Long-term Wage Effects of Mexico's PROGRESA

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ECON 6073 Masters Thesis
CU Denver

Submitted December 14, 2018

Abstract

Mexico's PROGRESA conditional cash transfer program has been operational for 20 years. Children who benefitted from the earliest days of the program until the present are beginning to enter the workforce. Since CCTs generally aim to stimulate human capital development in children and thereby reduce poverty in their adulthood, this affords me a chance to study whether and to what extent PROGRESA has achieved its aim. Using cross-sectional data with a fixed-effects strategy, I find effects on likelihood of employment, skilled employment, and wages of exposure to Mexico's PROGRESA to suggest that there are returns to PROGRESA. I find further evidence that these effects are stronger for women and that the labor market may be rewarding vocational skills, implying PROGRESA's impact is in increasing numeracy, literacy, and interpersonal communication skills among the poor.

1 Introduction

Conditional cash transfer programs ("CCTs") have proliferated in recent decades throughout Latin America, Africa, and Southeast Asia as a tool for halting the intergenerational transmission of poverty by developing human capital. Their effects are well documented. They are generally successful in their aims to improve nutrition and education outcomes, the two primary mechanisms hypothesized for developing human capital. (Fiszbein & Schady 2009) However, their long-term effects remain unknown since most CCTs have not been in place long enough to observe the long-term economic outcomes of beneficiaries. Mexico's PROGRESA does not suffer from this problem. The program began in 1997, and children who were young enough to receive benefits throughout their childhood have entered the workforce. This presents researchers with an opportunity to observe their labor market outcomes and investigate whether the program has had the intended human capital development consequences. Doing so would either validate the success of CCTs, many of which are modeled on PROGRESA, or yield insight that could suggest resources are better used elsewhere.

This paper seeks to identify effects in wages for skilled employment resulting from exposure to PROGRESA. If increases in educational outcomes, such as more grades completed and more students graduating high school, are the primary channel for increasing human capital, then there should be observable wage effects in jobs that demand more human capital. I explore this hypothesis by testing the relationships between living in a municipality enrolled in PROGRESA and education and labor outcomes. I further explore heterogenous effects on the timing of exposure to PROGRESA, as well as differences between sexes and population density.

Understanding the dominant labor market forces in response to an influx of skilled labor is crucial in identifying the effects of PROGRESA because the returns to human capital are most likely determined by a combination of the actions of PROGRESA beneficiaries and dynamics of the labor market. As I will show, "skilled" labor may take more than one definition, and it may not necessarily correspond to "college educated" labor. Vocational education can replace preparatory and college education, and wage returns can be driven by the supply and demand conditions of individual sectors of the labor market. Specifically, if the effect of PROGRESA is to increase the supply of labor with high school and college educations, but the labor market has a higher demand for vocationally trained individuals (who also were beneficiaries of PROGRESA), then wage effects may be stronger in occupations that are more vocational than professional in nature. By estimating the effect of PROGRESA on the likelihood of being employed and the likelihood of working various skilled occupations, I am able to respond to this question.

The effects uncovered in the present analysis are necessarily intent-to-treat ("ITT") effects. I do not observe recipients and non-recipients. Instead, I track outcomes for all individuals and utilize variation in the timing of enrollment of different municipalities. My identifying assumption is that this timing is exogenous, and the result is the effect of being eligible to receive benefits irrespective of issues such as non-compliance and deviation from randomization, which is the definition of intent-to-treat. When combined with my identification strategy, ITT effects can avoid potential endogeneity issues associated with receiving benefits. For example, impoverishment is used to determine eligibility for PROGRESA benefits. Not observing who actually receives benefits means that I do not have to worry about controlling for endogenous covariates associated with poverty.

This work contributes to the PROGRESA literature in two ways. First, to my knowledge, there is little extant research into the long-term effects of PROGRESA. Second, whereas most research on PROGRESA uses randomized surveys created to track participants, this paper seeks to uncover effects in the Mexican population census. If indeed there are significant effects caused by PROGRESA participation, then it is possible that they can be identified in the broader, cross-sectional data provided by the census.

I find positive and significant effects on the likelihood of completing compulsory and high school education, of being employed, of being in a skilled occupation, and on the natural log hourly wages. While these effects allow for some inference about the labor market dynamics affecting wages, they do not provide sufficient evidence to conclude that education is the primary mechanism for increasing human capital.

