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Working Paper 33798
http://www.nber.org/papers/w33798

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2025

We are grateful for financial support from a Spencer Foundation Small Grant. We are also grateful for early support from the Núcleo Milenio MOVI at the Pontificia Universidad Católica de Chile. David Evans, Raissa Fabregas, Alvaro Hofflinger, Valerie Michelman, Marigen Narea, Harry Patrinos, Andrew Webb, Coleson Weir, and seminar participants at Dickinson College, the Pontificia Universidad Católica de Chile, the University of Delaware, the Economics Workshop on Marginalized Children and Long-term Outcomes, and the Inter-American Development Bank provided helpful advice and comments. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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Targeted Education Transfers Reduced Long-Run and Intergenerational Ethnic Inequality in Chile

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NBER Working Paper No. 33798
May 2025
JEL No. I24, I28, I38, O10, O15

ABSTRACT

Since 1991, Chile has provided large, renewable cash grants to indigenous children in lower-income households, conditional on school enrollment. We estimate intent-to-treat effects of grant exposure on indigenous adults and their children, leveraging variation in expected grant exposure across birth cohorts and never-treated adults, and using fixed effects to absorb unobserved variables shared by adults born in the same year and community. Cohorts with the greatest exposure had 0.6 more years of schooling, 10% more hours worked, and 22% higher labor earnings, reducing pre-treatment ethnic differences. Mothers' exposure increased their children's early-grade test scores and reduced second-generation grant receipt.

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1. Introduction

Latin America has stubbornly high rates of inequality in wealth and income (Levy and Schady, 2013; Chancel et al., 2022). For nearly three decades, conditional cash transfers (CCTs) have been proffered as a way of reducing inequality in the short- and long-run, via poverty-targeted transfers conditioned on child human capital investments (Fiszbein and Schady, 2009; Araujo et al., 2024; Banerjee et al., 2024). Latin America has 39 education-conditioned policies, in part because of robust evidence that CCTs increase school attainment.[1] However, whether CCTs increase long-run welfare of treated children or subsequent generations is unclear. First, increased schooling has not consistently increased student learning (García and Saavedra, 2022), perhaps because of lower school quality (Levy and Schady, 2013). Second, the findings on adult labor supply and earnings are mixed and still nascent, not least because many treated children are not yet in labor markets (Molina Millán et al., 2019; Araujo et al., 2024).[2] Third, evidence on the intergenerational spillovers of CCTs is lacking.

Using a quasi-experimental design, this paper estimates the long-run and intergenerational effects of a scaled-up, government-run, education CCT that has targeted indigenous, lower-income households in Chile since 1991. About 10% of the Chilean population claim indigenous ancestry, predominantly Mapuche. The *Beca Indígena* (or Indigenous Grants) program offers cash grants to indigenous students enrolled in upper-primary grades (five to eight), secondary grades, and tertiary programs in universities and vocational institutes. To be eligible, students must have certification of indigenous status, pass a proxy means test, receive average grades above a low but not failing threshold, and apply for the grant. A government agency prioritizes applicants with relatively lower incomes and higher grades. Once awarded, annual grants are renewable each year within the same level of education (primary, secondary, or tertiary). The

[1] For a comprehensive review, see García and Saavedra (2022). On schooling attainment, see Araujo and Macours (2021); Attanasio et al. (2021); Baird, McIntosh, and Özler (2019); Barham, Macours, and Maluccio (2024); Barrera-Osorio, de Barros, and Filmer (2023); Cahyadi et al. (2020); Duflo, Dupas, and Kremer (2025); Parker and Vogl (2023); and Barrera-Osorio, Linden, and Saavedra (2019). Cash transfers are also popular in Latin America because of political returns for incumbent parties (Galiani et al., 2019; Manacorda, Miguel, and Vigorito, 2011).

[2] A meta-analysis finds heterogeneous test score outcomes and cannot reject the null of a zero effect, although one-third of studies find a positive effect (García and Saavedra, 2022). Studies in Mexico and Nicaragua find mixed results on labor earnings that depend on gender (Araujo and Macours, 2021; Parker and Vogl, 2023; Barham et al., 2024). Experiments in Ghana, Honduras, and Indonesia find minimal effects on incomes (Cahyadi et al., 2020; Duflo et al., 2024; Molina Millán et al., 2020).

grants at each level are, respectively, 9%, 18%, and 56% of the median household income per capita of recipients.

Our research design leverages variation in exposure to indigenous grants across birth cohorts and indigenous status. Indigenous children born before 1975 were not exposed to grants, while birth cohorts in 1975 and later were increasingly exposed as the expanding program reached a larger proportion of each indigenous birth cohort, culminating in 2.4 years of expected grants years for indigenous individuals born in 2000. Non-indigenous children were never treated. The identification strategy is credible because potentially-treated and never-treated children are born in the same years and the same communes—Chile's smallest territorial units. We can therefore include fixed effects for every combination of 36 birth cohorts and over 300 birth communes. The unusually granular fixed effects control for unobserved variables such as local economic conditions, the quality of local public schools, and policies targeted to lower-income communities, all of which might be changing over time and space. We also include fixed effects for indigenous-status-by-birth-commune. Together, the fixed effects ensure that we estimate the difference-in-differences within birth communes.

We estimate event study regressions in large samples of Chilean adults born between 1965 and 2000, as well as their children. Our preferred intent-to-treat (ITT) estimates show that birth cohorts of indigenous adults with the greatest exposure to indigenous grants completed 0.6 additional years of schooling, had 22% higher labor earnings (including adults with zero earnings), and increased hours worked by 10%. Across all cohorts, the average causal response (ACR) to one year of expected grants is an increase of 0.38 years of school, 13% of labor earnings, and 6% of hours worked. Labor supply particularly increases on the extensive margin, increasing full-time work relative to none. The effects represent a substantial reduction in ethnic inequality. The marginal value of public funds is $190 in after-tax earnings benefits for each dollar of net government expenditure (Hendren and Sprung-Keyser, 2020).

We rule out several threats to internal validity. We find no evidence of differential pre-treatment trends in years of schooling or labor market outcomes. We also use an auxiliary sample of children and youth and find no evidence of differential post-treatment trends—also within birth communes—in childhood exposure to variables that could have affected adult schooling and earnings. These include children's household income per capita and exposure to concurrent education programs that affected indigenous and non-indigenous students (e.g., Chay, McEwan,

and Urquiola, 2005; Dominguez and Ruffini, 2023). The only post-treatment trend that we identify in the auxiliary sample is in childhood exposure to indigenous grants themselves. In section 6, we discuss and rule out other threats to internal validity, including endogenous selection into indigenous status.

In addition to increasing years of schooling, the grants increased learning per year of school. We find convergence of 0.1 to 0.2 standard deviations between the mean test scores of fourth, eighth, and tenth grade indigenous students, relative to non-indigenous students. This convergence largely eliminated ethnic test score gaps among observationally-similar students within communes. Grant exposure did not affect other plausible channels of earnings effects, including migration to other communes, improved adult health, or reductions in early fertility of women. However, grant exposure did increase the schooling of adults' co-resident partners, via assortative mating.

The increased human capital of indigenous mothers and their partners raises the possibility of intergenerational effects on children. Using national data on second- and fourth-grade test scores, we show that an additional year of expected grant exposure of mothers increased children's scores by 0.13 to 0.17 standard deviations. The effects are not explained by increases in children's own grant exposure. On the contrary, mothers' exposure to grants lowers the probability that their children receive the grants, due to the combination of rising incomes and a well-enforced proxy means test. Overall, we document a transformation in the socioeconomic environments of children, including increased parental schooling, household income per capita, child enrollment in tuition-charging private schools, and child exposure to school peers with more-educated parents.

The paper makes four substantial contributions to the economics literature. First, we report internally-valid and precise estimates of the long-run impact of a government-implemented, scaled-up CCT—relative to a never-treated control group—on labor market outcomes. Our paper is most closely related to Parker and Vogl (2023), who leverage birth cohort and continuous geographic variation in program intensity of Mexico's Progresa program. In contrast, our research design has a never-treated control group, which allows us to identify intent-to-treat (ITT) effects under a standard parallel trends assumption in an event study design, even with continuously-measured dosage groups (Callaway, Brantley, and Sant'anna, 2024). The never-treated control group also lets us transparently identify ITT effects of indigenous cohorts'

lifetime exposure to grants, rather than differential exposure due to "early" versus "later" treatment phase-in (Araujo and Macours, 2021; Barham et al., 2024). Related evidence from Chile is limited to the effects of means-tested tertiary loans (Solis, 2017; Bucarey et al., 2020; Aguirre, 2021).

Second, this is the first paper to identify intergenerational effects of a CCT on children's human capital and social safety net use in a developing country setting. Most evidence on the intergenerational effects of human capital investments is from high-income countries.[3] Limited developing country evidence from secondary scholarships (Ghana) or school construction (Indonesia) point to potential intergenerational effects on early education and health outcomes (Duflo, Dupas et al., 2024; Akresh, Halim, and Kleemans, 2023; Mazumder, Rosales-Rueda, and Triyana, 2023). We also find that maternal exposure to a well-targeted and effective CCT reduced the likelihood of second-generation participation in the same program, in contrast with evidence from developed countries on the intergenerational persistence in the use of social assistance (Hartley et al. 2022) and disability insurance (Dahl and Gielen, 2021).

Third, we report the first causal evidence of how a scaled-up, government policy can reduce ethnic inequality, both directly and via intergenerational spillovers. People of indigenous ancestry in Latin America have lower levels of schooling attainment, test scores, and earnings than non-indigenous adults (McEwan, 2004, 2008a; Ñopo, 2012; World Bank, 2015; Näsland-Hadley and Santos, 2022). We are not aware of research that comprehensively describes or causally explains trends in these differences across many years or birth cohorts, in contrast to a substantial U.S. literature on the school-related causes of racial inequality (Card and Krueger, 1992; Cook and Evans, 2000; Aaronson and Mazumder, 2011).

Fourth, we contribute to the literature on the targeting of social policies (Araujo et al., 2024; Ibarrarán et al., 2017; Banerjee et al., 2024). The literature advocates the targeting of education transfers to students likely to reap the greatest program benefits (Araujo et al., 2024). That is because CCTs are generally less cost-effective than other interventions at increasing a unified metric of enrollment and learning (Angrist et al., 2025). CCTs also have weaker schooling

[3] For example, tertiary education or Medicaid exposure improved second-generation birth outcomes (Currie and Moretti, 2003; East et al., 2023). The intergenerational effects of compulsory schooling laws is mixed (Oreopoulos et al. 2006; Black et al., 2005). Barrios-Fernández et al. (2024) document intergenerational transmission of elite educational opportunities—but not human capital—across relatively higher-income samples of Chileans than those of our paper.

effects if policies are untargeted (Dustan, 2020), if there is leakage to less-poor households (de Hoyos, Attanasio, and Meghir, 2021), and even among relatively less poor households in samples of extremely poor households (Galiani and McEwan, 2013). The Chilean policy provides suggestive evidence that indigenous status deserves more scrutiny as a component of proxy means tests in Latin America.[4]

2. Indigenous Grants in Chile

Since 1991, the *Beca Indígena* (or Indigenous Grants) program has offered renewable cash grants to indigenous students in exchange for school enrollment. This section describes the minimum requirements for grant eligibility, the procedures for grant application and renewal, grant values, and the program's scale-up between 1991 and 2021.

There are four minimum requirements for grant eligibility. First, individuals must obtain certification of indigenous ancestry from CONADI (the *Corporación Nacional de Desarrollo Indígena*), a government agency responsible for indigenous affairs. CONADI uses birth certificates to verify that the individual, a parent, or a grandparent has an indigenous surname.[5] Second, indigenous children and youth must be enrolled in upper-primary grades (from five to eight), secondary grades, or a tertiary program in a university or vocational institute. Third, students must demonstrate a prior-year grade point average of at least 5 out of 7 points in primary or secondary grades, or 4.5 out of 7 points in tertiary grades, a standard higher than failing thresholds.[6] Fourth, households must be in the lowest 70% of socioeconomic vulnerability, as defined by a national proxy means test (DIPRES, 2021).

Conditional on eligibility, indigenous students must apply to receive grants. The application process is managed by JUNAEB (the *Junta Nacional de Auxilio Escolar y Becas*), using data from other government agencies on indigenous status, school enrollment and grades, and the proxy means test. Applications typically exceed the number of grants, and JUNAEB chooses applicants with relatively lower incomes, but relatively higher grade point averages (JUNAEB,

[4] Brazil's *Bolsa Familia* program is the only other large-scale program to directly target indigenous populations (Ibarrarán et al., 2017). Smaller scale tertiary scholarships target indigenous students in Chile, Peru, and Mexico. In Honduras, a cash transfer had heterogenous effects by children's indigenous status (Molina Millán et al., 2020).
[5] Mapuche are 78% of the national indigenous population, followed by Aymara (9%), Diaguita (7%), Quechua (2%), and several smaller groups. Percentages are weighted estimates from the 2022 CASEN household survey.
[6] In publicly-funded schools, students fail a grade if they score below 4 in one subject and their average is less than 4.45 or they score below 4 in two subjects and their average is less than 4.95 (Díaz et al., 2021).

2024). Once a grant is awarded, it can be easily renewed for higher grades in the same level of schooling—whether primary, secondary, or tertiary—by meeting the minimum eligibility criteria. However, students must re-apply to receive grants for the next level of schooling.

Grant amounts increase across schooling levels. In 2021, the annual grant was 100,550 pesos (US$118) in primary grades; 208,280 pesos (US$245) in secondary grades; and 654,600 pesos (US$770) in tertiary grades (expressed in 2022 pesos). These were 9%, 18%, and 56% of the median annual household income per capita of grant recipients. Nominal grant amounts increased in many years, though inflation modestly eroded their value (Appendix Figure A1). Annual primary and secondary grants are paid in two installments and tertiary grants are paid in 10 installments. JUNAEB deposits grants into mandatory accounts in *BancoEstado,* Chile's state-owned bank. Deposits for primary-aged children are directed to a parent or guardian account, while others receive deposits in individual accounts.[7] JUNAEB places no restrictions on how grant funds can be spent.

The program expanded over three decades that spanned democratically-elected governments across the political spectrum. Panel A of Figure 1 illustrates the scale-up between 1991 and 2021, noting that school and calendar years overlap. The Ministry of Education piloted the program in 1991, awarding 300 tertiary grants. In the following year, the Ministry funded 2,500 primary grants; 1,000 secondary grants; and 750 tertiary grants. In subsequent years, the government steadily increased grants at all levels.[8] By 2021, over 92,000 grants were awarded. Despite this growth, most indigenous students did not receive grants, even in later program years. The next section uses census and administrative data to describe the expected years of grants received by indigenous adults across many birth years.

3. Research Design

This section explains our approach to causal identification. First, we describe the expected years of grants received by adults, which depend on indigenous status and birth year. Second, we describe an event study specification for the estimation of intent-to-treat (ITT) effects in successive birth cohorts of adults, and a complementary specification for the estimation of the

[7] Girls younger than 12, and boys younger than 14 receive deposits via a parent or guardian.
[8] The number of tertiary grants includes two small programs, also administered by JUNAEB, that use similar targeting criteria to defray housing expenses during tertiary studies.

average causal response (ACR) to an additional year of expected grants. Third, we discuss the estimation of intergenerational effects on the children of indigenous mothers.

A. Differential Grant Exposure by Ethnicity and Birth Year

Individual exposure to indigenous grants varies in two ways. First, non-indigenous children and youth were never eligible to receive grants. Second, indigenous children and youth were increasingly eligible in successive birth years. Using census and administrative data, we calculate E_{ic}, the expected years of indigenous grants received by adult i born in year c. It is the cohort-specific sum of annual probabilities of receiving a grant during 12 eligible grades:

$$E_{ic} = \sum_{c=1965}^{2000} \sum_{g=c+10}^{c+21} \frac{G_c^g}{N_c} \times I_{ic}$$

where I_{ic} takes the value of 1 for indigenous adults (such that $E_{ic} = 0$ for non-indigenous adults). N_c is the number of indigenous children born in year c that were alive just before grant eligibility in the fifth grade.[9] G_c^g is the number of indigenous grants awarded to those born in year c in each year g between $c + 10$ (the year of the fifth primary grade) and $c + 21$ (the year of the fourth tertiary grade). For example, the 2000 birth cohort was first eligible in $g = 2010$, such that $\frac{G_{2000}^{2010}}{N_{2000}} = \frac{5{,}178}{33{,}124} = 0.156$.[10] Summing across grade-specific probabilities yields the expected grant years for each cohort.

Panel B in Figure 1 shows that E_{ic} increased from 0.04 years for the indigenous cohort born in 1975 to 2.4 years for the 2000 cohort. The growth in E_{ic} reflects two aspects of program scale-up. First, cohorts born before 1983 were exposed to grants for fewer than 12 years, since they started fifth grade before 1991. Second, the grade-specific probabilities of receiving a grant increased as the total number of funded grants increased. For example, indigenous cohorts born in 1979 and 2000 received grants when they were 13 years old—or approximate eighth-

[9] We use 2017 census microdata (https://redatam-ine.ine.cl) to calculate the size of indigenous birth cohorts. Indigenous status is based on an affirmative response to one of nine mutually-exclusive indigenous groups, omitting the category of "other" (Appendix Table A1). We adjust 2017 cohort sizes with a life table, to reflect expected cohort sizes at 5 to 9 years of age.

[10] For each school year, we assume that grants were evenly allocated across eligible grades within a schooling level. In this example, 20,712 primary grants in 2010 were evenly divided across four primary grades.

graders—with probabilities of 0.01 and 0.2, respectively. Appendix Table A2 reports all values of $\frac{G_c^g}{N_c}$, which never exceed 0.24.

The cohort trend in E_{ic} motivates a quasi-experimental design, which we use to identify intent-to-treat effects on successive birth cohorts of indigenous adults, relative to non-indigenous adults. However, we emphasize that the design does not rely on individual-level grant receipt for two reasons. First, individual grant receipt is endogenous to many determinants of adult outcomes, given the targeting rules described in section 2. Second, this paper's survey data do not retrospectively measure the cumulative grants received by adults. That said, the survey data confirm that only indigenous children receive grants, and that child recipients have lower household incomes than indigenous non-recipients (Appendix Table A3). Instead, we use the exogenous expansion of the program to identify ITT effects on entire birth cohorts of indigenous adults.

