Conditional Cash Transfers, Schooling Decisions, and Export Manufacturing*

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Abstract

Does the effectiveness of an education policy depend on the job opportunities in the local labor market? This paper provides a theoretical and empirical investigation of how schooling decisions respond to conditional cash transfer programs, across areas with different exposure to export manufacturing. We show that Mexico's PROGRESA program, documented to have increased educational attainment, was less effective in areas with more export-oriented manufacturing jobs. Our theoretical model, combined with empirical evidence, suggests this is because these jobs generate more convex opportunity costs of schooling. Consistent with this, the heterogeneity we document is strongest among those old enough to be working in factory jobs. In addition, this heterogeneity is primarily driven by jobs that directly influence schooling opportunity costs: low-wage jobs and jobs for school-aged workers.

Keywords: conditional cash transfers, export manufacturing, Mexico, opportunity costs
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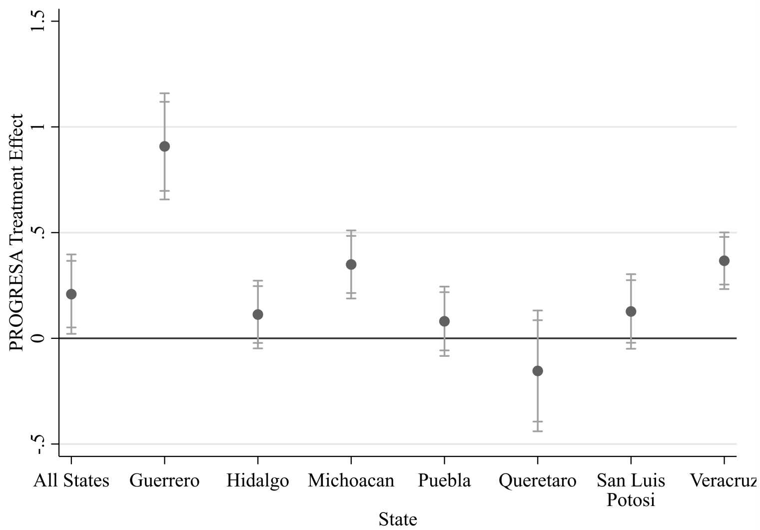
1 Introduction

Reducing the cost of schooling is a common policy lever used to promote human capital investment. The effects of such policies, like private school vouchers and financial aid, vary widely across settings (Epple et al., 2017; Herbaut and Geven, 2020). Conditional cash transfer (CCT) programs, which lower the cost of schooling by conditioning transfers on school attendance, are no exception (Glewwe and Muralidharan, 2016; Millán et al., 2019): Fiszbein and Schady (2009), for example, document impacts on attendance rates ranging from -3 to 31 percentage points.

What drives this heterogeneity in the effectiveness of education policies? Though some variation might be due to differences in program characteristics and their implementation, these are unlikely to be the only explanations. In fact, we find striking geographic heterogeneity in the effectiveness of a single program in a single country: the Programa de Educación, Salud y Alimentación (PROGRESA), which began in Mexico in 1997 and inspired similar CCT programs around the world. As we show in Figure 1, Mexico's PROGRESA program had large, positive, and statistically significant effects in three out of the seven states included in the initial evaluation, but statistically insignificant effects close to zero in the remaining four states.

To better understand potential sources of heterogeneity in education policy effectiveness, we begin with a model of the optimal schooling decision, which suggests that labor market conditions (specifically, the types of job opportunities available) could be important in determining the success of these education policies. Our parameter of interest is the response of optimal schooling to a price reduction, which captures how programs like CCTs affect eventual educational attainment. We show that the magnitude of this response depends on the convexities of the opportunity cost and wage functions, both of which are determined by the types of jobs that are available in a community. Adult jobs can affect perceptions about future returns to schooling, while jobs for school-aged youths can affect the forgone wage component of the opportunity cost function. Across countries, job opportunities have been

Figure 1: PROGRESA Impact on Educational Attainment Across States



Notes: Coefficients (along with 90% and 95% confidence intervals) are obtained from a regression of educational attainment in 2003 on a PROGRESA treatment village indicator, restricting to children aged 5 to 16 in 1997 in eligible (poor) households, controlling for age, gender, household size, household head age, household head gender, mother's and father's education, and dummies for mother's and father's indigenous language knowledge. State-specific coefficients are obtained using separate regressions for each state. Standard errors are clustered at the village level. Regressions reported in Appendix Table A1.

substantially affected by globalization and trade liberalization (Autor et al., 2016; Nallari et al., 2011), raising the question of what these changes might mean for education policy.

Our empirical investigation focuses on Mexico's PROGRESA program as our education policy of interest. PROGRESA was first implemented during the later part of a period of rapid trade liberalization, which brought about a large increase in export-oriented manufacturing jobs.[1] This allows us to investigate how PROGRESA's effectiveness was influenced by export manufacturing, thereby providing insight into the interaction of two common development policies: CCT programs and export promotion. Atkin (2016) shows that export manufacturing reduced schooling levels in this setting, which implies that export manufacturing increased marginal opportunity costs by more than it increased the returns to schooling. However, because this finding does not inform us about the convexities of the wage and cost functions, it remains unclear whether the expansion of export manufacturing would reduce

[1]Throughout the paper we use 'export-oriented manufacturing jobs' and 'export jobs' interchangeably.

or enhance the schooling impact of CCTs.

Taking advantage of the randomized rollout of the PROGRESA program across villages, we find that the impact of PROGRESA on attendance rates and eventual educational attainment was smaller in regions with more export jobs. This result comes from combining PROGRESA data with data from the Mexican Social Security Institute (IMSS), from which we calculate the number of export-oriented manufacturing jobs (for men and women separately) over time and across regions (subdelegations). We regress our educational outcomes on a PROGRESA treatment dummy and an interaction between PROGRESA treatment and subdelegation-level gender-specific export jobs (controlling for subdelegation fixed effects and a rich set of demographic controls), and estimate a negative and significant interaction coefficient. An important implication of this result is that, although CCTs have been proposed as a way to mitigate the negative education effects of export jobs (in Atkin (2016), for example), this may not be very efficient in practice since CCTs are least effective in precisely the areas with more of these jobs.

To understand why export manufacturing jobs are associated with a smaller PROGRESA impact, we explore data on wages and opportunity costs in areas with higher versus lower concentrations of export jobs. This descriptive analysis suggests that opportunity costs are more convex in areas with more export manufacturing jobs, perhaps because the wages at these export jobs increase faster with schooling than in other jobs (e.g., agriculture). In conjunction with the model, this helps explain why PROGRESA was less effective in these areas.

Investigating what types of individuals and what types of jobs are driving the heterogeneity we have documented, we find further support for the opportunity cost channel, which appears to dominate over the wage function channel. First, we find that heterogeneity is stronger for those who are old enough to be working in factory jobs (at least 15 years old). In addition, the heterogeneity is driven primarily by the types of export jobs likely to factor into the opportunity cost of schooling: low-wage jobs and those held by young workers.

These findings suggest export-oriented manufacturing jobs reduce the PROGRESA impact because they translate into more rapidly increasing forgone wages for children who are (or whose parents are) deciding on the optimal level of schooling.

We explore and rule out alternative explanations for the heterogeneity we document. We show that our interaction coefficient is not simply picking up gender differences in the PROGRESA treatment effect. We also show that the heterogeneity is not driven by correlations between export jobs and other characteristics, like subdelegation-level educational attainment, urban shares, or average income; a child's baseline educational attainment; or household-level migration, income, or occupation types. Our results are also robust to the use of an alternate export manufacturing variable: predicted export job growth generated using a shift-share strategy.

These findings speak to a broader empirical literature showing how schooling levels are influenced by opportunity costs and (perceived) returns to schooling (Cascio and Narayan, 2022; Jensen, 2010, 2012; Shah and Steinberg, 2019). Our work is particularly related to the set of studies documenting how trade-related changes to the labor market can influence schooling decisions by affecting returns and costs (Atkin, 2016; Blanchard and Olney, 2017; Edmonds et al., 2009; Greenland and Lopresti, 2016). Unlike these studies, our focus is not on schooling *levels*, but schooling *responses* to a price reduction, a policy-relevant parameter of interest that captures the effectiveness of a policy at increasing educational attainment. Importantly, knowing how schooling levels are affected by a particular shock or labor market characteristic is not enough to predict how that characteristic will affect the schooling response to a price reduction (i.e., the schooling impact of a program like PROGRESA).

This study also contributes to the literature on Mexico's rapid trade liberalization in the late 1900s. A number of studies have investigated how it affected employment, wages, schooling levels, and inequality across genders and skill levels (Aguayo-Tellez et al., 2013; Atkin, 2016; Hanson and Harrison, 1999; Juhn et al., 2014; Revenga, 1997). We expand on this work by documenting how the resulting changes in the labor market influenced the effectiveness of the PROGRESA program.

Finally, this study expands our understanding of the interactions between different development policies. Economic development is a multifaceted phenomenon, which often requires the simultaneous pursuit of a variety of different goals. Increasing educational attainment is one goal often prioritized by governments and international organizations (United Nations, 2016). The creation of a strong manufacturing sector, and in particular one that is export-oriented, is another goal that has featured prominently in the development path of many nations (Lederman et al., 2010; Lustig, 2001; Page, 1994). Both targets play an important role in government policy, but little is known about how the pursuit of one goal affects progress towards the other.

2 Theoretical Framework

We begin by outlining a simple theoretical framework that sheds light on how labor market conditions can influence the schooling impact of policies that reduce the price of education. Suppose parents maximize discounted future wages minus the cost of schooling:

$$\beta W(S) - c(S) - pS, \tag{1}$$

where wages are a function of schooling ($W(S)$), opportunity costs are forgone wages $c(S)$, and the price of one year of school is p. The optimal level of schooling is determined by the expression

$$\beta \frac{\partial W}{\partial S} = \frac{\partial c}{\partial S} + p.$$

Labor market conditions – specifically, the types of jobs that are available to an individual – affect this expression in two ways. First, jobs can affect perceptions about the future returns to schooling ($\frac{\partial W}{\partial S}$).[2] In addition, certain jobs, which are available to school-aged youth, can affect the marginal opportunity cost of schooling ($\frac{\partial c}{\partial S}$).

In this paper, more than the optimal level of schooling, we are interested in the response of optimal schooling to a decrease in p, which is given by

$$-\frac{dS}{dp} = \left(\frac{\partial^2 c}{\partial S^2} - \beta\frac{\partial^2 W}{\partial S^2}\right)^{-1}. \tag{2}$$

Assuming that the term inside the brackets is positive (i.e. that the second order condition for a maximum holds), this predicts what has been documented empirically – reducing the price of schooling typically increases educational attainment.

More importantly, however, this expression shows that the magnitude of the impact of a price reduction depends on the second derivatives of the opportunity cost and wage functions. In particular, in labor markets with more convex opportunity costs (larger $\frac{\partial^2 c}{\partial S^2}$), the magnitude of the response will be smaller. In labor markets with smaller $\frac{\partial^2 W}{\partial S^2}$ (that is, marginal benefits that are either increasing slower or decreasing faster), the schooling response will also be smaller.

This expression can also be interpreted in terms of the gap between benefits and forgone wages ($W(S) - c(S)$), or net benefits. In areas where net benefits decrease faster with schooling (i.e., where the marginal net benefits are more negative), the schooling response to a price reduction will be smaller.

Notably, these predictions hinge on the second derivatives rather than the first derivatives (the marginal opportunity cost, $\frac{\partial c}{\partial S}$, or the return to schooling, $\frac{\partial W}{\partial S}$), though these could also matter – in ambiguous ways – due to their role in determining the optimal level of schooling, which in turn influences the magnitude of the expression in (2). This means that knowing

[2] The wage function depends on the jobs and income that will be available when these youths eventually enter the labor market, which could be informed by the conditions in the labor market at the time of the decision. For example, 70% of survey respondents in the Dominican Republic report that people in their community were their primary source of information about expected income (Jensen, 2010). In Madagascar, Nguyen (2008) finds that expectations about future returns to schooling are influenced by information about current labor market conditions.

how a particular industry or occupation composition affects the optimal level of schooling (determined by equation (1)) does not allow us to predict how it will affect the schooling response to a price reduction (equation (2)). The importance of the second derivatives makes it difficult to predict which types of labor markets will enhance or reduce the schooling impact of these types of education policies.

We now turn to an empirical analysis of this question, focusing on a specific education policy: Mexico's CCT program, PROGRESA, which we describe in the next section. Other influential papers that have modeled schooling decisions in the context of PROGRESA allow for geographic variation in child wages but not in the returns to schooling (Attanasio et al., 2012; Todd and Wolpin, 2006), whereas we argue that geographic variation in either of the relevant functions could generate heterogeneous program effects across labor markets. In addition, unlike these papers – which focus primarily on the average effect of PROGRESA (as opposed to variation in the effect size based on labor market conditions) – we highlight the importance of the convexity of the opportunity cost or wage functions as potential drivers of heterogeneity.

3 Background

3.1 PROGRESA

CCTs are now widely used across the globe (World Bank Group, 2017), but one of the first CCT programs, PROGRESA, began in Mexico in 1997. The program provided cash transfers to poor families that satisfied certain education and health-related requirements.

The education component of PROGRESA, which is the focus of this paper, consisted of cash payments made to mothers whose children had school attendance rates of at least 85%. When the program first started, it covered children in third to ninth grade, but this was expanded to include high school students starting in 2001. Grant amounts increased with grade level, with higher amounts for girls than boys, and ranged from 105-660 pesos per month in 2003.[3] Since its inception, PROGRESA was expanded and renamed several times. It changed its name to Oportunidades in 2002 and was further restructured and renamed Prospera in 2015 (Ordóñez-Barba and Silva-Hernández, 2019). In 2019, Prospera was discontinued and replaced by the Benito Juárez scholarship program for education, providing grants to enrolled students and eliminating the health and nutrition components of the program (Diario Oficial de la Federación, 2019).

