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by

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Abstract

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Targeted to the poorest households, Conditional Cash Transfers (CCTs) may constitute up to 30 percent of the households' monthly consumption. An unexpected increase in income of such magnitude is likely to affect behavior of the beneficiaries beyond the changes envisioned by the creators of the program and imposed by the conditions. My dissertation focuses on such unintended impacts of the CCT programs. I explore the impact of Juntos, a CCT program in Peru, on political participation and intra-household allocations as well as estimate its effects on the targeted outcomes, such as education and utilization of medical services.

Using two alternative econometric techniques – difference-in-difference and panel data estimation and individual as well as district level data sets, I demonstrate in my first essay that the program increased turnout in presidential and regional elections in the incorporated districts. In the second essay I address the relationship between domestic violence and increases in the income of a victim, generated by the transfer. I develop a model, which incorporates two aspects of this relationship: increases in victim's income may exacerbate a rent-seeking motif behind domestic violence, and at the same time they may render her greater bargaining power. I empirically test the predictions of the model using difference-in-difference framework and matching techniques. I find that arrival of the program decreased the prevalence of domestic violence, and the decreases are higher among women whose outside of marriage utility is more affected by the transfer than their within marriage utility: women with less children and with cash-paying jobs. In my third essay I carry out an impact evaluation of the targeted outcomes of the program: consumption, education and health. Using matching and instrumental variables methods, I find significant improvements in all three areas.

To my mother, Irina Perova, with much love and deepest gratitude for making it all possible.

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Introduction

Since their inception in early nineties Conditional Cash Transfer (CCT) programs have gained the reputation of one of the most effective developmental interventions among economists and policy makers alike. The CCT programs generally pursue the dual objective of decreasing poverty in the short-run and eliminating inter-generational transmission of poverty in the long run. The former objective is a direct consequence of giving cash to families. The latter goal is achieved through creating both incentives and conditions for the beneficiary families to invest in human capital of their children: in order to collect the transfer, parents need to ensure that their children receive education and proper medical care.

The intended impacts of the CCT programs, such as higher school achievement and lower disease rates, have been scrutinized in various impact evaluations. However, the impacts of an unexpected increase in the household budget, sometimes up to 30 percent of monthly income, is unlikely to be exhausted by a set of changes envisioned by a policy-maker.

My dissertation explores unexpected as well as intended impacts of a CCT program Juntos in Peru. In my first essay, I address the question of whether a cash transfer from the government affects the most basic form of political participation – voting. I demonstrate that arrival of the Juntos program in a district increased turnout among its residents. I further explore the channels through which this increase occurred, and provide suggestive evidence that the result is driven by a change in the attitude to political process, rather than by material gratifications.

In my second essay, I explore the impact of an increase in a woman's discretionary income generated by a CCT on domestic violence. I develop a model, which takes into account two distinct aspects of the relationship between domestic violence and the victim's income. Violence may be used as a rent-extraction mechanism; in this case increases in the victim's income create greater incentives for the abuser to extract rents. Alternatively, higher income of the victim improves her outside option and therefore bargaining threat point, and may reduce violence. The model yields a set of conditions under which increases in a victim's income decrease violence. For instance, in the marriages where a woman is pushed to her reservation utility, violence goes down as long as the outside of marriage utility increases in income at least at the same rate as the within marriage utility. I test the model using the data on domestic violence and participation in the Juntos program. I find that on average violence decreased in Juntos districts, and that consistent with the model, the decreases were higher among women who have less children, have never been exposed to violence between their parents and who have cash-paying jobs.

Finally, in my third essay I carry out an impact evaluation of the Juntos program and estimate its effect on intended outcomes – consumption, education and health. I find that the program did improve the targeted outcomes of the beneficiaries, and did not trigger undesired behaviors, such as higher fertility.

CCT programs are usually targeted at the poorest households, and consequently generate a considerable change in their budgets. Such non-negligible increase in monthly income is bound to set forth multiple changes in the life of a household as well as its individual residents. Thus CCTs present a fortuitous opportunity for an economic inquiry into various aspects of individual and household behavior, such as intra-household allocations or political preferences. In my dissertation I take advantage of the rollout of a CCT program Juntos in Peru to address the question of the impact of increases in a woman's income on domestic violence and the impact of the arrival of the

Juntos program on the turnout among the residents in the district. I also estimate the impact of the program on intended outcomes in consumption, education and health.

Overall, my dissertation provides a comprehensive evaluation of the CCT program Juntos – apart from estimating the intended impacts of the program, I evaluate its impact on domestic violence and political participation. My dissertation also contributes to the body of literature on intra-household allocation and the political economy of the governmental transfer programs.

Chapter 1: Buying Votes, or Fostering Civic Conscientiousness? How Do Conditional Cash Transfers Affect Civic Participation?

Introduction

The impacts of conditional cash transfers (CCTs) have been researched with varying extent of rigor in a number of areas, including nutrition, school attendance, early childhood development and poverty as well as their potential to serve as a risk-coping mechanism in presence of shocks[1]. There is considerably less research dedicated to political economy aspects of CCTs. Maracorda, Miguel and Vigorito [2009] examine the changes in the likelihood to vote for the incumbent as a result of participating in the PANES CCT program in Uruguay. Using aggregate data, De La O [2008] estimates the impact of Progresa in Mexico on turnout and voting for incumbent. Rodriguez-Chamussy [2009] addresses the question whether local government is successful at claiming credit for welfare programs run by the central government.

In this paper we investigate the impact of a CCT program Juntos in Peru on civic participation. We focus on the most basic form of civic participation – voting, and also explore the channels through which CCTs may potentially impact the decision to vote. While Maraconda, Miguel and Vigorito [2009] demonstrate that CCT programs may switch political affinities of the beneficiaries, the focus of this paper is on determining whether receipt of a CCT serves as a tipping point to go to the polls for a beneficiary who would abstain otherwise. Given the experimental evidence that voting is habit-forming (Gerber, Green and Shachar [2003]), the impact of a CCT may last beyond a single election, when an individual ceases to be a recipient of the transfer and is no longer impacted by material considerations in his/her political views. In this context it would be incorrect to interpret the impact of CCTs as pure "vote-buying". Instead, CCTs may be more likely to contribute to forming politically active electorate. There is empirical evidence that informed and politically active electorate strengthens incentives for governments to be responsive (Besley and Burgess [2002]). This evidence renders potential positive impact of CCTs on turnout highly conducive to the accomplishment of development goals.

[1] de Janvry, Finan, Sadoulet and Vakis [2006] study the role of Progresa as a protection against shocks, Chaudhury and Parajuli [2008] assess the impacts on school enrollment and attendance in Pakistan, Gertler [2004] establishes the impacts of Progresa on child health outcomes, Macours, Schady and Vakis [2008] evaluate the impact of Atención a Crisis in Nicaragua on child cognitive development. These papers represent a small sample of a broad literature on the impacts of CCT programs.

Civic participation is inherent to the very definition of democracy. It is also one of the features that determine the place of a country in a continuum between de-jure democratization and transformation into a full-fledged democracy where citizens are not only entitled to the right to participate in politics, but actively execute it. Given the body of evidence that democracy contributes to a set of developmental outcomes – ranging from higher life expectancy at birth (Besley and Kudamatsu [2006]) to reduction in infant mortality (Kudamatsu [2007][2]) – policies that can foster civic participation deserve greater attention in the debate on development. Our paper contributes to this debate by rigorously establishing the impact of one such policy.

Background: Juntos program and voting legislation

CCT Program Juntos

Peru's CCT program Juntos was started in 2005. Over the course of three years it expanded from covering approximately 37,000 households in 110 districts to providing benefits to over 454,000 households in 637 districts. Associated budget expenditures during this period grew from 116 million soles to 344 million soles[3]. 637 districts where the program is run were identified among the poorest 880 districts in Peru under the CRECER (umbrella for social programs) initiative. Program expansion plans include incorporation of remaining 243 CRECER districts.

As most CCT programs, Juntos pursues the dual objective of reducing poverty in the short run by providing households with cash transfers, and breaking intergenerational transmission of poverty through investments in human capital via improved access to education and medical services. The program aims to achieve these objectives by providing eligible families with cash transfer as long as they comply with a set of educational and health conditions, summarized in Table 1.

Eligibility of the households is determined as a result of a three-stage process. First, districts are selected to participate in the program on basis of the following five criteria: (i) exposure to violence during the Sendero Luminoso guerilla; (ii) poverty level, measured as a proportion of population with unsatisfied basic needs; (iii) poverty gap; (iv) child malnutrition level; and (v) poverty severity. A weighted average of these characteristics (indicador sintético) was used to select 637 eligible districts. These districts were incorporated into the program in several stages; though an attempt was made to roll out the program according to the magnitude of indicador sintético, starting with districts in greater need, due to administrative shortcomings this order was not followed. Figure 1 shows the values of indicador sintético plotted against district enrollment dates. The rollout clearly was not carried out in the order of decreasing magnitude of the district score. In informal interviews the management of the program mentioned quality of roads and

[2] Literature on the link between democracy and development is not limited to the above examples. Acemoglu and Robinson (2000) show that democratization increases growth if the gains from relaxing the credit constraints exceed the costs of distortionary taxation. In the seminal paper of the impact of institutions on development Acemoglu, Johnson and Robinson (2001) find that presence of democratic institutions such as constraints on the executive is associated with higher income per capita. Rodrik [1999] shows that democracies are associated with higher manufacturing wages. Rodrik and Wacziarg 2005] dispel the hypothesis predominantly based on anecdotic evidence that democratic transitions are followed by bad economic outcomes. Przeworski and Limogni [93] survey statistical studies and conclude that differences in growth are better captured by variation in institutions, and not regimes.

[3] 1 nuevo sol is approximately equal to $0.35.

weather conditions among the reasons that changed the planned order of district incorporation.

At the second stage of selection Instituto Nacional de Estadistica y Informatica (INEI) collected a census of households in the selected districts. INEI constructed a score based on observable household characteristics to determine the poverty status of the households[4]. Households with at least one child under 14 and the score above a certain threshold qualified as eligible at the second stage.

At the final stage in order to minimize both inclusion and exclusion errors, the list of eligible according to the INEI score went through the process of community validation by community members, local authorities and representatives from the Ministries of Education and Health.

Voting in Peru

Voting in Peru is considered not only a right of citizenship, but a civic responsibility as well. The obligation to vote in the elections is regulated by law, and sanctions are imposed for non-compliance. The sanctions include a fine, which varies depending on the poverty level of the district, and possible infringements of civil rights in the future: a proof of having voted is required in order to obtain certain services from some public offices[5]. The proof of having voted is not required in order to receive Juntos transfers[6].

Though Peru has a compulsory voting law and enforces it by punishing non-compliers, turnout reaches 100 percent in less than 0.5 percent of districts, and there is significant variation in the fraction of eligible that actually cast a vote. Figures 2, 3, and 4 show turnout distribution for the two rounds of the national elections and for regional elections of 2001. Despite the high average turnout at 78, 75 and 80 percent respectively, in some districts turnout is as low as 25 percent (for the first round of national elections), while in other it reaches universal participation (first round of national elections and regional elections).

Conceptual framework: why CCTs may affect a decision to vote

Based on economics and political science literature, we can suppose three channels through which CCTs may affect voting: selective material gratification, changes in the attitude to the government and social interactions.

The transfer of 100 soles equals approximately 30% of the average household monthly consumption of the beneficiaries. If the transfer recipients believe that the flow of benefits is contingent on the incumbent's staying in power, they have a strong material incentive to go to the polls and vote for the incumbent. If the beneficiaries discount material incentive by the low probability of changing the outcome of the national election, even the transfer equal to almost one third of the household's monthly consumption may not be sufficient inducement to go to the polls. However,

[4]The score is a weighted average of the following characteristics: the presence of a school-age child out of school, or adult illiterate woman, type of materials used in construction of the walls, roof and floor of the house, access to potable water, sewage and electricity and an indicator for not having any of the list of assets (such as TV, vehicle, refrigerator).

[5]Ley Nº 28859, "Ley que suprime las restricciones civiles, comerciales, administrativas y judiciales; y reduce las multas en favor de los ciudadanos omisos al sufragio", published in El Peruano on August 3, 2006.

[6]Author's interview with Juntos management.

Aldrich [1993] notes that voting is a low cost low benefit decision, and consequently even a minor shift in benefits may trigger a change in turnout.

Another channel through which a CCT program may affect turnout is a change in the attitude to the government. 637 districts where the program operates were chosen as districts in the highest need of governmental assistance. The selection criteria included exposure to violence during the Cendero Luminoso guerilla. While fighting the guerilla the governmental troops killed or violated human rights of many civilians[7]. Additionally, Juntos districts are among the poorest in Peru – with bad infrastructure and barely existent public services. It is highly likely that prior to the arrival of the program the residents hardly had any other interactions with the government, apart from human rights violations in the 80s.

In this setting a situation of low political efficacy (Aldrich [1993]) is likely to arise – when citizens believe that the government - either of the candidates participating in the elections - is either unwilling or unable to address their problems. Aldrich [1993] shows that abstaining is a dominant strategy in this situation. Arrival of the transfer may change the perceptions of the government's capacity and willingness to resolve the problems of the voters, and thus lower, if not terminate, the situation of low political efficacy.

The change in the attitude to government may also affect turnout through making the very act of voting more gratifying. According to research based on US data, most prevalent gratifications from voting include civic gratifications – such as the pleasure from fulfilling one's duty (Verba, Schlozman and Brady [2000]). Political participation may not be regarded as an honorable civic duty in the locations where population experienced governmental abuses only two decades ago, like Juntos villages. By changing the attitude to the government, the arrival of the program may render intrinsic motivation to previously burdensome and poorly enforced obligation to vote. The change in attitude to government may take place not only among transfer recipients but also among non-beneficiaries, who merely know about the policy but do not benefit from it directly.

Turnout among non-beneficiaries may also increase due to social interactions. There is a vast empirical literature that convincingly demonstrates the presence of social interactions in various areas of human life[8]. An individual decision to vote is equally plausibly impacted by the peers' decisions to go to the polls. Casting a vote may enter the ranks of gratifications only once the community starts seeing it as an honorable civic duty, and not a nuisance imposed by authorities. Gerber, Green and Larimer [2008] provide experimental evidence that compliance with social norms is one of the strong determinants of an individual decision to go to the polls. Juntos program, by giving material incentives or changing the attitude to the government of the beneficiaries only, could have impacted the dominant social norms, and through them increased the turnout beyond the recipients of the transfer.

[7]According to Human Rights Watch, governmental security forces were responsible for approximately one third of the killings.

[8]Hanushek et al [2003] and Sacerdote [2001] show that individual academic achievement is strongly impacted by average achievement in the classroom. Raphael and Gaviria [2001] demonstrate that social interactions with classmates impact not only achievement, but also the likelihood to engage in truant behavior. Mas and Moretti [2009] as well as Falk and Ichino [2006] find strong evidence of peer effects on individual productivity. Glaeser, Sacerdote and Scheinkman [1996] demonstrate the role of social interactions in criminal activity. Other areas of economic activity, where empirical research has demonstrated strong presence of social interactions, include (but by far are not limited to): retirement plans decisions (Duflo and Saez [2003]), going to the movies (Moretti [2009]), technology adoption (Miguel and Kremer [2004]).

Data

In order to identify the impact of Juntos in voting we use two alternative methodologies. Our preferred specification estimates impact in a district level panel. As a robustness checks, we use difference-in-difference estimator with an individual level data set. The direction and the magnitude of the effect are consistent across specifications.

In order to carry out estimation with these alternative methodologies, we took advantage of a number of data sources. Oficina Nacional de Procesos Electorales (ONPE) collects detailed information on the results of national, regional and municipal elections from 1998 to 2007. These data make it possible to construct a panel of district-level turnout, spanning 2 presidential and 2 regional elections. These elections took place in 2001 and 2006. In order to establish that the estimation results are not driven by a trend we use the data on 2000 presidential elections[9].

The second data source is the Registro Nacional de Municipalidades (RENAMU) for the period from 2002 to 2007. RENAMU is a census of Peruvian municipalities, which through the interviews with municipal authorities collects information on infrastructure, public services, economic activity, presence of governmental programs and other characteristics of the districts. We use these data to control for time-variant district level characteristics.

Juntos administrative data provide information on the rollout of the program, including the dates when districts were incorporated into the program, and the numbers of people who were receiving the transfers for every month after incorporation.

The household survey Encuesta Nacional de Hogares (ENAHO) includes a question about voting in the last presidential elections in 2002, 2007 and 2008 rounds. Thus ENAHO provide self-reported data on voting in 2001 and 2006 presidential elections. It also contains an extensive set of individual and household characteristics, as well as all the variables used in the formula to determine a household's eligibility for Juntos program. The survey is a cross-section with a panel component, which is terminated and started anew every four years. Unfortunately, the last interruption took place in 2004, between the two presidential elections, and we cannot take advantage of the panel dimension of the ENAHO data.

Estimation strategy

The absence of randomized rollout of the program creates a major difficulty for producing unbiased estimates of its impacts. The estimation may be confounded by unobserved variables potentially correlated with both: the outcome of interest and the placement of the program. In order to circumvent this difficulty we will take advantage of the panel dimension of the available data. Repeated observations of voting outcomes for a set of districts will allow us to control for district level time-invariant characteristics, as well as for the differences between election years. However, panel data estimation does not eliminate the potential bias from the presence of unobserved time-variant characteristics simultaneously correlated with the outcome of interest and the placement of the program. Moreover, the estimates will also be biased if the placement of the program was determined by pre-intervention trends.

To assert the validity of our identification strategy, we rule out the possibility that the rollout of the program was impacted by pre-intervention trends. Furthermore, we assess the relative impor-

[9]Due to the ousting of Fujimori, presidential elections took place in two successive years: 2000 and 2001.

tance of observed and unobserved characteristics in explaining the impact. Finally, we validate our results by carrying out estimation with a different data set and an alternative econometric specification: we employ a difference-in-difference approach using individual level data from the ENAHO survey.

District level panel data estimation

Using the ONPE data on electoral outcomes, and district level characteristics from RENAMU, we construct a panel, which spans two rounds of presidential[10] and regional elections (in 2001 and 2006). In a number of districts the last election of each type took place after the districts were incorporated in the Juntos program. We identify the impact of Juntos on the voting patterns in the following fixed effects regression:

$$Y_{jt} = \alpha_j + \beta_t + \gamma T_{jt} + \delta \mathbf{X_{jt}} + \xi_{jt}, \tag{1}$$

where Y_{jt} is the ratio of voters to all eligible to vote in district j at time t, T_{jt} is the treatment variable, which captures exposure to Juntos in district j at time t, $\mathbf{X_{jt}}$ captures time-variant district level characteristics; α_j and β_t are district and time fixed effects, respectively. We use three different treatment indicators T_{jt} : (i) a dummy equal to 1 if a district was incorporated in Juntos prior to 2006 elections, (ii) the number of months during which the residents of a district were receiving Juntos transfers by the time of the elections, and (iii) the percent of eligible enrolled in the program at the time of the elections. The identification relies on the assumption that ξ_{jt} and T_{jt}, conditional on $\mathbf{X_{jt}}$ and fixed effects are not correlated:

$$Cov(T_{jt,}\xi_{jt}|\alpha_j, \beta_t, \mathbf{X_{jt}}) = 0. \tag{2}$$

This assumption will be violated in case of dynamic endogenous placement of the program: $\triangle T_{jt} = g(\triangle Y_{jt-1})$. We provide suggestive evidence that this possibility is unlikely by checking whether changes in voting outcomes prior to the rollout of the program determine the placement of the program. Using the data on election outcomes in 2000, we estimate the following regression:

$$T_j = \alpha_0 + \alpha_1 \triangle Y_j + \alpha_2 \triangle \mathbf{X_j} + \zeta_j, \tag{3}$$

where $\triangle Y_j = Y_{j2001} - Y_{j2000}$ is pre-intervention trend in the outcome and $\triangle \mathbf{X_j} = \mathbf{X_{j2001}} - \mathbf{X_{j2000}}$ contain pre-intervention trends in the time-variant district level characteristics. T_j captures the order of the program rollout. We use two variables as the outcome T_j: the dummy equal to 1 if a district received Juntos prior to 2006 elections and the number of months in the program by 2006 election. Table 2 shows that the coefficients on $\triangle Y_j$ are not significant in either regression. As the differences in outcomes do not predict the order of rollout, it is unlikely that the results are driven by the trend.

The assumption (31) would also be violated if there were unobserved time-variant variables correlated with both: voting outcomes and the placement of the program. This may happen if districts, which received the program earlier, were targeted by the outgoing administration on the basis

[10] There were two rounds of presidential elections in 2001 and in 2006. We use the average turnout in the two rounds as a preferred outcome of interest. The results with turnout in each of the two rounds are similar in significance and magnitude.

of characteristics potentially correlated with turnout such as better publicly funded infrastructure, or greater presence of existing social programs. If this unknown placement criterion, absorbed in the error term, is correlated with the outcome of interest, our estimates will be biased. For example, the following scenario is not implausible: districts with low presence of existing social programs had lower approval ratings of incumbent government, and therefore were the first ones to receive the program. The same scarcity of social programs could make the population more discontent, and consequently politically active. The correlation between this unobserved targeting criterion and the outcome of interest will result in the upward bias of our estimates.

In order to assess the importance of such omitted variables, we include in the regressions an extensive set of time variant characteristics, potentially related to the outcome. RENAMU data enables us to include into the vector $\mathbf{X_{jt}}$ such characteristics as municipal revenue and spending, and transfers received by the municipality (including transfers from the national government, for various social programs, for improvements in infrastructure, etc.). We also control for the number of beneficiaries in the three most popular social programs: Vaso de Leche, Comedor Popular and Club de Madres, and for the level of economic activity in the district: the number of small and medium enterprises registered within a year and the number of licenses obtained by service providers. Furthermore, we include controls for the supply of medical services: number of private and public hospitals, health centers and pharmacies. Robustness of coefficients of interest to this extensive set of controls would provide reassurance that non-random selection of districts into the earlier phases of the program is not driving the results.

Tables 3 and 4 present the results. Table 3 contains the coefficients from the regressions with the number of months in Juntos by the election date as the treatment indicator. The results from regressions with the dummy for participation in Juntos by the election date and the percent of beneficiaries among all eligible are in Table 4. The estimated effects are not only positive and significant, but not negligible in magnitude. Table 3 shows that an additional month of receiving Juntos transfers increases turnout by 1 percentage point in case of presidential elections, and by 0.6 percentage points in case of regional. Thus if a district were incorporated in the program during one year the turnout would increase by 12 percentage points. Participation in the program captured by a dummy is associated with 6 percentage points increase in turnout in presidential elections, and with 4 percentage points increase in turnout in regional elections (Table 4). Increase in the fraction of beneficiaries by 0.1 (or 10 percent) triggers an increase in the turnout by 1 percentage point in case of presidential elections, and 0.9 percentage points in case of regional (Table 4).

Table 3 also shows how the estimated impact of the program does not change with the addition of new controls in the regressions. We gradually add characteristics, which capture district revenue and spending, presence of other social programs, level of medical services and overall economic activity in the district. The impact of Juntos on turnout is significant at 99% level in all specifications for both presidential and regional elections. The reduction in magnitude of the coefficient is not large: it drops from 0.0115 in the regression with no controls to statistically identical 0.0109 in the regression with complete set of controls for presidential elections, and from 0.0063 to 0.006 for regional elections. For other treatment indicators – dummy for participation in Juntos before the election and percentage of eligible enrolled in the program by the election date – we only show 2 specifications, with no control variables and all control variables in Table 4. The results of the intermediary stages are similar. The coefficients remain positive and significant, and the changes in magnitude are small.

