

## Estimates of the Benefit Incidence of Workfare

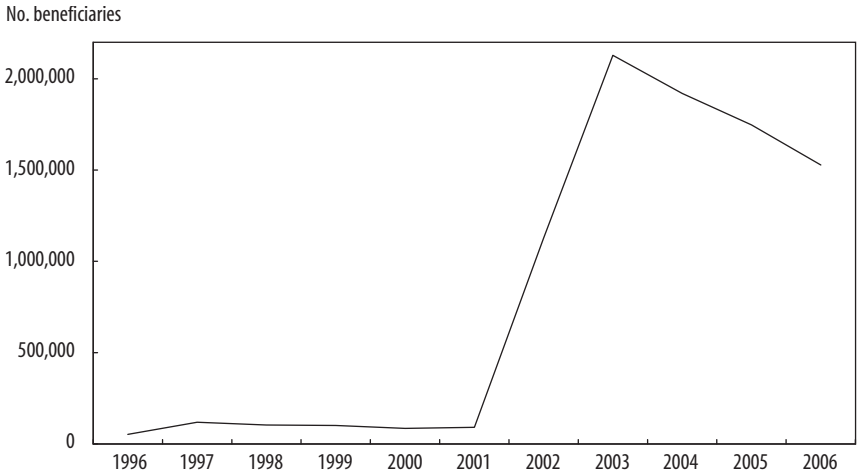
In 1996 the Argentine government, with financial and technical assistance from the World Bank, implemented a temporary public works program known as *Programa Trabajar*. The program was targeted toward poor and unemployed people, and a main objective was to increase the current income of recipients. Program participants received a maximum benefit of 200 pesos per month (equal to the legal minimum wage) in exchange for working in community projects. Approximately 80,000 people a year participated in the program between 1996 and 2002, representing less than one percent of total employment.

In 2001 Argentina suffered a severe political and economic crisis. The poverty rate increased from 30 percent to more than 50 percent in a year. A new government took office in 2002, and one of its principal policies was to make the public works program universal. The number of beneficiaries expanded exponentially, reaching two million people in a country of thirty-nine million. The name of the program was also changed from *Trabajar* to *Jefes de Hogar*. Figure 1 shows the monthly average number of beneficiaries of workfare programs between 1996 and 2006. The change in program coverage is clearly evident where the figure spikes in 2003.

A fundamental belief underlying this policy is the effectiveness of self-targeting. The idea is that, even if the government has a poor capacity to enforce the eligibility criteria, only those who are in real need will participate

Lucas Ronconi is with the University of California at Berkeley.

I thank María Laura Alzúa, John Cockburn, Rafael di Tella, Habiba Djebbari, Francisco Ferreira, Carmen Pagés, Steven Raphael, and David Rosas for helpful comments. This work was carried out with financial and scientific support from the Poverty and Economic Policy (PEP) Research Network, which is financed by the Australian Agency for International Development (AusAID) and the Government of Canada through the International Development Research Centre (IDRC) and the Canadian International Development Agency (CIDA).

**FIGURE 1. Beneficiaries of Workfare Programs in Argentina, 1996–2006a**

Source: Argentine Ministry of Labor, Employment, and Social Protection, *Boletín de Estadísticas Laborales* (available online at [www.trabajo.gov.ar/left/estadisticas/bel/index.asp](http://www.trabajo.gov.ar/left/estadisticas/bel/index.asp)).

a. Includes beneficiaries of *Programa Trabajar*, *Servicios Comunitarios*, *Programa de Emergencia Laboral*, and *Programa Jefes de Hogar*.

in a program that imposes work requirements and provides low compensation.<sup>1</sup> People who are not in need will choose not to participate since their opportunity cost is higher than the program benefit. Furthermore, the effect of the program on the current income of participants is expected to be positive and large (that is, close to the program benefit), since forgone income is low among those who decide to enter the program. This argument, however, assumes that the work requirement is effectively enforced. If this is not the case, the opportunity cost of participation is lower, and self-targeting weakens. Moreover, the effect of treatment on current income can be negative for some participants. If the hourly earnings during treatment are sufficiently high, people with a strong preference for leisure will prefer participation instead of employment even at the cost of a reduction in total income.<sup>2</sup>

1. Subbarao (1997, 2003).

2. For participants with a low preference for leisure, conversely, ignoring the program's work obligations would generate larger income effects, since they have the time to perform other income-generating activities.

