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Rolling Back *Progresa*: How the Sudden Ending of a Landmark Anti-Poverty Program Affected School and Labor*

Fernanda Marquez-Padilla† Susan W. Parker‡ Tom S. Vogl§

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Abstract

Mexico's pioneering conditional cash transfer program—originally *Progresa*, later renamed *Prospera*—operated over two decades in a shifting educational 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 was still effective at raising enrollment and reducing work in children and youth. Comparing areas with high and low program penetration before and after rollback, we find that rollback immediately reduced school enrollment, especially at high school ages and especially in boys. Effects on enrollment were as large at rollback as 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 successfully adapted to the rise of high school, but Mexico's poor were unable to protect their children from the its unexpected rollback.

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†El Colegio de Mexico, Centro de Estudios Económicos. E-mail: fmarquez@colmex.mx.

‡University of Maryland; School of Public Policy. E-mail: swparker@umd.edu.

§University of California San Diego, Department of Economics. E-mail: tvogl@ucsd.edu.

1 Introduction

Conditional cash transfer (CCT) programs, which link monetary transfers to 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 continued raising school enrollment and reducing school-year employment two decades later, despite extensive changes in the educational landscape since rollout.

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 focus primarily on whether positive effects of 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 in the 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 so for high school. During times of changing educational strength and weakness, do initially successful long-standing programs like *Progresa* continue to be successful? We shed light on this question by estimating the enrollment and employment effects of the sudden rollback in 2019.

We estimate the effects of rollback on enrollment and work using a difference-in-differences design, comparing outcomes in localities with high and low initial 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. 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 analysis 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

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

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 enrollment among high-school-aged boys declined by 12.3 percentage points in localities with full program penetration, relative to localities with no program penetration—a fall of approximately 17%. Comparing localities at the 75^{th} and 25^{th} percentiles of program penetration, the implied decline is about 8%. High-school-aged boys have higher employment rates than other youth in Mexico, implying a larger tradeoff between school and work. Along these lines, we find that rollback raised employment in this group, with our estimates suggesting that 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 *BBJ* and *Prospera* using administrative data on recipients of both programs. While overall spending is similar pre- and post-rollback, we find that progressivity worsened substantially, in the sense that poorer localities received a far smaller share of *BBJ* spending than *Prospera* spending. Perhaps as a result, our results remain unchanged when we control for early *BBJ* penetration.

Our estimated impacts of the rollback of *Prospera* on the school enrollment of boys are quite substantial and, in fact, larger than the initial rollout effects, albeit at different schooling levels. Schultz (2004) assesses the overall effect on enrollment in secondary school (7th through 9th grade) in the first two years of *Progresa* operation to be increases of 5.2-6.2 percentage points for boys and 7.1-9.2 percentage points for girls. Our effects of *Prospera* rollback suggest few negative effects on enrollment at the secondary levels but significant reductions at the high school level for boys on the order of 12 percentage points. Rollback, however, did not lead to reductions in school enrollment for girls, suggesting the gains in school enrollment of girls were better protected from rollback.

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 the program's 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 improvements in the education and health of the next generation. *Progresa* and other CCTs 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*, finding 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. Evaluation studies using the randomized controlled trial find that the program raised school enrollment, reduced grade repetition, and raised completed grades of schooling. Analyzing data from the 18-month experiment, Schultz (2004) find that the program significantly increased the probability of transitioning to middle (lower secondary) 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 program impacts on enrollment, repetition, dropout, and school re-entry at each age. Consistent with Schultz (2004), they find few effects on enrollment at primary school ages and larger effects on enrollment 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, and negative effects on time spent working. For youth aged 12 to 17—middle and high school ages—they find increases in school attendance of 4-6 percentage points for boys and 8-10 percentage points for girls. Reductions in the probability of working outside the home ranged from 3-5 percentage points for boys and about 2 percentage points for girls.

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-run 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 full grade for children who participate in the program for 6 years beginning at ages 9 to 12, compared to nonparticipating children. In a difference-in-differences design based on cohort exposure to the non-experimental rollout of the program, Parker and Vogl (2023) similarly 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.

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 25^{th}, 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 grant program called *Becas Benito Juárez* (*BBJ*), operated by the Secretary of Public Education (as opposed to the Secretary of Social Development as with *Prospera*). The government's 2019 budget also stated that *Prospera*'s resources would be reassigned to the new substitute program.[2]

Comparing the benefits and coverage of *BBJ* and *Prospera*, both programs provide transfers conditional on school enrollment, but the *BBJ* program loosened conditionality and does not monitor 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 for 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 1 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

[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 these components.

[3] *Prospera* monitored the enrollment and attendance of each child, and an 85% attendance record was required to receive the monthly grant.

[4] A smaller, third component, *Jovenes Escribiendo el Futuro*, provides transfers to students linked to enrollment in college.

Table 1: Monthly grants for *Prospera* (2017) and *BBJ* (2019)

Prospera		BBJ	
Per child transfer to HH	$350 (grade 6) $660 (grade 9)	Flat transfer to HH	$800 (grades 3-9)
Per youth transfer to HH	$1120 (grade 12)	Transfer to youth	$800 (grades 10-12)
Nutrition grant to HH	$335		

Notes: *Prospera* monthly amounts for selected grades shown for girls. Children include those enrolled in grades 3–9; youths include those enrolled in grades 10–12.

resource-sharing between teenagers and their parents.

These somewhat nuanced differences in program benefits and rules were arguably swamped by disruption and changes in program reach. A number of newspapers report 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 there is little written documentation of the operational process through which *Prospera* beneficiaries were transitioned to the *BBJ* program (Jaramillo-Molino, 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 compared with the previous *Prospera* program by locality, 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. To illustrate this point, Figure 1 plots transfers per household under *Prospera* and under *BBJ* by the 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. Furthermore, 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, household received on average more than double what they had received pre-rollback, suggesting less coverage of households in these poor areas under *BBJ* grants than *Prospera*, at least in the first year of operation of *BBJ*. [5] These shifts are consistent with a constant budget because most Mexican households are located in the least marginalized localities (which include major cities), as shown in the population distribution at the bottom of the figure.

In summary, while the *Prospera* program pre-rollback showed a high degree of progressivity, with transfers per household increasing with locality marginalization, this progressivity is largely lost under the new substitute *BBJ* program, at least during its first year. The net result is that the *BBJ* substitute program provides much lower resources per household, particularly in the poorest communities. We thus hypothesize significant disruption of rollback for *Prospera* households. [6]

3 Data and Methods

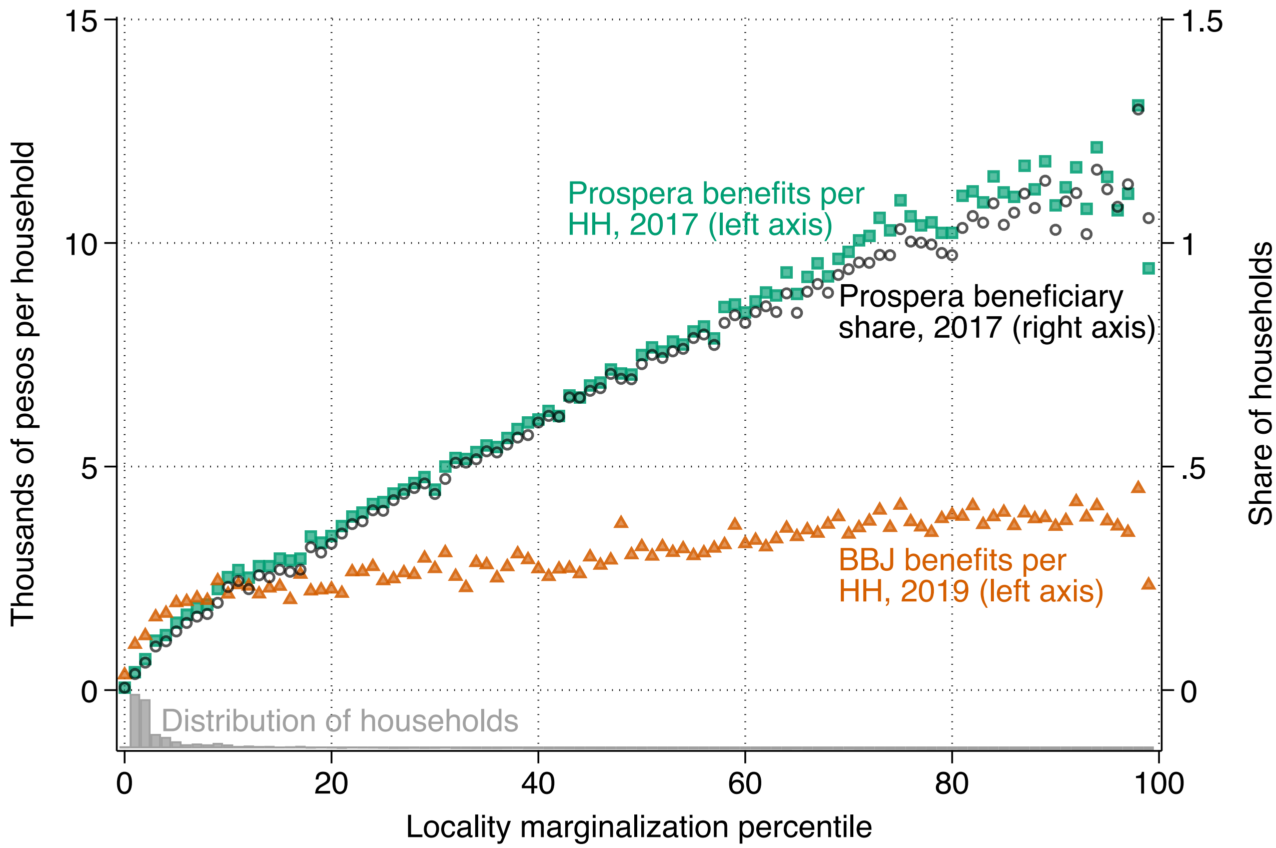
3.1 Data

Our main outcome is school enrollment, which we study for children of primary school ages (6-11), middle school ages (12-14), and high school ages (15-17). For youth in the last age range, we also study employment and hours worked. We measure these outcomes in the National Survey of Occupation and Employment (ENOE), a large quarterly labor market survey carried out since 2005 by INEGI, the Mexican statistical agency. The ENOE is Mexico's equivalent to the US

[5] The operating rules of *BBJ* Basica describe the application process and eligibility criteria for household to receive the *BBJ* Basica grant. For the *BBJ* transfers to high school students, eligible schools provided list of their enrolled students to *Prospera* and served as intermediaries for verifying identifications and other application requirements.

