Building opportunity: The long-run and intergenerational effects of Chilean school construction*

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Abstract: In 1965–1966, Chile built and staffed thousands of new primary classrooms in supply-constrained communities. Using an event study design and large census samples, we show that childhood exposure to school construction substantially improved the schooling and labor market outcomes of adults and closed a persistent female disadvantage in school attainment. Women's exposure to the policy had large intergenerational spillovers on their children's on-time grade progression and completed schooling. The marginal value of public funds is 16, including direct effects on adults and intergenerational spillovers.

JEL codes: H52, I24, I26, I28, J16

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> I am convinced that there is no problem that distresses mothers and fathers more than education. In remote rural areas and marginalized urban ones, when asked what their first aspiration was—among many serious problems that afflict these households—the overwhelming response was always: "education for our children." My commitment was that in the coming year there would be no child without enrollment in Chile. At this moment, I am taking the first steps to realize this aspiration and to ensure that by March 1965, no child who wants to study will be left without the opportunity to do so.
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> Address of President Eduardo Frei Montalva, November 18, 1964

1. Introduction

Parent schooling attainment is strongly associated with child schooling attainment, especially in developing regions like Latin America (Torche, 2021; Celhay and Gallegos, 2025; Fernández et al., 2025). Low educational mobility plausibly reinforces patterns of poverty and inequality, since schooling is a building block of household and country well-being (Psacharopoulos and Patrinos, 2018; Hanushek and Woessmann, 2020). More encouragingly, parent-child schooling associations have declined in Latin America, roughly coinciding with the childhood exposure of younger cohorts to public school expansions in the 1960s and beyond (Celhay and Gallegos, 2025).

Despite suggestive findings, there is scarce causal evidence from developing countries on the direct and intergenerational impacts of public school expansion. A key exception is Indonesia's school construction campaign in 1973–1978, which increased the schooling outcomes of directly-affected cohorts and their children (Duflo, 2001, 2004; Breierova and Duflo, 2004; Mazumder, Rosales-Rueda, and Triana, 2019, 2023; Akresh, Halim, and Kleemans, 2023). Given Latin America's high inequality and challenges in maintaining quality during mass expansion, studies from the region offer additional, generalizable lessons (Frankema, 2009; Hanushek and Woessmann, 2012).

This paper evaluates a 1965 Chilean reform—the Special Plan for Primary Instruction—that rapidly expanded public school infrastructure in grades 1–8, especially in rural and marginalized urban areas (Frei, 1964; Blakemore, 1976; Bellei and Pérez, 2016). In the presidential election of September 1964, Eduardo Frei Montalva defeated the leftist Salvador Allende, campaigning for a "Revolution in Liberty" that would dramatically increase education spending. Just after

inauguration, the government conducted a special enrollment census to identify communities with supply constraints and rapidly built new classrooms using prefabricated construction techniques and labor provided by the military and university students (SCEE, 1964, 1982; MEP, 1964; Chile Films, 1969; Schiefelbein, 1976). New classrooms were staffed with teachers who graduated from accelerated 32-week training courses (Bellei and Pérez, 2016).

We identify the long-run effects of the plan using temporal and geographic variation in exposure to school construction. For temporal variation, we compare the outcomes of never-exposed birth cohorts of adults (1940–1948) with those of increasingly-exposed cohorts (1949–1959). The 1949 cohort was potentially exposed to new classrooms for just one year, since it was at the top of the eligible age range for enrollment (MEP, 1964). Each subsequent cohort was exposed for an additional year. For geographic variation, we compare outcomes of adults born in Chilean departments with higher treatment intensity with those of adults born in less-treated departments (Schiefelbein, 1976).

We pool four census samples between 1982 and 2017 and estimate event study specifications that allow us to identify dynamic treatment effects on post-reform birth cohorts. The maximally-exposed 1959 cohort had 1.1 additional years of schooling if born in the most-treated versus least-treated department, with effects concentrated in upper-primary grades (5–8), the grades most targeted for expansion. We show that women had larger schooling effects than men, despite similar exposure within departments, which ultimately led to convergence in women's and men's attainment. As adults, women and men had increased labor supply, worked in higher-skill occupations, and partnered with more-educated adults.

We use the same research design to identify impacts on the co-resident children of adults. We show that women's exposure to the plan increased on-time grade progression among their younger children (ages 7–21) and completed schooling among older children (age 22 and older). In contrast, we find no evidence that men's exposure to school construction affected the schooling outcomes of their co-resident children, although imprecise estimates imply larger effects when mothers are also co-resident, consistent with assortative mating by education. The intergenerational spillovers are robust to controls for school reforms in the 1990s and 2000s that could have affected second-generation birth cohorts (Dominguez and Ruffini, 2023; Lucas, McEwan, and Torres Irribarra, 2025).

We estimate the marginal value of public funds (MVPF) for treated cohorts (Hendren and Sprung-Keyser, 2020). The first generation received $14 of after-tax earnings for each $1 of net government expenditures (including a fiscal externality from increased tax revenues). The MVPF is 16 if we include earnings and net expenditures for the second generation.

We contribute to three literatures in economics. First, we provide the first estimates of the long-run and intergenerational impact of primary school construction in Latin America.[1] Beyond the Indonesian literature cited above, the most closely-related papers analyze the longer-run effects of rural schools for African-Americans in the U.S. south (Aaronson and Mazumder, 2011), "girl-friendly" schools in Burkina Faso (Ingwersen et al., 2019), and four colonial schools in Benin (Wantchekon, Klasnjam, and Novta, 2015). In 1965, Chile's per capita income placed it within the World Bank's lower-middle-income category. Thus, our results also complement recent literatures on demand-side approaches to increasing schooling in poor settings, including conditional cash transfers (Garcia and Saavedra, 2022) and fee removal (e.g., Lucas and Mbiti, 2012).

Second, we add to the scarce evidence on intergenerational spillovers induced by parental education in developing-country settings and confirm the particular importance of women. In Indonesia, the childhood exposure of women—but not men—to school construction yielded intergenerational spillovers on children's test scores (Mazumder et al., 2019, 2023) and attainment (Akresh et al., 2023).[2] In Chile, exposure to a large-scale conditional cash transfer improved the school attainment of indigenous women born in the 1990s, as well as their children's test scores (Lucas, McEwan, and Torres Irribarra, 2025).[3] Finally, the association between *grandparent* and child schooling across Latin American cohorts has declined (Celhay

[1] There are related papers on the short-run effects of new primary schools in Afghanistan (Burde and Linden, 2013); national pre-primary expansion in Argentina (Berlinski, Galiani, and Gertler 2009; Berlinski et al., 2025); classroom construction to accommodate longer school days in Chile (Dominguez and Ruffini, 2023) and the Dominican Republic (Dinerstein, Neilson, and Otero, 2020); and a 1980s expansion of Chilean private schools, in the context of universal primary enrollments (Hsieh and Urquiola, 2006).

[2] Dupas et al. (2024) find that secondary school scholarships in Ghana increase the cognitive skills of children born to treated mothers (who also married more-educated partners). There are no effects among male scholarship recipients, who married less-educated partners. The elimination of racial restrictions on school attendance in Zimbabwe improved first-generation and second-generation schooling, regardless of gender (Agüero and Ramachandran, 2020).

[3] At the tertiary level, test-based admission to elite university programs strongly affected tertiary access of the second generation (Barrios-Fernández, Neilson, and Zimmerman, 2024).

and Gallegos 2025). Our paper provides causal evidence that the attenuation can be explained by intergenerational spillovers of public school construction.

Third, we provide a policy-based explanation for variation in educational gender inequality across countries. In recent cross-sections, women have lower school attainment than men in the lowest-income countries where overall school access is lower, yet the gaps are zero or reversed in higher-income countries (Evans, Mendez Acosta, and Yuan, 2023). Within Latin America, adult women born in the 1940s had lower mean attainment than men, but this gap converged across cohorts born in the 1950s and 1960s (Duryea et al., 2007). In Chile, we confirm that a half-year gender gap was stable across 1940s cohorts, though it had been eliminated by 1959 due to women's heterogenous response to school construction. We provide new historical evidence that gender-neutral, access-based interventions can be more effective for women than men (cf. Evans and Yuan, 2022).

2. Chile's Special Plan for Primary Instruction

In the early 1960s, Chile's school system consisted of primary schools (grades 1–6) and secondary schools (grades 7–12). Although primary schooling had been compulsory since the 1920s, enrollment remained far below this goal, especially outside major cities (MEP, 1961; Blakemore, 1976). The 1960 census reported national net enrollment rates (NERs) of 78% in primary school and 26% in secondary school, with lower rural rates (66% and 4%). A 1961 report on the crisis in public education highlighted constrained school supply as a key factor behind low enrollment (MEP, 1961; Schiefelbein, 1976; Bellei and Pérez, 2016).

Two weeks after taking office in November 1964, President Eduardo Frei Montalva announced the *Plan Extraordinario de Enseñanza Primaria*, or Special Plan for Primary Instruction, to boost enrollments by the start of the 1965 school year (Frei, 1964; see Figure 1.A). The plan aimed to rapidly expand public school access for grades 1–8 through an accelerated program of classroom construction and teacher training in the early years of the Frei administration (Blakemore, 1976; Bellei and Pérez, 2016).

In December 1964, the Ministry of Public Education organized a two-week nationwide registration campaign (MEP, 1964), urging parents to register all children aged 7–15—whether never enrolled, behind grade level, or having dropped out. Registration took place at schools and,

where no school existed, at local police stations. Local authorities were instructed to register every child regardless of capacity constraints.

The resulting enrollment census recorded birthdates and prior grade completion, identifying 186,106 new students, close to the 200,000 expected (Frei, 1965b). Using these census data, the Ministry identified communities where new schools or classrooms (from grades 1–8) were required to satisfy parent demand (Schiefelbein, 1976). In December 1965, a law formally extended compulsory primary schooling to include grades 7 and 8 (Bellei and Pérez, 2016).

Public school construction was coordinated by a specialized agency, the *Sociedad Constructora de Establecimientos Educacionales* (SCEE), often using prefabricated construction methods and labor provided by the military and university students (SCEE, 1965, 1982; Bellei and Pérez, 2016; Exss Cid and Torrent, 2018). Construction surged in 1965 (Figure 1.B), with 3,539 new classrooms and one million square meters built (Frei, 1965b; SCEE, 1982). In the late 1960s, "nobody…could fail to the notice the simple new school buildings dotting the countryside" (Sater and Collier, 2023, p. 333). The Ministry of Public Education also used the enrollment census to redistribute students among nearby schools and, when feasible, to increase utilization of existing classrooms through double shifts (Bellei and Pérez, 2016).

To staff new classrooms, the government created an accelerated 32-week teacher-training program, delivered in four sessions across 1965 and 1966; its graduates became known as "pressure cooker" teachers (Frei, 1964; Chile Films, 1969; Bellei and Pérez, 2016). Some existing teachers also expanded their contracted hours to teach additional shifts. The Ministry supplied new classrooms with instructional guides, textbooks, and related materials (Schiefelbein, 1976).

3. Research Design

We use a quasi-experimental, difference-in-differences design to estimate the long-run effects of school construction on schooling and labor outcomes. The design compares long-run outcomes across birth cohorts (the first difference) and across departments (the second difference) that received more or less construction. Children born before 1949 were unexposed because those older than 16 were both ineligible and unlikely to enroll in primary school in 1965

(Frei, 1964; MEP, 1965).[4] Each subsequent birth cohort gained an additional year of potential exposure. Geographic variation arose because primary school construction was allocated to communities in proportion to the number of unenrolled, primary-aged children in each community (Schiefelbein, 1976). The next section details our estimation strategy.

A. Estimation of First-Generation Effects

We estimate the event study regression:

$$Y_{idct} = \sum_{\substack{1940 \leq c \leq 1959, \\ c \neq 1948}} \beta_c (I_d \cdot 1\{C_i = c\}) + \theta_d + \delta_{ct} + X'_{idct}\pi + \varepsilon_{idct} \quad (1)$$

where Y_{idct} is a schooling or labor market outcome of adult *i* born in department *d* and year *c,* and surveyed in census year *t*. I_d is the continuously-measured intensity of school construction in birth department *d.* The specification includes fixed effects for 79 birth departments (θ_d) and 80 birth-year-by-census cells (δ_{ct}). We estimate the regression in pooled census samples from 1982, 1992, 2002, and 2017. Additional control variables (X_{idct}) include a female dummy variable and proxies of the parental schooling and household wealth to which adults were exposed when younger. Standard errors are clustered by birth department. Section 4 details the measurement of variables and sample construction.

The β_c are cohort-specific differences-in-differences estimates relative to 1948, which was the last untreated cohort. We expect pre-treatment coefficients to be close to zero—a necessary though not sufficient condition for parallel trends. For post-treatment cohorts, we expect β_c to increase with the years of primary-eligible exposure to school construction. The 1949 cohort was exposed to newly-constructed classrooms for, at most, one year. The 1959 cohort—approximately 6 years old in 1965—was exposed for the full duration of the primary-eligible age range.

