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### *Poverty and Employability Effects of Workfare Programs in Argentina*

Lucas Ronconi  
Juan Sanguinetti  
Sandra Fachelli  
Virginia Casazza  
Ignacio Franceschelli

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Ronconi (Universidad de San Andrés, Buenos Aires, Argentina)  
[lrnconi@udesa.edu.ar](mailto:lrnconi@udesa.edu.ar)  
Sanguinetti (CEDI)  
[jsangu@house.com.ar](mailto:jsangu@house.com.ar)  
Fachelli (Ministerio de Economía, Argentina)  
[sfache@mecon.gov.ar](mailto:sfache@mecon.gov.ar)



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## Summary

In 1993 Argentina began implementing workfare programs, and workfare has become a central public policy starting 2002 when the government increased the number of beneficiaries from 100,000 to 2 million people in a country of 38 million. We explore targeting, poverty and employability effects of workfare before 2002 based on the permanent household survey (EPH). We find that the program was pro-poor although more than one third of participants did not satisfy the eligibility criteria. Our estimates suggest that the income of participants increased during treatment - particularly for women - indicating beneficial short run poverty effects. However, the long run effects of the program are not obvious due to selection on treatment completion. We present evidence suggesting that –for a large fraction of participants- the program generated dependency and did not increase their human capital.

**Keywords:** Workfare, evaluation, Argentina

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## 1 Introduction

Argentina suffered a deep economic, social and political crisis in the last few years. The economy shrunk by about 11 percent in 2002, and due to the currency's depreciation, GDP per capita dropped off to approximately US\$ 3,000 (down from US\$ 8,000 at its peak in 1998).

The crisis sharply aggravated the country's already difficult social situation. During 2002 poverty and unemployment reached their maximum historical level: more than 50 percent of Argentine households were below the poverty line, and almost 20 percent of the labor force was unemployed. Unemployment was particularly severe among the least-skilled workers, the rate being higher than 30 percent. This extremely negative context also had an impact on the education and health sectors where there is growing evidence of deterioration in service delivery. The combined effect of all these factors was an increasingly volatile social situation with high levels of violence and protests (see Fiszbein et al. 2002).

**Table 1 Long term trends in Poverty, Unemployment, Economic Growth and Workfare Programs in Buenos Aires (1980-2004)**

Year	Poverty Rate (Buenos Aires)	Unemployment Rate	Growth real GDP (%)	Workfare programs	
				Nº Participants (monthly average)	Annual Expenditure (millions of pesos)
1980	8.0	2.0	4.5	-	-
1985	16.0	6.1	-2.0	-	-
1990	38.1	7.5	-1.8	-	-
1991	25.2	6.5	10.5	-	-
1992	18.6	7.0	9.9	-	-
1993	17.3	9.6	5.7	26,236	94
1994	17.6	11.5	5.8	33,365	118
1995	23.5	17.5	-2.8	48,909	125
1996	27.3	17.2	5.5	62,083	134
1997	26.2	14.9	8.1	126,264	299
1998	25.1	12.8	3.9	112,076	259
1999	26.9	14.2	-3.4	105,895	241
2000	29.3	15.1	-0.8	85,665	162
2001	34.1	17.4	-4.4	91,806	160
2002	52.0	19.7	-10.9	1,126,387	2,030
2003	51.7	15.6	8.8	2,171,265	3,924
2004*	42.7	14.6	9.0	2,017,165	3,631 <sup>e</sup>

Source: Ministerio de Trabajo and INDEC

Notes: (\*) Estimates for the first semester of 2004.

<sup>e</sup> The figure is the annual estimated expenditure based on an expenditure equal to \$2,723 million up to September 2004. From 1991 to 2001, 1 peso was equal to 1 US dollar; since 2002, 1 peso was approximately equal to 0.33 US dollars.

In 2002, one of the main policies implemented by the government to deal with the crisis was to significantly increase the budget allocated to active labor policies. The number

of beneficiaries of workfare programs increased from 90,000 in December 2001, to 1,200,000 in October 2002, and to 2,000,000 in 2003. The recent decline in unemployment and poverty has been presented by the government as evidence of the positive income and employability effects of workfare programs (Ministerio de Trabajo, 2003).

Allocating more funds to social sectors, and particularly to labor programs, seems to be an adequate policy considering the current difficult situation. However, several questions have been raised in Argentina regarding the fairness and effectiveness of workfare programs. These programs have been pointed out as a source of political clientelism and corruption<sup>1</sup>, and many analysts argued that their employment effects are questionable. In spite of the topic's importance, most of the arguments are based on anecdotal evidence: there are very few empirical evaluations of these programs. Our research objective is to contribute to the debate by providing an econometric evaluation of the poverty and employability effects of workfare programs in Argentina, using the Encuesta Permanente de Hogares (Permanent Household Survey, hereafter EPH).

While our focus is on the Argentine case, we consider that the study is relevant to other countries, particularly those in Latin America, where active labor policies have been advocated as a way to soften the shocks generated by market-oriented reforms (Heckman et al., 1998; Goldbert L. and C. Giacometti, 1998; Marquez, 1999).

The paper is organized in five sections. The next section briefly describes the characteristics of workfare programs in Argentina. The third section presents our research objectives, a review of the empirical evidence and the knowledge gaps. The fourth section presents the methodology and the data. The fifth section presents the results, and the last section cites our conclusions.

## **2. Brief background of workfare programs in Argentina**

Currently, *Jefes de Hogar* is the main workfare program. It was implemented a few weeks after president Duhalde took office in February 2002. However, workfare programs in Argentina have been implemented since 1993, and while the program names have changed<sup>2</sup>, they all have all the same basic characteristics and objectives<sup>3</sup>. In this paper we

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<sup>1</sup> Ronconi (2001) surveys the main Argentine newspapers and finds that most of the press reports related to workfare programs mention the existence of political clientelism and corruption in the funds allocation process.

<sup>2</sup> In 1993 it was called *Programa Intensivo de Trabajo*, from 1995 to 2001 *Programa Trabajar*, and since 2002 *Programa Jefes de Hogar*. Provincial governments also implemented their own workfare program with similar characteristics to the federal ones. In terms of magnitude the most important program was *Barrios Bonaerenses* implemented by the provincial government of Buenos Aires.

evaluate workfare programs during 2000 and 2001 (i.e. before the implementation of *Jefes the Hogar*). The main program during these years was called *Trabajar*. The common features of these workfare programs were as follows:

- These programs targeted the least-skilled unemployed workers, preferably the heads of household. People who receive unemployment insurance benefits or a pension, or hold a job (even if it is in the informal sector) were not allowed to participate<sup>4</sup>.
- Participants received a monthly benefit below the minimum wage<sup>5</sup>, during a certain period (between three and six months) paid by the government<sup>6</sup>.
- During that period participants received training and had to work between twenty and forty hours per week<sup>7</sup> on communitarian projects at public or non-profit organizations<sup>8</sup>.
- The objectives of the program were: To act as a short-term safety net, and to increase employability among the least-skilled unemployed workers.

### 3 Research objectives and knowledge gaps

The following three components help define our research objectives:

#### 3.1 Program targeting

A review of workfare programs in OECD and some developing countries found that public-service jobs are well targeted at low-income unemployed workers when the wage rates have been set very low (See Dar and Tzannatos, 1999).

In Argentina, the benefit is below the minimum wage so we might expect self-targeting as argued by Jalan and Ravallion (2003). However, the state has low enforcement capacity, hence some benefits might be assigned to individuals who are not unemployed, but already hold a job. However, a second and more important concern is that, due to lack of

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<sup>3</sup> See the Argentine executive's decree number 327/1998 for the *Trabajar*, and the executive's decrees number 165/2002 and 565/2002 for the *Jefes de Hogar*. A detailed description of workfare programs in Argentina is provided in Ministerio de Economía (2002) and Ronconi (2001).

<sup>4</sup> *Jefes de Hogar* includes the following additional restriction: Applicants, in order to be eligible, have to show proof that their children are attending school and receiving appropriate medical treatment (such as vaccines).

<sup>5</sup> The maximum monthly benefit in the *Trabajar* was US\$200 – and most of the beneficiaries actually earned \$200- while the monthly benefit in the *Jefes de Hogar* is fixed at US\$150 per month. The minimum wage in the formal sector in Argentina is between US\$300 and US\$350 per month depending on the industry.

<sup>6</sup> Both the *Trabajar* and the *Jefes de Hogar* were partially financed through a loan from the World Bank.

<sup>7</sup> In *Jefes de Hogar* the work requirement is 20 hours per week, while in *Trabajar* it was between 30 and 40 hours per week.

<sup>8</sup> *Jefes de Hogar* allows participants to work in private companies provided that the employer pays the payroll tax and the necessary additional amount of money to meet the minimum wage.

sound political institutions, some benefits might be allocated based on political patronage, and not economic need (e.g. benefits assigned to friends, relatives or clienteles of influential politicians). Furthermore, anecdotal evidence suggests that these beneficiaries do not comply with the workfare work requirement. Therefore, in a poor institutional environment such as the one that characterizes Argentina, it is not necessarily true that imposing work requirements and setting the benefit below the minimum wage implies self-targeting.

