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 adults as complements to children? 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 the supply of child farm labor is accompanied by an increase in the demand for adult farm labor. This increase was not directly caused by treatment money reaching employers: there were no significant effects on expenditures on non-labor inputs, on hectares of land used, on output prices or on 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 and 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 more seriously harm household welfare, and thus such interventions may need to be accompanied by more 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: complementarity can arise for simple reasons such as the necessity of adults to monitor child workers. 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 Rosanna Galli cites 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.

Applying this new strategy to Mexico's PROGRESA experiment, I find that a decrease in child farm 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 expenditures on non-labor inputs, on hectares of land used, on food prices, or on harvest size (Section VII). 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 (Section VII).

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 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." Levinson, Anker, Ashraf and Barge (1996) use a survey of 362 carpet-weaving firms in India to conclude that children and adults perform similar tasks with similar levels of productivity – thus rejecting the "nimble fingers" argument for complementarity between adults and children. 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), household surveys (which, in the absence of some exogenous shift in labor supply, simply produce estimates of parameters of labor supply), or task-based evidence (which is not ideal, because inputs may be used for seemingly similar tasks without necessarily being substitutes), it is not possible to draw good conclusions from this literature about causal relationships between child labor supply and adult labor demand.

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

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. Throughout, I am careful to account for the effects of the PROGRESA treatment on any variables that are related to my identification. I develop the strategy below.

III. Conceptual Framework and Identification Strategy

In this section, I present a standard theoretical model of production. I use the model to structure the identification strategy, to harmonize apparently contradictory potential effects of an increase in child wages, and to explain how the results of such an identification strategy can and cannot be generalized.

(A) The Model: Farm Production

Suppose that there are a large number of farms buying and selling in competitive input and output markets. Each farm has the following production function:

$$Y = F\left(X^1,\ldots,X^i,\ldots,X^K\right) \tag{1}$$

(where Y is the quantity of output and X^i is the quantity of factor i used in production).

I assume that F is strictly concave and strictly increasing in each argument.[2] Each farm solves its production problem in two steps. First, it calculates how to minimize the total cost associated with the production of a given quantity Y of output. Second, it calculates the quantity of output that maximizes its profits.

[2] Note that much of the notation in this section is adapted from Cahuc and Zylberberg (2004).

Let w^i be the strictly positive wage paid to factor *i*. The cost minimization problem can then be written as follows:

$$\underset{(X^1,\ldots,X^K)}{Min} \sum_{i=1}^{K} w^i X^i \text{ subject to } F\left(X^1,\ldots,X^K\right) \geq Y \tag{2}$$

The Lagrangian associated with this problem is:

$$L = \sum_{i=1}^{K} \left(w^i X^i + \lambda \left[F\left(X^1,\ldots,X^K\right) - Y \right] \right) \tag{3}$$

I define F_i to be the partial derivative of F with respect to its i^{th} argument. The first order conditions are thus:

$$\partial L / \partial X^i = w^i + \lambda F_i = 0, \; \forall i = 1,\ldots,K \tag{4}$$

Because w^i and F_i are strictly positive, the production constraint is binding. Because F is strictly concave, these necessary conditions for optimality are also sufficient conditions for optimality.

I can then use these first order conditions and the binding production constraint to implicitly define the conditional factor demands: $\overline{X^i}$. These are the demands for each factor holding output constant (while the unconditional factor demands, X^i, are the demands for each factor assuming that output has been optimized). I define the cost function to be the minimum value of the total cost. The cost function thus depends on the factor prices and on output: $C\left(w^1,\ldots,w^K,Y\right)$.

Given these definitions, it is easy to show that the cost function satisfies Shephard's Lemma:

$$\overline{X_i} = C_i\left(w^1,\ldots,w^K,Y\right), \forall i = 1,\ldots,K \tag{5}$$

I define adult workers to be factor 1 and child workers to be factor 2. I can then obtain an expression for the effect of a change in the wage of child workers (w^2) on the unconditional demand for adult workers (X^1), by taking the derivative of equation (5) with respect to w^2, allowing both the optimal output Y and the demand for other factors to adjust to the new price of child labor:

$$\partial X^1 \big/ \partial w^2 = C_{1,2} + C_{1,Y} \, \partial Y \big/ \partial w^2 + \sum_{i=3}^{K} \left(C_{1,i} \, \partial w^i \big/ \partial w^2 \right) \qquad (6)$$

I define children and adults to be gross-substitutes if: $\partial X^1 \big/ \partial w^2 > 0$

I define children and adults to be gross-complements if: $\partial X^1 \big/ \partial w^2 \leq 0$

A priori, the sign of equation (6) is unknown. If there are only two inputs, then the first term of (6) is necessarily greater than zero; else, its sign is indeterminate. If the production function is homogenous, then the second term of (6) is necessarily less than zero; else, its sign is indeterminate. Thus, regardless, the sign of (6) is undetermined theoretically, and I must apply an identification strategy to empirical data in order to identify its sign in any given setting.[3]

(B) Basic Identification Strategy

Based on equation (6) and the definitions of gross-substitutes and gross-complements above, it is clear that the foundation of the identification strategy will be to observe an exogenous change in w^2 and a response to this change in the function X^1. In order for the change in w^2 to be exogenous, it must be caused by forces outside the farm; in other words, it

[3] My definition of gross-substitutes allows for the optimal output to change in response to the changing cost structure. It is thus *not the same as that definition of substitutes that assumes constant output*. However, I will be working throughout in a setting in which the number of farms that are perturbed by the change in child wages is small relative to the total number of farms that make up the entire world market for the output, corn. Thus, the price of output is being held constant, and my definition of substitutes must be assumed to be: gross-*substitutes when the price of output is held constant*. This is therefore not the same as that notion of gross-substitutes in which the price of output potentially changes due to the increase in child wages. I will discuss this more in section III(F) below.

must be caused by a change in the supply of child labor (factor 2). Thus, the basic identification strategy consists of identifying a change in the supply of child labor and a response in the unconditional demand for adult labor (X^1).

