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Social Assistance and Labor Market Outcomes: Evidence from the Uruguayan *PANES*

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Abstract

This paper uses matched social security and program micro data from the Uruguayan *Plan de Atención Nacional a la Emergencia Social* to investigate the effect of social assistance on formal labor market outcomes. One specific feature of this program is that households could gain and retain eligibility only conditional on their formal income being below a predetermined level. Using a regression discontinuity estimator, we show that, consistent with a textbook model of labor supply, the program reduced formal employment and earnings, primarily among men. Although there is evidence of a modest rebound, by and large the adverse effects on formal labor supply and earnings persist even two years after the end of the program. We speculate that these long lasting effects are due to the dynamic incentives of entitlement for social assistance.

JEL Codes: I1, J01, J2, O17

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The aim of this paper is to identify the effect of social assistance - namely the Uruguayan *Plan de Atención Nacional a la Emergencia Social* - on the formal labor market outcomes. For this purpose we use longitudinal monthly social security micro data on around 375,000 adult applicants aged 14-65 for almost six years.

In order to be eligible for the program, *PANES* applicants had to fulfill simultaneously two conditions: a level of household income from formal sources below a predetermined level (income test) and a <u>predicted</u> poverty score - in practice a linear combination of a number of baseline household characteristics - above a predetermined threshold (proxy means test). Conditional on meeting the income test, proxy means test targeting offers the opportunity to identify program effects based on a Regression Discontinuity design (Guido Imbens and Thomas Lemieux, 2008; David S. Lee and David Card, 2008) allowing us to circumvent problems of reverse causality and omitted variables that plague many observational studies.

For the purpose of the analysis, we match program data for the universe of both successful and unsuccessful applicants to data on earnings and employment from social security records. While *PANES* was a temporary program running from April 2005 to December 2007, social security data span from March 2004 to December 2009, hence both before and after the program. Pre-treatment data allow us to test the validity of the identification assumption underlying the consistency of the RD estimates, while post-program data allow us to investigate medium-term effects of the program.

Section I discusses the evidence on the effect of welfare transfers on labor supply, Section II introduces the program and discusses its potential effects on labor market outcomes. Section III presents the data and basic descriptive statistics, Section IV presents the regression results and Section V finally summarizes our findings, offers some conclusions and outlines future steps.

I. SOCIAL ASSISTANCE AND LABOR MARKET OUTCOMES

There is a well established empirical literature on the impact of income support programs on adult labor supply. Standard economic theory predicts that income transfers will cause a fall in beneficiaries' labor supply, due to an income effect, with an ensuing efficiency loss (see for example Robert Moffitt, 2002a). Additionally, the implicit tax on labor earnings induced by income-testing, a feature of many welfare programs, creates additional disincentives to work.

Concerns about the negative effects of welfare transfers potentially arise also in the medium and long terms due to welfare dependency, possibly induced by losses in marketable skills, welfare stigma or asymmetric effects of welfare eligibility on the transitions into and out of social assistance.

I.a US Evidence

The most convincing evidence on the impact of social assistance on labor market outcomes comes from the US. Early quasi-experimental studies find clear evidence of negative labor supply effects of the *Aid to Families with Dependent Children* (*AFDC*), a cash transfer program originally targeted to low-income single parent households (Hilary Hoynes, 1996; Moffitt, 2002b). A complementary body of research finds evidence that, by establishing time limits on welfare recipiency and introducing work requirements, the *Temporary Assistance for Needy Families* (*TANF*), which replaced *AFDC* starting in 1996, reduced the disincentives to engage in the labor market (Moffitt, 2002b; Robert F. Schoeni and Rebecca M. Blank, 2000; Blank, Card and Philip K. Robins, 2000; Blank, 2006). The concurrent expansion of the *Earned Income Tax Credit* (*EITC*), a negative income tax schedule scheme, encouraged employment of single mothers, while leading to a small reduction in hours of work among those already in employment (Timothy J. Bartik, 2000; Blank, 2006; Nada Eissa and Hoynes, 2004, 2005; V. Joseph Hotz and John Karl Scholz, 2006).[1]

I.b Evidence from Latin America

Most of the recent surge in welfare transfers in Latin America has come in the form of conditional cash transfers (CCT). Typically, CCTs do not include a negative income tax or a work requirement component so one will expect participation to lead to a fall in labor supply due to an income effect.

When programs are additionally income-tested, a feature of *PANES* common to other but not all programs in the region, a potentially relevant margin of adjustment is through reduced labor market formalization. Workers might be willing to accept informal jobs and possibly lower

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[1] In a paper that is somewhat related to ours, which is also based on regression discontinuity design, Lemieux and Kevin Milligan (2008) find a pronounced fall in labor supply among low educated men in Quebec as a result of social assistance receipt.

pay in exchange for welfare payment, as, for obvious reasons, income‑testing is typically based on formal income sources.[2]

Because of these potentially undesirable effects, some welfare programs explicitly include work conditionalities (e.g. public works components) as a way to monitor work involvement and make welfare recipiency more costly to those with alternative (informal) sources of labor income.

The existing evidence on the labor market effects of social assistance in Latin America is mixed. Analyses of the most popular and most studied CCT program, the Mexican *Progresa‑Oportunidades*, for example, find no evidence of significant labor supply disincentives (Susan W. Parker and Emmanuel Skoufias, 2000; Skoufias and Vincenzo Di Maro, 2008; Skoufias and Teresa González-Cossío, 2008). Similarly, analyses of the Ecuadorian *Bono de Desarollo Humano* ‑ a *de facto* unconditional cash transfer ‑ find a reduction in children's economic activity but no effect among the household heads (Eric V. Edmonds and Norbert Schady, 2010). Along similar lines, there is evidence of a mechanical contemporary increase in labor supply (public employment programs, support for self‑employed, training, etc.) among beneficiaries of *Chile Solidario*, although this effect does not translate into increases in labor supply in the medium term (Emanuela Galasso, 2006).

Other studies find positive labor supply effects of social assistance. Ana Maria H. C. de Olivera *et al.* (2007), using propensity score matching, for example, find a positive impact of the Brazilian *Bolsa Familia*, an income-tested program, on the level of adult employment and activity. Existing evaluations of the Colombian *Familias en Accion* (IFS, Econometría S.A. and SEI, 2004) also show a positive effect on labor supply and hours of work, at least among males in rural areas.

Only a few studies among those reviewed show negative labor supply effects. In their analysis of labor market effects of *PRAF*, the Honduran conditional cash transfer program, Maria Laura Alzua *et al.* (2010) find a small and negative effect of the program on adult employment but none of the estimated coefficients is statistically different from zero at conventional levels. There is also evidence of negative labor supply effects among (male) recipients of the *Red de*

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[2] A related argument is made by Santiago Levy (2008). In a world where formal workers access bundled obligatory benefits through social security, unbundled voluntary benefits via social protection may act as a subsidy to informal employment, with potential increased spending, reduced social security contributions, and negative impacts on productivity and economic growth.

Protección Social, the Nicaraguan cash transfer program (John Maluccio, 2007 and Alzua *et al.*, 2010).

The evidence on welfare programs on labor market informality is scarce, not least because of data limitations. Based on household survey data and a matching technique, Leonardo Gasparini *et al.* (2009) find evidence that the Argentinean *Jefes de Hogar*, for which participation was conditional on the household head not being in formal employment, led to an increase in the rate of labor market informality.

In sum, there is some disagreement as to whether social assistance in Latin America has necessarily the same adverse labor supply consequences as those found for the US and other rich economies. However, there are difficulties to extrapolate from existing studies, partly due to the specificities of the programs, and partly because not all studies rely on convincing experimental or quasi-experimental designs.

The apparent contrast between the results found for the US and Latin America is often explained by the fact that these programs are targeted towards very poor populations, with arguably low income elasticity of leisure. Additionally, health and children's schooling conditionalities that are attached to many CCTs programs can also induce positive labor supply responses. Increased school attendance, in particular, due to schooling conditionalities, with its associated direct (school fees, uniforms, books, etc.) and indirect costs (reduction in child labor) can potentially lead to a rise in adults' labor supply due to a simple income effect. Conditionalities can also in principle enhance the human and social capital of participants and hence their employability, at least in the medium and long run.

Also, only a small number of programs are based on income targeting, with an implicit subsidy to non-employment and informal employment, so one should not assume a priori that these programs have the same effect on labor supply as income-tested programs available in other parts of the world.

In this study we aim to provide evidence on the effect of social assistance on labor market outcome that is based on a quasi-experimental identification strategy. By focusing on an income-tested program that was *de facto* unconditional, we can also credibly isolate the role of transfers in isolation from other program ingredients (conditionalities) that might induce offsetting effects.

II. *PANES*: INSTITUTIONAL DETAILS AND PREDICTED EFFECTS

The Uruguayan *PANES* was a temporary social assistance program running from April 2005 to December 2007. The program was conceived in response to the severe economic crisis of 2001‑2002, when per capita income fell by more than 10%, unemployment reached its highest level in twenty years, and the poverty rate doubled.