Kremenchutzky (1997) and Ministerio de Trabajo (1999) have surveyed a small number of workfare program participants (60 and 159 respectively), and they find few cases (less than 10%) where participants do not meet the eligibility requirements to receive the benefit (i.e. the participant is well-educated or already holds a job)<sup>9</sup>. Ronconi (2001) presents anecdotal evidence showing several cases where the jobs are assigned on a political-clientele basis, or different forms of corruption in the allocation process<sup>10</sup>.

Thus, our first objective is to describe the socio-economic characteristics of participants and non participants, using the Permanent Household Survey, in order to verify if the participants are in fact those who need the program most. We answer several questions, such as: Do participants have any other source of income? How many of them are heads of poor households? Do participants have low educational levels? Which proportion of the unemployed and poorly educated workers does not receive the benefit<sup>11</sup>?

### **3.2 Poverty effects**

A second concern is related to the poverty effect of workfare programs. Even in the case where the program is well targeted, it is necessary to measure the income gain conditional on income in the absence of the program, to assess its impact. Common practice has been to estimate the gains by the gross wages paid, assuming that the labor supply to the program came only from the unemployed and from people who were out of the labor force. But, even if a participating worker was unemployed at the time she joined the program, it does not mean that she would have remained unemployed had the program not existed.

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<sup>9</sup> Ronconi (2001) argues that these figures should be interpreted with caution because the cases were not randomly selected from the population of workfare participants.

<sup>10</sup> Just to mention a few examples: In greater Buenos Aires, participants reported receiving 2/3 of the benefit, while the remaining third was held by the political boss who gave them access to the benefit. In La Matanza, funds were distributed by local leaders instead of being assigned directly by the government to the participant, as the legal procedure stipulates. Some participants were forced to participate in political demonstrations in order to receive the benefits.

<sup>11</sup> Regrettably, the EPH does not include any political variable. Hence, we are not able to check if in fact corruption and political clientelism characterize funds allocation. However, we consider that providing a reliable estimation of the percentage of participants who do not meet the eligibility requirements constitutes an improvement given the poor quality of the existing empirical evidence, and also an input for further studies.

Ministerio de Trabajo (2003) argues that the *Programa Jefes de Hogar* helped 29.3 percent of households that were below the indigence line to move out of indigence, and 6.5 percent of households that were below poverty to become non-poor. This ‘estimation’ is done assuming that benefits are targeted towards the poorest, and that the income gain of participating in the program is equal to the benefit. For the aforementioned reasons this analysis is not very informative.

Jalan and Ravallion (2003) estimate the net income gains of workfare programs in Argentina during 1997 constructing the counterfactual from a group of non-participants. They have exploited the cross section characteristic of the *Encuesta de Desarrollo Social* (EDS), and find average gains of approximately \$100 per participant per month (i.e. 50% of the benefit). While this is the first serious attempt to measure the effects of the program, the results may suffer a bias as suggested in Ronconi (2001)<sup>12</sup>.

In this paper we compute the average net income gain of workfare programs<sup>13</sup>, using a different database (i.e. the Permanent Household Survey) and a matching pairs approach<sup>14</sup>. The data and our empirical approach allow us to estimate the short and medium run poverty effect of workfare programs<sup>15,16</sup>.

### 3.3 Employability effects

Finally, we assess the employability effects of workfare. According to Bartik (2001), public service programs significantly increased the long-run earnings of participants in the US, since they provide some work experience and the needed soft skills. Do we observe this

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<sup>12</sup> Jalan and Ravallion’s (2003) results are based on a sample of 2,802 participants. Is this a representative sample of workfare participants? The authors randomly selected 350 projects, and mention that some participants were dropped from the random sample because their addresses could not be found, or because they did not want to respond. Assuming that the average number of participants per project is 20 –which is a conservative estimate- implies that 350 projects includes 7,000 participants. Ronconi (2001) questions how representative are the analyzed 2,802 participants.

<sup>13</sup> The EPH allows distinguishing participants from non participants, but it does not inform in which specific workfare program the participants are participating. During the period we analyzed (i.e. 2000-2001) the main program was *Trabajar*, although other federal and provincial workfare programs were in place such as *Programa de Emergencia Laboral* and *Barrios Bonaerenses* in the province of Buenos Aires.

<sup>14</sup> Since we do not follow a general equilibrium approach, we ignore indirect effects such as an increase in income due to the increase in aggregate demand generated by the program. These indirect effects were probably small before December 2001 because the number of participants was 1 percent of the labor force. However, they presumably have become important after 2002 when the government increased the number of participants to almost 15 percent of the labor force.

<sup>15</sup> We estimate if the direct income gains generated by the program helped the participants to move out of poverty and/or indigence. We use the official poverty and indigence lines which are described in the next section.

effect in the Argentine case? How did participants perform in the labor market after treatment? Are participants more or less likely to be employed than individuals in the control group after program completion? Does participation affect the odds of getting a formal job<sup>17</sup>? Do ex-participants receive higher wages due to treatment? Or is the workfare program a disguised income transfer? Furthermore, did workfare have any negative impact, such as 'signaling' or stigma effects on participants? Did the program generate dependency among participants?

None of these questions have been appropriately answered in Argentina. As far as we are aware, there are no statistically reliable evaluations of the employment effects of workfare programs. Our objective is to contribute towards filling this gap, exploiting the panel characteristic of the EPH and implementing a matching pairs approach to construct the control group<sup>18</sup>.

We also analyze the predisposition of employers to hire workfare program participants, based on a poll conducted during 2002 by the Ministerio de Trabajo (*Encuesta de Indicadores Laborales*).

To summarize, our research objective is to analyze how well targeted is the program, and how effective it is in reducing poverty and increasing employability.

#### **4 - Methodology and data sources**

The empirical strategy adopted in this study is the result of the research objectives advanced in the previous discussion and the characteristics of the available data.

The main analysis is based on the Permanent Household Survey (EPH). The data is collected and processed by the *Instituto Nacional de Estadísticas y Censos* (INDEC). The survey has been conducted bi-annually (in May and October) since 1974, and covers 28 urban agglomerates representing 62 percent of total population of the country and 98 percent of the population living in centers with more than 100,000 inhabitants. Each survey contains approximately 80,000 individuals and 25,000 households. The sampling and data

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<sup>16</sup> The long-run poverty effect of the program is much harder to assess. It would be necessary to measure how the program affects several outcomes. For example, Franceschelli and Ronconi (2002) argue that there exists a causal relation between the introduction of workfare programs and the emergence of the *Piquetero* movement in Argentina. To the extent that the *Piquetero* movement constitutes an empowerment of poor and previously unorganized people, it presumably affects how income is distributed.

<sup>17</sup> We define formal jobs as those jobs where the worker gets contributions to the social security system (i.e. health insurance, pension, and unemployment insurance).

<sup>18</sup> Galasso and Ravallion (2003) also estimate poverty effects of workfare programs using the EPH, but while they focus on the *Jefes de Hogar* program, we study poverty and employability effects of the programs that were in place before *Jefes*, mainly the *Trabajar*.



collection techniques used by the INDEC ensures the validity and reliability of the information (See Appendix 1 for more details).

The EPH contains information related to occupational, educational and socioeconomic characteristics, both at the individual and household level. Since October 2000, it has included a specific question that allows the econometrician to determine if the individual participates in a workfare program.

The EPH has a rolling panel structure: Each household is surveyed for four successive periods, and each period 25 percent of the surveyed households are replaced by new ones. This characteristic of the survey is quite useful for this study since, by providing information before and after treatment, it allows estimating the employability effect of the program and controlling for unobserved time invariant characteristics. Regrettably, each individual is followed only for four waves (i.e. two years), thereby impeding an estimation of the long run effects of the program.

There are two additional advantages of using the EPH as the source of information to estimate the effects of the program: First, the same questionnaire was administered to both participants and non-participants. Second, the EPH contains information regarding the urban agglomerate where the individual works, allowing us to construct the comparison sample with individuals who reside in a similar local labor market as program participants. As Heckman et al. (1998) point out, these two characteristics of the data prevent important bias from arising. Furthermore, they show that bias due to the use of different surveys and differences in the distribution of participants and comparison groups across the local labor market is often large relative to selection bias.

#### **4.1 Targeting**

To assess how well targeted workfare is, we analyze several socioeconomic characteristics of participants and non-participants, both at the individual and household level, such as: education, work experience, type of residence, access to basic services and household income per capita. We follow INDEC's definition of poverty and indigence to measure the proportion of beneficiaries below poverty, and the proportion of poor people not receiving the benefit<sup>19</sup>.

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<sup>19</sup> The poverty line is calculated based on the 1986/87 income and expenditure survey, and updated using price indices for food and non-food components. For example, in October 2000 the poverty line is US\$151.1 per male adult, per month. To calculate poverty, household composition is converted into male adult equivalents using standard conversion factors. The indigence line is based exclusively on the food consumption portion of the poverty line, and for 2000, is equal to US\$62.4 per male adult, per month. See more details in Appendix 2.

## 4.2 Poverty Effects

A simple way to analyze the income effect of workfare programs during treatment is to compare the income of participants during treatment relative to their income before treatment. However, this estimate is presumably biased for several reasons as we suggested earlier. First, those individuals who participate in the program presumably have different characteristics than those who do not participate, and if those characteristics also affect labor performance the estimate would be biased. Second, the macroeconomic situation in Argentina deteriorated significantly during the period 1998-2002, hence it would be misleading to attach all changes in participants' income to the program.

