

# Assisting the Transition from Workfare to Work: Argentina's Proempleo Experiment

Emanuela Galasso, Martin Ravallion and Agustin Salvia<sup>1</sup>

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**Abstract:** Randomly sampled workfare participants in a welfare-dependent region of Argentina were each given a voucher that entitled an employer to receive a sizable wage subsidy. Another sample also received voluntary skill training, while a third randomized-out sample was retained as the control group. Double-difference and instrumental-variables methods are used to deal with potential experimental biases, including selective compliance. Compared to the control group, voucher recipients had a significantly higher probability of employment after 18 months, though their current incomes were no higher. The impact was largely confined to women and younger workers. The experiment proved to be cost effective, given low voucher take-up by employers.

**Keywords:** workfare, wage subsidies, training programs, evaluation, randomization

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<sup>1</sup> Galasso and Ravallion are with the Development Research Group of the World Bank and Salvia is with the Ministry of Labor, Government of Argentina. The work reported in this paper is part of the ex-post evaluation of the World Bank's Social Protection III Project in Argentina. The authors' thanks go to staff of the Trabajar project office in the Ministry of Labor, Government of Argentina, who have helped in countless ways, and to the Bank's Manager for the project, Polly Jones, for her continuing support of the evaluation effort, and many useful discussions. These are the views of the authors, and need not reflect those of the Government of Argentina or the World Bank. Correspondence: Martin Ravallion, World Bank (MSN MC3-306), 1818 H Street NW, Washington DC, 20433 USA; [mravallion@worldbank.org](mailto:mravallion@worldbank.org).

## I. Introduction

Governments have tried often to get able-bodied adults off the welfare or unemployment rolls into regular jobs. Two main types of policies are found, namely wage subsidies and training programs. While there is some evidence that both interventions can help in the transition from unemployment to regular work, results have varied greatly according to the setting and the method used to assess impact.<sup>2</sup> It has proved difficult to get robust estimates of impact using non-experimental methods.<sup>3</sup>

This paper studies the “Proempleo Experiment” in Argentina in 1998-2000. The experiment was motivated by concerns about welfare dependency in “company towns” that had seen sharp reductions in employment due to retrenchments by the principal employer. The main form of welfare assistance provided to such towns in Argentina (in common with many developing countries) is temporary work, at a relatively low wage, oriented to social infrastructure or community services. In the study towns, the heavy dependence on such workfare programs emerged in the wake of the privatization of the public oil refinery in 1993 and subsequent sharp contraction in employment; an unusually higher take-up rate for workfare programs was being observed in these towns even five years later. Workfare participants in these towns may well need assistance in getting

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<sup>2</sup> An overview of the arguments for and against wage subsidies can be found in Katz (1996), and impact assessments can be found in Burtless (1985), Woodbury and Spiegelman (1987) and Dubin and Rivers (1993). The theory and evidence on training programs are reviewed by Heckman et al. (1999), and empirical studies include Lalonde (1986), Heckman et al., (1997), Dehejia and Wahba (1999) and Smith and Todd (2001).

<sup>3</sup> A classic study by Lalonde (1986) found large biases in non-experimental methods when compared to a randomized evaluation of a US training program. On the same data set, Dehejia and Wahba (1999) found that propensity-score matching achieved a good approximation — much better than the non-experimental methods studied by Lalonde. However (again using the same data set), Smith and Todd (2000) question this finding, arguing that Dehejia and Wahba’s PSM estimates are sensitive to choices made in sample selection and model specification.

regular employment in the private sector. Wage subsidies and/or training programs seems obvious responses. But will they work, and at what cost?

The Proempleo Experiment was designed to assess the efficacy of providing a wage subsidy and specialized training as means of assisting the transition from workfare to regular work. The wage subsidy and training were provided to a random sample of workfare participants. At the time the sample frame was formed, all participants in the experiment were registered on workfare programs, mainly Argentina's "Trabajar" program. The design features of this program assured that the scheme was well targeted, with about 80% of participants coming from households in the poorest quintile nationally on the basis of income per person (Jalan and Ravallion, 2001). Thus the Proempleo Experiment was implicitly targeted to workers from poor families.

Under the experiment, one random sample received a Proempleo voucher that entitled a private-sector employer to receive a sizable wage subsidy that covered part of the total wages paid to the employee. A second sample was offered limited training as well; this was job-specific training (depending of the participant's skills and interests) of up to about two weeks. A third random sample formed the control group. After a baseline survey, there was a series of follow-up surveys spanning 18 months.

The paper reports our attempt to measure the direct impact of this experiment on the employment and incomes of those who received the voucher and training. To our knowledge this is the first time that a randomized experiment has been done to assess options for promoting the private sector-employment of workfare participants, and is one of only a handful of randomized evaluations of active labor market programs (ALMP's) more generally.

