The Effects of Conditional Cash Transfers on Social Engagement and Trust in Institutions

Evidence from Peru's Juntos Programme

Luis A. Camacho

The effects of conditional cash transfers on social engagement and trust in institutions

Evidence from Peru's Juntos Programme

Luis A. Camacho

Bonn 2014

Dr. Luis A. Camacho, Department III: Governance, Statehood, Security, German Development Institute / Deutsches Institut für Entwicklungspolitik (DIE), Researcher

Email: luis.camacho@die-gdi.de

Acknowledgements

Bonn, July 2014   Luis A. Camacho

Abstract

Conditional cash transfer (CCT) programmes have rapidly expanded throughout the developing world. These programmes have been relatively effective at improving living standards and producing behavioural changes expected to improve human capital among beneficiary children. Little is known, however, about the effect of CCTs on outcomes other than those they intend to modify but which may nonetheless be important for achieving these programmes' longer-term objectives and improving development prospects more generally. This paper addresses this gap by evaluating the effects of CCTs on membership in social organisations and trust in institutions, using evidence from Peru's Juntos programme. The analysis employs household- and district-level data and a difference-in-differences strategy to estimate the programme effects, focusing on the 133 districts in which the programme was implemented during 2006. Its effects among both eligible and non-eligible households are estimated. The programme does not have effects on membership in social organisations on either group of households. It increases trust in institutions related to programme conditions among the eligible population, but it decreases trust in the ombudsman's office, a public institution that has channelled grievances arising from exclusion from the programme. While the results regarding eligible households are encouraging news for proponents, those pertaining to non-eligible households reveal a negative unintended consequence that should be a source of concern.

Contents

Abbreviations

CCT	Conditional cash transfer
DD	Difference-in-differences
ENAHO	Encuesta Nacional de Hogares (National Household Survey)
GDP	Gross domestic product
INEI	Instituto Nacional de Estadística e Informática (National Institute of Statistics and Informatics)
ITT	Intent to treat
OLS	Ordinary least squares
PCA	Principal components analysis
RENIEC	Registro Nacional de Identificación y Estado Civil (National Registry of Identification and Marital Status)
USD	United States Dollars

1   Introduction

Since they were first introduced in Brazil and Mexico in the 1990s, conditional cash transfer (CCT) programmes have quickly expanded throughout the developing world, especially in Latin America and the Caribbean. These programmes give cash payments to poor households under the condition that they comply with a set of requirements aimed at developing their children's human capital, such as attending school, receiving vaccinations, and getting regular health check-ups. In this way, CCTs seek to reduce poverty while also attempting to break its inter-generational transmission.

Many studies have shown that CCTs have been effective at reducing poverty as well as at inducing behavioural changes expected to improve children's human capital, especially when it comes to education (e.g., Bourguignon / Ferreira / Leite 2003; Rawlings / Rubio 2005; Schady / Araujo 2008; Stampini / Tornarolli 2012). In addition, there is some preliminary evidence suggesting that CCTs may also have longer-term positive impacts on income or economic vulnerability.[1] Despite all the scholarly interest that CCTs have generated, little is known about the effects they may have on outcomes other than those directly linked to the programmes' outcomes of interest, but that may nonetheless be important for achieving these programmes' longer-term objectives and improving development prospects, more generally.

In particular, there is a dearth of studies analysing CCTs' social and political effects. Social effects have only been explored by a few studies based on in-depth interviews and focus groups (e.g., Adato 2000; Adato / Roopnaraine 2004; Huber et al. 2009). These studies document that targeting of CCTs can have divisive consequences in close-knit, poverty-stricken communities. Although initial division between beneficiaries and non-beneficiaries sometimes gives way to solidarity between them, overall these studies suggest a bleak future for social ties that cut across eligibility lines. They do, however, offer optimism regarding CCT programmes' potential to enhance social engagement among beneficiaries.

When it comes to political effects, existing studies have focused on turnout and electoral preferences (e.g., De La O 2013; Zucco 2013). State–society relations have been neglected, despite good reasons to expect CCT programmes to have consequences in this regard by providing benefits to citizens who are mostly neglected by traditional social protection systems. Yet, any potential improvements in the links between the state and programme beneficiaries is likely to come at a cost, as a result of the exclusion of other citizens who consider themselves poor and deserving of assistance. Indeed, the above-mentioned studies of the consequences of CCTs on social relations also document that targeting of CCT results in negative feelings among non-beneficiaries, although they do not explore whether more critical views of the state accompany these feelings.

This paper addresses the identified gaps by evaluating the effects of Peru's Programa Nacional de Apoyo a los más Pobres Juntos (National Programme to Support the Poorest Together; hereafter referred to as "Juntos") on social engagement and trust in governmental and political institutions. The first outcome is important to the extent that it contributes to

---

1   See Fiszbein / Schady (2009, 123–124) for a review of this evidence.

social cohesion and the building up of individuals' social capital stocks. As such, it can play an important role in enhancing welfare, helping the poor not only to get by in the short run, but also to overcome and stay out of poverty in the long run (Narayan 1999). Trust in governmental and political institutions is an indicator of the quality of state–society relations. It is important for development prospects because it enhances governance by providing incumbents with leeway to enact long-term oriented policies, and making citizens more willing to comply with governmental demands and regulations (Gamson 1968; Levi 1998).

The Juntos programme targets poor households in poor districts that have one or more children up to 14 years old or a pregnant woman. It imposes conditions related to identity registration, health, and education. The programme's intent-to-treat (ITT) effects are estimated using household-level data from a national living standards measurement survey complemented with administrative records and a difference-in-differences strategy. The analysis focuses on a cohort of 133 districts in which the programme was implemented prior to or during 2006, and examines the programmes' short-term effects among both eligible and non-eligible households.

The results indicate that the programme had no effects on social engagement among either group of households. These findings suggest that fears of CCTs having negative consequences on intra-community relations as well as optimism regarding their potential to enhance social engagement may have been premature. The programme did have effects on trust. Among eligible households, it increased trust in government institutions directly related to conditions. Among non-eligible households, the programme resulted in decreases in trust in the ombudsman's office, which channelled grievances arising from perceptions of unfairness in the selection of beneficiaries, but was not able to influence ultimate programme enrolment. While the results regarding eligible households are encouraging news for proponents, those pertaining to non-eligible households reveal a negative unintended consequence that should be a source of concern.

The remainder of the paper is organised as follows. The second section discusses theoretical expectations and available evidence about the effects of social programmes, in general, and conditional cash transfers, in particular, on social engagement and trust in institutions. The third section introduces the main characteristics of Juntos, including the targeting strategy the programme used to identify eligible households in its first years of operation. The data, estimation strategy, and outcome variables are discussed in the fourth section. The fifth section presents and discusses the results. The final section concludes with a summary of the findings and a discussion of their implications for theory and policy-making.