We further assess the importance of omitted variables bias using technique developed by Altonji et al. [2005] and modified by Bellows and Miguel [2008]. If inclusion of additional controls substantially attenuates the coefficients of interest, it is possible that the inclusion of even wider set of controls would eliminate the effect completely. Following Bellows and Miguel [2008], we derive a ratio of the influence of omitted variables relative to observed variables that would be needed to fully explain away the impact of Juntos on voting in the regressions with number of months in Juntos as the treatment indicator. We find that targeting on unobserved variables would need to be over 17 times greater than on the entire set of RENAMU variables for presidential elections, and over 21 times greater for regional elections. The results are similar in magnitude if other treatment indicators are used.

Difference-in-difference estimation

As a robustness checks, we carry out a difference-in-difference estimation using ENAHO data. ENAHO survey contains the question about whether a respondent voted in the last elections. Though there is a small panel component of the survey, it was discontinued in 2006 – consequently, the data that would allow comparing voting decisions of the same individuals in 2001 and 2006 elections are not available. In order to estimate the impact of Juntos on voting we pool three cross-sections - ENAHO 2002, 2007 and 2008. ENAHO 2002 contains information about the decision to vote in 2001 national elections. Respondents in both 2007 and 2008 were asked a similar question with respect to 2006 elections.

A potential problem with the use of the survey data is the possibility of bias in reporting voting outcomes. The availability of ONPE data on the exact turnout at the district level allows us to assess the magnitude of the bias in different years. Table 5 shows national turnout according to ONPE and as estimated from the ENAHO survey. ENAHO data clearly does not reproduce the national averages. However the presence of reporting bias in the dependent variable will jeopardize difference-in-difference estimation only if the differences in bias between 2001 and 2006 are statistically distinguishable across treatment and control district. Taking advantage of the information on actual turnout at district level from ONPE data, we can rule out this possibility.

Let T_{jpt}^{ONPE} be the actual turnout according to ONPE in district j province p in year t, and T_{jpt}^{ENAHO} be the turnout estimated from ENAHO data. Then district level bias may be calculated as $B_{jpt} = T_{jpt}^{ONPE} - T_{jpt}^{ENAHO}$.

We allow for province-specific trends, and model the bias as follows:

$$B_{jpt} = \psi_p + \rho_p t + u_{jpt}, \tag{4}$$

where ψ_p is province fixed effect, $\rho_p t$ is province trend and u_{jpt}, captures district-level heterogeneity in misreporting and/or measurement error. Then we can define $\triangle B_{jp} = B_{jp2006} - B_{jp2001}$, and estimate the difference in bias between two election years across treatment and control districts by running the following regression:

$$\triangle B_{jp} = \psi_p + \beta T_{jp} + \zeta_{jp}, \tag{5}$$

where T_{jp} is a dummy equal to 1 if district j in province p received Juntos before 2006 elections. The coefficient β captures the differences in changes in bias across treatment and control districts, conditional on the province level change in bias. Table 6 presents estimation results. Once we control for the province level change, the difference in the change in bias across two election years between control and treatment districts is negligible: it is not statistically distinguishable from 0 and the point estimate is low -0.00139.

Having validated the use of difference-in-difference framework in presence of reporting bias, we capture the impact of Juntos on voting by estimating the following regression:

$$Y_{ijpt} = \alpha_1 E_t + \varphi_p + \alpha_2 E_t \varphi_p + \alpha_3 T_{jp} + \alpha_4 E_t T_{jp} + \alpha_5 \mathbf{X_{ijpt}} + \alpha_6 \mathbf{D_{jp}} + \alpha_7 \mathbf{C_{jpt}} + \xi_{ijpt}, \tag{6}$$

where Y_{ijpt} is a dummy equal to 1 if an individual i residing in district j, province p voted in the presidential elections held in year t, E_t is a dummy to control for structural changes between the two national elections, and is equal to 1 if the observation belongs to 2007 or 2008 ENAHO and 0 otherwise. T_{jp} is a district-level treatment indicator, equal to 1 if a district received Juntos before 2006 presidential elections, and 0 otherwise. $\mathbf{X_{ijpt}}$ is a vector of individual controls; $\mathbf{D_{jp}}$ and $\mathbf{C_{jpt}}$ are vectors of time invariant and time variant district level controls, correspondingly. To warrant consistency between our model of bias presented in equation (33) and estimation of the impact of Juntos on voting, we include province fixed effects, φ_p and interactions between province dummies and indicator for the election year E_t. We ensure robustness of our results by also estimating the impact of Juntos in a regression with district fixed effects:

$$Y_{ijpt} = \alpha_1 E_t + \phi_{jp} + \alpha_2 E_t T_{jp} + \alpha_3 \mathbf{X_{ijpt}} + \alpha_4 \mathbf{C_{jpt}} + \xi_{ijpt}, \tag{7}$$

where ϕ_{jp} is district fixed effect, and the rest of the notation is the same as in equation (35).

The dummy E_t captures aggregate factors that would trigger the change in voting patterns across time even in the absence of the intervention; the dummy T_{jp} captures time-invariant differences between treatment and control districts. In specification (17) we control for these differences with district fixed effects. As long as the unobserved factors that could potentially impact voting did not contemporaneously follow distinct patterns of changes in the two groups of districts, the impact of Juntos on voting will be captured in the coefficient on the interaction between the election year dummy and the indicator for early enrollment districts. Alternatively, the required identification assumption can be described as follows: in the absence of Juntos, the expected changes in voting patterns in the two groups of districts would not have been different. As in the previous specification, we assess the relative importance of omitted variables compared to observed control variables by gradually introducing controls and tracking changes in the magnitude of the coefficient of interest.

Using data from a number of sources: ENAHO, RENAMU, and Juntos administrative data - allows us to control for a rich set of variables. Vector $\mathbf{X_{ijpt}}$ contains individual and household characteristics. Individual level variables include age, sex, education level, relationship to the household head and employment related variables, such as employment status, number of hours worked during the week of the interview, indicators for salaried and agricultural work. Household level variables include the score used to determine whether a household was eligible for a transfer and its square. We also explicitly control for the variables used in the construction of the score: indicator for availability of three services (electricity, a hygienic restroom and potable water), indicators of the type of materials used in the construction of roof, walls and floor of the dwelling, and an indicator equal to one if the household is using industrial fuel. The vector $\mathbf{D_{jp}}$ includes the variables used in the construction of indicador sintetico (index used to determine district eligibility for Juntos): monetary poverty, severity of poverty, indicator for chronic child malnutrition, average index of unsatisfied basic needs and percentage of communities exposed to violence during Sendero Luminoso guerilla. It also contains percent of households where the head speaks Spanish. The 2001 and 2006 elections were the first in Peru when one of the presidential candidates was indigenous (Toledo in 2001 and Humala in 2006). This may have increased turnout in the predominantly indigenous regions of Peru. Vector $\mathbf{C_{jpt}}$ includes the same set of time-variant district characteristics that was used in district level panel estimation[11].

We present the results from our preferred specification – equation (35) – in Table 7. Column (1) shows coefficients from the regression with only dummies, which distinguish observations from control and treatment districts and different election years. As expected, due to the bias in reporting voting the coefficient on the dummy equal to 1 for 2006 observations does not capture the average increase in turnout between 2001 and 2006 elections. The coefficient on the interaction between district level exposure to Juntos and election year dummy is estimated in Columns (2) through (5) with varying number of control variables. The coefficient is positive and significant in all specifications. It reduces in magnitude by no more than 0.003 as we move from column to column. We conclude that it is highly unlikely that the impact we estimate may be fully attributed to the influence of the unobserved variables. We show estimation results using the specification with district fixed effects in Table 8. The coefficient of interest is lower: 0.05, but still significant at 10 percent level.

Overall, our results using ENAHO data and difference-in-difference methodology generally confirm our findings using district panel regressions. The direction of the effect of Juntos on voting is the same and its magnitude is similar: we find 0.056 increase in turnout in Juntos districts with district panel regressions (Table 4).

Why does turnout increase? Possible mechanisms.

Econometric analysis above convincingly shows that Juntos had a positive impact on the average turnout in the districts, which received the program. This analysis however does not reveal the mechanisms through which district incorporation into the program increased turnout. In this section we analyze suggestive evidence in favor and against the three channels considered in Section 3 through which the effect could potentially occur: selective material gratification, changes in the attitude to government and social interactions.

If selective material gratification is the primary channel at play, we should observe impacts among the beneficiaries only. We test this hypothesis using both: district level data from ONPE and individual level ENAHO data. Taking advantage of the administrative information about the number of people who were receiving benefits each month, we can test whether the impact of Juntos varied depending on the fraction of beneficiaries among all voters. If only the beneficiaries drive the impact, we will see higher effects in the districts where beneficiaries form higher fraction of the population. Let F_{jt} be the fraction of beneficiaries among all eligible to vote in district j at time t, F_t average of F_{jt} across all districts. We can estimate:

[11] 400 districts were not included in the RENAMU 2001 survey. These districts constitute a high fraction of districts surveyed in ENAHO in 2001. To avoid considerable reduction in the sample size, we use the data from RENAMU 1999 for the districts not surveyed in 2001.

$$Y_{jt} = \alpha_j + \beta_t + \gamma T_{jt} + \theta T_{jt}(F_{jt} - F_t) + \delta X_{jt} + \xi_{jt}, \tag{8}$$

where Y_{jt} is the ratio of voters to all eligible to vote in district j at time t, T_{jt} is a dummy equal to 1 if a district j was incorporated in Juntos before 2006 elections, $\mathbf{X_{jt}}$ is a vector of time-variant district level characteristics; α_j and β_t are district and time fixed effects, respectively. Coefficient θ on the interaction between treatment and the difference between the fraction of beneficiaries in the district and average fraction across all districts captures incremental changes in turnout depending on the fraction of beneficiaries. Table 9 presents estimation results.

The turnout in presidential elections does not significantly vary depending on the fraction of beneficiaries among eligible to vote. The coefficient is significant in the regressions with regional elections turnout; however the point estimate of 0.16 is sufficiently low to conclude that the effect of the program reaches beyond the beneficiaries. Lowering the fraction of beneficiaries from its average amount by half – from 20 to 10 percent – would decrease the effect of Juntos by a smaller fraction - 36 percent.

We can further explore the possibility that selective material gratification primarily accounts for increase in turnout using individual level data and difference-in-difference estimation method. The program was started in 2005, consequently, 2001 ENAHO data do not have data on beneficiary status. However, ENAHO survey contains all the variables used by INEI in the construction of the eligibility score. Using this score, we can test whether the impacts are different among eligible and not eligible. We introduce interaction between treatment, election year and eligibility status as well as corresponding pair-wise interactions in equation (35) and estimate:

$$Y_{ijpt} = \alpha_1 E_t + \varphi_p + \alpha_2 E_t \varphi_p + \alpha_3 T_{jp} + \alpha_4 B_{ijpt} + \alpha_5 T_{jp} B_{ijpt} + \alpha_6 E_t B_{ijpt} +$$

$$\alpha_7 T_{jp} E_t + \alpha_8 E_t T_{jp} B_{ijpt} + \alpha_{\mathbf{9}} \mathbf{X_{ijpt}} + \alpha_{\mathbf{10}} \mathbf{D_{jp}} + \alpha_{\mathbf{11}} \mathbf{C_{jpt}} + \xi_{ijpt}, \tag{9}$$

where $\mathbf{B_{ijpt}}$ is a dummy equal to 1 if individual i from district j and province p in year t is eligible to receive the transfer according to the eligibility score. The rest of the notation is the same as in equation (35). The coefficient α_8 on the triple interaction will capture the differences in voting between eligible and not eligible in districts enrolled in Juntos prior to 2006 elections. Table 10 presents estimation results. We fail to reject the null hypothesis that the impact on eligible is the same as the impact on not eligible – the coefficient on the triple interaction is not significant in the specifications with and without controls.

Estimation results based on ENAHO data confirm the conclusion we derived using district level ONPE data: it is unlikely that selective material gratification is fully accountable for the observed increase in turnout. The impact of the program goes beyond the immediate recipients of the transfer. What are the channels through which the program increased turnout among non-recipients? The residents of the Juntos districts who do not directly benefit from the program may choose to go to the polls either due to social interactions, or because the government's policy on CCTs triggered a change in their attitude to government. We cannot rigorously test to what extent one or the other of these channels is accountable for the observed increase in turnout. However, using Democracia, Gobernabilidad y Transparencia module of the ENAHO survey we can provide suggestive evidence that a change in the attitude to government indeed took place.

Gobernabilidad, Democracia y Transparencia module of the ENAHO survey includes questions on the views about the importance of democracy, the current state of Peruvian politics, and also asks opinions about the role of government in the changes in wellbeing of the household and the community. We use these questions to capture changes in the attitude to government, associated with the arrival of the Juntos program.

We construct a district level panel of average attitude to democracy, interest in politics, opinions about the government, and other perception variables. This panel spans 4 years – from 2004 to 2008. Estimation of the regression (30) with perception variables from this panel as dependent variables yields unbiased estimates of the impact of Juntos on these variables. Table 11 shows the results. We present the coefficients from regressions with the treatment indicator only in column 1, and also with the interaction between treatment indicator and the difference between fraction of beneficiaries in a district and average fraction of beneficiaries in all participating districts in columns 2 and 3. The treatment indicator is a dummy equal to 1 if the district was enrolled in the program in a given year.

Participation of a district in the Juntos program during one year triggers an increase in the proportion of residents who believe that democracy is important by 5 percentage points. Fraction of respondents who believe that fair elections are respected increases by the same amount. There is also a 9 percentage points increase in the proportion of respondents who find public administration to be well functioning. The arrival of the program did not significantly affect the belief that democracy in Peru functions well; that democracy improved over the last year; that politicians care about people and general interest in politics. The fractions of respondents who find improvements in their wellbeing or wellbeing of the community on average remained the same – which is not surprising given the size of the transfer and low fractions of beneficiaries among eligible voters. The proportion of respondents who believe their wellbeing to have improved due to governmental transfers however significantly increased by 27 percentage points.

Interestingly, the impacts on the belief in the importance of democracy, respect for elections and quality of public administration does not significantly vary with incremental changes in the fraction of beneficiaries. Our results indicate that mere knowledge about the program may be sufficient to affect political views. Not surprisingly, the fraction of respondents, who find improvements in the wellbeing of their household and especially, attribute these improvements to the governmental transfers, significantly changes as the fraction of beneficiaries deviates from the national average.

Though we cannot establish causality between the observed increase in turnout and either of the three considered channels the empirical analysis renders suggestive evidence that selective material gratification is unlikely to be fully accountable for the observed impact on turnout. The program affected voting behavior of not only beneficiaries, but also of non-beneficiary residents of Juntos districts. This effect can be driven by the change in attitude to the political process in response to learning about the new government's policy. Our empirical analysis based on Democracia, Gobernabilidad y Transparencia module of the ENAHO survey suggests that such a change indeed took place.

Finally, we would like to explore the possibility of purely mechanical effect of the program due to the specifics of the electoral law and the technicalities of the program. The beneficiaries have to receive a DNI in order to collect the transfer money. DNI is also necessary in order to cast a vote at the election polls. Consequently the receipt of a DNI may increase turnout by lowering the costs of going to the polls. Our test of heterogeneity in impacts on eligible and not eligible with individual data or heterogeneity in impacts depending on the fraction of beneficiaries with district level data already suggest that this scenario is implausible.

However, we can provide further evidence against this hypothesis by testing for differences in impacts on men and women. Only mothers – the recipients of the transfer - need to get a DNI. Consequently, should the mechanical channel be a predominant one, we would expect a much stronger impact on women. We explore heterogeneity in impacts among men and women using individual level ENAHO data. More specifically, we estimate regression (35) with a triple interaction between a dummy equal to 1 if a respondent is a female, early enrollment status and election year dummies, and corresponding pair-wise interactions. Table 12 shows the results. The coefficient on the triple interaction is not statistically different from 0 in the specification with and without controls. We conclude that the mechanics of the program are unlikely to fully account for the increase in turnout.

Conclusion

There is extensive evidence that democratic governance is conducive to a wide set of developmental outcomes. Civic participation determines to what extent citizens partake in the design and implementation of policies affecting them, and therefore is likely to be one of the features of democratic regimes, that contribute to development. In fact, civic participation is inherent to the definition of democracy – albeit not always prevalent in the countries that proclaim themselves democratic.

In this paper we attempt to answer the question whether CCT programs have an impact on one of the most basic forms of civic participation – voting. There are several theoretical reasons why CCT programs may increase turnout. Cash transfers create a monetary incentive to go to the polls for the beneficiaries, if the beneficiaries believe that continuation of benefits is contingent on the incumbent's continuation in the office. Alternatively, the effect may be due to a change in the views on government's capacity and willingness to address the problems of its citizens, and as a result, stronger interest in the political process. The impact of the program may reach beyond the beneficiaries due to social interactions – if there is an element of contagiousness in the decision to go to the polls, i.e. if it is a function of the number of people who choose to vote in the immediate surroundings of an individual. The evidence that the first channel is fully accountable for the effect would make it difficult to claim that the impact of CCT programs on voting behavior is different from any other vote-buying technique. If other two channels also play a role, we should rather interpret the impact of CCTs on turnout as a contribution to forming politically active electorate and greater civic engagement of the population.

We find that incorporation of a district in the CCT program Juntos indeed increased turnout in the district. Our findings are robust to using different datasets and two alternative methodologies: estimation with district level panel and difference-in-difference estimation with individual level data. We also provide suggestive evidence that material gratification is unlikely to fully drive the observed impact. The program affects voting behavior of both: beneficiaries and non-beneficiaries. We also find that arrival of the program is associated with a change in the attitude to democracy and politics. Overall, our findings support the hypothesis that the Juntos program increased turnout through affecting civic engagement of the population.

Chapter 2: Buying out of abuse - how changes in women's income affect domestic violence

Introduction

Domestic violence has been recognized as an important human rights, social, and health problem. Extensive literature documents adverse effects of domestic violence on reproductive, mental and physical health outcomes of women and children[12]. According to the WHO, violence against women is an obstacle for achievement of virtually each of the Millenium Development Goals (WHO, 2005), and domestic violence is one of its most common forms (Vyas and Watts, 2009). It is indeed a highly pervasive phenomenon: in an analysis of population-based surveys in 10 countries Garcia-Moreno et al. (2006) find that between 15 and 71 percent of women experienced domestic violence at some point in their lives.

Surprisingly, the problem received very little attention in economic research. There is no comprehensive theory of the economics of domestic violence, and no consensus in either theoretical or empirical literature regarding the interplay between domestic violence and discretionary income of the victim.

Nash-bargaining models (Manser and Brown, 1980; McElroy and Horney, 1981; Lundberg and Pollak, 1993) suggest that increases in the utility at the threat point leads to the increase in the utlity within marriage. Hence, as long as the victim does not derive masochistic enjoyment from violence, these models predict that increases in her[13] income triggers reduction in domestic violence. There is empirical literature consistent with this prediction. Using the data on Californian battered women, Tauchen et al. (2001) find that increases in victim's income generally decrease violence for low and middle income families. Aizer (2007) shows that improvements in local labor market conditions decrease female hospitalization for assault. However, this empirical evidence is limited to developed countries.

Based on the qualitative evidence from India, Bloch and Rao (2006) develop an alternative model in which domestic violence is used as a rent-extraction mechanism in the context of informational asymmetries. Husbands use violence as a bargaining instrument to extract rents from the wives' families. The model of Bloch and Rao (2006) predicts that women from richer families are at an increased risk of violence. This prediction is validated by the data. Bobonis et al. (2009) adopt the model in Bloch and Rao (2005) to examine the potential consequences of an increase in women's income due to a conditional cash transfer (CCT). They posit that increases in a wife's discretionary income creates a stronger incentive for a husband to demand a monetary transfer. Their model predicts that while physical violence will decrease as women concede to the demands, the threats of violence and emotional violence should increase. Using Mexico's National Survey on Relationships within the Household (ENDIREH) 2003 in the villages which received

[12] See Campbell (2002) for review of research.

[13] I focus on the cases of domestic violence where the man is the abuser, as they represent the majority of domestic violence incidents (see, for example, Bureau of Justice Statistics).

Oportunidades program, they find that their predictions are supported by the data. However, in a follow-up paper, when 2009 round of ENDIREH data becomes available, Bobonis and Castro (2010) no longer find significant differences in the prevalence of physical and emotional abuse or threats of abuse between beneficiaries and non-beneficiaries.

In this paper I develop a model which takes into account the possibility that domestic violence may be used as a bargaining instrument, as well as the potential impact of the threat point on the equilibrium level of violence. I allow for two effects of an increase in the wife's income: (i) it may improve her threat point and (ii) it may create greater incentives for the husband to extract rents. I derive the conditions under which increases in a woman's income trigger reduction in domestic violence, and test these predictions using the data on domestic violence before and after the roll-out of a CCT program Juntos in Peru.

Juntos program provided women from poorer households with a monthly cash transfer of 30 soles, which constitutes approximately 30 percent of the average household monthly income in the districts where the program was implemented. As the majority of the CCT programs, Juntos requires that the transfer is received by a woman. I use the positive shock to a woman's income, generated by the arrival of the program, to explore the interplay between the victim's discretionary income and domestic violence.

The contributions of this paper are twofold: first, I further understanding of the economic motivations behind domestic abuse. Second, I provide solid empirical evidence on the potential of the CCT programs to reduce domestic violence. Given the gravity and prevalence of the problem, recommendations on policies that may decrease domestic violence are of particular value. The paper proceeds as follows: Section 2 presents the model. Section 3 describes Juntos program and social context of the villages where it is implemented. Sections 4 and 5 describe the data and the construction of domestic violence variables. Section 6 presents econometric strategy and the results. I carry out heterogeneity analysis in Section 7 and robustness check in section 8. Section 9 concludes.

The Model

Let h denote husband, w denote wife. I consider a utilitarian model of marriage with domestic violence, where the husband's gains from the marriage U^h are determined as a function of a transfer from his wife t and violence v. To simplify the model, I assume that only husbands can be abusive and can exert violence. Let t be a function of the wife's discretionary income I^w and violence v. The transfer $t(I^w, v)$ includes both monetary transfers and behaviors desirable for the husband, but not for the wife, into which he may coerce her by using violence. The transfer is increasing and strictly concave in both violence and the wife's income: $t_v > 0$, $t_{I^w} > 0$, $t_{vv} < 0$, $t_{I^w I^w} < 0$. I assume that the husband does not enjoy violence per se: $U^h_v < 0$. However, his utility increases in the transfer $t(I^w, v)$, which in turn, increases in violence - thus for some values of violence its net impact on the husband's utility may be positive. The husband's utility is strictly concave in both violence and the transfer.

I model the wife's utility within marriage as a function of her discretionary income, the transfer to the husband and violence: $U^w = U^w(I^w, t(I^w, v), v)$. The wife's within marriage utility increases in income, decreases in violence and the transfers she makes to the husband, and is strictly concave in all these arguments: $U^w_{jj} < 0$ for $j = I^w, t, v$. The outside of marriage utility of the wife is an increasng strictly concave function of her income $V^w(I^w)$. The differences between utility functions U^h and V^h capture availability of public goods within marriage and the conditions in the marriage market a woman faces. Women whose utility functions are such that $U^w(I^w) < V^w(I^w)$ do not get married in this framework.

