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AN EVALUATION OF ARGENTINA'S *JEFES Y JEFAS DE*
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Randall C Juras

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AN EVALUATION OF ARGENTINA'S *JEFES Y JEFAS DE HOGAR* PUBLIC
EMPLOYMENT PROGRAM

By

Randall C Juras

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ABSTRACT

AN EVALUATION OF ARGENTINA'S *JEFES Y JEFAS DE HOGAR* PUBLIC EMPLOYMENT PROGRAM

By

Randall C Juras

This dissertation concerns the use of public employment as an anti-poverty measure following Argentina's 2001-2002 economic collapse. All three chapters focus on Argentina's *Jefes y Jefas de Hogar* social safety net employment program.

The first chapter addresses the program's unexpectedly high proportion of partnered female applicants, which had caused the government some concern insofar as many of these workers had not previously been counted among the unemployed, and their participation thus did little to lower the official unemployment rate. I estimate a structurally derived multinomial probit model of application to *Jefas*, and find that individuals' opportunity costs of time were strongly correlated with the potential to earn income in market employment, with the result that the optimal enrollee in a household was often a female "secondary earner," who had inferior access to the labor market. I also show that public schooling likely played an important role in enabling women to enroll in *Jefas*.

The second chapter addresses how changes to the design of *Jefas* would have affected the gender composition and poverty levels of applicants. In particular, I use the reservation wages estimated in the previous chapter to estimate the effect of two hypothetical changes: removal of the "one person per

household” rule, and perfect enforcement of the requirement that applicants be primary earners who lost their jobs during the crisis. Counterfactuals are based on weighted averages of characteristics, using probabilities predicted by a probit model of participation. I find that in terms of the overall goal of the heads of household program, which was to reduce poverty given a limited budget, it does not appear that any of the potential changes outlined in this paper would significantly improve performance, nor would allowing enrollment by more than one member of a household have significantly changed applicants’ gender composition.

The third chapter evaluates Jefes’ impact on child labor and school attendance, using a comparison group of children whose parents applied to the program but did not receive benefits. I develop both parametric and semiparametric estimates of the program’s impact, which require different assumptions, and reveal different information about the relationship between the treatment and outcomes of interest. Using cross-sectional and longitudinal data, I find that children age 10-14 whose parents enrolled in the *Jefes* program and received benefits were less likely to report working by around 1.3 percentage points, and more likely to report attending school by approximately 2.0 percentage points, compared with similar children whose parents were not enrolled.

DEDICATION

To Anna, for somehow making
graduate school fun.

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CHAPTER ONE

Structural Estimation of a Model of Workfare Enrollment: An Analysis of Argentina's *Heads of Household* Program

1.1 Introduction

Beginning in December 2001, Argentina experienced a serious economic crisis accompanied by rapidly falling real wages and crushing poverty. At the height of the crisis, the poverty rate roughly doubled from 30 percent to around 58 percent of the population, with 27 percent unable to meet even the most basic needs. To help households cope with the effects of the crisis, the government instituted a large-scale public employment program called *Jefes y Jefas de Hogar Desocupados* (Unemployed Heads of Household), or *Jefas*, that provided a cash transfer in exchange for twenty hours per week of work and was modeled after an existing workfare program in Argentina. Although lacking a specific expiration date, *Jefas* was designed to provide temporary support for impoverished workers with children and was never intended to provide long-term assistance (World Bank, 2002). Program rules limited enrollment to one individual per household.

Jefas was notable for the high rate of take-up among partnered women, a majority of whom had never before been active labor market participants and a

majority of whose male partners were employed.¹ Aside from taking officials by surprise (Galasso and Ravallion, 2004; World Bank, 2003), this phenomenon created some political difficulties for the government because the “new” workers did little to lower the official unemployment rate. At the height of enrollment in 2002, women accounted for over 70% of the nearly 2 million program beneficiaries, and more than 700,000 women entered the labor force specifically to enroll in the program. Women with domestic partners—married or unmarried—were responsible for the bulk of this phenomenon, accounting for 87% of the new labor force entrants enrolled in the program. Viewed another way, nearly 60% of partnered women who enrolled in *Jefas* entered the labor force to do so, whereas single women who enrolled were largely drawn from unemployment. Why was there such a surge in demand for workfare employment from partnered women? The variables typically thought to have contributed to the observed long-run increase in female labor force participation rates did not change suddenly as a result of the crisis, so they cannot be responsible for the behavior of these workers. Nor was there a corresponding increase in female labor force participation outside the program. Rather, the workfare enrollment decision, in the face of exogenous shocks to household income and employment, must have been governed by a separate set of considerations. The goal of this study is to elucidate the factors that led so many partnered women to apply for *Jefas* employment, as a step towards understanding household labor supply responses

¹ I say ‘partnered’ rather than ‘married’ because formal marriage is relatively uncommon in the demographic of interest; these relationships are typically equivalent to marriage for practical purposes.

to economic downturns, and the wide range of women's take-up rates in public employment programs in general.

In this paper, I develop a model of participation in which the probability that an individual enrolls in workfare depends on that individual's opportunity cost of time, or reservation wage, as well as the reservation wage of his or her partner. Reservation wages are in turn functions of market wage potential, household financial resources, and the value of time spent in home production. Because enrollment in *Jefas* is limited to one individual per household, family members must compare the values of their reservation wages. In partnered households, the final enrollment decision depends on the relative values of the man's and woman's reservation wages, and the wage offered by the government for *Jefas* employment. I use likelihood maximization techniques to generate parameter estimates.

I find that higher potential market wages in non-*Jefas* employment were associated with lower probabilities of participation in the program. Presumably the rigid, mandatory work requirement associated with *Jefas* served as a deterrent for individuals who had market employment possibilities—which was, after all, its purpose. At the same time, *Jefas* was attractive to other family members in those same households who did not have the potential to earn significant income, and who may even have been unemployable at prevailing market wages. I also find that having more school-age children increased a woman's probability of applying to the program, which seems inconsistent with

previous studies showing that fertility decreases women's labor force participation. I attribute this phenomenon to the wide availability of low-cost, high-quality public schooling.

The paper is structured as follows: I describe institutional details of the *Jefas* program in Section 2. Section 3 models the household's workfare enrollment decision. Section 4 details the econometric specification. Section 5 describes the data. Section 6 presents empirical results. Section 7 analyzes the results in terms of the added-worker effect, and Section 8 concludes.

1.2 Institutional Details of the *Jefas* Program

Recognizing the severity of the 2001-2002 economic crisis, the Argentine government instituted the emergency social safety net program *Jefas* in April 2002, with partial financing from the World Bank in the form of a \$600 million loan. *Jefas* was closely modeled after an existing workfare program that predated the crisis (*Plan Trabajar*) but *Jefas* imposed more restrictions on enrollment.

The stated purpose of *Jefas* was to provide direct income support for families with dependents who had lost their main source of earnings due to the crisis, and an explicit secondary goal was to reduce the official unemployment rate by classifying participants as employed. This program was the government's primary safety net response to the crisis. Most other safety net programs were eliminated or reduced in order to shift funding to *Jefas*: at the height of participation in 2002, government spending on *Jefas* was about 1% of GDP.

As originally implemented, *Jefas* transferred AR\$150 per month (about US\$50) to participants, who were enrolled on a first-come, first-serve basis subject to eligibility. Four eligibility criteria required beneficiaries to:

- be unemployed;
- be the head of a household;
- live in a household with at least one minor below the age of 18, a pregnant woman, or a handicapped individual of any age;
- work (or participate in training or education activities) for 4-6 hours a day in exchange for the payment.

Notably, the “head of household” requirement limited enrollment to one individual per household, which was an attempt to curtail costs while spreading benefits to as many families as possible.

Jefas was overseen at the national level by the Ministry of Labor and Social Security, but the Ministry only provided the general guidelines of the program. Administration took place at the local level, with municipalities, churches, and organizations of the unemployed (*piqueteros*) enrolling participants and later organizing workfare activities. This was meant to ensure that projects were well targeted to the specific needs of the communities, and the tasks were organized by non-governmental organizations and non-profits that already operated within these localities. In theory, this should also have screened out individuals whom local administrators knew to be ineligible.² By registering

² Increasingly, this sort of decentralized, or community-based targeting is a precondition for World Bank financing. Ravallion (2006) reviews the literature on income transfer

workers in a national database and assigning them social security numbers, it was also intended to help formalize the labor market.

To prove eligibility, participants were required to show documentation to program administrators that verified unemployment status and legal guardianship of a minor. These documents were then cross-checked against Social Security Administration lists. Verifying unemployment proved not to be feasible, however, since 50% of economic activity in Argentina is estimated to take place in the informal economy, and only employment in formal sector jobs is verifiable. Thus, this requirement appears to have been rarely enforced in practice. Administrators were also not able to check whether an applicant was really the head of a household, and in fact the concept was not even well defined, so this requirement was also not enforced.³

In contrast, there is ample evidence that households believed the work requirement would be binding at the time they registered. The requirement was heavily publicized in advance of enrollment, and a program evaluation by the

programs, and Conning and Kevane (1999) discuss community-based targeting mechanisms. In the case of *Jefas*, this has caused some problems. There have been widespread complaints of rent-capture and favoritism by the *piqueteros*, who administer about 10% of the workfare activities. Participants also complain that if not for *Jefas*, the government would have to hire them to perform the same work anyway, and pay them a higher wage.

³ While not formally defined, it is my impression from reading program documents that the government intended to give benefits to primary wage earners who had become unemployed. “Head” status was entirely self-reported. Nonetheless, the name of the program, “Heads of Household,” reflects the centrality of this concept to *Jefas*’ designers.

Ministry of Labor reports that the majority of beneficiaries (93% of women and 81.6% of men) fulfilled the conditional work requirement (MTESS, 2004).⁴

Other than the work requirement and the dependent-child requirement, it appears that in practice the eligibility requirements were not enforced (which, in some cases, would have been nearly impossible to do). **Table 1.1** gives some perspective regarding the lack of enforcement. Thus, in practice, *Jefas* was effectively a cash-for-work public employment program targeted at families with children. Due to lax enforcement, it allowed one household member to participate, regardless of official employment status or head of household status.

Besley and Coate (1992) establish that self-targeting to individuals through a work requirement can be efficient if the wage is set below the market wage for unskilled labor in the informal sector, so that only individuals with a very low reservation wage have an incentive to participate, and Ravallion and Datt (1995) show that such a scheme can be cost-effective in practice. The *Jefas* wage was set to meet this criterion: *Jefas* required that beneficiaries participate in an eligible work or job-training activity no less than 4 hours per day, 5 days per week, and the monthly transfer of AR\$150 was set below the expected full-time wage rate for households in the bottom decile of the income distribution.⁵

⁴ The World Bank (2006) reports that while initially high, compliance had fallen to about 69% by the beginning of 2005, and is expected to deteriorate further. For the purposes of studying enrollment, what matters is the expectation that the requirement is binding.

⁵ The World Bank calculates the expected hourly wage rate (averaging formal and informal work) in the first income decile as AR\$0.97 and in the second decile as AR\$2.01. This corresponds to monthly incomes of AR\$180 and AR\$370, respectively. In my data, only 8% of men and 12.6% of women who work at least 20 hours per week earn

Jefas initially experienced rapid enrollment growth, increasing in size from 574,000 participants in May 2002 to 1,857,000 in Dec. 2002, with aid reaching an estimated 10 million family members in participating households. Mostly as a response to budget constraints in light of the rapid growth, registration unofficially closed in Sept. 2002 and officially closed in May 2003 with close to 2 million enrolled,⁶ and enrollment steadily decreased to about 1.4 million in 2006. The growth in the program exceeded what both the Bank and the government had expected based on estimates of the target population (World Bank, 2006).

1.3 Model of Participation Decision

There is a reasonably large literature on targeting through workfare. Early contributors include Akerlof (1978) and Nichols and Zeckhauser (1982), who say that the imposition of “ordeals” on welfare claimants may improve targeting. The first detailed theoretical analysis of the screening argument for workfare was Besley and Coate (1992). In their model, developed to explore the incentive case for workfare, an individual decides between spending time working as part of the government program and using that time for other purposes, such as leisure, home production, or wage labor. A prediction of their model is that, once the wage offered as part of the benefits package has been set, an individual will

less than AR\$150 /month. Mean income for men is AR\$600 /month and for women is AR\$450 /month.

⁶ Some additional registration appears to have occurred in the year following the unofficial end of registration, which did not carry legal weight. Beneficiary numbers increased significantly just before the presidential election of 2003, and registration was legally closed in May 2003 (World Bank, 2006).

accept the government's offer if and only if his or her opportunity cost of time is below that wage offer. Then, modeling an individual's opportunity cost of time as a function of observable characteristics, with some error, it is possible to predict the probability that an individual enrolls as:

$$(1) \quad \text{pr}(\text{individual enrolls}) = \text{pr}(\text{opportunity cost} \leq \text{government's wage offer})$$

An analysis of the *Jefas* program must differ slightly from this model because *Jefas* allows enrollment of only one person in any household. Thus, the individual's decision will necessarily be affected by the incentives of other family members who may wish to enroll, especially the individual's partner, and this process must be explicitly modeled. Assuming that household-level decision making involves only the household head and his or her partner (and that all households contain such a pair), the final decision on whether to enroll will take place only after negotiation between those partners.⁷ Nonetheless, it seems reasonable that the same basic principle applies—individuals compare their utility from enrollment with the alternative of non-enrollment, and wish to enroll only if they consider enrollment the more valuable option. Simultaneously, partners must also have some negotiating mechanism by which they compare their relative opportunity costs. At the conclusion of negotiations, an individual will

⁷ Any framework for intra-household decision-making that implies a Pareto efficient outcome would be sufficient for the purposes of this model. For examples of potential bargaining frameworks see Chiappori (1992). The purpose of this research is not to test any one such framework.

apply to enroll if and only if two conditions are met: (a) that individual's opportunity cost of time is judged to be the lower of the two, and (b) the value associated with enrollment in *Jefas* exceeds the opportunity cost of time. Hereafter, I refer to the lowest pecuniary wage offer that would satisfy condition (b) for an individual as that individual's reservation wage.

There are three observable outcomes of the application decision: y_0 , y_1 , $y_2 \in \{0,1\}$ which correspond to the events that both decline the government's offer ($y_0 = 1$), the man applies ($y_1 = 1$), and the woman applies ($y_2 = 1$), respectively. Denote the *Jefas* wage offer W_J , which is a scalar, the man's reservation wage W_M , and the woman's reservation wage W_F . The probability of observing each outcome can be fully described as:

$$(2) \quad \text{pr}(y_0 = 1) = \text{pr}(W_J \leq W_M \text{ and } W_J \leq W_F)$$

$$(3) \quad \text{pr}(y_1 = 1) = \text{pr}(W_M \leq W_F \text{ and } W_M \leq W_J)$$

$$(4) \quad \text{pr}(y_2 = 1) = \text{pr}(W_F \leq W_M \text{ and } W_F \leq W_J)$$

where $\text{pr}(y_0 = 1) + \text{pr}(y_1 = 1) + \text{pr}(y_2 = 1) = 1$. In other words, the individual will apply to enroll if his/her reservation wage is lower than the *Jefas* wage offer and if his/her reservation wage is also low *relative* to his/her partner.

In practice, this model allows for estimation not only of the man's and woman's enrollment probability, but also of the determinants of the man's and woman's reservation wages. I allow the reservation wage to depend primarily on

three components: individuals' market wage opportunities and working conditions, the value of their time spent in home production, and the value of other financial resources (assets) available to help weather the crisis.

Unlike much of the existing literature, I do not incorporate the assumption that the reservation wage is equal to the market wage for employed individuals. This assumption would be troublesome in the context of this study for two reasons: First, it implies that work hours are flexible for those working, so that individuals may adjust them to equate the shadow price of time with the market wage. This was clearly not possible for many in the Argentine economy in 2002, when employers were restricting hours and cutting employment in the face of the recession. Second, it implies that individuals will take a competing job offer if and only if the wage rate is higher than their current wage rate, which rules out the idea of compensating differentials. Workfare employment in particular is likely to differ from other market employment in a number of dimensions, including the perceived temporary nature of the program, the fixed hours, potential job-training, and/or potentially better working conditions mandated for government employment. To mitigate these potential problems, I allow the reservation wage to differ from the observed market wage for employed individuals.

1.4 Model Estimation

I estimate equations (2), (3), and (4) by specifying the functional form of the two reservation wage equations for any given household i as follows (suppressing the i subscript for clarity):

$$(5) \quad \log W_M = \beta_M^* X + \varepsilon_M$$

$$(6) \quad \log W_F = \beta_F^* X + \varepsilon_F$$

where X is a vector of individual and household characteristics (described below), and ε_M and ε_F are jointly normally distributed according to:

$$\begin{pmatrix} \varepsilon_M \\ \varepsilon_F \end{pmatrix} \sim N \left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma^2 & \rho\sigma^2 \\ \rho\sigma^2 & \sigma^2 \end{pmatrix} \right)$$

The likelihood for household i can be written as

$$\ell_i = \left(\text{pr}(y_1 = 1) \right)^{y_1} \left(\text{pr}(y_2 = 1) \right)^{y_2} \left(\text{pr}(y_0 = 1) \right)^{1-y_1-y_2}$$

where $\text{pr}(y_0 = 1) + \text{pr}(y_1 = 1) + \text{pr}(y_2 = 1) = 1$. The likelihood function for all N observations is

$$L = \prod_{i=1}^N \ell_i.$$

With small w 's signifying $(\log W_i / \sigma)$, and defining $\beta_M = (\beta_M^* / \sigma)$ and $\beta_F = (\beta_F^* / \sigma)$

the probabilities of the events corresponding to (2), (3), and (4) are:

$$\text{pr}(y_0 = 1) = \Phi_2(\beta_M' X - w_J, \beta_F' X - w_J \mid \rho)$$

$$\text{pr}(y_1 = 1) = \Phi_2 \left(w_J - \beta'_M X, \frac{\beta'_F X - \beta'_M X}{\sqrt{2-2\rho}} \mid \frac{1-\rho}{\sqrt{2-2\rho}} \right)$$

$$\text{pr}(y_2 = 1) = \Phi_2 \left(w_J - \beta'_F X, \frac{\beta'_M X - \beta'_F X}{\sqrt{2-2\rho}} \mid \frac{1-\rho}{\sqrt{2-2\rho}} \right).$$

Φ_2 is the bivariate standard normal cumulative distribution function. The resulting estimator is the maximum likelihood estimator of a trinomial probit with correlated errors, and is consistent, asymptotically efficient, and asymptotically normally distributed.⁸ Estimates of β_M and β_F were obtained using Stata's `<ml>` command for maximizing user-generated likelihood functions. Predicted probabilities of each outcome were also generated for each household.

