Targeted education transfers reduced long-run and intergenerational ethnic inequality in Chile[*]

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March 18, 2025
Most recent version

Abstract: Since 1991, Chile has provided large, renewable cash grants to indigenous children in lower-income households, conditional on enrollment in primary, secondary, and tertiary grades. We estimate intent-to-treat effects of grant exposure on indigenous adults, leveraging variation in expected grant years 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 grades, but reduced second-generation use of the grants.

[*] We received financial support from a Spencer Foundation Small Grant. We are also grateful for support from the Núcleo Milenio MOVI (Intergenerational Mobility) at the Pontificia Universidad Católica de Chile. David Evans, Raissa Fabregas, Alvaro Hofflinger, Valerie Michelman, Marigen Narea, 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.

1. Introduction

Latin America has stubbornly high rates of inequality in wealth and income, despite progress in the 2000s (Levy and Schady, 2013; Chancel et al., 2022). For nearly three decades, conditional cash transfers (CCTs) have been proffered as a way of reducing short-run and long-run inequality, via poverty-targeted transfers that are conditioned on investments in children's human capital (Fiszbein and Schady, 2009; Araujo et al., 2024; Banerjee et al., 2024). Latin America currently has 39 education-conditioned policies, in part because of robust evidence that CCTs increase school attainment.[1] However, the empirical record is still uncertain as to whether CCTs increase long-run welfare of either treated children or subsequent generations. 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 effects on adult labor supply and earnings are mixed and still nascent, not least because treated children and youth are not yet in labor markets (Molina Millán et al., 2019; Araujo et al., 2024).[2] Third, there is no evidence on inter-generational spillovers of cash transfers in Latin America, and very little elsewhere (Duflo et al., 2024; Baird et al., 2019).

Using a quasi-experimental design, this paper estimates the long-run and intergenerational effects of a scaled-up, government-run CCT that has targeted education grants to indigenous, lower-income households since 1991. In 2022, 10% of 20 million Chileans claimed indigenous ancestry, predominantly Mapuche.[3] The *Beca Indígena* (or Indigenous Grants) program offers cash grants to indigenous students enrolled in upper-primary grades (five to eight), secondary

[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 (2024); Parker and Vogl (2023); and Barrera-Osorio, Linden, and Saavedra (2019). Notwithstanding this evidence, cash transfers are also popular in Latin America because they have political returns for incumbent parties (Galiani et al., 2019; Manacorda, Miguel, and Vigorito, 2011).

[2] García and Saavedra (2022) report a meta-analysis of studies with heterogeneous test score outcomes and cannot reject the null of a zero effect, although one-third of studies find a positive effect. In Mexico, an experimental follow-up found robust effects on earnings (Araujo and Macours, 2021), though a quasi-experimental evaluation found large effects on labor participation and earnings for women, but null effects for men (Parker and Vogl, 2023). A Nicaraguan experiment found increased earnings for men but not women (Barham et al., 2024). A Honduran study found no effects, but with caveats about statistical power and the length of time spent by young adults in the labor market (Molina Millán et al., 2020). Ghanaian and Indonesia experiments also find minimal effects on incomes (Cahyadi et al., 2020; Duflo et al., 2024).

[3] Mapuche are 78% of the national indigenous population, followed by Aymara (9%), Diaguita (7%), and Quechua (2%). Other groups include Rapa-Nui, Atacameño, Collas, Kawashkar, Yámana, Diaguita, and Chango. In this paper, we code Chango as non-indigenous to maintain consistency with earlier CASEN surveys that omitted it (see Appendix Table A1).

grades, and tertiary degrees in universities and vocational institutes. To be eligible, students must obtain ancestry-based certification of indigenous status, pass a proxy means test, and receive average grades above a low but not failing threshold. Eligible students must then apply for grants. A government agency awards grants to applicants with relatively lower incomes but higher grades. Once awarded, annual grants are renewable within primary, secondary, or tertiary levels. The grants at each level are meaningful, constituting 9%, 18%, and 56%, respectively, of the median household income per capita of recipients.

The 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. In contrast, non-indigenous children were never treated. The identification strategy is possible 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 Chileans born between 1965 and 2000 and surveyed as adults (ages 22 to 57) in household surveys between 2006 and 2022. 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, since the schooling, labor earnings, and hours worked of pre-treatment indigenous cohorts were 1.36 years, 28%, and 8% lower, respectively, than non-indigenous cohorts. Using ITT estimates and administrative data, we

calculate a large marginal value of public funds (Hendren and Sprung-Keyser, 2020). For each dollar of net government expenditures—substantially offset by increased consumption tax revenues—there are $190 in after-tax earnings benefits.

We rule out several threats to the internal validity of the preferred estimates. We find no evidence of differential pre-treatment trends in years of schooling or labor market outcomes. In addition, we 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 demonstrably effective education programs of the same era (e.g., Chay, McEwan, and Urquiola, 2005). Indeed, the only post-treatment trend that we identify in the auxiliary sample is in childhood exposure to the grants themselves.

In addition to increasing years of schooling, the grants increased human capital in the form of learning per year of school in primary and secondary grades. We find within-commune 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. The convergence largely eliminated ethnic test score gaps among observationally-similar students within communes. Grant exposure did not affect other potential channels of earnings effects, including migration to another commune, improved adult health, or reductions in early fertility of women (although it 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. Indeed, one year of expected grant exposure of mothers increased the second- and fourth-grade test scores of children by 0.13 to 0.17 standard deviations. The effects are not explained by increases in children's grant exposure. Indeed, we find 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. As potential channels of these effects, we document a broad transformation in the socioeconomic environments of children with mothers exposed to the indigenous grants. This includes better-educated mothers and fathers, increased household income per capita, increased enrollment in tuition-charging private schools, and exposure to school peer groups with more-educated parents.

The paper makes four substantial contributions. 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. Identification under continuous treatment intensity imposes restrictions on treatment effect heterogeneity across 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). There is no evidence from Chile on the long-run effects of cash transfers, though several studies use discontinuous variation to identify the schooling attainment and earnings effects of means-tested loans in Chile for tertiary grades (Solis, 2017; Bucarey et al., 2020; Aguirre, 2021).

Second, this is the first paper to identify intergenerational effects of a Latin American CCT on child human capital. In Ghana, secondary school scholarships for girls increased first-generation schooling attainment, but not earnings (Duflo et al., 2024). The program delayed childbearing and improved early education and health outcomes of the second generation, the plausible result of maternal schooling gains. A related literature uses national shifts in education policy to identify intergenerational spillovers of education, in developing and developed countries. In Indonesia, for example, school construction increased the school and health outcomes of second-generation children (Akresh, Halim, and Kleemans, 2023; Mazumder, Rosales-Rueda, and Triyana, 2023).[4] Finally, we contribute to a literature on the intergenerational transmission of social safety net use, finding that maternal exposure to well-targeted grants actually reduced the likelihood of children's participation in the same program. This contrasts 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).

[4] There is a larger literature in developed countries on the intergenerational transmission of education and health outcomes. For example, tertiary education improved second-generation birth outcomes (Currie and Moretti, 2003), and treated generation exposure to Medicaid increased second-generation health at birth (East et al., 2023). The effects of compulsory school laws on offspring education is mixed (Oreopoulos et al. 2006; Black et al., 2005). In Chile, Barrios-Fernández et al. (2024) document intergenerational transmission of elite educational opportunities—but not human capital—across relatively higher-income samples than those of our paper.

Third, we report the first causal evidence of how a scaled-up, government-implemented policy can substantially reduce ethnic inequality, directly and via intergenerational spillovers.[5] People of indigenous ancestry are 8% of Latin America's population, and 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 large 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., in press). The literature advocates focusing education transfers on students likely to reap the greatest program benefits (Araujo et al., 2024), in part because CCTs are less cost-effective than other interventions at increasing a unified metric of enrollment and learning (Angrist et al., in press). Conditional cash transfers have weak effects if untargeted (Dustan, 2020) or poverty-targeted but with leakage to less-poor households (de Hoyos, Attanasio, and Meghir, 2021), plausibly because inframarginal students receive transfers but do not invest more in human capital. Even in policies that primarily reach poor households, the size of effects varies substantially across experimental strata defined by a poverty proxy (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, although it is rarely employed.[6]

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. The following sections describe

[5] Bilingual education programs are a common but infrequently evaluated school-based policy for addressing ethnic inequality in Latin America (PEIB, 2017; Näsland-Hadley and Santos, 2022). The programs are usually targeted to rural communities with many speakers of indigenous languages. In 2022, our data show that 80% of Chileans with indigenous ancestry do not speak or understand indigenous languages, and more than 25% live in Chile's capital.
[6] Brazil's *Bolsa Familia* program is the only large-scale program to directly target indigenous populations (Ibarrarán et al., 2017), along with scholarships targeted at indigenous students in a few countries like Chile, Peru, and Mexico. Except for Chile, these are focused on tertiary education, and relatively smaller in scale. Molina Millán et al. (2020) find that a Honduran cash transfer had negligible effects on the school attainment of a sizable indigenous minority, despite larger and durable effects for non-indigenous children.

(1) the minimum requirements for grant eligibility, (2) the procedures for grant application and renewal, (3) grant values, and (4) 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. Second, indigenous children and youth must be enrolled in upper-primary grades (from five to eight), secondary grades, or a tertiary degree in a university or vocational institute. Third, students must demonstrate a prior-year grade point average of 5 out of 7 points in primary or secondary grades and 4.5 out of 7 points in tertiary grades, a standard higher than failing thresholds.[7] 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, 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 in the next level of schooling.

Grant amounts vary by primary, secondary, and tertiary levels. In 2021, the grants were, respectively, 100,550 pesos (US$118); 208,280 pesos (US$245); and 654,600 pesos (US$770). These were 9%, 18%, and 56% of the median annual household income per capita of grant recipients.[8] While nominal grant amounts have increased in many years, inflation has modestly eroded their value (see 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 obligatory accounts in *BancoEstado*, Chile's state-owned bank. Deposits for primary-aged

[7] In publicly-funded schools, students fail a grade if (1) they score below 4 in one subject and their average is less than 4.45, or (2) they score below 4 in two subjects and their average is less than 4.95 (Díaz et al., 2021).
[8] Grant values and incomes are expressed in 2022 prices. We estimate the median of the annual per capita household income for indigenous grant recipients in the auxiliary child sample (FG2) described in Table 1.

children are directed to a parent or guardian account, while others receive deposits in individual accounts.[9] JUNAEB places no restrictions on how grant funds can be spent.

The program expanded over three decades that spanned democratically-elected governments on the left, center, and right. 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.[10] By 2021, over 92,000 grants were awarded. Despite this growth, the majority of indigenous students do 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 born in a given year.

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

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

[9] Girls younger than 12, and boys younger than 14 receive deposits via a parent or guardian.
[10] The number of tertiary grants in Figure 1 includes two small programs, also administered by JUNAEB, that use similar targeting criteria and values. These include a cash stipend to support the housing of students during tertiary studies, and the in-kind offer of housing in a small number of indigenous dorms in cities.

where I_{ic} takes the value of 1 for indigenous adults (such that E_{ic} is always zero for non-indigenous adults). N_c is the number of indigenous children born in year c that were alive just before grant eligibility in fifth grade.[11] 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 $\frac{G_{2000}^{2010}}{N_{2000}} = \frac{5{,}178}{33{,}124} = 0.156$.[12] 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-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 an event study design, which we use to identify intent-to-treat effects on successive birth cohorts of indigenous adults, relative to non-indigenous adults. We emphasize that the design does not rely on grant dosage of adults measured at the individual level, for two reasons related to data and causal identification. First, our survey data do not retrospectively measure the cumulative grants received by adults. However, the data confirm that only indigenous children receive grants, and that indigenous recipients have lower incomes than indigenous non-recipients (see Appendix Table A4). Second, the allocation of grants—including eligibility, application, and awards—are endogenous to individual and household determinants of adult outcomes. Instead, we use the exogenous expansion of the program to identify ITT effects on entire birth cohorts of indigenous adults.

[11] 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" (see Appendix Table A1). We adjust 2017 cohort sizes with a life table, to reflect expected cohort sizes at 5 to 9 years of age, just before grant eligibility.

[12] 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.

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 omitted years of 1973 and 1974.[13]

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.[14] These include local economic conditions, the quality of school and primary health services provided by municipal governments, and national policies that disproportionately target lower-income or rural areas. Together, the fixed effects ensure that differences-in-differences are estimated within birth communes. We use birth communes—rather than communes of adult residence—because migration is potentially endogenous to childhood exposure to indigenous grants.

The specification includes fixed effects for survey years (μ_t), since the dataset pools adult observations from multiple household surveys. The controls in X'_{icmt} include age dummies, a gender dummy, and the parental schooling of indigenous and non-indigenous adults. 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 $\gamma_e = 0\ \forall\ e$.

[13] The research design avoids well-documented challenges to the interpretation two-way fixed effects models with staggered rollout (Roth et al., 2023). Rather, it is a difference-in-differences design with a common start and increasing treatment intensity thereafter (Goodman-Bacon, 2021; Miller, 2023).

[14] Chile is divided into 346 communes, although 28 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 (see Appendix Table A3).

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 born in overlapping years.

For ease of interpretation, we 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.[15] 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, though in program years with fewer grants (an average E_{ic} of 0.65 years). Finally, cohorts from 1992 to 2000 were exposed for 12 years in program years with relatively more grants (an average E_{ic} of 1.86 years).

We report a complementary specification with the continuous measure of expected grant years (E_{ic}):[16]

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

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 parallels 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 affects the school outcomes of co-resident children. In these specifications, we restrict samples to mothers born from 1965 to 1991 (a decision imposed by survey timing and sample size). The dependent variable is now Y_{ijcmt}, the outcome of child *j* born to mother *i*. As before, mothers were born in year *c* and commune *m*, and children and mothers were jointly surveyed in year *t.* The fixed

[15] The specification assumes that $E_{ic} = 0$ in birth years from 1970 to 1974. In Appendix Table A4, 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). Given the compelling evidence of parallel pre-treatment trends, the estimates are similar to the preferred ITT estimates.

[16] 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.

effects are otherwise similar, but the X'_{ijcmt} include indicators of mothers' ages, children's birth years, and child gender.

We consider two additional threats to internal validity in the discussion of results. First, the program's scale-up (see Figure 1) raises the possibility that "intergenerational" effects are confounded with indigenous children's own exposure to grants. Second, women's grant exposure may affect fertility and/or the likelihood that children are co-resident with mothers at the time of a survey, perhaps introducing sample selection bias.

