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

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Abstract: Since 1991, Chile has provided renewable cash grants to indigenous students from lower-income households, conditional on enrollment in primary, secondary, or tertiary grades. We estimate intent-to-treat effects of grants on indigenous adults, leveraging variation in expected grant years across birth cohorts and never-treated adults, and using fixed effects to absorb unobserved variables shared by adults born in the same year and community. Cohorts with the greatest exposure had 0.6 more years of schooling, 10% more hours worked, and 22% higher labor earnings, reducing pre-treatment ethnic differences. After-tax earnings gains are 190 times larger than net government expenditures.

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1. Introduction

Latin America's marquee social policies offer cash payments to poor households in exchange for school enrollment of children and adolescents, among other conditions (Fiszbein and Schady, 2009; García and Saavedra, 2022; Araujo et al., 2024; Banerjee et al., in press). Considerable evidence now suggests that conditional cash transfers (CCTs) increase total schooling attainment—even after transfers end—but with surprisingly few effects on learning.[1] A smaller literature finds a mix of positive and null effects on adult labor earnings (Molina Millán et al., 2019; Araujo et al., 2024), with results that differ by gender, both within and across several evaluations.[2] One study finds no effects, but with caveats about statistical power and the length of time spent by young adults in the labor market (Molina Millán et al., 2020). Many programs are simply too recent for long-run follow-ups of older adults (Molina Millán et al., 2019). In summary, there is still much to learn about the long-run welfare implications of Latin America's 39 education-focused CCTs (García and Saavedra, 2022).

There is also little causal evidence on the potential of CCTs to improve the well-being of historically marginalized ethnic groups.[3] People of indigenous descent are 8% of Latin America's population, concentrated in Andean South America, Central America, and Mexico (World Bank, 2015). Regionwide, indigenous adults have lower levels of schooling attainment, test scores, and earnings than non-indigenous adults (McEwan, 2004, 2008a; Ñopo, 2012; Näsland-Hadley and Santos, 2022). Given the relationship between schooling and earnings, targeted human capital investments are a promising but under-researched intervention for improving labor market outcomes and reducing the entrenched disadvantages of indigenous people.

[1] On schooling attainment, see Araujo and Macours (2021); Attanasio et al. (2021); Baird, McIntosh, and Özler (2019); Barham, Macours, and Maluccio (2024); Barrera-Osorio, de Barros, and Filmer (2023); Cahyadi et al. (2020); Duflo, Dupas, and Kremer (2024); Parker and Vogl (2023); and Barrera-Osorio, Linden, and Saavedra (2019). García and Saavedra (2022) report a meta-analysis of many studies with heterogeneous test score outcomes and cannot reject the null of a zero effect, although one-third of studies find a positive effect.

[2] In Mexico, an experimental follow-up found robust effects on earnings (Araujo and Macours, 2021), though a quasi-experimental evaluation found large effects on labor participation and earnings for women, but null effects for men (Parker and Vogl, 2023). In a Nicaraguan experiment, earnings increased for men but not women (Barham et al., 2024).

[3] Brazil's *Bolsa Familia* program is the only large-scale program to directly target indigenous populations (Ibarrarán et al., 2017), along with scholarships targeted at indigenous students in a few countries like Chile, Peru, and Mexico. Except for Chile, these are focused on tertiary education, and relatively smaller in scale. Molina Millán et al. (2020) find that a Honduran cash transfer had negligible effects on the school attainment of a sizable indigenous minority, despite larger and durable effects for non-indigenous children.

This paper uses quasi-experimental methods to estimate the long-run effects of a Chilean program that has targeted education grants to indigenous, lower-income households since 1991, or six years longer than Mexico's pioneering CCT program (Parker and Vogl, 2023). About 10% of 20 million Chileans belong to an indigenous group, largely Mapuche. The *Beca Indígena* (or Indigenous Grants) program offers cash grants to indigenous students enrolled in the 5th grade of primary school through the end of tertiary. To qualify, enrolled students must obtain ancestry-based certification of indigenous status, pass a proxy-means test, and receive average grades above a low, but not failing, threshold. The primary, secondary, and tertiary grants are large, constituting 9%, 18%, and 56%, respectively, of the median household income per capita of grant recipients. Once awarded, grants are renewable if students comply with enrollment and minimum grade conditions.

The research design leverages variation in exposure to indigenous grants across birth cohorts and indigenous status. Indigenous children born before 1975 were not exposed, while birth cohorts in 1975 and later were increasingly exposed as the expanding program reached a larger proportion of each cohort, culminating in 2.4 years of expected grants years for indigenous individuals born in 2000. In contrast, non-indigenous children were never treated. Our identification strategy hinges on the fact that potentially-treated and never-treated children are born in the same communes—Chile's smallest territorial division. We can therefore include fixed effects for every combination of 36 birth cohorts and 318 birth communes. The unusually granular fixed effects absorb cohort- and location-specific unobservables that might otherwise lead to violations of the parallel trends assumption.[4] We also include separate intercepts for indigenous adults born in each commune. Together, they control for characteristics that are shared by individuals born in the same year and community, ensuring that estimates are based on differential exposure to education grants within birth communes.

We estimate event study regressions in large samples of Chileans born between 1965 and 2000 and surveyed as adults (ages 22 to 57) in household surveys from 2006 to 2022. The preferred intent-to-treat (ITT) estimates show that the later birth cohorts of indigenous adults—i.e., those with the most expected years—completed 0.6 additional years of schooling and had 22% higher labor earnings than the control group. The effects are large in male and female

[4] Chile has 346 communes, but some were created between 1981 and 2004. We therefore recode communes to pre-1981 borders to ensure consistent definitions across household surveys and birth cohorts (see section 3 for details).

samples, although schooling effects are somewhat larger for women than men, and earnings effects are slightly smaller. The effects represent a substantial reduction in ethnic inequality, since the schooling and earnings of pre-treatment indigenous cohorts were 1.36 years and 28% lower, respectively, than non-indigenous cohorts. We complement the event study findings with estimates of the average causal response to continuous years of potential grant exposure. One year of exposure causes an increase of 0.38 years of schooling and 13% in labor earnings, although this rests on a "strong" version of the parallel trends assumption that we assess (Callaway, Brantley, and Sant'anna, 2024).

The total earnings effects may reflect labor supply responses on the intensive and extensive margins, since our Poisson quasi-maximum likelihood regressions include adults with zero earnings (Chen and Roth, 2023). Indeed, birth cohorts with the greatest exposure to grants increased their probability of working at all by 4.3 percentage points and hours worked by 10% (with similarly robust effects for men and women). However, grant exposure did not affect other potential causal channels of earnings effects among adults, including migration to another commune in Chile, improved adult health, or early fertility of teen women.

The earnings effect is also plausibly explained by the wage returns to human capital investments, including total schooling attainment and learning. We assess the latter using administrative test score data on indigenous and non-indigenous children born during the period of greatest expansion in indigenous grants. We estimate within-commune convergence of 0.1 to 0.2 standard deviations between the mean test scores of fourth, eighth, and tenth grade indigenous students, relative to non-indigenous students, across birth cohorts from 1983 to 2000. The convergence mostly eliminated test score gaps among observably-similar indigenous and non-indigenous students within communes. We document a similar convergence for parents' expectations for their children's tertiary education attainment. The leading explanation for this conditional convergence is increasing exposure to indigenous grants, although it occurs in a truncated sample without pre-1975 birth cohorts.

Finally, we estimate the marginal value of public funds (Hendren and Sprung-Keyser, 2020) using ITT estimates for the two most recent birth cohorts of adults. The present value of after-tax earnings benefits are 190 times larger than net government expenditures (i.e., expenditures from grants and additional years of schooling, offset by a fiscal externality due to higher consumption tax revenues).

We rule out several threats to the internal validity of our preferred estimates. Most prominently, we find no evidence of differential pre-treatment trends for the dependent variables, including years of completed schooling and labor earnings. In addition, we use an auxiliary sample of children and adolescents and find no evidence of differential post-treatment trends in childhood exposure to many household and school variables that could have affected adult schooling and earnings. Indeed, the only post-treatment trend that we identify in this sample is the increase in the childhood exposure to indigenous grants.

The paper makes several contributions to the economics literature. First, we report precise and internally valid estimates of the effects of a scaled-up, government-run CCT on completed schooling and long-run labor market outcomes. Chile's policy is unusual and important for several reasons: (1) a government agency has consistently implemented and expanded the program since 1991, although it has not been previously evaluated;[5] (2) the annual grants are large and renewable across primary, secondary, and tertiary grades, providing strong incentives to households for up to 12 years of schooling; and (3) Chile has relatively strong school institutions, which provides a test of whether transfers can increase human capital and earnings when the activity being incentivized is higher-quality. The final point is salient in Latin America, given the generally weak effects of CCTs on learning across many evaluations, despite robust effects on enrollment and attendance (García and Saavedra, 2022).

Among the evaluations cited above, our paper is most closely related to Parker and Vogl (2023), who leverage birth cohort and continuous geographic variation in program intensity of Mexico's Progresa program. The estimation strategy requires a "strong" version of the parallel trends assumption that imposes restrictions treatment effect heterogeneity across all continuously-measured dosage groups (Callaway, Brantley, and Sant'anna, 2024). In contrast, our research design has a never-treated control group, which allows us to identify ITT effects for birth cohorts of indigenous adults under the standard parallel trends assumption. An added virtue of the never-treated control group is that we can identify the ITT effect of a cohort's lifetime exposure to grants, rather than differential exposure due to "early" versus "later" treatment phase-in that occurred in some experimental follow-ups (e.g., Araujo and Macours, 2021;

[5] Mexico's well-known Progresa CCT started in 1997 and ended in 2019 (Parker and Vogl, 2023). Honduran CCTs have undergone repeated shifts in targeting rules, amounts, conditions, and coverage between the early 1990s and the present (Galiani and McEwan, 2013; Benedetti, Ibararrán, and McEwan, 2016).

Barham et al., 2024). Lastly, our paper is related to Chilean studies that use discontinuous variation to identify the schooling attainment and earnings effects of means-tested loans for tertiary grades on marginally-eligible students (Solis, 2017; Bucarey et al., 2020). However, there is no comparable Chilean evidence on how targeted transfers in primary or secondary grades might affect earnings.

Second, our paper contributes to the wider literature on the targeting of policies like CCTs (Araujo et al., 2024; Ibarrarán et al., 2017; Banerjee et al., in press). The literature advocates focusing resources on students likely to reap the greatest program benefits (Araujo et al., 2024), in part because existing CCTs are generally less cost-effective ways of increasing a unified metric of enrollment and learning (Angrist et al., in press). Conditional cash transfers have weak effects if either untargeted (Dustan, 2020) or poverty-targeted but with substantial leakage to less-poor households (de Hoyos, Attanasio, and Meghir, 2021), plausibly because many inframarginal students receive transfers but do not increase their educational investments. Even in policies that primarily target poor households, program effects have been shown to differ substantially across experimental strata defined by poverty (Galiani and McEwan, 2013). The Chilean policy provides suggestive evidence that indigenous status deserves additional scrutiny as a component of proxy means-tests in Latin American countries, although it is rarely employed (Ibarrarán et al., 2017).

Third, our paper reports the first causal estimates of how a scaled-up, government-implemented, and well-targeted policy can substantially reduce educational and economic inequality for Latin America's large and historically marginalized indigenous population. The multiple disadvantages of indigenous relative to non-indigenous adults have been documented in a descriptive literature that relies on cross-sectional data from household surveys and schools (e.g., Ñopo, 2012; World Bank, 2015). We are not aware of research that comprehensively describes or causally explains trends in these differences across many years or birth cohorts, in contrast to a large U.S. literature on the determinants of racial inequality (e.g., Card and Krueger, 1992; Cook and Evans, 2000; Aaronson and Mazumder, 2011). Finally, we note that bilingual education programs are a common but infrequently evaluated school-based policy for addressing ethnic inequality in Latin America (PEIB, 2017; Näsland-Hadley and Santos, 2022). The programs are targeted to rural communities with many speakers of indigenous languages, but this definition overlooks many indigenous households. In 2022, our data show that 80% of Chileans

with indigenous ancestry no longer understand an indigenous language, and more than one-quarter live in Chile's capital.

2. Indigenous Grants in Chile

Since 1991, the *Beca Indígena* (or Indigenous Grants) program has offered renewable cash grants to indigenous students from lower-income households. To receive or renew a grant, recipients must be enrolled in school and maintain a minimum grade point average. The program is notable for its consistent implementation and expansion across more than three decades of democratically-elected presidents from parties on the left and right. The following paragraphs describe the eligibility criteria, the grant amounts, and the program's scale-up.[6]

Only indigenous students are eligible for the grants, hinting at the possible manipulation of indigenous status. To guard against this likelihood, the program requires students to obtain formal certification of indigenous ancestry from CONADI (the *Corporación Nacional de Desarrollo Indígena*), a government agency responsible for indigenous affairs. CONADI uses birth certificates to verify that the individual, a parent, or a grandparent has an indigenous surname. In 2022, 10% of 20 million Chileans self-identified as a member of one of nine indigenous groups. The largest is Mapuche, accounting for 78% of the national indigenous population, followed by Aymara (9%), Diaguita (7%), and Quechua (2%).[7] A qualitative literature has documented that the program is popular and well-understood among students with indigenous ancestry, and regarded as an important tool of economic mobility (Webb, 2015; 2022).

In addition to indigenous certification, students must be enrolled in primary school (from fifth and eighth grade), secondary school (from ninth to twelfth), or a tertiary degree program, including universities and vocational institutes. Students must maintain a grade point average of

[6] The program characteristics are promulgated in a series of publicly-available laws and decrees. Most prominently, the Indigenous Law of 1993 (Law 19253) mandated the appropriation of government resources for the program; established a legally-binding definition of indigenous ancestry; and created CONADI, the agency responsible for certifying indigenous status. In 1994, Decree 35 of the Ministry of Education specified the minimum grade point averages for recipients. In 2005, Decree 126 of the Ministry of Education shifted responsibility for program implementation to JUNAEB. Between 1991 and 2005, annual decrees of the Ministry of Education specify the number of funded grants and their amounts. From 2006 and the present, annual budget reports of JUNAEB provide the same information.

[7] The population estimates are from the 2022 CASEN household survey. The other groups include Rapa-Nui, Atacameño, Collas, Kawashkar, Yámana, Diaguita, and Chango. Throughout the paper, we code Chango as non-indigenous to maintain consistency with earlier CASEN surveys (see Appendix Table A1).

at least 5 points out of a possible 7 in primary and secondary education, and 4.5 points out of 7 in tertiary. This is above the failing threshold in publicly-funded schools, but still within reach of most students.[8] Enrollment and grade conditions are enforced at the time of grant application and renewal using Ministry of Education data.

Among applicants, the program is required to favor lower-income students. Formally, this is determined with a proxy-means test applied by the *Junta Nacional de Auxilio Escolar y Becas* (JUNAEB), a government agency within the Ministry of Education that targets and distributes all publicly-funded grants. Eligibility is restricted to indigenous households in the lowest 70% of household vulnerability as defined in national targeting system (DIPRES, 2021). In practice, the targeting rules are generally followed. Using the auxiliary child and adolescent dataset described in section 4, we confirm that all grant recipients are indigenous, and have per capita household incomes that are 24% lower than indigenous non-recipients and 43% lower than non-indigenous children.

The annual grants increase in value across primary, secondary, and tertiary grades. In 2021, the grants were, respectively, 100,550 pesos (US$118); 208,280 pesos (US$245); and 654,600 pesos (US$770). These were 8.6%, 17.8%, and 56%, respectively, of the median annual household income per capita—from all other sources—of grant recipients.[9] Primary and secondary grants are paid in two installments and tertiary grants are paid in 10 installments. The grants are deposited into obligatory accounts in *BancoEstado*, Chile's state-owned bank. Deposits for primary-aged children are directed to a parent or guardian account, while adolescents receive deposits in their own accounts.[10]

The program expanded steadily over 30 years. Panel A of Figure 1 illustrates the number of funded grants between 1991 and 2021, noting that calendar years overlap with school years. The Ministry of Education piloted the program in 1991, awarding 300 tertiary grants. In the following year, the Ministry funded 2,500 primary grants; 1,000 secondary grants; and 750 tertiary grants. In subsequent years, the government steadily increased the number of grants at all levels, while

[8] According to national rules for publicly-funded schools, students fail a grade if (1) they score below 4 in one subject and their average is less than 4.45, or (2) they score below 4 in two subjects and their average is less than 4.95 (Díaz et al., 2021).

[9] We estimate the median of the annual per capita household income for indigenous grant recipients in the auxiliary sample of children and adolescents described in section 4. Grant values and incomes are adjusted to 2022 prices using the Consumer Price Index.

[10] Girls younger than 12, and boys younger than 14 receive deposits via a parent or guardian.

also increasing nominal grant amounts in most years (though not enough to maintain the real value over 30 years).[11] By 2021, more than 92,000 grants were awarded annually at all levels.

Despite the growth illustrated in panel A, not all indigenous students received grants, even in later program years. Some students were ineligible because they were not enrolled in school, did not have sufficiently high grades, or did pass a proxy means test. Some might have been eligible but simply did not apply. Finally, some eligible students applied, but did not receive the grants because demand exceeded the number of funded grants in a particular year. In the following section, we use administrative data to estimate the probabilities that indigenous birth cohorts received grants in eligible school years. We sum the probabilities to estimate the expected years of grants received by each indigenous birth cohort.

3. Research Design

This section explains our approach to causal identification. We first describe the expected childhood exposure of adults to indigenous grants, which depends on their indigenous status and birth year. We next describe an event study specification for the estimation of intent-to-treat (ITT) effects in successive birth cohorts of adults that were increasingly exposed to grants. Finally, we describe an additional specification that estimates the average causal response to an additional year of expected grant exposure.

A. Differential Grant Exposure by Ethnicity and Birth Year

Individual exposure to indigenous grants varies along two dimensions. First, non-indigenous children and adolescents were never eligible to receive grants. Second, the exposure of indigenous children and adolescents depended on their birth year—and thus the years spent in grant-eligible grades—in relation to the steady increases in the number of funded grants. Using administrative data, we calculate E_{ic}, the expected years of indigenous grants received by adult i born in year c. E_{ic} is the cohort-specific sum of annual probabilities of receiving a grant during each eligible grade:

[11] The number of tertiary grants includes two programs that are much smaller, but with similar targeting criteria and values as the tertiary grants. These include a cash stipend to support the housing of students during tertiary studies, and the in-kind offer of housing in a small number of indigenous dorms in larger cities. Nominal grant amounts have increased steadily, but Appendix Figure A1 illustrates a decline in real value in the recent program years (e.g., by 24% between 2000 and 2021).

