Do Enrollment Gains From Conditional Cash Transfers Sustain Disruption? Evidence From Mexico[*]

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Abstract

Anti-poverty programs may lose their effectiveness over time, due to weakening implementation or failure to adapt to changing policy conditions. Mexico's pioneering conditional cash transfer program *Prospera*, well-known for raising school enrollment in youth from poor families, operated over two decades in a shifting educational landscape. We estimate *Prospera*'s effects on school enrollment 22 years after rollout, exploiting the program's sudden and unexpected rollback in early 2019. Comparing areas with high and low program penetration before and after rollback, we find that the cessation of benefits immediately reduced school enrollment, especially at high school ages and especially in boys. Rising work mirrored falling enrollment in boys of high school age. Enrollment effects were at least as large at rollback as they were at rollout, albeit shifted from middle-school ages to high-school ages, implying the program's successful adaptation to the rise of high school in the decades since rollout.

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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—demonstrated improvements in children's education, children's health, and household economic outcomes, as summarized in Parker and Todd (2017). These studies contributed to the program's 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 initial findings on program effectiveness continue to be instructive decades after a program's inception.

The question is broadly relevant to development policy because policy conditions change, 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 changed since; while enrollment rates at high school ages, originally excluded from Progresa's objectives, 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. Are longstanding programs like Progresa able to adapt to changing policy conditions like these? We shed light on this question by estimating the enrollment effects of the sudden rollback of Prospera in 2019.

Our research contributes to a new thread of research on transfer programs, on whether program impacts persist after transfers end. Existing studies on this topic focus primarily on whether positive effects of short-term pilot studies are maintained post-program (Haushofer and Shapiro, 2018; Baird et al., 2019; Blattman et al., 2020). We study the sudden and unexpected rollback of one of the oldest

and best-known CCT programs in the world, 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. Do program-based gains in education survive, or do they disappear with the program? If the latter, do the losses occur at the same schooling level as the original gains, or do they shift to higher levels as they become more relevant to the marginal student?

We estimate the effects of rollback on school enrollment using a difference-in-differences design, comparing enrollment 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 at the start of 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-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 enrollment among high-school-aged boys declined by 12.3 percentage points in localities with full program penetration, relative to

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[1]The municipality is the administrative unit between the locality and the state in Mexico, akin to counties in the United States.

localities with no program penetration—a fall of approximately 17%. Comparing localities as the 75th and 25th percentiles of program penetration, the decline

After the cancellation of Prospera was announced, the government implemented a substitute program of education grants linked more loosely to school enrollment called *Programa Nacional de Becas para el Bienestar Benito Juárez* (BBJ). Using administrative municipal level data on recipients of Prospera and Becas Benito Juarez grants, we study coverage and transfers received in the new substitute program compared with under Prospera. While overall spending a similar amount of resources pre- versus post-rollback, we find that the progressivity in payments to families declined substantially. (Progressivity is measured using the locality level marginalization index, an official measurement of community level poverty indicators developed by the Mexican Population Council, CONAPO.) Our analysis suggests that the average Prospera household under the new program received substantially lower transfers post rollback. Further, households in the poorest localities experienced the largest reductions in transfer payments. Finally, evidence suggests there were significant delays in getting the new program up and running (Jaramillo-Molino (2020).

The paper develops as follows. Section 2 describes the Prospera program, its rollback and the substitute program implemented in the aftermath. Section 3 presents hypotheses and section 4 presents the data and empirical model. Section 5 presents the main results and section 6 concludes.

2   Background

2.1   Rolling Out Prospera

First implemented as Progresa in 1997, Prospera 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 families through direct monetary transfers

conditioned on school enrollment and attendance as well as preventive health clinic visits, increasing its average beneficiaries' incomes by about 20 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. Prospera 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 Prospera, 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)). 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 levels 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 4-5 percentage points for boys and 8-10 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. 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. Finally, both Behrman et al. (2009) and Todd and Wolpin (2006) show that the program reduced the age of entry to primary school.[2]

Later studies examine the medium- and longer-term impacts 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 participating in the program leads to significant improvements in 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

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 Federacion*, a daily publication of the Mexican Federal government akin to the United States' *Federal Register*, announced that during 2019 Prospera would abruptly end and 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 *Presupuesto de Egresos de la Federacion* published in late 2018, which presented the government's 2019 budget, also stated that Prospera's resources would be reassigned to the new substitute program.[3]

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[2]Studies estimating structural models with the the randomized evaluation data find sized effects on education. (Todd and Wolpin (2006) and Attanasio et al. (2012).

[3]Prospera also had a health and nutrition component, including a fixed monetary transfer linked to preventive health clinic visits, but no new program substituted for these components was created.

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

Prospera		BBJ	
Per child transfer to HH	$350 (grade 6)	Flat transfer to HH	$800 (grades 3-9)
	$660 (grade 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.

We now turn to a comparison of Prospera and the new BBJ program in terms of benefits provided and coverage. With regard to benefits, both programs provide transfers conditional on school enrollment, although for the BBJ program conditionality was greatly loosened and attendance was not monitored. [4] In terms of amounts, the BBJ program, *Beca de Educación Basica*, 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 Prospera's payments, which depended on the number of children enrolled and their grades.

BBJ, *Beca de Educación Media Superior*, similarly 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.[5] 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 resource-sharing between teenagers and their parents.

These somewhat nuanced differences in program benefits and rules were, however, swamped by disruption and changes in program reach. A number of newspapers report complaints and

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[4]Prospera monitored the enrollment and attendance of each child, and an 85% attendance record was required to receive the monthly grant for that particular child. For BBJ, only one child per family in primary and middle school is required to be enrolled, and attendance is not monitored. Similarly, at the high school level, BBJ also requires enrollment but does not monitor attendance.

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

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. 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. The net result is that the substitute program provides much lower resources per household in poor communities. We thus hypothesize significant disruption