1.5 Data

Prior studies of labor force participation, including Heckman (1974), Gronau (1973), Dooley (1982), and Cebula and Coombs (2008), guide the selection of independent variables, which I describe in **Table 1.2**. In general, these characteristics will affect reservation wages either through the expected market wage, the value of home production, or access to financial resources. A higher market wage opportunity, or higher-valued home productivity, will positively affect the reservation wage if either must be foregone to participate. Access to financial resources is expected to increase the reservation wage through the income effect, assuming that leisure is a normal good.

⁸ The appendix gives the full model specification.

Data come from Argentina's Permanent Household Survey (EPH), which the National Institute for Census and Statistics (INDEC) conducts twice per year, in May and October. The EPH collects information from households in large urban areas that account for 70% of the Argentine population; rural areas are not sampled. Each household is interviewed in four consecutive surveys, so that at any given time up to a half of the interviewed households will be linked as a panel to the corresponding survey from the previous year, depending on attrition. This study uses data from October 2001, two months before the economic collapse, and October 2002, nearly a year after the crisis and after the bulk of enrollment in *Jefas* had occurred.

An advantage of the EPH is its commitment to anonymity, which means that respondents may report information that would have resulted in disqualification from *Jefas* without fear of reprisal. In fact, about 45% of partnered female *Jefas* beneficiaries report being the "spouse of the household head" in the EPH survey. Likewise, about 5% of beneficiaries are in a household having more than one beneficiary. Both of these situations are in clear violation of program rules. Households that report having more than one beneficiary are excluded from the data, because that situation is incompatible with the empirical model.

In the EPH, individuals within a household are separately interviewed, but a common household identification number links their answers. Each person's relation to the household head and civil status (single, married, partnered, widowed, or separated/divorced) are reported. Because the focus of this study is

on partnered couples, households are included in the sample only if there is a male-female cohabitating couple identified as married or partnered, and complete information on both individuals. This leaves 10,678 households, of which 4,435 are linked as a panel to the 2001 data.

In the October 2002 survey there was also a special module on *Jefas* participation, for the purposes of program evaluation, which was administered to both *Jefas* participants and individuals who had applied as of the survey date but were still on the waiting list.⁹ In the empirical analysis that follows, both of these groups—enrolled and non-enrolled applicants—are pooled together into a group of “applicants,” for the reason that all applicants, including those not yet enrolled, should have self-selected into the program based on program requirements and their opportunity costs of time in a similar manner. That is, I assume that all applicants, at the time they applied to *Jefas*, anticipated accepting the government’s offer of enrollment if and when it came. There are two reasons that this assumption might prove troublesome, and applicants still on the waiting list might differ from enrollees. First, if applying was not costly, individuals who were unsure of their future reservation wages may have signed up, knowing that if the state of nature were good at the time of enrollment they could simply decline the government’s offer at that time. Second, enrollment was on a first-come, first-serve basis, and the neediest households, or those first affected by the crisis, might have been motivated to apply for program benefits at the earliest dates.

⁹ Galasso and Ravallion (2004) use the group of *Jefas* applicants as a control group in their analysis of foregone income and of the program’s effect on the overall unemployment rate.

Those households would then be more likely to appear in the group of enrollees.

To test whether applicants on the waiting list differ in observable ways from enrollees, I estimate a probit regression of enrollment on household characteristics, conditional on application (**Table 1.3**). Individuals who were unemployed prior to the crisis, construction workers, public employees, and those from large households appear more likely to receive benefits, which is consistent with anecdotal evidence that the construction industry was one of the hardest hit sectors immediately following the economic collapse, and that early in the program, municipalities fraudulently enrolled their employees as a budget-cutting strategy. For enrollees (but not applicants still on the waiting list), the data also include the date of enrollment. Because enrollment was by and large first-come, first-serve, I can test whether similar characteristics are correlated with the date of application for program enrollees. A regression of enrollment date on household and individual characteristics, conditional on enrollment (**Table 1.4**), shows that women who had an unemployed spouse were likely to enroll earlier, as were female domestic workers (who have a flexible schedule and are usually paid under the table) and men who have more dependent children. Nonetheless, enrolled and non-enrolled applicants look quite similar compared with the general population, and I will combine the two groups—hereafter referred to simply as applicants—for this study.¹⁰ Additionally, in approximately 0.5% of the original

¹⁰ In addition, Galasso and Ravallion (2004) conduct a more thorough analysis and conclude that non-enrolled applicants are a suitable control group for enrollees in their study of foregone income and of the program's effect on the overall unemployment rate. I perform a sensitivity analysis by excluding non-enrolled applicants from the data; the

sample of applicant households, the applicant was an individual other than the head or spouse. These households have been dropped from the sample.

Table 1.5 contrasts descriptive statistics of non-applicants with the group of applicants. These statistics paint a compelling, if rudimentary, picture of factors influencing the enrollment decision. Applicants are on average younger, lower-income, have more children, and have fewer assets than non-applicant couples.

Table 1.6 compares the descriptive statistics of male applicants with female applicants. While there are fewer differences between these two groups, a few intriguing patterns emerge. Specifically, men are more likely to have worked in 2001, worked more hours, and earned more income in households with a female applicant. Likewise, women worked more often and, conditional on working, earned more income in households with a male applicant. Finally, women in the latter households were more likely to have had non-labor income. While this begins to elucidate some of the important factors involved in the application decision, the maximum likelihood analysis will provide additional insight into the factors that drive the gender of the enrollee.

1.6 Empirical Results

Factors used to predict the enrollment decision include variables correlated with individuals' potential market wages, the value of home production,

results are presented in the appendix in **Table 1.8**, and are qualitatively similar to those from the main regression.

and household assets available to help weather the crisis. Because the potential post-enrollment market wage is not observable, especially for enrollees who receive benefits in exchange for working, I include several factors that I expect to be correlated with it among the vector of characteristics used to explain reservation wage. These factors include applicants' 2001 monthly earned income, education, and age. The need for data on pre-crisis monthly income limits the sample to the 4298 households linked as a panel, including 653 applicants.¹¹ A possible concern with this approach is that applicants might have experienced larger declines in real income as a result of the economic collapse than did non-applicants. If that were the case, the model would underestimate the magnitude of the effect that income has on the household's decision. Results from the maximum likelihood estimation are presented in **Table 1.7**. Coefficients represent the estimated effect of the explanatory variables on individuals' reservation wages; these are then fed back into the model to generate the predicted probabilities of each observable outcome for each household.

Variables associated with an individual's market wage prospects are estimated to have the biggest effect on the application and enrollment decision, while variables related to the partner's market wage contribute to a lesser

¹¹ As a robustness check, I estimate an identical model using individuals' actual 2002 monthly income and hours in place of 2001 income. Applicants who are actually enrolled are not included in this regression. Results are presented in the appendix in **Table 1.9**. Despite endogenously truncating the sample by excluding participants, the estimates are similar to those from the main regression. For common observations, the predicted probabilities of men and women applying to *Jefas* ($y_1 = 1$ and $y_2 = 1$) generated by the two models have correlations of .75 and .78, respectively. As would be expected if applicants experienced larger drops in income than non-applicants, the estimated effect of income on enrollment is relatively larger in the model using actual 2002 income.

degree. The most important predictor, in terms of both statistical and practical significance, is monthly labor income from the previous year's survey. Evaluated at values of the independent variables typical of an applicant,¹² and holding hours constant, an increase in a man's 2001 monthly income from zero to 400 pesos decreases the estimated probability that he applies to the program by roughly two thirds, from 0.163 to 0.059, with the biggest marginal change at *income* = 0 (Figure 1.1).¹³ Simultaneously varying 2001 weekly hours from 0 to 40 makes little difference, giving estimated probabilities of 0.175 and 0.056. I take this as evidence that reservation wages are influenced by an individual's total income earning ability, rather than that individual's hourly wage. For the same increase in the man's income, the predicted probability that the woman applies declines by less than one third, from 0.193 to 0.152.

Likewise, an increase in the woman's income from zero to 400 pesos/month decreases her predicted application probability from 0.210 to 0.063—a 70% drop—while the probability of her partner's application remains relatively constant, even slightly increasing, as shown in Figure 1.2. This occurs because while the man's reservation wage increases, it is far more likely that the woman's reservation wage rises above his than that his rises above the threshold.

¹² To describe the “typical applicant,” I use the mean values of explanatory variables for the pool of enrolled and non-enrolled applicants in the panel sample.

¹³ Median 2001 male income in the sample is about 565 pesos/month, and median female income is 425 pesos/month for those who earn income.

Education, which has long been shown to be positively correlated with market wages, has a similar effect. An increase in the man's education from completed primary to completed secondary education (7 years to 12 years) results in a small but significant 16% decrease in his predicted probability of applying to *Jefas*, from 0.102 to 0.086, while the same increase in a woman's education decreases her application probability from 0.189 to 0.176, *ceteris paribus*. Flexible working conditions, which may increase applicants' ability to retain current employment while working for *Jefas* benefits, should be expected to negatively affect the opportunity cost of time and lead to increased participation. This may be the case: a typical man who reported being self-employed in 2001 is estimated to be three percentage points more likely to have applied to the program. On the other hand, the estimated effect of a woman's status as a domestic worker indicates that the causal interpretation may not be so clear—women who were employed as domestic workers are significantly less likely to be participants, which may indicate that they chose domestic employment because they value flexible work hours. Age variables are estimated to increase reservation wages, consistent with previous studies that use them to proxy for labor market experience.

The estimated coefficient on the *weekly hours* variable is more difficult to interpret. Holding income constant, an increase in the number of work hours implies a lower hourly wage, which I would expect to be correlated with a lower reservation wage. On the other hand, moving from a very small number of hours

to a large number may result in less flexibility, and a higher reservation wage, as discussed above. The estimated coefficient on an individual's weekly hours is positive and significant for each individual—implying a higher reservation wage—but has little effect on the predicted probabilities of application. For example, an increase from 10 to 30 in an individual's weekly work hours, holding income constant, changes the predicted probability of that individual's participation by less than 10%. I interpret this to mean that the hourly wage rate is less important than total income in the decision to apply for *Jefas* public employment. This is consistent with evidence that employers curtailed hours in the face of the crisis, with the result that employees could not increase their work hours to earn extra income.

Controlling for the market wage variables above that appear central to the enrollment decision, several other factors were estimated to have influenced participation to a lesser, but still noteworthy, extent. These include non-earned income and assets, and demographic variables that contribute to the value of home production. These are each briefly discussed below.

Non-earned income and assets should theoretically be important contributors to the work decision through the income effect. Indeed, the man's and the woman's non-earned incomes (for example, investments and pensions) are both significant variables in the woman's reservation wage equation, with both leading to a decreased probability of applying to *Jefas*. The marginal effect of unearned income on participation appears large in magnitude—in fact, as

large as the estimated effect of earned income. Yet it seems not to have played a large practical role in the overall rate of participation, since the variation in non-earned income in the population of interest is quite small. The man's non-earned income is similarly significant in his reservation wage equation. The data include three measures of assets: *rooms per person*, which is a metric sometimes used by the World Bank; *lives in shantytown*, an indicator of poverty that should be negatively correlated with unobserved assets; and *lives rent-free* (that is, with a friend or family member), which should also be negatively correlated with financial resources. Of these, both *rooms per person* and *lives rent-free* are practically and statistically significant in the man's reservation wage equation. An increase of one standard deviation centered around the mean in the number of rooms per person is associated with a 27% decrease in the predicted probability that a man applies (from 0.115 to 0.091); living rent-free, which describes about 7% of the population, is associated with a 58% increase in the same (from 0.093 to 0.147). Both estimates suggest that assets have a strong upward effect on the man's reservation wage. Neither is significant in the woman's estimated reservation wage equation.

Variables related to home production are in theory likely to be more important factors for women than for men, since women often have a comparative advantage in those activities. The presence of children or elderly individuals in the household is identified in the data. Having more dependent children is often found to increase the value of an individual's time spent in home

production because children require significant care from adults, while the elderly could affect home production positively or negatively, depending on the amount of help they provide or care they require. For women, the estimated coefficient on *elderly*, a dummy variable indicating the presence of an adult over the age of 65, is significant and positive, suggesting that the elderly require more care than they provide. Having an elderly family member decreases the predicted probability that a woman applies to *Jefas* (again, at values of the other explanatory variables typical for an applicant), by 28%, from 0.192 to 0.138. It does not change the prediction for the man.

Contrary to what is commonly seen in the literature, I estimate that having more school-age children decreases, rather than increases, the woman's reservation wage. For a household of four with two children, adding an additional child age 6-10 (with corresponding increase in household size to 5) raises the predicted probability that a woman applies for *Jefas* employment from 0.177 to 0.208. Similarly, an additional child age 11-15 raises the probability to 0.202. This is likely a function of the specific population being considered.¹⁴ Families debating enrollment in *Jefas* are by and large extremely poor, with many unable to meet even the most basic household financial needs. To the degree that additional children increase the overall cost of living for a given family unit, they may increase the need for both adult members of the household to seek temporary employment for the family to meet its subsistence needs. Additionally,

¹⁴ This is not the first study to find such an effect. Kamitewoko and Jin (2004) see a similar effect among poor women in the Congo, which they attribute to childcare provision by older children.

Lee and Cho (2005) note that high-quality publicly funded education is an important area of public policy in Argentina. Free public schools are widely available and mandatory for children between the ages of 6 and 14, which may facilitate the labor force participation of mothers who have children in the appropriate age range. While imprecisely estimated, the coefficients on child age groups below and above the typical schooling ages reinforce this interpretation, since they are actually estimated to decrease the probability of a woman's participation by an insignificant amount. The estimates of children's effect on the man's reservation wage are not significant for any age groups. Finally, formal marriage is correlated with an increase in both men's and women's reservation wages, which is consistent with previous studies.

1.7 Analysis

Taken together, these estimates begin to explain the high take-up of *Jefas* among partnered women with no prior labor market experience. The work requirement apparently served as a deterrent to the enrollment of primary workers, even in low-income households, who had better market opportunities than *Jefas* employment. At the same time, it did not serve as much of a deterrent to other family members in those same households, for whom market opportunities were relatively poor. Thus, in low-income households that saw a fall in the real wage of the primary earner—as many did during the 2001 crisis—and households whose primary worker was unemployed but had a reasonable

expectation of market success, a secondary worker could have applied for *Jefas* employment in an attempt to maintain household consumption. Evidence in this paper suggests that this was especially true in households with no assets available to help weather the crisis. High take-up by women can be explained by the observation that women are typically secondary workers in Argentine households, as they are in much of the developing world.

This behavioral response appears to be consistent with the added-worker hypothesis, but seems surprising in view of that literature, which seldom finds evidence of an effect. The added-worker effect is often defined as the increase in the labor supply of one household member as the consequence of the unemployment of another member, and is usually viewed as an insurance mechanism. Available evidence from the added-worker literature suggests that the effect is small; presumably the fact that the primary worker becomes unemployed sends a signal of poor job prospects to the secondary worker, who becomes discouraged from seeking employment. In fact, the literature has found virtually no evidence of an added-worker effect in the developing world. Bardhan (1984) finds that the male unemployment rate in India has a strong negative effect on female labor supply, and concludes that a job search discouragement effect outweighs the income effect related to the unemployment of men in the household. Serneels (2004) investigates the added-worker effect in middle-class Ethiopian households using both actual labor supply and a measure of desired labor supply, and also finds that there is no added-worker effect. He concludes

that households have other ways to cope with unemployment, including using savings and selling assets.

A possible reconciliation with this existing literature is that, as a public program not concerned with the skill of its employees, *Jefas* provided a guaranteed outlet for otherwise-unemployable family members' desired labor supply responses during the Argentine crisis, and thus the desired added-worker effect was made visible. The absence of skill or job-experience related screening would certainly have been attractive to inexperienced workers who could not otherwise have found employment at the effective minimum wage. This suggests that an added-worker type response may be visible during crises when employment opportunities are available to absorb the desired labor supply of inexperienced secondary workers. It appears that this response can occur as a result of declining income, even in households that have not experienced a job loss. Further work in this area is needed to lead to a fuller understanding of the mechanisms at play.

1.8 Conclusion

As workfare programs become an important component of poverty alleviation schemes in the developing world, policymakers designing them will wish to know more about the incentives they create for potential labor market participants. This paper contributes to that understanding by explaining why secondary workers in low-income households were drawn to workfare

employment following Argentina's 2001 economic collapse. I find that *Jefas* workfare discouraged participation by active labor market participants but was appealing to other family members who wished to increase their labor supply but had few market opportunities.

Specifically, I estimate that the correlates of household members' expected market wages were particularly important predictors of the workfare application decision. Greater income-earning potential led to lower predicted application probabilities for an individual, while a partner's potential income contributed to a lesser extent. Individuals in households with few assets are also shown to have been more likely to apply for workfare employment. I estimate that having more school-age children increased a woman's probability of applying to the program, which may be due to the provision of free, high-quality public schooling by the government. The implications of this finding for child welfare and women's labor force participation may warrant further inquiry.

This research demonstrates that unskilled, low-wage, public employment is attractive to secondary workers with few other labor market opportunities. Such employment may be useful for households that wish to smooth income during economic downturns in developing countries, even in cases when the primary earner does not suffer unemployment. A possible policy implication is that restricting workfare participation to the unemployed may be misguided if the goal is to transfer income to needy households, as long as a low-wage work component is a strict requirement of continued program participation. I explore

this question further in the second chapter of my dissertation. An important caveat is that this prescription is unlikely to apply in OECD countries, where households have other forms of insurance against unemployment and underemployment.