Identifying changes in the unconditional demand for adult labor can be challenging.[4] This is because if something changes household utility sufficiently to affect the supply of child labor, then it is impossible to rule out that this change in household utility also affected the supply of adult labor. Thus, I must identify changes in adult labor demand without assuming constant adult labor supply. This identification is possible by considering both the price and quantity of adult labor. In Figure 1 below, I show graphically that if the unconditional demand schedule for adult labor is downward-sloping (which it is in the model above), then an increase in the price of adult labor without a decrease in the quantity hired of adult labor (or vis-versa) implies that the labor demand schedule must have increased (or decreased) – in other words, that the optimal X^1 must have increased. But if price and quantity change in opposite directions, then 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.[5]

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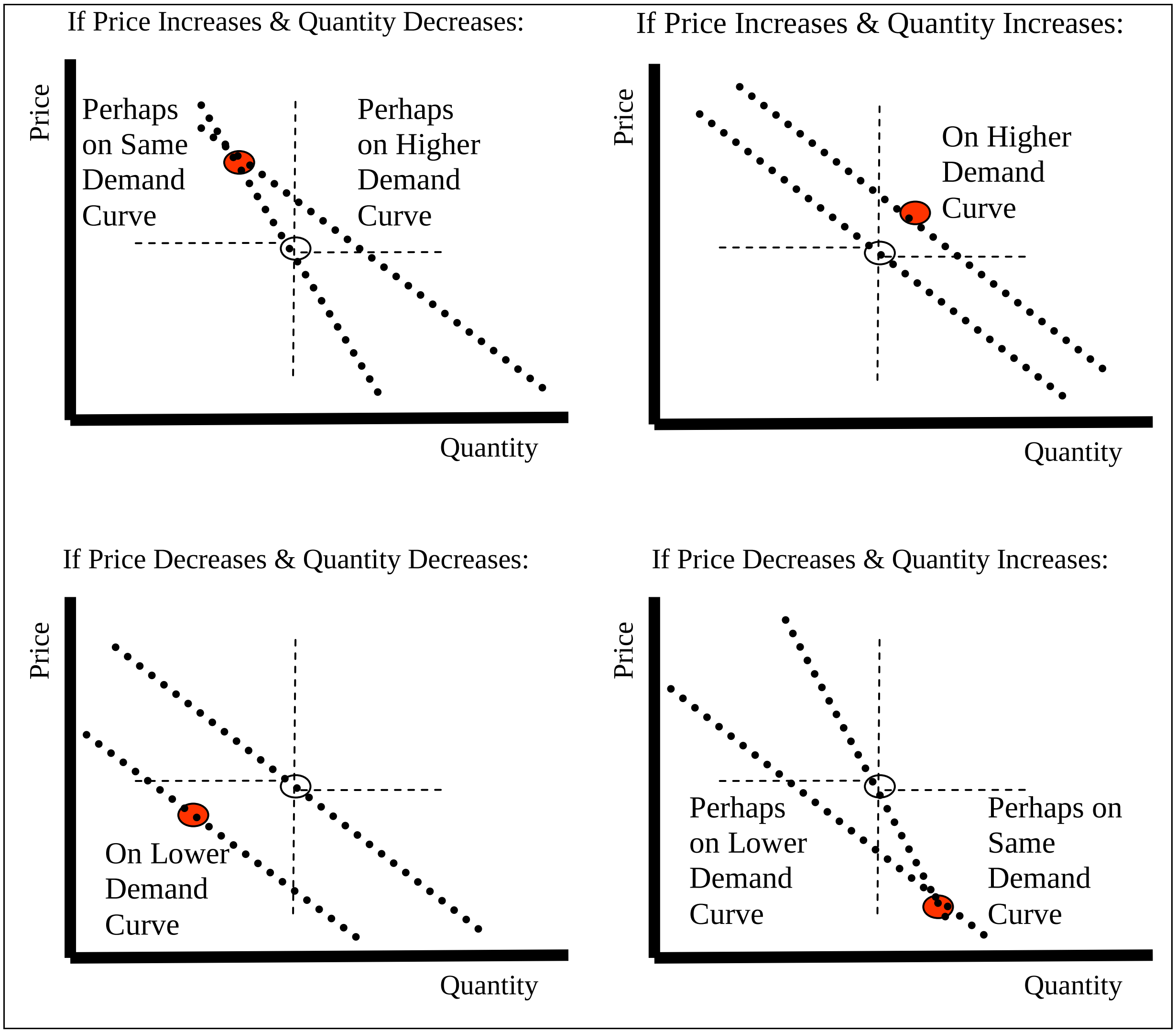
[4] Note that throughout this paper, when I refer to "adult labor demand" (or, "the demand for adult labor"), I am referring to the function describing the desired quantity of adult labor for firms as a function of the wage, given a set of other parameters. When I wish to distinguish the particular point in price and quantity space that is actually chosen on the labor demand schedule in any particular equilibrium, I instead always refer to the "price of adult labor," or the "quantity of adult labor," or the "quantity demanded of adult labor." Thus, an increase in adult labor demand refers to a shift in the entire demand function, whereas a change in the quantity demanded of adult labor refers to a change in the particular point in price and quantity space that is actually chosen in equilibrium.
[5] 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 1: Determining Demand Movements When Supply Moves as Well



In the next subsections, I consider whether equilibrium in other factor markets and the output market imply any testable predictions that can be used to distinguish between the gross-complementarity or the gross-substitutability of children and adults.

(C) Accounting for the Supply of other Factors

I must be careful of the supply of other factors. If the supply of a third factor changed, then I cannot determine whether the observed sign of the change in X^1 came from the change in the supply of child labor, or from the change in the supply of the third factor. In other words,

$\partial X^1 / \partial w^2$ could be negative or positive without affecting the sign of the observed change in X^1, if the magnitude of $\partial X^1 / \partial w^3$ was sufficiently large. Therefore, I will also need to check whether the supplies of other factors remained constant. In the absence of other information, the only way to be sure of this is to check the price and quantity of each of the other inputs and measure whether they each remained constant. If so, it is not possible that their supplies changed. I relate this identification requirement to the zero profit condition below.

(D) Accounting for the Farms' Zero Profit Condition

A change in the wage of children (denoted by Δw^2) will potentially affect the equilibrium in the other factor markets. In the long-run, the change in the price of output, Δp, must satisfy the following equation:

$$\Delta p = \sum_{i=1}^{K} \theta^i \cdot \Delta w^i \text{, where } \theta^i = \text{the cost share of factor } i \qquad (7)$$

In this agricultural setting, the price of corn is likely set on a world market; in any case, it is not altered by the treatment in comparison with non-treated villages (as I show later empirically). Thus Δp will be 0 in the equation above. Using this fact, it is clear that if Δw^2 and Δw^1 are both positive (as would be the case if adults are substitutes with children and the supply of children decreases), then it is necessarily the case that Δw^3 should be negative for some third factor.

Technically, this does not introduce another testable prediction. Rather, this conclusion is drawn from an equation that is only required to hold in long-term equilibrium. In the short-term, firms can and will produce output at prices that exceed average variable cost but are exceeded by average total cost, thus breaking the equality of equation (7). Since the relative decrease in child labor supply in my data covers only the period of one or at most two harvests, it is impossible to rule out that short-term effects will dominate, leaving the prices of third factors potentially unchanged by the increase in child wages. Nevertheless, it is likely that the demand for at least one other factor must decrease. I discuss the empirical evidence for this, and the implications for my identification, on pages 38-40.