2   Links between social programmes, social engagement, and trust in institutions

At a general level, there are two opposing views about the consequences of social programmes on social engagement (Woolcock 1998, 156–158). On the one hand, it has been argued that the larger the welfare state, the less space there is for autonomous associations. Specifically, social programme expansion has been credited with crowding out mutual solidarity associations and undermining the reciprocity that they facilitate. On

the other hand, there is the view that welfare state enlargement has provided an ideal environment for new forms of associations to develop. In particular, social programmes provide resources and incentives that can result in the creation of organised interest groups (Pierson 1993). The evidence seems to side with the second view, with the proliferation of non-profits and voluntary associations set up to provide welfare services on behalf of the state, as well as organisations of service users and beneficiaries providing cases in point (e.g., Skocpol 1995; Salamon 1995).

Evidence suggests that CCTs provide opportunities for beneficiaries to develop or strengthen ties among themselves. In the case of Mexico's Progresa, for example, bonds between participants have been strengthened by participation in monthly meetings, health education talks, and community work activities (Adato 2000, 44–45). In the specific case of Juntos, one study indicates that mothers of beneficiary households tend to form organisations that provide opportunities for interaction as well as support for members in the event of unexpected shocks such as illness (Arroyo Laguna 2010, 73). Yet, these opportunities can be a double-edged sword for social ties: since participation is in most cases restricted to those receiving transfers, they can result in divisions across eligibility lines, severing ties between beneficiaries and non-beneficiaries within intervened communities.

Indeed, the available evidence also suggests that targeting can have adverse effects on social engagement, especially in close-knit communities in which poverty is prevalent. Studies of Progresa and Nicaragua's Red de Protección Social have documented sentiments of envy, resentment, and sadness among those not receiving subsidies, mostly because they consider themselves poor and deserving of government assistance (Adato 2000; Adato / Roopnaraine 2004). These studies also report specific instances in which existing social ties have weakened, because beneficiaries and non-beneficiaries see themselves as having less in common, and non-beneficiaries refuse to take part in community activities.

It is worth noting, however, that negative sentiments were not always directed toward transfer recipients, with non-beneficiaries often attributing their exclusion to bad luck and hoping to receive assistance in the future. In several of these cases, solidarity across groups was often also reported, with communities getting together to help those perceived of as needy but who had failed to qualify for the programme. This suggests that targeting and the subsequent response to it could also bring communities together and thus create opportunities for strengthening intra-community relations (Adato 2000, 26–30; Adato / Roopnaraine 2004, 78–79).

Another important reason to expect CCTs to have negative effects on social engagements is that means testing often leads to the stigmatisation of programme participants by the general population as well by the non-beneficiaries in their communities.[2] For example, beneficiaries of social assistance programmes are often viewed as lacking the will to get ahead without public assistance, or are thought to engage in legally or morally questionable strategies to qualify for programmes. Stigmatised individuals may not only

---

2   Goffman (1963, 3) defines stigma as an "attribute that is deeply discrediting" to individuals or groups of individuals and that reduces bearers from being "whole or usual" persons to being "tainted" or "discounted".

be rejected or excluded from social interactions but also refrain from engaging others due to low self-esteem.[3] In sum, stigmatisation can contribute to severing existing social ties and, perhaps more importantly, to hindering the development of new ones.

Some evidence suggests that CCTs may indeed lead to the stigmatisation of beneficiaries. A study based on a nationally representative survey in Argentina finds that a large majority of the population thinks that beneficiaries of cash transfers "lie to receive a plan", that "many of those who receive plans could find a job if they really wanted to", and that plans lead to people "not going out to look for jobs", "not knowing how to solve their problems", and "becoming less prone to solidarity" (Cruces / Rovner 2008, 73–74).[4] One study about Juntos documented the stigmatisation of beneficiaries in their own communities, identifying three "myths" that were prevalent among those not receiving transfers: beneficiaries "do not want to work anymore" and "become lazy"; women get pregnant to qualify for the programme or to remain enrolled; and beneficiaries do not use the transfers appropriately, spending them on alcohol (Huber et al. 2009, 100–101).

Finally, it is worth noting that CCTs may have positive effects on social engagement via two additional routes. First, conditions imposed on beneficiaries can open additional opportunities for engagement, such as joining and participating in parent associations in schools. Second, increases in income (and social status) associated with transfers may result in higher engagement, given the positive association that has been documented between the two (e.g., Verba / Schlozman / Brady 1995). Beyond defraying the costs associated with membership or active participation in organisations, CCTs may have a positive effect by encouraging recipients to interact with others with whom they have had little or no prior contact or by increasing the demand for leisure activities.

As in the case of social engagement, there are reasons to expect conditional cash transfers to have positive and negative effects on individuals' trust in governmental and political institutions. Trust is considered to be the product of performance, although the connection between the two is not synchronic and likely flows in both directions: building trust requires sustained competent performance, while reservoirs of trust enable government to act more effectively and pursue long-term oriented policies (Hetherington 1998). Importantly, performance on its own is generally not enough to generate trust; to do so, it must be accompanied by the perception that government is acting in the interests of the people and following fair and impartial procedures (Levi 1998).

In the case of social programmes, it has been argued that individuals are more likely to believe that they have received fair and equal treatment after interacting with

---

3　The stigmatisation of those receiving public assistance is a widespread phenomenon. In the United States, for example, Katz (1990) identifies two images of poverty that were constructed in that country in the 1970s and 1980s: single mothers and African-Americans living in inner-city ghettos. Public opinion easily labelled these groups as "undeserving poor", because they appeared foreign to the rest of society. In Europe, several studies have also documented stigmatisation. In England, Spicker (1984) identifies various stigmas, including labelling those receiving public assistance as sick and incapacitated, mentally ill, and lacking morals. Colton et al. (1997) identify numerous stigmas attributed to mothers benefitting from social assistance programmes in England, Holland, and Spain.

4　"Plan" is the term used by Argentines to refer to the various cash transfer and workfare programmes introduced in that country since the economic crisis of the early 2000s.

bureaucracies administering universal programmes. In turn, perceptions of fair and equal treatment not only increase trust in political and state institutions but also trust in others. This would not be the case with selective, targeted programmes because they leave room for bureaucratic discretion in determining eligibility, at least in the eyes of those applying for benefits (Kumlin / Rothstein 2005; Rothstein / Stolle 2008). Interacting with such programmes may lead individuals to believe that public service bureaucrats are not trustworthy and that those in need have to engage in questionable behaviour – e.g., cheating and distorting information – to make sure they get the benefits they feel entitled to (Kumlin / Rothstein 2005, 349).

While this argument is clear about why one should expect universal programmes to increase trust more than selective programmes, it is not clear about whether the latter can be more beneficial for trust than no programmes at all. Selective programmes should have a positive effect on trust among beneficiaries because of the positive affect resulting from receiving government assistance and attention. This is even more likely in the case of CCTs, given that these target populations who have generally not benefited from social programmes in the past. This positive affect should offset any negative affect that may arise from observing that the programme has excluded other community members who are deserving of assistance. Among this last group, trust is likely to decrease as a result of feelings of exclusion. This is especially likely in the case of CCTs because they target communities in which all or most residents consider themselves to be poor.

Although there is no evidence about the effects of CCTs on trust in governmental and political institutions, studies do report positive feelings as a result of inclusion in such programmes and the opposite as a result of exclusion. Feelings of gratitude toward government were reported among beneficiaries of the Juntos programme (Jones / Vargas / Villar 2007, 67). In the case of Progresa, non-beneficiaries were reported to blame the government – rather than beneficiaries – for not receiving subsidies and to complain about a lack of support and recognition from government (Adato 2000, 25, 38).

