Challenges to Promoting Social Inclusion of the Extreme Poor: Evidence from a Large Scale Experiment in Colombia

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Abstract

We evaluate the large scale pilot of an innovative and major welfare intervention in Colombia, which combines homes visits by trained social workers to households in extreme poverty with preferential access to social programs. We use a randomized control trial and a very rich dataset collected as part of the evaluation to identify program impacts on the knowledge and take-up of social programs and the labor supply of targeted households. We find no consistent impact of the program on these outcomes, possibly because the way the pilot was implemented resulted in very light treatment in terms of home visits. Importantly, administrative data indicates that the program has been rolled out nationally in a very similar fashion, suggesting that this major national program is likely to fail in making a significant contribution to reducing extreme poverty. We suggest that the program should undergo substantial reforms, which in turn should be evaluated.

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1 Introduction

Households that live in extreme poverty often face a multitude of interacting constraints that prevent them from improving their lives (see, for example, Duflo, 2012). The causes of 'poverty traps' have been much debated, and yet it is not always obvious where market imperfections and frictions arise, nor how to effectively tackle them. Existing work emphasizes capital and skill constraints as an important mechanism leading to the persistence of poverty (e.g. Banerjee and Newman, 1993; Galor and Zeira, 1993; Ghatak and Jiang, 2002). Related literature further highlights coordination problems (e.g. Kremer, 1993) and psychological and behavioral constraints that arise due to poverty (e.g. Mullainathan and Shafir, 2013; Dalton, Ghosal and Mani, 2014). In order to address these issues, it is common for countries to set up a range of social programs, usually aimed at addressing one constraint at a time. However, many of the individuals most likely to benefit are often the least likely to enroll in such programs. It has been suggested that this is due to a lack of knowledge, stigma, over-complex programs and a lack of self-control (e.g. Currie, 2006). This paper evaluates a large-scale social program in Colombia which aims to address these issues.

In 2007, the Colombian government launched a large-scale pilot program called Juntos, designed to tackle extreme poverty. This program aimed to address a number of different monetary and non-monetary constraints to improve economic outcomes and the welfare of the poorest families along a number of dimensions. This included improvements in health, housing, nutrition and labor supply outcomes. The main purpose of the program was to build in indigent families the basic capacities to sustainably manage their own development, and to stimulate demand for existing social programs. The program attempted to achieve these goals through home visits from social workers over a five year period, in addition to expanding and improving the supply of existing programs in a coordinated effort by federal, regional and local government agencies. It was rolled out on a national scale in the second half of 2011 under the name of Unidos, with broadly the same scheme and aims.[1] The national program now targets 1.5 million families and accounted for 5% of the total public budget for social inclusion in 2013.[2] This program was inspired by Chile Solidario, introduced in Chile in 2002. Programs similar in nature to Unidos have become increasingly popular as a core strategy to alleviating poverty in a number of other Latin American countries, including Brazil, Mexico and Peru.[3] Understanding the impacts of this program is therefore of

[1]Since the data that we analyze pertain to the initial phase of the program, we will refer to the program as Juntos except when specifically referring to the current program in Colombia.

[2]See Cuadro 31 in "MENSAJE PRESIDENCIAL PROYECTO DE PRESUPUESTO GENERAL DE LA NACION 2013", last accessed 20 October 2014. Note that Familias en Accion accounts for almost 40% of this budget, serving around 2.2 million of poor families.

[3]Chile was the first country to introduce this type of program in 2002, and it is called Chile Solidario. Brazil has introduced a similar program called Brasil sem Miseria in 2011, and Mexico is implementing a variant called Contigo Vamos Por Mas. Each program places different emphasis on the different components of the program: demand side and psychosocial support versus coordination between demand and supply.

high priority to policymakers across a number of countries.

In this paper, we examine the short-run impact of Juntos on the knowledge of a range of existing social programs, the take-up of the main Colombian conditional cash transfer program, Familias en Accion,[4] and labor market outcomes. These labor market outcomes include the participation rate, employment rate (and type of employment), unemployment rate, and hours worked, as well as employment earnings and tenure. Impacts cover the initial 18-month period following the implementation of the program across three main groups within the extreme poor in Colombia: rural, urban and displaced households. The impact results are estimated using a large dataset collected as part of a large randomised control trial. We observe some selection into the treatment group as well as into the panel sample that could be potentially non-random, especially for the urban population. We carefully document this for the three different representative samples and provide difference-in-difference ITT and IV estimates that aim to correct for these potential biases.

Our results suggest that Juntos had no systematic and significant effects on the outcomes of interest. For example, focusing on rural households, we find no impact on the knowledge of existing social programs. We find a positive impact on the use of Familias en Accion, relatively large in magnitude but only statistically significant at the 10%. We find no positive impact on labor market outcomes. In fact we find negative effects on the probability of employment for rural women, driven by a decrease in self-employment within this group. This is accompanied by a decrease in the hourly pay for rural women. These results are consistent with recent empirical evidence on conditional cash transfers, which suggests that these programs are associated with a decrease in the labor supply of beneficiary individuals. This is particularly likely for women with young children (see for example Alzua et al., 2013). However, given the large number of hypotheses being tested simultaneously, we would expect to find some significant effects merely by chance. Hence, we conclude that Juntos had no impact on the outcomes of interest overall. These results are also consistent with a preliminary evaluation analysis of Juntos on a set of restricted labor market outcomes and other broader indicators,[5] which found no impacts of the policy.

Our main hypothesis for explaining why we observe no consistent impacts of the program is that treatment intensity was extremely low. Under the initial plans, social workers were intended to have an average caseload of 120 households per year under the intensive treatment arm and 180 households under a non-intensive (or classic) arm, and it was expected that households receiving the intensive treatment would experience the greatest positive effects. In practice, there was no distinction between intensive and non-intensive treatment, social workers received large caseloads,

[4] The take-up of other social programs is not evaluated since the proportion of households using these programs at baseline is extremely low and there is insufficient statistical power.

[5] A preliminary simple analysis of a restricted set of outcomes shows no consistent program impacts. This analysis was conducted in a short time period to provide a quick assessment of the program impact on broad variables such as employment, income, or poverty at the household level, without considering differences between genders. See "Evaluación de Impacto de Juntos (hoy Unidos). Red de Protección Social para la Superación de la Pobreza Extrema Informe de Evaluación – Diciembre de 2011" by Fedesarrollo, Econometria, SEI and IFS.

treating an average of 180 families per year across both arms. This has potential implications for both the quality and quantity of treatment. For example, households received an average of only three visits over an 18 month period across both treatment arms, much lower than the intended number of visits per year set out initially by the program plan. As a result, it is unlikely that such treatment would have significant effects on household outcomes, even if such effects may be possible under a more intensive treatment scheme.

The home visits had two main objectives: (i) to build up the psychosocial capabilities of the extreme poor that may be constraining their behavior, such as self-control, and (ii) to improve access to and the use of available social programs through information provision and preferential access. It is unlikely that the first objective was reached within such a low number of visits. Furthermore, administrative data suggests that the way that Juntos operated implied a high degree of variation in the quality of home visits. In many municipalities, a set of new social workers were hired each year to support targeted families, resulting in a lack of continuity in the relationship between the social worker and the household. Additionally, the qualifications and experience of social workers varied markedly.

The second objective comprises an important channel through which targeted households could improve their economic outcomes if these social programs were targeted to their needs, and were of sufficient quality. This channel could potentially be activated through the provision of information in the initial visits. However, there does not seem to have been significant improvements in the knowledge about these programs or the use of these. Focus groups carried out by the initial evaluation consortium (see footnote 4) found that targeted households did not feel that they had preferential access. Moreover, they felt that there existed a range of barriers that prevented access to these programs, including a mismatch between the design of these programs (along many dimensions) and the needs of these target households. Hence, without improving the supply of these programs, access and use will remain low.

One may think that, given the complexity of the program and the number of agencies involved, there may have been an initial period where there were significant issues in terms of coordination and implementation that resulted in teething problems in setting-up and running the program, but that these problems would dissipate over time as the program was rolled out nationally under the name of Unidos. However, administrative data shows that the treatment in the nationally rolled out program Unidos, despite being more intensive than in the pilot, remains very light. Social workers in each municipality are assigned to approximately 130 households on average per year.[6] This contrasts with the case of Chile Solidario, a much stronger version of this type of program, which was targeted at a comparable population and formed the basis for the design of Juntos and later Unidos. Treatment in Chile Solidario was more intense, with social workers assigned 50 households on average, a much smaller caseload than the average caseload in Unidos. As a

[6] Information provided to authors by private correspondence with ANSPE in August 2014.

result, households received an average of 10 visits per year for a maximum period of 24 months in Chile Solidario. In addition to this, households were guaranteed access to monetary subsidies to compensate them for participating in the program. Carneiro et al. (2014) evaluated the effects of Chile Solidario using a quasi-experimental approach. They found a positive impact on the take-up of a family allowance for poor children (Subsidio Unico Familiar), but found evidence of no impacts for labor market or other economic outcomes.

Taken together with our results, this evidence has policy implications that are important not only for Colombia, but in a wider context. Unidos is unlikely to make a significant contribution to the reduction of extreme poverty in Colombia. The results from the evaluation of Juntos suggest very little impact on the economic outcomes of participants in the short term, and no impact on the take-up of existing programs. The evidence from Chile Solidario suggests that even a stronger version of this program is unlikely to have significant impacts in improving the outcomes of their target population. These households are difficult to work with since they face constraints in different key areas such as skills, capital and psychological traits. Although recent empirical evidence shows that some interventions are successful in alleviating such constraints in different developing countries, these are usually small scale interventions of high quality, often provided by a non-government organization. A good recent example is given in Bandiera et al. (2012) on providing skills and vocational training to adolescent girls in Uganda. Programs that are available at a large scale are usually provided through the welfare system and there is a tradeoff between quantity and quality. First, it may be extremely difficult to deliver high-quality interventions that provides psychosocial support (either through home or group visits) of good quality at scale and at a reasonable cost, as the evidence discussed in this paper shows. Recent experimental evidence discussed in Attanasio et al (2014) about how to use the infrastructure of Familias en Accion to deliver a scalable and integrated early childhood program through home visits in Colombia may provide some positive policy lessons in this area. Second, even if the home visit component of Unidos was effective, its impact is expected to be mediated through the use of other effective social programs. Hence, the extent to which a program such as Unidos may be effective will depend on the quality of these other social programs and the extent to which they are tailored to the needs of the extreme poor.

Having said this, it may be possible that we would observe more significant results if the program was improved. This would take the form of improving the quantity and quality of social workers, including the relationship or bond between the social worker and the households, and improving the supply of existing social programs in terms of quality and quantity. In order to investigate this properly, a further pilot program with an experimental evaluation is required. An experimental design could be used to determine whether improvements in the quality of social worker (e.g. through better training and/or higher wages) and the reduction in caseload (through hiring new workers) lead to improvements in the policy impacts. It could also test if some of the social programs available to the extreme poor are effective at all. This would indicate whether the program can be modified to have significant impacts, or should be replaced in its entirety.

The remainder of the paper is structured as follows. The next Section provides background information about the program. Section 3 describes the evaluation design and issues related to the implementation. Section 4 discusses the data and provides descriptive statistics. Section 5 shows our empirical methodology and presents the impact results. Section 6 concludes.

2 Background and description of the program

In 2009, the Colombian government launched Juntos, a smaller scale pilot of the Unidos program that had been rolled out nationally by late 2011. This is a social protection program for individuals living in extreme poverty in Colombia.[7]Juntos comprised the same objectives and design as the full Unidos program, and participants faced the same eligibility criteria. Unidos is a large-scale government intervention. It targets, and currently almost serves, 1.5 million families in extreme poverty in all 1,102 municipalities across the 32 departments of Colombia, at an annual cost of approximately US$ 140 millions, or 5% of the total budget to promote social inclusion.[8] The scale and the cost of the program clearly reflect that this program is of substantial importance in Colombia.