We compare event-study estimates to those of a parsimonious specification that imposes parallel pre-treatment trends ($\beta_c = 0 \; \forall c \in \{1940, \ldots, 1948\}$), and summarizes policy exposure

[4] The plan formally targeted 7–15 year-olds (MEP, 1964), but slightly older or younger children were allowed to enroll if capacity allowed.

with a single variable, $E_{dc} = I_d \cdot (C_i - 1948) \cdot 1\{C_i \geq 1949\}$, the product of departmental treatment intensity and each cohort's years of exposure:[5]

$$Y_{idct} = \gamma E_{dc} + \theta_d + \delta_{ct} + X'_{idc}\pi + \varepsilon_{idct} \quad (2)$$

We assess the fit of this specification by overlaying predicted values on event-study plots.

In both specifications, we conservatively remove a linear birth-year trend that varies with treatment intensity ($I_d \cdot C_c$). We do not directly control for $I_d \cdot C_c$ because it is potentially confounded with dynamic treatment effects (Lee and Solon, 2011; Miller, 2023). Instead, we estimate the trend in the pre-treatment sample (1940–1948) and subtract predicted values from the full sample before estimation (e.g., Goodman-Bacon, 2021).[6]

B. Estimation of Intergenerational Effects

We extend equations (1) and (2) to estimate how parents' policy exposure affects the schooling outcomes of their co-resident children. We estimate regressions in samples of children residing in the same household as their mothers and/or fathers. The dependent variable (Y_{ijdct}) is the outcome of child *j* of mother (or father) *i.* As in prior specifications, we include fixed effects for mothers' (or fathers') birth departments and years. The regression also includes indicator variables for categories of child gender, child birth order, the total number of co-resident children, and interactions between child birth years and census years.[7]

4. Data

This section describes the census data used to implement our research design. We first explain the measure of departmental treatment intensity (I_d). We then describe the sample of

[5] Specifications with a continuously-measured treatment require a "strong" addition to the usual parallel trends assumption: individuals across birth cohorts and departments would have experienced similar changes in outcomes had they experienced a different dose of the treatment (Callaway, Goodman-Bacon, and Sant'Anna, 2024). In support of this, we show that event study estimates from equation (1) are consistent with the effects being linearly increasing in years of exposure.

[6] Appendix Figure A1 reports estimates without applying this procedure. We also fail to reject the null of no pre-treatment trends for years of schooling, but estimates of effects are slightly larger than in Figure 2, leading us to prefer the more conservative specification for our main outcome (Bilinski and Hatfield, 2019). There are also positive estimates for labor outcomes, but there is clear visual evidence of a negative pre-treatment trend that biases effects downward.

[7] Birth order and total children are inferred from co-resident children linked to the same parent in a census sample. As in equation (1), we remove a linear parent birth year trend that varies with the treatment intensity in the parent's birth department.

first-generation adults pooled across four census samples from 1982–2017. Finally, we describe the samples of co-resident children linked to first-generation mothers or fathers.

A. Treatment Intensity

Section 2 explained how the Ministry of Public Education used a special enrollment census of 7–15 year-olds—collected in mid-December 1964—to allocate new classroom construction (MEP, 1964; Schiefelbein, 1976). We use Chile's 1960 IPUMS census sample (Ruggles et al. 2025), collected in late November, to construct a closely-related proxy of local treatment intensity (I_d). Using data for 7–15 year-olds across 79 departments—the smallest geographic unit in the 1960 census sample—we calculate each department's net enrollment rate (NER) for any level of schooling. NERs range from 0.5 to 0.9. Given the actual allocation rules described in Schiefelbein (1976), communities with more unenrolled children were allocated more classroom construction per child. We therefore define treatment intensity as $I_d = 1 - NER_d$.

B. First-Generation Adults

We estimate equations (1) and (2) using adults born from 1940–1959 who appear in 10% household samples of the 1982, 1992, 2002, and 2017 census samples (Ruggles et al. 2025). Depending on the year, these adults are surveyed between the ages 23 and 77. Treatment intensity (I_d) is assigned to individuals based on their birth departments, since migration may be endogenous to childhood policy exposure.[8] We further match adults to a proxy of parental schooling and household wealth from the 1970 IPUMS census sample (Ruggles et al. 2025).[9] There is no evidence of pre-treatment or post-treatment trends in the unconditional means of

[8] Departments were used as lower-level territorial designations in the 1960 and 1970 census samples. Since 1982, IPUMS samples report communes of birth, a smaller territorial unit. We assign each commune to the department that occupied the largest proportion of the commune's territory, based on 2023 commune boundaries (https://www.geoportal.cl/geoportal/catalog/download/912598ad-ac92-35f6-8045-098f214bd9c2) and harmonized department boundaries from 1960 and 1970 (https://international.ipums.org/ international/gis.shtml). Across 345 communes, the median commune has 97% of its territory in the imputed department, while communes at the 10th percentile have 81%.

[9] Within 1,580 birth-department-by-birth-year cells, we calculate the (i) mean years of schooling and (ii) the mean household wealth index—in the pooled sample of co-resident mothers and fathers—and merge this to the pooled census sample of adults observed 1982–2017. The 1970 census sample maximizes the number of co-resident parents and children among individuals born between 1940 and 1959. The 1960 IPUMS sample cannot be used for this purpose because it does not record household relationships. The household wealth index uses nine measures of household assets (see Appendix B).

these variables, when comparing adults born in more-treated and less-treated departments (Appendix Figure A2).

The main dependent variable of interest is completed years of schooling. We also examine two labor market outcomes: (i) employment in the week preceding the April census dates, and (ii) employment in a higher-skill occupation (relative to unskilled or no employment).[10] To explore mechanisms that underlie the estimated schooling and labor market effects, we also analyze (i) migration from the commune of birth; (ii) women's total fertility (i.e., cumulative live births reported by women); (iii) child mortality (i.e., the share of children ever born to women who died before the census date); (iv) whether adults co-reside with a partner (married or not); and (v) the partner's years of schooling. Table A1 contains descriptive statistics.

C. Second-Generation Children of First-Generation Adults

We estimate intergenerational effects using four samples of second-generation children who reside with at least one first-generation parent. The first sample includes younger children (ages 7–21) of first-generation women surveyed in 1982, 1992, and 2002. The 10^{th} (90^{th}) percentile of children's birth year is 1968 (1987). The second sample includes older children (ages 22 and above) of first-generation women surveyed in 1992, 2002, and 2017. There is a similar distribution of children's birth years: the 10^{th} (90^{th}) percentile is 1966 (1988). The third and fourth samples include younger and older children of first-generation men.

For younger children, we measure on-time grade progression using the indicator $1\{A - S \leq 6\}$, where A denotes age and S denotes highest grade completed. On the April census dates—shortly after the start of a school year in March—the modal 7-year-old has completed first grade on time. Deviations from this benchmark may reflect delayed enrollment, grade repetition, or dropout. For older children, the main outcome is completed years of schooling.

5. Effects on the First-Generation

In this section, we document the impact of treatment intensity (I_d) on department-level enrollment rates of 7–15 year-olds between 1960 and 1970. We then use the event study regressions described in section 3 to examine long-run effects on completed schooling and labor

[10] We define skilled occupations as managers, professionals, and technicians based on the International Standard Classification of Occupations.

market outcomes, also exploring heterogeneity by gender. Finally, we report evidence on potential channels of schooling and labor market effects.

A. Child Enrollment

Net enrollment rates for 7–15 year-olds rose from 76 percent to 90 percent between the 1960 and 1970 census samples, with even larger gains (62 percent to 84 percent) in rural areas that experienced greater treatment intensity. Figure 1.C plots the departmental change in NERs against I_d. The 79 circles are proportional in size to the 1960 population of 7–15 year-olds, confirming that larger cities like Santiago had lower treatment intensities. A one percentage point increase in I_d is associated with a 0.65 percentage point increase (s.e. = 0.04) in the departmental NER, consistent with reduced supply constraints due to school construction.

Importantly, these enrollment changes are not explained by differential, department trends in the socioeconomic status of adults, as proxied by the mean schooling and wealth of 25–65 year-olds, cohorts that were too old to be treated. Figure 4.D shows no systematic relationship between I_d and the change in mean adult schooling from 1960 to 1970. Appendix Figure A3 reports similarly null associations for the change in a mean household wealth index (see Appendix B). The estimated association between I_d and changes in NERs is stable when controlling for schooling and wealth (0.65), and when weighting by the 1960 population of 7–15 year-olds (0.63).

B. Adult Schooling and Labor Supply

Figure 2.A plots event-study coefficients ($\hat{\beta}_c$) from equation (1) for years of schooling. The pre-treatment coefficients show no evidence of differential pre-treatment trends and we fail to reject the null that coefficients are jointly zero ($p = 0.32$).[11] Post-treatment coefficients are positive and increasing with birth year (and thus with potential exposure), and are statistically different from zero at the 5 percent level for all but the 1949 cohort. The dashed line plots predicted values that are the product of $\hat{\gamma}$ from equation (2) and a cohort's years of exposure. The

[11] Because we estimate equation (1) in a sample that already removes $I_d \cdot C_c$ (see section 3), the F-test that pre-treatment coefficients are jointly zero should be interpreted as a test of non-linear pre-treatment trends (Miller, 2023).

predictions lie within the pointwise confidence intervals, indicating that schooling effects are approximately linear in years of exposure.

In the 1959 cohort, an individual born in the most-treated department versus the least-treated department completed 1.1 additional years of schooling ($= \hat{\beta}_{1959} \cdot (\max(I_d) - \min(I_d)) = 3.19 \cdot (0.47 - 0.11)$). The nearly-identical estimate from equation (2) is 1.2 years ($= \hat{\gamma} \cdot 11 \cdot (\max(I_d) - \min(I_d)) = 0.306 \cdot 11 \cdot (0.47 - 0.11)$). The effect is large relative to the mean schooling of 6.5 years among adults born in pre-treatment years in more-treated departments (Appendix Table A1).[12]

The program aimed to alleviate supply constraints in grade 1–8, especially in higher grades. We therefore re-estimate equation (2) with the dependent variable $1\{Y_{idct} \geq x\}$, indicating that adults attained at least *x* years of schooling. Figure 2.B reports the $\hat{\gamma}$ from equation (2), which are largest for completion of grades 5–8, consistent with the policy. Scaling to the 1959 cohort, individuals born in the most-treated department were 20 percentage points more likely to complete at least seventh grade than those born in the least-treated department ($= 0.05 \cdot 11 \cdot (0.47 - 0.11)$. Estimates for secondary grades (9–12) are 20–40 percent as large, but remain statistically significant. In summary, the program substantially increased attainment in the grades directly targeted by construction, with smaller effects on secondary grades.

We next examine two measures of adult labor supply: (i) employment in the week preceding the census and (ii) employment in a higher-skill occupation, versus lower-skill or no employment (Figures 2.C and 2.D). As with schooling, pre-treatment coefficients are not statistically distinguishable from zero. Both sets of estimates imply sizable and statistically significant effects that are consistent across estimates from equations (1) and (2). In Figure 2.C, employment among the 1959 cohort increased by 11 percentage points for individuals born in the most- versus least-treated departments ($= 0.026 \cdot 11 \cdot (0.47 - 0.11)$). In Figure 2.D, skilled work increases by 6 percentage points. These effects are large relative to the pre-treatment shares in more-treated departments (43 percent employed, and 7 percent employed in higher-skill occupations). Though we cannot measure earnings, the especially large effect on skilled work suggests that productivity is increasing due to increased schooling.

[12] We define more-treated departments as those with above-median I_d (0.21) when weighted by the 1960 population of 7–15 year-olds.

The results are robust to three alternate samples. First, we exclude the 2017 sample because it includes older adults (ages 55–77). Selective mortality might bias estimates, but the estimates are similar (Appendix Figure A4). Second, we exclude the 1982 sample because it was collected in the midst of a recession, but again the results are similar (Appendix Figure A5). Third, we exclude birth departments affected by a large earthquake in May 1960.[13] The schooling estimate is similar, and the labor estimates are slightly larger (Appendix Figure A6).

C. Gender Heterogeneity and Convergence

There are positive and statistically significant estimates for schooling and labor outcomes in separate samples of women and men (Appendix Figure A7). We formally test for heterogeneity using a pooled specification:

$$Y_{idct} = \tau Female_{idct} E_{dc} + \theta Female_{idct} + \delta_{dct} + \varepsilon_{idct} \quad (3)$$

where the δ_{dct} are 6,320 fixed effects for the triple interaction of birth department, birth year, and census year. The fixed effects absorb the main effect of policy exposure, parent schooling and wealth proxies, and unobserved variables shared by similarly-aged adults born in the same place and year. The coefficient on the interaction term, $\hat{\tau}$, measures the differential effect of the policy on women. The schooling effect is larger for women: $\hat{\tau} = 0.145\ (0.013)$. It is similar to the simple difference in female and male coefficient estimates in Appendix Figure A7. The pre-treatment gender difference ($\hat{\theta}$) is –0.48 years of schooling.

Gender heterogeneity in the effects of school construction provides a causal explanation for Chile's well-documented gender convergence in schooling attainment (see Duryea et al., 2007). In Figure 3.A, the solid circles plot the mean difference in schooling between adult women and men. Women born in the 1940s had lower mean schooling than men by about half a year, consistent with equation (3). The difference converges among 1950s cohorts, and by 1959 we cannot reject the null of no difference. Would convergence have occurred in the absence of heterogeneous effects of school construction? To assess this, we subtract $\hat{\tau} \cdot E_{dc}$ from each female observation and plot the counterfactual gender difference as hollow circles. The counterfactual difference in the absence of construction is persistently large and statistically

[13] The 1960 census was collected about six months after the Valdivia earthquake. We conservatively exclude 30 of 79 departments where at least 10% of the territory had an earthquake intensity of at least 7, indicating "very strong" shaking and "moderate" damage (https://earthquake.usgs.gov/earthquakes/eventpage/official19600522191120_30/shakemap/intensity).

different from zero (given cluster-bootstrapped confidence intervals that account for the two-step estimation).

