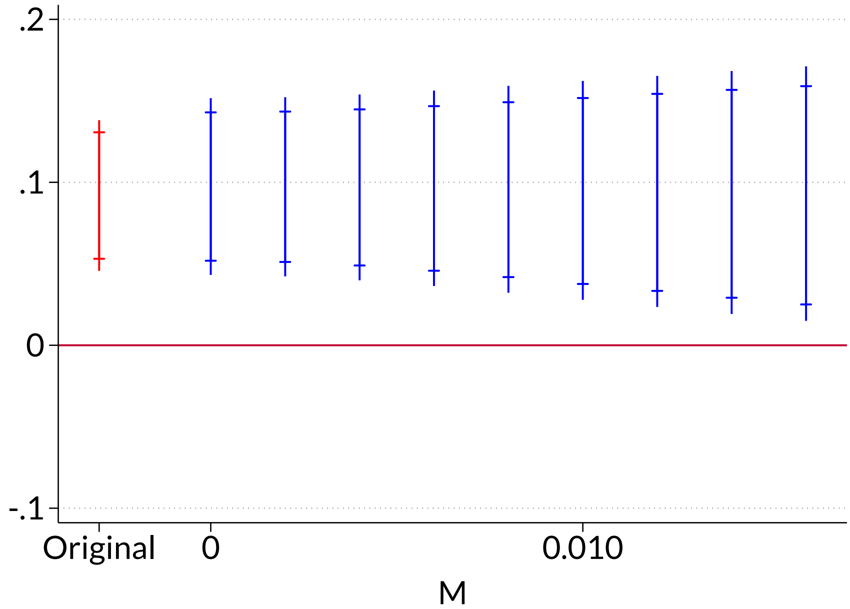
We estimate the following equation, which is a modification of our specifications:

$$Y_{icpj} = \mu_j + \mu_c + \sum_{c \leq 1987} \beta_c \left[d_c \times D_j\right] + \beta_{post} \left[post_c \times D_j\right] + \gamma_{cp} + Enroll80_{cj} + \varepsilon_{icpj} \tag{F2}$$

The coefficient β_{post} is the ITT presented in our main tables. The intuition is to use the information of the pre-treatment coefficients (β_c, with $c \leq 1987$) to restrict the possible differential trends in the post-treatment period and assess the robustness of the inference of β_{post}.

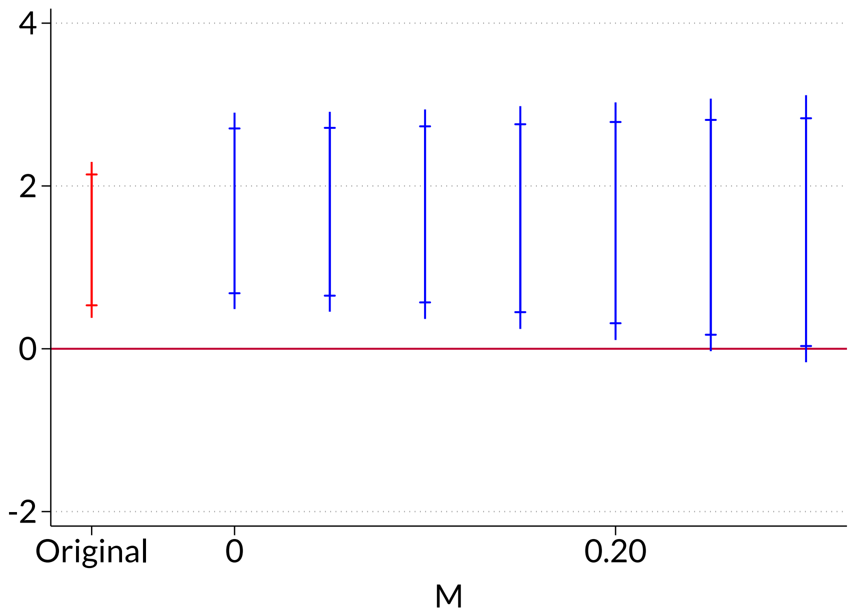
M in equation (F1) governs the maximum amount by which the slope of δ can change between consecutive periods. In the special case where $M = 0$, the assumption is that the difference in trends is exactly linear. We perform a sensitivity analysis with different values of M. Unfortunately, nothing in the data itself can place an upper bound on the parameter M (Rambachan & Roth, 2023). To select a value for M, we use point estimates and the variance-covariance matrix of the pre-treatment cohorts' coefficients. With simulations and assuming normally distributed error, we calculate the median of average (absolute) deviations from the trend in the pre-treatment period.

Figure F1: Years of basic school



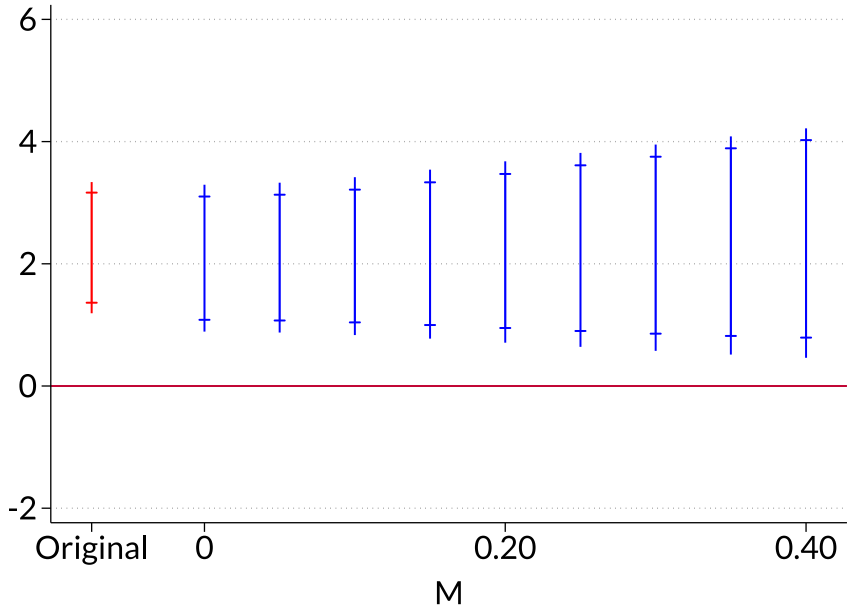
Note: We implement the approach of Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bound on the change in the slope.

