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Evidence from Argentina

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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 enrolling in post-secondary education by 7.1 percentage points. For women, access to the program also reduces completed fertility: an additional seat lowers the number of live births per woman by 0.18. We find no evidence that selective migration biases these 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.

JEL codes: J13, J16, J38, O15.

Keywords: early childhood education; human capital; long-term effects; developing country; preschool.

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1 Introduction

Expanding pre-primary education is considered a good investment in a country's future by improving children's cognitive and social skills, increasing long-term educational outcomes, and creating 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, contributing to an increase in pre-primary enrollment of 7.5 percentage points (Berlinski & Galiani, 2007). 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 and on fertility. 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 place of pre-primary education led to an increase of 0.5 years of (post pre-primary) education resulting in an 11.9 percentage point gain in the probability of finishing secondary school and a 7.1 percentage point

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

gain in the probability of being enrolled in post-secondary education. Furthermore, an additional place led to a decline of 0.18 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 place and estimate a benefit-cost ratio (BCR) of approximately 10.98 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.

The results of this paper 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 completed 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 completed 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. 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.

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. Perry Preschool and the Abecedarian Project are canonical small-scale programs (Campbell *et al.*, 2002; Heckman *et al.*, 2013). Our study is related to the long-run evidence on the effects of Head Start provided by Garces *et al.* (2002).

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 enrollment, reduced fertility among beneficiary women at census ages.

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 a substantial number of beneficiaries of the construction program had reached 18 years of age. 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 C 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 of education services was insufficient to meet the increased demand. 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. Between 1991 and 2001, the program generated an increase in enrollment of at least 10 percentage points in all provinces (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]

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

[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.

[3] Additional results available upon request.

2001 (post-treatment). The distribution shows a substantial shift to the right, indicating an increase in pre-primary enrollment. The average increase in departmental pre-primary enrollment was about 15 percentage points (pp.).

2.3 Identification strategy

Our identification strategy exploits the plausibly exogenous rollout of the policy across departments. 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.

Our main treatment measure is a dummy variable indicating that the total number of student places per 3- to 5-year-old children of 1991 in the department exceeds the median of the distribution by department (approximately 0.098).[4] Treated departments are thus those where the treatment was positive and high, while the control group

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

consists of departments where few or no rooms were built. Our estimates, then, reflect the average impact of the policy on a cohort's outcomes for departments with high treatment levels, 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-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 6.

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 on the treated (ATT) effect. ATT is the ITT rescaled by the probability of treatment (i.e., the intensity of treatment in treated groups related with the control group). We can interpret it as the ATT of an additional place of pre-primary education.

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.[5] Since equation (1) 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

[5] 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 E.

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 further test whether the expansion of classrooms correlates with the outcomes of the cohorts which were not exposed to the program. To rule out this possibility, we estimate a more flexible specification:

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

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 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 2 plots the estimates of β_{post} (the intent-to-treat effect, ITT) for $m = 0, 1, \ldots, 12$, covering the primary and secondary cycles.[6] We find no effect for the first six post-kindergarten years—consistent with historically high primary completion in Argentina[7]—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 2. 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 post-secondary education. All effects in Table 1 are significant at the 1% level.

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 some higher 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 Table B1 (Appendix B) 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 3a 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 B1–B3 show similar patterns for related outcomes.

[6] We also show the estimates of β_{pre} in Figure B8 (Appendix B). Reassuringly, these indicate no systematic pre-trends in our outcomes of interest.

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

3.2 Effects on fertility

Given the documented education impacts, we also study the effects of the program on fertility.[8] 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, 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.

Table 2 summarizes the main results. Column (1) shows that an additional pre-primary place reduces the probability of being a mother by 10.63 percentage points (about 17% of the pre-treatment mean). Column (2) shows a reduction of 0.18 live births per woman (about 15% 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 -1.135 pp, -0.010 live births, and 0.672 pp, respectively, none statistically significant, supporting the absence of pre-trends. The dynamic specification (equation 2) for total live births in Figure 3b 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 B4 and B5 display similar patterns for related outcomes. We also replicate the fertility findings using administrative birth records from the Ministry of Health; results are consistent (Appendix J).

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 1.18 percentage points (less than 2% of the pre-treatment mean), not statistically significant. Column (2) shows a 0.985 percentage point increase in the probability of formal

[8] 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.

employment (about 3.5% 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: -0.262 percentage points for formal employment (not significant) and -0.945 percentage points for employment (significant at 10% but quantitatively minor). They are unlikely to account for our main results. Gender-specific estimates (Table B1, Appendix B) are similar for women and men, and we cannot reject equality of effects. The dynamic estimates for employment and formal employment (Figures B6 and B7) show no significant impacts for either pre-treatment (1985–1987) or post-treatment cohorts (1989–1992).

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 enrollment, 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 Potential mechanisms and interpretations

In this section, we discuss potential mechanisms through which the policy may have generated effects on educational and fertility outcomes. The program we study promoted the development of cognitive and non-cognitive skills in the treated children, measured by test scores in primary school (Berlinski *et al.*, 2009).[9] It is plausible that

[9] However, we use a different measure of treatment than that in Berlinski *et al.* (2009), and this could undermine the comparison between the two sets of results. In Appendix G, we address this concern by

these new skills explain our main results, aligning with the literature on the long-term effects of early education and care interventions (see Cunha and Heckman, 2007 and the works cited therein). However, we cannot rule out that these effects may be due to other changes associated with the new pre-primary facilities.

The increased availability of pre-primary education may effectively subsidize child-care, as the education system assumes part of caregiving responsibilities. In this way, the program could have influenced the decisions of those responsible for the treated children, giving them more time to devote to other tasks, potentially affecting the household's economic situation (Humphries *et al.*, 2024). For example, if more hours are now devoted to work, the household may have more resources. If these changes are associated with higher educational attainment for the children, the estimates presented earlier would reflect this channel.

Empirical evidence suggests that caregiving tasks mostly fall on mothers (see, for example, Berniell *et al.*, 2021; Kleven *et al.*, 2019), so it is particularly important to study whether their decisions changed as a result of the program. In this context, the mothers of the treated children might have allocated the additional available hours to the labor market, increasing their participation at both the extensive and intensive margins. Evidence provided by Berlinski and Galiani (2007) suggests that the impact of the program on maternal employment was small and mostly concentrated among the mothers who have no younger children, making it unlikely that our results can be entirely explained by this mechanism.[10]

5 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

re-estimating their effects on test scores with our program exposure measure. Reassuringly, we obtain similar results to those in the original paper.

[10] 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.

estimated increase in schooling. We discount at 3% throughout and use our estimated ATT of 0.48 additional years of completed (post-K) schooling per additional seat created. We map schooling to earnings with a standard Mincer return; in the baseline, we conservatively apply a 4.1–4.3% wage premium per treated child implied by $\theta = 0.48 \times r_s$ with $r_s \simeq 9\%$.[11]

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 over 25 years at 10% using the equivalent-annual-cost formula, $EAC = 15{,}000 \cdot \frac{0.10}{1-(1.10)^{-25}} =$ $1,653 per year and add recurrent operations of $750 per month (teacher plus materials), that is, $9,000 per year. The implied direct annual cash requirement is therefore $10,653 at the classroom level, 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 at the prevailing average per-pupil public expenditure 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 $100/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 induced participant. Let w_a denote average (gross, real) earnings at age a, e_a the employment probability, and δ the discount factor. For each induced child, the proportional earnings effect is θ. The present value of benefits per treated child is $PV_B = \sum_{a=a_0}^{A} \theta\, w_a\, e_a\, \delta^a$ with labor-market entry at $a_0 = 18$ and terminal working age $A = 64$. 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 equal the discounted value, at 3%, of one year of the capital EAC and one year of recurrent spending attributable to the entering cohort, plus the indirect cost items described above.

Under these assumptions, the baseline calibration yields a benefit-cost ratio (BCR) of approximately 10.98 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

[11]Using $r_s = 9\%$ implies $0.48 \times 0.09 = 4.32\%$. Our baseline calibration rounds to 4.1% to be conservative; results are virtually unchanged at 4.32%.

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. 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% EAC for capital 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. Given the large benefit-cost ratio we estimate, it is mechanically implied that, for any reasonable income tax rate, the MVPF falls in the "win–win" region. We make this link explicit with the following simple exercise. We compute the MVPF following Hendren and Sprung-Keyser (2020) and building on the BCR parameters and assumptions above. Let C denote the present value of direct fiscal costs per preschool seat and B the present value of the resulting increase in beneficiaries' gross lifetime earnings, as in our baseline benefit-cost analysis. Our estimate of the benefit-cost ratio, $B/C \approx 11$, implies that one peso of program spending generates about 11 pesos of additional earnings. With a proportional tax rate of $\tau = 0.25$, the government recovers $\tau B \approx 2.75C$ in extra tax revenue. The net fiscal cost is therefore $\Delta G = C - \tau B \approx C - 2.75C = -1.75C < 0$: the program both raises beneficiaries' after-tax income (by $(1-\tau)B$) and reduces net government spending. In our setting, the very high benefit-cost ratio implies that the program is attractive both from the social planner's perspective and from the government's budgetary perspective, placing it in the "win–win" region of the MVPF criterion: the standard MVPF=$(1-\tau)B/(C-\tau B)$ is effectively infinite, since the denominator is negative while the welfare effect is positive.

6 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 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.

6.1 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 E).

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 D 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, 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 B for our main dynamic specification, suggesting that potential flaws in the TWFE specification are not a major concern in our setting.

6.2 Berlinski *et al.* (2009) results and approach

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 G 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 H 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.

6.3 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.

First, 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. Likewise, the public pre-primary expansion program may have caused migration from the private to the public school system. In both cases, the concern is that our results may reflect changes in the relative composition of students rather than the impact of attending pre-primary. To test these hypotheses, we examine whether the public pre-primary expansion affected either the distribution of students enrolled in each department within a given province, or, alternatively, the distribution of students between public and private primary schools within a department. For this additional check, we rely on administrative data from the National Education Ministry on public and private primary school enrollment by department and primary grade (i.e., not on Census data). This information is available for 1994 and yearly for 1996–2002. We use year and grade to define the school cohort. We estimate the compact specification (1) to assess the effect on the departmental share of students in primary school and the share of students in public primary schools. The results for these robustness checks, presented in Table F1 in Appendix F, are not significant at conventional levels. Accordingly, this additional evidence indicates that is its unlikely that the pre-primary expansion program resulted in substantial geographical mobility of students between departments in a given province, or from private to public schools within a department.

We also analyze the impact of the new pre-primary facilities on departmental cohort sizes. First, we estimate the effect on the ratios between cohort sizes for two different years to evaluate whether the policy was correlated with changes in departmental population structure over time. In the first and second columns of Table F2 of Appendix F, we present the estimation for the years 2010–1991 and 2001–1991, respectively, and we do not find significant results. In the third column, we assess whether the departmental cohort size in 2010 is correlated with the policy, and again we do not find significant results.

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 F for educational attainment and fertility in Tables F4 and F5, respectively. These results are broadly in line with those of our main specifications.

Finally, selective migration might have altered the composition of households or their characteristics, which would bias our estimates if these changes in parent or household characteristics were correlated with pre-primary construction. To address this concern, we assess whether changes in the average characteristics of heads of households in our sample between 1991 and 2001 are correlated with the new pre-primary facilities. These results are presented in Appendix F in Table F3. In the first column, we present the estimate of the impact of the construction program on the ratio of the 2001 departmental average head of household years of schooling to the same departmental average in 1991. 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. In the second column of Table F3, we present the estimate 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.[12] 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

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.

6.4 Alternative approach: estimates near the cut-off

To further assess the robustness of our findings, we implement an alternative estimation strategy in Appendix I 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

[12] 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.

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 I1, I2, and I3, 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. Labor-market and fertility outcomes also align with expected signs, although not all estimates reach conventional significance levels. Importantly, these effects are consistent across bandwidths, reinforcing the robustness of our main findings.

6.5 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 2005–2018. 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 J are consistent with our main estimates based on individual data from the Census.[13]

7 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 enrollment. Additionally, the expansion of pre-primary education has a notable demographic impact, as evidenced by the decline in fertility rates among

[13]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 of the total live births for each woman, while the results in Appendix J use aggregated data at the department-cohort panel, which is why we only claim a partial replication with this alternative data source.

women who were induced to attend pre-primary by the expansion program.

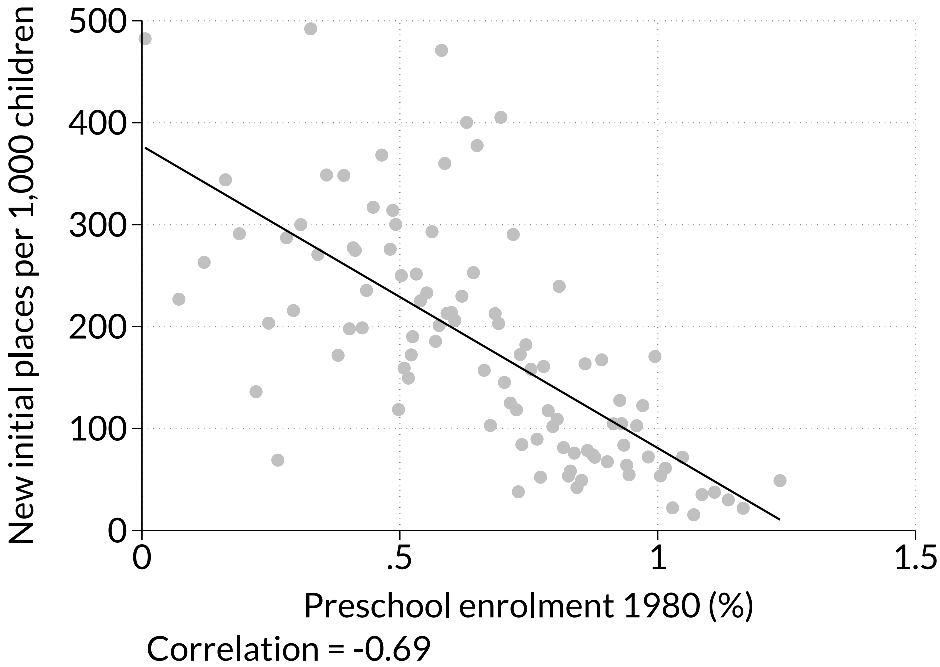
These results underscore the importance of early childhood education as an investment in human capital, particularly in developing and middle-income countries. Our research contributes to the broader literature on early childhood development by offering empirical support for the positive long-term effects 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.

Figure 1: First Stage

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



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

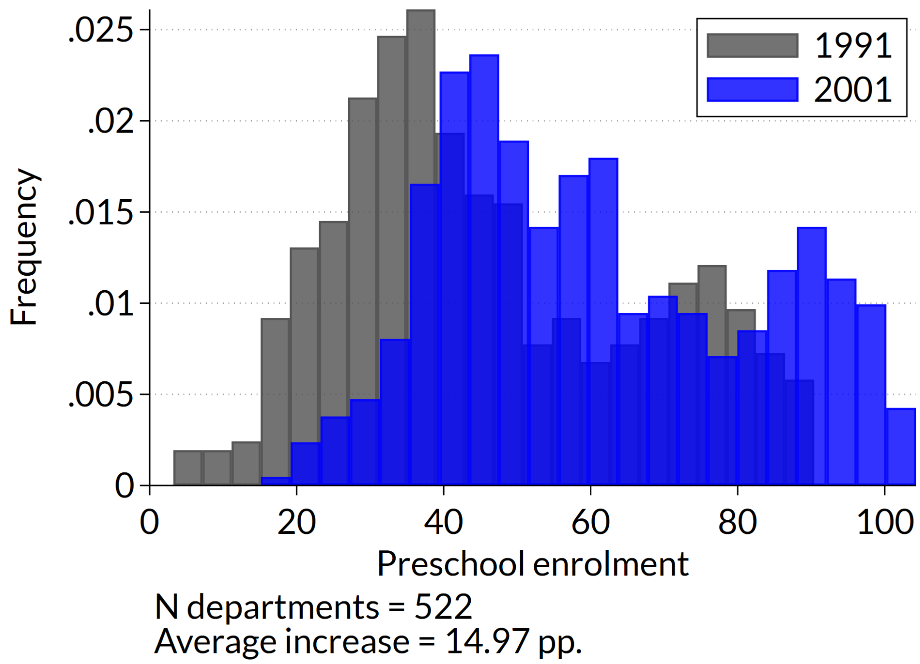
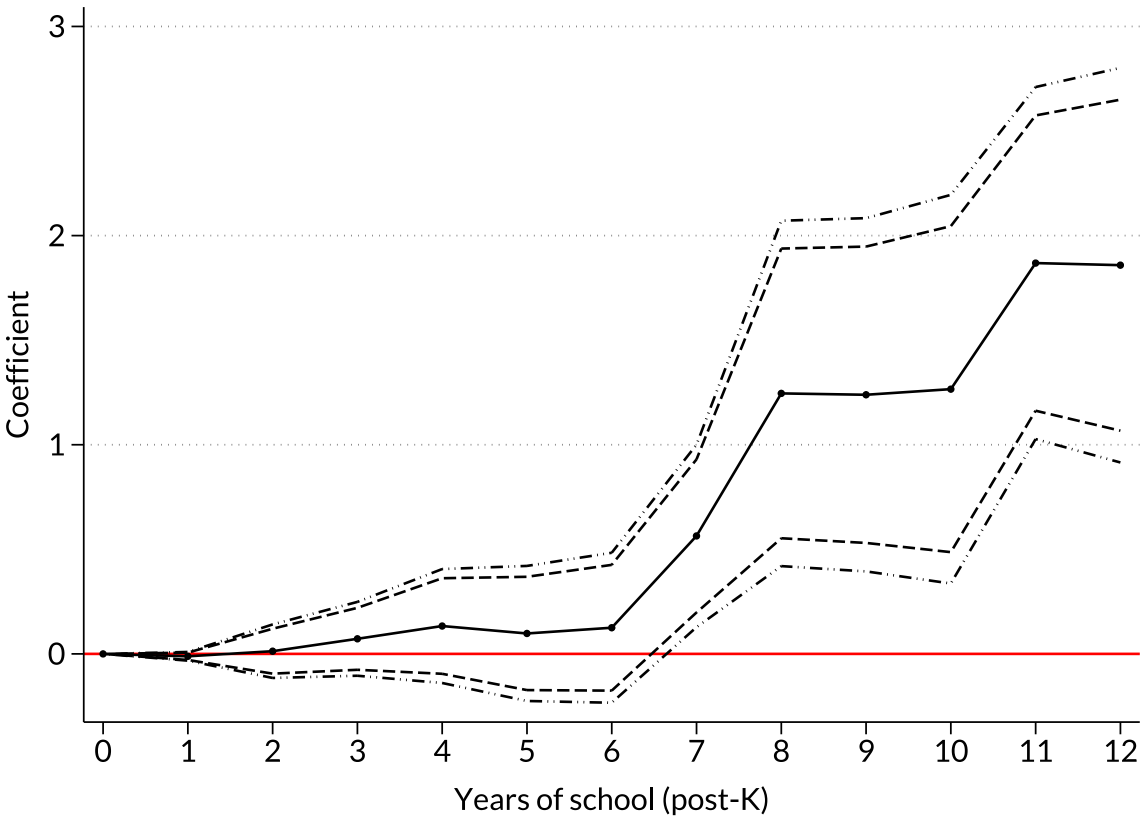


Figure 2: Difference-in-differences in the cumulative distribution function of post-kindergarten years of school. Post-treatment coefficients



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 and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors clustered by department of residence.

