Labor informality effects of a poverty-alleviation program *

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Abstract

In the midst of a serious macroeconomic crisis Argentina implemented a large social program – the *Programa Jefes de Hogar* (PJH) – that provides cash transfers to unemployed household heads meeting certain criteria. In practice, giving the difficulties in monitoring informal jobs, the unemployment requirement of the PJH would imply a disincentive for the program participants to search for a formal job. By applying matching techniques we evaluate the empirical relevance of this prediction during the period of strong economic growth that followed the crisis. We find some evidence on the informality bias of the PJH when the value of the transfer was relatively high compared to wages in the formal labor market.

Keywords: informality, employment, Argentina, evaluation, program, Jefes

Códigos JEL: D3, J2, J3

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* We are very grateful to Omar Arias, Daniel Kostzer and Juan Manuel Moreno for helpful insights, comments and suggestions.

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1. Introduction

In the midst of one of the most serious economic crisis of its history Argentina implemented a large poverty-alleviation program, named *Programa Jefes de Hogar* (henceforth, PJH). This program combines features of a workfare and a conditional cash transfer program. The PJH was aimed at providing cash transfers to those unemployed household heads with children at school. The belief that poverty was closely related to unemployment led to include the unemployment requirement as a targeting device.

The conditioning on unemployment implies in theory a full taxation on incomes for the program participants: getting a job means the loss of the program benefits. However, in practice the monitoring of the program requirements is not perfect, and hence a worker may manage to hold a job and the PJH benefits at the same time. The probability of being in this situation is highly dependent on the visibility of the job. In particular, formal jobs that provide social security benefits are included in administrative records, so they are difficult to hold along with PJH benefits. Therefore, the unemployment requirement of the PJH would imply a disincentive for beneficiaries to search for a formal job.

In practice, however, the informality bias of the program might be negligible. The monitoring of the program may be so weak that participants may not fear losing the transfer by accepting a formal job. It could also be the case that the supply of formal jobs for the typical beneficiary of the PJH is so low that the informality effect of the program is insignificant, or that in a segmented market framework, wages and labor benefits in the formal sector are so high that all workers would accept an offer from a formal job even when having to resign the program cash transfer.

In this paper we apply non-experimental matching techniques to assess the impact of the PJH on labor informality during a period of strong economic growth. Between 2002 and 2005 the Argentine economy grew at an annual 8% rate, driving both employment and formal employment up. We investigate whether PJH participants were more reluctant to accept formal jobs in this booming economy than their non-participants counterparts. To that aim we take advantage of the short panel structure of the Argentine household survey.

We find some evidence on the informality bias of the PJH in the period when the value of the transfer was relatively high compared to wages in the formal labor market. The effect of the distortion vanished when earnings in registered jobs went substantially up. The results, however, are not robust to all the specifications, and then call for further research.

The rest of the paper is organized as follows. In section 2 we briefly describe the main characteristics of the Programa Jefes de Hogar, while in section 3 we discuss the theoretical predictions about the effects of the program on employment and labor informality. Section 4 is aimed at presenting the basic data and some preliminary evidence. The main results of the paper are shown and discussed in section 5. We end in section 6 with some concluding remarks.

2. The Programa Jefes de Hogar

Argentina fell into a severe economic and social crisis at the end of 2001. The resignation of President de la Rua was followed by the collapse of the Convertibility Plan (pegging the Argentine peso to the dollar), the subsequent devaluation and the default on the external debt. The economy entered a phase of severe contraction.

Unemployment rate rose sharply from 16% in May 2001 to 21% in May 2002. The proportion of people living below the official poverty line reached 38.4% by October 2001 and jumped to 57.5% one year after.[1] In this scenario the Programa Jefes de Hogar was implemented in the first half of 2002, as an emergency response to the economic and social unrest.[2]

The PJH quickly became the largest poverty-alleviation program in Argentina ever. In the second half of 2002 the program had 2 million beneficiaries, representing 10% of the adult population (aged 18 to 65) in Argentina. The cash transfers of the PJH reached around 15% of all households in the country. In certain regions the share was as high as 40% (*e.g*. Formosa and Chaco). The number of beneficiaries has been decreasing since 2003 due to more strict administrative controls, and the recovery of the labor market. Yet, the program remains large: the number of participants is still around 1.5 million. The size of the program is also large when compared to international standards. For instance, although the famous Progresa/Oportunidades in Mexico has around 1.6 times more beneficiaries, the Mexican population is 2.8 times greater than the Argentina's population.

The PJH is a poverty-alleviation conditional cash transfer program. The main benefit is a cash transfer of 150 pesos per month to each eligible individual. The level of the cash transfer was established below the average market wage for full-time unskilled workers to encourage people to seek for a genuine job. According to the program rules, the PJH should provide aid only to unemployed household heads with dependents under 18 who are enrolled in school (or any age who are disabled). The program is also available to spouses or partners who are pregnant.

The conditionality of the transfer is based on two characteristics that are difficult to define and monitor: being head of the household, and being unemployed. In particular, verifying unemployment is problematic in an economy where more than half of the employment is in the informal sector. All what the government can do at a reasonable cost is checking whether a PJH participant has a formal registered job, and denying the benefits if so. In fact, after the program was launched the government started to cross information of the participants with the national register of formal jobs (SIJP), the unemployment insurance, and other small social programs. Workers holding a formal job recorded in the SIJP system were eventually deleted from the PJH list. For the most part, the unemployment requirement became a requisite for not having a formal registered job.

The requirement of being the head of the household to apply for the program is also ambiguous and difficult to monitor. In practice it became a restriction to avoid that both the actual head of the household and her/his spouse apply to the program at the same time. More than half of the PJH participants are not household heads, according to what they report to the national household survey (EPH).

In addition, the PJH requires the individual to carry out counterpart work in order to receive the cash transfer. Participants have to do 20 hours of basic community work, training activities, school attendance or employment in a private company with a wage subsidy for six months. The municipalities (together with local NGOs) are in charge of organizing the work activities. Provincial offices of the Ministry of Labor, together with municipal and provincial councils are responsible for monitoring the work activities under the PJH, while federal government provides the funds. Some studies and plenty of anecdotal evidence reveal the unclearness of how strictly the work requirements are enforced. The work condition is difficult to prove, given the weak capacity to organize, supervise and enforce this requirement at the local level. The assessment of Ministerio

[1] Own estimates using the official moderate poverty line. See CEDLAS (2006).

[2] The program was formally created by the Decreto 565/2002 in April, 2002.

de Trabajo (2004) is more positive: according to a survey to participants, 80% of the respondents were engaged in a work activity required by the program; most of them in community projects (60%) and in administrative activities in municipalities (20%).