In order to compute the program's short-run poverty effects, we need to measure the income gain conditional on income in the absence of the program (Heckman et al., 1998). The "with" data is provided by the EPH (i.e. we observe the income of participants). But the "without" data (i.e. what would have been the income of participants in the absence of the program) is fundamentally unobserved, since an individual cannot be both a participant and a non-participant at the same time. Following the conventional evaluation literature, we assume the existence of a group of individuals comparable to participants except for not having received benefits. We use propensity score matching methods to draw a comparison group to workfare participants from the large number of non-participants available in the EPH<sup>20</sup>. More specifically:

Let  $D_i=1$  if individual  $i$  participates in the program, and  $D_i=0$  if she/he does not participate. Let  $X_i$  be a vector of variables that helps predict participation in the program; and  $P(X) = Prob(D=1/X)$  is the probability of participating conditional on  $X$ , the propensity score.

We calculate the propensity score for each individual in the Permanent Household Survey using standard probit model. In order to ensure that participants and their matched non-participants are affected by the same local labor market conditions we run six separate regressions, one for each region<sup>21</sup>.

Then, for each region, we select from the group of non-participants the five individuals who have the most similar propensity score to each participant. These selected non-participants constitute the comparison group.

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<sup>20</sup> The reason we use a non-experimental method to estimate the effects of the workfare program is because experimental data is not available. Also, we use a control group drawn from "external" sources (i.e. non-participants) because there is no available information to distinguish between those non-participants who applied but were not selected from those who did not apply. Bell et al. (1995) provides a clear exposition about the pros and cons of using a control group drawn from 'external' versus 'internal' sources.

One way to measure the mean income effect of the program during the period when participants are receiving the benefit is by estimating  $\mu$ :

$$(1) \quad Y_i = W_i\beta + \mu D_i + U_i$$

Where  $Y_i$  is the monthly income of individual  $i$ ,  $W_i$  is a set of variables that affect income,  $D_i$  is a dummy variable equal to 1 if the individual  $i$  is participating in the workfare program and equal to 0 if she/he is from the comparison group. (Notice that only participants and their respective 'nearest neighbor non-participants' are included in the sample; non-participants who are not part of the comparison group are excluded).

This estimator is based on a cross-section of individuals so it does not confront the problem of attaching changes in the macroeconomic situation to the program. Also, by construction, the estimator controls for observed characteristics of individuals. However, a potential problem with this estimator is that program participants and their respective non-participating nearest neighbors may differ according to unobservable characteristics, and if those unobserved characteristics also affect labor performance the estimates would be biased.

However, by considering the income of both participants and the control group in a period before treatment and computing a difference-in-difference estimator, it is possible to remove the bias generated by time invariant unobserved factors. The difference-in-difference estimator of the income effect of the program during treatment is given by coefficient  $\delta$ :

$$(2) \quad \Delta Y_i = \Delta W_i\theta + \Delta D_i\delta + \varepsilon_i$$

Where  $\Delta Y_i = Y_{it} - Y_{it-1}$  is the change in income of individual  $i$  between periods  $t$  and  $t-1$ , and treatment takes place during period  $t$ . The sample is also restricted to participants and the comparison group.

Finally, we compute the percentage of participant households who moved out of poverty using the average income gain estimated via equation 2. While this strategy does not allow us to claim any long-term effects over poverty reduction, at the least it assesses the importance of workfare programs as short run safety nets.

### 4.3 Employability Effects

Our third objective is to estimate the employability effects of workfare. We evaluate if participants are more or less likely to find a job in the formal sector, earn a higher wage and a higher hourly wage after program completion. As in the previous case, the fundamentally

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<sup>21</sup> We consider that this strategy leads to a reliable comparison group since the EPH contains a wide range of socio-economic characteristics for each individual, allowing us to control for a large number

unobserved data is the employment performance and salaries of participants ‘without’ treatment. Again, our empirical strategy is to use propensity score matching methods to draw a comparison group to workfare participants from the large number of non-participants available in the EPH.

But unlike the previous case where we were interested in the income effect during treatment, now we need after-treatment outcomes. Taking advantage of the panel structure of the Permanent Household Survey we compute ‘before and after’ treatment difference-in-difference estimators. For each of the dependent variables of interest, we run a model similar to equation (2) except that in this case we focus on changes in  $Y_i$  between  $t+1$  and  $t-1$  (Recall that  $t$  refers to the period when participants receive treatment).

Considering that the rolling panel structure of the EPH allows to follow the same individual for four waves (i.e. two years), we can estimate the employability effects of the program six and twelve months after program completion.

At this stage an important point should be re-emphasized: During the period under consideration, the overall state of the Argentine economy suffered major changes. GDP per capita decreased 25 percent, the unemployment rate went up 3 percentage points, and the share of informal employment increased from 37 percent to 50 percent. Under such a crisis, it would be incorrect to attach all the negative changes in participants’ outcomes to the workfare program. In other words, a ‘before and after’ estimator based exclusively on a sample of participants should be discarded, or at least taken with extreme caution. However, since we also work with a comparison group of non-participants, and under the assumption that the crisis had a similar effect over the outcomes of participants and their respective non-participants nearest neighbors, we can isolate the workfare program’s effects from the economic crisis by computing a difference-in-difference estimator.

Summing up, we follow the conventional evaluation literature. The value added of our paper is that we use these standard methods to explore a database and answer several questions that have not been analyzed yet<sup>22</sup>.

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of observable factors. Obviously, unobserved heterogeneity is a potential problem, and we discuss it below.

<sup>22</sup> Let us mention that there are several potential extensions to the methodology we follow in this paper. For example, instead of taking May 2001 as the unique treatment period, a more flexible methodology would be to construct a larger dataset including those individuals who received treatment during different periods of time; in which case, time dummies should be included to account for changes in the macroeconomic performance. A second plausible extension would be to construct the comparison group not only based on the characteristics of participants and non-participants during treatment, but also on their characteristics before treatment allowing controlling for Ashenfelter’s dip. While this alternative clearly improves the quality of the matching, we decided not to implement it because it significantly reduces the number of observations in our sample. (Recall the rolling panel structure of the EPH). In any case, these may be constructive extensions to this paper, and we leave them for further work.

## 5 Results

### 5.1 Targeting

In this section we analyze how well targeted towards the low skilled and unemployed workers were workfare programs in October 2000 and May 2001, before the large expansion occurred in early 2002 (i.e. before the *Jefes de Hogar* program was implemented).

During the periods we note that the number of beneficiaries was a very small share of those who needed support. While beneficiaries were approximately 100,000, the number of unemployed people was 1.5 million, and the number of people living in households below the poverty line was 9 million. Therefore, we expect to find that a large share of poor and unemployed people did not receive the benefit. But were the scarce benefits allocated properly? Tables 2, 3 and 4 present basic socioeconomic characteristics for program participants and all non-participants.

**Table 2 Socioeconomic Characteristics of Participants and Non-participants of Workfare Programs, May 2001**

Characteristics	Participants	Non-participants
Age	35.0 years	37.1 years
Gender (% female)	58.3%	52.3%
Head of Household	38.0%	38.9%
Number of members in the household	5.0	4.5
Residence located in a shantytown	4.0%	2.0%
Lack of access to water, electricity or sanitary installations	6.4%	5.2%
Residence ownership (yes=1)	72.2%	72.1%
No. observations	655	45,242

Note: The sample is restricted to all individuals between 18 and 65 years old.

Almost 60 percent of participants are female, less than 40 percent are head of household and 4 percent live in a shantytown. The average participant is 2.1 years younger and has a higher probability of living in a shantytown and not having access to water, electricity or sanitary installations than the average non-participant. The differences are not large.

Regarding educational attainment, we observe that participants are on average less educated than non-participants. But, again, the difference is not very large. While 42.6 percent of the beneficiaries have 7 or less years of schooling and 17.4 percent have at least some college education (i.e. more than 12 years of schooling), the figures for the group of non-participants are 34.1 percent and 26.0 percent respectively.

**Table 3 Educational Attainment of Participants and Non-Participants of Workfare Programs, May 2001**

Maximum education attained	Participants	Non-participants
Primary school dropout	13.6%	9.6%
Primary school graduate	29.0%	24.5%
High school dropout	23.4%	20.7%
High school graduate	16.3%	19.2%
Some college	11.8%	15.7%
College graduate	5.6%	10.3%
Total	100%	100%

Note: Primary school graduate implies 7 years of schooling, and High school graduate implies 12 years of schooling.

We also observe that 18.2 percent of workfare participants not enrolled in school report at least one of the following sources of income in addition to the workfare benefit: a formal job, an informal job, self-employment income, pension or unemployment insurance.

This is preliminary evidence that a large share of the benefits have not been assigned to the poorest unemployed and least skilled workers as established in the normative. The inadequate allocation of benefits becomes more evident when we analyze household income per capita: Only 22.1 percent of participants were below the indigence line, 35.4 percent were below the poverty line but above the indigence line, and the remaining 42.6 percent were above the poverty line<sup>23</sup>. Table 4 shows that the average participant was poorer than the average non-participant, but the differences are not that large. On the other hand, only 4.8 percent of the total number of indigent households in Argentina had a member participating in the program.