Table 1: Program effects on educational attainment

	Years basic school	Lower secondary (pp.)	Secondary (pp.)	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 the results of estimate (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the probability of treatment (i.e., the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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 and differential trends by departmental pre-primary gross enrollment in 1980. Standard error clustered at department of residence in parentheses. p-values 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	-10.633**	-0.175**	0.192
ITT	-2.025**	-0.033**	0.037
	(0.964)	(0.017)	(0.512)
	[0.036]	[0.046]	[0.943]
Pre coef.	-1.135	-0.010	0.672
	(0.782)	(0.013)	(0.535)
	[0.147]	[0.450]	[0.209]
Mean pre-treat	63.969	1.141	27.131
N	1,013,536	1,013,536	992,532
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 estimate (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 probability of treatment (i.e., the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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 and differential trends by departmental pre-primary gross enrollment in 1980. Standard error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

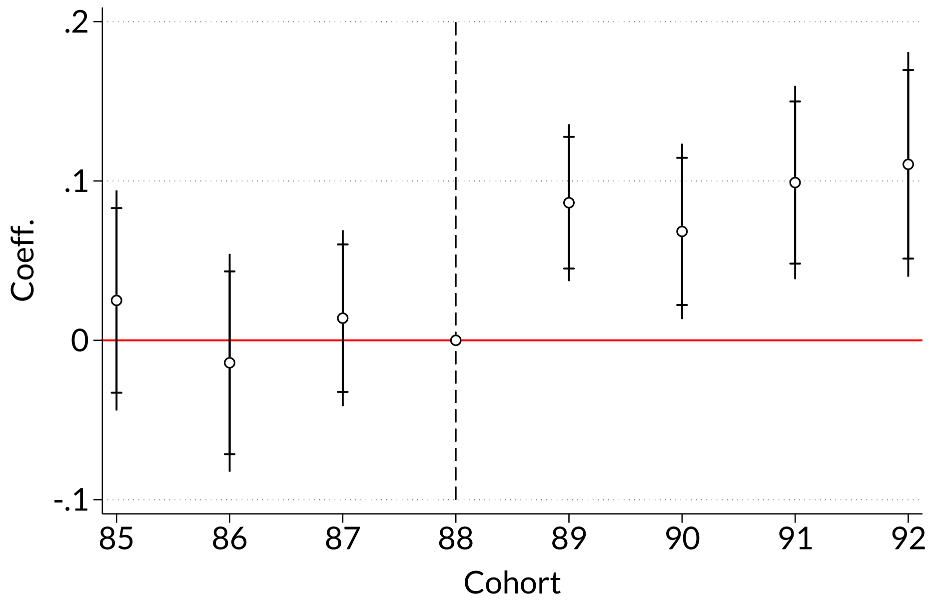
Table 3: Program effects on labor-market outcomes

	Employment (pp.)	Formal employment (pp.)
ATT	1.177	0.985
ITT	0.224	0.187
	(0.600)	(0.512)
	[0.709]	[0.714]
Pre coef.	-0.945*	-0.262
	(0.534)	(0.644)
	[0.076]	[0.684]
Mean pre-treat	59.651	27.256
N	2,027,662	2,027,662
N clusters	522	522
Controls and FE		
Department FE	Yes	Yes
Cohort FE	Yes	Yes
Coh. × Province	Yes	Yes
Coh. × Enr. 80	Yes	Yes

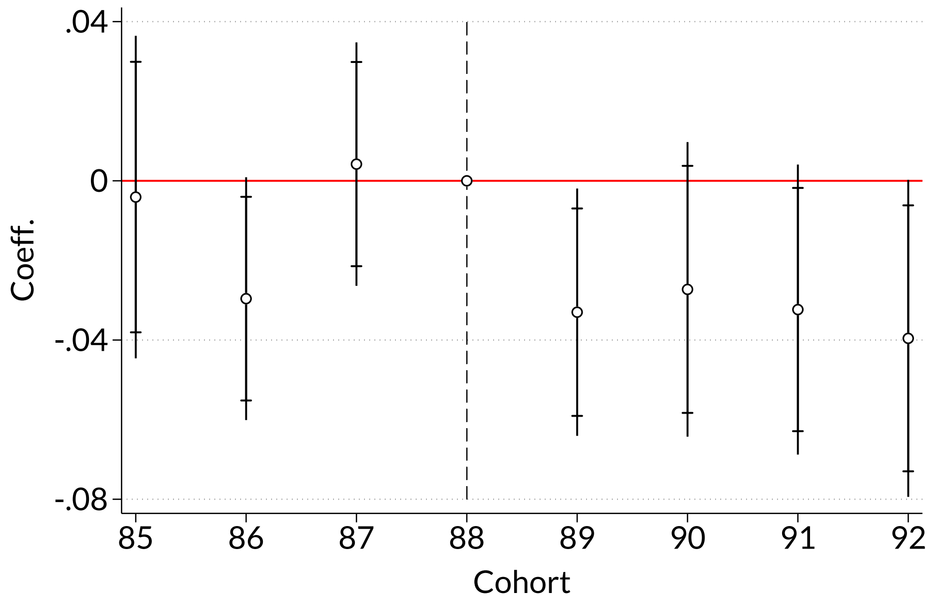
Note: The table presents the results of estimate (1) for different outcomes. We define "Employment" as a dummy variable indicating that the individual was employed at the time of the interview. We define "Formal employment" as a dummy variable indicating that the individual was employed and was contributing to social security at the time of the interview. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the probability of treatment (i.e., the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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 and differential trends by departmental pre-primary gross enrollment in 1980. Standard error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 3: Dynamic specification

(a) Years of basic school (Post-K)



(b) Total live births



Note: The figures present the results of estimate (2) for different outcomes. Each point of the figures represents an estimation for 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 and differential trends by departmental pre-primary gross enrollment in 1980. Confidence intervals at 90% and 95% are presented. Standard errors clustered by department of residence.

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Appendices

A Distribution of treatment by department

In this section, we present further details on the distribution of treatment levels by department. The treatment measure is a dummy variable indicating that the department built a total number of places per 3- to 5- aged children of 1991 that exceeds the median of the department distribution (approximately 0.098). Accordingly, we consider treated departments where the treatment was positive and high (relative to children), while the control group consists of departments where few or no rooms were built. Our estimates, then, reflect the average impact of the policy on outcomes of the cohort of departments with high treatment. The complete distribution is presented in Figure A1.

Figure A1: Distribution of places of pre-primary level per children

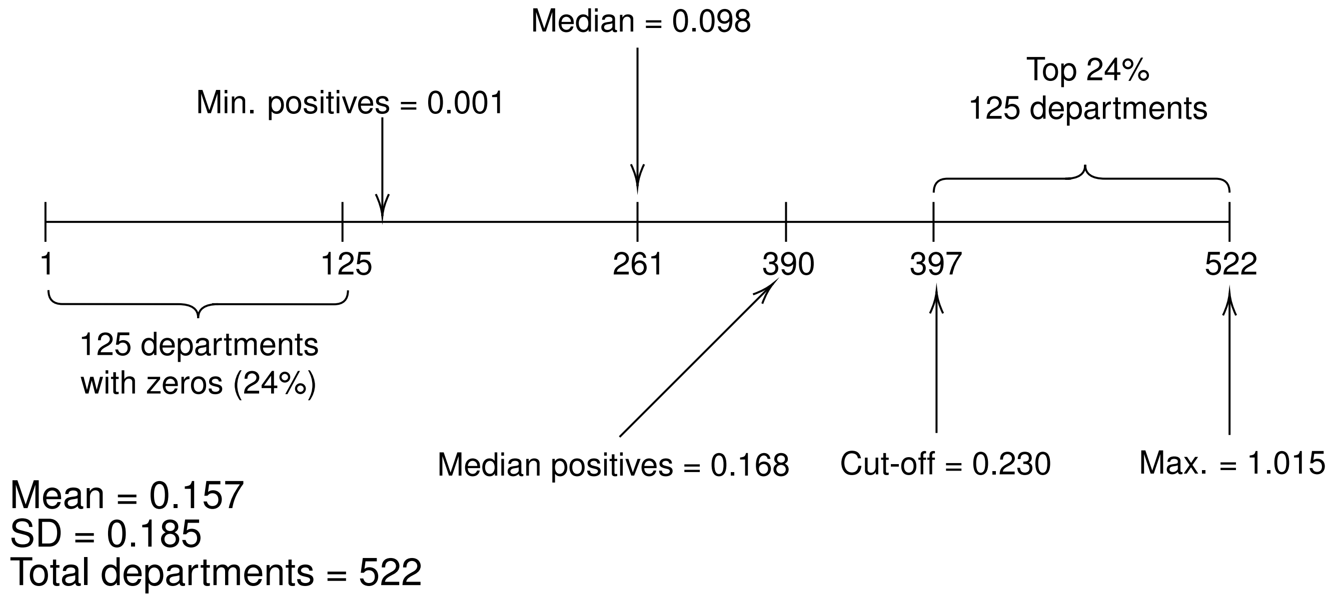


Table A1: Description of treatment measure

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	

B 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, Gertler, and Schargrodsky (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)).

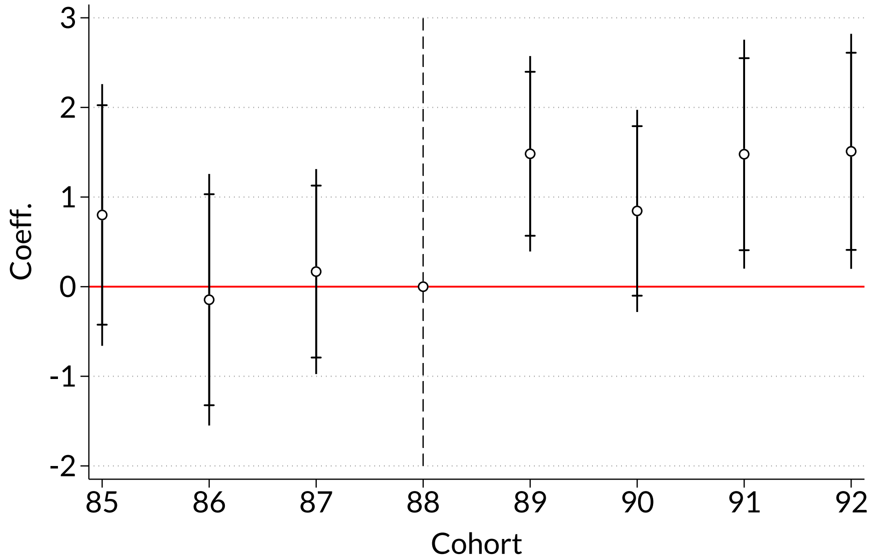
If the aforementioned changes were associated with the pre-primary construction program, the consistency of the estimates in model (1) would be threatened. However, interventions that similarly affect individuals in a given cohort and province are controlled for cohort-province fixed effects. Therefore, only the interventions that deferentially 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:

$$Y_{icpj} = \mu_j + \mu_c + \sum_{c \neq 1988} \beta_c \left[d_c \times D_j \right] + \gamma_{cp} + Enroll80_{cj} + \varepsilon_{icpj} \tag{B1}$$

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

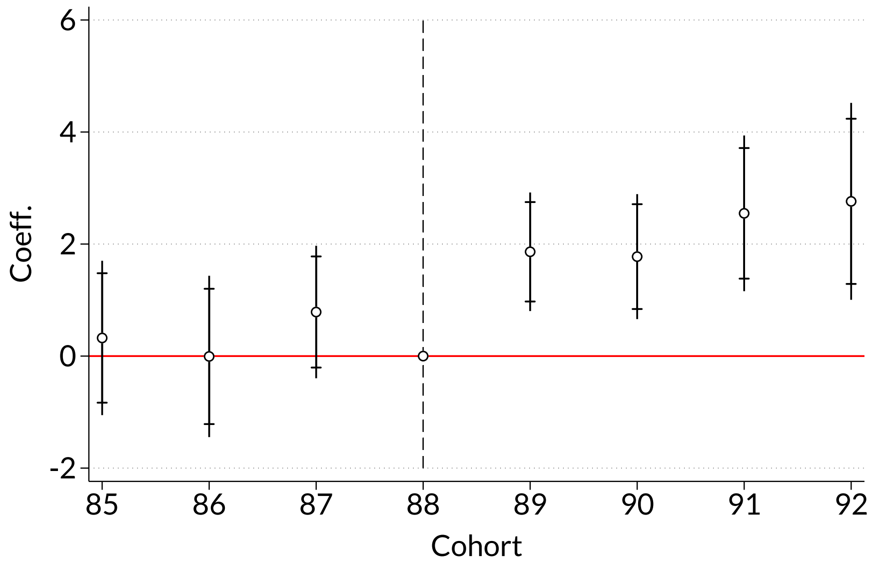
With this specification, it is possible to estimate a coefficient β_c for each cohort c, which offers at least two advantages. First, it also allows us to observe whether the policy had effects on untreated cohorts. Second, it allows us to observe whether there were heterogeneous effects among the treated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

Figure B1: Impact of new pre-primary facilities on the probability of complete lower secondary. Dynamic specification



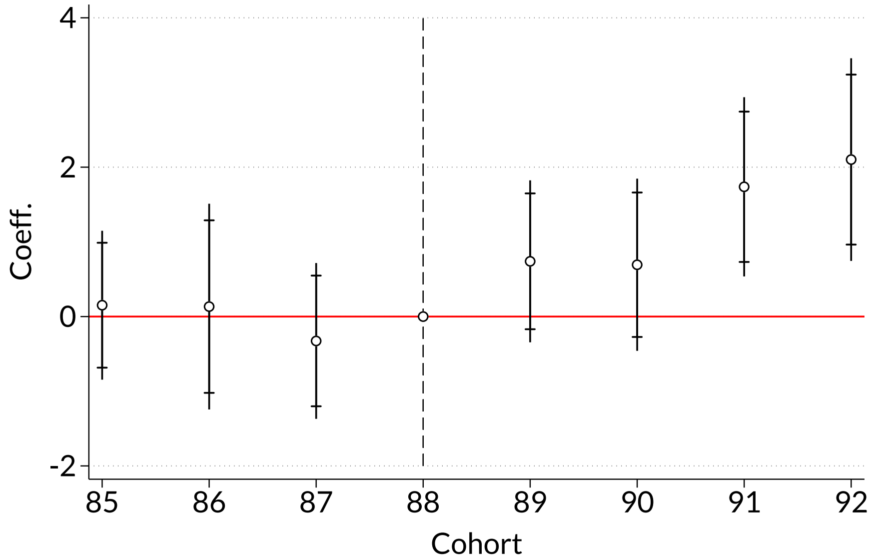
Note: The figure presents the results of estimate (2) for different outcomes. Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort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

Figure B2: Impact of new pre-primary facilities on the probability of complete secondary. Dynamic specification



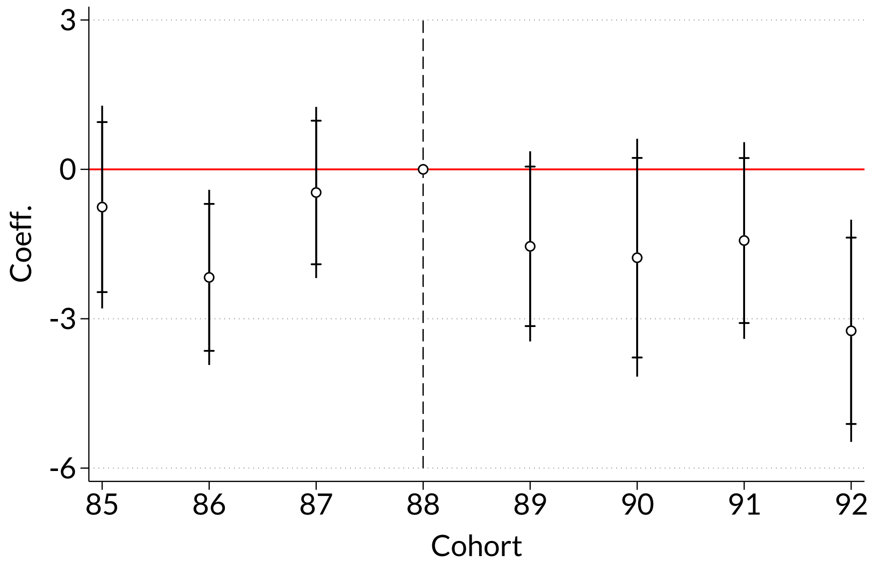
Note: The figures present the results of estimate (2) for different outcomes. Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort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

Figure B3: Impact of new pre-primary facilities on the probability of having post-secondary education. Dynamic specification



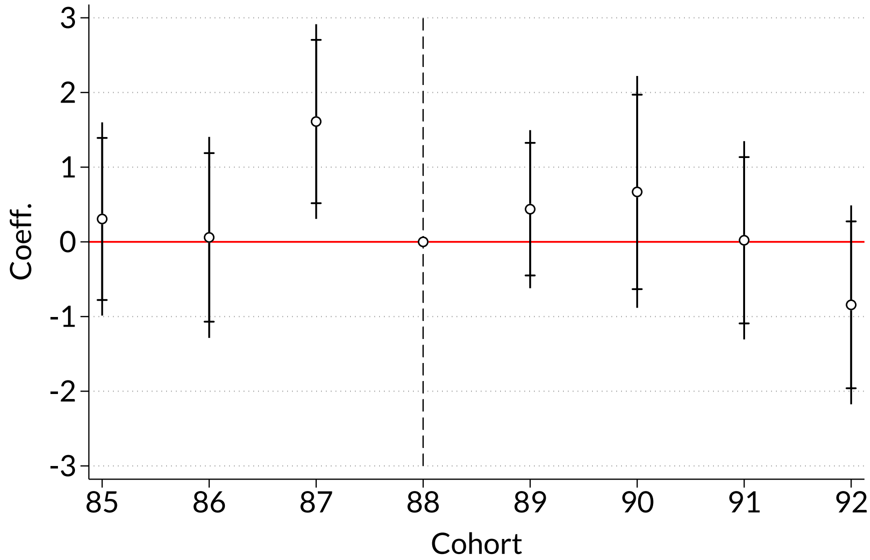
Note: The figures present the results of estimate (2) for different outcomes. Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort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

Figure B4: Impact of new pre-primary facilities on the probability of being mother (only women). Dynamic specification



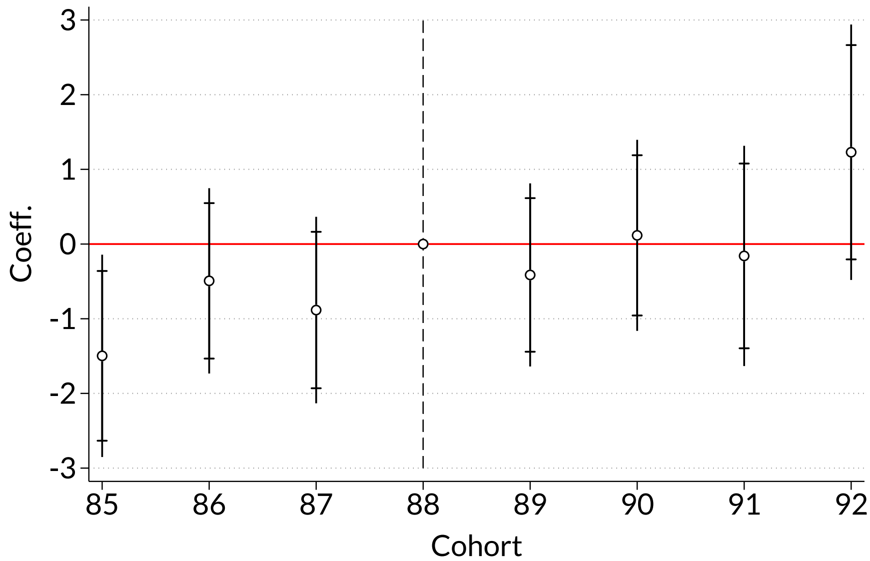
Note: The figures present the results of estimate (2) for different outcomes. Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort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

Figure B5: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). Dynamic specification



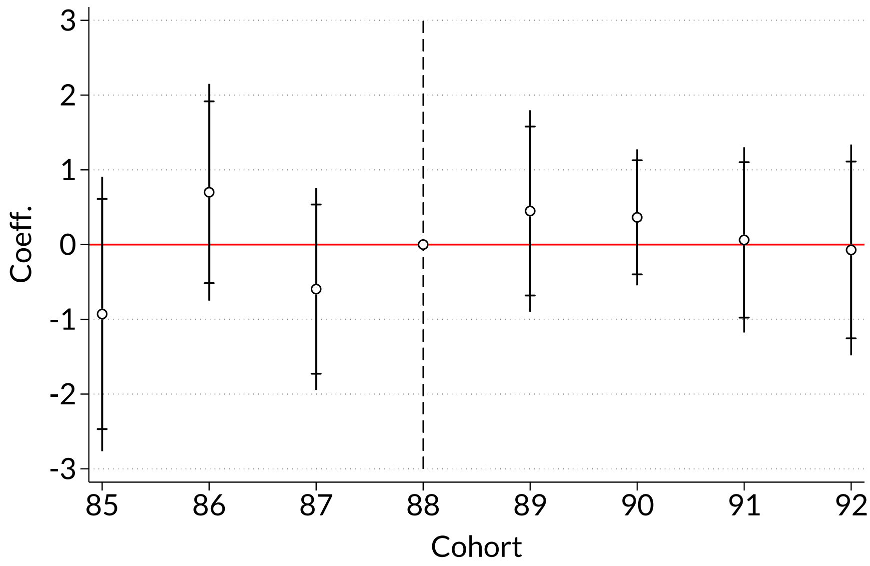
Note: The figures present the results of estimate (2) for different outcomes. Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort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

Figure B6: Impact of new pre-primary facilities on the probability of employment. Dynamic specification



Note: The figures present the results of estimate (2) for different outcomes. Each point of the figure represents an estimation for 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 clustered by department of residence.

