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Conditional cash transfers, spillovers and informal health care: Evidence from Peru *

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Abstract

The use of low-quality informal health-care providers (IHCPs) is still prominent in developing countries despite the efforts of their governments to expand institutional services. The use of conditional cash transfer (CCT) programs have become instrumental in encouraging the use of formal health services, but little is known about their direct effect on the use of IHCPs. We use a large survey of rural households and a regression discontinuity design to estimate the effects of the Peruvian CCT program on the demand for IHCP. We find a sizeable reduction in the use of IHCPs not only in targeted but also in non-targeted members of treated households. This finding indicates the existence of spillover effects within the household. We also provide evidence that beyond the direct increase in income, the availability of better information about institutional services is a potential mechanism that drives these effects. We also find a corresponding improvement in the self-perception of health status. Our results are robust to a number of sensitivity analyses.

JEL Classification	**:**	C21, I18, O17
Keywords	**:**	Informal health-care providers, cash transfer program, regression discontinuity.

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1 Introduction

Informal health care providers (IHCPs) account for a substantial proportion of the health-care system in developing countries (cf., Bloom et al., 2011) despite the fact that IHCPs tend to deliver low-quality services (cf., Cross and MacGregor, 2010). Governmental efforts to promote the use of institutional services have not always been effective (cf., Dupas, 2011; Randive et al., 2013). Affordability, cultural biases, a mistrust in modern medicine and the lack of information about the low-quality services are the main factors behind the demand for IHCPs (cf., Sudhinaraset et al., 2013).[1]

Demand-side programs such as conditional cash transfers (CCT) might become a key instrument to address this issue. A large body of literature gives substantial evidence on the positive effects of CCT programs in several health outcomes (see, *inter alia*, Gertler, 2004; Morris et al., 2004; Behrman and Hoddinott, 2005; Lagarde et al., 2007; Barham and Maluccio, 2009; Evans et al., 2017; Parker and Todd, 2017).[2] However, little is known about the impact of CCT programs on the use of IHCPs.[3] Moreover, the literature often focuses only on the effects on target populations (i.e., children and pregnant women), but targeted interventions, such as CCT, might also induce behavioral responses in other family members. Yet, the evidence about spillover effects of CCT programs on health outcomes is scarce (see, *inter alia*, VerPloeg, 2009; Shei et al., 2014; Contreras and Maitra, 2013). However, this evidence is particularly important because whether these programs are able to expand their positive effects to non-target members of treated households is crucial for the programs to be effective in encouraging the use of institutional health services.

This paper studies the effect of a CCT program on the demand for IHCPs once users fall ill. We analyze not only the effect on the target population, but also the spillover effect on non-target members of the household. To the best of our knowledge, this is the first study to analyze the direct relation between CCT programs and the use of informal health-care services, and to explore the spillover effects. Our main contributions are to show that CCTs can be effective in reducing the use of IHCPs, regardless of whether individuals are the target population of the program or not, and to point out an income effect and the availability of better information of formal health-care as potential mechanisms behind our results. Understanding these effects can provide new insights for policymakers on how CCT programs can help reshape health systems in poor countries.

We study the Peruvian CCT program, named *Juntos*. Since 2012, the government has based eligibility in the program on the value of a socioeconomic score. The government computes this score from the information in household surveys, and we can verify that when it crosses a threshold, it generates a discontinuity in the probability of participation in the program. Thus, we can estimate the causal effects of the program within a "fuzzy" regression discontinuity design (see Lee and Lemieux, 2010).

Using a large survey of rural households, we find robust evidence that the CCT program reduces the use of IHCPs. In particular, the probability that an ill individual from the target group will seek medical attention at an ICHP reduces in approximately 0.25 to 0.37. This is significant because the CCT program explicitly requires the beneficiaries to attend regular medical checkups, but not to seek formal medical care when ill. Thus, the finding indicates that families with beneficiaries develop the habit of attending the formal facilities, as they become more familiar with it and its staff, which consequentially reduces the usage of IHCPs. Further, we also observe a somewhat smaller, but still significant, effect (between 0.22 and 0.29) for non-target members. The very habit of attending a formal facility extends to all family members, not only the program beneficiaries. Thus, this spillover effect reinforces the overall effect of the CCT program.

[1] Besides encouraging the use of formal health services, a complementary policy to improve the supply of affordable health-care in developing countries might be to increase the quality of IHCP through formal training (see, *inter alia*, Das et al., 2016).

[2] Most studies focus on the use of services that are part of the program's health requirements. Few papers analyze if CCT programs also increase the use of preventive health-care services (cf., Witvorapong and Foshanji, 2016) or the likelihood of seeking institutional treatment when the children fall ill (cf., Evans et al., 2017).

[3] It is not necessarily clear that the evidence that CCT programs increase the use of formal health services represents a net substitution of IHCPs or, given the increase in income due to the CCT, a greater demand for both types of services (i.e. formal and informal). For instance, substitution from IHCPs to formal services is evident in the case of increasing institutional births, but this is not obvious in the case of other health-care services.

We also explore the mechanism driving our findings by analyzing heterogeneous effects. In fact, we provide evidence that beyond the direct increase in income, the availability of better information about institutional services and the greater confidence generated by the frequent interaction with formal health facilities are potential mechanisms that drive these effects. Regarding this issue, significant effects are stronger if the ill individual belongs to (i) a household without land (i.e., poorer households in rural areas), (ii) a beneficiary household with young children of less than seven years of age (i.e., program beneficiaries with frequent checkups), or (iii) a household that is targeted by another social program (i.e., more used to social services). In all the cases, the income and informative shocks that are generated by participation in the CCT program play an important role in our main results.

We also find evidence that the CCT program is associated with a corresponding improvement in the self-perception of members' health status. This effect can be especially observed in the case of non-target members of the household who almost exclusively benefit from the spillover effects of the program. However, we fail to find a significant effect on outcomes that are related to actual (not perceived) health status, such as the probability of falling ill. This result is similar to that in the literature that finds that in the CCT context, perceptions change first whereas actual health improvements take more time to materialize (see, *inter alia*, Evans et al., 2017).

This paper relates to two different strands of the literature. First, it complements the literature on the effect of CCT programs on health (see Gertler, 2004; Lagarde et al., 2007; Attanasio et al., 2015; Parker and Todd, 2017) by including its direct effect on informal health services. Second, it contributes to a growing literature on the spillover effects of social programs on health outcomes (see Handa et al., 2001; Miguel and Kremer, 2004; Banerjee et al., 2010; Avitabile, 2012; Baird et al., 2013; Benjamin-Chung et al., 2017) by exploring the effects of the CCT program on members of the treated household that do not belong to the target population. This topic that has been rarely investigated.