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

where I_{ic} takes the value of 1 for indigenous adults (such that E_{ic} is always zero for non-indigenous adults). N_c is the number of indigenous children born in year c that were alive just before grant eligibility in fifth grade.[12] G_c^g is the number of indigenous grants awarded to those born in year c in each year g between $c + 10$ (the year of the fifth primary grade) and $c + 21$ (the year of the fourth tertiary grade). For example, the 2000 birth cohort was first eligible in $g = 2010$, such that $\frac{G_{2000}^{2010}}{N_{2000}} = \frac{5{,}178}{33{,}124} = 0.156$.[13] Summing across 12 grade-specific probabilities yields the expected years of grant exposure for each cohort.

Panel B in Figure 1 shows that E_{ic} increased from 0.003 years for the indigenous cohort born in 1970 to 2.4 years for the 2000 cohort. The growth in E_{ic} reflects two aspects of program scale-up. First, cohorts born before 1983 were exposed to grants for fewer than 12 years, since they started fifth grade before program initiation in 1991. Second, the grade-specific probabilities of receiving a grant increased each year as the total number of funded grants increased. For example, cohorts born in 1979 and 2000 received a grant when they were aged 13 (or nominal eighth graders) with probabilities of 0.02 and 0.21, respectively.[14]

The cohort trend in E_{ic} motivates an event study design, which we use to identify intent-to-treat effects on successive birth cohorts of indigenous adults, relative to non-indigenous adults. We emphasize that the design does not rely on grant dosage measured at the individual level, for reasons related to data and identification. First, the household survey data do not retrospectively measure the childhood grant receipt of adults born in the relevant years. Second, the decision to apply to a grant—among eligible indigenous children—is likely endogenous to individual and household determinants of adult outcomes. Therefore, we use the exogenous expansion of the program to identify intent-to-treat effects on entire birth cohorts of indigenous adults.

[12] We use 2017 census microdata (https://redatam-ine.ine.cl) to estimate the size of indigenous cohorts, where indigenous status is based on affirmative responses to nine mutually-exclusive indigenous groups, omitting the non-specific category of "other" (see Table A1). We calculate the expected size of each cohort at 5 to 9 years of age, using 2015 life tables (https://apps.who.int/gho/data/view.main.LT62010?lang=en).

[13] For each school year, we assume that grants were evenly allocated across the eligible grades within each schooling level. In this example, there were 20,712 primary grants awarded in the 2010 program year across four eligible primary grades.

[14] Appendix Table A2 reports grade-specific probabilities used to estimate years of exposure. The 1975 cohort was the first one exposed to any secondary grants, while the 1979 cohort was the first one exposed to any primary grants.

B. Event Study Specification

The program's scale-up lends itself to a quasi-experimental research design that compares later and earlier birth cohorts (the first difference) across indigenous and non-indigenous adults (the second difference). We estimate the following specification in large samples of adults born between 1965 and 2000:

$$Y_{icmt} = I_{icmt}\left[\sum_{e=1965}^{1973}\gamma_e 1\{c=e\} + \sum_{e=1975}^{2000}\lambda_e 1\{c=e\} + \sum_{m\in M}\theta_m\right] + \delta_{cm} + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt} \quad (1)$$

where Y_{icmt} is the outcome (e.g., schooling or labor earnings) of adult *i*, born in year *c* and commune *m*, and surveyed as an adult in year *t*.[15] I_{icmt} is a dummy variable indicating indigenous status. The γ_e and λ_e are difference-in-differences estimates of intent-to-treat effects in each birth year—relative to omitted birth years in 1973 and 1974—since the 1975 cohort was the first cohort exposed to primary or secondary grants.[16] Recall that expected grant years (E_{ic}) increases from near-zero for the 1975 cohort to 2.4 years for the 2000 cohort (Figure 1, panel B). Thus, we expect that the γ_e should be zero, and the λ_e should be increasing in *e*.

Causal identification hinges on the inclusion of fixed effects for each combination of 36 birth years and 318 birth communes (δ_{cm}), as well as separate intercepts for indigenous adults born in each commune ($\sum_{m\in M}\theta_m I_{icmt}$).[17] The δ_{cm} absorb unobserved determinants of adult outcomes that are shared by indigenous and non-indigenous individuals born in the same year and commune—Chile's smallest territorial unit—including local economic conditions, the quantity and quality of services provided by locally-elected governments, and national policies that may have disproportionately targeted lower-income or rural communes. (Municipal governments in communes are responsible for the administration of public schools and primary health care facilities, among other services.) Together, the fixed effects ensure that the difference-in-differences coefficients (i.e., the γ_e and λ_e) are estimated within 318 birth communes. We use

[15] The research design avoids the well-documented challenges to interpreting two-way fixed effects models with staggered rollout in which some units (like states) are treated earlier or later than others (Roth et al., 2023). Rather, it is a difference-in-differences design with a common start and increasing treatment intensity thereafter (Goodman-Bacon, 2021; Miller, 2023).

[16] Cohorts from 1970 to 1974 were exposed to a very small number of tertiary grants.

[17] Chile is divided into 346 communes, although 28 were created between 1981 and 2004. To ensure consistent comparisons across self-reported birth communes in various birth cohorts, we assign adults to 318 "super-communes" based on pre-1981 boundaries (see Appendix Table A3).

birth communes rather than communes of adult residence because post-birth migration is potentially endogenous to grants exposure.

The specification further includes fixed effects for survey years (μ_t), since the dataset pools adult observations from multiple household surveys. The observed controls in X'_{icmt} include age dummies, a gender dummy, and census-derived proxies of the parental schooling of indigenous and non-indigenous adults in each birth cohort (section 4 describes these variables). Finally, the standard errors are clustered separately by (1) birth communes and (2) communes of adult residence.[18]

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

For ease of interpretation, we also report ITT estimates from a simpler version of equation (1) where we impose the assumption of parallel pre-trends for cohorts from 1965 to 1974 (i.e., $\gamma_e = 0 \; \forall \; e$), and estimate λ_e for three groups of post-treatment birth cohorts with increasing exposure to indigenous grants.[19] Cohorts from 1975 to 1981 were exposed for less than 12 school years (an average E_{ic} of 0.13 years). Cohorts from 1982 to 1991 were exposed for 12 years, though relatively fewer grants were available in those years (an average E_{ic} of 0.65 years). Finally, birth cohorts from 1992 to 2000 were exposed for 12 years in program years with relatively more grants available (an average E_{ic} of 1.86 years).

C. Average Causal Response to Expected Grant Years

[18] We cluster by commune of residence as a conservative approach to account for multi-stage sampling in the CASEN household survey, as well as serial correlation in adult outcomes across local labor markets. The CASEN primary sampling units were nested within communes, but did not overlap across survey years. We separately adjust for clustering by birth communes to account for treatment assignment, since communes with larger proportions of indigenous households received more grants, in expectation.

[19] None of the results presented below are sensitive to the implied assumption that $E_{ic} = 0$ in birth years from 1970 to 1974. In Appendix Table A4, we present ITT regressions that reassign these cohorts from the pre-treatment group to the group of partially-treated birth years (from 1970 to 1981), and simply use a control group with fewer birth years (from 1965 to 1969). Given the compelling visual evidence of parallel pre-treatment trends, it is not surprising that the estimates are quite similar to the preferred ITT estimates.

We also report a specification with the continuous measure of expected grant years (E_{ic}), instead of the cohort-specific interactions in equation (1):[20]

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

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

If the assumptions are satisfied, then $\hat{\tau}$ is the weighted average of causal responses between adjacent values of E_{ic} (Callaway et al., 2024).[21] To estimate the components of the weighted average, we estimate a regression with a saturated set of expected "dose" indicators, relative to never-treated individuals (cf. Sun and Shapiro, 2022). Since each treated birth cohort has a different value of E_{ic}, this is simply the event study in equation (1) with pre-treatment coefficients constrained to zero:

$$Y_{icmt} = I_{icmt}\left[\sum_{e=1975}^{2000} \lambda_e 1\{c = e\} + \sum_{m \in M} \theta_m\right] + \delta_{cm} + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt} \quad (3)$$

The average causal response to an additional year for cohort *e* relative to cohort *e - 1* is $\widehat{ACR}_e = \frac{(\hat{\lambda}_e - \hat{\lambda}_{e-1})}{(E_e - E_{e-1})}$, for adjacent pairs of birth years.[22] $\hat{\tau}$ in equation (2) is a weighted average of the $\widehat{ACR}_e$ obtained from equation (3), in which the OLS weights are positive and sum to 1 (Callaway et al., 2024).

4. Data

To implement the empirical strategy, we use multiple years of the CASEN (*Encuesta de Caracterización Socioeconómica Nacional*) household survey, collected by the Ministry of Social Development. We complement these data with a 10% sample of the 2002 census (*XVII*

[20] Recall that $E_{ic} = 0$ for non-indigenous adults and for any birth cohort not exposed to grants. We carry forward the earlier assumption that $E_{ic} = 0$ for indigenous cohorts before 1975.
[21] E_{ic} is continuous, but we can treat it as multi-valued discrete across 26 values for each post-treatment cohort.
[22] Note that $\widehat{ACR}_{1975} = \frac{\hat{\lambda}_{1975} - 0}{E_{1975} - 0}$.

Censo Nacional de Población), and multiple years of Chile's national test score data, collected by SIMCE (*Sistema de Medición de la Calidad de Educación*).[23]

A. CASEN Household Survey Data

The main sample pools adult observations from CASEN household surveys in 2006, 2009, 2011, 2013, 2015, 2017, 2020, and 2022. The surveys have two advantages vis-à-vis the research design. First, the questionnaires ask whether individuals "belong to or are a descendant of" nine indigenous groups (see Appendix Table A1). The phrasing is consistent with ancestry-based eligibility rules that are enforced by CONADI to determine grant eligibility (see section 2). Second, the surveys collect individuals' communes of residence *and* birth, in contrast to pre-2006 surveys which only record residence.

We construct a sample of adults between the ages of 22 and 57, and born between 1965 and 2000 (see Table 1). The primary dependent variables are (1) years of completed schooling, and (2) monthly labor earnings (in 2022 pesos), including individuals who reported zero labor earnings. We construct additional dependent variables to corroborate and explain the main results on schooling and earnings. First, we identify adults in tier A of FONASA (the *Fondo Nacional de Salud*), which is the public health insurance system. Tier A eligibility is limited to individuals with the lowest, administratively-determined household incomes, and so it functions as an unusually credible income proxy. Second, we measure individuals' labor supply in two ways: (1) whether individuals report any work in the week prior to the survey, and (2) the weekly hours that individuals typically work in their main job, coded to zero for all individuals who do not usually work. Third, a migration dummy variable indicates whether individuals no longer reside in their commune of birth. Fourth, we measure health status with: (1) a "grade" of overall health, reported from 1 to 7 (but standardized to a z-score in our sample), and (2) a dummy variable indicating a recent illness or accident. Fourth, we measure early fertility with a dummy variable indicating that women had a live birth at age 17 or younger.

We also use CASEN surveys to construct an auxiliary sample of children and adolescent between the ages 6 and 21, and born between 1985 and 2000. We use the sample to test for post-

[23] To obtain CASEN surveys, see https://observatorio.ministeriodesarrollosocial.gob.cl/encuesta-casen. To obtain the IPUMs 10% sample, see https://international.ipums.org/international-action/sample_details/country/cl#tab_cl2002a. SIMCE microdata are available by application to https://www.agenciaeducacion.cl/simce/.

treatment trends in childhood determinants of adult outcomes—such as household and school inputs—which would threaten the internal validity of ITT estimates (see Appendix Table A5 for descriptive statistics on this sample). The sample does not include children born before 1985, although the range usefully coincides with the largest increases in potential exposure to the treatment (see panel B in Figure 1). We discuss the variables and tests in section 6.

B. Other Data

Equation (1) includes controls for parental education, but these variables are not retrospectively reported by adult respondents in CASEN surveys. Instead, we construct approximations of parental education from a 10% sample of the 2002 census.[24] Specifically, for each birth year by indigenous status by birth commune cell, we calculate the mean years of schooling of mothers and fathers in the census whose children were born from 1965 to 2000. We also calculate the proportion of observations within each cell with missing values of mothers' and fathers' schooling. We merge the four variables to each adult in the CASEN sample.[25]

Finally, we use national testing data from SIMCE to assess whether grants exposure narrowed achievement gaps between indigenous and non-indigenous students. SIMCE is a near-census of students that includes both student and parent variables. Test scores are from exams in fourth, eighth, and tenth grades. We use 21 grade-by-school-year rounds between the 1997 and 2015 school years, assigning each round to the most common birth cohort of surveyed students. The rounds contain data for cohorts born between 1983 and 2000.[26] Appendix Table A6 reports the sample sizes in each round, defined as students in the public-release data files with a language and/or mathematics score. There are 4.89 million tested students, and we observe

[24] We use the 2002 census (rather than 2017) to maximize the likelihood that individuals born between 1965 to 2000 are still co-resident with a parent. The 1992 census could also be used for birth years before 1992, but the two census rounds used slightly different questions to measure indigenous status and obtained different population proportions (see Appendix Table A1). We only use the 2002 round to ensure consistent schooling comparisons across indigenous and non-indigenous birth cohorts.

[25] Appendix Figure A2 illustrates the means of the four variables. Parental schooling is increasing across birth years, and the proportion of missing values is diminishing, but it evolves similarly in the indigenous and non-indigenous samples. Equation (1) also controls for dummy variables indicating observations with missing values of the four merged variables.

[26] Not all students were born in the modal birth year because of grade repetition. A repetition variable is not available in all SIMCE rounds, particularly in the 1990s birth cohorts, but controlling for repetition does not affect the main conclusions reported in section 7.

whether 79% of the students have an indigenous or non-indigenous mother, as reported by a parent or guardian.[27]

5. Intent-to-Treat Effects on Schooling and Earnings

A. Event Study Estimates

Figure 2 plots the estimates of γ_e and λ_e from equation (1), along with pointwise 95% confidence intervals. Panel A corresponds to years of schooling. There is no evident pre-treatment trend, and we fail to reject the null hypothesis that pre-1973 coefficients are jointly equal to zero.[28] In contrast, the post-1974 coefficients are increasing in proportion to E_{ic} (see Figure 1), albeit with noise. For birth years between 1975 and 1982, the ITT estimates for schooling are mostly positive, but small and not statistically distinguishable from zero, which is consistent with the lower grant exposure of these cohorts. The estimates are increasingly positive and statistically different from zero for cohorts starting in 1983. By the 2000 birth cohort, there is an ITT effect on schooling of just under one year. The conclusions are similar when the event studies are separately estimated for women and men (see Appendix Figure A4).

Panel B corresponds to labor earnings, and the sample includes adults with zero earnings. We therefore apply a Poisson quasi-maximum likelihood estimator and report exponentiated coefficients, $exp\ (\hat{\gamma}_e)\ - 1$ and $exp\ \left(\hat{\lambda}_e\right)\ - 1$, which are level ITT effects on earnings expressed as a percentage of control means (Chen and Roth, 2023; Wooldridge, 2010).[29] Standard errors are obtained with the delta method. As with years of schooling, there are no pre-treatment trends and the post-treatment ITT effects are increasing in E_{ic}. By the 2000 birth cohort, the ITT effect on labor earnings is just under 40%, with results that are robust for men and women. The most

[27] Earlier SIMCE rounds collected the indigenous status of only mothers, rather than mothers and fathers as in later rounds (see Appendix Table A6). We apply this limited definition across all rounds for consistency. SIMCE did not collect indigenous status between 2001 and 2005, and in 2013. We infer indigenous status for some of these rounds by using the Ministry of Education's masked student identifier to impute indigenous status from earlier or later appearances of the student in other grades. Finally, our dataset codes students as indigenous if their mothers identified as indigenous in *any* grade, using the panel structure implied by the rows in Appendix Table A6.

[28] We define 1965 as the first cohort because it was the first to enter primary school (in 1971) after the conclusion of the presidential administration of Eduardo Frei Montalva (1964-1970). A school-building campaign in rural areas alleviated supply constraints in upper primary grades (Pérez Navarro, 2018). Appendix Figure A3 includes event study plots for cohorts born between 1960 and 1964. For these cohorts, there is clear visual evidence of a pre-trend in the case of schooling, but not labor earnings.

[29] Chen and Roth (2023) show that other approaches to including zero values of the dependent variable (e.g., log[Y+1]) are sensitive to the scale of earnings.

apparent difference is that some estimates for female labor earnings are slightly lower, and the confidence intervals are more likely to include zero (see Appendix Figure A4).

In section 6, we provide additional evidence that the estimates in Figure 2 are not explained by differential, post-treatment trends in variables *within* communes—such as household income or school quality experienced during childhood—that might affect adult schooling and earnings. In addition, the earnings effects may reflect labor supply responses on the extensive or intensive margins—and potentially other mechanisms—in addition to the wage returns of more schooling. We revisit the causal channels of earnings effects in section 7.

B. ITT Estimates

In Table 2, panel A reports summary ITT estimates for the three groups of post-treatment indigenous birth cohorts, relative to untreated cohorts. The full sample ITT effects for partially-exposed cohorts (from 1975 to 1981) are positive, but small and not statistically different from zero (columns 1 and 4). In contrast, the effects are larger and statistically significant for the fully-exposed cohorts, especially those born from 1992 to 2000. The ITT estimates for these cohorts imply 0.6 years of increased schooling (column 1) and 22% higher labor earnings (column 4). For each variable, we reject the null hypotheses that the three coefficients are jointly equal or jointly equal to zero. To help interpret the magnitudes, Table 2 reports the means of the dependent variables in samples of never-treated adults born from 1965 to 1974. Indigenous adults in these cohorts had 1.36 fewer years of schooling than non-indigenous adults, and 28% lower earnings. Given the ITT estimate for cohorts between 1992 and 2000, the program reduced the pre-treatment schooling gap by 44% and the earnings gap by 79%.