Figure B7: Impact of new pre-primary facilities on the probability of formal employment. Dynamic specification

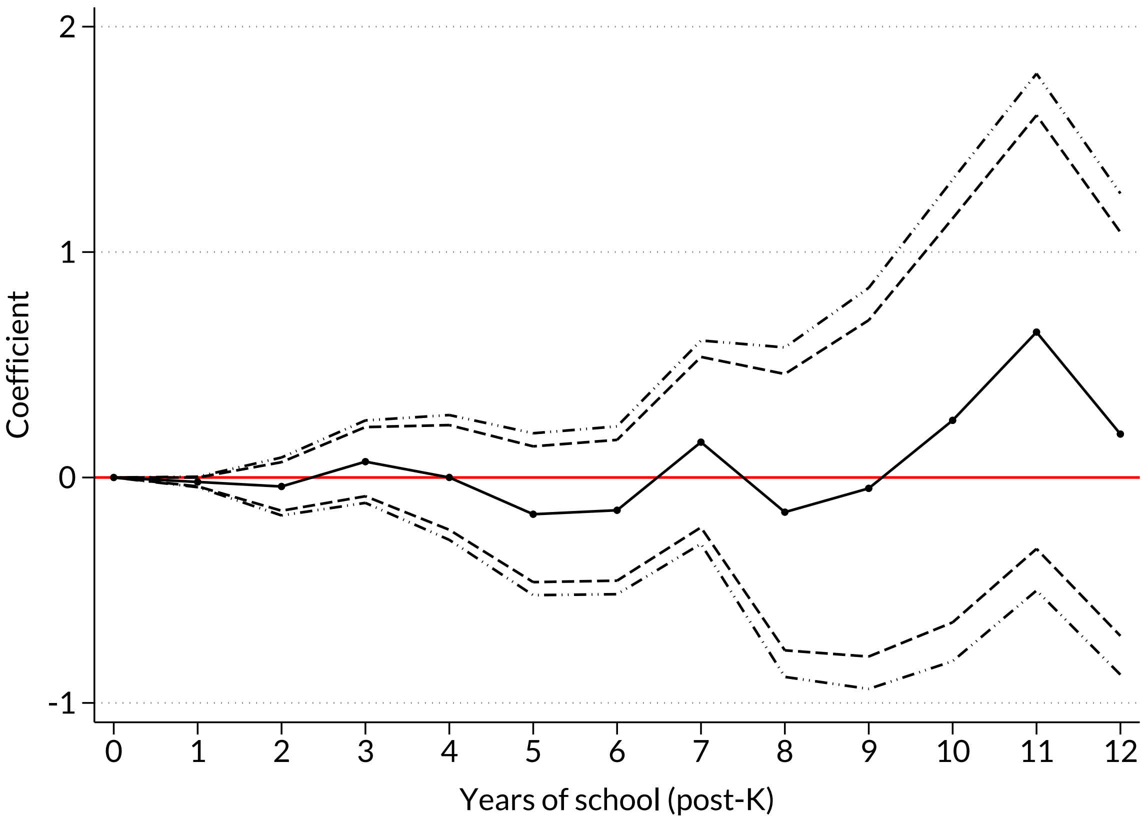


Note: The figures present the results of estimate (2) for different outcomes. Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, cohort, province-cohort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

We estimate the equation (1) considering S_{icpjm} as dependent variables, that is a a dummy variable indicating whether individual i completed m years of education or more. This is equivalent to estimate the impact on the cumulative distribution function for the post-kindergarten years of schooling. We present the estimations of β_{pre} (ITT) for $m = 0, 1, \ldots, 12$ in Figure B8, that corresponds to basic school (primary and secondary).

We also assess heterogeneity by gender in Tables B1 and B2. Specifically, we estimate equation (1) for women and men in our sample. As we found very similar point estimates to 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 B8: 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} of the equation (1) where the outcomes is a dummy that indicates that the individual reach m post-kindergarten years of basic school or more, with $m = 0, 1, ..., 12$. 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 clustered by department of residence.

Table B1: 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 the results of estimate (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the probability of treatment (i.e. the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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 error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B2: Labor market outcomes by gender

	Employment (pp.)		Formal employment (pp.)	
	Women	Men	Women	Men
ATT	1.093	2.503	0.455	2.164
ITT	0.208	0.477	0.087	0.412
	(0.943)	(0.667)	(0.718)	(0.630)
	[0.826]	[0.475]	[0.904]	[0.513]
Pre coef.	-1.196	-0.545	-0.800	0.271
	(0.782)	(0.607)	(0.836)	(0.763)
	[0.127]	[0.370]	[0.339]	[0.722]
Mean pre-treat	42.109	77.714	18.637	36.131
N	1,013,536	1,014,126	1,013,536	1,014,126
N clusters	522	522	522	522
Diff. post p-val.	[0.813]			
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 estimate (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the probability of treatment (i.e. the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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 error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

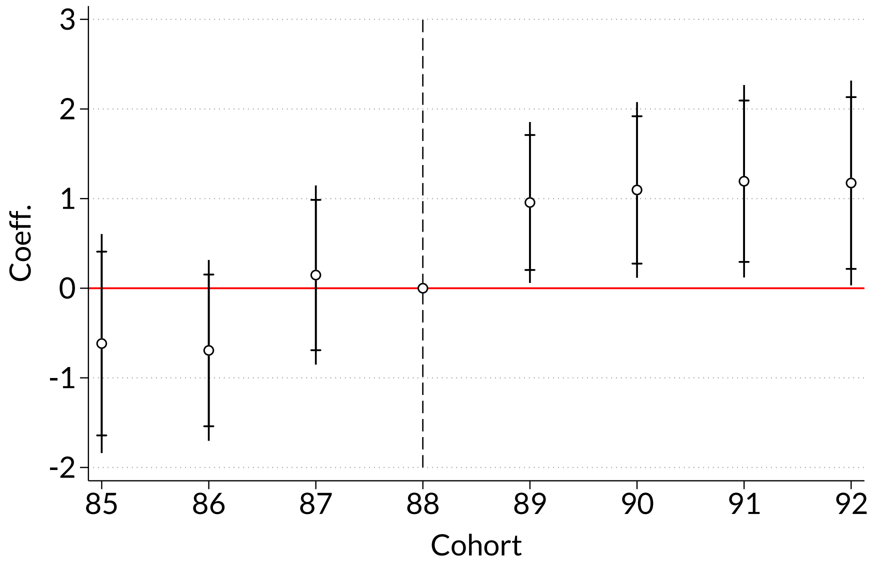
C Census data: Comparison of results with the expanded and basic questionnaire samples

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.

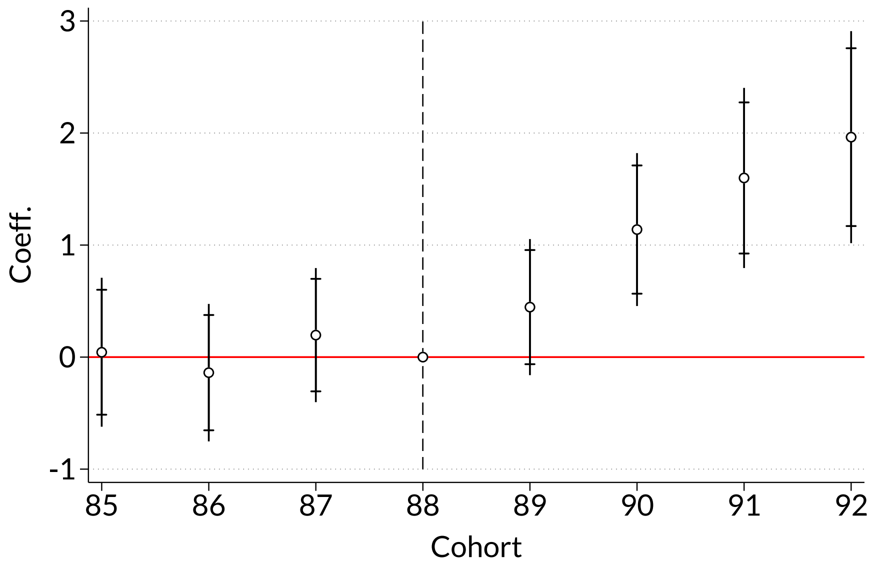
In this section, we suggest the equivalence between the weighted sample and the total population. We use both expanded and basic questionnaires data to re-estimate our main results. In order to assign treatment, we also re-define cohorts because birth date was not asked in basic questionnaire. Then,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

Figure C1: Some secondary completed

(a) Expanded



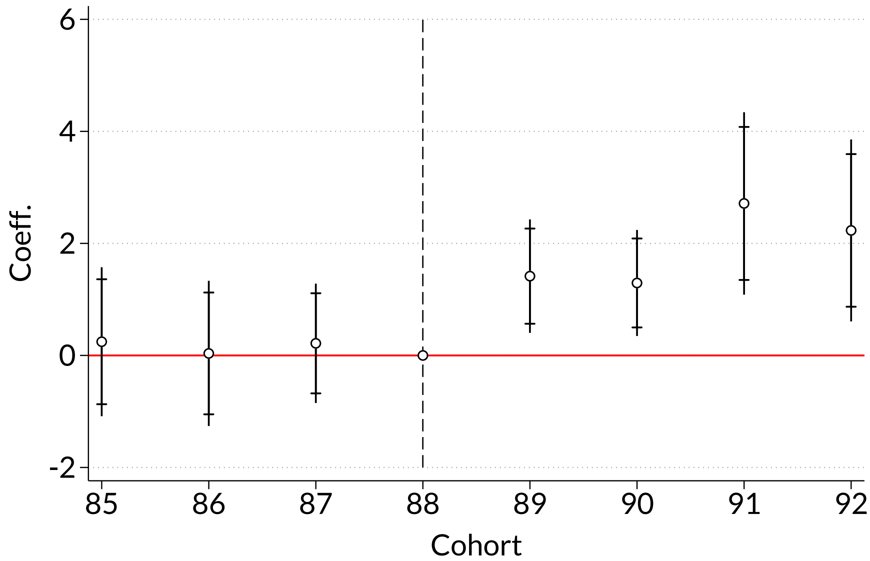
(b) Basic



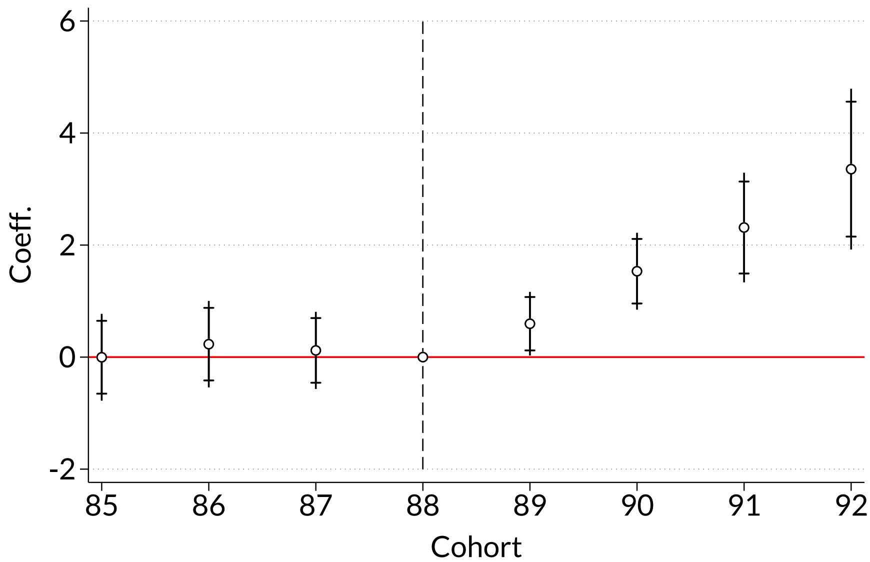
Note: The figures present the results of estimate (2) with data of basic and expanded surveys. Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, 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 clustered by department of residence.

Figure C2: Secondary completed

(a) Expanded



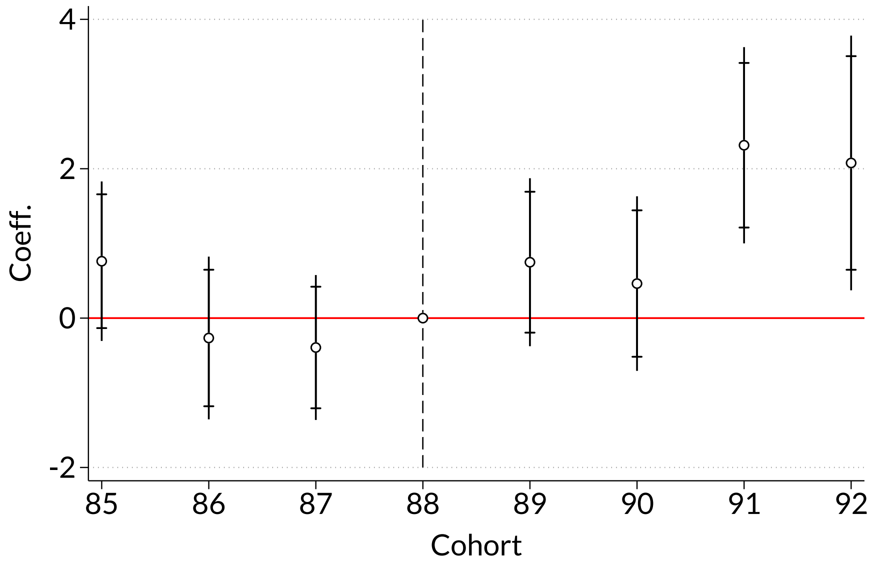
(b) Basic



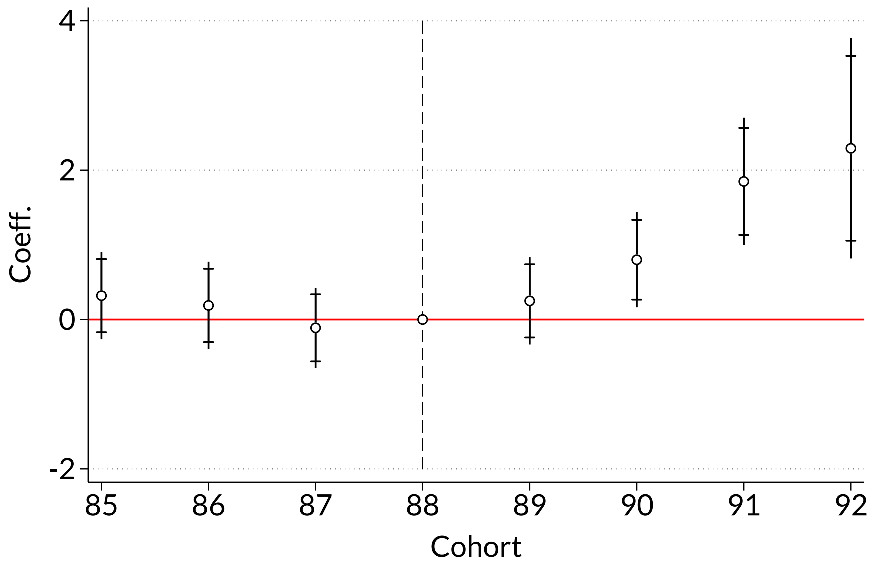
Note: The figures present the results of estimate (2) with data of basic and expanded surveys. Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places per child. We include department, 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 clustered by department of residence.

Figure C3: Post-secondary education

(a) Expanded



(b) Basic



Note: The figures present the results of estimate (2) with data of basic and expanded surveys. Each point of the figure represents an estimation for 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 clustered by department of residence.

D Alternative difference-in-differences estimators

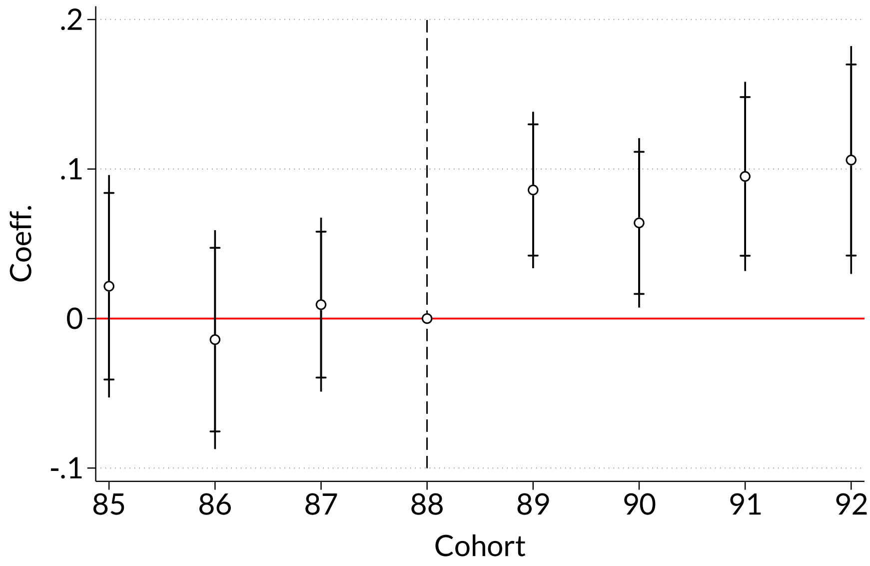
Recent literature on difference-in-differences has proposed alternative methods that are robust to various flaws in the traditional two-way fixed effects (TWFE) approach (Borusyak *et al.*, 2024; Callaway & Sant'Anna, 2021; Dube *et al.*, 2025; Goodman-Bacon, 2021; Sun & Abraham, 2021). Specifically, 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 this concern, we re-estimate our results using the method proposed by Dube *et al.* (2025). This method combines local projections with a cleaner control set of treated and untreated units to avoid forbidden comparisons. Our findings do not reveal significant differences from our main estimates, suggesting that this approach offers a useful alternative to other solutions proposed in the recent literature (Borusyak *et al.*, 2024; Callaway *et al.*, 2021; de Chaisemartin & D'Haultfuille, 2021; Sun & Abraham, 2021).

