Are Adults in Demand When Children Leave the Land?
Evidence from Rural Mexico

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Abstract

Do employers substitute adults for children, or do they treat them as complements? Any policy to reduce child labor depends on the answer to this question, but any empirical strategy to answer it must overcome two obstacles: (1) whatever program reduces child labor supply must have no direct impact on adult labor demand. (2) Any program that changes child labor supply will almost certainly affect adult labor supply – therefore, changes in adult labor demand must be identified without assuming constant adult labor supply. I hypothesize and establish that schooling experiments can reduce child labor supply without directly affecting adult labor demand. Furthermore, my strategy can identify changes in adult demand without assuming constant adult supply, by analyzing coordinated movements in price and quantity. Applying this strategy to a Mexican schooling experiment, I find that a decrease in child field work participation is accompanied by an increase in adult labor demand. This increase was not directly caused by treatment money reaching employers: there were no significant effects on food prices, hectares of land used, or harvest size. Furthermore, the wages of healthy non-treated adults living around children who stopped working also increased, suggesting that treatment-related health increases were not responsible for the wage change. Thus, the fall in child labor supply caused the increase in adult labor demand: in other words, employers substituted adults for children.

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I. Introduction

What happens to adult labor market outcomes when children are removed from the labor force? The empirical evidence regarding this question is scant, while the policy implications are far-reaching (Galli 2001). According to the International Labor Organization's recent estimates, there are 186.3 million child laborers worldwide (Basu & Tzannatos 2003). If we wish to propose government interventions to reduce child labor and encourage education, then the optimal manner of intervention depends on whether or not children and adults are labor substitutes. Where employers substitute adults for children, an increase in adult wages and/or hours will accompany a decrease in child labor, partially offsetting the short-term welfare loss that families face when some of their children are no longer working. In particular, the work of Basu and Van (1998) shows that in this case a ban on child labor could increase household welfare. But where adults complement children – i.e., where adult wages and/or hours decrease when children leave the workforce – interventions to reduce child labor can seriously harm household welfare, and thus such interventions may need to be accompanied by extensive government programs to make up for this loss.

Indeed, the possibility that in developing countries adult labor complements that of children is not necessarily remote: popular wisdom famously cites the supposed "nimble fingers" of children as a reason why children and adults may be complements in industries such as carpet weaving in India (ILO 1996). In addition, from their empirical work on aggregate production functions, Diamond and Fayed conclude that children and adult men are complements in Egyptian industry (Diamond and Fayed 1998). Finally, the 2001 survey by Rosalind Galli cites (apparently phenomenological, task-based)

evidence that suggests that in household production and agriculture, children complement

adults (Galli 2001). However, Galli herself concludes that there is not yet enough good

empirical evidence to support either complementarity or substitutability, and she cites this

issue as a main gap in the empirical literature on child labor.

Despite the mixed evidence and lack of good empirical studies, governments and

international organizations have argued that child labor is a major determinant of adult

unemployment, i.e. that children and adults are substitutes. Thus, there is a pressing need

for empirical work to address the goals and assumptions of policy makers. Galli states:

> The. . .Child Labor Deterrent Act introduced in the United States in 1993 argued that a worldwide
> ban on trading goods produced by child labour would benefit the exporting countries practicing
> child labour through reduced adult unemployment.. . .This idea is not exclusive to the Act, and
> has been often stated by researchers and by the ILO itself in the book 'Combating Child Labour',
> where it is asserted that "…child labour is a cause of, and may even contribute to, adult
> unemployment and low wages …" (ILO 1988: 90). Notwithstanding its popularity, there are very
> few theoretical and applied studies examining the child labour impact on [the] adult labour
> market.

In this paper, I address this empirical gap.

There are two challenges that any such empirical strategy must overcome: (1)

whatever program reduces child labor supply must have no direct impact on adult labor

demand – this allows any changes in adult labor demand to be traced to the change in

child labor supply. (2) Any program that changes child labor supply is almost certain to

affect adult labor supply – therefore, the changes in adult labor demand must be

identified without assuming constant adult labor supply. I hypothesize and establish that

schooling experiments can reduce child labor supply without directly affecting adult

labor demand. Furthermore, my strategy, as developed in Section III, can identify

changes in adult demand without assuming that adult supply has remained constant, by

analyzing coordinated movements in price and quantity. I thereby obtain experimental evidence on the effect of child labor supply shifts on adult labor market outcomes.

I apply this new strategy to Mexico's PROGRESA experiment. I find that a decrease in child field work participation (Section V) is accompanied by an increase in adult labor demand (Section VI). This increase was not directly caused by treatment money reaching employers: there were no significant effects on food prices, hectares of land used, or harvest size (Section VI). Furthermore, the wages of healthy non-treated adults living around children who stopped working also increased, suggesting that treatment-related health increases were not responsible for the wage change (VI).

Thus, the decline in child labor supply must have caused the increase in adult labor demand, or, in other words, employers substituted adults for children.

II. Literature Review

There are very few studies of child labor demand, or of employers' elasticity of substitution between the labor of children and that of other age groups. Parameters of labor demand functions are in general difficult to measure: establishment data is rare, and it is not easy to gather it consistently across multiple establishments. This leaves aggregate data or household surveys; but estimates based on aggregate data suffer from simultaneous equations bias, and household surveys measure the decisions of workers, so in either case one needs a reliable exogenous shift in labor supply or wages. With child labor, these difficulties are compounded because of the problems in identifying the employers, the parents, or the children themselves, and because even when identified

they may be unwilling to share information about their employment, especially where child work is illegal.

Perhaps because of these obstacles, the literature on the parameters of labor demand interactions across age groups is sparse and permits few generalizations. But a survey by Hammermesh (1993) concludes that the (then) current results suggested that most elasticities of substitution "are quite small, implying that changes in the relative [labor] supply of one group will not greatly affect wages received by workers in other groups." Brown, Deardorff and Stern (2002) report the results of Diamond and Fayed (1998), who estimate aggregate production functions from Egyptian household survey data to conclude that "the elasticity of substitution between children and adult females is. . .quite a high figure," but that "adult male and child labor are complementary." Finally, Ray (2000) claims to test Basu and Van's substitution axiom via household surveys in Peru and Pakistan, but only finds evidence of substitution in the case of adult males and children in Peru.[1]

Galli (2001) interprets the existing empirical evidence to conclude: "Whether children actually do substitute [for] adult workers creating adult unemployment and/or reducing adult wage rates remains an open question. . . Further qualitative and scattered evidence suggests that in household-based production activities and in agriculture the complementarities between children and adults are stronger."

However, since each study in this small set uses either aggregate data (producing estimates that suffer from simultaneous equations bias), or household surveys (which, in the absence of some exogenous shift in labor supply, simply produce estimates of

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[1] Ray did *not* test Basu & Van's Substitution Axiom of labor demand, b/c he measured the household's decision to *supply* labor.

parameters of labor supply), it is not possible to draw good conclusions from this literature about causal relationships between child labor supply and adult labor demand.
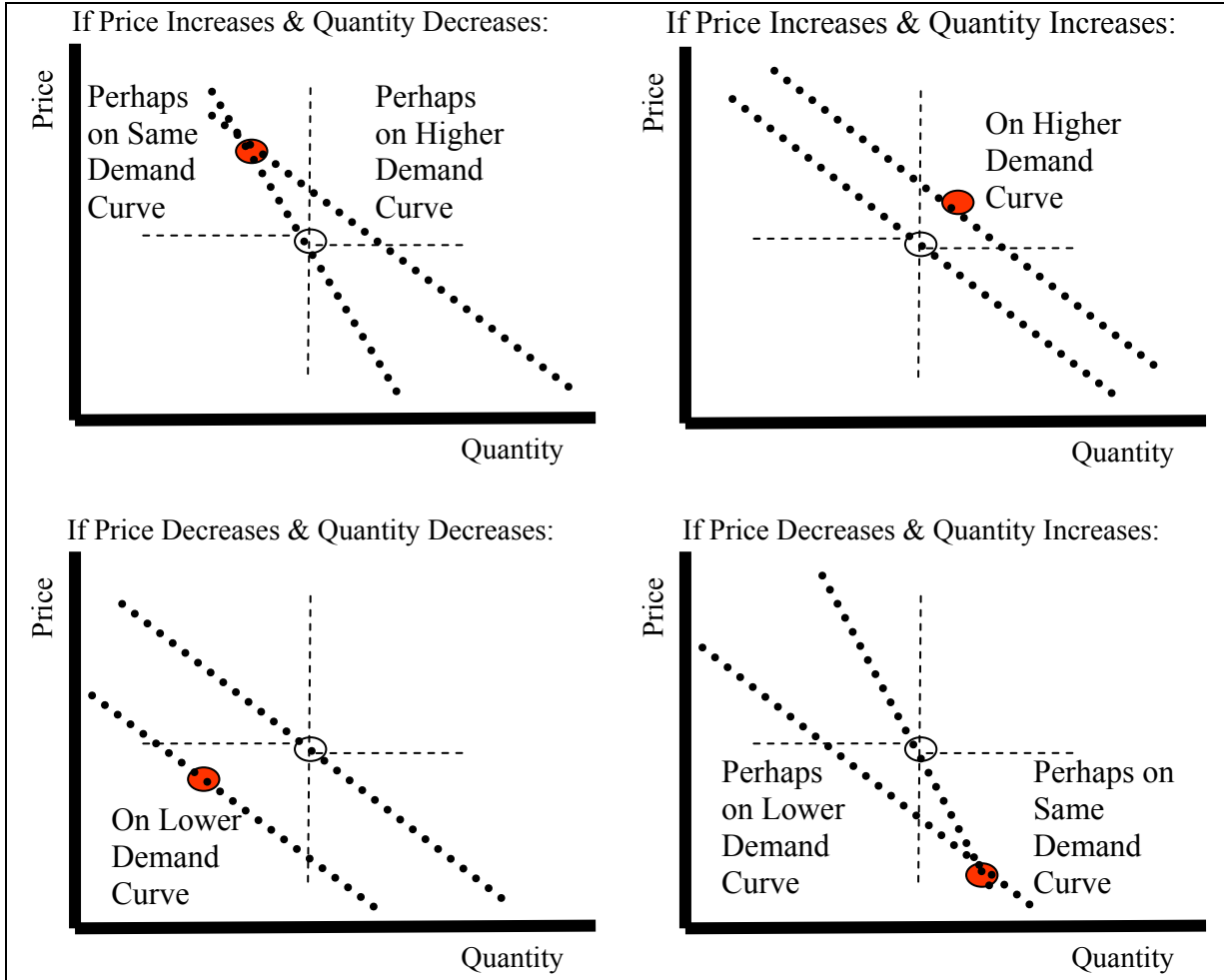
I circumvent these difficulties by using data from PROGRESA, a randomized controlled experiment performed in about 500 villages in rural Mexico, which exogenously reduced the supply of child labor in treatment villages. Employing a strategy that can identify movements in adult demand without assuming constant adult supply, I exploit this exogenous shock to child labor in order to estimate the effect of a decrease in the supply of child labor on the demand for adult labor.

III. Theory and Identification

(A) Intuition

In general, a profit-maximizing firm may treat the labor of adults as either a substitute for or a complement to the labor of children. Empirical evidence can provide the answer by determining how changes in the supply of child labor affect the demand for adult labor. The fundamental problem with this approach is that any treatment that changes the supply of child labor is almost guaranteed to change the supply of adult labor as well. For example, if treatment money is offered to households that supply both child labor and adult labor, then household income effects should reduce adult labor supply. And even if child labor can be reduced without offering treatment money, the change in child labor supply affects household utility, which means the household may re-optimize and change adult labor supply. Thus, it is necessary to find a way to determine the effect of child labor supply on adult labor demand *that does not rely on adult labor supply remaining constant*. I propose such a strategy here.