The eligible population comprises two groups. First, households are eligible based on their low overall economic well-being. In Colombia, all households are registered to one of six SISBEN levels. SISBEN summarizes economic well-being, and is already used to identify eligible households for a number of different national welfare programs.[9] All households registered as SISBEN level 1 are eligible to enroll in the Unidos program, which includes roughly 20% of the poorest households. Around 1.2 million households qualified for Unidos under this criteria.

Second, households registered in the Unique Register of Displaced Population (Registro Unico de Poblacion Desplazada or RUPD) are eligible for participating in Unidos. Colombia is among the countries with the highest proportion of internally displaced people in the world.[10] To be registered on the RUPD, households must prove that they have been internally displaced by providing an oral account of the facts to a public office. This population is considered to be largely marginalized

[7]See the official website for more details:
https://www.dnp.gov.co/Programas/DesarrolloSocial/Pol%C3%ADticasSocialesTransversales/RedUnidosparaSuperaci%C3%B3ndelaPobrezaExtrema.aspx, last accessed on 26 February 2014.

[8]Information provided to authors by ANSPE by correspondence in August 2014, in turn sourced from Reporte CIIF - MINHACIENDA y Oficina Asesora de Planeación. Reporte CIIF - MINHACIENDA y Oficina Asesora de Planeación.

[9]For more information, see www.sisben.gov.co

[10]The UN Refugee Agency, see for example http://www.unhcr.org/pages/49c3646c23.html, last accessed 11 November 2014.

from society, and the program aims to facilitate their use of existing social security programs.[11] Eligibility for displaced families is irrespective of their SISBEN classification, with many of these households classified in higher levels.[12] There are 300,000 such households targeted by the program.

These are the same criteria used for Familias en Accion,[13] a conditional cash transfer program targeted at poor households with children, and which positive impacts have been widely reported.[14] As a result, a significant proportion of households targeted by Unidos are already registered in Familias en Accion.

The program employs a two-arm strategy to lift the most impoverished members of society out of poverty. The first arm aims to improve household skills and to increase their demand for social programs through home visits and providing information about the programs, while the second strengthens the supply of existing social programs. The first arm is delivered through home visits and has two specific objectives:

1. **Improving knowledge of, and access to, social welfare programs.** The first objective is to improve the knowledge of existing social welfare programs, and to improve access to these programs by removing the constraints that prevent the poorest families from becoming recipients. For example, social workers can provide assistance in completing sometimes complex and confusing application processes for enrolling in existing programs.

2. **Helping families to manage their own development.** The second objective is to provide a sustainable long-term escape from poverty through helping families to manage their own development by focusing in specific strategic areas. This is expected to be achieved via the home visits, by social workers *(cogestores social)* working with each of the families to identify areas of vulnerability and to develop bespoke strategies or action plans, fitted to the unique circumstances of each family and taking into account their own capabilities, in order to address the identified issues and identify social programs that can help them to overcome these challenges. These strategies focus on nine dimensions important for sustainable development: i) personal identification cards; ii) income and jobs; iii) education and training; iv) health; v) nutrition; vi) housing; vii) family dynamics; viii) banking and savings; and ix) access to justice.[15]

[11] See Unidad para La Atencion y Reparacion Integral a las Victimas (Junio 2013).

[12] See point 2.4 Manual Operativo InfoJuntos 2009.

[13] As of June 2012, the law governing Familias en Accion additionally includes indigenous families. http://www.dps.gov.co/documentos/FA/LEY-FAMILIAS-ACCION.pdf

[14] *Familias en Accion* is a country-wide conditional cash transfer program reaching 2.6 million families. It is aimed at encouraging beneficial health and education related behavior amongst deprived and/or displaced families with at least one child under the age of 18. There is a vast number of papers that report the impact of this program. For example, Attanasio, Fitzsimons and Gomez (2005) report a positive impact on school enrollment and time spent in school. Baez, Camacho and Nguyen (2011) provide a useful and broad review of the evidence.

[15] Our study focuses on the second dimension: income and jobs, since this is the one that if improved is more likely to get the households out of poverty in a sustainable way. Furthermore, the preliminary analysis on the impact

Social workers play an important role in achieving these objectives. The program is designed in theory to provide an intensive period of social support to households through home visits by social workers. These visits occur for up to five years, with the frequency of visits decreasing over time.

These visits are divided into two stages. First, in the initial visits, the social worker works with the household to identify weaknesses and issues which they need to address in order to escape poverty. This is achieved through the completion of the Family Baseline (Linea de Base or LB) questionnaire, which provides an assessment of 45 indicators (or logros). Second, households identify the actions which they need to take in order to achieve their objectives and with the help of the social worker, the household devise a 'family plan' that sets out the main priorities and how to address them. Follow-up visits are then provided to the family, according to its needs and to check on progress towards their objectives. Households 'graduate' from Unidos if they achieve all of their objectives within five years of enrolling in the program. According to the Agencia Nacional de Superación de Pobreza Extrema (ANSPE) around 16% of the beneficiaries families have graduated from Unidos as of August 2014.[16]

The second arm of the program aims to improve the access to existing social programs for those who are eligible from the supply side. This is achieved in two ways. First, the program provides Unidos-eligible families with preferential access to existing social programs. This ensures that households can access these programs. Second, it aims to strengthen these programs by providing improved support for the agencies which manage the provision of welfare benefits. This is done to ensure that sufficient supply of social welfare programs is available for any eligible households who want to enroll and that these programs meet the needs of the targeted population. The combination of the two program arms should therefore serve the dual objectives of increasing the demand for social welfare programs among the poorest households, while simultaneously ensuring that sufficient supply of these services is available to meet any increased demand.

3 The evaluation of Juntos

3.1 Evaluation design

The Juntos pilot program and its evaluation design were initial components of the wider Unidos program. The evaluation was planned in collaboration with ANSPE, the government implementing agency. This evaluation took place in 77 municipalities, which were selected to provide a representative sample of all municipalities in Colombia.[17] As a result, our estimates should be interpreted

of Juntos performed by the Evaluation consortium (see source in footnote 5) showed no impact on any outcomes associated with the other dimensions.

[16] Idem footnote 7.

[17] The consortium, consisting of Econometria, the IFS, Fedesarrollo and S.E.I., who conducted the initial program design and evaluation, found that the selected municipalities did not differ in their observable characteristics from

as externally valid with respect to the impacts of the program across Colombia.

The evaluation employed an experimental design to ensure that individuals in treatment and control groups were comparable along observable and unobservable dimensions. Random assignment to treatment and control groups followed a structured process. First, the population of eligible families within participating municipalities were identified in early 2008. Second, each participating municipality was divided into several neighborhoods, or 'barrios'. Third, between September 2008 and April 2009, each neighborhood was randomly assigned to one of four groups or cohorts. The program was rolled out to cohorts sequentially, so that the treatment began at different times across different neighborhoods. Given random assignment to cohorts, prior to the roll-out of the program the characteristics of households across neighborhoods should be identical on average. This provides us with an opportunity to use neighborhoods in the fourth cohort as a control group for neighborhoods in the first cohort.

Due to the fact that the intensity of treatment was heterogeneous within the treatment group, the evaluation was in theory designed to allow for a more detailed analysis of treatment impacts. More specifically, it permitted testing of whether the impacts of the treatment varied by the intensity, or the number of home visits, received by the household. This was achieved by further dividing the treatment group into 'classic' and 'intensive' treatment groups. This process was conducted in the following way. First, social workers were recruited and randomly allocated to neighborhoods. Second, these social workers were randomly assigned to providing classic or intensive treatment. This process meant that household allocation into the two treatment arms was also random. Social workers who provided intensive treatment were, in theory, assigned fewer cases. This lower caseload would allow a greater focus on each household and enable the social workers to provide a greater number of visits to each household. This was not implemented in practice, as we discuss in the next section.

This design was intended to produce three distinct groups of interest: the control group (fourth cohort), the classic treatment group (first cohort), and the intensive treatment group (first cohort). Given random assignment, the characteristics of households across groups should be identical in the absence of the program. The impact of each treatment type can therefore be estimated by comparing mean outcomes between each treatment group and the control group in the post-program period. The evaluation design also separately identified three sub-populations of interest: rural, urban and displaced. Even within the population of the extreme poor, the impacts of the program are likely to be highly heterogeneous across the three populations.[18] All subsequent analysis will therefore examine each population separately.

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excluded municipalities. See "Evaluación de Impacto de Juntos (hoy Unidos) . Red de Protección Social para la Superación de la Pobreza Extrema Informe de Evaluación – Diciembre de 2011" by Fedesarrollo, Econometria, SEI and IFS.

[18] Most of the displaced households live in urban areas (around 95% of the displaced households in our sample), so their behavior is likely to be most similar to urban households in a number of ways.

In this paper, we evaluate the short-run impact of Juntos over the period between 2009 and mid-2011. The collection of baseline data occurred between November 2009 and March 2010. This period was prior to the initial treatment of all cohorts. Follow-up data was collected between June and August 2011, prior to the roll out of the program in neighborhoods assigned to cohort four. During the period between survey waves, households assigned to cohort one should have received home visits from social workers, while cohort four households should have received no visits. The evaluation finished in December 2011, and treatment had (in theory) subsequently been rolled out to all eligible households across the country.

3.2 Evaluation and program implementation

Large scale evaluations often face a number of challenges in their design and implementation. In this case we need to consider two main issues when estimating the casual effect of the program. First, there is some contamination between the treatment and control groups. More broadly, a low number of visits are reported by all groups at follow up. This suggests that treatment was only weakly implemented for the majority of participants. Second, households in the intensive treatment group did not systematically receive a higher number of visits than those in the classic treatment. We use two measures of the number of social worker home visits received by households in each wave to investigate these issues. These measures are: (i) the official number of visits recorded by the social workers, and (ii) the number of visits which the household report in the household questionnaire (perceived visits).[19]

Figure 1 shows the number of official home visits made to households at baseline and follow up, and distinguishes between households assigned to the treatment and control groups. Figure 2 displays the same information for self-reported or perceived home visits. As exaplained in section 2, social workers provide support to families through home visits, and these visits are organised in principle in sessions according to specific tasks. The social worker and the family co-produce the Family Baseline (first session of the home visits) and Family Plan (second session of the home visits) and each session is expected to comprise two visits to the household. The number of sessions, and their associated number of visits, needed to implement the Family Plan[20] is expected to vary across households according to their needs.[21] Together, these figures present three main points.

First, there is a large discrepancy between the number of official and perceived visits. The initial evaluation suggested that some respondents may have mistaken the evaluation interviewer or public officials for social workers, and therefore report a higher number of visits than they received from

[19] We describe the surveys and questionnaires in more detail in the next section.

[20] The implementation of the Family Plan involves mainly linking the families to the specific social programs that are tailored to their identified needs and strategic priorities organised around the nine dimensions discussed in Section 2.

[21] This information has been taken from the Operative Manual of Juntos, version 24/03/2009, Section 4.1.4. "Fases del Acompanamento Familiar".

the social worker. Anectodal evidence also suggests that some social workers may have visited some households informally more frequently. Unfortunately, there is no information about social workers' characteristics that could shed light on potential variation in the quality of social workers and the quality of the visits in terms of their duration.

Second, treatment was either weak, or not administered at all, for many members of the treatment group. Figure 1 suggests that 25% of the treatment group had received no visits at follow-up, while an additional 20% received only one or two visits. Figure 2 shows a similar pattern in self-reported visits. This low intensity of treatment is unlikely to have produced significant changes in the outcomes of households over the period (even if a more intense version of the treatment would do so).

Finally, 70% of households in the control group report at least one visit at follow up. This is in contrast to the official data, which record no visits to households in the control group. This suggests that households in the control group were visited (perhaps informally) by a social worker; or as mentioned above that they mistook an official or interviewer for a social worker.

Taken together, these figures suggest that some control group households received visits during the pilot phase. Meanwhile, many households who were assigned to the treatment group received no, or only weak, treatment. As a result, randomly assigned treatment status may not accurately represent the actual treatment received.

Tables 1 and 2 show the average number of home visits at baseline and follow-up by treatment group, using official and self-reported visits respectively. These statistics are presented separately for each population type. They suggest that households in the intensive and classic treatment groups did not receive a significantly different number of (perceived or official) home visits at follow-up. Both treatment groups had on average approximately 1.8 official visits at follow-up, regardless of population type. The numbers of perceived visits were higher for all groups. Classic treatment households report a higher number of visits at follow-up on average as compared to intensive treatment households, but the difference is statistically insignificant.

