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OF CONDITIONAL CASH TRANSFERS:
EVIDENCE FROM CHILE'S INDIGENOUS GRANTS

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The Long-Run and Intergenerational Effects of Conditional Cash Transfers: Evidence from Chile's Indigenous Grants

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ABSTRACT

Since 1991, Chile has provided renewable cash transfers to indigenous children enrolled in school. We estimate intent-to-treat effects of expected grant exposure on the recipient generation and their children, exploiting cohort variation in exposure and comparisons to never-treated adults. Fixed effects absorb attributes shared by those born in the same year and community. Cohorts with the greatest exposure had 20 percent higher earnings, and higher labor supply, schooling attainment, and learning per year of school. Maternal exposure improved children's test scores and grades, and reduced household receipt of indigenous grants and means-tested transfers.

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For three decades, conditional cash transfers (CCTs) have been promoted as tools to reduce poverty and inequality, via means-tested transfers conditioned on children’s human capital investments (Fiszbein and Schady, 2009; Araujo et al., 2024; Banerjee et al., 2024). Latin America alone has implemented more than 30 education-focused CCTs, motivated in part by robust experimental evidence that programs increase school attainment (García and Saavedra, 2022).¹ Yet, their long-run welfare effects on treated children—and on the next generation—remain unclear. First, gains in schooling have not consistently translated into improved skills (Levy and Schady, 2013; García and Saavedra, 2022). Second, existing evidence on adult labor supply and earnings is mixed, and in many settings treated individuals have not entered labor markets (Molina Millán et al., 2019; Araujo et al., 2024).² Third, there is a dearth of evidence on intergenerational human capital spillovers, except for a smaller-scale Ghanaian experiment on secondary school scholarships (Dupas et al., 2024).

We study the long-run and intergenerational effects of a large-scale, government-run CCT for indigenous Chileans. Ten percent of Chile’s population has indigenous ancestry, predominantly Mapuche (Appendix Table A1). The *Beca Indígena* program provides grants to indigenous students enrolled in upper-primary (grades 5–8), secondary, and tertiary education. Eligibility requires certified indigenous ancestry, a proxy-means test, minimum prior-year grades, and an application. A government agency prioritizes applicants with higher grade point averages and lower incomes (JUNAEB, 2024). Once awarded, grants are renewable within education levels conditional on meeting eligibility criteria. The program expanded from 300 grants in 1991 to over 92,000 in 2021.

Our research design exploits differential grant exposure across indigenous status and birth cohorts. Non-indigenous children were never treated. Indigenous birth cohorts born before 1975 had no exposure, while cohorts born from 1975 onward were increasingly exposed as the program expanded. We estimate intent-to-treat (ITT) effects using event-study regressions for adults born between 1965 and 2000. Specifications include fixed effects for all pairs of 36

¹ On schooling attainment, see Araujo and Macours (2021); Attanasio et al. (2021); Baird, McIntosh, and Özler (2019); Barham, Macours, and Maluccio (2024); Barrera-Osorio, de Barros, and Filmer (2023); Cahyadi et al. (2020); Duflo, Dupas, and Kremer (2025); Parker and Vogl (2023); and Barrera-Osorio, Linden, and Saavedra (2019).

² Studies in Mexico and Nicaragua find mixed results on labor earnings that depend on gender (Araujo and Macours, 2021; Parker and Vogl, 2023; Barham et al., 2024). Experiments in Ghana, Honduras, and Indonesia find minimal effects on incomes (Cahyadi et al., 2020; Duflo et al., 2024; Molina Millán et al., 2020).

cohorts and 318 birth communes—Chile’s smallest territorial unit—which absorb time- and place-varying confounders such as local economic conditions, school and health-center quality, and place-based policies targeted to rural or low-income communities. We also include indigenous-status-by-birth-commune fixed effects. Identification comes from within-commune differences between indigenous and non-indigenous cohorts with differential exposure to the program.

ITT estimates indicate that the most-exposed indigenous cohorts (1992–2000) completed 0.57 additional years of schooling, worked 7.8 percent more hours, and earned 20 percent more. Scaling by cohort-level exposure, the average causal response (ACR) to one additional year of expected grant exposure is 0.31 years of schooling, a 4.1 percent increase in hours worked, and a 10 percent increase in earnings. Earnings estimates include non-workers with zero earnings, and therefore reflect increased labor supply, the returns to schooling attainment, and other causal channels discussed below. Using the ITT earnings estimates, we calculate a marginal value of public funds (MVPF) for the 1999–2000 cohorts (Hendren and Sprung-Keyser, 2020). Each dollar of net government expenditure generates approximately \$29 in after-tax lifetime earnings.

The research design requires parallel trends between indigenous and non-indigenous outcomes *within birth communes*, conditional on controls such as parental schooling. We provide three pieces of evidence that support this requirement. First, pre-treatment event-study coefficients are small and statistically indistinguishable from zero. Second, we find no differential post-treatment trends in childhood covariates plausibly correlated with adult outcomes, including household income, other grant exposure, private-school enrollment, and major contemporaneous education reforms (Chay, McEwan, and Urquiola, 2005; Dominguez and Ruffini, 2023). Third, results cannot be explained by concurrently-implemented policies or the endogenous misreporting of indigenous status.

We explore multiple causal channels for the labor market effects. We document within-commune gains in test scores and parent expectations for child education attainment, consistent with the program’s high-stakes grade incentives. We find no effects on migration, health, teenage fertility, and partner co-residence, although we find increases in ethnic intermarriage and partners’ schooling. Finally, we assess whether the substantial effects on labor supply and earnings can be realistically explained by increased human capital, using auxiliary estimates of

the returns to schooling and cognitive skills. Predicted labor market effects of grant exposure align closely with our estimated long-run impacts.

We examine intergenerational impacts of maternal grant exposure using the same research design. A one-year increase in mothers' expected grant exposure raises children's test scores and grades by 0.15–0.18 standard deviations. The effects are consistent with children's improved socioeconomic environments along multiple margins, including parental schooling, household income, private-school enrollment, and school-based peer composition. Maternal exposure *reduces* children's indigenous grant receipt and household income from means-tested transfers, consistent with improved household incomes and program targeting rules. Therefore, the intergenerational effects operate through maternal exposure rather than children's direct program participation.

The paper makes three contributions. First, we contribute to the literature on the long-run effects of CCTs (Araujo and Macours, 2021; Parker and Vogl, 2023; Barham et al., 2024; Cahyadi et al., 2020; Duflo et al., 2024; Molina Millán et al., 2020).³ Our first-generation design is related to Parker and Vogl (2023), who exploit quasi-experimental variation in CCT exposure across cohorts and municipalities in Mexico. A key difference is that we have a never-treated control group of non-indigenous Chileans. This confers three advantages. First, it allows commune-by-cohort fixed effects that absorb fine-grained geographic variation in economic conditions and local policies. Second, the design identifies impacts of lifetime exposure relative to a no-treatment counterfactual, rather than earlier-versus-later exposure among treated groups, as in some experimental settings (Araujo and Macours, 2021; Barham et al., 2024). Third, it permits ITT estimation under a standard, and not strong, parallel trends assumption within communes (Callaway, Goodman-Bacon, and Sant'anna, 2024). Our average causal response estimates require strong parallel trends, as in Parker and Vogl (2023).

Second, we provide evidence of the intergenerational effects of a large-scale, government-implemented CCT. At a smaller scale, secondary scholarships in Ghana increased the cognitive

³ The Chilean setting provides a particularly strong test of whether large-scale, government-implemented CCTs can affect schooling, skills, and labor market outcomes. The program has operated for over 30 years under stable and transparent eligibility rules. Grants are renewable from upper-primary through tertiary education and are conditioned on both enrollment and minimum grade thresholds. Moreover, indigenous students are well-informed about the program and view it favorably (Webb, 2015, 2022). There is no comparable evidence in Chile, although several papers identify the effects of means-tested loans on tertiary attainment and earnings of marginally-eligible students (Solis, 2017; Bucarey et al., 2020; Aguirre, 2021).

skills of children born to treated mothers (Dupas et al. 2024). Other government-implemented education policies in developing countries have affected second-generation outcomes, including school construction in Indonesia (Mazumder, Rosales-Rueda, and Triyana, 2019, 2023; Akresh, Halim, and Kleemans, 2023); school construction in Chile (Lucas and McEwan, 2026); and the elimination of racial school restrictions in Zimbabwe (Agüero and Ramachandran, 2020).⁴ We also contribute to a related literature on the intergenerational persistence of social program participation. In contrast to European and U.S. evidence documenting positive intergenerational transmission of social programs (Dahl, Kostøl, and Mogstad, 2014; Dahl and Gielen, 2021; Hartley, Lamarche, and Ziliak, 2022), we find that maternal grant exposure reduces children’s participation in means-tested transfers, reflecting higher household incomes and strict eligibility thresholds.

Third, we provide causal evidence that education policy can reduce ethnic inequality, both directly and through intergenerational channels. Indigenous populations in Latin America exhibit persistent gaps in schooling and earnings (McEwan, 2004, 2008a; Ñopo, 2012; World Bank, 2015; Näsland-Hadley and Santos, 2022). While the U.S. literature documents racial inequality and the effects of schooling reforms (e.g., Card and Krueger, 1992; Cook and Evans, 2000; Aaronson and Mazumder, 2011), much less is known about CCTs or other policies specifically designed to reduce ethnic gaps in Latin America. In Chile, we show that the *Beca Indígena* program substantially narrowed first-generation gaps in schooling, test scores, and earnings, and reduced second-generation gaps in test scores and grades. These findings suggest that indigenous targeting may warrant greater consideration in CCT design (Ibarrarán et al., 2017).

I. Indigenous Grants in Chile

Since 1991, the indigenous grants program has provided cash transfers to indigenous students enrolled in primary, secondary, and tertiary education. This section describes grant amounts and distribution, and then describes program expansion across school years and birth cohorts.

A. Grant Amounts and Distribution

⁴ In Chile, Barrios-Fernández, Neilson, and Zimmerman (2024) find that test-based admission to elite university programs affected tertiary access of the second generation.

In 2021, annual grant amounts were CLP 100,550 (USD 118) for primary students; CLP 208,280 pesos (USD 245) for secondary students; and CLP 654,600 (USD 770) for tertiary students.⁵ These amounts correspond to 9, 18, and 56 percent of recipients' median annual household income per capita, respectively. The program is administered by a government agency, the *Junta Nacional de Auxilio Escolar y Becas*. JUNAEB deposits grants in mandatory accounts at the state-owned *BancoEstado*, in two installments for primary and secondary schooling and ten installments for tertiary. For primary students, funds are deposited into a parent or guardian account, while older students receive funds in their own accounts. There are no restrictions on how funds can be used.

Eligibility is determined by four criteria (JUNAEB, 2024). First, individuals must obtain certification of indigenous ancestry from the *Corporación Nacional de Desarrollo Indígena* (CONADI), based on birth certificates indicating that the individual, a parent, or a grandparent has an indigenous surname. Second, individuals must be enrolled in upper-primary (grades 5–8), secondary (grades 9–12), or tertiary education. Third, individuals must meet minimum prior-year academic requirements: a grade point average of at least 5.0 (on a seven-point scale) in primary and secondary grades, or 4.5 in tertiary.⁶ Fourth, households must fall within the bottom 60 percent of socioeconomic vulnerability, as measured by a national proxy-means test, the *Registro Social de Hogares* (RSH).

Students submit online applications before each academic year begins in early March. JUNAEB verifies eligibility using administrative data from CONADI (on indigenous status), the Ministry of Education (on enrollment and prior-year grades), and the RSH (on socioeconomic status). Among eligible applicants, students with higher prior-year GPAs and lower proxy incomes receive priority within Chile's 13 regions (JUNAEB, 2024).⁷ Grants are easily renewable within an education level if eligibility criteria are still met. However, students must reapply when transitioning to secondary or tertiary education.

⁵ Throughout the paper, we adjust to 2022 CLP using the consumer price index. Nominal grant amounts increased in many years, though inflation modestly eroded their value (Appendix Figure A1).

⁶ In primary and secondary grades, students are retained if they score below 4 in any subject, if they score below 4 in one subject and their GPA is less than 4.45, or they score below 4 in two subjects and their GPA is less than 4.95 (Díaz et al., 2021).

⁷ Priority is also given to applicants in single parent households, with child dependents, enrolled in schools with low socioeconomic status, who participate in indigenous civic groups, and reside in rural areas.

B. Program Scale-Up Across School Years and Birth Cohorts

The number of grants expanded steadily over three decades (Figure 1, panel A). The program began in 1991 with 300 tertiary grants. By 1992, it funded 2,500 primary, 1,000 secondary, and 750 tertiary grants. Grants increased at all levels thereafter, reaching more than 92,000 by 2021.⁸ Despite this growth, most indigenous students did not receive grants, even in later years.

To measure cohort-level exposure, we construct the expected years of grant receipt for individual i born in year c :

$$E_{ic} = I_{ic} \times \sum_{g=5}^{16} \frac{G_c^g}{N_c},$$

where I_{ic} equals one for indigenous individuals (and zero otherwise), N_c is the number of indigenous children born in year c who survived to primary-grade grant eligibility, and G_c^g is the number of grants awarded in grade g to indigenous children born in year c .⁹ For example, the 2000 birth cohort first became eligible in grade 5 (the 2010 school year), such that $G_{2000}^5/N_{2000} = 5,178/33,124 = 0.156$.¹⁰ E_{ic} is the cohort-specific sum of receipt probabilities across 12 eligible grades between the fifth primary grade and the fourth tertiary year.

Figure 1 (panel B) shows that expected grant years increased from 0.04 for the 1975 indigenous birth cohort to 2.4 for the 2000 cohort. Two factors drive this pattern. First, earlier cohorts were eligible for fewer than 12 grades because the program began after they had already completed some eligible grades. Second, grade-specific probabilities increased with the number of funded grants. For example, the probability of receiving a grant in eighth grade (age 13) was 0.01 for the 1979 cohort and 0.20 for the 2000 cohort. Even for later cohorts, grade-specific probabilities never exceeded 0.24 (Appendix Table A2), underscoring that grants are not universal.

The cohort-level and indigenous status variation motivates a quasi-experimental design comparing earlier and later birth cohorts (the first difference) of indigenous and non-indigenous

⁸ The number of tertiary grants includes two small programs, also administered by JUNAEB, that use similar targeting criteria to subsidize housing expenses during tertiary education.

⁹ We use 2017 census microdata (<https://redatam-ine.ine.cl>) to calculate the size of indigenous birth cohorts. Indigenous status is based on an affirmative response to one of nine mutually-exclusive indigenous groups, omitting the category of “other” (Appendix Table A1). We estimate the expected cohort sizes at 5–9 years of age by adjusting census cohort sizes for expected mortality.

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

adults (the second difference). The design does not rely on individuals' cumulative grant receipt, which is endogenous and not observed in available microdata. Although eligibility rules could support a regression-discontinuity design, JUNAEB does not retain applicant-level microdata for these cohorts. Instead, we exploit the exogenous expansion of program scale to estimate intent-to-treat effects on indigenous cohorts.

II. Research Design

This section describes our empirical strategy for estimating the long-run and intergenerational effects of indigenous grants. We first describe specifications to estimate dynamic intent-to-treat (ITT) effects of grant exposure across birth years, as well as the average causal response (ACR) to one year of expected grant exposure. We also describe tests of the conditional parallel trends assumption, and our strategy for identifying causal channels of labor market effects. Finally, we describe a related strategy to estimate the intergenerational effects of maternal grant exposure.