The husband will use violence to maximize gains from the marriage subject to maintaining the level of utility that ensures that the wife stays in the marriage:

$$\max_v U^h(t(v,I^w),v) \text{ subject to } U^w(I^w,t(v,I^w),v) \geq V^w(I^w) \tag{10}$$

To solve this maximization problem, I set up a Lagrangian:

$$\mathscr{L} = U^h(t(v,I^w),v) - \lambda[V^w(I^w) - (U^w(I^w,t(v,I^w),v)] \tag{11}$$

and derive the following equilibrium conditions:

$$\frac{\partial \mathscr{L}}{\partial v} = \frac{\partial U^h}{\partial t(v,I^w)}\frac{\partial t(v,I^w)}{\partial v} + \frac{\partial U^h}{\partial v} + \lambda[\frac{\partial U^w}{\partial t(v,I^w)}\frac{\partial t(v,I^w)}{\partial v} + \frac{\partial U^w}{\partial v}] = 0 \tag{12}$$

$$\lambda[V^{w(}I^w) - (U^w(I^w,t(v,I^w),v)] = 0 \tag{13}$$

$$\lambda \geq 0 \tag{14}$$

$$V^{w(}I^w) \leq (U^w(I^w,t(v,I^w),v) \tag{15}$$

I will consider two cases: the case where the constraint is binding and the wife is pushed to her reservation utility ($\lambda > 0$), and the case where the constraint is not binding ($\lambda = 0$). I will refer to the former case as "unhappy marriages" and to the latter as "happy marriages". In the case of a "happy marriage", the equilibrium is described by the following conditions:

$$\frac{\partial U^h}{\partial t(v,I^w)}\frac{\partial t(v,I^w)}{\partial v} + \frac{\partial U^h}{\partial v} = 0 \tag{16}$$

$$V^w(I) < (U^w(I^w,t(v,I^w),v) \tag{17}$$

Total differentiation of equation (35) combined with some algebraic manipulations yield the derivative of violence with respect to the wife's discretionary income:

$$\frac{dv}{dI^w} = -\frac{\frac{\partial^2 U^h}{\partial t^2}\frac{\partial t}{\partial I^w}\frac{\partial t}{\partial v} + \frac{\partial U^h}{\partial t}\frac{\partial}{\partial I^w}(\frac{\partial t}{\partial v})}{\frac{\partial^2 U^h}{\partial t^2}(\frac{\partial t}{\partial v})^2 + \frac{\partial U^h}{\partial t}\frac{\partial^2 t}{\partial v^2} + \frac{\partial^2 U^h}{\partial v^2}} \tag{18}$$

The denominator of equation (18) is negative: the husband's utility increases in t, is strictly concave in t and in v, and t is strictly concave in v. The numerator cannot be unambiguously signed. The first term of the sum $\frac{\partial^2 U^h}{\partial t^2}\frac{\partial t}{\partial I^w}\frac{\partial t}{\partial v}$ is negative: husband's utility is strictly concave in t, and t is an increasing function of both income and violence. The sign of the second term depends on the cross-partial derivative of the transfer with respect to the wife's income and violence: $\frac{\partial}{\partial I^w}(\frac{\partial t}{\partial v})$.

Consider the case where $\frac{\partial}{\partial I^w}(\frac{\partial t}{\partial v}) < 0$. This condition implies that at higher levels of income the same amount of violence extracts lower transfers. This is likely to happen if marginal economic empowerement has a psychological effect on a woman - a marginal advance to the possibility of economic independence makes her more willing to defend both herself and her endowment. In this case the numerator of the expression (18) is negative, and the model predicts that marginal increase in a woman's income will trigger a decrease in violence.

On the other hand a positive cross-partial $\frac{\partial}{\partial I^w}(\frac{\partial t}{\partial v}) > 0$ lends itself to an intuitive interpretation equally easily. Violence may extract higher transfers at higher income levels due to diminishing marginal utility of income. In this case, violence will either decrease or increase depending on the interplay of the two forces: the change in the husband's utility due to the transfer, and the changes in the transfer driven by income and violence. Violence will decrease as long as the following condition holds:

$$-\frac{\frac{\partial^2 U^h}{\partial t^2}}{\frac{\partial U^h}{\partial t}} > \frac{\frac{\partial}{\partial I^w}(\frac{\partial t}{\partial v})}{\frac{\partial t}{\partial I^w}\frac{\partial t}{\partial v}} \tag{19}$$

In order to simplify and give intuitive interpretation to the condition (19), I impose additional assumption that the transfer is linear in income. In this case:

$$\frac{\frac{\partial}{\partial I^w}(\frac{\partial t}{\partial v})}{\frac{\partial t}{\partial v}} = \frac{1}{I^w} \tag{20}$$

Substituting (20) into (19) and multiplying both sides by t, I can rewrite equation (19) as :

$$-\frac{\frac{\partial^2 U^h}{\partial t^2}}{\frac{\partial U^h}{\partial t}}t > \frac{1}{\frac{\partial t}{\partial I^w}\frac{I}{t}} \tag{21}$$

The left hand side of this expression is relative risk aversion, and the right hand side is the inverse of elasticity of the transfer with respect to income. Though empirical estimates of the coefficient of relative risk aversion are limited, most economists believe that it is bound between 1 and 5[14]. Following this convention, I conclude that the low bound of income elasticity of the transfer for which the equation (21) holds is 0.2. The more elastic the transfer is, the more likely the condition (21) is satisfied. Intuitively, if the woman is not at her reservation utlity and violence becomes more effective at higher levels of income, increases in her income will trigger the reduction in violence as long as she voluntarily increases the transfer to the husband by at least 20 percent of the fraction of increase in I^w.

Now consider the case of an "unhappy marriage": the constraint is binding and $\lambda > 0$. The equilibrium can be described by equations (31) and

$$V^w(I^w) - U^w(I^w, t(v, I^w), v) = 0 \tag{22}$$

To derive the expression for $\frac{\partial v}{\partial I^w}$, I totally differentiate equations (31) and (22) with respect to v, I^w and λ, and solve the resulting system of equations:

[14]Chetty (2006).

$$\frac{\partial v}{\partial I^w} = \frac{\frac{\partial V^w}{\partial I^w} - \frac{\partial U^w}{\partial I^w} - \frac{\partial U^w}{\partial t}\frac{\partial t}{\partial I^w}}{\frac{\partial U^w}{\partial t}\frac{\partial t}{\partial v} + \frac{\partial U^w}{\partial v}} \tag{23}$$

The denominator of this expression is always negative, as the wife's utility decreases in the transfer and violence, and the transfer increases in violence. The impact of a marginal change in the woman's income on violence depends on the sign of the numerator. It will be negative, as long as:

$$\frac{\partial V^w}{\partial I^w} > \frac{\partial U^w}{\partial I^w} + \frac{\partial U^w}{\partial t}\frac{\partial t}{\partial I^w} \tag{24}$$

If I assume that the wife's within marriage utility changes in her discretionary income and in the transfer at the same rate, i.e. $\frac{\partial U^w}{\partial I^w} = -\frac{\partial U^w}{\partial t}$, I can rewrite (24) as:

$$\frac{\partial V^w}{\partial I^w} > \frac{\partial U^w}{\partial I^w}(1 - \frac{\partial t}{\partial I^w}) \tag{25}$$

Given that the increase in the transfer cannot exceed the increase in woman's discretionary income, or $\frac{\partial t}{\partial I^w} \leq 1$, condition (25) will hold as long as utility as married and outside marriage change in income at the same rate. However, the prediction is ambiguous if outside of marriage utility is increasing in income at a lower rate than within the marriage utility. This is likely to happen for women who face less favorable marriage markets, for example, women with children, or for women with very low human capital, whose opportunities of economic self-sufficiency are limited.

Overall, the model predicts that an increase in the victim's discretionary income will cause reduction in domestic violence under the following conditions: (i) the constraint is not binding and $\frac{\partial}{\partial I^w}(\frac{\partial t}{\partial v}) < 0$; (ii) the constraint is not binding, $\frac{\partial}{\partial I^w}(\frac{\partial t}{\partial v}) > 0$ and $-\frac{\frac{\partial^2 U^h}{\partial t^2}}{\frac{\partial U^h}{\partial t}}t > \frac{1}{\frac{\partial t}{\partial I^w}\frac{I}{t}}$; (iii) the constraint is binding, and $\frac{\partial V^w}{\partial I^w} \geq \frac{\partial U^w}{\partial I^w}$. Intuitively, violence will decrease in "happy" marriages, as long as it becomes less effective at higher levels of income, or if it becomes more effective, but the wife voluntarily increases the transfer by the amount greater than the inverse of the husband's relative risk aversion. Using conventional estimates of relative risk aversion, one can calculate that the increase in transfer should be at least 20 percent of the increase in income. In "unhappy" marriages violence will decrease as long as the outside of marriage utility increases in income at least at the same rate as the within marriage utility.

In order to empirically test the relationship between violence and the increases in the woman's discretionary income I take advantage of the data on the receipt of cash transfers as part of a CCT program Juntos in Peru. Using the data on the roll-out of the program, I can empirically test whether increases in the wives' discretionary income due to the transfer triggered decrease in violence. Due to the data limitations, I cannot empirically distinguish between "happy" and "unhappy" marriages, and cannot test whether the reduction in violence is due to the fact that one, two or all of the above mentioned conditions are satisfied. However, I can test whether violence decreases differentially among the women whose outside of marriage utility is likely to change in income at lower rate than their within marriage utility: women who are facing worse marriage market conditions in case of separation, or who have less opportunities for economic independence. The first group comprises women with many children and women who witnessed violent episodes between their parents. Exposure to violence between her parents makes a woman more likely to match with partners who have high propensity to exerting violence (Pollack, 2004), which makes the next possible marriage hardly preferable to the current one at least in terms of violence. The second group comprises women who either do not work, or work, but do not earn cash.

Juntos program and cultural context

Juntos program - conditions, eligibility, rollout

Peru's CCT program Juntos was started in 2005. Over the course of three years it expanded from covering approximately 37,000 households in 110 districts to providing benefits to over 454,000 households in 638 districts. Associated budget expenditures during this period grew from 116 million soles to 344 million soles. 637 districts where the program is run were identified among the poorest 880 districts in Peru under the CRECER (umbrella for social programs) initiative. Program expansion plans include incorporation of remaining 243 CRECER districts.

As most CCT programs, Juntos pursues the dual objective of reducing poverty in the short run by providing households with cash transfers, and breaking intergenerational transmission of poverty through investments in human capital via improved access to education and medical services. The program aims to achieve these objectives by providing eligible families with cash transfers as long as they comply with a set of educational and health conditions, summarized in Table 1.

Eligibility of the households is determined as a result of a three-stage process. First, districts are selected to participate in the program on basis of the following five criteria: (i) exposure to violence during Sendero Luminoso guerilla; (ii) poverty level, measured as a proportion of population with unsatisfied basic needs; (iii) poverty gap; (iv) child malnutrition level; and (v) poverty severity[15]. A weighted average of these characteristics (indicador sintético) was used to select 637 eligible districts. These districts were incorporated into the program in several stages; though an attempt was made to roll out the program according to the magnitude of indicador sintético, starting with districts in greater need; due to administrative shortcomings this order was not followed. Figures 1 shows the values of indicador sintético plotted against district enrollment dates. The rollout clearly was not carried out in the order of decreasing magnitude of the district score. In informal interviews the management of the program mentioned quality of roads and weather conditions among the reasons that changed the planned order of district incorporation. At the second stage of selection Instituto Nacional de Estadistica y Informatica (INEI) collected a census of households in the selected districts. INEI constructed a score based on observable household characteristics to determine the poverty status of the households. Households with at least one child under 14 and the score above a certain threshold qualified as eligible at the second stage.

At the final stage in order to minimize both inclusion and exclusion errors, the list of eligible according to the INEI score went through the process of community validation by community members, local authorities and representatives from the Ministries of Education and Health.

[15] Poverty gap and poverty severity are defined as Foster, Greer and Thorbecke (1984) poverty measures with $\alpha = 1$ in case of poverty gap and $\alpha = 2$ in case of poverty severity: $P_\alpha(y,z) = \frac{1}{n}\sum_{i=1}^{q}(\frac{z-y_i}{z})^\alpha$, where n is the number of people, q is the number of people below poverty line, z is the poverty line and y_i is i's household income.

Could other features of the program, apart from the transfer, affect domestic violence?

None of the conditions of the program require partners of the beneficiaries to stop domestic violence - women are eligible for the transfer regardless of the extent of abuse in the household. Moreover, the reduction of domestic violence has never been mentioned as a desirable outcome in the social marketing of the program or even in the internal administrative documents. Qualitative evaluation of Juntos conducted by Jones, Vargas and Villar (2006) includes a module where respondents are asked on what conditions they are receiving the transfer. None of the focus group participants mentioned abstaining from domestic violence. Moreover, the same qualitative evaluation renders some evidence of the reduction in domestic abuse due to participation in Juntos. Focus group participants report decreases in domestic violence. They attribute it to greater autonomy and negotiation power of the women due to receipt of additional discretionary income and to the reduction in the pressure to ensure daily subsitence - the model takes into account both channels.

Although in the empirical work I cannot separate the effect of the transfer from the other features of the program[16], there are neither theoretical reasons, nor evidence from qualitative work to believe that other features of the program could affect domestic violence. Therefore, by estimating the impact of participation in Juntos, I can provide an estimate for the effect of an increase in the woman's discretionary income on domestic violence against her.

Is separation a feasible option?

The model developed in Section 2 assumes that separation or divorce is a feasible option: $V^w(I^w) > 0$. Other models of domestic violence - Bloch and Rao (2005), Bobonis et al. (2009) rely on the assumption that separation is not feasible and yield a set of alternative predictions. To ensure that my model is a good fit for the actual dynamics of domestic violence in Peru, I check the plausibility of this crucial assumption with the data on divorce and separation rates in Juntos villages. Table 13 shows the percentages of divorced and separated women in Peru and in Juntos districts. Though the rate of formal divorces is low all over the country, separation remains a feasible exit strategy - 10 percent of evermarried women in Peru are separated from their husbands. The fraction of separations is lower in Juntos districts, which is probably due to differences in cultural norms across urban and rural, predominantely indigenous or white areas. However, dissolution of marriage remains a feasible "threat point".

Data

To estimate the relationship between women's discretionary income and domestic violence I combine several sources of data.

My primary data set is Demographic and Family Health Survey (Encuesta Demografica y de Salud Familiar - ENDES). The primary objective of the survey is to provide data on demographic and health information of mothers and children under 5. The survey contains detailed information

[16]Table 1 gives the list of program conditionalities. Other changes in the life of the community associated with the arrival of the program that can potentially have impact on domestic violence are discussed in Section 7.

on individual and household socio-economic characteristics, as well as a module on domestic violence. The survey is a rotating panel of districts and was administered every four years prior to 2004, and yearly afterwards. I use the data from 2000, and 2004 through 2008 rounds. Domestic violence questions are administered to women aged 15 to 49, who have ever been married or lived together with a partner. In 2004 through 2007 approximately 3.5 thousand women were interviewed about their experiences of domestic violence, in 2000 and 2008 there are 18,182 and 6,689 respondents, respectively.

The second data source is the census of household conducted in all Juntos districts in 2005 by INEI. These data were used to derive the eligibility score and allow me to reproduce the exact number of eligible families according to eligibility score.

Juntos administrative data provide information on the dates when districts were incorporated into the program, as well as monthly data on the number of transfer recipients.

Construction of domestic violence variables

Domestic violence is a complex phenomenon, which takes multiple forms. ENDES survey addresses the challenge of capturing various aspects of domestic abuse by including in the interview approximately 20 questions about different types of violence. I present the complete list of the questions used for the construction of measures of domestic violence in the Appendix.

I aggregate these questions into three major categories: physical, sexual and emotional violence, trying to adhere to the internal logic of the survey. The interview is divided into several parts, where questions about emotional or physical violence are grouped together: this grouping may affect interpretation of the question by the respondent.[17]

I use the questions about domestic abuse which consist of two parts: first, an interviewer asks a respondent whether she has ever experienced some type of violence, and then if the answer is positive inquires whether it happened during the last 12 months. My primary outcome of interest is the change in violence during the last 12 months; however, I also report the fractions of women who have ever experienced the three types of violence.

The physical violence indicator is equal to one if a woman experienced at least one type of physical violence, such as pushing, slapping, hitting, attacking with weapons or attempts to strangulate or burn during the last 12 months. The sexual violence indicator is equal to one if during the last 12 months a respondent's partner/husband forced her to have sexual relations or to participate in sexual acts she did not approve of. The emotional violence indicator is equal to one if a woman either reports having been humiliated by her husband/partner, or if he threatened to do harm to her or to anyone she cares about, or if he threatened to leave and deprive her of economic aid in the last 12 months. To ensure that the observed impacts are not artifacts of the variable construction, I also estimate the effects of increase in discretionary income on all the components of these indicators

[17] For example, there are several questions that ask about the threats, but one of them ("Has your husband ever threatened you with a weapon?") follows the long list of questions about clearly physical violence, such as pushing and kicking. The other two questions ("Has your husband ever threatened to do you harm?" and "Has your husband ever threatened to leave you and deprive you of economic aid?") are asked together with questions about humiliation and other types of emotional violence. I only use the latter two questions in the construction of the emotional violence measure.

individually[18].

Table 14 shows the fractions of women who have experienced these types of violence at some point in their lives and during the last 12 months in 2005, prior to the arrival of the program to any of the districts. Though the program was started in 2005, none of the districts which received Juntos in the first year is included in ENDES sample. The prevalence of violence presented in Table 14 demonstrates that domestic abuse is an acute problem in Peru: 38 percent of the women in the sample reported having ever experienced physical violence. This is high compared not only to Western Europe and the USA, but to other Latin American and even African countries. Garcia-Moreno (2006) reports similar estimates from the WHO study conducted in 9 other countries: Bangladesh, Brazil, Ethiopia, Japan, Namibia, Samoa, Serbia, Montenegro, Thailand and Tanzania. Of all these countries, only in Ethiopia, Tanzania, Samoa and Bangladesh the fraction of women who experienced physical violence is a few percentage points higher: 48.7, 46.7, 40.5 and 41.7 percent, respectively. The prevalence of sexual violence (10 percent) compares favorably with African and Asian countries, where the same indicator mostly exceeds 20 percent and with Brazil, where the prevalence of sexual violence reaches 14 percent in rural areas (Garcia-Moreno, 2006), but is higher than in Japan and Serbia and Montenegro. The fractions of women who reported having been subjected to violence during the last 12 months are also high: 14 percent for physical violence, 4 and 15 percent for sexual and emotional violence, respectively.

Identification strategy and results

In order to estimate the impact of Juntos on the incidence of domestic violence using repeated cross-section as a primary data source, I apply difference-in-difference approach, and run the following regression:

$$V_{ijt} = \alpha_j + \beta_t + \gamma J_{jt} + \mathbf{X}_{ijt}\lambda + \varepsilon_{ijt}, \tag{26}$$

where V_{ijt} is a dummy variable equal to 1 if a woman i in district j in year t experienced physical, sexual or emotional violence. α_j and β_t are district and year fixed effects. J_{jt} is a treatment variable which captures exposure to Juntos: it is equal to 1 if a district j received the program in year t, and 0 otherwise. $\mathbf{X}_{ijt}$ is a vector of individual and household characteristics. It contains data on age, number of children, indicators for completion of primary and secondary education, literacy indicator and indicator for cohabitation (as opposed to formal marriage), a dummy equal to 1 if a woman is insured in order to proxy access to medical services and a dummy equal to 1 if a woman has a cash-paying job. Given theoretical evidence that there is intergenerational transmission of violence (Pollack, 2004), I also include a dummy equal to 1 if a woman has ever seen her father beat her mother.

Household characteristics include Juntos eligibility score and its square as well as the variables used in the construction of the score: access to running water, hygienic restroom and electricity, indicator equal to 1 if a household uses industrial fuel, indicators of the types of materiasl used in the construction of the dwelling, and a variable used in the calculation of eligibility score which captures the number of durable goods (such as TV, vehicle, bicycle) that a household does not have.

[18]Results available upon request from the author

In addition, I include an indicator for land ownership and an indicator equal to 1 if the household is in an urban area. Table 15 presents the means and standard deviations of all control variables.

The identification strategy relies on the assumption that in the absense of the program the changes in incidence of domestic violence would not have been different in treated and control districts. To increase the likelihood that this assumption holds I limit the sample to 880 districts designated as the districts in the highest need of governmental assistance under CRECER initiative. Of these districts, 637 were integrated in Juntos between 2005 and 2008; however, the government of Peru considers the remaining 243 districts similarly in need of governmental assistance and plans to incorporate them in the program in the future. Furthermore, I can assess the validity of the identification assumption, taking advantage of the fact that ENDES survey was administered several years prior to the rollout of the program.

Using ENDES 2000 and 2004, I can test whether there are systematic changes in violence outcomes that are different between treatment and control groups prior to the rollout of the program. Unfortunately, the question about violent experiences in the last 12 months does not appear in ENDES prior to 2004; moreover, there were no questions about emotional and sexual violence in ENDES 2000. Consequently, I will only be able to test for systematic differences in the dynamics of domestic abuse using the question about having ever experienced physical violence. I estimate equation (30), where the indicator t takes values "2000" and "2004". J_{jt} takes the value of 1 if an observation is from a district that received the program after roll-out was initiated, and comes from 2004 survey. Otherwise, the notation remains the same. Coefficient γ captures differences in changes in outcome variable from 2000 to 2004 between Juntos districts and districts that never received the program. Given that none of the Juntos districts was enrolled in the program prior to 2005, as long as there are no systematic changes in violence outcomes that are different across treatment and control groups, the coefficient γ should not be significantly different from 0. I present the results from estimation of regression (30) with 2000 and 2004 data in Table 16[19]. The coefficient on the interaction between the 2004 dummy and the indicator of participation in Juntos program is not statistically distinguishable from zero in both specifications: with and without controls. Thus I can rule out the possibility that the changes in the incidence of domestic violence in Juntos districts after the introduction of the program are due to the trend.

Nevertheless, the test described above does not address the possibility that the error term in equation (30) may conceal time-variant omitted variables, which simultaneously affect the outcome variable and the roll-out of the program. For example, consider a hypothetical situation where Juntos districts were also more likely to enjoy an expansion of medical services. A visit of a battered woman to the doctor makes public the abuser's behavior. Such exposure may serve as a deterrent to exert violence, and I run the risk of erroneously attributing its effect to the increases in the discretionary income of women due to the CCT program.

In order to mitigate this risk, I include a rich set of controls. Additionally, I can assess the importance of omitted variables compared to the variables I can control for, using methodology proposed by Altonji (2005) et al. and modified by Bellows and Miguel (2009). As in Bellows and Miguel (2009), I derive the estimate of the importance of omitted variables compared to observed controls by examining the changes in the magnitude of γ as the controls are added. Table 17

[19]Two regressors are not available in 2000 and 2004 rounds of ENDES: insurance indicator and land owndership indicator. Excluding them from the estimation of the impact of Juntos on domestic violence with 2005-2008 data does not affect the results.

presents the results of estimating regression (30) with and without controls.