B. Estimation

The quasi-experimental research design compares later and earlier birth cohorts (the first difference) across indigenous and non-indigenous adults (the second difference). We estimate event study regressions in samples of adults born between 1965 and 2000:

$$Y_{icmt} = I_{icmt}\left[\sum_{e=1965}^{1972}\gamma_e 1\{c=e\} + \sum_{e=1975}^{2000}\lambda_e 1\{c=e\} + \sum_{m\in M}\theta_m\right] + \delta_{cm} + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt} \quad (1)$$

where Y_{icmt} is the adult outcome (e.g., years of schooling) of adult *i,* born in year *c* and commune *m,* and surveyed as an adult in year *t.* I_{icmt} is a dummy variable indicating indigenous status. The γ_e and λ_e are coefficients for each pre-treatment and post-treatment birth year, relative to the omitted years of 1973 and 1974. The difference-in-differences design has a common start and increasing treatment intensity thereafter (Goodman-Bacon, 2021; Miller, 2023), thus avoiding challenges to interpreting two-way fixed effects models with staggered rollout (Roth et al., 2023).

Causal identification hinges on the inclusion of fixed effects for each combination of 36 birth years and 318 birth communes (δ_{cm}), as well as separate intercepts for indigenous adults born in each commune ($\sum_{m\in M}\theta_m I_{icmt}$). The δ_{cm} absorb determinants of adult outcomes that are shared by indigenous and non-indigenous individuals born in the same year and commune, Chile's

smallest territorial unit.[11] These include local economic conditions, the quality of school and health services provided by municipal governments, and national policies that disproportionately target lower-income or rural areas. Together, the fixed effects ensure that difference-in-differences are estimated within birth communes. We use communes of birth (and not later residence) because migration decisions are potentially endogenous to grant exposure.

The specification includes fixed effects for survey years (μ_t), since we pool adult observations from multiple household surveys. The controls in X'_{icmt} include age dummies, a gender dummy, and parental schooling. We apply multiway clustering of standard errors by birth communes and communes of adult residence.

We conduct two tests of the identifying assumption of conditional parallel trends. First, there should be no "effects" before the roll-out of indigenous grants. Thus, we visually examine the $\hat{\gamma}_e$ for evidence of pre-treatment trends in outcomes and test the null hypothesis that $\gamma_e = 0 \; \forall \; e$. Second, we test for post-treatment trends in a range of household and school variables that are plausibly associated with adult outcomes, using an auxiliary sample of children and youth whose birth years overlap with our main sample.

To improve power, we also report coefficient estimates from a simpler specification that imposes parallel pre-treatment trends ($\gamma_e = 0 \; \forall \; e$), and includes λ_e for three groups of cohorts.[12] Cohorts from 1975 to 1981 were exposed for less than 12 school years (an average E_{ic} of 0.13 years). Cohorts from 1982 to 1991 were exposed for 12 years during school years with fewer available grants (an average E_{ic} of 0.65 years). Finally, cohorts from 1992 to 2000 were exposed for 12 years, in school years with more available grants (an average E_{ic} of 1.86 years).

We report a complementary specification with the continuous measure of expected grant years (E_{ic}):[13]

$$Y_{icmt} = \tau E_{ic} + \sum_{m \in M} \theta_m I_{icmt} + \delta_{cm} + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt} \qquad (2)$$

[11] Chile has 346 communes, 28 of which were created between 1981 and 2004. To ensure consistent comparisons across self-reported birth communes in multiple survey and birth years, we assign adults to 318 super-communes based on pre-1981 boundaries (Appendix Table A4).

[12] The specification assumes that $E_{ic} = 0$ in birth years from 1970 to 1974. In Appendix Table A5, we present ITT regressions that reassign these cohorts from the pre-treatment group to the group of partially-treated birth years (from 1970 to 1981), and use a control group with fewer birth years (from 1965 to 1969). The estimates are similar to the preferred ITT estimates.

[13] Recall that $E_{ic} = 0$ for non-indigenous adults and for any birth cohort not exposed to grants. We carry forward the earlier assumption that $E_{ic} = 0$ for indigenous cohorts before 1975.

The coefficient τ is the average causal response to one year of expected grant exposure under a "strong" parallel trends assumption (Callaway et al., 2024). The assumption can be formulated as the standard parallel trends assumption—relative to never-treated units—and an additional restriction on heterogeneity across values of E_{ic}. In this context, the ITT effects for any two birth cohorts must be the same, had they received the same E_{ic}. We later discuss the validity of the added assumption.

C. Intergenerational Effects

We extend equations (1) and (2) to assess whether mothers' grant exposure affected the school outcomes of their co-resident children. These specifications use samples of mothers born between 1965 and 1991, a restriction imposed by available data. The dependent variable is Y_{ijcmt}, a schooling outcome of child j born to mother i. Other subscripts and fixed effects are the same as above. However, the X'_{ijcmt} also include indicators of child gender, birth year, and age. In the results, we discuss and rule out two important threats to internal validity. First, our second-generation birth years are during a period of relative stability in the number of available grants, removing the possibility that "intergenerational" effects are confounded with the second generation's increasing grant exposure. Second, we show that intergenerational effects are not an artifact of first-generation effects on fertility or the likelihood that children are co-resident with mothers at the time of a survey.

4. Data

We estimate the long-run effects of exposure to indigenous grants using first-generation samples of individuals born between 1965 and 2000. We estimate intergenerational spillovers in second-generation samples of co-resident children. In each case, we use multiple years of the CASEN household survey (*Encuesta de Caracterización Socioeconómica Nacional*), complemented by multiple years of Chile's national SIMCE tests (the *Sistema de Medición de la Calidad de Educación*).[14] Table 1 summarizes five samples used to implement and evaluate the research design.

[14] CASEN data are available at https://observatorio.ministeriodesarrollosocial.gob.cl/encuesta-casen. SIMCE data are available by application to https://www.agenciaeducacion.cl/simce/.

A. First-Generation Samples

We estimate equations (1) and (2) with a first-generation sample, henceforth FG1, that includes adults between 22 and 57 years-old who were born from 1965 to 2000. It pools observations from CASEN surveys in 2006, 2009, 2011, 2013, 2015, 2017, 2020, and 2022. The surveys record communes of birth and an ancestry-based indicator of indigenous status that is consistent with formal eligibility rules (Appendix Table A1).[15] The main dependent variables are (1) years of schooling; (2) monthly labor earnings in 2022 pesos, including individuals with zero earnings; (3) whether individuals report any work in the week before the survey; and (4) the weekly hours that individuals typically work in their main job, equal to zero for individuals who do not typically work.

We construct additional dependent variables to assess causal channels of effects. A migration dummy variable indicates whether adults reside outside their birth commune. Health variables include an overall health "grade" between 1 and 7 (but standardized to a z-score) and a dummy variable indicating a recent illness or accident. A dummy variable indicates that women had a live birth at age 17 or younger. Finally, we measure whether adults have a co-resident partner and, when present, the partner's indigenous status and years of schooling.

A second sample of the same generation (FG2) includes children and youth between 6 and 21 years-old who were born between 1985 and 2000. The sample facilitates tests for differential post-treatment trends in household and school variables, as discussed in section 6. A third sample of this generation (FG3) pools 21 SIMCE rounds that contain fourth, eighth, and tenth grade test scores. The grade-specific rounds were collected between 1997 and 2015 from students who were born between 1983 and 2000. The sample facilitates test for effects on language and mathematics test scores, as described in section 7.[16]

B. Second-Generation Samples

[15] We use the IPUMS 10% sample of the 2002 census (https://international.ipums.org/international-action/sample_details/country/cl) to calculate the proxy for parental schooling in Equations (1) and (2). For each cell defined by combinations of birth year, indigenous status, and birth commune, we calculate (1) the mean years of mothers' and fathers' schooling, and (2) the proportion of observations with missing values of each variable. Appendix Figure A2 illustrates the means of the four variables for FG1.

[16] SIMCE parent surveys have incomplete coverage of parental indigenous status with information for mothers only 1997 to 2000, neither parent 2001 to 2005, and both parents 2006 to 2012, 2014, and 2015. When possible, we infer parental indigenous status by using the masked student identifier (MRUN) to impute indigenous status from earlier or later appearances of the same student. We use mothers' indigenous status across all rounds for consistency, coding students as indigenous if their mothers identified as indigenous in any round.

We study the school outcomes of second-generation children who reside in households with mothers born between 1965 and 1991. The SG1 sample pools SIMCE test scores of second-graders (between 2012 and 2015) and fourth-graders (between 2014 and 2017).[17] Each SIMCE record is linked to administrative records of the Ministry of Education that includes each student's cumulative grade point average and attendance, commune of residence, and birth year.[18] We assess the causal channels of intergenerational effects with SG2, a CASEN sample of children under 22 who are co-resident with FG1 mothers born between 1965 and 1991. Importantly, the sample includes a measure of children's receipt of indigenous grants, allowing us to test for intergenerational persistence in the use of indigenous grants.

5. Long-Run Effects on Schooling and Labor Outcomes

A. Event Study Plots and ITT Estimates

Figure 2 includes event study plots for the schooling and labor outcomes of adults. Each panel plots the $\hat{\gamma}_e$ and $\hat{\lambda}_e$ for a dependent variable, bracketed by 95% confidence intervals. The solid lines are $\hat{\tau} * E_{ic}$, the predicted effect given the ACR estimate and expected grant years in each birth year.

The top-left panel corresponds to years of schooling. There is no evident pre-treatment trend, and we fail to reject the null hypothesis that pre-treatment coefficients are jointly equal to zero. For birth years between 1975 and 1982, the $\hat{\lambda}_e$ are mostly positive, small, and not statistically distinguishable from zero, which is consistent with relatively low grant exposure. The estimates are increasingly positive and statistically different from zero for cohorts starting in 1983. By 2000, the ITT estimate is just under one year of schooling.

The top-right panel corresponds to labor earnings. We include adults with zero earnings to avoid sample selection in labor supply, which is induced by grant exposure. Instead of using scale-sensitive transformations such as $\log(Y+1)$, we employ a Poisson quasi-maximum likelihood estimator (Chen and Roth, 2023; Wooldridge, 2010). This yields ITT effects in levels, expressed as the approximate percent of control means. The panel reports exponentiated

[17] Eighty-five percent of respondents to parent questionnaires in SIMCE are mothers, and they report their ages. We use the masked student identifier (MRUN) to impute a mother's age if missing and reported in an earlier or later appearance of the student. In cases of disagreement, we calculate the rounded mean of the implied birth year and impute it to all observations.

[18] See https://datosabiertos.mineduc.cl/rendimiento-por-estudiante-2/.

coefficients, $exp\ (\hat{\gamma}_e)\ - 1$ and $exp\ (\hat{\lambda}_e)\ - 1$, and confidence intervals calculated with the delta method. There are no evident pre-treatment trends and post-treatment ITT estimates are increasing in E_{ic}. By 2000, the ITT estimate is just under 40%.

Increased earnings may reflect increased labor supply on the extensive and intensive margins. The bottom panels use two variables: an indicator of whether individuals worked in the past week and the weekly hours worked in a typical week, including those with zero hours. Neither plot shows evidence of pre-treatment trends, and ITT estimates are increasing in the post-treatment period, albeit with wider confidence intervals. By 2000, there is approximately a 4 percentage point increase in the probability of working, and a more than 15% increase in weekly hours worked. As with earnings, the latter are from Poisson regressions.

To increase power, Table 2 (panel A) reports summary estimates for three groups of indigenous birth cohorts. The estimates for partially-exposed cohorts (from 1975 to 1981) are positive, but small and not statistically different from zero (except for work in the past week). Consistent with the event study plots, the estimates are larger and statistically significant for the fully-exposed cohorts, especially those born from 1992 to 2000. The ITT estimates for these cohorts imply 0.6 years of increased schooling (column 1), 22% higher labor earnings (column 2), a 4.3 percentage points increase in any work (column 3), and a 10% increase in weekly hours worked (column 4). In each column, we reject the null hypotheses that the coefficients are jointly equal or jointly equal to zero.

To interpret the magnitudes of estimates, the bottom of Table 2 reports variable means for adults born between 1965 and 1974. Indigenous adults in pre-treatment cohorts had 1.4 fewer years of schooling and 28% lower earnings than non-indigenous adults. They were 4 percentage points less likely to work and worked 2.4 hours (or 8%) less than non-indigenous adults. The ITT estimates imply a 44% reduction in the pre-treatment schooling gap, a 79% reduction in the earnings gap, and the elimination of pre-treatment differences in labor supply.

B. Average Causal Response to Expected Grant Years

Table 2 (panel B) reports estimates of the average causal response from equation (2). A one-year increase in E_{ic} increased indigenous adults' schooling by 0.38 years (column 1), labor earnings by 13% (column 2), work participation by 2.3 percentage points (column 3), and weekly hours worked by 6% (column 4). To compare these with ITT estimates, Figure 2 plots

predicted values ($\hat{\tau} * E_{ic}$) for each birth year. The solid lines generally pass through pointwise confidence intervals of ITT estimates, with a few exceptions for schooling, and one for hours worked. The evidence suggests that causal responses are roughly linear across the support of E_{ic}.

To see this more clearly, consider that $\hat{\tau}$ is a weighted average of causal responses between adjacent, "discrete" values of E_{ic} (Callaway et al., 2024). To recover these causal responses, we can estimate a regression with a saturated set of expected "dose" indicators, relative to never-treated individuals (cf. Sun and Shapiro, 2022). This is simply equation (1) with pre-treatment coefficients constrained to zero ($\gamma_e = 0 \; \forall \; e$). Appendix Figure A3 plots the $\hat{\lambda}_e$ against E_{ic} rather than birth years, overlaying the linear prediction from $\hat{\tau} * E_{ic}$. For adjacent birth years, the average causal response is $\widehat{ACR}_e = \frac{(\hat{\lambda}_e - \hat{\lambda}_{e-1})}{(E_e - E_{e-1})}$. $\hat{\tau}$ is the average of these slopes, with positive OLS weights that sum to 1 (Callaway et al., 2024). In the figure, the slopes between adjacent values of E_{ic} are consistent with linear ACRs.

C. ACRs in the Distribution of Schooling, Earnings, and Hours Worked

Children are eligible for indigenous grants between the fifth grade of primary and the last tertiary grade. The effects on school attainment may not be uniformly observed across the schooling distribution. We estimate equation (2) for 16 dummy dependent variables, $1\{Y_i \geq x\}$, where Y_i is years of schooling and the *x* are successively higher grades. The top-left panel of Figure 3 plots the estimates and confidence intervals. We anticipate small estimates for lower-primary grades, given near-universal enrollment during our sample coverage, and grant eligibility not starting until grade 5.[19] The largest marginal probabilities, between 0.03 and 0.06, are in the final primary grades (7 and 8) and, especially, secondary grades (9 to 12). Despite larger grants in tertiary grades, the estimates are small and only statistically distinguishable from zero in the first two years (grades 13 and 14). The results suggest that tertiary grants alone are not sufficient to overcome academic, financial, or other barriers that some indigenous students face in completing tertiary studies.

The top-right panel repeats the exercise for dummy variables indicating successively higher labor incomes ($1\{Y_i > x\}$), such that the coefficient for $1\{Y_i > 0\}$ is the marginal probability that

[19] Several very small indigenous groups are eligible for indigenous grants in earlier primary grades (JUNAEB, 2024).

individuals earned a positive income in the last month.[20] The median monthly labor earnings of indigenous adults in the sample (including zeros) is 264,000 (US$311) and the 95th percentile is 1,100,000 (US$1,294) in 2022 pesos. The largest marginal probability is just over the median income, but still above 0.02 for lower and much higher earnings.

Finally, the bottom panel illustrates effects on successively higher hours of work in a typical week ($1\{Y_i > x\}$), such that the coefficient for $1\{Y_i > 0\}$ indicates any hours of work in a typical week, relative to none. The coefficient on more than zero is about 0.02 with a slight upward trend between zero and 40 hours suggesting a smaller effect on intensive-margin labor supply than on the probability of working at least 40 hours per week.

D. Gender

The effects are broadly similar when estimated separately for men and women (Appendix Table A6).[21] The ACR estimate for years of schooling is modestly larger for women (0.43 versus 0.32), while the ACR for labor earnings is modestly smaller for women (11% versus 14%). One year of expected grants similarly increases weekly hours worked by 5.5% for men and 5.8% for women. The contrasting effects for schooling and earnings might imply that indigenous women face gender-specific barriers to higher labor earnings, such as discrimination (Galarza and Yamada, 2017). Appendix Figure A5 reports gender-specific ACRs across the distribution of schooling, labor earnings, and weekly hours worked. In the male sample, estimates for tertiary enrollment are modestly smaller, the largest earnings estimates are shifted rightward in the distribution, and there are relatively larger effects on intensive-margin labor supply.

6. Threats to Internal Validity

This section evaluates threats to internal validity. First, we show that the results cannot be explained by differential post-treatment trends in the household and school inputs to which indigenous and non-indigenous adults were exposed as children. Second, we show that the results cannot be explained by endogenous selection of indigenous status. Third, we estimate separate effects for regions with higher and lower indigenous concentrations, since these regions

[20] This is not perfectly collinear with the indicator of working in the last week (r=0.92) since a few adults worked in the past week for zero pay, and a larger number of adults did not work in the previous week but still earned positive earnings in the previous month.
[21] Appendix Figure A4 reports event study plots for men and women.

may have been differentially affected by concurrently-implemented programs, but find robust effects. Fourth, we review evidence in favor of a "strong" parallel trends assumption, which undergirds the interpretation of $\hat{\tau}$ as the ACR of expected grant years (Callaway et al., 2024).

A. Post-Treatment Trends in Household and School Inputs

Suppose that indigenous children in post-treatment birth cohorts are exposed to increasingly favorable household and school inputs relative to non-indigenous children, even within birth communes. We test for such trends using the FG2 sample of children and youth born between 1985 and 2000. The birth years do not include the pre-treatment period, but overlap with notable increases in both grant receipt, E_{ic}, and our ITT estimates (Figures 1 and 2).

We estimate

$$Y_{icmt} = \sum_{e=1985}^{2000} \lambda_e 1\{c = e\} \times I_{icmt} + \delta_{cm} + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt} \quad (3)$$

where Y_{icmt} is a household or school input experienced by child *i* that is plausibly associated with adult schooling and earnings. The $\hat{\lambda}_e$ are conditional mean differences between indigenous and non-indigenous children born in year *e*. We visually examine the $\hat{\lambda}_e$ for trends and formally test the null of equality. We interpret rejected nulls as evidence of potential bias in ITT estimates.