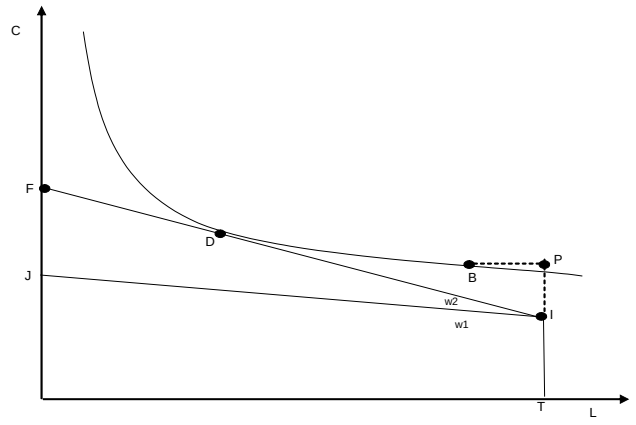
Although the program was promoted as a social right available to everyone complying with the requirements, the limited funds forced the authorities to apply some rationing mechanism from the beginning. The Program was officially launched in April 2002, while the application process closed on June 20, 2002. In theory no additional applications were accepted after that deadline, even when the applicant met the program conditions. Applying to the PJH was very simple, and the program was promoted by the national media. However, the time period was brief for being aware of the program, understanding its characteristics and applying in the local centers. In fact, although people could apply personally, most applications were made by social organizations that acted quickly. In most cases people were assigned the program on a first-come-first-served basis. Some of those who applied later, even before the deadline, were rationed out of the program.

3. Labor incentives

The structure of the PJH implies a potential distortion on the supply for labor. The program is aimed at unemployed individuals. According to the law, a person who works in the labor market is not entitled to the program even when the job is sporadic, or the wage rate is so low that the worker is considered poor. The program requires participants to work a certain number of hours in community works, which turns the PJH into a workfare program.

Figure 3.1 depicts the consumption (C)-leisure (L) choice of an eligible person (household head with children attending school) who initially faces a wage rate w_1 equal to the slope of the line *IJ*, and is endowed with a non-labor income *I*. The kinked budget constraint is *TIJ*. The Programa Jefes de Hogar provides a cash transfer to all unemployed persons. Without any requirement to get the subsidy, the budget constraint would now include point *P*. However, participants are supposed to comply with the work requirement. For simplicity, we model that requirement as a reduction in the time available for leisure or for working in the market. If the time needed to comply with the requirement is the distance *BP*, then the new available point is *B* instead of *P*. The new budget constraint is then the original one before the program (*TIJ*) plus point *B*.

Figure 3.1
Labor incentives of the Programa Jefes de Hogar



Notice that the structure of the program in theory implies full disincentives to work. If a program participant located at *B* decides to work one hour, s(he) would lose the whole transfer and go to a point in the initial budget constraint close to *I*, which would be very likely worse than staying at *B*.[3]

The individual will abandon the program when the wage goes up to w_2 and the budget constraint shifts to *TIF*. Notice that there is a period when the economy recovers and the real wage grows from w_1 to w_2, in which there are jobs in the market, wages are increasing, but program participants prefer to stay in the program.

Anecdotal evidence points to the fact that many participants do not comply with the work requirement. In that case the program adds point *P* (not *B*) to the budget constraint, and the negative effects on the labor supply are even larger. Figure 3.1 shows that while an individual is indifferent between working in the market at a wage rate w_2 (point *D*) and accepting a program with work requirement (point *B*), s(he) would prefer the program without work requirements (point *P*) than a job in the labor market. Notice that the individual depicted in figure 3.1 can earn in the market more than the PJH transfer, but s(he) still prefers to participate in the program since point *P* is on a higher indifference curve than point *D*.

Although in theory the program is intended only for the unemployed (the program is commonly known as Program for *Unemployed* Household Heads), in practice the unemployment condition is difficult to monitor. Administrative weakness precludes the government to check the unemployment condition for people who carry out informal activities or hold informal salaried jobs. In addition, as the program was mainly intended to alleviate poverty, not unemployment, many local officials in charge of administering the program find difficult to deny the transfer to a poor family, even when knowing that the head has some labor activity.

For the reasons mentioned above, in practice the unemployment requirement is replaced by an implicit structure in which the probability of losing the program is much higher for workers in the formal registered sector, than for the unemployed/inactive and those working in the hidden economy. If the individual has a registered job with social security rights it is relatively easy for authorities to detect the fraud and to deny the program. According to information from the national household survey, in 2003 only 19 out of 2,678 PJH participants worked in the formal sector.

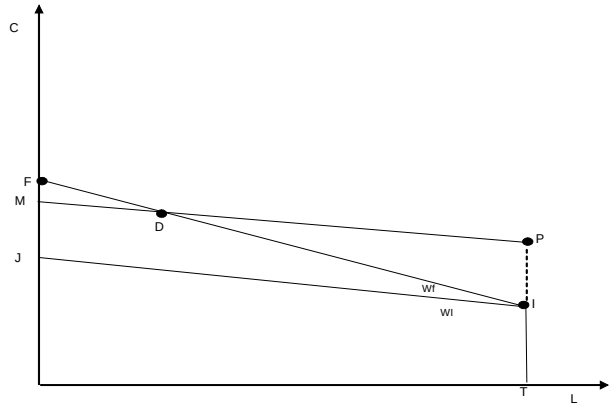
Suppose a situation of economic crisis with high unemployment. The individual depicted in Figure 3.2 faces a zero wage rate and then s(he) is initially at point *I*. Assuming no work requirement, the program allows that individual to go to point *P*. Trivially, there is not distortion in this case, due to the lack of labor market opportunities. Assume now that the economy recovers, and that in a segmented market framework, the individual has the opportunity to work in a formal job at hourly wage w_F or in an informal activity at hourly wage w_I. In principle the formal job is better, but it has a caveat: while the participant can keep the PJH while working in the informal sector, s(he) has to quit the program if chooses the formal job. The budget constraint in the former case is *TPM*, while it becomes *TIF* if s(he) chooses a registered employment. Many individuals will find optimal to choose a point in the segment *DP*, *i.e*. to choose to stay in the program and work in the informal sector, even when having an offer for a formal job at a higher wage rate.[4] Of course, as the formal earnings grow

[3] The choice depends on the indifference map: individuals with strong preferences for leisure may prefer a point on the original budget line close to *I* than point *B*.

[4] The bias toward labor informality may extend to other members of the household. The PJH is mostly held by women. If the female participant's husband is offered a formal registered job, he should take into account the increase in the probability of losing the PJH benefits, and might prefer to reject the offer.

relative to the program transfer and the informal wage, the individual will eventually quit the program.

Figure 3.2
Informal work incentives of the Programa Jefes de Hogar



Although the worker may prefer to refuse an offer for a formal job if the wage rate is not sufficiently high, local authorities may push him (her) to accept it, if by doing that they could have an additional slot available for a new PJH beneficiary. In practice, however, federal authorities did not easily allow new participants into the program. The policy was to progressively reduce the PJH, as the labor market recovered and original participants left the program. In that scenario, and given that the program was nationally funded, local governments had low incentives in promoting people to find formal jobs and quit the program.

There are some reasons why the disincentive to formal jobs may not operate in the real world. A trivial one is low enforcement capacity: authorities might not want or be able to find and punish beneficiaries working in the formal sector. However, as documented above, estimates from the EPH do not support this possibility: most workers are quickly deleted from the program as they find a formal registered job.

A second possibility arises in a segmented labor market where the wage distance (including social benefits) between a formal and an informal job is larger than the cash transfer of the program. In that case, there might be no individuals in the “margin”. Faced to the opportunity of a formal position, a PJH participant has no doubt in accepting it, even when that implies quitting the program. Formal jobs are better than informal jobs, and any worker, either having to resign the benefits of the poverty-alleviation program or not, prefers the formal employment. In that case, trivially, the program does not imply any additional distortion on the workers’ choices.