2 Background & Literature Review

PROGRESA was initiated in 1997. It was originally intended to consolidate several disparate government assistance programs into one and focus on stemming poverty among rural inhabitants in Mexico by giving families both an incentive and financial aid to afford their children better health and education outcomes. By fostering health and education among children, they would increase their human capital and increase their skills and chances of employment thereby reducing the chances of poverty and eliminating poverty over time. Participation was initially restricted to rural areas of Mexico. Localities, the lowest administrative unit, were evaluated along observable characteristics characteristics associated with poverty, and households that scored above a certain threshold measured by this scale were offered participation in the program.[1] About 97% of households enrolled, and cash transfers began in 1998. Mothers received benefits for both nutrition and education conditioned on enrolling their children in school, providing sufficient food, and regularly taking their children to the doctor.

For the first three years of PROGRESA, due to budget constraints, cash transfers were distributed randomly in three waves to localities. The Mexican Census Bureau ("CONAPO"), in coordination with various researchers, took advantage of this randomness and designed a series of surveys to collect information on the effects of the cash transfers.[2] In 2002, following the election of Vicente Fox, the program was reformed into Oportunidades. Changes were

[1] This is CONAPO's *marginalization index*

[2] These are called the "Survey of Socio-economic Characteristics of Households" ("ENCASEH") and the "Household Evaluation Survey" ("ENCEL"), and they constitute a longitudinal data set covering PROGRESA participants. They are available at *https://evaluacion.prospera.gob.mx/es/bases/bases.php*.

made to the eligibility criteria and the distribution of benefits, and urban households were incorporated into the program. Of the more notable expansions of the program, families with teenage girls were given larger education benefits. By this time, any exogeneity in the program design was gone. In 2014, the program was again modified to become PROSPERA, by which name it is currently known. The scope of the program has been broadened to include other objectives adjacent to human capital development and poverty alleviation.

Parker and Todd (2017) catalog the extant research on PROGRESA. In most cases, the data used to conduct research on the program and its effects are the longitudinal data collected in the surveys mentioned above. Using predominantly difference-in-differences estimation, positive effects are found on school enrollment and attendance at all ages and on progression of completed grade levels, while negative effects are found on days absent, grade repetition, dropout rates, and school-aged children in the labor force. Further positive effects are found in health and nutritional outcomes, and reductions are found in anemia, behavioral problems of children, cortisol levels of children, aggressive behavior of children, rural infant mortality, and childhood obesity. Other studies uncover favorable outcomes in adults, though the results are mixed.

Along the household dimension, general food consumption increases, and labor force participation appears to be unaffected for both men and women. Entrepreneurship and non-agricultural enterprises increase, while agricultural spending and land use also increase. Livestock ownership and the use of draft animals appear to increase among households who were early recipients of PROGRESA funds. Women's status improves: there are notable reductions in husband-only decision-making regarding children and expenses for children, and physical and emotional violence toward women notably decreases. Generally, effects of PROGRESA are felt within the first 10 years of the initiation and expansion of the program, and they are felt among the recipients and their children.

However, given the proliferation of CCTs throughout the world, it is useful to address whether they meet their goals of human capital development and poverty alleviation by examining whether and to what extent the effects endure past childhood and compulsory schooling. In other words, if outcomes do not seem to persist over the long term and in the next generation, then the construction of these programs should be re-assessed. Since many programs have not operational long enough to see next-generation effects, there are challenges to this endeavor. Parker and Vogl (2018) attempts to address long-term educational and labor market outcomes to answer this question. This strategy compares two birth cohorts with differential exposure to the program, complete and partial. The cohorts are selected based on the program enrollment phases in 1997 and 2001. Municipality variation is captured based on the ratio of enrolled families to unerolled families within a municipality. Using a difference-in-differences strategy that exploits this spatio-temporal variation, these researchers find large positive effects in completed education at primary and secondary levels for both men and women who were exposed to PROGRESA. They further find an overall increase in the labor force participation of women driven by increases in paid work, hours worked, and earnings. Among men, there are decreases in agricultural work accompanied by increases in formal sector employment and hours worked.

My study differs from Parker and Vogl (2018) in two ways. First, I expand the variation to include multiple birth year cohorts. This gives me greater precision. Second, their analysis considers only the most marginalized communities based on CONAPO's assessment of poverty. Thus severe poverty is highly correlated with their identifying variation. My study uses all enrolled municipalities without distinguishing among the most poor. As I explain later, this correlation with poverty could be a threat which my strategy circumvents.

3 Data

Data on schooling, wages, and demographics are taken from the Mexico Population Census in 2010, obtained from IPUMS International. Since labor market outcomes are generally only measured at the decennial census, I am unable to observe 2015 outcomes. However, effects should still be observable in 2010. Data on municipal PROGRESA enrollment are taken from the CONAPO.[3] These include enrollment every year from 1998 to the present, although my research stops at 2010 in accord with the Census data. I restrict my sample to individuals born in Mexico in or after 1980 and in the labor force, and I am left with a sample size of 1,380,807. I construct two variables, *Exposure* which measures an individual's exposure to PROGRESA, and *Skilled* which denotes whether an occupation likely requires a college degree. I move to a discussion of the construction of these variables.

