ROLLING BACK PROGRESA:
SCHOOL AND WORK AFTER THE END OF
A LANDMARK ANTI-POVERTY PROGRAM

Fernanda Marquez-Padilla
Susan W. Parker
Tom S. Vogl

Rolling Back Progresa: School and Work After the End of a Landmark Anti-Poverty Program
Fernanda Marquez-Padilla, Susan W. Parker, and Tom S. Vogl
NBER Working Paper No. 33527
February 2025
JEL No. I25, J22, O15

ABSTRACT

Mexico's pioneering conditional cash transfer program Progresa, later renamed Prospera, operated over two decades in a shifting policy landscape. We exploit the program's sudden and unexpected rollback to estimate whether, two decades after rollout studies documented its initial impacts on schooling and labor, the program still raised enrollment and reduced work in youth. Comparing areas with high and low program penetration before and after rollback, we find that rollback immediately reduced school enrollment, especially in boys of high school age. Effects on enrollment were larger at rollback than they were at rollout, albeit shifted from middle school ages to high school ages. Rising work mirrored falling enrollment in boys of high school age. Our results suggest the program had successfully adapted to the rise of high school, but Mexico's poor were unable to protect their children from its unexpected demise.

Fernanda Marquez-Padilla
El Colegio de Mexico
Mexico
fmarquez@colmex.mx

Susan W. Parker
University of Maryland
7805 Regents Dr
Thurgood Marshall Hall 2104
College Park, MD 20742
swparker@umd.edu

Tom S. Vogl
Department of Economics
University of California at San Diego
9500 Gilman Drive
La Jolla, CA 92093
and NBER
tvogl@ucsd.edu

1 Introduction

Conditional cash transfer (CCT) programs, which link monetary transfers for poor households to investments in children, were pioneered by Mexico and Brazil in the late 1990s and now operate in more than 60 mostly low- and middle-income countries (Ibarrarán et al., 2017). The initial randomized evaluation and later follow-up studies of Mexico's program *Progresa*—later renamed *Oportunidades* and then *Prospera*—found improvements in children's education, health, and labor outcomes, as well as household economic outcomes, as summarized in Parker and Todd (2017). These studies—mainly based on variation in *Progresa*'s rollout—contributed to its scale-up and endurance within Mexico, and to the spread of its key features to new programs around the world. This paper asks whether the program was still achieving its goals of raising school enrollment and reducing school-year employment two decades later, despite extensive changes in the policy landscape since rollout. Similar questions arise for any program that is scaled up following a successful initial evaluation and then goes years without major changes to its design.

To answer this question, we study the sudden and unexpected rollback of *Prospera*, which at the moment of rollback provided benefits to approximately 7 million households nationwide, nearly one fourth of the Mexican population. This stoppage at scale provides a unique research context to study the extent to which households can protect their children's schooling from the sudden loss of a two-decade-old transfer program. Our research informs a new thread of research on transfer programs, regarding whether program gains persist after transfers end. Existing studies on this topic primarily focus on whether positive effects in short-term pilot studies are maintained post-pilot (Haushofer and Shapiro, 2018; Baird et al., 2019; Blattman et al., 2020). We study whether the Mexican program's success in keeping youth in school and out of the workforce survives or disappears with rollback. We further investigate whether setbacks, if they occurred, were at the same schooling level as the original gains, or whether they instead shifted higher with the overall distribution of schooling levels.

Beyond specifically illuminating resilience to the rollback of a pioneering cash transfer program, our research is broadly relevant to development policy because policy conditions change over time after initial evaluations, and indeed Mexico's educational landscape has shifted in the decades since rollout in 1997. Enrollment rates at middle school ages, originally a primary target for *Progresa*, increased from 84% to 90% between 1995 and 2005 but have not sustained any changes since; enrollment rates at high school ages, originally excluded from *Progresa*, steadily grew from 51% in 1995 to 72% in 2020 (Appendix Figure A1). At both levels, girls had lower enrollment rates than boys in 1995 but higher enrollment rates in 2020—particularly for high school. As educational strengths and weaknesses change, do long-standing programs like *Progresa* continue their initial successes?

We estimate the effects of rollback on enrollment and work using a difference-in-differences design, comparing outcomes in localities with high and low program penetration, before and after the program ended. We combine administrative data on locality *Prospera* penetration just before rollback with household survey data from the quarterly National Survey of Employment and Occupation (ENOE) to study enrollment at primary, middle, and high school ages, as well as teenage employment and earnings. Rollback occurred suddenly and unexpectedly in early 2019, leaving one school-year transition to observe dropout decisions before the onset of COVID-related shutdowns. Our comparisons over time of localities with differing exposure to a long-standing anti-poverty program raise questions about differential trends, but we verify robustness to a variety of regression specifications, comparing localities over time nationwide, or within the same state, or within the same municipality, or at the same level of economic disadvantage.[1]

We find that rollback bore a substantial burden for youth living in high-*Prospera* penetration localities. Following the cessation of program benefits, school enrollment rates declined relative to low-penetration localities, with effects especially pronounced at high school ages (15-17) and among boys. Estimates from our preferred specification imply that school en-

---

[1]The municipality is an administrative unit in Mexico akin to the county in the United States.

2

rollment among boys of high school age declined by 12 percentage points—or 17% of the mean—in localities with full program penetration, relative to localities with no program penetration. Comparing localities at the 75th and 25th percentiles of program penetration, the implied decline is 8 percentage points. High-school-aged boys have higher employment rates than other youth in Mexico, implying a larger trade-off between school and work. Indeed, we find that rollback raised employment in this group, with our estimates suggesting that more than 1 in 2 rollout-attributable dropouts started working upon leaving school.

After announcing the cancellation of *Prospera*, the government implemented a substitute grant program linked more loosely to school enrollment, called *Becas Benito Juárez* (BBJ). Our results are all the more striking because they are *net* of the implementation of this substitute program. We compare coverage and transfers under the two programs using administrative data on recipients. While overall spending is similar pre- and post-rollback, we find that progressivity worsened substantially, such that poorer localities received a far smaller share of BBJ than *Prospera* spending. Our results remain unchanged when we control for early BBJ penetration.

The impact of *Prospera*'s rollback on boys' school enrollment is substantial and, in fact, larger than the impact of *Progresa*'s rollout, albeit at different schooling levels. Schultz (2004) estimates that *Progresa* raised middle school enrollment in its first two years of operation by 5-6 percentage points for boys and 7-9 percentage points for girls. We find larger effects of rollback on enrollment for boys at high school ages but more limited effects for boys at middle school ages and girls at all ages. Employment rates are lower in these less-affected groups, suggesting that school-work trade-offs may be especially important to understanding the impact of rollback.

Conditional cash transfer programs are thought to increase schooling partly by relieving liquidity constraints (Progresa, 1997), allowing poor households to finance education investments that will take years to generate returns. Consistent with this idea, we present descriptive evidence suggesting that the near-term return to completing high school is small

for boys on the margin of high school dropout. An economically significant high school wage premium emerges only in middle age (early forties), when graduates earn more than 25% higher wages than dropouts. Before then, working in construction without a high school degree earns as good a wage as any other option, and indeed, the modal rollback-attributable job is in construction. Circumstances like these are a textbook case for government intervention to relieve liquidity constraints.

We present novel evidence that, more than 20 years after its implementation, *Prospera* kept teenagers in school and out of work. Its discontinuation led to an immediate drop in high school enrollment, particularly for young men, even despite the somewhat haphazard implementation of a substitute program. The results suggest that the conditional transfers provided by *Prospera* were still promoting human capital accumulation for young Mexicans, particularly those in the most marginalized communities. Our findings thus provide critical and timely new empirical evidence to inform the design and continuation of CCT programs in Mexico and across the world, more than two decades after their original creation.

2 Background

2.1 Rolling Out *Progresa*

Implemented in 1997, *Progresa* was among the first CCT programs, along with the Brazilian program *Bolsa Escola*. Before the Mexican government announced its rollback in early 2019, it supported 7 million low-income households through direct monetary transfers conditioned on school enrollment and attendance as well as preventive health clinic visits, increasing its average beneficiaries' incomes by about 30 percent (Parker and Todd, 2017). CCT programs have the dual objectives of reducing current poverty directly, through cash, and future poverty indirectly, through education and health improvements in the next generation. They are thought to improve children's education and health by easing the financial constraints their parents face and by subsidizing parental investments in education and health.

A well-known randomized controlled trial in 1997 served as the basis for a number of evaluations in the early years of *Progresa*, which found positive effects on school enrollment (Schultz, 2004; Skoufias and Parker, 2001), child health (Gertler, 2004; Gertler and Boyce, 2003; Rivera et al., 2004), household consumption (Hoddinott and Skoufias, 2004), and women's status (Adato et al., 2000), as well as negative effects on youth employment (Skoufias and Parker, 2001). CCT programs rapidly spread through Latin America and to other continents as well. By 2013, 137 million individuals across Latin America were receiving CCTs (Ibarrarán et al., 2017).

The program's effects on schooling and work have been of central interest throughout its existence. Analyzing data from the 18-month experiment, Schultz (2004) finds that the program significantly increased the probability of transitioning to middle school after completing primary (from the 6th to 7th grade), with increases on the order of 5-6 percentage points for boys and 7-9 percentage points for girls. Behrman et al. (2005) estimate a Markov schooling transition model that compares transition matrices between the treatment and control groups, analyzing enrollment, repetition, dropout, and school re-entry at each age. Consistent with Schultz (2004), they find few effects at primary school ages and larger effects at middle school ages. Skoufias and Parker (2001) focus on time use data from the experimental evaluation, finding positive impacts on enrollment and time spent in studies, as well as negative effects on time spent working. For youth aged 12 to 17—middle and high school ages—they find school attendance rises 4-6 percentage points for boys and 8-10 percentage points for girls, and work outside the home falls 3-5 percentage points for boys and 2 percentage points for girls. These results suggest greater vulnerability for girls but a stronger school-work trade-off for boys.

Later studies on medium- and long-term impacts establish that the contemporaneous increases in school attachment translated to lasting effects on accumulated schooling levels. In medium-term follow-ups of the experimental evaluation, Behrman et al. (2009) and Behrman et al. (2011) estimate that extended time in the program raises grades completed, about 1

5

full grade for children who participate in the program for 6 years beginning at ages 9 to 12, compared to nonparticipating children. In a long-term follow-up, Araujo and Macours (2021) study children and youth from the original study sample twenty years later, focusing on those born into the program (which they term "early childhood cohort") and those at the transition between primary and secondary school when the program began ("school cohort"). They find impacts of 0.3 to 0.4 years of increased schooling for the early childhood cohort and 0.2 to 0.3 years for the school cohort, although impacts for males in the school cohort are not statistically significant at conventional levels. In a difference-in-differences design based on cohort exposure to the non-experimental rollout of the program across all of Mexico, Parker and Vogl (2023) find education impacts for children who grew up with the program to be about 1.4 grades completed for women and 1.0 for men. The estimates from the two long-term follow-ups differ in levels, perhaps due to differing levels of aggregation in program exposure, but both suggest larger effects for women.

2.2 Rolling Back *Prospera*

Progresa lasted through three presidential transitions largely unscathed, save for name changes to *Oportunidades* and then *Prospera*. When Andrés Manuel López Obrador won Mexico's presidential election in June 2018, rumors purported that he planned to end the longstanding program. He initially denied these plans, but on February 25th, 2019, less than three months after he took office, the *Diario Oficial de la Federación*, a daily publication of the Mexican federal government akin to the United States' *Federal Register*, announced that during 2019 *Prospera* would transition to a new education grant program called *Becas Benito Juárez* (BBJ).[2] The government's 2019 budget also stated that *Prospera*'s resources would be reassigned to the new substitute program. In practice, as we show below, poor families experienced massive disruption during rollback and had little access to BBJ in the pre-COVID period.

---

[2]*Prospera* also had a health and nutrition component, including a fixed monetary transfer linked to preventive health clinic visits, but the government created no new program substituting for it.

Benefits and rules differ somewhat between BBJ and *Prospera*. Both programs provide transfers conditional on school enrollment, but BBJ reduced the use of means-testing, loosened conditionality, and stopped monitoring attendance.[3] At the primary and middle school levels, BBJ provides a fixed family grant of 800 pesos (approximately $50 USD) monthly for families who have at least one child enrolled in school in ninth grade or below. This flat grant contrasts with *Prospera*'s payments, which depended on the number of children enrolled and the grades in which they were enrolled. At the high school level, BBJ provides a monthly grant of 800 pesos to each youth enrolled in high school, with the grant going directly to the high school student, rather than the female head of household as under *Prospera*.[4] Table A1 compares the structure of benefits across both programs. In a household that transitioned from *Prospera* to BBJ, transfers received by parents might have increased or decreased, depending on the number of children, their current grades in school, and the extent of resource-sharing between (high-school-age) teenagers and their parents.

These nuanced differences in program benefits and rules were arguably swamped by disruption and changes in program reach. Newspapers reported complaints and demonstrations by *Prospera* beneficiary families during the spring of 2019, suggesting that many received no payments during the first half of 2019. While the operational process through which *Prospera* beneficiaries were transitioned to the BBJ program is not well documented (see Jaramillo-Molino (2020) and CONEVAL (2020)), we obtained administrative data on the number of *Prospera* and BBJ beneficiaries by locality just prior to and just after rollback, allowing us to analyze how coverage of this new program evolved, both in terms of beneficiaries and peso amounts. Parker and Vogl (2024) compare transfers and total beneficiaries under the two programs, showing that while rollback disrupted payments in the first half of 2019, total transfers by year's end were similar to previous years.

Nevertheless, the geographic distribution of transfers changed substantially. Under *Pros-*

---

[3]BBJ ended means-testing at the high school level but continued it at the primary and middle school levels. BBJ also eliminated *Prospera*'s 85% attendance requirement for students to receive grants.

[4]A smaller, third component, *Jóvenes Escribiendo el Futuro*, provides transfers to college students.

pera, a disproportionate share of benefits went to Mexico's poorest localities; BBJ substantially muted this progressivity, as might be expected given the reduced use of means-testing. To illustrate this point, Figure 1 plots transfers per household under *Prospera* and BBJ across the government's index of locality marginalization, computed as the first principal component of various census-based measures of community disadvantage. Outside the 10% least marginalized localities, resources per household declined after rollback. The poorer the community, the larger the reduction in transfers per household. After rollback, households living in localities with above-median marginalization received on average less than half the transfers they received before rollback. Meanwhile, in the 10% least marginalized localities, households received on average more than double what they had received pre-rollback. These shifts are consistent with a constant budget because most Mexican households reside in the least marginalized localities (which include major cities), as shown in the population distribution at the bottom of the figure.

In summary, the *Prospera* program showed a high degree of progressivity, with transfers per household increasing with locality marginalization, but this progressivity largely disappeared under the new substitute BBJ program. The net result is that the BBJ substitute program provides far fewer resources per household in poor communities. Qualitative evidence also suggests that household-level BBJ transfers started reaching beneficiary households only three to six months after the official start of the program (CONEVAL, 2024). *Prospera* households experienced acute disruption in their benefits, accompanied by pronounced uncertainty about whether benefits would resume, and if so, at what level.