A third possibility is that the program participants have certain characteristics that make them less demanded in the formal labor market. Most program participants are unskilled and many of them are young and women. Even when the economy starts to grow and the economic perspectives are good, firms may be reluctant to offer a formal contract to newly-hired unskilled young female workers. If that is the case, the program might have a theoretical bias toward labor informality, but in practice it could be quantitatively negligible.

A fourth related case pictures program participants with preferences biased against formal jobs, given that this kind of jobs usually requires certain commitments (hours of work, place of work, schedules) that are too costly for some people. Women in charge of raising children may participate in the PJH, since they may find easy to negotiate a more flexible work requirement, or to avoid it altogether. In contrast, they may refuse a formal job with more strict work conditions. Hence, in practice the informality bias may

be quantitatively very small, since many PJH participants may not look hard for formal jobs.

In summary, as the economy recovers program participants may be more reluctant to accept formal jobs than non-participants with the same characteristics. The PJH implies in theory an informality bias that, however, for many reasons may not be quantitatively relevant in practice. The rest of the paper is aimed at providing evidence on that issue.

4. Data and preliminary evidence

The national household survey in Argentina (Encuesta Permanente de Hogares, EPH) has information on labor variables, and identifies the beneficiaries of the Programa Jefes de Hogar. The EPH is carried out by the Instituto Nacional de Estadística y Censos (INDEC) since the early 1970s in the Greater Buenos Aires area, and since the 1980s in most large cities (more than 100,000 inhabitants). During 2003 a major methodological change was implemented by INDEC, including changes in the questionnaires and in the timing of the survey visits. The new survey (know as EPH Continua, or EPHC) is now conducted over the whole year. The modifications of the questionnaire in the labor section were particularly important, and led to substantial jumps in the series of all employment variables. For instance, Gasparini (2006) reports that the share of salaried workers in informal jobs (defined as absence of social security rights) is 0.388 when using the May 2003 EPH, and 0.437 when using the EPHC for the second half of 2003.[5]

The PJH started in 2002 and questions on that program showed up in the EPH of that year. Given the changes in the survey commented above we decided to work only with the EPHC starting in the second half of 2003. In addition 2003 is also the year when the economic recovery began in Argentina after four years of stagnation and recession. The EPHC covers 28 large urban areas which are home to around 70% of the Argentine urban population. Since the share of urban areas in Argentina is 87%, the sample of the EPHC represents around 60% of the total population of the country.

The EPHC is a rotating panel: individuals are interviewed in two consecutive rounds (quarters), left for two rounds, and surveyed again in the next two quarters. These short panels are used to assess changes in the labor status of individuals. We work with the datasets containing information for each half of the year. Some individuals are interviewed twice in each half. To avoid overweighting of these individuals, we ignore the second observation in our panels.

As discussed above, the PJH is a large program. In 2003 12% of the households surveyed by the EPHC reported being beneficiaries of the program (table 4.1). That share has been descending over time as the labor market recovered. The relevance of the program in the lowest quintiles of the income distribution is high, although the PJH is far from being universal. In the second half of 2005 30% of the households in the poorest quintile received transfers from the PJH. That share falls to 17% in the second quintile and to 6% in the third. The program is almost inexistent in households of the top quintile, or households in which the head has high (tertiary) education.

The Programa Jefes has been reasonably targeted to the poor population. Table 4.2 shows that around 80% of the participants belong to the two bottom income quintiles of the population. The degree of targeting in the poorest 20% of the population has been increasing over time.

[5] The EPHC of the first half of 2003 is not available.

Table 4.3 stresses the fact that the number of program participants has been falling since 2003 as the labor market recovered. The share of PJH participants in the adult population fell from 6.2% in 2003 to 4.9% in 2005. Participants complying with the labor requirement are officially counted as employed. The share of "employed" PJH participants in total employment fell from 8.7% to 5.8% in two years. The second panel restricts the sample to adults in the six poorest deciles of the household income distribution without any tertiary education (complete or incomplete). As expected, the level of participation in the PJH is higher in this group. The program is substantially more extended among women.

This paper is aimed at exploring the potential effect of the program on labor informality. There are at least two different concepts that are referred by the term *labor informality*. The "productive" definition pictures informal workers as those in low-productivity, unskilled, marginal jobs, while the "legalistic" or "social protection" definition stresses the lack of labor protection and social security benefits. The productive definition is concerned with the type of job (*e.g*. salaried vs. self-employed, large vs. small firms), while the social-protection/legalistic definition is concern with the compliance of the labor relationship with some rules, mainly labor protection.

In this paper we deal with the latter definition of informality. By the fact of being registered, holding a formal job (in the legalistic sense) may reduce the probability of keeping the cash-transfer program. Given the information available in the EPHC we define a formal job as one in which the employee reports having the right to a pension (*jubilación*) when retired. Unfortunately, the relevant question is only asked to salaried workers. Given that the tax and social security system in Argentina is poorly-developed for the self-employed, in particular for the typical beneficiary of the PJH, we assume that all self-employed workers are informal.

In summary, we include as formal those salaried workers that report having the right to pensions when retired. Salaried workers with no right to pensions, the self-employed and family workers with no earnings are considered informal workers. We exclude the group of entrepreneurs (*patrones*) from the analysis. We also restrict the sample to people aged 18 to 60, in deciles 1 to 6, and without tertiary education.

In the period 2003-2005, as the economy recovered from a long recession, the unemployment rate substantially fell, fueled by a sizeable increase in formal employment, and an increase in inactivity (Table 4.4). While in 2003 12.9% of people in the sample had a formal job, that share grew to 14.2% in 2004, and 15.5% in 2005. The share of people in informal jobs remained roughly unchanged.

In order to keep the PJH, the worker in principle should not accept a formal job. Table 4.4 shows that the great majority of PJH participants are informal workers.[6]

As discussed above, while the law establishes a set of conditions to apply to the PJH, in practice some of them are not enforced by the authorities. Table 4.5 classifies adults according to four conditions: (i) being head of the household, (ii) having children under 18, (iii) being unemployed, and (iv) having a formal job. The column labeled "qualifies 1" identifies people who qualify to the PJH according to the law (unemployed household heads with children), while the next column marks those that qualify according to a looser criterion (adults with children without a formal job). The last panel in table 4.5 records the share of PJH beneficiaries for each group. It is interesting to notice that only a small share of people who legally qualify to the program are actual beneficiaries, and that participation is higher for those individuals who do not legally qualify for the program (not unemployed not holding a formal job).

[6] Notice that showing that formal employment grew for non-participants but not for participants is not a sign of the informality bias of the program, since participants may be abandoning the program to become formal workers.

As mentioned above, the incentives to look for a formal job substantially varied across the period under analysis. The cash transfer of the program was fixed in nominal terms ($150), while the labor market recovered and wages in the formal sector substantially increased. While the PJH transfer represented 75% of the minimum wage in the formal sector in 2003, that share fell to just 24% in 2005.[7]

In table 4.6 we show earnings in the formal sector, and labor income in part-time informal jobs.[8] The column labeled *Diff 1* reports the difference between earnings in a formal job and the PJH transfer, *i.e*. the gap relevant for the decision of a PJH participant not working in the market, or expecting to keep his current informal activities if hired in a formal job. That difference doubled between 2003 and 2005. If by working in the formal sector the worker had to abandon his activities in the informal sector, the difference reported in the last column would be more relevant. In this case the earnings gap more than doubled. The results suggest that the monetary incentives to look for a formal job greatly increased between 2003 and 2005. Many workers for whom leaving the PJH to get a formal job was not optimal in 2003 probably modified that decision in 2005. The distortion introduced by the unemployment requirement of the PJH might have been losing relevance over time.