Exposure to PROGRESA is jointly determined by the enrollment status of the municipality in which an individual attends primary and secondary schooling and by an individual's birth year. Families begin receiving benefits in 1998, and new municipalities are enrolled every year following 1998 through the present. A municipality is considered enrolled once a locality within the municipality is enrolled, and within municipalities, localities are enrolled in different years. Once a municipality is enrolled, it is never unenrolled. Therefore, an individual has varying exposure to PROGRESA as result of the municipality in which she attends primary and secondary schooling: two individuals of the same age may receive benefits for different periods if they attend school in municipalities that enroll in PROGRESA in different years. Ideally, I would like to utilize locality level variation. Because I measure exposure based on municipal enrollment, I risk coding an individual as exposed who lives in a locality that was never enrolled. However, I do not observe outcomes (wages, years of schooling) at the locality level. [Systemic relationship between locality and outcomes?]An individual's birth year also determines the total number of years of exposure to PROGRESA. Benefits are given to enrolled families whose children are enrolled in grades three through nine, which correspond to ages nine through 15. Therefore, two children in the same municipality will have varying exposure according to their birth years. I claim that this combination of municipality and birth year variation in enrollment is exogenous.

[3] These data are also maintained at $https://evaluacion.prospera.gob.mx/es/bases/bases.php$.

This leads me to create the variable *Exposure*, which maps an individual's birth year and municipality enrollment year to the total number of years of exposure to PROGRESA. Exposure increases as the difference between an individual's birth year and a municipality's enrollment year decreases. Some consideration must be made for individuals born in years 1983 – 1985. In 1997, families are eligible for benefits when children are attending grades three through nine. In 2001, this is extended to grades nine through twelve. Birth year cohorts 1983 - 1985 are old enough to be ineligible for benefits during all of grades ten through twelve prior to 2001 but young enough to receive partial exposure after 2001. Children born up to and including 1982 will have zero years of exposure, and children born in and after 1989 will have at most ten years of exposure. The function that determines exposure is reproduced in Appendix A. A shortcoming of this construction related to the data is that I can not distinguish between different times of the birth year when children are born. Children born at different times in the same year will potentially start school at different times. Therefore, it is possible that children born in the same year may have different exposure based on when they begin school. However, this can only be a problem if mothers make fertility decisions in a way that is systemically coordinated with the timing of PROGRESA enrollment, and this seems unlikely.

Table 1 presents summary information for *Exposure*, as well as other covariates of interest. Note that Exposure behaves as expected. The values are zero for people in the sample born in or before the year 1982, which is expected. These individuals are too old to receive benefits. Exposure is increasing in birth year, and the average increases as expected. People born in 1983 should have at least one year and at most two years of benefits because they are young enough to receive benefits in their ninth grade year and possibly in their twelfth grade year, based on the 2001 change in the mandate, depending upon when they entered school. This benefits structure is roughly the same for people born in 1984 and 1985: they are old enough to receive benefits in their seventh through ninth grade years and young enough to receive benefits in their tenth through twelfth grade years. After 1985, the average increases by approximately one for every year increase in birth year until 1990. Since students can not be exposed to benefits for longer than 10 years, people born in and after 1989 should have an average exposure value approaching 10 years.

I introduce the variable *Skilled* to identify individuals who work at jobs that likely require a college degree based on the methodology and occupational descriptions in the International Standard Classification of Occupations ("ISCO"), published by the International Labor Organization ("ILO"). The ISCO structure uses four skill levels that approximately correspond with completed schooling levels: primary, secondary, high school, and college. The occupational definitions categorize jobs based on duties and responsibilities, resulting in a uniform reference for research. I use ISCO-08 definitions, which is the fourth version of classifications. The ISCO designations are harmonized in the Census data based on the source descriptions taken from the CONAPO. In my data, the variable *occisco* takes on 10 different values, and I link each of these to a value of *Skilled = 1* if the ISCO description states a skill level of three or four, indicating that a college degree is most likely required, or a value of *Skilled = 0* if the ISCO description states a skill level of one or two, indicating that a college degree is not required.[4]

Of critical importance is the nature of the difference between skill levels one and two and skill levels three and four. The ISCO structure explains that occupations at skill levels three and four generally require a high degree of literacy and numeracy and highly developed interpersonal communication skills, whereas occupations at skill levels one and two require basic or at most advanced literacy and numeracy and good interpersonal communication skills. Occupations at skill levels three and four are regularly involved in complex problem-solving in highly technical and specialized fields. If the effect of PROGRESA is to increase the likelihood of going to and completing high school and college, then there should be a

[4]An important exception to this categorization is *armed forces*. The ISCO acknowledges that people in the armed forces have jobs at all skill levels and does not attempt to differentiate armed forces jobs. As a consequence, these people are coded as *Skilled = 0* in the data. There are 4,244 individuals in the armed forces in the sample.

corresponding effect on the likelihood of obtaining a skilled job. Summary information for *Skilled* and other outcome variables is presented in Table 2.

