

How Does Child Labor Affect the Demand for Adult Labor? Evidence from Rural Mexico

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Abstract

Do employers substitute adults for children, or do they treat them as complements? Using data from a Mexican schooling experiment, I find that decreasing child farm work is accompanied by increasing adult labor demand. This increase was not caused by treatment money reaching farm employers: there were no significant increases in harvest prices and quantities, non-labor inputs, or non-farm labor supply. Furthermore, coordinated movements in price and quantity can distinguish this increase in demand from changes in supply induced by the treatment's income effects. Thus, declining child supply caused increasing adult demand: 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, but the policy implications are far-reaching. According to the International Labor Organization (International Labor Office 2010), there are 215 million child laborers worldwide. The design of government interventions to reduce child labor and encourage education depends crucially on whether or not children and adults are labor substitutes. If employers substitute adults for children, then a decrease in child labor will lead to an increase in adult wages and hours, partially offsetting the short-term welfare loss that families face when some of their children are no longer working (Basu and Van 1998). If, however, adults and children are complements, then interventions that reduce child labor (such as discouraging the purchase of products produced by child labor) will reduce the demand for adult labor and thus reduce working households' welfare. In this situation, interventions may need to be coupled with government transfers to compensate families for the drop in welfare.

Many of the policy proposals designed to reduce child labor assume that child and adult labor are substitutes. For example, the Child Labor Deterrence Act introduced in the United States in 1999 argues: "The employment of children under the age of 15 . . . ignores the importance of increasing jobs, aggregated demand, and purchasing power among adults as a catalyst to the development of internal markets and the achievement of broadbased, self-reliant economic development in many developing countries." Likewise, the International Labor Organization's book "Combating Child Labour," claims that "...child labour is a cause of, and may even contribute to, adult unemployment and low wages ..." (Bekele and Boyden 1988).

Despite these claims, the evidence that removing children from the workforce improves adult labor market outcomes is sparse and contradictory, a point first noted by Galli (2001) in her

review of the literature. In his *Handbook of Development Economics* chapter, Edmonds (2008) notes that whether child labor depresses adult wages “is a critical question in the child labor literature,” but that despite the critical nature of the question, “direct evidence on whether child labor affects adult labor markets is scarce.”¹ In this paper, I address this empirical gap.

The reason for this gap is no surprise. Consider a simple production function with two inputs, A and B. The prices for the inputs are jointly determined, but an exogenous shock to the supply curve for input A will produce a corresponding change in the demand for B. The subsequent change in market conditions for input B will indicate whether the goods are complements or substitutes. But when we apply the model to a setting with child labor, we face a further complication. The two production inputs I consider (adult and child labor) come from the same household and as a result, any program that changes child labor supply will almost certainly affect adult labor supply as well. Therefore, any test must allow for the possibility that the adult labor demand and supply curves are moving at the same time. My strategy, as developed in Section II, can identify changes in adult demand without assuming that adult supply has remained constant, by analyzing coordinated movements in price and quantity. As I outline below, given an *exogenous* reduction in child labor supply (that is, one without alternative causal pathways), if adult wages and employment both increase (decrease) then adult and child labor must be substitutes (complements). However, if adult wages and employment move in opposite directions, the joint co-movement in adult supply and demand curves makes it impossible to determine the sign of the parameter of interest.

I apply this strategy using data from Mexico's PROGRESA program, a conditional cash transfer experiment that produced large reductions in child farm work participation, as well as large unconditional increases in income for eligible families not on the relevant decision margins

(Section IV). I then document a subsequent increase in both the quantity and wages of adult work, indicating that employers substituted adults for children (Section V), regardless of what happened to adult labor supply due to the unconditional income effects of the program. This increase in adult labor demand was not directly caused by alternative causal pathways: most importantly, there were no significant treatment effects on the demand for the output of production, or on the supply of other inputs to production (Section VII). Furthermore, the wages of healthy non-treated adults living around children who stopped working also increased, suggesting that neither treatment-related health and nutrition increases nor health and nutrition spillovers were responsible for the increase in demand for adult labor (Section VII).

II. Conceptual Framework and Identification Strategy

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:

$$(1) \quad Y = F(X^1, \dots, X^i, \dots, X^K)$$

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.

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

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

The Lagrangian associated with this problem is:

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

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

$$(4) \quad \frac{\partial L}{\partial X^i} = w^i + \lambda F_i = 0, \quad \forall i = 1, \dots, K$$

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. The first order conditions and the binding production constraint produce the conditional factor demands: \bar{X}^i . The cost function is defined as the minimum value of the total cost given output Y and factor prices, or $C(w^1, \dots, w^K, Y)$.

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

Lemma:

$$(5) \quad \bar{X}_i = C_i(w^1, \dots, w^K, Y), \quad \forall i = 1, \dots, K$$

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:

$$(6) \quad \frac{\partial X^1}{\partial w^2} = C_{1,2} + C_{1,Y} \frac{\partial Y}{\partial w^2} + \sum_{i=3}^K (C_{1,i} \frac{\partial w^i}{\partial w^2})$$

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

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

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. In either case, 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.

(A) 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 X^2 and w^2 and a response to this change in the function X^1 .

Identifying changes in the unconditional demand for adult labor can be challenging. 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. The identification strategy is graphically depicted in Figure 1, where I present the market for adult labor. Within Figure 1, the hollow circles represent the original price/quantity combination while the solid circles represent the new equilibrium after the introduction of the program that reduces child labor supply. The strategy can best be described by considering the top row of figures in Figure 1. In this case, note that both wages and quantity of adult labor increase after children leave the workforce. The second figure in this row indicates one possible scenario: the demand for adult labor increases, indicating that adult and child labor are substitutes. This simple scenario ignores the possibility of a shift in adult labor supply. However, as the third and fourth figures in this row indicate, regardless of whether the

supply of adult labor increases or decreases, a simultaneous increase in wages and quantities means that adult labor demand must have increased, and thus adult labor and child labor are substitutes.³

A similar story is outlined in the second row of figures in Figure 1. If the wages and employment of adults fall when children leave the workforce, then it must be the case that adult and child labor are complements. In this case, regardless of the movement in labor supply, the demand curve must have shifted in. The story is not definitive, however, if wages and quantities move in opposite directions. Consider the third row of figures. In this case, a decline in the supply of child labor increases adult wages but reduces the quantity of adult labor. The figures in this row show that this change in equilibrium could be generated either by an increase in supply and a drop in demand (the third graph in the row) or a drop in supply and an increase in the demand for adult labor. Therefore, if wages and quantities move in opposite directions, we cannot say definitively without further data whether adult and child labor are complements or substitutes.

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.

(B) Accounting for 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, $\frac{\partial X^1}{\partial w^2}$ could be negative or positive without affecting the sign of the observed change in X^1 , if the magnitude of $\frac{\partial X^1}{\partial w^3}$ 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.