These findings have two implications for our analysis. First, the absence of differences in the number of home visits between the classic and intensive treatment groups suggests that the analysis should ignore the distinction between the groups. We therefore group all treated households together in the remainder of this paper.

Second, the issues of contamination means that in addition to intention to treat (ITT) estimates, we also obtain estimates using an instrumental variables approach. Specifically, we define two treatment dummy variables. The first one is based on the assigned treatment (call this variable 'T'), which gives the ITT estimates. This variable 'T" takes the value of one if a household was originally allocated to intensive or classic treatment and zero otherwise, regardless of the number of official or perceived visits they actually received. The second one is based on self-reported or perceived visits by a Juntos social worker (call this variable 'TR') at the time of the follow-up data collection, which we consider the real treatment. This is because it seems likely that only households that perceive visits from a social worker will be affected by the program, by changing their knowledge and behavior in terms of their use of the available programs and overcoming their extreme poverty condition. The variable 'TR' takes the value of one if a household self-reported to have received three home visits prior to the follow-up and zero otherwise. Hence, households that self-reported less than three visits at the time of follow-up are the controls. We instrument this 'real' treatment variable 'TR' using the variable 'T' that reflects initial random allocation to treatment. We discuss the assumption underlying this empirical strategy in more detail in Section 5.[22]

Table 3 cross tabulates our real treatment 'TR' (according to perceived visits) and randomly assigned treatment 'T' variables, and summarizes the issue of contamination and imperfect compliance for the whole sample. As we explain in the next section, in our analysis we use a selected sample of households and individual members of these households, for which we observe a range of variables of interest in both periods, and the patterns observed in Figures 1 and 2 and Tables 1 to 3 are very similar.

4 Data and descriptive statistics

4.1 Data collection and sample selection

A rich set of data was collected as part of the evaluation. Data were collected in two separate waves. Initial data collection took place between November 2009 and March 2010, prior to the implementation of the Juntos pilot in cohort one neighborhoods (baseline). A second wave of data was collected between June and August 2011 (follow-up). The data contains a rich set of information, including socio-demographic characteristics at both the household and individual level, and individual labor market experiences. In addition, the follow-up data contain information on the knowledge and usage of existing social welfare programs.

These data allow us to focus our analysis on three type of outcomes of particular importance given the aims of the program. These are 1) the knowledge of a range of existing social programs, 2) the take-up of the main Colombian conditional cash transfer program, Familias en Accion, and 3) labor market outcomes, in particular participation rate, employment rate (and type of employment), unemployment rate, and hours worked as well as employment earnings and tenure.

[22] Note that our results are robust to a number of definitions. Results are robust if official number of visits are used instead. Results change little if treatment is defined as two visits or four visits. Results are available upon request.

Given the scale of the evaluation, it was not feasible to sample the entire population of participants for the study. We use a random sample of participants collected across each of the municipalities. These samples were stratified by population type (urban, rural and displaced) and the type of treatment (control, classic and intensive treatment). The sample size for each population was determined prior to data collection by power analysis. In addition, questionnaires of different lengths were administered to different households. Three types of questionnaire were administered (short, medium and long) within cells defined by population type and the survey wave. These assignments were made according to power calculations specific to each outcome of interest.[23] As a consequence, information is available for specific variables of interest for a subsample of households in both waves of the data. This means that we focus on the sample of households and individual members who provided a full set of information for all our variables of interest, at both baseline and follow-up.

Program knowledge and usage information is contained only at follow-up in all cases. A total of 5,872 households were surveyed at baseline but we cannot use all of them in our analysis. Some households drop from the original sample due to classical attrition. Additional households were added at follow-up to increase sample size, increasing the sample to 8,091. When we focus attention to households providing information in both waves, we have a balanced panel of 5,166 households.[24] We further restrict our sample of interest to households with an adult head of household (aged 18 years or older), who provided a full set of answers to questions relating to labor market outcomes. This yields a final sample of 2,446 households. Our analysis also includes individual labor supply outcomes for individuals aged between 18 and 60 years old. The final sample includes 5,042 individuals who fulfill these criteria.

To summarize, selection of households and individuals into our sample may arise from three channels:

1. Classical household attrition - households appear at baseline but are not included in the follow-up sample.
2. Individual attrition - individuals appear at baseline but are not surveyed at follow-up. This may occur even if other members of their household remain in the sample.[25]
3. Questionnaire-type attrition - in principle, households were assigned to different questionnaire types at random.

[23]The use of different questionnaires was due to a limited budget for data collection. For more details of this process, see the evaluation report cited in footnote 5.

[24]13% of the initial sample did not appear in the follow-up survey. This is in line with the average attrition rates in large RCTs.

[25]Individuals may have left the household between waves. In addition, some individuals did not have consistent identifiers across the two periods. We match these individuals across waves using name and gender. We managed to successfully match 82% of all individuals that appear in households in the panel.

One potential implication of the sample restrictions is that selection into treatment and control group is no longer random. We assess whether selection into the final sample was systematically related to treatment status in the following way. First, we take all the sample at baseline and create a binary variable that takes the value of one if a household (or an individual) is in the final sample selected, and zero otherwise. Second, we regress the probability of appearing in the final sample on an indicator of whether the household was originally assigned to treatment, which is available to all households and individuals at baseline as opposed to other key variables for our analysis. We run a second regression that includes some baseline characteristics that are available for all observations, and interacts these characteristics with an indicator of assigned treatment status. This tests whether the interaction of assigned treatment and baseline characteristics are systematically associated with appearing in the final sample among all households present at baseline. If this association is significant, the impact estimates obtains from the sample could be biased. For example if the households who were initially assigned to random treatment only remain in the sample if they have greater income than the households who leave the sample, we would overestimate the impact of the treatment on incomes. This analysis is conducted for each of the three populations to examine selection issues in each sample. We conduct a similar analysis at the individual level, by gender, and by population type.

Table 4 shows the relationship between assignment to treatment and the likelihood of a household appearing in the final sample using the sample of almost 6,000 households at baseline (note that only 2,446 households end up in our final sample). Results are displayed separately for each population type. The table provides two main insights. First, columns 1, 3 and 5 show that treatment status does not predict selection into the final sample across the three samples. However, columns 2, 4 and 6 show a slightly different picture. The F-test, which tests the joint significance of all interactions between household baseline characteristics and assigned treatment status, is significant at the 5% and 10% levels for the urban and displaced populations respectively. This suggests that selection into the final sample appears to be non-random for the urban and, to a lesser extent, the displaced population. In contrast, the results indicate that selection into the final rural sample is random.

Table 5 conducts a similar analysis at the individual level, considering over 14,000 individuals that appear at baseline, and shows the relationship between assigned treatment and the likelihood of an individual appearing in the final sample of 5,042 individuals, by gender and population type. The results are similar to the household analysis, and indicate that the samples of male and female rural individuals are randomly selected. In addition, the male urban sample appears to be randomly selected. The urban female and displaced individual samples remain non-randomly selected.

Taken together, these findings suggest that the estimates of the program effects for the urban and displaced populations should be interpreted with some caution. To address this issue, we estimate regressions using first differences when data are available at baseline.[26] This accounts for permanent differences across individuals which could influence selection into the sample. This would reduce potential bias arising from the non-random selection into the sample for affected samples. We discuss our empirical strategy in more detail in Section 5.

4.2 Pre- and Post-Treatment Characteristics

In this section we present descriptive statistics on pre-treatment and post-treatment income generating activities of household heads and their key socio-demographic characteristics for our panel of households. In addition, we document the knowledge and use of public programs. We do not distinguish between the randomly assigned treatment groups. The following section explores differences across participants in this dimension.

It is important to note that we report unconditional means throughout this section. This means that statistics relating to employment, earnings, tenure, and hours all include zeros for those not active or unemployed. Hence, for example, observed changes in average earnings over time may be a result of a genuine increase in earnings for individuals who are employed, or an increase in the proportion of people who are in employment, and hence reporting any positive earnings.[27]

Socio-demographic characteristics and labor market outcomes of head of households Table 6 shows the mean socio-demographic characteristics and labor market outcomes for our panel of households both at baseline and follow-up by population type. Labor market outcomes refer to the head of households in the panel sample. The table presents two interesting, and broadly positive patterns in the labor market outcomes of these households. First, labor market participation remained relatively stable across the period. This is true for all three populations, with approximately 70% of household heads recorded as economically active.[28] However, the composition of activity changed substantially over the period. Employment rates of household heads increased significantly within each population type, while unemployment fell substantially. For example, 52% of displaced household heads were employed in the baseline. This had increased to 65% for the same sample of households by the follow-up, an increase of 25%. Much of this growth in employment was driven by increases in self-employment. We also observe similar patterns for the other populations. The striking increase in employment rates and decrease in unemployment rates could be related to seasonality, or could be related to a sustained improvement in the labor outcomes of the extreme poor. One thing that we know is that these changes are not a consequence of the introduction of the Juntos program, since we find no systematic and significant difference

[26] This is the case when examining labor market outcomes. Data on social program knowledge and use are unavailable at baseline, necessitating the comparison of levels at follow-up only.

[27] Appendix A includes a detailed description of how the various variables were constructed.

[28] See Appendix A for the exact definition of active used.

between treatment and control heads of households as discussed in Section 5. A further investigation of the factors driving theses changes is an important and interesting question for future research.

Second, wage and salary earnings, and self-employment earnings of household heads increased over the same period. For example, in the displaced sample, the average head of household's wage and salary earnings at baseline was Colombian $96,710 per month (approximately US$ 50). This increased by 13% to Colombian $109,414 at follow-up. Even greater rises are observed in the other populations, with incomes rising by 31% and 38% for urban and rural households respectively. Self-employment earnings increase proportionally more over time across the three populations, although the levels are lower than wage and salary earnings at baseline. These increases in employment income are largely driven by increases in the employment rate of the head of household.[29] Tenure also increased over the period, suggesting that employment was more sustainable.

Table 6 also reveals large differences in the socio-demographic characteristics of households across the three population types. These differences persist throughout the period. Displaced households have, on average, younger head of households (45 years old at follow-up) relative to urban (52) or rural households (55). These head of households are also more likely to be female and more likely to be the main respondent in the survey, less likely to be in a relationship, and have a higher level of education. There are no significant differences in the size of households across population type, although displaced households tend to be younger.

In the final row, we present a municipality level composite index that reflects the quality of public service delivery in each municipality. This increases over time, and is relatively higher for displaced households. This indicates that displaced households are typically located in areas with a higher quality of public services. In contrast, rural households live in areas where the quality is lower.

Taken together, these characteristics suggest that displaced households generally live in better overall economic conditions than households in the other populations. It is important to again note that displaced households are eligible for enrollment in the program, regardless of their SISBEN rating. Evidence of such a pattern is therefore not surprising.

Knowledge, use and supply of public programs Table 7 presents the self-reported knowledge and usage of a selected group of public programs. These aim to provide support to individuals or households in order to foster income generating activities. This features programs that provide

[29] However, it is important to note that despite these large increases in earnings, the monthly employment-conditional earnings or wages of these households' members remain below the minimum monthly wage, as expected for extreme poor households registered as SISBEN level 1. For example, take the individuals showing the highest employment-conditional earnings in our samples: male employees. Their average earnings conditional on employment were Colombian $393,616 (or approximately U$S198), $384,617 (US$193), and $261,732 (US$132) at follow-up for the displaced, urban and rural samples respectively. The minimum monthly wage stood at Colombian $535,600 (or approximately US$269) per month in 2011.

access to credit for micro enterprises, credits for education and subsidized training activities. The table also includes the important program of Familias en Accion. As mentioned in the precious section, Familias en Accion was launched in 2002, and is an established conditional cash transfer program aimed at improving the health and education outcomes of children in poor households.