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 tests from SIMCE (the *Sistema de Medición de la Calidad de Educación*).[17] Table 1 summarizes five samples used to implement and evaluate the research design.

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. Each survey measures indigenous status with an ancestry-based question that is consistent with CONADI's legal definition (see Appendix Table A1). In contrast to pre-2006 rounds, the surveys collect the commune of individuals' birth. To estimate equations (1) and (2), we merge CASEN observations to proxies of adults' parental education from the IPUMS 10% sample of the 2002 census.[18]

[17] CASEN microdata are available at https://observatorio.ministeriodesarrollosocial.gob.cl/encuesta-casen. SIMCE microdata are available by application to https://www.agenciaeducacion.cl/simce/. Earlier CASEN surveys are available, but they do not collect individuals' birth communes.

[18] See https://international.ipums.org/international-action/sample_details/country/cl. 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. We merge the four variables to individuals in FG1 and FG2 (see Table 1). Appendix Figure A2 illustrates the means of the four variables in FG1. Parental schooling is increasing across birth years, and the proportion of missing values is

The main dependent variables are (1) years of completed 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—including migration, health, fertility, and partner attributes—to assess the causal channels of long-run and intergenerational 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. Using retrospective data on the women's ages at first birth, we construct a dummy variable indicating a 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.

We complement this main sample with an auxiliary first-generation sample (FG2) that contains fewer birth cohorts because of CASEN survey timing. FG2 includes children and youth between 6 and 21 years-old who were born from 1985 to 2000. This sample facilitates tests for post-treatment trends in household and school variables. The motivating concern is that our findings conflate the effects of indigenous grants with those of similarly-trending household and school variables. Thus, any post-treatment trends observed in FG2 cast doubt on the internal validity of estimates from equations (1) and (2). FG2 does not include children born before 1985. However, the range of birth years coincides with large increases in cohorts' exposure to indigenous grants (see Figure 1). We discuss the variables and tests in section 6.

Finally, a first generation sample of test scores (FG3) includes children and youth in fourth, eighth, or tenth grades between 1997 and 2015 who were born from 1983 to 2000. We pool 21 grade-specific SIMCE assessments and use the sample to test for post-treatment trends in language and mathematics test scores between indigenous and non-indigenous children.[19] We do

diminishing, but the variables evolves similarly in the indigenous and non-indigenous samples. Regressions also include dummy variables indicating observations with missing values of the merged census variables.

[19] SIMCE parent surveys between 1997 and 2000 only recorded the indigenous status of mothers, rather than mothers and fathers as in subsequent years. We apply this limited definition across all rounds for consistency. Parent surveys did not collect indigenous status between 2001 and 2005, and in 2013. When possible, we infer indigenous status for rounds by using the implied panel structure of SIMCE rounds and a masked student identifier (MRUN) to impute indigenous status from earlier or later appearances of the same student. We code students as indigenous if their mothers ever identified as indigenous in any round.

not observe test scores for children born in prior to 1983. However, as with FG2, the range of birth years coincides with large increases in exposure to indigenous grants.

B. Second-Generation Samples

We study the educational outcomes of second-generation children who reside in households with mothers born between 1965 and 1991. The main sample, SG1, includes SIMCE test scores of second-graders (from 2012 to 2015) and fourth-graders (from 2014 to 2017).[20] We link SIMCE records to administrative data of the Ministry of Education that include students' cumulative grade point averages, attendance, communes of residence, and birth years.[21] We focus on these datasets and birth cohorts of children to minimize the likelihood of confounding intergenerational effects of indigenous grants with children's own grant exposure. We elaborate on this issue in our discussion of the results.

We complement results from the SG1 sample with evidence from SG2, a CASEN sample of children under 22 who are co-resident with FG1 mothers born between 1965 and 1991. We use the sample to assess likely channels of intergenerational effects on child test scores, such as increased household incomes. Importantly, the sample includes a direct measure of children's receipt of indigenous grants, allowing us to test for intergenerational persistence in the take-up of a social program.

5. Long-Run Effects on Schooling, Earnings, and Labor Supply

A. Event Study Plots and ITT Estimates

Figure 2 includes event studies for the four main dependent variables. Each panel illustrates the $\hat{\gamma}_e$ and $\hat{\lambda}_e$ for a dependent variable, bracketed by 95% confidence intervals. The solid line in each panel plots $\hat{\tau} * E_{ic}$, the predicted effect from the ACR estimate multiplied by expected grant years. 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

[20] Eighty-five percent of respondents to parent questionnaires are mothers, and they report their ages. As with indigenous status, we use the implied panel structure of SIMCE rounds and a masked student identifier to impute mother's age when 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.
[21] See https://datosabiertos.mineduc.cl/rendimiento-por-estudiante-2/.

zero.[22] For birth years between 1975 and 1982, the early years of grant availability, the $\hat{\lambda}_e$ are mostly positive, small, and not statistically distinguishable from zero, which is consistent with relatively low grant exposure (see Figure 1). 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 may be induced by grants exposure. Instead of using scale-sensitive transformations of zeroes (e.g., $\log(x+1)$), 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 figure reports exponenitated coefficients, $exp\,(\hat{\gamma}_e) - 1$ and $exp\left(\hat{\lambda}_e\right) - 1$, and standard errors calculated with the delta method. As with schooling, 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 potentially reflect increased labor supply on the extensive and intensive margins. Event studies in 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 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 simplify interpretations and 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%

[22] We define 1965 as the first cohort because it was the first to enter primary school (in 1971) after the conclusion of the presidential administration of Eduardo Frei Montalva (1964-1970). A school-building campaign in rural areas alleviated supply constraints in upper primary grades (Bellei and Pérez, 2016). Appendix Figure A3 includes event study plots for cohorts born between 1960 and 1964. For these cohorts, there is visual evidence of a pre-treatment trend for schooling, but not labor earnings.

increase in weekly hours worked (column 4). For each variable, we reject the null hypotheses that the three coefficients are jointly equal, or jointly equal to zero.

To interpret the magnitudes of ITT estimates, the bottom of Table 2 reports the means of the dependent variables in samples of never-treated adults born from 1965 to 1974. Indigenous adults in these 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 $\hat{\tau}$, the average causal response to expected grant years under a strong parallel trends assumption (see section 3). A one-year increase in E_{ic} increases 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 1 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 A4 plots these $\hat{\lambda}_e$ against E_{ic} rather than birth years, but still 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}, while noisy, are broadly 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 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.[23] 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 individuals earned a positive income in the last month.[24] 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 ($1\{Y_i > x\}$), such that the coefficient for $1\{Y_i > 0\}$ indicates any hours of work in a typical week, relative to none. There is a very modest increase in the coefficients between 0 and 35 hours per week, suggesting a smaller effect on intensive-margin labor supply than on the probability of working at least 40 hours per week.

D. Gender

[23] Several very small indigenous groups are eligible for indigenous grants in earlier primary grades (JUNAEB, 2024), so that the evidence of small effects in these grades is still consistent with formal rules.
[24] 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.

There are robust and statistically significant effects on the schooling and labor market outcomes of men and women (see Appendix Table A6).[25] The ACR estimate for 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 labor market discrimination (Galarza and Yamada, 2017). Appendix Figure A6 reports ACRs across the distribution of schooling, labor earnings, and weekly hours worked, in male and female samples. 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 several threats to internal validity. First, we use the auxiliary sample of first-generation children (FG2) to test for post-treatment trends in household inputs to which indigenous and non-indigenous adults were exposed as children. Second, we present tests for post-treatment trends in school inputs. In both cases, the concern is that parallel pre-treatment trends do not ensure that parallel trends hold in post-treatment cohorts (Roth et al., 2022).[26] Third, we assess whether increasing ITT effects can be explained by endogenous selection of indigenous status. Fourth, we assess whether estimates are very different in regions with higher indigenous concentrations, perhaps because of policies targeted at indigenous households in these regions. Fifth, we discuss evidence for a "strong" parallel trends assumption, necessary to interpret $\hat{\tau}$ as the ACR of expected grant years (Callaway et al., 2024).

A. Post-Treatment Trends in Household Inputs

[25] Appendix Figure A5 reports event study plots for men and women. While noisier, they coincide with the estimates from Appendix Table A5.

[26] As a simple illustration, Appendix Figure A7 plots the preferred event study estimates against those of a "naïve" specification that that omits cohort-by-commune interactions: $Y_{icmt} = I_{icmt}[\alpha + \sum_{e=1965}^{1972} \gamma_e 1\{c = e\} + \sum_{e=1975}^{2000} \lambda_e 1\{c = e\}] + \delta_c + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt}$. The estimates and p-values are consistent with parallel pre-treatment trends in both specifications. However, there are larger estimates in the naïve specification, most clearly for birth cohorts in mid-1980s and later.

Suppose that indigenous children in post-treatment birth cohorts are exposed to increasingly favorable household inputs relative to non-indigenous children, even within birth communes and controlling for parent schooling. We test for such trends using the FG2 sample of children and youth born between 1985 and 2000 (see Table 1). The birth years overlap with notable increases in both E_{ic} and ITT estimates (see 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 input of 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 in a given birth year. 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.

To illustrate the test, consider two contrasting examples. The top-left panel of Figure 4 illustrates the $\hat{\lambda}_e$ for a dependent variable indicating that a child received an indigenous grant in the survey year. The dashed line is a coefficient estimate constrained to be equal across all birth years. Between the 1985 and 2000 cohorts, indigenous children were increasingly likely to report receiving an indigenous grant, and we reject the null of coefficient equality at 1%. The solid line plots the sample average of the probabilities that children in each cohort-by-age group received indigenous grants. (The probabilities from Appendix Table A2 are calculated with census and administrative data.) Thus, two data sources confirm that successive birth cohorts of indigenous children were increasingly exposed to grants.

In contrast, the top-right panel illustrates the $\hat{\lambda}_e$ for a dummy variable indicating that children received any other education grant. Other grants were allocated on geography, income, or merit, but never indigenous status.[27] There is no evident trend, and we fail to reject the null of equality at conventional significance levels. The bottom panels of Figure 4 report similar tests for household total income per capita and household transfer income per capita, neither of which includes education grants. The former is a general proxy of household inputs and socioeconomic status, while the latter summarizes exposure to a variety of government transfer programs that

[27] As two examples, 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.

are usually means-tested.[28] There are no evident trends, and we fail to reject the nulls of coefficient equality. Appendix Figure A8 illustrates similar results for other child and household variables, including whether children migrated from their birth commune and whether children experienced a recent illness or accident.

B. Post-Treatment Trends in School Inputs

We present similar tests for differential, post-treatment exposure of children to school inputs (see Appendix Figure A8). First, there is no evidence of trends in the marginal probability of enrolling in municipal schools or private subsidized schools introduced by Chile's "voucher" reform in 1981.[29] Relatedly, there is no trend in the marginal probability that indigenous children pay tuition to any type of school. Second, there is no evidence of trends in relative exposure to a range of national school policies in publicly-subsidized primary and secondary schools. These include free school meals for lower-income schools and children (McEwan, 2013) and a Full School Day (FSD) program that increased instructional time 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 introduced larger school subsidies for students in the lowest 40% of household incomes (Aguirre, 2022). The program started with students between pre-kindergarten and fourth grade in 2008, and added a single grade in each subsequent year until full secondary grade coverage was attained in 2016 (Neilson, 2020). Given this roll-out, birth cohorts in 1999 and 2000 were potentially affected by increased subsidies, although indigenous status was not directly used to target student subsidies. Appendix Table A7 shows that the results are robust to the exclusion of both cohorts.

[28] For example, there are monthly, unconditional transfers available to poor households with children (the *Subsidio Único Familiar*), the disabled and elderly poor (*Pensión Asistencial*), and for household water supply (*Subsidio de Agua Potable*). Another program that started in 2002, *Chile Solidario*, provided additional cash transfers and supplementary services to extremely poor households (Carneiro et al., 2018).

[29] The reform offered similar per-student subsidies to municipal and private schools, and private market share increased sharply in the 1980s (Hsieh and Urquiola, 2006). This preceded the largest growth in ITT estimates among cohorts born the 1990s.

C. Endogenous Selection of Indigenous Status

Suppose that non-indigenous children and youth manipulate their indigenous status to receive a grant. If marginal "switchers" have unobserved attributes associated with higher adult earnings, then endogenous switching that increases with grants exposure could bias the ITT estimates in Figure 2. This behavior is likely not happening. Switching behavior would likely introduce post-treatment trends in childhood variables correlated with the behavior, such as household income. However, we found no evidence of such trends for many variables.

In fact, indigenous status is a legal definition. To be considered indigenous by the Chilean state, individuals must obtain legal certification of indigenous status, based on surnames recorded on birth certificates. To apply for a grant, households must have this certification and pass a proxy means test based on administrative data. Given the legal barriers, there is no incentive to mis-represent indigenous status in household surveys, especially for higher-income households. On the contrary, there is a likely stigma to doing so, since non-indigenous Chileans hold negative stereotypes of indigenous identity, and indigenous respondents perceive discriminatory treatment in varied domains (Saiz et al., 2008; Merino et al., 2009).

Even so, 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 (see Table A1).[30] One hypothesis is that stigma has diminished across survey years, leading fewer indigenous adults to mis-report *non*-indigenous status. Equation (1) therefore 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 ($\sum_{m\in M,t\in T,} \theta_{mt} I_{icmt}$). The event study plots are visually similar (see Appendix Figure A9), and the summary estimates yield similar conclusions (see Appendix Table A8). Finally, we re-estimated equations (1) and (2) in a restricted sample of CASEN surveys between 2013 to 2022, with similar results.[31]

[30] We report weighted proportions. Unweighted proportions are larger because surveys over-sample smaller and rural communes that tend to have larger indigenous populations (see Appendix Table A1).

[31] 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.

D. Policies Targeted to Heavily-Indigenous Regions

Two national programs targeted indigenous households in heavily-indigenous regions. Our concern is that effects of these programs are confounded with those of indigenous grants. In the first program, CONADI subsidized land purchases for some indigenous households in 125 communes, although many of these received very small subsidies. 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 between those born in seven northern and southern regions, and those born in six central regions.[32] Twenty-five percent of the former sample is indigenous, versus 5% of the latter sample. Indigenous adults in the former sample were much more exposed to both programs.[33] Table 3 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 in panel B show a similar pattern. In brief, there are robustly positive and large effects in Chilean regions with lower proportions of indigenous adults, and where communes were relatively unaffected by the policies.

