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Abstract

We study the impact of welfare payments in Ecuador on the probability that women and men work, and on whether they are employed in the formal or informal sectors. Our analysis is based on two distinct identification strategies and two separate sources of data spanning more than 10 years. We find no evidence that welfare discouraged work. However, among women, welfare payments led to reductions in social security contributions (which are mandated for salaried workers) and payment of VAT and income taxes (which are mandated for the self-employed and firm owners), although the magnitude of these effects is small.

JEL Classification: H55, I38, J22
Keywords: welfare, work, formality

1. Introduction

The effect of welfare programs on work choices is one of the most studied topics in labor economics. A substantial body of evidence using data from the United States and other developed countries has shown that changes in the eligibility for, and in the generosity of, welfare can have large effects on labor supply (Hoynes 1997 and Moffitt 2002 are reviews). The magnitude of these effects, including whether any observed changes occur primarily on the extensive or intensive margins, depends on several characteristics of welfare programs, including their generosity and the implicit marginal tax rate on earnings.

Less is known about the extent to which the findings from the U.S. literature are relevant in poorer settings. This is an important question because many developing countries, especially in Latin America, have introduced sizeable welfare programs in the last two decades. Programs that transfer cash to poor households now have budgets that are roughly half a point of GDP in several countries.[1] Transfers account for as much as one-fifth of the pre-transfer income of the average recipient household (Fiszbein and Schady 2009; Levy and Schady 2013).[2]

Recipients of welfare programs in developing countries are substantially poorer than those in the United States; it is therefore possible that the income effect on labor-leisure choices would be absent (or much weaker) in these settings. Also, many welfare programs in developing countries use a composite measure of household assets (often referred to as a "poverty score"), rather than income, to determine eligibility; because income is not measured directly, it is not clear that welfare payments will reduce employment by placing a high marginal tax rate on earnings. For these reasons, establishing whether welfare programs in developing countries discourage work is an empirical question.

Recent reviews of the short-term effects of cash transfer programs on adult labor supply in developing countries conclude that they have had no short-term effects (or at most very small effects) on the propensity to work and on hours worked, for men and women (Alzúa et al. 2013; Banerjee et al. 2015).[3] This is an important finding for policy-makers. However, this research has left two important questions unanswered: how do welfare payments affect labor supply in the

[1] By way of comparison, Hoynes et al. (2016) estimate that, in the US, the Earned Income Tax Credit, Food Stamps, and cash welfare (TANF) jointly cost about US $100 billion, which is about 0.69 percent of GDP.
[2] We use the terms welfare programs and cash transfer programs interchangeably throughout the paper.
[3] Several papers have shown that cash transfers to the poor reduce child labor, as intended. See Attanasio et al. (2006), Edmonds and Schady (2012), Filmer and Schady (2014), Skoufias and Parker (2001).

medium term (not just the short term)? And do welfare programs affect the choice between formal and informal work?

Many welfare programs, especially in Latin America, included a randomized evaluation during an initial pilot phase. Within a relatively short period, however, households that had originally been assigned to the control group of the evaluation were made eligible for transfers. As a result, evaluations to date have focused on the short-term effects of welfare payments on work.[4] Economic theory going back to the permanent income hypothesis suggests that households will treat permanent increases in income differently from temporary income shocks. Plausibly, welfare recipients could have perceived payments as temporary in the short run, including in the period covered by earlier evaluations, but as a permanent form of income support in the long run, especially for programs that have been in place for 15 years or more (as is the case in Brazil, Colombia, Ecuador, and Mexico). Thus, evaluations that make use only of data one or two years after a program began may not be a good guide to the longer-term effects of welfare.

Labor markets in developing countries present two distinct types of jobs, so-called "formal" and "informal" jobs. Formal workers enjoy the benefits of a social insurance package (pensions, health care, and other services) in exchange for contributions, normally made by the employer and employee. Informal jobs, on the other hand, refer to a variety of salaried and non-salaried jobs that do not comply with social insurance schemes, regulations, and taxes. The informal economy in developing countries is huge, often accounting for half or more of total output and employment (La Porta and Shleifer 2014).[5] The presence of a large informal sector is another reason why the conclusions from the large literature on welfare in the United States may not be transferable to the developing country context.

There are two ways in which receiving welfare could in principle reduce attachment to formal labor institutions. First, welfare transfers could shift some workers from salaried jobs to self-employment (as in Bianchi and Bobba 2013); the self-employed are much more likely to be informal than salaried workers. Second, some salaried workers could shift into businesses that do

[4] Among the data sets that are used by Alzúa et al. (2013) and Banerjee et al. (2015), the periods considered are: Mexico (*Progresa*): 1.5 years (1997-1998); Mexico (*Programa de Apoyo Alimentario*): 2 years (2003-2005); Honduras (*Programa de Asignación Familiar*): up to two years (2000-2002); Nicaragua (*Red de Protección Social*): up to 1.25 years (2000-2001); Morocco (*Tayssir*): 1.5 years (2009 -2010); Philippines (*Pantawid Pamilyang Pilipino Program*): 2.5 years (2009-2011); Indonesia (*Program Keluarga Harapan*): 2 years (2007-2009).

[5] Sixty percent of jobs in low and middle income countries are informal (Pallares-Miralles et al. 2012). In Latin America and the Caribbean, the proportion is 55 percent, and in Ecuador the share of informal workers is around 70 percent (Bosch et al. 2013).

not register them for social security, if this helps them hide income that they believe (correctly or incorrectly) could disqualify them from receiving welfare. Depending on the magnitude of the effects, these choices may have important implications for the coverage of the pension and health systems; on tax revenues and tax evasion; and, possibly, on productivity, because smaller firms, which are more likely to hire workers informally, are substantially less productive than larger firms in developing countries (Busso et al. 2012; 2013; Hsieh and Klenow 2009).

In this paper, we analyze the impact of welfare payments on labor market decisions in Ecuador, a middle-income country. The program we study is large, covering roughly one-quarter of households in the country.[6] We study whether individuals in households that received transfers were less likely to work, and whether welfare payments made it less likely that they worked in the formal sector. Our analysis covers both women, who are the main recipients of welfare, and men. The results we present are based on two entirely separate data sets, and two distinct sources of identification. The data we use span more than a decade, which allows us to carefully analyze both the short- and medium-term effects of welfare on labor market decisions.

We first use data from an evaluation that randomly assigned households to treatment and control groups. Households in the original treatment group were made eligible for transfers almost three years before those in the original control group. The delay in eligibility, and lower take-up rates among households in the control group even after they became eligible, means that there were large differences in total transfers received in the two groups. Identification is straightforward: We use multiple rounds of household survey data to compare the labor market decisions of individuals in the original treatment and control groups of the evaluation at different points in time.

The second part of the analysis makes use of the fact that the cash transfer program we analyze initially had no systematic selection criteria, began using a poverty score to determine eligibility in 2003, and updated this score in 2009 and 2014. These scores were calculated on the basis of "poverty censuses" collected in 2000/02, 2007/08 and 2013/14. We use a regression discontinuity strategy to test whether individuals in households that were just-eligible for welfare were less likely to work than those in just-ineligible households, and less likely to work in the formal sector.

[6] This makes the Ecuador program larger in proportional terms than the better-known *Progresa* and *Bolsa Familia* programs in Mexico and Brazil, which cover 18.5 percent and 20.8 percent of households, respectively. (These are authors' calculations, based on a combination of administrative and household survey data.)

The strength of our identification strategy and the richness of the data we use allow us to make two important contributions to the literature on welfare programs. First, we find strong evidence that individuals in households that received transfers did not work less. This result holds for women, the main recipients of the transfer, and for men. Our results extend earlier work by focusing not only on program effects in the short run, after approximately 2 years, but also in the medium term, 6 years after one group of households started receiving welfare payments and a comparable group did not. Furthermore, we show that welfare did not have a negative effect on work for two different samples of households—those who were relatively poor, and those at the welfare eligibility cutoff.

However, although individuals in households that received welfare did not work less, women (but not men) were less likely to contribute to social security (which is mandated for salaried employees), and less likely to be registered with the tax authorities (which is mandated for the self-employed or firm owners). Using both measures of formality, we present compelling evidence that the proportion of welfare-eligible women in formal work began to decline precisely at the time when eligibility was "switched on". Nevertheless, although these effects are statistically significant, they are small in magnitude: Six years after welfare payments began, women just-eligible for welfare were 0.5 percentage points less likely to contribute to social security and 1.5 percentage point less likely to have a business or self-employment status registered with the tax authorities than just-ineligible women.

The rest of the paper proceeds as follows. In section 2, we discuss how welfare payments could affect labor supply when there are formal and informal sectors, and when the effect of increased earnings on eligibility is uncertain to welfare recipients. In section 3 we briefly describe the context, and discuss the data and identification strategy. Section 4 presents results, and section 5 concludes.

2. Expected effects of welfare on labor supply and formal employment in countries with informal jobs

In the canonical, static labor supply model, the creation of a welfare program reduces labor supply though two well-known effects (Moffitt 2002). First, because leisure is a normal good, welfare reduces work through an income effect; second, labor supply is further reduced via a substitution effect, as welfare imposes an implicit tax on labor income.

In thinking of the likely effects of welfare on work in developing countries one must take account of the fact that, in addition to their choice between work and leisure, individuals make choices between formal and informal employment. Labor income in the informal sector cannot be measured reliably. This is one of the main reasons why welfare programs in developing countries generally use a composite measure of household assets (the poverty score), rather than income, to determine eligibility. In principle, this should reduce or eliminate the implicit tax on labor earnings, and reduce the substitution effect, at least for informal workers.

However, a different kind of substitution effect may arise if potential beneficiaries believe that holding a formal job reduces the likelihood they will be eligible for welfare. In some countries, formal sector workers were explicitly disqualified from receiving welfare payments (as occurred in the Uruguayan welfare program, *Plan de Atención Nacional a la Emergencia Social*, PANES); in others, formal employment was one of the variables used to calculate the poverty score (as in the Colombian poverty census, *Sisben*), making it less likely that a household whose members held formal jobs would be eligible; in yet others, including the well-known PROGRESA program in Mexico and *Bolsa Familia* in Brazil, whether an individual holds a formal or informal job had no bearing on eligibility.[7] Nevertheless, because it was well-known that welfare programs were intended for the poor, and because formal income is more easily observable than informal income, beneficiaries may still have believed that holding a formal job made it less likely that they would receive welfare payments. In all of these cases, and to a varying degree, there is an implicit tax on formal work.

The static model of labor supply can be used to analyze the effects of welfare payments on labor market decisions when there are both formal and informal jobs. Individuals can work in the formal or informal sectors. Formal and informal jobs differ in three dimensions: (i) they have different net wages; (ii) they offer different benefits—in particular, formal (but not informal) employment makes a worker eligible for social security and health insurance; and (iii) only formal income can be monitored directly. Figure 1 shows the budget constraints imposed by supplying labor formally (ABC) or informally (DE) in the absence of welfare payments. The Y-axis plots

[7] The cash transfer program in Mexico has changed names multiple times. We refer to it as PROGRESA because much of the research that has been carried out on this program is based on a randomized roll-out of the program across municipalities in its first 3 years, when the program was called PROGRESA.

income from work plus the monetary valuation that the individual gives to the package of benefits associated with formal employment.

Even in this simple framework, several potential equilibria emerge depending on the initial assumptions about the two types of jobs—in particular, how formal workers value social security and health insurance, and the net wages in the two sectors. Figure 1 illustrates a case in which the benefits package is valued by the amount BC;[8] net formal wages are lower than net informal wages because the cost of the benefits package is (at least in part) passed through in the form of lower wages (which is why DE is steeper than AB).[9] In the case we depict in the figure, formal jobs are preferred in the absence of welfare, and the equilibrium is given by Y_1. However, different net wages across sectors or different valuations of the benefits package yield different initial equilibria depending on preferences.

The impact of welfare on labor supply is illustrated in Figure 2. To make a sharp distinction with work choices in the absence of welfare, we depict a case in which formal employment disqualifies workers from receiving welfare, and the value of the transfer GE is larger than that of the benefits package BC. Because formal workers cannot receive welfare, introducing welfare payments does not change the budget constraint for formal work. On the other hand, the budget constraint for informal work shifts from DE to FGE. The new equilibrium is given by Y_2 or Y_3. In either case, some workers will shift from formal to informal jobs. The effect on total hours worked is ambiguous: hours worked can increase (equilibrium Y_3) or decrease (equilibrium Y_2).

There are other possible scenarios, depending on the value of welfare payments relative to the benefits package, the difference in net wages in the formal and informal sectors, and whether and how individuals believe that formal employment affects their eligibility for welfare. Nevertheless, the general intuition behind the figures is that under some circumstances the introduction of welfare payments can make informal jobs more attractive than before, and may thus result in a shift from formal to informal employment, with an ambiguous effect on hours worked.

[8] We assume that there is a minimum number of hours (EC) required to hold a formal job and be eligible for the benefits package.

[9] Juarez (2008) finds evidence that in Mexico, informal workers are paid in net around 23 percent more than formal workers for similar jobs.

3. Setting and data

A. <u>The Ecuadorean labor market</u>

Ecuador is a middle-income country in South America. In 2015 its GDP per capita (in PPP US dollars) was 11,168, similar to that of neighboring countries like Colombia and Peru, and its population was 16.3 million. Between 2001 and 2012 (the period we study in this paper) GDP in Ecuador grew at an average rate of 4.5 percent per year.

The labor market in Ecuador shares many features with that in other middle-income countries. Seventy percent of men age 15-64, and 40 percent of women, work or are looking for employment. Sixty percent of those who are working are salaried employees. The regulations that workers must comply with depend on whether they are salaried or not. Employers are mandated to register salaried workers with the Ecuadorean Social Security Institute (IESS), and employers and employees jointly make contributions for pensions and health insurance; taken together, these contributions amount to roughly 30 percent of a worker's wage. In practice, only 28 percent of those who are employed (less than 50 percent of salaried workers) contribute to social security, although in Ecuador this figure has gone up substantially over the period we study in this paper.

Self-employed workers and firm owners do not have to make contributions to social security for themselves.[10] However, they must register their firms or their status as self-employed and obtain a tax identification number (*Registro Único de Contribuyentes*, RUC, by its Spanish acronym). With this tax identification number, they must report sales and income to the Internal Revenue Service (*Servicio de Rentas Internas*, SRI, by its Spanish acronym) and pay income and VAT taxes.