Figure 1: Determining Demand Movements When Supply Moves as Well

If Price Increases & Quantity Decreases:

If Price Increases & Quantity Increases:

If Price Decreases & Quantity Decreases:

If Price Decreases & Quantity Increases:

If a treatment has increased both the price and quantity of adult labor, then this is sufficient to show that it increased the demand for adult labor – even if the treatment may also have affected the supply of adult labor.   The intuition of this argument is contained in Figure 1. If the demand for adult labor is downward-sloping, then a simultaneous increase in the equilibrium price and quantity of adult labor (or, technically, an increase in one without a decrease in the other) implies that the new price and quantity are on a new, higher, demand curve. If they were on the same demand curve, then that demand curve would not be everywhere downward-sloping. Supply movements only obscure the direction of demand movements when they are large enough to cause prices and

quantities to move in opposite directions. In such a case, the new equilibrium price and quantity may indeed lie on the same demand curve, on a higher demand curve, or on a lower demand curve, making demand movements ambiguous.[2]

Intuitively, the only assumptions are therefore: prices and quantities are always on a demand curve and not off of it (i.e., the economy is in *an* equilibrium, though not necessarily the only equilibrium); and all such demand curves are downward-sloping everywhere. By demonstrating that it is not necessary to find a treatment that had no effect on adult labor supply, I am able to use the PROGRESA treatment to measure the effect of decreases in child labor supply on adult labor demand – even though PROGRESA is similar to any hypothetical treatment in that it potentially changes adult labor supply wherever it changes child labor supply.

The next part of the argument depends on a decrease in child labor supply being the cause of this identified change in adult labor demand. In order to make a conclusion about causality, I must rule out other causal pathways connecting treatment money and incentives to adult labor demand. The obvious causal pathways to rule out involve treatment money affecting inflation, investment by employers, or productivity of employees. Note I do not need to rule out any of the many and various ways that treatment money may affect adult labor supply, by the argument above. I merely must rule out treatment money directly affecting adult labor demand.
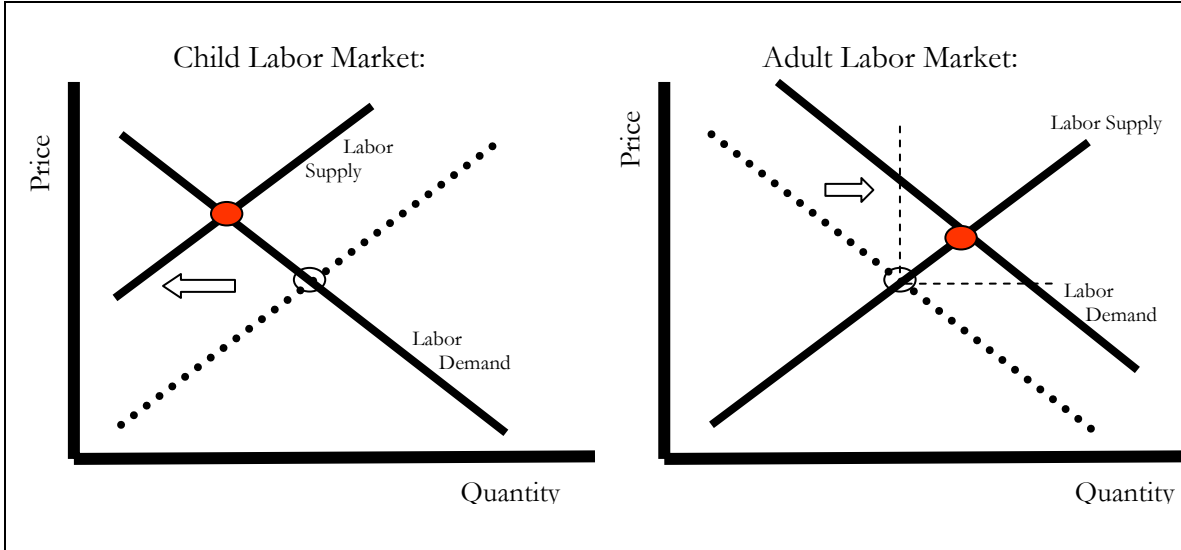
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[2] The labor economics literature has long known of the usefulness of coordinated movements in price and quantity to estimate the direction of demand movements. For a much earlier example of this strategy, see the classic paper (Katz and Murphy 1992).
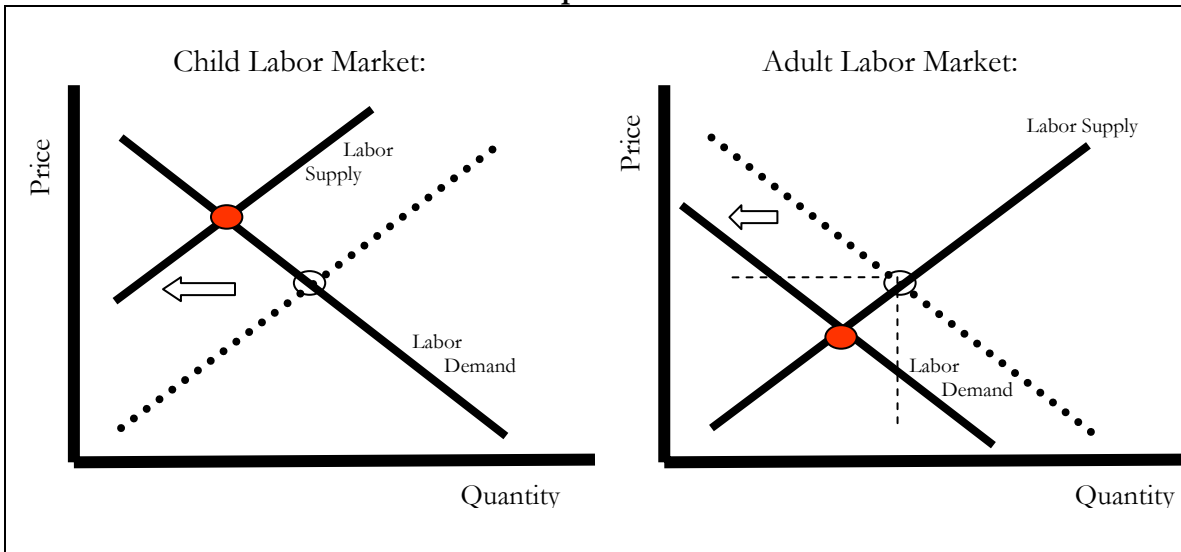
Figure 2: Identification Strategy

Substitutes:

Child Labor Market:

Price / Quantity — Labor Supply, Labor Demand

Adult Labor Market:

Price / Quantity — Labor Supply, Labor Demand

Complements:

Child Labor Market:

Price / Quantity — Labor Supply, Labor Demand

Adult Labor Market:

Price / Quantity — Labor Supply, Labor Demand

Thus, the intuition of the identification strategy is contained in Figure 2. I find a treatment that changes child labor supply. If prices and quantities of adult labor move in the same direction, then I can identify the direction of any changes in adult labor demand. Treatment money is used to change child labor supply. If I can rule out other pathways between this treatment money and adult labor demand, then I can identify whether employers treat adults as substitutes or complements to children.

(B) More Details

Formally, I define substitution and complementarity as follows:

Definition: *Substitutability and Complementarity*

Let w_C^i = the wage paid to children in period i, and let w_A^i = the wage paid to adults in period i, where $i = 1, 2$.

Let $D_A^*(w_A, w_C)$ = a firm's profit-maximizing demand for the labor of adults as a function of the wages paid to adults and the wages paid to children.

Adults *substitute* for children if:

$\forall \; w_C^1$, and $\forall \; w_C^2 > w_C^1$, it is true that $D_A^*(w_A, w_C^1) > D_A^*(w_A, w_C^2)$, $\forall \; w_A$

Adults *complement* children if:

$\forall \; w_C^1$, and $\forall \; w_C^2 > w_C^1$, it is true that $D_A^*(w_A, w_C^1) < D_A^*(w_A, w_C^2)$, $\forall \; w_A$

Intuitively, if the labor supply of children decreases, thus increasing their wages, then the children's employers could either increase demand for adult labor (in which case adults and children are substitutes) or decrease it (in which case they are complements). If there are other inputs in the firm's production function – such as capital – then in fact adults may be substitutes for children while children complement adults. Thus, since the exogenous variation in this paper is over child labor supply, here I will only study whether adults substitute for or complement children, not the converse question.

In order to determine the effect of a treatment that varies child labor on the demand for adult labor, I first must determine the effect of that treatment on the equilibrium price and quantity of adult labor. I assume that the treatment effect on the equilibrium price and equilibrium quantity of adult labor is equivalent to the treatment effect on the distributions of prices and quantities of adult labor. Thus, I am assuming

that if the equilibrium wage increases in treatment areas, then the distribution of wages in treatment areas will shift out. Such a shift can potentially be measured by a change in the mean wage, a change in various deciles of the wage distribution, or by a test of whether the latter wage distribution first order stochastically dominates the former. Using the effect of the treatment on the observed price and quantity of adult labor, I can infer with few assumptions the effect of the treatment on adult labor demand.

Assume that market demand for adult labor may always be represented by some function of its price, $D(p)$. Let the control group demand for adult labor be $D_{Control}(p)$, and suppose that this function describes the pre-treatment demand in the treatment group as well.

Proposition 1:   If, $\forall p$,

$$\partial D_{Control}(p) \Big/ \partial p < 0 \text{, and}$$

if $p_{Treated} > p_{Control}$ and $q_{Treated} \geq q_{Control}$, then:

$\exists$ a new demand function $D_{Treated}(p)$ such that $q_{Treated} = D_{Treated}(p_{Treated})$.

Proof:

At the control group competitive equilibrium market price, $p_{Control}$, the equilibrium quantity will be

$$q_{Control} = D_{Control}(p_{Control}) .$$

We are given that

$$p_{Treated} > p_{Control} \text{, and}$$

$$q_{Treated} \geq q_{Control} = D_{Control}(p_{Control}) .$$

Then, because $\forall p$, $\partial D_{Control}(p) \big/ \partial p < 0$,

it is clear that

$$p_{Treated} > p_{Control} \;\Rightarrow\; D_{Control}(p_{Treated}) < D_{Control}(p_{Control}).$$

Therefore,

$$D_{Control}(p_{Treated}) < D_{Control}(p_{Control}) \leq q_{Treated}, \text{ so:}$$

$$D_{Control}(p_{Treated}) \neq q_{Treated}.$$

Thus, there must $\exists$ a new demand function $D_{Treated}(p)$, such that

$$q_{Treated} = D_{Treated}(p_{Treated}). \hspace{4cm} \text{QED.}$$

While it is thus clear that the demand for labor has shifted to a new function, it is not

necessarily the case that $D_{Treated}(p) > D_{Control}(p)$, $\forall p$. Thus, such a shift in equilibrium

price and quantity proves that demand has shifted in some way, and that in particular it

has *increased* for at least one price level. More complete evidence in support of the

hypothesis that demand has *increased everywhere* (i.e. for all p, as in the definition of

substitutability), would of course consist of other observations of price and quantity along

the same new demand curve. If this increase in adult labor demand is caused by a

treatment whose only effect on adult labor demand is through a change in child labor

supply, then this increase in adult labor demand is evidence that adults and children are

substitutes, by the definition of substitutability above.