In fact, this may understate the ACR estimates in central regions, and overstate ACRs in northern and southern ones. In 2017, for example, a central region (Metropolitan Santiago) received 14.9% of grants, though 27.1% of indigenous Chileans were born there (JUNAEB, 2018). A southern region (Araucanía) received 28.7% of grants, though 18.7% of indigenous Chileans were born there. The respective ratios are 1.54 and 0.55. As an additional robustness check, we multiply E_{ic} by the 2017 ratios for all 13 regions.[34] Table 3 (panel C) reports ACR

[32] We employ Chile's original numbering of 13 administrative regions. Northern regions are 1 to 3, southern regions are 9 to 12, and central regions are 4 to 8 and 13. Chile has added three regions since the original division. As with newly-created communes (see Appendix Table A3), we consistently code individuals' birth regions according to the original territorial division.

[33] We calculated the cumulative hectares distributed per indigenous person, within each combination of birth communes and survey years. (We obtained 20A and 20B land distribution records from https://siic.conadi.cl, and commune-level indigenous popuations 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.

[34] This is an application of Bayes' rule, since $P(G=1|R=r) = \frac{P(R=r|G=1)}{P(R=r)} \times P(G=1)$, where G indicates grant receipt and R indexes the 13 regions. Recall that E_{ic} is the sum of unconditional national probabilities of receiving a grant (i.e., P(G=1)).

estimates using the regionally-weighted E_{ic}. In this case, ACR estimates are closer, with slightly higher point estimates in central regions.

E. Strong Parallel Trends

The interpretation of $\hat{\tau}$ as the ACR of expected grant years depends on a heterogeneity restriction across E_{ic} (Callaway et al., 2024). Specifically, any two birth cohorts must experience the same ITT if exposed to the same value of E_{ic}. We discuss three potential violations. First, it is possible that indigenous birth cohorts systematically chose values of E_{ic} based on expected gains in schooling and earnings. However, this would unrealistically require that parents had the foresight to manipulate birth years to influence grants exposure 10 to 21 years after birth, based on heterogenous expected gains.

Second, the household attributes of treated indigenous children could vary within birth cohorts (e.g., by income). This seems unlikely given the consistently-applied targeting rules described in section 2. However, if treatment effects are heterogeneous with respect to income, the ITT effects of two indigenous cohorts may differ even if both receive the same E_{ic}. In fact, we find no evidence of post-treatment trends in household income per capita of successive birth cohorts of indigenous children who received, or did not receive, an indigenous grant (see Appendix Figure A10).

Third, supply-side school programs may have complementarities with demand-side grants (e.g., 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}. This does not threaten the internal validity of ITT estimates, because there were no relative changes (within communes) in the exposure of indigenous and non-indigenous children to school inputs such as the Full School Day program. However, it may threaten the degree to which relative changes in cohort-specific estimates exclusively reflect the average causal response to E_{ic}. In our example, 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). Circumstantially, the $\widehat{ACR}_e$ for later birth cohorts—which received more complementary inputs—are not steeper than earlier ones (see Appendix Figure A4).

7. Causal Channels of Long-Run Effects

One year of expected grants increases 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 test scores, migration, adult health, and 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 may have increased student effort due to GPA-related conditions (Scott-Clayton, 2011). Prior-year GPAs must exceed a low threshold to be eligible for grant application and renewal (see section 2). And, when there is excess demand for grants, the chances of receiving a grant increase linearly with prior-year GPAs, all else equal. 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 schooling as an engine of social and economic mobility (Webb 2015, 2022).

We test for effects on learning using SIMCE test score data. The FG3 sample (see Table 1) 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 grades.[35] The specification also controls for student-by-grade indicators of gender and parents' schooling attainment, school-by-grade indicators of private or municipal status, and whether

[35] We do not observe the birth commune of each student and instead use school communes, a reasonable alternative given evidence the absence of post-treatment trends in the migration status of children (see Appendix Figure A8).

schools ever participated in the Full School Day program, P-900, or MECE-Rural.[36] We cluster standard errors by school communes.

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 $\hat{\lambda}_{eh}$ and 95% confidence intervals. In the earliest available birth years (1983 and 1984), test scores of indigenous students are 0.15 to 0.2 standard deviations lower than non-indigenous students, even for students in the same commune with similar parent and school variables.

In the latest birth years, the differences are not statistically different from zero for language scores and approximately -0.05 for mathematics scores. Conditional convergence within communes is more notable for language than mathematics, but is similarly evident in the samples of girls and boys, and in regional subsamples (see Appendix Figure A11). The pattern is similar for all grades, even though fourth grade is prior to grant receipt. However, end-of-year GPAs in the fourth grade determine grant eligibility and awards in the fifth grade. This suggests that GPA conditions are more important than income effects in primary grades.

The results in Figure 5 are a truncated event study, in which the earliest cohorts were less treated, but not truly untreated. The implied differences-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 on schooling for these cohorts. The interpretation is tempered by the lack of data to measure pre-treatment trends.

B. Migration, Health, Fertility, and Partners

We assess several 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

[36] 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.

pregnancies that disrupt schooling or labor participation (Breierova and Duflo, 2004).[37] Fourth, schooling may affect marriage or co-residency with partners, and the attributes of partners via assortative mating (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 shows that nearly all confidence intervals include zero, before and after the omitted years, with a few outliers (see Figure 6). There are similar patterns in the event study plots for self-reported health grades and recent illness or accidents. We fail to reject the nulls (at 5%) that ITT coefficients are equal, or jointly equal to zero (see Table 4), and ACR estimates are small and not statistically different from zero. Figure 6 and Table 4 also show no evidence that women are less likely to have children at age 17 or younger. The ACR estimate is close to zero, and not statistically different from zero.

Grants modestly increased the probability of having a co-resident partner by 0.02 for each expected grant year, relative to 66% of indigenous adults in pre-treatment cohorts (see Table 4, and event study plots in Appendix Figure A12). Grants substantially changed partner attributes. One year of expected grants increased the partners' schooling by 0.5 years, and reduced the probability of having an indigenous partner by 9 percentage points. (Fifty-two percent of indigenous adults in pre-treatment cohorts have an indigenous partner, versus 6% of non-indigenous adults.) Lower rates of indigenous inter-marriage is a plausible byproduct of assortative mating on correlated variables like schooling and earnings. The results differ somewhat by gender, since effects on the presence of co-resident partners is driven by the male subsample (see Appendix Table A9).

8. Intergenerational Effects

This section uses samples of mothers and co-resident children to test for intergenerational effects of mothers' exposure to indigenous grants. The SG1 sample pools four rounds of second-grade test scores (from 2012 to 2015) and four rounds of fourth-grade test scores (from 2014 to 2017). The SG2 sample includes children under 22 who are co-resident with mothers from the FG1 sample.

[37] Berthelon and Kruger (2011) show that 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. We ruled out post-treatment trends in exposure to longer school days (see Appendix Figure A8), but the possibility remains that more years of school (relative to dropping out) may cause similar reduction in teen pregnancies.

A. Effects on Test Scores, Grades, and Attendance

There are two advantages of focusing on the SIMCE rounds in SG1. First, students were in early primary grades that were not eligible to receive indigenous grants. Second, the children are in birth cohorts from 2005 to 2008, when the expected years of indigenous grants in primary grades had stopped increasing, in contrast to pre-2000 cohorts (see Appendix Figure A13, which extends Figure 1 using program data until 2023).[38] We estimate the modified event study specification described in section 3, including dummy variables for child gender and birth years. Because we are stacking second- and fourth-grade observations, the regressions fully interact a fourth-grade dummy with all fixed effects and control variables. (This does not apply to math regressions because second-graders did not take the assessment.)

Figure 7 reports event study plots for language and mathematics tests, the cumulative grade point average in the same school year, and the annual proportion of school days attended. There is no visual evidence of pre-treatment trends for the test scores, and we fail to reject the null that pre-treatment coefficients are jointly zero. The ITT estimates for mothers born from 1975 to 1981 are very small—from 0.03 to 0.04 standard deviations—but statistically distinguishable from zero (see Table 5, columns 1 and 2). The estimates for mother born from 1982 to 1991 are larger, from 0.08 to 0.11 standard deviations. Each expected grant year of mothers increased child test scores by 0.13 to 0.17 standard deviations. The estimates are large, given unconditional test score gaps among children of pre-treatment mothers that are 0.20 and 0.27 standard deviations, respectively, in language and math.

Year-end grade point averages (GPAs) are standardized to z-scores within grade-by-year rounds. In Figure 7, there is no evidence of pre-treatment trends for GPA. There are positive and increasing ITT estimates, comparable in magnitude to estimates for test scores. In Table 5, one year of expected grants mothers' increases GPAs by 0.16 standard deviations (see column 3). Finally, there is no evidence of intergenerational effects on early-grade school attendance. Confidence intervals in the event study plot mostly include zero, except for a few pre-treatment coefficients that apparently drive the rejection of the null of no pre-trends. There is a positive and

[38] The figure extends Figure 1 (panel B) from 2001 to 2010. There is a flattening of grant exposure in upper primary grades, beginning with the 2004 birth cohort until 2010. Therefore, indigenous birth cohorts in SG1 had between 1.03 and 1.08 expected years of primary grants. The source of the flattening is evident in panel A of Figure 1: after the 2015 school year, growth in total primary grants slowed and then stopped entirely.

significant ACR estimate for attendance in Table 5, but it is very small relative to the 93% attendance rates among pre-treatment cohorts of indigenous adults.

Endogenous selection into childbearing could affect the characteristics of the children who appear in the sample. If indigenous grants affect the fertility of indigenous women and/or the co-residency of children, then estimates are potentially biased. We do not find evidence of an effect of grant exposure on the total number of live births or the number of children under 22 who are co-resident with women in the FG1 sample (see Appendix Table A10). This coincides with earlier evidence (see Table 4) that exposure to grants does not affect women's early fertility.

B. Causal Channels of Intergenerational Effects

To explore the causal channels of intergenerational test score effects, we report additional regressions for five school-related variables in SG1 (see Table 5).[39] Mothers' exposure to indigenous grants reduces the probability of attending municipal and, to a lesser extent, private subsidized schools, but increases the probability of attending a private tuition school (see 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 higher tuition than publicly-funded private schools, enroll students with higher incomes, and exercise discretion in admissions that usually includes parent interviews and entrance exams, and often includes the verification of marriage and baptismal certificates (McEwan, Urquiola, and Vegas, 2008). Therefore, private enrollment almost certainly changed the school peer groups of indigenous children. We measure a child's peer attributes as the proportion of children with mothers who are indigenous or had 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

[39] In these regressions (and those in Appendix Table A11), we include dummy variables for child gender, birth year, and age. We report corresponding event study plots in panel A of Appendix Figure A14. There is evidence pre-treatment trends for several variables. However, these tend to reinforce our main conclusions. For example, there is a downward pre-treatment trend for private tuition schools that implies even larger effects on the probability of enrolling.

percentage points, also relative to a pre-treatment indigenous mean of 27% (see columns 8 and 9).

We further explore causal channels of intergenerational effects in SG2, the sample of children under 22 who are co-resident with mothers in our main sample, FG1.[40] Indigenous mothers' grant exposure did not affect the probability that children have a co-resident father (see Table 6). This is consistent with evidence that grants affected the probability of having a co-resident partner for men but not women (see Appendix Table A9). Grant exposure did not affect the migration of mothers (and co-resident children) from birth communes, or childhood illness or accidents.

In contrast, mothers' exposure increased household income per capita by 18% per year of expected grants (column 2), which is larger than the point estimate for women's labor earnings, and consistent with the higher schooling of co-resident fathers. There is a negative effect of 8% per year of expected grants on household transfer income per capita (column 3), likely driven by means-tested government transfers. Finally, there is clear evidence that 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 6). There were no effects on receipt of other education grants (column 7). Recall that eligibility to apply for indigenous grants—and the allocation of grants among applicants—depends on a proxy means test (see section 2).

In summary, indigenous grants improved mothers' schooling, labor supply, and labor earnings, and caused indigenous women to find more-educated partners. Net household income per capita rose despite reductions in transfer income. Higher household income relaxed constraints on schooling choices for children of mothers exposed to indigenous grants. As a consequence, children were exposed to peers with more-educated parents. 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 schools.

[40] Appendix Table A11 uses the SG2 sample to corroborate earlier results on municipal and private enrollments. Additional results shows that children are less likely to pay any tuition or to receive free school meals (which are offered in publicly-funded schools). Panel B of Appendix Figure A14 reports the event study plots for all variables. They are generally noisier and less conclusive, although the results for household income per-capita, indigenous grant receipt, and private tuition school enrollment all show no evidence of pre-trends, and clear evidence of ITT effects that rise (or fall) in proportion to mothers' grant exposure.

However, the evidence strongly suggests that children's gains in early-primary grades were mediated by the transformed economic circumstances of their households, and not their own exposure to indigenous grants or other means-tested 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 costs (Hendren and Sprung-Keyser, 2020). For two practical reasons, we estimate the MVPF for the combined 1999 and 2000 birth cohorts of indigenous students. First, program costs can be accurately calculated using disaggregated expenditure data since the 2009 program year (the first year of primary school eligibility for the 1999 cohort). Second, recent cohorts' ITT estimates have the greatest external validity for policy decisions.

The present value of grant-related expenditures is US$797 per *eligible* indigenous child in the 1999 and 2000 cohorts.[41] Increased schooling attainment leads to added school expenditures of US$2,452 per eligible child.[42] These are substantially offset by a fiscal externality, due to increased revenues from a 19% consumption tax.[43] 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.[44] The estimate is conservative because we do not value intergenerational benefits. The MVPF is still 16 under more conservative assumptions, including a 7% discount rate, doubled administrative overhead for grants distribution, and 25% higher costs of program-induced schooling.

[41] All values are in 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 school) and 2020 (fourth-grade 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.

[42] 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.

[43] 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 are not taxed in 2022, while incomes up to 1.8 million are taxed at 4% (https://www.sii.cl/).

[44] 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 tax consumption tax payments.

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 of Latin America's indigenous population. 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 within Chile, improved adult health, or lower teen fertility among women. We show suggestive evidence, based on a narrower range of birth cohorts, that the gains may be explained by increased learning, as proxied by Chile's national SIMCE tests.