The top-left panel of Figure 4 illustrates the $\hat{\lambda}_e$ for a variable indicating that a child currently receives an indigenous grant, a variable that *should* exhibit differential post-treatment trends over this period. Between the 1985 and 2000 cohorts, indigenous children were increasingly likely to receive grants, and we reject the null of coefficient equality at 1% (the horizontal dashed line is a coefficient estimate constrained to be equal across all birth years). We can also compare the estimates to those obtained earlier with census and administrative data. The solid line plots the sample average of the cohort-by-age probability of a child receiving an indigenous grants based on our data used above. The similar pattern corroborates that successive birth cohorts of indigenous children were increasingly exposed to indigenous grants.

In contrast, the top-right panel illustrates the $\hat{\lambda}_e$ for a dependent variable indicating that a child received any other education grant. Such grants were allocated based on geography,

household income, or student merit, but never indigenous status.[22] There is no evident trend, and we fail to reject the null of equality. The bottom panels of Figure 4 report similar tests for household total income per capita and household transfer income per capita, net of education grants. The former is a general proxy of household inputs and socioeconomic status, while the latter summarizes exposure to government transfer programs that are often means tested.[23] There are no trends, and we fail to reject the nulls of coefficient equality. Appendix Figure A6 illustrates a similar lack of post-treatment trends for whether children migrated from their birth commune and whether children experienced a recent illness or accident.

The same appendix figure reports tests for differential post-treatment trends in exposure to school inputs. First, we do not find trends in the marginal probability of paying private school tuition or enrolling in the private subsidized schools introduced by Chile's "voucher" reform in 1981 (Hsieh and Urquiola, 2006). Second, there are no trends in relative exposure to a range of national school policies, including free school meals for lower-income schools and children (McEwan, 2013) and a Full School Day (FSD) program rolled out in publicly-funded schools (Raczynski, 2001; Dominguez and Ruffini, 2023). Third, we find no evidence of trends in exposure to primary school reforms in the 1990s, including the 900 Schools Program for low-achieving schools (Chay, McEwan, and Urquiola, 2005) and the MECE-Rural Program for rural, multigrade schools (McEwan, 2008b). Finally, the Preferential Subsidy law in 2008 potentially affected the 1999 and 2000 birth cohorts (Neilson, 2020; Aguirre, 2022), and Appendix Table A7 shows that the results are robust to the exclusion of these cohorts.

B. Endogenous Selection of Indigenous Status

An additional concern is program-induced selection into indigenous status. If non-indigenous children—with attributes associated with higher adult earnings—manipulated their indigenous status to receive a grant, then such endogenous switching could bias the estimates in Figure 2. This behavior is likely not happening. First, such switching behavior would likely introduce differential post-treatment trends in childhood variables such as household income, but we found

[22] For example, the *Beca Presidente de República* offered grants to secondary and tertiary students with low incomes and high achievement, and the *Beca de Apoyo a la Retención Escolar* targeted lower-income students at risk of dropping out of secondary school.

[23] For example, monthly, unconditional transfers are available to poor households with children (the *Subsidio Único Familiar*), to the disabled and elderly poor (*Pensión Asistencial*), for household water supply (*Subsidio de Agua Potable*), and to extremely poor households (*Chile Solidario*) (Carneiro et al., 2018).

no evidence of this. Second, indigenous status is legally certified by CONADI, based on surnames recorded on birth certificates. CONADI certification likely guides individual responses to the household surveys used in this paper, since both use ancestry-based definitions. There is no obvious incentive for legally non-indigenous Chileans to mis-represent their status in the household survey, since survey mis-representation cannot alter grant eligibility and non-indigenous Chileans are more likely to hold negative rather than positive stereotypes of indigenous identify (Saiz et al., 2008).

That said, the proportion of indigenous adults has risen across CASEN surveys used in the adult sample, from 7% in the 2006 survey, to 9% in 2013, and 10% in 2022 (Appendix Table A1). One hypothesis is that the stigma of indigenous identity has diminished, leading fewer indigenous adults to mis-report as non-indigenous. Our main specification already includes survey year fixed effects. In a related specification, we replace the δ_{cm} and μ_t with a triple interaction of 36 birth cohorts, up to 318 birth communes, and eight survey years (δ_{cmt}). This allows birth year shocks to vary within combinations of birth commune and survey year, and absorbs the separate age dummies. The specification also includes separate intercepts for indigenous adults in combinations of birth commune and survey years ($\sum_{m\in M,t\in T,} \theta_{mt} I_{icmt}$). These ITT estimates yield similar conclusions (Appendix Table A8). Finally, we re-estimated equations (1) and (2) in a restricted sample of household surveys between 2013 to 2022, with similar results.[24]

C. Policies Targeted to Heavily-Indigenous Regions

Two government programs targeted indigenous households in heavily-indigenous regions. In the first program, CONADI provided subsidies for land purchases to some indigenous households in 125 communes. Second, the Ministry of Education funded bilingual education programs between 2002 and 2010 in 44 rural communes with high indigenous concentrations (Dascal, Campaña, and de la Fuente, 2010; PEIB, 2016; Webb, 2022). We divided adults in the FG1 sample into two subsamples based on indigenous concentration: (1) those born in seven northern and southern regions (with 25% indigenous in the subsample) and (2) those born in six

[24] The ACR estimates and standard errors are 0.33 (0.063) for schooling, 0.133 (0.029) for earnings, 0.022 (0.008) for work last week, and 0.052 (0.017) for weekly hours worked.

central regions (with 5% indigenous).[25] Indigenous adults in the former sample were much more exposed to both programs.[26] Appendix Table A9 reports estimates in the two subsamples. Among indigenous adults born from 1992 to 2000, the ITT estimates (panel A) are 0.43 and 0.67 years of schooling in less-indigenous and more-indigenous regions, respectively. The earnings estimates are both 22%. The ACR estimates (panel B) show a similar pattern. In brief, there are robustly positive and large effects across both subsamples.[27]

D. Strong Parallel Trends

The interpretation of $\hat{\tau}$ as the ACR of expected grant years depends on a strong parallel trends assumption (Callaway et al., 2024). Specifically, any two birth cohorts must experience the same ITT if exposed to the same value of E_{ic}. This is violated, firstly, if indigenous parents selected into birth years (and values of E_{ic}) based on expected gains in schooling and earnings. This is unlikely as it unrealistically assumes that parents had the foresight to manipulate birth years to influence grants exposure at least 10 years after birth.

Second, the attributes of treated indigenous children—such as household income—could vary between adjacent cohorts. This is unlikely since we found no evidence of differential post-treatment trends in household income per capita of indigenous children in section 6A. Further, the targeting rules described in section 2 were consistently applied, and we find no evidence of differential post-treatment trends in household income per capita of indigenous children who received, or did not receive, an indigenous grant (Appendix Figure A7).

[25] We employ Chile's original 13 administrative regions. Chile added three regions since the original division but we consistently code individuals' birth regions according to the original division.

[26] We calculated the cumulative hectares distributed per indigenous person, within combinations of birth communes and survey years, using 20A and 20B land distribution records from https://siic.conadi.cl, and commune-level indigenous populations from the 2017 census. In central regions, the median among indigenous adults in FG1 is zero; in northern and southern regions, the median is 0.027 hectares per indigenous person.

[27] The estimates assume uniformity in grant exposure across all regions by indigenous status and birth year. Grants were not necessarily distributed uniformly across the country based on regional percent indigenous (JUNAEB, 2018). In 2017, for example, a central region (Metropolitan Santiago) received 15% of grants, though 27% of indigenous Chileans were born there. A southern region (Araucanía) received 29% of grants, though 19% of indigenous Chileans were born there. This non-uniformity favoring non-central regions could understate the ACR estimates in central regions and overstate the ACRs in northern and southern ones even though it was correct on average across the nationwide sample. As an additional robustness check, we multiply E_{ic} by the 2017 ratios of receipt percent over indigenous percent for all 13 regions and report weighted ACR estimates in Appendix Table A9 (panel C). The ACR estimates in the subsamples are even more similar to each other, with slightly higher point estimates in central regions.

Third, supply-side school programs may have complementarities with demand-side grants (Galiani and McEwan, 2013; Mbiti et al., 2019). If two cohorts differ in complementary inputs, then one cohort's ITT effects may be higher despite the same E_{ic}. Since later birth cohorts received more school inputs, the $\widehat{ACR}_e$ for some later years may be "too high"—leading to upward bias in the weighted average, $\hat{\tau}$—because the ACRs at higher values of E_{ic} conflate the effect of higher grants exposure with an upward shift in the entire dose-response curve due to more complementary inputs (Callaway et al., 2024). As circumstantial evidence against this likelihood, the $\widehat{ACR}_e$ for later birth cohorts are not systematically steeper than earlier ones (Appendix Figure A3).

7. Causal Channels of Long-Run Effects

One year of expected grants increased schooling by 0.38 years and labor earnings by 13%. The total earnings effect encompasses the returns to schooling attainment as well as a 10% increase in weekly hours worked. The following sections evaluate other causal channels, including increased child learning, post-birth migration, improved health, and reduced early fertility.

A. Child Test Scores

Indigenous grants potentially increased children's learning per grade via three channels. First, households had more income to spend on child-related investments (although the previous section ruled out increased spending on school tuition). Second, grants could have increased student effort due to the grade-related targeting rules (Scott-Clayton, 2011). Prior-year GPAs must always exceed an eligibility threshold, and applicants' chances of receiving a grant increase linearly with prior-year GPAs, all else equal (JUNAEB, 2024). Third, the labeling of education grants as "indigenous"—independently of conditions—may change beliefs about the expected returns to effort (cf. Jensen, 2010; Benhassine et al., 2015). Anecdotally, indigenous students are keenly aware of grants and perceive formal schooling as an engine of social and economic mobility (Webb, 2015; 2022).

We test for the overall effect on learning using national SIMCE data. The FG3 sample pools 21 grade-by-year rounds, assigned to their implied birth cohorts between 1983 and 2000. Across

these cohorts, expected grant years increased from 0.34 years to 2.44 years. Similar to equation (3), we estimate

$$Y_{icgm} = \sum_{e=1983}^{2000} \sum_{h\in\{4,8,10\}} \lambda_{eh} 1\{c = e \ \wedge g = h\} \times I_{icgm} + \delta_{cgm} + X'_{icgm}\beta + \varepsilon_{icgm} \qquad (4)$$

where Y_{icgm} is a test score of child *i,* in birth cohort *c,* assessed at the end of grade *g,* and enrolled in a school in commune *m.* We transform test scores to z-scores within grade-by-year cells. The δ_{cgm} are fixed effects for the triple interaction of birth cohorts, school communes, and grade.[28] The X'_{icgm} include child-by-grade indicators of gender and parents' schooling attainment, school-by-grade indicators of private or municipal status, and whether schools ever participated in the Full School Day, P-900, or MECE-Rural programs.[29] We cluster standard errors by the communes of schools.

The $\hat{\lambda}_{eh}$ are conditional mean differences between indigenous and non-indigenous students' test scores in each cohort and grade combination. Figure 5 plots the $\hat{\lambda}_{eh}$—with numbers indicating fourth, eighth, or tenth grades—and 95% confidence intervals. In earlier cohorts, test scores of indigenous students were 0.15 to 0.2 standard deviations lower than non-indigenous students. In later cohorts, scores were statistically similar for language and approximately 0.05 standard deviations lower in mathematics. This convergence is similarly evident in the samples of girls and boys and in regional subsamples (Appendix Figure A8). The pattern is similar for all grades, even though fourth grade is prior to grant receipt. However, students' end-of-year GPA in the fourth grade affects grant eligibility and awards in the fifth grade, suggesting GPA-related incentives are important, net of any income effects.

The results in Figure 5 are a truncated event study, in which the earliest cohorts were less treated, but not truly untreated. The implied difference-in-differences suggest that grant exposure increased indigenous language scores by approximately 0.2 standard deviations and math scores by approximately 0.1. This seems conservative, since Figure 2 already showed small ITT effects

[28] We use school communes instead of students' birth communes, a reasonable alternative given evidence the absence of post-treatment trends in the migration status of children (Appendix Figure A6).
[29] We construct consistent measures of parent schooling across all SIMCE rounds, separately coding mothers' and fathers' schooling as 16 categories, including missing. We fully interact the dummy indicators of each parent's schooling with dummies indicating students' grade levels. Gender is not measured in the fourth-grade assessment in 1999, and we include a dummy indicator for missing observations.

on schooling for the earliest cohorts in the test score data. The interpretation is tempered by the lack of test score data to compare pre-treatment trends.

B. Migration, Health, Fertility, and Partners

Using the FG1 sample, we can assess additional channels through which the grants may have affected the earnings of first-generation adults. First, education may spur individuals to migrate from their birth communes to urban areas with more opportunities for schooling or skilled labor (Araujo and Macours, 2021). Second, education potentially increases the health of adults, with related effects on earnings (Clark and Royer, 2013). Third, schooling may reduce early, unintended pregnancies that interrupt formal schooling or labor supply (Breierova and Duflo, 2004).[30] Fourth, own schooling may affect the presence and attributes of adults' partners (Torche, 2010).

We find no evidence that exposure to indigenous grants affected migration, adult health, or teen child-bearing. The event study plots for migration, self-reported health grades, recent illness or accident, and women's likelihood of a live birth at age 17 or younger show that nearly all confidence intervals include zero with a few outliers (Appendix Figure A9). We fail to reject the null hypothesis that the ITT coefficients are equal, or jointly equal to zero, and ACR estimates are small and not statistically different from zero (Table 3, columns 1 to 4).

In contrast, one year of expected grants increased the probability of having a co-resident partner by 1.7 percentage points (column 5), although this is driven by effects in the male subsample (Appendix Table A10). Grants also affected the attributes of co-resident partners. One year of expected grants reduced the probability of having an indigenous partner by 9 percentage points (column 6) and increased the partners' schooling by 0.5 years (column 7). A lower rate of indigenous inter-marriage is a plausible byproduct of assortative mating on correlated variables like schooling and earnings.

8. Intergenerational Effects

[30] Girls exposed to the Full School Day program in Chile were less likely to become mothers, plausibly due to the incapacitation effects of increased time in school (Berthelon and Kruger 2011). More years of school (relative to dropping out) may cause a similar reduction.

This section uses two samples of mothers and co-resident children (Table 1) to test for intergenerational effects of mothers' exposure to indigenous grants. We report intergenerational effects on school outcomes in lower-primary grades, as well as complementary evidence on the potential channels of these effects.

A. Effects on Test Scores, Grades, and Attendance

We use the SG1 sample to test for intergenerational effects on language and math scores, the cumulative GPA, and annual attendance in lower-primary grades. SG1 pools four years of second-grade outcomes (from 2012 to 2015) and four years of fourth-grade outcomes (from 2014 to 2017), for children whose mothers were born from 1965 to 1991. There are two key advantages of focusing on these datasets. First, the children were enrolled in primary grades that were still not eligible to receive indigenous grants. Second, most children were born between 2005 and 2008, when the expected years of indigenous grants in primary grades were no longer increasing, in contrast to pre-2000 cohorts (see Appendix Figure A10, which extends Figure 1 using program data until 2023).

We estimate event study regressions that are a variant of equation (1), following the discussion in section 3.[31] Figure 6 reports event study estimates for language and mathematics tests, the cumulative grade point average, and the annual proportion of school days attended. There is no visual evidence of pre-treatment trends for any of the measures, and we fail to reject the null that pre-treatment coefficients are jointly zero for the first three variables.

For test scores, the ITT estimates in Figure 6 are increasing in mothers' exposure to indigenous grants. Table 4 reports summary ITT estimates for children in two treatment groups—those with mothers born from 1975 to 1981 and those with mothers born from 1982 to 1991—relative to an omitted category of children with mothers born prior to 1975. Test score effects for the first group are small, from 0.03 to 0.04 standard deviations (columns 1 and 2). Estimates for the second group are larger, from 0.08 to 0.11 standard deviations. ACR estimates imply that one expected mother's grant year increased child test scores by 0.13 to 0.17 standard deviations. The estimates are large, given unconditional ethnic test score gaps among children of

[31] The SG1 sample stacks second- and fourth-grade observations; we fully interact a fourth-grade dummy with the fixed effects and control variables. This does not apply to math regressions because second-graders did not take that assessment.

pre-treatment mothers that are 0.20 and 0.27 standard deviations, respectively, in language and math.

Grade point averages are standardized to z-scores within grade-by-year rounds. The ITT estimates in Figure 6 are positive and increasing, and comparable in magnitude to estimates for test scores. In Table 4, an additional year of expected grants among mothers increases GPAs by 0.16 standard deviations (column 3). Finally, there is minimal evidence of intergenerational effects on attendance. Confidence intervals in the event study plot mostly include zero, except for a few pre-treatment coefficients. There is a positive and significant ACR estimate for attendance in Table 4, but it is very small (0.4 percentage points) relative to the 93% attendance rate among children with pre-treatment cohort mothers.

The previous estimates are potentially biased by sample selection if indigenous grants affect the fertility of indigenous women and/or the likelihood that their children are co-resident. However, we find no effects of grant exposure on (1) women's self-reported total fertility or (2) the number of children under 22 who are co-resident with adult women in the FG1 sample (Appendix Table A11). This is consistent with earlier evidence in Table 3 that exposure to grants does not affect early fertility.

B. Causal Channels of Intergenerational Effects

To assess the causal channels of intergenerational test score effects, we also report regressions for school-related inputs in the SG1 sample (Table 4, columns 5 to 9).[32] Mothers' exposure to indigenous grants reduced the probability that children were enrolled in municipal and, to a lesser extent, private subsidized schools, but increases the probability that children enrolled in a private tuition school (columns 5 to 7). Only 3% of children of indigenous mothers in pre-treatment cohorts were enrolled in private tuition schools, relative to 17% of children of non-indigenous mothers. The ACR estimate suggests that this gap decreased by 9 percentage points for one expected year of grants. Private tuition schools charge much higher tuition than subsidized private schools, enroll students with higher incomes, and exercise discretion in admissions that usually includes parent interviews and entrance exams, and often includes the

[32] Panel A of Appendix Figure A11 reports the corresponding event study plots. There is evidence pre-treatment trends for several inputs, implying that the main conclusions are conservative. For example, there is a downward pre-treatment trend for private tuition schools that implies even larger effects on the probability of enrolling.

verification of marriage and baptismal certificates (McEwan, Urquiola, and Vegas, 2008). Regardless of school type, students with more highly exposed mothers had different school peers. We measure two peer attributes: the proportion of children with mothers who are indigenous and proportion with mothers with any tertiary schooling (excluding one's own mother). One year of expected grants reduced the former by 2.6 percentage points, relative to a pre-treatment indigenous mean of 27%. It raised the latter by 9 percentage points, also relative to a pre-treatment indigenous mean of 27% (columns 8 and 9).