5. Exploring the effect of the PJH on labor informality

We apply non-experimental methods of impact evaluation to assess the labor informality bias of the PJH.[9] The population is divided into two groups: those who receive the program (treated) and those who do not (non-treated). Let *D* be a binary variable that captures treatment, *i.e*. D_i=1 if treated and D_i=0 if not treated. Let Y_i^k be the outcome to evaluate. In this paper we are interested in the type of the job (formal or informal). Hence, in most applications *Y* will be a binary variable equal to one if the worker has a formal job. Ideally, we would like to estimate the whole distribution of $G_i = Y_i^T - Y_i^C$, where Y_i^T is the outcome if the individual participates in the program and Y_i^C is the outcome if (s)he does not participate. The problem of impact evaluation is that one of the two terms in G_i is unobservable. We just observe outcome Y_i ($Y_i = Y_i^T$ if D_i=1 and $Y_i = Y_i^C$ if D_i=0). In particular, in our case we observe the formal/informal decision of the PJH participants, but we do not know what that decision would have been if they had not received the program, *i.e*. the counterfactual outcomes for the participants.

Given that estimating the whole distribution of G_i is very difficult, in most cases the analysis is restricted to estimate specific parameters. In particular, the vast majority of the impact evaluation literature is aimed at estimating the average treatment effect on the treated conditional on other covariates *X*

$$\text{ATET}\ (X_i) = E(Y_i^T - Y_i^C \backslash X_i, D_i=1)$$

[7] The minimum wage was fixed at $200 from 1994 to the end of 2004 when it jumped to $450. In 2005 it scaled up to $630.

[8] The table displays wages at percentile 25, but results are robust to this choice.

[9] Ministerio de Trabajo (2004) does a rich characterization of those PJH beneficiaries that found formal jobs, based on a special survey, but the lack of a control group impedes them carrying out an impact evaluation exercise.

A simple estimator of ATET is just the mean difference in outcomes between participants and the non-treated group (MDO). The general condition for zero bias is mean independence, also known as ignorability[10]

$$E(Y^k \backslash D) = E(Y^k) \qquad k=T,C$$

The best way to assure ignorability is to design a social experiment in which individuals are randomly assigned into the treatment and control group, so that both groups become statistically equivalent. The PJH was not designed as a social experiment. Participants were chosen according to certain rules, and in principle those meeting the requirements were entitled to the program. In practice, however, not all potential beneficiaries got the program. As shown in table 4.5 only a small share of those individuals legally in conditions of claiming the program, report being actual beneficiaries. One reason may be due to the fact that some unemployed people were not poor, and although legally entitled to the program they understood the program was not intended for them. But even among the poor the take up rate is not large.

There are many reasons why some people are participants and some not. Some of them are related to observable characteristics. To control for these factors we use matching techniques. But of course participants may differ from the rest in some unobservable dimensions. We control for some of these factors by using longitudinal data.

Our strategy, given the available information, is simple. In a scenario of strong increase in the availability of formal jobs, we compare the proportion of PJH participants who find a formal job between t_1 and t_2 with that proportion for a group of non-participants with similar observable characteristics. To improve the matching we restrict the analysis to individuals who are not holding a formal job at time t_1: almost all PJH participants are in that situation In that sense, although we are using panel data the first stage of the implicit diff-in-diff procedure is trivial, as the outcome variable (the share of formal workers) is zero for both treatment and control groups.

The key assumption in this strategy is that in the absence of the program the PJH participants will be moving to the formal sector at the same rate as a matched group of non-participants. Of course, the assumption may be restrictive, as the factors affecting treatment may also affect the changes in the outcome (labor formality). See below for a discussion of this crucial point.

Formally, we estimate

$$ATET = \frac{1}{n_T} \sum_{i \in T} \left(Y_{i2}^T - \sum_{j \in C_i} W_{ij} Y_{j2}^C \right)$$

where n_T is the size of the panel, T the set of treatment observations, C_i the set of control observations, and W_{ij} the weight of observation j in the group of matches for participant i. Y_{i2}^T is a binary variable equal to 1 if individual i, who participated in the program in the first year of the panel, finds a formal job in the second year. Similarly, Y_{j2}^C is a binary variable equal to 1 if individual j, who did not participate in the program in the first year of the panel (and was not a formal worker), finds a formal job in the second year.

The matching is carried out based on the propensity score, *i.e*. the probability of receiving treatment conditional on pre-treatment characteristics $P(Z_i)$. Rosenbaum and Rubin (1983) show that if the participation decisions are independent across individuals, and if outcomes Y are independent from actual participation given Z_i, then

[10] See Lee (2005).

outcomes are independent from participation given $P(Z_i)$. This theorem allows carrying out the matching on scalars $P(Z_i)$ rather than in the multidimensional space of variables Z.[11]

To improve the results we perform the matching over the propensity scores conditional on gender and income decile, *i.e*. matched observations should be similar in terms of the propensity score, and identical in terms of gender and income group.

Exact matching on the $P(Z_i)$ is difficult as this is a continuous variable. The literature has proposed several schemes to weight observations with similar propensity scores. In this paper we use radius matching as the main weighting scheme, as it uses only good quality matches, and does not consider those treatment units who are very different from the comparisons units.[12] We also use kernel and nearest neighbor methods to assess the robustness of the estimates.

Summing up, the outcome *Y* of each participant *i* is compared to the weighted mean of a set of non-participants belonging to the same gender and income group, and whose propensity scores are "close" to that of participant *i*. To further improve the matching we restrict the analysis to adults aged 18 to 60 without a tertiary education, belonging to the bottom six deciles of the household income distribution, in the labor force, and without a formal job in the first year of the panel.[13]

In table 5.1 we show the results of estimating a model of participation in the PJH for the sample mentioned above. The probit is estimated in the initial year using only the observations that belong to the panel. The results are similar to those in Galasso and Ravallion (2003), who estimate a somewhat similar model for the crisis period 2001-2002. Women are more likely to be participants. The probability of participation is decreasing in the individual's education (incomplete primary is the omitted category). Even when controlling for family size, the number of children under 18 affects participation. There are significant differences in participation across cities, even when controlling for other observable characteristics.

Table 5.2 shows the differences in the mean values of a set of covariates between the treatment and the control group. Both groups seem pretty similar in terms of observable characteristics. None of the differences between groups are statistically significant in any of the two datasets considered.

The results of the propensity score matching analysis over the panels 2003-2004 and 2004-2005 using the radius method are reported in table 5.3. Each column shows the share of formal workers in the second year of each panel for two groups: those who were PJH participants in the first year and those in the control group. As argued above the vast majority of participants are informal. We build the control group by restricting the choice to informal workers. Therefore, the outcome variable in year 1 is zero by definition in both treatment and control groups. The table shows the share of formal workers in the second year of the panel, when some former PJH participants and some former informal workers in the control group find a formal job.

For each panel we report results for the whole sample, and then separately for men, females, those individuals whose incomes are between the moderate and the extreme poverty line, and those that are below the extreme line. In each column we report the

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[11] See the Appendix for an explanation of this method.

[12] The radius used in the paper is 0.01.