I find that Juntos had significant negative impact on the prevalence of physical violence and emotional violence. District enrollment in Juntos decreases the fraction of women exposed to physical violence by 9 percentage points on average, and the fraction of women exposed to emotional violence by 11 percentage points. These are over 50 percent decreases relative to pre-treatment levels of these types of violence (as reported in Table 14). The magnitude of these coefficients hardly changes when I add the extensive set of controls - it drops from 0.1 to 0.09 in case of physical violence and from 0.1066 to 0.1062 in case of emotional violence. Using Bellows and Miguel (2009) methodology, I find that in order to explain away the impact of Juntos, the influence of omitted variables should exceed the influence of all observed variables by more than 7 times in case of physical violence, and by more than 200 times in case of emotional violence. The point estimates of the coefficients on district enrollment in Juntos in the regressions with sexual violence are negative, but are not precisely estimated. Overall, the data present strong evidence that district participation in the CCT program decreased domestic violence.

Heterogeneity analysis

Are the impacts driven by beneficiaries?

The analysis above convincingly shows that arrival of Juntos program triggered a decrease in the prevalence of domestic violence. There are neither theoretical reasons, nor qualitative evidence that other features of the program could also affect domestic abuse. However, I can strengthen the case that the reduction in domestic violence associated with participation in Juntos is indeed due to increases in women's discretionary income by demonstrating that it is driven by the beneficiaries.

Consider the following example. The majority of beneficiary districts are poor, isolated districts with low presence of government. The mere presence of administrative officials due to the program generates a big change in the life of these communities, and may serve as a reminder to potential abusers that domestic violence is a punishable crime, thus detering its exertion. Showing that the observed impacts are driven by beneficiaries only would rule out this possibility: presence of administrative officials affects all residents, regardless of their beneficiary status.

Unfortunately, only the 2008 round of ENDES survey contains the question about participation in Juntos program. Therefore I cannot estimate regression (30) separately on the subsample of beneficiaries and non-beneficiaries. An alternative approach would be to use eligibility status as a proxy for receiving benefits. ENDES survey contains almost all of the questions that were used in construction of eligibility score with 2005 INEI census. However, the phrasing of the questions is not always the same, and while actual eligibility was determined using 2005 answers, I can only use the data from the year when an individual participated in ENDES survey.

Consequently eligibility score based on ENDES data may differ from the actual eligibility score, and may not be a good proxy for receiving Juntos benefits. I can assess its quality using 2008 round of the survey, which contains both: actual beneficiary status and the necessary inputs for calculating eligibility score. Figure 5 shows the fraction of misassigned observations for every value of eligibility score, weighted by the total number of observations with this value of eligibility score. I refer to an observation as misassigned if a respondent is eligible according to the calculated score, but does not receive benefits, or if she is not eligible but reports receiving Juntos transfer.

Figure 5 shows that the fraction of misassignments is high for all the values of the eligibility score, not only close to the eligibility threshold, and rules out the use of ENDES-based eligibility score as a proxy for beneficiary status.

Alternatively, I can explore the variation in impacts depending on the fraction of beneficiaries among the eligible at the time when ENDES interviews were conducted. Juntos administrative data show that the enrollment of eligible into the program was happening gradually - not everyone who qualified for benefits started receiving them in the first month. If the impact is entirely driven by beneficiaries, I would expect it to be higher in the districts where 90 percent of eligible women are already receiving benefits compared to the districts where only 10 percent are.

I calculate the number of women who received the transfer in any given month after the district was incorporated in the program using Juntos administrative data, and the number of eligible women using the INEI census of Juntos districts. To determine whether the impacts are driven by beneficiaries, I estimate the following regression:

$$V_{ijt} = \alpha_j + \beta_t + \gamma J_{jt} + \theta J_{jt}(F_{jt} - \bar{F}) + \mu(F_{jt} - \bar{F})\beta_t + \mathbf{X}_{ijt}\lambda + \varepsilon_{ijt}, \quad (27)$$

where F_{jt} the fraction of beneficiaries among eligible in district j in year t, and $\bar{F}$ is the average of these fractions across all districts and years. The rest of the notation is the same as in equation (30). As the number of beneficiaries varied each month, I use the number of beneficiaries in the month by which all ENDES interviews in the district were conducted. To account for the trend in the number of beneficiaries, I include interactions between time effects and district deviation from the mean fraction of beneficiaries: $F_{jt} - \bar{F}$. Coefficient θ on the interaction term captures the marginal change in the impact of the program on domestic violence as the fraction of beneificiaries changes. Table 18 presents estimation results. The coefficient on the interaction term is negative and strongly significant. For the districts where the fraction of beneficiaries is 10 percentage points above the average, domestic violence goes down by additional 5 percentage points, and emotional violence - by additional 4 percentage points. The test confirms that the impacts significantly vary depending on the fraction of beneficiaries.

Heterogeneity in impacts depending on the out-of-marriage prospects

The model developed in Section 2 outlines the conditions under which increases in the wife's income lead to a reduction in domestic violence. In the "unhappy marriages" violence goes down if a woman's outside of marriage utility changes in income at least at the same rate as her utility within marriage. If this prediction is consistent with the data, I should observe higher decreases in domestic violence among women with better outside of marriage options. I test this prediction by comparing impacts among several groups of women: (i) women with different number of children; (ii) women who as children witnessed violence among their parents; and (iii) women who have a cash-paying job.

Higher number of children implies that to support them in case of separation a woman needs higher independent income. As the system of alimony is barely existent and not enforced in rural Peru, the threshold level of income at which separation is feasible increases with every child. Additionally, a woman with many children faces a worse marriage market after separation: she is less likely to find a new partner. Exposure to violence between parents, according to Pollack (2004) increases the likelihood that a woman matches with a violent partner. Such predisposition for violent relationships results in lower outside of marriage utility as the marriage market faced by these women is worse at least in terms of domestic violence compared to the marriage market faced by women who are less likely to self-select in violent relationships. It is almost self-evident that women who have cash-paying jobs have better outside of marriage options: they are more likely to be able to support themselves and their children in case of separation. While Juntos transfer alone may not be sufficient to support a family - it constitutes 30 percent of average household spending, for women who have a cash-paying job the transfer may raise their independent income to the threshold level needed for the separation.

Let D_{ijt} be a variable that distinguishes women of different types in these 3 groups. In the first case, D_{ijt} is the number of children a woman i in district j in year t has; in the second case, D_{ijt} is a dummy variable equal to 1 if a woman reports having ever seen her father beat her mother; and in the third case D_{ijt} is equal to 1 if a woman is working and is paid in cash for her work.

In order to capture heterogeneity in impacts depending on the out-of-marriage prospects, I estimate the following regression:

$$V_{ijt} = \alpha_j + \beta_t + \gamma J_{jt} + \eta J_{jt} D_{ijt} + \lambda D_{ijt} + \mathbf{X}_{ijt}\varphi + \varepsilon_{ijt}, \tag{28}$$

where the notation is the same as in equation (30). The coefficient η on the interaction between treatment variable and group indicator D_{ijt} captures differences in the impact of the enrollment in Juntos depending on the number of children, exposure to violence as a child or availability of a cash-paying job. Table 19 presents the results.

Consistent with the model's predictions, coefficients are positive on the interactions of treatment with the number of children and with the exposure to violence indicator, and negative on the interactions with indicators for availability of a cash-paying job. Not all of the coefficients are precisely estimated. The number of children appears to significantly affect only the decrease in emotional violence. Every additional child reduces the impact of the program by 2 percentage points. While the arrival of the program triggered 15 percentage points decrease in violence among women without any children, among women with 3 children violence decreased by 9 percentage points only. The average number of children in the sample is 2.3 (Table 15).

The reductions in all types of violence are lower for women who reported having witnessed violent interactions among their parents; however, they are precisely estimated only for sexual violence. The coefficient on the interaction is 0.03 and is higher in magnitude than the coefficient on the treatment variable - 0.02.

For women who have cash-paying jobs sexual and emotional violence fell more than for women who either do not work, or who are paid in-kind. While the overall change in sexual violence is estimated to be 0, it fell by 5 percentage points among women with cash-paying jobs. Their exposure to emotional violence went down by 16 percentage points, compared to 9 percentage points among women who either do not work or are paid in-kind. Overall, the empirical results are consistent with the predictions of the model: the change in incidence of violence varies depending on the woman's outside of marriage options. It is lower for women who face worse marriage markets: either have many children or have witnessed violence among their parents, and is higher for women who are more likely to be able to support themselves without a partner - have a cash-paying job.

Robustness check

As a robustness check, I assess the impact of Juntos on domestic violence applying the matching estimator developed by Abadie and Imbens (2006) to the 2008 round of ENDES survey. In 2008 a question whether a respondent was receiving Juntos transfers was added to the survey. This allows me to identify individual beneficiaries and estimate the impact of the program by comparing them to individuals with similar observable characteristics who are not receiving the program.

For the matching estimator to provide unbiased estimate of the impact of Juntos on domestic violence, an "ignorable treatment" assumption should hold (Imbens (2004)). This assumption requires that potentital outcomes, conditional on the observed characteristics, are independent of treatment assignment. In other words, matching is an appropriate estimation technique for the settings where, conditional on observed variables, one can assume the treatment to be exogenous.

In the context of Juntos program participating districts and beneficiary households are selected according to a clearly specified rule. Moreover, while a subset of CRECER districts - 637 - were incorporated in the program between 2005 and 2008, the government plans to expand the program to the remaining CRECER districts. All 880 CRECER districts were selected on basis of the same criteria and are regarded equally in need of governmental assistance. If the matches are drawn according to the same selection criterion that was used by the Juntos program to select the current beneficiaries and the pool of control units is limited to the districts included in the program expansion plans, the ignorable treatment assumption is likely to hold. Intuitively, I will be matching beneficiaries to the individuals that would have become beneficiaries, had the program already arrived to their districts.

More formally, I will estimate the impact of Juntos on domestic violence as:

$$\theta_m = \frac{1}{N}\sum_{N}(\hat{V}_i(1) - \hat{V}_i(0)) \tag{29}$$

If respondent i is a beneficiary, $\hat{V}_i(1)$ is an observed outcome, and $\widehat{V}_i(0)$ is the weighted average of outcomes of similar individuals who are not receiving the transfer. If respondent i is a beneficiary, then $\widehat{V}_i(0)$ is an observed outcome, and $\hat{V}_i(1)$ is the weighted average of outcomes of similar individuals who are receiving the transfer. To derive the matching estimator of the average treatment effect, I average the differences between $\hat{V}_i(1)$ and $\widehat{V}_i(0)$ over all the respondents in the sample: Juntos beneficiaries and non-beneficiaries from CRECER districts[20]. Juntos beneficiaries are matched to individuals in CRECER districts that are still not incorporated in the program on basis of "indicador sintetico" - district level poverty score, the score used to determine eligibility and its square as well as all the covariates that I used in the difference-in-difference regression estimation.

Table 20 presents the results. The matching coefficients are consistent with the results from difference-in-difference estimation: the impacts on physical and emotional violence are negative and similar in magnitude: 8 and 10 percentage points, respectively; the impacts on sexual violence is not statistically distinguishable from 0.

[20]See Abadie et al. (2004) for more formal presentation of the matching estimator

Conclusion

Despite universal recognition of the gravity and prevalance of the problem of domestic abuse, very little attention has been dedicated to it in economic literature. Existing theoretical models address two distinct aspects of the relationship between violence and the income of the victim separately. Nash-bargaining models focus on the fact that increases in income improve the victim's threat point, and therefore equilibrium allocation of goods. The models of bargaining with asymmetric information, pioneered by Bloch and Rao (2006) and later adopted by Bobonis et al. (2009) treat violence as a bargaining tool, and predict that increases in the victim's income create greater incentive for the abuser to extract rents.

In this paper I develop a model which incorporates both these features: a husband may use violence to extract rents from the wife, however he is constrained by the need to maintain her within marriage utility at least at the level of the threat point - her outside option. The model yields a set of conditions under which increases in the wife's discretionary income trigger decrease in domestic violence.

I test the model using the data on domestic violence and participation in a CCT program Juntos in Peru. As part of the program, beneficiary women receive 100 soles. I argue that the transfer is the predominant aspect of the program that affects domestic violence, and estimate the effect of the program on domestic abuse. Two estimation techniques - difference-in-difference analysis and matching - yield similar results. I find decrease in the incindence of physical and emotional violence of 9 and 11 percentage points, respectively, when using difference-in-difference, and of 8 and 10 percentage points when using matching. I validate the assumption that the predominant chanel through which the program affected violence is the transfer by demonstrating that the impacts are driven by beneficiaries.

The model also predicts that for women pushed to their reservation utility, decreases in violence should be lower among those whose utility outside of marriage increases in income at a lower rate than within marriage utility. I empirically test this prediction by exploring heterogeneity in impacts among women with better and worse outside of marriage options. I find that higher number of children as well as exposure to violence as a child are associated with lower decreases in violence, while having a cash-paying job decreases violence by higher than average margin.

The paper contributes to furthering understanding of economic motivation behind the phenomenon of domestic abuse. It shows that despite the possibility that higher income may create stronger incentives to use violence as a rent extraction mechanism, in poor rural areas in Peru where Juntos program was rolled out the "better threat-point effect" dominates rent-extraction effect of the increases in the wives' discretionary income. These findings are likely to hold for similarly poor communities - where domestic violence is particularly prevalent (Jewkes, 2002) - and therefore can serve as an input for policy debate on how to mitigate domestic violence. Finally, the paper contributes to the extensive impact evaluation literature of the CCT programs. I convincingly demonstrate that reduction in domestic violence is yet another impact of the CCT programs.

Chapter 3: Welfare Impacts of the Juntos Program in Peru: Evidence from a non-experimental evaluation[21]

Introduction

Peru's conditional cash transfer (CCT) program, JUNTOS, commenced in 2005. It has since grown from operating in 110 districts and covering about 37,000 households, to 637 districts and about 454,000 households. Associated budget expenditures increased from 116 million soles in 2005 to 344 million soles in 2008. The Program ultimately plans to expand to all 880 of the poorest districts in Peru.

Despite the great success of CCT programs around the world in the last decade, the introduction of Juntos as one of Peru's flagship social programs has received mixed reactions. Partly, this cool welcome was due to the fact that the discussions about the program have been centered around political issues instead of the actual evidence of its merits. Unfortunately, Juntos did not integrate a systematic impact evaluation in its initial design. As such, little quantitative information has been available about the impact of Juntos and its ability to achieve its key objectives of reducing poverty and building human capital.

This study provides the first quantitative impact evaluation of the Juntos program. Using available sources, we construct a data set which allows us to evaluate the impact of Juntos on beneficiaries during 2006 and 2007. The study is organized as follows. Section 2 provides a description of the Juntos program and its main components. Section 3 discusses the econometric methodology; section 4 presents the results, while section 5 concludes and offers a number of recommendations.

Program description

As most CCT programs, Juntos pursues the dual objective of reducing poverty in the short run by providing households with cash transfers, and in the long run breaking intergenerational transmission of poverty through investments in human capital via improved access to education and medical services. The program achieves these objectives through the provision of eligible households with a monthly cash transfer of S./ 100 (soles)[22] conditional on a number of requirements, which vary depending on age and gender of beneficiaries and are summarized in Table 1. Unlike in other CCT programs, Juntos transfer is a lump -sum payment and does not vary depending on the number of children.

The selection of the beneficiary households is comprised of three stages: selection of eligible districts, selection of eligible households within the eligible districts and finally a community level validation. At the first stage, participating districts were selected on the basis of the five criteria: (i) exposure to violence during Sendero Luminoso guerilla; (ii) poverty level, measured as a proportion of population with unsatisfied basic needs; (iii) poverty gap; (iv) level of child

[21] This chapter is based on a paper co-authored with Renos Vakis

[22] 1 nuevo sol is approximately equal to \$0.35.

malnutrition; and (v) presence of extreme income poverty[23]. A weighted average of these characteristics - indicador sintético - was used to select 637 districts. These districts were incorporated into the program in several stages; although an attempt was made to roll out the program according to the magnitude of indicador sintético, starting with districts in greater need, due to administrative shortcomings this order was not followed. Figure 1 shows the values of indicador sintético plotted against district enrollment dates. The rollout clearly was not carried out in the order of decreasing magnitude of the district score. In informal interviews the management of the program mentioned weather conditions and shortage of promotoras[24] among the reasons that changed the planned order of district incorporation.

In the second stage, a census of all households in each of the eligible districts was collected by the Instituto Nacional de Estadistica e Informatica (INEI). On basis of these data, a poverty score was calculated for each household[25]. Households with the score above a certain threshold qualified as eligible. Given that the primary focus of the program is on young children and pregnant mothers, only households with children under 14 years or a pregnant woman were selected. In the final stage in order to minimize both exclusion and inclusion errors the list of eligible according to poverty score went through the process of community validation by community members, local authorities and representatives from the Ministries of Education and Health.

Impact evaluation methodology

Unfortunately, an impact evaluation framework was not incorporated in the design of the Juntos program prior to its rollout. Consequently, the feasibility of an impact evaluation depends on the existence of data on program beneficiaries and the possibility to credibly construct counterfactual control groups through the use of econometric techniques. This section describes the estimation methodology employed to accomplish this goal and the data used in the process.

Data

A number of data sources can be combined to facilitate a non-experimental impact evaluation. First, the household survey Encuesta Nacional de Hogares (ENAHO) allows us to identify individuals who participated in Juntos in 2006 and 2007. ENAHO is a continuous survey (annual) and contains rich data on household consumption and spending patterns, household assets, education and health. Based on the question about participation in welfare programs, 1,262 ENAHO households were identified as beneficiaries of Juntos in 2006 or in 2007.

The second source of data is the census of Juntos districts carried out by INEI in 2005. The census was used to determine households' eligibility - the poverty score described above is based

[23]This information comes from various sources including Ministry of Economy and Finance, FONCODES poverty map, reports of the Truth and Reconciliation Commission and the national census.

[24]Promotoras are women responsible for explaining the essense, rules and goals of the program to the beneficiaries and verifying compliance with the conditions of the program.

[25]Poverty score is a weighted average of the following household characteristics: the presence of a school-age child out of school, the presence of adult illiterate woman, type of materials used in construction of the walls, roof and floor of the house, access to potable water, sewage and electricity and a score equal to the number of assets that a household does not have (such as TV, vehicle, refrigerator).

on the data from the census. Consequently, this database includes detailed information on household assets, characteristics of the dwelling, demographic characteristics and the level of education of the household members.

The third data source is the Registro Nacional de Municipaidades (RENAMU) for 2006 and 2007. This database contains information on infrastructure, public services, economic activity and other characteristics of the districts and provides an opportunity to take into account district-level heterogeneity.

Juntos administrative data provide information on the dates when districts were incorporated into the program, as well as monthly data on the number of transfer recipients.

Finally, we use the national population census of 2005. Though it is impossible to identify Juntos beneficiaries in the census data, we can distinguish between participating and non-participating districts and calculate pre-treatment averages of the variables of interest at the district level.

Estimation methodology

Given non-random rollout of the program, matching seems to be the natural choice for the impact evaluation. Matching techniques allow one to construct an artificial counterfactual – a control group, created of households who are similar to the beneficiaries except for the fact that they did not receive the transfer. These techniques provide a credible empirical framework for impact evaluation in the absence of random assignment (Abadie and Imbens [2006], Imbens [2004], Rosenbaum and Rubin [1983]).

Matching has been widely used in empirical work, and a variety of matching estimators have been developed (Imbens [2004]). We choose to evaluate the impact of Juntos by matching observationally similar households to beneficiary households on basis of propensity score, and conducting regression analysis on the matched sample.

Propensity score matching

Let T_i be an indicator of participation in Juntos, where $T_i = 1$ if a household i is a beneficiary of the program, and $T_i = 0$ otherwise. Let Y be an outcome of interest and $\mathbf{X}$ a vector of observable characteristics. Following Ronsenbaum and Rubin [1983], a control group can be constructed out of observationally similar households using "propensity scores" or probabilities of participation conditional on a vector of observable characteristics, given by

$$P(T_i) = P(T_i|\mathbf{X_i}) \tag{30}$$

The effect of the program can be identified using propensity score matching, if two assumptions, the "overlap" and "ignorable treatment", hold (Imbens [2004]). The first assumption implies that there should be significant overlap in the distributions of the observed covariates of treated and control units. The ignorable treatment assumption requires that potential outcomes, conditional on the observed characteristics, are independent of treatment assignment:

$$Y(0), Y(1) \perp T|\mathbf{X} \tag{31}$$

In order to increase the probability that untestable ignorable treatment assumption holds, we include in the vector of matching covariates $\mathbf{X}$ the components of the formula used for the selection of beneficiary districts and the poverty score used for beneficiary selection. Additionally, we limit the pool of potential matches to the CRECER districts only. Under the CRECER initiative, the Government of Peru selected 880 districts as the ones in the highest need of governmental assistnace. While 637 CRECER districts were incorporated in Juntos between 2005 and 2008, the government plans to expand the program to the remaining CRECER districts. All 880 CRECER districts were selected on basis of the same criteria and are regarded equally in need of governmental assistance. If the matches are drawn according to the same selection criterion that was used by the Juntos program to select the current beneficiaries and the pool of control units is limited to the districts included in the program expansion plans, the ignorable treatment assumption is likely to hold.

Intuitively, we will be matching beneficiaries to the individuals that would have become beneficiaries, had the program already arrived to their districts[26]. Finally, we include in vector **X** district level averages of some of the components of the poverty score, calculated from 2005 national census. These variables allow us to control for pre-treatment district heterogeneity.

The final set of matching covariates is chosen to ensure that the common support is balanced (Becker and Ichino [2007]), i.e. that for all covariates used in the propensity score regression, there is no statistical difference between control and treatment along the propensity score distribution. Following common practice, we include interactions and non-linear terms of the basic matching covariates in the propensity score equation in order to facilitate balancing of the common support (Ho et al. [2007]).

Table 21 shows the probit regression used for estimating the propensity score function. Most of the covariates used are significant predictors of participation in Juntos. In addition, overall predictive power of the probit is high (pseudo R-squared is 0.214).

We carry out analysis using observations from the common support only, i.e. to observations, which belong to the overlapping regions of the empirical densities of matching covariates for treated and control units. The region of overlapping in the distribution of propensity score (Figure 6) indicates that high proportion of treated and control units are similar in their observed characteristics. The range of common support is between 0.01-0.99. Even with this wide range, 416 observations out of 6,151 fall outside of the common support and are not used in the analysis.

In order to create the final dataset for the analysis, we use "nearest neighbor" matching. Specifically, every beneficiary household is matched to a non-Juntos household within the common support that has the closest estimated propensity score. If $C(i)$ denotes the set of observations matched to unit i with propensity score p_i, then the nearest neighbor is defined as: $C(i) = \min|p_i - p_j|$[27](Becker and Ichino [2007]). Using the sample of beneficiaries and their "nearest neighbors", we estimate the program impact by comparing means of outcome indicators for these two groups in the regression framework.

Average treatment effect estimator

We use parametric estimation on the matched sample for a number of reasons. Firstly, propensity score matching leaves some correlation between the probability to be treated and matching variables (Ho et al. [2007]). Estimating the treatment effect parametrically allows us to purge

[26]To ensure that Juntos beneficiaries are not matched to non-beneficiaries in the same districts, we exclude non-beneficiaries in Juntos districts from the matching exercise.