(C) 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:

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

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, in Section VII(B).

(D) 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 $\frac{\partial X^1}{\partial w^2}$ does not determine the sign of $\frac{\partial Y}{\partial w^2}$. 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. This implies, of course, that general equilibrium effects on the supply of output are ambiguous.

However, exogenous changes in the demand for output can obscure 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.

(E) Summary

I summarize the identification strategy as follows: First, I must observe a decrease in the supply of child labor 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 key “third” factors of production (allowing for the probability of a simultaneous decrease in price and quantity of at least one other factor of production).

III. 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.⁴

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 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, and 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; many

eligible families not on the relevant decision margins experienced large unconditional increases in their income.⁵ 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 farm worker was about 600 pesos, and that of a child farm worker 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 Oportunidades 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 in south central Mexico.

When locals in each village were asked about their village's principal activity and principal crop, 97.8 percent said agriculture and 88.2 percent said corn. The primary corn harvest in Mexico lasts from October through December (USDA), 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.

Table 1 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, 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).

Table 2 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.

I classify people who are ages 16 and under as children and people ages 17 to 59 as adults (I have also re-run all the subsequent results with sixteen year-olds defined as adults, and none of them change appreciably). *Children are a substantial portion of the farm workforce:* in 1997, children made up 9 percent of the total farm workforce, while adults made up an additional 80 percent, and this holds with weighting by hours worked per week. 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 0.0003 pesos per hour to 7,506 pesos per hour. With bounds this 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 I do run mean regressions as well, attempting to eliminate the bias caused by the incorrect measurements at the top and the bottom of the distribution by dropping observations with wages in the top and bottom five percent for each of the six comparison groups (control vs. treatment, 1997 vs. 1998 vs. 1999).⁶

The setting described by this data is highly policy relevant, since corn production in Mexico in the late 1990s is quite representative of what we know about the modal child worker's occupation. Regarding occupations, Edmonds (2008) reports in the Handbook of Development Economics that "in almost every listed country, a majority of economically active children are involved with agriculture, forestry, or fishing industries." Edmonds explains further that: "Children involved in agriculture and related industries are involved in the growing of cereals, vegetables, poultry farming, and inland fishing. Cereal cultivation is the largest single sector with 39 percent of all economically active children directly involved." Edmonds writes as well:

“Information at the 3 digit level is available in the Bangladesh child labor force survey, . . . 46 percent of children 5-17 are farm crop workers. The next largest occupations are salesmen and shop assistants (7 percent), poultry farmers (5 percent), sales supervisors (4 percent), fisherman (3 percent), and non-motorized road vehicle drivers (3 percent).”

Finally, farm households in south-central Mexico in the late 1990s had access to machinery that would help in production, harvesting, and transport of corn. In PROGRESA’s October 1998 household survey, about 15 percent of the land was controlled by households that owned one or more of the following types of equipment: a truck or van, a tractor, a thresher, a sprayer or pump, or a windmill. This is a larger percentage than the percentage of households whose children worked for pay in the fields. Furthermore, this percentage vastly underestimates the percentage of smaller households that rent the richer households’ threshers and other equipment for the duration of their harvest, as is common in areas with many small farms. Thus, this is a setting in which there is sufficient capital to switch technology and introduce less labor-intensive means of production.

IV. 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 farm workforce. But by 1999, 18 months after the program started, the treatment has clearly caused a decline in child participation in the farm workforce as well as an increase in the wages of child farm workers. These results are demonstrated in the difference-in-difference estimates of the treatment effect described below.

My primary empirical strategy is to estimate reduced form equations of the treatment effects on labor market outcomes such as work participation, hourly wages, etc. The unit of observation is an individual at a point in time. I use a difference-in-difference approach to address the small ways in which treatment and control villages differed before the treatment even started as well as to control for persistent sources of regional variation (Schultz 2004). I also control for individual-level characteristics and cluster the standard errors at the village level in most specifications.

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

$$(8) \quad Y_{it} = (\alpha \cdot P_t \cdot E_i) + (\beta \cdot P_t) + (\gamma \cdot E_i) + (\delta \cdot X_{it}) + \varepsilon_{it}$$

Where P_t is an indicator for post-treatment, E_i is an indicator for a treatment village, and X_{it} is a vector of personal characteristics; i indexes people (or households, depending on the case), and t indexes time.

I include in X_{it} dummies for gender, age, schooling, language abilities and marriage status (where these are age and specification appropriate).⁷ I run this specification separately for the 1997 vs. 1998 comparison and the 1997 vs. 1999 comparison. When the dependent variable was not recorded in the pre-treatment survey, then I cannot include the P_t dummy (because it is always 1) or the $P_t \cdot E_i$ dummy (because it always equals the P_t dummy). Thus, the coefficient on the E_i dummy becomes my estimate of the treatment effect, and my estimation equation becomes:

$$(9) \quad Y_{it} = (\gamma \cdot E_i) + (\delta \cdot X_i) + \varepsilon_i$$

where i indexes people (or households) and t indexes time.

In specifications based on equation (9), I cannot control for village-level fixed effects, because that would require me to omit the TreatmentVillage dummy.

I add to the previous studies of this experiment (such as (Schultz 2004) and (Skoufias and Parker 2001)) that estimated significant decreases in work participation for children, by estimating specifically the treatment effect on child participation in the farm workforce. I create a dependent variable dummy for working on a farm by assigning the dummy the value 1 if the person worked on a farm for pay in the last week and 0 if they did not work or worked in a different job category. I regress the dummy for working on a farm on my independent variables as outlined in Equation 1. Table 3 reports the results of probit specifications of this regression model. I find that by 1998, there was no significant effect on child paid farm work participation. However, by 1999, paid child farm work participation saw a significant decrease of four percent due to the treatment.⁸ When I extend my definition of child labor to a broader measure of work, I find an even larger effect of 12 percent.⁹

I include both eligible and ineligible children in the specification because I am interested in the program effects on overall child wages and quantities (note that the ineligible children in treatment villages likely increased their labor supply in response to the wage increase, so the treatment's impact on eligible children's labor supply is likely larger than what I report in this specification).

If the supply schedule of child farm labor slopes upward, then one can always tell that the supply schedule has shifted backward (decreased) when a decrease in the quantity of child farm labor is accompanied by an increase in the price of child farm labor. Thus, in Table 4, 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 a large increase in mean child hourly wages, and that this increase is statistically significant. This wage increase necessarily implies a decrease in the supply of child farm workers. Even if the magnitude of the wage increase is

inflated because of selection (as one would expect given the large magnitude), selection of low productivity children out of the workforce *presupposes a decline in the supply of child workers* (whose quantity effect is seen in Table 3). Thus, nothing other than a decline in child labor supply succinctly explains the participation results in Table 3 coupled with the wage results in Table 4.

