

DISCUSSION PAPER SERIES

IZA DP No. 14667

**Conditional Cash Transfers and Labor
Market Conditions**

Teresa Molina
Joaquim Vidiella-Martin

AUGUST 2021

DISCUSSION PAPER SERIES

IZA DP No. 14667

Conditional Cash Transfers and Labor Market Conditions

Teresa Molina

*University of Hawaii at Manoa
and IZA*

Joaquim Vidiella-Martin

*Erasmus School of Economics
and University of Oxford*

AUGUST 2021

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

ABSTRACT

Conditional Cash Transfers and Labor Market Conditions*

Do local labor markets influence the effectiveness of educational policies? To answer this question, we focus on Mexico's conditional cash transfer program, PROGRESA, documented to have increased educational attainment. We show that PROGRESA's impact on schooling was smaller in areas with more export-oriented manufacturing jobs and argue 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.

JEL Classification: I28, F16, I38, O14

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

Corresponding author:

Teresa Molina
University of Hawaii at Manoa
2424 Maile Way
Saunders Hall 515A
Honolulu, HI 96822
USA
E-mail: tmolina@hawaii.edu

* We thank Achyuta Adhvaryu, David Atkin, Chris Karbownik, Anant Nyshadham, Owen O'Donnell, Tom Vogl, and seminar participants at BREAD, UVA, NYU, Emory, JEES, UWA, and UH Manoa for helpful comments.

1 Introduction

The effects of policies that reduce the cost of schooling vary widely across settings. Impacts of financial aid policies on college attendance rates range from 0 to 10 percentage points (Herbaut and Geven, 2020), while evaluations of private school voucher programs have reported both positive and negative effects on graduation rates (Epple et al., 2017). The effectiveness of conditional cash transfer (CCT) programs, which have attracted much attention in the economics literature, also vary widely across and within settings (Fiszbein and Schady, 2009; Glewwe and Muralidharan, 2016; Molina-Millán et al., 2019).¹ In fact, we find striking geographic heterogeneity in the effectiveness of a single program in a single country. As we show in Figure 1, Mexico’s PROGRESA program had large, positive, and statistically significant effects in three out of seven states, but statistically insignificant effects close to zero in the remaining four states.²

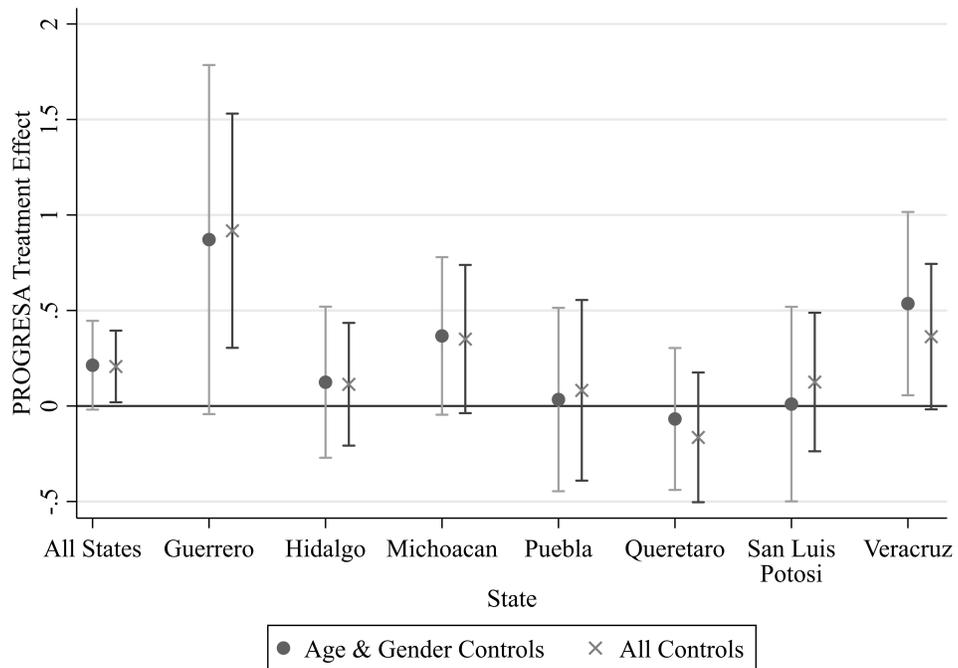
What drives this heterogeneity in the effectiveness of education policies? We begin by seeking insight from a model of the optimal schooling decision, which suggests that labor market conditions could be important. In this model, the parameter of interest is the response of optimal schooling to a price reduction. We show that the magnitude of this response depends primarily on the convexities of the opportunity cost and wage functions, both of which are determined by the types of jobs that are available to the individual. Because these convexities are typically hard to measure, it is difficult to predict how a specific occupation or industry composition will moderate the effect of a schooling price reduction. We therefore shift to an empirical investigation of this question, focusing on Mexico’s PROGRESA program as our education policy of interest.

PROGRESA was initially rolled out experimentally, with a randomly selected set of treatment villages obtaining the program in 1998 and the remaining control villages receiv-

¹For example, Fiszbein and Schady (2009) document impacts on attendance rates ranging from -3 to 31 percentage points.

²Table A1, which reports treatment-control differences 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.

Figure 1: PROGRESA Impact on Educational Attainment Across States



Notes: Coefficients (and 95% confidence intervals) are obtained from a regression of educational attainment in 2003 on a PROGRESA treatment locality indicator, restricting to children aged 5 to 16 in 1997 in eligible (poor) households. State-specific coefficients are obtained using separate regressions for each state. “All Controls” include 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 locality level.

ing it in 1999. As a result, causal estimates of the program’s impact can be obtained by comparing outcomes of eligible households in treatment and control villages, both of which were surveyed in several rounds of follow-up. Examining school attendance in 1998 and 1999 (before the control group was exposed to the PROGRESA program), we obtain estimates of PROGRESA’s contemporaneous effect. Using data from 2003, we compare educational attainment in treatment and control villages in order to obtain estimates of PROGRESA’s longer-run impact.

We first document a strong negative correlation between the magnitude of a state’s PROGRESA impact and its share of workers in blue-collar and manufacturing jobs, calculated from Mexican census data. While these results support the hypothesis that labor market conditions can moderate CCT effectiveness, it is difficult to rule out other explanations without better data and more granular geographic variation. We therefore focus on a specific type of blue-collar, manufacturing job that played an important role in Mexico’s development in the 1990s: export-oriented manufacturing. Because the availability of these jobs in a given location is determined in part by external demand shocks (and not just local demand and supply), export manufacturing shares have the advantage of being less correlated with local characteristics than overall blue-collar or manufacturing shares. In addition, because export manufacturing jobs are typically in the formal sector, this allows us to use data from the Mexican Social Security Institute (IMSS), which has several advantages over the 10% sample of the Mexican census.

Shifting our attention to export-oriented manufacturing, we find that the impact of PROGRESA on attendance rates and eventual educational attainment was smaller in subdelegations with more export jobs.³ This result comes from combining PROGRESA data with IMSS data, from which we calculate the number of export-oriented manufacturing jobs (for men and women separately) in each month and each subdelegation (a geographic region larger than both village and municipality, but smaller than state). We regress our educational

³Throughout the paper we use ‘export-oriented manufacturing jobs’ and ‘export jobs’ interchangeably.

outcomes on a PROGRESA treatment dummy and an interaction between PROGRESA treatment and subdelegation-level gender-specific export jobs (controlling for a rich set of demographic controls and fixed effects), and estimate a negative and significant interaction coefficient.

To understand why export manufacturing jobs lead to 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, which – in conjunction with the model – 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 this opportunity cost channel. First, we find that heterogeneity is stronger for those who are old enough to be working in factory jobs. In addition, the heterogeneity is driven primarily by the types of export jobs that are likely to factor into the opportunity cost of schooling: low wage jobs and those held by young workers. These findings suggest that export-oriented manufacturing jobs reduce the PROGRESA impact because they translate into more rapidly increasing foregone 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.

These findings speak to a broader empirical literature showing how schooling levels are influenced by opportunity costs and (perceived) returns to schooling (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., 2010, 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 an education policy.

Given the widespread popularity and use of CCTs in vastly different settings, the finding that labor market conditions can affect CCT effectiveness is important. Previous work has examined heterogeneity in the effect of CCTs 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; Maluccio and Flores, 2005), and other household characteristics (Angelucci et al., 2010; Djebbari and Smith, 2008; Handa et al., 2010). However, our documentation of heterogeneity driven by labor market conditions highlights the need to consider whether contemporaneous labor market policies could hinder or enhance CCT effectiveness.

This study also contributes to the literature on Mexico’s rapid trade liberalization in the late 1900’s. 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 affected the effectiveness of the PROGRESA program.

Finally, this study also contributes to 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 effectiveness of policies that reduce the price of schooling. Suppose parents maximize discounted future wages minus the opportunity cost of schooling:

$$\beta W(S) - c(S) - pS,$$

where wages are a function of schooling ($W(S)$), and opportunity costs are composed of foregone wages $c(S)$ and the price of school 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.⁴ In addition, certain jobs, which are available to school-aged youth, can affect the opportunity cost of schooling.

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

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

⁴The 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.

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 average 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 foregone wages ($W(S) - c(S)$), which we refer to as 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 (1). Because it is easier to characterize labor markets based on their first derivatives (whether there are high returns to schooling, or high marginal opportunity costs), the importance of the second derivatives makes it difficult to predict which types of labor markets will enhance or reduce the effectiveness of these types of education policies.

We therefore turn to an empirical analysis of this question, focusing on a specific education policy: Mexico’s conditional cash transfer program, PROGRESA, which we describe in the next section.

3 Background

CCTs are now widely used across the globe (World Bank Group, 2017), but one of the first CCT programs, Programa de Educación, Salud y Alimentación (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 2003.⁵ 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 localities in seven states: Guerrero, Hidalgo, Michoacán, Puebla, Queretaro, San Luis Potosí and Veracruz. Localities were randomized into either treatment or control: the treatment group (320 localities) started receiving benefits in the spring of 1998 and the control group (186 localities) 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

⁵See Skoufias and Parker (2001), Skoufias (2005), Behrman et al. (2009a), and Behrman et al. (2011) for more program details.

not yet been rolled out to the control group), and show that PROGRESA increased school attendance, enrollment, and grade progression, and reduced dropout (Behrman et al., 2005; Schultz, 2004; Skoufias and Parker, 2001). Longer-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).⁶ Because the control group was already exposed to PROGRESA by this time, these estimates capture the effect of being exposed to two additional years of PROGRESA due to living in a treatment locality.