Finally, we assess other household characteristics that might have influenced second generation schools outcomes using the SG2 sample of children who are co-resident with mothers in FG1.[33] Indigenous mothers' grant exposure did not affect the probability that children have a co-resident father (Table 5, column 1), consistent with evidence that grants did not affect women's probability of having a co-resident partner (Appendix Table A10). Grant exposure also did not affect the migration of households from mothers' birth communes (Table 5, column 2). In contrast, mothers' exposure increased household income per capita by 18% per year of expected grants (column 3), which is larger than the point estimate for women's labor earnings, and consistent with the increased schooling of co-resident fathers. There is a negative effect of 8% per year of expected grants on household transfer income per capita (column 4), likely driven by a reduction in means-tested government transfers.

Finally, indigenous mothers' grant exposure *lowered* their children's own receipt of indigenous grants. Overall, 13% of children of indigenous mothers in pre-treatment cohorts received a grant. The ACR estimate shows this declined by 6 percentage points for each year of expected grants (column 5). There were no effects on receipt of other education grants (column 6). Recall that eligibility to apply for indigenous grants—and the allocation of scarce grants among applicants—relies on a proxy means test (JUNAEB, 2024). Thus, rising household incomes excluded some children from eligibility for the grants and other government transfers.

[33] In addition to the results reported in the text, Appendix Table A12 uses the SG2 sample to corroborate results from the SG1 sample on municipal and private enrollments, and further shows that children of indigenous mothers are less likely to pay any tuition or to receive free school meals (which are only offered in publicly-funded schools). Panel B of Appendix Figure A11 reports the event study plots for all variables. Given the sample sizes, the plots are noisier and less conclusive, although the results for household income per-capita, indigenous grant receipt, and private tuition school enrollment show no evidence of pre-trends, and clear evidence of ITT effects that rise (or fall) in proportion to mothers' grant exposure.

In summary, indigenous grants increased mothers' schooling, labor supply, and labor earnings, and led indigenous women to find more-educated partners. Net household income per capita rose despite reductions in government transfer income. Higher household income plausibly relaxed income constraints on schooling choices of indigenous children. Therefore, children were exposed to peers with more-educated parents. To be clear, we cannot ascertain whether primary school environments caused increases in language and math skills, or whether higher skills among preschool-aged children facilitated enrollment in selective private tuition schools (McEwan et al., 2008). However, the evidence does not suggest that children's gains in early-primary grades were mediated by their own exposure to indigenous grants or other government transfer programs.

9. Welfare Analysis

The marginal value of public funds (MVPF) is the ratio of beneficiaries' willingness-to-pay (WTP) to the government's net program expenditures (Hendren and Sprung-Keyser, 2020). We estimate the MVPF for the combined 1999 and 2000 birth cohorts of indigenous students. The present value of grant-related expenditures is US$797 per *eligible* indigenous child in both cohorts.[34] Increased schooling attainment causes increased school expenditures of US$2,452 per eligible child.[35] These are substantially offset by a fiscal externality, due to increased revenues from a 19% consumption tax.[36] Overall, the present value of net government expenditures is US$155 per eligible child. The present value of increased after-tax earnings is US$29,482 per eligible child, implying a MVPF of 190.[37] The estimate is conservative because we do not value intergenerational benefits. The MVPF is 16 under very conservative assumptions, including a 7%

[34] We use 2022 prices, applying a discount rate of 5.5% (SNI, 2024). We assume that program costs are incurred for the 1999 cohort between 2009 (fifth grade of primary) and 2020 (fourth year of tertiary). They include grant-related expenditures across all grade levels, and 5% administrative overhead for grant distribution. The cohort sizes are from the 2017 census, as in Figure 1.

[35] We use ITT estimates for schooling and earnings. We assume that school costs correspond to secondary education, incurred at 16 years of age. We estimate the annual expenditure per secondary student as the product of (1) GDP per capita in 2022 prices and (2) the ratio of government secondary expenditures to GDP per capita.

[36] We assume a marginal propensity to consume of 0.5 (Barrero et al., 2020). We conservatively assume that no additional income taxes are paid, since monthly labor incomes below 0.825 million pesos are not taxed in 2022 (https://www.sii.cl/).

[37] We calculate mean labor earnings for 18- to 65-year-olds in the 2022 CASEN survey, including adults with zero earnings (Hendren and Sprung-Keyser, 2020). We multiply earnings by 0.73, or the ratio between mean indigenous earnings in pre-treatment cohorts and mean full sample earnings in the same cohorts. We adjust cross-sectional age-earnings profiles for 0.5% real wage growth, multiply age-specific earnings by the ITT estimates, and deduct consumption tax payments.

discount rate, doubled overhead for grant distribution, and 25% higher expenditures on grant-induced schooling.

10. Conclusions

Despite the ubiquity of education-related CCTs, there is scant evidence on their long-run and, especially, intergenerational effects. Further, there is little causal evidence of how government policies might reduce economic inequality stemming from historical marginalization. We use the scale-up of the Chile's *Beca Indígena* (Indigenous Grants) program as a natural experiment to study the impact of a large, government-implemented conditional cash transfer program on the schooling and labor market outcomes of individuals who received the grants as children.

Birth cohorts of non-indigenous children never received the grants. Indigenous children only received grants if they were born after 1974. The average exposure of indigenous birth cohorts increased thereafter, reaching 2.4 expected years of grants by the 2000 cohort. Indigenous and non-indigenous children lived in the same communities, facilitating controls for unobserved variables shared by children born in the same year and commune. An additional year of expected grants among cohorts increased formal schooling by 0.38 years, hours worked by 6%, and labor earnings by 13 percent. There is no evidence that earnings effects were the result of differential migration, improved adult health, or lower teen fertility among women. We show suggestive evidence, based on a narrower range of birth cohorts, that some of the gains may be explained by increased learning, as measured by Chile's national SIMCE tests. The first-generation effects have a MVPF of 190.

We further show that the effects on mothers produce intergenerational spillovers for their children of 0.13 to 0.17 standard deviations per expected grant year on language and mathematics scores. We cannot pinpoint specific causal channels, but we document a broad transformation in the economic and educational environments of young children, including higher household incomes and a great chance of enrolling in selective primary schools. We also show mothers' grant exposure lowered the likelihood that children received indigenous grants or other household social safety net transfers, which are subject to a proxy means test.

This successful, government-implemented program provides insights into the potential for targeted, school-aged interventions to substantially change human capital investments and outcomes for first- and second-generation children, and to reduce ethnic inequality. The

program's design, implementation, and context hint at the degree to which these results might be replicated elsewhere. First, the *Beca Indígena* program was consistently implemented and scaled-up by stable government agencies, including JUNAEB, the Ministry of Education, and CONADI. Second, the annual grants were large and potentially renewable through tertiary schooling, providing unusually strong financial incentives for forward-looking households. Third, Chile has invested in primary and secondary school quality, an oft-cited impediment to learning gains in other CCT policies (e.g., Levy and Schady, 2013). Fourth, Chile enjoyed relatively strong growth over the 1990s and 2000s, suggesting that the welfare impact of transfers may depends on a robust labor market for higher-skilled workers.

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Table 1: First-generation and second-generation samples

Sample	First-generation		Second-generation	Datasets	Number of observations
	Birth years	Age at survey	Age at survey		
Panel A: First-generation samples					
FG1	1965–2000	22–57	—	CASEN: 2006–2022	659,883
FG2	1985–2000	6–21	—	CASEN: 2006–2020	266,423
FG3	1983–2000	9, 13, 15	—	SIMCE: fourth grade, 1999–2009; eighth grade, 1997–2013; tenth grade, 1998–2015	3,832,101
Panel B: Second-generation samples					
SG1	1965–1991	22–50	7, 9	SIMCE: second grade, 2012–2015; fourth grade, 2014–2017	1,391,868
SG2	1965–1991	22–57	0–21	CASEN: 2006–2022	396,608

Notes: CASEN is the *Encuesta de Caracterización Socioeconómica Nacional.* SIMCE is the *Sistema de Medición de la Calidad de Educación.* FG1, FG2, and SG2: samples exclude observations with missing indigenous indicator or birth commune of first-generation. FG3: sample excludes observations missing both language and mathematics tests, indigenous indicator, or school commune. SG1: sample excludes observations missing both language and mathematics tests, an indigenous indicator, mother's age, commune of residence, or child birth year.

Table 2: Effects on schooling, labor earnings, and labor supply

	Years of schooling	Labor earnings (%)	Worked last week	Weekly hours worked (%)
	(1)	(2)	(3)	(4)
Panel A: Intent-to-treat				
Indigenous, born 1975–1981	0.063	0.022	0.012*	0.019
	(0.060)	(0.019)	(0.005)	(0.010)
Indigenous, born 1982–1991	0.360**	0.081**	0.018*	0.038*
	(0.078)	(0.026)	(0.007)	(0.015)
Indigenous, born 1992–2000	0.600**	0.215**	0.043**	0.099**
	(0.103)	(0.051)	(0.013)	(0.029)
p-value: equal	<0.001	0.001	0.027	0.007
p-value: zero	<0.001	<0.001	0.005	0.004
Panel B: Average causal response				
Expected grant years	0.375**	0.127**	0.023**	0.058**
	(0.056)	(0.029)	(0.008)	(0.017)
Means: adults born 1965–1974				
$\bar{Y}_{IND}$	9.315	347.792	0.661	28.558
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-1.359	-134.010	-0.040	-2.365
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.127	-0.278	-0.057	-0.076
N (panels A and B)	657,133	646,382	659,595	581,448

Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: The coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The coefficient estimate is $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 2 and 4: Exponentiated coefficients from Poisson quasi-maximum-likelihood regressions, and standard errors obtained with the delta method. Column 4: The sample omits the 2020 survey.

Table 3: Effects on migration, health, early fertility, and partners

	Migrated from birth commune	Adult health grade	Recent illness or accident	Birth at ≤17 years (women)	Co-resident partner	Indigenous partner	Co-resident partner's schooling
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Intent-to-treat							
Indigenous, born 1975-1981	0.012*	0.006	-0.004	0.014	0.011	-0.030**	0.077
	(0.005)	(0.016)	(0.004)	(0.007)	(0.006)	(0.008)	(0.061)
Indigenous, born 1982-1991	0.010	0.023	-0.004	0.006	0.026**	-0.089**	0.377**
	(0.006)	(0.015)	(0.004)	(0.007)	(0.007)	(0.008)	(0.080)
Indigenous, born 1992-2000	0.009	—	-0.007	-0.012	0.028**	-0.120**	0.712**
	(0.009)		(0.007)	(0.010)	(0.009)	(0.014)	(0.116)
p-value: equal	0.949	0.316	0.866	0.010	0.093	<0.001	<0.001
p-value: zero	0.158	0.292	0.695	0.012	0.003	<0.001	<0.001
Panel B: Average causal response							
Expected grant years	0.003	0.024	-0.002	-0.005	0.017**	-0.093**	0.499**
	(0.005)	(0.019)	(0.004)	(0.005)	(0.005)	(0.008)	(0.073)
Means: adults born 1965-1974							
$\bar{Y}_{IND}$	0.368	-0.288	0.171	0.163	0.661	0.519	9.148
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.046	-0.087	0.011	0.028	-0.008	0.461	-1.580
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.111	0.433	0.071	0.209	-0.012	7.831	-0.147
N (panels A and B)	659,595	329,254	652,494	230,292	659,595	351,347	350,113

Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The coefficient estimate is $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Column 2: the sample omits survey in 2006, 2009, 2020, and 2022. Column 4: the sample only includes women, and omits surveys in 2006, 2009, and 2020. Columns 6 and 7: the sample is conditional on a co-resident partner.

Table 4: Intergenerational effects of mothers' exposure on children's school outcomes

	Language test	Math test	Year-end grade point average	Year-end attendance (proportion of days)	Municipal school	Private subsidized school	Private tuition school	Proportion indigenous mothers in same grade	Proportion mothers with any tertiary in same grade
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Intent-to-treat									
Indigenous mother, born 1975-81	0.032**	0.037**	0.038**	0.001	-0.006	-0.018**	0.024**	-0.000	0.014**
	(0.010)	(0.010)	(0.009)	(0.001)	(0.005)	(0.005)	(0.004)	(0.002)	(0.004)
Indigenous mother, born 1982-91	0.084**	0.107**	0.102**	0.002**	-0.040**	-0.023**	0.062**	-0.013**	0.058**
	(0.010)	(0.013)	(0.013)	(0.001)	(0.006)	(0.008)	(0.008)	(0.004)	(0.007)
p-value: equal	<0.001	<0.001	<0.001	0.001	<0.001	0.317	<0.001	<0.001	<0.001
p-value: zero	<0.001	<0.001	<0.001	0.002	<0.001	0.002	<0.001	<0.001	<0.001
Panel B: Average causal response									
Expected grant years	0.131**	0.172**	0.162**	0.004**	-0.072**	-0.018	0.090**	-0.026**	0.094**
	(0.014)	(0.017)	(0.018)	(0.001)	(0.008)	(0.010)	(0.012)	(0.006)	(0.010)
Means: children of mothers born 1965-1974									
$\bar{Y}_{IND}$	-0.021	-0.084	0.013	0.933	0.410	0.559	0.031	0.270	0.269
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.199	-0.271	-0.177	-0.002	0.115	0.021	-0.136	0.163	-0.161
N (panels A and B)	1,377,909	699,256	1,391,412	1,391,412	1,390,954	1,390,954	1,390,954	1,390,854	1,376,912

Notes: Standard errors are adjusted for clustering by commune of mother's residence. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The coefficient estimate is $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Column 2: the sample omits second-graders because a math test was not applied.

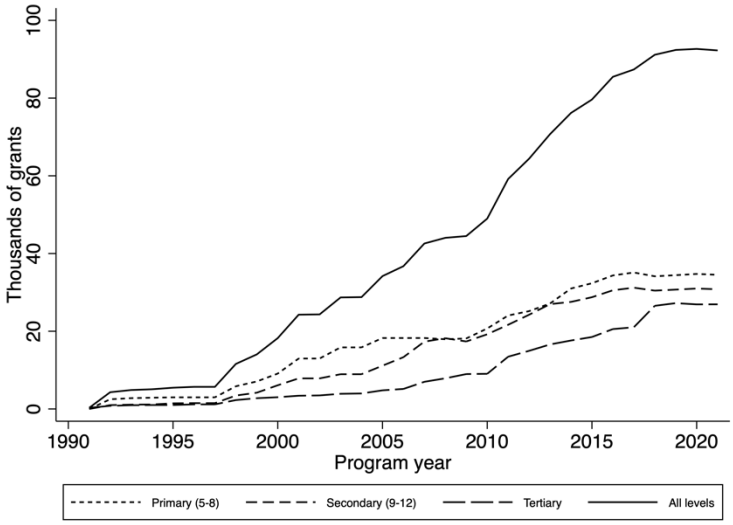
Table 5: Intergenerational effects of mothers' exposure on children under 22

	Children ages 0 to 21				Children ages 6 to 21	
	Co-resident father	Migrated from mother's birth commune	HH income per capita (%)	HH transfer income per capita (%)	Indigenous grant	Other education grant(s)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intent-to-treat						
Indigenous mother, born 1975-1981	0.005	0.007	0.019	0.005	-0.006	0.008*
	(0.010)	(0.008)	(0.026)	(0.021)	(0.006)	(0.003)
Indigenous mother, born 1982-1991	0.003	0.012	0.116**	-0.051*	-0.034**	0.005
	(0.008)	(0.012)	(0.027)	(0.026)	(0.006)	(0.004)
p-value: equal	0.878	0.660	0.001	0.021	<0.001	0.632
p-value: zero	0.957	0.565	<0.001	0.057	<0.001	0.073
Panel B: Average causal response						
Expected grant years	0.000	0.012	0.179**	-0.076*	-0.061**	0.007
	(0.012)	(0.019)	(0.037)	(0.035)	(0.010)	(0.006)
Means: adults born 1965-1974						
$\bar{Y}i$	0.666	0.378	177.409	10.257	0.130	0.060
$\bar{Y}i - \bar{Y}ni$	-0.003	-0.060	-79.281	2.683	0.125	-0.020
$(\bar{Y}i - \bar{Y}ni) / \bar{Y}ni$	-0.005	-0.137	-0.309	0.354	24.100	-0.254
N (panels A and B)	396,603	396,603	396,600	396,439	263,819	263,819

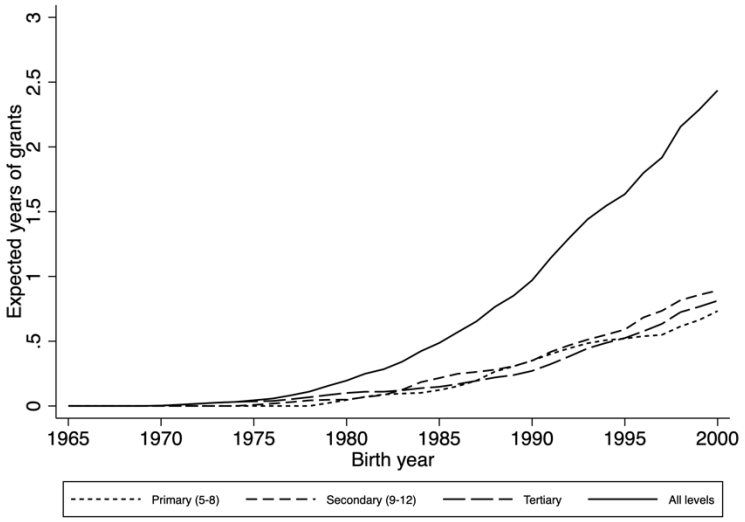
Notes: Standard errors are adjusted for multi-way clustering by commune of mother's birth, and commune of mother's residence. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous mothers in the specified birth years. Panel B: The coefficient estimate is $\hat{\tau}$. Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 3 and 4: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained with the delta method.