[6] Qualitative evidence also suggests that receipt of the first transfers for *BBJ* Basica beneficiary households took, in the majority of cases, three to six months. Coneval (2024)

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*.

Current Population Survey. It interviews approximately 127,000 households every quarter, and is representative at the national and state level as well as at the urban, semi-urban, and rural levels.[7] We use cross-sectional ENOE data rounds between 2014 and 2020.

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 an 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 data stopped providing locality identifiers for respondents, 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 (fixed in 2017), using administrative data on *Prospera* enrollment. Geographical identifiers both at the locality level are provided in the ENOE, allowing us to merge local program penetration ratios of households to enrollment data from ENOE (Parker and Vogl, 2023). *Prospera* penetration is defined as the proportion of households enrolled in *Prospera* in 2017 divided by the number of total households in the locality in 2010. We use 2017 as it is the last "stable" pre-rollback year, before the election of 2018. Mexican law prohibits the government from distributing of public benefits during elections.

We include in our estimation sample all localities with fewer than 100,000 inhabitants. We exclude larger localities because a relatively low proportion of households in these areas were beneficiaries of *Prospera* at the time of rollback. Figure A3 shows that only about 5% of households living in localities above 100,000 were beneficiaries. The ENOE is designed to be representative of localities both above and below 100,000 inhabitants. We verify that our main results are robust to

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

removing this sample restriction.

3.2 Design and Estimation

The rollback of *Prospera* began during the first two months of 2019, after Lopez Obrador took office in December, 2018. We hypothesize impacts on school enrollment mainly at the beginning of the following school year, which started in the late summer of 2019. We thus study impacts about 9 months post rollback, prior to the onset of COVID-19 in March 2020. Because 2018 was an election year and there could potentially be anticipation effects on school enrollment related to expectations on *Prospera*'s future, we allow for rollback effects to begin in school year 2018-2019. Our empirical strategy analyzes potential effects of rollback by quarter, allowing us to trace the entire pattern of enrollment responses before and after rollback, including during the academic school year. [8] Our empirical strategy, described below, compares changes in school enrollment pre- and post-rollback in localities with a higher versus lower level of program penetration.

Our main estimation equation is a variant of a standard continuous difference-in-differences specification:

$$Enrolled_{ilst} = \alpha Prospera_{ls} + \gamma Prospera_{ls}\mathbb{1}_{2018/19} + \beta Prospera_{ls}\mathbb{1}_{2019/20} + \tau_{st} + \epsilon_{ilst} \tag{1}$$

for individual i from locality l in state s at academic year quarter t. 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.

[8] Appendix Figure A4 shows that the largest share of children leave school between the end of one academic school year and the beginning of the next as opposed to dropping out during the academic school year.

We interact with a 2019 indicator (our post-rollback variable) to identify the effect of rollback. The coefficient on the interaction term, β, captures the the effect of rolling back *Prospera* in a fully-saturated locality relative to a locality with no *Prospera* households. We also interact $Prospera_{ls}$ with a 2018 indicator, which allows for rollback impacts to begin in school year 2018-2019, given potential anticipatory effects on school enrollment related to the election in 2018 as well as rollback taking place during the latter part of the 2018-2019 school year.

To complete the difference-in-differences design, we also include quarter fixed effects. Our preferred specification allows the quarter fixed effects to vary by state, τ_{st}, so that we only compare changes in school enrollment between localities in the same state.[9] This specification 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 estimate 2-by-2 difference-in-differences comparing localities with complete *Prospera* penetration and no *Prospera* penetration, finding effects at least

[9] Our robustness tests include results which allow quarter fixed effects to vary by municipality. Municipalities are the next administrative unit above localities in Mexico, akin to counties in the United States. These results are in fact similar, but we do not use this specification as our preferred one, because nearly half of municipalities in our survey sample have only one locality and thus drop out of the estimation.

as large as the coefficients estimated using equation 1.

We also estimate an event study specification:

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

Here we modify the main specification by interacting the cross-sectional exposure variable with indicators for every quarter but the second quarter of 2018, the quarter leading up to the presidential election. The parallel trend assumption implies β_q to be zero for all quarters years prior to the third quarter of 2018.

For both the main and event study specifications, we use pre-rollback ENOE data from 2014 onwards, leading to a six-year pre-rollback window. This window corresponds to a period of stability in *Prospera* enrollment, and is long enough to allow us to assess differential pre-rollback trends. Standard errors are clustered at the locality level.

4 Results

We begin by estimating impacts of rollback on school enrollment at primary, middle, and high school ages. We then turn to labor supply for relevant ages.

4.1 Enrollment effects

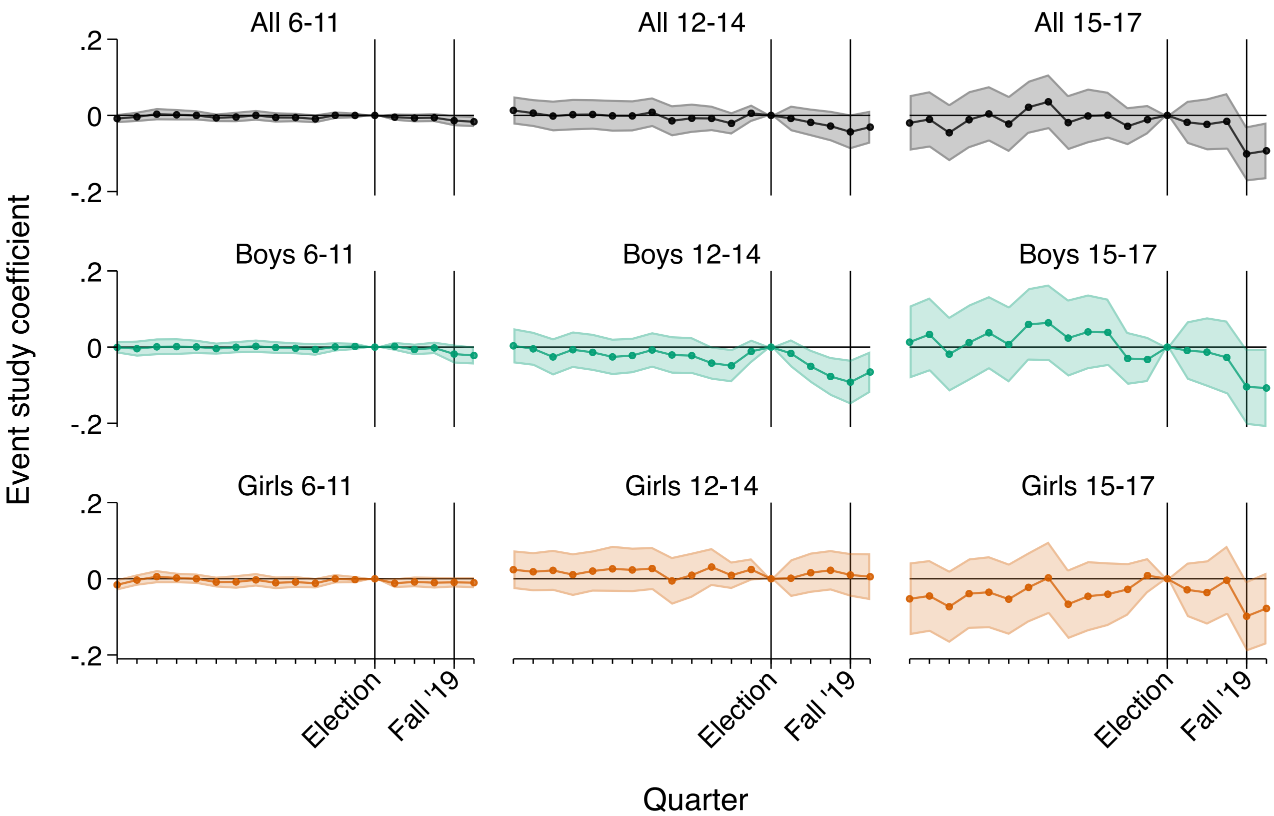
We present impacts by age group, age 6-11, 12-14 and 15-17, which largely correspond to primary (grades 1 to 6), middle (grades 7-9), and high school (grades 10 to 12 or high school) enrollment ages and by gender. We begin with event study graphs for school enrollment (Figure 2). A vertical line marks the election quarter and a second vertical line marks the fall quarter of 2019, e.g. the beginning of school year 2019-2020. The event studies for all age groups with boys and girls combined (first row of graphs) are consistent with no evidence of pre-rollback trends in enrollment over the pre-rollback period. Immediately after the election, all three event studies show a negative trend but continue to be insignificant prior to the fall of 2019. For ages 6-11 and 12-14 the event study coefficients trend more negatively in 2019-2020 but remain insignificant. At the beginning of school year 2019-2020, however, there is a striking drop in the event study coefficient for the 15-17 year olds, implying a sharp and significant fall in enrollment due to rollback.