4 Data

We combine three sources of data to explore the heterogeneous effects of PROGRESA due to local labor market conditions. We merge the data collected for the evaluation of the PROGRESA program with Mexican census data collected by the National Institute of Statistics, Geography, and Informatics (INEGI), and employment data from the Mexican Social Security Institute (IMSS).

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 localities 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 the 320 localities

⁶Two more follow-up surveys were collected in 2007 and 2017. These are not often used in the economics literature due to the large rates of attrition.

placed in the treatment group, and value zero for the 186 localities 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). This consists of over 27,000 individuals, belonging to over 8,000 households in 506 localities.

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. 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 Census Data

To capture geographic variation in the types of jobs that are available in a specific location, we use the 10% samples of the 1990 and 2000 Mexican censuses provided by IPUMS (Minnesota Population Center, 2015). We explore job types as defined by both occupation and industry. Specifically, we calculate the share of workers in a given region (either a state or subdelegation) that are in white-collar and blue-collar jobs, where we define white-collar as ISCO occupation codes 1-3 and blue-collar as codes 8-9.⁷ We also calculate the share of workers that are in each of the following three industries, which were the largest three industries in Mexico in both the 1990 and 2000 censuses: agriculture/fishing/forestry, manufacturing,

⁷White-collar jobs include legislators, senior officials and managers; professionals; and technicians and associate professionals. Blue-collar jobs include plant and machine operators and assemblers as well as elementary occupations.

Table 1: Summary Statistics for Individual and Household Characteristics

Variable	Mean			Difference
	(1) Full sample	(2) Treatment	(3) Control	(4) Treatment - Control
Age	10.00 (3.32)	9.99 (3.32)	10.01 (3.32)	-0.01 (0.05)
Female	0.48 (0.50)	0.48 (0.50)	0.49 (0.50)	-0.01 (0.01)
Attending School	0.85 (0.36)	0.85 (0.36)	0.84 (0.36)	0.00 (0.01)
Educational Attainment	3.39 (2.71)	3.38 (2.69)	3.40 (2.76)	-0.02 (0.07)
<i>N Individuals</i>	23,272	14,420	8,852	
Household Size	6.67 (2.16)	6.67 (2.16)	6.67 (2.16)	-0.00 (0.07)
Household Head Age	42.02 (12.13)	41.80 (11.96)	42.39 (12.40)	-0.59 (0.37)
Female Household Head	0.07 (0.25)	0.07 (0.25)	0.07 (0.25)	-0.00 (0.01)
No. Children Aged 0-2	0.55 (0.66)	0.55 (0.66)	0.55 (0.66)	0.00 (0.02)
No. Children Aged 3-5	0.74 (0.73)	0.74 (0.73)	0.73 (0.73)	0.00 (0.02)
No. Females Aged 6-7	0.27 (0.47)	0.27 (0.47)	0.28 (0.47)	-0.01 (0.01)
No. Females Aged 8-12	0.64 (0.74)	0.63 (0.74)	0.64 (0.75)	-0.01 (0.02)
No. Females Aged 8-12	0.50 (0.73)	0.50 (0.73)	0.51 (0.73)	-0.01 (0.02)
No. Males Aged 6-7	0.28 (0.48)	0.28 (0.47)	0.28 (0.48)	-0.00 (0.01)
No. Males Aged 8-12	0.67 (0.75)	0.68 (0.75)	0.66 (0.74)	0.02 (0.02)
No. Males Aged 13-18	0.54 (0.76)	0.55 (0.77)	0.53 (0.74)	0.01 (0.02)
No. Females Aged 19-54	1.12 (0.51)	1.12 (0.52)	1.12 (0.51)	0.00 (0.01)
No. Females Aged 55+	0.15 (0.37)	0.14 (0.37)	0.16 (0.38)	-0.01 (0.01)
No. Males Aged 19-54	1.03 (0.56)	1.04 (0.57)	1.03 (0.54)	0.01 (0.02)
No. Males Aged 55+	0.16 (0.37)	0.16 (0.37)	0.16 (0.38)	-0.01 (0.01)
Mother's Education	1.05 (0.23)	1.05 (0.23)	1.06 (0.24)	-0.01 (0.01)
Missing Mother's Education	0.39 (0.49)	0.38 (0.49)	0.40 (0.49)	-0.02 (0.03)
Father's Education	1.06 (0.26)	1.07 (0.26)	1.06 (0.27)	0.01 (0.01)
Missing Father's Education	0.32 (0.47)	0.32 (0.47)	0.33 (0.47)	-0.01 (0.02)
Mother Speaks Indigenous Lang.	0.42 (0.49)	0.42 (0.49)	0.43 (0.49)	-0.01 (0.06)
Missing Mother's Language	0.03 (0.17)	0.03 (0.17)	0.03 (0.18)	-0.00 (0.00)
Father Speaks Indigenous Lang.	0.43 (0.50)	0.43 (0.49)	0.44 (0.50)	-0.02 (0.06)
Missing Father's Language	0.07 (0.25)	0.07 (0.25)	0.07 (0.25)	-0.00 (0.01)
<i>N Households</i>	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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

and wholesale/retail trade. All calculations use the person-level population weights provided by the census.

4.3 IMSS Data

For the part of our analysis that focuses on export-oriented manufacturing, we use data 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.

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 the 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.

⁸The 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 at the village-year level, both pooled and separately by treatment arm. On average, 2.5% of jobs are categorized as export-oriented. 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 for the Share of Export-oriented Manufacturing Jobs

Variable	Mean			Difference
	(1) Full sample	(2) Treatment	(3) Control	(4) Treatment - Control
All Jobs	0.029 (0.059)	0.025 (0.040)	0.035 (0.081)	-0.010 (0.006)
Female	0.027 (0.065)	0.023 (0.043)	0.034 (0.090)	-0.011 (0.007)
Male	0.031 (0.055)	0.028 (0.039)	0.036 (0.074)	-0.008 (0.006)
High Wage	0.012 (0.043)	0.010 (0.026)	0.017 (0.062)	-0.007 (0.005)
Low Wage	0.016 (0.022)	0.015 (0.020)	0.018 (0.025)	-0.003 (0.002)
Young	0.012 (0.022)	0.010 (0.017)	0.014 (0.030)	-0.004 (0.002)
Old	0.017 (0.037)	0.015 (0.024)	0.021 (0.052)	-0.006 (0.004)
<i>N Village x Year</i>	2,497	1,583	914	

Notes: Summary statistics calculated at year-village level, restricting to villages with at least one child 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.4 Motivating Evidence

In this section, we present preliminary evidence for the hypothesis that job types play a role in determining CCT effectiveness. As discussed in section 2, job types can influence the slopes of the marginal benefit and cost functions, which could in turn determine the magnitude of the effect of a schooling price reduction. Figure 1 documented substantial heterogeneity across states in the effectiveness of PROGRESA; in Figure 2, we explore the extent to which state-level differences in job types could be driving this. In each panel, we show the relationship between the state-level PROGRESA schooling effect (plotted in

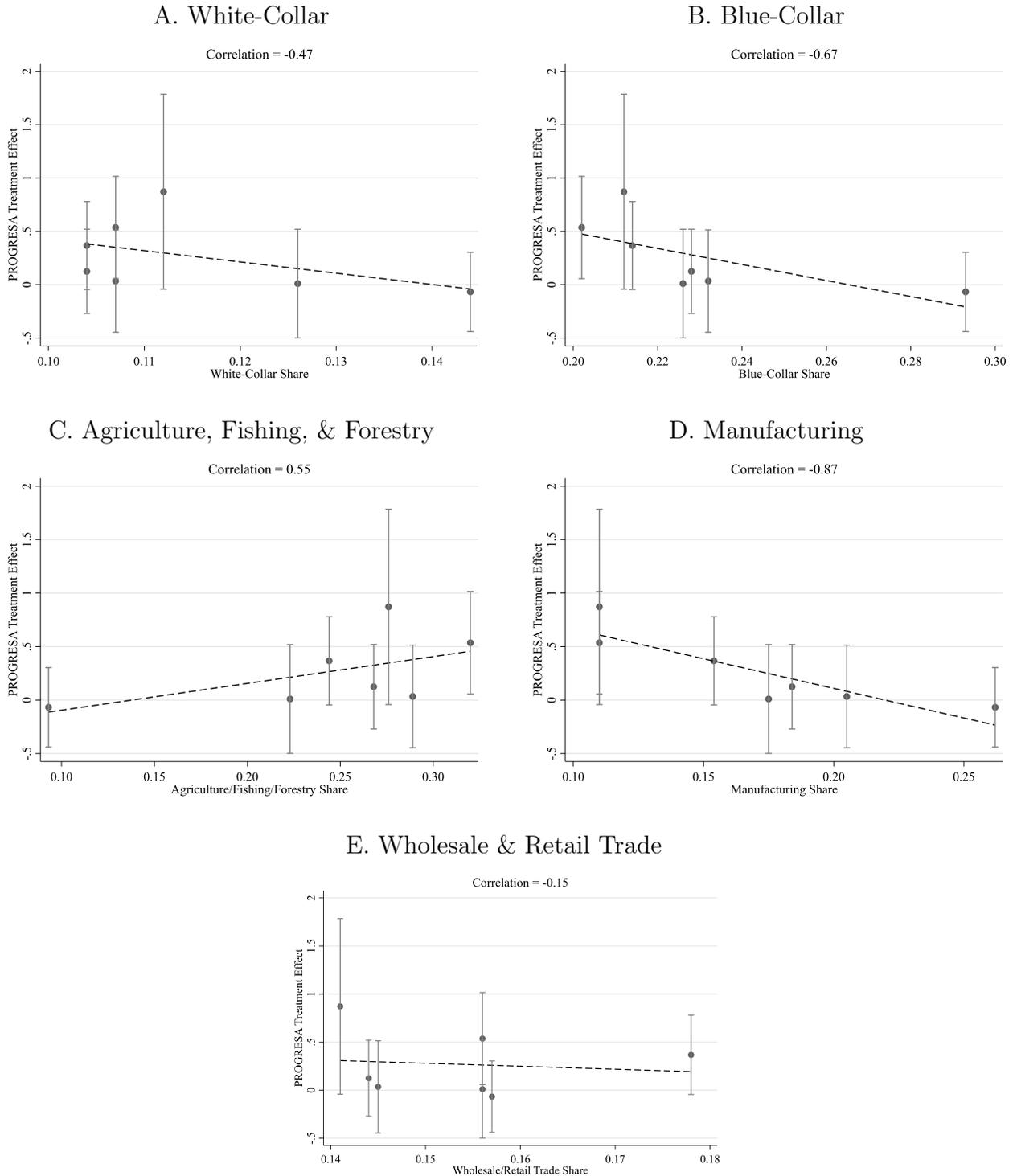
Figure 1) and the state-level share working in a particular occupation or industry: white-collar, blue-collar, agriculture, manufacturing, and trade. For some job types, especially blue-collar and manufacturing shares, there are strong negative relationships between the two variables. Blue-collar shares and PROGRESA treatment effects have a correlation of -0.67, while manufacturing shares and PROGRESA treatment effects have a correlation of -0.87. PROGRESA had the largest effects in states with lower shares of blue-collar and manufacturing jobs, which suggests that occupation and industry composition may have played a role in determining the program's effectiveness. Correlations between PROGRESA treatment effects and other occupation and industry shares are weaker.