The remainder of Figure 3 reports the same analysis for labor outcomes. In our sample, women are about 40 percentage points less likely to work than men, and school construction did not alter this gap. In contrast, there is some evidence that school construction had larger effects on women's participation in skilled occupations, which modestly closed a gender gap. However, the change is only statistically significant for the 1959 cohort.

D. Migration and Demographics

School construction did not affect the probability that adults migrated from their birth commune in the full, female, or male samples (Appendix Figures A8–A10). It also did not appreciably affect the probability of having a co-resident partner.[14] Conditional on having a partner, own-policy exposure increased partners' schooling, a plausible byproduct of rising school attainment among potential partners of a similar age who were born in the same communities. The effect on partners' schooling is 80% of the own-schooling effect for women from equation (2), and 74% of the effect for men.

The policy had an unexpectedly positive but quite small effect on women's total fertility (Appendix Figure A9). The estimate implies that the 1959 cohort had 0.2 additional live births in the most-treated department versus the least, relative to 3.6 births among women born in pre-treatment cohorts in more-treated departments (Appendix Table A1). A possible explanation is that the total number of pregnancies remained the same, but live births increased due to the improved health of mothers. Conditional on a live birth, there is no evidence that women's policy exposure affected the child mortality rate (Appendix Figure A9).

6. Effects on Second-Generation Children of the First-Generation

This section reports estimates of the intergenerational effects of school construction on co-resident children of the first-generation. In Figure 4, the left panels correspond to the effects of mothers' policy exposure on co-resident children, while the right panels correspond to those of

[14] There is no effects in the full or female samples. In the male sample, the estimate from equation (2) shows a small and statistically significant effect on having a co-resident partner, but the confidence intervals of event study coefficients always include zero.

fathers' exposure. The top and bottom panels include, respectively, school-aged children and youth (7–21 years-old) and adult children (22 and older). Each panel reports estimates of equations (1) and (2) with the extensions described in section 3.B.

Mothers' policy exposure had positive effects on children's schooling (Figure 4.A and 4.C). For both schooling outcomes, there is no evidence of pre-treatment trends, and post-treatment coefficients are positive, increasing, and statistically different from zero for mothers born in the mid-1950s and later. Estimates from the parsimonious specification imply that children of mothers born in 1959 are 9.5 percentage points more likely to have on-time grade progression, if mothers are born in the most-treated department versus the least ($= 0.024 \cdot 11 \cdot (0.47 - 0.11)$). The effect is a 22% increase relative to the 44% of children with on-time grade progression who were born in pre-treatment years in more-treated departments. In the sample of older children, the comparable effect on years of schooling is 0.98 ($= 0.248 \cdot 11 \cdot (0.47 - 0.11)$), relative to a mean of 10.7 years.

In contrast to these results, fathers' policy exposure did not affect on-time grade progression or years of schooling. The confidence intervals of event study coefficients nearly always include zero in pre-treatment and post-treatment cohorts (Figure 4.B and 4.D). The parsimonious specification in equation (2) yields more precise estimates that are close to zero.

There are several threats to the causal interpretation of these results. First, the census samples do not include children unless they are co-resident with adults on the survey date. This may introduce sample selection bias if school construction affects the probability of co-residency. We find no effect of mothers' policy exposure on the number of children who are co-resident at the time of the census, using either specification (Appendix Figure A9). Therefore, the results in Figure 4 are not explained by selection. There is a positive and statistically significant effect on the number of co-resident children in the male sample, but it is quite small (Appendix Figure A10).[15]

Second, the intergenerational effects of mothers (or fathers) may operate, in part, through the increased schooling of their partners, given effects on assortative mating described in section 5.D. The effects of own-schooling and partner-schooling are challenging to separately identify. As a simple exercise, we re-estimated the four panels of Figure 4 in the sub-sample of children

[15] The 1959 cohort of men resides with 0.16 more children ($= 0.041 \cdot 11 \cdot (0.47 - 0.11)$) relative to a mean of 1.5 in pre-treatment cohorts in more-treated departments.

residing with both parents (Appendix Figure A11). The intergenerational effects of mothers are nearly identical in the much smaller samples of children with a father present, suggesting that the presence of more-educated fathers plays a minimal role in explaining mothers' effects. In contrast, the intergenerational effects of fathers on completed schooling are somewhat larger but still statistically insignificant in the presence of both mothers and fathers. The confidence interval still includes zero, but the point estimate from equation (2) increased from –0.01 to 0.04, despite losing only 12% of children without co-resident mothers. Both results reinforce the particular importance of women in determining intergenerational effects.

Third, some children were potentially exposed to school policies in the 1990s and 2000s, such as conditional cash transfers for indigenous children (Lucas et al., 2025) and longer school days in publicly-funded schools (Dominguez and Ruffini, 2023).[16] We already control for birth-year-by-census-year fixed effects, and so our main concern is that children whose mothers were increasingly exposed to school construction were also differentially exposed to more recent policies. To address the first issue, we exclude the 6% of children in the census samples identified as indigenous. To address the second, we construct each child's expected years of exposure to longer school days, given their birth year and commune (recalling that communes are nested within departments).[17] The estimates are similar in all samples (Appendix Figure A12).

7. Welfare Analysis

We estimate the marginal value of public funds ($MVPF = \frac{\Delta W}{\Delta E - \Delta C}$) for each treated cohort born in more-treated versus less-treated departments (Hendren and Sprung-Keyser, 2020; see Appendix C for details on our data and assumptions).[18] ΔW is additional after-tax earnings, ΔE is additional government expenditures, and ΔC is additional tax revenues that offset expenditures.

[16] The indigenous grants program began in 1991, and first affected indigenous children born in 1975. The implementation of longer school days began in 1997 and first affected children born in 1980.

[17] Dominguez and Ruffini (2023) construct a related measure. We use administrative data on (1) grade-by-school treatment status and (2) annual school enrollments in grades 1–12. For each birth cohort in a given birth commune, we calculate the probability of exposure to a longer school day in each grade (assuming enrollment at age 6 and on-time grade progression), and sum across cohort probabilities within each commune to calculate the expected years of exposure.

[18] We define a dummy treatment variable as $1\{I_d \geq 0.21\}$, where the cut-point is the sample median of I_d, weighted by the 1960 population of 7–15 year-olds.

All are expressed in 1965 present value and 2006 prices, using a discount rate of 5.5% (SNI, 2024).

The average individual born in 1959 in more-treated departments earned an additional CLP 1,574,392 or US$1,852. US$1,686 were after-tax earnings (ΔW) and US$166 were value-added tax revenues (ΔC), assuming a marginal propensity to consume of 0.5 (Escalona Caba, 2014; Barrero et al., 2020). The average individual born in 1959 in more-treated departments incurred additional expenditures (ΔE) of CLP 271,733 or US$320. These include annualized capital expenditures, assuming a 30-year building lifetime, and current expenditures on teacher salaries, materials, and many other categories described in Schiefelbein (1976). The MVPF of the 1959 cohort is 11, and ranges from 10.2–17.1 for 1949–1959 cohorts. The population-weighted average MVPF is 14.3.

We further estimate MVPFs that value intergenerational spillovers. The average individual born in 1959 in more-treated departments had children with additional after-tax earnings of US$255, tax revenues of US$27, and additional secondary school expenditures of US$34. Including these estimates, the 1959 MVPF increases to 12.1, and the population-weighted average across cohorts to 15.7. Thus, the omission of intergenerational spillovers modestly understates the MVPF, despite increased government expenditures on child schooling induced by mothers' policy exposure.

8. Conclusions

In 1965, the new Chilean administration of President Eduardo Frei Montalva implemented the Special Plan for Primary Instruction. Within a few years, the government built thousands of new classrooms for grades 1–8. The Ministry of Public Education targeted communities with supply constraints, as determined by a special enrollment census (Schiefelbein, 1976). We use a closely-related proxy of local treatment intensity from the 1960 population census, in concert with pooled census samples of adults from 1982, 1992, 2002, and 2017. With these data, we implement a difference-in-differences design that compares the schooling and labor outcomes of adults born before and after their potential treatment exposure (a first difference), with those of adults born in more-treated and less-treated departments (a second difference).

The results show that childhood exposure to the policy substantially increased completed years of schooling among adults, increased labor force participation, and increased the

proportion working in higher-skill occupations. We show that schooling effects were larger for women, despite similar exposure within departments. We find that women's schooling produced large intergenerational spillovers among co-resident children. Specifically, on-time grade progression increased among school-aged children, as did completed schooling among children 22 and older. There are $16 of after-tax earnings benefits—including directly-affected adults and their children—for each $1 of net government expenditures.

Our results provide the first Latin American evidence on the long-run effects of primary school construction, and causal confirmation of the important role played by 1960s public school expansion in improving educational mobility in Chile (Celhay and Gallegos, 2025). We also provide a policy-based, causal explanation for the convergence of gender gaps in educational attainment (Duryea et al., 2007). Finally, we provide add to the scarce evidence on intergenerational spillovers caused by exogenous increases in parent schooling in developing countries, complementing work on more recent social and education policies in Chile (Lucas et al., 2025; Barrios-Fernández et al., 2024).

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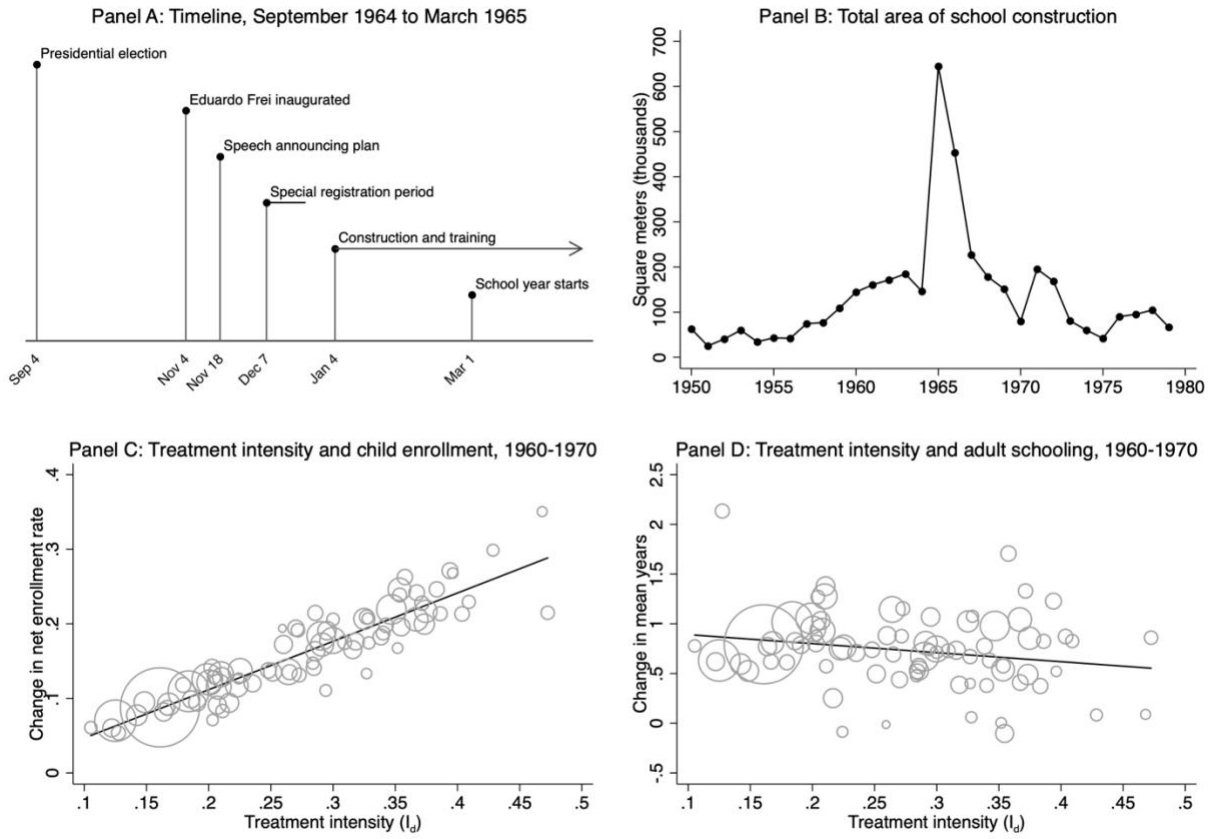
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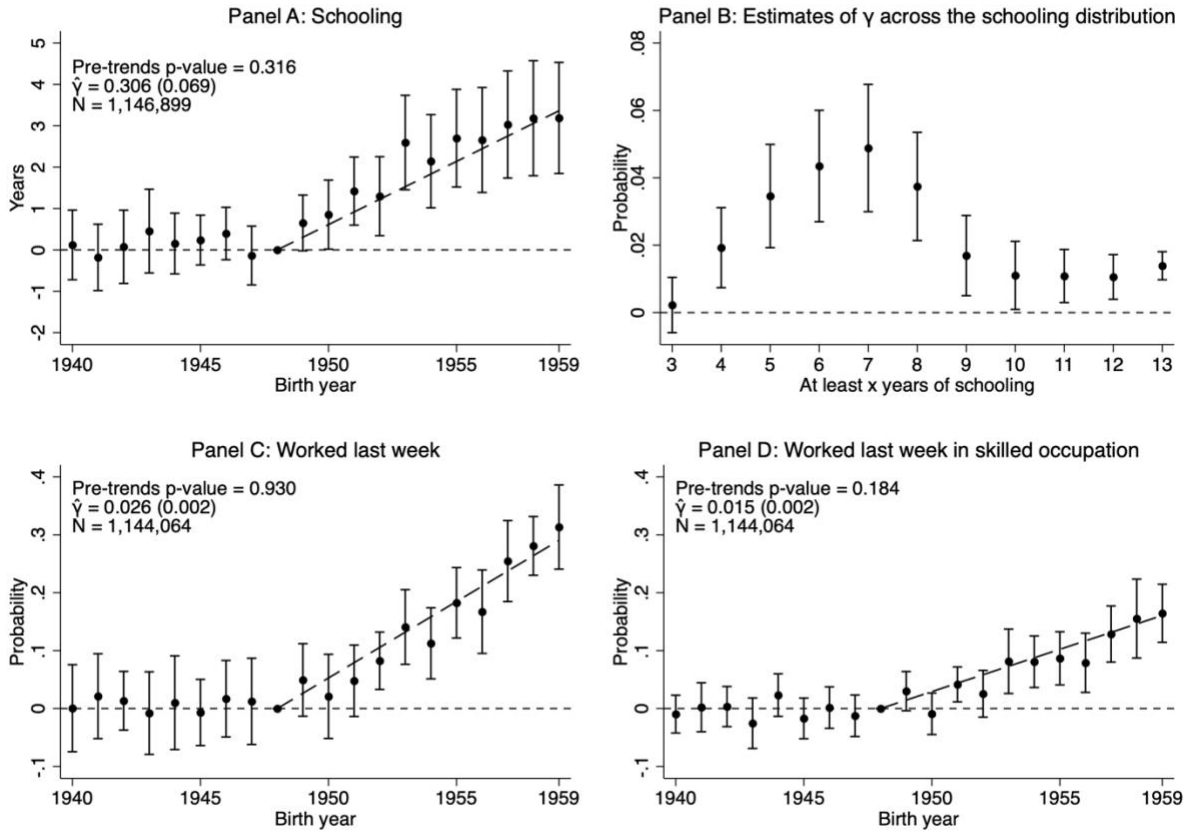
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Figure 1: Rollout and short-run effects of the "Special Plan for Primary Instruction"