We estimate the same regressions in samples of women and men (Table 2, panel A, columns 2, 3, 5, and 6) and confirm two main results. First, the ITT estimates are larger for women's schooling (0.72 for the latest birth years, versus 0.46 for men, in columns 2 and 3), though both groups had similar mean schooling in untreated birth years. The estimates are consistent with descriptive evidence that girls are more likely to receive grants than boys, even though gender is not a formal criterion for the awarding or renewal of grants.[30] One explanation is that boys were

[30] Appendix Table A5 shows that girls are 56% of indigenous grant recipients and 49% of indigenous non-recipients. Girls are 0.025 more likely than boys to receive grants, conditional on household attributes and birth commune fixed effects (see Appendix Table A7).

less likely to apply for (or to renew) grants—and therefore to continue in school—perhaps because of higher forgone earnings of schooling investments relative to grant amounts. Second, the ITT estimates are somewhat larger for men's labor income (23% for the latest birth cohorts, versus 18% for women, in columns 5 and 6). The modestly smaller earnings effects for women contrast with the larger schooling effects, perhaps implying that indigenous women face gender-specific barriers to higher labor earnings, such as labor market discrimination (Galarza and Yamada, 2017).

C. Average Causal Response to Expected Grant Years

In Table 2, panel B reports estimates of the ACR, $\hat{\tau}$, from equation (2). The average effect of a one-year increase in E_{ic} is 0.38 years of schooling (column 1) and a 13% of labor earnings (column 4). Like the ITT estimates, a one-year increase in E_{ic} has larger average effects on women's schooling, relative to men (columns 2 and 3), while men have slightly larger effects on earnings (columns 5 and 6).

Figure 3 compares the full-sample estimates of the ACR (i.e., the slope of solid line) to estimates from the cohort-specific $\hat{\lambda}_e$ from equation (3), plotted at the each cohort's value of E_{ic} instead of birth year. As section 3 explained, the slopes between adjacent $\hat{\lambda}_e$ might be interpreted as average causal responses ($\widehat{ACR}_e$) to additional units of E_{ic}. Mechanically, $\hat{\tau}$ is the weighted average of these slopes, with positive weights that sum to one (Callaway et al., 2024). The figure shows that the average causal responses are roughly linear across the observed range of E_{ic}.

However, the interpretation of $\hat{\tau}$ as an average causal response hinges on the heterogeneity restriction described in section 3: each birth cohort must experience the same ITT effect as another, if exposed to the same E_{ic}. Figure 3 provides circumstantial evidence that this is the case, since the slope of the solid line is not being obviously pulled by those at either the highest or lowest values of E_{ic}. In section 6 we discuss additional evidence on this assumption. In section 7, we leverage the ITT estimates in Figure 3 to estimate the marginal value of public funds for the two latest birth cohorts (Hendren and Sprung-Keyser, 2020).

D. ITT Estimates Across the Distribution of Schooling and Earnings

Indigenous grants were available to students enrolled between the fifth grade of primary through the end of tertiary schooling. However, the effects on school attainment may not be

uniformly observed across the schooling distribution. To explore this, we estimate 16 summary ITT regressions with the dummy dependent variable $1\{Y_i \geq x\}$, where Y_i is years of schooling and the x are successively higher grades of schooling. Panel A of Figure 4 plots ITT estimates and confidence intervals for the indigenous adults born from 1992 to 2000. The largest marginal probabilities, between 0.05 and 0.1, are in upper primary (7 to 8) and secondary grades (9 to 12). We anticipate small estimates for the lowest years of schooling, given near-universal primary enrollment and grant eligibility that only begins in fifth grade. Despite relatively large grants in tertiary grades, the estimates are also smaller and, except for the first year, not statistically distinguishable from zero. The results suggest that even the more sizeable grants for tertiary are not sufficient to overcome academic, financial, or other barriers that some indigenous students face in continued tertiary studies, especially for men relative to women (see Appendix Figure A5).

Panel B repeats this exercise for dummy variables indicating successively higher labor incomes ($1\{Y_i > x\}$), such that the coefficient for $1\{Y_i > 0\}$ is the marginal probability that individuals earned a positive income in the last month.[31] Note that the median monthly labor income of indigenous adults in the sample (including zeros) was 264,000 (US$311) and the 95th percentile was 1,100,000 (US$1,294) in 2022 pesos. The largest marginal probability of just over 0.06 is near the median income—since the program increased the likelihood that an indigenous person earned at least 250,000 pesos by 6 percentage points—and still above 0.02 for incomes above 1,500,000. Consistent with Table 2, the effects are smaller for women than men (see Appendix Figure A5).

6. Threats to Internal Validity

In this section we show that ITT and ACR estimates are robust to several threats to internal validity. First, we use the auxiliary sample of children and adolescents to test for differential, post-treatment trends in the household inputs to which indigenous and non-indigenous adults were exposed as children. Second, we conduct similar tests for differential, post-treatment trends in school inputs. In both cases, the essential concern is that increasing ITT effects are explained

[31] This is not perfectly collinear with the indicator of working in the last week (r=0.92) since a few adults worked in the past week for zero pay, and a larger number of adults did not work in the previous week but still earned positive earnings in the previous month.

by trends in unobserved variables, rather than increasing grants exposure. Third, we discuss whether increasing ITT effects can be explained by the endogenous switching of indigenous status. Fourth, we assess whether the main results can be explained by two policies targeted towards communes with high concentrations of indigenous households. Fifth, we provide evidence on the validity of the heterogeneity restriction implied by a "strong" parallel trends assumption, required to interpret $\hat{\tau}$ as the average causal response (Callaway et al., 2024).

A. Post-Treatment Trends in Household Inputs

Parallel pre-treatment trends do not guarantee that the parallel trends assumption holds for post-treatment birth cohorts (Roth et al., 2022).[32] Suppose, for example, that indigenous children in post-treatment birth cohorts are exposed to increasingly favorable household environments, relative to non-indigenous children, even within communes. We control for parental schooling in equation (1) to partially address this concern. This section formally tests for differential, post-treatment trends in other variables, using the auxiliary sample of children and adolescents born between 1985 and 2000 (see Appendix Table A5). These cohorts overlap with notable increases in both E_{ic} and ITT estimates (see Figures 1 and 2). We estimate

$$Y_{icmt} = \sum_{e=1985}^{2000} \lambda_e 1\{c = e\} \times I_{icmt} + \delta_{cm} + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt} \quad (4)$$

where Y_{icmt} is a household input of child *i* that is plausibly associated with adult schooling and earnings. The $\hat{\lambda}_e$ are differences between indigenous and non-indigenous children in a given birth year, controlling for cohort-by-commune fixed effects (δ_{cm}) and observed variables, as in equation (1). We visually examine the $\hat{\lambda}_e$ for trends and test the null of equal coefficients. If the dependent variable affects adult schooling and earnings, then a rejected null suggests bias in ITT estimates. We find no evidence of such trends.

To illustrate the test, consider two contrasting examples. The top-left panel of Figure 5 illustrates the $\hat{\lambda}_e$ for a dependent variable indicating that a child was receiving an indigenous

[32] As a simple illustration, Appendix Figure A6 plots the preferred event study estimates against those of a "naïve" specification that that only includes birth year fixed effects and an indicator for indigenous adults: $Y_{icmt} = I_{icmt}[\alpha + \sum_{e=1965}^{1973} \gamma_e 1\{c = e\} + \sum_{e=1975}^{2000} \lambda_e 1\{c = e\}] + \delta_c + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt}$. The estimates and p-values are consistent with parallel pre-treatment trends in both specifications. However, there are larger estimates in the naïve specification, most clearly for birth cohorts in mid-1980s and later. The implication is that the δ_{cm} play a key role in controlling for post-treatment trends in unobserved variables that would otherwise be mistaken for "effects."

grant at the time of the survey. The dashed line is an estimate constrained to be equal across all birth years. Between the 1985 and 2000 cohorts, indigenous children and adolescents were increasingly likely to report receiving an indigenous grant, and we can reject the null of coefficient equality at 1%. We overlay a solid line that plots the sample average of the probabilities that children in each cohort-by-age group received indigenous grants (the probabilities are from administrative data, as in Figure 1 and Appendix Table A2). Therefore, survey and administrative data confirm that indigenous birth cohorts were increasingly exposed to grants.

The top-right panel of Figure 5 illustrates the $\hat{\lambda}_e$ for a dummy variable indicating that individuals were receiving any other grant or scholarships, all of which are allocated with criteria other than indigenous status.[33] There is no evident trend, and we fail to reject the null of equality. Therefore, differential trends in other grants or scholarships cannot explain the increasing ITT estimates in Figure 2. Figure 5 also reports tests for household total income per capita and transfer income per capita, both of which exclude education grants. The former is a broad proxy of household inputs and socioeconomic status, while the latter summarizes exposure to government transfer programs that are allocated to lower-income households.[34] The estimates show no evident trends, and we fail to reject the null of coefficient equality. Appendix Figure A7 illustrates similar results for other child and household variables, including tier A membership in the public health insurance system (a proxy for lower-income households); whether children migrated from their birth commune; and whether children experienced a recent illness or accident.

B. Post-Treatment Trends in School Inputs

We implement the same tests for variables that measure childhood exposure to school inputs. First, there is no evidence of differential trends in the marginal probability of enrolling in municipal schools or the private subsidized schools introduced by Chile's "voucher" reform in

[33] For example, the *Beca Presidente de República* offered grants to secondary and tertiary students with low incomes and high achievement, and the *Beca de Apoyo a la Retención Escolar* targeted lower-income students at risk of dropping out of secondary school. For a description of grants and scholarships, see https://www.junaeb.cl/becas-junaeb/.

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

1981 (see Figure 6).[35] Similarly, there is no post-treatment trend in the marginal probability that indigenous children pay school tuition to any private or public school (see Appendix Figure A7). Second, we find no evidence of post-treatment trends in relative exposure to national school reforms in primary and secondary schools. These include free school meals for lower-income schools and children (McEwan, 2013) and a Full School Day (FSD) program that increased instructional time in publicly-funded schools (Raczynski, 2001; Dominguez and Ruffini, 2023) (see Figure 6).[36] Third, we find no evidence of differential trends in exposure to targeted primary school reforms in the 1990s, including the 900 Schools Program for low-achieving schools (Chay, McEwan, and Urquiola, 2005) and the MECE-Rural Program for rural, multigrade schools (McEwan, 2008b) (see Appendix Figure A7).

Lastly, the Preferential Subsidy law in 2008 gradually increased school subsidies paid for students in the lowest 40% of household incomes (Aguirre, 2022). The program started with students between pre-kindergarten and 4th grade in 2008, and added a single grade in each subsequent year until full secondary grade coverage was attained in 2016 (Neilson, 2020). Given this roll-out, birth cohorts in 1999 and 2000 were affected by increased subsidies, although indigenous status was not directly used to target student subsidies. Appendix Table A8 shows that the main results in Table 2 are robust to the exclusion of both cohorts.

C. Endogenous Switching of Indigenous Status

Suppose that non-indigenous children and adolescents arbitrarily claim indigenous status to receive a grant. If marginal "switchers" have unobserved attributes associated with higher earnings, then endogenous switching into indigenous status that increases with grants exposure (as in Figure 1) could explain the ITT estimates (as in Figure 2). However, recall that grant eligibility requires individuals to obtain legal certification based on indigenous surnames, and also pass a proxy-means-test using administrative data. Given these hurdles, there is no obvious benefit to mis-representing indigenous status on the CASEN survey, especially among higher-

[35] The reform offered similar per-student subsidies to municipal and private schools, and private market share increased sharply in the 1980s (Hsieh and Urquiola, 2006). This preceded the largest growth in ITT estimates among cohorts born the 1990s, although "shared financing" rules in 1993 further allowed private subsidized schools to charge higher tuition in exchange for sliding reductions in subsidies.

[36] We measure exposure to both programs using self-reported data from the CASEN surveys (see Figure 6), and confirm the result for the Full School Day program with administrative school data linked to CASEN with unique school codes (see Appendix Figure A7).

income adults. However, there is a plausible stigma associated with doing so, since non-indigenous respondents hold negative stereotypes of indigenous identity, and indigenous respondents perceive discriminatory treatment (Saiz et al., 2008; Merino et al., 2009). Nonetheless, suppose that switching increasingly occurs across birth cohorts, and that switching is more common among individuals with attributes associated with higher adult earnings, such as household income per capita in childhood. This should introduce differential, post-treatment trends in these variables, yet we found no evidence of such trends for many household inputs.

Even so, the proportion of indigenous adults has risen across CASEN surveys used in the adult sample, from 7% in the 2006 survey, to 9% in 2013, and 10% in 2022 (see Table A1).[37] A plausible hypothesis is that stigma has diminished across survey years, leading fewer indigenous adults to mis-report ethnicity in survey data. Equation (1) therefore includes the μ_t to absorb survey-specific unobserved variables. In a related specification, we replace the δ_{cm} and μ_t in equation (1) with a triple interaction of 36 birth cohorts, 318 birth communes, and eight survey years (δ_{cmt}). This allows birth year shocks to vary within combinations of birth commune and survey year, and absorbs the separate age dummies. The specification also includes separate intercepts for indigenous adults in combinations of birth commune and survey years ($\sum_{m\in M, t\in T,} \theta_{mt} I_{icmt}$). The event study plots are visually similar (see Appendix Figure A8), and the summary estimates yield substantively similar conclusions in the full sample and in subsamples of men and women (see Appendix Table A9), albeit with slightly larger standard errors. Finally, we re-estimated equation (1) in a sample that is limited to CASEN surveys from 2013 to 2022 (and for which indigenous proportions are similar), also with results similar to Table 2.[38]

D. Policies Targeted at Heavily-Indigenous Communes

Two national policies targeted indigenous households residing in rural communes with higher concentrations of such households. First, the Indigenous Law of 1993 authorized CONADI to subsidize households' land purchases in indigenous communities. Using

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

[38] In the summary ITT regression for years of schooling, the coefficient (standard error) for the latest birth cohorts is 0.55 (0.13). For labor earnings, the exponentiated coefficient (standard error) for the latest birth cohorts is 0.23 (0.04).

administrative data, we identified 125 communes that received *any* land subsidy between 1993 and 2022, although the distribution is skewed. The mean (median) commune received 0.46 (0.09) hectares per capita.[39] In the 125 communes, 20.5% of adults in the CASEN sample are indigenous, versus 6.4% in all others. Second, the Indigenous Law directed the government to develop bilingual education programs. Between 2002 and 2010, the externally-financed Origins Program funded bilingual programs in 160 primary schools of 44 rural communes with a high concentration of indigenous households (Dascal, Campaña, and de la Fuente, 2010; PEIB, 2016; Webb, 2022).[40]

As a first exercise, we calculated the cumulative hectares per capita in each commune, and for each survey year between 2006 to 2022, and linked this to adults in the CASEN sample with the same birth commune and survey year. Including this as a covariate in equation (1) does not substantively affect the estimates, although we prefer the specification in Appendix Figure A8 and Appendix Table A9 that includes fixed effects for a triple interaction of birth communes, birth years, and survey years. This absorbs separate age and survey year fixed effects, as well as the land variable.

As a second exercise, Table 3 reports ITT and ACR estimates by indigenous concentration in communes, a proxy of potential policy exposure. Column 1 repeats the full-sample estimates from Table 2. Column 2 limits the sample to adults born in the 204 communes in which the indigenous proportion is lower than 10% (the national proportion), while column 3 further removes 37 communes that had received even small land subsidies by 2022.[41] The ITT estimates on schooling are smaller than column 1, but still consistent with effects between 0.43 and 0.45 years in the later birth cohorts. The effects on labor earnings are essentially unchanged. Not surprisingly, given the exclusion of many rural communes, the samples in columns 2 and 3 also have higher baseline levels of schooling and labor earnings among indigenous adults in pre-treatment cohorts, and smaller differences with non-indigenous adults (including no difference for completed schooling). A plausible interpretation is that treated indigenous cohorts in better-off communes included more inframarginal children and adolescents who would have received

[39] A record of 20A and 20B land subsidies—referencing the types of subsidies described in the law—is available at https://siic.conadi.cl. We calculate per-capita measures using the total indigenous population of each commune in the 2017 census, excluding the "other" category.
[40] Prior to 2002, small pilot programs affected relatively few students (PEIB, 2016).
[41] We calculate weighted proportions of indigenous individuals in each birth commune of the full adult sample, applying survey weights.

more school even in the absence of indigenous grants. Nonetheless, the program had similar effects on labor earnings.

The estimates in columns 4 and 5 include communes with indigenous population shares of at least 10%, which includes the communes targeted by the policies. The ITT estimates are larger in the case of schooling, but again very similar for labor earnings. The baseline levels of schooling and labor income are much lower in this sample. This again suggests that household attributes contribute to heterogeneous schooling effects. Column 5 excludes Temuco, a large commune that is the locus of Mapuche culture in southern Chile. The point estimates change, but the estimates largely align with the other results. The differences between samples in columns 2 to 5 suggest the existence of positive spillovers in years of schooling, since communes with more potentially treated students had larger effects. They also suggest the possibility of congestion or general equilibrium effects in labor markets, since the differential schooling effects did not result in additional increased labor market returns. Overall, the evidence does not suggest that the paper's main results are the result of correlated treatments.

E. Assumptions Underlying the Average Causal Response

In addition to the validity of parallel trends—with respect to never-treated adults—the interpretation of the ACR depends on a heterogeneity restriction across values of E_{ic} (Callaway et al., 2024). Specifically, any two birth cohorts must experience the same ITT if exposed to the same value of E_{ic}. There are three potential violations. First, it is possible that indigenous birth cohorts systematically chose values of E_{ic} based on their expected gains in schooling and earnings. However, this would (unrealistically) require that parents had the foresight to manipulate birth years in order to influence grants exposure 10 to 21 years after birth, based on heterogenous expected gains.

Second, it is possible that (1) the composition of treated households varies within indigenous birth cohorts (e.g., by household income), and (2) individual treatment effects are heterogeneous with respect to such attributes. In this case, the intent-to-treat effects of two indigenous cohorts may differ even if both receive the same E_{ic}. However, we find no evidence of post-treatment

trends in household income experienced by successive birth cohorts of indigenous children who received, or did not receive, an indigenous grant.[42]

Third, concurrently-implemented programs might have complementarities with indigenous grants.[43] If two cohorts differ in their amounts of complementary school inputs, then one cohort's ITT effects may be higher despite the same E_{ic}. We showed that this does not threaten the internal validity of ITT estimates, because there were no *relative* changes (within communes) in the exposure of indigenous and non-indigenous children to school inputs such as the Full School Day (FSD) program. However, it may threaten the degree to which relative changes in cohort-specific estimates exclusively reflect an average causal response to E_{ic}.