The following section discusses evaluation methods, including the problem of endogenous compliance in randomized policy experiments. We then describe the setting for our study (section III) and provide an overview of the data (section IV). The results are then presented in detail (section V), while the concluding section offers some interpretations.

## **II. Evaluation methods for active labor market programs**

There can be no presumption that either a targeted wage subsidy or a training program will have any impact on the employment prospects of the unemployed or those on workfare programs. The literatures on both types of interventions point to a number of reasons why public expenditures on such interventions could be largely or wholly wasted. Take the case of a wage subsidy. There are possible substitution effects, whereby an employer simply replaces a current worker with a subsidized worker, and deadweight losses arising from the possibility that the worker would have been employed either way.<sup>4</sup> Similar arguments can be made about training programs.

In the light of these uncertainties about impact, there have been a number of attempts to assess the efficacy of active labor market programs in promoting the transition to regular employment and raising incomes of poor or otherwise disadvantaged groups. Most past evaluations have been plagued by concerns over non-random assignment. Selective placement (through individual choice or administrative targeting) means that data

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<sup>4</sup> For recent discussions of these and other arguments as to why wage subsidies may have little net gain in employment see Katz (1996) and Bell et al., (1999).

on non-participants amongst those eligible does not reveal well the likely circumstances of participants in the absence of the program.

Various methods of dealing with this problem can be found in the literature. One possibility is to assess the counterfactual against a control group of non-participants matched on observable characteristics or some scalar aggregate of those characteristics, such as the propensity score (following Rosenbaum and Rubin, 1983). An alternative approach is to use an instrumental-variables estimator, in which the instrumental variable (IV) identifies the exogenous variation in participation. Naturally these non-experimental methods require assumptions to make up for the missing data on outcomes in the absence of the intervention. Matching on the basis of propensity scores requires the conditional independence (sometimes called “strong ignorability”) assumption, namely that outcomes are independent of participation given the observable covariates. Instrumental-variables methods require an alternative conditional independence assumption, namely the exclusion restriction that the IV is uncorrelated with outcomes given participation and the control variables.

In some (rather rare) cases, evaluations of ALMP’s have used randomized assignment. In the case of training programs, two examples are the US Job Training and Partnership Act (see, for example, Heckman et al., 1997), and the US National Supported Work Demonstration (studied by Lalonde, 1986, and Dehejia and Wahba, 1999, amongst others). For wage subsidy programs, randomized evaluations have been done by Burtless (1985), Woodbury and Spiegelman (1987) and Dubin and Rivers (1993) — all for targeted wage subsidy schemes in the US; indeed, Woodbury-Spiegelman and Dubin-Rivers

studied the same experiment by the Illinois Department of Employment Security in the mid-1980s.

Randomization is the theoretical ideal in that it balances the distributions of all (observed or unobserved) covariates between the treatment and control groups. If everyone who was given access to the training automatically took it up and access was assigned randomly then we would have no difficulty estimating the impact on those treated by comparing measured outcomes with those observed for the randomized-out control group. Under weak conditions, an unbiased estimate of mean impact for those treated can then be obtained by taking the mean difference in the outcome measure (employment say) between the treatment and control groups. This is equivalent to the regression coefficient of the outcome measure on a dummy variable for which group one belongs to (treatment/control). This works in providing an unbiased estimate of impact because the latter dummy variable is exogenous given randomization and full compliance.

However, it is often the case in randomized policy interventions that some of those randomly selected for the program do not want to participate. A common method of dealing with this problem is to first calculate what is referred to as the “intention to treat” (ITT) effect in the evaluation literature. This is given by the difference in mean outcomes between those who are assigned the program (whether they take it up or not) and those not assigned. The ITT effect is then deflated by the “compliance rate,” given by the proportion of those assigned the program who take it up. This method appears to be due to Bloom (1984).<sup>5</sup>

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<sup>5</sup> An example in the context of assessing the Illinois experimental wage subsidy program can be found in Dubin and Rivers (1993).

An alternative econometric method views selective take-up as a classic endogeneity problem, for which the now standard solution is to find an instrumental variable that is correlated with actual treatment but uncorrelated with outcomes given treatment. While finding a valid IV is often difficult, the randomized assignment is a natural choice. As with all IV estimators, this requires an exclusion restriction, namely that the fact of being randomly assigned to the program only affects outcomes via actual participation. Following this approach, the impact estimator is the Two-Stage Least Squares (2SLS) regression coefficient of the outcome measure on a treatment dummy variable, using a dummy variable for assignment as the IV. Angrist, Imbens and Rubin (1996) provide a precise statement of the conditions under which this 2SLS estimator gives a consistent estimate of the impact of treatment on the treated.<sup>6</sup>

We will use the 2SLS method to correct for endogenous compliance. However, it is not difficult to show that these alternative methods found in the literature give identical results under seemingly weak conditions; the Appendix provides details.

