The effect of welfare payments on work: Regression discontinuity evidence from Ecuador[☆]

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ABSTRACT

We study the impact of welfare payments in Ecuador on the probability that adults work, and on whether they are employed in the formal or informal sectors. Our identification strategy exploits the fact that welfare was limited to individuals below a cutoff value on a household "poverty score". We find no evidence that transfers discouraged work. However, among women, welfare payments led to reductions in social security contributions (which are mandated for salaried workers), although the magnitude of these effects is small.

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.

In this paper, we analyze the impact of welfare payments on the labor market choices made by adults in Ecuador, a middle-income country in South America.[1] We note, like others before us, that it is not clear whether the findings from the U.S. literature apply in poorer settings, for a variety of reasons. Households 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, as we discuss below, 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, welfare payments may not reduce employment by placing a high marginal tax rate on earnings.

Finally, 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. Depending on how they are designed, welfare programs could affect the choice between formal and informal employment.

Our analysis seeks to answer two questions. First, did welfare recipients work less? If cash transfers reduce work, the effect they have on poverty will be blunted, and programs may encourage welfare dependency. Second, did welfare affect the choice between formal and informal employment? Workers in the informal sector do not contribute to social security, and will therefore not receive a (contributory) pension in old age. Also, there is evidence that firms in the informal sector in developing countries are substantially less productive than those in the formal sector (Busso et al., 2012, 2013; Hsieh and Klenow, 2014; Levy,

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[1] We use the terms welfare programs and cash transfer programs interchangeably throughout the paper.

2018). Therefore, if welfare payments shift recipients from the formal to the informal sector, they could have negative effects on productivity and increase poverty among the elderly in the future.

To answer these questions, we analyze the labor market effects of the *Bono de Desarrollo Humano* (BDH) program in Ecuador. The BDH (originally called the *Bono Solidario*) was created in 1999, so it is one of the oldest cash transfer programs in Latin America. It is large, covering roughly one-quarter of households in the country.[2] As is the case with most other welfare programs in Latin America, payments are made to women. The transfers the BDH makes are generous, amounting to roughly 20 percent of household income during the period we analyze.

We estimate the effects of transfers on work choices using a regression discontinuity design. In practice, we make use of the fact that the poverty score that was used to determine welfare eligibility was revised in 2009, which implied considerable reshuffling of households in and out of eligibility.

The main results we present in the paper are two. We first show that the BDH did not reduce adult work.[3] This result holds both for women who received the transfers, and for other adults in their households. Our estimates are very precise and, for the sample as a whole, we can rule out negative effects on work of 0.5 percentage points or larger. We also estimate effects separately for households whose eligibility for welfare was "switched on" in 2009, relative to other households who continued to be ineligible, and for households whose eligibility was "switched off", relative to other households who continued to be eligible to receive transfers. In both comparisons, we cannot reject the null of no effects of welfare on work.

Next, we show that welfare eligibility led to some reallocation of women who received the transfers from the formal to the informal sector. Most of the increase in informal employment occurred *within* industries and occupations, but there were also shifts of women out of the most formal sectors of the economy. Nevertheless, although the effects we estimate are statistically significant, they are small. In our preferred specification, they imply reductions in formal employment of 1.2 percentage points, from a counterfactual level of 16.3 percent.

Our results extend the literature on welfare programs in developing countries in several ways. Alzúa et al. (2013) and Banerjee et al. (2017) convincingly show that cash transfers generally did not reduce the labor supply of adults in Honduras, Indonesia, Mexico, Morocco, Nicaragua, and the Philippines. However, their analysis focuses on changes after 1–2 years, immediately after the programs they study were created, and some programs were small (and short-lived) pilots.[4] It is possible that households had not yet fully adjusted their labor market decisions, or that they regarded welfare payments from these new programs as a temporary income windfall. In contrast, we study whether individuals changed their labor supply 4–5 years after they were made eligible to receive transfers from a program that was nationwide in scope, and had been in place for roughly a decade. Moreover, our results are more precise than those estimated in Alzúa et al. (2013) and Banerjee et al. (2017)—a key concern when a null hypothesis cannot be rejected (in this case, failing to reject the null hypothesis that welfare does not affect work).[5]

Our analysis of the effects of welfare on the choice between formal and informal employment builds on earlier work on Uruguay (Amarante et al., 2011; Bergolo and Cruces, 2016) and Brazil (De Brauw et al., 2015). In these countries (especially in Uruguay), program administrators cross-checked data on welfare eligibility with administrative data on income from formal employment using social security and tax records. Individuals could—and were, at least in some instances—be disqualified from receiving welfare payments if their earnings from formal employment were above an established cutoff. The implied marginal tax rate on formal sector income was therefore very high, at least in the vicinity of the eligibility cutoff. Not surprisingly, this led to a substantial reallocation of work from the formal to the informal sectors.[6]

In Ecuador, on the other hand, although BDH program guidelines initially established that individuals who held a formal job would be disqualified from welfare, in practice payments were never discontinued for welfare recipients who were formally employed. Consistent with this, the reductions in work in the formal sector in Ecuador were much smaller.

The rest of the paper proceeds as follows. In section 2, we briefly describe the context, and in Section 3 we discuss the data and identification strategy. Section 4 presents results, and section 5 concludes.

2. Setting

2.1. The Ecuadorean labor market

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 2005 and 2015, GDP in Ecuador grew at an average rate of 4.3 percent per year. The country is divided into provinces (24), cantons (251), and parishes (1,399).

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. Self-employed workers and firm owners do not have to make contributions to social security for themselves.[7] In practice, only 28 percent of those who are employed (less than 50 percent of salaried workers) contribute to social security in any given month, although in Ecuador this figure has gone up substantially over the period we study in this paper.

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[2] 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.)

[3] We do not focus on BDH effects on child labor because these have been studied elsewhere. See Edmonds and Schady (2012).

[4] Among the data sets that are used by Alzúa et al. (2013) and Banerjee et al. (2017), 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 2 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] As we show in Table 3 below, we can rule out reductions in the probability that adult women who receive the transfer work larger than 0.4 percentage points. In contrast, based on Table 3 in Banerjee et al. (2017), the authors can rule out negative effects larger than 2 percentage points in Mexico, the Philippines, and Indonesia, 5 percentage points in Nicaragua, and 6 percentage points in Honduras and Morocco. Results in Alzúa et al. (2013), Tables 2 and 3, are similar.

[6] De Brauw et al. (2015) find that beneficiaries of the *Bolsa Familia* program in Brazil report working 8.0 fewer hours per week in the formal sector and 7.8 more hours in the informal sector, on average, relative to individuals who did not receive transfers. Bergolo and Cruces (2016) find declines in formal employment of 8.6 percentage points in Uruguay; Amarante et al. (2011) also report reductions in formal employment that resulted from eligibility for an earlier welfare program in Uruguay, but they cannot establish whether these are a result of declines in work, or a shifting of employment from the formal to the informal sectors.