Altogether, this discussion suggests that CCT programmes are likely to have differentiated effects among beneficiaries and non-beneficiaries in intervened communities. It is hard to make predictions about social engagement because there are good reasons to expect effects in both directions. First, targeting may sever ties between beneficiaries and non-beneficiaries, but it could well result in increased solidarity and interaction across these groups. Second, stigmatisation could decrease social engagement opportunities for beneficiaries. Finally, arguments related to programme conditions and income increases suggest a positive effect on social engagement among beneficiaries. Expectations when it comes to trust in governmental and political institutions are clearer. CCTs are likely to have a positive effect among beneficiaries and a negative effect among non-beneficiaries.

3 An overview of the Juntos programme

Juntos targets poor households in poor districts with one or more children up to 14 years old or a pregnant woman. In exchange for a bimonthly monetary incentive of about

USD 78,[5] beneficiary households commit to fulfilling a series of obligations related to identity, health, and education. The programme was introduced in 2005 and originally implemented in 70 districts. Since then, the geographical coverage of the programme has gradually expanded, reaching 1,011 districts by December of 2012. During the same period, the number of beneficiary households increased from 22,550 to 619,723 (Juntos 2013a; Juntos 2013b).

The programme selects eligible households in a two-stage process.[6] First, "geographical targeting" selects districts to be intervened according to a "poverty index". The first version of the index, which was used to select the initial 70 districts, combined the following four, equally weighted, indicators: (i) percentage of households with two or more unsatisfied basic needs; (ii) poverty gap; (iii) infant malnutrition rate; and, (iv) percentage of population settlements highly affected by political violence. This last indicator was included as one way to provide public compensation to communities affected by political violence during the 1980s and early-1990s. This index has been subsequently modified. The first change added one additional indicator to the index, the percentage of households living in extreme poverty, while continuing to weight all indicators equally. This version was used for the programme expansions in 2006, the cohort under study here.

Once districts are selected, the programme implements the "individual targeting" stage to identify eligible households. The specifics of individual targeting have changed throughout the years. Up until the 2007–08 expansions, the process involved carrying out censuses in all selected districts. After identifying households that have one or more children up to 14 years old or a pregnant woman, the collected data were used to classify households according to whether they were poor or not. This classification relied on the results of a logit regression model estimating the probability of a household being poor. Using the estimated coefficients and the socioeconomic data collected in the censuses, a probability of being poor was predicted and used as a score to classify households. Those with a score below a certain threshold were considered eligible and enrolled in the programme. The idea behind this proxy means targeting strategy was to identify poor households by using easy-to-collect information about household composition, human capital, dwelling characteristics, and asset ownership – and thus avoid collecting income or expenditure data.[7]

---

5   As a reference, in 2006, the minimum wage was about USD 150 and the monthly GDP per capita was about USD 275. The transfer is thus quite sizable for Peru's poorest households, which are the target group of the programme.

6   For a detailed description of the selection process, see Linares García (2009). The ensuing discussion draws heavily from this source.

7   The predictors of the probability of being poor were: (i) percentage of illiterate women in the household; (ii) percentage of members younger than 18 years old attending a regular educational course or programme; (iii) a dichotomous variable indicating whether the household has access to industrial fuels (gas, oil, and kerosene) for cooking; (iv) number of durable goods not available at home (out of predefined set); (v) number of public services (electricity, water, and sewage) available at home; and, (vi) a set of dichotomous variables identifying four categories of building materials used in the dwelling's floors, walls, and ceilings.

The implementation model of the programme also involves community validation meetings in which lists of eligible households are presented to those in attendance in order to avoid inclusion or exclusion errors. Despite their well-intended objective of incorporating community input in the targeting process, validation meetings often create opportunities for conflict and division to emerge. Indeed, those left out often push for inclusion in the programme, leaving meetings with hard feelings when they fail to achieve this. Moreover, the input of communities sometimes is very limited – although local authorities and promoters of the programme do seem to have a say in the targeting process at this stage – with several validation meetings turning out to be events at which beneficiary lists are merely shared with the local population (Huber et al. 2009, 46–49, 82–83).

4   Empirical strategy

The main source of data used is the Peruvian National Household Survey (ENAHO). The ENAHO is a nationally representative living standards measurement survey that collects information about household composition, income, and expenditures, and education and health status of household members, among other questions. It also contains items on membership in social organisations and trust in various institutions, which are used to construct the outcome variables for the analysis. The survey interviews about 20,000 households every year and is carried out by the National Institute of Statistics and Informatics (INEI). These data are complemented with administrative records from the Juntos programme and the Ministry of Economics and Finance regarding the programme's rollout dates by district and the index used for geographical targeting, respectively.

4.1   Estimation

The analysis estimates Juntos's effects in Peru's poorest districts, which are those in which the programme was first implemented. A difference-in-differences (DD) strategy is employed to estimate the intent-to-treat effect of the programme, that is, its effect among eligible households regardless of whether they were actually enrolled in the programme. The choice to estimate the ITT effect rather than the more common effect of the treatment on the treated is determined by data availability, as the ENAHO did not ask respondents whether they were Juntos beneficiaries during the first two years of the programme's operation, which is the period under analysis here. A side advantage of estimating the ITT effect is that it guards effect estimates against selection bias due to unobservable characteristics at the household level.[8]

---

8   For example, households that enrolled in the programme might differ from other eligible households in that they possess motivations that predispose them or skills that enable them to participate. If this were the case, eligible households would not constitute a good counterfactual for enrolled households, and the effect of the programme would be overestimated. In theory, selection bias should not be a major concern in the case of Juntos given that enrolment in the programme within targeted districts was the product of censuses. In practice, however, it is a concern given that the censuses' non-response rates were 35.2%, on average (Linares García 2009, 13).

Following Bertrand, Duflo, and Mullainathan (2004), the estimating equation for the model is:

$$Y_{ijt} = \alpha_j + \tau_t + \beta T_{jt} + v_{jt} + \varepsilon_{ijt}, \tag{1}$$

where i indexes households, j indexes districts, and t indexes time; Y_{ijt} is a given outcome variable corresponding to household i in district j at time t; α_j and τ_t are district and time-period fixed effects, respectively; T_{jt} is a dichotomous variable that takes the value of one in districts intervened by Juntos for the period after the programme was rolled out and zero otherwise; v_{jt} is a district-time-specific error; and ε_{ijt} is a household-specific error. The coefficient of the treatment variable, β, is the DD estimate of the ITT effect of Juntos; it captures the change in the outcome variable among eligible households in intervened districts after the rollout of the programme. Note that the counterfactual in this comparison is the group of eligible households in non-intervened districts and that all non-eligible households are excluded from the analysis.