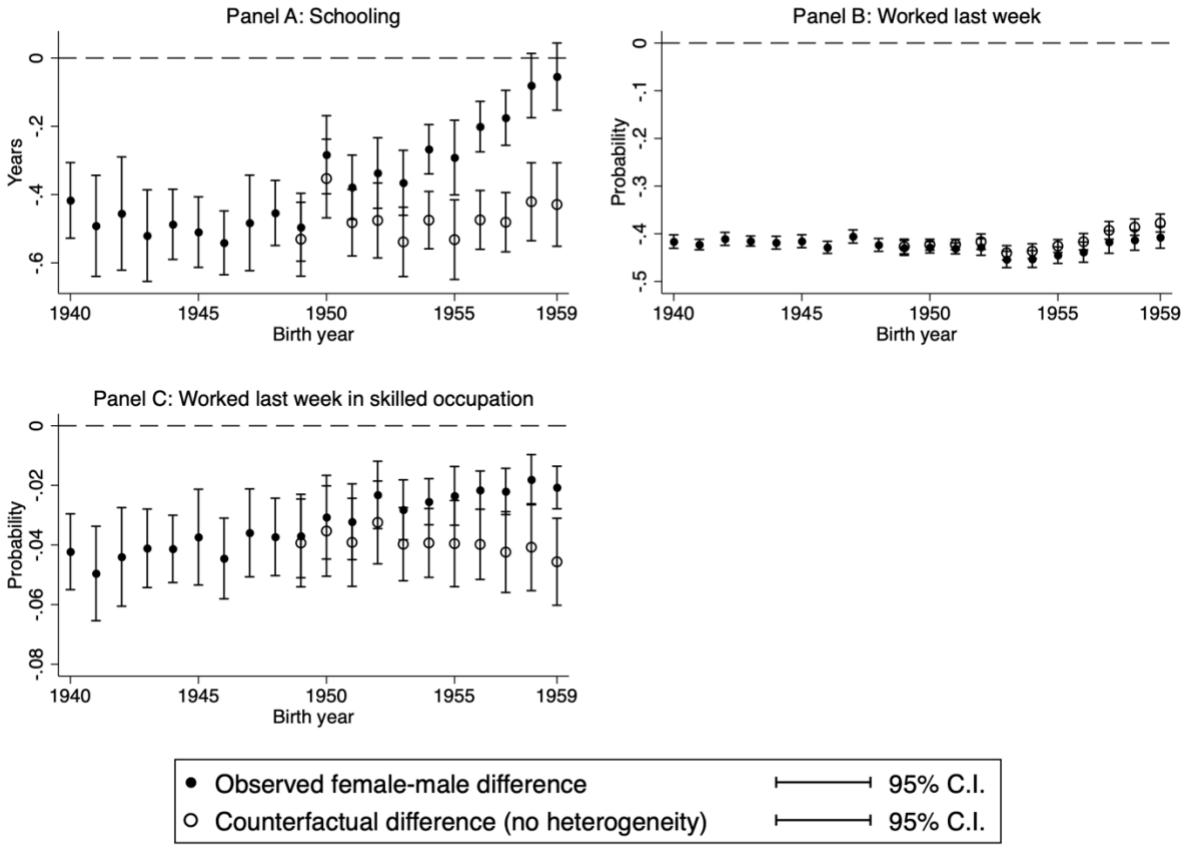
Notes: Panel A: See Frei (1964) and MEP (1964). Panel B: Total area includes all public school construction by the *Sociedad Constructora de Establecimiento Educacionales* (SCEE), the *Ministerio de Educación Pública,* the *Ministerio de Obras Públicas,* and the *Corporación de la Vivienda* (SCEE, 1982). From 1950–1959, SCEE (1982) only reports square meters built by SCEE. We therefore inflate 1950–1959 values by the ratio of the total square meters to SCEE square meters in 1960–1963. Panel C: Y-axis is the department-level change between 1960 and 1970 IPUMS census samples in the proportion of 7–15 year-olds enrolled in any grade of school. Panel D: Y-axis is the department-level change between the 1960 and 1970 IPUMS census samples in the mean years of schooling of 25–65 year-olds.

Figure 2: Effects on first-generation schooling and labor supply



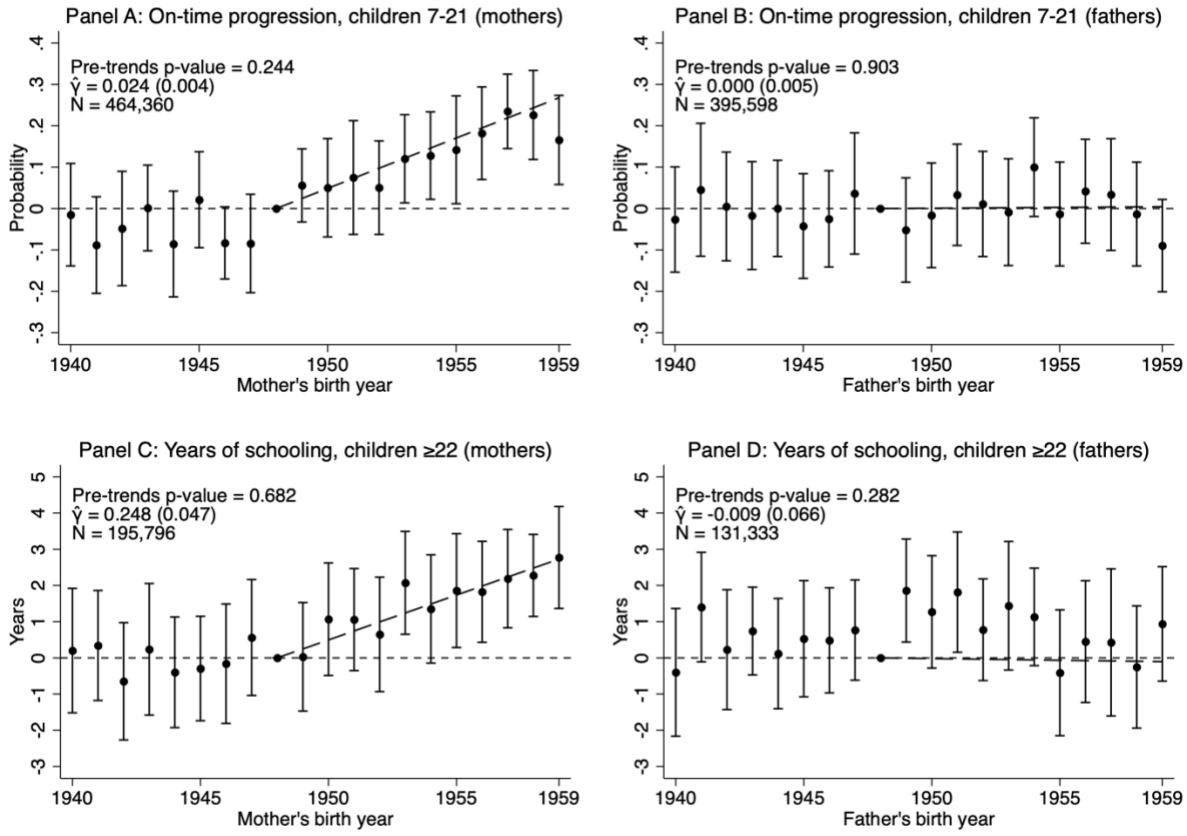
Notes: Panels A, C, and D plot $\hat{\beta}_c$ and 95% confidence intervals from equation (1), after removing a linear cohort trend that varies with treatment intensity ($I_d \cdot C_c$); see text for details. Standard errors are clustered by birth department. Dashed lines are predicted values, the product of $\hat{\gamma}$ from equation (2) and cohorts' years of exposure. Panel B: $\hat{\gamma}$ are from equation (2) with the dependent variable, $1\{Y_{idct} \geq x\}$, where x is a successively higher year of completed schooling.

Figure 3: Female-male differences in schooling and labor outcomes



Notes: The solid circles are unconditional mean differences, within birth years, between female and male observations in the pooled census sample. The hollow circles are unconditional differences, after subtracting $\hat{\tau} \cdot E_{dc}$—obtained with equation (3) —from female observations. Confidence intervals are estimated with 499 cluster-bootstrapped samples (by birth department) that account for the two-step estimation procedure.

Figure 4: Effects on second-generation schooling



Notes: All panels plot $\hat{\beta}_c$ and 95% confidence intervals from equation (1)—including the additional controls described in section 3.B—after removing a linear cohort trend that varies with treatment intensity ($I_d \cdot C_c$); see text for details. Standard errors are clustered by birth department. Dashed lines are predicted values, the product of $\hat{\gamma}$ from equation (2) and cohorts' years of exposure.