Figure 1: Scale-up of the indigenous grants program

Panel A: Number of indigenous grants awarded, by program year

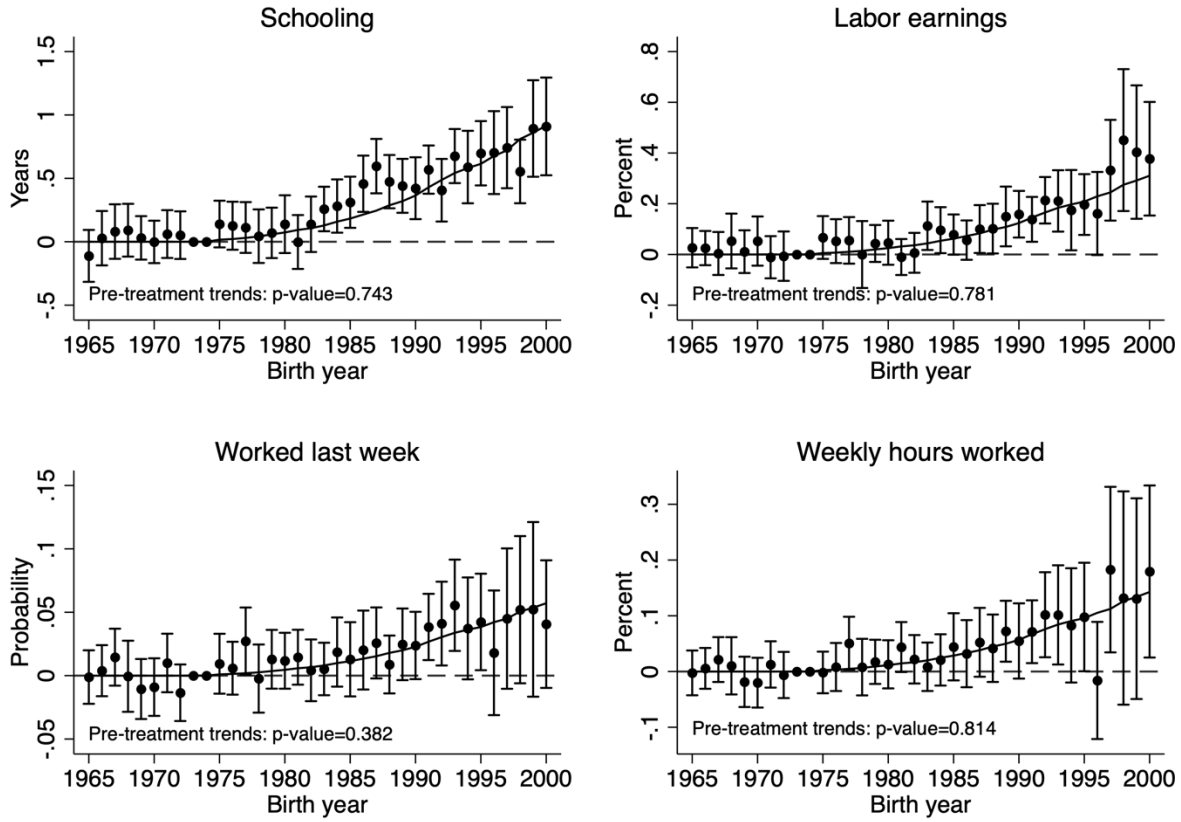


Panel B: Expected years of indigenous grants, by birth year



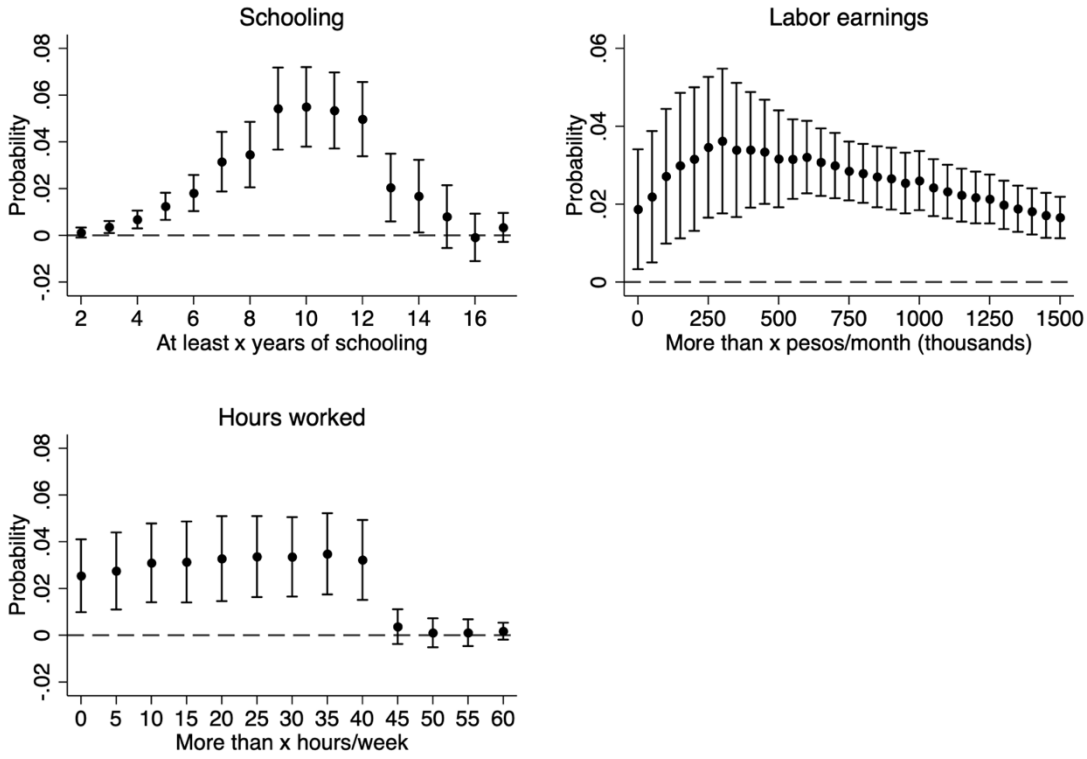
Notes: Panel A: 1991 to 2005 data from decrees of the Ministry of Education; 2006 to 2021 data from annual reports of JUNAEB to DIPRES, a supervisory unit of the Ministry of Finance. Panel B: The sum of grade-specific probabilities of receiving a grant for each birth cohort (see Appendix Table A2).

Figure 2: Event study plots for schooling, labor earnings, and labor supply



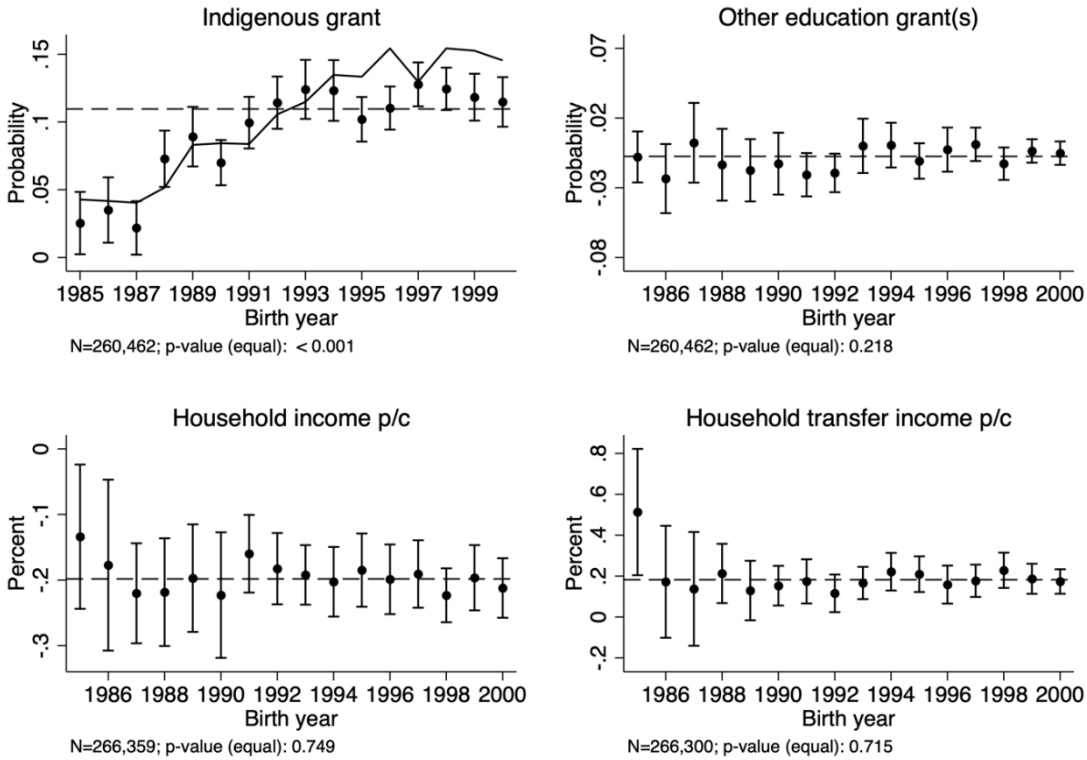
Notes: Standard errors are adjusted for multi-way clustering by commune of birth and commune of residence as adults. Sample sizes in each panel are the same as regressions in Table 2. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$. Left-hand panels: The circles are $\hat{\gamma}_e$ and $\hat{\lambda}_e$ from OLS estimates of equation (1), with pointwise 95% confidence intervals. Right-hand panels: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained with the delta method. Solid lines are $\hat{\tau} * E_{ic}$, using estimates from Table 2.

Figure 3: Average causal responses in the distributions of schooling, labor earnings, and hours worked



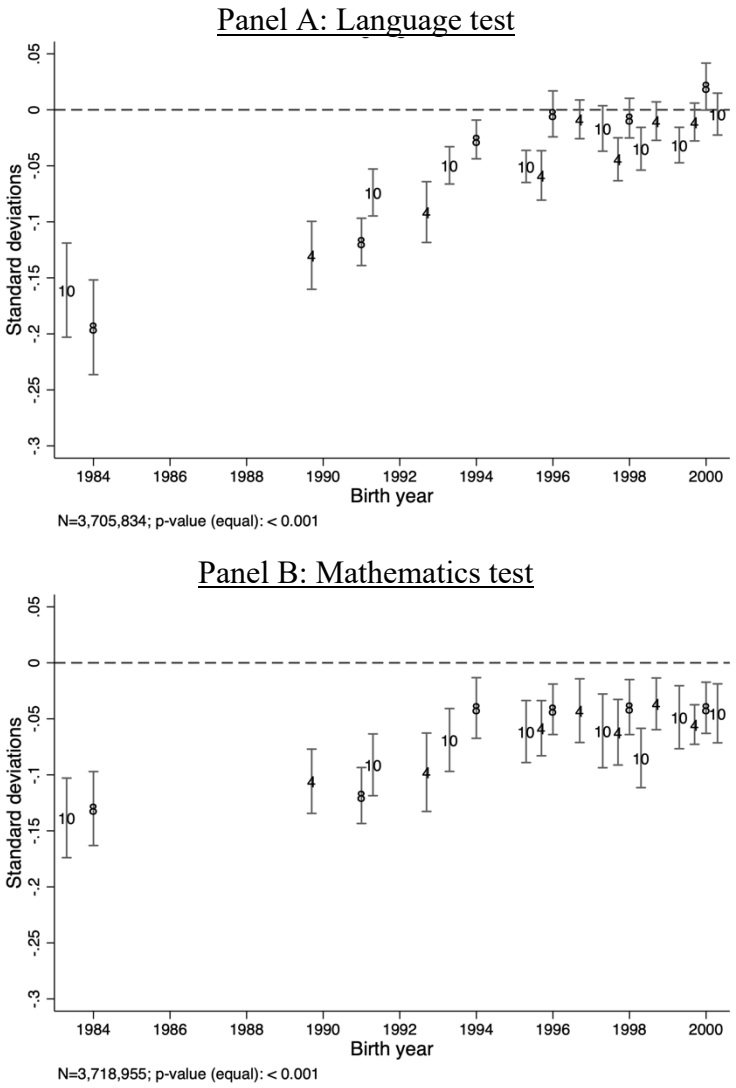
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Table 2. Each point and confidence interval corresponds to a $\hat{\tau}$ from equation (2). Top-left panel: Each circle corresponds to the dependent variable $1\{Y \geq x\}$, where Y is years of schooling and x is the value on the x-axis. Top-right and bottom panels: Each circle corresponds to the dependent variable $1\{Y > x\}$, where Y is labor earnings or hours worked, and x is the value on the x-axis.

Figure 4: Post-treatment trends in household income



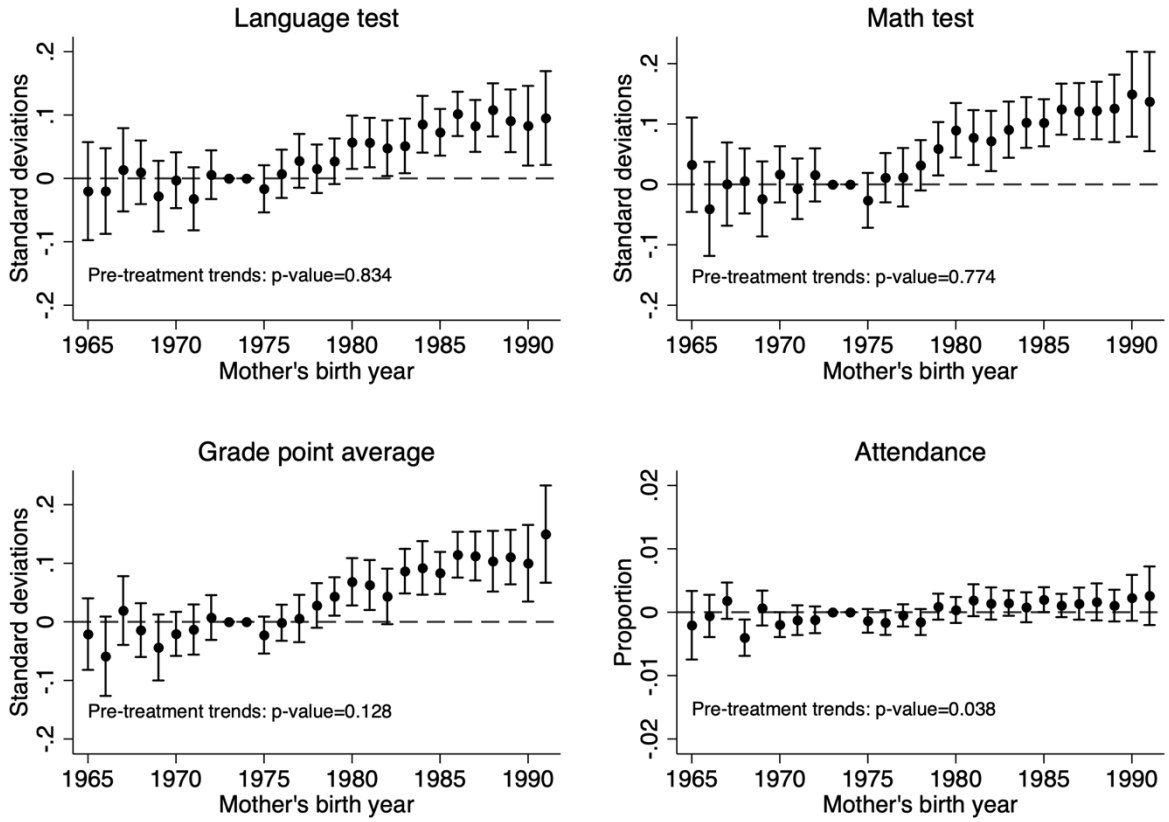
Notes: Panels use the FG2 sample (see Table 1). The circles are $\hat{\lambda}_e$ from equation (3), with pointwise 95% confidence intervals. Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. P-values correspond to the null hypothesis that coefficients are jointly equal. The dashed lines are estimates in which the coefficients are constrained to be equal. Top-left panel: the solid line is the sample average of probabilities—merged from Appendix Table A2—that indigenous children in cohort-by-age groups received indigenous grants. Bottom panels: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and confidence intervals obtained with the delta method.

Figure 5: Post-treatment trends in test scores



Notes: Panels use the FG3 sample (see Table 1). The labeled points are $\hat{\lambda}_{eh}$ for each grade from equation (4), with pointwise 95% confidence intervals. Standard errors are adjusted for clustering by schools' communes. P-values correspond to the null hypothesis that coefficients are jointly equal.

Figure 6: Intergenerational effects of mothers' exposure on children's school outcomes



Notes: Panels use the SG1 sample (see Table 1). Standard errors are adjusted for clustering by commune of residence. Sample sizes in each panel are the same as regressions in Table 4. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$. The circles are $\hat{\gamma}_e$ and $\hat{\lambda}_e$ from a modified equation (1), as described in the text, with pointwise 95% confidence intervals.

Appendix A: Additional Tables and Figures

Table A1: Measurement of indigenous status in household survey and census data

Year	Proportion indigenous		Question	Mutually-exclusive categories
	Weighted	Unweighted		
Panel A: CASEN household surveys				
2022	0.101	0.142	En Chile, la ley reconoce diez pueblos indígenas. ¿[Nombre] pertenece o es descendiente de alguno de ellos?	Aimara, Rapa-Nui o Pascuenses, Quechua, Mapuche, Atacameño (Likan-Antai), Collas, Kawashkar o Alcalufes, Yámana o Yagán, Diaguita, Chango
2020	0.106	0.130		
2017	0.095	0.122	En Chile, la ley reconoce la existencia de nueve pueblos indígenas. ¿[Nombre] pertenece o es descendiente de alguno de ellos?	Aimara, Rapa-Nui o Pascuenses, Quechua, Mapuche, Atacameño (Likan-Antai), Collas, Kawashkar o Alcalufes, Yámana o Yagán, Diaguita
2015	0.090	0.117		
2013	0.091	0.126		
2011	0.082	0.124		
2009	0.069	0.107		
2006	0.066	0.109		
Panel B: IPUMs census samples				
2017	0.122	—	¿Se considera perteneciente a algún pueblo indígena u originario?	Mapuche, Aymara, Rapa Nui, Lican Antai, Quechua, Colla, Diaguita, Kawésqar, Yagán o Yámana, Otro
2002	0.055	—	¿Pertenece usted a alguno de los siguientes pueblos originarios o indígenas?	Alcalufe (Kawashkar), Atacameño, Aimara, Colla, Mapuche, Quechua, Rapa Nui, Yámana (Yagán)
1992	0.106	—	Si usted es chileno, ¿se considera perteneciente a alguna de las siguientes culturas?	Mapuche, Aymara, Rapanui

Notes: The CASEN questionnaires in 2006 and 2009 added the text “la existencia de.” The estimates for CASEN surveys in 2020 and 2022 exclude Chango to maintain comparability with earlier surveys. The estimate for the 2017 census excludes a non-specific category of “other.”