The rest of the paper is organized as follows. Section 2 provides some background on the Peruvian CCT program and presents its assignment rule that is key to the identification of the treatment effect of interest. Section 3 describes the econometric framework: the fuzzy regression discontinuity design. Section 4 presents the data and the main empirical findings for the direct effects on the target population and for the spillover effects on the non-target population, along with a number of robustness checks. This section also explores the heterogeneity in the effects across several population groups to investigate the potential mechanisms that underlie our results. The results of the effect on individuals health status are also presented in this section. Section 5 concludes and suggests some avenues for further research.

2 Institutional background

Driven by the successful experience of the Mexican program Progresa/Oportunidades (see, *inter alia*, Parker and Todd, 2017), Peru launched its own CCT program, called *Juntos*, in 2005. The program has operated ever since and is considered the most important social program in the country, both in terms of coverage (it benefits around 70% of rural households) and budget (about 22% of all social programs budget).

A growing body of literature has studied the effects of this CCT program on various well-being indicators. Perova and Vakis (2009) present early evidence on positive but small effects on the expenditure of nutritious food and on school enrollment. Gahlaut (2011) finds significantly negative effects on the incidence of child labor. Sánchez et al. (2016) find that the program reduces severe stunting and improves height-for-age ratios among treated children between the ages of five and seven. Perova and Vakis (2012) find the positive effects of the program to be stronger for longer treatment spells; yet some effects remain small in magnitude, which indicates that the program has room for improvement. And, Pérez-Lu et al. (2017) show that *Juntos* reduces the number of underweight pregnant women and also reduces anemia among preschoolers.

2.1 Conditionalities and benefits

The CCT program's objective is to promote and speed up the access to quality social services by acknowledging not only poverty but also other sources of vulnerability such as political violence and disabilities. The target population are children or teenagers up to 19 years old, pregnant women, breastfeeding mothers, and people with disabilities. The program's transfers are conditional on the targeted household's members meeting a number of requirements: namely, attending regular health and nutrition checkups, school attendance and enrollment in the case of children and teenagers, attending prenatal checkups in the case of pregnant women, and postnatal checkups for breastfeeding mothers (see Programa Juntos, 2016, for further details).

The recipient of the cash transfer is usually the children's mother, the woman in charge of the children, or the beneficiary woman. Preference is given to female recipients due to the abundant evidence that they are most likely to devote the extra income towards the well-being of the family (see, *inter alia*, Bergolo and Galván, 2018). The government sends the money transfer monthly or every two months. Households receive S/ 100 (about US$ 30) every month or S/ 200 (about US$ 60) every two months. These amounts are relevant for rural families: they represented 44% of the rural poverty line (which is a per capita figure) in 2014 or, according to Sánchez and Rodríguez (2016), about 13% of the average household's expenditures.

2.2 Eligibility

Prior to 2012, eligibility was based exclusively on geographic targeting (see Escobal and Benites, 2012). In particular, all households in districts (municipalities) with a poverty rate above 30 percent were eligible. The National Statistical Office (INEI) determined the poverty rate by using a Poverty Map *à la* Elbers et al. (2003) that it computed with a combination of data from the Census and the National Household Survey (ENAHO). In 2012, following the creation of the Ministry of Social Inclusion and Development (MIDIS), the eligibility process was thoroughly refined and became a three-stage procedure. The first stage remains geographic where only districts with a poverty rate above 40 percent pass to the next stage.

Then, the MIDIS computes a socioeconomic score, the "household targeting index" (IFH), as a weighted average of a large set of preestablished predictors of poverty. For rural areas, the main variables include the fuel used for cooking, the number of members with health insurance, asset ownership, the education level of the household head, the highest level of education among any member of the household, access to electricity, and the flooring material. To determine eligibility, the country is divided into 15 clusters defined by areas with similar monetary poverty, and the IFH is compared to cluster-specific thresholds. Alternatively, these thresholds can be deducted from the IFH at the cluster level to normalize the threshold to zero. The final score ranges from -4 to 4.[4]

The second stage is household targeting, based on three criteria: (i) the IFH classifies the household as poor (i.e., IFH > 0); (ii) at least one member of the household belongs to the target population; and (iii) the household is considered poor according to the administrative (and confidential) data on income. Eligible families have to demonstrate that they had lived in the same district for the last six months. Also, they have to register with the health center and the school where the conditions are fulfilled.

The last stage is community validation. The MIDIS staff meet with a number of community members, local authorities, and representatives of other ministries (notably, education and health) to validate the results of the second stage and to update the administrative records. The purpose is to reduce inclusion and exclusion errors, because at this stage a household can prove, through updated income records that it needs the transfer despite IFH < 0; analogously, a household can be considered ineligible despite IFH > 0. There is evidence that the program has problems with leakage as well as under-coverage, though not as severe as other social programs (see Programa Juntos, 2016).

[4] The IFH is used for the assignment of various social programs in Peru. Bernal et al. (2017) study the case of social health insurance and provide a very detailed and comprehensive online appendix on the computation of the IFH (Bernal et al., 2017, Appendix F).

Further, there are some exceptions where only geographic targeting applies to politically sensitive areas. In particular, all households that are affected by violence from drug-trafficking and terrorism (such as the region named VRAEM) or districts that protect indigenous communities are eligible regardless of the values of the IFH. Also, households with members with disabilities are automatically eligible.

3 Methodology

In practice, we are able to compute the IFH from the information in household surveys (see, for instance, Bernal et al., 2017) and to sort households accordingly. Also, we observe the treatment status of the CCT program (i.e., whether a household is a beneficiary or not). Moreover, although the classification of the population into eligible and non-eligible households by the IFH predicts whether a household is a *Juntos* beneficiary, it does so imperfectly. Thus, as carefully discussed in Lee and Lemieux (2010), the nature of the problem lends itself to considering a "fuzzy" regression discontinuity design (RDD) for inference.

With observational data, individuals self-select into the treatment. However, in an RDD a given rule depends on the value of a running variable relative to a cut-off point to assign the treatment. Near the cut-off point, the assignment can be considered "as good as random", that is, treated and control units are comparable in all aspects but the treatment. But, as shown in Hahn et al. (2001), in a "fuzzy" RDD the running variable cannot predict the treatment perfectly. However, the eligibility rule still generates a discontinuity at the cut-off point in the probability of treatment, so the treatment effect can be identified using eligibility as an instrument for the treatment.