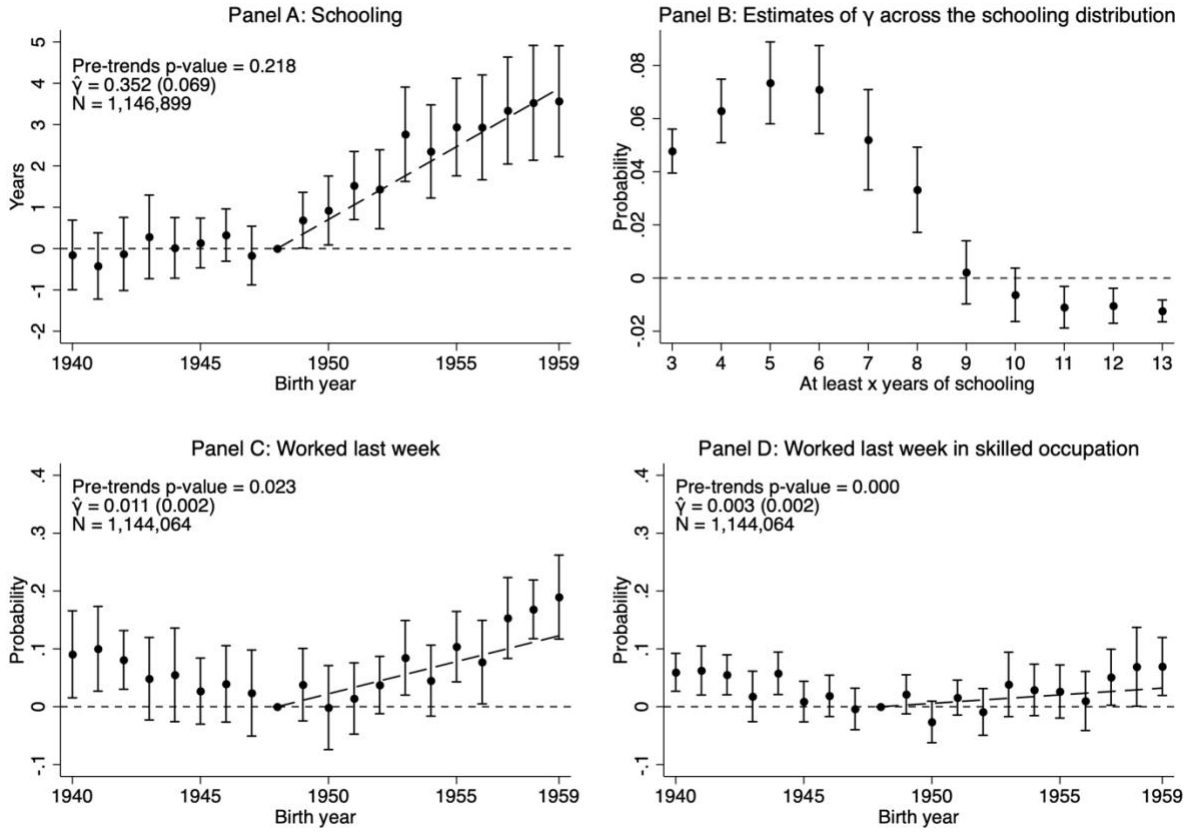
Appendix A: Additional Table and Figures

Table A1: Descriptive statistics in the pooled census samples

	First generation born in less-treated departments				First-generation born in more-treated departments			
	Born 1940–1948		Born 1949–1959		Born 1940–1948		Born 1949–1959	
	Mean (SD)	N	Mean (SD)	N	Mean (SD)	N	Mean (SD)	N
Panel A: First-generation adults (1982, 1992, 2002, and 2017 censuses)								
Exposure to treatment (*E*)	0	191252	1.11 (0.57)	383182	0	218281	1.95 (1.06)	360326
Years of schooling	8.76 (4.37)	190191	10.1 (4.08)	381275	6.47 (4.28)	217030	8.21 (4.20)	358403
Worked last week	0.47	189652	0.57	379626	0.43	216876	0.54	357910
Worked last week in skilled occupation	0.12	189652	0.13	379626	0.067	216876	0.082	357910
Migrated from birth commune	0.68	191252	0.65	383182	0.63	218281	0.58	360326
Number of co-resident children	1.42 (1.49)	191252	1.26 (1.32)	383182	1.47 (1.63)	218281	1.26 (1.37)	360326
Co-resident partner	0.66	191252	0.62	383182	0.66	218281	0.62	360326
Years of schooling of co-resident partner	8.79 (4.32)	126582	9.98 (4.06)	236699	6.74 (4.25)	143068	8.22 (4.18)	223653
Number of live births	3.08 (2.04)	98581	2.39 (1.60)	192022	3.55 (2.48)	111979	2.61 (1.81)	181354
Mortality rate of live births	0.047 (0.13)	89192	0.030 (0.12)	167938	0.058 (0.14)	101526	0.036 (0.12)	159845
Panel B: Second-generation children, 7-21, residing with mothers (1982, 1992, and 2002 censuses)								
Mothers' exposure to treatment	0 (0)	82882	1.04 (0.56)	142468	0 (0)	100402	1.82 (1.05)	138611
On-time grade progression	0.54 (0.50)	82882	0.66 (0.47)	142468	0.44 (0.50)	100402	0.59 (0.49)	138611
Panel C: Second-generation children, 22 and over, residing with mothers (1992, 2002, and 2017 censuses)								
Mothers' exposure to treatment	0	43574	1.02 (0.57)	54291	0	49586	1.77 (1.04)	49697
Years of schooling	11.9 (3.72)	43358	12.8 (3.44)	53804	10.7 (3.91)	49370	11.9 (3.74)	49267

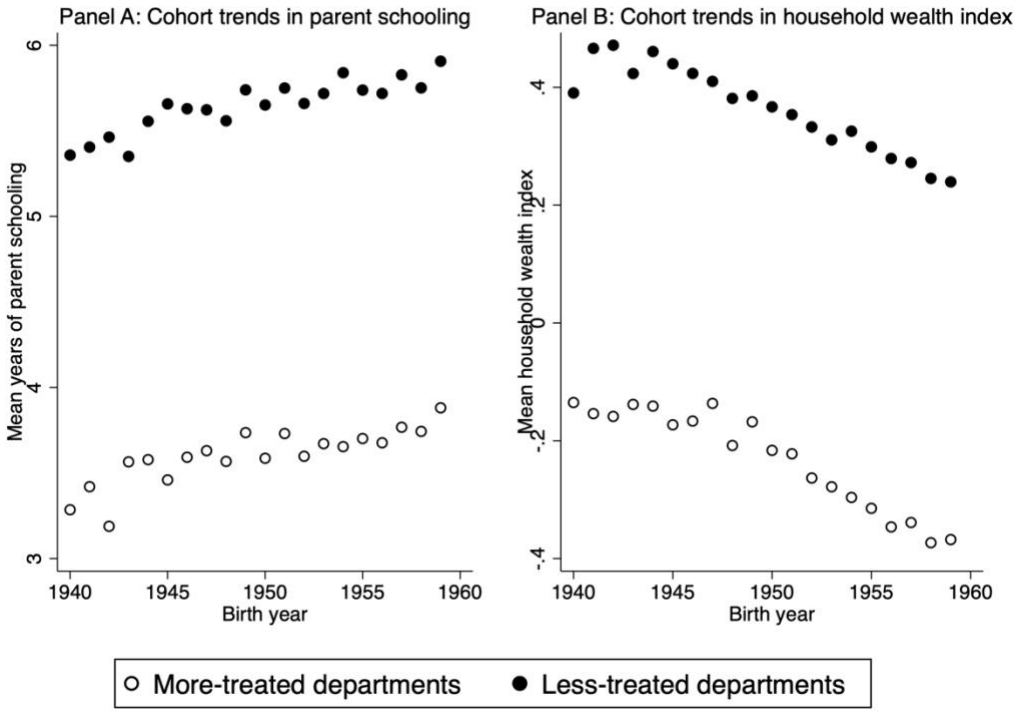
Notes: More-treated department have treatment intensities of 0.21 or more, while less-treated departments are less than 0.21; see text for details.

Figure A1: Effects on first-generation schooling and labor supply (without removing $I_d \cdot C_c$)



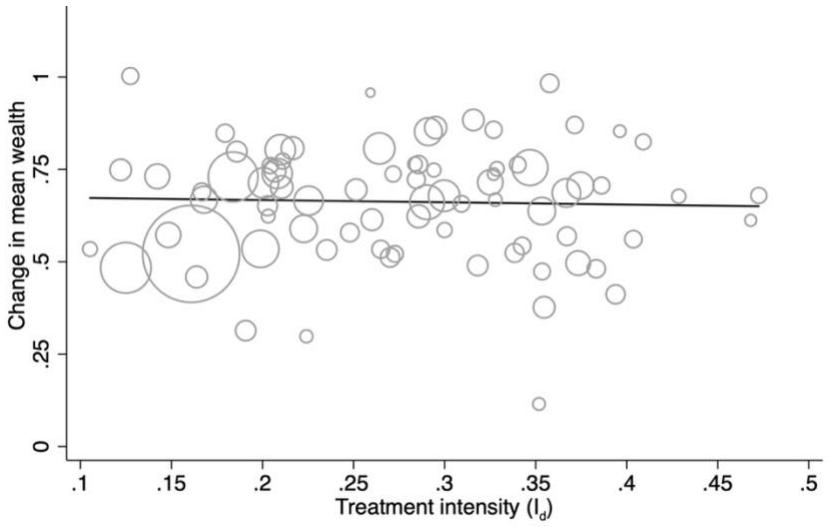
Notes: Specifications are the same as Figure 2, but they do not remove a linear birth-year trend that varies with treatment intensity ($I_d \cdot C_c$); see text for details. Also see notes to Figure 2.

Figure A2: Mean parent schooling and household wealth index in more-treated and less-treated departments



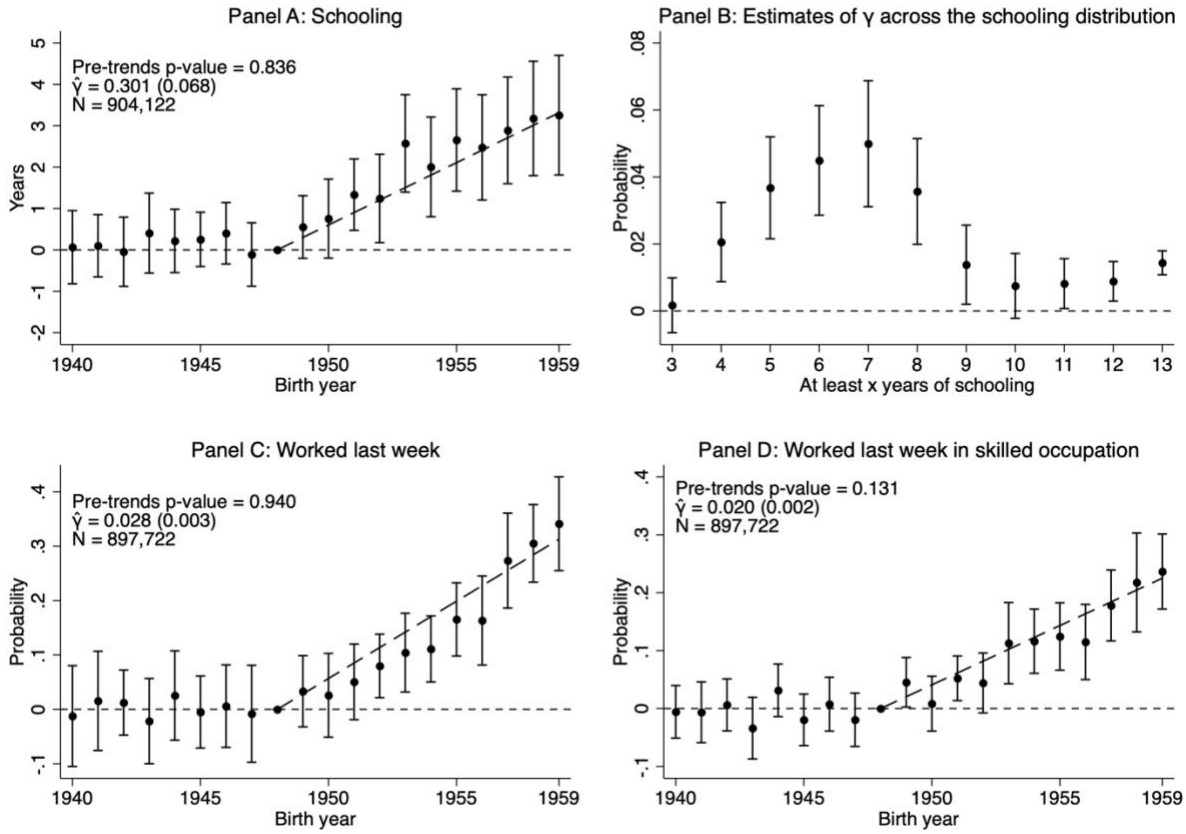
Notes: Panels report the unconditional means for birth cohorts in the pooled census sample of adults (see section 4.B), splitting the sample between adults born in more-treated departments (a treatment intensity of 0.21or more) and less-treated departments. Each panel refers to a census proxy of adult schooling and household wealth (see section 4.B and Appendix B for details).

Figure A3: Treatment intensity and household wealth index, 1960–1970



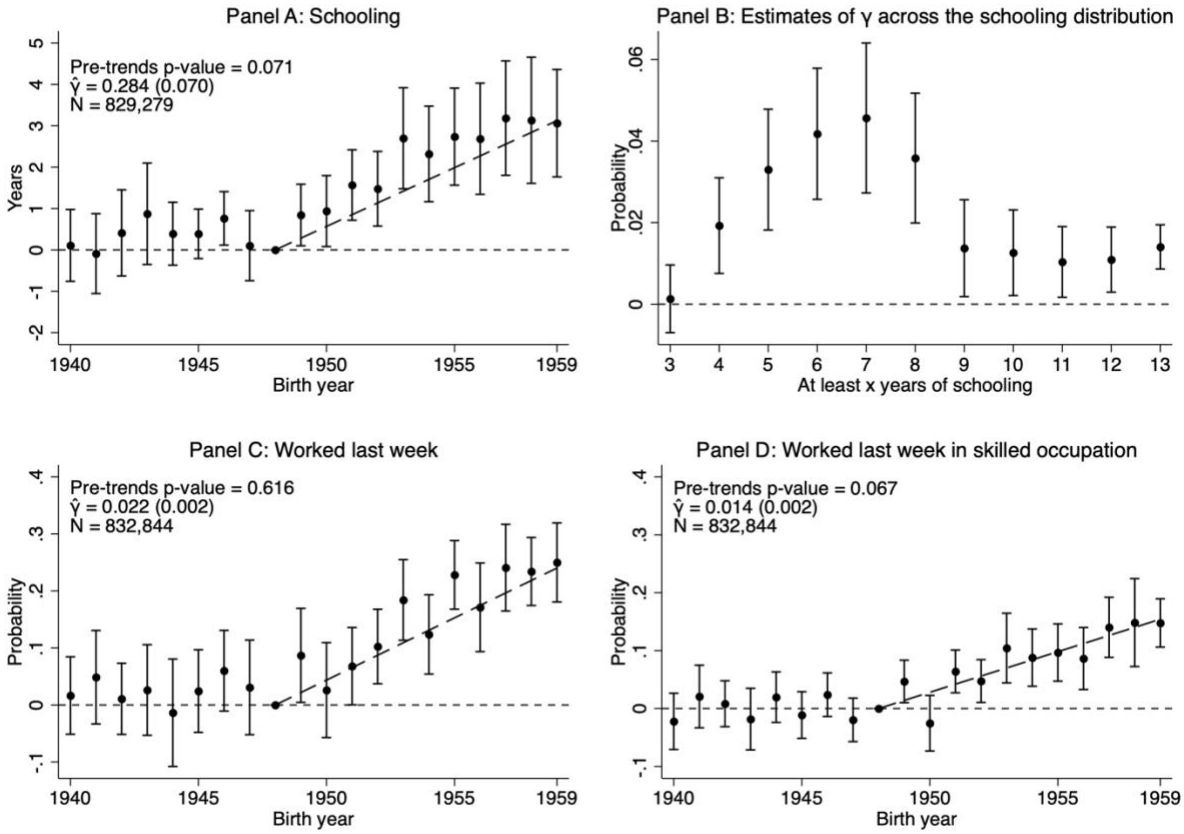
Notes: Y-axis measures the department-level change in the mean household wealth index of 25–65 year-olds (see Appendix B for details).

