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 means-tested, renewable cash grants to indigenous students in upper-primary, secondary, and tertiary education, conditional on enrollment and grades. We estimate effects of expected grant exposure on indigenous cohorts and their children, comparing indigenous and non-indigenous Chileans born in the same year and commune. The most-exposed cohorts completed more schooling, worked more, and earned 20 percent more. Test-score gaps narrowed, even in grades with performance incentives but no payments. Maternal exposure raised children's early-grade test scores and grade point averages, and reduced second-generation receipt of indigenous grants and means-tested transfers. The first-generation marginal value of public funds is 29.

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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, the long-run effects of CCTs on treated children and subsequent generations 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 from cash transfers (but for an exception using scholarships, see Duflo 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 cash grants to indigenous students enrolled in upper-primary (grades 5–8), secondary, and tertiary education. Eligibility requires verified indigenous ancestry, passing a proxy-means test, minimum prior-year grades, and an online application. A government agency then allocates grants to applicants with higher grade point averages and lower incomes (JUNAEB, 2024). Once awarded for a single school year, grants are renewable for subsequent years in the same level of education, if students meet eligibility criteria. The program therefore combines features of canonical CCTs—cash conditioned on enrollment, targeted by a means test (Fiszbein and Schady, 2009)—with the performance-based eligibility and renewal conditions of merit scholarships (e.g., Barrera-Osorio and Filmer, 2016).

Our research design exploits differential grant exposure across indigenous status and birth cohorts. Non-indigenous children were never eligible. Indigenous birth cohorts before 1975 had no exposure, while cohorts from 1975 onward were increasingly exposed as the program

[1] On 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 (2024); 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).

expanded from 300 grants in 1991 to over 92,000 in 2021. We estimate intent-to-treat (ITT) effects on indigenous cohorts using event-study regressions and pooled survey samples of adults born between 1965 and 2000. The difference-in-differences specification is not a simple comparison of national cohort trends across indigenous and non-indigenous adults. Rather, the design relies on a legally defined ancestry rule that assigns eligibility at the individual rather than the geographic level. Indigenous and non-indigenous children born in the same commune and year therefore differed in eligibility. The strategy is feasible because indigenous birthplaces are dispersed across rural and urban communes, so most commune-by-cohort cells contain both groups.[3] Our preferred specification includes over 11,000 fixed effects for the interaction of 36 birth cohorts and 318 birth communes, Chile's smallest territorial unit. The fixed effects absorb time- and birthplace-varying confounders, including local economic trends, the evolving quantity and quality of public schools and health centers, and place-based policies targeted to rural and lower-income communities. We also include indigenous-by-birth-commune fixed effects, so that our preferred specification aggregates 318 intra-commune differences-in-differences.

ITT estimates show 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. The estimates pertain to entire indigenous cohorts, encompassing direct effects on recipients and, as we shall argue, incentive-based effects on untreated, potential recipients. Because of this—and the lack of retrospective treatment data for adults in household surveys—we cannot recover local average treatment effects of individual grant dosage. As a tractable alternative, we scale ITT effects by cohort-level grant exposure—estimated with administrative and census data—and report the average causal response (ACR) to one additional year of a cohort's expected grant exposure. The ACR 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 the returns to increased extensive-margin labor supply, additional years of schooling, and other causal channels discussed below, including higher cognitive skills and parent expectations for school attainment. Using the ITT earnings estimates for the 1999–2000 birth cohorts, we calculate the marginal value of public funds (MVPF) (Hendren and Sprung-

[3] Chileans with indigenous ancestry are born (and reside) throughout Chile. The communes with the highest indigenous ancestry concentration are in southern and northern regions, yet one-quarter of the indigenous adults in our survey samples were born in one of the 34 communes in the central Metropolitan Region, which includes Santiago.

Keyser, 2020). Each dollar of net government expenditure generates approximately $29 in after-tax lifetime earnings.

The internal validity of ITT estimates depends on the assumption of parallel trends between indigenous and non-indigenous outcomes *within birth communes,* conditional on controls such as parental schooling. We test this assumption in four ways. First, across all dependent variables, the pre-treatment event-study coefficients are small, statistically indistinguishable from zero, and inconsistent with differential pre-treatment trends. The post-treatment ITT coefficients increase across birth cohorts in a pattern that closely resembles the same cohorts' increasing exposure to indigenous grants.

Second, we find no post-treatment differential trends—within birth communes—for many child and household covariates typically associated with schooling and earnings, including household income per capita, transfer income per capita from other government programs, the receipt of education grants (other than *Beca Indígena*), children's private-school enrollment, and children's exposure to other education reforms (Chay, McEwan, and Urquiola, 2005; Dominguez and Ruffini, 2023). In fact, we find a post-treatment trend for just one variable: whether a child receives an indigenous grant, consistent with administrative data on indigenous cohorts' rising grant exposure from 1975–2000.

Third, the results are not explained by two government programs that specifically targeted indigenous households in some communes of northern and southern regions, since ACR estimates are similar in central regions that were entirely unexposed to these programs. We further consider endogenous misreporting of indigenous status on household surveys, which could have increased across birth cohorts in response to the growing availability of indigenous grants. But few Chileans have an incentive to report indigenous status falsely, given consistently negative stereotypes associated with indigeneity (Saiz, Rapimán, and Mladinic, 2008) and the absence of monetary benefits to misreporting, since grant eligibility is based on formal government verification and not survey responses.

Fourth, the program's rollout coincided with Chile's return to democracy, raising the possibility that a gradual decline in discrimination against indigenous Chileans, rather than the program itself, explains the steady increase in ITT estimates across cohorts. Several pieces of evidence argue against this interpretation. Declining labor-market discrimination would have produced convergence between indigenous and non-indigenous adults from every birth cohort in

our post-2006 survey samples, yet cohorts born before 1975 show no relative gains. It would also have raised indigenous earnings at any given level of schooling, but the earnings effect falls from about 10 percent to 1.6 percent once we condition on completed schooling. Declining discrimination inside schools is harder to separate from the program, because it would raise schooling and skills through the same channels. However, we find no changes in observable features of indigenous children's schools that discrimination might plausibly affect, such as private school enrollment, and measured discriminatory attitudes toward indigenous Chileans persisted well past 1991 (Saiz et al., 2008; Haye et al., 2010; Becerra, Merino, and Mellor, 2015; Webb and Radcliffe, 2016; Webb, 2022).

We explore many causal channels for the labor market effects of exposure to indigenous grants. The labor market improvements are plausibly due to relative gains in test scores and parents' expectations for schooling attainment for indigenous children. However, they cannot be explained by migration away from the birth commune, self-reported health, teenage fertility, or co-residence with a partner (although we do find increases in ethnic intermarriage and partners' schooling). We further show that the large effects on labor market outcomes can be realistically explained by human capital channels, including years of schooling and cognitive skills.

Finally, we examine the intergenerational impacts of maternal grant exposure. 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 (of mothers and partners), household income, private-school enrollment, and school-based peer composition. Maternal grant exposure, and the resulting improvements in children's socioeconomic environments, further reduces the likelihood that their own children receive indigenous grants, confirming that intergenerational test score effects are not confounded with children's ongoing program participation. These results also show that participation in an effective social program is not necessarily transmitted to the second generation, as long as income-based targeting rules are enforced.

The paper makes three contributions to the economics literature. 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).[4] Our first-generation design is most 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, the design identifies impacts of lifetime exposure relative to a no-treatment counterfactual, rather than full-versus-partial exposure across treated arms, as in some experimental settings (Araujo and Macours, 2021; Barham et al., 2024). Second, individual eligibility rules allow us to control for commune-by-cohort fixed effects that absorb fine-grained geographic variation in economic conditions and local policies. Third, the never-treated control group permits ITT estimation under a standard rather than a "strong" parallel trends assumption within communes (Callaway, Goodman-Bacon, and Sant'Anna, 2024). That said, our ACR estimates do require strong parallel trends (as in Parker and Vogl, 2023) because they rely on continuously measured program exposure. We discuss evidence for this assumption in the results.

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 skills of children born to treated mothers (Duflo et al., 2024). Other government-implemented education policies in developing countries have affected second-generation outcomes, including school construction (Mazumder, Rosales-Rueda, and Triyana, 2019, 2023; Akresh, Halim, and Kleemans, 2023; Lucas and McEwan, 2026) and the elimination of racial school restrictions in Zimbabwe (Agüero and Ramachandran, 2020).[5] We also contribute to a related literature on the intergenerational persistence of social program participation. In contrast to positive intergenerational transmission of social programs in Europe and the United States (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, consistent

[4] 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).

[5] In Chile, Barrios-Fernández, Neilson, and Zimmerman (2026) find that test-based admission to elite university programs affected tertiary access of the second generation.

with rising first-generation incomes and a proxy means test applied to households of second-generation children.

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; Näslund-Hadley and Santos, 2022). While the U.S. literature exhaustively documents racial inequality and the effects of many schooling reforms (e.g., Card and Krueger, 1992; Cook and Evans, 2000; Aaronson and Mazumder, 2011), much less is known about CCTs and 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 schooling. This section describes grant amounts and the rules governing their distribution. It then describes program expansion across school years and birth cohorts.

A. Grant Amounts and Distribution

In 2021, annual grant amounts were CLP 100,550 (USD 118) for primary students; CLP 208,280 (USD 245) for secondary students; and CLP 654,600 (USD 770) for tertiary students.[6] 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). JUNAEB deposits grants in mandatory accounts at the state-owned *BancoEstado*, in two installments per year for primary and secondary schooling and ten installments for tertiary. For primary students, funds are deposited

[6] 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).

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.[7] 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 each of Chile's 13 regions (JUNAEB, 2024).[8] 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.

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.[9] 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:*

[7] In primary and secondary grades, students repeat the grade if they score below 4 in three or more subjects, 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).
[8] 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 who reside in rural areas.
[9] The number of tertiary grants in Figure 1 also includes two small programs administered by JUNAEB that use similar targeting criteria to provide housing subsidies to indigenous students in tertiary education.

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

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.[10] For example, the 2000 birth cohort first became eligible in grade 5 (the 2010 school year), such that $G_{2000}^5/N_{2000} = 5{,}178/33{,}124 = 0.156$.[11] 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 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.

[10] 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.

[11] 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.

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, 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.[12] 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 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 quantity and quality of public schools and health centers, and other 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 cohorts, respectively, relative to omitted cohorts in 1973–1974. We use event-study plots to

[12] 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.

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 defines three logical groups of post-treatment cohorts. Birth cohorts in 1975–1981 were exposed for fewer than 12 grades ($\bar{E} = 0.13$); cohorts in 1982–1991 were exposed for 12 grades, but during years of relatively lower grant availability ($\bar{E} = 0.65$); and cohorts in 1992–2000 were exposed for 12 grades during years of relatively 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).[13] 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 from (1).