Figure F2: Lower secondary completed



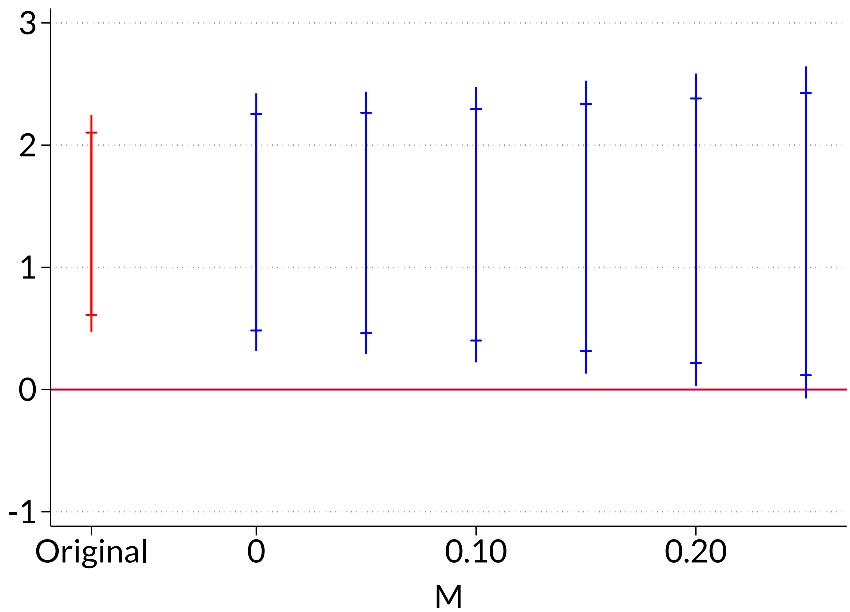
Note: We implement the approach of Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bound on the change in the slope.

Figure F3: Secondary completed



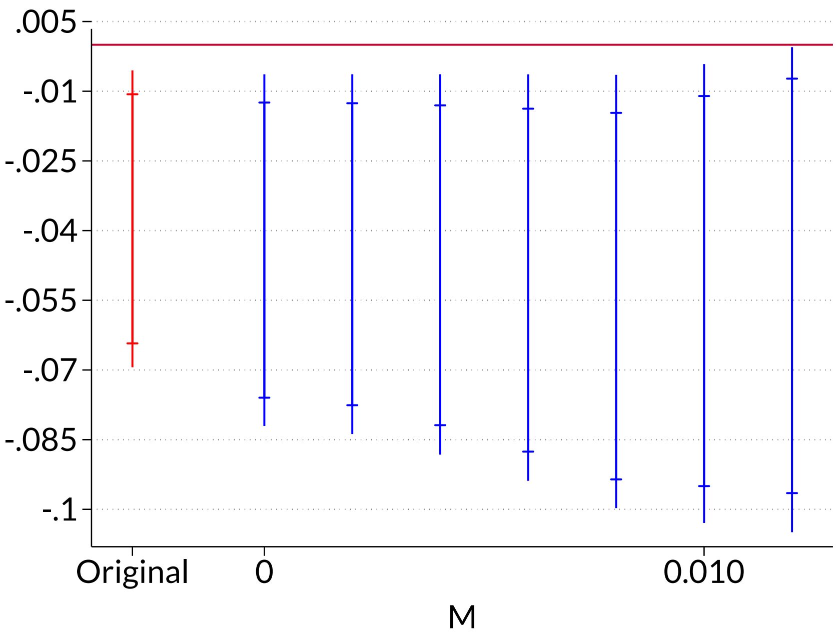
Note: We implement the approach of Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bound on the change in the slope

Figure F4: Post-secondary education



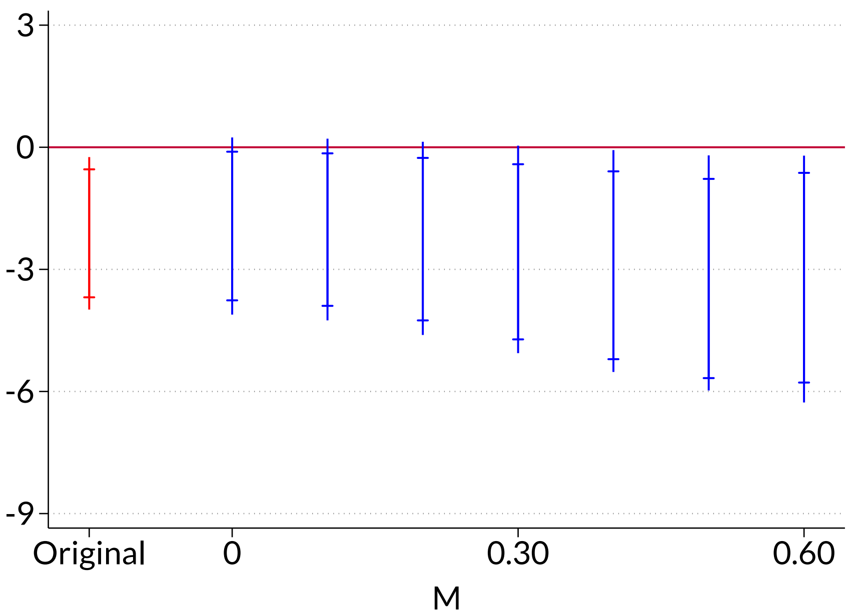
Note: We implement the approach of Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bound on the change in the slope.

Figure F5: Total live births



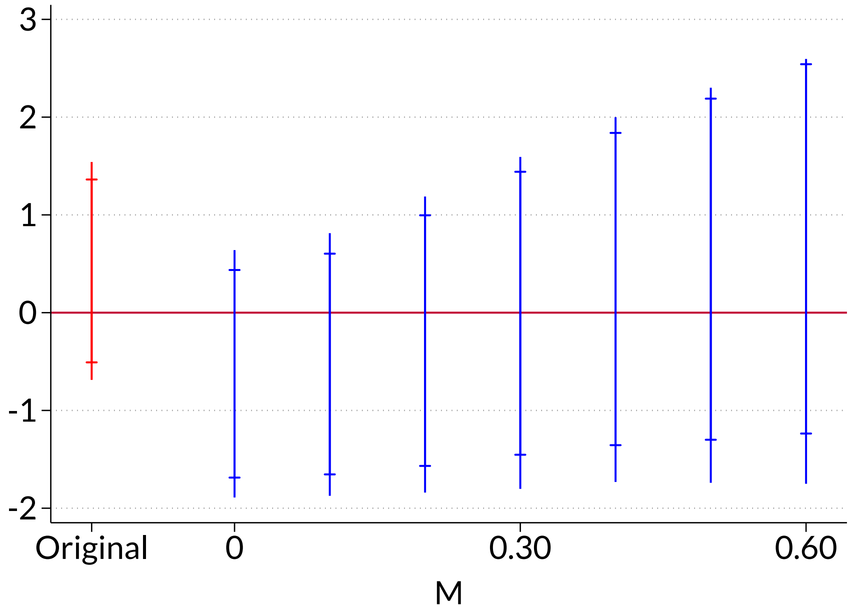
Note: We implement the approach of Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bound on the change in the slope.