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Figure 1: Program penetration by locality marginalization
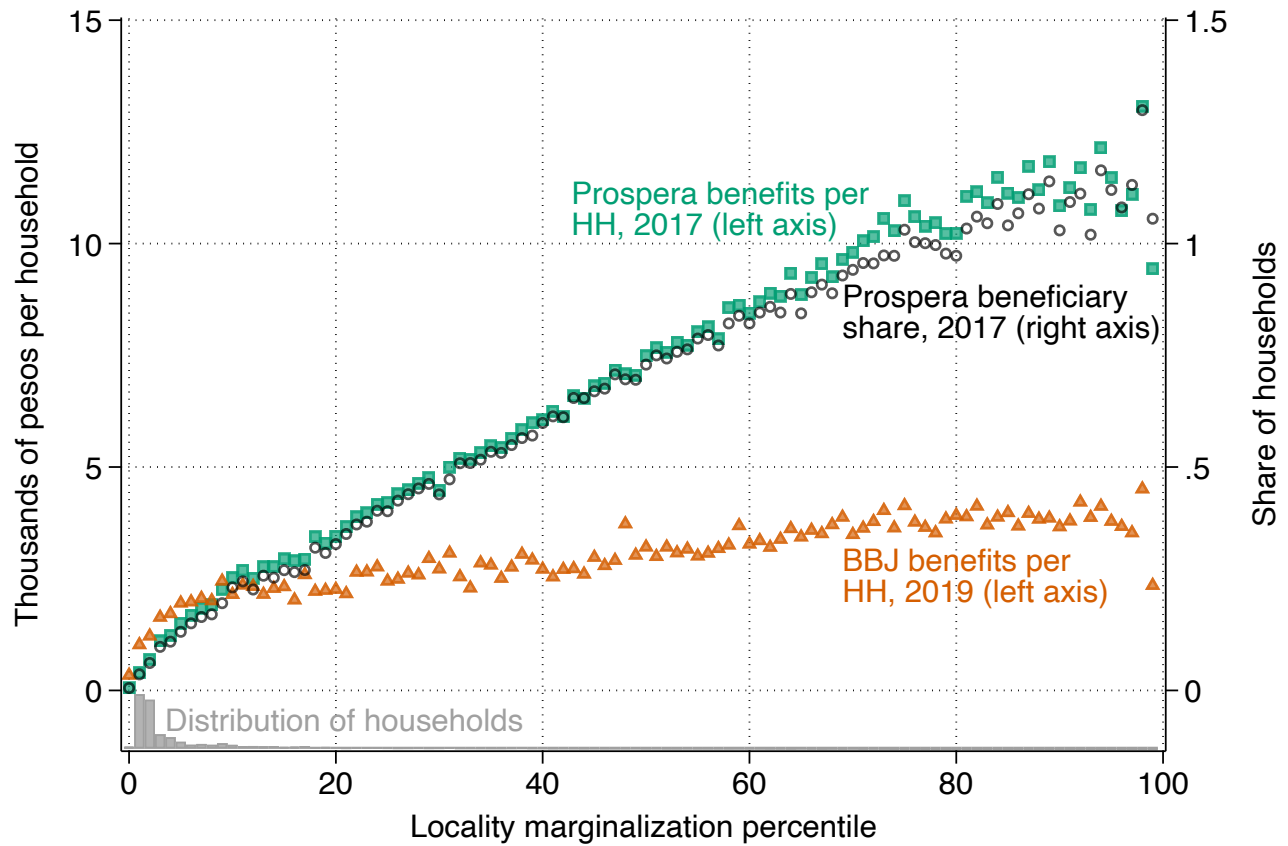
Note: Sample includes localities with at least 100 residents, which contain 98% of the Mexican population. Beneficiary data are from program administrative records; household counts are from ITER; marginalization data are from CONAPO. Prospera data are for the last non-electoral year preceding rollback, 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.

for Prospera households of rollback.

3   Hypotheses

CCT programs like Prospera are commonly thought to protect children's schooling by easing the financial constraints their parents face and by conditioning benefits on school enrollment and health clinic visits, effectively subsidizing investment in school and health. The rollback, through both reducing income and largely eliminating the conditionality on enrollment, might lead to an immediate

worsening of school outcomes among children. On the other hand, beneficiary households may have become better off economically due to years of receiving benefits or more knowledgeable about the benefits of education, making them better able to weather rollback. Rollback may also reduce the amount of resources controlled by women in the household, who were the typical recipients of the transfers, which some studies have suggested was also a program feature affecting investment in children (Rubalcava et al. (2009)).[6]

The substitute program BBJ described in the previous section might reduce or eliminate potential negative effects of the rollback of Prospera. However, as we documented, the extent and reach of this program in its first year post rollback was significantly lower than that of Prospera, with households in above the median marginalization localities on average receiving less than half of the total benefits of Prospera post-rollback. The impacts of rollback that we estimate in this paper are net of the implemented BBJ substitute program.

4 Data and Methods

4.1 Data

The primary educational outcome we evaluate is school enrollment defined using a binary variable. For youth aged 15 to 17, we also study labor market participation and hours worked. We use 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 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

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[6]An additional way in which rollback might affect enrollment is through the reduction of positive spillovers. Both Lalive and Cattaneo (2009) and Bobonis and Finan (2009) report that the program increased significantly the school enrollment of program ineligible children and youth who were living in the treatment communities.

levels. [7] In addition to labor market information, the ENOE includes variables measuring current enrollment in school, completed years of schooling, and time studied during the previous week for all members of the household. We use cross sectional ENOE data rounds between 2014 and 2020.

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, as 2018 was an electoral year.

4.2   Design and Estimation

The rollback of Prospera began during the first bimester of 2019, after Lopez Obrador took office in December, 2018. Consequently, we hypothesize impacts on school enrollment at the beginning of the following school year 2019-2020, comparing fall and winter enrollment in school year 2019-2020 with fall and winter enrollment in previous years (2013-2014 through 2018-2019). We thus study the impacts on school enrollment about 9 months post rollback, and 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

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[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.

during the academic school year. [8] We focus only on the effects through the first quarter of 2020, due to the disruptions which occurred in the fieldwork of ENOE during the Covid pandemic.[9] 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:

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

for child 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.

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 pre-

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[8]Although Appendix Figure A3 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.

[9]During the pandemic, INEGI carried out interviews by telephone and reduced the sample, becoming the ETOE (Encuesta Telefonica de Ocupacion y Empleo). When the ENOE returned to regular operations in 2021, it stopped including a locality identifier in public-use data releases, so we cannot replicate our empirical strategy after the 2019-20 school year.

ferred 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.[10]. Our preferred 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 as large as the coefficients estimated using equation 1.

We also estimate an event study specification:

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

Here we modify the main specification by interacting the cross-sectional exposure variable with indicators for every quarter but the third quarter of 2018,the quarter of the presidential elections.

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[10]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.

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 adequately test for differential pre-rollback trends. Standard errors are clustered at the locality level.

We include in our estimation sample all households living in localities with less 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 A1 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.

5   Results

In this section, we present the main impacts of program rollback on enrollment and labor market participation in the year following program rollback, using our preferred specification (state quarter fixed effects). We also carry out a number of robustness checks and falsification tests.

5.1   Enrollment

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 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 program 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 program 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 boys 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 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

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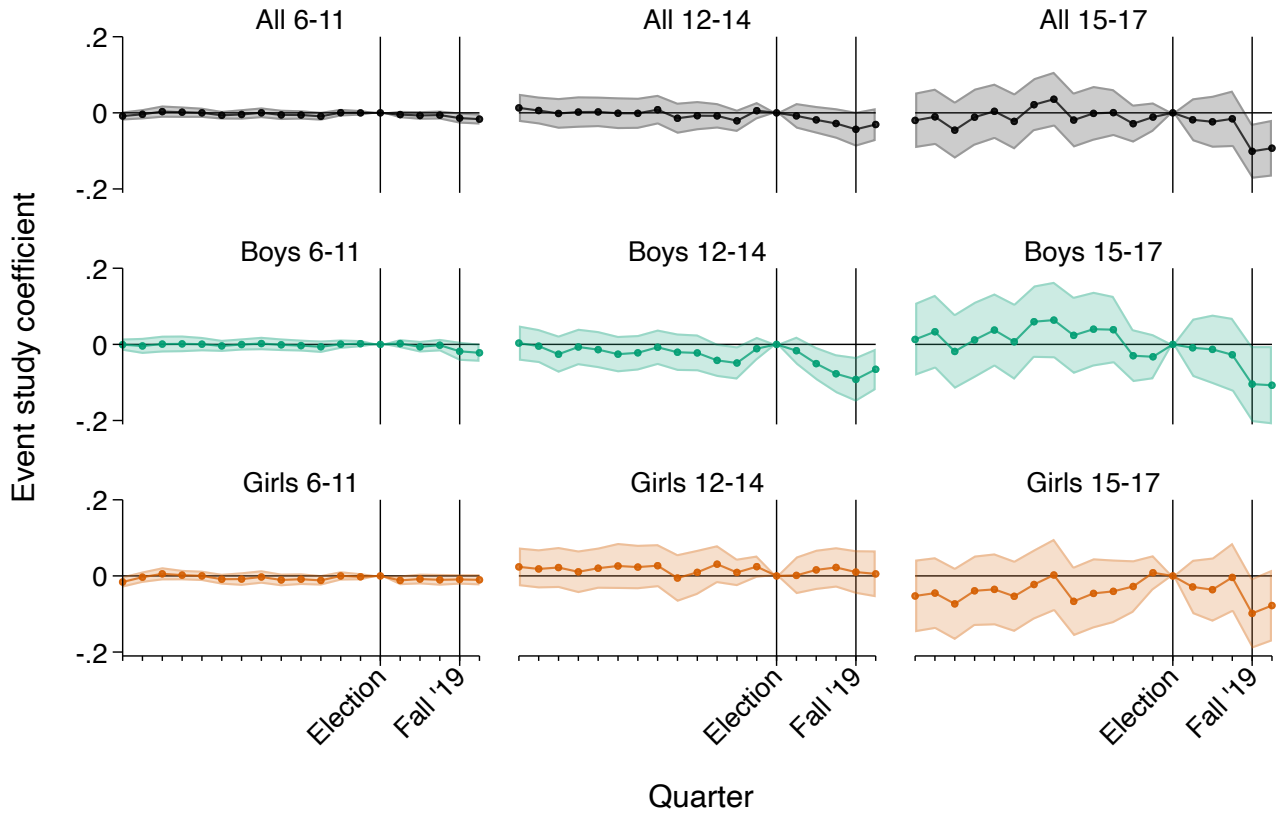
Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. All regressions include the Prospera share and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents.