[13] Galasso and Ravallion (2004) compare the group of PJH beneficiaries with those that have applied to the program but have not received it yet. This *pipeline comparison* allows them to construct a better counterfactual as applicants may be more similar to actual beneficiaries than non-applicants. Unfortunately, the EPH included the question on application to the PJH only during 2002, so we were not able to use that variable in our study.

mean value for the treated and the control groups, the difference, the standard error, the corresponding *t* value, and the number of treated observations.

According to the first panel in table 5.3, 1.2% of all PJH participants in 2003 found a formal job one year later. The corresponding share for the control group was greater: 4.6%. The difference is statistically significant and economically large. The gap in the jump into formality is substantially larger for males than for females and approximately the same for the extreme poor and the rest. Similar results arise when using the kernel and the nearest neighbor methods (see table A.1 in the Appendix).

Results are qualitative similar in the panel 2004-2005 but quantitatively much weaker. While 3.9% of PJH participants moved from the program to a formal job, the corresponding figure for the control group was larger (5.7%) but not statistically different at 5%. In fact, the gap between participants and the control group vanishes in the case of males.[14]

We interpret these results as preliminary evidence of the informality bias of the PJH in the early stage of the program, when the transfer was significant compared to wages in the formal sector. As the nominal value of the transfer remained fixed while the formal labor market strongly recovered, the gap shrunk, and hence the distortion became less important, and possibly negligible.

A multivariate regression framework can provide results on the same issue based on a parametric model. Table 5.4 reports the results of a probit model for a dummy variable equals to 1 if the individual holds a formal job in year 2 of the panel. The model is run for the same sample discussed above. As right-hand-side variables we include program participation and a set of typical controls. The results in table 5.4 are similar than when applying propensity score matching. In the early panel 2003-2004 all marginal effects are negative, and most of them statistically significant. In particular, the impact of the program on the informality decision of males seems large. In contrast, most effects vanish in the panel 2004-2005 when the relative value of the program subsidy collapses.

Table 5.5 shows the results for the share of individuals in the labor force holding a genuine job, *i.e*. a job not related to the workfare program. PJH participants are less likely to have a genuine job. However, notice that in both panels the employment rate grew faster for the group of participants. The diff-in-diff estimator suggests that PJH participants in fact did better in terms of genuine employment than their non-participant counterparts. The results in table 5.5 do not support the view that the design of the PJH implied a bias against employment during the economic expansion.

We have shown evidence suggesting that program participants moved into formal jobs at lower rates than non-participants with similar observable characteristics, at least in the early stage of the program when the gap between the transfer and earnings in the formal sector was not large. If the assignment into the program had been random, the results could be interpreted as the causal effect of the program on labor informality. However, as discussed above, the program was offered to everyone meeting certain conditions and then rationed by queues given the limited funds.

One plausible reason why certain people got the program, while some others with similar observable characteristics did not, is having political and social contacts (sometimes also known as *social capital*) that facilitated the access to the information about the program and eased the application process (Giovagnoli, 2005; Ronconi *et al*., 2005). As mentioned above, social organizations and NGOs had a key role in helping people to successfully apply to the program. On the other hand, the economic literature has stressed the positive impact of social capital on the access to the labor

[14] Some results even change signs when using other matching methods (see table A.1).

market.[15] For instance, better political contacts could increase the probability of finding a formal job in the local public sector. If having more political and social contacts affects participation in the program and increases employability in the formal sector, then our results on the informality effect of the program will be downwardly biased. The results in table 5.3 would suggest that despite being in a better position to enter the formal labor market due to more social capital, PJH participants were more reluctant to accept formal offers, presumably because of the distortion introduced by the program.

Another possibility is that participants were on average people with low preferences for a formal job, or with unobservable characteristics that make them less attractive for formal firms. If that were the case we should find that following the recovery of the labor market a relative low proportion of participants are hired in formal jobs, even in the absence of any distortion coming from the structure of the program. Notice, however, that in this case it is likely that there always exists a gap between participants and non-participants in their formal-job take-up rates. The evidence is not inconsistent with this fact in the case of women: in both panels while around 1.5% of female participants found a formal job, the corresponding figure for non-participants (matched) women is 4%. The hypothesis of PJH participants with low preferences or productivity in formal jobs (relative to their non-participants matched counterparts) is less consistent with the evidence in the case of males. Table 5.3 shows that when formal wages substantially increased, the rates of entry into the formal sector were similar for male participants and non-participants.

A related alternative states that PJH participants are less likely to receive or less prone to accept any job offer (formal or informal), and hence the rate of entry into the formal labor market should be lower than for their non-participant counterparts. However, as the results in table 5.5 suggest, that does not seem the case: the employment rate of program participants (excluding work in the program) grew faster than the rate for non-participants.

Another possibility is that at the time of the program launching some people who qualified for the program had better perspectives of finding a formal job, and then they did not apply for the program. However, that fact may not affect our results since we start the analysis (due to data reasons) one year after the plan was implemented. Our control group is formed by people without a formal job in 2003. These workers would have found optimal to apply for the program since the cost of doing that was low, and their perspectives of finding a formal job were surely low in May 2002, given that one year later they are still in the informal sector.

The geographical distribution of the program was not uniform. Some cities captured a disproportionate share of benefits, even when controlling for observable characteristics. Giovagnoli (2005) argues that areas with local authorities “closer” to the federal government were allowed to include proportionally more participants. Cities with a high share of PJH participants may be places where people were less prone to self-select out of the program. In particular, workers with unobservable characteristics that make them more productive, and hence with higher probability of quickly finding a job may have been more likely to apply and obtain the program in cities where the access to the PJH was easier. If that had been the case, when the economy recovered the jump from the program to formal employment should have been stronger in cities favored by the allocation of the PJH benefits. To provide some evidence on this issue we include in model 5.4 an interaction variable between the individual participation in the program and the local share of PJH participants. The first column in table 5.6 shows the coefficients of this variable. The second column takes into account that the allocation of the PJH across geographical areas was likely determined by observable characteristics

[15] See Toledo (2006) for a recent study for Argentina on this issue.

of the area, like the poverty and the unemployment rate. We then construct a second interaction variable between the individual participation in the program and the residuals of a model of PJH allocation at the city level. The main result is that the interaction effect is positive and significant for females, but not for males. Female participants seem to have been more likely to find a formal job in cities with high coverage of the program.

6. Concluding remarks

Although the Programa Jefes de Hogar is legally intended to the unemployed, in practice only those participants that accept a formal job face serious risks of losing the benefits. This feature of the program implies in theory a labor informality bias. By applying matching techniques we provide some evidence in favor of this hypothesis. During the period of economic expansion the number of formal jobs available in the economy increased. According to some estimations, the share of PJH participants that found a formal job was significantly lower than the corresponding share for non-participants with similar observable characteristics. In some cases the difference is not only statistically significant, but economically large. For instance, when carrying out the propensity score matching with the radius method, in the period 2003-2004 the difference between PJH male participants and non-participants in the share of workers moving to a formal job is 5 points. The effect on informality vanishes in the period 2004-2005 when the gap between the PJH transfer (fixed in nominal terms) and wages in the formal sector greatly widened.

Although some evidence in the paper points out to the informality bias of the PJH, the results should be taken with care. As discussed above, the control group surely differs in some unobservable dimensions from the treatment group. If these factors also affect the probability of finding a formal job, the results could be biased.

In summary, basic economic theory suggests an informality bias arising from the design of the PJH. With the information available (which is not enough to be conclusive), we find preliminary evidence that the prediction holds in practice, in periods when the difference between the value of the transfer and formal wages is small.