B. Tests of Conditional Parallel Trends

The research 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 therefore test for differential pre-treatment trends in adult outcomes using the pre-1975 coefficients in equation (1). In addition, we show that putative treatment effects are not plausibly caused by differential post-treatment trends in variables correlated with adult

[13] 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).

outcomes, such as childhood socioeconomic status and school quality using data on children (see section III) born between 1985 and 2000. Cohorts before 1985 are unavailable, but the sample overlaps with a large increase in grant exposure from 0.5 to 2.4 years (Figure 1) and, as we will show, the ITT estimates from equation (1). Similar to equation (1), 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 other controls closely follow equation (1). The λ_e are the conditional mean differences between indigenous and non-indigenous children born in year *e*. We visually examine the λ_e for trends and formally test the null hypothesis of joint equality. We interpret rejected nulls as prima facie evidence of bias in ITT estimates from equation (1).

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 strongest in application years because higher GPAs increase the probability of grant receipt, all else equal (see Section I.A and JUNAEB, 2024). Students typically apply using prior-year GPAs from grades 4, 8, and 12, although applications are allowed in any grade.[14] We use national assessment data in grades 4, 8, and 10 from 1997–2015, spanning cohorts born from 1983–2000, and estimate

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

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, all interacted with student grade. Standard errors are clustered by commune.

[14] The first eligible primary grade is 5, which relies on the GPA in grade 4. The first eligible secondary grade is 9, which relies on the GPA in grade 8. Finally, tertiary applications rely on GPA received in the final secondary grade.

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 that rising grant exposure led to increases in student learning (conditional on passing earlier tests for differential trends in child and household variables). We carry out the same exercise for an indicator of whether parents expect their child to attain any tertiary schooling. In this setting, program-induced increases in achievement may also increase parent expectations for child attainment (Celhay and Gallegos, 2025), plausibly magnifying longer-run effects on test scores and schooling.[15]

Migration, Health, Fertility, and Partners. Using the event-study framework and adult survey outcomes, we test for effects on other potential channels of the earnings effect. 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 test score 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 (either composite test score or GPA, which Chilean schools calculate even in early primary grades) for child *j* of mother *i*. The specification mirrors equation (1), but further includes indicators of child gender, child birth years, and child-grade-by-survey-year cells. Beyond parent schooling, we examine additional channels for the effects of mothers' grant exposure on children's primary-grade learning, including children's household income, private school enrollments, school-based peer groups, and children's own receipt of indigenous grants.

[15] Chilean parents update beliefs about children's expected school attainment in response 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).

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). The CASEN waves consistently measure two variables necessary for estimation: 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 all other education grants; household income per capita; and household transfer income per capita, net of grants.[16] We also measure two categories of school variables. In

[16] 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

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.[17] 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) that primarily include children born from 2005–2008. Parent questionnaires collect respondents' ages, and 85 percent of respondents are mothers. We use ages to infer maternal birth cohorts.[18] We link SIMCE records, including a composite test score, to administrative datasets that include students' communes of residence, birth years, and grade point averages.[19] The estimation sample

at risk of dropping out. Transfer income derives from social assistance programs with heterogenous 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, Galasso, and Ginja, 2019).

[17] 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.

[18] We impute mother's birth year, 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.

[19] 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.

includes students with non-missing test scores and mothers' indigenous status, limited to mothers born between 1965 and 1991.[20] In this dataset, the 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 whose mothers have any tertiary education.

Second generation, observed as children and youth (SG2). We examine causal channels of test score effects using a pooled sample of children (ages 0–21) who reside with mothers in CASEN (i.e., the FG1 sample). Following the test score analysis, we also 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 contemporaneous 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, 2024; 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 $e^{\beta} - 1$ and confidence intervals using delta-method standard errors (Chen and Roth, 2024). There are no detectable pre-treatment trends, and post-treatment estimates increase with grant exposure. By 2000, the ITT

[20] 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.

estimate is approximately 30 percent, which potentially includes both changes to labor supply and changes to earnings per hour worked.

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 non-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 for both 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). In addition, the ACR estimates are robust to three alternate specifications.[21]

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 in tertiary grades.

The top-right panel of Figure 3 repeats the analysis for indicators of labor earnings in the last month, $1\{Y_i > x\}$. The first coefficient, when $x = 0$, is the ACR on earning *any* labor income.[22] 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 indicators of hours worked in the last week, $1\{Y_i > x\}$, 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, the standard full-time work week. The evidence suggests a

[21] 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 with fixed effects for indigenous adults in each combination of birth commune and survey year ($I_{icmt}\theta_{mt}$) (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).

[22] This is not perfectly collinear with the indicator of labor participation in the last week (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.

relatively large extensive-margin effect on the probability of full-time work in the full sample, especially for women (Appendix Figure A4).

V. Threats to Internal Validity

This section assesses four threats to the internal validity of ITT and ACR estimates. First, we report tests of conditional parallel trends, as described in sections II.B and III.A. Second, we provide evidence that our results are not explained by other concurrently implemented programs or by the endogenous misreporting of indigenous status. Third, we provide evidence that the "strong" parallel trends assumption is satisfied, necessary to interpret estimates of equation (2) as the average causal response to continuously measured grant exposure (Callaway et al., 2024). Fourth, we note that a secular decline in ethnic discrimination cannot explain the pattern of effects.

A. Tests for Conditional Parallel Trends

Figure 2 showed that adult outcomes do not exhibit pre-treatment trends, but this does not ensure that parallel trends hold for post-treatment cohorts. The concern is that increasing relative exposure to indigenous grants—within birth communes—is confounded with increasing relative exposure of indigenous children to household or school determinants of human capital and earnings. Figure 4 therefore reports the tests of differential post-treatment trends described in section II.B, based on equation (3) using the FG2 sample of children and youth.

The top-left panel plots estimated coefficients for a single variable that *should* exhibit differential post-treatment trends within birth communes: contemporaneous receipt of an indigenous grant by a child. For comparison, the dashed line shows a constrained estimate that imposes equal coefficients across birth cohorts. Indigenous children were increasingly likely to receive grants across birth years, and we reject the null of joint coefficient equality at 1%. This is consistent with the administrative data in Figure 1 that showed rising grant exposure across birth cohorts of indigenous children.

In contrast, the remaining panels show no evidence of differential post-treatment trends for child and household variables that could determine later human capital and earnings. These include the receipt of any other education grants, household income per capita (not including education grants), and households' transfer income per capita from all other sources, including

government programs. Further, Appendix Figure A5 shows no within-commune, post-treatment trends for other household and school-related variables, including migration; recent illness or accident; school tuition payments and private school enrollment (Hsieh and Urquiola, 2006); free school meals, full-day instruction, and other education reforms (Chay et al., 2005; McEwan, 2008b; Dominguez and Ruffini, 2023).[23] Collectively, the results lend support to the assumption of conditional parallel trends within birth communes.

B. Concurrent Programs and Misreported Indigenous Status

Except for *Beca Indígena,* no nationwide education or social programs targeted indigenous households. However, two government programs targeted modest numbers of indigenous households in some northern and southern communes. The first offered land subsidies to some indigenous households and communities, and the second introduced bilingual school programs in heavily indigenous communes.[24] Table 3 therefore reports ACR estimates in regional subsamples. In northern and southern regions, 26 percent of the sample is indigenous. In central regions, including Santiago, 5 percent are indigenous. Nevertheless, 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 cannot explain the main results.

An additional concern is that non-indigenous children (or their parents) might strategically misreport indigenous status in survey responses, with manipulation increasing across cohorts. This behavior is unlikely for two reasons. First, such behavior would plausibly generate differential post-treatment trends in child and household variables, but we found no evidence of this in section V.A. Second, there are no incentives to misreport indigenous status on household surveys, since it has no effect on grant eligibility, although it plausibly carries stigma in schools and labor markets (Saiz et al., 2008). For actual grant receipt, CONADI verifies indigenous

[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).

[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, 2017; 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.

status using birth certificates, which are difficult to manipulate. We also re-estimated equation (2) in a restricted sample of household surveys from 2013 to 2022.[25] Despite a relatively stable proportion of indigenous adults in these samples, the results are similar (Appendix Table A8, panel F).

C. Strong Parallel Trends

ITT estimates from equation (1) require conditional parallel trends within communes. In equation (2), the interpretation of τ as the average causal response (ACR) further requires that any two birth cohorts would have the same ITT effect when exposed to the same value of E_{irc}, the "strong" parallel trends assumption (Callaway et al., 2024). Two concerns arise.[26] First, later cohorts were exposed to additional public school investments, such as longer school days (Dominguez and Ruffini, 2023). If such inputs are complements to indigenous grants, then ACRs at higher values of E_{irc} would be overstated, increasing $\hat{\tau}$ by conflating greater grant exposure with an upward shift in the entire dose-response function (Callaway et al., 2024). Two results argue against this concern. Figure 2 shows that ACRs are approximately linear across E_{irc}, and ACR estimates are similar when later birth cohorts from 1992–2000 are excluded (Table A8, panel E).

Second, the characteristics of treated children contributing to ITT estimates may differ systematically across adjacent cohorts, in which case heterogeneous treatment effects would undermine the strong parallel trends assumption. We find no evidence of such differences. 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).

D. Secular Decline in Discrimination

[25] The proportion of indigenous adults has risen across CASEN waves, from 7% in the 2006 survey to 10% in 2022 (Appendix Table A1). A related explanation is that stigma of indigenous identity has diminished, leading fewer indigenous adults to misreport as non-indigenous.

[26] 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.

The program's expansion coincided with the return to democracy. Augusto Pinochet ceded power in 1990, and a 1993 Indigenous Law formally recognized indigenous rights, including *Beca Indígena* itself. The concern is that discrimination against indigenous Chileans steadily declined over subsequent decades, progressively reducing the penalties associated with indigenous identity in labor markets or in schools.