**Table 4 Indigence and Poverty rates for Participants and Non-Participants, October 2000**

	Participants	Non-participants
Non-Poor	42.6%	67.8%
Poor	57.4%	32.2%
- <i>Poor but not Indigent</i>	35.4%	22.9%
- <i>Indigent</i>	22.1%	9.3%
Total	100%	100%

Note: The figures are for individuals. However, since indigence and poverty are defined according to household income per capita, we categorized a household as 'participant' if at least one member is participating in the workfare program. In October 2000, the number of participating households was lower than the number of participating individuals because 13 percent of participating individuals were members of a household that had two or more members participating in the workfare program.

<sup>23</sup> These figures are computed including the benefit as a component of income. The alternative extreme assumption is to compute poverty based on the income of participants without including the benefit. As we mentioned before any of these alternatives is adequate: In order to properly analyze how well targeted was the workfare program according to family income, it is necessary to compute the income of participants in the absence of the program. This analysis is done in the next section. See table 12.

We also observe that, while most of participants are members of households located in the poorest quintiles, one fourth of beneficiaries are members of a household that ranks in the top 50 percent of the income per capita distribution in both October 2000 and May 2001.

**Table 5 Distribution of Participants according to household income per capita, October 2000 and May 2001**

Deciles familiar income p/c	October 2000	May 2001
1 <sup>st</sup> decile (Poorest 10%)	30.6 %	25.5%
2 <sup>nd</sup>	13.8 %	18.2%
3 <sup>rd</sup>	11.1 %	15.3%
4 <sup>th</sup>	11.8 %	10.4%
5 <sup>th</sup>	8.2 %	9.9%
6 <sup>th</sup>	6.7 %	6.5%
7 <sup>th</sup>	5.8 %	5.2%
8 <sup>th</sup>	6.0 %	4.2%
9 <sup>th</sup>	3.6 %	2.6%
10 <sup>th</sup> (Richest 10%)	2.5 %	2.3%

In order to get a measure of inclusion/exclusion errors in workfare targeting we categorize an individual as eligible if: She/he has dependent children, has no job, does not receive a pension or unemployment insurance, and is located in the bottom half of the family income per capita distribution<sup>24</sup>.

Table 6 compares eligibility between workfare participants and non-participants aged 18-65 years old. We observe that 43 percent of participants did not satisfy the eligibility criteria, while only 4 percent of eligible people received the benefit.

**Table 6 Distribution of Participants and non-participants according to eligibility criteria, May 2001**

	Eligible	Ineligible	Total
Participants	341	264	605
% row	56.4%	43.6%	100%
% column	3.7%	0.8%	1.4%
Non-participants	8,897	33,281	42,178
% row	21.1%	78.9%	100%
% column	96.3%	99.2%	98.6%
Total	9,238	33,545	42,783

Note: The sample is restricted to individuals 18 to 65 years old with complete information in all the variables used to define eligibility.

<sup>24</sup> Having dependent children refers to children less than 18 years old residing in the household. The 'no job' requirement for participants actually means that we categorize them as ineligible if they report income as self-employees, or report having a formal job, or report having more than one job. We thank Habiba Djebbari for suggesting a table of inclusion/exclusion errors.

Summing up, the evidence suggests that the limited number of benefits was not appropriately distributed<sup>25</sup>. This evidence is consistent with the argument that setting work requirements and the benefit below the minimum wage are not sufficient conditions to ensure self-targeting in countries with a lack of sound political institutions such as Argentina<sup>26</sup>.

## 5.2 Poverty effects

The first and critical step in estimating program poverty effects is to find a comparison group of non-participants who has sufficiently similar characteristics to participants except for not participating in the program.

We run standard probit models –one for each local labor market<sup>27</sup>– to estimate propensity scores. The vector  $X_i$  of individual and household characteristics includes the following variables: age, age squared, gender, marital status, migration status<sup>28</sup>, work experience, maximum level of education attained, if attending school, if the person is head of household, number of dependents in the household, household members per room, quality of the residence (e.g. if the residence has access to water, electricity and sanitary installations), location of the residence (i.e. if the residence is located in a shantytown), and ownership of residence.

We take May 2001 as the base state. The reason we choose this survey, and discarded using more recent surveys as the base state, is because it allows us to analyze the performance of ex-participants several months after treatment. Furthermore, since workfare became almost universal by October 2002, it might not be possible to find a reliable matching pair to each participant from the group of non-participants for that –or more recent– period. We also discarded October 2000 as the base state because it does not allow us to observe the situation of participants before treatment. Recall that October 2000 is the first survey that included a specific question about participation.

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<sup>25</sup> As a caveat it should be mentioned that, while the INDEC assures the confidentiality of the collected information, it is always possible that some individuals do not report accurate information. For example, an individual who participates in the program and does not meet the eligibility criteria might have the incentive to report that he does not participate, in which case the true targeting of the program would be worse than the figures presented above suggest. We suspect this is not a large problem because the eligibility requirements are seldom enforced by the government.

<sup>26</sup> As Besley and Kanbur (1990) argue, perfect targeting (i.e. benefits allocated to the poorest) is not optimal in practice since it implies high administrative costs (i.e. the government has to spend too much money collecting information and monitoring if it tries to reach the poorest). However, we consider that the extent of misallocation of workfare funds cannot be justified by administrative costs.

<sup>27</sup> We use the region as an indicator of local labor market. There are six regions in the EPH: Northwest, Northeast, Cuyo, Pampeana, Patagonica and Greater Buenos Aires. A more disaggregated definition of the local labor market is to use the urban agglomerate. There are 28 urban agglomerates in the EPH. We choose the region because there are several urban agglomerates with very few workfare participants impeding an adequate identification of a comparison group. However, we include indicators of urban agglomerates as controls in the estimation of propensity scores.

<sup>28</sup> Individuals are categorized into three groups: Foreign born, born in a different province, and born in the same province where they reside.



In May 2001 the EPH reports 655 individuals who were participating in the six month workfare program. The group of non-participants includes almost 50,000 observations. From this large group we extracted the five nearest neighbors for each participant, creating a comparison group totaling 3,275 individuals. The following table presents the probit regressions, one for each region.

**Table 8 – Probits for calibrating the propensity score, by region, May 2001**

Regressors	Greater Buenos Aires					
	Aires	Northwest	Northeast	Cuyo	Pampeana	Patagonica
	Coef. (t-stat)	Coef. (t-stat)	Coef. (t-stat)	Coef. (t-stat)	Coef. (t-stat)	Coef. (t-stat)
Age	0.05 (1.39)	0.09 (2.91)	0.09 (2.82)	0.05 (1.88)	0.02 (0.96)	0.05 (2.05)
Age^2	-0.00 (-1.15)	-0.00 (-3.61)	-0.00 (-2.85)	-0.00 (-2.09)	-0.00 (-0.96)	-0.00 (-1.84)
Male	-0.04 (-0.29)	0.08 (0.85)	0.02 (0.15)	-0.11 (-1.03)	-0.18 (-2.65)	-0.25 (-2.55)
Primary school graduate	-0.18 (-1.00)	0.12 (0.67)	-0.19 (-1.11)	0.05 (0.29)	0.04 (0.39)	-0.27 (-2.00)
High school dropout	-0.16 (-0.75)	0.13 (0.74)	-0.11 (-0.63)	0.15 (0.81)	-0.08 (-0.70)	-0.22 (-1.58)
High school graduate	-0.29 (-1.23)	-0.03 (-0.16)	0.05 (0.30)	0.09 (0.45)	-0.16 (-1.35)	-0.50 (-3.18)
Some college	-0.29 (-0.96)	0.04 (0.20)	0.01 (0.06)	0.15 (0.63)	-0.21 (-1.39)	-0.25 (1.34)
College graduate	-0.15 (-0.54)	-0.15 (-0.61)	0.07 (0.30)	0.10 (0.46)	-0.23 (-1.67)	-0.91 (-3.29)
Enrolled in Educ. Inst.	-0.21 (-0.80)	-0.19 (-1.07)	0.01 (0.04)	-0.35 (-1.75)	-0.12 (-0.96)	-0.16 (-1.01)
Born in other province	-0.24 (-1.56)	-0.26 (-2.03)	-0.47 (-2.47)	-0.09 (-0.79)	-0.07 (-0.87)	-0.15 (-1.48)
Born in other country	-0.37 (-1.36)	-	0.08 (0.28)	-0.10 (-0.35)	0.03 (0.14)	-0.12 (-0.97)
Partner	-0.45 (-2.38)	0.00 (0.03)	0.09 (0.61)	-0.01 (-0.09)	-0.07 (-0.74)	-0.19 (-1.53)
Married	-0.56 (-3.28)	-0.04 (-0.31)	-0.21 (-1.42)	-0.30 (-2.26)	-0.29 (-3.42)	-0.49 (-3.97)
Divorced	-0.22 (-0.88)	-0.27 (-0.93)	-0.22 (-0.75)	-0.06 (-0.26)	-0.11 (-0.85)	-0.03 (-0.14)
Widow	-0.54 (-1.29)	0.07 (0.19)	-0.21 (-0.49)	-0.03 (-0.10)	-0.39 (-1.92)	-0.79 (-1.94)
Head of household	0.23 (1.54)	-0.11 (-0.92)	0.02 (0.15)	0.02 (0.13)	0.17 (2.20)	0.10 (0.90)
No. dependents	0.05 (1.40)	0.04 (1.65)	0.02 (0.85)	0.01 (0.16)	0.06 (3.29)	0.03 (1.17)
Experience	-0.01 (-0.56)	-0.02 (-1.92)	-0.01 (-1.51)	-0.04 (-3.14)	-0.00 (-0.75)	-0.03 (-2.86)
Lack access to basic services	0.25 (0.98)	-0.39 (1.99)	-0.03 (-0.19)	0.29 (1.48)	0.23 (1.96)	-0.12 (-0.45)
Resides in shantytown	0.03 (0.08)	-	-0.04 (-0.15)	-	0.26 (1.63)	0.53 (2.82)
Property ownership	-0.07 (-0.50)	0.01 (0.11)	0.01 (0.05)	0.09 (0.86)	0.03 (0.39)	0.07 (0.68)
Rooms per member	-0.39 (1.99)	-0.06 (-0.53)	-0.10 (-0.80)	-0.04 (0.37)	0.01 (0.11)	-0.26 (-2.42)
No. participants	48	95	71	106	211	124
No. observations	7,065	9,892	5,711	4,840	14,146	5,837
Log-Likelihood	-262.2	-474.7	-352.8	-427.4	-1027.2	-532.7

