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 labor force participation mirrored falling enrollment in boys of high school age. Enrollment effects were at least at 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 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; 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. Are longstanding programs like Progresa—later renamed Oportunidades and then Prospera—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 they 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 enrollment 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 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, enrollment rates declined relative to low-penetration localities, with effects especially pronounced at high school ages (15-17) and among boys.

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*

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

Becas para el Bienestar Benito Juárez. Using administrative municipal level data on recipients of Prospera and Becas Benito Juarez (BBJ) 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 worsened substantially. (Progressivity is measured by the locality level marginalization index, an official measurement of community level poverty indicators developed by the Mexican Population Council) Our analysis suggests that the average Prospera household under the new program received substantially lower transfers post rollback. Further, households in the poorest localities were those receiving 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 average its 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 conditional cash transfers (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 25^{th}, 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 transition to a new scholarship program called *Becas Benito Juárez* (BBJ), operated by the Secretary of Public Education. 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]

The new BBJ program contains two main components. The first component, *Beca de Educación Basica*, is 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

[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.

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.

payments, which depended on the number of children enrolled and their grades. The second component, *Beca Universal de Educación Media Superior*, is a monthly grant also equal to 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. A smaller, third component, *Jovenes Escribiendo el Futuro*, provides transfers linked to enrollment in college. 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. Table 1 compares the transfers received across both programs for households.

Table 2 illustrates these changes by presenting potential transfers under either program to different types of families. Evidently, families with a large number of children in primary (1st through 6th grade) or middle (7th through 9th grade) school would likely receive more resources under Prospera than under BBJ, which provides only a fixed grant for primary/middle school enrollment, independent of the number of children actually enrolled in primary or middle school. However, other types of families would receive more under BBJ.

A final key program difference is in the degree of conditionality. 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. BBJ's conditionality is much looser. Only one child per

Table 2: Transfer received under alternative programs for different family types

Children in HH	4th grader	4th and 5th grader	7th and 8th grader	10th and 11th grader	4th, 10th and 11th grader
Prospera monthly	205	405	1405	1980	2185
Benito Juarez monthly	800	800	800	1600*	2400*

Monthly pesos $US1=20 pesos
* 800 goes to each HS student

family in primary and middle school is required to be enrolled, and attendance is not monitored. At the high school level, BBJ also requires enrollment but does not monitor attendance.

However, these nuanced differences in program rules were 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.

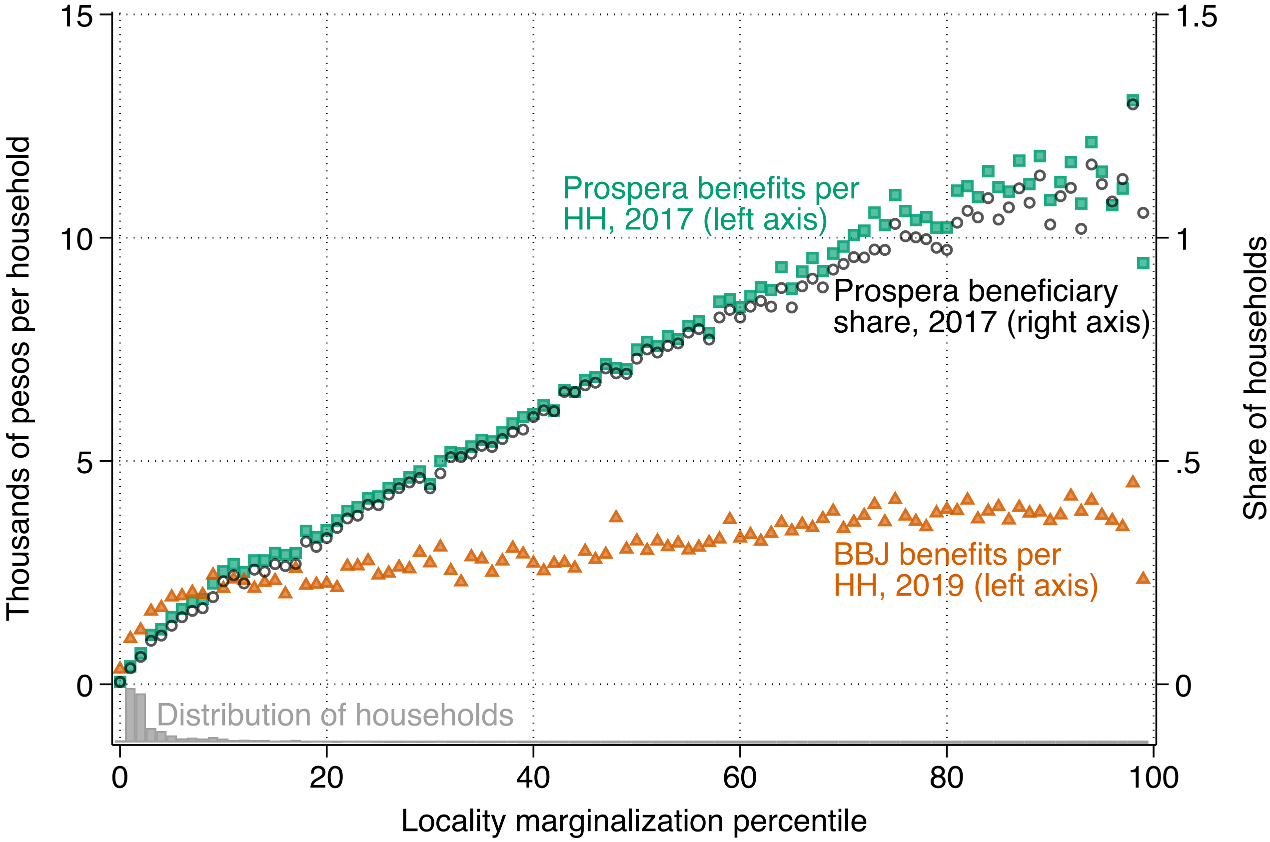
Nevertheless, the geographic distribution of transfers changed substantially. To illustrate this point, Figure 1 plots transfers per household under Prospera and under BBJ by the the government's index of locality marginalization, computed as the first principal component of various census-based measures of community disadvantage. Outside the 10% least marginalized localities, resources per household declined after rollback; 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 programs. The net result is that the substitute program provides much lower resources per household in poor communities. We thus hypothesize significant disruption for Prospera households.