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

15-17.

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 (relative to a 93.6% base enrollment rate) and for ages 6-11, the negative effect is 1.4 percentage points, relative to a base of 98.7% enrollment rate. The coefficients on γ are generally negative but statistically insignificant, implying no overall 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). 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 statistically significant at the 10% level.

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 larger for boys and at ages corresponding

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to high school enrollment.

5.2   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 [11] for our three different age groups for a number of different specifications which 1) vary the set of individual level control variables, 2) include additional controls for municipal and locality level poverty, 3) control for the potential confounding role of the BBJ grants, and 4) include a time trend. In our last specification test, we 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 age group, but these results are sensitive to the specification and much less robust than the results for boys ages 15-17.

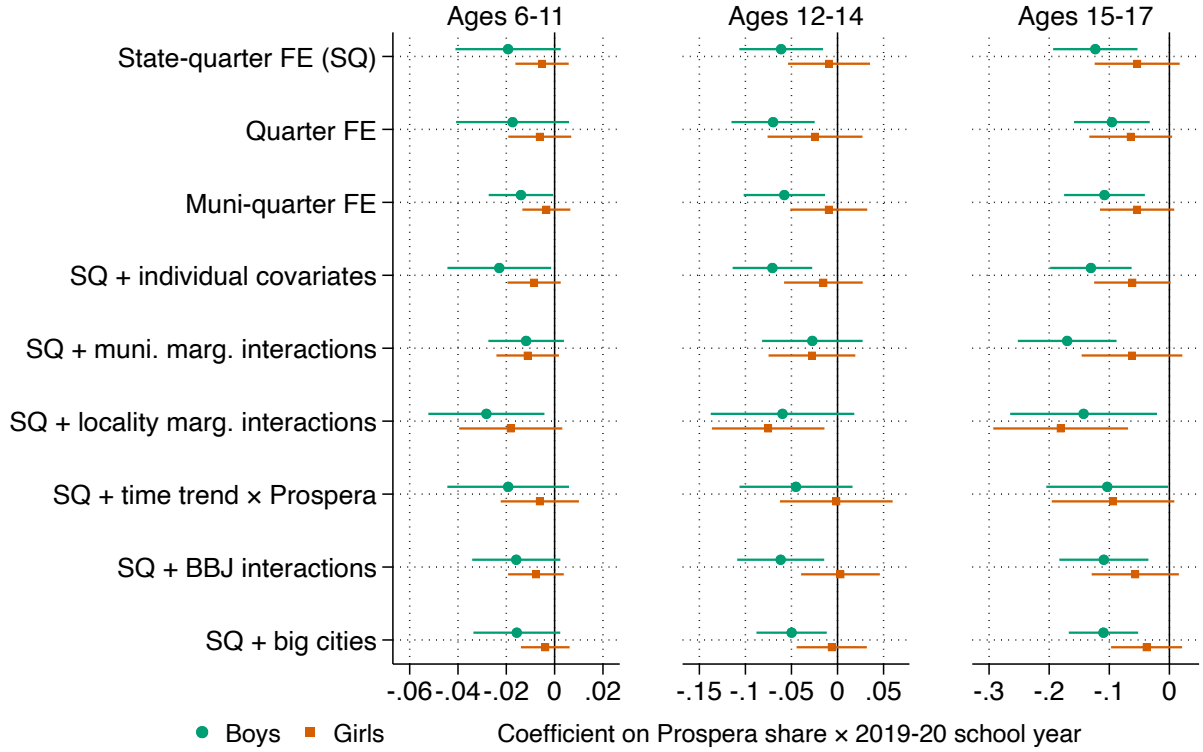Our main specification results also suggested negative and statistically significant effects of

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[11]Appendix Figure A6 presents a similar graph for boys and girls together.

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.

Finally, given potential state level differences in trends in school enrollment, Figure A7 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 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

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 "BBJ
interactions" regressions, we include 2019 BBJ benefits per household and its interactions with
indicators for the 2018-19 and 2019-20 school years. In the "big cities" regressions, we estimate the
baseline model in an expanded sample that includes cities with populations over 100,000.

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their interactions with indicators for the 2018-19 and 2019-20 school years.

Appendix Figure A8reports 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.

5.3 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 the program's targeting to poor 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. Impacts for youth whose mother's 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 2500 inhabitants) versus non-rural communities, with a reduction in the probability of enrolling in school in 14.9 percentage points in rural areas versus 18.5 percentage points in non rural areas. (Recall our sample excludes localities with more than 100,000 inhabitants so that non-rural are communities with 2,500-100,000

residents.). Finally, consistent with the greater targeting of Prospera to poor areas, the 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 2500) 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. The negative and significant results in rural areas and in highly marginalized localities furthermore suggest that negative effects of rollback for girls may be concentrated in the subset of poorer rural beneficiaries.

Appendix Figure A10 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 A10 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

Table 3: Enrollment effect heterogeneity, ages 15-17

	Mother education level		Locality pop.		Locality marg.	
	≤ primary (1)	> primary (2)	< 2,500 (3)	≥ 2,500 (4)	High (5)	Low (6)
A. Boys						
Prospera share × 19-20 school year	-0.186*** [0.054]	-0.069 [0.045]	-0.149*** [0.050]	-0.184*** [0.067]	-0.154*** [0.054]	-0.136 [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 × 19-20 school year	-0.030 [0.056]	-0.065 [0.046]	-0.097** [0.049]	-0.091 [0.074]	-0.117** [0.056]	-0.120 [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. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

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 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 in rural and highly marginalized communities.

5.4 Labor market effects

School and work may be substitutes, Ravallion and Wodon (2001); Skoufias and Parker (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 A4 in the Appendix presents school and work participation in the ENOE for ages 6 to 17, demonstrating very high school enrollment rates e.g. above 95% for boys and girls until about age 12 when enrollment 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. [12] Table 4 presents impacts of rollback by gender and shows rollback increases the probability of working for boys age 15 to 17 in 6.1 percentage points, compared with a baseline mean of 27.3 and an increase in unconditional hours worked of 3.7 hours per week, 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.