### **III. The Proempleo Experiment**

Workfare programs impose work requirements on welfare recipients, typically at benefit levels that are lower than prevailing market wages for relatively unskilled labor. Two incentive arguments are commonly made in favor of such programs. The first is that by setting a sufficiently low benefit level, a workfare program will be self-targeted to those most in need; few of the non-poor in particular will want to participate. The second

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<sup>6</sup> For an example, see Katz et al. (2001), in the context of a randomized mobility experiment with selective take up.

argument is that a low benefit level assures that participants do not become dependent on the program, in that they turn to regular work when available.<sup>7</sup>

Workfare wages are typically fixed across participants and across geographic areas, and respond very little to differences in recipient circumstances. This is a defensible design feature, given both fairness considerations and constraints on the information available to policy makers.<sup>8</sup> However, with a fixed benefit level and heterogeneity in local labor market conditions, there can be a non-negligible sub-set of workfare participants who become dependent on the scheme even though this is not generally so.

The Proempleo Experiment was done in two adjacent cities in the province of Neuquen; they form a traditional, homogeneous, social and urban conglomerate. Cutral Co is the larger and more heterogeneous town, with 40,000 inhabitants. Right beside it, Plaza Huincul has a population of about 10,000, and it was the regional operations center of state-owned oil company until 1993.<sup>9</sup> Both towns had been affected by the severe contraction in employment following the downsizing, first, and then privatization of the oil refinery that had been the main employer.

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<sup>7</sup> Besley and Coate (1992) provide a unified theoretical model of workfare incorporating both incentive arguments.

<sup>8</sup> Indeed, the ability to be well targeted with such modest information requirements is one of the well-recognized advantages of workfare programs over alternatives, notably (though not only) in developing countries where there are severe constraints on the information available for targeting. For overviews of these arguments and the literature see Besley and Kanbur (1993) and Lipton and Ravallion (1995, section 6).

<sup>9</sup> The state-owned oil company (Yacimientos Petrolíferos Fiscales) had in Plaza Huincul and Cutral Co massive facilities for scouting, extracting and refining oil. That made this urban aggregate the logistic and demographic center of an ample area of oil-based activity that covered the entire province of Neuquen and the south-east area of the province of Rio Negro. For historical and sociological studies on the formation and development of this towns and on the socio-laboral effects generated by the downsize of the oil company, see Favaro (1999) and Salvia (2001).

The target population for the experiment is the set of beneficiaries of temporary employment programs, managed by the Ministry of Labor. The main program is the aforementioned Trabajar program. (There was a similar but much smaller provincial program; the beneficiaries of the latter were not included in the study.) The baseline survey aimed to cover everyone on the official list of participants in temporary programs drawn in October 1998, though some had to be dropped because they could not be located. In all, 953 households with workfare participants answered the baseline questionnaire in full.

Three roughly equal random samples were then drawn by a lottery, one getting just the voucher, one getting the voucher and training option, and one reserved as the control group. All were given the same questionnaire as for the Permanent Household Survey (a labor force survey collected twice a year by the statistical office of the government of Argentina) with a number of questions added specific to temporary employment programs. All interviews were done at the beneficiaries' homes. The experiment was not announced publicly by the Ministry of Labor. Nor were any of the beneficiaries (in either treatment group or the control group) were told that they were part of an experiment, and they only believed that they were selected for a special survey. There is no official channel by which they could have found out that others had been chosen at the same time to receive benefits that they did not have. The local Ministry of Labor staff were not informed of how the selection of beneficiaries had been done. So there does not appear to be any way in which program participants or potential employers knew about the fact that the assignment was random. Efforts were taken to avoid members of the two treatment groups meeting each other (by scheduling their visits to the Ministry's local offices on different days). About 40 of the control group members did somehow hear about the voucher and the training

program (presumably from co-workers or neighbors) and asked if they could join it. This was refused.

The Proempleo voucher entitled a hiring employer to a wage subsidy of \$150 per month for workers aged above 45 years and \$100 for younger workers. This subsidy was paid directly to the beneficiary as a part of his/her salary, and the employer had to discount the amount of the subsidy from the gross wages paid to the worker. The minimum wage rate in Argentina at this time was \$200 per month.

The subsidy was received for 18 months, conditional on the employer registering the worker formally, and so incurring the government's social security charges for that worker. The latter represent 30% of the gross wage.<sup>10</sup> The firm only received the subsidy if the social security charges had been paid. The level of the subsidy was set such that hiring firms would be unlikely to perceive an incentive to simply fire a current registered worker to hire and register a Proempleo worker, taking account of the severance pay requirements for firing regular workers. Those assigned the Proempleo voucher also received an instruction lasting 2-3 hours which explained the program and how to use the voucher. The voucher had the participant's name on it, and was non-transferable.