(E) Accounting for the Supply and Demand of Output

If the fact that children and adults are gross-substitutes (or gross-complements) has a necessary implication for the supply of output, then this implication would have to be checked in a complete identification strategy, and if it was verified, it would serve as a useful piece of evidence for my conclusion. Rearranging terms in equation (6) clearly shows that the sign of $\partial X^1 / \partial w^2$ does not determine the sign of $\partial Y / \partial w^2$. Likewise the fact that w^2 increases or decreases does not in itself imply anything about the direction of any changes in the marginal cost schedule, or hence the direction of any changes in *Y* when prices are held constant.[6] This implies, of course, that general equilibrium effects on the supply of output are ambiguous.

However, exogenous changes in the demand for output can blur my identification in an analogous way to that specified in III(C) above. Thus, I must rule out any changes in the demand for output. This requires verifying that the price and quantity of output remained the same.[7]

[6] This is surprising. It has often been asserted that an increase in the price of one factor always increases the marginal cost schedule, thus necessarily reducing optimal output when output prices are held constant. In fact, this is only true of normal factors – inferior factors of production cause the opposite effect. Likewise, while an increase in the price of one factor always increases average costs, it does not always decrease the optimal output at the bottom of the average cost curve, i.e. the optimal output when prices adjust through free entry. This is only true of superior factors (those whose output elasticity of demand exceed one).

[7] Given the facts that I assert in footnote 6 above, it is clear that this restriction in general neither rules out a particular direction for the change in the supply of child labor, nor the gross-complementarity or gross-substitutability of children and adults.

(F) Generalizability

The identification strategy outlined above can be applied to any given setting of production, with particular production technologies and particular factor prices. If the production technologies differ in other settings, then the cost structures may be radically different in other settings, making the predictions from one setting about the sign of $\partial X^1 / \partial w^2$ irrelevant for other settings. But even holding production technologies constant, the shape of the average cost curve (including the sign of its second derivative) can change radically as relative factor prices change (Takayama 1993). Thus, the results from applying this identification strategy cannot be generalized to settings with the same production technologies but different relative factor prices. In other words, this paper's estimates suffer from the same lack of generalizability found in any paper estimating production parameters.

Furthermore, as I stated in Section III(A), throughout the paper my setting has restricted the price of output to be constant (because the perturbation to child wages in my data occurs in only a very small fraction of the total number of farms which together make up the much larger corn market). Thus, my measure of substitutability would not necessarily be useful in the context of a world-wide elimination of child labor which in turn affected the world-wide cost structure of corn production. However, I note that according to the USDA, Mexican corn production in 2007 accounted for less than three percent of total world corn output (USDA, (b)). Thus, even the end of all child labor in Mexico would be unlikely to affect the world price of corn. Therefore, my measure of substitutability should be useful for estimating the effects of a nation-wide end of child labor in any individual nation that itself makes up a very small portion of the world corn market. Since the only nations that individually produce more than three percent of total world corn output are the United States, China, and Brazil (and since at least eleven other nations produce between 0.5 and three percent of world corn output each), this leaves my measure of substitutability useful for most individual corn-producing nations that use child labor around the world (USDA, (b)).

(G) Summary

I summarize the identification strategy in Table 1. First, I must observe the price and quantity hired of child labor moving in opposite directions due to some treatment. Second, I must observe the price and quantity of adult labor moving in the same direction in the areas in which child labor has been treated. Third, I must observe constant price and quantity of output. Fourth, I must observe constant price and quantity of almost all of the other factors of production (allowing for the probability of a simultaneous decrease in price and quantity of at least one other factor of production).[8] This information is necessary to determine whether adults and children are gross-substitutes or gross-complements in this setting.

Table 1: Mapping from Treatment Effects to Gross-Substitutes vs. Complements

	Gross-Substitutes	Gross-Complements
Wage of Children	Increase	Increase
Quantity of Children Hired	Decrease	Decrease
Wage of Adults	Increase	Decrease
Quantity of Adults Hired	Increase	Decrease
Price of Output	Constant	Constant
Quantity of Output	Constant	Constant
Price of Land	Constant	Constant
Quantity of Land	Constant	Constant

In Section IV, I introduce the data set and treatment program that I use to carry out this strategy. In Section V, I report that the treatment increased child wages and decreased child work participation. In Section VI, I report that the treatment increased adult wages without

[8] In Section VII, I explain why the empirical evidence for a decrease in demand for one factor of production does not also open up the possibility of a decrease in the supply of that factor of production – thus my identification is unaffected by the empirical evidence of a decrease in demand for that factor.

decreasing the quantity of adult work. In Section VII, I report that the treatment held constant the price and quantity of output as well as the price and quantity of all non-labor inputs except for land (whose quantity held constant but whose price may have decreased). Thus, my results satisfy the necessary theoretical requirements for children and adults to be gross-substitutes. In Section VII, I further rule-out other empirical challenges to identification, concluding that employers substituted adults for children when children left the workforce.

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 and Parker 2001). Of these villages, 320 were randomly selected to form the treatment group. The remaining 186 villages formed the randomized control group.[9]

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 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

[9] See Behrman and Todd 1999 for a discussion of the randomization procedure.

surveys are pre-treatment, and the latter three surveys are during the treatment. After the experimental phase was complete, eligible control families began receiving benefits as well.

PROGRESA administrators used the results of the October 1997 census to determine, based 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.[10] If a child under 18 missed fewer 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 child's grade, 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 and Parker 2001).

I make use of data from this experimental phase of PROGRESA. I obtained the data from the Opportunidades office. I primarily 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 2.

[10] 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 2: 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 2: 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 2, 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, (a)), although a smaller corn harvest occurs in the summer. Thus, I interpret my results as information about production technology and labor demand during the primary 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 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* (farm workers), and *obreros* (non-farm 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 4) 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).[11]

Table 4 reports summary statistics for important variables across both treatment and control villages over the three years in my sample: 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.

[11] 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 3: Pre-treatment Distribution of Adults and Children across job categories

Year	% with Job Title:	Adults	Children
1997	Jornalero (farm worker)	15,675 (50%)	1,701 (38%)
	Obrero (non-farm 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%)

Table 4: 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.

I classify people who are ages 16 and under as children and people ages 17 to 59 as adults. 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 often 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).[12]

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 most mean regressions of hourly variables using daily variables – i.e., with daily income instead of hourly wages, and days worked per week instead of hours per week. This helps ensure that measurement error in hours is not driving the results.