PROGRESA was implemented experimentally in 506 rural villages in seven states: Guerrero, Hidalgo, Michoacán, Puebla, Queretaro, San Luis Potosí and Veracruz. Villages were randomized into either treatment or control: the treatment group (320 villages) started receiving benefits in the spring of 1998 and the control group (186 villages) did not receive benefits until the end of 1999.

The randomized variation has allowed for rigorous evaluations of the program's effects on a wide range of outcomes, summarized in Parker et al. (2017). The most relevant findings for our study are those related to educational outcomes. Short-run evaluations of the program compare treatment and control villages in 1998 and 1999 (when PROGRESA had not yet been rolled out to the control group), and find PROGRESA increased school attendance, enrollment, and grade progression, and reduced dropout (Behrman et al., 2005; Schultz, 2004; Skoufias and Parker, 2001). Medium-run evaluations compare educational attainment in treatment and control villages in a 2003 follow-up survey, and show higher educational attainment and grade progression in treatment villages (Behrman et al., 2009b, 2011). Because the control group was already exposed to PROGRESA by this time, these estimates capture the effect of being exposed to PROGRESA 18 months earlier. More recently, researchers have sought to explore the program's long-run effects on education and labor market outcomes using data collected 10-20 years after the program first started (Araujo and Macours, 2021; Parker and Vogl, 2018).

Previous work has examined heterogeneity in the effect of PROGRESA (and other CCTs)

[3]See Skoufias and Parker (2001), Skoufias (2005), Behrman et al. (2009a), and Behrman et al. (2011) for more program details.

across a number of other dimensions – child gender (Lee and Shaikh, 2014; Manley et al., 2013), early-life circumstances (Adhvaryu et al., 2018), household and village poverty levels (Dammert, 2009; Galiani and McEwan, 2013; Maluccio and Flores, 2005), and other household characteristics (Angelucci et al., 2010; Djebbari and Smith, 2008; Handa et al., 2010). The focus of our study, however, is on heterogeneity driven by labor market conditions.

Figure 1 documented substantial heterogeneity across states in the education effects of PROGRESA.[4] Interestingly, this variation in PROGRESA treatment effects appears to be related to certain state-level labor market characteristics. For example, as shown in Appendix Figure 1, PROGRESA treatment effects have a strong negative correlation with state-level manufacturing shares (-0.87) and blue-collar shares (-0.67); correlations with other occupation (white-collar) shares and industry (agricultural, wholesale and retail trade) shares are weaker. Motivated in part by these descriptive statistics, this paper will focus on the influence of export manufacturing – a specific type of blue-collar, manufacturing job – on PROGRESA's education effects.

3.2 Export Manufacturing

The beginning of the PROGRESA program coincided with a period of rapid trade liberalization in Mexico. After pursuing an import substitution strategy for decades, Mexico sharply reversed course by joining the General Agreement on Trade and Tariffs in 1986, followed by the North American Free Trade Agreement (NAFTA) in 1994. The manufacturing sector in Mexico was considered to be the key driver of economic growth and industrial development since the 1980s (Cámara de Diputados, 2004), and these free trade agreements were part of a deliberate strategy to improve Mexico's economy using the manufacturing industry (Moreno-Brid, 2007).

As a result of this shift in policy, Mexico saw a large increase in manufacturing jobs

[4]Table A2, which reports treatment-control differences in individual and household characteristics by state, reveal a few imbalances in each state, though no consistent pattern of differences across states that had significant PROGRESA effects and states that did not. Note that we control for a full set of individual and household level controls in Figure 1.

at factories producing goods for export. From 1986 to 2000, the number of formal sector jobs in export manufacturing sectors more than tripled, from less than 900,000 to over 2.7 million (Atkin, 2016). Notably, employment growth was concentrated primarily in the manufacturing industry: agricultural employment declined substantially in the decade following NAFTA, which meanwhile had little effect on employment in the services sector (Polaski, 2003).

The expansion of export manufacturing certainly affected opportunity costs for school-aged youths. Using the IMSS data, we estimate that the monthly wage of a factory worker under the age of 20, in our PROGRESA subdelegations of interest, was approximately 2,200 pesos per month in 2003, about three times as large as the monthly PROGRESA education transfer for the oldest beneficiaries.[5]

Atkin (2016) shows the expansion of export-oriented jobs increased the marginal cost of schooling more than the marginal benefit, subsequently reducing average schooling levels. In the context of the model in section 2, this finding informs us about the expression in equation (1), which determines the optimal level of schooling, but does not allow us to predict whether export jobs will increase or decrease the schooling impact of CCTs, captured by equation (2). The simulations described in section B help illustrate this point. We provide one example of a cost function that reduces optimal schooling (as export jobs have done) while increasing the schooling response to a CCT, and another cost function that reduces optimal schooling while reducing the schooling response.

An important advantage of focusing on export-oriented manufacturing jobs is that they tend to be driven in large part by external demand, not just by local demand and supply. Perhaps because of this, shares of export manufacturing jobs in our setting are not strongly

[5]To estimate the potential wages of PROGRESA beneficiaries, we take the monthly IMSS data from 2003 and restrict our analysis to employees below 20 years of age. As described in Section 4, salaries are reported as multiples of the minimum wage. The average salary for our sample of interest is 2.6 times the minimum wage, which was set at 40 daily pesos in 2003 in the subdelegations of interest. Assuming employees in the manufacturing industry work for 22 days a month, the average monthly wage equals approximately 2,200 pesos. We compare this to the PROGRESA monthly transfers for the oldest beneficiaries, which amount to 660 pesos (Behrman et al., 2011).

correlated with other socioeconomic characteristics. For example, the correlation between subdelegation-level export manufacturing jobs and average income in our sample is 0.09, while the correlation between overall manufacturing shares and average income is 0.45. For education, these correlations are 0.06 for export manufacturing and 0.47 for overall manufacturing. In addition, in Atkin (2016), which uses an instrumental variables strategy to account for the potential endogeneity of export manufacturing job growth, naive OLS and IV estimates do not differ substantially. Although he uses year-to-year changes in export jobs (while our focus is on the total stock of jobs), this provides some evidence that many determinants of export manufacturing jobs are arguably exogenous.

4 Data

We merge the data collected for the evaluation of the PROGRESA program with employment data from the IMSS, both of which we describe below. In addition to these two sources, we use Mexican census data collected by the National Institute of Statistics, Geography, and Informatics (INEGI) and provided by IPUMS (Minnesota Population Center, 2015).

4.1 PROGRESA Data

The data collected for the evaluation of the PROGRESA program include a baseline survey of all households in PROGRESA villages in October 1997 and three years of follow-up surveys every six months, from 1998 to 2000. A new follow-up survey was carried out in 2003 in all 506 villages that were part of the original evaluation sample. These surveys collected detailed information on household composition and demographics, education, health, employment status, and income. In our analysis, we use the 1997 baseline survey, three surveys that took place in 1998-1999 before the control group received PROGRESA, and the 2003 follow-up.

We define a treatment dummy that is equal to one for households in one of the 320 villages placed in the treatment group, and value zero for the 186 villages in the control group. In our main analysis, we use two education outcome variables. The first is years of educational attainment, measured using the 2003 wave of PROGRESA. The second is an indicator for school attendance, which we measure in each of the 1998-1999 waves. As control variables, we use individual information on age and gender. Our main sample consists of all children aged 6-15 in the original survey (October 1997), living in households eligible for PROGRESA and with non-missing educational attainment in 2003.[6] This consists of over 23,000 individuals, belonging to over 8,000 households in 506 villages.

In Table 1 we report summary statistics of individual and household characteristics in our sample of interest, both pooled and separately by treatment arm, using data from the first available wave (which is the baseline survey in most cases). At baseline, treated individuals are comparable (in terms of age, gender, school attendance, years of schooling, household composition, and parental characteristics) to those in the control group.[7] The PROGRESA survey also collects information on the employment status, labor market income, and migration status of other household members, which we use in some of the analyses.

4.2 IMSS Data

We also use data on all formal private-sector jobs from the IMSS, from 1997 until 2003. The IMSS data include monthly records of the number of insured workers in each category, where a category is defined by location, industry, employer size, employee age, employee gender, and employee salary range. For example, one observation of this dataset provides the number of formal sector female workers employed in a particular month in a particular municipality, aged between 20 and 25, earning between 2 and 3 times the minimum salary, and working at a firm that hires between 51 and 250 employees, in a specific industry.

[6] The last restriction applies only to the educational attainment regressions, not the attendance regressions. In section 6.3, we discuss how attrition from the sample between baseline and 2003 might be affecting our results.

[7] For all parental variables, we assign children the values belonging to the household head and the household head's spouse. Over 90% of the children in our sample are sons or daughters of the household head.

Table 1: Summary Statistics for Individual and Household Characteristics

	Mean			Difference
	(1)	(2)	(3)	(4)
Variable	Full sample	Treatment	Control	Treatment - Control
Age	10.00	9.99	10.01	-0.01
	(3.32)	(3.32)	(3.32)	(0.05)
Female	0.48	0.48	0.49	-0.01
	(0.50)	(0.50)	(0.50)	(0.01)
Attending School	0.85	0.85	0.84	0.00
	(0.36)	(0.36)	(0.36)	(0.01)
Educational Attainment	3.39	3.38	3.40	-0.02
	(2.71)	(2.69)	(2.76)	(0.07)
N Individuals	23,272	14,420	8,852	
Household Size	6.67	6.67	6.67	-0.00
	(2.16)	(2.16)	(2.16)	(0.07)
Household Head Age	42.02	41.80	42.39	-0.59
	(12.13)	(11.96)	(12.40)	(0.37)
Female Household Head	0.07	0.07	0.07	-0.00
	(0.25)	(0.25)	(0.25)	(0.01)
No. Children Aged 0-2	0.55	0.55	0.55	0.00
	(0.66)	(0.66)	(0.66)	(0.02)
No. Children Aged 3-5	0.74	0.74	0.73	0.00
	(0.73)	(0.73)	(0.73)	(0.02)
No. Females Aged 6-7	0.27	0.27	0.28	-0.01
	(0.47)	(0.47)	(0.47)	(0.01)
No. Females Aged 8-12	0.64	0.63	0.64	-0.01
	(0.74)	(0.74)	(0.75)	(0.02)
No. Females Aged 8-12	0.50	0.50	0.51	-0.01
	(0.73)	(0.73)	(0.73)	(0.02)
No. Males Aged 6-7	0.28	0.28	0.28	-0.00
	(0.48)	(0.47)	(0.48)	(0.01)
No. Males Aged 8-12	0.67	0.68	0.66	0.02
	(0.75)	(0.75)	(0.74)	(0.02)
No. Males Aged 13-18	0.54	0.55	0.53	0.01
	(0.76)	(0.77)	(0.74)	(0.02)
No. Females Aged 19-54	1.12	1.12	1.12	0.00
	(0.51)	(0.52)	(0.51)	(0.01)
No. Females Aged 55+	0.15	0.14	0.16	-0.01
	(0.37)	(0.37)	(0.38)	(0.01)
No. Males Aged 19-54	1.03	1.04	1.03	0.01
	(0.56)	(0.57)	(0.54)	(0.02)
No. Males Aged 55+	0.16	0.16	0.16	-0.01
	(0.37)	(0.37)	(0.38)	(0.01)
Mother Attended Secondary School	0.05	0.05	0.06	-0.01
	(0.23)	(0.22)	(0.24)	(0.01)
Missing Mother's Education	0.39	0.38	0.40	-0.02
	(0.49)	(0.49)	(0.49)	(0.03)
Father Attended Secondary School	0.07	0.07	0.07	0.00
	(0.25)	(0.25)	(0.25)	(0.01)
Missing Father's Education	0.32	0.32	0.33	-0.01
	(0.47)	(0.47)	(0.47)	(0.02)
Mother Speaks Indigenous Lang.	0.42	0.42	0.43	-0.01
	(0.49)	(0.49)	(0.49)	(0.06)
Missing Mother's Language	0.03	0.03	0.03	-0.00
	(0.17)	(0.17)	(0.18)	(0.00)
Father Speaks Indigenous Lang.	0.43	0.43	0.44	-0.02
	(0.50)	(0.49)	(0.50)	(0.06)
Missing Father's Language	0.07	0.07	0.07	-0.00
	(0.25)	(0.25)	(0.25)	(0.01)
N Households	8,296	5,162	3,134	

Notes: Summary statistics calculated from the baseline survey, restricting to children aged 6-15 at baseline, with a non-missing educational attainment variable in 2003. Standard deviations (in columns 1-3) and standard errors clustered at village level (in column 4) in parentheses (* p< 0.1, ** p< 0.05, *** p< 0.01).

The IMSS data assign each firm to one of 276 industry categories, without indicating whether firms are export-oriented or not. Following the empirical strategy in Atkin (2016), we define export-oriented manufacturing firms as those which belong to a three-digit International Standard Industrial Classification (ISIC) industry where more than 50 percent of output was exported for at least one-half of the study's sample years (1986-2000).[8] For each month we calculate the number of export-oriented manufacturing jobs, relative to the size of the working-age population (between 15 and 49 years of age), obtained from Mexico's 1990 census.

We also categorize jobs based on the salary range and age of the insured individual. We define low-wage jobs as those with a salary up to two times the statutory minimum salary, and high-wage jobs as those with a salary above this threshold. Similarly, we define young (old) export jobs as those with registered ages below (above) 25 years old. Finally, we separate jobs by gender.