V. 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 farm workforce by 1999, I need to check whether the demand for adult labor increased by 1999 as well.¹⁰ If the treatment increased the price of adult farm labor without decreasing its quantity (or vice versa), then this implied that it increased the demand for the labor of adult farm workers.

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 test clearly shows 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 2, which plots the cumulative distribution functions of the hourly wages of adult farm workers in 1997 and in 1999. The wage distribution is too lumpy for all deciles to increase, but the quantile regressions by decile reported in Table 5 show that three deciles experienced large and significant increases (two below the median and one above) and none decreased significantly.

It is clear that by 1999 the hourly wages of adult farm workers have increased due to the treatment. Furthermore, the adult wage increase appears to be real, not only nominal: the study by Handa et al. (2000) concludes that the treatment did not produce food price inflation in the treated villages. I further consider the treatment's effect on mean wages by estimating OLS regressions on log hourly wages and log daily income according to equation (8) (with the effect of the tails diminished via the cropping discussed in Section III), reporting the results in Table 6, specifications 1 and 2. There is an increase in mean adult farm wages of about three percent. I also estimate treatment effects on mean work outcomes for adults. From Table 6, specifications 3 and 4, it is clear that the treatment increased both adult hours worked per week and adult days worked per week conditional on working. Finally, Table 6, specification 5 shows that the treatment did not decrease the probability of adult participation in the farm workforce. The first four specifications in Table 6 have standard errors that are sensitive to the degree of cropping of the tails of the wage distribution, as well as to controls for village-level fixed effects.¹¹

It is instructive to consider how the program may have had different labor supply effects for different groups of adults, and what the implications of this would be for identification of a labor demand shift. Specifically, some low productivity adults may have decreased their labor supply while other adults may have increased their labor supply. In general, if there are two groups within the adult labor pool, and if the high productivity group faces a very elastic demand curve, and if the high productivity group increases its labor supply by more than the low productivity group, then it is definitely the case that the quantity of labor would increase. And if this quantity increase happened at the extensive margin, then the average wage would increase as well (if it didn't happen at the extensive margin, then the fact that I am estimating wages in a "one person, one observation" setting means that average wages would not go up). This simultaneous increase in price and quantity could therefore happen where labor demand has stayed constant, as long as each of the assumptions above is satisfied.

This alternative hypothesis affects mean wages, due to a composition effect on the extensive margin. My decile regressions are therefore useful here. Under the scenario above, the wage increase is entirely an artifact of a changing labor pool in which high productivity people have out-numbered low productivity people. This should increase the levels of each wage percentile at all percentiles below the one in which we have added workers. Importantly, though, this should decrease the levels of each wage percentile at all percentiles above the one in which we have added workers. But the quantile difference-in-differences regressions in Table 5 show that the 80th percentile increased substantially (as well as the 20th and the 30th), and that none decreased significantly, including the 90th percentile. Thus, increasing the relative number of high productivity people can't explain the observable changes in the full wage distribution from top to bottom. Furthermore, the assumption that all demands for labor are strictly

downward-sloping implies that increasing the labor supply of high productivity people should substantially decrease the levels at the highest wage percentiles, meaning that this alternative hypothesis disagrees significantly with the observable changes in the empirical wage distribution.

There is another empirical implication of this composition bias story: in order for changes in composition of the workforce to have affected mean wages, the participation rates of highly productive adults must have increased more than the participation rates of adults with less than mean productivity. To test this implication, I run additional regressions on participation of prime-age adults in farm day labor by schooling category. Breaking the schooling variable into two categories of roughly equal size (low and high), I observe that the wages of the low category are less than the mean wages overall, and the wages of the high category are greater than the mean wages overall. Thus, schooling is a useful proxy for productivity. Running regressions of farm work on the extensive margin, I find that the low productivity group had a large and significant 7 percent increase in participation rates in farm labor, while the high productivity group had a much smaller and statistically insignificant change of less than 1 percent. Thus, there is no evidence of increased relative work in the extensive margin for high skilled workers. In fact, any composition bias is pushing downward my observed wage increase from what it would have been without a composition change.

Taken together, both the effects on the wage distribution and the participation results suggest that composition bias is not at work in this wage increase. Rather, the combined increase in price and quantity of adult farm labor implies that the demand for adult labor must have increased.

VI. Comparison of the Size of the Effects

Table 3 shows that the reduction in the number of child farm workers was anywhere from - 4 percent (for the most restrictive definition of child farm workers) to - 12 percent (for the broadest definition of child farm workers). *Children made up 9 percent of the paid farm workforce, and 12 percent of the broadest definition of farm workers.* Thus, this reduction in the number of *child* farm workers amounted to a reduction in the *total* number of farm workers of anywhere from -0.3 percent (using the most restrictive definition of farm workers) to -1.4 percent (using the broadest definition).

It is reasonable that a -1.4 percent decrease in the total quantity of labor could cause a + 3 percent increase in the price of labor among at least one set of substitutable workers. It is also reasonable that when 1.4 percent of the workforce stops working, some remaining workers will have to work 3 percent more hours per week (as reported in Table 6).

Finally, the total number of hours of farm work lost due to decreasing child farm work participation has a similar order of magnitude to the total number of hours of farm work added by adult farm workers. Child farm workers worked an average of 43 hours per week in the pre-treatment data. Thus, a 12 percent reduction in the number of child farm workers could account for as much as 5.2 fewer farm hours per week per child. A 3.3 percent increase in the hours of adult farm work per week (Table 6) accounts for 1.4 more farm hours per week per adult. Rescaling by the larger number of adult farm workers, this amounts to 11.3 additional adult hours per week per child farm worker. Accounting for the large standard errors in Table 6, this is a number well within the range of 5.2 fewer child farm hours per week per child farm worker.¹²

At the end of the next section I show that some of the impact on adult workers is due to health and nutrition benefits. In particular, I show that the impacts that remain when I account for health and nutrition benefits are still positive, though likely of a smaller magnitude that is more similar to the magnitude of the reduction in overall labor supply caused by the child labor supply shock.

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

I explained in Section II 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 farm labor was through the decrease in child labor supply; or, rather, that there is still an effect on the demand for adult farm labor even when all non-child pathways have been accounted for. 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 farm workers' 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 farm labor (these first two alternative pathways were considered theoretically in Section II). The third alternative pathway is that the *direct treatment benefits* in income, nutritional consumption, or medical consumption lead to improved health for those who received them, thus leading to better productivity and hence to better adult wages.¹³ The fourth is that the *indirect treatment benefits* (for example *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 as the sole causes of the increase in adult demand below.

(A) Ruling-out an increase in derived demand

The program has many components which could plausibly affect non-farm markets. Any of these effects on non-farm markets could circle back to the farm labor market through changes in derived demand: that is, a change in the demand for the products produced by farm labor. For example, the income transfer aspects of the program are likely to directly increase the consumption of local families; some of this increase may be spent on local farm products. Likewise, the program should directly increase the demand for schooling and services related to the provision of schooling. This could increase the demand for construction labor, as well as the demand for meal provision and transport, in turn increasing the consumption of local farm products. The component to provide health care to treatment villages may have led to increased hiring of local labor as well, with a subsequent effect on the demand for local farm products.