Dependent variable is 1 if participated in workfare in May 2001 and 0 otherwise. Omitted categories are primary school dropout, single, and born in the same province. All equations include urban agglomerates dummies.

The average propensity score across regions for those who were participating in workfare is 0.038 while for non-participants it is 0.012, which indicates that these two groups have different observable characteristics. The propensity score for the comparison group (i.e. the group formed by non-participants that we selected as nearest neighbors) is also 0.038 making us confident to carry on our strategy<sup>29</sup>. It should be mentioned that this is not always the case. In those cases where the program is well targeted at a particular socioeconomic group and most of eligible individuals receive treatment it may not be possible for the researcher to draw a reliable comparison group from the sample of non-

<sup>29</sup> Appendix 3 presents basic statistics for participants and the comparison group. Both groups present very similar observable characteristics as expected.

participants<sup>30</sup>. However, during the period being analyzed, the number of beneficiaries was a small proportion of the objective population of the program, and the scarce benefits were allocated to different socioeconomic groups as shown in the previous section. This situation allowed us to construct a comparison group with similar observable characteristics to program participants.

The first question we consider is by how much the monthly income of participants changed during participation in the program due to treatment. In other words, which would have been the income of participants during May 2001, if they were not beneficiaries? The first estimates we compute are based on equation (1)<sup>31</sup>. Table 9 presents the results.

**Table 9 Estimates of Workfare Program Effect during treatment, May 2001, Participants and comparison group**

Dependent variable	All	Males	Females
Income in the main occupation	25.56 (2.11)	-50.06 (-2.07)	76.90 (6.88)
Total Income	-8.28 (-0.60)	-79.83 (-2.89)	39.40 (3.06)
Hourly Income in the main occupation	0.87 (6.74)	0.39 (2.80)	1.21 (6.14)
No. observations	3,930	1,645	2,285
Number of Workfare participants	655	273	382

Note: t-values reported in parentheses.

We find that, on average, participants had an income in their main occupation that is US\$26 higher than the comparison group but a total income that is US\$8 lower –but statistically not different from zero. We also find that participants have an hourly income in their main occupation that is US\$0.87 higher. These estimates differ significantly across gender. On the one hand, female participants had an income in their main occupation US\$77 higher, a total income US\$39 higher and an hourly income US\$1.2 higher than females in the comparison group. On the other hand, male participants had an income in their main occupation US\$50 lower, a total income US\$80 lower and an hourly income US\$0.4 higher than males in the comparison group.

There are several possible interpretations for these results. One possibility is that participants and the comparison group differ in unobservable characteristics and the results reflect not only program effects but also selection bias. However, assume for a moment these are unbiased estimates and discuss selection bias later.

<sup>30</sup> We suspect this would be the case if we had chosen October 2002 –or a more recent survey- as the base period since by that time workfare programs were almost universal.

<sup>31</sup> The vector  $W_i$  of control variables includes the same variables used to estimate the propensity score plus regional dummies.

It appears that females benefit more from program participation than males. This result is not surprising given the fact that program benefits are the same for both genders and women presumably have fewer opportunities than men in the labor market. The higher incentive that females have relative to males to participate may also explain the fact that more women than men actually participate in the program.

Another finding is that treatment appears to increase the hourly earnings and income in the main occupation, but has a negative/zero<sup>32</sup> effect on total earnings. This last result is driven by male participants, who present a lower total income than the comparison group. Why would men thus choose to participate if they could earn a higher total income in the labor market? An answer could be gathered after comparing working conditions between participants and non participants. Men in the comparison group work an average of 45 hours per week, 56 percent of those who are employed have informal jobs, 18 percent are unemployed, and 14 percent inactive. Male participants, on the other hand, are all employed and work on average 31.3 hours per week (see Table 10). Therefore, a potential explanation is that male participants choose to participate because they may prefer to earn a lower total income but work less hours instead of working more hours in the informal sector. An alternative explanation is that they choose to participate because otherwise they would have remained unemployed. Table 10 also suggests that a large share of female participants were presumably 'inactive' (i.e. not working or looking for a job in the labor market) before joining the program.

**Table 10 Labor Performance of Participants and Comparison Group, during Treatment by Gender, May 2001**

Variable	Participants		Comparison Group	
	Female	Male	Female	Male
No. Hours worked per week	24.2	31.3	35.5	44.6
Unemployment rate	0%	0%	10.9%	17.5%
Labor force participation rate	100%	100%	49.8%	86.0%
No. observations	382	273	1,903	1,372

The estimates in table 9 may suffer a bias as mentioned in the methodology section. Controlling for observed heterogeneity between participants and non-participants does not eliminate latent heterogeneity that could bias the program's impact estimates. For example, it may be that participants have higher social capital than the comparison group. And it may be that it is this higher level of social capital that explains both the higher probability of participating in the program as well as the higher level of hourly earnings. Since we do not observe social capital, we cannot control for it. Therefore, the estimates would inappropriately consider the effect of social capital as part of the program effect. A similar argument applies for unobserved factors such as ability, effort or motivation.

<sup>32</sup> While the coefficient is negative (-8.3), it is not statistically different from zero.

However, if the source of heterogeneity is time-invariant we can eliminate it by computing difference-in-difference estimators. For example, the level of social capital, ability, effort and motivation might be quite constant through short periods of time. Hence, we can solve the problem by analyzing the performance of participants and the comparison group at different points in time.

We begin analyzing the change in income for both groups –participants and the comparison group- between October 2000 and May 2001. Recall that May 2001 is the period when participants were receiving treatment and October 2000 is the most immediate survey before treatment.

In the EPH for October 2000 we find that 356 people were surveyed out of the 655 who were workfare program participants in May 2001<sup>33</sup>. Running the model described in equation (2) we find that (see column 1 in Table 11): Program participation is related with an increase on total monthly income equal to US\$44.9, increase on monthly income in the main occupation by US\$53.2 and on hourly earnings US\$0.67<sup>34</sup>.

**Table 11 Difference in difference estimates of Workfare Program, May 2001 (i.e. during treatment) and October 2000 (i.e. before treatment), Participants and control group**

Dependent variable	All	Males	Females
Income in the main occupation	53.3 (3.98)	31.4 (1.26)	67.6 (4.77)
Total Income	44.9 (2.99)	12.8 (0.46)	67.9 (4.28)
Hourly Income in the main occupation	0.67 (3.07)	0.29 (1.73)	0.94 (2.75)

Note: t-values reported in parentheses. The sample includes 356 workfare participants (208 females and 148 males) and 1,898 non-participants.

As with the previous findings, the program appears to be relatively more successful in increasing the income of women than men during treatment. For females, the coefficients

<sup>33</sup> The socioeconomic characteristics of the 655 workfare participants surveyed in May 2001 are not clearly different than the characteristics of the 356 participants that were also surveyed during October 2000. For example, the average (standard error) number of household members is 4.99 (2.46) and the percentage living in a shantytown is 3.97 percent (0.20) in the first group while the figures are 5.17 percent (2.51) and 4.21percent (0.20) in the second group suggesting the later is poorer. But on the other hand, the percentage without access to basic services is 6.41percent (0.25) and property ownership 0.72 (0.45) in the first group compared to 5.62percent (0.23) and 0.79 (0.41) in the second group suggesting the former is poorer. We thank Habiba Djebbari for suggesting a discussion of the potential attrition bias.

<sup>34</sup> Habiba Djebbari suggested estimating workfare program effects excluding the province of Buenos Aires, since a large provincial workfare program was implemented in that province (i.e. *Barrios Bonaerenses*) in addition to *Trabajar*. When excluding Buenos Aires, the estimates (and t-values) are slightly higher: increase in total monthly income US\$53.4 (3.19), increase in income in main occupation US\$61.4 (4.11), and increase in hourly earnings US\$0.74 (3.22). The number of observations used to compute these estimates is 1,870.

are: US\$67.9 increase in total monthly income, US\$67.6 increase in income in their main occupation and US\$0.94 increase in hourly earnings. All estimates are statistically significant. For males, we find US\$12.8 increase in total income, US\$31.4 increase in income in their main occupation and US\$0.29 increase in hourly earnings; but the first two estimates are statistically not different from zero.