A regression-adjusted version of Equation (1) can also be estimated to control for household-level characteristics and improve the precision of the estimates. In addition to the variables included therein, this specification adds a vector of observable household characteristics, X_{ijt}, with its corresponding vector of parameters, δ. The resulting equation is the following:

$$SC_{ijt} = \alpha_j + \tau_t + \beta T_{jt} + X'_{ijt}\delta + v_{jt} + \varepsilon_{ijt}. \tag{2}$$

Controlling for these observable household-level characteristics improves the precision of the estimate of β if these characteristics are correlated with T_j conditional on district and time fixed effects (Angrist / Krueger 1999, 1299). The controls included in the regressions below are an index of dwelling characteristics, an index of durable goods ownership, the head of household's age and its square, the head of household's year of schooling, and the number of household members.[9] Estimation results for both Equation (1) and Equation (2) are reported below.

The model's parameters are estimated using ordinary least squares (OLS). Given that the district-time-specific errors cannot be estimated with this method, cluster-robust standard errors are computed to address clustering and heteroskedasticity. This correction performs well when the number of clusters is relatively high – about 50 or more, but no less than 30 (Bertrand / Duflo / Mullainathan 2004; Cameron / Gelbach / Miller 2008) – as is the case here.

As discussed in the previous section, it is likely that CCTs also have an effect on social engagement and trust in institutions among non-beneficiary households. The ITT effect of Juntos on this group is estimated using Equation (1) and Equation (2), but employing only the sample of non-eligible households. In this analysis, the treatment group comprises households in intervened districts, while the control group consists of households in non-intervened districts.

---

9   Following Filmer and Pritchett (2001), the dwelling characteristics and durable goods ownership indices are expected to capture relative household wealth and are constructed using principal components analysis (PCA).

The analysis employs data from the 2004, 2005, and 2006 ENAHO surveys to estimate the effects of the programme in a cohort of 133 districts in which the programme was implemented prior to or during 2006. The focus on this single cohort of districts is motivated by two considerations. The first one is data availability. Given that the items gauging trust in institutions are available only from July 2004 onwards, the first twelve-month period of (pre-treatment) data available is July 2004–June 2005.

The other important consideration is identification. The key assumption to identify an effect using a DD strategy is that the over-time evolution of the variable of interest would have been the same in treated and non-treated districts had the treatment not been introduced – this is known as the common trends assumption. In the case of Juntos, one source of concern is that surveys conducted in the months immediately preceding rollout dates may have already captured some of the programme's effects on social engagement and trust in institutions. This is because preparatory activities like censuses and validation meetings were taking place therein. To guard against this possibility, a six-month buffer period between pre-treatment and post-treatment data is established. This makes calendar year 2006 the first twelve-month period of post-treatment data available for estimating the programme's effect. A second source of concern arises from the sequential implementation of the programme, as estimating effects for more than one cohort – or including more than a single cross-section of post-treatment data – would result in a decrease in the number of districts in the non-treated subsample and thus in a comparison of treated and non-treated districts that are progressively less similar.

Finally, to make the common trends assumption more plausible, the sample of non-intervened districts is restricted to the poorest ones according to the index used by the programme for geographical targeting. Only as many non-treated districts as needed to approximate the number of households in intervened districts are included. The final sample includes 133 treated and 101 non-treated districts. Treated districts comprise 2,827 households (1,330 eligible and 1,497 non-eligible); non-treated districts, 2,805 households (1,441 eligible and 1,364 non-eligible).

4.2   Outcome variables

The ENAHO includes an item that asks respondents whether they or any other member of their household belongs to a list of 16 organisations. Social engagement is measured with two variables that indicate the number of organisations to which at least one household member belongs. These variables reflect membership in organisations that are more likely to facilitate horizontal ties and vertical ties, respectively. Horizontal ties refer to those established by individuals within the boundaries of any given community; vertical ties, to those that cut across these boundaries, integrating individuals with diverse backgrounds, life experiences, and interests.[10] A list of the organisations included in each category is provided in the Annex.

---

[10]   Examples of organisations fostering horizontal ties would be those comprising individuals with similar economic status, place of origin, or religious creed. More encompassing collectives like peak unions and political parties would be examples of organisations fostering vertical ties. Whether a particular organisation fosters intra-community or inter-community ties is a relational matter, of course. All networks likely strengthen both types of ties to a certain degree.

This distinction is important because membership and participation in these organisations has been linked to the creation of different types of social capital. Bonding social capital is a product of horizontal ties and is particularly useful for the poor to get by, as it can provide resources for coping with everyday life and adverse economic shocks. Bridging social capital is a product of vertical ties and is essential for the poor to get ahead, as it can provide access to resources that are not available within poor communities (Woolcock 1998, 167–175; Woolcock / Narayan 2000, 230–234).

To measure trust in institutions, the survey asks respondents about the extent to which they trust several civil society, governmental, and political institutions. Responses to the trust items are recorded on a four-point scale that goes from "not at all" (0) to "a lot" (3). The programme's effects on the following institutions are considered: (i) state institutions that Juntos's beneficiaries are required to engage with due to conditions, namely the national office in charge of identity registration (RENIEC), the Ministry of Health and the Ministry of Education;[11] (ii) the national ombudsman's office (Defensoría del Pueblo); (iii) subnational governments (regional governments and municipalities); (iv) branches of government (legislative and judiciary); and, (v) political parties.

Descriptive statistics for all outcome variables before and after programme implementation for eligible and non-eligible households in intervened and non-intervened districts are presented in Table 1. The table also presents descriptive statistics for the household-level controls included in the regression-adjusted estimations.

5 Results

5.1 The effects of Juntos on social engagement

Figure 1 presents the estimates of Juntos's effects on membership in horizontal and vertical social organisations among both eligible and non-eligible households. The full regression results are presented in the Annex. The figure depicts the point estimates of the DD estimate of the intent-to-treat effect from models with and without household-level controls (the β coefficients in Equation (1) and Equation (2), respectively) along with 95% confidence intervals. Whenever these intervals cross the vertical line at zero, the estimated effects are not significantly different from it. As the figure shows, the effects of the programme on membership in either type of social organisations among both eligible and non-eligible households are not significant.

Despite good reasons to expect CCTs to have either positive or negative effects on social engagement among beneficiaries and non-beneficiaries, Juntos seems to have none. In turn, this finding can be interpreted in two ways that are not necessarily exclusive. First, it is possible that the specific effects associated with the various causal mechanisms cancel each other out to produce no overall effect. Indeed, as discussed earlier, targeting may sever ties between beneficiaries and non-beneficiaries, but it could well result in increased solidarity and interaction across these groups; stigmatisation could decrease social engage-