Figure A4: Effects on first-generation schooling and labor supply (excluding 2017 census)



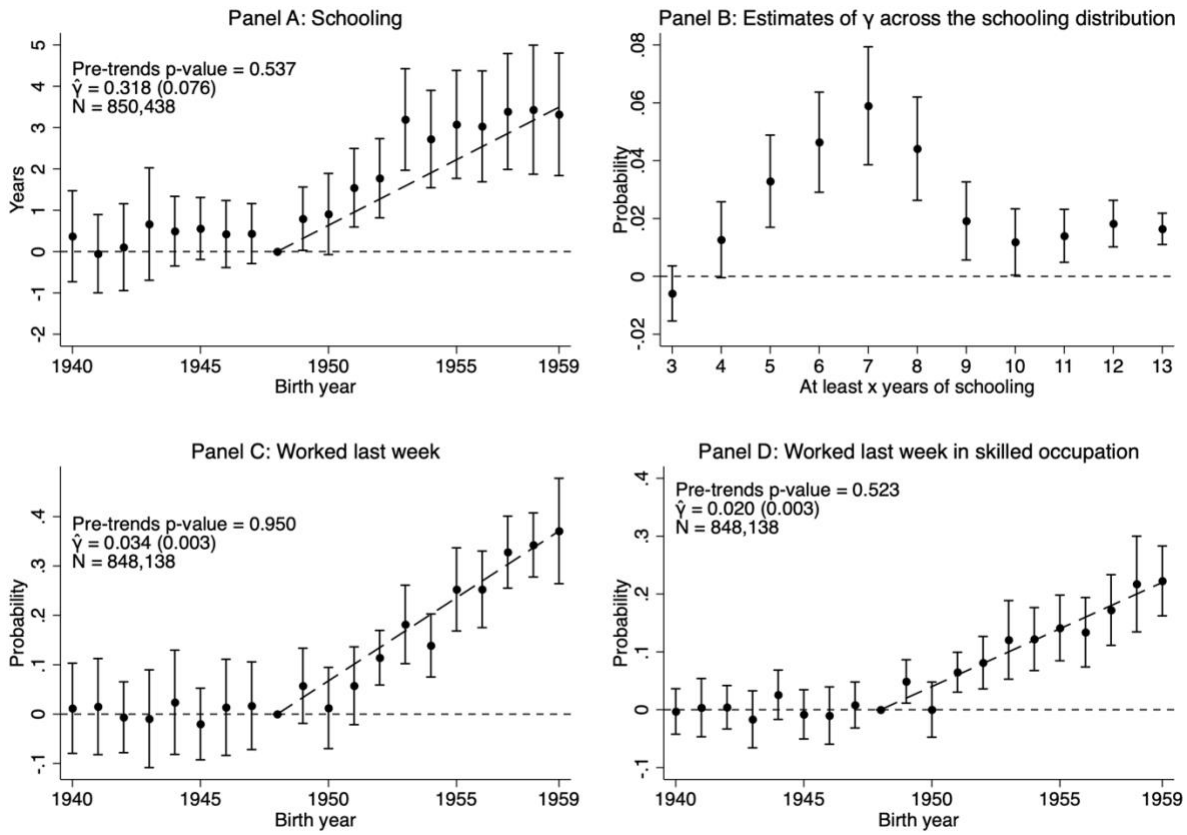
Notes: Specifications are the same as Figure 2, but exclude the 2017 census sample; see text for details. Also see notes to Figure 2.

Figure A5: Effects on first-generation schooling and labor supply (excluding 1982 census)



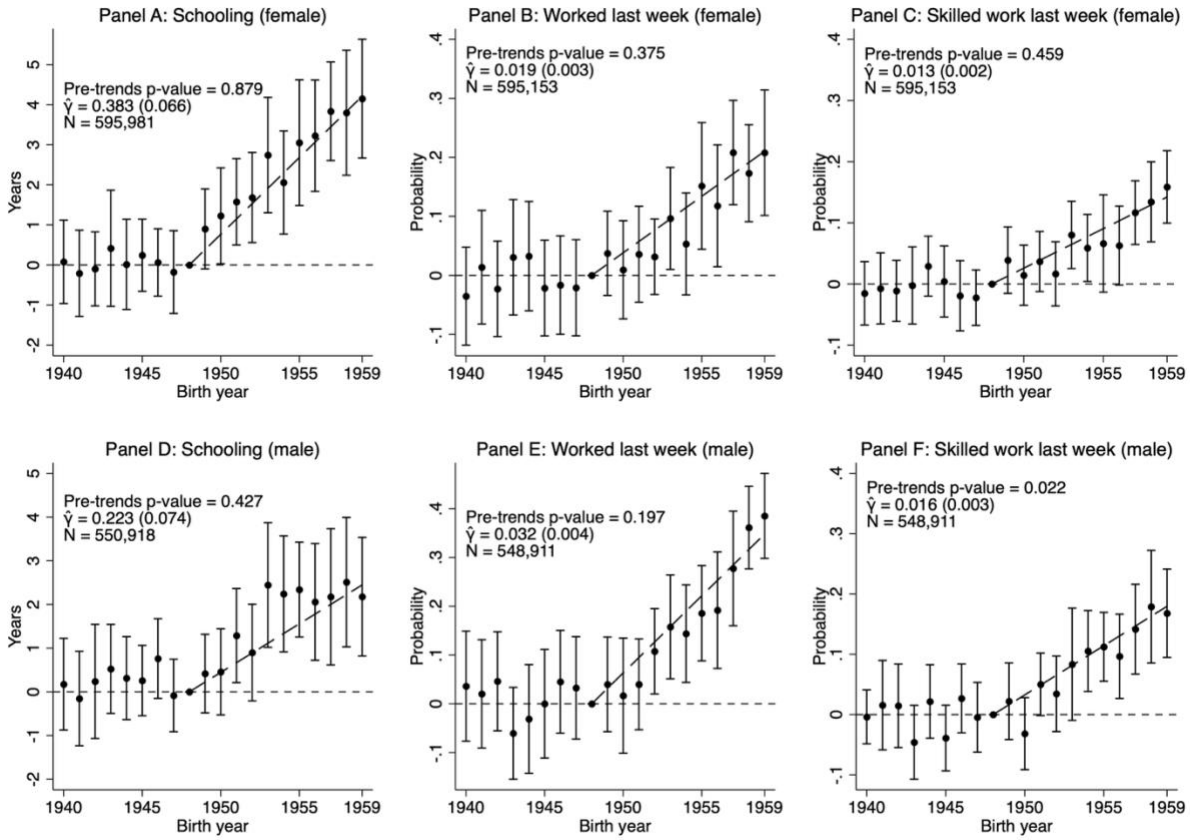
Notes: Specifications are the same as Figure 2, but exclude the 1982 census sample; see text for details. Also see notes to Figure 2.

Figure A6: Effects on first-generation schooling and labor supply
(excluding departments affected by May 1960 earthquake)



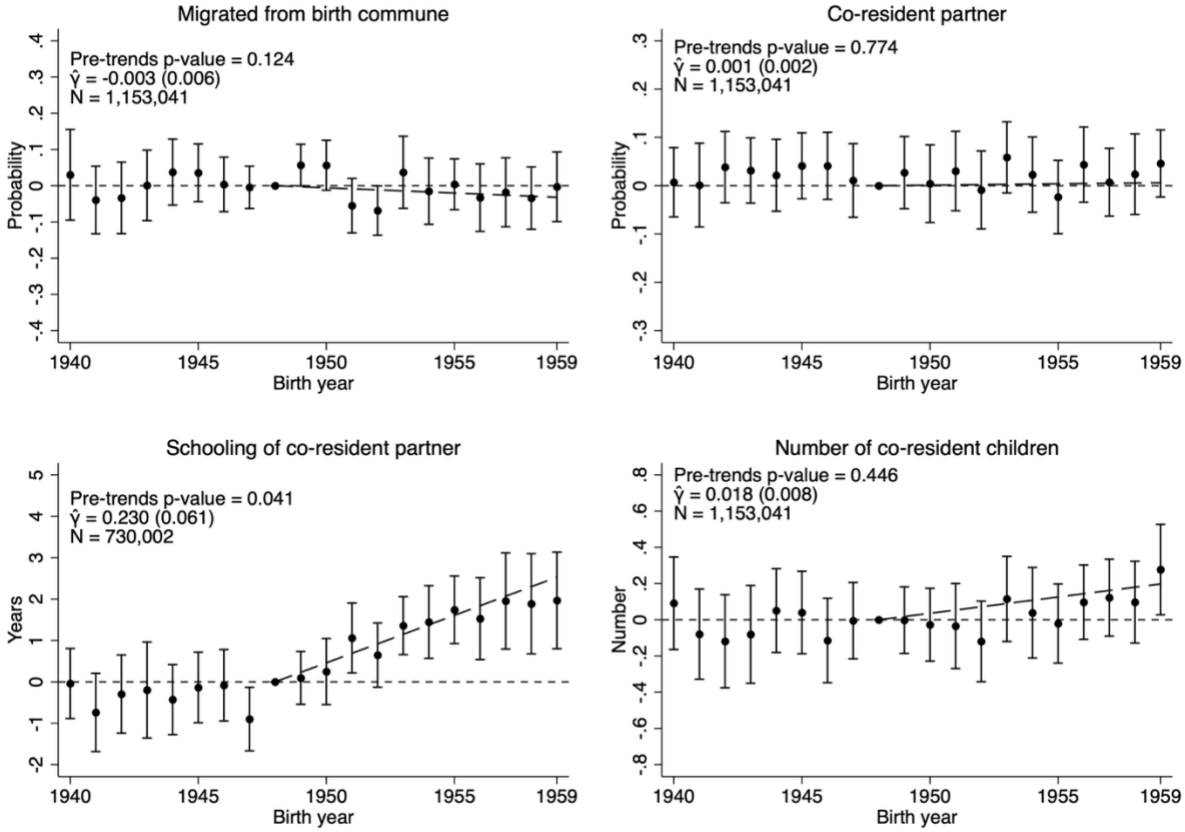
Notes: Specifications are the same as Figure 2, but exclude 30 birth departments most affected by the 1960 Valdivia earthquake; see text for details. Also see notes to Figure 2.