Figure F6: Being a mother



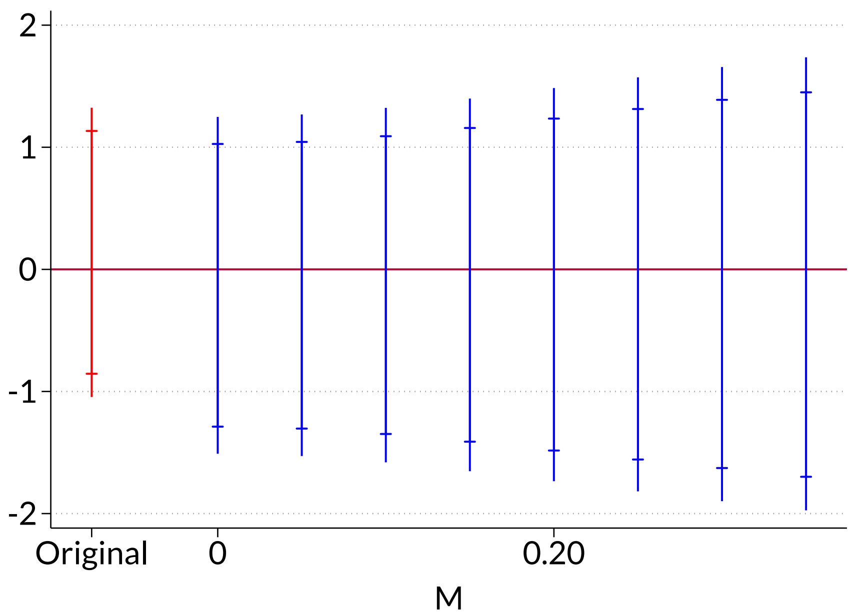
Note: We implement the approach of Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bound on the change in the slope.

Figure F7: Teenage mother



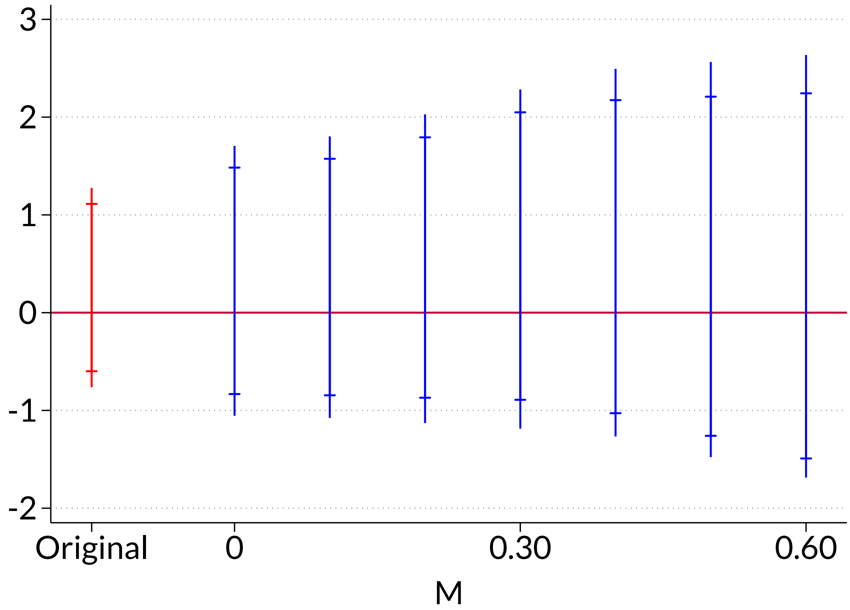
Note: We implement the approach of Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bound on the change in the slope.

Figure F8: Employment



Note: We implement the approach of Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bound on the change in the slope.

Figure F9: Formal employment



Note: We implement the approach of Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bound on the change in the slope.

G Addressing concerns about selective migration

A further concern for our identification strategy is selective migration. We assigned our treatment exposure measure based on the department of residence in 2010, since the Census does not collect information about where individuals lived at pre-primary age. However, these locations may not necessarily match, and this could be an issue if the migration patterns are correlated with the intensity of the policy. We address this potential issue with several robustness checks.

Parents could have moved to departments where pre-primary was expanding so they could benefit from the new pre-primary places. The characteristics of these parents could also be systematically correlated with their children's long-term outcomes.

First, we analyze the impact of the new pre-primary facilities on departmental cohort sizes. For each department and cohort in our analysis window (1985–1992), we compute the cohort size observed in the 1991, 2001, and 2010 censuses. We then use as outcomes the ratios of cohort size in 2010 to cohort size in 1991 and cohort size in 2001 to cohort size in 1991, reported in the first and second columns of Table G1, respectively. We do not find significant results. In the third column, we use the log cohort size in 2010 as the outcome, and again we do not find significant results.

Second, selective migration might have altered the composition of households or their characteristics even if cohort sizes remain unchanged. To address this concern, we identify households in the 1991 and 2001 censuses in which children from the relevant cohorts reside and assess whether changes in their parents' and households' characteristics are correlated with the new pre-primary facilities. These results are presented in Table G2. In the first column, we present the effect of the construction program on the ratio of the 2001 departmental average years of schooling of identified parents to the same average in 1991; parents are identified as the household head or the head's spouse when the child is listed as a child of the household head. We do not find significant effects on this ratio. We also investigate whether the policy is correlated with changes in the socioeconomic situation of households where children in our sample lived. In the second column of Table G2, we present the estimate of the effect of new pre-primary facilities on the ratio of the 2001 departmental proportion of households with some unsatisfied basic need (UBN) to the same proportion in 1991.[17]

[17] Census data allows us to determine five types of UBN: 1) Overcrowding, meaning that the house

In the third column, we present the results of the same estimation but with the total UBN per household as the dependent variable. We do not find any significant effects of the pre-primary construction program on these variables either.

Third, as a further check of the potential bias introduced in our results by selective migration, we re-estimate equation (1) using only individuals who resided in 2010 in the same province where they were born (the Census collects information on province of birth, not department). We present these additional results for educational attainment, fertility, and labor-market outcomes in Tables G3, G4, and G5, respectively. The education estimates are close to the baseline, while the fertility estimates are somewhat smaller (a reduction of 0.15 rather than 0.20 live births per woman) and significant at the 10 percent level. Since conditioning on non-migration can itself change the estimand if the program affected schooling and schooling in turn affected mobility, we read this exercise as a robustness check rather than as a preferred specification.

Finally, we test whether exposure to the construction program predicts individual migration directly. Tables G6 and G7, presented at the end of this appendix, report estimates of equation (1) using the 2001 and 2010 censuses, respectively. The first outcome equals 100 when the individual's province of residence differs from the province of birth. The second equals 100 when the individual lived in a different locality five years before the census but remained within the same province, while the third equals 100 when the individual lived in a different province five years earlier. The regressions use the department of residence observed in each census and include department and cohort fixed effects, cohort-by-province fixed effects, a female indicator, and cohort-specific controls for departmental pre-primary enrollment in 1980.

Across both censuses, none of the post-treatment coefficients is statistically significant at conventional levels. In 2001, the estimated post-treatment effects are close to zero for migration relative to province of birth and interprovincial migration, and modest but imprecise for intraprovincial migration. The 2010 estimates are also statistically insignificant for all three outcomes. The positive pre-treatment coefficient for interprovincial migration in 2010 indicates a pre-existing differential pattern for that outcome and warrants caution in interpreting the corresponding specification.

has more than three people per room; 2) Housing, meaning that people live in precarious or non-conventional housing; 3) Sanitary, meaning that the house does not have a toilet; 4) School attendance, meaning that some children of school age are not enrolled in school; 5) Subsistence, meaning that the household has four or more people per employed member.

Nonetheless, the absence of a post-treatment association in both censuses provides no evidence that the program systematically induced migration among eligible cohorts.