Figure 1.1 Estimated effect of increasing man's income on the probabilities of application

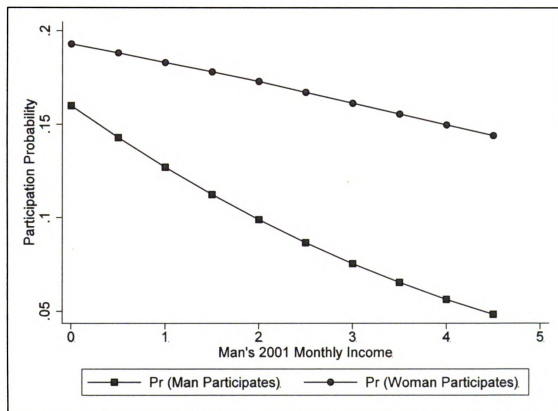


Figure 1.2 Estimated effect of increasing woman's income on probabilities of application

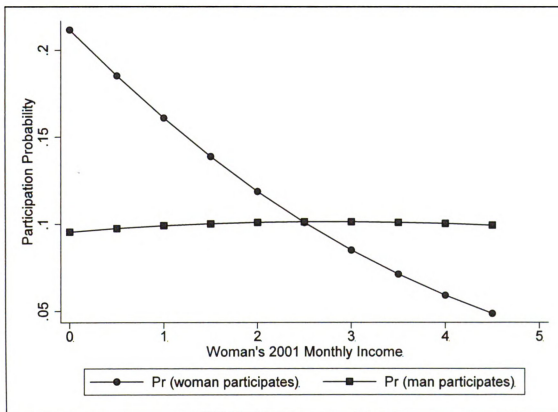


Table 1.1 Percent of All Beneficiaries Complying with Requirements

Child under 18	95.0%
Complies with work requirement	89.3%
Unemployed	62.1%
Head of household	50.3%
No household member employed	43.0%

Source: Encuesta Permanente de Hogares data, collected by INDEC, except compliance with work requirement, which was estimated in MTESS (2002). "Head of Household" status is self-identified in the survey. "Unemployed" means the beneficiary did not report being out of the labor force in the previous survey.

Table 1.2 Data Definitions

Variable	Description
Income & Market Wage	
Earns income	1 if individual earns labor income, 0 otherwise (men and women)
Earned income amt	Amount of earned income per month, measured as pesos/100 (men and women)
Education	Years of formal education (men and women)
Domestic worker	1 if individual is a domestic worker, 0 otherwise (women only)
Self-employed	1 if individual reports income from self-employment, 0 otherwise (men and women)
2001 weekly work hours	Number of hours worked for pay during survey reference week
Household characteristics	
Age	Individual's age in years (men and women)
Size	Number of individuals living in the household
Children	Number of children in the household
Elderly	1 if person age >65 in h'hld, 0 otherwise
Married	Couple is formally married
Eligible	1 if couple has children or cares for a disabled person, 0 otherwise
Assets	
Has non-labor income	1 if individual has non-labor income, 0 otherwise (men and women)
Non-labor income amt	Amount of non-labor income per month, pesos/100 (men and women)
Shantytown	1 if h'hld lives in a shantytown, 0 otherwise
Rent-free	1 if h'hld lives with friend or relative rent-free, 0 otherwise
Rooms per person	Number of rooms divided by number of persons in h'hld
Region Dummies	Four variables indicating geographic regions of the country, Northwest, Northeast, Cuyo, Pampeana. The dummy takes a value of 1 if the household is located in the corresponding region.

All data are from the Encuesta Permanente de Hogares (EPH), INDEC Argentina

Table 1.3 Probit Estimation of the Probability of Participation, Conditional on Applying to *Jefas*

	Coeff.	t-stat
Age 18-24	0.772	3.33
Age 25-29	0.522	2.64
Age 30-39	0.464	2.93
Age 40-49	0.494	3.37
Male	-0.47	-3.1
Head	0.225	1.59
Single	-0.202	-1.14
Married	0.043	0.41
Shantytown	-0.32	-0.15
Apartment	-0.263	-1.37
Rooms	-0.074	-1.67
Bathroom	0.021	0.14
Renter	-0.637	-3.06
Rent-free	-0.177	-1.3
Masonry	-0.054	-0.36
Childratio	0.196	0.73
Elderly	-0.088	-0.57
Size	0.052	2.11
Employed 2001	0.066	0.47
Unemployed 2001	0.566	3.57
Public Employee	0.667	3.11
Teacher	-0.499	-0.91
Construction worker	0.482	2.38
Domestic worker	0.137	0.67
Street vendor	0.633	2.08
Migrant	-0.533	-2.24
Illiterate	0.144	0.56
Observations	903	
Pseudo R2	0.1026	

Note: dependent variable = 1 if individual participated in *Jefas* in October 2002. Panel data from Oct. 2001 and Oct. 2002 EPH.

Table 1.4 Factors Influencing Date of Enrollment for Those Enrolled*Dependent Variable: Month of Application, January - October (1-10)*

	Men	Women	Combined
Man's 2001 earned income	0.0101 [0.1185]	-0.0139 [0.0524]	-0.0004 [0.0458]
Woman's 2001 earned income	-0.3445 [0.1514]**	-0.2784 [0.1962]	-0.2686 [0.1097]**
Partner unemployed in 2002	0.5098 [0.6120]	-0.5072 [0.2511]**	-0.3188 [0.2323]
Man's education	-0.0591 [0.0531]	-0.0365 [0.0429]	-0.043 [0.0333]
Woman's education	-0.0159 [0.0553]	0.0203 [0.0423]	0.0272 [0.0331]
Woman is domestic worker	-0.2415 [0.4769]	-3.8471 [0.7659]***	-0.1005 [0.4080]
Man is self-employed	0.0401 [0.3612]	-0.0827 [0.2226]	-0.0131 [0.1801]
Woman is self-employed	0.2718 [0.6942]	0.517 [0.3584]	0.3468 [0.2925]
Man's 2001 non-earned income	-0.1512 [0.2146]	0.1077 [0.2505]	0.0359 [0.1506]
Woman's 2001 non-earned income	0.0905 [0.2011]	0.2486 [0.2717]	-0.0538 [0.1430]
Rooms per person	0.0621 [0.6069]	0.0404 [0.0269]	0.0308 [0.0270]
Lives in a shantytown	1.1048 [0.5410]**	-0.1285 [0.8215]	0.4434 [0.5080]
Lives rent free	0.2567 [0.4318]	-0.0247 [0.3300]	0.0545 [0.2415]
Household size	0.1524 [0.1663]	0.1051 [0.1034]	0.1416 [0.0773]*
Number of children	-0.466 [0.2089]**	-0.0942 [0.1311]	-0.209 [0.1041]**
Elderly in household	0.0169 [0.6280]	-0.1578 [0.3997]	-0.0604 [0.3356]
Married	-0.2627 [0.3428]	0.1692 [0.2465]	0.0025 [0.1934]
Man's age	-0.0331 [0.0297]	0.0212 [0.0177]	0.0117 [0.0148]
Woman's age	-0.0015 [0.0350]	-0.0218 [0.0205]	-0.0195 [0.0169]
Eligible	1.066 [0.8831]	-1.5019 [0.6094]**	0.3136 [0.5912]
Constant	9.5056 [1.4688]***	5.3225 [1.2936]***	6.7041 [1.1972]***
Observations	130	281	411
R-squared	0.17	0.07	0.05

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: Negative coefficients indicate earlier enrollment date.

Table 1.5 Summary Statistics of Independent Variables by Applicant Status

Characteristic	All Applicant Households		Non-applicant Households		Difference Significant?
	Mean	St. Err.	Mean	St. Err.	
Man earns income	0.72	0.02	0.66	0.01	-2.99***
Man's earned income amt (if employed, 2001)	285.03	8.35	620.05	10.96	13.02***
Woman earns income	0.27	0.02	0.35	0.01	4.00***
Woman's earned income amt (if employed, 2001)	171.89	11.70	459.50	12.38	8.29***
Man's weekly hours of work (if employed, 2001)	43.37	0.90	46.99	0.35	3.96***
Woman's weekly hours of work (if employed, 2001)	28.40	1.54	35.33	0.51	4.49***
Man has non-labor income	0.08	0.01	0.21	0.01	7.68***
Man's non-labor income amt (if positive)	199.96	18.35	494.14	15.58	4.77***
Woman has non-labor income	0.07	0.01	0.10	0.00	3.00***
Woman's non-labor income amt (if positive)	191.27	33.07	372.64	20.15	2.80***
Man's education (years)	7.29	0.12	9.66	0.07	13.64***
Woman's education (years)	7.81	0.13	9.78	0.07	11.38***
Woman is domestic worker	0.06	0.01	0.05	0.00	-0.84
Man is self-employed (2001)	0.39	0.02	0.23	0.01	-8.90***
Woman is self-employed (2001)	0.10	0.01	0.10	0.00	-0.48
Household size	5.50	0.09	4.32	0.03	-13.72***
Number of children (if > 0)	3.00	0.07	2.33	0.03	-9.91***
Rooms per person	0.55	0.03	0.87	0.01	13.89***
Married	0.58	0.02	0.82	0.01	14.47***
Elderly	0.10	0.01	0.29	0.01	10.49***
Man's age	40.98	0.42	49.59	0.23	14.75***
Woman's age	37.52	0.41	46.5	0.22	15.95***
Lives in shantytown	0.05	0.01	0.02	0.00	-5.73***
Lives rent-free	0.14	0.01	0.07	0.00	-6.35***

Data: Encuesta Permanente de Hogares, INDEC, Oct. 2001 and Oct. 2002.

Table 1.6 Summary Statistics of Independent Variables by Gender of Applicant

Characteristic	Couple with Male Applicant		Couple with Fem. Applicant		t=
	Mean	St. Err.	Mean	St. Err.	
Man earns income	0.70	0.03	0.76	0.02	-1.47
Man's earned income (if positive, 2001)	224.27	11.99	314.34	10.57	-5.19***
Woman earns income	0.33	0.03	0.23	0.02	2.54**
Woman's earned income (if positive, 2001)	210.34	22.47	143.84	11.14	2.87***
Man's Weekly Hours of Work (if positive, 2001)	40.37	1.72	44.52	1.05	-2.14**
Woman's weekly hours of work (if positive, 2001)	30.65	2.33	26.83	1.99	1.24
Man has non-labor income	0.09	0.02	0.08	0.01	0.79
Man's non-labor income amt (if positive)	204.85	31.61	196.80	22.68	0.21
Woman has non-labor income	0.09	0.02	0.04	0.01	2.79***
Woman's non-labor income amt (if positive)	229.00	53.61	144.13	30.40	1.29
Man's education	7.34	0.21	7.26	0.15	0.31
Woman's education	7.68	0.23	7.86	0.16	-0.67
Man is self-employed (2001)	0.39	0.03	0.37	0.02	0.52
Woman is self-employed (2001)	0.08	0.02	0.09	0.01	-0.24
Household Size	5.32	0.16	5.62	0.10	-1.60
Number of children (if > 0)	2.89	0.13	3.07	0.09	-1.16
Rooms per person	0.54	0.02	0.55	0.05	-0.03
Married	0.59	0.03	0.60	0.02	-0.20
Elderly	0.14	0.02	0.09	0.01	1.98**
Man's age	41.97	0.80	40.65	0.51	1.43
Woman's age	38.81	0.78	37.10	0.49	1.95*
Lives in shantytown	0.08	0.02	0.04	0.01	2.10**
Lives rent-free	0.17	0.03	0.12	0.02	1.86*

Data: Encuesta Permanente de Hogares, INDEC, Oct. 2001 and Oct. 2002.

Note: Includes households with male and female couple as head and spouse, at least one of whom is an applicant.

Table 1.7 Men's and Women's Reservation Wage Equations

	Men	Women
Man's 2001 earned income	0.2265 [0.0311]***	0.1295 [0.0206]***
Woman's 2001 earned income	0.0317 [0.0394]	0.2476 [0.0513]***
Man's 2001 weekly hours	0.0046 [0.0024]*	0.0029 [0.0020]
Woman's 2001 weekly hours	0.0037 [0.0035]	0.0056 [0.0032]*
Man's education	0.0474 [0.0182]***	0.0864 [0.0160]***
Woman's education	0.0439 [0.0185]**	0.0205 [0.0160]
Woman is domestic worker (2001)	0.0043 [0.1878]	0.6296 [0.2031]***
Man is self-employed (2001)	-0.4794 [0.1108]***	-0.4231 [0.0964]***
Woman is self-employed (2001)	0.0951 [0.1998]	-0.1893 [0.1735]
Man's 2001 non-earned income	0.2873 [0.0652]***	0.2359 [0.0579]***
Woman's 2001 non-earned income	-0.0521 [0.0443]	0.2193 [0.1036]**
Rooms per person	0.3848 [0.2015]*	0.0842 [0.0748]
Lives in a shantytown	-0.3019 [0.2481]	0.2957 [0.2467]
Lives rent free	-0.3718 [0.1559]**	-0.0496 [0.1449]
Household size	0.0292 [0.0524]	-0.0262 [0.0427]
Number of children age 0-5	-0.057 [0.0865]	0.0733 [0.0748]
Number of children age 6-10	-0.1178 [0.0802]	-0.1447 [0.0682]**
Number of children age 11-15	-0.0887 [0.0822]	-0.1132 [0.0685]*
Number of children age 16-18	0.1708 [0.1213]	0.0513 [0.0965]
Elderly in household	0.2228 [0.1771]	0.3154 [0.1597]**
Married	0.1567 [0.1208]	0.1733 [0.1046]*
Man's age	0.0109 [0.0092]	0.0135 [0.0079]*
Woman's age	0.0088 [0.0097]	0.0242 [0.0085]***
Constant	0.3804 [0.5237]	0.1495 [0.4820]
Observations	4298	4298

* significant at 10%; ** significant at 5%; *** significant at 1%.

Asymptotic (inverse, negative Hessian) standard errors are in brackets.

Notes: Data from Oct. 2001 and Oct. 2002 EPH; both equations include region and eligibility dummies. Rho was estimated to be 0.366 with standard error 0.153.

APPENDIX TO CHAPTER ONE

Letting lower-case w 's represent log wages throughout, the two reservation wage equations for each household i are of the form

$$w_M^* = \beta_M^{*'} X + \varepsilon_M^*$$

$$w_F^* = \beta_F^{*'} X + \varepsilon_F^*$$

and serve as latent variables in the model. The *Jefas* wage offer is represented

by w_J^* . The errors ε_M^* and ε_F^* follow a joint normal distribution with mean zero,

fully characterized by

$$\begin{pmatrix} \varepsilon_M^* \\ \varepsilon_F^* \end{pmatrix} \sim N \left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma_M^2 & \rho \sigma_M \sigma_F \\ \rho \sigma_M \sigma_F & \sigma_F^2 \end{pmatrix} \right).$$

To allow for identification of the covariance parameter ρ I set $\sigma_M = \sigma_F$. (For a

more complete explanation of the requirements for identification, see Train

(2003) or Bunch (1990).) Thus,

$$\begin{pmatrix} \varepsilon_M^* \\ \varepsilon_F^* \end{pmatrix} \sim N \left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma^2 & \rho \sigma^2 \\ \rho \sigma^2 & \sigma^2 \end{pmatrix} \right)$$

Normalizing the reservation wage equations and defining $\beta_M = (\beta_M^* / \sigma)$,

$\beta_F = (\beta_F^* / \sigma)$, $w_i = (w_i^* / \sigma)$, and $\varepsilon_i = (\varepsilon_i^* / \sigma)$ the joint density can then be written

$$\phi_2 = \frac{(1 - \rho^2)^{1/2}}{2\pi} \exp \left\{ -\frac{1}{2(1 - \rho^2)} (\varepsilon_M^2 + \varepsilon_F^2 - 2\rho \varepsilon_M \varepsilon_F) \right\}.$$

and the probability associated with condition (2) in the paper is

$$\begin{aligned}
\text{pr } (y_0 = 1) &= \text{pr } (W_J \leq W_M, W_J \leq W_F) \\
&= \text{pr } (\varepsilon_M \geq w_J - \beta_M'X, \varepsilon_F \geq w_J - \beta_F'X) \\
&= \text{pr } (\varepsilon_M \leq \beta_M'X - w_J, \varepsilon_F \leq \beta_F'X - w_J) \\
&= \int_{-\infty}^{\beta_M'X - w_J} \int_{-\infty}^{\beta_F'X - w_J} \phi_2(z_1, z_2) dz_1 dz_2 \\
&= \Phi_2(\beta_M'X - w_J, \beta_F'X - w_J | \rho)
\end{aligned}$$

where Φ_2 is the bivariate standard normal cumulative distribution function. The probability associated with condition (3) in the paper is

$$\begin{aligned}
\text{pr } (y_1 = 1) &= \text{pr } (W_M \leq W_J, W_M \leq W_F) \\
&= \text{pr } (\varepsilon_M \leq w_J - \beta_M'X, \varepsilon_M - \varepsilon_F \leq \beta_F'X - \beta_M'X) \\
&= \text{pr} \left(\begin{pmatrix} \varepsilon_M \\ \varepsilon_M - \varepsilon_F \end{pmatrix} \leq \begin{pmatrix} w_J - \beta_M'X \\ \beta_F'X - \beta_M'X \end{pmatrix} \right)
\end{aligned}$$

with bivariate covariance matrix

$$\begin{pmatrix} 1 & 0 \\ 1 & -1 \end{pmatrix} \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix} \begin{pmatrix} 1 & 1 \\ 0 & -1 \end{pmatrix} = \begin{pmatrix} 1 & 1-\rho \\ 1-\rho & 2-2\rho \end{pmatrix}$$

thus

$$\text{pr } (y_1 = 1) = \Phi_2 \left(w_J - \beta_M'X, \frac{\beta_F'X - \beta_M'X}{\sqrt{2-2\rho}} \mid \frac{1-\rho}{\sqrt{2-2\rho}} \right)$$

The probability associated with condition (4) is likewise

$$\begin{aligned}
\text{pr } (y_2 = 1) &= \text{pr } (W_F \leq W_J, W_F \leq W_M) \\
&= \text{pr } (\varepsilon_F \leq w_J - \beta_F'X, \varepsilon_F - \varepsilon_M \leq \beta_M'X - \beta_F'X)
\end{aligned}$$

$$= \text{pr} \begin{pmatrix} \varepsilon_F \\ \varepsilon_F - \varepsilon_M \end{pmatrix} \leq \begin{pmatrix} w_J - \beta'_F X \\ \beta'_M X - \beta'_F X \end{pmatrix}$$

with associated bivariate covariance matrix

$$\begin{pmatrix} 0 & 1 \\ -1 & 1 \end{pmatrix} \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix} \begin{pmatrix} 0 & -1 \\ 1 & 1 \end{pmatrix} = \begin{pmatrix} 1 & 1-\rho \\ 1-\rho & 2-2\rho \end{pmatrix}$$

thus

$$\text{pr}(y_2 = 1) = \Phi_2 \left(w_J - \beta'_F X, \frac{\beta'_M X - \beta'_F X}{\sqrt{2-2\rho}} \mid \frac{1-\rho}{\sqrt{2-2\rho}} \right)$$

The resulting likelihood function

$$L = \prod_{y_0=1} \text{pr}(y_0 = 1) \prod_{y_1=1} \text{pr}(y_1 = 1) \prod_{y_2=1} \text{pr}(y_2 = 1)$$

is a trinomial probit with correlated errors. The maximum likelihood estimator is consistent, asymptotically efficient, and asymptotically normally distributed.