Three pieces of evidence suggest that a reduction in labor-market discrimination cannot substantially explain the earnings effects. First, a reduction in wage discrimination operates in calendar time: it would raise the earnings of indigenous adults from all birth cohorts working over the 2006–2024 period spanned by the pooled CASEN surveys in the FG1 sample. All waves include pre-treatment birth cohorts from 1965 to 1974. The absence of pre-treatment trends in earnings and labor supply (Figure 2) is difficult to reconcile with such a process. Second, higher earnings among indigenous parents would improve the economic circumstances of indigenous children's households across successive cohorts. Yet household income per capita shows no differential trends for indigenous children within birth communes (Figure 4). Third, a decline in discrimination should raise indigenous earnings at any given level of schooling. The earnings effect should therefore remain after conditioning on completed schooling, but little of it does.[27]

A decline in school-based discrimination would have to occur without a corresponding decline in labor-market discrimination, which the preceding evidence makes unlikely. Two further observations argue against it. First, we find no relative improvement in features of indigenous children's schools that falling discrimination would plausibly affect, such as private-school enrollment (Appendix Figure A5). Second, descriptive evidence consistently shows that discriminatory attitudes persisted in Chilean schools and society throughout this period. Non-indigenous Chileans' stereotypes of Mapuches were unchanged between 1988 and 2006; teachers held prejudiced views of Mapuche students two decades after the return to democracy; and implicit-attitude measures suggest larger bias against Mapuches than explicit survey

[27] We augment equation (2) with fixed effects for cells defined by the interaction of birth cohort, birth commune, and each year of completed schooling. The unconditional ACR is 10 percent per expected grant year. Conditional on schooling, the ACR falls to a statistically insignificant 1.6 percent (Appendix Table A8, panel G). This is consistent with the modest contribution of cognitive skills documented in Section VI.C, but not with a decline in labor market discrimination large enough to generate the unconditional estimates.

measures.[28] We cannot separate the school-based channel from the program as cleanly as the labor-market channel, but the available evidence points against it.

VI. Causal Channels of Long-Run Effects

This section examines causal channels of long-run effects on schooling and labor outcomes. 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 the magnitude of shorter-run human capital gains can plausibly account for the 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). Labels indicate the grade level (4, 8, or 10) of each coefficient estimate; not all grade levels take the test in each year. Test score data are first available for students born in 1983 and 1984. For these 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 reject the null at 1% that coefficients (λ_{eh}) are jointly equal. Increasing indigenous test scores are also evident in subsamples defined by gender and region (Appendix Figure A7).

A similar pattern is evident for all grades, including grade 4, a year in which students were not eligible for grants but subject to merit incentives based on grade point averages. GPAs in grade 4 partially determine eligibility for fifth-grade grant-receipt (Section II.C), and evidence in Figure 5 suggests that primary-school students and their families increase effort in response to merit-based scholarships (Barrera-Osorio and Filmer, 2016; Kremer, Miguel, and Thornton, 2009).

[28] Qualitative studies in southern Chilean high schools and urban and rural schools document persistent verbal, behavioral, and institutional discrimination against Mapuche students, originating in teachers' beliefs about intellectual and cultural deficits and low expectations (Becerra et al., 2015; Webb, 2022; Webb and Radcliffe, 2016). Non-indigenous university students both maintained and added unfavorable Mapuche stereotypes between 1988 and 2006 (Saiz et al., 2008). Finally, even non-indigenous Chileans who expressed no explicit bias against Mapuches on direct attitude measures implicitly devalued them on the Implicit Association Test (Haye et al., 2010).

Panel B reports similar estimates for an indicator that parents expect their child to enroll in tertiary education. For the earliest cohorts, parents 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.

We interpret these patterns as reflecting grant-induced convergence for three reasons. First, we find 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 parent questionnaire. Second, Section V showed no evidence of differential post-treatment trends in many other child and household variables, with the sole exception of indigenous grants. Third, time-varying differential selection by ethnicity into the school-based SIMCE sample cannot account for conditional convergence (Appendix Figure A8). Enrollment at primary ages is near-universal for indigenous and non-indigenous children across these cohorts; the 4th- and 8th-grade test-taking populations are essentially unselected. At secondary ages, indigenous enrollment rose across cohorts relative to non-indigenous children, shifting the composition of 10th-grade test-takers toward marginal students, which, if anything, biases those estimates toward zero.

The implied difference-in-differences suggests that indigenous test scores rose by at least 0.12 standard deviations and tertiary expectations 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 for whom we do not have test score data or expectations data.

B. Migration, Health, Fertility, and Partners

We find no evidence that grant exposure affected migration away from birth communes, and little evidence that it affected health (Table 4, columns 1–3). The adult health grade marginally increases and the likelihood of a recent illness marginally decreases for the later cohorts, but this is not corroborated by ACR estimates. 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 A9).

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-schooling effects. This reflects compositional changes 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 return of 32 percent $\left(\frac{10}{0.31}\right)$, because wage returns to years of schooling are not the only causal mechanism. 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 supply 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 an auxiliary dataset of Chilean women that includes detailed measures of labor earnings, schooling, and both cognitive and non-cognitive skills.[29] One year of schooling increases labor force participation by 3.2 percentage points, all else equal, 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—including zeroes for non-working adults—and therefore encompasses earnings effects due to increased labor supply.

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

[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 year is 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 (Appendix Table A10).

standard deviations, implying an increase in labor force participation of $\underbrace{(0.348 \times 0.032)}_{Years\ of\ school} +$ $\underbrace{(0.05 \times 0.030)}_{Cognitive\ skills} = 0.013$.[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 (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 human capital.

VII. Intergenerational Effects

This section reports estimates of maternal exposure to indigenous grants on their 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, passing through all but one of the ITT confidence intervals.

[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.

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

We rule out two main threats to internal validity: sample selection and correlated treatments. First, the results are not plausibly explained by sample selection into childbearing or child co-residence. In the FG1 sample, we find small and statistically insignificant effects on the number of children ever born (Appendix Table A11, column 1) and on the number of co-resident children under the age of 22 (column 2). Neither does grant exposure affect the probability of having a child before the age of 18 (Table 4, column 4).

Second, the estimates are not measuring the direct (rather than intergenerational) effects of indigenous grants. Primary-aged children in the SG1 sample were mostly born from 2005–2008. In these cohorts, primary-grade exposure to indigenous grants was stable, having plateaued in 2004.[32] Differential changes in child outcomes were therefore caused by rising parent exposure. More importantly, the rising incomes of indigenous women in treated cohorts imply that their own children were *less* likely to receive grants because households were less likely to pass the proxy-means test for grant eligibility. We explicitly test this below.

B. Causal Channels

Intergenerational effects on child learning could arise through household or school channels. Higher maternal schooling and cognitive skill may directly raise children's achievement. Higher household income—from mothers' own earnings and from the increased schooling of their partners—may finance educational investments within households. And higher income, together with parent schooling, may affect choices regarding children's schools. We assess these channels using the school-based SG1 sample (Table 5) and the household SG2 sample (Table 6). We report ITT and ACR estimates, but focus on the latter because they are estimated more precisely, particularly in the survey-based SG2 sample.[33]

In the SG1 sample, maternal exposure reduced enrollment in municipal and, to a lesser extent, private subsidized schools, and substantially raised enrollment in private tuition-charging

[32] See Appendix Figure A10, which extends Figure 1 (panel B) using program data until 2024.

[33] Small ITT estimates are harder to detect in survey-based samples, particularly for the 1975–81 cohort, whose low exposure (0.13 expected grant years) implies smaller effects. This precision gap also reconciles an apparent tension between the first-generation and intergenerational results. The 1975–81 cohort shows small, insignificant first-generation effects on women's schooling (0.027; SE 0.071; Appendix Table A7, panel A, column 1) yet significant effects on children's test scores (0.035; SE 0.009; Table 5, panel A, column 1). The first-generation ITT estimate is too imprecise to detect the small schooling gain of roughly 0.05 years implied by the women's ACR, whereas the population SIMCE data detect the smaller corresponding learning effect.

schools (Table 5, panel B, columns 3–5).[34] In pre-treatment cohorts, children of indigenous mothers were 13 percentage points less likely to attend private tuition schools, which charge higher fees, enroll higher-income students, and select applicants through interviews and examinations (McEwan, Urquiola, and Vegas, 2008). Each expected grant year narrowed this gap by 8 percentage points (column 5). Exposure marginally changed the share of indigenous classmates (column 6), but each expected grant year raised the share of classmates with tertiary-educated mothers by 8 percentage points, against a pre-treatment indigenous mean of 27 percent (column 7).

Maternal exposure also increased net household resources.[35] Using the SG2 sample of children co-resident with mothers in the FG1 sample, we find no effect on whether children live with fathers (Table 6, panel B, column 1) or on migration from mothers' birth communes (column 2). But household income per capita rose by 13 percent per expected grant year (column 3). This exceeds the ACR for women's own labor earnings of 8.5 percent (Appendix Table A7, column 3), consistent with the increased schooling of co-resident partners (Appendix Table A9, column 6). In contrast, household transfer income per capita fell by 7 percent per expected grant year (Table 6, column 4), likely reflecting reduced eligibility for means-tested transfers as incomes rose.

The same rising incomes reduced children's own grant receipt. In pre-treatment cohorts, 13 percent of children of indigenous mothers received an indigenous grant; each expected maternal grant year lowered this share by 3 percentage points (Table 6, panel B, column 5), with no offsetting effect on other education grants (column 6). Because grant allocation depends on a proxy-means test, the higher incomes of treated mothers plausibly moved households above the eligibility threshold or reduced the probability that eligible applicants were selected. The intergenerational learning effects therefore cannot be attributed to children's own program receipt, which fell rather than rose with maternal exposure.

[34] Appendix Figure A11 (panel A) reports corresponding event study plots. There is evidence of downward pre-treatment trends for several variables, implying that the results are conservative. For example, a downward pre-treatment trend for private tuition schools implies even larger effects on the probability of enrolling. 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 less likely to pay any tuition or to receive free school meals (which are offered in publicly funded schools).

[35] Appendix Figure A11 (panel B) reports event study plots in the SG2 sample. 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 of ITT effects that rise (or fall) in relation to maternal grant exposure.

Taken together, these channels describe a broad shift in the socioeconomic environment of young children: more educated and higher-earning women (with more educated partners), higher household incomes, and more selective schools and peers. Our design identifies their combined contribution to children's learning rather than the separate effect of any one.

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 ≈ 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.[36] Increased schooling attainment causes increased school expenditures of USD 1,480 per eligible child.[37] These are offset by a fiscal externality, due to increased revenues from a 19 percent consumption tax.[38] 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.[39] The MVPF is 6 even when halving the ITT earnings estimate for these cohorts (from 32 to 16 percent).

IX. Conclusions

[36] 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).
[37] 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.
[38] 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.
[39] 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.

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 across both urban and rural areas of Chile, 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.

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 means-tested transfers.