3   Data and Methods

3.1   Data

We measure school and labor market outcomes in the National Survey of Occupation and Employment (ENOE), a quarterly labor market survey carried out since 2005 by INEGI, the

Figure 1: Program penetration by locality marginalization
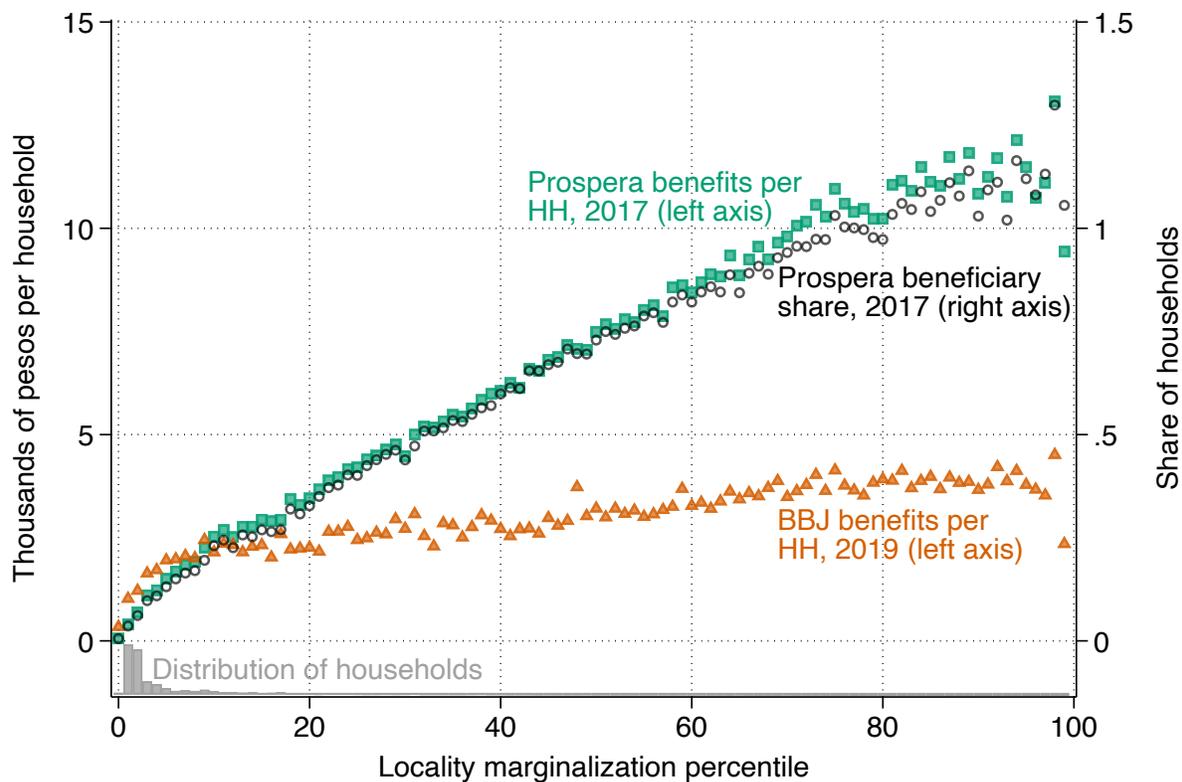
Note: Sample includes localities with at least 100 residents, which contain 98% of the Mexican population. Beneficiary data are from program administrative records; household counts are from ITER; marginalization data are from CONAPO. *Prospera* data are for the last non-electoral year preceding rollback, 2017; Becas Benito Juárez (BBJ) data are for the first year of operation, 2019. Household counts and marginalization are for 2010, the most recent census preceding the rollback of *Prospera*. Some localities have beneficiary shares above 100 due to population growth since the most recent census.

Mexican statistical agency. The ENOE is Mexico's equivalent to the US Current Population Survey. It interviews approximately 127,000 households every quarter, and is representative at the national and state levels as well as at the urban, semi-urban, and rural levels.[5] We use rounds between 2014 and 2020, excluding summers, when schools are largely out of session. Our main outcome is school enrollment, which we study separately for boys and girls of primary-school (6-11), middle-school (12-14), and high-school (15-17) age. In the last age range, we also study labor market outcomes and time use.

We focus on survey rounds collected before the March 2020 onset of the COVID-19 pandemic in Mexico, for three reasons. First, the pandemic closed Mexican schools for over a year, changing incentives for school enrollment while also making survey responses about it less informative. Second, the ENOE shut down in Spring 2020 and then temporarily switched from in-person to telephone-based interviews, with consequences for representativeness that are not yet fully understood. Third, even after the ENOE returned to in-person interviews, the public-use files stopped providing locality identifiers, and these identifiers are crucial to our research design.

To identify the effects of rollback, we track school enrollment and labor market outcomes over time across geographic areas with varying levels of pre-rollback *Prospera* penetration, using administrative data on *Prospera* enrollment. The ENOE provides geographic identifiers at the locality level, allowing us to merge local program penetration to it. Our primary measure of *Prospera* penetration is the beneficiary share: the ratio of households enrolled in *Prospera* in 2017 to the number of total households enumerated in the locality in the most recent census, in 2010. In sensitivity analyses, we also rely on program benefits per household, but we preference the beneficiary share because it is easier to interpret and less intrinsically correlated with a locality's demographic structure. Appendix Figure A2 plots the benefits per household against the beneficiary share, finding a linear relationship, with 2017 benefits per household rising 104 pesos for each percentage point increase in the beneficiary share.

---

[5]The survey design also includes a rotating panel in which every household is interviewed up to five times, allowing the construction of a new panel beginning in each quarter.

We use 2017 as the last stable pre-rollback year, before the 2018 election. Mexican law prohibits the government from the distribution of public benefits during elections.

We include in our main estimation sample all localities with fewer than 100,000 inhabitants. We exclude large cities because they had few beneficiaries—*Prospera* was concentrated in poor, rural areas—and we wish to avoid relying on comparisons between large cities and small localities to identify the effects of rollback. Large cities are likely to experience different trends and shocks from *Prospera* target areas so are not plausible as a control group. We thus track changing outcomes over time in more versus less penetrated localities *outside* large cities. Figure A3 shows that only about 5% of households in localities above 100,000 population were beneficiaries, compared with nearly 60% in localities below 2,500 population.[6] The ENOE is designed to be representative of localities below 100,000 inhabitants, so the restriction does not introduce non-representativeness in our sample. In robustness analyses, we verify that our main results are robust to including large cities.

3.2 Design and Estimation

The rollback of *Prospera* began during the first two months of 2019, after López Obrador took office in December 2018. We hypothesize impacts on school enrollment and related labor market outcomes mainly at the beginning of the following school year, which started in the late summer of 2019. We thus study impacts starting about 9 months after the start of rollback, prior to the onset of COVID-19 in March 2020. Because the policy conversation during the presidential election may have shifted expectations regarding *Prospera*'s future already in 2018, we also allow for anticipation effects in school year 2018-2019. Our empirical strategy leverages quarterly variation, allowing us to trace the entire pattern of school and labor market responses before and after rollback, including within the academic year. We compare changes in outcomes from before to after rollback across localities with higher versus lower levels of program penetration.

---

[6]Less populous localities were more likely to lose benefits during the transition from *Prospera* to BBJ. Among localities under 100,000, 92% received less under BBJ; over 100,000, 14% received less under BBJ.

The dynamics of school dropout motivate the hypothesized timing of effects. Appendix Figure A4 uses the ENOE rotating panels to show that school-leaving is concentrated between the end of one academic year and the beginning of the next, rather than during the academic year. For example, among 15-17 year-old boys enrolled in fall or winter, roughly 4% drop out by the next season; among those enrolled in spring, over 8% drop out by the following fall. Dropout levels are somewhat lower for girls and for 12-14 year-olds of either gender, but the summer spike is similar in relative terms.

Our main estimating equation is a variant of a standard dose-response specification for continuous difference-in-differences. For individual i from locality l in state s at quarter t, we estimate:

$$y_{ilst} = \alpha Prospera_{ls} + \gamma Prospera_{ls} \mathbb{1}_{2018/19} + \beta Prospera_{ls} \mathbb{1}_{2019/20} + \tau_{st} + \epsilon_{ilst} \qquad (1)$$

Cross-sectional variation in rollback exposure is captured by $Prospera_{ls}$, the share of locality ls's households enrolled in $Prospera$ in 2017, the last stable year of the program. We include this variable directly, rather than absorbing cross-sectional variation with locality fixed effects, because most localities do not appear in the survey for more than two consecutive years.

We interact $Prospera_{ls}$ with an indicator for the 2019-20 academic year to identify the effect of rollback. The coefficient on the interaction term, β, captures how outcomes changed from before to after rollback across localities with more versus less $Prospera$ penetration. We also interact $Prospera_{ls}$ with an indicator for the 2018-19 academic year, which allows for rollback impacts to begin in school year 2018-2019, given potential anticipation related to the election in 2018 as well as rollback taking place during the latter part of the 2018-2019 academic year. However, the dropout patterns described above suggest that enrollment effects should be most visible after summer dropout, during the 2019-20 academic year.

To complete the regression specification, we include quarter fixed effects. Our preferred

approach allows the quarter fixed effects to vary by state, τ_{st}, so that we only compare changes in outcomes between localities within the same state. We verify that our results are robust to allowing the quarter fixed effects to vary by municipality, akin to a county in the United States, but we prefer the state-quarter specification because in our survey sample, nearly half of municipalities have only one locality. With state-by-quarter fixed effects, our design assumes that more- and less-saturated localities within the same state would have experienced the same enrollment changes in the absence of rollback.

We use equation (1) to analyze a two-period difference-in-differences design with a continuous treatment, which Callaway et al. (2024) point out has a fraught interpretation under treatment effect heterogeneity. We mainly interpret β as an average causal response of enrollment to a marginal decrease in *Prospera* penetration, which is identified under strong parallel trends. In our context, the strong parallel trends assumption requires that the evolution of outcomes for localities at a given *Prospera* penetration represents what other localities would have experienced, on average, had they been assigned the same *Prospera* penetration. We also discuss an alternative interpretation of β as the effect of rollback on a locality in which all households were *Prospera* beneficiaries. This extrapolation works if the causal response function is linear, but Callaway et al. (2024) show that it does not otherwise. In the Appendix, we report 2-by-2 difference-in-differences comparing fully-penetrated localities with fully-unpenetrated localities, finding larger effects than those estimated using equation (1). We thus view our core estimates as conservative.

To shed light on pre-existing trends, we also estimate an event study specification:

$$y_{ilst} = \alpha Prospera_{ls} + \sum_{q \neq 2018q2} \beta_q Prospera_{ls} \mathbb{1}_{t=q} + \tau_{st} + \epsilon_{ilst}. \tag{2}$$

This specification modifies equation (1) by interacting the cross-sectional exposure variable with indicators for every quarter except the spring of 2018, just preceding the presidential election. The parallel trends assumption implies β_q to be zero for all quarters years prior to

this reference period. Given summer dropout, we expect rollback effects to materialize in the fall of 2019 and winter of 2020, with β_q negative for enrollment and positive for employment, labor hours, and earnings. In between, from the fall of 2018 to the spring of 2019, the effects of anticipated or early-stage rollback are uncertain, so that β_q may be the same sign or zero.

For both specifications, we use ENOE data from 2014 onward, leading to a four-year window before López Obrador's election. This window corresponds to a period of stability in *Prospera* enrollment, and it is long enough to allow us to assess differential pre-rollback trends in the event studies. We cluster standard errors at the locality level.

4   Effects of Rollback on School Enrollment

4.1   Main Results

Our analysis begins by estimating the impacts of rollback on school enrollment by gender and age group. We separate ages 6-11, 12-14, and 15-17, corresponding approximately to the ages of primary (grades 1 to 6), middle (grades 7 to 9), and high (grades 10 to 12) school. Figure 2 reports event studies for school enrollment. A first vertical line marks the quarter leading up to the election, the reference period. A second vertical line marks the fall quarter of 2019, the beginning of the 2019-20 academic year. We expect estimates to be flat at 0 to the left of the first vertical line, consistent with parallel trends, and negative for affected age groups to the right of the second vertical line, after summer dropout. Predictions are ambiguous for the period in between.

The first row of Figure 2 pools boys and girls, with the event study estimates showing clear evidence of negative effects at high school ages and suggestive evidence of negative effects at earlier ages. All three age groups show no evidence of differential pre-rollback trends, nor of anticipation or early-stage effects between spring 2018 and fall 2019. For primary and middle school ages, small gaps emerge in fall 2019, with magnitudes of 1-2 percentage points at primary school ages and 3-4 percentage points at middle school ages.

14

Across the two age groups and the two post-dropout quarters, however, the gaps are not consistently significant at conventional levels, with p-values ranging from 0.02 to 0.16. In contrast, for high school ages, the estimated coefficients drop abruptly and significantly in fall 2019, implying a fall in enrollment due to rollback. The estimates for fall 2019 and winter 2020 have magnitudes of 9-10 percentage points and p-values below 0.02.

The second and third rows of Figure 2 estimate the event studies separately for boys and girls, revealing that the evidence of negative effects in the pooled sample primarily reflects effects on boys. For girls, the event studies are flat throughout ages 6-11 and 12-14, displaying neither differential pre-trends nor rollback impacts. For ages 15-17, the event study suggests a reduction in enrollment in the fall of 2019, but many pre-rollback coefficients are also negative. Thus, the event studies exhibit no evidence of effects on younger girls and only suggestive evidence of effects on high-school-aged girls.

For boys, the timeline looks markedly different. All three age groups show no differential pre-trends but at least marginally-significant negative effects in the 2019-20 school year. Mirroring the pooled estimates, the estimated coefficients for the youngest boys are small and only marginally significant, with magnitudes around 2 percentage points and p-values of 0.07-0.16. At older ages, however, the estimated coefficients in the post-dropout period are larger and consistently significant: 7-9 percentage points at middle school ages and 10-11 percentage points at high school ages, in all cases with $p < 0.05$.

Table 1 summarizes these temporal patterns using regression specification (1), again reporting both pooled and gender-specific results. Panel A analyzes the pooled sample, while Panels B and C split the sample by gender. Column (1) restricts to ages 6-11, column (2) to ages 12-14, and column (3) to ages 15-17. For each sample and age group, the table reports the estimated coefficients on the *Prospera* share and its interaction with indicators for the 2018-19 and 2019-20 academic years, along with the dependent variable mean.

The interaction terms are of central interest because they measure the effects of rollback, but we highlight three descriptive patterns first. First, the dependent variable mean—the

Figure 2: Event studies for school enrollment, by age group and gender
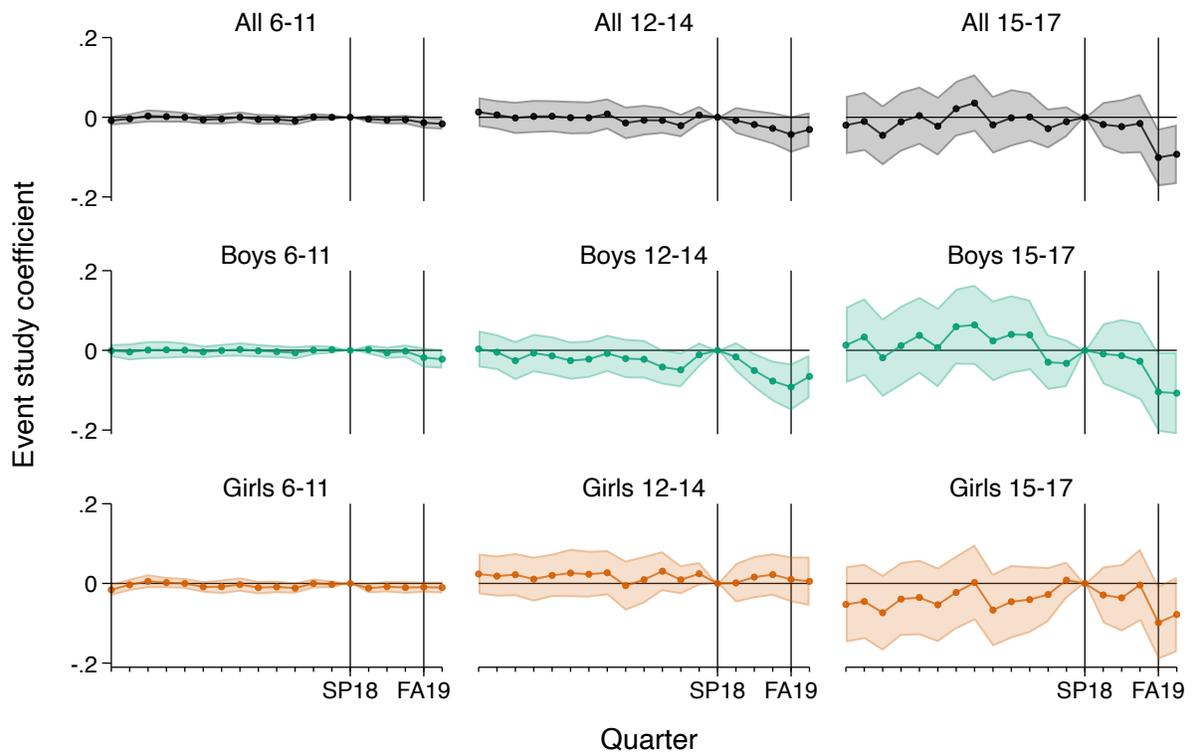
Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. All regressions include the *Prospera* share and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents.