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

2.2. Welfare in Ecuador

The current welfare system in Ecuador dates back to the late 1990s. In 1999 the country suffered from a severe banking crisis, real per capita GDP fell by 6.7% 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. However, because there were no clear selection criteria, many recipients were non-poor, and many poor households did not receive transfers.

Since 2000, the government has periodically carried out "poverty censuses" to determine eligibility for welfare payments in a more systematic way. These censuses, which were conducted in 2000/02, 2007/08, and 2013/14, gathered information on household composition, education levels, work, dwelling characteristics, and access to services. In each case, information was aggregated into a poverty score by principal components, and a (single) eligibility cutoff was set at an arbitrary value of the score. The process that was used to determine eligibility for transfers in Ecuador is similar to that used by many other programs in Latin America—including, for example, by the much-studied PROGRESA program in Mexico.

When a household is declared eligible for transfers, the woman who heads the household, or is the spouse or partner of the household head, is designated as the recipient.[8] To receive transfers, she must have a valid *cédula*, a national identification number (comparable to the social security number in the United States).

When the BDH was first created, program rules stated that households who were registered with social security would be ineligible for transfers. In practice, however, this condition was never enforced. Also, and unlike many of the conditional cash transfer (CCT) programs in Latin America (like *Progresa* in Mexico and *Bolsa Familia* in Brazil), the BDH did not impose any additional requirements on households, such as taking young children for preventive health checkups or enrolling school-aged children in school.

Welfare payments in Ecuador have grown in magnitude over time, from US $7 per household per month in 1999, to US $15 in 2003, to US $35 in 2009, and to US $50 in 2014. 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.[9]

3. Data and identification strategy

Our analysis focuses on comparisons of outcomes for individuals who were eligible and those who were ineligible to receive welfare payments after August 2009, when poverty scores calculated from the 2007/08 census were used to determine eligibility.[10]

Because our identification strategy is based on regression discontinuity (RD), we begin with a sample of households whose 2007/08 poverty score placed them within five points of the eligibility cutoff. We focus on the labor market choices of "prime-age" adults who were between 20 and 55 years of age in 2007/08.

We bring together various data sets for our analysis. In each case, merging was done using the *cédula* of women who were the household head, or the spouse or partner of the household head, as reported in the 2007/08 poverty census. These focal women are the *potential* beneficiaries of the program, with actual eligibility given by the poverty score. *Cédulas* are available for virtually all (98.3 percent) focal women in the 2007/08 poverty census.

To analyze the effects of welfare on work, we used the *cédula* to merge data from the 2007/08 and 2013/14 poverty censuses. Both censuses asked all household members, ages 15 and older, whether they worked in the last week; no information on hours worked or earnings are available in either census. The merge rate was 60.4 percent of focal women in the 2007/08 census.[11] The final data set for this part of the analysis includes 226,911 focal women and 174,956 other adults living in the households of these focal women at the time of the 2013/14 poverty census.[12]

To analyze the effects of transfers on the choice between formal and informal employment, we used the *cédula* of focal women, as reported in the 2007/08 poverty census, and merged in data on contributions to social security from IESS. Data on social security contributions are available monthly from January 2000 until December 2012. We note, also, that a *cédula* is necessary to make social security contributions. Therefore, unless there are keying errors, all focal women whose data did not merge are women who did not make contributions. The final data set for this part of the analysis therefore includes the 375,900 focal women with *cédulas* in the 2007/08 poverty census.[13]

The regressions we run to estimate the effects of welfare all take the following form:

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[8] In practice, in our data, 94.5 percent of all recipients are women. The remaining 5.5 percent of cases are almost exclusively households where there were no women, and we exclude these from our analysis.

[9] Earlier evaluations of the BDH program have shown that transfers improved child development (Paxson and Schady, 2010; Fernald and Hidrobo, 2011); reduced chronic malnutrition among children (Buser et al., 2017); reduced anemia among adult women (Schady, 2012); increased school enrollment (Schady and Araujo, 2008); reduced child labor (Edmonds and Schady, 2012); and led to increases in the probability that girls complete high school (Araujo et al., 2019). There are no evaluations of the effects of the BDH on household income.

[10] In Araujo et al. (2017) we also present RD results from an earlier period, as well as results from an alternative identification strategy which exploits the fact that a group of households newly-eligible for transfers was randomly assigned to an early or a late treatment groups. These results are both consistent with those we report in this paper.

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[11] There are numerous reasons why we would not expect a perfect merge rate between the 2007/08 and 2013/14 poverty censuses: (1) neither census had nationwide coverage, and the localities that were covered in the two censuses did not overlap perfectly; (2) migration rates, both within the country and overseas (especially Spain) are high in Ecuador. Households that migrated internationally, or from an area covered in the 2007/08 poverty census to one not covered in the 2013/14 census, would fail to merge; (3) a small proportion of individuals may have died between the application of the two censuses; (4) households whose living conditions had improved substantially between 2007/08 and 2013/14 may have judged that they would be unlikely to qualify for welfare by the 2013/14 census, and may therefore have chosen not to be surveyed, or made no effort to be at home when enumerators visited; (5) there could be keying mistakes in the *cédula* in either census.

[12] Two clarifications are in order. First, welfare may have resulted in changes in household composition—for example, if additional adults are more likely to move in with households that are eligible for transfers. We do not find any obvious evidence that this is the case. In an RD specification in which the dependent variable is the number of household members in 2013/14, the coefficient (standard error) on welfare eligibility is 0.006 (0.015); in a comparable regression in which the dependent variable is the number of prime-age adults, the coefficient (standard error) is −0.021 (0.013). Second, our age restriction is based on age in the 2007/08 poverty census, as described above. For the focal women, we know their age in 2007/08. For other members in the households of these women in 2013/14, however, we only know their age as reported in the 2013/14 census. To make the ages of focal women and other prime-age adults comparable, our analysis of welfare effects on work of other household members focuses on prime-age adults between the ages of 26–61 in 2014.

[13] In addition to collecting the *cédulas* of focal women, the 2007/08 and 2013/14 poverty censuses also collected the *cédulas* of other household members. However, these data were collected somewhat less systematically: 85.9 percent of the *cédulas* of other adults in the households of the focal women are available. This raises the possibility that other adults for whom the *cédulas* are available are a selected sample. For this reason, our analysis of the effects of welfare on contributions to social security is limited to focal women. In Araujo et al. (2017) we show, however, that when we use the *cédulas* of the men who are household heads, or spouses or partners of the household head, to merge in their data on social security contributions, we find no effects of welfare on the choice between formal and informal employment of these men.

$$Y_{ih} = \alpha_c + S_{ih}\beta_1 + I(S_{ih} < C)\beta_2 + I[(S_{ih} < C)*S_{ih}]\beta_3 + \varepsilon_{ih}, \qquad (1)$$

where Y_{ih} is a variable that takes on the value of one if an individual worked in the last week (as reported in the 2013/14 poverty census) or contributed to social security (as given by the IESS records); α_c is a set of canton fixed effects; S_{ih} is a parametrization of the running variable, the poverty score on the 2007/08 census; $I(S_{ih} < C)$ 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; and ε_{ih} is the error term.