V. Did the Experiment Reduce the Supply of Child Labor?

In the first few months of the program, as measured by the 1998 survey, it is unclear whether the experiment has yet reduced the supply of children to the jornalero workforce. But by 1999, 18 months after the program started, the treatment has clearly caused a decline in child participation in the jornalero workforce as well as an increase in the wages of child jornaleros. 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 work participation, hourly wages, etc. My unit of observation is an individual at a point in time. As Table 2 showed, some

[12] This cropping is carried out relative to the sample used in each regression (usually, this is all adult jornaleros, but sometimes, for the purpose of identification, it is a subsample, as in Section VII(C)). The statistical significance of some mean wage regressions is sensitive to wide variation in the level of cropping, but it is fairly robust.

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.[13] 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 including controls for important personal characteristics.[14] 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.[15]

Thus, in summary, the difference-in-difference equations are of the following pattern:

$$Y_{i,t} = a \cdot Diff-in-Diff_{i,t} + b \cdot TreatmentVillage_{i,t} + c \cdot Post_{i,t} + d \cdot PersonalChars_{i,t} + \varepsilon_{i,t}$$

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

The *Diff-in-Diff* 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 (this dummy is not included in specifications which include village-level fixed effects). The *Post* dummy is 1 whenever the observation is from a post-treatment survey. I include in *Personal Characteristics* dummies for gender, age,

[13] Furthermore, a key PROGRESA paper 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).

[14] 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).

[15] I use the "robust cluster" command in Stata. The variance-covariance matrix is determined by the following formula: $V_{cluster} = (X'X)^{-1} \sum_{j=1}^{n_c} u_j' \cdot u_j (X'X)^{-1}$

(where $u_j = \sum_{j_{cluster}} e_i x_i$, n_c is the total number of clusters, and e_i is the residual of observation i).

schooling, language abilities and marriage status (where these are age and specification appropriate). 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

I add to the previous studies of this experiment ((Schultz 2004) [16] and (Skoufias and Parker 2001)[17]) that estimated significant decreases in work participation for children, by estimating specifically the treatment effect on child 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. Table 5 reports the results of probit specifications of this regression model. I find that by 1998, there was no significant effect on child jornalero farm work participation.[18] However, by 1999, child jornalero work participation saw a significant decrease of seven percent due to the treatment.[19] This corresponds with Skoufias and Parker's result that only by 1999 did 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) see a significant decrease in child work participation.

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[16] 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.

[17] 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."

[18] The 1998 wage change is statistically significant if I do not adjust the standard errors for village-level clustering

[19] The percentage change is calculated by dividing the coefficient on the Diff-in-Diff dummy from Table 4 by the pre-treatment mean value of the independent variable, 0.054.

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. Hence, the treatment caused a decline in the quantity of paid child farm labor by 1999.[20]

Table 5: 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
Diff-in-Diff (post = 1 & treatment village = 1)	-0.003 (0.002)	-0.004** (0.002)
Post-treatment Dummy	-0.005** (0.002)	-0.006*** (0.002)
Male Dummy	0.047*** (0.001)	0.043*** (0.001)
Age Dummies	YES	YES
Village Fixed Effects	YES	YES
# Observations	61128	58852
Pseudo R2	0.28	0.30

Coefficients reported are the marginal effects.
Standard errors, adjusted for village-level clustering, are in parenthesis.
* = significant at 10%, ** = at 5% *** = at 1%

(C) The Increase in Child Jornalero Wages

If the supply schedule of child jornalero labor slopes upward, then it will always be true that the supply schedule has shifted backward (decreased) when a decrease in the quantity of child jornalero labor is accompanied by an increase in the price of child jornalero labor. Thus, in

[20] There is also a statistically significant decrease in the total hours of paid child farm work, according to a tobit regression of the hours of paid farm work per week, censored at zero for non-participants.

Table 6, I report the results of OLS estimation of the difference-in-difference treatment effects on child hourly wages. It is clear that the treatment has caused an over ten percent increase in mean child hourly wages, and that this increase is statistically significant.

Table 6: Treatment effects on child jornalero log hourly wages

Dependent Variable: log hourly wage
for children aged less than 17 years old who report working as a jornalero (Baseline year: 1997).

Explanatory Variables	(1) Post-Treatment: 1998	(2) Post-Treatment: 1999
Diff-in-Diff (post = 1 & treatment village = 1)	0.08* (0.04)	0.12*** (0.05)
Post-treatment Dummy	0.08*** (0.03)	0.26*** (0.03)
Male Dummy	0.07* (0.04)	0.05 (0.04)
Age Dummies	YES	YES
Village Fixed Effects	YES	YES
# Observations	2736	2666
R^2	0.04	0.13

Standard errors, adjusted for village-level clustering, are in parenthesis.
* = significant at 10%, ** = at 5% *** = at 1%

Because the participation rate of children in jornalero labor has declined (as reported in Table 5) and the hourly wage of children in jornalero labor has increased (as reported in Table 6), I conclude that the labor supply of children to the jornalero workforce has declined. In Section VI, I estimate the treatment effects on the quantity and price of adult labor to assess any change in the demand for adult labor. In Section VII, I then look for additional evidence to determine whether the decline in child labor supply to the farms was responsible for the change in farms' demand for adult labor.

VI. Was there an Increase in the Demand for Adult Labor?

Since the results in the previous section showed that there was a decrease in child labor supply to the jornalero workforce by 1999, I need to check whether the demand for adult labor increased by 1999.[21] As I explained in Section III, 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. I thus check whether by 1999 there was an increase in the price of adult jornalero labor without an accompanying decrease in the quantity. I consider first the treatment effect on the price of adult labor, and second 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 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.

[21] 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.