The training had two components. The first was a three-day "labor market orientation" workshop. This was mandatory to all of the beneficiaries, and it taught them about labor demand in the area, how to look for work, and how to develop themselves to become their own employers. Once this workshop was completed, training coupons were

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<sup>10</sup> For example, an employer hiring someone over 45 with a voucher at the minimum wage of \$200 and registering this fact with the Ministry of Labor would incur a social security charge of \$60 but receive a subsidy of \$150, implying a net wage of \$110; for a worker under 45, the net wage would be \$160.

then issued (for those most apt and eager) for the second component, which provided training in a specific skill and required 2-300 hours of attendance. In this second part, the beneficiaries were given working materials and had to fulfill labour practices. They also received economic support benefits, paid at a 10% lower rate than for the Trabajar program.<sup>11</sup> According to their personal interest and the available quota participants proceeded to select from a list of 12 subjects, chosen in the light of local labor demand and the profiles of participants. Two of the courses were on the management of small-scale enterprises, two on industrial welding, two on home-building, one on professional cooking, one on raising pigs, one on greenhouse cultivation and one on skills needed for an electrician.

The data collection began in December 1998 with a baseline survey. The invitation to join the treatment groups for those selected was made in January and February of 1999, either by a house visit or at the place of work. The sample was re-interviewed for three waves at intervals of 5-6 months (June 1999, December 1999, May 2000). We focus here on the comparison of the results for the last wave, May 2000 with those of the baseline survey (December 1998). We did a similar analysis of earlier waves after the baseline survey but found no significant impacts on employment or incomes. However, as we will see later, signs of significant impact on employment did emerge by the final survey round.

The period between the baseline and the fourth wave saw a sizable retrenchment of national ALMP's, including the Trabajar program, to help keep within macroeconomic targets. Aggregate spending on the Trabajar program in the first five months of 2000 was

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<sup>11</sup> The courses were part of a larger training program of the Ministry of Labor, called "Project Joven", and financed by the Inter-American Development Bank.

only 29% of its level in the last five months of 1999. Existing projects were completed, but the number of new projects approved shrank sharply in the latter part of 1999, to bring down the center's outlays. There was also a reduction in the benefit level under Trabajar, from \$200 to \$160 per month. This was initially partly compensated by an expansion of the local employment programs operated by the Provincial and Municipal governments. However, in December 1999 both Trabajar and the provincial temporary employment programs had been heavily retrenched. By the time of the third follow up in Apr/May 2000 (4<sup>th</sup> wave), the recession continued and there had been a significant cut in the outlays per unit time on the Trabajar program. These changes over the study period reinforced the case for having a control group that can reliably reveal the employment and earnings of workfare participants in the absence of Proempleo.

#### **IV. Baseline characteristics and attrition**

Table 1 compares the randomized assignment in the baseline with that actually observed.<sup>12</sup> There is perfect take-up for the "voucher only" case. However, 30% of those offered "voucher and training" did not take up the training component and so were re-assigned to the voucher-only component. (Our results for that group were robust to excluding these re-assigned workers.)

The fact that so many people did not take-up the training offer suggests that there may be a potentially serious compliance problem, as discussed above in general terms. So we cannot rule out the possibility of some latent correlate of the outcome measure that

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<sup>12</sup> We excluded five workers who were assigned to controls but mistakenly ended up getting the treatment. We also excluded members of Fogoneros a political activist group who had access to the program for that reason, and those who dropped out between the baseline and wave four.

influenced the choice to take up the program amongst those who are assigned access. A seemingly plausible argument in the present setting is that those with lower prospects of employment amongst workfare participants are the ones with relatively lower skills. In turn, this leads them to expect higher returns from taking up the training — to help bridge their skill deficiency, relative to those they compete with in the labor market. In this situation, ignoring the endogeneity of take-up would lead one to underestimate the true impact of the training on employment. We return to address this problem in the next section, using the method outlined earlier.

The top panel of Table 2 provides summary statistics on the employment status of the various random treatment groups and the control group in the baseline survey. In each case, the vast bulk (85-90%) of sampled workers were in temporary employment programs. Although all were in such programs at the time the sample frame was constructed, there was an unavoidable two-month delay in doing the baseline survey. Some Trabajar projects terminated in that two-month period. There is a slight sign of higher participation in the temporary employment programs in the control group. This might reflect some contamination of treatment effects in the baseline survey. We will check robustness of our results to using a double difference estimator.

Table 2 also gives a comparison of the various sub-samples in terms of other worker characteristics (age, sex, household size etc). There are no significant differences, suggesting that the randomization has adequately balanced the means of observable characteristics. Similarly, observable worker and household characteristics were (individually and jointly) insignificant in regressions for whether an observation was in a treatment group or the control group.