Our analysis requires the use of a geographic unit within which there is sufficient variation in PROGRESA treatment status. Because over half of the municipalities in the sample were comprised of either all treatment or all control villages, we aggregate municipality-level counts of export jobs to the subdelegation level. The IMSS divides the country into delegations, which are further divided into subdelegations. Subdelegations are regional offices that serve as local branches of the IMSS, providing various administrative services for the region such as enrollment and registration of affiliated individuals, collection of contributions from employers, and coordination of hospital admissions. Publicly available health and employment data from the IMSS is provided at the state, delegation, subdelegation, and municipality levels (e.g., Doubova et al., 2021). We choose to use subdelegations over the larger delegations and states because there are only seven states and nine delegations in the PROGRESA evaluation sample but 23 subdelegations.

[8] The resulting export industries are: Apparel; Footwear; Leather and Leather Products; Wood and Cork Products; Petrochemical Refinement; Metal Products; Electronic and Mechanical Machinery; Electrical Machinery; Transport Equipment; Scientific and Optical Equipment.

Table 2 shows summary statistics for the share of export-oriented manufacturing jobs, both pooled and separately by treatment arm. On average, the number of export-oriented jobs is 2.6% of the working-age population. This value is not statistically different across treatment and control villages. This holds when we split by gender, wage, and age.

Table 2: Summary Statistics: Export-oriented Manufacturing Jobs

	Mean			Difference
	(1)	(2)	(3)	(4)
Variable	Full sample	Treatment	Control	Treatment - Control
All Jobs	0.026	0.022	0.032	-0.010
	(0.056)	(0.037)	(0.079)	(0.006)
Female	0.024	0.020	0.032	-0.011
	(0.064)	(0.042)	(0.090)	(0.007)
Male	0.028	0.025	0.033	-0.008
	(0.051)	(0.036)	(0.070)	(0.006)
High Wage	0.011	0.008	0.015	-0.007
	(0.040)	(0.024)	(0.059)	(0.005)
Low Wage	0.015	0.014	0.017	-0.003
	(0.021)	(0.019)	(0.025)	(0.002)
Young	0.011	0.010	0.014	-0.004
	(0.023)	(0.016)	(0.031)	(0.002)
Old	0.015	0.013	0.018	-0.006
	(0.034)	(0.022)	(0.048)	(0.004)
N Villages	502	319	183	

Notes: Summary statistics calculated at the village level, restricting the sample to villages with at least one child aged 6-15 at baseline, with a non-missing educational attainment variable in 2003. Each row reports the number of export-oriented manufacturing jobs in the village's subdelegation in 1997 for the specified group, divided by the total working-age population (gender-specific where relevant). Standard deviations (in columns 1-3) and standard errors (in column 4) in parentheses. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$.

5 Empirical Strategy

Our empirical strategy is composed of two parts. We begin by describing our estimation of the heterogeneous effects of PROGRESA on educational attainment, which involves comparing treatment and control villages in 2003. We then describe our estimation of the intermediate attendance effects, which focuses on the contemporaneous effect of PROGRESA on school attendance using multiple survey waves prior to 2003.

Because only households classified as poor were considered eligible for PROGRESA, we restrict our analysis, as most existing studies do, to this subset of the population. In addition, we restrict to children of school-going age during the experimental period – specifically, those aged 5 to 16 in 1997.[9]

5.1 Educational Attainment

Our first outcome of interest is educational attainment in 2003. By this time, PROGRESA was operating in both treatment and control villages, but treatment villages had been exposed to the program for 18 additional months. To estimate the heterogeneous effects of this additional exposure, we estimate the following specification:

$$E_{igjs} = \beta_1 T_j J_{sg} + \beta_2 T_j + \beta_3 J_{sg} + \beta_4 X_{ig} + \mu_s + \epsilon_{igjs}, \tag{3}$$

where E_{igjs} is the educational attainment of child i of gender g in village j and subdelegation s, as of 2003. T_j is an indicator equal to one for the randomly assigned treatment villages. J_{sg} is the number of export-oriented jobs in subdelegation s for gender g in 1997 (as a fraction of the subdelegation's working-aged population according to the 1990 census). We use 1997 because it is the earliest year of publicly available IMSS data and the only year before the rollout of PROGRESA (which ensures it could not have been affected by the program itself).[10] To facilitate the interpretation of coefficient magnitudes, we standardize this variable. This means that β_2 represents the effect of PROGRESA for a subdelegation with the average number of export jobs. β_1 is our coefficient of interest, which captures heterogeneity in the PROGRESA effect across varying levels of export job availability. A positive coefficient would indicate that PROGRESA is more effective in areas with more export jobs, while a negative coefficient would indicate that PROGRESA is less effective in these areas.

[9] If we assume that children start first grade at age six and do not repeat grades, children aged 5 to 13 in 1997 would have been in PROGRESA-eligible grades during the first 18 months of the program, while only the treatment group was exposed. We include three older age cohorts as they might have also been eligible due to schooling interruptions and grade repetitions.

[10] In robustness checks, to further alleviate concerns about the endogeneity of export jobs, we also use an alternate export manufacturing variable: predicted export job growth generated using a shift-share strategy.

We include subdelegation fixed effects (μ_s), which control for subdelegation-specific unobservables that are fixed over time. Even with these fixed effects, we are relying on variation across subdelegations as well as variation across genders to estimate the interaction coefficient of interest.[11] X_{ig} is a vector of child-level controls. In our baseline specification, we include age and gender dummies. We later add demographic controls from the baseline survey: household size, age of household head, gender of household head, maternal and paternal education (dummies for secondary school attendance), and maternal and paternal language dummies.[12] Because our export jobs variable (J_{sg}) is gender-specific, to ensure that β_1 is not capturing gender differences in PROGRESA's education effects, we also add a treatment-by-female interaction in subsequent specifications. Finally, we cluster our standard errors at the village level, which was the level of treatment assignment.[13]

In order to ensure that our estimate of β_1 is not being confounded by PROGRESA treatment heterogeneity due to other variables potentially correlated with export jobs, we also estimate specifications that include interactions between treatment and other subdelegation-level and household-level characteristics, denoted C_{ijs} in the following regression:

$$E_{igjs} = \beta_1 T_j J_{sg} + \beta_2 T_j + \beta_3 J_{sg} + \beta_4 X_{ig} + \beta_5 T_j C_{ijs} + \beta_6 C_{ijs} + \mu_s + \epsilon_{igjs}. \tag{4}$$

We run separate regressions using different definitions of C_{ijs}: for example, subdelegation-level average schooling, income, and urban shares from the 2000 census (in which case the

[11]Note that even if J_{sg} were not gender-specific, the inclusion of subdelegation fixed effects would still allow us to estimate the interaction coefficient β_1, but not the coefficient on the main effect, β_3.

[12]For continuous variables, we replace missing values with the sample mean. For parental education and language, we include a dummy for missing values.

[13]We base this decision off of Abadie et al. (2022). There are two "treatments" to consider in our case: the randomized PROGRESA treatment at the village level, and the effect of export jobs interacted with treatment (at the subdelegation level). At the village level, there is clustering in the sampling as well as in the treatment assignment, and we are arguing that there is heterogeneity in the effect of PROGRESA across villages: according to Abadie et al. (2022), we must therefore at least cluster at the village level. At the subdelegation level, however, because we are including subdelegation fixed effects, the conditions that determine whether to cluster are slightly different. In particular, if there are no heterogeneous treatment effects at the subdelegation level then we should not be clustering at the subdelegation level. The relevant "treatment" to consider here is the export jobs interaction with treatment, and we argue this variable affects outcomes in the same way across individuals.

main effect is absorbed by the subdelegation fixed effects). We also use the following (all taken from the 1997 survey): child i's baseline educational attainment, a vector of father and mother occupation category dummies, older sibling work status, household per capita labor income, and a vector of proxies for the temporary migration of household members (separate dummies indicating if a father or mother is not living at home, as well as the continuous share of household members not living at home).

5.2 Attendance

We next explore the contemporaneous effect of PROGRESA on school attendance, using all waves before 2003 (the October 1997 baseline survey, October 1998, October 1999, and November 1999). Specifically, for child i of gender g in village j and subdelegation s, observed in wave w, we estimate

$$\begin{aligned} A_{igjsw} =& \alpha_1 T_j P_w J_{sg} + \alpha_2 T_j P_w + \alpha_3 T_j J_{sg} + \alpha_4 P_w J_{sg} + \\ & \alpha_5 T_j + \alpha_6 J_{sg} + \alpha_7 X_{ig} + \mu_s + \delta_w + \epsilon_{igjsw}. \end{aligned} \tag{5}$$

A_{igjsw} is a school attendance dummy variable. As before, J_{sg} captures the number of export-oriented jobs in subdelegation s for gender g (as a fraction of the working-aged population and standardized, as above) in 1997. P_w is a dummy for post-treatment waves (all waves except the 1997 baseline).

The main coefficient of interest is α_1. This captures heterogeneity in the PROGRESA treatment effect across areas with varying export job exposure. Including the baseline wave helps improve statistical precision and also builds in some validity checks. For example, we would expect α_5 (the difference between treatment and control villages prior to the rollout of PROGRESA) and α_3 (heterogeneity in this difference by export jobs) to be equal to zero.

As in the first specification, we include a vector of child and household controls (X_{ig}). We also estimate versions of this regression that add female interactions: a female dummy interacted with T_j, P_w, and T_jP_w. We once again cluster standard errors at the village level.

We conduct a similar robustness exercise to the one described above, outlined in the regression below:[14]

$$\begin{aligned} A_{igjsw} = &\alpha_1 T_j P_w J_{sg} + \alpha_2 T_j P_w + \alpha_6 J_{sg} + \alpha_7 X_{ig} + \alpha_8 T_j P_w C_{ijs} + \alpha_9 C_{ijs} + \\ &\mu_s + \delta_w + \epsilon_{igjsw}. \end{aligned} \tag{6}$$

We use C_{ijs} to denote various subdelegation and household-level characteristics. Subdelegation-level schooling, income, and urban shares are taken from the 2000 census, as above. However, the household-level characteristics (parental and sibling work, per capita income, and temporary migration) are obtained from the relevant wave w (instead of the baseline). Child baseline schooling is the only exception, which is constant across all waves for each child.

6 Results

6.1 Main Results

We begin with the effect of PROGRESA on educational attainment. Table 3 reports the results of equation (3). In column 1, we estimate that PROGRESA increased educational attainment by 0.16 years for areas with the average number of export jobs, which is similar to estimates of around 0.2 years for the full sample (Behrman et al., 2011).[15] For an area that lies one standard deviation above the mean in terms of export jobs, the interaction coefficient of -0.37 implies no PROGRESA effect at all (while the sum of the two coefficients is in fact negative, it is not significantly different from zero). In Appendix Figure A2, we show the entire distribution of treatment effect magnitudes: the vast majority are positive, and only a small share are negative.

[14] Note that we drop T_jJ_{sg}, P_wJ_{sg}, and T_j for parsimony after having established that these coefficients are small in magnitude and not statistically significant. In this specification T_jP_w can be thought of as a dummy variable equal to 1 if village j is treated in wave w.

[15] The export jobs variable is standardized to facilitate the interpretation of the main effect.

Across all columns, there is a positive and significant coefficient on the treatment dummy and a negative and significant coefficient on the interaction term, which indicates that PROGRESA improved educational attainment, but less so in areas with many export jobs. This pattern of results is robust to the inclusion of additional controls (in columns 3 and 4) and treatment-by-female interactions (in columns 2 and 4). The latter indicates that the *Treat*-by-*Export Jobs* coefficient is not simply picking up gender differences in the PROGRESA impact.

Table 3: Heterogeneous Effects of PROGRESA on Educational Attainment

	(1) Educational Attainment	(2) Educational Attainment	(3) Educational Attainment	(4) Educational Attainment
Treat x Export Jobs	-0.37	-0.37	-0.27	-0.28
	(0.15)**	(0.15)**	(0.13)**	(0.13)**
Treat	0.16	0.21	0.16	0.21
	(0.096)*	(0.11)**	(0.087)*	(0.097)**
Export Jobs	0.081	0.068	0.00070	-0.012
	(0.15)	(0.15)	(0.14)	(0.14)
Treat x Female		-0.095		-0.098
		(0.085)		(0.083)
Observations	23272	23272	23272	23272
Mean of DV	6.89	6.89	6.89	6.89
Controls	Basic	Basic	All	All
p-value for sum	0.28	0.42	0.51	0.71

Notes: Standard errors (clustered at village level) in parentheses. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$. These regressions use the 2003 survey wave, restricting to children aged 5 to 16 at baseline (in 1997). *Treat* is an indicator for PROGRESA treatment villages. *Export Jobs* is the ratio of the number of export-oriented jobs in the subdelegation in 1997, over the subdelegation's working-aged population according to the 1990 census, standardized. *Basic* controls include gender, cohort fixed effects, and subdelegation fixed effects. *All* controls add household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values). *p-value for sum* reports the p-value testing the null hypothesis that $\beta_1 + \beta_2=0$.

Because these regressions use educational attainment in 2003, when PROGRESA was available in both treatment and control villages, the estimated treatment effects can be interpreted as the effect of being exposed to the program 18 months earlier. We now move on to investigate the intermediate changes leading up to these increases in educational attainment – that is, the contemporaneous effects of PROGRESA on school attendance during the years in which the control group had not yet received the program.