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

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.

children (Rubalcava et al. (2009)).[4]

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 levels. [5] 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

[4] 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.

[5] 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.

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 during the academic school year. [6] 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.[7] Our empirical strategy, described below, compares changes in school enrollment pre- and post-rollback

[6]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.

[7]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.

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:

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

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 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.[8]. 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. Finally, as rollback was not staggered but

[8] 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.

simultaneous, concerns regarding heterogeneous treatment in time are less of a concern.

To shed light on the parallel trend assumption, we also estimate an event study specification:

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

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. 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.

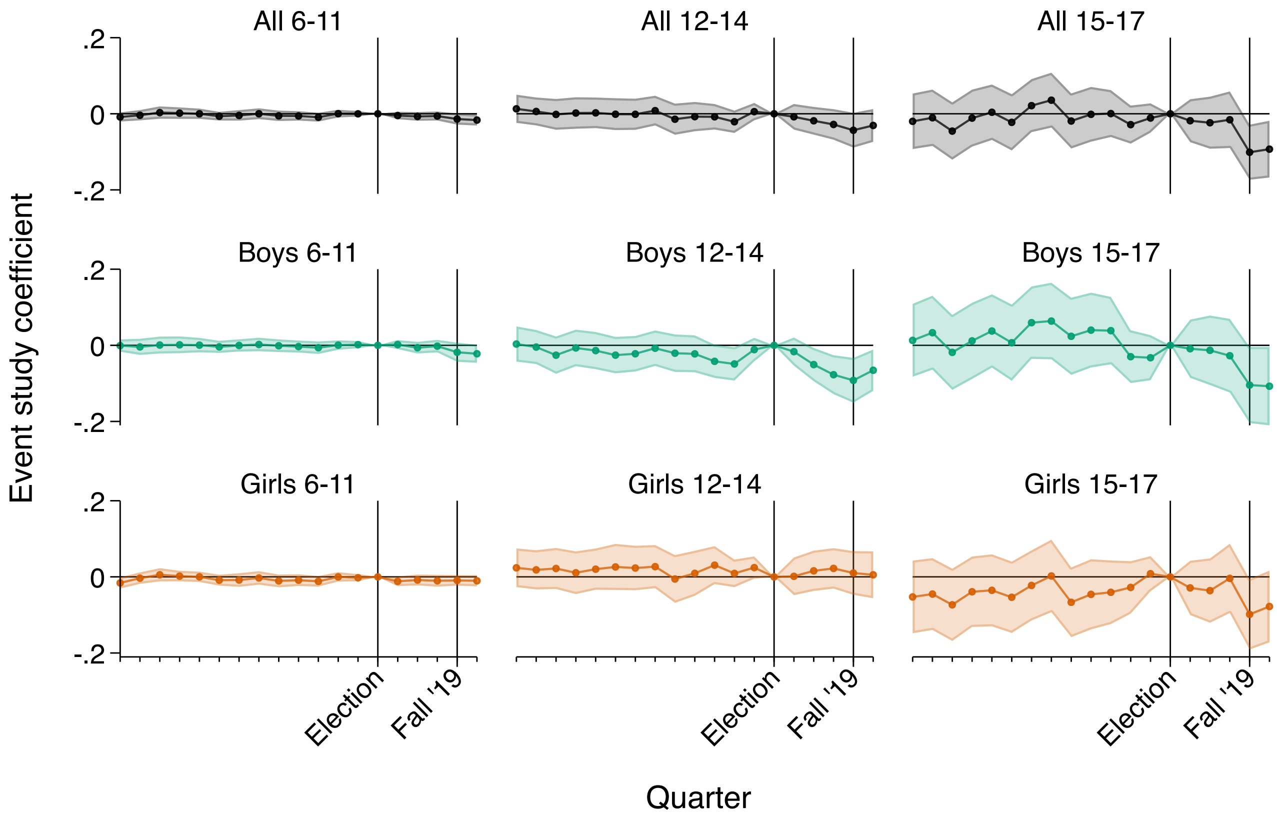
5.1 Enrollment

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

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.

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 3, 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.