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

We estimate event-study regressions in large samples of adults (ages 22–65), born between 1965 and 2000:

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

Y_{icmt} is an adult outcome for individual i , born in cohort c and commune m , and observed in survey year t . We focus on individuals' completed years of schooling, labor supply, and earnings. Chile has 318 communes (or municipalities), the smallest territorial unit.¹¹ We use birth communes because migration decisions (and communes of adult residence) are potentially endogenous to childhood grant exposure. I_{icmt} indicates indigenous status, and additional controls include gender, age indicators, and a proxy for parental schooling. Standard errors are multiway clustered by birth commune and commune of adult residence.

The specification includes fixed effects for survey years (μ_t), birth-cohort-by-birth-commune cells (δ_{cm}), and indigenous-status-by-birth-commune cells ($\theta_m I_{icmt}$). The δ_{cm} play a pivotal role

¹¹ There are currently 346 communes. We consistently re-code responses to 318 super-communes (Appendix Table A3) to account for the creation of new communes since the 1980s.

in identification: they absorb unobserved variables shared by indigenous and non-indigenous individuals born in the same commune and year. These include commune-specific economic trends (including regional convergence), the quality of public schools and health centers, and place-based policies. Identification is therefore based on within-commune difference-in-differences.

The coefficients γ_e and λ_e trace cohort-specific ITT effects for pre- and post-treatment birth years, respectively, relative to omitted cohorts in 1973–1974. We use event-study plots to assess two requirements of the research design. First, there should be no differential pre-treatment trends, conditional on the fixed effects and covariates. We inspect estimates of γ_e for differential trends, and test the joint null that $\gamma_e = 0$ for all e . Second, estimates of λ_e should be increasing in cohorts’ grant exposure (Figure 1, panel B).

We also estimate a parsimonious version that imposes parallel pre-treatment trends ($\gamma_e = 0$), and groups post-treatment cohorts by grant exposure. Cohorts in 1975–1981 were exposed for fewer than 12 grades ($\bar{E} = 0.13$); cohorts in 1982–1991 were exposed for 12 grades during years with lower grant availability ($\bar{E} = 0.65$); and cohorts in 1992–2000 were exposed for 12 grade during years with greater availability ($\bar{E} = 1.86$).

Finally, we estimate a continuous-exposure specification:

$$Y_{icmt} = \tau E_{irc} + \theta_m I_{icmt} + \delta_{cm} + \mu_t + X'_{icmt} \beta + \varepsilon_{icmt}, \quad (2)$$

where E_{irc} measures expected grant years by indigenous status, birth year, and 13 birth regions (the geographic unit above communes).¹² Regional variation arises because JUNAEB sets per-capita budgets that are larger in northern and southern regions, increasing grade-specific selection probabilities and their sum, E_{irc} . The coefficient τ is the average causal response (ACR) to an additional year of expected grant exposure. It is the weighted average of ACRs between adjacent values of E_{irc} , with positive weights that sum to one (Callaway et al. 2024). We compare estimates from equations (1) and (2) by overlaying predicted outcomes from (2) on event-study plots.

¹² We compute $E_{irc} = \omega_r \times E_{ic}$, which is national exposure (E_{ic}) from Appendix Table A2 multiplied by regional weights (ω_r). We measure ω_r as the ratio of region r ’s share of total indigenous grants to its share of the national indigenous population. Weights are the sample analogue implied by Bayes’ rule: $P(G = 1|R = r) = \frac{P(R=r|G=1)}{P(R=r)} \times P(G = 1)$, where $P(G = 1|R = r)$ is the conditional probability of receiving an indigenous grant in region r . Weights vary from 0.52–1.54 (Appendix Table A4). They exceed one in northern and southern regions that are indigenous homelands (regions 1–3 and 9–12) and are below one in central regions that include Santiago (4–8 and 13).

B. Tests of Conditional Parallel Trends

The design assumes that *within-commune* trends in non-indigenous outcomes are a valid counterfactual for trends in indigenous outcomes, conditional on observed variables such as parental schooling. We show that this is satisfied by testing for differential pre-treatment trends in adult outcomes using equation (1). We also show that putative treatment effects are not caused by differential post-treatment trends in unobserved variables correlated with adult outcomes, such as childhood socioeconomic status and school quality.

To allay the post-treatment trends concern, we use children born from 1985–2000. Cohorts before 1985 are unavailable, but the sample overlaps with the large increase in grant exposure from 0.5 to 2.4 years (Figure 1) and, as we will show, ITT estimates. 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 child or household variable that is plausibly correlated with adult outcomes. The fixed effects and controls mirror equation (1), and λ_e captures conditional mean differences between indigenous and non-indigenous children born in year e . We examine the λ_e for trends, and, as required, fail to reject the null of joint equality.

C. Causal Channels of Long-Run Effects

Test Scores and Parent Expectations. Grant eligibility and renewal require students to exceed a minimum *prior-year* GPA threshold in all years, creating grade-related financial incentives to increase effort in such years (Scott-Clayton, 2011). The incentives are more salient in application years because even higher GPAs increase the probability of grant receipt, all else equal (Section I.A). Students apply most frequently using prior-year GPAs from grades 4, 8, and 12, although applications are allowed in any grade.¹³ We use national assessment data in grades 4, 8, and 10 from 1997–2015, spanning cohorts born from 1983–2000, and estimate

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

¹³ The first eligible primary grade is 5, which relies on the grade point average in grade 4. The first eligible secondary grade is 9, which relies on the grade point average in grade 8. Finally, tertiary applications rely on grades received in the final secondary grade.

where Y_{icgm} is a composite test score for student i in birth cohort c , grade g , and commune m . The δ_{cgm} are cohort-by-grade-by-commune fixed effects. Additional controls in X'_{icgm} include indicators for gender and parental schooling, interacted with student grade. Standard errors are clustered by commune.

The λ_{eh} are conditional mean differences between indigenous and non-indigenous students' test scores by cohort and grade. We test for joint equality of the λ_{eh} and interpret rejected nulls as evidence of gains in student learning (conditional on passing earlier tests for differential post-treatment trends in child and household variables). We conduct the same test for an indicator of whether parents expect their child to attain any tertiary schooling. In this setting, program-induced increases in achievement could increase parent expectations, plausibly magnifying longer-run effects on test scores and schooling.¹⁴

Migration, Health, Fertility, and Partners. Using the event-study framework and adult survey outcomes, we test for effects on other potential channels. First, cash transfers may induce migration from birth communes to urban areas with greater opportunities for schooling or skilled labor (Araujo and Macours, 2021). Second, schooling and cognitive skills may improve adult health, with related earnings effects (Clark and Royer, 2013). Third, schooling may reduce early pregnancies that interrupt schooling or work (Breierova and Duflo, 2004; Berthelon and Kruger, 2011). Fourth, schooling may affect the presence and attributes of partners (Torche, 2010).

D. Intergenerational Effects of Mothers' Exposure

We use a similar research design to estimate the effect of mothers' grant exposure on their primary-aged children. We use national assessment and GPA data for students in grades 2 and 4, collected between 2012 and 2017. The dependent variable, Y_{ijcmt} , is a primary-grade learning outcome for child j of mother i . Outcomes include a composite test score and the end-of-year grade point average (which Chilean schools calculate even in early primary grades). The specification mirrors equation (1), and also includes indicators of child gender, birth years, and grade-by-survey-year cells. We examine several channels for the effects of mothers' grant

¹⁴ Chilean parents update beliefs about children's expected school attainment in responses to exogenous increases in achievement (Celhay and Gallegos, 2025). Grant exposure may also directly affect parent expectations, since the explicit labeling of conditional grants as "indigenous" may affect perceived returns to effort and schooling for indigenous individuals (Jensen, 2010; Benhassine et al., 2015).

exposure on child outcomes, including children’s household income, private school enrollments, school-based peer groups, and their own indigenous grant receipt.

III. Data

We implement the empirical strategy using CASEN household surveys, SIMCE national assessment data, and linked administrative grade data. Table 1 summarizes three first-generation samples (FG1–FG3) and two second-generation samples (SG1–SG2).

A. First-Generation Samples

First generation, observed as adults (FG1). We estimate equations (1) and (2) using adults ages 22–57 and born 1965–2000, pooling nine CASEN waves: 2006, 2009, 2011, 2013, 2015, 2017, 2020, 2022, and 2024 (Appendix Table A5). CASEN consistently measures commune of birth and indigenous status. The indigenous survey question is ancestry-based and closely aligns with CONADI certification criteria (Appendix Table A1).

The four main outcomes are (i) completed years of schooling; (ii) monthly labor earnings in 2022 CLP (including zeros); (iii) an indicator for work in the previous week; and (iv) the usual weekly hours worked in the main job (zero for non-workers). Variables measuring causal channels include migration from the commune of birth; self-reported health on a 1–7 scale (standardized to a z-score); a recent illness or accident; whether women had a live birth at age 17 or younger; and co-residence with a partner (married or not), as well as the partner’s indigenous status and years of schooling.

All regressions control for a parent schooling proxy constructed from the IPUMS 2002 census sample (Ruggles et al., 2025). We compute the mean schooling of co-resident mothers and fathers within cells defined by birth year, birth commune, and indigenous status, and merge the variable to FG1 adults. Parent schooling steadily increases across birth years, but there is no evidence of differential pre- or post-treatment trends (Appendix Figure A2).

First generation, observed as children and youth (FG2). We estimate equation (3) using children and youth ages 10–21 and born 1985–2000, pooling seven CASEN waves between 2006 and 2020 (Appendix Table A6). Child and household variables include receipt of an indigenous grant; receipt of other education grants; household income per capita; and household transfer

income per capita, net of grants.¹⁵ We measure two categories of school variables. In CASEN, households directly report whether children receive free school meals; attend full-day instruction; pay school tuition; and enroll in a municipal or private school. Using school identifiers in CASEN, we also merge administrative indicators of participation in major reforms from the 1990s, including P-900, a remedial tutoring program (Chay et al., 2005); MECE-Rural, a package of quality improvements for rural schools (McEwan, 2008b); and the Full School Day reform, which increased instructional time in publicly-funded schools (Dominguez and Ruffini, 2023).

First generation, observed as children and youth (FG3). We estimate equation (4) using 21 SIMCE rounds in grades 4 (1999–2009), 8 (1997–2013), and 10 (1998–2015), assigned to students’ modal birth year.¹⁶ Outcomes include composite test scores and whether parents expect the child to attain any tertiary education. Additional controls are derived from the parent questionnaires of each SIMCE round.

B. Second-Generation Samples

Second generation, observed as children and youth (SG1). We estimate intergenerational effects using SIMCE rounds in grade 2 (2012–2015) and grade 4 (2014–2017). Parent questionnaires collect the age of respondents—85 percent of whom are mothers—which we use to identify maternal birth cohorts.¹⁷ We link SIMCE records, including a composite test score, to administrative datasets with students’ commune of residence, birth year, and grade point

¹⁵ Other education grants include the *Beca Presidente de República* for secondary and tertiary students with low incomes and high achievement, and the *Beca de Apoyo a la Retención Escolar* for lower-income secondary students at risk of dropping out. Transfer income derives from social assistance programs with heterogeneous eligibility rules, none of which depend on indigenous status. Monthly transfers are available to poor households with children (the *Subsidio Único Familiar*), to the disabled and elderly poor (*Pensión Asistencial*), for household water supply (*Subsidio de Agua Potable*), and to extremely poor households (*Chile Solidario*) (Carneiro et al., 2018).

¹⁶ Students’ exact birth years are not available for earlier cohorts in FG3. Approximately 90 percent of enrolled students report a composite test score, and 77 percent of the test score sample have non-missing indigenous status. We estimate coverage rates using administrative enrollment counts from 2002–2015. The composite test score is the mean of language and mathematics scores that are separately standardized within grade-by-year cells. Indigenous status is reported on a parent questionnaire, but incomplete, with information for only mothers in 1997–2000; neither parent in 2001–2005 and 2013; and both parents in 2006–2012 and 2014–2015. To maintain a consistent definition, we use mothers’ indigenous status across all years and, when possible, we impute missing status with the same student’s response in another SIMCE grade.

¹⁷ We impute mother’s age, when possible, using parent questionnaires of the same student in earlier or later SIMCE grades. In cases of disagreement, we calculate the rounded mean of the implied age and birth year and impute it to all student observations.

average.¹⁸ The estimation sample includes students with non-missing test scores and mothers' indigenous status, with mothers born from 1965–1991.¹⁹ Variables measuring causal channels include school type (municipal, private subsidized, or private tuition) and school-based peer composition. The latter is measured as the share of grade-level peers whose mothers are indigenous, and the share who have any tertiary education.

Second generation, observed as children and youth (SG2). We examine additional channels using CASEN children ages 0–22 who reside with mothers in FG1. As in the SG1 sample, we restrict the SG2 sample to mothers born from 1965–1991. Variables include father co-residence; migration from the mother's birth commune; household income per capita; transfer income per capita; and children's receipt of either an indigenous grant or another education grant.

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

A. *Intent-to-Treat Effects*

Figure 2 presents event-study estimates for schooling and labor market outcomes. Each panel plots the estimated cohort coefficients from equation (1) with 95% confidence intervals. The top-left panel corresponds to years of schooling. There is no evidence of pre-treatment trends, and we fail to reject the null that pre-treatment coefficients are jointly zero. Estimates for cohorts born from 1975–1982 are smaller and not statistically distinguishable from zero, consistent with lower exposure. For cohorts born in 1983 and later, the estimates are larger and statistically significant. By the 2000 cohort, the ITT effect is just under one additional year of schooling.

The top-right panel reports labor earnings. We include individuals with zero earnings to avoid sample selection induced by extensive-margin labor supply. We estimate Poisson quasi-maximum likelihood models to avoid scale-sensitive transformations of zeros (Chen and Roth, 2023; Wooldridge, 2010). The estimates are ITT effects in levels, expressed as the approximate percent of control means. For a given Poisson coefficient β , we report $1 - e^{-\beta}$ and confidence intervals using delta-method standard errors (Chen and Roth, 2023). There are no detectable pre-

¹⁸ The composite test score is the mean of language and mathematics scores, separately standardized within grade-by-year cells. Only language scores are available in grade 2.

¹⁹ Ninety percent of enrolled students report a composite test score in these years, and 80 percent of test score records have non-missing mothers' indigenous status for mothers born from 1965–1991.

treatment trends, and post-treatment estimates increase with grant exposure. By 2000, the ITT estimate is approximately 30 percent, which potentially includes labor supply responses.

We therefore analyze two measures of labor supply: an indicator of whether individuals worked in the past week and the weekly hours worked in a typical week, including zeros. Neither outcome shows evidence of pre-treatment trends, and ITT estimates are increasing in the post-treatment period, albeit with wider confidence intervals. By 2000, grant exposure increases the probability of working by about 4 percentage points and weekly hours by 10 percent. As with earnings, the latter is derived from Poisson regressions.