Taken together, the findings demonstrate that a nationwide CCT program can meaningfully increase human capital for first-generation child recipients and their children, reducing 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). In this respect, the program's merit conditions distinguish it from enrollment-conditioned CCTs, which have generally raised attainment without commensurate learning gains, and align

with experimental evidence that performance-linked scholarships raise achievement where cash alone does not (Barrera-Osorio and Filmer, 2016). 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		
Panel A: First-generation samples					
FG1	1965–2000	22–57	—	CASEN	2006–2024
FG2	1985–2000	10–21	—	CASEN	2006–2020
FG3	1983–2000	Primary school and and secondary school aged	—	SIMCE (grade 4) SIMCE (grade 8) SIMCE (grade 10)	1999–2009 1997–2013 1998–2015
Panel B: Second-generation samples					
SG1	1965–1991	22–50	Primary school aged	SIMCE (grade 2) SIMCE (grade 4) Administrative GPA 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 GPA data is *rendimiento académico* (https://datosabiertos.mineduc.cl/rendimiento-por-estudiante-2/). Section III provides additional 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)
Panel A: Intent-to-treat				
Indigenous, born 1975–1981	0.039	0.020	0.011*	0.016
	(0.053)	(0.018)	(0.005)	(0.010)
Indigenous, born 1982–1991	0.321**	0.078**	0.015*	0.033*
	(0.067)	(0.024)	(0.006)	(0.013)
Indigenous, born 1992–2000	0.565**	0.198**	0.038**	0.078**
	(0.090)	(0.042)	(0.011)	(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
Panel B: Average causal response				
Expected grant years	0.309**	0.099**	0.018*	0.041**
	(0.046)	(0.025)	(0.007)	(0.014)
Means: adults born 1965–1974				
$\bar{Y}_{IND}$	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 by regional subsamples

	Years of schooling		Labor earnings (%)		Worked last week		Weekly hours worked (%)	
	Central regions	N and S regions	Central regions	N and S regions	Central regions	N and S regions	Central regions	N and S regions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Expected grant years	0.314**	0.309**	0.106**	0.092**	0.020*	0.018*	0.035	0.042*
	(0.063)	(0.050)	(0.033)	(0.025)	(0.009)	(0.008)	(0.019)	(0.016)
Means: adults born 1965-1974								
$\bar{Y}_{IND}$	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)
Panel A: Intent-to-treat							
Indigenous, born 1975-1981	0.008	0.010	-0.004	0.009	0.006	-0.031**	0.064
	(0.005)	(0.016)	(0.004)	(0.006)	(0.006)	(0.007)	(0.054)
Indigenous, born 1982-1991	0.000	0.032*	-0.005	0.002	0.018**	-0.093**	0.374**
	(0.006)	(0.014)	(0.004)	(0.006)	(0.005)	(0.008)	(0.075)
Indigenous, born 1992-2000	0.001	—	-0.011*	-0.011	0.022**	-0.139**	0.671**
	(0.008)		(0.005)	(0.007)	(0.006)	(0.014)	(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
Panel B: Average causal response							
Expected grant years	0.001	0.029	-0.002	-0.006*	0.009**	-0.086**	0.408**
	(0.004)	(0.015)	(0.003)	(0.003)	(0.003)	(0.008)	(0.063)
Means: adults born 1965-1974							
$\bar{Y}_{IND}$	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**	0.041**	-0.007	-0.016**	0.024**	-0.001	0.015**
	(0.009)	(0.009)	(0.005)	(0.005)	(0.004)	(0.002)	(0.004)
Indigenous mother, born 1982–91	0.094**	0.109**	-0.039**	-0.022**	0.062**	-0.014**	0.059**
	(0.010)	(0.013)	(0.006)	(0.007)	(0.008)	(0.004)	(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**	0.183**	-0.062**	-0.016	0.079**	-0.009	0.084**
	(0.014)	(0.017)	(0.008)	(0.011)	(0.010)	(0.009)	(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 regressions 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.

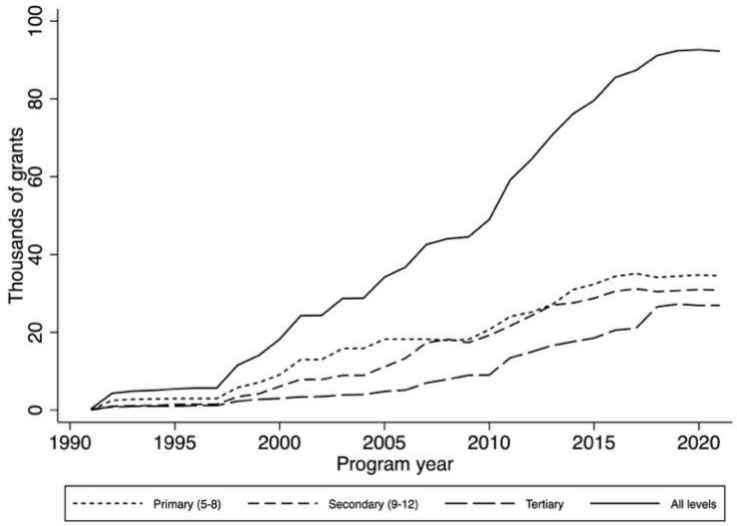
Table 6: Intergenerational effects of maternal exposure to indigenous grants (SG2 sample)

	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	0.005	0.003	0.011	-0.003	0.005
	(0.010)	(0.008)	(0.023)	(0.021)	(0.005)	(0.003)
Indigenous mother, born 1982–91	-0.001	0.010	0.096**	-0.052*	-0.025**	0.007*
	(0.009)	(0.011)	(0.024)	(0.024)	(0.006)	(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	0.014	0.128**	-0.066*	-0.032**	0.008
	(0.011)	(0.014)	(0.027)	(0.027)	(0.009)	(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

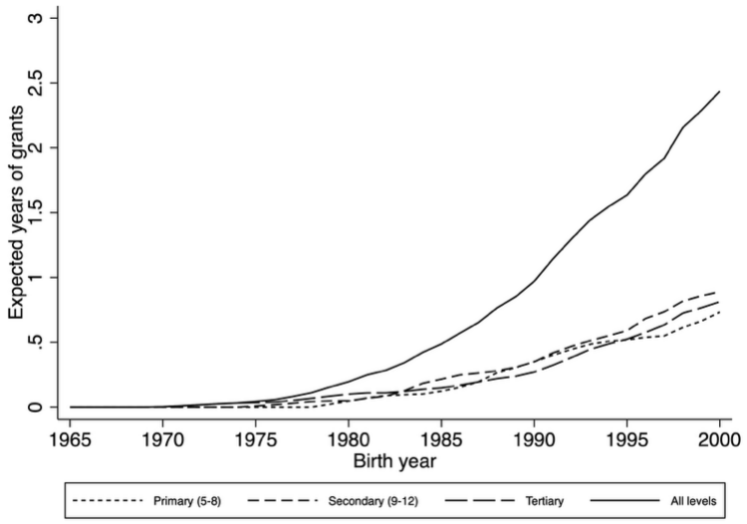
Notes: All regressions use the SG2 sample (Table 1). Standard errors are multi-way clustered by commune of mother's birth and commune of residence. ** (*) indicates statistical significance at 1% (5%). Panel A: Coefficient estimates correspond to dummy variables for indigenous mothers in the specified 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