The attendance results are reported in Table 4. In columns 1, 2, 4, and 5, we report the results of equation (5). The *Treat*-by-*Post* interaction provides the contemporaneous effect of PROGRESA on school attendance. We estimate that PROGRESA increased attendance rates by approximately 3 percentage points for the average subdelegation. However, for subdelegations one standard deviation above the mean, the effect is 2 percentage points smaller (and not significantly different from zero). As was the case with educational attainment, attendance improved due to PROGRESA, but less so for areas with many export jobs. Results are robust to the inclusion of additional demographic controls (columns 4 and 5) and female interactions (in columns 2 and 5). The histogram of PROGRESA attendance effects (in Appendix Figure A3) reveals the majority of subdelegations demonstrated positive effects (most of which are significantly different from zero) and only a small share saw negative (but insignificant) effects.

Because treatment was randomly assigned and the program was not rolled out until after the baseline survey, we would expect to see no differences across treatment and control during the baseline survey. The small and statistically insignificant coefficient on *Treat* shows this is true. For similar reasons, we would not expect any job-related heterogeneity in the treatment-control gap in the baseline survey, which is confirmed by the statistically insignificant coefficient on *Treat*-by-*Export Jobs*.

In columns 3 and 6, we show the results of a simplified specification that drops the last three variables, none of which are significantly different from zero (in any specification). Because *Treat*-by-*Post* can also be described as an indicator equal to 1 for villages that are treated in the current wave, this specification estimates the effect of being treated in the current wave, while allowing for heterogeneity in this impact. These regressions yield similar results, though with slightly smaller and less precisely estimated interaction coefficients. We will use this simplified specification in later regressions, where we add additional interaction terms, in order to limit the number of additional interactions needed.

Table 4: Heterogeneous Effects of PROGRESA on School Attendance

	(1) School Attendance	(2) School Attendance	(3) School Attendance	(4) School Attendance	(5) School Attendance	(6) School Attendance
Treat x Post x Export Jobs	-0.022	-0.021	-0.020	-0.022	-0.021	-0.019
	(0.0082)***	(0.0083)**	(0.0093)**	(0.0083)***	(0.0083)**	(0.0090)**
Treat x Post	0.029	0.026	0.031	0.029	0.026	0.030
	(0.0059)***	(0.0078)***	(0.0085)***	(0.0059)***	(0.0078)***	(0.0083)***
Export Jobs	-0.012	-0.012	-0.011	-0.013	-0.013	-0.012
	(0.017)	(0.017)	(0.016)	(0.017)	(0.017)	(0.016)
Treat x Export Jobs	0.0012	0.00061		0.0022	0.0016	
	(0.011)	(0.011)		(0.011)	(0.011)	
Post x Export Jobs	0.00082	0.00078		0.00070	0.00067	
	(0.0035)	(0.0035)		(0.0035)	(0.0035)	
Treat	0.0058	0.0066		0.0051	0.0059	
	(0.0077)	(0.0086)		(0.0076)	(0.0084)	
Observations	95705	95705	95705	95705	95705	95705
Mean of DV	0.83	0.83	0.83	0.83	0.83	0.83
Controls	Basic	Basic	Basic	All	All	All
Additional Treatment Interactions	None	By Female	By Female	None	By Female	By Female
p-value for sum	0.49	0.64	0.38	0.53	0.69	0.41

Notes: Standard errors (clustered at village level) in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. These regressions use the 1997, 1998, and both 1999 survey waves, restricting to children aged 5 to 16 at baseline (in 1997). *Treat* is an indicator for PROGRESA treatment villages. *Export Jobs* is the ratio of the number of export-oriented jobs in the subdelegation in 1997, over the subdelegation's working-aged population according to the 1990 census, standardized. *Post* is an indicator for all waves after 1997. *Basic* controls include gender, cohort fixed effects, wave fixed effects, and subdelegation fixed effects. *All* controls add household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values). *By Female* treatment interactions include a female indicator interacted with *Treat*-by-*Post* (in all columns), in addition to a female indicator interacted with *Treat* and *Post* in columns 2 and 5. *p-value for sum* reports the p-value testing the null hypothesis that $\alpha_1 + \alpha_2$=0.

6.2 Mechanisms

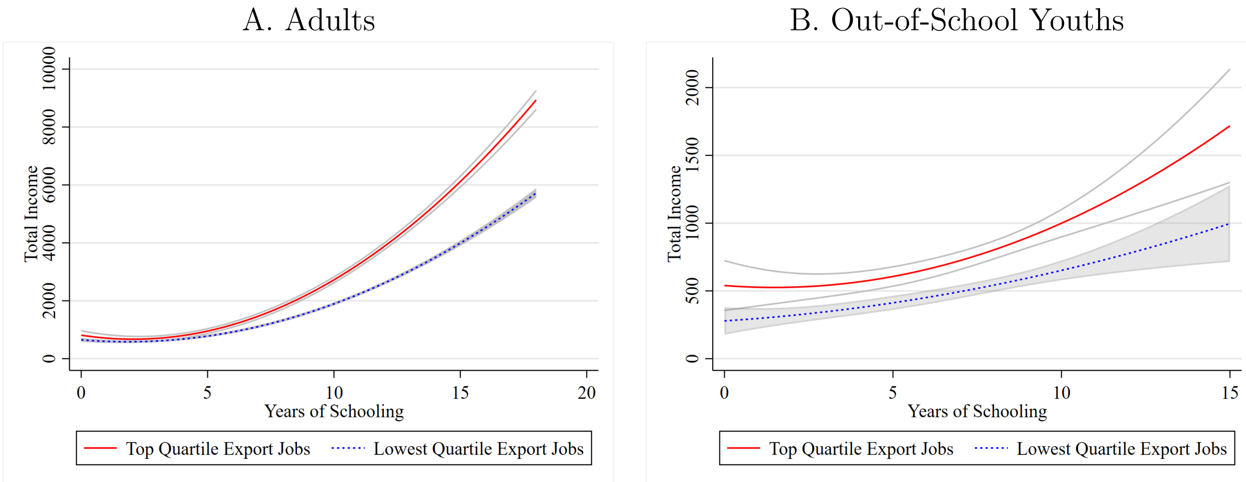
According to the model in section 2, the finding that export manufacturing jobs reduce the schooling impact of PROGRESA implies these jobs result in net benefits that decrease faster with each additional year of schooling. This could be due to export jobs changing the convexity of the perceived future wage function or the convexity of the forgone wage function. To determine which of these mechanisms are in play, we need to know how the second derivatives of the wage and cost functions differ in areas with high concentrations versus low concentrations of export manufacturing jobs.

While second derivatives are generally difficult to measure, we present a few figures that provide some insight into these questions. First, in Panel A of Figure 2, we plot the quadratic relationship between total income and schooling among adults. We do this separately for subdelegations in the top quartile and those in the bottom quartile in terms of export jobs, in order to shed light on how export jobs might affect the convexity of the wage function. The solid red line, which represents high-export areas, has a steeper and more rapidly increasing slope compared to the dotted blue line, which represents low-export areas. In other words, the marginal benefits of schooling appear to be increasing faster in high-export areas. This implies a larger $\frac{\partial^2 W}{\partial S^2}$, which would predict larger CCT schooling effects for high-export areas – the opposite of what our results show. It therefore appears that the convexity of the wage function is not the driving force behind our results.

However, Panel B of Figure 2 shows that a comparison of opportunity costs leads to a different prediction – lower CCT schooling effects for high-export areas. Here, we plot the quadratic relationship between total income and schooling among out-of-school youths – a proxy for the relationship between forgone wages and schooling. This relationship is steeper for areas with many export jobs, suggesting that opportunity costs are more convex in these areas. This translates into a larger $\frac{\partial^2 c}{\partial S^2}$ for export areas, which would predict lower CCT education effects.

Our findings of smaller CCT schooling effects in areas with more export jobs suggest

Figure 2: Income-Schooling Relationship, by Export Job Quartiles



Notes: Solid red and dotted blue lines depict the predicted quadratic relationship between income and schooling using the 2000 Mexican census, among individuals in the seven PROGRESA states. Panel A restricts to adults aged 25-55. Panel B restricts to out-of-school youths aged 13-20. Gray lines/regions represent 95% confidence intervals. Quartiles are defined by classifying subdelegations according to the number of export-oriented jobs (as a share of the total population) in 2000.

that the opportunity cost channel (specifically, more convex costs) dominates over the wage function channel. We provide further evidence for this claim by exploring what types of individuals and what types of jobs are driving the heterogeneity documented.

We first show the heterogeneity is stronger for those old enough to be actually working a factory job. We use 15 as the cutoff age, as this is the median of the official minimum working age at the time (14) and the minimum working age without parental consent (16) (Atkin, 2016). The first two columns of Table 5 show that while the interaction term is negative and significant for those who would have been aged 15 for at least one year in the sample period (those 16 and older in 2003), it is smaller and insignificant for those who would have been too young. This is made even clearer in Appendix Figure A4, which plots the entire distribution of treatment effects for each group, revealing substantially greater variance for the working-aged group.[16]

[16] What is important here is the extent of the heterogeneity and not the size of the main effect. It has been documented previously that PROGRESA's effect on educational attainment was larger among those who were in later grades, and this can be seen by comparing the size of the *Treated* coefficient in columns 1 and 2. This is likely due to the fact that more students were on the margin of dropping out at these ages and does not necessarily speak to the argument that the opportunity cost channel was the dominant force

We document a similar result for attendance effects, for which we split the sample into younger than 15 and those 15 and older at the time of the survey. As with educational attainment, the last two columns reveal a significant negative interaction term only for the working-aged and not the younger sample. Moreover, Appendix Figure A5 reveals a much larger variance of treatment effects for the working-aged group.

Table 5: Heterogeneous Effects of PROGRESA for Working-Aged versus Younger Cohorts

	(1) Educational Attainment	(2) Educational Attainment	(3) School Attendance	(4) School Attendance
Treated x Export Jobs	-0.14	-0.39	-0.0045	-0.080
	(0.094)	(0.19)**	(0.0079)	(0.020)***
Treated	0.12	0.31	0.031	0.026
	(0.069)*	(0.14)**	(0.0069)***	(0.020)
Export Jobs	0.12	-0.12	-0.024	0.057
	(0.12)	(0.23)	(0.014)*	(0.041)
Observations	10906	12366	80149	15556
Mean of DV	5.785	7.871	0.916	0.410
Controls	All	All	All	All
Additional Treatment Interactions	By Female	By Female	By Female	By Female
Sample	Non Working Age	Working Age	Non Working Age	Working Age

Notes: Standard errors (clustered at village level) in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. Columns 1 and 3 use the 2003 survey wave, columns 2 and 4 use the 1997, 1998, and both 1999 survey waves, and all columns restrict to children aged 5 to 16 at baseline (in 1997). In columns 1 and 3, *Treated*=1 for PROGRESA treatment villages; in columns 2 and 4, *Treated*=1 if a village has PROGRESA at the time of the survey. *Export Jobs* is the ratio of the number of export-oriented jobs in the subdelegation in 1997, over the subdelegation's working-aged population according to the 1990 census, standardized. *All* controls include gender, cohort fixed effects, wave fixed effects, subdelegation fixed effects, household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values). "Working Age" is defined as those older than 15 (for educational attainment regressions) or those currently aged 15 or older (for attendance regressions).

Table 6 provides further support for the opportunity cost channel. Here, we examine whether the negative interaction coefficients reported above are being driven by the types of export jobs that would actually factor into the opportunity costs of school. Specifically, we differentiate between export jobs that are low-wage and held by younger workers. These jobs are more obtainable for someone who drops out of school before graduating high school and are therefore more relevant to the opportunity cost function.

driving heterogeneity by export jobs. It is the fact that the *heterogeneity* (by labor market conditions) was larger for working-age individuals that adds support for this argument.

The results of Table 6 reveal that the negative interaction coefficients reported above are indeed being driven by low-wage and young jobs. In columns 1 and 2, we include one interaction between treatment and export jobs among low-wage workers (earning less than double the minimum salary), and one interaction between treatment and export jobs among high-wage workers. For both educational attainment and school attendance, it is only the low-wage job interaction that generates a negative and significant coefficient. In columns 3 and 4, we repeat the exercise, this time including treatment interactions with young export jobs (25 years old and under) and older export jobs. In both columns, it is only the young export jobs variable that generates a negative interaction coefficient.

Table 6: Heterogeneous Effects of PROGRESA using Different Types of Export Jobs

	(1) Educational Attainment	(2) School Attendance	(3) Educational Attainment	(4) School Attendance
Treated x Export Jobs (Type 1)	-0.21	-0.018	-0.37	-0.047
	(0.11)*	(0.0080)**	(0.34)	(0.023)**
Treated x Export Jobs (Type 2)	0.023	0.0079	0.14	0.034
	(0.12)	(0.012)	(0.37)	(0.025)
Treated	0.20	0.030	0.20	0.029
	(0.095)**	(0.0084)***	(0.096)**	(0.0083)***
Export Jobs (Type 1)	0.068	0.0085	0.13	0.036
	(0.094)	(0.0079)	(0.27)	(0.020)*
Export Jobs (Type 2)	-0.080	-0.026	-0.16	-0.054
	(0.16)	(0.017)	(0.35)	(0.028)*
Observations	23272	95705	23272	95705
Mean of DV	6.894	0.833	6.894	0.833
Controls	All	All	All	All
Type 1	Low Wage	Low Wage	Young	Young
Type 2	High Wage	High Wage	Old	Old
Additional Treatment Interactions	By Female	By Female	By Female	By Female

Notes: Standard errors (clustered at village level) in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. Columns 1 and 3 use the 2003 survey wave, columns 2 and 4 use the 1997, 1998, and both 1999 survey waves, and all columns restrict to children aged 5 to 16 at baseline (in 1997). In columns 1 and 3, *Treated*=1 for PROGRESA treatment villages; in columns 2 and 4, *Treated*=1 if a village has PROGRESA at the time of the survey. *Export Jobs* is the ratio of the number of export-oriented jobs (defined by the specified type) in the subdelegation in 1997, over the subdelegation's working-aged population according to the 1990 census, standardized. *All* controls include gender, cohort fixed effects, wave fixed effects, subdelegation fixed effects, household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values).