The negative effects of rollback are particularly large for the age group 15-17, corresponding to a

Table 3: 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. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

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 3 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, strikingly, 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 concentrated on boys and are particularly large at level of 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 [9] 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) impact of rollback on enrollment of at least 10 percentages points. For girls aged 15-17, where our main specification suggested negative but insignificant impacts of rollback, our alternative specifications in general suggest negative but largely statistically insignificant effects.

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.

[9] Appendix Figure A5 presents a similar graph for boys and girls together.

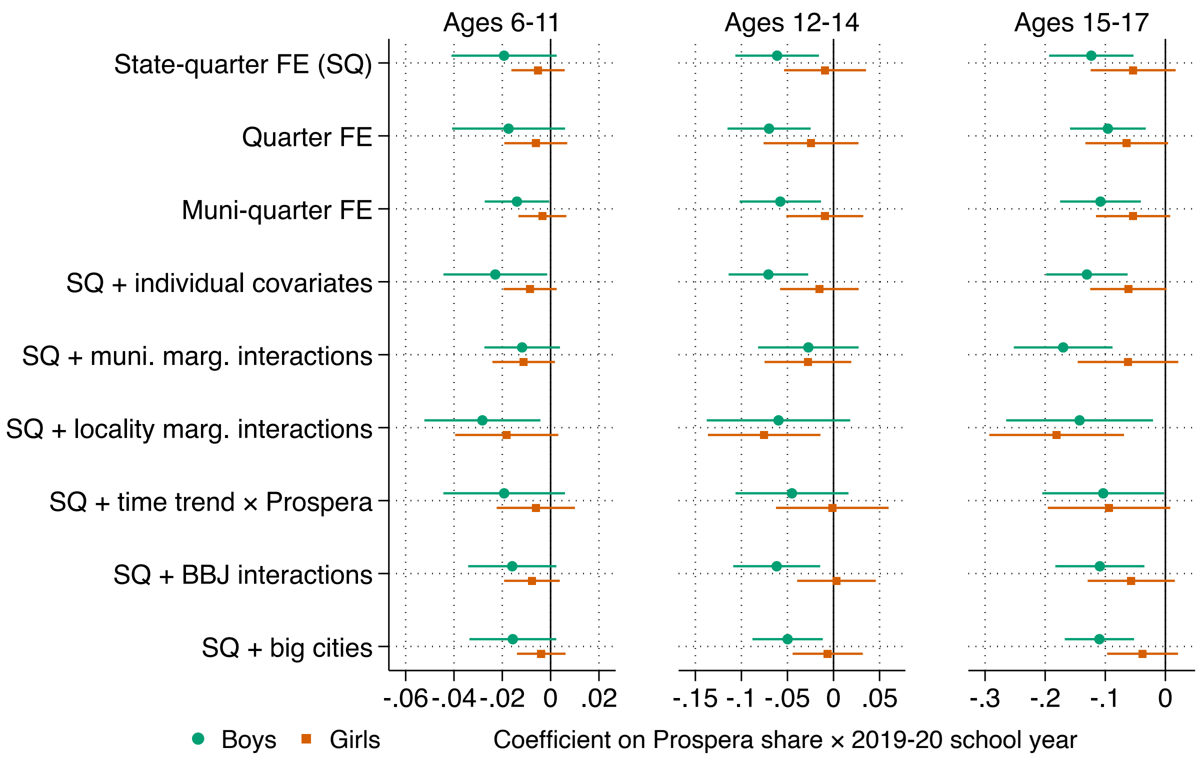
Specification checks for enrollment effects for girls ages 12 to 14 and girls ages 6 to 11 confirm our main specification results, which showed no significant impact of rollback on the enrollment of either group.

Finally, given potential state level differences in trends in school enrollment, Figure A6 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.

5.3 Heterogeneity and labor market effects

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 4 presents enrollment impacts by mother's education level, locality population, and locality marginalization. For boys, impacts of rollback are concentrated for youth whose mother's have lower levels of education. 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 **??** 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 much greater

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.

Table 4: Enrollment effect heterogeneity, ages 15-17

	Mother education level		Locality pop.		Locality marg.	
	$\leq$ primary (1)	$>$ primary (2)	$< 2,500$ (3)	$\geq 2,500$ (4)	High (5)	Low (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. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

than 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%.

As school and work may be substitutes Skoufias and Parker (2001), we now turn to a study of impacts of rollback on labor market participation, focusing on the group ages 15 to 17. Figure A3 in the Appendix presents school and work participation 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

Table 5: 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. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

40% by age 17. For girls, labor market participation rates are about 3% for age 12 and rise to nearly 20% by age 17. [10] Table 5 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 A7 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 reducation 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

[10] 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.

(2004)). These reductions in school enrollment are accompanied for boys by a significant increase in the probability of labor market participation. For girls, our evidence does not suggest significant impacts of rollback on either school enrollment or labor market participation. [11].

[11] 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 enrollment principally for boys. The estimated reductions in school enrollment are quite large, with an implied reduction in school enrollment of 12 percentage point for males age 15 to 17. For girls, we do not find evidence of significant reductions in the probability of school enrollment due to rollback in this first year. Nevertheless, the large effects on males are suggestive of 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.