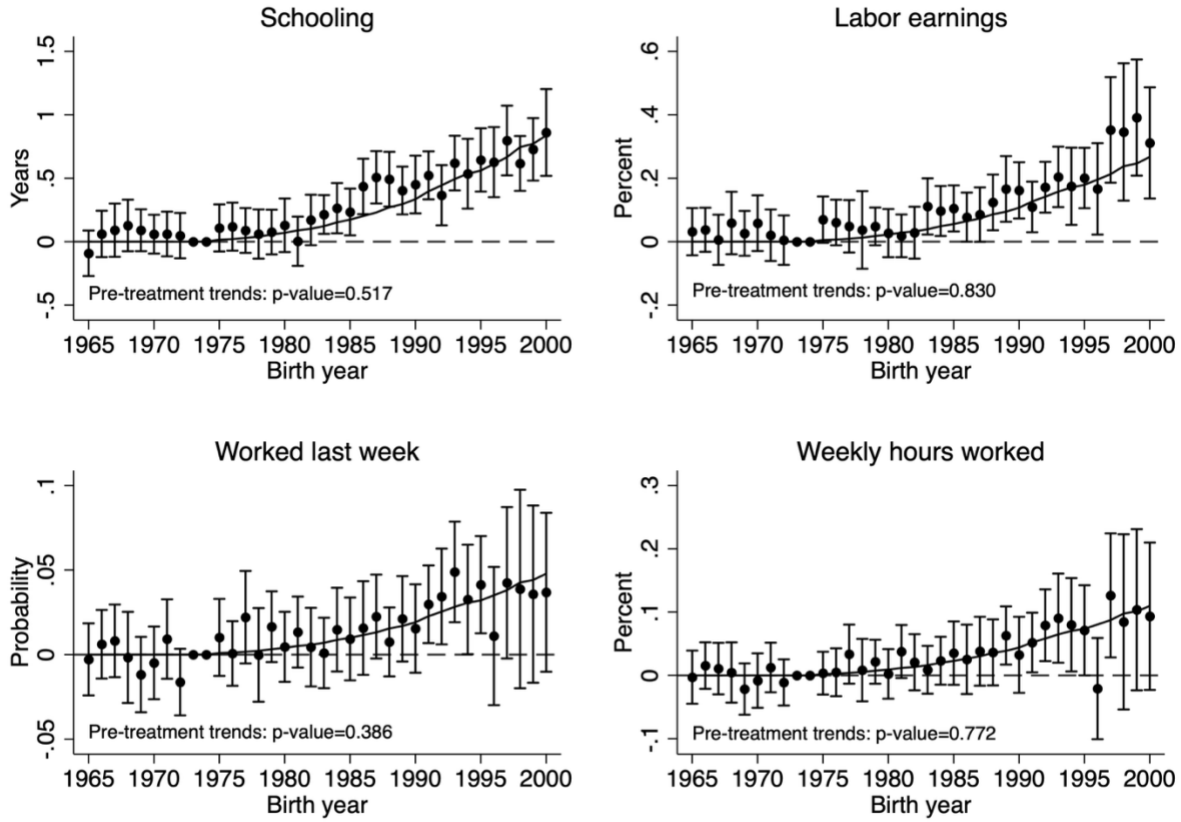


Panel B: Expected years of grants, by birth year



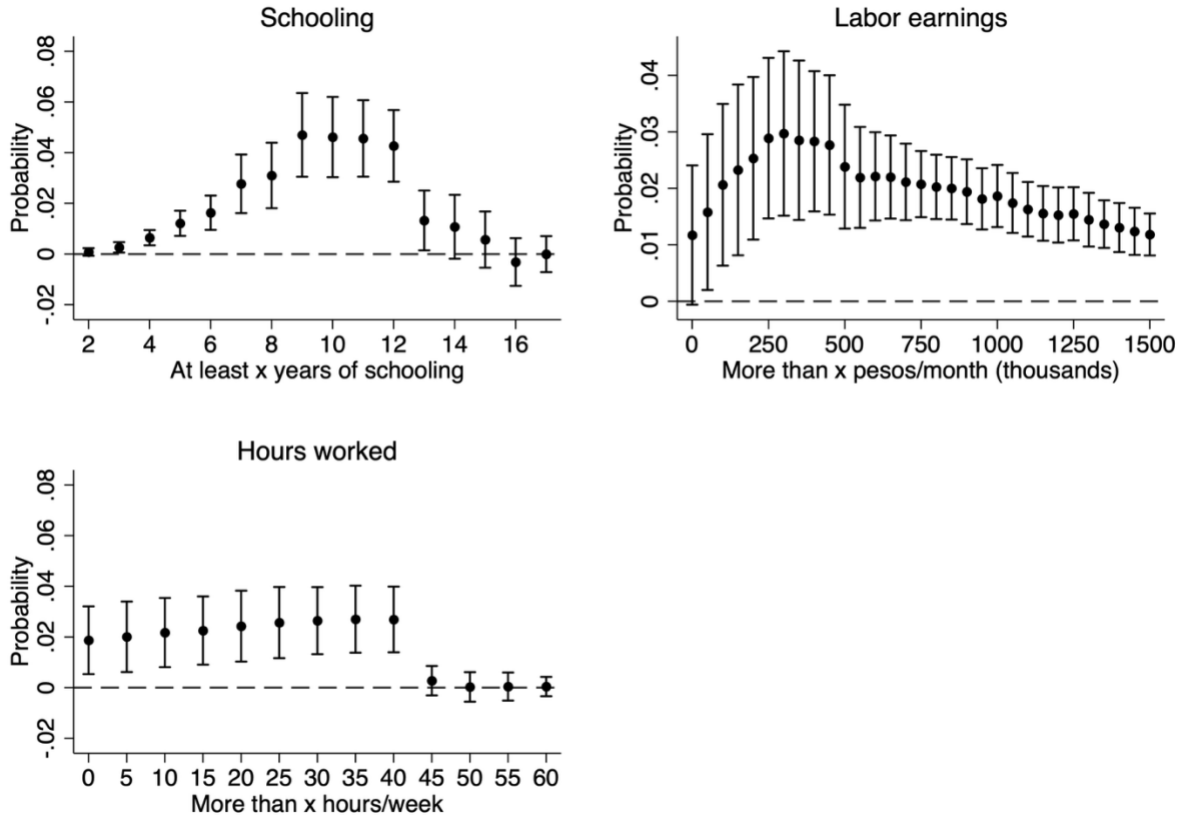
Notes: Panel A: 1991–2005 data from Ministry of Education decrees; 2006–2021 data from annual reports of JUNAEB to DIPRES, a supervisory unit of the Ministry of Finance. Panel B: The sum of grade-specific probabilities of receiving a grant for each birth cohort (Appendix Table A2).

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



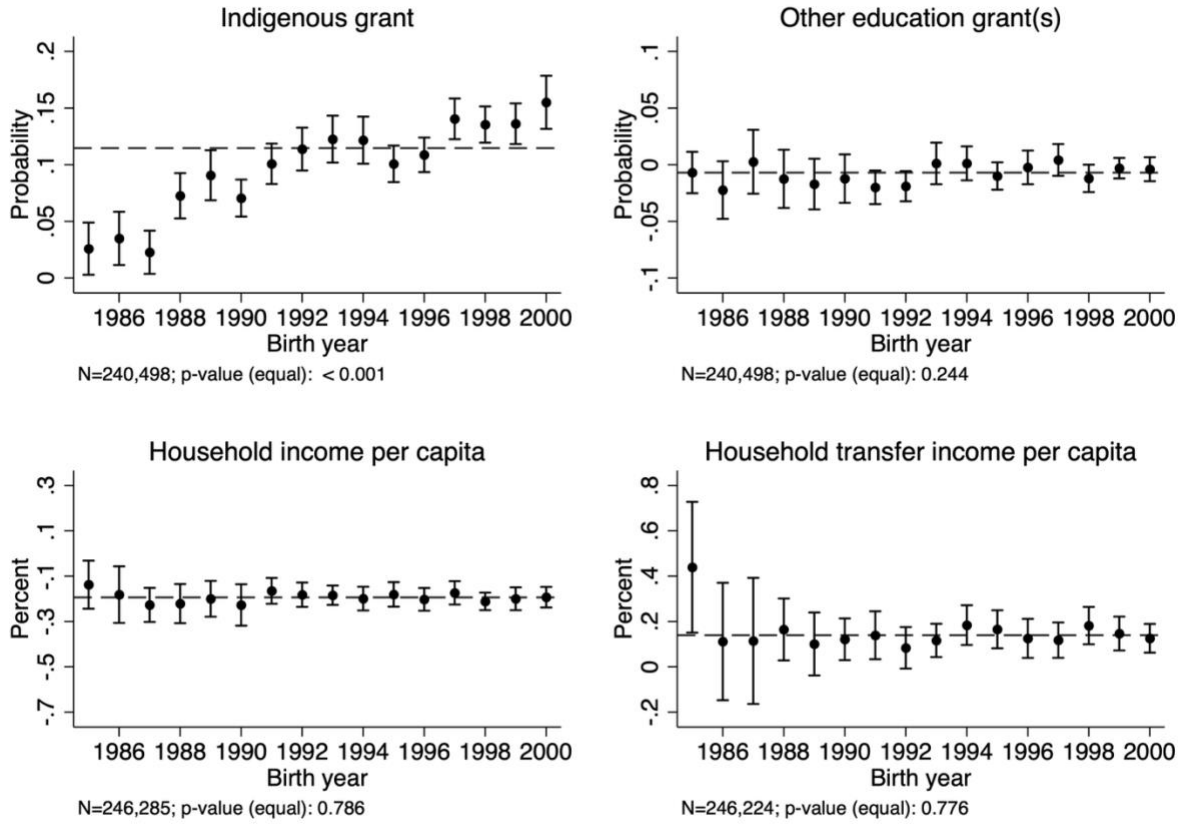
Notes: All panels use the FG1 sample (Table 1). Standard errors are multi-way clustered by commune of birth and commune of residence. Sample sizes in each panel are the same as regressions in Table 2. Each p-value corresponds to the null that $\gamma_e = 0$ for all e. Solid lines are the mean prediction 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.

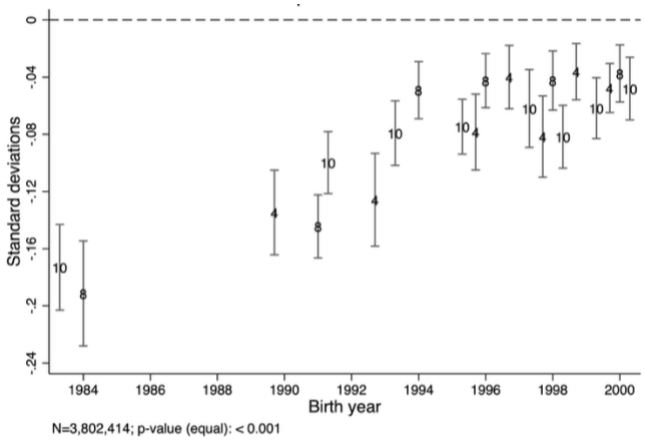
Figure 4: Tests for differential post-treatment trends in grants and income



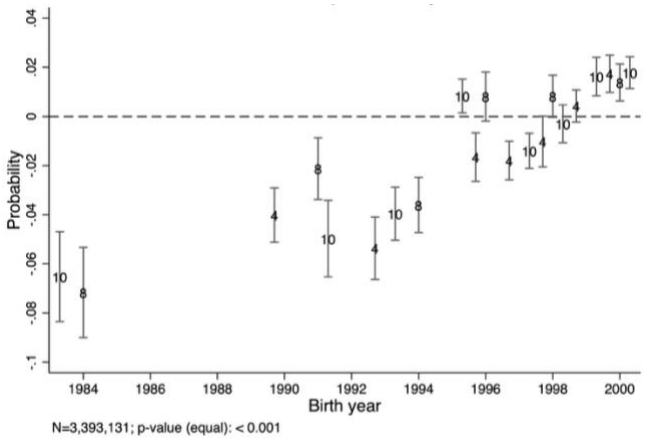
Notes: All panels use the FG2 sample (Table 1). Standard errors are multi-way clustered by commune of birth and commune of residence. The circles are estimates of λ_e from equation (3), with pointwise 95% confidence intervals. Each p-value corresponds to the null that coefficients are jointly equal. 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



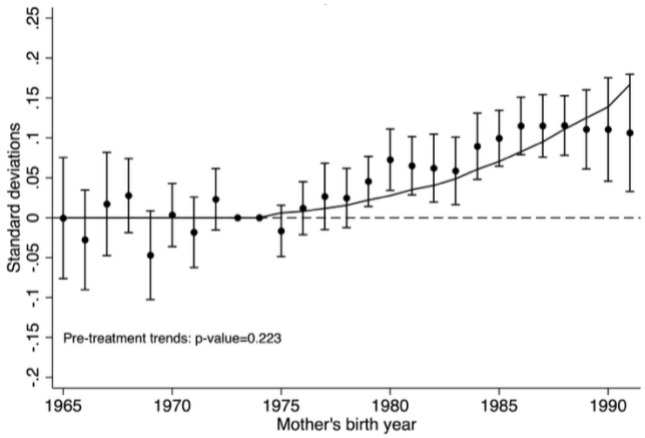
Panel B: Parent expectations for tertiary education



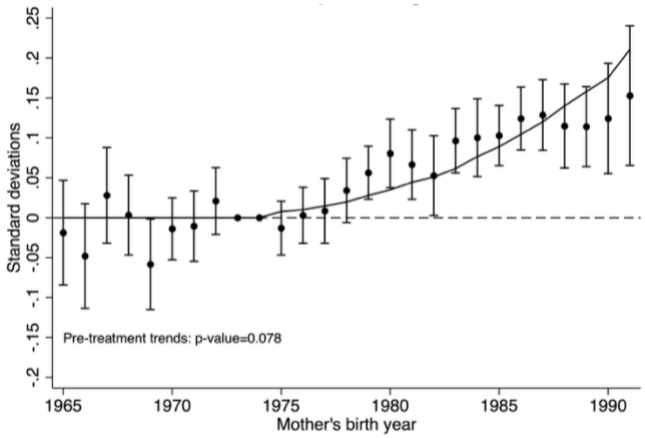
Notes: All panels use the FG3 sample (Table 1). The labeled points are estimates of λ_{eh} for each grade from equation (4), with pointwise 95% confidence intervals. Standard errors are clustered by school communes. Each p-value corresponds to the null that coefficients are jointly equal.