PANES aimed to provide direct assistance to households who had experienced a worsening in living standards during the economic crisis of the early 2000s and to strengthen the human and social capital of a chronically poor population. The program was replaced in January 2008 by a comprehensive system of family allowances, as part of a broader reform of the welfare and tax system (*Plan de Equidad*).[3]

As *PANES* recipients were less likely to enter the formal labor market during the program period, they might also have been more likely to take up the *Plan de Equidad,* even if in principle they were as likely to be eligible as *PANES* non-recipients.

II.a Enrollment and eligibility

The program target population consisted of the 20% poorest households among those below the national poverty line. Enrollment occurred in two phases. All low income households were publicly invited to apply, while the government made a large outreach effort, sending enumerators to poor communities in an attempt to boost applications among the most marginalized.

Among applicant households, only those with a level of per‑capita income (as resulting from social security data) below approximately US$ 50 were visited by ministry personnel and administered a detailed baseline questionnaire. This income requirement was maintained throughout the life of the program, so that households automatically lost eligibility if their formal income from work plus contributory benefits increased at any point in time above the US$ 50 threshold. Applications were accepted for the entire period of life of the program and rejected households could eventually reapply if their circumstances had in the meantime changed. Most households applied for the program at its inception (soon after April 2005) although, due to the

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[3] The *Plan de Equidad* also encompassed a major reform of the tax system, (from July 2007) and a health care reform.

large number of applications, there was a large delay in visiting households and administering the baseline survey. Figure A1 in the appendix shows the key program dates.

Conditional on household satisfying the income test, assignment to *PANES* was determined using a predicted poverty score (proxy means targeting) that depended on household socioeconomic characteristics collected at baseline survey.[4] Only households with a score above a predetermined level were assigned to the program.[5] Existing evidence shows successful targeting relative to most Latin American cash transfer programs (World Bank, 2007).

Of the 188,671 applicant households (around 700,000 individuals), around 102,000 eventually became program beneficiaries, approximately 10% of all Uruguayan households (and 14% of the population of around 3.3 million).[6]

II.b Program components

The main component of *PANES* was a monthly cash transfer (*ingreso ciudadano*, "citizenship income"), whose value was originally set at US$ 56 (UY $1,360 at April 2005 exchange rate, around US$ 103 at PPP adjusted exchange rate) independent of household size, amounting to approximately 50% of average pre-program household self-reported income. This amount was periodically adjusted for inflation. Together with the first payment, households received arrears dating back to the date of application.

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[4] The poverty score, devised by researchers at Universidad de la República (Verónica Amarante *et al.*, 2005), including some of the authors of this paper, was based on a probit model of the likelihood of being below a critical per capita income level, using a highly saturated function of household variables (household age structure and headship, an indicator for public employees in the household, an indicator for pensioners in the household, average years of education of individuals over age 18 and its square, interactions of age indicators with gender, indicators for age of the household head, residential overcrowding, whether the household was renting its residence, toilet facilities and an index of durables ownership). The model was first estimated using the 2003 and 2004 National Household Survey (*Encuesta Continua de Hogares*). The resulting coefficient estimates were used to predict a poverty score for each applicant household using *PANES* baseline survey data. Neither the enumerators nor households were ever informed about the exact variables that entered into the score, the weights attached to them, or the program eligibility threshold, easing concerns about its manipulation. The eligibility thresholds were allowed to vary across five regions.

[5] The decision of using a predicted score rather than income itself was driven by a number of factors. First, many households in the objective population had highly unstable income flows, so current income was seen as a bad proxy for permanent income. Second, because the target population was often employed in the informal sector, it was difficult to verify their reported income against Social Security records, opening up the possibility of misreporting. By using a wide array of socioeconomic characteristics, as opposed to self-reported income, the government also hoped to minimize strategic misreporting.

[6] The total cost of the program - which was financed by internal resources - was almost 250 million US$, i.e. almost US$ 2,500 per beneficiary household. On an annual basis, this represents 0.41% of GDP and 1.95% of government social expenditures.

Households with children and pregnant women were also entitled to a food card (*tarjeta alimentaria*), an in-kind transfer that operated through an electronic debit card whose monthly value varied between US$ 13 and US$ 30, depending on the number of children and pregnant women in the household.[7] The food card, which was launched only in the second semester of 2006, allowed households to purchase food and goods for personal hygiene in participating stores.[8]

Similar to other Latin American CCT programs, *PANES* participation was in principle conditional on children's school attendance and health checks. Children aged 6 to 14 years old were expected to be enrolled and attend school regularly, pregnant women had to attend to monthly prenatal visits and health checks, while children aged 0 to 5 were supposed to comply with the mandatory pediatric checks and vaccinations prescribed by the Ministry of Health. Due to scarce inter-institutional coordination conditionalities were, however, *de facto* not enforced, an issue publicly acknowledged by *MIDES* after the end of the program. Evidence from a small follow-up survey also shows that most program recipients were unaware of conditionalities being attached to the program.

II.c Predicted program effects

Simple economic theory suggests that the cash transfer and in-kind components of *PANES* together with income-testing should have led to a reduction in labor supply and potentially in the probability of formal employment, conditional on being in work.

As the target population was largely excluded from the formal labor market at baseline, and because Uruguay underwent a period of rapid economic and job growth starting from the mid 2000s, one related question is whether and to what extent different trends in employment and earnings between successful and unsuccessful applicants - if any - manifested in differential rates of transition from non-employment into employment (accessions) as opposed to different

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[7] 70% of beneficiary households eventually received the food card. Prior to scaling up of the program, food baskets were distributed to all households (whether *PANES* or not) whose children attended schools with a high proportion of *PANES* beneficiaries.
[8] Other minor components of *PANES* were *Rutas de Salida* (training and educational activities aimed at fostering social inclusion, promoting knowledge of rights and strengthening social ties) and a workfare program, *Trabajo por Uruguay* that provided a stipend of twice the amount of the *Ingreso Ciudadano* for a period of up to six months. Only 15% of *PANES* households took part in *Rutas de Salida*, mainly due to supply constraints, and 17% of households benefited from *Trabajo por Uruguay.*

rates of transition from employment to non-employment (separations). The longitudinal nature of the data allows us to distinguish between these two mechanisms.[9]

A second question is whether and to what extent the effect on the level of formal employment - if any - can be attributed to different labor market participation rates, as opposed to different rates of formalization. At this stage, we have no way to ascertain the relative role of these two competing explanations, although more advances could be made with auxiliary data, as argued in the conclusions.[10]

Theory provides less clear-cut indications about the effects of the program in the medium term. As one of the aims of *PANES* was to increase the social and human capital of beneficiaries, hence potentially their employability, one might expect any negative labor supply effects to disappear and even possibly to turn positive in the medium term.[11] State-dependence in welfare eligibility or employment, however, might lead to persistent negative labor supply effects. In this respect, as the program was replaced in 2008 by a new system of welfare (*Plan de Equidad*), whose pillar was a generous system of child allowances, this might have impacted differently on *PANES* eligible and ineligible households. Although eligibility for the new *Plan de Equidad* was uncorrelated with the *PANES* eligibility criteria,[12] to the extent that social assistance only affected accession rates into formal employment (but not the separation rates out of formal employment), one might expect original *PANES* beneficiaries to display persistently higher levels of welfare participation and lower levels of labor market participation even after *PANES* came to an end. The dynamic incentives of social assistance hence potentially play a significant role in explaining trends in labor supply.

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[9] At this stage, however, we are unable to distinguish between the income effect of the cash transfer and the substitution effect induced by the implicit taxation on benefits stemming from income-testing. A priori, though, we suspect that the income effect of the transfer should be relatively modest, as participant households had very low income levels and hence, arguably, low income elasticity of labor supply. Investigating the relative role of the income and substitution effect would require analyzing the propensity of program beneficiaries to bunch in the proximity of the income eligibility threshold, something that at present we are unable to do with the available data.

[10] A related question is whether households might have responded to their program beneficiary status via behavioral adjustments other than labor supply, for example reducing their reliance on other sources of welfare.

[11] Potential influences through conditionalities do not seem to be a priori important as, as said, conditionalities were not enforced and most of the beneficiaries did not even know of their existence. Moreover, previous empirical evidence indicates that *PANES* did not have any impact on child school attendance and child labor (Amarante *et al.*, 2010), so second round effects via these channels can be reasonably supposed not to be important.

[12] The *PANES* poverty score was also used to determine eligibility for the *Plan de Equidad* (one other condition being that households had at least one child aged less than 18) but the eligibility threshold was increased across the board.

III. DATA AND DESCRIPTIVE STATISTICS

III.a Administrative program data

The different sources of data available for this study are summarized in Table A1 and Figure A1 in the appendix alongside the period covered by each data set. The first source of data is given by administrative program data on all applicants, whether beneficiaries or not. For all individuals in applicant households who met the income eligibility criterion, we have baseline information collected at the time of enrollment (which, as said, might vary across households). These data include a very large array of socioeconomic variables, including those used to compute the poverty score, which in turn determined program eligibility. These data also allow us to identify individuals belonging to the same household at baseline. Program data also include month by month disbursements of different program components.