The positive estimate of the mean income effect of the program captures the fact that the average total monthly income of the comparison group decreased from US\$269.2 in October 2000 to US\$260.3 in May 2001 due to the economic recession that the country was suffering, but increased from US\$202.1 to US\$238.1 for the group of participants.

Considering that in May 2001 the workfare program benefit was US\$200 per month, the estimated net income gain of participating in the program represents approximately 25 percent of the benefit. Our estimate is half with respect to the one computed by Jalan and Ravallion (2003). They estimated a net income gain of US\$100 per month –or 50 percent of the benefit<sup>35</sup>.

The program's positive income effect during treatment could be explained by considering that many participants would presumably have remained unemployed or inactive, and hence without income, in the absence of the program. However, the estimated income effect is smaller than the benefit probably because many participants would presumably have gotten a job and worked more hours in the absence of the program.

We also observe that 154 individuals appear to be participating in the program during both May 2001 and October 2000, representing 43 percent of participants in May 2001. We discuss this issue later, but so far we want to emphasize that such a high rate of dependency is unexpected, since the normative establishes that the length of participation in the program is between three and six months.

An additional exercise is to compute poverty effects excluding all those individuals who actually participated in the program, but according to their socioeconomic characteristics were not eligible to participate. As we discussed in the targeting section more than one third of participants did not meet the eligibility criteria. While this exercise leads to biased estimates of the program effects, it is useful from a policy perspective since it provides an estimate of what the program effects would have been if benefits were allocated properly. The difference-in-difference estimator, including only eligible individuals, is a positive effect on total monthly income equal to US\$80.2 (and t-value equal 6.84). On the other hand, the difference-in-difference estimator including only ineligible individuals is

US\$11.1 (0.44), suggesting that the inadequate allocation of benefits partially explains why the average income effect is quite low<sup>36</sup>.

Summing up, we estimate that during treatment participants received a monthly income US\$44.9 higher than what they would have earned in the absence of the program, representing almost 25 percent of the benefit. This result does appear to be partially driven by the inadequate allocation of some of the benefits. When we restrict the sample to eligible individuals, the estimated income effect is US\$80.2. We also find that women benefited more from the program than men. Finally, we observe that 43 percent of those individuals who were participants during May 2001 were also participating in the program during October 2000. We will return to this issue later. Now we turn to compute the percentage of households who moved out of poverty and/or indigence due to the program.

Following INDEC's definition of poverty (see Appendix 2) and using the estimated mean income effect of the program (US\$44.9 per month), we find, for October 2000, that: While the actual percentage of participants below the indigence line was 22.1 percent, in the absence of the program the figure would have been 28.1 percent. Similarly, while the actual percentage of participants below poverty was 57.4 percent, in the absence of the program it would have been 60.9 percent.

**Table 12 Estimates of Indigence and Poverty rates for Participants in the absence of treatment (based on an estimated income effect equal to US\$44.9 per month), October 2000**

	Participants
Non-Poor	39.1%
Poor	60.9%
- Poor but not Indigent	32.8%
- Indigent	28.1%
Total	100%

Note: The figures are for individuals.

Considering that during October 2000 the number of beneficiaries was 100,000 we estimate that as a consequence of the program 6,000 households moved out of indigence, and 3,500 moved out of poverty. We expand these figures by average household size, leading to an estimated reduction in the number of people below the indigence line of 38,000 and a reduction in people below poverty of 19,700, implying approximately a 1.2 percentage point reduction in Argentina's indigence rate and a 0.2 percentage point reduction in the poverty rate.

<sup>35</sup> Their estimation is for the year 1997 and they used a different database. These two factors might explain the discrepancy in the results. However, we suspect that the difference could also be due to a potential bias on the sample used in Jalan and Ravallion (2003) as discussed in footnote 12.

<sup>36</sup> According to the news reports surveyed by Ronconi (2001), another plausible reason that explains why the program effect on income is smaller than the benefit is that a fraction of the benefit (between 5 and 50 pesos) was actually not received by the participant, but kept by local political bosses in exchange for giving people access to the program.

### 5.3 Employability effects

In this section we analyze the labor performance of participants after treatment. We estimate program effects using equation (2) as described in the methodological section. In other words, we study the labor market performance of participants after program completion, both with respect to their labor performance before entering into the program, and with respect to the comparison group.

We begin studying labor outcomes five months after treatment (i.e. October 2001). Out of the 655 individuals who were program participants during May 2001, we only observe 212 during both October 2000 and October 2001. We also observe the performance of 1,146 individuals in the comparison group during the same period. Column 1, table 13, presents the difference-in-difference estimates of the program effects between October 2000 and October 2001. We observe an increase in total monthly income equal to US\$26.5, an increase in monthly income in the main occupation US\$23.3 and an increase in hourly earnings US\$0.13<sup>37</sup>.

**Table 13 Difference in difference estimates of Workfare Program. October 2001 (i.e. after treatment) and October 2000 (i.e. before treatment), Participants and Control Group**

Dependent variable	Column (1)	Column (2)
Income in the main occupation	23.3 (1.68)	24.1 (0.88)
Total Income	26.5 (1.65)	29.4 (0.89)
Hourly Income in the main occupation	0.13 (0.98)	0.09 (0.49)
Be employed	-	0.037 (0.66)
Formal Job (social security coverage)	-	0.043 (0.093)
Participate in Labor Force	-	0.090 (2.08)
Be unemployed	-	0.053 (1.26)

Note: t-values reported in parentheses. The sample used in column (1) includes 212 workfare participants, and the sample used in column (2) includes 74 workfare participants.

However, 67 individuals out of the 212 appear to be participating in the program during the three periods under consideration. Additionally, 45 appear to be participating both during May 2001 and October 2001, and 26 appear to be participating both during May 2001 and October 2000.

We drop all those 138 observations, and keep the individuals who were participating during May 2001, but were not participating neither during October 2000 nor during October

2001 in order to properly measure before and after treatment outcomes<sup>38</sup>. The estimates for this restricted sample are in column 2. We find that treatment is correlated with an increase in total monthly income by US\$29.4, monthly income in the main occupation by US\$24.1 and hourly earnings by US\$0.09. Treatment is also correlated with a higher probability of being employed 3.7 percent, higher probability of having a formal job 4.3 percent, higher probability of participating in the labor force 9 percent and higher probability of being unemployed 5.3 percent<sup>39</sup>.

Before proceeding to discuss these results we should notice that, except for the estimate of the program's impact on the probability of participating in the labor force, all the other estimates are not statistically significant. Three different interpretations for the lack of statistical significance of the estimates are plausible. First, by taking into account that the labor market is not frictionless, it could be argued that ex-participants actually improved their skills during their participation in the program but were not able to take advantage of those new skills few months after program completion. According to this hypothesis it is necessary to analyze the performance of participants several months after program completion to uncover the employability effects of the program. A second interpretation is that the program actually had an impact on the performance of ex-participants even a few months after participation, but the results we find are statistically insignificant due to the low number of observations. A third interpretation is that participants did not improve their human capital during participation and the results are simply reflecting that.

Besides this discussion some results are worth considering. A few months after program completion ex-participants appear to have a statistically significant higher propensity to participate in the labor market. The labor force participation rate of the comparison group did not change much (it was 63.9 percent in October 2000 and 63.1 percent in October 2001), but the labor force participation rate of participants increased by 8.1 percentage points during that period. This result is mainly driven by females.

We now study program effects by analyzing the performance of participants twelve months after treatment. The May 2002 survey includes 116 individuals out of the 655 individuals who were program participants during May 2001 and were also surveyed in

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<sup>37</sup> When we compute the estimates by gender we find that females benefited more than males as before. The estimates (and t-values) for females are: US\$35.2 (2.05) effect in total income, US\$30.8 (2.16) effect in income in main occupation, and US\$0.21 (1.08) in hourly earnings. For males the figures are: US\$13.6 (0.45), US\$15.1 (0.56), and US\$0.02 (0.10) respectively.

<sup>38</sup> Dropping participants biases the estimates as we discuss below. However, we consider worth presenting the estimates obtained using this restricted sample since they literally compare the income of participants before and after treatment with respect to the comparison group.

<sup>39</sup> The seemingly contradicting result that treatment increases both the probability of being employed and unemployed is simply explained by the finding that treatment increases the probability of participating in the labor force by a larger amount than finding a job.



October 2000, and 580 individuals in the comparison group. The difference-in-difference estimates of the program effects between October 2000 and May 2002 are (column 1, Table 14): An increase in total monthly income equal to US\$59.7, an increase in income in the main occupation equal to US\$48.5, and an increase in hourly earnings US\$0.31.

**Table 14 Difference in difference estimates of Workfare Program. May 2002 (i.e. after treatment) and October 2000 (i.e. before treatment), Participants and Control Group**

Dependent variable	Column (1)	Column (2)
Income in the main occupation	48.5 (2.43)	53.3 (1.54)
Total Income	59.7 (3.07)	84.2 (2.38)
Hourly Income in the main occupation	0.31 (1.53)	0.15 (0.41)

Note: t-values reported in parentheses. The sample used in column (1) includes 116 workfare participants, and the sample used in column (2) includes 34 workfare participants.