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

Notes: Each probability is $\frac{G_c^g}{N_c}$ (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 Joaquin, Pedro Aguirre Cerda[c]	1981	DFL 1-3260
La Granja	La Pintana, San Ramón	1981	DFL 1-3260
Santiago	Independencia[c]	1981	DFL 1-3260
La Cisterna	El Bosque[c], Lo Espejo[c]	1981	DFL 1-3260
Quinta Normal	Estación Central[c]	1981	DFL 1-3260
Maipu	Cerrillos[c]	1981	DFL 1-3260
Pudahuel	Lo Prado[c], Cerro Navia[c]	1981	DFL 1-3260

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

Table A4: Regional weights used to compute expected grant years (E_{irc})

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	659,045	11.07	91,297
	(3.70)		(3.77)	
Labor earnings (thousands of 2022 CLP)	472.5	649,305	361.8	89,593
	(879.2)		(552.8)	
Worked last week	0.680	661,388	0.651	91,564
Weekly hours worked	29.40	593,500	27.82	81,416
	(23.06)		(23.43)	
Migrated from birth commune	0.382	661,388	0.337	91,564
Adult health (z-score)	0.009	290,726	-0.070	38,685
	(0.998)		(1.013)	
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)
Panel A: Ages 10–21						
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
Panel B: Ages 10–15						
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
Panel C: Ages 10–13						
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)
Panel A: Intent-to-treat								
Indigenous, born 1975-1981	0.027	0.042	-0.019	0.046*	0.003	0.023**	0.002	0.030**
	(0.071)	(0.059)	(0.024)	(0.022)	(0.008)	(0.006)	(0.018)	(0.010)
Indigenous, born 1982-1991	0.343**	0.284**	0.047	0.094**	0.007	0.022**	0.021	0.041**
	(0.080)	(0.073)	(0.032)	(0.025)	(0.008)	(0.007)	(0.020)	(0.011)
Indigenous, born 1992-2000	0.664**	0.442**	0.147**	0.222**	0.031*	0.043**	0.075*	0.081**
	(0.101)	(0.103)	(0.046)	(0.047)	(0.012)	(0.013)	(0.036)	(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
Panel B: Average causal response								
Expected grant years	0.348**	0.264**	0.085**	0.104**	0.016*	0.018*	0.042*	0.040**
	(0.051)	(0.052)	(0.025)	(0.028)	(0.007)	(0.007)	(0.020)	(0.013)
Means: adults born 1965-1974								
$\bar{Y}_{IND}$	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)
Panel A: Baseline (Table 2)				
Expected grant years	0.309**	0.099**	0.018*	0.041**
	(0.046)	(0.025)	(0.007)	(0.014)
N	750,134	738,531	752,744	674,500
Panel B: Additional fixed effects				
Expected grant years	0.305**	0.114**	0.021*	0.050**
	(0.050)	(0.028)	(0.008)	(0.017)
N	741,252	726,417	743,913	664,190
Panel C: Survey weights				
Expected grant years	0.356**	0.126**	0.020**	0.042**
	(0.062)	(0.028)	(0.005)	(0.011)
N	750,070	738,470	752,680	674,438
Panel D: Drop assumption that $E_{irc} = 0$ for 1970–1974 cohorts				
Expected grant years	0.310**	0.099**	0.018*	0.041**
	(0.046)	(0.025)	(0.007)	(0.014)
N				
Panel E: Exclude 1992–2000 birth cohorts				
Expected grant years	0.418**	0.100**	0.022*	0.048**
	(0.075)	(0.033)	(0.009)	(0.018)
N	664,393	654,727	666,606	606,677
Panel F: Drop CASEN surveys before 2013				
Expected grant years	0.287**	0.100**	0.016*	0.034*
	(0.048)	(0.023)	(0.007)	(0.014)
N	509,373	501,866	511,784	435,041
Panel G: Fixed effects for years of schooling				
Expected grant years	—	0.016	0.001	0.007
		(0.017)	(0.005)	(0.011)
N		686,147	709,529	621,961

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 of samples and specifications.

Table A9: Effects on migration, health, and partner attributes (by gender)

	Migrated from birth commune	Adult health grade	Recent illness or accident	Co-resident partner	Indigenous partner	Co-resident partner's schooling
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intent-to-treat (women)						
Indigenous, born 1975-1981	0.001	0.022	-0.013*	-0.000	-0.027**	0.039
	(0.007)	(0.023)	(0.006)	(0.008)	(0.007)	(0.067)
Indigenous, born 1982-1991	-0.001	0.051**	-0.008	0.009	-0.077**	0.340**
	(0.007)	(0.019)	(0.005)	(0.007)	(0.008)	(0.093)
Indigenous, born 1992-2000	-0.006	—	-0.016**	0.007	-0.113**	0.794**
	(0.009)		(0.006)	(0.008)	(0.014)	(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
Panel B: Average causal response (women)						
Expected grant years	-0.002	0.054**	-0.004	0.004	-0.072**	0.427**
	(0.004)	(0.019)	(0.003)	(0.003)	(0.008)	(0.071)
Means: female adults born 1965-1974						
$\bar{Y}_{IND}$	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
Panel C: Intent-to-treat (men)						
Indigenous, born 1975-1981	0.017*	-0.002	0.007	0.012	-0.034**	0.107
	(0.007)	(0.022)	(0.005)	(0.007)	(0.009)	(0.070)
Indigenous, born 1982-1991	0.002	0.013	-0.002	0.023*	-0.106**	0.465**
	(0.008)	(0.016)	(0.005)	(0.009)	(0.009)	(0.086)
Indigenous, born 1992-2000	0.009	—	-0.006	0.030**	-0.160**	0.559**
	(0.011)		(0.008)	(0.011)	(0.019)	(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
Panel D: Average causal response (men)						
Expected grant years	0.004	0.011	-0.001	0.012	-0.100**	0.425**
	(0.005)	(0.019)	(0.004)	(0.006)	(0.010)	(0.076)
Means: male adults born 1965-1974						
$\bar{Y}_{IND}$	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**	0.219**
	(0.001)	(0.007)
Cognitive skills (s.d.)	0.030**	0.236**
	(0.005)	(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)
Panel A: Intent-to-treat		
Indigenous, born 1975–1981	0.017	0.014
	(0.025)	(0.019)
Indigenous, born 1982–1991	-0.008	0.041*
	(0.023)	(0.018)
p-value: equal	0.173	0.082
p-value: zero	0.393	0.051
Panel B: Average causal response		
Expected grant years	-0.017	0.033
	(0.021)	(0.019)
Means: female adults born 1965-1974		
$\bar{Y}_{IND}$	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

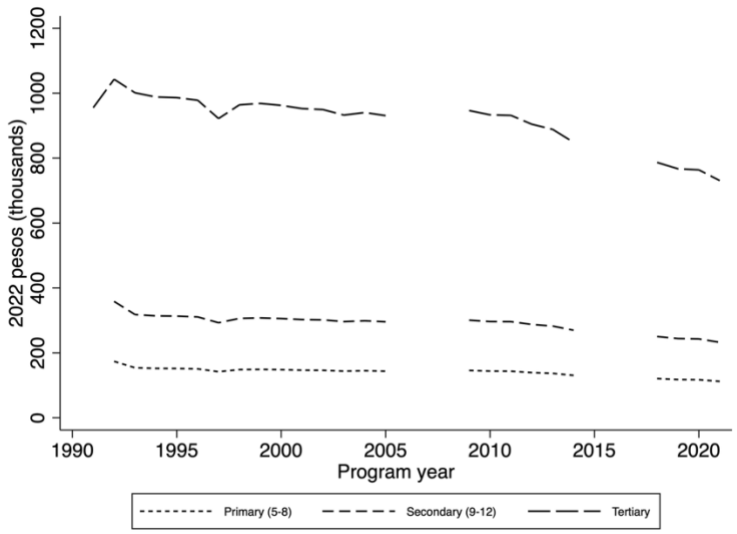
Notes: Standard errors are multi-way clustered by commune of birth and commune of residence. ** (*) indicates statistical significance at 1% (5%). Panel A: The coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls 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)
Panel A: Intent-to-treat						
Indigenous mother, born 1975-1981	-0.021*	0.016	0.006*	0.023**	-0.013	-0.010
	(0.010)	(0.010)	(0.003)	(0.008)	(0.010)	(0.009)
Indigenous mother, born 1982-1991	-0.039**	0.016	0.023**	0.040**	-0.059**	-0.017
	(0.012)	(0.012)	(0.006)	(0.009)	(0.012)	(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
Panel B: Average causal response						
Expected grant years	-0.026	0.001	0.026**	0.039**	-0.073**	-0.008
	(0.014)	(0.014)	(0.005)	(0.012)	(0.014)	(0.012)
Means: adults born 1965-1974						
$\bar{Y}i$	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

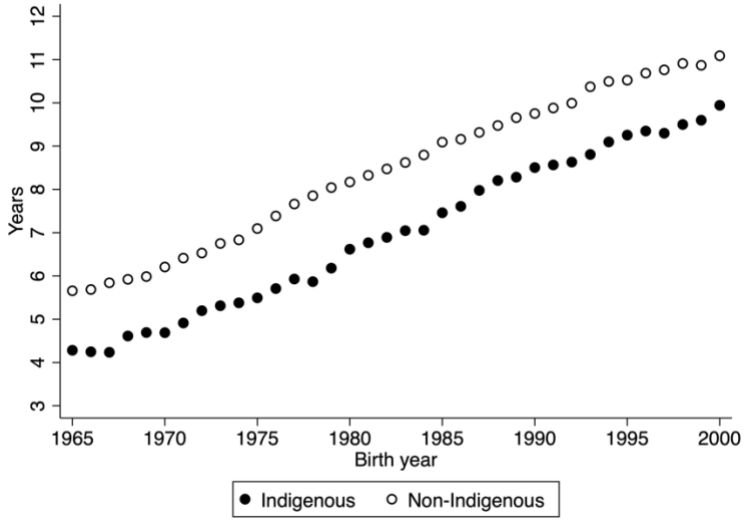
Notes: Standard errors are adjusted multi-way clustered by commune of mother's birth and commune of residence. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous mothers in the specified birth years. Panel B: The coefficient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