4 Empirical Strategy

The purpose of the analysis is two-fold: first, to determine the effects of PROGRESA enrollment on labor outcomes, and second, to inform an assessment of labor market responses to the change in improved human capital. The ideal experiment for estimating these effects would assign treatment to participants and control to non-participants and observe their employment and wage outcomes, as well as a number of measures to act as controls. Attrition and migration in response to treatment assignment should also be observed. In fact, the previously mentioned survey data attempts this design (citation here). Among the potential problems with this design, one problem stands out. Assignment of treatment is highly correlated with poverty which may result in a selected sample. Indeed, the determination of eligibility in the earliest years of the program was defined by poverty.[5] Since the people who are eligible to receive benefits can be viewed as the "most impoverished," coefficient estimates will be influenced by factors associated with extreme poverty that may not be shared by the control group. To illustrate the potential problems with this, if extreme poverty is highly correlated with participation, then the control group will initially be better off, and estimates could be biased toward finding no effect. Since there was roughly 96-97% participation among those who were offered benefits, this could be a threat to unbiased estimation.

I overcome this selection problem by employing a fixed effects identification strategy to estimate ITT effects. Since I am measuring the effect of living in an enrolled municipality regardless of an individual's participation, I am by definition measuring an ITT effect. Fixed effects control for time-invariant factors within municipalities and birth year cohorts. The

[5] Add further description here as a footnote.

result, which I explain presently, avoids the selection problem at the cost of estimating an average treatment effect.

I implicitly hypothesize that the primary channel for improving human capital via PROGRESA is increased levels of education. More education leads to improved labor outcomes. I do not attempt to control for the supply response to increased demand in the education market. If the quality of education improves in response to more children attending school, then coefficient estimates may be biased toward finding an effect. Nor do I attempt to disentangle effects on human capital caused by improved nutrition and other health factors. Since one of the requirements of mothers receiving PROGRESA benefits is to maintain certain standards in nutrition and health check-ups, these may also bias estimates toward finding an effect. These are limitations of my strategy, and further research may explore the long-term effects of these changes on labor outcomes in an endeavor to determine which of health or education drives returns to human capital in PROGRESA. To arrive at an estimate of human capital improvements, I estimate the effect of exposure on four outcomes: completed grade levels, likelihood of being employed, likelihood of being employed in a skilled job, and the natural log of hourly wages.

A naïve specification to uncover the relationship of interest is:

$$y_{im} = \beta exposure_{im} + \varepsilon_{im} \tag{1}$$

where y is completed levels of schooling, likelihood of being employed, likelihood of being in a skilled occupation, and natural log of hourly wages; i is an individual, m is a municipality, and ε_{im} is an error term. Results from (1) will be biased if there are unobserved covariates in ε_{im} that are simultaneously correlated with the outcomes of interest and with the timing of PROGRESA enrollment. To resolve this bias, I include birth year cohort and municipality fixed effects. Consider that people born in the later years of the sample will not have been alive long enough to attend more schooling, such as college, or to attain higher wages resulting from more job experience. Similarly, since *Skilled* is correlated with years of education by construction, one expects that younger people in the sample will be less skilled. This poses an issue for bias in coefficient estimates if people who are born in the later years of the sample receive systemically different wages than people born in earlier years of the sample would have received at the same age. That is, if someone born in 1990, who is 20 in 2010, received a different wage measured in real terms than someone born in 1980 would have received at age 20 for a reason that is correlated with receipt of PROGRESA benefits, then this will cause bias in coefficient estimates. Municipality and birth year fixed effects offer a solution to this problem. My strategy assumes birth year cohorts are adequate control groups for consecutive birth year cohorts, and unenrolled families and localities are adequate control groups for enrolled families and localities within the same enrolled municipality. My strategy further assumes that unenrolled municipalities are adequate control groups for enrolled municipalities within the same birth year cohort. Therefore, I update (1) to be:

$$y_{im} = \beta exposure_{im} + \delta_{yob} + \mu_m + \varepsilon_{im} \tag{2}$$

In (2), δ_{yob} captures all factors that are constant and shared among individuals in the same birth year cohort, and μ_m captures all factors that are constant within a municipality over time. This strategy is threatened by anything that changes within a birth year cohort that differentially affects people in that cohort. One example is intrayear migration. If parents systemically migrate out of enrolled municipalities and into enrolled municipalities, then this breaks down. By employing the municipality and birth year fixed effects, I am able to treat specification (2) as a difference-in-differences estimation. This will have the effect of controlling for a person's age and birthplace[6] such that schooling and wages are comparable across cohorts.