[27]In case of ties, potential controls were randomly chosen

this correlation by including matching covariates directly in the regression. Secondly, regression analysis also offers a more intuitive interpretation of the coefficients, consistent with theory. For example, apart from simply measuring average effect, one can explore its variation depending on the presence of infrastructure or individual characteristics. Other advantages of parametric framework include the possibility to control for variables not included in the matching algorithm, which can nevertheless affect the outcome –such as pre-treatment outcome levels, or survey dates. With these considerations in mind, we estimate the average effect of Juntos for individual or household-level outcomes parametrically by running the following regression:

$$Y_i = \alpha_1 + \alpha_2 T_i + \alpha_3 \mathbf{X_i} + \alpha_4 \mathbf{Z_i} + \xi_i \tag{32}$$

where Y_i denotes an outcome of interest, T_i is a dummy equal to one if an individual/household i benefits from the program, $\mathbf{X_i}$ is a vector of matching covariates, and $\mathbf{Z_i}$ is a vector of additional individual, households and district level characteristics potentially correlated with the outcome. The vector $\mathbf{X_i}$ includes pre-program district levels of poverty, childhood malnutrition, violence, per capita household monthly spending and the household level proxy means indicator. In addition, the vector $\mathbf{Z_i}$ includes 2005 (pre-program) district averages of the outcome variable (when it exists)[28], household propensity score, and household size. In individual level regressions vector $\mathbf{Z_i}$ also includes age, age squared and gender. The impact of Juntos is captured in the coefficient α_2.

Regression framework also allows us to explore heterogeneity in the effect of the treatment by age and gender by estimating equation (32) separately on corresponding sub-samples.

Intensity (dose) analysis

Combining Juntos administrative data with the ENAHO data makes it possible to calculate a good proxy for the number of months a given household has been "treated". The operational guildelines of Juntos require that all beneficiaries in the district are enrolled in the program approximately at the same time; thus district enrollment date available from Juntos administrative data is a good proxy for the month when an individual in this district started receiving benefits. ENAHO survey contains the date of the interview. The difference between these two dates yields a good approximation of individual participation time.

District level variation in Juntos enrollment dates as well as individual level variation in the timing of ENAHO interviews allows for exploring treatment intensity effects: we can examine how the impacts vary depending on how long a household has been receiving benefits. Let L_{1i} be a dummy equal to 1 if a respondent i participated in the program for 12 months or less, L_{2i} is equal to 1 if a respondent participated in Juntos from 13 to 25 months. To capture heterogeneity in the effects depending on the length of the treatment, we introduce the interactions between these variables and the treatment in individual and household level regressions:

$$Y_i = \gamma_0 + \gamma_1 T_i L_{1i} + \gamma_2 T_i L_{2i} + \gamma_3 \mathbf{X_i} + \gamma_4 \mathbf{Z_i} + \varepsilon_i \tag{33}$$

Coefficients γ_1 and γ_2 capture the effect of the program for beneficiaries who have been receiving the transfer during one and two years, respectively. The comparison between control and treated units, regardless of whether they are separated or not by the length of the treatment, is

[28]For the districts, which were not included in 2005 ENAHO, district level average was replaced with average calculated at the department level.

subject to the limitations inherent to any non-experimental program evaluation. For the resulting estimates to be unbiased, ignorable treatment assumption, which is not testable, should hold. However, the estimate of the marginal impact of an additional year in the program, given by $\gamma_2 - \gamma_1$ from equation (33) relies on exogenous variation due to the differences in the timing of ENAHO interviews. The ENAHO interviews were spread throughout the year; therefore for any two districts enrolled in Juntos at the same time, the length of participation will be longer for households in the district where the ENAHO interviews took place later in the year. As this variation is exogenous, it allows us to provide unbiased estimate of the marginal impacts across treatment spells.

Limitations and potential biases

Ideally, matching procedures should be based on pretreatment characteristics for both treated and control units. However, data limitations preclude this approach. The ENAHO data contain 1,262 households, which participated in Juntos between 2006 and 2007. The majority of these households were observed only once[29]. Consequently, the estimation can be carried out with a reasonably large sample only if a cross-section of households is used, and the households are matched on contemporaneous covariates. This could possibly lead to a downward bias, as the program may have already affected matching covariates in the treated group. Juntos beneficiaries will be compared not to similar, but to somewhat wealthier matched control households. In this case, the effect of the program would be underestimated and our results are a lower bound of the actual program impacts.

There are a number of reasons to believe that such bias is unlikely. First, poverty score, used as a household level matching covariate, is composed of characteristics, which are unlikely to change in the course of one year due to a budget increase of 30 dollars a month. These characteristics include the type of construction materials or access to electricity or sewage. Second, in order to further reduce the likelihood of bias, district level pre-treatment covariates are used. These data are available from 2005 national census and Juntos administrative data. Intuitively, matching on the pre-treatment community characteristics reduces the bias as it makes it possible to control for the likelihood that the change in the household or individual characteristics will occur after the treatment. Though the changes in household characteristics, such as improvements in dwelling materials, or construction of a sanitary bathroom may be triggered by windfall gain of a cash transfer, they will be much easier to implement in the communities with better infrastructure. For example, getting a bathroom connected to a sewage is more likely to take place in a neighborhood where the sewage system is already in place, and only needs to be extended to the house, that in a neighborhood with no sewage at all. By drawing the matches from the communities with the same average characteristics in pre-treatment years, one can ensure that households with similar chances of experiencing changes in the matching covariates are compared.

Another bias may arise from the fact that districts that were enrolled in Juntos earlier differ in important dimensions from the ones that are still awaiting incorporation in the program. As described in Section 2, the government of Peru originally planned to roll out the program in the decreasing order of indicador sintético, or from poorer districts to less poor. Albeit this order was not followed for seemingly random reasons, such as heavy rains in some districts during program rollout, t-tests of differences in means show that poverty rate, prevalence of malnutrition,

[29]ENAHO contains a small panel component which was discontinued in 2006

and exposure to violence are higher in the treated districts in our sample[30]. Such district level differences are likely to result in the underestimation of the effect of the program: the base level of outcome indicators is likely to be higher, and compliance with conditionality may be easier (for example, due to higher supply of medical services) in the control districts. In order to decrease the likelihood of this bias, we include pre-treatment district average of the outcome variable in the set of the controls.

Finally, eligibility does not perfectly predict beneficiary status. Either due to self-selection, or through the process of community validation, some of the families with the poverty score below eligibility threshold receive the benefits, while some families with the poverty score above the threshold never became beneficiaries. Matching approach does not allow us to control for the unobserved heterogeneity among the respondents which affects the final selection into the group of beneficiaries. In order to address this limitation, we use instrumental variables approach as a roubustness check. By instrumenting the treatment variable - dummy equal to one if a respondent reports receiving Juntos benefits - we can purge the estimates from the effect of unobservables potentially correlated with both outcome and treatment variables.

Robustness check

We use as an instrument the interaction between eligibility status and an indicator for district enrollment in the program at the time of the interview. We can calculate the poverty score on basis of ENAHO data, which contain all its components. Juntos administrative data provide the date of incorporation for each of the participating districts.

Let D_{ij} be equal to 1 if a district j was enrolled in the Juntos program by the time individual i was interviewed, and E_{ij} be equal to 1 if an individual i from district j is eligible for the program according to poverty score. Then we can estimate the impact of Juntos in the following second stage regression:

$$Y_{ij} = \beta_0 + \beta_1 D_{ij} + \beta_2 E_{ij} + \beta_3 \bar{T}_{ij} + \beta_4 \mathbf{X_{ij}} + \beta_5 \mathbf{Z_{ij}} + \varsigma_{ij}, \tag{34}$$

where the remaining notation is the same as in regression (32). The corresponding first stage is:

$$T_{ij} = \delta_0 + \delta_1 D_{ij} + \delta_2 E_{ij} + \delta_3 D_{ij} E_{ij} + \delta_4 \mathbf{X_{ij}} + \delta_5 \mathbf{Z_{ij}} + \nu_{ij}. \tag{35}$$

For a variable to be a valid instrument, it should be strongly correlated with the instrumented regressor, and be only correlated with the outcome of interest through this regressor. The former condition is easily tested in the first stage regression. Table 23 shows that the interaction between eligibility status and district enrollment indicator are a strong predictor of beneficiary status. Though we cannot test whether our instrument satisfies exclusion restriction, it is quite plausible that conditional on the eligibility status and district enrollment status at the time of the interview, the interaction between them will only affect respondents' health, school attendance, consumption and other outcomes of interest through increasing their chances of becoming a Juntos beneficiary.

The use of instrumental variable approach allows us to address an important limitation of matching - the fact that when matching, we are unable to take into account unosberved heterogeneity which determines the discrepancy between being eligible for and actually receiving Juntos

[30]Results available from authors upon request.

benefits. Both approaches - matching and instrumental variables - have limitations; however, comparison of the estimates they yield provides important information on the credibility of the impact evaluation.

Program impacts on beneficiary households

Impacts on consumption and income

On average, the Juntos transfer represents 13 percent of the total monthly household consumption[31]. While this is in the mid-range of transfer size levels with respect to other CCT programs (Figure 7), the analysis suggests that Juntos is having a significant impact on household welfare. Table 22 shows that among Juntos beneficiaries per capita household monetary income increased by 28 percent, while monetary spending increased by 18 percent. The latter was primarily driven by an increase in monetary spending on food - by 34 percent. We do not find significant effects on total income or total consumption - possibly, due to the low size of the transfer and the contributions of other non-monetary sources of income and consumption. Overall, these results show moderate welfare improvements for Juntos beneficiaries.

Impacts on health and nutrition

Changes in the utilization of medical services

As with most CCT programs, Juntos increases the use of medical services. This increase can be due to a number of reasons: the transfer itself, the conditionalities, changes in attitude towards heath and nutrition practices due to the work of promodoras[32]Table 23 shows that for children under 5, the intensity of use of health services increased for all the indicators available from the ENAHO: children from beneficiary households are 37 percentage points more likely to go through health checks, 22 percentage points more likely to get medical attention, if they experience any illness, and 7 percentage points more likely to get vaccinated. These patterns also remain when decomposed by gender and age groups (Tables 24 and 25).

Despite the positive impacts, the overall level of utilization of medical services among Juntos beneficiary children is below the program's goal of universal access. However, the impact of Juntos on the use of these services is similar to (and in some cases higher than) the impacts of other CCT programs. For example, in Nicaragua, Red de Proteccion Social increased the fraction of child health controls by 13 percentage points, PRAF (Honduras) by 20 percentage points and PATH (Jamaica) by 28 percentage points. In Colombia the fraction of children under 2 who received health controls grew by 23 percentage points, and by 33 percentage points for children aged between 2 to 4. Finally, Chile Solidario, Bono de Desarollo Humano in Ecuador or PROGRESA/Opportunidades in Mexico did not affect the rate of health center visits for the corresponding age group (Fiszbein and Schady [2009]). While these results show the range of impacts across these countries, these comparisons should be interpreted with some caution because although the indicators are in principle the same, they may not correspond to the same age groups,

[31] Using the matched control sample

[32] Employees of the Juntos program in charge of explaining goals and procedures of the program to the beneficiaries.

the baseline attendance levels may vary, as well as the recall periods for questions about visits to medical facilities.

Juntos also increased the utilization of health services for beneficiary women of childbearing age: the fraction of women who saught medical attention in case of illness, received vaccinations, used contraceptives and participated in family planning activities went up (Table 26). Despite clear evidence of some positive impacts, there are some indicators where no effect is found: such as doctor-assisted deliveries or receipt of iron supplements. Similarly, the share of women who give birth in medical facilities or attended health campaigns between beneficiaries and control group are not statistically different.

In summary, estimation results show that Juntos increased the use of medical services for both program target groups: children under 5 and women of childbearing age. However, no positive impacts are found on a number of indicators, for example doctor assisted deliveries and receipt of iron supplements. Similarly, increase in indicators such as immunization, are far below universality for both women and children. It is impossible to distinguish whether significant impacts reflect a behavioral change as opposed to mechanical effects driven by program conditionalities. The fact that use of services where no conditionalities exist, for example seeking medical attention in case of illness, increased among beneficiaries is indicative of the former explanation; however it cannot be tested formally.

Changes in nutrition - food consumption

Table 27 shows that per capita monthly spending increased almost in every food category. Participation in Juntos triggered increase in spending on such food categories as breads and cereals, butter and oils, vegetables, fruit, grains, sugar and tubers. Interestingly, consumption of alcoholic beverages is a notable exception. Nonetheless, the program did not affect spending on seafood, meats, milk, cheese and eggs: though the corresponding coefficients have a positive sign, they are not significant. The analysis of impacts of Juntos overtime indicates that increases in spending on some of the more nutritious foods groups become larger over time. For example increases in consumption of vegetables is twice as much for the households that participated in Juntos for longer than one year (Table 39).

Our estimates suggest that Juntos households not only consume more but also consume calories of higher nutritional value (such as vegetables and fruit). This change in diet may take place due to a number of reasons. Firstly, changes in nutritional preferences could be attributed to the work of promotoras, or participation in health campaigns, which are part of the program benefits. It is plausible that the information about the value of a more balanced diet, beneficial effects of proteins and vegetables, was instrumental in the change in consumption patterns. Alternatively, families may be switching to more nutritious foods simply due to the transfer and income increase, which allows substituting for more expensive products. Finally, qualitative work suggests that many beneficiaries believed buying food was one of the conditions of the program (Jones et al. [2006], Huber et al. [2009]). Unfortunately with the existing data one cannot distinguish between these alternatives.

Changes in final outcomes in health and nutrition

We find that participation in Juntos triggered changes in the beneficiaries' use of services and diets. Nonetheless, the ultimate goal of CCT programs is to induce behavioral changes that can serve as inputs for improvements in final outcome indicators in health and nutrition. Unfortunately the data that would allow one to trace the impact of Juntos on beneficiaries' final outcome indicators of health and nutrition are scarce. For 2006 and 2007, the ENAHO contains a question on self-reported health. Using these data, we find that Juntos children less than five were less likely to experience illnesses in the month prior to the survey by six percentage points; however, there are no changes in self-reported health of women of reproductive age (Table 28).

In addition, during the last trimester of 2007 a new anthropometric module was introduced to the ENAHO survey, which includes data on weight, height and hemoglobin (which can be used to calculate indicators of malnutrition). Information on z-scores is available for children under 5, while data on hemoglobin were collected for children under 2 and breast-feeding women. We don't find any impact in any of these indicators (Table 29). These results may be driven by the shortcomings of the data – the sample size is small as the anthropometrics module was administered only during the last trimester of 2007 ENAHO. This also restricts further decompositions by gender and age. In summary, consistent with other CCT programs, Juntos shows a number of positive impacts on health inputs: an increase in service utilization and improvement in diets. There is also some evidence of improvement in the health of children but for the most part, Juntos does not seem to affect final outcome indicators of health and nutrition. However, we need to keep in mind that these results may be driven by small sample sizes.

Juntos impacts in education

Table 30 shows that participation in Juntos increased registration, but not attendance rate among beneficiaries. Disaggregation of these results by primary school age reveals a number of interesting trends. First, the positive impact of Juntos on schooling is driven by impacts at transition points. For example, the effect on school registration is concentrated among younger children, especially 7 year olds (Table 31). In addition, school attendance among 7 year olds is also significantly higher for Juntos households (Table 32).

Taken together, these results suggest that the observed Juntos impacts are concentrated at transition points – entry in primary school and transition from primary to secondary. This result is consistent with the findings from impact evaluations of CCT programs in other countries: the impacts of CCT programs in contexts with high initial enrollment and attendance rates tend to be more focused on such transition points. In this sense, the Juntos effects compare favorably with the impacts of CCT programs in other Latin American countries with similar context. For example, enrollment increased by 3.3 percentage points in the case of PRAF in Honduras (for children aged 6 to 13, from a baseline enrollment of 66 percent), 7.5 percentage points for Chile Solidário (for children aged 6 to 15, from a baseline enrollment of 61 percent), and by 12.8 percentage points for the Red de Proteccion Social in Nicaragua (for children aged 7 to 13, from a baseline enrollment of 72 percent (Fiszbein and Schady [2009]).

In addition to schooling outcomes, participation in Juntos also induces beneficiary households to spend more on educational supplies. Specifically, among households with at least one child aged between 6 and 14, an increase of approximately 30 soles a year in spending on uniforms can be attributed to participation in the program (Table 33). This increase constitutes 70 percent of the annual spending in the control group. Juntos does not seem to affect spending on other types of supplies, such as books. However, the fact that the average spending on these items is generally very low – on average less than 1 sol a year per household in the control group for books and transportation – may account for the lack of impacts.

In summary, with respect to educational outcomes, our analysis suggests that Juntos has had limited impacts on school registration and attendance. Still, these impacts are consistent with international experience: as the baseline enrollment is high (75 percent for the control group), and the transfer constitutes a moderate 15 percent of the average household monthly consumption, the transfer is more crucial at points where the opportunity costs are more binding, namely entering and finishing primary school. Unfortunately, data limitations make it impossible to estimate the impact of the program on learning.

Unintended impacts

Despite abundant evidence of the positive impacts of CCTs, policy makers are usually preoccupied with unintended and undesirable changes in behavior that CCT programs may trigger. Such undesirable changes include the use of transfers in ways that may be inconsistent with the goals of the program – for example, spending it on alcohol and tabacco. Similarly, fertility rates may increase if beneficiary families believe that this will provide them with additional transfers. Available data allow us to explore whether these scenarios occur in case of Juntos.

We do not find any unintended, undesirable impacts of the program on beneficiaries' behavior. While there are large program impacts on various food consumption categories, we find an over 50 percent reduction in consumption of alcohol (Table 27). Although the ENAHO survey does not provide information to directly test for "intentional pregnancy", there is a question where female respondents are asked to provide information about births they gave during the last three years. Using this variable, we find no significant differences in the birth rates of beneficiaries and non-beneficiaries (Table 34) – Juntos does not have an effect on fertility over the last three years. These result are also consistent with the findings of the qualitative study carried out by UNICEF (Jones et al., [2006]).

Heterogeneity in impacts depending on treatment intensity

Comparison of marginal impacts across different treated groups based on length of time in the program is likely to provide an unbiased estimate of differential impact of an additional year in the program. Specifically, as the ENAHO interviews were spread throughout the year, for any two districts enrolled in Juntos at the same time, the length of participation will be longer for households in the district where the ENAHO interviews took place later in the year. The variation in the timing of ENAHO interviews is exogenous to the program placement; consequently, we can cleanly identify the marginal impact of additional year in Juntos. We have already discussed the marginal impact of an additional year in the program on some outcomes of interest. This section presents the results for the remaining indicators.

Table 35 shows that receiving Juntos benefits during 13 months or longer increases per capita monthly monetary spending by 19 soles, monetary spending on food by 8 soles, and monetary income by 17 soles, compared to the families who have participated in the program during one year or less.

Table 36 shows that the fraction of children under 5 who seek medical attention and attend health checks increases by 5 and 7 percentage points respectively among children who participated in the program for 13 months or longer, compared to the children who have been receiving benefits during one year or less. However, F-test confirms that the differences are significant only in the former case. Similarly, an additional year in Juntos increases the fraction of women who seek medicatl attention by 7 percentage points and the fraction of women who give birth with medical assistance by 18 percentage points (Table 37).

The effect of the program on the vaccination rate among children who have been receiving benefits for longer than one year may seem counterintuitive - the coefficient of interest is negative and significant. The negative effect may be driven by the phrasing of the question about vaccinations in ENAHO and the specifics of Juntos requirements regarding vaccinations. ENAHO questions only capture information about vaccinations received during the last three months. Juntos requires that beneficiary children receive 11 vaccinations during the first 24 months of their lives. If joining the program induces beneficiaries to immediately comply with the vaccination requirements in their entirety, mechanically one year after joining there will be no vaccinations left to receive.

Table 38 shows that food consumption significantly increases overtime in a number of categories: milk, cheese and eggs (by 0.78 soles), butter and oil (by 0.21 soles), vegetables (by 0.45 soles), tubers (by 0.37 soles), sugar (by 0.33 soles). These results suggest that Juntos beneficiaries are improving their diets over time. Program impacts in self reported health increase in magnitude overtime for both children, and women of childbearing age (Table 39). Decompositions in time for other health outcomes - z-scores for children and hemoglobin - do not yield any results (Table 40).

The magnitudes of Juntos impacts on both registration and attendance is higher among Juntos children that have been in the program longer (Table 41).

Instrumental variables estimation

Instrumental variables regressions yield similar results to the regressions on the matched sample. As in the regressions on the matched sample, we find that Juntos triggers increase in monetary spending, monetary spending on food, and monetary income. However, the estimates are higher, and we also find impacts on monetary spending on non-food and food consumption (Table 43).

In the "utilization of medical services"category we find impacts on all the outcomes available in ENAHO for children under 5 with both methods: instrumental variables estimation confirms that the fraction of children who seek medical attention, receive vaccinations and health checks increased due to Juntos (Table 44). For women, we find significant impacts on the likelihood to get vaccinated and to participate in family planning activities (Table 45). The impact on the likelihood to seek medical attention and to receive contraceptives is positive, but unlike in the regressions on the matched sample, not significant.

Table 46 shows that as matching, instrumental variables regressions yield positive and significant estimates of the effect of Juntos on spending on various food categories. The overlap of coefficients which are positivie and significant in both regressions is not complete; however, spending on breads and cereals, vegetables and grains significantly increases according to both estimation methods. Unfortunately, in the instrumental variables regressions the sign on the effect of Juntos on spending on alcohol is reversed - purchases of alcohol increase, but by low amount: 0.28 soles per month.

As in the regressions on the matched sample, we find that children from beneficiary families are less likely to report having been sick during the last three weeks prior to the interview (Table 47). The magnitude of the estimate is higher (0.22 as opposed to 0.6); and unlike with matching, we also find impact on weight for age for children under 5 (Table 48). We may be able to precisely estimate this parameter due to larger sample compared to the sample used in matching. Other anthropometric measures - height for age and hemoglobin - are not significant according to both methods.

Instrumental variables estimation does not confirm the positive impact of Juntos on registration at school (Table 49); however, as in the regressions on the matched sample, we find that spending on tuition increased (Table 50). We do not find any evidence of an increase in fertility (Table 51).

Conclusion

This study presents the first quantitative impact evaluation of the CCT program Juntos in Peru. Our findings suggest that Juntos is having an impact on a number of key welfare indicators among program beneficiaries. Specifically, Juntos moderately increases monetary spending and consumption. In addition, and similar to evidence from other countries, the program increases the utilization of health services for both target groups: children under 5 and women of reproductive age, and improves nutritional intake of participating households. We find that as other CCT programs in the countries where primary school attendance is high, Juntos increases school registration and attendance at transition points, ensuring that children enter and finish primary school. Finally, there is no evidence of such unintended effect as increases in fertility rates. All these results, except for the impacts in education, are robust to using two estimation techniques - regression on the matched sample and instrumental variables regression.

Despite these positive effects, there is little evidence that the program had impact on final health outcomes, such as z-scores and hemoglobin. We only find significant effect on the weight for age among children under 5 using instrumental variables approach: this finding is not robust to using matching. Such lack of impacts on final outcome indicators is consistent with the international experience, which suggests that for these types of impacts, CCT schemes need to be complemented by adequate supply of health services (in both quantity and quality) as well as interventions that can better promote health and education practices. In this sense, the potential of Juntos to improve on these areas remains untapped.