Table 1: Effect of rollback on school enrollment, by age group and gender

	Ages 6-11	Ages 12-14	Ages 15-17
	(1)	(2)	(3)
A. All			
Prospera share	-0.005*	-0.062***	-0.233***
	[0.002]	[0.007]	[0.014]
Prospera share × 2018-19	-0.003	-0.017	-0.011
	[0.005]	[0.014]	[0.026]
Prospera share × 2019-20	-0.013**	-0.036**	-0.089***
	[0.006]	[0.018]	[0.028]
Dependent variable mean	0.987	0.936	0.731
N	351,505	177,985	174,998
B. Boys			
Prospera share	-0.004	-0.053***	-0.201***
	[0.003]	[0.009]	[0.017]
Prospera share × 2018-19	-0.001	-0.030*	-0.033
	[0.006]	[0.018]	[0.031]
Prospera share × 2019-20	-0.019*	-0.061***	-0.123***
	[0.011]	[0.023]	[0.036]
Dependent variable mean	0.986	0.932	0.725
N	179,266	90,341	89,275
C. Girls			
Prospera share	-0.005*	-0.071***	-0.268***
	[0.003]	[0.010]	[0.017]
Prospera share × 2018-19	-0.006	-0.004	0.012
	[0.006]	[0.019]	[0.033]
Prospera share × 2019-20	-0.005	-0.009	-0.054
	[0.006]	[0.023]	[0.036]
Dependent variable mean	0.988	0.941	0.737
N	172,239	87,644	85,723

Note: Brackets contain standard errors clustered by locality. All regressions include state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents. *Prospera* share equals the number of households enrolled at the start of 2017 divided by the number of households in the 2010 census. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

enrollment rate—is highest at primary school ages, 99%, lower at middle school ages, 93-94%, and lower still at high school ages, 73-74%. Second, in every age group, it is also slightly lower for boys. Third, the estimated coefficient on the *Prospera* share is consistently negative, and more so at older ages. School enrollment is lower in localities with higher *Prospera* penetration. At high school ages, enrollment rates are 20-27 percentage points lower in full-penetration localities compared with zero-penetration localities. Before rollback, the program was reaching communities where youth had high dropout risk.

As already displayed in the event studies, the results for the interaction terms indicate that this gradient steepened substantially in the 2019-20 academic year, particularly in the pooled sample and among boys. Using the pooled sample, Panel A reports negative and significant estimates of β, the coefficient on the interaction between pre-rollback program intensity and the 2019-20 academic year, for all three age groups. For ages 6-11 and ages 12-14, the estimates are significantly different from zero at the 5% level; for ages 15-17, it is significant at the 1% level. Estimates of γ, the coefficient on the interaction between program intensity and the 2018-19 academic year, are negative but statistically insignificant, implying the absence of significant anticipatory effects.

The estimated coefficient on the 2019-20 interaction is particularly large for high-school-aged youth, -0.089. Under the strong parallel trends assumption, it can be interpreted as a variance-weight average causal response, implying that a 1 percentage point decline in the share of households covered by *Prospera* caused 9 in 10,000 high schoolers to drop out. With an additional linearity assumption, it can also be interpreted as the average effect of rollback in localities that started with full *Prospera* penetration. Seen this way, the coefficient implies rollback-attributable enrollment declines of 8.9 percentage points in full-*Prospera* localities. This effect amounts to 12% of the sample-wide enrollment rate of 73% among 15-17 year-olds. We assess robustness to relaxing these assumptions in the next section.

The estimated effects of rollback on enrollment are smaller for younger age groups. At middle school ages, 12-14, the negative effect of rollback on enrollment (under strong parallel

trends and linearity) is 3.6 percentage points. This result contrasts with studies of *Prospera* rollout from two decades earlier, which found larger impacts on middle school enrollment at a time when enrollment rates were lower than they are at present. At primary school ages, 6-11, the effect is 1.3 percentage points. The small impact on primary enrollment matches conclusions from the original *Prospera* rollout studies. Primary enrollment is nearly universal, with 99% of 6-11 year-olds enrolled.

Also consistent with the event studies of Figure 2, Panels B and C of Table 1 find that the negative effects of rollback on school enrollment are concentrated in boys. In Panel B, estimates of β are negative and statistically significant for boys in all three age groups (at the 1% level for 12-14 and 15-17 year-olds and 8% level for 6-11 year-olds). They are also larger than the pooled effects estimated in Panel A. The negative effect of rollback on school enrollment is particularly large in boys ages 15-17, with a coefficient of -0.123. Under strong parallel trends and linearity, this coefficient implies that rollback reduced enrollment by 12.3 percentage points in full-*Prospera* localities, relative to an overall enrollment rate of 73%. As in the pooled sample, estimates for boys are smaller at earlier ages. The corresponding effect for boys ages 12-14 is 6.1 percentage points, relative to an an enrollment rate of 93%; that for boys ages 6-11 is 1.9 percentage points, relative to an enrollment rate of 99%.

Panel C of Table 1 turns to girls, for whom all estimates of β are negative but small and not significantly different from zero. The largest is for girls ages 15-17: -0.054 ($p = 0.14$), less than half the corresponding estimate for boys. The difference in effects between high-school-aged boys and girls is significantly different from zero only at the 12% level, however. Overall, the negative enrollment effects of rollback appear to be largest for boys and at high school ages, but we cannot reject moderate effects for high-school-aged girls.

4.2 Specification Checks

Figure 2 and Table 1 suggest that the rollback of *Prospera* had large negative effects on school enrollment, principally among boys, principally at high school ages. This section examines

the sensitivity of our main results. Figure 3 reports point estimates of β in a number of alternative regression specifications that address potential threats to identification. We estimate each alternative regression nine times: for the pooled sample, for boys, and for girls in each of the three age groups. For comparison, the first row shows our baseline estimates from Table 1: based on the regression specification with state-quarter fixed effects (SQ), using our main sample, which omits cities over 100,000 population.

The remaining rows consider perturbations to our main estimations. First, we assess whether our difference-in-differences estimates are sensitive to the specific parallel trends assumption we impose. Our main specification includes state-time fixed effects, thus requiring parallel trends within states but not between them. We alternatively estimate regressions with un-interacted time fixed effects or with municipality-time fixed effects, which respectively require nationwide parallel trends or within-municipality parallel trends. Second, we include covariates to control for individual characteristics. Third, we further probe the parallel trends assumption by allowing time fixed effects to vary by the municipality's or locality's percentile rank in the distribution of the marginalization index, a census-based measure of area disadvantage. Specifically, we interact the state-time indicators with percentile bin indicators. Fourth, we allow for linear differential trends by including an interaction between the *Prospera* share and a linear term in time. Finally, we estimate the original regression specification in an expanded sample that includes large cities.

Beginning with the sensitivity of the results for 15-17 year olds, Figure 3 demonstrates that for boys, the numerous specification checks do not appreciably change the point estimate or significance level. Every one of the alternative specifications implies a negative and significant ($p < 0.05$) impact of rollback on enrollment of at least 10 percentage points. For girls aged 15-17, where our main specification suggested negative but insignificant impacts of rollback, the robustness tests paint a more nuanced picture. While the majority of the alternative specifications find statistically insignificant effects, the estimated coefficients are all negative, and several are statistically significant and comparable in magnitude to the
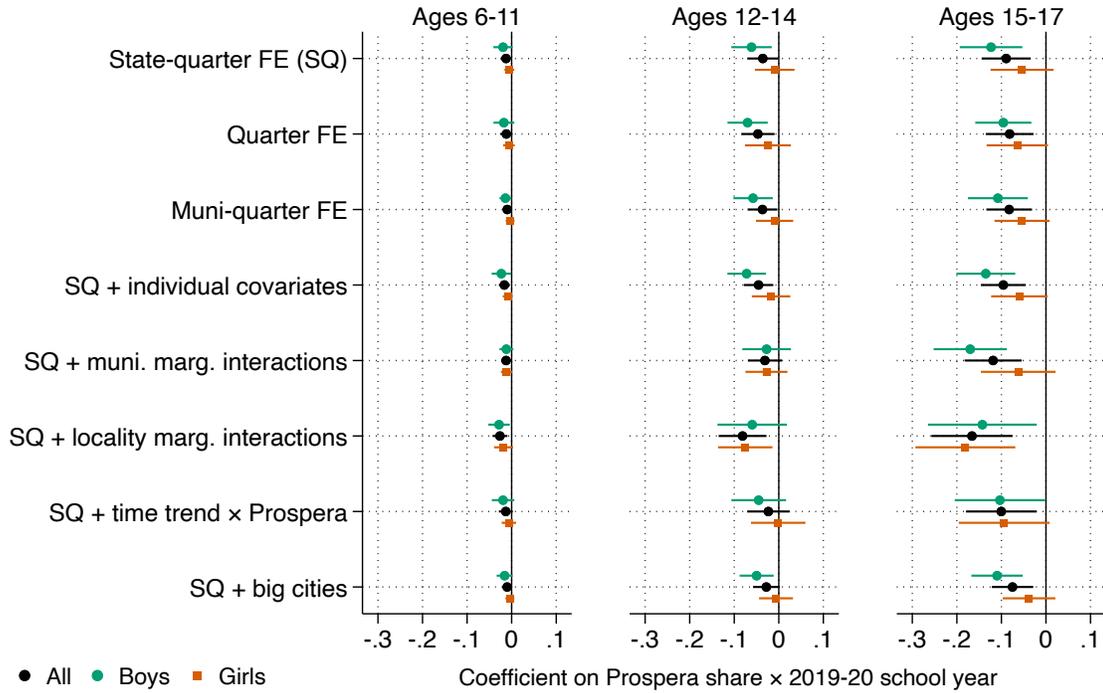
boys' results. The evidence thus suggests possible effects for girls in this age group, but results are sensitive to the specification and less robust than the results for boys.

Our main results also indicated negative effects of rollback on enrollment for boys ages 12-14 and 6-11, which were significant for the former group and marginally significant for the latter. However, the specification checks for these age groups suggest that the results are not robust. Most strikingly, specifications controlling for differential linear time trends or for area marginalization interactions fail to find significant impacts of rollback on enrollment. For boys ages 6-11, five out of eight estimated coefficients are statistically insignificant; for boys ages 12-14, three out of eight are insignificant. We consider the enrollment impacts for boys in these age groups insufficiently robust to alternative specifications. Specification checks for enrollment effects for girls 12-14 and 6-11 confirm our main results, which showed no significant impact of rollback on enrollment in either group.

To probe the role of specific states in these results, Appendix Figure A5 repeats our main specification omitting one state at a time. For all three age groups and for both genders, the results are remarkably consistent across all 32 states. The only notable exceptions are the estimations for 6-11 and 12-14 year-olds excluding the state of Chiapas. These estimates are smaller and markedly less significant than the corresponding full-sample results from the main regression specification. However, the results from Figure 3 already led us to conclude that effects in these age groups are not robust.

Callaway et al. (2024) demonstrate that under strong parallel trends, our regression model identifies an average causal response to locality *Prospera* penetration, albeit with unintuitive weighting. A more comprehensible alternative is the average effect of rollback on localities that formerly had complete *Prospera* penetration. Our regression model identifies this quantity only under linearity. To relax the linearity assumption, we generate a binned version of the beneficiary share with bins in increments of 0.1: $[0, 0.1), [0.1, 0.2), \cdots [0.9, 1.0)$, and a final category for values greater than or equal to 1. Values strictly greater than 1 are due to population growth between the census in 2010 and *Prospera* measurement in 2017.

Figure 3: Effect of rollback on school enrollment: alternative regression specifications

Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. All regressions include the *Prospera* share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes summers. Individual covariates include child sex, child age, the household head's education level, an indicator for the mother being present in the household, and the mother's age group, marital status, education level, and literacy if she is present. In the "marginalization interaction" regressions, we interact quarter indicators with indicators for single-percentile bins of the municipality or locality marginalization index. In the "time trend × *Prospera*" regressions, we interact a linear time trend with the *Prospera* share. In the "big cities" regressions, we estimate the baseline model in an expanded sample that includes cities with populations over 100,000.

As such, we consider the top category to reflect full *Prospera* penetration. We estimate a semiparametric version of equation 1 that includes bin indicators and their interactions with indicators for the 2018-19 and 2019-20 school years.

Appendix Figure A6 reports the semiparametric results, finding that the effects of rollback are concentrated in the localities that were most saturated with *Prospera*. Comparing full-penetration localities with the lowest-penetration localities over time, we estimate that rollback reduced enrollment by 12 percentage points among 15-17 years-olds overall, and by 18 percentage points among 15-17 year-old boys, with both estimates statistically significant at the 1% level. These quantities are somewhat larger than the rollback effects implied by the continuous specification: 9 percentage points overall and 12 percentage points for boys only. We conclude that our continuous specification provides a conservative estimate of full rollback effects.

The analyses so far use the share of households receiving benefits to measure *Prospera* penetration, but the program administrative data also allow calculation of benefits per household, an alternative measure. Appendix Figures A7 and A8 re-estimate the results of Figures 2 and 3 defining $Prospera_{ls}$ as 2017 benefits per household (in thousands of pesos) rather than the beneficiary share. This alternative measure of program intensity reduces measurement error due to variation in the sizes of transfers received by different types of beneficiary households, but it is more directly endogenous to schooling choice and household structure in the pre-rollback period. It also eludes an intuitive binary characterization of rollback effects.

Appendix Figures A7 and A8 broadly confirm the earlier results, with somewhat stronger results for high-school-aged girls. The event studies in Appendix Figure A7 show that the high school enrollment gap between high- and low-*Prospera* localities widened significantly in the fall of 2019. And the difference-in-differences estimations in Appendix Figure A7 now find significant rollback effects for both boys and girls of high school age across all regression specifications. Estimation of equation (1) now finds β to be -0.013 for boys ($p < 0.01$) and -0.007 for girls ($p = 0.04$). These magnitudes suggest that reducing *Prospera* transfers by

1,000 pesos per month per household causes 13 dropouts per thousand boys and 7 dropouts per thousand girls. Appendix Figure A2 found that localities with full *Prospera* penetration received roughly 10,000 pesos per household per month in 2017, implying that rollback causes 13% of boys and 7% of girls to drop out in fully-penetrated localities. These magnitudes are similar to our main estimates using beneficiary shares, but the results for girls have smaller *p*-values.

The specification checks consistently find that rollback caused a large share of high-school-aged boys to drop out of school in communities where *Prospera* was widespread. They also find suggestive evidence of a dropout response among high-school-aged girls, albeit less consistently. Estimates at middle and primary school ages are smaller and less robust. Given these findings, we focus on high school ages for all remaining analyses. We continue to study impacts for girls, given possible effects on their labor supply and time use. However, because the results so far suggest considerable gender differences in rollback impacts, we carry out all remaining analyses separately by gender.

4.3 Accounting for the Replacement Program

Another set of questions relates to the rollout of *Prospera*'s replacement program, BBJ. If BBJ mitigated the negative effect of *Prospera* rollback, then our estimates may understate the extent to which *Prospera* boosted school enrollment until its demise. As documented in Section 2, BBJ spending over 2019 approached the total previously spent on *Prospera* in 2017, but with substantial declines in progressivity. Outside the least marginalized localities, transfers per household were substantially lower under BBJ than *Prospera*. In our main sample, 72% of children live in localities that received less from BBJ in 2019 than from Prospera in 2017. Even so, the BBJ transfers might have mitigated the effect of *Prospera* rollback on school enrollment, even in the most marginalized localities.