Analyzing the allocation of funds is not trivial, given that enforcement is usually low in Argentina and political institutions are far from transparent.<sup>3</sup> The benefit incidence of the program is thus an empirical question. In an earlier paper, I report anecdotal evidence, mainly from newspapers, documenting cases in which political connections determined selection into the program, participants did not meet the eligibility requirements or did not comply with the work requirement, and political leaders used the resources to buy votes.<sup>4</sup> Evidence on the magnitude of the phenomenon, however, is lacking.

The Argentine Ministry of Labor, Employment, and Social Protection (MOL) collected a sample of *Trabajar* participants in 1997 (hereafter, the MOL sample). Jalan and Ravallion combine this sample of participants with a national household survey (namely, *Encuesta de Desarrollo Social*), and find that the program was very well targeted toward people in need: 80 percent of participants were in the poorest quintile of the household per capita income distribution.<sup>5</sup> They also estimate an average income gain during treatment between 92 and 157 pesos per month, representing between half and three-quarters of the program benefit. Good targeting and a large income gain led to an important reduction in poverty in the short run.

The international evidence on the effectiveness of public works programs is mixed. Dar and Tzannatos review about a hundred evaluations—mainly in developed countries—and find that program design and the context in which the program operates matter a great deal.<sup>6</sup> Betcherman, Olivas, and Dar expand the previous study by adding eighty-seven evaluations—many from developing countries—and arrive at the same conclusion.<sup>7</sup> Subbarao suggests that the observed variation in the effectiveness of public works programs across countries is partially explained by differences in institutional capacity.<sup>8</sup>

In this paper, I use alternative samples of workfare participants in Argentina to reassess targeting and income effects between 1997 and 2002. The next section explores poverty targeting using four samples of workfare

3. IDB (2005).

4. Ronconi (2002).

5. Jalan and Ravallion (2003). Beyond estimating the benefit incidence of the *Trabajar* program, this paper makes a number of important methodological contributions to the evaluation literature. It is one of the first applications of propensity score matching, and it underscores the importance of using the same questionnaire for both participants and nonparticipants.

6. Dar and Tzannatos (1999).

7. Betcherman, Olivas, and Dar (2004).

8. Subbarao (2003). For country-specific program evaluations, see Haddad and Adato (2001) on South Africa; Gaiha and Imai (2002) and Lanjouw and Ravallion (1999) on India; Newman, Jorgensen, and Pradhan (1991) on Bolivia; and Lee (2000) on Korea.

participants collected by the National Institute of Statistics and Censuses (INDEC). I find that while workfare beneficiaries are poorer, on average, than nonparticipants, targeting in all these samples is worse than in the MOL sample. I also find that 11 percent of beneficiaries participate in political parties, compared to only 3 percent in MOL. The paper then computes average income effects during treatment using longitudinal data between October 2000 and May 2002, controlling for pretreatment income and unobserved time-invariant heterogeneity. I also allow treatment effects to be negative. I find an average treatment effect on current income of 60 pesos per month, approximately one-third of the benefit.

A reevaluation is merited for three reasons. First, the MOL sample is likely to exclude participants who do not meet the eligibility criteria. Second, income effects have been estimated assuming that treatment has a nonnegative impact on current income, which ignores the possibility that participants might choose a lower total income in exchange for leisure. Third, the cross sectional nature of the MOL sample prevents controlling for pretreatment income dynamics and time-invariant heterogeneity, two factors that, when ignored, usually lead to biased estimates.

## Targeting

Temporary public works programs have been increasingly used in Argentina since the mid-1990s. Between 1996 and early 2002, a number of programs were in operation, among which *Trabajar* was the largest one.<sup>9</sup> These programs were replaced in 2002 by *Programa Jefes de Hogar*. While the programs had different names, they all shared the same basic features: they were targeted toward poor and unemployed workers, participants were required to work between thirty and forty hours per week, treatment lasted between three and six months, and participants received a maximum compensation of 200 pesos per month.<sup>10</sup> The minimum wage during that period was also 200 pesos, although about three-fourths of the economy was covered by collective agreements establishing a minimum wage that varied between 300 and 350 pesos per month depending on the industry.

Were the benefits allocated among poor and unemployed workers as intended? Given that the wage rate was set low in the public works programs

9. The other programs were *Servicios Comunitarios*, *Programas Especiales de Empleo*, and *Programa de Emergencia Laboral*.

10. The *Programa de Emergencia Laboral* provided a benefit of 160 pesos.

and that work requirements were legislated, only low-income people are expected to have entered the program. This argument, however, is based on the assumption that the work requirement is effectively enforced. If the government does not enforce the work requirement, people who are not in need would find it profitable to enter the program, even if the program benefit is set low. Evaluating targeting is an empirical question. It appears to be a simple task, since it only requires a representative sample of participants and non-participants. Nevertheless, collecting a representative sample of participants can be an important challenge, particularly when benefits are not allocated as established in the normative. Participants who are aware that they do not fulfill the eligibility criteria have incentives to misreport their true status or refuse to answer a questionnaire.