The table shows the proportion of households who live in municipalities in which each program is active.[30] We do not condition on the availability of services in the municipality, and so do not directly account for differences in the local supply of programs. It is therefore important to note that there is significant variation across populations, and across specific programs, in the availability of these programs. Some programs are available in all municipalities. This includes Familias en Accion, Jovenes Rurales Emprendedores (fosters income generating activities in rural areas),[31] Red Banca de las Oportunidades (provides access to formal microcredit, saving groups and financial education for deprived households and individuals, as well as micro and small enterprises),[32] and Programa para el Desarrollo de las Oportunidades de Inversion y Capitalizacion (fosters income generating activities of poor rural households).[33] Other programs are unavailable to some households in our sample. For example, Jovenes en Accion, which aims to provide vocational training for disadvantaged youth, is not available in all municipalities. This was available to 65% of displaced households, but only 20% of rural households.

The table also shows that knowledge and use of the majority of the programs is very low. With the exception of Familias en Accion, the proportion of households who have knowledge of these programs ranges from 0 to 0.15. Furthermore, use is extremely low, and in most cases is close to zero. Even programs specifically targeted towards the rural population are unknown and infrequently used by rural households in our sample.[34]

Overall, these findings suggest that knowledge and use of existing social programs is low among sample households. This highlights the importance of the objective of trying to promote and improve access to these programs for this population. However, it is concerning that knowledge and use is so low, particularly given that these statistics are reported after the intervention was launched.

[30] This information was provided by the RUAF system administered by the Colombian Ministry of Social Protection.

[31] See http://www.sena.edu.co/oportunidades/emprendimiento-y-empresarismo/Jovenes%20Rurales%20Emprendedores/Pagin Rurales-Emprendedores.aspx for more information.

[32] See http://www.bancadelasoportunidades.com/contenido/contenido.aspx?catID=298&conID=673 for more information.

[33] See https://www.minagricultura.gov.co/tramites-servicios/desarrollo-rural/Paginas/Programa-desarrollo-de-las-oportunidades-de-inversi%C3%B3n-y-capitalizaci%C3%B3n-de-los-activos-de-las-microempresas-rurales-V2.aspx for more information.

[34] Carneiro et al. (2014) report similar findings for the Chilean population enrolled in the Chile Solidario program.

4.3 Baseline Comparisons of Treatment and Control

The availability of baseline data allows us to test whether the randomly assigned treatment and control samples were balanced before the program started. If randomization was successful, baseline characteristics of those assigned to the treatment group (cohort one) will not differ in a statistically significant way from those assigned to the control group (cohort four). We test for balance in each household sample, based on household and head of household characteristics. We also test for balance in each individual sample for both genders.

4.3.1 Household samples

Table 8 compares the baseline means of the assigned treatment and assigned control group of an array of household demographic characteristics and labor market outcomes, for each of the three populations. Columns 1, 3 and 5 report the baseline means of control sample households for displaced, urban and rural households, respectively. Columns 2, 4 and 6 report the estimated difference between treatment and control households.

Overall, the results suggest that the three samples are highly balanced. When we conduct a test of joint significance of the differences in all baseline characteristics, we cannot reject the hypothesis that the characteristics of households in the treatment and control groups are the same in the urban and rural samples. The F-statistics (p-values) are 1.29 (0.214) and 0.93 (0.529) for urban and rural respectively. This is consistent with very few individual statistically significant differences in certain characteristics when examined separately. The displaced sample is marginally unbalanced due to some imbalances in a number of household head characteristics and the age of the household members. This translates into an F-statistic (p-value) of 1.556 (0.095). However, labor market outcomes seem balanced in this sample.

4.3.2 Individual samples

Table 9 presents the results of conducting a test of joint significance of the differences in all baseline characteristics between individuals assigned to the treatment and cohort groups for displaced, urban and rural individuals respectively. We present results separately for males and females. [35]

The first row of first two columns shows that the sample of displaced female individuals is not balanced. In particular, female individuals in the treatment group were more likely to be economically active and to be a formal wage-earner (not displayed in the table). The F-statistic (p-value) for this sample is 2.012 (0.02). However, the sample of displaced male individuals (second row, first two columns of Table 9appears to be balanced with an F-statistic (p-value) of 0.816 (0.67).

[35] The coefficients for each individual characteristic for each sample are available to readers upon request, in some instance we comment in the text on individual variables if these are driving some of the imabalances.

Columns 3 and 4 of the same table show that the sample of both males and females was not balanced at baseline for urban individuals. For females, this is driven by the fact that females in the treatment group were more likely to be formal wage-earners, lived in smaller households, and earn higher wages than their control group counterparts. This results in an F-statistic (p-value) of 1.758 (0.04). Similarly, urban males in the treated sample lived in smaller households (with fewer children), although the labor market variables were not statistically different when tested separately. However, when tested jointly, the male individual characteristics of treatment group participants were statistically significantly different to males in the control group, with an F-statistic (p-value) of 2.036 (0.01).

In contrast, columns 5 and 6 suggest that the individual samples of rural females and males were balanced. Despite some differences in characteristics when tested separately, we cannot reject that the characteristics of females and males in the treatment and controls groups are the same when testing all variables jointly. The F-statistics (p-values) are 0.961 (0.51) and 0.723 (0.77) for females and males respectively.

Together, the results suggest that only a subset of the individual samples are balanced: the rural samples and the displaced sample of males. The individual samples are largely unbalanced for the displaced female individuals and both urban samples, driven to some extent by a few labor market outcomes. It should be noted that that the initial random assignment was made at the household level, and therefore the findings that some sample imbalances occur at the individual level are perhaps unsurprising. Many of the outcomes that we examine are at the household level, for which the samples appear to be balanced for all populations. Nevertheless, these results suggest that we should exercise some caution when interpreting the estimates of program impacts specifically on displaced and urban individuals.

5 Estimating Program Effects

Usually, under a randomized control trial, with no contamination between the assigned treatment[36] ($T = 1$) and control ($T = 0$) groups, it is straight-forward to identify the average treatment effect of a program by taking the difference in the empirical means of the outcome of interest between the two groups. Since the evaluation under consideration has random assignment to treatment, this is the general approach that we adopt here in order to estimate the effects of the program. However, in order to control for the potential influence of (i) contamination of the treatment and control group (i.e. selection into the treatment group); and (ii) selection on observables and unobservables into our sample, we use the baseline information in our panel to augment this basic approach and ensure that our estimates are more robust. The precise approach we take to doing this is discussed in detail below.

[36] Let $T = 1$ for those who were randomly assigned to treatment, 0 otherwise.

Define y_i to be an outcome of interest for an individual (or a household) i. We can now write the expected average treatment effect (D) of Juntos on outcome y for the extremely poor households that received the treatment as: $D = E[y_i|T = 1] - E[y_i|T = 0]$. As mentioned above, since households were randomly assigned to treatment and denoting the sample average $\hat{E}$, we could obtain the average treatment effect by comparing the empirical means of the treatment and control group:[37]

$$\hat{D} = \hat{E}[y_i|T = 1] - \hat{E}[y_i|T = 0] \tag{1}$$

However, as documented in Section 3, there is substantial contamination between the randomly assigned treatment and control groups, and therefore, in the context of the Juntos evaluation under consideration, expression (1) reflects the intention to treat estimate (ITT) as opposed to the average treatment effect (ATE). In order to account for the fact that the evaluation design used cluster randomization, we can rewrite (1) in terms of a linear regression, where the cluster is a "barrio" or neighborhood denoted by j.[38] This specification assumes that v_j and u_{ij} are i.i.d.with constant variance:

$$y_{ij} = \alpha + \beta T + v_j + u_{ij} \tag{2}$$

It can be easily shown that under the stated assumptions, the estimated β reflects the impact of Juntos on the outcome of interest, y_{ij} - i.e. $\hat{\beta}_{OLS} = \hat{E}[y_{ij}|T = 1] - \hat{E}[y_{ij}|T = 0]$. While specification (2) is sufficient for the estimation of the effects of the Juntos program in theory, we take advantage of having panel data in order to increase the precision of our results and to ensure that they are robust. The approach we take to doing this is to augment specification (2) in three ways.

First, in order to improve the precision of the estimates and control for any remaining baseline imbalances, which are important for urban individuals and displaced female individuals, we control for baseline (pre-treatment) relevant characteristics X_{ik} (at the individual and municipality level, municipality denoted by k). Doing this yields the following specification:

$$y_{ijk} = \alpha + \beta T + X_{ik}\gamma + \tilde{v}_j + \tilde{u}_{ijk} \tag{3}$$

Second, as mentioned above $\hat{\beta}_{OLS}$ in specification (3) will estimate the ITT estimate, but not the effect of the actual treatment on those that actually received visits by Juntos social workers. As described in Section 3, we define a variable that we call *real treatment* (TR) that takes the value 1 if households received at least three (perceived) visits by the time of the followup data collection.

[37]See, for example, Duflo et al (2007) for an accessible review of impact evaluation methodologies.

[38]Unfortunately, the survey data did not contain consistent identifiers of these neighborhoods. This variable is important for robust inference in the context of clustered samples. Given this, we take a conservative approach and aggregate neighborhoods in bigger clusters defined by the three original different treatment (control, classic treatment and intensive treatment) within each municipality. See, for instance, Pepper (2002) for a discussion of considering a more aggregate level of clusters in cluster samples. This gives a smaller number of clusters, hence decreasing the power of the analysis. However, as a robustness check we calculate our impact results without clustering the standard errors and the main results are consistent.

In order to estimate the effect of Juntos on those that actually received treatment, we need to use an instrumental variable approach. Therefore, we adopt the standard approach of using the 'assigned treatment' variable (T) as an instrument for actual treatment (TR). By virtue of the fact that 'assignment to treatment' was randomized, it should satisfy the standard independence and relevance assumptions:

$$y_{ijk}|TR \perp T \mid X \tag{4}$$

$$cov(TR, T) \neq 0 \tag{5}$$

Importantly, in addition to the relevance assumption, we also need the stronger assumption of monotonicity, that is the instrument makes every household either weakly more or less likely to actually participate in the Juntos program, which in this case is a reasonable assumption. It is clear that assignment to treatment should increase an individual's propensity to acquire treatment, and randomization should ensure that the *exclusion restriction* is satisfied, with assignment to treatment exerting no influence on the outcome variable, except through treatment itself. This provides identification of the treatment effect in the presence of contamination between the treatment and control groups:

$$\beta_{IV} = \frac{E[y_{ijk}|T_i = 1] - E[y_{ijk}|T_i = 0]}{P(TR_i = 1|T_i = 1) - P(TR_i = 1|T_i = 0)} \tag{6}$$

Under the stated assumptions, this parameter is a measure of the average impact of the Juntos program on a particular outcome, y_i, for households (or individuals) in the sample that received the treatment (or the compliers) as a result of the random assignment.

Third, in our preferred specification we take first differences of the outcome variables in order to remove any unobserved (time-invariant) differences in the level of the outcome variables that may have been present at baseline between the treatment and control group, and that cannot be accounted for by observable characteristics. Removing unobserved time-invariant characteristics can also help correct for selection into our selected sample that can generate a bias. In our empirical analysis, we report estimates using difference-in-difference for both the ITT (OLS estimates using the assigned treatment), as well as the IV estimates.[39] The ITT specification is as follows:

$$\triangle y_{ijkt} = \gamma_0 + \beta T_i + \mathbf{X}_{ik,t-1}\gamma + \mu_{ijkt} \tag{7}$$

In order to implement the IV approach in the difference-in-difference set-up, we substitute T for TR in equation (7) and instrument TR with T as in the level regressions. Before turning to the impact results, Table 10 reports the first stage regressions that predict the probability of a household having reported that they received treatment, defined by the variable TR (which

[39] The results for the levels specification above (specification 3) are very similar. As mentioned above, the results are also robust to: (i) the precise cutoff used in defining the *real treatment* dummy variable and also (ii) to the use of *perceived* or *official* visits as a measurement of treatment. Results available on request.

equals 1 if the household perceived having received at least 3 visits by a Juntos social worker by the time of the followup interview as discussed in Section 3) using assigned treatment as the instrumental variable, for each of the samples analyzed in this paper. The positive and significant coefficient on the assigned treatment variable indicates that on average this variable positively and significantly predicts having received treatment for all the samples. The F-statistics to test for weak instruments are also reported and overall these reject the hypothesis that the instrument is weak in each regression. Results can be seen in Table 10

5.1 Knowledge and use of public programs

We first look at the impact of Juntos on the knowledge and usage of key social programs reported by the household survey respondent. One would expect that this would be one of the first areas in which a social worker would be able to have an influence, since the baseline knowledge of most social programs is low and providing knowledge of, and assisting these families in accessing, the programs they are eligible for seems like an appealing first step in helping to lift them out of poverty.