To improve precision, Table 2 (panel A) reports pooled estimates for three groups of indigenous birth cohorts. For partially-exposed cohorts (1975–1981), estimates are positive but small, and generally not statistically significant, except for labor participation in column 3. In contrast, the estimates are larger and statistically significant for fully-exposed cohorts, especially those born from 1992–2000. For these cohorts, the ITT estimates imply 0.57 more years of schooling (column 1), 20 percent higher earnings (column 2), a 4 percentage point increase in any work (column 3), and an 8 percent increase in weekly hours (column 4). In each column, we reject the null that the cohort coefficients are jointly equal or jointly zero. The earnings effects should not be interpreted as a strictly Mincerian return to schooling because of increased labor supply and other channels discussed in Section VI.C.

To benchmark magnitudes, the bottom of Table 2 reports unconditional outcome means for adults born in pre-treatment cohorts from 1965–1974. Indigenous adults had 1.3 fewer years of schooling, 28 percent lower earnings, a 4 percentage point lower employment rate, and worked 2.4 fewer hours per week (7.7 percent) than indigenous adults. The ITT estimates therefore imply a 44 percent reduction in the schooling gap, a 71 percent reduction in the earnings gap, and the elimination of pre-treatment differences in labor supply.

B. Average Causal Responses

Table 2 (panel B) reports estimates of the average causal response (ACR) from equation (2). A one-year increase in expected grant exposure raised indigenous adults' schooling by 0.31 years (column 1), labor earnings by 10 percent (column 2), employment by 1.8 percentage points (column 3), and weekly hours worked by 4 percent (column 4). To facilitate comparisons with the ITT estimates, Figure 2 overlays mean predictions for each cohort from equation (2).

Predicted values generally fall within the pointwise confidence intervals of the ITT estimates, with a few exceptions. The evidence is consistent with approximately linear causal responses over cohort variation in E_{irc} .

ACR estimates are uniformly positive, large, and statistically significant in samples of women and men. The schooling estimate for women is modestly larger (0.35 versus 0.26 years), the earnings estimate is modestly smaller (9 percent versus 10 percent), and labor supply responses are similar (Appendix Table A7; Appendix Figure A3). The ACR estimates are robust to three alternative specifications and samples.²⁰

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

The effects on schooling attainment may vary across the distribution, since primary school is near-universal and grant eligibility only starts in the fifth grade. We re-estimate equation (2) for 16 indicator outcomes, $1\{Y_i \geq x\}$, where Y_i is years of schooling and x indexes grades. The top-left panel of Figure 3 plots the estimates with 95% confidence intervals. As expected, schooling effects are close to zero for early-primary grades. The largest marginal effects are in secondary school (grades 9–12) and, to a lesser extent, upper-primary grades 7–8. Despite larger grants in tertiary education, the estimates are smaller, suggesting that grants are insufficient to surmount academic or financial barriers.

The top-right panel of Figure 3 repeats the analysis for indicators of labor earnings, $1\{Y_i > x\}$. The first coefficient, when $x = 0$, is the ACR on earning *any* labor income in the last month.²¹ The largest marginal effects occur around the median of the earnings distribution at CLP 264,000 (USD 311), but remain above 0.02 even at substantially higher thresholds. Finally, the bottom panel examines effects for indicator of hours worked, $1\{Y_i > x\}$, where $x = 0$ indicates any work. The estimated effect on working positive hours is about 0.02, with a slight upward pattern through 40 hours. This suggests a relatively large extensive-margin effect on the

²⁰ First, there are similar results in specifications with fixed effects for the triple interaction of birth cohorts, birth communes, and survey years (δ_{cmt}), and for indigenous adults in pairs of birth communes and survey years ($I_{icmt}\theta_{mt}$) (Appendix Table A8, panel B). Second, the results are not sensitive to the use of CASEN survey weights (panel C). Third, the baseline estimates (panel A) assume that $E_{irc} = 0$ for 1970–1974 cohorts, despite very small exposure to tertiary grants (Appendix Table A2). ACR estimates are not sensitive to a control group with fewer birth cohorts (1965–1969) (Appendix Table A8, panel D).

²¹ This is not perfectly collinear with the indicator of labor participation ($r=0.92$) since a few adults worked for no pay, and a larger number did not work last week but earned income in the past month.

probability of full-time work in the full sample, that is especially pronounced in the subsample of women (Appendix Figure A4).

V. Threats to Internal Validity

This section evaluates threats to the internal validity of ITT and ACR estimates. We confirm conditional parallel trends, and then provide evidence that our results are not due to concurrent treatments or endogenous misreporting of indigenous status. Finally, we provide evidence that the “strong” parallel trends assumption is satisfied, required to interpret estimates from equation (2) as the ACR of continuously-measured grant exposure (Callaway et al., 2024).

A. Tests for Conditional Parallel Trends

Our first test of parallel trends appeared in Figure 2, in which no outcomes exhibited pre-treatment trends prior to the start of the intervention. Figure 4 reports tests of differential post-treatment trends, based on equation (3). The top-left panel plots estimated coefficients for the only variable that *should* exhibit differential post-treatment trends within birth communes: receipt of an indigenous grant. The dashed line shows a constrained estimate that imposes coefficient equality. Indigenous children were increasingly likely to receive grants across birth years—consistent with Figure 1—and we reject the null of joint coefficient equality at 1%.

In contrast, the remaining panels show no evidence of differential trends for variables that should be unaffected by the policy, including receipt of other education grants, household income per capita, and transfer income.²² Moreover, we find no within-commune, concurrent trends for school-related variables (Appendix Figure A5), including tuition payments and private school enrollment (Hsieh and Urquiola, 2006); free school meals and full-day instruction, and major education reforms (Chay et al., 2005; McEwan, 2008b; Dominguez and Ruffini, 2023).²³ Together, these results lend support to the assumption of conditional parallel trends within birth communes.

²² Appendix Figure A5 illustrates a lack of post-treatment trends for whether children migrated from their birth commune and whether children experienced a recent illness or accident.

²³ The Preferential Subsidy law of 2008 increased per-student funding for some students, but it affected only the 1999 and 2000 birth cohorts (Neilson, 2020; Aguirre, 2022). ACR estimates are robust to excluding the youngest cohorts between 1992 and 2000 (Appendix Table A8, panel E).

B. Concurrent Programs and Misreported Indigenous Status

Concurrent programs are another concern. Any program that also affected education grants, transfer income, or household income per capita would have appeared in Figure 4, where we found no trends. Two government programs targeted modest numbers of indigenous households in a limited number of communes in northern and southern regions.²⁴ Table 3 therefore reports ACR estimates in regional subsamples.²⁵ In northern and southern regions, 26 percent of the sample is indigenous. In central regions, including Santiago, 5 percent are indigenous. The ACR point estimates are similar, and even slightly larger in central regions that were not exposed to other programs. Overall, positive and sizable effects in both subsamples suggest that differential exposure to these programs is unlikely to explain the main results.

A final concern is that non-indigenous children might strategically misreport indigenous status in survey responses, with manipulation increasing across cohorts. This behavior is unlikely for several reasons. First, such behavior would plausibly generate differential post-treatment trends in child and household variables, but we found no evidence of this. Second, there are no incentives to misreport indigenous status on the survey, since it has no effect on grant eligibility, though it carries stigma (Saiz et al., 2008).²⁶ For actual grant receipt, CONADI verifies indigenous status using birth certificates, which are difficult to manipulate (Section I.A).

C. Strong Parallel Trends

Intent-to-treat estimates from equation (1) must assume conditional parallel trends. The interpretation of τ as the average causal response (ACR) further assumes that any two birth cohorts would have the same ITT effect when exposed to the same value of E_{irc} , referred to as

²⁴ CONADI subsidized land purchases by indigenous households in 125 communes, and the Ministry of Education implemented a bilingual education program in 44 communes with high indigenous concentrations (Dascal, Campaña, and de la Fuente 2010; PEIB 2016; Webb 2022). We calculated the cumulative hectares distributed per indigenous person, within combinations of birth communes and survey years, using 20A and 20B land distribution records from <https://siic.conadi.cl>, and commune-level indigenous populations from the 2017 census. In central regions, the median among indigenous adults is zero; in northern and southern regions, the median is 0.027 hectares per indigenous person.

²⁵ Chile added three regions since the original division but we consistently code individuals' birth regions according to the original 13 divisions.

²⁶ The proportion of indigenous adults has risen across CASEN waves, from 7% in the 2006 survey 10% in 2022 (Appendix Table A1). One hypothesis is that stigma of indigenous identity has diminished, leading fewer *indigenous* adults to mis-report as non-indigenous. We re-estimated equation (2) in a restricted sample of household surveys between 2013 to 2022, when the indigenous proportion is stable, with similar results (Appendix Table A8, panel F).

strong parallel trends (Callaway et al., 2024). Two potential concerns arise.²⁷ First, later cohorts were exposed to additional school inputs, such as longer school days (Dominguez and Ruffini, 2023). If inputs are complements to indigenous grants, then ACRs at higher values of E_{irc} could be overstated, increasing $\hat{\tau}$ by conflating greater grant exposure with an upward shift in the entire dose–response function (Callaway et al., 2024). As evidence against this concern, Figure 2 showed that ACRs are approximately linear across E_{irc} , and ACR estimates are similar when excluding birth cohorts from 1992–2000 (Table A8, panel E).

Second, the characteristics of treated children contributing to ITT estimates may differ systematically across adjacent cohorts, but we find no evidence of such differences. For example, there are no differential post-treatment trends in household income per capita between indigenous and non-indigenous children (Figure 4), or between indigenous grant recipients and indigenous non-recipients (Appendix Figure A6).

VI. Causal Channels of Long-Run Effects

This section examines the causal channels of long-run effects. We first analyze test scores and parents’ educational expectations, and then assess channels operating through migration, health, fertility, and partnering. We conclude by assessing whether shorter-run human capital gains can plausibly account for longer-run effects on labor supply and earnings.

A. Test Scores and Parent Expectations

Figure 5 (panel A) reports conditional mean gaps and 95 percent confidence intervals from equation (4), with labels indicating the grade level of each estimate (4, 8, or 10). Not all grade levels took the test each year, and the data start with students born in 1983. For the 1983 and 1984 cohorts, indigenous students scored 0.17–0.19 standard deviations below non-indigenous students, comparing indigenous to non-indigenous students within communes and controlling for parent schooling. By the 2000 cohort, the gap narrows to 0.04–0.05 standard deviations. We easily reject the null of joint coefficient equality. Panel B reports estimates for an indicator that parents expect their child to enroll in tertiary education. For the 1983 and 1984 cohorts, parents

²⁷ Less realistically, the assumption could be violated if indigenous parents selectively timed births in anticipation of future gains in schooling or earnings associated with grant exposure, but this would require parents to foresee grant eligibility and its effects at least a decade in advance.

of indigenous students were 7 percentage points less likely to hold this expectation. By the 2000 cohort, the conditional gap reverses, showing a 1–2 percentage point advantage for indigenous students. Conditional convergence is evident in grade 4 for both measures, consistent with GPA-linked incentives inducing effort in advance of fifth-grade applications (Section II.C).²⁸ Conditional convergence is also evident in subsamples defined by gender and region (Appendix Figure A7).

We interpret these patterns as reflecting grant-induced convergence for two reasons. First, we found no evidence of differential cohort trends in parents' schooling (Appendix Figure A2), and equation (4) flexibly controls for maternal and paternal schooling from the SIMCE questionnaire. Second, Section V showed no evidence of differential post-treatment trends in child and household variables, except for indigenous grants.

The implied difference-in-differences suggests that indigenous test scores rose by at least 0.12 standard deviations and tertiary expectation by at least 8 percentage points, over a period in which expected grant exposure rose by 2.1 years. This estimate is conservative, since Figure 2 already showed positive and statistically significant ITT effects on schooling for early-1980s cohorts.

B. Migration, Health, Fertility, and Partners

We find no evidence that grant exposure affected migration or health. We fail to reject joint equality of the ITT coefficients (or joint equality to zero), and the ACR estimates are small and statistically indistinguishable from zero (Table 4, columns 1–3). The ACR estimate for teen childbearing is slightly negative (column 4), but it is not corroborated by the ITT estimates or the event-study plot (Appendix Figure A8).

In contrast, we find effects on partnering. One additional expected year of grants increased the probability of living with a partner by about one percentage point (Table 4, column 5), driven primarily by men (Appendix Table A9). Grant exposure strongly shifted partner characteristics: one additional expected year of grants reduced the probability of having an indigenous partner by 9 percentage points (column 6) and increased partners' schooling by 0.4 years (column 7). Partner-schooling effects are *larger* than own-effects. This reflects compositional changes

²⁸ Learning effects in grade 4 cannot be plausibly explained by increased income, since they occur in a year with a high-stakes GPA incentive but no grant payments.

arising from reduced indigenous intermarriage, given higher mean schooling among non-indigenous partners.

C. Interpreting the Magnitude of Labor Market Effects

One additional year of expected grants increased schooling by 0.31 years, labor participation by 1.8 percentage points, and labor earnings by 10 percent. However, these estimates do not imply a Mincer school return of 32 percent $\left(\frac{10}{0.31}\right)$. First, the earnings effect incorporates extensive- and intensive-margin labor supply responses (Section II.A). Second, earnings are plausibly affected by increased cognitive skills (Section VI.A).

We therefore assess whether the large effects on labor participation and earnings can be realistically explained by gains in human capital. To do so, we estimate labor market returns to schooling and cognitive skills using a rich auxiliary dataset of Chilean women.²⁹ One year of schooling increases labor force participation by 3.2 percentage points, while one standard deviation in cognitive skills increases participation by 3 percentage points (Appendix Table A10, column 1). Schooling and cognitive skills increase earnings by 22 and 24 percent, respectively (column 2). The latter is estimated with Poisson quasi-maximum likelihood, and encompasses labor supply effects on earnings.

We previously showed that one additional year of expected grants increases women’s schooling by 0.348 years (Appendix Table A7, column 1), and cognitive skills by about 0.05 standard deviations, implying an increase in labor force participation of $\frac{(0.348 \times 0.032)}{\text{Years of school}} + \frac{(0.05 \times 0.030)}{\text{Cognitive skills}} = 0.013$.³⁰ This is close to our female ACR estimates of 1.6 percentage points for labor participation (Appendix Table A7, column 5). Using a similar approach, we predict an ACR of 8.8 percent for earnings, again remarkably close to our estimate of 8.5 percent

²⁹ The ELPI survey is a representative sample of Chilean women with children born between 2006 and 2011. Mothers are ages 22–56 when surveyed; the 10th (90th) percentile of birth years are 1970 (1987). We regress each labor market variable on years of schooling, cognitive skills (including vocabulary and digit span from the Wechsler Adult Intelligence Scale), non-cognitive skills corresponding to the Big Five personality inventory, and the log of adult height, a proxy for pre-labor-market endowments such as early childhood health and nutrition (see Appendix Table A10 for details).

³⁰ In Section VI.A, grant exposure increased by 2.1 expected years, and composite test scores by at least 0.12 standard deviations, implying an approximate ACR of 0.05 standard deviations.

(Appendix Table A7, column 3).³¹ In summary, the substantial magnitudes of longer-run labor market effects are consistent with the predicted returns to shorter-run gains in schooling and cognitive skills.

VII. Intergenerational Effects

This section reports estimates of maternal exposure to indigenous grants on children’s learning outcomes in early-primary grades. It then presents complementary evidence on likely causal channels.