We run regressions by OLS. The eligibility cutoff has been re-centered at zero. Standard errors in all regressions are clustered at the parish level. The parameter of interest in these regressions is β_2, the intent-to-treat effect of welfare payments on work choices.

As is the case in other applications of RD, it is important to verify that

our results are not sensitive to how we parametrize 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). However, we also present results that use smaller and larger bandwidths, and those that use the full sample of individuals within five points of the eligibility cutoff and control for a polynomial in the control function.

For our identification strategy to make sense, there must be a "first stage": Eligibility, as established by the poverty score from the 2007/08 census, must affect the probability of receiving welfare payments thereafter. To check whether this was the case, we used the *cédula* to merge in administrative data on payments, which are available on a monthly basis from January 2005 to November 2012. Because a woman needs a *cédula* to receive payments, those whose data did not merge are women who did not receive payments.

Fig. 1 shows the relationship between welfare eligibility and payments. To generate the figure, we first divided the sample into women

Panel A: Proportions receiving transfers, by eligibility by the 2007/08 poverty census
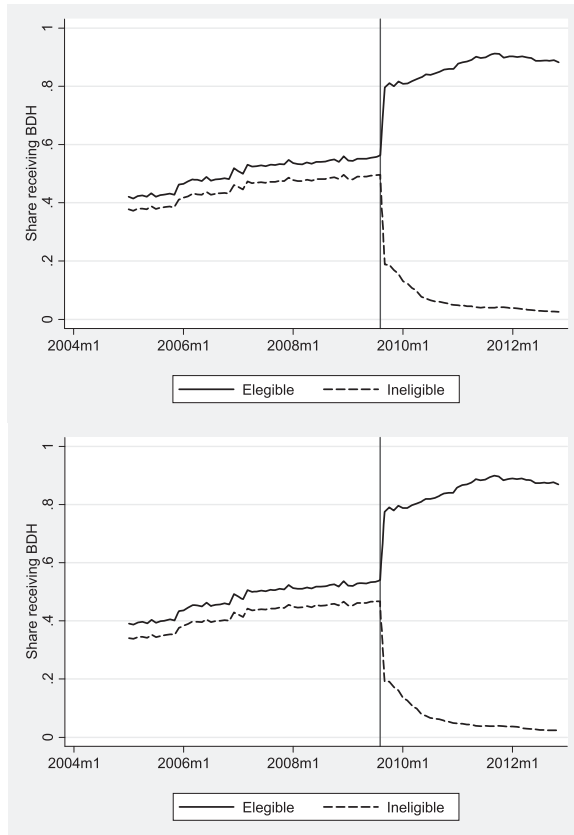
Panel B: Regression discontinuity estimates (coefficients and confidence intervals) of payments, by month, on eligibility by the 2007/08 poverty census
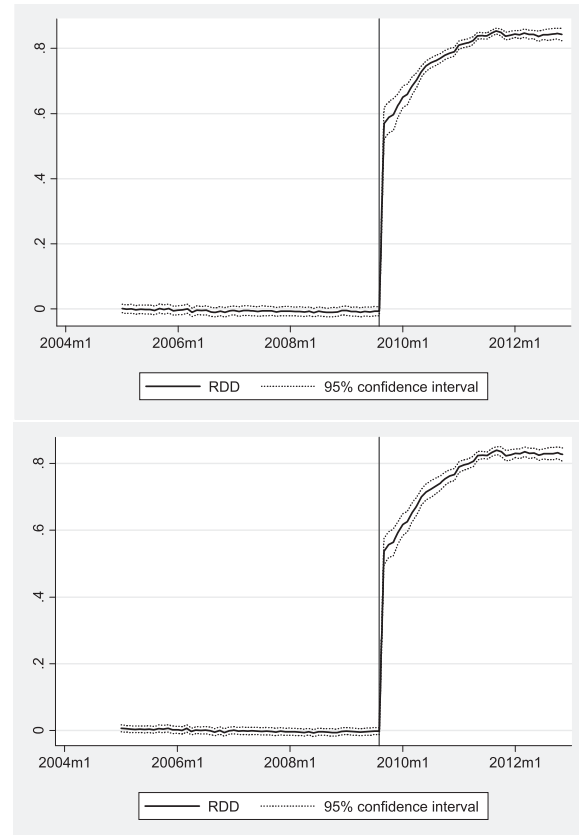

Note: The figure depicts estimates of the effect of welfare eligibility on the probability of receiving welfare payments for the two samples used in the analysis. Panel A depicts the share of eligible and ineligible households (up to 5 points above and below the eligibility cutoff) receiving welfare payments for the sample used for the analysis of welfare on work (226,991 focal women, upper figure) and formality (375,900 focal women, lower figure). Panel B depicts the coefficients and confidence intervals on the variable $I(S_{ih} < C)$ from local linear regressions (a separate regression for each month) 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 August 2009, the month in which the 2007/08 poverty census was first used to determine eligibility for transfers.

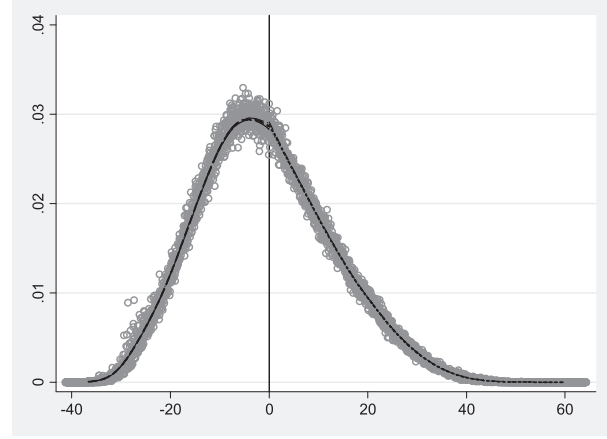**Fig. 1.** Welfare eligibility and welfare payments.