The first thing to note is that an increase in the demand for adult labor in non-farm industries cannot directly affect the demand for adult labor in farms. Rather, what the increase in demand for adult labor in non-farm industries directly affects is the supply of adult labor to farms. The way in which an increase in demand for adult labor in non-farm industries affects the demand for adult labor in farms is indirectly, through increased purchases of farm products by adults in non-farm industries who now have more income.

Thus, all of the alternative scenarios which fall under the category of a change in derived demand must involve an increase in the demand for farm products. In the absence of such an increase in the demand for farm products, there will be no increase in derived demand for adult farm labor, even if non-farm industries experience treatment effects. In the presence of such an increase in the demand for farm products, there will be an increase in the derived demand for

adult farm labor, regardless of how small the observable treatment effects are in non-farm industries.

Therefore, since there is an increase in derived demand for adult farm labor if and only if there is an increase in the demand for farm products, the empirical question is: is it possible to rule out an increase in the demand for farm products? The answer depends on examining not only the quantity of farm products, but also their price. If the price and quantity of farm products both increased, then the demand for farm products definitely increased. But, if the price and quantity of farm products both stayed constant, then the demand for farm products definitely stayed constant.

In particular, the key point is the following: it is impossible for a combination of supply decreases and demand increases to lead to constant price and quantity of farm products. A combination of supply decreases and demand increases could lead to constant quantity of farm products, but it would also lead to a much higher price of farm products. Thus, the only way to rule out that demand for farm products increased is to observe that neither their price nor their quantity changed due to the treatment (as further explained in Section II(D)).

I test for quantity changes first. I use two different measures of quantity of output: the probability of a household bringing in a harvest (that is, the number of working farms), and the average size of the harvest. Table 7 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.¹⁴

Next, I test for changes in price. Given the agricultural products listed in the location surveys (see Section III), 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 et al. (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 on 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.

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 spend their treatment money locally. Finally, Mexico is integrated into an international corn market (it is frequently a large corn importer); local corn price changes ought to be significantly moderated by competition with the world price.

I close this section with two ancillary points about the likelihood of increases in the derived demand for farm labor. First, I ran regressions of non-farm work hours equivalent to specifications (3) – (4) of Table 6. The treatment effects are negative and significant. I also ran a regression of the probability of being a non-farm paid worker equivalent to specification (5) in Table 6. The treatment effect is negative and insignificant. Thus, there does not appear to be a treatment-induced increase in non-farm work, which helps explain the lack of an increase in local demand for local corn. Second, I note that the treatment-related increases in food consumption reported by Hoddinott, Skoufias, and Washburn (2000) were concentrated on expensive fruits and vegetables and animal products, not on the staple grains which make up the majority of the agricultural products produced in these villages.

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

(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 II(B) and II(C)).

First, I consider the input of 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 8 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.

While lacking direct 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 9. 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 8), then the decline in rental income implies a decline in land prices. While the number of observations is small, there is thus at least marginal evidence of a statistically-significant decrease in the rental price of land.

If the supply of land is strictly upward-sloping (neither perfectly inelastic nor perfectly elastic) 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 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 almost perfectly inelastic. This would mean that constant quantity of land in use (as suggested by Table 8) 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. In fact, the fact that the price of land may have declined makes sense in this setting because of the farm's zero-profit condition, which, as explained in Section II, 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 10. All the point estimates of the treatment effects indicate percentage changes of less than two percent, and none of these changes are significant.

(C) Ruling-out direct health and nutrition 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 their nutrition, in turn leading 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. 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 consider a smaller restricted sample of all people in treatment villages who were not eligible to receive money by 1999 and the equivalent ineligibles from the control villages (whose eligibility was calculated by the same criterion). On this restricted sample, kolmogorov smirnov tests show no significant difference between wages in control and treatment villages in 1997 before the treatment began, but they show that by 1999 the control wage distribution was significantly smaller. Likewise, quantile regressions show that 24 percentiles of the wage distribution experienced positive and significant increases due to the treatment, and only 7 percentiles experienced negative and significant decreases. That the treatment increases the wages on this restricted sample suggests that the results are not dependent on receiving treatment money (for example 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.¹⁵ On this restricted sample, kolmogorov smirnov tests show no statistically significant difference between control and treatment wage distributions in 1997 before the treatment began, but they show that by 1999 the control wage distribution was significantly smaller. Likewise, quantile regressions show that 21 percentiles of the wage distribution experienced positive and significant increases due to the treatment, and only two percentiles experienced negative and significant decreases.¹⁶ 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 explanations that involve changes in the supply of adult labor to the farms are irrelevant to this identification strategy: as I explained in Section II, 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. If migrants responded to the treatment with a positive migration shock towards treated villages, then this would increase labor supply in the treated villages. A change in labor supply

cannot by itself produce an increase in the price and quantity of labor, and thus migration of labor is not an alternative explanation for the increase in the demand for adult labor which I observe. Furthermore, migration of non-farm businesses or commercial entities to the treated villages also cannot explain the increase in demand for adult farm labor: an increase in demand for adult labor in other industries does not directly affect the demand for adult labor in the farm industry; rather, it directly affects the supply of adult labor to the farm industry. Finally, migration of farms themselves is not possible, since they make use of a geographically-fixed asset: land.

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 1999.¹⁷ 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 farm work in the treatment villages caused farm employers to increase their demand for adult labor. This result occurs without any changes in the supply of other inputs or in the demand for output. 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.

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).¹⁸ 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 2005) shows that in the agricultural setting of Vietnam, the Luxury Axiom seems to hold. My results suggest that in this agricultural area of Mexico, the Substitution Axiom seems to hold. Furthermore, the fact that these results are both from agricultural settings is useful. As Udry (2006) points out: “Child labor is overwhelmingly a

rural and agricultural phenomenon. For example, in Pakistan, 70 percent of working children are employed in agriculture.” Thus, together with Basu and Van (1998), Edmonds (2005), 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.

IX. Conclusions

There has been little empirical research examining 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 such an answer. This paper surmounts the two key hurdles: (1) it finds a program that reduces child labor supply without directly affecting adult labor demand, and (2) it identifies changes in adult labor demand without assuming constant adult labor supply.

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 first implications of these results are theoretical. The results of this paper provide empirical support for the assumption that child and adult labor are substitutes which underlie models such as those of Basu and Van (1998), and Ranjan (2001). By providing evidence for Basu and Van’s 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 *some* 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 2005).

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 rather than large injections of cash from *outside* communities to make up for lost child and adult wages.

Finally, this paper provides an *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, such as those described by De Janvry and Sadoulet (2006). 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.