To address this concers, in this section we present the estimation on 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 B for our main dynamic specification, suggesting that potential flaws in the TWFE specification are not a major concern in our setting.

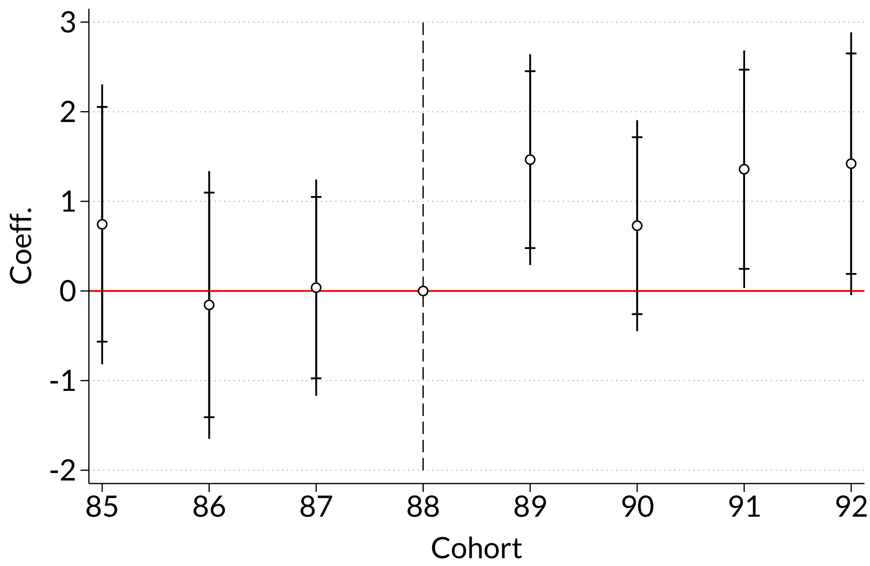
D.1 Estimation using the approach of Dube *et al.* (2025) (LP-DID)

Figure D1: Impact of new pre-primary facilities on the years of basic school (Post-K). LP-DID



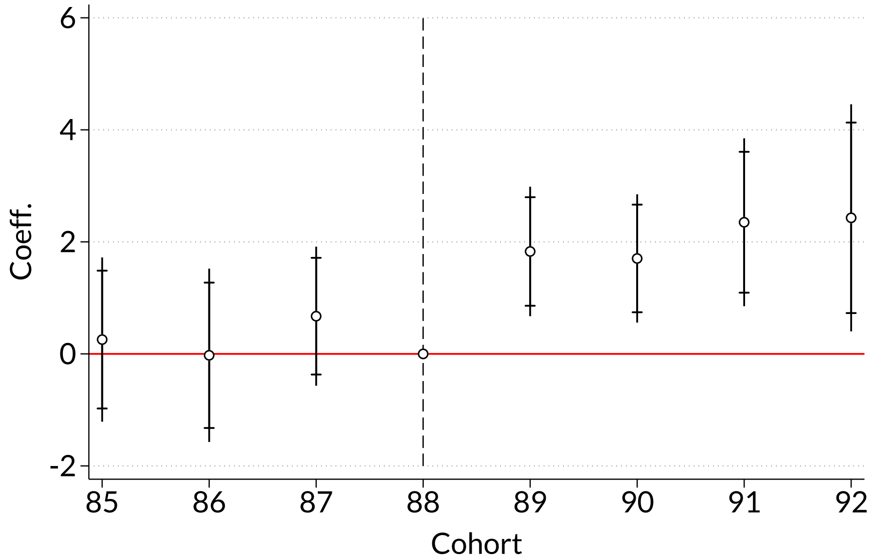
Note: The figure presents the results of the estimate of the effect on the years of basic school using Dube *et al.* (2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places 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 clustered by department of residence.

Figure D2: Impact of new pre-primary facilities on the probability of complete lower secondary. LP-DID



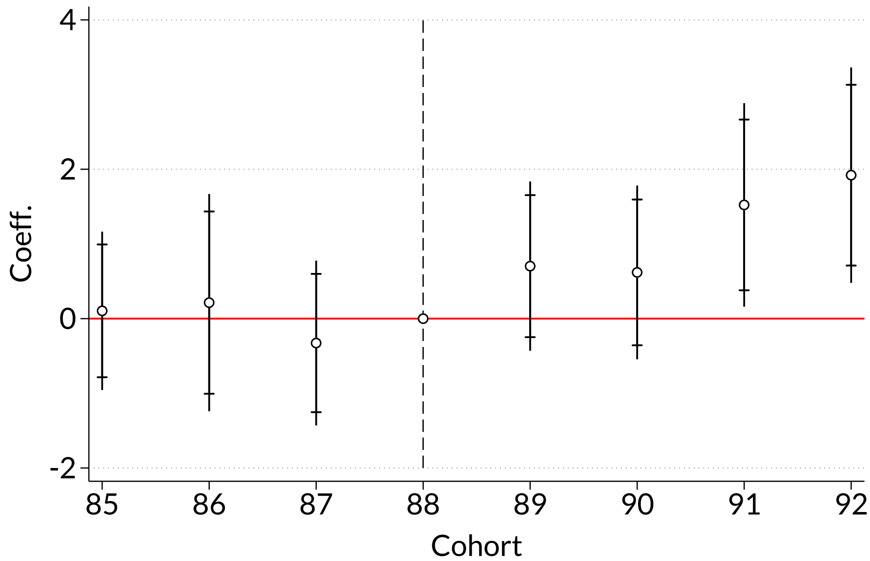
Note: The figure presents the results of the estimate of the effect on the probability of complete lower secondary using Dube *et al.* (2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total 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 clustered by department of residence.

Figure D3: Impact of new pre-primary facilities on the probability of complete secondary. LP-DID



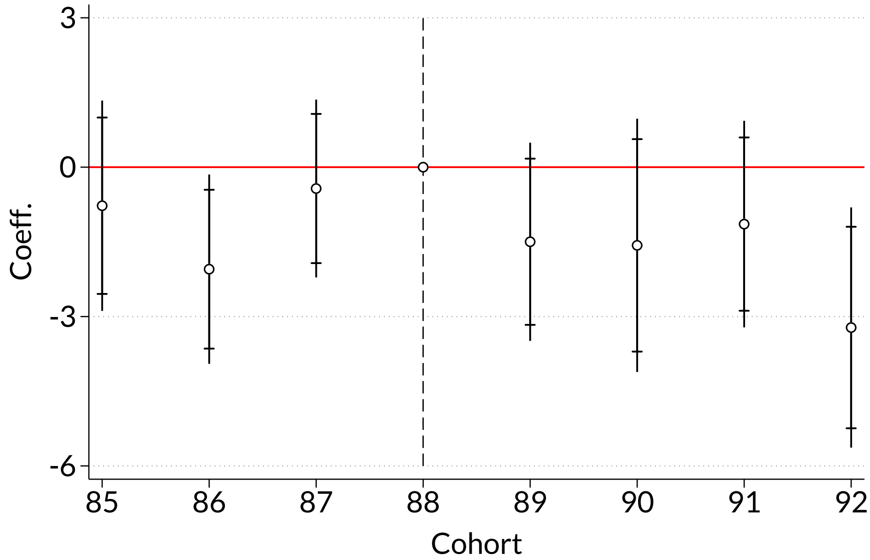
Note: The figure presents the results of the estimate of the effect on the probability of complete secondary using Dube *et al.* (2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number 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 clustered by department of residence.

Figure D4: Impact of new pre-primary facilities on the probability of having post-secondary education. LP-DID



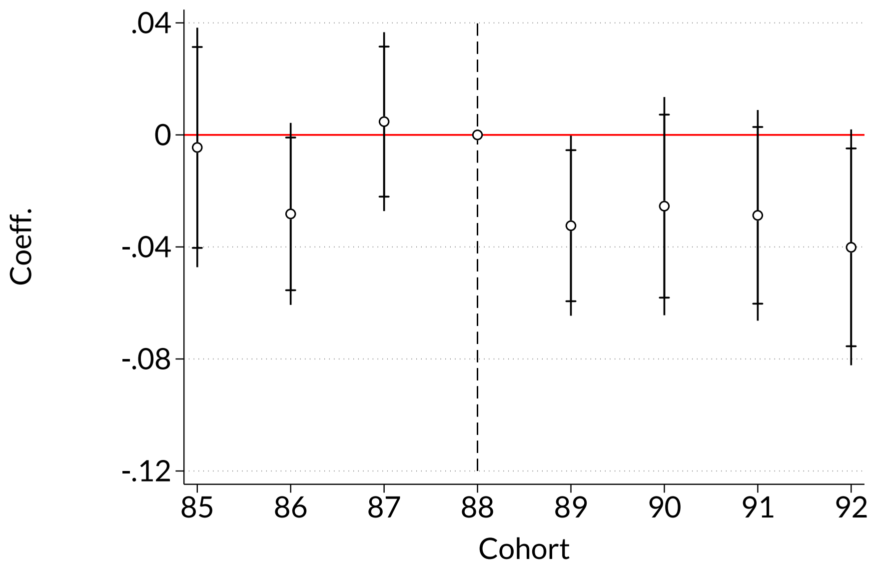
Note: The figure presents the results of the estimate of the effect on the probability of having post-secondary education using Dube *et al.* (2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median 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 clustered by department of residence.

Figure D5: Impact of new pre-primary facilities on the probability of being mother (only women). LP-DID



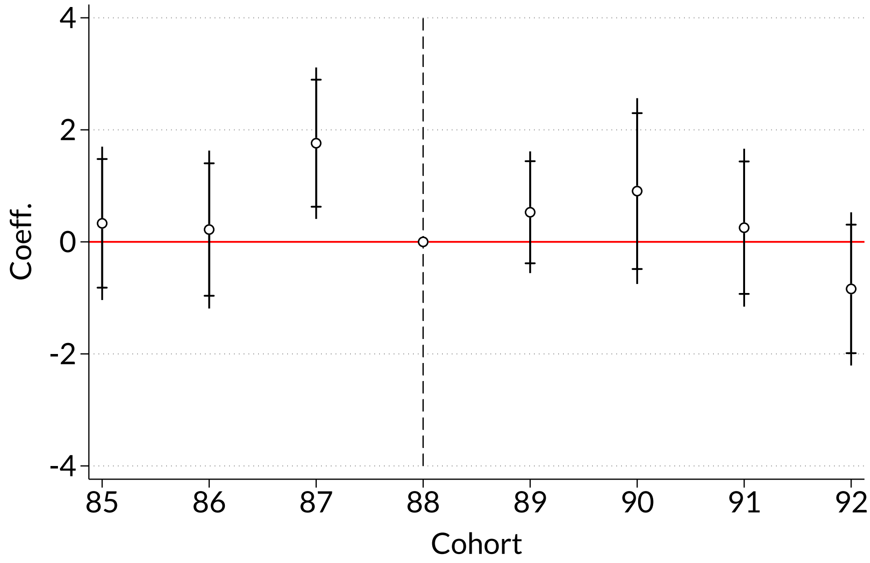
Note: The figure presents the results of the estimate of the effect on the probability of being mother using Dube *et al.* (2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of 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 clustered by department of residence.

Figure D6: Impact of new pre-primary facilities on the total of live births (only women). LP-DID



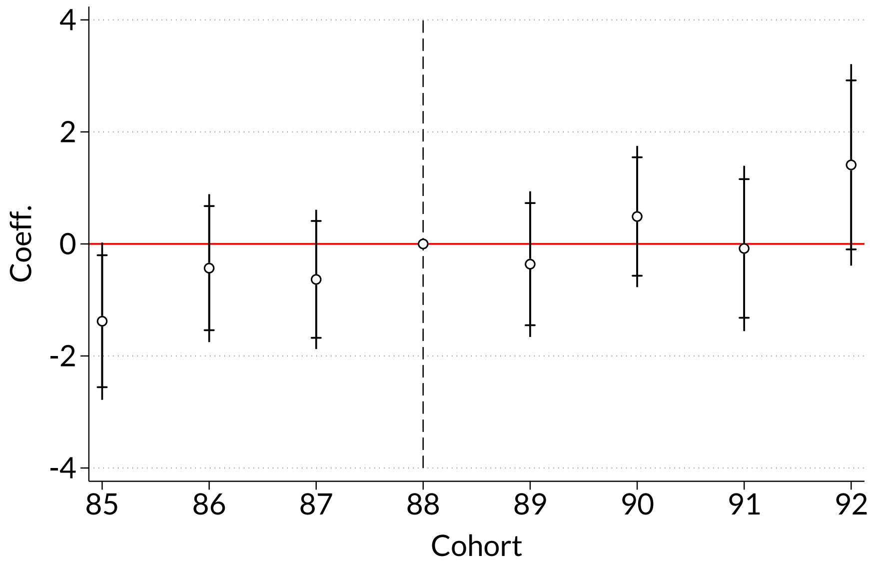
Note: The figure presents the results of the estimate of the effect on the total of live births per woman using Dube *et al.* (2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number 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 clustered by department of residence.

Figure D7: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). LP-DID



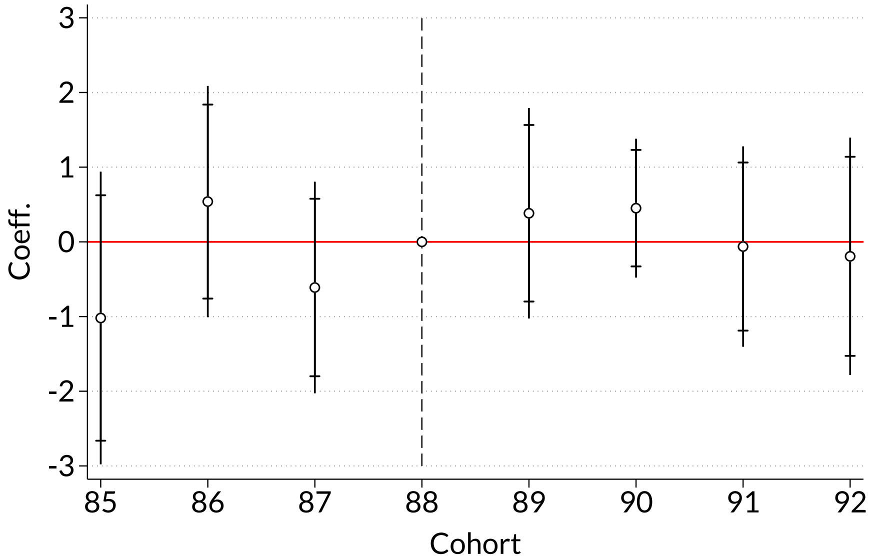
Note: The figure presents the results of the estimate of the effect on the probability of teenage motherhood using Dube *et al.* (2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number 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 clustered by department of residence.

Figure D8: 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). Each point of the figure represents an estimation for 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 clustered by department of residence.

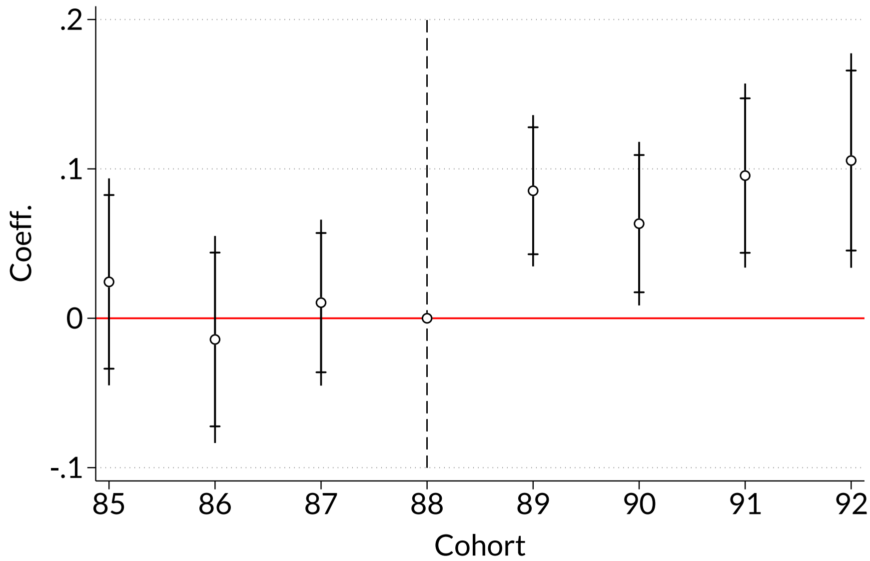
Figure D9: Impact of new pre-primary facilities on the probability of formal employment. LP-DID



Note: The figure presents the results of the estimate of the effect on the probability of formal employment using Dube *et al.* (2025). Each point of the figure represents an estimation for 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 clustered by department of residence.

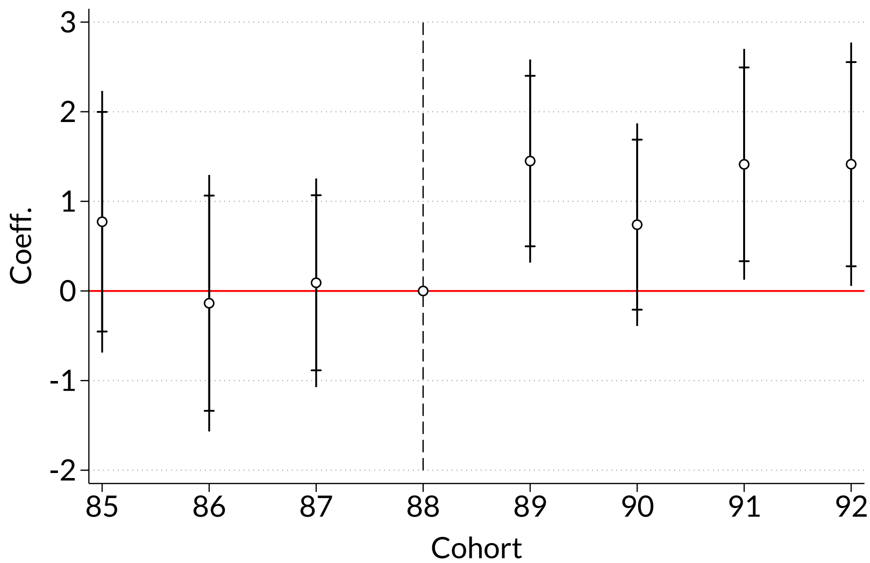
D.2 Estimation using the approach of Wooldridge (2023, 2025) (JW-DID)

Figure D10: Impact of new pre-primary facilities on the years of basic school (Post-K). JW-DID



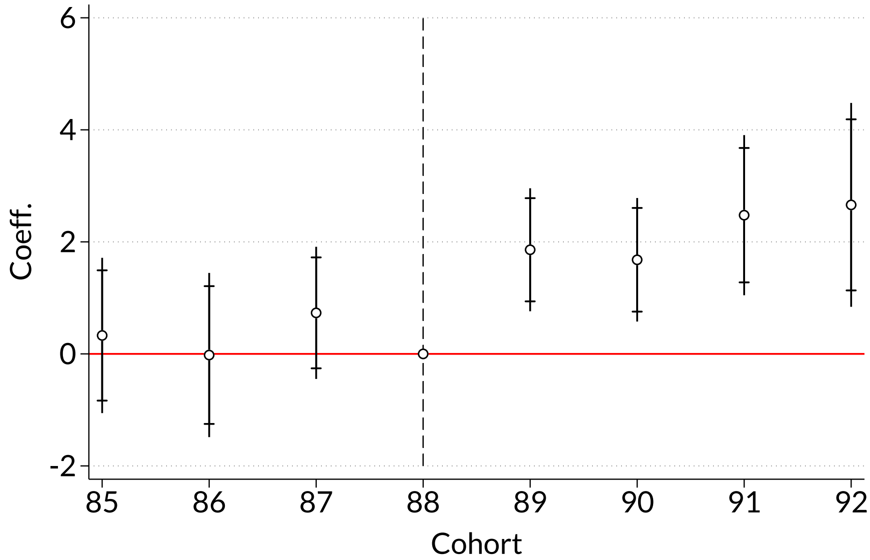
Note: The figure presents the results of the estimate of the effect on the years of basic school using Wooldridge (2023, 2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places 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 clustered by department of residence.