---

11 The prompt for the Ministry of Health also mentions public health clinics and hospitals; the one for the Ministry of Education, public schools.
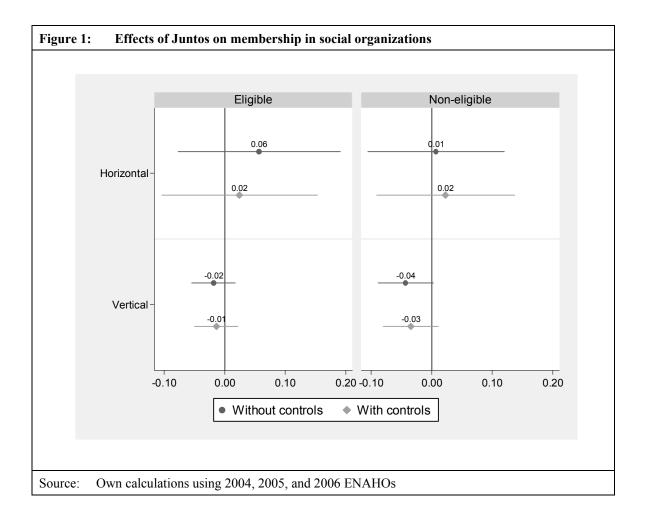
Table 1: Descriptive statistics of outcome variables and household-level control covariates

| | Intervened districts | | | | | | Non-intervened districts | | | | | |
| | Pre-implementation | | | Post-implementation | | | Pre-implementation | | | Post-implementation | | |
	No.	Mean	St. Dev.	No.	Mean	St. Dev.	No.	Mean	St. Dev.	No.	Mean	St. Dev.
1. Elegible population												
Number of organisations												
Horizontal social organisations	671	0.03	0.18	643	0.02	0.14	696	0.10	0.31	732	0.10	0.31
Vertical social organisations	671	0.76	0.72	643	0.74	0.65	696	0.75	0.78	732	0.66	0.70
Trust in institutions												
Institutions related to Juntos's conditions	617	1.41	0.71	575	1.43	0.67	660	1.60	0.70	671	1.48	0.68
National ombusdman's office	412	0.99	0.87	344	0.89	0.79	516	1.20	0.87	473	1.10	0.86
Political parties	520	0.54	0.72	509	0.47	0.65	618	0.61	0.75	618	0.58	0.69
Subnational goverments	585	0.94	0.73	557	0.96	0.68	649	1.00	0.70	644	0.98	0.71
Branches of government	558	0.81	0.74	502	0.76	0.70	614	0.89	0.73	618	0.83	0.71
Household-level controls												
Index of dwelling characteristics	678	-1.83	0.84	652	-1.90	0.78	700	-2.02	0.75	741	-2.10	0.77
Index of durable goods ownership	676	-1.70	0.84	651	-1.85	0.74	697	-1.73	0.75	740	-1.71	0.88
Head of household's age	678	46.08	13.96	652	46.61	13.54	700	45.80	13.75	741	46.45	13.72
Head of household's years of schooling	678	3.61	3.50	652	3.32	3.58	700	3.93	3.70	741	3.85	3.63
Number of household members	678	5.70	2.03	652	5.74	2.08	700	6.21	2.30	741	5.98	2.20
2. Non-elegible population												
Number of organisations												
Horizontal social organisations	721	0.06	0.25	765	0.02	0.15	673	0.10	0.32	677	0.12	0.33
Vertical social organisations	721	0.50	0.67	765	0.49	0.63	673	0.63	0.70	677	0.56	0.70
Trust in institutions												
Institutions related to Juntos's conditions	637	1.39	0.72	654	1.34	0.69	631	1.44	0.72	624	1.35	0.71
National ombusdman's office	475	1.10	0.90	455	0.87	0.81	489	1.10	0.88	467	1.07	0.88
Political parties	580	0.55	0.73	609	0.44	0.64	589	0.52	0.69	587	0.55	0.70
Subnational goverments	630	0.95	0.69	646	0.88	0.63	624	0.88	0.70	608	0.86	0.67
Branches of government	579	0.75	0.68	607	0.74	0.67	593	0.77	0.73	580	0.73	0.70
Household-level controls												
Index of dwelling characteristics	724	-1.59	0.99	773	-1.69	1.04	679	-1.79	0.89	685	-1.82	0.88
Index of durable goods ownership	723	-1.49	1.18	773	-1.64	1.07	678	-1.54	1.01	683	-1.46	1.15
Head of household's age	724	50.80	18.00	773	51.47	17.67	679	49.32	17.01	685	48.71	17.32
Head of household's years of schooling	724	4.85	4.59	773	4.72	4.57	679	4.74	4.54	685	5.13	4.40
Number of household members	724	3.19	2.06	773	3.22	2.06	679	3.50	2.08	685	3.68	2.05

Source: Own calculations using 2004, 2005, and 2006 ENAHOs

ment opportunities for beneficiaries; and arguments related to programme conditions and income increases suggest a positive effect on social engagement among this group.

A second possibility is that the results are the product of an inadequate measurement strategy. Specifically, formal membership in organisations could be a limited measure of social engagement in contexts characterised by poverty and in which most interactions may take place through informal channels. Given data limitations, employing more comprehensive strategies to identify the programmes' specific effects through alternative mechanisms or to measure social engagement is precluded.

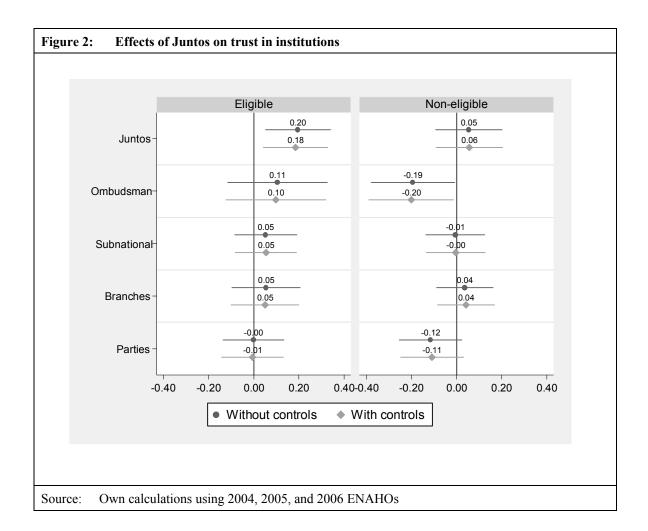## 5.2 The effect of Juntos on trust in institutions

Figure 2 presents the estimates of the programme's effects on trust in various governmental and political institutions. As before, the full regression results are presented in the Annex. The programme has a positive and significant effect among eligible households on trust in institutions with which beneficiaries are required to engage due to

Figure 1:   **Effects of Juntos on membership in social organizations**

Source: Own calculations using 2004, 2005, and 2006 ENAHOs

conditions: the national office in charge of identity registration, the Ministry of Health, and the Ministry of Education. The effect is robust to controlling for household-level covariates and it is also substantially important. Given that the pre-intervention mean trust in these institutions among the eligible was 1.41 points, the effect amounts to a 13.12% increase (according to the results from the model that includes household-level controls). The programme has no significant effects on trust in other institutions among eligible households.

In the case of the non-eligible households, Juntos has a negative and significant effect on trust in the national ombudsman's office, which is robust to controlling for household-level covariates. According to the results from the model that includes control covariates, and given that the pre-intervention mean trust in this office among the non-eligible was 1.10 points, the effect amounts to an 18.22% decrease. Negative feelings due to not being benefited by the programme are likely to begin the causal chain explaining this negative effect. These feelings may have in turn prompted them to file complaints against the programme through the ombudsman's office, something that has indeed happened in the past (e.g., Defensoría del Pueblo 2011). Finally, animosity toward and distrust in this office may have resulted from its inability to enrol those who do not qualify for the programme. This result is particularly important because it provides support for the argument that targeting can lead citizens to have disappointing interactions with state institutions and to a corresponding decline in trust (Kumlin / Rothstein 2005; Rothstein / Stolle 2008).