Panel A: 2007/08: Women with *cédula* in the 2007/08 poverty census

McCary Test: -0.007(006)
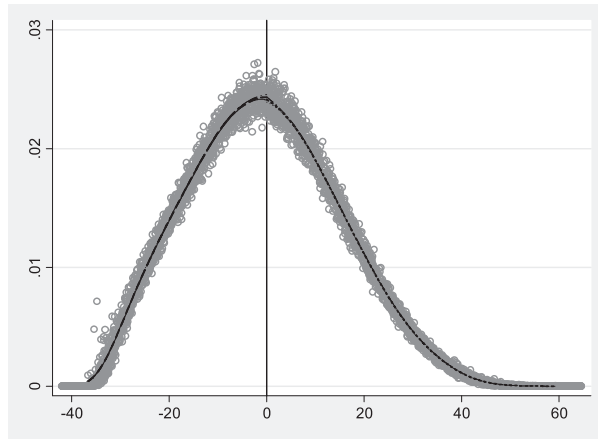
Panel B: 2007/08 Women with *cédula* who merged between 2007/08 and 2013/14 poverty censuses

McCary Test: -0.006(008)

Note: Each panel represents the density of the poverty score where 0 is the elegibility threshold. Sample sizes are 1,119,473 focal women in Panel A (which include the 375,900 women in our sample), and 840,784 focal women in Panel B (which include the 226,991 women in our sample).

Fig. 2. Density test.

who were *eligible* to receive BDH payments after August 2009, and those who were *ineligible*.

In Panel A of the figure, we graph the proportions of eligible and ineligible women who received payments, by month. Before the 2007/08 poverty census was used to establish eligibility, there were only small differences (about 5 percentage points) in the likelihood of receiving payments between women who would, and would not, be eligible for payments thereafter. This changed quickly after August 2009. Both panels in the figure show that, by early 2012, 80 percent of welfare-eligible women, but only 10 percent of welfare-ineligible women, received transfers in any given month.

It is also useful to show how the poverty score affected welfare payments at the eligibility cutoff. To analyze this, Panel B depicts the coefficients and confidence intervals from RD specifications of payments on eligibility, with separate regressions for each month of data. This panel shows that, before August 2009, there was *no* difference in the probability that a woman who would be just-eligible and one who would be just-ineligible received payments. By September 2009, this difference was roughly 50 percentage points, and by the beginning of 2012 it was 75 percentage points.

Although the two panels in Fig. 1 make clear that changes in eligibility led to rapid and large changes in actual welfare transfers, they also show that (as with most real-life programs) these changes did not mean that all eligible households, and no ineligible households, received payments. As such, our estimates are best seen as "fuzzy" rather than "sharp" RD. For this reason, we also present the results from two-stage least squares regressions in which we instrument an indicator variable for women who received welfare payments at least once after August 2009 with welfare eligibility as given by the poverty score on the 2007/08 census. These regressions provide estimates of the effect of receiving transfers (as opposed to being eligible for transfers) on work choices at the eligibility cutoff.

We carry out various checks on identification. First, in Fig. 2, we show that there is no unusual heaping of mass on either side of the eligibility cutoff, and we comfortably pass a McCary density test (McCrary, 2008). We conclude that there is no evidence that the poverty score was systematically manipulated around the cutoff.

Next, we take the sample of focal women with *cédulas* in the 2007/08 census, and run an RD specification in which the dependent variable is an indicator variable that takes on the value of one for women that merged with the 2013/14 census. In this regression, the coefficient (standard error) on just-eligible households is 0.006 (0.004). This regression clearly indicates that, at the cutoff, welfare-eligible and -ineligible households were equally likely to merge, which is consistent with the evidence in Fig. 2.

Finally, we test for differences in the baseline characteristics of households and individuals at the eligibility cutoff. We do this by reporting the results from RD estimates of a given baseline characteristic on welfare eligibility These results are in Table 1 (household characteristics) and Table 2 (individual characteristics).

In each table, there are six columns. Columns (1) and (2) correspond to the full sample of women with *cédulas* in the 2007/08 poverty census. This is the sample we use to estimate welfare effects on formality, as discussed above. Columns (3) and (4) correspond to households who merged between the 2007/08 and 2013/14 censuses. This is the sample we use to estimate welfare effects on work. Columns (5) and (6) correspond to households who did *not* merge between the 2007/08 and 2013/14 censuses. We present these results as an additional check to verify that the fact that we were only able to merge 60 percent of observations in the two poverty censuses does not introduce any obvious problems into our analysis.

Tables 1 and 2 show that, in all three panels, there are a handful of cases in which the coefficients on welfare eligibility are significant. In every instance, however, the magnitude of the implied differences

Table 1
Descriptive statistics, households.

Merge	2007/08 census- IESS		2007/08 census-2013/14 census (Merged sample)		2007/08 census-2013/14 census (Unmerged sample)	
	Mean, Ineligibles	RD	Mean, Ineligibles	RD	Mean, Ineligibles	RD
	(1)	(2)	(3)	(4)	(5)	(6)
Household characteristics						
Lives in an urban area	0.72	0.002	0.70	0.003	0.75	0.001
		(0.004)		(0.005)		(0.006)
Lives in house or apartment	0.48	0.011	0.53	0.008	0.42	0.011
		(0.004)		(0.004)		(0.006)
Has unfinished floor	0.21	−0.005	0.20	−0.009	0.22	0.004
		(0.003)		(0.003)		(0.006)
Has toilet connected to network	0.81	0.004	0.78	0.012	0.86	−0.006
		(0.004)		(0.005)		(0.005)
Has exclusive shower	0.49	0.007	0.52	0.008	0.44	−0.001
		(0.005)		(0.005)		(0.010)
Has gas kitchen	0.98	−0.000	0.98	0.001	0.99	−0.000
		(0.001)		(0.002)		(0.002)
Connected to electricity network	0.99	−0.001	0.99	−0.001	0.99	−0.000
		(0.000)		(0.001)		(0.001)
Owns land	0.09	0.002	0.11	0.002	0.07	0.003
		(0.002)		(0.003)		(0.003)
Number of rooms	2.3	0.017	2.43	0.005	2.14	0.037
		(0.010)		(0.013)		(0.017)
Number of household members	4.02	−0.007	4.03	0.004	4.01	−0.006
		(0.013)		(0.013)		(0.018)
Number of prime-age adults	1.89	0.004	1.86	−0.002	1.73	0.015
		(0.005)		(0.008)		(0.013)
N		375,900		226,991		148,909

Note: Columns (1), (3), and (5) report the mean value of a given characteristic in 2007/08 for just-ineligible households, columns (2), (4), and (6) report RD coefficients and standard errors from regressions of a given characteristic on the variable $I(S_{ih} < C)$ from local linear regressions with optimal bandwidth using the approach recommended in Imbens and Kalyanaraman (2012). These regressions also include the poverty score, the interaction between the cutoff and the poverty score, and canton fixed effects. The number of observations correspond to the maximum sample size in each subsample and may vary in each specific regression depending on the optimal bandwidth. Standard errors are clustered at the parish level.

between just-eligible and just-ineligible households and individuals is extremely small.[14] Importantly, we find no evidence that just-eligible focal women were either less likely to work, or less likely to contribute to social security than those who were just-ineligible before the 2007/08 poverty census was used to determine eligibility.