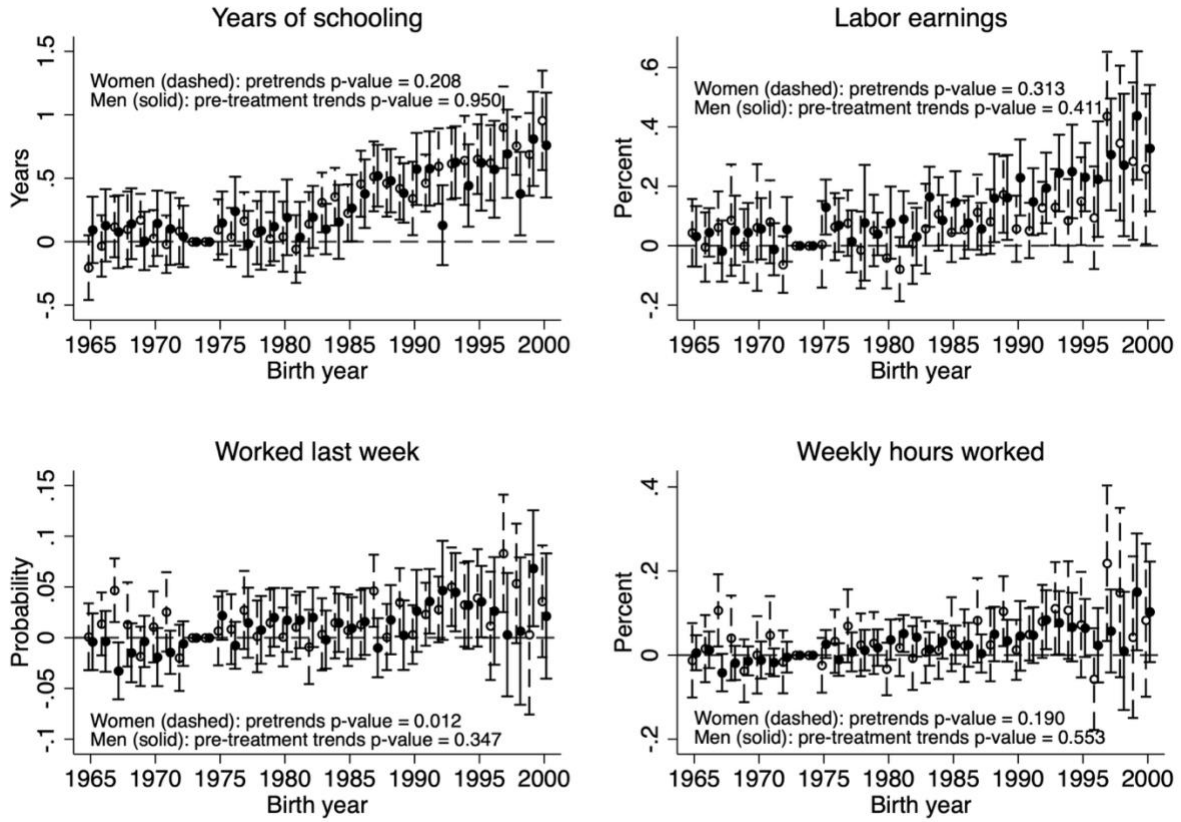
Notes: Each line plots the annual value (in 2022 CLP) of primary, secondary, or tertiary indigenous grants. Missing segments indicate years with missing data in the sources described in Figure 1.

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 (solid) and non-indigenous (outline) adults.

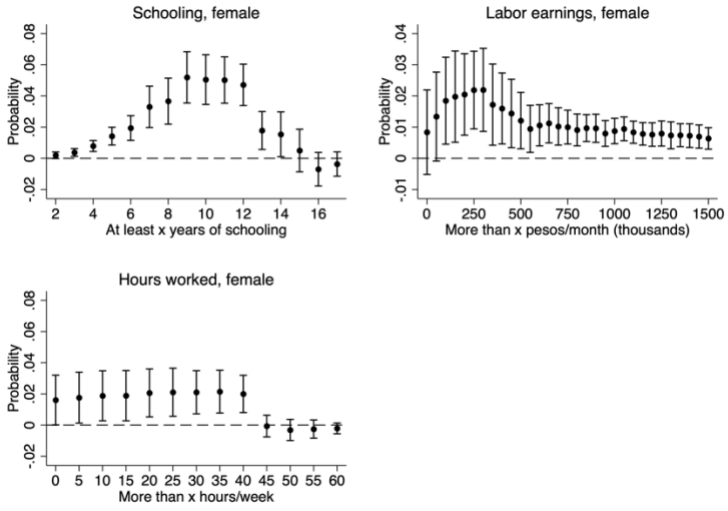
Figure A3: Event study plots for schooling, labor earnings, and labor supply by gender



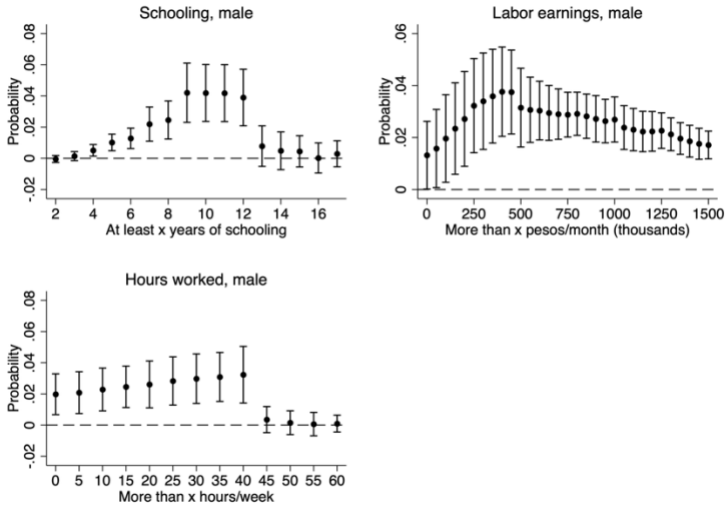
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Samples in each panel are the same as regressions in Table A7. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$. Left-hand panels: The circles are $\widehat{\gamma}_e$ and $\widehat{\lambda}_e$ 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 gender

Panel A: Female sample

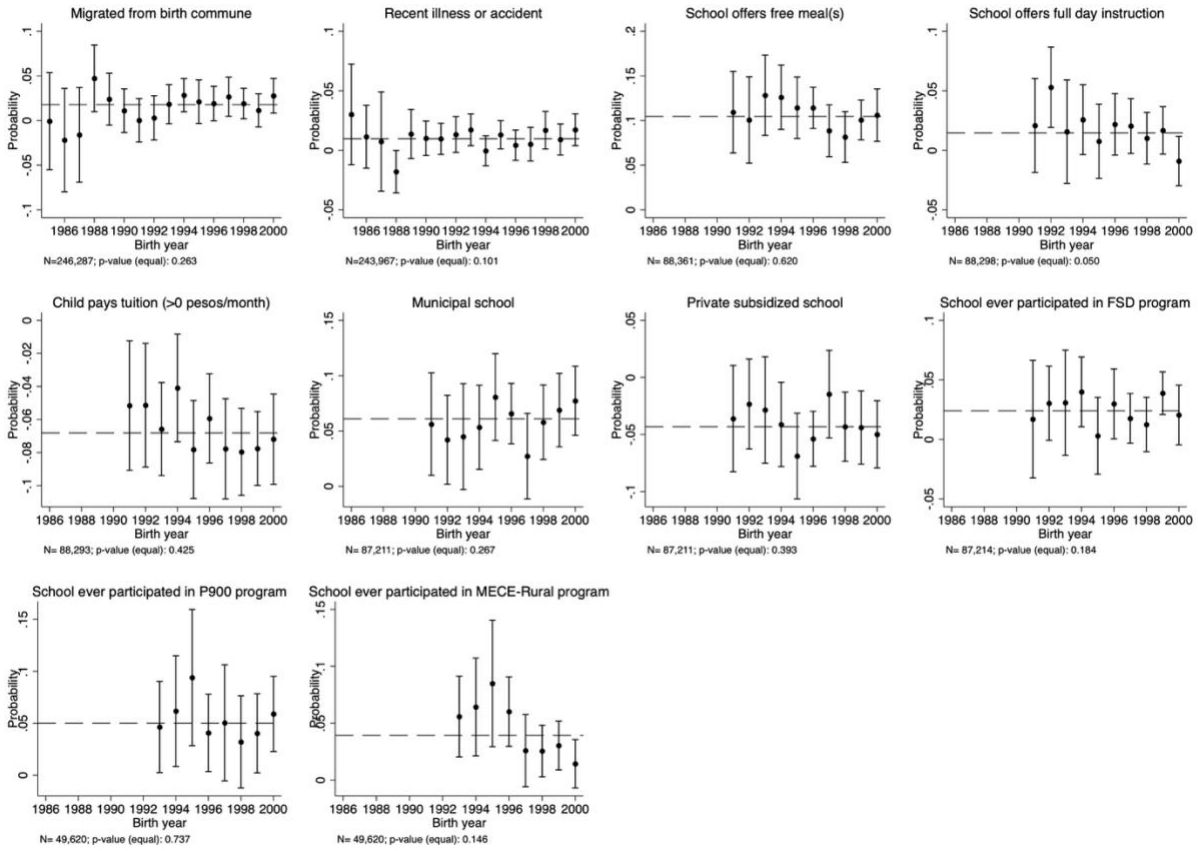


Panel B: Male sample



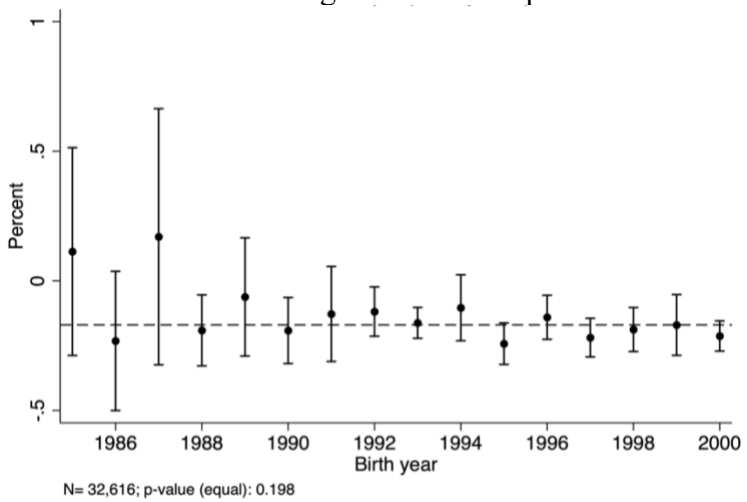
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions in Appendix Table A7. Each point and confidence interval corresponds to a $\hat{\tau}$ from equation (2). Top-left figures: Each 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 figures: 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 $\widehat{\lambda}_e$ 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.