A. Test Scores and Grades

We estimate intergenerational effects of maternal grant exposure on children’s composite test scores and grade point averages, using the SG1 sample of students in grades 2 and 4. Figure 6 presents event-study plots for each outcome, based on the specification described in Section II.D. There is no evidence of pre-treatment trends in either outcome. Among post-treatment cohorts, the ITT estimates increase with mothers’ grant exposure. The estimates are statistically distinguishable from zero (Table 5, panel A, columns 1–2). Among maternal indigenous cohorts born from 1982–1991, children’s test scores and GPAs are higher by 0.09 and 0.11 standard deviations, respectively. These magnitudes are large relative to unconditional pre-treatment gaps of –0.23 standard deviations for test scores and –0.19 standard deviations for GPAs (Table 5).

The corresponding ACR estimates show that an additional year of grant exposure for mothers increases test scores for children by 0.15 standard deviations and GPA by 0.18 standard deviations (Table 5, panel B, columns 1–2). In Figure 6, the solid line is the mean prediction for the children in each maternal cohort, always passing through 95% confidence intervals.

We rule out two threats to internal validity. First, the results are not explained by sample selection of who has children, since grant exposure does not affect the number or timing of births across indigenous cohorts of mothers (see Appendix Table A11, column 1 on total fertility, and Table, column 4 on teenage fertility). Neither does grant exposure affect the probability that mothers are co-resident with children (Appendix Table A11, column 2).

³¹ The predicted earnings effect is $\frac{(0.348 \times 0.219)}{\text{Years of school}} + \frac{(0.05 \times 0.236)}{\text{Cognitive skills}} = 0.088$.

Second, the estimates are not measuring direct (rather than intergenerational) effects of indigenous grants. First, primary-aged children in the SG1 sample were mostly born from 2005–2008, when primary-grade grant exposure was flat. Differential changes in child outcomes would therefore have to be caused by rising parent exposure.³² Second, the rising incomes of indigenous women in treated cohorts made their own children *less* likely to pass the proxy-means test for grant eligibility. We test for the magnitude of this change in the next section.

B. Causal Channels

We examine several channels for effects on learning outcomes. Maternal exposure reduced enrollment in municipal and, to a lesser extent, private subsidized schools, and increased enrollment in private tuition-charging schools (Table 5, columns 3–5).³³ In pre-treatment cohorts, children of indigenous mothers were 13 percentage points less like to enroll in private tuition schools. The ACR estimate implies that this gap narrowed by 8 percentage points with each year of maternal grant exposure. Private tuition schools charge higher fees, enroll higher-income students, and use interviews and exams to select students (McEwan, Urquiola, and Vegas, 2008). These features imply that students’ peer groups also changed. There is no consistent evidence that grant exposure affected the share of indigenous classmates (column 6). However, one year of maternal exposure increased the share of classmates whose mothers have tertiary schooling by 9 percentage points, relative to pre-treatment indigenous mean of 27 percent (column 7).

We examine household channels using the SG2 sample of children residing with mothers in the FG1 sample.³⁴ Maternal grant exposure did not affect whether children also live with fathers (Table 6, column 1), consistent with null effects on women’s likelihood of having a co-resident partner (Appendix Table A9). Nor did it affect migration from mothers’ birth communes (column 2). In contrast, each additional year of expected maternal grant exposure increased household income per capita by 13 percent (panel B, column 3). This estimate exceeds the point

³² See Appendix Figure A9, which extends Figure 1 (panel B) using program data until 2024. The expected years of primary-grade grant exposure plateaued starting with the 2004 cohort.

³³ Appendix Figure A10 (panel A) reports corresponding event study plots. Downward pre-treatment trends for several variables, including private tuition schools, imply that the results are conservative.

³⁴ Appendix Table A12 uses additional variables in the SG2 sample to corroborate results from the SG1 sample on municipal and private enrollments, and further shows that children of indigenous mothers are more likely to pay any tuition and less likely to receive free school meals (offered in publicly-funded schools). Appendix Figure A10 (panel B) reports event study plots. The plots are noisier and less conclusive given sample sizes, although the results for household income per capita, indigenous grant receipt, and private tuition school enrollment show no evidence of pre-treatment trends, and clear evidence that ITT effects change in relation to maternal grant exposure.

estimate for women’s labor earnings (8.5 percent), which is consistent with increased schooling of fathers (Appendix Table A9). At the same time, household transfer income per capita declined by 7 percent (panel B, column 4), reflecting the reduced eligibility of households for means-tested government transfers.

Finally, maternal grant exposure reduced children’s own receipt of indigenous grants. In pre-treatment cohorts, 13 percent of children of indigenous mothers received a grant. The ACR estimate implies that this share fell by 3 percentage points for each year of expected grant exposure (Table 6, panel B, column 5). There is no corresponding effect on other education grants (column 6). Because grant distribution depends on a proxy-means test, rising household incomes plausibly excluded some children from eligibility or reduced the probability that eligible applicants received a grant.

In summary, indigenous grants increased women’s schooling, labor supply, earnings, and the schooling of their partners. Household income per capita rose despite reductions in transfer income. Consistent with higher incomes, indigenous mothers shifted children to selective fee-charging private schools with different peer environments. Child learning outcomes increased, although we cannot determine whether schools caused such effects, or whether earlier skill improvements in households facilitated access to selective schools. Improved learning outcomes cannot be explained by children’s direct receipt of indigenous grants or means-tested transfers.

VIII. Welfare Analysis

We estimate the marginal value of public funds ($MVPF = \frac{\Delta W}{\Delta E - \Delta C}$) for the combined 1999 and 2000 birth cohorts of indigenous students (Hendren and Sprung-Keyser, 2020). ΔW is additional after-tax earnings, ΔE is additional government expenditures, and ΔC is additional tax revenues that offset expenditures. All are expressed in 2022 prices (1 USD \approx 850 CLP) and 2009 present value, using a discount rate of 5.5% (SNI, 2024).

The present value of program expenditures is USD 777 per *eligible* indigenous child in both cohorts.³⁵ Increased schooling attainment causes increased school expenditures of USD 1,480

³⁵ We assume that program expenditures are incurred between 2009 (grade 5) and 2020 (year 4 of tertiary) for the 1999 cohort. Expenditures include grants and 5 percent administrative overhead for distribution. Indigenous cohort sizes are from the 2017 census with a mortality adjustment (as in Figure 1, panel B).

per eligible child.³⁶ These are offset by a fiscal externality, due to increased revenues from a 19 percent consumption tax.³⁷ Overall, the present value of net government expenditures is USD 566 per eligible child. The present value of increased after-tax earnings is USD 16,106 per eligible child, implying an MVPF of 29 even without valuing intergenerational spillovers.³⁸ The MVPF is 6 even when halving the ITT earnings estimate for these cohorts (from 32 to 16 percent).

IX. Conclusions

Despite the ubiquity of education-focused CCTs, there is limited evidence on their long-run and, especially, intergenerational effects. We use the scale-up of Chile's *Beca Indígena* program as a natural experiment to study the impact of a government-implemented CCT on schooling and labor market outcomes of indigenous birth cohorts.

Non-indigenous cohorts never received the grants. Indigenous cohorts born after 1974 became increasingly eligible, with expected grant years reaching 2.4 for the 2000 cohort. Indigenous and non-indigenous children were born in the same communities, enabling comparisons within commune-by-cohort cells. An additional expected grant year increased schooling by 0.31 years, hours worked by 4 percent, and labor earnings by 10 percent. These earnings gains are not explained by differential migration, improved adult health, or lower teen fertility among women. We show some of the gains are plausibly explained by increased skills, as measured by test scores. We further show that effects on schooling and cognitive skills correctly predict effects on labor supply and earnings, using auxiliary estimates of the labor market returns to human capital.

³⁶ We use the average ITT estimate for 1999 and 2000 cohorts (0.75 years) from equation (1), imposing parallel pre-treatment trends. We assume that additional school expenditures correspond to secondary education incurred at 16 years of age. We estimate the annual expenditure per secondary student as the product of GDP per capita in 2022 prices and the ratio of government secondary expenditures to GDP per capita.

³⁷ 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 earnings below 0.825 million CLP are not taxed in 2022.

³⁸ We calculate mean labor earnings for 18–65 year-olds in the 2022 CASEN survey, including adults with zero earnings (Hendren and Sprung-Keyser, 2020). We multiply earnings by 0.73, 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 earnings estimates, and deduct consumption tax payments. We use the average ITT earnings estimate for 1999 and 2000 cohorts (32 percent) from equation (1), imposing parallel pre-treatment trends.

Maternal grant exposure produces intergenerational spillovers for their children of 0.15–0.18 standard deviations—per expected grant year—on test scores and grade point averages. We cannot isolate causal channels, but we document a broad transformation in the socioeconomic environment of young children, including higher household incomes and increased enrollment in selective private schools. We also show mothers’ grant exposure lowered the likelihood that children and households received indigenous grants or other mean-tested transfers.

Taken together, the findings demonstrate that a nationwide CCT program can meaningfully increase human capital for first-generation child recipients and their own children, and can reduce ethnic inequality at scale. Several features of the program and context elucidate the external validity of the results. First, the program was consistently implemented and expanded by stable institutions, including JUNAEB, the Ministry of Education, and CONADI. Second, grant amounts were large and renewable from primary through tertiary schooling, generating substantial financial incentives for forward-looking individuals. Third, Chile invested in school quality over this period, and enforced grade-related conditions that plausibly increased student effort. Both addressed likely constraints to the learning effects of CCTs in other contexts (Levy and Schady, 2013). Finally, Chile’s relatively sustained economic growth has promoted favorable labor market conditions for increasingly skilled workers.

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Table 1: Samples and datasets

Sample	First-generation		Second-generation	Datasets	Survey years
	Birth years	Age at survey	Age at survey		
<u>Panel A: First-generation samples</u>					
FG1	1965–2000	22–57	—	CASEN	2006–2024
FG2	1985–2000	10–21	—	CASEN	2006–2020
FG3	1983–2000	Primary- and secondary-aged	—	SIMCE (grade 4) SIMCE (grade 8) SIMCE (grade 10)	1999–2009 1997–2013 1998–2015
<u>Panel B: Second-generation samples</u>					
SG1	1965–1991	22–50	Primary-aged	SIMCE (grade 2) SIMCE (grade 4) Administrative grade data	2012–2015 2014–2017 2012–2017
SG2	1965–1991	22–57	0–21	CASEN	2006–2024

Notes: CASEN is the *Encuesta de Caracterización Socioeconómica Nacional* (<https://observatorio.ministeriodesarrollosocial.gob.cl/encuesta-casen>). SIMCE is the *Sistema de Medición de la Calidad de Educación* (<https://www.agenciaeducacion.cl/simce/>). Administrative grade data (*rendimiento académico*) datasets include student-level GPA and related variables (<https://datosabiertos.mineduc.cl/rendimiento-por-estudiante-2/>). The final column indicates the largest number of observations in stacked, pooled samples. Section III provides details on samples and variables.

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

	Years of schooling	Labor earnings (%)	Worked last week	Weekly hours worked (%)
	(1)	(2)	(3)	(4)
<u>Panel A: Intent-to-treat</u>				
Indigenous, born 1975–1981	0.039 (0.053)	0.020 (0.018)	0.011* (0.005)	0.016 (0.010)
Indigenous, born 1982–1991	0.321** (0.067)	0.078** (0.024)	0.015* (0.006)	0.033* (0.013)
Indigenous, born 1992–2000	0.565** (0.090)	0.198** (0.042)	0.038** (0.011)	0.078** (0.023)
p-value: jointly equal	<0.001	<0.001	0.024	0.010
p-value: jointly zero	<0.001	<0.001	0.004	0.008
<u>Panel B: Average causal response</u>				
Expected grant years	0.309** (0.046)	0.099** (0.025)	0.018* (0.007)	0.041** (0.014)
<u>Means: adults born 1965–1974</u>				
\bar{Y}_{IND}	9.42	352.5	0.659	28.3
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-1.32	-133.9	-0.040	-2.4
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.123	-0.275	-0.057	-0.077
N (panels A and B)	750,134	738,531	752,744	674,500

Notes: All regressions use the FG1 sample (Table 1). Standard errors are multi-way clustered by commune of birth and commune of residence. ** (*) indicates statistical significance at 1% (5%). Panel A: 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: 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. Mean labor earnings are thousands of 2022 CLP. Column 4: Weekly hours worked not measured in 2020 survey.

Table 3: Average causal responses on schooling, earnings, and labor supply
(regional subsamples)

	Years of schooling		Labor earnings (%)		Worked last week		Weekly hours worked (%)	
	Central regions	N and S regions	Central regions	N and S regions	Central regions	N and S regions	Central regions	N and S regions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Expected grant years	0.314** (0.063)	0.309** (0.050)	0.106** (0.033)	0.092** (0.025)	0.020* (0.009)	0.018* (0.008)	0.035 (0.019)	0.042* (0.016)
<u>Means: adults born 1965-1974</u>								
\bar{Y}_{IND}	10.3	9.2	393.3	339.6	0.700	0.700	28.9	28.1
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.5	-1.5	-90.6	-152.9	-0.00	-0.00	-1.7	-2.6
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.046	-0.143	-0.187	-0.310	-0.039	-0.064	-0.056	-0.083
N	486,100	264,034	478,635	259,896	487,814	264,930	436,922	237,578

Notes: All regressions use the FG1 sample (Table 1). Standard errors are multi-way clustered by commune of birth and commune of residence. ** (*) indicates statistical significance at 1% (5%). Central regions include 4–8 and 13; northern regions include 1–3, and southern regions include 9–12. 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. Labor earnings are thousands of 2022 CLP. Columns 7 and 8: Weekly hours worked not measured in 2020 survey.

Table 4: Effects on migration, health, early fertility, and partners

	Migrated from birth commune	Adult health grade	Recent illness or accident	Birth at ≤17 years (women)	Co- resident partner	Indigenous partner	Partner's schooling
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<u>Panel A: Intent-to-treat</u>							
Indigenous, born 1975-1981	0.008 (0.005)	0.010 (0.016)	-0.004 (0.004)	0.009 (0.006)	0.006 (0.006)	-0.031** (0.007)	0.064 (0.054)
Indigenous, born 1982-1991	0.000 (0.006)	0.032* (0.014)	-0.005 (0.004)	0.002 (0.006)	0.018** (0.005)	-0.093** (0.008)	0.374** (0.075)
Indigenous, born 1992-2000	0.001 (0.008)	—	-0.011* (0.005)	-0.011 (0.007)	0.022** (0.006)	-0.139** (0.014)	0.671** (0.105)
p-value: jointly equal	0.397	0.200	0.389	0.013	0.106	<0.001	<0.001
p-value: jointly zero	0.335	0.068	0.213	0.032	0.003	<0.001	<0.001
<u>Panel B: Average causal response</u>							
Expected grant years	0.001 (0.004)	0.029 (0.015)	-0.002 (0.003)	-0.006* (0.003)	0.009** (0.003)	-0.086** (0.008)	0.408** (0.063)
<u>Means: adults born 1965-1974</u>							
\bar{Y}_{IND}	0.372	-0.288	0.176	0.161	0.652	0.514	9.24
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.045	-0.087	0.012	0.026	-0.010	0.454	-1.54
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.108	0.433	0.072	0.194	-0.015	7.506	-0.143
N (panels A and B)	752,744	329,254	745,009	279,708	752,744	400,326	398,997

Notes: All regressions use the FG1 sample (Table 1). Standard errors are adjusted for clustered by commune of birth and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: 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: Coefficient estimate is $\hat{\tau}$ from equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Column 2: Health grades only measured in 2011–2017. Column 4: Sample includes women, and omits surveys in 2006–2009 and 2020. Columns 6 and 7: Sample is conditional on co-resident partner.