Figure 2 also presents the set of event studies separately for boys and girls. Here, there are striking differences by gender. The event studies for girls ages 6-11 and 12-14 show no evidence of pre-trends pre-rollback or evidence that there is a significant impact of the rollback on school enrollment post election (and in the fall of 2019). The event study for girls 15-17 suggests a reduction in enrollment in the fall of 2019, but the pre-rollback coefficients also are consistently negative.

For boys, however, the story looks quite different. For all three age groups, there is no evidence of pre-trends prior to the election. However for all three groups the event study coefficients become clearly negative and significant by the fall of 2019. Further, the negative effects suggested by the event studies on enrollment of both boys ages 12-14 and 15-17 appear substantial, on the order of 5 percentage points for ages 12-14 and 10 percentage points for ages 15-17 by the end of 2019. For boys ages 6-11, an age group with enrollment rates of 99% pre program, the event studies suggest very small decreases with rollback in the probability of being enrolled.

We now turn to regression results on the impact of rollback on school enrollment, presented in Table 2, which provides estimates of the impact of rollback by age group and by gender. Beginning with the combined group of boys and girls, post rollback, β, the coefficient interaction between program intensity and the 2019-2020 school year is negative and significant for all three age groups. For ages 6-11 and ages 12-14 β is significant at the 5% level and significant at the 1% level for ages 15-17.

Figure 2: Enrollment event study 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 2: Enrollment effects by age group

	Ages 6-11 (1)	Ages 12-14 (2)	Ages 15-17 (3)
A. All			
Prospera share	-0.005*	-0.062***	-0.233***
	[0.002]	[0.007]	[0.014]
Prospera share × 2018-19 school year	-0.003	-0.017	-0.011
	[0.005]	[0.014]	[0.026]
Prospera share × 2019-20 school year	-0.013**	-0.036**	-0.089***
	[0.006]	[0.018]	[0.028]
Dep. var. 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 school year	-0.001	-0.030*	-0.033
	[0.006]	[0.018]	[0.031]
Prospera share × 2019-20 school year	-0.019*	-0.061***	-0.123***
	[0.011]	[0.023]	[0.036]
Dep. var. 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 school year	-0.006	-0.004	0.012
	[0.006]	[0.019]	[0.033]
Prospera share × 2019-20 school year	-0.005	-0.009	-0.054
	[0.006]	[0.023]	[0.036]
Dep. var. 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. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

The negative effects of rollback are particularly large for the age group 15-17, corresponding to a reduction in the probability of enrollment for 15-17 year olds of 8.9 percentage points. The average level of school enrollment pre-rollback for ages 15-17 was 73%, implying that rollback, for a locality going from full *Prospera* penetration to total program rollback, (and net of the substitute program *BBJ*) would lead to a reduction of about 12% in the probability of enrolling in school for 15-17 year olds. For ages 12-14, the negative impact on enrollment is 3.6 percentage points (or a 3.8% decrease relative to a 93.6% base enrollment rate) and for ages 6-11, the negative effect is 1.4 percentage points, a decrease of 1.4% relative to a base of 98.7% enrollment rate. The coefficients on γ are generally negative but statistically insignificant, implying no overall anticipatory effects on enrollment in the 2018-2019 school year.

By gender, consistent with the event studies of Figure2, Table 2 demonstrates that the large negative effects of rollback on school enrollment are concentrated on boys. For boys, β is negative and significant for all three age groups (at the 1% level for age groups 12-14 and 15-17 and 10% level for 6-11 year olds). The effect of rollback on school enrollment for boys age 15-17 is substantial, implying a reduction in 12.3 percentage points relative to a base enrollment of 72.5. This implies that for a locality with full *Prospera* penetration, the rollback would lead to a reduction in the probability of boys ages 15-17 of attending school of 17.5%. For boys ages 12 to 14, rollback implies a negative impact of 6.1 percentage points on school enrollment, from a base of 93.2 percent. And for boys ages 6 to 11, the size of reduction is 1.9 percentage points from a base of 98.6 percent, although this coefficient is only marginally statistically significant.

For girls, while all coefficients are negative, there are no statistically significant effects of rollback for any age group. The closest to statistical significance is for girls ages 15-17 with a negative coefficient of 5.4 percentage points, close to being statistically significant at the 10 percent level. The overall negative effects of rollback thus appear to be largest for boys and at ages corresponding to high school enrollment.

4.2 Robustness

The previous section suggested large impacts of the rollback of *Prospera* on school enrollment, principally concentrated on boys. This subsection examines the sensitivity of our main specification results. Figure 3 provides point estimates of β for boys and girls separately [10] for our three different age groups for a number of different specifications to address potential threats to the identification strategy. In particular, we first address the challenge of differential enrollment trends across areas of Mexico by testing specifications which include un-interacted time fixed effects and municipality-time fixed effects. Second, we address potential differential enrollment trends across rich and poor localities by including interactions of marginalization percentile dummies with time dummies. Finally, we include specifications with a time trend, and expand the sample to include large cities. Our main specification (State-quarter FE) are shown in Figure 3 as the first specification for comparative purposes.

Beginning with the sensitivity of the results for the 15-17 year olds, Figure 6 demonstrates that for boys, the numerous different specification checks do not appreciably change the point estimate or significance level. Every one of the alternative specifications implies a negative and significant (at the 5% level or greater) 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 provide a bit more nuanced picture. While the majority of our alternative specifications suggest largely statistically insignificant effects, it is noteworthy that the estimated coefficients in every specification are negative and several are statistically significant and nearly of the same size of boys. The evidence is thus potentially suggestive of some effects for girls in this

[10] Appendix Figure **??** presents a similar graph for boys and girls together.

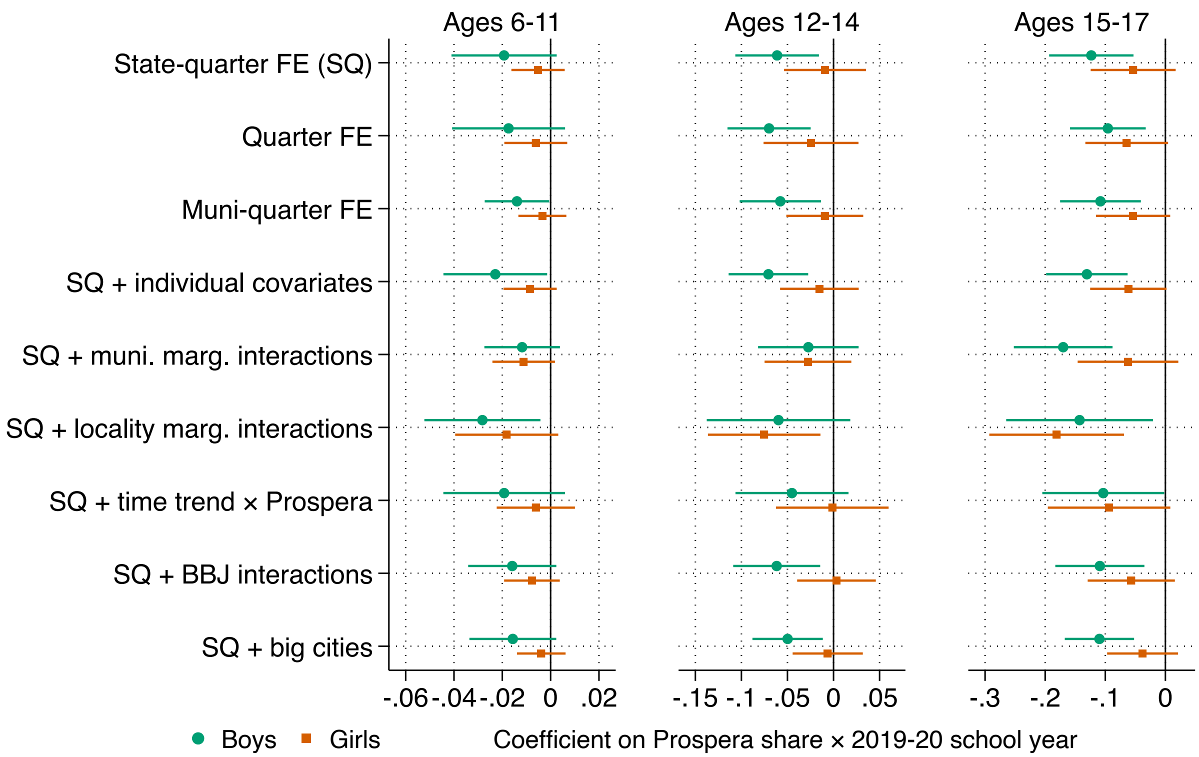
age group, but these results are sensitive to the specification and much less robust than the results for boys ages 15-17.

Our main specification results also suggested negative and statistically significant effects of rollback on enrollment for boys ages 12 to 14 and ages 6 to 11. Our specification checks, however, for both of these age groups, suggest that the results vary somewhat with several of our alternative specifications. In particular, specifications controlling for time trends, and for locality and/or municipal level marginalization interactions suggest insignificant impacts of rollback on enrollment. For boys ages 6 to 11, five out of eight alternative specifications show insignificant effects and for boys ages 12 to 14, three out of eight show insignificant effects. Consequently, we consider the enrollment impacts for boys in these age groups are insufficiently robust to our specification checks. Specification checks for enrollment effects for girls ages 12 to 14 and girls ages 6 to 11 confirm our main specification results, which showed no significant impact of rollback on the enrollment of either group.

Given potential state level differences in trends in school enrollment, Figure A5 in the Appendix repeats our main specification results by age group and gender, omitting individual states. For all three age groups and by gender, the results remain remarkably consistent in this exercise for the 32 states. The only exception are the results for 6-11 year old boys which exclude the state of Chiapas and suggest no impact of rollback on school enrollment of boys in this age group.