We further show that long-run 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. Children in this sample had similar probabilities of receiving indigenous grants, regardless of their mothers' birth cohort. We also show mothers' grant exposure lowered the likelihood that children received indigenous grants themselves, 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 in Latin America. 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 the JUNAEB, the Ministry of Education, and CONADI. Second, the annual grants were large and potentially renewable until 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 economic growth and labor market performance over the 1990s and 2000s, suggesting that the welfare impact of transfers may depends on a robust job 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 of mothers and co-resident children					
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: exclude observations with missing indigenous indicator or birth commune of first-generation. FG3: excludes observations missing language and mathematics tests, indigenous indicator, or school commune. SG1: excludes observations missing language and mathematics tests, indigenous indicator, or mother's age; also excludes observations missing commune of residence or child birth year from merged administrative data; see text for details.

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	0.000	0.001	0.027	0.007
p-value: zero	0.000	0.000	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 schooling, labor earnings, and labor supply (by regions)

	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	0.000	0.000	0.075	0.003	0.032	0.163	0.211	0.040
p-value: zero	0.000	0.000	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 4: Effects on migration, health, early fertility, and partner attributes

	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	0.000	0.000
p-value: zero	0.158	0.292	0.695	0.012	0.003	0.000	0.000
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 5: Intergenerational effects of mothers' exposure on second- and fourth-graders

	Language test	Math test	Year-end grade point average	Year-end attendance	Municipal school	Private subsidized school	Private tuition school	% indigenous mothers in grade	% mothers with any tertiary in 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	0.000	0.000	0.000	0.001	0.000	0.317	0.000	0.000	0.000
p-value: zero	0.000	0.000	0.000	0.002	0.000	0.002	0.000	0.000	0.000
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 mathematics test was not applied.

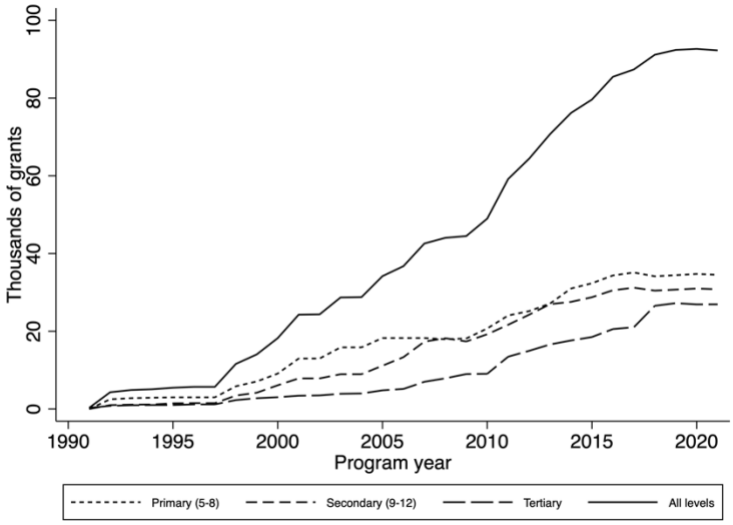
Table 6: Intergenerational effects of mothers' exposure on children under 22

	Children ages 0 to 21					Children ages 6 to 21	
	Co-resident father	HH income per capita (%)	HH transfer income per capita (%)	Migrated from mother's birth commune	Recent illness or accident	Indigenous grant	Other education grant(s)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Intent-to-treat							
Indigenous mother, born 1975-1981	0.003	0.019	0.006	0.007	0.008	-0.007	0.008*
	(0.009)	(0.026)	(0.021)	(0.008)	(0.005)	(0.006)	(0.003)
Indigenous mother, born 1982-1991	0.001	0.116**	-0.051*	0.012	0.013**	-0.034**	0.005
	(0.008)	(0.027)	(0.026)	(0.012)	(0.005)	(0.006)	(0.004)
p-value: equal	0.878	0.003	0.071	0.660	0.348	0.000	0.710
p-value: zero	0.957	0.002	0.179	0.565	0.021	0.000	0.078
Panel B: Average causal response							
Expected grant years	-0.003	0.180**	-0.075*	0.011	0.015	-0.062**	0.007
	(0.012)	(0.037)	(0.035)	(0.019)	(0.008)	(0.010)	(0.006)
Means: adults born 1965-1974							
$\bar{Y}i$	0.684	177.413	10.254	0.378	0.117	0.130	0.060
$\bar{Y}i - \bar{Y}ni$	0.002	-79.278	2.682	-0.060	-0.004	0.125	-0.020
$(\bar{Y}i - \bar{Y}ni) / \bar{Y}ni$	0.003	-0.309	0.354	-0.138	-0.035	24.181	-0.254
N (panels A and B)	396,512	396,509	396,348	396,512	392,236	263,738	263,738

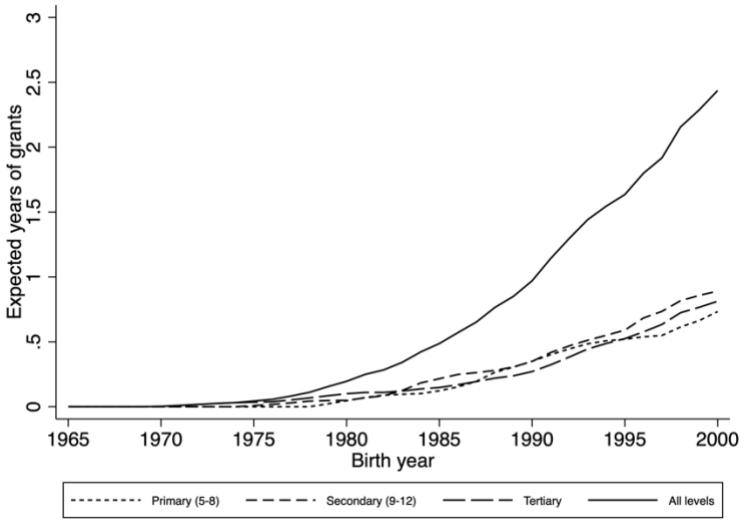
Notes: Standard errors are adjusted for multi-way clustering by commune of mother's birth, and commune of mother's residence. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous mothers in the specified birth years. Panel B: The coefficient estimate is $\hat{\tau}$. Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 2 and 3: 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

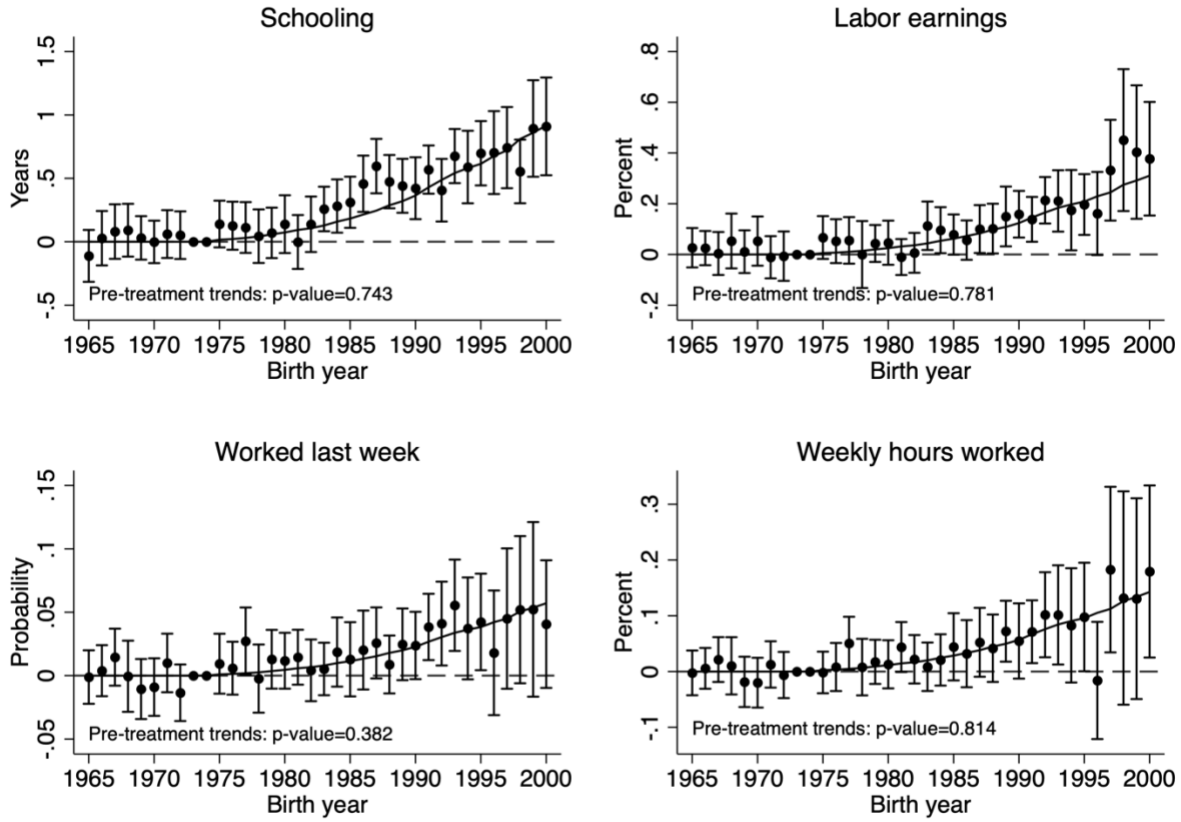


Panel B: Expected years of indigenous grants, by birth year



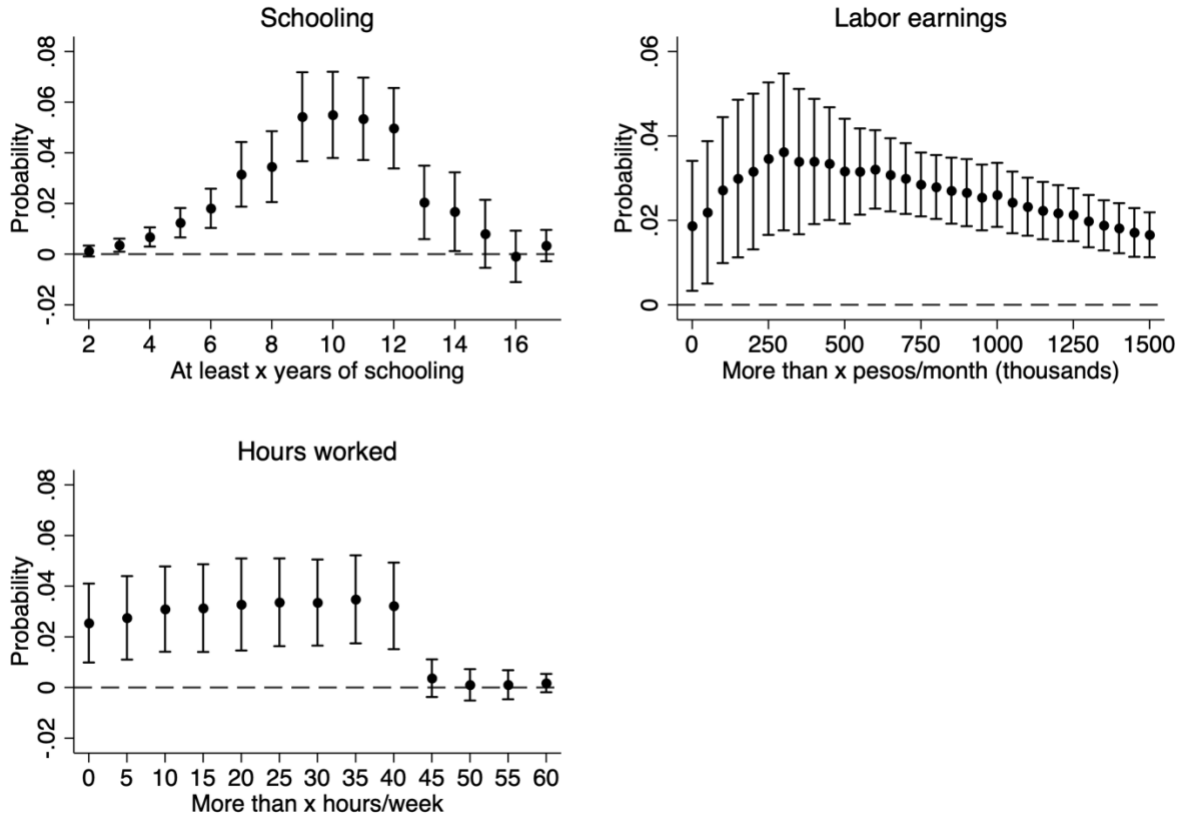
Notes: Panel A: 1991 to 2005 data from decrees of the Ministry of Education; 2006 to 2021 data from annual reports of JUNAEB to DIPRES, a supervisory unit of the Ministry of Finance. Panel B: The sum of grade-specific probabilities of receiving a grant for each birth cohort (see Appendix Table A2).

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



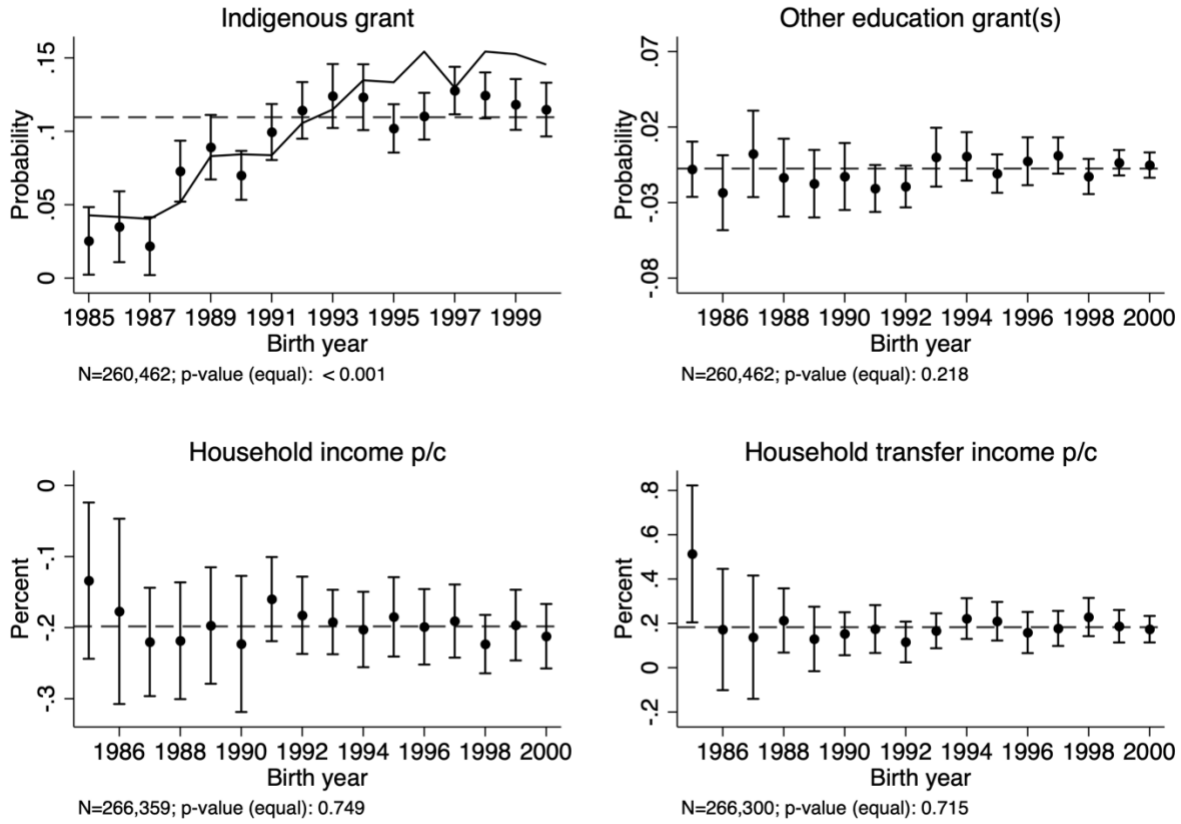
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Table 2. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$. Left-hand panels: The circles are $\hat{\gamma}_e$ 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 the corresponding $\hat{\tau}$ 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.