In sum, this evidence supports the idea that PROGRESA was less effective in areas with more export manufacturing because these types of jobs increase the convexity of the opportunity cost function. Although Figure 2 showed that export manufacturing jobs also increase the convexity of the wage function (which should lead to larger PROGRESA effects), our results indicate the marginal cost channel appears to dominate over the marginal benefits channel.

One possible reason for the importance of opportunity costs relative to future wages is migration, which could weaken the relationship between local (subdelegation-level) labor market conditions and perceived future wages. Individuals might form their expectations about the future wage function using information from areas outside their subdelegation, especially in a setting like Mexico where 18% of residents (in 2000) were living in a state different from their state of birth; the share who have migrated across subdelegations is likely much larger.[17] Another possibility is that parental preferences might play an important role in the optimal schooling decision. If parents value current income more than their child's future income, this would result in marginal costs receiving a heavier weight in the maximization problem.

6.3 Robustness

Taken together, these results support the argument that export jobs reduce PROGRESA schooling effects by changing the shape of the opportunity cost function (as opposed to the wage function), consistent with the discussion in section 2. The validity of this interpretation, however, requires that the heterogeneity we document is not caused by some other correlate of the export jobs variable.[18]

To evaluate this, we begin by exploring possible correlates at the subdelegation level. If

[17] We calculate this share using state-level migration data provided by INEGI. The data include the total population and the number of individuals living in a state different than the one they were born in. Data are available at `https://www.inegi.org.mx/temas/migracion/`.

[18] Given the results discussed in Table 5, any problematic correlate would have to generate heterogeneity for certain age groups and not others.

exporting firms make decisions about where to locate or where to expand based on characteristics of a subdelegation, these characteristics might be generating the heterogeneity we document. For example, if exporting firms tend to build new factories or expand existing factories in areas with higher levels of education, and if PROGRESA is less effective in areas where schooling levels are already high, this would also generate a negative coefficient on the *Treat*-by-*Export Jobs* interaction in our earlier results.

In Appendix Figure A6, we show that there are small positive relationships between export jobs and various indicators of socioeconomic status at the subdelegation level, though none of these are statistically significant. To generate this figure, we regress subdelegation average schooling, log income, and urban shares on our standardized export job share variable (using data from the 1990 census with the 1997 IMSS, the earliest publicly available year, and the 2000 census with the 2000 IMSS). All coefficients (which represent the result of a one-standard-deviation change in export jobs relative to the dependent variable mean) are positive but small and statistically insignificant, though with only 23 subdelegations we may lack statistical power. We will later explore whether our results are robust to the inclusion of interaction terms between the treatment indicator and each of these subdelegation-level characteristics.

Another possibility is that export jobs are correlated with household or individual characteristics and that PROGRESA treatment effects vary across these characteristics rather than export jobs. For example, mothers might be more likely to work in areas with export jobs, and PROGRESA may be less effective in households where mothers spend less time at home. We show in Appendix Figure A7 that export jobs do appear to be correlated with several household and individual characteristics. Using data from all four 1997-1999 waves, we regress various characteristics on the 1997 export jobs variable. The first, second, and third panels examine variables related to father's, mother's, and siblings' jobs (or lack thereof). The last panel explores proxies for temporary household migration, household labor income, as well as child baseline schooling levels.

We find, for example, that fathers are more likely to be employees and less likely to be self-employed in areas with many export jobs. Children are more likely to have working siblings, and a larger share of the household is living away from home (possibly because of migration from rural PROGRESA villages to areas where export jobs are located). While these are all consistent with export jobs changing the labor market opportunities of these villages, they also demonstrate the need to test whether our results are being driven by treatment effect heterogeneity based on these characteristics.

We conduct this test in Appendix Tables A3 (for educational attainment) and A4 (for attendance). Each column represents a different regression that controls for treatment interacted with a different subdelegation, household, or individual characteristic. We allow for heterogeneity with respect to subdelegation-level schooling, income, and urban shares (columns 1 to 3). In column 4, we allow for differential effects based on the child's educational attainment as of 1997. At the household level, we allow for heterogeneity by temporary migration proxies, household per capita labor income, father's occupation type, mother's occupation type, and sibling work status (columns 5 to 9). In the final column, we include all interaction terms from the previous columns. Reported coefficients can be interpreted as the effects for the average child (for continuous variables) or modal child (for categorical variables).[19]

In both tables, all specifications reveal treatment main effects and export job interactions that are almost identical to those estimated in Tables 3 and Table 4. Even in column 10 of Appendix Tables A3 and A4, which include the entire set of interaction terms, the magnitudes of the coefficients of interest are similar to baseline estimates (though not statistically significant in Appendix Table A3). In short, the treatment effect heterogeneity we document does appear to be driven by the availability of export jobs, and not by any of these other

[19] Continuous variables are standardized so that the other coefficients can be interpreted as effects for an individual with average levels of the particular variable. For categorical variables, where interactions with several dummy variables are included in the regression, the omitted category is the model category, which means that coefficients represent effects for the modal individual. For example, most fathers are employees, which means that this is used as the omitted category and the coefficients reported in the table represent the effect of PROGRESA (and export job heterogeneity) for children whose fathers are employees.

characteristics. It is worth noting that some of these characteristics do drive treatment heterogeneity. For example, attendance effects are smaller for children with higher baseline schooling and educational attainment effects are smaller for children with mothers who are employees (coefficients not reported but available upon request). Importantly, however, these other dimensions of heterogeneity do not appear to be confounding the estimates in our main specifications, which seem to be capturing what it was intended to – heterogeneity based on export job availability.

Our next robustness check explores alternatives to the export jobs variable. We construct variables similar to a Bartik instrument, which combines industry composition in a baseline period with national-level industry growth rates to create a predicted employment growth variable arguably uncorrelated with location-specific changes that could be generating endogeneity problems. Specifically, for each subdelegation, we calculate the employment share in each export-oriented industry in a baseline period, multiply this by the national growth rate of the industry from the baseline period to period t, and sum across all export manufacturing industries to predict growth in export manufacturing from baseline to period t. We do this using the IMSS data in columns 1 and 2 (where the only possibility for a baseline year is 1997) and census data in columns 3 and 4 (for which we use the 1990 census as our baseline). In the educational attainment regressions, we use the shift-share variable for $t = 2003$. In the attendance regressions, we use a time-varying variable, assigning each survey wave to the predicted year from the year before the survey. All of these regressions are reduced form regressions, where we simply replace our original export jobs variable with these shift-share variables. These results yield similar conclusions as our main regressions: positive PROGRESA treatment effects that are smaller in areas with lower predicted growth in export manufacturing.

Finally, we explore whether differential attrition due to PROGRESA or export job exposure might be driving our main results. We begin with the full sample of eligible children aged 5 to 16 at baseline and generate a dummy equal to 1 for those who have a non-missing

Table 7: Heterogeneous Effects of PROGRESA using Alternative Export Jobs Variables

	(1) Educational Attainment	(2) School Attendance	(3) Educational Attainment	(4) School Attendance
Treated x Export Jobs	-0.38	-0.010	-0.24	-0.016
	(0.19)**	(0.0067)	(0.13)*	(0.0086)*
Treated	0.15	0.036	0.18	0.029
	(0.12)	(0.0084)***	(0.11)*	(0.0083)***
Export Jobs	0.23	0.0061	-0.048	0.0023
	(0.100)**	(0.0045)	(0.094)	(0.0069)
Observations	23272	95705	23272	95705
Mean of DV	6.894	0.833	6.894	0.833
Controls	All	All	All	All
Additional Treatment Interactions	By Female	By Female	By Female	By Female
Export Job Variable	Predicted growth (Bartik) from IMSS	Predicted growth (Bartik) from IMSS	Predicted growth (Bartik) from Census	Predicted growth (Bartik) from Census

Notes: Standard errors (clustered at village level) in parentheses. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$. Columns 1, 3, and 5 use the 2003 survey wave, columns 2, 4, and 6 use the 1997, 1998, and both 1999 survey waves, and all columns restrict to children aged 5 to 16 at baseline (in 1997). In educational attainment regressions, *Treated*=1 for PROGRESA treatment villages; in attendance regressions, *Treated*=1 if a village has PROGRESA at the time of the survey. *Export Jobs* represents the specified export job variable, standardized. *All* controls include gender, cohort fixed effects, wave fixed effects, subdelegation fixed effects, household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values).

educational attainment variable in 2003 (and are therefore included in our analysis). We then use this as a dependent variable and estimate equation (3). As we report in Appendix Table A5, the coefficient on the treatment indicator is negative and statistically significant, indicating that – for a village with the average level of export jobs – treatment individuals were 3 percentage points less likely to be included in our sample. In addition, the interaction coefficient is negative and significant, which means this gap was significantly larger in areas with more export jobs. While there are a number of potential reasons for this result, one explanation is that the cash transfers allowed treatment households to migrate away from their village (therefore leaving the sample) and this was more likely to happen in subdelegations with promising export-related job opportunities in nearby areas (not within commuting distance).

Regardless of the reason, this could complicate the interpretation of our main findings if the children who dropped out of the sample had systematically different education outcomes from those who remained. To address this concern, we repeat our analysis using an inverse probability weighting procedure similar to that of other studies on PROGRESA (Behrman et al., 2009a, 2011). Specifically, we estimate a probit regression using the same outcome variable as in Appendix Table A5 (an indicator for sample inclusion) and the following set of independent variables: the PROGRESA treatment indicator, the export jobs variable, their interaction, and all three of these variables interacted with the full set of demographic controls. This allows us to predict each individual's probability of being included in the sample given their treatment status, export job level, and observable covariates (which are allowed to contribute to the prediction in different ways for different treatment groups and export job levels). We then repeat our main regressions using the inverse of these predicted probabilities as weights, therefore accounting for changes in sample composition due to differential attrition. Results, reported in Appendix Tables A6 and A7, are almost identical to our original results, which suggests that differential attrition was not responsible for the findings documented above.

7 Conclusion

In this paper, we highlight two theoretical channels through which labor market conditions can influence the effectiveness of policies that reduce the cost of schooling. Job types can alter the convexity of an individual's perceived wage function, or the convexity of their opportunity cost function. The relative importance of these channels can determine the effectiveness of educational policies across labor markets. Empirically, we focus on Mexico, which rolled out its landmark CCT program, PROGRESA, during a period of trade liberalization that substantially increased the availability of export-oriented manufacturing jobs. This allows us to examine whether PROGRESA was more or less effective in areas with greater exposure to these export-oriented jobs. This exercise sheds light on the broader issue of how export promotion and CCTs – two common development policies – interact.

Although previous literature showed that export manufacturing jobs led to lower educational attainment in Mexico, whether these jobs reduce or enhance CCT effectiveness at increasing educational attainment remained an open question. Our empirical analysis answers this question, showing that PROGRESA was less successful at improving schooling outcomes in areas with greater exposure to export manufacturing. These results, combined with insights from our model and additional descriptive evidence, demonstrate that the opportunity cost channel dominates over the wage benefits channel in this context. Consistent with this, we show that the heterogeneous effects of PROGRESA are driven primarily by jobs that are likely to factor into the opportunity cost of schooling – specifically, low-wage jobs and jobs for younger workers.

Our results highlight and explain why conclusions from individual evaluations may not generalize to settings with different labor markets. This echoes the lessons of recent work highlighting the difficulties involved in generalizing from the results of individual well-identified studies: treatment effects often vary widely across settings, individuals, and over time (Card et al., 2018; Dehejia et al., 2021; Meager, 2022; Rosenzweig and Udry, 2020). Given the widespread popularity of CCTs across the developing world, it is important to understand what drives variation in the success of these programs, and our findings show that the types of jobs available to program beneficiaries play an important role. More generally, this paper provides evidence that labor market conditions influence the effectiveness of government policies, which could be one understudied explanation for why the effects of minimum wage policy, health insurance expansions, financial aid programs, and other government policies differ drastically across settings.

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A Appendix

Table A1: PROGRESA Impact on Educational Attainment Across States

	(1) All	(2) Guerrero	(3) Hidalgo	(4) Michoacan	(5) Puebla	(6) Queretaro	(7) San Luis Potosi	(8) Veracruz
PROGRESA Treatment	0.21	0.91	0.11	0.35	0.081	-0.15	0.13	0.37
	(0.096)**	(0.13)***	(0.082)	(0.082)***	(0.084)	(0.15)	(0.090)	(0.068)***
Observations	23272	2210	3664	3164	3641	1035	2950	6608
Mean of DV	6.89	5.75	7.05	6.76	6.92	6.51	7.18	7.18
p-value testing state diff.'s	0.03							

Notes: Standard errors (clustered at village level) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The outcome variable for all regressions is educational attainment in 2003. "PROGRESA Treatment" is a dummy equal to 1 for PROGRESA treatment villages. Regressions restrict to children aged 5 to 16 in 1997 in eligible (poor) households and control for age, gender, household size, household head age, household head gender, mother's and father's education, and dummies for mother's and father's indigenous language knowledge. The *p-value testing state diff.'s* is obtained from a χ^2 test of equality across all state-specific coefficients.