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Table 1: Program conditionalities

For children under 5 years:	Attend regular health and nutrition controls (for periodic monitoring of height and weight, complete series of vaccinations, iron and Vitamin A supplements and anti-parasite checks)
For children 6-14 years with primary school incomplete:	School attendance at least 85% of the school year
For pregnant and breast-feeding mothers:	Attend prenatal and post-natal checks (tetanus vaccination, folic acid and iron supplements and anti-parasite checks)

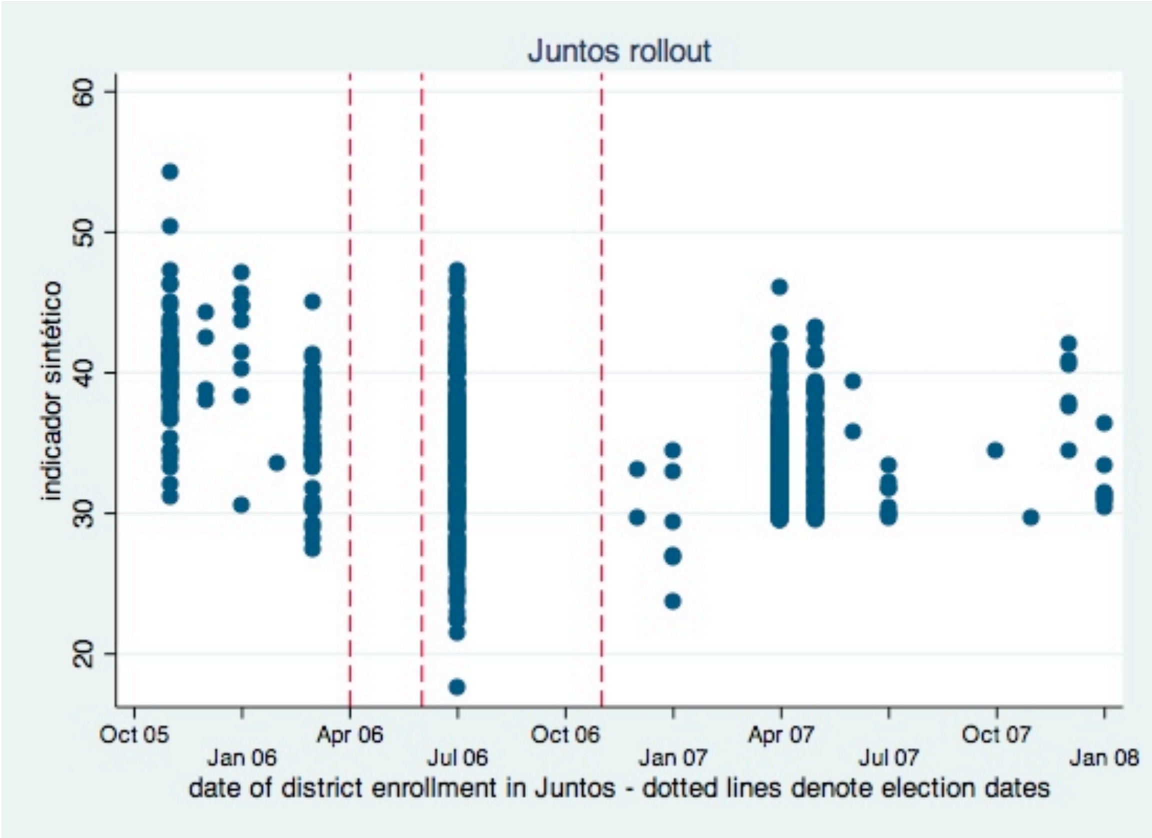

Figure 1: Juntos rollout

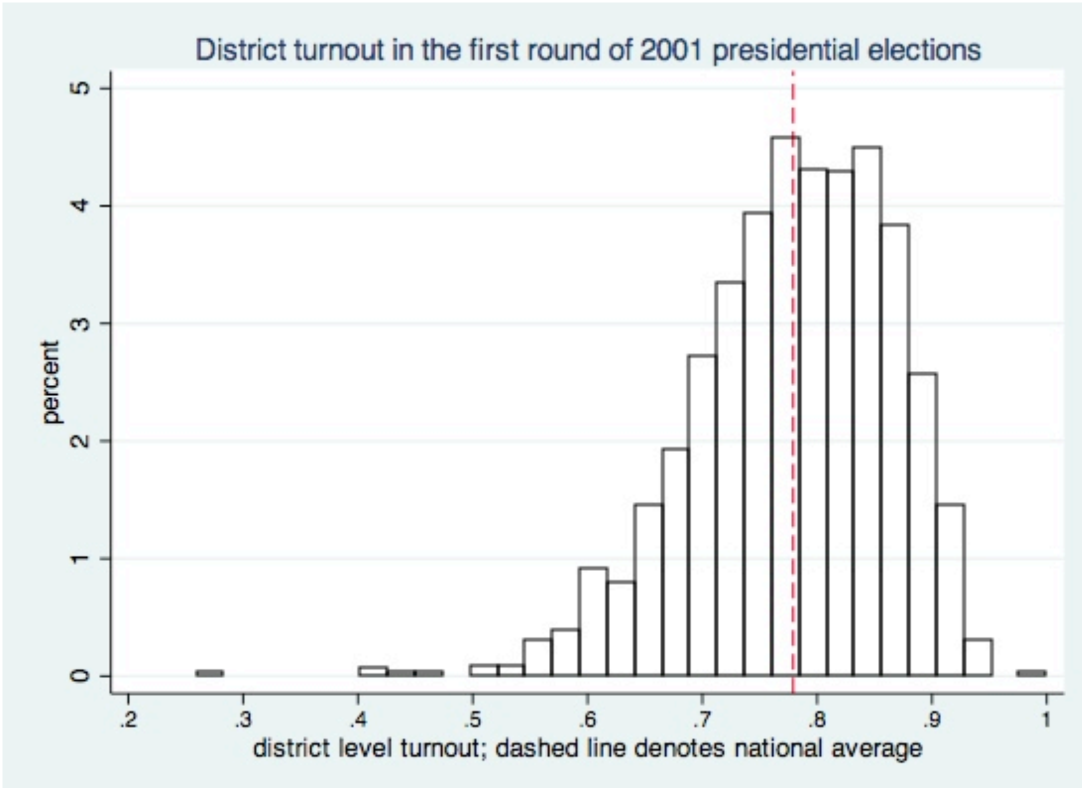

Figure 2: district turnout distribution, 2001 presidential elections, first round

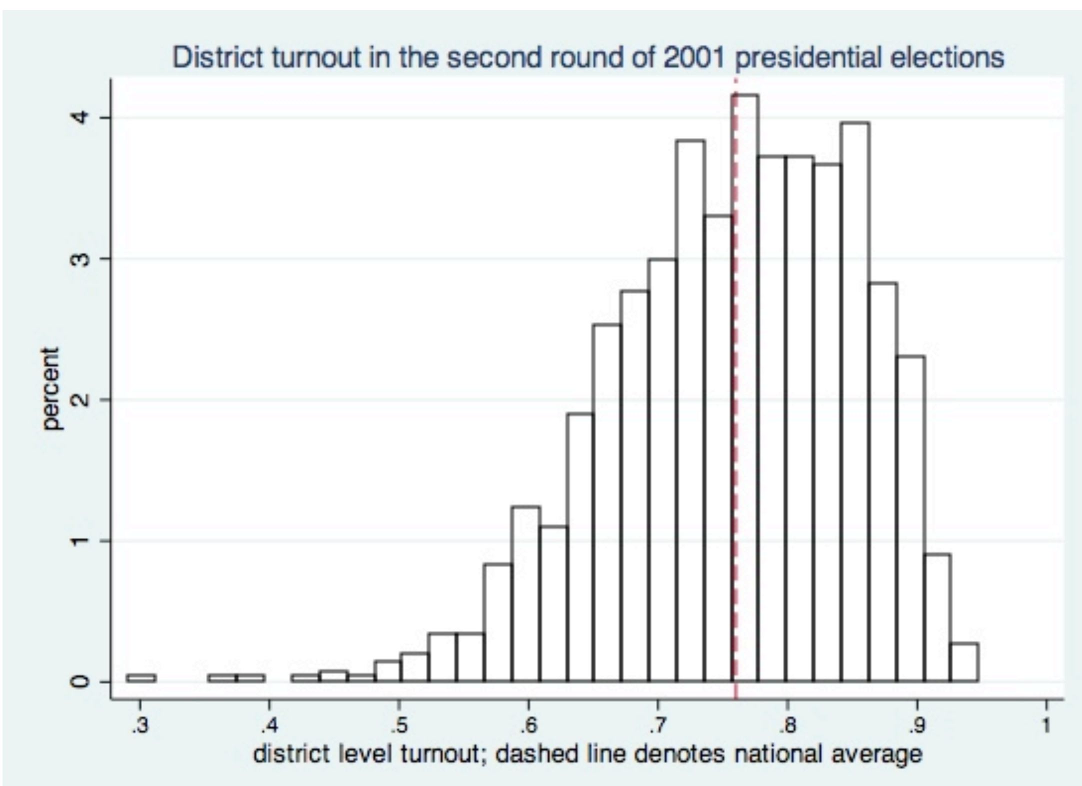

Figure3: district turnout distribution, 2001 presidential elections, second round

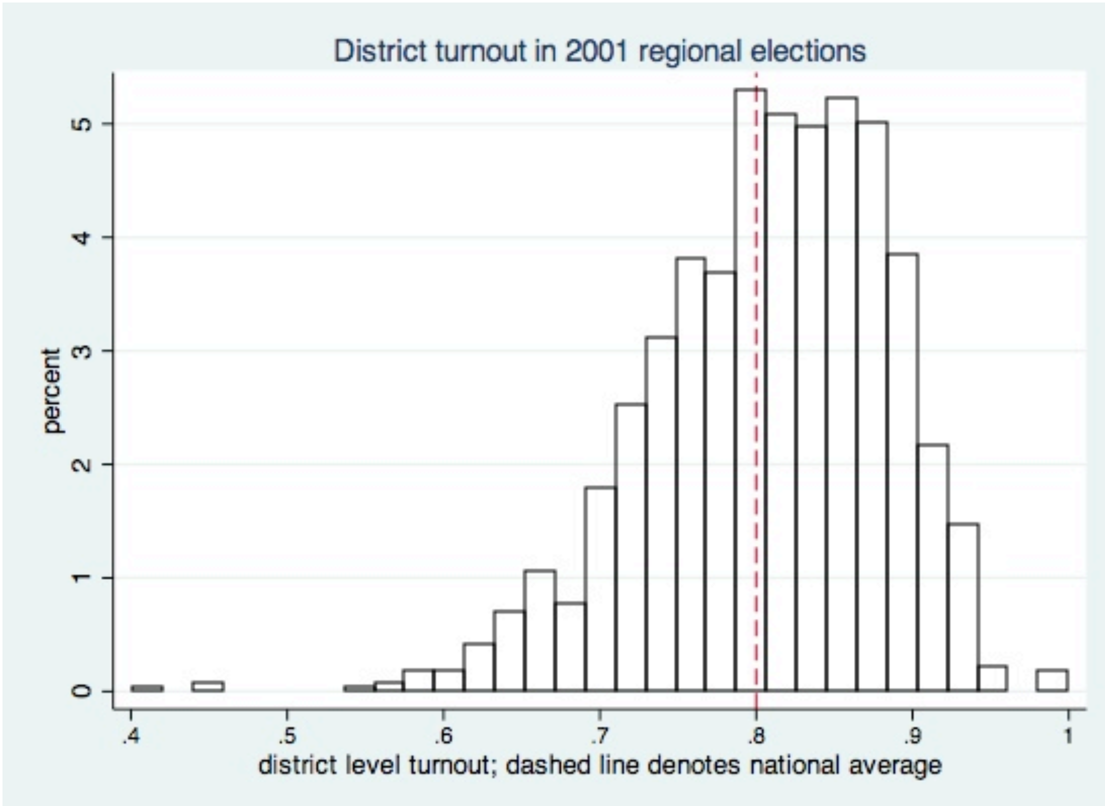

Figure 4: district turnout distribution, 2001 regional elections

Table 2: do outcome trends explain the rollout?

	Dummy, equal to 1 if a district was enrolled in Juntos prior to 2006 elections	Number of months the district was receiving Juntos by the date of 2006 elections
Change in turnout between 2001 and 2000 elections	0.0284	0.1517
	(0.0813)	(0.2899)
municipal revenue	0.0000	0.0000
	(0.0000)	(0.0000)
transfers to the municipality	-0.0000*	-0.0000*
	(0.0000)	(0.0000)
municipal spending	-0.0000	-0.0000
	(0.0000)	(0.0000)
number of beneficiaries of Vaso de Leche	-0.0000	-0.0000
	(0.0000)	(0.0000)
number of beneficiaries of Club de Madres	-0.0000*	-0.0000**
	(0.0000)	(0.0000)
number of beneficiaries Comedor Popular	0.0000	0.0000
	(0.0000)	(0.0000)
public clinics and hospitals	0.0046	0.0075
	(0.0045)	(0.0076)
private clinics and hospitals	0.0002	-0.0002
	(0.0005)	(0.0007)
public health centers	0.0063	0.0053
	(0.0040)	(0.0043)
private health centers	0.0002	0.0001
	(0.0002)	(0.0003)
private pharmacies	-0.0002	-0.0001
	(0.0001)	(0.0002)
number of SME registered	-0.0000	-0.0000
	(0.0000)	(0.0000)
number of licenses for professional activities	0.0000	0.0000
	(0.0000)	(0.0000)
constant	0.0191***	0.0502***
	(0.0045)	(0.0150)
Number of observations	985	985
R-squared	0.023	0.002

note: *** p<0.01, ** p<0.05, * p<0.1

Robust standard errors clustered at district level

Table3: Impact of Juntos on turnout, district level panel regressions

Presidential elections					
	(1)	(2)	(3)	(4)	(5)
Number of months a district was in Juntos by the election date	0.0115***	0.0114***	0.0113***	0.0112***	0.0109***
	(0.0020)	(0.0020)	(0.0020)	(0.0020)	(0.0020)
municipal revenue		0.0000*	0.0000*	0.0000*	0.0000*
		(0.0000)	(0.0000)	(0.0000)	(0.0000)
transfers to the municipality		0.0000*	0.0000***	0.0000***	0.0000***
		(0.0000)	(0.0000)	(0.0000)	(0.0000)
municipal spending		-0.0000*	-0.0000***	-0.0000***	-0.0000***
		(0.0000)	(0.0000)	(0.0000)	(0.0000)
number of beneficiaries of Vaso de Leche			-0.0001	-0.0001	-0.0001
			(0.0001)	(0.0001)	(0.0001)
number of beneficiaries of Club de Madres			0.0000**	0.0000	0.0000**
			(0.0000)	(0.0000)	(0.0000)
number of beneficiaries Comedor Popular			-0.0007**	-0.0007**	-0.0008**
			(0.0003)	(0.0003)	(0.0003)
public clinics and hospitals				-0.8524	-0.7802
				(1.0304)	(1.0308)
private clinics and hospitals				0.5619	0.4891
				(0.7127)	(0.7222)
public health centers				-0.0051	-0.0028
				(0.0241)	(0.0240)
private health centers				-0.1629***	-0.0637
				(0.0505)	(0.0565)
public pharmacies				0.0081	0.0073
				(0.0307)	(0.0305)
private pharmacies				-0.0095	-0.0112
				(0.0380)	(0.0322)
number of SME registered					0.0000***
					(0.0000)
number of licenses for professional activities					0.0008
					(0.0009)
population density					-0.0002
					(0.0003)
Number of observations	2,647	2,647	2,647	2,647	2,647
R-squared	0.719	0.724	0.727	0.729	0.735

Regional elections					
	(1)	(2)	(3)	(4)	(5)
Number of months a district was in Juntos by the election date	0.0063***	0.0062***	0.0062***	0.0062***	0.0060***
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0007)

	(1)	(2)	(3)	(4)	(5)
municipal revenue		0.0000*	0.0000*	0.0000*	0.0000*
		(0.0000)	(0.0000)	(0.0000)	(0.0000)
transfers to the municipality		0.0000***	0.0000***	0.0000***	0.0000***
		(0.0000)	(0.0000)	(0.0000)	(0.0000)
municipal spending		-0.0000*	-0.0000**	-0.0000**	-0.0000**
		(0.0000)	(0.0000)	(0.0000)	(0.0000)
number of beneficiaries of Vaso de Leche			-0.0000	-0.0000	-0.0000
			(0.0001)	(0.0001)	(0.0001)
number of beneficiaries of Club de Madres			0.0000***	0.0000**	0.0000***
			(0.0000)	(0.0000)	(0.0000)
number of beneficiaries Comedor Popular			-0.0003	-0.0003	-0.0004
			(0.0003)	(0.0003)	(0.0003)
public clinics and hospitals				-1.2177	-1.1694
				(0.9564)	(0.9474)
private clinics and hospitals				0.5530	0.4478
				(0.6950)	(0.6911)
public health centers				0.0021	0.0032
				(0.0238)	(0.0238)
private health centers				-0.1179***	-0.0436
				(0.0453)	(0.0489)
public pharmacies				-0.0043	-0.0054
				(0.0178)	(0.0176)
private pharmacies				-0.0309	-0.0338
				(0.0356)	(0.0312)
number of SME registered					0.0000***
					(0.0000)
number of licenses for professional activities					0.0015*
					(0.0008)
population density					-0.0005*
					(0.0003)
Number of observations	2,647	2,647	2,647	2,647	2,647
R-squared	0.565	0.570	0.571	0.574	0.579

all regressions include district and year fixed effects

note: *** p<0.01, ** p<0.05, * p<0.1

Table 4: Impact of Juntos on turnout, district level panel regressions

Presidential elections				
	Dummy equal to 1 if a district was in Juntos before elections		Percent of beneficiaries among eligible by election date	
	(1)	(2)	(3)	(4)
Treatment indicator	0.0596*** (0.0088)	0.0564*** (0.0088)	0.1073*** (0.0152)	0.1019*** (0.0151)
municipal revenue		0.0000* (0.0000)		0.0000* (0.0000)
transfers to the municipality		0.0000*** (0.0000)		0.0000*** (0.0000)
municipal spending		-0.0000*** (0.0000)		- 0.0000*** (0.0000)
number of beneficiaries of Vaso de Leche		-0.0001 (0.0001)		-0.0001 (0.0001)
number of beneficiaries of Club de Madres		0.0000** (0.0000)		0.0000** (0.0000)
number of beneficiaries Comedor Popular		-0.0008** (0.0003)		-0.0008** (0.0003)
public clinics and hospitals		-0.7937 (1.0276)		-0.7923 (1.0277)
private clinics and hospitals		0.4920 (0.7203)		0.4919 (0.7201)
public health centers		-0.0042 (0.0236)		-0.0038 (0.0236)
private health centers		-0.0631 (0.0559)		-0.0625 (0.0558)
public pharmacies		0.0079 (0.0305)		0.0072 (0.0305)
private pharmacies		-0.0093 (0.0318)		-0.0095 (0.0318)
number of SME registered		0.0000*** (0.0000)		0.0000*** (0.0000)
number of licenses for professional activities		0.0008 (0.0009)		0.0008 (0.0009)
population density		-0.0002 (0.0003)		-0.0002 (0.0003)
Number of observations	2,647	2,647	2,647	2,647
R-squared	0.722	0.737	0.723	0.738

Regional elections				
	Dummy equal to 1 if a district was in Juntos		Percent of beneficiaries among eligible by	

	before elections		election date	
	(1)	(2)	(3)	(4)
Number of months a district was in Juntos by the election date	0.0464***	0.0442***	0.0917***	0.0877***
	(0.0052)	(0.0052)	(0.0093)	(0.0094)
municipal revenue		0.0000*		0.0000*
		(0.0000)		(0.0000)
transfers to the municipality		0.0000***		0.0000***
		(0.0000)		(0.0000)
municipal spending		-0.0000**		-0.0000**
		(0.0000)		(0.0000)
number of beneficiaries of Vaso de Leche		-0.0000		-0.0000
		(0.0001)		(0.0001)
number of beneficiaries of Club de Madres		0.0000***		0.0000***
		(0.0000)		(0.0000)
number of beneficiaries Comedor Popular		-0.0004		-0.0004
		(0.0003)		(0.0004)
public clinics and hospitals		-1.1718		-1.1950
		(0.9418)		(0.9477)
private clinics and hospitals		0.4650		0.4537
		(0.7001)		(0.6861)
public health centers		0.0014		0.0050
		(0.0247)		(0.0251)
private health centers		-0.0472		-0.0438
		(0.0485)		(0.0482)
public pharmacies		-0.0040		-0.0051
		(0.0173)		(0.0173)
private pharmacies		-0.0307		-0.0333
		(0.0309)		(0.0310)
number of SME registered		0.0000***		0.0000***
		(0.0000)		(0.0000)
number of licenses for professional activities		0.0014*		0.0013
		(0.0008)		(0.0008)
population density		-0.0005*		-0.0005*
		(0.0003)		(0.0003)
Number of observations	2,647	2,647	2,647	2,647
R-squared	0.565	0.579	0.569	0.583

all regressions include district and year fixed effects

note: *** p<0.01, ** p<0.05, * p<0.1

Table 5: bias in reporting voting in ENAHO survey

Source	National turnout	
	2001	2006
ONPE, actual turnout	0.78	0.88
ENAHO estimate	0.87	0.84
standard error	0.0031	0.0028

Table 6: differences in reporting bias between treatment and control districts

Variables	OLS coefficient
The district was incorporated in Juntos before 2006 elections	-0.00139
	(0.03500)
Constant	0.11801***
	(0.00742)
Number of observations	609

province fixed effects included

note: *** p<0.01, ** p<0.05, * p<0.1

Table 7: Difference-in-difference estimation of the impact of Juntos on voting

	(1)	(2)	(3)	(4)	(5)
the district was incorporated in Juntos before 2006 elections (i)	-0.07***	-0.07**	-0.08***	-0.08***	-0.08***
	(0.00)	(0.03)	(0.02)	(0.02)	(0.02)
dummy for 2007 observations (ii)	-0.04***	-0.08***	-0.04*	-0.04**	-0.04**
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
interaction between early rollout indicator (i) and time indicator (ii)		0.06**	0.07***	0.07***	0.07***
		(0.03)	(0.03)	(0.02)	(0.02)
proxy means index			0.23***	0.26***	0.26***
			(0.05)	(0.05)	(0.05)
proxy means index squared			-0.38***	-0.38***	-0.39***
			(0.04)	(0.04)	(0.04)
Dwelling has electricity			-0.01	-0.01	-0.01
			(0.01)	(0.01)	(0.01)
Dwelling has access to hygienic restroom			-0.00	-0.00	-0.00
			(0.01)	(0.01)	(0.01)
Walls made of solid materials			0.01	0.01	0.01*
			(0.01)	(0.01)	(0.01)
Dwelling materials classification: type 1			-0.00	-0.01	-0.01
			(0.01)	(0.01)	(0.01)
Dwelling materials classification: type 2			0.00	0.00	0.00
			(0.01)	(0.01)	(0.01)
Dwelling materials classification: type 3			-0.01*	-0.01	-0.01
			(0.01)	(0.01)	(0.01)
Dwelling has access to three basic services			0.01	0.01**	0.01**
			(0.00)	(0.00)	(0.00)
Household uses industrial fuel			-0.01**	-0.01	-0.01
			(0.01)	(0.01)	(0.01)
Household does not have basic equipment			-0.02***	-0.02***	-0.02***
			(0.00)	(0.00)	(0.00)
age in years				-0.00	0.00
				(0.00)	(0.00)
gender				-0.01**	-0.01**
				(0.00)	(0.00)
years of education				0.01***	0.02***
				(0.00)	(0.00)
relationship to the head of the household				-0.03***	-0.03***
				(0.00)	(0.00)

employment status	0.05***	0.05***
	(0.01)	(0.01)
number of hours worked during the week	0.00***	0.00***
	(0.00)	(0.00)
dummy equal to 1 if works in agriculture	0.02	0.02
	(0.02)	(0.02)
salaried agricultural worker	-0.04*	-0.04*
	(0.02)	(0.03)
salaried non-agricultural worker	-0.01***	-0.01***
	(0.00)	(0.00)
non-salaried agricultural worker	0.00	0.00
	(0.03)	(0.03)
district level monetary poverty in 2005		-0.00*
		(0.00)
severity of poverty in 2005		0.00
		(0.00)
child malnutrition in 2005		-0.00
		(0.00)
not satisfied basic needs index in 2005		0.00
		(0.00)
percent of centros poblados affected by violence		-0.00
		(0.00)
percent of household heads who speak Spanish		-0.08***
		(0.02)
municipal revenue		-0.00***
		(0.00)
transfers to the municipality		-0.00*
		(0.00)
municipal spending		0.00
		(0.00)
number of beneficiaries of Vaso de Leche		-0.00***
		(0.00)
number of beneficiaries of Club de Madres		-0.00
		(0.00)
number of beneficiaries Comedor Popular		0.00*
		(0.00)
public clinics and hospitals		0.00
		(0.00)
private clinics and hospitals		-0.00

					(0.00)
public health centers					-0.00*
					(0.00)
private health centers					-0.00
					(0.00)
number of SME registered					0.00
					(0.00)
number of licenses for professional activities					-0.00
					(0.00)
Number of observations	36,193	36,193	36,193	36,193	36,193

note: *** p<0.01, ** p<0.05, * p<0.1

Dependent variable is a dummy equal to 1 if an individual reported voting in the last presidential elections, and 0 otherwise

Robust standard errors clustered at district level

Table 8: Difference-in-difference estimation of the impact of Juntos on voting - regressions with district fixed effects

	(5)
dummy for 2007 observations (ii)	-0.02***
	(0.01)
interaction between early rollout indicator (i) and time indicator (ii)	0.05*
	(0.03)
Number of observations	36,193

note: *** p<0.01, ** p<0.05, * p<0.1

Dependent variable is a dummy equal to 1 if an individual reported voting in the last presidential elections, and 0 otherwise

District fixed effects included, robust standard errors clustered at district level

Controls include: proxy means index, its square, indicators for access to electricity, hygienic restroom, indicators for various types of materials used in the house construction, indicator for access to three basic services, use of industrial fuel, availability of basic equipment, age, gender, years of education, relationship to household head, employment status, number of hours worked during the week; indicators for working in agriculture and receiving salary, municipal revenue and spending, transfers to the municipality, number of beneficiaries of three major social programs: Vaso de Leche, Club de Madres and Comedor Popular, numbers of public and private clinics, hospitals and health centers, number of SME registered and number of licenses for professional activities.