In conclusion, our analysis of the impacts of rollback of Prospera on school enrollment in the year following rollback suggests large negative effects of rollback, concentrated among boys, particularly those aged 15 to 17, a group corresponding to enrollment in high school. The reductions in enrollment due to rollback correspond to 12.3 percentage points, for a locality going from 100% coverage to 0 coverage. Relative to a baseline enrollment of boys ages 15-17 of 72.5%, this represents a reduction of 17% nationwide. 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

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[12]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 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** [0.026]	3.684*** [1.088]	-0.013 [0.018]	-0.503 [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. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

(2004)). These reductions in school enrollment are accompanied for boys by a significant increase in the probability of labor market participation. For girls, the evidence suggests overall insignificant although negative effects on school enrollment. However, heterogeneity analysis suggested some significant negative effects of rollback on enrollment, concentrated on rural and high marginalization localities. Finally, we do not find evidence for labor market effects of rollback on girls. [13].

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[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)

6 Conclusions

In this paper, we study the immediate short run effects of the unexpected rollback of the pioneering Prospera program after over two decades of operation. Our estimates suggest that the rollback led to significant declines in school enrollment, principally for youth of high school ages. 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%.

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 in 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 evidence of a significant fall in school enrollment for the subgroups of girls in rural and in high marginalization communities. Our analysis thus suggests important costs of rollback in terms of future educational attainment of the children of former Prospera households.

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

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

effects.