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. As an easier-to-understand alternative, consider 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 $Prospera_{ls}$ 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

Figure 3: Robustness of enrollment effects by age group and sex



Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. All regressions include the 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, mother's age group, mother's marital status, mother's education level, mother's literacy, and an indicator for the mother being present in the household. 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.

or equal to 1. Values greater than 1 are due to population growth between the census in 2010 and *Prospera* measurement in 2017. 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 percent 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.

4.3 Heterogeneity

We now turn to a heterogeneity analysis of rollback's effects on enrollment, focusing on the group of 15-17 year olds, the group for whom we found large and robust negative effects of rollback on school enrollment and presenting results by gender. Table 3 presents enrollment impacts by mother's education level, locality population, and locality marginalization. For boys (Panel A), impacts of rollback are concentrated for youth whose mother's have lower levels of education, consistent with marginal high school students being more likely to live in lower SES households. The impacts for boys whose mothers have a primary education or less (66.5 percent of the sample) shows a 18.6 percentage point reduction in school enrollment due to rollback (a large decline of almost 30% with respect to pre-rollback enrollment of 62.1%). Impacts for youth whose mothers have higher levels of schooling while negative, are smaller and statistically insignificant. Table 3 also shows that impacts of rollback for boys were relatively similar in rural communities (less than 2,500 inhabitants) versus non-rural communities, with a reduction in the probability of enrolling in school of 14.9 percentage points in rural areas versus 18.5 percentage points in non-rural areas. In percentage terms, these reductions look even more similar as they correspond to a 22.2% fall in enrollment for rural localities and 23.9% for non-rural. (Recall our sample excludes localities with more than 100,000 inhabitants so that non-rural are communities with 2,500-100,000 residents.) Finally, impacts in high and very high marginalized areas are larger and more precise than for localities with very low to medium levels of marginalization. Communities over the median level of marginalization show a reduction in 15.4 percentage points in the probability of youth enrolling in school after rollback. From the baseline of 66.2 percent enrolled in school, complete rollback of *Prospera* in the poorest areas of Mexico suggests a reduction in the school enrollment of 15-17 year olds of 23%.

Heterogeneity results for girls ages 15 to 17 are shown in Panel B of Table 3. By mother's level of education, there are no significant impacts of rollback for either category. However, disaggregating by size of locality suggests that for girls ages 15-17 living in rural localities (less than 2,500) inhabitants, there is a large and significant effect of rollback of 9.7 percentage points. Further, for the set of localities with high or very high level of marginalization, for girls there is also a large and significant effect of rollback of 11.7 percentage points. While the overall results for girls ages 15-17 generally showed insignificant effects of rollback on enrollment (with some sensitivity to the specification), these heterogeneity results are suggestive of significant negative effects of rollback for some subgroups—arguably in precisely the localities where high school enrollment may be more sensitive to program loss. The negative and significant results in rural areas and in highly marginalized localities furthermore suggest that to the extent there are some negative effects of rollback for girls, they may be concentrated in the subset of poorer rural beneficiaries.

Appendix Figure A7 presents event studies by marginalization category. For high marginalization localities, the event studies do not suggest significant pre-trends prior to rollback for boys or girls. Post rollback, the event studies are, however, extremely similar for boys and girls, with both event studies suggesting negative and significant effects of rollback beginning in the fall of 2019 school year. For boys, the effects are larger and more precisely estimated.

Appendix Figure A7 also presents event studies for the set of low marginalization localities for girls and boys. For boys, the event study generally shows no pre-trends pre-rollback and a dip in enrollment post rollback in the fall of 2019. However, the period just prior to the election and post election is somewhat noisier than that for high marginalization localities. The event study for girls in low marginalization localities suggests some evidence of a negatively sloped pre-trend and is not suggestive of rollback effects.

Overall, the heterogeneity results suggests large negative effects of rollback for boys ages 15-17, impacts which are larger and more precise for those with lower levels of maternal education and those living in high marginalization localities. For girls, whereas the overall effects of rollback on school enrollment were generally insignificant, our heterogeneity analysis suggests statistically significant and negative effects of rollback on some subgroups, including girls ages 15-17 in rural and highly marginalized communities. Overall, however, our evidence supports larger and more general negative effects of rollback on school enrollment for boys.

4.4 Labor market effects

School and work may be substitutes, (Ravallion and Wodon (2001)) and early studies of *Prospera*'s initial effects suggested significant reductions in labor market participation of the program, mainly concentrated on boys (Skoufias and Parker (2001)). Figure A8 in the Appendix presents school and work participation in the ENOE for ages 6 to 17, throughout the sample period, demonstrating very high school enrollment rates e.g. above 95% for boys and girls until about age 12 when enrollment

Table 3: Enrollment effect heterogeneity, ages 15-17

	Mother education level		Locality pop.		Locality marg.	
	$\leq$ primary	$>$ primary	$< 2,500$	$\geq 2,500$	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
A. Boys						
Prospera share ×	-0.186***	-0.069	-0.149***	-0.184***	-0.154***	-0.136
19-20 school year	[0.054]	[0.045]	[0.050]	[0.067]	[0.054]	[0.094]
Dep. var. mean	0.621	0.837	0.670	0.768	0.662	0.775
N	33,257	46,526	33,553	55,722	32,283	56,992
B. Girls						
Prospera share ×	-0.030	-0.065	-0.097**	-0.091	-0.117**	-0.120
19-20 school year	[0.056]	[0.046]	[0.049]	[0.074]	[0.056]	[0.096]
Dep. var. mean	0.665	0.893	0.676	0.785	0.654	0.805
N	29,643	42,992	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. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 4: Labor market effects, ages 15-17

	Boys		Girls	
	Any work (1)	Hours (2)	Any work (3)	Hours (4)
Prospera share × 2019-20 school year	0.061**	3.684***	-0.013	-0.503
	[0.026]	[1.088]	[0.018]	[0.715]
Dep. var. mean	0.273	9.125	0.117	3.476
N	204,943	204,943	197,620	197,620

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. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

declines continuously reaching about 65% for both by age 17. Boys have higher labor market participation at all ages than girls. About 10% of boys participate in the labor market at age 12, rising to over 40% by age 17. For girls, labor market participation rates are about 3% for age 12 and rise to nearly 20% by age 17.[11] Table 4 presents impacts of rollback by gender and shows rollback increases the probability of working for boys age 15-17 by 6.1 percentage points, a 22.3% increase compared with a baseline mean of 27.3 and an increase in unconditional hours worked of 3.7 hours per week, an increase of about 40% compared with a baseline mean of 9.1 hours per week. For girls, there are no statistically significant effects of rollback on labor market participation or hours worked. Appendix Figure A9 presents event studies which are consistent with impacts of boys on work and hours.

4.5 Accounting for replacement programs

Our estimation results have demonstrated strong and negative effects on rollback on school enrollment. In this section, we further explore if the *BBJ* program mitigated the effects of rollback on

[11]The ENOE includes agricultural and unpaid work outside the home as participation in the labor market. The ENOE labor market questions are applied only to children age 12 and over). Domestic work is not included in this definition.

Table 5: Comparing *Prospera* rollback effects with *BBJ* rollout effects, ages 15-17

	(1)	(2)	(3)	(4)
A. All				
Prospera share	-0.089***	-0.084***		
× 2019-20 school year	[0.028]	[0.029]		
Prospera benefits per HH			-0.010***	-0.009***
× 2019-20 school year			[0.003]	[0.003]
BBJ benefits per HH		-0.006		-0.005
× 2019-20 school year		[0.004]		[0.004]
Δ benefits per HH				
× 2019-20 school year				
N	174,998	174,998	174,998	174,998
B. Boys				
Prospera share	-0.123***	-0.109***		
× 2019-20 school year	[0.036]	[0.038]		
Prospera benefits per HH			-0.013***	-0.011***
× 2019-20 school year			[0.003]	[0.004]
BBJ benefits per HH		-0.009*		-0.008
× 2019-20 school year		[0.005]		[0.005]
Δ benefits per HH				
× 2019-20 school year				
N	89,275	89,275	89,275	89,275
C. Girls				
Prospera share	-0.054	-0.057		
× 2019-20 school year	[0.036]	[0.037]		
Prospera benefits per HH			-0.007**	-0.007**
× 2019-20 school year			[0.003]	[0.003]
BBJ benefits per HH		-0.003		-0.003
× 2019-20 school year		[0.005]		[0.005]
Δ benefits per HH				
× 2019-20 school year				
N	85,723	85,723	85,723	85,723

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. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

school enrollment. As documented in Section 2, the amount of resources spent on the substitute *BBJ* program approached the total amount previously spent on *Prospera*, but with substantial changes in the progressivity of spending. With the exception of households in the least poor localities, average transfer received per households were substantially less under *BBJ* than *Prospera*, with households in the highest poverty localities receiving less than half the total amount of transfers under *Prospera*. Still, the *BBJ* transfers, even in the poorest localities, might have mitigated the rollback effects of *Prospera* on school enrollment.

We explore this hypothesis in Table 5. Column 1 repeats our principal rollback results on school enrollment for all youth ages 15-17 and by gender, shown in Table 2. In Column 2, we augment our main specification to include an additional program interaction of *BBJ* benefits per household interacted with our post 2019-2020 dummy variable, to control for differential changes in school enrollment between localities with high and low *BBJ* penetration. Doing so reduces slightly the size of the rollback β coefficient, from 8.9 percentage points to 8.4 percentage points for all youth ages 15-17 and from 12.3 percentage points to 10.9 for boys ages 15-17. Strikingly, the *BBJ* interaction does not have a positive coefficient, perhaps consistent with *BBJ* spending having little overall effect on school enrollment. Columns 3 and 4 repeat columns (1) and (2) but replaces the *Prospera* share with *Prospera* benefits per household in order to more directly compare impacts of spending on *Prospera* with impacts of spending on *BBJ* [12]. Columns 3 and 4 again suggest that rollback significantly reduces school enrollment. However, the *BBJ* program interaction is not positive or significant in any of the three groups, including all youth 15-17, as well as boys and girls 15-17.