[6] I implicitly assume that a person is born and educated in the same municipality since I do not observe the municipality of birth, only the municipality in which a person attends school.

In light of this, a better schooling outcome to measure is grade levels completed. I therefore estimate (2) on primary, secondary, and high school completion, and on completion of some college. The result is a linear probability model that yields the likelihood of completing these grade levels. I also use (2) to test the association between labor outcomes and *Exposure*. Coefficient estimates from (2) will describe an increase in the likelihood of obtaining a job and an increase in the likelihood of obtaining a job that requires a college degree.

5 Results

Main Results

Estimation of specification (2) for the effect of *Exposure* on grade levels completed is presented in Table 3. Since this is a linear probability estimation, the relationship is given by: $coefficient\, estimate/mean\, of\, dependent\, variable = \%\, increase\, in\, probability$. For example, in column (1) of Table 2, an additional year of exposure to PROGRESA is associated with a $0.00929/0.8827048 = 1.05\%$ increase in the likelihood of completing primary school controlling for municipality and birth year fixed effects. These estimates suggest that there is a uniform increase in the relationship between exposure to PROGRESA and the likelihood of completing primary, secondary, and high school. However, the relationship is negative for the likelihood of at least some college and exposure to PROGRESA for the whole sample. Thus there is evidence to suggest that the effect of PROGRESA is to increase the level of compulsory and high school education. By contrast, there is weak evidence to suggest that PROGRESA negatively affected college attendance. In light of the research question, this may suggest that the labor market was incentivizing secondary education, or possibly vocational as will be discussed later, over college education in response to the effects of PROGRESA. A simple explanation is that the PROGRESA funds enabled families to pay for secondary and high schooling, and once those children graduated, they had insufficient funds to pay for college.

Estimates of specification (2) on labor outcomes are presented in Table 3. There is a uniform increase in labor outcomes in response to PROGRESA. People are more likely to be employed, be employed in a skilled occupation, and the estimate of the natural log of hourly wages can be interpreted as approximately 1% increase in hourly wages associated with an additional year of PROGRESA exposure. Sample sizes change because variables are coded conditionally. For example, the effect of finishing secondary education is only estimated for those who finished primary education. My sample is restricted to those already participating in the labor force. The effect in *Employed* should be interpreted as an increase in the likelihood of finding a job conditioned on looking for a job. The effect on *Skilled* indicates the likelihood of being in a job that requires a high degree of numeracy, literacy, and interpersonal communication skills (and likely a college degree) conditioned on being employed. Taken together, these results provide some evidence to suggest that the ITT effect of PROGRESA enrollment is a higher likelihood of completion of compulsory and high school education leading to higher employment, more skilled employment, and an increase in wages. There is insufficient evidence to conclude that the increase in skilled employment is due to higher college attendence.

Heterogeneity Analysis

In this section, I present estimates of (2) on different segments of the sample. PROGRESA was originally restricted to rural populations. Results in Table 5 for suggest that PROGRESA had the effect of increasing primary and secondary completion rates but had no effect on high school completion rates. Moreover, they had little to no effect in labor outcomes. This may likely be due to lack of rural area job opportunities. Parker and Vogl (2018) find some evidence that agricultural employment decreased for men. While my results do not contradict this, I do not find sufficient evidence to suggest that there is a shift from unskilled (i.e. - agricultural) to skilled labor for rural individuals.

Tables 6 and 7 compare results for men and women. Estimates for the likelihood of completing compulsory education indicate that the effect is stronger for men than for women. However, this switches at high school: women are more likely than men to graduate high school. Once again, the effect is negative for some college, and it is stronger for women. Effects are similar for employement, and women are more likely to have a skilled occupation. It is possible that cultural norms may be working to prevent women from college educations even though they exhibit stronger likelihood of completing high school. Completing high school, however, appears to translate into more skilled opportunities for working women. Men experience a stronger wage effect.

6 Robustness Checks

In this section, I test the robustness of estimates from (2). First, I restrict the sample by removing individuals with zero years of exposure and test whether, conditioned on any exposure, the effects are stronger for more exposure. Second, I add state-specific linear time trends. Following this, I alter the definition of *Skilled* to assess the sensitivity of estimates to different classifications of skilled jobs. Finally, I attempt to control for migration.