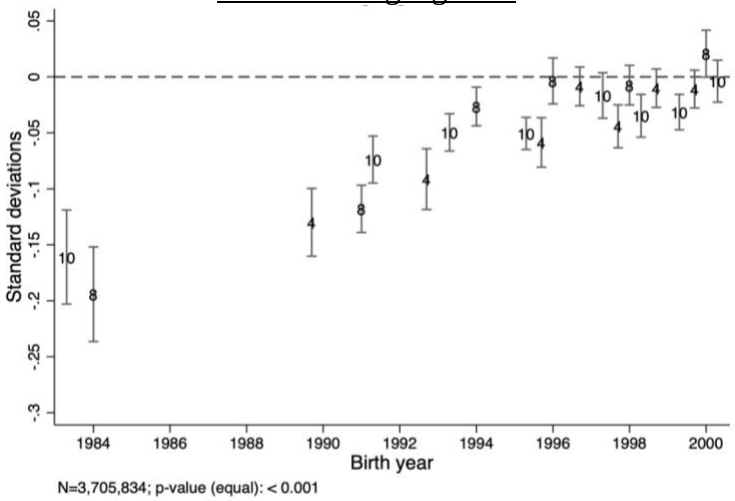
Figure 4: Post-treatment trends in household income



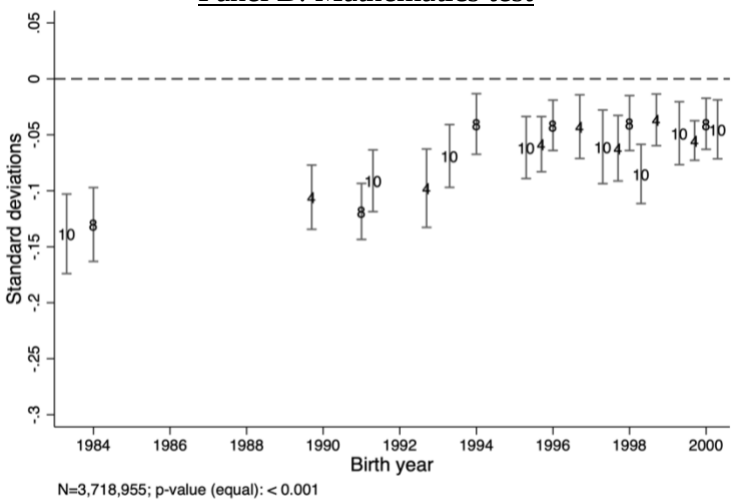
Notes: Each panel uses the FG2 sample (see Table 1). The circles are $\hat{\lambda}_e$ from equation (3), with pointwise 95% confidence intervals. Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. P-values correspond to the null hypothesis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

Figure 5: Post-treatment trends in test scores

Panel A: Language test

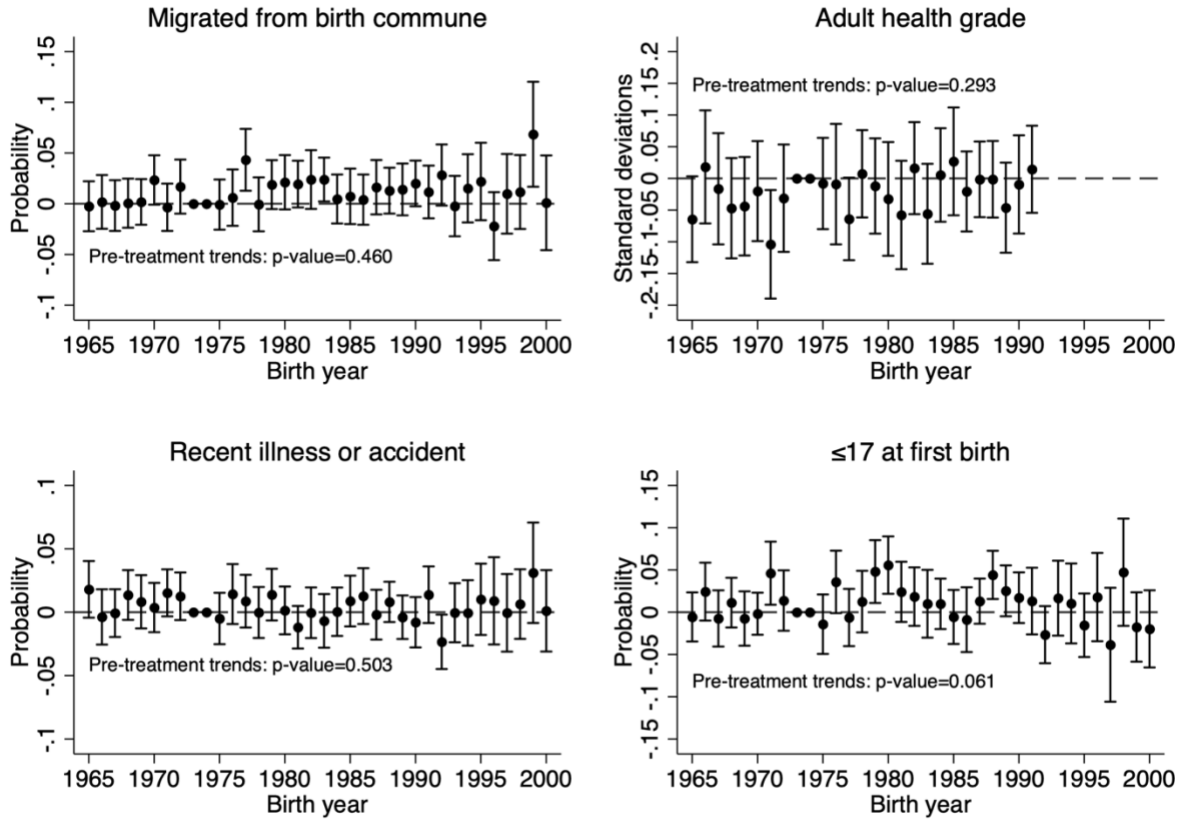


Panel B: Mathematics test



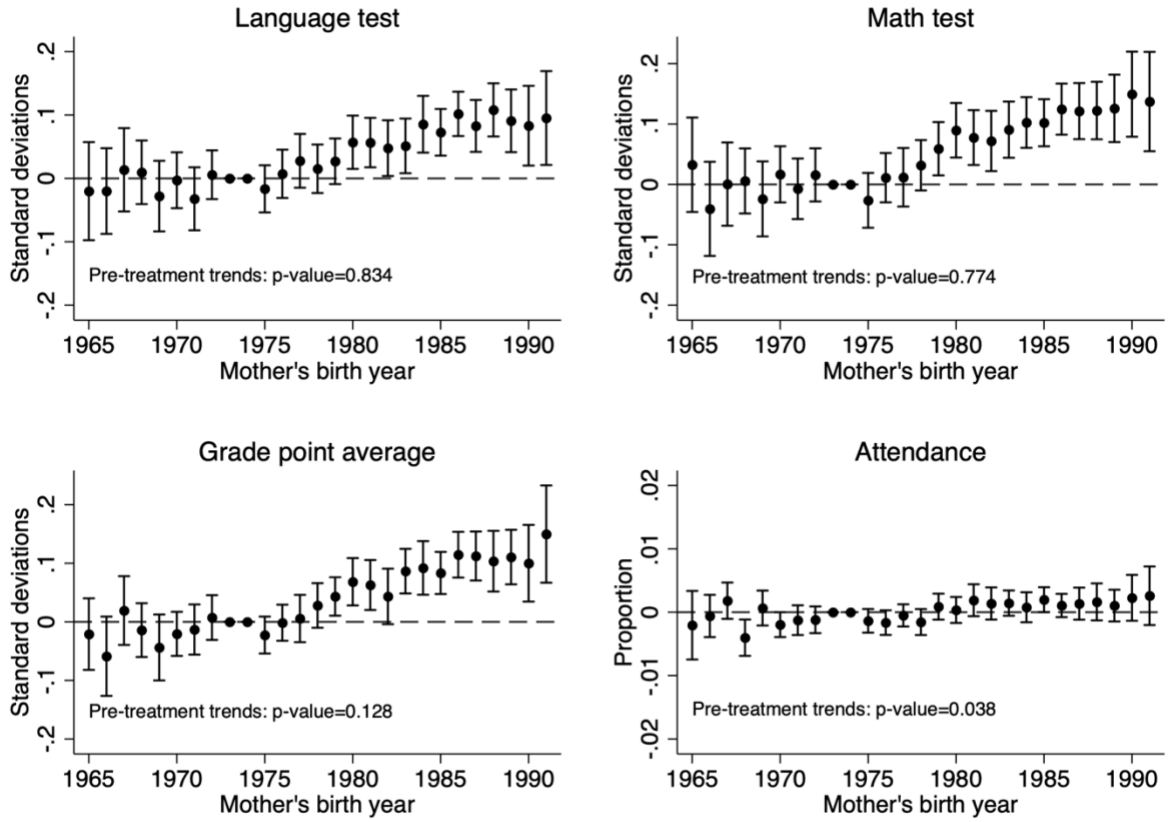
Notes: Each panel uses the FG3 sample (see Table 1). The labeled points are $\hat{\lambda}_{eh}$ for each grade from equation (4), with pointwise 95% confidence intervals. Standard errors are adjusted for clustering by schools' communes. P-values correspond to the null hypothesis that coefficients are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

Figure 6: Event study plots for migration, health, and early fertility



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 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 OLS estimates of equation (1), with pointwise 95% confidence intervals. Solid lines are $\hat{\tau} * E_{ic}$, using the corresponding $\hat{\tau}$ from Table 2.

Figure 7: Intergenerational effects of mothers' exposure on second- and fourth-graders



Notes: Standard errors are adjusted for clustering by commune of residence. Sample sizes in each panel are the same as regressions in Table 5. 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 (X), 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 survey				
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 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 grade of each birth cohort

Birth year	Grade of individuals between the ages of 10 (grade 5) and 21 (grade 16)												
	5	6	7	8	9	10	11	12	13	14	15	16	E_{ic}
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 estimated with $\frac{G_c^g}{N_c}$, as described in section 3. The numerator is the number of grants awarded to a given birth cohort in a given grade, using administrative data. The denominator is the size of the indigenous birth cohort from the 2017 census, with a mortality adjustment. See the text for additional details and methods and data.

Table A3: 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 A4: 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	239.2	229372	175.8	37049	180.5	137.5
(thousands/month)	(355.5)		(198.8)		(204.9)	(128.7)
Household transfer income p/c	6.95	229372	9.56	37049	8.83	12.11
(thousands/month)	(12.07)		(14.34)		(13.61)	(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 the mentioned 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 (see the text for details).

Table A5: Effects on schooling, labor earnings, and labor supply (without imposing assumption that $E_{ic} = 0$ between 1970 and 1974)

	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	0.000	0.000	0.017	0.005
p-value: zero	0.000	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	0.000	0.000	0.001	0.008	0.020	0.204	0.045	0.052
p-value: zero	0.000	0.000	0.004	0.000	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	0.000	0.001	0.037	0.017
p-value: zero	0.000	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	0.000	0.002	0.056	0.021
p-value: zero	0.000	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 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	0.000	0.000
p-value: zero	0.831	0.254	0.318	0.196	0.000	0.000
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	0.000	0.000
p-value: zero	0.072	0.771	0.378	0.032	0.000	0.000
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 A10: 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

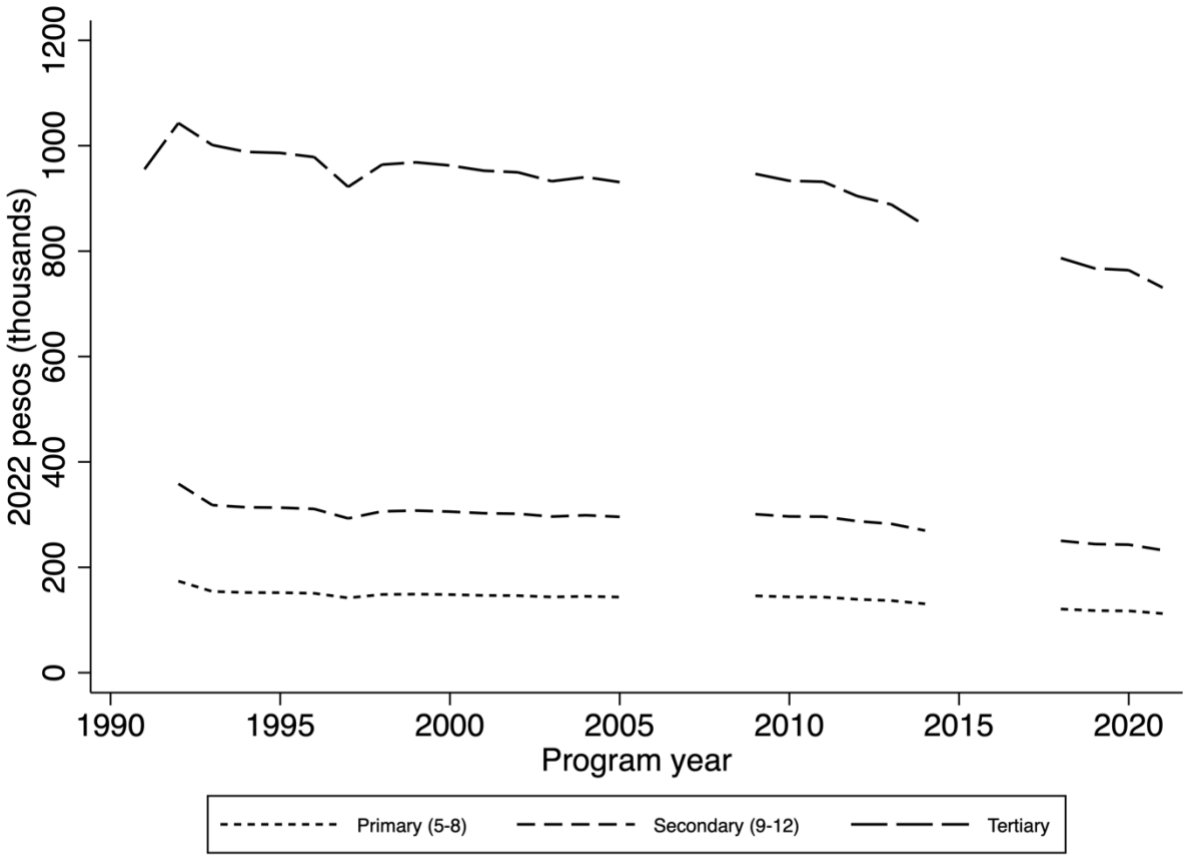
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: The coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise 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.