The gender differences in the effects of rollback are particularly noteworthy. In particular, 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. [12]. However, we also observed reductions at the primary school level in enrollment for males, a school level at which presumably parents are making decisions on school enrollment and attendance for their children.

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.

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

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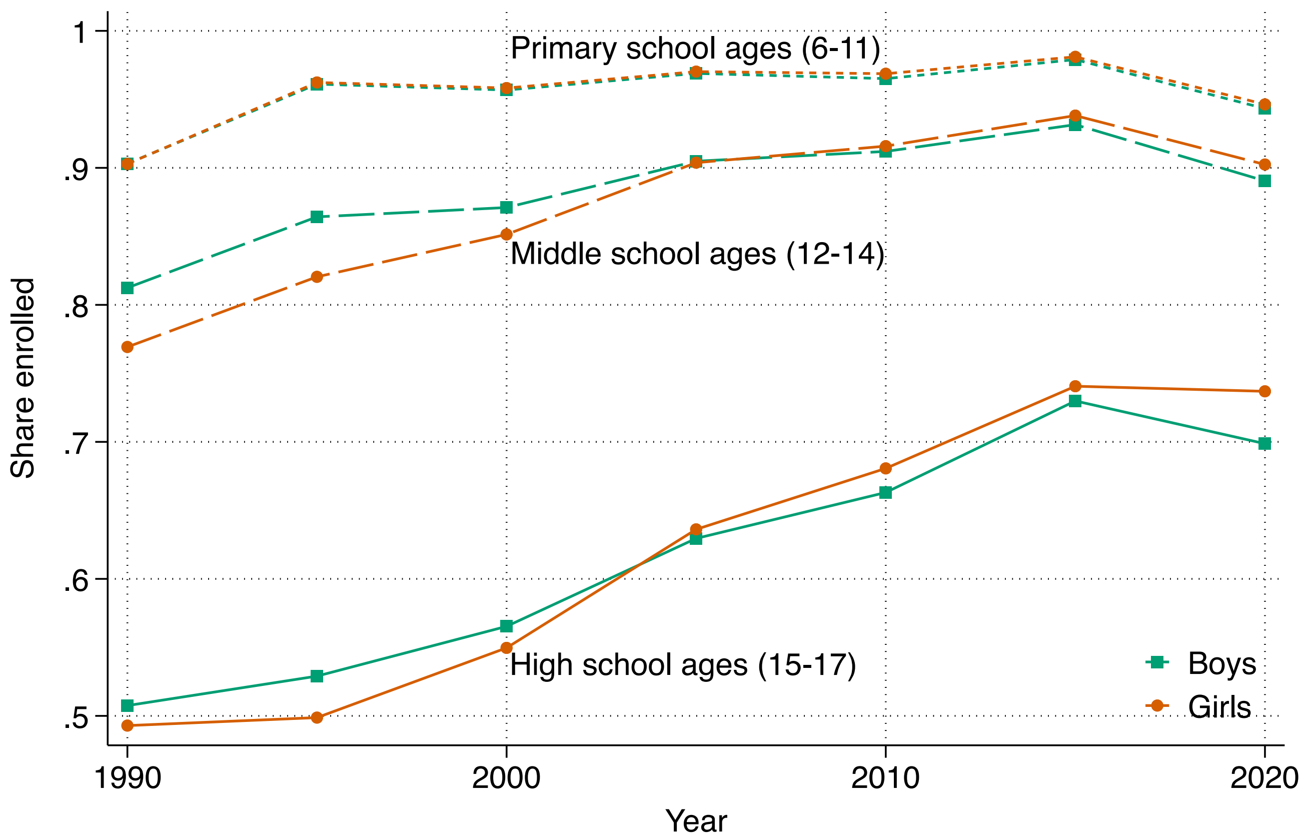
Figure A1: School enrollment over time, census data