More precisely, let Y be the outcome of interest (i.e., the use of an IHCP when falling ill), D be the CCT treatment dummy variable ($D = 1$ when treated, $D = 0$ otherwise); X be the running variable (i.e., the IFH) whose cut-off point is set with no loss of generality to zero; and let T be the eligibility dummy variable ($T = 1$ when $X \geq 0$, $T = 0$ when $X < 0$). There is an important connection between the fuzzy RDD and the two equation system:

$$Y = \alpha_1 + \tau D + f(X) + \varepsilon_1\,, \tag{1a}$$

$$D = \alpha_2 + \delta T + g(X) + \varepsilon_2\,, \tag{1b}$$

where $f(\cdot)$ and $g(\cdot)$ are flexible functions of X, ε_1 and ε_2 are two possibly correlated error terms, and τ is the treatment effect of interest. Hahn et al. (2001) show that if $f(\cdot)$ and $g(\cdot)$ are of the same form (specifically, the same polynomial degree and bandwidth), as system (1) indicates, then τ can be consistently estimated from an IV regression of Y on D by using T as an instrument that controls for flexible functions of X to remove the effect of other factors that vary smoothly around the cut-off point.

Regarding the choice of the regression functions $f(\cdot)$ and $g(\cdot)$, two approaches dominate. The first is a parametric polynomial regression (see, *inter alia*, Gelman and Imbens, 2018) where $f(\cdot)$ is parameterized as follows:

$$f(X) = \beta_1 X + \beta_2 X^2 + \cdots + \beta_d X^d + T \times (\gamma_1 X + \gamma_2 X^2 + \cdots + \gamma_d X^d) \tag{2}$$

for some polynomial degree d. The inclusion of interaction terms between T and the powers of X adds flexibility to the specification by letting the regression function differ on both sides of the cut-off point. Function $g(\cdot)$ is specified analogously.

The second method is a nonparametric local linear regression (see, *inter alia*, Imbens and Kalyanaraman, 2012). Here d is generally set to $d = 1$ and larger weights are given to observations closer to the cut-off point. The weighting depends on bandwidth parameters. A popular choice is a uniform scheme where the sample is constrained to observations within narrow bandwidths of the cut-off point. In particular, for bandwidths h_L and h_R, the sample is such that $h_L \leq X \leq h_R$. To simplify matters, we consider a unique bandwidth h such that $h_L = -h$ and $h_R = h$.

In applications, a hybrid of both approaches is used (see, for instance, Pinotti, 2017; Bergolo and Galván, 2018): τ is estimated from equation (1a) with T as an instrument for D when using the polynomial functional form in (2) for $f(X)$ and $g(X)$ but conditional on $-h \leq X \leq h$. Under this approach, the estimation of τ and its standard error can be straightforwardly obtained from the output of an IV regression.

The choice of d and h involves a trade-off between bias and efficiency (see Lee and Lemieux, 2010, for futher details) where higher order polynomials and smaller bandwidths reduce the bias but at the cost of greater asymptotic variance. The choice of the order of the polynomials d is a standard model selection issue and is often assisted by information criteria. Recently, Gelman and Imbens (2018) show that, due to overfitting, estimates might become very sensitive, and confidence intervals might be misleading when d is too large. Therefore, as a rule-of-thumb, they advise against using $d > 2$. But, there are many studies on the choice of h based on the minimization of approximate cross-validation (see Imbens and Kalyanaraman, 2012) or mean-square criteria (see Calonico et al., 2014). All in all, a good practice in empirical work is to report the results of a number of varying specifications in order to determine the extent to which the results are sensitive to the selection of d or h.

4 Empirical analysis

In this section, we present the results on the effect of the *Juntos* program on the demand for IHCPs. We also present a number of robustness checks and explore the heterogeneities across different population subgroups.

4.1 Data

We use two main data sources. First, we use the National Registry of Social Programs Users that provides the MIDIS's administrative records on the beneficiaries and the enrollment dates for all Peruvian social programs.

Second, we use ENAHOR 2014, which is a special edition of the household survey ENAHO that is dedicated to rural areas. The INEI manages both surveys. The surveys are nationally representative and are aimed at measuring the poverty rate and general living conditions. They contain detailed information on education, employment, income and expenditure, housing, and perceptions that are required to compute the IFH and to control for socioeconomic characteristics. The questionnaire for ENAHOR 2014 is longer and more detailed than that for ENAHO. However, the most important difference is that ENAHOR 2014 is representative at the rural provincial level, whereas ENAHO is representative at the department (i.e., a collection of provinces) level only. The reason is sheer sample size as ENAHOR 2014 is ten times as large as the typical ENAHO: respectively, about 111,480 households versus 9,770 rural households in total, and 84,947 versus 7,691 households in target *Juntos* rural districts.

Regarding health outcomes, the surveys first asks individuals what their health status was during the last month. In particular, we consider individuals as ill if they experienced a severe illness such as influenza, colitis, or the relapse of a chronic disease. The usual symptoms and minor discomforts such as headaches, fever, or nausea are not considered an illness.

In addition, ill individuals also indicate if they sought medical attention and where or with whom. Patients can choose a formal provider from a long list of public health-care facilities and centers, that are administered mainly but not exclusively by the Ministry of Health (MINSA). Or they can choose private emergency centers, clinics, polyclinics, or the doctor's office. Moreover, they can also choose from informal providers that include an apothecary or a pharmacy, unidentified relative or friend, unlicensed chiropractor (*huesero*), traditional healer (*yerbero* or *curandero*), or other unspecified providers.

To construct our final database, we match the data from ENAHOR 2014 to the administrative records to identify the CCT program beneficiaries. Then, following the criteria outlined in subsection 2.2, we restrict the sample to only target districts (excluding VRAEM areas due to their exceptionality), households with at least one member under the age of 20 or one adult under 65 years old (poor elderly are eligible for *Pensión 65*, another social program), and households that became recipients of the CCT program after 2012 (i.e., the period when eligibility was influenced by the IFH). These criteria result in a sample of 6,763 households, with a total of 9,625 ill individuals.