We explore this hypothesis by adding BBJ penetration and associated time interactions

Table 2: Effects of *Prospera* rollback versus BBJ rollout, ages 15-17, by gender

	(1)	(2)	(3)	(4)
A. Boys				
Prospera share × 2019-20	-0.123*** [0.036]	-0.109*** [0.038]		
Prospera benefits per HH × 2019-20			-0.013*** [0.003]	-0.011*** [0.004]
BBJ benefits per HH × 2019-20		-0.009* [0.005]		-0.008 [0.005]
N	89,275	89,275	89,275	89,275
B. Girls				
Prospera share × 2019-20	-0.054 [0.036]	-0.057 [0.037]		
Prospera benefits per HH × 2019-20			-0.007** [0.003]	-0.007** [0.003]
BBJ benefits per HH × 2019-20		-0.003 [0.005]		-0.003 [0.005]
N	85,723	85,723	85,723	85,723

Note: Brackets contain standard errors clustered by locality. Dependent variable is school enrollment. Benefits per household are measured in thousands of pesos per month. All regressions include state-by-quarter fixed effects. Columns (1)-(2) include the *Prospera* share and its interaction with a 2018-19 indicator. Columns (3)-(4) include *Prospera* benefits per household (in thousands of pesos per month) and its interaction with a 2018-19 indicator. Columns (2) and (4) include BBJ benefits per household (in thousands of pesos per month) and its interaction with a 2018-19 indicator. Sample excludes summers and localities with more than 100,000 residents. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

to equation (1):

$$
\begin{aligned}
y_{ilst} = {} & \alpha_P Prospera_{ls} + \gamma_P Prospera_{ls} \mathbb{1}_{2018/19} + \beta_P Prospera_{ls} \mathbb{1}_{2019/20} \\
& + \alpha_B BBJ_{ls} + \gamma_B BBJ_{ls} \mathbb{1}_{2018/19} + \beta_B BBJ_{ls} \mathbb{1}_{2019/20} + \tau_{st} + \epsilon_{ilst}
\end{aligned}
\tag{3}
$$

where the new covariate, BBJ_{ls}, is 2019 BBJ penetration, measured using benefits per household (in thousands of pesos per month). BBJ penetration cannot be measured as a share because the program disburses grants to both households and teenagers, and the two are not easily linked. $Prospera_{ls}$ is 2017 *Prospera* penetration, measured either using the share of households receiving benefits (as in our standard approach) or using benefits per household (to match the BBJ measure). As in equation (1), the regression includes state-quarter fixed effects.

The BBJ interaction controls for differential changes in school enrollment between localities with high and low BBJ penetration in the first year. To the extent that localities with high and low BBJ penetration would have followed parallel trends in the absence of BBJ rollout, the coefficient also provides an estimate of the early effect of BBJ. However, the goal is not to evaluate BBJ, which was in the early stages of rollout, but instead to assess whether accounting for its rollout alters the estimated effects of *Prospera* rollback.

Results for 15-17 year-olds appear in Table 2, with boys in Panel A and girls in Panel B. Neither gender shows evidence of BBJ boosting school enrollment, nor of BBJ rollout patterns explaining the estimated effect of *Prospera* rollback. Column (1) repeats our benchmark school enrollment estimations, as previously reported in Table 1. Column (2) augments this specification by including BBJ benefits per household and associated interactions. Including the BBJ interaction slightly shrinks the estimate of β for boys, from -12.3 percentage points to -10.9 for ages 15-17. The interaction of BBJ penetration with the 2019-20 indicator has a small, negative coefficient. Localities receiving more BBJ benefits experienced relative *declines* in enrollment, although this result is not always statistically significant. This striking finding suggests that BBJ expenditures did not boost enrollment.

26

Columns (3)-(4) redo the exercise using *Prospera* benefits per household, allowing us to more directly compare the impacts of *Prospera* spending with the impacts of BBJ spending. Here again, the estimated effects of rollback are not sensitive to controlling for BBJ rollout. Cuts to *Prospera* of 1,000 pesos per household per month reduce boys' enrollment by 1.1 and 1.3 percentage points, respectively, in the specifications with and without BBJ interactions. The same cuts reduce girls' enrollment by 0.7 percentage points, irrespective of the specification. That the effect on girls' enrollment is significant when we use benefits but not when we use shares matches results from Appendix Figure A8, as we discussed in Section 4.2.

In summary, our analysis suggests that the BBJ program, despite spending a similar amount to *Prospera*, had no mitigating effect on school enrollment in its first year. The lack of an effect likely reflects the drastic reduction in progressivity relative to *Prospera*. BBJ spent a great deal of money in relatively affluent localities, apparently with little benefit for school enrollment. The lack of an effect may also be attributable to BBJ's program structure, which paid youth rather than their parents and reduced incentives for being in school. Our finding that BBJ failed to raise school enrollment is consistent with analysis by CONEVAL (2024). It is also consistent with Dustan's 2020 finding that *Prepa Sí*, a predecessor to the high school component of BBJ that rolled out in Mexico City a decade earlier, failed to raise high school enrollment.

4.4 Assessing Selectivity

Rollback could have influenced the composition of the sample, a concern for internal validity. Children begin leaving their parents' homes during their teenage years, and both they and their parents may adjust their migration decisions in response to rollback. Virtually all primary-school-aged children live with a parent or guardian, but the share falls to 98% for boys and 94% for girls by age 17 (Appendix Figure A9). Patterns of attrition from the 5-wave rotating panel are somewhat different, with younger children more likely to leave the panel early (Appendix Figure A10). This surprising result reflects the higher propensity of

households to attrit when their children are young.[7] At high school ages, the round-to-round attrition rate is 3% for both boys and girls.

Rates of home-leaving and attrition are not large, but they still raise an identification concern. If rollback affects either, then the sample will be selected on the policy change of interest, and the effect estimate may be biased. To assess this possibility, we estimate the effect of rollback on attrition and co-residence with a parent or guardian using the difference-in-differences specification of equation 1. We examine attrition by entire households and by individuals irrespective of their households.[8]

Appendix Table A3 reports estimates of the effect of rollback on attrition and parental co-residence for boys and girls aged 15-17. The results reveal no evidence of endogenous sample selection for either sex. Across all six regressions (household attrition, individual attrition, and co-residence, for boys and girls), all point estimates are smaller than the associated standard errors. In all cases, the 95% confidence intervals exclude effects exceeding 3 percentage points in either direction.

4.5   Assessing Heterogeneity

We now turn to an analysis of heterogeneity in the effect of rollback on enrollment, focusing again on 15-17 year-olds by gender. *Prospera*'s program design predicts specific forms of effect heterogeneity. First, the program was means-tested, implying larger effects in poorer households. We do not observe poverty status, so we instead split the sample on the household head's education. Insofar as the head's education level can proxy for the household's economic status, we expect larger effects for households with less-educated heads. Second, the program was geographically targeted toward rural, high-marginality localities. Although one might intuit larger effects in targeted areas, the locality *Prospera* share already incorporates geographic targeting. The causal response to the *Prospera* share may not differ

---

[7]The ENOE does not track households if they leave the community where they resided in the baseline panel wave, so attrition is a proxy for migration.

[8]Attrition is defined only for the first four rounds of the rotating panel, and only through the fall of 2019.

Table 3: Heterogeneity in effect of rollback on school enrollment, ages 15-17, by gender

	Head's education level		Locality pop.		Locality marg.	
	≤ primary	> primary	< 2,500	≥ 2,500	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
A. Boys						
Prospera share	-0.179***	-0.057	-0.149***	-0.184***	-0.154***	-0.136
× 2019-20	[0.050]	[0.043]	[0.050]	[0.067]	[0.054]	[0.094]
Dep. var. mean	0.617	0.839	0.670	0.768	0.662	0.775
N	41,957	47,268	33,553	55,722	32,283	56,992
B. Girls						
Prospera share	-0.063	-0.013	-0.097**	-0.091	-0.117**	-0.120
× 2019-20	[0.049]	[0.052]	[0.049]	[0.074]	[0.056]	[0.096]
Dep. var. mean	0.641	0.837	0.676	0.785	0.654	0.805
N	39,574	46,115	31,401	54,322	30,691	55,032

Note: Brackets contain standard errors clustered by locality. All regressions include the *Prospera* share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents. For locality marginalization, "high" indicates high and very high marginalization; "low" indicates very low, low, and medium marginalization. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

between localities with low and high shares. On the one hand, the binned estimates in Appendix Figure A6 did suggest non-linearities favoring fully-penetrated localities. On the other hand, Figure 3 found that effect estimates for 15-17 year-olds grew after conditioning on marginality-by-quarter interactions.

Table 3 displays enrollment impacts by the household head's education level, locality population, and locality marginalization. For boys (Panel A), we find the expected household-level heterogeneity but no clear locality-level heterogeneity. For girls (Panel B), we find no evidence of heterogeneity.

In Panel A, columns (1)-(2) show that the impacts of rollback on boys are concentrated in households with less educated heads, consistent with the means testing of the program. Boys from households with heads who completed primary or less (47% of the sample) experienced

an 18 percentage point reduction in school enrollment due to full rollback, a decline of almost 30% with respect to pre-rollback enrollment of 62%. Impacts for youth from more educated households are smaller and statistically insignificant, though still negative. The estimates in columns (1) and (2) are significantly different at the 6% level.

Continuing in Panel A, columns (3)-(4) show similar impacts of rollback on boys in rural localities (less than 2,500 inhabitants) and non-rural localities. Full rollback reduced enrollment by 15 percentage points in rural areas and 18 percentage points in non-rural areas. In percentage terms, these reductions look even more similar, as they correspond to a 22% decline in rural localities and a 24% decline in non-rural localities.[9]

Completing Panel A, columns (5)-(6) indicate similar program impacts for boys in localities with higher and lower marginalization, the official measure of area poverty used in Figure 1. Enrollment reductions are 15 percentage points for boys living in higher-marginalization localities and 14 percentage points for their counterparts in lower-marginalization localities. The effect is not statistically different from zero in the latter group, but this lack of significance reflects a larger standard error, not a substantively smaller point estimate. Even with these similar estimates of β, however, higher-marginality localities had much higher *Prospera* shares and therefore experienced a larger dose of rollback. In this sense, rollback did have larger impacts in the most disadvantaged localities.

Panel B of Table 3 reports heterogeneity results for girls ages 15-17. By mother's level of education, we find no significant impacts of rollback in either category (columns [1]-[2]). However, disaggregating by locality size or marginalization suggests that girls living in rural or poor localities experienced significant effects of rollback, on the order of 10-12 percentage points (columns [3] and [5]). These significant results do not signal treatment effect heterogeneity; the point estimates for non-rural or non-poor localities are similar, with larger standard errors (columns [4] and [6]). Instead, columns (3)-(6) suggest that one can find large effects on girls if one conditions sufficiently on area disadvantage. This same

---

[9]Recall that our sample excludes localities with more than 100,000 inhabitants, so non-rural localities have populations of 2,500-100,000.

insight emerged from Figure 3's regression with locality marginalization interactions, which similarly found a large and significant estimate for high-school-aged girls.

5   Effects of Rollback on Teenage Labor Supply

School and work may be substitutes (Ravallion and Wodon, 2001), and indeed, early studies of *Prospera*'s initial effects found significant reductions in labor market participation, mainly in boys (Skoufias and Parker, 2001). The results so far suggest pronounced impacts on teenage boys' school enrollment. If these boys work when they drop out of school, then high opportunity costs of schooling may explain the enrollment impacts of *Prospera* rollback.

Patterns of school and work by age confirm that the trade-off between the two uses of time are likely to be most pronounced for teenage boys. Appendix Figure A11 displays school and employment by age in our sample, demonstrating near-universal school enrollment, above 95% for both boys and girls, until about age 12, when enrollment starts to decline continuously, reaching about 65% by age 17. Boys have higher employment rates at all ages than girls. About 10% of boys work at age 12, rising to over 40% by age 17. For girls, employment rates are about 3% at age 12 and nearly 20% by age 17.[10]

We estimate effects on labor supply and other time use for the principally-affected age group: teenagers of high school age, 15-17. Our main interest is boys, given their pronounced dropout responses to rollback. But given the mixed evidence on girls, we report results for both genders. We first analyze labor supply outcomes: participation, hours, and income.[11] We then turn to other uses of time, which may be particularly important for girls, who are more involved than boys in household tasks.

We first estimate event studies based on equation (2). The results, reported in Figure 4, suggest that rollback raised boys' but not girls' labor supply. Pre-rollback trends appear rel-

---

[10]The ENOE work definition includes agricultural and unpaid work outside the home but not domestic work. The labor market questions are applied only to children age 12 and over.

[11]Due to the large number of zeros, we analyze income in levels rather than logs. At ages 15-17, 79% of boys and 92% of girls have no earnings.

Figure 4: Event studies for labor supply, ages 15-17, by gender

Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. All regressions include the *Prospera* share and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents. Earnings are measured in pesos.

atively parallel between high- and low-penetration localities for both genders. After rollback, however, boys in high-penetration localities display a large relative upswing in employment, hours, and earnings. The winter 2020 coefficient is significantly positive for employment at the 7% level, for hours at the 5% level, and for income at the 1% level. For girls, all event studies are flat.

Difference-in-differences estimations based on equation (1) summarize the event studies and increase power by pooling quarters. As reported in Table 4, these summary estimates indicate significant impacts on boys' labor supply but not girls'. For boys ages 15-17, rollback increased the probability of working by 8 percentage points ($p = 0.02$), 22% of the mean of 35%. Unconditional hours worked rose by 4.7 hours per week ($p < 0.01$), 41% of the mean of 11 hours per week. Earnings rose by 455 pesos per month ($p < 0.01$), 72% of the mean of 629. The hours and earnings effects together imply a wage rate of 22 pesos per hour, slightly more than 2 US Dollars at 2017 PPPs.

The average increase in boys' earnings was insufficient to recover the benefits lost to rollback, but boys who took on work may have made back more than their households lost. According to Appendix Figure A2, the average household in a fully penetrated locality received approximately 10,000 pesos in benefits in 2017. According to Appendix Table A1, a high-school-aged boy received approximately 900 pesos per month in school, with some variation by grade, or approximately 9,000 pesos over the year. Full rollback raised average earnings by 5,460 pesos on an annualized basis, too little to make back these amounts. However, if earnings growth were entirely on the extensive margin and therefore concentrated in the 7.8% of boys who started working, then these boys would have earned 70,000 pesos a year due to rollback, well in excess of any lost benefits. Some of the earnings growth likely occurred on the intensive margin, but this thought experiment clarifies that work by teenage boys likely recouped lost benefits for some families.

For girls, we observe no statistically significant effects of rollback on any of the outcomes in Table 4. However, especially for girls, school and work do not capture the full spectrum

Table 4: Effect of rollback on labor supply, ages 15-17, by gender

	Employment	Weekly hours	Monthly earnings
	(1)	(2)	(3)
A. Boys			
Prospera share	0.078**	4.68***	455***
× 2019-20	[0.032]	[1.30]	[114]
Dependent variable mean	0.353	11.38	629
N	89,272	89,272	89,272
B. Girls			
Prospera share	-0.016	-0.789	-30
× 2019-20	[0.022]	[0.874]	[53]
Dependent variable mean	0.134	3.86	194
N	85,721	85,721	85,721

Note: Brackets contain standard errors clustered by locality. All regressions include the *Prospera* share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents. Earnings are measured in pesos. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

of time use activities. The ENOE's time use module allows us to explore whether time use responded to rollback in other ways besides enrollment and labor supply data. Four categories of time use exceed 30 minutes per week on average for high-school-aged boys and girls: study, chores, household administration like shopping or accounting, and unpaid care. On average, girls and boys spend similar time studying, but girls devote 156% more time to chores, 62% more time to household administration, and 474% more time to unpaid care (Appendix Table A4).

Appendix Table A4 uses equation 1 to estimate effects on hours devoted to these major non-labor activities for high-school-aged boys and girls. The results uncover no new effects. Boys' study time declines by 2.7 hours ($p = 0.051$) on average, amounting to 58% of the increase in labor hours documented in Table 4. Otherwise, we find no other significant effects on non-labor time use. We conclude that, despite some suggestive evidence of enrollment impacts for younger boys and for high-school-aged girls, rollback was most consequential for high-school-aged boys.

6 How Costly Is Dropout For Teenage Boys?

For boys of high school age in fully-saturated localities, rollback reduced school enrollment by 12 percentage points and raised employment by 8 percentage points. How costly will these changes be over the lifetimes of affected boys? Conditional cash transfers are premised on the idea that schooling is worthwhile, but families are liquidity-constrained and struggle to finance their children's studies. In this view, the dropout we observe is inefficient; the boys who left school as a result of rollback would have been better off staying in school with a loan to finance it. However, perhaps *Prospera* had been inefficiently subsidizing teenage boys' education, keeping them in school despite negligible returns. The crux of the question is whether the value of high school exceeds the value of the earnings and work experience that these boys would give up to go to high school.[12]

---

[12]This framing sidesteps the value of cash to the poor, which we consider outside the scope of this paper.

In principle, one could eventually study these issues in a long-term follow-up of the cohort affected by rollback. However, the COVID pandemic may complicate long-term comparisons, and large Mexican datasets do not currently provide detailed histories on the locality of residence. We build evidence on these issues by further probing school and work impacts in teenage boys, and by examining the observational trade-off between work and school.