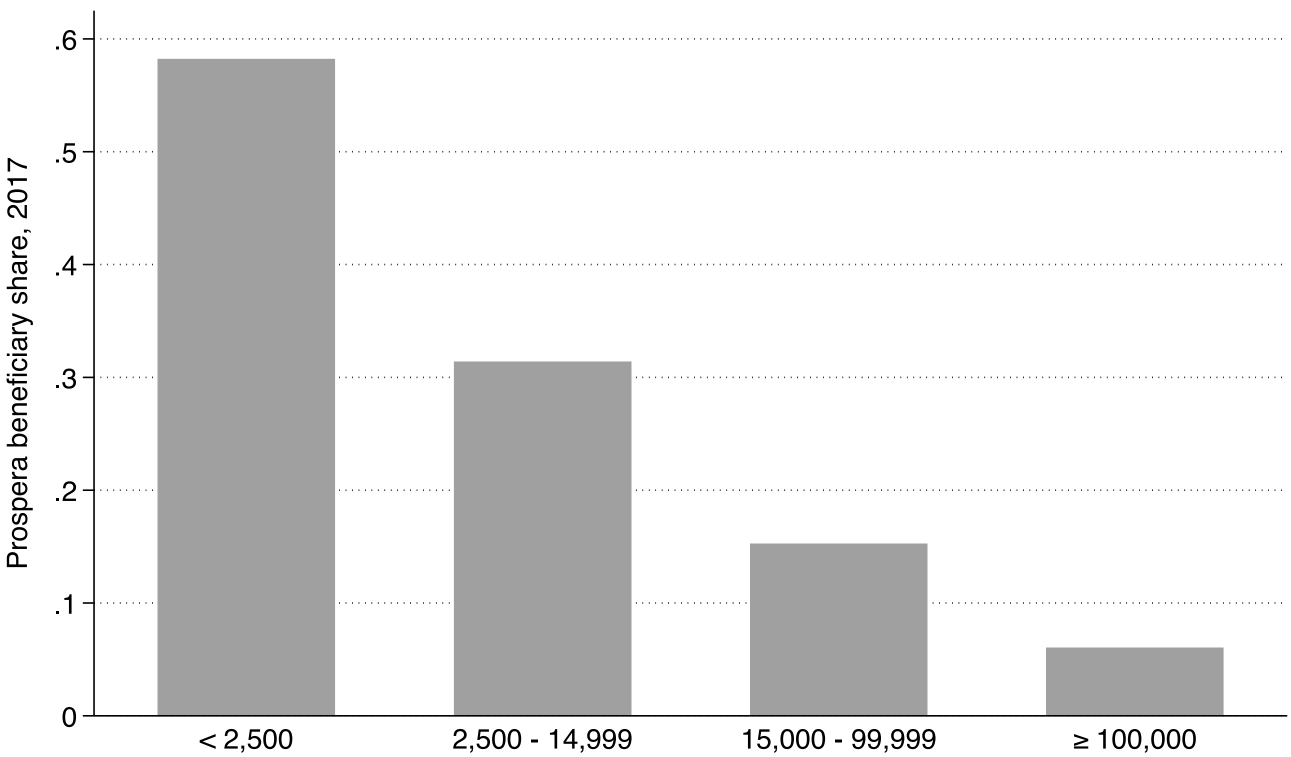
Note: Data are from the 1990, 2000, 2010, and 2020 censuses and the 1995, 2005, and 2015 intercensal surveys. The age ranges for primary, middle, and high school follow a typical student's grade progression in the Mexican system. The 2020 census was collected throughout March, with an official reference 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

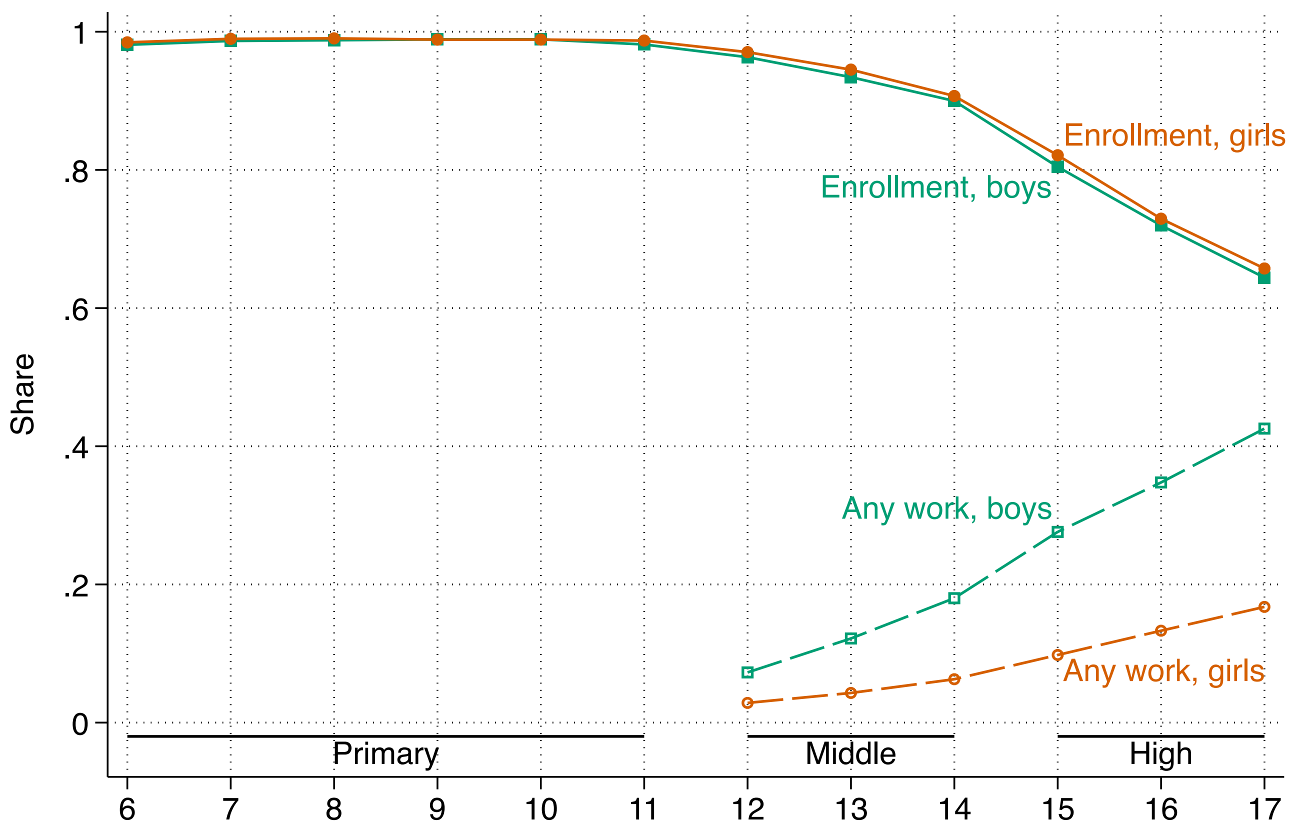
Note: Sample consists of CONAPO localities with more than 100 residents in the 2010 census that could be uniquely matched with Prospera data. Large cities are defined as having more than 100,000 residents in the 2010 census. The ENOE is designed to be representative with and without large cities. The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