Restricted Sample

Removing individuals with zero years of exposure alters coefficient estimates. Conditioned on any exposure to PROGRESA, the marginal effects become uniformly negative in sign, and only the effects on *Some College* and *Skilled* remain significant. Thus, conditioned on any exposure there seems to be no effect from *when* a person is exposed. PROGRESA has the effect of enrolling more people in school, but there is no evidence that people at different stages in their education experience greater or lesser effects.

State Linear Trends

Adding state-specific linear time trends controls for everything that moves linearly through time within a state. Important to this is that these linear time trends will be common to all municipalities within a state. Therefore, macroeconomic conditions that evolve linearly are controlled for. In addition, this may provide some control for the education supply response to PROGRESA to the extent that education policy is set at the state level.[7] These time trends dramatically reduce the magnitudes of the education effects, but they remain significant. The effect on *Some College* become more negative. For employment effects, the effect on the likelihood of being employed is reduced but remains significant, and the effects on *Skilled* and *ln(Hourly Wage)* evaporate.

Alternative Definitions of ***Skilled***

Changing the definition of *Skilled* yields potentially fruitful insights. Recall the previous discussion about the correlation between education levels and skill levels assignments of occupational categories. Skill levels three and four both require college degrees. Skill level two does not necessarily require a college degree, but may benefit from vocational education. In this portion of the analysis, I explore the sensitivity of the definition of *Skilled* to *Exposure* to ascertain whether human capital development is responding to professional or vocational education.

Skilled2 restricts the definition to skill level four. These occupations include executives and legislators and require the highest degree of literacy and numeracy. They regularly engage in solving the most challenging problems. *Skilled3* expands the definition to include skill level two. These occupations include a wide spectrum of skills and generally require completion of secondary school. They likely require specialized on-the-job training and benefit from vocational education. If the effect of PROGRESA is to increase secondary school and high

[7]Limited in computational resources due to the large amount of data restricts me from estimating municipal linear time trends.

school graduation but not college enrollment or completion, significant effects of *Exposure* on *Skilled3* could provide evidence that the labor market is incentivizing vocational training and skills in addition to professional skills. In other words, wage effects may be driven by a combination higher demand for vocational occupations than for professionals, suggesting the distribution of human capital development is concentrated at skill levels two and three rather than three and four.

Estimates in Table 7 provide some evidence to support this line of reasoning. This table presents the estimates of *Some College* and *Skilled* for comparison alongside a probability estimate for graduating college and the variables *Skilled2* and *Skilled3* described above. There is a negative likelihood of achieving *some college* and no effect on completing a college degree, and there is a significant, positive likelihood estimate for *Skilled3*. Taken together with the education estimates, the interpretation here is that an additional year of PROGRESA enrollment is associated with a lower likelihood of going to college, a negligible likelihood of earning a college degree, and a 0.2% increase in the likelihood of obtaining a job at skill level two, three, or four. Thus there is some evidence for the increases in secondary and high school education driving increases in *vocational* employment. Further work is required to find more conclusive evidence for whether supply or demand in the labor market is the predominant driver of wage returns.

Migration

To control for migration, I observe the respondent's migration status five years prior to the survey. The effects are significant and almost identical in magnitude. Although this is an imperfect measure of migration, it suggests that effects are not being driven by migration. Moreover, roughly 89.8% of individuals in the sample have not migrated outside of the same major or minor administrative unit in the previous five years.

7 Conclusion

PROGRESA, and CCTs generally, represent a pivotal shift in treating chronic, inter-generational poverty by attempting to treat a cause of such poverty, namely inadequate human capital development. Although these programs have lofty aims, policymakers lack evidence to support or refute that these programs achieve their goals because child recipients have yet to be observed as adults. Thus policymakers can not make an informed decision about dedicating more resources to the widespread distribution of cash to families. My research shows that PROGRESA has been effective achieving better economic outcomes in early adulthood for children who benefited from the program.

Work remains to be done. Given more time, I would attempt to control more rigorously for systemic migration. My research does not provide conclusive evidence that the primary channel for increasing skilled labor is through more college education. However, I do provide some evidence that increases in human capital are substantially driving vocational labor supply. It is possible that the labor market is favoring these jobs over professional, college educated jobs. After some time has passed, it will be useful to test whether these effects are sustained. It would also be useful for future research to estimate the response of education supply to the increase in enrollment and completion rates of student beneficiaries of PROGRESA.

To the extent that CCTs in other countries are modeled after PROGRESA, my results suggest that other developing countries can experience returns to CCTs in the labor market. However, given that there are two channels by which PROGRESA worked, nutrition and education, other countries' programs would benefit from more detailed study about whether nutrition or education effects dominate to target their programs to their unique circumstances.