Figure 2:   Effects of Juntos on trust in institutions

Source: Own calculations using 2004, 2005, and 2006 ENAHOs

The programme has no significant effects on trust in other institutions among non-eligible households. Although not significant at conventional levels, effects on trust in political parties are worth discussing to the extent that they are substantially important and consistent with the argument that was just developed to explain the effect on trust in the ombudsman's office. While the estimated effect from the model that includes household-level control is only significant at the 86% level, it amounts to a 19.58% decrease in trust, given that the pre-intervention mean trust in this office among the non-eligible was 0.55 points. This effect could again be attributed to targeting and the negative feelings it creates among this group. These may have been channelled not only into making complaints against the programme through the ombudsman's office, but also into assigning blame to political parties for a perceived unjust situation. Politicians and parties are often the most recognisable actors associated with governmental institutions and they are also among the least trusted institutions. This makes them easy scapegoats for potential resentment arising from programme exclusion.

In sum, Juntos has effects on trust in institutions with which beneficiaries and non-beneficiaries have interacted as a consequence of programme implementation. In the case of beneficiaries, interactions with institutions required by the programme's conditions can be presumed to be satisfactory and thus result in the creation of trust. In the case of beneficiaries, interactions with the ombudsman's office are likely motivated by feelings of exclusion from the programme and result in diminished trust as a consequence of the

office's inability to change this situation. These findings support the argument that trust depends on actual performance as well as on whether governmental behaviour is perceived to follow fair and impartial procedures (Levi 1998); in this case, the payment of a cash transfer and the rules used to determine programme eligibility, respectively. Importantly, feelings toward these institutions or the programme itself have not been channelled toward more distant institutions, the only possible exception being political parties. This is likely due to the fact that those in contact with the programme view it as a very specific initiative of the incumbent government, rather than as a long-term oriented public policy.

6   Conclusion

CCTs have rapidly expanded throughout the developing world. These programmes have been relatively effective at improving living standards and producing behavioural changes expected to improve human capital among beneficiary children. However, to date, little is known about the effects of CCTs on outcomes beyond those that they intend to modify. This paper set out to explore the potential effect of Peru's Juntos programme on two such outcomes: social engagement measured as membership in social organisations and trust in governmental and political institutions.

The analysis shows that the programme has no effects on membership in social organisations among eligible or non-eligible households. The programme does, however, have a positive effect on trust in institutions with which beneficiaries are required to interact due to programme conditions among the eligible, as well as a negative effect on trust in the ombudsman's office among the non-eligible. Juntos has no effects on trust in other institutions.

Before discussing the implications of these findings, it is important to acknowledge some limitations of the analysis. First, given that the analysis examines a cohort of districts in which the programme was implemented at an early stage, and uses a single cross-section of post-treatment data, it is only able to identify the programmes' short-term effects on households in the poorest districts. It is certainly possible that the programme may have different effects in the long run, or that the observed short-term effects only take place in very poor districts. A second limitation arises from the inability to analyse the specific effects associated with the various mechanisms linking CCTs and social engagement and to employ additional measures of social engagement or state–society relations.

However limited, the analysis has at least three important implications. First, the results suggest that fears of CCTs having negative consequences on intra-community relations may have been premature. They also suggest that the stigmatisation of programme beneficiaries may be limited to rhetoric and not accompanied by their exclusion from social organisations. In addition, the findings do not support the arguments of sceptics of state expansion about social programmes crowding out mutual solidarity associations. While these are all positive news, it would have been more encouraging to find that the programme has positive consequences for social engagement, at least among its beneficiaries.

Second, and perhaps more importantly, the paper indicates that CCTs have unintended consequences on trust in institutions with which households have to engage as a result of

the programme. While increases in trust provide additional reasons to justify and promote these programmes, decreases in trust should be a source of concern, especially in contexts like those in which CCTs are implemented, where trust is already low and links between the state and society are weak. In the specific case of Peru, decreases in trust in the state institution that is in charge of safeguarding citizens' rights could be particularly damaging. Avoiding these negative unintended effects should be an additional consideration for policy makers and programme officials to weigh when making decision about targeting strategies. Echoing other studies' recommendations (e.g., Adato 2000, 46–47; Adato / Roopnaraine 2004, 84–85; Huber et al. 2009, 106), abandoning household targeting within the poorest communities or adopting mechanisms to improve this procedure should be considered.

Finally, the findings regarding trust in institutions provide evidence about the effects of government performance on state–society relations. They suggest that social programmes create opportunities for interaction between citizens and governmental institutions, which can in turn shape citizens' views about these institutions. Whether or not these interactions translate into positive or negative assessments depends on actual performance – e.g., the payment of a cash transfer – as well as on whether observed governmental behaviour is perceived to adhere to principles of fairness and impartiality (Levi 1998). It remains to be seen whether CCTs can shape citizens' views about institutions beyond those with which they have to interact or about the state as a whole. If anything, these more general effects are likely to materialise in the long run and only to the extent that citizens view these programmes as initiatives aimed at fulfilling the state's end of the social contract.

Bibliography

Adato, M. (2000): El impacto de Progresa sobre las relaciones sociales en la comunidad, Final report, International Food Policy Research Institute (IFPRI)

Adato, M. / T. Roopnaraine (2004): A social analysis of the Red de Protección Social (RPS) in Nicaragua, Final report, International Food Policy Research Institute (IFPRI)

Angrist, J. D. / A. B. Krueger (1999): Empirical strategies in labor economics, in: *Handbook of Labor Economics* 3, 1277–1366

Arroyo Laguna, J. (2010): Estudio cualitativo de los efectos del Programa Juntos en los cambios de comportamiento de los hogares beneficiarios en el distrito de Chuschi : avances y evidencias, Lima: Presidencia del Consejo de Ministros, Programa Nacional de Apoyo Directo a los Más Pobres, Juntos

Bertrand, M. / E. Duflo / S. Mullainathan (2004): How much should we trust differences-in-differences estimates?, in: *Quarterly Journal of Economics* 119 (1), 249–275

Bourguignon, F. / F. Ferreira / P. Leite (2003): Conditional cash transfers, schooling, and child labor : micro-simulating Brazil's Bolsa Escola Programme, in: *World Bank Economic Review* 17 (2), 229–54

Cameron, C. / J. Gelbach / D. Miller (2008): Bootstrap-based improvements for inference with clustered errors, in: *Review of Economics and Statistics* 90 (3), 414–427

Chiarella, C. (2010): Efectos del capital social en una sociedad heterogénea : una evaluación para el caso peruano, Final research report, Consorcio de Investigación Económica y Social (CIES) and Grupo de Análisis para el Desarrollo (GRADE)

Colton, M. et al. (1997): Stigma and social welfare : an international comparative study, Brookfield, VT: Ashgate

Cruces, G. / H. Rovner (2008): Los programas sociales en la opinión pública : resultados de la Encuesta de Percepción de Planes Sociales en la Argentina, in Guillermo Cruces et al., comp. Los programas sociales en Argentina hacia el Bicentenario: visiones y perspectivas, Buenos Aires: World Bank

Defensoría del Pueblo (2011): Aportes de la Defensoría del Pueblo a la promoción de la ética pública en los programas sociales del estado, Lima: Defensoría del Pueblo

De La O, A. (2013): Do conditional cash transfers affect electoral behavior? Evidence from a randomized experiment in Mexico, in: *American Journal of Political Science* 57 (1), 1–14

Filmer, D. / L. Pritchett: Estimating wealth effects without expenditure data or tears : an application to educational enrollments in states of India, in: *Demography* 38 (1), 115–132

Fiszbein, A. / N. Schady (2009): Conditional cash transfers : reducing present and future poverty, Washington, DC: World Bank

Gamson, W. (1968): Power and discontent, Homewood, IL: Dorsey Press

Goffman, E. (1963): Stigma : notes on the management of spoiled identity, New York, NY: Simon & Schuster Inc.