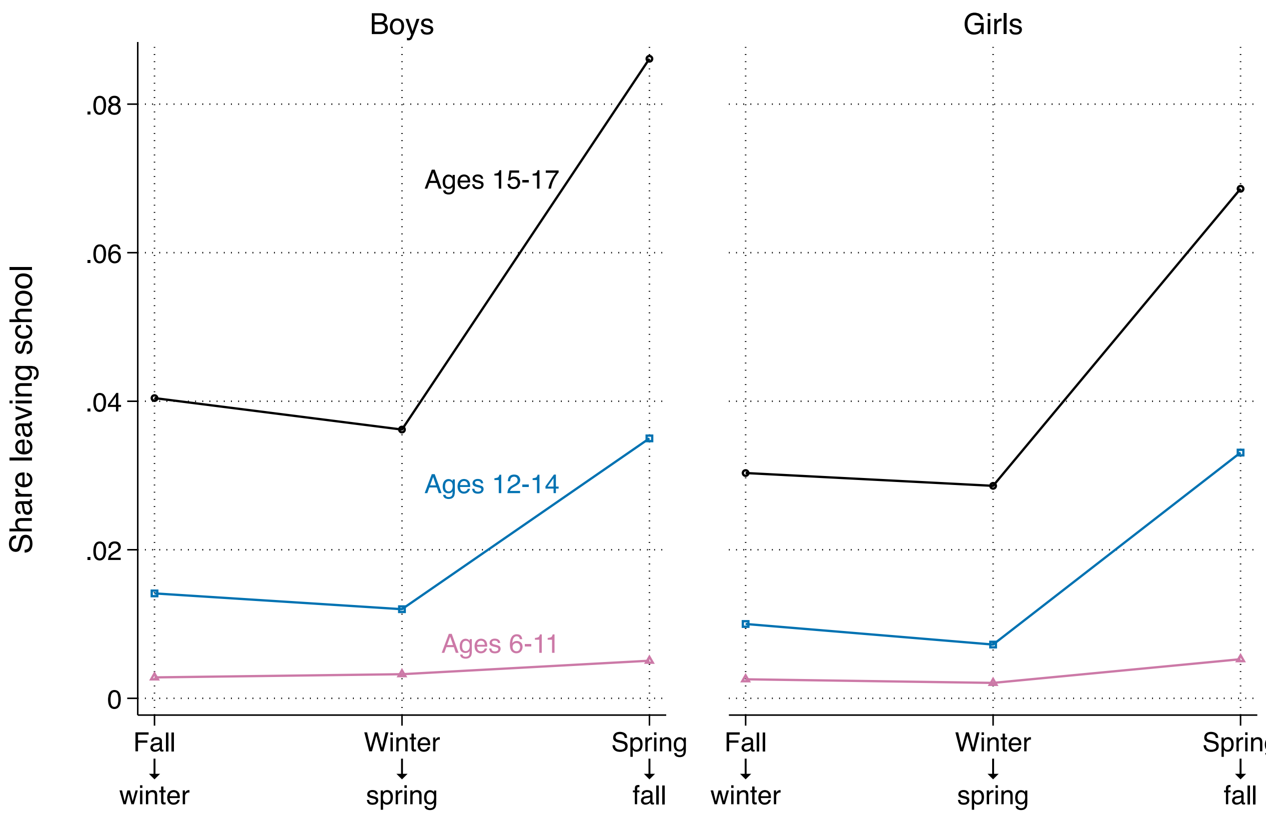
Note: Sample includes localities with at least 100 residents, which contain 98% of the Mexican population. The ENOE is designed to be representative of localities in each of the population categories.