In our example, the $\widehat{ACR}_e$ for some later years may be "too high"—leading to upward bias in the weighted average, $\hat{\tau}$—because the ACRs at higher values of E_{ic} conflate the effect of higher grants exposure with an upward shift in the entire dose-response curve due to larger amounts of complementary inputs such as FSD (Callaway et al., 2024). Circumstantially, the higher values of E_{ic} that occur for later cohorts do not appear to be driving the slope of the ACR. However, our research design does not have enough independent variation in school inputs to compellingly test for complementarities.

7. Causal Channels of Earnings Effects

We found large ITT effects on schooling (0.6 years) and labor earnings (22%) for recent birth cohorts (see Table 2). The implied return to a year of schooling is very high at more than 35%, but the total earnings effect likely reflects causal channels other than the wage returns to years of schooling.[44] In the following sections, we show that grants exposure plausibly increased the test scores of indigenous children, and parents' educational expectations for the same children. It also

[42] In the auxiliary sample of indigenous children and adolescents (see Appendix Table A5), we estimate a variant of equation (4) for two income variables: (1) household income per capita, and (2) an indicator of tier A membership in public health insurance. We replaced I_{icmt} in equation (4) with a dummy variable indicating that indigenous children and adolescents currently receive indigenous grants. Appendix Figure A9 plots estimates of the $\hat{\lambda}_e$, which show no trends in relative incomes across birth cohorts. We also fail to reject the null of coefficient equality at conventional levels.

[43] The literature on cash transfers hypothesizes such complementarities but reports few experimental tests. A Tanzanian experiment shows that school inputs and teacher incentives are complements in production (Mbiti et al., 2019). A Honduran CCT experiment proposed a factorial design to study complementarities, but the school investments were not implemented (Galiani and McEwan, 2013).

[44] As a simple exercise to illustrate this point, the ITT effect on the log of labor earnings—which excludes adults with zero earnings—is substantially lower at 13%. However, this introduces sample selection bias (Chen and Roth 2023).

increased adult labor supply, but did not affect other variables that might have affected labor earnings, including migration within Chile, overall health of adults, and early births among women.

A. Test Scores and Parent Expectations

Indigenous grants might have increased students' test scores in a given grade via three channels. First, households had additional income (or parent time) to spend on human capital investments, although section 6 ruled out increased spending on private school tuition. Second, the enrollment and grade conditions may have directly affected students' incentives to exert effort, especially for students near the grade eligibility threshold for the awarding or renewal of grants (Scott-Clayton, 2011).[45] Students faced incentives even before receiving a grant, since grades at the end of the fourth primary grade determined eligibility in the fifth grade. Third, the labeling or framing of education grants as exclusively "indigenous"—independently of attached conditions—could have affected the beliefs of indigenous households about their expected returns to higher effort, achievement, or grade attainment in formal schooling (cf. Jensen, 2010; Benhassine et al., 2015). A qualitative literature has shown that indigenous students were keenly aware of the program, and perceived grants as a source of economic advancement for indigenous communities (Webb, 2015).

To test for effects of the program on test scores using equation (1), we would need data for birth cohorts from 1965 to 2000. In practice, student test scores in grades 4, 8, and 10 are available from SIMCE between the 1983 and 2000 cohorts (see section 4). Across these cohorts, expected grant years increased from 0.34 years to 2.44 years. We estimate a modified version of equation (4) in a stacked sample of SIMCE test scores across all grades:

$$Y_{icgm} = \sum_{e=1983}^{2000} \sum_{h\in\{4,8,10\}} \lambda_{eh} 1\{c = e \ \wedge g = h\} \times I_{icgm} + \delta_{cgm} + X'_{icgm}\beta + \varepsilon_{icgm} \qquad (5)$$

where Y_{icgm} is a test score of child *i*, in birth cohort *c*, assessed at the end of grade *g*, and enrolled in a school in commune *m*. We transform test scores to z-scores within grade-by-year

[45] We linked the SIMCE dataset to publicly-available enrollment datasets with year-end student grade point averages, using a masked student identifier, and calculated a dummy indicator of whether the GPA met or exceeded the grant-qualifying cutoff of 5. These data paint a murkier picture for two reasons. First, there are no observations for cohorts before 1991. Second, there is little compelling evidence of a trend in for 1990s cohorts, using equation (5).

cells. The δ_{cgm} are fixed effects for all combinations of birth cohorts, grades, and communes.[46] The specification also controls for student-by-grade indicators of gender and parents' schooling attainment; and school-by-grade indicators of private or municipal status, and whether schools ever participated in government programs (i.e., Full School Day, P-900, and MECE-Rural).[47] We cluster standard errors by communes.

The $\hat{\lambda}_{eh}$ are estimates of the conditional cohort-by-grade mean differences between indigenous and non-indigenous students' test scores. The top panels of Figure 7 plot the $\hat{\lambda}_{eh}$ and 95% confidence intervals, using the corresponding grade to label point estimates. In the earliest available birth years (1983 and 1984), test scores of indigenous students are 0.15 to 0.2 standard deviations lower than non-indigenous students, even for students in the same commune with similar parent and school variables.

In the latest birth years, the differences are not statistically different from zero for language scores and about –0.05 for mathematics scores. The pattern of conditional convergence within communes is more notable for language than mathematics, but is similarly evident in the samples of girls and boys (see Appendix Figure A10). In all cases, the pattern is evident for the "qualifying" fourth grade year before official grant eligibility in the fifth grade, suggesting that direct income effects are less important than the incentives or framing of indigenous grants.

Forward-looking parents might update their educational expectations for young recipients in response to the channels mentioned above or simply in response to higher achievement. We constructed an indicator of whether parents expect that students will complete any years of tertiary schooling, including vocational institutes or universities. In the earliest cohorts, indigenous parents are more than 5 percentage points less likely to expect tertiary schooling, relative to 62% for non-indigenous parents in the 1983 cohort (see the bottom-left panel of Figure 7). By later birth cohorts, indigenous parents have slightly higher tertiary expectations than non-indigenous ones within the same communes, controlling for parent and school variables.

[46] We do not observe the birth commune of each student and instead use school communes, a reasonable alternative given evidence the absence of post-treatment trends in the migration status of children (see Appendix Figure A7).
[47] We construct consistent measures of parent schooling across all SIMCE rounds, separately coding mothers' and fathers' schooling as 16 categories, including missing. We fully interact the dummy indicators of each parent's schooling with dummies indicating students' grade levels. Gender is not measured in the 4th grade SIMCE in 1999, and so we include a dummy indicator for missing observations.

The results in Figure 7 are effectively a truncated event study, in which the earliest cohorts are "pre-treatment" years (though they are, in fact, less treated), and the "differences-in-differences" suggest that grant exposure increased indigenous language scores by approximately 0.2 standard deviations, math scores by approximately 0.1, and tertiary expectations by at least 5 percentage points. This seems like a conservative estimate, since Figure 2 already showed small ITT effects on schooling for the earlier cohorts. The interpretation is somewhat tempered by the lack of data to measure pre-treatment trends.

B. Labor Supply, Migration, Health, and Fertility

This section uses the CASEN adult sample to assess four channels through which the program could have affected earnings. First, schooling plausibly increases labor supply on the extensive and intensive margins, especially for women with lower rates of labor force participation in settings with high demand for skilled labor (Erten and Keskin, 2018; Heath and Jayachandran, 2017). Second, education may spur individuals to migrate to Santiago or another urban area with more opportunities for skilled labor. Third, education potentially increases the health of adults, with related effects on earnings (Clark and Royer, 2013). Fourth, schooling may reduce early, unintended pregnancies that disrupt schooling or labor participation (Breierova and Duflo, 2004).[48]

Figures 8 and 9 report event study plots for seven additional variables from the CASEN adult sample, while Tables 4 and 5 report summary ITT estimates. We first confirm that labor earnings increased, using a dummy indicator of whether adults belong to tier A of the public health insurance system, a proxy for low-income households. Exposure to indigenous grants substantially decreased the likelihood of being in the lowest tier.[49]

Figures 8 and 9 further show that increased labor supply likely contributed to higher labor earnings, but not changes to migration, health, or early fertility. Figure 8 illustrates increasing

[48] Berthelon and Kruger (2011) show that girls exposed to the Full School Day program in Chile were less likely to become mothers, plausibly due to the incapacitation effects of increased time in school. We ruled out post-treatment trends in exposure to longer school days (see Figure 6), but the possibility remains that more years of school (relative to dropping out) may cause similar reduction in teen pregnancies.

[49] The top-left panel of Figure 8 shows increasingly negative ITT effects on tier A membership, with no clear evidence of a pre-trend (although the p-value is at the margin of conventional significance levels). The ITT estimate for later birth cohorts (1992 to 2000) implies a decrease of 4.3 percentage points, representing just over a one-quarter reduction in the indigenous and non-indigenous difference in pre-1975 cohorts (see Table 4). The ITT estimates are modestly smaller for women (see Table 5), again consistent with gender differences in effects on labor income.

ITT effects on the probability of working in the last week, as well as the total hours worked in a typical week, albeit with less precision than Figure 2. The summary ITT estimates in Table 4 show that indigenous adults in the latest birth cohorts were 4.3 percentage points more likely to work in the prior week and worked 10% more hours in a typical week.[50] The ACRs of one year of expected grants are 2.3 percentage points and 5.8%, respectively.

In pre-treatment cohorts, indigenous men had much higher labor participation than women: 86% and 50% worked in the week before the survey, respectively, and they worked 39 and 20 hours in a typical week (see Table 5). Even with these baseline differences, we find robust effects on the labor supply of men and women, and for both measures of labor participation. Focusing on the latest cohorts, the ITT estimate for women is 3.7 percentage points versus 4.8 percentage points for men. In both samples, there is approximately a 10% increase in hours worked relative to the control group.

In contrast, we find little evidence that exposure to indigenous grants affected migration from birth communes, adult health, or women's teen fertility. The event study plot for migration shows that nearly all confidence intervals include zero, before and after the omitted years, with a few outliers (see Figure 9). There are similar patterns in the event study plots for self-reported health grades and recent illness or accidents (see Figure 9). Across the full, male, and female samples, we generally fail to reject the nulls (at 5%) that ITT coefficients are equal, or jointly equal to zero (see Tables 4 and 5).[51] Finally, there is no evidence that teen women are less likely to have children (see Figure 9 and Table 5).

8. Welfare Analysis

The marginal value of public funds (MVPF) is the ratio of beneficiaries' willingness-to-pay (WTP) to the government's net program costs (Hendren and Sprung-Keyser, 2020). For two practical reasons, we estimate the MVPF for the combined 1999 and 2000 birth cohorts of indigenous students. First, program costs can be accurately calculated using disaggregated expenditure data since the 2009 program year (the first year of primary school eligibility for the

[50] "Hours worked" includes adults with zero hours, and so we report exponentiated Poisson coefficients that are interpreted as percentage changes in hours worked relative to the control.
[51] In Tables 4 and 5, there is a statistically significant ITT coefficient for adult health grade, but the event study plot shows that it is driven by a single birth cohort.

1999 cohort). Second, recent cohorts' ITT estimates have greater external validity for policy decisions.

We first discuss net government expenditures, inflating values to 2022 prices and applying a discount rate of 5.5% (SNI, 2024). The present value of grant-related expenditures is US$797 per *eligible* indigenous child.[52] (Tertiary grants account for just over 60% of the program costs, given the substantially higher value of the annual grants.) Increased schooling attainment leads to added school expenditures of US$2,452 per eligible child.[53] These are substantially offset by a fiscal externality, due to increased revenues from a 19% consumption tax, assuming a marginal propensity to consume of 0.5 (Barrero et al., 2020).[54] Overall, the present value of net government expenditures is US$155 per eligible child. The present value of increased after-tax earnings is US$29,482 per eligible child, implying a MVPF of 190.[55] The MVPF is still 16 under conservative assumptions, including a 7% discount rate, doubled administrative overhead for grants distribution, and 25% higher schooling costs.

9. Conclusions

Despite the ubiquity of educational transfers in Latin America, scant evidence exists on the long-run effects of childhood exposure to conditional cash transfers on adults' labor participation and earnings. Further, there is little causal evidence of how government policies might reduce economic inequality stemming from historical marginalization of Latin America's indigenous population. We use the scale-up of the Chile's *Beca Indígena* (Indigenous Grants) program as a natural experiment to study the impact of a large, government-implemented conditional cash transfer program on the education, migration, health, fertility, and labor market outcomes of

[52] We assume that program costs are incurred for the 1999 cohort between 2009 (5th grade primary) and 2020 (4th year of tertiary). They include grant-related expenditures across all grade levels, and 5% administrative overhead for grant distribution. Census-derived cohort size are the same as those used in Figure 1.
[53] The ITT estimates for each cohort are from equation (3), reported in Figure 3. We assume that school costs correspond to secondary education, incurred at 16 years of age. We estimate the annual expenditure per secondary student as the product of GDP per capita in 2022 prices, and the ratio of total government secondary expenditures to GDP per capita.
[54] We conservatively assume that no additional income taxes are paid, since monthly labor incomes below 0.825 million are not taxed in 2022, while incomes up to 1.8 million are taxed at 4% (https://www.sii.cl/).
[55] We calculate mean labor earnings for 18- to 65-year-olds in the 2022 CASEN survey, including adults with zero earnings (cf. Hendren and Sprung-Keyser, 2020). We multiply earnings by 0.73, or the ratio between mean indigenous earnings in pre-treatment cohorts and mean full sample earnings in the same cohorts. We adjust cross-sectional age-earnings profiles for 0.5% real wage growth, multiply age-specific earnings by the ITT estimates, and deduct tax consumption tax payments. We use ITT estimates for 1999 and 2000 from equation (3) and reported in Figure 3.

individuals who received the grants as children and adolescents. Birth cohorts of non-indigenous children never received the grants. Indigenous children only received grants if they were born after 1974. The average exposure of indigenous birth cohorts increased thereafter, reaching 2.4 years by the 2000 cohort. Given the program design, indigenous and non-indigenous children lived in the same communities. Thus, we can control for unobserved variables shared by children born in the same year and commune, estimating the difference-in-differences within birth communes.

We find that the most intensive exposure to *Beca Indígena*—among cohorts born from 1992 to 2000—increased formal schooling by 0.6 years and labor earnings by 22 percent. The labor supply of indigenous adults also increased, with a higher likelihood of any work in the last week (a 4 percentage points increase) and increased hours worked in a typical week (a 10% increase). There is no consistent evidence that earnings effects were the result of differential migration within Chile, improved adult health, or lower teen fertility among women. We show suggestive evidence, based on a narrower window of birth cohorts, that the gains may be explained by increased learning, as proxied by Chile's national testing data.

This successful, government-implemented program provides insights into the potential for targeted, school-aged interventions to fundamentally alter life choices and outcomes and reduce ethnic inequality in Latin America even without additional policy interventions in labor markets. The program's design, implementation, and context hint at the degree to which these results might be replicated elsewhere. First, the *Beca Indígena* program was consistently implemented and scaled-up by credible government agencies, including the Ministry of Education, JUNAEB, and CONADI. Second, the annual grants were both large and potentially renewable until tertiary schooling, providing unusually strong financial incentives for forward-looking households. Third, Chile's primary and secondary schools invested in school-based improvements over the treatment period, in ways that apparently promoted increased learning among indigenous students. Fourth, Chile enjoyed relatively strong economic growth and labor market performance over the 1990s and 2000s, suggesting that the welfare impact of transfers ultimately hinges on a robust job market for increasingly-skilled workers.

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Table 1: Descriptive statistics for the CASEN adult sample

	Indigenous = 0		Indigenous = 1	
	Mean (SD)	N	Mean (SD)	N
Female = 1	0.524	581,325	0.541	78,558
Years of completed schooling	11.90	579,112	10.89	78,308
	(3.70)		(3.76)	
Labor earnings (thousands/month)	455.5	570,233	344.9	76,801
	(890.2)		(550.6)	
Public health insurance (tier A) = 1	0.261	506,267	0.400	67,575
Worked last week = 1	0.673	581,325	0.643	78,558
Weekly hours worked	29.29	513,762	27.66	68,471
	(23.30)		(23.67)	
Migrated from birth commune = 1	0.378	581,325	0.334	78,558
Adult health grade	0.009	374,696	-0.069	48,909
	(0.997)		(1.017)	
Recent illness or accident = 1	0.138	575,093	0.146	77,689
Any live birth, 17 or younger = 1	0.131	201,669	0.158	29,320

Notes: The sample includes adults aged 22 to 57, born from 1965 to 2000, in CASEN household surveys collected in 2006, 2009, 2011, 2013, 2015, 2017, 2020, and 2022. Standard deviations are reported in parentheses for continuous variables. Sample sizes differ for some variables because some surveys do not collect some variables. “Public health insurance” and “weekly hours worked” are unavailable in the 2020 survey. “Adult health grade” is unavailable in the 2020 and 2022 surveys, and we exclude the 2006 survey because the question wording was substantially different. “Any live birth” is not available for men in any survey, or for women in the 2006, 2009, and 2020 surveys.

Table 2: Effects of indigenous grants exposure on adult schooling and labor earnings

	Years of schooling			Labor earnings (percent)		
	Full sample	Women	Men	Full sample	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intent-to-treat						
Indigenous, born 1975-1981	0.063	0.074	0.042	0.022	-0.005	0.043
	(0.039)	(0.061)	(0.047)	(0.019)	(0.024)	(0.023)
Indigenous, born 1982-1991	0.360**	0.393**	0.312**	0.081**	0.056	0.094**
	(0.081)	(0.089)	(0.102)	(0.026)	(0.039)	(0.028)
Indigenous, born 1992-2000	0.600**	0.724**	0.456**	0.215**	0.178**	0.231**
	(0.102)	(0.110)	(0.134)	(0.051)	(0.056)	(0.057)
p-value: equal	<0.001	<0.001	<0.001	0.001	0.001	0.008
p-value: zero	<0.001	<0.001	<0.001	<0.001	0.004	<0.001
Panel B: Average causal response						
Expected grant years	0.375**	0.426**	0.322**	0.127**	0.110**	0.135**
	(0.058)	(0.064)	(0.074)	(0.024)	(0.029)	(0.028)
Means: adults born 1965-1974						
$\bar{Y}_{IND}$	9.315	9.318	9.312	347.8	219.0	503.3
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-1.359	-1.428	-1.280	-134.0	-78.9	-189.5
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.127	-0.133	-0.121	-0.278	-0.265	-0.274
N (panels A and B)	657,133	345,153	311,270	646,382	339,298	305,811

Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: the coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The ACR estimate corresponds to $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Columns 4-6: Exponentiated coefficients from Poisson quasi-maximum-likelihood models and standard errors obtained via the delta method.