X. References

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Table 1
Pre-treatment (1997) distribution of adults and children across job categories

Year	Job Title	Adults (ages 17 to 59)	Children (ages 8 to 16)
1997	Jornalero (farm worker)	15,675 (28 percent)	1,701 (5 percent)
	Obrero (non-farm worker)	5,320 (9 percent)	642 (2 percent)
	Self-employed	4,472 (8 percent)	317 (1 percent)
	Pattern Work	150 (0 percent)	9 (0 percent)
	Family Work, No Pay	3,428 (6 percent)	1,654 (5 percent)
	Other Work, No Pay	119 (0 percent)	50 (0 percent)
	Member of Cooperative	28 (0 percent)	3 (0 percent)
	Communal Farmer	2,245 (4 percent)	21 (0 percent)
	Other	229 (0 percent)	25 (0 percent)
	NO WORK	24,715 (44 percent)	26,788 (86 percent)
Total	56,558 (100 percent)	31,298 (100 percent)	

Note: 91 percent of adults who report not working are women, many of whom work in the home.

Table 2
Some summary statistics by treatment village status and year

Year	Variable	Control Villages	Treatment Villages
1997	Total # families	9,221 families	14,856 families
	Total # people	48,475 people	77,199 people
	percent male	<i>50.0 percent</i>	<i>50.7 percent</i>
	percent child (< 17 years)	46.8 percent	47.3 percent
	percent adult (17 to 59 years)	45.3 percent	44.8 percent
	percent worked last week	<i>40.0 percent</i>	<i>41.9 percent</i>
	percent paid farm work	15.6 percent	15.2 percent
	Mean farm wage	3.36 pesos / hour	3.38 pesos / hour
	Median adult farm hrs/week	48 hours / week	48 hours / week
	Median child farm hrs/week	48 hours / week	48 hours / week
1998	Total # families	9,919 families	15,927 families
	Total # people	52,299 people	85,141 people
	percent male	50.0 percent	50.6 percent
	percent child (< 17 years)	<i>47.5 percent</i>	<i>48.1 percent</i>
	percent adult (17 to 59 years)	<i>44.7 percent</i>	<i>44.1 percent</i>
	percent worked last week	35.7 percent	36.2 percent
	percent paid farm work	21.4 percent	21.8 percent
	Mean farm wage	4.39 pesos / hour	4.37 pesos / hour
	Median adult farm hrs/week	48 hours / week	48 hours / week
	Median child farm hrs/week	42 hours / week	42 hours / week
1999	Total # families	10,498 families	16,474 families
	Total # people	55,793 people	83,631 people
	percent male	<i>49.6 percent</i>	<i>50.3 percent</i>
	percent child (< 17 years)	45.9 percent	46.3 percent
	percent adult (17 to 59 years)	46.0 percent	45.5 percent
	percent worked last week	35.6 percent	36.0 percent
	percent paid farm work	22.7 percent	22.5 percent
	Mean farm wage	5.1 pesos / hour	5.65 pesos / hour
	Median adult farm hrs/week	48 hours / week	48 hours / week*
	Median child farm hrs/week	45 hours / week	48 hours / week

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

Hours per week are reported without zeros. * several lower percentiles have increased in the treatment group relative to the control group since 1997; this explains the mean results reported in Table 9.

Table 3
Treatment effects on children’s participation in paid farm work and broader measures of farm work (probit model)

Explanatory Variables	(1) Indicator for working as a paid farm worker (1997 vs. 1998)	(2) Indicator for working as a paid farm worker (1997 vs. 1999)	(3) Indicator for working as an unpaid family worker or self-employed worker (1997 vs. 1999)	(4) Indicator for working as a paid farm worker or unpaid family worker or self-employed worker (1997 vs. 1999)
Diff-in-Diff (post = 1 & treatment village = 1)	-0.002 (0.001)	-0.002** (0.001)	-0.009 (0.007)	-0.014** (0.006)
(Treatment Effect Percentage Change)	- 3.5 percent insignificant	- 3.8 percent significant	- 14.5 percent insignificant	- 11.8 percent significant
Post-treatment Dummy	-0.003** (0.001)	-0.003*** (0.001)	-0.020*** (0.005)	-0.023*** (0.005)
Male Dummy	0.030*** (0.001)	0.024*** (0.001)	0.027*** (0.002)	0.07*** (0.002)
# Observations	61127	58851	50190	61228
Pseudo R2	0.33	0.36	0.21	0.29

The unit of observation is an individual child (a person aged less than 17). Specification 1 includes years 1997 (pre-treatment) and 1998 (post-treatment). Specifications 2 through 4 include years 1997 (pre-treatment) and 1999 (post-treatment). Coefficients reported are the marginal effects calculated from the probit coefficients. The estimated equation is Equation 8 in Section IV, with age dummies and village-level fixed effects. The Difference-in-Differences coefficient is interpreted as a percentage change in the row “Treatment Effect Percentage Change,” by dividing the Diff-in-Diff coefficient by the percentage of children for whom the

dependent variable is 1 in the pre-treatment survey. Standard errors, adjusted for village-level clustering, are in parentheses.

* = significant at 10 percent, ** = at 5 percent *** = at 1 percent

Table 4
Treatment effects on hourly wages of child farm workers

Explanatory Variables	(1) log hourly wage (1997 vs. 1998)	(2) log hourly wage (1997 vs. 1999)
Diff-in-Diff (post = 1 & treatment village = 1)	0.07 (0.04)	0.12*** (0.04)
(Treatment Effect Percentage Change)	+ 6.8 percent insignificant	+ 12.0 percent significant
Post-treatment Dummy	0.07** (0.03)	0.24*** (0.03)
Male Dummy	0.09*** (0.03)	0.08** (0.04)
# Observations	2597	2530
R2	0.04	0.14

The unit of observation is an individual child farm worker (a person aged less than 17).

Specification (1) includes years 1997 (pre-treatment) and 1998 (post-treatment). Specification (2) includes years 1997 (pre-treatment) and 1999 (post-treatment). Coefficients reported are the marginal effects from a linear regression model with village-level fixed effects. The estimated equation is Equation 8 in Section IV. Both specifications include dummy variables for each year of age, and village-level fixed effects. The Difference-in-Differences coefficient is interpreted as a percentage change in the row “Treatment Effect Percentage Change.”

Standard errors, adjusted for village-level clustering, are in parentheses.

* = significant at 10 percent, ** = at 5 percent *** = at 1 percent

Table 5
Quantile difference-in-difference treatment effects on hourly wages of adult farm workers, 1997 vs. 1999

Percentile of Wages	Treatment effect
10 th Percentile	0.000 (0.000)
20 th Percentile	0.250 (0.145)
30 th Percentile	0.179 (0.101)
40 th Percentile	0.000 (0.000)
50 th Percentile	-0.083 (0.131)
60 th Percentile	0.069 (0.058)
70 th Percentile	0.000 (0.102)
80 th Percentile	0.625 (0.083)
90 th Percentile	-0.020 (0.382)
median pesos per hour in 1997:	2.86
20 th percentile pesos per hour:	2
30 th percentile pesos per hour:	2.5
80 th percentile pesos per hour:	3.75

The sample is restricted to prime-age adults (ages 17 through 59) who are paid farm workers.