In the following sections, I describe my empirical strategy to determine whether

the PROGRESA experiment: (1) decreased child labor supply; (2) increased the price and

quantity of adult labor; and (3) affected adult labor demand only through changes in child

labor supply, and not through treatment benefits (direct or indirect) to adults or through

treatment money reaching the farms who hired adult labor. Based on the empirical results that verify these three points, and on the proposition proved above, I conclude that the PROGRESA experiment provided evidence that adults and children are substitutes.

IV. Data

Mexico's Program in Educación, Salud y Alimentación (ProgrESA) or "The Program in Education, Health and Nutrition", was the first large scale schooling experiment in Latin America. PROGRESA was designed to promote education and health in poor rural areas of Mexico. It began with an experimental phase, one of whose primary aims was to determine whether, if payments were made to families conditional on their children's school attendance, school attendance would increase in the treatment group. Census and administrative data identified 506 villages in rural Mexico as "poor" (Skoufias & Parker 2001). Of these villages, 320 were randomly selected to form the treatment group. The remaining 186 villages formed the randomized control group.[3]

Five surveys were conducted over households in all 506 villages at the following times: October 1997, March 1998, October 1998, May 1999 and November 1999. In the Spring of 1998, the Mexican government announced that it would give benefits (conditional on the children's school attendance and family participation in health and nutrition programs) to the eligible families of the treatment group. The first payments were made in May 1998. Thus, the first two surveys are pre-treatment, and the latter three surveys are during the treatment. After the experimental phase was complete, eligible families in the control group began receiving benefits as well.

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[3] See Behrman & Todd 1999 for a discussion of the randomization procedure.

PROGRESA administrators used the results of the October 1997 census to determine, bsed on variables associated with household welfare, the families that were relatively poor. It assigned these families to the eligible group, assigning relatively well-off families to the non-eligible group (Skoufias, Davis, Behrman 1999). This assignment was conducted for families in both control and treatment villages. Eligible families in the treatment group of villages received conditional benefits targeted towards improving education and health.[4] If a child under 18 missed less than 15 percent of the school days in a particular month, then PROGRESA provided a cash award that month to the mother of the child. Cash awards increased to keep pace with inflation, increased with the grade of the child, and were higher for girls than boys. These monthly grants ranged from about 80 pesos for third graders to 280 pesos for ninth grade boys and 305 pesos for ninth grade girls. As a comparison, in 1997 the average monthly salary income of an adult jornalero was about 600 pesos, and that of a child jornalero was about 500 pesos. The program also provided basic health care for all family members and a fixed monetary transfer for nutritional supplements (Skoufias & Parker 2001).

I make use of data from this experimental phase of PROGRESA. I obtained the data from the Opportunidades office, which is the new name for the agency that currently runs PROGRESA. The same raw data set that I used to construct my own data set can be downloaded from their website. I make use of three surveys that were conducted at the same time in the agricultural cycle (October/November): the pre-treatment survey in 1997 and two post-treatment surveys in 1998 and 1999. The 506 villages in the experiment were located in seven Mexican states, shown shaded in Figure 3.

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[4] The eligibility status was revised in 1998, and according to my data the number of eligible families was higher in 1998 than in 1997 and higher still in 1999.

Figure 3: States in Mexico where the PROGRESA Experiment took place

State	Number of Observations in 1997	Percent
Guerrero	10,419	8.29
Hidalgo	21,645	17.22
Michoacán	15,133	12.04
Puebla	19,683	15.66
Queretaro	7,310	5.82
San Luis Potosí	20,125	16.01
Veracruz	31,359	24.95
Total	*125,674*	*100.00*

Table 1: First Response to Principal Activity & Crop Questions, Local Survey, 1997

Question	Response	Villages listing response	Percentage
Principal Activity in this village?	Agriculture	491	97.8%
	Commerical	3	0.6%
	Ganaderia	3	0.6%
	Artisan Production	1	0.2%
	Construction	1	0.2%
	Industrial Production	1	0.2%
	Services	1	0.2%
	Other	1	0.2%
Principal Crop in this village?	Corn (Maiz)	443	88.2%
	Beans	20	4.0%
	Coffee	19	3.8%
	Haba	2	0.4%
	Other	18	3.6%

In Table 1, I report the first response locals in each village gave when asked about their village's principal activity and principal crop. It is clear that these village economies were mostly agricultural, and that the main crop in these villages was corn. The primary corn harvest in Mexico lasts from October through December (USDA). Thus, I interpret my results as information about production technology and labor demand during the corn harvest. It is of course possible that production technology and labor demand are different for corn planting or for the planting or harvesting of other crops in other regions.

Table 2 shows some summary statistics across both treatment and control villages for the three years in my sample. In the data sets from all three surveys there is information regarding whether individuals were eligible for the program, whether they were working for a salary, what their job title was, measures of their income, and measures of the amount of time they worked.

Table 3 shows the distribution of adults and children across the job categories listed in the main job category variable (one that is available each year). Workers in two job types consistently report salary information: *jornaleros* (field workers), and *obreros* (non-agricultural workers) – those in other categories typically do not report earning a salary. This paper analyzes the jornalero workforce, which has nearly three times as many observations as the obrero workforce (see Table 3) and – given the corn-heavy nature of agriculture in this sample – is presumably more homogenous than the obrero workforce (which seems to potentially include *all* regularly paid non-agricultural jobs).[5]

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[5] It would be nice, following Katz and Murphy (1992), to estimate demand changes for multiple industries. However, here paid work occurs only in two industries, and only one of these has both enough children and enough homogeneity to admit useful analysis.

Table 2: Some Summary Statistics by Treatment Village Status and Year

Year	Variable	Control Villages	Treatment Villages
1997	Total # families	9,221 families	14,856 families
	Total # people	48,475 people	77,199 people
	% male	*50.0%*	*50.7%*
	% child (< 17 years)	46.8%	47.3%
	% adult (17 to 59 years)	45.3%	44.8%
	% worked last week	*40.0%*	*41.9%*
	% worked as jornalero	15.6%	15.2%
	Mean jornalero wage	3.36 pesos / hour	3.38 pesos / hour
1998	Total # families	9,919 families	15,927 families
	Total # people	52,299 people	85,141 people
	% male	50.0%	50.6%
	% child (< 17 years)	*47.5%*	*48.1%*
	% adult (17 to 59 years)	*44.7%*	*44.1%*
	% worked last week	35.7%	36.2%
	% worked as jornalero	21.4%	21.8%
	Mean jornalero wage	4.39 pesos / hour	4.37 pesos / hour
1999	Total # families	10,498 families	16,474 families
	Total # people	55,793 people	83,631 people
	% male	*49.6%*	*50.3%*
	% child (< 17 years)	45.9%	46.3%
	% adult (17 to 59 years)	46.0%	45.5%
	% worked last week	35.6%	36.0%
	% worked as jornalero	22.7%	22.5%
	Mean jornalero wage	5.1 pesos / hour	5.65 pesos / hour

Entries are italicized if they are significantly different between control and treatment at the 5% level.

Table 3: Pre-treatment Distribution of Adults and Children across job categories

Year	% with Job Title:	Adults		Children	
1997	Jornalero (field worker)	15,675	(50%)	1,701	(38%)
	Obrero (non-field worker)	5,320	(17%)	642	(15%)
	Self-employed	4,472	(14%)	317	(7%)
	Pattern Work	150	(0%)	9	(0%)
	Family Work, No Pay	3,428	(11%)	1,654	(37%)
	Other Work, No Pay	119	(0%)	50	(1%)
	Member of Cooperative	28	(0%)	3	(0%)
	Communal Farmer	2,245	(7%)	21	(0%)
	Other	229	(1%)	25	(1%)
	Total	31,666	(100%)	4,422	(100%)

Figure 4: Age Frequency of Jornaleros earning a salary
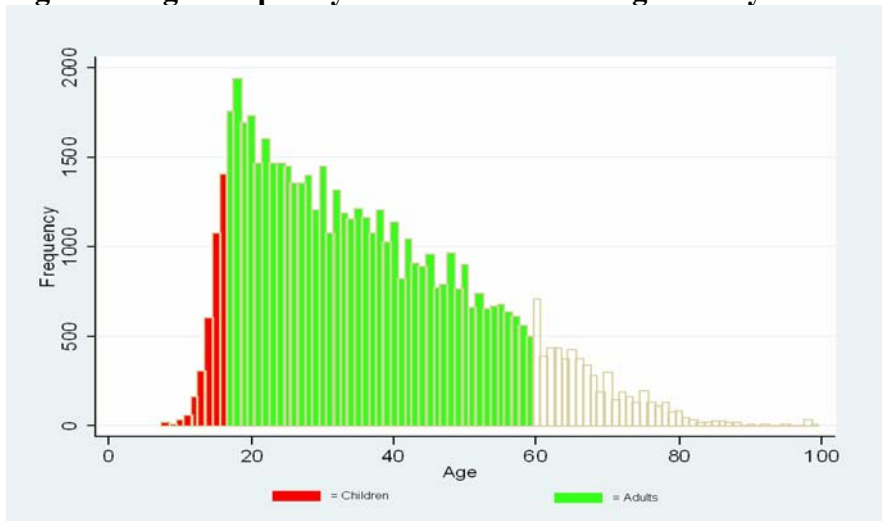

Figure 4 shows the age frequency histogram of jornaleros earning a salary. The first shaded area shows the jornaleros I classify as children (ages 16 and under), and the second larger shaded area shows the jornaleros I classify as adults (ages 17 to 59). In 1997, children made up 8.78 percent of the total jornalero workforce, while adults made up an additional 80.22 percent. I have tried to measure the sensitivity of my results to changes in the definitions of these age groups, and I have found the results to be robust.

Everyone who reports income reports it in *one* of the following measures: pesos per day, pesos per week, pesos per two weeks, pesos per month, or pesos per year. The measures of the amount of time worked are hours per day and days per week, and most people who report income report the amount of time they worked using both of these measures. About 90 percent of the income observations are in pesos per day or pesos per week. For people who report daily salaries, I impute hourly wages by dividing the daily salary by the number of hours worked per day. For people who report weekly earnings, I impute hourly wages by dividing earnings by the number of days worked per week multiplied by the number of hours worked per day. For the remaining 10 percent of

income observations, I assume that bi-weekly reporters work both weeks, that monthly reporters work four weeks per month, and that yearly reporters work fifty weeks per year.

The resulting hourly wages range from .0002857 pesos per hour to 7506.25 pesos per hour. With bounds these extreme, it is likely that the very high and very low hourly wages suffer from measurement error. Mean regressions of wages are thus likely to be biased by the incorrect measurements at the top of the distribution, and mean regressions of log wages may be biased by the incorrect measurements at the bottom of the distribution. Thus, in later sections I will perform two tests that do not depend only on means in order to establish the existence and direction of any treatment effect on the distribution of wages: a kolmogorov smirnov test of first-order stochastic dominance; and estimation of quantile regressions by decile. But, once the existence and direction of the treatment effect have been established by the above tests, in order to get one number for the size of the treatment effect, I do run mean regressions as well, attempting to eliminate the bias caused by the incorrect measurements at the top and the bottom of the distribution by dropping observations with wages in the top and bottom five percent for each of the six comparison groups (control vs treatment, 1997 vs. 1998 vs. 1999).[6]

In asking about workers' hours, the surveys asked workers how many hours a day they tended to work last week, or simply how many hours a day they worked. Thus, if workers worked a different number of hours each day, the estimate of the hours per week will be noisy unless the workers correctly averaged their hours when responding to this question. Because of this, in the analysis below I replicate all mean regressions of hourly variables using daily variables – i.e., with daily income instead of hourly wages, and days

[6] This cropping is carried out relative to the sample used in each regression (usually, this is all adult jornaleros, but sometimes it is a subsample designed, e.g., to determine the impact of living in a treatment village without directly receiving treatment money, as in Section VI(C)).

worked per week instead of hours per week. This also helps ensure that measurement error in hours is not driving the results.