Both equations in (1) can be augmented with controls variables without altering in any significant way the methodological framework used for inference. As controls variables, we use individual characteristics such as a female dummy variable, age and age squared, maximum education level achieved, an illiterate dummy variable, and a set of dummy variables for the mother tongue (Spanish, Quechua, Aymara and others). Also, we add a dummy variable for whether the district is considered an indigenous community. Importantly, we also consider three supply-side variables akin to the public health provision in the country: first, the reported time it takes to get from the house to the closest formal health center administered by MINSA; second, the Euclidean distance, that we obtain from the geographical coordinates, between the closest health center (the georeferencing is provided by MINSA) and the house (the coordinates are given in ENAHOR 2014); and third, an indicator of whether the household is covered by public health insurance. Some descriptive statistics are given in Table 1.

4.2 Baseline results

In our empirical analysis we sequentially examine whether the CCT program affects important health outcomes that will eventually determine our estimation sample. First, using the whole sample we fail to find a significant and robust effect on the probability of falling ill. Evans et al. (2017) find no improvements in health outcomes after 18-21 months (about 1.5 years) of transfers from a CCT program in Tanzania but find a significant reduction in the number of sick days per month after 31-34 months (about 2.5 years). These findings indicate that health improvements take more time to materialize. Other authors find similar results for educational outcomes.[5]

Second, individuals that are falling ill decide whether to seek medical assistance or not, yet we do not find, again, any significant effect of the program on this choice. However, we do find significant results when it comes to choosing between formal and informal care. Furthermore, the finding of no significant effects on the attendance rate means that a decrease in the usage of informal care comes in tandem with an increase in the usage of formal care of about the same magnitude. Therefore, we only present the results on the use of ICHPs.[6]

Figure 1 depicts the estimation of the treatment effect τ for various choices of d and h, and for the non-target population. Panels A1, B1, and C1 show the first-stage equation (1b), that is, D as a function of X; whereas panels A2, B2, and C2 show the "reduced form", that is, Y as a function of X, that results from replacing (1b) into (1a):

$$Y = \alpha_{\text{RF}} + \pi T + f_{\text{RF}}(X) + \varepsilon_{\text{RF}} \tag{3}$$

where $f_{\text{RF}}(X) = f(X) + \tau g(X)$ is of the same form as $f(\cdot)$ and $g(\cdot)$, $\alpha_{\text{RF}} = \alpha_1 + \tau\alpha_2$, $\varepsilon_{\text{RF}} = \varepsilon_1 + \tau\varepsilon_2$, and importantly $\pi = \delta\tau$.

It is quite apparent from panels A1, B1 or C1, that as X crosses the cut-off point (i.e., T changes from $T = 0$ to $T = 1$), the probability that $D = 1$ increases discreetly. The distance between the two segments of the discontinuous regression function at zero is the parameter δ. Furthermore, this distance does not change significantly as we vary the degree of the polynomial in $f(X)$ and the bandwidth h correspondingly. Similarly, a discontinuity is also generated at the cut-off in panels A2, B2, and C2. In this case, the length between the

[5] Filmer and Schady (2014) studies a program that gives scholarships to poor children in Cambodia and finds that even when the program had significant effects on school enrollment and grade attainment after three years, it does not have any impact on learning outcomes or earnings.

[6] Our baseline results in this section are conditional on illness; we present a full set of additional results conditional on medical attendance in the Appendix (Tables 5 and 6). In this two-choice setup, a decrease in the proportion of individuals using IHCPs necessarily implies an increase of exactly the same magnitude in the use of formal providers. However, it is worth mentioning that in our sample the proportion of ill individuals not looking for assistance is low, about 18%, which explains why the additional results are similar to our baseline results.

two segments of the regression function at cero is the parameter π, which is also not sensitive to changes in $f_{\text{RF}}(X)$. The treatment effect is given by the ratio $\tau = \pi/\delta$. Since δ and π are stable across specifications, we also estimate them robustly.

Table 2 presents the main results of our study. The table shows several estimates of τ for the target and non-target populations under various specifications: namely, different polynomial degrees d, bandwidths chosen optimally by using the criterion proposed by Calonico et al. (2014), bandwidths chosen to be arbitrarily below and above the optimal value, and regressions with or without control variables. The estimates are significantly negative for both population groups and robust to specification changes and to the inclusion of control variables.[7]

An interesting pattern is that the estimated effects remain negative and tend to be larger in magnitude when we add control variables. Among, we include supply-side indicators with the purpose of imposing the *ceteris paribus* condition on the supply side of the health provision. Thus, we interpret the estimates without controls as the result of a demand contraction along with a small positive supply expansion, whereas we interpret the estimates with controls as a measure of the demand effect only.[8] Since this is the effect of interest, we now focus on the regressions including control variables.

In panel (A), the estimated effect is sizeable for the target group: the probability that an ill individual from the *Juntos* target group will seek medical attention at an IHCP is reduced by approximately 0.25 to 0.37. Even though the CCT program does not explicitly require beneficiaries to seek formal medical care when ill, it does require them to attend regular medical checkups at the pre-established formal facility. Thus, the beneficiaries and their families become familiar enough with the facility itself and the staff running the facility to develop the habit of attending there, at least when it comes to child care, which thereby reduces the demand for informal health-care. In panel (B), the estimated effect is smaller but remains significant for the non-target group. Thus, the above conclusion regarding the shift in the demand of informal health-care applies to all household members, not only beneficiaries.

4.3 Heterogenous effects

We now study the potential heterogeneities in the estimated effects to understand the mechanisms behind our main results. We focus on the spillover effects of *Juntos* on non-target members within the household. Table 3 shows the treatment effect τ under various specifications (i.e., different polynomial degrees d and bandwidths h) for several subsamples.

In panel (A) of Table 3, we classify the individuals according to land ownership, because ownership is a covariate of income and living conditions that the government does not explicitly use to compute the IFH. In the sample, there are 5,347 out of 9,625 individuals who belong to a household whose head reports is the owner of a plot, and 4,278 individuals from landless households. The estimates show the importance of an income effect in the decision to replace informal services with formal ones, since the estimated effects are concentrated among those households that do not own any land. Budget and time constraints are still major barriers to the use of institutional health services, and the cash transfer can help alleviate such constraints. In particular, the cash transfer can be enough to cover the opportunity cost of members of destitute households joining the queue for formal health-care.