Hetherington, M. (1998): The political relevance of political trust, in: *American Political Science Review* 92 (4), 791–808

Huber, L. / P. Zárate / A. Durand / O. Madalengoitia / J. Morel (2009): Programa Juntos : certezas y malentendidos en torno a las transferencias condicionadas. Estudio de caso de seis distritos rurales del Perú, Lima: United Nations Children's Fund, Instituto de Estudios Peruanos, United Nations Population Fund

Jones, N. / R. Vargas / E. Villar (2007): El programa Juntos y el bienestar de la infancia, in Niños del Milenio. Relaciones con condiciones : el estado peruano frente a su infancia, Lima: Niños del Milenio

Juntos (2013a): Cobertura geográfica; online: http://www.juntos.gob.pe/?page_id=3159 (accessed April 24, 2013)

Juntos (2013b): Quienes son los beneficiarios; online: http://www.juntos.gob.pe/?page_id=3156 (accessed April 24, 2013)

Katz, M. B. (1990): The undeserving poor : from the war on poverty to the war on welfare, New York, NY: Pantheon Books

Kumlin, S. / B. Rothstein (2005): Making and breaking social capital : the impact of welfare-state institutions, in: *Comparative Political Studies* 38 (4), 339–365

Levi, M. (1998): A state of trust, in: Valerie Braithwaite and Margaret Levi, eds., Trust and governance, New York, NY: Russell Sage Foundation

Linares G. / I. del Rocío (2009): Descripción y diagnóstico de los instrumentos y procesos vigentes de focalización y registro de beneficiarios del programa Juntos, Final consulting report, Inter-American Development Bank

Narayan, D. (1999): Bonds and bridges : social capital and poverty, Policy Research Working Paper No. 2167, World Bank

Pierson, P. (1993): When effect becomes cause : policy feedback and political change, in: *World Politics* 45 (4), 595–628

Rawlings, L. / G. Rubio (2005): Evaluating the impact of conditional cash transfer programmes, in: *World Bank Research Observer* 20 (1), 29–55

Rothstein, B. / D. Stolle (2008): The state and social capital : an institutional theory of generalized trust, in: *Comparative Politics* 40 (4), 441–459

Salamon, L. (1995): Partners in public service : government-nonprofit relations in the modern welfare state, Baltimore, MD: Johns Hopkins University Press

Schady, N. / M. C. Araujo (2008): Cash transfers, conditions, and school enrollment in Ecuador, in: *Economía* 8 (2), 43–70

Skocpol, T. (1995): *Social* policy in the United States : future possibilities in historical perspective, Princeton, NJ: Princeton University Press

Spicker, P. (1984): Stigma and social welfare, New York, NY: St. Martin's Press

Stampini, M. / L. Tornarolli (2012): The growth of conditional cash transfers in Latin America and the Caribbean : did they go too far? Policy Brief No. 185, Inter-American Development Bank

Verba, S. / K. Schlozman / H. Brady (1995): Voice and Equality : Civic Voluntarism in American Politics, Cambridge, MA: Harvard University Press

Woolcock, M. (1998): Social capital and economic development : toward a theoretical synthesis and policy framework, in: *Theory and Society* 27 (2), 151–208

Woolcock, M. / D. Narayan (2000): Social capital : implications for development theory, research, and policy, in: *World Bank Research Observer* 15 (2), 225–249

Zucco, C. (2013): When payouts pay off : conditional cash transfers and voting behavior in Brazil 2002–10, in: *American Journal of Political Science* 57 (4), 810–822

Annex

Classification of organisations

Bridging social organisations

> *Comités de gestión municipal* (local community entities in charge of executing public works and projects funded with municipal funds)
>
> *Comités locales de administración de salud* (non-profits that manage and operate state-owned health clinics)
>
> *Mesas de concertación* (dialogue roundtables formed to address grievances and solve conflicts involving local communities and other actors such as businesses or the national government)
>
> *Núcleos ejecutores* (entities similar to the comités de gestión that spend national government funds instead of municipal monies)
>
> Political groups or parties
>
> Professional associations
>
> Water users' associations
>
> Workers' associations or unions

Bonding social organisations

> *Comedores populares* (community facilities comprising a kitchen and a dining hall)
>
> Clubes de madres (community-based associations of mothers that provide services mostly related to childcare, including providing meals, both to members and non-members)
>
> Cultural clubs
>
> Neighborhood associations
>
> *Rondas campesinas* (community-based policing organisations present in rural areas)
>
> School parent associations
>
> Sport clubs and associations
>
> Vaso de leche (nutritional social programme distributing enriched milk to a primary target population of children up to six years old and pregnant mothers)

Source: Chiarella (2010, 61). Water users' associations were reclassified into the bridging category while neighborhood associations were reclassified into the bonding category

Full regression results

	Horizontal organisations		Vertical organisations		Juntos Institutions		Ombudsman's office		Subantional government		Branches of government		Political parties	
	Without controls	With controls	Without controls	With controls	Without controls	With controls	Without controls	With controls	Without controls	With controls	Without controls	With controls	Without controls	With controls
1. Eligible population														
Mean(α_j)	0.750***	0.794***	0.067***	-0,006	1.499***	1.486***	1.100***	1.068***	0.966***	1.076***	0.842***	0.998***	0.568***	0.902***
	(0,017)	(0,161)	(0,005)	(0,047)	(0,018)	(0,165)	(0,026)	(0,235)	(0,017)	(0,161)	(0,018)	(0,186)	(0,017)	(0,175)
τ_t (Post-treatment=1)	-0,066	-0,041	0,003	0,002	-0.122*	-0.112*	-0,125	-0,117	-0,006	-0,004	-0,059	-0,054	-0,026	-0,022
	(0,043)	(0,042)	(0,016)	(0,016)	(0,050)	(0,050)	(0,072)	(0,073)	(0,050)	(0,050)	(0,049)	(0,050)	(0,046)	(0,046)
T_{jt} (ITT effect; DID)	0,057	0,025	-0,019	-0,014	0.196**	0.185*	0,105	0,098	0,053	0,054	0,055	0,050	-0,001	-0,005
	(0,068)	(0,065)	(0,018)	(0,018)	(0,073)	(0,073)	(0,112)	(0,112)	(0,070)	(0,069)	(0,077)	(0,077)	(0,068)	(0,069)
Dwelling characteristics index		-0.092**		0,012		0,049		0,040		0,045		0.073*		0,018
		0,028		(0,010)		(0,029)		(0,043)		(0,031)		(0,032)		(0,033)
Asset index		-0,021		0.017*		-0,042		-0,027		-0,015		-0,032		-0,003
		(0,025)		(0,008)		(0,025)		(0,032)		(0,022)		(0,020)		(0,026)
Age of household head		-0.022***		0.004*		0,001		-0,001		-0,001		-0,003		-0,014
		(0,006)		(0,002)		(0,006)		(0,009)		(0,006)		(0,007)		(0,008)
Squared age of household head		0.000**		0,000		0,000		0,000		0,000		0,000		0,000
		(0,000)		(0,000)		(0,000)		(0,000)		(0,000)		(0,000)		(0,000)
Education of household head (years)		0,004		0.005**		0,006		0,009		0,002		-0,006		-0,004
		(0,004)		(0,002)		(0,005)		(0,006)		(0,005)		(0,005)		(0,005)
Number of household members		0.060***		-0,003		0,007		0,007		-0,007		0,005		0,011
		(0,007)		(0,002)		(0,007)		(0,010)		(0,006)		(0,008)		(0,008)