Of course, there are many state-level characteristics that are correlated with occupation and industry composition which could be responsible for these relationships. In particular, it might be the case that some states were simply better at implementing the program than others (and this institutional quality could be correlated with labor market conditions).

Appendix Figure A1 suggests this is not the case. If implementation quality were an important factor, we would expect to see a strong correlation between the size of a state's PROGRESA effect on educational attainment and that state's PROGRESA effect on health. PROGRESA is documented to have reduced morbidity for young children (Gertler, 2004); we should therefore see a negative correlation between state-level morbidity effects and education effects. Instead, when we plot state-level PROGRESA morbidity effects on the y-axis against state-level PROGRESA education effects on the x-axis, there is no clear visual relationship and only a small positive (rather than negative) correlation.

Exploring other state-level characteristics, we show in panels A and B of Appendix Figure A2 that the relationships between the PROGRESA schooling effect and state-level average income and urban shares are much weaker than the relationships with manufacturing and blue-collar shares. However, in Panel C, we show that average educational attainment also has a strong negative relationship with the PROGRESA schooling effect. Although the correlation between these two variables is slightly lower than the manufacturing share corre-

Figure 2: State-Level Job Types and PROGRESA Impact



Notes: The x-axis in each panel represents the share of the state’s workers in the specified occupation or industry. All of occupation/industry variables are taken from the 2000 census. Coefficients (and 95% confidence intervals) are obtained from state-specific regressions of educational attainment in 2003 on a PROGRESA treatment locality indicator, restricting to children aged 5 to 16 in 1997 in eligible (poor) households, controlling for 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 locality level.

lation, this highlights that job types may not be the only important driver of the state-level heterogeneity documented here.

With only seven states, it is difficult to pinpoint which characteristics are the most important drivers of the heterogeneity documented in this section. In our main analysis, described in the following section, we use smaller geographic areas (subdelegations) and shift our focus to export manufacturing, an industry that played an important role in Mexico's development in the 1990's.⁹

5 Empirical Strategy

Having established that the effect of PROGRESA varied widely across states, and that this variation is highly correlated with state-level blue-collar and manufacturing shares, we shift our attention to a specific type of blue-collar, manufacturing job in order to more rigorously examine these relationships. Specifically, we examine how the effectiveness of PROGRESA varied across areas with varying exposure to export manufacturing.

5.1 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).

⁹In a large share of municipalities (which are the smallest possible aggregation level) there is no variation in PROGRESA treatment, since all villages in them are either in the treatment or control group. Meanwhile, aggregating at state level would leave us only with 7 data points. As a result, we choose subdelegations as the unit of aggregation for labor market data.

As a result of this shift in policy, Mexico saw a large increase in manufacturing jobs 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). This 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.¹⁰

Focusing on export-oriented manufacturing jobs, which are typically in the formal sector, allows us to make use of the IMSS data, which have a number of advantages over the census data used in the previous section. First, the IMSS is available for every year from 1997 to 2003 (as opposed to only in 1990 and 2000). Second, the IMSS provides counts from the entire universe of formal sector jobs, whereas the census data used is only a 10% sample.

Another benefit of focusing on export-oriented manufacturing jobs is that they tend to be driven in large part by external demand shocks, not just by local demand and supply. Because of this, shares of export manufacturing jobs tend to be less strongly 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.

¹⁰To 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 displayed 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 up to 660 pesos (Behrman et al., 2011).

5.2 Specification

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 1997.¹¹

5.2.1 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 two additional years. To estimate the heterogeneous effects of this additional exposure, we estimate the following specification:

$$E_{igjs} = \beta_1 T_j \bar{J}_{sg} + \beta_2 T_j + \beta_3 \bar{J}_{sg} + \beta_4 X_{ig} + \mu_s + \epsilon_{igjs}. \quad (2)$$

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. \bar{J}_{sg} is the number of export-oriented jobs in subdelegation s for gender g , averaged over the 1997-2003 period (as a fraction of the subdelegation’s working-aged population according to the 1990 census). To facilitate the interpretation of coefficient magnitudes, we standardize this variable. This means that β_2 represents the effect of PROGRESA for a subdelegation

¹¹If 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 two years 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.

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.

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 category dummies, and maternal and paternal language dummies.¹² Because our export jobs variable (\bar{J}_{sg}) is gender-specific, to ensure that β_1 is not capturing gender differences in PROGRESA's effectiveness, 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.

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. For example, we calculate subdelegation-level average schooling, income, and urban shares from the 2000 census, and add interactions between the treatment dummy and each of these variables (in separate regressions).¹³ We also include the main effects and treatment interactions of the following variables (all taken from the 1997 survey): child i 's baseline educational attainment, father and mother occupation category dummies, older sibling work status, household per capita labor income, and 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).

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

¹³The main effects are absorbed by subdelegation fixed effects μ_s .

5.2.2 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

$$A_{igjsw} = \alpha_1 T_j P_w J_{sg,w} + \alpha_2 T_j P_w + \alpha_3 T_j J_{sg,w} + \alpha_4 P_w J_{sg,w} + \alpha_5 T_j + \alpha_6 J_{sg,w} + \alpha_6 X_{ig} + \mu_s + \delta_w + \epsilon_{igjsw}. \quad (3)$$

A_{igjsw} is a school attendance dummy variable. Because we have multiple waves of data, the export jobs variable $J_{sg,w}$ is now time-varying. It captures the number of export-oriented jobs in subdelegation s (as a fraction of the working-aged population and standardized, as above) in the year prior to survey wave w .¹⁴ 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 (measured in post-treatment waves) across areas with varying export job exposure. Including the baseline wave helps improve statistical precision and also builds in a few 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 $T_j P_w$. We once again cluster standard errors at the village level.

We conduct a similar robustness exercise to the one described above. We add, in separate regressions, treatment interactions (and the main effects of) various subdelegation and household-level characteristics. Subdelegation-level schooling, income, and urban shares are

¹⁴The available IMSS data starts in August 1997. Therefore, for the first wave the number of export-oriented manufacturing jobs is calculated 3 months before the survey date.

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 (2). In column 1, we estimate that PROGRESA increased educational attainment by 0.16 years for areas with the average number of export jobs.¹⁵ For an area that lies one standard deviation above the mean in terms of export jobs, the interaction coefficient of -0.35 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 A3, we show the entire distribution of treatment effect magnitudes: the vast majority are positive, and only a small share are negative.

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.

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 having two additional years of the program. We now move on to

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

Table 3: Heterogeneous Effects of PROGRESA on Educational Attainment

	(1)	(2)	(3)	(4)
	Educational Attainment	Educational Attainment	Educational Attainment	Educational Attainment
Treat x Export Jobs	-0.36 (0.15)**	-0.36 (0.15)**	-0.26 (0.13)**	-0.27 (0.13)**
Treat	0.16 (0.096)*	0.21 (0.11)**	0.16 (0.086)*	0.20 (0.097)**
Export Jobs	0.14 (0.19)	0.13 (0.20)	0.042 (0.19)	0.034 (0.19)
Treat x Female		-0.097 (0.085)		-0.095 (0.083)
Observations	23272	23272	23272	23272
Mean of DV	6.894	6.894	6.894	6.894
Controls	Basic	Basic	All	All

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 (averaged over the 1997-2003 period), 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 category dummies (including dummies for missing values).

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. In addition to providing validation for our educational attainment results, this approach is useful because it allows us to use time-varying export job variables and some built-in falsification checks.

The attendance results are reported in Table 4. In columns 1, 2, 4, and 5, we report the results of equation (3). 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 A4) 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* (which captures the difference between treatment and control when *Post* is zero) shows that 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	-0.018 (0.0082)**	-0.017 (0.0083)**	-0.017 (0.0091)*	-0.019 (0.0082)**	-0.018 (0.0083)**	-0.017 (0.0088)*
Treat x Post	0.030 (0.0060)***	0.027 (0.0079)***	0.032 (0.0085)***	0.030 (0.0060)***	0.027 (0.0079)***	0.031 (0.0083)***
Export Jobs	-0.012 (0.017)	-0.012 (0.017)	-0.011 (0.016)	-0.013 (0.017)	-0.013 (0.017)	-0.012 (0.016)
Treat x Export Jobs	0.00064 (0.011)	0.000033 (0.011)		0.0015 (0.011)	0.00086 (0.011)	
Post x Export Jobs	0.0010 (0.0038)	0.00096 (0.0038)		0.00098 (0.0037)	0.00092 (0.0038)	
Treat	0.0057 (0.0078)	0.0064 (0.0087)		0.0049 (0.0076)	0.0057 (0.0085)	
Observations	95705	95705	95705	95705	95705	95705
Mean of DV	0.833	0.833	0.833	0.833	0.833	0.833
Controls	Basic	Basic	Basic	All	All	All
Additional Treatment Interactions	None	By Female	By Female	None	By Female	By Female

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 the year prior to relevant survey wave), 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 category 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.