B. <u>Welfare in Ecuador</u>

The current welfare system in Ecuador dates back to the late 1990s. In 1999 the country suffered from a severe banking crisis, GDP per capita fell by 32 percent in a single year, and unemployment increased from 9 percent to 17 percent. In this context, the Ecuadorean government created a cash transfer program, the *Bono Solidario* (Solidarity Bond). Payments were made to women. However, because the program did not have clear selection criteria, many recipients were non-poor, and many poor households did not receive transfers.

[10] They can make voluntary contributions but, in practice, less than 1 percent do.

In 2000/02, the government carried out a poverty census which covered about 90 percent of households in rural areas, and about the same fraction of households in selected urban areas that were judged to have a high incidence of poverty. It gathered information on household composition, education levels, work, dwelling characteristics, and access to services. This information was aggregated into a poverty score by principal components. Beginning in March 2003, this poverty score was used to determine eligibility for welfare payments. The name of the program was also changed, from *Bono Solidario* to *Bono de Desarrollo Humano* (Human Development Bond).

In 2007/08, a new poverty census was carried out. The questionnaire used for this census was very similar to that used five years earlier. Once again, the information was aggregated by principal components, and a new poverty score was calculated. The change in the poverty score resulted in considerable reshuffling of households in and out of eligibility.[11] Beginning in August 2009, newly-eligible households (those whose 2000/02 score had been above the cutoff for eligibility, but whose 2007/08 score was below) were brought into the program, while payments were gradually discontinued for newly-ineligible households (those whose 2000/02 score had been below the cutoff for eligibility, but whose 2007/08 score was above). A similar process was followed after a new poverty census was carried out in 2013/14.

Since the creation of the *Bono Solidario*, and continuing with the *Bono de Desarrollo Humano*, transfers have been made to women. In practice, when a household is declared eligible for transfers, one woman is designated as the recipient. This woman is generally the head of the household, or the spouse or partner of the household head. To receive transfers, eligible women must have a valid *cédula*, a national identification number (comparable to the social security number in the United States).[12]

Importantly, all three poverty censuses (2000/02, 2007/08 and 2013/14) asked respondents about work, and whether they were registered with social security. Households knew that their responses to questions on the poverty census would determine their eligibility for payments.

[11] Thirty-six percent of all households in the first poverty census were within five points of the cutoff for eligibility for welfare payments. Among these households, 46 percent of those eligible for welfare by the 2000/02 poverty census became ineligible by the 2007/08 census, and 42 percent of households who were ineligible became eligible.

[12] In the early years of the current welfare system, women would travel to program offices or other approved payment points to collect their transfers. More recently, recipients have been required to open a bank account with their *cédula*, and monthly payments are deposited in the account. Welfare recipients can then withdraw their benefits at the bank or using an ATM card.

Earlier, the eligibility rules for the *Bono Solidario* had established that workers registered with social security would be disqualified from welfare. This provision was widely advertised, although it was not enforced in practice. The response to the question about registration with social security in the poverty censuses was ultimately not used to calculate a household's poverty score. However, respondents did not know which variables would be included in the score, and what weight would be given to each one. Many may have concluded that holding a formal job disqualified them from receiving transfers (or, at least, that it reduced their poverty score).[13]

Welfare payments in Ecuador have grown in magnitude over time. *Bono Solidario* began with a transfer of US $7 dollar per household per month. With the creation of the *Bono de Desarrollo Humano* in 2003, the transfer increased to US $15, was revised upwards in 2009 (to US $35), and again in 2014 (to US $50). Payments have also increased as a proportion of household income—from 13 percent of the pre-transfer income of the poorest 40 percent of the population in 1999 to 20 percent a decade later.

C. Data and identification strategy

We bring together a number of data sets for the analysis we carry out in this paper.

Experimental analysis: One set of estimates is based on data from a panel of households that have been followed since 2003. The baseline survey for this panel was collected between October 2003 and March 2004; follow-up surveys have been carried out in 2005, 2008, and 2011. The original sample included households in urban and rural areas. Since 2005, however, only households in rural areas have been followed. For this reason, our analysis is restricted to households in the rural sample. Also, because our focus is on the labor market behavior of working-age adults, we limited the sample to those who were 18-54 years of age in 2002, shortly before the baseline survey was carried out.

The data for this panel were part of a randomized evaluation of the impact of welfare payments on child health and development (see Paxson and Schady 2010 for a discussion). Random assignment was done at the parish level, with 51 parishes assigned to treatment and 26 assigned to the control group. Within these parishes, a sample of households who were in principle

[13] There is a great deal of anecdotal evidence that this was indeed the case. Most government officials we interviewed for this project indicated that domestic workers working in their own homes asked them not to register them with social security for fear they would lose welfare payments.

eligible for transfers given their 2000/02 poverty score, but had never received payments, was selected. An additional requirement given the focus of the original evaluation on children was that every household in the sample should include a woman who had at least one child under the age of five years, and no older siblings.[14] This woman was also designated as the recipient for welfare payments (with an earlier eligibility date set for those in the treatment than in the control group, as discussed below).

Table 1 summarizes the baseline characteristics of households and individuals, restricting the sample to those for whom data are available in every survey wave (in 2003, 2005, 2008, and 2011). This is 80 percent of all households in the 2003 baseline survey, and is the sample we use for our main estimates. In the Data Appendix we show that the observable characteristics of individuals and households in the treatment and control groups were balanced at baseline; that attrition is uncorrelated with treatment status and that, among attritors, the observable characteristics of treated and control households were similar; and that households in the treatment group received transfers about three years before those in the control group.

The 2005 and 2008, surveys asked the focal woman in the household (the mother of the children that were the focus of the original evaluation) whether she worked in the last week. In addition, in the 2011 survey (but not the others), all household members were asked whether they worked and, if so, how many hours they had worked in the last week and their earnings.

Given random assignment, identification is straightforward. We report the results of intent-to-treat regressions that take the following form:

$$(1)\ Y_{ihpt} = \alpha_c + Z_{ihp}\beta_1 + \mathbf{X}_{ihp}\beta_2 + \varepsilon_{ihpt},\ t{=}1\ldots T$$

where the i, h, and p subscripts refer to individuals, households, and parishes; the t subscript indicates that the regressions are run separately by survey year. Y_{ihpt} is a dummy variable that takes on the value of one if an individual worked at least one hour in the last week, zero otherwise (for the focal women in 2005 and 2008, and for women and men in the 2011 survey), or a variable for

[14] Payments made by the *Bono de Desarrollo Humano* are not conditional on any pre-specified household behaviors. At an early stage, however, program administrators considered making the program conditional on regular health check-ups for households with young children, and on school attendance for households with older children. It was not clear which conditions would apply to households who had both younger and older children. For this reason, the evaluation design required that households in the sample have young children, but not older children.

the number of hours worked or earnings (for the 2011 survey only); α_c is a set of canton fixed effects;[15] Z_{ihp} is a dummy variable for whether the household in question was assigned to the treatment or control groups; $\mathbf{X}_{ihp}$ includes the characteristics in Table 1, which we include to increase precision; and ε_{ihpt} is the error term.

We run regressions by OLS, and cluster standard errors at the parish level. The parameter of interest is β_1, the intent-to-treat estimate of the effect of receiving welfare on work choices at various points in time.

Regression discontinuity (RD) analysis: To generate the data sets for the RD analysis, we used the *cédula* to merge data from six different sources: data from the three poverty censuses, and monthly data on welfare payments, contributions to social security, and registration of self-employment or an own-business with the tax authorities.[16]

The three poverty censuses included questions about work. Therefore, we can test whether individuals who were just-eligible for welfare based on their poverty score constructed from one census reported working less in the next census. Specifically, we constructed two data sets. In one, eligibility is determined by the first poverty census, and data from the second poverty census are used as outcomes. In the other, eligibility is given by the second poverty census, and responses about work in the third census are used as the dependent variable.

To analyze the effects of welfare on formality we merged data from the poverty censuses with administrative data on social security contributions and tax payments. With these data, we can test whether individuals who were just-eligible for welfare were less likely to hold a formal job than those who were just-ineligible. Once again, there are two data sets, corresponding to eligibility by the first and second poverty censuses, respectively.

Because our identification strategy is based on regression discontinuity (RD), we focus on individuals whose poverty score (based on either the first or second poverty census, depending on the analysis) placed them within five points of the cutoff that determined eligibility for transfers. As with the experimental analysis, we limited the sample to those who were 18-54 years of age in 2002, around the time the first poverty census was carried out.

[15] Cantons are administrative units at a higher level than parishes, comparable to municipalities in the United States.
[16] These data are all confidential. The process of merging the various data sets was carried out by staff of the *Bono de Desarrollo Humano* program, IESS and SRI. The data we use have been made anonymous by removing the *cédulas*.

Table 1 summarizes the baseline characteristics of households and individuals in the first and second poverty censuses. In the Data Appendix we show that the baseline characteristics of just-eligible and just-ineligible households were balanced; that there is no evidence of an unusual heaping of mass on either side of the eligibility cutoff; and that the poverty scores calculated from the poverty censuses were in fact used as the criterion for welfare eligibility.

The regressions we run all take the following form:

$$(2)\ Y_{iht} = \alpha_c + S_{ih}\beta_1 + I(S_{ih}<C)\beta_2 + I(S_{ih}<C)\ S_{ih}\beta_3 + \mathbf{X}_{ih}\beta_4 + \varepsilon_{iht},\ t=1\ldots T$$

where Y_{iht} is a variable that takes on the value of one if an individual worked, contributed to social security, or made tax payments (separate regressions); α_c is a set of canton fixed effects; S_{ih} is a parametrization of the running variable, the poverty score; $I(S_{ih}<C)$ is an indicator variable that takes on the value of one for individuals with a poverty score below the cutoff for eligibility; $I(S_{ih}<C)\ S_{ih}$ is an interaction term between the running variable and the eligibility dummy, which allows for the relationship between outcomes and the poverty score to vary for welfare-eligible and -ineligible households; $\mathbf{X}_{ih}$ includes the variables in Table 1; and ε_{iht} is the error term.

We run regressions by OLS. The parameter of interest is β_2, the intent-to-treat effect of welfare payments on work choices. In addition, for the period after 2005, when welfare payment data are available, we report the results from regressions in which receiving payments is instrumented with eligibility.[17] Standard errors are clustered at the parish level throughout.

As in other applications of RD, it is important to ensure that our results are not driven by a particular parametrization of the control function. In our preferred specification, we use local linear regressions (LLRs) and determine the optimal bandwidth using the approach recommended in Imbens and Kalyanaraman (2012).[18] To check for robustness, we also report results that use the full sample of individuals within five points of the eligibility cutoff and control for a polynomial in the control function.

[17] The first-stage coefficient in a regression of a dummy variable for receiving transfers at least once between January 2005 and August 2009 on eligibility by the first poverty census is 0.7 (with a standard error of 0.007); in a comparable regression of a dummy variable for receiving transfers at least once between September 2009 and December 2012 on eligibility by the second poverty census, the coefficient is 0.63 (with a standard error of 0.017).
[18] In every case, the optimal bandwidth is between 2 and 3. We also present results in which the bandwidth is 1.25 or 5.

There are a number of additional considerations for our RD analysis. One is that in the first poverty census, *cédulas* were mainly collected for women. As a result, when we merged the data from the first and second poverty censuses, more than 80 percent of the observations were women. The second poverty census, on the other hand, collected data for many more men. When we merged the data from the second and third poverty censuses, 41 percent of the observations were men. Men for whom *cédulas* were collected in the first census are a highly-selected sample. For this reason, our analysis of the effect of welfare on work for the 2003-09 period is limited to women, while that for the 2009-14 period includes men and women.

A second consideration is that, because of changes of staff at SRI, we were only able to merge the tax data with data from the first, but not the second, poverty census. Therefore, for our analysis of the effect of welfare on formality during the 2009-14 period, we have only one measure of formality (contributions to social security).

Finally, as in other applications of RD, it is important to note that the results we present are local in the sense that they only apply to individuals at the eligibility cutoff. These individuals have higher income than the average welfare recipient in Ecuador, and this could mean that they respond to transfers differently from those who are poorer.

4. Results

A. Do welfare recipients work less?

Our estimates of the effect of welfare payments on work using the experimental data are presented in Table 2. The regressions for every year estimate effects on any work and, separately, work for pay and work without pay for women. In addition, the 2011 regressions estimate program effects on hours worked and total earnings for both women and men.[19]

The results in Table 2 show no evidence that welfare reduced work. In 2005, when treated households had been eligible for welfare payments for 18 months, while control households were ineligible, the intent-to-treat coefficient for women is 0.046 (with a standard error of 0.051). In 2011, when both groups were eligible, but households in the original treatment group had received twice as much in accumulated transfers as those in the original control group (because of the later eligibility date and lower take-up), the coefficient is -0.030 (with a standard error of 0.029) for

[19] In 2011 there are an additional 118 women who are not the focal women. We chose to leave them out. Their inclusion does not alter the results.

women, and 0.002 (with a standard error of 0.010) for men. Also in 2011, we find no difference in hours worked or in log earnings of women or men.

The results for the RD sample are in Figure 3 and Table 3. In every panel in the figure, the vertical axis corresponds to the proportion of individuals who worked, while the horizontal axis corresponds to the poverty score, which has been standardized so the cutoff is equal to zero. The left-hand side graphs refer to the period before the first (second) poverty census was used to determine eligibility (pre-treatment period); the right-hand side graphs refer to the period in which eligibility was determined by the poverty score on the first (second) census. Consistent with our identifying assumption, there were no baseline differences in work between women and men who would become just-eligible or just-ineligible for transfers. More importantly, the right-hand side graphs show no evidence that being eligible for transfers for approximately 6 years had any effect on work for women or men.[20]

Table 3 confirms the findings in the figure. In our preferred specification, the RD coefficients range from -0.003 to 0.003 (with standard errors of around 0.005). The results are not sensitive to how we parametrize the running variable. Our estimates are very precise, allowing us to rule out negative effects of welfare on work of 1 percentage point or larger, relative to counterfactual employment rates of 43-48 percent for women, and 91 percent for men.

In sum, using two completely different sources of data and two distinct identification strategies, we find no evidence that welfare payments reduced work for men or women, both in the short run (using the 2005 survey in the experimental sample) and in the medium run (using later years in the experimental sample and, most clearly, in the RD results for two sample periods).