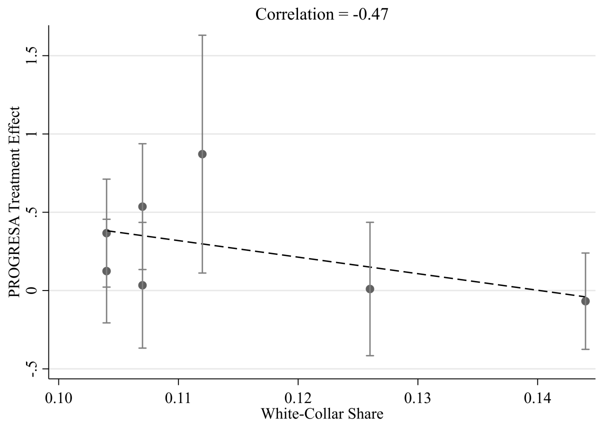
Table A2: Summary Statistics by State

	Mean	Treatment - Control Difference, By State						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All States	Guerrero	Hidalgo	Michoacan	Puebla	Queretaro	San Luis Potosi	Veracruz
Age in 1997	10.00	0.03	-0.03	-0.14	-0.12	0.49**	0.22*	-0.03
	(3.32)	(0.16)	(0.14)	(0.12)	(0.10)	(0.22)	(0.13)	(0.10)
Female	0.48	-0.01	-0.02	-0.02	-0.02	-0.03	-0.00	0.00
	(0.50)	(0.03)	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.01)
Attending School	0.85	-0.00	0.00	0.01	0.00	-0.03	-0.02	0.02
	(0.36)	(0.04)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)
Educational Attainment	3.39	0.29	0.05	0.02	-0.11	0.05	0.10	-0.09
	(2.71)	(0.21)	(0.16)	(0.14)	(0.15)	(0.14)	(0.14)	(0.13)
N Individuals	23272	2210	3664	3164	3641	1035	2950	6608
Household Size	6.67	0.06	0.07	-0.14	-0.32	0.16	0.33**	-0.07
	(2.16)	(0.19)	(0.19)	(0.19)	(0.21)	(0.33)	(0.14)	(0.13)
Household Head Age	42.02	0.66	-0.99	0.04	-2.16*	4.47***	-0.06	-1.07*
	(12.13)	(1.17)	(0.87)	(0.85)	(1.09)	(1.29)	(0.97)	(0.63)
Female Household Head	0.07	0.06***	0.00	-0.01	-0.01	-0.03	-0.02	-0.00
	(0.25)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)
No. Children Aged 0-2	0.55	0.02	0.01	-0.05	0.08*	-0.09	0.03	-0.04
	(0.66)	(0.06)	(0.07)	(0.04)	(0.04)	(0.11)	(0.04)	(0.03)
No. Children Aged 3-5	0.74	0.06	-0.00	-0.08	0.01	-0.12	0.06	-0.01
	(0.73)	(0.05)	(0.06)	(0.06)	(0.05)	(0.09)	(0.05)	(0.04)
No. Females Aged 6-7	0.27	0.01	-0.00	-0.01	-0.08**	-0.02	0.01	0.02
	(0.47)	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.02)	(0.02)
No. Females Aged 8-12	0.64	-0.06	-0.05	-0.08	-0.05	0.02	-0.01	0.04
	(0.74)	(0.08)	(0.04)	(0.05)	(0.06)	(0.12)	(0.05)	(0.03)
No. Females Aged 8-12	0.50	-0.01	0.03	-0.01	-0.09*	0.06	0.04	-0.02
	(0.73)	(0.05)	(0.04)	(0.05)	(0.05)	(0.07)	(0.04)	(0.03)
No. Males Aged 6-7	0.28	-0.01	-0.01	0.07**	-0.02	-0.04	-0.02	-0.02
	(0.48)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.02)
No. Males Aged 8-12	0.67	0.06	0.07	-0.03	-0.06	0.10	0.02	0.03
	(0.75)	(0.08)	(0.05)	(0.05)	(0.04)	(0.06)	(0.05)	(0.04)
No. Males Aged 13-18	0.54	0.00	-0.01	0.00	-0.06	0.15	0.11***	-0.01
	(0.76)	(0.08)	(0.05)	(0.06)	(0.04)	(0.10)	(0.04)	(0.03)
No. Females Aged 19-54	1.12	0.07**	0.01	0.03	-0.05	-0.04	0.04	-0.00
	(0.51)	(0.03)	(0.03)	(0.04)	(0.04)	(0.07)	(0.03)	(0.03)
No. Females Aged 55+	0.15	0.01	-0.00	0.01	-0.04*	0.05	-0.03	-0.01
	(0.37)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)
No. Males Aged 19-54	1.03	-0.11**	0.09**	0.02	0.06	-0.01	0.05	-0.03
	(0.56)	(0.04)	(0.04)	(0.06)	(0.04)	(0.05)	(0.04)	(0.02)
No. Males Aged 55+	0.16	0.03	-0.04*	-0.01	-0.03	0.09***	0.02	-0.01
	(0.37)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Mother Attended Secondary School	0.05	-0.01	-0.04	-0.00	-0.00	0.01	-0.02	-0.00
	(0.23)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)
Missing Mother's Education	0.39	-0.06	-0.05	-0.07*	0.04	0.10	0.02	-0.08*
	(0.49)	(0.08)	(0.06)	(0.04)	(0.05)	(0.07)	(0.05)	(0.04)
Father Attended Secondary School	0.07	0.03	-0.03	0.01	-0.01	0.01	-0.02	0.01
	(0.25)	(0.04)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)
Missing Father's Education	0.32	-0.03	-0.06	-0.04	-0.03	0.13*	0.02	-0.04
	(0.47)	(0.07)	(0.05)	(0.04)	(0.05)	(0.07)	(0.04)	(0.04)
Mother Speaks Indigenous Lang.	0.42	-0.38**	0.09	-0.01	0.08	0.15	-0.06	0.09
	(0.49)	(0.15)	(0.12)	(0.01)	(0.13)	(0.15)	(0.13)	(0.09)
Missing Mother's Language	0.03	0.00	-0.01	-0.03***	0.00	0.03*	0.02**	-0.01*
	(0.17)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Father Speaks Indigenous Lang.	0.43	-0.40**	0.09	-0.01*	0.07	0.14	-0.08	0.09
	(0.50)	(0.15)	(0.12)	(0.01)	(0.13)	(0.13)	(0.13)	(0.09)
Missing Father's Language	0.07	0.05***	-0.00	-0.01	-0.00	-0.03	-0.01	-0.00
	(0.25)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)
N Households	8296	833	1340	984	1266	355	1081	2437

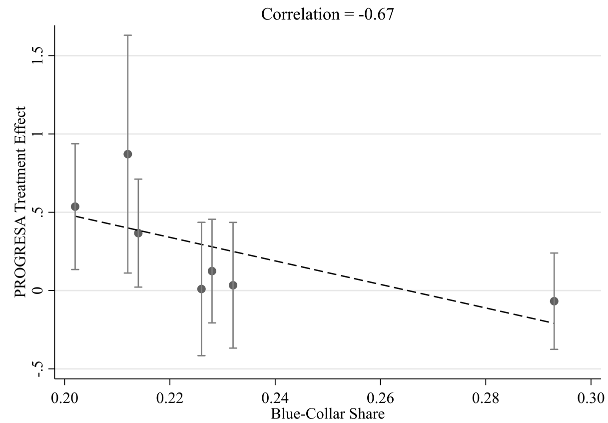
Notes: Summary statistics calculated from the baseline survey, restricting to children aged 6-15 at baseline, with a non-missing educational attainment variable in 2003. Standard deviations (in column 1) and standard errors clustered at village level (in columns 2-8) in parentheses (* p< 0.1, ** p< 0.05, *** p< 0.01).

Figure A1: State-Level Job Types and PROGRESA Impact

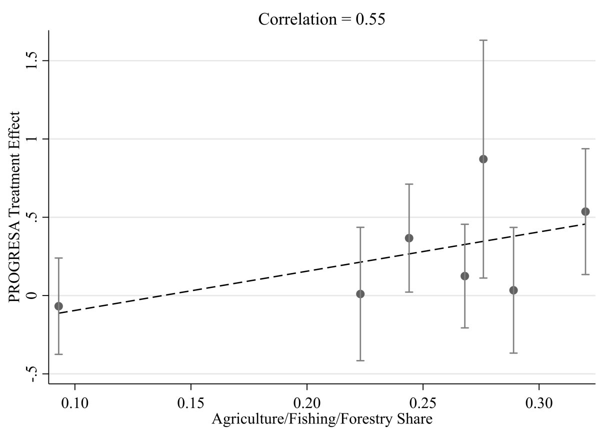
A. White-Collar



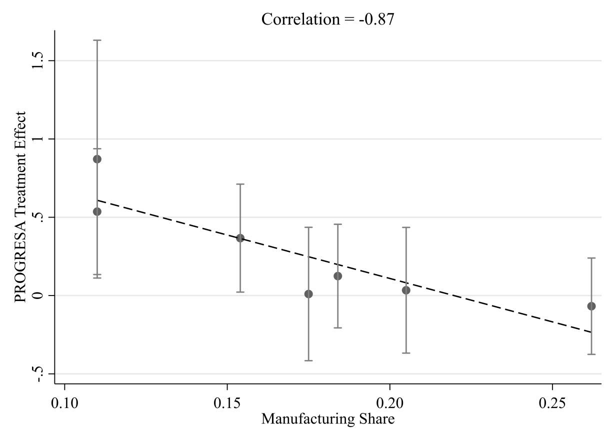
B. Blue-Collar



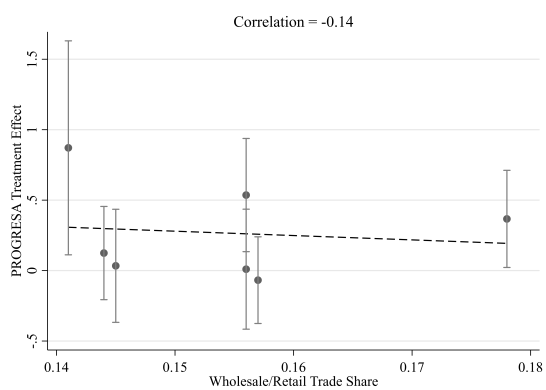
C. Agriculture, Fishing, & Forestry



D. Manufacturing

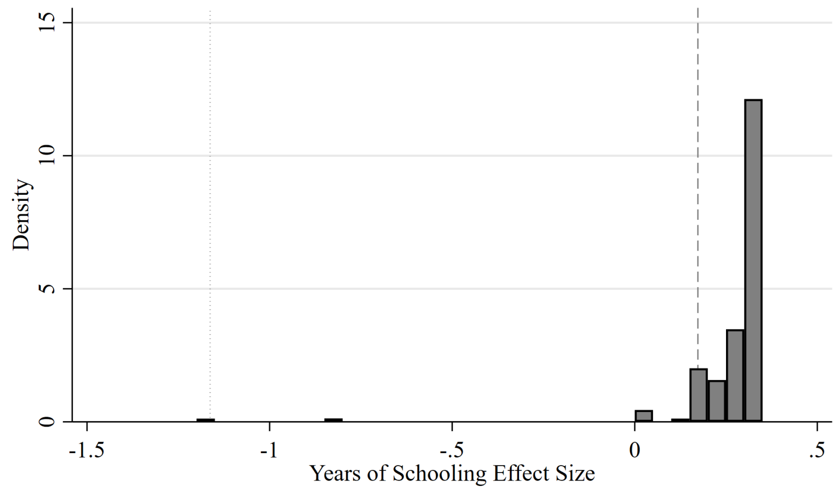


E. Wholesale & Retail Trade



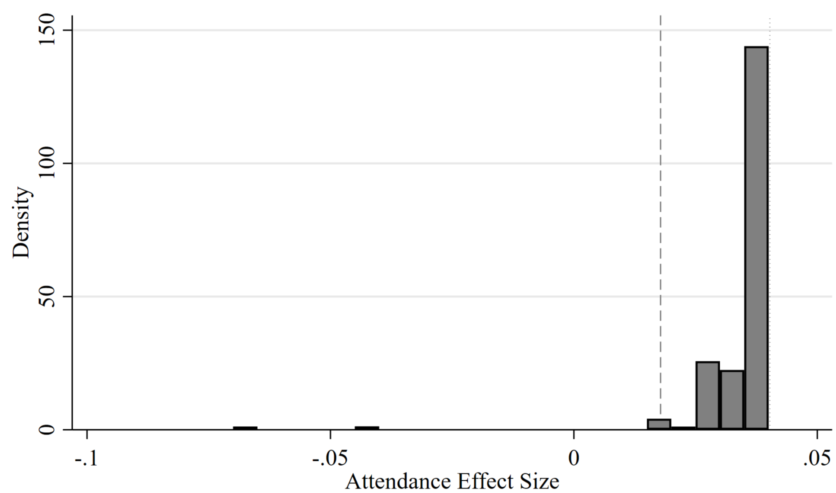
Notes: The x-axis in each panel represents the share of the state's workers in the specified occupation or industry. All of the occupation/industry variables are taken from the 2000 Mexican census. Coefficients (and 95% confidence intervals) are obtained from state-specific regressions of educational attainment in 2003 on a PROGRESA treatment village indicator, restricting to children aged 5 to 16 in 1997 in eligible (poor) households, controlling for age, gender, household size, household head age, household head gender, mother's and father's education, and dummies for mother's and father's indigenous language knowledge. Standard errors are clustered at the village level.