Figure D11: Impact of new pre-primary facilities on the probability of complete lower secondary. JW-DID



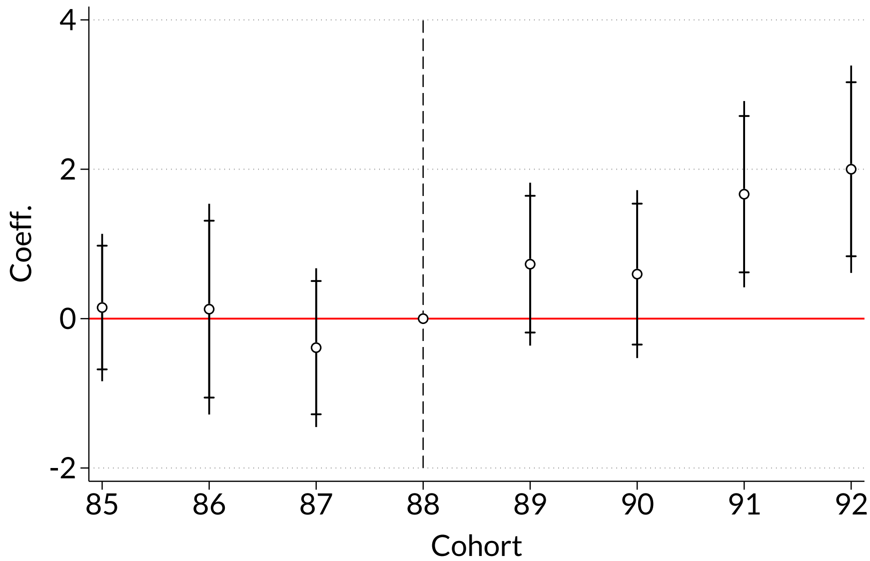
Note: The figure presents the results of the estimate of the effect on the probability of complete lower secondary using Wooldridge (2023, 2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median 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 clustered by department of residence.

Figure D12: Impact of new pre-primary facilities on the probability of complete secondary. JW-DID



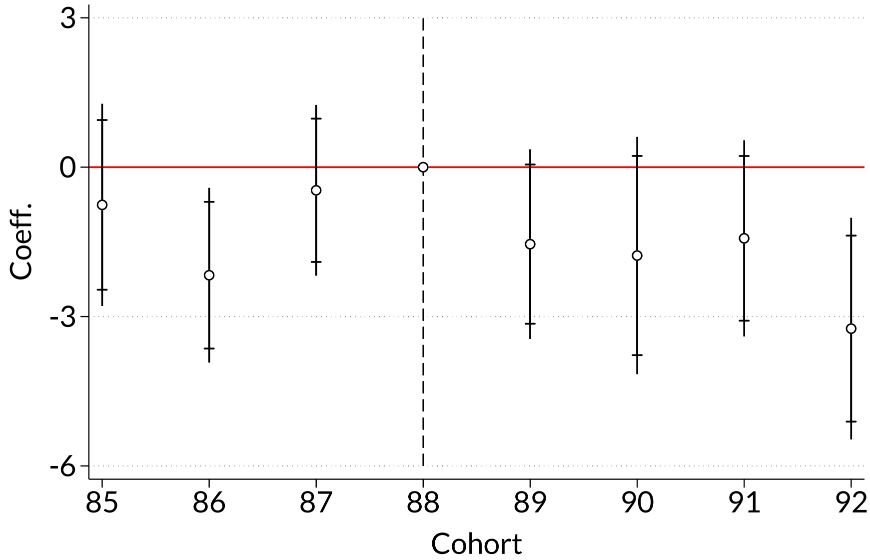
Note: The figure presents the results of the estimate of the effect on the probability of complete secondary using Wooldridge (2023, 2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total 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 clustered by department of residence.

Figure D13: Impact of new pre-primary facilities on the probability of having post-secondary education. JW-DID



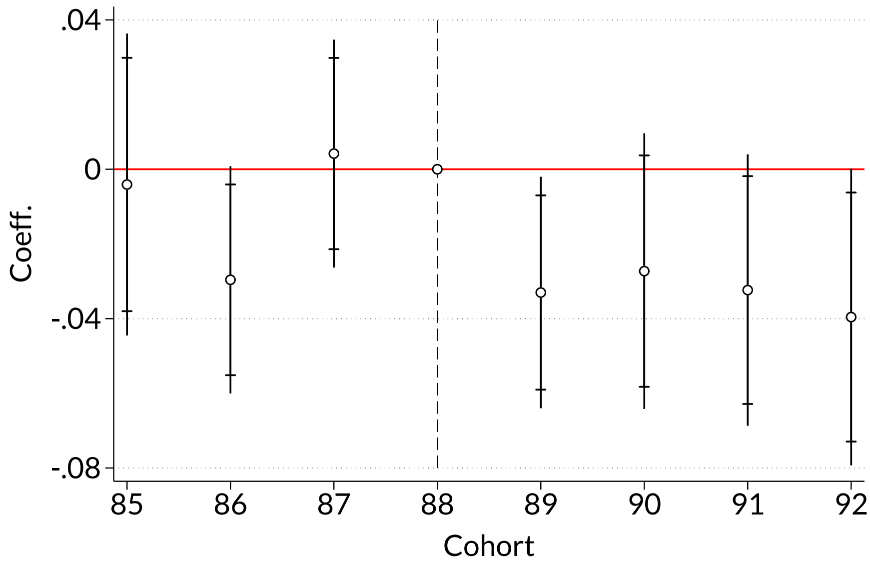
Note: The figure presents the results of the estimate of the effect on the probability of having post-secondary education using Wooldridge (2023, 2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median 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 clustered by department of residence.

Figure D14: Impact of new pre-primary facilities on the probability of being mother (only women). JW-DID



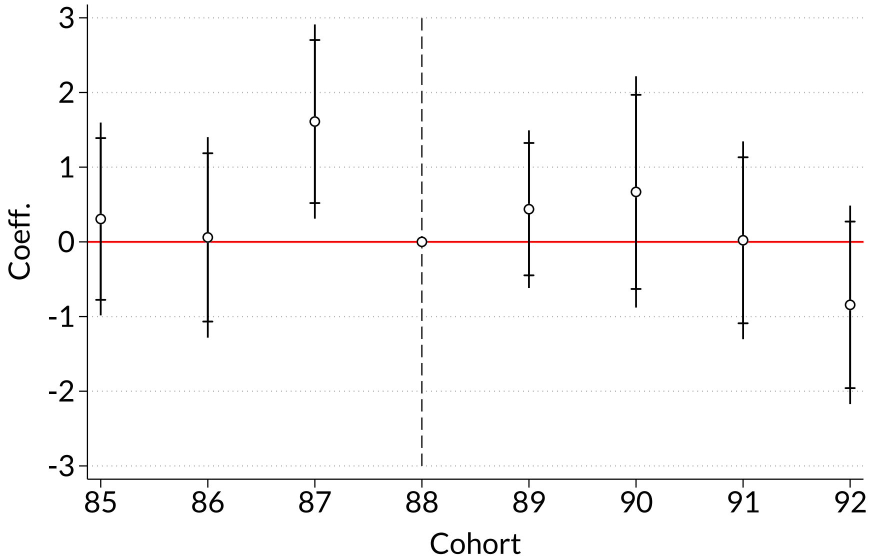
Note: The figure presents the results of the estimate of the effect on the probability of being mother using Wooldridge (2023, 2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number 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 clustered by department of residence.

Figure D15: Impact of new pre-primary facilities on the total of live births (only women). JW-DID



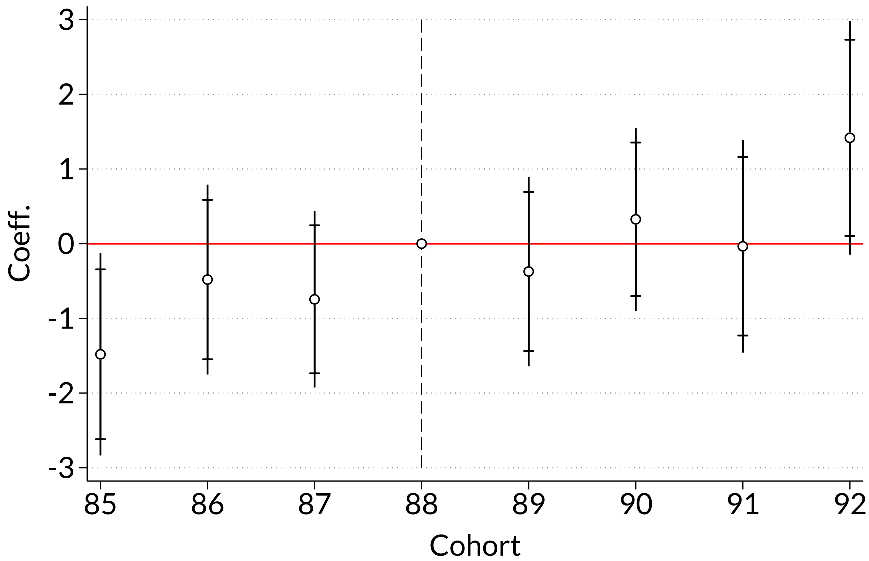
Note: The figure presents the results of the estimate of the effect on the total of live births per woman using Wooldridge (2023, 2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number 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 clustered by department of residence.

Figure D16: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). JW-DID



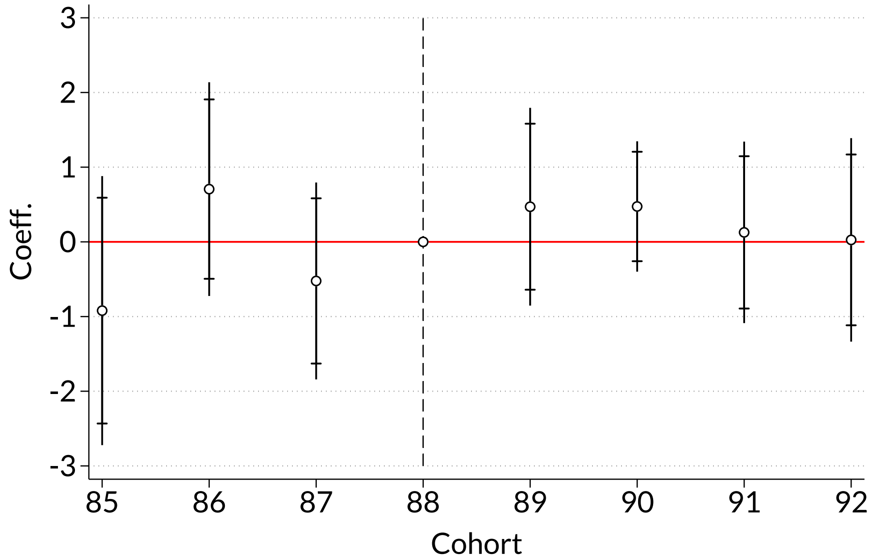
Note: The figure presents the results of the estimate of the effect on the probability of teenage motherhood using Wooldridge (2023, 2025). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total 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 clustered by department of residence.

Figure D17: 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 of the figure represents an estimation for 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 clustered by department of residence.

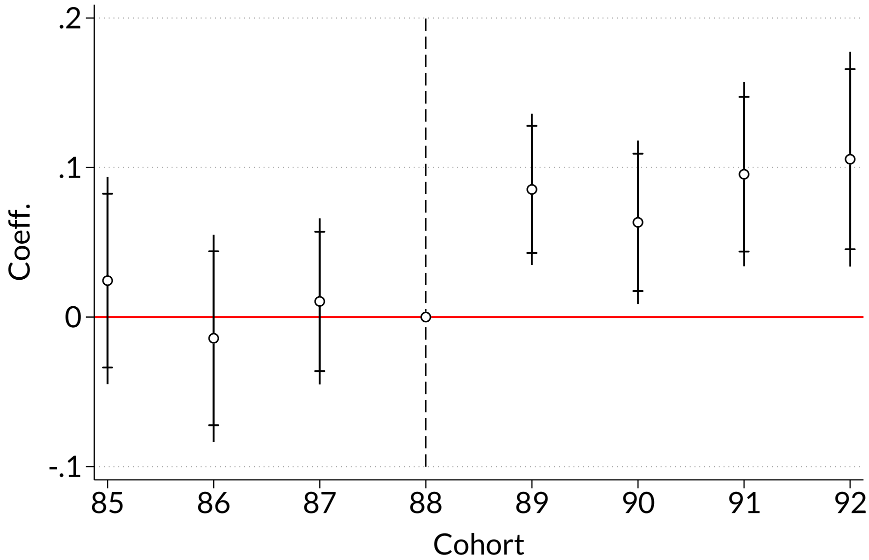
Figure D18: Impact of new pre-primary facilities on the probability of formal employment. JW-DID



Note: The figure presents the results of the estimate of the effect on the probability of formal employment using Wooldridge (2023, 2025). Each point of the figure represents an estimation for 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 clustered by department of residence.

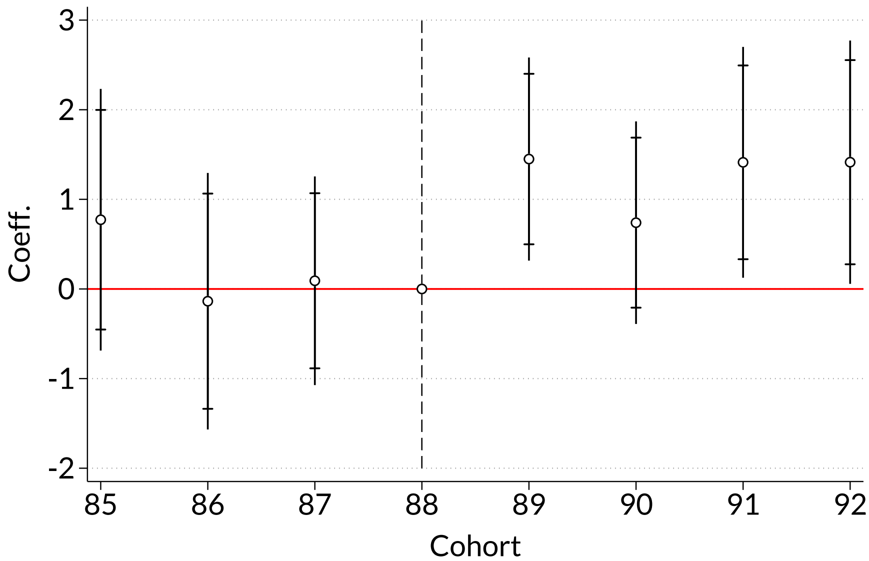
D.3 Estimation using the approach of Sun and Abraham (2021) (SA-DID)

Figure D19: Impact of new pre-primary facilities on the years of basic school (Post-K). SA-DID



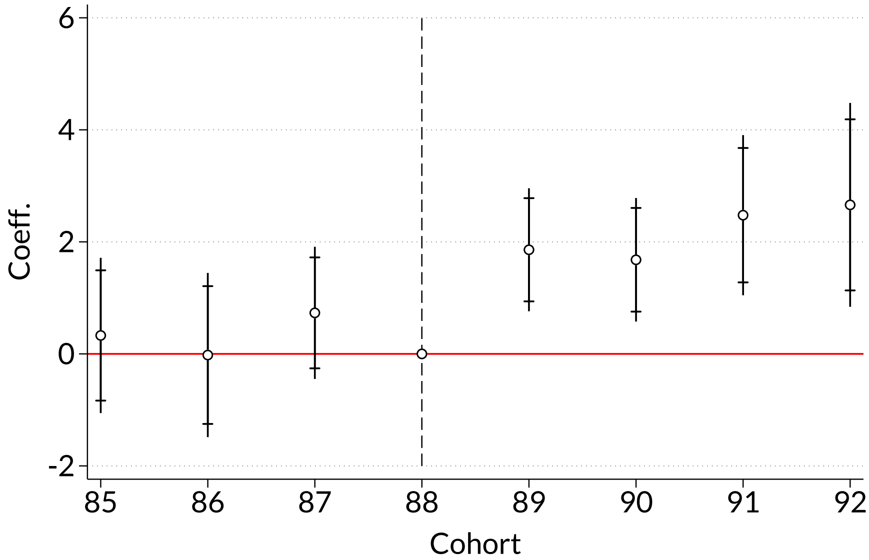
Note: The figure presents the results of the estimate of the effect on the years of basic school using Sun and Abraham (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number of places 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 clustered by department of residence.

Figure D20: Impact of new pre-primary facilities on the probability of complete lower secondary. SA-DID



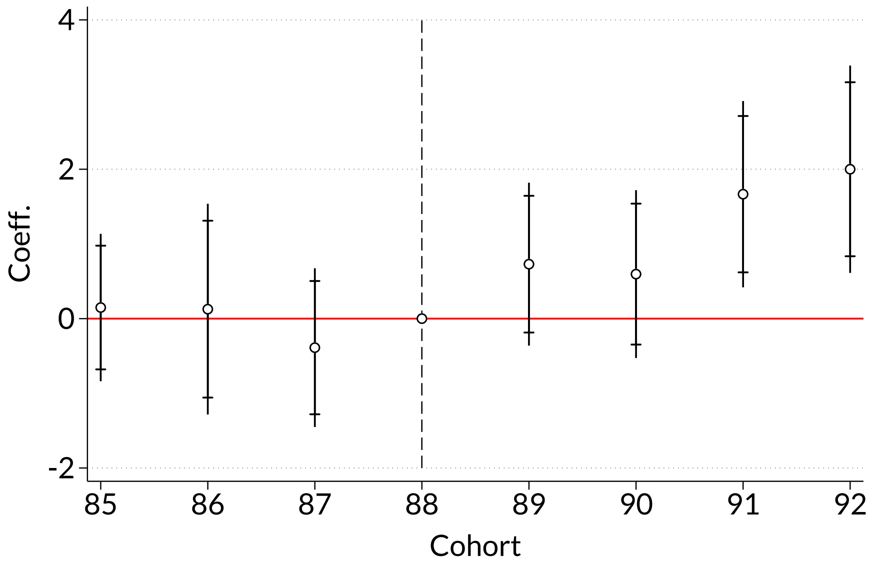
Note: The figure presents the results of the estimate of the effect on the probability of complete lower secondary using Sun and Abraham (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median 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 clustered by department of residence.

Figure D21: Impact of new pre-primary facilities on the probability of complete secondary. SA-DID



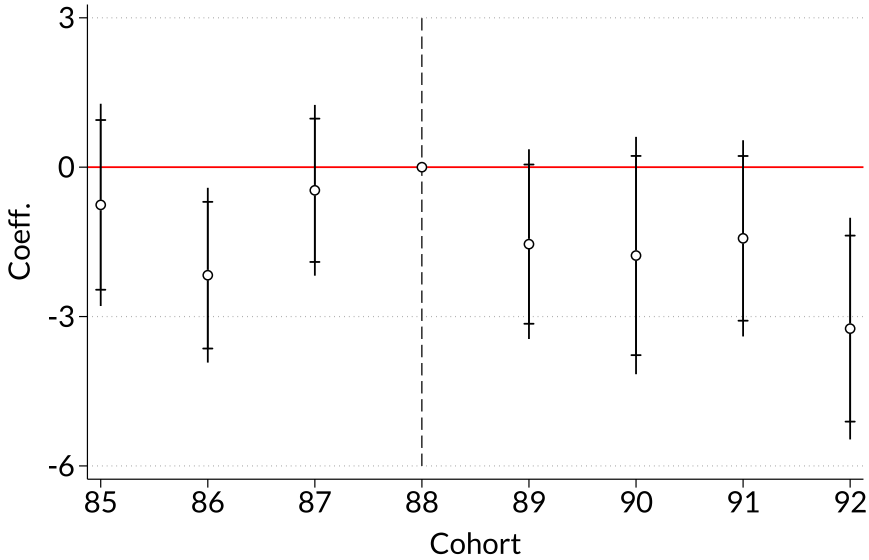
Note: The figure presents the results of the estimate of the effect on the probability of complete secondary using Sun and Abraham (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total 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 clustered by department of residence.