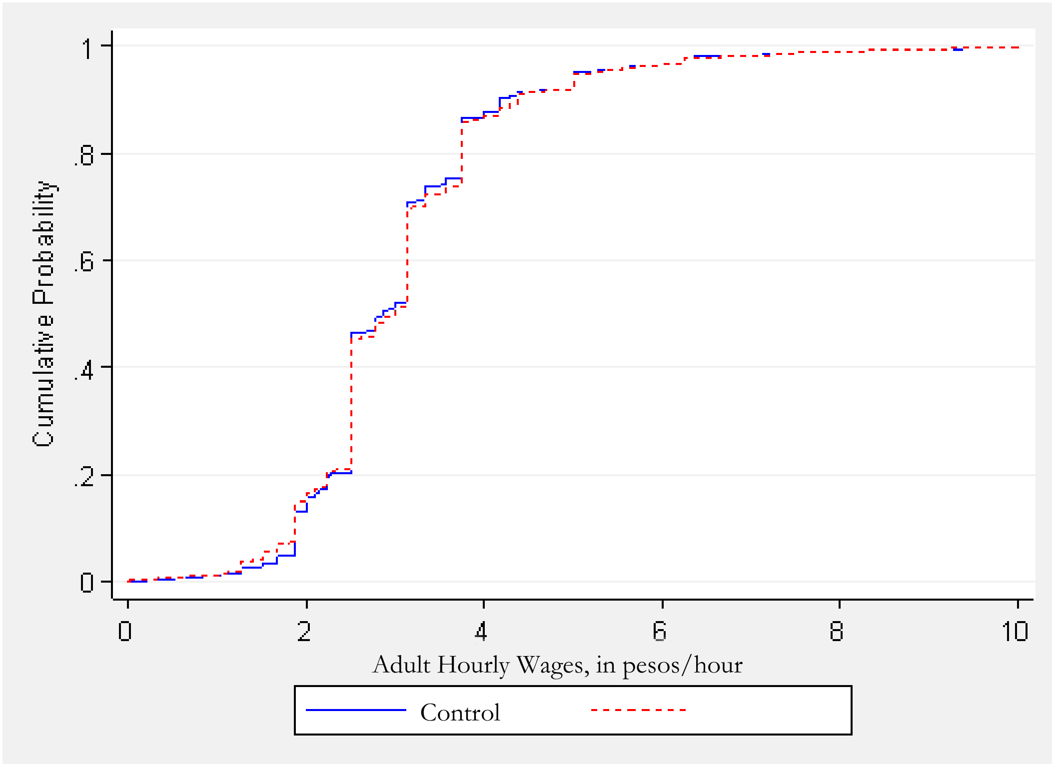
The kolmogorov smirnov test on the pre-treatment distribution functions shows that the pre-treatment distribution of wages in treatment villages is first-order stochastically dominated by that in the control villages. The p-value for the null hypothesis that the two distributions are identical – when 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 3, 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 7 show that four deciles increased significantly (two below the median and two above) and none decreased significantly.

Figure 3: Cdfs of Hourly Wages, Control vs. Treatment, 1997 & 1999

1997:



1999:

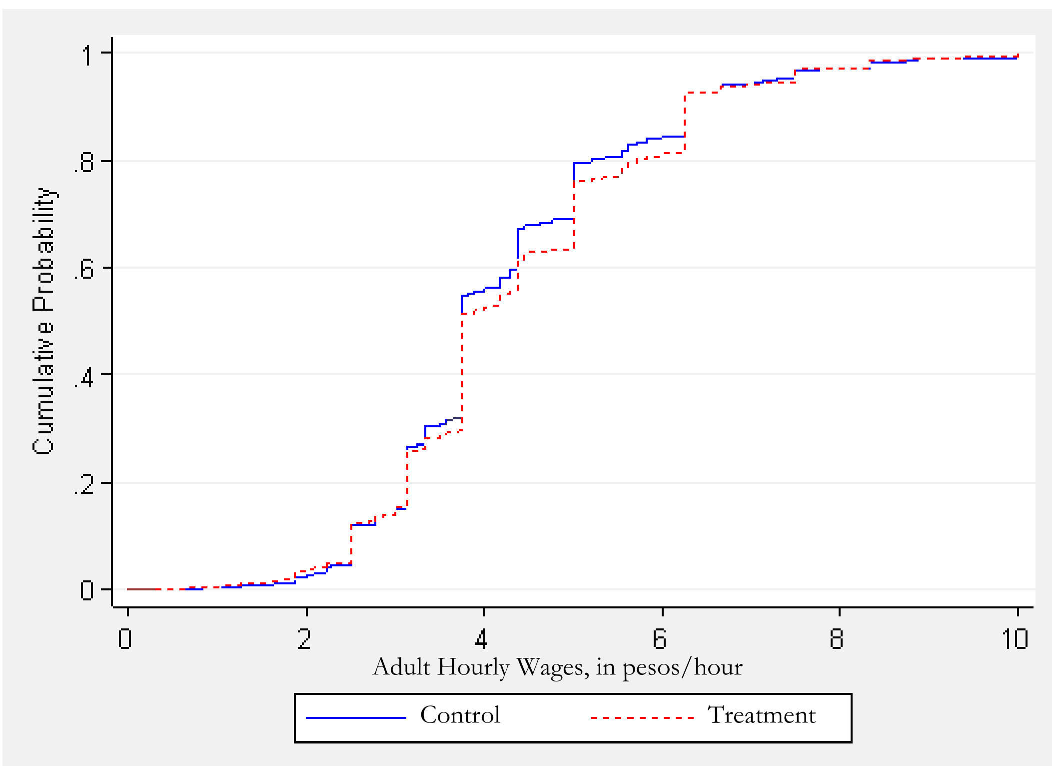


Table 7. Quantile Difference-in-Difference Treatment effects on Hourly Wages, no controls or cropping

	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.

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 et al. 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 Section V (with the effect of the tails diminished via the cropping discussed in Section IV), reporting the results in Table 8. There is an increase in adult jornalero wages of between three and six percent. A replication of Table 8 with weekly earnings gives similar results.

Table 8: Treatment effects on adult log hourly wages and log daily income from 1997 to 1999

Explanatory Variables	(1) Log Hourly Wage	(2) Log Hourly Wage	(3) Log Daily Income	(4) Log Daily Income
Diff-in-Diff (post = 1 & treatment village = 1)	0.065*** (0.016)	0.041*** (0.014)	0.061*** (0.006)	0.031** (0.014)
Treatment Village Indicator	-0.039** (0.018)		-0.029*** (0.005)	
Post-treatment Indicator	0.332*** (0.011)	0.326*** (0.009)	0.314*** (0.005)	0.308*** (0.009)
Male Indicator	0.010 (0.011)	0.022** (0.009)	0.037*** (0.007)	0.056*** (0.009)
Age, Schooling Level, Langauage Skills, and Marriage Status Indicators	YES	YES	YES	YES
Village Fixed Effects		YES		YES
Constant	1.12*** (0.023)	1.08*** (0.014)	3.17*** (0.014)	3.12*** (0.014)
# Observations	24605	24605	24605	24605
R2	0.42	0.37	0.41	0.35

Standard Errors, corrected for village-level clustering, are in parenthesis.
* = significant at 10%, ** = at 5% *** = at 1%

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

Having established that, by 1999, the treatment increased the price of adult jornalero labor, I turn now to the quantity of adult labor hired. I estimate treatment effects on mean work outcomes for adult jornaleros between 1997 and 1999. From Table 9, it is clear that the treatment increased both adult hours worked per week and adult days worked per week conditional on working.