In sum, we conclude from these various checks that the RD estimates we report are likely to provide us with unbiased estimates of the effects of welfare on work choices at the eligibility cutoff.

4. Results

4.1. Welfare effects on work

Our main results on the effects of welfare on work are in Fig. 3 and Table 3. Fig. 3 has two panels, corresponding to focal women (Panel A) and all other adult household members (Panel B). Each figure has 40 points, and each point corresponds to the proportion working in 2013/14 at 0.25 points of the 2007/08 poverty score. We also plot the regression line from a cubic polynomial, estimated separately for points to the left and the right of the eligibility cutoff. If welfare reduced work, we would expect to see a jump in the proportion working just above the eligibility cutoff. No such jump is apparent in either panel.

Table 3 has two panels, corresponding to focal women (Panel A) and other adult household members (Panel B). In both panels, the first row

reports the results from regressions for the full sample of focal women (or other adults). These regressions therefore correspond to the visual evidence presented in Fig. 3.

The second row in both panels refers to focal women (or other adults in their households) who had received welfare payments at least once during the period between January 2005 and August 2009; for some of these individuals, eligibility was "switched off" in August 2009, while others continued to be eligible. In the third row, finally, we limit the sample to focal women (or other adults in their households) who had not received welfare payments between January 2005 and August 2009; for some of these focal women, eligibility was "switched on" in August 2009, while others continued to be ineligible.[15]

Panel A shows that, in the full sample of focal women, the coefficient (standard error) on welfare eligibility in our preferred specification is 0.008 (0.006); we can therefore rule out negative effects on work larger than 0.4 percentage points (from a counterfactual level of 46.5 percent working). Among women who had received at least one welfare payment, the coefficient (standard error) for those whose eligibility was switched off is 0.012 (0.009), and among women who had received no welfare payments, the coefficient (standard error) for those whose eligibility was switched on is −0.005 (0.008).

Results are similar for other adults in the households of the focal women, as can be seen in Panel B. In the full sample, the coefficient on welfare eligibility is −0.001 (0.003); for these other household members, we can therefore rule out negative effects on work larger than 0.7 percentage points (from a counterfactual level of 90.6 percent working).

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[14] Take, for example, the merged sample in Panel B. There are 19 separate regressions, and in only 2 cases are the differences significant. In these two instances, the implied differences at baseline (in 2007/08) are very small: (1) Just-eligible households were 0.9 percentage points less likely to have an unfinished floor than those who were just-ineligible (from a counterfactual level of 20.4 percent); and (2) just-eligible households were 1.2 percentage points more likely to have a flush toilet than those who were just-ineligible (from a counterfactual level of 78.5 percent).

[15] Fig. 1 shows that only between 65 and 80 percent of welfare-eligible individuals, with eligibility determined by the 2007/08 poverty census, collected welfare payments in any given month. If these proportions also apply to take-up rates for individuals who were eligible for welfare given their score on the 2000/02 poverty census, then a fraction of the individuals in the sample in Panel B were already eligible for welfare before August 2009.

Table 2
Descriptive statistics, women.

Merge	2007/08 census- IESS		2007/08 census-2013/14 census (Merged sample)		2007/08 census-2013/14 census (Unmerged sample)	
	Mean Ineligibles	RD	Mean Ineligibles	RD	Mean Ineligibles	RD
	(1)	(2)	(3)	(4)	(5)	(6)
Characteristics of focal woman						
Age in years	41.56	0.045	42.35	0.006	40.43	0.018
		(0.075)		(0.106)		(0.141)
Married	0.38	0.007	0.39	0.006	0.37	0.008
		(0.004)		(0.006)		(0.005)
Indigenous	0.04	0.000	0.03	0.001	0.05	−0.001
		(0.002)		(0.002)		(0.003)
Incomplete primary schooling	0.19	0.000	0.19	−0.001	0.19	−0.002
		(0.003)		(0.004)		(0.006)
Completed primary schooing	0.62	0.002	0.62	0.005	0.62	0.002
		(0.004)		(0.004)		(0.004)
Completed secondary schooling	0.19	−0.002	0.18	0.001	0.19	−0.008
		(0.002)		(0.003)		(0.005)
Worked in last week (baseline)	0.43	−0.004	0.39	−0.002	0.48	−0.004
		(0.004)		(0.006)		(0.006)
Contributed at least once to social security between January 2002 and August 2009	0.17	−0.002	0.15	0.007	0.20	−0.004
		(0.003)		(0.004)		(0.006)
N		375,900		226,991		148,909

Note: Columns (1), (3), and (5) report the mean value of a given characteristic in 2007/08 for just-ineligible focal women, columns (2), (4), and (6) report RD coefficients and standard errors from regressions of a given characteristic on the variable $I(S_{ih} < C)$ from local linear regressions with optimal bandwidth using the approach recommended in Imbens and Kalyanaraman (2012). These regressions also include the poverty score, the interaction between the cutoff and the poverty score, and canton fixed effects. The number of observations correspond to the maximum sample size in each subsample and may vary in each specific regression depending on the optimal bandwidth. Standard errors are clustered at the parish level.

Panel A: Eligibility by 2007/08 census and share working, 2013/14 poverty census, focal women
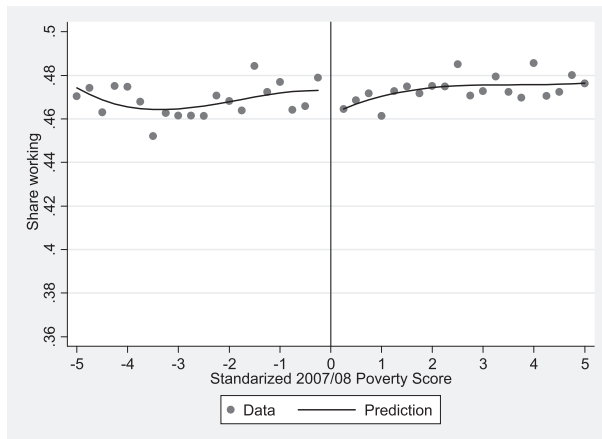
Panel B: Eligibility by 2007/08 census and share working, 2013/14 poverty census, other adult household members
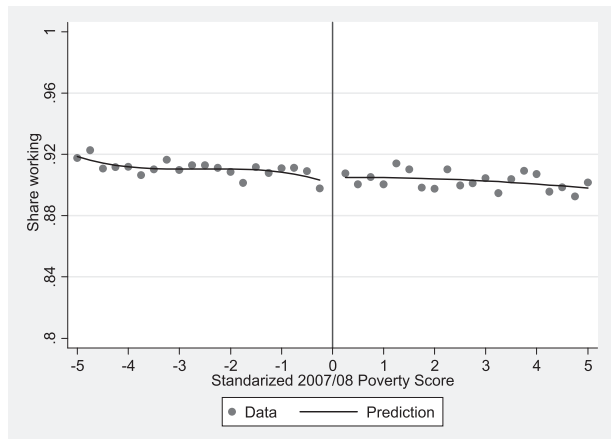
Note: The figure depicts estimates of the effect of welfare eligibility on work. Circles corrspond to each 0.25 points of the poverty score, and the solid line corresponds to an RD estimation fit using a cubic polynomial, estimated separately on each side of the cutoff, with a bandwidth of 5.