V. Did the Experiment Reduce Child Labor Participation?

In the first few months of the program, as measured by the 1998 survey, it is unclear whether the experiment has yet reduced child participation in the jornalero workforce. But by 1999, 18 months after the program started, the treatment has clearly caused child participation in the jornalero workforce to decline. These results are demonstrated in the difference-in-difference estimates of the treatment effect described below.

(A) Empirical Strategy

My usual empirical strategy in this section and the next is to estimate reduced form equations of the treatment effects on labor market outcomes such as the work participation rate, hourly wages, etc. My unit of observation is an individual at a point in time. As Table 2 showed, some characteristics of treatment villages and control villages differed in small but significant ways before the treatment even started, so it is important to use a difference-in-differences approach.[7] This entails a treatment village dummy variable, a post-treatment dummy and an interaction dummy – with the interaction coefficient being the difference-in-difference estimate of the treatment effect. In addition to differencing out the pre-program differences between the control and experimental group, I also control for the effect of composition differences between the two groups by

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[7] Furthermore, in a key PROGRESA paper, Schultz argues, "even if the randomization of program placement is not challenged, . . ., the difference in difference estimators are preferred to the post-program differences, because they remove persistent sources of regional variation. . . that might exist" (Schultz 2004).

including controls for important personal characteristics.[8] Finally, to ensure that I control

for village-specific components of the variance of the error term, I include clustering at

the village level in most specifications. Thus, in summary, the difference-in-difference

equations are of the following pattern:

$$Y_{i,t} = a \cdot Treated_{i,t} + b \cdot TreatmentVillage_{i,t} + c \cdot Post_{i,t} + d \cdot PersonalChars_{i,t} + \varepsilon_{i,t}$$

where *i* indexes people and *t* indexes time.

The *Treated* dummy variable is 1 when the observation is from a treatment village and is

also from a post-treatment survey. The *Treatment Village* dummy is 1 whenever the

observation is from a treatment village. The *Post* dummy is 1 whenever the observation

is from a post-treatment survey. I include in *personal characteristics* dummies for

gender, age, schooling, language abilities and marriage status. I run this specification

separately for the 1997 vs. 1998 comparison and the 1997 vs. 1999 comparison.

(B) The Decline in Child Jornalero Work Participation

First, I add to the previous studies of this experiment ((Schultz 2004) [9] and

(Skoufias & Parker 2001)[10]) that have estimated significant decreases in work

participation for children, by estimating specifically the treatment effect on child

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[8] Schultz (2004) explains the logic of this: "It may still be useful to add additional explicit control variables and estimate their marginal effects jointly with those of the program on the enrollment of poor children, because this should increase the statistical power of the model estimated at the level of the individual child to isolate significant effects attributable to the program treatment, if there are any." This is also a justification for making the unit of observation as small as possible in my specifications (usually it is at the level of the individual).

[9] Based on differences between means, Schultz (2004) concludes: "All of the differences in child work between treatment and control populations are negative, as expected, and they are statistically significant at least at the 10% level for the probability of paid work for primary school females and males and for secondary school males, for household and market work for secondary school females, for paid work for secondary school males, for the OLS hours for primary school boys, and for the Tobit hours for primary school females and males and secondary school males" (I deleted references to Schultz's tables in this sentence). He goes on to use more sophisticated IV estimates to further conclude that the program had statistically significant negative effects on child work.

[10] Based on a difference in differences estimate, (Skoufias and Parker 2001) conclude: "The results. . . show that PROGRESA has had a clear negative impact on children's work."

participation in the jornalero workforce. I create a dependent variable dummy for working as a jornalero by assigning the dummy the value 1 if the person worked as a jornalero in the last week and 0 if they did not work or worked in a different job category. I regress the dummy for working as a jornalero on my independent variables as outlined in Equation 1. The OLS results are reported in Table 4a and summarized in Table 4b. I find that by 1998, there was no significant effect on child jornalero field work participation. However, by 1999, child jornalero work participation saw a large and significant decrease due to the treatment. This corresponds with Skoufias and Parker's result that 12 to 17-year-old males (51 percent of whom are jornaleros if they work at all, and who make up 87 percent of the child jornalero workforce) only saw a significant decrease in child work participation by 1999. I also run probit specifications of the same difference-in-difference equations, and report the results in Table 4c and Table 4d. According to these results, there is a significant decrease in child work participation by 1998 that grows through 1999, but only the 1999 increase is robust to clustering at the village level.

Thus, while the initial 1998 treatment effects on child labor participation are inconclusive, it is clear that by 1999 child labor participation in the jornalero workforce has significantly decreased.

In the next section, I estimate the treatment effects on the quantity and price of adult labor. I then look for additional evidence to determine whether the decline in child work participation in the fields that I observed in this section was responsible for the change in the demand for adult labor that I observe in the next section.

Table 4a: OLS Treatment effects on Child Jornalero Work Participation

Dependent Variable: work participation in jornalero work force,
for children aged less than 17 years old (Baseline year: 1997).

Explanatory Variables	(1) Post-Treatment: 1998	(2) Post-Treatment: 1999
Treated (post = 1 & treatment village = 1)	-0.0053 (0.0033)	-0.0071** (0.0032)
Post-treatment Dummy	-0.0101*** (0.0026)	-0.0129*** (0.0025)
Male Dummy	00727*** (0.0016)	0.0700*** (0.0016)
Age Dummies	YES	YES
Village Fixed Effects	YES	YES
Constant	0.1430*** (0.0028)	0.0991*** (0.0027)
# Observations	63488	62075
R2	0.1009	0.1025

Standard Errors are in Parenthesis, * = significant at 10%, ** = at 5% *** = at 1%

Table 4b Summary of OLS Treatment effects on Child Jornalero work:

Percentage change in the probability of a Child reporting Jornalero work from 1997 to . .

1998	1999
-9.81% (0.103)	-13.05% (0.028)

P-values for t-tests of significant difference from 0 are given in parenthesis.
Percentage changes are calculated by dividing the coefficient on the treated dummy from table 4a by the pre-treatment mean value of the independent variable to obtain the treatment effect.

Table 4c: Probit Treatment Effects on Child Jornalero Work Participation

Dependent Variable: work participation in jornalero work force,
for children aged less than 17 years old (Baseline year: 1997).

Explanatory Variables	(1) Post-Treatment: 1998	(2) Post-Treatment: 1999
Treated (post = 1 & treatment village = 1)	-0.0019** (0.0009)	-0.0021*** (0.0006)
Post-treatment Dummy	-0.0026*** (0.0007)	-0.0031*** (0.0006)
Male Dummy	0.0297*** (0.0013)	0.0243*** (0.0000)
Age Dummies	YES	YES
Village Fixed Effects	YES	YES
# Observations	61128	58852
Pseudo R2	0.33	0.36

Coefficients reported are the marginal effects
Standard Errors are in Parenthesis, * = significant at 10%, ** = at 5% *** = at 1%

Table 4d Summary of Probit Treatment effects on Child Jornalero work:

Percentage change in the probability of a Child reporting Jornalero work from 1997 to . .

1998	1999
-3.52%	-3.86%
(0.036)	(0.002)
[0.146]	[0.045]

P-values for t-tests of significant difference from 0 are given in parenthesis, while those calculated from standard errors clustered at the village level are in brackets. Percentage changes are calculated by dividing the coefficient on the treated dummy from Table 4c by the pre-treatment mean value of the independent variable to obtain the treatment effect.

VI. Did the Reduction in Child Labor Cause an Increase in the Demand for Adult Labor?

The results in the previous section showed that there was a decrease in child work participation in the jornalero workforce by 1999. Thus I need to check whether the demand for adult labor increased by 1999.[11] According to Proposition 1, if a treatment has increased the price of adult jornalero labor without decreasing its quantity, then this is sufficient to show that it increased the demand for the labor of adult jornaleros. Thus, I check whether by 1999 there was an increase in the price of adult jornalero labor without an accompanying decrease in the quantity. First, I consider the treatment effect on the price of adult labor, and secondly the treatment effect on the quantity of adult labor.

(A) Treatment effects on the price of adult labor:

I estimate treatment effects on adult hourly wages and daily income. As explained in Section IV, I establish the existence and direction of these treatment effects from kolmogorov smirnov tests on the distribution of wages, and from quantile regressions by decile. I then estimate a single number for the size of the treatment effect by following the empirical strategy outlined in the previous section, estimating OLS hourly wage and daily income specifications. These results show that by 1999, there are positive and significant treatment effects on both adult jornalero hourly wages, and daily income.

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[11] The fact that there was no robust decrease in child labor participation by 1998 suggests another test: if PROGRESA did not directly impact adult labor demand (i.e., without the mechanism of changing child labor supply), then there should have been no robust increase in adult labor demand by 1998. This is what I find. In regressions similar to those reported in this section, I find that by 1998 there may have been an increase in adult labor demand, but that not all specifications show such an increase. This corresponds well with the lack of robustness in the decrease in child labor participation by 1998 that I reported above.

The kolmogorov smirnov test on the pre-treatment distribution functions shows that the pre-treatment distribution of wages in treatment villages is first-order stochastically dominated by that in the control villages. The p-value for the null hypothesis that the two distributions are identical – when the alternative hypothesis is that the treatment distribution is stochastically dominated by the control distribution – is 0.02, and is thus rejected. The p-value for the null hypothesis that the two distributions are identical – when the alternative hypothesis is that the control distribution is stochastically dominated by the treatment distribution – is 0.20, and cannot be rejected.

But the kolmogorov smirnov tests clearly show that the post-treatment distribution of wages in the treatment villages first-order stochastically dominates that in the control villages. The p-value for the null hypothesis that the two distributions are identical – when the alternative hypothesis is that the control distribution is stochastically dominated by the treatment distribution – is 0.00, and is thus rejected. The p-value for the null hypothesis that the two distributions are identical – when the alternative hypothesis is that the treatment distribution is stochastically dominated by the control distribution – is 0.38, and cannot be rejected.

This shift can be seen visually in Figure 5, which plots the cumulative distribution functions of the hourly wages of adult jornaleros in 1997 and in 1999. The wage distribution is too lumpy for all deciles to increase, but the quantile regressions by decile reported in Table 5 show that four deciles increased significantly (two below the median and two above) and none decreased significantly.