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Figure A1: School enrollment over time, census data
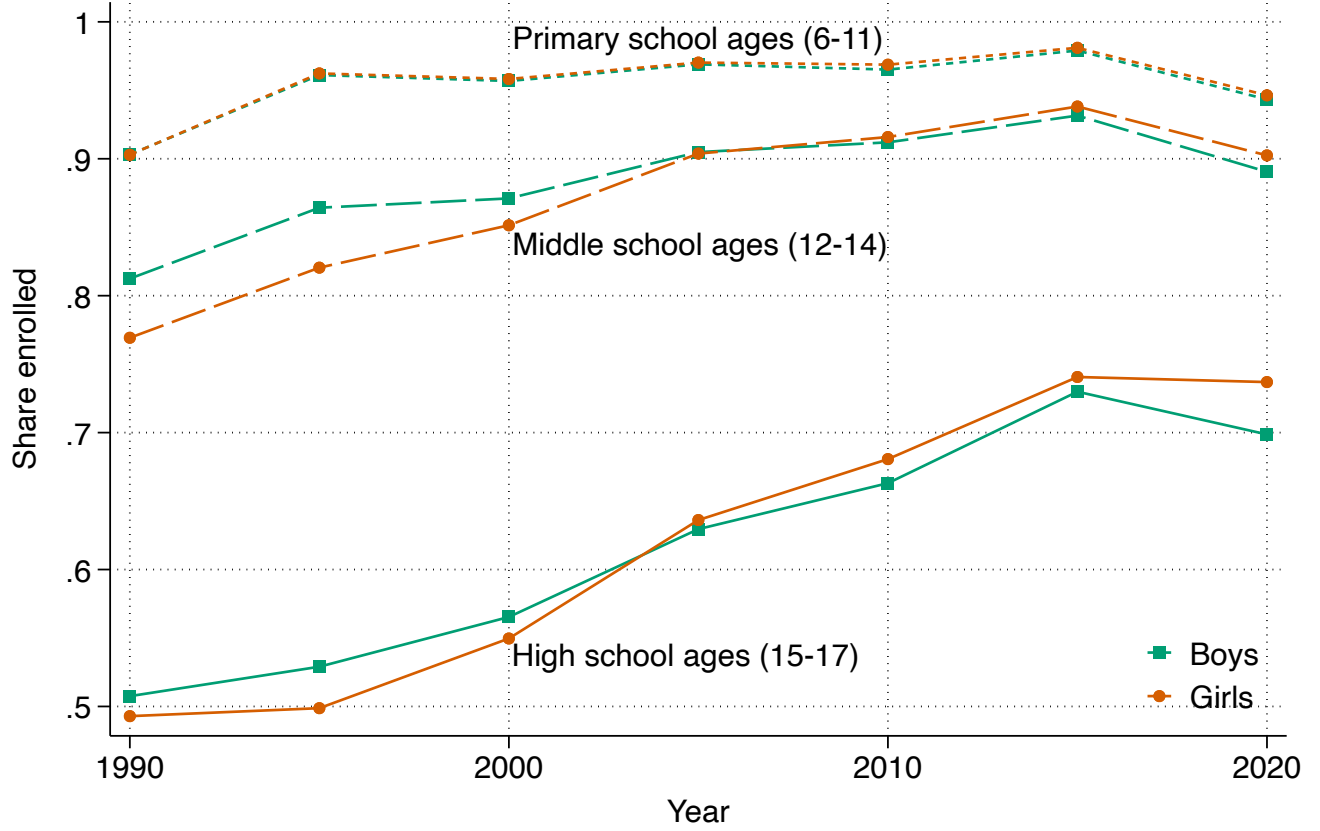
Note: Data are from the 1990, 2000, 2010, and 2020 censuses and the 1995, 2005, and 2015 intercensal surveys. The age ranges for primary, middle, and high school follow a typical student's grade progression in the Mexican system. The 2020 census was collected throughout March, with an official reference 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.	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.
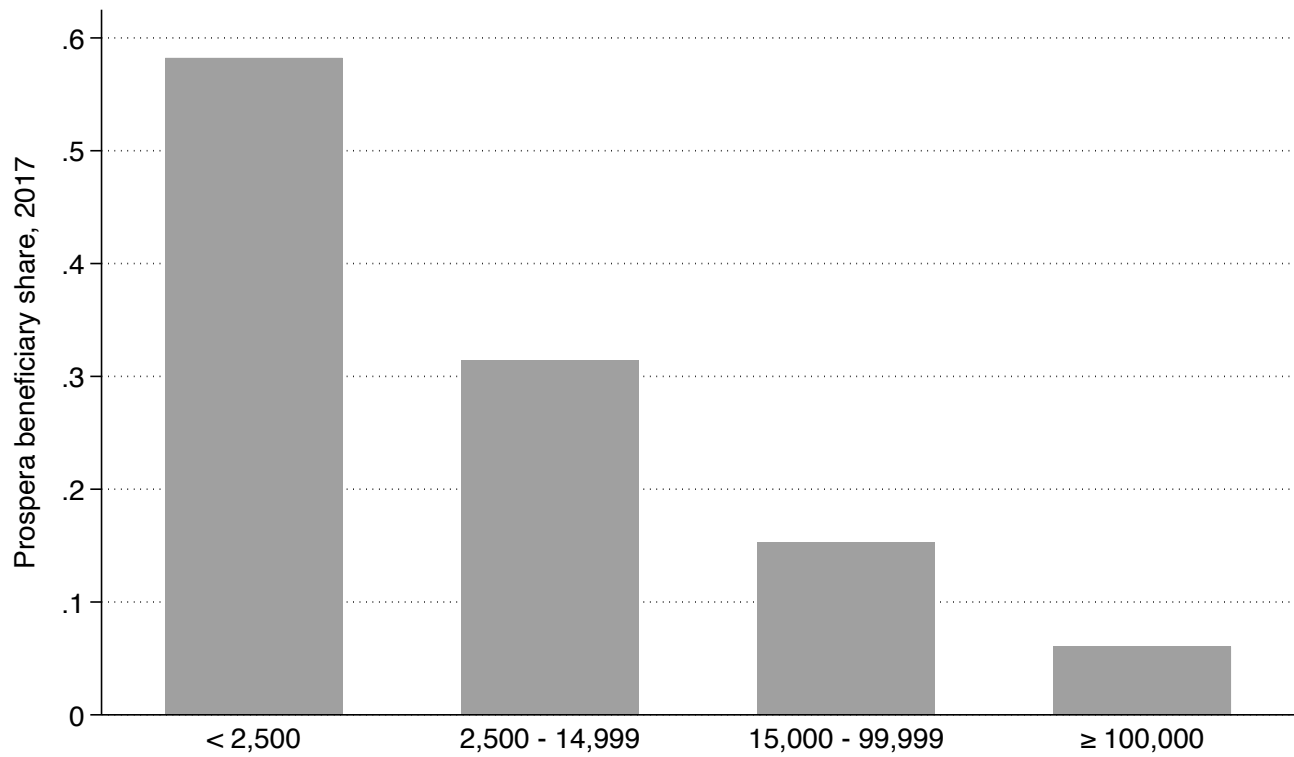
Figure A2: Benefits by locality Prospera beneficiary share
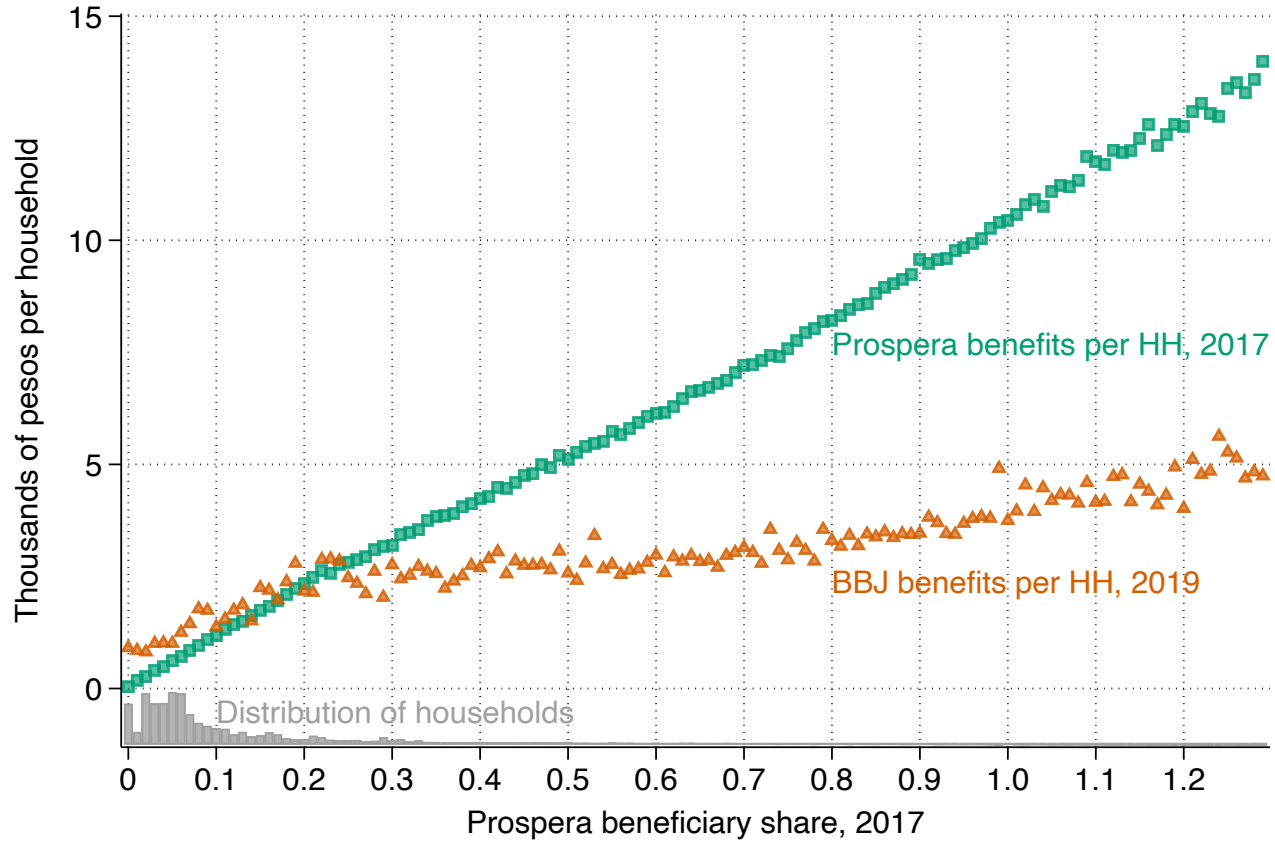
Note: Sample includes localities with at least 100 residents, which contain 98% of the Mexican population. Beneficiary data are from program administrative records; household counts are from ITER. Prospera data are for the last non-electoral year preceding rollback, 2017; Becas Benito Juárez (BBJ) data 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.
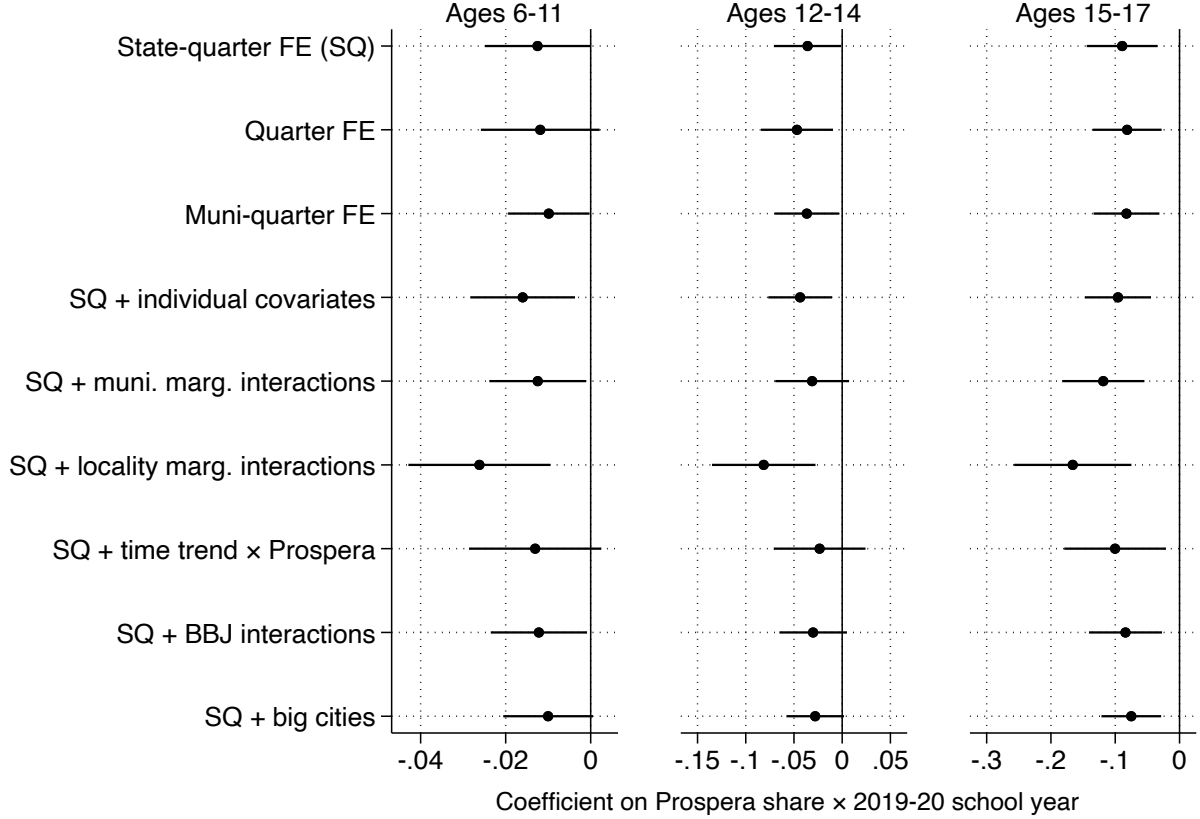
Figure A4: School enrollment and work by age and sex
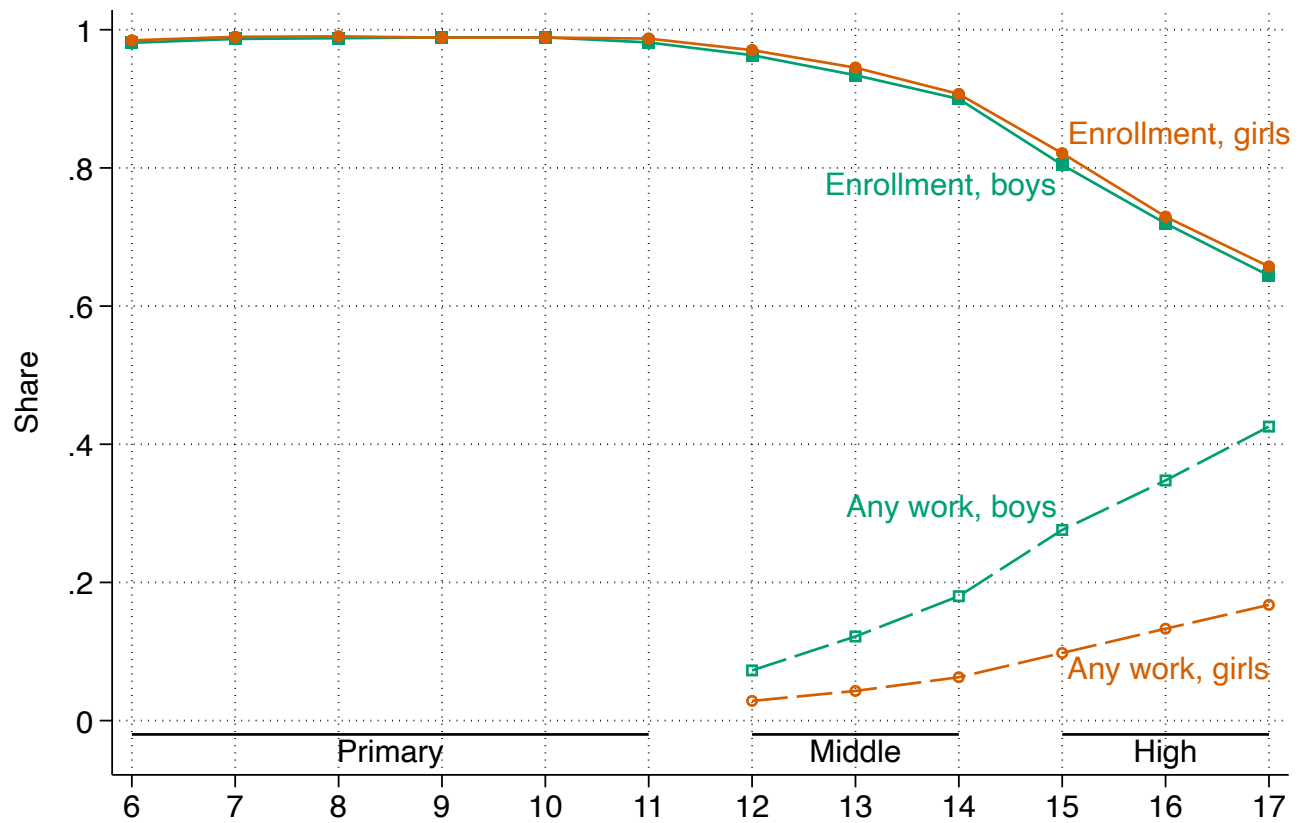
Note: Sample excludes summers and localities with more than 100,000 residents. The ENOE does not ask about labor market outcomes for children under 12. The age ranges for primary, middle, and high school follow a typical student's grade progression in the Mexican system.