Table 9: Do impacts vary depending on the fraction of beneficiaries? District level panel regressions

	presidential elections	regional elections
Early enrollment dummy	0.0568***	0.0442***
	(0.0087)	(0.0051)
fraction of beneficiaries among all eligible to vote	0.1577	0.1671**
	(0.1115)	(0.0685)
Number of observations	2,647	2,647
R-squared	0.738	0.583

note: *** p<0.01, ** p<0.05, * p<0.1

District and year fixed effects included. Robust standard errors clustered at district level in the parenthesis. Control variables include:municipal revenue, transfers to the municipality, municipal spending, number of beneficiaries of Vaso de Leche, Club de Madres and Comedor Popular, number of private and public hospitals, health centers and pharmacies, number of SME registered, number of licenses for professional activities, population density

Table 10: heterogeneity in impacts depending on eligibility status. Individual level regressions

	No controls	Controls
the district was incorporated in Juntos before 2006 elections (i)	-0.07**	-0.09***
	(0.03)	(0.02)
dummy for 2007 observations (ii)	-0.12***	-0.06**
	(0.03)	(0.03)
eligibility status (iii)	-0.02	0.09***
	(0.01)	(0.01)
interaction between early rollout indicator (i) and time indicator (ii)	0.05	0.06
	(0.04)	(0.04)
interaction between time indicator (ii) and eligibility indicator (iii)	0.08*	0.03
	(0.04)	(0.04)
interaction between early rollout indicator (i) and eligibility indicator (iii)	-0.00	0.03***
	(0.01)	(0.01)
interaction between (i), (ii) and (iii)	0.03	0.02
	(0.05)	(0.05)
Number of observations	36,193	36,193

note: * p<0.01, ** p<0.05, *** p<0.1

Province and year fixed effects included. Robust standard errors clustered at district level in the parenthesis.

Controls include: proxy means index, its square, indicators for access to electricity, hygienic restroom, indicators for various types of materials used in the house construction, indicator for access to three basic services, use of industrial fuel, availability of basic equipment, age, gender, years of education, relationship to household head, employment status, number of hours worked during the week; indicators for working in agriculture and receiving salary, municipal revenue and spending, transfers to the municipality, number of beneficiaries of three major social programs: Vaso de Leche, Club de Madres and Comedor Popular, numbers of public and private clinics, hospitals and health centers, number of SME registered and number of licenses for professional activities.

Table 11: Juntos impacts on the opinions about democracy and politics

Dependent variable	Regressions without fraction of beneficiaries	Regressions with fraction of beneficiaries		Number of observations
	Dummy, equal to 1 if a district was receiving Juntos that year	Dummy, equal to 1 if a district was receiving Juntos that year	Percent of beneficiaries - average percent of beneficiaries	
	(1)	(2)	(3)	
Believe that democracy is sufficiently or very important	0.05** (0.02)	0.05** (0.02)	-0.08 (0.18)	3,098
Believe that democracy functions well in Peru	0.02 (0.02)	0.02 (0.02)	-0.01 (0.21)	3,105
Believe that fair elections are respected	0.05* (0.03)	0.05* (0.03)	0.07 (0.23)	3,093
Are interested in politics	-0.03 (0.02)	-0.03 (0.02)	-0.23 (0.21)	2,900
Believe that politicians are concerned about the people	-0.00 (0.01)	-0.00 (0.01)	0.08 (0.08)	3,104
Believe that community well-being improved over the last year	-0.01 (0.01)	-0.01 (0.01)	0.10 (0.09)	3,112
Believe that household well-being improved over the last year	-0.01 (0.01)	-0.01 (0.01)	0.16* (0.09)	3,112
Household well-being improved due to governmental transfers	0.27*** (0.04)	0.25*** (0.04)	1.59*** (0.39)	1,801
Believe that public administration functions well	0.08** (0.04)	0.09** (0.05)	0.24 (0.57)	1,766
Believe that democracy improved over the last year	0.01 (0.03)	0.02 (0.04)	0.16 (0.51)	1,759

note: *** p<0.01, ** p<0.05, * p<0.1
Robust standard errors clustered at district level

Table 12: heterogeneity in the impact of Juntos on voting by gender

	No controls	Controls
dummy for 2007 and 2008 observations (i)	-0.07**	-0.08*
	(0.03)	(0.02)
the district was incorporated in Juntos before 2006 elections (ii)	-0.05***	-0.02
	(0.03)	(0.02)
female	-0.00	0.01**
	(0.01)	(0.01)
interaction between early rollout indicator (ii) and time indicator (i)	0.05	0.07**
	(0.03)	(0.03)
interaction between female and (i)	-0.05***	-0.05
	(0.03)	(0.03)
interaction between female and (ii)	-0.01	0.00
	(0.01)	(0.01)
interaction between early rollout indicator (ii) and time indicator (i) and female	0.02	0.01
	(0.04)	(0.04)
Number of observations	36,193	36,193

note: * p<0.01, ** p<0.05, *** p<0.1

Province and year fixed effects included. Robust standard errors clustered at district level in the parenthesis.

Controls include: proxy means index, its square, indicators for access to electricity, hygienic restroom, indicators for various types of materials used in the house construction, indicator for access to three basic services, use of industrial fuel, availability of basic equipment, age, gender, years of education, relationship to household head, employment status, number of hours worked during the week; indicators for working in agriculture and receiving salary, municipal revenue and spending, transfers to the municipality, number of beneficiaries of three major social programs: Vaso de Leche, Club de Madres and Comedor Popular, numbers of public and private clinics, hospitals and health centers, number of SME registered and number of licenses for professional activities.

Table 13: percent of divorced and separated women

	Formally divorced		Separated	
	Peru	Juntos districts	Peru	Juntos districts
2006	0.3	0.1	10	5
2007	0.4	0.2	10	7
2008	0.5	0.1	10	6

due to the data limitations, this calculation excludes women in their second marriage

Table 14: Average levels of violence in Juntos and non-Juntos districts

Percent of respondents who have ever experienced	Mean	Standard deviation
ever experienced physical violence	0.38	0.48
ever experienced sexual violence	0.10	0.30
ever experienced emotional violence	0.31	0.46
physical violence in the last 12 months	0.14	0.34
sexual violence in the last 12 months	0.04	0.21
emotional violence in the last 12 months	0.15	0.36
Number of observations:	3,238	

The table reports coefficients from regression of violence indicators in 2005 on a constant and a dummy equal to 1 if a district is one of 637 Juntos districts. The sample is limited to CRECER districts.

*** p<0.01, ** p<0.05, * p<0.1

Table 15: Summary statistics

Variable	mean	standard deviation	number of observations
proxy means score	0.61	0.23	5162
proxy means score squared	0.42	0.25	5162
age	33.14	8.49	5178
primary school completed	0.32	0.47	5177
secondary school completed	0.13	0.33	5177
has health insurance	0.29	0.45	4392
illiterate	0.25	0.43	5162
living together (not married)	0.50	0.50	5178
number of children	2.32	1.42	5029
dummy, = 1 if father used to beat her mother	0.48	0.50	4775
household has access to electricity	0.42	0.49	5178
household has access to hygienic restroom	0.09	0.28	5178
household has access to piped water	0.48	0.50	5177
number of asserts from Juntos score list that the household does not have	4.70	0.95	5178
household owns land usable for agriculture	0.77	0.42	4390
type of materials 1	0.38	0.48	5178
type of materials 2	0.32	0.47	5178
type of materials 3	0.12	0.33	5178
type of materials 4	0.06	0.24	5178
household uses industrial fuel	0.08	0.27	5178
dummy, =1 if urban	0.11	0.31	5178

The table reports summary statistics for the control variables in CRECER districts during the period 2005-2008

Table 16: difference-in-difference estimation with 2000 and 2004 data

Regressors	Dependent variable - having ever experienced physical violence	
	No controls	All controls
dummy, =1 for 2004 observations	0.10*	0.06
	(0.05)	(0.07)
interaction between dummy for 2004 observations and dummy equal to 1 for 637 Juntos districts	0.01	0.03
	(0.07)	(0.07)
proxy means score		0.11
		(0.29)
proxy means score squared		-0.15
		(0.21)
age		0.01***
		(0.00)
primary education completed		-0.02
		(0.02)
secondary education completed		-0.08***
		(0.03)
dummy, =1 if illiterate		0.02
		(0.04)
dummy, =1 if living together (not married)		0.00
		(0.02)
number of children		0.05***
		(0.01)
dummy, = 1 if father used to beat her mother		0.11***
		(0.02)
household has electricity		0.02
		(0.04)
household has access to hygienic restroom		-0.01
		(0.03)
household has access to piped water		-0.03
		(0.02)
number of asserts from Juntos score list that the household does not have		0.01
		(0.02)
type of materials 1		0.01
		(0.07)
type of materials 2		-0.03
		(0.07)
type of materials 3		0.06

		(0.08)
type of materials 4		-0.04
		(0.04)
household uses industrial fuel		0.00
		(0.04)
dummy, =1 if urban		0.08
		(0.06)
constant	0.38***	-0.01
	(0.01)	(0.10)
Number of observations	4,252	4,252

note: *** p<0.01, ** p<0.05, * p<0.1

district fixed effects included, robust standard errors clustered at district level in the parenthesis

Table 17: Juntos impact on domestic violence - difference-in-difference regressions

No controls regressions

	experienced in the last 12 months		
	any physical violence	any sexual violence	any emotional violence
dummy, =1 if a district enrolled in Juntos in a given year	-0.10**	-0.04	-0.11**
	(0.04)	(0.03)	(0.05)
constant	0.24***	0.08***	0.24***
	(0.03)	(0.03)	(0.04)

All controls regressions

	experienced in the last 12 months		
	any physical violence	any sexual violence	any emotional violence
dummy, =1 if a district enrolled in Juntos in a given year	-0.09**	-0.03	-0.11**
	(0.04)	(0.03)	(0.05)
proxy means score	0.26	-0.13	0.29
	(0.22)	(0.15)	(0.24)
proxy means score squared	-0.08	0.08	-0.17
	(0.17)	(0.12)	(0.17)
age	0.00	0.00**	0.00*
	(0.00)	(0.00)	(0.00)
primary education completed	0.01	0.00	0.01
	(0.02)	(0.01)	(0.02)
secondary education completed	0.00	-0.01	-0.01
	(0.02)	(0.01)	(0.03)
dummy, =1 if has health insurance	-0.01	-0.01	-0.02
	(0.01)	(0.01)	(0.01)
dummy, =1 if illiterate	-0.06*	0.01	-0.02
	(0.04)	(0.02)	(0.04)
dummy, =1 if living together (not married)	0.04***	0.01	0.04***
	(0.01)	(0.01)	(0.01)
number of children	0.00	0.01*	-0.00
	(0.00)	(0.00)	(0.00)
dummy, = 1 if father used to beat her mother	0.07***	0.02***	0.06***
	(0.01)	(0.01)	(0.01)
has a job and is paid in cash	0.02	0.02	0.03*
	(0.02)	(0.01)	(0.02)
household has electricity	0.05**	0.01	0.03

	(0.02)	(0.01)	(0.02)
household has access to hygienic restroom	-0.01	-0.02	0.00
	(0.03)	(0.02)	(0.03)
household has access to piped water	-0.01	-0.01	-0.02
	(0.02)	(0.01)	(0.02)
number of asserts from Juntos score list that the household does not have	-0.01	0.00	-0.01
	(0.01)	(0.01)	(0.02)
household owns land usable for agriculture	-0.02	-0.00	-0.02
	(0.02)	(0.01)	(0.02)
type of materials 1	-0.05*	-0.01	-0.03
	(0.03)	(0.01)	(0.03)
type of materials 2	-0.01	-0.01	-0.05
	(0.03)	(0.01)	(0.03)
type of materials 3	0.01	0.00	0.02
	(0.04)	(0.02)	(0.04)
type of materials 4	-0.00	-0.03	0.05
	(0.05)	(0.03)	(0.06)
household uses industrial fuel	0.02	-0.02	-0.02
	(0.04)	(0.02)	(0.04)
dummy, =1 if urban	-0.07**	0.01	-0.08**
	(0.03)	(0.03)	(0.03)
constant	0.14	0.05	0.13
	(0.10)	(0.06)	(0.10)
Number of observations	3,904	3,904	3,903

note: *** p<0.01, ** p<0.05, * p<0.1

all regression include district and year fixed effects; robust standard errors clustered at district level reported in the parenthesis

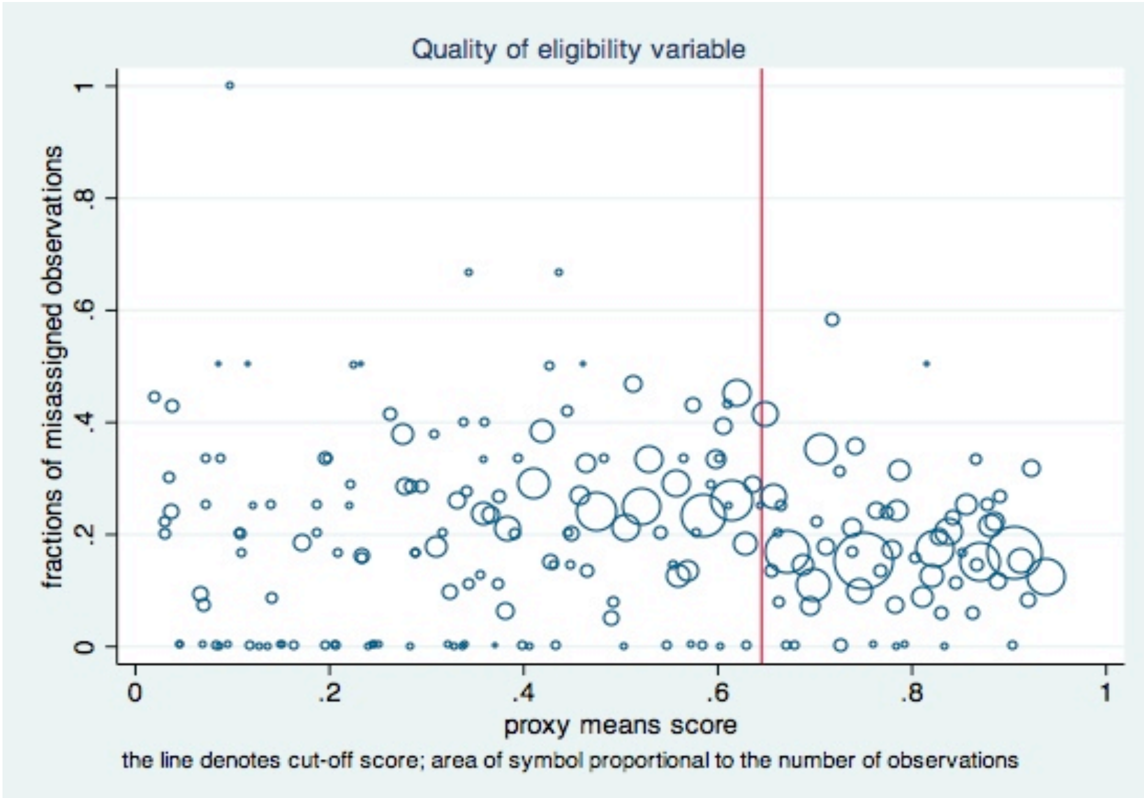
Quality of eligibility variable
fractions of misassigned observations
0
.2
.4
.6
.8
1
proxy means score
the line denotes cut-off score; area of symbol proportional to the number of observations

Figure 5

Table 18: heterogeneity in impacts depending on the fraction of beneficiaries in the district

	experienced in the last 12 months		
	any physical violence	any sexual violence	any emotional violence
dummy, =1 if a district enrolled in Juntos in a given year	-0.06	-0.04	-0.10*
	(0.04)	(0.03)	(0.05)
Deviation from the average fraction of beneficiaries	-0.49*	0.21	-0.40*
	(0.26)	(0.15)	(0.21)
Number of observations	3,904	3,904	3,903

note: *** p<0.01, ** p<0.05, * p<0.1

all regression include district and year fixed effects; robust standard errors clustered at district level reported in the parenthesis

Control variables include: proxy means score and its square, age, number of children, dummies for completion of primary and secondary education, indicators for health insurance, illiteracy, cohabitation, having a job with cash earnings, dummy equal to 1 if father used to beat her mother, dummies for household access to electricity, hygienic restroom, piped water, agricultural land ownership, types of materials used in construction, use of industrial fuel, number of asserts from Juntos score list that the household does not have and urban/rural indicator

Table 19: heterogeneity in impacts depending on out-of-marriage prospects

	experienced in the last 12 months					
	any physical violence		any sexual violence		any emotional violence	
depending on the number of children						
dummy, =1 if a district enrolled in Juntos	-0.09**	-0.13**	-0.03	-0.03	-0.11**	-0.15***
	(0.04)	(0.05)	(0.03)	(0.03)	(0.05)	(0.05)
number of children a woman has	0.00	0.00	0.01*	0.01	-0.00	-0.01
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
interaction between Juntos indicator and the number of children		0.02		0.00		0.02*
		(0.01)		(0.01)		(0.01)
depending on exposure to violence as a child						
dummy, =1 if a district enrolled in Juntos in a given year	-0.09**	-0.11**	-0.03	-0.04	-0.11**	-0.12**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.05)	(0.05)
dummy, = 1 if father used to beat her mother	0.07***	0.02	0.02***	-0.01	0.06***	0.03
	(0.01)	(0.04)	(0.01)	(0.02)	(0.01)	(0.03)
interaction between Juntos indicator and exposure to violence as a child		0.05		0.03**		0.04
		(0.03)		(0.02)		(0.03)
depending on the availability of a cash-paying job						
dummy, =1 if a district enrolled in Juntos	-0.09**	-0.08*	-0.03	-0.02	-0.11**	-0.09*
	(0.04)	(0.04)	(0.03)	(0.03)	(0.05)	(0.05)
has a job and is paid in cash	0.02	0.07**	0.02	0.06***	0.03*	0.09**
	(0.02)	(0.04)	(0.01)	(0.02)	(0.02)	(0.04)
interaction between Juntos indicator and having a cash-paid job		-0.02		-0.05**		-0.07*
		(0.04)		(0.02)		(0.04)
Number of observations	3,904		3,904		3,903	

note: *** p<0.01, ** p<0.05, * p<0.1

all regression include district and year fixed effects; robust standard errors clustered at district level reported in the parenthesis

Control variables include: proxy means score and its square, age, dummies for completion of primary and secondary education, indicators for health insurance, illiteracy, cohabitation, having a job with cash earnings, dummy equal to 1 if father used to beat her mother, dummies for household access to electricity, hygienic restroom, piped water, agricultural land ownership, types of materials used in construction, use of industrial fuel, number of asserts from Juntos score list that the household does not have, and urban/rural indicator

Table 20: Juntos impacts on domestic violence - matching estimator

	any physical violence	any sexual violence	any emotional violence
Average treatment effect on the treated	-0.08*	0.01	-0.10**
	(0.04)	(0.02)	(0.04)
Number of observations	929	929	928

note: *** p<0.01, ** p<0.05, * p<0.1

control and treatment units matched on basis of district poverty score, Juntos eligibility score, its square, age, dummies for completion of primary and secondary education, indicators for health insurance, illiteracy, cohabitation, number of children, having a job with cash earnings, dummy equal to 1 if father used to beat her mother, dummies for household access to electricity, hygienic restroom, piped water, agricultural land ownership, types of materials used in construction, use of industrial fuel, number of asserts from Juntos score list that the household does not have, and urban/rural indicator. Number of matches = 3

Table 21: Probits for calibrating the propensity scores

Variables used in probit regressions

Variable	Variable definition
Sp1	severity of poverty in 2005
Sp2	dummy equal to 1, if a district belongs to the third quartile in Sp1 distribution
Mp1	poverty headcount in 2005
Mp2	dummy equal to 1, if a district belongs to the third quartile in Mp1 distribution
Mn1	percent of children affected by cronical malnutrition
Mn2	dummy equal to 1 if a district belongs to the fourth quartile in Mn1 distribution
Av1	percent of centros poblados affected by violence
Av2	dummy equal to 1 if a district belongs to the third quartile in the Av1 distribution
Wc	dummy equal to 1 if a district belongs to the second quartile in the distribution of district averages of households with hygienic latrines
C1	district average of per household monthly spending in 2005
C2	dummy equal to 1 if a district belongs to the first or second percentile in C1 distribution
y	proxy means score
y1	dummy equal to 1 if a household belongs to 10th to 25th percentile in the distribution of y
y2	dummy equal to 1 if a household belongs to top 10 percent in the distribution of y