However, as can be seen from the rest of the table, budget considerations are not the only mechanism driving the results. A lack of information seems to be a key element in this context. In panel (B), the sample is divided between people able to read (3,362) and illiterates (6,263). We use these classifications as a proxies

[7] Thus, the differences in the individual characteristics and other control variables cannot explain the discontinuity in the probability of benefiting from *Juntos* at the cut-off point. This is a condition for the satisfactory identification of the treatment effect of interest. However, the McCrary (2008) test could not reject the hypothesis of continuity of the running variable around the cut-off point, that indicates the possible manipulation of the IFH is not able to explain the discontinuity in the probability of using *Juntos* either. These results are not reported, but are available on request.

[8] Valdivia (2004) and Bernal et al. (2017) describe the increase in the supply of health services and in health insurance provided by the Peruvian government.

for informed and uninformed people. The estimated effects are significant for the illiterates, who presumably are those are most affected by the information shock associated with the CCT program.

In panel (C), the population is divided by gender, with women (5,292) being the most likely to interact with the formal health system and to obtain more first-hand information about the quality of their services. This ability because they take care of children or are themselves beneficiaries. Thus, the effects on women should be stronger than on men, which is precisely what we obtain in the estimation.

Panel (D) shows the number of individuals that belong to households with at least one child below seven years old (5,475), and those that do not (4,150). The requirements of *Juntos* about attending formal medical checkups are considerably tougher for young children of less than five years old. Thus, households with young children are more likely to become familiar with formal health-care faster.[9] The panel shows that the effects of the first group are stronger, which indicates that frequent interaction with formal health facilities their staffs can lead to increased familiarity with these services. In time, this familiarity leads to attending formal health facilities once the individual is ill.

Panel (E) shows the individuals from households who are beneficiaries of other social programs, and therefore have interacted previously with government representatives (4,818), and those who are not (4,806). The estimated effects are significant regardless of whether the ill individual belongs to a household that is targeted by another social program or not. The magnitude, however, is substantially larger if the household had previously received the assistance from other governmental social programs. This result is evidence that greater interaction with public services generates greater confidence among rural inhabitants, that in turn leads to a reduction in the use of informal health services.

To sum up, the compelling evidence shows that beyond the direct increase in income, the availability of better information about institutional services and the greater confidence generated by frequent interaction with formal health facilities are potential mechanisms driving our main effects.

4.4 Examining the effects on health status

By reducing the demand for low-quality informal health services, we expect the health status of those affected by the program to tend to improve. This is so because in our sample a decrease in the usage of informal care represents an increase of a similar magnitude in the usage of formal care usage, which indicates that the poor may be obtaining better quality health services.

To measure health status, however, we do not have information from medical records for our sample. So, as in many other studies, we rely on self-reported health statuses to infer any effect from the program on health conditions (see, *inter alia*, Evans and Garthwaite, 2014; Clemens, 2015).[10] To do so, we use the information from a question on individuals' perceptions of their health in the questionnaire: Compared with other people of the same age, how would you say that your health is? (much better/better/equal/worse/do not know). With this information, a binary variable is constructed as follows: those individuals who ranked their health status as "much better" or "better" get a value of one, and zero otherwise. In the case of minors, the question is answered by the head of the household.

Table 4 shows the effect of the CCT program on our self-reported health variable for target and non-target members of the household. Again, we report the treatment effect under various specifications (i.e., different polynomial degrees d and bandwidths h). The estimates are significantly positive for both groups and robust to different specifications and to the inclusion of control variables. These findings indicate that CCTs positively affect the self-reported health statuses.

In particular, panel (A) of Table 4 presents the results for the beneficiaries of the program. According to these results, the CCT program increases the probability that an individual reports having a better (or much

[9] Recall that the survey is from 2014 and includes beneficiaries of *Juntos* since 2012. Thus, a seven year old at the moment of the survey would have been five years old in 2012, and thus subjected to the more demanding health control requirements.

[10] Moreover, Schnittker and Bacak (2014) show that self-rated health accurately predicts actual health conditions (i.e., mortality).

better) health status than their counterparts with a similar age by a range between 0.19 and 0.34. However, as many different factors drive this effect, mainly the medical checkups associated to the CCT's requirements, it is difficult to disentangle that are effects directly related to the shift in the demand for institutional health services for ill individuals due to the program.

Panel (B) of Table 4 shows the results for the non-target members within the household. These results are smaller than those for beneficiaries (a difference of around 0.10) but are still very significant and sizeable. Since these individuals do not directly benefit from the program, the potential mechanisms behind these results are mainly the income effect due to the CCT's transfer, the contagion effects because of the better health status of the benefited members of the household, and the indirect effects that are related to the reduction in the use of IHCPs. In that sense, these results might better reflect how a CCT program can improve the health status of the poor through a reduction in the use of informal health services.

Regarding other outcomes we failed to find a significant and robust effect on the probability of falling ill using the whole sample. This result is similar to Evans et al. (2017) who also find that health improvements take more time to materialize (about 2.5 years after the beginning of the transfers from the CCT program).

In conclusion, we find evidence that the CCT program through the reduction in the usage of informal health services might be associated with an improvement in the self-perception of health, which is a proxy for the short-term health status. This effect is especially evident in the case of non-target members of the household, who almost exclusively benefit from the spillover effects of the program on the demand for health services. However, we do not find evidence of a significant effect on the outcomes that are related to more objective long-term measures of health status, such as the probability of falling ill in the last month, which indicates that more time is required to observe such effects.

5 Closing remarks

We have provided robust evidence on a negative effect of CCT programs on the demand for informal health care in rural Peru. This effect is in part an income effect, as the extra cash received by the household covers the cost of getting medical attention in institutional health facilities, such as the actual transport costs and the opportunity cost of queueing. Nonetheless, it also is due to the acquisition of better information and, possibly, a different attitude towards modern medicine. CCT requires medical checkups for children and pregnant women in formal health-care facilities, and so the regular and frequent interactions of the members of a treated household will help develop the habit of consuming formal health care instead of an informal, low-quality alternative. Furthermore, we also find a corresponding improvement in the self-perception of an individual's health status.

These results provide new insights for policymakers on how CCT programs induce behavioral responses in every member of treated households. By changing individual's behaviors, these interventions are able to effectively reshape health systems in poor countries. This evidence also indicates that some of the main effects of CCTs might not be necessarily due to the program's medical checkups, but actually through the change in attitude toward formal health services that these programs encourage. Estimating the effect of these programs without considering these aspects would underestimate its total impact.

Due to data limitations, we cannot assess whether this reduction in the demand for informal health care due to CCTs is associated to an actual improvement in health. Similarly, we are not able to examine if the preference for formal services is focused on specific types of health services, which would help us to understand more about this shift in preferences. Finally, given our estimates on spillover effects within the household, it would be particularly relevant to estimate whether CCTs are also able to generate positive indirect effects among households. Examining these issues is beyond the scope of this paper but warrants future research.