III.b Social security data

We match program data to monthly micro data from the administrative records of the social security administration (*Banco de Previsión Social*: BPS) using individuals' unique national identity number (*cedula*). For each individual aged 14 to 65 we have information on formal employment (for both employees and self-employed workers, including information on multiple jobs) and household social benefits and transfers, such as unemployment insurance, disability transfers, child allowances, contributory and non-contributory pensions (see Table A1). For those in work, the data also provide number of jobs in the month, a unique identifier for the firm(s) where they work, monthly earnings and number of days worked in each job.

Social security information is available from March 2004 to December 2009, so before, during and after the program, allowing us to identify potential pre- and post-trends. The resulting data set is extremely large, with information on more than 25 million individual-time observations (around 375,000 individuals, 56% of which women, for seventy months).

III.c Descriptive statistics

Table 1 presents descriptive statistics for three sub-periods: before (March 2004 to March 2005), during (April 2005 to December 2007) and after (January 2008 to December 2009) *PANES*. All

statistics are reported separately by *PANES* eligibility status and gender and in this and the following tables monetary values are expressed in April 2005 UY$ (exchange rate: US$ 1=UY $26).

The data show a very low level of participation in formal employment at baseline, especially among women and *PANES* eligible individuals: 9% of eligible women are in formal employment at baseline versus 13% for ineligible women, while for men the figures are respectively 13% and 20%. Due to sustained economic and job growth during the second half of the 2000s,[13] participation grows markedly over time, being respectively on the order of 34% and 23% for *PANES* ineligible men and women in the post-*PANES* period. Unsurprisingly, *PANES* eligible individuals are less likely to be in work than ineligible individuals at baseline, although there is also evidence that this difference persists over time. In the pre-*PANES* period, daily wages are on the order of UY $120 for women and UY $180 for men, between approximately US$ 5 and 7 (around twice as much at PPP adjusted exchange rate), by all means low wage rates. Real daily wages grow by around 70-80% for both men and women between the pre- and post-*PANES* period but there is no discernible difference in trends between eligible and non-eligible individuals.

Participation in *PANES* is - as expected - much higher among eligible than non-eligible households, although there is also some evidence of non-enforcement (see below).

IV. EMPIRICAL ANALYSIS

IV.a Basic evidence: program participation and formal employment

Figure 1 reports the proportion of individuals whose household participated in *PANES* as a function of the poverty score in the second semester of 2007 (when the program was well under way). The figure reports separate results for men and women: unsurprisingly there is essentially no difference in participation across the two groups. The poverty score is standardized to the value of the eligibility threshold, so that, according to the criteria illustrated above, households

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[13] Between 2005 and 2007 the Uruguayan economy grew at a sustained rate, led by an increase in the international commodity prices. Labor market indicators experienced a significant improvement: activity and employment rates grew significantly whereas unemployment declined (Table A3). At the same time, the restoration of a centralized wage setting mechanism and a strong government campaign led to a marked increase in the rate of formalization: the percentage of workers not contributing to the social security decreased from 39% in 2005 to 32% in 2009. At the same time, earnings increased significantly, due not only to the economic recovery but also to a substantial increase in legislated minimum wages.

on the right of zero are eligible and those to the left are ineligible. We restrict to households in a neighborhood of the eligibility threshold of size 0.2 (i.e. with a deviation in the probability of being in the objective population of 20 p.p.), with values of the poverty score between -0.1 and 0.1. There are 102,352 households and 220,795 individuals in this sample. For the sake of presentation - as the poverty score is a continuous variable - we have grouped the data into 60 equally spaced intervals.

The figure shows a clear increase in the probability of program participation as the score increases. More important, and consistent with the rules determining eligibility, there is a very clear discontinuity - on the order of 40 p.p. (from 20% to 60%) - in the probability of program receipt at the zero cutoff. [14]

We have also superimposed to the data a parametric polynomial in the poverty score. In practice, if S is the predicted income score standardized to the eligibility threshold, we follow Lee and Card (2008) and we regress the variable of interest y, on a constant, an indicator for households above the threshold $1(S>0)$, and two parametric polynomials in the normalized score ($f(S)$ and $g(S)$), one on each side of the threshold, such that $f(0)=g(0)=0$:

(1) $y=\beta_0 + \beta_1 \, 1(S>0) + f(S) + 1(S>0) \, g(S) + u$

In Figure 1 we report a third order polynomial fit to the data together with the 95% confidence interval (obtained by clustering standard errors by household). One can see that the

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[14] One should not be surprised that, among eligible applicants, some were not in receipt of the program by the second semester of 2007. As said, treatment status could be lost if household income exceeded at any point in time the income threshold or households might have refused to take the program up or voluntarily given up participation. More interestingly, some ineligible households appear to having been treated. There are several explanations for this: other than imperfect enforcement due to administrative errors or manipulation on the part of program officials, which we tend to, rule out, there were some exceptions to the eligibility rule (i.e. for homeless people). Second, the score was slightly changed at one point towards the beginning of the program as the Minister realized that some elderly individuals were systematically excluded. The score hence was recomputed for households who had entered the program before September 2005 and their eligibility status was re-assessed. It follows that ineligible households might still have received the program at some point in time. Third and more important, households could be revisited and their score recomputed following an appeal if they thought they had been unfairly disqualified or if their circumstances had in the meantime changed (e.g. due to the birth of a child). This could have led to a new poverty score and potentially a change in the eligibility status. In order to keep to the spirit of an intent-to-treat estimator, in this paper we only use the score initially assigned to each household. This explains why some households with an initial score below the eligibility threshold might have been eventually treated. For the purpose of the paper we consider these households as ineligible. Note that in Marco Manacorda *et al* .(2010) we show a graph with perfect enforcement. This is because we exclude homeless individuals and those who entered the program before September 2005. To enhance the precision of the estimates, in this paper we prefer to use the entire sample.

polynomial fits the data remarkably well and the jump at the discontinuity is sizeable and significant.

Figure 2 presents information on average earnings, including zeros for those out of work, in the same period (second semester of 2007). One can see that average earnings fall with the poverty score. For men (but not for women), there is clear evidence of a negative gap in formal earnings between those just to the right and those to the left of the eligibility threshold.[15]

Although the evidence in Figures 1 and 2 can be interpreted as a negative effect of program receipt on labor supply, at least for men, some caveats are in order. The identification assumption required to interpret the estimates in Figure 2 as program causal effects is that the outcome variable is a continuous function of the score other than for the effect of treatment (see for example Imbens and Lemieux, 2008). This can be tested on pre-program data.

A related concern is that assignment around the discontinuity is not as good as random, due to score manipulation. Figure A2 in the appendix, though, shows that the poverty score is smooth around the discontinuity, something that one would not expect if the score had been manipulated (see Justin McCrary, 2008).

Even if the identification assumption holds, this design provides an estimate of local average treatment effect in the neighborhood of the discontinuity point. In the presence of heterogeneous effects, hence, these findings cannot be necessarily generalized to the beneficiaries of the program as a whole.

In the rest of the analysis we expand on these early results, by investigating the effect on a variety of outcomes and presenting a variety of specification checks.

IV.b Estimates over time

Figure 3 presents month by month RD estimates of program eligibility on the probability of program receipt, i.e. being in a household that is a *PANES* beneficiary, together with estimated confidence intervals from model (1). Data, as above, refer to those in the neighborhood [-0.1, 0.1] around the threshold and are obtained by fitting third order polynomials to the data. Standard errors are clustered by household in each month.

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[15] Another remarkable feature of the data is that, while earnings fall with the poverty score to the left of the threshold, they are essentially flat to the right of it. We suspect that is precisely due to income testing. While in the absence of the program, earnings are likely to fall with the poverty score (explaining the negative slope to the left of the threshold), program households will tend to cluster at the income threshold (explaining why earnings are independent of the poverty score).

Two vertical lines refer respectively to the months when the program was launched (April 2005) and then discontinued (January 2008). Obviously, program receipt is zero outside this interval. One can clearly see that the effect of eligibility on program receipt increases rapidly with time, as households get incorporated into the program. By the early 2006, eligible individuals are approximately 70% more likely to receive the program than non-eligible ones. By the end of the program, this figure is on the order of 40%, implying that many eligible individuals have exited the program, presumably to enter the labor market. There is no substantial difference in program take-up between men and women.

Figure 4 reports the same information as in Figure 3 using the actual amount of the *PANES* transfer. Here we report total transfers, including zeros, divided by the number of individuals aged 14-65. Per capita *PANES* transfers are on the order of UY $300 for men and UYS400 for women, (i.e. between US$ 11 and $15), implying that women are presumably in smaller households, most likely due to a high proportion of single mothers.

Figure 5 reports month by month estimates of the effect of the program on monthly earnings. The data include zeros for those out of work, implying that the estimates possibly mix employment effects (at both the intensive and the extensive margin) with the effect on daily or hourly wage rates. The results for men are remarkable: although we notice some slight negative correlation between eligibility and earnings in the pre-*PANES* period, coefficients are insignificant. The gap between eligible and ineligible individuals decreases monotonically with time. By June 2007, this gap is on the order of $UY200 (around USD 8$) and is significant at conventional levels. Even more remarkable is that this gap remains roughly constant over the two years following the end of the program. Surprisingly, the effect is much smaller for women, although there is some very weak evidence of coefficients turning negative towards the end of the program.