Probit regressions results

Variables	Coefficient	t-stat
Sp1*Sp2	0.0277118	2.5
Sp1*Sp2*Mp1*Mp2	0.0001638	2.61
Mn1*Mn2	0.0218699	23.14
Mn1*Mn2*Sp1*Sp2	-0.0013804	-11.03
Mn1*Mn2*Mp1*Mp2	0.0000821	4.17
Av1*Av2*Wc	0.3289244	7.01
Av1*Av2*Wc*Sp1*Sp2	0.0378576	2.61
C1*C2	0.0049853	2.85
y	2.272822	10.36
y*y1*Sp1*Sp2*Mn1*Mn2	0.0000755	0.12
y2*C1*C2*Sp1*Sp2	-0.0001398	-1.63
y*y2	-0.1172429	-1.4
(y*Sp1*Sp2)^2	-0.0015558	-1.48
y*C1*C2	-0.0060681	-2.66
Number of observations	6144	
Number of treated off common support	0	
R2	0.2147	

Note: dependent variable is equal to 1 if household participated in Juntos and equal to 0 otherwise

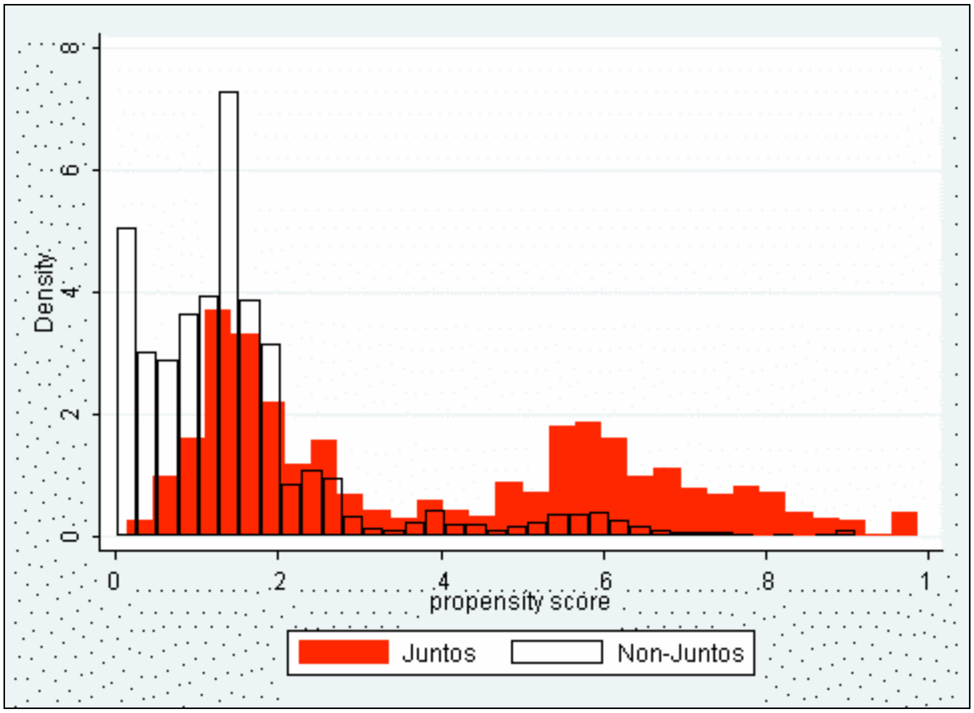

Figure 6: Predicted propensity scores for Juntos beneficiaries and potential controls

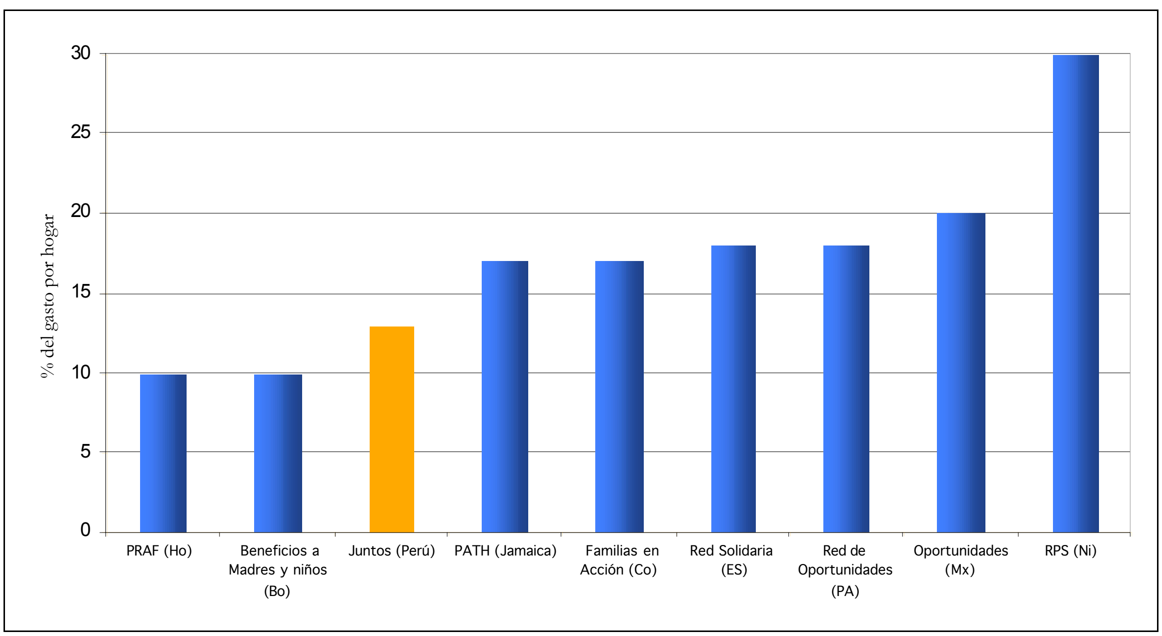

Figure 7: Transfer size as a share of total consumption

Table 22: Juntos impacts in household consumption and income (per capita monthly)

Variable	Average for control group (soles)	Juntos effect	Number of observations
Consumption	188.40	-0.02	2,513
		(0.03)	
Food consumption	94.54	0.01	2,505
		(0.03)	
Non-food consumption	94.40	0.03	2,501
		(0.04)	
Monetary spending	89.62	0.18***	2,067
		(0.05)	
Monetary spending on food	45.65	0.34***	2,076
		(0.06)	
Monetary spending on non-food	41.07	0.02	2,048
		(0.06)	
Total income	197.00	0.01	2,065
		(0.04)	
Monetary income	107.65	0.28***	2,510
		(0.06)	

note: *** p<0.01, ** p<0.05, * p<0.1; all dependent variables are in logs

Table 23: Juntos impacts in the use of health services, children under 5

Variable	Average for control group	Juntos effect	Number of observations
in case of illness, sought medical attention	0.43	0.22***	1,205
		(0.05)	
received vaccinations in the last 3 months	0.36	0.07***	2,293
		(0.03)	
received health checks in the last three months	0.46	0.37***	2,297
		(0.03)	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 24: Juntos impacts in use of health services, by gender, children under 5

Variable	Average for control group	Juntos effect	Number of observations
Girls			
in case of illness, sought medical attention	0.50	0.16**	554
		(0.07)	
received vaccinations in the last 3 months	0.51	0.09**	1,114
		(0.04)	
received health checks in the last three monts	0.40	0.38***	1,101
		(0.04)	
Boys			
in case of illness, sought medical attention	0.44	0.28***	599
		(0.06)	
received vaccinations in the last 3 months	0.33	0.06	1,189
		(0.04)	
received health checks in the last three monts	0.50	0.32***	1,184
		(0.04)	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 25: Juntos impacts in use of health services, by age, children under 5

Variable	Average for control group	Juntos effect	Number of observations
in case of illness, sought medical attention			
0 to 12 months	0.49	0.18**	414
		(0.08)	
13 to 36 months	0.59	-0.03	408
		(0.08)	
37 to 59 months	0.35	0.39***	367
		(0.09)	
received vaccinations in the last 3 months			
0 to 12 months	0.51	0.06	705
		(0.05)	
13 to 36 months	0.35	0.04	804
		(0.05)	
37 to 59 months	0.28	0.11**	806
		(0.05)	
received health checks in the last three monts			
0 to 12 months	0.67	0.29***	695
		(0.05)	
13 to 36 months	0.45	0.40***	805
		(0.05)	
37 to 59 months	0.25	0.39***	811
		(0.05)	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 26: Juntos impacts in the use of health services, women of child-bearing age

Variable:	Average for control group	Juntos effect	Number of observations
in case of illness, sought medical attention	0.29	0.12***	2,261
		(0.03)	
received vaccinations in the last 3 months	0.34	0.17***	3,948
		(0.02)	
received contraceptives in the last 3 months	0.10	0.07***	3,936
		(0.02)	
delivery was assisted by a doctor	0.41	0.04	646
		(0.06)	
participated in the family planning activities	0.11	0.07***	3,704
		(0.02)	
participated in the health campaigns	0.00	0.01	3,955
		(0.00)	
received iron supplements	0.32	0.10	84
		(0.26)	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 27: Juntos impacts in food consumption

Variable	Average for control group (soles)	Juntos effect	Number of observations
Breads and cereals	8.15	1.99***	2,510
		(0.40)	
Meat	2.21	0.21	2,521
		(0.27)	
Seafood	1.44	0.05	2,522
		(0.16)	
Milk, cheese, eggs	1.65	0.10	2,518
		(0.20)	
Butter and oils	1.91	0.36***	2,515
		(0.10)	
Vegetables	2.52	0.34**	2,530
		(0.15)	
Fruit	1.40	0.66***	2,515
		(0.14)	
Grains	0.72	0.42***	2,523
		(0.10)	
Tubers	1.65	0.62***	2,531
		(0.15)	
Sugar	2.65	0.51***	2,517
		(0.14)	
Coffee, tea, cacao	0.28	0.06	2,531
		(0.04)	
Other	1.45	0.48***	2,518
		(0.11)	
Non-alcoholic beverages	0.59	0.09	2,525
		(0.09)	
Alcoholic beverages	0.28	-0.15**	2,525
		(0.07)	

note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$; dependent variable is per capita monthly consumption at household level

Table 28: Impacts in self-reported health

Variable	Average for control group	Juntos effect	Number of observations
did not experience any illness in the last 4 weeks			
Children under 5	0.46	0.06*	2,307
		(0.03)	
Women of childbearing age	0.46	0.01	3,977
		(0.03)	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 29: Juntos impacts in final outcome indicators

Variable	Average for control group	Juntos effect	Number of observations
Children under 5			
Hemoglobin	11.45	-0.45	128
		(0.90)	
Height for age	-1.56	-0.16	264
		(0.42)	
Weight for age	-1.12	0.44	262
		(0.40)	
Women of childbearing age			
Hemoglobin	12.71	0.19	347
		(0.36)	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 30: Juntos impacts on education

Variable	Average for control group	Juntos effect	Number of observations
	0.81	0.04***	4,570
Registered at school		-0.01	
	0.8	0.01	4,557
Attendance		-0.01	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 31: Juntos impacts in education, disaggregated by age

Variable	Average for control group	Juntos effect	Number of observations
Registered at school			
	0.72	0.11**	524
age 6		-0.05	
	0.83	0.10**	530
age 7		-0.04	
	0.88	-0.01	558
age 8		-0.04	
	0.89	-0.05	498
age 9		-0.04	
	0.79	-0.01	541
age 10		-0.04	
	0.83	0.01	525
age 11		-0.04	
	0.76	0.04	553
age 12		-0.04	
	0.68	0.08	524
age 13		-0.05	
	0.72	-0.01	488
age 14		-0.06	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 32: Juntos impacts in education, disaggregated by age

Variable	Average for control group	Juntos effect	Number of observations
School attendance			
	0.7	0.12**	527
age 6		-0.05	
	0.8	0.13***	528
age 7		-0.04	
	0.87	-0.04	556
age 8		-0.04	
	0.87	-0.03	500
age 9		-0.04	
	0.76	-0.06	535
age 10		-0.04	
	0.8	0.01	521
age 11		-0.04	
	0.74	0	553
age 12		-0.04	
	0.67	0.04	527
age 13		-0.05	
	0.68	-0.06	492
age 14		-0.06	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 33: Juntos impacts on educational spending

Household spending, per year, soles	Average for control group (soles)	Juntos effect	Number of observations
Uniforms	40.85	30.03***	2,503
		5.81	
Books and other supplies	1.01	-1.20	2,496
		0.86	
Tuition	61.86	-32.93**	2,497
		16.39	

note: *** p<0.01, ** p<0.05, * p<0.1

Table 34: Juntos impacts in fertility, women of child-bearing age

Variable	Average for control group	Juntos effect	Number of observations
Gave birth in the last three years	0.25	-0.01 (0.02)	3,671

note: *** p<0.01, ** p<0.05, * p<0.1

Table 35: Juntos impacts in household consumption and income (per capita monthly) - intensity effects

Variable	Average for control group (soles)	1 year or less	13 to 25 months	F-test	Number of observations
Consumption	188.40	-0.03 (0.03)	0.02 (0.04)	2.14	2,506
Food consumption	94.54	-0.02 (0.03)	0.00 (0.05)	0.37	2,491
Non-food consumption	94.40	-0.01 (0.04)	0.04 (0.06)	1.31	2,498
Monetary spending	89.62	0.29*** (0.05)	0.48*** (0.08)	11.49	2,067
Monetary spending on food	45.65	0.20*** (0.06)	0.28*** (0.10)	1.12	2,046
Monetary spending on non-food	41.07	0.03 (0.06)	0.10 (0.08)	1.2	2,067
Total income	197.00	0.00 (0.04)	0.05 (0.06)	1.42	2,080
Monetary income	107.65	0.34*** (0.06)	0.51*** (0.08)	7.1	2,504

note: *** p<0.01, ** p<0.05, * p<0.1; all dependent variables in logs

Table 36: Impacts in use of health services, children under 5 - intensity effects

Variable	Average for control group	1 year or less	13 to 25 months	F-tests	Number of observations
in case of illness, sought medical attention	0.43	0.24***	0.29***	4.5	1,129
		(0.05)	(0.07)		
received vaccinations in the last 3 months	0.36	0.00	-0.11**	6.87	2,300
		(0.03)	(0.04)		
received health checks in the last three monts	0.46	0.40***	0.47***	0.96	2,294
		(0.03)	(0.04)		

note: *** p<0.01, ** p<0.05, * p<0.1;

Table 37: Impacts in use of health services, women of child-bearing age - intensity effects

Variable	Average for control group	1 year or less	13 to 25 months	F-tests	Number of observations
in case of illness, sought medical attention	0.29	0.11***	0.18***	5.59	2,213
		0.03	0.04		
received vaccinations in the last 3 months	0.34	0.12***	-0.01	29.86	3,957
		0.02	0.03		
received contraceptives in the last 3 months	0.10	0.04***	0.04	0.24	3,970
		0.02	0.02		
received pre-natal checks in the last 12 months	0.08	-0.03**	-0.01	1.5	3,961
		0.01	0.02		
delivery was assisted by a doctor	0.41	0.00	0.18*	5.24	691
		0.06	0.09		
participated in the family planning activities	0.11	0.06***	0.07***	0.14	3,692
		0.02	0.02		
participated in the health campaigns	0.00	0.00	-0.00	2.93	3,968
		0.00	0.01		
received iron supplements	0.32	0.00	0.42	3.65	91
		0.24	0.32		

note: *** p<0.01, ** p<0.05, * p<0.1;

Table 38: Juntos impacts in food consumption - intensity effects

Variable	Average for control group (soles)	1 year or less	13 to 25 months	F-test	Number of observations
Breads and cereals	8.15	1.58***	2.32***	2.92	2,530
		(0.40)	(0.58)		
Meat	2.21	0.42	0.54	0.17	2,515
		(0.27)	(0.38)		
Seafood	1.44	-0.13	0.22	4.02	2,514
		(0.16)	(0.23)		
Milk, cheese, eggs	1.65	0.19	0.78***	7.7	2,529
		(0.20)	(0.29)		
Butter and oils	1.91	0.48***	0.69***	4.03	2,535
		(0.10)	(0.14)		
Vegetables	2.52	0.32**	0.77***	7.64	2,523
		(0.15)	(0.22)		
Fruit	1.40	0.45***	0.36*	0.41	2,521
		(0.13)	(0.19)		
Grains	0.72	0.42***	0.48***	0.27	2,525
		(0.11)	(0.16)		
Tubers	1.65	0.38**	0.75***	4.51	2,511
		(0.16)	(0.23)		
Sugar	2.65	0.28**	0.61***	4.91	2,527
		(0.14)	(0.20)		
Coffee, tea, cacao	0.28	0.03	0.03	0	2,502
		(0.05)	(0.07)		
Other	1.45	0.30***	0.77***	15.44	2,523
		(0.11)	(0.16)		
Non-alcoholic beverages	0.59	0.09	0.19*	1.42	2,535
		(0.08)	(0.11)		
Alcoholic beverages	0.28	-0.17**	-0.22**	0.43	2,524
		(0.07)	(0.11)		
Food consumed outside	11.62	-3.17**	-5.49***	2.53	2,531
		(1.35)	(1.94)		

note: *** p<0.01, ** p<0.05, * p<0.1

Table 39: Juntos impacts in self-reported health, intensity effects

Variable	Average for control group	1 year or less	13 to 25 months	F-tests	Number of observations
did not experience any illness in the last 4 weeks					
Children under 5	0.46	-0.00	0.09*	11.52	2,307
		(0.03)	(0.05)		
Women of childbearing age	0.46	-0.01	0.06*	7.71	3,954
		(0.03)	(0.04)		

note: *** p<0.01, ** p<0.05, * p<0.1;

Table 40: Juntos impacts in final outcome indicators, intensity effects

Variable	Average for control group	1 year or less	13 to 25 months	F-tests	Number of observations
Children under 5					
Hemoglobin	11.45	0.17	-0.19	0.43	135
		(0.81)	(0.90)		
Height for age	-1.56	-0.14	-0.35	0.57	265
		(0.40)	(0.41)		
Weight for age	-1.12	0.51	0.32	0.49	271
		(0.35)	(0.37)		
Women of childbearing age					
Hemoglobin	12.71	0.17	-0.19	1.4	135
		(0.81)	(0.90)		

note: *** p<0.01, ** p<0.05, * p<0.1;

Table 41: Juntos impacts in education: intensity effects

Variable	Average for control group	1 year or less	13 to 25 months	F-tests	Number of observations
All					
Registered at school	0.81	0.08***	0.14***	14.75	4,581
		(0.01)	(0.02)		
School attendance	0.80	0.03*	0.09***	17.17	4,581
		(0.01)	(0.02)		

note: *** p<0.01, ** p<0.05, * p<0.1

Table 42: instrumental variables regressions, first stage

Dependent variable is a dummy, equal to 1 if a respondent receives Juntos benefits	
=1 if eligible and in district enrolled in Juntos at the time of the interview	0.33***
	(0.01)
=1 if the respondent is eligible for Juntos according to proxy means	-0.05***
	(0.01)
=1 if district incorporated in juntos at the time of the interview	0.01
	(0.01)
Number of observations	10,671

note: *** p<0.01, ** p<0.05, * p<0.1

Table 43: IV regressions - Juntos impacts in household consumption and income (per capita monthly)

Variable	Juntos effect	Number of observations
Consumption	0.07 (0.06)	10,671
Food consumption	0.67*** (0.11)	10,490
Non-food consumption	0.21** (0.09)	10,671
Monetary spending	0.60*** (0.10)	10,639
Monetary spending on food	0.67*** (0.11)	10,490
Monetary spending on non-food	0.54*** (0.11)	10,625
Total income	0.04 (0.07)	10,671
Monetary income	0.67*** (0.13)	10,594

note: *** p<0.01, ** p<0.05, * p<0.1; all dependent variables in logs

Table 44: IV regressions: Juntos impacts in the use of health services, children under 5

Variable	Juntos effect	Number of observations
in case of illness, sought medical attention	0.33** (0.14)	3,470
received vaccinations in the last 3 months	0.20** (0.10)	6,765
received health checks in the last three months	0.33*** (0.09)	6,765

note: *** p<0.01, ** p<0.05, * p<0.1

Table 45: IV regressions - Juntos impacts in the use of health services, women of child-bearing age

Variable:	Juntos effect	Number of observations
in case of illness, sought medical attention	0.06 (0.05)	28,758
received vaccinations in the last 3 months	0.14*** (0.04)	50,845
received contraceptives in the last 3 months	0.04 (0.03)	50,845
delivery was assisted by a doctor	0.01 (0.18)	6,133
participated in the family planning activities	0.05*** (0.03)	50,750
participated in the health campaigns	0.01 (0.01)	50,845
received iron supplements	0.08 (0.25)	1,615

note: *** p<0.01, ** p<0.05, * p<0.1

Table 46: IV regressions - Juntos impacts in food consumption

Variable	Juntos effect	Number of observations
Breads and cereals	2.86*** (1.03)	10,671
Meat	2.63*** (0.86)	10,671
Seafood	2.35*** (0.43)	10,671
Milk, cheese, eggs	1.31** (0.63)	10,671
Butter and oils	0.09 (0.22)	10,671
Vegetables	1.34*** (0.43)	10,671
Fruit	0.37 (0.43)	10,671
Grains	1.23*** (0.26)	10,671
Tubers	0.46 (0.39)	10,671
Sugar	0.27 (0.33)	10,671
Coffee, tea, cacao	0.45*** (0.14)	10,671
Other	-0.17 (0.29)	10,671
Non-alcoholic beverages	0.98*** (0.26)	10,671
Alcoholic beverages	0.28* (0.16)	10,671

note: *** p<0.01, ** p<0.05, * p<0.1; dependent variable is per capita monthly consumption at household level

Table 47: IV regressions - Juntos impacts in self-reported health

Variable	Juntos effect	Number of observations
did not experience any illness in the last 4 weeks		
Children under 5	0.22** (0.11)	6,796
Women of childbearing age	-0.03 (0.04)	50,845

note: *** p<0.01, ** p<0.05, * p<0.1

Table 48: IV regressions - Juntos impacts in final outcome indicators

Variable	Average for control group	Juntos effect
Children under 5		
Hemoglobin	0.62 (0.67)	1,004
Height for age	0.08 (0.33)	2,014
Weight for age	0.76** (0.35)	2,014
Women of childbearing age		
Hemoglobin	0.36 (0.29)	4,333

note: *** p<0.01, ** p<0.05, * p<0.1

Table 49: IV regressions - Juntos impacts on education

Variable	Juntos effect	Number of observations
Registered at school	0.02 (0.04)	12,595
Attendance	-0.04 (0.04)	12,616

note: *** p<0.01, ** p<0.05, * p<0.1

Table 50: IV regressions - Juntos impacts on educational spending

Household spending, per year, soles	Average for control group (soles)	Juntos effect
Uniforms	68.65*** (10.67)	10,671
Books and other supplies	-4.04** (1.96)	10,671
Tuition	-57.79 (51.44)	10,671

note: *** p<0.01, ** p<0.05, * p<0.1

Table 51: IV regressions: Juntos impacts in fertility, women of child-bearing age

Variable	Juntos effect	Number of observations
Gave birth in the last three years	-0.02 (0.03)	50,829

note: *** p<0.01, ** p<0.05, * p<0.1

Appendix

I list the questions in the same order and grouped similarly to the way they appear in ENDES survey:

Emotional violence questions:

Has your partner/spouse ever:

Said or done anything to humiliate you?

Threatened to do harm to you or anyone you care about?

Threatened to leave the house, take the children away or leave you without economic help?

Physical violence questions:

Has your partner/spouse ever:

Pushed you, shaken you, knocked you down?

Slapped you or twisted your arm?

Hit you with his fists or with anything that could hurt you?

Kicked or dragged you?

Tried to strangle or burn you?

Attacked you with a knife, a gun or any other weapon?

Threatened you with a knife, gun or any other type of weapon?

Used physical force to have sexual relations with you, though you did not want to?

Forced you to do sexual acts that you did not approve of?