Table 5: Intergenerational effects of maternal exposure to indigenous grants (SG1 sample)

	Learning outcomes				School variables		
	Composite test score	Grade point average	Municipal school	Private subsidized school	Private tuition school	Proportion indigenous mothers in grade	Proportion mothers with any tertiary in grade
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Intent-to-treat							
Indigenous mother, born 1975–81	0.035** (0.009)	0.041** (0.009)	-0.007 (0.005)	-0.016** (0.005)	0.024** (0.004)	-0.001 (0.002)	0.015** (0.004)
Indigenous mother, born 1982–91	0.094** (0.010)	0.109** (0.013)	-0.039** (0.006)	-0.022** (0.007)	0.062** (0.008)	-0.014** (0.004)	0.059** (0.007)
p-value: jointly equal	<0.001	<0.001	<0.001	0.263	<0.001	<0.001	<0.001
p-value: jointly zero	<0.001	<0.001	<0.001	0.003	<0.001	<0.001	<0.001
Panel B: Average causal response							
Expected grant years	0.145** (0.014)	0.183** (0.017)	-0.062** (0.008)	-0.016 (0.011)	0.079** (0.010)	-0.009 (0.009)	0.084** (0.009)
Means: mothers born 1965–1974							
\bar{Y}_{IND}	-0.060	-0.001	0.412	0.557	0.031	0.270	0.267
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.225	-0.185	0.115	0.020	-0.134	0.163	-0.161
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-1.366	-1.007	0.385	0.036	-0.815	1.530	-0.376
N (panels A and B)	1,366,515	1,366,513	1,366,515	1,366,515	1,366,515	1,365,875	1,352,191

Notes: All regression use the SG1 sample (Table 1). Standard errors are clustered by commune of 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.

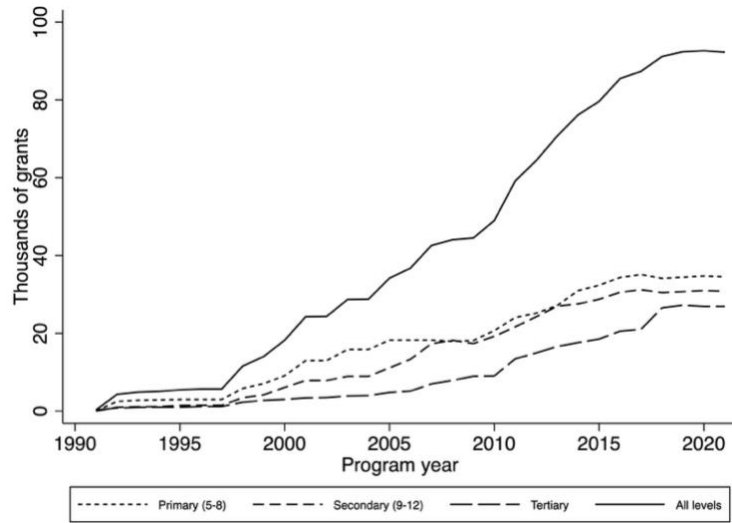
Table 6: Intergenerational effects of maternal exposure to indigenous grants (SG2 sample)

	Ages 0–21			Ages 10–21		
	Co-resident father	Migrated from mother's birth commune	Household income per capita (%)	Household transfer income per capita (%)	Indigenous grant	Other education grant(s)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intent-to-treat						
Indigenous mother, born 1975–81	0.002 (0.010)	0.005 (0.008)	0.003 (0.023)	0.011 (0.021)	-0.003 (0.005)	0.005 (0.003)
Indigenous mother, born 1982–91	-0.001 (0.009)	0.010 (0.011)	0.096** (0.024)	-0.052* (0.024)	-0.025** (0.006)	0.007* (0.004)
p-value: jointly equal	0.676	0.662	0.001	0.006	<0.001	0.529
p-value: jointly zero	0.916	0.638	<0.001	0.016	<0.001	0.070
Panel B: Average causal response						
Expected grant years	-0.002 (0.011)	0.014 (0.014)	0.128** (0.027)	-0.066* (0.027)	-0.032** (0.009)	0.008 (0.004)
Means: mothers born 1965-1974						
\bar{Y}_{IND}	0.660	0.379	179.7	10.5	0.132	0.059
$\bar{Y}_i - \bar{Y}_{ni}$	-0.004	-0.061	-80.2	2.7	0.126	-0.021
$(\bar{Y}_i - \bar{Y}_{ni}) / \bar{Y}_{ni}$	-0.006	-0.139	-0.309	0.350	23.801	-0.260
N (panels A and B)	431,418	431,418	431,412	431,258	293,955	293,955

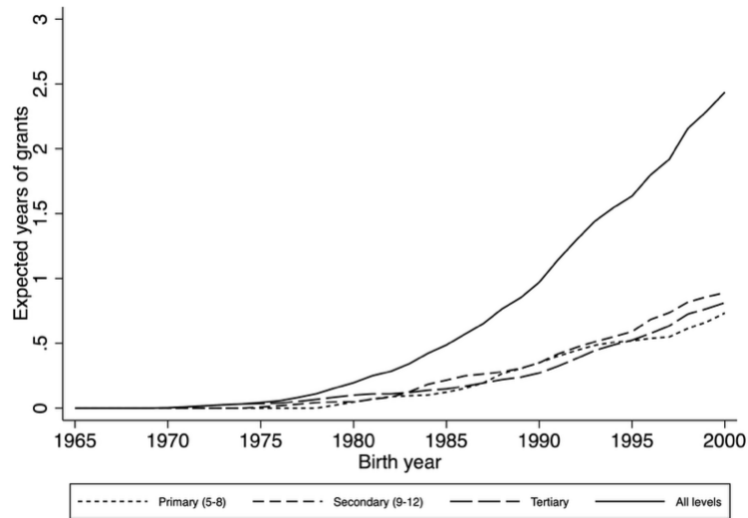
Notes: All regression use the SG2 sample (Table 1). Standard errors are multi-way clustered by commune of mother's birth and commune of residence. ** (*) indicates statistical significance at 1% (5%). Panel A: Coefficient estimates correspond to dummy variables for indigenous mothers in the specified birth years. Panel B: Coefficient estimate is $\hat{\tau}$. Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 3 and 4: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained with the delta method. Mean incomes are thousands of 2022 CLP.

Figure 1: Scale-up of the indigenous grants program

Panel A: Number of grants, by school year

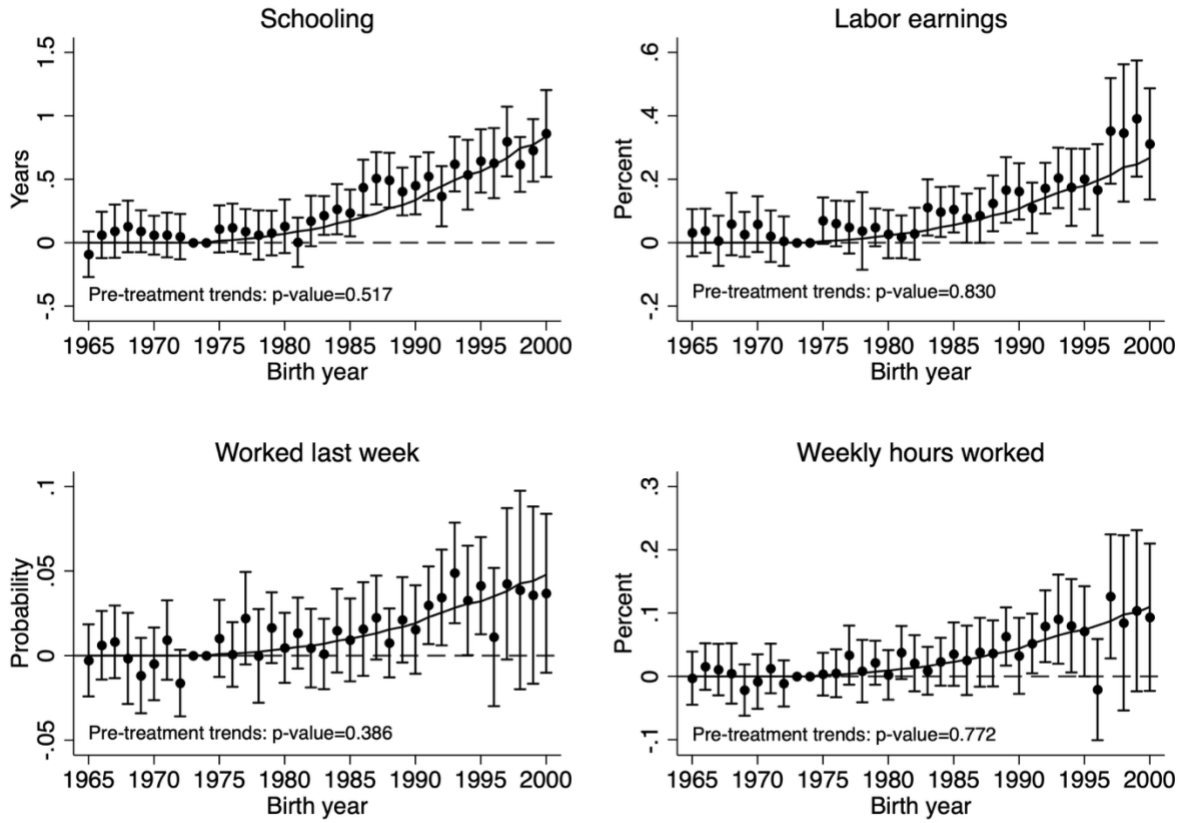


Panel B: Expected years of grants, by birth year



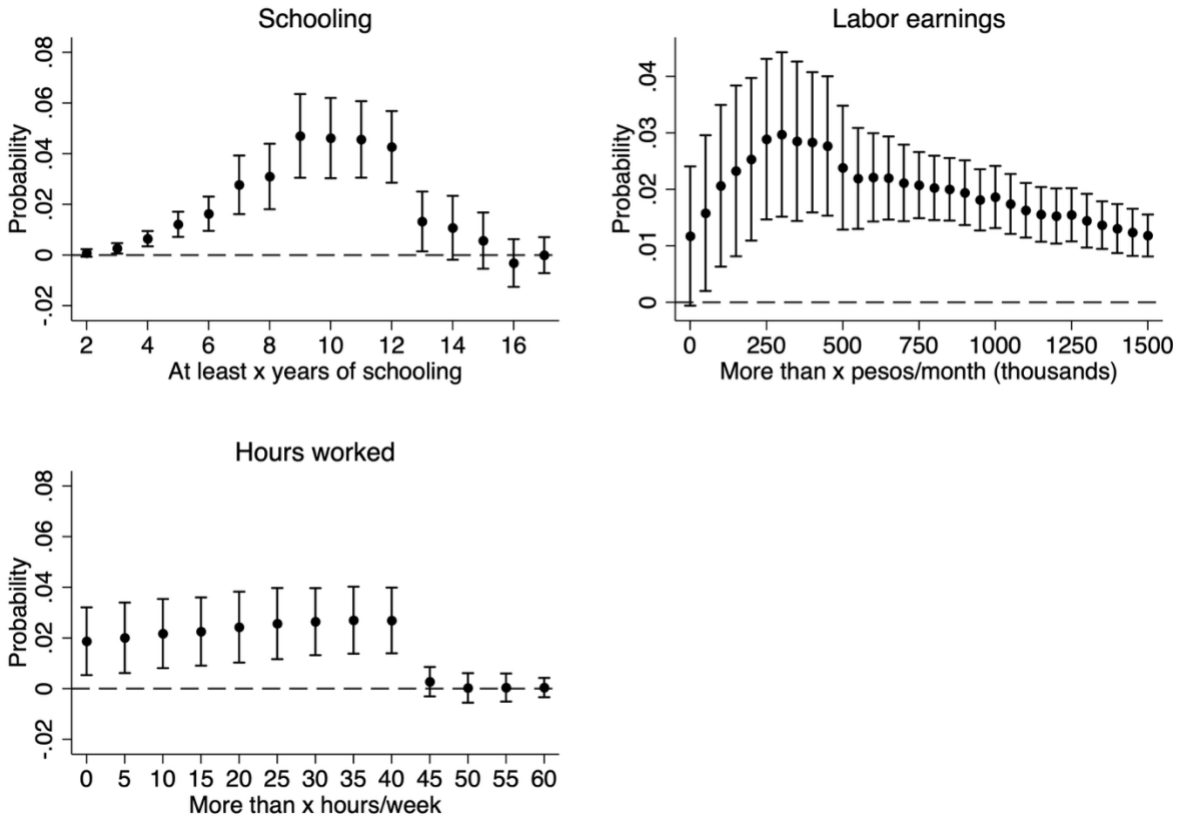
Notes: Panel A: 1991–2005 data from Ministry of Education decrees; 2006–2021 data from annual reports of JUNAEB to DIPRES, a supervisory unit of the Ministry of Finance. Panel B: The sum of grade-specific probabilities of receiving a grant for each birth cohort (Appendix Table A2).