The following figures investigate additional outcomes. Consistent with Figure 5, we see some negative effect of eligibility on the fraction employed (Figure 6) and days of work (Figure 7). We see no evidence of the effects being driven by differences in daily wages (Figure 8), suggesting that it is the variation in labor supply rather than in wage rates to give reason of the evidence in Figure 5.

Figures 9 and 10 report separate estimates on earnings for those with at least one month of employment and those with no employment spells (the majority) in the pre-*PANES* period. All

effects seem to be driven by the latter group, implying that it is different rates of accession to employment rather than different exit rates from formal employment that drive the results in Figure 5. This is perhaps no surprise as, as said, this was a period of rapid rise in formal employment and the cost of leaving formal employment is clearly high.

In sum, the data show unequivocal negative labor supply effects, especially for men, which persist after the end of the program.

IV.c Specification checks

In Tables 2A and 2B we report estimated program impacts on earnings in each semester using different parametric and non‑parametric specifications. We present results for different intervals around the discontinuity, separately for men and women. Columns 1 to 4 refer to individuals in an interval around the threshold for the poverty score [‑0.05 to 0.05] while the remaining columns refer to an even more localized interval around the threshold: [‑0.02 to 0.02].The number of individual‑time observations varies between approximately 1.2 and 3 million for men and 1.4 and 4 million for women, depending on the interval used. For each sample, we report specifications that include increasingly higher order polynomials (of degree 2, 3 and 4) interacted with a semester dummy (in columns 1 to 3 and 5 to 7). These regressions pool observation from the entire period and standard errors are clustered by individual. Non-parametric regressions (in columns 4 and 8) use instead local linear polynomial and are run separately by semester. Standard errors are not clustered (as this option is not available in the *rdob* Stata command we use) so one should be cautious in making any firm inference based on these non‑parametric regressions.

For men, point estimates in Table 2A are largely robust to the polynomial used, although their precision decreases when we include higher order polynomials or we restrict the range of data; also, estimates are systematically more precise when we use local linear polynomials, although standard errors are likely to be underestimated in these specifications. There is some weak evidence of differences at pre‑treatment but these effects are small and generally insignificant. The data show a significant fall in earnings towards the end of the *PANES* period with no obvious rebound.

For women, the effects are smaller (consistent with their smaller levels of earnings): we see some fall during the *PANES* period with some rebound afterwards.

Tables 3A and 3B report the same parametric specifications as in columns 1 to 3 and 5 to 7 of Tables 2A and 2B, with the inclusion of individual fixed effects and age dummies.[16] The inclusion of individual fixed effects allows us to control for permanent differences in outcomes across individuals. For both men and women results are essentially in line with the estimates in Tables 2A and 2B. For women, though, point estimates are largely insignificant.

To get a sense of the magnitudes involved, it appears that a rise in *PANES* transfers on the order of UY$ 400 to 500 as a result of eligibility (see Figure 4) leads to a contemporaneous fall in formal earnings on the order of UY$ 200 for men and around $UY50 for women. These are sizeable effects, implying that, at least for men, ten additional dollars in transfers translate into five less dollars in earnings.

V. PRELIMINARY CONCLUSIONS AND FUTURE STEPS

Results based on matched program and social security data show a sizeable fall in labor supply among *PANES* beneficiaries (relative to non‑beneficiaries) as a result of program receipt. These effects persist over time.

Consistent with much literature from the US, but in contrast to some findings for Latin America, we find that individuals are responsive to income‑tested welfare programs in a fashion that is consistent with standard labor supply theory. Effects are sizeable, at least for men. One obvious explanation for the differences between men and women is that effects for women are harder to identify due to much lower participation rates. Another possibility is that women lower market wages make income testing less of a binding constraint for them, implying that the disincentives effect of the program on formal labor supply are more likely to manifest among men.

Perhaps puzzlingly, we also find very persistent effects among men. One possibility is that the dynamic incentives produced by subsequent welfare reforms discouraged former *PANES* beneficiaries from entering the labor market. As *PANES* eligible individuals were less likely to be in the formal labor market as *PANES* ended, they might also have had greater incentives to take up the new *Plan de Equidad*, although at this stage we cannot rule out pronounced state-dependence in employment. In all cases, this result is consistent with our finding that different

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[16] Clearly we are unable to include individual fixed effects in the non-parametric regressions in columns 4 and 8 of Tables 2A and 2B.

rates of formalization between *PANES* eligible and ineligible individuals were driven by differences in employment accession rates rather than separation rates.

At this stage, we are unable to say whether and to what extent the effect of the program on the rate of formal employment can be ascribed to a genuine reduction in labor supply, increased informality or even possibly *PANES* beneficiaries being involved in other activities (i.e. schooling). The welfare implications of our work are not neutral to what use *PANES* beneficiaries eventually made of their time out of formal employment. We plan to investigate this by using firm level data. If lower labor market participation among recipients is largely ascribable to informality, and in particular to hours underreporting among those in formal work, we would expect total hours of work to grow less in firms that happened to hire *PANES* versus non-*PANES* workers. We also plan to use information from two waves of a small sample follow-up survey that collects self reported data on employment, earnings and formal status for both successful and unsuccessful *PANES* applicants to investigate informality effects.[17]

Our analysis has not yet fully incorporated information on benefits. This is crucial to understanding potential substitution towards other sources of income among program beneficiaries and to fully investigate the effect of the income-tested nature of the program on labor supply, including what standard economic theory predicts being a bunching of program beneficiaries at the income eligibility threshold. We plan to investigate all these dimensions, including the labor supply decisions of the households as a whole, in the next version of the paper.

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[17] This follow up survey was specifically designed to carry out the impact evaluation of the program, and two waves were administered. To exploit the discontinuity design, the original survey sample contained data on 3,000 households, including both eligible and non-eligible applicants, in the neighborhood of the program eligibility threshold score. The first wave was carried out between December 2006 and March 2007, roughly eighteen months after the beginning of the program. A second follow-up household survey round was collected between February and April 2008, shortly after the temporary *PANES* program had already ended. These surveys contain detailed information on activity, employment, unemployment, hours of work, labor income and reported formal status. In addition to information on housing, household composition, durables possession, work, income and schooling (as in the baseline survey), the follow-up survey collected information on health, economic expectations, knowledge of political, labour and civil rights, trust in a wide set of institutions, participation in social groups, people or institutions he/she asks for help when in trouble, opinions about the *PANES* program, and political attitudes.

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Figure 1: Fraction in program as a function of poverty score - 2nd semester 2007
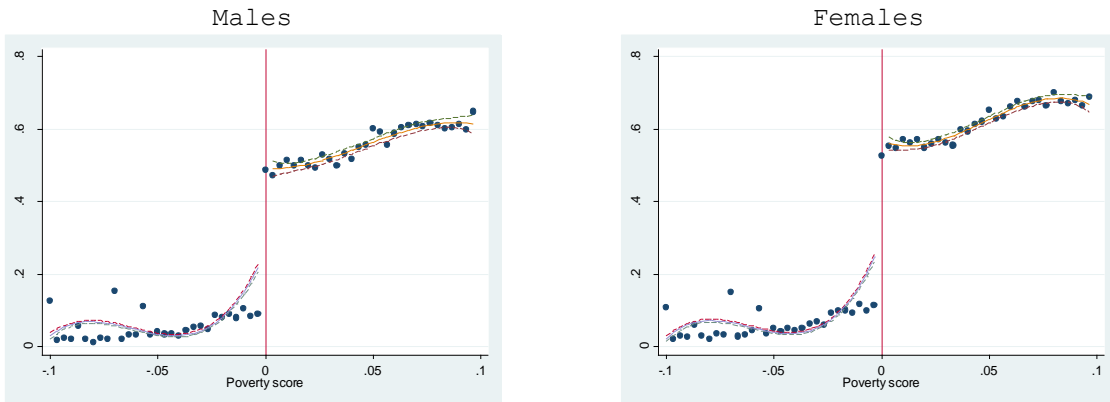
Notes. The figure reports the fraction of individual-months observations with positive *PANES* transfers in the second semester of 2007 as a function of the *PANES* score (rounded to the closest multiple of 200.0033). Data only refer to observations in a neighborhood [-0.1, 0.1] around the eligibility threshold. A third order polynomial in the poverty score and the associated 95% confidence interval (derived from standard errors clustered by household) are also reported.

Figure 2: Average earnings (including zeros) as a function of poverty score - 2nd semester 2007
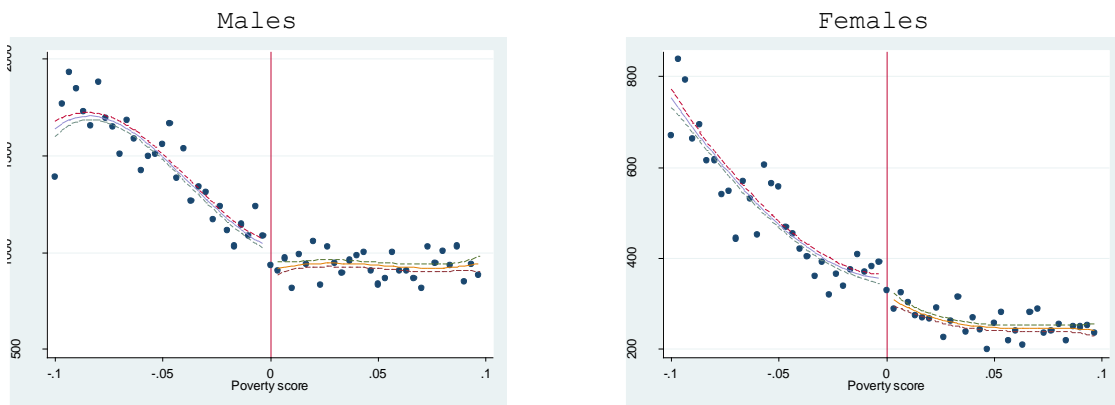
Notes. The figure reports average earnings (including zeros) in the second semester of 2007 as a function of the *PANES* score. See also notes to Figure 1.