Table 3: Effects on schooling and labor earnings by communes' indigenous concentration

	Full sample	<10% indigenous	<10% indigenous, no land subsidies	≥10% indigenous	≥10% indigenous, excluding Temuco
	(1)	(2)	(3)	(4)	(5)
Panel A: Intent-to-treat,years of schooling					
Indigenous, born 1975-81	0.063	-0.018	-0.083	0.086	0.044
	(0.060)	(0.066)	(0.070)	(0.071)	(0.058)
Indigenous, born 1982-91	0.360**	0.224**	0.238*	0.407**	0.340**
	(0.078)	(0.082)	(0.093)	(0.093)	(0.062)
Indigenous, born 1992-00	0.600**	0.429**	0.450**	0.677**	0.565**
	(0.103)	(0.086)	(0.095)	(0.131)	(0.085)
p-value: equal	<0.001	<0.001	<0.001	<0.001	<0.001
p-value: zero	<0.001	<0.001	<0.001	<0.001	<0.001
Panel B: Average causal response, years of schooling					
Expected years of grants	0.375**	0.278**	0.301**	0.426**	0.364**
	(0.056)	(0.050)	(0.057)	(0.075)	(0.047)
Means: adults born 1965-1974					
$\bar{Y}_{IND}$	9.3	10.7	10.9	8.9	8.9
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-1.4	-0.1	-0.0	-1.5	-1.4
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.127	-0.010	-0.004	-0.142	-0.136
N (panels A and B)	657,133	443,250	361,380	213,883	201,637
Panel C: Intent-to-treat, labor earnings					
Indigenous, born 1975-81	0.022	0.054	0.039	0.007	0.006
	(0.019)	(0.041)	(0.046)	(0.022)	(0.024)
Indigenous, born 1982-91	0.081**	0.088*	0.094*	0.081**	0.073*
	(0.026)	(0.041)	(0.047)	(0.031)	(0.031)
Indigenous, born 1992-00	0.215**	0.219**	0.221**	0.219**	0.197**
	(0.051)	(0.071)	(0.084)	(0.067)	(0.065)
p-value: equal	0.001	0.064	0.119	0.003	0.006
p-value: zero	0.000	0.017	0.064	0.009	0.015
Panel D: Average causal response, labor earnings					
Expected years of grants	0.127**	0.120**	0.130**	0.133**	0.119**
	(0.029)	(0.040)	(0.045)	(0.037)	(0.035)
Means: adults born 1965-1974					
$\bar{Y}_{IND}$	347.8	423.8	437.1	326.2	328.3
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-134.0	-63.7	-70.5	-140.4	-135.4
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.278	-0.131	-0.139	-0.301	-0.292
N (panels C and D)	646,382	436,124	355,567	210,258	198,153
Birth communes in sample	314	204	168	110	109

Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). The dependent variable means correspond to indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Panel A: Coefficients are dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panel B: The ACR estimate corresponds to $\hat{\tau}$ in equation (2). Panels C and D: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained via the delta method. Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974.

Table 4: Effects of indigenous grants exposure on other dependent variables

	Public health insurance (tier A)	Worked last week	Weekly hours worked (%)	Migrated from birth commune	Adult health grade	Recent illness or accident
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intent-to-treat						
Indigenous, born 1975-1981	-0.007	0.012**	0.019*	0.012*	0.006	-0.004
	(0.006)	(0.004)	(0.009)	(0.005)	(0.015)	(0.004)
Indigenous, born 1982-1991	-0.017**	0.018**	0.038**	0.010*	0.024	-0.004
	(0.006)	(0.006)	(0.014)	(0.004)	(0.015)	(0.004)
Indigenous, born 1992-2000	-0.043**	0.043**	0.099**	0.009	0.061**	-0.007
	(0.007)	(0.011)	(0.024)	(0.008)	(0.022)	(0.007)
p-value: equal	<0.001	0.003	<0.001	0.944	0.063	0.885
p-value: zero	<0.001	0.001	<0.001	0.061	0.050	0.664
Panel B: Average causal response						
Expected grant years	-0.025**	0.023**	0.058**	0.003	0.033*	-0.002
	(0.005)	(0.007)	(0.015)	(0.005)	(0.013)	(0.003)
Means: adults born 1965-1974						
$\bar{Y}_{IND}$	0.440	0.661	28.558	0.368	-0.308	0.171
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	0.161	-0.040	-2.365	-0.046	-0.087	0.011
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	0.575	-0.057	-0.076	-0.111	0.396	0.071
N	573,445	659,595	581,448	659,595	350,485	652,494

Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panel A: 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 ACR estimate corresponds to $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Column 3: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained via the delta method.

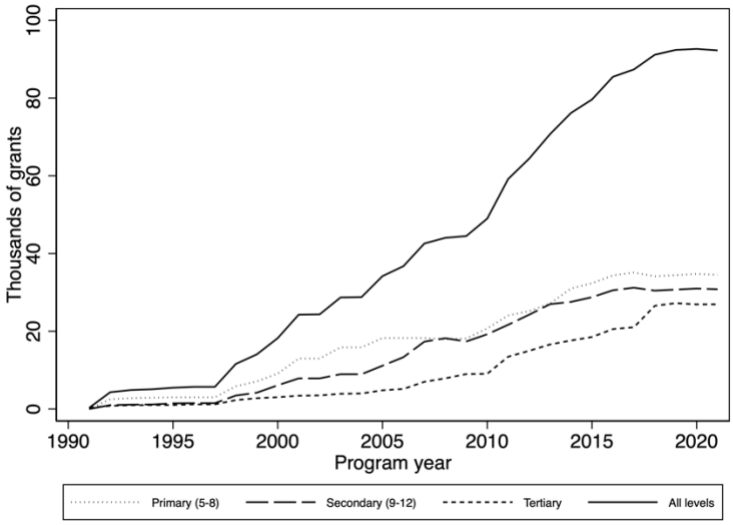
Table 5: Effects of indigenous grants exposure on other dependent variables (by gender)

	Public health insurance (tier A)	Worked last week	Weekly hours worked (%)	Migrated from birth commune	Adult health grade	Recent illness or accident	Any live birth, 17 or younger
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Intent-to-treat (women)							
Indigenous, born 1975-1981	0.003	0.004	0.008	0.006	0.015	-0.011*	0.014
	(0.008)	(0.008)	(0.021)	(0.007)	(0.026)	(0.005)	(0.011)
Indigenous, born 1982-1991	-0.010	0.010	0.026	0.007	0.036	-0.005	0.006
	(0.007)	(0.010)	(0.022)	(0.006)	(0.018)	(0.005)	(0.008)
Indigenous, born 1992-2000	-0.039**	0.037**	0.099*	0.005	0.047*	-0.010	-0.012
	(0.007)	(0.012)	(0.039)	(0.011)	(0.022)	(0.008)	(0.012)
p-value: equal	<0.001	0.018	0.041	0.937	0.567	0.440	0.119
p-value: zero	<0.001	0.025	0.077	0.560	0.146	0.173	0.161
Panel B: Average causal response (women)							
Expected grant years	-0.021**	0.021**	0.055*	0.002	0.035	-0.002	-0.005
	(0.005)	(0.007)	(0.022)	(0.006)	(0.019)	(0.004)	(0.007)
Means: female adults born 1965-1974							
$\bar{Y}_{IND}$	0.480	0.496	19.669	0.382	-0.407	0.200	0.163
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	0.161	-0.044	-2.194	-0.041	-0.082	0.011	0.028
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	0.504	-0.082	-0.100	-0.097	0.251	0.060	0.209
N (panel A and B)	301,333	346,382	303,476	346,382	184,189	342,716	230,292
Panel C: Intent-to-treat (men)							
Indigenous, born 1975-1981	-0.020*	0.024**	0.031*	0.020**	-0.002	0.005	—
	(0.008)	(0.006)	(0.012)	(0.007)	(0.025)	(0.005)	
Indigenous, born 1982-1991	-0.027*	0.025*	0.046**	0.013	0.012	-0.005	—
	(0.011)	(0.010)	(0.015)	(0.007)	(0.027)	(0.006)	
Indigenous, born 1992-2000	-0.048**	0.048**	0.097**	0.010	0.073**	-0.006	—
	(0.013)	(0.015)	(0.028)	(0.009)	(0.026)	(0.008)	
p-value: equal	0.061	0.204	0.052	0.645	0.042	0.214	
p-value: zero	0.002	0.003	0.008	0.082	0.096	0.378	
Panel D: Average causal response (men)							
Expected grant years	-0.029**	0.023*	0.058**	0.002	0.034	-0.003	—
	(0.009)	(0.009)	(0.017)	(0.006)	(0.021)	(0.004)	
Means: male adults born 1965-1974							
$\bar{Y}_{IND}$	0.393	0.858	39.111	0.351	-0.186	0.137	—
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	0.158	-0.025	-2.111	-0.053	-0.087	0.010	—
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	0.677	-0.029	-0.051	-0.131	0.877	0.076	—
N (panel C and D)	271,282	312,506	276,449	312,506	165,540	309,069	

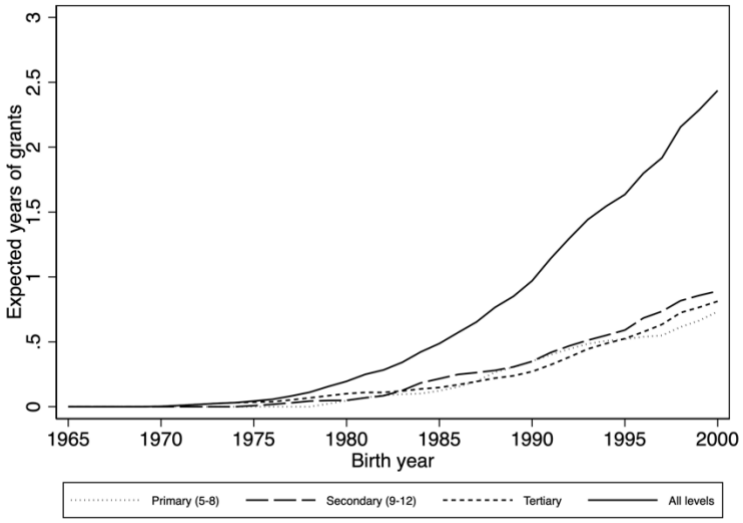
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. ** (*) indicates statistical significance at 1% (5%). Panels A and C: Coefficient estimates correspond to dummy variables for indigenous adults in the specified birth years, but otherwise include the same controls as equation (1). Panels B and D: The ACR estimate corresponds to $\hat{\tau}$ in equation (2). Means: indigenous (IND) and non-indigenous (NIND) adults born between 1965 and 1974. Column 3: Exponentiated coefficients from Poisson quasi-maximum-likelihood models, and standard errors obtained via the delta method.

Figure 1: Scale-up of the indigenous grants program

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



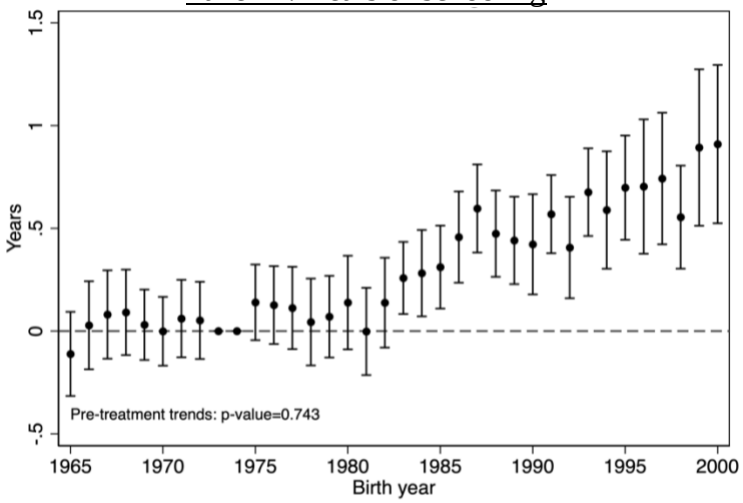
Panel B: Expected years of indigenous grants, by birth year



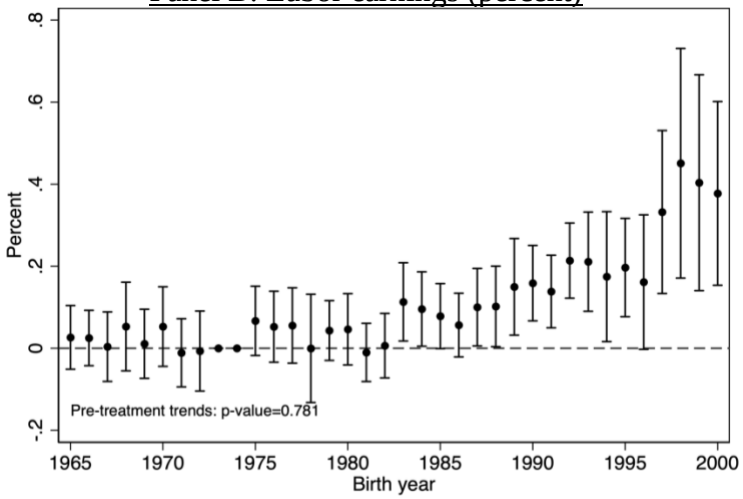
Notes: Panel A: 1991 to 2005 sums from decrees of the Ministry of Education; 2006 to 2021 sums from annual reports of the implementing agency (JUNAEB) to a supervisory unit of the Ministry of Finance. Panel B: The sum of age-specific probabilities of receiving a grant—between ages 10 to 21—for each birth cohort (see the text for details, and Appendix Table A2 for the underlying probability estimates).

Figure 2: Event study plots for schooling and labor earnings

Panel A: Years of schooling

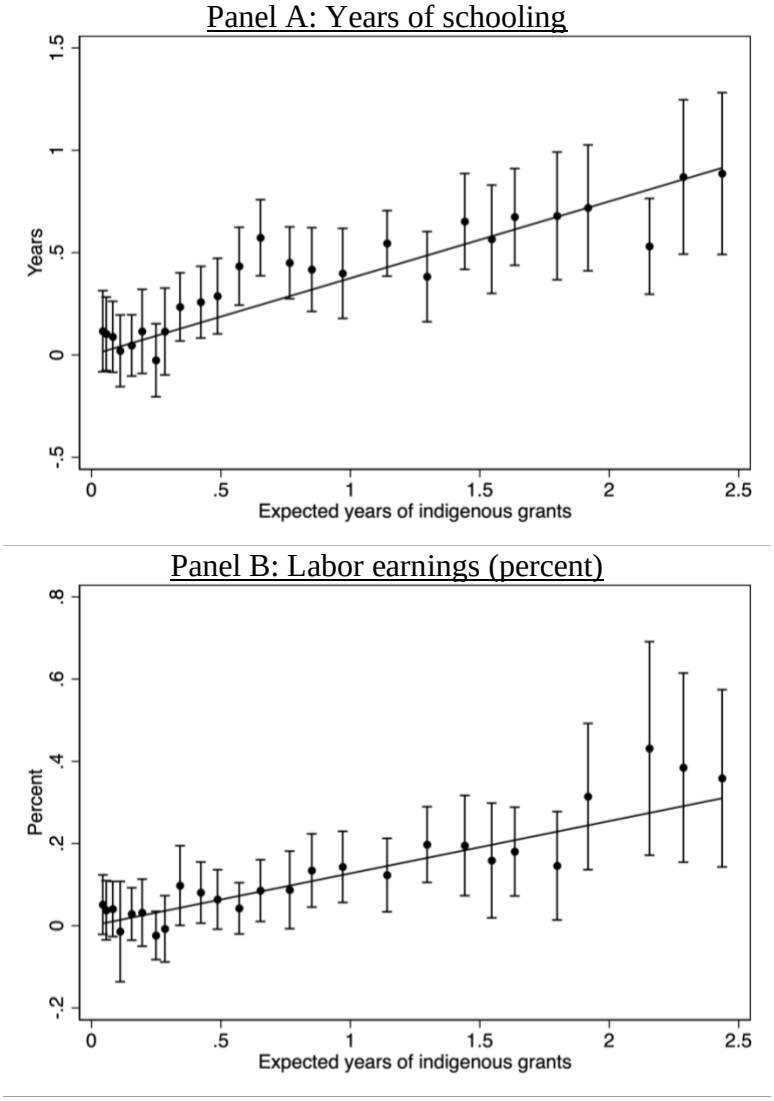


Panel B: Labor earnings (percent)



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 reported in Table 2. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$. Panel A: The circles are $\hat{\gamma}_e$ and $\hat{\lambda}_e$ from OLS estimates of equation (1), with pointwise 95% confidence intervals. Panel B: The circles are exponentiated coefficients from Poisson quasi-maximum likelihood estimates of equation (1).