Figure A2: Distribution of PROGRESA Schooling Effects



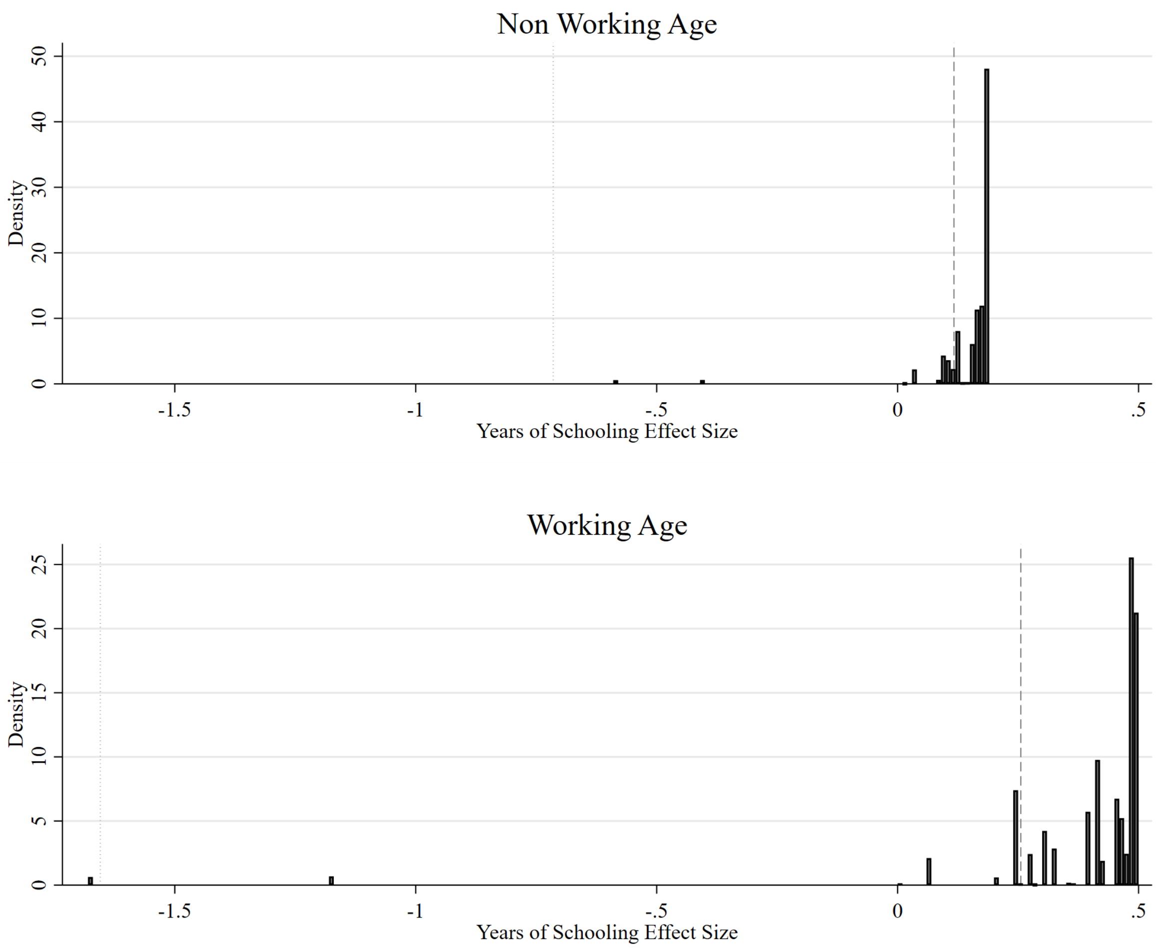
Notes: This figure plots the distribution of PROGRESA treatment effects, calculated from the results of column 4 of Table 3. Estimates to the left of the dotted line or to the right of the dashed line are significantly different from zero at the 5% level.

Figure A3: Distribution of PROGRESA Attendance Effects



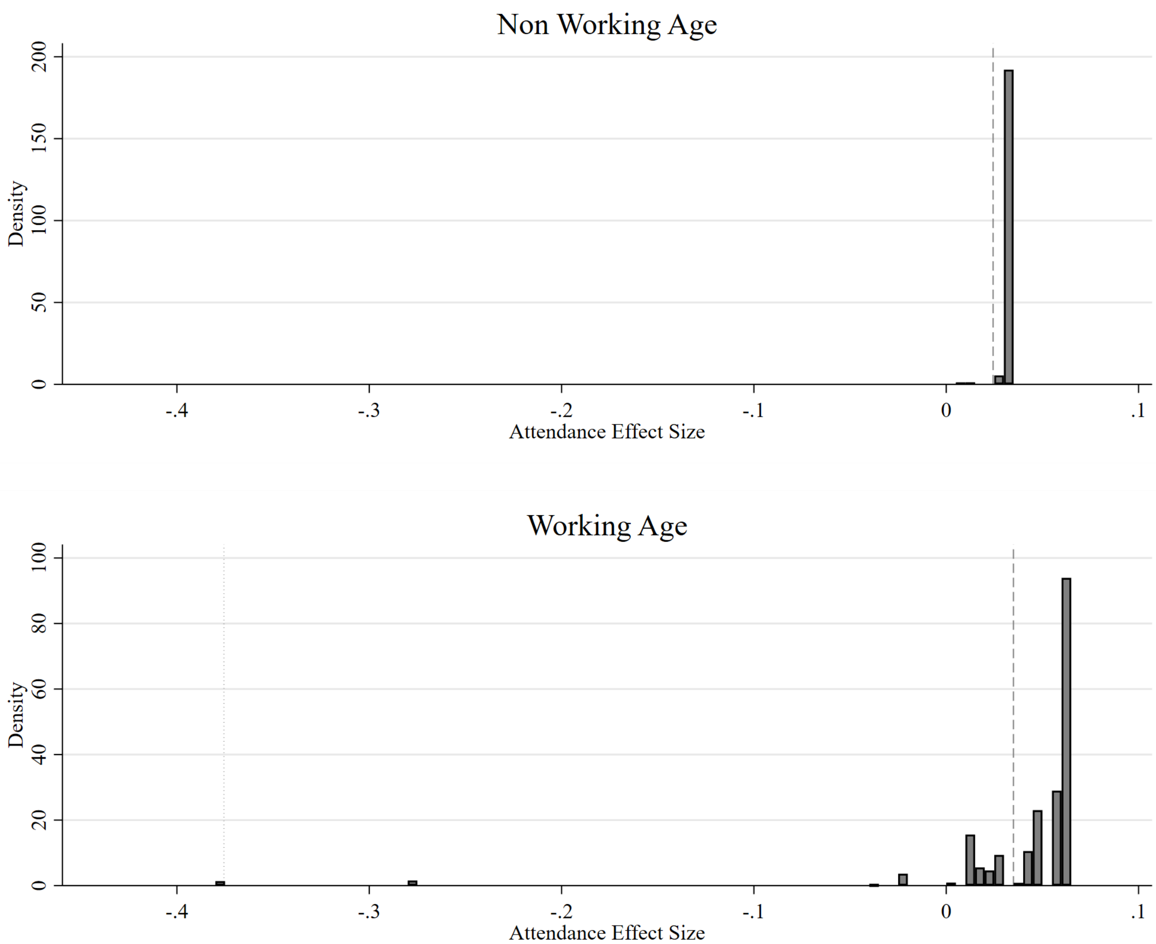
Notes: This figure plots the distribution of PROGRESA treatment effects, calculated from the results of column 6 of Table 4. Estimates to the right of the dashed line are significantly different from zero at the 5% level.

Figure A4: Distribution of PROGRESA Schooling Effects by Age



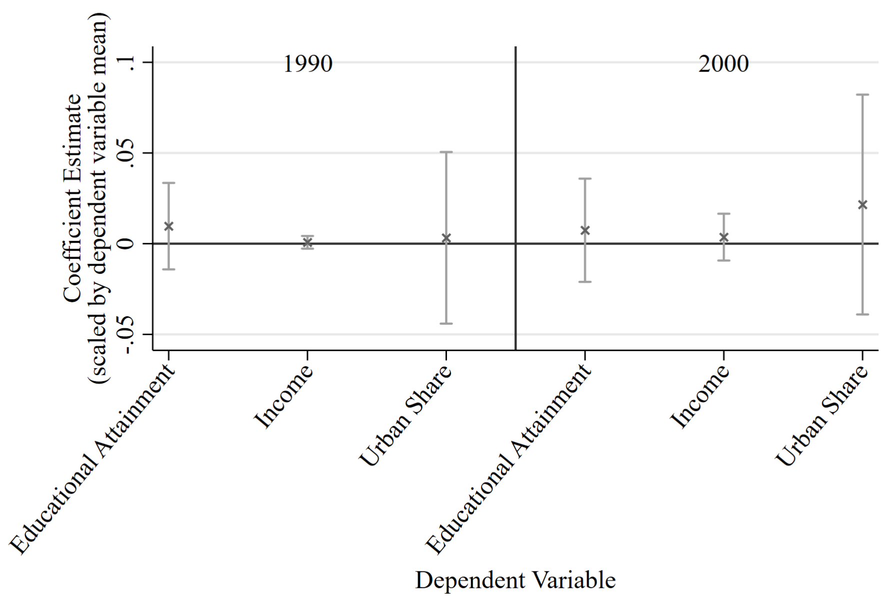
Notes: These figures plots the distribution of PROGRESA treatment effects separately for those younger than working age and those of working age, calculated from the results of columns 1 and 2 of Table 5. Estimates to the left of the dotted line or to the right of the dashed line are significantly different from zero at the 5% level.

Figure A5: Distribution of PROGRESA Attendance Effects by Age



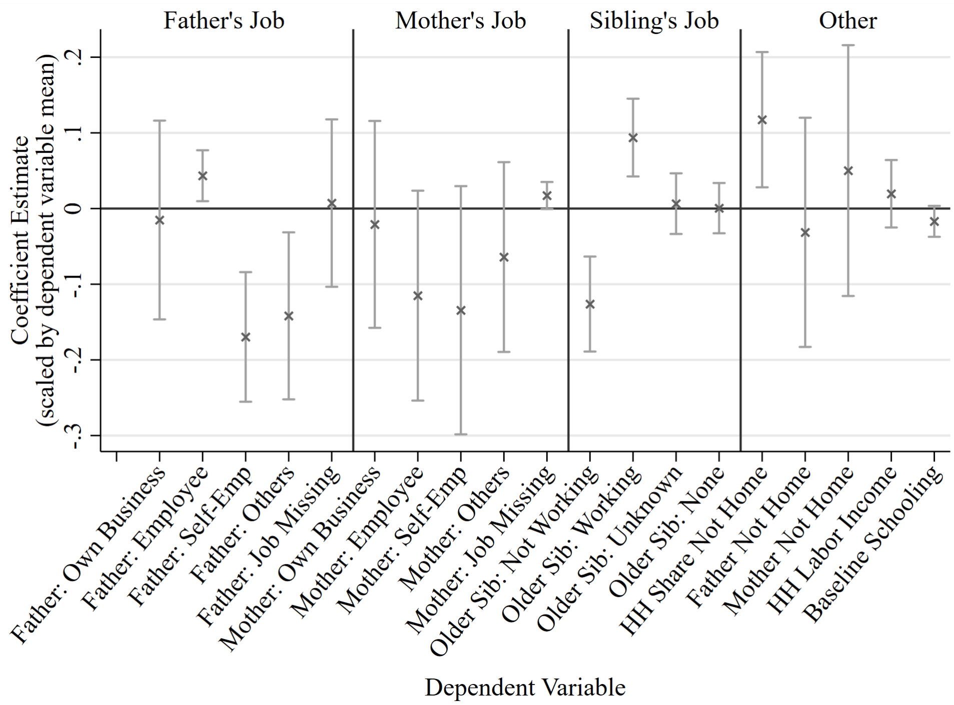
Notes: These figures plots the distribution of PROGRESA treatment effects separately for those younger than working age and those of working age, calculated from the results of columns 3 and 4 of Table 5. Estimates to the left of the dotted line or to the right of the dashed line are significantly different from zero at the 5% level. "Working Age" is defined as those older than 15 (for educational attainment regressions) or those currently aged 15 or older (for attendance regressions).

Figure A6: Export Jobs and Subdelegation Characteristics



Notes: Figure displays scaled coefficients and 95% confidence intervals (using robust standard errors) from six separate regressions, where the independent variable is the the number of export-oriented jobs in the subdelegation (in 1997 for the 1990 census and 2000 for the 2000 census), divided by the subdelegation's working-aged population according to the 1990 census, standardized. Dependent variables are subdelegation-level averages calculated from the 1990 or 2000 census (as specified).

Figure A7: Export Jobs and Household Characteristics



Notes: Figure displays scaled coefficients and 95% confidence intervals (using standard errors clustered at village level) from 20 separate regressions, where the independent variable is the number of export-oriented jobs in the subdelegation in 1997, divided by the subdelegation's working-aged population according to the 1990 census, standardized. These regressions use the 1997, 1998, and both 1999 survey waves, restricting to children aged 5 to 16 at baseline (in 1997), with the exception of the *Baseline Schooling* regression which restricts to the 1997 wave.