Figure D22: Impact of new pre-primary facilities on the probability of having post-secondary education. SA-DID



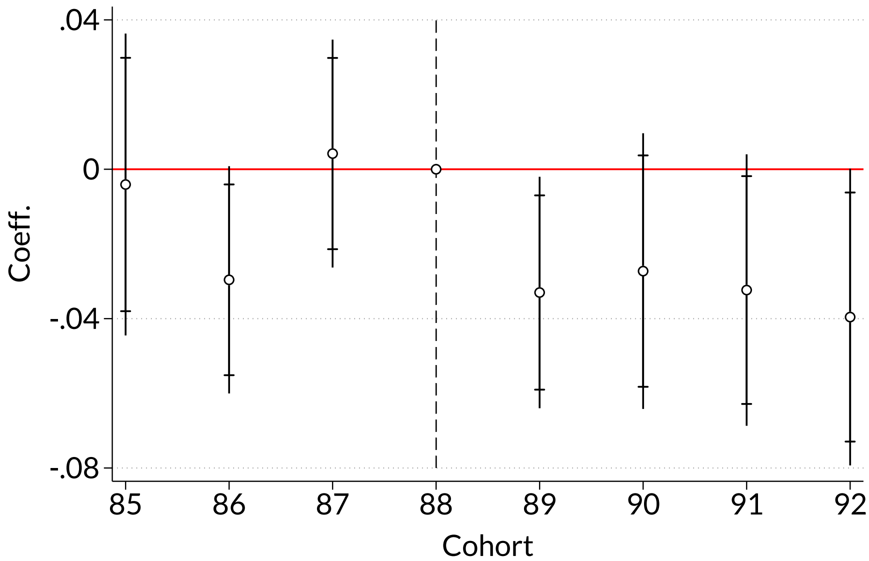
Note: The figure presents the results of the estimate of the effect on the probability of having post-secondary education using Sun and Abraham (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median 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 clustered by department of residence.

Figure D23: Impact of new pre-primary facilities on the probability of being mother (only women). SA-DID



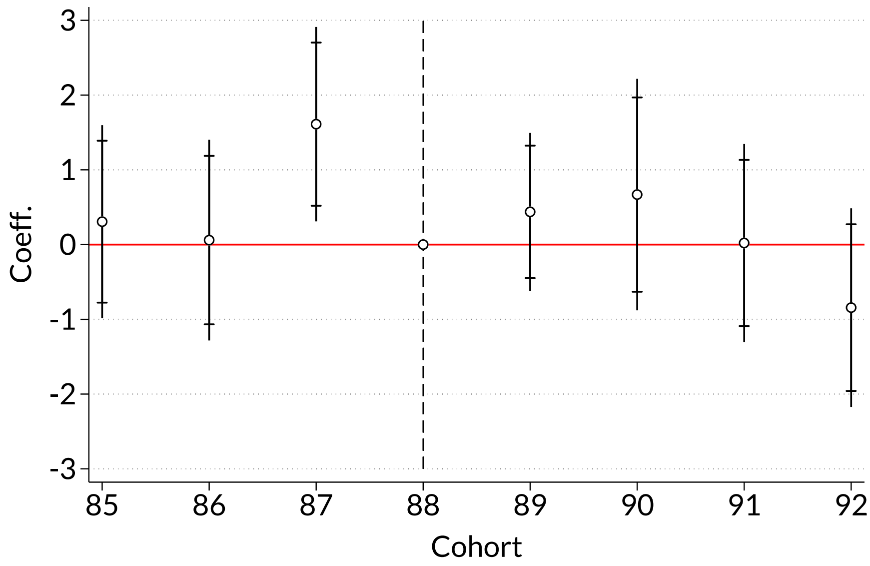
Note: The figure presents the results of the estimate of the effect on the probability of being mother using Sun and Abraham (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number 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 clustered by department of residence.

Figure D24: Impact of new pre-primary facilities on the total of live births (only women). SA-DID



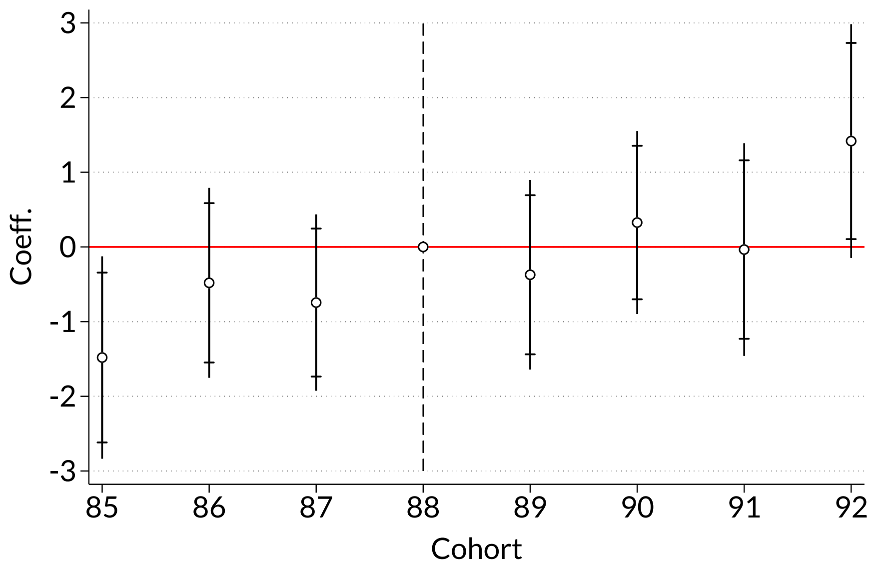
Note: The figure presents the results of the estimate of the effect on the total of live births per woman using Sun and Abraham (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total number 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 clustered by department of residence.

Figure D25: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). SA-DID



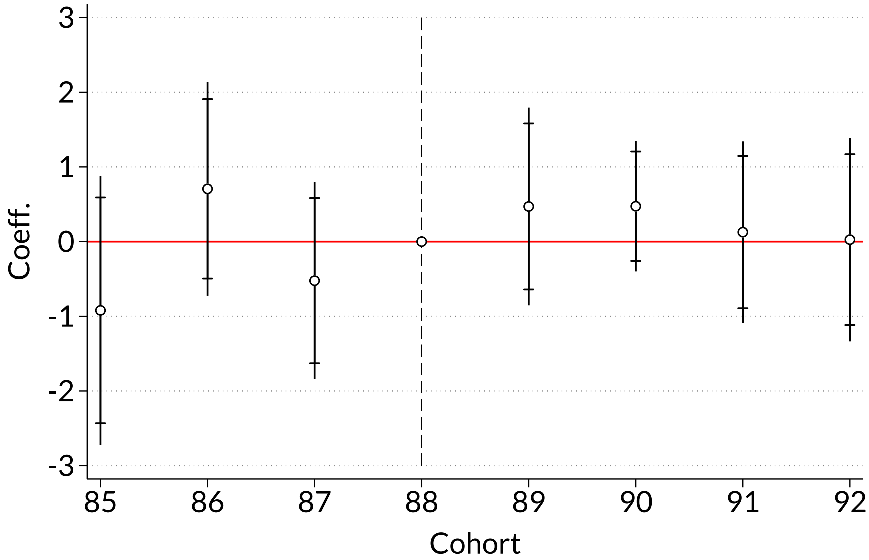
Note: The figure presents the results of the estimate of the effect on the probability of teenage motherhood using Sun and Abraham (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total 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 clustered by department of residence.

Figure D26: 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 of the figure represents an estimation for 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 clustered by department of residence.

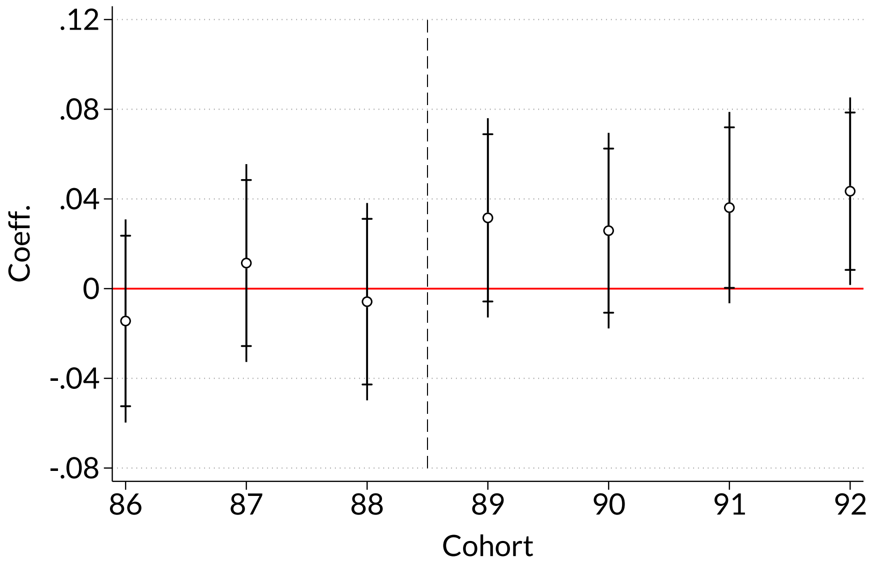
Figure D27: Impact of new pre-primary facilities on the probability of formal employment.SA-DID



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 of the figure represents an estimation for 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 clustered by department of residence.

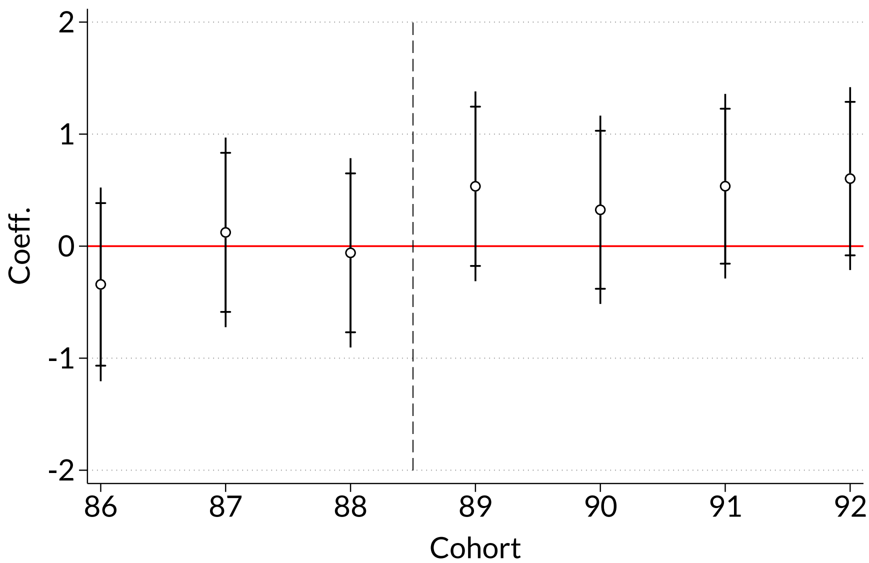
D.4 Estimation using the approach of Callaway and Sant'Anna (2021) (CS-DID)

Figure D28: Impact of new pre-primary facilities on the years of basic school (Post-K). CS-DID



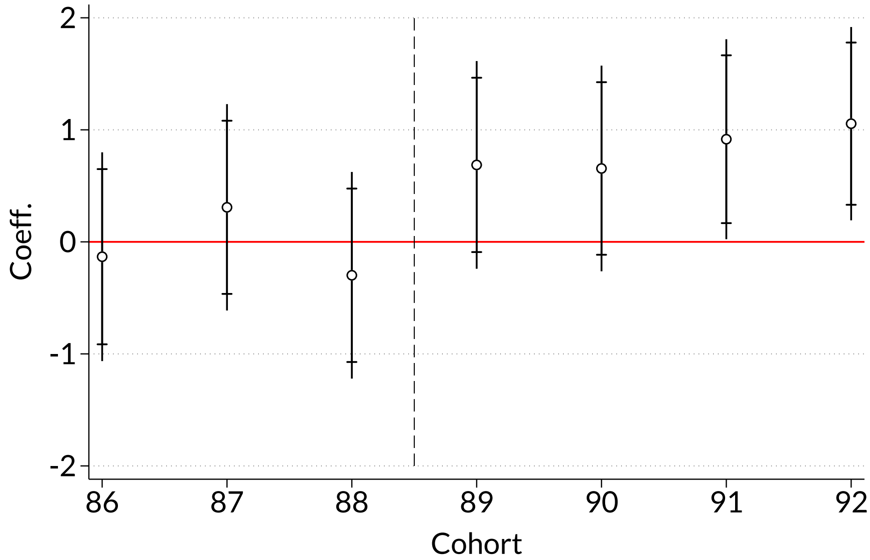
Note: The figure presents the results of the estimate of the effect on the years of basic school using Callaway and Sant'Anna (2021). Each point of the figure represents an estimation for β_c, c = 1985, ..., 1992. We consider treated departments to be those that received an above-median total number 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 clustered by department of residence.

Figure D29: Impact of new pre-primary facilities on the probability of complete lower secondary. CS-DID



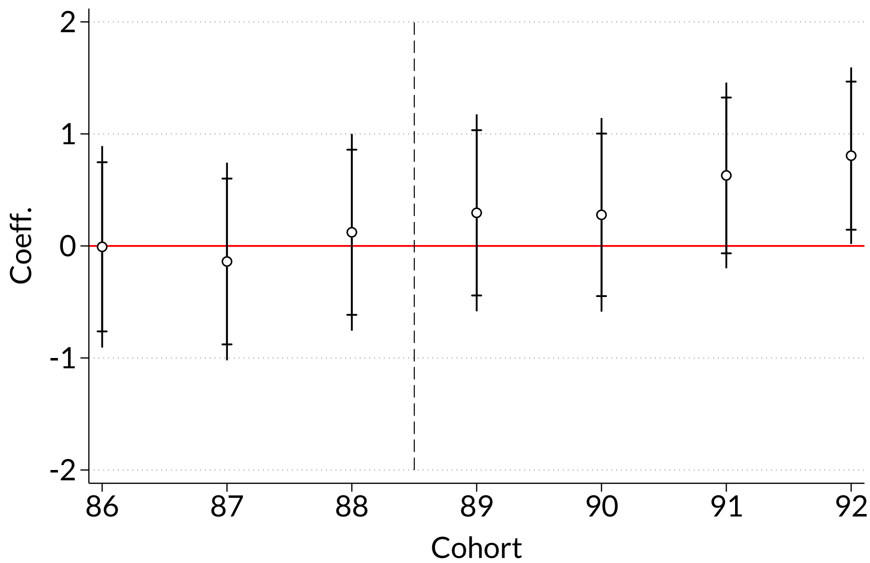
Note: The figure presents the results of the estimate of the effect on the probability of complete lower secondary using Callaway and Sant'Anna (2021). Each point of the figure represents an estimation for β_c, c = 1985, ..., 1992. We consider treated departments to be those that received an above-median 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 clustered by department of residence.

Figure D30: Impact of new pre-primary facilities on the probability of complete secondary. CS-DID



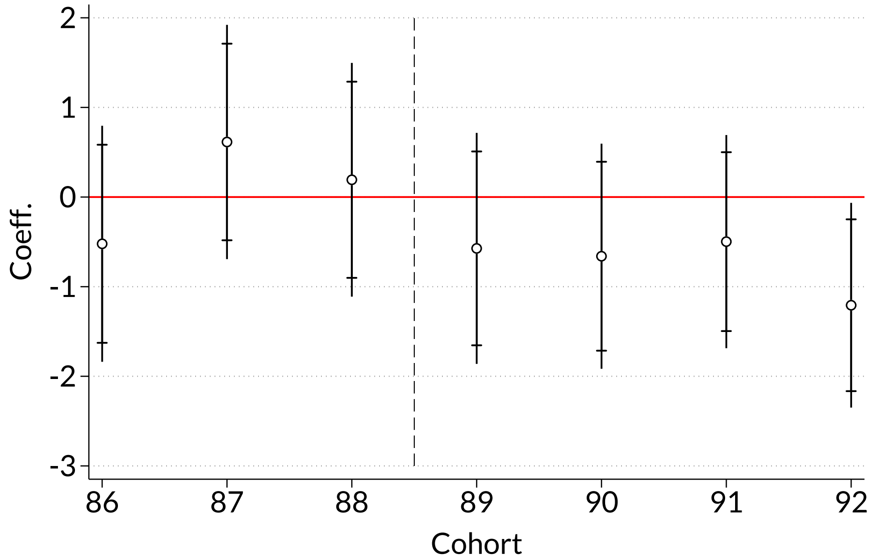
Note: The figure presents the results of the estimate of the effect on the probability of complete secondary using Callaway and Sant'Anna (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median 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 clustered by department of residence.

Figure D31: Impact of new pre-primary facilities on the probability of having post-secondary education. CS-DID



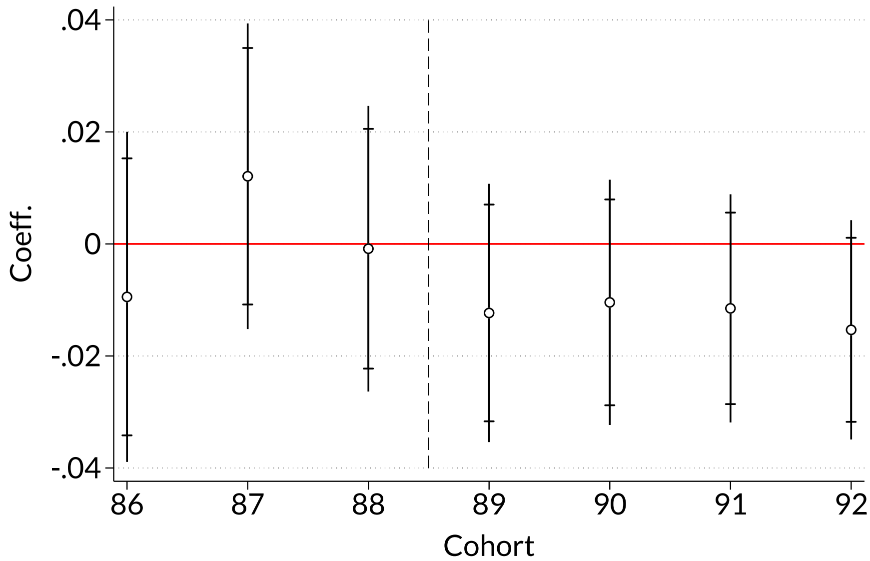
Note: The figure presents the results of the estimate of the effect on the probability of having post-secondary education using Callaway and Sant'Anna (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an 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 clustered by department of residence.