I analyze four household surveys conducted by the National Institute of Statistics and Censuses (INDEC): the October 2000 and October 2001 surveys of the Permanent Household Survey (*Encuesta Permanente de Hogares*, EPH), the 2001 Quality of Life Survey (*Encuesta de Calidad de Vida*, ECV), and the 1997 Social Development Survey (*Encuesta de Desarrollo Social*, EDS). The EPH covers thirty major urban agglomerates, the ECV covers fifty-two localities, and the EDS covers 114 localities. The number of individuals fifteen to sixty-four years old who report that they are participating in a workfare program (with earnings between 150 and 200 pesos a month) is 297 in the October 2000 EPH, 295 in the October 2001 EPH, 187 in the 2001 ECV, and 159 in the 1997 EDS. Regrettably, these surveys do not identify the program in which people participate. All temporary public works program, however, were targeted toward poor and unemployed workers, the wage was set at a maximum of 200 pesos per month, and work requirements were legislated. According to the self-targeting hypothesis, only people in need would enter into any of these programs.

Columns 1 through 4 in table 1 compare the location of these four samples of workfare participants in the national distribution of per capita household income. Column 5 reports Jalan and Ravallion's findings based on a sample of *Trabajar* participants collected by the Argentine Ministry of Labor, Employment, and Social Protection (MOL) in 1997.<sup>11</sup> While the samples collected by INDEC cover different jurisdictions and years, and targeting can vary across regions and over time, columns 1 through 4 indicate that a similar proportion of beneficiaries in each sample is in the bottom quintile (between 53 and 58 percent) and in the top half of the distribution (between

11. Jalan and Ravallion (2003).

**TABLE 1. Location of Different Samples of Workfare Participants in the National Distribution of Household Income per Capita<sup>a</sup>**

<i>Decile</i>	<i>EPH October 2000</i> (1)	<i>EPH October 2001</i> (2)	<i>ECV 2001</i> (3)	<i>EDS 1997</i> (4)	<i>MOL 1997<sup>b</sup></i> (5)
First (poorest)	36.7	37.2	40.1	42.1	60.1
Second	16.5	21.1	15.0	10.7	18.5
Third	11.5	12.9	11.2	15.7	9.5
Fourth	8.1	6.2	7.5	8.2	5.8
Fifth	10.8	7.8	7.5	4.4	1.9
Sixth	8.1	5.9	7.5	6.9	1.6
Seventh	4.4	4.3	7.0	4.4	1.6
Eighth	2.7	0.6	2.7	4.4	0.5
Ninth	1.0	3.4	1.1	1.9	0.3
Tenth (richest)	0.3	0.7	0.5	1.3	0.1
Total	100.0	100.0	100.0	100.0	100.0

a. Income from participation is excluded.

b. Column 5 is from table 2 in Jalan and Ravallion (2003).

15 and 19 percent). These results contrast with column 5 (the MOL sample), where 80 percent of participants are in the poorest quintile and less than 5 percent are in the top half. Workfare beneficiaries in the samples collected by INDEC are poorer, on average, than nonparticipants, but workfare programs appear to be less well targeted toward the poor than the MOL sample suggests.

Table 2 compares the socioeconomic characteristics of participants in the MOL sample with workfare participants and nonparticipants in the EDS. Both surveys were conducted in 1997. The average monthly per capita household income is only 73 pesos in the MOL sample, but it is 127 pesos among workfare participants in the EDS, and the difference is significant at the 1 percent level. The figure among nonparticipants is 336 pesos, which supports the argument that workfare benefits have usually been allocated to poorer people, but targeting does not appear to be as good as the MOL sample suggests. The same conclusion emerges after comparing other socioeconomic characteristics: workfare participants in the EDS are more likely to own various consumer durables than participants in the MOL sample, but less likely to do so than the population of nonparticipants. Interesting, only 3 percent of workfare beneficiaries participate in some form of political organization in the MOL sample, versus 11 percent among beneficiaries in the EDS. One possible explanation is that the MOL sample may be biased toward excluding participants who entered the program as a result of political favoritism. Workfare beneficiaries who participate in a political party

**TABLE 2. Descriptive Statistics of Two Samples of Workfare Participants and a National Sample of Nonparticipants<sup>a</sup>**