However, these are not literally 'before and after treatment' estimates because a large fraction of those who participated during May 2001 appear also to be participating in October 2000 and/or in May 2002. More specifically, 34 percent of those individuals who were participating during May 2001 were also participating during both October 2000 and May 2002. This means that one third of participants received the benefit for at least 19 consecutive months. Also, 20 percent were participating during both October 2000 and May 2001 but not during May 2002, and 16 percent were participating during both May 2001 and May 2002 but not during October 2000. Adding up these figures, they imply that 70 percent of those individuals who entered the program stayed for more than six months.

This result is unexpected since the normative establishes that the length of the program is between three and six months. While renewal of benefits was not explicitly prohibited, there was an implicit solidarity objective in the program. The idea was to distribute the scarce benefits among as many poor people as possible. Hence, those applicants who did not participate before have priority over those who did participate and - as we already showed - during the period under consideration there was a large number of low skilled and unemployed people who never received the benefit. While this result does not prove the existence of political clientelism in the allocation of benefits, it is consistent with that presumption. Furthermore, it is consistent with the claim that a poorly implemented social policy would lead to dependency.

Since the estimates in column 1 are not literally 'before and after treatment' estimates, we proceed to compute truly 'before and after treatment' estimates by keeping only all those individuals who participated during May 2001 but did not participate during

both October 2000 and May 2002 (column 2, table 14): Treatment is related to an increase in total monthly income equal to US\$84.2, and an increase –although statistically not different from zero- in hourly income.

While these are truly ‘before and after estimates’ they are presumably not true treatment effects. The reason is that dropping from the sample those participants who stayed longer than the established program length biases the estimates if those who left the program are different from those who stayed. To the extent that workfare has operated more as permanent unemployment insurance than as a fixed term program, then restricting the sample to those who left the program overestimates treatment effect: Once individuals are able to enter into the program they remain participating until being offered a sufficiently good job in the labor market. The fact that the estimated total monthly income effect of the program excluding those who stayed participating is 40 percent larger with respect to the estimate obtained including them (i.e. column 2 versus column 1 in Table 14), supports this interpretation. We leave the need to control for *selection out of the program* for future research.

Another obvious limitation of the available data is that we can only follow the same individual for a relatively short period of time: only two years. Presumably, a more accurate assessment of the program would be obtained if data for several periods before and after treatment were available.

#### **5.4 Are Employers willing to hire workfare participants?**

Before concluding, it is interesting to analyze the opinion of employers regarding their predisposition to hire participants of workfare programs. The *Ministerio de Trabajo* conducted a survey among 1,290 firms asking employers if they would hire a workfare participant in case she/he meets the qualifications needed for the job<sup>40</sup>: 78 percent of the employers answered yes, and 22 percent no. Among those who answered yes, 86 percent said that they would hire a program participant conditional on the skills of the worker, 47 percent conditional on the experience of the worker, and 24 percent because hiring a workfare participant implies that the firm receives a subsidy from the state<sup>41</sup>. Among those who answered negatively, 52 percent considers that workfare participants do not have enough skills and experience, and 45 percent mentioned that they would not hire a participant because they are problematic and not trustworthy.

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<sup>40</sup> The survey was conducted among medium and large firms that operate in the formal sector, during 2002 (when the *Jefes de Hogar* program was already implemented), in Greater Buenos Aires, Rosario, Cordoba and Mendoza (the four largest urban agglomerates in Argentina). See the *Encuesta de Indicadores Laborales* at the *Ministerio de Trabajo* webpage: [www.trabajo.gov.ar](http://www.trabajo.gov.ar)

**Table 15 Predisposition of employers in the private sector to hire workfare program participants: “In case a participant of the program *Jefes de Hogar* meets the qualifications needed for a job, would you hire her/him?” (multiple answer).**

Yes if...	78%	No because...	22%
She/he meets the qualifications	84%	Most of them do not have either the skills or the experience	52%
She/he has experience	47%	They do not have references	49%
She/he has the needed education	31%	They are problematic, not trustworthy	45%
Reduces costs due to the \$150 state subsidy	24%	Their skills are obsolete	27%
Others	2%	Others	16%

Source: Ministerio de Trabajo, Encuesta de Indicadores Laborales

The survey does not provide the ideal information to assess the employability effect of workfare programs. Employers are asked about their predisposition to hire participants -in which case they would receive a subsidy. However, we would like to know if employers have any preference for *ex*-participants relative to comparable workers who never participated in the program. Second, the questionnaire only allows employers to express their motives choosing between the options: “Yes, if” and “No, because”. More adequate options would have been: “Yes, because” and “No, because”. In any case, and taking into consideration these caveats, we interpret the opinions of the employers as evidence that workfare programs may not improve the skills of participants. The fact that 22 percent of the employers mentioned that they would not hire program participants despite the US\$150 subsidy supports that interpretation. Moreover, the results suggest that participating in workfare programs may have, to some extent, a negative effect on the employability of workers because some employers believe that workfare participants are prone to conflict. However, this stigmatization effect does not appear to be large since only 10 percent of all employers reported that concern.

### Concluding Remarks

A mixed picture arises after analyzing targeting, poverty and employability effects of workfare programs in Argentina during 2000 and 2001.

On the one hand, an active labor policy targeted at the least skilled seems particularly appropriate considering the increase in poverty and unemployment that Argentine society suffered during this period. We observe that the policy was actually pro-poor, and the average participant had less human capital than the average non-participant.

<sup>41</sup> A firm that hires a *Jefes de Hogar* participant receives a subsidy equal to US\$150 from the state, and has to pay the payroll tax and at least the minimum wage. These two costs account for approximately US\$350.

Targeting towards the poor also improved over time. More women than men received treatment, which is presumably a positive characteristic of the program considering that women have fewer opportunities in the labor market. The program was effective in increasing participants' income and reducing poverty particularly during treatment: We estimated that during treatment income increased by approximately US\$50, helping 3.5 percent of participating households move out of poverty and 6 percent out of indigence, implying that 38,000 people moved out of indigence and 20,000 out of poverty thanks to workfare programs. Moreover, we estimate that in the absence of the program one third of participants would have had remained inactive or unemployed. After program completion, we observe that ex-participants perform worse than during treatment but better than before treatment, which is remarkable considering the deterioration of the economy. The program also increased the propensity to participate in the labor force. These results are consistent with the hypothesis that at least some ex-participants improved their human and/or social capital during treatment.

On the other hand, we observe that a large share of the scarce benefits was allocated to non eligible individuals. During May 2000, approximately one third of participants were individuals with at least some college education or members of households ranked in the top 50 percent of the income distribution. At the same time only 4 percent of eligible individuals were participating. While we estimate that during treatment income increased by US\$50, this figure only represents 25 percent of the benefit. During treatment, the biggest positive program effect was on hourly earnings, mainly due to a reduction in working hours, suggesting that some participants may have chosen to participate in the program instead of working longer hours, probably in the informal sector. We also observe that more than half of those who entered the program stayed for more than six months, which was the program's length established in the normative. Furthermore, 34 percent of participants received the benefit for at least 19 consecutive months. These facts, together with the inadequate allocation of some benefits, are consistent with the hypotheses that some benefits were allocated on the basis of political patronage and that the program generated dependency among recipients.

Finally, the fact that some participants were able to stay for more than the time established in the normative obscures the positive after treatment effect mentioned in the previous paragraph. It seems plausible that the estimated income gain not only reflects an improvement in the human and social capital of participants, but also reflects that those participants who were offered a good job in the labor market are the ones that chose to leave the program. This is consistent with the claim that workfare in Argentina operated more as unemployment insurance than as a training program. Finally, according to the

opinions of employers, they do not express much interest in hiring program participants despite the subsidy they would receive. Moreover, 10 percent of employers expressed their reluctance to hire workfare participants because they consider them prone to conflict.

Regarding the statistical model, there are several plausible alternatives and extensions to the model we used that were already mentioned. Presumably, the most important extension is to model the fact that some participants select when to exit the program.

Finally, from a policy perspective, we want to emphasize that nowadays approximately 2 million individuals –equivalent to almost 15 percent of the labor force- are participating in the *Jefes de Hogar* workfare program. The magnitude of the figure is impressive. While our estimates refer to the workfare programs that preceded *Jefes* (e.g. *Trabajar*), and extrapolating is always risky, these programs are very similar in several dimension making us confident that the estimates obtained in this paper are informative enough to discuss the adequacy of the *Jefes de Hogar* program.

In our opinion, a drastic reduction on the number of beneficiaries is not recommended considering the impact it would have on the well being of participants and their dependents, which in most cases are below poverty. However, it is hard to imagine a healthy future for the Argentine society if such a large percentage of its members continue depending on workfare subsidies. First, the productivity of participants during treatment seems to be very low –actually, many of them are not working at all- affecting growth prospects. Second, we are worried about the effects of workfare on political liberty. So far, the program has been mainly controlled by the executive branch of government and not by an independent body. Incumbent politicians seem to be more interested in maintaining power instead of improving societal welfare, leading to a clientele usage of workfare funds. How can workfare participants vote freely if their main source of income is a workfare benefit received from the executive power and used in exchange for supporting an incumbent politician?