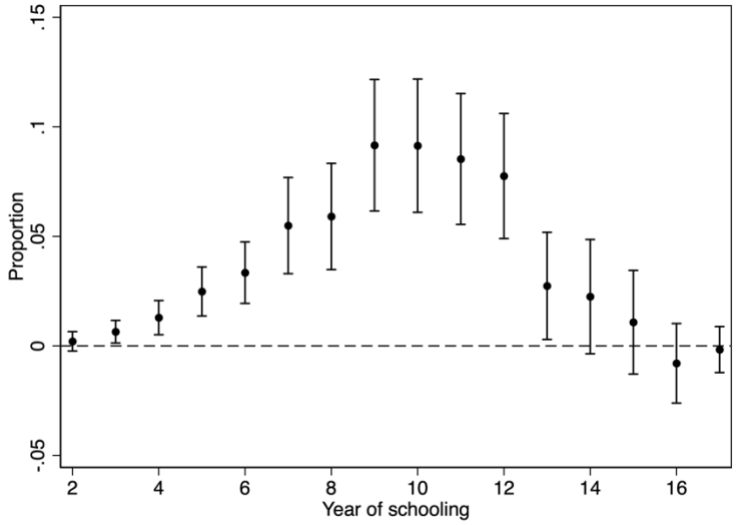
Figure 3: Average causal response of expected years of indigenous grants for schooling and labor earnings



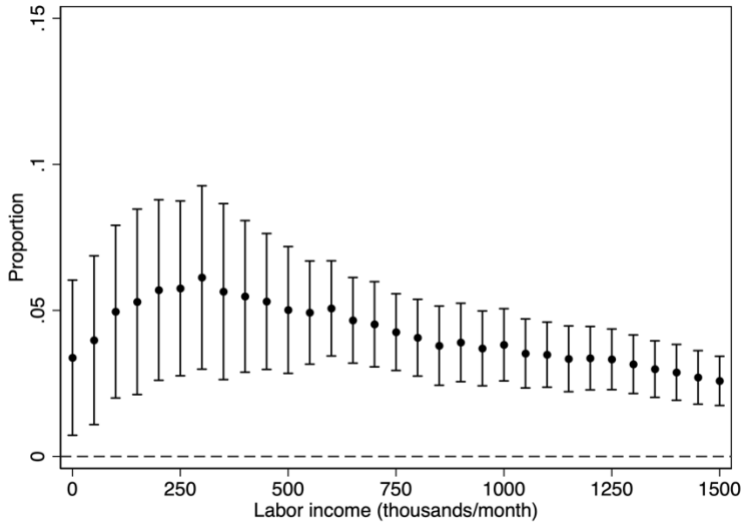
Notes: Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. Sample sizes in each panel are the same as regressions reported in Table 2. Panel A: The solid line is a linear prediction ($E_{ic} \times \hat{\tau}$) of the dependent variable using the ACR estimate in Table 2. The circles are $\hat{\lambda}_e$ from equation (3), with pointwise 95% confidence intervals, graphed against the corresponding expected years of grants. Panel B: The solid line is a linear prediction using the exponentiated coefficient from a Poisson quasi-maximum likelihood model. The circles are $\hat{\lambda}_e$, estimated as exponentiated coefficients from a Poisson quasi-maximum likelihood model, from equation (3), with pointwise 95% confidence intervals, graphed against the corresponding expected years of grants.

Figure 4: Intent-to-treat estimates across the distribution of schooling and labor earnings

Panel A: Probability of attaining at least *x* years of schooling

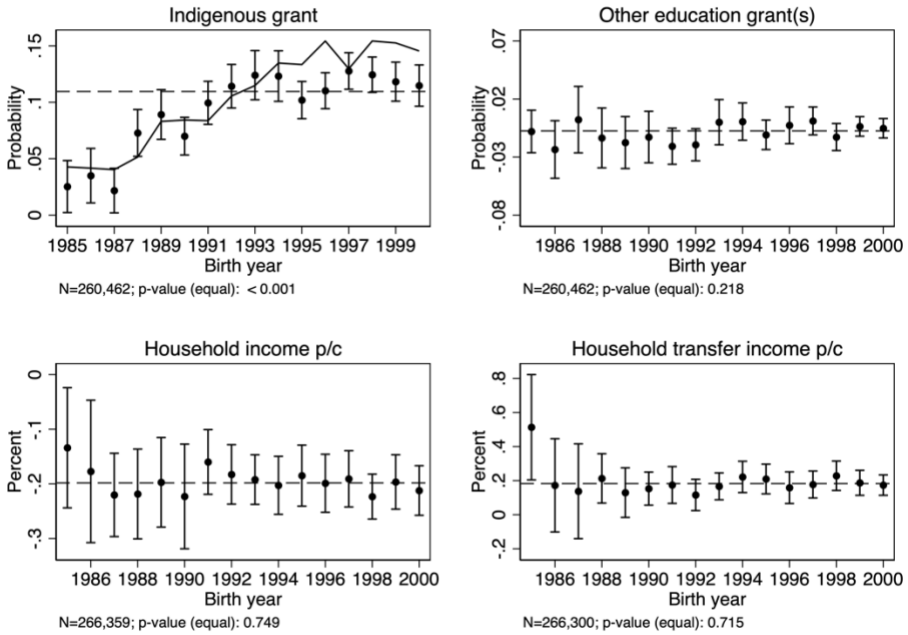


Panel B: Probability of earning more than *x* pesos per month



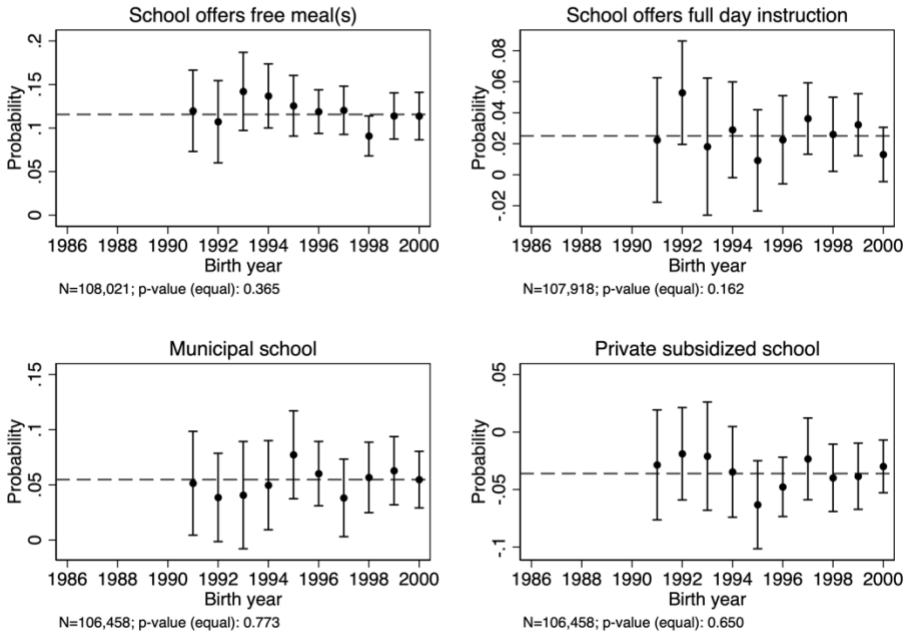
Notes: The ITT estimates correspond to indigenous adults born between 1992 and 2000, using the same specification as Table 2 (panels A and C). Panel A: Each circle corresponds to the dependent variable $1\{Y \geq x\}$, where Y is years of schooling and x is the value on the x-axis. Panel B: Each circle corresponds to the dependent variable $1\{Y > x\}$, where Y is labor earnings and x is the value on the x-axis.

Figure 5: Post-treatment trends in relative household inputs



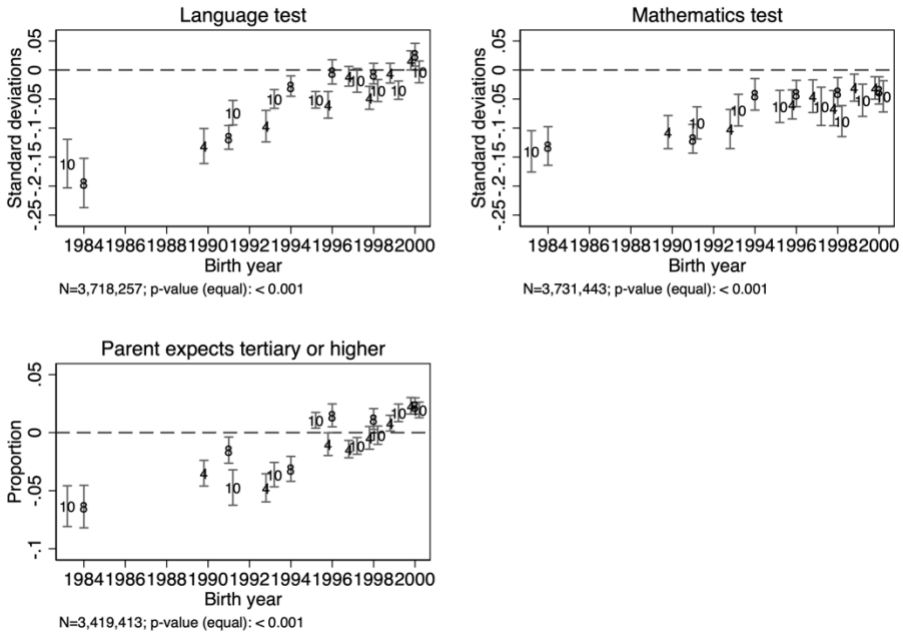
Notes: Each panel uses the auxiliary CASEN sample of children and adolescents (see Appendix Table A5). The circles are $\hat{\lambda}_e$ from equation (4), with pointwise 95% confidence intervals. Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. P-values correspond to the null hypothesis that γ_e are jointly equal. The dashed lines are estimates in which the γ_e are constrained to be equal for all birth years. In the top-left panel, the solid line is the mean probability of receiving an indigenous grant, estimated with administrative data (see text for details). The bottom panels report exponentiated coefficients from Poisson quasi-maximum-likelihood models.

Figure 6: Post-treatment trends in relative school inputs



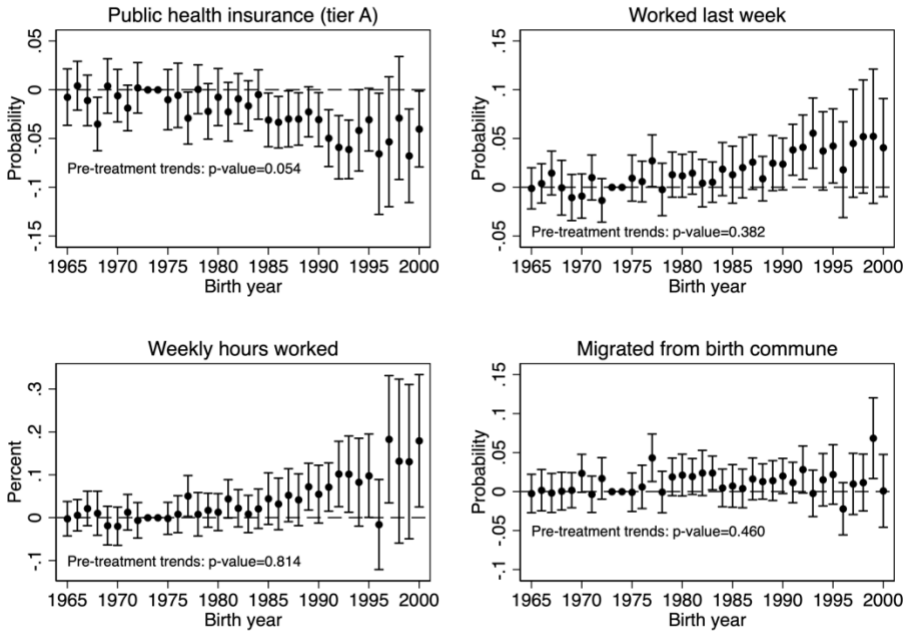
Notes: Each panel uses the auxiliary CASEN sample of children and adolescents (see Appendix Table A5). The circles are $\hat{\lambda}_e$ from equation (4), with pointwise 95% confidence intervals. Standard errors are adjusted for multi-way clustering by commune of birth and commune of residence as adults. P-values correspond to the null hypothesis that γ_e are jointly equal. The dashed lines are estimates in which the γ_e are constrained to be equal for all birth years.

Figure 7: Test scores and parent expectations



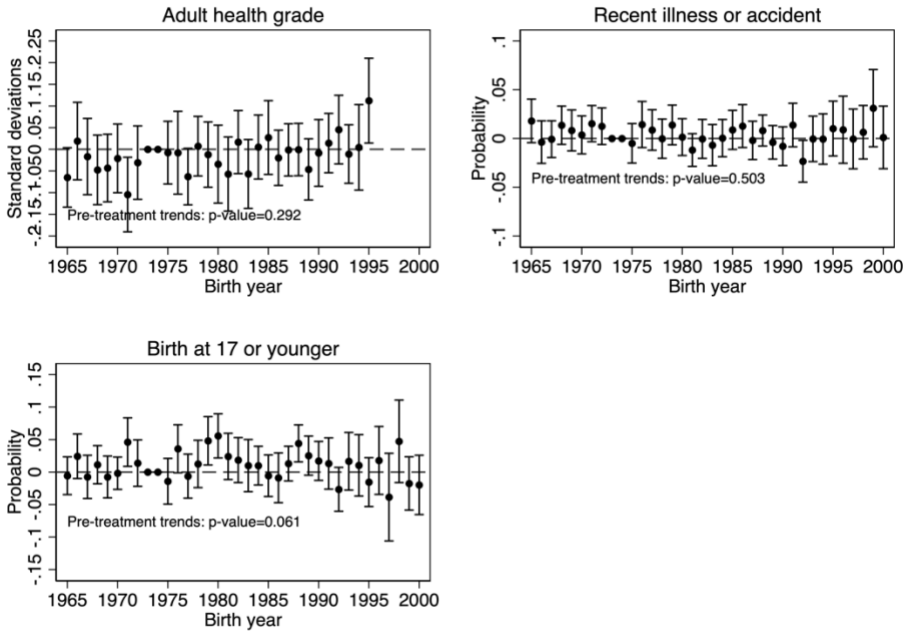
Notes: The sample is all SIMCE observations for the indicated birth cohorts and grades that include both the relevant outcome and mothers' indigenous status (see Appendix Table A6). The labeled midpoints of confidence intervals are $\hat{\lambda}_{eh}$ from equation (5), with pointwise 95% confidence intervals. The midpoint label of 4, 8, or 10 indicates the grade level in which the test was taken. Standard errors are adjusted for clustering by the communes of schools. P-values correspond to the null hypothesis that coefficients are jointly equal.

Figure 8: Event study plots for additional dependent variables (labor and migration)



Notes: The circles are $\hat{\gamma}_e$ and $\hat{\lambda}_e$ from OLS estimates of equation (1), with pointwise 95% confidence intervals. 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 reported in Table 4. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$. Weekly hours worked: The circles are exponentiated coefficients from Poisson quasi-maximum likelihood estimates of equation (1).

Figure 9: Event study plots for additional dependent variables (health and fertility)



Notes: The circles are $\hat{\gamma}_e$ and $\hat{\lambda}_e$ from OLS estimates of equation (1), with pointwise 95% confidence intervals. 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 reported in Table 4. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$.

Appendix A: Additional Tables and Figures

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

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

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

Table A2: Estimated probabilities of receiving indigenous grants, by birth year and nominal grade of each birth cohort

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

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

Table A3: Communes created between 1981 and 2004

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

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

Table A4: Effects on schooling and labor earnings(without assumption that $E_{ic} = 0$ between 1970 and 1974)

	Years of schooling			Labor earnings (%)		
	Full sample	Women	Men	Full sample	Women	Men
Panel A: Intent-to-treat						
Indigenous, born 1970-1981	0.030	0.072	-0.017	-0.001	-0.011	0.010
	(0.062)	(0.078)	(0.075)	(0.020)	(0.026)	(0.025)
Indigenous, born 1982-1991	0.354**	0.413**	0.279**	0.068*	0.049	0.079*
	(0.095)	(0.104)	(0.107)	(0.028)	(0.041)	(0.036)
Indigenous, born 1992-2000	0.594**	0.744**	0.423**	0.201**	0.171**	0.215**
	(0.115)	(0.134)	(0.126)	(0.050)	(0.060)	(0.059)
p-value: equal	<0.001	<0.001	<0.001	<0.001	0.001	0.002
p-value: zero	<0.001	<0.001	<0.001	0.001	0.003	0.003
Panel B: Average causal response						
Expected grant years	0.376**	0.428**	0.322**	0.128**	0.110**	0.135**
	(0.057)	(0.063)	(0.064)	(0.029)	(0.032)	(0.033)
Means: adults born 1965-1969						
$\bar{Y}_{IND}$	8.9	8.9	9.3	339.7	207.1	503.3
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-1.4	-1.5	-1.3	-124.5	-72.2	-189.5
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.134	-0.146	-0.121	-0.268	-0.258	-0.274
N	657,133	345,153	311,270	646,382	339,298	305,811

Notes: The table reports results for a modified version of equation (1) in Table 2, in which $E_{ic}=0$ is not assumed to be zero between 1970 and 1974 (the values are reported in Appendix Table A2). See section 5 for details. The notes are otherwise identical to Table 2.

Table A5: Descriptive statistics for the auxiliary CASEN sample of children and adolescents

	Indigenous = 0		Indigenous = 1			
			All		Indigenous grant = 0	Indigenous grant = 1
	Mean (SD)	N	Mean (SD)	N	Mean (SD)	Mean (SD)
Panel A: Ages 6–21						
Female = 1	0.491	229374	0.498	37049	0.488	0.561
Indigenous grant = 1	0.002	224386	0.136	36138	—	—
P(indigenous grant)	0	224386	0.124	36138	—	—
	—		(0.057)			
Other school grant(s) or scholarship(s) = 1	0.085	224386	0.069	36138	0.069	0.069
Household total income p/c (thousands/month)	239.2	229372	175.8	37049	180.5	137.5
	(355.5)		(198.8)		(204.9)	(128.7)
Household transfer income p/c (thousands/month)	6.95	229372	9.56	37049	8.83	12.11
	(12.07)		(14.34)		(13.61)	(15.04)
Public health insurance (tier A) = 1	0.374	221530	0.565	35742	0.541	0.717
Migrated from birth commune = 1	0.231	229374	0.213	37049	0.215	0.194
Recent illness or accident = 1	0.100	227338	0.103	36708	0.102	0.108
Panel B: Ages 6–15						
Child receives free school meal(s) = 1	0.635	92604	0.828	15441	0.814	0.898
Child attends full day schedule = 1	0.761	92512	0.862	15430	0.854	0.903
Child enrolled in municipal school = 1	0.595	91254	0.625	15227	0.623	0.637
Child enrolled in subsidized private school = 1	0.362	91254	0.365	15227	0.367	0.352
Child pays any school tuition = 1	0.282	92578	0.145	15429	0.153	0.104
Child's primary or secondary school ever in FSD = 1	0.759	91256	0.841	15228	0.836	0.873
Panel C: Ages 6–13						
Child's primary school ever in P900 = 1	0.342	59186	0.433	9702	0.427	0.469
Child's primary school ever in MECE-Rural = 1	0.066	59186	0.190	9702	0.184	0.219

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

Table A6: SIMCE observations in birth years from 1983 to 2000

Panel A: Number of observations with at least one language or mathematics score

	Grade			
Birth year	4	8	10	Total
1983			186,498 [b]	186,498
1984		220,540 [b]		220,540
1990	288,939 [b]			288,939
1991		279,619 [a]	244,568	524,187
1993	254,205 [a]		229,988	484,193
1994		259,982		259,982
1995			228,854	228,854
1996	254,168 [a]	235,115		489,283
1997	249,351		207,425	456,776
1998	242,809	226,233	201,800	670,842
1999	238,022		190,445	428,467
2000	222,605	224,551 [a]	205,111	652,267
Total	1,750,099	1,446,040	1,694,689	4,890,828

Panel B: Proportion of observations in Panel A with non-missing indigenous variable

	Grade			
Birth year	4	8	10	Total
1983			0.72 [b]	0.72
1984		0.85 [b]		0.85
1990	0.81 [b]			0.81
1991		0.56 [a]	0.73	0.64
1993	0.56 [a]		0.72	0.64
1994		0.78		0.78
1995			0.80	0.80
1996	0.65	0.75		0.70
1997	0.89		0.87	0.88
1998	0.82	0.87	0.83	0.84
1999	0.88		0.92	0.90
2000	0.86	0.85 [a]	0.91	0.87
Total	0.78	0.77	0.81	0.79

[a] These rounds did not measure the indigenous status of mothers or fathers on the parent survey. We impute indigenous status from matched observations—using a masked identifier—in SIMCE rounds in the same row.
[b] These rounds did not measure the indigenous status of fathers on the parent survey.