As one would expect, there is sample attrition over the study period. 77.5% of those interviewed in the baseline survey stayed through to the fourth round. We tested for possible covariates of attrition, following Fitzgerald, Gottschalk and Moffitt (1998) in testing whether the selection out of the sample is based on observable characteristics in the baseline, including the outcome indicator ( $Y_0$ ). The attrition indicator (whether an observation stays in the sample) is then regressed on  $Y_0$  and other characteristics of the worker and household in the baseline. For control variables we used gender, age, and schooling. The test for attrition bias is equivalent to testing whether  $Y_0$  is significant. In our case, the fact that the beneficiary group is mainly workfare participants attenuates the observed variance in outcome indicators in the baseline survey. However, we have seen that there is still some variation in incomes and private sector employment (Table 2); indeed, the CV of incomes is 35-45%. If there is a serious problem of endogenous attrition then this test should be able to pick it up.

We found no sign of attrition bias. Neither individual income nor employment in the private sector were significant predictors of attrition (with t-ratios around 0.5 or lower; details are available from the authors). The only significant variables were age (attrition is more likely among the young) and education (those with better than primary schooling are more likely to stay in the survey).

## **V. Participant impacts and employer take-up**

The impact estimates for those receiving any treatment (voucher, or voucher plus training) can be found in the top panel of Table 3, while the lower panel gives the results for those who only received the voucher. We find that there is a significant effect of either

treatment (voucher or voucher plus training) on those becoming employed in the private sector of the order of six percentage points. The gain in wage employment clearly came from temporary employment programs (Table 3, lower panel). There is no significant effect on other outcomes: being self-employed, being employed in a temporary employment program or wage earnings.

In Table 4 we provide a breakdown of the impact estimates by demographic groups. We split the sample by gender and whether or not the worker was under 30, the median age. Remarkably, the significant impacts are confined to the wage employment of women and those under 30 years of age.

Table 5 gives the estimates for those who received both the voucher and training. Here we give both the ordinary estimates (analogous to Table 3) as well as the 2SLS estimates allowing for endogenous take-up of the training component. The 2SLS estimate gives higher impact — a gain to the proportion in wage employment of 7.5 percentage points. This is consistent with the expectation that people with lower skills (and hence lower prospects of employment) perceived higher gains from taking up the training. However, the extra impact of the training (an increment to the employment rate of 7.5% points versus 6.1% for the voucher only) is statistically significant at the 5% level (z-score=0.70).<sup>13</sup>

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<sup>13</sup> The estimated standard error ( $s$ ) of the difference between the two incremental employment rates (treatment minus control) is 0.020. This was calculated using the formula:

$s^2 = \bar{p}(1 - \bar{p})(n_1^{-1} + n_2^{-1})$  where  $\bar{p} = (n_1 p_1 + n_2 p_2) / (n_1 + n_2)$  and  $n_1, p_1$  are the sample sizes and estimated incremental employment probabilities for the voucher-only sample, while  $n_2, p_2$  are the corresponding numbers for the voucher + training sample (see, for example, Hamburg, 1977). This calculation treats the control group employment probability as non-stochastic; factoring in the sampling variance in the latter estimate will make the difference in the incremental employment probabilities between the two treatment samples even less significant.

Analogously to Table 5, we give a breakdown by demographic group in Table 6. Again significant impacts on wage employment are confined to women and those under 30, though significant impacts of the voucher and training on the self-employment of men and workers over 30 do emerge.

Recall that some of the control sample were more likely to be employed in temporary employment programs in the baseline survey (Table 2). Thus our results so far might over-state the employment gains from Proempleo. To address this concern, we use a double difference (“difference-in-difference”) approach in which we net-out the baseline differences when calculating mean impacts. Table 7 gives the double-difference estimates. The effect on private employment holds in the double difference estimates. And again there are no other significant effects on other outcomes. The close correspondence between the double- difference and single-difference results is consistent with randomized assignment.

Turning to administrative records of the Ministry of Labor, we found that take up of the wage subsidy by firms hiring a worker with a voucher was very low. Indeed, only three of the workers in the treatment group who were hired by private firms were in fact registered by their new employer. (And one firm registered all three workers.) This finding echoes results of O’Neill (1982), Burtless (1985) and Woodbury and Spiegelman (1987) for wage subsidy schemes in the US. (For example, Woodbury and Spiegelman report that only 12% of the employers who were eligible for the wage subsidy took it up.)

In the present context, there is an explanation for low employer take up of the Proempleo program. Registering a worker so as to receive the subsidy was far from costless, since it also meant incurring the government’s social charges and assuming the

costs of the administrative management of the program. While the subsidy was greater than the social charges for as long as the subsidy lasted (18 months), the employer would then have faced severance payments to fire the worker. Many potential employers were also outside the formal sector, and did not register any workers. (This applies to about half of Argentina's workforce.) For such firms, registering one worker to receive the subsidy may well have been seen as risky, in that other workers would demand to be registered, with possible legal action against the firm by workers and the government.