Variable	<i>Trabajar</i> participants in MOL sample (1)	<i>Workfare</i> participants in EDS sample (2)	<i>Nonparticipants</i> in EDS sample (3)
Per capita income (pesos/person/month)	73.21 (101.84)	126.59 (123.51)	335.72 (479.50)
Share of households that need to borrow to meet basic needs (%)	32.78 (0.89)	21.55 (0.41)	17.12 (0.38)
Share of households own telephone (%)	22.66 (0.79)	26.26 (0.37)	60.34 (0.49)
Share of households own color TV (%)	75.60 (0.81)	90.92 (0.29)	91.58 (0.28)
Share of households owning a refrigerator with built-in freezer (%)	26.45 (0.83)	31.27 (0.47)	48.44 (0.50)
Share of households owning an automatic washing machine (%)	11.66 (0.61)	14.81 (0.36)	37.80 (0.48)
Share of population participating in some form of political organization (%)	2.91 (0.32)	10.98 (0.31)	3.13 (0.17)

a. Population weighted averages. Column 1 is from table 1 in Jalan and Ravallion (2003). As in that paper, the sample of participants is restricted to people between 15 and 64 years old who report earnings from the program between 150 and 200 pesos per month; this implies a sample of 2,802 participants in the MOL sample and 159 in the EDS sample. Differences in means between column 1 and 2 are statistically different from zero at the 1 percent level for all variables except the share owning a telephone, refrigerator, or washing machine. Standard deviations are in parentheses.

earn almost twice as much as program beneficiaries who do not participate in a political party.<sup>12</sup>

There are reasons to suspect the MOL sample is not representative of *Trabajar* participants. The sample includes 2,802 participants obtained from 350 randomly selected projects, but there is no information on how many people were participating in those 350 randomly selected projects. According to the program guidelines, each project should have between five and a hundred participants.<sup>13</sup> When I interviewed Osvaldo Giordano, who was Secretary of Employment and Labor Training in 1997, he mentioned that most projects had between twenty-five and a hundred participants, implying that 350 projects would usually include between 8,750 and 35,000 individuals. Furthermore, in the samples of workfare participants collected by INDEC, the

12. The average monthly per capita household income is 200 pesos for the first group and 117 pesos for the second group.

13. See article 4 of Resolution 202/1997, Argentine Ministry of Labor, Employment, and Social Protection, Secretariat of Employment and Labor Training.

participants report that, on average, sixty-eight individuals worked in the same place they did.<sup>14</sup> Based on a conservative assumption that the average *Trabajar* project included thirty individuals, then a sample of 2,800 participants obtained from 350 randomly selected projects only represents a quarter of the truly random sample.

Those 2,800 participants would still be representative of the population of *Trabajar* participants if the missing observations were randomly selected. The authors report, however, that the lost observations involve participants who provided an incomplete or nonexistent address or refused to respond. Given that participants who do not meet the eligibility criteria are presumably less likely to provide their addresses to the Ministry of Labor, Employment, and Social Protection or to respond to interviews, the sample is probably biased towards excluding ineligible participants.

The randomness of the samples of participants in the EDS, EPH, and ECV surveys can also be questioned, since ineligible participants may be more likely to misreport their true status to INDEC. If this is the case, then targeting would be even worse than the figures reported in tables 1 and 2. Considering that INDEC has no control over workfare funds and that it ensures the confidentiality of the data, misreporting is probably less severe than in the MOL sample. While these arguments are speculative, I believe that all the evidence presented above supports the claim that targeting was less pro-poor than previous estimates suggest. Setting the program benefit low and legislating work requirements does not necessarily imply self-targeting if the work requirements are not enforced.

## **Income Effects during Treatment**

Let  $D$  be a binary workfare treatment indicator, where  $D = 1$  denotes treatment and  $D = 0$  otherwise, and  $Y^1$  and  $Y^0$  are the outcome with and without treatment. The difference in outcomes with and without treatment,  $Y^1 - Y^0$ , measures the effect of treatment. The fundamental difficulty in estimating the effect of treatment is that the two potential outcomes are never both observed for any one person.

14. The figure is obtained from the 1997 EDS, the 2001 ECV, and the October 2000, May 2001, October 2001, and May 2002 EPH surveys. In all cases, I restrict the sample to workfare participants aged fifteen to sixty-four, who report earnings between 150 and 200 pesos a month from the program.



Estimating program effects would be simple if  $D$  were statistically independent of  $(Y^1, Y^0)$ , as would occur when treatment is randomly assigned. However, participation in workfare programs was not randomly assigned in Argentina. People chose to apply, and the government allocated benefits among a share of applicants. Under these circumstances, it is necessary to rely on nonexperimental methods to measure treatment effects.