Table 11 shows the ITT and IV results for the level specification described in equation (3) only, since these variables were only collected at follow-up. As explained above, ideally we would like to estimate a difference-in-difference approach to deal with non-random selection into our panel sample, which is an issue particulalry for urban households, as discussed in section 4.1. Given data limiations, we cannot implement this approach for these outcomes and focus on the level specification. However, we can look at first differences for labor market outcomes as shown in the next section. We can only look at the effect of Juntos on the usage of Familias en Accion, since the usage of the other programs is close to zero (as shown in Table 7) and there is insufficient variation across treatment status. Results are consistent with an increase in the knowledge of Jovenes Rurales Emprendedores, significant at the 5% level, and on the knowledge of Programa para el Desarrollo, significant at the 10% level, by displaced households as a consequence of Juntos (columns 1a and 1b), although we have already seen that the sample of displaced households seems to suffer marginally from imbalances at baseline. Columns 6a and 6b show a positive impact on the proportion of rural households that use Familias en Accion, that is significant at the 10% level only. An ITT estimate shows that Juntos induced an increase of 7.5 percentage points in the probability of using Familias en Accion, the IV estimate is higher at 17.3 percentage points. The overall take-up of Familias en Accion among the poor in both treatment and control rural households is estimated at 57% in our sample. Overall, given the large number of hypotheses being tested, and the small number of coefficients statistically significant, only at the 5 or 10% level, we conclude that there were no positive impacts on the knowledge and use of social programs as a consequence of Juntos.

5.2 Labor market variables

5.2.1 Displaced sample

Table 12 reports the ITT impact estimates and IV regressions for the first difference specification described in equation (7) for the main outcomes of interest. Firstly, note that overall the magnitude of the IV coefficients is larger than the magnitude of the ITT coefficients, as one may expect from the fact that the ITT coefficients use the variable assigned treatment to estimate effect of treatment, and that there has been contamination of households assigned to the control group and imperfect compliance of those assigned to treatment.

Secondly, the IV standard errors are larger, as one may expect. This is also true for the impact estimates for the other urban and rual populations discussed in the next subsections. The only statistically significant result that holds across both the ITT and the IV specification is a positive impact of the Juntos program on the probability of being active for displaced household heads (column 1a). This result is robust to using the level specification described in equation (3). The IV results suggest that displaced head of households are 27 percentage points more likely to be active as a result of the program relative to those households that did not receive treatment. This is a substantial increase, relative to the baseline level for households in the randomly assigned control group of 72% (as shown in Table 8). This positive effect on the probability of being active for household heads is mirrored to some extent in the magnitudes of the estimates for the probability of being employed (column 2a) and the probability of being self-employed (column 3a), and this may provide suggestive evidence that the impact on active status may be driven partially by the group who enter self-employment. However, these coefficients are not statistically significant.

The remainder of the impact estimates for the displaced sample suggest that there is no impact of the program on overall earnings, hours worked or hourly pay. Given that the number of hypothesis being tested using only the IV specification in the tables for each of the three population groups (displaced, urban, rural) is 30, and that the number of significant coefficients is at maximum three, it could well be that these results are found by chance. Furthermore, the sample of displaced households is marginally unbalanced between treatment and control groups (as shown in Table 8). Taken together, we interpret these results as indicative that the Juntos program did not have any effect on the labor outcomes of individuals in the displaced sample.

5.2.2 Urban sample

Table 13 shows similar results for the urban sample. From Table 9 we know that individual samples showed imbalances between assigned treatment and control at baseline, and assigned treatment was shown to be systematically associated with the probability of being in the sample for the household level sample (see Table 4). Results should therefore be considered with some caution.

Only the coefficient on the probability of being active for the sample of women is statistically significant in this Table, and only at the 10 percent level. Overall, there seems to be no consistent impact of Juntos on the population of individuals living in urban areas.

5.2.3 Rural sample

Table 14 reports the results for the rural sample. Here, there are three out of 30 coefficients that are significant in the IV results, both for levels and first difference specifications. This again suggests that the results could be found by chance. Furthermore, the only significant results indicate a negative impact for the sample of women of the Juntos program on the probability of being employed, mirrored by a decrease in the probability of being self-employed and a decrease in hourly wages or pay (largely due to a composition effect or a decrease in the number of individuals that are employed in the first place). However, this negative impact on female self-employment, when combined with the marginally significant positive impact on the take-up of Familias en Accion, is consistent with recent empirical evidence that looks at the impact of conditional cash transfer programs on labor supply and finds a small negative impact for rural women. See, for example, Alzua et al. (2013), who provide evidence of a small negative effect on the probability of being employed for rural women of the conditional cash transfer program Progresa in Mexico.

6 Discussion and concluding remarks

This paper provides an evaluation of the initial phase of the large scale intervention Unidos (i.e. the pilot program, Juntos) in relation to its impact on access to social programs as well as on the labor market outcomes of the extreme poor in Colombia. In addition, this paper makes use of a rich dataset to provide a detailed description of the labor market lives of this traditionally understudied population.

In terms of the evaluation, we find no consistent short-run impact of Juntos on our outcomes of interest: knowledge of a range of existing social programs; the take-up of the main Colombian conditional cash transfer program, Familias en Accion; and a range of labor market outcomes, including the participation rate, employment rate (and type of employment), unemployment rate, and hours worked, in addition to employment earnings and tenure. The estimated zero effect of the program on labor market outcomes is not surprising given: (i) the program failed to influence participants' knowledge about the available social programs, as well as the use of them, and (ii) the fact that the availability of several public programs is low for both the treatment and control groups (see Table 7). One would expect that if the Juntos intervention were to have a substantive effect, the knowledge and usage of social programs would be one of the first important constraints that would be relaxed.

As discussed above, we believe the lack of impact is driven in large part by the lightness of the treatment (i.e. the low number of home visits received by treated households). Given that Unidos, the nationally rolled out counterpart of Juntos, seems to have very similar features, the evidence would suggest that it is unlikely to transform the lives of the extreme poor in Colombia. Even if under Unidos social workers have on average a lower caseload than under the pilot Juntos, this is still more than double the caseload that social workers have under the similar program Chile Solidario. Moreover, although Chile Solidario seems to have an effect on the take up of social programs, it still does not significantly transform the lives of the poor in Chile. This could be rationalized by a lack of direct impact of home visits on the behavior of the poor beyond the take-up of social programs and by uncertainty around the quality of social programs available through the welfare system, take-up of which is supposed to be incentivized through programs such as Unidos and Chile Solidario.

Furthermore, working with these households is particularly difficult given the multitude of constraints they face in different key areas. This includes a lack of skills and capital, and the presence of certain psychological traits. Banerjee et al. (2015) provide empirical evidence that shows that the multifaceted 'Graduation' program implemented in six different developing countries can generate progress in reducing extreme poverty, by improving self-employment income and well-being more generally. This is a multipronged approach that sequentially tackles capital, skill, psychological and informational constraints. The authors document that these programs are very costly to run, though their calculations suggest they are cost-effective in most countries. However, the scale of these programs is small, covering fewer than 11,000 households in six countries. This compares to Unidos, which aims to cover around 1.5 million housholds at a national scale using only public resources. How to optimize and cost-effectively implement programs such as 'Graduation' at a national scale in countries with limited state capacity remains an open question.

In this context, we reiterate the suggestion to policy makers that the Unidos program requires substantial reforms, and these reforms should be evaluated to understand what (if anything) is effective, for which population, and the mechanisms through which these impacts occur. Specific areas should include: (i) an expansion of the supply of programs and widespread promotion to boost knowledge of their existence among potential beneficiaries; (ii) the assessment of which programs are most effective in improving the lives of the poor, in order to guide the selection of programs that should be made available through Unidos, and so improving not only the quantity but also the quality of social programs; (iii) an increase in the budget allocated to social workers to ensure they are suitably skilled and have workloads at least similar to other programs of the same kind (such as Chile Solidario). Importantly, policymakers should strive to guarantee the quality of these evaluations, by avoiding the contamination of the samples and the dilution of the treatment. This last point seems particularly crucial in the case of this evaluation, in which a failure to fully implement the program due to lack of coordination across different state agencies involved, in addition to insufficient funds, led to a poorly implemented policy in both the pilot phase evaluated in this paper and in its final, scaled-up version.

Given the evidence presented in this paper, it seems unlikely that in its current form Unidos is having a substantial positive influence on the livelihoods of the extreme poor in Colombia. One would hope that by iteratively adjusting and improving the program, and appropriately evaluating these changes in order to learn what works and what does not, we might converge on a more cost-effective way to assist this population. Precisely how to do this remains an open question left to future work.

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Appendix A

The data used in this paper was collected primarily for the evaluation of the Juntos program. A secondary advantage of this data is that it contains extremely detailed descriptive and behavioral information pertaining to the lives of a very understudied group of individuals - the poorest members of society. Data collection comprised two waves - a baseline survey, conducted between November 2009 and March 2010 before the start of treatment, and a followup survey, conducted between June 2011 and August 2011, after the treatment group began treatment.

The survey included 77 municipalities, chosen to be representative of the country as a whole. Each municipality was divided into several neighborhoods (clusters), which served as the unit of randomization. Clusters were randomized into one of four cohort groups. Each of these groups commenced with treatment at the different point in time. The impact analysis in this paper compares the outcomes of cohort 1, which received treatment first and therefore was treated prior to the second wave of data collection, with cohort 4, which received treatment last and therefore were designated to be untreated at the second wave of data collection.

Survey Structure

The survey consisted of two parts. The first part collected information on the characteristics of the household, as well as general information on all the members of the household. This part consisted of several detailed modules relating to different aspects of the lives of these individuals. The module containing questions regarding the knowledge and usage of social programs is of particular interest to the current analysis in this paper. The second part of the survey collected detailed health, education and labor market information at an individual level information for all the relevant members of the household.

In order to satisfy the resource constraints of any project, there is often a tradeoff made between the size of the sample and the level of detail of the survey. In this project, this tradeoff was addressed by conducting a shorter survey to a wide sample, while administering a more detailed survey to a smaller subsample of individuals. Consequently, there were two types of questionnaires at baseline ('Long' and 'Short') and three types of questionnaires at followup ('Long', 'Medium' and 'Short'). The Short questionnaire contained core questions that were asked of every household, while the Medium and Long questionnaires asked individuals more detailed information and were administered to a subset of households. The allocation of households to each questionnaire type was done randomly and therefore should not have influenced the selection of our sample for analysis, however this is examined in some detail in the main text.

Matching individuals across waves

Due to the way in which the data was encoded, individuals in the dataset were not assigned a personal identifier number that corresponded across the two waves. Therefore, while it was straight-forward to match households across waves, it was slightly more challenging to match the individuals within these households across waves. In order to do this, we made use of (i) the names of these individuals, as well as, (ii) information regarding their date of birth. However, since there appeared to be a substantial number of inconsistencies in both of these variables,[40] we employed a matching algorithm that used the available information to match individuals who lived in the same household in both waves and appeared to be the same person, up to a small number of errors in their recorded data.

The matching algorithm started by matching individuals within a given household with date of birth and name data that agreed perfectly across waves, and thereafter we matched using a sequence of criteria that relaxed perfect consistency along each of these dimensions. As every step of this matching process, we matched only amongst individuals who are in the same household at baseline and followup, and we matched only amongst the unmatched individuals. Therefore, by starting with the strictest criterion for matching individuals and moving to more relaxed criteria, we limited the chance of making an incorrect match as may occur for example if we were to only use the most relaxed matching criterion for example. Therefore, the guiding principle behind this method for matching individuals was to strike a balance between matching as many individuals as possible, while minimizing the likelihood of making an incorrect match.