Figure A7: Effects on first-generation schooling and labor supply (female and male samples)



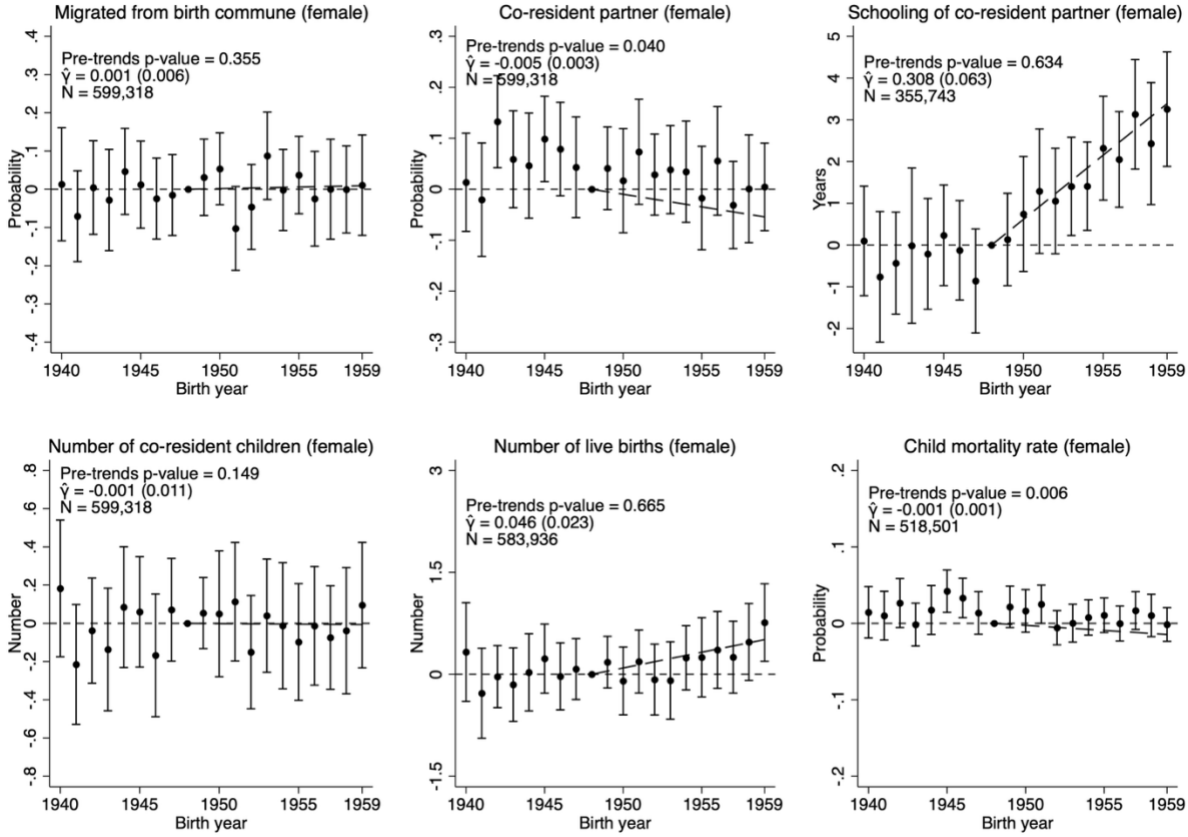
Notes: Specifications are the same as Figure 2, but include only female observations; see text for details. Also see notes to Figure 2.

Figure A8: Effects on first-generation migration and demographic outcomes (full sample)



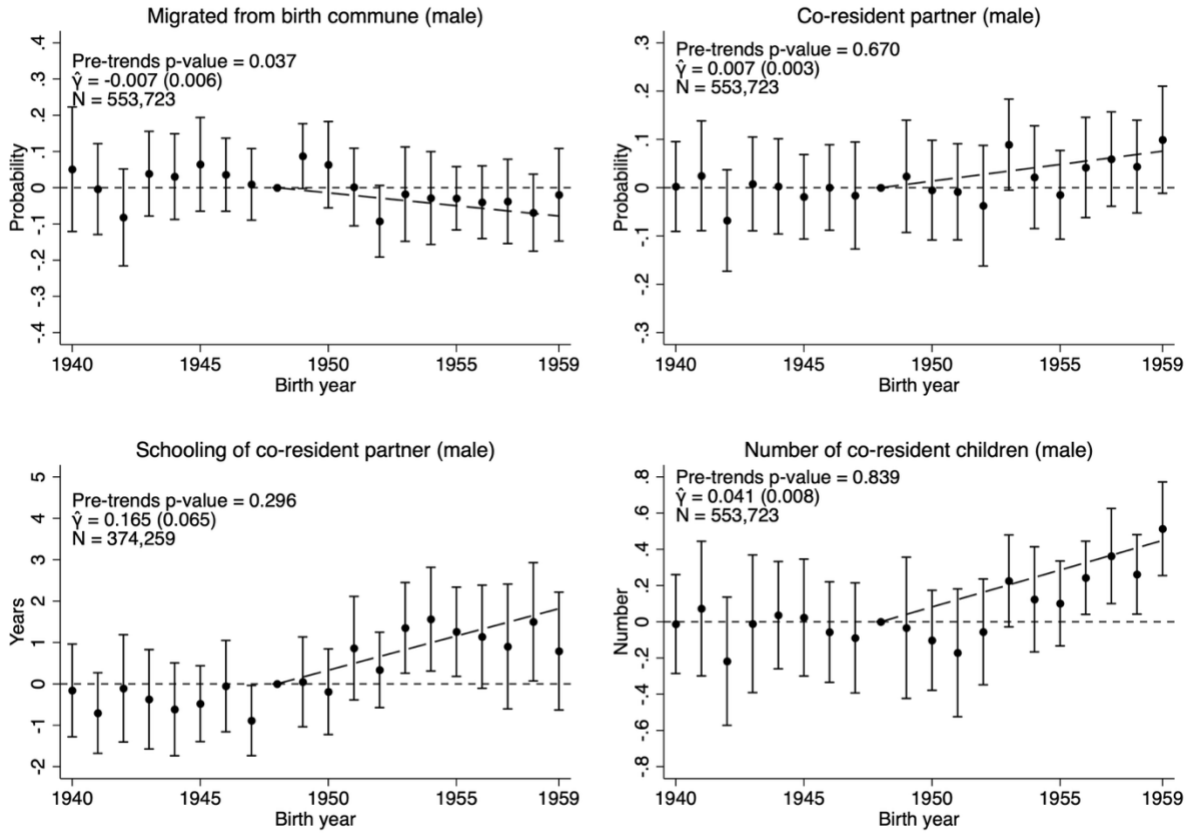
Notes: See notes to Figure 2.

Figure A9: Effects on first-generation migration and demographic outcomes (female sample)



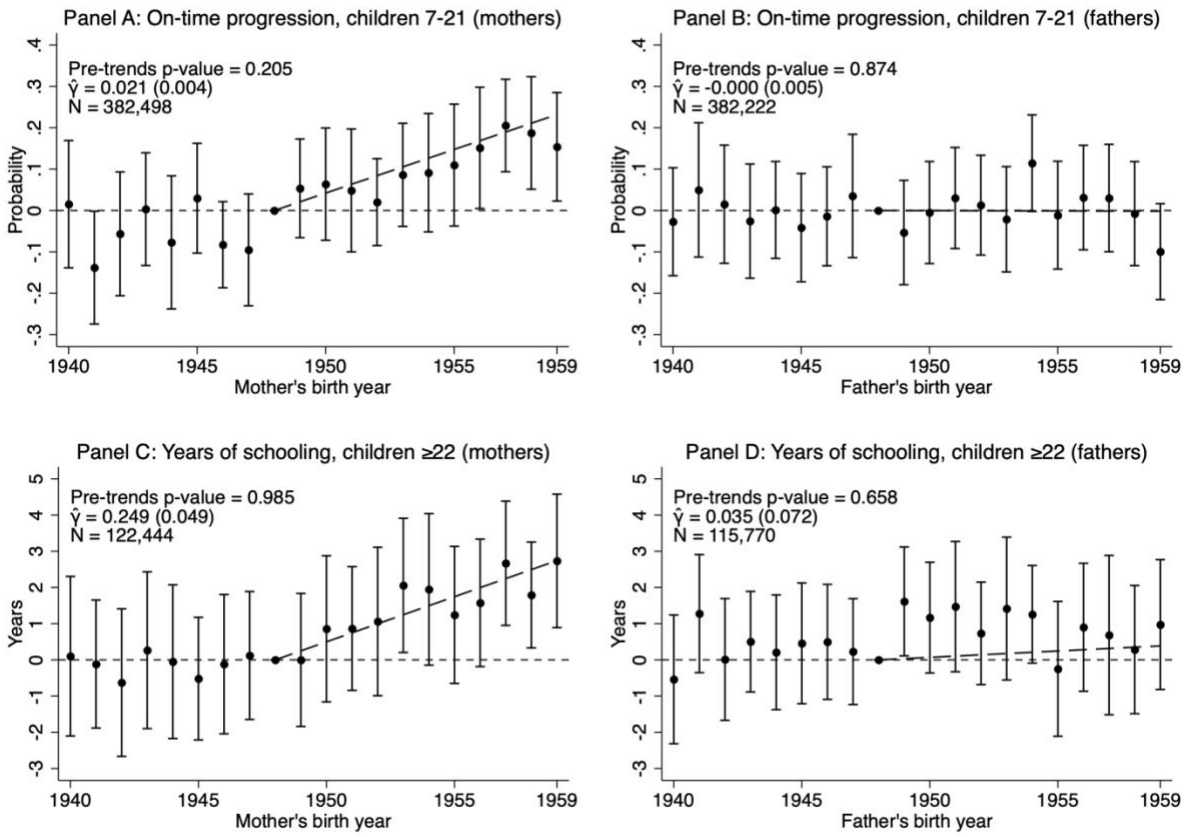
Notes: Specifications are the same as Figure 2, but include only female observations; see text for details. Also see notes to Figure 2.

Figure A10: Effects on first-generation migration and demographic outcomes (male sample)



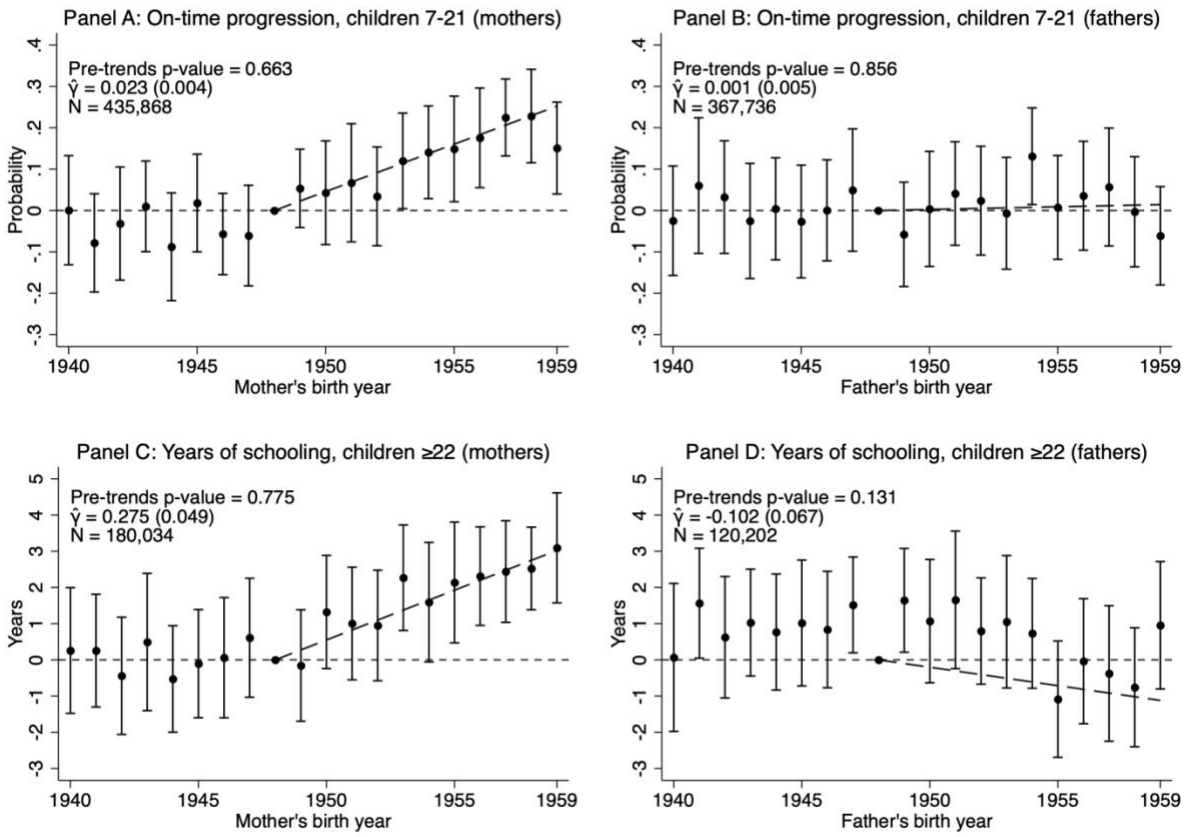
Notes: Specifications are the same as Figure 2, but include only male observations; see text for details. Also see notes to Figure 2.