Number of districts	233	233	233	233	232	232	224	224	232	232	231	231	230	230
Observations	2742	2739	2742	2739	2523	2520	1745	1742	2435	2432	2292	2289	2265	2262
R-squared (within)	0,002	0,061	0,001	0,018	0,006	0,011	0,004	0,007	0,001	0,003	0,001	0,005	0,000	0,004
2. Non-eligible population														
Mean(α_j)	0.565***	0.831***	0.078***	0,000	1.427***	1.516***	1.101***	0.956***	0.915***	0.954***	0.767***	0.802***	0.543***	0.606***
	(0,015)	(0,136)	(0,006)	(0,047)	(0,019)	(0,138)	(0,023)	(0,176)	(0,016)	(0,130)	0,016	(0,134)	(0,018)	(0,140)
τ_t (T=1)	-0,048	-0,061	0,014	0,009	-0.120*	-0.124*	-0,034	-0,034	-0,037	-0,038	-0,057	-0,060	0,003	-0,001
	(0,041)	(0,043)	(0,021)	(0,020)	(0,051)	(0,051)	(0,064)	(0,065)	(0,050)	(0,050)	(0,046)	(0,047)	(0,051)	(0,052)
T_{jt} (ITT effect)	0,007	0,023	-0,043	-0,035	0,054	0,057	-0.194*	-0.201*	-0,005	-0,003	0,037	0,043	-0,116	-0,108
	(0,057)	(0,058)	(0,023)	(0,023)	(0,075)	(0,075)	(0,094)	(0,095)	(0,066)	(0,066)	(0,063)	(0,064)	(0,071)	(0,071)
Dwelling characteristics index		-0.055*		0.033**		-0,018		0,005		0,007		-0,007		-0.066**
		0,023		(0,011)		(0,021)		(0,032)		(0,021)		(0,026)		(0,024)
Asset index		-0,013		0.019*		-0,014		-0,024		0,008		0,021		0,033
		(0,019)		(0,009)		(0,016)		(0,025)		(0,014)		(0,015)		(0,017)
Age of household head		-0.024***		0.003*		-0,007		0,004		0,000		-0,004		-0,006
		(0,004)		(0,002)		(0,005)		(0,007)		(0,005)		(0,005)		(0,005)
Squared age of household head		0.000***		0,000		0,000		0,000		0,000		0,000		0,000
		(0,000)		(0,000)		(0,000)		(0,000)		(0,000)		(0,000)		(0,000)
Education of household head (years)		-0,003		0.009***		0,006		0,006		-0,001		-0,001		0,000
		(0,004)		(0,002)		(0,004)		(0,006)		(0,003)		(0,004)		(0,004)
Number of household members		0.087***		0,000		0.026***		0,009		0,004		0.016*		0,000
		(0,007)		(0,003)		(0,007)		(0,009)		(0,006)		(0,007)		(0,007)
Number of districts	234	234	234	234	233	233	226	226	232	232	231	231	231	231
Observations	2836	2835	2836	2835	2546	2546	1886	1886	2508	2508	2359	2359	2365	2365
R-squared (within)	0,001	0,102	0,002	0,065	0,005	0,030	0,010	0,013	0,001	0,002	0,001	0,005	0,004	0,008

* prob < 0.05; ** prob<0.01; *** prob<0.001

Source: Own calculations using 2004, 2005, and 2006 ENAHOs.

Publications of the German Development Institute /
Deutsches Institut für Entwicklungspolitik (DIE)

Studies

82   *Knörich, Jan / Axel Berger:* Friends or foes? Interactions between Indonesia's international investment agreements and national investment law, 158 pp., Bonn 2014, ISBN 978-3-88985-589-3

81   *Holzapfel, Sarah:* The role of indicators in development cooperation : an overview study with a special focus on the use of key and standard indicators, 237 pp., Bonn 2014, ISBN 978-3-88985-588-6

80   *Pietschmann, Elena:* Forgotten or unpromising? The elusive phenomenon of under-aided countries, sectors and sub-national regions, 147 pp., Bonn 2014, ISBN 978-3-88985-587-9

[Price: EUR 10.00; publications may be ordered from the DIE or through bookshops.]

Discussion Papers

23/2014   *Bruhn, Dominique:* Global value chains and deep preferential trade agreements: promoting trade at the cost of domestic policy autonomy? 41 pp., Bonn 2014, ISBN 978-3-88985-645-6

22/2014   *Pauw, Pieter / Steffen Bauer / Carmen Richerzhagen / Clara Brandi / Hanna Schmole:* 关于有区别的责任的不同观点 国际谈判中有关共同但有区别的责任概念的最新评述, 60 pp., Bonn 2014, ISBN 978-3-88985-636-4

21/2014   *Faust, Jörg / Svea Koch:* Foreign aid and the domestic politics of European budget support, 35 pp., Bonn 2014, ISBN 978-3-88985-644-9

20/2014   *Keijzer, Niels*: Reforming the hand that feeds you? Managing capacity development support in Cambodia and Malawi, 60 pp., Bonn 2014, ISBN 978-3-88985-641-8

19/2014   *Pietschmann, Elena:* Managing diversity : what does it mean? An analysis of different approaches and strategies, 50 pp., Bonn 2014, ISBN 978-3-88985-639-5

18/2014   *Akude, John Emeka:* Knowledge for development : a literature review and an evolving research agenda, 50 pp., Bonn 2014, ISBN 978-3-88985-638-8

17/2014   *Lema, Rasmus / Johan Nordensvärd / Frauke Urban / Wilfried Lütkenhorst:* Innovation paths in wind power: insights from Denmark and Germany, 76 pp., Bonn 2014, ISBN 978-3-88985-637-1

16/2014   *Lundsgaarde, Erik:* Bureaucratic pluralism in global development challenges for Germany and the United States, 47 pp., Bonn 2014, ISBN 978-3-88985-635-7

[Price: EUR 6.00; publications may be ordered from the DIE or through bookshops.]

For a complete list of DIE publications:
http://www.die-gdi.de