6.2 Mechanisms

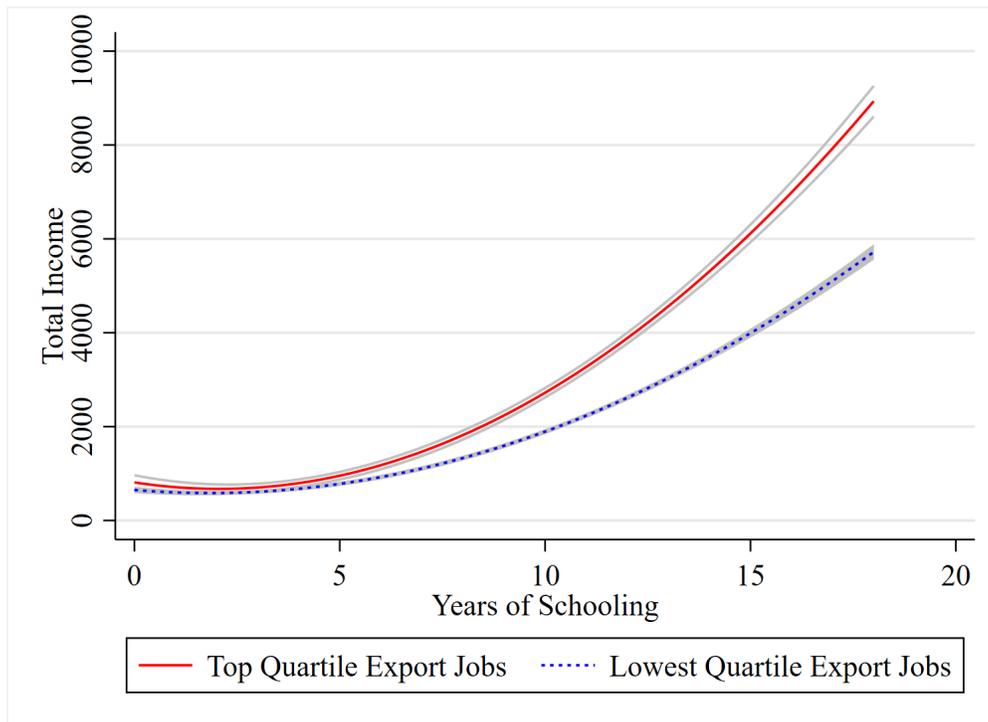
According to the model in section 2, the finding that export manufacturing jobs reduce the effectiveness of PROGRESA implies that these jobs result in net benefits that decrease faster with each additional year of schooling. This could be due to export jobs changing the convexity of the future wage function or the convexity of foregone 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 help shed light on these relationships. First, in Figure 3, we plot the relationship between schooling and income, separately for subdelegations in the top quartile in terms of export jobs and those in the bottom quartile. 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 higher CCT effectiveness for high-export areas – the opposite of what our results show.

However, Figure 4 shows that a comparison of opportunity costs leads to a different prediction – lower CCT effectiveness for high-export areas. Panel A depicts the income-age relationship for high-export and low-export areas for youths aged 12 to 18. Panel B shows the relationship between employment rates and age for this same age range. While neither of these are direct measures of opportunity cost convexity, incomes and employment rates that increase more quickly with age are a reasonable indication of more convex opportunity costs. Both of these panels show steeper curves 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 effectiveness.

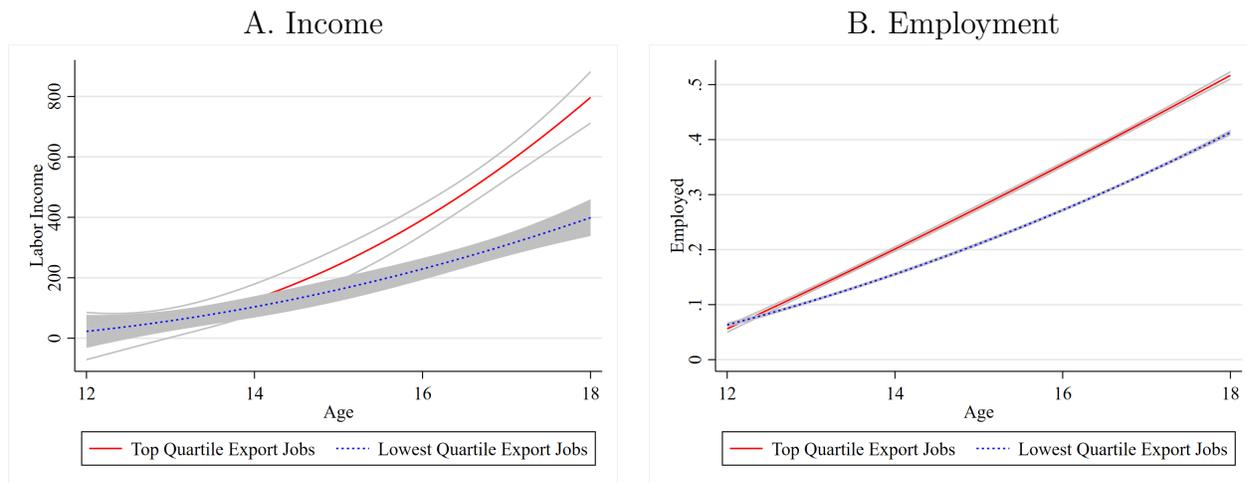
Our findings of lower CCT effectiveness in areas with more export jobs suggest that the opportunity cost channel (specifically, more convex costs) dominates over the wage function

Figure 3: Income-Schooling Relationship, by Export Job Quartiles



Notes: Red and blue lines depict the predicted quadratic relationship between income and schooling using the 2000 Mexican census, restricting to adults aged 25-55 in the seven PROGRESA states. 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 total population) in 2000.

Figure 4: Youth Income, Youth Employment, and Age, by Export Job Quartiles



Notes: Red and blue lines depict the predicted quadratic relationship between income/employment rates age using the 2000 Mexican census, restricting to youths aged 12 to 18 in the seven PROGRESA states. 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 total population) in 2000.

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 that 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 A5, which plots the entire distribution of treatment effects for each group, revealing substantially more variance for the working-aged group.

We document a similar result for attendance effects, for which we split the sample into those younger than 15 and those older than 15 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 A6 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)	(2)	(3)	(4)
	Educational Attainment	Educational Attainment	School Attendance	School Attendance
Treated x Export Jobs	-0.13 (0.092)	-0.38 (0.19)**	-0.0020 (0.0077)	-0.077 (0.020)***
Treated	0.12 (0.069)*	0.29 (0.14)**	0.031 (0.0069)***	0.026 (0.020)
Export Jobs	0.11 (0.15)	-0.025 (0.30)	-0.022 (0.014)	0.058 (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). *Treat* is an indicator for PROGRESA treatment villages. *Export Jobs* is the ratio of the number of export-oriented jobs in the subdelegation (in the year prior to relevant survey wave), 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 category 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 4. “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. Low wage and young jobs are the ones that are more obtainable for someone who drops out of school before graduating high school, and are therefore more relevant for the opportunity cost function.

In Table 6, we show 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. Although neither treatment interaction is significant (which could be due to the high correlation between the young and old export job variables), the fact that they have opposite signs is telling.

In sum, this evidence suggests that PROGRESA was less effective in areas with more export manufacturing because these types of job increase the convexity of the opportunity cost function. Although Figure 3 showed that export manufacturing jobs also increase the convexity of the wage function (which should lead to larger PROGRESA effects), the evidence reveals the marginal cost channel appears to dominate over this marginal benefits channel. One possible reason for this is migration, which could weaken the relationship between local (subdelegation-level) labor market conditions and perceived future wages. In settings where migration is common, individuals might form their expectations about the future wage function using information from areas outside their local community. 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 effectiveness by changing the opportunity costs, rather than by changing the returns to schooling, consistent with the discussion in section 2. The validity of this interpretation,

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)	-0.18 (0.11)*	-0.014 (0.0073)**	-0.30 (0.29)	-0.030 (0.019)
Treated x Export Jobs (Type 2)	-0.018 (0.12)	0.0072 (0.013)	0.078 (0.33)	0.021 (0.023)
Treated	0.20 (0.096)**	0.031 (0.0084)***	0.20 (0.095)**	0.031 (0.0083)***
Export Jobs (Type 1)	0.080 (0.093)	0.0090 (0.0073)	0.19 (0.23)	0.027 (0.018)
Export Jobs (Type 2)	-0.082 (0.22)	-0.031 (0.018)*	-0.15 (0.26)	-0.044 (0.026)*
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). *Treat* is an indicator for PROGRESA treatment villages. *Export Jobs* is the ratio of the number of export-oriented jobs (defined by the specified type) in the subdelegation (in the year prior to relevant survey wave), 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 category 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 4.

however, requires that the heterogeneity we document is not caused by some other correlate of the export jobs variable.¹⁶

To evaluate this, we begin by exploring possible correlates at the subdelegation level. If 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 A7, 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 effect 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 A8 that export jobs do appear to be correlated with several

¹⁶Given the results discussed in Table 5, any problematic correlate would have to generate heterogeneity for certain age groups and not others.

household and individual characteristics. Using data from all four 1997-1999 waves, we regress various characteristics on the wave-specific export job 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, 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 Tables 7 (for educational attainment) and 8 (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 8). Reported coefficients can be interpreted as the effects the average child (for continuous variables) or modal child (for categorical variables).¹⁷

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 8. In other words, the treatment effect heterogeneity we document does appear to be driven by the availability of

¹⁷Continuous 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 modal 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.

export jobs, and not by any of these other 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.

7 Conclusion

This paper provides evidence that labor market conditions influence the effectiveness of education policies. We focus specifically on Mexico, which implemented its landmark CCT program, PROGRESA, during a period of trade liberalization that substantially increased the availability of export-oriented manufacturing jobs. The impact of these export-oriented jobs on PROGRESA’s effectiveness is theoretically ambiguous, as these jobs are associated with a more convex opportunity cost as well as a more convex wage function.