Figure A3: School enrollment and work by age and sex



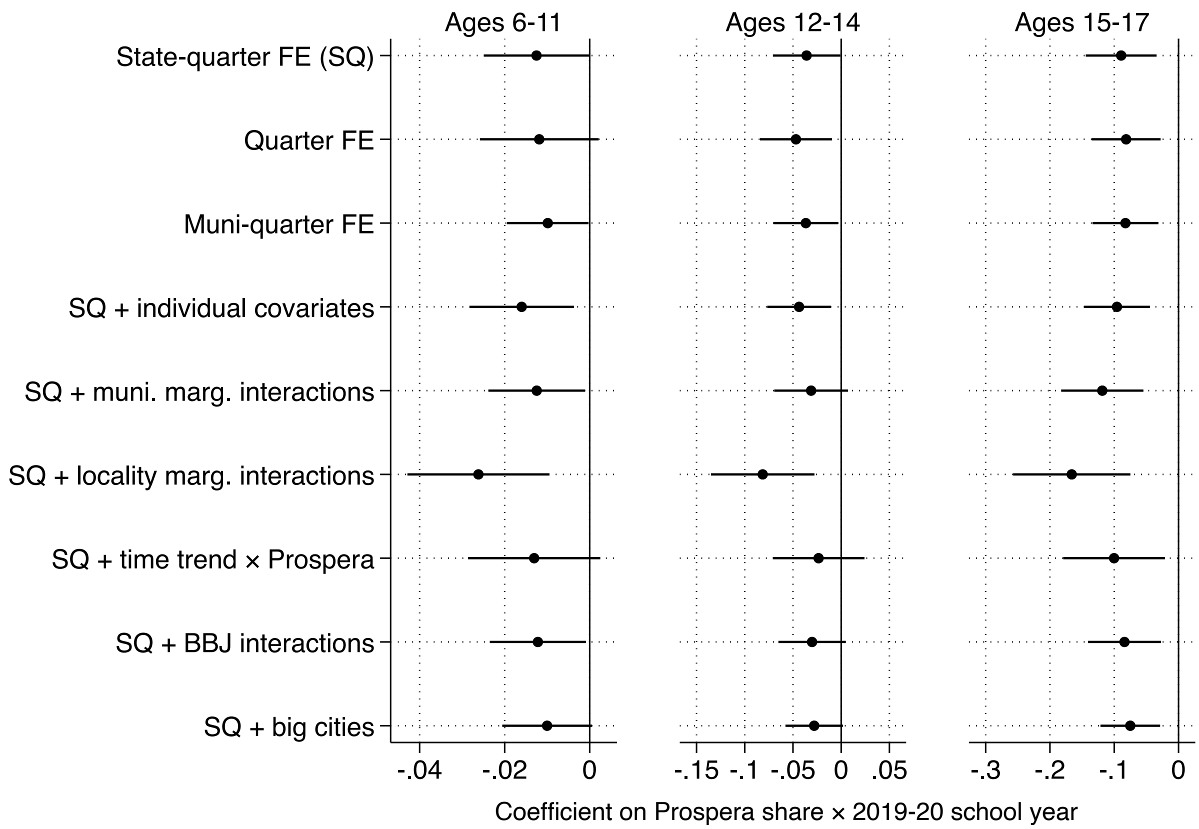
Note: Sample excludes summers and localities with more than 100,000 residents. The ENOE does not ask about labor market outcomes for children under 12. The age ranges for primary, middle, and high school follow a typical student's grade progression in the Mexican system.

Figure A4: School-leaving rates by season



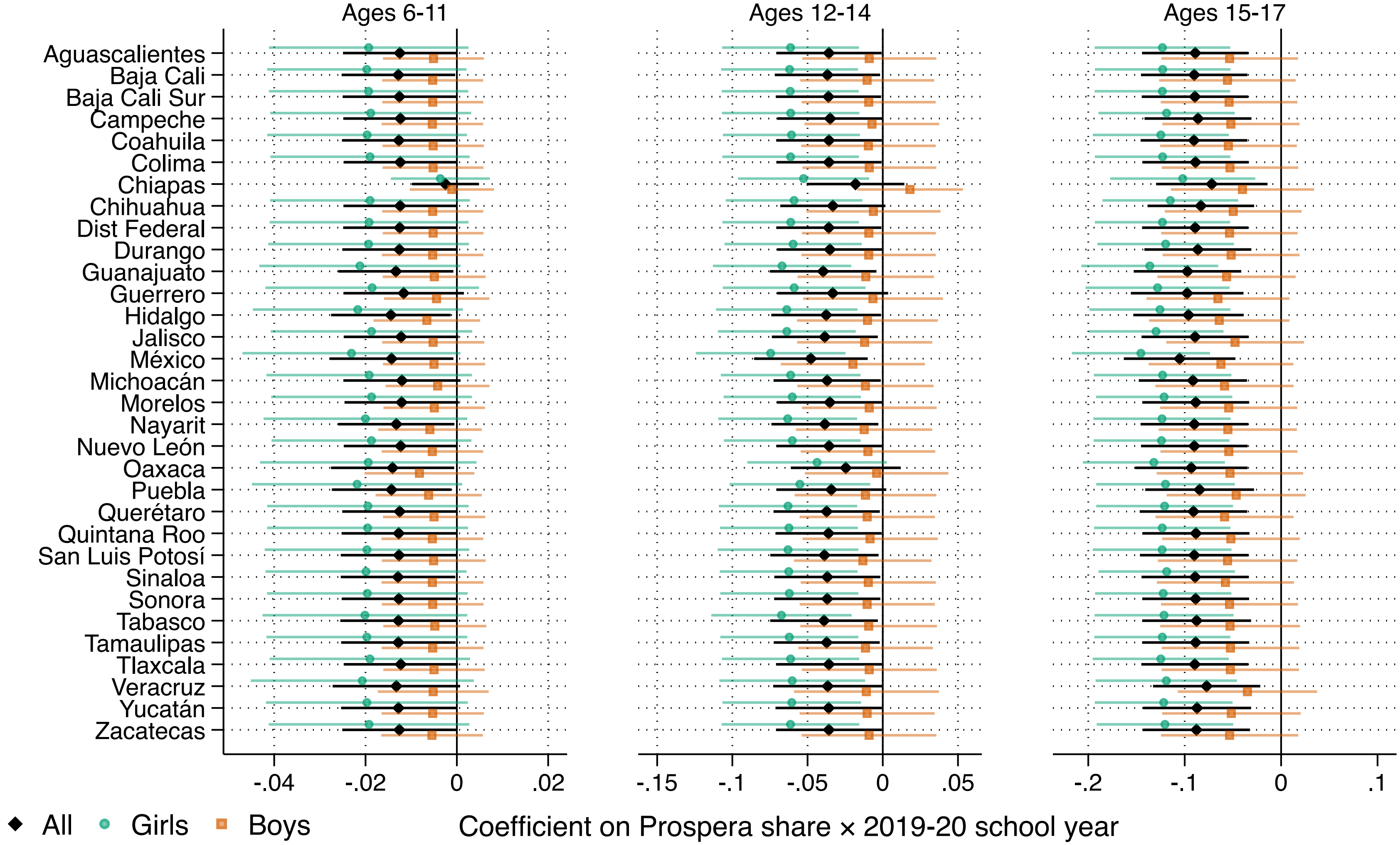
Note: Share of children enrolled in the starting season who were not enrolled in the ending season. Age is measured in the starting season; 17-year-olds who turned 18 are excluded. Sample excludes summers and localities with more than 100,000 residents.