Table 1.8 Sensitivity Analysis - Main Regression with Only Enrolled Applicants and Non-applicants

	Men	Women
Man's 2001 earned income	0.2462 [0.0413]***	0.1281 [0.0245]***
Woman's 2001 earned income	0.1287 [0.0675]*	0.1895 [0.0621]***
Man's 2001 weekly hours	0.0037 [0.0029]	0.0038 [0.0024]
Woman's 2001 weekly hours	0.0029 [0.0045]**	0.0103 [0.0041]
Man's education	0.058 [0.0222]***	0.0945 [0.0190]***
Woman's education	0.0335 [0.0226]	0.0121 [0.0191]
Woman is domestic worker (2001)	0.0715 [0.2420]	1.8992 [0.4592]***
Man is self-employed (2001)	-0.5702 [0.1338]***	-0.5066 [0.1124]***
Woman is self-employed (2001)	-0.1299 [0.2355]	-0.444 [0.1966]**
Man's 2001 non-earned income	0.2587 [0.0813]***	0.2949 [0.0878]***
Woman's 2001 non-earned income	-0.0212 [0.0745]	0.3236 [0.1649]**
Rooms per person	0.221 [0.2498]	0.059 [0.0792]
Lives in a shantytown	-0.2711 [0.2910]	0.4406 [0.2971]
Lives rent free	-0.3209 [0.1920]*	0.0399 [0.1742]
Household size	0.0072 [0.0611]	0.0177 [0.0520]
Number of children age 0-5	-0.0436 [0.1033]	0.0242 [0.0893]
Number of children age 6-10	-0.0832 [0.0973]	-0.2008 [0.0815]**
Number of children age 11-15	-0.1158 [0.0970]	-0.1646 [0.0810]**
Number of children age 16-18	0.1682 [0.1437]	-0.1289 [0.1124]
Elderly in household	0.325 [0.2272]	0.1655 [0.1934]
Married	0.1702 [0.1453]	0.2371 [0.1216]*
Man's age	0.0174 [0.0117]	0.0103 [0.0093]
Woman's age	0.0044 [0.0125]	0.0317 [0.0102]***
Constant	1.1562 [0.6897]*	1.1539 [0.7295]
Observations	4071	4071

* significant at 10%; ** significant at 5%; *** significant at 1%.

Asymptotic (inverse, negative Hessian) standard errors are in brackets.

Notes: Data from Oct. 2001 and Oct. 2002 EPH; both equations include region and eligibility dummies. Rho was estimated to be 0.366 with standard error 0.153.

Table 1.9 Men's and Women's Reservation Wage Equations Using 2002 Data

	Men	Women
Man's 2002 earned income	0.24 [0.0304]***	0.1459 [0.0198]***
Woman's 2002 earned income	-0.0192 [0.0271]	0.3435 [0.0622]***
Man's 2002 weekly work hours	0.006 [0.0022]***	0.0002 [0.0018]
Woman's 2002 weekly work hours	0.0023 [0.0030]	0.0057 [0.0032]*
Man's education	0.0288 [0.0153]*	0.0505 [0.0135]***
Woman's education	0.0618 [0.0157]***	0.0298 [0.0139]**
Woman is domestic worker	-0.2235 [0.1479]	-0.3936 [0.1516]***
Man is self-employed	-0.4751 [0.0986]***	-0.1566 [0.0873]*
Woman is self-employed	-0.1151 [0.1709]	-0.5569 [0.1644]***
Man's 2002 non-earned income	0.2902 [0.0576]***	0.181 [0.0434]***
Woman's 2002 non-earned income	-0.0015 [0.0571]	0.8118 [0.1651]***
Rooms per person	0.4221 [0.1622]***	0.3583 [0.1430]**
Lives in a shantytown	-0.2929 [0.2173]	-0.1781 [0.2081]
Lives rent free	-0.2124 [0.1330]	-0.2112 [0.1149]*
Household size	0.0453 [0.0489]	-0.0075 [0.0418]
Number of children age 0-5	-0.0442 [0.0787]	0.0304 [0.0680]
Number of children age 6-10	-0.1123 [0.0724]	-0.0904 [0.0618]
Number of children age 11-15	-0.0681 [0.0747]	-0.0425 [0.0627]
Number of children age 16-18	0.0446 [0.1053]	-0.0213 [0.0844]
Elderly in household	-0.2041 [0.1499]	0.4097 [0.1474]***
Married	0.1116 [0.1043]	0.2181 [0.0912]**
Man's age	0.0039 [0.0075]	0.0062 [0.0069]
Woman's age	0.0185 [0.0078]**	0.0141 [0.0072]*
Constant	0.1057 [0.4125]	0.3819 [0.3862]
Observations	9715	9715

* significant at 10%; ** significant at 5%; *** significant at 1%.

Asymptotic (inverse, negative Hessian) standard errors are in brackets.

Notes: Data from Oct. 2002 EPH; both equations include region and eligibility dummies. Rho was estimated to be 0.368, with a standard error of 0.138

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CHAPTER TWO

Design Changes and the Resulting Profiles of *Heads of Household* Beneficiaries

2.1 Introduction

In the first chapter of the dissertation, I developed a model to describe participation in Argentina's Heads of Household public employment program based on individuals' reservation wages, which I estimated as functions of household and individual characteristics. I took the program's design as given and made certain assumptions about how program requirements were enforced or not enforced. In this chapter, I estimate how changing the design and implementation of *Jefas* would have affected the gender composition and poverty level of beneficiaries. In particular, I estimate the effect of two hypothetical changes: removal of the "one person per household" rule, and perfect enforcement of the requirement that applicants be primary earners who lost their jobs during the crisis.

While *Jefas* was originally intended as a way to combat poverty during the economic crisis, its stated goal was to assist primary earners who had lost their jobs as a result of the downturn. Much of the subsequent criticism of the program concerned the failure to achieve this stated goal, in light of the gender and

employment history of many participants. To the degree that lifting the somewhat unusual “one person per household” requirement would have changed the gender composition of applicants, it might have alleviated some of this political distress and re-focused the public’s attention on the goal of poverty alleviation. Moreover, the World Bank has stated that it views the difficulty of enforcing the originally specified eligibility criteria—especially the dual requirements that participants be ‘heads of household’ and ‘unemployed’—as a serious shortcoming of program implementation (World Bank, 2006). At the same time, the Bank acknowledges that *Jefas* compares well to other social assistance programs in Latin America in terms of the share of benefits going to the poor, and coverage of the poor. The Bank takes this as evidence that “the difficulties in enforcing these two eligibility criteria did not adversely affect the performance of the Program in reaching or targeting to the poor.” Yet, this seems naive at best. Had the government taken steps to remedy these issues, the program might have done a much better—or much worse—job of reaching the poor, and there is no reason to expect other socioeconomic attributes of beneficiaries to have remained unchanged. While imperfect information in the hands of program administrators seriously limited the ability to target benefits to heads of household or the unemployed, it is worth assessing whether that would have even been a desirable goal. Public employment programs like *Jefas* are a growing component of social assistance in developing countries (Subbarao et al., 1997), and

information on the optimal design should be helpful to policymakers going forward.

In this study, I find that by allowing any individual to access the program, rather than only one member of each household, the number of male applicants would have increased substantially, by around 46%, while the number of female applicants would have increased by only around 16% assuming the work requirement were still enforced. However, the fraction of applicants who were male would only have increased from 0.34 to 0.39 and the poverty level of applicants would not have dramatically changed. This finding indicates that the “one person per household” requirement was not responsible for the high proportion of female applicants as compared with other programs, and so an explanation of that phenomenon must be found elsewhere. On the other hand, restricting application to primary earners who lost their jobs, while continuing to enforce the work requirement, would have led to a dramatically lower-income, but much smaller, group of applicants, 70% of whom would have been men. The small size of the group of applicants would have rendered *Jefas* largely ineffective as a social assistance program. Enforcement of the work requirement is key to the low income level of this group—overall, primary earners who lost their jobs are wealthier than actual applicants to the Heads of Household program, but still primarily male. These results suggest that, if the goal of *Jefas* was to reduce poverty, the government should not worry about failure to target

benefits to primary earners who lost their jobs, because doing so would not have improved the program's performance.

The remainder of this chapter is structured as follows. First, I briefly describe the current thinking on evaluating workfare programs. Then, I describe how I assess the well being of households, discuss the general methodology I use, and outline the program changes that I study. Finally, I discuss the empirical results, and make recommendations.

2.2 Background on Evaluating Antipoverty Programs

How should a workfare program be evaluated? Two often contradictory considerations—coverage of the poor and exclusion of the non-poor—are the most commonly used measures of the performance of antipoverty programs.¹ Perfect coverage of the poor implies that no member of the poor population should be excluded, and could be achieved by transferring income to every individual in society without regard to income. However, the cost of such a program would often be quite large. Perfect exclusion of the non-poor implies rigid screening to ensure that all participants are eligible and could, in theory, be achieved by not transferring income to anyone. Aside from being costly or difficult to implement, the effectiveness of such a program is sometimes questionable: Many poor

¹ Ravallion and Datt (1995) give a much more comprehensive summary of current thinking on program evaluation. Many of their recommendations are incorporated in this study.

individuals may be excluded or deterred from applying, resulting in a too-small group of beneficiaries to result in much social benefit.

Apart from these most commonly used considerations, the literature also identifies a number of secondary measures of targeting performance. For instance, many evaluations focus on cost-effectiveness; that is, the cost to the government for a given impact on poverty. These sorts of evaluations often take into account the dollar amount of the transfer, as well as administrative costs, to be weighed against the reduction in the poverty rate or the income gains of participants. In these analyses, it is important to take into account the foregone income of participants, which is not always trivial. The appropriate measure of “income gains” should then be the income net of participation costs. For the *Jefas* program, Galasso and Ravallion (2004) have estimated the foregone incomes of participants, and note that they appear positive but quite low. This is consistent with the observation that many participants were drawn from labor market inactivity. Another issue with workfare is that often its proponents implicitly do not value leisure – that is, they define poverty as too little income as opposed to too little utility. Sen (1979) points out that “a previously idle person who takes up workfare employment may be only slightly better off in terms of utility (allowing for the loss of leisure), but much better off in terms of income.” For this reason, some welfarist analyses of workfare, for example Cuff (2000), specify the poverty-alleviation objective in utility terms. As other authors have noted, this sort of perspective appears to carry little weight with policymakers. and if anything,

society often appears to attach positive value to employment. For that reason, in this analysis I primarily evaluate design changes in terms of coverage of the poor and exclusion of the non-poor, but I will address these secondary issues as appropriate.

2.3 General Methodology

In this chapter I will examine two hypothetical changes to the program: removing the requirement that only one person per household is eligible to enroll, and enforcing the requirement that applicants be primary earners who lost their jobs during the crisis. Why these two changes? The one-person-per-household requirement is not a feature of all public employment programs, including Argentina's *Trabajar II*, and may have been partly responsible for the large number of female applicants. Then, the fact that there were so many applicants who were not unemployed primary earners (primarily women) came as a surprise to the government and was viewed negatively by policymakers. Yet, this does not imply that it interfered with the program's original aim of supplementing the incomes of the poor.

In the preceding chapter, I made several assumptions about the way the program was implemented, and I briefly outline them again here in the interest of clarity. First, I assumed that applicants expected the work requirement to be binding, so that their reservation wages were determined by the decrease in household utility from working 20 hours per week. I will retain that assumption in

this chapter, except where I explicitly indicate otherwise. Second, while it appears that applicants were for the most part required to have children or a disabled dependent, I modeled “eligibility” in that sense as a component of the reservation wage, rather than a binding requirement. Unlike the requirement that applicants be unemployed, which was an attempt to identify the poor in the absence of satisfactory data, I perceive the requirement that applicants have children as a political decision about who was most deserving of aid, and so I will not estimate the effect of changing that requirement.

2.3.1 Using Current Income to Measure Well Being

A key issue in evaluating the success of an anti-poverty program at reaching the poor or excluding the non-poor is assessing the counterfactual well being of affected households. According to the World Bank, when programs like *Jefes* are implemented during economic crises, “typically, the main aim of workfare is to raise the current incomes of poor families hurt by the crisis” (Jalan and Ravallion, 1999, p.2). Because household wealth—another characteristic of interest—is so difficult to measure given the data at hand, I follow the World Bank in using current per-capita household income to measure the well being of households. For non-participant households, I use observed 2002 per-capita household income. In households with a *Jefes* participant, current income is endogenous, so I impute values of per-capita income for those households as discussed below.

In the terminology of Little and Rubin (2002), the values of household per-capita income for participant households in the data set are missing for “nonignorable” reasons—that is, the reason the data are missing is correlated with participation in the program, and participants were likely to have experienced larger drops in income than non-participants. Thus, estimates of per-capita income for participant households, derived from relationships observed in other households, are likely to be biased upwards. Fortunately, in this case the missingness can be made “accessible” (Graham and Donaldson, 1993) by comparing participants to applicants not yet receiving benefits. If applicants are an acceptable control group, then the household income of participants can be thought of as “missing at random” (MAR) in comparison with the group of applicants.² When data are MAR, missing data can be thought of as depending on known values of the other variables, and accounting for the values that “cause” the missing data will produce unbiased results in an analysis.

In this case, I impute the missing values using a parametric regression method, in which a regression model is fitted on the group of applicants, and then the fitted model is used to impute missing values based on the observed values of the other variables. Variables are chosen either because they are correlated with the missing variable (e.g. 2001 per-capita household income, which is correlated with 2002 per-capita income), the reason for missingness (e.g. losing one’s job, which is correlated with participation), or both. As opposed to using,

² In the third chapter, I more thoroughly make the case that applicants can indeed be used as a control group for participants.

say, mean values, this method maintains at least the part of the original variability of the missing data that can be predicted from observables. I use both tobit and linear regression models to fit the data. Imputed values from the linear model are trimmed so that they are strictly positive, by assigning all negative values a value of zero. Using the tobit model, income is predicted as the “unconditional” expectation $E(y | x) = \Pr(y > 0 | x) \cdot E(y | x, y > 0)$. Because the tobit model already restricts imputed values to a plausible range, I prefer it *a priori* to the linear model. Alternately, I considered using 2001 per-capita household income as a proxy for 2002 per-capita income for all households including non-participants due to its simplicity. Although the direction of bias using this measure is clear, I will argue later that it is a particularly bad proxy for those who lost their jobs, which is a primary group of interest in this study. Although I present descriptive results for all three measures, I primarily analyze and discuss results in terms of observed (for non-participants) or tobit-imputed (for participants) 2002 per-capita income.

Variables used to fit the model and their coefficients are shown for the tobit model in **Table 2.1** for households headed by couples and **Table 2.2** for households headed by singles, and for the linear model in **Table 2.3** for couples and **Table 2.4** for singles. Variables include previous (2001) income and employment, household attributes, and a dummy variable indicating whether each individual lost their job during the crisis. (The construction of this variable is

explained in detail in a later section of the paper.) All data come from the Encuesta Permanente de Hogares, described in detail in the previous chapter.

On a cautionary note, a possible reason for skepticism regarding imputed per-capita household income is that for some participants, imputed 2002 income is higher than observed 2002 income, which includes the *Jefes* transfer payment. This is the case for 33% of participant households using tobit-imputed income and for 39% of participant households using linear-regression-imputed income. It is possible that for some households income would have been higher in the absence of the program because, for example, participants might have found higher-paying jobs. However, that seems unlikely to be the case for such a large proportion of participants, especially in light of the fact that so many came from out of the labor force, and probably would not have been employed at all if not for the program. Nonetheless, even if the fitted model does a poor job predicting income for any given individual, the analysis that follows will be valid as long as imputed income is representative of actual income on average for relevant subsets of the group of participants.

2.3.2 Participation of Singles

The first chapter was primarily concerned with households headed by couples, because the intent was to explain the large number of partnered women applying for benefits. While those households continue to be the main focus of analysis in this chapter, there may be important changes involving single-headed

households as well—for example, design changes may increase enrollment by single parents at the expense of couples. In order to expand the scope of the analysis to include single heads of household, I must estimate probabilities of participation for that group. Singles' application decision follows the model proposed by Besley and Coate, where an individual applies for workfare if that individual's opportunity cost of time is lower than the wage offered by the government. Following the notation outlined in the first chapter, person i 's log reservation wage is given by:

$$w_i^* = \beta_i^{*'} X + \varepsilon_i^*$$

where w_i^* is the log of the reservation wage and in this case ε_i^* follows a normal distribution with standard deviation σ . As in the first chapter, define w_J as the log of the government's wage offer. Dividing through by σ so that $\beta_i = (\beta_i^* / \sigma)$ and $w_J = (w_J^* / \sigma)$ and rearranging, the probability of participation can be written

$$\text{pr}(y = 1) = \Phi(w_J - \beta_i' X)$$

where Φ is the standard normal CDF. Coefficients are estimated using data from the Oct. 2002 Encuesta Permanente de Hogares, and are presented separately

for men and women in **Table 2.5**. Probabilities of participation are then generated from the fitted model for each individual using Stata's `<predict>` command.