Empirically, we show that PROGRESA was less successful at improving schooling outcomes in areas with greater exposure to export manufacturing, particularly for those old enough to work in export manufacturing. This suggests that the opportunity cost channel dominates over the wage benefits channel. 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.

Given the widespread popularity of CCTs across the developing world, it is important to understand what drives variation in the success of these programs. Our findings highlight 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. This could be one understudied explanation for why the effects

Table 7: Heterogeneous Effects of PROGRESA on Educational Attainment with Additional Interactions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Educational Attainment	Educational Attainment	Educational Attainment	Educational Attainment	Educational Attainment	Educational Attainment	Educational Attainment	Educational Attainment
Treat x Export Jobs	-0.24 (0.12)**	-0.26 (0.12)**	-0.26 (0.13)**	-0.22 (0.13)*	-0.26 (0.13)**	-0.27 (0.13)**	-0.25 (0.13)*	-0.25 (0.13)*	-0.24 (0.13)*
Treat	0.21 (0.096)**	0.20 (0.099)**	0.20 (0.098)**	0.20 (0.090)**	0.21 (0.096)**	0.047 (0.13)	0.20 (0.11)*	0.26 (0.099)**	0.20 (0.12)*
Treat x Female	-0.094 (0.082)	-0.093 (0.082)	-0.093 (0.082)	-0.069 (0.078)	-0.094 (0.082)	-0.095 (0.083)	-0.091 (0.082)	-0.093 (0.083)	-0.091 (0.082)
Observations	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
Controls	All	All	All	All	All	All	All	All	All
Additional Treatment	Schooling (Census Avg)	Income (Census Avg)	Urban (Census Avg)	OwnSchool (Individual)	Migration (Household)	LaborIncome (Household)	FatherOcc (Household)	MotherOcc (Household)	SiblingWork (Household)
Interactions									

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 (averaged over the 1997-2003 period), over the subdelegation's working-aged population according to the 1990 census, standardized. *Basic* controls include gender, cohort fixed effects, subdelegation fixed effects, and a female-by-treatment interaction. *All* controls add household size, household head age, household head gender, as well as parental education and language category dummies (including dummies for missing values). Categorical variables are included as multiple dummy variable interactions, including a dummy for missing values; the omitted category is the modal category.

Table 8: Heterogeneous Effects of PROGRESA on School Attendance with Additional Interactions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	School Attendance	School Attendance	School Attendance	School Attendance	School Attendance	School Attendance	School Attendance	School Attendance	School Attendance
Treat x Post x Export Jobs	-0.020 (0.0090)**	-0.021 (0.0096)**	-0.023 (0.0098)**	-0.020 (0.0067)**	-0.018 (0.0084)**	-0.019 (0.0085)**	-0.018 (0.0085)**	-0.019 (0.0085)**	-0.016 (0.0081)*
Treat x Post	0.029 (0.0082)***	0.031 (0.0083)***	0.031 (0.0082)***	0.041 (0.0066)***	0.028 (0.0083)***	0.041 (0.011)***	0.024 (0.011)**	0.034 (0.015)**	0.028 (0.0085)***
Treat x Post x Female	0.010 (0.0060)*	0.010 (0.0060)*	0.010 (0.0060)*	0.0029 (0.0053)	0.0100 (0.0060)*	0.010 (0.0060)*	0.010 (0.0060)*	0.011 (0.0060)*	0.0094 (0.0060)*
Observations	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
Controls	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)

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 the year prior to relevant survey wave), 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, subdelegation fixed effects, and a female indicator interacted with *Treat-by-Post*. *All* controls add household size, household head age, household head gender, as well as parental education and language category 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.

of minimum wage policy, health insurance expansions, financial aid programs, and other government policies differ drastically across settings.

References

- Adhvaryu, A., Nyshadham, A., Molina, T., and Tamayo, J. (2018). Helping children catch up: Early life shocks and the progresca experiment. Technical report, National Bureau of Economic Research.
- Aguayo-Tellez, E., Airola, J., Juhn, C., and Villegas-Sanchez, C. (2013). Did trade liberalization help women? the case of Mexico in the 1990s. *Research in Labor Economics*, 38.
- Angelucci, M., De Giorgi, G., Rangel, M. A., and Rasul, I. (2010). Family networks and school enrolment: Evidence from a randomized social experiment. *Journal of Public Economics*, 94(3-4):197–221.
- Atkin, D. (2016). Endogenous skill acquisition and export manufacturing in Mexico. *American Economic Review*, 106(8):2046–85.
- Behrman, J. R., Parker, S. W., and Todd, P. E. (2009a). Medium-term impacts of the Oportunidades conditional cash transfer program on rural youth in Mexico. *Poverty, Inequality and Policy in Latin America*, pages 219–70.
- Behrman, J. R., Parker, S. W., and Todd, P. E. (2009b). Schooling Impacts of Conditional Cash Transfers on Young Children: Evidence from Mexico. *Economic Development and Cultural Change*, 57(3):439–77.
- Behrman, J. R., Parker, S. W., and Todd, P. E. (2011). Do conditional cash transfers for schooling generate lasting benefits? A five-year followup of PROGRESA/Oportunidades. *Journal of Human Resources*, 46(1):93–122.
- Behrman, J. R., Sengupta, P., and Todd, P. (2005). Progressing through PROGRESA: An impact assessment of a school subsidy experiment in rural Mexico. *Economic Development and Cultural Change*, 54(1):237–275.

- Blanchard, E. J. and Olney, W. W. (2017). Globalization and human capital investment: Export composition drives educational attainment. *Journal of International Economics*, 106:165–183.
- Cámara de Diputados (2004). Evolución del sector manufacturero de México, 1980-2003 “. Technical report, Mexico City: Centro de Estudios de las Finanzas Públicas.
- Dammert, A. C. (2009). Heterogeneous impacts of conditional cash transfers: Evidence from Nicaragua. *Economic Development and Cultural Change*, 58(1):53–83.
- Diario Oficial de la Federación (2019). Decreto por el que se crea la Coordinación Nacional de Becas para el Bienestar Benito Juárez. https://www.dof.gob.mx/nota_detalle.php?codigo=5561693&fecha=31/05/2019. Accessed: 2021-01-02.
- Djebbari, H. and Smith, J. (2008). Heterogeneous impacts in progress. *Journal of Econometrics*, 145(1-2):64–80.
- Edmonds, E. V., Pavcnik, N., and Topalova, P. (2010). Trade adjustment and human capital investments: Evidence from Indian tariff reform. *American Economic Journal: Applied Economics*, 2(4):42–75.
- Edmonds, E. V., Topalova, P., and Pavcnik, N. (2009). Child labor and schooling in a globalizing world: Some evidence from urban India. *Journal of the European Economic Association*, 7(2-3):498–507.
- Epple, D., Romano, R. E., and Urquiola, M. (2017). School vouchers: A survey of the economics literature. *Journal of Economic Literature*, 55(2):441–92.
- Fiszbein, A. and Schady, N. R. (2009). *Conditional cash transfers: reducing present and future poverty*. World Bank Publications.
- Gertler, P. (2004). Do conditional cash transfers improve child health? Evidence from

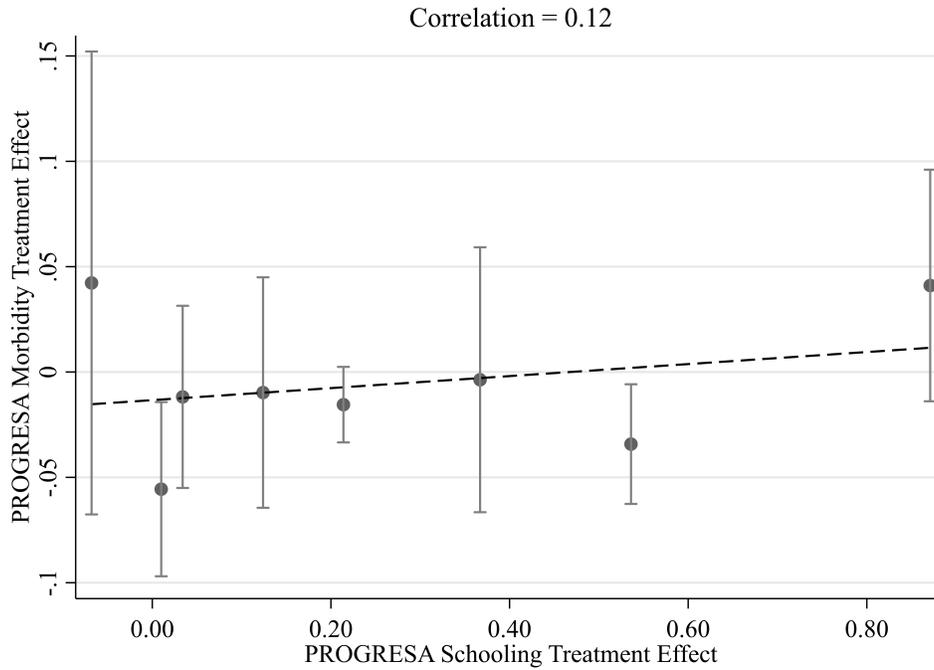
- PROGRESA's control randomized experiment. *American Economic Review*, 94(2):336–341.
- Glewwe, P. and Muralidharan, K. (2016). Improving education outcomes in developing countries: Evidence, knowledge gaps, and policy implications. In *Handbook of the Economics of Education*, volume 5, pages 653–743. Elsevier.
- Greenland, A. and Lopresti, J. (2016). Import exposure and human capital adjustment: Evidence from the US. *Journal of International Economics*, 100:50–60.
- Handa, S., Davis, B., Stampini, M., and Winters, P. C. (2010). Heterogeneous treatment effects in conditional cash transfer programmes: assessing the impact of progresa on agricultural households. *Journal of Development Effectiveness*, 2(3):320–335.
- Hanson, G. H. and Harrison, A. (1999). Trade liberalization and wage inequality in Mexico. *ILR Review*, 52(2):271–288.
- Herbaut, E. and Geven, K. (2020). What works to reduce inequalities in higher education? a systematic review of the (quasi-) experimental literature on outreach and financial aid. *Research in Social Stratification and Mobility*, 65:100442.
- Jensen, R. (2010). The (perceived) returns to education and the demand for schooling. *Quarterly Journal of Economics*, 125(2).
- Jensen, R. (2012). Do labor market opportunities affect young women's work and family decisions? Experimental evidence from India. *The Quarterly Journal of Economics*, 127(2):753–792.
- Juhn, C., Ujhelyi, G., and Villegas-Sanchez, C. (2014). Men, women, and machines: How trade impacts gender inequality. *Journal of Development Economics*, 106:179–193.
- Lederman, D., Olarreaga, M., and Payton, L. (2010). Export promotion agencies: Do they work? *Journal of Development Economics*, 91(2):257–265.