B. <u>Are welfare recipients more likely to work in the informal sector?</u>

We next turn to an analysis of how welfare affected the choice between formal and informal employment. Figure 4 and Table 4 focus on the effects of welfare eligibility on the social security contributions of women. Once again, as can be seen in the left-hand side graphs, we find no baseline differences in the probability of contributing to social security between women who

[20] A comparison of panels A and B, and panels C and D, shows that there has been a sharp increase in the proportion of women working. About one-quarter of this increase in employment can be accounted for by the fact that women in the panel are aging, and older women in our sample are more likely to work than younger women. Other factors, including secular trends in female employment and the fact that the economy in Ecuador grew robustly over the period, are likely to be the reasons behind most of the change in employment levels.

would become just-eligible or just-ineligible for transfers. The middle panels show that, while they were eligible for welfare, just-eligible women were about 1 percentage point less likely to contribute to social security. The right-hand panel shows that, once the poverty score from the first census was no longer used as the criterion for eligibility, the social security contributions of women who had been just-eligible and just-ineligible for welfare were very similar. These findings are corroborated in Table 4, which shows that the probability of contributing to social security was about 1 percentage point lower among just-eligible women in the period in which they were eligible for welfare (relative to counterfactual rates of 14 to 19 percent) but not before or after.

We carry out the same analysis for the social security contributions of men in Figure 5 and Table 5. We find no evidence that just-eligible men were less likely to contribute to social security than those who were just-ineligible, either before or during the period that the second poverty census was used to determine eligibility.

In Figure 6 and Table 6, we turn to welfare effects on the probability that women registered as business owners or self-employed. The figure indicates that, while they were eligible, just-eligible women were less likely to register with the tax authorities than those who were just-ineligible. Table 6 shows that the magnitude of this effect is between 1.5 and 2 percentage points, relative to a counterfactual rate of 14 percent. Importantly (and unlike the results for contributions to social security), women continued to register at lower rates even after the criterion for eligibility changed.

Figure 7 provides additional evidence on the time pattern of the effects of welfare on formal work. Specifically, we run our preferred intent-to-treat RD regressions of social security contribution or registration with the tax authorities on welfare eligibility, separately by month, and plot the coefficients and confidence intervals on $I(S_{ih}<C)$. The figures also include vertical lines corresponding to the month in which the first and second poverty censuses were first used to determine eligibility.

Panel A shows that, as soon as the first poverty census was used to determine welfare eligibility, women who became just-eligible for transfers began to contribute to social security at lower rates than those who became just-ineligible. This difference in contribution rates increased over time, and gradually decreased with the transition from the first to the second poverty census.

Panel B shows that there was no difference between just-eligible and just-ineligible women in tax registration before the first poverty census; once the score calculated with the first poverty

census was used to determine eligibility, just-eligible women began to register at lower rates than those who were just-ineligible. This difference increased over time, and persisted even after the second poverty score was used to determine eligibility for transfers (although there is a hint of a possible reversal beginning in late 2011).

Panel C shows that, before the second poverty census was used to determine eligibility, there was no difference in social security contributions between women who would become just-eligible or just-ineligible for welfare. Once the transition from the first to the second census began, however, newly-eligible women started to contribute to social security at lower rates, and this difference increased over time. Panel D, finally, shows no effect of welfare eligibility on the social security contributions of men, a result that is consistent with those in Table 5.

We provide some additional results to describe how the declines in formal employment came about in Table 7. For this analysis, we use the third poverty census, which collected richer information on work, including information on type of work, industry, and occupation. Panel A shows that, after approximately 6 years, fewer just-eligible women appear to hold salaried jobs, although this estimate is somewhat sensitive to changes in the specification. Importantly, the table also shows that welfare eligibility did not increase the proportion of women who were self-employed. This makes it unlikely that the main reason for the increase in informality we observe is that the steady stream of welfare income gave women partial protection against risk, and therefore made them more willing to assume the risks inherent in self-employment (as in Bianchi and Bobba 2013).

Panels B and C in Table 7 analyze whether just-eligible women changed industries or occupations as they left formal jobs. For this purpose, we created five groups of industries (occupations), from less to more formal, each comprising 20 percent of employment, and tested whether the proportions of just-eligible and just-ineligible women in these groups were different.[21]

[21] Specifically, we ordered industries (and, separately, occupations) by the share of women who were contributing to social security in each industry (occupation) in December 2012, and created quintiles of industries (occupations), from less to more formal. We then generated 10 indicator variables, each of which takes on the value of one if a woman works in quintiles 1, 2, … 5 of industries, and quintiles 1, 2, … 5 of occupations. There are 1,648 categories for industries in the 2013/14 poverty census data, and 5,536 categories for occupations. As a result, the categories are quite narrow. For example, under industry, two examples are "retail sale of soft drinks" and "washing clothes in private house", and under occupation, two examples are "street vendor of candy or chewing gum" and "owner or administrator of bakery". There are large differences in the proportions formal in the five groups of industries or occupations. Only 2 percent (1.3 percent) of women who worked in the least formal industries (occupations) contributed to social security in December 2012. Comparable values for the most formal industries (occupations) are 42 percent (52 percent). We ran regressions of the indicator variables for industry or occupation quintiles on the explanatory variables in our basic

Table 7 shows that just-eligible women were significantly less likely to work in the most formal industries and occupations: Our preferred RD specification indicates there was a 1.7 percentage point decline in employment in the most formal industries, and a 1 percentage point decline in employment in the most formal occupations. Employment shifts from more to less formal industries, and from more to less formal occupations, explain about 0.2 percentage points (40 percent) and 0.1 percentage points (20 percent), respectively, of the decline in women contributing to social security.[22]

In sum, welfare increased informality both *within* industries and occupations, as women found different jobs (or asked employers in their current job not to pay social security contributions for them), and *across* industries and occupations, as they shifted out of the most formal sectors of the economy.

C. <u>The magnitude of changes in formal employment</u>

Our estimates make clear that women who were just-eligible for welfare were significantly less likely to work formally than those who were just-ineligible. However, it is important to think carefully about the magnitude and economic significance of these results.[23]

First, we note that our RD estimates refer to the marginal woman. This is important because women close to the cutoff are probably those most responsive to taxes on formal labor. Women further away from the cutoff are substantially less formal (in the absence of welfare), as can be seen in Figures 4 and 6, and hence the disincentive effects for them should be weaker.

Second, our estimates refer to the effect of welfare eligibility at the *individual* level, not the *aggregate* level. To see why this distinction matters, consider an extreme scenario in which the number of formal and informal jobs is fixed. In this case, every formal job vacated by a welfare recipient is taken by someone else, and the net aggregate effect of welfare on formality is zero. In practice, such a one-for-one offset is unlikely as there is a good deal of evidence from Latin

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RD specification (2). We report the coefficient and standard error corresponding to the cutoff, $I(S_{ih}<C)$, from each of these 10 regressions in Table 7. If all the increase in informality we observe were a result of women finding more informal jobs *within* the same industries or occupations (or industries and occupations with a similar level of average formality), then the coefficients on $I(S_{ih}<C)$ should be zero. If, on the other hand, the increase in informality were at least in part explained by shifts of women from more to less formal industries or occupations, then we would expect that some of the coefficients on $I(S_{ih}<C)$ would be significant.

[22] For this calculation, we multiply the changes in employment for welfare-eligible women in Table 7 by the share of formal employment in each group of industries or occupations, and rescale by the share of working women in the 2013/14 poverty census (47.6 percent).

[23] We thank an anonymous referee for suggestions that substantially improved this section.

America that many firms hire some employees formally and others informally (Bosch et al. 2013; Levy 2008). Moreover, in the case of tax payments, women who are self-employed can choose whether to pay VAT and income taxes and, if they do not, there is no reason to believe that another worker increases payments. Nevertheless, the general point that the individual effects we estimate may overstate any market-wide effects of welfare on formal employment is important.

In any event, even at the individual level, the effects of welfare payments are modest: For every 100 women who were made eligible for welfare, approximately 70 received transfers in practice, and 1 or 2 of them switched from formal to informal employment. Another way of putting the effect sizes in context is by comparing them with changes in formal employment in Ecuador. Over the period we analyze, the proportion of women in our sample who contributed to social security increased by 7 percentage points; the proportion of women who registered a firm or self-employment status increased even more sharply, by 19 percentage points. Our instrumental variables results indicate that the marginal women who received welfare were 1.3 percentage points less likely to contribute to social security, and 2.4 percentage points less likely to register with the tax authorities. Relative to other factors that were driving formal employment in Ecuador, the distorting effects of welfare appear to be small.

The results we have presented so far have focused on welfare effects on the extensive margin of formal versus informal employment. In part, this is because most women in our sample, 86 percent of just-ineligible women over the 2003-09 period, never made contributions to social security or VAT and income tax payments. The large number of zeros means that RD estimates of welfare effects on total contributions or tax payments are sometimes sensitive to functional form choices. Nevertheless, when we run these regressions, the implied effects are also very small.

On average, just-ineligible women contributed to social security for only 3.8 months over the entire six-year period when the first poverty census determined eligibility. The coefficient on $I(S_{ih}<C)$ in an RD regression of the number of months women contributed to social security (including women who never contributed) is -0.252 (with a standard error of 0.110), indicating that every fourth woman just-eligible for welfare contributed one month less.

Turning to tax payments between 2008 and 2013, the period in which the effects of welfare on tax payments was largest (as seen in Table 6 and Figure 7), mean tax payments among the just-ineligible were US $32.3, or about 50 cents per month. The distribution of payments among those who made tax payments at least once is very right-skewed—once the top 5 percent of the

distribution is trimmed, the average payment of the remaining 95 percent of just-ineligible women is US $6.99 over the six-year period. The coefficient on $I(S_{ih}<C)$ in an RD regression of total tax payments over this period on welfare eligibility is -0.821 (with a standard error of 0.241), indicating that foregone tax payments are roughly US 80 cents per just-eligible woman.[24]

In sum, although welfare payments led to reductions in formal employment of women that are statistically significant, the magnitude of the effects is very small. Plausibly, too, the market-wide effects are smaller than what one would estimate by multiplying the value of the RD coefficient on $I(S_{ih}<C)$ by the number of welfare recipients.

5. Conclusion

The process of economic development involves a number of transitions—from rural to urban, from employment in agriculture to manufacturing and then services, and from informal to formal work. In most countries, this process is also accompanied by an extension of various social services, including health insurance, pensions, and welfare programs. One fundamental question in development economics is whether the policies that governments put in place facilitate or impede the transition from a traditional to a modern economy.

Most workers in developing countries supply their labor informally. They do not have a contract, and they do not contribute to social security. This is the case even in countries where the majority of the population lives in urban areas, and in which there are well-established contributory pension and health care systems. Why is this so, and how much does it matter?

There are competing views of the causes of informality in developing countries. In the canonical work of Lewis (1954) and Harris and Todaro (1970), informality is the unproductive residual sector of a dualistic segmented market. La Porta and Shleifer (2014) suggest that, on its own, economic growth will gradually reduce the size of the informal sector. Looking at one measure of formality, the capacity of a state to collect income and other taxes (which is near-impossible among small, unregistered firms), Besley and Persson (2014) point out that the levels and structure of tax revenues in developing countries today are similar to those of modern high-income countries when they were at a similar level of development.

[24] These estimates refer to the trimmed sample. The LLR coefficient on $I(S_{ih}<C)$ for the full sample, without trimming the top 5 percent, is 22.9, with a standard error of 35.1. However, (and unlike the coefficient for the trimmed sample), this estimate is very sensitive to functional form. The full set of regressions is available from the authors upon request.

Others have stressed the role of government policy. In De Soto (1989), informality is the response of potentially productive entrepreneurs to red tape and ill-conceived government regulations. Maloney (2004) argues that the informal sector in developing countries is partly a response to the high levels of taxation of formal work. Some workers place a low value on social insurance (health care, pensions), relative to the cost in foregone wages, and therefore choose to supply their labor informally. Levy (2008) argues that the combination of large taxes on formal jobs and poorly designed social programs can be an incentive for workers to switch from formal to informal work.[25]

In this paper, we analyze the relationship between the provision of welfare, employment, and informality. Understanding how, if at all, welfare affects the work choices made by beneficiaries, in the short- and medium-run, is a critical concern for policy-makers in developing countries.

Our paper makes two important contributions. First, using two entirely different data sets and two distinct identification strategies, we show that cash transfers made to poor women did not reduce their work effort, or that of adult males in their households. The estimates are very precise: We can rule out negative effects of welfare on work of 1 percentage point or larger (from a counterfactual employment rate of 43 to 48 percent for women, and 91 percent for men). Our findings, which refer to the effect of welfare on work 6 years after one group of individuals became eligible for welfare and a comparable group did not, complement earlier work which focuses on short-term effects (Alzúa et al. 2013; Banerjee et al. 2015). These results are important because it is more likely one would observe negative effects of welfare on work in the medium term (when beneficiaries may see transfers as a permanent form of income support) than in the short term (when they may see transfers as a temporary income shock).

Second, we show that some women who became eligible for welfare switched from formal to informal employment. We find reductions in formal employment both when we measure formality on the basis of contributions to social security (which is mandated for salaried workers), and on the basis of registration with the tax authorities (which is mandated for firm owners and

[25] There is some empirical support for this idea. In Uruguay (Amarante et al. 2011, and Bergolo and Cruces 2016) cash transfers reduced formal earnings of recipients. In Mexico (Bosch and Campos-Vazquez 2014) and Colombia (Camacho et al. 2014), health insurance provided at no cost to workers in the informal sector led to labor reallocations from formal to informal employment. In Argentina, a Universal Child Allowance provided only to workers who were not in formal employment reduced transitions from informal to formal work (Garganta and Gasparini 2015).

the self-employed). The decline in formal employment occurred precisely at the time when women became eligible for welfare and, in the case of tax registration, it continued for years after welfare eligibility changed.

However, the effects we estimate, although significant, are very small in magnitude. For every 100 women who were just-eligible for welfare, 1 or 2 switched from formal to informal employment. Over a six-year period, women who were just-eligible for welfare contributed to social security one-quarter of a month less, and made about US 80 cents lower VAT and income tax payments, than those who were just-ineligible.