In summary, our analysis suggests that the *BBJ* program, in spite of the similar amount of total

[12]Because the *BBJ* program has components at both the individual level (for high school) and the household level, we cannot directly construct a *BBJ* share e.g. households receiving *BBJ* divided by total households in the locality as we can with *Prospera*.

spending compared with *Prospera* pre rollback, had little mitigating effect on rollback's negative effects on school enrollment. The two programs, while both targeting education, differed somewhat in structure, in who receives the benefits and in the extent of conditionality. These differences might affect the relative impacts of the two programs, although as described earlier, the largest difference is likely to be due to the change in progressivity.

5 Conclusions

The pioneering conditional cash transfer *Prospera* was unexpectedly rolled back after more than two decades of successful operation. We study the effects of this rollback on school enrollment just following rollback. Over its more than two decades of operation, the program had demonstrated clear and accumulating impacts on increasing education levels. Further, while initial effects of the program were primarily to increase enrollment in middle school, as education levels generally increased in Mexico, impacts spread to the high school level (Parker and Vogl (2023), suggesting adaptation of the program to changing economic conditions.

Our estimates suggest that the rollback led to significant declines in school enrollment, principally for youth of high school ages, where enrollment lags behind lower schooling levels. Our main specification results suggest important effects, with rollback leading to a decrease in the probability of school enrollment of 8.9 percentage points for youth ages 15-17, relative to a base enrollment of 73.1 percent. This corresponds for a community going from 100% coverage of *Prospera* to 0, this impact implies a reduction of school enrollment of more than 10%. Strikingly, our estimated impact of the initial effects of rollback for boys is as large as the initial positive effects found in early evaluations of *Prospera* (Schultz (2004)), albeit at different schooling levels.[13].

[13]The initial results however in Schultz (2004) and others were based on the experimental evaluation sample consisting of 506 communities in seven states, whereas our results here reflect nationwide impacts, as in Parker and Vogl (2023)

Our results suggest that the effects are significantly larger overall for boys than girls ages 15-17. The estimated reductions in school enrollment for boys are quite large, with an implied reduction in school enrollment of 12 percentage points for males age 15 to 17, which corresponds to a reduction of 17% in school enrollment for a locality going from 100% to 0 in program intensity. For girls, while overall, we do not find strong evidence of a significant reduction in the probability of school enrollment, we do find some evidence of a significant fall in school enrollment for the subgroups of girls in rural and in high marginalization communities, suggesting there may be some effects for girls, although less prevalent than for boys. The reductions in school enrollment are accompanied for boys by a significant increase in the probability of labor market participation, but we do not find evidence for labor market effects of rollback on girls.

Overall, our analysis thus suggests important costs of rollback in terms of future educational attainment of the children of former *Prospera* households, especially for marginal high school students. Our results are particularly striking because they are *net* of the implementation of a substitute program, the *BBJ* program. This substitute program was implemented within several months of the rollback of *Prospera* and in fact received and spent, by the end of 2019, a comparable amount of resources as pre-rollback on education grants as were previously spent on *Prospera*. However, we demonstrate that the substitute program led to significantly reduced resources for many *Prospera* families, likely a major factor leading to the impacts we observe here. The conditionality of the *BBJ* program was much looser also than the *Prospera* program where attendance was verified continuously, which may also have played a role in the reduction of school attendance that we have observed for males. A final factor is that under the substitute program, the majority of education transfers go directly to high school students, rather than their mothers as was the case under *Prospera*.

Our results suggest greater effects of rollback on the school enrollment of boys. But, why would the school enrollment of girls be more protected than that of boys post-rollback? In fact, the structure of the *Prospera* grants at lower and upper high school (6th through 12th grade) was such that girls received larger transfers linked to education (averaging 15% higher), so that one might have expected all else equal that the rollback would have a larger negative effects on females rather than males. A countervailing factor may be that the higher grants paid to girls as well as the gender focus of the *Prospera* program led to a greater emphasis on improving attitudes towards girls' education relative to boys' among beneficiary families. At the high school level, where the recipients of the grants under *BBJ* are now the students themselves, the decisions on school enrollment are more likely to be made by the students themselves relative to the parents. This might help explain differences in the effects of rollback at the high school level by gender if, for instance, boys have greater opportunity costs, higher discount rates, or different preferences on additional schooling. [14]

We close with a caveat and related directions for future research. The rollback and development/implementation of a new substitute program would naturally be expected to take some amount of time, and so it may be that some of the initial negative impacts on enrollment will fall or disappear with the greater regularization and implementation of the substitute program. Studying the effects of rollback on educational attainment past the initial one year effects studied here is a clear priority. The onset of the pandemic one year after rollback increases the importance of understanding the later impacts of the rollback of *Prospera* as well as the difficulties of disentangling effects.

[14]At a global level, there is increasing evidence of females out performing males in high school and above UNESCO (2022)

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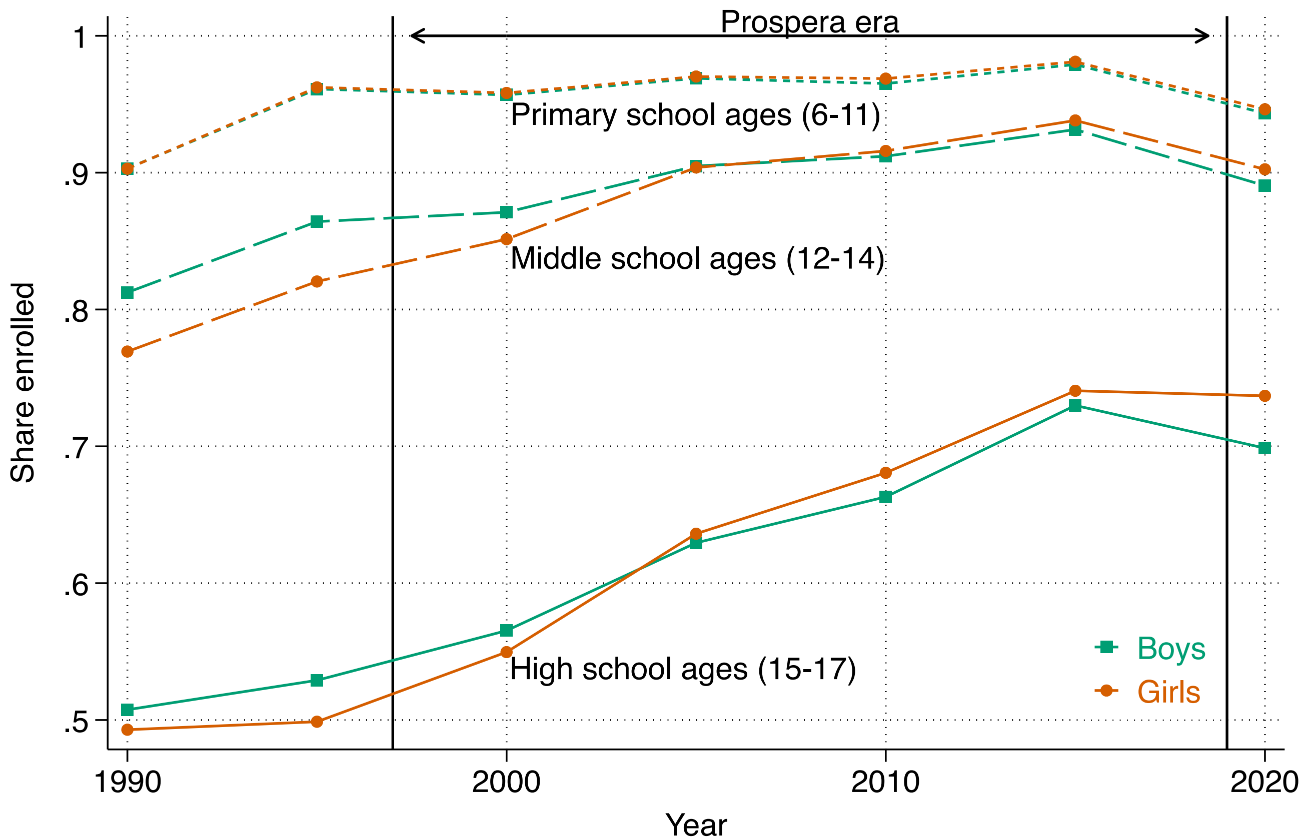
Figure A1: School enrollment over time, census data