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Table 1. *Descriptive statistics*

	Elegible				Non-elegible				All	
	Target		Non-target		Target		Non-target			
Variable	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Juntos (first stage)	0.943	0.231	0.950	0.217	0.498	0.500	0.425	0.494	0.714	0.452
(A) *Outcomes*										
Attendance	0.781	0.414	0.794	0.403	0.802	0.399	0.828	0.377	0.803	0.398
Informal care	0.241	0.428	0.248	0.432	0.230	0.421	0.242	0.429	0.242	0.428
Self-reported health status	0.169	0.375	0.073	0.260	0.196	0.397	0.112	0.316	0.125	0.331
(B) *Individual characteristics*										
Female	0.519	0.499	0.564	0.496	0.499	0.500	0.534	0.499	0.534	0.498
Age	10.355	5.239	41.947	12.046	10.128	5.269	40.571	11.644	29.709	18.009
Pre-school education	0.164	0.371	0.152	0.359	0.149	0.356	0.057	0.232	0.126	0.331
Primary education	0.491	0.500	0.365	0.481	0.508	0.500	0.228	0.419	0.375	0.484
Secondary education	0.344	0.475	0.483	0.499	0.343	0.475	0.715	0.452	0.499	0.500
Literacy	0.373	0.484	0.406	0.491	0.238	0.426	0.285	0.451	0.334	0.472
Mother tongue is Spanish	0.689	0.463	0.477	0.499	0.906	0.291	0.787	0.410	0.686	0.464
Indigenous community	0.211	0.408	0.229	0.421	0.161	0.368	0.125	0.330	0.183	0.387
(C) *Health supply indicators*										
Distance to closest health center (kms)	7.834	0.527	7.769	0.516	8.079	0.401	8.036	0.393	7.915	0.484
Distance to closest health center (mins)	89.406	81.025	89.183	80.294	77.599	72.829	76.619	69.751	83.412	76.361
Public health insurance	0.855	0.362	0.780	0.414	0.741	0.438	0.638	0.480	0.745	0.435
Observations	2,987	2,987	5,105	5,105	2,745	2,745	4,520	4,520	15,357	15,357

Source: ENAHOR 2014. Own elaboration.

Table 2. *Estimation of the effect of Juntos on the use of informal health-care providers*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) *Target population (direct effects)*								
No controls	-0.217^{***}	-0.309^{***}	-0.213^{***}	-0.347^{***}	-0.297^{***}	-0.343^{***}	-0.345^{***}	-0.249^{***}
	(0.081)	(0.096)	(0.084)	(0.124)	(0.127)	(0.107)	(0.115)	(0.105)
With controls	-0.260^{***}	-0.361^{***}	-0.252^{***}	-0.399^{***}	-0.362^{***}	-0.389^{***}	-0.365^{***}	-0.286^{***}
	(0.084)	(0.096)	(0.085)	(0.127)	(0.120)	(0.109)	(0.115)	(0.106)
Poly. degree (d)	1	1	1	2	2	2	3	3
Bandwidth (h)	0.46	0.36	0.55	0.84	0.70	0.98	1.59	1.79
Observations (n)	3,210	2,768	3,291	4,404	3,947	4,671	5,360	5,411
(B) *Non-target population (indirect effects)*								
No controls	-0.204^{***}	-0.232^{***}	-0.164^{***}	-0.186^{***}	-0.190^{*}	-0.235^{***}	-0.227^{***}	-0.174^{***}
	(0.057)	(0.058)	(0.054)	(0.079)	(0.104)	(0.065)	(0.071)	(0.065)
With controls	-0.232^{***}	-0.264^{***}	-0.185^{***}	-0.234^{***}	-0.243^{***}	-0.250^{***}	-0.259^{***}	-0.237^{***}
	(0.060)	(0.061)	(0.056)	(0.078)	(0.103)	(0.066)	(0.071)	(0.063)
Poly. degree (d)	1	1	1	2	2	2	3	3
Bandwidth (h)	0.42	0.38	0.46	0.69	0.54	1.03	1.59	1.98
Observations (n)	4,545	4,413	4,984	6,298	5,116	7,719	8,972	9,197

Source: ENAHOR 2014. Own elaboration.
Notes: IV estimates of the treatment effect on the use of IHCPs, τ in system (1), for various values of d and h.
Robust standard errors are in parentheses. The *, **, and *** indicate statistical significance at the 10% 5% 1% confidence levels respectively. All regressions include a flexible polynomial of degree d with interaction terms with T (the eligibility indicator), as in equation (2). h is the bandwidth and n is the number of observations such as $-h \leq X \leq h$, where X is the IFH. In columns (1), (4), and (7), the bandwidth is chosen optimally (for the regressions with controls) by following Calonico et al. (2014); in columns (2) and (5) the bandwidth is set deliberately below the optimal, whereas in columns (3), (6), and (8) it is set deliberately above the optimal.
The regressions with controls also include individual characteristics such as female indicator, age and age squared, maximum education level achieved, an illiterate indicator, and indicators for the mother tongue (Spanish, Quechua, Aymara and others). They also contain indicators of whether the town is considered an indigenous community, the distance (both in meters and in time) to the closest health center, and an indicator of whether the household is covered by public health insurance.

Table 3. *Heterogenous effects of Juntos on the use of informal health care providers for non-target groups*