2.3.3 Attributes of Applicants

For households headed by both couples and singles, attributes A_w of the group of applicants estimated under the various scenarios that I study are computed as a weighted average of the attributes A_i of each individual who has a positive probability of applying to the program, as follows, with $y_i = 1$ in the event that an individual participates:

$$A_w = \frac{\sum_i (\text{pr}(y_i = 1) \cdot A_i)}{\sum_i \text{pr}(y_i = 1)}$$

The number of applicants n is estimated from the N observations in the sample as

$$n = \sum_{i=1}^N \text{pr}(y_i = 1)$$

Both the weighted average of attributes and the number of participants are computed separately for men and women because the probability of participation is computed separately.

Unsurprisingly, using the estimated probabilities of participation and weighting procedure to compute the attributes of applicants to the Heads of Household program as it was implemented leads to values that are similar to those of actual observed applicants, with the exception of per-capita household income, which was not used to fit the regression model in chapter 1, and which is over-estimated in all cases. It is these estimated values, given in **Table 2.6**, that I will compare with the estimated values from program design changes to determine the magnitude of the effect. I refer to this estimated group of participants as the comparison group. To the degree that the unobserved characteristics causing bias are similar among various groups, using estimated participants instead of observed participants should make comparisons more accurate.

2.4 Allowing Enrollment by All Individuals

As a way to address budget concerns in light of a 65% poverty rate, program designers limited participation to one individual per household. In this section I present the methodology for estimating the effects of removing that requirement, and opening participation to any interested individual.

Although I no longer restrict participation to one person per household, I will retain the assumption that the work requirement was, in fact, a requirement. That is, individuals are weighing the benefit of enrollment against the cost of working 20 hours per week, with reservation wages defined as in the previous chapter. However, I will now make one additional strong assumption. In the first chapter, the reservation wage of an individual was conditional on that individual's partner not participating in the program. Now, I assume that an individual's reservation wage is unaffected by their partner's participation. I believe that formally, this is equivalent to saying that within a household, the amount the government must pay to employ both individuals, W_B , is the sum of the amount it would have to pay the man, W_M , and the woman, W_F . This is unlikely to be literally true, and depends on the complementarities of home production and leisure among members of the household, as well as the impact of earnings on the amount of time devoted to home production. I will argue that this condition is likely to represent a lower bound on the joint reservation wages of both individuals within a household, and thus the estimated change in participation will represent an upper bound on the increase in the number of applicants, for the following reasons. Recent empirical evidence suggests that husbands and wives are substitutes in home production and that income is at most an imperfect substitute for home production (Leeds and von Allmen, 2004). That is, one member of the couple taking work increases the marginal productivity of the other at home. Through this first effect, enrollment of one family member should serve

to increase the reservation wage of the “second enroller.” Contributing to this upward pressure on the second enroller’s reservation wage is the income effect, through which the demand for leisure of the second enroller should increase due to the increase in family income from participation of the first enroller. However, other research suggests that spousal leisure is complementary, which drives the second earner’s reservation wage in the opposite direction. That is, when one spouse takes work, it makes the other spouse’s leisure time less valuable. It is not obvious in theory which effect should dominate in these circumstances, implying that the reservation wage of the second earner is imprecisely estimated. Nonetheless, it seems reasonable that for poor households during an economic crisis, the estimated increase in participation represents an upper bound, because home production is of more importance to the household than is complementary leisure time.

In the absence of the restriction that only one person per household can participate, enrollment should proceed as it does for single household heads, which is according to Besley and Coate’s (1992) basic model of workfare participation. An individual will apply for benefits if and only if their opportunity cost of time is lower than the wage offered by the government. With a reservation wage estimated as W_i , the probability that an individual applies to the program is $\text{pr}(y = 1) = \text{pr}(W_i < W_j)$, and the probability of declining to apply is $\text{pr}(y = 0) = 1 - \text{pr}(y = 1)$, where $y = 1$ in the event that an individual applies for benefits. Log reservation wages $w_i = w_m, w_f$ are estimated as

$$w_i = \beta_i' X + \varepsilon_i$$

where the coefficients $\beta_i = (\beta_i^* / \sigma)$ were estimated from the full model in the previous chapter and error terms are assumed to be normally distributed. The probability that any individual applies for benefits can then be estimated as

$$\text{pr}(y = 1) = \Phi(w_i - \beta_i' X)$$

This leads to an estimated probability of participation for the man in each household, and for the woman in each household. For single men and women, the estimated probabilities are unchanged from those found using the fitted model in section 2.3.2 above.

Characteristics of the group of expected participants can then be calculated as a weighted average. Results are presented in columns 2 and 5 of **Table 2.7** for couples and will be discussed in section 2.6. There is no change for single men and women.

2.5 Limiting Enrollment to Those who Lost Their Jobs

As *Jefas'* was originally conceived, program benefits were to be available only to the primary earner in a household, and only if that person had suffered a

job loss due to the economic crisis (World Bank, 2002). In this section, I outline a method for estimating the characteristics of beneficiaries assuming the government had been able to successfully target benefits only to this group, while either enforcing or not enforcing the work requirement. Although this requirement would have proved impossible to enforce in practice, it is worth assessing whether it would even have been a desirable goal in light of the government's and Bank's focus on it as a measure of targeting success.

I will estimate the characteristics of primary earners who lost their jobs both under the assumption that the work requirement continued to be enforced, and assuming it had not. Assuming enforcement of the work requirement necessitates generating probabilities of participation for each primary earner, for which I use the estimated probabilities from section 2.3.4. In this case, the assumption that reservation wages are unaffected by a partner's participation is less critical, because most households have only one primary earner.

I identify the primary earner in each household as the individual (head or spouse) who earned the most money in 2001. In households with no reported earnings, an individual is counted as a primary earner if they were unemployed in 2001 and their partner was inactive. In cases where the primary earner is unclear, both individuals are assigned primary status. Using this methodology, men are the primary earner in 65.52% of households, and women in 15.84%. The primary earner is unclear in the remaining 18.64% of households.

The next task is to identify which individuals lost their jobs during the crisis (as there is no way to identify which lost their jobs *due to* the crisis.) This requires two pieces of information: whether an individual had a job prior to the crisis, and whether they were jobless following the crisis. On the first count, I assume that an individual had a job prior to the crisis if that individual reported (1) being employed in 2001 or (2) earning significant positive income (more than 100 pesos monthly) “from working.” (I do this because of the way the employment variable is constructed in the survey – if you didn’t work the previous week, you might not be counted as employed.)

On the second count, I take a relatively comprehensive approach to identifying job loss: I count individuals who reported being without employment (unemployed or inactive) in October 2002 as jobless, as well as individuals who reported being employed as of October 2002 but who have held their job for less than 5 months, meaning they would have been jobless at some point during the *Jefas* application window. Even though *Jefas* participants are all listed as “employed” in the EPH, I count them as employed only if their reported income is greater than 250 pesos per month. This will almost certainly over-count joblessness among *Jefas* participants, as some with very low incomes might have had low-wage employment prior to enrolling, yet after enrollment would be earning the 150 peso minimum. Galasso and Ravallion (2004) estimate that participants suffer very little foregone income, and many participants came from inactivity, so the degree of over-counting among participants is likely small.

Using these criteria, there are 667 male and 344 female primary earners who lost their jobs, including 59.6% of male participants and 20.9% of female participants. As in the previous section, I estimate characteristics of participants with the work requirement enforced as a weighted average, using the probabilities determined from the model in section 2.3.4 above, which assumes that any individual can participate. Results are given in columns 3 and 6 of **Table 2.7** for couples and columns 3 and 6 of **Table 2.8** for single men and women. **Table 2.9** compares the characteristics of primary job-losers with and without the work requirement.

2.6 Discussion

I estimate that lifting the “one person per household” requirement would have increased the number of male applicants in households headed by a couple by 46%, while increasing the number of female applicants in those households by just 16% (**Table 2.7**). However, this would only have changed the fraction of applicants who were male from .34 to .39 in households headed by a male/female pair, while leaving the number and gender ratio of single applicants unchanged.

Additional applicants come from households that are poorer than the average applicant household in terms of per-capita income, but are otherwise remarkably similar. Given an unlimited budget, this would imply better coverage of poor individuals. However, since the budget was limited (with many applicants

turned away), coverage of the poor could not have improved, and the distribution of benefits might have ended up more uneven at the household level, with some households receiving benefits from two participants while others were turned away. With first-come, first-serve enrollment, there would be no way to ensure that those households enjoying additional benefits would be among the worse off in terms of income. Given these considerations, if the goal is to give a minimum income to as many poor households as possible, the results from this study indicate that goal would be best achieved by maintaining eligibility of only one person per household.

On the other hand, assuming the work requirement were still enforced, limiting application to primary earners who lost their jobs would have resulted in a much poorer group of applicants, both among households headed by couples and by single earners (**Tables 2.7 and 2.8**, columns 3 and 6). This group of applicants would also have been overwhelmingly male, with men comprising 70% of likely participants in households with a man and woman.

Limiting enrollment in this way would have been far superior in terms of exclusion of the non-poor, as measured by per-capita income. However, it would come at a non-trivial cost in terms of covering of the poor, with only 33% as many men and 6% as many women likely to have applied for benefits, implying a far weaker safety net. The cost of the program under these circumstances, however, would have been fairly low.

If the work requirement had not been enforced, and benefits had been given to all primary earners who lost their jobs, the number of male applicants would have increased by 277%, and the number of female applicants would have decreased by 50% compared to the group of actual participants (**Table 2.9**). This would have resulted in a group of applicants that was 63% male. Per-capita income would have been somewhat higher in households with a male applicant, while female applicants would have been far better off. On average, applicants would have been less poor.³ Kernel density estimates of the per-capita incomes of this group of applicants, compared with actual applicants, are given in **Figure 2.1**. Due to the huge increase in the number of applicants, coverage of the poor would certainly have been good, but exclusion of the non-poor would have suffered and the monetary cost of the program would have been unsustainable.

Another way for the government to reduce the budget would have been to give benefits to primary earners who lost their jobs, but only if they had children. The characteristics of this group are given in the last two columns of **Table 2.9** for couples, again assuming the work requirement was not enforced. The size of this group is more comparable with, though still larger than, the group of actual applicants, and still heavily male. Household per-capita income is lower for men, but higher for women, compared with the group of actual applicants, and higher on average (116.59 pesos/month compared with 81.76 pesos/month for actual

³ Note that 2001 per-capita income is much higher than observed/imputed 2002 per-capita income for job losers, as would be expected since these are people who, by virtue of losing their jobs, experienced a substantial decline in income. Observed/imputed income is probably the better measure in this case.

applicants). Kernel density estimates of the per-capita incomes of this group of applicants, compared with actual applicants, are given in **Figure 2.2**.

Figure 2.3 compares the per-capita incomes of primary earners who lost their jobs depending on whether or not they have children. As can be seen, those who have children appear worse off. In neither of these scenarios do participants have to work, which may be a benefit in terms of household utility. However, in light of participants' self-reported satisfaction with the work requirement, this should not be a foregone conclusion. According to a survey conducted by Tcherneva and Wray (2005), more female participants reported satisfaction with the program due to the opportunity to "do something" outside the home than for any other reason. When asked whether they would prefer to receive equivalent welfare payments to stay home, the vast majority of women said "no." Thus, it is far from certain the increase in household utility from dropping the work requirement would be very high.

2.7 Conclusion

In terms of the overall goal of the heads of household program, which was to reduce poverty given a limited budget, it does not appear that any of the potential changes outlined in this paper would significantly improve performance. In light of the limited budget, coverage of the poor is best achieved, or at least not significantly harmed, by maintaining the "one-person" requirement, even though the fraction of male participants could be increased somewhat by dropping it.

Increasing the number of male applicants is not, however, a meaningful goal in terms of reducing poverty, and the slight change in the gender ratio of applicants would probably have negligible political benefits. Exclusion of the non-poor would be harmed by targeting benefits to primary earners who lost their jobs, whether or not they had children, unless the work requirement were maintained. In that case coverage of the poor would suffer. Absent purely political considerations, the government should not be too concerned about the failure to target benefits to primary earners who lost their jobs. Compared with the alternatives covered here, the *Heads of Household* program was effective in achieving its original goal of targeting income to the poor.

Figure 2.1 Kernel Density Comparison of Applicants with Primary Job Losers

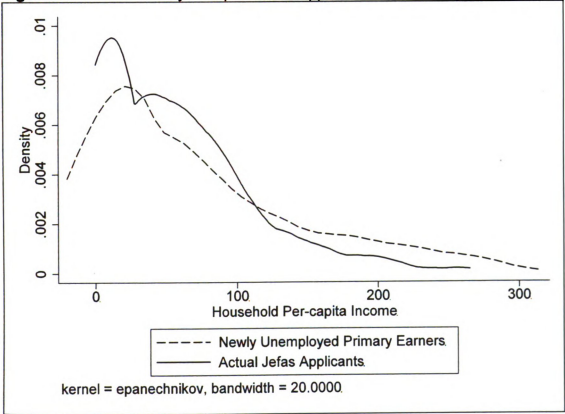


Figure 2.2 Kernel Density Estimate – Primary Job Losers w/ Children

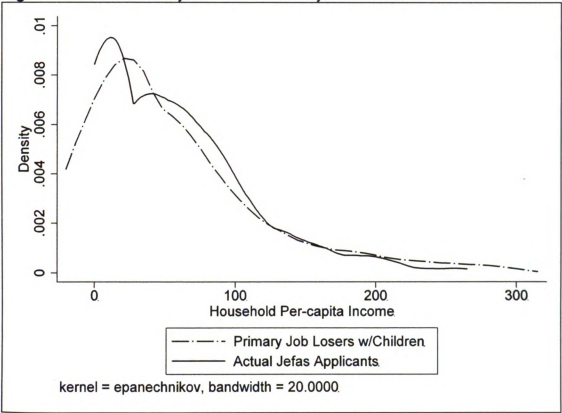


Figure 2.3 Kernel Density Comparison of Job Losers

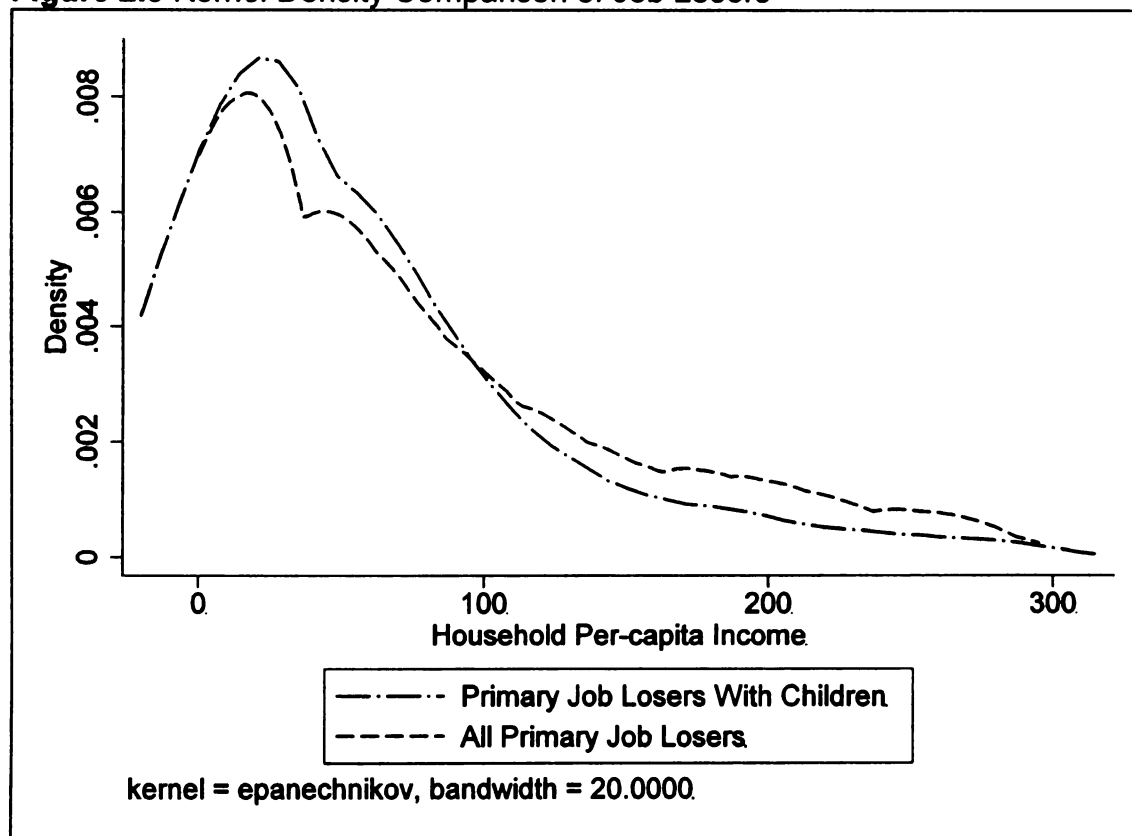


Table 2.1 Tobit Regression Used for Imputing Income for Couples

Dependent variable is 2002 household income per capita

	Coefficient	Std. Error
Household 2001 Income per-capita	0.37	[0.07]***
Number of earners in hhld in 2001	-3.71	[4.26]
Household size	0.97	[4.27]
Man employed in 2001	-25.42	[12.78]**
Man unemployed in 2001	-32.57	[14.33]**
Woman employed in 2001	10.59	[10.25]
Woman unemployed in 2001	-22.63	[16.95]
Man lost job during crisis	-17.34	[7.94]**
Woman lost job during crisis	-13.32	[13.64]
Man's education (years)	-0.68	[1.28]
Woman's education (years)	1.15	[1.20]
woman is domestic worker	-2.93	[11.04]
Man self-employed in 2001	-14.54	[7.15]**
Woman self-employed in 2001	-38.35	[17.67]**
Rooms per person	21.11	[14.50]
Lives in a shantytown	-1.12	[15.93]
Number of children age 0-5	-8.68	[6.17]
Number of children age 6-10	7.17	[5.17]
Number of children age 11-15	-8.27	[5.73]
Number of children age 16-18	-5.39	[9.06]
Elderly in household	7.47	[12.07]
Couple is formally married	18.47	[8.31]**
Man's age	0.59	[0.57]
Woman's age	-0.37	[0.60]
Constant	61.4	[34.89]*
Observations	227	
Number of left-censored observations	23	

* significant at 10%; ** significant at 5%; *** significant at 1%.