Appendix Table A11: 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	-0.024*	0.016	0.009*	0.023**	-0.019	-0.010
1975-1981	(0.011)	(0.010)	(0.004)	(0.008)	(0.010)	(0.009)
Indigenous mother, born	-0.044**	0.018	0.026**	0.039**	-0.064**	-0.019
1982-1991	(0.012)	(0.012)	(0.006)	(0.009)	(0.011)	(0.010)
p-value: equal	0.078	0.901	0.000	0.061	0.000	0.502
p-value: zero	0.002	0.225	0.000	0.000	0.000	0.141
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)	178,018	178,018	178,018	161,889	180,519	179,572

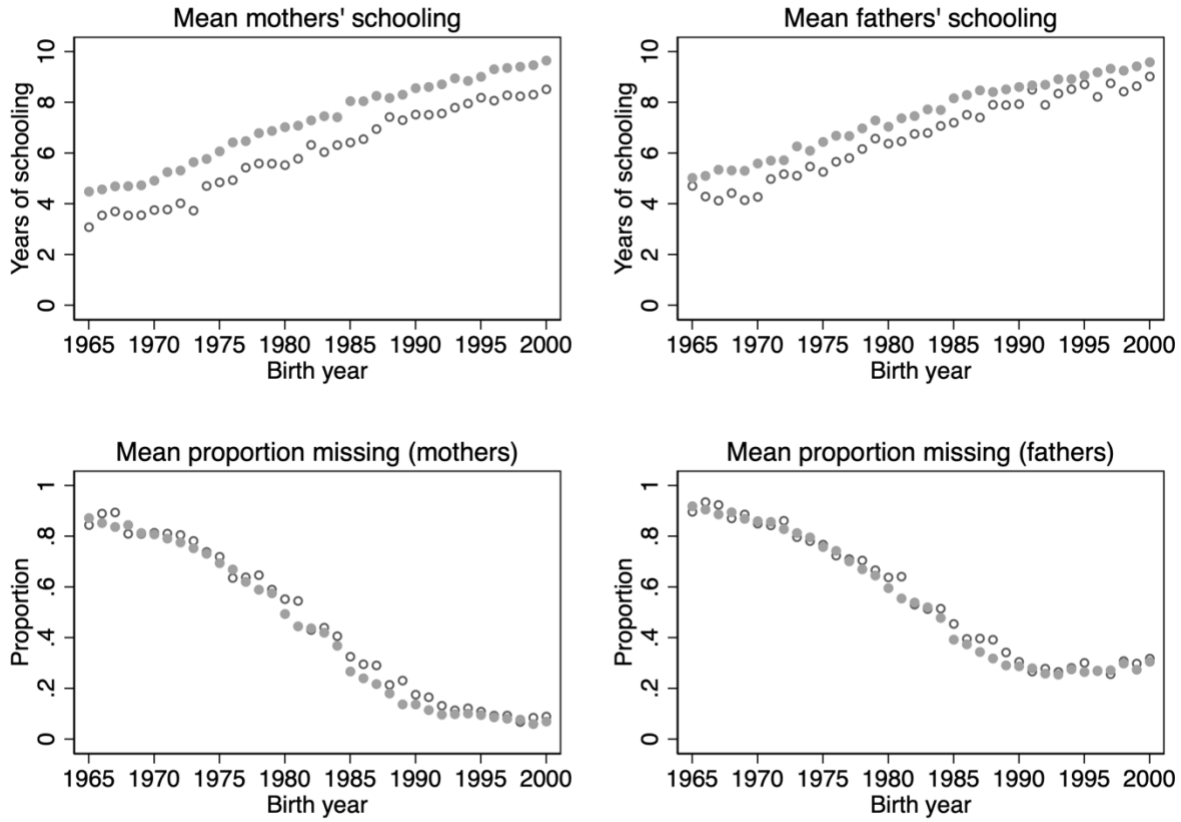
Notes: Standard errors are adjusted for multi-way clustering by commune of mother's birth, and commune of mother's residence. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous mothers in the specified birth years. Panel 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



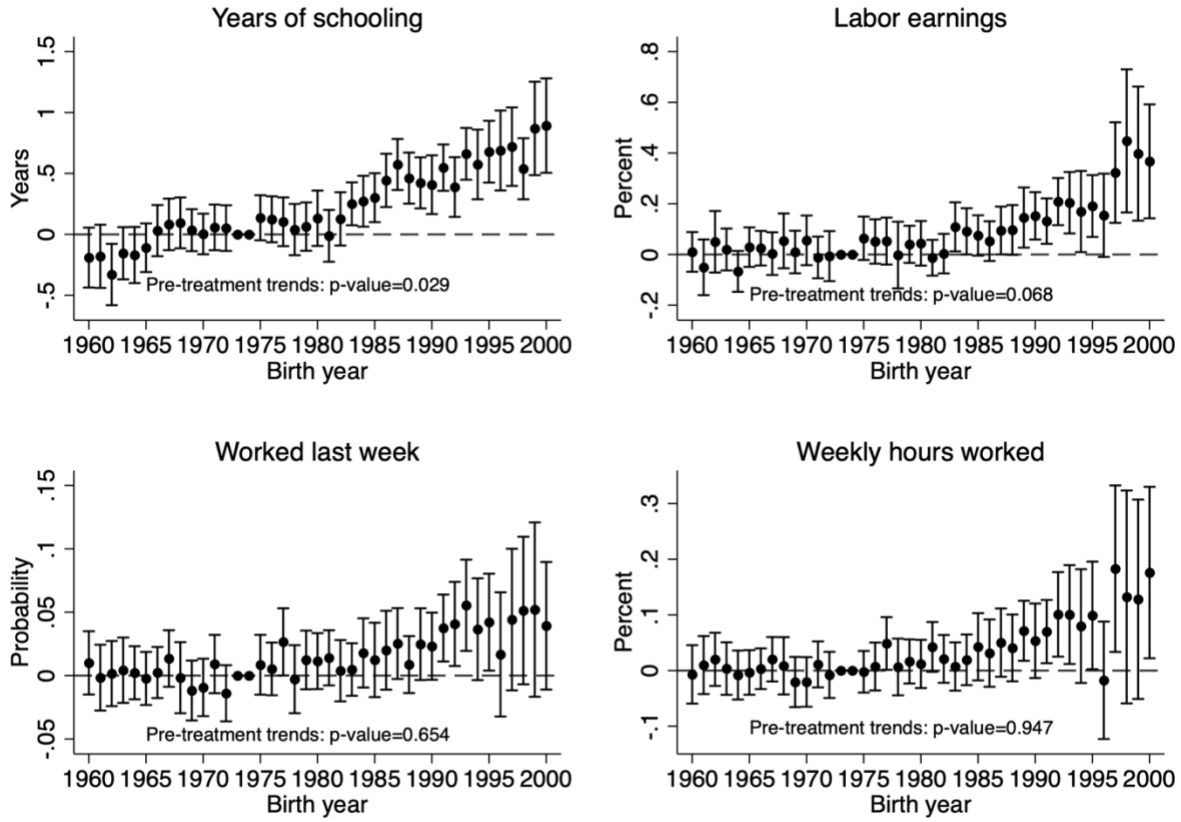
Notes: Each line plots the annual value (in 2022 pesos) of primary, secondary, or tertiary indigenous grants. Missing segments indicate years with missing data in sources mentioned in the notes to Figure 1.

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



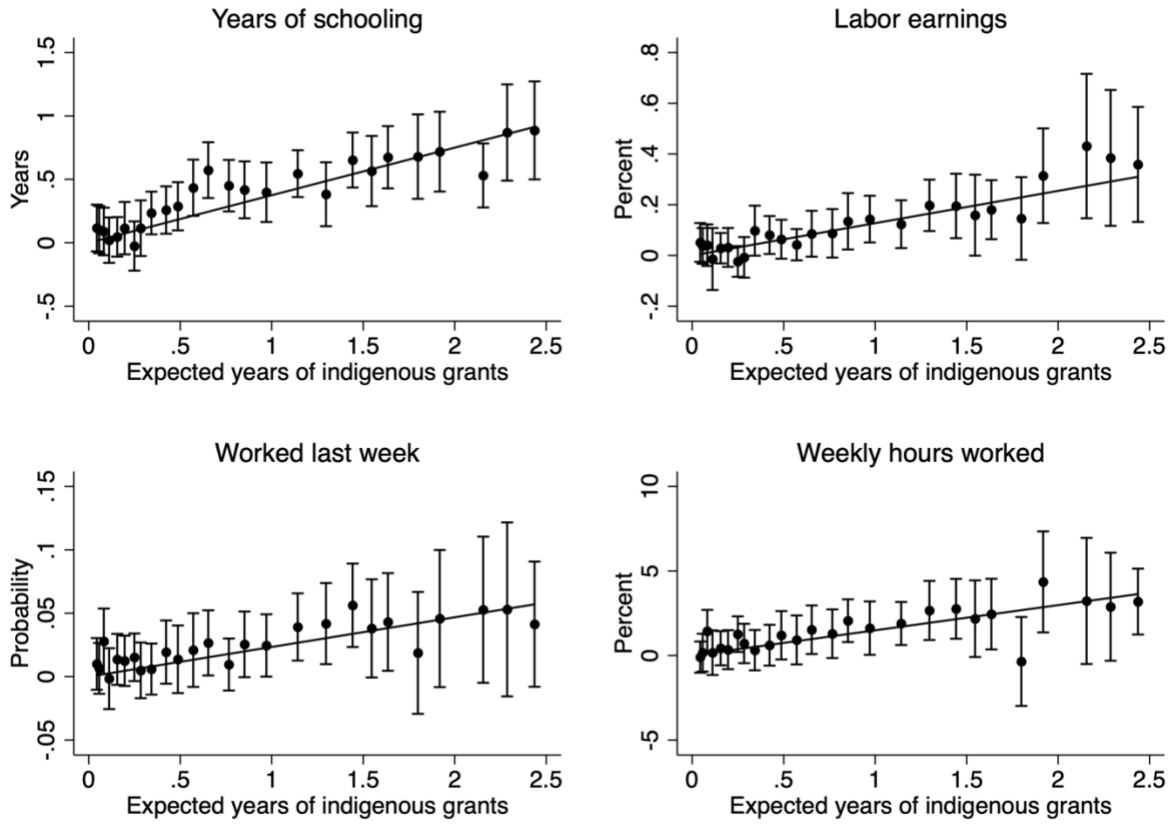
Notes: We use the 10% IPUMs sample of the 2002 census to construct parental schooling variables within combinations of birth year, birth commune, and indigenous status. The variables in the four panels correspond to the mean years of mothers' schooling, the mean years of fathers' schooling, the proportion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

Figure A3: Event study plots for schooling, labor earnings, and labor supply (1960 to 2000)



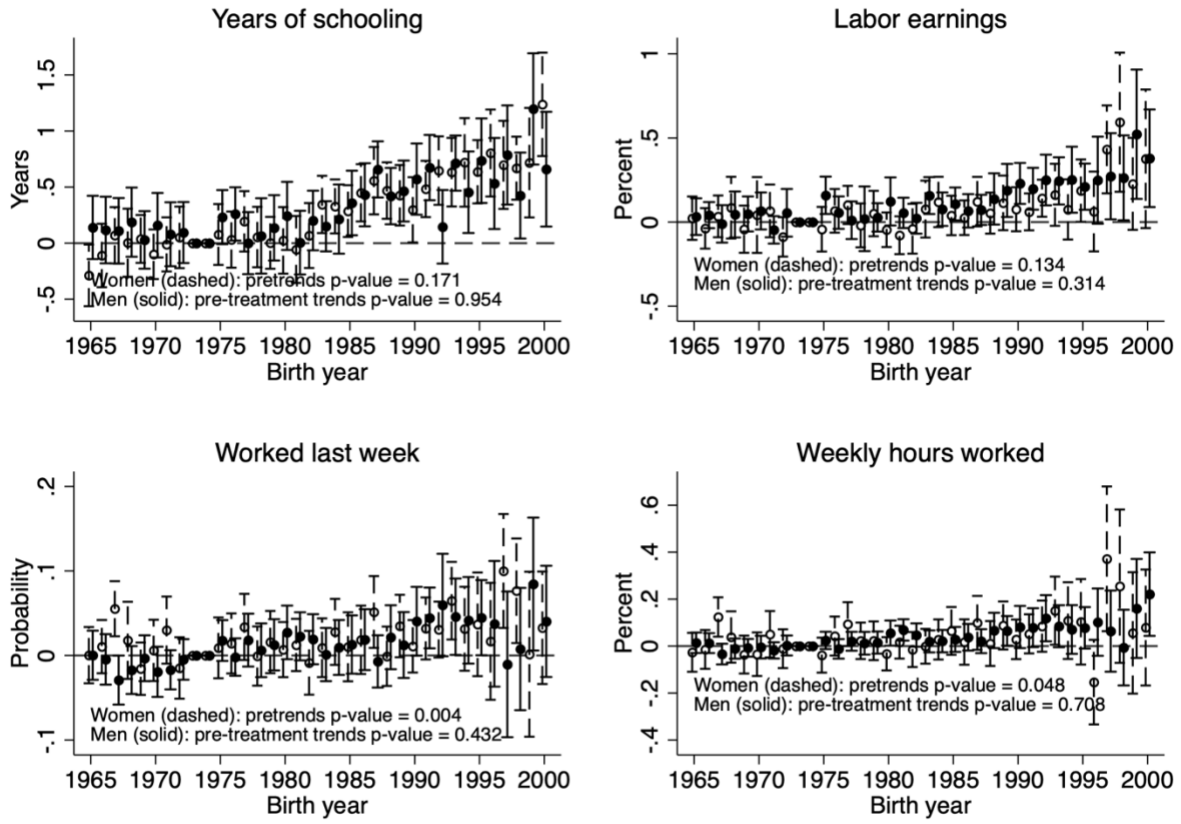
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Table 2. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$. Left-hand panels: The circles are $\hat{\gamma}_e$ 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.