Table A2: Estimated probabilities of receiving indigenous grants, by birth year and age

Birth year	Grade of individuals between the ages of 10 (grade 5) and 21 (grade 16)												E_{ic}
	5	6	7	8	9	10	11	12	13	14	15	16	
1970	—	—	—	—	—	—	—	—	—	—	—	0.003	0.003
1971	—	—	—	—	—	—	—	—	—	—	0.003	0.007	0.010
1972	—	—	—	—	—	—	—	—	—	0.003	0.007	0.008	0.018
1973	—	—	—	—	—	—	—	—	0.002	0.007	0.008	0.008	0.026
1974	—	—	—	—	—	—	—	—	0.007	0.008	0.008	0.008	0.032
1975	—	—	—	—	—	—	—	0.008	0.008	0.009	0.009	0.010	0.044
1976	—	—	—	—	—	—	0.009	0.010	0.009	0.009	0.010	0.010	0.057
1977	—	—	—	—	—	0.009	0.010	0.011	0.009	0.011	0.011	0.021	0.082
1978	—	—	—	—	0.009	0.010	0.011	0.013	0.011	0.011	0.021	0.025	0.111
1979	—	—		0.023	0.010	0.011	0.013	0.014	0.011	0.021	0.025	0.028	0.155
1980	—	—	0.022	0.024	0.010	0.013	0.013	0.013	0.020	0.024	0.026	0.030	0.196
1981	—	0.022	0.024	0.025	0.013	0.013	0.013	0.030	0.024	0.026	0.030	0.030	0.249
1982	0.020	0.022	0.023	0.024	0.012	0.012	0.028	0.033	0.024	0.027	0.028	0.031	0.284
1983	0.023	0.024	0.025	0.025	0.012	0.028	0.034	0.050	0.028	0.029	0.032	0.033	0.342
1984	0.025	0.025	0.025	0.025	0.029	0.036	0.052	0.067	0.030	0.033	0.034	0.041	0.422
1985	0.025	0.025	0.025	0.048	0.035	0.051	0.065	0.065	0.032	0.033	0.040	0.043	0.487
1986	0.024	0.024	0.047	0.057	0.049	0.064	0.064	0.072	0.032	0.039	0.042	0.057	0.571
1987	0.023	0.046	0.055	0.071	0.062	0.062	0.070	0.070	0.038	0.040	0.055	0.062	0.653
1988	0.044	0.054	0.069	0.099	0.060	0.068	0.068	0.084	0.039	0.053	0.060	0.068	0.766
1989	0.051	0.066	0.094	0.094	0.065	0.065	0.081	0.097	0.051	0.057	0.065	0.066	0.851
1990	0.063	0.089	0.089	0.109	0.062	0.077	0.092	0.119	0.054	0.062	0.062	0.093	0.970
1991	0.090	0.090	0.110	0.110	0.077	0.093	0.121	0.127	0.062	0.063	0.094	0.104	1.142
1992	0.092	0.112	0.112	0.129	0.094	0.123	0.129	0.123	0.064	0.095	0.106	0.117	1.296
1993	0.113	0.113	0.130	0.130	0.123	0.129	0.123	0.137	0.096	0.106	0.118	0.125	1.442
1994	0.114	0.131	0.131	0.131	0.131	0.125	0.138	0.156	0.108	0.120	0.127	0.133	1.546
1995	0.131	0.131	0.131	0.128	0.124	0.138	0.155	0.174	0.119	0.126	0.132	0.147	1.635
1996	0.135	0.135	0.133	0.135	0.143	0.161	0.180	0.200	0.131	0.137	0.153	0.156	1.799
1997	0.134	0.131	0.133	0.152	0.159	0.177	0.197	0.202	0.135	0.150	0.154	0.194	1.918
1998	0.136	0.138	0.157	0.183	0.184	0.205	0.209	0.218	0.156	0.160	0.202	0.207	2.155
1999	0.137	0.156	0.181	0.190	0.203	0.207	0.217	0.230	0.158	0.200	0.205	0.203	2.287
2000	0.156	0.182	0.190	0.205	0.208	0.217	0.231	0.236	0.200	0.206	0.203	0.203	2.436

Notes: Each probability is $\frac{G_c^g}{N_c}$, as described in section 3. The numerator is the number of grants awarded to a given birth cohort in a given grade. The denominator is the size of the indigenous birth cohort from the 2017 census, with a mortality adjustment. See the text for details.

Table A3: Descriptive statistics for the auxiliary CASEN sample of children and youth (FG2)

	Indigenous = 0		Indigenous = 1			
			All		Indigenous grant = 0	Indigenous grant = 1
	Mean (SD)	N	Mean (SD)	N	Mean (SD)	Mean (SD)
Panel A: Ages 6–21						
Female	0.491	229374	0.498	37049	0.488	0.561
Indigenous grant	0.002	224386	0.136	36138	—	—
Other school grant(s)	0.085	224386	0.069	36138	0.069	0.069
Household total income p/c (thousands/month)	239.2 (355.5)	229372	175.8 (198.8)	37049	180.5 (204.9)	137.5 (128.7)
Household transfer income p/c (thousands/month)	6.95 (12.07)	229372	9.56 (14.34)	37049	8.83 (13.61)	12.11 (15.04)
Migrated from birth commune	0.231	229374	0.213	37049	0.215	0.194
Recent illness or accident	0.100	227338	0.103	36708	0.102	0.108
Panel B: Ages 6–15						
Receives free school meal(s)	0.635	92604	0.828	15441	0.814	0.898
Attends full day schedule	0.761	92512	0.862	15430	0.854	0.903
Enrolled in municipal school	0.595	91254	0.625	15227	0.623	0.637
Enrolled in subsidized private school	0.362	91254	0.365	15227	0.367	0.352
Pays any school tuition	0.282	92578	0.145	15429	0.153	0.104
Primary or secondary school ever in FSD	0.759	91256	0.841	15228	0.836	0.873
Panel C: Ages 6–13						
Primary school ever in P900	0.342	59186	0.433	9702	0.427	0.469
Primary school ever in MECE-Rural	0.066	59186	0.190	9702	0.184	0.219

Notes: The sample includes children and adolescents (ages 6 to 21), born from 1985 to 2000, in CASEN household surveys collected in 2006, 2009, 2011, 2013, 2015, 2017, and 2020. Standard deviations are reported for continuous variables. Panels B and C impose additional age restrictions. FSD is the Full School Day Program, P900 is the 900 Schools Program, and MECE-Rural is a program for rural, multigrade schools.

Table A4: Communes created between 1981 and 2004

Super-commune	Newly-created commune(s)	Year	Law
Santa Bárbara	Alto Biobio	2004	Law 19959
Nueva Imperial	Cholchol	2004	Law 19944
Iquique	Alto Hospicio	2004	Law 19943
Talcahuano	Hualpén	2004	Law 19936
Concepción	Chiguayante	1996	Law 19461
Concepción	San Pedro de la Paz	1995	Law 19436
Pelarco	San Rafael[a]	1995	Law 19435
Chillán	Chillán Viejo	1995	Law 19434
Viña del Mar	Concón[b]	1995	Law 19424
Temuco	Padres Las Casas	1995	Law 19391
Peñaflor	Padre Hurtado	1994	Law 19340
Las Condes	Vitacura, Lo Barnechea	1981	DFL 1-3260
Conchalí	Huechuraba, Recoleta[c]	1981	DFL 1-3260
Ñuñoa	Macul, Peñalolen	1981	DFL 1-3260
San Miguel	San Joaquin, Pedro Aguirre Cerda[c]	1981	DFL 1-3260
La Granja	La Pintana, San Ramón	1981	DFL 1-3260
Santiago	Independencia[c]	1981	DFL 1-3260
La Cisterna	El Bosque[c], Lo Espejo[c]	1981	DFL 1-3260
Quinta Normal	Estación Central[c]	1981	DFL 1-3260
Maipu	Cerrillos[c]	1981	DFL 1-3260
Pudahuel	Lo Prado[c], Cerro Navia[c]	1981	DFL 1-3260

[a] According to the law, San Rafael was part of Pelarco, Talca, and Rio Claro. According to IPUMS shapefiles, it was primarily taken from Pelarco.
[b] According to the law, Concón was part of Viña del Mar, Limache, and Quintero. According to IPUMS shapefiles, it was primarily taken from Viña del Mar.
[c] These communes were created from more than one commune in the metropolitan region. We assume that the super-commune is the first commune listed in the relevant article of DFL 1-3260.

Table A5: Effects on schooling, labor earnings, and labor supply, reclassifying 1970 to 1974 as partially treated

	Years of schooling	Labor earnings (%)	Worked last week	Weekly hours worked (%)
	(1)	(2)	(3)	(4)
Panel A: Intent-to-treat				
Indigenous, born 1970–1981	0.030	-0.001	0.003	0.005
	(0.062)	(0.020)	(0.005)	(0.009)
Indigenous, born 1982–1991	0.354**	0.068*	0.015	0.033*
	(0.095)	(0.028)	(0.008)	(0.016)
Indigenous, born 1992–2000	0.594**	0.201**	0.040**	0.094**
	(0.115)	(0.050)	(0.013)	(0.029)
p-value: equal	<0.001	<0.001	0.017	0.005
p-value: zero	<0.001	0.001	0.026	0.013
Panel B: Average causal response				
Expected grant years	0.376**	0.128**	0.023**	0.059**
	(0.057)	(0.029)	(0.008)	(0.018)
N (panels A and B)	657,133	646,382	659,595	581,448

Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: The coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The coefficient estimate is $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 2 and 4: Exponentiated coefficients from Poisson quasi-maximum-likelihood regressions, and standard errors obtained with the delta method. Column 4: The sample omits the 2020 survey.

Table A6: Effects on schooling, labor earnings, and labor supply (by gender)

	Years of schooling		Labor earnings (%)		Worked last week		Weekly hours worked (%)	
	Women	Men	Women	Men	Women	Men	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Intent-to-treat								
Indigenous, born 1975-1981	0.074	0.042	-0.005	0.043	0.004	0.024**	0.008	0.031**
	(0.079)	(0.066)	(0.024)	(0.023)	(0.008)	(0.007)	(0.020)	(0.011)
Indigenous, born 1982-1991	0.393**	0.312**	0.056	0.094**	0.010	0.025*	0.026	0.046**
	(0.086)	(0.091)	(0.039)	(0.028)	(0.010)	(0.010)	(0.025)	(0.015)
Indigenous, born 1992-2000	0.724**	0.456**	0.178**	0.231**	0.037**	0.048**	0.099*	0.097**
	(0.116)	(0.113)	(0.056)	(0.057)	(0.012)	(0.018)	(0.040)	(0.032)
p-value: equal	<0.001	<0.001	0.001	0.008	0.020	0.204	0.045	0.052
p-value: zero	<0.001	<0.001	0.004	<0.001	0.020	0.003	0.089	0.008
Panel B: Average causal response								
Expected grant years	0.426**	0.322**	0.110**	0.135**	0.021**	0.023*	0.055*	0.058**
	(0.063)	(0.063)	(0.032)	(0.033)	(0.008)	(0.010)	(0.023)	(0.017)
Means: adults born 1965-1974								
$\bar{Y}_{IND}$	9.318	9.312	218.977	503.297	0.496	0.858	19.669	39.111
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-1.428	-1.280	-78.880	-189.526	-0.044	-0.025	-2.194	-2.111
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.133	-0.121	-0.265	-0.274	-0.082	-0.029	-0.100	-0.051
N (panels A and B)	345,153	311,270	339,298	305,811	346,382	312,506	303,476	276,449

Notes: Standard errors are adjusted for multi-way clustering by the commune of birth, and the commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The coefficient estimate is $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 3, 4, 7, and 8: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained with the delta method. Columns 7 and 8: the sample omits the 2020 survey.

Table A7: Effects on schooling, labor earnings, and labor supply, excluding 1999 and 2000 birth years

	Years of schooling	Labor earnings (%)	Worked last week	Weekly hours worked (%)
	(1)	(2)	(3)	(4)
Panel A: Intent-to-treat				
Indigenous, born 1975–1981	0.061	0.022	0.012*	0.019
	(0.059)	(0.019)	(0.005)	(0.010)
Indigenous, born 1982–1991	0.358**	0.080**	0.018*	0.038*
	(0.078)	(0.026)	(0.007)	(0.015)
Indigenous, born 1992–2000	0.571**	0.205**	0.043**	0.094**
	(0.100)	(0.050)	(0.014)	(0.029)
p-value: equal	<0.001	0.001	0.037	0.017
p-value: zero	<0.001	0.001	0.007	0.008
Panel B: Average causal response				
Expected grant years	0.379**	0.125**	0.025**	0.058**
	(0.058)	(0.029)	(0.009)	(0.019)
N (panels A and B)	651,698	641,220	654,141	576,248

Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: The coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The coefficient estimate is $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 2 and 4: Exponentiated coefficients from Poisson quasi-maximum-likelihood regressions, and standard errors obtained with the delta method. Column 4: The sample omits the 2020 survey.

Table A8: Effects on schooling, labor earnings, and labor supply, with additional fixed effects

	Years of schooling	Labor earnings (%)	Worked last week	Weekly hours worked (%)
	(1)	(2)	(3)	(4)
Panel A: Intent-to-treat				
Indigenous, born 1975–1981	0.053	0.025	0.013*	0.022*
	(0.060)	(0.020)	(0.006)	(0.011)
Indigenous, born 1982–1991	0.358**	0.099**	0.019*	0.041*
	(0.080)	(0.031)	(0.008)	(0.017)
Indigenous, born 1992–2000	0.552**	0.232**	0.045**	0.102**
	(0.111)	(0.061)	(0.016)	(0.033)
p-value: equal	<0.001	0.002	0.056	0.021
p-value: zero	<0.001	0.002	0.013	0.009
Panel B: Average causal response				
Expected grant years	0.354**	0.142**	0.025*	0.063**
	(0.064)	(0.034)	(0.010)	(0.020)
N (panels A and B)	649,977	636,590	652,488	573,665

Notes: The additional fixed effects specifications includes a triple interaction of birth cohort, birth commune, and survey year (δ_{cmt}); see text for details. Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: The coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The coefficient estimate is $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 2 and 4: Exponentiated coefficients from Poisson quasi-maximum-likelihood regressions, and standard errors obtained with the delta method. Column 4: The sample omits the 2020 survey.

Table A9: Effects on schooling, labor earnings, and labor supply (by regional subsamples)

	Years of schooling		Labor earnings (%)		Worked last week		Weekly hours worked (%)	
	Central regions	N and S regions	Central regions	N and S regions	Central regions	N and S regions	Central regions	N and S regions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Intent-to-treat								
Indigenous, born 1975-1981	-0.050	0.100	0.060	0.006	0.017	0.009	0.019	0.018
	(0.064)	(0.071)	(0.041)	(0.021)	(0.010)	(0.006)	(0.018)	(0.012)
Indigenous, born 1982-1991	0.209*	0.412**	0.087*	0.082**	0.011	0.021*	0.020	0.046*
	(0.082)	(0.094)	(0.041)	(0.030)	(0.011)	(0.009)	(0.019)	(0.019)
Indigenous, born 1992-2000	0.429**	0.672**	0.220**	0.219**	0.042*	0.044*	0.074*	0.113**
	(0.085)	(0.133)	(0.074)	(0.067)	(0.017)	(0.018)	(0.036)	(0.041)
p-value: equal	<0.001	<0.001	0.075	0.003	0.032	0.163	0.211	0.040
p-value: zero	<0.001	<0.001	0.016	0.008	0.021	0.053	0.154	0.033
Panel B: Average causal response								
Expected grant years	0.296**	0.415**	0.117**	0.134**	0.019	0.026*	0.039	0.069**
	(0.047)	(0.077)	(0.041)	(0.037)	(0.010)	(0.011)	(0.022)	(0.024)
Panel C: Average causal response (regional weights)								
Expected grant years	0.353**	0.321**	0.142**	0.103**	0.025*	0.021*	0.058*	0.055**
	(0.072)	(0.062)	(0.053)	(0.031)	(0.011)	(0.009)	(0.026)	(0.020)
Means: adults born 1965-1974								
$\bar{Y}_{IND}$	10.2	9.0	390.2	334.7	0.7	0.7	29.3	28.3
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.5	-1.6	-88.9	-153.4	-0.0	-0.0	-1.7	-2.5
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.049	-0.147	-0.186	-0.314	-0.040	-0.061	-0.054	-0.080
N (panels A, B and C)	425,364	231,769	418,465	227,917	426,986	232,609	376,105	205,343

Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Central regions include 4 to 8 and 13, northern regions include 1-3, and southern regions include 9-12 (see text for details). Panel A: the coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The coefficient estimate is $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 3, 4, 7, and 8: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained with the delta method. Columns 7 and 8: the sample omits the 2020 survey.