6.1　More Evidence on Labor Supply Effects for Teenage Boys

We estimate effects of rollback on two more detailed aspects of the school and work outcomes of boys ages 15-17. First, we consider the joint distribution of school and work, asking whether dropout led to idleness in some boys. Second, we consider the sectoral composition of rollout-attributable employment growth, with an eye toward whether the relevant sectors provide opportunities for the wages of high school dropouts to keep up with those of high school graduates, for example through the returns to experience.

As in many Latin American countries, a large number of Mexican youth neither work nor study (De Hoyos et al., 2016). Any effect of rollback on this circumstance, which presumably has no remuneration nor experience return, would push against arguments about efficient dropout. Indeed, Tables 1 and 4 revealed a larger enrollment response than employment response among teenage boys, suggesting a possible increase in idleness. In the Appendix, we probe this result further by estimating the effects of rollback on particular work-school combinations: study without work, study with work, work without study, and the absence of both. We generate an indicator for each combination and estimate its response to rollback using equation (1).

Appendix Table A5 reports estimated effects on each work-school combination, with results indicating a large impact on exclusive employment and none on idleness. Exclusive work rose by 14 percentage points ($p < 0.01$) in full-rollback relative to no-rollback localities, while idleness did not change significantly, with a point estimate of -1 percentage points. Meanwhile, exclusive study fell by 7 percentage points ($p = 0.049$), and combined work and

study fell by 6 percentage points ($p = 0.01$). The estimated impact on exclusive work is larger than the estimated impact on employment, reported in Table 4. Thus, rollback pushed both working and non-working students into working exclusively. In principle, exclusive work of this sort has the potential to generate human capital through experience.

The sector of employment may also offer clues into the scope for dropouts to keep up with graduates. Table 5 assigns jobs to sectors using three categorizations: formality, industry, and occupation. We use equation (1) to estimate the effect of rollback on the probability of having a job in each sector. We do not condition on working, so we expect positive effects in possibly multiple sectors. A larger positive coefficient for a given sector implies that it accounts for a larger share of the rise in employment.

Panel A of Table 5 breaks down the employment effects by formality, finding a larger effect in the informal sector. The share of 15-17 year-old boys working in formal jobs rose by 1.5 percentage points in full-rollback compared to no-rollback localities ($p < 0.01$), relative to a sample-wide share of 1%. The share working in the informal sector rose by 6.2 percentage points ($p = 0.06$), relative to a sample-wide share of 34%. These estimates imply that roughly 4 in 5 new jobs were informal. Most workers in this demographic group have informal jobs, so the employment response to rollback was concentrated in the informal sector. In observational data, the returns to experience tend to be higher in formal than informal jobs (World Bank, 2019), a pattern confirmed in a structural model estimated using Mexican data (Bobba et al., 2021). The larger effects on informal jobs may limit the scope for human capital in the form of experience.

Nevertheless, the industry and occupation results in Panels B and C suggest that affected boys took on jobs in industries and occupations that turn out to have higher returns to experience. Panel B looks at industry, including the five major industry groups defined in the survey. The share working in construction rose by 3.8 percentage points ($p = 0.02$), in commerce by 2.4 percentage points ($p = 0.04$), and in services by 1.8 percentage points ($p = 0.09$). We find no significant effects on the shares working in manufacturing or agriculture.

Table 5: Effect of rollback on work by sector, boys ages 15-17

A: Formality	Formal	Informal			
	(1)	(2)			
Prospera share	0.015***	0.062*			
× 2019-20	[0.006]	[0.033]			
Share in sector	0.009	0.341			
N	88,848	88,848			
B: Industry	Construct.	Manuf.	Commerce	Services	Agric.
	(3)	(4)	(5)	(6)	(7)
Prospera share	0.038**	0.017	0.024**	0.018*	-0.018
× 2019-20	[0.017]	[0.016]	[0.011]	[0.011]	[0.030]
Share in industry	0.041	0.041	0.046	0.059	0.163
N	88,848	88,848	88,848	88,848	88,848
C: Occupation	Industrial	Mercantile	Personal	Agric.	
	(8)	(9)	(10)	(11)	
Prospera share	0.056***	0.012	-0.002	0.006	
× 2019-20	[0.021]	[0.011]	[0.008]	[0.029]	
Share in occupation	0.097	0.045	0.030	0.163	
N	88,848	88,848	88,848	88,848	

Note: Brackets contain standard errors clustered by locality. All regressions include the *Prospera* share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents. The industry and occupation categories account for >99% and 97% workers, respectively. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Panel C's breakdown by occupation leads to similar results. The Mexican occupational classification has ten categories; we focus on the four that account for 97% of our sample. The share working in an industrial occupation rose by 5.6 percentage points ($p < 0.01$), relative to a sample-wide share of 10 percentage points. The estimated effects on employment as a merchant, a personal services worker, or an agricultural worker are close to zero and statistically insignificant.

The results for agriculture in Panels B and C are striking. Agriculture is the most common industry and occupation in our sample, accounting for nearly half of jobs among 15-17 year-old boys.[13] However, when rollback pushed boys out of school and into work, none selected into agriculture. Agriculture has shallow experience-wage profiles (Islam et al., 2018), so in this sense, rollback dropouts selected into industries and occupations that afford more opportunity for keeping up with high school graduates.

## 6.2 Observational Evidence on the School-Work Trade-off

For further insight into the relative return to high school versus teenage work experience across sectors, we provide observational evidence on adult male workers in the ENOE. We use the same localities and survey waves as the main sample, but we now focus on men aged 20-49 who are not in school, work at least 30 hours per week, and have positive labor income. We analyze mean hourly earnings by 5-year age group, comparing men who finished high school but went no further to men who finished middle school but dropped out before finishing high school.

Wage differences between these groups by age speak to the relative labor market returns to high school over teenage work experience at different stages in the life cycle. Appendix Figure A12 plots mean hourly earnings by age group for each group, finding that high school graduate wages exceed high school dropout wages at every age, with wider gaps at older ages. Graduates outearn dropouts by 3% at ages 20-24 and 29% at ages 40-45. These comparisons

---

[13]The industry and occupation shares in Table 5 are not conditioned on working. Agriculture accounts for 47% of jobs worked by 15-17 year-old boys, using either the industry or occupation classification.

Figure 5: Age profiles of men's hourly earnings by industry and highest completed schooling

A. Hourly earnings by industry

B. Industry shares
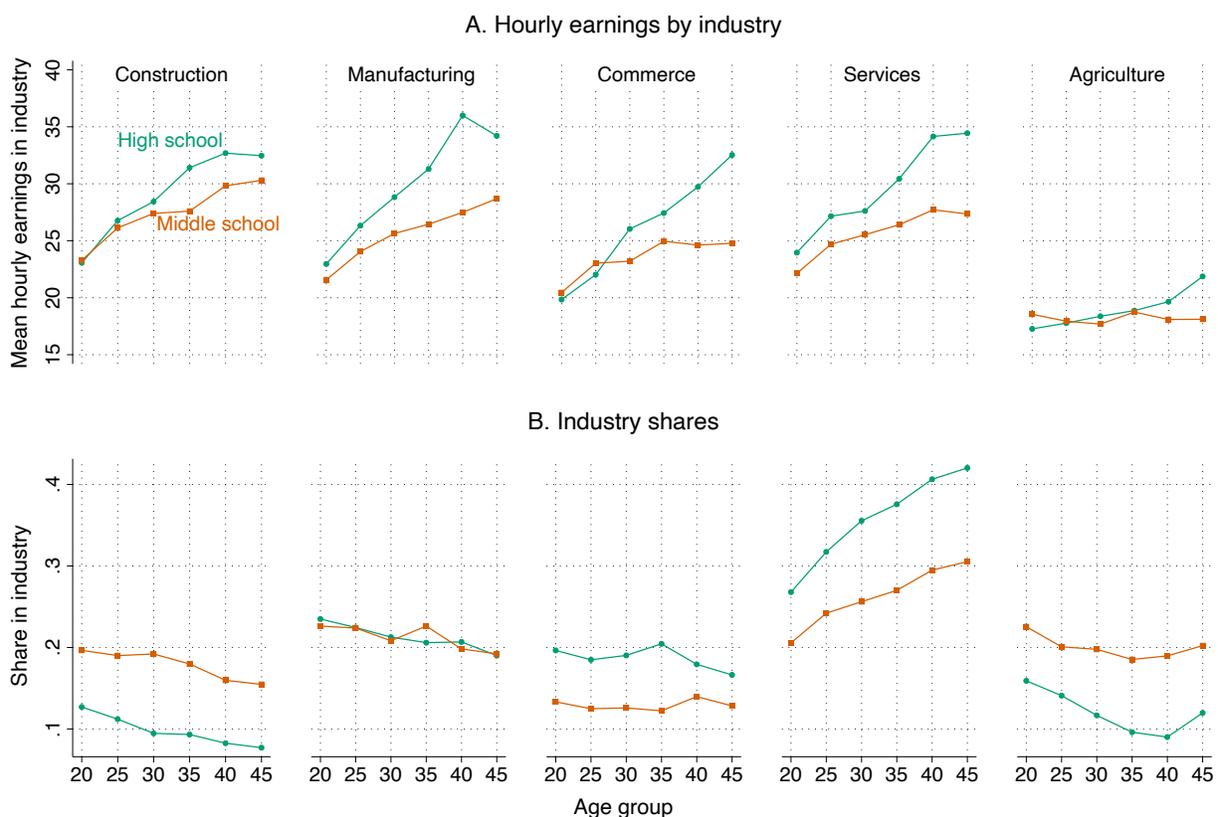
Note: Mean hourly earnings among male workers who completed middle school but not high school or left school after completing high school. Sample includes men who are not enrolled in school, work at least 30 hours per week, and have nonzero labor earnings. Sample excludes summers and localities with more than 100,000 residents.

40

are observational, so they raise the usual concerns about selection into education level. The largely cross-sectional differences across ages may also confuse cohort effects with age effects. Nevertheless, the widening gaps suggest that boys contemplating dropout may need to wait a number of years before a high school degree pays off.

These overall wage gaps reflect both inter- and intra-industry differences in the wages of graduates and dropouts. Given the previous section's results on the industrial composition of the employment effects, we take special interest in these industry patterns.[14] Figure 5 divides the sample into the five major industry groups. Panel A plots the mean hourly earnings of graduates and dropouts across age groups by industry. Panel B plots the share of workers from each age group who work in each industry.

The industry shares in Panel B of Figure 5 make clear that industry selection is a major contributor to graduate-dropout wage gaps. Compared to dropouts, graduates are 6-12 percentage points more likely to work in services, depending on the age group, and 8-11 percentage points less likely to work in agriculture. Services jobs pay more than agricultural jobs, with a 20-53% wage premium even among dropouts.

Within industries, graduate-dropout wage gaps over the life cycle start small but grow with age. At ages 20-24, graduates average 2% more than dropouts in the same industry; at ages 40-44, they average 21% more. Disaggregating by industry in this way introduces a second margin of selection, since dropouts and graduates may differentially sort into industries on unobservable characteristics. But the within-industry results confirm the sample-wide pattern that graduate-dropout wage gaps are small in the 20s and large in the 40s. This pattern is most pronounced in construction and commerce, the industries that account for the most rollback-attributable jobs. In both of these industries, graduates do *not* outearn dropouts at ages 20-24. But just as in other industries, gaps emerge with age, reaching 10% in construction and 21% in commerce at ages 40-44.

---

[14]We focus on industry rather than occupation because the former has fewer categories in the ENOE, allowing for a more succinct description of patterns across sectors. As we describe below, analysis by occupation has similar takeaways.

The results by industry clarify the decision problem facing teenage boys on the margin of dropout. Suppose first that they plan to stay in the same industry their entire careers. Those planning careers in construction or commerce can expect to see no wage benefit from staying in school for at least a decade, at least based on Figure 5's average wage variation. The fixed industry assumption may seem implausible, but the logic holds if we relax it too. At ages 20-24, the average wage for dropouts working in construction exceeds the average wage for graduates working in every industry but services. Relative to this option, opportunities for earning substantially more as a graduate emerge only in the mid-30s, principally in construction, manufacturing, and services. These inferences are subject to critiques about selection bias, but if a boy considering dropout looks around himself, he may not see much near-term benefit to staying in school. The payoffs in middle age appear substantial, but they are a long way off. Without liquidity, dropping out and working in construction may be very attractive.

6.3 Summary

Overall, the results suggest that rollback pushed teenage boys into work that is as lucrative as high school in the short run but not the long run. Boys who dropped out of school due to rollback predominantly worked informal construction jobs, which pay at least as well as most jobs for high-school graduates through the mid-30s. Rollback did not expand the ranks of Mexico's *ninis*, who neither study nor work.

A major motivation for conditional cash transfers was that they boosted schooling by relieving poor households' liquidity constraints (Progresa, 1997). The effects of rolling back *Prospera* are consistent with this view, in the sense that rollback led teenage boys to forego far-off schooling returns. Arguably, this circumstance is a classic setting for government intervention to relieve liquidity constraints.

7 Conclusions

The Mexican government unexpectedly rolled back its pioneering CCT program after more than two decades of successful operation. Over its more than two decades of operation, the program had demonstrated clear and accumulating positive impacts on educational attainment. While the program's initial effects were concentrated in middle school, impacts spread to the high school level as education levels increased in Mexico (Parker and Vogl, 2023), suggesting the program's adaptation to changing economic conditions.

We study the effects of this rollback on school enrollment and teenage work in its immediate aftermath. Our estimates suggest that rollback led to significant declines in school enrollment, principally for youth of high school ages, where enrollment rates lag behind those at younger ages. Our benchmark regression estimates suggest that in communities with full program penetration in the lead-up to rollback, the cessation of benefits decreased school enrollment by 9 percentage points among high-school-aged youth, relative to an overall enrollment rate of 73 percent. For boys in this age group, the effect was 12 percentage points. These negative effects of *rollback* are larger than the corresponding positive effects of *rollout* found in early program evaluations (Schultz, 2004), albeit at different schooling levels.[15]

Our results suggest larger effects for boys than girls. For boys ages 15-17, the 12 percentage point decline in enrollment corresponds to 17% of average enrollment, a pronounced effect. The decline grows to 18 percentage points, or 29% of average enrollment, among boys from disadvantaged households. For girls ages 15-17, estimates are smaller and not consistently different from zero, although we do see significant effects in particular subsamples or using particular alternative regression specifications. *Prospera* provided larger transfers to girls, so that one might expect rollback to have had larger effects on girls. Perhaps the program's preferential treatment of girls shielded them from rollback by improving beneficiary families' attitudes towards their education. But work opportunities more clearly play

---

[15]The initial evaluation results in Schultz (2004) were based on an experimental evaluation sample of 506 communities in seven states. Our results here reflect nationwide impacts, as in Parker and Vogl (2023).

a role in the gender heterogeneity. Teenage boys are more likely to work outside of the home than teenage girls, and rising work accompanied boys' falling school enrollment.[16] Our explorations of the school-work trade-off suggest clear returns to completing high school for boys on the margin of dropping, but they may not materialize until middle age.

Overall, our analysis suggests large and important costs of rollback to educational attainment in children of former *Prospera* households, especially marginal high school students. Our results are particularly striking because they are *net* of the implementation of a substitute program. The substitute, BBJ, was implemented within months of *Prospera*'s rollback and in fact spent a comparable amount in 2019 to *Prospera* in 2017. However, we demonstrate that the transition to BBJ significantly reduced resources for *Prospera* communities and families, and we find little evidence that spending on the new program boosted enrollment. At least in the pre-pandemic period we study, the new program moved resources away from the poor, rural communities at the center of *Prospera*, with little apparent benefit.

Our findings provide critical and timely new empirical evidence to inform the design and continuation of CCT programs in Mexico and the rest of the world, more than two decades after their original creation. Conditional cash transfer programs have a credible and large body of evidence on short and medium run effects, and evidence is just beginning to accumulate on the positive effects of CCTs in the longer run (Molina Millán et al., 2020; Araujo and Macours, 2021; Parker and Vogl, 2023; Barham et al., 2024). Our results suggest that the sudden ending or reversal of these programs can quickly erase progress. They also provide a cautionary tale on the perils of substituting a new untested program for one where two decades of rigorous evidence had suggested positive impacts.

---

[16]In Mexico as across the globe, mounting evidence indicates that females outperform males in high school and above (UNESCO, 2022).