Standard errors are in brackets.

Regression includes region dummies

Table 2.2 Tobit Regression Used for Imputing Income for Singles

Dependent variable is 2002 household income per capita

	Coefficient	Std. Error
2001 Household income per capita	0.25	[0.08]***
Number of earners in household (2001)	6.81	[8.49]
Household size	-1.61	[7.10]
Employed in 2001	27.43	[18.06]
Unemployed in 2001	27.05	[16.72]
Weekly work hours in 2001	-0.02	[0.08]
Lost job during crisis	28.20	[18.62]
Education (years)	-2.61	[1.91]
Self-employed in 2001	-4.82	[17.56]
Rooms per person	6.27	[8.78]
Lives in a shantytown	-5.81	[27.86]
Number of children age 0-5	-1.01	[12.99]
Number of children age 6-10	1.18	[11.15]
Number of children age 11-15	2.17	[11.28]
Number of children age 16-18	-11.38	[12.27]
Elderly in household	16.50	[16.28]
Age	-1.00	[0.62]
Constant	94.95	[42.52]**
Observations	96	
Number of left-censored observations	10	

* significant at 10%; ** significant at 5%; *** significant at 1%.

Standard errors are in brackets.

Regression includes region dummies, and a dummy for single men.

Table 2.3 Linear Regression Used for Imputing Income for Couples

Dependent variable is 2002 household income per capita

	Coefficient	Std. Error
Household 2001 income per-capita	0.37	[0.07]***
Number of earners in hhld in 2001	-4.27	[4.20]
Household size	1.35	[4.17]
Man employed in 2001	-24.09	[12.60]*
Man unemployed in 2001	-30.71	[14.08]**
Woman employed in 2001	9.70	[10.03]
Woman unemployed in 2001	-16.53	[16.19]
Man lost job during crisis	-13.79	[7.72]*
Woman lost job during crisis	-11.54	[13.21]
Man's education (years)	-0.40	[1.25]
Woman's education (years)	1.27	[1.17]
woman is domestic worker	-1.69	[10.69]
Man self-employed in 2001	-13.24	[7.00]*
Woman self-employed in 2001	-30.38	[16.81]*
Rooms per person	18.33	[14.09]
Lives in a shantytown	0.71	[15.37]
Number of children age 0-5	-8.25	[6.03]
Number of children age 6-10	5.57	[5.03]
Number of children age 11-15	-7.86	[5.58]
Number of children age 16-18	-5.78	[8.83]
Elderly in household	9.60	[11.71]
Couple is formally married	16.53	[8.11]**
Man's age	0.42	[0.56]
Woman's age	-0.28	[0.58]
Constant	65.16	[34.18]*
Observations	227	
R-squared	0.44	

* significant at 10%; ** significant at 5%; *** significant at 1%.

Standard errors are in brackets.

Regression includes region dummies

Table 2.4 Linear Regression Used for Imputing Income for Singles

Dependent variable is 2002 household income per capita

	Coefficient	Std. Error
2001 Household income per capita	0.25	[0.09]***
Number of earners in household (2001)	2.53	[8.53]
Household size	-0.38	[7.14]
Employed in 2001	20.87	[18.41]
Unemployed in 2001	23.42	[17.08]
Weekly work hours in 2001	0.00	[0.08]
Lost job during crisis	28.23	[19.29]
Education (years)	-2.23	[1.94]
Self-employed in 2001	-2.94	[18.14]
Rooms per person	7.06	[9.03]
Lives in a shantytown	-6.00	[28.91]
Number of children age 0-5	-0.32	[13.28]
Number of children age 6-10	-2.82	[11.47]
Number of children age 11-15	2.07	[11.22]
Number of children age 16-18	-11.63	[12.47]
Elderly in household	12.19	[16.76]
Age	-0.80	[0.64]
Constant	94.18	[43.94]**
Observations	96	
R-squared	0.48	

* significant at 10%; ** significant at 5%; *** significant at 1%.

Standard errors are in brackets.

Regression includes region dummies, and a dummy for single men.

Table 2.5 Single Men's and Single Women's Reservation Wage Equations

	Men	Women
2001 monthly earned income	0.3124 [0.0728]***	0.1278 [0.0345]***
2001 weekly hours of work	0.0003 [0.0031]	-0.001 [0.0009]
Education	0.0703 [0.0242]***	0.0629 [0.0170]***
Woman is domestic worker		-0.1472 [0.1460]
Self-employed (2001)	0.0677 [0.2128]	-0.313 [0.1640]*
2001 monthly non-earned income	0.5704 [0.1359]***	0.1794 [0.0488]***
Rooms per person	0.0629 [0.0954]	0.1473 [0.1122]
Lives in a shantytown	0.1591 [0.4117]	-0.0145 [0.3562]
Lives rent free	0.1565 [0.2154]	-0.084 [0.1734]
Household size	0.5358 [0.1206]***	0.0934 [0.0580]
Number of children age 0-5	-1.0092 [0.2623]***	-0.1229 [0.1003]
Number of children age 6-10	-0.875 [0.2243]***	-0.1855 [0.0913]**
Number of children age 11-15	-0.5857 [0.2875]**	-0.2219 [0.0877]**
Number of children age 16-18	-0.7319 [0.3627]**	-0.2473 [0.1152]**
Elderly in household	-0.6591 [0.2651]**	-0.0277 [0.1762]
Age	0.0399 [0.0084]***	0.027 [0.0059]***
Constant	-3.1906 [0.6208]***	-0.1011 [0.4914]
Observations	1173	1597

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors are in brackets.

Notes: Estimates are conditional on having at least one child or disabled individual in the household, using 2001 EPH data. Coefficients were estimated in two separate regressions. A positive sign on the coefficient should be thought of as increasing the reservation wage, which corresponds to a decrease in the probability of applying.

Table 2.6 Comparison of Estimated and Actual Applicants

	Partnered Men		Partnered Women	
	Estimated	Actual	Estimated	Actual
Number of observations	4298	4298	4298	4298
Number of applicants	212	213	409	410
Per-capita hhld income (tobit)	79.76	54.26	76.19	58.44
Per-capita hhld income (linear)	78.98	52.83	75.29	56.85
Per-capita hhld income (2001)	70.84	71.15	70.77	68.38
Education	7.31	7.34	7.83	7.85
Rooms per person	0.54	0.54	0.54	0.55
Lives in villa	0.07	0.08	0.04	0.04
Number of children	2.67	2.66	2.97	2.99
... age 0-5	0.91	0.91	0.88	0.89
... age 6-10	0.82	0.82	0.94	0.94
... age 11-15	0.66	0.66	0.80	0.80
... age 16-18	0.27	0.27	0.34	0.34
Married	0.59	0.59	0.60	0.60
Own weekly hours (2001)	27.04	26.91	6.75	6.61
... if positive	41.37	40.37	30.97	26.83
Own earned income (2001)	159.40	157.93	32.90	33.68
... if positive	237.24	224.27	159.71	143.84
Fraction earning income	0.67	0.70	0.21	0.23
Own age	41.91	41.97	37.02	37.09
Elderly in household	0.13	0.14	0.09	0.09

Note: Per-capita household income is imputed for participants

Table 2.6 (continued) Comparison of Estimated and Actual Applicants

	Single Men		Single Women	
	Estimated	Actual	Estimated	Actual
Number of observations	1173	1173	1597	1597
Number of applicants	62.99	63	149.68	150
Per-capita hhld income (tobit)	128.54	93.91	88.52	73.91
Per-capita hhld income (linear)	128.44	94.41	88.11	73.15
Per-capita hhld income (2001)	90.92	93.56	81.52	73.43
Education	6.93	7.05	7.87	7.91
Rooms per person	1.24	1.21	0.73	0.72
Lives in villa	0.04	0.05	0.04	0.04
Number of children	0.76	0.81	2.64	2.63
... age 0-5	0.27	0.29	0.67	0.67
... age 6-10	0.28	0.29	0.72	0.73
... age 11-15	0.13	0.14	0.79	0.78
... age 16-18	0.08	0.10	0.46	0.46
Married				
Own weekly hours (2001)	25.45	25.67	17.98	18.38
... If positive	46.51	38.50	45.31	55.13
Own earned income (2001)	83.69	83.57	102.47	99.78
... If positive	150.94	146.25	195.52	182.52
Fraction earning income	0.55	0.57	0.52	0.55
Own age	43.04	42.76	41.89	41.96
Elderly in household	0.24	0.24	0.16	0.15

Note: Per-capita household income is imputed for participants

Table 2.7 Comparison of Applicants in Households Headed by Couples
(Work requirement enforced)

	Male Applicants				Female Applicants			
	Actual (estimated)	No Partner Restriction	Primary Earner Lost Job		Actual (estimated)	No Partner Restriction	Primary Earner Lost Job	
Number of observations	4298	4298	588		4298	4298	205	
Implied # applicants	212	309	69		409	476	29	
Hhld per-capita income (tobit)	79.76	73.54	44.97		76.19	74.15	54.83	
Hhld per-capita income (linear)	78.98	72.61	43.10		75.29	73.22	52.89	
Hhld per-capita income (2001)	70.84	65.32	58.85		70.77	68.37	53.77	
Education	7.31	7.10	6.65		7.83	7.75	7.99	
Rooms per person	0.54	0.523	0.47		0.54	0.53	0.56	
Lives in villa	0.07	0.07	0.08		0.04	0.04	0.07	
Number of children	2.67	2.83	3.01		2.97	2.98	2.83	
... age 0-5	0.91	0.95	1.14		0.88	0.90	0.77	
... age 6-10	0.82	0.89	0.89		0.94	0.94	0.81	
... age 11-15	0.66	0.72	0.73		0.80	0.74	0.89	
... age 16-18	0.27	0.28	0.24		0.34	0.33	0.36	
Married	0.59	0.56	0.48		0.60	0.59	0.61	
Own weekly hours	27.04	27.68	37.16		6.75	6.84	28.40	
... if positive	41.37	41.29	38.71		30.97	30.74	30.23	
Own earned income (2001)	159.40	162.67	204.20		32.90	34.11	144.14	
... if positive	237.24	233.99	212.71		159.71	160.70	154.99	
Fraction earning income ('01)	0.67	0.70	0.96		0.21	0.21	0.93	
Own age	41.91	41.00	39.34		37.02	36.89	38.01	
Elderly in household	0.13	0.11	0.11		0.09	0.09	0.08	
Fraction Male:	0.34	0.39	0.7					

Notes: Data are from 2001 and 2002 EPH. Reported values are a weighted average of the characteristics of individuals having a positive probability of applying to the program.

Table 2.8 Comparison of Applicants in Households Headed by Single Men and Women
(work requirement enforced)

	Male Applicants			Female Applicants		
	Actual (estimated)	No Partner Restriction	Primary Earner Lost Job	Actual (estimated)	No Partner Restriction	Primary Earner Lost Job
Number of observations	1173	1173	156	1597	1597	186
Implied # applicants	62.99	62.99	14.31	149.68	149.68	40.41
Hhld per-capita income (tobit)	128.54	128.54	99.12	88.52	88.52	75.59
Hhld per-capita income (linear)	128.45	128.45	97.55	88.11	88.11	74.43
Hhld per-capita income (2001)	90.92	90.92	98.93	81.52	81.52	64.40
Education	6.93	6.93	6.42	7.87	7.87	7.82
Rooms per person	1.24	1.24	1.09	0.73	0.73	0.66
Lives in villa	0.04	0.04	0.06	0.04	0.04	0.06
Number of children	0.76	0.76	1.55	2.64	2.64	2.81
... age 0-5	0.27	0.27	0.61	0.67	0.67	0.73
... age 6-10	0.28	0.28	0.71	0.72	0.72	0.87
... age 11-15	0.13	0.13	0.15	0.79	0.79	0.78
... age 16-18	0.08	0.08	0.07	0.46	0.46	0.43
Own weekly hours (2001)	25.45	25.45	28.51	17.98	17.98	22.22
... if positive	46.51	46.51	46.18	45.31	45.31	59.12
Own earned income (2001)	83.69	83.69	123.04	102.47	102.47	127.83
... if positive	150.94	150.94	129.17	195.52	195.52	141.41
Fraction earning income ('01)	0.55	0.55	0.95	0.52	0.52	0.90
Own age	43.04	43.04	38.56	41.89	41.89	38.45
Elderly in household	0.24	0.24	0.18	0.16	0.16	0.09
Fraction Male:	0.30	0.30	0.26			

Notes: Data are from 2001 and 2002 EPH. Reported values are a weighted average of the characteristics of individuals having a positive probability of applying to the program.

Table 2.9 Comparison of Primary Earners Who Lost Jobs (Couples)

Participant is:	Actual (estimated) Work Requirement		Primary Earner Lost Job Work Requirement	
	Man	Woman	Man	Woman
Number of observations	4298	4298	588	205
Implied # applicants	212	409	69	29
Hhld per-capita income (tobit)	79.76	76.19	44.97	54.83
Hhld per-capita income (linear)	78.98	75.29	43.10	52.89
Hhld per-capita income (2001)	70.84	70.77	58.85	53.77
Education	7.31	7.83	6.65	7.99
Rooms per person	0.54	0.54	0.47	0.56
Lives in villa	0.07	0.04	0.08	0.07
Number of children	2.67	2.97	3.01	2.83
... age 0-5	0.91	0.88	1.14	0.77
... age 6-10	0.82	0.94	0.89	0.81
... age 11-15	0.66	0.80	0.73	0.89
... age 16-18	0.27	0.34	0.24	0.36
Married	0.59	0.60	0.48	0.61
Own weekly hours	27.04	6.75	37.16	28.40
... if positive	41.37	30.97	38.71	30.23
Own earned income (2001)	159.40	32.90	204.20	144.14
... if positive	237.24	159.71	212.71	154.99
Fraction earning income ('01)	0.67	0.21	0.96	0.93
Own age	41.91	37.02	39.34	38.01
Elderly in household	0.13	0.09	0.11	0.08
Fraction Male:	0.34		0.70	

Notes: Data from 2001 and 2002 EPH. Reported values are a weighted average of the characteristics of individuals having a positive probability of applying to the program.

Table 2.9 (continued) Comparison of Primary Earners Who Lost Jobs (Couples)

Participant is:	Primary Earner Lost Job No Work Requirement		Primary Earner Lost Job No Work / Has Children	
	Man	Woman	Man	Woman
Number of observations	588	205	457	148
Implied # applicants	588	205	457	148
Hhld per-capita income (tobit)	88.86	129.28	62.19	91.18
Hhld per-capita income (linear)	88.22	128.66	61.37	90.35
Hhld per-capita income (2001)	137.31	132.89	98.48	102.90
Education	8.07	8.49	7.91	8.61
Rooms per person	0.66	0.78	0.54	0.62
Lives in villa	0.04	0.04	0.05	0.06
Number of children	2.17	1.76	2.79	2.44
... age 0-5	0.71	0.48	0.91	0.67
... age 6-10	0.62	0.42	0.80	0.58
... age 11-15	0.58	0.56	0.75	0.78
... age 16-18	0.26	0.30	0.34	0.41
Married	0.69	0.77	0.65	0.72
Own weekly hours	42.61	31.67	42.21	32.10
... if positive	43.93	33.46	43.52	34.18
Own earned income (2001)	376.38	232.74	358.69	240.07
... if positive	388.02	258.6	369.79	258.14
Fraction earning income ('01)	0.97	0.90	0.97	0.93
Own age	44.79	45.02	41.57	41.31
Elderly in household	0.20	0.28	0.10	0.16
Fraction Male:	0.63		0.67	

Notes: Data from 2001 and 2002 EPH. Reported values are a weighted average of the characteristics of individuals having a positive probability of applying to the program.

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CHAPTER THREE

The Impact of *Jefes y Jefas de Hogar* on Children's Work and School Attendance

"As long as there is family poverty, there will be child labor."

-UNICEF

3.1 Introduction

Many organizations that deal with child labor, including the International Labor Affairs Bureau (ILAB) at the U.S. Department of Labor, argue that outright bans on child labor may be counterproductive. Rather, they propose that helping families alleviate poverty is one of the best ways to address the issue of child labor in developing countries. Conditional Cash Transfer (CCT) programs are designed to address such issues explicitly, for example by requiring participants' children to attend school as a condition of receiving benefits. The Department of Labor believes public employment programs like *Jefes y Jefas de Hogar* in Argentina, despite their lack of conditionalities, may also have a significant impact on the incidence of child labor in those countries by decreasing households' need for additional income. This view is consistent with theoretical work done by Basu and Van (1998), who argue that the primary cause of child

labor is parental poverty, and empirical work by Edmonds (2005), who finds that economic status improvements can explain declines in child labor.

At the same time, child labor is rarely included as an outcome variable in impact evaluations of non-CCT development programs, and the effect of Argentina's *Jefas* program on child labor has not yet been evaluated (Sipos and Lyon, 2009). This study directly evaluates *Jefes'* impact on child labor and school attendance, using a comparison group of children whose parents applied to the program but did not receive benefits because they applied after what turned out to be the cutoff date. I develop both semiparametric and parametric estimates of the program's impact, because they require different assumptions, and reveal different information about the relationship between the treatment and outcomes of interest. Using cross-sectional and panel data, I find that children age 10-14 whose parents enrolled in the *Jefas* program and received benefits were 59 percent (1.3 percentage points) less likely to report working, and 38 percent (2 percentage points) less likely to forgo schooling, compared with similar children whose parents were not enrolled.

The remainder of the chapter is structured as follows. I describe relevant details of child work and school attendance in Argentina in section 3.2. Section 3.3 outlines the methodology for estimating treatment effects using cross-sectional data, and section 3.4 does the same for panel data. I describe the data and descriptive statistics in section 3.5. In section 3.6, I present and describe results. Section 3.7 concludes.