**Table 5. Quantile Difference-in-Difference Treatment effects on Hourly Wages, no controls or cropping**

	1997 vs. 1999
10th Percentile	0.000
	(0.017)
20th Percentile	**0.131**
	(0.023)
30th Percentile	**0.179**
	(0.008)
40th Percentile	0.000
	(0.050)
50th Percentile	-0.083
	(0.076)
60th Percentile	**0.069**
	(0.032)
70th Percentile	0.000
	(0.056)
80th Percentile	**0.625**
	(0.061)
90th Percentile	-0.020
	(0.270)

Standard Errors are in parenthesis. Results significant at the 5% level are bolded

Figure 5: Cdfs of Hourly Wages, Control vs. Treatment, 1997 & 1999
1997:
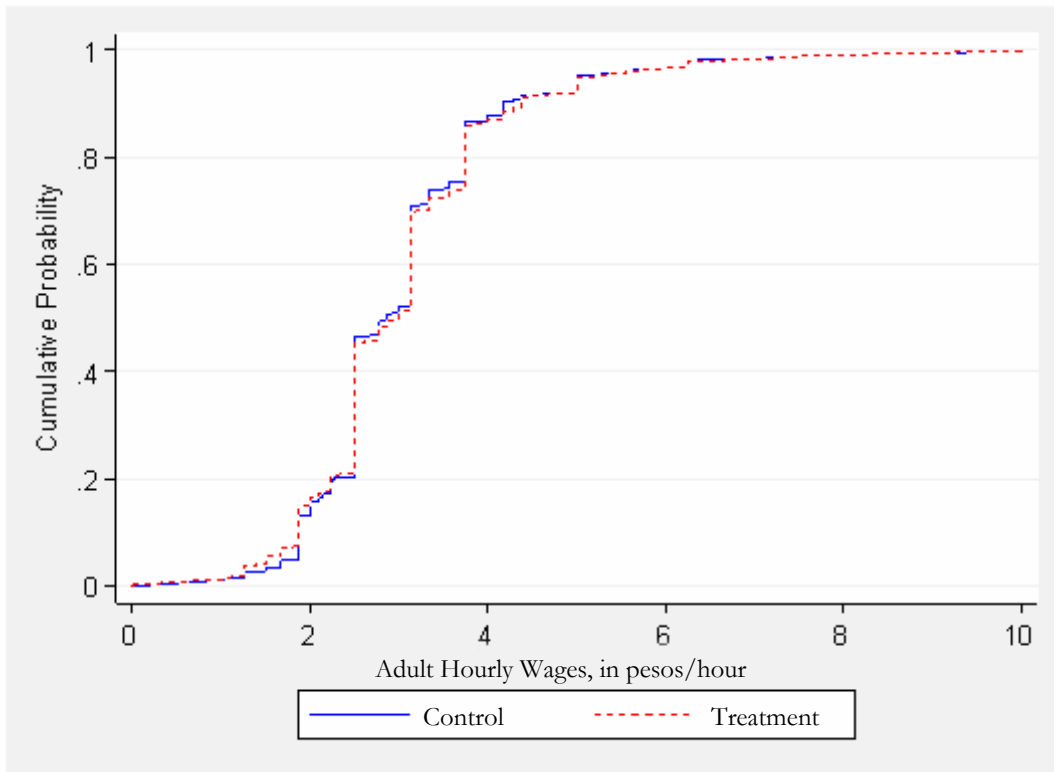
1999:
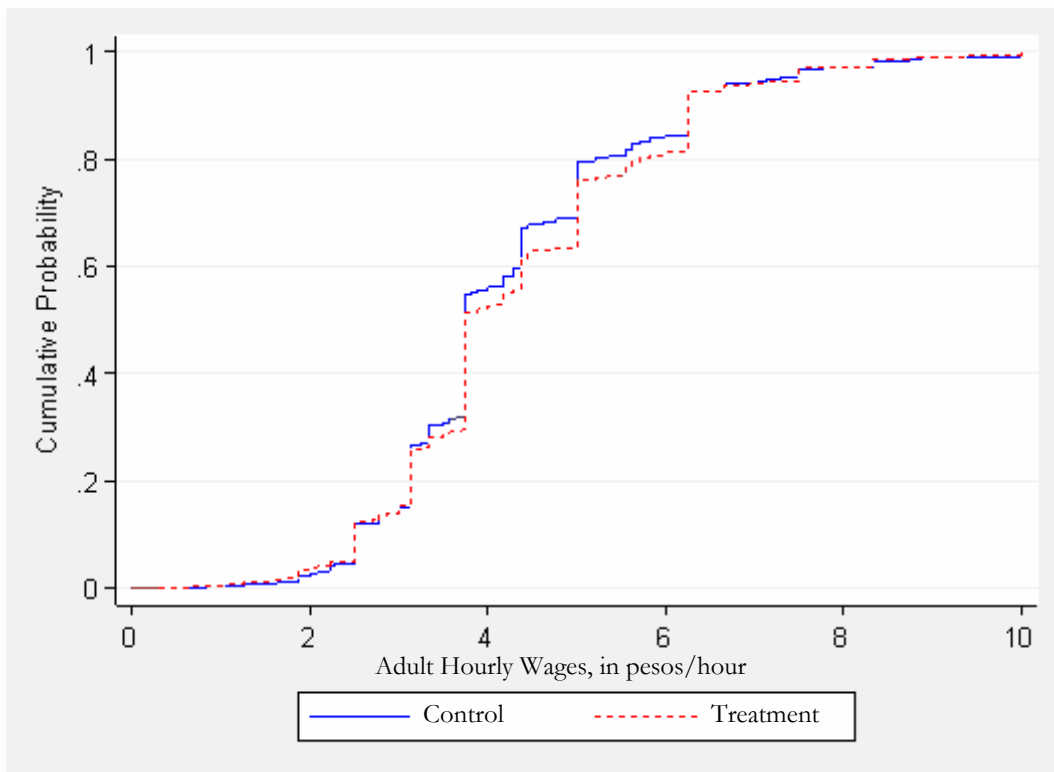
Thus, it is clear that by 1999 the hourly wages of adult jornaleros have increased due to the treatment. Furthermore, the adult wage increase appears to be real, not only nominal: the 2000 study by Handa, Huerta, Perez & Straffon concludes that the treatment did not produce food price inflation in the treated villages.

What number summarizes the size of this increase? I consider the treatment's effect on mean wages, by estimating OLS regressions on log hourly wages and log daily income according to the difference-in-differences strategy discussed in the previous section, with the effect of the tails diminished via the cropping discussed in Section IV, reporting the results in Table 6. The results suggest an increase in adult jornalero wages of over 6%.

Table 6: Treatment effects on log hourly wages and log daily income from 1997 to 1999

Dependent Variables:
Log Hourly Wages or Log Daily Income for Adult (ages 17 to 59) Jornaleros

Explanatory Variables	(1) Log Hourly Wage	(2) Log Hourly Wage	(3) Log Daily Income	(4) Log Daily Income
Treated (post = 1 & treatment village = 1)	0.065*** (0.006)	0.065*** (0.02)	0.061*** (0.006)	0.061*** (0.016)
Treatment Village Indicator	-0.039*** (0.005)	-0.039** (0.018)	-0.029*** (0.005)	-0.029 (0.018)
Post-treatment Indicator	0.332*** (0.005)	0.332*** (0.011)	0.314*** (0.005)	0.314*** (0.010)
Male Indicator	0.010 (0.07)	0.010 (0.011)	0.037*** (0.007)	0.037*** (0.012)
Age, Schooling Level, Langauage Skills, and Marriage Status Indicators	YES	YES	YES	YES
Village Clusters		YES		YES
Constant	1.12*** (0.014)	1.12*** (0.023)	3.17*** (0.014)	3.17*** (0.023)
# Observations	24605	24605	24605	24605
R2	0.42	0.42	0.41	0.41

Standard Errors are in Parenthesis, * = significant at 10%, ** = at 5% *** = at 1%

(B) Treatment Effects on the Quantity of Adult Labor:

Having thus established that, by 1999, the treatment increased the price of adult jornalero labor, I turn now to the quantity of labor hired. I estimate treatment effects on mean work outcomes for adult jornaleros between 1997 and 1999. From Table 7, it is clear that the treatment increased both adult hours worked per week and adult days worked per week conditional on working.

Table 7: Treatment effects on log hours worked and days worked per week from 1997 to 1999

Dependent Variables:
Log Hours Worked and Log Days Worked per Week for Adult (ages 17 to 59) Jornaleros

Explanatory Variables	(1) Log Hours per Week	(2) Log Hours per Week	(3) Log Days per Week	(4) Log Days per Week
Treated (post = 1 & treatment village = 1)	0.035*** (0.009)	0.035* (0.020)	0.039*** (0.009)	0.039** (0.018)
Treatment Village Indicator	-0.026*** (0.007)	-0.026 (0.016)	-0.036*** (0.006)	-0.036** (0.015)
Post-treatment Indicator	-0.077*** (0.007)	-0.077*** (0.015)	-0.060*** (0.007)	-0.059*** (0.013)
Male Indicator	0.102*** (0.011)	0.102*** (0.017)	0.075*** (0.010)	0.075*** (0.015)
Age, Schooling Level, Langauage Skills, and Marriage Status Indicators	YES	YES	YES	YES
Village Clusters		YES		YES
Constant	3.60*** (0.021)	3.60*** (0.027)	1.56*** (0.019)	1.56*** (0.024)
# Observations	24575	24575	24575	24575
R2	0.02	0.02	0.01	0.01

Standard Errors are in Parenthesis, * = significant at 10%, ** = at 5% *** = at 1%

Table 8 shows that it is likely – though not necessarily – true that the treatment increased the probability of adult participation in the jornalero workforce as well. I interpret these results to mean that the treatment increased the quantity of adult jornalero labor hired in treatment villages. At the least, these results suggest that it is very unlikely that the quantity of adult labor decreased due to the treatment.[12]

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[12] All of the specifications in Table 7 show significant increases in adult jornalero hours (conditional on jornalero work participation). Specification (1) of Table 8 shows a significant increase in jornalero work

Table 8: Treatment effects on other quantity of labor measures from 1997 to 1999

Explanatory Variables	(1) Probit: Worked as Jornalero	(2) Probit: Worked as Jornalero	(3) OLS: Hours per Week with 0's	(4) OLS: Hours per Week with 0's
Dependent Variables: Quantity of Labor Measures for adult (ages 17 to 59) Jornaleros				
Treated (post = 1 & treatment village = 1)	3.6%* [0.08]	4.1% [0.428]	0.635*** (0.212)	0.690 (0.533)
Treatment Village Indicator		-1.5% [0.78]		-0.501 (0.561)
Post-treatment Indicator	22.7%*** [0.01]	21.9*** [0.00]	1.07*** (0.165)	-1.10*** (4.00)
Male Indicator	203%*** [0.00]	200%*** [0.00]	22.8*** (0.106)	22.9*** (0.434)
Age, Schooling Level, Langauage Skills, and Marriage Status Indicators	YES	YES	YES	YES
Village Fixed Effects	YES		YES	
Village Clusters		YES		YES
Constant			1.07*** (0.319)	38.5*** (1.2)
# Observations	103402	103402	102517	102517
R2	0.39	0.34	0.31	0.32

Standard Errors are in Parenthesis, * = significant at 10%, ** = at 5% *** = at 1%
P-values for Probits are in brackets

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participation as well. But in specification (2) of Table 8, where village fixed effects are replaced with clustering at the village level, the increase in adult jornalero work participation is no longer significant, leaving open the statistical possibility that work participation decreased by a small amount (since the 95% confidence interval of the change in work participation overlaps 0). Thus, if heterscodasticity is being correctly adjusted by village level clustering, and if the true change in adult work participation is on the low end of this confidence interval, and if the large increase in adult hours reported in Table 7 came about *only* because people who would have worked low hours left the workforce, then it is possible that in fact the quantity of adult labor actually decreased due to the treatment. Given the number of conditions that seem to be necessary to conclude that the quantity of adult labor decreased, I believe it is likely that the quantity of adult labor did not decrease.