Figure D32: Impact of new pre-primary facilities on the probability of being mother (only women). CS-DID



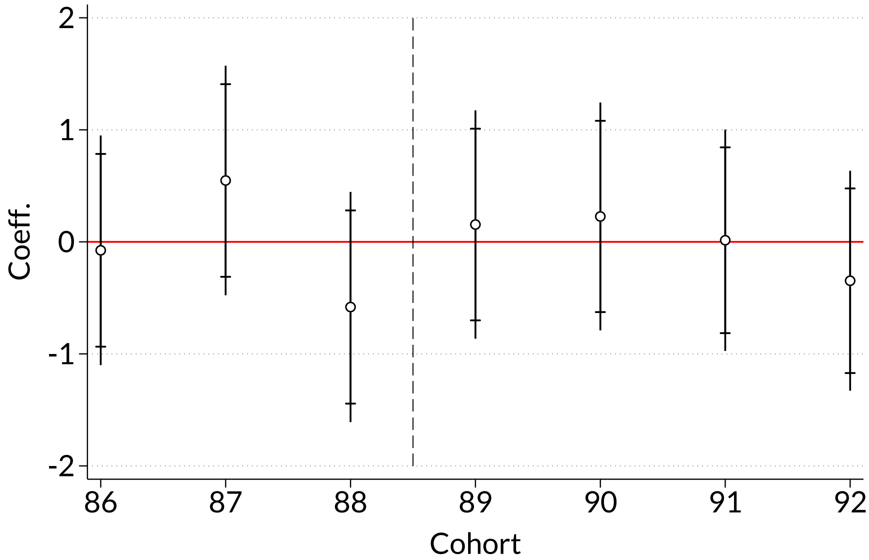
Note: The figure presents the results of the estimate of the effect on the probability of being mother using Callaway and Sant'Anna (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total 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 clustered by department of residence.

Figure D33: Impact of new pre-primary facilities on the total of live births (only women). CS-DID



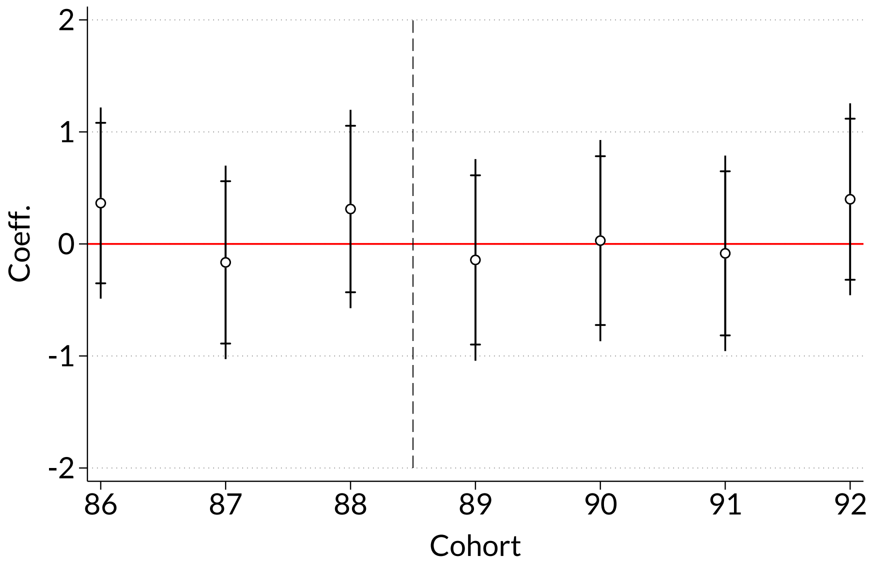
Note: The figure presents the results of the estimate of the effect on the total of live births per woman using Callaway and Sant'Anna (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median total 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 clustered by department of residence.

Figure D34: Impact of new pre-primary facilities on the probability of teenage motherhood (only women). CS-DID



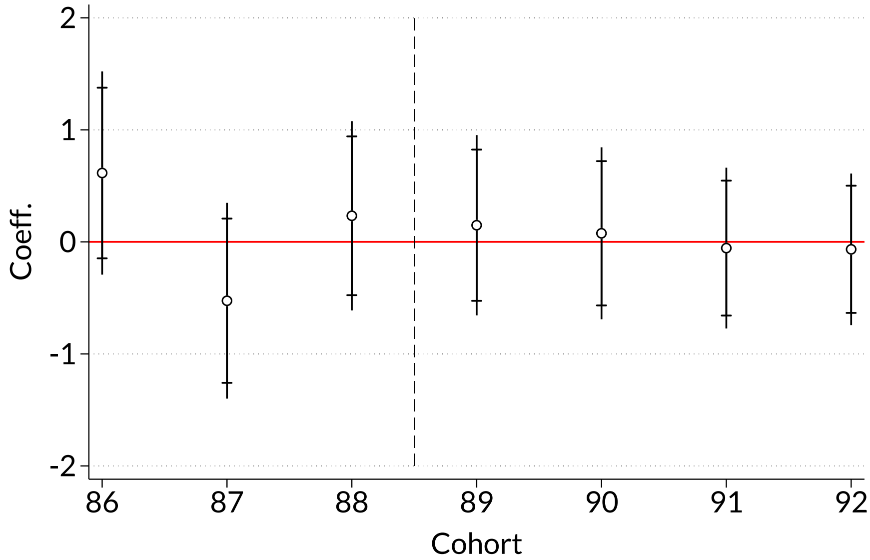
Note: The figure presents the results of the estimate of the effect on the probability of teenage motherhood using Callaway and Sant'Anna (2021). Each point of the figure represents an estimation for β_c, $c = 1985, ..., 1992$. We consider treated departments to be those that received an above-median 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 clustered by department of residence.

Figure D35: 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 of the figure represents an estimation for 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 clustered by department of residence.

Figure D36: 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 of the figure represents an estimation for 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 clustered by department of residence.

E Alternative results and robustness checks: Assessing the parallel trends assumption (Rambachan & Roth, 2023)

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:

$$\Delta^{SD}(M) := \{\delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq M, \forall t\} \tag{E1}$$

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

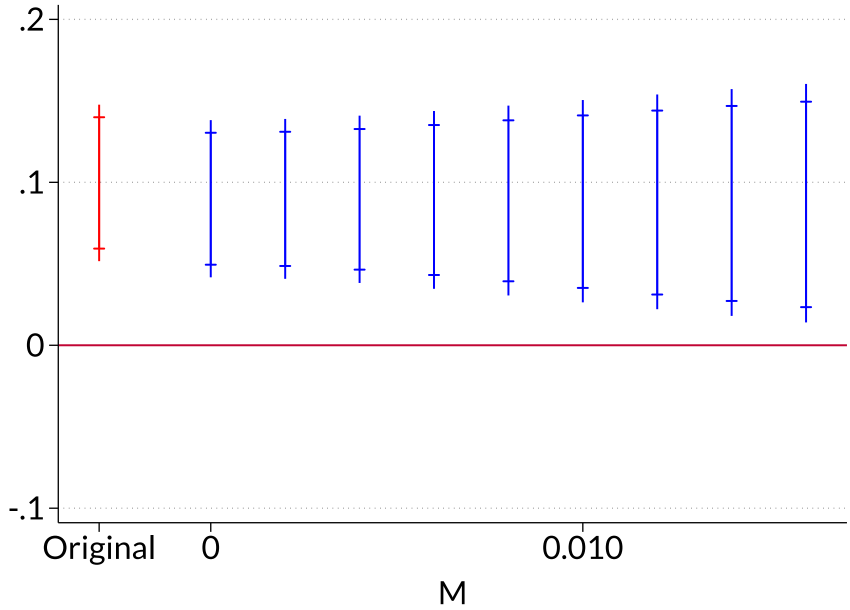
We estimate the following equation, which is a modification of our specifications:

$$Y_{icpj} = \alpha + \sum_{c \leq 1987} \beta_c \left[d_c \times D_j\right] + \beta_{post} \left[post_c \times D_j\right] + \mu_j + \mu_c + \varepsilon_{icpj} \tag{E2}$$

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}.

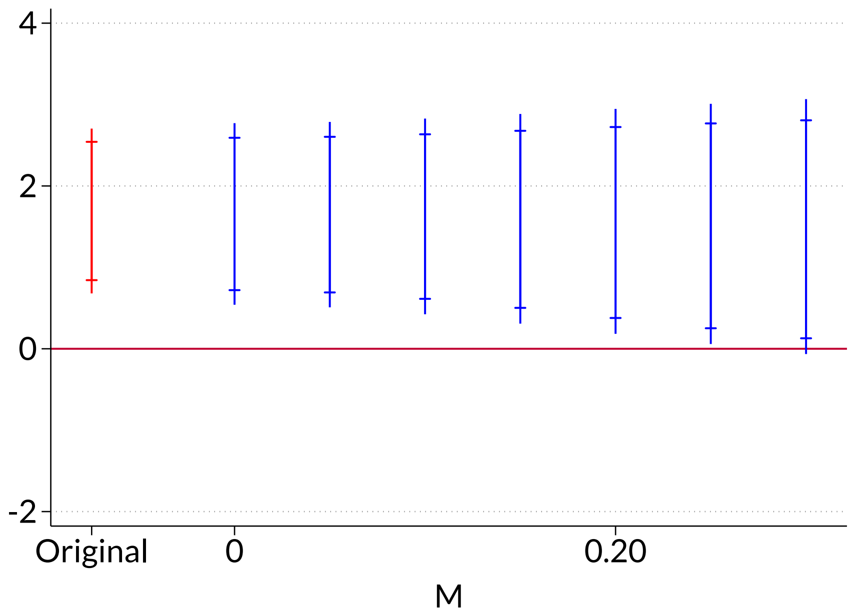
M in equation (E1) governs the maximum amount by which the slope of δ can change between consecutive periods. In the special case where $M = 0$, the assumption is that the difference in trends is exactly linear. We perform a sensitivity analysis with different values of M. Unfortunately, nothing in the 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 E1: Years of basic school (post-K)



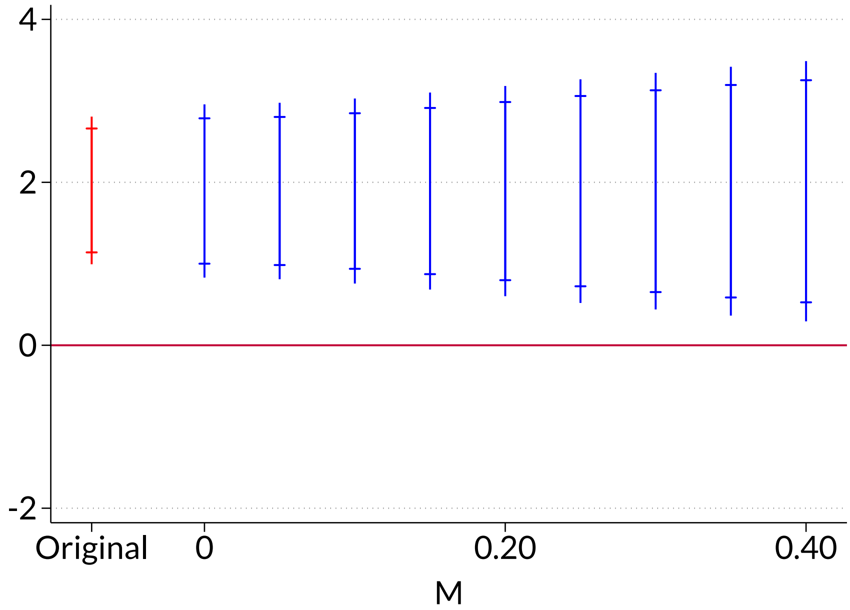
Note: We implement the Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bounds on change in the slope.

Figure E2: Lower secondary completed



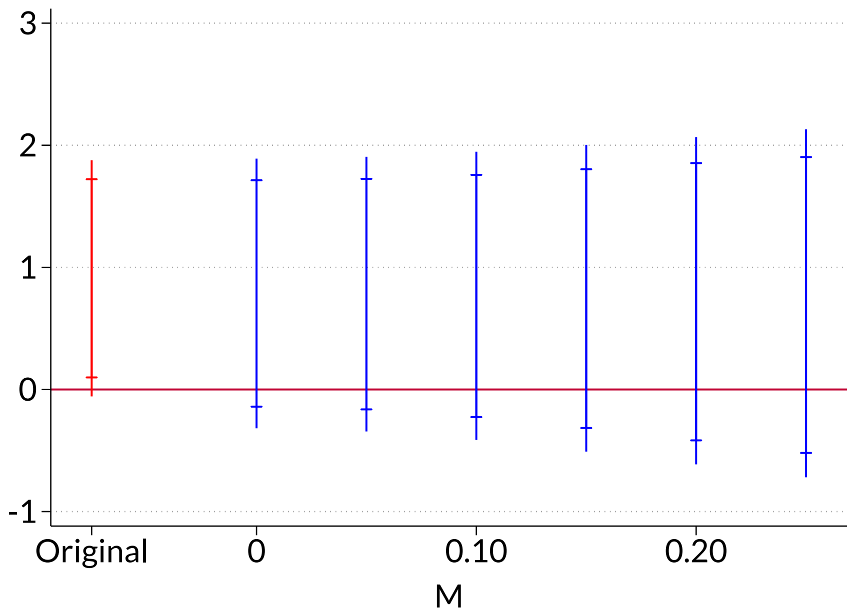
Note: We implement the Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bounds on change in the slope.

Figure E3: Secondary completed



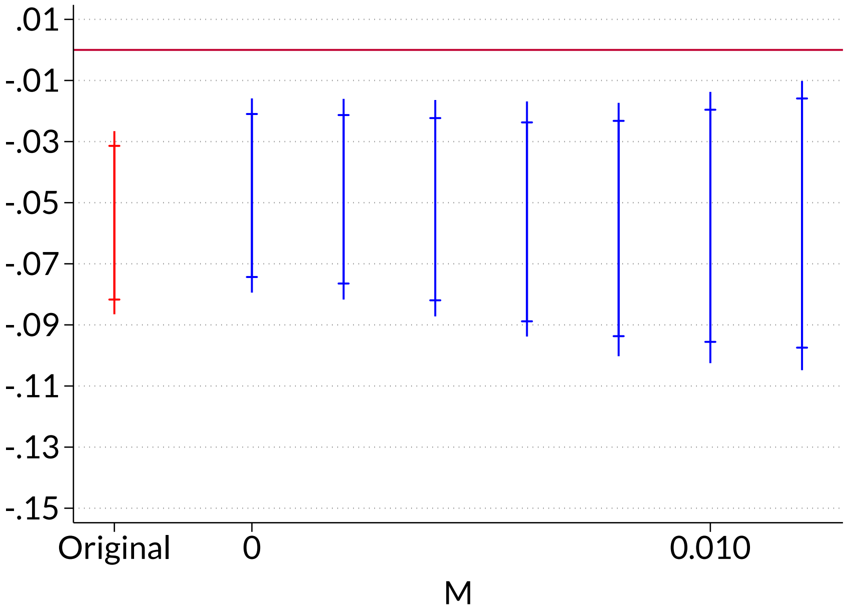
Note: We implement the Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bounds on change in the slope

Figure E4: Post-secondary education



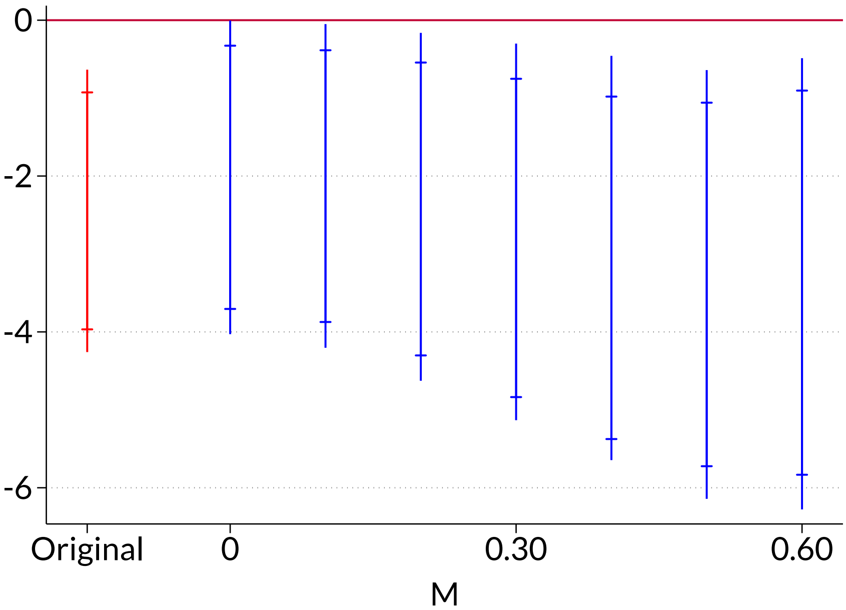
Note: We implement the Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bounds on change in the slope.

Figure E5: Total live births



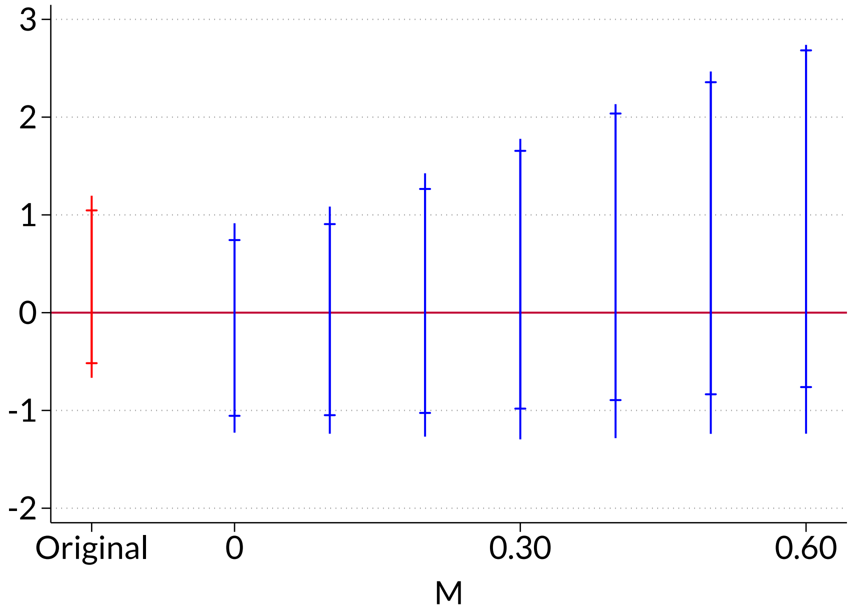
Note: We implement the Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bounds on change in the slope.

Figure E6: Being mother



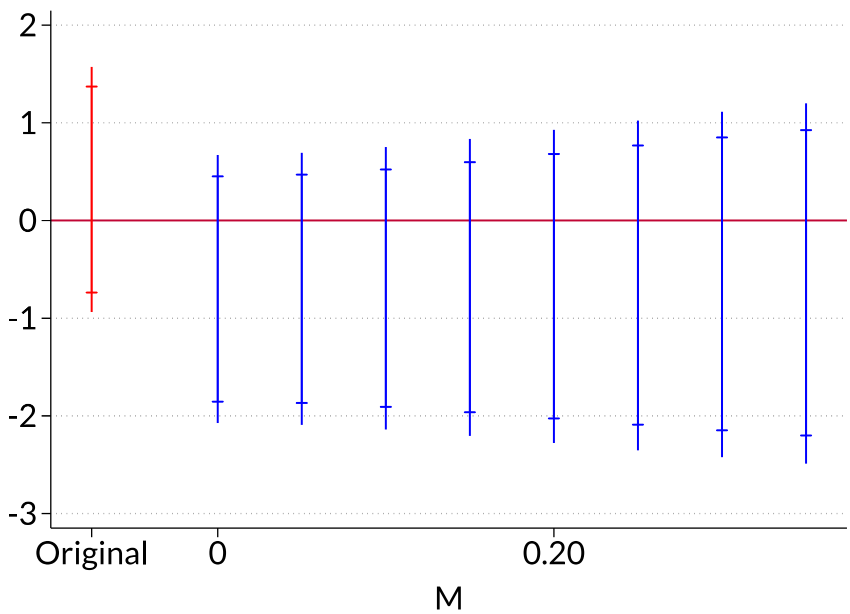
Note: We implement the Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bounds on change in the slope.