The way we implemented this matching process was as follows. First, we matched only those individuals within the same household across waves who have exactly the same name and date of birth recorded in both waves, with no mistakes. Secondly, amongst the unmatched individuals, we allowed for small lexicographical deviations and common spelling mistakes, provided the date of birth is the same. Thirdly, we relaxed perfect consistency along the date of birth dimension, by allowing for deviations in one of: year, month, or day of birth, provided the individual's first and last name matched between waves. Fourthly, we allowed for small errors along both dimensions. Fifthly, amongst the remaining unmatched individuals we matched individuals who had a perfect match for either first and last name, or for their date of birth only. Finally, we manually checked the remaining unmatched individuals within households that were observed in both waves. After completing this process, we selected 1000 individuals randomly to check for accuracy of the procedure, this exercise showed the procedure was extremely accurate. In the end, around 82% individuals members of households appearing in our panel of households were

[40] For example, there were frequently spelling mistakes in names, or alternatively first and second names were often switched. In addition, by examining the raw data, it was often the case that an individual in baseline and followup who was clearly the same person, had a deviation in either day, month or year of birth between the two waves.

matched.

Variable definitions

Active Status: the active variable is an indicator variable, defined for individuals over the age of 17, that takes a value of 1 if the person is either (i) currently employed, (ii) has spent the majority of the last week working, or (iii) has searched for work in the last 4 weeks, and takes a value of 0 otherwise.

Employment: the employed variable is an indicator variable that takes a value of 1 if the person has listed at least one job in which she is currently employed, and a 0 otherwise. Notice that since this variable takes a value of 0 for inactive individuals, this variable reflects unconditional employment, as opposed to employment, conditional on being active. We consider that this way of defining employment status makes much more sense than defining employed as being employed at any point in the last year in the context of the evaluation of the Juntos program. Since treatment only began during the year, it would be a very noisy measure to consider any employment during the preceding year; even in the followup questionnaire much of this employment would have occurred prior to treatment.

Self-Employed and Wage Earners: employed workers in our dataset are divided into two categories - those that work for a wage and those who are self-employed. We therefore define a dummy variable 'wage earner' that equals 1 for those individuals who state that they are currently employed in a job in which they earn a wage, and zero otherwise. Correspondingly, the 'self-employed' variable is a dummy variable equal to 1 for all individuals who state that they are either self-employed or a business owner, and zero otherwise. Both these variables take a value of zero if the individual is unemployed or inactive.

Formal and Informal Workers: wage earners are classified as either formal or informal workers on the basis of whether they reported holding an employment contract. The variable 'wage earner formal' takes a value of one for workers who report holding a contract and otherwise takes a value of zero. Similarly the wage earner informal' takes a value of one for workers who report not holding an employment contract.[41,42]

Wage and Salary Earnings and Self-Employed Earnings: wage and salary earnings are the monthly wages or salaries reported by individuals whose primary current job was as a wage earner. Self-

[41] In this sample the group of self-employed workers would fall into the category of informal workers under most internationally used informality definitions, however since this group is quite different from the set of informal wage earners, we examine the two groups separately.

[42] We also considered two alternative definitions of informality: firstly, one that defines a worker as informal if she works in a firm with fewer than 6 employees; and secondly, one that combines the two definitions, with workers defined as informal if they don't hold a contract *or* work in a firm of fewer than 6 employees. The contract informality and firm informality definitions are highly correlated, and yield similar results. Once we use the combined contract and firm size definition, the proportion of the wage earners defined as informal increases to approximately 90%.

employment earnings are the monthly earnings net of costs for the self-employed. For those who are unemployed or inactive we impute zero values. In addition, for those who reported earning a minimum wage, we did not observe their monthly salary or earnings. We therefore imputed these values using their monthly hours worked and the national minimum wage for the relevant point in time.

Hours Worked and Hourly Earnings: for the set of individuals who reported holding a current job, this variable reflects their self-reported number of hours worked in the last week. In order to calculate the hourly earnings, we multiply the weekly wage by 4.33 to get an approximate number of hours worked in the month. We then divide the wage and salary earnings individuals who are wage earners, or self-employed earnings for self-employed individuals, by this monthly hours worked variable to obtain an hourly earnings.

Tenure: the tenure variable reports the number of months that the individual has spent in her current job, truncated at the date of the interview. This variable was calculated using the start date reported for employed individuals' current job. For unemployed or inactive individuals, we impute a zero value for the tenure variable.

Composite Municipality Level Index: This variable reflects a municipality level variable that is a composite index reflecting the quality of public service delivery in each municipality.

Figure 1: Number of official home visits

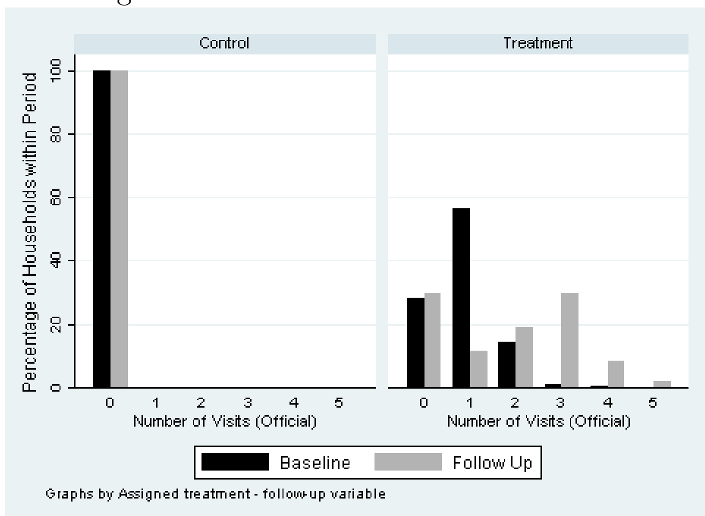

Figure 2: Number of perceived home visits

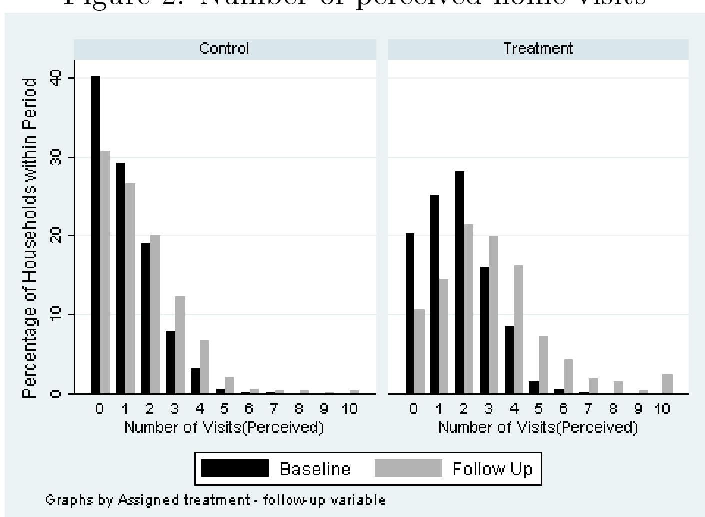

Table 1: Average number of official home visits, by treatment group

	Control	Classic	Intensive
Displaced			
Baseline	0	0.63	0.60
	(0)	(0.69)	(0.69)
Followup	0	1.82	1.87
	(0)	(1.45)	(1.56)
Urban			
Baseline	0	0.95	0.98
	(0)	(0.68)	(0.58)
Followup	0	1.63	1.84
	(0)	(1.37)	(1.49)
Rural			
Baseline	0	1.08	1.04
	(0)	(0.68)	(0.64)
Followup	0	1.85	1.80
	(0)	(1.30)	(1.41)

Notes: (i) The results in this table apply to the entire household panel collected in this survey. However, the results here closely reflect those obtained when examining only the subsample we use for our analysis.

Table 2: Average number of perceived home visits, by treatment group

	Control	Classic	Intensive
Displaced			
Baseline	1.00	1.60	1.42
	(1.09)	(1.21)	(1.24)
Followup	1.60	2.86	2.48
	(1.57)	(2.00)	(1.88)
Urban			
Baseline	0.96	1.84	1.76
	(1.18)	(1.37)	(1.44)
Followup	1.49	3.27	3.08
	(1.58)	(2.14)	(2.19)
Rural			
Baseline	1.21	1.91	1.81
	(1.20)	(1.31)	(1.39)
Followup	1.51	3.05	3.01
	(1.55)	(2.14)	(2.28)

Notes: (i) The results in this table apply to the entire household panel collected in this survey. However, the results here closely reflect those obtained when examining only the subsample we use for our analysis.

Table 3: Real versus assigned treatment

	Perceived Control	Perceived Treatment
Displaced		
Assigned Control	78.14	21.86
Assigned Treatment	52.04	47.96
Urban		
Assigned Control	78.03	21.97
Assigned Treatment	40.42	59.58
Rural		
Assigned Control	75.59	24.41
Assigned Treatment	45.30	54.70

Notes: (i) The results in this table apply to the entire household panel collected in this survey. However, the results here closely those obtained when examining only the subsample we use our analysis.

(ii) Rows in the table indicate percentages within assigned groups.

Table 4: Impact of assigned treatment on the likelihood of being in the household sample

	Displaced		Urban		Rural	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Assigned Treatment	-0.040	-0.328	-0.023	-0.111	-0.015	-0.091
	(0.051)	(0.214)	(0.024)	(0.153)	(0.022)	(0.160)
Baseline Characteristics	No	Yes	No	Yes	No	Yes
F-test (Test of Joint Significance of Interaction of Demographics and Treatment)		1.816		2.175		1.161
P-value		0.064		0.021		0.321
Observations (N)	1 872		1 720		2 280	

Notes: (i) Clustered standard errors are reported in parentheses [*** p<0.01, ** p<0.05, * p<0.1];

(ii) Columns (2), (4) and (6) control for pre-treatment characteristics: labour market status (active/ inactive), age and education level of household head, an indicator for whether thehousehold head is also the household respondent; household size and composition; and an index variable for municipal well-being; as well as the interaction of each of these with the treatment dummy.

(iii) The F-test tests the null hypothesis that the coefficient on all of the pre-treatment characteristics interacted with treatment are jointly equal to zero.

Table 5: Impact of assigned treatment on the likelihood of being in the individual sample

	Displaced				Urban				Rural			
	Female		Male		Female		Male		Female		Male	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)	(6a)	(6b)
Assigned Treatment	-0.027	-0.213	-0.035	-0.392*	-0.024	-0.031	-0.020	0.143	0.013	0.015	-0.008	-0.052
	(0.049)	(0.229)	(0.048)	(0.212)	(0.026)	(0.124)	(0.027)	(0.127)	(0.023)	(0.137)	(0.025)	(0.144)
Baseline Characteristics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
F test (Test of Joint Significance)		3.496		2.097		2.242		0.795		0.640		0.262
P-value		0.000		0.029		0.017		0.634		0.778		0.988
Observations	2 486		2 030		2 422		2 179		2 325		2 646	

Notes: (i) Clustered standard errors are reported in parentheses [*** p<0.01, ** p<0.05, * p<0.1];

(ii) Columns numbered with #b control for pre-treatment characteristics: labour market status (active/ inactive), age, education level, household size and composition, an indicator for the household respondent and household head; and an index variable for municipal well-being; as well as the interaction of each of these with the treatment dummy.

(iii) The F-test tests the null hypothesis that the coefficient on all of the pre-treatment characteristics interacted with treatment are jointly equal to zero.

Table 6: Basic descriptive statistics of pre and post treatment variables at the household level, by population (unconditional means).