References

Adato, M., de la Briere, B., Mindek, D., and Quisumbing, A. R. (2000). The impact of progresa on women's status and intrahousehold relations: Final report. *International Food Policy Research Institute.*

Araujo, M. C. and Macours, K. (2021). Education, income and mobility: Experimental impacts of childhood exposure to Progresa after 20 years.

Baird, S., McIntosh, C., and Özler, B. (2019). When the money runs out: Do cash transfers have sustained effects on human capital accumulation? *Journal of Development Economics*, 140:169–185.

Barham, T., Macours, K., and Maluccio, J. A. (2024). Experimental evidence from a conditional cash transfer program: Schooling, learning, fertility, and labor market outcomes after 10 years. *Journal of the European Economic Association*, 22(4):1844–1883.

Behrman, J. R., Parker, S. W., and Todd, P. E. (2009). Schooling impacts of conditional cash transfers on young children: Evidence from Mexico. *Economic Development and Cultural Change*, 57(3):439–477.

Behrman, J. R., Parker, S. W., and Todd, P. E. (2011). Do conditional cash transfers for schooling generate lasting benefits? A five-year followup of PROGRESA/Oportunidades. *Journal of Human Resources*, 46(1):93–122.

Behrman, J. R., Sengupta, P., and Todd, P. (2005). Progressing through PROGRESA: An impact assessment of a school subsidy experiment in rural Mexico. *Economic Development and Cultural Change*, 54(1):237–275.

Blattman, C., Fiala, N., and Martinez, S. (2020). The long-term impacts of grants on poverty: Nine-year evidence from Uganda's Youth Opportunities Program. *American Economic Review: Insights*, 2(3):287–304.

Bobba, M., Flabbi, L., Levy, S., and Tejada, M. (2021). Labor market search, informality, and on-the-job human capital accumulation. *Journal of Econometrics*, 223(2):433–453.

Callaway, B., Goodman-Bacon, A., and Sant'Anna, P. H. (2024). Difference-in-differences with a continuous treatment. *National Bureau of Economic Research Working Paper # 32117.*

CONEVAL (2020). Evaluación de diseño con trabajo de campo del Programa Becas de Educación Básica, para el Bienestar Benito Juárez 2019-2020. *Consejo Nacional de Evaluacion de la Politica de Desarrollo Social.*

CONEVAL (2024). Evaluación de impacto del Programa de Becas de Educación Básica para el Bienestar Benito Juárez. *Consejo Nacional de Evaluación de la Política de Desarrollo Social.*

De Hoyos, R., Rogers, H., and Székely, M. (2016). Out of school and out of work: Risk and opportunities for Latin America's ninis. *World Bank Research Working Paper #8402*.

Dustan, A. (2020). Can large, untargeted conditional cash transfers increase urban high school graduation rates? Evidence from Mexico City's Prepa Sí. *Journal of Development Economics*, 143:102392.

Gertler, P. (2004). Do conditional cash transfers improve child health? Evidence from PROGRESA's control randomized experiment. *American Economic Review*, 94(2):336–341.

Gertler, P. J. and Boyce, S. (2003). An Experiment in Incentive-Based Welfare: The Impact of PROGRESA on Health in Mexico. Royal Economic Society Annual Conference.

Haushofer, J. and Shapiro, J. (2018). The long-term impact of unconditional cash transfers: Experimental evidence from kenya. working paper.

Hoddinott, J. and Skoufias, E. (2004). The impact of progresa on food consumption. *Economic Development and Cultural Change*, 53(1):37–61.

Ibarrarán, P., Medellín, N., Regalia, F., Stampini, M., Parodi, S., Tejerina, L., Cueva, P., and Vásquez, M. (2017). *How Conditional Cash Transfers Work*. Inter-American Development Bank.

Islam, A., Jedwab, R., Romer, P., and Pereira, D. (2018). Returns to experience and the misallocation of labor. *Background paper for World Development Report 2019: The Changing Nature of Work*.

Jaramillo-Molino, M. (2020). Despues de prospera. *Nexos*.

Molina Millán, T., Macours, K., Maluccio, J. A., and Tejerina, L. (2020). Experimental long-term effects of early-childhood and school-age exposure to a conditional cash transfer program. *Journal of Development Economics*, 143:102385.

Parker, S. W. and Todd, P. E. (2017). Conditional cash transfers: The case of Progresa/Oportunidades. *Journal of Economic Literature*, 55(3):866–915.

Parker, S. W. and Vogl, T. (2023). Do Conditional Cash Transfers Improve Economic Outcomes in the Next Generation? Evidence from Mexico. *The Economic Journal*, 133(655):2775–2806.

Parker, S. W. and Vogl, T. (2024). Becas Benito Juárez versus Prospera: Efectos de la transición en la política social sobre las transferencias monetarias recibidas por los hogares en México. *Foro Economico*.

Progresa (1997). *Progresa: Programa de Educacion, Salud y Nutricion*.

Ravallion, M. and Wodon, Q. (2001). Does Child Labour Displace Schooling? Evidence on Behavioural Responses to an Enrollment Subsidy. *The Economic Journal*, 110(462):C158–C175.

Rivera, J., Sotres-Alvarez, D., Habicht, J., Shamah, T., and Villalpando, S. (2004). Impact of the Mexican program for education, health, and nutrition (Progresa) on rates of growth and anemia in infants and young children: a randomized effectiveness study. *JAMA*, 291(21):2563–2570.

Schultz, T. P. (2004). School subsidies for the poor: Evaluating the Mexican Progresa poverty program. *Journal of Development Economics*, 74(1):199–250.

Skoufias, E. and Parker, S. W. (2001). Conditional cash transfers and their impact on child work and schooling: Evidence from the Progresa program in Mexico. *Economia*, 2(1):45–96.

UNESCO (2022). *Leave No Child Behind: Global Report on Boys' Disengagement from Education.* Paris: UNESCO.

World Bank (2019). *World Development Report 2019: The Changing Nature of Work.* World Bank.

Figure A1: School enrollment over time, census data
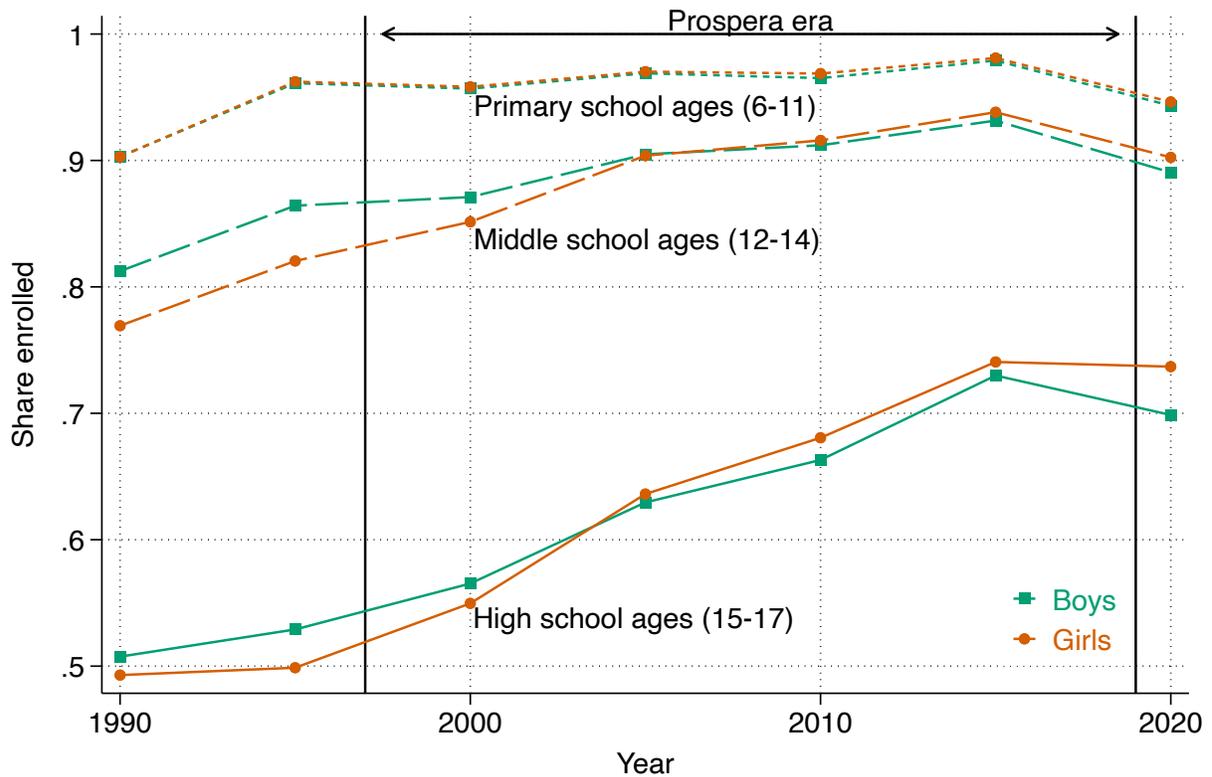
Note: Data are from the 1990, 2000, 2010, and 2020 censuses and the 1995, 2005, and 2015 intercensal surveys. The age ranges for primary, middle, and high school follow a typical student's grade progression in the Mexican system. The 2020 census was collected throughout March, with an official reference date of March 15. Mexican public schools shut down due to the coronavirus pandemic on March 20.

Figure A2: Benefits by locality *Prospera* beneficiary share
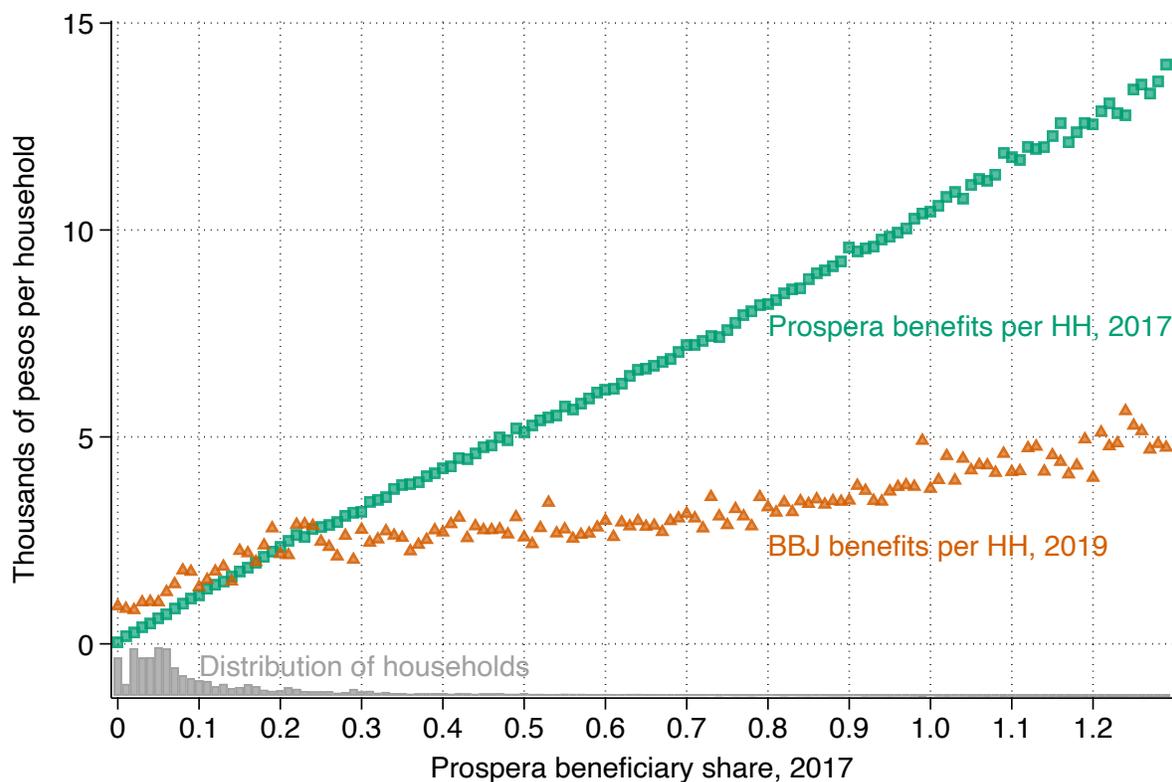
Note: Sample includes localities with at least 100 residents, which contain 98% of the Mexican population. Beneficiary data are from program administrative records; household counts are from ITER. *Prospera* data are for the last non-electoral year preceding rollback, 2017; Becas Benito Juárez (BBJ) data are for the first year of operation, 2019. Household counts are for 2010, the most recent census preceding the rollback of *Prospera*. Some localities have beneficiary shares above 100 due to population growth since the most recent census.

Figure A3: *Prospera* beneficiary share by locality size
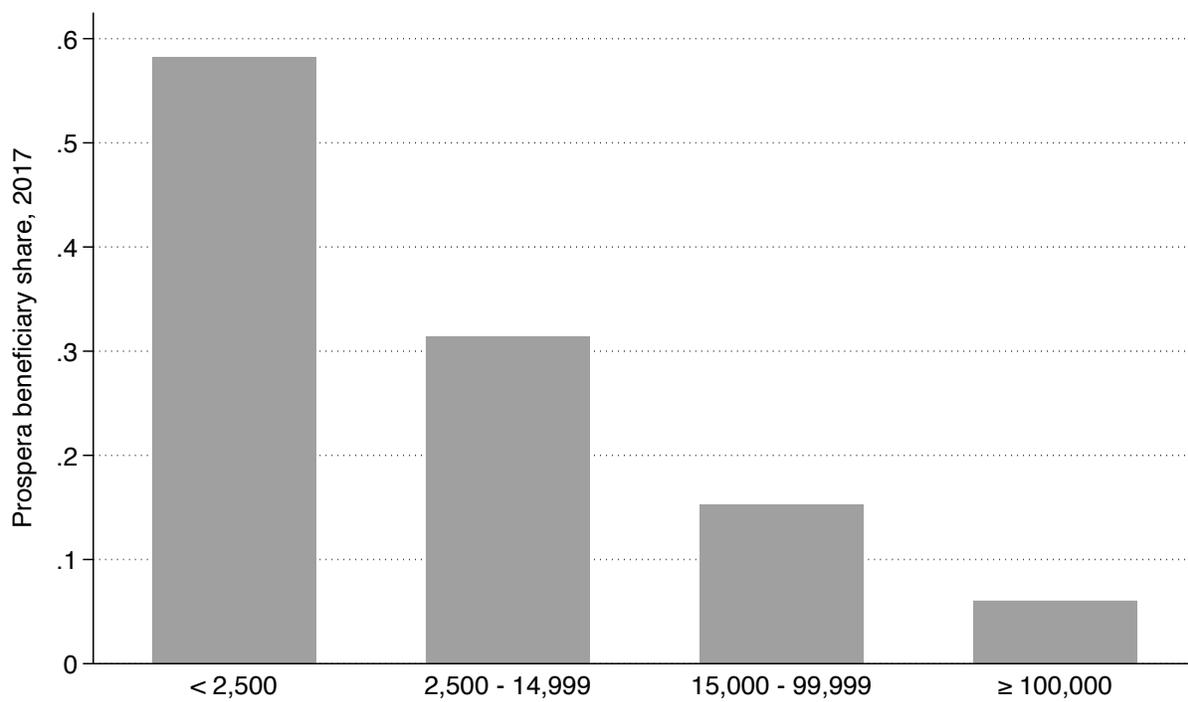
Note: Sample includes localities with at least 100 residents, which contain 98% of the Mexican population. The ENOE is designed to be representative of localities in each of the population categories.