A convenient and conceptually appealing nonexperimental method is to use propensity score matching to select a comparison group from a sample of nonparticipants, and then compare outcomes between participants and nonparticipants with similar observable characteristics.<sup>15</sup> Let  $p(X)$  denote the propensity score, and let  $p(X) = \text{Prob}(D = 1|X)$ , the probability of participating conditional on the observed set of covariates  $X$ . Based on the estimated propensity score, nonparticipants are matched to participants, and the average effect of treatment on the treated ( $ATT$ ) is then computed as follows:

$$(1) \quad ATT = \frac{1}{N} \sum_{j=1}^N \left( Y_j^1 - \frac{1}{NP_j} \sum_{i=1}^{NP_j} Y_{ij}^0 \right),$$

where  $Y_j^1$  is the income of participant  $j$  during treatment,  $Y_{ij}^0$  is the income of the  $i$ th nonparticipant matched to the  $j$ th participant,  $N$  is the total number of participants, and  $NP_j$  is the total number of nonparticipants matched to the  $j$ th participant.

The credibility of propensity score matching, as well as other nonexperimental methods, hinges on the assumption that treatment depends on a set of covariates the econometrician observes ( $X$ ) and not on the unobservables determining  $(Y^1, Y^0)$ . Whether nonexperimental methods can replicate experimental results is open to debate.<sup>16</sup> Researchers generally agree, however, that nonexperimental methods are more credible when the econometrician controls for pretreatment income histories, since participants usually experience a decline in earnings before entering the program.<sup>17</sup>

Jalan and Ravallion use propensity score matching to estimate the income effect of workfare in Argentina.<sup>18</sup> They use the MOL sample of participants

15. Rosenbaum and Rubin (1983).

16. Dehejia and Wahba (1999); LaLonde (1986).

17. Ashenfelter and Card (1985).

18. Jalan and Ravallion (2003).

described in the previous section and obtain the comparison group from the EDS. The authors find large average income gains of 92 to 157 pesos a month during treatment, which represents between half and three-quarters of the program benefit. These estimates raise three issues. First, if the MOL sample of participants is biased toward excluding people who do not meet the eligibility criteria as argued above, and if treatment effects differ between eligible and ineligible participants, then the estimated average treatment effect is biased. Jalan and Ravallion find larger treatment effects among participants with lower estimated pretreatment income per capita, suggesting that excluding ineligible participants leads to an overestimation of the true average treatment effect.

Second, Jalan and Ravallion compute the income gains from the program as follows: "If the income of the participant is less than the income of the matched nonparticipant, we treat the impact to be zero rather than the observed negative number."<sup>19</sup> The rationale for imposing this restriction derives from the self-targeting hypothesis, according to which people choose to enter into the program if the expected income during participation is higher than the income from not participating. This restriction is realistic when working conditions are equal between the two choices. Participants earning 200 pesos per month, however, report working an average of only 26.5 hours per week, which means they earn 1.7 pesos an hour. Employed nonparticipants earning the minimum wage (also 200 pesos per month), in contrast, report working an average of 35.2 hours per week, with hourly earnings of 1.3 pesos.<sup>20</sup> This evidence suggests that some individuals might have preferred to participate in a *Trabajar* project so as to work fewer hours, despite earning a lower total income than they would receive in the labor market. Under these circumstances, assuming a nonnegative income effect for each participant is too restrictive and overestimates the true effect.

Finally, the lack of longitudinal data prevented the authors from controlling for pretreatment income histories and unobserved time-invariant heterogeneity. Presumably, individuals who had suffered a large reduction in income were more likely to join the program than individuals who maintained a steady income. Ignoring pretreatment income thus underestimates income gains during participation.

19. Jalan and Ravallion (2003, p. 21).

20. The figures are computed based on the EDS, the ECV, and the October 2000, May 2001, October 2001, and May 2002 EPH surveys.

## Data

The Permanent Household Survey (EPH) has been conducted twice a year (in May and October) since 1974, and it has identified whether the individual is participating in a workfare program since October 2000. An important feature for this paper is the survey's rolling panel structure, which allows researchers to follow individuals over time and thus to control for pretreatment income and time-invariant heterogeneity.

I analyze four surveys, from October 2000 to May 2002. By October 2002, workfare became almost universal among the poor population, suggesting that attempts to obtain a reliable comparison group after that date would be ineffective. The EPH, regrettably, does not specify the workfare program in which participants are enrolled.