Figure A5: School-leaving rates by season
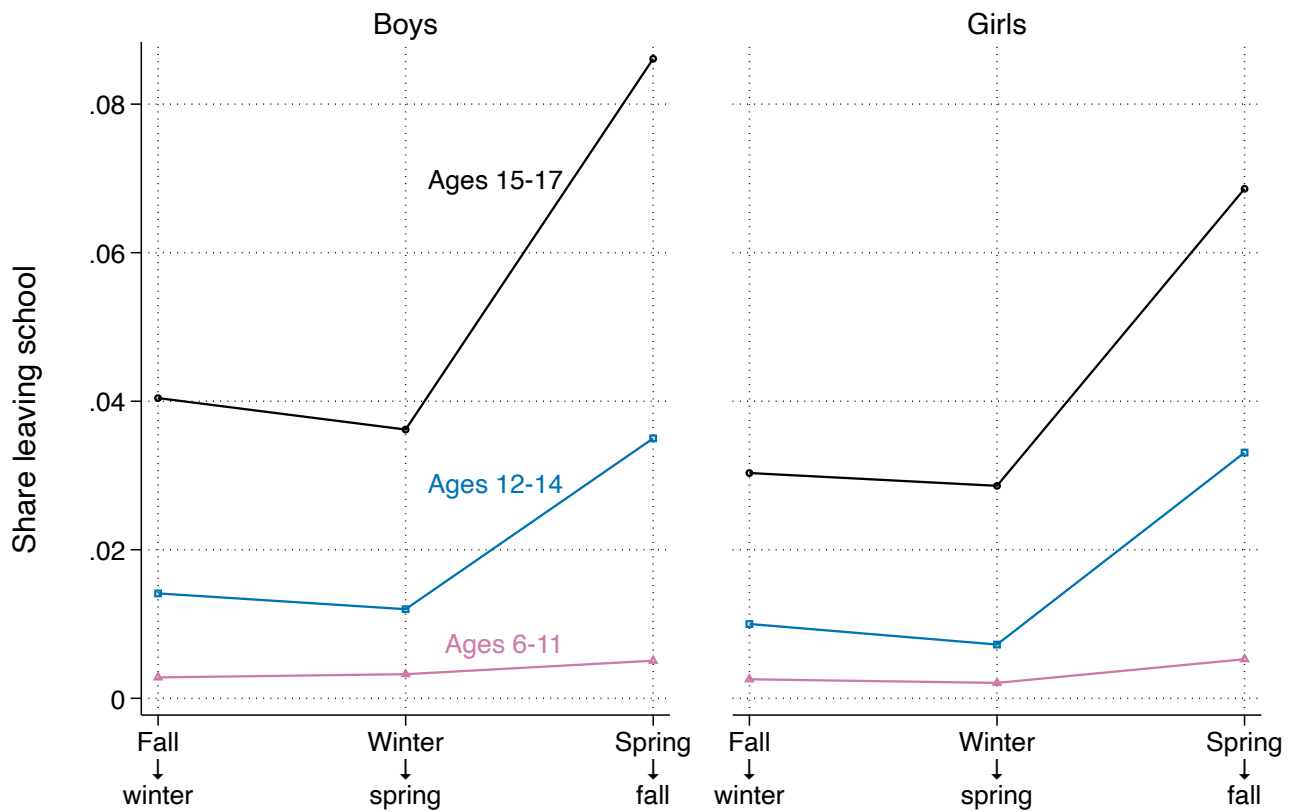
Note: Share of children enrolled in the starting season who were not enrolled in the ending season. Age is measured in the starting season; 17-year-olds who turned 18 are excluded. Sample excludes summers and localities with more than 100,000 residents.