	Displaced		Urban		Rural	
	Baseline	Followup	Baseline	Followup	Baseline	Followup
	(1)	(2)	(3)	(4)	(5)	(6)
Labour Market Outcomes						
Active	0.72	0.73	0.70	0.72	0.68	0.68
	(0.45)	(0.44)	(0.46)	(0.45)	(0.47)	(0.47)
Employed	0.52	0.65	0.56	0.68	0.45	0.65
	(0.50)	(0.48)	(0.50)	(0.47)	(0.50)	(0.48)
Self-employed	0.21	0.33	0.26	0.36	0.18	0.32
	(0.41)	(0.47)	(0.44)	(0.48)	(0.38)	(0.47)
Wage earner	0.31	0.32	0.30	0.32	0.27	0.32
	(0.46)	(0.47)	(0.46)	(0.47)	(0.44)	(0.47)
Wage earner formal	0.24	0.27	0.26	0.29	0.26	0.31
	(0.43)	(0.44)	(0.44)	(0.45)	(0.44)	(0.46)
Wage earner informal	0.06	0.05	0.04	0.04	0.00	0.02
	(0.24)	(0.23)	(0.19)	(0.19)	(0.07)	(0.13)
Unemployed	0.20	0.09	0.14	0.04	0.24	0.03
	(0.40)	(0.28)	(0.35)	(0.18)	(0.43)	(0.17)
Wage and Salary earnings	96,710.77	109,413.55	89,923.74	117,865.00	61,307.79	84,691.82
	(180,689.63)	(194,376.30)	(176,192.21)	(215,291.82)	(126,852.94)	(156,234.61)
Self-employment earnings	80,256.46	101,522.70	72,174.68	129,025.71	52,622.36	91,278.83
	(615,045.74)	(192,962.95)	(150,718.21)	(248,960.13)	(295,143.48)	(337,786.21)
Tenure	37.33	64.23	76.02	109.38	95.18	153.22
	(87.71)	(114.94)	(132.25)	(156.55)	(170.88)	(197.08)
Demographic Characteristics:						
Age	42.86	44.75	50.60	51.92	53.03	54.57
	(13.15)	(13.10)	(13.95)	(13.71)	(14.90)	(14.68)
Household Respondent	0.66	0.65	0.59	0.56	0.60	0.55
	(0.47)	(0.48)	(0.49)	(0.50)	(0.49)	(0.50)
In Relationship	0.66	0.66	0.73	0.70	0.73	0.73
	(0.47)	(0.48)	(0.44)	(0.46)	(0.44)	(0.45)
Number of households members	5.23	5.08	5.45	5.00	4.80	4.86
	(2.26)	(2.15)	(2.51)	(2.39)	(2.41)	(2.42)
Male	0.50	0.51	0.64	0.69	0.75	0.77
	(0.50)	(0.50)	(0.48)	(0.46)	(0.43)	(0.42)
No. of Household Members under 10	1.36	1.19	1.24	1.00	1.01	0.98
	(1.23)	(1.16)	(1.31)	(1.20)	(1.26)	(1.24)
No of Household Members over 60	0.24	0.27	0.45	0.46	0.54	0.58
	(0.53)	(0.57)	(0.68)	(0.70)	(0.74)	(0.77)
Years of schooling	4.59	4.82	3.61	3.68	2.33	2.30
	(3.68)	(3.82)	(3.31)	(3.39)	(2.55)	(2.55)
Municipality Level Characteristics:						
Municipality Composite Indice	62.07	63.41	56.24	60.64	54.83	56.86
	(12.49)	(14.13)	(17.78)	(19.00)	(14.93)	(13.94)
N	1,121	1,121	656	656	669	669

Notes: (i) All of the labour market outcomes are unconditional variables in the sense that they take a value of one if true and otherwise take a value of zero. For example, 'employed' equals 1 if employed and 0 if unemployed or inactive.

Table 7: Supply, and self-reported use and knowledge of public programs *post treatment* at the household level, by population (unconditional means).

	Displaced			Urban			Rural		
	Supply	Knowledge	Usage	Supply	Knowledge	Usage	Supply	Knowledge	Usage
Familias en Accion	1.00	0.97	0.76	1.00	0.92	0.62	1.00	0.90	0.57
	(0.00)	(0.18)	(0.43)	(0.00)	(0.28)	(0.49)	(0.00)	(0.30)	(0.50)
Jóvenes en Acción	0.65	0.14	0.00	0.42	0.15	0.00	0.21	0.10	0.00
	(0.48)	(0.35)	(0.07)	(0.50)	(0.36)	(0.06)	(0.40)	(0.30)	(0.00)
Jóvenes Rurales emprendedores	1.00	0.08	0.00	1.00	0.06	0.00	0.98	0.08	0.01
	(0.00)	(0.27)	(0.05)	(0.06)	(0.24)	(0.06)	(0.15)	(0.28)	(0.08)
Crédito ACCES del ICETEX	0.53	0.12	0.01	0.30	0.10	0.01	0.05	0.07	0.00
	(0.50)	(0.33)	(0.07)	(0.46)	(0.29)	(0.08)	(0.22)	(0.26)	(0.00)
Red Banca de la oportunidades	1.00	0.11	0.00	1.00	0.09	0.00	1.00	0.07	0.00
	(0.00)	(0.32)	(0.05)	(0.00)	(0.28)	(0.04)	(0.00)	(0.26)	(0.07)
Generación de ingresos de	0.63	0.31	0.06	0.51	0.14	0.01	0.58	0.14	0.01
	(0.48)	(0.46)	(0.23)	(0.50)	(0.35)	(0.07)	(0.49)	(0.35)	(0.10)
Alianzas productivas	0.20	0.07	0.00	0.20	0.06	0.00	0.25	0.08	0.00
	(0.40)	(0.25)	(0.04)	(0.40)	(0.24)	(0.00)	(0.43)	(0.28)	(0.04)
Programa para el Desarrollo	1.00	0.02	0.00	1.00	0.03	0.00	1.00	0.02	0.00
	(0.00)	(0.15)	(0.03)	(0.00)	(0.16)	(0.00)	(0.00)	(0.14)	(0.00)
Asistencia técnica rural	0.22	0.05	0.00	0.23	0.04	0.00	0.43	0.09	0.00
	(0.42)	(0.21)	(0.03)	(0.42)	(0.19)	(0.00)	(0.50)	(0.29)	(0.04)
N		1121			656			669	

Notes: (i) As discussed in the main text, several programmes appear to be used by almost none of the sample under consideration, and in many cases knowledge of these programmes is very low.

Table 8: Baseline differences between treatment and control groups at the household level

	Displaced		Urban		Rural	
	Control	Treatment - Control	Control	Treatment - Control	Control	Treatment - Control
	(1)	(2)	(3)	(4)	(5)	(6)
Labour Market Outcomes						
Active	0.72	0.00	0.67	0.04	0.69	-0.01
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)
Employed	0.51	0.01	0.56	0.00	0.49	-0.06
	(0.04)	(0.04)	(0.04)	(0.05)	(0.03)	(0.05)
Self-employed	0.20	0.02	0.26	0.01	0.20	-0.03
	(0.03)	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)
Wage earner	0.31	0.00	0.31	-0.01	0.29	-0.04
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)
Wage earner formal	0.05	0.02	0.03	0.01	0.01	-0.01
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
Wage earner informal	0.26	-0.02	0.27	-0.02	0.28	-0.03
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)
Unemployed	0.21	-0.02	0.11	0.05	0.20	0.05
	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.04)
Wage and Salary earnings	93291.46	5172.80	96539.73	-9797.04	67111.28	-8844.05
	(14,839.69)	(18,361.41)	(11,800.52)	(15,017.70)	(11,690.35)	(13,895.82)
Self-employment earnings	60888.00	29301.02	68720.30	5115.30	46339.84	9574.04
	(10,862.09)	(29,922.13)	(11,764.81)	(15,074.85)	(9,314.25)	(20,206.92)
Tenure	39.60	-3.44	80.83	-7.13	105.82	-16.22
	(6.07)	(7.17)	(8.36)	(11.07)	(12.09)	(16.33)
Demographic Characteristics:						
Age	44.37	-2.286**	50.76	-0.23	52.37	1.00
	(0.96)	(1.14)	(1.12)	(1.27)	(1.21)	(1.48)
Household Respondent	0.62	0.063*	0.56	0.04	0.59	0.03
	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
In Relationship	0.69	-0.046*	0.70	0.05	0.72	0.01
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)
Number of households members	5.18	0.08	5.77	-0.467*	5.08	-0.427*
	(0.10)	(0.15)	(0.20)	(0.24)	(0.21)	(0.25)
Male	0.55	-0.062*	0.65	-0.01	0.74	0.01
	(0.02)	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)
No. of Household Members under 10	1.34	0.02	1.40	-0.239**	1.13	-0.18
	(0.11)	(0.12)	(0.08)	(0.10)	(0.09)	(0.12)
No of Household Members over 60	0.30	-0.103**	0.46	0.00	0.52	0.03
	(0.04)	(0.05)	(0.05)	(0.06)	(0.05)	(0.07)
Years of schooling	4.44	0.23	3.29	0.48	2.44	-0.17
	(0.23)	(0.29)	(0.36)	(0.41)	(0.23)	(0.26)
Municipality Level Characteristics:						
Municipality Composite Indice	62.81	-1.12	56.20	0.06	54.05	1.19
	(3.10)	(3.67)	(4.39)	(5.40)	(2.76)	(3.60)
F-Test	F(127,15) =	1.556	F(145,15) =	1.29	F(117,15) =	0.93
P-value	0.095		0.214		0.529	
Clusters	128		146		118	
N by Group (Control / Treatment)	380	741	213	443	230	439
N	1,121		656		669	

Notes: (i) The variables wage earner, wage earner formal, wage earner informal and unemployed have been omitted from the F-test due to perfect collinearity;
(ii) standard deviations in parentheses, t-tests: *=10%, **=5%, ***=1%

Table 9: Baseline differences between treatment and control groups at the individual level by gender, by population

	Displaced		Urban		Rural	
	F-test	P-value	F-test	P-value	F-test	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
Female	F(120,17)=2.012	0.02	F(141,17)=1.758	0.04	F(112,17)=0.961	0.51
Male	F(112,17)=0.816	0.67	F(135,17)=2.036	0.01	F(109,17)=0.723	0.77

Notes: The variables included in the F-test are the same variables included in Table 11.

Table 10: First-stage regressions

	Displaced			Urban			Rural		
	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men
Dependent Variable: 1(Perceived Treatment)	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)
Assigned Treatment	0.295***	0.276***	0.221**	0.486***	0.507***	0.498***	0.430***	0.461***	0.522***
	(0.076)	(0.081)	(0.087)	(0.047)	(0.054)	(0.057)	(0.061)	(0.057)	(0.056)
Age	0.000	-0.001	-0.001	-0.001	0.000	0.001	0.000	0.000	0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Education	-0.006	-0.006*	-0.004	0.002	0.004	0.002	0.003	0.006	0.004
	(0.004)	(0.003)	(0.005)	(0.006)	(0.005)	(0.005)	(0.007)	(0.006)	(0.006)
Number of HH members	-0.018*	-0.006	-0.003	0.014	0.005	-0.007	0.016	0.018	0.009
	(0.011)	(0.013)	(0.012)	(0.009)	(0.013)	(0.013)	(0.010)	(0.012)	(0.011)
# HH Members under 10	0.046***	0.036*	0.036*	-0.022	-0.014	0.003	-0.021	-0.022	-0.011
	(0.017)	(0.020)	(0.020)	(0.021)	(0.025)	(0.026)	(0.020)	(0.023)	(0.020)
# HH Members over 60	0.030	0.020	0.029	-0.073**	-0.044	-0.030	-0.041	-0.022	-0.050
	(0.031)	(0.040)	(0.040)	(0.033)	(0.040)	(0.039)	(0.035)	(0.038)	(0.050)
In Relationship (= 1)	0.075*	0.058*	0.082*	-0.019	-0.002	0.059	0.034	-0.033	0.010
	(0.040)	(0.030)	(0.048)	(0.050)	(0.038)	(0.060)	(0.055)	(0.045)	(0.059)
Household Respondent (= 1)	0.052	-0.009	0.012	-0.040	0.021	-0.060	-0.040	0.050	-0.054
	(0.049)	(0.041)	(0.053)	(0.037)	(0.032)	(0.047)	(0.041)	(0.045)	(0.046)
Municipality Composite Indice	-0.005**	-0.005*	-0.004	-0.003**	-0.002**	-0.003*	-0.002	0.000	-0.001
	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Male (= 1)	-0.008			-0.067			-0.079		
	(0.039)			(0.046)			(0.048)		
Household Head (= 1)		0.033	-0.075		0.036	-0.041		0.004	-0.030
		(0.036)	(0.050)		(0.040)	(0.051)		(0.060)	(0.063)
Constant	0.517***	0.522***	0.477***	0.433***	0.270**	0.309**	0.309**	0.088	0.105
	(0.176)	(0.184)	(0.175)	(0.139)	(0.128)	(0.137)	(0.157)	(0.131)	(0.150)
F-statistic	15.000	11.500	6.470	106.910	89.040	75.450	50.180	65.710	87.850
P-value	0.000	0.001	0.012	0.000	0.000	0.000	0.000	0.000	0.000
R Squared	0.117	0.103	0.0776	0.229	0.243	0.245	0.175	0.196	0.253
Clusters	128	121	113	146	142	136	118	113	110
Observations (N)	1,121	1,354	966	656	790	648	669	632	652

Notes: Standard deviations reported in parentheses, t-tests: * significant at 10%, ** at 5%, *** at 1%. The F-statistic is equivalent to Kleibergen-Paap rk Wald F statistic and the Angrist-Pischke multivariate F test of excluded instruments.