I match three consecutive surveys and construct two panels of participants and nonparticipants. The first panel (panel A) includes all individuals who were participating in the program in May 2002 and were also surveyed in October 2001 and May 2001 (200 people), together with individuals who were surveyed during the same period and did not participate in the program (18,795 people). The second panel (panel B) includes all participants enrolled in the program in October 2001 who were also surveyed in May 2001 and October 2000 (97 people), and nonparticipants interviewed during the same period (17,929 people).<sup>21</sup> Both samples are restricted to individuals between fifteen and sixty-four years old who report earnings between 150 and 200 pesos from the program.<sup>22</sup> Hereafter, I refer to the survey in which participants were receiving treatment as the baseline and denominate it  $t$ . The surveys conducted six months and one year before the baseline are termed  $t - 1$  and  $t - 2$ , respectively.

Table 3 presents some characteristics of participants and nonparticipants in these two panels at the baseline. On average, workfare participants are poorer and less educated than nonparticipants, have larger families, have more kids out of school, are more likely to have migrated from other provinces (but less

21. While the total number of program participants in the sample is small (297), it does not introduce any bias to the estimates as long as it is representative of the population of program participants. The sample size affects the power of the test. Influential papers in the evaluation literature use a sample that includes between 200 and 300 participants (LaLonde, 1986; Dehejia and Wahba, 1999).

22. Matching four consecutive surveys is also possible, but it leads to very few observations. Only 86 individuals who were workfare participants in May 2002 have been surveyed since October 2000.

TABLE 3. Descriptive Statistics of Two Panels of Participants and Nonparticipants<sup>a</sup>

Variable	Panel A (May 2002)		Panel B (October 2001)	
	Participants	Nonparticipants	Participants	Nonparticipants
Age	36.76 (10.56)	36.57 (14.50)	39.08 (13.48)	36.21 (14.41)
Gender (male = 1)	0.49 (0.50)	0.47 (0.50)	0.47 (0.50)	0.47 (0.50)
Married	0.69 (0.46)	0.54 (0.50)	0.60 (0.49)	0.55 (0.50)
Head of household	0.37 (0.48)	0.34 (0.47)	0.48 (0.50)	0.34 (0.47)
Foreign-born	0.10 (0.30)	0.05 (0.21)	0.02 (0.13)	0.05 (0.22)
Born in a different province	0.29 (0.45)	0.18 (0.39)	0.32 (0.48)	0.18 (0.38)
Newly arrived	0.02 (0.13)	0.04 (0.19)	0.02 (0.14)	0.03 (0.18)
Attending educational institution	0.03 (0.18)	0.21 (0.41)	0.03 (0.16)	0.21 (0.41)
High school dropout	0.83 (0.38)	0.45 (0.50)	0.83 (0.38)	0.46 (0.50)
No. members in the household	6.37 (2.90)	4.59 (2.17)	4.94 (2.77)	4.56 (2.11)
Rooms per member	0.50 (0.31)	0.84 (0.52)	0.71 (0.69)	0.85 (0.51)
Share of dependents (6 to 16 years old) not attending school	0.040 (0.139)	0.015 (0.098)	0.033 (0.163)	0.014 (0.095)
Residence in shantytown	0.024 (0.152)	0.011 (0.105)	0.062 (0.242)	0.022 (0.148)
Lack of access to basic services	0.10 (0.31)	0.03 (0.17)	0.03 (0.18)	0.02 (0.15)
Property ownership	0.59 (0.49)	0.78 (0.42)	0.69 (0.46)	0.78 (0.41)
No. observations	200	18,795	97	17,929

a. Population weighted averages. Standard deviations are in parentheses.

likely to have only recently arrived at the current location), and are more likely to reside in a shantytown.

Table 4 compares the income and employment histories of both groups in  $t - 1$  and  $t - 2$ . For the group of participants in panel A, income decreased by 29 percent and unemployment increased 2.5 percentage points between May 2001 and October 2001. Nonparticipants also suffered a negative shock in the same period, but it was considerably smaller: their income decreased by 9 percent and unemployment increased 1.4 percentage points. The results are

**TABLE 4. Pretreatment Income and Employment Histories of Participants and Nonparticipants**

Variable	Panel A		Panel B	
	Participants	Nonparticipants	Participants	Nonparticipants
Monthly income in $t - 1$ (pesos)	93.2	308.9	158.9	345.1
Monthly income in $t - 2$ (pesos)	132.5	339.0	184.1	363.7
Percentage unemployed in $t - 1$	14.5	10.9	10.2	10.6
Percentage unemployed in $t - 2$	12.0	9.5	7.0	8.5

similar when participants and nonparticipants are compared in panel B. Between October 2000 and May 2001, the income of participants decreased by 14 percent and unemployment increased by 3.2 percentage points, compared with a 5 percent reduction in income and a 2.1 percentage point increase in unemployment among nonparticipants. The evidence that participants suffered a larger negative shock than nonparticipants underscores the importance of controlling for pretreatment histories in constructing the comparison group.