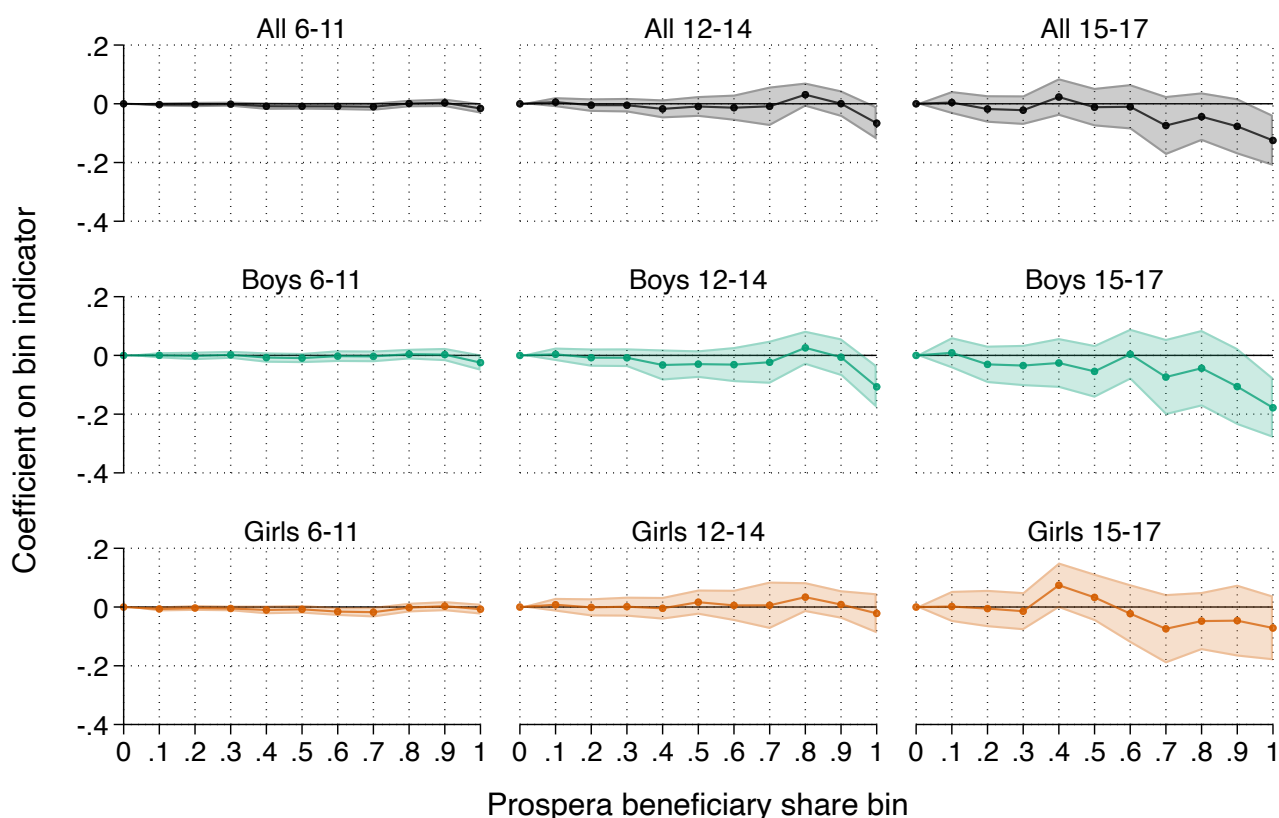Figure A6: Robustness of enrollment effects by age group, both sexes

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 "BBJ interactions" regressions, we include 2019 BBJ benefits per household and its interactions with indicators for the 2018-19 and 2019-20 school years. In the "big cities" regressions, we estimate the baseline model in an expanded sample that includes cities with populations over 100,000.