Fig. 3. Effect of welfare payments on work.

Among adults in households that had received at least one welfare payment, the coefficient (standard error) for those whose eligibility was switched off is 0.001 (0.005), and among adults in households that had received no welfare payments, the coefficient (standard error) for those whose eligibility was switched on is −0.001 (0.004).

In sum, we find no evidence that being eligible for welfare reduced work. This result holds for focal women and for other adults in their households, and no matter whether we consider all households, or carry out the analysis separately for those whose eligibility was switched on, and those whose eligibility was switched off.

4.2. Welfare effects on formality

We next turn to an analysis of how welfare affected the choice between formal and informal employment. Fig. 4 has a similar structure to Panel A in Fig. 3, but the x-axis now refers to the proportion of focal women contributing to social security at least once after August 2009. The figure shows a clear jump in formal employment just above the eligibility cutoff.

Our main results on the effects of welfare on formal employment are in Table 4. The table shows that, in the full sample of focal women, the

Table 3
The effect of welfare on work.

	Mean, ineligibles	(1)	(2)	(3)	(4)	(5)	(6)
		Intent-to-treat estimates					IV
Panel A: Eligibility by 2007/08 census, focal women							
All	0.47	0.008	0.006	0.002	0.012	0.010	0.009
		(0.006)	(0.008)	(0.005)	(0.006)	(0.008)	(0.007)
At least 1 payment, Jan. 2005–Jul. 2009	0.46	0.012	0.007	0.007	0.019	0.016	0.013
		(0.009)	(0.010)	(0.007)	(0.009)	(0.010)	(0.009)
No payments, Jan. 2005–Jul. 2009	0.48	−0.005	0.001	−0.007	−0.000	−0.001	−0.006
		(0.008)	(0.012)	(0.006)	(0.008)	(0.012)	(0.008)
N		103,962	57,374	226,911	226,911	226,911	103,962
Panel B: Eligibility by 2007/08 census, all other HH members							
All	0.91	−0.001	0.001	−0.001	0.001	−0.003	−0.001
		(0.003)	(0.005)	(0.003)	(0.004)	(0.005)	(0.003)
At least 1 payment, Jan. 2005–Jul. 2009	0.90	0.001	−0.002	−0.001	0.003	−0.003	0.001
		(0.005)	(0.006)	(0.003)	(0.005)	(0.007)	(0.005)
No payments, Jan. 2005–Jul. 2009	0.91	−0.001	0.004	−0.001	−0.002	−0.003	−0.001
		(0.004)	(0.009)	(0.004)	(0.006)	(0.009)	(0.004)
N		107,673	44,546	174,956	174,956	174,956	107,673
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 regressions of an indicator variable that takes on the value of one if an individual reported they worked in the 2007/08 census (30 regressions). In IV regressions in column (6) welfare receipt is instrumented with welfare eligibility at the household level. These regressions also include the poverty score, the interaction between the cutoff and the poverty score, and and canton fixed effects. The number of observations correspond to "All" sample in each column. The share of households in the regression who were beneficiaries between 2005 and 2009 is approximately 60%. Standard errors clustered at parish level.
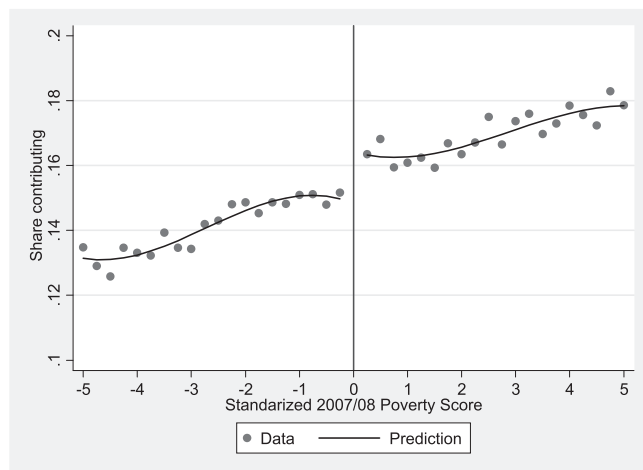
Note: The figure depicts estimates of the effect of welfare eligibility on the proportion of focal women contributing to social security at least once between August 2009 and December 2012. Circles correspond to each 0.25 points of the poverty score, and the solid line corresponds to an RD estimation fit using a cubic polynomial, estimated separately on each side of the cutoff, with a bandwidth of 5.

Fig. 4. Effect of welfare payments on contributing to social security.

coefficient on welfare eligibility in our preferred specification is −0.012 (0.004). This implies that just-eligible women were 1.2 percentage points less likely to contribute to social security than those who were just-ineligible (from a counterfactual level of 16.3 percent). Comparable co-efficients are −0.012 (0.004) and −0.016 (0.006) for the samples of women who had, and had not, received welfare payments before August 2009, respectively.

Fig. 5 provides additional evidence on the time pattern of welfare effects on formal work. To generate the figure, we run our preferred RD specification of social security contribution on welfare eligibility, separately by month. We then plot the coefficients and confidence intervals on $I(S_{ih} < C)$, and include a vertical line corresponding to August 2009, when the 2007/08 poverty census was first used to determine eligibility.

The figure shows that, before August 2009, focal women who would be just-eligible for welfare contributed to social security at the same rate as those who would be just-ineligible. As soon as the 2007/08 census was used to determine welfare eligibility, however, the proportion of just-eligible women contributing to social security began to fall, and continued falling for (at least) the next 3 ½ years.

We next analyze how the declines in formal employment of women came about. Panel A in Table 5 shows that fewer just-eligible women appear to hold salaried jobs, although this estimate is not always significant at conventional levels.

Next, 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.[16] Panels B and C in Table 4 show that just-eligible women were significantly less likely to work in the most formal

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[16] Specifically, we ordered industries (and, separately, occupations) by the share of women who contributed 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 1648 categories for industries in the 2013/14 poverty census data, and 5536 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 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 5. If all the increase in informality we observe were a result of just-eligible 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 just-eligible 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.