Figure E7: Teenage mother



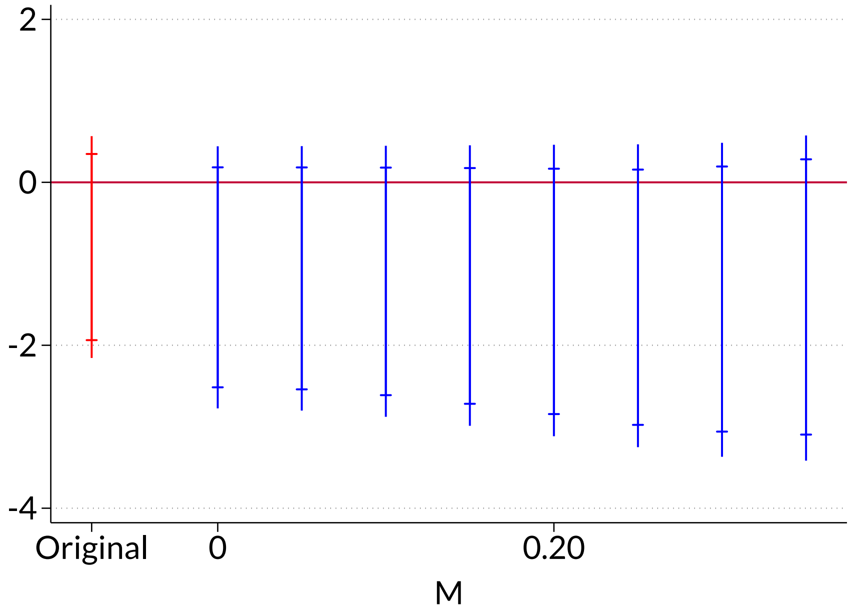
Note: We implement the Rambachan and Roth (2023) and calculate the confidence set for various values of M, where M is the bounds on change in the slope.

Figure E8: Employment



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

Figure E9: formal employment



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

F Addressing concerns on the potential impact of selective migration on the identification strategy

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.

First, parents could have moved to departments where pre-primary was expanding to benefit from the new pre-primary places. The characteristics of these parents could also be systematically correlated with their children's long-term outcomes. Likewise, the public pre-primary expansion program may have caused migration from the private to the public school system. In both cases, the concern is that our results may reflect changes in the relative composition of students rather than the impact of attending pre-primary. To test these hypotheses, we examine whether the public pre-primary expansion affected either the distribution of students enrolled in each department within a given province, or, alternatively, the distribution of students between public and private primary schools within a department. For this additional check, we rely on administrative data from the National Education Ministry on public and private primary-school enrollment by department and primary grade (i.e., not on Census data). This information is available for 1994 and yearly for 1996-2002. We use year and grade to define the school cohort. We estimate the compact specification (1) to assess the effect on the departmental share of students in primary school, and the share of students in public primary schools. The results for this robustness checks, presented in Table F1 in Appendix F, are not significant at conventional levels. Accordingly, this additional evidence indicates that is its unlikely that the pre-primary expansion program resulted in substantial geographical mobility of students between departments in a given province, or from private to public schools within a department.

We also analyze the impact of the new pre-primary facilities on departmental cohort sizes. First, we estimate the effect on the ratios between cohort sizes for two different years to evaluate whether the policy was correlated with changes in departmental population structure over time. In the first and second columns of Table F2 of Appendix F, we present the estimation for the years 2010-1991 and 2001-1991, respectively, and we do not find significant results. In the third column, we assess whether the departmental cohort size in 2010 is correlated with the policy, and again we do not find significant results.

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 F for educational attainment and fertility in Tables F4 and F5, respectively. These results are broadly in line with those of our main specifications.

Finally, selective migration might have altered the composition of households or their characteristics, which would bias our estimates if these changes in parent or household characteristics were correlated with pre-primary construction. To address this concern, we assess whether changes in the average characteristics of heads of households in our sample between 1991 and 2001 are correlated with the new pre-primary facilities. These results are presented in Appendix F in Table F3. In the first column, we present the estimate of the impact of the construction program on the ratio of the 2001 departmental average head of household years of schooling to the same departmental average in 1991. 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. In the second column of Table F3, we present the estimate 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

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

[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.

expected to change if selective migration interfered with our identification strategy.

Table F1: The impact of construction program on the departmental share of students in each province and the share of public primary school students per department

	Departamental share of students in primary school	Share of students in public primary schools
Post	0.013	0.073
	(0.019)	(0.129)
Pre	-0.016	-0.056
	(0.027)	(0.140)
R^2	1.00	0.97
N	15,056	15,056
Controls and Fixed effects		
Cohort FE	Yes	Yes
Department FE	Yes	Yes
Cohort $\times$ Province FE	Yes	Yes
Year $\times$ Province FE	Yes	Yes
Cohort $\times$ Enr. 80	Yes	Yes

Note: this table presents an estimation of the equation (1). The post 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 error clustered at department of residence in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table F2: The impact of construction program on relative changes in size of cohorts per department in years 2010-1991 and 2001-1991 and 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. $\times$ Province	Yes	Yes	Yes
Coh. $\times$ Enr. 80	Yes	Yes	Yes

Note: this table presents an estimation of the equation (1). 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 error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table F3: The impact of construction program on changes on household head's education and household UBN between 2001-1991

	Head's education	UBN household	
	Years school Post-K	Some UBN	UBN
ITT	-0.004	0.033	0.010
	(0.013)	(0.041)	(0.044)
	[0.734]	[0.414]	[0.816]
Pre coef.	0.016	0.017	0.017
	(0.013)	(0.042)	(0.045)
	[0.224]	[0.693]	[0.705]
N	3,652	3,644	3,644
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 the equation (1). 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 error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table F4: Educational attainment without migrants

	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 estimate (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the probability of treatment (i.e. the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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. We exclude people that migrated from their province of birth. Standard error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table F5: Fertility, without migrants

	Being a mother (pp.)	Total live births	Teen mother (pp.)
ATT	-7.872*	-0.132*	1.874
ITT	-1.550*	-0.026*	0.369
	(0.901)	(0.015)	(0.620)
	[0.086]	[0.088]	[0.552]
Pre coef.	-0.582	-0.002	1.189*
	(0.761)	(0.012)	(0.648)
	[0.445]	[0.868]	[0.067]
Mean pre-treat	63.969	1.125	27.131
N	888,937	887,915	870,386
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 estimate (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 probability of treatment (i.e. the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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. We exclude people that migrated from their province of birth. Standard error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

G Replication Berlinski *et al.* (2009)

The program we analyzed in this paper promoted the development of skills in the treated children in primary school, as show Berlinski *et al.* (2009). It is plausible that these new skills explain our main results, aligning with the literature on the long-term effects of early education and care interventions (see Cunha and Heckman, 2007 and the works cited therein). However, we use a different treatment measure of that previous evidence, that could undermine our interpretation. In this section, we answer this concern re-estimating the results of Berlinski *et al.* (2009) with our measure.

Berlinski *et al.* (2009) estimate the impact of new pre-primary facilities in test score of Mathematics and Spanish in primary. They use data on student performance in the third grade, that consist on standardized achievement test administered by Argentine National Education Ministry. The sample are students of untreated cohorts 1986, 1987 and 1998 and of treated cohort 1989,1990 and 1991, when they were in the third grade.

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

$$Y_{icpjt} = \mu_j + \mu_c + \gamma_{cp} + \lambda_{tp} + \beta_{post}\left[I(\text{cohort} \geq 1989) \times D_j\right] + \beta_{pre}\left[I(\text{cohort} \leq 1987) \times D_j\right] + \varepsilon_{icpj} \tag{G1}$$

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. $I(\text{cohort} \geq 1989)$ indicate 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.

In Table G1, we present our estimates. We report the impact on mathematics test score in column (1), and we find an average impact of 2.06 points. In column (2) we make the same regression but with interaction cohort and pre-treatment pre-primary enrollment (in 1991), and we find similar results. We report the average impact on spanish test score in column (3), that is 1.212 points. This effect is robust to control for pre-treatment pre-primary enrollment, and we find similar results.

Table G1: The impact of construction program on standardized achievement test-scores in the third grade

	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 $\times$ province effects	Yes	Yes	Yes	Yes
Year $\times$ province effects	Yes	Yes	Yes	Yes
Cohort $\times$ Enroll. 91	No	Yes	No	Yes

Note: this table presents a replication of Table 5 results of Berlinski *et al.* (2009), with the equation (1). The post 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 error clustered at department of residence and treatment/controls status in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

H Using the Berlinski *et al.* (2009) approach

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 2009 paper consists of a difference-in-differences design in which each cohort is assigned the number of classrooms that correspond to them. The calculation of this measure of treatment exposure is given by the following equation:

$$\text{Stock}_{cj} = \frac{0.5 \times \text{Stock}_{5,cj} + 0.36 \times \text{Stock}_{4,cj} + 0.14 \times \text{Stock}_{3,cj}}{\text{Cohort Size}_{cj}} \tag{H1}$$

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:

$$Y_{icjp} = \mu_j + \mu_c + \beta Stocks_{cj} + \varepsilon_{icjp} \tag{H2}$$

Due to the way $Stocks_{cj}$ is calculated, 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 interpret interpret directly with our main results. These 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.

We present the results of re-estimate our main results with Berlinski *et al.* (2009) approach and the comparable estimate with our methodology in tables H1, H2 and H3. We do not find significant differences.

Table H1: Education Outcomes. ATT per place-year and ATT 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. × Province	Yes	Yes	Yes	Yes
Coh. × 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 access to some higher 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, and cohort-by-pre-primary gross enrollment in 1980 trends. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table H2: Labor Market Outcomes. ATT per place-year and ATT Berlinski *et al.* (2009)

	Employment (pp.)	Formal employment (pp.)
ATT per place-year	0.593	0.496
	(1.588)	(1.354)
	[0.709]	[0.714]
ATT Berlinski *et al.* (2009)	1.028	0.465
	(0.859)	(0.845)
	[0.232]	[0.582]
p-val difference	0.763	0.981
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, and cohort-by-pre-primary gross enrollment in 1980 trends.$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table H3: Fertility Outcomes. ATT per place-year and ATT Berlinski *et al.* (2009)

	Being a mother (pp.)	Total live births	Teen mother (pp.)
ATT per place-year	-5.356**	-0.088**	0.097
	(2.551)	(0.044)	(1.354)
	[0.036]	[0.046]	[0.943]
ATT Berlinski *et al.* (2009)	-2.776**	-0.079***	-2.050**
	(1.270)	(0.026)	(0.801)
	[0.029]	[0.003]	[0.010]
p-val difference	0.277	0.811	0.099
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 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, and cohort-by-pre-primary gross enrollment in 1980 trends. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

I 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:

$$Y_{ibpj} = \mu_j + \mu_b + \beta \left[Post_b \times D_j \right] + \gamma_{bp} + Enroll80_{bj} + \varepsilon_{ibpj} \tag{I1}$$

where Y_{icpj} is an outcome of interest for individual i, born on c, 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 born on 1st July 1988 or after. Finally, ε_{ibpj} is an error term. We cluster standard errors at the level of department of residence.

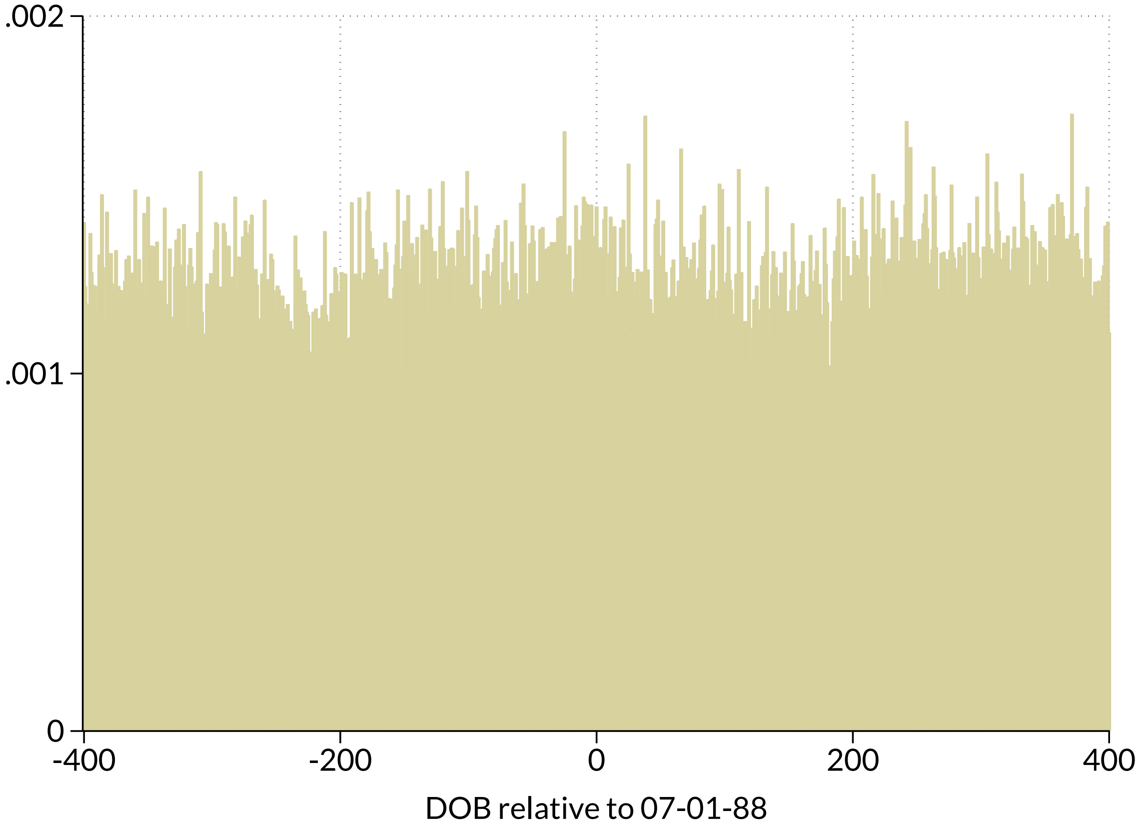
We estimate equation (I1) using three different bandwidths (before and after the birthdate cut-off): 75 days, 100 days, and 125 days. The idea behind this strategy is similar to that of Grembi *et al.* (2016), who use a "difference-in-discontinuities" approach. In our case, we exploit the discontinuity 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 I1. 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 I1, I2, and I3, 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. Although most coefficients are not statistically significant, they all show the expected sign.

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 I4, I5, and I6. In this case, we find only one significant effect—on the probability of formal employment.

Figure I1: 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 I1: Estimates around the cut-off: Education outcomes

	Years basic school	Lower secondary (pp.)	Secondary (pp.)	Post secondary (pp.)
h=75	0.143**	2.058*	2.541**	1.459
	(0.061)	(1.095)	(1.215)	(1.212)
	[0.019]	[0.060]	[0.037]	[0.229]
h=100	0.150***	2.275**	3.605***	1.044
	(0.052)	(1.000)	(1.151)	(0.945)
	[0.004]	[0.023]	[0.002]	[0.269]
h=125	0.117***	1.602*	2.994***	0.800
	(0.045)	(0.923)	(0.993)	(0.809)
	[0.010]	[0.083]	[0.003]	[0.323]
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 Intention-to-Treat (ITT) estimates for educational outcomes using equation (I1) from Section I. 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 access to some higher 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 I2: Estimates around the cut-off: Labor market outcomes

	Employment (pp.)	Formal employment (pp.)
h=75	0.480	0.446
	(1.043)	(1.208)
	[0.645]	[0.712]
h=100	0.170	-0.515
	(0.947)	(1.142)
	[0.858]	[0.652]
h=125	0.216	-0.351
	(0.891)	(1.073)
	[0.809]	[0.744]
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 (I1) from Section I. 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 I3: Estimates around the cut-off: Fertility outcomes

	Being a mother (pp.)	Total live births	Teen mother (pp.)
h=75	-1.280	-0.060*	1.341
	(1.829)	(0.034)	(1.567)
	[0.484]	[0.079]	[0.392]
h=100	-0.612	-0.045	0.718
	(1.533)	(0.030)	(1.441)
	[0.690]	[0.132]	[0.618]
h=125	-0.923	-0.043*	0.892
	(1.270)	(0.025)	(1.179)
	[0.467]	[0.080]	[0.449]
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 (I1) from Section I. 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 I4: Estimates around the cut-off: Education outcomes. Placebo

	Years basic school	Lower secondary (pp.)	Secondary (pp.)	Post secondary (pp.)
h=75	0.021	1.436	1.332	0.402
	(0.074)	(1.530)	(1.638)	(1.890)
	[0.780]	[0.348]	[0.416]	[0.831]
h=100	0.041	1.382	1.748	0.206
	(0.068)	(1.337)	(1.570)	(1.740)
	[0.547]	[0.301]	[0.265]	[0.906]
h=125	0.005	0.496	0.964	-0.186
	(0.063)	(1.220)	(1.357)	(1.490)
	[0.938]	[0.684]	[0.478]	[0.900]
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: The table presents the results of estimate (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the probability of treatment (i.e. the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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 error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table I5: Estimates around the cut-off: Labor market outcomes. Placebo

	Employment (pp.)	Formal employment (pp.)
h=75	0.898	2.263*
	(1.145)	(1.276)
	[0.433]	[0.076]
h=100	0.519	-0.042
	(1.010)	(1.176)
	[0.607]	[0.972]
h=125	0.132	-0.197
	(0.920)	(1.108)
	[0.886]	[0.859]
Controls and FE		
Department FE	Yes	Yes
DOB FE	Yes	Yes
DOB × Province	Yes	Yes
DOB × Enr. 80	Yes	Yes

Note: The table presents the results of estimate (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the probability of treatment (i.e. the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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 error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table I6: Estimates around the cut-off: Fertility outcomes. Placebo

	Being a mother (pp.)	Total live births	Teen mother (pp.)
h=75	0.659	-0.019	-1.184
	(1.694)	(0.033)	(1.287)
	[0.697]	[0.556]	[0.358]
h=100	1.502	-0.003	0.108
	(1.492)	(0.030)	(1.136)
	[0.314]	[0.916]	[0.924]
h=125	1.316	0.003	1.114
	(1.389)	(0.029)	(1.081)
	[0.343]	[0.905]	[0.303]
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: The table presents the results of estimate (1) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the probability of treatment (i.e. the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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 error clustered at department of residence in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

J Additional results on fertility based on administrative birth records

We have access to administrative records of live births of Argentine Health Ministry. In particularly, we have data about the total live births per department, calendar year and year of birth cohort of the mother, for years 2005 to 2018. We constructed panels of departments and year of birth cohorts adding the live births in the following manner:

$$\text{Tot births pw}_{cjT} = \frac{\sum_{t=2005}^{T} \text{Births}_{cjt}}{\text{Women}_{2010cj}} \tag{J1}$$

where $births_{cjt}$ is the live births of the woman of year-birth cohort c in the department j at the calendar year t, and Women_{2010} is the total of women in 2010 of the cohort c in the department j. In other words, we calculated for each department-cohort the total live births per woman since 2005 until T. We make this calculation for $T = 2010, 2015, 2018$.

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

$$\text{Tot births pw}_{cjT} = \mu_j + \mu_c + \gamma_{cp} + \beta_{post}\left[I(c \geq 1989) \times D_j\right] + \beta_{pre}\left[I(c \leq 1987) \times D_j\right] + \varepsilon_{cpj} \tag{J2}$$

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. D_j is the dummy variable indicating that department j received a high intensity of treatment. $I(\text{cohort} \geq 1989)$ indicate 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 departments. We weight our estimates by the total number of women.

We present the results of estimate J2 in table J1. We can see that the results are similar to (2).

The approach of this section is an imperfect proxy of our main results. In benchmark estimations we have data of the total live births for each woman, while in this Appendix we use aggregated data of a department-cohort panel. Furthermore, assuming that women do not move among departments, administrative records allows us to calculate live births only for the window 2005-2018.

Table J1: 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 the results of estimate (J2) for different outcomes. The ITT is the coefficient β_{post}. ATT is the ITT re-scaled by the probability of treatment (i.e. the intensity of treatment in treated groups related with the control group). Pre coef. is the estimation 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 error clustered at department in parentheses. p-values in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.