3.2 Child Work, Schooling, and *Jefas* in Argentina

Argentina's 2001-2002 economic crisis caused a substantial decline in the real incomes of many workers, and a doubling of poverty and extreme poverty. According to McKenzie (2004), most workers were unable to draw from savings, and unable to increase their work hours, as a way to mitigate the effects of the crisis. One possible labor market response, outlined in the first chapter, would be for affected families to send other individuals, including children, into the workforce to compensate for the decline in household income. Indeed, while estimates of child work in Argentina vary considerably, news sources at the time of the crisis reported an increase in child work (Palacios, 2003) and corresponding decline in school attendance (Hennigan, 2003).¹ Likewise, UNICEF (2003) reported a 600 percent increase in child work between 1995 and 2003, with 40 percent of those children who worked abandoning school. ILO-IPEC (2002) reported an increase in child work between 1997 and 2002, which it attributes to the economic crisis. To my knowledge, there have been no formal economic studies of this phenomenon.

Urban child labor is a visible problem in Argentina, with children working in jobs including trash recycling, street sales, begging, selling trinkets on the subway, and shoe shining (CONAETI, 2009). Seeing young children engaged in

¹ In 2001, the ILO estimated that 2.2 percent of urban children ages 10 to 14 years in Argentina were working, and in 2002, the Ministry of Labor estimated that 7.1 percent of children ages 5 to 14 were working, including in rural areas. In 2002, a UNICEF representative reported that in urban areas 6 of every 10 children ages 13 to 17 were working rather than studying.

difficult labor, often late into the night, is a daily occurrence in Buenos Aires, and even tourists will encounter the problem. This happens even though it is not legal: According to ILAB (2009), Argentine law prohibits employment for “minors who have not completed compulsory education, which normally ends at age 15, and with few exceptions bars employment of children under the age of 14 outright. Children age 14-18 require special permission from administrative authorities to work. In addition, children ages 14 to 18 are prohibited from working more than 6 hours per day, and must present medical certificates attesting to their ability to perform such work. Children under the age of 18 are prohibited from working between the hours of 8pm and 6am in work that could endanger their safety, health, or moral integrity.” It appears that these laws are at best loosely enforced, at least outside the formal sector.

One of the goals of the *Jefas* program was to help children, and the program was explicitly targeted to families with minor dependents, because children were perceived as being among the most vulnerable during the crisis. In the media, many of the complaints about the program stemmed from the large number of mothers participating instead of staying at home with their children, which was perceived to harm those children. However, it is possible that by allowing the mother to earn income, a goal she may have been unable to achieve in the market, the program may have significantly alleviated the need for a child to fill that role.

The primary goal of this study is to assess the effect of the *Jefas* program on the incidence of child work. However, a child who is not working may engage in any number of other activities, including substituting for the foregone home production of parents who enroll in *Jefas*. For that reason, I will evaluate whether there is a corresponding increase in school attendance. Argentina has one of the best public education systems in Latin America, and education is free and compulsory for 10 years, beginning at age 5.² According to Argentina's Census and Statistics Institute (INDEC, 2001), in 2000 more than 90 percent of children who enrolled in primary school in Argentina reached grade five and 79 percent completed primary education, which comprises grades 1-9 (ages 6-14).

3.3 Semiparametric and Parametric Estimation of Treatment Effects

This study addresses the impact of the *Jefas* program on child work and school attendance. It is common practice in the evaluation literature to define the "impact" of a social assistance program as the difference between some measure of the outcome of interest with the program and its counterfactual value for participants in the absence of the program, where the estimate of the counterfactual is based on a matched comparison group of non-participants. Such estimators are semiparametric in the sense that only assignment to the treatment group is modeled parametrically. Estimating treatment effects in this

² See Ministerio de Cultura y Educacion de la Nacion, "Argentine Education in the Society of Knowledge." Available at <http://www.zona.lacarabela.com/zona98/EASC/eng/home.html>

manner, outlined by Rosenbaum and Rubin (1983), is appealing in cases where (1) assignment to the treatment group can be made plausibly random conditional on observed covariates, and (2) there is sufficient overlap in the conditional probabilities of treatment between the treated and comparison groups. In this study, I use as the treated group the sample of children with a parent who is enrolled in the *Jefas* program and is receiving benefits. I will refer to members of this group as “participants.” The comparison group is the sample of children with a parent who had applied to the program, but was not receiving benefits, at the time of the October 2002 survey. I will call members of this group “applicants.” Applicants are similar to participants in that, although they are not receiving benefits, they have indicated a preference for program participation. Galasso and Ravallion (2004) argue that applicants and participants are sufficiently similar that assignment to the treatment group can be treated as random conditional on observed characteristics. I will show that the children of applicants and participants can be treated in the same manner.

In chapter 1, I outlined some differences between participants and applicants—for example, that participants were more likely to have come from larger households or to have been employed in construction prior to the crisis than were applicants (see **Table 1.3**). To control for this sort of observable heterogeneity, I construct a counterfactual outcome from the group of applicants using their propensity scores, or probabilities of participation conditional on observed covariates. I estimate propensity scores parametrically by fitting a

probit model of participation (i.e. assignment to the treatment group), and using the fitted model to predict the propensity scores.

Once the propensity score for each individual has been estimated, I estimate counterfactual outcomes of working and school attendance for children of participants in the absence of the program from the outcomes for children of applicants not yet receiving the program by taking weighted averages over outcomes for individuals in the latter group, who are observationally similar to the participants in terms of their propensity scores. There are N children of participants indexed $i = 1, \dots, N$ and P children of applicants indexed $j = 1, \dots, P$. Let Y_i^K be the outcome of interest for individual i in state K , where $K = T$ for participants (the treated group) and $K = C$ for applicants (the control group). Applying weights W_{ij} to calculate the counterfactual for each participant, the estimate of the mean impact is

$$\frac{1}{N} \sum_{i=1}^N \left(Y_i^T - \sum_{j=1}^P W_{ij} Y_j^C \right)$$

where for each i

$$\sum_{j=1}^P W_{ij} = 1$$

Consistent with much of the impact evaluation literature, I use local linear weights, which are constructed using all individuals in the control group and which have been found to perform better at the boundaries of the propensity score. Local linear matching is competitive with other estimators in terms of bias

so long as there is good common support, and achieves lower variance than many estimators because more information is used (Busso, et al. 2009). Matching is implemented in Stata using `<psmatch2>` with a default bandwidth of 0.8 (Leuven and Sianesi, 2003). Standard errors are bootstrapped with 100 repetitions.

Because I believe conditions (1) and (2) above are likely to be satisfied (which I will verify later in the paper) the propensity-matched estimate of the average treatment effect on the treated (ATT) will be my preferred estimate of the *Jefas* treatment effect. However, for comparison with the semiparametric approach outlined above, I find it useful to also parametrically estimate the program's impact on the outcomes of interest using a nonlinear model. Whereas semiparametric estimation requires only that assignment into the treatment group be random conditional on observable characteristics, traditional parametric estimation of treatment effects requires assumptions about the full relationship between the observed characteristics and outcomes of interest. If such assumptions can be reasonably made, parametric regression estimates can yield additional insight, unavailable in the context of semiparametric estimation, into the possible causal effect of covariates other than program participation on the outcomes. In this study I estimate probit models of the effect of participation on child work and school attendance, in which concerns about unobserved differences governing selection into the group of participants is mitigated by using as a comparison the group of children whose parents are applicants, and whose

parents have thus indicated a preference for participation, as above. If selection into the treatment group is sufficiently random (as will be suggested by the low explanatory power of the probit used to compute the propensity score), then this analysis should be appropriate. The primary coefficient of interest in the probit models is the one indicating the effect of having a parent who is a participant, and the standard error on that coefficient is a rough approximation of the statistical certainty of the average treatment effect on the treated. To construct an estimate of ATT, I use the fitted probit to predict the probability of the outcome variable for each treated observation, and find the average. To estimate the counterfactual outcome, I recalculate the predictions with the treatment dummy set to zero, and again take the average. The difference in averages is the estimate of ATT. To illustrate, let x_K be a dummy variable representing treatment. After fitting the model to estimate $\hat{\beta}_0 \dots \hat{\beta}_k$ on the whole sample, ATT is estimated using only the sample of participants as

$$\frac{1}{N} \sum_{i=1}^N \left[\Phi(\beta_0 + \hat{\beta}_1 x_{i1} + \dots + \hat{\beta}_{k-1} x_{i,k-1} + \hat{\beta}_k) - \Phi(\beta_0 + \hat{\beta}_1 x_{i1} + \dots + \hat{\beta}_{k-1} x_{i,k-1}) \right]$$

3.4 Estimation of Treatment Effects for the Longitudinal Sub-sample

One possible source of concern about the cross-sectional estimates of the average treatment effect described above is that there might be pre-existing (before treatment) differences in the average reported child work and school

attendance rates between the treatment and control groups, which would bias the estimate of the treatment effects. This could happen if selection into the group of participants was caused by some unobservable trait which is also correlated with work or school attendance—for instance, if children of participants were working more than children of applicants before the crisis, because the families of eventual participants were worse off, applied first, and were thus more likely to have been selected into the program.

For this reason, I re-estimate semiparametric estimates of ATT using the sub-sample of children for whom there is panel data from before the crisis. Weighted averages of all covariates are computed again for this group, using the propensity scores that were previously computed using the entire sample. I then compare the weighted averages of the pre-crisis values of the dependent variables to verify that, after weighting, there are no significant differences in those values between the treatment and control groups that would be of concern.³

3.5 Data and Descriptive Statistics

As in the first chapter, I use data from the October 2001 and October 2002 rounds of the *Encuesta Permanente de Hogares* (EPH), which covers urban areas accounting for 70% of the Argentine population. These data identify *Jefas*

³ A common way to address pre-treatment differences in the dependent variables due to unobservables is to construct difference-in-difference (DD) estimates of ATT. I outline this approach, and construct DD estimates of ATT for both outcome variables as a robustness check, in the appendix to this chapter.

participants, as well as individuals who applied to the program but were not yet receiving benefits, whom I call “applicants.” In the first chapter of the dissertation, I showed that the observed characteristics of participants and applicants are remarkably similar, which I believe makes the group of applicants a suitable control group for program participants. This was the approach followed by Galasso and Ravallion (2004) in their analysis of foregone incomes.

In this paper, the individuals of interest are children under the age of 15, each having a parent who is either a participant (the “treatment group”) or applicant (the “control group”). Because data are unavailable in the EPH for children under the age of 10, the sample will be children age 10-14.⁴ Descriptive statistics for this group, which includes 2,307 individual children in 1,536 households, are given in **Table 3.1**, where they are divided into two columns depending on the application status of the parent. In the 2001 cross-section, there are 1706 children with parents who are participants, and 601 with parents who are applicants. Despite their similarities, there are some significant differences in the explanatory characteristics of participants and applicants, which motivates propensity matching to control for these observed differences.

The dependent variables that I use are dummy variables indicating whether the child reports working (during the survey week), and whether the child reports attending school. Both characteristics differ significantly between participants and applicants. Children of participants appear about half as likely to

⁴ The results are sensitive to inclusion of older children. Propensity-matched estimates of ATT using children up to age 16, which are smaller in magnitude and statistically insignificant, are given in **Table 3.9**.

work (0.9 percent of the children of participants vs. 2.0 percent of the children of applicants), and somewhat more likely to attend school. Other observed characteristics are similar, as would be expected if assignment to the treatment group were truly random, yet there are some significant differences. Participant households are larger and have more children, for example. For that reason, I will use propensity matching to balance the distribution of observables between the two groups. With respect to published estimates of child work in Argentina's urban areas, it also appears that child work is under-reported in the EPH. This will be a problem only to the degree that under-reporting varies between children of applicants and children of participants.⁵

To find the estimates of the average treatment effect on the treated for the longitudinal sample, I use the 2001-2002 panel of children who are age 10-14 in 2002, which includes data on 1034 individuals. Because subsequent interviews are conducted at the same residence, rather for the same family, there is no guarantee that panel observations will be of the same individual child in 2001 and 2002. For that reason, I verify age and sex for consistency, dropping observations for which sex changes between 2001 and 2002, or for which age does not increase by one year. This leaves 980 individuals in the panel data set. Among the non-participant households observed in this data, the overall percentage of children working tripled from 0.84% in 2001 to 2.52% in 2002, while the percentage attending school fell from 98.32% to 94.54%. The observed

⁵ The most likely reason for this to occur would be if, despite the survey's guarantee of anonymity, children of participants or their parents (they answer the survey together) were less likely to report child work for fear of being dropped from the program.

overall increase in working and decline in school attendance is consistent with anecdotal accounts that some families sent children to work as a way to cope with lost income during the crisis. There are, additionally, no statistically significant pre-treatment differences in either child work or school attendance between the treatment and control groups, although the children of participants were 0.2 percentage points more likely to have reported working in 2001 and 0.2 percentage points more likely to have attended school. The observation that there were no significant pre-treatment differences helps allay concerns about such differences causing bias in the estimate of ATT.

3.6 Results and Discussion

The probit model used to generate propensity scores for participants and applicants is given in **Table 3.2**. Certain variables are significant in this regression—children from larger households, as well as households with a female applicant or in which the household head is an applicant, do appear somewhat more likely to be in the group of participants, which is consistent with the descriptive statistics. Nonetheless, the probit has relatively low explanatory power, which is consistent with both the historical reason for applicants not receiving benefits due to an arbitrary cutoff date, and with the clear *ex ante* similarity of observable characteristics between applicants and participants. With such similar treatment and control groups, it is not surprising that there is a large region of overlapping support, shown graphically in **Figure 3.1**. There is also no

mass near the corners, which implies consistency of the propensity-matched estimator. Because assignment to the treatment group appears to have been made plausibly random, and there is good overlap in the conditional probabilities of treatment between applicants and participants, I consider semiparametric estimation based on the propensity score to be an appropriate, and my preferred, method of estimating ATT.

The propensity-matched estimates of the average effect of participation on the reported incidence of work and school attendance using cross-sectional data, along with weighted averages of the explanatory variables, are shown in **Table 3.3**. A parent's participation in *Jefas* is estimated to decrease the probability that a child works by 1.3 percentage points, from 2.2% to 0.9%, and increase the probability of school attendance by 2.0 percentage points, from 94.7% to 96.7%. These estimates are significant at a 90% confidence level, with p values of around 0.06. Also, note that weighting based on the propensity score seems to have evened out the distribution of the explanatory variables between treatment and control groups. Matching quality can be assessed using two-sample t-tests to assess whether there are significant differences between covariate means for both groups. By this standard, only the number of children and household size remain unbalanced in the weighted sample.

Probit regressions used to parametrically estimate ATT for work and school attendance, for comparison with the propensity-matched estimates, are

shown in **Table 3.4**.⁶ Point estimates of the average effects of participation on the outcome variables (ATT) are given in **Table 3.5**, and are similar to the propensity-matched estimates both in magnitude and significance. A parent's participation in *Jefas* is estimated to cause a 61% (1.4 percentage point) reduction in the probability that a child reports working, from 2.3% to 0.9%. Similarly, the estimated probability that a child attends school increases by 1.9 percentage points, from 94.8% to 96.7%.

Note that the estimated effect of participation varies significantly with age. For the children most likely to work and least likely to attend school, those age 14, the estimated effect of participation is to decrease the probability of working by 2 percentage points, from 5.6% to 3.6%, and to increase the probability of attending school by 4.3 percentage points, from 88.1% to 92.4%.⁷

The probit analysis also suggests that the sex of the child and the wealth of the household are significantly correlated with reported work and school attendance for this group of children, independent of participation. Male children are more likely to work and less likely to attend school. Greater household wealth, measured by the number of rooms per person, is—not surprisingly—

⁶ Because some households contain more than one child, I report standard errors robust to clustering at the household level. Accounting for clustering appears to have little effect on the standard errors, which increases confidence in the original assumptions. I know of no way to account for clustering in the propensity-matched estimates, and so I re-estimate those results for the cross-section and longitudinal samples using only one randomly selected child per household, and present the results in **Table 3.8**. Estimates are generally consistent with the main results in **Table 3.3**.

⁷ Note that the estimated 5.6% rate of child work for the counterfactual group is still low for this age group, compared with some of the published estimates reported in section 3.2.

associated with a lower probability of working and a higher probability of attending school. This is consistent with the proposed explanation for why participation should help alleviate child labor: increasing the income of a family should decrease the incidence of child labor because families use child work to compensate for inadequate adult income or financial resources.

Propensity-matched estimates of the effect of participation on the reported incidence of work and school attendance using longitudinal data are shown in **Table 3.6**, along with weighted means of the covariates for the longitudinal subsample, and lend confidence to the cross-sectional estimates. As can be seen, after weighting on the propensity score there remain no significant differences in pre-treatment reported work or school attendance between the treatment and control groups, which reduces concern about selection into the treatment group based on unobservable differences that are correlated with the outcomes. Because a difference-in-difference (DD) analysis is intended to account for pre-treatment differences in the outcome variable between treatment and control groups that would bias the results, this finding of no significant pre-treatment differences means that a DD analysis should be unnecessary in this case. Point estimates of ATT using this group are similar to the cross-sectional estimates, with a decrease in work of 1.9 percentage points and an increase in school attendance of 2.4 percentage points, although the effect on school attendance is no longer significant.

There are around 2 million participants in the heads of household program, each having, on average, 0.896 children between ages 10-14, or about 1,792,800 children in this age group. Taken at face value, the point estimates, which are summarized in **Table 3.7**, suggest a reduction in the number of such children working of between 23,300 and 34,000, and an increase in the number attending school of between 34,000 and 43,000. The propensity-matched estimates indicate a reduction in working of 23,300 and an increase in school attendance of 35,800. Given that the base rate of child work is likely to be under-reported, these estimates are probably a lower bound on the number of affected children.