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

	Mean, ineligibles	(1)	(2)	(3)	(4)	(5)	(6)
		Intent-to-treat estimates					IV
Panel A: Eligibility by 2007/08 census, women							
All	0.16	−0.012	−0.013	−0.006	−0.008	−0.015	−0.014
		(0.004)	(0.005)	(0.003)	(0.005)	(0.005)	(0.005)
At least one payment, Jan 2005–Jul 2009	0.14	−0.012	−0.012	−0.006	−0.008	−0.015	−0.013
		(0.004)	(0.005)	(0.003)	(0.005)	(0.006)	(0.005)
No payments Jan 2005–Jul 2009	0.19	−0.016	−0.018	−0.008	−0.010	−0.017	−0.019
		(0.006)	(0.007)	(0.004)	(0.006)	(0.007)	(0.007)
N		137,051	95,143	375,900	375,900	375,900	137,051
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. These regressions also include the poverty score, the interaction between the cutoff and the poverty score, and canton fixed effects. In IV regressions in column (6) welfare receipt is instrumented with welfare eligibility at the household level. The number of observations correspond to "All" sample in each column. The share of households in the regression who were beneficiaries between 2005 and 2009 is approximately 60%. Standard errors clustered at parish level.
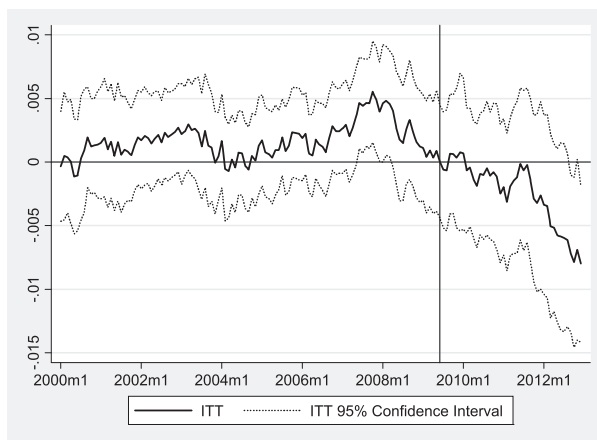
Note: The figure shows estimates of the effect of welfare eligibility on formality. Specifically, it depicts coefficients and 95 percent confidence intervals on $I(S_{ih} < C)$ in local linear regressions of an indicator variable that takes on the value of one if an individual contributed to social security in that month. Regressions are estimated separately by month (130 regressions). These regressions also include the poverty score, the interaction between the cutoff and the poverty score, and canton fixed effects. Standard errors are clustered at the parish level.

Fig. 5. Time-pattern of welfare effects on formal work.

industries and occupations than those who were just-ineligible: Our preferred RD specification indicates there were 1.6 percentage points fewer just-eligible women working in the most formal industries, and 0.9 percentage points fewer just-eligible women in the most formal occupations. A back-of-the-envelope calculation indicates that employment shifts from more to less formal industries, and from more to less formal occupations, explain about 40 percent and 20 percent, respectively, of the lower levels of formal employment of just-eligible women.[17]

In closing, we briefly discuss alternative explanations for the shift of welfare-eligible women from formal to informal employment in Ecuador. First, it could be that the steady stream of welfare income made some women more willing to assume the risks inherent in self-employment, which is generally carried out informally (Bianchi and Bobba, 2013).

This does not appear to be the main driver of the results we observe, however, as Panel A of Table 5 shows that welfare did not increase self-employment.

Second, the introduction of a so-called "non-contributory" pension program for the elderly age 65 and older could have changed the perceived benefits to formality. This program, which did not require the elderly to have made contributions to social security during their working life, began in 2006. Importantly, it was operated under the BDH. Non-contributory pensions were initially limited to elderly people who met two criteria: they could not have a formal pension, and they had to live in households with a poverty score below the cutoff that was used to determine eligibility for BDH transfers (as established by the 2000/02 poverty census).[18] It is possible that the introduction of non-contributory pensions reduced the perceived value of being formally employed, if households currently receiving welfare payments believed there was a good chance that they, too, would be eligible for such a pension in the future.

However, this explanation is not a good fit for the time pattern of effects we observe. In fact, as we show in Araujo et al. (2017), there were also reductions in formal employment among just-eligible welfare recipients between 2003 and August 2009, when poverty scores from the 2000/02 poverty census were used as the criterion for eligibility. In other words, the decline in formal employment among welfare recipients antedated the introduction of the non-contributory pension by roughly 3 years. Moreover, beginning in 2009, the government gradually made all elderly who did not have a contributory pension eligible for a non-contributory pension, regardless of their poverty score. This, in turn, should have eliminated any incentive for women who were just-eligible for welfare and those who were just-ineligible to respond differently to the non-contributory pension program.

Third, it could be that the small reductions in formality we observe are related to some degree of misinformation about how formal employment affected eligibility for welfare. Although individuals who held a formal job were not disqualified from welfare in practice, BDH program guidelines initially stated that they could be. All three poverty censuses asked respondents whether they had made contributions to social security. The introduction of the non-contributory pension program for elderly people who had not had a formal job for long enough to receive a contributory pension, administered by the BDH and (initially) using the same poverty score as that used to determine eligibility for welfare, may have created further confusion. All of these circumstances

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[17] For this calculation, we multiply the changes in employment for welfare-eligible women in Table 5 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).

[18] Thus, in a welfare-eligible household there could potentially be two or more recipients of transfers: the focal woman and any elderly person. The value of the this "non-contributory" pension was initially set at US $11.50 per month, and was revised upwards to US $35 in 2009, and to US $50 in 2014.

Table 5

Effect of welfare on reallocation across types of jobs, industries and occupations.

	Mean, ineligibles	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable: Type of job in 2014						
Salaried Work	0.45	−0.012	−0.008	−0.008	−0.018	−0.007
		(0.007)	(0.011)	(0.006)	(0.009)	(0.012)
Self employed	0.40	0.004	−0.002	0.002	0.007	0.007
		(0.006)	(0.011)	(0.006)	(0.008)	(0.010)
Domestic Worker	0.09	0.008	0.013	0.004	0.010	0.007
		(0.007)	(0.007)	(0.003)	(0.008)	(0.009)
Unpaid Workers	0.07	−0.003	−0.003	0.001	0.001	−0.007
		(0.005)	(0.006)	(0.003)	(0.005)	(0.006)
Panel B: Dep. variable: Employment by industry in 2014						
20% Most informal	0.02	0.016	0.015	0.009	0.015	0.014
		(0.007)	(0.009)	(0.005)	(0.008)	(0.009)
Next 20%	0.07	−0.009	−0.006	−0.008	−0.010	−0.006
		(0.005)	(0.009)	(0.004)	(0.007)	(0.010)
Next 20%	0.13	−0.002	−0.010	0.003	−0.001	−0.013
		(0.006)	(0.009)	(0.004)	(0.007)	(0.009)
Next 20%	0.18	0.008	0.022	0.003	0.011	0.016
		(0.008)	(0.009)	(0.006)	(0.010)	(0.010)
20% Most formal	0.42	−0.016	−0.021	−0.007	−0.009	−0.011
		(0.006)	(0.010)	(0.005)	(0.006)	(0.009)
Panel C: Dependent variable: Employment by occupation in 2014						
20% Most informal	0.01	−0.007	−0.010	0.007	−0.004	−0.010
		(0.006)	(0.009)	(0.005)	(0.007)	(0.010)
Next 20%	0.03	0.007	0.014	0.004	0.007	0.010
		(0.008)	(0.010)	(0.005)	(0.009)	(0.012)
Next 20%	0.11	−0.003	−0.003	−0.007	−0.002	−0.009
		(0.006)	(0.008)	(0.004)	(0.006)	(0.009)
Next 20%	0.21	0.015	0.020	0.005	0.013	0.020
		(0.010)	(0.011)	(0.005)	(0.011)	(0.012)
20% Most informal	0.53	−0.009	−0.021	−0.008	−0.014	−0.011
		(0.004)	(0.009)	(0.007)	(0.007)	(0.009)
N		106,823	26,951	106,823	106,823	106,823
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. These regressions also include the poverty score, the interaction between the cutoff and the poverty score, and canton fixed effects. The number of observations corresponds to the maximum sample size in each subsample and may vary depending on the optimal bandwidth. Standard errors clustered at parish level.