The estimated equation is Equation 8 in Section IV, with no controls and no cropping.

Bootstrapped standard errors are in parenthesis. Results significant at the 10 percent level are bolded.

Table 6
Treatment effects on adult farm workers, from 1997 to 1999

Explanatory Variables	(1) Log Hourly Wage	(2) Log Daily Income	(3) Log Hours per Week	(4) Log Days per Week	(5) Paid Farm Work
Diff-in-Diff (post = 1 & treated village = 1)	0.032* (0.019)	0.028 (0.019)	0.033* (0.019)	0.037** (0.017)	0.011 (0.014)
(Treatment Percentage Change)	+ 3.2 percent significant	+ 2.8 percent insignificant	+ 3.3 percent significant	+ 3.7 percent significant	+ 4.1 percent insignificant
Treatment Village Indicator	-0.010 (0.022)	-0.001 (0.022)	-0.030* (0.016)	-0.039*** (0.015)	-0.004 (0.015)
Post-treatment Indicator	0.327*** (0.014)	0.316*** (0.014)	-0.073*** (0.014)	-0.059*** (0.012)	0.061*** (0.011)
Male Indicator	0.026** (0.012)	0.051*** (0.012)	0.097*** (0.016)	0.072*** (0.015)	0.554*** (0.009)
Constant	1.08*** (0.026)	3.12*** (0.027)	3.60*** (0.025)	1.56*** (0.002)	
# Observations	26290	26290	26254	26254	103385
R2	0.26	0.26	0.35	0.33	0.34

The unit of observation is an individual prime-age adult farm worker (a person aged 17 through 59). Observations are dropped at the tails of the wage distribution according to the cropping rules discussed in Section III. Coefficients reported in specifications (1) through (4) are the marginal effects from a linear regression model, controlling for gender, age dummies, schooling level, language skills, and marriage status. The coefficients reported in specification (5) are the

marginal effects from a probit regression model. The estimated equation is Equation 8 in Section IV. Standard Errors, corrected for village-level clustering, are in parentheses.

* = significant at 10 percent, ** = at 5 percent *** = at 1 percent

Table 7
Treatment effects on the corn harvest in October 1998 and May 1999

Independent Variables:	Dependent 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)
Treatment Village Dummy	-0.00 (0.02)	-0.05 (0.10)	-0.00 (0.02)	-0.01 (0.08)
(Treatment Percentage Change)	-1.0 percent insignificant	-5.0 percent insignificant	-1.0 percent insignificant	-1.0 percent insignificant
Constant		0.26*** (0.08)		-0.11 (0.07)
# Obs	23143	7172	21961	6713
R2	0.00	0.00	0.00	0.00

The unit of observation is an individual household. There is no data for corn harvest size in the pre-treatment 1997 survey, thus the treatment village dummy represents the treatment effect (a first-differences treatment effect, rather than a difference-in-differences treatment effect).

Coefficients reported are the marginal effects from a probit model in specifications (1) and (3), and are ordinary least squares coefficients in specifications (2) and (4). The estimated equation is Equation 9 in Section IV. Standard Errors, corrected for village-level clustering, are in parentheses.

* = significant at 10 percent ** = significant at 5 percent, *** = significant at 1 percent

Table 8
Treatment effects on hectares used or owned, total and agricultural

Independent Variables:	Dependent Variables		
	(1) Total Hectares in October 1998 versus Total Hectares in November 1997	(2) Agricultural Hectares in October 1998 versus Agricultural Hectares in November 1997	(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 percent insignificant	+2.6 percent insignificant	+0.7 percent 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

The unit of observation is an individual household. The estimated equation is Equation 8 in Section IV. Standard Errors, corrected for village-level clustering, are in parentheses.

* = significant at 10 percent, ** = 5 percent, *** = 1 percent.

Table 9
Quantile difference-in-differences treatment effects on land rental income, in pesos per day

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

The estimated equation is Equation 8 in Section IV, with no controls and no cropping. The median daily rental income was 5.5 pesos per day, and the mean was 15 pesos per day. Standard Errors are in parentheses.

* = significant at 10 percent, ** = 5 percent, *** = 1 percent.

Table 10
Treatment effects on expenditures on non-labor agricultural inputs from December 1998 through May 1999: seeds, fertilizers, pesticides, machinery, and yoke labor.

Independent Variables:	Dependent 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 percent insignificant	-1.9 percent insignificant	-1.0 percent Insignificant
Constant		6.10 (0.053)	
# Obs	22139	7668	20508
R2	0.00	0.00	0.00

The unit of observation is an individual household. There is no data for expenditures on non-labor inputs in the pre-treatment 1997 survey, thus the treatment village dummy represents the treatment effect (a first-differences treatment effect, rather than a difference-in-differences treatment effect). 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. The estimated equation is Equation 9 in Section IV. Percentage changes in columns 1 and 3 are calculated by dividing the marginal effects by the control group mean. Standard errors, corrected for village-level clustering, are in parentheses.