Thus, the treatment increased the price of adult jornalero labor without decreasing its quantity, so by Proposition 1, I conclude that the treatment increased the demand for adult labor. I do not conclude that the treatment had no effect on adult labor supply, but only that any such effects were outweighed by the increase in adult labor demand. For example, if the treatment reduced the labor supply of adults through an income effect then this reduction was clearly outweighed by the increase in demand for adult labor, because the quantity of adult labor probably increased.[13] Likewise, the increase in demand for adult labor must have outweighed any increases in adult labor supply, because adult wages increased.

Thus, by November 1999, comparison of treatment and control villages shows a significant decrease in the work participation of child jornaleros, accompanied by a significant increase in the price of adult jornalero labor and no significant decrease in the quantity of adult labor. If the only effect of the treatment on adult labor demand was through the decrease in child labor supply, then these results are sufficient to conclude that adults and children are *substitutes* in production: when children became more difficult to hire, employers increased wages for adults, thus increasing both the hours adults worked per week and weekly earnings.[14]

(C) Did the decrease in child labor supply cause the increase in adult labor demand?

I now give results which suggest that the only effect of the treatment on the demand for adult jornalero labor was through the decrease in child labor supply. There are three alternative pathways to consider. One is that the treatment families spent their

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[13] e.g., labor supply could decrease due to an income effect caused by receipt of treatment money.
[14] I replicate the difference and difference regressions for weekly earnings, finding a large and significant treatment effect on weekly earnings.

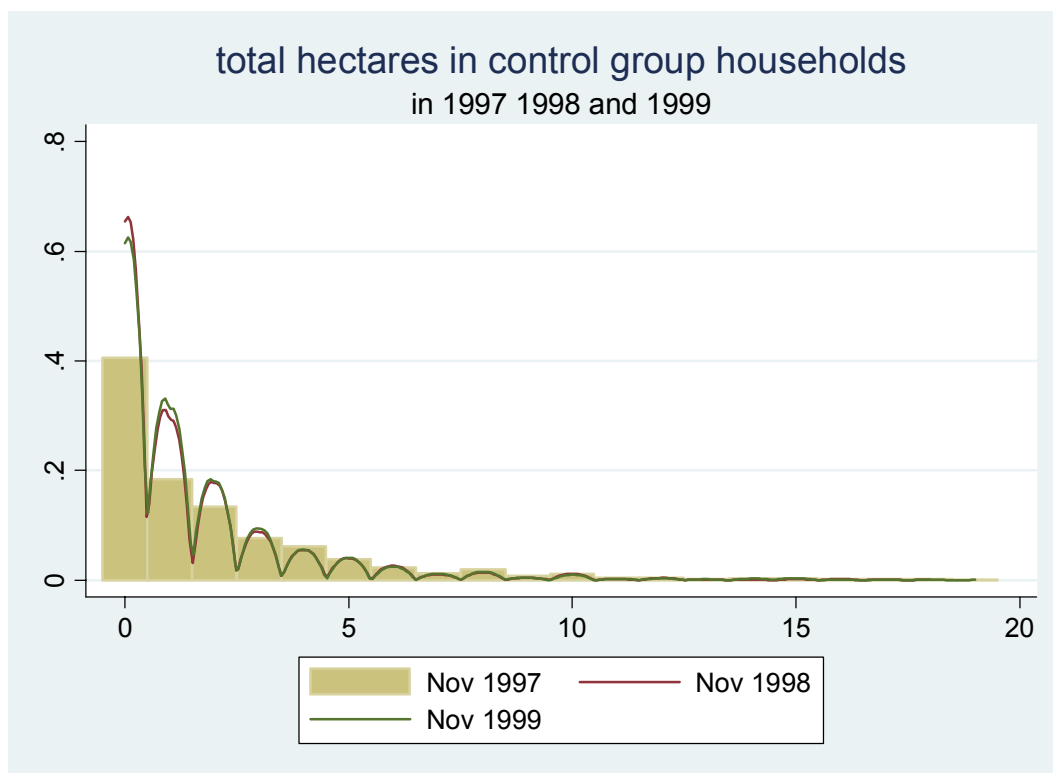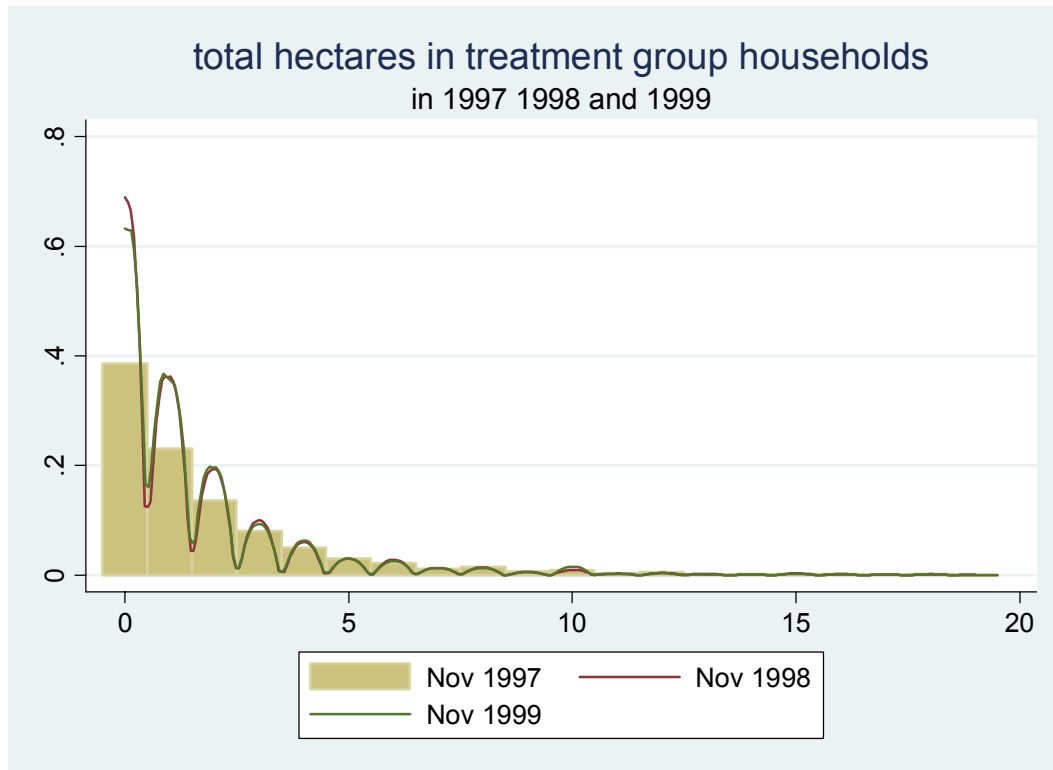**Table 9. Treatment Effects on Hectares used or owned, Total and Agricultural**

Independent Variables:	Dependent Variables		
	(1) Total Hectares in 1998 minus Total Hectares in 1997	(2) Total Hectares in 1999 minus Total Hectares in 1997	(3)Agricultural Hectares in 1998 minus Agricultural Hectares in 1997
Treatment Village Dummy	0.002 (0.069)	0.018 (0.065)	0.027 (0.057)
Constant	-0.521** (0.054)	-0.608** (0.051)	-0.413** (0.045)
Village Clustering	NO	NO	NO
# Obs	22265	20648	24077
R2	0.00	0.00	0.00

Standard Errors in Parenthesis, * = significant at 10%, ** = 5%, *** = 1%. The unit of observation is an individual household, with differences calculated from a panel data set of households. Village clustering is not performed because this shows that even under generous standard errors (i.e., those without clustering), there is no statistically significant treatment effect on hectares.

money in a way that would increase the demand for the jornaleros' labor. The second is that the wage increase had something to do with receiving treatment money (e.g. *direct benefits* in income, nutritional consumption, or medical consumption that lead to improved health, leading to better productivity and hence to better wages). The third is that the wage increase had something to do with receiving indirect treatment benefits (e.g. *spillovers* in income, nutritional consumption, or medical consumption that lead to improved health, leading to better productivity and hence to better wages).

The first alternative hypothesis depends on the possibility of treatment money encouraging farm production, causing more adults to be hired. But Table 9 and the graphs in Figure 6 show that there was no treatment effect on the number of hectares of land used or owned in the treatment villages.

Figure 6: Total Hectares used or owned by treatment group vs. control group households, from 1997 through 1999

total hectares in treatment group households
in 1997 1998 and 1999

Nov 1997   Nov 1998
Nov 1999

total hectares in control group households
in 1997 1998 and 1999

Nov 1997   Nov 1998
Nov 1999

Table 10. Treatment Effects on the Corn Harvest in 1998

Independent Variables:	Dependent Variables	
	(1) Indicator for a non-zero Harvest in 1998 (probit)	(2) Log # Tons Corn Harvested during the 1998 Harvest (OLS)
Treatment Village Dummy	-0.023 (0.028)	-0.051 (0.096)
Age	-0.001*** (0.000)	0.006*** (0.001)
Male	0.020 (0.019)	-0.224*** (0.067)
Constant		-0.30** (.12)
Village Clustering	YES	YES
# Obs	11461	7007
R2	0.002	0.005

Standard Errors are in Parenthesis, * = significant at 10%** = significant at 5%, *** = significant at 1%

Likewise, first differences of the number of tons of corn harvested in treatment households vs. control households in 1998 show that the treatment did not change the size of the harvest (Table 10). These results suggest that the treatment did not increase the total amount of field work that employers needed to be done by jornaleros.

Eligible families in the treatment villages did buy more of some types of animals, but it is not clear whether the addition of these animals required more labor or less (since horses, e.g., could substitute for field work) (Angelucci & De Girogi, 2005). Finally, I reported above that there is no evidence of food price inflation due to the treatment.[15]

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[15] Given the agriculture products listed in Table 2, the prices that matter in determining whether the demand for local agricultural goods has increased are (mostly) the price of corn, and (secondarily) the price of beans and coffee. Unfortunately, not every locality reports prices, and (Handa Huerta Perez and Straffon 2000) do not have information on corn (only on corn paste and corn tortillas). Their work shows that the

Thus there is no consistent evidence that the treatment money was spent in a way that would increase the demand for jornaleros' labor. This is not surprising, since it seems likely that the markets for basic foodstuffs such as corn are considerably larger in geographic scale (perhaps even international) than those for short-term labor assistance during the corn harvest.[16]

The second alternative hypothesis is that something associated with receiving treatment money may have caused the wage increase in treatment villages. An example of a pathway from receiving treatment money to increased price and quantity of labor would be increased nutrition, leading to improved health. Some of the families in both treatment and control villages were not eligible to receive treatment because their wealth was too high, and some did not receive treatment money because of administrative errors. So I can use these families to see if these wage increases only occur for families that receive treatment money, or if other families living in treatment villages also experienced wage increases. If the only families living in treatment villages that experienced wage increases were those that received treatment money, then that would suggest that

price of beans appears to have increased by similar amounts in both treatment and control villages; that the price of coffee may have decreased in treatment villages and stayed constant in control; that the price of corn paste appears to have increased by similar amounts in both treatment and control villages; and that the price of corn tortillas may have increased by about the same amount in both treatment and control villages, though only the treatment increase was significant. My own regressions show no significant difference between treatment and control prices for corn flour, corn paste, or corn tortillas in the 1999 post-treatment survey used in this paper. This overall evidence is difficult to reconcile with any large positive treatment effect in the price of the crops most local farmers produce. This is not surprising, considering that the above authors believe that government-run Diconsa stores (which are equally distributed across villages) are likely to "maintain a relatively constant supply of basic items at a fixed price," and hypothesize that this should have a stabilizing effect on prices. Furthermore, the authors report that people in outlying communities travel to the municipal centers to receive their benefit checks, and spend money there; thus, people do not always buy goods in the village that they live in.