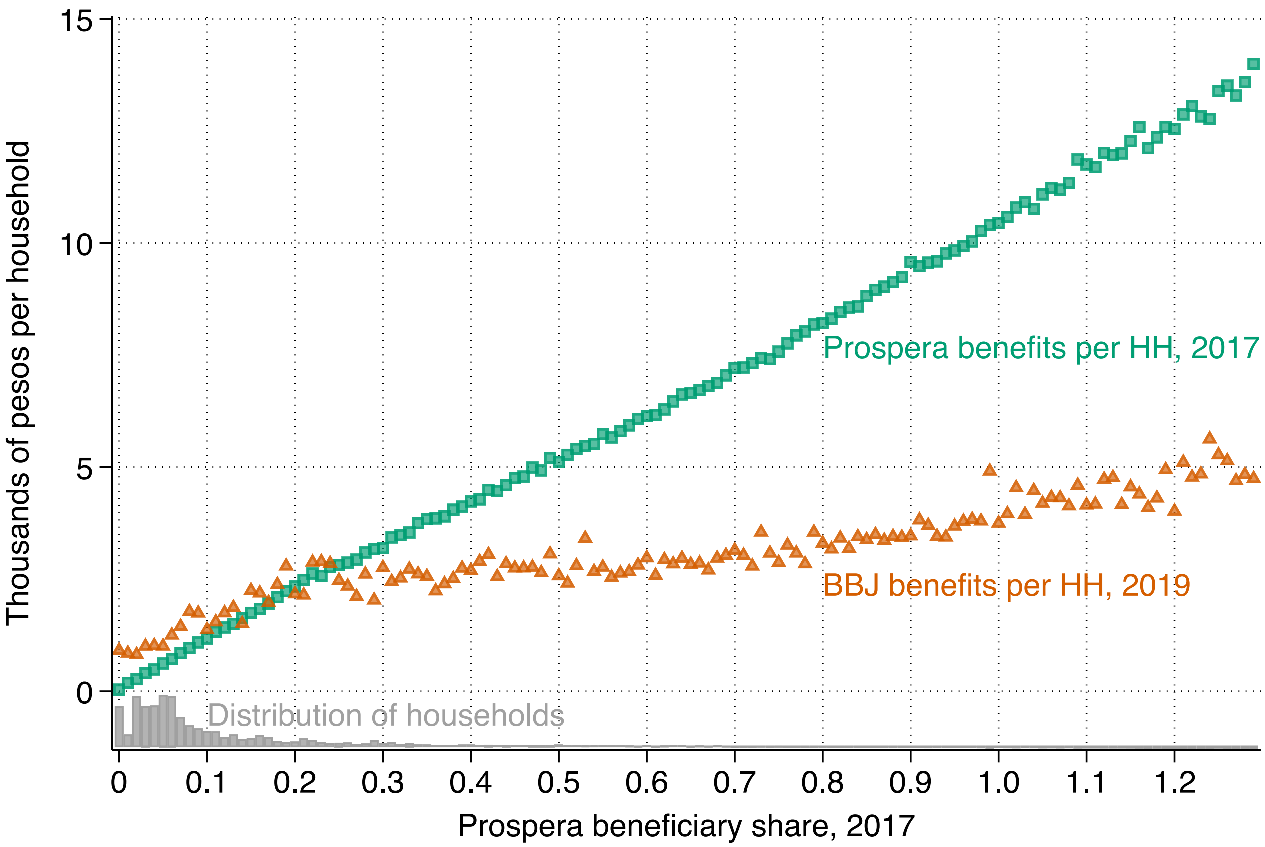
Note: Data are from the 1990, 2000, 2010, and 2020 censuses and the 1995, 2005, and 2015 intercensal surveys. The age ranges for primary, middle, and high school follow a typical student's grade progression in the Mexican system. The 2020 census was collected throughout March, with an official reference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

Table A1: Descriptive statistics on the 2017 *Prospera* beneficiary share

	Localities	Mean	Std. Dev.	25^{th} %-ile	75^{th} %-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

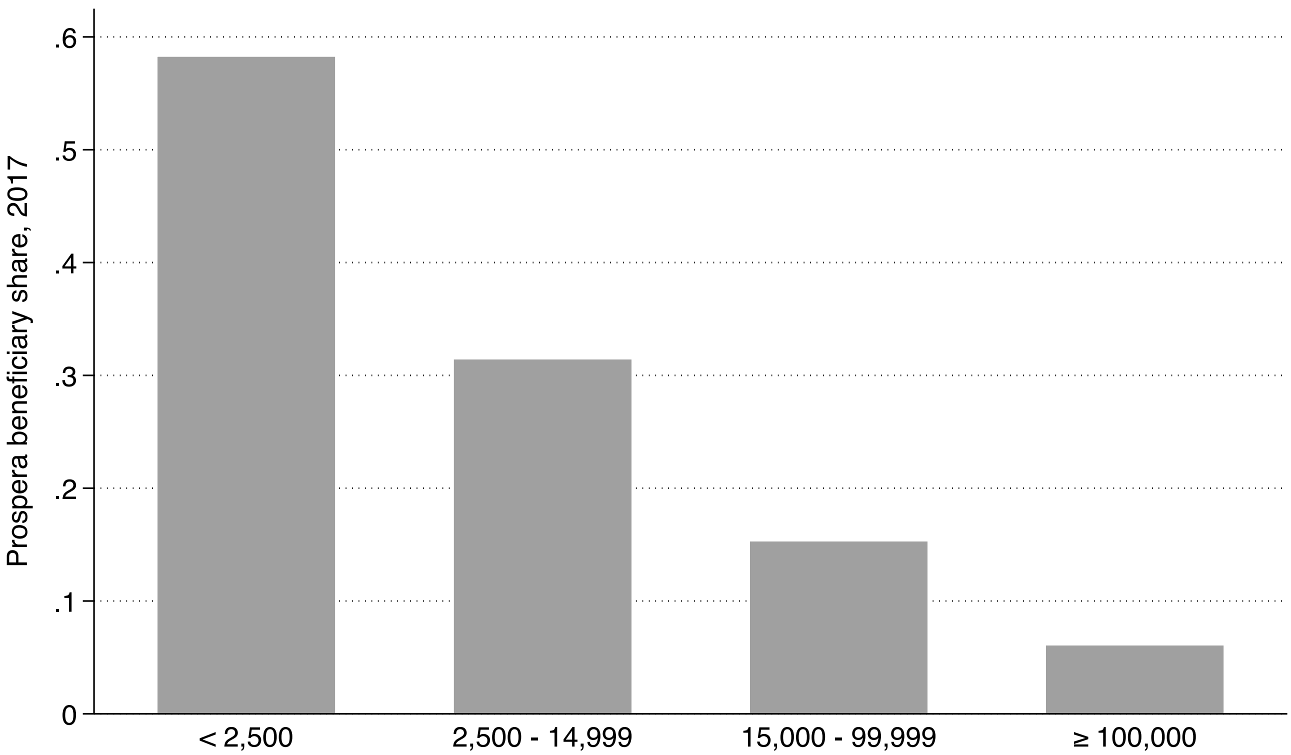
Note: Sample consists of CONAPO localities with more than 100 residents in the 2010 census that could be uniquely matched with *Prospera* data. Large cities are defined as having more than 100,000 residents in the 2010 census. The ENOE is designed to be representative with and without large cities. The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

Figure A2: Benefits by locality *Prospera* beneficiary share



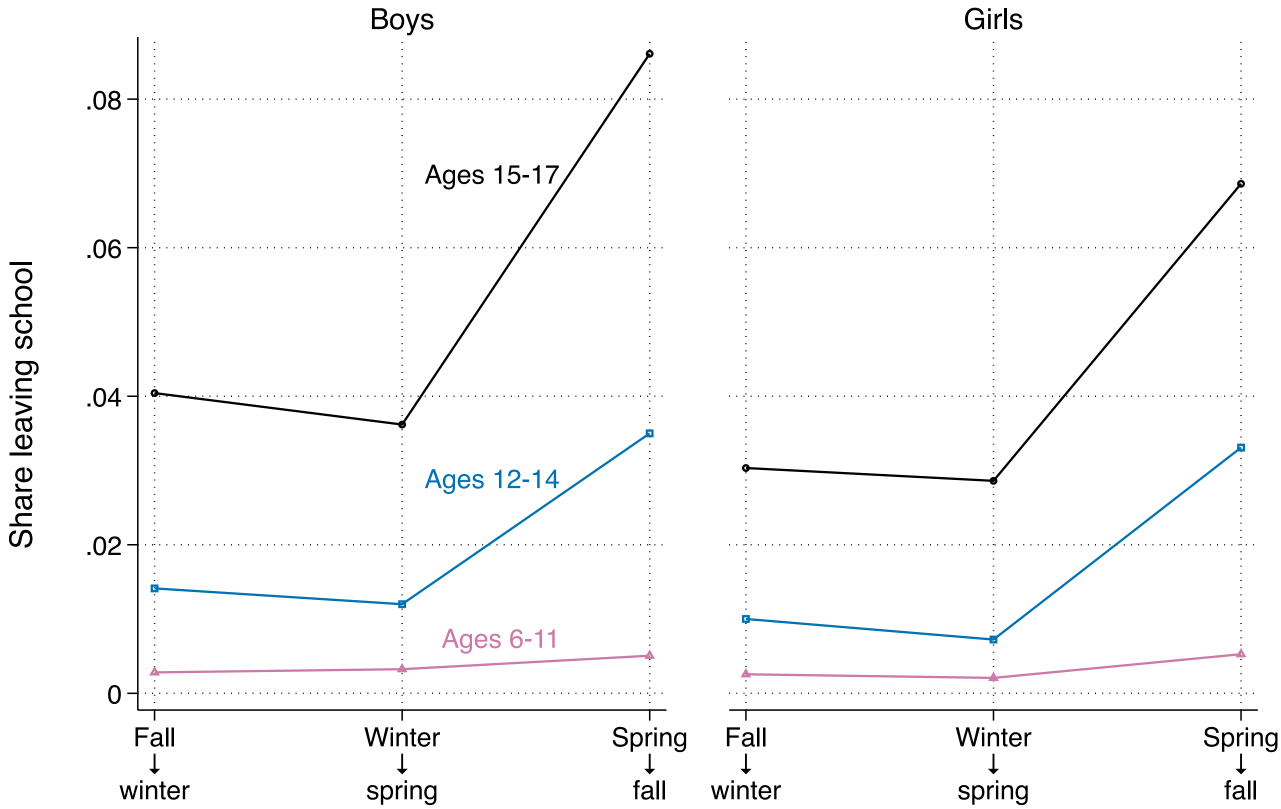
Note: Sample includes localities with at least 100 residents, which contain 98% of the Mexican population. Beneficiary data are from program administrative records; household counts are from ITER. *Prospera* data are for the last non-electoral year preceding rollback, 2017; Becas Benito Juárez (*BBJ*) data are for the first year of operation, 2019. Household counts are for 2010, the most recent census preceding the rollback of *Prospera*.