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Appendix A: Exposure Function

Birth year	Enrollment year	Birth year - Enrollment year		Years of Exposure
[1980, 1992]	[1998, 2010]	[-20, -19]		0
		[-18, -16]	*Case 1:* $yob \leq 1982$	0
		[-18, -16]	*Case 2:* $yob = 1983$	1
		[-18, -16]	*Case 3:* $yob = 1984$	2
		[-18, -16]	*Case 4:* $yob \geq 1985$	3
		-15	*Case 1:* $yob = 1983$	2
		-15	*Case 2:* $yob = 1984$	3
		-15	*Case 3:* $yob \geq 1985$	4
		-14	*Case 1:* $yob = 1984$	4
		-14	*Case 2:* $yob \geq 1985$	5
		-13		6
		-12		7
		-11		8
		-10		9
		[-9, -6]		10

Appendix B: Summary Information

Table 1: Summary Statistics for Independent and Demographic Variables

	(1) Exposure	(2) Female	(3) Literate	(4) Rural
1980	0	0.304	0.939	0.399
	(0)	(0.460)	(0.240)	(0.490)
1981	0	0.306	0.955	0.381
	(0)	(0.461)	(0.208)	(0.486)
1982	0	0.310	0.954	0.382
	(0)	(0.462)	(0.209)	(0.486)
1983	1.457	0.312	0.959	0.382
	(.716)	(0.463)	(0.198)	(0.486)
1984	3.151	0.319	0.964	0.386
	(1.287)	(0.466)	(0.185)	(0.487)
1985	4.889	0.318	0.963	0.399
	(1.872)	(0.466)	(0.189)	(0.490)
1986	5.977	0.314	0.966	0.406
	(1.733)	(0.464)	(0.181)	(0.491)
1987	6.928	0.311	0.970	0.420
	(1.876)	(0.463)	(0.170)	(0.494)
1988	7.892	0.302	0.971	0.434
	(2.039)	(0.459)	(0.167)	(0.496)
1989	8.874	0.302	0.976	0.437
	(2.123)	(0.459)	(0.1534)	(0.496)
1990	9.313	0.292	0.971	0.472
	(1.834)	(0.455)	(0.167)	(0.499)
1991	9.51	0.289	0.976	0.484
	(1.599)	(0.453)	(0.154)	(0.500)
1992	9.718	0.258	0.970	0.522
	(1.096)	(0.437)	(0.170)	(0.500)
Observations		1,380,807		

Standard deviations in parentheses

Table 2: Summary Statistics for Outcome Variables

	(1) Years of Schooling	(2) Employed	(3) Skilled	(4) ln(Hourly Wage)
1980	8.799	0.960	0.132	3.068
	(4.563)	(0.195)	(0.338)	(0.842)
1981	9.251	0.960	0.143	3.093
	(4.460)	(0.197)	(0.351)	(0.820)
1982	9.266	0.958	0.141	3.075
	(4.435)	(0.201)	(0.348)	(0.813)
1983	9.474	0.956	0.143	3.061
	(4.370)	(0.206)	(0.350)	(0.806)
1984	9.597	0.952	0.141	3.055
	(4.291)	(0.214)	(0.348)	(0.798)
1985	9.561	0.948	0.131	3.014
	(4.209)	(0.223)	(0.338)	(0.781)
1986	9.553	0.945	0.121	2.977
	(4.060)	(0.228)	(0.326)	(0.771)
1987	9.451	0.941	0.100	2.930
	(3.811)	(0.236)	(0.300)	(0.749)
1988	9.277	0.939	0.076	2.877
	(3.498)	(0.240)	(0.264)	(0.717)
1989	9.255	0.936	0.061	2.847
	(3.165)	(0.244)	(0.239)	(0.692)
1990	8.909	0.928	0.048	2.787
	(3.057)	(0.258)	(0.213)	(0.692)
1991	8.814	0.922	0.040	2.754
	(2.828)	(0.267)	(0.196)	(0.668)
1992	8.200	0.914	0.028	2.691
	(2.673)	(0.280)	(0.164)	(0.681)
Observations		1,380,807		

Standard deviations in parentheses

Appendix C: Main Results

Table 3: Completed Levels of Schooling

	(1) Primary	(2) Secondary	(3) High School	(4) Some College
Exposure	0.00929***	0.00834***	0.00919***	-0.00255*
	(0.000568)	(0.000564)	(0.00133)	(0.00130)
Means	0.883	0.755	0.475	0.467
Observations	1,380,103	1,218,262	919,470	436,338

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$
Standard errors are clustered at the municipality level.