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



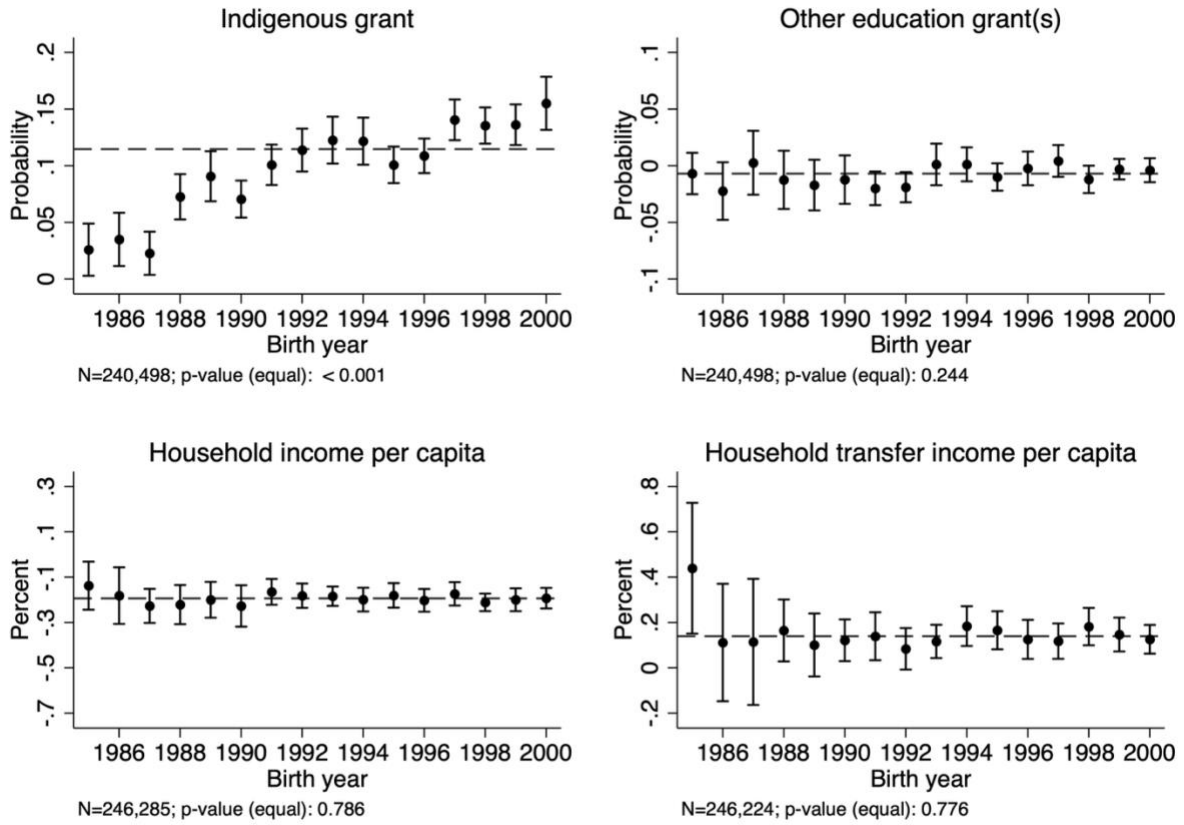
Notes: All panels use the FG1 sample (Table 1). Standard errors are multi-way clustered by commune of birth and commune of residence. Sample sizes in each panel are the same as regressions in Table 2. Each p-value corresponds to the null that $\gamma_e = 0$ for all e . Solid lines are the mean prediction for indigenous adults from equation (2). Left-hand panels: The circles are OLS estimates of γ_e and λ_e in 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 3: Average causal responses in the distributions of schooling, labor earnings, and weekly hours worked



Notes: All panels use the FG1 sample (Table 1). Standard errors are multi-way clustered by commune of birth and commune of residence. Sample sizes in each panel are the same as regressions in Table 2. Each point and 95% confidence interval corresponds to an estimate of τ from equation (2). Top-left panel: Each circle corresponds to the indicator outcome $1\{Y \geq x\}$, 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 indicator outcome $1\{Y > x\}$, where Y is labor earnings or weekly hours worked, and x is the value on the x-axis.

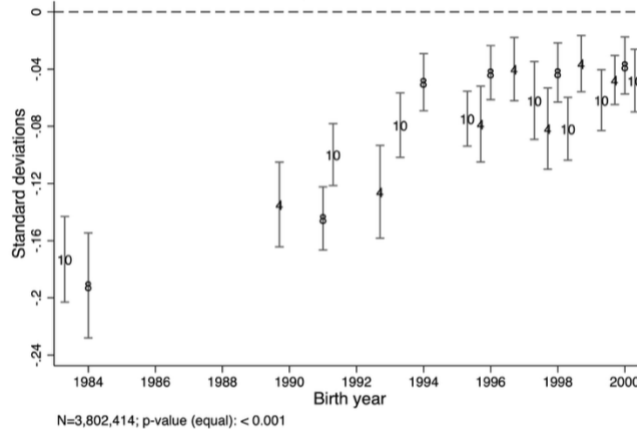
Figure 4: Tests for differential post-treatment trends in grants and income



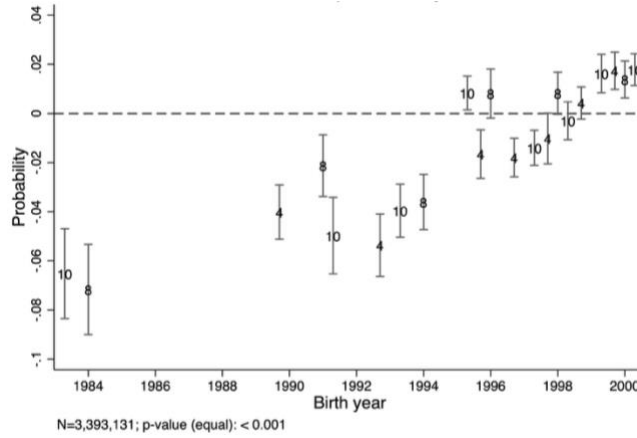
Notes: All panels use the FG2 sample (Table 1). Standard errors are multi-way clustered by commune of birth and commune of residence. The circles are estimates of λ_e from equation (3), with pointwise 95% confidence intervals. Each p-value corresponds to the null that coefficients are jointly equal. Dashed lines are estimates from equation (3) that impose an equality constraint on cohort coefficients. Bottom panels: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and confidence intervals obtained with the delta method.

Figure 5: Effects on student test scores and parent education expectations

Panel A: Composite test score



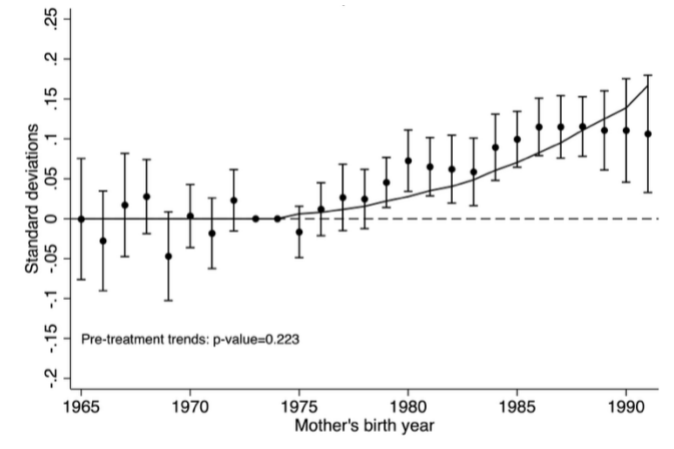
Panel B: Parent expectations for tertiary education



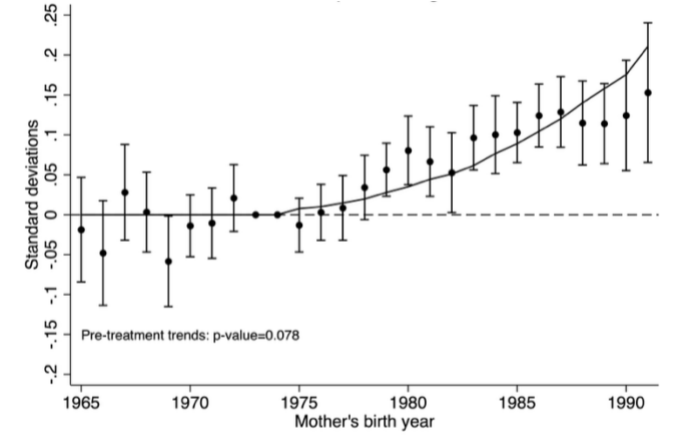
Notes: All panels use the FG3 sample (Table 1). The labeled points are estimates of λ_{eh} for each grade from equation (4), with pointwise 95% confidence intervals. Standard errors are clustered by school communes. Each p-value corresponds to the null that coefficients are jointly equal.

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

Panel A: Composite test score



Panel B: End-of-year grade point average



Notes: All panels use the SG1 sample (Table 1). Standard errors are clustered by commune of residence. Sample sizes in each panel are the same as regressions in Table 6 (columns 1–2). Each p-value corresponds to the null that $\gamma_e = 0$ for all e . The circles are estimates of γ_e and λ_e from an extended equation (1), as described in the text, with pointwise 95% confidence intervals. Solid lines are the mean prediction from an extended equation (2).

Appendix A: Supplementary Tables and Figures

Table A1: Indigenous status in CASEN household surveys and IPUMS census samples

Year	Proportion indigenous		Survey question	Mutually-exclusive categories (ordered as on survey form)
	Weighted	Unweighted		
<u>Panel A: CASEN household surveys</u>				
2024	0.104	0.139	In Chile, the law recognizes [NUMBER] indigenous groups. Is [NAME] a member or descendent of one of them?	Aimara, Rapa-Nui o Pascuense, Quechua, Mapuche, Atacameño (Likan-Antai), Collas, Kawashkar o Alcalufe, Yámana o Yagán, Diaguita, Chango, Selk’nam
2022	0.101	0.142		
2020	0.106	0.130		
2017	0.095	0.122		
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		
<u>Panel B: IPUMS census samples</u>				
2017	0.122	—	Do you consider yourself a member of an indigenous group?	Mapuche, Aymara, Rapa Nui, Lican Antai, Quechua, Colla, Diaguita, Kawésqar, Yagán o Yámana, Other
2002	0.055	—	Are you a member of one of the following indigenous groups?	Alcalufe (Kawashkar), Atacameño, Aimara, Colla, Mapuche, Quechua, Rapa Nui, Yámana (Yagán)
1992	0.106	—	If you are Chilean, do you consider yourself a member of one of the following cultures?	Mapuche, Aymara, Rapanui

Notes: CASEN questionnaires in 2006 and 2009 added “the existence of” after “recognizes.” Surveys in 2006–2017 do not include Chango or Selk’nam. Surveys in 2020 and 2022 do not include Selk’nam. The estimates exclude Chango and Selk’nam 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

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

Notes: Each probability is $\frac{G_c^g}{N_c}$ (see Section I.B). The numerator is the number of grants awarded to a birth cohort in a given grade. The denominator is the size of the indigenous birth cohort from the 2017 census, with a mortality adjustment to reflect the size at 5–9 years old.

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 Joaquín, 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 Río 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.

Appendix Table A4: Regional weights used to compute expected grant years (*Eirc*)

Region	Number of grants in 2017	(a): Regional share of indigenous grants	Indigenous population in 2017	(b): Regional share of indigenous population	(a)/(b): Regional weight
1	7,412	0.087	11,772	0.060	1.46
2	1,902	0.022	6,469	0.033	0.68
3	2,417	0.028	5,492	0.028	1.02
4	1,224	0.014	5,419	0.028	0.52
5	3,056	0.036	8,942	0.045	0.79
6	1,773	0.021	4,604	0.023	0.89
7	2,286	0.027	4,687	0.024	1.13
8	7,715	0.091	19,528	0.099	0.91
9	24,358	0.287	36,730	0.187	1.54
10	16,690	0.197	34,105	0.173	1.13
11	1,613	0.019	2,651	0.013	1.41
12	1,830	0.022	2,907	0.015	1.46
13	12,627	0.149	53,298	0.271	0.55
National	84,903	1.000	196,604	1.000	1.00

Notes: Chile consists of 13 regions, using the old regional numbering. Regional distribution grant shares are from JUNAEB's *Balance de Gestión Integral* in 2017 (https://www.dipres.gob.cl/597/articles-172573_doc_pdf.pdf). Regional shares of the indigenous population are from the IPUMS 2017 census sample, excluding a non-specific "other" category (Appendix Table A1).

Table A5: Descriptive statistics for the FG1 sample of adults

	Indigenous = 0		Indigenous = 1	
	Mean (SD)	N	Mean (SD)	N
Female	0.524	661,388	0.542	91,564
Years of school	12.02 (3.70)	659,045	11.07 (3.77)	91,297
Labor earnings (thousands of 2022 CLP)	472.5 (879.2)	649,305	361.8 (552.8)	89,593
Worked last week	0.680	661,388	0.651	91,564
Weekly hours worked	29.40 (23.06)	593,500	27.82 (23.43)	81,416
Migrated from birth commune	0.382	661,388	0.337	91,564
Adult health (z-score)	0.009 (0.998)	290,726	-0.070 (1.013)	38,685
Recent illness or accident	0.142	654,630	0.151	90,589
Live birth at ≤ 17 (women)	0.130	243,844	0.154	36,421
Co-resident partner	0.533	661,388	0.527	91,564
Partner's years of schooling	11.71	351,512	10.50	48,069
Indigenous partner	0.069	352,717	0.472	48,188

Notes: The sample includes adults (ages 22–65), born from 1965 to 2000, in CASEN household surveys collected in 2006, 2009, 2011, 2013, 2015, 2017, 2020, 2022, and 2024. Standard deviations are reported for continuous variables. See text for variable descriptions.

Table A6: Descriptive statistics for the FG2 sample of children and youth

	Indigenous = 0		Indigenous = 1			
			All		Indigenous grant = 0	Indigenous grant = 1
	Mean (SD)	N	Mean (SD)	N	Mean (SD)	Mean (SD)
<u>Panel A: Ages 10–21</u>						
Female	0.492	211971	0.498	34383	0.487	0.562
Indigenous grant	0.00238	207080	0.144	33487	—	—
Other school grant(s)	0.0902	207080	0.0739	33487	0.0746	0.0697
Household total income p/c (thousands/month)	242.7 (357.7)	211969	179.2 (201.3)	34383	184.8 (208.1)	137.7 (129.2)
Household transfer income p/c (thousands/month)	7.123 (12.35)	211969	9.759 (14.68)	34383	8.993 (13.96)	12.23 (15.12)
Migrated from birth commune	0.232	211971	0.212	34383	0.214	0.193
Recent illness or accident	0.098	209987	0.102	34047	0.101	0.108
<u>Panel B: Ages 10–15</u>						
Receives free school meal(s)	0.638	75526	0.821	12866	0.804	0.897
Attends full day schedule	0.798	75470	0.876	12859	0.870	0.905
Enrolled in municipal school	0.584	74530	0.615	12711	0.612	0.629
Enrolled in subsidized private school	0.370	74530	0.373	12711	0.376	0.360
Pays any school tuition	0.292	75470	0.156	12854	0.167	0.105
Primary or secondary school ever in FSD	0.769	74532	0.849	12712	0.843	0.875
<u>Panel C: Ages 10–13</u>						
Primary school ever in P900	0.337	42462	0.434	7186	0.424	0.478
Primary school ever in MECE-Rural	0.0558	42462	0.158	7186	0.146	0.207

Notes: The sample includes children and youth (ages 10–21), born from 1985 to 2000, in CASEN household surveys collected in 2006, 2009, 2011, 2013, 2015, 2017, and 2020. Standard deviations are reported for continuous variables. Panels B and C impose additional age restrictions. FSD is the Full School Day Program, P900 is the 900 Schools Program, and MECE-Rural is a program for rural, multigrade schools. See text for variable descriptions.