Table A3: Heterogeneous Effects of PROGRESA on Educational Attainment with Additional Interactions

	(1) Educational Attainment	(2) Educational Attainment	(3) Educational Attainment	(4) Educational Attainment	(5) Educational Attainment	(6) Educational Attainment	(7) Educational Attainment	(8) Educational Attainment	(9) Educational Attainment	(10) Educational Attainment
Treat x Export Jobs	-0.26	-0.27	-0.28	-0.24	-0.27	-0.28	-0.27	-0.27	-0.25	-0.21
	(0.12)**	(0.13)**	(0.13)**	(0.13)*	(0.13)**	(0.13)**	(0.13)**	(0.14)*	(0.13)*	(0.14)
Treat	0.20	0.21	0.21	0.21	0.22	0.056	0.21	0.26	0.21	0.17
	(0.099)**	(0.10)**	(0.10)**	(0.091)**	(0.097)**	(0.13)	(0.11)*	(0.099)***	(0.12)*	(0.14)
Treat x Female	-0.098	-0.098	-0.097	-0.071	-0.098	-0.099	-0.096	-0.098	-0.095	-0.071
	(0.083)	(0.083)	(0.083)	(0.079)	(0.083)	(0.083)	(0.083)	(0.084)	(0.083)	(0.079)
Observations	23272	23272	23272	23272	23272	23272	23272	23272	23272	23272
Mean of DV	6.894	6.894	6.894	6.894	6.894	6.894	6.894	6.894	6.894	6.894
Controls	All	All	All	All	All	All	All	All	All	All
Additional Treatment	Schooling	Income	Urban	OwnSchool	Migration	LaborIncome	FatherOcc	MotherOcc	SiblingWork	All
Interactions	(Census Avg)	(Census Avg)	(Census Avg)	(Individual)	(Household)	(Household)	(Household)	(Household)	(Household)	

Notes: Standard errors (clustered at village level) in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. These regressions use the 2003 survey wave, restricting to children aged 5 to 16 at baseline (in 1997). *Treat* is an indicator for PROGRESA treatment villages. *Export Jobs* is the ratio of the number of export-oriented jobs in the subdelegation in 1997, over the subdelegation's working-aged population according to the 1990 census, standardized. *All* controls include gender, cohort fixed effects, subdelegation fixed effects, a female-by-treatment interaction, household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values). Continuous variables used as additional treatment interactions are standardized, and missing values are replaced by the sample mean. Categorical variables are included as multiple dummy variable interactions, including a dummy for missing values; the omitted category is the modal category.

Table A4: Heterogeneous Effects of PROGRESA on School Attendance with Additional Interactions

	(1) School Attendance	(2) School Attendance	(3) School Attendance	(4) School Attendance	(5) School Attendance	(6) School Attendance	(7) School Attendance	(8) School Attendance	(9) School Attendance	(10) School Attendance
Treatd x Post x Export Jobs	-0.023	-0.024	-0.025	-0.023	-0.021	-0.021	-0.020	-0.021	-0.018	-0.024
	(0.0096)**	(0.010)**	(0.010)**	(0.0073)***	(0.0088)**	(0.0089)**	(0.0089)**	(0.0089)**	(0.0083)**	(0.0081)***
Treated x Post	0.030	0.031	0.032	0.043	0.026	0.036	0.024	0.031	0.027	0.067
	(0.0083)***	(0.0084)***	(0.0085)***	(0.0066)***	(0.0084)***	(0.011)***	(0.011)**	(0.015)**	(0.0085)***	(0.015)***
Treated x Post x Female	0.0086	0.0086	0.0086	0.0015	0.0084	0.0084	0.0084	0.0092	0.0081	0.00024
	(0.0061)	(0.0061)	(0.0061)	(0.0053)	(0.0061)	(0.0061)	(0.0061)	(0.0061)	(0.0060)	(0.0053)
Observations	95705	95705	95705	95705	95705	95705	95705	95705	95705	95705
Mean of DV	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833
Controls	All	All	All	All	All	All	All	All	All	All
Additional Treatment Interactions	Schooling (Census Avg)	Income (Census Avg)	Urban (Census Avg)	OwnSchool (Individual)	Migration (Household)	LaborIncome (Household)	FatherOcc (Household)	MotherOcc (Household)	SiblingWork (Household)	All

Notes: Standard errors (clustered at village level) in parentheses. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$. These regressions use the 1997, 1998, and both 1999 survey waves, restricting to children aged 5 to 16 at baseline (in 1997). *Treated*=1 if a village has PROGRESA at the time of the survey. *Export Jobs* is the ratio of the number of export-oriented jobs in the subdelegation in 1997, over the subdelegation's working-aged population according to the 1990 census, standardized. *Post* is an indicator for all waves after 1997. *All* controls include gender, cohort fixed effects, wave fixed effects, subdelegation fixed effects, a female indicator interacted with *Treated*-by-*Post*, household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values). Continuous variables used as additional treatment interactions are standardized, and missing values are replaced by the sample mean. Categorical variables are included as multiple dummy variable interactions, including a dummy for missing values; the omitted category is the modal category.

Table A5: Sample Attrition

	(1) In 2003	(2) In 2003
Treat x Export Jobs	-0.056	-0.054
	(0.024)**	(0.023)**
Treat	-0.026	-0.028
	(0.013)*	(0.013)**
Export Jobs	0.012	0.0097
	(0.019)	(0.018)
Observations	28591	28591
Mean of DV	0.814	0.814
Controls	Basic	All

Notes: Standard errors (clustered at village level) in parentheses. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$. These regressions restrict to children aged 5 to 16 at baseline (in 1997). The outcome variable is an indicator equal to 1 for individuals with a non-missing educational attainment variable in 2003. *Treat* is an indicator for PROGRESA treatment villages. *Export Jobs* is the ratio of the number of export-oriented jobs in the subdelegation in 1997, over the subdelegation's working-aged population according to the 1990 census, standardized. *Basic* controls include gender, cohort fixed effects, and subdelegation fixed effects. *All* controls add household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values).

Table A6: Heterogeneous Effects of PROGRESA on Educational Attainment, Inverse Probability Weighting

	(1) Educational Attainment	(2) Educational Attainment	(3) Educational Attainment	(4) Educational Attainment
Treat x Export Jobs	-0.33	-0.33	-0.27	-0.27
	(0.12)***	(0.12)***	(0.10)***	(0.10)***
Treat	0.18	0.23	0.17	0.21
	(0.095)*	(0.10)**	(0.085)**	(0.095)**
Export Jobs	0.11	0.10	0.051	0.040
	(0.14)	(0.14)	(0.14)	(0.14)
Treat x Female		-0.093		-0.094
		(0.085)		(0.083)
Observations	23272	23272	23272	23272
Mean of DV	6.87	6.87	6.87	6.87
Controls	Basic	Basic	All	All
p-value for sum	0.33	0.52	0.46	0.69

Notes: Standard errors (clustered at village level) in parentheses. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$. These regressions use the 2003 survey wave, restricting to children aged 5 to 16 at baseline (in 1997). *Treat* is an indicator for PROGRESA treatment villages. *Export Jobs* is the ratio of the number of export-oriented jobs in the subdelegation in 1997, over the subdelegation's working-aged population according to the 1990 census, standardized. *Basic* controls include gender, cohort fixed effects, and subdelegation fixed effects. *All* controls add household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values). *p-value for sum* reports the p-value testing the null hypothesis that $\beta_1 + \beta_2{=}0$. Observations are weighted by the inverse of the predicted probability of having non-missing educational attainment in 2003, predicted using a probit regression on *Treat*, *Export Jobs*, their interaction, and all three of these variables interacted with *All* controls.

Table A7: Heterogeneous Effects of PROGRESA on School Attendance, Inverse Probability Weighting

	(1) School Attendance	(2) School Attendance	(3) School Attendance	(4) School Attendance	(5) School Attendance	(6) School Attendance
Treat x Post x Export Jobs	-0.023	-0.022	-0.011	-0.022	-0.022	-0.012
	(0.0066)***	(0.0067)***	(0.0085)	(0.0066)***	(0.0067)***	(0.0083)
Treat x Post	0.030	0.027	0.035	0.030	0.026	0.033
	(0.0058)***	(0.0078)***	(0.0086)***	(0.0058)***	(0.0078)***	(0.0084)***
Export Jobs	-0.010	-0.0097	-0.0069	-0.011	-0.010	-0.0079
	(0.018)	(0.018)	(0.017)	(0.017)	(0.018)	(0.016)
Treat x Export Jobs	0.014	0.014		0.013	0.013	
	(0.0094)	(0.0096)		(0.0091)	(0.0092)	
Post x Export Jobs	-0.00070	-0.00072		-0.00089	-0.00092	
	(0.0035)	(0.0035)		(0.0035)	(0.0035)	
Treat	0.0095	0.010		0.0081	0.0091	
	(0.0078)	(0.0087)		(0.0076)	(0.0084)	
Observations	95705	95705	95705	95705	95705	95705
Mean of DV	0.83	0.83	0.83	0.83	0.83	0.83
Controls	Basic	Basic	Basic	All	All	All
Additional Treatment Interactions	None	By Female	By Female	None	By Female	By Female
p-value for sum	0.46	0.70	0.05	0.43	0.68	0.08

Notes: Standard errors (clustered at village level) in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. These regressions use the 1997, 1998, and both 1999 survey waves, restricting to children aged 5 to 16 at baseline (in 1997). *Treat* is an indicator for PROGRESA treatment villages. *Export Jobs* is the ratio of the number of export-oriented jobs in the subdelegation in 1997, over the subdelegation's working-aged population according to the 1990 census, standardized. *Post* is an indicator for all waves after 1997. *Basic* controls include gender, cohort fixed effects, wave fixed effects, and subdelegation fixed effects. *All* controls add household size, household head age, household head gender, as well as parental education and language dummies (including dummies for missing values). *By Female* treatment interactions include a female indicator interacted with *Treat*-by-*Post* (in all columns), in addition to a female indicator interacted with *Treat* and *Post* in columns 2 and 5. *p-value for sum* reports the p-value testing the null hypothesis that $\alpha_1 + \alpha_2$=0. Observations are weighted by the inverse of the predicted probability of having non-missing educational attainment in 2003, predicted using a probit regression on *Treat*, *Export Jobs*, their interaction, and all three of these variables interacted with *All* controls.

B Simulations

Figure B1 illustrates the results of three simulations of the model in section 2. In each panel, the two solid lines denote the marginal benefit and marginal cost functions, which are the left hand side and right hand side of equation (1), in a situation with no CCT. The intersection of the two solid lines, therefore, identifies the optimal amount of schooling without a CCT program. The dashed line depicts the marginal cost curve in the presence of a CCT program – it is lower than the original marginal cost curve and therefore results in a higher level of optimal schooling in all three panels.

We use panel A as our baseline case, a setting with very few export-oriented jobs. In this scenario, the optimal amount of schooling is 6.3 years without a CCT program but 7.7 years with a CCT program. That is, a CCT program increases schooling by 1.4 years.

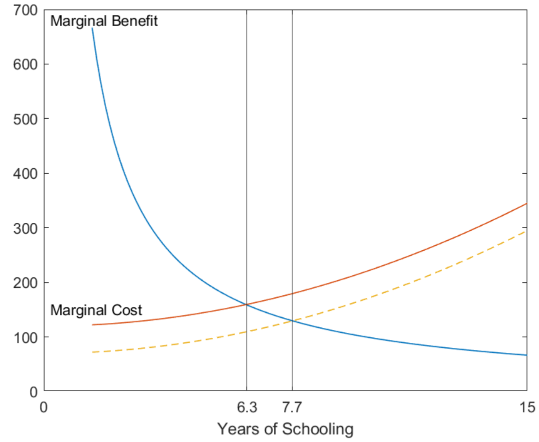
Using the same marginal benefit function, panels B and C illustrate two possible marginal cost curves for a setting with many export-oriented jobs. Because we know from Atkin (2016) that the expansion of export manufacturing led to lower levels of schooling, both of these panels depict a marginal cost curve that leads to a lower level of schooling without a CCT program than the baseline case: 5.5 years (compared to 6.3 years) in both cases.

Despite the fact that the marginal cost curves in panels B and C result in the same level of schooling in a scenario without a CCT program, the changes in schooling generated by a CCT program are different. In Panel B, where the marginal cost curve is flatter, schooling increases by 1.9 years – more than in the baseline case. In Panel C, however, where the marginal cost curve is steeper (reflecting a more convex cost function), schooling increases only by 1.1 years – the smallest increase across all three examples.

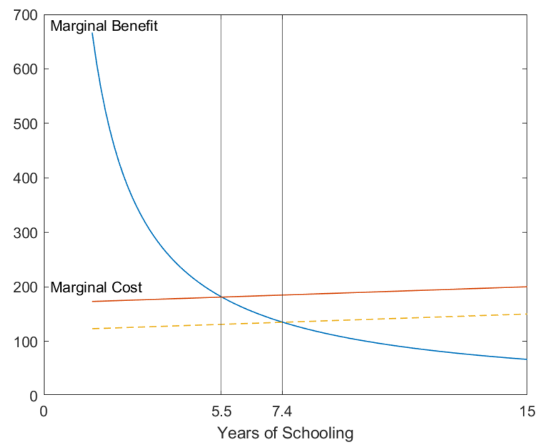
In short, the fact that export-oriented manufacturing led to lower levels of schooling in our setting does not allow us to predict whether it will increase or decrease the schooling impact of a CCT program in the same context.

Figure B1: Simulations of the Optimal Schooling Response to a Price Reduction

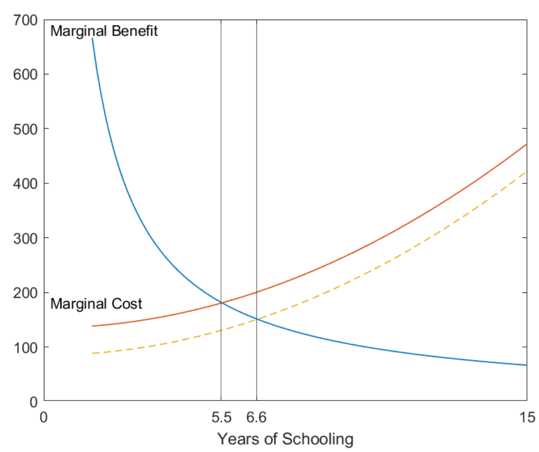
A. Baseline Case



B. Export Manufacturing Setting, Example 1



C. Export Manufacturing Setting, Example 2



Notes: Marginal benefits and marginal costs (as a function of years of schooling, S) are defined as follows:
Marginal benefit (in all panels): $MB(S) = 1000/S$
Marginal cost (Panel A): $MC(S) = S^2 + 120$
Marginal cost (Panel B): $MC(S) = 2S + 170$
Marginal cost (Panel C): $MC(S) = 1.5S^2 + 135$