Table 4: Labor Outcomes

	(1) Employed	(2) Skilled	(3) ln(Hourly Wage)
Exposure	0.00147***	0.00845***	0.0104***
	(0.000240)	(0.00126)	(0.00224)
Means	0.943	0.102	-
Observations	1,380,103	1,290,539	1,004,514

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$
Standard errors are clustered at the municipality level.

Appendix D: Heterogeneity Analysis

Table 5: Rural == 1

	(1) Primary	(2) Secondary	(3) High School	(4) Some College	(5) Employed	(6) Skilled	(7) ln(Hourly Wage)
Exposure	0.0102***	0.00955***	0.00244	-0.0156***	0.00129*	0.000918	0.00175
	(0.00210)	(0.00165)	(0.00228)	(0.00400)	(0.000774)	(0.000903)	(0.00233)
Means	0.825	0.658	0.356	0.313	0.946	0.051	2.755
Observations	582,246	480,159	315,798	112,563	582,246	545,837	352,893

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$
Standard errors are clustered at the municipality level.

Table 6: Male == 1

	(1) Primary	(2) Secondary	(3) High School	(4) Some College	(5) Employed	(6) Skilled	(7) ln(Hourly Wage)
Exposure	0.00955***	0.00895***	0.00882***	-0.00104	0.00142***	0.00782***	0.0116***
	(0.000680)	(0.000686)	(0.00139)	(0.00142)	(0.000341)	(0.00115)	(0.00246)
Means	0.866	0.720	0.413	0.416	0.935	0.069	2.947
Observations	961,554	832,612	599,349	247,350	961,554	891,395	667,834

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$
Standard errors are clustered at the municipality level.

Table 7: Female == 1

	(1) Primary	(2) Secondary	(3) High School	(4) Some College	(5) Employed	(6) Skilled	(7) ln(Hourly Wage)
Exposure	0.00813***	0.00729***	0.00962***	-0.00461**	0.00140**	0.00828***	0.00703**
	(0.000558)	(0.000796)	(0.00151)	(0.00154)	(0.000453)	(0.00152)	(0.00217)
Means	0.921	0.830	0.590	0.534	0.963	0.176	2.960
Observations	418,549	385,650	320,121	188,988	418,549	399,144	336,680

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$
Standard errors are clustered at the municipality level.

Appendix E: Robustness Checks

Table 8: Restricted Sample

	(1) Primary	(2) Secondary	(3) High School	(4) Some College	(5) Employed	(6) Skilled	(7) ln(Hourly Wage)
Exposure	-0.00263**	-0.000869	-0.00403**	-0.0101***	-0.00149*	-0.00647***	-0.00440
	(0.00107)	(0.00146)	(0.00189)	(0.00249)	(0.000836)	(0.00116)	(0.00269)
Observations	997,510	890,793	670,718	302,369	997,510	928,006	713,178

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$
Standard errors are clustered at the municipality level.

Table 9: State Linear Time Trends

	(1) Primary	(2) Secondary	(3) High School	(4) Some College	(5) Employed	(6) Skilled	(7) ln(Hourly Wage)
Exposure	0.00534***	0.00444***	0.00235**	-0.00462**	0.000655**	0.00125	0.00157
	(0.000615)	(0.000832)	(0.00114)	(0.00140)	(0.000323)	(0.000822)	(0.00157)
Observations	1,380,103	1,218,262	919,470	436,338	1,380,103	1,290,539	1,004,514

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$
Standard errors are clustered at the municipality level.

Table 10: Changing Definition of Skilled

	(1) Some College	(2) College Degree	(3) Skilled	(4) Skilled2	(5) Skilled3
Exposure	-0.00255**	0.000369	0.00845***	0.00887***	0.00159***
	(0.00130)	(0.00173)	(0.00126)	(0.00125)	(0.000757)
Mean	0.467	0.308	0.102	0.053	0.816
Observations	436,388	436,388	1,290,539	1,290,539	1,290,539

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$
Standard errors are clustered at the municipality level.

Table 11: Controlling for Migration

	(1) Primary	(2) Secondary	(3) High School	(4) Some College	(5) Employed	(6) Skilled	(7) ln(Hourly Wage)
Exposure	0.00938***	0.00822***	0.00890***	-0.00267**	0.00144***	0.00766***	0.0107***
	(0.000571)	(0.000566)	(0.00135)	(0.00132)	(0.000241)	(0.00121)	(0.00225)
Migration status	0.00137***	-0.00193***	-0.00537***	-0.00354***	-0.000454***	-0.00320***	0.00562***
	(0.000120)	(0.000165)	(0.000351)	(0.000499)	(0.0000915)	(0.000157)	(0.000413)
Observations	1,378,159	1,216,539	918,158	435,420	1,378,159	1,288,910	1,003,279

Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.
Standard errors are clustered at the municipality level.