To sum up, there does not seem to be a significant correlation between the pre-primary construction program and different proxies of outcomes which we would have expected to change if selective migration interfered with our identification strategy.

Table G1: The impact of the construction program on department-cohort size ratios and log cohort size in 2010

	2010-1991	2001-1991	Log size in 2010
ITT	0.010	-0.003	0.025
	(0.015)	(0.015)	(0.017)
	[0.499]	[0.836]	[0.135]
Pre coef.	0.024	-0.008	0.022
	(0.018)	(0.013)	(0.016)
	[0.188]	[0.526]	[0.187]
N	4,176	4,176	4,176
N clusters	522	522	522
Controls and fixed effects			
Department FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes

Note: this table presents an estimation of equation (1). Cohorts are defined as census year minus declared age in the 1991, 2001, and 2010 censuses. For each department-cohort cell, we compute cohort size in each census. The first two columns use the ratios of cohort size in 2010 to cohort size in 1991 and cohort size in 2001 to cohort size in 1991, respectively; the third column uses the log cohort size in 2010. Since the 1992 cohort is not observed in the 1991 census, its 1991 size is projected using department-specific cohort growth between earlier cohorts. The ITT is β_{post} coefficients, while the pre is β_{pre} coefficient. We consider treated departments to be those that received an above-median total number of places per child. Standard errors (in parentheses) are clustered at the department-of-residence level. p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table G2: The impact of the construction program on changes in identified parents' education and household UBN between 2001 and 1991

	Head's education	UBN household	
	Years school Post-K	Some UBN	UBN
ITT	-0.004	0.002	-0.026
	(0.013)	(0.031)	(0.037)
	[0.734]	[0.948]	[0.476]
Pre coef.	0.016	-0.007	-0.017
	(0.013)	(0.033)	(0.038)
	[0.224]	[0.824]	[0.647]
N	3,652	3,645	3,645
N clusters	522	522	522
Controls and fixed effects			
Department FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes

Note: this table presents an estimation of equation (1). Cohorts are defined as census year minus declared age in the 1991 and 2001 censuses. The first column uses the average years of schooling of identified parents, defined as the household head or the head's spouse when the child is listed as a child of the household head. The second and third columns use household-level UBN measures. The ITT is β_{post} coefficients, while the pre is β_{pre} coefficient. We consider treated departments to be those that received an above-median total number of places per child. Standard errors (in parentheses) are clustered at the department-of-residence level. p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table G3: Educational attainment, individuals who reside in their province of birth

	Years basic school	Lower secondary (pp.)	Secondary (pp.)	Post secondary (pp.)
ATT	0.405***	5.294**	11.286***	7.597***
ITT	0.080***	1.043**	2.223***	1.496***
	(0.023)	(0.461)	(0.552)	(0.455)
	[0.001]	[0.024]	[0.000]	[0.001]
Pre coef.	0.000	0.099	0.159	-0.076
	(0.024)	(0.526)	(0.546)	(0.487)
	[0.994]	[0.851]	[0.771]	[0.877]
Mean pre-treat	9.448	54.977	42.219	19.762
N	1,752,918	1,753,688	1,753,688	1,753,688
N clusters	522	522	522	522
Controls and FE				
Department FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes	Yes

Note: The table presents the results of equation (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups. The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. Pre coef. is the estimate of β_{pre}. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. We exclude people who migrated from their province of birth. Standard errors (in parentheses) are clustered at the department-of-residence level. p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table G4: Fertility, individuals who reside in their province of birth

	Being a mother (pp.)	Total live births	Teen mother (pp.)
ATT	-8.638*	-0.148*	2.679
ITT	-1.701*	-0.029*	0.528
	(0.893)	(0.015)	(0.696)
	[0.057]	[0.053]	[0.449]
Pre coef.	-0.727	-0.006	0.705
	(0.773)	(0.013)	(0.654)
	[0.347]	[0.667]	[0.281]
Mean pre-treat	64.052	1.123	17.374
N	877,233	877,233	877,233
N clusters	522	522	522
Controls and FE			
Department FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes

Note: The table presents the results of equation (1) for different outcomes. We define "Being a mother" as a dummy variable indicating that the woman has had at least one live-born child. We define "Teenage mother" as a dummy variable indicating that the woman had at least one live-born child when she was 19 years old or younger. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups. The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. Pre coef. is the estimate of β_{pre}. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, a female indicator when the sample includes both genders, and differential trends by departmental pre-primary gross enrollment in 1980. We exclude people who migrated from their province of birth. Standard errors (in parentheses) are clustered at the department-of-residence level. p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table G5: Labor market, individuals who reside in their province of birth

	Employment (pp.)	Formal employment (pp.)
ATT	1.812	4.728*
ITT	0.357	0.931*
	(0.670)	(0.543)
	[0.595]	[0.087]
Pre coef.	-0.709	0.386
	(0.575)	(0.640)
	[0.218]	[0.546]
Mean pre-treat	60.088	27.552
N	1,752,918	1,752,918
N clusters	522	522
Controls and FE		
Department FE	Yes	Yes
Cohort FE	Yes	Yes
Coh. × Province	Yes	Yes
Coh. × Enr. 80	Yes	Yes

Note: The table presents the results of equation (1) for labor-market outcomes, restricting the sample to individuals who resided in 2010 in the same province in which they were born. We define "Employment" as a dummy variable equal to 100 if the individual was employed at the time of the Census, and 0 otherwise. We define "Formal employment" as a dummy variable equal to 100 if the individual was employed and contributed to social security at the time of the Census, and 0 otherwise. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups. The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. Pre coef. is the estimate of β_{pre}. Treated departments are those that received an above-median total number of places per child. All regressions include department fixed effects, cohort fixed effects, cohort-by-province fixed effects, a female indicator when the sample includes both genders, and cohort-by-1980 pre-primary gross enrollment trends. Standard errors clustered at the department of residence level are reported in parentheses. p-values are reported in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table G6: Program effects on migration outcomes, 2001 Census

	Province of residence differs from province of birth	Different locality than five years ago, same province	Different province than five years ago
Post	0.039	0.129	-0.004
	(0.197)	(0.091)	(0.075)
	[0.843]	[0.156]	[0.955]
Pre coef.	0.097	-0.088	0.003
	(0.142)	(0.098)	(0.079)
	[0.494]	[0.367]	[0.969]
Mean pre-treat	8.818	4.201	2.607
N	5,214,617	5,214,617	5,214,617
N clusters	522	522	522
R2	0.064	0.021	0.010
Controls and FE			
Department FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Female	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes

Note: The table presents estimates of equation (1) using the 2001 Census. The first outcome equals 100 if the province of residence differs from the province of birth. The second equals 100 if the individual lived in a different locality five years earlier but within the same province. The third equals 100 if the individual lived in a different province five years earlier. Post is β_{post} and Pre coef. is β_{pre}. All regressions include department and cohort fixed effects, cohort-by-province fixed effects, a female indicator, and cohort-specific controls for departmental pre-primary enrollment in 1980. Standard errors clustered by department of residence are reported in parentheses and p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table G7: Program effects on migration outcomes, 2010 Census