We close with a reflection on the possible external validity of the results. Making allowance for all the difficulties inherent in applying coefficients from one setting to others that may be dissimilar in many ways, it seems reasonable to conclude that our estimates of the effects of welfare on formality may be a lower bound for programs that explicitly disqualify formal workers from transfers (like PANES in Uruguay), but an upper bound for programs in which formal employment does not enter into the poverty score and no information is given to suggest that it is a criterion for eligibility (like PROGRESA in Mexico and *Bolsa Familia* in Brazil). In any event, the results from the voluminous literature on welfare in the U.S. and other developed countries is likely to provide poor guidance on the short- and medium-term effects of cash transfers on work choices in developing countries.

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Figure 1: Labor supply with informal jobs

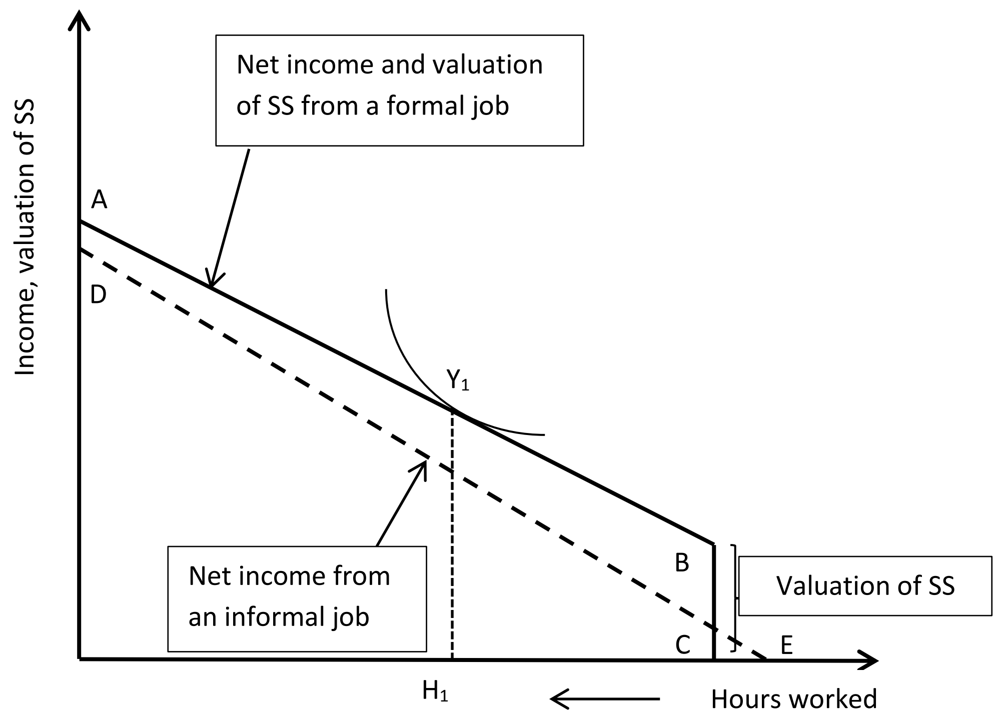


Figure 2: Effects of welfare programs in developing countries with no monitoring

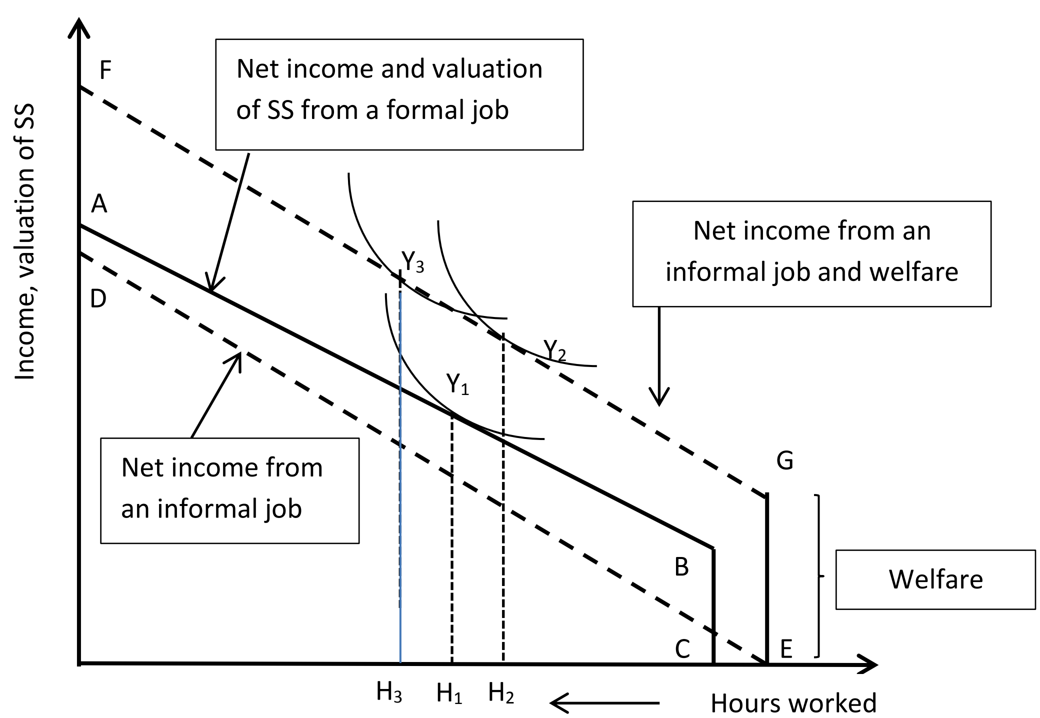
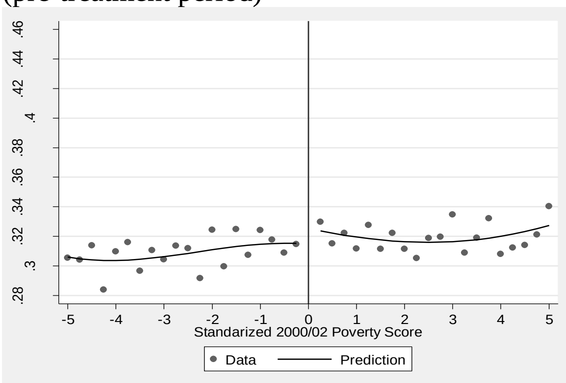
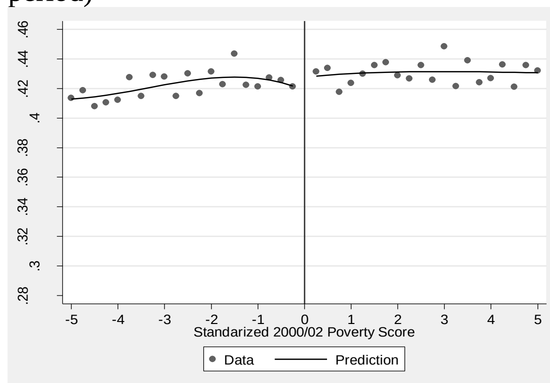


Figure 3: Effect of welfare payments on work, RD sample

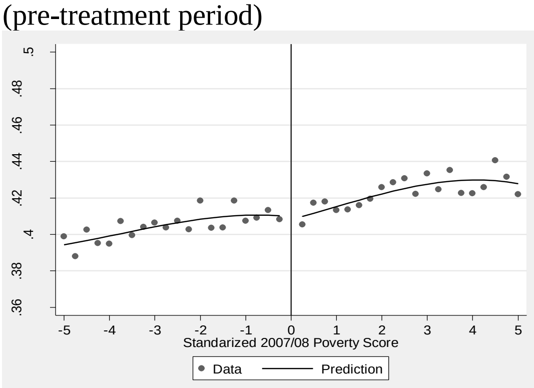
<u>Panel A</u>: Eligibility by 2000/02 census and probability of work, women, 2002 (pre-treatment period)



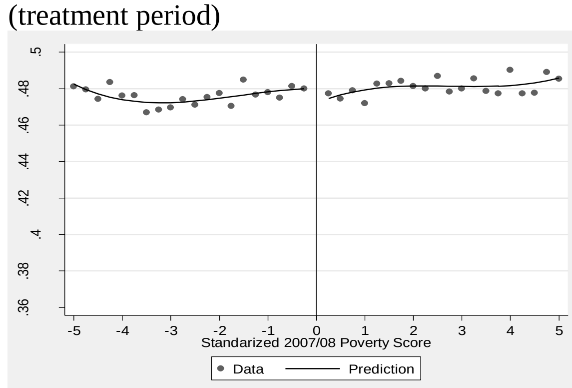
<u>Panel B</u>: Eligibility by 2000/02 census and probability of work, women, 2008 (treatment period)



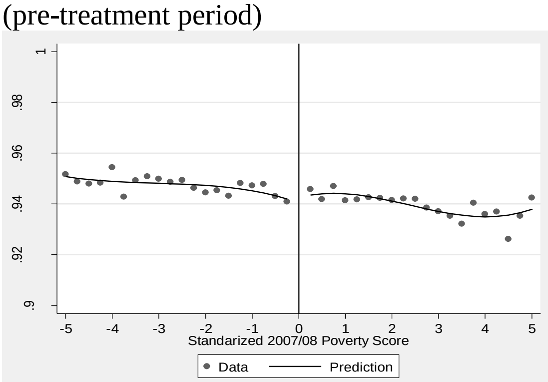
<u>Panel C</u>: Eligibility by 2007/08 census and probability of work, women, 2008 (pre-treatment period)



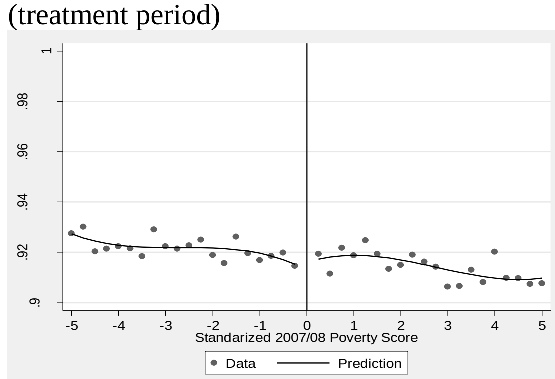
<u>Panel D</u>: Eligibility by 2007/08 census and probability of work, women, 2014 (treatment period)



<u>Panel E</u>: Eligibility by 2007/08 census and probability of work, men, 2008 (pre-treatment period)



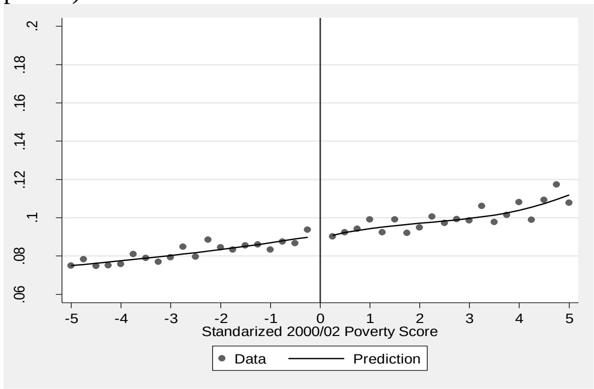
<u>Panel F</u>: Eligibility by 2007/08 census and probability of work, men, 2014 (treatment period)



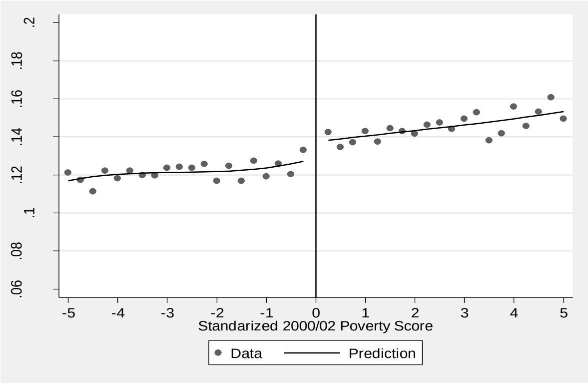
Note: The figure depicts estimates of the effect of welfare eligibility on work. Each panel presents the proportion working at each 0.25 points of the poverty score, and the RD estimation fit using a cubic polynomial, estimated separately on each side of the cutoff, with a bandwidth of 5.

Figure 4: Effect of welfare payments on contributions to social security, women

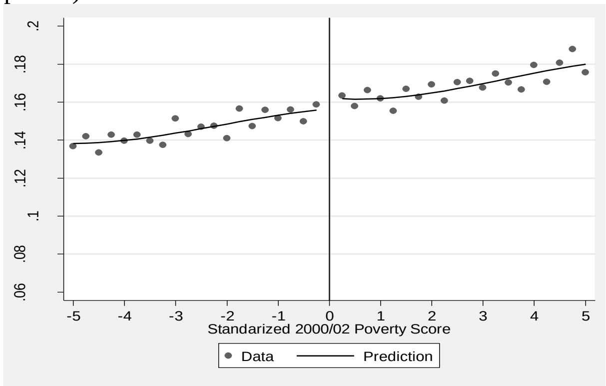
Panel A: Eligibility by 2000/02 census and probability of contributing to social security at least once between January 2000 and December 2002 (pre-treatment period)



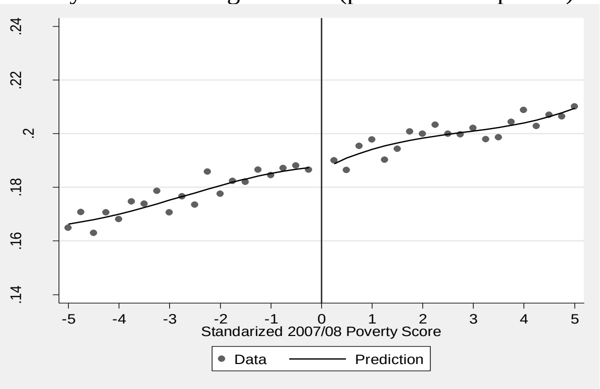
Panel B: Eligibility by 2000/02 census and probability of contributing to social security at least once between January 2003 and August 2009 (treatment period)