- Lee, S. and Shaikh, A. M. (2014). Multiple testing and heterogeneous treatment effects: re-evaluating the effect of PROGRESA on school enrollment. *Journal of Applied Econometrics*, 29(4):612–626.
- Lustig, N. (2001). Life is not easy: Mexico’s quest for stability and growth. *Journal of Economic Perspectives*, 15(1):85–106.
- Maluccio, J. and Flores, R. (2005). *Impact evaluation of a conditional cash transfer program: The Nicaraguan Red de Protección Social*. International Food Policy Research Institute.
- Manley, J., Gitter, S., and Slavchevska, V. (2013). How effective are cash transfers at improving nutritional status? *World Development*, 48:133–155.
- Minnesota Population Center (2015). *Integrated Public Use Microdata Series, International: Version 6.4 [Machine-readable database]*. University of Minnesota, Minneapolis.
- Molina-Millán, T., Barham, T., Macours, K., Maluccio, J. A., and Stampini, M. (2019). Long-term impacts of conditional cash transfers: review of the evidence. *The World Bank Research Observer*, 34(1):119–159.
- Moreno-Brid, J. C. (2007). Economic development and industrial performance in Mexico post-nafta. *Taller Nacional sobre “Migración interna y desarrollo en México: diagnóstico, perspectivas y políticas*, 16.
- Nguyen, T. (2008). Information, Role Models and Perceived Returns to Education: Experimental Evidence from Madagascar. MIT.
- Ordóñez-Barba, G. M. and Silva-Hernández, A. L. (2019). Progres-a-oportunidades-prospera: avatares, alcances y resultados de un programa paradigmático contra la pobreza. *Papeles de Población*, 25(99):77–111.
- Page, J. (1994). The East Asian miracle: four lessons for development policy. *NBER Macroeconomics Annual*, 9:219–269.

- Parker, S. W., Todd, P. E., et al. (2017). Conditional cash transfers: The case of progresa/oportunidades. *Journal of Economic Literature*, 55(3):866–915.
- Polaski, S. (2003). Jobs, wages, and household income. *NAFTA's Promise and Reality: Lessons from Mexico for the Hemisphere*, pages 11–38.
- Reventa, A. (1997). Employment and wage effects of trade liberalization: the case of mexican manufacturing. *Journal of labor Economics*, 15(S3):S20–S43.
- Schultz, T. P. (2004). School subsidies for the poor: evaluating the Mexican Progresa poverty program. *Journal of Development Economics*, 74(1):199–250.
- Shah, M. and Steinberg, B. M. (2019). Workfare and human capital investment: Evidence from india. *Journal of Human Resources*, pages 1117–9201R2.
- Skoufias, E. (2005). Progresa and its impacts on the welfare of rural households in mexico. Technical Report 139, International Food Policy Research Institute.
- Skoufias, E. and Parker, S. W. (2001). Conditional Cash Transfers and their Impact on Child Work and schooling: Evidence from the progresa program in Mexico. *Economia*, 2(1):45–96.
- United Nations (2016). Transforming our world: The 2030 agenda for sustainable development.
- World Bank Group (2017). Closing the gap: The state of social safety nets 2017. Technical report.

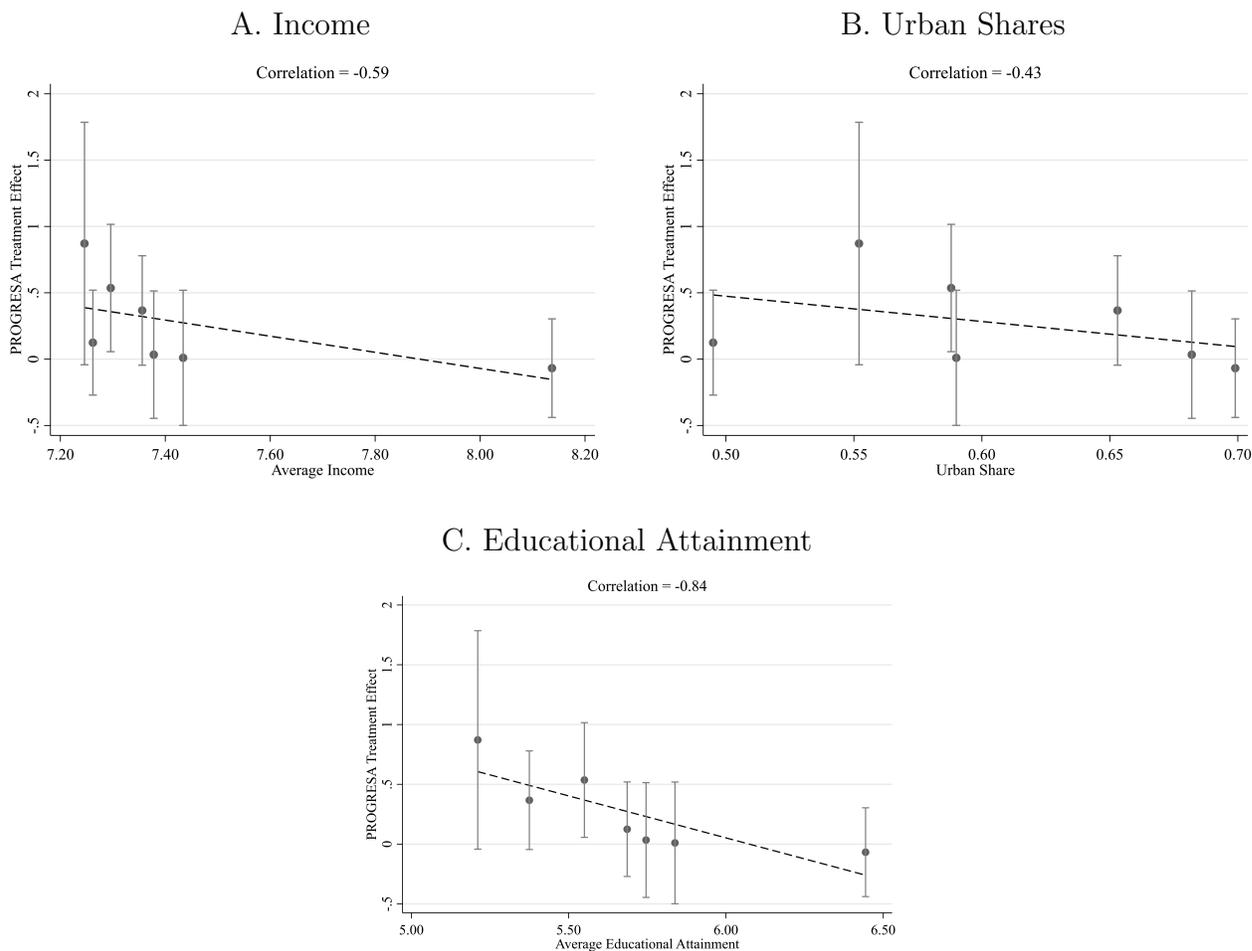
A Appendix

Figure A1: PROGRESA Impact on Health and Educational Attainment Across States



Notes: Coefficients (and 95% confidence intervals) are obtained from state-specific regressions of an indicator for being sick in the past month on a PROGRESA treatment locality indicator, restricting to children aged 3 and younger in 1997 in eligible (poor) households, using the three follow-up surveys in 1998-1999. Values along the x-axis are obtained from state-specific regressions of educational attainment in 2003 on a PROGRESA treatment locality indicator, restricting to children aged 5 to 16 in 1997 in eligible (poor) households. Both regressions control for 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 locality level.

Figure A2: State-Level Characteristics and PROGRESA Impact



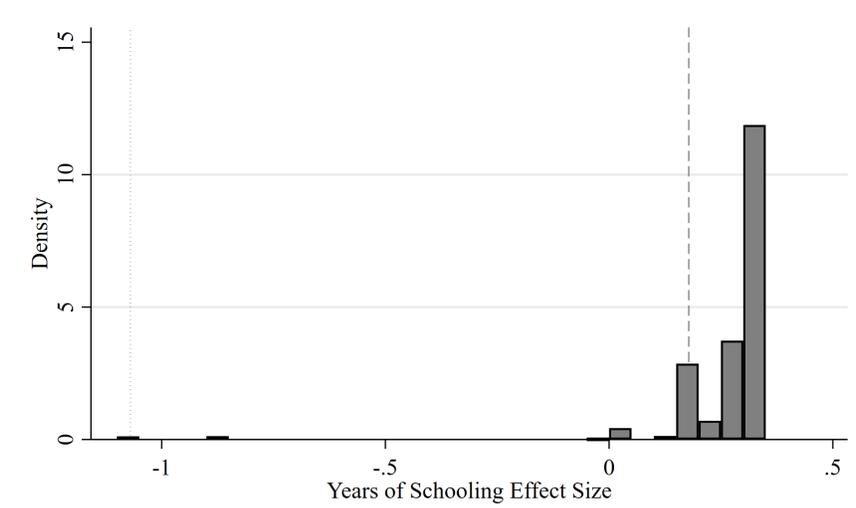
Notes: In Panel A, income is the log of the state’s average labor income among adults aged 25-55. In Panel B, urban share is the share of the state’s population living in urban areas. In Panel C, educational attainment is the state’s average educational attainment. All of the aforementioned variables are taken from the 2000 census. Coefficients (and 95% confidence intervals) are obtained from state-specific regressions of educational attainment in 2003 on a PROGRESA treatment locality indicator, restricting to children aged 5 to 16 in 1997 in eligible (poor) households, controlling for 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 locality level.