	Province of residence differs from province of birth	Different locality than five years ago, same province	Different province than five years ago
Post	0.516	-0.305	-0.414
	(0.318)	(0.187)	(0.255)
	[0.105]	[0.103]	[0.105]
Pre coef.	0.452	0.267	0.925**
	(0.313)	(0.371)	(0.381)
	[0.149]	[0.473]	[0.015]
Mean pre-treat	11.053	5.385	4.655
N	1,999,633	1,999,633	1,999,633
N clusters	522	522	522
R2	0.082	0.016	0.052
Controls and FE			
Department FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Female	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes

Note: The table presents estimates of equation (1) using the 2010 Census. The first outcome equals 100 if the province of residence differs from the province of birth. The second equals 100 if the individual lived in a different locality five years earlier but within the same province. The third equals 100 if the individual lived in a different province five years earlier. Post is β_{post} and Pre coef. is β_{pre}. All regressions include department and cohort fixed effects, cohort-by-province fixed effects, a female indicator, and cohort-specific controls for departmental pre-primary enrollment in 1980. Standard errors clustered by department of residence are reported in parentheses and p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

H Replication of Berlinski *et al.* (2009)

The program analyzed in this paper improved treated children's skills in primary school, as shown by Berlinski *et al.* (2009). These skill gains provide a plausible mechanism for our long-run results and are consistent with the broader literature on early education and skill formation (Cunha & Heckman, 2007). However, because our main specification uses a different treatment measure from the one used in Berlinski *et al.* (2009), the comparison is not mechanical. In this section, we re-estimate their test-score specification using our treatment measure.

Berlinski *et al.* (2009) estimate the impact of new pre-primary facilities on mathematics and Spanish test scores in primary school. They use third-grade standardized achievement tests administered by the Argentine National Ministry of Education. The sample consists of students in untreated cohorts 1986, 1987, and 1988 and treated cohorts 1989, 1990, and 1991, observed when they were in third grade.

We re-estimate the impact of new pre-primary places on test scores with the following equation:

$$Y_{icpjt} = \mu_j + \mu_c + \gamma_{cp} + \lambda_{tp} + \beta_{post}\left[I(\text{cohort} \geq 1989) \times D_j\right] + \beta_{pre}\left[I(\text{cohort} \leq 1987) \times D_j\right] + \varepsilon_{icpjt} \tag{H1}$$

where Y_{icpjt} is the test score of student i in cohort c residing in province p and department j and taking the exam in year t. μ_j are departmental fixed effects, which control for time-constant departmental characteristics. μ_c are fixed effects by cohort, which control constant characteristics of the cohort across departments. γ_{cp} is a set of interactions between cohort and province of residence, which control unobservable differences between cohorts by province. λ_{tp} is a set of interactions between province and year of exam, which control for time-varying effects at the province level. D_j is the dummy variable indicating that department j received a high intensity of treatment. $I(\text{cohort} \geq 1989)$ indicates that the individual belongs to the 1989 cohort or younger, and $I(\text{cohort} \leq 1987)$ is analogous for 1987 or older. Finally, ε_{icpjt} is an error term.

Table H1 reports the estimates. Column (1) shows an average effect of 2.06 points on mathematics test scores. Column (2) adds cohort interactions with pre-treatment pre-primary enrollment in 1991 and yields similar results. Column (3) shows an average effect of 1.21 points on Spanish test scores, and column (4) shows that this estimate is robust to adding the same pre-treatment enrollment controls.

Table H1: The impact of the construction program on standardized third-grade test scores

	Mathematics test score		Spanish test score	
	(1)	(2)	(1)	(2)
Post	2.060***	2.101***	1.212*	1.233*
	(0.756)	(0.748)	(0.707)	(0.699)
Pre	0.999	1.017	0.030	0.066
	(0.870)	(0.860)	(1.009)	(1.004)
R^2	0.16	0.16	0.16	0.16
N	126,106	126,106	117,515	117,515
Controls and Fixed effects				
Cohort FE	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes
Cohort × province effects	Yes	Yes	Yes	Yes
Year × province effects	Yes	Yes	Yes	Yes
Cohort × Enroll. 91	No	Yes	No	Yes

Note: this table presents a replication of Table 5 results of Berlinski *et al.* (2009), using equation (H1). Post is the β_{post} coefficients, while Pre is the β_{pre} coefficient. We consider treated departments to be those that received an above-median total number of places per child. Standard errors clustered at department of residence and treatment/controls status in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

I Alternative treatment measure based on Berlinski *et al.* (2009)

In this section, we present the results of replicating our estimates using the approach of the 2009 paper (Berlinski *et al.*, 2009). This consists of a difference-in-differences approach, where the treatment variable measures the number of pre-primary places available to each cohort. The coefficient obtained is interpreted as the ATT of an additional year of pre-primary education.

The Berlinski *et al.* (2009) design assigns each department-cohort the number of pre-primary places available when that cohort was preschool-aged. The calculation of this measure of treatment exposure is given by the following equation:

$$\text{Stock}_{cj} = \frac{0.5 \times \text{Stock}_{5,cj} + 0.36 \times \text{Stock}_{4,cj} + 0.14 \times \text{Stock}_{3,cj}}{\text{Cohort Size}_{cj}} \tag{I1}$$

where $\text{Stock}_{h,cj}$ is the cumulative flow of pre-primary places available in department j at the moment cohort c was h years old ($h = 3, 4, 5$). This measure is normalized by cohort size.

Then, the following difference-in-differences regression is estimated:

$$Y_{icjp} = \mu_j + \mu_c + \beta\,\text{Stock}_{cj} + \varepsilon_{icjp} \tag{I2}$$

Because Stock_{cj} is constructed in this way, the estimate of β corresponds to the ATT of a pre-primary place-year available to treated children. Since each slot is used by one child per year when it is available, the estimate can be interpreted as the ATT of one additional year of pre-primary education.

The ATT generated by this exercise cannot be directly compared with our main results. This is because our ATT is the ATT of an additional pre-primary place available at some point during the treatment period. In contrast, the ATT calculated using the 2009 paper's approach is the ATT of an additional place-year, interpreted as the ATT of one additional year of pre-primary education.

However, we believe it is possible to calculate an ATT from the results of our paper that is comparable to the 2009 methodology. For this, we change the re-scaling coefficient: instead of using the difference in constructed places between High and Low departments, we calculate the difference in available places-years between High and Low departments.

Tables I1, I2, and I3 report our main estimates using the Berlinski *et al.* (2009) treatment measure, together with comparable estimates from our baseline design. We do not find statistically significant differences between the two sets of estimates.