I estimate a probit model to predict participation in the program using all the covariates presented in tables 3 and 4 and obtain the propensity score.<sup>23</sup> I run separate regressions for both panels (see table 5). The mean propensity score for participants is 0.293 in panel A and 0.354 in panel B (with a standard deviation of 0.323 and 0.312, respectively), and the mean propensity score for nonparticipants is 0.008 (with a standard deviation of 0.025) in panel A and 0.004 (0.021) in panel B. There is some overlap in support, with 13 percent of nonparticipants in panel A having a score below the lowest value for participants (21 percent in panel B) and 10 percent of participants having a score above the highest value for nonparticipants in panel A (11 percent in panel B).

I then construct the comparison group (one for each panel) by selecting from the sample of nonparticipants the nearest neighbor for each participant based on the estimated propensity scores.<sup>24</sup> To limit the bias from location differences, I match within geographic regions only.<sup>25</sup> Participants and their

23. Except for the share of dependents attending school, since this factor is likely to be affected by participation in workfare. I also include geographic region dummies, educational attainment indicators, and pretreatment information in  $t - 1$  and  $t - 2$  about employment status and job characteristics (namely, hours worked, willingness to work more hours, whether the individual is self-employed, has access to social security benefits, or is employed in the public sector, and whether employment is temporary).

24. Alternative matching estimators are described in Heckman, Ichimura, and Todd (1998).

25. The regions are Greater Buenos Aires, Northwest, Northeast, Cuyo, Pampeana, and Patagonia.

TABLE 5. Probit Regression of Participation in a Workfare Program<sup>a</sup>

Variable	Panel A		Panel B	
	Coefficient	Std. error	Coefficient	Std. error
Age	-0.014	0.004	—	
Gender (male=1)	-0.180	0.088	—	
Head of household	0.394	0.101	—	
Attending school	-0.491	0.147	—	
Completed high school	—		-0.468	0.198
Incomplete college	—		-0.682	0.292
Completed college	-0.982	0.279	-1.055	0.335
Rooms per capita	-0.324	0.133	—	
Monthly income in $t-1$	-0.001	0.001	-0.002	0.001
Formal employment in $t-1$	-0.556	0.190	-0.725	0.228
Public sector job in $t-1$	1.099	0.164	1.357	0.190
Monthly income in $t-2$	-0.001	0.001	-0.001	0.001
Formal employment in $t-2$	-0.954	0.199	-0.527	0.233
Public sector job in $t-2$	0.904	0.162	0.922	0.205
Permanent job in $t-2$	—		-0.714	0.198
Region Patagonia	0.312	0.143	0.488	0.220
Log likelihood	-668		-298	

a. The table only reports coefficients that are statistically significant at the 5 percent level.

nearest neighbors have very similar observable characteristics, as expected by construction: the propensity score among the selected nonparticipants in panel A is 0.288 and in panel B 0.345 (with a standard deviation of 0.318 and 0.299, respectively). Given that the program was not well targeted to a particular group and few eligible individuals received treatment, it is likely that the comparison group serves as a reliable counterfactual. Differences in unobservables, of course, cannot be ruled out.

Finally, I pool the two panels and compute the average effect of treatment on current income using the formula in equation 1.<sup>26</sup> For illustrative purposes, I first compute the difference in current income ( $t$ ) between participants and their nearest neighbors assuming the impact was nonnegative for each individual. Second, I allow the effects to be negative. Third, I compute two difference-in-differences estimators (that is, the difference in the change in income between participants and their nearest neighbors between  $t$  and  $t-1$  and between  $t$  and  $t-2$ ) and eliminate time-invariant heterogeneity. I

26. Estimates do not differ significantly between panels. Results are available on request. Standard errors are estimated by bootstrapping, but they should be interpreted with caution. Recent work by Abadie and Imbens (2006) shows that bootstrapping generally provides an inconsistent estimate of the true error.