The PJH was implemented under pressure. In 2002 Argentina faced a deep crisis, and poverty skyrocketed in a few months. In that context the government quickly implemented a program that helped alleviating the consequences of the economic crisis. When the economic crisis was overcome, the program was maintained as an instrument for poverty reduction and income redistribution, with basically no changes in its design. While certain features of the program were not relevant in a period of high unemployment and falling incomes, they become relevant in a period of economic expansion. The results of this paper suggest that the design of the PJH might have slowed down the process of formalization of the program participants. This fact calls for a careful revision of the alternatives to avoid this unwanted bias in the future.

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Appendix: propensity score matching

Rosenbaum and Rubin (1983) defined the propensity score as the probability of receiving treatment conditional on pre-treatment characteristics:

$$p(X) \equiv Pr(D = 1 \mid X) = E\{D \mid X\}$$

At the same time they state two important propositions:

Proposition 1: Balancing of pre-treatment variables given the propensity score

If $p(X)$ is the propensity score then $X \perp D \mid p(X)$

If the balancing hypothesis is satisfied, individuals with the same propensity score must have the same distribution of observable characteristics independently of treatment status. In other words, for a given propensity score, exposure to treatment is random and therefore T and C units should be on average observationally identical.

Proposition 2: Unconfoundedness given the propensity score

Let $p(X)$ be the probability of a unit i having been assigned to treatment then:

$$Y_i^T, Y_i^C \perp D \mid X \quad \Rightarrow \quad Y_i^T, Y_i^C \perp D \mid p(X)$$

In words, if the exposure to treatment is random within cells defined by X, it is also random within cells defined by the values of the mono-dimensional variable $p(X)$.

As a result of these two propositions and if it is known the individual propensity score ($p(X_i)$), the ATET can be estimated as follows:

$$\begin{aligned} \text{ATET} &\equiv E\,(Y_i^T - Y_i^C \mid D_i=1) = E\{E\,(Y_i^T - Y_i^C \mid D_i=1, p(X_i))\} \\ &= E_{p(X)}\ \{E\,(Y_i^T \mid D_i=1, p(X_i)) - E\,(Y_i^C \mid D_i=1, p(X_i)) \mid D_i = 1\} \\ &= E_{p(X)}\ \{E\,(Y_i^T \mid D_i=1, p(X_i)) - E\,(Y_i^C \mid D_i=0, p(X_i)) \mid D_i = 1\} \end{aligned}$$

Where the outer expectation is over the distribution of $p(X_i) \mid D_i = 1$.

This methodology is essentially a weighting scheme, which determines what weights are placed on comparison units when computing the estimated treatment effect.

$$\text{ATET}_E = \frac{1}{N_T} \sum_{i \in T} \left(Y_i^T - \sum_{j \in C_i} w_{ij} Y_j^C \right)$$

where N_T: number of units in the treatment group and w_{ij} are the weights applied in calculating the counterfactual for each participant.

Expectations are replaced by sample means, and we condition on $p(X_i)$ by matching each T unit i to a set of comparison units C with similar propensity score. Taken literally. conditioning on $p(X_i)$ implies exact matching on $p(X_i)$. This is difficult in practice since the probability of observing two units with exactly the same value of the propensity score is in principle zero since $p(X_i)$ is a continuous variable.

This restriction is relaxed in applied research and several methods have been proposed in the literature to overcome this problem. The question of how many C units to match with each T units is closely related to the issue between the precision of the estimate (less variance) and the bias. In this sense, a single C unit used for each T unit ensures the smallest propensity-score distance between C and T. More C unit used will reduce the variance but will increases the bias.

The Nearest Neighbor consists of searching the m C units with the closest propensity score to each T unit. The Radius method uses all of the control units within a pre-

defined propensity-score radius. If the dimension of the radius is very small it is possible that some treated units are not matched because there are no control units in it. On the other hand, the smaller the size of the radius the better is the quality of the matches. With Kernel Matching all T are matched with a weighted average of all C with weights that are inversely proportional to the distance between the propensity-scores of treated and controls.

In this paper we use these techniques of matching *with replacement.* This decision minimizes the propensity-score distance between the matched C units and the T unit: each T unit can be matched to the nearest C unit, even if a C unit is matched more than once. This is beneficial in terms of bias reduction but it is not in the precision of the estimates. At the same time, all estimations were done under the *common support region*. In other words, the values of the $p(X_i)$ were overlapped across the C and T groups.

This methodology identifies the impact of the program in expectation if there is no *hidden bias* or selection bias on unobservable, *i.e*, the condition for unbiasedness is that:

$$E\,(Y_i^C \mid D_i=1,\, p(X_i)) = E\,(Y_i^C \mid D_i=0,\, p(X_i))$$

where the expectation is taken over the distribution of unobservables.

Table 4.1
Share of households with PJH coverage

By income quintiles

	Quintiles					
	1	2	3	4	5	Mean
2003-II	31.6	23.1	9.9	4.4	0.6	11.6
2004-I	32.4	22.4	9.9	3.2	0.6	11.4
2004-II	33.6	20.9	8.4	3.2	0.7	11.0
2005-I	33.5	19.2	7.8	2.4	0.6	10.4
2005-II	30.3	16.6	5.8	2.5	0.3	9.0

By education of the household head

	Low	Medium	High	Mean
2003-II	17.1	8.9	2.2	10.9
2004-I	16.4	9.6	1.8	10.8
2004-II	16.3	7.9	1.4	10.3
2005-I	15.5	7.0	1.4	9.6
2005-II	14.4	7.1	1.0	9.0

Source: own calculations based on the EPHC.
Note: Low: primary incomplete
Medium: secondary complete
High: tertiary or college complete

Table 4.2
Distribution of PJH beneficiaries
By equivalized income quintiles

Households

	1	2	3	4	5	Total
2003-II	41.4	32.0	16.7	8.6	1.3	100.0
2004-I	42.8	32.2	17.5	6.3	1.3	100.0
2004-II	45.3	31.5	15.0	6.5	1.7	100.0
2005-I	47.8	30.3	15.3	5.2	1.5	100.0
2005-II	49.2	30.6	13.0	6.2	1.0	100.0

Individuals

	1	2	3	4	5	Total
2003-II	40.8	33.2	16.2	8.5	1.2	100.0
2004-I	42.1	33.3	17.1	6.2	1.2	100.0
2004-II	45.2	31.7	14.9	6.6	1.6	100.0
2005-I	48.3	30.0	15.1	5.4	1.4	100.0
2005-II	49.1	31.3	12.6	6.1	1.0	100.0

Source: own calculations based on the EPHC

Table 4.3
Share of PJH participants in population and employment

	2003-II	2004-I	2004-II	2005-I	2005-II
Adults aged 18 to 60					
Share in population	6.2	6.0	5.6	5.3	4.9
Share in employment	8.7	8.1	7.5	6.7	5.8
Sample: Adults aged 18 to 60, deciles 1 to 6, without tertiary education					
All					
Share in population	12.9	12.6	11.4	11.5	10.1
Share in employment	19.9	18.9	16.6	15.7	13.0
Males					
Share in population	8.8	7.6	6.8	6.4	5.0
Share in employment	11.3	9.5	8.2	7.7	5.6
Females					
Share in population	16.7	17.0	15.7	16.1	14.8
Share in employment	33.7	33.4	30.3	29.1	25.7

Source: own calculations based on the EPHC.