Polynomial degree (d)	1	2	3	1	2	3
(A)	*Land ownership*			*No land ownership*		
Effect (τ)	−0.052	−0.117	−0.089	−0.238***	−0.503***	−0.400***
	(0.055)	(0.085)	(0.087)	(0.068)	(0.133)	(0.105)
Bandwidth (h)	0.63	0.94	1.74	0.63	0.84	1.89
Observations (n)	3,509	4,340	5,082	2,651	3,134	4,020
(B)	*Literate*			*Iliterate*		
Effect (τ)	−0.159	−0.212	−0.194	−0.189***	−0.210***	−0.240***
	(0.120)	(0.180)	(0.137)	(0.063)	(0.085)	(0.073)
Bandwidth (h)	0.49	0.60	1.79	0.48	0.63	1.82
Observations (n)	1,618	1,711	3,205	3,385	4,105	5,903
(C)	*Male*			*Female*		
Effect (τ)	−0.139	−0.200	−0.262**	−0.197***	−0.303***	−0.257***
	(0.092)	(0.126)	(0.131)	(0.070)	(0.092)	(0.089)
Bandwidth (h)	0.52	0.67	1.43	0.51	0.76	1.62
Observations (n)	2,306	2,806	3,933	2,777	3,642	4,938
(D)	*Children younger than 7 years*			*Children older than 7 years*		
Effect (τ)	−0.226***	−0.317***	−0.298***	−0.129*	−0.183**	−0.129
	(0.084)	(0.097)	(0.097)	(0.070)	(0.086)	(0.082)
Bandwidth (h)	0.49	1.07	1.86	0.58	0.91	1.87
Observations (n)	3,069	4,570	5,195	2,108	3,152	3,939
(E)	*Other social programs*			*No other social programs*		
Effect (τ)	−0.165***	−0.372***	−0.361***	−0.104**	−0.187**	−0.163**
	(0.070)	(0.123)	(0.130)	(0.052)	(0.085)	(0.078)
Bandwidth (h)	0.63	0.94	1.74	0.63	0.84	1.89
Observations (n)	3,135	3,963	4,595	3,024	3,492	4,511

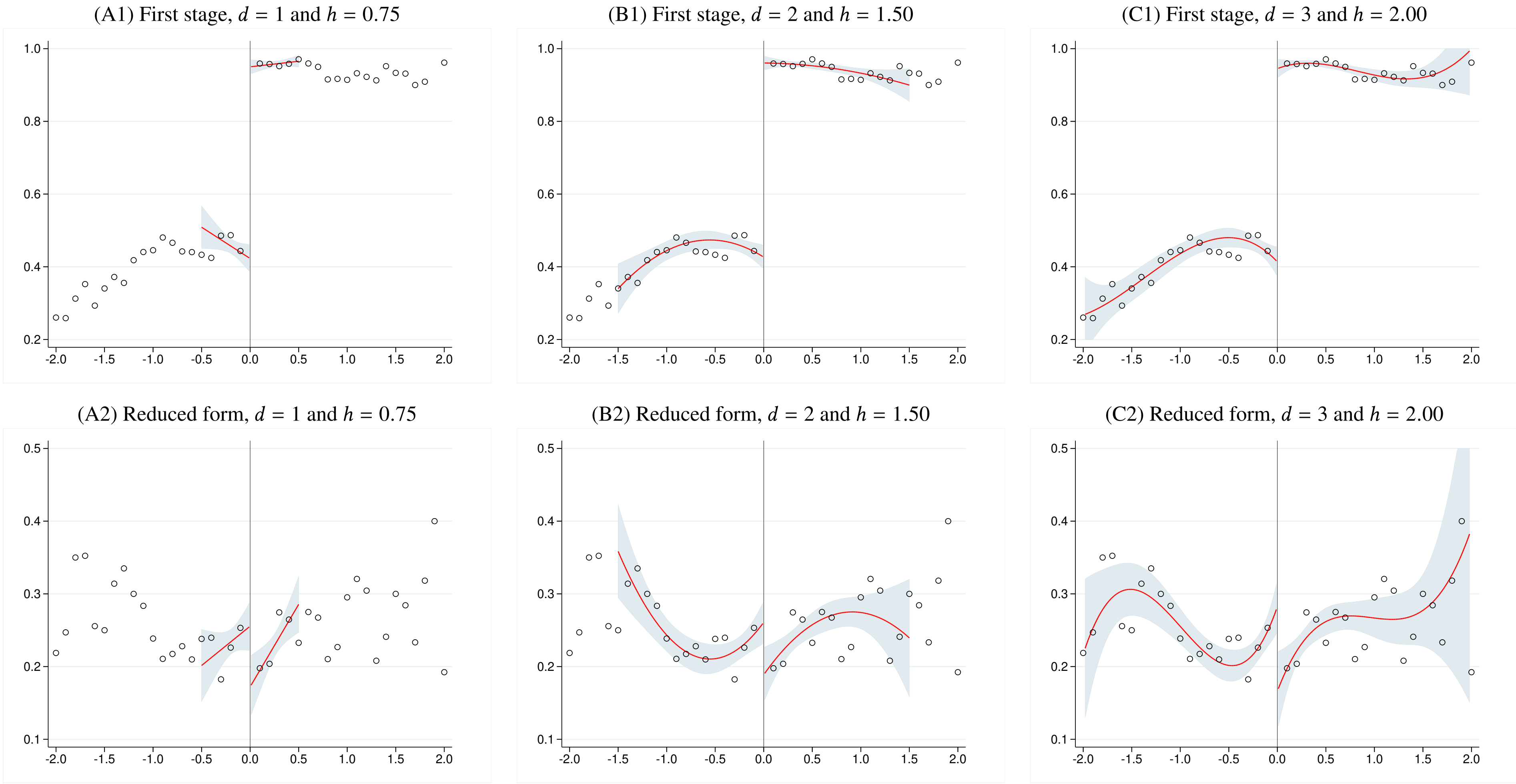
Source: ENAHOR 2014. Own elaboration.
Notes: IV estimates of the treatment effect on the use of IHCPs, τ in system (1) for various values of d and h, and for subsamples. Robust standard errors are in parentheses. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels respectively. All regressions include a flexible polynomial of degree d with interaction terms with T (the eligibility indicator) as in equation (2), and the control variables listed in the notes to Table 2. h is the bandwidth, chosen as in Calonico et al. (2014), and n is the number of observations such as $-h \leq X \leq h$, where X is the IFH.

Table 4. *Estimation of the effect of Juntos on the self-perception of health status*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) *Target population (direct effects)*								
No controls	0.283***	0.365***	0.272***	0.393***	0.454***	0.339***	0.320***	0.307***
	(0.076)	(0.090)	(0.079)	(0.115)	(0.121)	(0.099)	(0.104)	(0.095)
With controls	0.229***	0.298***	0.186***	0.325***	0.337***	0.311***	0.293***	0.287***
	(0.080)	(0.091)	(0.079)	(0.116)	(0.114)	(0.101)	(0.105)	(0.097)
Poly. degree (d)	1	1	1	2	2	2	3	3
Bandwidth (h)	0.46	0.36	0.55	0.84	0.70	0.98	1.59	1.79
Observations (n)	3,210	2,768	3,291	4,404	3,947	4,671	5,360	5,411
(B) *Non-target population (indirect effects)*								
No controls	0.156***	0.182***	0.143***	0.234***	0.233***	0.176***	0.203***	0.161***
	(0.039)	(0.040)	(0.036)	(0.053)	(0.066)	(0.042)	(0.046)	(0.043)
With controls	0.180***	0.198***	0.184***	0.265***	0.226***	0.182***	0.215***	0.147***
	(0.040)	(0.042)	(0.037)	(0.053)	(0.067)	(0.043)	(0.046)	(0.042)
Poly. degree (d)	1	1	1	2	2	2	3	3
Bandwidth (h)	0.42	0.38	0.46	0.69	0.54	1.03	1.59	1.98
Observations (n)	4,545	4,413	4,984	6,298	5,116	7,719	8,972	9,197

Source: ENAHOR 2014. Own elaboration.
Notes: IV estimates of the treatment effect on the use of IHCPs, τ in system (1), for various values of d and h.
Robust standard errors are in parentheses. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels respectively. See notes to Table 2.