Notes: Each cell in panel A reports the number of observations with a language score, a mathematics score, or both. The birth year of each round correspond to Y–9 for grade 4, Y–13 for grade 8, and Y–15 for grade 10, where Y is the school year.

Table A7: Variables associated with receipt of an indigenous grant

	Dependent variable: Indigenous grant = 1; 0 =not	
	(1)	(2)
Female = 1	0.025**	0.025**
	(0.004)	(0.004)
Log (household income per capita)	-0.025**	-0.018**
	(0.004)	(0.003)
Public health insurance (A) = 1	0.051**	0.039**
	(0.006)	(0.006)
Age=7	0.013	-0.006
	(0.016)	(0.017)
Age=8	0.021	0.033
	(0.017)	(0.017)
Age=9	0.011	0.008
	(0.010)	(0.014)
Age=10	0.046**	0.048**
	(0.015)	(0.015)
Age=11	0.091**	0.089**
	(0.016)	(0.017)
Age=12	0.153**	0.156**
	(0.022)	(0.023)
Age=13	0.124**	0.123**
	(0.015)	(0.016)
Age=14	0.126**	0.130**
	(0.017)	(0.018)
Age=15	0.100**	0.105**
	(0.014)	(0.015)
Age=16	0.119**	0.116**
	(0.014)	(0.015)
Age=17	0.093**	0.093**
	(0.012)	(0.012)
Age=18	0.069**	0.071**
	(0.011)	(0.012)
Age=19	0.018*	0.014
	(0.008)	(0.008)
Age=20	0.034**	0.028
	(0.013)	(0.015)
N	36,138	36,138
Survey year fixed effects	Yes	Yes
Birth year fixed effects	Yes	No
Birth-year-by-birth-commune fixed effects	No	Yes

Notes: The sample includes indigenous children and adolescents (ages 6 to 21), born from 1985 to 2000, in CASEN household surveys collected in 2006, 2009, 2011, 2013, 2015, 2017, and 2020. The dependent variable is a dummy variable indicating that an individual currently receives an indigenous grant. The excluded age category is age = 6. Age = 21 is excluded because it is absorbed by other fixed effects, given the structure of the dataset. Both regressions use survey weights.

Table A8: Effects on schooling and labor earnings (excluding 1999 and 2000 birth years)

	Years of schooling			Labor earnings (%)		
	Full sample	Women	Men	Full sample	Women	Men
Panel A: Intent-to-treat						
Indigenous, born 1975-1981	0.059	0.122	0.060	0.025	-0.002	0.056
	(0.060)	(0.086)	(0.074)	(0.020)	(0.027)	(0.030)
Indigenous, born 1982-1991	0.356**	0.430**	0.302**	0.099**	0.074	0.117**
	(0.080)	(0.092)	(0.100)	(0.031)	(0.048)	(0.033)
Indigenous, born 1992-1998	0.541**	0.731**	0.343**	0.222**	0.181*	0.244**
	(0.113)	(0.136)	(0.132)	(0.060)	(0.072)	(0.067)
p-value: equal	<0.001	<0.001	0.021	0.003	0.019	0.031
p-value: zero	<0.001	<0.001	0.017	0.003	0.044	<0.001
Panel B: Average causal response						
Expected grant years	0.367**	0.443**	0.284**	0.139**	0.112**	0.157**
	(0.067)	(0.079)	(0.080)	(0.034)	(0.041)	(0.037)
Means: adults born 1965-1974						
$\bar{Y}_{IND}$	9.3	9.3	9.3	347.8	219.0	503.3
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-1.4	-1.4	-1.3	-134.0	-78.9	-189.5
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.127	-0.133	-0.121	-0.278	-0.265	-0.274
N	639,148	326,996	292,896	631,427	315,634	289,054

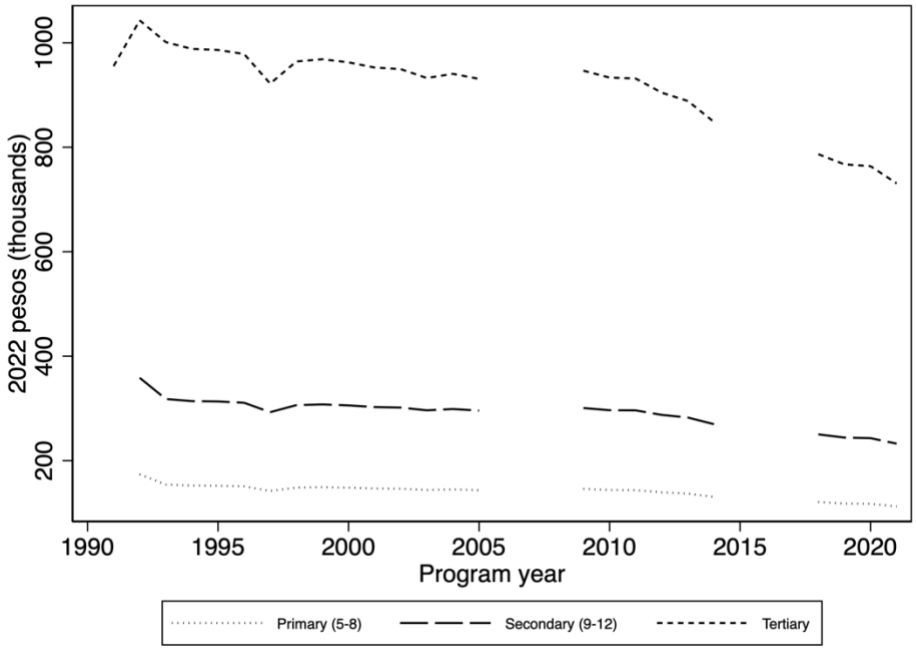
Notes: The table reports results for a modified sample of equation (1) in Table 2, which excludes birth years in 1999 and 2000. See section 6 for details. The notes are otherwise identical to Table 2.

Table A9: Effects on schooling and labor earnings (with additional fixed effects)

	Years of schooling			Labor earnings (%)		
	Full sample	Women	Men	Full sample	Women	Men
Panel A: Intent-to-treat						
Indigenous, born 1975-1981	0.053	0.114	0.057	0.025	-0.002	0.056
	(0.060)	(0.085)	(0.074)	(0.020)	(0.027)	(0.030)
Indigenous, born 1982-1991	0.358**	0.428**	0.305**	0.099**	0.074	0.116**
	(0.080)	(0.092)	(0.101)	(0.031)	(0.048)	(0.033)
Indigenous, born 1992-2000	0.552**	0.721**	0.400**	0.232**	0.189**	0.255**
	(0.111)	(0.129)	(0.133)	(0.061)	(0.072)	(0.069)
p-value: equal	<0.001	<0.001	0.010	0.002	0.012	0.025
p-value: zero	<0.001	<0.001	0.010	0.002	0.028	<0.001
Panel B: Average causal response						
Expected grant years	0.354**	0.416**	0.302**	0.142**	0.115**	0.158**
	(0.064)	(0.071)	(0.078)	(0.034)	(0.040)	(0.038)
Means: adults born 1965-1974						
$\bar{Y}_{IND}$	9.3	9.3	9.3	347.8	219.0	503.3
$\bar{Y}_{IND} - \bar{Y}_{NIND}$	-1.4	-1.4	-1.3	-134.0	-78.9	-189.5
$(\bar{Y}_{IND} - \bar{Y}_{NIND}) / \bar{Y}_{NIND}$	-0.127	-0.133	-0.121	-0.278	-0.265	-0.274
N	649,977	332,629	297,915	636,590	318,062	291,515

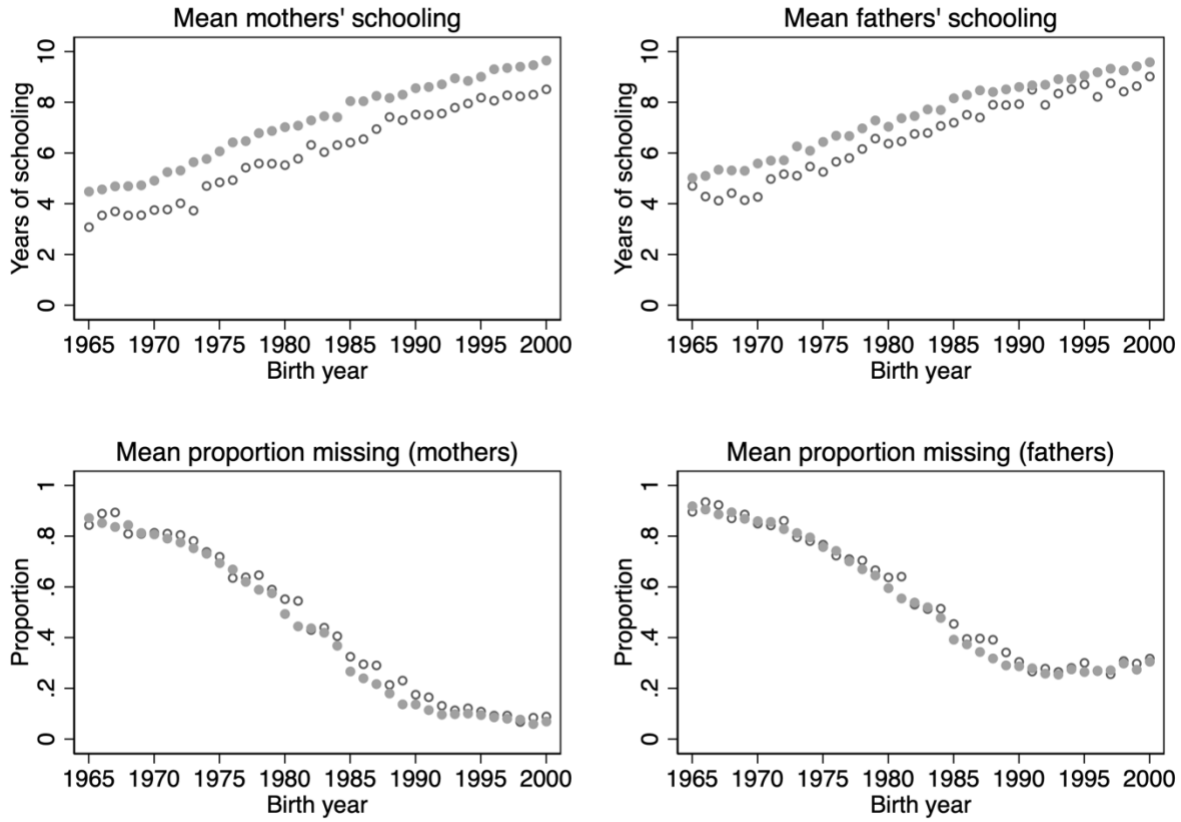
Notes: The table reports results for a modified version of equation (1) in Table 2, including fixed effects for all combinations of birth year, birth commune, and survey year (δ_{cmt}). See section 6 for details. The notes are otherwise identical to Table 2.

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



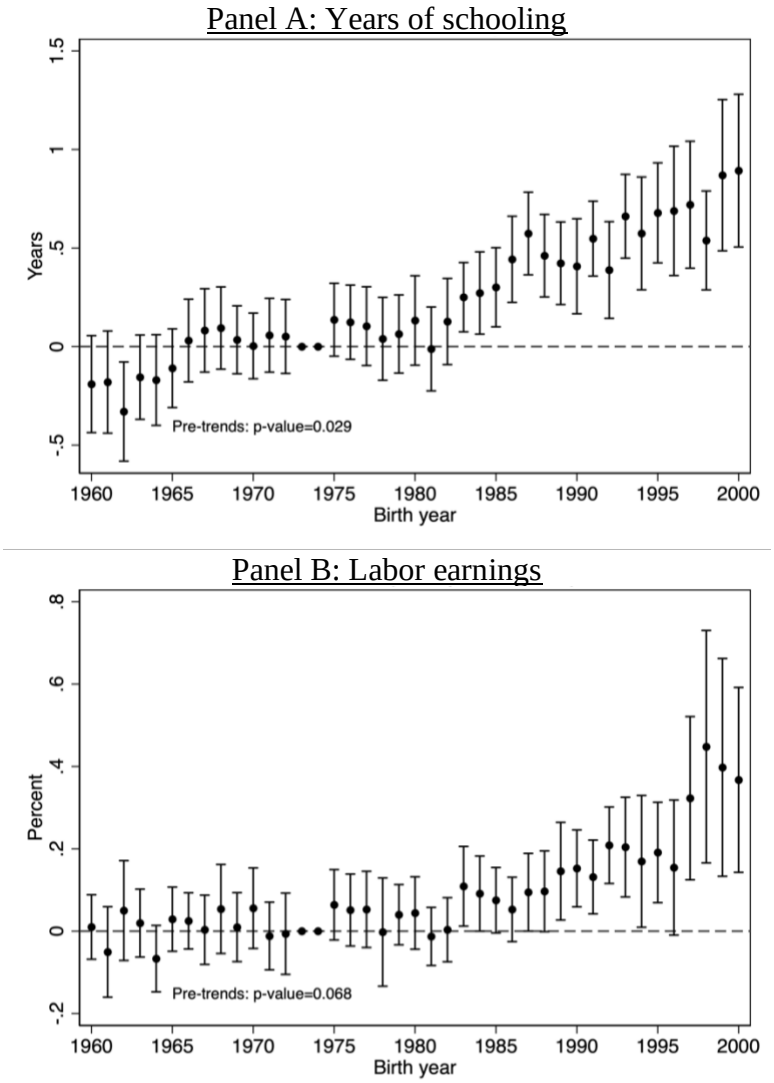
Notes: Each line plots the annual value (in 2022 pesos) of primary, secondary, or tertiary indigenous grants. Missing segments indicate years with missing data in official sources cited in the notes to Table 1.

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



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

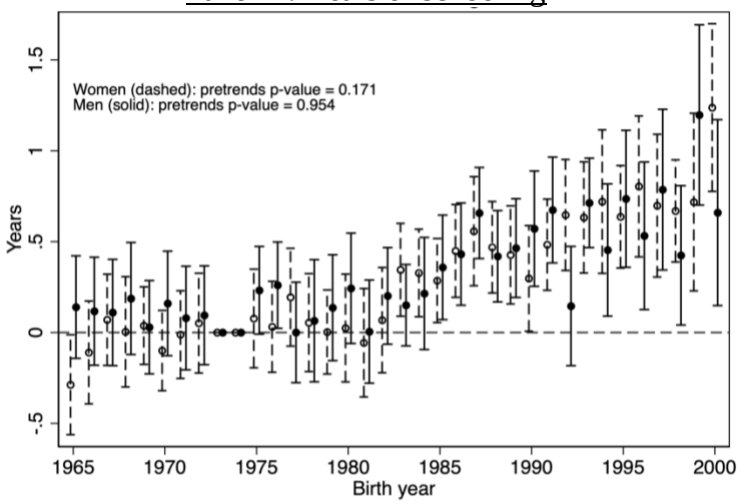
Figure A3: Event study plots with earlier birth years (1960 to 2000)



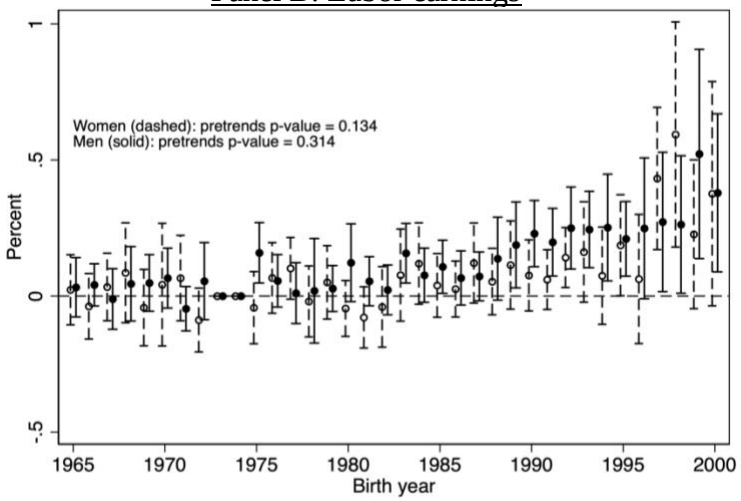
Notes: The sample include adults in birth cohorts from 1960 to 2000. The notes are otherwise identical to those in Figure 2.