Table 4.4
Labor status
Adults in deciles 1 to 6 without tertiary education

	2003-II	2004-I	2004-II	2005-I	2005-II
All					
Employed - formal	12.9	13.4	14.2	14.2	15.5
Employed - informal	44.6	44.8	45.2	44.4	44.4
Unemployed	15.1	14.6	12.9	11.9	10.7
Inactive	27.4	27.1	27.6	29.5	29.4
Non-participants					
Employed - formal	14.7	15.3	16.0	16.0	17.2
Employed - informal	38.0	38.7	39.9	39.8	40.7
Unemployed	17.1	16.4	14.2	13.1	11.5
Inactive	30.2	29.6	29.9	31.2	30.7
Participants					
Employed - formal	0.5	0.5	0.6	0.4	0.6
Employed - informal	88.8	87.5	86.7	80.1	77.2
Unemployed	2.1	2.1	3.0	3.4	4.2
Inactive	8.5	9.9	9.7	16.1	18.0

Source: own calculations based on the EPHC.

Table 4.5
Share of participants by group

						Share of participants - 2003-II			
Head	With children	Unemployed	With a formal job	Qualifies 1	Qualifies 2	All	Sample	Males	Females
yes	yes	yes	no	yes	yes	0.02	0.03	0.04	.
no	yes	yes	no	no	yes	0.02	0.04	.	0.04
yes	no	yes	no	no	no	0.01	0.03	.	.
no	no	yes	no	no	no	0.01	0.01	0.01	0.01
yes	yes	no	no	no	yes	0.18	0.26	0.19	0.40
no	yes	no	no	no	yes	0.13	0.21	0.10	0.21
yes	no	no	no	no	no	0.03	0.08	0.09	0.08
no	no	no	no	no	no	0.05	0.10	0.08	0.12
yes	yes	no	yes	no	no	0.00	0.00	0.00	0.02
no	yes	no	yes	no	no	0.00	0.01	.	0.01
yes	no	no	yes	no	no	0.00	0.00	0.00	.
no	no	no	yes	no	no	0.00	0.01	0.02	.

Source: own calculations based on the EPHC.
Note1 : We do not report results for cells with less than 100 observations.
All = adults aged 18 to 60.
Sample= adults with no tertiary education belonging to deciles 1 to 6.
Males and females restricted to this sample.

Table 4.6
Earnings and PJH transfers
Adults in deciles 1 to 6 without tertiary education
Values at percentile 25

	PJH transfer (i)	Formal wage (ii)	Informal wage (iii)	Diff 1 (ii)-(i)	Diff 2 (ii)-(i)-(iii)
All					
2003-II	150	366	120	216	96
2004-I	150	397	149	247	98
2004-II	150	445	149	295	145
2005-I	150	502	155	352	198
2005-II	150	565	180	415	235
Males					
2003-II	150	398	150	248	98
2004-I	150	417	197	267	69
2004-II	150	485	199	335	135
2005-I	150	515	206	365	159
2005-II	150	595	242	445	203
Females					
2003-II	150	295	99	145	46
2004-I	150	305	100	155	55
2004-II	150	352	101	202	102
2005-I	150	402	121	252	131
2005-II	150	406	121	256	135

Source: own calculations based on the EPHC.

Table 5.1
Probit models for calibrating the propensity scores
Dependent variable: PJH participants

	Data from 2003-2004 panel				Data from 2004-2005 panel			
	Coef.	Std.Err	z	P>|z|	Coef.	Std.Err	z	P>|z|
age	0.026	0.04	0.7	0.472	-0.002	0.03	-0.1	0.944
age squared	0.000	0.00	-1.0	0.345	0.000	0.00	0.0	0.985
Male	-1.371	0.12	-11.9	0.000	-1.167	0.10	-12.1	0.000
Head	-0.153	0.25	-0.6	0.539	0.123	0.20	0.6	0.538
Spouse	-0.762	0.28	-2.7	0.006	-0.442	0.23	-1.9	0.053
Married	0.592	0.13	4.5	0.000	0.117	0.11	1.1	0.286
Complete primary	-0.214	0.11	-1.9	0.060	-0.178	0.11	-1.6	0.105
Incomplete secondary	-0.165	0.12	-1.4	0.175	-0.266	0.12	-2.3	0.022
Complete secondary	-0.351	0.14	-2.6	0.011	-0.343	0.13	-2.7	0.007
Hosehold size	0.026	0.02	1.4	0.156	0.017	0.02	1.0	0.326
Children under 18	0.625	0.22	2.9	0.004	0.518	0.17	3.1	0.002
Per capita income	-0.001	0.00	-0.6	0.534	-0.003	0.00	-3.6	0.000
Unemployed	-1.712	0.17	-10.2	0.000	-1.675	0.17	-10.0	0.000
City dummies	Yes				Yes			
Observations	1951				2113			
Pseudo R2	0.265				0.244			

Source: own calculations based on the EPH.

Table 5.2
Difference in means: treatment and control group

	Data from 2003-2004 panel				Data from 2004-2005 panel			
	Control	Treatment	Diff.	t	Control	Treatment	Diff.	t
age	34.10	33.92	0.18	0.72	34.89	34.77	0.13	0.78
Male	0.31	0.31	0.00	1.00	0.27	0.27	0.00	1.00
Head	0.42	0.46	-0.04	0.11	0.50	0.50	0.00	0.88
Spouse	0.38	0.37	0.01	0.71	0.31	0.31	0.00	0.95
Married	0.71	0.70	0.00	0.85	0.61	0.61	-0.01	0.73
Incomplete primary	0.13	0.14	-0.01	0.68	0.16	0.16	0.01	0.69
Complete primary	0.42	0.40	0.02	0.45	0.40	0.41	-0.01	0.73
Incomplete secondary	0.30	0.32	-0.02	0.54	0.26	0.27	0.00	0.90
Complete secondary	0.15	0.15	0.00	0.87	0.17	0.17	0.00	0.83
Hosehold size	5.70	5.56	0.14	0.27	5.56	5.48	0.08	0.50
Children under 18	0.78	0.81	-0.03	0.23	0.76	0.78	-0.02	0.44
Per capita income	77.98	78.25	-0.27	0.89	89.37	90.76	-1.39	0.56
Employed	0.99	0.99	0.00	0.71	0.98	0.98	0.00	0.84
Unemployed	0.01	0.01	0.00	0.71	0.02	0.02	0.00	0.84

Source: own calculations based on the EPH.

Table 5.3
Share of individuals with a formal job in year 2
Average treatment effect on the treated using propensity score matching
Radius method
Panels 2003-2004 and 2004-2005
Group of adults aged 18 to 60, in the labor force, from deciles 1 to 6, without a formal job in year 1

2003-2004-II

	All	Males	Females	Moderate poor	Extreme poor
Treated	0.012	0.011	0.013	0.021	0.004
Control	0.046	0.061	0.039	0.056	0.038
Difference	-0.034	-0.050	-0.026	-0.035	-0.034
Std.Err.	0.012	0.015	0.012	0.022	0.014
t	-2.888	-3.351	-2.114	-1.632	-2.377
Observations	562	184	378	241	242

2004-2005-II

	All	Males	Females	Moderate poor	Extreme poor
Treated	0.039	0.091	0.019	0.037	0.041
Control	0.057	0.095	0.041	0.048	0.057
Difference	-0.017	-0.004	-0.022	-0.011	-0.016
Std.Err.	0.014	0.026	0.013	0.022	0.024
t	-1.207	-0.148	-1.660	-0.495	-0.670
Observations	585	164	421	244	220

Source: own calculations based on the EPH.