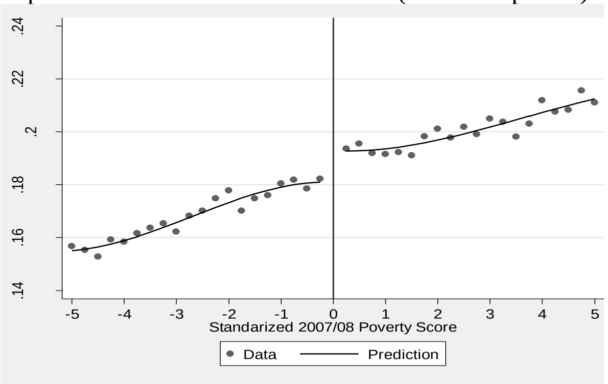
Panel C: Eligibility by 2000/02 census and probability of contributing to social security at least once between September 2009 and December 2012 (post-treatment period)



Panel D: Eligibility by 2007/08 census and probability of contributing to social security at least once between January 2000 and August 2009 (pre-treatment period)



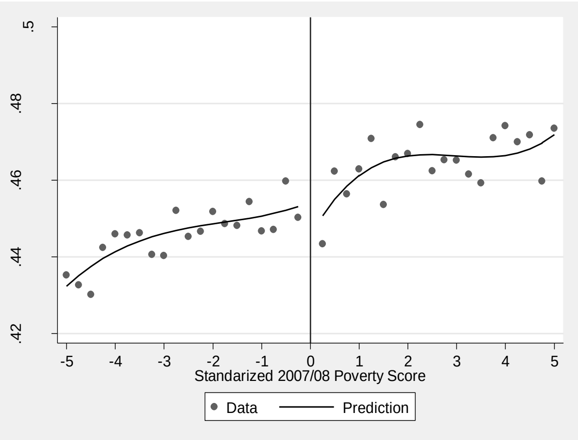
Panel E: Eligibility by 2007/08 census and probability of contributing to social security at least once between September 2009 and December 2012 (treatment period)



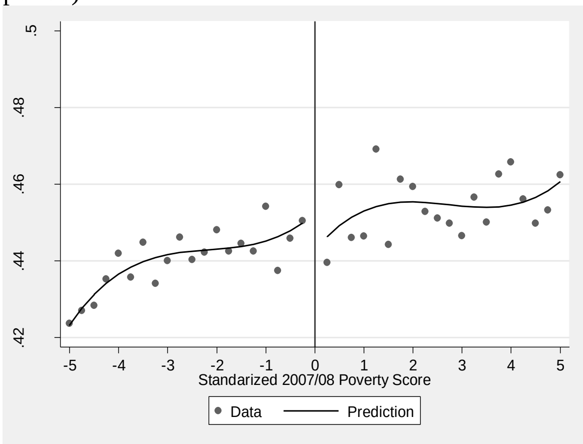
Note: The figure depicts estimates of the effect of welfare eligibility on social security contributions for women. In panels A, B, and C, eligibility is determined by the poverty score calculated from the 2000/02 poverty census, and the periods refer to pre-treatment, treatment, and post-treatment, respectively. In panels D and E, eligibility is determined by the poverty score calculated from the 2007/08 poverty census, and the periods refer to pre-treatment, and treatment, respectively. Each panel presents the proportion contributing to social security at least once over the period at each 0.25 points of the poverty score, and the RD estimation fit using a cubic polynomial, estimated separately on each side of the cutoff, with a bandwidth of 5.

Figure 5: Effect of welfare payments on contributions to social security, men

Panel A: Eligibility by 2007/08 census and probability of contributing to social security at least once between January 2000 and August 2009 (pre-treatment period)



Panel B: Eligibility by 2007/08 census and probability of contributing to social security at least once between September 2009 and December 2012 (treatment period)



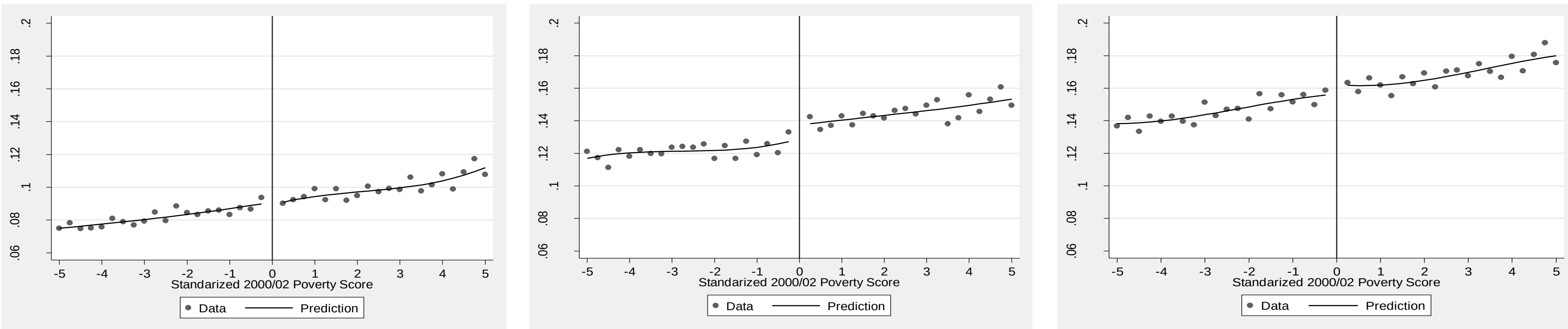
Note: The figure depicts estimates of the effect of welfare eligibility on social security contributions for men. Eligibility is determined by the poverty score calculated from the 2007/08 poverty census, and the periods refer to pre-treatment, and treatment, respectively. Each panel presents the proportion contributing to social security at least once over the period at each 0.25 points of the poverty score, and the RD estimation fit using a cubic polynomial, estimated separately on each side of the cutoff, with a bandwidth of 5.

Figure 6: Effect of welfare payments on registration of own business or self-employment status with tax authorities, women

Panel A: Eligibility by 2000/02 census and probability of paying VAT and income taxes at least once between January 2000-December 2002 (pre-treatment period)

Panel B: Eligibility by 2000/02 census and probability of paying VAT and income taxes at least once between January 2003 and August 2009 (treatment period)

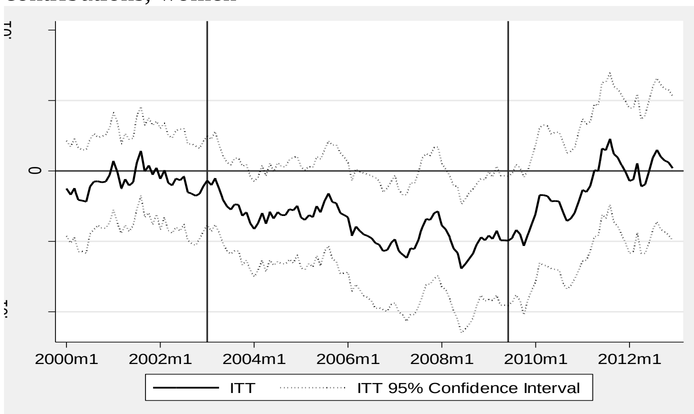
Panel C Eligibility by 2000/02 census and probability of paying VAT and income taxes at least once between September 2009 and December 2012 (post-treatment)



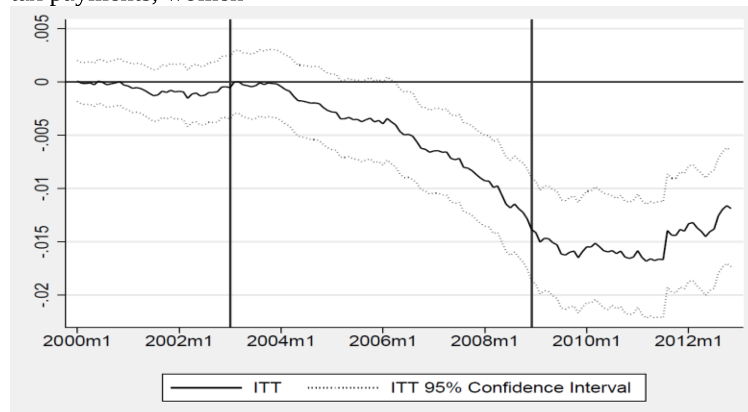
Note: The figure depicts estimates of the effect of welfare eligibility (according to the 2000/02 poverty census) on the probability of registering with the tax authorities during the pre-treatment (Panel A), treatment (Panel B), and post-treatment (Panel C) periods, respectively. Each panel presents the proportion registering with the tax authorities over the period at each 0.25 points of the poverty score, and the RD estimation fit using a cubic polynomial, estimated separately on each side of the cutoff, with a bandwidth of 5.

Figure 7: Time-pattern of welfare effects on formal work

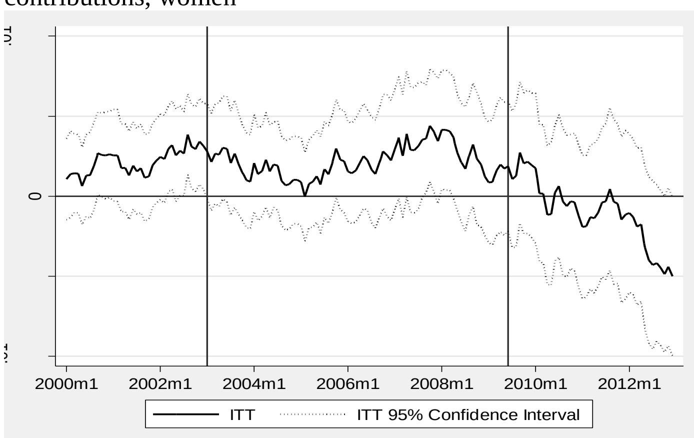
Panel A: Eligibility by 2000/02 census and probability of making social security contributions, women



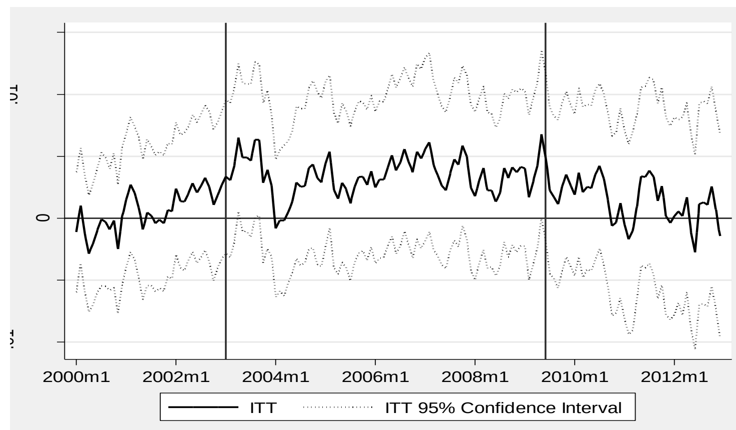
Panel B: Eligibility by 2000/02 census and probability of making VAT and income tax payments, women



Panel C: Eligibility by 2007/08 census and probability of making social security contributions, women



Panel D: Eligibility by 2007/08 census and probability of making social security contributions, men



Note: The figure depicts estimates of the effect of welfare eligibility on formality. All panels depict coefficients and 95 percent confidence intervals on I(S_{ih}<C) in local linear regressions of an indicator variable that takes on the value of one if an individual contributed to social security (panels A, C, and D) or registered with the tax authorities (panel B). Regressions are estimated separately by month (130 regressions per panel). These regressions also include the poverty score, the interaction between the cutoff and the poverty score, the controls in Table 1, and canton fixed effects. Standard errors are clustered at the parish level.

Table 1: Baseline characteristics, different samples

	Experimental sample		RD sample, 2000/02 census		RD sample, 2007/08 census	
	Mean	SD	Mean	SD	Mean	SD
Women's characteristics						
Age in years	25.33	5.70	34.49	10.07	32.83	9.70
% with incomplete primary	0.16	0.37	0.19	0.39	0.17	0.37
% with incomplete secondary	0.62	0.48	0.61	0.49	0.58	0.49
% with complete secondary	0.21	0.41	0.21	0.40	0.25	0.43
% married	0.49	0.50	0.42	0.49	0.32	0.47
% indigenous	0.03	0.12	0.03	0.17	0.03	0.18
Observations (women)	1,500		395,478		500,913	
Men's characteristics						
Age in years	28.03	7.89	35.39	10.23	33.33	9.90
% with incomplete primary	0.16	0.36	0.18	0.38	0.15	0.35
% with incomplete secondary	0.63	0.48	0.64	0.48	0.63	0.48
% with complete secondary	0.21	0.41	0.18	0.38	0.23	0.42
% married	0.24	0.43	0.48	0.50	0.40	0.49
% indigenous	0.01	0.10	0.05	0.21	0.04	0.19
Observations (men)	1,381		124,803		344,573	
Household characteristics						
% living in an urban area	-	-	0.76	0.42	0.74	0.44
% who lives in a house or apartment	0.81	0.39	0.73	0.44	0.55	0.50
% with unfinished floor	0.49	0.50	0.33	0.47	0.22	0.41
% with toilet connected to network	0.51	0.50	0.54	0.50	0.82	0.39
% with exclusive shower	0.29	0.45	0.23	0.42	0.53	0.50
% with gas kitchen	0.86	0.35	0.97	0.18	0.97	0.17
% connected to electricity network	0.94	0.23	0.99	0.09	1.00	0.06
% who owns land	0.23	0.42	0.13	0.34	0.09	0.29
Number of rooms	2.20	1.32	2.12	1.13	2.53	1.24
Observations (Households)	1,500		438,969		547,031	

Note: Table reports mean value of a given characteristic and standard errors for the experimental sample, and the two baseline poverty censuses (2000/02 and 2007/08).

Table 2: Welfare effects on work, experimental sample

		Women			Men
		2005	2008	2011	2011
Dependent variable					
Any work	Coefficient	0.046	0.048	-0.030	0.002
	S.E.	(0.051)	(0.032)	(0.029)	(0.010)
	Mean	0.65	0.67	0.77	0.97
Work for pay	Coefficient	0.058	0.015	-0.030	0.042
	S.E.	(0.033)	(0.033)	(0.034)	(0.026)
	Mean	0.41	0.45	0.35	0.72
Work w/out pay	Coefficient	-0.035	0.068	0.000	-0.040
	S.E.	(0.047)	(0.035)	(0.04)	(0.024)
	Mean	0.32	0.31	0.42	0.25
Hours worked	Coefficient			-0.553	-0.106
	S.E.			(1.358)	(2.036)
	Mean			24	47
Log earnings	Coefficient			-0.011	0.028
	S.E.			(0.171)	(0.082)
	Mean			3.55	5.42

Note: The table reports coefficients and standard errors on indicator variable for random assignment to treatment group. Mean refers to the mean of the control group. All regressions include canton fixed effects and the controls in Table 1. Standard errors are corrected for clustering at the parish level.