Figure A4: School-leaving rates by season
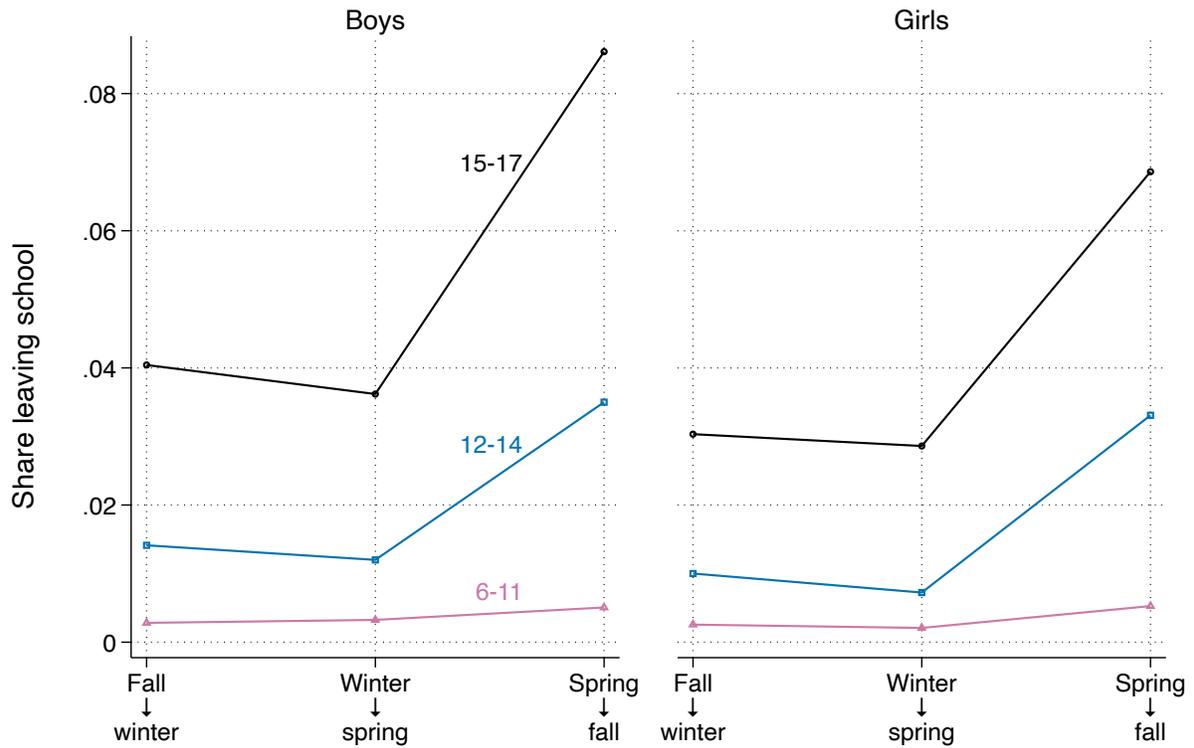
Note: Share of children enrolled in the starting season who were not enrolled in the ending season, based on the ENOE rotating panels. Age is measured in the starting season; 17-year-olds who turned 18 are excluded. Sample excludes summers and localities with more than 100,000 residents.

Figure A5: Robustness of enrollment results to omission of individual states
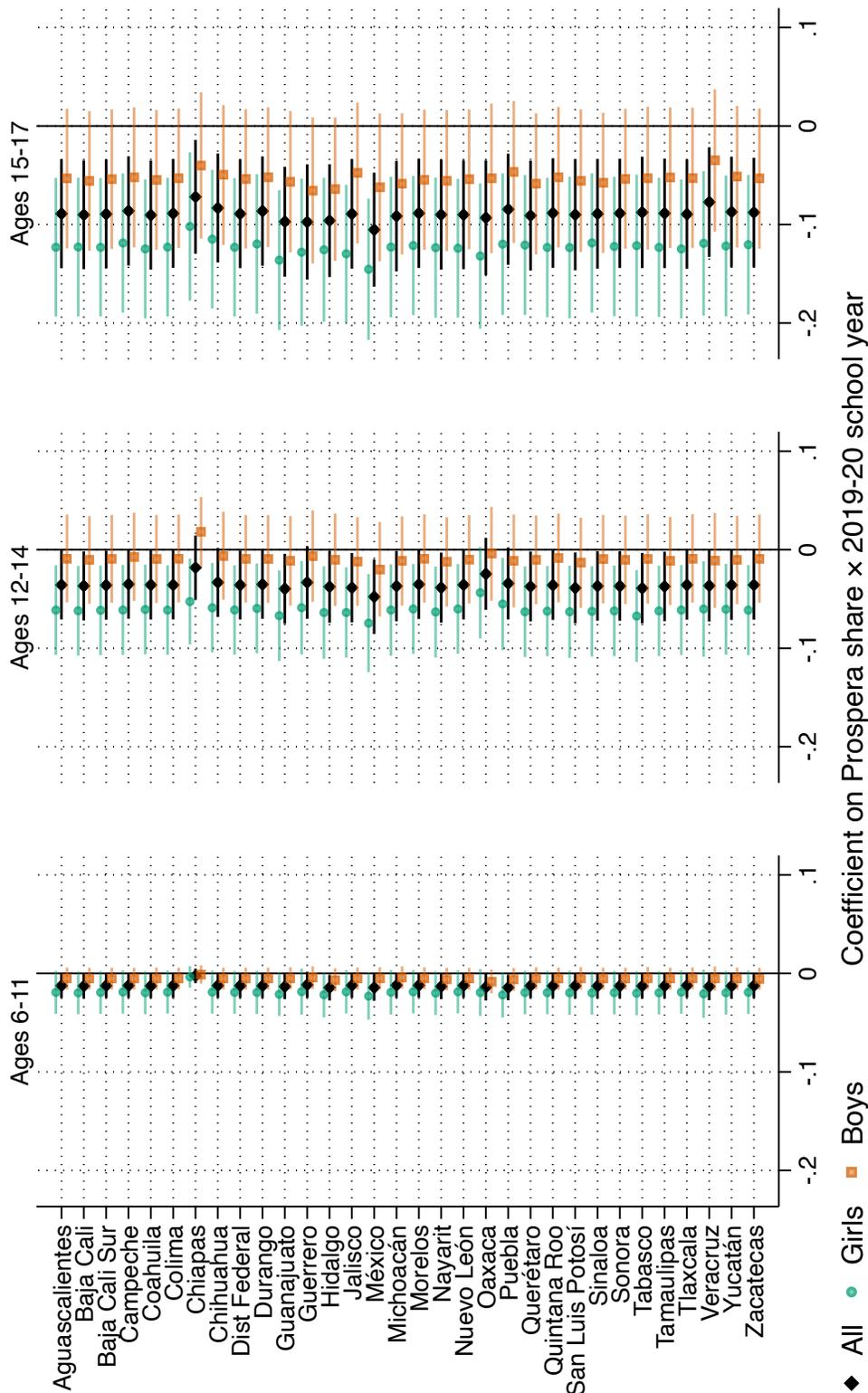
Coefficient on Prospera share × 2019-20 school year
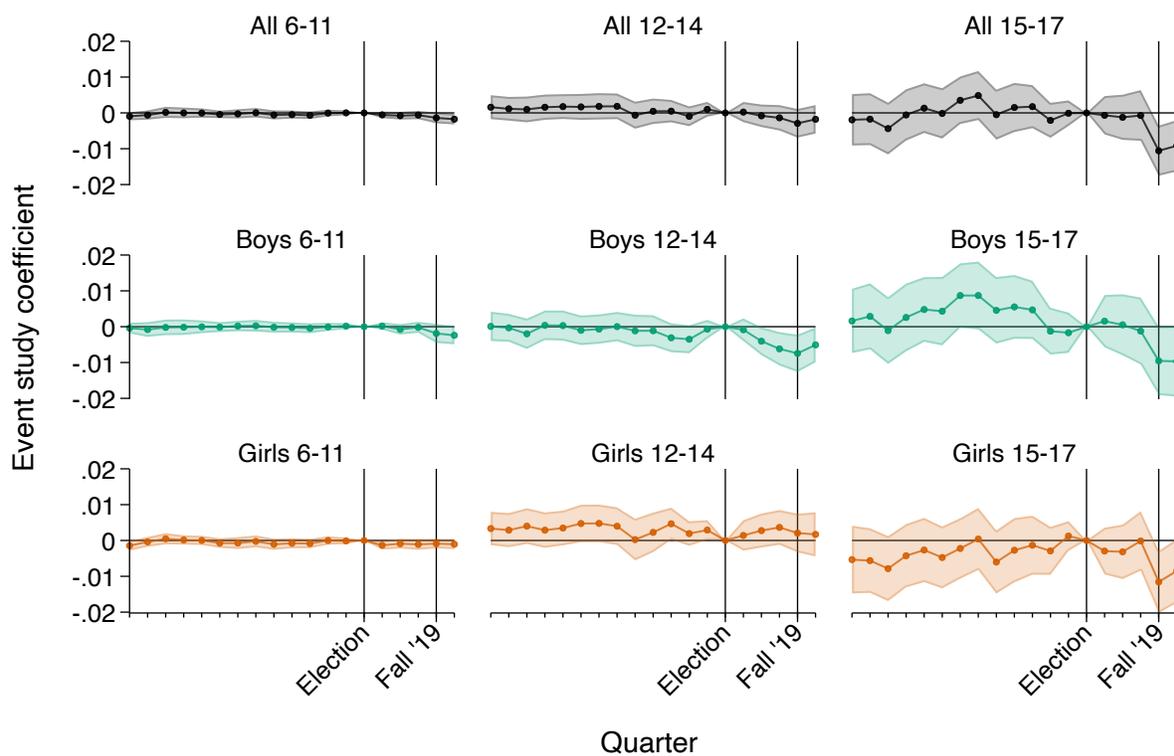
◆ All   ● Girls   ■ Boys

Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. Each row omits the state indicated on the left. All regressions include the *Prospera* share, its interaction with an indicator for the 2018–19 school year, and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents.

Figure A6: Binned estimates of enrollment effects by age group and sex
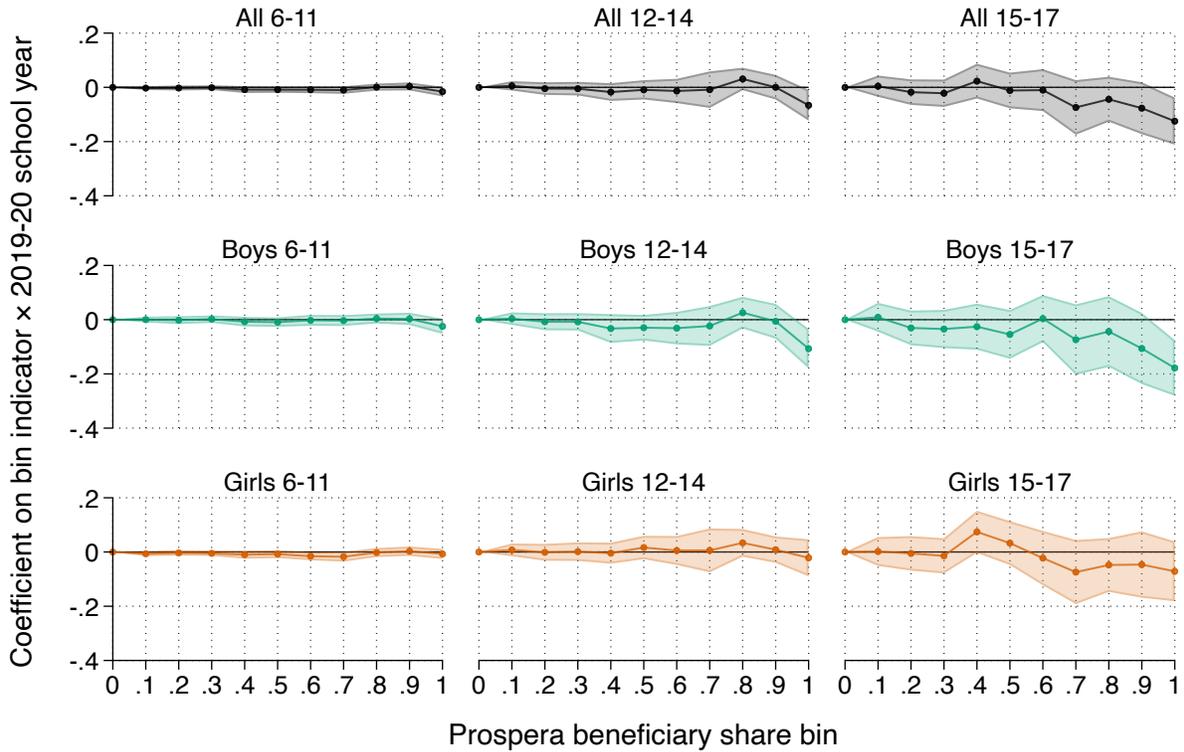
Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. Coefficients on the interaction each bin indicator with an indicator for the 2019-20 school year. All regressions include bin indicators, their interactions with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Bins start as specified by the horizontal axis labels: $[0, 0.1), [0.1, 0.2), \cdots, \geq 1.0$. Sample excludes summers and localities with more than 100,000 residents.

Figure A7: Event studies for school enrollment using benefits per household

Note: Re-estimation of Figure 2 using benefits per household (in thousands of pesos) as the measure of *Prospera* penetration. Point estimates and 95% confidence intervals, based on standard errors clustered by locality. All regressions include the *Prospera* share and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents.

Figure A8: Alternative regression specifications for school enrollment: benefits per household
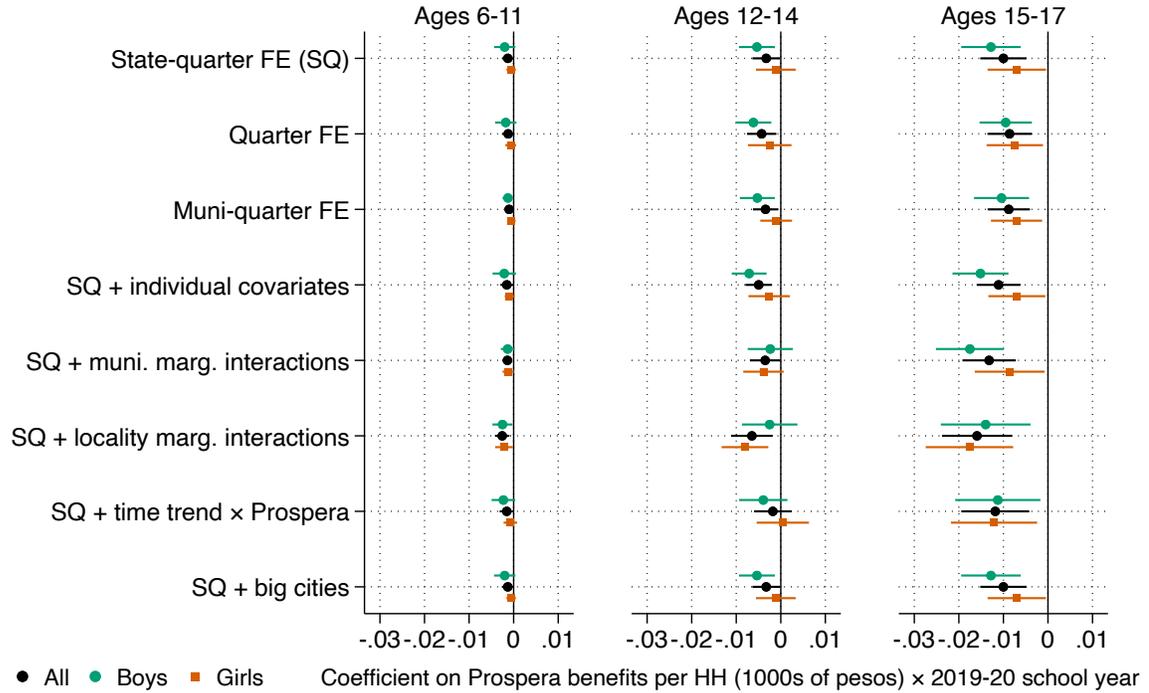

Note: Re-estimation of Figure 3 using benefits per household (in thousands of pesos) as the measure of *Prospera* penetration. Point estimates and 95% confidence intervals, based on standard errors clustered by locality. All regressions include *Prospera* benefits per household, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes summers. Individual covariates include child sex, child age, the household head's education level, an indicator for the mother being present in the household, and the mother's age group, marital status, education level, and literacy if she is present. In the "marginalization interaction" regressions, we interact quarter indicators with indicators for single-percentile bins of the municipality or locality marginalization index. In the "time trend × *Prospera*" regressions, we interact a linear time trend with the *Prospera* share. In the "big cities" regressions, we estimate the baseline model in an expanded sample that includes cities with populations over 100,000.