could have led some women to worry that they would not receive transfers if they held a formal job.

5. Conclusion

In this paper, we analyze the relationship between the provision of welfare, employment, and informality. Understanding how, if at all, welfare affects work choices is a critical concern for policy-makers in developing countries. We show that in Ecuador welfare did not reduce work, but led some women to work informally rather than formally. However, the magnitude of the decline in formal employment was small.

We close by discussing some of the limitations of our results. First, we cannot pinpoint the reasons why welfare payments in Ecuador led to a (small) reallocation of work from the formal to the informal sector. It seems reasonable that programs that explicitly cross-check earnings, and actually stop welfare payments to households with formal earnings above a pre-established cutoff, as was the case in Uruguay, would have larger negative effects on formal employment than those that do not check employment status and formal earnings in this way, as was the case in Ecuador. However, we do not have good data or an identification strategy to test this or other hypotheses. There would be high returns to research that seeks to identify concrete design features of welfare programs that make it more—or less—likely that they have a negative effect on work generally, and on formal employment specifically.

Second, we underline that our estimates—like any other RD estimates—refer to individuals at the eligibility cutoff, rather than to the average welfare recipient. In Ecuador, employment rates of adult women are reasonably constant in the lower half of the distribution of household

income. The proportion of women who contribute to social security, on the other hand, rises monotonically with income.[19] This pattern suggests, perhaps, that the effects we estimate at the cutoff may be a reasonable approximation to the average effects of welfare in the case of work (since employment rates are similar for the poorest and for somewhat better-off women), but may overstate the negative effects on informality (since contribution rates to social security are much lower among the poorest women).

Third, all of our results are estimates of individual effects, and therefore ignore any possible general equilibrium effects. The fact that

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[19] To get a sense of differences between just-eligible and other welfare recipients, we carried out some ancillary calculations using the 2014 nationally-representative labor force survey in Ecuador. This survey is known as the ENEMDU (*Encuesta Nacional de Empleo, Desempleo y Subempleo*). Specifically, we sorted households into deciles of household income, and then tabulated employment rates and, separately, the proportion contributing to social security, among women between 25 and 60 years of age, who are household heads, or the spouse or partner of the household head. These tabulations show that, in the lower half of the distribution (where welfare recipients would be concentrated) the proportions working are reasonably constant: They are 54.6 percent in the 1st (lowest) income decile, and 56.9 percent in the 5th decile. On the other hand, the proportion of women who are employed formally rises monotonically with household income: It is 9.9 percent in the 1st income decile, and 16.8 in the 5th decile. It would have been preferable to carry out these calculations using data from the poverty censuses, but we only have data on social security contributions for households within a limited range around the eligibility cutoff (±5 points).

roughly one-quarter of all households in Ecuador regularly received transfers that, on average, accounted for about 20 percent of their income, means that economy-wide impacts could in principle be large. Higher household incomes may have resulted in more demand for various goods and services and this, in turn, could have led to more labor demand. Perhaps, too, the increase in labor demand was concentrated in goods and services that are more likely to be produced in the formal sector, with implications for the choice between formal and informal employment.[20] All of these are conjectures, and our data and the design of the BDH program are not well suited to study such economy-wide impacts.[21] Future research to establish the magnitude of any general equilibrium effects of welfare on work would be useful.

Fourth, while we provide precise estimates of welfare effects on informal employment, the data we have do not allow us to convincingly estimate the impact that shifts from formal to informal employment may have had on other outcomes—most importantly, perhaps, on productivity. One would need data from a census of firms, covering both those that work in the formal and informal sectors, and including rich data on inputs into production (capital, labor) and plant output to calculate productivity. No such data exist for Ecuador.[22] Providing credible estimates of the effects of welfare (or other social programs that have been shown to increase informality in some contexts) on productivity should be an important priority for future research.[23]

Nevertheless, despite these limitations, our paper adds to the literature on welfare programs in developing countries in several ways. Like earlier research, we show that cash transfers did not reduce adult work in Ecuador. Notably, however, our results refer to a nationwide program that was well established, they cover a somewhat longer time period than those previously reported, and they are very precise. We also show that reallocations of employment from the formal to the informal sector were small, perhaps indicating that welfare programs that do not condition eligibility on (the absence of) formal income lead to only small distortions on this margin. It is always important to be alert to any unintended consequences of interventions, as underlined by Levy (2008) in his discussion of social policy in Mexico, but our results suggest that welfare programs that are sensibly designed are likely to have at most small negative effects on adult work choices.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jdeveco.2019.01.008.

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[20] There is another reason why our estimates of the welfare effects on informality may over-estimate aggregate effects. To see this, consider an extreme scenario in which the number of formal and informal jobs in an economy 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 America that many firms hire some employees formally and others informally (Bosch et al., 2013; Levy, 2008). Nevertheless, the general point that the individual effects we estimate may overstate any market-wide effects of welfare on formal employment is important.

[21] See Angelucci and De Giorgi (2009) for a careful analysis of the effects of the *Progresa* cash transfer program in Mexico on the local economy, which finds that such effects are large. The authors exploit the fact that, at an initial stage, *Progresa* targeted specific localities. In the case of the BDH, there was no geographic targeting of transfers, which would preclude an estimation along these lines.

[22] To the best of our knowledge, Mexico is the only country in Latin America where a firm census of these characteristics is available. See Hsieh and Klenow (2014) and Levy (2018).

[23] See, for example, Bosch and Campos-Vazquez (2014) on the extension of health insurance to informal workers in Mexico, and Bosch and Guajardo (2012) on an extension of pensions to workers who had not met the minimum number of years of contributions in Argentina.

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