Table I1: Education outcomes: ATT per place-year and ATT based on Berlinski *et al.* (2009)

	Years basic school	Lower secondary (pp.)	Secondary (pp.)	Post secondary (pp.)
ATT per place-year	0.243***	3.539***	5.989***	3.591***
	(0.062)	(1.293)	(1.449)	(1.199)
	[0.000]	[0.006]	[0.000]	[0.003]
ATT Berlinski *et al.* (2009)	0.237***	3.121***	3.693***	2.726***
	(0.051)	(0.858)	(1.116)	(1.007)
	[0.000]	[0.000]	[0.001]	[0.007]
p-val difference	0.921	0.738	0.109	0.440
Controls and FE				
Department FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Coh. $\times$ Province	Yes	Yes	Yes	Yes
Coh. $\times$ Enr. 80	Yes	Yes	Yes	Yes

Note: This table compares the estimated average treatment effects on the treated (ATT) for various educational outcomes using two alternative treatment measures: the ATT per place-year (our specification) and the ATT following the approach of Berlinski *et al.* (2009). Each column presents results for a different outcome: years of basic schooling, completion of lower secondary education, completion of secondary education, and attainment of any post-secondary education. Standard errors, clustered at the department level, are shown in parentheses. p-values for the test of equality between estimates are shown in the third row. All regressions include department fixed effects, cohort fixed effects, cohort-by-province fixed effects, a female indicator when the sample includes both genders, and cohort-by-pre-primary gross enrollment in 1980 trends. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table I2: Labor market outcomes: ATT per place-year and ATT based on Berlinski *et al.* (2009)

	Employment (pp.)	Formal employment (pp.)
ATT per place-year	0.368	0.680
	(1.599)	(1.376)
	[0.818]	[0.621]
ATT Berlinski *et al.* (2009)	0.921	0.450
	(0.868)	(0.851)
	[0.289]	[0.597]
p-val difference	0.703	0.864
Controls and FE		
Department FE	Yes	Yes
Cohort FE	Yes	Yes
Coh. $\times$ Province	Yes	Yes
Coh. $\times$ Enr. 80	Yes	Yes

Note: This table compares the estimated average treatment effects on the treated (ATT) for two labor market outcomes: employment and formal employment. The estimates are presented using two alternative treatment definitions: ATT per place-year (our specification) and ATT based on the methodology of Berlinski *et al.* (2009). Standard errors clustered at the department level are reported in parentheses, and p-values are shown in brackets. The third row reports p-values for the test of equality between the two ATT estimates. All regressions include department fixed effects, cohort fixed effects, cohort-by-province fixed effects, a female indicator when the sample includes both genders, and cohort-by-pre-primary gross enrollment in 1980 trends.$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table I3: Fertility outcomes: ATT per place-year and ATT based on Berlinski *et al.* (2009)

	Being a mother (pp.)	Total live births	Teen mother (pp.)
ATT per place-year	-5.596**	-0.099**	1.126
	(2.528)	(0.043)	(1.504)
	[0.027]	[0.022]	[0.454]
ATT Berlinski *et al.* (2009)	-2.801**	-0.079***	-1.376
	(1.270)	(0.026)	(0.838)
	[0.027]	[0.002]	[0.101]
p-val difference	0.238	0.605	0.093
Controls and FE			
Department FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes

Note: This table compares the average treatment effects on the treated (ATT) for fertility outcomes using two alternative treatment measures: ATT per place-year (our specification) and ATT following the methodology of Berlinski *et al.* (2009). The outcomes include the probability of being a mother, the number of total live births, and the probability of being a teen mother. Standard errors clustered at the department level are reported in parentheses, and p-values are in brackets. The third row reports the p-values from a test of equality between the two sets of ATT estimates. All regressions include department fixed effects, cohort fixed effects, cohort-by-province fixed effects, a female indicator when the sample includes both genders, and cohort-by-pre-primary gross enrollment in 1980 trends. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

J Estimates around the cut-off

In this section, we present alternative estimations to assess the robustness of our main results. These estimations focus on units that were located "very near" the cut-off, allowing us to estimate the effect of the pre-primary construction program more precisely.

As defined in the Identification Strategy section, in both equations (1) and (2), we exploit the fact that some school cohorts had access to the new pre-primary facilities while others did not. The first post-treatment cohort is 1989, referring to individuals who were eligible to attend the 5-year-old pre-primary level in 1994, when the first facilities became available. According to enrollment criteria in Argentina, the 1989 cohort includes children born between July 1, 1988, and June 30, 1989.

Therefore, we consider July 1, 1988, as the birthdate cut-off that determines exposure to the construction program. We estimate the effect of the program within a neighborhood around this birthdate. Specifically, we estimate the following equation:

$$Y_{ibpj} = \mu_j + \mu_b + \beta \left[Post_b \times D_j \right] + \gamma_{bp} + Enroll80_{bj} + \varepsilon_{ibpj} \tag{J1}$$

where Y_{ibpj} is an outcome of interest for individual i, born on date b, resident in province p and department j. μ_j are departmental fixed effects. μ_b are fixed effects by date of birth. γ_{bp} is a set of interactions between date of birth and province of residence. $Enroll80_{bj}$ represents a set of interactions between date of birth and departmental pre-primary gross enrollment in 1980. D_j is the dummy variable indicating that department j received a high intensity of treatment. $Post_b$ is a dummy that indicates that the individual was born on July 1, 1988, or after. Finally, ε_{ibpj} is an error term. We cluster standard errors at the level of department of residence.

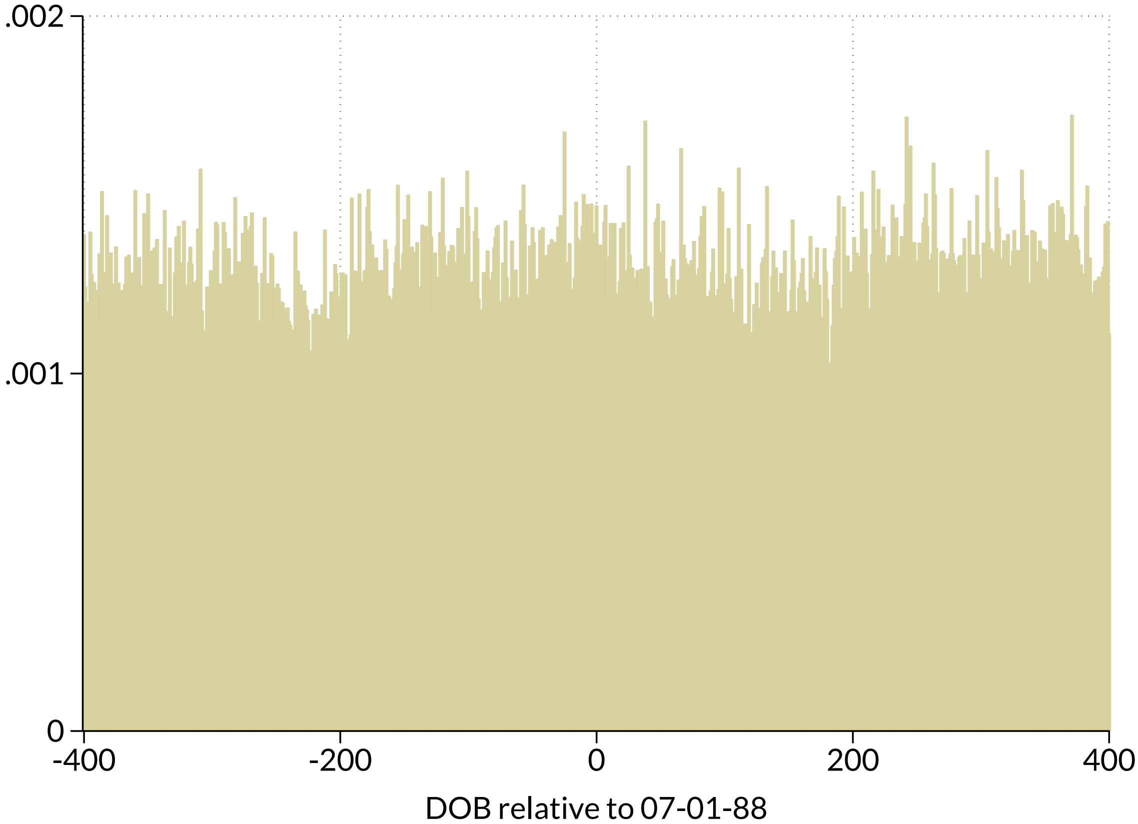
We estimate equation (J1) using three different bandwidths (before and after the birthdate cut-off): 75 days, 100 days, and 125 days. The idea behind this strategy is similar to that of Grembi *et al.* (2016), who use a "difference-in-discontinuities" approach. In our case, we exploit the discontinuity created by the enrollment criteria described above. However, our method differs in that we include the running variable (date of birth) as dummies rather than as a continuous variable.