Figure A5: Robustness of enrollment effects by age group, both sexes



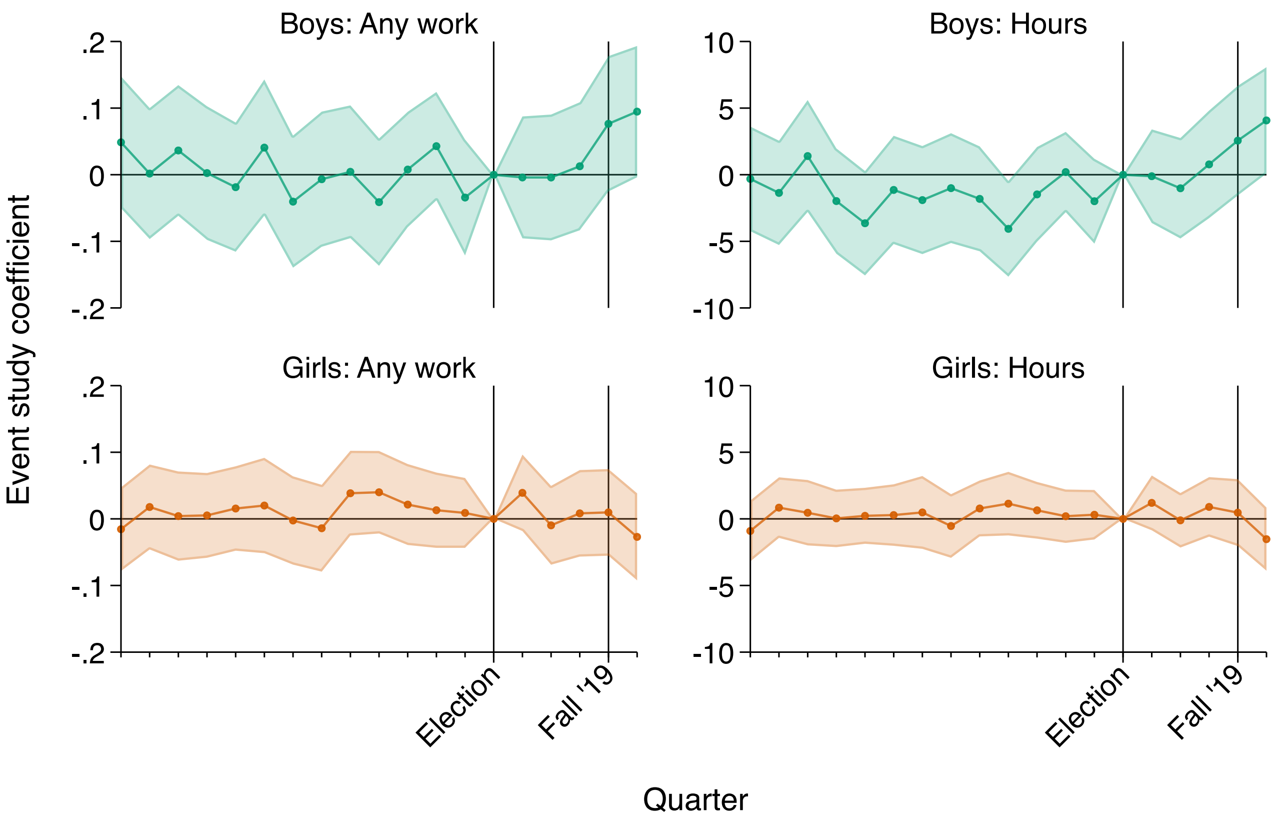
Note: Point estimates and 95% confidence intervals, based on standard errors clustered by locality. All regressions include the the Prospera share, its interaction with an indicator for the 2018-19 school year, and state-by-quarter fixed effects. Sample excludes summers. Individual covariates include 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 A6: 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 A7: 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.