[16] At the least, the fact that it is more difficult to move people than it is to move corn suggests that distant labor markets would take longer to respond to local wage variation than distant goods markets would for local price variation. Thus, in the short-run, the relevant geographic scale for a labor market should be smaller than that for a corn commodity market, although in the long run international migration shows that labor markets seek to be global as well. This wage increase lasted one season before the experiment was extended to the control group, so one should consider the possible short-term responses, not long term ones.

something associated with receiving treatment money may have caused the wage increase in treatment villages (although it would not necessarily imply this, since the families that received treatment money tended to be poorer).

Thus, in order to rule out that such a pathway is the sole cause of the wage increase, I estimate the same wage regression on a smaller restricted sample. I restrict my sample to all people in the experimental group who were not eligible to receive money in 1997 and did not receive any money by 1999 (this includes people who did not receive money because of administrative error) and a similar sample from the control group (see the Appendix for a description of how these samples were constructed). On this restricted sample, by 1999 there is a 2.2% wage increase due to the treatment, which is significant at the five percent level. Likewise, there is a 2.0% increase in daily income due to the treatment, a 3.6% increase in hours worked per week and a 3.5% increase in days worked per week. The results are reported in Tables A1b and A1c. That the treatment increases the wages on this restricted sample suggests that the results are not dependent on receiving treatment money (e.g. a causal pathway from treatment money to increased nutrition to increased productivity is not responsible for all of the wage increases).

Finally, the above robustness check must itself face a robustness check in the form of the third hypothesis: might treatment spillovers have been responsible for the increase in wages seen in the sample of non-treated adults who were living in treatment villages? To rule out the pathway of treatment *spillovers* leading to health leading to better productivity and wages, I restrict the above sample again by considering in any

year only those non-treated adults who report perfect health according to ten criteria.[17]

On this restricted sample, I find that the wages paid to healthy adults are again about 2%

higher due to the treatment. This suggests health improvements were not necessary for

workers to experience the wage increase; the only necessity was to live in a village where

child labor decreased.

(D) Other Robustness Issues

One potential cause for concern is a connection between the labor market for field

workers and other labor markets. For a variety of reasons, PROGRESA may have caused

non-farm employers to increase their demand for labor as well, and at first glance this

seems problematic for my identification. However, if PROGRESA increased the demand

for labor in other industries, then this would not affect the demand for labor in the fields;

it would affect the supply of labor to the fields. And all stories that involve changes in

the supply of adult labor to the fields are irrelevant to this identification strategy: as I

explained in Section III, simultaneous changes in wages and quantities can identify the

direction of changes in demand regardless of the existence of changes in supply.

Another potential cause for concern is the implicit assumption that labor markets

are local. This is a strong assumption, because it would imply that people in control

villages do not supply labor in treatment villages, even though random assignment of

villages may have placed some villages close together. In reality, however, this is not a

problem. If treatment and control labor markets sometimes overlap, then this would

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[17] The ten criteria are: days of difficulty performing activities due to bad health in the past month are 0;
days of missed activities due to bad health in the past month are 0; days in bed due to bad health in the past
month are 0; yes, I can currently perform vigorous activities; yes, I can currently perform moderate
activities; yes, I can carry an object of 10kg 500meters with ease; yes, I can easily lift a paper of the floor;
yes, I can walk 2 km with ease; yes, I can dress myself with ease; I have had no physical pain in the last
month.

attenuate the program's effects on both the supply of child labor and the demand for adult labor. Thus, when I find significant program effects on the supply of child labor and the demand for adult labor, I have enough information to conclude that the program's impacts without overlapping labor markets would have been significant as well.

I therefore conclude that by 1999, a reduction in child jornalero field work participation in the treatment villages had a positive and significant hourly wage effect on adult field workers, which in turn increased adult hours worked per week (conditional on working).[18] This result occurs without food price inflation, increased land holdings in treatment villages, or a significant change in the size of the corn harvest, and it is not consistent with shifts in adult labor supply alone. The result does not disappear when I restrict to a much smaller sample that did not receive treatment money, or a subsample of that which includes only the wages of healthy adults. Thus, by Proposition 1, in this region and time period, employers appear to substitute adults for children, not to treat them as complements: when child labor supply decreases, the demand for adult labor increases. In the next section, I explore how this exogenous increase in adult jornaleros' wages affected the jornaleros' families.

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[18] The relative magnitudes of these percentage changes depend on the specification. Since the overall decrease in the jornalero workforce by 1999 (caused by the decrease in child work participation) was likely smaller than the increase in adult wages by that point (compare Table 4d with Table 6), the natural question is: what else was changing adult wages? The most obvious answer is a backward shift in adult labor supply, caused by the large increases in household non-wage income instituted by PROGRESA. I reiterate that such simultaneous changes in supply are irrelevant for my identification strategy (see Section III).

VII. How did the Wage Increase Affect the Families of Adult Jornaleros?

A significant increase in adult jornalero wages should have positively impacted the health and consumption of families with adult jornaleros. Almost half of the families living in treatment villages that did not receive treatment had an adult jornalero in their household. Since the wages of these adult jornaleros increased, any health and nutrition spillovers to non-treated households in treatment villages should be larger for the subset that included adult jornaleros. Indeed, I find in Table 11 that consumption spillovers in grains and cereals, and meats and dairy occur only in families that had an adult jornalero. Families that did not have an adult jornalero apparently experienced no significant consumption spillovers in this sample. I also find in Table 12 that families with an adult jornalero saw increases in health, although it is not clear if these health spillovers differ from those experienced by families without an adult jornalero. Comparison of the consumption results at least suggests that the increase in the demand for adult jornalero labor had positive consequences for the welfare of their families.

**Table 11. Effect of increased wages for Adult Jornaleros on their Families'
Consumption in 1999**

Indep. Variables	For Families *with* an Adult Jornalero			For Families *without* an Adult Jornalero		
	(1) Log kg of fruits & vegetables	(2) Log kg of grains & cereals	(3) Log kg of meats and dairy	(1) Log kg of fruits & vegetables	(2) Log kg of grains & cereals	(3) Log kg of meats and dairy
Treatment Village Dummy	0.059 (0.042)	0.050* (0.027)	0.084** (0.036)	0.014 (0.040)	0.038 (0.026)	0.017 (0.038)
# of household members	0.078*** (0.006)	0.113*** (0.005)	0.071*** (0.005)	0.104*** (0.007)	0.153*** (0.006)	0.083*** (0.007)
Gender, Age, Schooling, Langauage, Marriage Status	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES
Constant	1.1*** (0.15)	2.2*** (0.10)	.08 (0.14)	.92*** (0.18)	1.9*** (0.09)	-.01 (0.13)
# Obs	3747	3766	3482	3839	3874	3574
R2	0.11	0.27	0.12	0.20	0.37	0.15

Standard Errors are in Parenthesis, * = significant at 10%, ** = at 5% *** = at 1%

Table 12. Probit Treatment Effects on Health Outcomes in 1999

Indep. Variables	For Families *with* an Adult Jornalero			For Families *without* an Adult Jornalero		
	(1) Difficulty Every Day	(2) Missed Activities Every Day	(3) In Bed Every Day	(1) Difficulty Every Day	(2) Missed Activities Every Day	(3) In Bed Every Day
Treatment Village Dummy	-24% (0.001)	-16% (0.073)	-9% (0.426)	-18% (0.019)	-19% (0.010)	-18% (0.027)
Gender, Age, Schooling, Langauage, Marriage Status	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES
R2	0.22	0.22	0.20	0.19	0.20	0.19
# Obs	13,468	13,449	13,447	11,115	11,114	11,114
Control Group Mean	14%	11%	0.8%	28%	25%	1.8%

P-values for t-tests of signficance of difference from zero in parenthesis

VIII. Interpretation of Results

What are the theoretical and practical implications of this result?   Any solution to the child labor phenomenon depends on the question of complementarity versus substitutability. If employers treat children and adults as complements, then governments must count the cost not only of lost child wages but of lower adult wages in the welfare programs they introduce in conjunction with mandatory schooling and strict child labor

laws. If, on the other hand, employers substitute adults for children, then this suggests

that child labor may be an instance of a poverty trap: adults must send their children to

work because adult wages are low, and adult wages are low because adults are competing

with their working children. This in turn suggests that a ban on child labor may move the

economy from an equilibrium with low household welfare (where children must work) to

one with higher household welfare (where adults' wages are high enough to support their

families and children can afford to leave the workplace for the classroom).

This intuition is formalized in the work of Kaushik Basu and Pham Van (Basu &

Van 1998) (Basu 2000).[19] They show that sufficient conditions for the possibility of two

equilibria (one with child work and one without it) are the so-called *Luxury Axiom* and

Substitution Axiom, with the latter axiom being that employers substitute adults for

children.[20] The work of Eric Edmonds (Edmonds 2003) shows that in the agricultural

setting of Vietnam the Luxury Axiom seems to hold. My results suggest that in this

agricultural area of Mexico the Substitution Axiom seems to hold. Furthermore, the fact

that these results are both from agricultural settings is useful for generalizability. As

Udry (2004) points out: "Child labor is overwhelmingly a rural and agricultural

phenomenon. For example, in Pakistan, 70% of working children are employed in

agriculture." Thus, together with (Basu and Van 1998) and (Edmonds 2003), my results

suggest the possibility – in the types of labor markets that most children work in

throughout the world – of a poverty trap that can be escaped through stricter child labor

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[19] In the presence of multiple equilibria in Basu and Van's model, a minimum wage w' will eliminate child labor if the child market wage < w' < adult market wage, and if child productivity is low enough such that there exists excess demand when only children are working.

[20] The Luxury Axiom is that parents only send their children to work to keep household income above a subsistence level.

laws and better schools, without the additional injection of large household welfare payments.

These results also provide the material for a back-of-the-envelope calculation of the effects of a ban on child labor on household budgets.[21]   In this data set, the average family has 2.5 children under age 17, of which 0.13 are working as jornaleros. If the government implemented a ban on child labor in the jornalero workforce, the lost earnings per week of the children in such a family would be approximately 0.13 * (125 pesos per week) = 16.6 pesos per week. But of the 2.4 adults in an average household, 0.68 tend to work as jornaleros, and (based on the coefficient in Table 4b, and the bottom of the confidence interval for the coefficient in Table 6) it seems that these workers might experience an hourly wage increase of 100% * 4.7% /13 % = 36%. Assuming as a lower bound that there was no increase in adult hours worked per week, this would lead to an increase in weekly earnings of 36% * (131 pesos per week) = 47 pesos. This would then entail an increase in adult earnings per week in an average family of (0.68 adult jornaleros per household) * (47 pesos per adult jornalero) = 32 pesos. Thus, 100% of the child earnings lost by the ban would be recovered by the improved adult wages. It is unlikely that the adult wage increase that I observed associated with a 4 % to 13% drop in child labor participation is representative of the new equilibrium wage that would occur in the event of a 100% drop in child labor supply. But this calculation gives some indication that the order of magnitude of the observed wage increase is large enough to potentially counteract much of the welfare loss for poor families due to a ban on paid child labor in the fields.