Table A7: 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)
<u>Panel A: Intent-to-treat</u>								
Indigenous, born 1975-1981	0.027 (0.071)	0.042 (0.059)	-0.019 (0.024)	0.046* (0.022)	0.003 (0.008)	0.023** (0.006)	0.002 (0.018)	0.030** (0.010)
Indigenous, born 1982-1991	0.343** (0.080)	0.284** (0.073)	0.047 (0.032)	0.094** (0.025)	0.007 (0.008)	0.022** (0.007)	0.021 (0.020)	0.041** (0.011)
Indigenous, born 1992-2000	0.664** (0.101)	0.442** (0.103)	0.147** (0.046)	0.222** (0.047)	0.031* (0.012)	0.043** (0.013)	0.075* (0.036)	0.081** (0.023)
p-value: equal	0.000	0.000	0.000	0.001	0.046	0.151	0.110	0.031
p-value: zero	0.000	0.000	0.001	0.000	0.067	0.002	0.212	0.002
<u>Panel B: Average causal response</u>								
Expected grant years	0.348** (0.051)	0.264** (0.052)	0.085** (0.025)	0.104** (0.028)	0.016* (0.007)	0.018* (0.007)	0.042* (0.020)	0.040** (0.013)
<u>Means: adults born 1965-1974</u>								
\bar{Y}_{IND}	9.42	9.43	225.1	507.2	0.501	0.851	19.7	38.5
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-1.39	-1.24	-79.5	-188.8	-0.043	-0.028	-2.1	-2.2
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.128	-0.116	-0.261	-0.271	-0.078	-0.032	-0.096	-0.054
N (panels A and B)	394,492	355,060	388,271	349,213	395,779	356,385	353,005	320,207

Notes: Standard errors are multi-way clustered by commune of birth and the commune of residence. ** (*) indicates statistical significance at 1% (5%). Panel A: 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: Coefficient is estimate of τ 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 A8: Average causal response (ACR) estimates for different samples and specifications

	Years of schooling	Labor earnings (%)	Worked last week	Weekly hours worked (%)
	(1)	(2)	(3)	(4)
<u>Panel A: Baseline (Table 2)</u>				
Expected grant years	0.309** (0.046)	0.099** (0.025)	0.018* (0.007)	0.041** (0.014)
N	750,134	738,531	752,744	674,500
<u>Panel B: Additional fixed effects</u>				
Expected grant years	0.305** (0.050)	0.114** (0.028)	0.021* (0.008)	0.050** (0.017)
N	741,252	726,417	743,913	664,190
<u>Panel C: Survey weights</u>				
Expected grant years	0.356** (0.062)	0.126** (0.028)	0.020** (0.005)	0.042** (0.011)
N	750,070	738,470	752,680	674,438
<u>Panel D: Drop assumption that $E_{irc} = 0$ for 1970–1974 cohorts</u>				
Expected grant years	0.310** (0.046)	0.099** (0.025)	0.018* (0.007)	0.041** (0.014)
N				
<u>Panel E: Exclude 1992–2000 birth cohorts</u>				
Expected grant years	0.418** (0.075)	0.100** (0.033)	0.022* (0.009)	0.048** (0.018)
N	664,393	654,727	666,606	606,677
<u>Panel F: Drop CASEN surveys before 2013</u>				
Expected grant years	0.287** (0.048)	0.100** (0.023)	0.016* (0.007)	0.034* (0.014)
N	509,373	501,866	511,784	435,041

Notes: Standard errors are multi-way clustered by commune of birth and commune of residence. ** (*) indicates statistical significance at 1% (5%). Coefficients are estimates of τ from equation (2). 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. See text for details.

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)
<u>Panel A: Intent-to-treat (women)</u>						
Indigenous, born 1975-1981	0.001 (0.007)	0.022 (0.023)	-0.013* (0.006)	-0.000 (0.008)	-0.027** (0.007)	0.039 (0.067)
Indigenous, born 1982-1991	-0.001 (0.007)	0.051** (0.019)	-0.008 (0.005)	0.009 (0.007)	-0.077** (0.008)	0.340** (0.093)
Indigenous, born 1992-2000	-0.006 (0.009)	—	-0.016** (0.006)	0.007 (0.008)	-0.113** (0.014)	0.794** (0.118)
p-value: equal	0.677	0.226	0.439	0.465	<0.001	<0.001
p-value: zero	0.853	0.035	0.017	0.492	<0.001	<0.001
<u>Panel B: Average causal response (women)</u>						
Expected grant years	-0.002 (0.004)	0.054** (0.019)	-0.004 (0.003)	0.004 (0.003)	-0.072** (0.008)	0.427** (0.071)
<u>Means: female adults born 1965-1974</u>						
\bar{Y}_{IND}	0.386	-0.386	0.206	0.634	0.490	9.084
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.040	-0.081	0.012	0.000	0.438	-1.521
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.095	0.267	0.064	0.000	8.451	-0.143
N (panel A and B)	395,779	173,691	391,765	395,779	210,660	209,897
<u>Panel C: Intent-to-treat (men)</u>						
Indigenous, born 1975-1981	0.017* (0.007)	-0.002 (0.022)	0.007 (0.005)	0.012 (0.007)	-0.034** (0.009)	0.107 (0.070)
Indigenous, born 1982-1991	0.002 (0.008)	0.013 (0.016)	-0.002 (0.005)	0.023* (0.009)	-0.106** (0.009)	0.465** (0.086)
Indigenous, born 1992-2000	0.009 (0.011)	—	-0.006 (0.008)	0.030** (0.011)	-0.160** (0.019)	0.559** (0.137)
p-value: equal	0.156	0.505	0.183	0.270	<0.001	<0.001
p-value: zero	0.085	0.667	0.308	0.033	<0.001	<0.001
<u>Panel D: Average causal response (men)</u>						
Expected grant years	0.004 (0.005)	0.011 (0.019)	-0.001 (0.004)	0.012 (0.006)	-0.100** (0.010)	0.425** (0.076)
<u>Means: male adults born 1965-1974</u>						
\bar{Y}_{IND}	0.356	-0.167	0.140	0.674	0.543	9.425
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-0.052	-0.087	0.009	-0.021	0.473	-1.550
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.127	1.080	0.071	-0.030	6.798	-0.141
N (panel C and D)	356,385	155,083	352,663	356,385	188,540	187,974

Notes: Standard errors are multi-way clustered by commune of birth and commune of residence. ** (*) 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: Schooling, cognitive skills, and labor market outcomes

	Worked last week	Labor earnings (%)
Years of schooling	0.032** (0.001)	0.219** (0.007)
Cognitive skills (s.d.)	0.030** (0.005)	0.236** (0.021)
Year-by-age fixed effects	Y	Y
Region fixed effects	Y	Y
Additional controls	Y	Y
N	21,969	21,966

Notes: ELPI is the *Encuesta Longitudinal de Primera Infancia* (<https://observatorio.ministeriodesarrollosocial.gob.cl>). The sample includes mothers ages 22–56, surveyed in 2010 and 2012. Some mothers are surveyed in both rounds; standard errors are clustered by mother. **(*) indicates statistical significance at 1% (5%). The cognitive skills variable is the average of z-scores for two variables in the Wechsler Adult Intelligence Scale (WAIS): vocabulary and digit span. Additional controls include an indigenous indicator, z-scores for five variables in the Big Five personality inventory, the log of mothers’ adult height, and indicators for missing values of these variables. Column 1: indicator of whether women worked in the week prior to the survey. Column 2: monthly labor earnings, including zeros, in 2022 CLP; exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained with the delta method.

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

	Number of children ever born	Number of co-resident children under 22
	(1)	(2)
<u>Panel A: Intent-to-treat</u>		
Indigenous, born 1975–1981	0.017 (0.025)	0.014 (0.019)
Indigenous, born 1982–1991	-0.008 (0.023)	0.041* (0.018)
p-value: equal	0.173	0.082
p-value: zero	0.393	0.051
<u>Panel B: Average causal response</u>		
Expected grant years	-0.017 (0.021)	0.033 (0.019)
<u>Means: female adults born 1965-1974</u>		
\bar{Y}_{IND}	2.525	1.298
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	0.206	0.075
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	0.089	0.061
N (panels A and B)	245,538	352,317

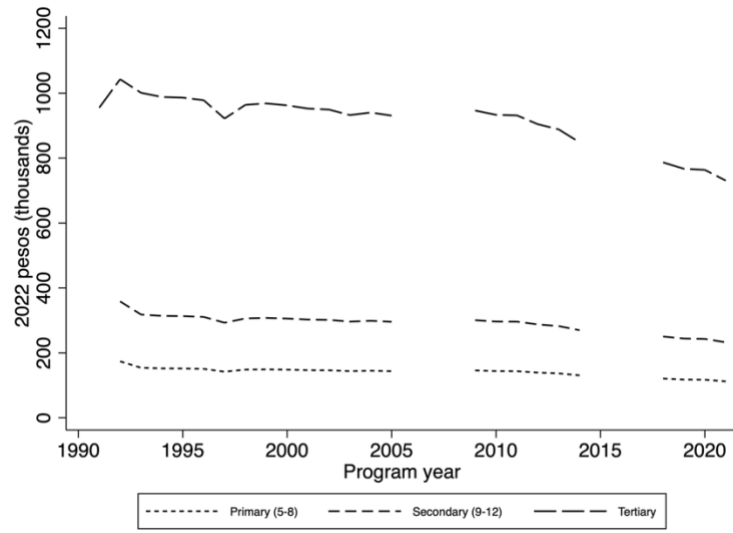
Notes: Standard errors are multi-way clustered by commune of birth and commune of residence. ** (*) indicates statistical significance at 1% (5%). Panel A: The coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls 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 1: the sample omits surveys in 2006, 2009, and 2020.

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

	Children ages 6 to 15					
	Municipal school	Private subsidized school	Private tuition school	HH pays any tuition	School offers free meal(s)	School offers full day instruction
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Panel A: Intent-to-treat</u>						
Indigenous mother, born 1975-1981	-0.021* (0.010)	0.016 (0.010)	0.006* (0.003)	0.023** (0.008)	-0.013 (0.010)	-0.010 (0.009)
Indigenous mother, born 1982-1991	-0.039** (0.012)	0.016 (0.012)	0.023** (0.006)	0.040** (0.009)	-0.059** (0.012)	-0.017 (0.010)
p-value: equal	0.100	0.990	<0.001	0.054	<0.001	0.554
p-value: zero	0.004	0.222	<0.001	<0.001	<0.001	0.201
<u>Panel B: Average causal response</u>						
Expected grant years	-0.026 (0.014)	0.001 (0.014)	0.026** (0.005)	0.039** (0.012)	-0.073** (0.014)	-0.008 (0.012)
<u>Means: adults born 1965-1974</u>						
\bar{Y}_i	0.575	0.409	0.010	0.148	0.799	0.841
$\bar{Y}_i - \bar{Y}_{ni}$	0.053	-0.004	-0.046	-0.154	0.200	0.085
$(\bar{Y}_i - \bar{Y}_{ni}) / \bar{Y}_{ni}$	0.101	-0.011	-0.816	-0.510	0.334	0.113
N (panels A and B)	196,143	196,143	196,143	161,888	198,678	180,322

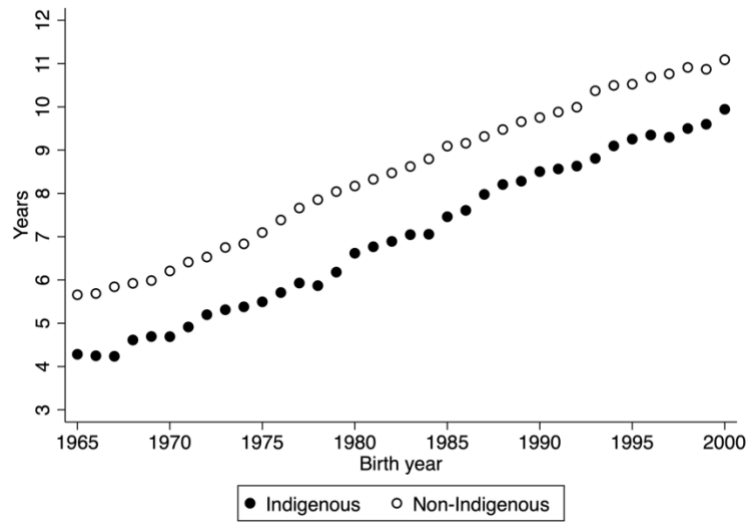
Notes: Standard errors are adjusted multi-way clustered by commune of mother's birth and commune of residence. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous mothers in the specified birth years. Panel B: The coefficient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

Figure A1: Annual value (2022 CLP) of indigenous grants, 1991–2021



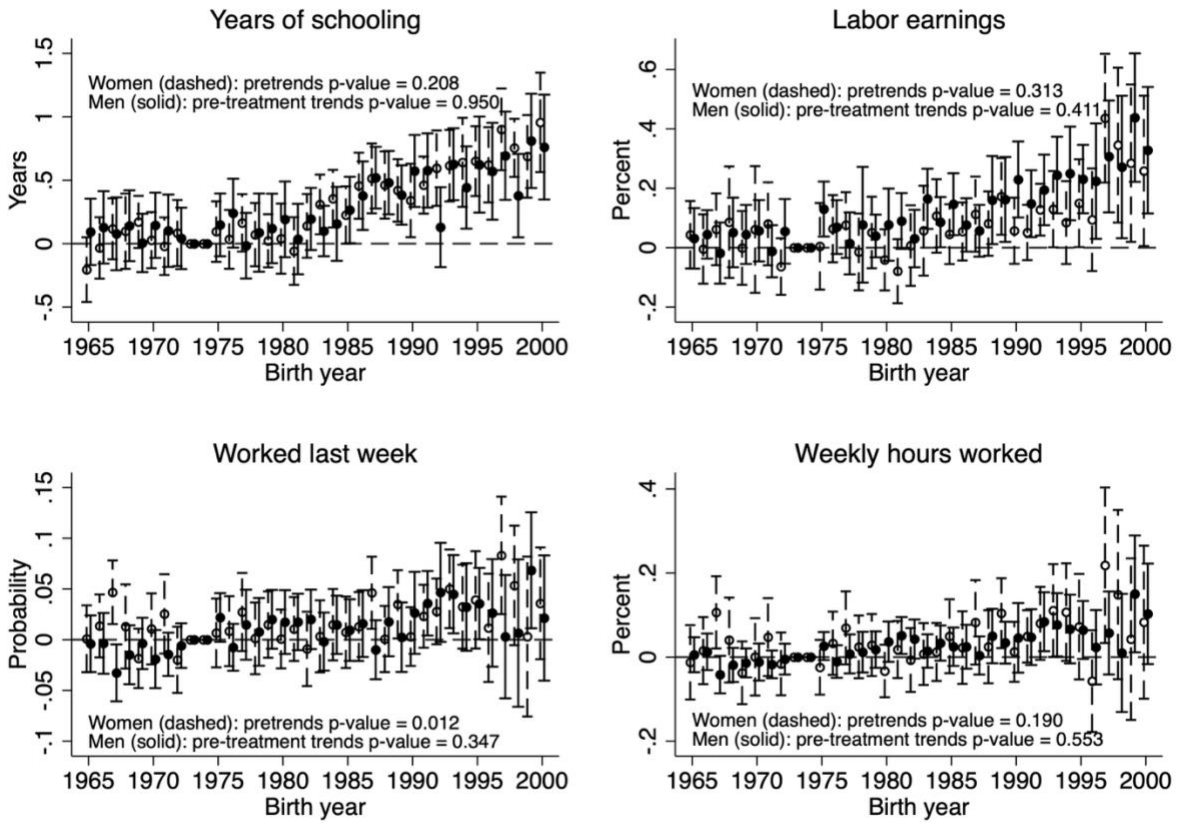
Notes: Each line plots the annual value (in 2022 CLP) of primary, secondary, or tertiary indigenous grants. Missing segments indicate years with missing data in the sources described in Figure 1.

Figure A2: Parental schooling variable from the 2002 census



Notes: We use the 10% IPUMs sample of the 2002 census to compute the mean parent schooling—including fathers and mothers—within combinations of birth year, birth commune, and indigenous status. We attach means to adults in the FG1 sample. Circles report sample means for indigenous and non-indigenous adults.