## **VI. Interpretations**

Signs of significant impacts from this experiment took 18 months to emerge. By then we found that the proportion of the sample of workfare participants getting a private sector job was 14% for randomly selected voucher recipients versus 9% for the control group. This difference is statistically significant. On disaggregating the aggregate impact we find that the gain in wage employment was largely confined to women and younger workers. There was a slightly higher impact on employment for those who also took up the offer of training, in addition to the voucher. An impact of training only emerges once one corrects for the endogeneity of take-up amongst those randomly assigned to the treatment. However, we cannot reject the null hypothesis that the training had no impact.

Some features of our results require considerable care in interpretation, and in drawing conclusions about the effects of scaling up the program. Although employment improved amongst voucher recipients, there are no signs of an impact on their incomes, at least by 18 months. Here it appears that voucher recipients took up private sector jobs in the expectation of a higher and/or more stable stream of incomes in the future. Given that

opportunities for workfare jobs were contracting, this would not be an unreasonable expectation. The fact that current earnings were no higher than for the control group, and at a level below the minimum wage (and market wages), is also suggestive that voucher holders were willing and able to undercut the going wage so as to get a job.

Take up of the wage subsidy by hiring firms was low, echoing experience with other wage subsidy schemes (O’Neill, 1982; Burtless, 1985; Woodbury and Spiegelman, 1987). In our case, we suspect that firms were reluctant to register Proempleo workers for fear of incurring a longer-term commitment to the labor charges that are attached to formal sector jobs in Argentina (possibly extending to other unregistered workers in the same firm). The impact of the voucher was clearly not through access to the wage subsidy. Of course, the apparent willingness of voucher recipients to under-cut the minimum wage would have been attractive to employers.

The voucher may well have encouraged workers to make more effort to find work. By this interpretation, the voucher had an “empowerment” effect, in making these workers — notably young and female workers — more confident in approaching employers. Here it should be recalled that the eligible participants came mainly from the poor (through the self-targeting of workfare participation). Our informal interviews with Trabajar participants indicated that many of them had little or no experience in approaching employers for regular private sector jobs, and relied heavily on more casual labor markets and informal networks; it is plausible that many were “discouraged workers”.

In addition, the Trabajar workers in these company towns had developed a reputation locally as “trouble-makers” because of their involvement in various protests about economic conditions in the towns and the perceived inaction by the government.

This reputation may well have made them less employable as a group. Possibly receipt of the voucher may then have made employers feel that these workers were more trustworthy than other Trabajar workers, since the government was so willing to help get them jobs. (Recall that efforts were made to avoid any local knowledge about how the vouchers had in fact been assigned.) In this setting the voucher may well have induced the opposite response from employers to the stigmatizing effect that Burtless (1985) identified as a possible explanation for his negative results on the employment impact of a wage subsidy experiment in the U.S. Of course this is a conjecture; one might just as well expect the opposite effect: that in this setting, holding the voucher would be perceived as a bad sign, on the assumption that the government was targeting the trouble makers to help get them into regular jobs.

Clearly, Proempleo did not succeed in achieving a major transition to private sector jobs amongst workfare recipients in the study area. Arguably, without greater labor demand in these company towns, this would have been difficult. However, even in this setting, there appears to be scope for assisting the transition to regular jobs. Given the low take-up of the subsidy, the gain in private sector employment attributed to the voucher was achieved at very little cost to the government. Since the workfare wage was roughly the same as the subsidy paid to firms, the government saved 5% of its expenditure on workfare wages for those receiving the voucher in return for an outlay on wage subsidies that represented only 10% of that saving.

There are a number of reasons to be cautious about this calculation. It does not take account of any difference in administrative or other costs between the workfare and wage subsidy programs. It is difficult to compare those costs given that the wage subsidy was a

small pilot scheme. In considering the policy implications, one would also have to acknowledge the possibility that scaling up would increase the take up rate amongst firms, and/or reduce the empowerment effect of the voucher. Also, the signal value to employers of the voucher could well be different in a national version of the program. Finally, scaling up might occur at the expense of the non-participants (by displacing their jobs); the general equilibrium costs of a larger program would need to be considered. Nonetheless, this experiment is at least suggestive that wage-subsidy vouchers can cost effectively assist the transition from workfare to regular paid employment.

## Appendix: Correcting for Endogenous Compliance

Let  $D$  be an indicator of observed treatment (=1 if treated, 0 if control),  $Y$  the observed outcome, and  $Z$  an indicator of exposure to the program (=1 if assigned to treatment, 0 if control). Randomization of the assignment allows unbiased estimation of the average effect of  $Z$  on  $D$ , and of  $Z$  on  $Y$ . In a regression setting we can write these equations as:

$$D_i = Z_i \mathbf{p}_1 + \mathbf{h}_{1i} \quad (\mathbf{p}_1 \neq 0) \quad (1.1)$$