Figure 3: RD estimates of eligibility on fraction treated - by month

Males

Females
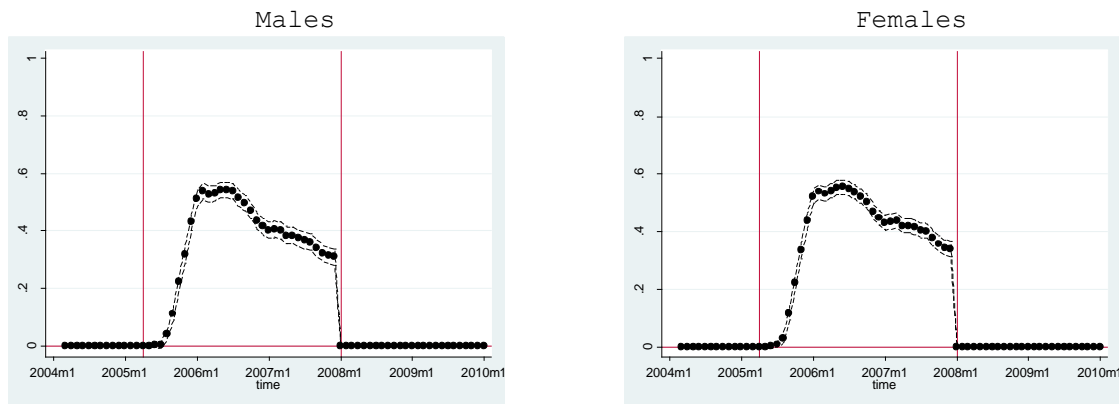
Notes. The figure reports the estimated effects of eligibility on the probability of receiving a *PANES* transfer in each month separately by gender. 95% confidence intervals (obtained from separate regressions by month with standard errors clustered by household) are also reported. In addition to the eligibility dummy (a dummy for the poverty score above the threshold), regressions include controls for two third order parametric polynomials in the poverty score on each side of the eligibility threshold. Two vertical lines correspond respectively to the first and last month of the *PANES* program (April 2005 and January 2008). The sample refers to individuals aged 14 to 65 in a range [-0.1, 0.1] around the eligibility threshold.

Figure 4: RD estimates of eligibility on *PANES* transfers - by month
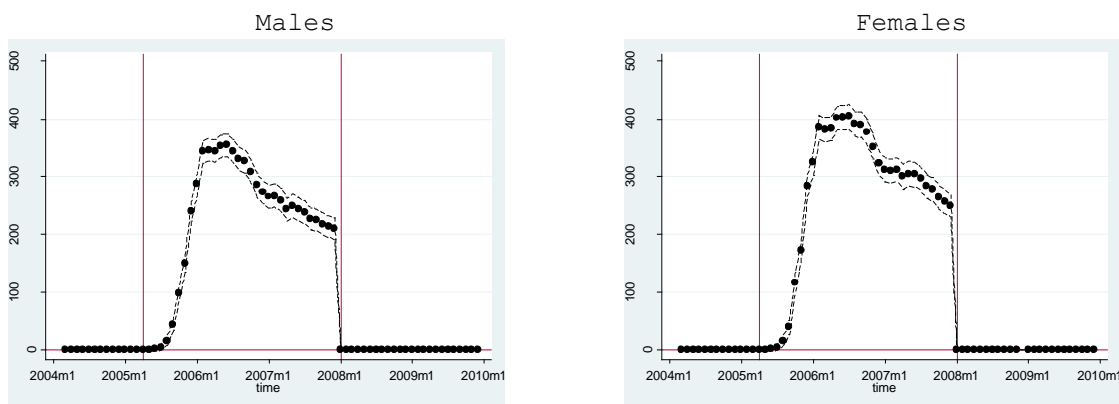
Males

Females

Notes. The figure reports similar coefficients to those in Figure 3, where the dependent variable is now per capita *PANES* transfers (computed dividing total *PANES* transfers by number of individuals aged 14-65 in household and including zeros for those not in the program). See also notes to Figure 3.

Figure 5: RD estimates of eligibility on earnings (including zeros) by month
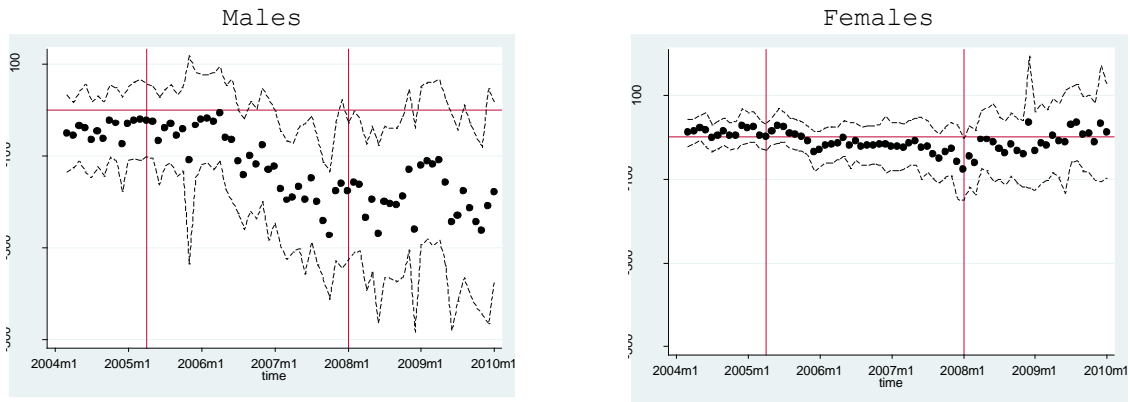
Males

Females

Notes. The figure reports similar coefficients to those in Figure 3, where the dependent variable is now total earnings (including zeros for those out of work). See also notes to Figure 3.

Figure 6: RD estimates of eligibility on fraction employed by month

Males

Females

Notes. The figure reports similar coefficients to those in Figure 3, where the dependent variable is now a dummy for positive earnings. See also notes to Figure 3.

Figure 7: RD estimates of eligibility on days of work (including zeros) by month
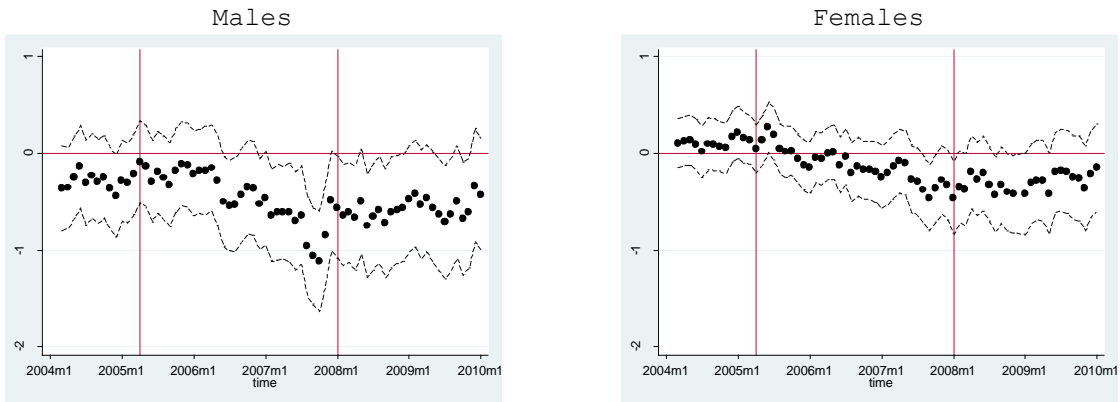
Notes. The figure reports similar coefficients to those in Figure 3, where the dependent variable is now the number of days worked in the month in all occupations (including zeros for those out of work). See also notes to Figure 3.
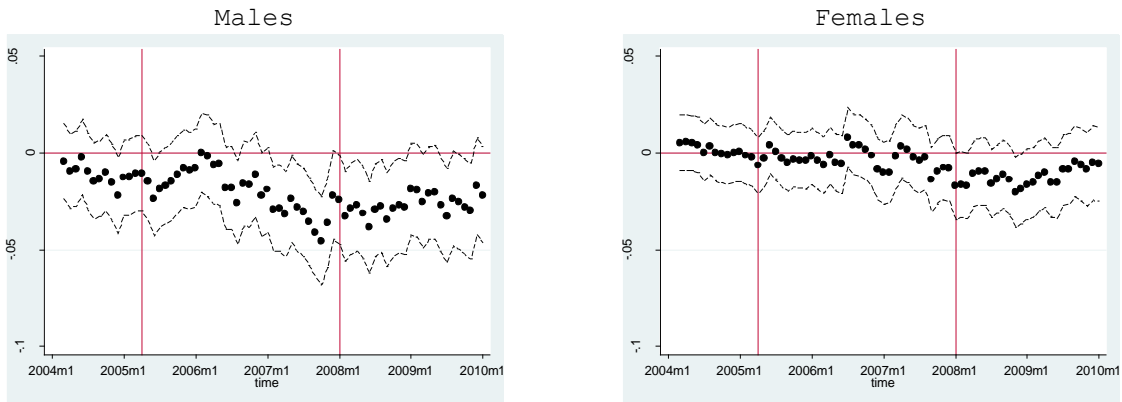
Figure 8: RD estimates of eligibility on daily wages (if in work) by month
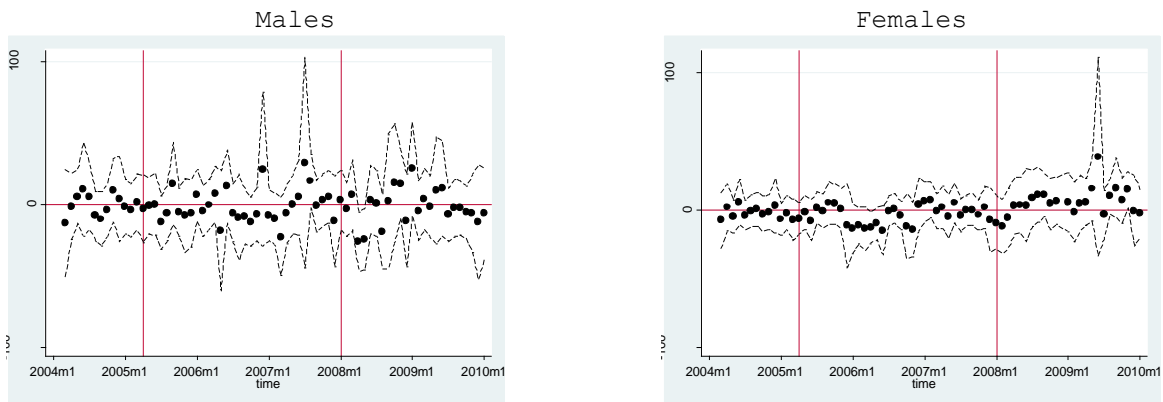