Table 5.4
Model of the share of individuals with a formal job in year 2
Marginal effect of the PJH participation
Panels 2003-2004 and 2004-2005
Group of adults aged 18 to 60, in the labor force, from deciles 1 to 6, without a formal job in year 1

2003-2004-II

	All	Males	Females	Moderate poor	Extreme poor
Marginal effect	-0.03	-0.05	-0.02	-0.03	-0.04
Coefficient	-0.49	-0.90	-0.34	-0.44	-0.91
Std. Error	0.01	0.01	0.01	0.01	0.01
z	-3.30	-3.86	-1.46	-1.83	-3.19
Observations	2841	1511	944	1197	657

2004-2005-II

	All	Males	Females	Moderate poor	Extreme poor
Marginal effect	-0.01	0.02	-0.02	-0.01	-0.01
Coefficient	-0.10	0.10	-0.34	-0.14	-0.17
Std. Error	0.01	0.02	0.01	0.02	0.02
z	-0.87	0.75	-1.77	-0.64	-0.72
Observations	3226	1663	1256	1348	772

Source: own calculations based on the EPH.

Table 5.5
Share of individuals with a non-PJH job
Propensity score matching
Radius method
Panel 2003-2004 and 2004-2005
Group of adults aged 18 to 60, in the labor force, from deciles 1 to 6, without a formal job in year 1

2003-2004

	All	Males	Females	Moderate poor	Extreme poor
Year 1					
Treated	0.216	0.325	0.169	0.255	0.151
Control	0.478	0.780	0.350	0.471	0.467
Difference	-0.263	-0.455	-0.181	-0.216	-0.316
Std.Err.	0.020	0.037	0.023	0.032	0.029
t	-12.875	-12.143	-7.833	-6.738	-11.007
Observations	667	200	467	294	298
Year 2					
Treated	0.340	0.568	0.244	0.350	0.322
Control	0.529	0.811	0.409	0.534	0.519
Difference	-0.188	-0.244	-0.165	-0.183	-0.197
Std.Err.	0.022	0.039	0.025	0.034	0.034
t	-8.448	-6.274	-6.545	-5.409	-5.839
Observations	667	199	468	294	298
Changes					
Treated	0.124	0.243	0.074	0.095	0.171
Control	0.050	0.031	0.059	0.063	0.052
Diff-in-diff	0.074	0.211	0.015	0.033	0.120

2004-2005

	All	Males	Females	Moderate poor	Extreme poor
Year 1					
Treated	0.246	0.475	0.166	0.279	0.170
Control	0.500	0.780	0.401	0.542	0.433
Difference	-0.254	-0.305	-0.235	-0.263	-0.263
Std.Err.	0.021	0.040	0.024	0.033	0.035
t	-11.908	-7.547	-9.957	-7.989	-7.626
Observations	686	179	507	305	259
Year 2					
Treated	0.392	0.659	0.298	0.416	0.351
Control	0.532	0.847	0.421	0.564	0.483
Difference	-0.140	-0.188	-0.123	-0.148	-0.132
Std.Err.	0.023	0.038	0.027	0.035	0.039
t	-6.107	-4.930	-4.620	-4.249	-3.370
Observations	686	179	507	305	259
Changes					
Treated	0.146	0.184	0.132	0.138	0.181
Control	0.032	0.067	0.020	0.022	0.050
Diff-in-diff	0.114	0.117	0.112	0.115	0.131

Source: own calculations based on the EPH.

Table 5.6
Model of the share of individuals with a formal job in year 2
Marginal effect of interaction variable (individual PJH participation*intensity of the PJH in city)
Intensity defined as (i) share of PJH participants and (ii) residuals from a participation regression

	Interaction term	
	share	residuals
2003-2004		
All	0.1321	-0.0029
Males	0.4690	0.5487
Females	0.1324***	0.0912
2004-2005		
All	0.3794***	0.4997***
Males	0.0602	-0.0705
Females	0.1407***	0.1806***

Source: own calculations based on the EPH.

Table A.1
Share of individuals with a formal job in year 2
Average treatment effect on the treated using propensity score matching
Nearest neighbor and kernel methods
Group of adults aged 18 to 60, in the labor force, from deciles 1 to 6, without a formal job in year 1

Panel 2003-2004-II

Kernels

	All	Males	Females	Moderate poor	Extreme poor
Treated	0.014	0.010	0.016	0.021	0.007
Control	0.043	0.068	0.030	0.050	0.034
Difference	-0.029	-0.058	-0.015	-0.029	-0.027
Observations	633	195	438	285	284

Nearest neighbor

	All	Males	Females	Moderate poor	Extreme poor
Treated	0.011	0.012	0.011	0.021	0.008
Control	0.035	0.066	0.027	0.058	0.033
Difference	-0.024	-0.054	-0.016	-0.038	-0.024
Std.Err.	0.011	0.023	0.011	0.024	0.016
t	-2.187	-2.389	-1.523	-1.571	-1.501
Observations	633	195	438	285	284

Panel 2004-2005-II

Kernels

	All	Males	Females	Moderate poor	Extreme poor
Treated	0.048	0.104	0.028	0.045	0.036
Control	0.053	0.089	0.040	0.045	0.051
Difference	-0.005	0.015	-0.012	0.000	-0.015
Observations	641	173	468	291	248

Nearest neighbor

	All	Males	Females	Moderate poor	Extreme poor
Treated	0.042	0.093	0.024	0.046	0.038
Control	0.062	0.053	0.054	0.019	0.068
Difference	-0.020	0.040	-0.031	0.028	-0.030
Std.Err.	0.016	0.030	0.017	0.020	0.029
t	-1.245	1.324	-1.804	1.398	-1.023
Observations	641	173	468	291	248

Source: own calculations based on the EPH.

Table A.2
Share of individuals with a formal job in year 2
Average treatment effect on the treated using propensity score matching
Group of adults aged 18 to 60, in the labor force, from deciles 1 to 6, without a formal job in year 1
Aggregate panels 2003-2005

radio

	All	Males	Females	Moderate poor	Extreme poor
Treated	0.035	0.062	0.023	0.037	0.022
Control	0.051	0.086	0.036	0.046	0.046
Difference	-0.016	-0.024	-0.013	-0.009	-0.024
Std.Err.	0.007	0.012	0.007	0.012	0.010
t	-2.239	-1.991	-1.678	-0.773	-2.464
Observations	1849	566	1283	815	766

Nearest neighbor

	All	Males	Females	Moderate poor	Extreme poor
Treated	0.034	0.058	0.021	0.037	0.022
Control	0.047	0.072	0.031	0.041	0.045
Difference	-0.012	-0.013	-0.010	-0.004	-0.023
Std.Err.	0.009	0.016	0.009	0.014	0.013
t	-1.371	-0.809	-1.090	-0.273	-1.749
Observations	1890	574	1316	853	792

Kernels

	All	Males	Females	Moderate poor	Extreme poor
Treated	0.024	0.039	0.019	0.031	0.019
Control	0.048	0.082	0.034	0.047	0.044
Difference	-0.023	-0.044	-0.015	-0.017	-0.026
Observations	1572	429	1143	758	782

Source: own calculations based on the EPH.