$$Y_i = Z_i \mathbf{p}_2 + \mathbf{h}_{2i} \quad (1.2)$$

where the  $\mathbf{h}$ 's represent other determinants of  $D$  and  $Y$ . Assuming that  $E(D|Z=0) = 0$  (treatment is only possible if one is assigned),  $\mathbf{p}_1$  in equation (1.1) is simply the treatment take-up rate. (Note that randomization implies that  $E(\mathbf{h}_1|Z=0) = E(\mathbf{h}_1|Z=1)$  while  $E(D|Z=0) = 0$  implies that  $E(\mathbf{h}_1|Z=0) = 0$ .) The ITT effect (section 2) is given by  $\mathbf{p}_2 = E(Y|Z=1) - E(Y|Z=0)$ . For a pure randomization,  $Z$  is exogenous in (1.1)-(1.2), i.e.,  $\text{cov}(Z_i, \mathbf{h}_{1i}) = \text{cov}(Z_i, \mathbf{h}_{2i}) = 0$ . So both equations are consistently estimated by OLS, giving  $\hat{\mathbf{p}}_1$  and  $\hat{\mathbf{p}}_2$ . The ratio:

$$\frac{\hat{\mathbf{p}}_2}{\hat{\mathbf{p}}_1} = \frac{\sum (Y_i - \bar{Y})(Z_i - \bar{Z})}{\sum (D_i - \bar{D})(Z_i - \bar{Z})} \quad (2)$$

is both the ITT effect deflated by the compliance rate and the 2SLS regression coefficient of  $Y$  on  $D$  with  $Z$  as the IV, i.e., the 2SLS estimator using the randomized assignment as the instrument for treatment gives the mean treatment effect on the treated. These two methods of dealing with endogenous compliance are equivalent.

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**Table 1: Sample breakdown between treatments and control and assigned versus actual participation**

		Actual			Total
		Voucher + Training	Voucher	Control group	
Assign- ment	Voucher + training	210	90	0	300
	Voucher	3	264	0	267
	Control group	0	0	281	313
	Total	213	354	281	<u>848</u>

**Table 2: Descriptive statistics from the baseline survey**

	Control	Either treatment	Voucher only	Voucher + Training	Difference: Treatment minus control (p-value) <sup>††</sup>		
					Either treatment	Voucher only	Voucher + training
<b>Employment status</b>							
Unemployed/inactive	0.075 (0.26)	0.102 (0.30)	0.107 (0.31)	0.094 (0.29)	(1) 0.19 (2) 0.10	0.16 0.08	0.45 0.22
Self-employed	0.011 (0.10)	0.007 (0.04)	0.008 (0.09)	0.005 (0.07)	(1) 0.58 (2) 0.29	0.46 0.23	0.78 0.39
Employed in the private sector	0.011 (0.10)	0.019 (0.14)	0.023 (0.15)	0.014 (0.12)	(1) 0.35 (2) 0.17	0.25 0.13	0.73 0.37
Temporary employment program	0.900 (0.30)	0.871 (0.33)	0.862 (0.34)	0.887 (0.31)	(1) 0.22 (2) 0.11	0.14 0.07	0.64 0.32
<b>Other characteristics</b>							
Age	32.33 (12.12)	32.20 (11.63)	32.24 (11.92)	32.14 (11.15)	(1) 0.88	0.92	0.85
Sex (proportion female)	0.470 (0.50)	0.437 (0.49)	0.437 (0.49)	0.437 (0.49)	(1) 0.37	0.42	0.46
Household size	4.29 (2.05)	4.36 (2.25)	4.33 (2.24)	4.41 (2.26)	(1) 0.68	0.84	0.55
Individual income (\$/month)	188.4 (67.1)	182.6 (76.9)	181.4 (81.6)	184.5 (68.7)	(1) 0.27	0.24	0.52
Household income (\$/month)	406.6 (267.2)	424.8 (332.8)	411.7 (315.5)	446.5 (359.5)	(1) 0.42	0.55	0.15
No. observations	281	567	354	213			

Note: <sup>††</sup>  $H_0=0$  vs. (1)  $H_1 \neq 0$  (2)  $H_1 >/< 0$  (one sided-test); standard deviations in parentheses.

**Table 3: Aggregate impact estimates**

Outcome variable	Means		Difference $\bar{Y}(D=1) - \bar{Y}(D=0)$
	Treated $\bar{Y}(D=1)$	Control $\bar{Y}(D=0)$	
<b>Either treatment</b>			
any employment	0.478	0.452	0.026
wage employment	0.143	0.085	<u>0.057</u> **
self-employment	0.035	0.021	0.014
employment program	0.296	0.345	<u>-0.049</u> *
labor income	120.591	119.271	1.320
<b>Voucher only</b>			
any employment	0.469	0.452	0.017
wage employment	0.147	0.085	<u>0.061</u> **
self-employment	0.037	0.021	0.015
employment program	0.282	0.345	<u>-0.063</u> *
labor income	123.184	119.271	3.913

Note:  $D=1$  if received either voucher or voucher plus training;  $D=0$  if control;  
 \* indicates significance at 10% level, \*\* at 5% level.