Table 3: Welfare effects on work, RD sample, various periods

		(1)	(2)	(3)	(4)	(5)
	Mean, ineligibles	Intent-to-treat estimates				
	Panel A: Eligibility by 2000/02 census, women					
2000/02 census (pre-treatment)	0.33	0.001	-0.003	0.005	0.003	-0.001
		(0.005)	(0.007)	(0.003)	(0.005)	(0.007)
2007/08 census (treatment)	0.43	0.001	-0.004	0.004	0.001	-0.003
		(0.006)	(0.008)	(0.005)	(0.006)	(0.007)
	Panel B: Eligibility by 2007/08 census, women					
2007/08 census (pre-treatment)	0.41	0.002	-0.001	0.003	0.002	0.000
		(0.005)	(0.006)	(0.003)	(0.006)	(0.006)
2013/14 census (treatment)	0.48	0.003	0.003	-0.000	0.005	0.005
		(0.005)	(0.007)	(0.004)	(0.006)	(0.007)
	Panel C: Eligibility by 2007/08 census, men					
2007/08 census (pre-treatment)	0.95	-0.001	-0.005	-0.001	-0.003	-0.001
		(0.003)	(0.004)	(0.002)	(0.003)	(0.004)
2013/14 census (treatment)	0.91	-0.003	0.003	-0.003	-0.002	-0.001
		(0.003)	(0.005)	(0.002)	(0.003)	(0.005)
Bandwidth		Optimal IK	1.25	5	5	5
Polynomials		Linear	Linear	Linear	Quadratic	Cubic

Note: "Mean, ineligibles" refers to the value of ineligibles at the cutoff. Intent-to-treat columns (1) through (5) report coefficients and standard errors on I(S_{ih}<C) in local linear regressions of an indicator variable that takes on the value of one if an individual reported they worked in the census listed in the first column (30 regressions). These regressions also include the poverty score, the interaction between the cutoff and the poverty score, the controls in Table 1, and canton fixed effects. Standard errors clustered at parish level.

Table 4: The effect of welfare on contributions to social security, women

		(1)	(2)	(3)	(4)	(5)	(6)
	Mean, ineligibles	Intent-to-treat estimates					IV
	Panel A: Eligibility by 2000/02 census						
2000-2002	0.09	-0.001	0.002	0.000	-0.001	0.000	
(pre-treatment)		(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	
2003-2009	0.14	-0.010	-0.011	-0.011	-0.012	-0.010	-0.013
(treatment)		(0.003)	(0.004)	(0.002)	(0.003)	(0.004)	(0.004)
2009-2012	0.16	-0.003	-0.008	-0.002	-0.003	-0.006	
(post-treatment)		(0.003)	(0.004)	(0.002)	(0.003)	(0.004)	
	Panel B: Eligibility by 2007/08 census						
2000-2009	0.19	-0.002	-0.004	-0.001	-0.001	-0.001	
(pre-treatment)		(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	
2009-2012	0.19	-0.007	-0.012	-0.005	-0.008	-0.007	-0.011
(treatment)		(0.003)	(0.004)	(0.002)	(0.003)	(0.003)	(0.005)
Bandwidth		Optimal IK	1.25	5	5	5	Optimal IK
Polynomials		Linear	Linear	Linear	Quadratic	Cubic	Linear

Note: "Mean, ineligibles" refers to the value of ineligibles at the cutoff. Intent-to-treat columns (1) through (5) report coefficients and standard errors on $I(S_{ih}<C)$ in local linear regressions of an indicator variable that takes on the value of one if a woman contributed at least once to social security over the period (25 regressions). These regressions also include the poverty score, the interaction between the cutoff and the poverty score, the controls in Table 1, and canton fixed effects. In IV regressions in column (6) welfare receipt is instrumented with welfare eligibility at the household level. Standard errors clustered at parish level

Table 5: The effect of welfare on contributions to social security, men

	Mean, ineligibles	(1)	(2)	(3)	(4)	(5)	(6)
		Intent-to-treat estimates					IV
2000-2009	0.46	-0.000	0.005	-0.003	-0.002	-0.003	--
(pre-treatment)		(0.004)	(0.007)	(0.003)	(0.005)	(0.004)	
2009-2012	0.45	0.003	0.009	0.000	-0.003	-0.003	--
(treatment)		(0.005)	(0.007)	(0.003)	(0.005)	(0.004)	
Bandwidth		Optimal IK	1.25	5	5	5	Optimal IK
Polynomials		Linear	Linear	Linear	Quadratic	Cubic	Linear

Note: "Mean, ineligibles" refers to the value of ineligibles at the cutoff. Intent-to-treat columns (1) through (5) report coefficients and standard errors on I(S_{ih}<C) in local linear regressions of an indicator variable that takes on the value of one if a man contributed at least once to social security over the period (10 regressions). These regressions also include the poverty score, the interaction between the cutoff and the poverty score, the controls in Table 1, and canton fixed effects. Standard errors clustered at parish level.

Table 6: The effect of welfare on registration of own-business or self-employment with tax authorities, women

	Mean, ineligibles	(1)	(2)	(3)	(4)	(5)	(6)
		Intent-to-treat estimates					IV
2000-2002	0.03	0.001	-0.000	-0.002	-0.000	0.002	
(pre-treatment)		(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	
2003-2009	0.14	-0.017	-0.017	-0.021	-0.018	-0.015	-0.024
(treatment)		(0.004)	(0.003)	(0.002)	(0.003)	(0.004)	(0.005)
2009-2012	0.22	-0.018	-0.018	-0.017	-0.016	-0.021	
(post-treatment)		(0.005)	(0.003)	(0.002)	(0.004)	(0.005)	
Bandwidth		Optimal IK	1.25	5	5	5	Optimal IK
Polynomials		Linear	Linear	Linear	Quadratic	Cubic	Linear

Note: "Mean, ineligibles" refers to the value of ineligibles at the cutoff. Intent-to-treat columns (1) through (5) report coefficients and standard errors on $I(S_{ih}<C)$ in local linear regressions of an indicator variable that takes on the value of one if a woman registered as a business owner or as self-employed with the tax authorities (15 regressions). These regressions also include the poverty score, the interaction between the cutoff and the poverty score, the controls in Table 1, and canton fixed effects. In IV regression in column (6) welfare receipt is instrumented with welfare eligibility at the household level. All specifications include controls in Table 1 and canton fixed effects. Standard errors clustered at parish level.

Table 7: Effect of welfare on reallocation across types of jobs, industries and occupations

		(1)	(2)	(3)	(4)	(5)
	Mean, ineligibles	Panel A: Dependent variable: Type of job in 2014				
Salaried Work	0.45	-0.014	-0.004	-0.010	-0.019	-0.004
		(0.007)	(0.012)	(0.005)	(0.008)	(0.010)
Self employed	0.40	0.004	-0.008	0.003	0.006	-0.000
		(0.008)	(0.011)	(0.005)	(0.009)	(0.010)
Domestic Worker	0.09	0.010	0.013	0.005	0.012	0.009
		(0.008)	(0.008)	(0.004)	(0.007)	(0.009)
Unpaid Workers	0.07	-0.002	-0.001	0.002	0.002	-0.004
		(0.004)	(0.005)	(0.002)	(0.004)	(0.005)
	Mean formal, by industry	Panel B: Dependent variable: Employment by industry in 2014				
20% Most informal	0.02	0.004	0.009	0.005	0.007	0.007
		(0.006)	(0.008)	(0.004)	(0.006)	(0.008)
Next 20%	0.07	0.002	-0.002	0.003	-0.001	0.005
		(0.006)	(0.008)	(0.004)	(0.006)	(0.008)
Next 20%	0.13	0.000	-0.003	-0.003	0.002	-0.006
		(0.005)	(0.008)	(0.004)	(0.006)	(0.008)
Next 20%	0.18	0.010	0.019	0.007	0.012	0.015
		(0.007)	(0.008)	(0.005)	(0.007)	(0.009)
20% Most formal	0.42	-0.017	-0.022	-0.012	-0.020	-0.020
		(0.005)	(0.008)	(0.004)	(0.005)	(0.008)
	Mean formal, by occupation	Panel C: Dependent variable: Employment by occupation in 2014				
20% Most informal	0.01	-0.001	-0.006	0.004	-0.002	-0.005
		(0.006)	(0.008)	(0.005)	(0.007)	(0.008)
Next 20%	0.03	0.001	0.004	0.003	0.005	0.004
		(0.006)	(0.008)	(0.004)	(0.006)	(0.008)
Next 20%	0.11	-0.001	-0.003	-0.000	-0.003	-0.003
		(0.005)	(0.008)	(0.004)	(0.006)	(0.008)
Next 20%	0.21	0.015	0.017	0.004	0.010	0.011
		(0.007)	(0.008)	(0.004)	(0.008)	(0.009)
20% Most informal	0.53	-0.010	-0.012	-0.010	-0.009	-0.007
		(0.004)	(0.007)	(0.003)	(0.006)	(0.007)
Bandwidth		Optimal IK	1.25	5	5	5
Polynomials		Linear	Linear	Linear	Quadratic	Cubic

Note: "Mean, ineligibles" is value of ineligibles at the cutoff, and "mean formal" is average proportion of women contributing to social security in December of 2012 in each group of industries or occupations. Values in columns (1) through (5) are coefficients and standard errors on $I(S_{ih}<C)$ in local linear regressions of an indicator variable that takes on the value of one if a woman worked in the type of job, industry or occupation listed in first column (70 regressions). These regressions also include the poverty score, the interaction between the cutoff and the poverty score, the controls in Table 1, and canton fixed effects. Standard errors clustered at parish level.

For Online Publication: Data Appendix

In this appendix we discuss various matters related to the construction of the data sets we use for the analysis we carry out in the paper, focusing in particular on possible threats to identification.

A. Data for experimental analysis:

We constructed a panel data set of women based on surveys carried out in 2003, 2005, 2008, and 2011. This data set includes 1,500 women and 1,381 men between the ages of 18 and 54 at baseline. Because the data were part of a randomized experiment, the observable characteristics of women and men assigned to the treatment and control groups were balanced at baseline. We show this in Table A1. The main threat to identification is attrition.

Overall, attrition was reasonably low. Twenty percent of women who were interviewed at baseline are missing from one or more follow-up surveys. Attrition is uncorrelated with assignment at the beginning of the experiment: In a regression of an indicator variable for attrited women on an indicator for women who were randomly assigned to the treatment group, the coefficient on treatment is -0.015 (with a standard error of 0.031).[26]

We next ran regressions of attrition on the indicator for treatment, a given baseline characteristic (say, age), and the interaction term between the two. We do this separately for all of the characteristics of women and households in Table A1; we cannot do this for men, as the baseline survey did not collect these data on all adult men in the household. The coefficient of interest in these regressions is the interaction term, which indicates whether the baseline characteristics of attritors in the treatment group were different from those of attritors in the control group. The results in Table A2 show that this is not the case. There are 15 regressions, and in only one, corresponding to the indicator variable for women who are elementary school dropouts, is the coefficient on the interaction term significant at conventional levels. We conclude that it is unlikely that attrition is a threat to the internal validity of the results we report in the paper.

We also verified that the random assignment to treatment and control groups determined when households received transfers. Figure A1 shows that over the period we study, there were

[26] As is the case throughout the paper, all the regression results we report in this Data Appendix are based on specifications that include canton fixed effects, with standard errors clustered at the parish level.

large differences in the probability of receiving welfare between households in the treatment and control groups. The proportion of households in the treatment group that received payments rose sharply after June 2004, when they were first made eligible; by March 2005, roughly 50 percent of households in this group were receiving transfers in any given month. The figure also shows that the proportion of households in the control group that received payments increased steadily after March 2007, when they in turn were first made eligible; however, the take-up of welfare increased more slowly in this group, and never fully caught up with the treatment group. It is likely that this occurred because some households in the control group never realized that their eligibility status had in fact changed.[27] In any event, by end-2011, households in the treatment group had received approximately twice as much in transfers as those in the control group (US $1,200, compared to $625).

Figure A1 also shows that, beginning in December 2009, the proportion of households in the evaluation sample that received payments began to decline, and by September 2011 had fallen by roughly 20 percentage points in the treatment group (15 percentage points in the control group, where take-up was lower). This decline is a result of the change in the poverty score from the 2000/2002 to the 2007/08 poverty census. The change in the score meant that a substantial proportion of households in the sample were no longer eligible for payments (because their 2007/08 poverty score placed them above the cutoff for eligibility); no new households became eligible because, by design, the evaluation sample included only households that were eligible to receive payments given their 2000/02 poverty score.

B. Data for RD analysis

To carry out the RD analysis, we constructed four different samples. These include: Sample (1), which we use to estimate the probability that women who were eligible for welfare payments during the 2003-09 period reported they were working in the second poverty census; Sample (2), which we use to estimate the probability that men and women who were eligible for welfare payments during the 2009-14 period reported they were working in the third poverty census;

[27] We do not know why, even among households in the treatment group, take-up rates only reached 60 percent. The literature on program take-up in developing countries has discussed various possible explanations, including stigma, lack of information, and difficulty of access (Coady et al. 2004). We note, however, that the take-up rates among households eligible for welfare in Ecuador appear to be roughly on par with those found elsewhere, as shown in Fiszbein and Schady (2009).

Sample (3), which we use to estimate the probability that women who were eligible for welfare payments during the 2003-09 period made social security contributions or registered with the tax authorities in this period, when the first census was used to determine eligibility, or in the period thereafter, when the second census was used to determine eligibility; and Sample (4), which we use to estimate the probability that men and women who were eligible for welfare payments during the 2009-14 period made social security contributions in this period, when the second census was used to determine eligibility. [28]

Because our identification strategy is based on regression discontinuity (RD), we first limited the sample to households whose poverty score (based on either the first or the second poverty census, depending on the analysis) placed them within five points of the cutoff that determined eligibility for transfers. As with the experimental analysis, we limited the sample to those who were 18-54 years of age in 2002, around the time the first poverty census was carried out.

To construct the data set that is the basis of our analysis of the effects of welfare on formality during the period when the first poverty census was used to determine eligibility (sample (3) above), we began with the 2000/02 data. Within each household, the 2000/02 census registered the *cédula* of the individual (normally a woman) who would potentially be eligible to receive welfare payments. We used the *cédula* to merge the 2000/02 data with monthly administrative data on welfare payments, data on contributions to social security, and data from the tax authorities. The monthly payment data from the program for this sample were only available from 2005 onwards, while the social security and tax data were available beginning in 2000.