Notes. The figure reports similar coefficients to those in Figure 3, where the dependent variable is now the average daily wages by month in all occupations among those in work. See also notes to Figure 3.

Figure 9: RD estimates of eligibility on earnings (including zeros),
individuals not in employment in pre-*PANES* period - by month
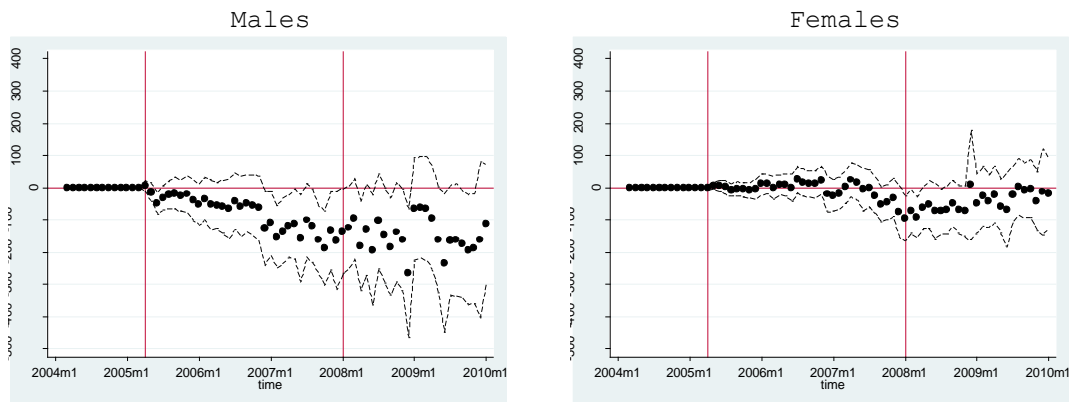
Notes. The figure reports similar coefficients to those in Figure 5, where the sample is
restricted to those with no formal labor earnings at any time between March 2004 and March 2005.
See also notes to Figure 5.

Figure 10: RD estimates of eligibility on earnings (including zeros),
individuals in employment in pre-*PANES* period - by month
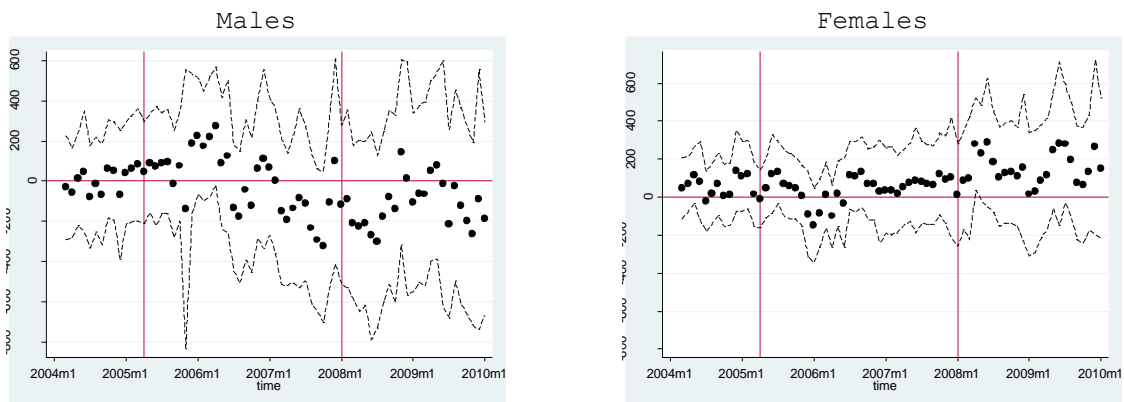
Notes. The figure reports similar coefficients to those in Figure 5, where the sample is
restricted to those with at least one month of positive formal labor earnings at any time between
March 2004 and March 2005. See also notes to Figure 5.

Table 1: Descriptive statistics by eligibility status and period

	Pre-*PANES* period (March 2004 – March 2005)		*PANES* period (April 2005 – December 2007)		Post *PANES* period (January 2008 – December 2009)	
	Eligible	Non-Eligible	Eligible	Non-Eligible	Eligible	Non-Eligible
Males						
Work	0.13	0.20	0.18	0.26	0.26	0.34
Monthly Earnings	404.66	743.47	733.97	1279.76	1425.97	2120.02
Monthly days work	2.49	4.38	3.26	5.62	5.13	7.35
Daily wage	176.66	179.69	241.54	242.1	297.44	305.49
PANES individual	-	-	0.60	0.05	-	-
Monthly *PANES* transfer	-	-	285.31	34.41	-	-
Females						
Work	0.09	0.13	0.10	0.16	0.14	0.23
Monthly Earnings	113.53	280.99	190.05	480.27	481.26	931.65
Monthly days work	1.03	2.34	1.29	3.06	2.54	4.66
Daily wage	113.92	127.97	151.02	166.96	201.68	210.45
PANES Household	-	-	0.62	0.05	-	-
Monthly *PANES* transfer	-	-	374.64	37.63	-	-

Notes. The table reports descriptive statistics on the sample of individuals aged 14-65 in three sub periods and as a function of *PANES* eligibility status. Total number of individual-time observations: 11,138,158 and 14,193,656 respectively for men and women.

Table 2A: RD estimates of eligibility on earnings - by semester - Males

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I-2004	-61.47	-67.18	-80.09	-69.12***	-85.55	-84.43	-8.631	-39.28**
	(38.95)	(51.13)	(63.57)	(26.02)	(59.56)	(80.96)	(104.5)	(17.04)
II-2004	-57.94	-72.96	-97.99	-72.26***	-79.54	-62.83	-20.43	-66.30
	(38.92)	(51.32)	(63.99)	(23.48)	(59.88)	(80.81)	(103.0)	(28.85)**
I-2005	-33.68	-17.71	-34.10	-24.44	-17.31	30.51	104.9	55.76
	(37.82)	(49.54)	(61.56)	(20.21)	(57.63)	(77.06)	(96.95)	(40.58)
II-2005	-70.05	-78.80	-51.05	-53.86**	-26.81	-12.18	44.56	-25.89
	(43.23)	(56.82)	(70.62)	(23.47)	(66.15)	(89.31)	(115.4)	(31.42)
I-2006	-75.70	-116.7*	-93.34	-82.91***	-49.60	-104.6	-129.8	-84.24
	(50.46)	(65.30)	(81.49)	(24.54)	(76.82)	(103.4)	(135.2)	(45.34)
II-2006	-161.4***	-217.5***	-199.9**	-174.90***	-131.6	-216.4*	-284.0*	-196.26***
	(59.30)	(78.12)	(97.82)	(28.81)	(92.36)	(123.5)	(158.7)	(44.31)
I-2007	-224.3***	-280.5***	-224.7**	-210.29***	-198.0**	-195.3	-221.2	-187.79***
	(62.72)	(81.96)	(102.1)	(37.46)	(96.62)	(130.0)	(167.1)	(51.07)
II-2007	-214.8***	-265.8***	-247.1**	-219.18***	-212.3*	-135.2	-103.4	-116.05*
	(69.60)	(91.99)	(115.4)	(40.47)	(109.1)	(147.8)	(191.8)	(70.54)
I-2008	-231.5***	-239.5**	-246.1*	-205.89***	-204.2*	-123.3	-156.6	-172.35**
	(76.61)	(102.2)	(130.0)	(45.77)	(122.4)	(168.5)	(219.9)	(83.09)
II-2008	-222.0**	-203.2*	-250.9*	-201.44***	-203.9	-86.88	-166.6	-134.95
	(86.42)	(116.0)	(148.1)	(50.08)	(139.3)	(194.8)	(257.5)	(79.48)
I-2009	-230.5**	-173.7	-239.6	-194.00***	-182.5	-178.3	-281.2	-187.48***
	(89.82)	(120.2)	(153.6)	(47.04)	(144.3)	(202.2)	(267.4)	(64.15)
II-2009	-329.0***	-317.3**	-363.0**	-316.84***	-343.5**	-329.3	-368.5	-324.56***
	(95.38)	(128.1)	(163.2)	(52.77)	(153.8)	(212.8)	(281.0)	(63.45)
Interval	[-.05,.05]	[-.05,.05]	[-.05,.05]	[-.05,.05]	[-.02,.02]	[-.02,.02]	[-.02,.02]	[-.02,.02]
Function of score	Param. 2	Param. 3	Param. 4	Non-param.	Param. 2	Param. 3	Param. 4	Non-param.