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[21] Without a precise theoretical framework, any estimates of the size and consequence of these effects will be conjectural at best.

The distributional consequences of the substitution of adults for children depend on how child workers and adult workers are distributed across families and across industries. In families where adults work in industry A and children work in industry B, a ban on child work in industry B will not necessarily lead to higher wages in industry A, and thus the welfare consequences for that family are likely to be negative. Likewise, in a family where children do not work, and adults work in industry B, a ban on child work in industry B will lead to an increase in the adult wage in industry B, improving welfare unambiguously for that family. Thus, even when adults substitute for children in every industry, in order for labor market outcomes of adults to mitigate the welfare losses across all families due to a ban on child labor, it must be the case that either (1) the ban on child labor is successfully implemented across all industries, and/or (2) there is a perfect correlation between the industry of employment of adults and that of children within a family. In the PROGRESA data, there are many households with jornalero adults that are without jornalero children, as well as many jornalero children living in households without jornalero adults, which suggests that the first condition must be kept in mind by policy makers.

IX. Conclusions

There has been little empirical research on the question of what happens to adult labor markets when children leave the workforce. Policy makers who need a reliable answer to this question in order to make child labor law effective have in fact been forced to assume the answer. Any empirical strategy to answer this question must surmount two hurdles: (1) it must find a program that reduces child labor supply without directly

affecting adult labor demand, and (2) it must identify changes in adult labor demand without assuming constant adult labor supply. I hypothesize and demonstrate that randomized schooling experiments can reduce child labor supply without directly affecting adult labor demand. Furthermore, I make use of coordinated movements in price and quantity to identify the direction of movements in adult labor demand without assuming constant adult labor supply.

I apply this strategy to Mexico's PROGRESA experiment. The results demonstrate that when the opportunity wage of not working increased, child workers responded by decreasing their labor participation rates. I rule out alternative pathways to conclude that this reduction in child labor participation is what caused an increase in the equilibrium price and quantity of adult labor. Thus, in these areas of rural Mexico during the autumn corn harvest, adult labor substitutes for child labor. The partial elasticity of adult hourly wages with respect to child work participation is clearly negative.

The first implications of these results are theoretical. Models such as those of Basu and Van (1998), and Ranjan (2001) – which assume that child and adult labor are substitutes – are reinforced by my result. Indeed, in the context of Basu and Van's 1998 model "The Economics of Child Labor," this paper's update of the previous empirical results – which had showed ambiguous effects of changes in child labor supply on adult wages – is very useful. By providing evidence for their labor demand assumption (the "Substitution Axiom"), the result of my paper reinforces the theoretical possibility that their paper introduced: stricter child labor laws may help labor markets escape a kind of poverty trap. Since Basu and Van's child labor supply assumption (the "Luxury Axiom")

has been supported by recent empirical evidence from another agricultural region, my result helps close a remaining empirical gap (Edmonds 2003).

Second, these results are of general use to policy makers, because they suggest that in environments similar to the one observed here (corn-based agriculture), efforts to reduce child labor may have positive impacts on adult wages and employment. This means that programs to reduce child labor may only require funds for better schools, better enforcement of labor laws, and better transfers *within* communities – they may not require large injections of cash from *outside* communities to make up for lost child and adult wages.

Finally, this paper is the first *experimental* estimate of labor demand parameters across labor input types. The idea of this paper can be easily applied to the many other schooling experiments recently conducted in Latin America and in other nations in the developing world, thus showing how these results vary across regions, time, level of industrialization, and cultures.[22] The results here may thus be the first of a set of useful estimates of the medium-term effects of child labor reduction on adult labor market outcomes.

XI. References

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[22] For other experiments see, e.g., Janvry & Sadoulet 2005

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Appendix

A1. Construction of the no-treatment money sample and comparison control group

Each person in both treatment and control villages can be identified from the surveys as a member of one of three eligibility categories: (1) originally eligible; (2) eligible under the re-calculation of eligibility status in 1998; and (3) never eligible. The PROGRESA administrators assigned people who were materially well-off to category three, and people who were less-well off to category one. Everyone in both treatment and control villages is in one of these three groups, and the method of assignment should not have varied depending on whether one is in a treatment or control village. Therefore, people within a given eligibility category should be relatively similar across treatment vs. control villages. Figure 7 shows the breakdown by eligibility status of families living in treatment villages in 1997.

In order to find out which individuals in particular did not receive treatment money, I obtained administrative records identifying the recipient households and the timing for all payments made during the PROGRESA evaluation from the PROGRESA evaluation website, at http://evaloportunidades.insp.mx/en/index.php. I found that almost everyone living in treatment villages who was in eligibility category three never received money, but that in addition many of the presumably poorer people in eligibility category two also never received money (about 60 percent of them). According to Hoddinott, Skoufias and Washburn 2000, the PROGRESA administration claims that of the households that were eligible to receive benefits but never did receive any, 85.7 percent did not receive benefits because the administrators never incorporated them into the program. Thus, it seems that there is little room for selection in this sample of non-

treated people living in the treatment group. In addition, because I am able to include people in eligibility category two, my sample of non-treated people in the treatment group includes households that are not restricted to be the richest in the villages.[23]

I construct a similar comparison sample in the control group by including everyone in the control group who is in eligibility category 3 and a random sample of 60 percent of the people in eligibility category 2.[24] Since the households within a given eligibility group should be fairly similar by administrative design, and since the administrators should not have used different standards for eligibility status in the control and treatment villages, this technique creates a control group comparison sample that should be fairly similar to the treatment group non-treated sample. Table A1a shows baseline (1997) summary statistics for the two samples.

Table A1b shows the results of my hourly wage specification on these samples, with five percent symmetric cropping and controls and village fixed effects as before. These results demonstrate that by 1999 there was a significant wage increase even for the much smaller group of people living in treatment villages who did not receive treatment money. Table A1c shows the results of my quantity specifications on this sample – they suggest that the quantity of adult jornalero labor in this sample also increased.

Finally, as a robustness check I consider a further subsample of the above adult jornaleros who are perfectly healthy according to the following ten criteria: days of

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[23] As a robustness check (to avoid potential problems with selection), I also consider only the richest people in each village: those in category three who were never eligible to receive treatment according to the criteria applied to both control and treatment villages. Performing kolmogorov smirnov tests on the wage distributions in 1997 and 1999 shows that before treatment I can reject their inequality, but after treatment I cannot reject that the control distribution is smaller. This holds for the overall sample, and for the healthy-only sample described at the end of this section. Thus, the results in this section seem to be robust to restricting the sample to only the never eligible, where there are fewer potential problems with selection.

[24] The results are similar when my control sample includes all the people in eligibility category two (with weights of 0.6) and all in eligibility category three (with weights of 1.0).

Table A1a: Comparison of baseline characteristics of no-treatment sample in treatment villages with comparison sample in control villages.[25]

Year	Variable	Control Villages	Treatment Villages
	# families	4,276 families	5,530 families
	# people	15,874 people	24,453 people
	% male	50.9%	51.4%
	% child (< 17 years)	33.2%	34.5%
	% adult (17 to 59 years)	54.7%	54.0%
	% worked last week	48.5%	46.7%
	% worked as jornalero	16.8%	16.5%
1997	Mean jornalero wage	3.80 pesos / hour	3.72 pesos / hour
	Mean age	*29.3 years*	*30.0 years*
	% with high schooling	*18%*	*20%*
	% speaking a dialect	*19.3%*	*22.4%*
	% literate	*79.3%*	*78.2%*
	% married	*39%*	*41%*
	% separated	17%	19%
	% divorced	0.18%	0.16%
	% widowed	5.4%	5.6%

difficulty performing activities due to bad health in the past month are 0; days of missed activities due to bad health in the past month are 0; days in bed due to bad health in the past month are 0; yes, I can currently perform vigorous activities; yes, I can currently perform moderate activities; yes, I can carry an object of 10kg for 500 meters with ease; yes, I can easily lift a paper of the floor; yes, I can walk 2 km with ease; yes, I can dress myself with ease; I have had no physical pain in the last month. Without updating the cropping from the larger subsample above, I perform the same difference and difference regression on wages. The results show that point estimates of the treatment effect are essentially unchanged, and remain statistically significant.

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[25] Italicized entries are significantly different at the 5% level in t-tests without clustering. In this baseline survey, no variables are significantly different at the 5% level in tests with clustering at the village level.

Table A1b: Treatment Effect on log hourly wages and log daily income from 1997 to 1999 for no-treatment sample and comparison control sample

Dependent Variable: log hourly wages or log daily income
for Adult (ages 17 to 59) Jornaleros

Explanatory Variables	(1) Log hourly wage	(2) Log hourly wage	(3) Log daily income	(4) Log daily income
Treated (post = 1 & treatment village = 1)	0.018** (0.009)	0.022** (0.009)	0.0120** (0.008)	0.020** (0.009)
Post-treatment Dummy	0.320*** (0.007)	0.318*** (0.007)	0.301*** (0.006)	0.300*** (0.009)
Male Dummy	0.022*** (0.009)	0.020** (0.010)	0.061*** (0.009)	0.060*** (0.009)
Age	-0.000 (0.000)		-0.000** (0.000)	
Age Dummies		YES		YES
Schooling Level Dummies		YES		YES
Language Skills Dummies		YES		YES
Marriage Status Dummies		YES		YES
Village Fixed Effects	YES	YES	YES	YES
Constant	1.14*** (0.01)	1.14*** (0.02)	3.20*** (0.01)	3.17*** (0.02)
# Observations	8944	8647	8977	8653
R2	0.31	0.31	0.30	0.30

Standard Errors in Parenthesis
** = significant at 5% level
*** = significant at 1% level

Table A1c: Treatment Effect on log hours per week and log days per week from 1997 to 1999 for no-treatment sample and comparison control sample

Dependent Variable: log hours per week or days per week
for Adult (ages 17 to 59) Jornaleros

Explanatory Variables	(1) Hours per week	(2) Hours per week	(3) Days per week	(4) Days per week
Treated (post = 1 & treatment village = 1)	0.032** (0.015)	0.036** (0.016)	0.028** (0.014)	0.035** (0.014)
Post-treatment Dummy	-0.085*** (0.012)	-0.087*** (0.012)	-0.065*** (0.011)	-0.068*** (0.011)
Male Dummy	0.112*** (0.016)	0.117*** (0.016)	0.079*** (0.015)	0.081*** (0.015)
Age	-0.001*** (0.000)		-0.001** (0.000)	
Age Dummies		YES		YES
Schooling Level Dummies		YES		YES
Language Skills Dummies		YES		YES
Marriage Status Dummies		YES		YES
Village Fixed Effects	YES	YES	YES	YES
Constant	3.64*** (0.019)	3.59*** (0.034)	1.58*** (0.017)	1.56*** (0.031)
# Observations	8997	8698	9019	8716
R2	0.01	0.02	0.01	0.01

Standard Errors in Parenthesis
** = significant at 5% level
*** = significant at 1% level

Figure 7. Percentage breakdown, by eligibility status, of families in treatment villages in 1997

9%: Eventually Eligible, Received Money

2%: Eventually Eligible, Did Not Receive Money (Unknown Cause)

59%: Originally Eligible

11%: Eventually Eligible, Did Not Receive Money (Administrative Error)

19%: Never Eligible