**Table 4: Impact estimates across different demographic groups**

Outcome variable	Difference in means: $\bar{Y}(D=1) - \bar{Y}(D=0)$			
	Males	Females	Age $\leq$ 30	Age $>$ 30
<b>Either treatment</b>				
any employment	0.044	0.013	-0.007	0.062
wage employment	0.034	0.076**	<u>0.092</u> **	0.020
self-employment	0.034	-0.001	0.0003	0.029
employment program	-0.028	-0.065	<u>-0.103</u> **	-0.010
labor income	2.009	2.345	14.639	-14.00
<b>Voucher only</b>				
any employment	0.028	0.009	-0.004	0.043
wage employment	0.042	0.078**	<u>0.088</u> **	0.029
self-employment	0.040	-0.003	0.004	0.029
employment program	-0.060	-0.065	<u>-0.102</u> **	-0.014
labor income	3.656	11.18	17.829	-12.464

**Table 5: Voucher and training allowing for endogenous take-up of training**

	Treated	Control	Difference	
	$\bar{Y}(D=1)$	$\bar{Y}(D=0)$	$\bar{Y}(D=1) - \bar{Y}(D=0)$	
any employment	0.493	0.452	0.035	
wage employment	0.136	0.085	<u>0.051</u> *	
self-employment	0.033	0.021	0.012	
employment program	0.319	0.345	-0.026	
labor income	119.271	116.282	2.989	

  

	Assigned	Not assigned	Difference		2SLS
	$\bar{Y}(Z=1)$	$\bar{Y}(Z=0)$	$\bar{Y}(Z=1) - \bar{Y}(Z=0)$		Y on D
					(Z as IV)
any employment	0.490	0.451	0.039		0.057
wage employment	0.140	0.088	<u>0.052</u> **		<u>0.075</u> **
self-employment	0.043	0.021	<u>0.022</u> *		0.032
employment program	0.303	0.345	-0.038		-0.055
labor income	119.927	119.067	0.860		1.247

Note:  $D=1$  if received voucher + training,  $D=0$  if control;  $Z=1$  if assigned to voucher + training,  $Z=0$  if control; \*indicates significance at 10% level, \*\* at 5% level.

**Table 6: Voucher and training allowing for endogenous take-up of training: impact across demographic groups**

Outcome of interest:	Difference in means:			
	$\bar{Y}(Z=1) - \bar{Y}(Z=0)$			
	Males	Females	Age $\leq$ 30	Age $>$ 30
any employment	0.064	0.021	-0.009	0.097
wage employment	0.0007	0.096**	<u>0.082</u> **	0.017
self-employment	0.052**	0.001	-0.001	0.050**
employment program	0.012	-0.080	-0.090*	0.023
labor income	4.904	-1.244	12.088	-12.434

  

	2SLS Y on D			
	(Z as IV)			
	Males	Females	Age $\leq$ 30	Age $>$ 30
any employment	0.098	0.029	0.016	0.121
wage employment	-0.001	0.133**	<u>0.137</u> **	0.021
self-employment	0.080*	-0.001	<u>-0.002</u>	0.062**
employment program	0.019	-0.111	-0.151*	0.028
labor income	7.554	-1.722	20.22	-15.59

**Table 7: Double difference estimates of impact**

Outcome of interest:	Double difference:			
	$\Delta\bar{Y}(D=1) - \Delta\bar{Y}(D=0)$			
	Treated	Control	Difference	
<b>Either treatment</b>				
any employment	-0.420	-0.473	0.054*	
wage employment	0.123	0.075	<u>0.049*</u>	
self-employment	0.028	0.011	0.018	
employment program	-0.575	-0.555	-0.020	
labor income	-58.377	-66.993	8.615	
<b>Voucher only</b>				
any employment	-0.424	-0.473	-0.050	
wage employment	0.124	0.075	<u>0.050*</u>	
self-employment	0.028	0.011	0.018	
employment program	-0.579	-0.555	-0.024	
labor income	-53.540	-66.993	13.453	
<b>Voucher and training</b>				
any employment	-0.413	-0.464	0.050	
wage employment	0.122	0.084	<u>0.039*</u>	
self-employment	0.028	0.022	0.007	
employment program	-0.568	-0.566	0.002	
labor income	-66.418	-67.081	0.663	
	Double difference:			2SLS
	$\Delta\bar{Y}(Z=1) - \Delta\bar{Y}(Z=0)$			$\Delta Y$ on $D$
				( $Z$ as IV)
any employment	-0.417	-0.475	0.059	0.085
wage employment	0.120	0.074	<u>0.046*</u>	<u>0.067*</u>
self-employment	0.037	0.011	<u>0.026*</u>	0.038
employment program	-0.577	-0.556	-0.020	-0.029
labor income	-67.764	-65.963	1.801	2.612

Note: wave 4 with respect to the baseline; \*indicates significance at 10% level, \*\* at 5% level.