Notes. Entries in the table are coefficients on an eligibility dummy in each semester from regressions where the dependent variable is monthly earnings (with zeros for those out of work). Coefficients refer to men aged 14-65. Regressions in columns 1 and 5, 2 and 6 and 3 and 7 are run on pooled data (from March 2004 to December 2009) and, in addition to semester of the year dummies, they control for two parametric polynomials in the poverty score on each side of the eligibility threshold of degree 2, 3 and 4 respectively. The coefficients on the polynomials are allowed to vary by semester. Entries in columns 4 and 8 are derived from separate regressions of monthly earnings on an eligibility dummy in each semester. Regressions control for a local linear polynomial in the poverty score, using Imbens and Kalyanaram (2009) optimal bandwidth selection method (*rdob* command in Stata). Regressions in columns 1 to 4 and 5 to 8 refer respectively to the range [-.05,.05] and [-.02,.02] around the eligibility threshold. Number of observations: 3,500,972 and 1,360,817 respectively in columns 1 to 4 and 5 to 8. Standard errors in brackets. Standard errors in columns 1 to 3 and 4 to 7 are clustered by individuals (across time). ***, **, *: significant at 1%, 5% and 10% level respectively.

Table 2B: RD estimates of eligibility on earnings - by semester - Females

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I-2004	-27.93*	-1.830	-24.98	-10.06	-21.44	-49.35	-76.33*	-76.63**
	(16.48)	(21.79)	(26.90)	(8.51)	(25.57)	(32.53)	(39.16)	(34.85)
II-2004	-29.67*	4.569	-16.71	-11.17	-11.85	-34.77	-52.21	-39.28**
	(16.55)	(22.15)	(27.56)	(6.67)	(26.23)	(33.55)	(40.41)	(17.03)
I-2005	-15.73	12.66	-6.775	2.75	6.046	-12.77	-39.04	-33.11**
	(15.65)	(20.67)	(25.82)	(7.78)	(24.32)	(31.64)	(40.13)	(13.19)
II-2005	-48.74***	1.834	-15.48	-12.69	-13.25	-30.27	-25.37	-13.97
	(17.84)	(23.80)	(29.72)	(8.62)	(27.98)	(37.02)	(47.55)	(13.45)
I-2006	-65.80***	-5.390	-21.06	-14.63	-0.679	-9.334	5.952	-20.80
	(20.63)	(27.59)	(34.58)	(13.49)	(32.71)	(43.00)	(56.45)	(14.15)
II-2006	-89.48***	-25.49	-28.42	-35.27***	-25.83	-24.78	-28.99	-13.23
	(24.04)	(32.18)	(39.90)	(13.45)	(38.01)	(49.01)	(62.82)	(15.89)
I-2007	-104.5***	-49.76	-53.85	-66.01***	-49.68	-57.08	-44.91	-34.80*
	(27.80)	(37.17)	(46.25)	(12.62)	(44.03)	(57.22)	(71.15)	(17.68)
II-2007	-128.0***	-61.92	-68.06	-75.89***	-58.71	-63.68	-31.66	-58.91***
	(31.67)	(42.81)	(53.68)	(16.51)	(51.08)	(67.09)	(83.61)	(21.06)
I-2008	-125.6***	-36.12	-49.07	-72.09***	-18.14	-89.15	-63.46	-70.68***
	(36.24)	(48.87)	(61.11)	(17.02)	(58.19)	(76.22)	(94.46)	(19.72)
II-2008	-120.3***	-13.13	-39.49	-47.95**	-6.615	-110.7	-100.5	-62.16**
	(41.56)	(55.56)	(68.71)	(21.84)	(65.59)	(85.40)	(105.9)	(31.07)
I-2009	-129.6***	11.82	-0.904	-37.62	31.50	-108.5	-83.76	-69.47**
	(46.09)	(61.70)	(76.92)	(29.90)	(73.30)	(95.13)	(116.7)	(33.61)
II-2009	-100.2**	79.75	72.69	20.90	95.59	-55.07	-118.1	-42.77*
	(48.27)	(64.08)	(79.54)	(30.27)	(75.75)	(98.28)	(121.7)	(31.63)
Interval	[-.05,.05]	[-.05,.05]	[-.05,.05]	[-.05,.05]	[-.02,.02]	[-.02,.02]	[-.02,.02]	[-.02,.02]
Function of score	Param. 2	Param. 3	Param. 4	Non-param.	Param. 2	Param. 3	Param. 4	Non-param.

Notes. Regressions in this table refer to women. Number of observations in columns 1 to 4 and 5 to 8: 4,588,499 and 1,782,160 respectively. See notes to Table 2A.

Table 3A: RD estimates of eligibility on earnings - by semester - Males - With individual fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
II-2004	52.53	60.59	49.62	89.85	126.5	84.52
	(40.54)	(55.78)	(69.52)	(62.54)	(84.85)	(102.6)
I-2005	12.46	-2.218	29.82	82.60	67.47	4.769
	(46.51)	(63.11)	(78.54)	(71.94)	(98.56)	(122.3)
II-2005	-0.0156	-41.18	-12.51	62.61	-23.76	-172.0
	(52.90)	(70.20)	(87.88)	(82.14)	(111.1)	(140.3)
I-2006	-94.43	-147.8*	-130.1	-22.22	-150.9	-354.4**
	(60.47)	(80.68)	(100.9)	(95.54)	(127.9)	(161.6)
II-2006	-178.4***	-220.6**	-168.5	-95.78	-145.1	-303.4*
	(64.83)	(86.23)	(107.9)	(102.9)	(138.2)	(175.2)
I-2007	-170.0**	-204.7**	-203.6*	-106.6	-69.35	-183.9
	(71.77)	(95.77)	(120.3)	(114.7)	(156.3)	(200.9)
II-2007	-188.0**	-178.0*	-205.7	-97.24	-53.60	-235.9
	(79.04)	(106.4)	(135.8)	(128.3)	(178.0)	(232.6)
I-2008	-178.9**	-138.9	-214.5	-94.64	-12.26	-241.9
	(88.24)	(118.9)	(152.2)	(143.9)	(202.9)	(268.1)
II-2008	-183.9**	-100.3	-196.1	-68.13	-95.00	-353.9
	(91.87)	(123.6)	(158.4)	(149.5)	(210.9)	(278.7)
I-2009	-280.6***	-236.0*	-313.7*	-222.6	-230.8	-431.7
	(96.45)	(130.5)	(166.5)	(158.0)	(219.7)	(289.4)
II-2009	-100.2**	79.75	72.69	95.59	-55.07	-118.1
	(48.27)	(64.08)	(79.54)	(75.75)	(98.28)	(121.7)
Interval	[-.05,.05]	[-.05,.05]	[-.05,.05]	[-.02,.02]	[-.02,.02]	[-.02,.02]
Function of score	Param. 2	Param. 3	Param. 4	Param. 2	Param. 3	Param. 4

Notes. Entries in the table are coefficients on similar regressions to those in columns 1 to 3 and 5 to 7 of table 2A, with the addition of age dummies and individual fixed effects. See notes to Table 2A.