Figure A11: Effects on second-generation schooling (children with both mother and father in household)



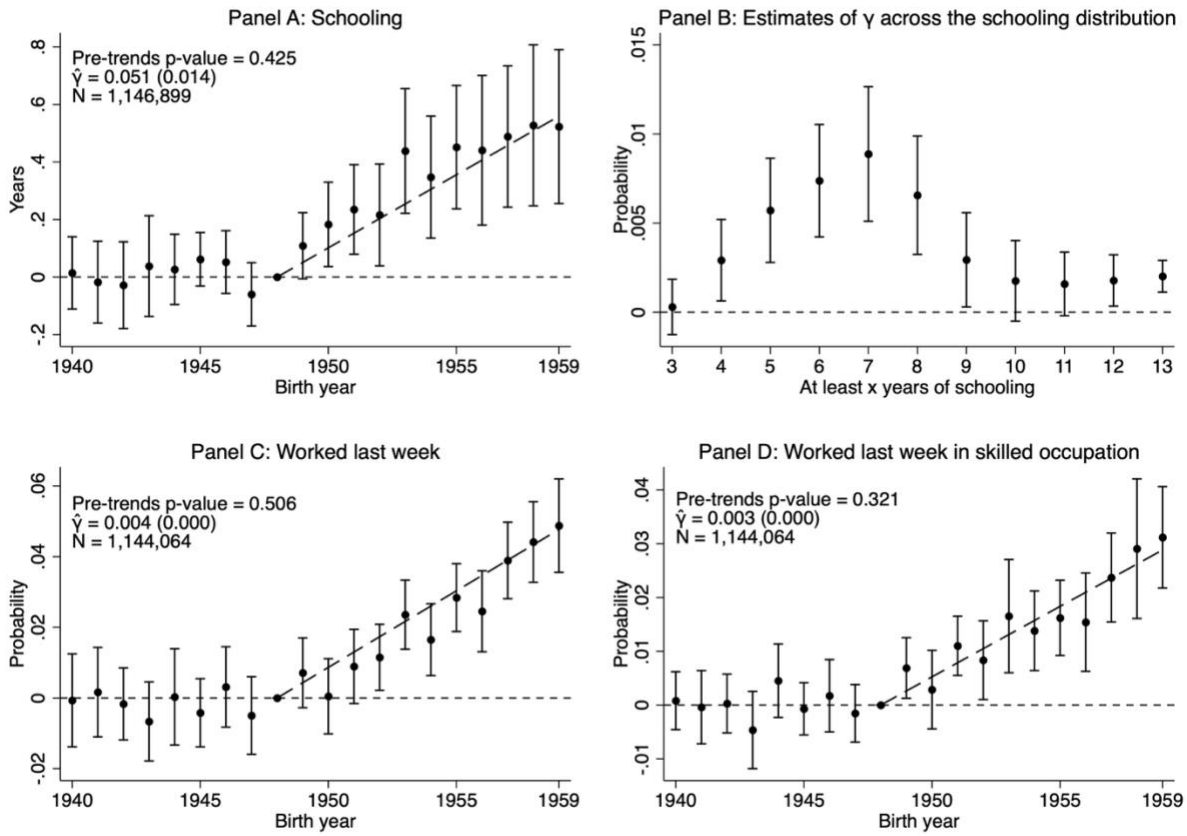
Notes: Specifications are the same as Figure 4, but include only child observations with both a mother and a father in the household; see text for details. Also see notes to Figure 4.

Figure A12: Effects on second-generation schooling (controlling for exposure to longer school days, and excluding indigenous children)



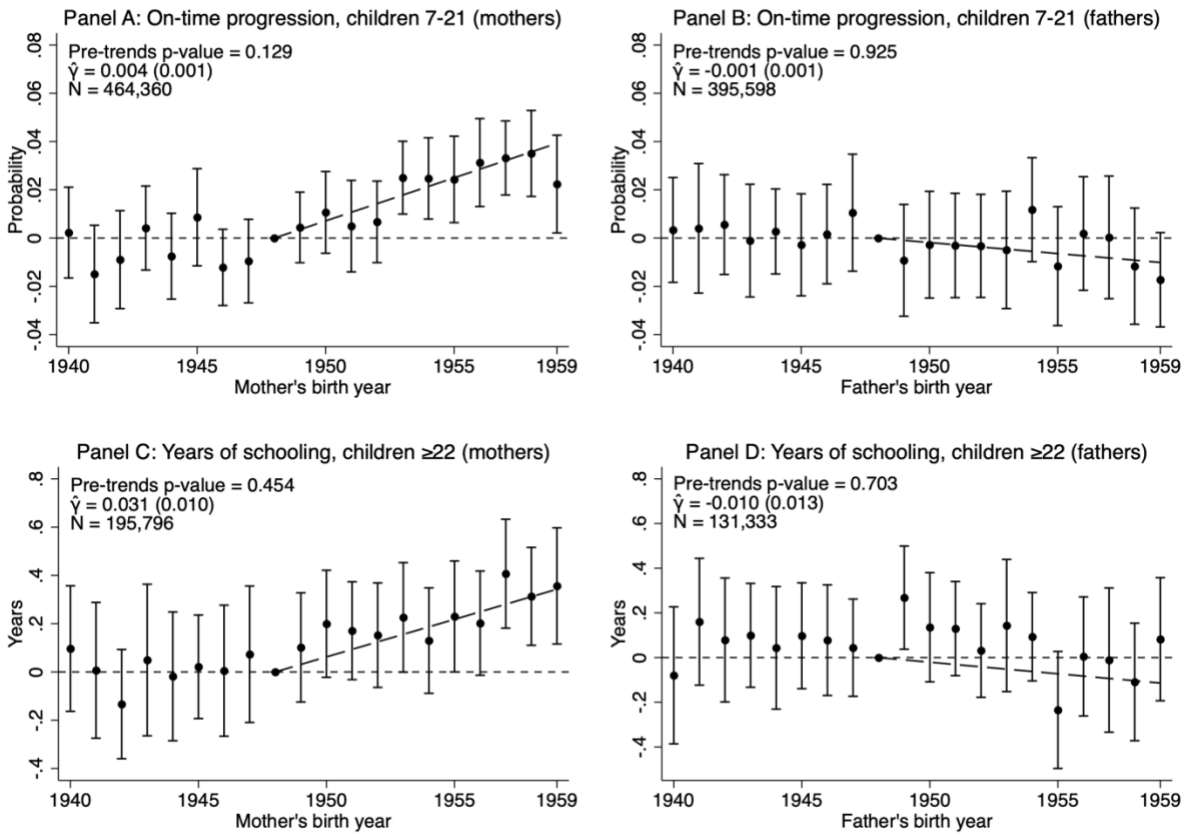
Notes: Specifications are the same as Figure 4, but (1) include an additional control for the expected years of a child's exposure to the longer school day reform, and (2) exclude indigenous students. Also see notes to Figure 4.

Figure A13: Effects on first-generation schooling and labor outcomes,
with binary treatment intensity



Notes: Specifications are the same as Figure 2, except that treatment intensity (I_d) is measured as a binary variable indicating adults born in more-intensively-treated departments (≥ 0.21) versus less-intensively treated (<0.21); see text for details. Also see notes to Figure 2.

Figure A14: Effects on second-generation schooling outcomes,
with binary treatment intensity



Notes: Specifications are the same as Figure 4, except that treatment intensity (I_d) is measured as a binary variable indicating adults born in more-intensively-treated departments (≥ 0.21) versus less-intensively treated (<0.21); see text for details. Also see notes to Figure 4.

Appendix B: Estimation of the Household Wealth Proxy

In order to construct the household wealth proxy, we use 9 variables that are harmonized across the 1960 and 1970 IPUMS census samples. The variables include the number of rooms in the dwelling, and 8 dummy variables indicating home ownership, electricity access, piped water supply, sewage or septic connection, hot water availability, flush toilet availability, bathing facility availability, and radio ownership. Pooling observations across the 1960 and 1970 samples, we construct a single index using the procedure described and implemented in Schwab et al. (2020). In 1960, the variables are only reported for household heads, and cannot be imputed to all household members because there is no information on household structure. The index has a mean of zero and a standard deviation of one.

We use the wealth index in two ways. First, we construct a proxy of household wealth included in equations (1) and (2). We limit the sample to 1970 observations of mothers and/or fathers residing with children born between 1940 and 1959, and calculate means within birth-department-by-birth-year cells (see section 4.B and Appendix Figure A2). Second, we construct department-specific mean changes in the wealth index of 25–65 year-olds between 1960 and 1970 (see section 5.A and Appendix Figure A3).

Reference

Schwab, B., Janzen, S., Magnan, N. P., & Thompson, W. M. (2020). Constructing a summary index using the standardized inverse-covariance weighted average of indicators. *The Stata Journal,* 20(4).

Appendix C: Estimation of the Marginal Value of Public Funds

This appendix describes the data and assumptions used to calculate the MVPF, including additional after-tax earnings (ΔW), additional value-added tax revenues (ΔC), and additional government expenditures (ΔE).

A. Estimation of ΔW and ΔC (cf. Hendren and Sprung-Keyser, 2020)

We calculate ΔW and ΔC for the mean individual in each cohort (1949–1959) born in most-treated departments. We proceed in the following steps.

1. We estimate cohort-specific age-earnings profiles for 18–65 year-olds using the 2006 CASEN household survey. We calculate the mean monthly labor earnings of all adults, including those with zero earnings. We multiply profiles by 0.856, the ratio between the mean earnings of adults born in pre-treatment cohorts (1940–1948) in more-treated departments, and national mean earnings of adults in pre-treatment cohorts. We adjust to annual earnings, and calculate cohort-specific age-earnings profiles by assuming 0.5% annual real-wage growth.
2. We multiply each cohort's profile by the cohort-specific effect on schooling in most-treated departments. This is the product of $\hat{\gamma} = 0.051$ (Appendix Figure A13) and a cohort's years of exposure (1–11).
3. We multiply this by an auxiliary estimate of the return to a year of schooling. In the 2006 CASEN survey, we regress adult earnings—including observations with zeros—on years of schooling, gender, and a full set of age dummies, using Poisson quasi-maximum-likelihood regression. The estimated return to a year of schooling is $e^{\widehat{\beta}} - 1 = 0.168$.
4. We estimate value-added tax revenues (ΔC) by multiplying cohort-specific earnings benefits at each age by the marginal propensity to consume of 0.5 (Barrero et al., 2020) and the value-added tax rate in each year, which is at least 18% in most years (Escalona Caba, 2014).
5. We calculate the present value (in 1965) of after-tax earnings and tax revenues using a discount rate of 5.5% (SNI, 2024).

B. Estimation of ΔE

We estimate ΔE for the mean individual in each cohort (1949–1959) born in most-treated departments. We proceed in the following steps.

1. *National, annual estimates of current expenditures, 1965–1975.* Drawing on archival materials and tabulations from the Ministry of Public Education, Schiefelbein (1976, p. 176) reports current expenditures required for classroom operations in 1970, relative to the pre-policy year 1964, expressed in millions of 1969 escudos. These include teacher salaries, textbooks and instructional materials, building maintenance, and all other recurrent expenditures. We inflate values to 2006 pesos—incorporating the 1975 conversion of 1 peso for 1,000 escudos—and impute this current expenditure to each of the 11 program years from 1965 through 1975.
2. *National annual estimates of capital expenditures, 1965–1975.* Schiefelbein (1976, p. 65) reports a construction cost of 700 escudos per square meter in 1969, which we inflate to 2006 pesos. We compute the annualized cost per square meter, assuming a 30-year building lifetime and a 5.5 percent discount rate (SNI, 2024). We estimate the total square meters attributable to the program by calculating the "excess" construction between 1965

and 1968 (Figure 1.B). Specifically, we linearly interpolate total square meters between 1964 and 1969 and subtract these counterfactual predictions from actual square meters. For example, we estimate that 497,413 additional square meters were constructed in 1965 due to the policy. Multiplying annual excess square meters by the annualized cost per square meter yields annual capital expenditures for each program year from 1965 to 1975.

3. *Allocating national expenditures across more-treated and less-treated departments.* We allocate national expenditures across more-treated and less-treated departments in order to compute the difference in expenditures. We estimate that 63.3 percent and 37.7 percent of treated children reside in more-treated versus less-treated departments, respectively (computed as the product of I_d and the departmental population of 7–15 year-olds in 1960, and then summed within each group). We assume national expenditures are distributed proportionally across the two groups. The implied difference—25.6 percent of total expenditures—represents the additional expenditures in more-treated departments. We multiply national current and capital expenditures in each program year by this share to obtain annual incremental costs in more-treated departments from 1965 to 1975.
4. *Allocating expenditures across treated birth cohorts.* We assume that current and capital expenditures in a given program year are evenly distributed across the 11 birth cohorts eligible in that year. For example, the 1949 cohort was exposed for one year, receiving one-eleventh of 1965 expenditures, whereas the 1959 cohort was exposed for 11 years and therefore receives one-eleventh of expenditures in each program year from 1965 onward. After allocating program-year expenditures across cohorts, we compute the present value of current and capital expenditures for each cohort.
5. *Cohort-specific expenditures per individual.* Finally, we divide each cohort's additional (current plus capital) expenditures by the total size of that birth cohort in more-treated departments. To obtain the latter, we first obtain exact national cohort sizes the 1960 census (Dirección de Estadística y Censos, 1964) and then multiply by 48.3 percent, the estimated share of a cohort residing in more-treated departments.

C. Estimation of Intergenerational Spillovers

We estimate ΔW, ΔC, and ΔE corresponding to the children of the mean individual in each birth cohort (from 1949–1959) born in most-treated departments. We proceed in the following steps.

1. We assume that mothers born from 1949–1959 gave birth to children in 1976–1986, respectively. The assumption is informed by children's mean birth cohorts in the pooled census sample of mothers' co-resident children. We calculate age-earnings profiles for each child birth cohort, following the same procedure as above.
2. We multiply each age-earnings profile by the cohort-specific effect on schooling in most-treated departments. This is the product of $\hat{\gamma} = 0.031$ (Appendix Figure A14) and the parental cohort's years of exposure (1–11).
3. We multiply this by an auxiliary estimate of the return to a year of schooling (see above).
4. We multiply this by (1) the proportion of women in the first-generation sample (0.52), and (2) the mean number of surviving children of first-generation women in more-treated departments, which is 2.1–2.9, depending on women's birth cohorts.
5. We estimate the present value of after-tax earnings and tax revenues as above.

6. We assume that additional schooling expenditures for a mean child occur at age 15, valued at the per-student secondary schooling expenditure in 2006 multiplied by the schooling estimate from step 2. We adjust the per-child estimate as in step 4, and estimate the present value of additional secondary school expenditures.

Reference

Dirección de Estadística y Censos. (1964). *Población del país: Características básicas de la población (censo 1960).* Santiago, Chile: Dirección de Estadística y Censos.