Table 9: Treatment effects on log hours worked and days worked per week from 1997 to 1999

Explanatory Variables	(1) Log Hours per Week	(2) Log Hours per Week	(3) Log Days per Week	(4) Log Days per Week
Diff-in-Diff (post = 1 & treatment village = 1)	0.035* (0.020)	0.026† (0.019)	0.039** (0.018)	0.035** (0.017)
Treatment Village Indicator	-0.026 (0.016)		-0.036** (0.015)	
Post-treatment Indicator	-0.077*** (0.015)	-0.082*** (0.014)	-0.059*** (0.013)	-0.064*** (0.012)
Male Indicator	0.102*** (0.017)	0.113*** (0.015)	0.075*** (0.015)	0.079*** (0.013)
Age, Schooling Level, Langauage Skills, and Marriage Status Indicators	YES	YES	YES	YES
Village Fixed Effects		YES		YES
Constant	3.60*** (0.027)	3.58*** (0.021)	1.56*** (0.024)	1.54*** (0.020)
# Observations	24575	24575	24575	24575
R2	0.02	0.02	0.01	0.01

Standard Errors, corrected for village-level clustering, are in Parenthesis.
 * = significant at 10%, ** = at 5% *** = at 1%
† = significant at the 1% level without village-level clustering.

Table 10 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.[22]

[22] Three of the four specifications in Table 9 show significant increases in adult jornalero labor (conditional on jornalero work participation). Specification (1) of Table 10 shows a significant increase in jornalero work participation as well. But in specification (2) of Table 10, 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

Table 10: 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
Diff-in-Diff (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	
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, and are corrected for village-level clustering in specifications (2) and (4). * = significant at 10%, ** = at 5% *** = at 1%. P-values for Probits are in brackets

Since the treatment increased the price of adult jornalero labor without decreasing its quantity, 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

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 labor reported in Table 9 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.

labor supply of adults through an income effect then this reduction was outweighed by the increase in demand for adult labor, because the quantity of adult labor probably increased. Likewise, the increase in demand for adult labor must have outweighed any increases in adult labor supply, because adult wages increased.

In sum, by November 1999, comparison of treatment and control villages shows a significant decrease in the supply of children to farm labor, accompanied by a significant increase in the price of adult farm labor and no significant decrease in the quantity of adult farm 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 their weekly earnings. In the next section, I verify this claim.

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

I explained in Section III that in order to determine whether adults and children are gross-substitutes or gross-complements, it is necessary to ensure that the treatment's only effect on the demand for adult jornalero labor was through the decrease in child labor supply. There are four alternative pathways to consider. One is that the treatment families spent their money in a way that would increase the demand for output, thus increasing the derived demand for the jornaleros' labor. The second is that the treatment caused a change in the supply of other factors of production, which in turn caused an increase in the demand for adult jornalero labor. These first two alternative pathways were considered theoretically in Section III. The third alternative pathway is that the *direct treatment benefits* in income, nutritional consumption, or medical consumption lead to improved health, thus leading to better productivity and hence to better adult wages. The fourth is that the *indirect treatment benefits* (e.g. *spillovers* in income, nutritional consumption, or medical consumption) lead to improved health, leading to better productivity and hence to better adult wages. I rule-out each of these four alternative pathways below.

(A) Ruling-out an increase in derived demand

It is essential to observe constant price and quantity of output in order to rule-out treatment effects on the demand for output (as further explained in Section III(E)). If I can observe such constancy, then this will rule-out the first alternative explanation.

I use two different measures of quantity of output: the probability of a household bringing in a harvest (i.e., the number of working farms), and the average size of the harvest. Table 11 reports the treatment effect on an indicator for bringing in a non-zero harvest, as well as the treatment effect on the number of tons of corn harvested (I report post-treatment first-differences rather than difference-in-differences because there was no pre-treatment data on harvest-size). These both demonstrate no statistically significant treatment effect on the quantity of production.[23]

[23] In the October 1998 data, I dropped 536 observations of harvest size 98.8 tons that appeared to have been intended to be listed as "do not know" and thus should have been coded as missing. The marginal treatment effects from tobit regressions of the expected number of tons harvested unconditional on a positive harvest are also statistically insignificant for both October 1998 and May 1999. There is also data on the number of tons of corn sold for both October 1998 and May 1999 (families do not necessarily sell all of the corn that they harvest). Specifications analogous to those in Table 11 with tons of corn sold as the dependent variable obtain similarly statistically insignificant results.

Table 11. Treatment Effects on the Corn Harvest in October 1998 and May 1999

Independent Variables:	(1) Indicator for a positive Harvest in October 1998 (probit)	(2) Log # Tons Corn Harvested during the October 1998 Harvest (OLS)	(3) Indicator for a positive Harvest in May 1999 (probit)	(4) Log # Tons Corn Harvested during May 1999 Harvest (OLS)
	Dependent Variables			
Treatment Village Dummy	-0.00 (0.02)	-0.05 (0.10)	-0.00 (0.02)	-0.01 (0.08)
(Treatment Percentage Change)	-1.0% insignificant	-5.0% insignificant	-1.0% insignificant	-1.0% insignificant
Constant		0.26*** (0.08)		-0.11 (0.07)
# Obs	23143	7172	21961	6713
R2	0.00	0.00	0.00	0.00

Standard Errors, corrected for village-level clustering, are in Parenthesis, * = significant at 10%** = significant at 5%, *** = significant at 1%

Given the agricultural 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. There is existing work on prices using the surveys of village leaders; but not every locality reports prices, and Handa, Huerta, Perez, and Straffon (2000) do not have information on corn itself (only on corn paste and corn tortillas). What their work does show is that the 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 November 1999 post-treatment survey used in this paper.

Furthermore, I divided the revenues that corn-producing farmers gained from their crops in October 1998 and in May 1999 by the size of their harvests to obtain the average price which each farmer received per ton of corn sold. Regressions of these prices on an indicator for a treatment village showed no treatment effect to the mean price per ton of corn (even with variation in the degree of cropping of outliers). Likewise, kolmogorov-smirnov tests showed that the treatment price distribution was not significantly likely to first-order stochastically dominate the control price distribution.[24]

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.

Since there were no significant treatment effects on the quantity or price of output, there is no evidence for an increase in the demand for output. If the demand for output remained constant, then it is not possible that the treatment money caused an increase in the local derived demand for adult labor, i.e. the demand for adult labor derived from the demand for output. This

[24] In the May 1999 data, there were significant treatment effects to the seventh and ninth deciles, but these effects were unable to influence the mean or the kolmogorov-smirnov test. In the October 1998 data, there were opposite and significant treatment effects to the median and higher quantiles, averaging out to no significant treatment effect on the mean, and no significant difference in the kolmogorov-smirnov test.