3.7 Conclusion

In this study I show some evidence to suggest that parents' participation in the *Jefas* public employment program in Argentina helped alleviate child labor and increase school attendance for children age 10-14. I find semiparametric estimates of the average treatment effect on the treated (ATT), using applicants to the program as a control group. Using cross-sectional data from 2002, I estimate that participation decreased the reported incidence of child work by 1.3 percentage points, and increased school attendance by 2.0 percentage points. The latter result suggests that children derive real benefits from their parents' participation in the program. Parametric estimates of ATT confirm these results. Longitudinal estimates of the treatment effect, using panel data from 2001-2002,

allay concerns that the treatment effect may be due to pre-existing differences between the treatment and control groups. Back of the envelope calculations show that the program may have caused a reduction in the number of children working of around 23,300, and an increase in the number attending school of around 35,800, which is likely an underestimate of the number of affected children.

Figure 3.1 Overlapping Support in the Distribution of the Propensity Score

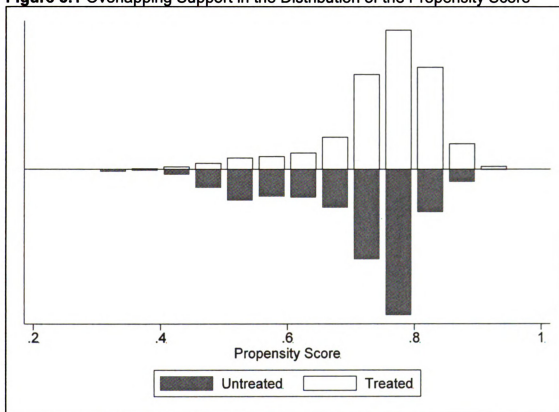


Table 3.1 Descriptive Statistics of Children of Applicants and Participants

	Participants		Applicants		Difference (part. - appl.)	
	Mean	st.dev.	Mean	st.dev.	Value	t-stat
<u>Dependent Variables</u>						
Works	0.009	[.093]	0.020	[.140]	-0.011	[-2.19]**
Attends school	0.967	[.179]	0.952	[.214]	0.015	[1.65]*
<u>Attributes of Children</u>						
Male	0.511	[.500]	0.517	[.500]	-0.007	[-0.29]
Age	11.889	[1.398]	11.935	[1.407]	-0.046	[-0.06]
<u>Attributes of Applicants</u>						
Applicant age	37.07	[7.988]	36.718	[8.954]	0.352	[0.90]
Applicant is male	0.315	[.465]	0.393	[.489]	-0.077	[-3.46]***
Applicant is household head	0.481	[.500]	0.466	[.499]	0.015	[0.65]
<u>Attributes of the Household</u>						
Lives in a shantytown	0.059	[.235]	0.065	[.247]	-0.006	[-0.56]
Bathroom	0.902	[.297]	0.874	[.333]	0.029	[1.96]**
Rooms per person	0.435	[0.231]	0.457	[0.243]	-0.022	[-1.94]*
Number of children <18 in hhld	4.104	[2.249]	3.897	[2.045]	0.207	[1.99]**
Number of applicants in household	1.202	[.532]	1.160	[.397]	0.042	[1.77]*
Single household head	0.195	[.396]	0.165	[.371]	0.031	[1.65]*
Married household head	0.379	[.485]	0.403	[.491]	-0.023	[-1.01]
Household size	6.885	[2.933]	6.542	[2.394]	0.342	[2.57]**
Number of observations	1706		601			
Standard deviations are in brackets.						

Notes: Sample is children age 10-14, who have a parent who has applied to Jefas. Data from Oct. 2002 EPH.

Table 3.2 Probit Regression for Calculating the Propensity Score

Dependent variable is a dummy indicating program participation

	Coeff	St.Err.
Lives in a shantytown	-0.04	[0.12]
Bathroom	0.12	[0.10]
Rooms per person	-0.14	[0.15]
Number of children <18	-0.05	[0.03]*
Number of applicants in household	0.13	[0.07]*
Applicant age	0.01	[0.00]
Applicant is male	-0.47	[0.09]***
Applicant is household head	0.33	[0.11]***
Single household head	-0.12	[0.12]
Married household head	-0.04	[0.07]
Child is male	-0.02	[0.06]
Child's age	-0.03	[0.02]
Household size	0.07	[0.03]***
Cuyo	-0.71	[0.10]***
Noroeste	-0.20	[0.07]***
Nordeste	-0.13	[0.08]
Number of observations	2306	
Number treated off support	0	
Pseudo R2	0.04	

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors are in brackets.

Notes: Sample is children age 10-14, who have a parent who has applied to Jefas. Data from Oct. 2002 EPH.

Table 3.3 Propensity-Matched Estimate of the Average Impact of Participation on Work and School Attendance

	Participants	Matched Controls	Difference (treatment - control)		
	Mean	Mean	Mean	t-stat	95% C.I.
Works	0.009	0.022	-0.013	[-1.88]*	[-.027,.001]
Attends school	0.967	0.947	0.020	[1.90]*	[-.001,.041]
Male	0.510	0.520	-0.010	[-0.73]	[-.035,.016]
Age	11.890	11.938	-0.048	[-1.31]	[-.120,.024]
Lives in a shantytown	0.059	0.060	-0.001	[-0.19]	[-.015,.012]
Bathroom	0.902	0.891	0.011	[1.31]	[-.005,.027]
Rooms per person	0.435	0.436	-0.001	[-0.19]	[-.012,.010]
Number of children <18 in hhld	4.104	3.951	0.154	[2.68]***	[.042,.267]
Number of applicants in household	1.202	1.198	0.004	[0.22]	[-.029,.036]
Applicant age	37.070	37.317	-0.241	[-0.90]	[-.763,.282]
Applicant is male	0.316	0.319	-0.004	[-0.30]	[-.028,.021]
Applicant is household head	0.482	0.499	-0.017	[-1.10]	[-.048,.014]
Single household head	0.195	0.191	0.004	[0.36]	[-.019,.028]
Married household head	0.379	0.403	-0.024	[-1.56]	[-.054,.006]
Household size	6.885	6.739	0.146	[2.05]**	[.006,.287]
Number of observations	1706	1706			

Standard deviations are in brackets.

Notes: Sample is children age 10-14, who have a parent who has applied to Jefas. Data from Oct. 2002 EPH.

Table 3.4 Probit Regressions for Work and School Attendance

	Works	Attends School
Parent is <i>Jefes</i> participant	-0.48 [0.21]**	0.24 [0.12]*
Lives in a shantytown	0.32 [0.27]	-0.09 [0.21]
Bathroom	0.08 [0.28]	0.15 [0.17]
Rooms per person	-1.22 [0.79]	1.1 [0.42]***
Number of children <18	-0.03 [0.07]	-0.05 [0.05]
Household size	0.05 [0.07]	0.02 [0.04]
Number of applicants in household	0.33 [0.20]	-0.04 [0.13]
Applicant age	0.01 [0.01]	-0.01 [0.01]
Applicant is male	-0.66 [0.25]***	0.15 [0.20]
Applicant is household head	0.68 [0.26]***	-0.11 [0.22]
Single household head	-0.53 [0.34]	0.01 [0.24]
Married household head	-0.17 [0.25]	0.25 [0.14]*
Child is male	0.47 [0.19]**	-0.36 [0.11]***
Child's age	0.33 [0.06]***	-0.31 [0.04]***
Constant	-7.39 [0.86]***	5.6 [0.62]***
Pseudo R2	0.25	0.14
Number of observations	2306	2306

* significant at 10%; ** significant at 5%; *** significant at 1%.

Standard errors are in brackets, and are robust to clustering at the household level.

Notes: Sample is children age 10-14, who have a parent who has applied to *Jefas*. Data from Oct. 2002 EPH. Both regressions include region dummies. Probit coefficients represent marginal effects.

Table 3.5 Probit Estimate of the Average Impact of Participation on Work and School Attendance (ATT)

	Child Works	Attends School
$E(Y \mid \text{Participant} = 1)$	0.009	0.967
$E(Y \mid \text{Participant} = 0)$	0.023	0.948
marginal change		
$E(Y \mid \text{participant} = 1) - E(Y \mid \text{participant} = 0)$	-0.014	0.019

Note: Point estimates of ATT using predicted probabilities from the probit in table 3.4. Data are from the 2002 EPH

Table 3.6 Propensity-Matched Estimate of the Average Impact of Participation on Work and School Attendance Using the Longitudinal Sub-sample

	Participants		Matched Controls		Difference (treatment - control)		
	Mean		Mean		Mean	t-stat	95% C.I.
Works	0.009		0.028		-0.019	[-1.66]*	[-.041,.003]
Attends school	0.969		0.945		0.024	[1.00]	[-.023,.071]
Worked in 2001	0.011		0.017		-0.006	[-0.57]	[-.030,.016]
Attended School in 2001	0.985		0.987		-0.002	[-0.22]	[-.020,.016]
Male	0.514		0.531		-0.017	[-0.41]	[-.101,.066]
Age	11.900		12.077		-0.177	[-1.47]	[-.414,.059]
Lives in a shantytown	0.053		0.054		-0.001	[-0.09]	[-.036,.033]
Bathroom	0.900		0.887		0.012	[0.51]	[-.035,.060]
Rooms per person	0.431		0.469		-0.037	[-1.94]*	[-.074,.000]
Number of children <18 in hhld	4.218		3.658		0.560	[3.15]	[.212,.908]
Number of applicants in household	1.228		1.229		-0.002	[-0.03]	[-.107,.103]
Applicant age	37.087		38.163		-1.076	[-1.53]	[-2.457,.306]
Applicant is male	0.316		0.285		0.031	[1.02]	[-.029,.091]
Applicant is household head	0.447		0.466		-0.019	[-0.47]	[-.097,.059]
Single household head	0.172		0.197		-0.025	[-0.67]	[-.097,.047]
Married household head	0.378		0.377		0.001	[0.04]	[-.078,.081]
Household size	7.142		6.621		0.521	[2.35]**	[.086,.957]
Number of observations	976		976				

Standard deviations are in brackets.

Notes: Sample is children age 10-14, who have a parent who has applied to Jefas. Data from EPH.

Table 3.7 Comparison of Point Estimates of Treatment Effect

Works	observed	matched	probit	matched (panel)
Treated	0.009	0.009	0.009	0.009
Untreated	0.020	0.022	0.023	0.028
Difference	-0.011	-0.013	-0.014	-0.019

Attends school	observed	matched	probit	matched (panel)
Treated	0.967	0.967	0.967	0.969
Untreated	0.952	0.947	0.948	0.945
Difference	0.015	0.020	0.019	0.024

Table 3.8 Sensitivity Analysis: Propensity-Matched Estimates with Only One Child Per Household

2002 Cross-Sectional Sample	Participants		Matched Controls		Difference (treatment - control)		
	Mean		Mean		Mean	t-stat	95% C.I.
Works	0.007		0.020		-0.013	[-1.67]*	[-.028,.002]
Attends school	0.974		0.942		0.032	[1.98]**	[.000,.063]
Number of observations	1536		1536				
Longitudinal Subsample	Mean		Mean		Value		
						t-stat	95% C.I.
Works	0.008		0.024		-0.016	[-1.37]	[-.038,.007]
Attends school	0.970		0.954		0.018	[0.75]	[-.028,.064]
Number of observations	664		664				

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Samples include only one child per household, selected at random. Standard errors are bootstrapped with 100 repetitions. Data from EPH.

Table 3.9 Sensitivity Analysis: Propensity-Matched Estimate of the Average Impact on Work and School Attendance for Children Age 10-16

	Child Works	Attends School
$E(Y_1 K = T)$	0.0121	0.9512
$E(Y_1 K = C)$	0.0189	0.9316
Matched difference		
$E(Y_1 K = T) - E(Y_1 K = C)$	-0.0068	0.0196
	(0.0056)	(0.0157)
95% confidence interval	[-0.0177, 0.0041]	[-0.0112, 0.0505]

Note: Standard errors are in parentheses. All standard errors are bootstrapped with 100 repetitions. Data from 2002 EPH.

APPENDIX TO CHAPTER THREE

A common way to address pre-existing differences between treatment and control outcomes based on unobservable differences is to construct a difference-in-difference (DD) estimator to compare the outcomes of interest (child work and school attendance) before and after the implementation of the *Jefas* program for the treatment and control groups. Although nonparametric DD estimators cannot be estimated consistently, they are common in the program evaluation literature. In this appendix, I construct such DD estimates for comparison with the cross-sectional estimates as a robustness check.

With outcomes Y_{it}^K for individuals i in time t and state K , the general form of the DD estimate is (suppressing the i subscript for now):

$$DD = \left[\hat{E}(Y_1^T) - \hat{E}(Y_0^T) \right] - \left[\hat{E}(Y_1^C) - \hat{E}(Y_0^C) \right]$$

If there are n individuals in the panel sample of participants and p individuals in the panel of applicants, a matched double difference estimate of the average treatment effect, which aligns the distribution of observables between participants and applicants, can be implemented as

$$DD = \frac{1}{n} \sum_{i=1}^n \left[Y_{i1}^T - Y_{i0}^T - \sum_{j=1}^p w_{ij} (Y_{j1}^C - Y_{j0}^C) \right]$$

Weights are found following the method in the previous section, using propensity scores calculated from the cross-section of applicants and participants.

A difference-in-difference estimate of the average treatment effect can also be found parametrically. Define a dummy variable T , with $T = 1$ indicating assignment to the group of participants and $T = 0$ indicating assignment to the group of applicants. The dummy variable t indicates the time period with $t = 1$ during the period following implementation of the program (2002) and $t = 0$ before program implementation (2001). The DD average treatment effect is found from the regression

$$Y_i = \alpha + \beta T_i + \gamma t_i + \delta(T_i \cdot t_i) + \varepsilon_i$$

where the ε_i are assumed to be normally distributed.

When the above equation is estimated as a linear regression, the interaction term δ can be thought of as the “true effect of treatment.” However, because both outcomes of interest (working and attending school) are binary variables, I run the regression as a probit model. In a non-linear DD regression, interpretation of the coefficients is less straightforward than for the linear regression. Puhani (2008) shows that the sign of the treatment effect in certain non-linear DD regressions, including the probit model, is equal to the sign of the coefficient on the interaction term. However, in any nonlinear model, the coefficient on the interaction term may not be a reliable estimator of the true interaction effect (see Ai and Norton, 2003). I therefore use the fitted model to find the DD estimate of the average treatment effect in the manner suggested by DeLeire (2004), by taking the discrete double difference of the standard normal

cumulative distribution function. Data used to find the DD estimate of ATT is the same as for the longitudinal sample in the body of the paper.

Propensity-matched difference-in-difference estimates of ATT are given in **Table 3.10**. Participation is estimated to decrease the probability that a child reports working by 1 percentage point, and increase school attendance by 2.1 percentage points, which is consistent with the main cross-sectional estimates. The reported incidence of work was estimated to slightly increase from 2001 to 2002 for the children of applicants but actually decrease for the children of participants. Neither estimate is significant at the 5% level, and I cannot reject that the program had no effect on school attendance at any reasonable level of significance.

Coefficients in the probit DD regressions are shown in **Table 3.11**. The coefficient on the interaction term representing the treatment effect is only marginally significant for working and not significant for school attendance. The implied treatment effect on the treated, reported in **Table 3.12**, is to decrease the reported incidence of child work by 3.0 percentage points and to increase school attendance by 2.4 percentage points. Again, the sex of the child and wealth of the household, using rooms per person as a proxy, are significantly correlated with the outcome variables. Older male children are more likely to work and less likely to attend school, while the opposite is true for children in wealthier households.

Table 3.10 Propensity-Matched Difference-in-Difference Estimate of the Average Impact of Participation on Work and School Attendance

	Child Works	Attends School
$E(Y_1 - Y_0 \mid K = T)$	-0.0014	-0.0162
$E(Y_1 - Y_0 \mid K = C)$	0.0086	-0.0372
Matched double difference		
$E(Y_1 - Y_0 \mid K = T) - E(Y_1 - Y_0 \mid K = C)$	-0.0100 (.0068)	0.0210 (.0218)
95% confidence interval	[-.0232,.0033]	[-.0217,.0637]

Note: Standard errors are in parentheses. All standard errors are bootstrapped with 100 repetitions. Panel data are from 2001 and 2002 rounds of the EPH.

Table 3.11 Difference-in-Difference Probit Regressions for Work and School Attendance

	Works	Attends School
After crisis	0.46 [0.43]	-0.39 [0.28]
Parent is <i>Jefes</i> participant	-0.12 [0.41]	0.05 [0.27]
After*participant	-0.88 [0.51]*	0.29 [0.32]
Lives in a shantytown	0.58 [0.44]	0.52 [0.46]
Bathroom	-0.21 [0.32]	-0.02 [0.20]
Rooms per person	-2.71 [1.14]**	1.81 [0.52]***
Number of children <18	0.16 [0.13]	-0.06 [0.06]
Number of applicants in household	0.49 [0.18]***	0.2 [0.14]
Applicant age	0.05 [0.02]**	-0.02 [0.01]**
Applicant is male	-1.08 [0.43]**	-0.32 [0.21]
Applicant is household head	0.63 [0.39]	0.27 [0.23]
Single household head	0.54 [0.50]	-0.67 [0.28]**
Married household head	0.95 [0.32]***	-0.1 [0.17]
Child is male	0.82 [0.28]***	-0.26 [0.14]*
Child's age	0.24 [0.09]***	-0.23 [0.05]***
Household size	0.03 [0.09]	0 [0.05]
Constant	-9.1 [1.68]***	5.43 [0.82]***
Pseudo R2	0.4	0.18
Number of observations	1960	1960

* significant at 10%; ** significant at 5%; *** significant at 1%.

Standard errors are in brackets.

Notes: Sample is panel of children age 10-14, who have a parent who has applied to Jefas. Panel data from Oct. 2001 and Oct. 2002 EPH. Both regressions include region dummies.

Table 3.12 Probit Difference-in-Difference Estimate of the Average Impact of Participation on Work and School Attendance

	Child Works	Attends School
$E(Y_1 - Y_0 \mid K = T)$	-0.003	-0.016
$E(Y_1 - Y_0 \mid K = C)$	0.027	-0.04
Matched double difference		
$E(Y_1 - Y_0 \mid K = T) - E(Y_1 - Y_0 \mid K = C)$	-0.030	0.024

Note: Panel data are from 2001 and 2002 rounds of the EPH.

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