**TABLE 6. Average Treatment Effect on Current Income<sup>a</sup>**

Pesos per month

<i>Comparison group</i>	<i>Earnings in period t: Treatment group minus comparison group</i>		<i>Growth in earnings: Treatment group minus comparison group</i>	
	<i>Nonnegative difference</i>	<i>Potentially negative difference</i>	<i>Growth between t and t - 1</i>	<i>Growth between t and t - 2</i>
Nearest neighbor	93.78 (4.62)	35.74 (10.44)	57.28 (11.72)	57.15 (12.03)
Five nearest neighbors	81.52 (4.34)	41.28 (7.81)	55.71 (10.24)	56.12 (10.58)

a. Standard errors are in parentheses.

then follow the same steps using the average income of the five nearest neighbors. Table 6 presents the results. The point estimates range between 36 and 94 pesos per month, and they are slightly below 60 pesos per month in the preferred specification. Relaxing the assumption that individual effects are nonnegative produces smaller coefficients, while controlling for time-invariant heterogeneity produces larger effects.<sup>27</sup>

To explore program effects by level of pretreatment income, I first categorize participants in five groups according to their pretreatment per capita household income and then compute the difference-in-differences estimator between  $t$  and  $t - 1$  for each quintile. Column 1 in table 7 presents the results using the nearest neighbor, and column 2 uses the nearest five neighbors. The mean effect is large and significant for the poorest participants, falls (although not continuously) with pretreatment income, and is basically zero for participants in the richest quintile.

How should these estimates be interpreted considering that Argentina suffered a severe crisis during the period under analysis? Are the expected income effects during treatment larger or smaller in periods of economic growth? When labor demand is low, wages and employment opportunities are dim, suggesting that participants would have performed poorly in the absence of workfare. In contrast, when labor demand is high, participants are likely to perform better in the absence of the program. In other words, income effects during treatment are likely to be higher in periods of crisis than in periods of economic prosperity.

27. The finding that the two difference-in-differences estimates are very similar suggests that the selected comparison group serves as a reliable counterfactual, since their members experienced pretreatment income dynamics similar to program participants.

**TABLE 7. Average Treatment Effect by Quintile of Pretreatment Family Income per Capita<sup>a</sup>**  
Pesos per month

<i>Quintile</i>	<i>Nearest neighbor</i>	<i>Five nearest neighbors</i>
First (poorest)	90.20 (18.52)	100.48 (14.83)
Second	70.10 (24.93)	72.48 (21.25)
Third	43.32 (28.71)	35.82 (25.82)
Fourth	78.63 (22.90)	71.27 (22.21)
Fifth (richest)	3.36 (31.49)	-2.57 (25.88)

a. Difference-in-differences estimator. Standard errors are reported in parentheses.

## Conclusion

In this paper, I reevaluate targeting and income effects of Argentina's workfare program using samples of participants collected by the National Institute of Statistics and Censuses (INDEC), including two cross-sectional samples (EDS and ECV) and a longitudinal sample (EPH). I find that while participants are poorer, on average, than nonparticipants, targeting is less pro-poor than in the MOL sample. Furthermore, nonpoor workfare beneficiaries report larger rates of participation in political parties, suggesting that political favoritism distorts the allocation of benefits. When I control for pretreatment employment histories and time-invariant heterogeneity, the estimated average income effect during participation is 60 pesos per month, or one-third of the benefit. This estimate refers to a period of severe economic crisis. Income effects during participation in the program are likely to be smaller in times of economic prosperity.

I do not claim that the estimates reported in this paper are the true effects. Ineligible participants might have misreported their status to INDEC, and estimates of treatment effects based on nonexperimental data are subject to biases stemming from unobservables. I have, however, shown substantial evidence suggesting that public works programs in Argentina are not as effective in reducing short-run poverty as found in previous studies.

This evidence does not necessarily suggest that the program should be eliminated. While targeting is far from perfect and income effects are not that large, the program does help reduce poverty in the short run. Other social pro-



grams arguably perform worse.<sup>28</sup> Furthermore, how workfare affects poverty in the short run is by no means the only measure for appraising the adequacy of this policy. Other micro- and macroeconomic outcomes should be considered, such as the program's effect on income inequality, its role in the 2001 crisis and later recovery, the indirect benefits flowing from the assets created, and long-term effects on participants and their dependents.<sup>29</sup>

Beyond assessing the effects of workfare in Argentina, this paper contributes to the design of workfare programs by emphasizing the importance of enforcement. Setting the program benefit low and legislating work requirements does not necessarily imply self-targeting if the normative is ignored. This is particularly relevant for countries with poor political institutions where political favoritism is likely to distort the allocation of public funds.

28. Betcherman, Olivas, and Dar (2004).

29. Ravallion and others (2005) examine what happens to participants' incomes when they leave the program in Chaco, Mendoza, and Tucumán. They find large income losses equal to about three-quarters of the program benefit in the first six months, falling to half in twelve months. Ronconi and Franceschelli (2007) analyze the relation between workfare policy and the emergence of a social movement (namely, *Piqueteros*); they claim that workfare fueled the development of insurgency in the 1990s, while the universalization of the program in 2002 had the opposite effect.