An individual must have a valid *cédula* to receive welfare payments, make social security contributions, or register an own business or self-employment status. Therefore, unless there are keying errors in the *cédula* in the first poverty census, all individuals whose 2000/02 data did not merge with the program payment data are individuals who did not receive welfare payments; similarly, all individuals whose 2000/02 data did not merge with the data from the IESS and data from the SRI are individuals who did not make social security contributions or register with the

[28] We excluded men from samples (1) and (3) because there are relatively few *cédulas* for men in the 2000/02 census, and because men who provided *cédulas* in 2000/02 seem to be a selected sample: For example, in an RD regression of work as reported in the first poverty census on *future* eligibility, the coefficient on the variable $I(S_{ih}<C)$ indicates that men who would become just-eligible were around 2 percentage points less likely to work than those who would become just-ineligible.

tax authorities. The final data set we use for this part of the analysis includes 395,478 working-age women in 389,943 households whose 2000/02 score placed them within five points of the eligibility cutoff. Columns (1) of Tables A3 and A4 show that the baseline characteristics of just-eligible and just-ineligible households and individuals in this sample were balanced. Panel A of Figure A2 shows that there is no unusual heaping of mass just above or just below the eligibility cutoff from the first poverty census.

To analyze the effects of welfare on formality when eligibility was determined by the second poverty census (sample (4) above), we proceeded in the same way, but took the 2007/08 census as the starting point. That is, we began with women or men within five points of the cutoff given by their poverty score on the second poverty census, and merged the data for these individuals with the data on welfare payments, and contributions to social security, as before. As discussed in the main body of the paper, we were not able to carry out the merge between welfare eligibility by the 2007/08 census and the tax data. The final data set we use for this part of the analysis includes 502,083 and 344,573 working-age women and men respectively, in 547,031 households whose 2007/08 score placed them within five points of the eligibility cutoff. Columns (3) of Tables A3 and A4 show that the baseline characteristics of just-eligible and just-ineligible households and individuals in this sample were balanced. Panel B of Figure A2 shows that there is no unusual heaping of mass just above or just below the eligibility cutoff from the second poverty census.

Our analysis of the impact of welfare payments on work (samples (1) and (2) above) is based on comparable questions asked in the 2000/02, 2007/08, and 2013/14 poverty censuses. As a first step, we used the *cédula* to merge the 2000/02 and 2007/08 data. We were able to merge the data for 302,047 women in the 2000/02 census, for a merge rate of 76 percent. This seems reasonable as there are many reasons why a woman could have been interviewed in one poverty census but not the other.[29]

[29] Migration rates, both within the country and to and from overseas (especially Spain) are high in Ecuador. Women who migrated overseas between the application of the first and second poverty censuses would not have been covered. Also, as we discuss below, the second poverty census was applied on a voluntary basis in some parts of the country and many women may not have understood the process, may have missed the dates in which they could have the eligibility questionnaire applied to them, or may have decided that their circumstances had changed in ways that would make it unlikely that they would be eligible for transfers. A small proportion of women may also have died between the application of the first and second poverty censuses.

The 2007/08 census was not collected in the same way in all of Ecuador. In some parts of the country, enumerators carried out door-to-door visits of all households living in a locality; in other areas, however, households were told that they should come to be surveyed at a pre-established location and time. In areas in which the 2007/08 census was applied on a voluntary basis, rather than through door-to-door visits, current welfare recipients were more likely to be surveyed than non-recipients. This is not surprising as current recipients would have been better informed about the process they needed to follow to continue to be eligible for transfers. However, the fact that welfare-eligible women (by the 2000/02 poverty census) were more likely to be surveyed in the 2007/08 census than those who were ineligible poses an estimation challenge because, as can be seen in Panel C of Figure A2, it results in a small heaping of mass in the merged data just below the 2000/02 eligibility cutoff. In this sample (but not the others), we fail to pass the McCrary density test (a coefficient of -0.064, with a standard error of 0.012), potentially invalidating our RD identification strategy.

To address this, we limited the sample to households in areas where the 2007/08 census was carried out with door-to-door visits. The results, in Panel D of Figure A2, show no piling of mass on either side of the cutoff, and we now comfortably pass the McCrary test. For this reason, our final estimation sample for the analysis of the impact of welfare payments on work includes 210,394 working-age women in 207,376 households who were interviewed in the first and second poverty censuses, are within 5 points of the 2000/02 cutoff, and lived in localities in which the 2007/08 census was carried out with door-to-door visits, rather than on a voluntary basis. Columns (2) of Tables A3 and A4 show that the baseline characteristics of just-eligible and just-ineligible households and individuals in this sample were balanced.

Using a similar procedure, we used the *cédula* to merge the 2007/08 and 2013/14 poverty censuses. The 2007/08 poverty census was carried out with door-to-door visits in 99 percent of cases, and we do not find a piling of mass on either side of the cutoff, as can be seen in Panel E of Figure A2. On the other hand, attrition in this sample is somewhat higher (around 40 percent). The final data set for this part of the analysis includes 297,914 and 196,793 working-age women and men, respectively, in 314,143 households whose 2007/08 score placed them within five points of the eligibility cutoff. Columns (4) of Tables A3 and A4 show that the baseline characteristics of just-eligible and just-ineligible households and individuals in this sample were balanced.

Finally, we verified that the poverty scores from the censuses were used to determine which households received welfare. Figure A3 has eight panels, two for each estimation sample. In the upper panels, the lines correspond to the proportion of eligible and ineligible women receiving transfers in each month; in the lower panels, the lines correspond to the RD coefficients from regressions of welfare eligibility on receiving transfers (where each point on the graph is the coefficient from a different regression, using data for that month only), based on our preferred LLR specification. All the panels in Figure 4 show that the poverty score had a substantial effect on the probability that a household received welfare payments. For example, in 2009, while the first poverty census was used to determine eligibility, 75 percent of eligible households, and no ineligible households, received transfers. In 2012, once the transition from the first to the second poverty census had been completed, 80 percent of households eligible to receive transfers by the second poverty census, but less than 5 percent of those who were ineligible, received transfers.[30]

References

Coady, David, Margaret Grosh, and John Hoddinott. 2004. *Targeting of Transfers in Developing Countries: Review of Lessons and Experience*. World Bank: Washington, D.C.

McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics* 142(2): 698-714.

[30] An RD regression of total transfers received between 2005 and 2009 on welfare eligibility, as determined by the first poverty census, indicates that just-eligible households received US $863 more in transfers than those who were just-ineligible; the comparable figure for the 2009-12 period, when the second poverty census gradually became the basis for eligibility, indicates that just-eligible households (by the second poverty census) received US $164 more in welfare payments than those who were just-ineligible.

Table A1: Baseline characteristics, experimental sample

	Treatment		Control		p-value
	Mean	**SD**	**Mean**	**SD**	
Women's characteristics					
Age in years	25.46	5.85	25.08	5.40	0.28
Proportion with incomplete primary	0.17	0.37	0.15	0.36	0.66
Proportion with incomplete secondary	0.61	0.49	0.65	0.48	0.36
Proportion with complete secondary	0.22	0.41	0.20	0.40	0.74
Proportion married	0.47	0.50	0.54	0.50	0.35
Proportion indigenous	0.02	0.13	0.01	0.10	0.39
Men's characteristics					
Age in years	28.23	7.89	27.58	7.84	0.17
Proportion with incomplete primary	0.16	0.37	0.14	0.35	0.54
Proportion with incomplete secondary	0.63	0.48	0.63	0.48	0.93
Proportion with complete secondary	0.20	0.40	0.23	0.42	0.59
Proportion married	0.24	0.43	0.26	0.44	0.60
Proportion indigenous	0.00	0.07	0.01	0.10	0.44
Household characteristics					
Proportion who lives in a house or apartment	0.80	0.40	0.82	0.38	0.57
Proportion with unfinished floor	0.50	0.50	0.46	0.50	0.60
Proportion with toilet connected to network	0.49	0.50	0.54	0.50	0.62
Proportion with exclusive shower	0.28	0.45	0.31	0.46	0.70
Proportion with gas kitchen	0.85	0.36	0.87	0.33	0.67
Proportion connected to electricity network	0.94	0.23	0.95	0.22	0.88
Proportion who owns land	0.24	0.43	0.22	0.41	0.56
Number of rooms	2.24	1.37	2.13	1.22	0.25
Observations (Households)	1009		491		

Note: Table reports mean and standard deviation of each variable at baseline for treatment and controls groups at baseline, and p-value of difference of means, adjusted for clustering at the parish level.

Table A2: Attrition in the experimental sample

Dependent variable = 1 if attrited in w1, w2 or w3	**Treatment**		**X**		**Treatment X**	
	Coeff	**SE**	**Coeff**	**SE**	**Coeff**	**SE**
Women's characteristics						
Age in years	-0.065	(0.083)	-0.001	(0.003)	0.002	(0.003)
Incomplete primary	0.008	(0.033)	0.121	(0.039)	-0.121	(0.049)
Completed primary	-0.056	(0.046)	-0.048	(0.039)	0.067	(0.045)
Completed secondary	-0.016	(0.030)	-0.042	(0.030)	0.014	(0.043)
Married	-0.047	(0.033)	-0.059	(0.034)	0.064	(0.041)
Indigenous	-0.014	(0.031)	0.134	(0.189)	-0.117	(0.205)
Household characteristics						
Lives in a house or apartment	-0.008	(0.055)	0.006	(0.045)	-0.010	(0.053)
Has unfinished floor	0.003	(0.039)	0.037	(0.026)	-0.040	(0.036)
Has toilet connected to network	-0.020	(0.027)	0.009	(0.038)	0.008	(0.045)
Has exclusive shower	-0.007	(0.027)	0.023	(0.040)	-0.028	(0.047)
Has gas kitchen	-0.024	(0.052)	-0.046	(0.049)	0.010	(0.059)
Connected to electricity network	0.009	(0.082)	0.013	(0.080)	-0.026	(0.088)
Owns land	-0.004	(0.030)	0.028	(0.043)	-0.054	(0.049)
Number of rooms	0.013	(0.042)	0.012	(0.012)	-0.014	(0.014)

Note: Table reports results of regressions of indicator variable for attrited observations on indicator variable for assignment to the treatment or control groups, the characteristic in the first column, and the interaction between assignment to treatment and the characteristic in question. All regressions include canton fixed effects. Standard errors clustered at the parish level..

Table A3: Descriptive statistics, RD samples: Households

Merge	2000/02 census- IESS		2000/02 census-2007/08 census		2007/08 census- IESS		2007/08 census-20013/14 census	
	Mean Ineligibles	RD	Mean Ineligibles	RD	Mean Ineligibles	RD	Mean Ineligibles	RD
		(1)		(2)		(3)		(4)
Household characteristics								
Lives in an urban area	0.77	0.008	0.84	0.007	0.74	0.002	0.72	0.006
		(0.003)		(0.004)		(0.002)		(0.003)
Lives in a house or apartment	0.74	-0.004	0.77	-0.009	0.55	0.005	0.59	0.003
		(0.003)		(0.005)		(0.004)		(0.005)
Has unfinished floor	0.30	0.004	0.28	0.004	0.21	-0.004	0.20	-0.005
		(0.004)		(0.007)		(0.004)		(0.004)
Has toilet connected to network	0.58	-0.004	0.54	-0.007	0.83	0.001	0.80	0.004
		(0.004)		(0.007)		(0.004)		(0.004)
Has exclusive shower	0.24	0.003	0.21	0.003	0.53	0.003	0.56	0.001
		(0.003)		(0.003)		(0.005)		(0.006)
Has gas kitchen	0.97	-0.001	0.98	0.003	0.97	0.001	0.97	-0.000
		(0.001)		(0.002)		(0.002)		(0.002)
Connected to electricity network	0.99	-0.000	1.00	-0.002	1.00	-0.001	1.00	-0.001
		(0.001)		(0.001)		(0.000)		(0.001)
Owns land	0.13	-0.002	0.11	-0.003	0.09	0.000	0.11	-0.000
		(0.002)		(0.003)		(0.002)		(0.003)
Number of rooms	2.10	0.000	2.10	0.010	2.52	0.002	2.63	-0.033
		(0.009)		(0.013)		(0.009)		(0.014)

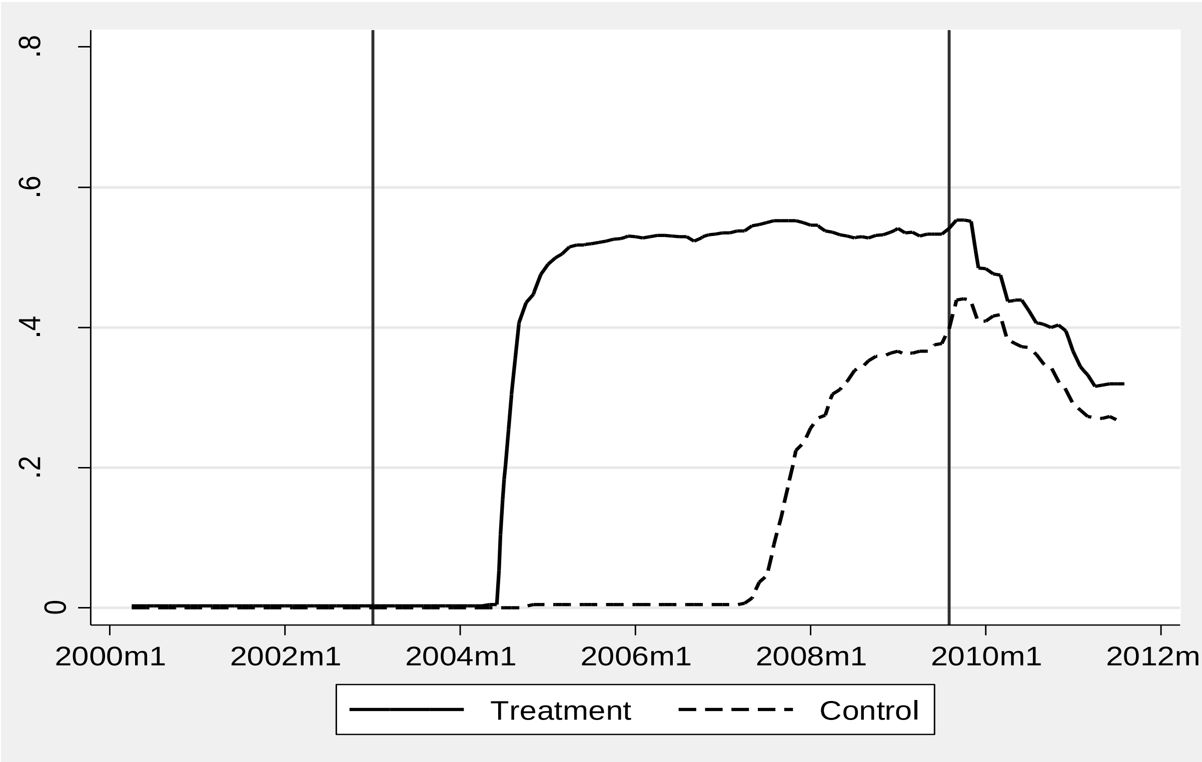
Note: Table reports the mean value of a given characteristic for ineligible individuals for the four samples used in the analysis, and the RD coefficients and confidence intervals on the variable $I(S_{ih}<C)$ from local linear regressions of being eligible for welfare payments on the poverty score, the eligibility cutoff, the interaction between the cutoff and the poverty score, and canton fixed effects. Standard errors are clustered at the parish level.