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## Appendix 1. *Encuesta Permanente Hogares –Permanent Household Survey- (EPH)*

The EPH is a sampling survey implemented by the *Instituto Nacional de Estadística y Censos* (INDEC) in 28 urban agglomerates. The rigorous application of statistical methods ensures the validity and reliability of collected information (selection of sample members is conducted using random selection techniques, data collection methods are uniform, etc). A detailed description of sampling and data collection techniques is available at [www.indec.gov.ar](http://www.indec.gov.ar). The sample has a wide representation of the Argentine population as the following table shows:

Provinces	Total population Census 91 (A)	ratio (%) of province to total population	EPH Agglomerates	Total population of EPH agglomerate Census 91 (B)	% EPH sampling over agglomerate population	ration (%) of urban population to total population (B/A)
Ciudad de Bs As	2965403	9,1	Ciudad de Bs As	2965403	100,0	10,4
Buenos Aires	7969324	24,4	Partidos del Conurbano	7948443	99,7	27,9
Catamarca	264234	0,8	Gran Catamarca	121815	46,1	0,4
Cordoba	2766683	8,5	Rio Cuarto(*)	138853	5,0	0,5
			Gran Córdoba	1175400	42,5	4,1
Corrientes	795594	2,4	Corrientes	258103	32,4	0,9
Chaco	839677	2,6	Gran Resistencia	292287	34,8	1,0
Chubut	375189	1,1	C. Rivadavia-Rada Tilly	127038	33,9	0,4
			Rawson-Trelew (***)	97355	25,9	0,3
Entre Ríos	1020257	3,1	Concordia(*)	116485	11,4	0,4
			Gran Paraná	207041	20,3	0,7
Formosa	398413	1,2	Formosa	147636	37,1	0,5
Jujuy	512329	1,6	Jujuy-Palpala	219924	42,9	0,8
La Pampa	259996	0,8	Santa Rosa-Toay	80592	31,0	0,3
La Rioja	220729	0,7	La Rioja	103727	47,0	0,4
Mendoza	1412481	4,3	Gran mendoza	773113	54,7	2,7
Misiones	788915	2,4	Posadas	210755	26,7	0,7
Neuquén	388833	1,2	Neuquén-Plottier	183579	47,2	0,6
Resto de Bs As	4625650	14,2	Bahia Blanca-Cerri	265885	5,7	0,9
			Mar del Plata-Batán(*)	519065	11,2	1,8
			Gran La Plata	642979	13,9	2,3
			San Nicolás de los Arroyos (***)	119302	2,6	0,4
			Carmen de Patagones (***)	17075	0,4	0,1
Río Negro (**)	506772	1,6	Viedma (***)	40398	8,0	0,1
Salta	866153	2,7	Salta	368659	42,6	1,3
San Juan	528715	1,6	Gran San Juan	352691	66,7	1,2
San Luis	286458	0,9	San Luis-El Chorrillo	113074	39,5	0,4
Santa Cruz	159839	0,5	Rio Gallegos	64640	40,4	0,2
Santa Fe	2798422	8,6	Gran Santa Fé	396991	14,2	1,4
			Gran Rosario	1117322	39,9	3,9
			Villa Constitución (***)	41161	1,5	0,1
Santiago del Estero	671988	2,1	Sgo del Estero-La Banda	261824	39,0	0,9
Tierra del Fuego	69369	0,2	Ushuaia-Río Grande	67303	97,0	0,2
Tucumán	1142105	3,5	Gran Tucumán-Tafí Viejo	652882	57,2	2,3
Total País	32633528	100,0		20208800	61,9	71,1
Total Urbano	28439499	87,1				

(\*)Agglomerates introduced in October 1995

Source: EPH-INDEC

## **Appendix 2. Indigence and Poverty line methodology**

This study follows the *Instituto Nacional de Estadísticas y Censos* (INDEC) definition of poverty and indigence. The method used by the INDEC to measure poverty and indigence is presented below.

### **Indigence**

The concept of “indigence level” (or indigence line), IL, aims to assess whether the households earn enough income to purchase a food basket that satisfies a minimum threshold of energetic and protein needs. Thus, a household that does not meet that threshold is considered indigent. The procedure is based on the use of a *Canasta básica de alimentos* -basic food basket- (CBA), determined as a function of the consumption patterns of a reference population defined according to the results of the 1985-86 Household Expenditure and Income Survey. The procedure also takes into account the prescribed kilocalories and protein requirements for that population (as specified in the “Basic Food Basket for the Equivalent Adult”, included below). Once the CBA components have been established, their prices are assigned according to the *Indice de Precios al Consumidor* (CPI) for each measurement period.

Since human nutritional requirements vary according to age and gender, INDEC adjusts for each person’s characteristics, taking as reference the requirements of a male aged between 30 and 59 years old. This reference unit is called the “equivalent adult” and is assigned the value 1. The table of equivalences of energetic requirements for each consumer unit in terms of equivalent adult is presented in the table below.

Each household’s composition in equivalent adults determines a specific CBA value for that household. (For example, in October 2000, the CBA value for an equivalent adult was \$62,4). As a final step, the specific value of each household’s CBA is compared to the household’s total income. If the total income is less than the household’s CBA, the household and its members are considered to be under the indigence level.

### **Poverty**

The measurement of poverty by the poverty level or “poverty line” (PL) method is based on determining, from the household’s reported income, whether the household in question is able to satisfy -through the purchase of goods and services- a set of nutritional and non-nutritional goods considered essential. In order to calculate the poverty line the INDEC determines the CBA value and compounds it with the inclusion of non-nutritional goods and services (clothing, transportation, education, health care, etc.) so as to obtain the value of the *Canasta básica total* -total basic basket (CBT).



**Table of equivalences**  
**Energetic needs and consumer units by age and sex**

Greater Buenos Aires		
Sex and age	Energetic needs (Kcal)	Consumer unit / Equivalent adult
Boys and girls		
Under 1 year old	880	0.33
1 year old	1170	0.43
2 yrs. Old	1360	0.50
3 yrs. Old	1500	0.56
4 to 6 yrs. Old	1710	0.63
7 to 9 yrs. Old	1950	0.72
Men		
10 to 12 yrs. Old	2230	0.83
13 to 15 yrs. Old	2580	0.96
16 to 17 yrs. Old	2840	1.05
Women		
10 to 12 yrs. Old	1980	0.73
13 to 15 yrs. Old	2140	0.79
16 to 17 yrs. Old	2140	0.79
Men		
18 to 29 yrs. Old	2860	1.06
30 to 59 yrs. Old	2700	1
60 yrs. old and over	2840	1.05
Women		
18 to 29 yrs. Old	2000	0.74
30 to 59 yrs. Old	2000	0.74
60 yrs. old and over	1730	0.64

For the purpose of compounding the CBA value, the so-called Engel coefficient (EC) is used. The EC is defined as the ratio of food expenditures to total expenditure observed in the reference population in the base year (1985-86). Thus:

$$\text{Engel coefficient} = \text{Food expenditures} / \text{Total expenditure.}$$

In each period, both the numerator and the denominator of the Engel coefficient are updated with the price variations obtained from the CPI. According to the relative price variation, the EC is determined each month for the purpose of measuring poverty. In order to compound the CBA value, in practice its value is multiplied by the reciprocal of the Engel coefficient:  $\text{CBT} = \text{CBA} \times 1/\text{Engel coefficient}$ .

In October, 2000, the reciprocal of the Engel coefficient was 2.42 and the CBA was 62.44 pesos. Thus we have \$ 62.44 (CBA) x 2.42 (reciprocal of EC) = \$ 151.10 (CBT) for an equivalent adult). As a last step, each household's CBT value is compared to the household's total income. If the household's income is less than the CBT value, the household and its members are considered under the poverty line; otherwise, they will be considered as non-poor.

For October 2001 the INDEC estimates that the CBA is \$61, and the CBT \$150.1. For October 2002 the CBA is \$104.9, and the CBT \$231.8 (The large increase in both CBA and CBT during October 2002 reflects the significant increase in prices experienced in Argentina during that year).

**Appendix 3. Basic statistics for Participants and Comparison group, May 2001**

	Participants		Comparison Group	
	Mean	Std. Dev.	Mean	Std. Dev.
Age	35.0	11.9	35.0	11.9
Male	0.42	0.49	0.42	0.49
Head Household	0.38	0.49	0.37	0.48
Born different province	0.20	0.40	0.20	0.40
Foreign born	0.05	0.23	0.05	0.21
Attending educational institution	0.11	0.31	0.10	0.30
Primary school graduate	0.29	0.45	0.29	0.45
High school dropout	0.23	0.42	0.22	0.42
High school graduate	0.16	0.37	0.17	0.38
Some college	0.12	0.32	0.11	0.31
College graduate	0.06	0.24	0.06	0.24
No. dependents	2.16	2.00	2.22	2.17
Rooms per member	0.72	0.52	0.72	0.50
Residence in shantytown	0.04	0.20	0.04	0.21
Lack of access to basic services	0.06	0.25	0.07	0.26
Property ownership	0.72	0.45	0.72	0.45
Northwest	0.15	0.35	0.15	0.35
Northeast	0.11	0.31	0.11	0.31
Cuyo	0.16	0.37	0.16	0.37
Pampeana	0.32	0.47	0.32	0.47
Patagonica	0.19	0.39	0.19	0.39
No. observations	655		3,275	

Note: The omitted categories are: Born in the same province, Primary school dropout and greater Buenos Aires.