Table A1: Summary Statistics by State

	Mean	Treatment - Control Difference, By State						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All States	Guerrero	Hidalgo	Michoacán	Puebla	Queretaro	San Luis Potosí	Veracruz
Age in 1997	110.00 (3.32)	0.03 (0.16)	-0.03 (0.14)	-0.14 (0.12)	-0.12 (0.10)	0.49** (0.22)	0.22* (0.13)	-0.03 (0.10)
Female	0.48 (0.50)	-0.01 (0.03)	-0.02 (0.02)	-0.02 (0.01)	-0.02 (0.02)	-0.03 (0.03)	-0.00 (0.02)	0.00 (0.01)
Attending School	0.85 (0.36)	-0.00 (0.04)	0.00 (0.02)	0.01 (0.02)	0.00 (0.02)	-0.03 (0.04)	-0.02 (0.02)	0.02 (0.02)
Educational Attainment	3.39 (2.71)	0.29 (0.21)	0.05 (0.16)	0.02 (0.14)	-0.11 (0.15)	0.05 (0.14)	0.10 (0.14)	-0.09 (0.13)
Mother's Education	1.04 (0.22)	-0.03 (0.04)	-0.03 (0.02)	-0.01 (0.02)	-0.01 (0.02)	0.02 (0.03)	-0.02 (0.03)	-0.00 (0.01)
Missing Mother's Education	0.37 (0.48)	-0.06 (0.09)	-0.03 (0.06)	-0.07 (0.04)	0.05 (0.05)	0.08 (0.09)	0.01 (0.05)	-0.09** (0.05)
Father's Education	1.05 (0.25)	0.06 (0.04)	-0.01 (0.02)	0.01 (0.02)	-0.01 (0.02)	0.03 (0.03)	-0.01 (0.03)	0.02 (0.01)
Missing Father's Education	0.31 (0.46)	-0.01 (0.07)	-0.03 (0.05)	-0.04 (0.04)	-0.03 (0.05)	0.12* (0.07)	-0.01 (0.04)	-0.05 (0.04)
Mother Speaks Indigenous Lang.	0.40 (0.49)	-0.38** (0.15)	0.09 (0.11)	-0.00 (0.00)	0.07 (0.13)	0.18 (0.17)	-0.04 (0.13)	0.08 (0.09)
Missing Mother's Language	0.03 (0.16)	0.01 (0.01)	-0.00 (0.01)	-0.03** (0.01)	-0.01 (0.01)	0.01 (0.01)	0.02** (0.01)	-0.01* (0.01)
Father Speaks Indigenous Lang.	0.41 (0.49)	-0.41** (0.16)	0.09 (0.12)	-0.01 (0.01)	0.06 (0.13)	0.16 (0.15)	-0.05 (0.13)	0.08 (0.09)
Missing Father's Language	0.06 (0.24)	0.05*** (0.02)	0.00 (0.01)	0.00 (0.02)	-0.00 (0.02)	-0.02 (0.03)	-0.02 (0.01)	-0.00 (0.01)
<i>N Individuals</i>	23272	2210	3664	3164	3641	1035	2950	6608
Household Size	6.67 (2.16)	0.06 (0.19)	0.07 (0.19)	-0.14 (0.19)	-0.32 (0.21)	0.16 (0.33)	0.33** (0.14)	-0.07 (0.13)
Household Head Age	42.02 (12.13)	0.66 (1.17)	-0.99 (0.87)	0.04 (0.85)	-2.16* (1.09)	4.47*** (1.29)	-0.06 (0.97)	-1.07* (0.63)
Female Household Head	0.07 (0.25)	0.06*** (0.02)	0.00 (0.02)	-0.01 (0.02)	-0.01 (0.02)	-0.03 (0.03)	-0.02 (0.01)	-0.00 (0.01)
No. Children Aged 0-2	0.55 (0.66)	0.02 (0.06)	0.01 (0.07)	-0.05 (0.04)	0.08* (0.04)	-0.09 (0.11)	0.03 (0.04)	-0.04 (0.03)
No. Children Aged 3-5	0.74 (0.73)	0.06 (0.05)	-0.00 (0.06)	-0.08 (0.06)	0.01 (0.05)	-0.12 (0.09)	0.06 (0.05)	-0.01 (0.04)
No. Females Aged 6-7	0.27 (0.47)	0.01 (0.03)	-0.00 (0.03)	-0.01 (0.04)	-0.08** (0.03)	-0.02 (0.05)	0.01 (0.02)	0.02 (0.02)
No. Females Aged 8-12	0.64 (0.74)	-0.06 (0.08)	-0.05 (0.04)	-0.08 (0.05)	-0.05 (0.06)	0.02 (0.12)	-0.01 (0.05)	0.04 (0.03)
No. Females Aged 8-12	0.50 (0.73)	-0.01 (0.05)	0.03 (0.04)	-0.01 (0.05)	-0.09* (0.05)	0.06 (0.07)	0.04 (0.04)	-0.02 (0.03)
No. Males Aged 6-7	0.28 (0.48)	-0.01 (0.03)	-0.01 (0.03)	0.07** (0.03)	-0.02 (0.03)	-0.04 (0.04)	-0.02 (0.03)	-0.02 (0.02)
No. Males Aged 8-12	0.67 (0.75)	0.06 (0.08)	0.07 (0.05)	-0.03 (0.05)	-0.06 (0.04)	0.10 (0.06)	0.02 (0.05)	0.03 (0.04)
No. Males Aged 13-18	0.54 (0.76)	0.00 (0.08)	-0.01 (0.05)	0.00 (0.06)	-0.06 (0.04)	0.15 (0.10)	0.11*** (0.04)	-0.01 (0.03)
No. Females Aged 19-54	1.12 (0.51)	0.07** (0.03)	0.01 (0.03)	0.03 (0.04)	-0.05 (0.04)	-0.04 (0.07)	0.04 (0.03)	-0.00 (0.03)
No. Females Aged 55+	0.15 (0.37)	0.01 (0.02)	-0.00 (0.03)	0.01 (0.02)	-0.04* (0.02)	0.05 (0.03)	-0.03 (0.03)	-0.01 (0.02)
No. Males Aged 19-54	1.03 (0.56)	-0.11** (0.04)	0.09** (0.04)	0.02 (0.06)	0.06 (0.04)	-0.01 (0.05)	0.05 (0.04)	-0.03 (0.02)
No. Males Aged 55+	0.16 (0.37)	0.03 (0.03)	-0.04* (0.02)	-0.01 (0.03)	-0.03 (0.03)	0.09*** (0.03)	0.02 (0.03)	-0.01 (0.02)
<i>N Households</i>	8296	833	1340	984	1266	355	1081	2437

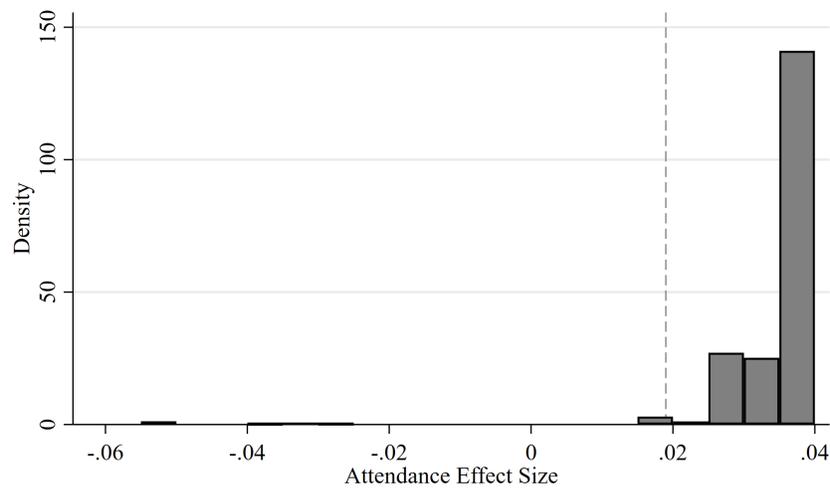
Notes: Summary statistics calculated from the baseline survey, restricting to children aged 6-15 at baseline, with a non-missing educational attainment variable in 2003. Standard deviations (in column 1) and standard errors clustered at village level (in columns 2-8) in parentheses. * $p < 0.1$, ** $p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

Figure A3: Distribution of PROGRESA Schooling Effects



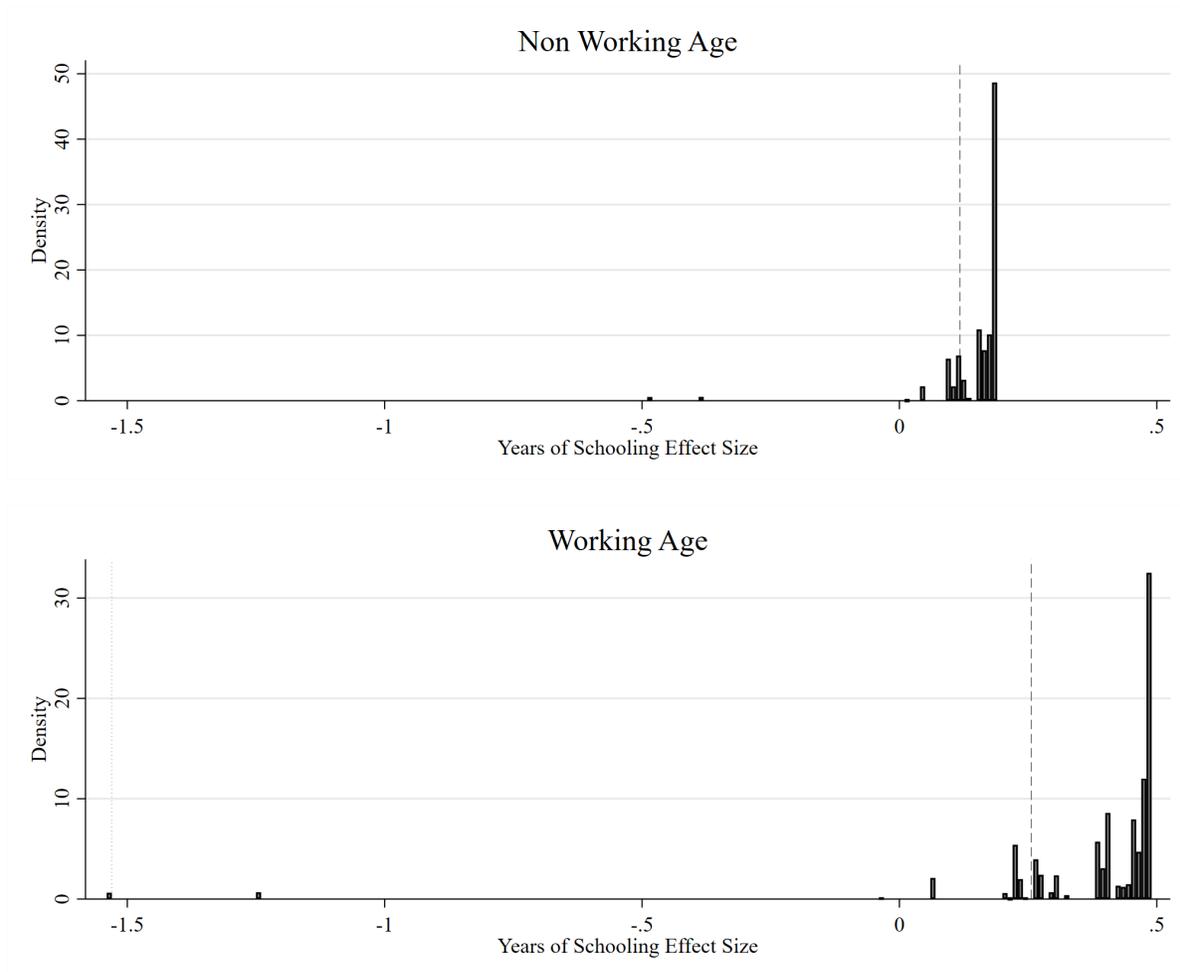
Notes: This figure plots the distribution of PROGRESA treatment effects, calculated from the results of column 4 of Table 3. Estimates to the left of the dotted line or to the right of the dashed line are significantly different from zero at the 5% level.