Figure A9: Coresidence with a parent or guardian by age and sex
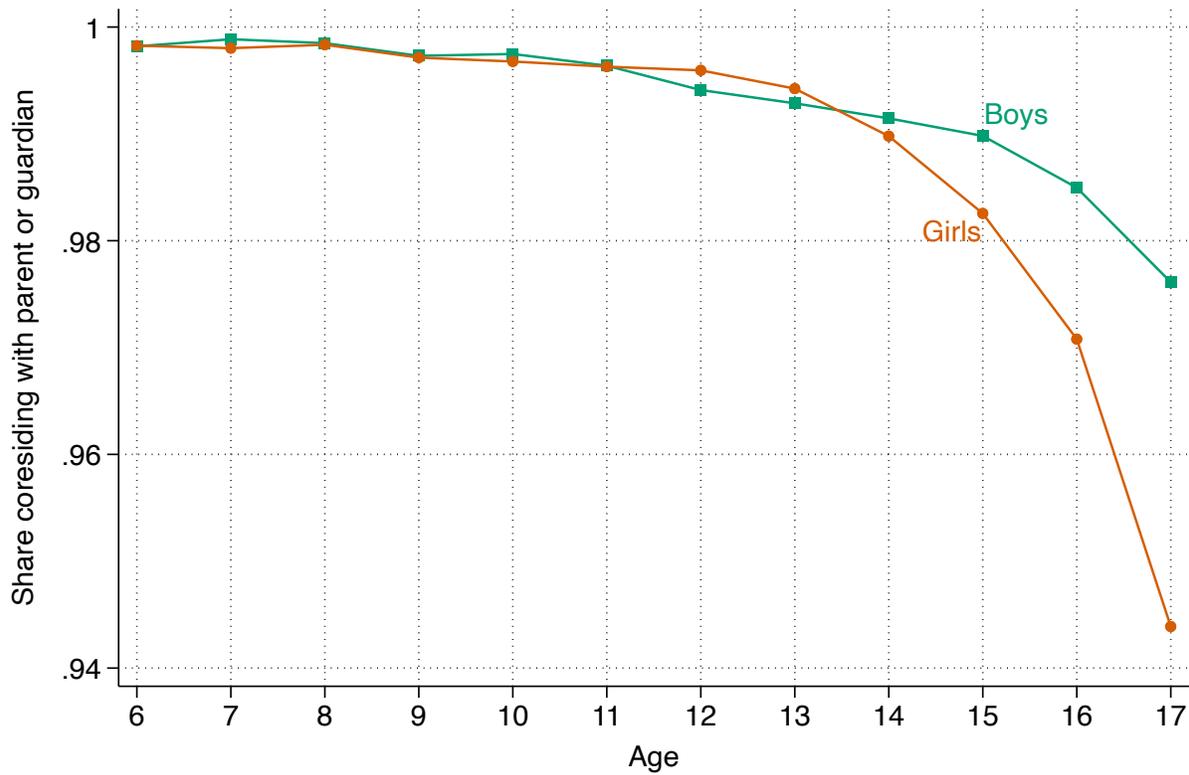
Note: A child is coded as coresident with a parent or guardian if the relationship to the household head is child, child-in-law, adopted child, grandchild, great-grandchild, great-great-grandchild, nephew, niece, godchild, non-legally adopted child, child of domestic worker, or child of guest or border. Sample excludes summers and localities with more than 100,000 residents.

Figure A10: Attrition by rotating panel wave
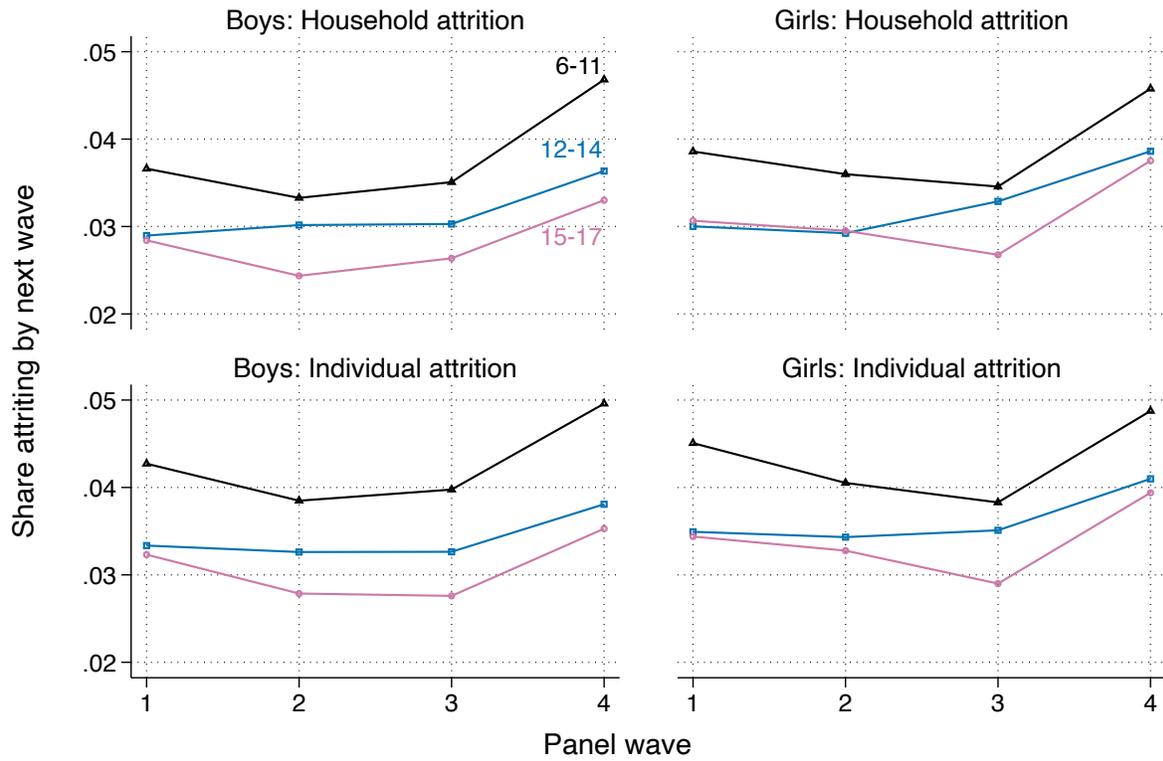
Note: Share of children attriting from the 5-wave rotating panel. Household attrition refers to losing all members to follow-up; individual attrition refers to losing the individual to follow-up. Sample excludes observations starting in the summer and localities with more than 100,000 residents.

Figure A11: School and work patterns by age, ENOE data
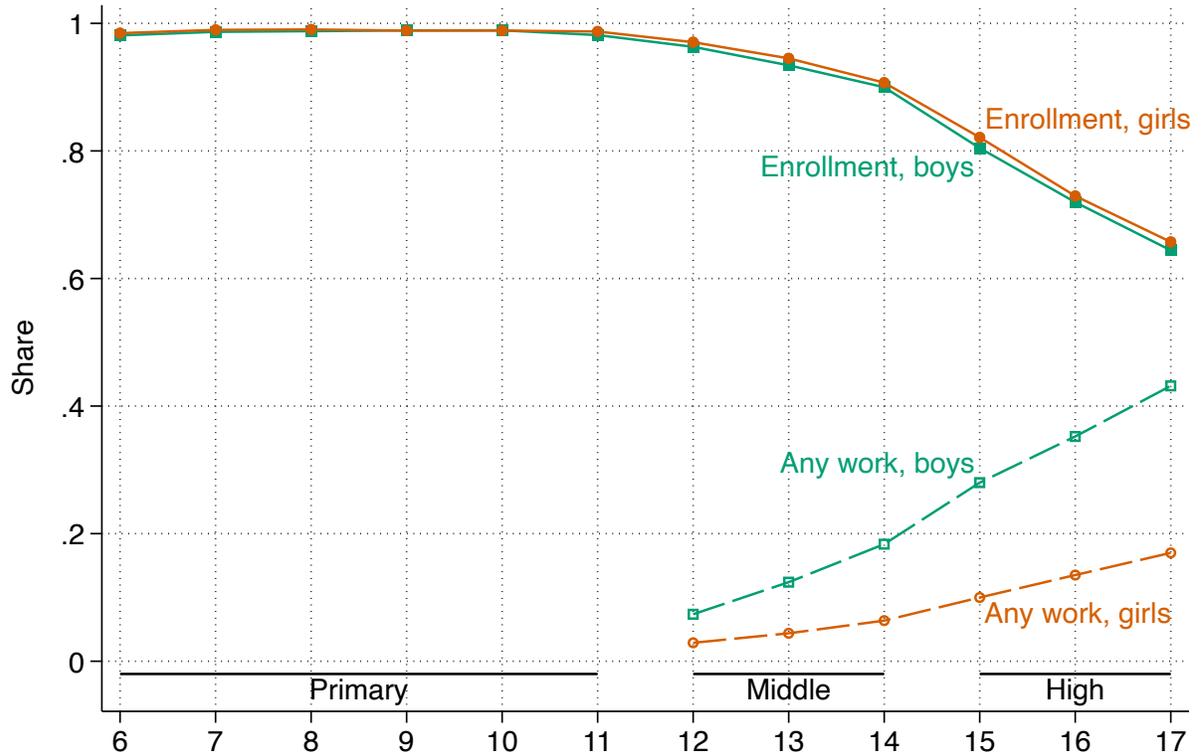
Note: Data are from the Encuesta Nacional de Ocupacion y Empleo 2013-2020. The age ranges for primary, middle, and high school follow a typical student's grade progression in the Mexican system.

Figure A12: Age profiles of men's hourly earnings by highest completed schooling level
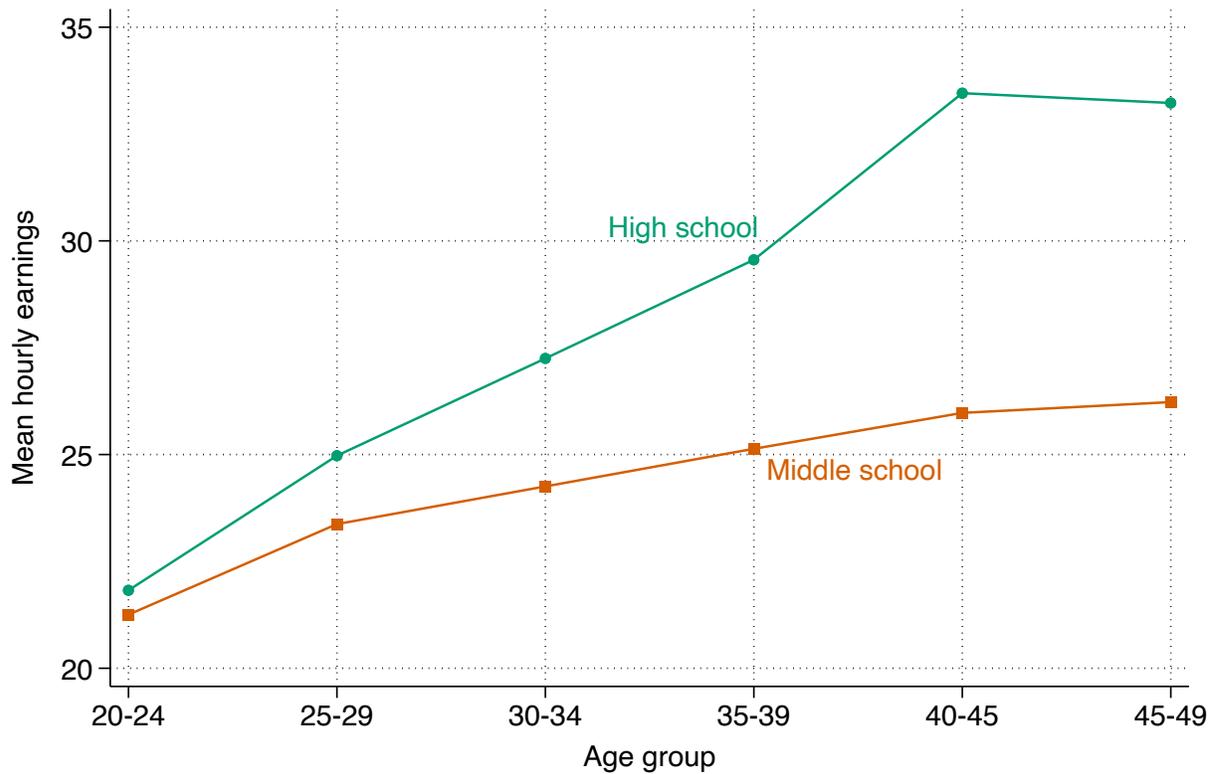
Note: Mean pesos per hour among male workers who completed middle school but not high school or who left school after completing high school. Sample includes men who work at least 30 hours per week and have nonzero labor earnings. Sample excludes summers and localities with more than 100,000 residents.

Table A1: Monthly grants for *Prospera* (2017) and Becas Benito Juárez (2019)

Prospera		Becas Benito Juárez	
Per child transfer to HH	$350 (grade 6)	Flat transfer to HH	$800 (grades 3-9)
	$570 (grade 9)	Transfer to youth	$800 (grades 10-12)
	$980 (grade 12)		
Nutrition grant to HH	$335		

Note: Amounts in pesos. *Prospera* amounts are for boys in selected grades; grants for girls were roughly 15% larger in middle and high school.

Table A2: Descriptive statistics on the 2017 *Prospera* beneficiary share

	Localities	Mean	Std. Dev.	25th %-ile	75th %-ile
Include large cities	52,736	0.62	0.39	0.30	0.89
Include large cities, weight by pop.	52,736	0.22	0.29	0.04	0.28
Exclude large cities	52,605	0.62	0.39	0.30	0.89
Exclude large cities, weight by pop.	52,605	0.38	0.34	0.12	0.56

Note: Sample consists of CONAPO localities with more than 100 residents in the 2010 census that could be uniquely matched with *Prospera* data. Large cities are defined as having more than 100,000 residents in the 2010 census. The ENOE is designed to be representative with and without large cities. The *Prospera* beneficiary share equals the number of beneficiary households at the start of 2017 divided by the number of households in the 2010 census.

Table A3: Effect of rollback on attrition and family coresidence, ages 15-17, by gender

| | Omitting final panel wave | | Full sample |
	Household attrition	Individual attrition	Family coresidence
	(1)	(2)	(3)
A. Boys			
Prospera share	0.004	0.007	-0.004
× 2019-20	[0.010]	[0.010]	[0.008]
Dependent variable mean	0.028	0.031	0.984
N	68,814	68,814	89,275
B. Girls			
Prospera share	-0.009	-0.008	-0.004
× 2019-20	[0.011]	[0.011]	[0.013]
Dependent variable mean	0.031	0.034	0.966
N	66,129	66,129	85,723

Note: Brackets contain standard errors clustered by locality. Household attrition refers to losing all members to follow-up in the next panel round; individual attrition refers to losing the individual. Family coresidence refers to living with a parent or guardian. A child is coded as coresident with a parent or guardian if the relationship to the household head is child, child-in-law, adopted child, grandchild, great-grandchild, great-great-grandchild, nephew, niece, godchild, non-legally adopted child, child of domestic worker, or child of guest or border. Sample sizes are smaller in columns (1) and (2) because attrition is not defined for a household's final round in the rotating panel, nor in winter 2020. All regressions include the *Prospera* share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes observations starting in the summer and localities with more than 100,000 residents. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A4: Effect of rollback on non-labor time use, ages 15-17, by gender

	Study	Chores	HH admin.	Care
	(1)	(2)	(3)	(4)
A. Boys				
Prospera share	-2.725*	0.049	0.058	-0.043
× 2019-20	[1.398]	[0.283]	[0.087]	[0.111]
Dependent variable mean	23.56	4.41	0.53	0.42
N	88,871	88,871	88,871	88,871
B. Girls				
Prospera share	-1.070	0.145	0.208	-0.038
× 2019-20	[1.494]	[0.588]	[0.140]	[0.434]
Dependent variable mean	24.63	11.29	0.86	2.41
N	85,251	85,251	85,251	85,251

Note: Dependent variables are measured in hours per week. Only time use categories averaging more than 30 minutes per week are included. Brackets contain standard errors clustered by locality. All regressions include the *Prospera* share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A5: Effect of rollback on school-work combinations, boys ages 15-17

	School, no work	School, work	No school, work	No school, no work
	(1)	(2)	(3)	(4)
Prospera share	-0.067**	-0.056**	0.135***	-0.011
× 2019-20	[0.034]	[0.022]	[0.032]	[0.018]
Dep. var. mean	0.57	0.15	0.20	0.07
N	89,272	89,272	89,272	89,272

Note: Dependent variables equal 1 if the condition in the column title is met, 0 otherwise. Brackets contain standard errors clustered by locality. All regressions include the *Prospera* share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$