Table 3B: RD estimates of eligibility on earnings - by semester - Females - With individual fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
II-2004	7.320	32.41	20.08	30.86	6.453	31.70
	(19.21)	(25.31)	(31.31)	(26.31)	(34.78)	(38.41)
I-2005	-27.57	19.40	8.007	9.273	-14.50	40.45
	(21.23)	(28.02)	(34.95)	(30.39)	(41.16)	(48.58)
II-2005	-45.45*	11.73	0.379	22.68	8.751	78.52
	(23.83)	(31.62)	(39.66)	(35.79)	(47.96)	(57.97)
I-2006	-70.98***	-10.92	-13.99	-6.479	-12.08	37.02
	(26.60)	(35.59)	(44.26)	(40.89)	(53.72)	(65.16)
II-2006	-87.39***	-41.38	-51.27	-39.69	-57.17	11.74
	(30.36)	(40.64)	(50.73)	(47.86)	(62.68)	(74.61)
I-2007	-111.0***	-53.60	-71.50	-55.01	-77.88	12.26
	(34.62)	(46.87)	(58.97)	(56.21)	(73.74)	(88.65)
II-2007	-107.5***	-29.49	-55.64	-15.62	-106.1	-20.76
	(38.62)	(52.03)	(65.26)	(62.42)	(81.61)	(98.14)
I-2008	-102.0**	-7.106	-47.85	-6.179	-127.6	-57.32
	(43.90)	(58.86)	(73.05)	(69.76)	(91.16)	(110.6)
II-2008	-111.7**	15.42	-12.30	29.47	-124.7	-38.27
	(48.10)	(64.72)	(81.15)	(77.42)	(101.3)	(122.6)
I-2009	-82.87*	82.15	58.62	90.17	-74.54	-73.42
	(50.18)	(66.98)	(83.41)	(79.40)	(103.5)	(126.0)
II-2009	-100.2**	79.75	72.69	95.59	-55.07	-118.1
	(48.27)	(64.08)	(79.54)	(75.75)	(98.28)	(121.7)
Interval	[-.05,.05]	[-.05,.05]	[-.05,.05]	[-.02,.02]	[-.02,.02]	[-.02,.02]
Function of score	Param. 2	Param. 3	Param. 4	Param. 2	Param. 3	Param. 4

Notes. See notes to Table 3A.

Figure A1: Timing of *PANES*, program, social security and vital statistics data

| Application

Apr. 05 → | Assessment visit
Apr 05 → | Administrative decision: eligibility | Payment of IC to successful applicants (with arrears to application time)
May 05 → | Program ends

Dec 07 |

Baseline survey
Apr 05 →

Social security data
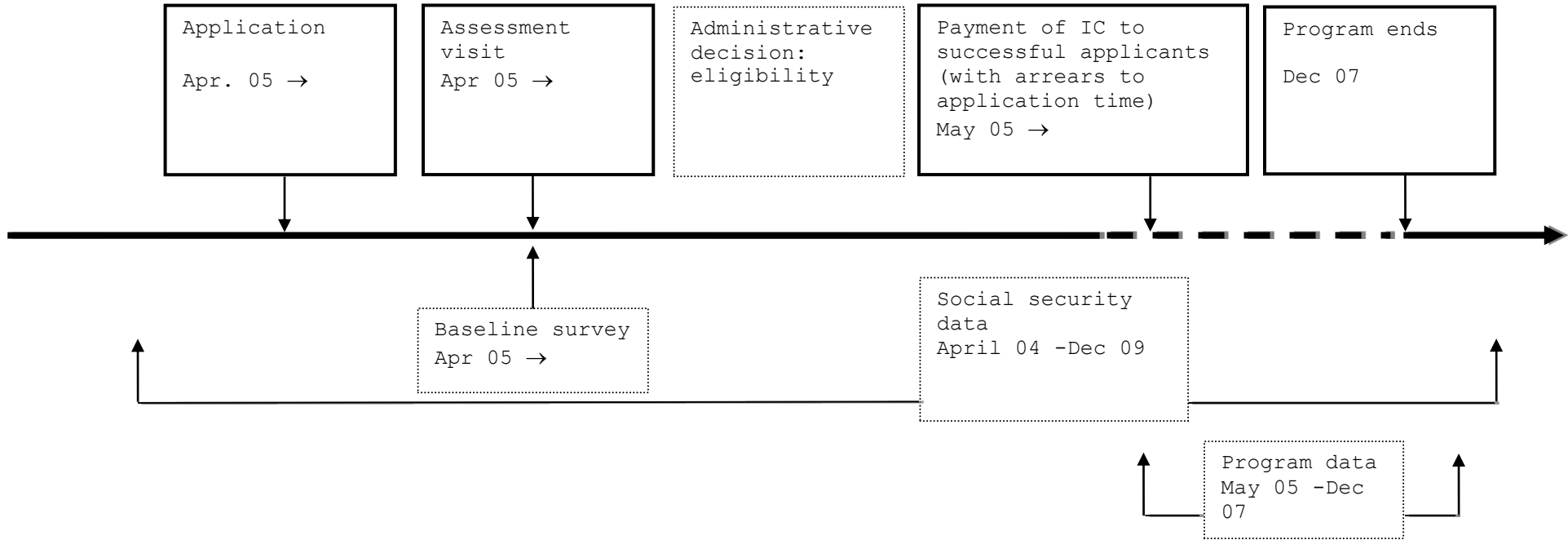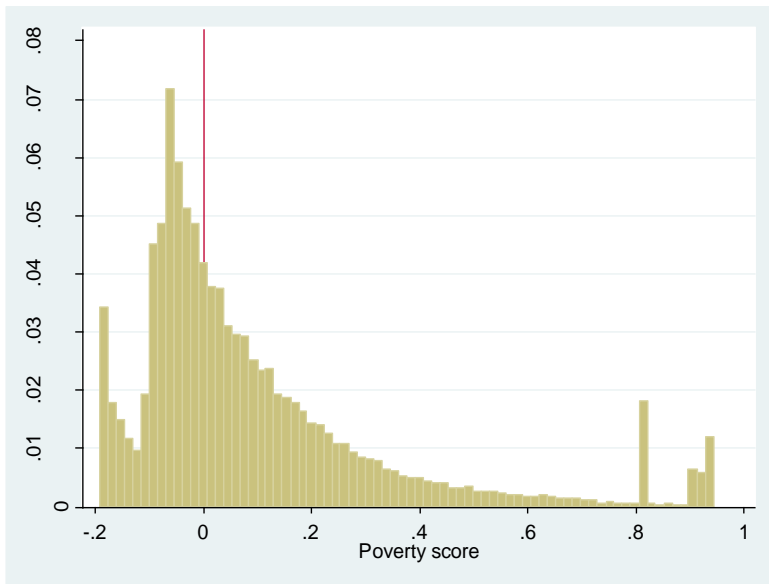April 04 -Dec 09

Program data
May 05 -Dec 07

Figure A2. Distribution of poverty score

Notes. The figure reports the pfd of the *PANES* score.

Table A1: Summary of information in Social security and program administrative records

Benefits/Transfers	Contributory	Characteristics and target population	Time period
Ingreso Ciudadano	No	*PANES* beneficiary population	Apr 2005- Dec. 2007
Unemployment insurance	Yes	Up to 6 months of subsidy for dismissed formal workers	Entire period- Reformed Feb. 2009
Maternity leave	Yes	3 months of paid maternity leave for formal workers	Entire period
Sickness benefits	Yes	Subsidy for formal workers on sick leave for more than three days	Entire period
Child allowances (contributory)	Yes	Allowances for formal workers with children under 18 years old	Entire period
Child allowances (non-contributory)	No	Allowance for low-income families with children under 18 years old	Entire period - reformed in April 2008
Retirement pensions	Yes	Pensions for formal workers aged over 60 with 35 years of formal work (30 years since July 2009), or aged over 70 with 15 years of formal work.	Entire period
Survivors' pensions	Yes	Retirement pensions for family members of a deceased formal worker.	Entire period
Old age pensions	No	Pension for people over 70 years old with low income.	Entire period – reformed in January 2008
Disability pensions	No	Pension for individuals from low income household with a disability that prevents them from working	Entire period
Other non-contributory pensions		Pension granted to individuals who have provided outstanding service to the country or to their immediate family members if they are in a situation of poverty.	Varies depending on the pension
		Pension granted to the victims of imprisonment, exile or dismissal during 1973-1985 or their immediate relatives if deceased.	
Tarjeta Alimentaria	No	Food card for *PANES* beneficiary households with children or pregnant women - extended later to beneficiaries of *Plan de Equidad*	Apr. 2006 -
Trabajo por Uruguay	No	Up to 6 months of workfare (at twice the amount of *Ingreso Ciudadano*) for selected *PANES* beneficiaries (replaced by *Uruguay Trabaja* in Jan. 2008, also covering non-*PANES* households)	Apr. 2005- Dec. 2007 (UT from Jan. 2008)

Table A2: Labor market indicators, Uruguay 2005-2009

	2005	2006	2007	2008	2009
Males					
Activity rate	69.3	71.7	73.6	72.7	73.1
Employment rate	62.7	65.4	68.4	68.5	68.9
Unemployment rate	9.5	8.8	7.1	5.7	5.8
Informality rate	38.5	34.4	33.9	32.3	31.1
Monthly earnings	9,466	9,385	11,993	13,927	15,964
Females					
Activity rate	49.5	51.8	53.7	54.3	55.0
Employment rate	41.9	44.4	47.0	48.8	49.5
Unemployment rate	15.3	14.4	12.6	10.3	10.0
Informality rate	39.1	35.5	35.1	33.7	32.1
Monthly earnings	6,548	6,354	7,964	9,099	10,628

Source: Uruguayan *Encuesta Continua de Hogares*.