Figure A4: Distribution of PROGRESA Attendance Effects



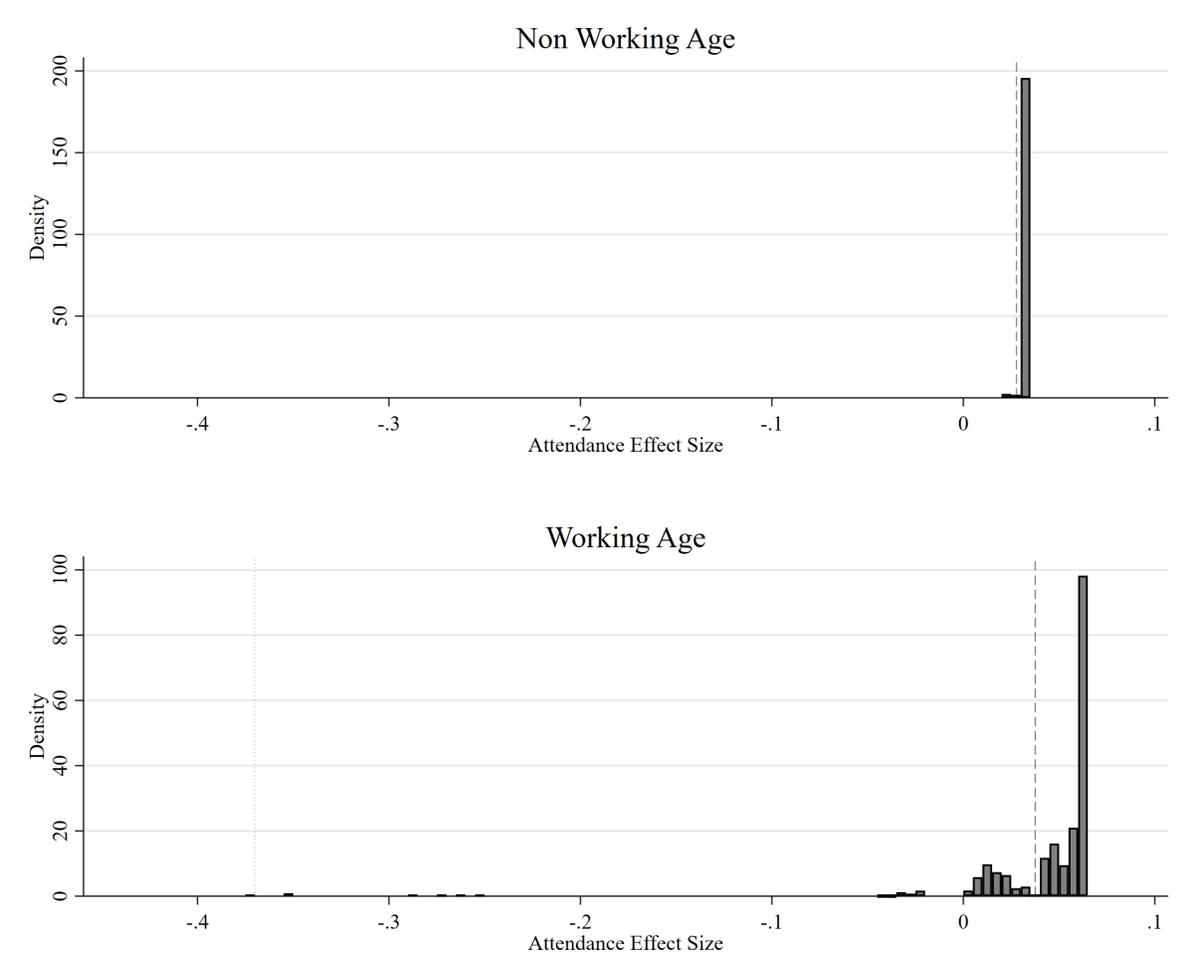
Notes: This figure plots the distribution of PROGRESA treatment effects, calculated from the results of column 6 of Table 4. Estimates to the right of the dashed line are significantly different from zero at the 5% level.

Figure A5: Distribution of PROGRESA Schooling Effects by Age



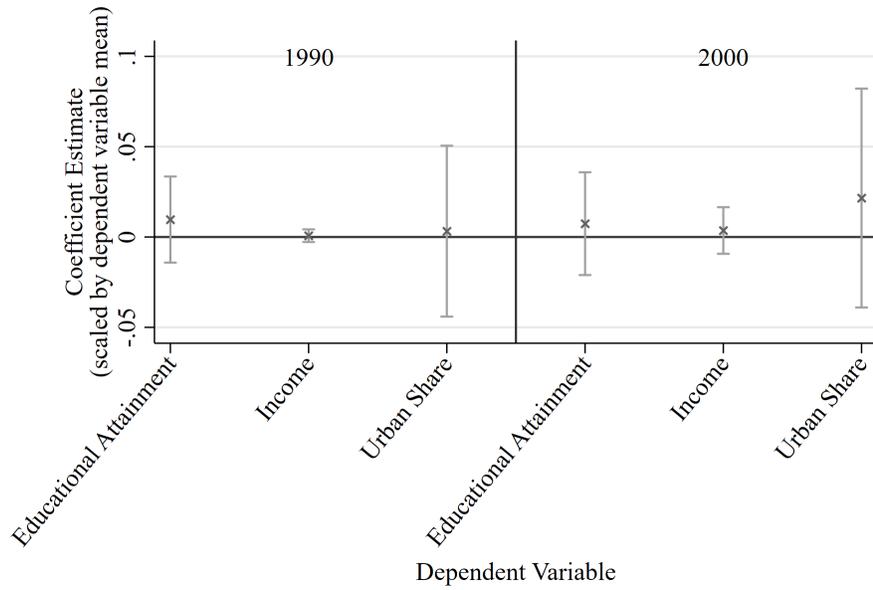
Notes: These figures plots the distribution of PROGRESA treatment effects separately for those younger than working age and those of working age, calculated from the results of columns 1 and 2 of Table 5. Estimates to the left of the dotted line or to the right of the dashed line are significantly different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

Figure A6: Distribution of PROGRESA Attendance Effects by Age



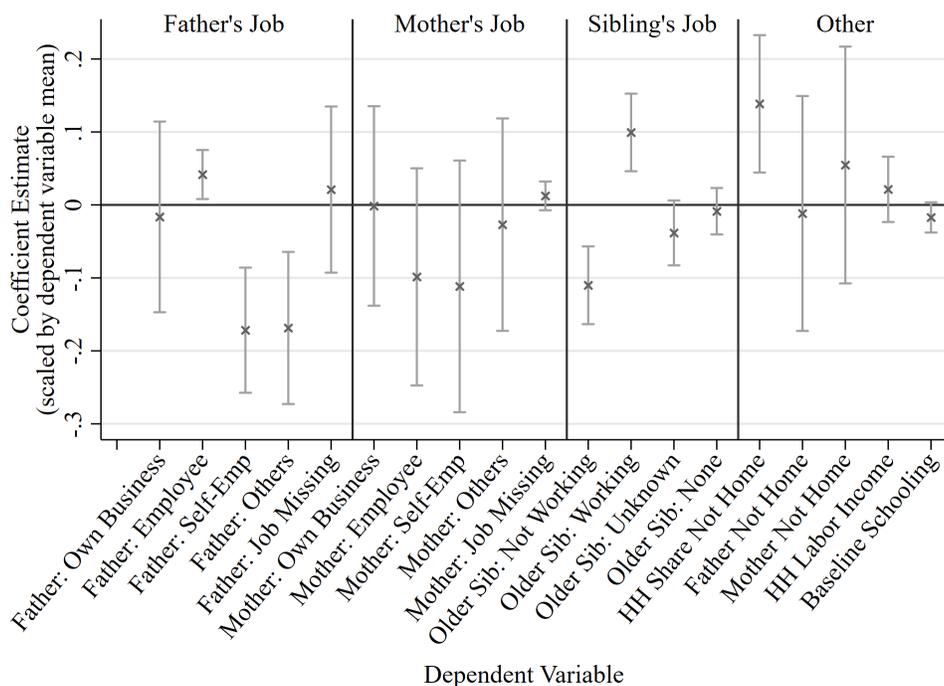
Notes: These figures plots the distribution of PROGRESA treatment effects separately for those younger than working age and those of working age, calculated from the results of columns 3 and 4 of Table 5. Estimates to the left of the dotted line or to the right of the dashed line are significantly different 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 A7: 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 A8: 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 the year before the relevant survey), 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.