Table 11: Treatment effect of Juntos on knowledge and usage of social programs

	Displaced				Urban				Rural			
	Knowledge		Usage		Knowledge		Usage		Knowledge		Usage	
	ITT	IV	ITT	IV	ITT	IV	ITT	IV	ITT	IV	ITT	IV
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)	(6a)	(6b)
Familias en Accion	0.009	0.031	0.046	0.155	0.007	0.015	0.015	0.030	0.037	0.087	0.075*	0.173*
	(0.013)	(0.042)	(0.039)	(0.134)	(0.041)	(0.084)	(0.044)	(0.089)	(0.033)	(0.079)	(0.043)	(0.104)
Jóvenes en Acción	0.014	0.048	--	--	0.006	0.013	--	--	0.021	0.049	--	--
	(0.045)	(0.158)			(0.035)	(0.072)			(0.029)	(0.065)		
Jóvenes Rurales emprendedores	0.039**	0.130**	--	--	-0.031	-0.064	--	--	-0.003	-0.006	--	--
	(0.017)	(0.058)			(0.022)	(0.045)			(0.026)	(0.061)		
Crédito ACCES del ICETEX	0.000	0.099	--	--	-0.001	-0.029	--	--	0	-0.002	--	--
	(0.001)	(0.103)			(0.002)	(0.053)			(0.002)	(0.053)		
Red Banca de la oportunidades	0.007	0.024	--	--	0.033	0.068	--	--	0.000	0.000	--	--
	(0.024)	(0.079)			(0.023)	(0.045)			(0.021)	(0.047)		
Generación de ingresos de Accion	0.046	0.145	--	--	-0.01	-0.020	--	--	-0.043	-0.099	--	--
Social	(0.049)	(0.143)			(0.031)	(0.065)			(0.049)	(0.110)		
Alianzas productivas	0.000	0.050	--	--	-0.001	-0.058	--	--	-0.000	0.004	--	--
	(0.001)	(0.052)			(0.002)	(0.043)			(0.002)	(0.058)		
Programa para el Desarrollo	0.013*	0.045	--	--	0.015	0.031	--	--	-0.004	-0.009	--	--
	(0.007)	(0.028)			(0.013)	(0.025)			(0.010)	(0.023)		
Asistencia técnica rural	0.004	0.013	--	--	-0.021	-0.043	--	--	-0.002	-0.004	--	--
	(0.013)	(0.043)			(0.017)	(0.035)			(0.028)	(0.063)		
N	1121				656				669			

Notes: (i) OLS regressions used to estimate the ITT using assigned treatment. IV regressions instrument perceived treatment with assigned treatment.

(ii) Robust and clustered standard errors reported in parentheses, t-tests: * significant at 10%, ** at 5%, *** at 1%. Regressions included the same baseline characteristics as those characteristics included in the first-stage regressions.

(iii) Impact on usage of programmes cannot be estimated due to lack of variation for all programmes except for Familias en Accion. As shown in section 4 usage is close to zero for most programmes.

Table 12: Treatment effect of Juntos on participation, employment and earnings, displaced population, first-difference specification

	Active			Employed			Self-Employed			Wage Earner			Unemployed		
	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)	(5a)	(5b)	(5c)
ITT															
Assigned Treatment	0.081**	0.015	0.048*	0.056	0.005	0.019	0.079	-0.006	0.017	-0.023	0.011	0.002	0.024	0.009	0.029
	(0.034)	(0.034)	(0.027)	(0.060)	(0.051)	(0.071)	(0.054)	(0.034)	(0.064)	(0.049)	(0.033)	(0.057)	(0.048)	(0.035)	(0.065)
IV															
Perceived Treatment (IV: Ass. Treatment)	0.273**	0.053	0.215	0.191	0.019	0.085	0.269	-0.021	0.077	-0.077	0.040	0.008	0.082	0.034	0.130
	(0.122)	(0.118)	(0.144)	(0.183)	(0.181)	(0.307)	(0.175)	(0.125)	(0.279)	(0.172)	(0.112)	(0.253)	(0.172)	(0.132)	(0.310)

	Hours Worked Per Week			Wage and Salary Earnings			Self-Employment Earnings			Hourly Wage			Tenure		
	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men
	(6a)	(6b)	(6c)	(7a)	(7b)	(7c)	(8a)	(8b)	(8c)	(9a)	(9b)	(9c)	(10a)	(10b)	(10c)
ITT															
Assigned Treatment	3.387	0.152	1.559	-24,654	-8,641	-19,468	-28,265	200	-4,946	-177.6	16.7	-89.9	7.8	3.9	4.0
	(3.638)	(2.669)	(4.332)	(22,958)	(14,768)	(25,153)	(45,044)	(7,621)	(18,448)	(326.9)	(87.5)	(135.8)	(15.3)	(5.8)	(14.0)
IV															
Perceived Treatment (IV: Ass. Treatment)	11.465	0.550	7.041	-83,444	-31,279	-87,943	-95,666	724	-22,341	-601.3	60.5	-406.3	26.4	14.1	18.2
	(11.130)	(9.495)	(18.733)	(86,864)	(57,610)	(123,258)	(154,468)	(27,275)	(86,114)	(1,108)	(305.4)	(684.5)	(48.3)	(18.7)	(59.6)
Clusters	128	121	113	128	121	113	128	121	113	128	121	113	128	121	113
Observations (N)	1,121	1,354	966	1,121	1,354	966	1,121	1,354	966	1,121	1,354	966	1,121	1,354	966

Notes: (i) Robust and clustered standard deviations reported in parentheses, t-tests: * significant at 10%, ** at 5%, *** at 1%. Regressions included the same baseline characteristics as those characteristics included in the first-stage regressions.

Table 13: Treatment effect of Juntos on participation, employment and earnings, urban population, first-difference specification

	Active			Employed			Self-Employed			Wage Earner			Unemployed		
	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)	(5a)	(5b)	(5c)
ITT															
Assigned Treatment	-0.044	-0.082*	-0.015	-0.006	-0.050	-0.004	0.067	-0.000	-0.021	-0.073	-0.050	0.018	-0.038	-0.032	-0.011
	(0.044)	(0.048)	(0.049)	(0.052)	(0.047)	(0.061)	(0.043)	(0.033)	(0.042)	(0.047)	(0.035)	(0.060)	(0.030)	(0.035)	(0.034)
IV															
Perceived Treatment (IV: Ass. Treatment)	-0.090	-0.163*	-0.030	-0.012	-0.099	-0.007	0.138	-0.001	-0.043	-0.15	-0.098	0.036	-0.078	-0.064	-0.022
	(0.091)	(0.097)	(0.097)	(0.107)	(0.094)	(0.121)	(0.087)	(0.065)	(0.083)	(0.098)	(0.067)	(0.119)	(0.062)	(0.068)	(0.067)

	Hours Worked Per Week			Wage and Salary Earnings			Self-Employment Earnings			Hourly Wage			Tenure		
	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men
	(6a)	(6b)	(6c)	(7a)	(7b)	(7c)	(8a)	(8b)	(8c)	(9a)	(9b)	(9c)	(10a)	(10b)	(10c)
ITT															
Assigned Treatment	-0.208	-1.259	4.869	-13,193	-13,837	31,085	7,663	964	-14,999	89.4	4.3	-23.9	6.8	-5.0	10.9
	(3.170)	(2.050)	(3.421)	(19,048)	(11,129)	(21,438)	(21,177)	(13,966)	(18,028)	(155.1)	(130.7)	(171.3)	(17.8)	(6.1)	(11.8)
IV															
Perceived Treatment (IV: Ass. Treatment)	-0.428	-2.484	9.773	-27,163	-27,295	62,401	15,777	1,901	-30,110	184.1	8.5	-48.0	14.0	-9.9	21.8
	(6.460)	(4.025)	(6.866)	(38,961)	(21,940)	(42,644)	(43,279)	(27,233)	(35,264)	(315.5)	(255.1)	(339.3)	(36.0)	(12.3)	(23.4)
Clusters	146	142	136	146	142	136	146	142	136	146	142	136	146	142	136
Observations (N)	656	790	648	656	790	648	656	790	648	656	790	648	656	790	648

Notes: (i) Robust and clustered standard deviations reported in parentheses, t-tests: * significant at 10%, ** at 5%, *** at 1%. Regressions included the same baseline characteristics as those characteristics included in the first-stage regressions.

Table 14: Treatment effect of Juntos on participation, employment and earnings, rural population, first-difference specification

	Active			Employed			Self-Employed			Wage Earner			Unemployed		
	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)	(5a)	(5b)	(5c)
ITT															
Assigned Treatment	-0.011	-0.081	0.001	0.049	-0.084*	0.063	0.021	-0.074**	0.006	0.028	-0.010	0.057	-0.060	0.003	-0.062
	(0.045)	(0.068)	(0.041)	(0.046)	(0.044)	(0.063)	(0.058)	(0.035)	(0.079)	(0.049)	(0.027)	(0.069)	(0.040)	(0.051)	(0.053)
IV															
Perceived Treatment (IV: Ass. Treatment)	-0.026	-0.175	0.001	0.114	-0.182*	0.121	0.049	-0.160**	0.011	0.065	-0.022	0.110	-0.140	0.007	-0.119
	(0.104)	(0.149)	(0.078)	(0.107)	(0.099)	(0.118)	(0.135)	(0.081)	(0.150)	(0.111)	(0.058)	(0.127)	(0.095)	(0.110)	(0.097)

	Hours Worked Per Week			Wage and Salary Earnings			Self-Employment Earnings			Hourly Wage			Tenure		
	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men	HH Head	Ind: Women	Ind: Men
	(6a)	(6b)	(6c)	(7a)	(7b)	(7c)	(8a)	(8b)	(8c)	(9a)	(9b)	(9c)	(10a)	(10b)	(10c)
ITT															
Assigned Treatment	2.984	-1.720	4.059	13,297	-4,808	18,945	4,423	-5,499	6,326	159.4	-200.3**	146.1	18.2	-9.2	23.3
	(2.350)	(1.774)	(2.990)	(14,410)	(5,367)	(18,503)	(31,2320)	(4,541)	(17,342)	(196.6)	(79.9)	(134.2)	(20.1)	(9.0)	(20.0)
IV															
Perceived Treatment (IV: Ass. Treatment)	6.935	-3.728	7.778	30,906	-10,422	36,304	10,279	-11,922	12,122	370.4	-434.2**	280.0	42.2	-20.0	44.5
	(5.397)	(3.859)	(5.546)	(32,846)	(11,543)	(34,142)	(71,663)	(10,048)	(32,883)	(449.3)	(188.8)	(250.2)	(45.9)	(19.7)	(37.1)
Clusters	118	113	110	118	113	110	118	113	110	118	113	110	118	113	110
Observations (N)	669	632	652	669	632	652	669	632	652	669	632	652	669	632	652

Notes: (i) Robust and clustered standard deviations reported in parentheses, t-tests: * significant at 10%, ** at 5%, *** at 1%. Regressions included the same baseline characteristics as those characteristics included in the first-stage regressions.