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.[25]

(B) Ruling-out a change in the supply of non-labor inputs

It is useful to observe constant price and quantity of other factors of production in order to rule-out that changes in the supply of other factors of production caused the farms to change their demand for adult labor (as further explained in Sections III(C) and III(D)). If I can observe such constancy, then this will rule-out the second alternative explanation.

First, I consider land. I consider two measures of the quantity of land used in production: total hectares of land used for any purpose, and total hectares of land used for agricultural purposes. Table 12 shows that there was no treatment effect on the number of hectares of land used for either purpose in the treatment villages; the point estimates were less than one percent and were insignificant.

[25] 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.

Table 12. Treatment Effects on Hectares used or owned, Total and Agricultural

	Dependent Variables		
Independent Variables:	(1) Total Hectares in October 1998 versus Total Hectares in November1997	(2)Agricultural Hectares in October 1998 versus Agricultural Hectares in November1997	(3) Total Hectares in November 1999 versus Total Hectares in November 1997
Diff-in-Diff	0.02 (0.08)	0.05 (0.06)	0.02 (0.11)
(Treatment Percentage Change)	+0.8% insignificant	+2.6% insignificant	+0.7% insignificant
Post-treatment	-0.56*** (0.06)	-0.38*** (0.05)	-0.65*** (0.09)
Village Fixed Effects	YES	YES	YES
Constant	2.26*** (0.03)	1.91*** (0.02)	2.27*** (0.03)
# Obs	47826	47595	47035
R2	0.00	0.00	0.01

Standard Errors, corrected for village-level clustering, are in parenthesis, * = significant at 10%, ** = 5%, *** = 1%. The unit of observation is an individual household.

While lacking data on land prices, I have a limited number of households that report income earned from renting land (157 observations from the October 1997 survey, and 53 observations from the November 1999 survey). I report the difference-in-differences treatment effects on the deciles of rental income in Table 13. The results suggest a treatment-related decline in rental income. If I hold the total amount of land rented constant (which is consistent with, though not implied by, Table 12), then the decline in rental income implies a decline in land prices. However, I note that the statistical significance of the decline in rental income measured on this very small data set is fragile.[26] Thus, either there is no evidence for a statistically significant change in land prices, or there is evidence of a statistically-significant decrease.

Table 13. Treatment Effects on Land Rental Income, in pesos per day

	1997 vs. 1999
10th Percentile	-1.84**
	(0.83)
20th Percentile	-1.78*
	(1.02)
30th Percentile	-3.99**
	(1.70)
40th Percentile	-4.12***
	(1.81)
50th Percentile	-9.80***
	(2.81)
60th Percentile	-14.37***
	(4.22)
70th Percentile	-14.89***
	(5.24)
80th Percentile	-6.05
	(10.79)
90th Percentile	19.05
	(29.62)
# Obs	210

Standard Errors are in parenthesis. The median daily rental income was 5.5 pesos per day, and the mean was 15 pesos per day.

If the supply of land is strictly upward-sloping and the demand for land is strictly-downward-sloping, then it is possible that a constant quantity of land and a decrease in the price of land could together occur through a decline in both the supply of land and the demand for land simultaneously. This would seem to be a problem for identification, because a decline in the

[26] The results in Table 13 are confirmed by kolmogorov smirnov tests of first order stochastic dominance. But the statistical significance of the decline is only confirmed by OLS mean regressions in the case of significant cropping of the tails of the rental income distribution. Likewise, the statistical significance of the decline is generally unconfirmed by regressions in which the log of daily rental income is the dependent variable as opposed to the level. Thus, this is the most fragile result reported in this paper. Given the very small number of observations, to seek certainty of the sign of the treatment effects on land prices is to ask too much of the data.

supply of land could have independently affected the demand for adult labor. However, this is unlikely for two reasons.

First, the supply of land is likely to be inelastic – and since the agricultural industry as a whole is being analyzed in this paper, it is likely that the supply of land to this industry is perfectly inelastic. This would mean that constant quantity of land in use (as suggested by Table 12) is inconsistent with a decline in the supply of land. Second, even if the supply of land is not perfectly inelastic, land is almost certainly a complement to adult labor. A decline in the supply of land could thus not be a reasonable alternative explanation for the increase in the demand for adult labor. For both of these reasons, I conclude that a decline in land prices is consistent with my overall argument that the increase in the demand for adult labor was caused by employers substituting adults for children. In particular, the decline in land prices is not consistent with a change in the supply of land being an alternative explanation of the increase in adult labor demand. One loose end is an explanation for why the price of land may have declined at all: this can be tied-up by pointing out that the farm's zero-profit condition, as explained in Section III, implies that the demand for some third factor must decline when the prices of child and adult labor both increase.

Next, I consider other non-labor inputs. In the May 1999 survey, respondents are asked about the total amount of money spent in the previous six months on seeds, fertilizers, pesticides, machinery, and yoke labor. I regressed on a treatment village indicator the following outcomes: an indicator for spending any money on non-labor inputs (a probit regression); the natural logarithm of the total amount of money spent conditional on spending any money at all (an OLS regression); and the unconditional total amount of money spent (a tobit regression, censored below at zero). I report the results in Table 14. All the point estimates of the treatment effects indicate percentage changes of less than two percent, and none of these changes are significant.[27]

Table 14. Treatment Effects on Expenditures on Non-labor Agricultural Inputs from December 1998 through May 1999: seeds, fertilizers, pesticides, machinery, and yoke labor.

	Dependent Variables		
Independent Variables:	(1) Indicator for Positive Expenditures (Probit)	(2) log of total expenditures, conditional on expenditures > 0 (OLS)	(3) total expenditures, unconditional (Tobit)
Treatment Village Dummy	0.006 (0.023)	-0.019 (0.067)	-2.97 (23.55)
(Treatment Percentage Change)	+1.5% insignificant	-1.9% insignificant	-1.0% Insignificant
Constant		6.10 (0.053)	
# Obs	22139	7668	20508
R2	0.00	0.00	0.00

Standard errors, corrected for village-level clustering, are in parentheses. The probit coefficient in column 1 is the marginal effect of the treatment village indicator on the probability of expenditures being positive. The tobit coefficient in column 3 is the marginal effect of the treatment village indicator on the unconditional expected value of total expenditures. Percentage changes in columns 1 and 3 are calculated by dividing the marginal effects by the control group mean.
* = significant at 10%, ** = at 5% *** = at 1%

(C) Ruling-out direct health benefits as the only cause of increased wages

The third alternative hypothesis is that the wage increase arose when eligible families in treatment villages spent their treatment money in a way that increased nutrition, in turn leading

[27] I crop the top one percent of expenditures in each case – the results are largely the same without cropping. In the first post-treatment October 1998 survey, respondents are asked about the total amount of money spent in the previous year on non-labor inputs such as seeds, fertilizers, insecticides, machinery, and yoke labor. This year covers about six months of pre-treatment decisions, yet the results are similar to those in Table 14. It is interesting to note that other questions on animal purchases show that 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 farm work) (Angelucci and De Girogi, 2005). In any case, there was no statistically significant change in the total expenditures on all non-labor inputs combined, as reported in Table 14.

to improved health and productivity. But under this alternative hypothesis, ineligible families in treatment villages would not receive higher wages. 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. If these ineligible families living in treatment villages experienced wage increases, then this suggests that health benefits from direct reception of the treatment money are not necessary for receiving higher wages; the only necessity is living around children who left the workforce.