* = significant at 10 percent, ** = at 5 percent *** = at 1 percent

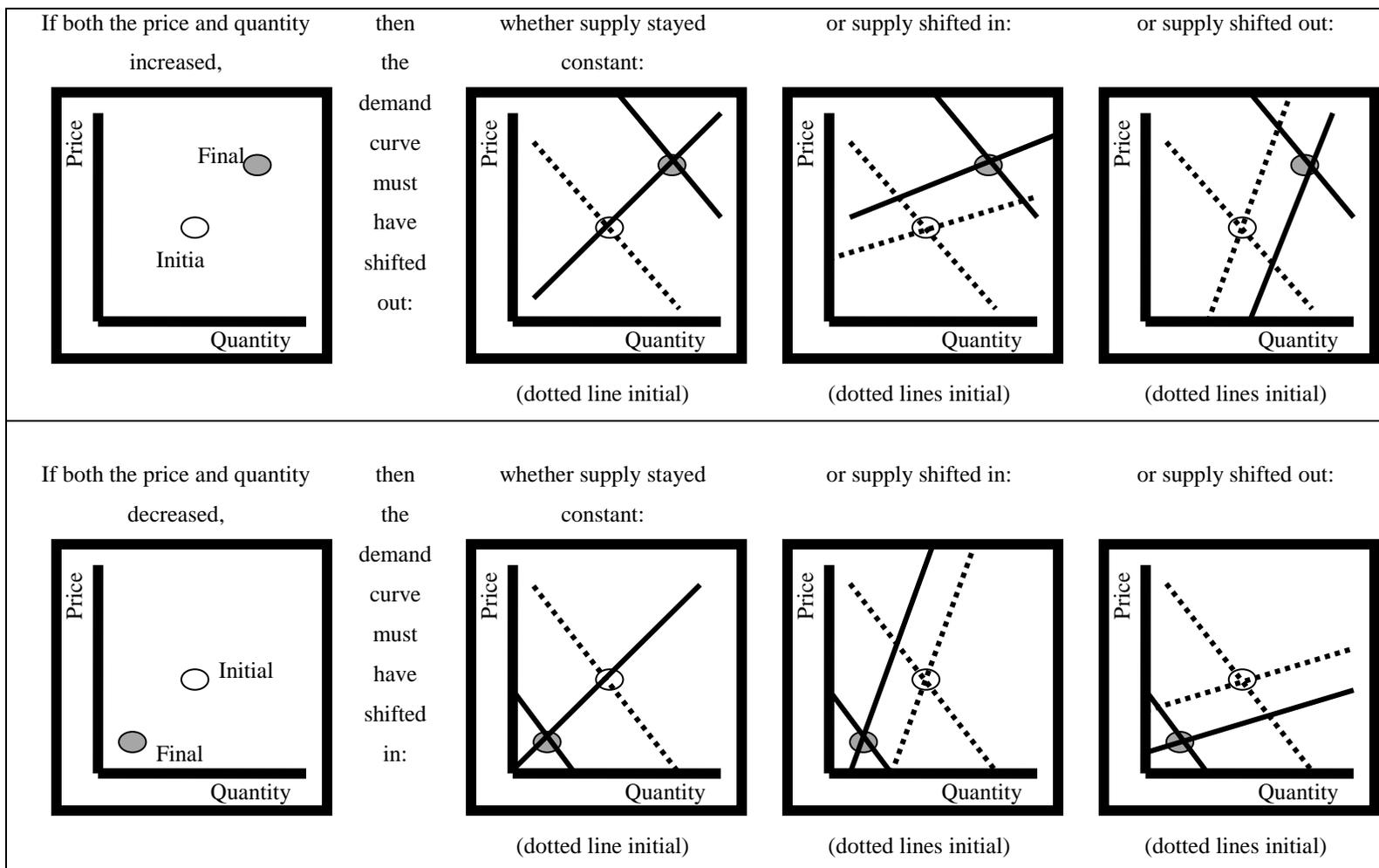


Figure 1

Determining the sign of the demand shift, when the sign of the supply shift is unknown

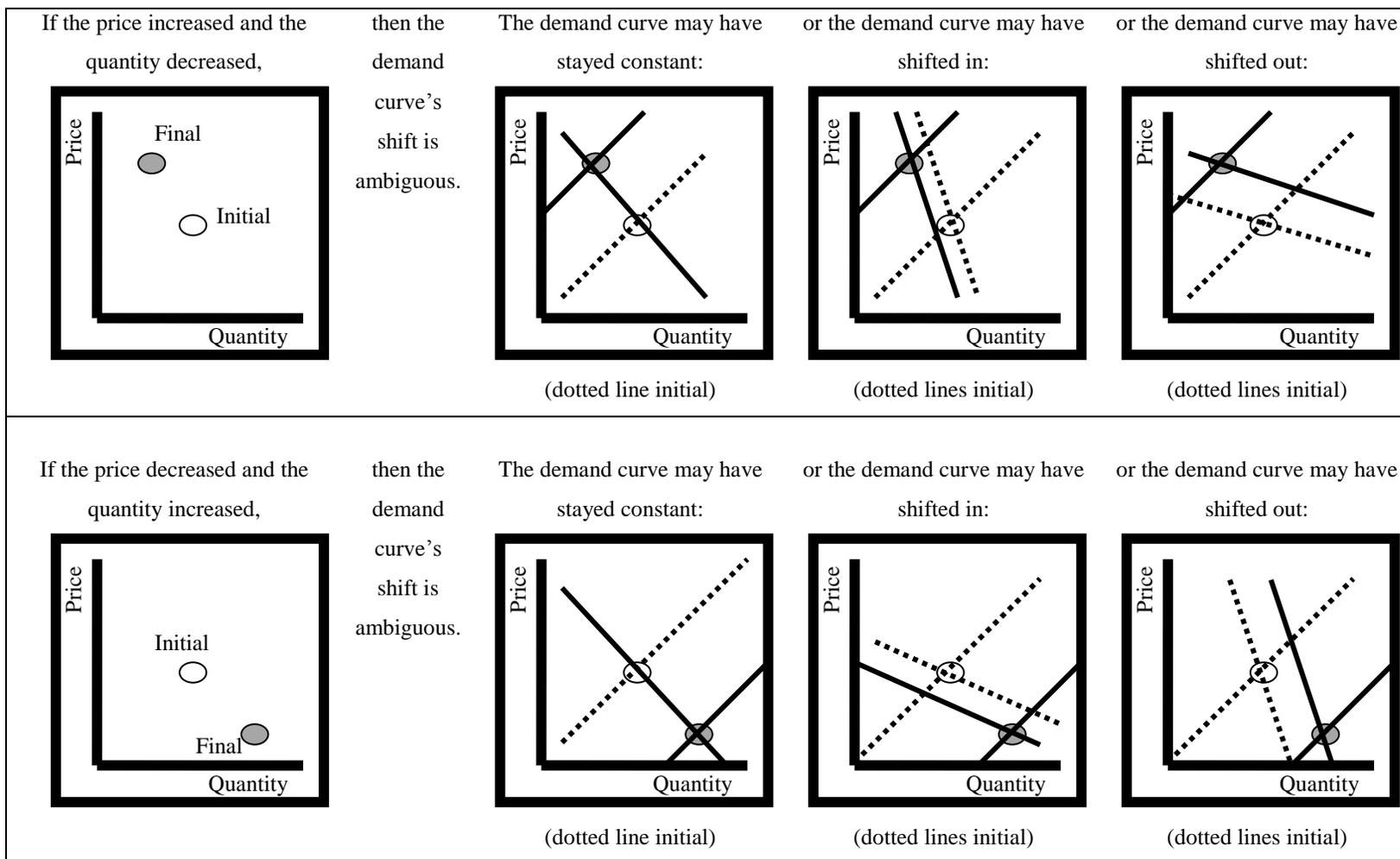


Figure 1 (continued)

Determining the sign of the demand shift, when the sign of the supply shift is unknown