Figure 1. *Regression functions for the non-target population*



Source: ENAHOR 2014. Own elaboration.
Notes: The estimates of the regression functions of the first stage (i.e., a regression of D on T and a flexible function of X) are in panels A1, B1, and C1, and the reduced form (i.e., a regression of Y on T and a flexible function of X) are in panels A2, B2, and C2 for various values of d and (approximately optimal) h. In all panels, the horizontal axis is the IFH, whose cut-off point of zero is marked by a vertical line; in panels A1, B1, and C1, the vertical axis is the proportion of households treated by the *Juntos* program; and in panels A2, B2, and C2, the vertical axis is the proportion of individuals using an IHCP. The IV estimates of Table 2 (with no controls) are given by the distance between the two segments of the regression function at zero in panels A2, B2, or C2 that are divided by the corresponding distance in panels A1, B1, or C1. The shaded areas are 95% confidence intervals. The circles are averages of D or Y across 40 intervals (20 below zero, 20 above zero) of X, each of width 1/20.

Appendix (not intended for publication)

Table 5. *Additional estimation results of the Juntos effect*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) *Target population (direct effects)*								
No controls	-0.213^{**}	-0.289^{***}	-0.197^{***}	-0.219	-0.255^{*}	-0.281^{***}	-0.352^{***}	-0.246^{**}
	(0.095)	(0.103)	(0.083)	(0.137)	(0.141)	(0.108)	(0.135)	(0.123)
With controls	-0.318^{***}	-0.406^{***}	-0.259^{***}	-0.369^{***}	-0.368^{***}	-0.335^{***}	-0.436^{***}	-0.328^{***}
	(0.096)	(0.103)	(0.096)	(0.126)	(0.139)	(0.111)	(0.138)	(0.125)
Poly. degree (d)	1	1	1	2	2	2	3	3
Bandwidth (h)	0.48	0.39	0.58	0.77	0.63	1.12	1.63	1.97
Observations (n)	2,567	2,251	2,701	3,210	3,007	3,841	4,255	4,333
(B) *Non-target population (indirect effects)*								
No controls	-0.154^{***}	-0.161	-0.139^{***}	-0.154^{*}	-0.207^{*}	-0.156^{***}	-0.161^{*}	-0.212^{***}
	(0.059)	(0.103)	(0.060)	(0.087)	(0.117)	(0.066)	(0.095)	(0.071)
With controls	-0.187^{***}	-0.295^{***}	-0.180^{***}	-0.235^{***}	-0.295^{***}	-0.188^{***}	-0.254^{***}	-0.246^{***}
	(0.062)	(0.102)	(0.062)	(0.085)	(0.112)	(0.068)	(0.096)	(0.071)
Poly. degree (d)	1	1	1	2	2	2	3	3
Bandwidth (h)	0.44	0.32	0.51	0.68	0.56	1.06	1.34	1.98
Observations (n)	3,951	2,885	4,161	5,083	4,262	6,424	6,984	7,466

Source: ENAHOR 2014. Own elaboration.
Notes: IV estimates of the CCT program effect on using an IHCPs, when the sample is conditioned on the attendance to medical care. Robust standard errors are in parentheses. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels respectively. See notes to Table 2.

Table 6. *Additional results on heterogenous effects of the Juntos program*

Polynomial degree (d)	1	2	3	1	2	3
(A)	*Land ownership*			*No land ownership*		
Effect (τ)	−0.029	−0.110	−0.049	−0.277***	−0.519***	−0.463***
	(0.071)	(0.094)	(0.096)	(0.082)	(0.142)	(0.126)
Bandwidth (h)	0.61	0.86	1.69	0.62	0.96	1.85
Observations (n)	2,490	3,425	4,126	2,148	2,677	3,245
(B)	*Literate*			*Iliterate*		
Effect (τ)	−0.170	−0.248	−0.214	−0.181***	−0.234***	−0.252***
	(0.127)	(0.173)	(0.148)	(0.071)	(0.095)	(0.084)
Bandwidth (h)	0.52	0.63	1.72	0.52	0.68	1.81
Observations (n)	1,311	1,631	2,522	2,854	3,419	4,859
(C)	*Male*			*Female*		
Effect (τ)	−0.168	−0.324**	−0.307**	−0.166**	−0.250***	−0.236**
	(0.104)	(0.146)	(0.149)	(0.076)	(0.100)	(0.115)
Bandwidth (h)	0.53	0.77	1.39	0.53	0.75	1.44
Observations (n)	1,816	2,363	3,063	2,355	3,045	4,008
(D)	*Children younger than 7 years*			*Children older than 7 years*		
Effect (τ)	−0.217***	−0.385***	−0.327***	−0.112*	−0.174*	−0.151
	(0.090)	(0.130)	(0.108)	(0.064)	(0.103)	(0.097)
Bandwidth (h)	0.46	0.84	1.80	0.64	0.93	1.86
Observations (n)	2,464	3,408	4,210	2,123	2,603	3,195
(E)	*Other social programs*			*No other social programs*		
Effect (τ)	−0.229***	−0.381***	−0.390***	−0.104*	−0.203**	−0.175*
	(0.097)	(0.141)	(0.140)	(0.061)	(0.092)	(0.090)
Bandwidth (h)	0.61	0.86	1.69	0.62	0.96	1.85
Observations (n)	2,286	3,102	3,737	2,458	2,994	3,641

Source: ENAHOR 2014. Own elaboration.
Notes: IV estimates of CCT program effect on using an IHCPs, conditional on the attendance to medical care, for various subgroups. Robust standard errors are in parentheses. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels respectively. See notes to Table 3.