Table A4: Descriptive statistics, RD Samples: Individuals

Merge	2000/02 census- IESS		2000/02 census census-2007/08		2007/08 census- IESS		2007/08 census census-2013/14	
	Mean Ineligibles	RD	Mean Ineligibles	RD	Mean Ineligibles	RD	Mean Ineligibles	RD
		(1)		(2)		(3)		(4)
Female Characteristics								
Age in years	34.9	-0.240	35.3	-0.184	32.8	-0.024	33.5	-0.017
		(0.122)		(0.136)		(0.067)		(0.087)
Married	0.42	-0.000	0.41	0.009	0.31	0.004	0.33	-0.000
		(0.001)		(0.006)		(0.004)		(0.005)
Indigenous	0.03	0.006	0.02	0.001	0.03	-0.001	0.03	-0.001
		(0.004)		(0.001)		(0.001)		(0.001)
Incomplete primary	0.18	0.002	0.18	0.004	0.16	-0.001	0.17	-0.002
		(0.004)		(0.005)		(0.002)		(0.003)
Completed primary	0.60	-0.001	0.61	0.006	0.58	0.002	0.59	0.002
		(0.006)		(0.006)		(0.004)		(0.004)
Completed secondary	0.22	-0.002	0.21	-0.007	0.26	-0.002	0.25	0.001
		(0.004)		(0.004)		(0.003)		(0.004)
Male Characteristics								
Age in years	35.5	-0.027	36.2	-0.109	33.4	-0.127	34.1	-0.109
		(0.113)		(0.207)		(0.082)		(0.095)
Married	0.49	-0.008	0.50	-0.010	0.40	0.003	0.41	0.001
		(0.008)		(0.011)		(0.004)		(0.005)
Indigenous	0.05	-0.002	0.05	-0.003	0.04	-0.001	0.03	0.002
		(0.003)		(0.003)		(0.002)		(0.002)
Incomplete primary	0.18	0.008	0.18	0.015	0.14	0.001	0.15	0.007
		(0.006)		(0.011)		(0.005)		(0.006)
Completed primary	0.65	-0.006	0.65	-0.001	0.63	0.006	0.63	0.007
		(0.007)		(0.010)		(0.004)		(0.006)
Completed secondary	0.18	-0.001	0.17	-0.013	0.23	0.000	0.23	-0.009
		(0.005)		(0.009)		(0.004)		(0.006)

Note: Table reports the mean value of a given characteristic for ineligible individuals for the four samples used in the analysis, and the RD coefficients and confidence intervals on the variable $I(S_{ih}<C)$ from local linear regressions of being eligible for welfare payments on the poverty score, the eligibility cutoff, the interaction between the cutoff and the poverty score, and canton fixed effects. Standard errors are clustered at the parish level.

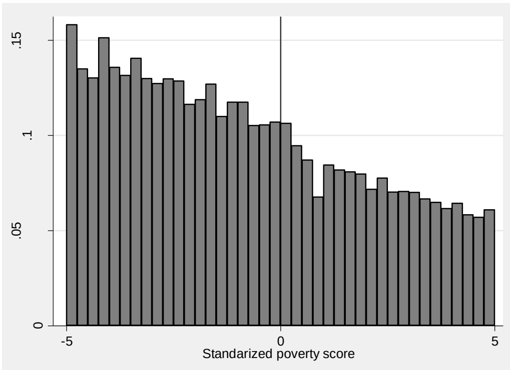
Figure A1: Assignment to experimental treatment and control groups and welfare payments



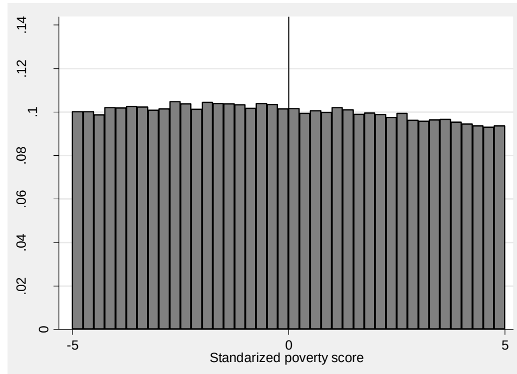
Note: Figure shows proportion of households in the treatment and control groups who received welfare payments, by month.

Figure A2: Density tests, RD samples

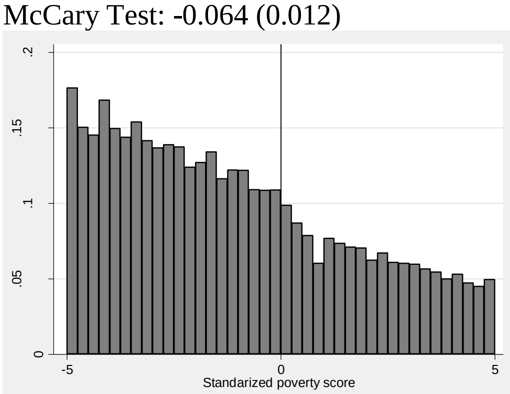
Panel A: 2000/02 sample: 389,943 households
McCary Test: 0.003 (0.009)



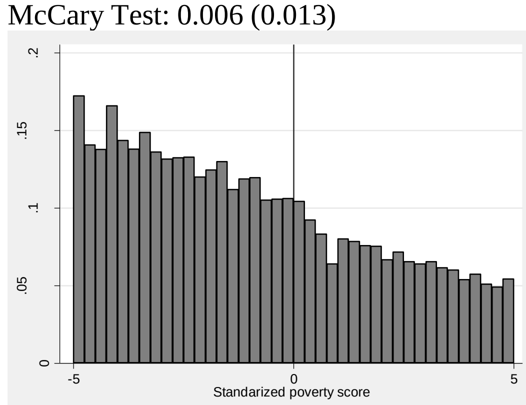
Panel B: 2007/08 sample: 547,031 households
McCary Test: -0.008 (0.009)



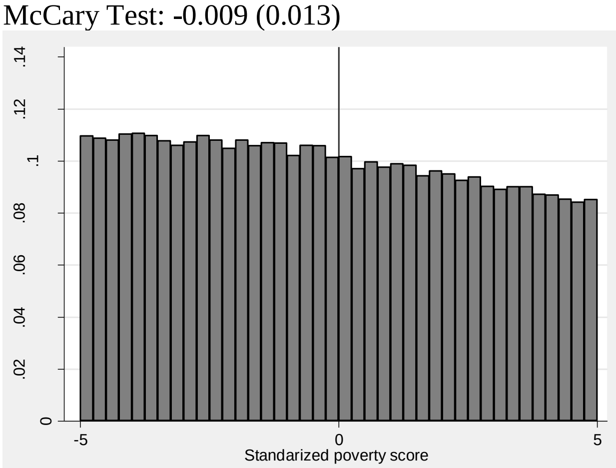
Panel C: 2000/02-2007/08 merged sample: 297,982 households
McCary Test: -0.064 (0.012)



Panel D: 2000/02-2007/08 merged sample, door-to-door visits only: 207,376 households
McCary Test: 0.006 (0.013)



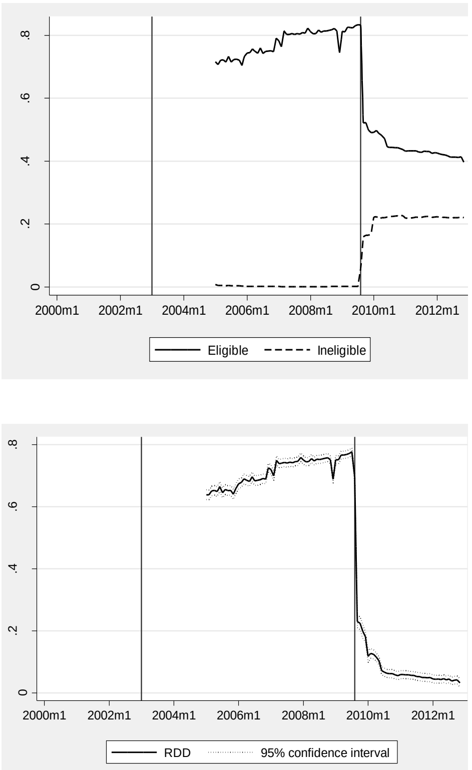
Panel E: 2007/08-2013/14 merged sample: 314,143 households
McCary Test: -0.009 (0.013)



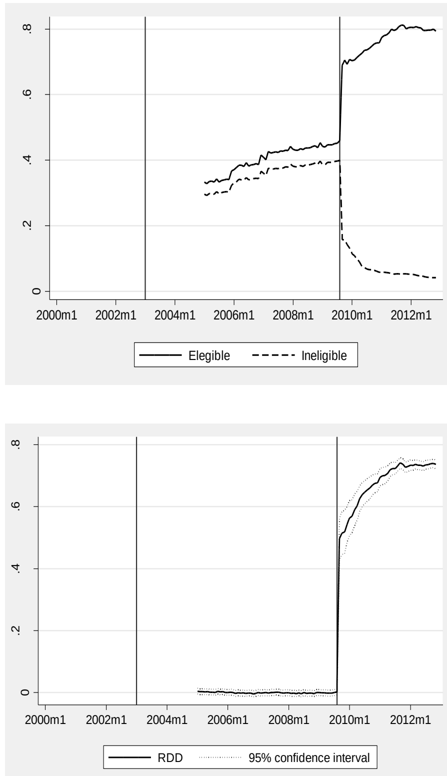
Note: Each panel presents histograms of the poverty score, with the data partitioned into 40 bins (so that each bin corresponds to 0.25 points of the poverty score).

Figure A3: Welfare eligibility and welfare payments, RD samples

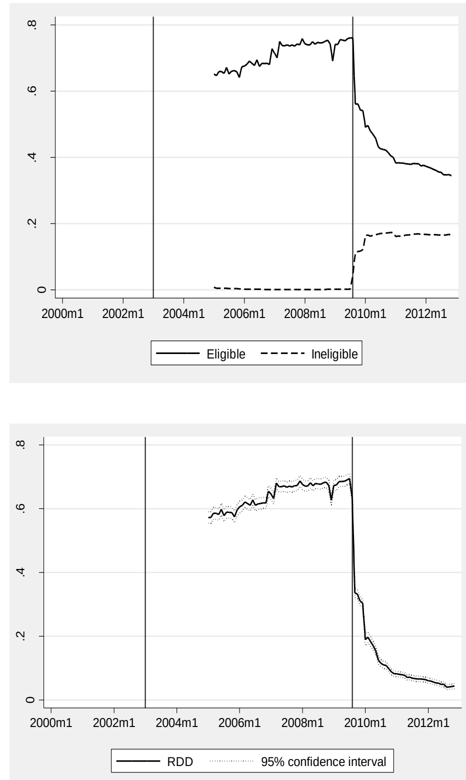
Sample 1, used to estimate effects of welfare on work, eligibility by 2000/02 census



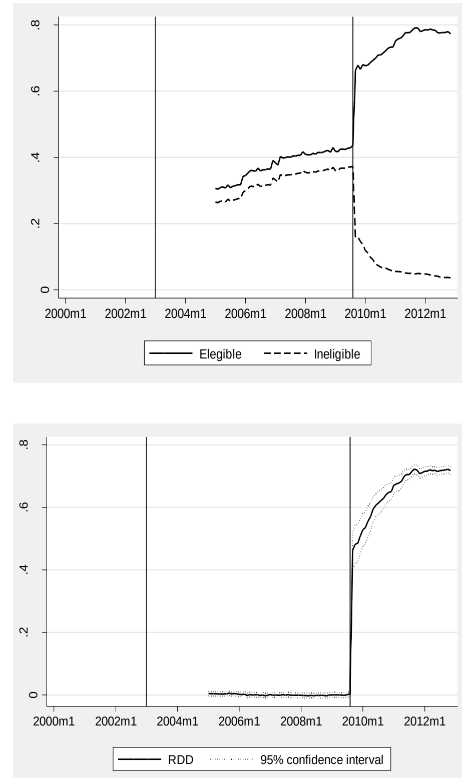
Sample 2, used to estimate effects of welfare on work, eligibility by 2007/08 census



Sample 3, used to estimate effects of welfare on formality, eligibility by 2000/02 census



Sample 4, used to estimate effects of welfare on formality, eligibility by 2007/08 census



Note: The figure depicts estimates of the effect of welfare eligibility on the probability of receiving welfare payments for the four samples used in the analysis. Top panels depict the share of eligible and ineligible households (up to 5 points above and below the eligibility cutoff) receiving welfare payments. Bottom panels depict the coefficients and confidence intervals on the variable I(S_{ih}<C) from local linear regressions of receiving welfare payments on the poverty score, the eligibility cutoff, the interaction between the cutoff and the poverty score, and canton fixed effects. Standard errors are clustered by parish, and bandwidths are 2.5 points. Vertical lines correspond to the month in which the first and second poverty censuses became operational, respectively. Sample sizes are 207,376 for sample 1, 314,143 for sample 2, 389,943 for sample 3, and 547,031 for sample 4.