Figure A4: 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), but imposing the assumption that $\gamma_e = 0 \; \forall \; e$.

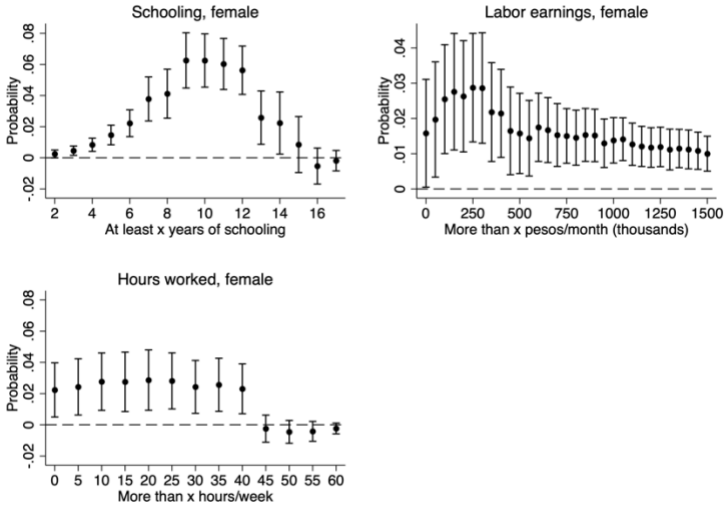
Figure A5: Event study plots for schooling, labor earnings, and labor supply
(female and male samples)



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 standard errors obtained with the delta method.

Figure A6: ITT estimates across the distributions of schooling and labor earnings (by gender)

Panel A: Female sample

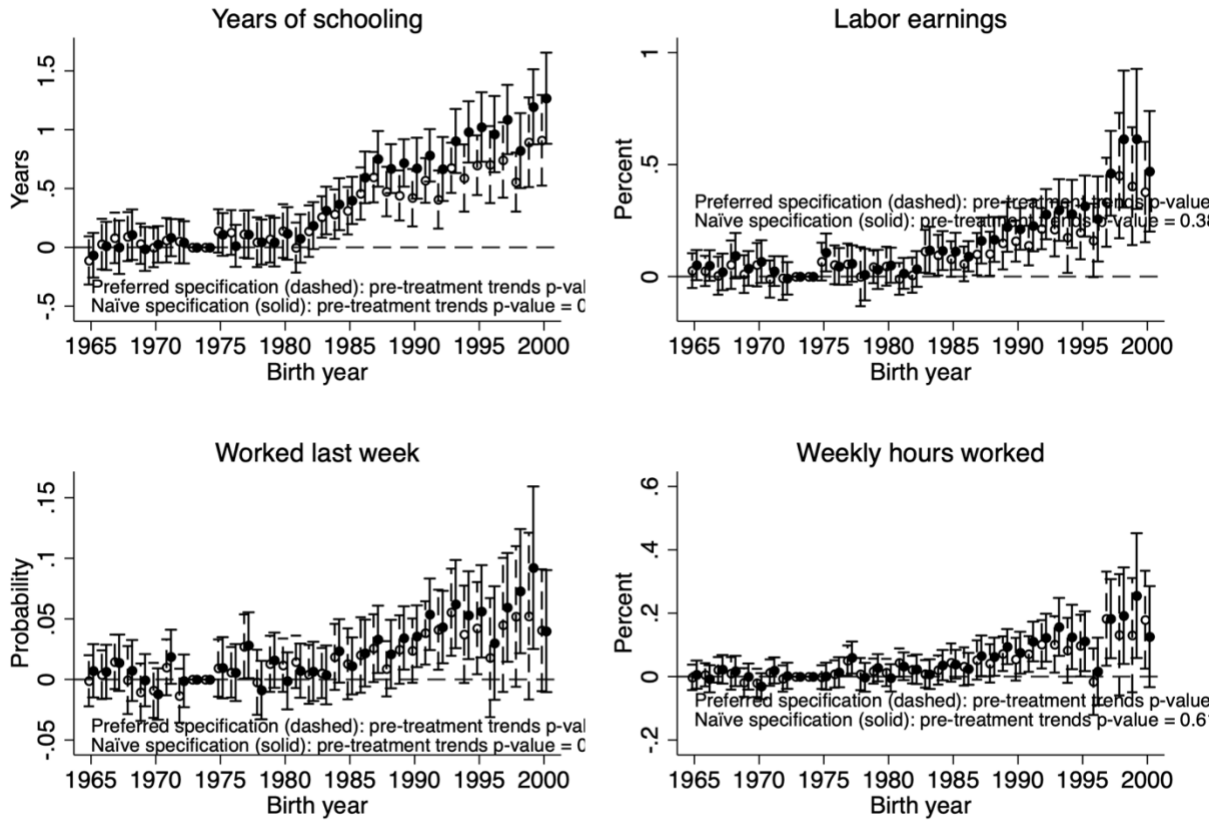


Panel B: Male sample



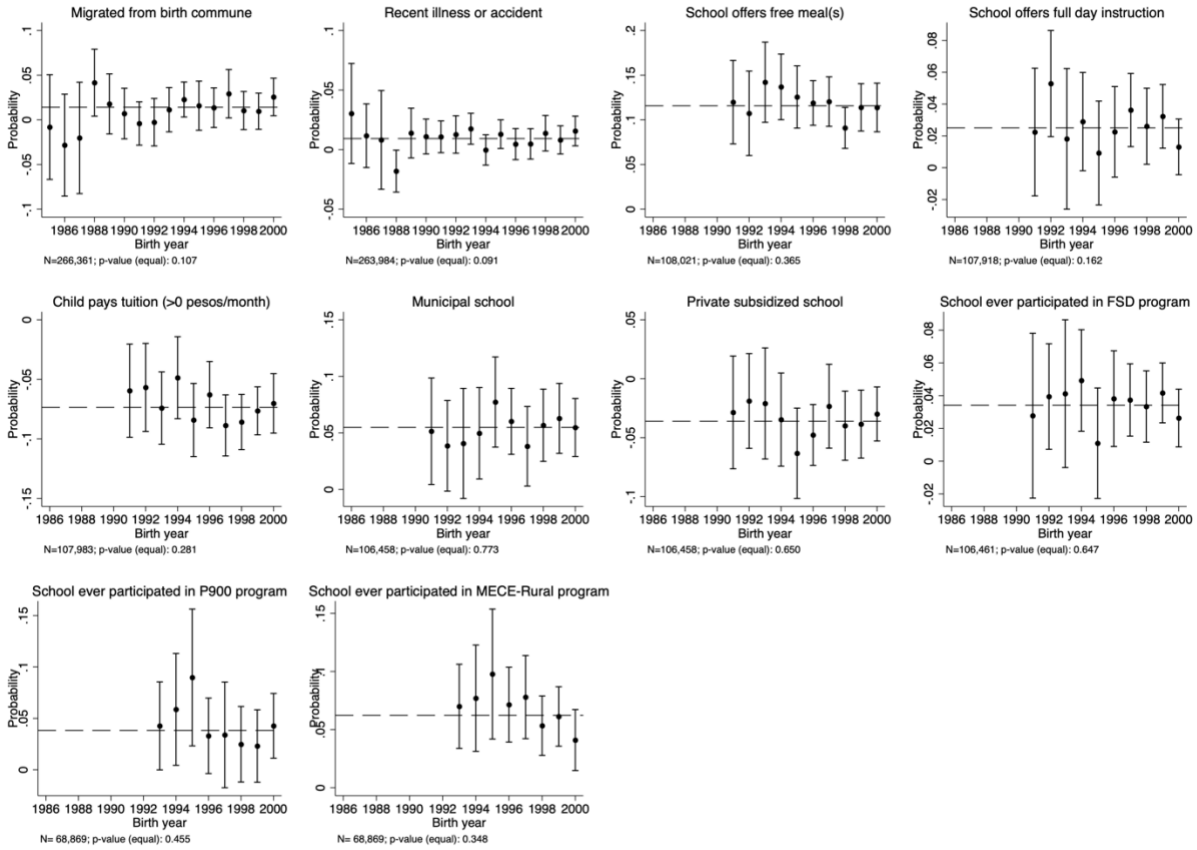
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Appendix Table A5. Each point and confidence interval corresponds to a $\hat{\tau}$ from equation (2). Top-left panels: Each 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 A7: Event study plots for schooling, labor earnings, and labor supply
(preferred and naïve specifications)



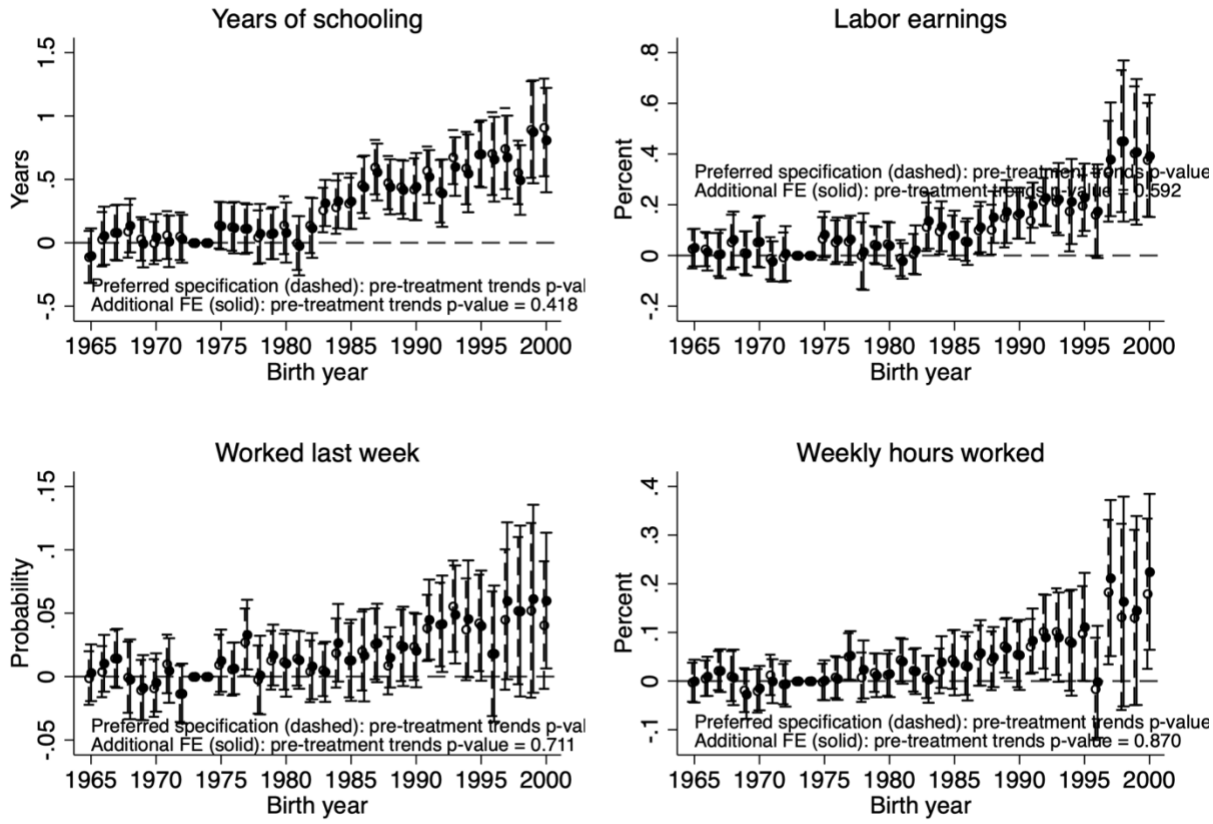
Notes: The preferred specification is the same as equation (1), while the naïve specification is $Y_{icmt} = I_{icmt}[\alpha + \sum_{e=1965}^{1972} \gamma_e 1\{c = e\} + \sum_{e=1975}^{2000} \lambda_e 1\{c = e\}] + \delta_c + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt}$. Standard errors are adjusted 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.