Figure A3: *Prospera* beneficiary share by locality size



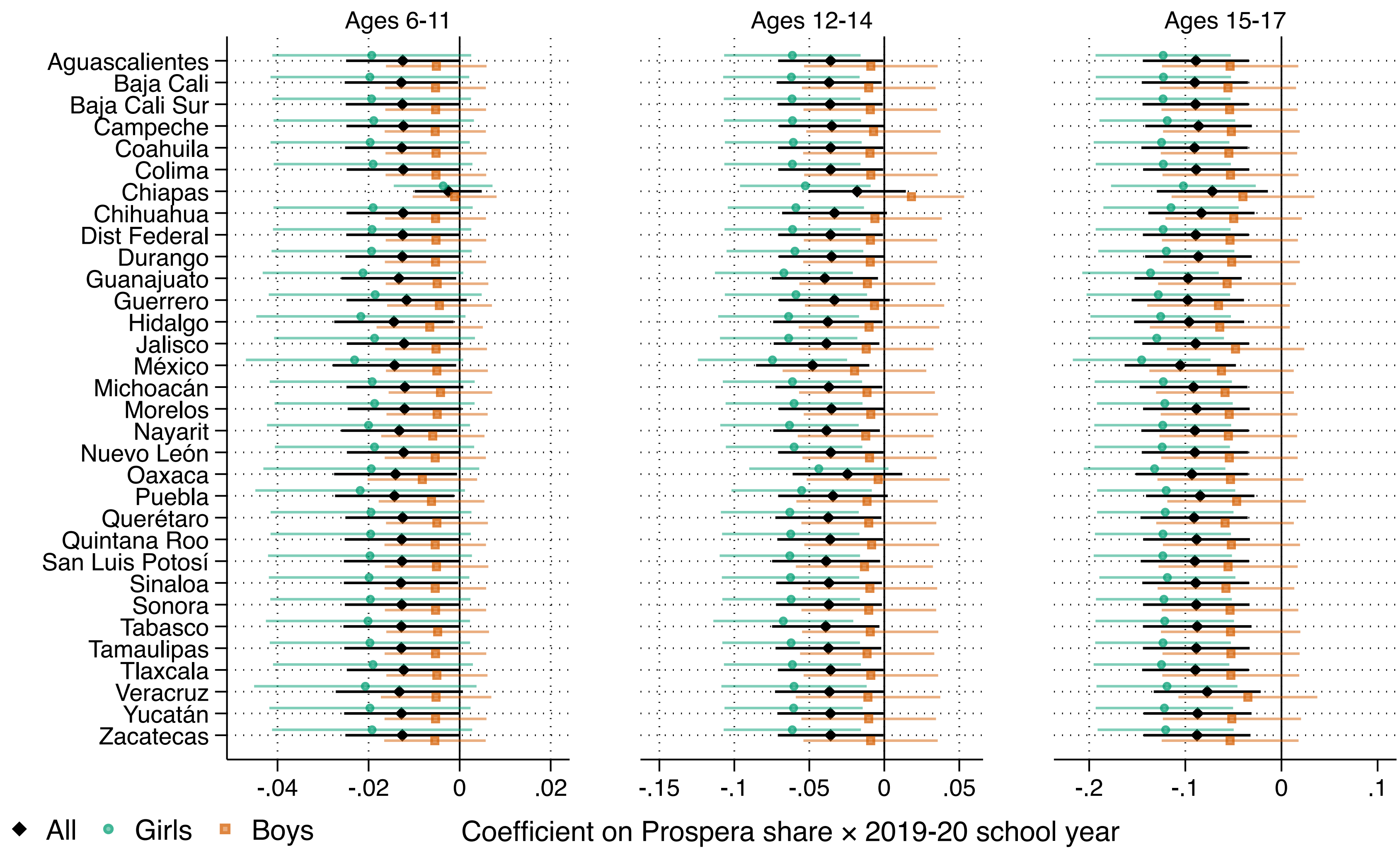
Note: Sample includes localities with at least 100 residents, which contain 98% of the Mexican population. The ENOE is designed to be representative of localities in each of the population categories.

Figure A4: School-leaving rates by season



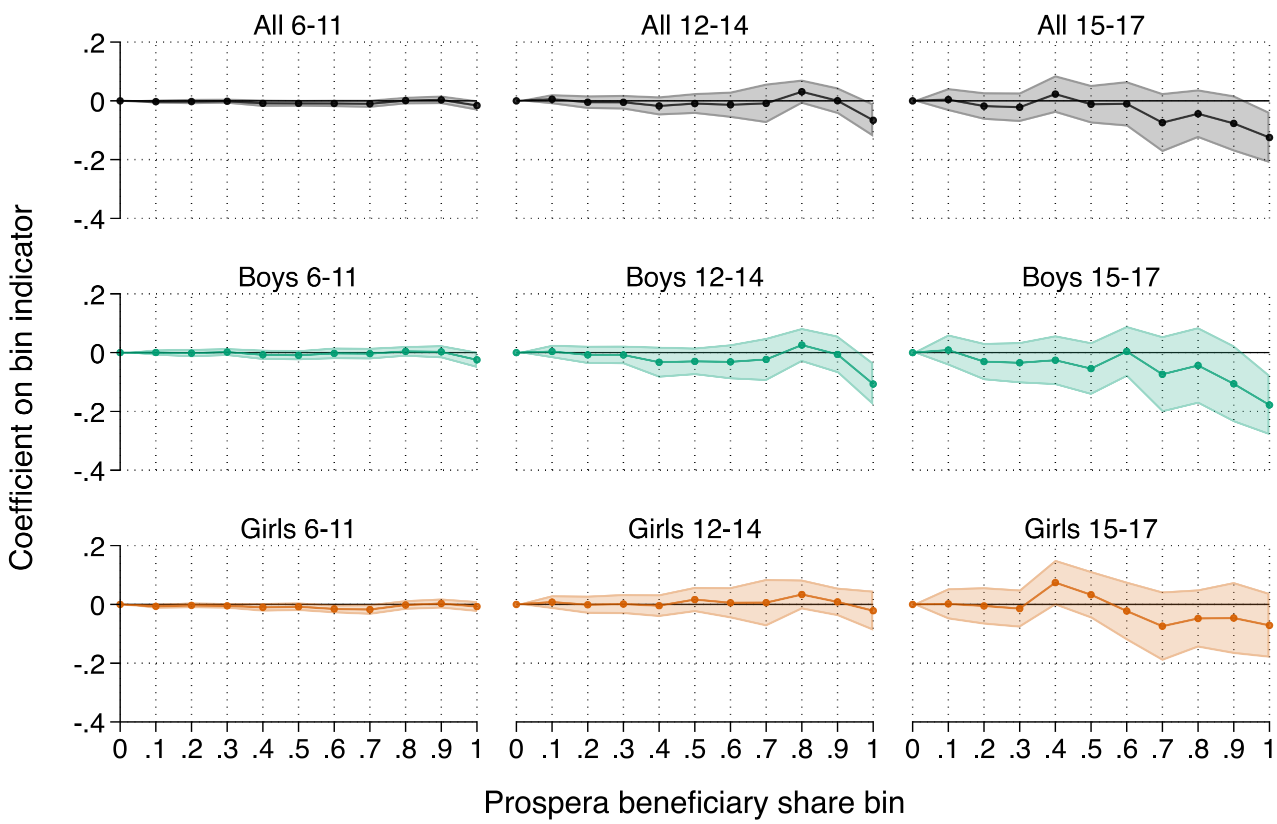
Note: Share of children enrolled in the starting season who were not enrolled in the ending season. Age is measured in the starting season; 17-year-olds who turned 18 are excluded. Sample excludes summers and localities with more than 100,000 residents.