Table A10: Effects on migration, health, and partner attributes (by gender)

	Migrated from birth commune	Adult health grade	Recent illness or accident	Co-resident partner	Indigenous partner	Co-resident partner's schooling
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intent-to-treat (women)						
Indigenous, born 1975-1981	0.006	0.014	-0.011	0.004	-0.028**	0.053
	(0.008)	(0.023)	(0.006)	(0.008)	(0.008)	(0.074)
Indigenous, born 1982-1991	0.007	0.036	-0.005	0.017*	-0.074**	0.330**
	(0.008)	(0.022)	(0.006)	(0.008)	(0.009)	(0.103)
Indigenous, born 1992-2000	0.005	—	-0.010	0.013	-0.096**	0.832**
	(0.012)		(0.008)	(0.011)	(0.014)	(0.134)
p-value: equal	0.963	0.357	0.516	0.374	<0.001	<0.001
p-value: zero	0.831	0.254	0.318	0.196	<0.001	<0.001
Panel B: Average causal response (women)						
Expected grant years	0.002	0.042	-0.002	0.010	-0.078**	0.525**
	(0.007)	(0.026)	(0.004)	(0.006)	(0.009)	(0.080)
Means: female adults born 1965-1974						
$\bar{Y}_{IND}$	0.382	-0.386	0.200	0.645	0.494	9.011
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.041	-0.081	0.011	0.003	0.444	-1.543
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.097	0.267	0.060	0.004	8.788	-0.146
N (panel A and B)	346,382	173,691	342,716	346,382	185,279	184,574
Panel C: Intent-to-treat (men)						
Indigenous, born 1975-1981	0.020	-0.002	0.005	0.016	-0.032**	0.122
	(0.008)	(0.023)	(0.005)	(0.008)	(0.010)	(0.078)
Indigenous, born 1982-1991	0.013	0.011	-0.005	0.032**	-0.101**	0.465**
	(0.008)	(0.018)	(0.005)	(0.011)	(0.011)	(0.096)
Indigenous, born 1992-2000	0.010	—	-0.006	0.037**	-0.141**	0.518**
	(0.011)		(0.008)	(0.014)	(0.021)	(0.156)
p-value: equal	0.644	0.548	0.214	0.158	<0.001	<0.001
p-value: zero	0.072	0.771	0.378	0.032	<0.001	<0.001
Panel D: Average causal response (men)						
Expected grant years	0.002	0.012	-0.003	0.021*	-0.110**	0.482**
	(0.006)	(0.024)	(0.005)	(0.008)	(0.011)	(0.093)
Means: male adults born 1965-1974						
$\bar{Y}_{IND}$	0.351	-0.167	0.137	0.680	0.548	9.303
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.053	-0.087	0.010	-0.020	0.481	-1.607
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.131	1.080	0.076	-0.028	7.111	-0.147
N (panel C and D)	312,506	155,083	309,069	312,506	164,932	164,408

Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panels A and C: Coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panels B and D: The ACR estimate corresponds to $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Column 2: the sample omits survey in 2006, 2009, 2020, and 2022. Column 4: the sample only includes women, and omits surveys in 2006, 2009, and 2020. Columns 5 and 6: the sample is conditional on a co-resident partner.

Table A11: Effects on women's total fertility and child co-residence in the FG1 sample

	Number of co-resident children under 22	Number of children ever born
	(1)	(2)
Panel A: Intent-to-treat		
Indigenous, born 1975–1981	0.015	0.012
	(0.021)	(0.025)
Indigenous, born 1982–1991	0.035	-0.018
	(0.019)	(0.024)
p-value: equal	0.097	0.217
p-value: zero	0.074	0.093
Panel B: Average causal response		
Expected grant years	0.041	-0.022
	(0.025)	(0.029)
Means: female adults born 1965-1974		
$\bar{Y}_{IND}$	1.412	2.535
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	0.097	0.217
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	0.074	0.093
N (panels A and B)	315,004	208,229

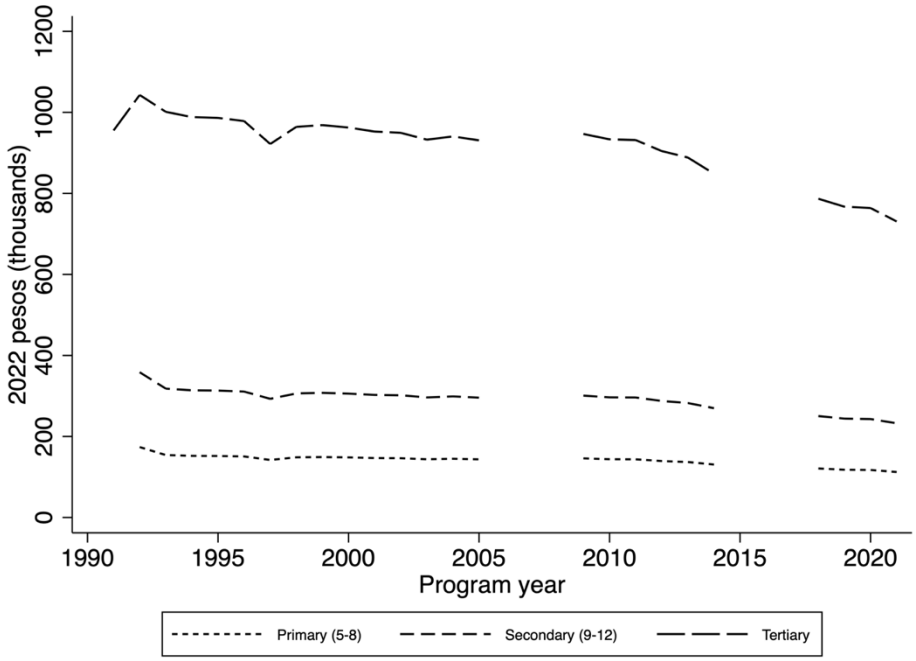
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: The coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The coefficient estimate is $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Column 2: the sample omits surveys in 2006, 2009, and 2020.

Table A12: Intergenerational effects of mothers' exposure on children between 6 and 15

	Children ages 6 to 15					
	Municipal school	Private subsidized school	Private tuition school	HH pays any tuition	School offers free meal(s)	School offers full day instruction
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intent-to-treat						
Indigenous mother, born 1975-1981	-0.024*	0.016	0.009*	0.023**	-0.019	-0.010
	(0.011)	(0.010)	(0.004)	(0.008)	(0.010)	(0.009)
Indigenous mother, born 1982-1991	-0.044**	0.018	0.026**	0.039**	-0.064**	-0.019
	(0.012)	(0.012)	(0.006)	(0.009)	(0.011)	(0.010)
p-value: equal	0.078	0.904	<0.001	0.059	<0.001	0.503
p-value: zero	0.002	0.225	<0.001	<0.001	<0.001	0.142
Panel B: Average causal response						
Expected grant years	-0.054**	0.016	0.038**	0.052**	-0.101**	-0.016
	(0.017)	(0.017)	(0.009)	(0.015)	(0.016)	(0.014)
Means: adults born 1965-1974						
$\bar{Y}i$	0.577	0.408	0.010	0.148	0.801	0.842
$\bar{Y}i - \bar{Y}ni$	0.052	-0.004	-0.046	-0.154	0.200	0.085
$(\bar{Y}i - \bar{Y}ni) / \bar{Y}ni$	0.099	-0.011	-0.819	-0.510	0.333	0.113
N (panels A and B)	178,017	178,017	178,017	161,888	180,519	179,572

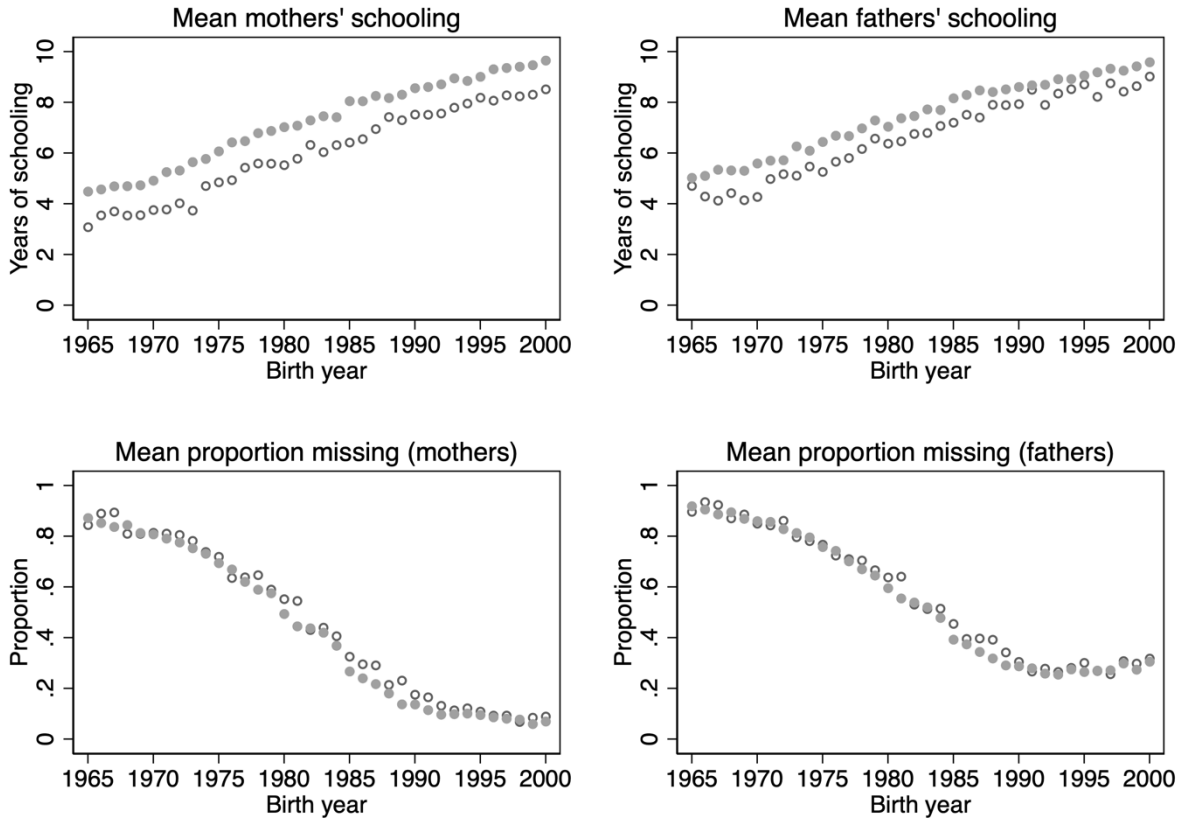
Notes: Standard errors are adjusted for multi-way clustering by commune of mother's birth, and commune of mother's residence. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous mothers in the specified birth years. Panel B: The coefficient estimate is $\hat{\tau}$. Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974.

Figure A1: Annual value (2022 pesos) of indigenous grants, 1991 to 2021



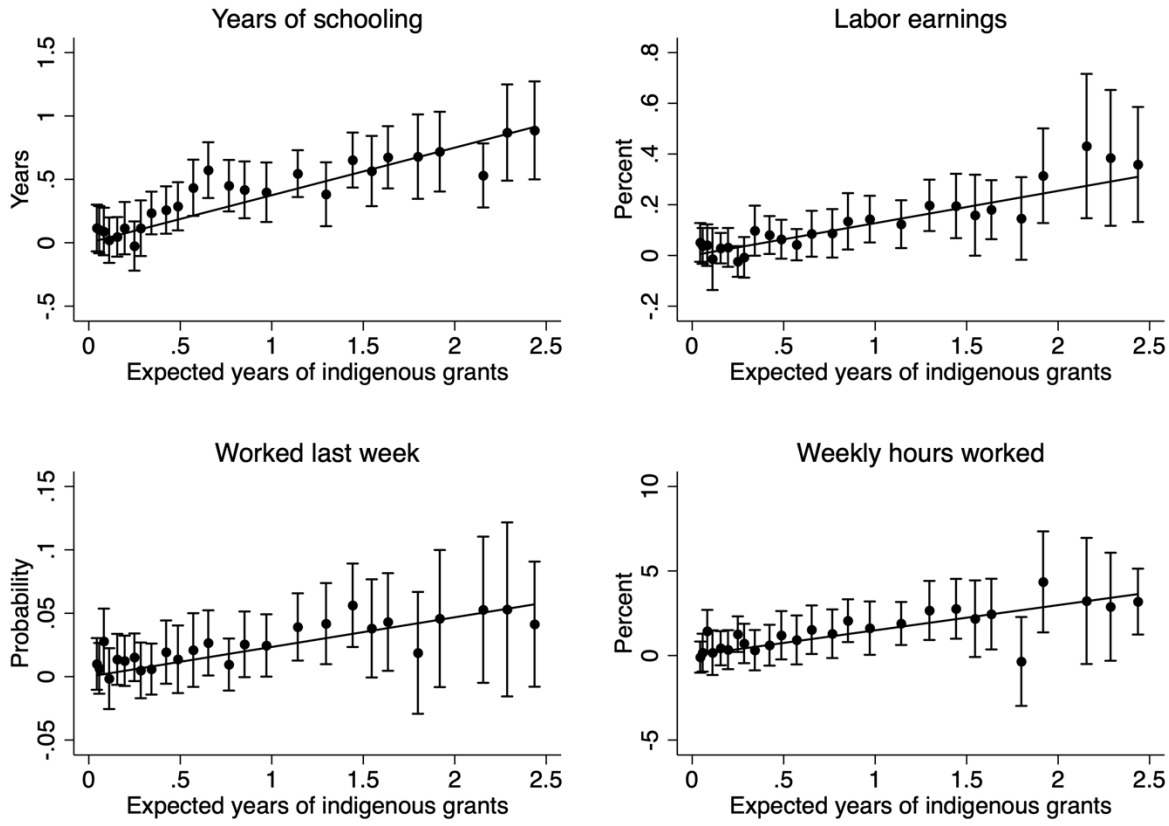
Notes: Each line plots the annual value (in 2022 pesos) of primary, secondary, or tertiary indigenous grants. Missing segments indicate years with missing data in sources mentioned in Figure 1 notes.

Figure A2: Parental schooling measures from the IPUMs 10% sample of the 2002 census



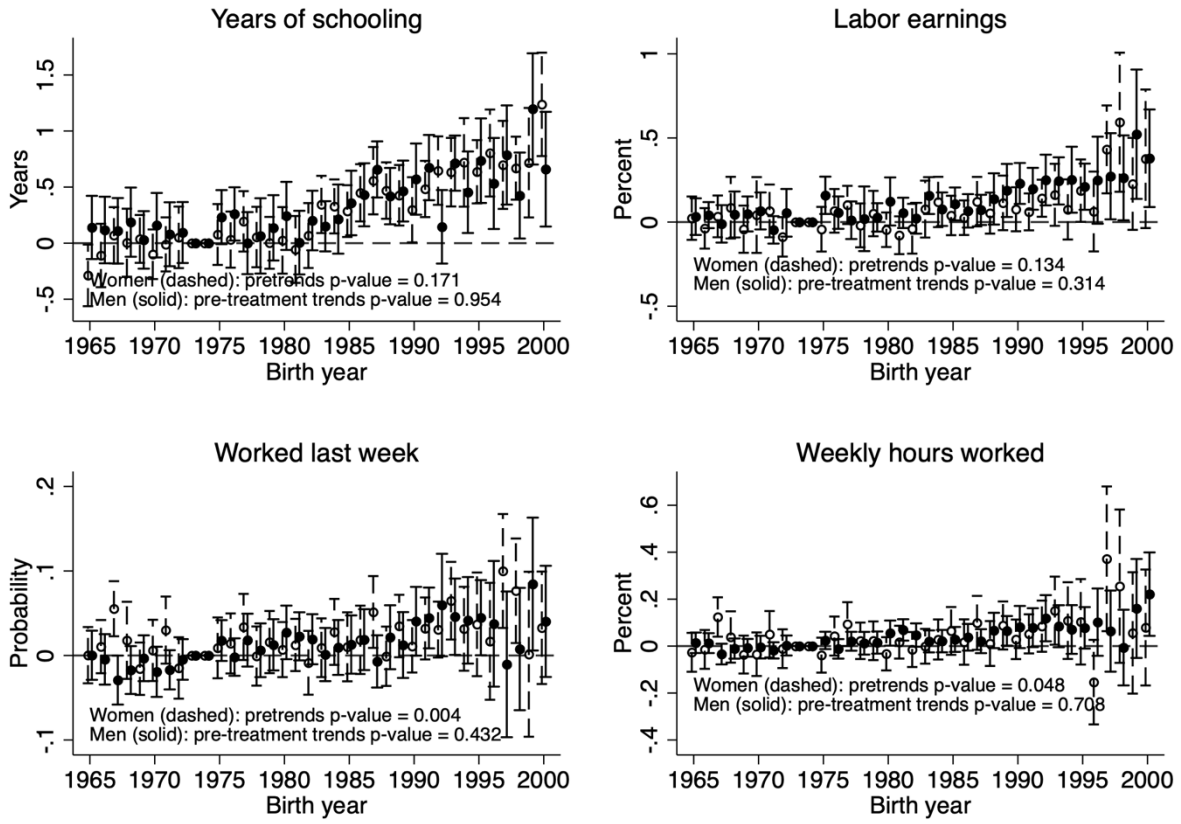
Notes: We use the 10% IPUMs sample of the 2002 census to construct parental schooling variables within combinations of birth year, birth commune, and indigenous status. Mothers and fathers of individuals within households are identified using harmonized IPUMs definitions. The variables in the four panels correspond to the mean years of mothers' schooling, the mean years of fathers' schooling, the proportion of missing observations for mothers, and the proportion of missing observations for fathers, all within cells. The solid circles are unweighted means of non-indigenous cells; hollow circles correspond to indigenous observations.

Figure A3: Approximate linearity of the average causal response, imposing the assumption of zero pre-treatment trends



Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Table 2. Left-hand panels: The circles are $\hat{\gamma}_e$ and $\hat{\lambda}_e$ from OLS estimates of equation (1)—but imposing the assumption that $\gamma_e = 0 \; \forall \; e$—with pointwise 95% confidence intervals. Right-hand panels: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained with the delta method. Solid lines are $\hat{\tau} * E_{ic}$, using the $\hat{\tau}$ from equation (2).

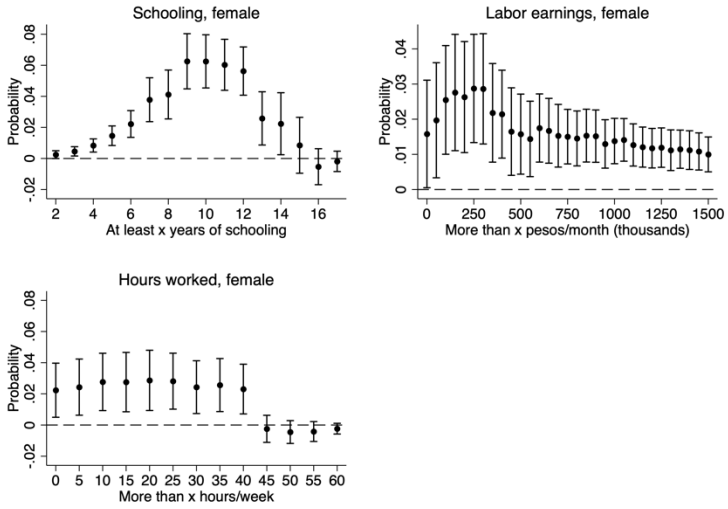
Figure A4: Event study plots for schooling, labor earnings, and labor supply
(female and male samples)



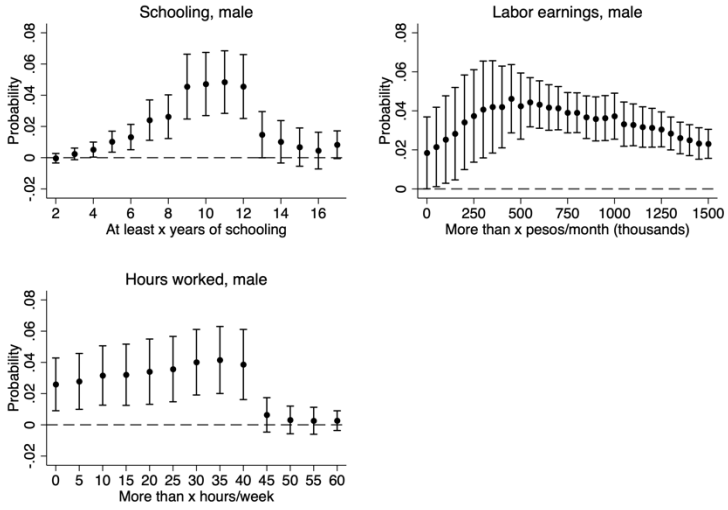
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Table 2. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$. Left-hand panels: The circles are $\hat{\gamma}_e$ and $\hat{\lambda}_e$ from OLS estimates of equation (1), with pointwise 95% confidence intervals. Right-hand panels: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and confidence intervals obtained with the delta method.