Figure A8: Post-treatment trends in childhood inputs of indigenous children and adolescents



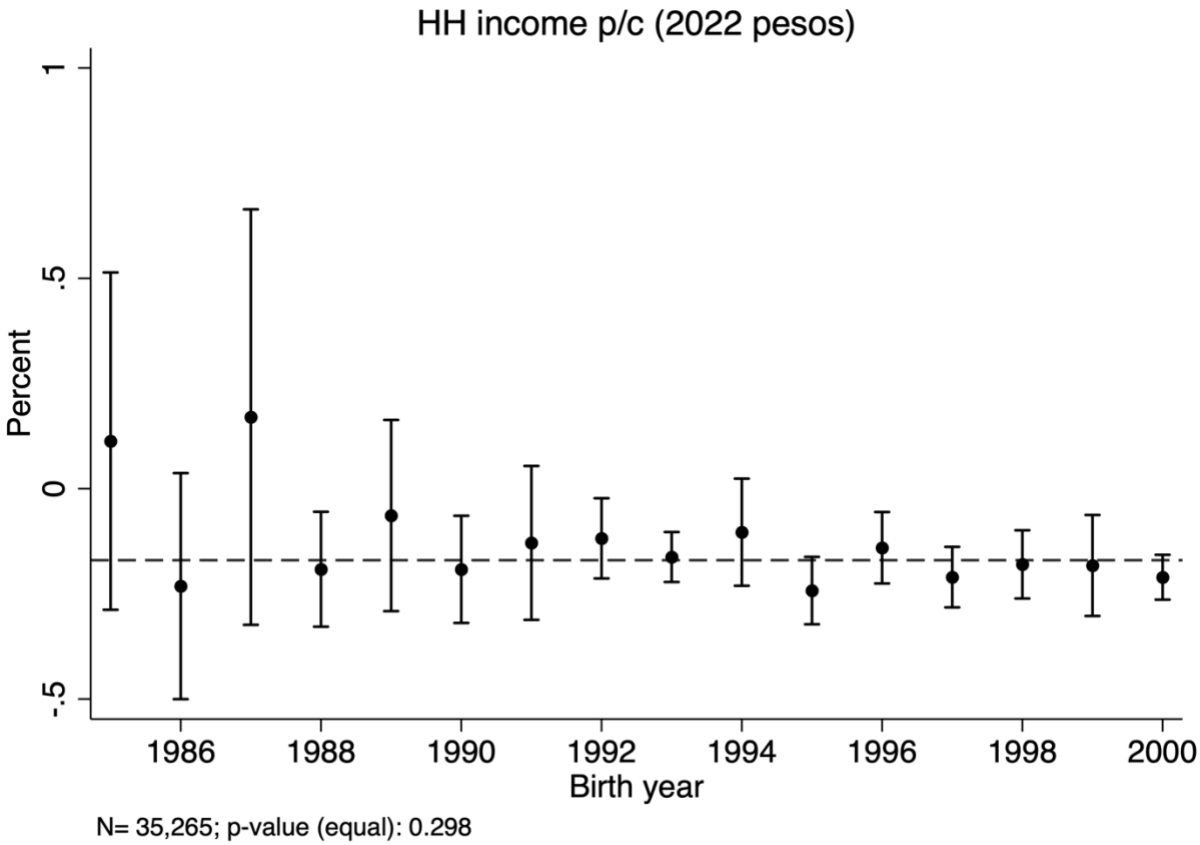
Notes: Each panel uses the FG2 sample (see Table 1). The circles are $\hat{\lambda}_e$ from equation (3), with pointwise 95% confidence intervals. Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. P-values correspond to the null hypothesis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

Figure A9: Event study plots for schooling, labor earnings, and labor supply
(with additional fixed effects)



Notes: The preferred specification is the same as equation (1), while 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. 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.

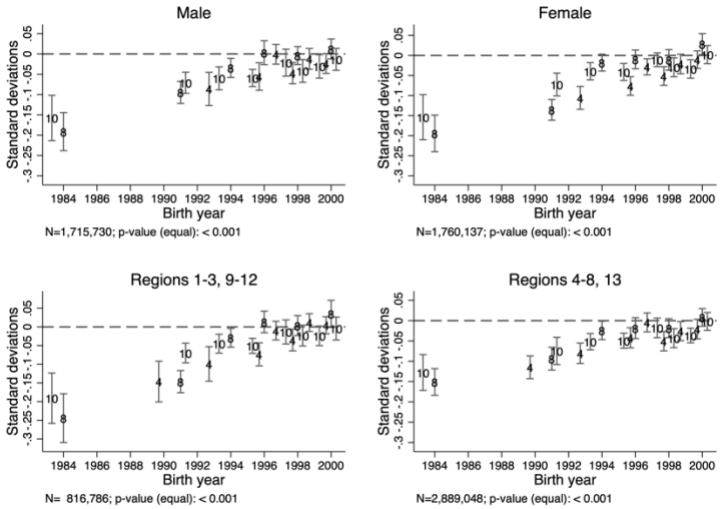
Figure A10: Post-treatment trends in household income of indigenous grant recipients, relative to indigenous non-recipients



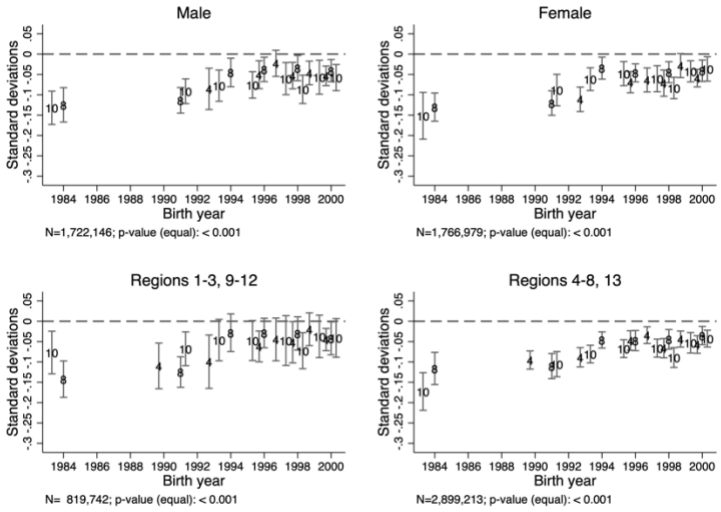
Notes: Each panel uses the FG2 sample (see Table 1), limited to indigenous individuals between 6 and 21. The circles are $\hat{\lambda}_e$ from a modified equation (3), with pointwise 95% confidence intervals, in which I_{icmt} is replaced with a dummy variable indicating that individuals currently receive 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 A11: Post-treatment trends in test scores (female, male, and regional samples)

Panel A: Language test

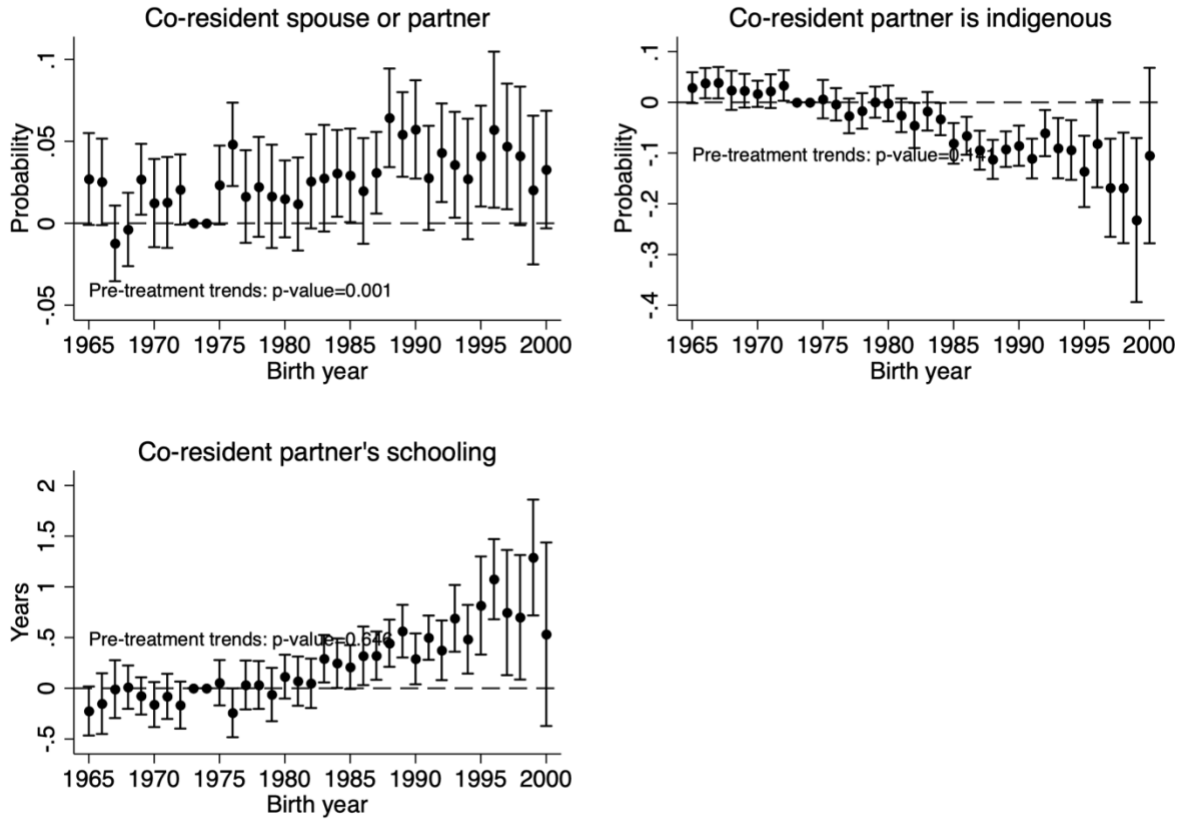


Panel B: Mathematics test



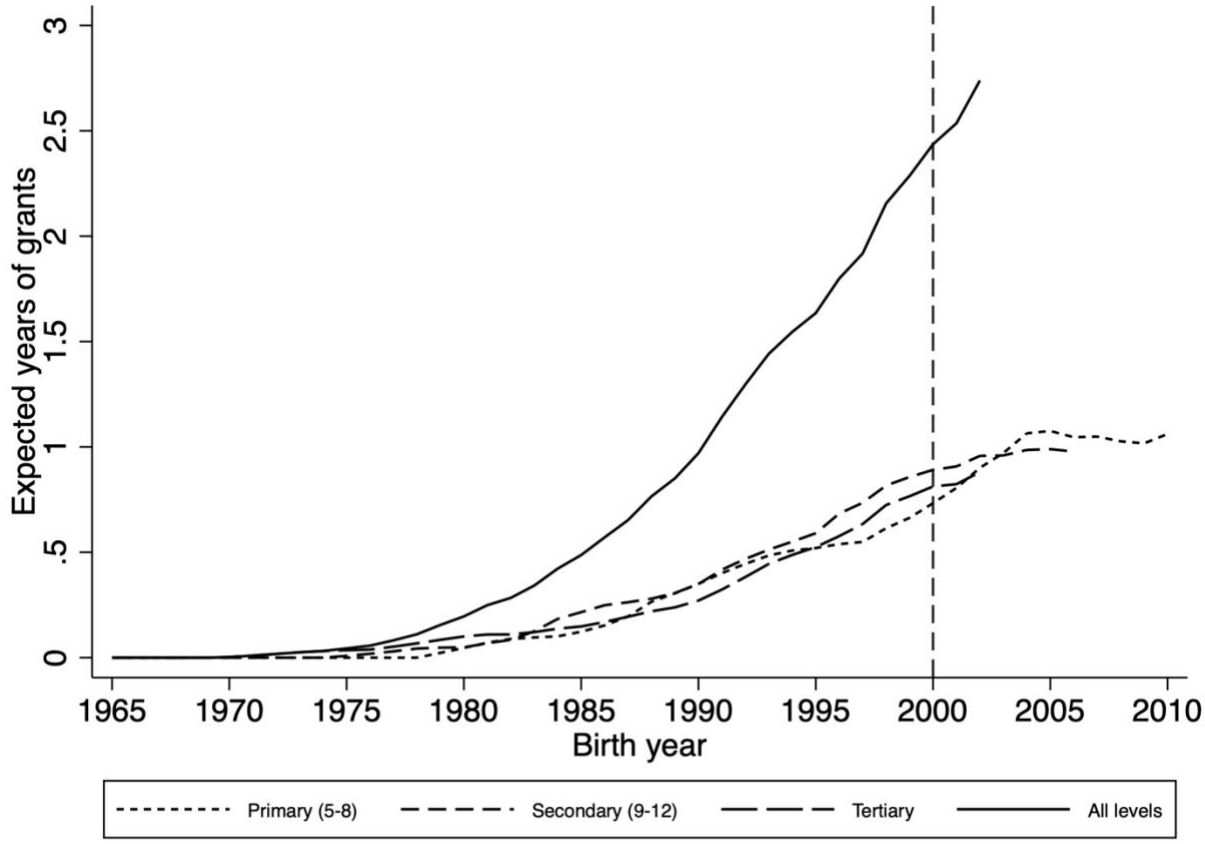
Notes: Each panel uses the FG3 sample (see Table 1), within four subsamples. The labeled points are $\hat{\lambda}_{eh}$ for each grade from equation (4), with pointwise 95% confidence intervals. Standard errors are adjusted for clustering by schools' communes. P-values correspond to the null hypothesis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

Figure A12: Event study plots for co-resident partner attributes



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 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 OLS estimates of equation (1), with pointwise 95% confidence intervals. Solid lines are $\hat{\tau} * E_{ic}$, using the corresponding $\hat{\tau}$ from Table 2.

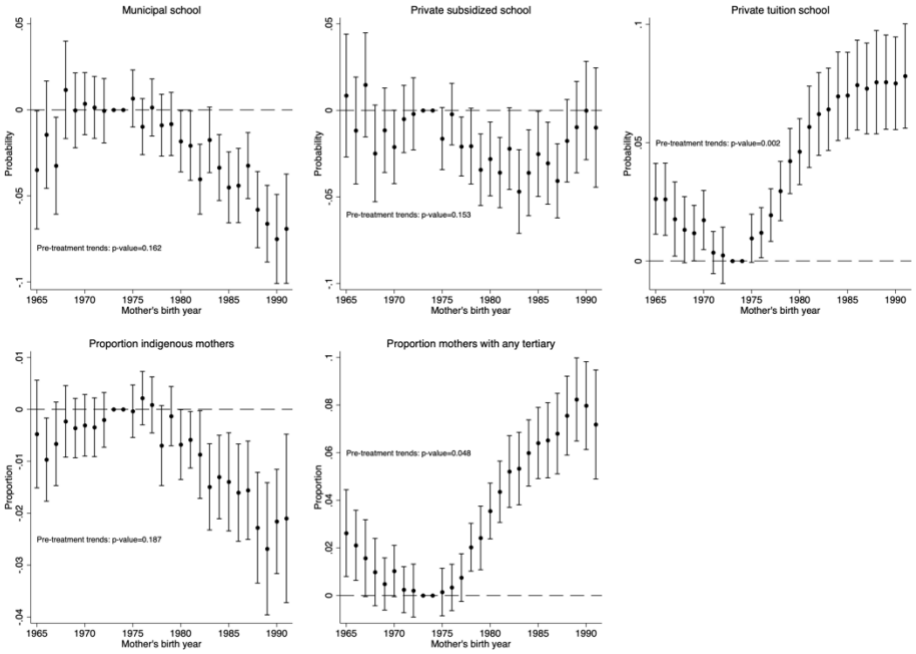
Figure A13: Expected years of indigenous grants, by birth year (1965 to 2010)



Notes: See notes to Figure 1. This estimates the expected years of grants, when feasible, using administrative data until 2023.

Figure A14: Event study plots for dependent variables in Table 5, Table 6, and Appendix Table 11

Panel A: Event study plots for SG1 sample (Table 5)



Panel B: Event study plots for SG2 sample (Table 6 and Appendix Table 11)