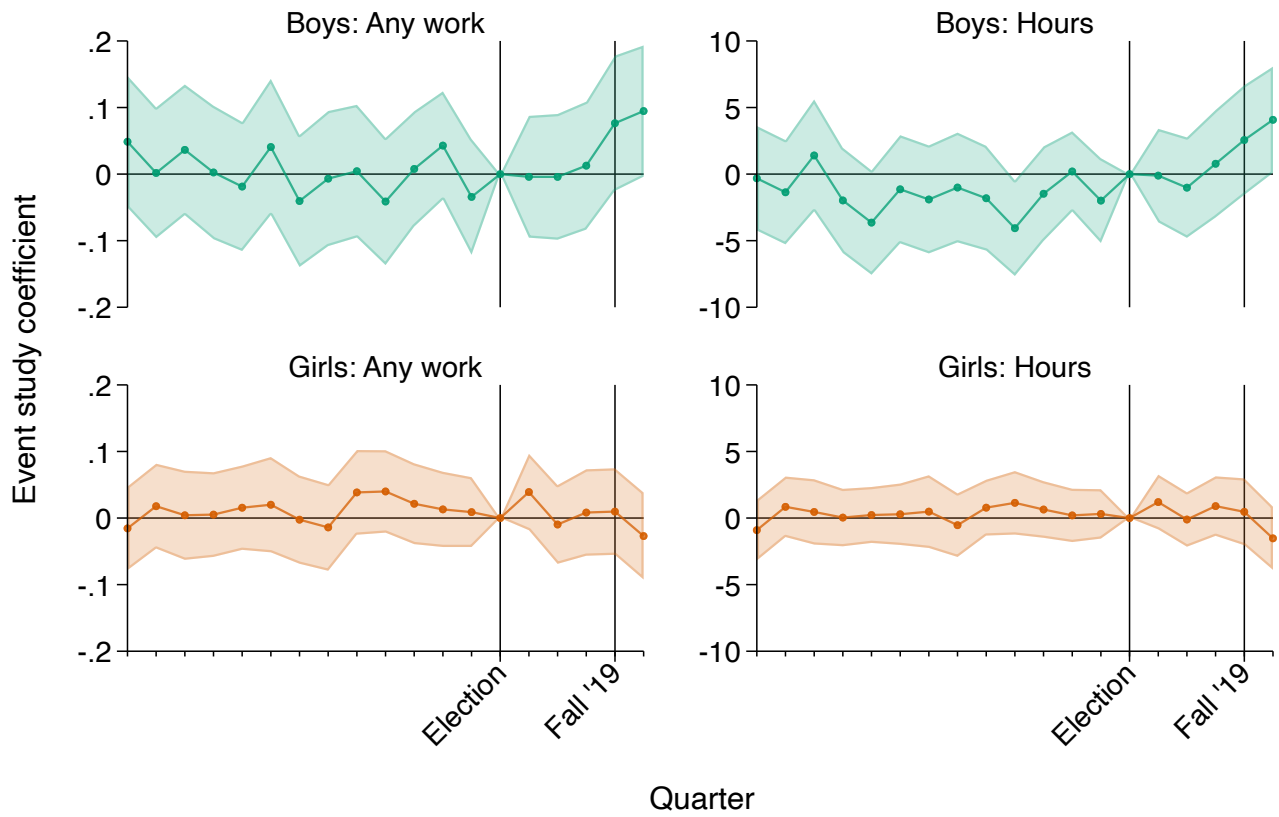Figure A7: Robustness of enrollment effects to omission of individual states
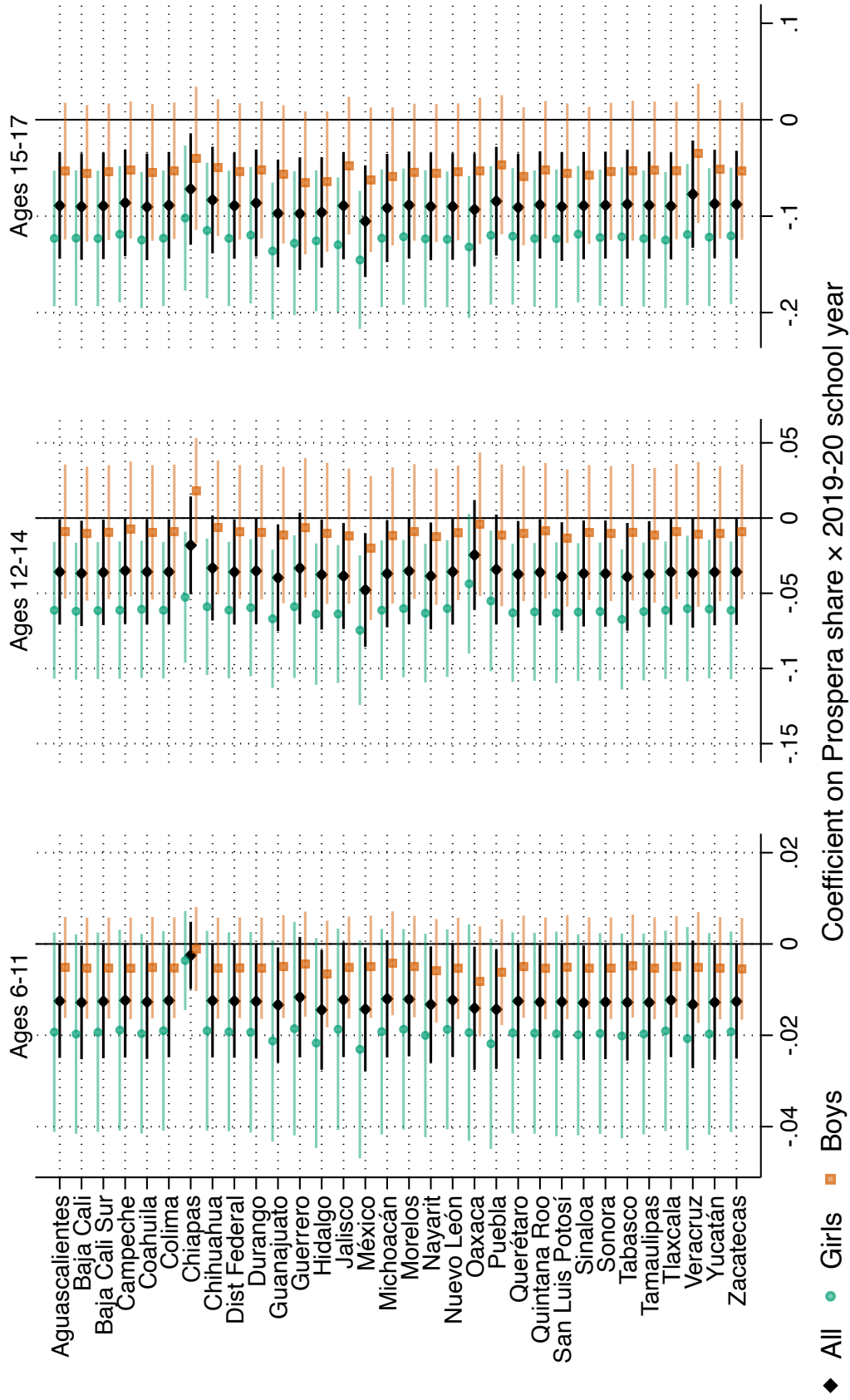
Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. Each row omits the state indicated on the left. All regressions include the the Prospera share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes summers and localities with more than 100,000 residents.

Figure A8: 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.
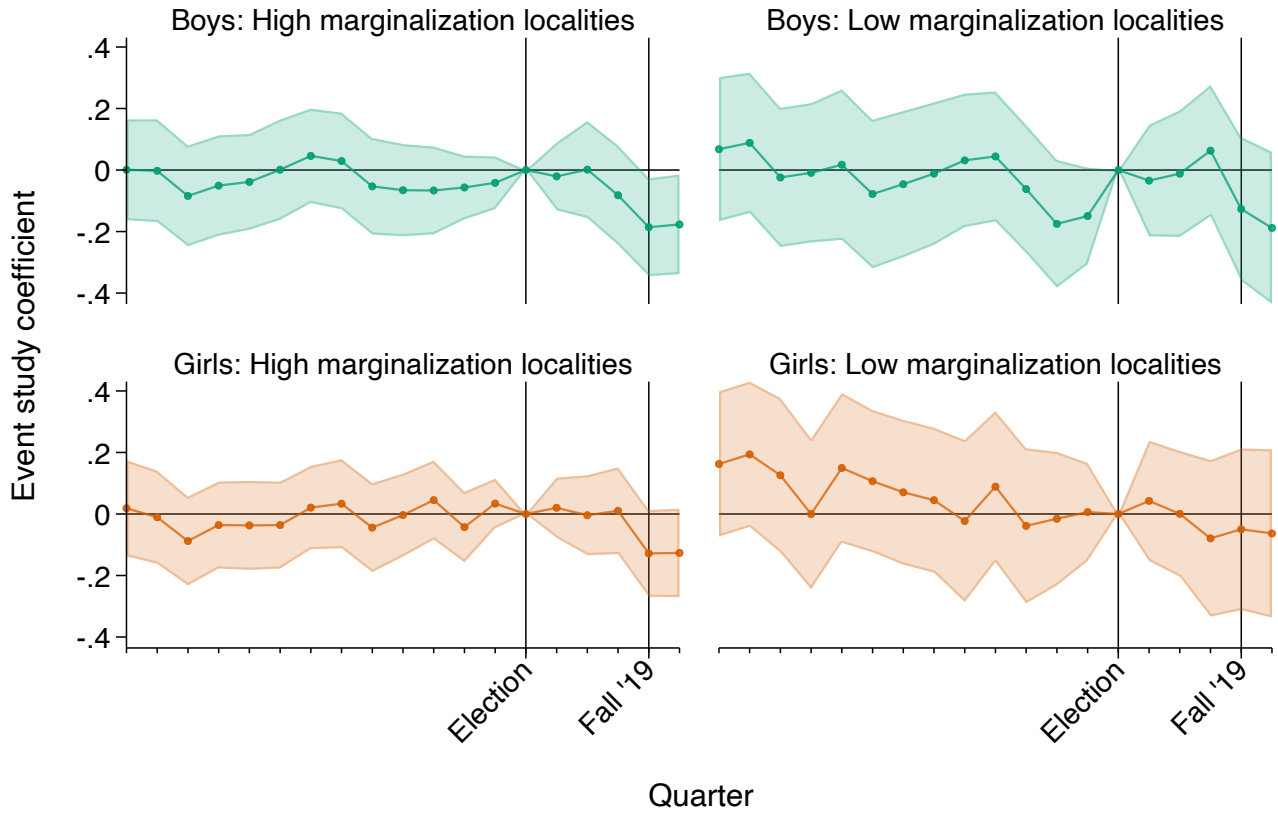
Figure A9: Labor market event study by sex, 15-17 year olds

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.

Figure A10: 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.