Figure A5: Average causal responses in the distributions of schooling, labor earnings, and hours worked (by gender)

Panel A: Female sample

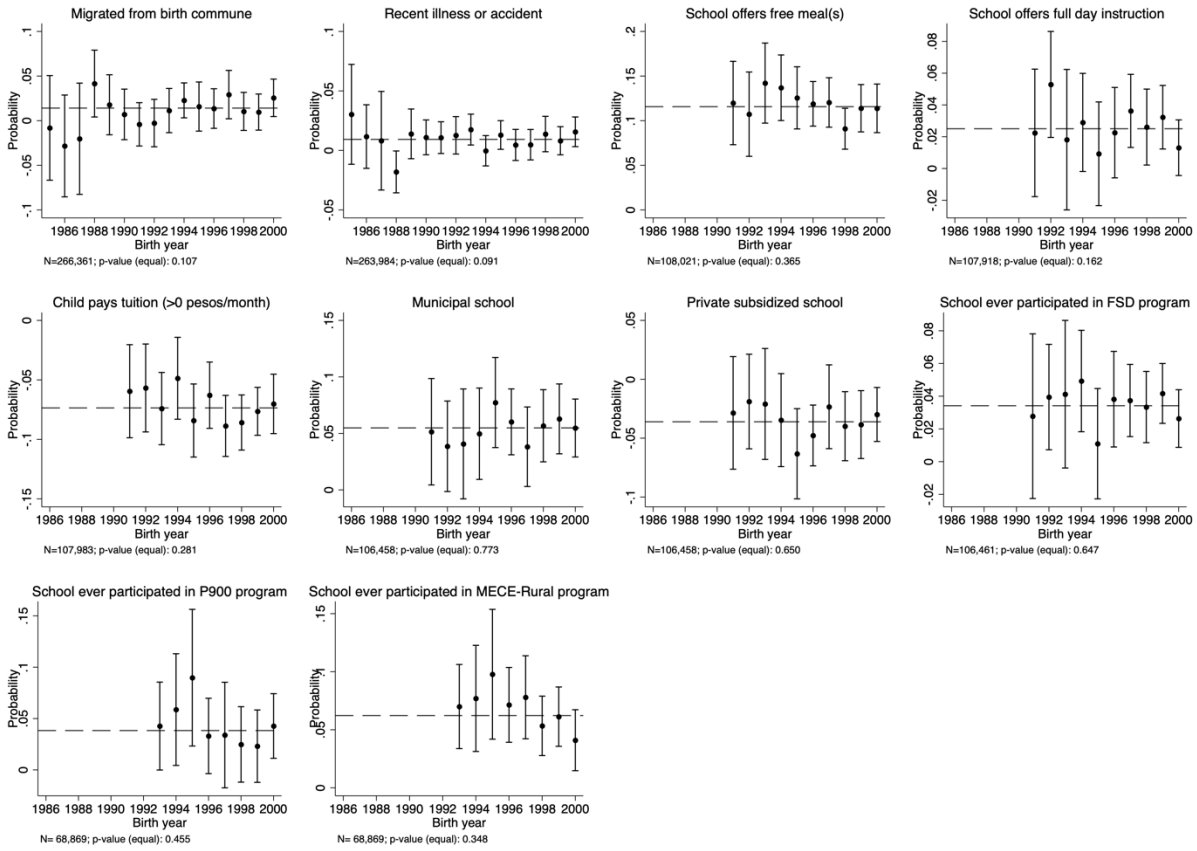


Panel B: Male sample



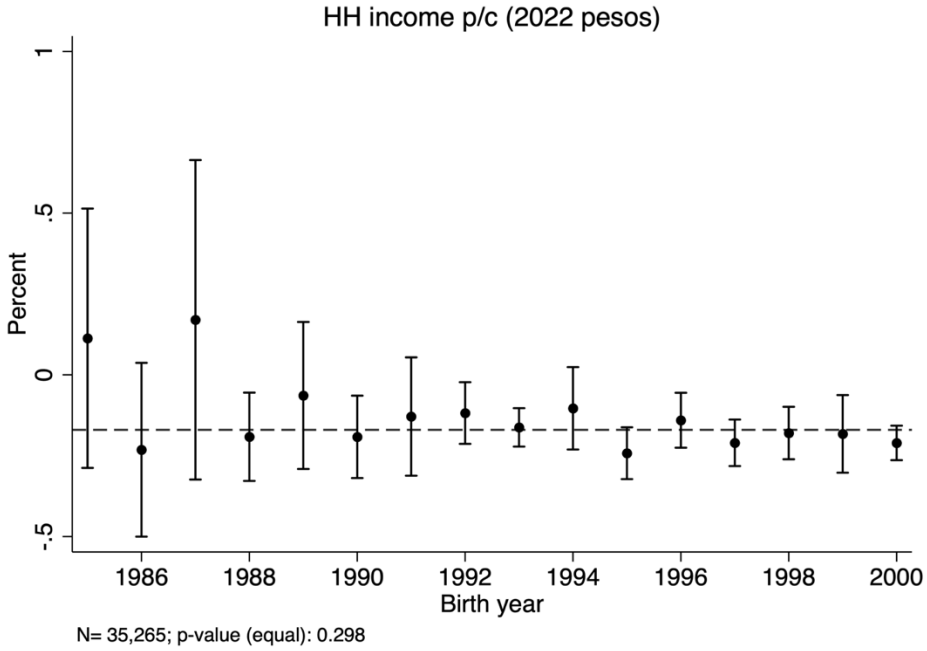
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Appendix Table A6. Each point and confidence interval corresponds to a $\hat{\tau}$ from equation (2). Top-left panels: Each circle corresponds to the dependent variable $1\{Y \geq x\}$, where Y is years of schooling and x is the value on the x-axis. Top-right and bottom panels: Each circle corresponds to the dependent variable $1\{Y > x\}$, where Y is labor earnings or hours worked, and x is the value on the x-axis.

Figure A6: Post-treatment trends in childhood inputs of indigenous children and adolescents



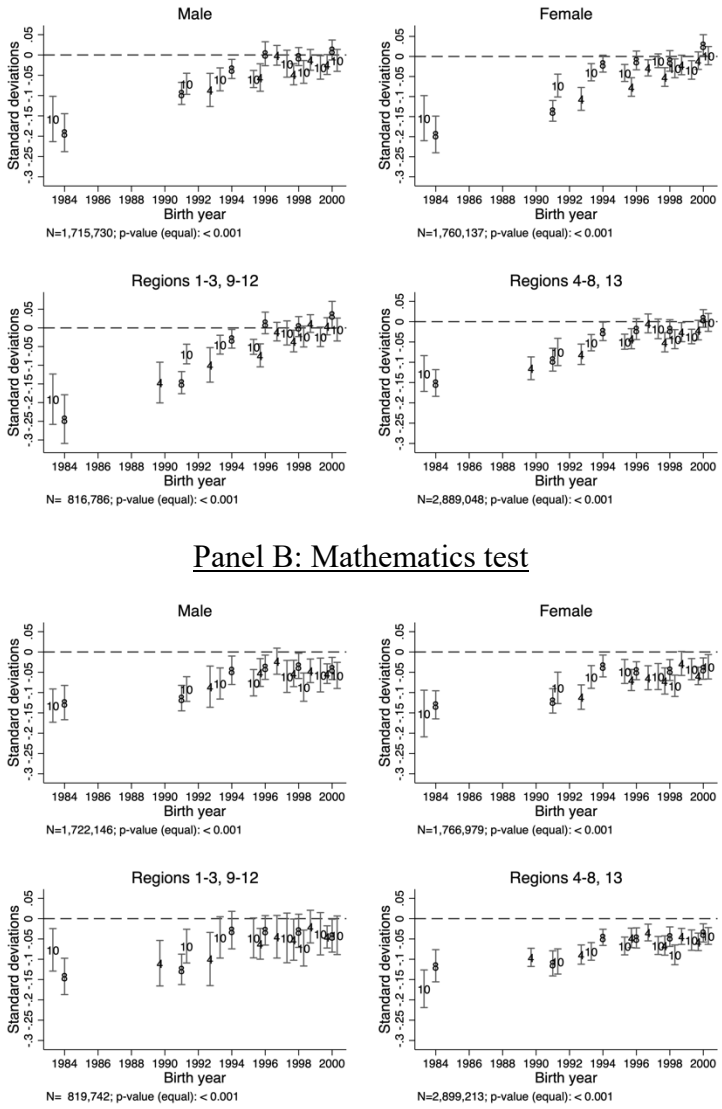
Notes: Each panel uses the FG2 sample (see Table 1). The circles are $\hat{\lambda}_e$ from equation (3), with pointwise 95% confidence intervals. Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. P-values correspond to the null hypothesis that coefficients are jointly equal. The dashed lines are estimates in which the coefficients are constrained to be equal.

Figure A7: Post-treatment trends in household income of indigenous grant recipients, relative to indigenous non-recipients



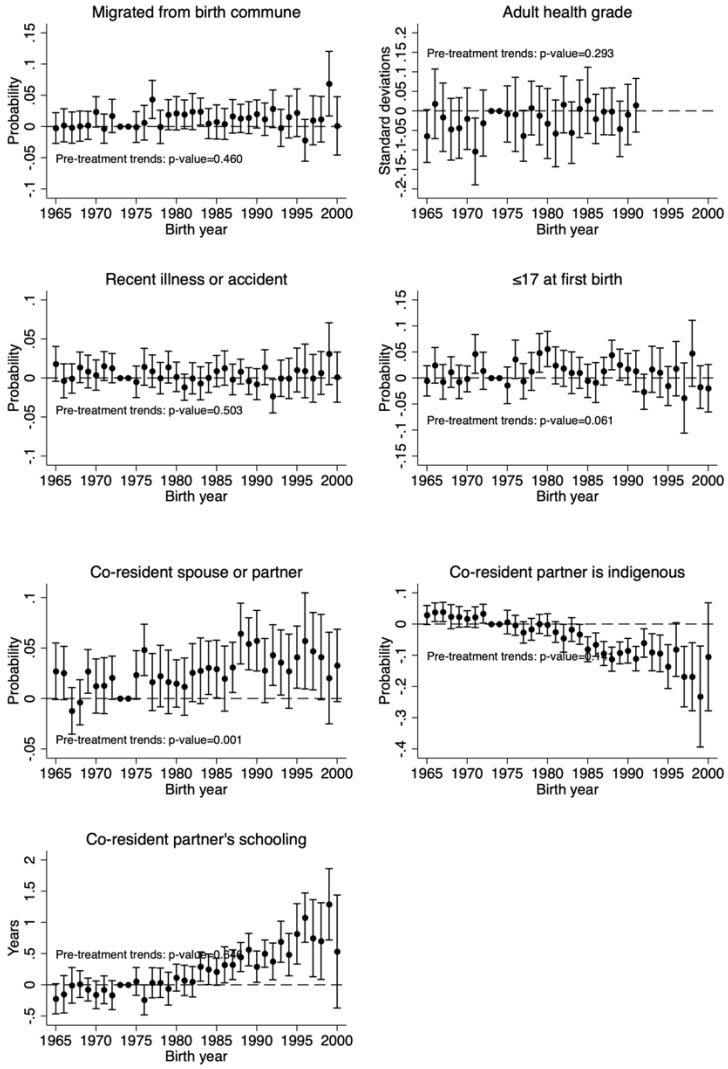
Notes: The panel uses the FG2 sample (see Table 1), limited to indigenous individuals between 6 and 21. The circles are $\hat{\lambda}_e$ from a modified equation (3), with pointwise 95% confidence intervals, in which I_{icmt} is replaced with a dummy variable indicating that indigenous individuals currently receive an indigenous grant. Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. P-values correspond to the null hypothesis that λ_e are jointly equal. The dashed lines are estimates for which the λ_e are constrained to be equal across birth years.

Figure A8: Post-treatment trends in test scores (female, male, and regional samples)



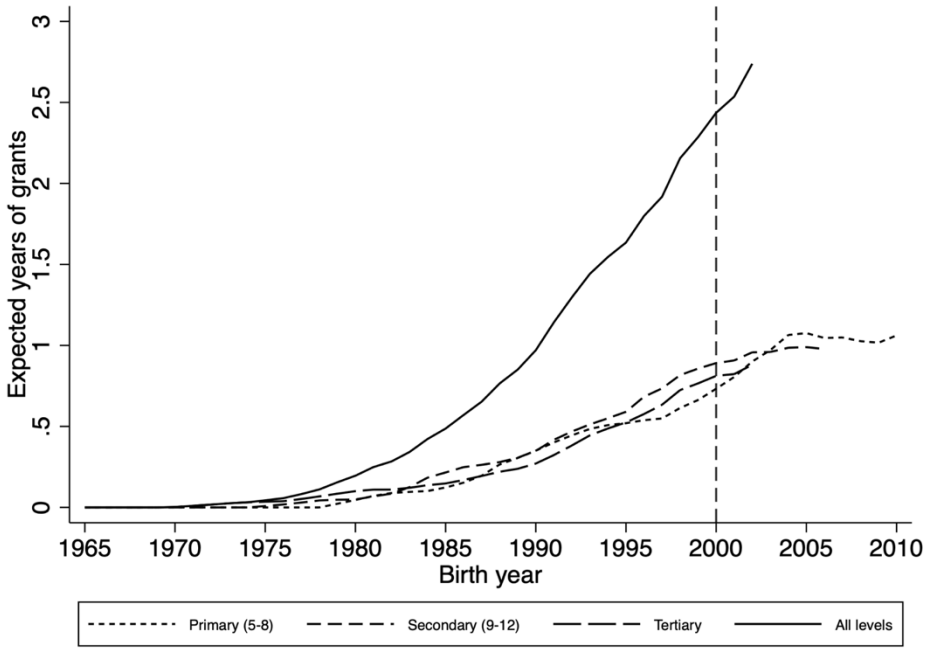
Notes: Each panel uses the FG3 sample (see Table 1), within four subsamples. Northern and southern regions are 1-3 and 9-12, while central regions are 4-8 and 13. The labeled points are $\hat{\lambda}_{eh}$ for each grade from equation (4), with pointwise 95% confidence intervals. Standard errors are adjusted for clustering by schools' communes. P-values correspond to the null hypothesis that coefficients are jointly equal.

Figure A9: Event study plots for additional variables in FG1



Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Table 3. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$. The circles are $\hat{\gamma}_e$ and $\hat{\lambda}_e$ from OLS estimates of equation (1), with pointwise 95% confidence intervals.

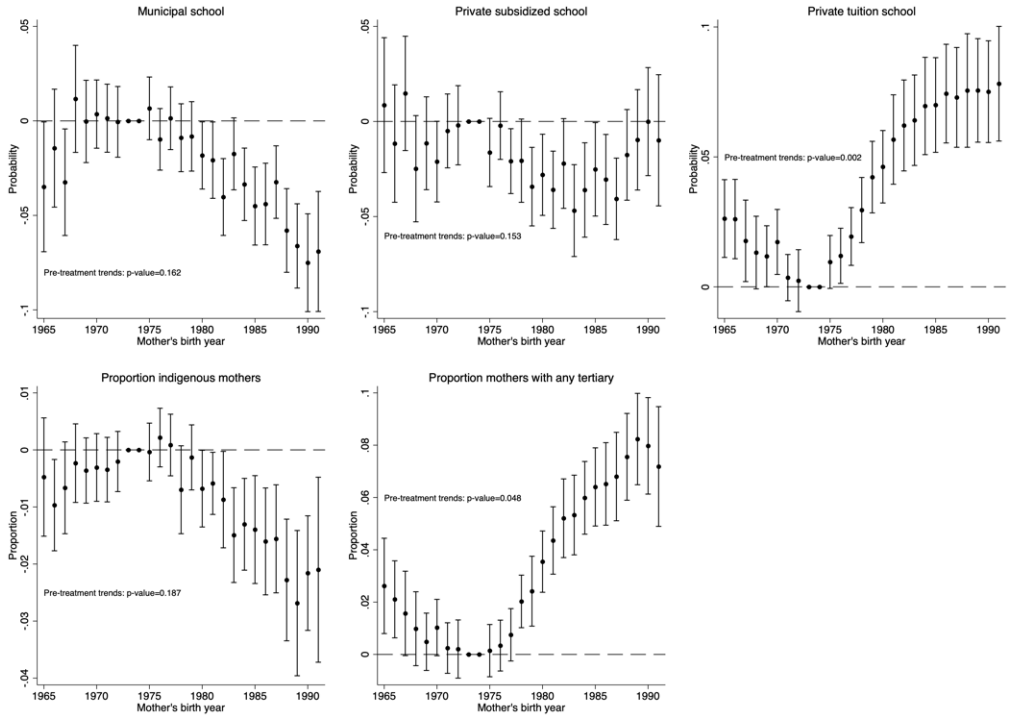
Figure A10: Expected years of indigenous grants, by birth year (1965 to 2010)



Notes: The figure extends Figure 1 (panel B) using all available administrative data until the 2023 program year.

Figure A11: Event study plots for dependent variables in Tables 4 and 5, and Appendix Table A12

Panel A: Event study plots for SG1 sample (Table 4)



Panel B: Event study plots for SG2 sample (Table 5 and Appendix Table A12)