We believe this specification is appropriate, as the date of birth shows seasonality, which is evident in Figure J1. In particular, the distribution of births around the cut-off exhibits weekly cycles, which may reflect underlying socioeconomic patterns—for example, births on certain days of the week might correlate with income level, health system usage, or parental background. These non-linear and non-monotonic patterns in the running variable raise concerns about potential bias if not properly accounted for. Therefore, including date-of-birth fixed effects allows us to flexibly control for such weekly seasonality and other correlated unobserved characteristics, and thus more accurately isolate the causal effect of interest.

We present results for education, labor market, and fertility outcomes in Tables J1, J2, and J3, respectively. We find positive and statistically significant effects on years of education (post-kindergarten), the probability of completing lower secondary education, and the probability of completing upper secondary education. The estimated effects remain relatively stable across different bandwidths. The reduction in total live births is significant across the three bandwidths, while the coefficients for being a mother are negative and imprecise. The teenage-motherhood coefficients are positive and marginally significant at two bandwidths, consistent in sign with the statistically insignificant positive estimate in Table 2, and the labor-market coefficients are small and indistinguishable from zero. We therefore read this exercise as supportive for the education results and for total live births rather than as a full replication across all outcomes.

To further test the robustness of our results, we repeat the estimation using a placebo cut-off date: July 1, 1986. The results are shown in Tables J4, J5, and J6. In this case, we find only one significant effect—on the probability of formal employment.

Figure J1: Frequency of Date of Birth (DOB)



Note: The figure is a histogram that presents the frequency of each date of birth (DOB) relative to the cut-off (1st July 1988).

Table J1: Estimates around the cut-off: Education outcomes

	Years basic school	Lower secondary (pp.)	Secondary (pp.)	Post secondary (pp.)
h=75	0.143**	2.121*	2.493**	1.449
	(0.061)	(1.094)	(1.218)	(1.214)
	[0.019]	[0.053]	[0.041]	[0.233]
h=100	0.150***	2.327**	3.567***	1.036
	(0.052)	(0.999)	(1.150)	(0.945)
	[0.004]	[0.020]	[0.002]	[0.273]
h=125	0.117***	1.645*	2.933***	0.765
	(0.045)	(0.923)	(0.997)	(0.808)
	[0.010]	[0.075]	[0.003]	[0.343]
Controls and FE				
Department FE	Yes	Yes	Yes	Yes
DOB FE	Yes	Yes	Yes	Yes
DOB $\times$ Province	Yes	Yes	Yes	Yes
DOB $\times$ Enr. 80	Yes	Yes	Yes	Yes

Note: This table reports Intention-to-Treat (ITT) estimates for educational outcomes using equation (J1) from Section J. The sample is restricted to individuals born within 75, 100, and 125 days around the pre-primary eligibility cut-off (July 1, 1988). Outcomes include years of basic schooling, completion of lower secondary education, completion of secondary education, and attainment of any post-secondary education. All regressions include department fixed effects, date of birth (DOB) fixed effects, DOB-by-province fixed effects, and DOB-by-pre-primary gross enrollment in 1980 trends. Standard errors clustered at the department level are shown in parentheses, and p-values are shown in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table J2: Estimates around the cut-off: Labor market outcomes

	Employment (pp.)	Formal employment (pp.)
h=75	0.393	0.545
	(1.055)	(1.211)
	[0.709]	[0.653]
h=100	0.102	-0.480
	(0.930)	(1.130)
	[0.912]	[0.671]
h=125	0.138	-0.286
	(0.880)	(1.065)
	[0.875]	[0.788]
Controls and FE		
Department FE	Yes	Yes
DOB FE	Yes	Yes
DOB × Province	Yes	Yes
DOB × Enr. 80	Yes	Yes

Note: This table reports Intention-to-Treat (ITT) estimates for labor market outcomes using equation (J1) from Section J. The sample is restricted to individuals born within 75, 100, and 125 days around the pre-primary eligibility cut-off (July 1, 1988). Outcomes include the probability of employment and the probability of formal employment. All regressions include department fixed effects, date of birth (DOB) fixed effects, DOB-by-province fixed effects, and DOB-by-pre-primary gross enrollment in 1980 trends. Standard errors clustered at the department level are shown in parentheses, and p-values are shown in brackets. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table J3: Estimates around the cut-off: Fertility outcomes

	Being a mother (pp.)	Total live births	Teen mother (pp.)
h=75	-1.285	-0.055*	2.595*
	(1.858)	(0.032)	(1.548)
	[0.489]	[0.091]	[0.094]
h=100	-0.685	-0.047*	1.872
	(1.551)	(0.028)	(1.446)
	[0.658]	[0.096]	[0.195]
h=125	-1.101	-0.055**	2.009*
	(1.262)	(0.023)	(1.131)
	[0.383]	[0.016]	[0.076]
Controls and FE			
Department FE	Yes	Yes	Yes
DOB FE	Yes	Yes	Yes
DOB × Province	Yes	Yes	Yes
DOB × Enr. 80	Yes	Yes	Yes

Note: This table reports Intention-to-Treat (ITT) estimates for fertility outcomes using equation (J1) from Section J. The sample is restricted to individuals born within 75, 100, and 125 days around the pre-primary eligibility cut-off (July 1, 1988). Outcomes include the probability of being a mother, the number of total live births, and the probability of being a teen mother. All regressions include department fixed effects, date of birth (DOB) fixed effects, DOB-by-province fixed effects, and DOB-by-pre-primary gross enrollment in 1980 trends. Standard errors clustered at the department level are shown in parentheses, and p-values are shown in brackets. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table J4: Estimates around the cut-off: Education outcomes. Placebo