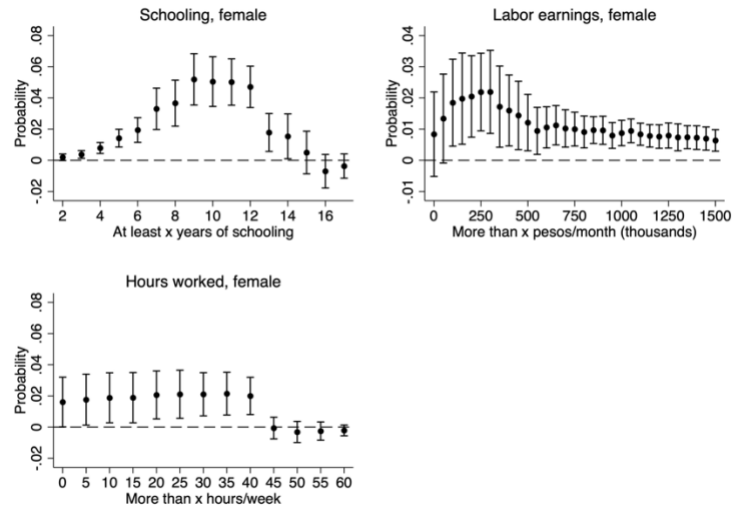
Figure A3: Event study plots for schooling, labor earnings, and labor supply (female and male samples)



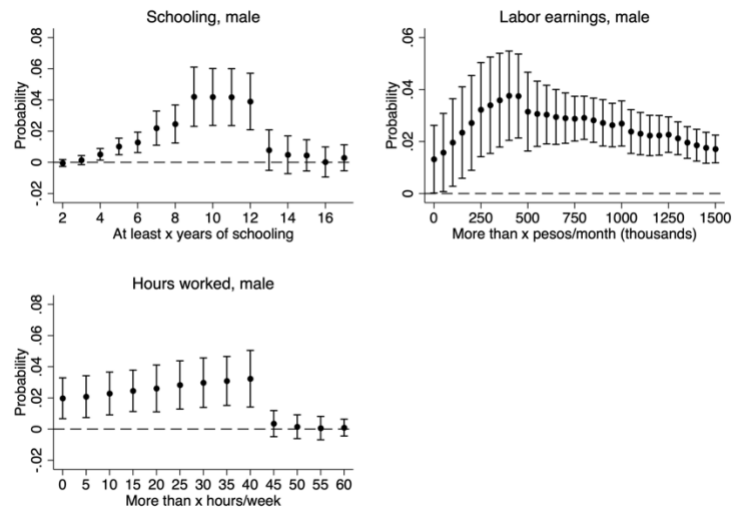
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Table 2. P-values corresponds to the null hypothesis that $\gamma_e = 0 \forall e$. Left-hand panels: The circles are $\hat{\gamma}_e$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

Figure A4: Average causal responses in the distributions of schooling, labor earnings, and weekly hours worked (by female and male)

Panel A: Female sample

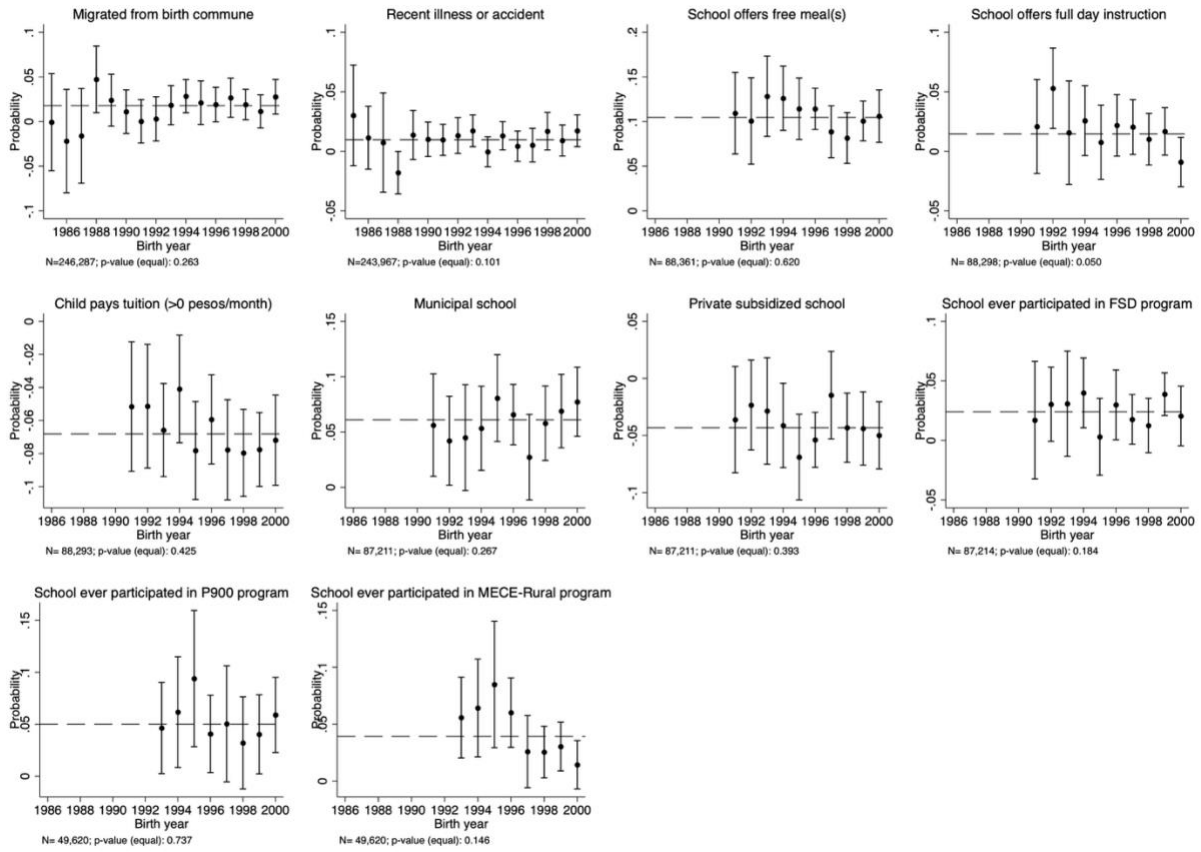


Panel B: Male sample



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

Figure A5: Additional tests for differential post-treatment trends



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.

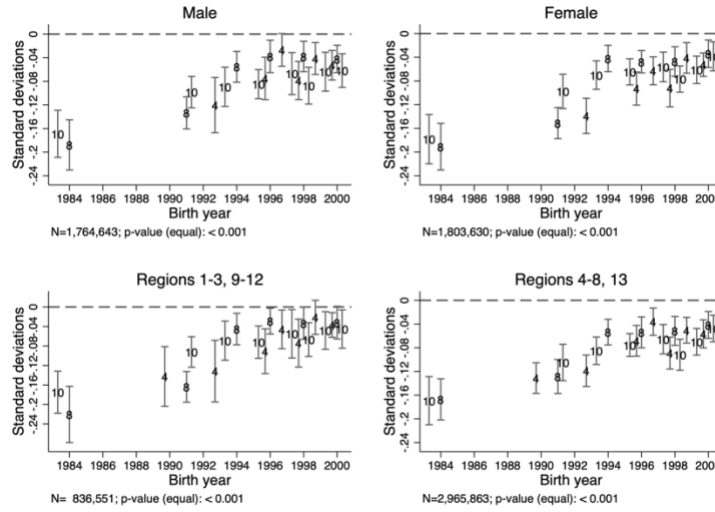
Figure A6: Post-treatment trends in household income of indigenous grant recipients, relative to indigenous non-recipients



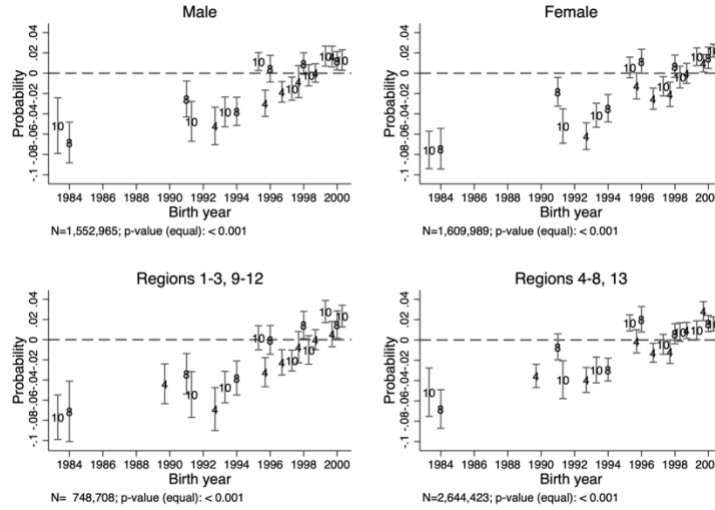
Notes: The panel uses the FG2 sample (see Table 1), limited to indigenous individuals ages 10–21. The circles are $\hat{\lambda}_e$ from a modified equation (3), with pointwise 95% confidence intervals, in which I_{icmt} is replaced with a dummy variable indicating that indigenous individuals currently receive an indigenous grant. Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. P-values correspond to the null hypothesis that λ_e are jointly equal. The dashed lines are estimates for which the λ_e are constrained to be equal across birth years.

Figure A7: Post-treatment trends in test scores (female, male, and regional samples)

Panel A: Composite test

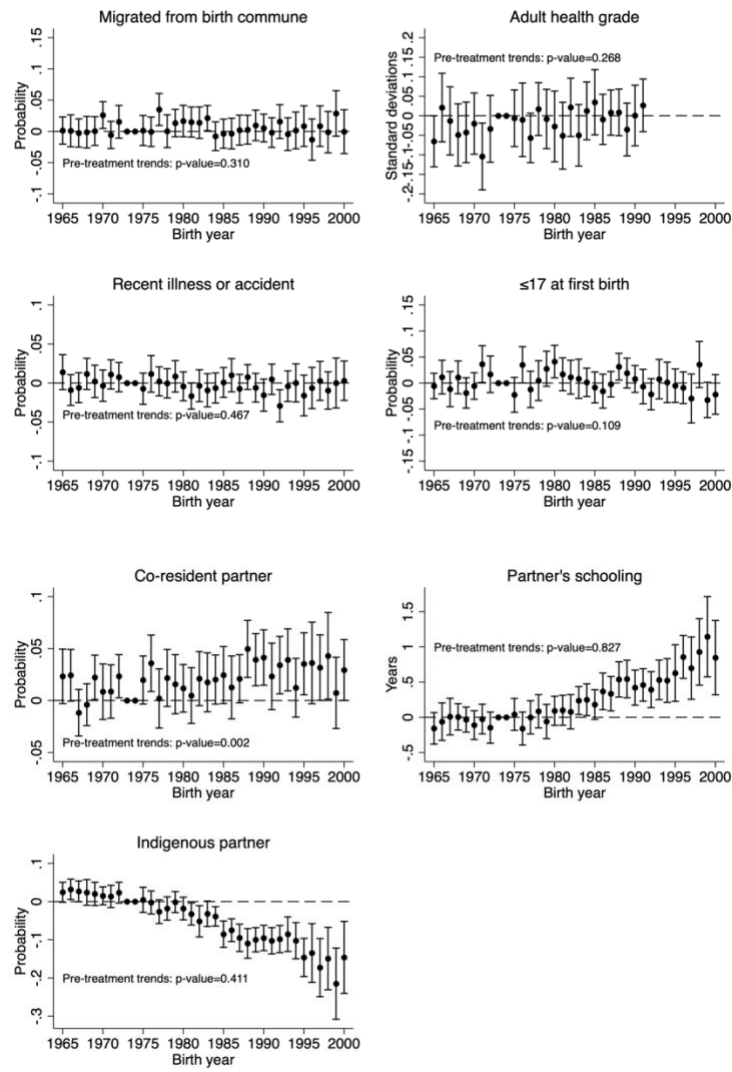


Panel B: Parent expectations for tertiary education



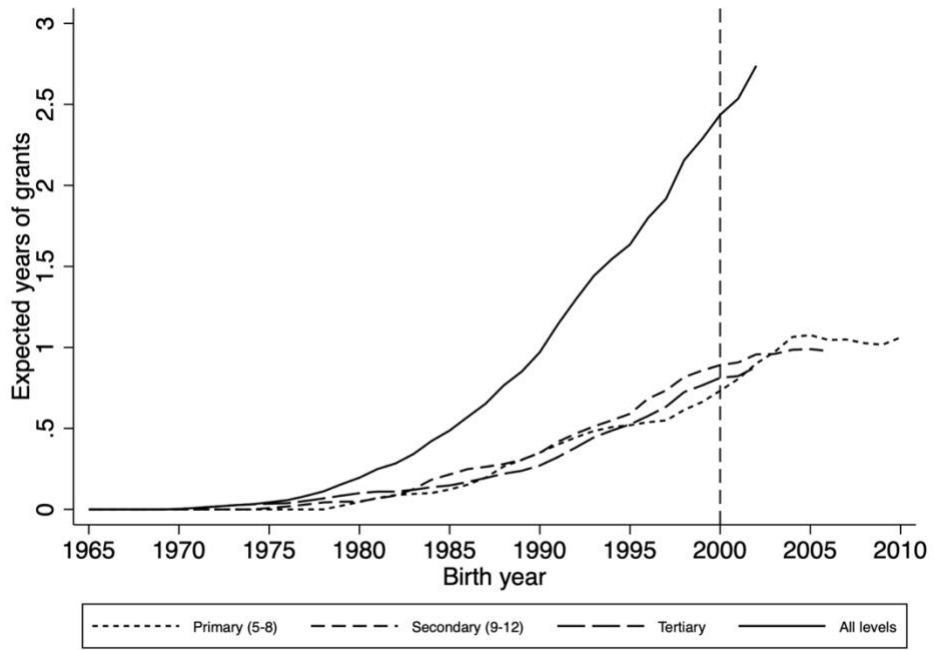
Notes: Each panel uses the FG3 sample (see Table 1), within four subsamples. Northern and southern regions are 1-3 and 9-12, while central regions are 4-8 and 13. The labeled points are $\hat{\lambda}_{eh}$ for each grade from equation (4), with pointwise 95% confidence intervals. Standard errors are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

Figure A8: Event study plots for additional variables in FG1



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

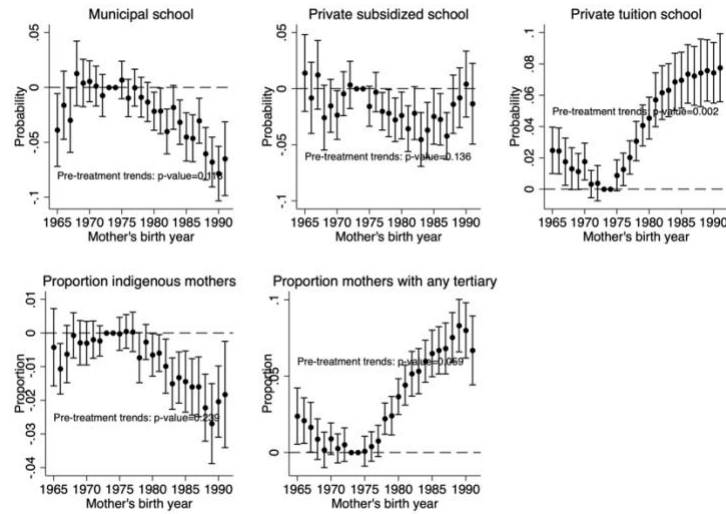
Figure A9: Expected years of indigenous grants, by birth year (1965 to 2010)



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

Figure A10: Event study plots for additional dependent variables in SG1 and SG2 samples

Panel A: SG1 sample



Panel B: SG2 sample