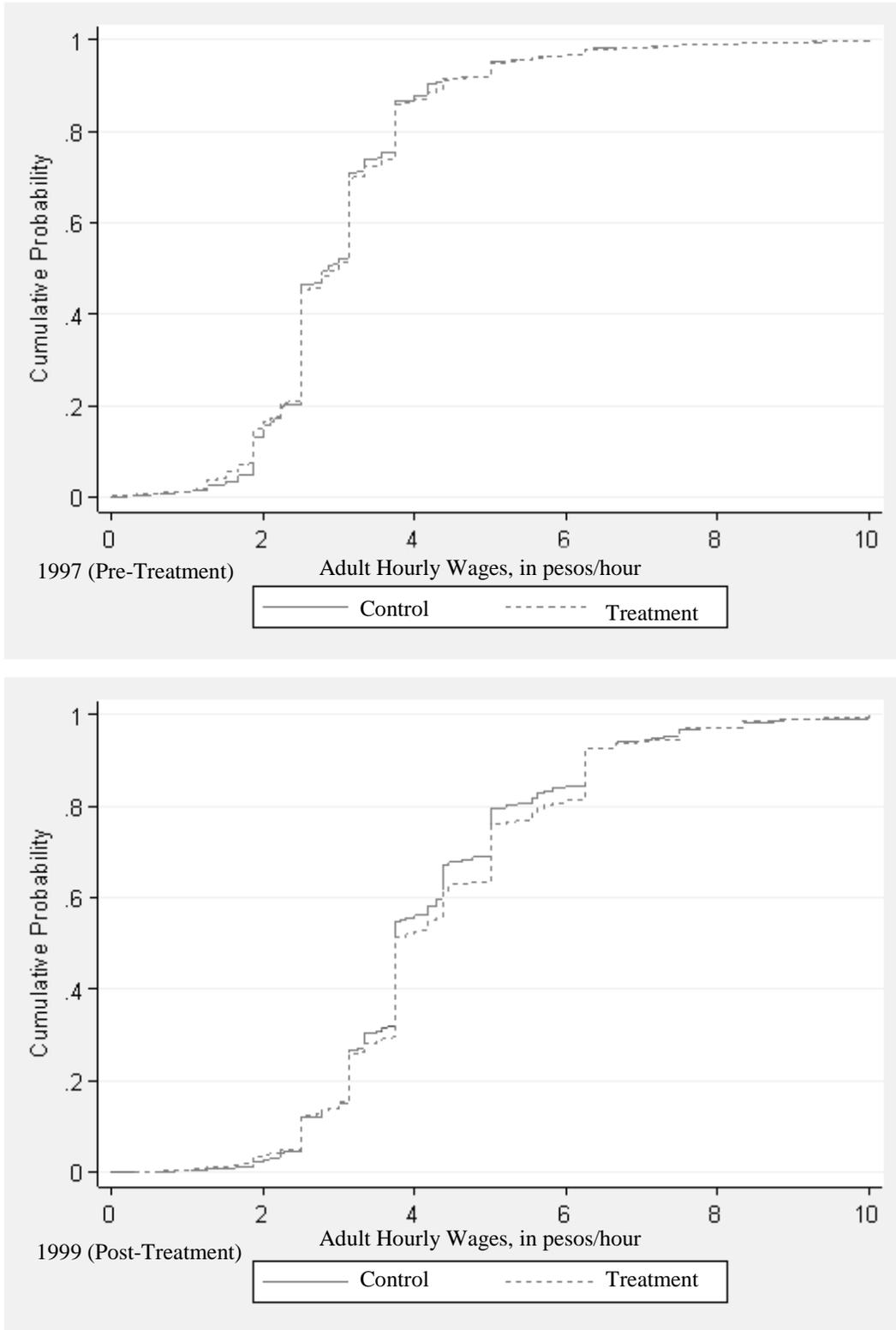


Figure 2
Cdfs of Hourly Wages of Adult Farm Workers, Control vs. Treatment

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1. Edmonds (2008) also asks: “Is it possible that variation in the activities of less than a hundredth of a percent of the economically active population can influence equilibrium wages in the labor market?” This is an important point, but as I show later in this paper, there exist important industries in which children make up significantly more than a hundredth of a percent of the economically active population – for these industries, the possibility of a relationship between child labor and adult labor market outcomes is more serious. Indeed, as Edmonds concludes: “to the extent that child labor suppresses adult wages, this may have long run implications for growth and development.” For related work, see: Brown, Deardorff, and Stern (2002), Diamond and Fayed (1998), Levinson et al. (1998), and Ray (2000).
 2. Note that much of the notation and organization in the first pages of this section are adapted from the simple model in Cahuc and Zylberberg (2004). Obviously, Section (A) and onward are not adapted from their organization.
 3. 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).
 4. See Behrman and Todd (1999) for a discussion of the randomization procedure.
 5. 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.
 6. This cropping is carried out relative to the sample used in each regression (usually, this is all adult farm workers, 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.

7. Some of these personal characteristics may have been affected by the treatment. I re-run all these specifications without the personal characteristics, and with the personal characteristics measured at baseline so that they are held constant. None of the results change appreciably.

8. The percentage change is calculated by dividing the coefficient on the Difference-in-Differences dummy from Table 5 by the pre-treatment mean value of the independent variable, 0.054.

9. According to Table 3, children classified as either working for their families without payment or self-employed make up almost all of the working children who are not paid farm workers. According to the May 1999 time-use supplement, these children work more than 10 times as many hours in their own family's fields than other children do. Thus, it is relevant to consider treatment effects on the broader measure of child farm labor that includes both paid farm workers and these other workers who work so much in their own family's fields. I do so in Specification (4) of Table 3: this broader measure of child farm work shows an even larger decline due to the treatment.

10. 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 (that is, 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.

11. Table 8 reports results with heteroskedasticity-corrected standard errors, but without village-level fixed effects. Under the current 5 percent symmetric cropping, the treatment coefficients

are still positive but no longer significant at conventional levels when I simultaneously control for village-level fixed effects and correct the standard-errors for heteroskedasticity using the robust cluster option in Stata, though significance returns for models with village-level fixed effects and uncorrected standard errors.

12. In this calculation, I am also assuming that adult participation in the farm workforce has stayed constant, which is consistent with the insignificant effect reported in specification 5 of Table 6.

13. Of course, the direct treatment benefits in income may have affected adult labor supply as well. But the main identification strategy already takes such simultaneous movements of adult labor supply into account, as explained in the introduction and in Section II. In particular, simultaneous movements in price and quantity of adult labor can identify the direction of a demand shift even when supply has changed as well, as long as those movements in price and quantity are co-directional.

14. 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. 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.

15. The ten criteria are: days of difficulty performing activities due to bad health in the past month are zero; days of missed activities due to bad health in the past month are zero; days in bed due to bad health in the past month are zero; yes, I can currently perform vigorous activities; yes, I can currently perform moderate activities; yes, I can carry an object of 10 kilograms 500

meters with ease; yes, I can easily lift a paper of the floor; yes, I can walk 2 kilometers with ease; yes, I can dress myself with ease; I have had no physical pain in the last month.

16. Treatment effects from mean regressions for these last two robustness checks are always positive and are closer to 2 percent, but are generally not statistically significant at conventional levels, perhaps due to the 80 percent reduction in sample size, or perhaps to the fact that the ineligible subsample is skewed towards richer higher-ability people who are less substitutable with children.

17. 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.

18. 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).