	Years basic school	Lower secondary (pp.)	Secondary (pp.)	Post secondary (pp.)
h=75	0.021	1.443	1.317	0.395
	(0.074)	(1.529)	(1.639)	(1.891)
	[0.780]	[0.345]	[0.422]	[0.834]
h=100	0.041	1.382	1.734	0.198
	(0.068)	(1.336)	(1.572)	(1.741)
	[0.547]	[0.301]	[0.270]	[0.909]
h=125	0.005	0.501	0.943	-0.195
	(0.063)	(1.221)	(1.358)	(1.490)
	[0.938]	[0.682]	[0.487]	[0.896]
Controls and FE				
Department FE	Yes	Yes	Yes	Yes
DOB FE	Yes	Yes	Yes	Yes
DOB × Province	Yes	Yes	Yes	Yes
DOB × Enr. 80	Yes	Yes	Yes	Yes

Note: This table reports placebo Intention-to-Treat (ITT) estimates for educational outcomes using equation (J1) from Section J, replacing the eligibility cut-off with July 1, 1986. The sample is restricted to individuals born within 75, 100, and 125 days around the placebo cut-off. Outcomes include years of basic schooling, completion of lower secondary education, completion of secondary education, and attainment of any post-secondary education. All regressions include department fixed effects, date of birth (DOB) fixed effects, DOB-by-province fixed effects, and DOB-by-pre-primary gross enrollment in 1980 trends. Standard errors clustered at the department level are shown in parentheses, and p-values are shown in brackets. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table J5: Estimates around the cut-off: Labor market outcomes. Placebo

	Employment (pp.)	Formal employment (pp.)
h=75	0.740	2.171*
	(1.149)	(1.305)
	[0.520]	[0.096]
h=100	0.425	-0.146
	(1.014)	(1.209)
	[0.675]	[0.904]
h=125	0.006	-0.307
	(0.935)	(1.141)
	[0.995]	[0.788]
Controls and FE		
Department FE	Yes	Yes
DOB FE	Yes	Yes
DOB × Province	Yes	Yes
DOB × Enr. 80	Yes	Yes

Note: This table reports placebo Intention-to-Treat (ITT) estimates for labor-market outcomes using equation (J1) from Section J, replacing the eligibility cut-off with July 1, 1986. The sample is restricted to individuals born within 75, 100, and 125 days around the placebo cut-off. Outcomes include the probability of employment and the probability of formal employment. All regressions include department fixed effects, date of birth (DOB) fixed effects, DOB-by-province fixed effects, and DOB-by-pre-primary gross enrollment in 1980 trends. Standard errors clustered at the department level are shown in parentheses, and p-values are shown in brackets. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table J6: Estimates around the cut-off: Fertility outcomes. Placebo

	Being a mother (pp.)	Total live births	Teen mother (pp.)
h=75	0.714	-0.011	-1.051
	(1.704)	(0.031)	(1.134)
	[0.675]	[0.731]	[0.354]
h=100	1.511	0.002	0.282
	(1.478)	(0.029)	(0.910)
	[0.307]	[0.934]	[0.756]
h=125	1.391	0.005	0.979
	(1.377)	(0.027)	(0.853)
	[0.312]	[0.845]	[0.251]
Controls and FE			
Department FE	Yes	Yes	Yes
DOB FE	Yes	Yes	Yes
DOB × Province	Yes	Yes	Yes
DOB × Enr. 80	Yes	Yes	Yes

Note: This table reports placebo Intention-to-Treat (ITT) estimates for fertility outcomes using equation (J1) from Section J, replacing the eligibility cut-off with July 1, 1986. The sample is restricted to individuals born within 75, 100, and 125 days around the placebo cut-off. Outcomes include the probability of being a mother, the number of total live births, and the probability of being a teen mother. All regressions include department fixed effects, date of birth (DOB) fixed effects, DOB-by-province fixed effects, and DOB-by-pre-primary gross enrollment in 1980 trends. Standard errors clustered at the department level are shown in parentheses, and p-values are shown in brackets. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

K Additional results on fertility based on administrative birth records

We have access to administrative records of live births from the Argentine Ministry of Health. In particular, we have data on the total live births per department, calendar year and year of birth cohort of the mother, for years 2004 to 2022. We constructed panels of departments and year of birth cohorts adding the live births in the following manner:

$$\text{Tot births pw}_{cjT} = \frac{\sum_{t=2004}^{T} \text{Births}_{cjt}}{\text{Women}_{2010cj}} \tag{K1}$$

where $births_{cjt}$ is the live births of women of birth cohort c in department j in calendar year t, and Women_{2010cj} is the total number of women in cohort c and department j in 2010. In other words, we calculated for each department-cohort the total live births per woman from 2004 through T. We make this calculation for $T = 2010, 2014, 2018, 2022$.

With this data, we estimate the following model that is an aggregated version of (1):

$$\text{Tot births pw}_{cjT} = \mu_j + \mu_c + \gamma_{cp} + Enroll80_{cj} + \beta_{post}\left[I(c \geq 1989) \times D_j\right] + \beta_{pre}\left[I(c \leq 1987) \times D_j\right] + \varepsilon_{cpj} \tag{K2}$$

where μ_j are departmental fixed effects, which control for time-constant departmental characteristics. μ_c are fixed effects by cohort, which control constant characteristics of the cohort across departments. γ_{cp} is a set of interactions between cohort and province of residence, which control unobservable differences between cohorts by province. $Enroll80_{cj}$ represents interactions between cohort and departmental pre-primary gross enrollment in 1980. D_j is the dummy variable indicating that department j received a high intensity of treatment. $I(c \geq 1989)$ indicates that the cohort is 1989 or younger, and $I(c \leq 1987)$ is analogous for 1987 or older. Finally, ε_{cpj} is an error term. We cluster standard errors at the level of departments. We weight our estimates by the total number of women.

We present the results from equation (K2) in Table K1. The results are similar to those in Table 2.

The approach of this section is an imperfect proxy of our main results. In the benchmark estimations we have data on the total live births for each woman, while in this Appendix we use aggregated data from a department-cohort panel. Furthermore, assuming that women do not move among departments, administrative records allow us to calculate live births only for the window 2004–2022.

Table K1: Fertility with administrative records

	Total live births 2004-10	Total live births 2004-14	Total live births 2004-18	Total live births 2004-22
ATT	-0.176***	-0.195***	-0.207***	-0.211***
ITT	-0.034***	-0.037***	-0.039***	-0.040***
	(0.011)	(0.013)	(0.013)	(0.014)
	[0.002]	[0.004]	[0.003]	[0.005]
Pre coef.	-0.001	-0.001	-0.007	-0.007
	(0.009)	(0.012)	(0.015)	(0.017)
	[0.914]	[0.927]	[0.654]	[0.693]
Mean pre-treat	0.824	1.276	1.648	1.912
N	4,176	4,176	4,176	4,176
N clusters	522	522	522	522
Controls and FE				
Department FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Coh. × Province	Yes	Yes	Yes	Yes
Coh. × Enr. 80	Yes	Yes	Yes	Yes

Note: The table presents results from estimating (K2) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the difference in treatment intensity between the treated and control groups. The re-scaling factor is approximately 0.19, corresponding to the difference in new pre-primary places per child between high- and low-treatment departments. Pre coef. is the estimate of β_{pre}. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort fixed effects, and differential trends by departmental pre-primary gross enrollment in 1980. Standard errors clustered at department in parentheses. p-values are in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.