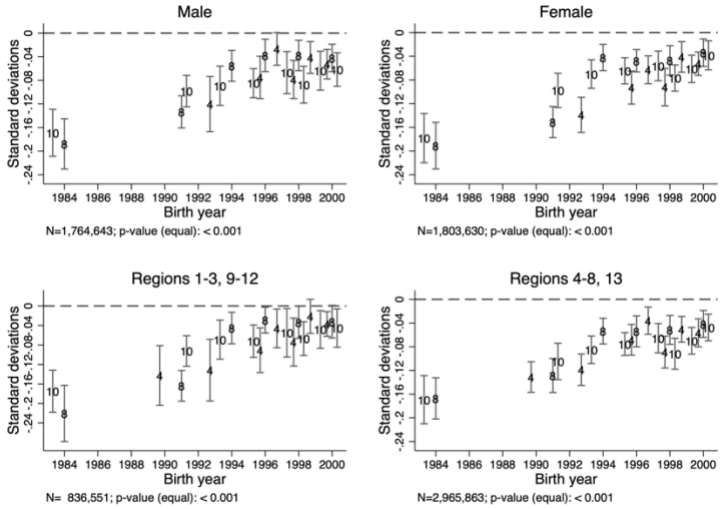
Figure A6: Post-treatment trends in household income of indigenous grant recipients, relative to indigenous non-recipients



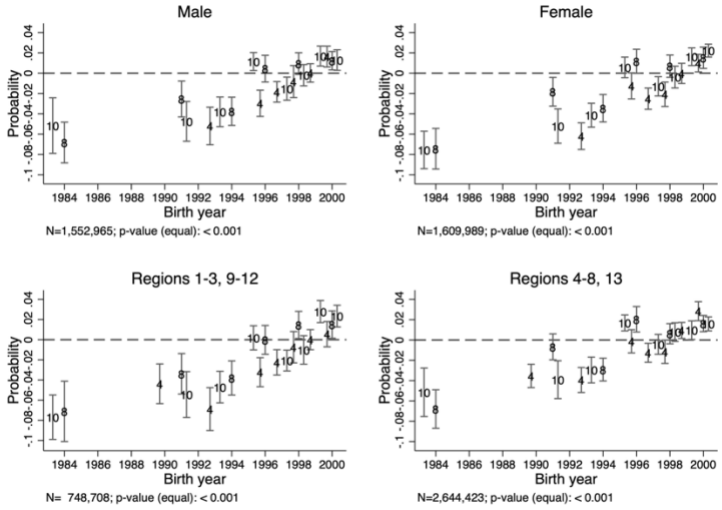
Notes: The panel uses the FG2 sample (see Table 1), limited to indigenous individuals ages 10–21. The circles are $\widehat{\lambda}_e$ from a modified equation (3), with pointwise 95% confidence intervals, in which I_{icmt} is replaced with a dummy variable indicating that indigenous individuals currently 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 test scores by gender and region

Panel A: Composite test

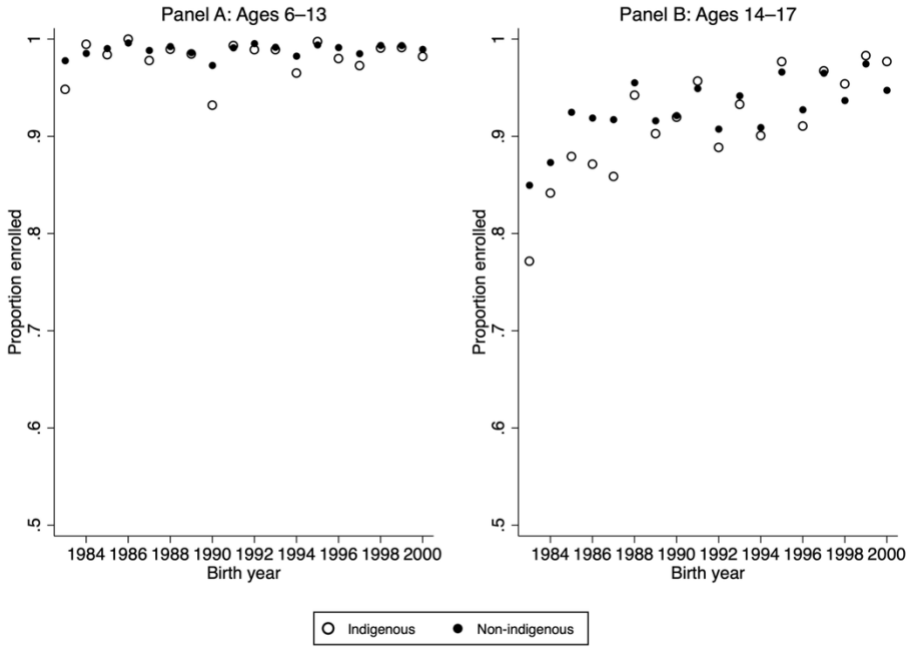


Panel B: Parent expectations for tertiary education



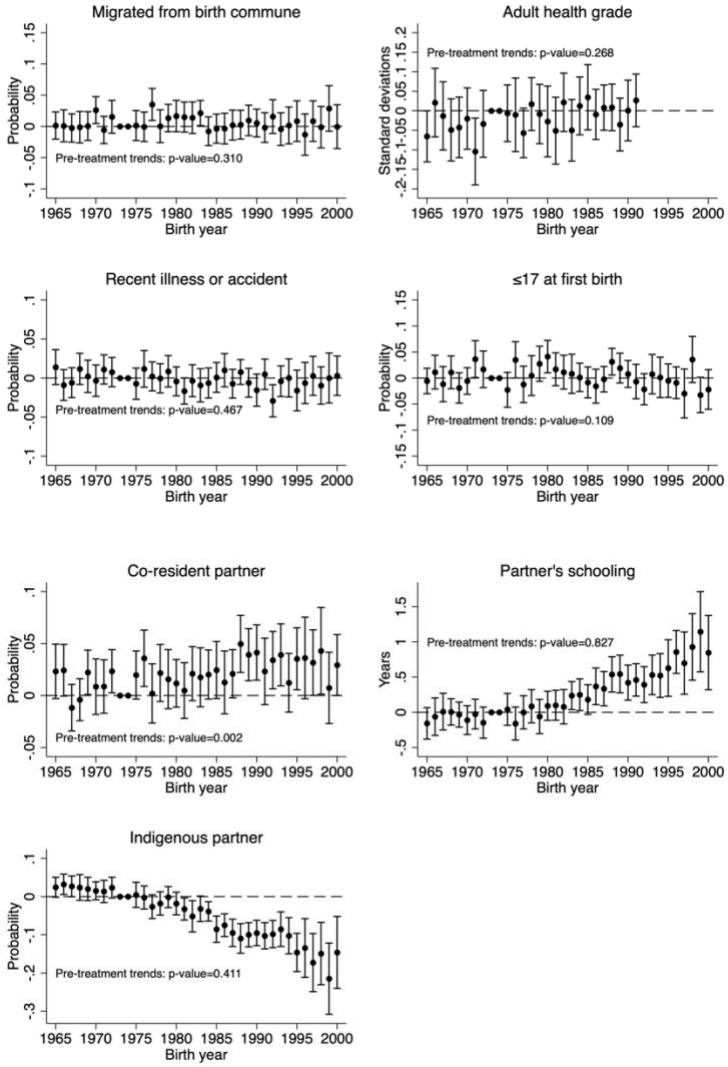
Notes: Each panel uses the FG3 sample (see Table 1), within four subsamples. Regions 1-3 and 9-12 are in the North and South and are more heavily indigenous. Regions 4-8 and 13 are Central. The labeled points are $\widehat{\lambda}_{eh}$ for each grade from equation (4), with pointwise 95% confidence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

Figure A8: School enrollment rates of indigenous and non-indigenous children, by birth cohort



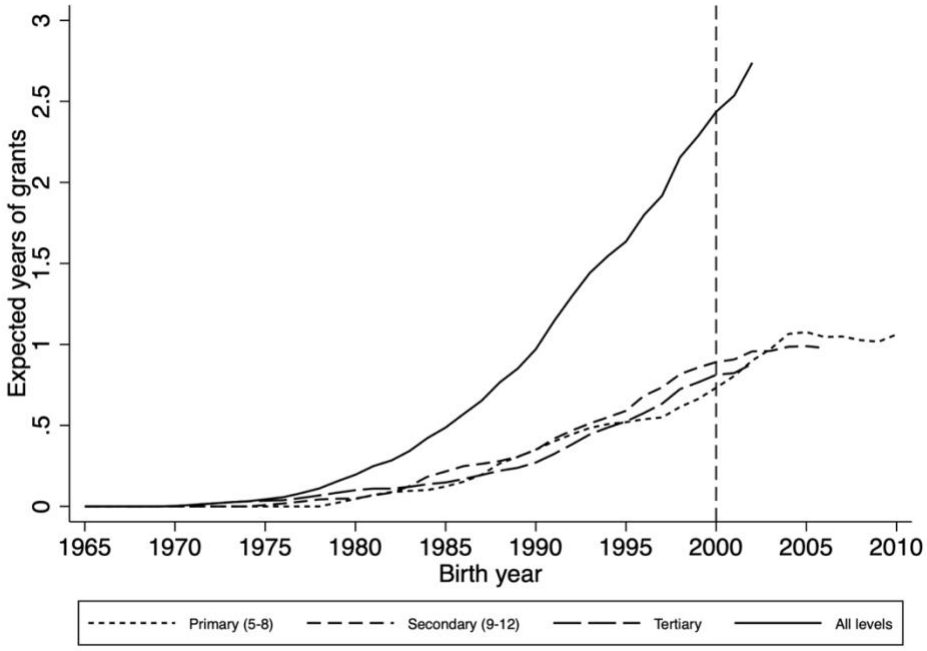
Notes: Panel A includes children ages 6–13, born from 1983 to 2000, in CASEN rounds in 1996, 2000, 2003, 2006, 2009, 2011, and 2013. Panel B includes children ages 14–17, born from 1983 to 2000, in CASEN rounds in 2000, 2003, 2006, 2009, 2011, 2013, 2015, and 2017. Each point is the weighted proportion of children enrolled in school at the time of the survey. Indigenous status in 1996–2003 is defined similarly to subsequent CASEN rounds included in the FG1 sample; note that these surveys do not include commune of birth.

Figure A9: Event study plots for additional variables in FG1



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

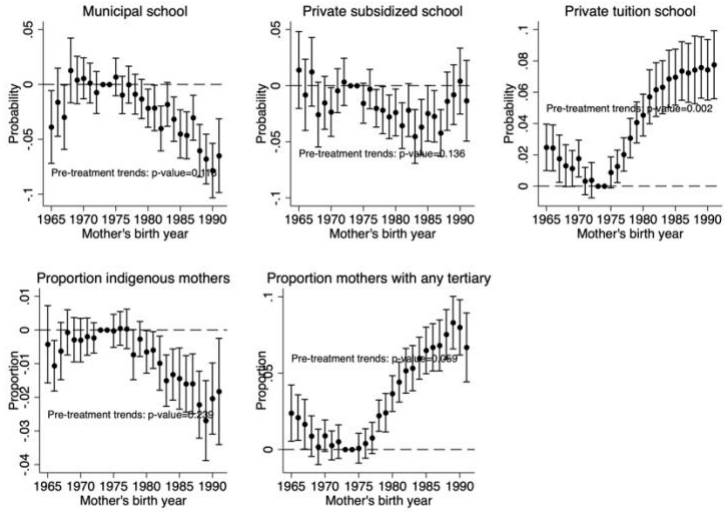
Figure A10: Expected years of indigenous grants, by birth year (1965 to 2010)



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

Figure A11: Event study plots for additional dependent variables in SG1 and SG2 samples

Panel A: SG1 sample



Panel B: SG2 sample