Thus, I estimate the same wage regression on a smaller restricted sample of 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 A2 and A3. 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).

(D) Ruling-out indirect health benefits (spillovers) as the only cause of increased wages

Finally, the above robustness check must itself face a robustness check in the form of the fourth alternative explanation: 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 better health which in turn leads 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.[28] 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.

(E) Other Robustness Issues

One potential cause for concern is a connection between the labor market for farm 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 farms; it would affect the supply of labor to the farms. And all stories that involve changes in the supply of adult labor to the farms 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 any 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 *attenuate* the program's effects on both the supply of child labor and the demand for adult labor. Thus, when I find significant program

[28] 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.

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.

Finally, a legitimate concern is that the householders' own labor supply to their own farms (or their own families' farms) may have declined due to the treatment. Such a decline could explain the increase in the demand for other adults' labor without children and adults being substitutes. But further tests show that the treatment effect on the probability of reporting self-employment as one's primary job is insignificant and of small magnitude between 1997 and 1999.[29] Thus, this alternative explanation is unlikely to be problematic for identification.

I therefore conclude that by 1999, a reduction in the supply of children to jornalero farm work in the treatment villages had a positive and significant hourly wage effect on adult farm workers, which in turn increased adult hours worked per week (conditional on working).[30] This result occurs without any changes in expenditures on non-labor inputs, land usage, food prices or harvest size. 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 to a subsample of that which includes only perfectly healthy adults. Thus, 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.

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[29] Likewise, regressions of the probability of working for your family without payment (which could represent working in your own household's farms) only show a statistically significant decline for people living in households that do not own or use any land.

[30] 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, 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).

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 whether adults complement children or substitute for them, as demonstrated theoretically by Kaushik Basu and Pham Van (Basu and Van 1998) (Basu 2000).[31] The authors set up a simple and plausible model in which restricting the possibility of children working can actually improve household welfare. Their two main assumptions are as follows. First, *the Luxury Axiom*: parents only send their children to work when not doing so would cause the family to fall below some subsistence level. Second, *the Substitution Axiom*: the production technology is a function of a linear aggregate of child and adult labor (hence, children and adults are substitutes in at least one sense of the word). The first assumption suggests a household labor supply curve that is capable of leading to two intersections with the labor demand curve, and the second assumption allows for one demand curve for effective household labor. Thus, there may be multiple equilibria, and depending on household utility, one equilibrium may involve higher welfare for the labor-supplying households than another.

Analyzing a specific example, the authors conclude: "There are at least two potential equilibria. Suppose an economy is caught in the bad equilibrium. . . Then a total ban on child labor could deflect the equilibrium all the way to the good equilibrium. . . Hence, all working-class households would be better off. And the policy would be self-liquidating in the sense that once in place it plays no role and constrains no one's behavior."

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

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[31] For example, substitutability allows for the possibility of multiple equilibria in Basu and Van's model, in which case 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).

Mexico, the Substitution Axiom seems to hold. Furthermore, the fact that these results are both from agricultural settings is useful. As Udry (2006) 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), Edmonds (2003), and Udry (2006), 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 laws and better schools, and in which programs used to escape the poverty trap could be "self-liquidating" in the sense that Basu and Van describe above.

However, 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. For instance, 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. Alternatively, 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 hourly wages is clearly positive.

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 mainly require funds for better schools, better enforcement of labor laws, and better transfers *within* communities – that 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.[32] 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.

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[32] 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.

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.[1]

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.[2] 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 A1 shows baseline (1997) summary statistics for the two samples.

Table A2 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 A3 shows the results of my quantity specifications on this sample – they suggest that the quantity of adult jornalero labor in this sample also increased.

[1] 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.

[2] 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 A1: Comparison of baseline characteristics of no-treatment sample in treatment villages with comparison sample in control villages.

Year	Variable	Control Villages		Treatment Villages	
		Mean	Std. Dev.	Mean	Std. Dev.
	total # families	4,276 families		5,530 families	
	total # people	15,874 people		24,453 people	
	% male	50.9%	(0.50)	51.4%	(0.50)
	% child (< 17 years)	34.5%	(0.48)	33.2%	(0.47)
	% adult (17 to 59 years)	53.9%	(0.50)	54.7%	(0.50)
	% worked last week	46.7%	(0.50)	48.5%	(0.50)
	% worked as jornalero	19.3%	(0.39)	19.5%	(0.40)
1997	Mean jornalero wage	3.69 pesos / hour	(3.97)	3.81 pesos / hour	(4.30)
	Mean age	29.3 years	(21.2)	30.0 years	(21.4)
	% with high schooling	18.5%	(0.39)	19.5%	(0.40)
	% speaking a dialect	19.3%	(0.39)	22.4%	(0.42)
	% literate	79.3%	(0.41)	78.2%	(0.41)
	% married	39.1%	(0.49)	40.5%	(0.49)
	% separated	1.7%	(0.13)	1.9%	(0.14)
	% divorced	0.2%	(0.04)	0.2%	(0.04)
	% widowed	5.4%	(0.23)	5.6%	(0.23)

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 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-in-differences regression on wages. The results show that point estimates of the treatment effect are essentially unchanged, and remain statistically significant.

Table A2: 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
Diff-in-Diff (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 A3: 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
Diff-in-Diff (post = 1 & treatment village = 1)	0.032**	0.036**	0.028**	0.035**
	(0.015)	(0.016)	(0.014)	(0.014)
Post-treatment Dummy	-0.085***	-0.087***	-0.065***	-0.068***
	(0.012)	(0.012)	(0.011)	(0.011)
Male Dummy	0.112***	0.117***	0.079***	0.081***
	(0.016)	(0.016)	(0.015)	(0.015)
Age	-0.001***		-0.001**	
	(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	3.64***	3.59***	1.58***	1.56***
	(0.019)	(0.034)	(0.017)	(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