Figure A4: Event study plots for schooling and labor income (female and male samples)

Panel A: Years of schooling

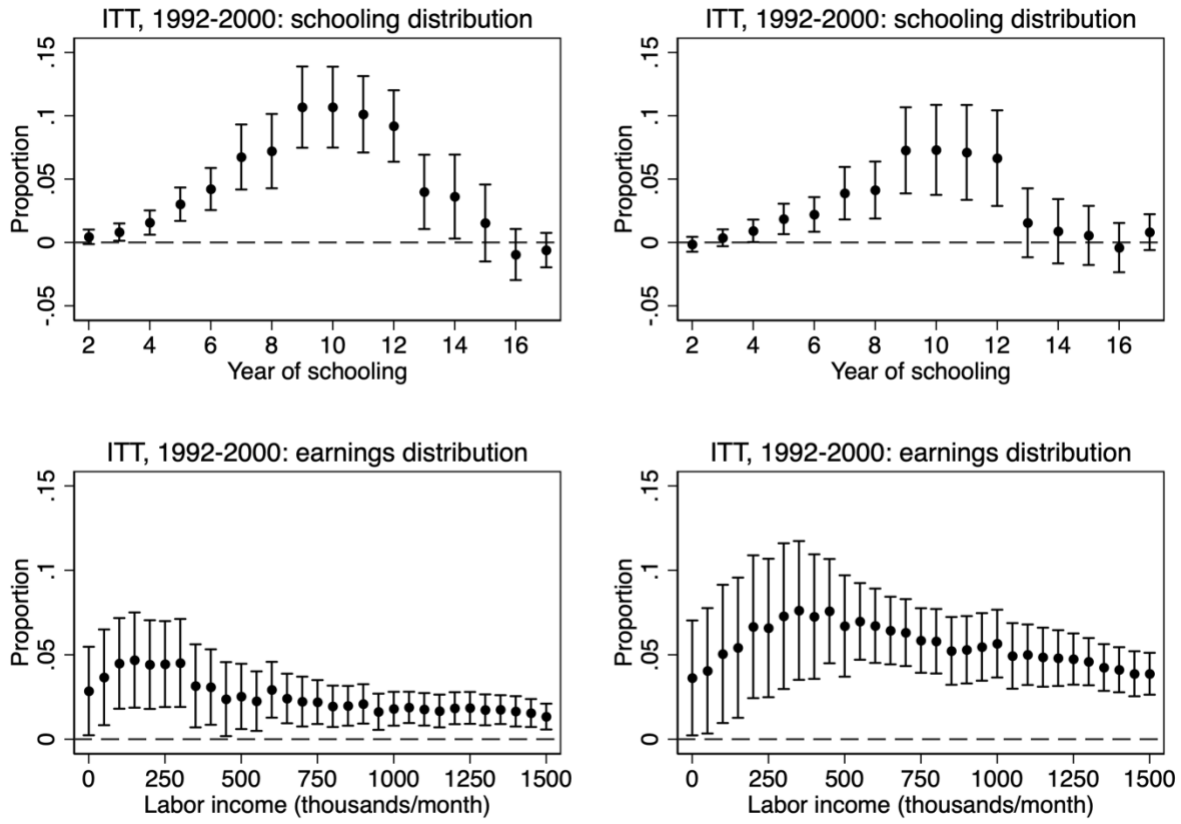


Panel B: Labor earnings



Notes: In panel A, the circles are $\hat{\gamma}_e$ and $\hat{\lambda}_e$ from OLS estimates of equation (1), with pointwise 95% confidence intervals. In panel B, the circles are exponentiated coefficients from Poisson quasi-maximum likelihood estimates of equation (1). 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 reported in Table 2. P-values corresponds to the null hypothesis that $\gamma_e = 0 \; \forall \; e$.

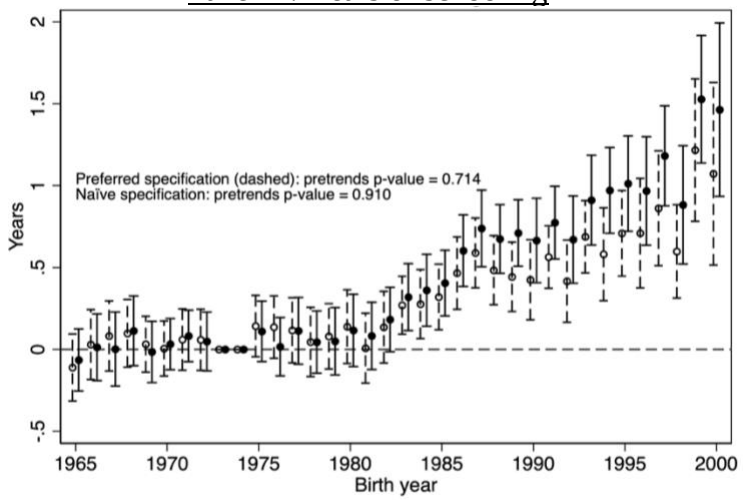
Figure A5: ITT estimates across the distributions of schooling and labor earnings (female and male samples)



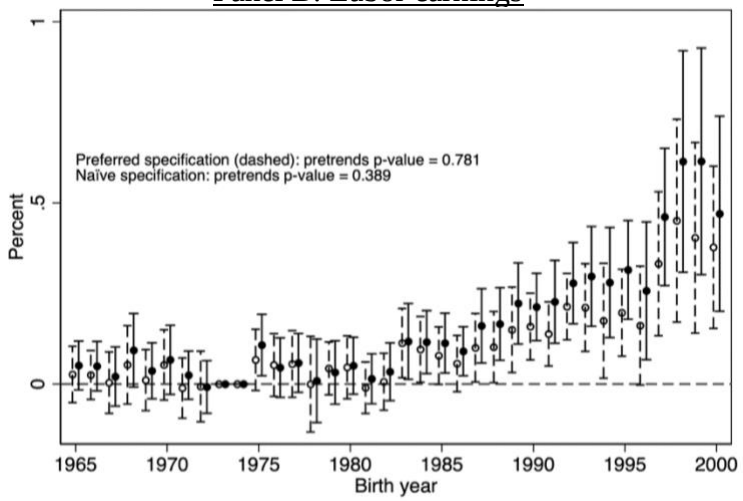
Notes: The left panels include the sample of women, and the right panels include the sample of men. Each circle in the top panels corresponds to the dependent variable $1\{Y \geq x\}$, where Y is years of schooling and x is the value on the x-axis. Each circle in the bottom panels corresponds to the dependent variable $1\{Y > x\}$, where Y is labor earnings (in thousands of 2022 pesos per month) and x is the value on the x-axis. The circles are intent-to-treat coefficients for a variable indicating indigenous adults born between 1992 and 2000, from models using the same specification as Table 2 (panels A and C).

Figure A6: Event study plots for schooling and labor income, by preferred and naïve specifications

Panel A: Years of schooling



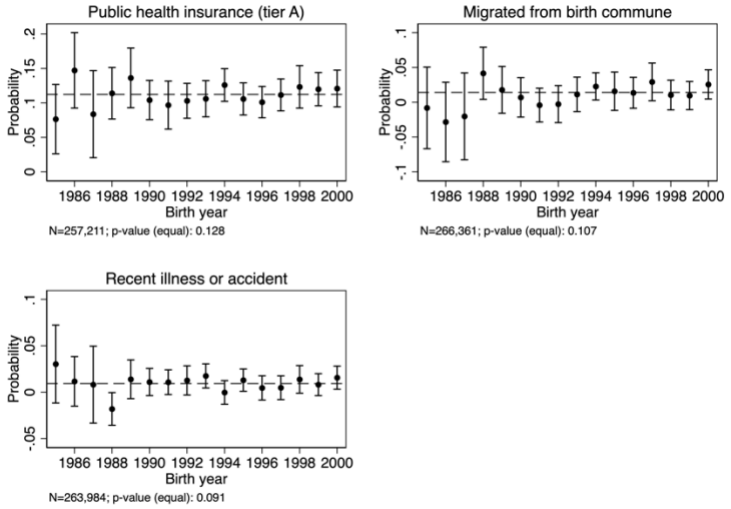
Panel B: Labor earnings



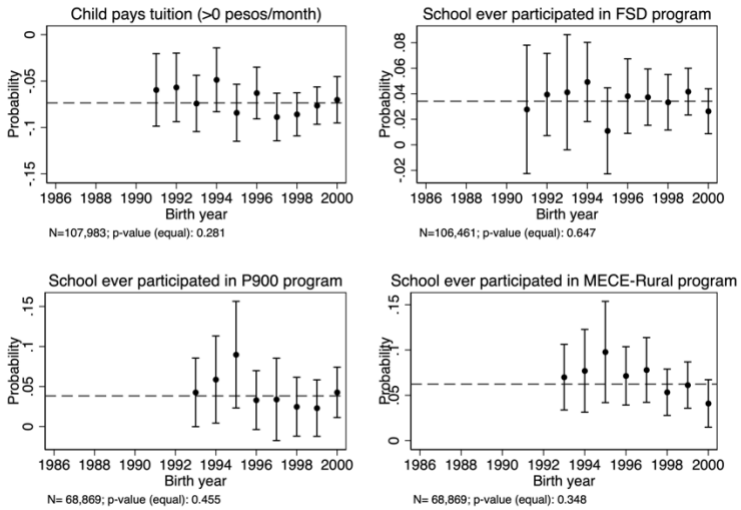
Notes: The preferred specification is the same as equation (1), while the naïve specification omits all birth commune fixed effects: $Y_{icmt} = I_{icmt}[\alpha + \sum_{e=1965}^{1973} \gamma_e 1\{c = e\} + \sum_{e=1975}^{2000} \lambda_e 1\{c = e\}] + \delta_c + \mu_t + X'_{icmt}\beta + \varepsilon_{icmt}$. See section 6 for details. The notes are otherwise identical to Figure 2.

Figure A7: Post-treatment trends in relative school inputs of indigenous children and adolescents

Panel A: Additional household and child variables



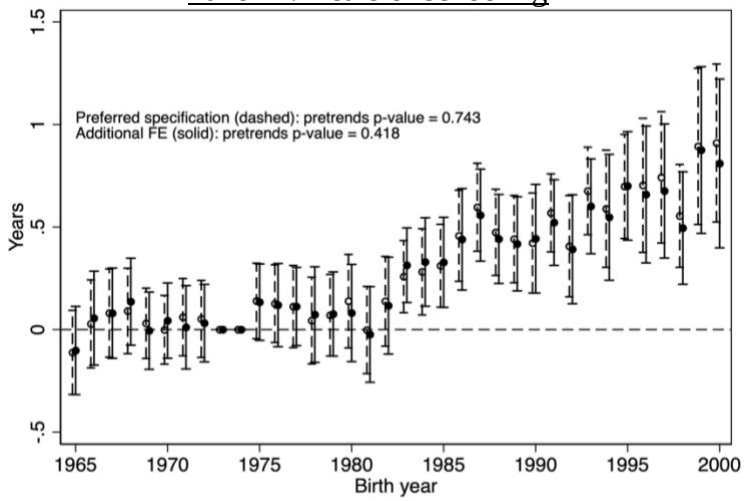
Panel B: Additional school variables



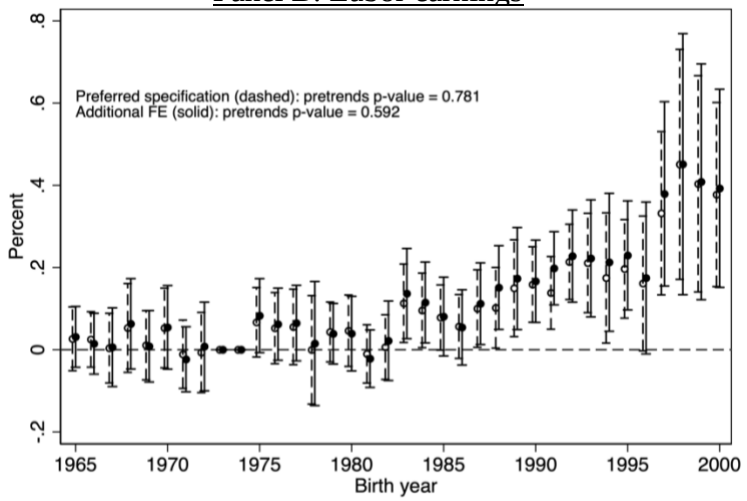
Notes: Each panel uses the auxiliary CASEN sample of children and adolescents (see Appendix Table A2). The circles are $\hat{\lambda}_e$ from equation (4), with pointwise 95% confidence intervals. Standard errors are adjusted for multi-way clustering by commune of birth, and commune of residence as adults. P-values correspond to the null hypothesis that γ_e are jointly equal. The dashed lines are estimates in which the γ_e are constrained to be equal for all birth years.

Figure A8: Event study plots for schooling and labor earnings (with additional fixed effects)

Panel A: Years of schooling

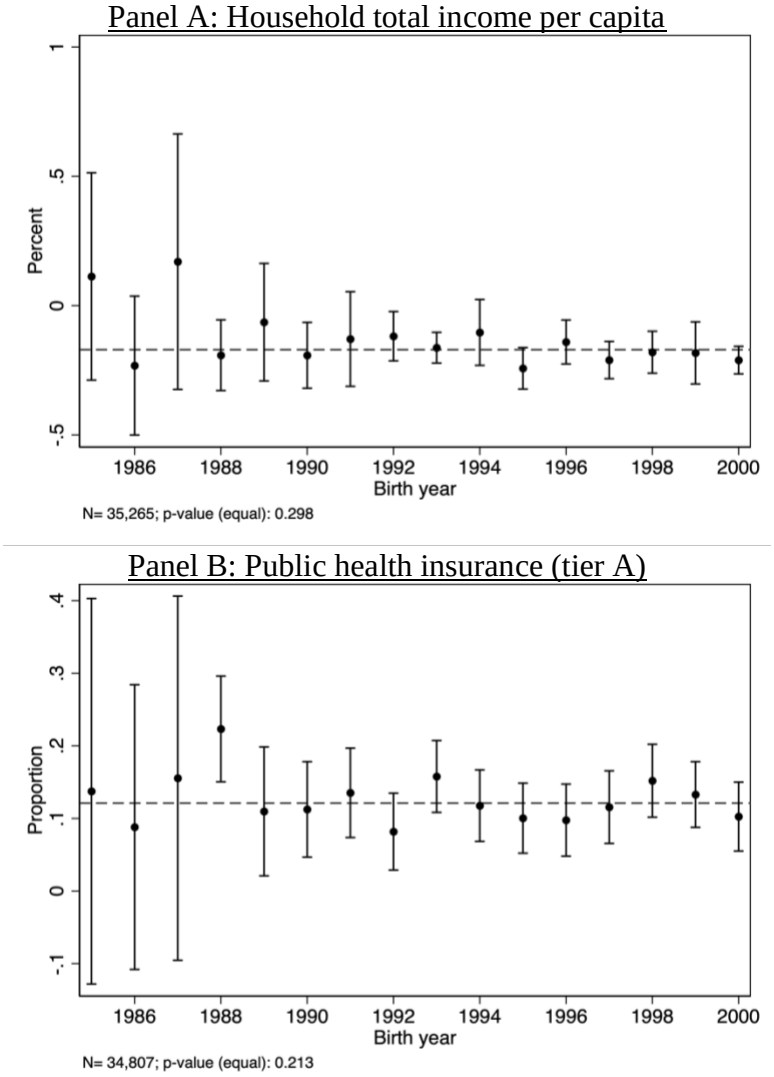


Panel B: Labor earnings



Notes: The event study plot reports results for a modified version of equation (1) in Table 2, including fixed effects for all combinations of birth year, birth commune, and survey year (δ_{cmt}). See section 5 for details. The notes are otherwise identical to Figure 2.

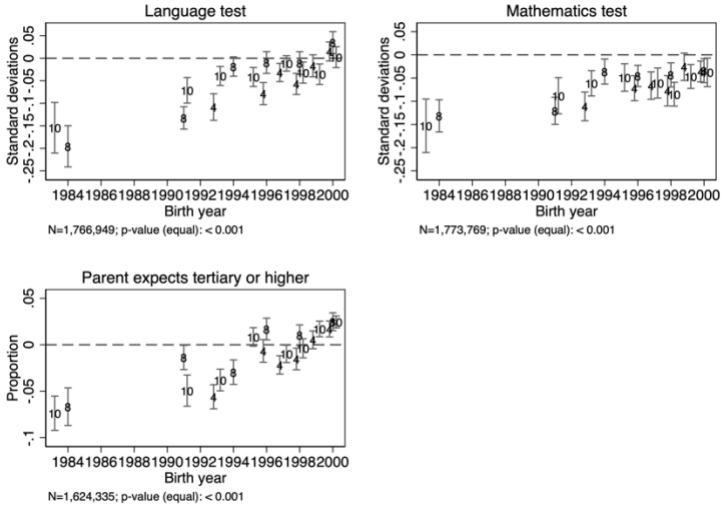
Figure A9: Post-treatment trends in household income of indigenous grant recipients, relative to indigenous non-recipients



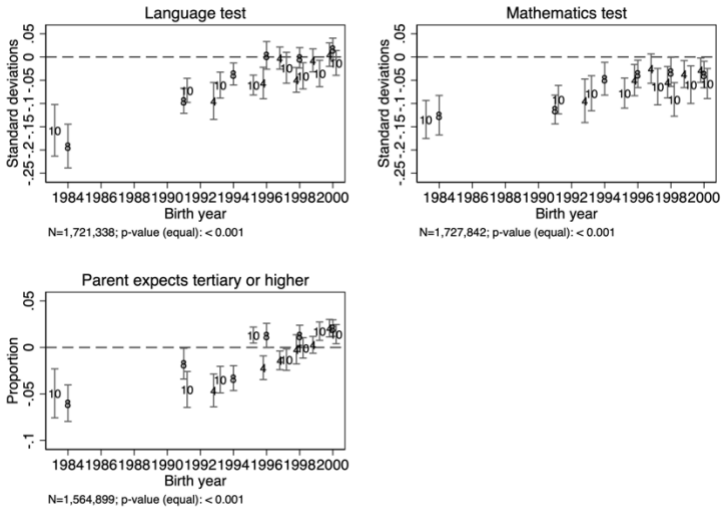
Notes: The circles are $\hat{\lambda}_e$ from a modified equation (4), with pointwise 95% confidence intervals, in which I_{icmt} is replaced with a dummy variable indicating that individuals currently receive an indigenous grant. The sample is limited to indigenous individuals between age 6 and 21 (see Appendix Table A2). 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 A10: Conditional mean differences between outcomes of indigenous and non-indigenous students (female and male samples)

Panel A: Female sample



Panel B: Male sample



Notes: The sample in each panel includes male or female SIMCE observations with non-missing observation of the dependent variable and mothers' indigenous status (see Table A6). The labeled midpoints of confidence intervals are $\hat{\lambda}_{eh}$ from equation (5), with pointwise 95% confidence intervals. Standard errors are adjusted for clustering by the communes of schools. P-values correspond to the null hypothesis that coefficients are jointly equal.