Figure A5: Robustness of enrollment effects to omission of individual states



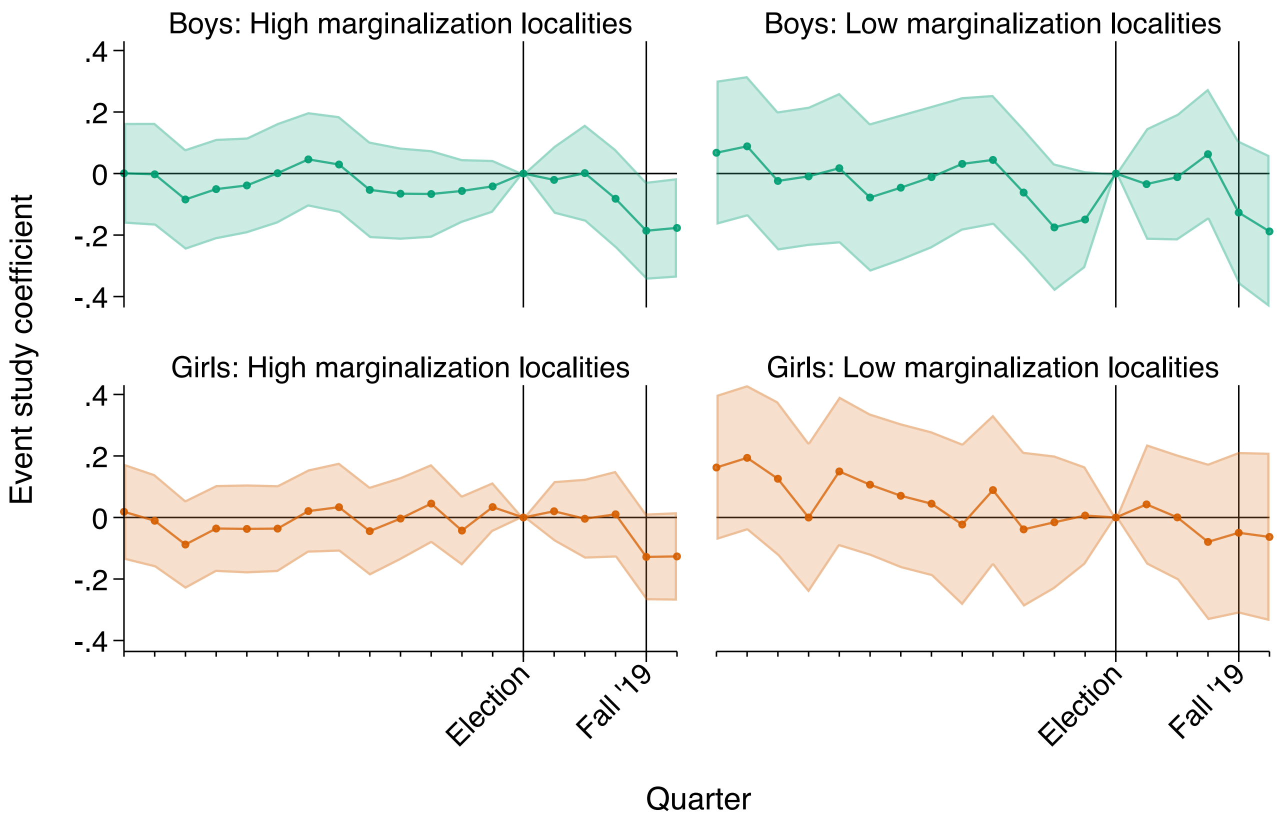
Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. Each row omits the state indicated on the left. All regressions include the the *Prospera* share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample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

Figure A6: Binned estimates of enrollment effects by age group and sex



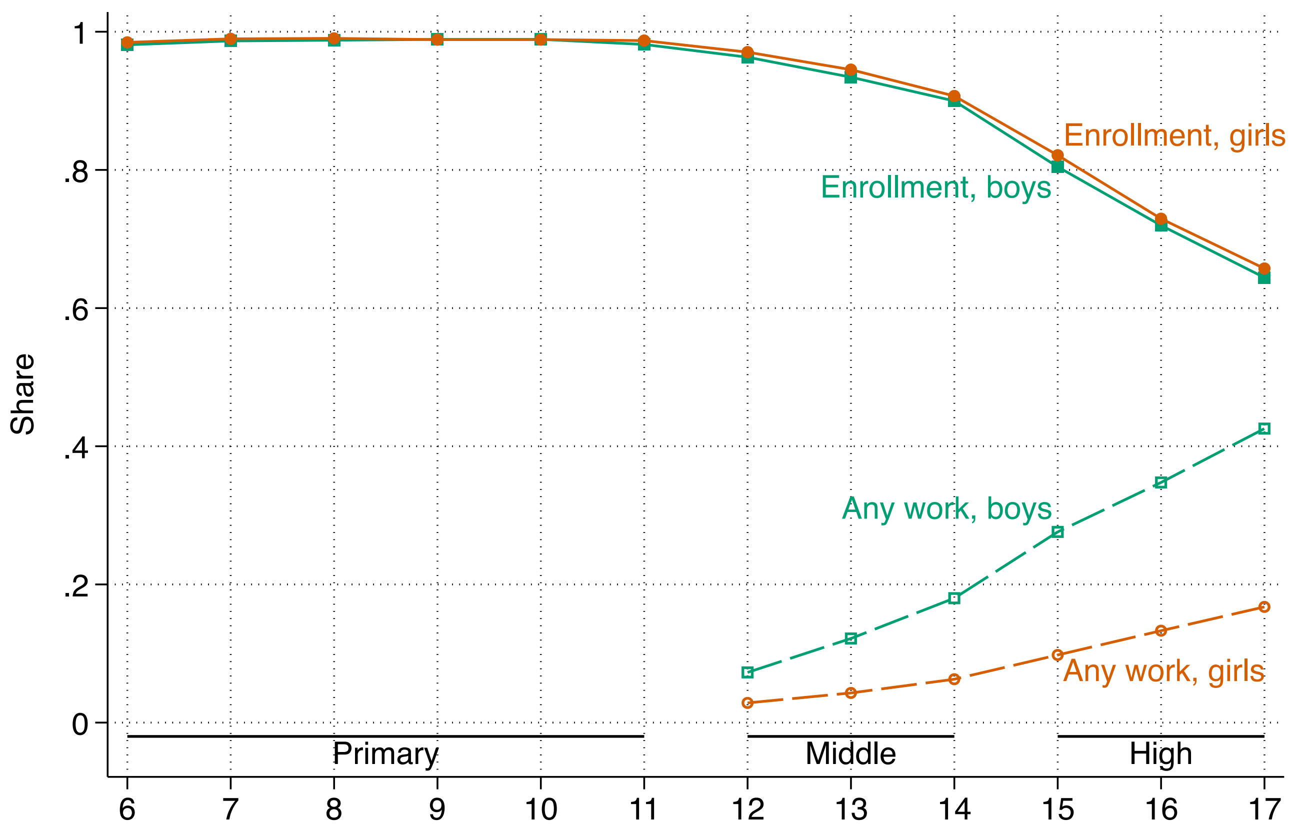
Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. Coefficients on the interaction each bin indicator with an indicator for the 2019-20 school year. All regressions include bin indicators, their interactions with an indicator for the 2018-19 school year, 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: Enrollment event study by sex and locality marginalization, 15-17 year olds



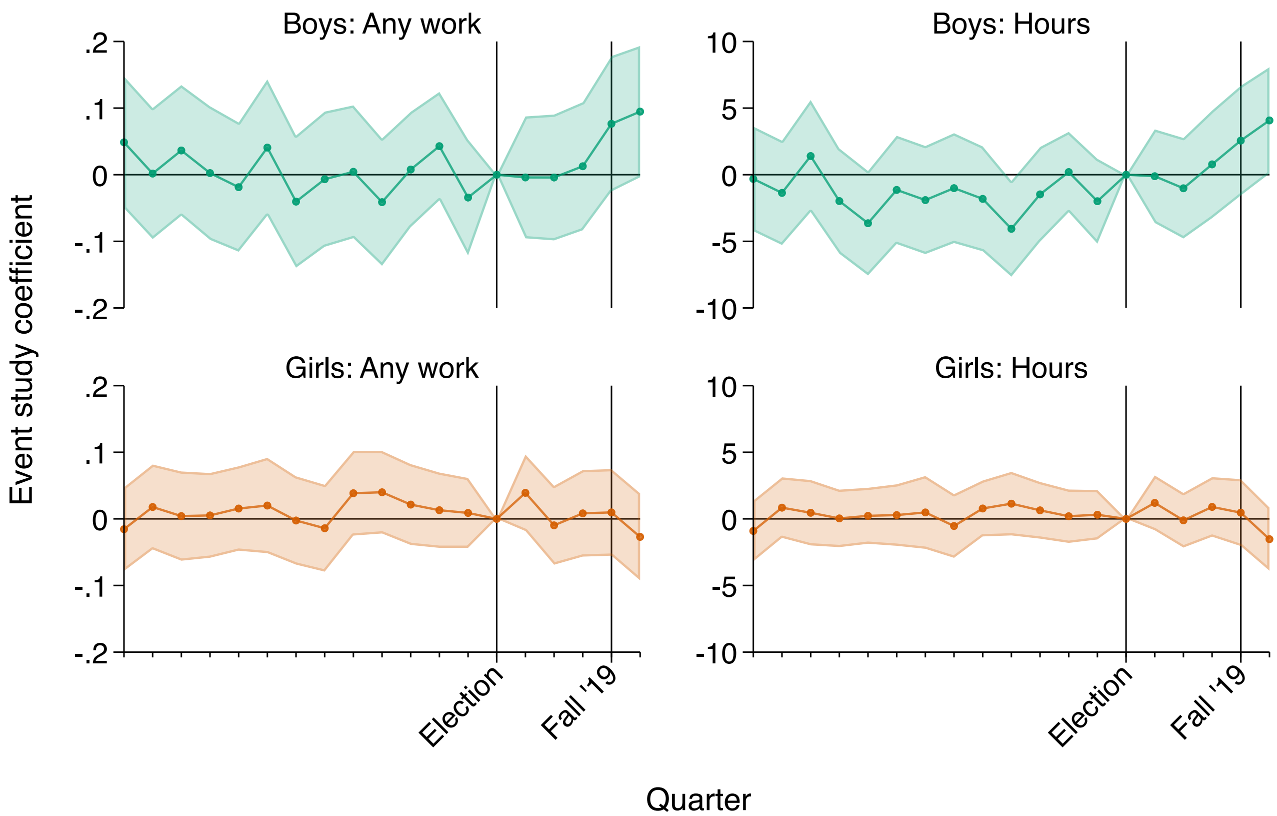
Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. All regressions include the *Prospera* share and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents. "High" indicates the high and very high marginalization categories; "low" indicates very low, low, and medium marginalization categories.

Figure A8: School enrollment and work by age and sex



Note: Sample excludes summers and localities with more than 100,000 residents. The ENOE does not ask about labor market outcomes for children under 12. The age ranges for primary, middle, and high school follow a typical student's grade progression in the Mexican system.

Figure A9: Labor market event study by sex, 15-17 year olds



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.